CHAPTER 2

ENTREPRENEURSHIP AS RESEARCH DOMAIN

WHY DISTINGUISH BETWEEN THE PHENOMENON AND THE RESEARCH DOMAIN?

Now that we have devoted an entire chapter to discussing what entrepreneurship is, there shouldn't be much need for a chapter delineating the research domain "entrepreneurship", should there? Entrepreneurship as a research domain aims at better understanding of the phenomenon we call "entrepreneurship", so now that we "know" what it is, why not just go out and study it?

Paradoxically, the research domain cannot be equated to the study of empirical cases known to qualify under the definition of entrepreneurship that we discussed in the previous chapter. How can that be? Most importantly, although including an outcome criterion is desirable when we discuss entrepreneurship as a societal phenomenon, it becomes a burden when we think of entrepreneurship as a research domain. This is because we have to be able to study entrepreneurship as it happens, before the outcome is known. It would be awkward indeed not to know until afterwards whether one was doing "entrepreneurship research" or not. It would also be a bit hard on the researcher to require that every empirical study of "entrepreneurship" should await and assess the outcome on every relevant level. Researchers must be allowed to go deeply into aspects of the process without following up on the outcomes—and still be acknowledged for doing "entrepreneurship research". That is, *attempts* to offer buyers new choices should suffice.

A second very important reason for making a distinction between the phenomenon and the research domain is that previous and current entrepreneurship practice does not necessarily have all the answers needed to develop *normative* theory about entrepreneurship. That is, there may be better ways to learn meaningful things about entrepreneurship than finding real cases of "best practice". To study what successful entrepreneurs *have* done is important, but an even more important and interesting question is what *could* be done. As entrepreneurship scholars we should be able to answer such questions, too, if we are the experts at abstracted sense making that we claim to be (Davidsson, 2002). This implies that the research domain should include purely theoretical development as well, and that empirical entrepreneurship research may be well advised to study also induced entrepreneurial situations, such as experiments or simulations (cf. Baron & Brush, 1999; Fiet & Migliore, 2001; Sarasvathy, 1999a).

Yet other reasons for distinguishing between the phenomenon and the research domain also deserve mentioning. The behavior-plus-outcome definition lures one into a retrospective view that compresses time and de-emphasizes the process aspects of entrepreneurship. It may therefore be advisable to have a domain delineation that explicitly highlights the process nature of entrepreneurship. To study the processes as they happen is important in order to avoid selection and hindsight biases; topics we will revert to in chapters to come. Further, although the inclusion of a socially beneficial outcome clarifies the role of entrepreneurship in the economy, it may have detrimental effects on the long term credibility of entrepreneurship research in political and fellow academic circles if we portrayed the micro-processes that we study as "always good". When the creation of new economic activity is studied real time or the outcomes for other reasons have not been carefully assessed, it is advisable for entrepreneurship researchers to have an open attitude to the possibility of different types of outcomes on different levels.

PREVIOUS ATTEMPTS AT A DOMAIN DELINEATION

I am certainly not the first to suggest that we need a delineation of the research domain, and not just a definition of the phenomenon. So maybe we can get some help here? The (American) Academy of Management Entrepreneurship Division Domain Statement (cited from Gartner, 2001) reads as follows:

> Specific Domain: the creation and management of new businesses, small businesses and family businesses, and the characteristics and special problems of entrepreneurs. Major topics include: new venture ideas and strategies; ecological influences on venture creation and demise; the acquisition and management of venture capital and venture teams; self-employment; the owner-manager; management succession; corporate venturing and the relationship between entrepreneurship and economic development.

Well, on second thought...maybe not? As I see it, there is absolutely nothing wrong with any of the areas of interest listed above. Neither is there anything wrong with a researcher embracing all of them. I tend myself to have a keen interest in most of what is included in the statement, as evidenced by the research I have conducted and published over the years. But over those same years I have become increasingly concerned about including them all under the same entrepreneurship label. That is, I share the fear that it is precisely this kind of all-inclusive delineation that gives the entrepreneurship domain a "hodgepodge" or "potpourri" appearance, which hinders theory development and academic legitimacy (Gartner, 2001; Low, 2001; Shane & Venkataraman, 2000). Referring back to Chapter 1, the Entrepreneurship Division Domain Statement is a disharmonic mix of the "independent business" view ("management of...small businesses and family businesses"; "self-employment; the owner-manager; management succession") and the "micro-level novel initiative" view ("creation of...new businesses"; "new venture ideas and strategies; ecological influences on venture creation"; "corporate venturing"). Despite probably being a fair description of the research interests members of that association represent—and, again, there is nothing wrong with those interests—the Entrepreneurship Division domain statement is not a very effective starting point for optimal knowledge accumulation concerning the phenomenon we were dealing with in Chapter 1.

