
Abstract

What is entrepreneurship research? Entrepreneurship as a research domain cannot be restricted to proven cases of entrepreneurship as defined in Chap. 1. This is because in order to understand the societal phenomenon as defined in Chap. 1, the research domain needs to understand also the choice not to engage in entrepreneurship and the reasons for failure to succeed at it. Combining ideas from prior literature, this chapter develops and discusses a delineation of the entrepreneurship research domain, focusing on the process of (completed or aborted) emergence of new economic ventures across organizational contexts.

2.1 Why Distinguish Between the Phenomenon and the 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? First, we cannot learn why some initiatives manage to perform the societal function of entrepreneurship while others do not by only studying the successful cases. In order to see what sets them apart, we need to study failed attempts as well. That is, 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 further emphasized by our need to be able to study entrepreneurial processes

concurrently, in real time, before the outcome is known. It would be awkward indeed not to know until afterward 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” 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, and even processes that unintentionally (at early stages) could lead to such outcomes should qualify.

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 (Fiet & Patel, 2008). That is, there may be better ways to learn meaningful things about entrepreneurship than finding real cases of “average practice” or even current “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 sensemaking that we claim to be (Davidsson, 2002). This implies that the research domain should also include purely theoretical development (e.g., Baron, 2008; McMullen & Shepherd, 2006) and that empirical entrepreneurship research may be well advised to study not only naturally occurring entrepreneurial behavior but also induced entrepreneurial situations, such as experiments or simulations (cf. Crawford & McKelvey, 2010; Grégoire & Shepherd, 2012).

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 deemphasizes the process aspects of entrepreneurship (Dimov, 2007; see also Chap. 8). It may therefore be advisable to employ 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 develop in chapters to come. Further, the inclusion of a socially beneficial outcome clarifies the role of entrepreneurship in the economy. However, 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 “good by definition.” I suggested “pick your heroes!” once already. When the creation of new economic activity is studied in 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. If a significant proportion of what we study in fact appear to be “redistributive ventures” (see Fig. 1.3 and surrounding text), we should see and report just that.

2.2 Previous Attempts at a Domain Delineation

Many readers may have been surprised—and more than so—that I did not include Shane and 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, achieving close to 8000 citations (at this point in time) on Google Scholar, receiving the AMR Decade Award for most cited paper (Shane, 2012; Venkataraman, Sarasvathy, Dew, & Forster, 2012), and having a major influence on the Entrepreneurship Division's new domain statement (cited above). The reason is not that the first version of this text was penned in 2003. I was well aware of the work back then, having heard Shane present it to us in Jönköping prior to publication in 1999 and also having discussed it with Venkat in 2000 when we were both keynote speakers at the same conference (in my current hometown Brisbane, as it were). Instead, the reason is that Shane and Venkataraman (2000)—originally Venkataraman (1997)—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. (Shane & Venkataraman, 2000: 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 and 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. Shane & Venkataraman, 2001; Venkataraman, 1997). In Davidsson (2003), I detailed the many merits I think this domain delineation has over what preceded it:

- They try to delineate the scholarly domain rather than suggesting yet another definition of the societal phenomenon. As just discussed, this distinction is important.
- Focusing on the creation of future goods and services, their delineation directs attention to the problem of emergence. This adds a distinctive feature to entrepreneurship research, an element that is missing in established theories in economics and management.
- They put the main focus on goods and services rather than including organizational change per se or creative behavior in any context. They thereby carve out a domain that has a manageable size and relatively clear boundaries and which is

consistent with Kirzner's (1973) notion that entrepreneurship is what drives the market process.

