According to Joseph Schumpeter, entrepreneurs introduce new combinations that create market disequilibria. However, entrepreneurs also create their businesses by acting on already existing opportunity, thereby moving markets towards equilibrium. In any case, entrepreneurs change the economy by creating new economic activity. The overarching theme of this thesis is the conceptualization, operationalization and application of new economic activity as level of analysis in entrepreneurship research. Using a unique representative real-time longitudinal dataset of genuinely new firms (7000+ cases), a number of central issues in entrepreneurship are investigated. Performance and survival of new independent ventures, the capacity of young firms for continued entrepreneurship, the search for opportunity and the market newness of new ventures are some of the topics investigated.

The thesis consists of two parts. Part one contains an introduction to common theory and methods as well as supplementing commentaries on the included papers. In addition, it also includes the overall conclusions on research methodology and entrepreneurship. Part two consists of six full papers on entrepreneurship that collectively assess the process and performance of the entrepreneurial event – the creation of new economic activity.
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Jonas Dahlqvist

Assessing New Economic Activity
Process and Performance in New Ventures
To my family
This thesis is concerned with the assessment of entrepreneurship construed as the emergence of new economic activity. Framing the entrepreneurial event this way brings venture newness and human action over time to the fore and makes the divide between independent and corporate entrepreneurship appear superficial; efforts to create new economic activity for the purpose of satisfying human wants drive the market in the same way whether carried out in a new or in an existing firm. This poses two major methodological challenges for empirical work. First, the concept of new economic activity needs to be operationalized for both independent and corporate ventures. Second, the actual samples used for empirical work need to be aligned with the theoretical assumptions.

Using a large proportional sample of genuinely new independent firms in Sweden (7000+ cases), the thesis demonstrates that it is possible to produce samples with entities in which the economic activities are genuinely new and that this can be carried out both for independent and corporate ventures. It is concluded that this either requires access to comprehensive existing data on new firm formation or the use of a strategy in which the entrepreneurial agent, whether firm or individual, is screened for their current involvement in creating new economic activity.

The empirical investigation spans over six papers on various aspects of the entrepreneurial process. These are mainly concerned with the prediction new venture performance, the ability of young firms to continue to create new economic activity, the assessment of the market newness of new ventures, and the search for opportunity in existing firms. The findings indicate that the survival of new independent ventures is difficult to predict when information on outside options is not available. More promising results were obtained when differentiating low from high performance, the most prominent explanatory factor being initial resource endowment. Further, continued entrepreneurship among young firms was shown to be relatively uncommon but that predictive models traditionally used for independent entrepreneurship obtained reasonable results also for young existing firms. The search for new opportunities in existing firms was shown to fall into three categories: proactive search, reactive search, and fortuitous discovery. In addition, opportunity search was linked to the level of market newness of the new ventures as well as the gestation process; ventures started by active searchers generally showed a higher degree of market newness, while those ventures founded on fortuitous discovery showed lower average market newness while also being more dispersed.
The reason I pulled this beast of a project off is that I got more than a little help from my friends. I would like to thank Frédéric Delmar who was the discussant on my final seminar and whose comments helped me improve an earlier draft of this thesis into the finished text. Thanks also to Leona Achtenhagen and Karin Hellerstedt who reviewed my original research proposal. Kudos to Mikael Samuelsson for plenty of things. However, writing a thesis is not only about academics, it is also a question of keeping one’s mental health. My sixth-floor colleagues were instrumental towards this end. In particular, I would like to extend my gratitude to past and present residents of the East Wing at JIBS who have put up with my inclination to drop by their offices and talk about anything that was on my mind. In addition to friends, surviving as a doctoral student also takes concrete acts of financial philanthropy. The Knut and Alice Wallenberg Foundation provided generous support in the early stages of my research. Further, an equally munificent grant from the Jan Wallander and Tom Hedelius Foundation enabled me to complete my studies and to collect additional data. NUTEK kindly provided access to the original dataset. In terms of direct contributors to my writing this thesis, there are six individuals in particular that should be cordially acknowledged.

Kristina Pettersson produced the spectacularly high quality dataset that formed the backbone of my research and which became affectionately known as the “Cohort ’94” among my JIBS associates. She has been a constant during my entire professional life and she has diligently watched over the collection of data on new independent ventures in Sweden for about two decades. Her extensive knowledge about the ins and outs of business survey methodology benefited me greatly. With her exceptional dedication, consistency, and sense of decorum, she is indeed an honor to her office.

Henrik Hall collaborated with me on the sample of new internal ventures that came as an extension of the original Cohort ’94 dataset. He also managed and set up our datasets at JIBS. Being a longitudinal study, our direct collaboration lasted more than three years. His extensive hands-on experience with survey methodology and his ability to manage complex datasets made my life so much easier. But more importantly, it was Henrik’s excellent understanding of entrepreneurship as a field of research that made the quality of my data much better than I had any right to expect.

Johan Wiklund turned out to be something of the older brother I never had. While he brotherly bullied me as he saw fit (and still does I suppose), he also showed me genuine respect as well as great concern for my well-being and intellectual development. Johan was the one who finally made me pull this one
off, and he literally saved me from myself. When he took over after Per Davidson as my chair, his unyielding attitude towards my completing my thesis was clearly a doctor’s order. Isaac Newton suggested that standing on the shoulders of giants is a Good Idea when trying to make progress in research. Well, anyone who has met Johan knows that you cannot do much better than him if your purpose is to look farther.

I first came across Per Davidson in my former position as government official. He was my first love in academia and the sole reason I moved to Jönköping to enroll in the doctoral program at JIBS. Truly a researcher’s researcher, his extraordinary mind and race-horse disposition has inspired research as well as awe. His way of treating his doctoral students as peers from day one inspired confidence and pride. In addition, the quality of his feedback on manuscripts turned out to be second to none, with startlingly short delays and invariably incisive comments. As my supervisor, he early on invited me to co-write papers, and so eased my way into academic writing without me really noticing it. As a colleague, his ability to “soar” in intellectual discussions was inspiring. I believe I learnt more from him than he will ever understand.

I met Gaylen Chandler late in 1998 when he visited JIBS for a meeting on a newly formed program on entrepreneurship and growth (the fabled PEG project). Later, Gaylen would spend his sabbatical at JIBS which was the starting-point of a collaboration that spawned several papers of which two are included in this thesis. In 2002, Gaylen cordially invited me to Utah State where I spent five very productive months with my family. During that period he provided much needed supervision when I was trying to order my thoughts on entrepreneurship (or "musings" as Jeff Covin aptly called them); I finished my research proposal in the high air of Logan (UT), thanks to Gaylen. However, his greatest gift was the personal development that took place there in the strange and beautiful scenery of northern Utah during that fall of 2002. Gaylen and his family met us with warmth and care and shared their way of life with us. The trip to Utah made everlasting impressions and left us wiser altogether.

In the years that has passed since my enrolment in the JIBS doctoral program, Åsa Dahlqvist has not only been the main family provider, but she also managed to marry me (including making the wedding arrangements), and to have our two kids Asta and Åke. For extended periods she has been close to a single parent due to my penchant for working late nights, weekends and during holidays. Yet, she never failed to provide all the love, care and attention her family needed. However, her most mysterious feat is that she apparently still loves me, even though the process has been considerably more trying than any of us bargained for. Åsa has been my most confident supporter throughout this project, although I suspect that there have been times when even she had her doubts, particularly after I missed a deadline for the umpteenth time. Men utan dig hade det aldrig gått. Vi har haft en sagolik tur, Asta, Åke och jag. Och jag älskar dig med.

Jonas Dahlqvist
CONTENT

1 Introduction .......................................................................................................................... 11
  1.1 Six variations on a common theme ............................................................................. 11
  1.2 Storyboard for six papers assessing entrepreneurship ........................................... 11
  1.3 Caveats and kudos ..................................................................................................... 14
  1.4 Further notes of direction ......................................................................................... 16

2 Defining the area of inquiry .............................................................................................. 17
  2.1 Introduction ................................................................................................................ 17
  2.2 Some contemporary conceptualizations of entrepreneurship .............................. 17
  2.3 Entrepreneurship and economic development ......................................................... 20
  2.4 The nature of opportunity ......................................................................................... 22
  2.5 Independent and corporate entrepreneurship ......................................................... 25
  2.6 Units of analysis in entrepreneurship research ....................................................... 27
  2.7 Summary .................................................................................................................... 31
  2.8 Purpose .................................................................................................................... 32

3 Methods ............................................................................................................................. 33
  3.1 Introductory note ....................................................................................................... 33
  3.2 Capturing new economic activity .............................................................................. 33
  3.3 Choice of design in general ...................................................................................... 35
  3.4 A quantitative method is characterized by quantification .................................... 36
  3.5 A probabilistic approach to social science ............................................................. 37
  3.6 Causality and process calls for longitudinal research designs ................................ 40
  3.7 Data collection and sample ...................................................................................... 41
    3.7.1 A panel of new independent ventures .......................................................... 42
    3.7.2 A panel of new internal ventures ................................................................. 44
  3.8 The feasibility of assessing entrepreneurship as new economic activity ............... 50
    3.8.1 Introduction ....................................................................................................... 50
    3.8.2 Methodological comments on Initial conditions as predictors of new venture performance: a replication and extension of the Cooper et al. study .......................................................... 52
    3.8.3 Methodological comments on Business start-up reasons and firm performance .......................................................... 56
    3.8.4 Methodological comments on Entrepreneurship as new business activity: Empirical evidence from young firms .......................................................... 60
    3.8.5 Methodological comments on Opportunity recognition processes: A taxonomy and outcome implications .......................................................... 62
    3.8.6 Methodological comments on Patterns of search and the newness of venture ideas .......................................................... 63
    3.8.7 Concluding remarks on the methodological approach .................................. 64
4 Conclusions on Assessing Entrepreneurship .................................................. 67
   4.1 Introduction .................................................................................................. 67
   4.2 Considerations in sampling ........................................................................ 68
   4.3 Assessing performance ............................................................................ 69
5 Conclusions ........................................................................................................ 75
   5.1 Summing up ................................................................................................ 75
   5.2 Start-ups and performance ........................................................................ 76
   5.3 Continued entrepreneurship in young firms .............................................. 78
   5.4 Opportunity and enterprising ................................................................... 79
6 Future research ................................................................................................ 83
   6.1 Taking stock .............................................................................................. 83
   6.2 Future challenges for methodology in entrepreneurship research .......... 83
   6.3 Future challenges in entrepreneurship research ....................................... 90
References ............................................................................................................. 93

Tables
Table 3.1 Classification scheme used to determine venture newness .................. 47
Table 3.2 A tabulated summary of the independent variables, independent variables, methods of analysis and results for the included papers ............... 51

Figures
Figure 2.1 A model of productive opportunities as the intersection of perceived possibilities and productive possibilities ........................................... 23
Figure 2.2 A typology of new ventures. ............................................................... 30
Figure 3.1 A schematic of the methodological issues at hand in operationalizing new economic activity ................................................................. 35

Appended papers
Initial conditions as predictors of new venture performance: A replication and extension of the Cooper et al. study
(Jonas Dahlqvist, Per Davidsson, & Johan Wiklund) ................................................. 103
Business start-up reasons and firm performance
(Jonas Dahlqvist and Per Davidsson) ...................................................................... 125
Entrepreneurship as new business activity: Empirical evidence from young firms
(Johan Wiklund, Jonas Dahlqvist and Per-Anders Havnes) ....................................... 137
Opportunity recognition processes: A taxonomy and outcome implications
(Gaylen N. Chandler, Jonas Dahlqvist, and Per Davidsson) ..................................... 153
Patterns of search and the newness of venture ideas
(Jonas Dahlqvist, Gaylen N. Chandler, and Per Davidsson) .................................... 167
Measuring the market newness of new ventures (Jonas Dahlqvist) ....................... 185
INTRODUCTION

1.1 Six variations on a common theme

This dissertation combines six papers written over a period of six years. Yet it claims the status of one thesis with unity of purpose. The most succinct version of this common purpose is to *assess entrepreneurship as the emergence of new economic activity*, but the commonality of the papers can be further divided into two overarching themes. The first theme is the scholarly field of *Entrepreneurship* to which these six papers claim to make their contributions. Although efforts have been made to avoid the re-invention of any entrepreneurial wheel, we will need to dwell on the topic somewhat since it is the basis for our choice of method. At this point it would suffice to say that entrepreneurship as a scholarly domain should "explain and facilitate the role of new enterprise in furthering economic progress" (Low & MacMillan, 1988, p. 141) is a fair representation of the perspective in this introduction as well as the six papers.

Not unexpectedly, the second theme is that of method, which also may be divided into two basic issues: *sampling* and *prediction*. All six papers use the same large, well-developed, and real-time longitudinal dataset for purposes of both exploration and theory testing. It was constructed to match the chosen definition of the entrepreneurial event as the introduction of new economic activity by using *sampling procedures aimed at capturing the appropriate level of analysis*. Further, data were collected at multiple points in time to strengthen arguments of causality beyond the necessary (but not sufficient) condition of correlation, and to meet basic requirement when trying to predict outcomes from in a process.

In addition to these major themes or axes, there are some special issues that are paper-specific rather than general. These issues will be dealt with in conjunction with the commentaries on each paper.

1.2 Storyboard for six papers assessing entrepreneurship

The development of the papers was influenced by two time-variant factors. The first factor is the development of the dataset. Data were collected from the same cohort of firms over a period of nine years, a considerable time for a panel of
new firms. As new data were added, new questions could be answered and this has had a definite bearing on how the papers were developed. The second factor is my own development regarding the matter at issue, entrepreneurship, as well as purported methodological proficiency. The chronological order of the papers is therefore slightly different from what might be expected from an ideal logical order in which there would be no restraints. I have however chosen to retain the original order in which the papers were written for the coherence of thought it offers; they were simply developed this way.

The first paper, Initial conditions as predictors of new venture performance: a replication and extension of the Cooper et al. study poses the question of whether it is possible to predict the performance of new ventures from factors known at start-up; these factors are chiefly construed as resources. The empirical setting is a representative large random sample of “greenfield” start-ups in Sweden. In this study, the clearest finding is that initial resource endowment matters enough to merit attention. Human capital was associated with better overall chances of survival as well as higher performance while financial resources only had a statistically significant positive impact on performance among the surviving firms. However, the study also confirms previous efforts in that it is more difficult to develop a model with strong predictive power in relation to survival than for performance among surviving firms. Although it could be argued that additional honing of such models could help reduce the unexplained variance, a major obstacle can be found in the alternatives outside the venture, which are not captured when measuring resources within the venture. Equaling exit with failure overlooks the fact that individuals may choose to exit to take advantage of other opportunities as these present themselves over time. Gimeno, Folta, Cooper, and Woo (1997) provide both theoretical and empirical backing for this interpretation of the findings, which have been corroborated in a recent study of a large representative sample of firms in the US (Bates, 2005).

The second paper is called Business start-up reasons and firm performance and changes perspective on the explanatory variables. While the first paper used a more objective set of variables overall, this paper singles out psychological motives for the start-ups and their relation to subsequent venture performance. Three performance measures are used: employment, sales and perceived profitability. In the full model with control variables, need for independence had a statistically significant positive effect for size and perceived profit, while the unemployment motive was statistically significant for perceived profit only. However, the statistically significant motives had effect sizes that were trivial at best and the findings corroborate several previous efforts in this area in which the predictive value of start-up motives on performance has been low. Indeed, this is in line with what might be expected from theory since the theoretical link
between start-up motive and start-up is considerably more convincing than the link between start-up motive and subsequent venture performance.

In the third paper, *Entrepreneurship as new business activity: Empirical evidence from young firms*, the empirical setting has developed since the original panel of new ventures has now moved from being “new ventures” to being “young firms”; the cohort is six years old at the last data point used. The paper is concerned with prediction of the entrepreneurial event itself, roughly defined as *new economic activity* (Davidsson, 2003). The explanatory models use an amalgamation of resource variables and psychological variables (motivation) to explain new economic activities undertaken by existing firms. The results point out general human capital (as indicated by higher education) and previous start-up experience as factors that enhance the propensity to engage in new economic activity. In addition, being a member of a business network, an indicator of social capital, showed to be an important predictor of new economic activity. Among the psychological factors, willingness to grow, to hire new employees and to take on new owner-partners were associated with a higher likelihood of engaging in new business initiatives. Generally, the findings coincide with those found in studies on new organizations, i.e. independent entrepreneurship. From a theoretical perspective, this is an expected result when construing entrepreneurship as new economic activity: the divide between corporate and independent entrepreneurship appears superficial in many respects and the important commonalities come to the fore.

The fourth paper is called *Opportunity recognition processes: A taxonomy and outcome implications* and looks at opportunity recognition processes and their impact on outcomes such as the speed of the gestation process and financial performance. A taxonomy of three types of opportunity search is empirically derived and differences by search style are assessed using parametric and non-parametric analyses of variance. The empirical setting is young firms that are taking new business initiatives, i.e. are trying to create new economic activity. The type of opportunity search (or lack thereof) was shown to have an influence on subsequent gestation activities and performance. In the early phases, proactive search was associated with a more rapid process on some key gestation behaviors: talking to new customers, acquiring new equipment, creating a separate budget, assigning people and completing a business plan for the new venture. However, this lead was not persistent and was reduced over time as new ventures belonging to reactive search and fortuitous discovery completed their processes. However, the reactive searchers continued to display indications of problemistic search and strong relatedness: persistently fewer separate budgets for their new business initiatives and less acquisition of equipment added to the picture that this group engaged primarily in filling excess resource capacity with a preference for integration of production.
The fifth paper, *Patterns of search and the newness of venture ideas*, uses the taxonomy of opportunity recognition derived in the previous paper and relates it to the market/industry newness of new ventures. Three clusters of firms representing three styles of opportunity search are compared: proactive search, reactive search and fortuitous discovery. The market newness of the resulting new ventures is measured by a formative index consisting of 10 binary variables. The results indicate that explicit search efforts produce ventures with a higher degree of market newness, and proactive more so than reactive. Fortuitous discovery is characterized by a higher variance (and lower average) of market newness among firms adopting that particular opportunity search style.

The sixth and last paper, *Measuring the market newness of new ventures*, is very much a methodological extension of the previous papers and compares two ways of creating indexes for the purpose of measuring the market (or macro-level) newness of new ventures. The fifth paper uses a formative index, which by definition cannot be checked for validity and reliability using standard covariance-based methods such as principal component analysis (PCA) and Cronbach’s alpha. In this last paper, the proposed formative index is validated by comparing its properties to a reflective index using PCA with polychoric correlation coefficients of the ordinal indicators. Simply put, an index derived from theory is compared to an index derived from data. The findings show only minor discrepancies between the two approaches, thus lending validity to the indices by reciprocity. However, the theoretical issue of whether newness is reflected or defined by its measures remains unsolved.

1.3 Caveats and kudos

Five out of the six appended papers were co-authored with colleagues who were associated with Jönköping International Business School (JIBS) at the time of writing. Since credit should be given where credit is due, it is imperative that my own intellectual contribution can be distinguished from those of my co-authors. Since the papers were co-authored, I have decided to leave the texts unedited. Only in cases of obvious mistakes, such as spelling errors or missing words, have these been corrected to improve readability. Additional information regarding, for instance, number of cases has also been added to comply with the standard level of disclosure for publication. However, leaving the papers in their original form has necessitated that a substantial part of my own contribution be placed in this introduction, which spans over chapters one through six. Partly, this contribution is provided by extending the discussions in specific papers, but also by situating them in the context of current methodological and theoretical “conversations” of the field (Huff, 1999).

One common feature of the appended papers is that all data come from a unique dataset that was supervised, arranged and co-developed by me. I also
provided all metadata for the co-authors using this dataset. The dataset has its origin in the production of the Swedish official statistics on new venture formation. In the period 1996-1998, I was (in my then capacity as government official) formally in charge of the production of this data, which was financed by the government agency NUTEK (Swedish National Board for Technical and Industrial Development) and produced by Statistics Sweden. In 1997, I was involved in negotiations with AMS (Swedish National Labor Market Administration) and obtained collaboration and co-funding on a three-year follow-up of new independent businesses that were started in 1994. In order to improve the quality of the questionnaire of this follow-up survey, I consulted with Per Davidsson and Johan Wiklund at Jönköping International Business School (JIBS) and in an effort to improve knowledge generation from this data, I also made them available (without identification data) to JIBS for research purposes. In 2000, I was (now as an employee of JIBS) involved in negotiating the co-sponsorship from NUTEK for the six-year follow-up on this dataset. This also resulted in that a large part of the full dataset was transferred to JIBS with full ownership. From 2000 and onwards, I supervised and coordinated the data collection on a subsample of 250 new internal ventures that was “harvested” from the dataset of then six year old firms. This longitudinal study lasted 30 months and involved six waves.

In paper no. 1, I provided the introduction, hypotheses, description of the sample, development of measures, and participated in the interpretation of the results. The paper was a true group effort and it is hard to find “pure” areas in the text since the co-authors have edited and appended each other’s original contributions. However, statistical analyses and results were produced by Johan Wiklund including the replication of the original procedure for multinomial logistic regression using LIMDEP.

In paper no. 2, statistical analyses were done jointly by Per Davidsson and me. The text was written by me with creative editing and polishing provided by Davidsson.

In paper no. 3, I provided the description of the sample and measures as well as some commenting on the findings.

For paper no. 4, Gaylen Chandler provided the section on search theory with some minor contributions by me. Explorative finding on the clustering were originally provided by Chandler and replicated by me using a different statistical package. I also did post hoc comparisons of the original Ward’s clustering method with other methods of linkage (though not reported). Results on the search – outcome link were produced by me as well as the analysis of missing data. Results and conclusions were a joint effort, though led by Chandler.
Paper no. 5 was developed by me, including the theory section on newness, hypotheses, sample description, measures, results and discussion. Gaylen Chandler provided the theory section for the search taxonomy with some editing and appending done by me.

Paper no. 6 was not co-authored so separating the contribution is not an issue. However, Frédéric Delmar provided important feedback on an earlier draft as did Per Davidsson and Johan Wiklund.