Many readers may have been surprised—and more than so—that I did not include Shane & Venkataraman's (2000) entrepreneurship definition in the opening of the previous chapter. This would seem a peculiar omission as theirs has arguably been the most influential conceptual contribution to entrepreneurship research in recent years (possibly equaled by Sarasvathy, 2001). The reason is that Shane & Venkataraman (2000) wisely suggested not just another attempt at defining the entrepreneurship phenomenon, but precisely the scholarly domain. So here is the more proper place to discuss their definition of the field of entrepreneurship, which reads:

> [T]he scholarly examination of how, by whom, and with what effects opportunities to create future goods and services are discovered, evaluated, and exploited (Venkataraman, 1997). Consequently the field involves the study of sources of opportunities; the processes of discovery, evaluation, and exploitation of opportunities; and the set of individuals who discover, evaluate, and exploit them (p. 218).

They further point out the following three sets of research questions as especially central: 1) why, when and how opportunities for the creation of goods and services come into existence; 2) why, when and how some people and not others discover and exploit these opportunities; and 3) why, when and how different modes of action are used to exploit entrepreneurial opportunities. In the subsequent dialogue they agreed with Zahra & Dess (2001) that the outcomes of the exploitation process represent a fourth important set of research questions, adding that outcomes on the level of industry and society should be considered as well (cf. Venkataraman, 1996, 1997; Zahra & Dess, 2001). As regards antecedents of the process and its outcomes they emphasize the characteristics of individuals and opportunities as the first-order forces explaining entrepreneurship and hold that environmental forces are second order (Shane & Venkataraman, 2001). They describe their approach as a disequilibrium approach (cf. Shane & Eckhardt, 2003). They highlight variations in the nature of opportunities as well as variations across individuals. In short, they depict the economy as fundamentally characterized by *heterogeneity*. Further, they point out that entrepreneurship does not require, but can include, the creation of new organizations (cf. Simon in Sarasvathy, 1999b, pp. 11; 41-42; Van de Ven, 1996).

I have detailed elsewhere (Davidsson, 2003a) the many merits I think this domain delineation has, and will not repeat all of that here. Suffice it to say that it is largely in line with the entrepreneurship definition we discussed in Chapter 1, and that the many positives arguably made Shane & Venkataraman's framework the best effort so far to delineate entrepreneurship as a distinct research domain. One of the few debatable points is the general primacy given to the individual and the "opportunity". This does not seem go give much room for entrepreneurship research on more aggregate levels of analysis (cf. Zahra & Dess, 2001). A more important question mark, perhaps, is their adopting Casson's (1982) definition of opportunity as "those situations in which goods, services raw materials and organizing methods can be introduced and sold at greater than their cost of production" (cf. Singh, 2001). I would argue that this makes their definition get stuck only halfway towards clearly distinguishing between the phenomenon and the domain. Shane & Venkataraman (2000) hold that, among other things, we should study with what effects "opportunities" are exploited. But with Casson's definition of opportunity, entrepreneurship becomes characterized by *certainty* rather than uncertainty regarding one important aspect of the effects of the pursuit of opportunity: it is profitable. As I see it, Casson's definition is compatible with the definition of the entrepreneurship phenomenon that we developed in the previous chapter, but largely unhelpful for entrepreneurship as a research domain. This is because defining "opportunity" this way is inconsistent with having the outcomes of entrepreneurship as an open research question. This apparent weakness of Shane & Venkataraman's exposition points at a more general problem in the entrepreneurship literature, namely that "opportunity" is becoming a central concept but one which often is ill-conceptualized or applied in an inconsistent manner. We will have reason to come back to this problem later on.

Several of Bill Gartner's many writings on entrepreneurship (for example, Gartner, 1988, 1990, 1993, 2001; Gartner & Brush, 1999) can also be regarded more as attempts to delineate the field of research than defining and describing the phenomenon. Gartner's view—which he is careful to present as a suggestion for re-direction rather than a formal "definition" (Gartner, 1988)—is that entrepreneurship is the creation (or emergence; cf. Gartner, 1993) of new organizations. This choice of focus has two origins. One was a perceived lack of treatment of organizational emergence in organization theory. Somehow organizations were assumed to exist; theories started with existing organizations (cf. Katz & Gartner, 1988). The other was a frustration with the pre-occupation that early entrepreneurship research had with personal characteristics of entrepreneurs. For these reasons, Gartner (1988) suggested that entrepreneurship research ought to focus on the *behaviors* in the process of organizational *emergence.*

This view certainly has a lot to commend it. For one thing, it has a clearly defined focus, addressing terrain that economics as well as management studies have treated in a step-motherly fashion. This clear focus gives promise of giving unique contribution and avoiding over-extending the field of entrepreneurship research. Further, Gartner's view has a strong process orientation. The main problem I have with Gartner's (1988) approach is that whereas organizing is an important aspect of the exploitation process, he does not emphasize the discovery process (cf. Shane & Venkataraman's domain delineation above). Further, his approach directs no or only cursory attention to the possibility of alternative modes of exploitation for given "opportunities" (Shane & Venkataraman, 2000; Van de Ven, Angle & Poole, 1989). If interpreted as a delineation of the (entire) research domain his take on entrepreneurship appears overly narrow in these regards. In short, I see Gartner's focus as the natural task for an organization theorist to take on *within* a somewhat broader domain.