- While retaining an interest in individuals, they emphasize their actions (entrepreneurship) and fit with the specific “opportunity” rather than general characteristics of entrepreneurs. They thereby avoid the dead end of “trait research.”¹
- As to openness, their domain delineation includes two partly overlapping processes, discovery and exploitation. This refutes the view that discovery is instantaneous and that entrepreneurship consists solely of discovery (cf. Kirzner, 1973).
- No mention is made of the age, size, or ownership of the organizations in which “opportunities” are pursued. They even point out the existence of alternative modes of exploitation for given “opportunities” as an important research question. Hence, the stated domain includes corporate entrepreneurship. By implication, small business research is included only when it deals explicitly with discovery and exploitation of “opportunities” to create future goods and service.
- They do not include purposefulness in their domain delineation. They thereby avoid an overly rationalistic view and make room for the possibility of luck and serendipity in entrepreneurial processes.
- Finally, if we disregard for the moment their definition of “opportunity,” their wording “...with what effects” makes the field open to different types of direct and indirect outcomes of processes of discovery and exploitation, e.g., satisfaction, learning, imitation, and retaliation in addition to financial success or failure. Importantly, the perspective suggests that an important task for entrepreneurship research is to assess outcomes not only on the microlevel but on other levels (e.g., societal wealth creation) as well (cf. Shane & Venkataraman, 2001).

That's not bad! As may be inferred from the above points, it is fair to say that it is largely in line with the entrepreneurship definition we discussed in Chap. 1. One of the few debatable points is affording general primacy to the microlevel by putting the main emphasis on the individual and the “opportunity.” This does not seem to give much room for entrepreneurship research on more aggregate levels of analysis (cf. Zahra & Dess, 2001). As Shane (2012) explains, this was—in contrast to emphasis on cultural, political, economic, and industry factors at the time—to draw attention to the fact that entrepreneurship requires agency (and despite the title of Shane's, 2003 book, one couldn't or shouldn't expect one framework to be the ideal tool for all approaches to researching entrepreneurial phenomena).

¹ That's what I said in 2003. Although trait explanations will never be my favorite, I should clarify that subsequent meta-analyses (e.g., Collins, Hanges, & Locke, 2004; Rauch & Frese, 2007; Zhao & Seibert, 2006) and—ironically—Shane's own, recent work on the genetic factor in entrepreneurship (Nicolau et al., 2011; Nicolaou et al., 2008) have to some degree reinstated stable person characteristics as explanations of entrepreneurial behavior and success.

A more important question mark is their definition of “opportunity” and, in fact, *any* use of that construct for research purposes. We’ll get back to that point in Chap. 8. Right now, we have reason to consider Bill Gartner’s many musings on entrepreneurship (e.g., Gartner, 1988, 1990, 1993, 2001; Gartner, Carter, & Reynolds, 2010; Gartner, Davidsson, & Zahra, 2006), which can also be regarded as attempts to delineate the field of research rather than defining or describing the phenomenon. Gartner’s view—which he is careful to present as a suggestion for redirection 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 appears to have had 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 preoccupation 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 focus on early-stage behavior has later been echoed—directly or indirectly and independently or as deliberate elaboration—by several other important contributors to conceptualizations of entrepreneurship (e.g., Alvarez, Barney, & Anderson, 2013; Baker & Nelson, 2005; Dimov, 2011; Foss & Klein, 2012; McMullen & Dimov, 2013; McMullen & Shepherd, 2006; Venkataraman, Sarasvathy, Dew, & Forster, 2012). So there would seem to be reason to consider what these people are saying.

In my opinion, Gartner’s 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 stepmotherly fashion. This clear focus gives promise of providing unique contributions and avoiding overextension of 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 particular instances of new economic activity (Shane & Venkataraman, 2000; Wiklund & Shepherd, 2008). 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 outline 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 (1988) and Shane and Venkataraman (2000), as well as their predecessors and some other, later contributors. 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.