1.4 Further notes of direction

The remaining part of this introduction will deal with theoretical and methodological issues in a decreasing degree of generality. Low and MacMillan (1988) enumerates six design dimensions that need clarification in entrepreneurship research: (1) purpose, (2) theoretical perspective, (3) focus, (4) level of analysis, (5) time frame, and (6) methodology. The thesis will hopefully treat each of these issues in a satisfactory manner. The first section of the thesis (chapters 1 and 2) contains a theoretical discussion of the chosen research topic, both as a social phenomenon and as a scholarly field. It also includes a statement of purpose. The second section (chapter 3) is devoted to methodology and contains a discussion on the consequences of the chosen theoretical view on entrepreneurship in relation to empirical research and the actual precautions that were taken in order to secure methodological coherence between theory and research practice. In particular, the level of analysis and the role of time are assessed, both theoretically and in relation to empirical work. In the third part of the introduction (chapter 4 through 6), we will turn to the subject matter and empirical contributions from the studies. Finally, conclusions and implications for future research will be presented and discussed.

1 Contrary to common practice, co-authored papers will sometimes not be referred to in plural (although I may not be consistent throughout this thesis). This is to mark my emphasis on treating scholarly articles as texts, rather than “recorded” voice or thought. Thus, in the case of multiple authors, the authors’ names are sometimes preceded by an invisible “the article by…”.
2 DEFINING THE AREA OF INQUIRY

2.1 Introduction

Since unification under the entrepreneurship banner seems nowhere near (Gartner, 1990, 2001; Shane &Venkataraman, 2000; Venkataraman, 1997), the reasonable thing to do is to clarify one’s own sense of scholarly belonging (Gartner, 2001). However, this is not to say that a chosen definition of entrepreneurship should be confessional. Rather, it is a question of making our inquiries interpretable and to avoid misunderstandings. The objective of this section is to show how the assessment of new economic activity (Herbert Simon in Sarasvathy, 2000, p. 11) is a suitable and worthwhile task for studies that aim to contribute to the understanding of entrepreneurship.

2.2 Some contemporary conceptualizations of entrepreneurship

Over forty years ago, Edith Penrose noted that entrepreneurship, or “enterprise” as she preferred to call it, is a “slippery concept” (Penrose, 1959, p. 33). However, the slippery character of entrepreneurship comes not from any lack of definitions, but rather from the multitude. The reification of the concept is fragmented and the “e-words” have been applied to a number of related but dissimilar phenomena (Hornaday, 1990). This may partly be explained by the generally positive connotations associated with the words “entrepreneur” and “entrepreneurship” (Gartner, 1990; McMullen & Shepherd, 2006), particularly in North American culture: many want to use the inherent positive value associated with the word “entrepreneurship” to attach legitimacy and importance to their own particular area of interest.

Recently, management scholars could witness the resuscitation of a discussion on the domain of entrepreneurship research. At the time of writing, it has resulted in a handful of conceptual papers and comments by prominent scholars of the field (Bruyat & Julien, 2001; Busenitz et al., 2003; Davidsson, 2003; Gartner, 2001; Low, 2001; Shane & Venkataraman, 2000, 2001; Singh, 2001; Zahra & Dess, 2001). The source of the argument is a perceived need to carve out a scholarly niche for entrepreneurship to facilitate the creation of cumulative knowledge as well as providing legitimacy to the field (Venkataraman, 1997). Gartner (1990) made some efforts in this direction by
mapping prevalent meanings of entrepreneurship using the Delphi method (Linstone & Turoff, 1975). This endeavor was explorative and mostly etymological in character. However, Gartner’s earlier assault on the “traits approach” to entrepreneurship (Carland, Hoy, & Carland, 1988; Gartner, 1988) provided more theoretical backing. The effects of that rather sharp academic discussion was to shift scholarly attention from what entrepreneurs are, to what they are actually doing in terms of manifest behaviors (Low & MacMillan, 1988). In the wake of this debate, articles emerged on the entrepreneurial process, defined in terms of specific actions undertaken by enterprising individuals in emerging firms (Carter, Gartner, & Reynolds, 1996; Katz & Gartner, 1988; Reynolds, 2000; Reynolds & Miller, 1992).

While Gartner (1988) defined entrepreneurship as “the creation of organizations” (p. 11), Shane and Venkataraman (2000) assert that “In contrast to previous research, we define the field of entrepreneurship as the scholarly examination of how, by whom, and with what effects opportunities to create future goods and services are discovered, evaluated and exploited” (Venkataraman, 1997, p. 218). The entrepreneurial process is thus believed to encompass three different functions: discovery, evaluation and exploitation. These functions or phases are suggested to be conceptually distinct, yet they appear to be indelibly intertwined in the real world venture (Carter et al., 1996). Thus, Davidsson (2003) advocates a reduced model with discovery and exploitation as the major components.

A distinct property of the definition provided by Shane and Venkataraman is that it includes the behaviors of enterprising individuals in conjunction with entrepreneurial opportunity. The message is simple, yet it has been largely overlooked in empirical research: before we start evaluating the effect of certain behaviors and strategies in new ventures, we should perhaps think a little about the opportunity thus pursued. Otherwise, we will end up confusing the properties of different types of opportunity with the effects of different modes of exploitation (of opportunity). Shane and Venkataraman point toward an integrated view of entrepreneurship research in which scholars must pay attention to the entrepreneurial process in its totality since the constituent parts cannot be fully understood in isolation.

Herbert Simon casually suggested that entrepreneurship is about “the important phenomena that are involved in the origins of new economic activity …” (cited in Sarasvathy, 2000, p. 11). Although admittedly broad, this deceptively simple phrasing of the central problem of entrepreneurship research seems to include a major part of the work done under the guise of

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2 In this thesis exploitation is used in a positive sense and denotes the process in which the potential value of economic opportunity is realized through the use of productive resources.
DEFINING THE AREA OF INQUIRY: Some contemporary conceptualizations of entrepreneurship

entrepreneurship. So what is the bickering about? The crux of the matter is that the different conceptualizations of entrepreneurship introduced in this section are linked to a set of underlying questions or problems that may be subjected to the irreverent “So what?” of scientific relevancy. These differences in perspective will make scholars of entrepreneurship direct their attention to various aspects of new economic activity.

As for the relevancy of entrepreneurship research in general, one obvious answer is that each day people around the world put in massive amounts of effort to make their new businesses fly. From a social point of view, this fact alone would make the business venture a phenomenon eminently worthy of scholarly scrutiny. Further, empirical research has pointed to the importance of new firm formation for job creation and regional wealth (Davidsson, Lindmark, & Olofsson, 1996; Kirchhoff & Greene, 1998). Thus, it is simply the empirical prevalence of actions and behaviors associated with many definitions of entrepreneurship that makes research on the topic an important endeavor. Interesting contributions in this stream of research can be found in the work of sociologist Paul Reynolds and associates (Carter et al., 1996; Carter, Stearns, Reynolds, & Miller, 1994; Reynolds, 2000; Reynolds, Carter, Gartner, & Greene, 2004; Reynolds & Miller, 1992). These studies are mostly explorative and are mainly concerned with the empirical issues because of their social importance.

Some authors have had other motives to study entrepreneurship beyond that of empirical prevalence, viewing it more as a means or mechanism for achieving something rather than an end in itself. One area where entrepreneurship has been given an important theoretical role is the workings of competitive markets (Kirzner, 1973). Markets do not clear without human agency and in Kirzner’s conceptualization of entrepreneurship it is the identification and closing of profitable gaps in the market that is the task of the entrepreneur. In his model, the entrepreneurs are individuals that take the economy towards greater efficiency by being alert to business opportunities that are there but hitherto unseen. By initiating new economic activity, the entrepreneur can reap entrepreneurial profit, free of risk.

The question posed by Joseph Schumpeter (1983/1934) was how societies achieve economic development (Ger. Entwicklung). Schumpeter postulates that economic development comes about through new means-ends combinations that lead to an expansion of the production capacity of a society. Resources are simply put to better use than they were before through these new combinations and this change is not gradual but disruptive. In this model, the entrepreneur is the economic agent that introduces the new combination for the first time when creating new economic activity. Others will follow suit, but until then the entrepreneur will enjoy so-called “entrepreneurial profits”. Although Schumpeter’s writings almost entirely focus on the particularities of being first
and without precedence in an industry, the process of economic development also depends on the “imitators” that reproduce a new and more efficient means-ends combination in their new ventures to compete away what is left in terms of economic profit.

The choice of which entrepreneur to follow is arbitrary and must be based on the researcher’s personal interest in the “broader issues”. However, this is not to say that one view of what constitutes the fundamental function of entrepreneurship on a social level must be at odds with research based on other perspectives. Personally, I find Schumpeter’s authorship fundamentally more interesting than, say, Kirzner’s, but again this is a personal preference and nothing that can or needs to be “proven”. Nevertheless, it is clear that Schumpeter’s thinking has had a profound impact on some contemporary economics theorists views on entrepreneurship (cf. Baumol, 1968; Casson, 1982). The next section will present Schumpeter’s (1983/1934) view on what entrepreneurship does to the economy. It will also be argued that although his views on the core function of the entrepreneurial event may differ, the scholarly domain is virtually identical to that of Kirzner’s (1973) and include phenomena described in contemporary conceptualizations in management literature (Gartner, 1988; Low & MacMillan, 1988; Shane & Venkataraman, 2000).

2.3 Entrepreneurship and economic development

One of the core assumptions in the neoclassical model of economics is the general equilibrium. In the neoclassical model, all markets simultaneously achieve a common equilibrium point where marginal productivity is equalized across industries. However, the growth of the economy beyond mere accumulation of resources and optimization has been a sore spot for the neoclassical model and considerable efforts have been made to turn the factors affecting growth into endogenous parts of this theoretical model (Romer, 1990). Joseph Schumpeter’s view on this dilemma is laid out in his *Theory of economic development*, in which enterprising individuals cause market disequilibria by introducing innovative “new combinations” to the economy (Schumpeter, 1983/1934). The new competitive situation thus created will drive out incumbents and cause a stream of new ventures to follow the example set by the entrepreneurial event that originally broke the existing equilibrium. Schumpeter specifies five instances of new combinations that cause this market disequilibrium (Schumpeter, 1983/1934, p. 66):
1. The introduction of a new product or service. To this type of new combination, Schumpeter also refers a new quality of a product or service.

2. The introduction of a new production process. The process does not have to be invented. Rather it is new to the industry where it is introduced. New ways of production are also extended to the distribution of goods.

3. The entering or “opening” of a new market. Note that Schumpeter does not write “creation” of a market. Rather, it is the act of finding new customers that is in focus.

4. The appropriation of a new source for raw materials. The obvious example is a mine of some sort, but the concept also includes existing sources of supply that are not known to the industry.

5. The reorganization of an industry. Schumpeter exemplifies this with creating or breaking up monopolies.

These “new combinations” do not just occur in the economy but are created through the actions of enterprising individuals named entrepreneurs (Ger. Unternehmer). Although Schumpeter’s account is liberally sprinkled with vivid descriptions of the heroic tasks that have to be performed, he repeatedly insists that the entrepreneur is a role, an economic function, rather than a social class or a psychological type. The entrepreneur is the economic agent that performs the function of carrying out the new combinations that are essential to economic development. When a certain combination is no longer new, the individual stops being an entrepreneur and reverts to a manager, owner or founder as the case may be.

The new combinations suggested by Schumpeter (1983/1934) are essentially new means-ends combinations. If the ends are changed, existing resource configurations available to the entrepreneur are used to provide products not currently produced by this particular resource configuration. If the means are changed, this corresponds to a novel way to produce an existing product or service. Although product innovation will play a key role, it is only one of five possibilities to bring about new combinations that lead to a more productive use of existing resources. It appears that the recombination of resources is the most prominent function of the entrepreneur in Schumpeter’s model. In modern terminology, one might say that Schumpeter adopts a “resource-based” view (Barney, 1986, 1991; Wernerfelt, 1984) of the entrepreneurial process. This is even more clearly developed by Kirzner (1973; 1997a) who states that to have a specific resource bundle is essentially the same thing as having the end product in terms of value. If the value of any particular resource bundle is not equal to the value of the product it can produce, markets are not in balance. Irrespective of whether the process is internally or externally stimulated (Bhave, 1994), the question is whether resources are put to better use through the endeavors of the entrepreneur. Thus, what will happen on the
market in terms of new market offers is a second order effect of entrepreneurship. This is somewhat contrary to contemporary authors who seem to focus almost exclusively on the product market part of new combinations (Davidsson, 2003; McMullen & Shepherd, 2006; Shane & Venkataraman, 2000; Stevenson & Jarillo, 1990; Venkataraman, 1997). Whether our main interest lies in the improved leverage on existing resources through new combinations conducive to economic development (Schumpeter, 1983/1934) or in the market process proper conducive to market efficiency (Kirzner, 1973), the empirical phenomena under study are rather the same: new economic activities (with varying levels of newness) are initiated by enterprising individuals and the opportunity to do so will be of central importance to the study of these phenomena.

2.4 The nature of opportunity

In a widely cited definition, Stevenson and Jarillo (1990) state that entrepreneurship is the process by which individuals pursue opportunity without regard to resources they currently control. This suggests that the true nature of opportunity lies in the firm’s environment in the form a potential market gap that requires a particular resource combination for exploitation; these resources may or may not be available in the enterprising organization. Apparently, the authors visualize a kind of opportunity that is separated from the resources used in the exploitation process and this is in accordance with other contemporary authors (Davidsson, 2003; McMullen & Shepherd, 2006; Shane & Venkataraman, 2000; Venkataraman, 1997). However, a view of opportunity as something chiefly external to the firm is partly at odds with the clear focus on the supply side in Schumpeter’s theory in which the producer assumes a very active role in creating new economic activity by recombining existing resources.

Shane and Venkataraman (2000) follow Casson (1982) when defining entrepreneurial opportunities as objectively existing phenomena that are “those situations in which new goods, services, raw materials, and organizing methods can be introduced and sold at greater than their cost of production...” (Shane & Venkataraman, 2000, p. 220). An important feature of this definition is the inclusion of alternative costs for the production of goods and services. This implies that opportunity depends on both ends (goods that can be sold) and means (use of resources that make up the cost of production). Since resources are not homogeneous, a given potential market gap will not create opportunity for just any resource combination; the use of the resources withdrawn from other uses has to be improved upon.
DEFINING THE AREA OF INQUIRY: The nature of opportunity

Figure 2.1 A model of productive opportunities as the intersection of perceived possibilities and productive possibilities. From “Markets, firms, and the process of economic development,” by P. Moran and S. Ghoshal, 1999, Academy of Management Review, 24, p. 398.

This is in line with the Schumpeterian problem of economic development (Schumpeter, 1934/1983) but also with Kirzner’s description of the market process (see Kirzner, 1997b for an introduction). An alternative approach to opportunity is suggested by Moran and Ghoshal (1999), and is illustrated in Figure 2.1. Their model is made up of two main categories. The productive possibilities (M) are defined as all possible means-ends combinations that could be of benefit to anyone, and perceived possibilities (P), defined as all means-ends combinations presently perceived by anyone. In the heart of the model lies the productive opportunity (O) (Penrose, 1959, p. 32), interpreted as all productive possibilities that are perceived by agents able and willing to act upon them (Moran & Ghoshal, 1999, p. 398). This implies that only a fraction of the productive possibilities are perceived (P ∩ M), which is represented by the eye-shaped area in the centre of Figure 2.1), and even fewer are acted upon for lack of motivation or ability (the small circle denoting productive opportunities). The entrepreneurs enter this framework as individuals who expand the number of productive possibilities that are actually exploited (O).

Shane and Venkataraman (2000) chose to use the terms discovery, evaluation and exploitation to describe the entrepreneurial process in
combination with a definition of opportunity as an objectively existing phenomenon (Casson, 1982) when they defined the domain of entrepreneurship research. This particular constellation is problematic prima facie since we cannot detect the existence of opportunity until a complete entrepreneurial process (discovery through exploitation) has shown that a subjectively “perceived possibility” is also a “productive opportunity”. Penrose (1959, p. 41) states very clearly that the “objective” productive opportunity is a matter of what can actually be accomplished. She is equally clear on the point that it is “subjective” opportunity and expectations that guide the actions of the venturing firm. A “recognized” or “discovered” opportunity is but a conjecture and it is the role of the entrepreneur to prove that products “can be introduced and sold at greater than their cost of production” (Shane & Venkataraman, 2000, p. 220). The entrepreneur does so by pursuing a “venture idea” (Davidsson, 2003) which would be the subjective theory of productive opportunity that would guide the entrepreneurial process.

Casson’s (1982) conception of the objective opportunity poses epistemological problems when studying the entrepreneurial process in real time, but this can be remedied by clarifying our ontological assumptions about the “unobservable”. The productive possibility of Moran and Ghoshal (1999) could be thought of as existing on an “ontological par” with the concept of opportunity suggested by Casson in the sense that they may be regarded as objective entities that are not directly observable as a complete set. Consequently, they are not accessible to random sampling procedures. Rather, it is in the entrepreneurial process that the entrepreneur explores whether a perceived possibility indeed is also a productive possibility. It therefore seems plausible to aim at efforts to explore perceived possibilities rather than de facto productive opportunities when researching entrepreneurship.

Following Davidsson (2003), it may be helpful to distinguish between the societal phenomenon particular to entrepreneurship research as a scholarly domain and what real-world processes we must study in order to understand it. By relaxing the assumption of successful exploitation that is implicit in the definition provided by Shane and Venkataraman (2000), we open up for real-time studies of not-yet-successful entrepreneurial ventures as well as studies of failed exploitation processes. The framework suggested by Moran and Ghoshal addresses the inherent problems when we try to reconcile Casson’s conception of opportunity with empirical research on the entrepreneurial process. Moreover, their model can be directly linked to the entrepreneurial process as described by Shane and Venkataraman. Therefore, the terminology introduced by Moran and Ghoshal will be retained in this study. The entrepreneurial process is truly a case where the proof of the pudding is in the eating and perhaps the use of the term “discovery” is more appropriate for describing the entrepreneurial process in full rather than its initial phase.
An alternative path to reconcile the term *discovery* with objective opportunity as used by Shane and Venkataraman (2000) is to interpret “discovery” from a logical empiricist point of view as it was formulated by Reichenbach (1947). From this perspective, the context of “discovery” is that of conjecture; the truth of this conjecture or belief will depend on the subsequent process of *justification*. In relation to entrepreneurship attempts at justification come in the form of new economic activities for the purpose of realizing the expected value of such conjecture. In the terminology introduced by Shane and Venkataraman, it is the exploitation phase.

2.5 Independent and corporate entrepreneurship

In the words of Schumpeter, the entrepreneurial venture is the force that powers the “perennial gales of creative destruction” in which innovative new combinations enter the economy and drive out incumbent firms (Schumpeter, 1942). While Schumpeter (1983/1934) emphasized the independent venture (as in a new firm) as the main vehicle for new combinations, he readily acknowledged that the corporate venture can perform the same economic function and that it may in time assume a position as the leading source of innovation (Schumpeter, 1942). However, whether independent or corporate, a venture is an intersection of productive opportunities, resources and human effort. It is the capacity of the venture to introduce “new combinations” that makes it conducive to economic development, not the organizational context *per se* (Schumpeter, 1942, 1983/1934). The study of economic development, and hence the entrepreneurial process, therefore includes both independent and internal ventures. Indeed, Schumpeter (1942; 1983/1934) speculated that the importance of the corporate manager as an entrepreneur would increase over time as knowledge about the entrepreneurial process would become codified.

In an effort to tackle some of the definitional issues in entrepreneurship, Sharma and Chrisman (1999) develop a typology of the research issues that are common to this field. Their typology starts from a broad construct of entrepreneurship, which they divide into two subcategories. *Independent* entrepreneurship is the case where individuals act independently of any existing organization, while *corporate* entrepreneurship is embedded in an existing organization (Sharma & Chrisman, 1999, p. 18). These authors explicitly apply a concept of corporate entrepreneurship suggested by Guth and Ginsberg (1990), defined as “(1) the birth of new businesses within existing organizations, i.e. internal innovation or venturing; and (2) the transformation of organizations through renewal of the key ideas on which they are built, i.e. strategic renewal” (p. 5). The basis of this definition is, again, the “new combination” criterion put forward by Schumpeter (1983/1934). The logic behind the inclusion of strategic renewal in a definition of corporate
entrepreneurship is based on the notion that the “carrying out of new combinations translates into changes in strategy that alter the pattern of resource deployment” (Guth & Ginsberg, 1990, p. 6). Thus, the internal (or corporate) venture is one aspect of corporate entrepreneurship according to these authors. However, it is difficult to understand how the strategic realignment of an existing firm could come about in such small increments that they could not be traced back to specific ventures. Further, it may seem like the wrong way around to regard strategic renewal as the cause of entrepreneurship rather than vice versa, since strategy defined as a pattern puts the emphasis on consequence and action rather than cause or intention.

Stopford and Baden-Fuller (1994) provide an illustration of an entrepreneurial process over time that involves corporate venturing, strategic renewal, and market-level Schumpeterian innovation. This process starts with efforts of individual middle-level managers to start new ventures that may or may not be constrained by the top-level management. In the case where this semi-autonomous behavior is tolerated, strategic renewal will result as a consequence of venturing as the scope of the firm is changed. In the fortunate case where the new stream of ventures involves what the authors call “frame-breaking Change” (p. 522), this may result in Schumpeterian innovation. This description of the entrepreneurial process in an existing firm is similar to the one presented by Burgelman (1984). Interestingly, the emphasis on middle management as a driver of change is echoed in texts on dynamic capabilities (Eisenhardt & Martin, 2000; Teece, Pisano, & Shuen, 1997). Stopford and Baden-Fuller (1994) also provides us with an illustration of the different levels of entrepreneurship. On the micro-level (firm), the entrepreneurial process involves new combinations of resources. On the macro-level (market or industry), it is a venture’s ability to spread and institutionalize a new combination that is the essence of entrepreneurship.