Below I will try to create precisely that: a somewhat broader, yet sufficiently precise, domain delineation. What an incredibly pretentious thing to do! Well, the reason that I dare try is that I can stand on the shoulders of Gartner and Shane & Venkataraman, as well as their predecessors. The little trick I will attempt below is the sewing together of their respective perspectives while ironing out the little wrinkles I think I've found, in order to arrive at a coherent domain delineation, tailor-made for entrepreneurship research.

MY SUGGESTED DOMAIN DELINEATION

First, I take from Gartner (1988) the idea that entrepreneurship research should study behavior in the process of emergence. That's three very important components right there: *behavior, process* and *emergence.* Based on Shane & Venkataraman (2000) I take the point that we should distinguish between two sub-processes: *discovery* and *exploitation* (I include "evaluation" in the discovery process). Further, in line with the view of entrepreneurship that we developed in Chapter 1, I agree with their notion that entrepreneurship research should not study only or primarily the emergence of new (independent) organizations, but the emergence of *new market offerings* (they say "new goods and services") through different *modes of exploitation.* From Venkataraman (1997), Shane & Venkataraman (2001) and Zahra & Dess (2001) I also adopt the idea that entrepreneurship research should study a variety of *outcomes* on different levels. The final element I take from Shane & Venkataraman (2000) is the idea that entrepreneurship research should adopt as a fundamental assumption that the economy is characterized by *heterogeneity* (they discuss this under the "disequilibrium" label).

To this I only need to add two little pieces, which I have touched upon already. The first is to adopt the additional fundamental assumption that the economy is also characterized by *uncertainty.* The second is that empirical entrepreneurship research need not and should not be restricted to the study of empirical cases known to qualify as "entrepreneurship" à la our definition of that phenomenon in the previous chapter. Entrepreneurship research should also study *failure* and *induced* processes of emergence.

Piecing it all together, I arrive at the following (cf. Davidsson, 2003a):

> Starting from assumptions of uncertainty and heterogeneity, the domain of entrepreneurship research encompasses the study of processes of (real or induced, and completed as well as terminated) emergence of new business ventures, across organizational contexts. This entails the study of the origin and characteristics of venture ideas as well as their contextual fit; of behaviors in the interrelated processes of discovery and exploitation of such ideas, and of how the ideas and behaviors link to different types of direct and indirect antecedents and outcomes on different levels of analysis.

Now, I can assure that there is no shortage of information hidden in those few lines, so it would be really nice if at this point the reader could stop, reflect, re-read—and perhaps start counter-arguing or asking follow-up questions. After playing that game for a couple of rounds, I'd be delighted if the reader imbibed my own elaborations below.

Uncertainty and Heterogeneity

It could be debated whether one should really let this type of assumptions restrict a research domain. My rationale for including them is that I firmly believe that a theory or a research design that assumes that economic aggregates (such as an *industry,* or *demand*) are made up of the sum of identical micro-level entities, is unlikely to be a fruitful starting point for understanding or researching the entrepreneurship phenomenon. For example, individuals are heterogeneous with respect to experience, skills and cognitive capacity (Cohen & Levinthal, 1990; Conner & Prahalad, 1996; Shane & Venkataraman, 2000; Van de Ven et al, 1989) and also have heterogeneous motivations (Birley & Westhead, 1994). Two important aspects of organizational heterogeneity are governance structure (Coase, 1937; Foss, 1993; Williamson, 1999) and resources (Barney, 1991; Cohen & Levinthal, 1990; Collins & Montgomery, 1995; Foss, 1993; Galunic & Rodan, 1998; Greene, Brush & Hart, 1999; Penrose, 1959; Teece, Pisano & Shuen, 1997). Whether or not a new venture evolves within an existing organization the external environment in a broader sense will also be heterogeneous (Baumol, 1990; Chandler & Hanks, 1994) and the characteristics of the external environment may have profound effects on what venture ideas are attractive and likely to succeed (Zahra & Dess, 2001). Heterogeneity also occurs over time. Individuals and organizations learn and change over time and whether or not they choose to remain in the "same" environment, the characteristics of the environment are not stable, either (Aldrich, 1999; Aldrich & Martinez, 2001; Miner & Mezias, 1996). It follows from all this heterogeneity that the universe of perceptible and profitable opportunity is not the same for all individuals or organizations, and that therefore they will come up with different venture ideas and different exploitation strategies. Importantly, they will also have different views on what constitutes a successful or acceptable outcome (Gimeno, Folta, Cooper & Woo, 1997; Venkataraman, 1997).

