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Thesis title:

Oiling the wheels of corporate entrepreneurship:
initiative development and survival in established companies

PhD in	Business Administration and Management
Cycle	XXI
Candidate's tutor	James Hayton
Year of discussion	2012

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BOCCONI UNIVERSITY
PhD in Business Administration and Management
XXI cycle

OILING THE WHEELS OF CORPORATE ENTREPRENEURSHIP: INITIATIVE DEVELOPMENT AND SURVIVAL IN ESTABLISHED COMPANIES

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To the ones that opened doors for me and showed me the way

José de Sousa Saramago – Journey to Portugal

ACKNOWLEDGEMENTS

Now that the end of this incredible journey is approaching I would like to express my gratitude to the many people that in different ways and times contributed to make it so special. First of all, my PhD committee: Professors Stefano Brusoni, Robert Grant, James Hayton and Markus Venzin. They provided me with excellent and extremely competent guidance in a truly engaged, encouraging, and inspiring manner throughout the process.

I would like to thank ENERCO and the people from the GC unit, which I was able to research and share time with, when others only dreamt of it. Their availability and passion for work truly impressed me. I will never forget the hours they spent helping me figure out how their entrepreneurial engine works. Those were meaningful hours in many respects.

I also wish to acknowledge my many student colleagues with whom I have shared this experience. Some of them have left academia, others live in different parts of the world. Together we managed to overcome hard obstacles but also to enjoy incredible time.

Most importantly, I would like to thank my family and friends who have provided me with more encouragement, support, and patience than I ever imagined possible.

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CHAPTER 1
Introduction

Increasing uncertainty and competitive pressures in the global business environment require established companies to systematically engage in entrepreneurial activities in order to be successful over time (McGrath & MacMillan, 2000; Zahra & George, 2002; Morris et al., 2008). Corporate entrepreneurship, defined as the process through which individuals or groups of individuals pursue the development of new businesses, or instigate strategic renewal and innovation in an existing organization (Sharma & Chrisman, 1999) is widely acknowledged as an effective growth strategy, and a means for firms to revitalize their existing businesses (McGrath et al., 1994; Bhardwaj et al. 2006; Ireland et al., 2009).

Scholarly interest in corporate entrepreneurship has been growing dramatically over the past few decades (Burgelman, 1983; Kanter, 1985; Pinchot, 1985; Guth & Ginsberg, 1990; Zahra, 1993; 1996; McGrath & MacMillan, 2000; Sathe, 2003; Phan et al., 2009). Alternative approaches to manage entrepreneurial activities in the established context, entailing different degree of structural separation and resource authority, have progressively emerged (Block & MacMillan, 1993;Wolcott & Lippitz, 2007; Morris et al., 2008). Our focus in this thesis is on the internal corporate venturing one, which involves the identification and exploitation of new business opportunities within a firm's ongoing operations.

Several studies highlight that the value of the internal corporate venturing approach lies in the creation of structurally-differentiated and semi-autonomous organizational entities, such as incubators, new business development divisions or corporate venture units, with a specific mandate to identify differentiating opportunities and experiment with novel solutions (Burgelman, 1985; Sykes, 1986; Block & MacMillan, 1993; Birkinshaw & Hill, 2005). These purposely designed entities expedite the exploratory processes underlying most venturing endeavours, shielding entrepreneurial activities from the dominant managerial cognitions and

inertia affecting mainstream businesses (Husted & Vintergaard, 2004; Burgers et al., 2009). Given their intrinsic risks and uncertainties, and the long timeframe on which they usually operate, most venturing initiatives wouldn't otherwise survive "the corporate immune system" (Birkinshaw & Riddestrale, 1999), which tends to favour exploitative activities over explorative ones (Burgelman, 1983; Dougherty, 1995; Kanter, 2006; McGrath et al., 2006).

Although much is known about the main antecedents and outcomes of firms' engagement in internal venturing activities, knowledge about the process through which entrepreneurial ventures are initiated and developed in the corporate context is still limited (Birkinshaw, 1997; Garvin, 2002; Dess et al., 2003; Narayanan et al., 2009). The long gestation time of most corporate entrepreneurial initiatives, which evolve through some key iterative stages entailing the *generation*, *evaluation and selection*, and i*mplementation* of innovative ideas, has often made it difficult to monitor and capture their underlying dynamics (Kazanijan, 1988; Van de Ven & Polley, 1992; Block & MacMillan, 1993;Van de Ven et al., 2000; O'Connor & Rice, 2001). This shortcoming is evident especially as concerns the *evaluation and selection phase* of the venturing process, that despite being acknowledged as central to the value-creating potential of firms' entrepreneurial activities has been surprisingly overlooked by most extant corporate entrepreneurship literature (Phan et al., 2009).

In fact, so far, scholarly attention has mainly focused on the early and later stages of internal corporate venturing processes, shedding substantial light on the variation-generating mechanisms employed to facilitate the emergence of innovative ideas (Covin & Slevin, 1991; Hansen, 1999; Kuratko et al., 2001; Perry-Smith & Shalley, 2003; Reid & De Brentani, 2004), and on aspects related to the organizational set-up, growth and development of new ventures (Van de Ven et al., 1984; Miller et al., 1991; Brazeal, 1993).

Knowledge about drivers and dynamics of internal venture selection is conversely rather limited (Kazanjian & Drazin, 1987). The nature of the process through which innovative initiatives are evaluated and selected in the venturing context is still ambiguous. Although prior studies on internal corporate venturing have framed selection as a single-stage event (Burgelman, 1991; Lovas & Ghoshal, 2000), anecdotal evidence suggests the existence of multiple selection phases proposed initiatives have to go through before decisions about their final implementation are made (Benford & Snow, 2000; Eckhardt et al., 2006).

Transition across these different phases seems to be regulated by go/ no-go decisions at various stages of the venture development, with increased resource commitment linked to the achievement of key milestones (MacMillan et al., 1985; Sahlman, 1990; Hamel, 1999).

To timely winnow out less promising initiatives and divert resources to those deemed potentially more valuable, firms normally base their selection decisions on some key pre-determined criteria reflecting both the venture's possible fit with corporate strategic goals, and more specific project-related characteristics such as novelty, relatedness with mainstream activities, time to profitability, expected revenue streams and the experience of initiative proponents and champions (Desarbo et al., 1987; Roussel et al., 1991; Block & MacMillan, 1993; Elfring & Foss, 2000). A thorough examination of the extant literature, however, reveals that the identified venture selection criteria derive almost exclusively from studies employing experimental and post-hoc research methods (MacMillan et al., 1987; Desarbo et al., 1987; Riquelme & Ricards, 1992; Hall & Hofer, 1993). Oversimplifications of the contexts of investigation (the venture-related decision-making one in our case) and self-reporting biases typically associated with these research methodologies may severely limit the accuracy and validity of the reported results (Heckman & Smith, 1995).

To effectively capture how actual venture selection decisions occur in a real decision-making environment, longitudinal studies tracking how ventures move through different evaluation and selection interfaces would be needed (Phan et al., 2009; Petty & Gruber, 2011). They would also allow to fill an additional gap in the extant literature, related to the investigation of potential differences in the relevance of specific criteria at different stages of the initiative evaluation process. Although some studies (Pinto & Prescott, 1988; Rietzschel et al., 2010) suggest that factors shaping selection patterns vary according to ventures' stage of development, there is no documented evidence, to our knowledge, of these differences.

A further limitation of most extant research on internal corporate venturing, that can partially explain the above-mentioned lack of evidence, is its almost exclusive focus on ventures surviving the internal selection process. Critical success factors are solely derived from the comparison of initiatives that achieve commercial success with those that don't (Cooper & Kleinschmidt, 1987;Zirger & Maidique, 1990). Early-stage ventures, initiated but discontinued along the way have been systematically omitted from consideration, overlooking

important information about factors influencing initiatives' transition across different selection stages, and misrepresenting their performance (Shortell & Zajac, 1988;Zahra, 1996).

In light of the identified gaps in the literature, this thesis aims at opening the black box of internal venture selection and looking at the critical evaluation drivers and decision-making dynamics underlying selection choices at different stages of venture development. To that end, it relies on a longitudinal investigation of the actual evaluation and selection process of early-stage entrepreneurial initiatives supported by the internal corporate venture unit of a leading energy company, widely regarded as an innovation leader in its field.

The thesis can be ideally divided in two main sections. A first one, comprised of two chapters, including a review of the relevant literature on internal corporate venturing and initiative selection, which identifies clear gaps to be filled, and sets the stage for a qualitative study that inductively investigates the process through which initiatives evolve within the internal corporate selection environment. And a second one, comprised of three chapters, including a quantitative study, and its related methods and findings, that deductively tests a set of hypotheses on the determinants of venture survival at different stages, developed by waving the results of the inductive study with existing theories.

More specifically, following a description of this thesis project, its aim and underlying motivations as presented in *Chapter 1*, *Chapter 2* provides a comprehensive overview of the extant literature on internal corporate venturing, highlighting the main gaps that will be addressed in the subsequent empirical investigations. The key stages characterizing the process through which internal corporate ventures are typically initiated and developed are identified, and their distinctive features described.

A significant gap in the knowledge of a critical phase of the internal venturing process, the evaluation and selection one, is highlighted. A thorough understanding of its underlying dynamics seems to be hindered, on the one hand, by its common conceptualization as a single-stage phenomenon and, on the other hand, by the severe limitations associated with the research methodologies that have been commonly employed in the corporate entrepreneurship field to identify venture evaluation criteria and critical success factors.

While highlighting the need for a more-fine grained analyses of the process and criteria on which firms base their venture selection decisions, and suggesting the existence of

possible stage-based differences in their determinants, both the lack and appropriateness of longitudinal studies investigating venture selection are emphasized.

Our literature review sets the stage for the subsequent qualitative study described in *Chapter 3* "*Exploring the initiative evaluation and selection process: critical factors and stage-based differences*". Following an inductive approach, it aims at uncovering criteria and decision-making dynamics underlying the evaluation and selection of internal corporate ventures, and at identifying the related decision-making roles and mechanisms.

A two-stage procedure entailing first a small sample case-based analysis, and then the examination of a large set of longitudinal archival data is adopted. In the first part, drawing on an exploratory study of the development of eight corporate entrepreneurial initiatives, the constituent stages of the process through which they evolve in the internal selection environment are empirically documented. The main criteria and factors influencing venture-related decision are identified, substantially supporting the validity of those presented in the extant literature. Evidence on the relevance of deployment-related aspects to the venture evaluation and selection process is also found. In the second part of the study, a thorough examination of a comprehensive set of archived venture files unveils how the importance of the identified evaluation criteria varies between different stages of venture development.

A multistage model for venture evaluation and selection inductively derived from our qualitative investigation sets the ground for our quantitative study described in *Chapter 4, "Uncovering the determinants of initiative survival: a multistage selection perspective",* that unveils predictors of venture survival across different selection stages using a competing event history model. Evidence from our qualitative study is waved with behavioural, resource-based and cognitive arguments to develop testable hypotheses on the influence of specific venture-level (*novelty, strategic alignment* and *expected time to deployment*) and human capital or other (*proponent* and *sponsor success experience, sponsor tenure, early involvement of deployment partners*) characteristics on initiatives' rate of acceptance at different selection stages. A unique and detailed dataset of 1527 ideas collected by the corporate venture unit of our focal firm between 1996 and 2009 is leveraged to test hypothesized relationships. *Chapter 5* illustrates the empirical setting, the sampling strategy and the data used for our quantitative analyses, as well as the measures for the key variables,

and the followed estimation procedure. In *Chapter 6* results from our quantitative study are reported, unveiling significant differences in the determinants of venture selection across stages. Quite unexpected findings on the impact of sponsor tenure on initiative acceptance are explained in light of considerations derived from our direct observation of the unit's functioning. Finally, *Chapter 7* summarizes the thesis, presenting its main conclusions.

Overall the study makes three main contributions to the extant corporate entrepreneurship literature. *First*, it suggests a multi-stage perspective on venture selection that enables a more fine-grained understanding of the process through which initiatives evolve, transcending the simple dichotomization of initiative outcomes that has been dominating research on corporate venturing over the past decade. *Second,* it uncovers unexplored factors influencing selection decisions, and unveils the existence of stage-based differences in the relevance of specific evaluation criteria to the decision-making process underlying investment choices. *Third*, it contributes to identify project-related and human capital dimensions explaining venture survival through different selection gates, providing a more dynamic perspective on the venture decision-making process.

CHAPTER 2
Literature review

In this section, we attempted to contextualize the evaluation and selection of venturing initiatives within the broad domain of corporate entrepreneurship research. After clarifying conceptual boundaries, and highlighting alternative approaches to new business creation in the corporate context, we specifically focused on the internal venturing process.

Our review addressed the distinctive structural choices underlying the development of internal venturing initiatives, and the key iterative stages through which they normally evolve. A special attention was paid to the evaluation and selection stage, on which our study is focused, revealing a significant gap in the knowledge of the criteria and mechanisms decision-makers use to identify promising ventures, and of the factors influencing their survival.

2.1. Corporate entrepreneurship: definitional issues and conceptual boundaries

Scholarly interest in entrepreneurial activities within established companies has been growing dramatically over the past few decades (Burgelman, 1983; Kanter, 1985; Pinchot, 1985; Guth & Ginsberg, 1990; Zahra, 1993; 1996; McGrath & MacMillan, 2000; Sathe, 2003; Phan et al., 2009). Research has shown that corporate entrepreneurship supports growth and international leadership (Birkinshaw, 1997), and enhances competitive and financial performance (Zahra, 1991; 1995). Entrepreneurial undertakings revitalize existing organizations and make them more innovative, increasing their odds of survival over time (Ireland et al., 2009). Through the recognition and exploitation of emerging business opportunities, corporate entrepreneurial initiatives lead to the development of new products, technologies and managerial practices, to the creation of new growth platforms, and to the organizational transformation and rejuvenation (Covin & Miles, 1999).

Extant literature provides multiple definitions and conceptualizations of corporate entrepreneurship, referring to heterogeneous activities characterized by different degrees of structural autonomy, innovativeness and relatedness to existing businesses (Pinchot, 1985; Guth & Ginsberg, 1990; Jones & Butler, 1992; Block & MacMillan, 1993; Hornsby et al., 1993; Sharma & Chrisman, 1999; Narayanan et al., 2009). Sharma's and Chrisman's (1999) conceptualization of corporate entrepreneurship as "*the process whereby an individual or a group of individuals, in association with an existing organization, create a new organization or instigate renewal or innovation within that organization*" reconciles the plethora of definitions used in the field and provides an overarching illustration of the "territory" that corporate entrepreneurship covers.

A distinction relevant for the purpose of this study is the one between dispersed and focused firm-level entrepreneurial processes (Birkinshaw, 1997; 2000; Wolcott & Lippitz, 2007). A *dispersed corporate entrepreneurship* approach assumes that entrepreneurial activities are scattered all over the organization and rely on the individual efforts of employees that proactively seize new business opportunities. Companies like Google, Boeing and Whirlpool systematically encourage and allow their employees to devote part of their time to explore new ideas, providing them with substantial rewards if their ventures succeed. A *focused corporate entrepreneurship* approach, conversely, conceives entrepreneurial activities as confined to specific organizational units such as new venture divisions, programs and teams, whose mandate is to identify and nurture new valuable ideas for the whole company (Burgelman, 1983; Sykes, 1986). This approach works on the premise that some degree of structural differentiation is needed to enable organizations to simultaneously experiment with new and creative opportunities and run the existing businesses (Jensen et al., 2009). At IBM, Siemens and Exxon separate units with significant dedicated funds and expert teams have been established over the past years to identify new growth platforms, discover breakthrough opportunities and develop step-changing innovations.

Both dispersed and focused corporate entrepreneurship models can be effective, depending on the organizational culture, the management system and the structural context of the company. In large corporations, they are often concurrently adopted at different organizational levels. Each approach provides some benefits and raises specific challenges.

On the one hand, dispersed corporate entrepreneurship allows to sense a great variety of opportunities but requires a diffused entrepreneurial culture and the enforcement of proper incentive systems to work effectively. On the other hand, focused corporate entrepreneurship provides an incubation environment where creative ideas are nurtured and matured, but may become a problematic strategy when the new ventures have to be integrated or re-integrated in the pre-existing businesses (Campbell et al., 2003; Birkinshaw, 2005).

Given the focus of this study, which investigates venture development and selection dynamics in the corporate context, we mostly draw on focused corporate entrepreneurship and corporate venturing literature to identify critical gaps and potential research avenues.

2.2. Corporate venturing: internal and external approaches to new business creation

Corporate venturing, according to a broadly accepted definition, is a process entailing the creation of new businesses within established companies (Burgelman, 1984; Guth & Ginsberg, 1990; Block & MacMillan, 1993;Venkataraman et al., 1992; Sharma & Chrisman, 1999). It normally involves the development of new competencies either internally or through external sourcing modes (Zahra et al., 1999; Keil, 2004; Nambisan & Sawhney, 2007; Keil et al., 2008). Although internal and external corporate venturing strategies can serve similar objectives, the latter often raise critical appropriation concerns. Therefore, when it is feasible, firms tend to set up internal venturing programs to develop new ideas and explore new business options (Burgelman, 1985; Burgelman & Valikangas, 2005). Internal modes, which generally involve the creation of new entities residing within the corporate structure, offer both protection and coordination benefits, enabling firms to achieve synergistic effects by leveraging existing assets and competencies (Miles & Covin, 2002).

Although the present study solely focuses on internal corporate venturing initiatives, external and internal venturing approaches are often combined to support firms' attempts to reap the benefits of increasingly distributed and complex value-creation systems (Morris et al., 2008). Sykes' (1986) investigation of Exxon's corporate venturing activities shows that established companies often engage in external venturing initiatives to prove and assess the

value of new businesses, before deciding to internalize them by setting up dedicated units.

In the following section the typical structural and processual aspects of internal corporate venturing activities, as well as their traditional outcomes, are discussed.

2.2.1. Internal corporate venturing: structure, process and outcomes

Several studies have emphasized that the value of the internal corporate venturing model lies in the creation of separate organizational entities, such as incubators, new business development divisions or corporate venture units, where innovative ideas can be explored, matured and eventually turned into viable projects (Burgelman, 1985; Sykes, 1986; Block & MacMillan, 1993; Leifer et al., 2000; Birkinshaw & Hill, 2005).

Venturing initiatives, given their intrinsic risks and uncertainties, require different management logics and systems compared to mainstream activities (Kanter, 1985; Sykes & Block, 1989; Chesbrough, 2000; McGrath et al., 2006). They usually operate over a longer timeframe, and need higher flexibility and decision-making speed (Kanter, 1989; Kanter et al., 1990). Establishing structurally differentiated and semi-autonomous units, with a specific mandate to pursue new business opportunities, expedites the exploratory processes underlying most venturing endeavors (Husted & Vintergaard, 2004; Burgers et al., 2009).

Notwithstanding the benefits of this approach, many corporate venture units report unsatisfactory performances and struggle to justify their continued existence (Garvin, 2002; Birkinshaw & Campbell, 2004; Birkinshaw, 2005). Multiple and conflicting goals, insufficient management commitment to venturing activities, and absence of high-powered incentive schemes for venture unit managers, emerged as critical failure factors in several studies (Burgelman, 1985; Birkinshaw & Hill, 2005; Govindarajan & Trimble, 2005). In their recent investigation of the performance and survival of different types of corporate venture vehicles, Hill & Birkinshaw (2008) show that the units geared towards the exploitation of parent firms' assets and capabilities tend to survive longer than those playing exploratory roles. They also emphasize that alignment between the units' strategic objectives and their organizational profiles improves their ability to achieve intended outcomes, creating value for

the company.

Strategic renewal and corporate growth are the outcomes traditionally sought from corporate venturing activities (Block & MacMillan, 1993; Garvin, 2002; Bhardwaj et al., 2006). However, with the emergence of the resource-based view of the firm, indirect benefits such as capability development and organizational learning, have been increasingly attracting scholarly attention, stimulating an ongoing debate on how to rethink the notions of success and failure commonly associated to venturing programs (McGrath et al., 1994; Dougherty, 1995; Zahra et al., 1999). Drawing on a longitudinal study of the entire population of internal corporate ventures of a large electronics manufacturer, Keil, McGrath & Tukiainen (2009) shed new light on the dynamics and outcomes of internal venturing processes, showing that also ventures that do not achieve commercial success can bring significant benefits to the organization, through the development and transfer of new capabilities.

While the association between venturing activities and corporate growth and profitability (Zahra, 1991; Van de Ven et al., 2000; Burgelman & Valikangas, 2005) has been largely investigated, improving our understanding of the main antecedents and outcomes of the internal corporate venturing phenomenon, knowledge about the process through which entrepreneurial ventures are initiated and developed within established companies is still limited (Garvin, 2002; Dess et al., 2003; Narayanan et al., 2009). The long gestation period of most of these initiatives and the complexity of their underlying dynamics, which are often characterized by the interplay of formal and informal activities, has made their development process and the role of the actors involved, difficult to observe (Phan et al., 2009). Available empirical evidence is mostly based on case studies tracking single or multiple ventures within corporate settings, and depicting internal corporate venturing as a multi-phase process characterized by the uncertainty and ambiguity typical of all entrepreneurial activities (Burgelman, 1983; 1984; Garud & Van de Ven, 1992; Hitt et al., 1999).

Despite the lack of univocal agreement on their classification, the extant literatures on corporate entrepreneurship and innovation allow us to identify some key iterative stages through which internal venturing initiatives evolve (Kazanijan, 1988; Van de Ven & Polley, 1992; Block & MacMillan, 1993;Van de Ven et al., 2000; Garvin, 2002;O'Connor & Rice, 2001). In the *generation stage*, ideas for innovative technological solutions or new business

options emerge either informally or through formal creative exercises involving actors from different part of the organization. The most relevant activities at this stage of the process are opportunity identification, problem structuring and idea formulation (Leifer et al., 2000; Zahra et al., 2006; Kijkuit & Van den Ende, 2007). Initiators need to clearly point out the gaps their ideas aim to fill and the performance improvements that promise to deliver, when selling their issues to the management (Lubart, 2001). Since the nature and value of the proposed ideas are generally still vague in this phase, defining their strategic positioning through proper framing efforts can be crucial to their subsequent buy-in (Dutton et al., 2001).

In the *evaluation and selection stage*, which is normally entrenched with early development activities, ideas are reviewed and further matured through the provision of funds and experts' support. Screening is based on evaluation criteria mainly referring to market prospects, technological feasibility and company fit (Cooper et al., 1997; Shah et al., 2008). In this phase, venturing ideas are defined in greater details, making their viability easier to assess. Decisions about their acceptance are usually made by panels of senior managers with relevant technical and business experience (Burgelman, 1985). Idea initiators and champions can play a critical role in this stage, reducing the uncertainty and ambiguity surrounding ventures' technical development and prospect value, and, consequently, enhancing their odds of surviving the selection steps (Boer, 1998; Khurana & Rosenthal, 1998).

In the *implementation* stage, action plans and milestones are defined to guide the entrepreneurial ventures' execution. Decisions on whether and how to bring new products or technologies to the market and integrate them in the existing business are made. This phase carries significant investment risks, and first implementors' feedbacks help better target the new solutions to the needs of the potential adopters (Moenaert et al., 1995).

Although most of the reviewed studies conceptualize the internal corporate venturing process as a sequence of linear stages, some contributions suggest that interdependencies and feedback loops characterizing the development of new venture initiatives make a non-linear view more suitable to account for the interplay of the structural and strategic contexts within which these initiatives evolve (Burgelman, 1983; 1991; Kijkuit & Van den Ende, 2007).