By tracing the references in journal articles by authors regularly publishing in *Journal of Business Venturing* and *Entrepreneurship Theory and Practice*, two strands of research relating to corporate entrepreneurship emerge from the literature as defined by their original sources. One strand, initiated by Burgelman (e.g. Burgelman, 1983), begins in the process of corporate venturing and ends in a definition of corporate entrepreneurship as relating to both corporate venturing and strategic renewal (Covin & Miles, 1999; Guth & Ginsberg, 1990). The other strand of research largely owes to the works by Miller (building on Khandwalla, 1976; Miller, 1983; Miller & Friesen, 1978, 1982) but the terminology has drifted towards that of *entrepreneurial orientation* rather than entrepreneurship. This relabeling actually seems to reflect the substantive content of this research which emphasizes strategic posture rather than specific actions. However, two major commonalities exist. The first is that research in corporate entrepreneurship has chiefly tried to explain behavior in
large corporations, i.e. there is a clear emphasis on a definite empirical context (Biggadike, 1979; Burgelman, 1983, 1984; Burgelman & Sayles, 1986; Miller, 1983; Miller & Friesen, 1978, 1982). It should, however, be recognized that a small body of research on entrepreneurial orientation has partly used small and medium-sized firms for empirical data (Wiklund, 1998). The second commonality is the Schumpeterian inspiration that is established by explicit references to Schumpeter (e.g. Lumpkin & Dess, 1996; Miller, 1983; Peterson & Berger, 1971; Sharma & Chrisman, 1999; Stopford & Baden-Fuller, 1994; Zahra, Jennings, & Kuratko, 1999).

To sum up, research into corporate entrepreneurship as defined by Guth and Ginsberg (1990) has largely been occupied with the same organizational context: large firms. The focus on the large firm in corporate entrepreneurship is easy to understand from a historical perspective since the stagnation of the large corporation was a major challenge to strategists during the 1970s (Zahra et al., 1999). However, despite the fact that the economic importance of small and medium-sized firms has been clearly established (Davidsson et al., 1996; Kirchhoff & Greene, 1998), the entrepreneurial effect of SMEs has mainly been construed as entry (or exit). Surely, new economic activity in the shape of new internal ventures may lead to strategic renewal or Schumpeterian innovation regardless of organizational size. Corporate entrepreneurship in small and medium-sized firms should therefore merit wider attention than what existing research seems to have devoted to it and this is an area to which this thesis claims to contribute. For this, a baseline need is a method that can capture new economic activity both as independent entry and as internal ventures in a context of SMEs. In the next section, units on analysis will be discussed and an argument presented that units of analysis for independent and corporate entrepreneurship converge when new economic activity is used as a basis for operationalizations in these two empirical settings.

2.6 Units of analysis in entrepreneurship research

The level of analysis is important in any study and no less so for studies in entrepreneurship. In a recent review of current practice in entrepreneurship research, Davidsson and Wiklund (2001) found that the firm and the individual are the dominant units of analysis; no articles explicitly using the venture as the unit of analysis were found. In something of a tribute to Low and MacMillan (1988), Davidsson and Wiklund suggest that entrepreneurship is “the creation of new enterprise” where enterprise may be interpreted as economic activity. This definition departs from the definition suggested by Shane and Venkataraman (2000), who clearly separate the opportunity from the processes used to exploit it. Rather, Davidsson and Wiklund emphasize the creative aspect of the entrepreneurial process that Schumpeter describes, i.e. the actual
“carrying out of new combinations”. The conception of entrepreneurship as the creation of new economic activity resembles the proposition that entrepreneurship is about new entry, regardless of whether the new venture comes in a start-up firm or as an internal venture (Lumpkin & Dess, 1996). The main difference between these two definitions seems to be one of assumptions: while entry can only be understood from the perspective of an existing market or industry into which entry occurs, new economic activity can be understood from an internal or an external perspective and it does not presuppose the existence of a market or industry. However, neither definition adheres to a strict Schumpeterian definition of entrepreneurship. In fact, they span over the two distinct functions in the process of economic development. While the entrepreneurial entry will displace existing industry equilibrium, the swarm of followers will not. Rather, they will take the industry towards a new equilibrium position, which revokes the function of the Kirznerian entrepreneur (Kirzner, 1973, 1979, 1997a, 1997b). While this distinction is important to make, this is not to say that ventures lacking potential for Schumpeterian innovation should be excluded from empirical research on entrepreneurship. The Schumpeterian process of economic development works through both types of new economic activity and the phenomenon would not be possible to understand if one or the other type were excluded from study. The fact that both innovation and imitation are part of the process of economic development is not the same as saying that both constitute acts of entrepreneurship in a Schumpeterian sense, but this discrepancy should not influence data collection procedures and sampling since both type of enterprise must be included in studies with either a Schumpeterian or a Kirznerian perspective. A selection criterion based on Schumpeterian innovation would introduce severe selection bias, since it would provide no comparison with less entrepreneurial ventures. In addition, the feasibility of such selection would be based on the existence of a naturally occurring dichotomy between innovation and imitation. It is however argued elsewhere in this thesis (notably in papers nos. 5 and 6), that this is not a defensible assumption.

In their conclusions, Davidsson and Wiklund suggest “Concerning the dominant levels of analysis, most notably the firm, we would urge researchers who aim at making a contribution to cumulative knowledge on entrepreneurship to carefully make sure that their study really addresses pursuit of opportunity and new combinations, i.e. new enterprise” (Davidsson & Wiklund, 2001, p. 94). Indeed, this signals Schumpeterian influence, but leave further explication open to interpretation. In a later article, Davidsson (2003) is more expansive on the topic. Siding with Kirzner, he defines entrepreneurship on the society level as “the competitive behaviors that drive the market process” (Kirzner, 1973, pp. 19-20). Davidsson explains this in a simple 2-by-2 matrix in which newness in economic activity is related to the market and the firm. In
Davidsson’s view, new economic activity that is new to the market is entrepreneurship.

A similar typology that relates new economic activity to Schumpeterian innovation is presented in Figure 2.2. This simple 2-by-2 matrix places firm (internal) and industry (external) as orthogonal dimensions and new versus existing as categories. There are two differences compared with the model previously referred to in Davidsson (2003). The first difference is that the “macro-level” is industry rather than market. This reflects the different foci of the conceptual models described by Kirzner (1973) and Schumpeter (1983/1934); while Kirzner focuses on the market process, Schumpeter’s aim is to explain the increasingly efficient use of resources through innovation. Since the Schumpeterian innovator by definition starts a new venture and the macro-level consequences of interest by definition are new patterns of resource deployment in the production of goods and services among the producers, the appropriate macro level for newness in this model is the industry rather than the market. The second difference certainly lies in what entrepreneurship is, i.e. the phenomenon to be explained. These are minor differences but important distinctions to make in order to avoid misunderstanding. A case where these differences lead to incompatible interpretations is when new economic activity is based on proprietary process innovation that cannot easily be imitated. In this case, a new venture that produces an existing product with a novel production technology can reap the full entrepreneurial profit and may not expand its market offer. Thus, nothing may happen on the product market despite the new economic activity. However, the factor market will change since the new production method will use a different input mix compared with existing production technology. Kirzner (1997a) is explicitly concerned with the fact that input markets are also susceptible to arbitrage (which is the distinct function of the entrepreneur). Davidsson, however, seem to part with Kirzner on this point and focus on effects downstream of the venture.

In the classification presented in Figure 2.2, only Schumpeterian innovations would be found in the case where the activities of the new venture are new to the firm and to the industry into which the firm enters (upper left). Moving to the right, we find instances where the new economic activity is new to the industry, but not necessarily to the firm. Using existing resources, combinations to new ends can be understood as process innovation. To exemplify, this would mean that a firm introduces a manufacturing process previously untried in that particular industry. In addition, geographical expansion may also count as an entrepreneurial entry. Innovations and novel business practices do not spread by themselves: they have to be introduced. Further, few markets are truly global. If the geographic expansion of a firm causes a geographically delimited industry to change, it would be an entrepreneurial entry. In the bottom left quadrant, we find firms that try to do
things that are new to them but not to the industry in which they are entering. According to the Schumpeterian definition of entrepreneurship, this type of venture is not entrepreneurial in itself. Still, it is a form of venturing that is essential to the process of economic development and that we may call equilibrium venturing (Samuelsson, 2001). This implies that the exploitation of an opportunity has an arbitrage function and takes the market towards equilibrium.

At the bottom right, we find ventures that are not new in any other respect than from an organizational point of view – the activities are neither different in kind to the firm nor to the industry and the venture can only be recognized based on its degree of organizational autonomy. This is an example of the expanding firm that exploits existing opportunities in an industry on the move toward equilibrium.

In terms of Schumpeterian innovation, only the upper two quadrants contain new combinations that can lead to disequilibrium, which is a macro-level phenomenon. However, to understand the whole process of economic development and creative destruction, we should also include new economic activities that emerge in the third quadrant (lower left) as they represent resource recombination, a micro-level phenomenon. Further, macro-level effects such as a new competitive landscape or imitation ventures will only come with considerable lag. In brief, entrepreneurship researchers with an interest in Schumpeterian economics and firm-level behavior should aim at capturing ventures that contain economic activity that is new, either to the firm or the industry into which it enters.

The “new economic activity” suggested by Davidsson and Wiklund (2001) can be thought of as an abstracted way of denoting new ventures, whether independent or corporate. In the case of new independent ventures, the “new
econcomic activity and the firm may be identical levels of analysis since the new firm would be the organizational (and legal) entity in which the new economic activities would be carried out. Thus, the suggestion by Davidsson and Wiklund for a new level of analysis seems to make the most difference in the context of corporate venturing in which the proposed unit of analysis is different from the firm. Using the venture as a unit of analysis also opens up the possibility for studies of the entrepreneurial process beyond relative firm performance, which is a dependent variable that is typical of strategic management rather than entrepreneurship (Venkataraman, 1997). A single-minded focus on relative firm performance may limit our understanding of positive effects of a new venture on wealth creation if it does not translate into superior firm performance (Davidsson & Wiklund, 2001). When the isolating mechanisms (Rumelt, 1984) of the new venture are too weak, the entrepreneurial profits will not accrue to the entrepreneur if they are eroded by the forces of competition. Thus, the new venture may have been a failure if measured as firm performance but not in term of general wealth creation. This is one instance where the use of firm performance as an indicator of success could underestimate the wealth-creating ability of a new combination that is easy to copy. An additional advantage of using the venture as a unit of analysis is that independent and corporate venture are conceptualized as comparable units, which makes it possible to study the impact of contextual differences.

2.7 Summary

Whether entrepreneurship is construed as the carrying out of new combinations (Schumpeter, 1983/1934) or behaviors that drive the market process (Kirzner, 1973), scholars of entrepreneurship must study different types of efforts to exploit economic opportunity to produce value above the alternative cost of the resources employed, regardless of macro-level newness or potential for revolutionizing an industry. We shall view these efforts as attempts to introduce new economic activity for the benefit of economic progress. By turning to new economic activity as the unit of analysis, we free the study of entrepreneurship from artificial boundaries based on legal form, making issues of independent vs. corporate entrepreneurship a question of contingencies rather than differences in kind. This opens up for comparative studies of entrepreneurship across different organizational forms. In spite of the theoretical appeal of new economic activity as a preferred unit of analysis for entrepreneurship research, empirical feasibility will prove whether the concept is a possible way forward.

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3 As in new economic activity.
2.8 Purpose

The purpose of this thesis was revealed in a shorthand version in the introduction. In the sections that followed, we have further developed the fundamental concepts of this purpose and may now formulate it more formally. Thus, the purpose of this thesis is to assess entrepreneurship as the creation of new economic activity by examining process and performance of entrepreneurial discovery and exploitation in independent and corporate ventures.

While we have already seen that new economic activity is a unit of analysis appropriate to entrepreneurship, its relevancy to empirical research depends on whether it can be operationalized in different organizational context. The upcoming chapter on methods will therefore dwell considerably on this topic to show that, a) new economic activity as unit of analysis is possible to operationalize, that b) it is feasibly to carry out empirical work using this unit of analysis, and c) it makes a difference compared to alternative units such as the firm or the individual.
3 Methods

3.1 Introductory note

The structure of this chapter begins in a classical manner by, a) stating the demands that the subject matter puts on methodology, b) explaining how the methods chosen meet these demands and, c) a detailed account of what was actually done in terms of data collection and analysis. However, in addition to this, the last section of the chapter (section 3.8) contains detailed commentaries on each paper concerning methodological fit. Although placed as a section in a “methods” chapter, these commentaries form an integral part of the contributions of this thesis and should consequently be considered to be on par with chapter 4. For this reason, I did consider putting the commentaries into a separate chapter. However, since section 3.8 specifically contains methodological comments, I have decided to retain these commentaries in this chapter together with this introductory note.

3.2 Capturing new economic activity

In the previous chapter, the overall purpose and scope of the studies were stated. In order to avoid any misunderstandings, we will restate central assumptions of this thesis that are of consequence to methodology.

- The fundamental function of entrepreneurship is the origin of new economic activity (Herbert Simon in Sarasvathy, 2000). To be accessible to empirical research, this concept must be operationalized.
- New economic activity as a unit of analysis has an inspiring advantage since it puts up no artificial barriers between different types of venture based on the type of agent that is actually venturing. Thus, methods have to be developed for both independent and corporate ventures for the concept to realize its full potential for empirical work.
- The perspective adopted in this thesis is that entrepreneurship as a scholarly domain should “explain and facilitate the role of new enterprise in furthering economic progress” (Low & MacMillan, 1988, p. 141.). Although this can be interpreted from a Schumpeterian or Kirznerian point of view, the qualifier “new” in and of itself naturally requires some sort of screening mechanism to separate “new” ventures from “steady-state” ventures.
Thus, the two major methodological challenges for this chapter are a) to operationalize new economic activity into a unit of analysis available for sampling both for independent and internal (corporate) ventures, and b) operationalize indicators of newness for the purpose of detecting new economic activity or empirically, new ventures. The line of thought is schematically illustrated in Figure 3.1. Briefly, when the core unit of analysis is new economic activity, the methodological problems can be broken down into two components. Going from left to right, the first problem is to assess how new is “new”. The newness of economic activity can further be divided into a) level or perspective (to whom is this new?) and b) degree (what is the extent of the newness involved?). The second problem is to capture the actual unit of analysis and for the purpose of the present line of inquiry, economic activity is defined as human action over time to create value from deployment of resources above their alternative cost. On the one hand, this is a way of describing the process of opportunity exploitation as suggested by Shane and Venkataraman (2000). It also matches the function of entrepreneurship suggested by writers such as Schumpeter (1983/1934), Kirzner (1973), and Casson (1982). The key issue is “the furthering of economic progress” (Low & MacMillan, 1988, p. 140), whether as in Schumpeterian development or as in Kirznerian increased efficiency, that follows from introduction of new economic activity: society enjoys more wealth because resources are put to more efficient use. Although this view of economic activity relies on objectively existing opportunity to create value or wealth, this phrasing is also in line with subjective drive and empirical reality: people venture to create new economic activity to find an alternative use for existing resources and in the process to be better off than before. If it were not so, it would be hard to understand the motives of the entrepreneur. It is suggested that defining new economic activity in this way makes entrepreneurship research less troubled by competing conceptualizations because regardless of theoretical perspective, the units of analysis converge in studies of the empirical.
METHODS: Choice of design in general

Figure 3.1 A schematic of the methodological issues at hand in operationalizing new economic activity

3.3 Choice of design in general

The notion that the choice of method should be based on the research question is somewhat simplistic because it implies that a) there is some sort of universal understanding of what is the right method for a specific problem, and b) that the researcher is able to choose freely from suitable research methods. However, depending on scientific persuasion, it seems that almost any method can be deemed appropriate, or at least adequate, by some “invisible college” (Kuhn, 1996). Further, as was pointed out in the theory section, “opportunity recognition” is dependent on prior knowledge and there is no apparent reason to assume the case to be any different with researchers. Methodological training limits our scholarly field of vision, but at the same time, it will enhance our ability to find interesting aspects of the phenomena that we actually catch in our epistemological headlights. From this perspective, it is the research questions that will be adapted to a method, not the other way around.

Given such a pessimistic view of the chances of achieving universal agreement on what is methodologically appropriate, the following chapter will still argue that the applied methods were appropriate for these particular studies. How can this be? The aim is to provide a justification of the methods given a specific set of beliefs about how research can be conducted. These beliefs may not be shared by everyone, but this is not a problem that can be addressed by the individual researcher in isolation. It is by giving a rationale for the methodological choices made that research can be made understandable regardless of ontological or epistemological persuasion.
The design chosen for the studies included in the thesis is a real-time longitudinal survey of a cohort of firms involved in the creation of new independent and internal business ventures. The following three subsections will state the rationale for selecting this particular type of approach and the argumentation for the proposed method goes along three lines. First, the concept of quantitative method is defined. Second, I discuss the probabilistic approach to social science in order to explain the rationale for using statistics in relation to prediction. Finally, the importance of a longitudinal research design is explained.

3.4 A quantitative method is characterized by quantification

There are many conceptions of what quantitative method actually is. Many people seem to think about statistics, but there are also connotations of hypothetic-deductive theory testing involved (Bryman & Cramer, 1999). The advent of computer programs such as NUD*IST (Gahan & Hannibal, 1998) has certainly eliminated computer-aided data processing as a usable delimiter between qualitative and quantitative methods.

Luckily, data do not seem to be aware of our treatment of them and the researcher has to make a choice based on research interest rather than on any intrinsic nature of the data. In principle, data can be split into two categories: quantified and not-yet-quantified. As soon as we have identified a phenomenon and categorized it as being something, we can choose to quantify it in a dichotomous variable with the values “existing” and “non-existing”. However, we need to have several instances of this phenomenon before we can start counting. Depending on the nature of our categorizations, we can then apply various mathematical methods in order to shed light on the proportions or associations of different categories, regardless of how we define our units of analysis. We do not need hypotheses to make the application of mathematical methods meaningful, neither do we need a large number of cases. I therefore see quantification (for the purpose of counting) as the distinguishing mark of quantitative methods. Consequently, I view qualitative as denoting methods in which data are not quantified. As for the number of cases, I can see no theoretical upper or lower limit. However, since the human mind has cognitive limitations, there is certainly a practical upper limit for qualitative assessment of data. Given that I regard all data to be quantifiable in the sense of countable instances, I do not believe it is meaningful to do so with all data or with all research questions. Still, I have miserably failed, in spite of my sincerest efforts, to see any inherent epistemological home for quantitative methods.

* However, many mathematical methods of estimation actually do put restrictions on the lowest number of cases needed.
3.5 A probabilistic approach to social science

For many researchers, a major quest is to learn something about the future from the past. Although it can be important and meaningful to understand a particular event in and of itself (Stake, 1994), researchers are often more interested in the lesson learnt rather than in a specific event. Whether researchers are using empirical observations to inductively construct theory or to test propositions deduced from theory, they create knowledge from the examination of historical data, knowledge with the potential to inform decisions about future actions. Given that we only have accounts of past behavior, can we predict future behavior? Experience shows us that we can, but only probabilistically so. We may tend to be right but we cannot be certain in any specific case. Thus, theories that invite us to make predictions will actually be inaccurate in most specific instances. They will be almost true. Why is this? I will offer two possible explanations, one ontological and one epistemological. However, a basic premise lies in the heart of both and that is the notion of stochastic processes. A stochastic process is a process that results in individual events that cannot be predicted with certainty but where the distribution of these events can be known. A consequence of this is a view in which individual idiosyncrasies are interpreted as belonging to the same distribution as does the “typical” representation of the process that generate particular events (McKelvey, 1997).

The adoption of a probabilistic ontology makes a strong case for a probabilistic epistemology. Assuming that the world is the result of stochastic processes would provide us with the only reason we need to use statistical methods. If we believe that properties, relations or meanings are inherently distributed around typical representations, then we can only make statements about probable properties or relations. I think that many are quite uncomfortable with such an ontology in relation to prediction; in a probabilistic world, relationships between actions or properties could change intrinsically from case to case. In practice, it would mean that the relation between one factor and another would change states not because of the influence from some overlooked auxiliary factor or contingency but from genuine distribution in that relation.

Another cause for theories to be almost true is one of epistemological deficiency; while theories are based on ideal types, verifiable predictions can only be made in relation to real world phenomena. Since empirical data do not exactly correspond to the ideal concepts of theory, we cannot expect theory to
yield exact predictions about the real world. This involves problems of both validity and reliability, but theories may still be approximately true.

Empirical data can also be fraught with measurement errors from methodological deficiencies. Sometimes the errors are small and sometimes large, but as long as the errors are the result of a stochastic process, theory and experience tell us that these errors will tend to cancel out on average. So, while we may actually be slightly wrong most of the time when attempting to predict something with a measurement error, we can calculate the probability of being off target by a certain quantity.

Another epistemological problem stems from the fact that it is impossible and even undesirable to take every conceivable relation into account when constructing a theory. Due to our limited cognitive capabilities, we need simplifications and this is where theories come in. Theories and models are necessarily reductions of the real world. Their task is to focus our attention on important relations rather than to recreate the world in full. Although excluding factors from a model may be necessary from a cognitive point of view, the effect of the omitted factors will still be there. When we deliberately underspecify our models, we do not include all the factors that may have an effect on the outcome we try to predict and we will know nothing of their influence in the specific case. Luckily, the influence of a large number of arbitrarily distributed factors will tend towards the Gaussian distribution, and we may draw conclusions about the expected influence of these omitted variables in a probabilistic sense. Still, the fact remains that our predictions will be wrong most of the time.

Regardless of whether we assume a deterministic or probabilistic ontology (or no ontology at all for that matter), being “wrong” most of the time seems to be an inherent problem when we try to make predictions from theory. If we accept that all theories that can generate verifiable predictions will be only almost true, how do we decide which one is best? This is another case where the probabilistic approach can accommodate the evaluation of predictions that are reasonably accurate but never exact. It is by describing the world in terms of expected outcomes (as in most likely) and the associated degree of uncertainty (probability) that we can evaluate the predictive power of theories that are

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5 This statement depends on the existence of truth. However, a discussion about the concept of truth would stray too far from the core problem of prediction. A working definition of a “true theory” could be something along the lines of “a theory that makes predictions that are in line with observable facts”.

6 Note the use of the term “underspecify”. This is to signal that a model is not (necessarily) subject to misspecification. In a modeling context, misspecification is the exclusion of independent variables that have a high covariance with other independent variables still in the model. This leads to biased coefficient estimates.
almost true (Haavelmo, 1944). In order to give the phenomena under study a chance to “get distributed”, i.e. to assume values in a probability distribution, we need to observe a sufficient number of instances.

Quantitative methods are often associated with inferential statistics. The basic premise of inferential statistics is the process of sampling in which only a small percentage of a large population is researched (Bryman & Cramer, 1999). From this sample, the researcher infers properties or relations to the totality of the population from which the sample was drawn. If the sampling procedure is a random process, some of the samples we obtain will not be representative of the population. However, if we know the sampling distributions, we can assign probabilities to the fluctuations of estimated population properties when they are inferred from a sample. The precision by which we can use a statistic (characteristic obtained from a sample) to estimate a parameter (characteristic obtained from a population) is proportional to the square root of the number of cases in the sample, hence the idea that quantitative methods need “many” cases. If we are sampling entire populations, such as all employees in a firm, we do not need inferential statistics to study this particular case. It is only when we do sample that sample size becomes an issue.