Neither do I think it is illuminating for the understanding of this phenomenon to start from a view of reality as characterized by certainty and calculable risk alone. I'd be the last to argue that all decisions for all actors are non-calculable. However, the situations in which behaviors aimed at creating new economic activity are undertaken often have this characteristic. That is, information collection and processing, careful planning and calculation cannot give a conclusive and reliable answer as to whether something will be successful or not. Only (trial) implementation will tell. In short, such situations have a substantial element of genuine, Knightian uncertainty (Knight, 1921). That is, the future is not only unknown, but also unknowable (Sarasvathy, Dew, Velamuri & Venkataraman, 2003).

Here I disagree with the same Kirzner (1973) that I leant so heavily on in the first chapter. Very rarely are entrepreneurial situations certain in the way Kirzner portrays them. In one passage Kirzner likens entrepreneurial opportunity with realizing that a free ten-dollar bill is resting in one's hand, ready to be grasped. If we should use the ten-dollar bill metaphor at all, I would suggest the true situation is more like spotting the bill from your balcony. From that distance one would face the (calculable) risk that the bill was for anything from one to a hundred dollars. But moreover, while you dash down the stairs it may blow away, or someone else may get it before you, or it may turn out upon closer look that it was not a real money note, after all. There is no way the finder can tell before she takes the decision to run down the stairs. Or maybe one should change the perspective and view opportunity as the uncertain prospect of perhaps being able to make one's own ten-dollar bills—and get away with it? Either way, in order to understand behaviors in such situations it is important to start from a theoretical perspective that acknowledges or even emphasizes uncertainty.

Processes of Emergence; Behaviors in the Interrelated Processes of Discovery and Exploitation

Shane & Venkataraman state as their point of departure (2000, p. 217) that "For a field of social science to have usefulness it must have a conceptual framework that explains and predicts a set of empirical phenomena not explained or predicted by conceptual frameworks already in existence in other fields." One of Gartner's (1988; 1993; 2001) great strengths is that he has pointed out one such empirical phenomenon: the process of emergence. Other fields of research simply haven't done a very good job here, and therefore entrepreneurship research can make a real contribution if it takes on this challenge.

I agree with Shane & Venkataraman (2000) that both discovery and exploitation are required for entrepreneurship to happen, and that both should be studied in entrepreneurship research. So again, I disagree with Kirzner's (1973, p. 47) claim that "Entrepreneurship does not consist of grasping a free ten-dollar bill which one has already discovered to be resting in one's hand; it consists of realizing that it is in one's hand and that it is available for the grasping." That is, he holds that entrepreneurship consists solely of discovery; exploitation is presumably "something else". But returning to the balcony, nothing much happens if we just note that a ten-dollar bill seems to be lying down there, does it? How Kirzner makes restricting entrepreneurship to (instantaneous) discovery match his notion that entrepreneurship consists of the "competitive behaviors that drive the market process" beats me. There seems to be an underlying assumption in his reasoning that every actor who perceives an opportunity not only knows with certainty that it really is an opportunity, but also necessarily acts upon it. Entrepreneurship researchers know that such is not the case. Many of us just have to exercise a little introspection to realize that.

Our emphasis on the interrelated processes of discovery and exploitation as new economic activities emerge implies that a very central set of research questions for entrepreneurship research concerns what individuals and other economic entities actually *do* when they initiate, refine, and realize ideas for new business ventures. With a slight rewrite of Shane & Venkataraman's third central research question we get:

> Why, when and how are different modes of action used to discover and exploit venture ideas?

This area needs much more investigation over and above the tentative steps that have been taken so far (e.g., Bhave, 1994; Carter, Gartner & Reynolds, 1996;

Chandler, Dahlqvist & Davidsson, 2002; 2003; Delmar & Shane, 2002; Fiet & Migliore, 2001; McGrath, 1996; Samuelsson, 2001; Sarasvathy, 1999a)

The term *discovery* may be suspected to reflect an objectivist view on venture ideas. That is, the term seems to suggest that they somehow exist "out there", ready to be discovered. This is not a perspective I purport. Rather, like Shane & Eckhardt (2003), I use the term "discovery" to maintain consistency with prior literature, despite its potentially misleading connotations. Discovery refers to the conceptual side of venture development, from an initial idea to a fully developed business concept where many specific aspects of the operation are worked out in great detail, especially as regards how value is created for the customer and how the business will appropriate some of the value (Amit & Zott, 2001; de Koning, 1999b, p. 121). Importantly, discovery is a *process*—the venture idea is not formed as a complete and unchangeable entity at a sudden flash of insight. Thus, it includes not only what is elsewhere called "idea generation", "opportunity identification" and "opportunity detection", but also "opportunity formation" and "opportunity refinement" (Bhave, 1994; de Koning, 1999a, 1999b; Gaglio, 1997).