2.3 My Suggested Domain Delineation

First, I take from Gartner (1988) the idea that entrepreneurship research should study behavior in the process of emergence. That introduces three very important components: *behavior*, *process*, and *emergence*. From Shane and Venkataraman (2000), I take the distinction between two subprocesses: *discovery* and *exploitation*. (If there were no prehistory, I would probably have chosen the labels *identification* and *implementation* instead. I include “evaluation” in the discovery process.) Further, in line with the view of entrepreneurship that we developed in Chap. 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*. Thus, when I speak of “new economic activities,” I mean market-related activities and not internal reorganization, and when I say “new ventures,” I do not restrict that notion to a particular mode of exploitation. From Venkataraman (1997), Shane and Venkataraman (2001), and Zahra and Dess (2001), I also adopt the idea that entrepreneurship research should study a variety of *outcomes* on different levels (see Chap. 7). The final element I take from Shane and Venkataraman (2000) is the fundamental assumption of disequilibrium: the economy *always* allows for *some* new initiatives to be successful. Further, and in part related to disequilibrium, I adopt the notion of *heterogeneity* of economic actors, which is prominent, e.g., in resource-based theory (Kraaijenbrink, Spender, & Groen, 2010).

To this I only need to add two little pieces. The first is to adopt the additional fundamental assumption that the economy is also characterized by *uncertainty* (McMullen & Shepherd, 2006). 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. Oh, there is one more not so little thing, which has to do with uncertainty and failure among other things: I avoid that o-word, which has been so prominent in entrepreneurship research during the past decade (Hansen, Shrader, & Monllor, 2011; Karlsson, 2009; Short, Ketchen, Shook, & Ireland, 2010). Patience for now; we’ll deal with it in Chap. 8.

Piecing it all together, I arrive at the following:

Starting from assumptions of uncertainty, heterogeneity, and disequilibrium, the domain of entrepreneurship research encompasses the study of processes of (real or induced, and completed as well as terminated) emergence of new economic ventures, across organizational contexts. This entails the study of new venture ideas and their contextual fit; of actors and their behaviors in the interrelated processes of discovery and exploitation of such ideas, and of how the characteristics of ideas, actors and behaviors link to 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, reread, and perhaps start counterarguing 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.

2.3.1 Uncertainty, Heterogeneity, and Disequilibrium

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 theories and research designs that do not build on such assumptions are unlikely to be useful tools for understanding the entrepreneurship phenomenon. Shane and Venkataraman (2000) have already made the disequilibrium argument quite well. As regards heterogeneity, assumptions that economic aggregates (such as an *industry* or *demand*) are made up of the sum of identical microlevel entities can hardly explain entrepreneurial action or success in meaningful, realistic ways.

Neither do I think it is illuminating for the understanding of entrepreneurship 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. It is also worth pointing out that we must allow theorists to build whatever fantasy worlds they like and then test the extent to which their theories have anything useful to say about the real world. This said, my belief is that to understand entrepreneurship, we need theories that admit that information collection and processing, careful planning, and calculation cannot give a conclusive and reliable answer as to whether an entrepreneurial initiative will be successful or not. Only (trial) implementation will tell. In short, entrepreneurial situations usually come with a substantial element of genuine, Knightian uncertainty (Knight, 1921). The future is not only unknown, but also unknowable (Foss & Klein, 2012; Sarasvathy, Dew, Velamuri, & Venkataraman, 2003).

On this point I disagree with the same Kirzner (1973)² that I leaned so heavily on in the first chapter. Very rarely are entrepreneurial situations certain in the way Kirzner portrays them. In one famous 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 in real life 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 1 to 100 dollars. Moreover, while you dash down the stairs, the wind might take it, or someone else might get it before you, or it turns out upon closer look that it was token money from some game or promotion rather than a real banknote. There is no way the finder can tell before she makes the decision to run down the stairs. In order to understand behaviors in such situations, it is important to start from a theoretical perspective that acknowledges or even emphasizes uncertainty.

²Please don’t counterargue that I misinterpret Kirzner on the basis that in later works Israel Kirzner shows a greater understanding or appreciation of the dynamic and uncertain elements of the economy (Kirzner, 2009; Pollack, Vanepps, & Hayes, 2012). “Kirzner (1973)” is a theoretical argument, not a flesh-and-blood individual, and for all its merits, that argument is relatively insensitive to issues of time and uncertainty.