Research has also recently highlighted that the knowledge and expertise required to manage the venturing process over its different phases are substantially different, and that

good opportunity identification skills don't necessarily imply proper idea selection and execution, and commercial success (Markides & Geroski, 2005; Hansen & Birkinshaw, 2007). Roles and responsibilities need, therefore, to be differentiated according to the venture development stages to be conducive to positive performance outcomes (Simon et al., 1999; Floyd & Lane, 2000; Kelley et al., 2009; Hornsby et al., 2009).

So far scholarly attention has been mostly focused on the early and later stages of the internal venturing process, addressing issues related to the generation of a steady flow of innovative ideas (Hargadon, 2002; Reid & de Brentani, 2004; Laursen & Salter, 2006; Alvarez & Barney, 2008; Girotra et al., 2010) and to the organizational set up, development and growth of new ventures (Van de Ven et al., 1984; Miller et al., 1991; Brazeal, 1993; Shrader & Simon, 1997; Heller, 1999). Selection dynamics have been only partially investigating, as well as their drivers and underlying mechanisms (Kazanjian & Drazin, 1987).

The main criteria and mechanisms firms use to evaluate and select venturing initiatives are reviewed in the following section. Critical gaps in the extant literature are identified and our intended contribution to the corporate entrepreneurship field is highlighted.

2.3. Venture selection: mechanisms, criteria and influential roles

Internal corporate ventures (henceforth "ventures", as they are commonly referred to in the corporate venturing literature) are entrepreneurial initiatives triggered by the identification of new opportunities for established companies to use or expand their resources (Kanter, 1982; Block & MacMillan, 1993; Birkinshaw, 1997; Sharma & Chrisman, 1999; Garvin, 2002). Commonly characterized as variety-enhancing experiments firms engage in to renew their portfolio of products and services, ventures involve a significantly higher risk of failure than mainstream activities, and bring more uncertain and distant outcomes (Burgelman, 1988; Jelinek & Schoonhoven, 1990; McGrath, 1999). They continuously compete with each other for management's attention and resource allocation, and only a limited number of them manages to survive the internal selection process and turn into successful stream of returns.

Although much is known about variation-generating mechanisms employed to expedite the emergence of innovative ideas (Sathe, 1988; Kuratko et al., 1990; Hornsby et al., 2002; Perry-Smith & Shalley, 2003), criteria and dynamics underlying venture selection in established organizations haven't been thoroughly investigated in the extant corporate entrepreneurship literature (Amit et al., 1993; Miles & Covin, 2002; Phan et al., 2009). Empirical evidence on how firms pick ventures to invest in is rather limited, and the evaluation lenses and screening mechanisms decision makers use to identify promising projects are only vaguely defined in most contributions (Desarbo et al., 1987; Ellis & Taylor, 1988; Cabral-Cardoso & Paine, 1996; Bordley, 1998; Eckhardt et al., 2006).

2.3.1. Selection mechanisms

Firms commonly resort to staged-investment strategies to deal with the intrinsic risks of the initiative selection process (MacMillan et al., 1985; Sahlman, 1990; Van de Ven et al., 2000). Procedures entailing regular go/no-go decisions at different phases of the venture development have been systematically used by several leading companies, over the last decade, to timely winnow out less promising ideas and divert resources to those deemed potentially more valuable (McGrath & Keil, 2007). Based on this approach, funds are assigned to proposed initiatives according to specific milestones, thus reducing the risk of investing in the wrong ventures by linking increased resource commitment to the achievement of key technical objectives, or to the involvement of valuable deployment partners (Cooper, 1990; Clark & Wheelwright, 1993, Hamel, 1999).

The stage-gate models employed in the venture selection context are structurally analogous to those traditionally applied to new product development processes (Cooper, 1993; Shaw et al., 2001; Cooper et al., 2002; Ettlie & Elsenbach, 2007). Those classify all activities ranging from "idea generation" to "post-implementation review" in sequential stages (normally from four to seven according to the organizational characteristics) regulated by specific decision-making gates where go/no-go choices are made (Kijkuit & Van den Ende, 2007). Resource commitment increases as uncertainties and risks decrease, helping

optimize product development activities and related investment decisions (Cooper, 1990).

As highlighted by several studies in the innovation field, the adoption of a stage-gate approach has helped firms dramatically improve cycle time and efficiency in their new product development activities (Cooper & Kleinschmidt, 1987; Griffin & Page, 1996). Over time, the focus of attention has progressively moved from aspects related to project execution to the front end of the new product development process (Shenar et al., 2002; Verworn et al., 2008). It is increasingly recognized as the phase where "*the greatest opportunities for improving the overall innovation process lie*" (Backman et al., 2007).

Typically represented as a single ideation stage, the fuzzy front end is an iterative and complex process covering all time and activities prior to the first screen of a new product idea, and ending with a go/no-go decision about the formal execution of a project (Smith & Reinertsen, 1991; Murphy & Kumar, 1997; Khurana & Rosenthal, 1998). Aspects of opportunity structuring/identification, idea development and evaluation are usually involved in the front-end phase of a new product development process (Urban & Hauser, 1993; Leifer et al., 2000; Crawford & Di Benedetto, 2003; Reid & De Brentani, 2004). The underlying objective of those activities is reducing the uncertainty surrounding a proposed idea to such an extent that its adherence to a set a pre-determined selection criteria can be assessed, and a decision about its execution can be made (Moenart et al., 1995; Kim & Wilemon, 2002).

Front-end activities closely resemble those normally characterizing the evaluation and selection of new venturing initiatives. However, while the front-end process ends with a single go/ no-go decision at the initial idea screen, where the first formal idea evaluation occurs, venture selection usually entails multiple screening interfaces before decisions about project execution are actually made. In fact, although extant corporate entrepreneurship literature still mainly conceptualizes venture selection as a single-stage phenomenon (Burgelman, 1991; Lovas & Ghoshal, 2000), similarly to the fuzzy front end, increasing anecdotal evidence on the existence of multiple and differentiated selection interfaces exists.

2.3.2. Evaluation criteria and influential roles

A major challenge in selecting entrepreneurial initiatives is related to the assessment of their value-creating potential. Most corporate ventures operate with high-technology products whose outcomes are projected so far in the future that decision-makers, when evaluating their embryonic proposals, can only have a vague idea of the technical hurdles, potential revenues and critical resources needed to deploy them (MacMillan et al., 1987; Bazerman, 1998; Forlani & Mullinis, 2000; Huchzeirmeir & Lock, 2001; Keh et al., 2002).

To lower the risk of either rejecting valuable ideas or supporting initiatives with limited market prospects and innovative potential, established firms normally base their venture selection decisions on some key predetermined criteria reflecting both the venture's possible fit with the corporate strategic goals, and more specific project-related characteristics such as technical feasibility, customer value or expected revenue streams (Knight, 1988; Roussel et al., 1991; Block & MacMillan, 1993; Elfring & Foss, 2000). Assessing initiative consistency with the defined selection criteria is complicated by the uncertainty and ambiguity surrounding most entrepreneurial projects especially in the early stage of their development (Daft & Lengel, 1986; Hall & Hofer, 1993). Over time, ventures' technical and business-related aspects become more clearly defined, enabling more reliable estimates of the required investments, development time and market potential (Pinto & Prescott, 1988).

Most studies analyzing factors driving venture selection decisions emphasize the need for firms to rely both on general and specific selection criteria when making their investment choices (Desarbo et al., 1987; MacMillan et al., 1987; Block & MacMillan, 1993). General criteria are those whose centrality to the decision-making process is independent on the nature of the investment, and that normally reflect the potential fit between the proposed venturing initiative and the corporate strategy (MacMillan et al., 1985). Specific criteria conversely are those reflecting distinctive aspects of the entrepreneurial project, such as the consumer needs it aims to fill, or its technological profile and market prospects (Block, 1982).

Strategic considerations normally dominate the evaluation of internal venturing initiatives (Burgelman, 1984). Prior research has argued that ventures' expected relevance to corporate development and their degree of relatedness with mainstream activities drive firms'

selection and deployment choices (Sorrentino & Williams, 1995; Thornill & Amit, 2001). Empirical evidence shows that although feasible and potentially valuable, entrepreneurial initiatives may end up being rejected if poorly aligned with the strategic goals of the organization (MacMillan, 1983; Meyer & Heppard, 2000). Decision-makers tend to select in ventures that are consistent with the defined corporate strategy and practices, while those only partially fitting with firms' core business areas and strategic priorities aren't normally getting any resource commitment without sustained championing and political maneuvering efforts (Starr & MacMillan, 1990; Greene et al., 1999; Leifer et al., 2000; Rietzschel et al., 2010).

An extensive body of research has also addressed the issue of *relatedness* between ventures' and parent companies' activities (Von Hippel, 1977; Roberts & Berry, 1985; Sykes, 1986). Pursuing highly related initiatives, entailing the introduction of products and technologies complementing firms' existing portfolios, allows firms to leverage their key competencies and achieve synergistic value (Fast, 1979; Sykes, 1986; Chesbrough, 2002). Based on these considerations, several authors have argued that a close fit between parent and ventures companies' business domains is highly desirable, and that related ventures should be preferred over unrelated ones (MacMillan et al., 1987; Miller et al., 1991; Dougherty, 1995). On the other hand, a few studies have shown that high relatedness doesn't necessarily lead to better venture performance, and that a tight coupling between ventures' and parents' activities may hamper their attempts to explore new areas and develop truly innovative solutions (Burgelman, 1985; Schildt et al., 2005).

The *stage of technology development* and the *degree of novelty* of proposed initiatives have been acknowledged as further influential selection criteria (Hambrick & MacMillan, 1985; Kanter et al., 1990). Ventures targeting technologies already under development seem to be more attractive than those aiming at the introduction of brand new solutions or relying on mature ones, which promise uncertain or limited stream of returns (Desarbo et al., 1987). Similar considerations apply to initiatives' degree of novelty. Ventures can center on incremental improvements of existing solutions or entail more radical changes requiring the development of new competencies. Projects targeting the development of path-breaking technologies or the exploration of new business domains, as options for firms to diversify their growth trajectories, only occasionally survive internal selection processes (Burgelman,

1983; Burgelman & Sayles, 1986; Block & MacMillan, 1993). Since they are generally perceived as risky investments with a higher probability of failure than more conservative initiatives, organizations are reluctant to support ventures grounded on new knowledge domains (Sitkin & Pablo, 1992; Smith, 1999; Garcia et al., 2003).

Innovation literature, conversely, highlights that to enhance their competitive standing firms increasingly invest in leading-edge projects and experiment with emerging technologies (Leifer et al., 2001; Ahuja & Lampert, 2001; Rosenkopf & Nerkar, 2001; Govindarajan & Trimble, 2005; O'Connor & De Martino, 2006). Novelty, together with related feasibility aspects, it is one of the most critical driver of venture selection according to several studies in this field (Thieme et al., 2003; Ettlie & Elsenbach, 2007; Verworn, 2009). However, since the uncertainty surrounding venturing projects increases with their degree of novelty, ideas' initiators and sponsors have to be extremely effective in conveying to decision-makers the differentiating functionalities of the new solutions to secure their support (Rice et al., 2008).

A parallel issue is related to the *IPRs* on the new technological solutions developed through the systematic engagement in venturing activities (Cooper, 1979, MacMillan et al., 1987). The possibility to employ proprietary technologies significantly influences the attractiveness of a venture, since it reflects its degree of insulation from possible competitive attacks (Sykes, 1986). A low competitive exposure normally enhances a venture's chances to be selected in. Analogous considerations hold for the *time to profitability* criteria. Ventures whose expected time to profit is long are normally perceived as riskier, since chances to get dominant market positions based on the introduction of innovative solutions are more likely to be missed, compromising potential stream of returns (Calantone & Di Benedetto, 2000).

Factors that clearly matter to the evaluation of a venturing initiative are those related to its *potential returns* and *investment size* (Desarbo et al., 1987). Ventures with higher gross margins on revenues are considered less vulnerable to unexpected losses, since margins act as a safeguard against risks (MacMillan et al., 1985). Similarly, initiatives requiring a limited investment in relation to total assets are deemed more viable and less risky to undertake compared with those entailing huge resource commitment (Riquelme & Rickards, 1992).

A final element widely acknowledged as critical to selection decisions is related to the *attributes of venture proponents and champions* (Maidique, 1980; Howell & Higging, 1990).

Empirical evidence shows that the quality of the idea proponent, judged mostly in terms of previous experience or track-record, is a key determinant of initiative funding decisions (MacMillan et al., 1985; O'Connor & Rice, 2001; Green et al., 2003; Kelley et al., 2009). Experience provides proponents with critical skills to successfully navigate the evaluation process, and a good track-record signals their ability to shepherd ventures through their different stages (Girotra et al., 2010). An analogous influence on decision makers' venture evaluation processes is attributed to champions' experience, although more related in this case to their internal and external networking ability (Garud & Van de Ven, 1992; Day, 1994).

Turning an idea into a concrete project requires heterogeneous resources and expertise, and in order to get them, idea proponents and champions need to develop novel interaction patterns and leverage their informal network relationships (Floyd & Wooldridge, 1996; Greene et al., 1999; Kuratko et al., 2005; Howell et al., 2005; Kelley et al., 2009). Sponsors and innovation brokers can help them connect with knowledgeable people in the organization, integrate information from different internal sources, and gain access to the resources they need for their ventures (Maidique, 1980; Tushman & Nadler, 1986; Jelinek & Schoonhoven, 1990; Garud & Van de Ven, 1992; Day, 1994; Hargadon, 1998; Hornsby et al., 2009).

While providing a comprehensive overview of the key factors influencing venture selection decisions in the corporate context, our thorough examination of extant literature revealed that they were almost exclusively derived from studies employing experimental and post-hoc research methods (MacMillan et al., 1987; Desarbo et al., 1987; Riquelme & Ricards, 1992; Hall & Hofer, 1993; Petty & Gruber, 2011). This may pose severe limitations to the accuracy and validity of their results, due respectively to possible oversimplifications of the investigated venture-related decision-making context (Heckman & Smith, 1995) or to the self-reporting biases traditionally associated with the use of post-hoc methods. Since in several cases, evaluation criteria underlying initiative selection processes were identified by simply asking decision-makers to list and rank the ones they based their choices on, it is impossible to rule out the potential impact of retrospective biases.

Also experimental methods, although overcoming these critical flaws by using data gathered in real-time, show some limitations. They allow to only partially account for the

complexity characterizing venture selection decisions, requiring researchers to include in their studies factors deemed relevant to the evaluation process, which may also not be representative of those critical to actual venture selection decisions. These considerations, on the one hand, contribute to explain why despite the acknowledged centrality of selection issues to venture success, we still have a limited knowledge of the phenomenon and its dynamics. On the other hand, they urge the need to overcome the highlighted methodological limitations, and pursue studies exploring the actual dynamics underlying venture evaluation and selection decisions in the corporate context (Phan et al., 2009; Petty & Gruber, 2011).

A further limitation of most studies on corporate ventures, which partially stems from the research methodologies commonly used in this research stream, is their focus on successful and mature initiatives, omitting early-stage ventures from consideration (Cooper & Kleinschmidt, 1987; Zirger & Maidique, 1990). Critical success factors are solely derived from the comparison of initiatives that achieved commercial success with those that didn't. This implies that ventures initiated but discontinued along the way are not taken into consideration, overlooking important information about factors influencing their development process, and misrepresenting their performance (Shortell & Zajac, 1988; Zahra, 1996).

Although there are reasons to believe that factors shaping selection patterns vary according to ventures' stage of development, evidence about the different relevance of the identified evaluation criteria to different stages of the decision-making process is lacking (Pinto & Prescott, 1988; Rietzschel et al., 2010). This is due both to the methodological limitations we have previously highlighted and to the single-stage conceptualization of the initiative selection process that still dominates extant literature on corporate entrepreneurship.

2.4. Concluding remarks

This chapter aimed at a providing a comprehensive overview of the extant literature on internal corporate venturing moving from the key distinction between dispersed and focused corporate entrepreneurship. Common challenges and critical issues established firms face in managing their internal venturing activities were highlighted. The key stages characterizing

the process through which entrepreneurial ventures are typically initiated and developed were identified, and their distinctive features described.

A significant gap in the knowledge of a critical phase of the internal venturing process, the evaluation and selection one, emerged. A thorough understanding of its underlying dynamics has been hindered, on the one hand, by its common conceptualization as a single-stage phenomenon shaped solely by top management selection logics, and, on the other hand, by the limitations posed by the research methodologies commonly used in the field to identify venture evaluation criteria and critical success factors.

The increasing anecdotal evidence on the existence of multiple and differentiated selection interfaces in the corporate context, urges a more fine-grained analysis of the processes and criteria on which firms base their venture selection decisions, and of the role played by key organizational actors in influencing related outcomes. Moreover, as highlighted by several recent studies (Phan, et al., 2009; Petty & Gruber, 2011) more longitudinal as well as case study research is required to develop a better understanding of the nature and dynamics of venture selection, a process that is more iterative than traditional models suggest.

Our literature review set the stage for subsequent empirical investigations. The qualitative study described in chapter 3 has been purposefully aimed at uncovering criteria and dynamics underlying the evaluation and selection of internal corporate ventures, and at identifying the related decision-making roles and mechanisms. In particular, through a case-based analysis it will empirically document the stages characterizing the process through which initiatives evolve in the internal selection environment, and will identify the criteria on which actual selection decisions are based. It will then unveil potential stage-based differences in the relevance of the identified evaluation criteria to the decision-making process through the analysis of a comprehensive longitudinal set of archival data.

CHAPTER 3

Exploring the initiative evaluation and selection process: critical factors and stage-based differences

Given our interest in gaining a more in-depth understanding of the drivers and dynamics underlying venture selection processes in established contexts, a qualitative study was conducted (Miles & Huberman, 1994). It is appropriate to the investigation of phenomena such as new venture development since it allows to trace processes as they unfold over time (Gartner & Birley, 2002; Dess et al., 2003; Phan et al., 2009; Narayanan et al., 2009).

The long-time horizon of most venturing initiatives, which may limit chances to follow their entire maturation path, induced us to collect data on innovative projects at different stages of their development, to capture relevant selection issues and process-related aspects (Burgelman, 1983). Eight venturing projects supported by the internal corporate venture unit of a leading energy company were examined, and their complete case-histories were developed. Project selection took account of various business domains, intended deployment strategies, and survival outcomes. The exploratory nature of the study, aiming at developing an encompassing view of internal venture selection, underlies the choice of an inductive approach (Strauss & Corbin, 1990). Venture selection is still a poorly documented phenomenon, mostly due to the lack of large scale empirical research at the project level.

After clear patterns and elements defining the initiative selection process emerged from our interview data analysis, and an initial inductively derived stage model for venture evaluation was defined, archival sources were used for triangulation purposes (Jick, 1979). Additional selection drivers and stage-based differences were identified through the content analysis of semi-structured minutes related to the evaluation process of the entire population (1,527) of EP-related venturing ideas submitted over the time window 1996-2009.

3.1. Research setting

We investigated the evaluation and selection process of entrepreneurial projects[1] in a major oil company, which we shall call ENERCO. It is widely regarded as an innovative leader in its field, successfully introducing leading-edge technologies and investing in frontier regions.

Like most oil majors, ENERCO has recently reacted to the strong competition of national oil companies, and to the need to develop enhanced recovery techniques for difficult-to-access oil reserves, by putting a renewed emphasis on new technology development. Combining internal and external modes to pursue innovative technological solutions has been the pattern followed by most oil companies over the last decade. However, their strategies to cope with those emerging challenges in the oil industry that have been triggering sustained investments in new domains, have been quite different (ENERCO and Texaco have mainly focused on exploring mature oilfields, while BP has been mostly targeting frontier regions).

In 1996, to nurture the development of early-stage entrepreneurial ideas that would have hardly found a place in the company to be tested and tried out, ENERCO set up an internal corporate venture unit, which we shall call Great Coalition (GC). The underlying objective was to give creative people an outlet to initiate innovative ideas, enabling the company to access and exploit new valuable opportunities.

The GC unit, made of full-time professionals with varied backgrounds in the business and technology of the energy industry, acts within ENERCO as an orchestrator, assembling teams of experts and industry players to evaluate the potential of new business and technology development projects, and their possible deployment routes.

Ventures relevant to GC and, hence, eligible for funding, are internally or externally originated ideas whose implementation has a potential to provide a significant profitability gain or open up new growth opportunities for ENERCO. Proposed ideas, which can be either extensions of the core business or white spaces (entirely new businesses), are reviewed, matured, and nurtured until they reach a technically working proof of concept, with a staged decision-making approach similar to that of a venture capitalist. Possible deployment options

[1] Although we acknowledge that there is a difference between the terms “initiative”, “project” and “venture”, for simplicity, they will be used as synonyms in this thesis.

for projects reaching the final stage of the selection and validation process, include internal development, licensing, partnering or the creation of new small and independent companies[2].

Information on the functioning and structure of the GC unit, its role within ENERCO and its relationships with other company divisions were obtained. In particular, the connection between GC and ENERCO's internal venture capital fund was studied, as well as the linking mechanisms employed to facilitate coordination between GC and other corporate R&D units. The key roles and interaction patterns within the GC group were investigated, and the most critical venture development and deployment activities were identified.

The main focus of our investigation was on eight venturing initiatives[3] which reached different stages in the GC selection process, and whose intended deployment strategies ranged from licensing to internal modes. Although all targeting upstream activities in the oil business, the selected projects covered different technological domains. For confidentiality reasons their names have been altered, and in the description of the underlying technologies and methods the most salient and differentiating technical aspects have been overlooked (see the data collection section for more details on the respondents' roles and interview protocol).

Down-hole Perforator (DP) was initiated in 2003 with the aim of developing an entirely new perforation technique to the Exploration and Production (EP) business. It survived various selection gates and was finally approved in 2007. An external deployment strategy was pursued, and ENERCO started a joint-venture with an external manufacturer to commercialize the new related technological products. Three people involved in this project were interviewed, some several times, between 2008 and 2009.

The initial idea for the *Well logging* (WL) venture was submitted to GC in 1996. The main objective was the development of a new method combining logging and casing operations. The proposed project progressed relatively smoothly through the GC review and funding interfaces and a prototype of the new logging tool was available already at the end of 1999. However, it took some time to get field trials arranged and reach the proof-of-concept

[2] These last three deployment options are normally defined as "external deployment strategies".
[3] Selected initiatives are representative of all possible selection and deployment outcomes and cover different technological domains in the upstream oil business.

stage. In 2005, to commercialize the different technological solutions originated from the initial project, that in the meanwhile had been taken over by ENERCO's internal corporate venture fund, a new company was founded. Four people involved in this project were interviewed, some several times, between 2008 and 2009.

The *Sand Management* (SM) project was initiated in 2002 with the aim of developing an alternative completion technique to improve sand control and minimize lost hydrocarbon production. The idea was well enough conceived since the beginning, so it easily survived the initial screening gates. However, getting field trials arranged posed huge problems as well as defining a convincing commercialization strategy. Despite several attempts to move the project forward, it was officially closed in 2005 without reaching the final stage of the GC process. Three people involved in this venture were interviewed between 2008 and 2009.