Exploitation is a negatively loaded word is some contexts, and may therefore evoke negative associations. In the present context, I would suggest it is a netral term referring to the decision to act upon a perceived opportunity, and the behaviors that are undertaken to achieve its realization. The exploitation process deals primarily with resource acquisition and co-ordination, as well as market making (see Shane & Eckhardt, 2003; cf. also Sarasvathy, 1999a; Van de Ven, 1996). This includes all research questions pertaining to the organizing of new ventures, that is, the research agenda that Gartner (1988; 2001) emphasizes. Exploitation thus simply means the attempted realization of ideas. Like discovery, exploitation is a process that may or may not lead to the attainment of profit or other goals.

The emphasis on the interrelatedness of the two processes is based on empirical insights (Bhave, 1994; Sarasvathy, 1999a, 2001). I think discovery and exploitation are best conceived of as overlapping processes. This is what Figure 2:1 tries to portray.

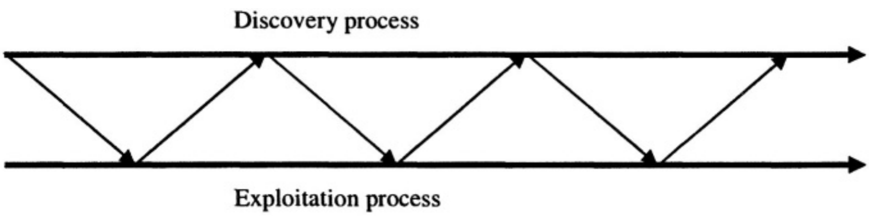

Figure 2:1 The interrelation between discovery and exploitation

For example, an entrepreneurial process may start with an individual perceiving what she thinks is an opportunity for a profitable business [discovery]. In the efforts to make this business happen, contacts with resource providers and prospective customers [exploitation] make it clear that the business as initially conceived will not be viable [feedback to discovery]. The individual changes the business concept accordingly [discovery] and continues her efforts to marshal and coordinate the resources needed for the realization of the revised business concept [exploitation]. Although the above process starts with an element of discovery, this is not necessarily always the case. Empirical research suggests that venture creation processes can follow almost any sequence (Carter et al, 1996; Gartner & Carter, 2003), and Bhave's (1994) study indicates that the insight (or discovery) that a problem solution one has developed for one's own needs may present a business opportunity often comes rather late in a process that initially did not have the creation of a new venture as a goal.

Real or Induced, and Completed as Well as Terminated

These are issues that we dealt with already in the beginning of the present chapter. The practicing entrepreneurs the world has seen so far do not necessarily have all the answers. That is, pure theory development and laboratory research methods may sometimes prove better avenues to arrive at normatively valid results and theories (Fiet, 2002). As a case in point, one of the most interesting and influential developments in recent years, namely Sarasvathy's reasoning on effectuation vs. causation processes, emanates from research on induced (or hypothetical) entrepreneurial processes (Sarasvathy, 1999a, 2001).

Further, if we were to study successfully completed cases only, there is no telling whether terminated cases shared the same characteristics. This is especially important with regard to risk-taking and its correlates. Risk-taking should increase the span of possible outcomes. That is, the entrepreneur who takes risks should be rewarded with a greater likelihood of great success. At the same time, however, that entrepreneur incurs an increased risk of making a big splash. If our research design censors the terminated cases, we will systematically misinterpret the effects of risky strategies and actions.

Across Organizational Contexts

Again, this has been thoroughly dealt with already. In Chapter 1 we parted from the "independent business" perspective on entrepreneurship. Shane & Venkataraman (2000) make a major point of this issue, emphasizing different modes of exploitation (such as internal venturing vs. the setting up of a new firm) as a core set of research questions for entrepreneurship research. This is in apparent conflict with Gartner's perspective. It is important to note, however, that Gartner's "creation of new organization" should not necessarily be read as "creation of new, owner-managed firms". Gartner (1988, p. 28) explicitly discusses internal venturing. Although he—arguably with good reason—regards the emerging new firm as a particularly promising arena for studying it, his interest is in "organizing" in the Weickian sense (Gartner, 2001, p. 30, cf. Gartner & Carter, 2003), not necessarily the creation of *formal* and legally defined organizations.