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

One of Gartner's (1988, 1993, 2001) great strengths is that he identified an important phenomenon—the process of emergence—on which other fields of research haven't done a very good job. Therefore, entrepreneurship research can make a real contribution if it takes on this challenge.

I agree with Shane and 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 presumably follows automatically or is "something else" altogether (Foss & Klein, 2012, p. 34). 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. This is still an area that needs much more investigation over and above the tentative steps that have been taken so far (e.g., Baker & Nelson, 2005; Bhawe, 1994; Lichtenstein, Carter, Dooley, & Gartner, 2007; Sarasvathy, 2008).

The term *discovery* may be suspected to reflect an objectivist view on the entities that entrepreneurs act on when trying to get a new economic activity going. This is not a perspective I purport. Rather, like Shane and 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 the identification of a rudimentary, initial idea to a fully developed business concept where many specific aspects of the operation are worked out in great detail, including how value is created for the customer and how the business will appropriate some of the value (Amit & Zott, 2001; G. George & Bock, 2011; Zott, Amit, & Massa, 2011). Importantly, discovery is a *process*—the venture idea is not formed as a complete and unchangeable entity at a sudden flash of divine insight (Ardichvili, Cardozo, & Ray, 2003; Dimov, 2007).

Exploitation is a negatively loaded word in some contexts and may therefore evoke negative associations. In the present context, it is a neutral term referring to action toward the realization of new economic activities. The exploitation process deals primarily with resource acquisition and coordination, as well as market

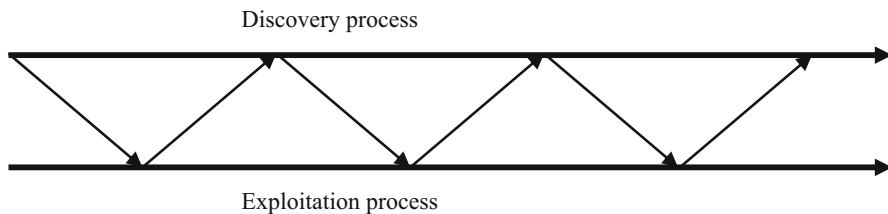


Fig. 2.1 The interrelationship between discovery and exploitation

making (see Brush, Greene, & Hart, 2001; Santos & Eisenhardt, 2009; Sarasvathy & Dew, 2005; Shane & Eckhardt, 2003). This includes all research questions pertaining to the organizing of new ventures, that is, the research agenda that Gartner (1988, 2001) emphasizes. Exploitation thus 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, 2008). I think discovery and exploitation are best conceived of as overlapping processes. This is what Fig. 2.1 tries to portray.

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, Gartner, & Reynolds, 1996; Gordon, 2012; Liao, Welsch, & Tan, 2005), and Bhave's (1994) study indicates that the insight (or discovery) that a problem solution one has developed for one's own needs may become a new venture idea rather late in a process that initially did not have the creation of a new venture as a goal.

Before closing this section, I should mention that "discovery" and "exploitation" do not represent the only way to conceptualize, subdivide, and label entrepreneurial processes. In my current research³, I instead discuss aspects of "discovery" as *identification of new venture ideas* and *perception of opportunity* (the latter referring only to the individual's evaluation of a situation or an idea). Similarly, most of "exploitation" I reassign as aspects of entrepreneurial *action*, subdivided into *initiation*, *mode of exploitation*, and *process pattern*. For other conceptualizations of sub-processes, see, e.g., Bhave (1994); Delmar and Shane (2004); Gatewood, Shaver, and Gartner (1995); Katz and Gartner (1988); and Reynolds (2007).

³At the time of writing under review for *Journal of Prestigious Conceptual Work*, but at the time of reading possibly appearing in *Journal of Entrepreneurship & Bicycle Repair* (credit to Norris Krueger for this wonderful, generic title for journals-no-one-reads-and-which-you-don't-even-want-to-be-seen-in).

2.3.3 Real or Induced and Completed as well as Terminated

These are issues that we dealt with in the beginning of this 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. As a case in point, one of the most interesting and influential developments in recent years, namely, Sarasvathy's theorizing about effectuation, emanates from research on induced (or hypothetical) entrepreneurial processes (Sarasvathy, 2008)⁴.