Many statistical methods are strictly valid only if certain assumptions are met. The ubiquitous classical normal linear regression analysis has no less than six fundamental assumptions that have to be met for the model to be strictly valid (Greene, 2000). However, the effects of violating these assumptions can usually be contained and the estimates will still be reasonably trustworthy. The only problem that definitely cannot be “contained” by any statistical technique is a sampling without known sampling probabilities since it violates the very heart of inferential statistics. Without a known distribution of the sampling probabilities, the probabilistic link between the sample and the population is broken and we have no probabilistic base for inferring our results to the population. Regardless of our choice of specific statistical method, we therefore need to assert that we are using sampling methods with known sampling probabilities.

Sometimes, the researcher may want to infer some properties to a theoretical population such as “innovative firms”, “radical youth”, or the disabled”. Being theoretical, the sampling probabilities of such a population cannot be known and they are therefore not directly accessible to inferential statistics. In these instances, the researcher must transform the theoretical definition into an empirical definition through a process analogous to the operationalization used by researchers to transform theoretical concepts into empirical categories. The appropriateness of such transformations can be discussed in terms of validity. Nevertheless, from a statistical viewpoint, we can only make inferences to the population actually sampled and this will always have to be defined in a way that is empirically accessible.
3.6 Causality and process calls for longitudinal research designs

Low and MacMillan (1988) called for longitudinal research designs when studying entrepreneurship. The main impetus behind this recommendation was an explicit hope to guide the field towards explanation and causality (p. 141). Interestingly, the call from Low and MacMillan seems to have had little effect on entrepreneurship research in the decade following their oft-cited article (Chandler & Lyon, 2001). So if not for theoretical reasons, longitudinal research design may still have important empirical contributions to make in the field of entrepreneurship.

A basic assumption in causal models is that the cause precedes the effect in time. In a cross-sectional design, the cause and the effect are measured at the same time, and without external information, we cannot know in what temporal order the variables assumed their observed states. Sometimes this is unproblematic: when one variable can be assumed to be constant across time, such as sex or ethnicity, there will be no challenging hypotheses about the direction of causality. If, on the other hand, there are competing hypotheses about an assumed causal order, there is no way by which we can empirically distinguish between these competing hypotheses from a cross-sectional design. This is the essence of the causality argument for longitudinal research designs. One illustrative example from research on growth is the relation between growth and growth aspirations. It may well be that aspirations to grow a firm will affect a firm’s growth rate. It is however equally plausible to suggest that previous growth influences growth aspirations, making growth an “acquired taste” (Delmar & Wiklund, 2003). To distinguish between these two hypotheses, we need a research design in which values are measured and collected at different points in time to establish temporal order between factors that we suspect to be correlated.

In research on firms or new ventures, the use of a longitudinal research design is also called for when researching inherently developmental properties such as growth. When studying processes such as growth we need to let time pass in order to let the focal process assume different states or to have effects that are measurable in some way (Wiklund, 1998). This is an additional argument for longitudinal research designs for the study of the emergence of new ventures and is separate from the causality argument set forth by Low and MacMillan (1988). Given the emphasis on emergence and change in contemporary writing on entrepreneurship, the implication is that time is inextricably woven into the fabric of entrepreneurship research. Thus, the design of a study that aims to follow the development of a venture needs to adopt a longitudinal method to capture the process of development.
3.7 Data collection and sample

The advantages of a longitudinal research design have previously been discussed in general terms, particularly in relation to causality and process. Causality is a concern for most studies bent on prediction and in addition the research questions for this study specifically involve process, and time enters as a variable. The fact that people may rationalize after the fact makes it difficult to establish credible arguments for causality if data are not collected at the point in time which they are supposed to measure. Menard (2002) suggests using the terms retrospective to denote studies in which the data is collected after the time to which measurement is referred. Not surprisingly, he suggests the term prospective for studies where measurement and collection of data are synchronized in time. Although somewhat upsetting to this nomenclature, we will henceforth refer to the latter type as a real-time longitudinal design because it is more descriptive of method: the process under study and the study itself proceed shoulder to shoulder as events unfold.

Retrospection bias apart, the real-time design has an additional advantage of being less prone to selection bias, which is a crucial methodological problem when studying processes across time. If we were to study attempt to introduce new economic activities with a retrospective design, we could only do so with the ones that were successful enough to survive and still be available for sampling. Selection bias is not uncommon and for the purpose of this thesis it is crucial that we avoid it. Important as it is to follow entrepreneurial processes in real time, we will also need historical data on the host firms of the new ventures since past performance and strategies may influence the ongoing venturing process.

The following two subsections will give detailed accounts of the actual data collection processes used to capture independent and internal ventures. The major challenges in this area are a) to capture the chosen unit of analysis, i.e. new economic activity in the form of new ventures regardless of organizational context, and b) to register indicators of newness.

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7 This is in contrast to various retrospective “not-so-longitudinal” datasets that are measured retrospectively at one point in time. The measurements will in this case be “longitudinal”, i.e. ordered in time, but the method will not.
3.7.1 A panel of new independent ventures

The studies included in this thesis use a panel of 7,256 new ventures that were started as "genuinely new enterprises" in 1994 (henceforth called "Cohort 94"). The methodology used to construct this dataset was developed by Statistics Sweden circa 1985 for the purpose of estimating the number of genuinely new firms in Sweden. The extensive pre-study involved attempts at using the high quality business registers that are available in Sweden. However, this line of approach was abandoned in favor of a survey methodology for primary data collection necessitated by the large amount of noise that was picked up when using business registrations only. Thus, a large survey is carried out annually. In addition, this panel has been surveyed in its entirety three times: in 1995, 1998, and 2000, and the author has been involved in the data collection from the second wave in 1998 and onwards. In more detail, the panel was created in the following way. In February 1995, a sampling frame was constructed by Statistics Sweden, covering all legal forms of business activities registered during 1994. To be considered a registered business activity (or enterprise) in this sample, the business founders did not have to make any formal registration with a business register. Reporting moms (VAT) or income from any business activity on the personal income statement was sufficient. By cross-referencing four different databases, it was found that 74,600 new business registrations were made in Sweden during 1994. From this first sampling frame, 14,500 businesses in the sectors agriculture, forestry, hunting, fishery and real estate were excluded. This was done to achieve economy of information. Agricultural registers show that very little new economic activity is created in the Swedish primary sector. However, many new business registrations are recorded in these sectors as takeovers. In addition, the real estate sector spawned new registrations by placing existing buildings in new and separate firms at a time when real estate development was at an all-time low in Sweden. Arguably, this has lead to under-coverage in these sectors but the problem was deemed containable in

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8 This term denotes “new business activity in a new legal entity”. The Swedish word that is used for “enterprise” is företag (as in genuint nya företag) which corresponds more closely to the German Unternehmen and to the English “enterprise”, than to “business” or “firm”. It denotes a “business” but it also has important connotations in the more general sense of “enterprise” or “undertaking”. Schumpeter’s original use of Unternehmen in the German version of his Theory of Economic Development may be the reason why Penrose (1959) uses the word “enterprising” when referring to phenomena currently labeled entrepreneurship. It is at any rate a more precise translation.

9 The Business Register at the Swedish Patent and Registrations Office (PRV), the Swedish VAT Register (Momsregistret), The Swedish Business Register at Statistics Sweden (FDB, formerly CFAR) and the Register of Statements of Income at the Swedish National Tax Board.
METHODS: Data collection and sample

relation to the few cases and money saved for sampling other, more dynamic, parts of the economy. Moreover, another 2,700 businesses in various industries were also excluded since it could be clearly established from register information that these were take-overs. Thus, the final sampling frame consisted of 57,400 newly registered businesses, from which a stratified random sample of 14,000 was drawn. Strata were constructed according to industry, legal form and municipality to enable inference and a more even precision of point estimates across categories, particularly regarding geographical region.

In the first wave, a mail questionnaire was sent out to the businesses in the sample, yielding a response rate of 86 %. This unusually high figure for an unsolicited mail survey was in part due to the fact that the core questions were part of a compulsory business survey. In addition, extensive use of telephone interviews in case of non-response in the mail survey contributed considerably to the final results. Out of the approximately 12,000 responses, 7,256 fell into the definition of genuinely new enterprises; these were later selected for the follow-up. According to the definition provided by NUTEK and Statistics Sweden, a “genuinely new enterprise” has to be a new business activity in a new (independent) legal entity. It cannot be a take-over or a mere re-registration of an already existing business (e.g. change of legal form). Based on the annual surveys of genuinely new enterprises, it has been shown that no less than 48 % of all new business registrations are not new business activities, but simply existing business activities put into new legal entities. This emphasizes the importance of a screening of some sort when addressing research questions related to new business activities, regardless of organizational context.

In August 1998, a second wave of questionnaires was issued to all of the 7,256 enterprises that were recognized as “genuinely new enterprises” in the initial 1995 survey. A similar method of inquiry was used, but there was a heavier reliance on telephone interviews to minimize non-responses. Out of the 7,256 businesses that were selected for the second wave, responses were obtained from 6,377, resulting in a response rate of 87.9 %. However, in 97 cases the original business had been sold or merged with another firm. Although these firms were apparently surviving at the time of sale, they were excluded from further analysis since it would be difficult to assess the performance of the original activity, both conceptually and empirically. Businesses that were non-response cases in the second mail survey became the objects of intense investigation with the aim of contacting individuals connected to these businesses. Since some of the questions in the follow-up were compulsory by law, simple refusal to answer was extremely rare. The final non-response cases are those businesses that could not be contacted by any means (telephone or mail), including all individuals registered with the business in question. Access to register information (telephone numbers or addresses) on the non-response businesses was not an issue to the authority that administered
the survey (Statistics Sweden), and a common characteristic of the final non-
responses was either a protected telephone number or simply no listing. It can
safely be assumed that it would be very difficult to run a legitimate business
under such circumstances and that it is likely that these businesses have
suspended operations, be it temporarily or definitely. The non-response cases in
the follow-up survey were therefore considered as “not active” and consequently
excluded from further analysis. The method used to produce this sample has the
advantage of excluding businesses that are not new. Since the dataset was
originally designed for inferential statistics, it is also representative of the
population of genuinely new businesses that were started in Sweden in 1994. In
summary, the Cohort 94 panel has the following features suggesting that it may
be adequate for the problems addressed in this thesis:

- The panel is screened for “greenfield” start-ups, which is a prerequisite for
  studying new economic activity. Empirically, about half of the new business
registrations in Sweden are not the result of new business activities but of
re-registration of ongoing businesses.
- The panel is representative and comprehensive in terms of geographic
coverage, legal forms and business sectors (except for primary sectors) and is
based on a random sampling procedure with known sampling probabilities.
Available commercial business registers such as Dun & Bradstreet only
include incorporated companies, which would exclude 80-90% of all new
ventures and has little scope for distinguishing genuinely new ventures.
- The panel contains firms of the same age, i.e. the panel is a true cohort,
which controls for economic factors at the time of start-up. Early processes
could be expected to have a critical impact on the development of new
ventures. Thus, a cohort is a means of controlling for those influences. This
is even more important in a representative sample such as this, since the
added variance that comes from sampling across all industries, legal forms
and geographic areas may threaten the power of statistical tests.
- The panel is well documented.

3.7.2 A panel of new internal ventures
To study new internal ventures, a sample of firms needed to be screened for
new internal ventures in gestation. For the purpose of these studies, the
screening procedure needed to meet two additional criteria. First, the degree of
newness should be measured. Contrary to new independent ventures, the
internal (or corporate) venture may show a degree of newness (or relatedness) in
relation to the host firm. Moreover, newness will also vary in relation to market
and industry in the same way as for the new independent venture. However,
since the original screening of new independent ventures in 1995 did not
include such explicit measures of market or industry newness, it appeared to be
even more important to develop and test such measures when launching a new study of new internal ventures.

Given that the new study would follow human action over time, a second criterion of great importance emerged. When measuring the venturing process by recording manifest activities, having the majority of activities in one spell translates into deflated variance in relation to time. This makes it difficult to distinguish between ventures that have different process speed. In addition, certain statistical techniques such as event history analysis are particularly sensitive to so-called left-hand censoring, which springs from the fact that we will lack information on projects that are abandoned before the first survey questionnaires are distributed (Yamaguchi, 1991). In order to avoid having too many activities completed in the first spell, it was imperative that the first survey wave be issued as soon as possible after the screening stage.

Another important aspect of time was the survey interval. If we choose too long an interval, we will run the risk that most of the activities associated with the new venture will occur in a single spell. The consequence will again be that we cannot measure variance due to time, and many of the advantages of a real-time longitudinal research design will be lost. Choosing a short survey interval may on the other hand exhaust the respondents and jeopardize further participation. The PSED project (Reynolds, 2000) used a twelve-month interval with retrospective time-stamping, apparently without excessive attrition from respondent fatigue. The Swedish version of PSED used an interval of six months (Samuelsson, 2004), again with negligible detriment to the panel size over time. Choosing an interval of six months over a period of 30 months seemed reasonable and was feasible given the economic constraints facing the project. In addition, this design would generate the four or more time points that are desirable in order to be able to fit any curve shape to the growth of the process variables (linear, curvilinear, u-shape or inverted u-shape).

The existence of the Cohort '94 panel of new ventures turned into firms and the fact that it was well documented made it a strong candidate for the purpose of sampling new internal ventures. In order to screen the Cohort '94 for new business initiatives, a two-stage screening procedure was conceived. In the first stage, the panel in full received a multi-item mail questionnaire that included the basic screening question:

"Do you presently have a new business activity in progress, to which you or other people in the firm devote time or other resources, but from which you have not yet received a steady stream of income?"

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10 The respondents’ answers were collected every twelve month and individual activities were time-stamped in retrospect according to the information provided by the respondents at that time (0, 12 and 24 months). In the Swedish survey, the initial spells were shortened to six months (0, 6, 12, 18, 24 and 75 months).
The second stage of the screening procedure used a telephone survey methodology, which has some specific advantages that were exploited in this study. First, a telephone survey with computer-assisted questionnaires and well-trained operators makes it possible to use very elaborate “skip patterns” without complicating the survey questionnaire as perceived by the respondents. This was a crucial feature since the second screening stage contained several iterative loops. Second, telephone surveys usually yield higher response rates than mail surveys. Experience from earlier studies in Sweden has shown that the higher response rates of telephone surveys contributes significantly to cutting the cost per usable response down to the same order of magnitude as mail surveys.

In order to screen and classify the venture in terms of newness, a series of questions were asked. The questionnaire used skips if the answer could be unambiguously derived from other questions, e.g. by beginning with the higher value in an ordinal series of questions. The scheme included four distinct areas of newness exploited in the new venture\(^\text{11}\). These are presented in Table 3.1.

The telephone survey used a looped multi-item scheme to refine the screening procedure. Because of the time lag from the first to the second screening stage, operators checked if (1) the new venture was still an ongoing project or if (2) it had started to generate a steady stream of income. In an effort to minimize attrition, firms that had either abandoned or completed new ventures were asked if they had other new ventures under way that might be eligible for this study. If that was indeed the case, the new venture was substituted for the terminated or completed venture. The classification scheme followed the order presented in Figure 3.2.

In August 2000, the Cohort 94 panel was updated in preparation for the third survey wave of the full panel and to provide a new sample of new internal ventures for the present study. Firms that had not answered the 1998 survey and firms that had merged with another firm since 1998 were excluded from the panel. Contact information was updated from government registers. The initial selection indicated that 4,950 firms would be included in the third wave. On September 14, 2000, a mail questionnaire was sent out to the updated panel. Two reminders were sent out in October 2000.

During November 2000, Statistics Sweden phoned about 200 firms that had not answered the mail survey in order to obtain maximum response rates from the panel. A second file containing the complete data from the mail survey was delivered on January 12, 2001. This added 213 new ventures to the initial 477. However, these additional ventures have not been further pursued since

\(^{11}\) To simplify notation, “product” will sometimes denote “product or service” in cases where space is premium, such as in Figures and tables. The choice is somewhat arbitrary and does not reflect the importance of either products or services. Approximately 90% of all new firms in Sweden are presently established in the service sector.
METHODS: Data collection and sample

the time lag between the first-stage screening and data collection risked to impair the real-time properties that the study aimed for.

On November 3, Statistics Sweden delivered a file containing survey results for those firms that had the organizational status “the firms continues as before” along with contact information for those firms that had given Statistics Sweden permission to reveal their identities. This file contained 477 firms that had consented to participate in the follow-up survey. The updated contact information provided by Statistics Sweden was used for the telephone survey, which started on November 27, 2000.

Table 3.1 Classification scheme used to determine venture newness

<table>
<thead>
<tr>
<th>Area</th>
<th>Items</th>
<th>Levels</th>
</tr>
</thead>
<tbody>
<tr>
<td>Firm</td>
<td>a) Product completely new to firm</td>
<td>yes/no</td>
</tr>
<tr>
<td></td>
<td>b) Product is a significant improvement of firm’s current offer</td>
<td>yes/no</td>
</tr>
<tr>
<td>Industry</td>
<td>a) Product is new to the industry</td>
<td>perceived industry</td>
</tr>
<tr>
<td>Market</td>
<td>a) Serving customer presently not served by any other firm</td>
<td>local; national; global</td>
</tr>
<tr>
<td></td>
<td>b) No similar product on the market</td>
<td>local; national; global</td>
</tr>
<tr>
<td></td>
<td>c) Customers or customer segment new to firm</td>
<td>local; national; global</td>
</tr>
<tr>
<td>Distribution</td>
<td>a) Mode of distribution is entirely new to firm</td>
<td>yes/no</td>
</tr>
<tr>
<td></td>
<td>b) Mode of distribution is a significant improvement to firm</td>
<td>yes/no</td>
</tr>
<tr>
<td></td>
<td>c) Mode of distribution new to industry</td>
<td>local; national; global</td>
</tr>
</tbody>
</table>

The second screening showed that the telephone survey method contributed constructively to the purpose of this study. The second stage of screening reduced the number of ventures eligible for further study from 477 to 250. Although no data were explicitly collected to validate the screening question in the mail questionnaire, operators anecdotally confirmed that miscomprehension was part of the causes for the reduction: some ventures were not really new in terms of activity. Even though every effort was made to minimize the time lag between the mail screening and the telephone survey, some ventures had already been abandoned and some had started to receive a “steady stream of income”. It could therefore be argued that the decision to retain only the first batch of responses was reasonable in the face of the changes encountered in the sample from September 14, 2000, to the end of October, 2000. Summing up, the new sub-panel of new ventures and host firms has the
following properties that speak in favor of its use for the purpose of the studies included in this thesis

- The sub-panel was constructed by screening a representative cohort of existing firms for new ventures regardless of organizational form.
- In addition, multiple measures of newness were recorded in order to map the nature and extent of newness of the new ventures in relation to the industry/market and to the host firms.
- Efforts were made in the design of the study to record development for the new ventures proper in parallel with that of the host firm.
- In order to reflect process, time was prioritized as a variable. Relatively short spells (six months) and minimum delay between screening and initial data collection are examples of this.
Figure 3.2 Flowchart showing the classification sequence used to determine the degree of newness of the new internal ventures sampled from the Cohort '94 panel of firms.
3.8 The feasibility of assessing entrepreneurship as new economic activity

3.8.1 Introduction
In the previous sections, the actual procedures for data collection were presented in some detail. In sum, all business registrations (very broadly defined) in Sweden during 1994 were used to establish a sampling frame for the screening of greenfield start-ups. Those ventures/firms that were retained after the screening and initial survey (about 7,250 valid responses) were later surveyed twice over a period of five years. In the year 2000, the panel of then six-year-old firms was screened for new internal ventures. Some 250 new internal ventures were found in this subsample and were followed along with their host firms for an additional period of 30 months.

In the following sections, the individual papers will be commented on regarding the appropriateness of the chosen design. Although the papers are located in the appendices, it is advisable to read them before reading these commentaries. To provide a brief overview “at a glance”, the papers are summarized in Table 3.2, including dependent variables, independent variables, methods of analysis and results. A second layer, containing the unit of analysis, may be placed on top of this table grid to illustrate the methodological challenges involved in capturing the appropriate unit of analysis. However, to retain the clarity provided by a simple table, this has not been done graphically. Instead, the reader is invited to study the separate commentaries, which all share a distinct focus on the particular methodological issue of sampling and level of analysis. Please note that the commentary on the first paper contains remarks on issues that are common to all papers and that are only briefly recalled in the other commentaries. This includes some of the fundamental sampling issues such as screening and cohort effects.
<table>
<thead>
<tr>
<th>Paper no.</th>
<th>Dependent variables</th>
<th>Independent variables</th>
<th>Method of analysis</th>
<th>Results</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Performance scale:</td>
<td>Initial conditions</td>
<td>Multinomial Logistic Regression n=7,159-4,091</td>
<td>Incorporation (+) Start-up allowance (+) Financial capital (+) Human capital (+) Retail &amp; Personal Service (-) Dissimilar predictors for marginal survival and high performance</td>
</tr>
<tr>
<td></td>
<td>Failure</td>
<td>(resources)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Marginal survival</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>High performance</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>Survival</td>
<td>Start-up reasons</td>
<td>Logistic regression Linear regression n=6,280-4,091</td>
<td>No effects of start-up reason on performance, although very small but statistically significant effects for Unemployment (+), Need for independence (+)</td>
</tr>
<tr>
<td></td>
<td>Employment (log)</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>Sales (log)</td>
<td></td>
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<td></td>
<td>Perceived profit</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>New economic activity</td>
<td>Human capital Social capital Entrepreneurial motivation</td>
<td>Logistic regression n=4,091</td>
<td>Education (+), Start-up experience (-), Parallel (+), Network (+), Remain small (+), Wanting to grow (+), Willing to employ (-), Equity (-)</td>
</tr>
<tr>
<td>4</td>
<td>Key gestation events</td>
<td>Opportunity search style</td>
<td>Cluster analysis ANOVA n=134-122</td>
<td>Proactive search (BP; marketing, new resources, sales) Reactive search Fortuitous discovery</td>
</tr>
<tr>
<td></td>
<td>Financial performance</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5</td>
<td>Market newness of venture ideas</td>
<td>Opportunity search: Proactive Reactive Fortuitous discovery</td>
<td>ANOVA n=250-134</td>
<td>Search increases market newness. Proactive somewhat more effective than reactive search.</td>
</tr>
<tr>
<td>6</td>
<td>Correlation between indices of market newness</td>
<td>Indicators of market newness</td>
<td>Parametric and non-parametric correlation analyses n=250</td>
<td>Market newness can be measured as a continuous construct. Reflective or formative logic leads to practically identical results.</td>
</tr>
</tbody>
</table>

Table 3.2: A tabulated summary of the independent variables, methods of analysis and results for the included papers.
3.8.2 Methodological comments on Initial conditions as predictors of new venture performance: a replication and extension of the Cooper et al. study

Unlike the other papers in this thesis, this paper is an explicit effort to replicate a previous study (Cooper, Gimeno-Gascon, & Woo, 1994), as far as our data allowed. It therefore seems reasonable to discuss this paper in the light of the original study. The latter is fairly conventional in scope but with a very high quality in terms of sample size, methodology and general attention to detail. The general quality of the study and the very central subject of performance prediction from factors known at start-up made this study a natural choice to select for replication.