Across organizational contexts has additional meaning beyond opening up for the study of discovery and exploitation both in emerging and existing firms, small and large, owner-managed or else. This is also where we can start inviting back to the party those organizational changes in quadrant II of Figure 1:2, which in the previous chapter were defined as not being instances of entrepreneurship. *Change* in organizational context as *explicitly related to* the creation of new, market-related activity is clearly within the entrepreneurship research domain. Studies referred to by Ucbasaran, Westhead & Wright (2001. p. 64) showing that management buy-outs are followed by increased development of new products, are therefore examples of entrepreneurship research. Likewise, longitudinal empirical tests of Stevenson's argument that certain organizational changes would facilitate entrepreneurship in established organizations (Stevenson, 1984; Stevenson & Jarillo, 1986) would clearly be instances of entrepreneurship research (Eliasson & Davidsson, 2003).

Those latter examples presume a shift of "organizational context" within the same organizational entity. The emerging venture may also lead a life that cuts across several different organizations. What originates as an idea by an independent inventor may be acquired into an existing small firm, which is later acquired by a large organization, which decides to spin out this particular part of their business operations. This points to the need for studies of processes of emergence that use the venture idea itself as the unit of analysis (Davidsson & Wiklund, 2001). Such studies would follow samples neither of individuals nor of organizations, but precisely *new, emerging activities*—i.e., venture ideas and what evolves around them—from their conception and through whatever changes in human champions and organizational contexts might occur along the way. In some cases what originated as a *de novo* start-up is transferred to an existing firms; in other cases what originates within a firm may be spun out at an early stage. This is something we will also return to in later chapters.

New Business Ventures; Venture Ideas and Their Contextual Fit

"Business ventures" here should be interpreted broadly. It includes independent start-ups as well as new internal ventures, and also new market offers that are so limited that the actors involved do not necessarily conceive of them as entire "ventures". However, in line with our putting entrepreneurship in a market context in the previous chapter the suggested domain delineation is restricted to new *business* ventures.

The reader may have noted that I have put "opportunities" within quotation marks in several places above, and started to sneak in the concept *Venture Idea* in its stead. This is very, very intentional, of course. The term "opportunity" refers to something not yet realized. The increased use of the this term in entrepreneurship research therefore signals the sound development that the field is really turning towards a focus on emergence, rather than starting from existing firms and established business founders. However, there is a huge linguistic problem with adopting "opportunity" as a central concept in entrepreneurship research. By almost any definition, an opportunity is something *known* to be favorable. But we just said we did not like that, by making uncertainty a fundamental assumption in our domain delineation. That is, the use of the term "opportunity" for an unproven venture idea is *fundamentally opposed to acknowledging uncertainty* as an inescapable aspect of the environment of the emerging activity and/or organization that the entrepreneurship scholar tries to understand. At the time, the actors cannot *know* whether or not they pursue an "opportunity". Neither can the researcher.

"Opportunity" also seems to be a notion that arouses a lot of controversy among entrepreneurship researchers. Some seem to regard opportunity as objectively existing in the environment, whereas others hold that opportunity is created by the entrepreneur (cf. Davidsson, 2003a). To a considerable extent this is a semantic battle, where a lot of the confusion arises from the fact that the term "opportunity" is used in at least four different ways in the entrepreneurship literature: i) for a set of external conditions known in retrospect to be favorable (to some people) for the successful discovery and exploitation of new business activities, ii) for a set of external conditions thought (by some people) but not proven to be existing and favorable for the successful discovery and exploitation of new business activities, iii) for specific new venture initiatives known in retrospect to be viable, and iv) for specific new venture initiatives that are currently being pursued but whose viability is not yet proven.

As I see it, "opportunity" should really only be used for the i) and possibly the iii) category. I suggest *Venture Idea* as a more appropriate and less controversial term for category iv) [which over time will also include category iii) as a subset]. A venture idea may start as a very rudimentary idea of a technically possible product, or the perception of an unsolved problem that a market segment would be willing to pay to get solved, if one could find a solution to the problem. Over time it may be changed, honed, and elaborated to qualify as what others would called a *business concept* or a fully developed (conception of a) *business model.* The venture idea is, so to speak, the focal object of the discovery and exploitation processes.

Referring back to Figure 1:2, venture ideas are ideas for new products or services or bundles thereof; introducing a new price/value relation; imitative entry, and new markets. Relating also to the heterogeneity issue, this shows that venture ideas come in different flavors. A seriously under-research area, I would argue, concerns the characteristics of new venture ideas and how these characteristics relate to antecedents, behaviors and outcomes. Samuelsson (2001; 2004) represents one of the few entrepreneurship studies that have explored the nature and effects of characteristics of venture ideas, and followers are needed. Although an abundance of studies have tried to assess the characteristics of entrepreneurs, very few have focused on the characteristics of the venture ideas they pursue. Interestingly, this disproportionate interest in the individual is shared by diffusion research, where only about one percent of the close to 4,000 studies have focused on the characteristics of the innovation, whereas more than half of them focus on the individuals who adopted them (Rogers, 1995). An explanation for this might be the general human tendency that psychologists have dubbed "the fundamental attribution error". This is to seek explanations to events in terms of the characteristics of the individuals involved, also when structural or situational factors are the true determinants (Ross, 1977). Researchers beware!