The idea for the *Hydrocarbon Sensing* (HS) venture arose from an interdisciplinary cross-business dialogue between an ENERCO's downstream R&D laboratory and an upstream research group. It was submitted to GC in 1997 with the aim of introducing a fundamentally different approach to gas detection employable in frontier exploration. Despite encouraging results from the initial experiments in 1998, the project progressed very slowly through the GC selection and validation process. Potential deployment partners were not seeing it as attracting enough, and this slowed down the pace of its field tests. The "sniffer" project eventually reached a successful proof-of-concept in 2005 and was taken up by an internal unit. Three people involved in this project were interviewed between 2008 and 2009.

The *Shut-off Fractures* (SF) project aimed at introducing a new method using cheap chemicals to seal fractures in carbonate reservoirs. The original idea was submitted in 2005 and the initial tests were run already after a few months with positive results. The low perceived market value, however, made it difficult for the project to the reach the final stage of the GC review process. To date, it is still open and under review, but further resource commitment is contingent on the achievement of successful field trials, and on the identification and involvement of potential external deployment partners. Three people involved in this project were interviewed, some several times, between 2008 and 2009.

The *Stochastic Dynamics* (SD) idea was submitted to GC in 2000. It suggested a new method to predict reservoir behaviors, with realistic uncertainty estimates, based on a new approach to stochastic analyses. Given the huge potential business impact, the project easily moved forward through the initial selection interfaces. However, emerging technical hurdles, and the amount of resources required to fix them, slowed down its pace of development. Nevertheless, a successful proof-of-concept was achieved in 2003 and the new modeling technique was quickly taken up by the business divisions. Five people involved in this project were interviewed between 2008 and 2009.

The *Gas Decontamination* (GD) project aimed at introducing a novel method to treat contaminated gas using the thermodynamic conditions already present in the reservoirs. The initial idea was submitted in 2004 and a small amount of money was quickly assigned to the research team to develop a detailed experimental program. Laboratory tests started in 2005 and additional funds were granted over the following years based on the progressive achievement of defined milestones. Despite positive intermediate results, technical issues and the lack of a clear opportunity framing showing the advantages of the proposed method compared with competing technologies, delayed the venture execution. To date, the project is still open and under review. Three people involved in this venture were interviewed, some several times, between 2008 and 2009.

The idea for the *Radar Imagery* (RI) venture was submitted to GC in 2001. The proposed solution aimed at improving the assessment of the hydrocarbon potential of new large unexplored areas. Despite its prospect significant impact on the EP business, the novel method lacked unique and revolutionary features, and required capabilities not easily available within the company. Therefore, after surviving the first screening gate, the initiated project was soon terminated and there was no further resource commitment. Three people involved in this venture were interviewed, some several times, between 2008 and 2009.

3.2. Data collection

Our data collection strategy entailed multiple and differentiated sources of information, and dealt with both qualitative and quantitative data, thus increasing the reliability and internal validity of the study. To develop a thorough understanding of the structure and role of the GC unit within the company, between February and April 2008 we conducted a first round of open-ended interviews with GC administrators, a few corporate R&D managers and senior executives from other units involved in the GC venture selection and development process. They helped us capture the process-related aspects of GC's venturing activities, and identify the main evaluation criteria driving decisions about the implementation of innovative initiatives. Moreover, they facilitated the identification and contact of target respondents for a second round of interviews, which took place between February and June 2009 (Table 3.1 indicates the distribution of respondents over job categories for both rounds of interviews).

Table 3.1 Distribution of respondents over job categories and roles

Respondent	Position	Interview date	Interview duration	Number of interviews	Interview focus
DM	○ Group strategy and GC senior manager	04-21-08	1h:18 min	1	Role of GC within ENERCO, GC process, link with long-term strategy, involvement of business counterparts
AM	○ EP research manager	04-21-08	1h:04 min	1	ENERCO' technology strategy, GC role and process, venture deployment issues
EV	○ Managing director of ENERCO's Internal Venture Capital Fund	04-22-08 02-11-09 02-18-09	1h: 13 min 1h: 22 min 1h: 03 min	3	- The role within ENERCO of its Internal Venture Capital Fund and of the GC unit - Venture selection criteria and deployment activities -New venture creation, informal networking and inventors' involvement in deployment activities

GB	○ GC senior manager and sponsor for a number of ventures including some of the more in-depth investigated projects	02-12-08 02-26-08 03-13-08 03-27-08 05-16-08 07-09-08 03-03-09 03-16-09 03-30-09 05-21-09 06-18-09	1h: 25 min 1h: 02 min 1h: 11 min 1h: 41 min 1h: 17 min 1h: 55 min 1h: 41 min 1h: 17 min 1h: 55 min 1h: 17 min 2h: 05 min	11	- Industry evolution and ENERCO's technology strategy, GC role and process overview, venture selection criteria and survival rates, deployment strategies and involvement of operating units - As concerns those ventures he was sponsor for, the focus of the interviews was on the project's origin and development, its technical and business potential, and the proponent's and sponsor's support to its successful deployment
WS	○ GC senior manager and former director	04-23-08 02-12-09	1h: 01 min 1h: 08 min	2	-The evolution of the GC unit and of the related venture development process over time -The main lenses used to evaluate new ventures , the valley of death and critical deployment issues
LR	○ GC senior manager and former director + sponsor for a number of ventures including some of the more in-depth investigated projects	02-12-08 02-27-08 03-14-08 03-28-08 05-17-08 03-04-09 05-20-09 06-18-09	1h: 03 min 1h: 16 min 1h: 07 min 1h: 34 min 1h: 01 min 1h: 42 min 1h: 59 min 1h: 23 min	8	- Industry evolution and ENERCO's technology strategy, GC role and process overview, venture selection criteria and survival rates, deployment strategies and involvement of operating units - As concerns those ventures he was sponsor for, the focus of the interviews was on the project's origin and development, its technical and business potential, and the proponent's and sponsor's support to its successful deployment
AK	○ Manager of Rock and Fluid Physics and for Experimental Research ○ Lead proponent of the DP project	04-22-08	1h: 11 min	1	Proponent background and experience, project overview and development process, key technical hurdles and business consideration, venture deployment
TA	○ Team Leader for geo-mechanics ○ Lead proponent of the SM project	03-11-09	1h: 03 min	1	Proponent background and experience, project overview and development process, key technical hurdles and business consideration, venture deployment

JR	○ Wells Technology Group ○ Lead proponent of the WL project	03-11-09 05-12-09	1h:12 min 1h:01 min	2	Proponent background and experience, project overview and development process, key technical hurdles and business consideration, venture deployment
HP	○ Exploratory research senior scientist ○ Lead proponents of the SD project	04-21-08	1h: 12 min	1	Proponent background and experience, project overview and development process, key technical hurdles and business consideration, venture deployment
SD	○ Exploratory research young scientist ○ Lead proponents of the SD project	04-21-08	1h: 02 min	1	Proponent background and experience, project overview and development process, key technical hurdles and business consideration, venture deployment
JVE	○ Chemical Engineer ○ Lead proponent of the SF project	04-23-08 02-11-09	1h: 04 min 1h: 12 min	2	Proponent background and experience, project overview and development process, key technical hurdles and business consideration, venture deployment and incentive issues
BH	○ Principal Scientist in EP Potential Fields and Remote Sensing Field ○ Lead proponent of the HS project	02-10-09	1h: 26 min	1	Proponent background and experience, project overview and development process, key technical hurdles and business consideration, venture deployment
GB2	○ GC senior manager ○ Lead proponent of the RI project	04-23-08 02-11-09 06-27-09	1h: 25 min 1h: 16 min 1h: 33 min	3	-Critical issues in venture development and how to build an enhanced value proposition for submitted projects - Proponent background and experience, project overview and development process, key technical hurdles and business consideration, venture deployment
MG	○ Senior scientist in Exploratory Research ○ Lead proponent of the GD project	04-23-08 02-10-09	1h: 09 min 1h: 12 min	2	Proponent background and experience, project overview and development process, key technical hurdles and business consideration, links with external partners for joint venture development and deployment
MR	○ GC senior manager and sponsor for a few ventures	02-11-09	1h: 03 min	1	Sponsor background and experience, project overview and development process, key technical hurdles and business consideration, linking activities with operating units and external partners

KM	○ GC senior manager and sponsor for a few ventures	02-10-09	1h: 22 min	1	Sponsor background and experience, project overview and development process, key technical hurdles and business consideration, linking activities with operating units and external partners

In the second stage of our interview process, which was more focused on venture-level dynamics and critical selection drivers, in addition to the proponents and sponsors of the eight selected venturing initiatives, we also interviewed a few GC panel meeting members. Following a semi-structured protocol, we asked respondents to trace the evolution over time of the ventures they were involved in (from their genesis to the final stage they reached), to identify the key issues they had to face throughout their development process and, the capabilities and organizational links that enabled successful deployment outcomes. This approach allowed us to assess the convergence and divergence in their views on several critical aspects of the venture selection dynamics. It was supplemented by our direct participation in multiple screening and extended review panel meetings, during which we were able to observe and capture information on the GC decision making process.

All in all, 42 interviews were conducted, mostly on site. They lasted from 1 to 2 hours, and were tape-recorded, transcribed and, subsequently, submitted to respondents for verification. Follow-up questions were asked by phone or e-mail when needed. We tried to ensure data triangulation and control for retrospective biases by using different data sources and having multiple informants (Graebner & Eisenhardt, 2007). Internal documentation (including technology portfolio reviews, business plans, project plans, budgets, and minutes of panel meetings) was reviewed to get specific information about each of the investigated ventures but also on the GC unit's project portfolio and overall activity. Information from several archival sources, including scholarly publications, were systematically used to back up interview data (see Table A.1 in the appendix for further details on data collection).

A final stage of our data collection process took place between January and May 2010, and entailed the download from the GC central electronic database of all the minutes related to the evaluation process of the EP-related new ideas submitted to the GC unit between 1996

and 2009. Thousands of semi-structured emails reporting discussions about the specific characteristics and potential value of more than 1,500 ventures were collected and subsequently analyzed to identify clear selection patterns and relevant drivers.

3.3. Data analysis

Our analysis aimed at uncovering key drivers and dynamics of corporate venture selection processes. We first conducted an accurate coding of the interviews (Miles & Huberman, 1984) searching for passages that contained references to the nature, structure and salient decision-making milestones of the GC's process of evaluation and selection of innovative ideas. At this stage our goal was to identify the main screening interfaces, the key mechanisms used by GC to nurture and mature selected ideas, and the most common technical hurdles and value-creating conditions that proposed initiatives needed to satisfy before decisions about their final implementation could be actually made.

The second step was aimed at the identification of the main criteria driving selection across the multiple venture development stages, and of the factors influencing projects' advancement, re-direction or rejection, and their related deployment routes. Discriminating variables emerged from the systematic search of analogies and differences across the eight in-depth investigated cases (Eisenhardt, 1989; Langley, 1999). Subsequent readings of our data were devoted to merging concepts into more general conceptual categories in order to gradually move from our informants' account of specific new venture selection and development processes to more general phenomena.

A further step, which stemmed from the outcomes of the previous ones, and aimed at providing evidence on additional venture selection drivers and possible stage-based differences, entailed the content analysis of the semi-structured minutes related to the evaluation process of 1,527 venturing ideas submitted to the GC unit since its creation to the end of 2009. Widely acknowledged keywords[4] were used for each of the identified selection

[4] Keywords were defined by matching those normally associated to the identified selection driver both in the minutes and in the relevant literature.

drivers, and their frequency was assessed across the key stages of the venture development process, reflecting the drivers' specific relevance to the different evaluation and selection gates (see Table 3.2). For standardization purposes, the sum frequencies of the defined keywords were then divided by the total number of words of the projects' minutes.

Table 3.2 Keywords for project minutes' content analysis

Key words for selection drivers and coding examples	
Selection drivers and keywords	Examples from project-related minutes
Novelty: novelty, novel, game-changing, breakthrough revolutionary, evolutionary, incremental, radical	**AED:** "It has been concluded that this project is clearly revolutionary for Enerco" **SCFM**:"While your idea may be game-changing, it is hoped that through discussion in the extended panel, GC will be convinced of its novelty" **DEM:**"This is a novel technology capable of improving our smart well and 4D monitoring capability significantly " **FOM:**"In summary your idea to develop the EK technology is radical, challenging, and certainly of very high business value"
Strategic alignment: fit, core domains, core areas, core business, relatedness, related	**EFG:** "All consider the potential value of the project to our EP core areas as a sufficient justification for further exploration" **GYU:** "The potential value to Enerco is clear for this proposal, which in principle fits our core strategy" **LGT:** "We don't believe there is sufficient potential application of the new tool within Enerco' s core business to justify continued development funding by GC" **DSM:** "In the first instance, I believe that the method is currently potentially valuable in our core areas, where we increasingly struggle with the need to figure out whether there are any hydrocarbons in our targeted remote fields"
Time to deployment: speed, implementation time, time to field trials, time to commercialization, acceleration	**PDO:** "What we have some concerns about is helping your project reach the proof of concept, accelerate its take up, and get it into the business in 12 months rather than years" **DSM**: "A critical issue for us is the acceleration of the project towards the arrangement of field trials and through the engagement of more people to achieve critical mass" **TYU**: " The potential added value of your idea is well recognized, although implementation is considered very difficult and requiring a long time" **FGT**: " As we discussed at the meeting, GC requests that you speed up the field testing of your technology before making a final decision. It is important to more clearly identify the scope of the business opportunity inherent in the new perforation system"
Deployment : commercialization, deployment, implementation, field trials	**DEM:** "The X-unit has indicated willingness to be an alpha customer for the project, and is keen on acting as focal point to take the idea to a potential field trial" **GYU**:"Involvement from other parties needs to be discussed to bring this idea to implementation" **LGT**: "We believe that the technology should be directed towards commercialization, as its uses do not directly fit with the current exploration portfolio of Enerco (i.e. frontier areas with no proven hydrocarbons)" **ZGH**:"I understood that a licensing option was recommended as the commercial path forward. You will need to clearly articulate this option to the Funding Panel as well as provide some financial projections in order for the Funding Panel to support this commercial strategy, and move the project forward"

3.4. Venture evaluation and selection at GC: a differentiated and multi-stage process

In their search for new growth platforms and business opportunities, firms put increasing efforts on sourcing and generating innovative ideas. Technology scouting, lead-users involvement, trend-spotting and kaizen sessions, are just some of the several mechanisms that firms currently employ to come up with new creative and value-adding solutions. However, having a good track-record at idea generation doesn't necessarily imply for firms to be equally good at turning embryonic projects into successful commercially available solutions. As one of our informants pointed out, "*idea generation is easy. The difficult part is to convert ideas into valid business propositions and take them to the market*" (DM,GC senior manager).

Most corporate screening and funding processes are often inadequate to discriminate between potentially valuable and low-quality initiatives, substantially compromising firms' innovation capabilities and outcomes (Markides & Geroski, 2005; Hansen & Birkinshaw, 2007). Budget constraints, strict funding criteria and managerial cognitive biases lead to the rejection of many novel and high-risk projects, hampering employees' creative efforts. On the other hand, some companies are systematically facing overflows of new initiatives which absorb critical organizational resources but end up going nowhere due to the lack of effective screening mechanisms (Hamel, 1999; Govindarajan & Trimble, 2005).

Designing evaluation and selection processes enabling the identification and development of new solutions fitting in the overarching corporate strategy, and attracting organizational support is increasingly a priority for firms willing to enhance their innovative performance. *"Selection is a very important ability since only very few ideas can turn into something successful, and you have to be able to stop project development if you think that is not going to deliver enough value. The successful entrepreneurial companies are those able to stop things very efficiently*", A.M., an EP research manager at ENERCO, reported.

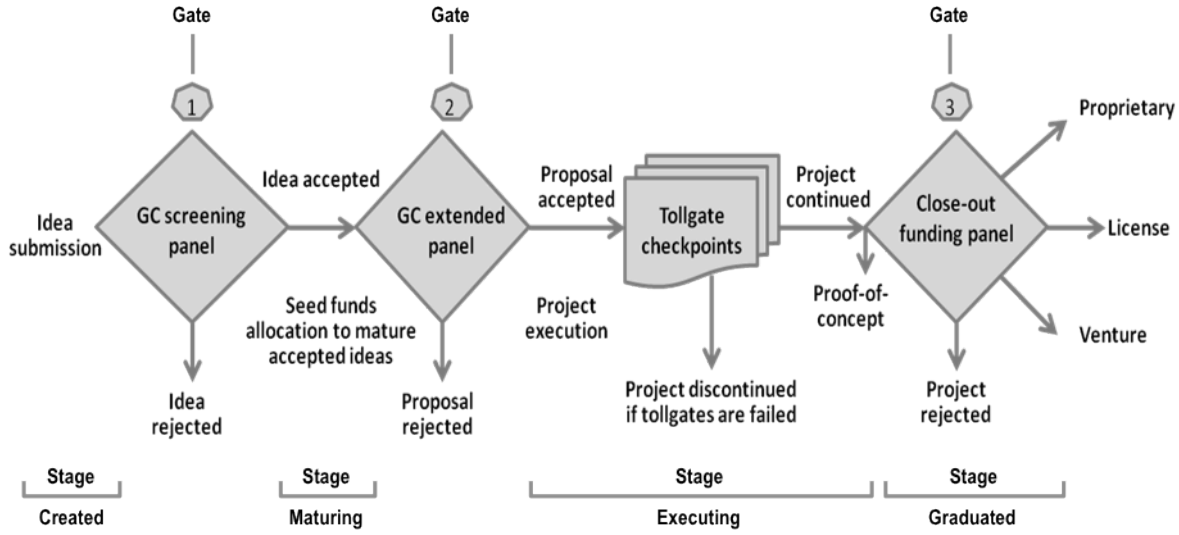

Figure 3.1 GC stage-gate venture development process (ENERCO's internal document)

At ENERCO's GC unit, the development of new ventures follows a disciplined staged decision process (see fig 3.1). "*A screening panel defines which ideas, out of those submitted, deserves to be further matured. Those surviving this initial selection gate, get around 20.-25.000 $ to cover initial research expenses. The project leader, which is normally the idea proponent, is supported by a GC sponsor who helps him connect with knowledgeable people within the organization. An extended panel, involving experts from ENERCO or other companies, evaluates the technical feasibility and the business value of the proposed projects, and decides whether to allocate funds for their further development, or to discontinue them. On average it takes 4-5 years for a project to reach the proof-of-concept, which is the final stage of the GC venture development process*"(G.B., GC senior manager).

Idea selection is based on peer reviews. Project milestones are formally set up and clear deliverables are defined for each tollgate (a pre-determined point where idea proponents and panel experts discuss the progress achieved and the possible way forward). "*Idea proponents get some questions they need to solve about technical development, business value and deployment strategy. As their knowledge about the project increases, they need to become more specific in addressing those issues. If their ideas fit with the general directions, they are*

given space"(L.R., GC senior manager and former director). For all ideas that survive the screening review and are meant to be further developed, GC nominates 2-3 extended panel members that evaluate, jointly with invited experts, the viability of the proposed ventures. *"Success factors, project plan, business value and deployment strategy are discussed at this stage. They are much better defined than at the screening phase, therefore enabling the panel to have a much clearer understanding of the technical hurdles and commercial value. Resources are allocated based on the proposed project plan which is usually revised according to the panel's recommendations. Toll-gate meetings are then organized to check on the subsequent idea's development steps until a proof-of-concept is achieved and the innovative solution is ready for deployment"* (G.B2, GC senior manager). Projects reaching the proof-of-concept are either moved to other ENERCO's units to be further developed, or follow alternative deployment routes such as licensing, partnering and new venture creation.

Since its creation in 1996, more than 4,000 ideas have been reviewed by the GC team, which can count on an annual seed-funding budget of $ 40 million. Only about 10% of the ideas originally submitted have reached the proof-of-concept stage, but roughly 40% of the Exploration and Production R&D portfolio built over the last decade have its roots in GC ventures. Although remarkable results have been achieved, the venture handover from GC to possible deployment partners is increasingly a critical passage. *"Deployment (application in the field) follows development and demonstration. It is quite a different skill set the one required to know who is better to involve to commercialize a promising technology, how to market it, how to make business out of it, and that was never truly the GC purpose"* (A.M., EP research manager). "*When GC was started more than 15 years ago, the emphasis was mainly on the proof-of-concept. It has been changing over these past few years and now the emphasis is increasingly on successful deployment strategies"* (G.B., GC senior manager). Since over time several GC projects have ended up without an organizational "home" due to the lack of incentive mechanisms supporting the adoption of new and more advanced solutions, GC professionals "*are increasingly asked from their management to spend also some time, when needed, to bring projects to operating units so that the new solutions can get more easily implemented. Therefore*- as one of our informants emphasized- *achieving the proof-of-concept is not enough anymore. For a GC venture to be considered successful, the*

new technology has to be retained by ENERCO's divisions or third-parties"(H.P., Exploratory Research senior scientist).

3.4.1. Evaluation criteria and influential roles

What drives firms' venture selection choices? And what is the role played by individuals at different organizational levels in supporting the development of innovative ideas? Empirical evidence shows that organizations are generally conservative in their investment decisions, avoiding high-risk projects with distant and uncertain outcomes (Cyert & March, 1963). Decision-making authority on venture selection is typically accredited to top management teams, whose specific evaluation logics bias the scope of firms' venturing activities. However, clear manifestations of multi-level initiative selection dynamics are increasingly common, urging the need to account for differentiated and often conflicting evaluation frames in explaining venture development processes and the underlying decision-making patterns.

In this section we rely on case-based data collected during our second round of interviews to illustrate the main criteria driving idea selection in the GC context. Table 3.3 provides a synthetic profile of the eight in-depth investigated venturing projects with respect to the identified criteria. Influential roles and linking mechanisms employed to support venture progression through the selection gates are also described, and an encompassing view of the GC entrepreneurial process is derived. The basic premise, on which most of our informants based their discussion about the key drivers of venture survival, is that assessing the quality and potential of early-stage entrepreneurial ideas is a challenging task. *"The value of a project is something that even when you reach the proof of concept you are not sure about. We try to cope with this uncertainty by systematically reviewing venture proposals according to a few basic criteria. Novelty is one. The second is time to deployment. Another one is the fit with ENERCO. And a forth one is potential business value. You can use many other criteria of course, but these are the key ones we decided to focus on. As far as novelty is concerned we check, for instance, whether it is something that has already been done in the oilfield, or whether has been done elsewhere, or never done before by anyone. A similar type*

of assessment is made with reference to the other criteria, and eventually those projects meeting the GC requirements and reporting high scores on the evaluation scale are selected in and further matured (G.B., GC senior manager).