Further, if we were to study successfully completed cases only, there is no telling whether terminated cases shared the same characteristics as the successful ones. 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 belly flop. If our research design censors the terminated cases, we will systematically misinterpret the effects of risky strategies and actions (Yang & Aldrich, 2012).

2.3.4 Across Organizational Contexts

This has been thoroughly dealt with already. In Chap. 1, we parted with the “independent business” perspective on entrepreneurship. Shane and Venkataraman (2000) make a major point of this issue, emphasizing different modes of exploitation (such as internal venturing; licensing; the setting up of a new, independent firm) as a core set of research questions for entrepreneurship research. Recently, disappointment has been expressed that while entrepreneurship researchers seem to accept the importance of different modes on a conceptual level, the vast majority of empirical studies focus on independent start-ups (Foss & Klein, 2012; Shane, 2012). The emphasis on different organizational modes is in apparent conflict with Gartner's perspective. It is important to note, however, that Gartner's “creation of new organizations” should not necessarily be read as “creation of new, owner-managed firms.” Gartner (1988, p. 28; cf. Gartner et al., 2010) 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.

⁴The underlying empirics were not presented in Sarasvathy (2001), presumably because in the absurd world of academic publishing, basing one's argument on armchair reasoning is sometimes more accepted than is basing it on careful and innovative empirical work that has visible warts.

“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 otherwise. This is also where we can start inviting back to the party those organizational changes in quadrant II of Fig. 1.2, which in the previous chapter were defined as not being instances of entrepreneurship. *Change* in the 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, and Wright (2001) (p. 64) showing that management buyouts are followed by increased development of new products are therefore examples of entrepreneurship research.

This example presumes 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 highlights the need for studies that use the emerging venture itself as the unit of analysis (Davidsson & Wiklund, 2001). Such studies would follow samples neither of individuals nor of organizations, but precisely of *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 firm; in other cases, what originated within a firm may be spun out at an early stage. This is something we will also return to in later chapters.

2.3.5 New Economic Ventures; New Venture Ideas and Their Contextual Fit

“New economic ventures” include 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” (yet). However, in line with our placing entrepreneurship in a market context in the previous chapter, the suggested domain delineation is restricted to new *economic* ventures.

The reader may have noted that I have so far avoided the o-word as best I could, putting “opportunities” within quotation marks and/or only using it when citing others or occasionally for some individual’s unproven, subjective *perception* of conditions as being potentially lucrative or otherwise beneficial. Instead, I have started to sneak in the concept *new venture idea* in its stead. This is very, very intentional, of course. In a draft version of this revised chapter, I decided this was where to slay the dragon (good luck with that, you say!), so I went on with a rant against the ills of “opportunity” over several pages. But I then decided to save you (and myself) from that grumpy-old-man detour and instead introduce a new chapter on “The Entrepreneurship Nexus” as Chap. 8.

As a sneak peek, instead of the elusive, contested, and otherwise problematic notion of “opportunity,” I suggest we place the *new venture idea* (NVI) as the actor’s main companion in the process. I conceive of the new venture idea as an “imagined new venture,” defined as *a set of imagined combinations of product/service offerings, markets, and means of bringing these offerings into existence* (cf. Davidsson, 2015). As ideas evolve and (initially) reside only in actors’ minds, this is certainly not an easy construct to work with. However, it is much more straightforward and less contentious than “entrepreneurial opportunity.” A new venture idea is more than a mere dream but less than a manifest business model; it is a cognitive construct. The idea is not limited to increasing the efficiency and profitability of existing operations; it concerns introduction of new activity, although this new activity need not be innovative. New venture ideas may be easy or impossible to convert into operational ventures, and their (attempted) implementation may have good or bad consequences for the actors and for the economic system.

An NVI thus may start as a very rudimentary hunch about 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 call a *business concept* or a fully developed (conception of a) *business model*. The NVI is, so to speak, the focal object of the discovery and exploitation processes.