Although some operationalizations are slightly different or by proxy, a major difference lies in the sampling of new ventures. In the original study, the sampling frame was constituted by individuals who had recently become members of the US National Federation of Independent Business (NFIB). This sampling frame of 13,000 business owners was actually screened in its entirety with a total response rate of 37%. The final share of valid responses was further reduced to 23% through the exclusion of individuals who had been registered business owners for more than 17 months at the time of the first survey wave. In addition, the researchers apparently spent great efforts to check the representativeness of the sample (Cooper, Dunkelberg, Woo, & Dennis, 1990). Overall, it was deemed an exemplary study well worthy of replication.

Contrasting the methods used by Cooper et al. (1994) and the replication study reveals some important differences regarding the unit of analysis and the associated newness. The original study used the individual as the primary sampling unit. Thus, the firms retained in the sample were not sampled directly but rather through their connection with their owners. In contrast, the replication study created a sampling frame consisting of new ventures, using four different registers that cover different aspect of new economic activity. First, the large business database of Statistics Sweden (formerly Sw. Centrala företags- och arbetsställeregistret, CFAR, now Sw. Företagsdatabasen, FDB) contains all registrations of businesses and establishment. In practical terms, it is a repository that is replenished from other government registers. One of these registers is the business register of the Swedish Patent and Registration Office (Sw. Patent- och registreringsverket) with which all incorporated business and limited partnerships must register. These two registers provide information on the “boundary” dimension of a new venture (cf. Katz & Gartner, 1988), i.e. a registration indicates that the new venture is forming a legal boundary of its own across which there can be exchange. These registers were supplemented with data from the VAT register (Swed. Momsregistret) and as well as data on private income statements collected by the Swedish tax agency. These databases primarily capture the “exchange” aspect of the new ventures (cf. Katz &
METHODS: The feasibility of assessing entrepreneurship as new economic activity

Gartner, 1988): all businesses are required to report VAT and this covers all legal entities and business activities. However, sole proprietors, who account for a large part of the new businesses, are not required to register with any business register at all in advance, not even for tax purposes. Instead, they may simply state income from their business on their annual private income statements. To capture this group, the register of annual private incomes statements was also consulted to detect new economic activity. These three primary registers (Patent and Registration Office, VAT and the database of private income statements) were crosschecked immediately prior to the sampling in order to create a sampling frame that was updated with the latest available information. In summary, the replication study constructs the sampling frame from two dimensions “boundary” and “exchange” (cf. Katz & Gartner, 1988). First, the business registers capture any legal boundary forming in term of new firms and the tax registers complement by tracing new patterns of exchange resulting from new economic activity. Compared with the original study, the described methodology lacks self-selection bias since inclusion in the sampling frame does not require membership or active registration other than reasonably lawful behavior.

Regardless of the level of analysis, the capturing of adequate cases in entrepreneurship research also depends on the assessment of newness. When using business registrations as an indicator of new economic activity, a major threat to validity is the phenomenon of re-registration. A steady-state business that has already been registered once may spawn one or several new business registrations. This can occur from changes in legal form or transfer of ownership, but also for taxation purposes or because of family matters such as succession. Of course, such registrations indicate that something has happened in these firms but they are not necessarily reflections of new economic activity or even purely organizational newness. As was previously stated, 48 % of the annual new business registrations in Sweden are not matched by any new economic activity whatsoever. Apart from self-selection bias that can enter when using registers with voluntary entry, the method of sampling new owners in order to find genuinely new ventures begs the question whether the businesses associated with the new owners are really new. There are several possible combinations in which the individual owner can be new but in which the economic activity is basically an existing business that is already up and running; family succession or a regular purchase/take-over are some of the possible scenarios where the new business owner takes over an established business. For assessment of new economic activity, this not only adds noise to the data but also seriously undermines the validity of the study with respect to analyses carried out at the firm and venture levels. However, sampling on potential entrepreneurial agents (individuals or firms) is an excellent idea if cost is less of a consideration and newness can be contained. In contrast to sampling
directly on the new ventures, sampling on the entrepreneurial agent can allow
the researcher to access the process before the new venture has made its mark in
any public database either by setting up a legal boundary or by creating a
pattern of exchange. This method has been used to good effect in the PSED
study on nascent entrepreneurship (Reynolds, 2000), for which it is probably
the only sampling method that can capture new ventures in their earliest stages
(e.g. “intentionality” and resource acquisition in the model suggested by Katz &
Gartner, 1988). The agent will always exist prior to the birth of a new venture
and sampling frames can be formed without any lag. However, not all potential
agents are actually involved in new ventures and the hit rate will be far lower
than when sampling directly on the new venture or on a new business owner.

An ex post comparison such as this is perhaps somewhat unfair to the
original study. The Cooper et al. paper does not contain explicit claims to
capture new economic activity and have no explicit definitions about what
constitutes a new venture. Overall, it is indeed a very thorough study but it also
serves as an illustration of the limits for accessing new economic activities from
voluntary business registers. Apart from the fact that such registers may not be
complete, they will suffer from self-selection bias and may contain only proxy
sampling units. In the replication study, efforts were made to overcome this
problem by cross-referencing multiple business databases that were compulsory
for all legal business activities.

A screening for newness is also crucial when trying to detect new economic
activity, new firms, or new ventures, and this holds true regardless of whether
one samples on the agent or the entrepreneurial event itself. The original study
by Cooper et al. (1994) used the newness of the business owner’s voluntary
registration as an indicator of organizational newness. However, there are no
safeguards against re-registrations and we can only speculate about the
proportions of greenfield start-ups in that sample. What we do know is that the
owner is new in that particular register. Thus, the original study confuses two
issues in relation to initial conditions. First, we have the question of initial
conditions vis-à-vis the new economic activity or new business organization.
Second, we have the question of initial conditions and a new owner-manager.
These are two separate issues and in our effort to perform a replication, the
choice was to go for the first question, i.e. initial condition in relation to new
economic activity rather than the individual. This is also one of the more
apparent sources for the discrepancies between the two studies.

In general, Sweden has extensive public data on its citizens and their
economic activities, yet it has proved nigh impossible to screen out not-so-new
ventures in the large annual number of business registrations solely through
informed manipulations of existing data. The brute-force strategy to control
this problem is to collect the necessary information through a survey-type
screening in which self-reported measures are used to gauge the newness from
METHODS: The feasibility of assessing entrepreneurship as new economic activity

various perspectives, using the new registrations only as a sampling frame. This was indeed the chosen strategy of Statistics Sweden, aimed at minimizing undue spread in organizational newness when sampling from records of existing businesses. As has already been stated, this type of screening resulted in a halving of the number of valid cases from a sampling frame that used business registrations from a single year.

One of the strong points of panel studies is that factors that do not vary over time (time-invariant) can be contained with standard analytical techniques (Yamaguchi, 1991) even if they are unobserved. The basic explanation is that since these variables are constant across the period of observation, they cannot induce changes in this period. Although the dataset constructed for this study admittedly has some strong relative merits, it is (as usual) hampered by uncontrolled methodological shortcomings. In relation to sampling, the fact that the dataset is a single cohort has left so-called cohort effects, mainly true cohort effects, period effects and age effects, unmitigated and uncontrollable by statistical methods (Menard, 2002).

Although there are no similar panels available for comparison, there are some circumstances that provide at least anecdotal evidence that the cohort used in this and other studies has some idiosyncrasies that could give rise to cohort effects. First, a true cohort effect may be suspected because, in 1994, Sweden experienced a 30% increase in the number of genuinely new enterprises compared with the previous year. This was in part due to a recovery in the business cycle but also to changes in the policies against unemployment. In the case of Sweden, unemployment benefits are immediately suspended if someone unemployed registers a firm in their own name. Given that the employment benefit at the time was approximately 80% of the previous income of the unemployed, the lock-in effect of the unemployment system is quite obvious: if the unemployed adheres to a few simple rules, unemployment benefits are practically inexhaustible and the threshold between a safe, albeit lower income and the uncertain benefits of self-employment is considerable. However, in 1994, a more or less dormant unemployment program to encourage business start-ups was revived. This was aimed at stimulating start-ups by providing the financial equivalent of the unemployment benefits for a period of six months, if the applicant tried to start a business and enrolled in a short course on business start-up. Given the fact that the recession from 1991 through 1993 was the worst in Sweden since the 1930s, there is reason to believe that the human capital of the cohort of 1994 is somewhat atypical, mainly for two reasons. First, the year 1994 was the first year of recovery and many individuals with an interest in starting their own business may have “hibernated” during the hard times in 1991, given the relatively generous unemployment benefits. In addition, the boost of the program of start-up allowances provided extra stimulation for unemployed people to start in that
particular year. So, in the previous years, some may certainly have been
discouraged by the lock-in effect of the Swedish system of unemployment
benefits as well as the dire business prospects that were a direct result of the
ailing demand of a rapidly shrinking economy: Sweden had about 900,000 less
people working in 1993 than in 1990; this on a workforce of approximately 4.5
million.

The literature provides no easy remedy for cohort effects (Menard, 2002).
Alas, the approach of setting up multiple parallel cohorts is as costly as it is
technically elegant and theoretically appealing. With two or more overlapping
cohorts, the cohort effects can be contained and assessed, adding new
dimensions to the analysis. First, true cohort effects (due to the year of “birth”)
are evidently controlled by adding another cohort. Second, the addition of
cohorts during part of the same period also makes it possible to separate period
effects (due to the particular period of observation) and age effect (how “old”
the cases are at specific time points). None of this has been undertaken for the
purpose of this study, which regretfully leaves the field open to speculation as
to how the specific time period has influenced the findings. Again, the fact that
the panel is not replenished and is a true cohort makes all cases experience the
same environmental factors, and time-invariant factors remain fixed for all the
survey waves. It seems reasonable to expect that possible cohort effects may have
had the strongest impact on point estimates for variables such as survival rates
or performance but they may have been less of a problem when estimating
relations between groups and variables, e.g. the relative merits of resources or
motives in relation to hypothesized outcomes. Again, without additional
cohorts we can only speculate on the basis of anecdotal evidence.

3.8.3 Methodological comments on Business start-up reasons
and firm performance

Although not a direct attempt towards replication of a single study, this paper
draws heavily on the empirical findings and theory development from an earlier
collaborative study that resulted in a number of interesting papers (for details
see e.g., Scheinberg & MacMillan, 1988; Shane, Kolvereid, & Westhead,
1991). The reader may already have noted that the theory development in this
paper is somewhat atypical since the major part of the conceptual discussion is
deferred to the methods section. However, the central references include
extensive reviews of the existing body of literature on the topic. It was therefore
concluded that restating these reviews would be somewhat off target and the
present paper has a primary focus on the empirical aspects.

Birley and Westhead (1994) used principal component analysis to collapse
22 indicators of start-up motives into seven latent dimensions of which five
matched those originally presented by Scheinberg and MacMillan (1988). The
slight discrepancies in the structure of the data were attributed to expected
cultural differences between the empirical contexts. As an additional step, Birley
and Westhead used the reduced set of latent motives to cluster cases into six
groups of founder “types”. Finally, they checked for differences across these
groups; results for size or recent growth (as well as aspirations for size and
growth) were not statistically significant.

A major difference between our study and earlier efforts can be found in
the approach to measurement. One could say that our dataset is rich on cases
but poor on variables, a least in the earliest waves. As a consequence, our study
only uses a single-item measures in which the respondent ranks a number of
alternatives and then selects the one with the highest rank. Although the
alternatives arguably belong to the separate dimensions suggested by Birley and
Westhead (1994) and have been extensively tested in their specific empirical
context, this approach has distinct shortcomings in terms of validity and
reliability. The main impetus for constructing an index is to improve
measurement validity and reliability. The use of multiple indicators that have a
high correlation with a latent trait or motive provides a favorable condition for
increasing the construct validity of a scale (Nunnally & Bernstein, 1994). In
addition, using multiple indicators with high mutual correlation mitigates the
effect of random errors in single items that are “diluted” if the scale is an
additive index. In our study, we revert to single-item indicators and this
undermines the validity and reliability of the measures as representative of latent
motives. An additional complication is that our study measures motives using a
single item consisting of the highest-ranking start-up motive. For unbiased
results, this procedure implicitly assumes orthogonality among items. In
hindsight, this assumption is unwarranted. Explorat
ive techniques for assessing
latent variables such as principal component analysis usually “extract” factors
that are mathematically uncorrelated, i.e. statistically independent. This means
that the factors (or latent variables) should be considered as separate
dimensions. However, single items can have considerable correlation with other
items as well as with other factors, which can be observed in the standard
output from statistical packages for principal component analysis or similar
routines. If two alternatives in the rank-order item exhibit considerable
correlation (if they are measured on e.g. separate Likert scales), the influence of
the unobserved alternative (when only the highest-ranking alternative is
recorded) will influence results because of the misspecification introduced by
using only the highest-ranking motive. Although previous studies have placed
the items in orthogonally oriented dimensions, the effects of this
misspecification are unfortunately not available for assessment in this study.

Although the measures we use are clearly inferior to those used by the cited
studies (single alternative rank-order item instead of multiple-indicators scales),
the sampling procedures are not. The large multi-country study (Scheinberg &
MacMillan, 1988) uses a variety of different methods to derive the full dataset while the single-country study in Great Britain (as used by Birley & Westhead, 1994) had new firms as the sampling unit. Obviously, some countries have better data than others but a common problem seems to be to assess the actual newness of the ventures and to control for age. In the British study, the age of the firms varies from one to six years and the organizational newness of the ventures is not systematically assessed. Further, the lack of any comprehensive list of new firms in Great Britain at the time puts the representativeness in question, although the researchers have admittedly gone to great lengths to ensure that this would not be the case (see e.g. Shane et al., 1991).

From a causality perspective, an additional problem is that the surveys used in the earlier studies are entirely retrospective (Menard, 2002), as independent and dependent variables are collected at a single point in time, even though the independent variables are supposed to influence the dependent variables in a process across time. This may not matter much regarding the structure of the motives but when assessing the causal relationship between motives and performance it becomes a confusing factor in terms of causality that remains uncontained with that particular research design. A challenging hypothesis could be set up along the lines that since data on the motives are collected in retrospect, they may very well be influenced by the performance of the venture (cf. Delmar & Wiklund, 2003 for an example on growth and motivation). The earlier studies use retrospection over a time span of up to six years, which is clearly a potential problem, but the large span in age also introduces selection bias since only surviving firms can be queried.

Some of these problems are solved with the design used in our study. First, the existence of a comprehensive sampling frame with minimum delay allowed us to construct a panel of the same age to control for differences due to process and the circumstances surrounding the start-up (in the environment). In addition, we checked that the new ventures were greenfield start-ups and not take-overs, which might otherwise influence size and other measures of performance. Further, data on the start-up motives were collected with a maximum delay of one year after the start-up, and performance data were collected three years after the start-up. This was done to minimize retrospection and survival bias as well as to strengthen any argument for causal links between start-up motives and performance.

When findings from the present study are juxtaposed with those of the cited papers, it seems that there is a) an agreement that there are 5-7 dominating broad start-up motives that are valid in many countries, but b) that these motives matter little in terms of size, growth, aspirations and performance. Although neither of these studies is flawless in terms of design, the lack of statistically significant results linking start-up motives and performance corroborates the conclusion that if the link between start-up motive and
performance does exist, it is certainly rather weak. It could be argued that since all of these studies have noise in the data, each in their own particular way, a better design overall might change these findings. This may possibly be the case, but there are some compelling arguments against it. Empirically, the absence of large effect sizes, regardless of performance measure, speaks against potentially significant effects masked by noise. Had they existed, it is reasonable to expect them to have surfaced by now. Moreover, regardless of the empirical evidence, the theoretical link between start-up motives and performance is markedly weak. It is natural to expect start-up motives to influence the start-up proper, e.g. the probability of a start-up or the initial conditions of the new ventures. However, it is not obvious that motives for starting up a new venture would necessarily influence performance, since performance is only one of many goals of enterprising individuals. In hindsight, the lack of solid theoretical underpinnings of the causal link between start-up motives and new venture performance is overall a stronger argument against further pursuing this line of inquiry than is the lack of empirical support in the studies that have looked at this issue so far.

Finally, some comments on the connection between level of analysis and sampling unit. In the case of relating psychological motives to venture performance, two levels of analysis are possible: the individual and the venture/firm. However, sampling is a separate issue. If we sample individuals, some will be involved in starting a new venture at any given time. Since no independent ventures will be started without people involved, we can safely assume that the ventures that are “attached” to the enterprising individuals are an unbiased sample of the populations of new ventures under way. If we instead sample on business registrations to find new ventures, these will have some enterprising individual(s) “attached” to them. Thus, under ideal circumstances, the two target populations are in a fixed ratio and sampling is possible on both units regardless of the unit of analysis that will be used later. However, from a practical viewpoint, many of the previously presented arguments for and against the applied method are valid here. Sampling on the entrepreneurial agent is generally a sound approach for studies with enough financial resources to allow this and when the ventures are screened for newness and process time.

Speaking for sampling on registrations is the completeness with which a sampling frame could be constructed in this empirical setting but even if the registers on new business registrations are fairly complete, the lag can be substantial. Sampling on the individual (or some other entrepreneurial agent) seems to be the only way to avoid retrospection and survival bias in studies where it is imperative to avoid truncation of the observed process time, such as in the PSED studies (Reynolds, 2000). Again, the problem of sampling on individuals is that the hit rate is markedly lower (about one tenth in Swedish studies) than for sampling on firms (Dahlqvist, Davidsson, & Wiklund, 2000; Samuelsson, 2004).
3.8.4 Methodological comments on Entrepreneurship as new business activity: Empirical evidence from young firms

One of the main contributions of the previous papers lies in the efforts made to sample firms that exclusively contain new economic activities; this was done in order to appropriately assess the consequences of entrepreneurship. This third paper, however, concerns the prediction of new economic activity itself in a panel of firms, i.e. the venture is no longer the sampling unit per se but rather the dependent variable. Using the same panel six years on, the original new ventures are now more appropriately conceptualized as a cohort of young firms. The research area concerned with established firms engaging in new economic activity is usually labeled “corporate entrepreneurship”. Given the earlier discussion on the entrepreneurial event, the important difference between independent and corporate entrepreneurship can be reduced to agency: who is the entrepreneur? When studying entrepreneurship as the origin of new economic activity, this issue needs extra consideration. In this study, the propensity to create new economic activity is related to attributes of the owner-manager of the firm, who is also free to act outside this firm. A major methodological challenge in this paper is thus to capture different ways in which a firm’s owner-manager can be involved in creating new economic activity. Starting a new venture that is legally independent from the existing firm is one possibility, and in that case the agent is reasonably not the existing firm but rather its owner-manager as an individual (alone or as part of a team). A second possibility is an internal venture in which the new economic activity coexists with the ongoing activities of the focal firm. To qualify as a new economic activity, a new internal venture needs to exhibit newness in relation to the firm. Even though the new venture may be lacking in novelty vis-à-vis the market, this is primarily a question of degree. However, if the host firm of the new venture simply carries on doing exactly the same, this is pure expansion of existing activities rather than entrepreneurship: some threshold degree of newness to the firm needs to be involved. In this study, two measures were used for internal ventures. To capture historic behavior, a retrospective question [sic] was used regarding the fact whether or not the firm had previously been involved in new internal ventures. Besides, an item concerning any ongoing project was added to capture the present situation. The final dependent variable was an ordinal binary variable of having either one or more of the three indicators (ongoing internal venture; previous internal venture; parallel start-up) or none at all. Even though the introduction of new economic activity was defined this broadly, no more than a third of the remaining panel was included in the group that had actually engaged in new economic activities after their start-up six years earlier.
METHODS: The feasibility of assessing entrepreneurship as new economic activity

Trying to capture new economic activity this broadly is a daunting task and the credibility of the findings perhaps calls for a small leap of faith. One obvious problem is the self-reported measures used in this study. It is not so much a question of subjectivity but rather a lack of omniscience on the part of the respondents. Although the majority of the firms in the panel are very small, it cannot be ruled out that influential cases actually have team management and that the respondent has not been aware of everything going on in the economic sphere of interest. We have no information on this, which may be regarded as a general criticism of the database as a whole, which relies heavily on self-report. For this type of research, in which the required data is not produced for any other purpose or where some degree of judgment is required, there are few alternatives to self-report in a survey. Of course, some remedial measures are available to increase validity and reliability. One such measure is to supplement with independent data even though it might be on a proxy level. The use of such data can be checked for validity and reliability using standard statistical techniques. Another strategy is to add more respondents from each firm or venture to collect self-report data from multiple sources. The basic idea is much like geometrical triangulation, and the reliability of the data can be checked by assessing the correspondence between data from multiple respondents that are supposed to cover the same factual circumstances (known as interrater reliability). In the database used for the six included papers, none of these strategies has been applied except for turnover, which was collected both from the respondents and from the tax agency’s database. Overall, this situation is less than ideal.