Table 3.3 Venture profile and selection outcomes

Project	Fit with ENERCO's strategy	Novelty	Potential Business Value	Time to deployment (years)	Status at project closure	Deployment strategy
DP	*Medium strategic fit* ○ Difficult hydrocarbons is a strategic area for ENERCO where upgrading technologies is needed ○ Lack of in-house expertise for the development of new perforating technologies (left almost completely to service companies)	Very novel technology for ENERCO and the whole E&P industry	(0.1-1 bln) ○ High-risk project but with high potential to improve productivity and broad application ○ IP rights all with QQ company, but ENERCO has an exclusive licence for a few years	Medium/ Long (6-10)	Graduated (stage 3)	External deployment through a joint venture
WL	*Medium strategic fit* ○ Enabling technology for a number of ENERCO's Trusts ○ Lack of internal expertise due to outsourcing to service companies ○ By upgrading logging technologies a significant cost reduction can be achieved in the core area of oil drilling	Very novel technology potentially opening a new technology play completely changing the way ENERCO and the industry acquire log data	(10-100 mln) ○ Potential of giving a global commercial edge to ENERCO as concerns data acquisition ○ Huge cost reduction and lower operational risks ○ IP rights with BETA company	Medium/ Long (6-10)	Graduated (stage 3)	External deployment through new venture creation
SM	*Medium strategic fit* ○ Effective sand management in well completion can increase productivity, so it's a strategic priority for Shell ○ Typical area of expertise of service companies	Not very novel, basically an alternative application of an existing technology	(1-10 mln) The commercial value is immediate, though not very relevant, but IPRs concerns are significant	Short (less than 2)	Executing (stage 2)	The intended deployment route would be internal
HS	*Medium strategic fit* ○ Sensing oil and gas in remote areas is not entirely aligned with ENERCO's exploration strategy	Very novel technique to detect gas presence in exploratory ventures	(10-100ml) ○ High-risk project but with a potential to considerably cut the cost of	Medium/ Long (6-10)	Graduated (stage 3)	Internal deployment

	○ In-house expertise		exploration surveys ○ IP rights with ENERCO			
SF	*Medium strategic fit* ○ Shutting-off fractures and voids by using cheap chemicals can increase productivity in carbonate reservoirs at a lower cost ○ Typical area of expertise of service companies	The proposed method, which is completely new to ENERCO, allows cheaper fracture sealing	(10-100mln) ○ Low-risk project and easy-to-implement solution ○ Limited potential applicability and IPRs concerns	Medium (4-6)	Executing (stage 2)	The intended deployment route would be external
SD	*Medium strategic fit* ○ The new probabilistic modelling method provides ENERCO with opportunities for business expansion in new areas and for consolidation in the existing businesses ○ The project proposes to solve a problem that is central both to ENERCO and the EP industry	Novel and revolutionary all-encompassing probabilistic modeling loop for seismic data	(10-100mln) ○ The new approach is expected to increase UR by 5% and to allow savings of 40% in RE ○ Potential applicability to all the EP units with significant learning and efficiency gains	Medium/ Long (6-10)	Graduated (stage 3)	Internal deployment
GD	*Medium strategic fit* ○ Using hydrates as the basis for gas separation to clean up highly contaminated reservoirs allows to target SE Asia regions ○ In-house expertise	Novel method to clean-up contaminated gas from the reservoirs based on a new separation technique	(10-100mln) ○ The new method is expected to lower significantly gas compression costs ○ Severe IPRs concerns	Medium (4-6)	Executing (stage 2)	The intended deployment route would be external
RI	*Medium strategic fit* ○ This non-invasive exploration technique can significantly improve the exploration success, which is a key strategic priority for ENERCO ○ In-house expertise	Novel application of an existing technology, with the possibility of achieving a first-mover advantage	(10-100mln) ○ Low-risk project and easy-to-implement solution ○ Possibility to achieve better quality and more reliable output than traditional optic data, and significantly speed up the screening process	Medium/ Long (6-10)	Maturing (stage 1)	The intended deployment route would be internal

Novelty. To stay competitive over time some firms decide to systematically invest in incremental product or technology developments while others decide to pursue highly differentiating opportunities in new business areas. *"Some projects in the GC portfolio have the potential to change the nature of our business in certain domains. Of course there are also a lot of little things that can help improve our current operations, but the big prize is if you can come up with a truly creative idea that nobody else in the industry has come up with and you make it work. That can revolutionize pieces of the business, and you can be ahead of competitors"*(A.M.,EP research manager). Although very novel solutions may enable companies to achieve competitive advantages and reap significant returns, they often have such long-term payback horizons and high-risk profiles that very few organizations are willing to bet on them. As far as GC is concerned, "*both pretty applied but very new stuff, and basic things with a step-changing potential are pursued. We don't want solutions for which there is already evidence that they can work. We want game-changing ideas, and if we think they are interesting to explore we will put money on them. That's why we have a rejection rate of 90%!"*(L.R..,GC senior manager and former director).

A significant project termination rate is pretty common in the corporate entrepreneurship context. "*Rejection is not considered a failure but just a possible outcome of the venture selection process"* (H.P., Exploratory Research senior scientist). The experimental nature of most venturing initiatives inevitably increases the uncertainty about their technical feasibility and business potential, inducing firms to progressively discontinue originally selected projects. "*We look for novel and unexplored ideas. They can fit with the core but they have to open new technology plays or provide truly differentiating business opportunities. We never engage in development work that is what instead evolutionary people are in charge of. We generally work on core business extensions and white spaces. If someone comes to us and says he can drill a well with a laser, that's what we would call an extension. It is something that has never been done in the oilfield, it is a totally novel technology, it is very uncertain and requires a lot of work. It can be novel from the point of view of the technology but also from the business one. In this latter case you use the technology but it is secondary. A different way to approach third-parties and do business with them can be a GC idea. So it can*

be both. But it will be anyway in the area of producing oil and gas. It can be different types of oil and gas: oil sands, contaminated gas,..but it has always to be in the hydrocarbon business, which means being related to finding and producing hydrocarbons. In that arena, if you find new business opportunities, novel ways of doing business, novel technologies, we call it an extension. But, in the past, we have also done projects dealing with marine renewables. We studied wave and tidal energy. That's outside the hydrocarbon business, so for us it is a truly white space" (G.B., GC senior manager).

Strategic alignment. Entrepreneurial initiatives aligned with corporate strategic priorities and distinctive competencies contribute to enhance firms' competitive and financial performance. They allow to leverage existing resources to introduce variation elements in mainstream business activities and support diversification attempts. Although a close fit between new ventures' domains and corporate core areas lowers the risk of possible organizational resistance, it may also hamper the search for explorative solutions, depriving corporate venturing activities of their truly innovative potential. At ENERCO, the GC unit has been purposefully created "*to allow deviations from the core strategy, because the core strategy might be wrong, so you may want to build alternative options. GC is meant to identify options around the core strategy, so that if, for instance, the world becomes electric in 10 years time, ENERCO has a few full functional projects ready, enabling the company to become electric as well, and preferably earlier than anybody else"* (L.R., GC senior manager and former director). Therefore, when evaluating and selecting new ideas, GC "*looks at what the existing business is doing, and that might be in the timeframe, but also at what might be worth exploring as future domains*" (D.M., GC senior manager).

Conflicting views about the link between venture alignment with corporate strategy and the related GC selection outcomes emerged from our interview data. "*I think there is no significant link between ENERCO's strategy and GC's project selection. There are a lot of good ideas out there that, in terms of development, don't fit with the portfolio of ENERCO's businesses but deserve our attention. However, for sure, it is relevant to the GC process to think about the implications of these projects for our mainstream activities, stopping those than can't be part of our business in a reasonable time"*(K.M., GC senior manager). On the other hand, one of our informants pointed out that "*traditionally there has been a loose*

connection between the work GC did thinking about the long-term direction of the company and the actual range of the pursued initiatives that were mostly selected in because of their intrinsic value. But there are some domains on which we are more focused and where we try to stimulate more innovation. What we do is highlighting areas of potential interest to the energy business, and let people come up with something new (G.B., GC senior manager).

Expected time to deployment. Empirical evidence shows that the time needed to turn original ideas into deployable solutions impacts on their possible returns. Potential dominant market positions may be compromised by long development times, which may also impact on the costs associated to the intended initiatives. "*The time scale is a critical issue in the oil industry since lead-times to bring new technologies to the market are normally pretty long. Solutions need to be developed now for implementation in the near future"* (A.M.,EP research manager).In building their venture portfolios, firms account for the risks related to the engagement in projects with a long expected time to deployment by selecting also easy-to-implement projects. "*Some concepts can be simply proven without the exploratory phase. They are already well-defined, limited in scope but can generate a lot extra-profits for the company, so they are quickly implemented"*(H.P.,Exploratory Research senior scientist).

To accelerate the field-proof of a new technology or the roll-out of a new product, firms often engage in collaborative initiatives with partners owning complementary assets. "*There have been projects GC and ENERCO would have been interested in keeping investing in, but they would have taken 10 to 20 years before being deployable. In those cases, it was often decided to go to third-parties to pursue their development"*(A.M.,EP research manager).

Over the last few years, concerns about the average length of venture development processes seem to have been pushing GC to revise its selection approach."*When GC started many of the selected projects required also 10 years to reach the proof-of-concept. My impression is that now they have moved more back to earth, and it is more like you need to be able to do that in 3 to 5 years. Their time span has reduced"*(J.V.E., chemical engineer and GC proponent). As another of our informants pointed out, "*GC projects can be much shorter in time, they can be quick winds -as they call them-, single elements created out of some innovative ideas whose implementation can be easily pursued"*(T.A., GC proponent).

Idea proponent. Initiators are engines of creative solutions and play a critical role in

conveying the value of their ideas to the entitled decision-makers. Their ability to strategically frame their projects, emphasizing possible synergies with existing activities, enhances the odds for their ideas to be retained. "*As an inventor, you have to test your ideas against experts. Many of them are often very skeptical, so, over time, you learn to strike the balance between listening to them and stamina to don't take a no for a no always*"(A.K, GC proponent).

Prior venture initiating experience can help proponents successfully navigate the multiple selection decision-making interfaces, and identify the most effective mechanisms to get senior managers' attention and support. "*For an idea initiator to be successful it is important to be both entrepreneur and intrapreneur. You have to be able to experiment, dealing with different organizational units to get your ideas out and tried. GC opens some doors but you have also to play your role"* (J.R., GC proponent). The individual networks that proponents have been able to build over the years can be critical to access complementary resources and take the projects quickly to the field. "*Based on my experience*, *even when the idea is proven, it is very much up to the individual proponent and its networks to get it deployed and tested. If they know you and they trust you it is much easier to get it to the field"* (T.A.,GC proponent). Raising interest around new solutions can facilitate their transition to deployment partners. "*I was updating constantly my contact persons in the operating units about my project. We were discussing about that every time we met and they gave me many inputs. I was the idea initiator, but I was collaborating with these scientists that were knowledgeable in that area of expertise. Linking with other people in the organization has always been critical to the project's success in my experience"* (A.K., GC proponent).

Although experience may help initiators to move their ideas forward in the selection process, it is also believed that it may have a detrimental effect on their creative potential. "*I joined ENERCO almost 25 years ago as a research geophysicist. In these past years I have been seeing many different corners of the geophysical research, looking into a large variety of subjects going from interpretation-related research to seismic processing, migration to theory, coral geophysics, and then later exploratory research. I have been involved in GC projects from the very beginning, proposing really new stuff, which most of the time was successful (the proposals were accepted), got funding and survived after the first phase. So I*

know well the GC decision-making dynamics"(H.P., Exploratory Research senior scientist). On the other hand, as one of our informants highlighted, "*it is true that young and new people have often the great ideas but it helps move your project forward if you are able to see different aspects, combine things and come up with new approaches. Just by working on many projects you get more creative and experienced"*(JVE, chemical engineer and GC proponent).

Idea sponsor. To turn ideas into viable businesses, skills that go beyond the initiators' ones are often required*"You can't be good in everything, you need someone with different skills and experience to help you further with your project. It's really about having someone leading you"*(M.G,GC proponent).Therefore, organizational sponsors are commonly assigned to help proponents mature their embryonic projects, and link them with knowledgeable people in the organization. "*GC sponsors act as knowledge brokers putting in contact people in the organization they think might be interested in working on specific ideas, or that have been dealing with similar projects, developing technical or market expertise. GC sponsors, which are normally experienced senior scientists, are aware of what is going on in other areas, and know more or less who is doing similar things in the organization"*(B.H., GC proponent).

Experienced sponsors offer proponents a sound guidance on how to survive the venture evaluation and selection process. "*After receiving some seed money I went on with my feasibility study work. It was at this time that I discovered the real need for a sponsor to help me carry this type of process forward. During the maturing stage it came out that the implementation of the idea wouldn't have been very practical, since the proposed hardware was not compatible with the existing drilling practices. Before the final closing-out meeting the GC sponsor helped me to pull out all the stops, adapt the method and persuade the panel members that the idea could still work. I wouldn't have succeeded without his lead"*(J. R., GC proponent). Their linking ability and reputation are crucial to secure resource commitment to the projects they are supporting, and to involve the right business and technology deployment counterparts. "*In a very early stage of the venture selection process*, *the GC sponsor has already some ideas of what to do with the project, and starts talking with internal research people and external counterparts"*(M.G.,GC proponent). As a GC sponsor recalled, "*we use the proponents' and our networks to increase the involvement of the operating units in our*

process. We email those guys, arrange a meeting, and explain what our technology is about. Most people are pretty interested in new stuff, but you need to keep them aware and updated about how it progresses, give them an extra-budget, send someone from here to help out when they are running the trials. If new solutions are not easily picked up, you need to step in yourself and fill that gap!"(M.R., GC senior manager and sponsor).

3.4.2. Deployment strategy, early involvement of potential partners and venture selection

In the previous section we illustrated the main criteria driving idea selection in the GC context, and the role played by proponents and sponsors in supporting the development of innovative solutions. An additional element that our informants repeatedly mentioned with reference to the GC venture evaluation and selection process was the intended deployment strategy. Normally deployment issues are taken into consideration only after a project reaches the proof-of-concept stage, and shouldn't significantly influence selection choices. However, our case-based evidence shows that involving target deployment partners (internal R&D managers, service companies, internal venture capital fund,..) already in the idea maturation and development process may have important implications for the initiative selection outcomes. "*It's key to its whole maturation and development to find out as early as possible where a project goes. Then you can start involving those people that can potentially be interested in picking that up. If we think that the project is a typical research one, which means it needs a lot of research work before getting to proof-of-concept, then we might have internal research managers involved in our panel or toll-gates meetings. If instead the idea is related, for instance, to drilling, we would get the head of drilling technologies to sit and discuss about the project. By doing that you get these people involved quite at an early stage. They know about the idea, they can ask questions, they can steer, and, then, when the concept is proven, they can take the project over"* (L.R., GC senior manager and former director).

The early involvement of potential deployment partners often contributes to enhance the quality of the proposed ideas and their chances to survive the internal selection process. As one of our informants emphasized, "*nothing drives progress quite like early deployment*

experiments. Early data is more valuable than anyone's opinion, including your own. So you have to put effort to connect with potential customers early in the process"(B.H.,GC senior manager). However, as a few GC proponents highlighted, early involvement of deployment counterparts can also constrain the scope of venture's technical and market applications. "*Usually the GC sponsor invites potential customers to the close-out panel. You normally involve people who can have a view of what can be done with the new solution. It is common that a potential customer that shows early an interest in a new solution, asks to modify the original concept. That makes managing the project quite difficult for GC. If you have the end-customer involved in a too-early stage, he might want to change it completely, and we don't like that. So it's a difficult balance to achieve*!" (J.R.,GC proponent).

To facilitate the arrangement of field trials and lower the risk of having orphan projects, over the last few years, GC has been increasingly trying to involve third-parties already in the venture maturation and validation process. "*This is a very critical passage for us. Even if potential customers are within the company, which means ENERCO's operating units, we want to get them exposed to the GC ideas early in the process so that they can familiarize with them, and capture their value-enhancing potential. We hope this can lead to a quick implementation in the field*" (G.B., GC senior manager). Similarly, K.M., GC senior manager, remarked "*we have all these wonderful ideas that need to cross the valley of death. There are people all over trying to kill them, either because are competing with their own stuff or don't fit with the core competencies, or because they think they can do better. You can prove the concept of the most wonderful ideas, but they have still to be taken over by the business. If they survive the valley of death they have basically succeeded. The trick is to involve people that can take the project up before getting to that critical stage*".

3.5. Emerging stage-based differences in the venture evaluation and selection process

Evidence from our field study suggests the existence of stage-based differences in the internal venture selection process. Factors influencing project transition across sequential screening and reviewing interfaces seem to vary according to the specific phase of the selection process.

To provide a more fine-grained examination of the criteria underlying venture-related decision making processes, and supplement our preliminary case-based evidence with robust conclusions, a content analysis of thousands GC project-related minutes was conducted.

As detailed in the data collection and analysis sections, semi-structured minutes referred to the evaluation process of 1,527 EP-related ideas submitted to the GC unit were examined. Relevant keywords were defined for the identified selection drivers and their frequencies across the main stages of the GC evaluation and selection process were measured.

The following table describes the distribution of the investigated projects according to their current development stage[5], and the average number of semi-structured emails and related attachments circulated during the different phases of their evaluation process.

Table 3.4 Project distribution over stages and minutes-related information

Current stage	Numb. of projects/stage	Average numb.of emails	Average numb. of attachments
Created	658	1,21	0,68
Maturing	518	1,81	1,34
Executing	225	3,79	3,66
Graduated	126	5,33	5,23

To investigate potential differences in the relevance of the identified selection drivers (*novelty*, *strategic alignment*, *time to deployment* and *deployment strategy)* to the specific venture development stages, the average numbers of words referred to each of them were compared. By taking each selection stage as a separate baseline, we checked whether the average numbers of words referred to the identified variables, measured over the same set of observations, differed significantly from each other.

For each stage, we performed a one-sample multivariate test of means (*mvtest*), which allows to simultaneously compare the means of more than two variables (Nel & Van Der Merwe, 1986; Krishnamoorthy & Yu, 2004). All tests confirmed that the differences between

[5] According to the structure of the GC evaluation and selection process, four different stages can be identified.1) *created*= the idea has been submitted but either not yet evaluated at the first selection gate or rejected.2) *maturing*= the idea has either passed the screening gate and not yet evaluated at the second selection gate or rejected there.3*) executing*= the idea has either passed the executing gate and not yet evaluated at the third selection gate or rejected there.4)= the idea has passed the third selection gate and now is closed-out.

the variable means were significant. Tables 3.5, 3.6, 3.7 and 3.8 report summary statistics for projects respectively in the creation, maturing, executing and graduated stage of development, and results from the related tests. The Hotelling's T-squared statistic reported in the tables below is just a multivariate generalization of the univariate t-statistic.

Table 3.5 Summary statistics for projects in the creation stage

Variable	Observations	Mean	Std. Dev	Min	Max
Novelty_creat	658	.881459	.607855	0	3
Stratalign_creat	658	.638297	.636192	0	2
Timetodepl_creat	658	.098784	.298599	0	1
Deploymstrat_creat	658	.095744	.299589	0	2

Hotelling T2 = 2121.59
Hotelling F(3,655) = 705.05
Prob > F = 0.0000

Table 3.6 Summary statistics for projects in the maturing stage

Variable	Observations	Mean	Std. Dev	Min	Max
Novelty_matur	518	1.990	.696706	1	4
Stratalign_matur	518	1.727	.647297	1	3
Timetodepl_matur	518	.156370	.368839	0	2
Deploymstrat_matur	518	.287644	.461561	0	2

Hotelling T2 = 4877.65
Hotelling F(3,515) = 1619.59
Prob > F = 0.0000

Table 3.7 Summary statistics for projects in the executing stage

Variable	Observations	Mean	Std. Dev	Min	Max
Novelty_execut	225	1.995	1.062	0	11
Stratalign_execut	225	.61333	.66660	0	4
Timetodepl_execut	225	2.386	.91943	0	9
Deploymstrat_execut	225	3.080	1.536	0	17

Hotelling T2 = 964.46
Hotelling F(3,222) = 318.61
Prob > F = 0.0000

Table 3.8 Summary statistics for projects in the graduated stage

Variable	Observations	Mean	Std. Dev	Min	Max
Novelty_grad	126	1.071	.887049	0	4
Stratalign_grad	126	.246031	.484751	0	2
Timetodepl_grad	126	2.809	1.063	0	6
Deploymstrat_grad	126	4.674	2.442	1	21

Hotelling T2 =	837.54
Hotelling F(3,123) =	274.71
Prob > F =	0.0000

The main purpose of our content analysis was to uncover possible stage-based differences in the centrality of specific factors to the decision-making process underlying venture selection. Based on the results summarized in the table below, we can observe that *novelty* and *strategic alignment* have higher mean values than the other identified variables in the first two stages of the venturing process (created and maturing). This means that the number of words in the minutes related to novelty and strategic alignment for projects in the creation and maturing stages is higher, on average, than the one for words related to time-to-deployment and deployment strategy. The reverse situation applies to projects in their executing and graduated stages, where, instead *time-to-deployment* and *deployment strategy* show higher mean values.

Table 3.9 Mean values for selection determinants across stages

STAGE\VARIABLE	Novelty	Strategic alignment	Time to deployment	Deployment strategy
Created	0,88	0,63	0,09	0,09
Maturing	1,99	1,72	0,15	0,28
Executing	1,99	0,61	2,38	3,08
Graduated	1,07	0,25	2,81	4,67

Our findings suggest that the venture review and evaluation process is centered on novelty and strategy-related considerations in the initial phases of the project development. Estimating ventures' time to deployment and defining their possible implementation routes is quite a difficult task when most ideas are still in embryonic state. Therefore, deployment-related issues are limitedly addressed in the early stages of the project evaluation process. However, as the proposed venture mature and their technical and business implications are easier to predict, deployment aspects become increasingly central to venture selection decisions.

A striking shift in the focus of the venture reviewing process across its different stages emerges from our data analysis. As the required resource commitment increases, due to the project progression towards more advanced development stages, implementation issues seem to progressively divert the evaluation panels' attention from strategic and novelty-related considerations. The time projects need to achieve a degree of technological readiness that allows first commercial applications, and the identification of clear deployment options become, then, increasingly central to funding decisions, shaping venture selection outcomes.

To shed further light on possible stage-based differences in the venture evaluation process, we also investigated whether a specific selection variable, for instance novelty, had a different relevance to the decision-making process for projects that failed and projects that succeeded at a specific gate. We compared mean values for each variable between accepted and rejected projects at each gate, and tested whether they differ significantly from each other. Several two-group tests (*ttest* with a variable discriminating between two groups, which in our case is the project survival through each selection gate) were performed (Welch, 1947). Tables 3.10, 3.11, 3.12 report summary statistics for projects which respectively went through the screening, executing and graduating gates, and were either accepted or rejected (A/F).

Table 3.10 Mean values for selection determinants in projects accepted/rejected at the first gate

		Novelty		Stratalignm		Timetodeploy		Deploymstrat	
SCREENING GATE	Observations	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
F	642	.889408	.608248	.640186	.636737	.101246	.301889	.093457	.296607
A	869	.163406	.369948	.135788	.346105	.004603	.067728	.032220	.176688
Combined	1511	.471872	.603825	.350099	.550615	.045665	.2088269	.058239	.237083

* 16 censored observations were removed

Wilcoxson rank-sum :	z = 23.522	z = 17.666	z = 8.893	z = 4.925
	Prob > |z| = 0.0000	Prob > |z| = 0.0000	Prob > |z| = 0.0000	Prob > |z| = 0.0000

Table 3.11 Mean values for selection determinants in projects accept/rejected at the *second* gate

		Novelty		Stratalignm		Timetodeploy		Deploymstrat	
EXECUTING GATE	Observations	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
F	448	2.020	.696458	1.732	.644816	.147321	.361072	.290178	.464095
A	351	3.313	1.128	2.623	1.020	.470085	.574296	.641025	.656330
Combined	799	2.588	1.114	2.123	.941069	.289111	.493333	.444305	.583043

* 70 censored observations were removed

Wilcoxson rank-sum :	z = -17.123	z = - 13.578	z = - 9.533	z = - 8.266
	Prob > |z| = 0.0000	Prob > |z| = 0.0000	Prob > |z| = 0.0000	Prob > |z| = 0.0000

Table 3.12 Mean values for selection determinants in projects accepted/rejected at the *third* gate

		Novelty		Stratalignm		Timetodeploy		Deploymstrat	
GRADUATING GATE	Observations	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
F	170	1.876	1.083	.594117	.692282	2.317	.906410	2.941	1.556
A	126	2.039	1.091	.841269	.889158	2.404	.821496	2.769	1.118
Combined	296	1.945	1.087	.699324	.790279	2.354	.870911	2.868	1.387

* 55 censored observations were removed

Wilcoxson rank-sum :	z = -1.757	z = - 2.301	z = - 1.376	z = 0.413
	Prob > |z| = 0.0789	Prob > |z| = 0.0214	Prob > |z| = 0.1689	Prob > |z| = 0.6794

Nearly all performed tests confirmed that the differences between the variable means for accepted and rejected venture proposals at each gate were significant. Only results related to the last selection gate (*graduating*) were not entirely supporting the significance of these differences for all variables. To increase the robustness of our predictions, in addition to the usual t-test, we run a non-parametric Mann-Whitney U test, also known as Wilcoxon rank-sum test., whose results are reported in the tables above.