Referring back to Fig. 1.2, NVIs 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-researched area, I would argue, concerns the characteristics of new venture ideas and how these characteristics relate to antecedents, behaviors, and outcomes. And I am as big a sinner as any; I can only point to a couple of published, empirical attempts at investigating the nature and effects of characteristics of NVIs (Davidsson, Hunter, & Klofsten, 2006; Samuelsson & Davidsson, 2009). New venture ideas have generalizable characteristics that may have generalizable effects. We need to conceptualize and operationalize such characteristics (novelty, scope, scalability; there should be many possibilities). By contrast, an abundance of studies have tried to assess the characteristics of entrepreneurs. Interestingly, this disproportionate interest in the individual is shared by diffusion research, where only about 1 % of the close to 4000 studies (now many more) have focused on the characteristics of the innovation (which may be possible to apply as NVI characteristics as well), 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 (Riggio & Garcia, 2009; Ross, 1977). Researchers beware!

Finally, as regards contextual fit, we have discussed fit between the actor and the new venture idea as the main entrepreneurship nexus. Other fit foci are certainly also possible. One obvious candidate would be the fit between NVIs and the environment. For all its qualities as an entrepreneurship hotbed, Silicon Valley might not have turned out the right environment for launching the Ice Hotel.

2.3.6 Actors (or Agents)

Having replaced one part of Shane and Venkataraman's (2000) "entrepreneurship nexus," we can now turn to the other half. They (p. 218) portray entrepreneurship as the nexus of lucrative you-know-what and enterprising individuals. I find the focus on individuals unnecessarily restrictive and somewhat misleading. For example, in the past decade we have seen a minor explosion of research on entrepreneurial *teams* (e.g., Harper, 2008; Ruef, Aldrich, & Carter, 2003; West, 2007) and the notion of *strategic entrepreneurship* (Hitt, Ireland, Sirmon, & Trahms, 2011; Ireland, Hitt, & Sirmon, 2003) where the firm or its management rather than single individuals is the most natural agent to focus on. The notion of "corporate entrepreneurship" was already well established in the year 2000 and has not died out since (Shepherd, Covin, & Kuratko, 2009).

Further, we have emphasized that different individuals and organizations can perform important roles at different stages of venture development and that it is important to study emerging ventures through changes of human champions and organizational affiliations. All in all, there are good reasons to generalize Shane and Venkataraman's individual to *actor (or agent)*, denoting one or more individuals or firms in conjunction or in sequence, making room for team-based, corporate, and strategic forms of entrepreneurship. Notably, Shane and Venkataraman (2000) acknowledge this in the context of discussing modes of exploitation. Thus, the idea that the entrepreneurial function is performed not only by single individuals or in one particular organizational context has gained broad acceptance.

2.3.7 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. The emphasis on different levels of analysis makes our framework more inclusive. However, in order to qualify as entrepreneurship research on any given level, it needs to be *explicitly related to discovery and exploitation of new venture ideas*. Thus, we can re-invite the organizational issues in quadrant II of Fig. 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 potential and challenges involved in researching entrepreneurship on those *different* levels of analysis will be the central theme in chapters to come.

On the outcome side, this means that entrepreneurship research is very, very far from restricted to the question of the financial performance of new ventures or of firms. In Chap. 7, I will discuss the dependent variable in entrepreneurship research more thoroughly and argue that the relative financial performance of new ventures is not even one of the core outcomes. Entrepreneurship is about emergence; what comes after may well be of interest to entrepreneurship researchers but definitely

not unique entrepreneurship terrain. At this stage of reading, migrants and visitors from strategic management should start to understand why the notion of entrepreneurship as a subfield of strategy is, should we say, a trifle incomplete. North American business schools may organize their departments any way they like; I don't think economists, sociologists, or psychologists around the world, who are interested in entrepreneurial phenomena, will care too much about that (although Baker & Pollock's, 2007, argument about hiring has some force). As we have delineated entrepreneurship research here, the core strategic management questions that are also entrepreneurship questions constitute but a corner of the totality of the entrepreneurship domain. It should be clear by now that many disciplines and subdisciplines cover different aspects of the research domain we have delineated—or *could* cover them as a natural part of their work. Hence, I do not believe in entrepreneurship as a distinct domain (Davidsson, 2003; Venkataraman, 1997) in an isolationist sense. It should be equally clear that no *one* other existing discipline or subdiscipline covers the entirety of what we here see as entrepreneurship research (Ács & Audretsch, 2010).