The above said does not mean that entrepreneurship research should forget about "real opportunities". For the first central research question that Shane &

Venkataraman (2000) pointed out, we may be well advised to maintain this objectivist stance. With a slight rewrite, this central research question reads:

> Why, when, where, how and for whom does opportunity for the creation of new goods and services come into existence?

I have here added "where" and "for whom". I have also changed "opportunities" to "opportunity" as an uncountable to veer away from the view that a finite number of well specified, ready-to-use opportunities exist out there, waiting to be discovered (such as lost ten-dollar bills, for example). Questions concerning "real" opportunity can be asked for different types of entities or levels of analysis, for example for nations, regions or other spatial units over time or across space, as well as for organizations, industries or population sub-groups. Asking such questions is a prerequisite for building strong theory about where opportunity will emerge in the future. Building such theory is a challenging but important aspect of scholarship in entrepreneurship, which feeds directly into entrepreneurship education (cf. Davidsson, 2002) where learning where to look for opportunity should be one of the most central features (cf. Drucker, 1985; Vesper, 1991). However, proven opportunity can only be studied in retrospect. And it gets worse. When you think of it, it is impossible to know the universe of not-yet-developed, but potentially viable, venture ideas. In one context there may be abundant opportunity but little actual venturing, because of cultural inhibitions or because non-entrepreneurial opportunity is also abundant. In another context there may be high levels of desperate venturing due to lack of other alternatives. Therefore, not even the number of venture ideas that are both acted upon and proven successful is a direct measure of objective opportunity density. It is inescapable that whatever measure is used for opportunity density, it will be a proxy measure.

Finally, as regards contextual fit, questions of this kind also arise from the heterogeneity issue. Seriously under-researched questions concern fit between individuals' prior knowledge and (information about) the new venture idea (e.g. Cooper, Folta & Woo, 1995; Fiet, 1999; Shane, 2000); relatedness between organizations' prior knowledge, resources or capabilities and (information about) the new potential venture (e.g., Cohen and Levinthal 1990; Teece et al, 1997; Van de Ven 1996); fit between existing resources and what strategies can lead to the venture's success (e.g., Chandler & Hanks, 1994) and fit between characteristics of the new potential venture and current user practices (e.g., Raffa, Zollo & Caponi, 1996). Empirically based knowledge on these issues is limited, which means abundant opportunity for research contributions.

Antecedents and Outcomes on Different Levels of Analysis

This should be easy enough. It is standard research practice to ask questions about antecedents and outcomes. But let's see. First, generalizing Shane & Venkataraman's (2000) second research question to several levels of analysis, and substituting "venture ideas" for "opportunities", we arrive at the following:

> Why, when and how do individuals, organizations, regions, industries, cultures, nations (or other units of analysis) differ in their propensity for discovery and exploitation of new venture ideas?

This is straightforward enough. One implication is that entrepreneurship research can be conducted on any level of analysis as long as antecedents on that level *are explicitly related to discovery and exploitation of new venture ideas.* Thus, again, we can re-invite the organizational issues in quadrant II of Figure 1:2. The relationships between organizational characteristics and change on the one hand, and discovery and exploitation of new venture ideas on the other, are important questions for entrepreneurship research. However, those who think narrowly of entrepreneurship as dealing with the firm level of analysis should reflect on the fact that there are many other levels of analysis that are of equal relevance on the entrepreneurship research agenda. The opportunities and challenges involved in researching entrepreneurship on those *different* levels of analysis will be the central theme in chapters to come.

The "propensity" in the question should not be limited to quantitative but also to qualitative differences. For example, due to the distinction between "opportunity-based" and "necessity-based" entrepreneurship, nations and regions may have similar firm start-up rates for very different reasons, and representing very different levels of real, profitable opportunity (Davidsson, 1995a; Reynolds, Camp, Bygrave, Autio & Hay, 2001). The same problem is likely to occur on the organizational level. A firm desperately struggling for its survival may take more new initiatives than a firm that is doing well, even if better objective opportunity is available for the latter (March & Sevón, 1988).

To Shane & Venkataraman's (2000) three original questions we can add a fourth:

> What are the outcomes on different levels (e.g., individual, organization, industry, society) of efforts to exploit venture ideas?