The underlying objective of our last investigation was to show that the same selection driver can have higher or lower relevance to acceptance or rejection decisions according to the stage of the evaluation process, and to the related degree of venture development. Based on the results summarized in the table below, we can observe that the mean value for *novelty* is higher for the rejected projects than for the accepted ones at the first gate, but lower in the two subsequent ones. This means that the average number of words related to novelty for those projects surviving the screening gate is lower than for those rejected. The reverse situation applies to the two following gates, where instead novelty has higher mean values for the accepted projects than for the rejected ones, but with a significant difference only in the intermediate stage (executing). As far as *strategic alignmen*t and *time to deployment* are concerned, a similar pattern emerges. A lower mean value for accepted projects than for rejected ones at the screening gate, but a higher mean for those surviving the executing and graduating gates than for those discontinued (though a significant difference between rejected and accepted projects in the last stage was found only for strategic alignment and not for time to deployment, as indicated by Table 3.12). The *deployment strategy* variable shows a higher mean value for rejected projects than for accepted ones at the screening gate, the reverse situation at the executing gate, and again a higher mean value for projects rejected at the graduating gate than for those accepted (though the difference in this case is not significant).

Table 3.13 Mean values for selection determinants across groups (F/A) and stages

Selection gate	Novelty	Stratalignm	Timetodeploy	Deploymstrat
SCREENING (Created→Maturing)	0,88 (F) /0,16 (A)	0,64 (F) /0,13 (A)	0,10 (F) /0,004 (A)	0,09 (F) /0,03 (A)
EXECUTING (Maturing→Executing)	2,02 (F) /3,31 (A)	1,73 (F) /2,62 (A)	0,14 (F) /0,47 (A)	0,29 (F) /0,64 (A)
GRADUATING (Executing→Graduated)	1,87 (F) /2,03 (A)	0,59 (F) /0,84 (A)	2,31 (F) /2,40 (A)	2,94 (F) /2,76 (A)

* (A= accepted; F= rejected)

Overall, our findings suggest that the centrality of specific factors to venture selection decisions varies across stages. Novelty-related considerations seem to be more critical to project acceptance when decisions about venture execution need to be made (*executing gate*). At this stage a significant amount of resources has to be allocated to turn original proposals into implementable solutions. Therefore, project evaluation panels aim at thoroughly assess the innovative potential of the proposed ideas before committing further funds. Novelty appears less relevant to acceptance decisions at the last gate of the idea evaluation process (*graduating gate*), when deployment-related aspects usually dominate panel discussions, and the innovative content of the pursued ventures normally has been already established. Surprisingly, novelty-related considerations seem to be more central to idea rejection than acceptance decisions at the initial gate of the review process (*screening gate*). Given the embryonic shape of most ideas at that stage, conveying their truly innovative potential to project evaluators is often very difficult. Moreover, novelty characteristics normally amplify the perceived risk associated to new ventures, increasing their chances to be rejected.

Similarly to novelty, also strategic alignment is more central to project rejection than acceptance at the screening gate. However, its relevance to venture approval grows at the later gates of the review process, when potential synergies with the core business and projects' value-enhancing characteristics are often emphasized to support acceptance decisions at the executing and graduating gates.

As concerns deployment-related aspects (time to deployment and deployment strategy), we observe, as for the other selection drivers, a higher relevance to the decision-

making process at the executing gate. Discussions about the time required to bring a new product or technology to the market, and about viable deployment options contribute to lower the uncertainty surrounding projects' execution, increasing their odds of survival. Conversely, deployment issues have low relevance to project evaluation as concerns initial screening decisions. Project characteristics are still poorly defined at this stage, so when deployment aspects are addressed they are normally meant to support venture rejection decisions.

3.6. Blending theory and case evidence: towards a differentiated multistage selection model

Ecological and resource-allocation process models have been applied to corporate decision-making contexts to explain strategic change and new business development choices (Bower, 1970; Burgelman, 1983; 1994; Noda & Bower, 1996). They have provided useful theoretical lenses to interpret entrepreneurial phenomena in established organizations, conceiving firms as ecologies of innovative initiatives competing for limited resources (Burgelman, 1991).

The conceptualization of the internal venturing process as an intra-organizational evolutionary phenomenon has shed light on the dynamics underlying the creation of new business ventures and their subsequent integration within the corporate strategy and structure. Nevertheless, the drivers and nature of venture selection decisions have not been fully specified, limiting our understanding of the evaluation process underlying investment choices.

The inductively derived stage model for venture evaluation presented below shows how factors influencing idea selection vary between different stages of the evaluation process. The decision-making interfaces observed at ENERCO's corporate venture unit (GC) largely resemble those described in the extant corporate entrepreneurship and innovation literatures (Tyebjee & Bruno, 1984; Cooper, 1990; 1993; Riquelme & Ricards, 1992; Hall & Hofer, 1993; O' Connor, 1998; Reid & De Brentani, 2004; Kijkuit & Van den Ende, 2007).

Each proposed idea goes through an initial screening gate, where ventures deemed as potentially interesting are selected for further maturation, and allocated some seed funds. Since decisions about further resource commitment are contingent on the perceived viability of the proposed projects, their technical and economic aspects need to be clearly defined at

this stage of the evaluation process. Original proposals need to turn into detailed plans, allowing a sound assessment of ventures' technical feasibility and business value, and tackling those technical and commercial hurdles potentially hampering their execution.

Clear milestones and deliverables are set up for projects surviving this second selection gate and entering the executing phase. During this long gestation period technology and product features are systematically revised until a proof-of-concept is achieved, and the new solution is ready for deployment. Toll-gate meetings are organized to regularly check on the achievement of specific project development steps and discuss implementation options.

After unbundling the venture evaluation and selection process in its main constituent stages and gates, consistently with the revised literature, the development of our inductive model entailed the identification of the key determinants of venture selection.

In addition to criteria such as *novelty* and *strategic alignment* that have been widely discussed in prior research (Moenaert et al., 1995; Griffin, 1997; Khurana & Rosenthal, 1998; Verworn, 2009) we found evidence for the relevance of two other project-related dimensions: *time to deployment* and *deployment strategy*. Although their centrality to the decision-making process underlying venture selection is quite intuitive, they have been largely overlooked by the extant literature on corporate entrepreneurship. Our case data suggest that these deployment-related factors significantly impact on ventures' odds of surviving selection gates, especially in the late stages of the venture development process.

Pressures on achieving quick and satisfactory returns and bringing innovative solutions to the market earlier than competitors often bias selection decisions towards projects with a reduced time to deployment. A critical role in ventures' progression through the reviewing interfaces is also played by the identification of clear deployment routes for them. Raising interest in potential deployment partners early in the venture development lowers the risk of having orphan projects after a proof of concept is achieved. As our interviews report, timely interactions with potential business counterparts also contribute to improve projects' features and find a suitable organizational home for those ventures eventually selected.

A key role in moving ventures forward in the selection and development process is played by their proponents and sponsors. Extant research highlights that their ability to strategically frame new ideas and link with the right people in the organization significantly

increases their ventures' chances to survive the reviewing process (Floyd & Wooldridge, 1996; O'Connor & Rice, 2001; Kuratko et al., 2005). Our findings suggest that proponents' and sponsors' support to project survival can be further enhanced by their prior venturing experience, which can help them properly address the most critical technical and commercial hurdles throughout the venture development process.

After identifying the main phases and factors influencing venture selection decisions, the final step in the development of our model entailed the investigation of potential stage-based differences in the relevance of the identified selection drivers (*novelty*, *strategic alignment*, *time to deployment* and *deployment strategy)*. Our findings reveal how their importance varies between different phases of the venture review and evaluation process. Novelty and strategy-related considerations tend to be more central to selection decisions in the early stages of project development, while the opposite applies to deployment-related factors which are instead more critical in the later stages. Further insights on the nature of the decision-making criteria driving project selection between different evaluation stages were drawn from the comparative analysis of accepted and rejected ventures. Our findings suggest that the executing gate is a very critical transition point. Since it entails the allocation of a significant amount of resources based on the assessment of ventures' technical and business potential, discussions about novelty, strategic alignment, time to deployment and deployment strategy are generally more relevant to acceptance than rejection decisions at this gate.

Figure 3.2 maps the stages/gates, drivers and influence patterns characterizing our inductively derived model for venture selection. It shows that venturing ideas go through multiple screening interfaces before decisions about their actual implementation are made. As their technical profile, business value and viability are assessed, the number of surviving initiatives progressively decreases. Critical selection drivers vary between different stages of the evaluation process and, identified entrepreneurial actors leverage their experience to facilitate venture transition across selection gates and explore possible deployment options.

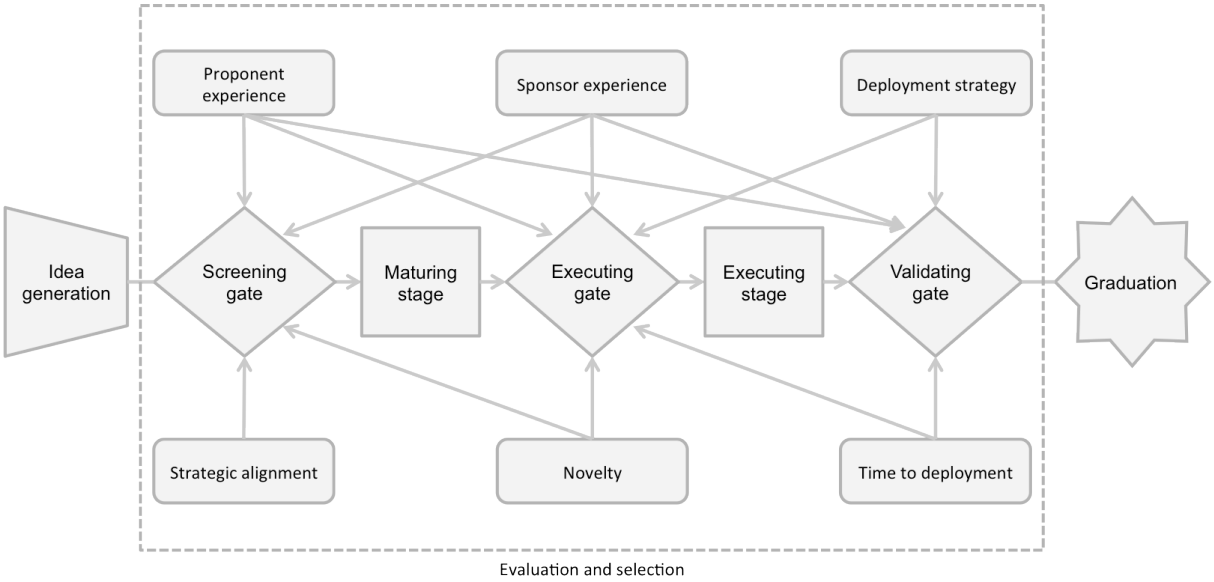

Figure 3.2 Initiative multistage evaluation and selection model

3.7. Concluding remarks

This chapter aimed at uncovering the drivers and dynamics underlying venture selection in established companies. Drawing on an exploratory study of the development of eight entrepreneurial initiatives supported by the internal corporate venture unit of a leading energy company, it unveiled the multi-stage and differentiated nature of initiative evaluation and selection processes. Through a thorough examination of a comprehensive set of archived venture files, the main criteria and factors influencing venture-related decisions were identified. Their relevance to different stages of the evaluation process was analyzed, contributing to a more fine-grained understanding of the determinants of venture survival.

The staged model for venture evaluation and selection that was inductively derived from our field study sets the ground for the following chapter. Testable hypotheses on the determinants of initiative acceptance at different screening gates are developed by weaving empirical evidence from our qualitative research with existing theory.

CHAPTER 4

Uncovering the determinants of initiative survival: a multistage selection perspective

Evidence, drawn from our qualitative investigation, on the existence of stage-based differences in the relevance of the evaluation criteria used to make initiative selection decisions has set the basis for this quantitative study. Real options arguments have been waved with qualitative evidence to derive a staged model of initiative selection. Testable hypotheses on the influence of specific initiative-related and human capital characteristics on the rate of initiative acceptance at different selection gates have been developed. The identification of these factors has entailed the integration of behavioural, resource-based and cognitive arguments with the evidence from our qualitative study. A unique dataset of 1527 ideas collected by the corporate venture unit of our focal firm between 1996 and 2009 will be used to test the hypothesized relationships.

4.1. Real options theory and a staged approach to initiative selection

Venture selection is one of the most critical tasks for corporate entrepreneurs. Ambiguities and uncertainties surrounding most entrepreneurial initiatives make it difficult to assess their potential contribution to corporate growth, especially in their early development stage (Daft & Lengel, 1986; MacMillan et al., 1987; Bazerman, 1998). Decision-makers face, then, the twofold risk of either rejecting valuable ideas or supporting solutions with limited market prospects and innovative potential (Balachandra, 1984; Elfring & Foss, 2000).

The complexity embedded in venture selection decisions mainly arises from the nature of the proposed initiatives, which are often forays in new business domains and target the development of high-technology solutions (O'Connor & Rice, 2001; Husted & Vintergaard,

2004). If on the one hand, corporate ventures need to be truly innovative to enable companies to reap the benefits of their entrepreneurial activities, on the other hand, the high level of risk commonly associated with explorative initiatives prevents any reliable prediction of their feasibility and returns, undermining the evaluation and selection process (Keh et al., 2002).

To ensure that the initiatives included in their corporate venture portfolios contribute to the achievement of the organizational strategic goals, venturing teams need, therefore, to enforce selection systems balancing the need of avoiding a detrimental escalation of commitment with that of preventing a premature rejection of early-stage ideas (Kazanjian & Drazin, 1990). Staged investment strategies, entailing regular go/no-go decisions at different initiative development phases, may be an effective means to deal with the intrinsic risks of the initiative selection processes, as evidence drawn from studies in the innovation field seems to suggest (MacMillan et al., 1985; Shalman, 1990; Van de Ven et al., 2000). By linking increased funds commitment to the achievement of specific milestones, and to the reduction of technical and market uncertainties casting doubts on the viability of the proposed initiatives, these strategies allow to make better decisions with respect to resource allocation and project advancement (Cooper, 1990; Desouza et al., 2007).

The appropriateness of adopting a stage-gate approach to corporate resource allocation decisions in highly uncertain contexts has been highlighted by a few studies applying real options theory to strategic investments (Mitchell & Hamilton, 1988; McGrath, 1997; 1999; Alvarez & Stenbacka, 2001). A real option is the organizational analogue of a financial option, which gives the right, based on a small upfront investment, to make a further decision in the future (Bowman & Hurry, 1993). It entails investments in real assets rather than in the financial markets, but shares the same underlying logic (Dixit & Pindick, 1994). Under conditions of uncertainty, related, for instance, to the outcome of a research project, investors holding an option wait until some of this uncertainty is revealed before deciding whether to use their option and invest further resources (Bowman & Moskowitz, 2001).

Several corporate strategic decisions have option-like features (Hurry et al, 1992). Venture selection is one of them (Copeland, 2002). The uncertainty normally surrounding the outcomes of venturing initiatives, and the fact that at any point in time firms may decide not to commit any further resources to them, inherent in sequential investments, have led some

authors to conceive corporate ventures as real options (Kumaraswamy, 1996; McGrath, 1999; MacMillan & McGrath, 2000). According to the option reasoning, firms invest in entrepreneurial initiatives because, based on a small upfront investment, they provide preferential access to potentially valuable opportunities and, at the same time, allow to limit their downside risk by offering the possibility of timely abandoning them (Adner & Levinthal, 2004). This logic helps justifying investments in early stage ventures that would be rejected based on traditional risk-related considerations, but that may become appealing due to the possibility for firms to stage their investments and postpone huge commitment until most venture-related uncertainty is revealed (McGrath, 1997; Alvarez & Stenbacka, 2001).

Although providing an ideal theoretical lens to interpret initiative survival dynamics in the internal corporate selection environment, consistently with the increasing anecdotal evidence on the multistage nature of venture selection decisions, the real options approach has been only limitedly applied in the entrepreneurship context to evaluate alternative investment opportunities, due to controversies and constraints stemming from its most critical assumptions (Bowman & Moscowitz, 2001; Adner & Levinthal, 2004). This has led most scholars adopting this view to rely on managerial heuristics rather than quantifications.

The appropriateness of the real option perspective and of its underlying staged investment logic to explain the selection and development process of early stage corporate entrepreneurial initiatives is supported also by evidence drawn from our qualitative study. Our findings suggest that the process through which investment decisions in innovative initiatives are made can be generally represented as a two stage-three gate model. These two stages, which we labeled *maturing* and *executing,* based on the nature of the main activities performed during the focal stage, are grounded in the corporate venturing and innovation literatures (Tyebjee & Bruno, 1984; Cooper, 1990; 1993; Riquelme & Ricards, 1992; Hall & Hofer, 1993; O' Connor, 1998; Garvin, 2002), and underlie a different degree of venture development. Overall, they cover all those incubative activities, following the *generation of new ideas*[6] and preliminary to the *testing* and i*mplementation* phases, through which early-

[6] Idea generation, in our case, is exogenous to the corporate venture unit's activities. Ideas created by internal employees or external inventors are submitted through an electronic interface and then stored in a central database before being reviewed.

stage ideas are turned into new feasible solutions (those for which a proof-of-concept is achieved), thanks to the provision of funds and experts' support.

In our inductively derived model, development activities are interpolated by evaluation ones. At the end of each stage there is a gate where decisions about further investments in ventures that survived up to that stage are made. Three decision-making gates that we labeled *screening*, *executing* and *validating*, based on the type of assessment normally entailed at the focal gate, regulate transition across stages together with increased resource commitment. The underlying logic is analogous to that of real options models. Initially an option to access a new potentially valuable solution, which is still in an embryonic shape, is purchased for a small amount of money. Then some due-diligence is conducted and decisions about whether to continue investing in the proposed initiative or not are made. A series of these "due-diligence and option-buy rounds" eventually results in a stage-gate decision-making model for initiative selection.

Each initiative development stage reflects different level of uncertainty, and requires a different management approach and amount of investments. At the *screening* gate, which is the first decision check-point after proposal submission, ventures are normally still in their embryonic state, not more than sketchy ideas. If they are deemed potentially interesting, based on the content of the submitted proposals, they are moved to the first stage of our focal process, the *maturing* one, and assigned some seed money to be further developed in their technical and commercial aspects. Before getting to the following selection gate, the *executing* one, initial proposals need to turn into detailed project plans, enabling the assessment of ventures' viability by assigned evaluation panels. Sound business cases for the proposed ventures need to be developed, and critical uncertainties influencing their technical and business potential need to be thoroughly addressed to survive this gate and enter the *executing* stage. This stage entails a long final gestation period, were a significant amount of resources is usually allocated, and technology and product features are systematically revised until a proof-of-concept is achieved. It ends if and when venture evaluators judge that all relevant uncertainties related to a project have been reduced to such a level that it can be transferred to the business and field-tested. Only ventures showing these attributes pass the *validating* gate, reach a close-out graduation stage, and are deemed ready for deployment.

Although our chosen labels may partially differ from those used in most available stage-gate models[7], which despite being structurally analogous have a broader focus, the initiative development and evaluation activities underlying our defined stages and gates are those commonly characterizing investments in early-stage ventures. These activities, preliminary to actual implementation decisions, have been traditionally conceived as part of a single venture evaluation and selection phase by most studies in the corporate entrepreneurship field (Burgelman, 1991; Lovas & Goshal, 2000; Garvin, 2002; Zahra et al., 2006). We argue that this conceptualization overlooks that, as ventures progress in their development process, and an increased resource commitment is required, also the nature of the perceived risks and uncertainties underlying investment decisions evolves, shaping accordingly the outcomes of the selection process (Pinto & Prescott, 1988; O'Connor & Ayers, 2005; Desouza et al., 2007; Eckhardt & Chiucta, 2008). And this is even more true for early-stage ventures, whose technical and commercial aspects are normally pretty undefined in the initial development phases. A multi-stage perspective, allows, therefore, to account for potential stage-based differences in venture selection dynamics, providing a more thorough understanding of the logics and factors influencing the related decision-making process.

In the following section we address different theoretical perspectives that can be useful to explain initiative selection choices, identifying common evaluation criteria and highlighting those biases traditionally affecting corporate resource allocation processes.

4.2. Alternative views on initiative selection

Although the importance of initiative selection decisions has been acknowledged both theoretically and empirically (Burgelman, 1991; Cabral-Cardoso & Paine, 1996; Miller, 1999; Eckhardt et al., 2006; Capron & Mitchell, 2009), little is known about how decision makers

[7] Stage-gate models, as described in the previous literature review chapter, have been extensively applied to new product development processes. They classify all activities going from idea generation to post-implementation review according to a framework that typically consists of five stages-gates (Cooper, 1990). The cover a broader range of activities compared to the ones we are focusing on in our study, which deeply investigates those preliminary to the actual project implementation.

discriminate among competing venturing projects (Desarbo et al., 1987; MacMillan et al., 1987; Hall & Hofer, 1993; Keh et al., 2002).

The different theoretical perspectives used to explain resource allocation processes in the corporate environment help capture, but only to a limited extent, how initiative selection decisions are made (Bower, 1970; Noda & Bower, 1996). They suggest factors influencing venture evaluation processes and final selection outcomes, providing useful lenses to interpret the strategic decision making underlying the development process of new corporate ventures.

However, based exclusively on their predictions, which rely on a common view of firms as conservative, risk-averse and myopic entities, it is difficult to explain why firms select highly innovative and risky ventures. Only considering also the role played by corporate venture units in partially removing those conditions biasing the decision-making process towards less daring investments, initiative selection choices can be thoroughly understood. As follows, perspectives on venture selection offered by central theories on organizational decision-making and resource allocation are briefly reviewed.

Behavioral theory. In the behavioral view, firms are conceived as boundedly rational entities, developing expectations based on available information (March & Simon, 1958; Cyert & March, 1963). The intensity of information search depends on the aspiration levels of the organization in relation to specific goals, which reflect its current and past performance. Different from the neoclassical economics perspective, decision-making entails finding satisfactory solutions rather than optimizing ones. Search for acceptable alternatives tends to be localized and begins in closely accessible areas, with familiar sources. Uncertainty avoidance is a key imperative that firms try to obey by heavily relying on routines and standard operating procedures, institutionalized as stable sets of search heuristics, emphasizing short-run reaction rather than anticipation of long-run events. Experience shapes behavior patterns through adaptive adjustments of procedures and aspirations, eventually resulting in path-dependencies constraining firms' learning opportunities (Greve, 2003).