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; Sorenson & Stuart, 2008; 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 microlevel is a loss—and that therefore the rational decision is to refrain from entrepreneurship (cf. Olson in Sarasvathy, 2000, 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, and this is to me an obvious question for strategic entrepreneurship: 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 Fig. 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 (Levie, Don, & Leleux, 2011; Wennberg, Wiklund, DeTienne, & Cardon, 2010). 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. One outcome we have already seen in deeper and more refined research into apparent “failure” is that pure failure as defined in Fig. 1.3 is far less common than previously thought. 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 (Eliasson, 1991; Pe’er & Vertinsky, 2008; 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. Outcome assessment may also concern, e.g., satisfaction, learning, imitation, and retaliation. For researchers who have the creativity and guts to be unconventional, there are plenty of **opportunities**, sorry, I should say “new research ideas,” that await your discovery and exploitation.

2.4 Summary and Conclusion

In this chapter, I have argued that even though the objective 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 as defined in Chap. 1. Instead, I suggested the following domain delineation for entrepreneurship research, making a particular point of giving a central role to new venture ideas rather than to “opportunities”:

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

Building on a combination and extension of earlier contributions by Gartner and Shane and Venkataraman, the domain I suggest for entrepreneurship research is broader than either of these predecessors. This combination and extension allows the following, broadening reformulation of Shane and Venkataraman’s (2000, 2001) core research questions. The focus on new venture ideas should not be interpreted as denying the existence of enabling and restricting external conditions or denying their importance. In Chap. 8, I will discuss them further under the label “external enablers.”

- Why, when, where, how, and for whom do new venture ideas come into existence?
- Why, when, and how do individuals, organizations, regions, industries, cultures, and nations (or other units of analysis) differ in their propensity for identification, evaluation, development, and exploitation of new venture ideas?
- Why, when, and how are different modes of action used to identify, develop, and exploit new venture ideas?
- What are the outcomes on different levels (e.g., individual, organization, industry, society) of efforts to exploit new venture ideas?

My more inclusive attitude may lead to a less distinctive domain. If one looks closely at the above domain delineation, it is clear that most core research questions in entrepreneurship would fit in *some* existing discipline or subdiscipline. I see no problem in that; the more scholars from various disciplines invest in understanding entrepreneurship, the happier I am! Entrepreneurship as a distinctive domain, to me, is not about being exclusive but about trying to make a well-defined contribution to something bigger. I think it is also clear that entrepreneurship is not in its entirety a subdivision of any *one* established discipline or field of research. If left solely to within-discipline work (Sorenson & Stuart, 2008), 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. Ács & Audretsch, 2003). *A failure to collectively cover the entrepreneurship research agenda is neither a problem nor a shortcoming on the part of the existing disciplines.* Well, at least it is not a problem for any individual scholar within them. When maximizing knowledge development about the entrepreneurship phenomenon is the vantage point (Wiklund, Davidsson, Audretsch, & Karlsson, 2011; Zahra & Wright, 2011), 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. Therefore, I think we need to be a multidisciplinary community of scholars who dedicate ourselves to this phenomenon and who interact enough in order to speak roughly the same language. In line with this notion, our leading journal, the *Journal of Business Venturing*, now explicitly defines itself as a multidisciplinary journal (www.journals.elsevier.com/journal-of-business-venturing) and is organized accordingly.

Now, after this long warming up, it's about time we get to the real stuff: method-related challenges of entrepreneurship research. So that's what we'll turn to next and 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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