However, it seems that reliability is more at risk than validity in most cases. In a quantitative context such as the present studies, reliability may be expressed as resistance against random deviations. Lack of reliability signifies that a method picks up noise data. This results in attenuation of effect sizes and decreases the statistical power of common tests of significance. For the studies based on the whole dataset (notably papers nos.1, 2, and 3), it is fortunate that the number of cases is large enough to counter any effects from undue noise; the large number of degrees of freedom thus obtained tend to counter the detrimental effects of noise on statistical power. As for validity, it is hard to say anything definite due to the lack of corroborating data, which leaves this matter open to speculation. For the paper presently discussed, one theoretically possible source of bias is size. The respondents are usually the owner, the CEO, the manager or someone which similar insights. However, it does not seem too far-fetched to suspect that as a firm gets larger, managers tend to underestimate the extent of new economic activity in (and in proximity to) the focal firm because of lack of omniscience. Although firm size can be entered as a control variable, the interpretation is ambiguous since there are two mechanisms at work in the same variable: the level of new economic activity (which could be expected to
increase with increasing firm size) and the level of information held by the respondents. While the problem mentioned above is indeed a possible source of bias, it is not very likely to influence the results in any major way given that the mean number of employees is less than 2 in the actual sample.

3.8.5 Methodological comments on Opportunity recognition processes: A taxonomy and outcome implications

In the remaining three papers, the entrepreneurial agent is the firm. Sampled ventures are internal or fully owned, at least in their initial phase. In small firms, however, it may be difficult or even futile to uphold the delineation between individuals and firms on an empirical level, especially when using self-reported measures. The concluding three papers in this thesis also share the same subsample of new ventures “harvested” from the Cohort 94 panel. The sampling procedures have been detailed previously in this introduction and in the paper proper. This first paper in the set includes analysis on two distinct levels. First, the venturing individuals connected to the host firm are mapped with regard to their opportunity-seeking style. Second, these search strategies are related to various outcomes, measured on the level of the new venture. The challenge in methodological design is to keep separate the firm, the venture, and the individual as levels of analysis and to make sure that data are collected at the appropriate level. In this study, the opportunity-seeking styles were conceived as taking place at the firm level, which in this case is the entrepreneurial agent, and outcomes measured on the venture level. As a practical detail, each new venture was given a code name that was consistently referred to throughout the survey in order to distinguish the new venture from the host firm.

One problem during the collection of data was that the questionnaire for tapping opportunity-seeking styles was not available until approximately six months after the initial screening of the panel. This is an example of a (potential) problem of retrospection, which this author has previously criticized in this introduction. Although it is impossible to eliminate the suspicion of retrospection bias, there are indeed some mitigating circumstances. First, the time gap was fairly short, or one six-month spell. Although perhaps not a valid argument in itself, it could be noted for reference that this period for retrospection (six months) is on par with the best available data on independent entrepreneurship (referring to PSED which is described in Reynolds, 2000; Samuelsson, 2004). Second, the data collected seem to be fairly uncontroversial from a social desirability perspective and is it is not apparent that it should be influenced by the developmental process of the venture under study. Finally, outcome data were collect in three separate surveys after the data on opportunity-seeking style had been collected. This means that even though
METHODS: The feasibility of assessing entrepreneurship as new economic activity

there could conceivably exist some retrospection bias, the causality of the findings cannot be reversed.

Collecting data on both the firm and venture level requires forethought, but in this author’s view the empirical problems are more difficult to overcome than the purely conceptual ones. The very nature of venturing implies process and change, and this means that as process time goes on, the unit of analysis may change its empirical appearance. When a new venture is purely internal and shows some degree of unrelatedness to the existing activities of the host firm, following progress at the venture level appears to be empirically straightforward. However, if the new venture shows progress, several situations can emerge in which it can be challenging to separate the firm and the new venture, both conceptually and empirically. In some cases in the data, the new venture becomes a separate firm but in other cases it becomes the firm; the scope of the host firm changes until a point is reached where the resources used by the original activities are completely transferred to those of the new venture. In these cases, there is, for instance, no longer any base for calculating the impact of the new venture on the original firm’s performance since the host firm and the new venture are no longer separable. Nevertheless, it is still possible to follow the progress of the venture in terms of absolute performance.

Another problem of a purely empirical nature is the case where the new venture (or perhaps only the venture idea) is sold to another company. Following the venture is still possible if access to the new host firm can be established, but the relation between the venturing firm and the venture changes in the midst of the venturing process. In these cases (<20 in the subsample of new internal ventures), it proved difficult to enroll the new host firm in the research program. Conceptually, it may be that the original venture simply ceased to exist, although market aspects of the venture idea may still be pursued by other agents.

3.8.6 Methodological comments on Patterns of search and the newness of venture ideas

This paper investigates the relation between different styles of opportunity search and the relative market newness of the resulting ventures. Again, this is an example of a two-level analysis of data at agent and venture levels. As in the previous paper, the taxonomy for opportunity search was derived from a set of statements given by the respondents of the host firms. This search, or lack of it, is thought to influence the market newness of venture ideas. Search brings the entrepreneurial agent into contact with pieces of information that can be used for crafting new ideas for business, and these ideas are embodied in specific ventures. To assess the implied relationship, we thus need to assess the search strategies of the agent, in this case the firm, and the newness of the ventures.
pursued. In this study, the new ventures are sampled on their agents, i.e. the host firms for new internal ventures, and this adds special considerations to this inquiry. First, the search strategies that a firm follows at any given point in time cannot be related to the firm in its entirety. While the measurement of search strategies could be thought of as a snapshot of present conditions, the overall innovativeness of a firm is a cumulated deposit of historical behaviors (or at least it is usually measured as such). Even though objective historical data including product introduction, product innovativeness, or patents (as a precursor to innovation) are possible indicators, it is the wrong way around to construe present strategies as the cause of historical success. Since present strategies are supposed to influence present and future venture ideas, we need a method that makes new ventures traceable and facilitates recording process and performance data at this level of analysis from and after the point in time at which search strategies are measured. This is an argument in favor of the real-time panel design, simply because it makes sense from a perspective of causality. It is certainly an embarrassing fact that the collection of data on search strategies was delayed for approximately six months after the items on newness were collected. To reiterate what was stated in the previous commentary, the damage done by this admittedly backward timeline in the data collection is mitigated by some favorable circumstances: *prima facie* the information seems to be reasonably uncontroversial in terms of social desirability, and the delay is only a single spell (of six months). Nevertheless, for this particular combination of items, the data is best characterized as retrospective longitudinal (Menard, 2002) or even cross-sectional.

The final paper in the thesis is a methodological complement on some of the measurement issues in paper no. 5. The paper does not add new empirical data but is an effort to validate the instrument used for measuring market newness. As it is largely a paper on methodology and has been written explicitly for this thesis, it would seem redundant to restate its arguments in a separate commentary.

### 3.8.7 Concluding remarks on the methodological approach

Part of the data on the *Cohort '94* panel of firms should be considered as secondary data since the screening methodology and the data collection were elaborated by Statistics Sweden. This point is valid for the first survey wave in 1995 as well as for part of the second wave in 1998, after which the author and associates could exert a more substantial influence on the data collection procedures and item development. Therefore, the decision to use this dataset was partly a matter of selection rather than design. In addition, it was also a case of acting opportunistically under favorable circumstances of chance. Nevertheless, it has hopefully been shown to the reader that for the purpose of
METHODS: The feasibility of assessing entrepreneurship as new economic activity

capturing new economic activity in independent ventures, the sampling methods used for this panel is in line with the theoretical arguments stated earlier in this introduction.

The very fact that the dataset used for this thesis was actually put together using survey methodology shows that empirical inquiries on the level of new economic activity are indeed feasible. Whether the reader finds it worthwhile is another story. Although the estimated financial cost of producing this relatively large dataset is more than SEK 3 million (or approximately USD 400,000) over a period of eight years (not counting researchers’ time for item development, etc.), the amount of data is very large, owing to high response rates and low attrition.

Even though the dataset was originally set up for other purposes, the data collection procedures captured independent ventures that were “new to firm” by definition and that created “new market offer”. This is enough to give indications of newness “to firm” and “to market”, which are important attributes in several definitions of entrepreneurship (such as Davidsson, 2003). However, the instruments used for the original screening in 1995 did not contain anything similar to the more elaborate scheme used to gauge the newness of new internal ventures in the subsample of 250 firms taken in the year 2000. To remind the reader, about half of the respondents who acknowledged that they had a new internal venture in gestation in the first screening by mail, were not qualified after the second screening by phone. However, the amount of effort that went into the latter study suggests that a two-step screening by mail and by telephone does not appear suitable for large-scale screenings of new independent ventures, mainly because of the large additional costs involved. Moreover, for the sake of information economy, the collection of this type of data only makes good sense in a longitudinal research setting where data is used to discover relations across time. Although general-purpose follow-up surveys have sometimes been undertaken in Sweden, this has been done on an ad hoc basis and only for obtaining point estimates or providing simple tables.

The study of new internal ventures did, however, have an instrument that allowed us to gauge venture newness more precisely and the conclusions drawn from papers four through six are that these indicators are at least adequate. The indicators used in the screening of new internal ventures have been shown to have reasonable statistical and theoretical backing as well as being meaningfully related to factors that could be construed as antecedents (search for opportunity) or as consequences (timing of various gestation behaviors and performance) to newness. In technical terms, the instrument has been shown to have content, construct, and predictive validity (Cronbach & Meehl, 1955; Nunnally & Bernstein, 1994). However, item generation and validation was
nowhere near the lengthy process usually prescribed (and insisted on) in more mature fields of research (Hinkin, 1995; Nunnally & Bernstein, 1994).

In addition, the indicators (or determinants) of market newness are based on respondents’ perceptions and are formally not objective measures. Although this may or may not matter depending on the research questions at hand, a supplement of independent measures would be a welcome development. A couple of such measures were indeed included in the survey of new internal ventures, e.g. various intellectual property rights (patents and copyrights). However, the low proportion of firms having these resources makes statistical analysis less meaningful because of the few cases and the associated low statistical power. Moreover, although some resource variables such as patents may be objective, they are at best proxy indicators of newness. Given the small share of new ventures that exhibited these indicators of newness, objective measures that can be used for gauging medium to low levels of venture newness would be a valuable line of development in order to construct more valid and reliable scales for measuring newness in entrepreneurship research.
4 CONCLUSIONS ON ASSESSING ENTREPRENEURSHIP

4.1 Introduction

By now it should be no secret to the reader that this thesis is about assessing entrepreneurship. A major part of this endeavor was to define and operationalize new economic activity as an appropriate level of analysis in the study of entrepreneurship. I suspect that many previous studies in this field would have adopted it had it only been practically feasible: many scholars of entrepreneurship in management have their fundamental interest in what goes on beyond the façade of the “firm” in terms of human action and resource deployment.

For clarity of exposition, we may separate the entrepreneurial process into two components, a) the entrepreneurial agent, and b) the new economic activity introduced. Drawing on the established distinction between independent and corporate entrepreneurship, the agent is either an individual entrepreneur or a firm. The partitioning of entrepreneurial agency and process has proven fruitful for several issues covered in this thesis and illustrates the largely superficial divide between corporate and independent entrepreneurship. While the agents differ, the entrepreneurial event is the same, i.e. the introduction of new economic activity. In the previous chapter, the issue of sampling was examined and clarity was enhanced by this mode of analysis.

The choice of level of analysis is intertwined with questions of what aspects to assess and from what perspective, once we have data on appropriate units of analysis. The latter problem could be called the “substance” aspect of assessing entrepreneurship. Some of the papers included draw on traditional measures used in entrepreneurship, mostly in the form of performance measures broadly defined. In these instances, it is the new combination of traditional measures and a novel level of analysis that makes up the contribution of this thesis. Not only may results be different prima fascie but also the interpretations of the results. In some papers, notably nos. 4, 5 and 6, the measures themselves are

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12 That the measures have been used in other studies does not imply that they are measured in a traditional fashion. The real-time longitudinal characteristics of the dataset are rare to the field and a distinct contribution of this thesis.
new. In these specific cases, the use of new economic activity as a unit of analysis is a prerequisite for the measures. In particular, the opportunity-search styles presented in papers nos. 4 and 5 only make sense when related to consequences at the venture level, as do the indicators of newness introduced in paper no. 5 (and further developed in paper no. 6). The rest of this chapter will dwell on the overall conclusions drawn from the theoretical introduction and the papers. Again, the two problems to be addressed are those of level of analysis (which is a common theme of all the papers) and the substantial issues dealt with in the individual papers.

4.2 Considerations in sampling

Sampling in entrepreneurship research can be done either on the entrepreneurial agent (individual or firm) or on the new economic activity in its empirical form of independent or corporate ventures. Sampling on the agent has the advantage of giving the researcher a chance to minimize left-hand censoring of the process, i.e. picking up ventures at a very early stage to avoid process time to elapse unobserved. This feature was expressly taken advantage of in the US and Swedish PSED studies in which new venture creation was followed from the very early stages of ideation (Reynolds, 2000; Samuelsson, 2004). In the present thesis, this design was used for capturing new internal ventures in the then six-year-old cohort of existing firms. Although this method will pick up new ventures at various points of process time, there is no inherent limit as to how early the study may intercept the entrepreneurial process.

In contrast, sampling on the new economic activity (independent or corporate ventures) usually introduces a lag, because before they appear in databases that can be used for constructing sampling frames, considerable process time may have elapsed: the new ventures are now clearly operating with structural autonomy and external exchange. However, a more serious problem is the fact that new internal ventures are not available to sampling at all, at least not by standard procedures of creating sampling frames from some sort of database. The only way to access this important vehicle of entrepreneurial activity is to sample on the agent, i.e. the host firm. However, empirically it has proven considerably more cost effective to sample directly on the new economic activity granted that problems of selection bias can be eliminated or at least mitigated. The pick-up ratio between sampling on the agent or on the new economic activity is about 1 to 10 when comparing the Swedish PSED with the sampling design used to create the Cohort '94 panel. However, there are additional issues of bias that may apply. In the Swedish setting, access to and quality of databases available to researchers are very good regarding both individuals and businesses, stemming from a long tradition of careful monitoring of the population for various purposes of government, notably tax
and military services. Thus, sampling frames can be constructed with good accuracy for entrepreneurial agents as well as for new ventures if the unavoidable initial delay can be accepted. In other empirical settings, this may, however, not be the case. In particular, business registers may not be as well updated or as accessible and a review of the sampling designs used in the literature certainly gives this impression. It might even be the case that a majority of empirical settings requires sampling on the agents as the only way to secure adequate coverage of the target populations of new economic activities.

4.3 Assessing performance

Tracing the concept of “performance” in the lists of references of management literature takes us back to the early 1980s and a flurry of articles on organizational efficiency. The then prevalent discussions on concepts such as “stakeholders” and “multiple constituents” on organizational efficiency can be summed up in the question “For whom are we measuring and evaluating organizational performance?” But this is actually two different questions rolled into one. The first question concerns the measuring of organizational efficiency from different points of view depending on who stands to benefit from the operations of the organization (Connolly, Conlon, & Deutsch, 1980; Zammuto, 1984). The idea of the firm as a coalition of interests has a long tradition in management (Barnard, 1938; Cyert & March, 1992/1963), which is reflected in the increased awareness about managing all stakeholders’ interests in the firm, including consumers, co-workers, executives, owners and the community at large (Freeman, 1984). For example, if we wanted to measure what co-workers receive from the firm, we would turn to wages and intrinsic job satisfaction while the owners’ perspective would focus on (but not necessarily be limited to) different measures of the residual such as profit and return measures. However, measuring is not the only part of the organizational performance puzzle. Implicitly, the question is not only what the performance of an organization is but also how good it is (Connolly et al., 1980), that is, what is the relative performance of an organization? In this case, we not only measure performance from different perspectives, we may also evaluate and pass a judgment on it from different perspectives. Given that there are multiple stakeholders holding multiple views, there seems to be little hope of finding one single measure of organizational performance.

One way out of this dilemma is to split the performance measure into two basic categories. The first category would be the traditional financial measures such as various profit margins and return on sales, while the second category would pertain to alternative measures of operational (non-financial) performance (Venkatraman & Ramanujam, 1986). However, the traditional owner’s perspective may not be such a bad candidate for an overall measure of
organizational performance after all even from a social perspective. The
different inputs used in a firm are often priced on factor markets; there are labor
markets, capital markets, and markets for equipment and raw materials.
Assuming efficient markets, factors of production will be paid their market
price, what they are “worth”. From a resource-based perspective, we may also
say that the holders of productive resources used by the firm will receive all of
the rent accruing to these resources (Wernerfelt, 1984). What is left when all
the resources have been compensated is the residual or “profit”, accruing to the
owner of the firm. Part of this residual will be rent on equity but any amount
above the average would be economic rent from the operations of the
organization. Given the assumption of efficient factor markets, there are then
good reasons to use the classical financial performance measures even though
they seem to take purely an owner’s perspective on performance. There may
very well be cases where the providers of input such as labor can receive more
that their equitable share of the rents accruing to the organization. However,
this cannot be expected to be a general trend; for the average of all firms, the
residual is thus an adequate overall measure of organizational performance.

Similar reasoning provides a basis for the use of survival as a measure of
organizational performance. What keeps a firm together (or an individual
going) is whether the benefits of interacting within the boundaries of the firm
are larger than the disadvantages. The existence of alternatives outside the firm
sets a limit when it is advantageous to continue and when to abort operations.
Individuals will usually have outside option of some sort and resources may
have alternative uses. Psychological bias aside, the fact that a particular firm is a
going concern means that a particular constellation of productive resources and
market opportunity is optimal or at least worthwhile. The idea that a business
idea is “dead” is rarely based on zero benefit to society from the product or
service provided, but on the fact that for most types of resource bundles that
could produce the goods or services in question there are actually alternatives
that are more worthwhile to engage in. Again, the matching of resources and
opportunity is the key issue (Andrews, 1971).

Using a stakeholder (coalition; multiple constituents) approach, the actual
performance measures used in this thesis cut across several groups. In some
cases, amalgamations are used which, in hindsight, may seem like an undue mix
of interests. In Initial conditions as predictors of new venture performance: a
replication and extension of the Cooper et al. study (paper no. 1), firm
performance is measured on a three-level ordinal scale with the categories
“failure”, “marginal survival” and “high performance”. The basic dichotomy
between “failure” and “survival” separates the lowest category from the other
two. Naturally, other performance data were lacking on the firms that were no
longer active at the time when performance measures were collected and this is
another reason why “survival” is a useful performance measures since all valid
CONCLUSIONS ON ASSESSING ENTREPRENEURSHIP: Assessing performance

cases have data on this variable. The distinction between “marginal survival” and “high performance” is based on three measures. Either the firm had 1) at least the equivalent of two full-time employees, or 2) minimum sales of 1 MSEK, or 3) were perceived by the founder/manager respondent as having “very high performance” and “providing well for me”. Measuring the number of employees is one of many indicators of the economic importance of a firm or venture. Although it is a measure of the input side, people have arguably chosen to participate in a productive coalition that they find worthwhile compared with outside options. The second measure, sales, is an indicator of market offer. Although value added would perhaps be a more precise indicator of the firm’s unique contribution, sales figures are more commonly available and could reasonably be used as a proxy if controlled for industry. However, it is clearly another perspective on performance than employment although they may correlate, at least within industries using the same technology of production (yielding similar ratios of output/input). Yet, innovation works by changing these input – output relations and substituting output for input measures for the sake of availability may be inappropriate in studies of ventures that display a potential for Schumpeterian innovation. That is, if we consider industry input/output ratios as fixed, there would be little point in studying entrepreneurship in the first place. The measures used in paper no. 1 also include subjective estimates of performance from the owner-manager. Although subjective and self-reported, they clearly tap a relevant aspect of performance in relation to the “survival” of the firm. Since it is clear that the owner-managers themselves are fairly satisfied with the performance of the firm (in the highest performance category), this is implicitly a comparison with outside options (alternative use of resources) that Venkataraman (1997) suggested as a main concern of entrepreneurship research contrary to the relative performance of the strategy field.

Admittedly, several views on performance are mixed in the described compound measure of performance. However, in cases where stakeholder remuneration is unfavorable from one perspective, other perspectives may find it to be quite positive. It is not self-evident that the assessment of the performance should be either/or. An alternative to compound measures would be to enumerate explicitly the stakeholders or resources that are to receive some remuneration from the new venture and to evaluate performance separately from each perspective. In this manner, the perspectives involved are clearly articulated and ease of interpretation is enhanced by not amalgamating dissimilar measures of performance.

On reflection, the term “performance” seems to downplay the difference between performance indicators and performance determinants. Indicators are empirical measures that are supposed to be influenced by an unobserved (latent) variable. Thus, the indicators correlate with the latent variable, and there is an
explicit direction of causality from the latent towards the observed variables. Viewing multiple measures as indicators, implicitly or explicitly, thus assumes a common latent construct, and covariance-based techniques should be used to assess underlying commonalities between observed variables used as indicators of the common latent variable. However, if the covariance structure does not support such interpretations of data, compound measures such as indexes are actually determinants of performance. That is, the measures define the latent construct and are identical to it. Discussing determinants as if they were indicators confuses the issue and blurs the fact that determinants are not derived from data but from theory. To some extent, this problem is present in paper no. 1, where the construction of the performance measure is a somewhat unmeditated choice with potentially "political" consequences as it actually constructs what is performance in this context. There is an implicit evaluation of outcomes in the ordinal nature of the scale and for the highest level of performance, three different outcomes are put on par.

In Business start-up reasons and firm performance (paper no. 2), similar performance measures are used but they are not compounded into a common scale and are evaluated separately. First, "survival" and "failure" are assessed while employment level, sales, and perceived profit are used for the "surviving" part of the sample. However, in the ensuing discussion, the perspectives again tend to blur behind a veil of "performance". Partly, this is due to a lack of significant findings: no further explanations were necessary and the issue is not probed in depth in this paper.