Overall, behavioral arguments suggest that organizations are generally biased towards conservative investments in their resource allocation decisions, favoring related and short-term initiatives over projects with more distant and less predictable outcomes (Cyert &

March, 1963; March & Shapira, 1987; Levinthal & March, 1993). This perspective suggests also that the *degree of novelty, relatedness* and the *time to profitability* of a proposed initiative are central to its evaluation process. Projects building on organizational competencies, and only incrementally advancing existing solutions, are preferable to more novel and explorative ventures, and long-term strategic options are disregarded when compared with easy-to-implement solutions (Sitkin & Pablo, 1992; Smith, 1999; Garcia et al., 2003).

Resource-based theory. In the resource-based view, firms are seen as bundles of idiosyncratic resources, of which their ability to generate and sustain superior profits is a function (Penrose, 1959; Wernerfelt, 1984). The more their resources are rare, valuable, inimitable/ non-substitutable, and efficiently employed to satisfy customer needs, the higher is their likelihood of achieving a competitive advantage (Barney, 1991; Conner, 1991). Central to the resource-based view is the assumption that resources, besides being heterogeneously distributed across firms, are also not perfectly mobile, which makes them able to keep being sources of superior rents over time (Barney, 1986). Serving as the basis for their adaptation, when deciding how to acquire and employ their idiosyncratic resources, firms consider both their current endowments and those resulting from new investments (Peteraf, 1993).

Resource-based arguments suggest that organizations suffer from a relatedness bias in their investment decisions. The incentive for them to pursue related diversification strategies arises from their superior rent potential compared to unrelated ones (Mahoney & Pandian, 1992). This stems from the increased asset specificity that firms achieve through investment in closely related activities which allow them to exploit their resources more effectively than potential competitors (Chatterjee, & Wernerfelt, 1991). According to this view, the possibility of achieving synergistic value, through the efficient integration of pre-existing and new resources, makes ventures characterized by a tight coupling between their required resources and those of their parent companies an attractive investment option (Fast, 1979; Haynie et al., 2009). As for the behavioral perspective, this approach suggests, then, that the *degree of relatedness* of a proposed initiative is central to its evaluation process (Fast, 1979; Sykes, 1986), and that a close fit between parent and venture companies' business domains is highly desirable (MacMillan et al, 1986; Dougherty, 1995; Thornhill & Amit, 2001).

Cognitive theory. Applied to organizational studies, cognitive theories allow to explain how organizational members make sense of and use available information to make decisions, define solutions and pursue specific courses of action (Brief & Downey, 1983; Cannon-Bowers et al, 1993; Mathieu et al., 2000). Their basic assumption is that individual and organizational behaviours can be fully understood only considering the cognitive elements of human action (Fiske & Taylor, 1991; Huff, 1997; Hodgkinson & Healey, 2008).

Central to the cognitive view are, on the one hand, the idea that mental maps and knowledge structures shape (but are also shaped by) human action (Morsella et al., 2008; Thagard, 2005); on the other hand, the consideration that since cognitive processes are normally constrained by various factors and dynamics (Kahneman & Tversky, 1996), individuals often rely on heuristics and other simplifying strategies to make decisions (Tversky & Kahneman, 1974; Gigerenzer & Goldstain, 1999).

As far as corporate resource allocation choices are concerned, cognitive arguments suggest that decision makers' dominant logics tend to favour initiatives that fall within firms' core strategic areas (Prahalad & Bettis, 1986). A dominant logic, conceived as the lens through which all emerging opportunities for a firm are seen (Prahalad, 2004), reflects a specific business conceptualization and develops through experience in established core areas, reinforced by repeated success (Ginsberg, 1990; Cote, Langley & Pasquero, 1999; Dixon & Day, 2007). Acting as an information filter which shapes the process through which relevant data are captured and used, a dominant logic leads to select out those initiatives that don't contribute to maintain focus and internal coherence among firms' activities (Lampel, & Shampsie, 2000). Therefore, this approach suggests that the *alignment* of a proposed initiative with a firms' strategic priorities is central to its evaluation process (MacMillan, 1983; Meyer & Heppard, 2000), and that those ventures that better fit with firms' core business areas are more likely to be supported (Starr & MacMillan, 1990; Greene et al., 1999; Leifer et al., 2000; Rietzschel et al., 2010).

The behavioural, cognitive and resource-based theories reviewed to explain initiative survival provide a common characterization of the internal corporate selection environment as not

particularly supportive of risky entrepreneurial ventures. Uncertainty avoidance, myopia and path-dependency in strategic investments seem to bias selection decisions towards conservative and related alternatives, constraining the scope of firms' innovative activities.

However, empirical evidence shows that organizations occasionally support also highly uncertain and rather unrelated initiatives, such as the exploration of new business domains or the development of path-breaking technologies (Burgelman, 1983; Burgelman & Sayles, 1986; Kanter et al., 1990; Block & MacMillan, 1993). This generally occurs through structurally-separated and semi-autonomous organizational entities, such as corporate venture units or skunk-works, where early stage ventures can be explored, incubated and provided with the opportunity of demonstrating their viability, thus escaping that pervasive anti-failure bias that, according to conventional thought, would normally lead to their early termination (McGrath, 1999; Leifer et al., 2001; Hackett & Dilts, 2004).

Thanks to the availability of dedicated funds, corporate venture units, consistently with the real options reasoning, tend to experiment also with high-variance initiatives that, against providing access to potentially valuable opportunities, are more likely to fail (Bowman & Hurry, 1993)[8]. However, by staging their resource allocation process according to the venture development phases, these units can limit potential losses by timely discontinuing investments in ventures that don't look that promising anymore or whose uncertainties can't be entirely revealed over time (Hacket & Dils, 2004).

By integrating behavioral, cognitive, resource-based and real options perspectives, we have developed a more fine-grained understanding of the logics and factors driving initiative selection decisions in corporate venture units. Investments in early-stage initiatives, that we were unable to explain based on traditional behavioral, cognitive and resource-based arguments, are now predictable according to the real options reasoning.

In the following section, testable hypotheses on the determinants of initiative survival at different selection gates are developed by weaving empirical evidence from our qualitative study with extant theories. Factors influencing selections decisions drawn from our reviewed perspectives are integrated with those emerged from our qualitative investigation and

[8] According to March & Simon (1958) motivation to pursue high-variance opportunities is a function of the availability of resources or slack.

literature review. Their influence on the acceptance of proposed initiatives at different phases of development is predicted based on a general multistage selection model.

4.3. Hypotheses development

Based on our inductively derived two stage-three gate model for initiative development and selection[9],which, as already emphasized, has general properties, since the activities and decisions underlying the defined stages and gates are those commonly characterizing investments in early-stage ventures, hypotheses about initiative selection determinants at different phases of development are made (see fig 4.1). Factors relevant to selection decisions were identified based on reviewed theories (*strategic alignment, degree of novelty, time to profitability*) and evidence drawn from our qualitative study (*proponent experience, sponsor experience, expected time to deployment, early involvement of deployment partners*). Some factors, such as *proponent* and *sponsor experience* were refined in the light of the relevant literature, and became proponent and sponsor success experience. For others, such as *time to profitability* and *expected time to deployment*, given the close analogy, inclusion in the model was based on considerations related to their relevance to early-stage ventures-related decisions..The latter factor was eventually retained.

As follows, hypothesized relationships between focal selection determinants and initiative acceptance at the three identified selection gates are proposed.

4.3.1. Initiative characteristics and selection outcomes

Strategic alignment

Pursuing innovative endeavors aligned with corporate strategy has positive performance implications (Collins & Porras, 1994; Englund & Graham, 1999; Terwiesch & Ulrich, 2009).

[9] See section 4.1. at page 59-60 for details about stages and gates definition.

By tapping opportunities that address key strategic needs, such as revitalizing existing markets or exploring new business domains, initiative development can contribute to strengthen firms' competitive advantage and enable competence diversification (McGrath et al., 1995; Anthony et al., 2006; Adner, 2006).

The extent to which entrepreneurial initiatives should be aligned to corporate strategic priorities has been largely debated (Birkinshaw et al., 2002; Meyer & Heppard, 2000; Thornill & Amit, 2001). On the one hand, ventures related to firms' strategic areas achieve better performances, due to the possibility of leveraging existing resources (Lengnick-Hall, 1992; Dougherty, 1995). On the other hand, high relatedness hampers distant search and lowers the flexibility needed to develop truly innovative solutions (Sykes, 1986).

Cognitive arguments suggest that at the initial gate *(screening)* of the initiative selection process, where technical and business value of the proposed solutions are difficult to assess, decision makers' cognitive maps and dominant logics play a critical role in defining which ideas should move forward. They tend to favour initiatives that fall within the realm of core strategic areas and that, building on existing knowledge and practices, are perceived as less challenging to implement (MacMillan, 1983; Starr & MacMillan, 1990). Strategic alignment becomes less critical to initiative acceptance and funding at the two later decision-making gates (*executing* and *validating*), since for those ventures that move forward in the selection process, it will be consistently high. Therefore, we expect that:

Hypothesis 1a: The strategic alignment has a positive influence on the rate of initiative acceptance at the screening gate of the selection process.

Hypothesis 1b: The strategic alignment has no significant influence on the rate of initiative acceptance at the executing gate of the selection process.

Hypothesis 1c: The strategic alignment has no significant influence on the rate of initiative acceptance at the validating gate of the selection process.

Novelty

Organizations tend to search for novel technologies in areas in which they have prior experience and where they can build on established competencies (Nelson & Winter, 1982; Dosi, 1982; Stuart & Podolny, 1996). Local search, which implies incremental developments in proximate knowledge domains, leads to increased specialization with higher chances of positive and immediate returns (Rosenkpof & Nerkar, 2001; Martin & Mitchell, 1998).

Traditional behavioral arguments suggest that firms have a strong bias against highly uncertain initiatives (Cyert & March, 1963). Since technology development projects grounded on new knowledge domains are generally perceived as risky investments with a higher probability of failure than closely related projects, organizations are reluctant to engage in exploratory ventures (Sitkin & Pablo, 1992; Smith, 1999).

Nevertheless, empirical evidence suggests that also highly innovative initiatives sometimes manage to survive the internal corporate selection environment (Block & MacMillan, 1993; Leifer et al., 2000). Since experimenting with novel, pioneering and emerging technologies enables firms to access new business domains and enhances their competitive standing (McGrath et al., 1994; Leifer et al., 2001; Ahuja & Lampert, 2001), initiatives pursuing the development of leading-edge solutions are quite appealing to them.

Based on the real options logic, we argue that at the initial gate (*screening*) of the selection process, that usually entails a preliminary review of the submitted ideas based on a short proposal, the potential leading-edge nature of a project enhances its chances to be considered for further maturation and development. Since a very low commitment of resources is required, prospects of future significant returns induce firms to retain ideas that seem to be highly innovative. Resolving uncertainties about technical hurdles, value-creating potential and possible deployment options, becomes more critical to acceptance decisions at a later stage of the venture development process, typically the *executing* one. Required resource commitment is much higher at this stage, so teams of experts and business professionals jointly review venture plans to assess their viability (Moenaert et al., 1995; Stevens et al., 1997; Verworn, 2009) and establish whether examined ventures have the requisites to pass the *executing* gate. Novelty-related considerations are less relevant to

acceptance decisions at the last decision-making gate (*validating*), where aspects related to ventures' technical profile and feasibility have already been addressed, and deployment-related concerns are more central to its further progression. Therefore, we expect that:

Hypothesis 2a: The degree of novelty has a positive influence on the rate of initiative acceptance at the screening gate of the selection process.

Hypothesis 2b: The degree of novelty has a negative influence on the rate of initiative acceptance at the executing gate of the selection process.

Hypothesis 2c: The degree of novelty has no significant influence on the rate of initiative acceptance at the validating gate of the selection process.

Expected time to deployment

Speeding up the development of innovative solutions increases the likelihood for firms to reap pioneering advantages improving their long-term returns (Emmanuelides, 1991; Kerin et al., 1993; Golder & Tellis, 1993; Kessler & Chakrabarty, 1996). Innovation literature emphasizes that reducing the time needed to turn ideas into deployable products or technologies normally increases their profitability, and that innovation speed has a substantial positive impact on initiative success (Lynn et al., 1999; Droge et al., 2000; Kessler & Bierly, 2002).

Technological risk and market uncertainties significantly influence initiatives' time to deployment, shaping decision-making dynamics and collaboration patterns (Forbes, 2005; Acur et al., 2010). By taking too long to field-proof a new technology or roll-out a new product, firms can lose the window of valuable opportunities and the chance to get a dominant market position (Calantone & Di Benedetto, 2000; Markides & Geroski, 2005). Moreover, longer development times are associated to higher costs due to reduced efficiencies in resource use and increased coordination needs (Meyer, 1993; Langerak et al., 2010).

At the initial gate (*screening*) of the initiative selection process, when the proposed ideas are still embryonic, it is difficult to reliably predict the time needed to achieve a degree

of technological readiness that allows first commercial applications. Detailed project plans with staged investments contingent on reaching key milestones are defined in the later stages of the initiative development (*maturing* and *executing*), when the main technical challenges and the possible deployment routes for the project are easier to envision. We argue that, consistent with behavioural arguments, given their more immediate returns and implementation options, initiatives with a shorter time to deployment are deemed less risky and challenging to pursue, which increases their chances to progress through the selection interfaces. Therefore, we expect that:

Hypothesis 3a: The expected time to deployment has no significant influence on the rate of initiative acceptance at the screening gate of the selection process.

Hypothesis 3b: The expected time to deployment has a negative influence on the rate of initiative acceptance at the executing gate of the selection process.

Hypothesis 3c: The expected time to deployment has a negative influence on the rate of initiative acceptance at the validating gate of the selection process.

4.3.2. Proponent and sponsor experience and initiative selection outcomes

Traditional learning arguments suggest that experience improves the outcomes of repeat activities by enabling organizational members to encode inferences from the past into routines that guide their ongoing actions (Levitt & March, 1988). Individuals tend to retain behaviors generating positive consequences and to persist with heuristics that worked in their prior activities (Audia et al., 2000; Greenberg & Baron, 2002; Greve, 2003). Empirical evidence supports the positive relation between experience and performance in the context of routine tasks with defined benchmarks (Argote et al., 1990; Epple et al, 1991; Haunschild & Rhee, 2004; Ingram & Baum, 1997). However, the role of experience in creative and proactive endeavors is less clear (Crant, 2000; Smith, 2003; Grant & Ashford, 2008).

Classical learning-by-doing outcomes such as increased task efficiency or error reductions are usually not that relevant to proactive initiatives, to which instead success rate is

more critical (Kijkuit & Van den Ende, 2007; Shepherd et al., 2009). Prior success and failure experiences can shape subsequent attitudes and behaviors, fostering or hampering repeat creative efforts, and influencing their quality (Shepherd et al., 2003; Madsen & Desai, 2010). These considerations induced us to refine our choice of the proponent experience factor, picking instead its prior success experience, deemed more relevant to our case.

Recent research on individual creativity shows that past success in inventive endeavours increases the rate of subsequent idea generation, but lowers the probability of developing path-breaking solutions (Audia & Goncalo, 2007). Successful innovators, although more productive, show over time a higher propensity to rely on heuristics that worked in the past and to focus on incremental improvements of existing ideas, rather than explore new territories with uncertain outcomes (Chua & Iyengar, 2008

In a context where creating new ideas is not part of one's job, but is a proactive initiative triggered by intrinsic motivational factors, organizational members don't fear the negative repercussions of failure experiences (Amabile et al., 2005; Amabile & Khaire, 2008). Especially if specific institutional processes, such as those underlying corporate venture units' activities, are set up to filter, guide and support proactive endeavors, failure is perceived as an inevitable and ordinary consequence of search and experimentation activities (Frese et al., 1999; Van Dijk & Van den Ende, 2002; Burgelman & Valikangas, 2005).

As far as the outcomes of their proposed initiatives are concerned, prior success experience provides idea initiators with a frame of reference they can rely on when engaging in subsequent idea submissions and developments, drawing their attention to those issues that matter more for the acceptance decision (Terwiesch & Ulrich, 2009; Girotra et al., 2010). As highlighted by literature on issue-selling, an effective proposal framing, has, for instance, a huge influence on decision makers' perceptions and opinions on innovative projects (Dutton & Ashford, 1993; Dutton et al.2001; Howard-Grenville, 2003; Pinchot, 1985).

Based on these considerations, we argue that at the initial gate (*screening*) of the initiative selection process, when the idea is still roughly defined, and there is high uncertainty about its technical feasibility and potential value, the proponent's ability to frame it in a way that meets the stated and latent selection criteria is crucial to its acceptance. As the creator, the champion and the one that knows the most about the idea, he can leverage his

prior experience as initiator to emphasize those aspects of the venture that are more aligned with the needs and preferences of the organization. At the later selection gates (*executing* and *validating*) framing efforts are less effective to secure project funding, since more detailed plans and business propositions make it easier for evaluators to assess the real content and potential of the proposed ideas. Therefore, it is the proponent's ability to get senior managers' support and access to resources to be crucial to survive these last stages of the development and selection process. Since this ability is influenced by a proponent's successful track-record, which signals his ability to handle the venture development process, we expect that:

Hypothesis 4a: The proponent's past success experience has a positive influence on the rate of initiative acceptance at the screening gate of the selection process.

Hypothesis 4b: The proponent's past success experience has a positive influence on the rate of initiative acceptance at the executing gate of the selection process.

Hypothesis 4c: The proponent's past success experience has a positive influence on the rate of initiative acceptance at the validating gate of the selection process.

Initiators are the engines of proactive endeavors, however, turning ideas into viable businesses requires skills that usually go beyond their individual expertise (Tushman & Nadler, 1986; Day, 1994; Van Dijk & Van den Ende, 2002). Inventors, in fact, are extremely knowledgeable about the technical aspects and possible applications of their innovative solutions, but have often a limited understanding of their market potential (O'Connor & Rice, 2001).

Organizational sponsors provide initiative proponents with technical and managerial support, and help them gain access to the resources they need to mature their ideas (Garud & Van den Ven, 1992). Besides offering guidance on how to best proceed with the initiative development, sponsors exercise a brokering role, connecting proponents with people that work on similar projects and sharing knowledge from different internal and external sources.

A key skill of organizational sponsors is their ability to get the right people involved with the initiative they are supporting (Howell et al., 2005). Through their informal networks they link with experts in the project's domain that help the proponents address the most

critical technical issues, and raise interest in the organization about their innovative endeavors (Maidique, 1980; Howell & Higgins, 1990; Venkataraman et al, 1992; Greene et al., 1999).

As for the proponents, prior success experience provides organizational sponsors with a better understanding of the micro-dynamics characterizing the initiative evaluation and selection process, and of the informal social structure they need to succeed (Kleinbaum & Tushman, 2007; Kelley et al., 2009). Moreover, past successful initiatives enhance sponsors' credibility and reputation in the organization, galvanizing internal support for their ventures and signalling to decision-makers their ability to push forward the ventures they supported (Van de Ven et al., 1999). At the later gates *(executing* and *validating*) of the initiative selection process, where the highest commitment of resources occurs, and where issues related to ventures' technical feasibility and future deployment need to be thoroughly addressed, the sponsor's linking ability and status are crucial to secure continuing investment in the proposed initiatives by the corporate venture unit, and to involve the right business and technological partners. At the initial gate *(screening*) of the selection process, instead, sponsor success experience is still relevant to the initiative acceptance decision, but the sponsor role mostly entails guiding the idea initiator in properly selling the venture proposal to the entitled decision-makers. Therefore, we expect that:

Hypothesis 5a: The sponsor's past success experience has a positive influence on the rate of initiative acceptance at the screening gate of the selection process.

Hypothesis 5b: The sponsor's past success experience has a positive influence on the rate of initiative acceptance at the executing gate of the selection process.

Hypothesis 5c: The sponsor's past success experience has a positive influence on the rate of initiative acceptance at the validating gate of the selection process.

Sponsor tenure

Initiative sponsors are usually experienced managers with in-depth technological and business knowledge. Their status and reputation within the organization help them gain attention and

resource commitment to the initiatives they patronize. Through their shepherding function they nurture the projects they are assigned to, using their critical networking and negotiation competencies to build support around them (Floyd & Wooldridge, 1996; Kuratko et al., 2005).

Prior research on internal corporate venturing highlights that sponsors with a long tenure within an organization have greater opportunities to create large informal networks through which they can get critical resources and link with experts in different fields (O'Connor & Rice, 2001; Garvin, 2002; Zahra et al., 2006). Long-tenured sponsors can expedite the involvement of knowledgeable partners that can help improve initiatives' technical feasibility and increase the odds of their successful implementation (Hoang & Antoncic, 2003). Moreover, sponsors' repeat engagement in initiative development can provide them with well-informed perspectives on how to navigate the selection process, drawing their attention to those issues that influence the initiative's perceived viability and its subsequent acceptance throughout the selection process.

Consistently with our previous argument that the different competencies that sponsors develop over time are critical to initiative survival at all initiative selection decision-making gates, although for different reasons, we expect that:

Hypothesis 6a: The sponsor's organizational tenure has a positive influence on the rate of initiative acceptance at the screening gate of the selection process.

Hypothesis 6b: The sponsor's organizational tenure has a positive influence on the rate of initiative acceptance at the executing gate of the selection process.

Hypothesis 6c: The sponsor's organizational tenure has a positive influence on the rate of initiative acceptance at the validation gate of the selection process.

4.3.3. Early involvement of deployment partners and initiative selection outcomes

Our qualitative study provides evidence on the relevance of the early involvement of potential deployment counterparts to venture selection decisions. Although deployment issues are

normally taken into consideration only after a ventures achieves a successful proof-of-concept, which means surviving the validating gate, trying to involve third-parties already in their maturation stage, seems to contribute to enhance the quality and business potential of the proposed ventures. Moreover, it seems also to lowers decision makers' concerns about the risks of ending up with an orphan venture after its successful validation. Therefore, we hypothesize that:

Hypothesis 7: The early involvement of deployment partners has a positive influence on the rate of initiative acceptance at the validating gate of the selection process.

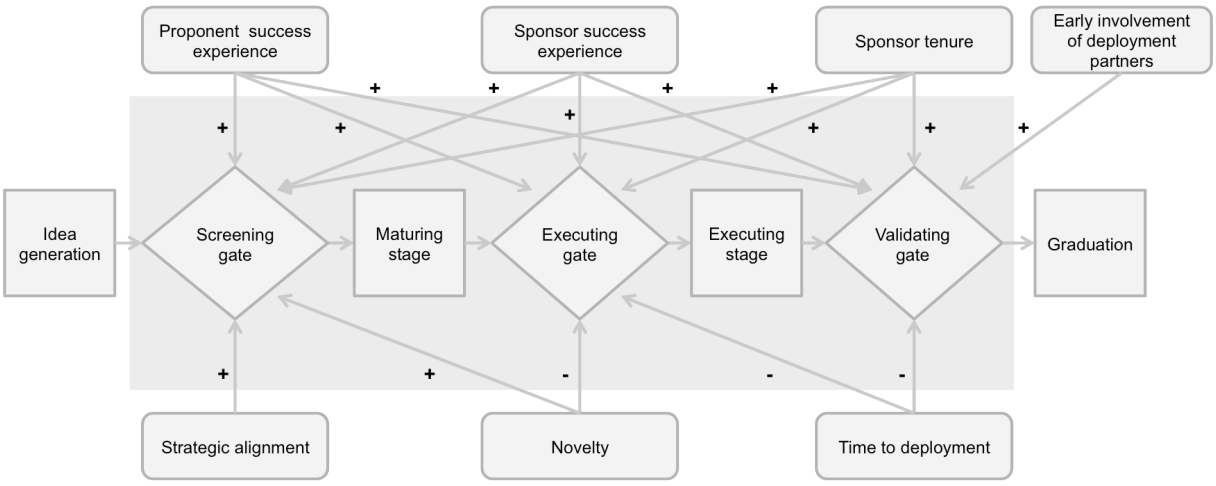

Figure 4.1 Integrated multistage selection model and hypotheses[10]

[10] Our multistage selection model includes three gates (screening, executing, validating) and two stages (maturing and executing). The idea generation stage, despite contributing to feed the whole process, is outside our focus of interest since includes inventive activities carried outside the boundaries of the corporate venture unit. The graduation stage is the outside our focus of interest as well. There are no more evaluation or selection activities going on at that stage, but it simply the status attributed to all those project that successfully survived the last selection gate (validating) and that are then ready for deployment.