The first implication here is that the interest of entrepreneurship research is very, very far from restricted to the question of the financial performance of firms. At this stage of reading, migrants and visitors from strategic management should start to understand why the notion of entrepreneurship as a sub-field of strategy is, should we say, somewhat incomplete. As we have delineated entrepreneurship research here, the strategic management questions that are also entrepreneurship questions constitute a corner of the totality of the entrepreneurship domain. It should be clear by now that many disciplines and sub-disciplines cover different aspects of the research domain we have delineated. It should be equally clear that no *one* other existing discipline or sub-discipline covers the entirety of what we here see as entrepreneurship research.

I would suggest that in showing a genuine interest in outcomes on different levels, and in providing a more refined and empirically informed view on "failure", entrepreneurship can distinguish itself from other fields and make strong contributions to social science at large (cf. Low, 2001; Venkataraman, 1997). The question of when successful venture level outcomes are and are not associated with successful outcomes on the societal level, and vice versa, is highly relevant but seldom asked. It is conceivable that under certain circumstances the successful pursuit of ideas for new ventures does not benefit society (cf. Baumol, 1990). It is also possible to conceive of a situation where entrepreneurial efforts on the whole benefit society while at the same time the most likely outcome on the micro-level is a loss—and that therefore the rational decision is to refrain from entrepreneurship (cf. Olson in Sarasvathy, 1999b, p. 35). Both of these situations represent important problems that entrepreneurship research can help societies to solve or avoid. The question of differential outcomes on different levels can also be asked from the perspective of the corporate manager: when and why does and does not new venturing—successful or not on the venture level—contribute to company performance? Again, because of potential learning and cannibalization the answer is not a simple one to one relationship between venture- and organizational level outcomes.

Referring back to Figure 1:3, the issue of catalyst ventures, then, is of particular interest. Too narrow or simplistic a view on "failure" may lead to gross misrepresentation of the benefits of attempts to create new business activity, on micro- as well as aggregate levels. What in a narrow perspective appears to be a "failure" may instead be a beneficial "catalyst" either because those directly involved in the "failure" learn for the future or because others imitate. A possible outcome of deeper and more refined research into apparent "failure" is that pure failure as defined in Figure 1:3 is far less usual than previously thought (cf. Gimeno et al, 1997, pp. 69, 72). I think one of the first things entrepreneurship scholars should try to get rid of is the bias against failure. In addition to the "catalyst" potential, both theory and empirical evidence actually suggest that experimentation that may end in failure as well as the demise of less effective actors are necessary parts of a well-functioning market economy (Davidsson et al, 1995; Eliasson, 1991; Reynolds, 1999; Schumpeter, 1934).

We should not forget that there are qualitatively different *types* of outcomes, too. Entrepreneurial processes do not only have financial outcomes, and affect not only those directly involved in the project. Supplementary outcome assessment may concern, e.g., satisfaction, learning, imitation and retaliation. For researchers who have the creativity and guts to be unconventional there are plenty of opportunities—or should I say "research ideas"?—that await your discovery and exploitation.

SUMMARY AND CONCLUSION

In this chapter I have argued that even though the object of entrepreneurship research is to understand the phenomenon we call entrepreneurship, our research cannot be delimited to the study of proven empirical instances of entrepreneurship. Instead, I suggested the following domain delineation for entrepreneurship research:

> Starting from assumptions of uncertainty and heterogeneity, the domain of entrepreneurship research encompasses the study of processes of (real or induced, and completed as well as terminated) emergence of new business ventures, across organizational contexts. This entails the study of the origin and characteristics of venture ideas as well as their contextual fit; of behaviors in the interrelated processes of discovery and exploitation of such ideas, and of how the ideas and behaviors link to different types of direct and indirect antecedents and outcomes on different levels of analysis.

Building on a combination and extension of earlier contributions by Gartner (1988; 1993; 2001) and Shane & Venkataraman (2000; 2001) the domain I suggest for entrepreneurship research is broader than either of these predecessors. This inclusive attitude may lead to a less distinctive domain. If one looks closely at the above domain statement, it is clear that most core research questions in entrepreneurship would fit in *some* existing discipline or sub-discipline. It is equally clear, however, that entrepreneurship is not in its entirety a sub-division of any *one* established discipline or field of research. More importantly, if left to the disciplines, there is no guarantee that a lot of research would be conducted on the most central questions of entrepreneurship, as we have here outlined that domain. Many of these questions may be peripheral to every discipline (cf. Acs & Audretsch, 2003b). Therefore, *a failure to collectively cover the entrepreneurship research agenda is neither a problem nor a shortcoming on the part of the existing disciplines.* When maximum knowledge development about entrepreneurship is the vantage point, however, this is a very real and important problem. This is the most important *raison d'être* for entrepreneurship research as a distinctive domain and research community.

Now, after this long warming up it's about time we get to the real stuff: method-related opportunities and challenges of entrepreneurship research. So that's what we'll throw ourselves over next, and stick to for the remainder of this book: empirical design and analysis issues. Oh, well, perhaps not...there was this little thing called "theory" that we have to deal with first...

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