A general comment on papers nos. 1 and no. 2 is the use of "survival" to denote firms in the panel that are still active going concerns. Survival is a term associated with the legacy of ecology and the Darwinian idea of the "survival of the fittest". As has been explained above, this metaphor may point too strongly towards explanations sought in the product or service provided. It also leaves out time. When the panel was established, we may cautiously assume that there was identity between new firm and new venture. However, as time passes on, some activities of the firm may have changed and new internal ventures could have added new economic activity in the firm and changed the original resource combination. Thus, what economic activities that are actually "surviving" has not been exactly determined, as exemplified in papers nos. 1 and 2. Some new internal ventures may have been abandoned for others that better satisfied the constituents of the firm. In such cases, the method used will overestimate the level of activity of the original ventures founded in 1994 since present activities are attributed to historical facts. Although some controls were actually used to mitigate this problem (such as asking whether the firm "continues as before"), the mechanism of the bias is clear. However, the problem of estimating "survival" may be regarded as a special case of separating "the firm" from "the venture". In greenfield start-ups, assumption of identity between the firm and
the venture seems to cause few problems. Still, when the activities of the firm start to change, the option to view the firm as a one-venture organization becomes increasingly problematic and there seems to be no remedy. The firm and the venture are two distinct levels of analysis. The former is the unit preferred by the strategy field and economics, the latter is a level of analysis particular to the field of entrepreneurship (defined as the study of new economic activity). This thesis has, to the extent possible, aimed at the venture level for analysis of performance and process. However, the largest obstacle to this goal is when time is involved. For example, some of the basic requirements for assessing causality in a context of quantified data is correlation and correct temporal order (cause preceding effect). Therefore, the designs used in this study are real-time longitudinal. But when we let time pass, the firm in which we assume that the firm-venture identity has evolved so that when we measure outcomes, we may be actually be assessing something else. Although it would definitely be premature to dismiss the venture as viable level of analysis for future studies, there seems to be no straightforward fix for this problem as it is inherent to the dynamic nature of business life itself.

In the paper *Opportunity recognition processes: A taxonomy and outcome implications* (paper no. 4) the assessment of entrepreneurship concerns “outcome” variables rather than “performance” variables. The assessed outcomes come in two flavors. First, we have key gestation behaviors, i.e., important activities that are typically undertaken when a new economic activity is initiated. These gestation behaviors include such activities as making a business plan, talking to customers, acquiring equipment or making the first sale. Second, some of the measured outcomes could perhaps be construed as performance variables (number of people working in new venture, venture sales and venture profit). However, the nature of the activities in the host firm is largely left out of the analysis. Thus, by not making any assumptions about whether or not the existing firm is identical to the new venture it once was, we sidestep the problem of identity due to the change over time in a firm’s business activities. However, the problem is not really solved. Rather, it has shifted to the new internal venture. However, in relation to the earlier development of the data set, the time between survey waves were only six months and information on the process were collected in each wave. In hindsight, the areas which was least covered was the venture idea. Although the respondents were systematically referred to a specific venture code name, there were no further controls that the new venture was actually pursuing a constant venture idea. Rather, the iterative process of trial-and-error could reasonably be expected to
influence the venture idea over time\textsuperscript{13}. Overall, the obstacles against tracing ventures ideas over time multiply as the process unfolds, but hopefully this thesis has shown that it may be worth the extra efforts to try it. This author, for one, certainly thinks so.

\textsuperscript{13} The changes in venture idea may come endogenously from the creative process of venturing as the entrepreneur discovers new information about possible combination. But change can also come exogenously from time. Windows of opportunity may open and close without any interference from the endogenous process of venturing.
5 CONCLUSIONS

5.1 Summing up

The previous chapter was primarily concerned with the methodological challenges associated with the assessment of new economic activity. In this penultimate chapter, we focus on what was found rather than how we found it. Among the six papers, there is a line of increasing detachment with previous research; the first paper is an outright effort to replicate an earlier study while the last three papers are treading on ground that is only just being populated by existing research. This is not to say that the results are of increasing importance, but that the discussion of the results will vary in character depending on the degree of connection with previous empirical results. The following bullet list is a brief reminder of the central conclusions drawn in the included papers:

- Predicting the performance of new ventures from factors known at start-up is a genuinely difficult task. This is particularly true on the low part of the performance scale, i.e. when trying to distinguish closure from marginal survival. This may to a large extent be contributed to unobserved variance in alternatives outside the focal firm, including the availability of salaried jobs (Gimeno et al., 1997).
- Our inquiries confirm previous results that human and financial capital increases the probability of survival and growth. In addition, financial capital also increases the probability of high performance.
- However, the association between performance and its predictors is not a linear function. Predictors of high performance are partly different from those predicting marginal survival (when compared to closure).
- Having a parental role model in enterprising and having previous start-up experience have opposite effects on marginal survival (Cooper et al., 1994; Dahlqvist, Davidsson, & Wiklund, 1999). While the former includes aspects of the choice of life style, previous start-up experience puts the entrepreneur in a better position to evaluate the real options involved during the “honeymoon” time of the new venture (Choi & Shanley, 2000). Thus, previous business experience may lead to either persistence or opting out by the entrepreneur depending on context and operationalizations.
- Start-up motives of the entrepreneur matters little in terms of predicting subsequent venture performance. This conclusion is
supported by the hitherto meager empirical results as well as theory; while the theoretical link between start-up motives and start-ups is rather obvious, the arguments for a motives – performance link are less convincing.

- Factors associated with an increased propensity to start new independent firms, such as the human capital and social capital of the owner/manager, are also associated with the probability of new economic activity in existing young firms. This suggests that models from independent start-up can be meaningfully applied to explain corporate entrepreneurship in small and/or young firms.

- The search for opportunity in existing firms may be categorized into three types, or styles: proactive search, reactive search and fortuitous discovery. This classification was derived from our empirical data while being well supported by theoretical models of search.

- Type of search for opportunity is associated with the market newness of the venture ideas that young existing firms discover and pursue. Our results show that search is associated with a higher degree of perceived market newness, and proactive search more so than reactive search. A marked characteristic of fortuitous discovery of venture ideas is the high variance in perceived market newness found in this group.

- Type of search for opportunity also has an impact on the subsequent exploitation process and key gestation behaviors in the resulting new venture. Opportunity found through search, typically results in a more rapid process in terms of key gestation behaviors, but differences tend to even out over time. This indicates differences in process speed but no systematic differences in ultimate performance.

- The newness of venture ideas can successfully be modeled as a continuous construct. Leaving dichotomous models (e.g. reproduction or innovation) puts more demand on the range and type of data that is required for analysis, but improvements can be had with fairly coarse scales as long as they provide reasonable range as to provide a approximation of true continuous measures.

5.2 Start-ups and performance

Paper no. 1 (Dahlqvist et al., 1999) is an illustration of how replication can be a worthwhile task in the social sciences. Our study partly confirms the results of the original study, but also adds something of its own by virtue of extension. Some of the differences in the results are most certainly due to differences in operationalizations, data collection and empirical context, but the basic notion that initial resource endowment enhances performance is corroborated. Being better off from the start seems to make a difference even five years after the
start-up. Moreover, a general observation is that the results are markedly in harmony given the differences in methodology and empirical context.

One result from our extension of the study is that access to markets was associated with performance quite along the lines suggested by our hypotheses. Traditionally, infrastructure investments seem mainly to have been concerned with access to labor, i.e. to enhance the function of local labor markets by cutting down travel time. However, enhanced access to markets also works in the reverse direction when firms search for customers, be it other firms or consumers. This conclusion may be relevant to policy-making in states where government incentive programs are fairly common and where entrepreneurship is touted as something of a panacea for unemployment and prosperity in communities outside the metropolitan areas. Entrepreneurship depends on the availability of profitable opportunity and the provision of such opportunity should be considered an alternative to policies that tries to spur business startups that are based on venture ideas for which there simply is no underlying long-term opportunity.

Models for prediction of venture survival and performance seem to be of interest to practitioners and policymakers alike. Analyses of probable outcomes in new ventures may also enhance the ability of business advisers to suggest adequate compensatory measures in new ventures. However, the precision of our models is still rather low. Some of the problems of predicting performance in new ventures seem to stem from factors outside the venture against which performance is judged, in particular when using measures which implicitly depends on an external references (Bates, 2005; Gimeno et al., 1997). In paper no. 1 (Dahlqvist et al., 1999), this is particularly true of the performance indicators “survival” and “failure”, as well as in the subjective appreciation of performance used in the performance scale separating “high performance” from “marginal survival”. The fact that these indicators rely on subjective appreciation of external options available to the entrepreneur does not give us any apparent reason to assume undue bias. However, what it certainly does is to leave much the influence of important explanatory factors unobserved. Availability of outside job options, and preferences towards self-employment vis-à-vis being employed would need to be factored in when assessing the real options provided by a new venture. These issues may be more related to size than to the entrepreneurial process itself. In cases where the entrepreneur has a key role in keeping the venture alive, outcomes are more dependent on idiosyncratic external options than in larger organization where the survival of the firm is less dependent on single individuals. In these larger organizations, outside options for the means of production may be adequately modeled by e.g. comparison to market returns in similar industries or wages in relevant labor markets.
While our results point to initial resource endowment as a Good Thing, there is little to suggest that the initial start-up motive has an effect on subsequent performance. Actually, it seems that directional hypotheses for a connection between start-up motive and performance could be reversed. For example, the motive of “independence” may be construed as an argument against performance if measured by size indicators such as turnover or number of employees. A wish to be independent might bring about an unwillingness to employ other people or to let the firm’s activities grow beyond what is manageable by the original entrepreneur. Further, a motive with a singular focus on financial incentives such as “to make money” can also work in both directions. It may of course indicate an emphasis on the firm as a vehicle for personal wealth creation, but can also be interpreted as a lack of interest in enterprising as in being “in it only for the money” for lack of other options. If interpreted as a push factor, the “to make money” motive should reasonably be associated with a higher propensity to opt out once outside options appear. Overall, the theoretical arguments for a clear connection between start-up motives and performance are weak and the empirical results are dismissive. Given that the null-hypothesis seems to have survived sincere efforts to have it falsified, it is perhaps time to give it some credibility. A review of entrepreneurship research published in this area reveals that this conclusion is shared by the field; the author has not found any published studies on this topic since the presentation of paper no. 2 (Dahlqvist & Davidsson, 2000).

5.3 Continued entrepreneurship in young firms

Although the assessment of new economic activity is the underlying theme of this thesis, it was explicitly studied as an outcome in paper no. 3 (Wiklund, Dahlqvist, & Havnes, 2001). Here, the “continued entrepreneurship” (Davidsson, 1989) in the empirical context of young firms is measured as their ability to generate new economic activity in the form of new ventures. First of all, it is important to note that the dataset at our disposal is unique in enabling us to research this issue at all. Not only does it require a panel of firms that are controlled for age, but it also means collecting data from the time of “birth” of the firms and onwards in order to avoid possible loss of data from left-hand censoring. Although Wiklund, Dahlqvist and Havnes (2001) is geared towards explanation, a notable fact it that 66 % of the then five-year-old firms had not engaged in any form of new economic activity in the entrepreneur’s sphere of control. This confirms the established “elitist” view of entrepreneurship in existing firms; the majority of firms starts out small and remains small. But how can we explain that some entrepreneurs do engage in new ventures, whether internal, parallel or as completely separate firms? Our study showed that factors traditionally used for explaining new firm formation were applicable also to
CONCLUSIONS: Opportunity and enterprising

existing firms. Human and social capital and a generally accepting attitude towards trading control for growth are factor that contribute positively to the creation of new economic activity in a context of existing young firm. An interesting detail in the data was the way in which our operationalizations of social capital gave us new insights into the links between social capital and the creation of new economic activity. In the category of social capital, we included “access to a formal business network” as an objective and fairly direct indicator of social capital (via networking). To this measure, we added two proxies in the form of gender and immigrant status. We argued that on average, female entrepreneurs may have less access to resources for lack of social capital compared to male entrepreneurs. We also argued that similar reasoning could be applied to immigrant vs. non-immigrant entrepreneurs. In the multivariate analysis, these latter indicators came out weak and statistically non-significant, while “access to business networks” was indeed statistically significant. A plausible conclusion is that in many cases, gender and ethnicity are proxies or distal variables that may be associated with mediating variables that are more closely related to the phenomenon under study. Making these proximate variables available makes a lot of sense. First, it is easier to build causal chains if we do not need to rely on ad hoc proxy measures. Second, having the proximate variables at hand will allow us to examine their relation to other variables of social importance, such a gender or ethnicity. Thus, when building models where we want to assess the influence of belonging to any particular social group, the quality, applicability and social relevance of our work will increase if we investigate models in which we can distinguish the consequences of belonging to any particular social group from the factors affecting entrepreneurial action and performance. This may sound commonsensical, but it is nevertheless important to constantly remind ourselves to make these extra efforts given the seemingly increasing public interest in entrepreneurship and associated policy measures to support it.

5.4 Opportunity and enterprising

The dominant theme of papers four through six (Chandler, Dahlqvist, & Davidsson, 2002; Dahlqvist, 2005; Dahlqvist & Chandler, 2004) is one of entrepreneurial opportunity; how it is detected and its consequences for outcomes of the entrepreneurial process. Shane and Venkataraman (2000) called for the field to embrace the study of opportunity in entrepreneurship research. The lesson of their argument for the inclusion of opportunity in entrepreneurship is as striking as it is simple: is it not reasonable that we should ask ourselves “what is the nature of the opportunity pursued?” before we try to sort out questions about competencies and strategies needed for successful exploitation? Yet, little has been published in terms of empirical work. With the
exception of Samuelsson (2004) and Amason, Shrader, and Tompson, (2006), little has been done to explicate the issue of opportunity variance and its assumed impact on the venturing process. Samuelsson found that ventures that were classified as “innovative” were less likely to skip steps in the gestation process while being less linear in this than their imitative counterparts. Amason et al. (2006) found that the degree of “novelty” (i.e. one aspect of market newness) was related to the tasks required of the top management team (TMT) in a sample of high-potential IPOs. Although not without limitations, these contributions are much needed advances of the field as they set out to measure the effects of opportunity variation regarding direct outcomes (gestation process) or as interactions with management practices (such as the TMT composition).

The papers included in this thesis add to and extend the finding of the works cited above. Paper no. 4 (Chandler et al., 2002) investigated whether the type of search for opportunity is associated with the subsequent entrepreneurial process of exploitation. Albeit inconclusive in some areas, the results point out areas of differences in the process. First, the group of proactive searchers are more resolute to produce a business plan. Second, the group of reactive searchers are lagging in acquiring new machines or premises, which, given their impetus for a “problemistic search” (Cyert & March, 1992/1963) is to be expected.

Paper no. 5 (Dahlqvist & Chandler, 2004) looks at the link between search for opportunity and the market newness of venture ideas that are pursued. The major finding is that search matters: searchers pursue venture ideas that have a higher degree of market newness than non-searchers. Further, proactive searchers (searching because they can) pursue more novel venture ideas than reactive searchers (searching because they have to). In addition, ventures pursuing venture ideas found through fortuitous discovery show lower degree of newness but they also have a markedly higher within-group variance. Chronologically, the search process should be placed before the actual gestation process, eventually reaching into the early phase of “intentionality” of the gestation process (Katz & Gartner, 1988). It so contributes to extend our understanding of the entrepreneurial process and how opportunity is discovered by providing a link between the set of “favorable circumstances” that are opportunity and the cognitive process that shapes actual venture ideas. To use the signal detection metaphor suggested by McMullen and Shepherd (2003), the cognitive aspect of the entrepreneurial process is how we detect and make sense of a signal (opportunity) in a stream of information dominated by irrelevant facts (noise). To carry on the imagery, the search process may be likened to the way we aim our antennas to improve the signal-to-noise ratio as much as possible before trying to evaluate what the signal means. Promising work on optimization of search has recently been initiated by Jim Fiet and associates (for theory see e.g. Fiet, 2002; Fiet, Piskounov, & Patel, 2005).
CONCLUSIONS: Opportunity and enterprising

As an appendage, Dahlqvist (2005) is to its character very much a methodological piece on the validity of the measure of market newness used in Dahlqvist and Chandler (2004). Although methodology is in the forefront, the argument is directly relevant to the subject matter and the development of the field. The question of whether opportunity varies across different ventures (the opportunity variance hypothesis) must be examined if causes or interactions due to opportunity should be explored. Part of the struggle with the construct of opportunity is to suggest plausible links with other “nomological” constructs (Cronbach & Meehl, 1955) that would increase the validity of the concept by anchoring it in a context of association. However, the literature on opportunity is less explicit on the impact of opportunity that it is on the nature of opportunity. Thus, several theoretical classes of opportunity are available for testing by the empirically minded researcher but models detailing the impact of possible differences in opportunity are less obvious. What is this a sign of? On the one hand, prominent scholars in entrepreneurship underline the importance of entrepreneurial opportunity. On the other hand, to the best of this author’s knowledge, no empirical measure of opportunity has appeared in the main entrepreneurship journals with the exception of Amason et al. (2006). It seems that challenges await those who ventures into this area of research, both in terms of empirical work and theory building.
6 Future research

6.1 Taking stock

The ubiquitous “this research is not without limitations” has not received its own heading in this thesis. Instead, I have tried to deal with limitations or “issues” directly in the commentaries, the aim being to treat strengths and weaknesses in a coherent and (hopefully) balanced assessment of each individual paper. However, there are some issues that are programmatic in character, and which transcend the specific problems that would be desirable to correct in the individual papers.

Notes on future research may sometime give the impression that the author suggests directions for other researchers, partly in terms of “leftovers” from the current study, partly in the form of new vistas to which the author is not set to go. However, in this chapter I will make suggestions on future studies mainly directed to myself. These suggestions are produced in the exact science of hindsight, by taking stock of the research streams that can be discerned in this thesis. Two types of questions arise from this exercise. First: “How can we remedy important shortcomings in existing studies?”, and second: “What new questions have risen in the light of these results?” These questions are not possible to answer without relating to the present state of the field of entrepreneurship research. Given that the included papers were written over a period of time during which the field has experienced inspiring progress and coming-of-age, pure introspection seems very much out of place. Finally, in this presentation, the same split between methodology and the subject matter can be made for future research as we have made in the reviews in chapters 4 and 5. In the ensuing discussion, we will follow that order.

6.2 Future challenges for methodology in entrepreneurship research.

The commentaries on methodology in chapter 4 contain mixed messages. On the one hand, it is suggested that the methods are flawed: the use of a single cohort, inevitable delays in the data collection procedures that may cause left-hand censoring, all combined with possibly crude or even ad hoc measures. On the other hand, the dataset used for this thesis has unique attributes and is of very high quality in relation to what has been presented in the reviewed
literature. In terms of competitive edge for improving the state of the field, this latter aspect of relative merit does matter. Making an improvement on the current status of knowledge in what scientific progress is about, viewed as a collaborative project among interacting scholars. However, relative merit matters less in terms of truth content. I believe truth is an ultimate quest for many researchers, and that indefinitely improving on a fundamentally unsatisfying state of knowledge is simply not enough to provide enough motivation for the endeavors normally associated with quality research. Thus, it is only in the light of the absolute that some limitations will appear. In terms of theoretically grounded advantages, the dataset used for these papers had the following important features and are listed in their logical order of importance:

- Theory-driven sampling units (genuinely new independent start-ups and internally generated ventures)
- A random sample from a comprehensive sampling frame of a defined target population (resulting in known sampling probabilities)
- High response rates and low attrition from methodological artifacts (lessening the risk of uncontrolled biases)
- Large number of cases (originally 7000+, to have a satisfactory number of degrees of freedom in multivariate analyses)

**Sampling units**
The way in which we conceive the entrepreneurial event or process should have definite consequences for how we operationalize the units of analysis in order to sample them. In entrepreneurship, the firm seems to be “always and everywhere” a proxy for what we really want to study, be it new economic activity or new organization. In contrast to e.g. the PSED study (Reynolds, 2000), the original sample was based on a procedure that sampled directly on new economic activity instead of the entrepreneurial agent, which put extra strain on our ability to find all forms of new economic activity through existing registers when setting up the sampling frame. Fortunately, this was possible in Sweden provided that a one-year delay in reporting was acceptable. However, there is admittedly some left-hand censoring of the data since some new ventures cease to exist within their first year of operation. Previously, I have argued in favor of sampling on the entrepreneurial agent to avoid this lag. However, using the new economic activity proper would not be impossible if a survey could be conducted continuously throughout the year. The one-year lag is not mainly caused by the sampling units not being available continuously during the year but rather from the restraints, mainly economic, on the survey methodology used by the original instigator of the database, Statistics Sweden. To them, the lag does not appear to be a validity problem, since their primary aim is not to provide process data but rather to achieve point estimates of the
number of genuinely new start-ups per annum. So, in the case of Sweden, it does seem at least theoretically possible to close the gap between the two sampling approaches for studies of the new economic activity that is actually initiated. However, to study the pre-organization stages of nascent entrepreneur, future studies must adopt sampling on the entrepreneurial agent.

**Data collection and sampling**

Known sampling probabilities is another issue where the present study is in compliance with theoretical assumptions. However, the issue is intimately intertwined with the issue of statistical inference, which seem to be a black-hole topic into which information and arguments are sucked in but never released. On the one hand, we have the Fisherian idea of constructing infinite hypothetical populations from which we draw a random sample. In contrast, we may recall the Pearsonian approach in which statistical testing acquires meaning in relation to real populations from which only a sample is studied (Oakes, 1986). The question of generalization is different between these two approaches. In the Fisherian approach, the hypothetical population is already an abstraction and results could be inferred to theory. In the Pearsonian approach, results are inferred to an actual population and any further inferences towards theory must be done in terms of validity: what sort of theoretical entities does this actual population contain?

The present study relies on the Pearsonian logic and the sample is suggested to be representative of all new independent economic activities initiated in Sweden during 1994, sampled on the firm level. Whether these results are generalizable to other populations is a question possibly left to judgment. However, regardless of how we choose to generalize to theory, the issue of known sampling probabilities is equally important. In the case of the infinite hypothetical population, the sampling probability for each case will asymptotically approach zero but it should be known. Yet, I believe that this is an issue that remains largely uncontrolled in current research. Partly this may be a question of “improvement” reasoning, that is, as long as a study is an improvement over current practice, it is also worthwhile. Although a plausible strategy for research, this line of thinking may also perpetuate methodological sins committed in earlier research. It is hard to see how progress can be had when neglecting basic rules of sampling since the basis for understanding what it is we are building knowledge about is lost. Access to data is restricted in many parts of the world, but simply surrendering to the “it’s an improvement” doctrine is not good enough. For those aspiring to at least approximate truth in their research, we cannot follow the current practice of accepting almost any sample as appropriate for significance testing while disregarding distributional assumptions. The implication for future research is that samples with known sampling properties are possible but cumbersome to create. Although it means
putting a lot of effort and thoroughness into data collection, the amount of data that are collected in a single survey has often proven to be “enough to go around”. The pooling of resources to obtain sound samples, such as the PSED study, is one obvious way to ease the burden of individual researchers. Thus, collaboration in the future should focus equally on the empirical context (as in what units we should sample) as well as in research questions on the subject matter (as in what items should we include).