CHAPTER 5
Methods for the quantitative study

In this dissertation we used a mixed-method approach to investigate the decision-making dynamics underlying the evaluation and selection of corporate entrepreneurial initiatives, and uncover the determinants of their survival. Our qualitative study helped us capture the multi-stage and differentiated nature of the venture selection process, and identify the key factors driving investment decisions in innovative initiatives. It also revealed how the relevance of specific decision-making criteria varies between different stages of the evaluation process. This set the basis for our quantitative study, which aimed at identifying the predictors of venture survival across different selection gates, using a competing risk event history model.

The present chapter describes the methods for the quantitative study we conducted. It illustrates the empirical setting, the sampling strategy and the data used for our analyses, the measures for the key variables, and the followed estimation procedure. To enable a thorough understanding of the chosen statistical model and its specific characteristics, a brief overview of event history methods[11] and their main applications is also provided (Allison, 1984).

5.1. Research setting

As for our qualitative study, initiative survival dynamics and their underlying determinants have been investigated in a major oil company we called ENERCO. It is a leading player in

[11] Event history analysis encompasses a large variety of methods to study events' occurrence and their related causes. Mainly rooted in the biostatistics studies, it often goes under the name of survival analysis or lifetime analysis. In fact, it was in the biomedical science field that these methods were first used to analyse survival data (Kalblfeisch and Prentice, 1980). Common applications can be also found in the engineering field where they usually go under the name of "failure time analysis", since they were originally used to analyse data on the breakdown of machines and electric components. In recent times, all these different approaches have been reconciled in a single stream of literature.

the energy industry, well-recognized for its pioneering efforts in frontier exploration fields and for the development of step-changing technologies. Through its internal corporate venture unit, which we called Great Coalition (GC), ENERCO has been systematically investing in early-stage innovative ideas targeting both existing and new business domains.

Since its creation in 1996 the GC team, made of full-time professionals with varied backgrounds in the energy industry, has been involved in the evaluation of more than 3,000 venture proposals, although only 10% of them have eventually reached the final proof-of-concept stage. As already emphasized in the qualitative chapter, GC works with an approach similar to that of a venture capital fund, allocating seed money to nurture promising embryonic ideas, and constantly monitoring their progress through further screening hurdles.

Idea selection is based on a staged peer review process. All submitted proposals are examined by a GC screening panel, and those deemed potentially interesting to ENERCO are granted some initial funds to be further matured in their technical and commercial aspects. Idea initiators are supported by assigned GC sponsors, whose role is mentoring and guiding them through the review process. For all the ideas that survive the initial screening gate, GC nominates 2-3 technical and/or business experts as members of the subsequent extended venture evaluation panels. Together with appointed GC professionals they assess the viability of the proposed initiatives, and allocate additional funds to those meant to be executed. Project milestones are then formally set up to check on how ventures progress, and timely discuss their possible way forward. The final step before getting to the actual GC venture "graduation" (when the project close-out occurs) entails a proof-of-concept review. The project team has to show that their implementation plan is indeed workable and deserves further funding from potential deployment counterparts. This last approval decision typically marks the end of the formal GC process, although panel members often try to help successful ventures find a suitable organizational home inside or outside ENERCO.

Initiatives' progress through the identified selection gates is tracked by a central database that records key information about each venture proposal. They include date of submission, of acceptance or rejection decisions at different evaluation panel meetings, proponent's and sponsor's names, as well as a short description of ideas' focus, source and target area. Additional documents such as panel presentations, value propositions, and venture

plans are accessible through the system, allowing a fine-grained understanding of ventures' technical and commercial profiles. The idea database includes also information on critical venture evaluation lenses normally used to rate projects in the GC portfolio review exercises. These are periodical meetings during which a board made of experienced GC professionals (review board) evaluates submitted projects according to a set of pre-defined criteria such as *novelty, strategic alignment* and *time to deployment*. Since the flow of venture proposals is quite constant, weekly meetings are normally arranged to discuss incoming ideas.

To get a thorough understanding of the database setup, of the involvement of different counterparts in the GC venture evaluation and selection process, and of the overall research setting, we worked extensively with GC staff in the Netherlands. By enriching our knowledge of the critical factors and dynamics underlying venture selection in the GC context, it helped us define an appropriate sample for our study, and properly interpret emerging evidence.

5.2. Sample and data

Since the purpose of the study is to identify the main predictors of venture survival across different selection stages, we needed to focus on a sample of entrepreneurial initiatives undergoing a stage-gated evaluation process. Accordingly, we examined the entrepreneurial activities of a specific internal corporate venture unit (GC) that systematically pursues the development of innovative solutions based on a structured review process.

All ideas submitted to GC between November 9, 1996 (the day ENERCO adopted the GC process) and December 22, 2009 (the day we extracted information from the database) were taken into considerations, which yielded a sample of 3,099 initiatives. After consultation with GC professionals assisting us in the data extraction process, we decided to retain only ideas submitted to the GC Exploration and Production (EP) group. Over the years, three additional GC groups targeting other ENERCO's business areas (Downstream, Gas & Power and Chemicals) had been created. However, besides being repeatedly dismantled, they often followed an ad-hoc procedure in selecting and assigning funds to promising ventures, which made them not suitable for our study. Therefore, we solely focused on EP ventures, relying on

a sample of 1,533 ideas reviewed by the GC team over a thirteen year time period. After an initial data screening, six projects were dropped being previous versions of re-submitted venture proposals. This led to a final sample of 1,527 ideas. They were distributed as follows: 151 (9,9%) were still in progress when we collected the data, so no definite acceptance or rejection decision had been made about them, while 1376 (90,1%) had either reached the final GC stage and been closed, or had been terminated in one of the earlier stages. The use of the hazard rate estimation enabled us to leverage all available data, including in our analysis also those projects still ongoing at the end of the study period (Greene, 2003).

The full set of 1,527 ventures had complete data on the idea source (internal or external to ENERCO), domain, proponent and sponsor name, number of co-proponents, decision-making panel members, allocated funds, status (stage of the selection process the ideas was in), and exact dates of transition from one selection stage to the subsequent ones. Therefore, it was possible to determine the number of days each initiative stayed in a specific phase of the selection process, necessary to perform the event-history analysis, and the outcomes (acceptance or rejection) of the review panel meetings. Additional information about ventures' profiles in terms of novelty, strategic alignment and time to deployment were also accessible through the central database, which reported specific scores assigned by the review board to each project according to pre-defined evaluation scales.

It should be noted that besides providing a unique data coverage, our study overcomes the traditional survivor bias characterizing most studies in the venturing context, which often solely focus on ventures surviving the whole selection process, neglecting those rejected.

5.3. Dependent variables

Given the structure of the study, which investigates the survival of entrepreneurial projects through multiple selection stages, our dependent variables are the hazard rates for idea acceptance at the three different decision-making gates regulating the project transition from one stage to the subsequent one. The hazard rate, sometimes known as conditional failure rate, indicates the rate of failure at a set point in time, conditional on survival up to that time

(Kiefer, 1988)[12]. In continuous time models it captures the instantaneous transition intensity, incorporating information on the occurrence of the focal failure event (idea acceptance in our case) and the time to its occurrence (Cox & Oakes, 1984; Jenkins, 2008)[13].

Idea acceptance at gate 1: hazard rate for idea acceptance at the screening gate. In our sample only 869 ideas out of the 1,527 submitted survived the first selection gate. Rejection is quite common in the internal corporate venturing context, especially in the initial phase of the evaluation and selection process. In fact, only a limited number of ideas, out of those originally proposed, normally gets funds to be further matured, and reach a more advanced stage of development. Consistent with the logic of event history analysis, the hazard rate indicates whether a project was accepted or not, but also, by construction, the time to that focal decision (expressed here in terms of days from the idea submission to the date of acceptance at the screening gate). Time-related interpretations are disregarded in the present study, which focuses solely on the rate of occurrence of the idea acceptance events.

Idea acceptance at gate 2: hazard rate for idea acceptance at the executing gate. Only 351 ideas out of the 869 that survived the screening gate made it through this second selection interface. A significant amount of resources is usually allocated at this stage, which induces firms to thoroughly assess the viability of the proposed projects before committing more funds and engage in further development work. Consistent with our prior illustration, the hazard rate in this case indicates the rate of occurrence of idea acceptance at the second selection gate, and incorporates, by construction, information on the time to this further approval decision (expressed in terms of days from the acceptance at the screening gate to that at the executing one). Our interpretation will be, however, limited to the event occurrence.

[12] See section 5.6 for an overview of event history/survival models.

[13] The hazard rate is a measure of risk and can vary from 0 (no risk at all) to infinity (certainty of the failure event at that instant). Although often interpreted as a conditional probability (basically the probability that an observation dies at a certain time t, conditional on its survival to that time t), the continuous-time hazard rate is not a probability since, unlike the discrete-time one, it refers to an exact point in time and not to a time interval.

Idea acceptance at gate 3: hazard rate for idea acceptance at the validating gate. In this sample only 126 out of the 351 projects that survived the executing gate were deemed as suitable candidates for deployment, and therefore, eventually accepted at the validating gate. The technical challenges associated to the achievement of a successful proof-of-concept, and the common lack of resources needed to arrange field trials, significantly compromise the chances for most entrepreneurial initiatives to survive this final selection gate. Our dependent variable in this case indicates the rate of occurrence of the acceptance event at the validating selection interface, and, as previously mentioned, incorporates, by construction, information on the duration of this final initiative development stage (expressed in terms of days from acceptance at the executing decision-making gate and acceptance at the validating one). Once more, time-related interpretations are disregarded in the context of the present study, whose focus is on the rate of occurrence of the project acceptance event at different selection gates.

5.4. Independent variables

Novelty. We measured the extent to which proposed ideas incorporate incremental or more radical elements of change by drawing on the information available in the initiative database. For each idea, it indicates a novelty value ranging from 1 to 5 (1= incremental improvement in the firm's current methods, techniques or products; 2= significant improvement in the firm's current methods, techniques or products; 3= method, technique or product new to the firm but currently available in the industry; 4= method, technique or product new to the industry but currently adopted in other industries; 5= method, technique or product never adopted before anywhere) that was assigned by the review board[14] during the periodical portfolio review meetings on the basis of the initiative's technical and commercial profile.

Expected time to deployment. To capture the expected time required to take an idea from its

[14] Detailed information on the structure of the review board and the nature of the GC portfolio review meetings have been provided in the research setting section at the beginning of the present chapter.

embryonic state to its first operational applications we drew on the information available in the initiative database. For each idea, it indicates a value ranging from 1 to 5 (1= <2 years; 2= 2-4 years; 3= 4-6 years; 4= 6-10 years; 5= >10 years) that was attributed by the review board during the periodical portfolio review meetings, based on considerations related to the need to source complementary resources, arrange field trials or involve external partners.

Strategic alignment. To capture the extent to which a proposed idea was aligned with the corporate strategic priorities we drew on the information available in the initiative database. For each idea, it indicates an alignment value ranging from 1 to 5 (1= the idea falls in areas other than the focus ones; 2= the idea falls in areas partially related to the focus ones; 3= the idea falls in the stated business focus areas; 4=; the idea falls in the stated R&D focus areas; 5= the idea falls in the potential new business focus areas) that was attributed by the GC review board during the periodical portfolio review meetings, based on the idea's fit with the corporate current and intended strategic directions.

Proponent success experience. We measured a proponent's success experience by counting the number of his prior accepted ideas at the different selection gates before the focal idea was examined. We differentiated a proponent's past successes between stages since we believe that the nature of the activities he performs and of the relations he establishes change across them. We labelled *proponent screening success* the variable measuring a proponent's success experience related to the screening phase of the selection process. It was constructed by counting the number of the proponent's ideas already accepted at the screening gate when the focal idea was reviewed. Similarly, we labelled *proponent executing success* and *proponent validating success* the variables measuring a proponent's success experience related to the executing and validating phases of the selection process. They were constructed by counting the number of the proponent's ideas already accepted respectively at the executing and validating gates when the focal idea was evaluated.

Sponsor success experience. We measured a sponsor's success experience by counting the number of his prior accepted ideas at the different selection gates before the focal idea was

examined. We differentiated a sponsor's past successes between stages, based on the same considerations advanced for the proponent's experience. We labelled *sponsor screening success* the variable measuring a sponsor's success experience related to the screening phase of the selection process. It was constructed by counting the number of the sponsor's ideas already accepted at the screening gate when the focal idea was reviewed. Similarly, we labelled *sponsor executing success* and *sponsor validating success* the variables measuring a sponsor's success experience related to the executing and validating phases of the selection process. They were constructed by counting the number of the sponsor's ideas already accepted respectively at the executing and validating gates when the focal idea was evaluated.

Sponsor tenure. We measured a sponsor's tenure within the organization as the number of years he had been employed at the time of the idea submission. This operationalization is consistent with most of the existing studies (Bergh, 2001; Lovett & Cole, 2003; Ng & Feldman, 2010), and reflects exposure to the organizational culture, structure, and systems, influencing a sponsor's ability to navigate the corporate socio-political environment.

Early involvement of deployment partners. To capture the implications for initiative final selection outcomes of involving target deployment partners (internal R&D managers, service companies, internal venture capital fund,..) already in the idea maturation and development process, we created a dummy variable coded "1" if there was evidence of their participation in the extended panel meetings related to the executing and validating gates, "0" otherwise.

5.5. Control variables

Co-proponents. We controlled for the number of individuals who jointly developed the proposed ideas. Solutions resulting from collaborative inventive efforts tend to be more creative than ideas originating from a single proponent (Perry-Smith & Shalley, 2003; Tesluk et al., 1997). Initiators can draw on a broader set of knowledge, resources and experiences leading to an increased variety in problem-solving approaches and search strategies (West &

Anderson, 1996; Nemeth & Rogers, 1996). Different perspectives can help improve the quality and framing of the submitted ideas and, therefore, increase the chance of their positive outcomes (Jackson, 1996; Milliken et al., 2003; Girotra et al., 2010).

Idea source. We controlled for the source of the submitted ideas. They can originate from opportunities identified by organizational members (dummy coded "1") or by entrepreneurs, scientists, consultants, and other individuals with no formal links with the organization. External ideas tend to be more innovative but less aligned to corporate strategic priorities than internal ideas, which are, generally, related to the firm's core business and rather incremental in nature (O'Connor & Rice, 2001; Menon & Pfeffer; 2003; Laursen & Salter, 2006). Intellectual property considerations and lower perceived risk, make internal ideas often meet managerial preferences, increasing their chances to survive internal selection processes and be finally implemented (Katz & Allen, 1982; Liebeskind, 1996; Capron & Mitchell, 2009).

Core domain. Initiatives related to core technological and business domains generally get more resources and managerial attention compared to mostly unrelated ventures (Burgelman & Sayles, 1986; Block & MacMillan, 1993). Firms positively value ideas that leverage existing assets to improve their competitive position in areas in which they hold a certain degree of specialization. To control for the influence that falling into a core (dummy coded "1") or non-core domain has on initiatives' outcomes, we drew on the classification adopted by ENERCO that maps domains according to their relevance to the firm's business portfolio, and that is reported in the initiative database.

Budget. The amount of resources an initiative is assigned influences the number of people that can be involved and the possibility of further improving the quality of the initial idea, therefore impacting on its future outcomes. We controlled for this effect by including the budget allocated to each initiative according to its current phase. *Budget-time1* is a control that applies to ideas that survived the screening gate, while *Budget-time2* applies to ideas that survived also the executing gate. Resource allocation is staged and contingent on the outcomes of prior selection screens. No funds are assigned before idea acceptance at the

screening gate and after acceptance at the validating one.

Before 2002. To account for events in the corporate venture unit's history that might have had an impact on the initiative selection process and its related outcomes, we included a dummy variable discriminating between ideas submitted before 2002 (coded "1"), and ideas submitted in 2002 and in the following years. Searching through the unit's archives and, based on multiple interviews with prior and current members of the GC team, we found out that the year 2002 marked a significant change in the GC's structure and range of activities. ENERCO's corporate venture capital arm, that closely cooperated with the GC unit to bring innovative ideas to the market, was dismantled. Some of its tasks were moved to the GC unit, which started paying increasing attention to deployment aspects in the idea selection process.

5.6. Event history analysis: principles and relevant models

Before describing the employed competing risk event history model, we provide a brief overview of event history methods and their key principles. The underlying purpose is to show their appropriateness to our hypotheses testing and to the investigation of venture selection processes in the chosen empirical setting.

The objective of event history analysis is to explain why certain individuals or units of interest are at higher risk of experiencing a focal event than others (Vermunt, 1997). Events are qualitative changes occurring at a specific point in time that mark the transition between various states (Allison, 1984). Also referred to as "failures", they include a wide range of potential occurrences. Death, births, marriages, divorces, migration, unemployment as well as machine breakdowns are the most typical "failure events" in those biostatistics, engineering and sociology studies that have employed what social scientists call "event history analysis" (Blossfeld & Rower, 1995). In our study, the focal failure event is the acceptance of the proposed entrepreneurial ideas at each of the defined selection gates. Ventures surviving the first selection step are at risk of being accepted in the subsequent one. Conversely, rejection is final, so ventures that have been rejected once have no chances to be accepted anymore.

Event history data incorporate information on the time spent within each state, and the date of each transition, if any occurred. One of their typical features is censoring, which means that either the relevant event occurred in an unspecified time before the observation period started (*left-censoring*) or that it had not yet occurred when it ended (*right-censoring*). In our case, data on the survival of the submitted entrepreneurial ideas are right-censored since for some of them, decisions about transition from a certain development and maturation stage to the following one had not yet been made when our observation time ended.

Event history methods deal effectively with censored observations, which instead create major problems to standard statistical methods such as multiple linear regression, leading to severe biases or loss of information (Yagamuchi, 1991). Binary dependent models (e.g. logit or probit) could get round the censoring issue by simply modelling whether or not the focal event occurred (dependent variable equal to "1" if it occurred, "0" in case of censored observations). However, this strategy may be problematic as well, and undermine the knowledge-enhancing potential of available information. In our case, for instance, it wouldn't allow us to distinguish between projects that didn't experience the acceptance event because they were not meeting the applied evaluation criteria and those that didn't because the observation period ended before the event occurrence. These considerations induced us to rely on an event history method to analyse our data on venture survival through different decision-making gates. In fact, as proposed initiatives progress through the review interfaces, the elapsed time between different selection panel meetings tend to increase, together with the number of right-censored observations. As a consequence of the development work venture teams need to handle before decisions about further commitment are made, several projects were still active and waiting for the next panel review when our observation time ended.

Event history data can be analysed using *continuous* or *discrete-time* methods. This is a critical distinction, since it underlies a different data structure and shapes model selection. In the continuous-time case, the time of event occurrence is precisely indicated in the recorded data, and the spell length is normally measured in days. Discrete-time methods are more appropriate, instead, to cases in which time units are larger, such as months, years or decades. In the present study, since information on the exact dates of idea submission, acceptance or rejection at the each of the selection decision-making gates are available, a continuous time

specification was chosen for the time variable. This has some relevant implications for the definition of a concept that is key to event history models: the *hazard rate*.

The hazard rate is the fundamental dependent variable in an event history model, since it controls both for the event occurrence and its timing (Allison, 1984). In the *discrete* case, it is the probability that an event will occur at a certain time *t* to a specific individual or unit of interest, conditional on him/it still being at risk of experiencing the event at that time. The *continuous* time hazard rate *h(t)* describes, instead, the instantaneous rate of failure, which, in our case, can be interpreted as the instantaneous probability that an entrepreneurial project will be accepted at a certain time *t*, conditional on it still being at risk of experiencing the event at that time *t*. Let *T* be a continuous random variable denoting the time of event occurrence, *f(t)* is its probability density function and *S(t)* a survivor function indicating the probability that $T > t$. The *continuous time hazard rate* is, then, defined as

$$h(t) = \frac{f(t)}{S(t)} = \lim_{\Delta t \to 0} \frac{P\,(t \leq T < t+\Delta t \mid T \geq t)}{\Delta t} \qquad (1)$$

in which $P(t \leq T < t + \Delta t | T \geq t)$ indicates the probability that the unit of interest (an entrepreneurial project in our case) will experience the focal event (acceptance at one of the evaluation gates) in the interval $(t \leq T < t + \Delta t)$, given that it didn't occur before *t*. Although it may be more intuitive to interpret the *continuous time hazard rate* as an instantaneous probability of event occurrence, it should be noted that, unlike the *discrete time hazard rate*, it is not a probability, since it can be greater than one.

So far, we have shown the hazard rate as solely dependent on time, overlooking differences in the characteristics of individuals/units of interest. We now account for these differences, expressing the hazard rate as a function of both time and some explanatory variables summarized by a vector *X*. It can, hence, be defined as

$$h(t, \boldsymbol{X}) = \alpha(t) + \boldsymbol{\beta X} \qquad (2)$$

where $\alpha(t)$ is an unspecified function of time, $\boldsymbol{\beta}$ is a column vector of parameters to be

estimated, and $\boldsymbol{X}$ our row vector of covariates (Allison, 1982; Cleves et al., 2004). The one reported above, known as proportional hazard model, is the most common functional form for the hazard rate in continuous time specifications. It is called proportional hazard model because absolute differences in $\boldsymbol{X}$ imply proportionate differences in the hazard (at each t). The effects of changes in the explanatory variables on the hazard, which, by definition, are constant over time in the proportional hazard case, are expressed by the $\boldsymbol{\beta}$ vector.

Depending on whether and how the hazard rate is specified, a choice between the most appropriate *nonparametric, parametric* or *semi-parametric* continuous time methods to use is required. *Nonparametric methods* make few if any assumptions about the distribution of event times (Allison, 1984). Hazard rates and survivor functions are estimated directly from the data, and compared across groups, without modelling any covariates effect. Although pretty popular among biostatisticians, social scientists usually tend to prefer parametric or semi-parametric methods for their estimations.

The use of *parametric methods* underlies the specification of every aspect of the chosen model, except for the value of some parameters that need to be estimated. Their application is appropriate to situations in which existing theoretical models and empirical evidence suggest a specific distribution for the time of events' occurrence. Restrictive assumptions about the time variable distribution are normally made, and hazard and survivor functions are derived accordingly (Kleinbaum & Klein, 2005). Popular parametric models, members of that general class of proportional hazard models specified in the equation (2), are the exponential, Weibull and Gompertz ones. They assume very specific shapes for the hazard functions, respectively constant, and monotonically increasing or decreasing over time. Alternative parametric models in which the hazard is a non-monotonic function of time are the lognormal and log-logistic ones. For them, proportional hazard interpretations are not possible, and accelerated failure time specifications hold. This means that, this time, absolute differences in $\boldsymbol{X}$ imply proportionate differences in survival times and not in hazard rates.