Controlling non-responses and attrition

When working with panel data, attrition is an inherent problem. Ventures do cease to exist and may hence exit the panel at some point in time. However, in addition to this natural attrition, there are also losses of data that are entirely due to methodological artifacts. A clear, but avoidable, example of this is lack of contact information, which means that the venture may still be running but that we cannot contact it. Another source is respondent fatigue, i.e. when a venture stops supplying data for reasons relating to the data collection methods. Both of these non-response mechanisms have been very low in our studies in comparison. Partly, this is probably a question of culture: Swedish firms and citizens generally have confidence in their government agencies and universities. However, much of the data used was actually part of compulsory government surveys, and the agency that collected the data (Statistics Sweden) had, at least theoretically, means available to force respondents to answer their questions. However, this possibility have been used extremely rarely according to personal communication with the staff of Statistics Sweden and then only as a “latent” threat. The general confidence enjoyed by Swedish authorities seemed to be enough and the social contracting suggested by Dillman (1978) may be at work here on a broader societal level.

However, it is in the follow-up studies that were conducted without any legal means to force responses that most clearly illustrated the importance of social contracting. The sub-sample of new internal ventures that provided data for papers 4 through 6 (and partly to paper no. 3) shows very little attrition due to methodological artifacts, despite being relatively burdensome. The telephone interviews lasted up to 20 minutes, and continued twice a year for three years. Despite this potential wearing down of the respondents, we were able to collect about 90-95% of the target panel in each wave. Part of this is probably the social contracting that actually seemed to be reinforced by repeated surveying. Showing that we cared and that we had noted what the respondents said last time seem to have built a lasting confidence in the interviewing staff. Key elements in this process are possible to include by design but it is not without cost. First of all, all interviews conducted by phone achieved approximately a 95% response rate per survey wave. The only paper survey that was used on the same sub-sample “only” reached 55 % despite being midway in the data.
FUTURE RESEARCH: Future challenges for methodology in entrepreneurship research.

One conclusion from this experience is that small panels such as this (original n=250) cannot rely on pen-and-paper methodology if internal non-responses are to be minimized. If mail surveys are deemed necessary, for example by the nature of the data collected, they should ideally be complemented by a telephone survey that walks the non-response cases through the paper questionnaire by phone. In commercial surveys, this practice in not uncommon but then the size of the budget per response may be substantially higher compared to the means available to academic research. Still, we must remember when using small samples in combination with multivariate techniques on panel data, holes in the data matrix cost us statistical power that we simply cannot afford to lose. Looking into the future, the development of telecommunications may be detrimental to the suggested advantages of telephone surveys, since a telephone number does no longer have an unambiguous connection to a fixed geographic location. Thus, sampling on telephone numbers may no longer be a viable solution in terms of coverage and comprehensiveness. Judging from its widespread use, online surveys seem to be very promising except for perhaps response rates. Again, in combination with telephone contacts to establish a social relation, this problem can probably be kept under control.

A second issue in respondent fatigue is in the behavior of the interviewers. In large sample surveys, the individual researchers cannot be expected to conduct the interviews. Instead, they have to rely on the selection and training of external interviewers. For collection of data for the included paper, the author had the good fortune to work with highly experienced professionals which safe to say saved us from a lot of trouble that would have been difficult to remedy ex post. In general, academic training is focused on theory building and item development, and less on the actual collection of data. While a sound understanding of theory development and analytical techniques is of the essence to any researcher, a poor understanding of what goes on in the “moment of truth” when respondents contribute their answers, threatens to undermine the validity and reliability of the research by introducing systematic or stochastic errors in the data. Having a staff that a) can understand when items “do not work”, and b) have an interest in adapting to the situation and forwarding their perceptions to the researchers, is of great advantage. Safe to say, it saved me more than once. Although, proper pilot testing can fix many errors before going into the main study, “issues” seem to have a way of appearing no matter what. Thus, one conclusion is that a trained staff is of the essence.

The use of large samples
The dataset used throughout this thesis originally contained over 7,000 cases. In contrast, the sub-sample of internal ventures used to investigate opportunity variance and opportunity search patterns comprised “only” 250 cases. If
nothing else, the substantial difference in sample size has been illuminating. Large sample sizes seem to be fairly uncommon in entrepreneurship research and the issues associated with large samples are rarely encountered by the average researchers. Although rather basic from a statistical point of view, some remarks may still be in order. First, a large sample in a milieu used to small to medium-sized sample will typically have lower associated probabilities (“p-values”) in significance tests. Technically, the associated probability of a significance test, let us say in a multiple regression setting, is determined by:

- number of cases
- number of variables estimated
- effect size or difference
- variance

The number of cases and the number of variables jointly determine what is called degrees of freedom which in turn determines the associated probabilities. Having a large number of cases increases the degrees of freedom and the statistical power of associated significance tests. Holding the other factors fixed, an increase in sample size also has the effect of boosting statistical power, and in the process “making everything statistically significant”. This was encountered in papers nos. 1 through 3, in which the majority of the cases were used. However, statistical significance in not the same thing as “importance” as illustrated by the fact a small effect size (or group difference) may come out “highly significant” in statistical terms (low associated probability) but be small and irrelevant in relation to the research question. My own impression has been that statistical significance is somewhat uncritically accepted as a quality mark. Although the associated probability may be useful as an indirect signal of a reasonable effect size, it requires a setting in which the other determinants of significance are well understood. My own impression has been that presenting results from a large dataset requires diligence since the generally low figures of associated probabilities may “blind” an audiences that is used to evaluating results obtained from smalls samples. Using a large dataset demands an increased awareness about what is “significant” from the perspective of the research question.

In stark contrast to the original large sample, the subsample used in the latter papers “only” contained 250 cases in the first survey wave. From this number, internal non-responses and, to a lesser extent, attrition, took away about half the sample in some analyses. What was learned? While the problems of large samples are “luxury items” that many researchers in entrepreneurship cannot afford, the problems of small samples are common and the implications go beyond that of associated probability. First, the number of cases sets an absolute limit as to how large a model can be estimated from a sample. Investigating large models simply requires many cases as to leave enough
degrees of freedom for estimation and to maintain statistical power. This problem is further complicated if analysis is done by group, e.g. industry, since additional degrees of freedom are quickly soaked up which boost standard errors and lessens the power of statistical tests. The analysis of group differences brings us over to an associated problem, namely that of heterogeneity and variance in sampling.

One of the unusual features of the original dataset used in this thesis is that it is representative of the whole population of genuinely new independent ventures started in Sweden during 1994. As such, it contains as much diversity as the population itself and this has shown to be a problem as well as an asset. To create a model of a population is a trade-off between relevance and generality. One approach is the “one size fits all” approach in which one model is forced upon the population. This might be sensible if the researcher has reason to suspect that the population is homogeneous enough to lend itself to such practice. However, if heterogeneity is a dominant characteristic of the population, other approaches might be more appropriate. It should be noted that model selection in not just a question of “adding controls” or dummies for contingent factors, but also to review the fundamental assumptions of the models. Is it, for instance, reasonable to model fast-growing companies in knowledge intensive industries the same way we model subsistence level businesses in retail? Can we explain the software industry with the same model as we use for food retail by shifting the curve with an industry dummy? These are empirical questions but my own default stance on this is “probably not”, at least not in every aspect.

To improve the field, future studies must carefully consider the generality of the conclusions as well as the relevance to determine appropriate sampling strategies. Obtaining representative samples is of course appealing since it provides a rational for inferring results to a real population. However, if such a sample is dived into smaller subsamples to keep heterogeneity at bay, sample size may decrease rapidly. Large representative samples of populations are apt to answer research questions that could reasonable be expected to be general across groups and where the contrast is needed to understand the phenomena under scrutiny. In hindsight, my own view is that representative samples (of the type exemplified by the Cohort ’94 of genuinely new enterprises and the sample of new internal venture) generate many problems because of the tremendous heterogeneity involved. Increasing heterogeneity will inevitably inflate variance which may be detrimental to the precision of the estimates and this can only be countered by increasing the sample size. In addition, explorative research is complicated because of the heterogeneity involved; it is simply too diverse to grasp. By restricting the population to e.g. an industry, firm type or geographical area, researchers can sacrifice generalizability of the results in order to gain relevance and precision. This is of course also a way to make individual
surveys more affordable since smaller samples can be accepted. As a general rule for future research in entrepreneurship, I believe that it is better to produce clear and relevant results for a particular industries or empirical settings rather than to reach imprecise conclusions for broader spectrum of entrepreneurial endeavors.

6.3 Future challenges in entrepreneurship research

The development of the papers included in this thesis ran over a period of six years. During this time, plenty has happened in entrepreneurship research (by which I refer to the strain of research found within Management and disseminated in journals such as *Academy of Management Journal, Academy of Management Review, Journal of Management, Journal of Business Venturing* and *Entrepreneurship Theory and Practice*).

Papers nos. 1 through 3 is what I could refer to as “traditional” entrepreneurship research. With this I imply using the initiation of new economic activity as a special empirical setting. In the case of these earlier papers, the discipline is strategic management and the basic issues of resource endowment and motivation are related to relative performance. However, paper no. 3 is somewhat of a hybrid and parts with traditional performance measures as the dependent factor since it looks at new economic activity as an outcome. Yet, it is still somewhat distant to an opportunity-based view of entrepreneurship (Eckhardt & Shane, 2003; Shane & Venkataraman, 2000; Venkataraman, 1997) in its implicit emphasis on the exploitation process. But what makes this contribution stand out is its effort to operationalize entrepreneurship as the creation of new economic activity across various organizational contexts. As such it is truly a first step. Capturing new economic activity is a daunting task indeed, especially if new ventures are to be followed over time. The organic nature of a venture makes it susceptible to change in such a way that by the end of a process, we may not agree that it is still the same entity that we are following. The biological metaphors of genomes and phenotypes simply do not apply to new ventures in which there is no biological genome that deterministically controls its development. Adaptation and change of its core is possible to such extent as to change it completely and fundamentally. This view may perhaps curb the enthusiasm of those who are struck the appealing simplicity of Herbert Simon’s delineation of entrepreneurship (Sarasvathy, 2000): following “new economic activity” over an extended period of time may not even be conceptually possible because of the fundamental change that new ventures can go through. However, I believe that giving up at this point would be premature. Exploring the new economic activity concept has proved to be quite feasible in cases where change has be moderate and where organizational structure is largely unaltered. Looking at the
results of the included studies, this seems to be the case for the majority of new ventures. However, it is cases of major change and metamorphosis that are the most pressing to understand. As researchers, it is the atypical that challenges out models and points out the limitations of our understanding. But we also dislike leaving it to the “inexplicable” without a fight. A first step to deal with the challenges of change in new ventures is to find descriptive models of the venture as to describe what it is that change: opportunity, venture idea, organization or resource bundles? But unless opportunity and ventures ideas are included in such models, the entrepreneurial venture cannot be made into one comprehensible entity.

The opportunity perspective is dominant in papers nos. 4 through 6, which could perhaps be called more “contemporary” in terms of research questions. There is clear presence of an opportunity-based notion of entrepreneurship represented by a) search for entrepreneurial opportunity and, b) measurements of the market newness of new ventures founded on perceived opportunities. This direction is strongly inspired by the literature that suggests that entrepreneurship research should be centered around entrepreneurial opportunity (e.g. Shane & Venkataraman, 2000; Stevenson & Jarillo, 1990; Venkataraman, 1997). Although it seems almost self-evident that opportunity varies in kind and degree, precisely little has been done in term of empirical work. Despite my sincerest efforts, only one peer-reviewed article could be found on opportunity classification (Amason et al., 2006). Why is this? Is the idea that the nature of the opportunity should be included as an explanation for venture development and outcomes not accepted? Is the newly spurred interest in opportunity and opportunity recognition too recent to produce empirical research? Is it irrelevant despite eloquent arguments in favor of it? I do not know, and frankly, this bothers me. My own view is that the role of opportunity is still badly under-researched in entrepreneurship research and I see several fronts on which work has to be done.

**Theoretical development of entrepreneurial opportunity**

The arguments of the “opportunity-based view” of entrepreneurship was succinctly put forward by Venkataraman (1997) and later developed by Shane and Venkataraman (2000) for a wider audience. Eckhardt and Shane (2003) further explicated some points of potential misunderstanding and shed new light on the **loci**, **sources** and **agents** of entrepreneurial opportunity. Sarasvathy, Dew, Velamuri, and Venkataraman (2003) contributed by detailing three types of opportunity processes: **recognition** (both supply and demand exist), **discovery** (either supply or demand exist) and **creation** (neither supply nor demand exist) prior to the entrepreneurial process. What has been significant in these articles is a particular focus on a) the definition of opportunity, and b) how it is found (antecedents and agents). However, the **consequences** of opportunity
“downstream” in the entrepreneurial process have not attracted so much attention and are yet a white spot in entrepreneurship research. While the lack of empirically tested measures is indeed a problem, so is the lack of theory that helps to explain the role of opportunity in the entrepreneurial process and which would serve as basis for developing hypotheses on the subject. The lack of theorizing had led me to move ahead by focusing on measurement with less regard to supporting structures that could link characteristics of opportunity (or rather venture ideas) to outcomes of the entrepreneurial process. In papers no. 3 through 6, two important linkages in the entrepreneurial process were explored. First, search for opportunity was linked to the gestation process. Second, the search process was linked to the market newness of venture ideas. Although these studies are far from the final word on this matter, it is a start and I hope that the field will develop in this direction. The iterative process of induction and deduction must start somewhere and I have proceeded in a rather explorative fashion. Future efforts may build on this, but new theory has to be developed.

The idea of “newness” (as in “without precedent”) is one important characteristic of entrepreneurial opportunity, evoking images of innovation and development. Indeed, much of the work in this thesis was inspired by Schumpeter (1983/1934) but with an objective to extend the research to cover the whole range of opportunity newness rather than solely focusing on the glory of “frame-breaking” innovation (Stopford & Baden-Fuller, 1994). The newness of venture ideas may be expected to correlate positively with failure because of the uncertainties involved but also with profitability because of the potential for entrepreneurial profit (Schumpeter, 1983/1934). However, newness could also be expected to impact the gestation of the new venture in term of activities that must be undertaken and information needed. While the “newness” of opportunity is a classic attribute of the entrepreneurial venture, other attributes can matter substantially but on other dimension. As already has been mentioned, Eckhardt and Shane (2003) points out that opportunity can be located in different places in a value chain and that the classical focus on product-markets or pure process innovation is not sufficient to describe opportunity that relies on the redesign of an existing business model. Such attributes may work in other dimension than, for instance, market newness but it is important for such classifications that theory is build, hypotheses are generated and empirical work carried out. Without the initiation of such work, classifications schemes will petrify in speculation and empirical work will remain without aim.
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Six papers on entrepreneurship


Initial conditions as predictors of new venture performance: A replication and extension of the Cooper et al. study

Jonas Dahlqvist, Per Davidsson, & Johan Wiklund

Abstract

In the words of Hubbard, Vetter and Little (1998, p. 252), “systematic replication replaces piecemeal, untested results with useful findings that address practical problems.” We agree with this. We further hold that for empirical relationships to be really interesting and meaningful one should be able to make a strong case that they represent causal and generalizable relationships. In addition, they should allow a meaningful theoretical interpretation. In this study we try to adhere to such ideals by replicating and extending a theory-driven study of the effects of initial conditions on new venture performance (Cooper, Gimeno-Gascon & Woo, 1994) using a very large (7000+ cases) and high quality, longitudinal data set. Data on initial conditions were collected in 1995 (within a year after first registration) and outcomes were assessed in 1998. On a conceptual level, our results confirm those obtained by Cooper et al. (1994) regarding how general human capital, management know-how and industry affect marginal survival probability, as well as concerning the effects of financial and general human capability on the likelihood of becoming a high performance venture. The results sometimes coincide also on a very detailed level, such as the differential effect of gender on marginal survival vs. its effect on high performance. Other parts of Cooper et al’s (1994) result could not be replicated. To some extent this may be due to weak operationalizations of certain constructs, but real sample and/or country differences may also play a role.

Business start-up reasons and firm performance

Jonas Dahlqvist and Per Davidsson

Abstract
This paper investigates the possible relationship between the most important start-up motive of the founder and subsequent performance of the firm, measured three years after the start-up. Using a large longitudinal database with 7000+ cases, the analysis was performed in two steps. First, the effect of the start-up motives on firm survival was investigated, while further analysis was done to study whether the performance of those firms that were still active were related to the start-up motives. In this study, start-up motives did not show any predictive power on survival. In relation to subsequent performance, the two motives “need to work independently” and, to a lesser extent, “unemployment” came out positively correlated and significant, although effect sizes were very small.

Introduction
It is intuitively appealing to expect the start-up reason of the entrepreneur to influence firm performance. In the newly formed firm, strategic scope and resource acquisition is closely linked to the entrepreneur and it is in these initial stages where the relationship between entrepreneurial drive and firm performance should reveal itself at its strongest. However, previous empirical findings have been rather dismissive of this intuitive logic. In the present study, five pre-defined start-up reasons assessed during the firms’ first year in operation are related to performance in terms of firm survival, number of employees, sales and perceived profitability.

Entrepreneurship as new business activity: Empirical evidence from young firms

Johan Wiklund, Jonas Dahlqvist and Per-Anders Havnes

Abstract

Contemporary as well as classical conceptualizations of entrepreneurship tend to view the entrepreneurial process as a set of activities where resources are recombined in new ways in the pursuit of profitable business opportunities. The entrepreneurial initiatives can be pursued either by existing firms or by new firms. This study takes predictors previously used to explain the relative success of start-ups and applies them to internal ventures in a cohort of young firms. The results suggest that a fair amount of predictive power can be extracted from established models of entrepreneurial success in start-ups, indicating that independent and internal ventures are different aspects of the same phenomenon.

Introduction

Over the years, several definitions of entrepreneurship have been proposed. They fall into two broad categories. The first, most clearly articulated by Gartner (1988), holds that entrepreneurship is the creation of new organizations. This focus originated in the lack of treatment of organizational emergence in organization theory. Somehow, organizations were assumed to exist; theories started with existing organizations (cf. Katz & Gartner 1988). This view has much merit, which has led other scholars to adopt it (Aldrich, 1999; Thornton, 1999; Sharma & Chrisman, 1999). The main problem with this approach is

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Opportunity recognition processes: A taxonomy and outcome implications

Gaylen N. Chandler, Jonas Dahlqvist and Per Davidsson

Abstract

We create a taxonomy of opportunity recognition processes for new business initiatives being developed in 136 firms that registered first sales in Sweden in 1994. The three categories include proactive search, reactive search, and fortuitous discovery. Results indicate that initiatives discovered through a proactive search are implemented more rapidly than those discovered through reactive searches or fortuitously. However, as time passes, the advantages in implementation speed are reduced and in some cases nullified. This implies that the opportunity discovery process has an impact on the timing and speed of implementation, and may have measurable longer-term impacts on profits.

Introduction

Shane and Venkataraman (2000) posit that entrepreneurship is the intersection between opportunities and enterprising individuals. The vast majority of published entrepreneurship research focuses on individuals and opportunity exploitation processes (Busenitz, et al., 2003); there is little published research focusing on opportunity detection processes. An emerging body of research seeks to address this deficiency by focusing on the cognitive processes used by entrepreneurs to detect opportunity (e.g. Gaglio, 1997; Hills, Shrader, & Lumpkin, 1999; Singh, Hills, Hybels, and Lumpkin, 1999). The logic of this stream of literature is that individuals create schemas that represent cumulative

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Abstract
Recently, opportunity has received a lot of attention in the entrepreneurship literature motivated by its expected impact on various outcomes such as the mode of exploitation or profitability. In this paper we related the type of opportunity search (proactive, reactive or fortuitous discovery) to the novelty of venture ideas in new business initiatives pursued by existing young firms (n=136). Using a typology developed Davidsson (2003), we derive indicators for a formative index of novelty. Results show that new business initiatives in firms that actively search for opportunity achieve higher scores on market newness than firms characterized as fortuitous discovers.

Introduction
In recent years, opportunity has been in the foreground of various efforts to delineate the scholarly domain of entrepreneurship (Davidsson, 2003; Shane & Venkataraman, 2000; Venkataraman, 1997). Although the sources of opportunity may still be under debate, it is clear that the very concept of entrepreneurial opportunity implies some classification of the venture ideas that entrepreneurs pursue; if there was no variation, why else would we care about the concept? One of the reasons why opportunity is important is that it may have differentiating effects on the discovery and exploitation processes as well as on profitability and potential market impact (Samuelsson, 2004; Shane & Venkataraman, 2000). These outcomes of the opportunity type could be thought of as being located “downstream” of opportunity in a causal relationship: the type of opportunity is supposed to influence the processes to

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Measuring the market newness of new ventures

Jonas Dahlqvist

Abstract

While entrepreneurial opportunity has been pointed out as a vital area for research (Shane & Venkataraman, 2000), the concept is not yet firmly attached in a nomological network (Cronbach & Meehl, 1955). With few empirically established “connections” with antecedents or consequences, researchers must now strive towards valid constructs and measurement of opportunity. Presently, the lack of instruments for measuring opportunity variance is seriously hampering progress in entrepreneurship research because basic hypotheses about opportunity variance cannot be tested. The purpose of this paper is to describe and validate a measure of market newness in new ventures. Items were generated from a typology of market newness (Davidsson, 2003) and empirically tested on a sample of 250 new internal ventures “harvested” from a cohort of approximately 4,000 young firms. For the purpose of measuring market newness, formative and reflective indices were constructed. Although the investigated measures stand on different assumptions about causality, they empirically converge towards identity. Although admittedly crude and exploratory, the results nevertheless indicate reasonable overall validity of the measured construct. An important conclusion is that measures of newness should preferably be built on items with as much intrinsic range as possible, e.g. indices from multiple Likert-scales. Worthwhile objectives for future research are to develop measures with a continuous latent construct in mind, to complement self-report with independent objective measures and to explore ways to increase resolution in the low-to-medium range of the newness scale.

6 This paper has been submitted to the Journal of Business Venturing for review. Pending the reviewing process, the bibliographical reference is: Dahlqvist, J. (2006). Measuring the market newness of new ventures. [Unpublished manuscript]. Jönköping International Business School, Sweden. The paper can be obtained from the author Jonas Dahlqvist, JIBS, Box 1026, SE-551 11 Jönköping, SWEDEN.
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