A more flexible and less restrictive way to estimate the relationship between hazard rate and explanatory variables is represented by *semi-parametric methods*. They make limited if any assumptions about the shape of the hazard function. The most popular semi-parametric

model is the *Cox's proportional hazard* one. It allows to estimate the effects of the covariates on the hazard rate without making parametric assumptions about the baseline hazard function. It is called proportional hazard model because for any two units of interest at any point in time the ratio of their hazards is constant (Blossfer & Rowher, 1995). As for all proportional hazard specifications, which satisfy a separability assumption, in a Cox model the hazard rate is the product of an unspecified baseline hazard function, $h_0(t)$, and a non-negative function of covariates $exp(\boldsymbol{\beta X})$ that has multiplicative effects on the baseline hazard[15].

$$h(t, \boldsymbol{X}) = h_0(t)\, exp(\boldsymbol{\beta X}) \qquad (3)$$

The possibility of providing robust estimates of regression coefficients and hazard ratios, even without any specification of the baseline hazard function, explains the wide popularity of the Cox model as an effective method to analyse continuous time data. In presence of censored observations and specific information on the time to events' occurrence, it is normally preferred over the logistic model (Kleinbaum & Klein, 2005).

Our overview of alternative methods for the analysis of event history data, and of their main applications, has been restricted, so far, to the occurrence of single kinds of failure events[16]. However, a subject of interest may be exposed to multiple risks at the same time, and this needs to be properly accounted for in the model specification and coefficient estimation.

Competing risk models tackle such issues in the specific case of competing events, namely events whose co-occurrence is impossible (Allison, 1984). In this context, the risk of experiencing the focal event for the subject/unit of interest is removed by the occurrence of any of the competing events. The typical approach for the analysis of competing risk data entails the estimation of separate models for each type of event (*cause-specific hazard functions*) using one of the methods we previously discussed, and treating competing event

[15] As shown in the equation (3) the baseline hazard is a function of t but not of $\boldsymbol{X}$, while the opposite holds for the exponential expression that involves $\boldsymbol{X}$ but not t. An extended version of the Cox model includes also time-dependent $\boldsymbol{X}$, although in that case the proportional hazard assumption is not valid anymore.
[16] What Jenkins (2008) defines "single risk".

types as censored categories (Prentice et al., 1978). For an event type *j* from the set J (*j* = 1,2,...,J), the cause-specific hazard can be expressed as:

$$h_j(t) = \lim_{\Delta t \to 0} \frac{P(t \leq T < t+\Delta t,\ J=j \mid T \geq t)}{\Delta t} \quad (4)$$

The overall hazard function *h(t)*, is then just the sum of the different cause-specific ones. However, if only one event type is of primary interest, estimation can be restricted just to that. To assess covariates effects on the hazard rates for each event type a Cox proportional hazards model specification is typically employed[17]. The key drawback of this approach is the need to assume the independence of competing risks, which is very difficult to test (Pintilie, 2006).

An alternative model to analyse data on competing events is the one focusing on the *cumulative incidence function* [18] (also known as the sub-distribution or the marginal probability function), rather than the survivor function, which provides estimates of the marginal risk of occurrence of a specific event in presence of competing risks, without making assumptions on their independence (Kalbfleisch & Prentice, 1980). It still does require, however, for the overall hazard to be the sum of all the specific hazards for each event type, which is normally the case for mutually exclusive occurrences. Since the cumulative incidence function is a complicated function of the cause-specific hazards, it may be difficult to estimate the covariates' effect on it. As pointed out by many authors, this effect may be also quite different from the one on the cause-specific hazard function (Gray 1988; Pepe 1991).

Fine and Gray (1999) developed a parsimonious semi-parametric model to estimate the effects of covariates on the cumulative incidence function using a proportional hazard assumption. It allows an easy estimation of the covariates effects on the cumulative incidence functions, and is mostly useful when only one of the competing events is of interest. In their

[17] The cause-specific hazard function with a Cox proportional hazard specification is $h_j(t, X) = h_j(t) \exp(\beta_j X)$.
[18] The cumulative incidence function for an event j is described by the following equation $F_j(t) = P(T \leq t, J = j)$.

model specification they refer to the cumulative incidence function as *sub-distribution function*. For an event type *j* the hazard of the sub-distribution, or *sub-hazard*, is then equal to:

$$\bar{h}_j(t) = \lim_{\Delta t \to 0} \frac{P\,(t \leq T < t+\Delta t,\ J=j \mid T \geq t \text{ or } T < t \ \&\ J \neq j)}{\Delta t} \qquad (5)$$

According to this formulation, subjects/units of interest experiencing competing events are still kept at risk, so that they can be considered as not having any chance of occurrence.

Further specifications about the Fine and Gray's model and its analogies with the Cox proportional hazards one will be provided in the following section, where the estimation procedure followed to derive predictors of venture survival across stages is described.

5.7. Estimation procedure

To estimate the hazard rates for initiative survival across different selection gates, we employed a competing risk event history model predicting idea acceptance versus project termination. In the internal corporate venturing context, proposed initiatives continuously compete for resource allocation, therefore, project termination is a rather common selection outcome. Including in our analyses projects that were discontinued in addition to those that ultimately succeeded, enabled us to leverage all the available data, overcoming a sample selection bias that would have been introduced if we had just focused on successful projects.

Idea acceptance and project termination are mutually exclusive events. Proposed initiatives are at risk of either surviving the selection gates or being rejected. Competing-risks analysis allows to estimate the hazard rates for those alternative outcomes at each stage of the selection process, accounting for right-censored observations (Allison, 1984). Given the nature of our data, which record the specific day in which transition to one of the two possible destination states occurred, continuous time methods were most appropriate.

The lack of theoretical models suggesting a specific distribution for the time of idea acceptance decisions, induced us to rely on a semi-parametric method to estimate the effects

of the selected covariates on the hazard functions for each event type. We followed the approached suggested by Fine and Gray (1999) to model the competing sub-distribution hazards rates (sub-hazard rates from now on) for each entrepreneurial project that either progressed to a subsequent selection stage or was discontinued.

Equation (5) in the previous section defines the sub-hazard for the idea acceptance event j, in presence of a competing event ($J \neq j$) that may prevent its occurrence. The variable T measures the time spent at risk of experiencing one of the two possible outcomes of the selection process. The probability P indicates the risk of experiencing the acceptance event in the time interval between t and $(t + \Delta t)$, conditional on the project still being at risk of experiencing that at time t, or having experience a competing event before t.

A proportional hazard specification was used to estimate covariates effects on the sub-hazard rates for each event type, though our primary interest was on the acceptance event j.

$$\bar{h}_j(t,\mathbf{X}) = \bar{h}_{j0}(\mathrm{t})\ exp(\boldsymbol{\beta X}) \qquad (6)$$

Similarly to the Cox model, the baseline sub-hazard is left unspecified and proportional effects of the covariates $\boldsymbol{X}$ on the sub-hazard rates are assumed. $\boldsymbol{\beta}$ is the vector of estimated parameters, that summarize the proportional effects on the sub-hazard of absolute changes in the corresponding variable of interest. A positive (negative) coefficient indicates a greater (lower) sub-hazard for the focal event occurrence (project acceptance in our case), and an increase (decrease) in the cumulative incidence function. Alternatively to estimated coefficients, sub-hazard ratios estimates were reported and their interpretation is similar to the one for the hazard ratios in the Cox regression. Finally, it is worth emphasizing that with the model specification we used, robust standard errors were estimated (Fine & Gray, 1999).

CHAPTER 6
Results of the quantitative study

6.1. Descriptive statistics

Tables 6.1, 6.2 and 6.3 show the descriptive statistics for the variables of interest, and the correlation matrix for each of the identified selection gates. In nearly all cases the binary correlations between the variables are sufficiently low. The only variables that show higher correlations are those referred to the *sponsor success experience* at the second and third selection gates (executing and validating gates). In particular, the correlations between these variables and the variable measuring the years of *sponsor tenure* seem to be higher than the average (respectively 0.57 and 0.62).

Table 6.1 Descriptive statistics and correlation matrix – screening gate[a]

Variable	Mean	s.d.	Min.	Max.	1	2	3	4	5	6	7	8	9
1. Idea source	0.87	0.33	0	1									
2. Core domain	0.25	0.43	0	1	0.05 *								
3. Co-proponents	0.87	1.27	0	12	0.15 *	0.04 *							
4. Sponsor tenure	3.74	2.08	1	10	-0.10 *	0.07 *	-0.07 *						
5. Novelty	2.10	0.89	1	5	-0.01	0.23 *	0.14 *	0.08 *					
6. Strategic alignment	2.42	0.88	1	5	0.04 *	0.35 *	0.15 *	0.11 *	0.49 *				
7. Time to deployment	2.82	1.18	1	5	0.04 *	0.13 *	0.13 *	-0.19 *	0.40 *	0.25 *			
8. Proponent success screening	1.21	1.68	0	16	0.07 *	0.15 *	0.14 *	0.11 *	0.30 *	0.28 *	0.13 *		
9. Sponsor success screening	18.79	15.76	0	75	-0.14 *	0.02 *	-0.04 *	0.07 *	0.04 *	0.03 *	-0.14 *	0.06 *	
10. Before 2002	0.42	0.49	0	1	0.31 *	0.04 *	0.21 *	-0.40 *	-0.04 *	-0.03 *	0.33 *	-0.05 *	-0.27 *

[a] $n = 1,527$. * $p < .05$

This result was not too unexpected, since it is plausible that the most successful sponsors are also those with more experience within the corporate venture unit. However, as

explained in section 5.4, the number of successful initiatives supported, and the years of experience within the organization are meant to capture two distinct types of sponsor experience. The appropriateness of this distinction is supported both by the extant literature on corporate entrepreneurship and by the evidence from our qualitative study. Therefore, we decided to include both variables in the competing risk model estimations.

Table 6.2 Descriptive statistics and correlation matrix – executing gate[a]

Variable	Mean	s.d.	Min.	Max.	1	2	3	4	5	6	7	8	9	10
1. Idea source	0.90	0.29	0	1										
2. Core domain	0.30	0.45	0	1	-0.00									
3. Co-proponents	1.17	1.40	0	12	0.13 *	-0.02 *								
4. Sponsor tenure	3.62	2.01	1	10	-0.06 *	0.11 *	-0.04 *							
5. Novelty	2.47	0.79	1	5	-0.03 *	0.10 *	0.02 *	0.17 *						
6. Strategic alignment	2.74	0.79	1	5	-0.03 *	0.34 *	0.01	0.29 *	0.36 *					
7. Time to deployment	3.02	1.08	1	5	0.04 *	0.03 *	0.06 *	-0.21 *	0.28 *	0.07 *				
8. Proponent success executing	0.74	1.01	0	6	-0.08 *	0.16 *	0.02 *	0.19 *	0.26 *	0.28 *	-0.07 *			
9. Sponsor success executing	8.92	10.10	0	49	-0.28 *	0.08 *	-0.05 *	0.57 *	0.21 *	0.20 *	-0.14 *	0.21 *		
10. Budget_time1	18619.1	7515.79	0	50000	0.04 *	0.07 *	0.01	-0.01	0.10 *	0.14 *	-0.04 *	0.17 *	-0.01 *	
11. Before 2002	0.46	0.49	0	1	0.29 *	-0.03 *	0.11 *	-0.47 *	-0.15 *	-0.21 *	0.38 *	-0.29 *	0.51 *	-0.08 *

[a] $n = 869$. * $p < .05$

Table 6.3 Descriptive statistics and correlation matrix – validating gate[a]

Variable	Mean	s.d.	Min.	Max.	1	2	3	4	5	6	7	8	9	10	11
1. Idea source	0.84	0.35	0	1											
2. Core domain	0.35	0.47	0	1	0.09 *										
3. Co-proponents	1.25	1.33	0	9	0.30 *	-0.04 *									
4. Sponsor tenure	3.50	1.77	1	10	0.08 *	0.13 *	0.05 *								
5. Novelty	2.76	0.71	1	5	0.05 *	0.05 *	0.08 *	0.05 *							
6. Strategic alignment	2.92	0.80	1	5	0.09 *	0.27 *	-0.02	0.22 *	0.35 *						
7. Time to deployment	2.92	0.99	1	5	-0.03	0.08 *	0.06 *	-0.16 *	0.39 *	0.20 *					
8. Proponent success validating	0.52	0.64	0	3	0.30 *	0.10 *	0.05 *	0.07 *	0.12 *	0.19 *	-0.09 *				
9. Sponsor success validating	3.97	4.27	0	18	-0.04 *	0.11 *	-0.00	0.62 *	0.13 *	0.20 *	-0.13 *	0.18 *			
10. Budget_time2	332873.2	290894.4	25000	2180000	0.17 *	0.08 *	0.14 *	0.11 *	0.23 *	0.19 *	0.06	0.29 *	0.10 *		
11. Before 2002	0.35	0.48	0	1	0.29 *	0.03 *	0.10 *	-0.34 *	0.01 *	-0.11 *	0.32 *	-0.01 *	-0.40 *	-0.18 *	
12. Early involved partners	0.40	0.49	0	1	0.08 *	0.03	0.06 *	-0.02	0.10 *	0.02	-0.10 *	0.29 *	0.71	0.20 *	-0.04 *

[a] $n = 351$. * $p < .05$

6.2. Results of competing risk regression of initiative acceptance at the screening gate

As previously mentioned, the estimation procedure entailed running three separate competing-risk models for each gate of the selection process (screening, executing, validating). Three separate regressions were, then, performed estimating the effects of the covariates on the sub-hazard rate of each specific decision gate. The baseline sub-hazard was left unspecified for all cases, so no assumption was made about the distribution of the focal event over time. For each gate, the results are reported in two different formats. The first one shows the regressions' coefficients, which indicate the effects of the covariates on the sub-hazard rate i.e. on the rate of acceptance of the initiative at a given point in time, conditional on the non-acceptance up to that time. A positive (negative) coefficient indicates that the variable is positively (negatively) influencing the acceptance rate of the initiative. The second table shows the results of the same regression reporting the sub-hazard ratios instead of the coefficients. In this case, a sub-hazard ratio greater than 1 indicates a positive effect of that variable on the rate of acceptance of the idea, while, in turn, a sub-hazard ratio smaller than 1 reflects a negative influence. For simplicity, only the regression coefficients will be commented; the tables with the hazard ratios are reported in the appendix.

It should be recalled that survival analysis is able to capture both the occurrence of the event (idea acceptance), and the time to its occurrence. However, the analysis of time-related issues is not a purpose of this study. Therefore, the interpretation of the coefficients will be limited to the rate of occurrence of the initiative acceptance.

The variables employed in the estimations can be divided in three groups. The first one includes the control variables (*idea source, core domain, co-proponents, budget_time1/2, and before 2002*). The second group comprises the independent variables at proponent and sponsor-level (*sponsor tenure, sponsor success screening/executing/validating, and proponent success screening/executing/validating*). Finally, the last group includes the independent variables at project-level (*novelty, strategic alignment, time to deployment, early involvement of deployment partners*). These groups of variables will be added in three steps. The first step (Model 1) represents the baseline estimation containing only the control

variables, Model 2 includes the variables regarding the proponents and the sponsors of the initiatives, and finally, Model 3 also adds the project-level variables.

Table 6.4 shows the coefficients of the regressions referred to the project acceptance at the initial screening gate (Table B.1 in the appendix shows the sub-hazard ratios). Model 1 represents the baseline hazard with the controls. In Model 2 we add the variables regarding proponents and sponsors of the innovative initiatives. *Proponent success experience* shows a positive and significant coefficient, also the *sponsor success experience* is significant but with negative sign, while *sponsor tenure* is non-significant. Model 3 includes also the variables at project-level.

Table 6.4 Results of competing risk regression (coefficients) – screening gate

Variable	Model 1			Model 2			Model 3		
Idea source	0.26	(0.11)	*	0.10	(0.11)		0.19	(0.11)	"
Core domain	0.25	(0.07)	**	0.09	(0.08)		-0.25	(0.08)	**
Co-proponents	0.20	(0.02)	***	0.17	(0.02)	***	0.13	(0.02)	***
Before 2002	0.30	(0.07)	***	0.43	(0.07)	***	0.56	(0.07)	***
Sponsor success screening				-0.01	(0.00)	**	-0.00	(0.00)	
Proponent success screening				0.20	(0.01)	***	0.15	(0.01)	***
Sponsor tenure				-0.01	(0.03)		-0.05	(0.02)	*
Novelty							0.52	(0.04)	***
Strategic alignment							0.23	(0.04)	***
Time to deployment							-0.05	(0.03)	
Log pseudolikelihood	-5987.62			-5889.92			-5771.84		
Wald χ^2	130.99		***	264.28		***	647.40		***
Observations	1527			1527			1527		

Robust standard errors are in parentheses. " $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

Both *novelty* and *strategic alignment* are positive and significant, respectively supporting the hypotheses 2a and 1a. *Time to deployment* is non-significant, (confirming hypothesis 3a) presumably because it is too difficult to evaluate at this initial stage, so doesn't affect the acceptance rate. However, in Model 3, while *proponent success experience* has a positive and significant effect, confirming hypothesis 4a, *sponsor success experience* is non-significant and *sponsor tenure* is significant but with a negative sign. These last results contradict hypotheses 5a and 6a. Finally, all controls show some level of significance. In particular, *idea source* (significant at 0.10 level), *co-proponents* and *before 2002* have a positive impact on the sub-hazard rate, while *core domain* has a negative impact.

In brief, initiative acceptance at this gate of the selection process seems to be mostly dependent on the characteristics of the project. A novel and strategically aligned project has a greater possibility to pass this gate, while successful projects do not have to be necessarily linked to the core domains of the company. In fact, projects out of the main domains seem to be privileged (see the negative coefficient of *core domain*). Moreover while the successful track-record of the proponent for that stage enhances the chances of his projects to pass the gate, the same doesn't hold for the sponsor. However, his years of experience inside the corporate venture unit (tenure) seem to have an opposite effect on the rate of venture acceptance.

6.3. Results of competing risk regression of initiative acceptance at the executing gate

Table 6.5 shows the results of the regression referred to the second gate of the selection process (executing). Table B.2 in the appendix reports the same results in terms of sub-hazard ratios. The first model estimated (Model 1) contains only the control variable. All of them are significant, although core domain only at 0.10 level. Moreover, at this stage another variable is included as a control. It's *budget_time1*, which indicates the amount of money that has been granted to the initiative after the screening phase. This variable, having a positive coefficient, has a positive impact on the rate of project approval.

Table 6.5 Results of competing risk regression (coefficients) – executing gate

Variable	Model 1			Model 2			Model 3		
Idea source	-0.89	(0.17)	***	-1.02	(0.17)	***	-0.93	(0.18)	***
Core domain	0.20	(0.11)	"	-0.00	(0.12)		-0.07	(0.12)	
Co-proponents	0.08	(0.03)	*	0.07	(0.03)	*	0.06	(0.03)	"
Before 2002	-0.42	(0.11)	***	0.25	(0.13)	"	-0.04	(0.14)	
Budget_time1	0.00	(5.68)	***	0.00	(6.66)	***	0.00	(5.98)	***
Sponsor success executing				0.02	(0.00)	***	0.02	(0.00)	***
Proponent success executing				0.42	(0.04)	***	0.39	(0.04)	***
Sponsor tenure				-0.22	(0.03)	***	-0.28	(0.03)	***
Novelty							0.60	(0.07)	***
Strategic alignment							0.05	(0.08)	
Time to deployment							-0.26	(0.05)	***
Log pseudolikelihood	-2217.73			-2142.96			-2109.84		
Wald χ^2	172.10		***	231.74		***	347.12		***
Observations	869			869			869		

Robust standard errors are in parentheses. " $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

Model 2 includes the proponent and sponsor-level variables. They are all very significant. In particular, both the *proponent* and *sponsor success experience* at the executing gate have a positive impact on the sub-hazard rate; while the *sponsor tenure* has a negative impact. These results are confirmed in Model 3, providing support to the hypothesis 4b, 5b, but not 6b. In Model 3 also the project-level variables are added. Project's *novelty* has a significant and positive impact on the project acceptance at this gate, which contradicts hypothesis 2b, while *strategic alignment* is not significant anymore, which supports hypothesis 1b. Finally, the variable *time to deployment* has a negative and significant impact. This means that projects that require a long development work before being implementable

have lower chances to pass this gate. This is in line with hypothesis 3b.

This second decision gate seems to have different characteristics compared with the previous one, especially because in this case, all the independent variables, with the only exception of strategic alignment, have a significant impact on the acceptance of the project. This may indicate that this is a very critical phase of the venture development, and involves a higher level of complexity with respect to the previous one. This is also reflected by the fact that only 351 initiatives out of 869 (40.4%) have survived this gate.

6.4. Results of competing risk regression of initiative acceptance at the validating stage

The final step before the projects reaches the proof-of-concept is the validating one. Table 6.6 shows the results of the analysis by reporting the coefficients of the regressions (Table B.3 in the appendix reports the sub-hazard ratios). In the first specification (Model 1) only *idea source* and *budget_time2* have significant coefficients. However, both of them lose significance in the following steps. The results of the estimation of Model 3 show that both *proponent* and *sponsor success experience* have a significant impact on the sub-hazard rate, supporting hypotheses 4c e 5c. *Sponsor tenure* is not significant, contradicting hypothesis 6c. In addition, as concerns the project-related variables, the only significant ones are *time to deployment* (with negative sign), and *early involvement of deployment partners*. These results confirm hypotheses 3c and 7. This is plausible, since acceptance at this gate leads to the actual realization of the initiative. For this reason, the most influential variables are those related to the project's deployment.

Overall, the results of the regressions for this third gate show a different pattern compared with the two previous ones. In this case, only a few independent variables are significant, and all of them (but *time to deployment*) are related to the network involving proponents, sponsors, and deployment partners. It seems that surviving the last gate before the realization of the project does not only depend on the quality and feasibility of the initiative, but also on the capacity of proponents and sponsors to link with deployment partners.

Table 6.6 Results of competing risk regression (coefficients) – validating gate

Variable	Model 1		Model 2		Model 3	
Idea source	2.01 (0.60)	**	0.87 (0.58)		0.82 (0.56)	
Core domain	0.19 (0.18)		0.14 (0.21)		0.21 (0.21)	
Co-proponents	-0.01 (0.06)		-0.03 (0.07)		-0.01 (0.07)	
Before 2002	0.00 (0.18)		0.71 (0.24)	**	0.78 (0.25)	**
Budget_time2	1.02 (2.03)	***	5.52 (3.06)	"	2.29 (3.20)	
Sponsor success validating			-0.05 (0.09)	*	0.07 (0.03)	*
Proponent success validating			1.47 (0.14)	***	1.39 (0.15)	***
Sponsor tenure			-0.05 (0.09)		-0.02 (0.08)	
Novelty					0.15 (0.18)	
Strategic alignment					-0.16 (0.15)	
Time to deployment					-0.27 (0.12)	*
Early involvement of deployment partners					0.80 (0.21)	***
Log pseudolikelihood	-663.15		-596.46		-584.36	
Wald χ^2	48.93	***	182.70	***	233.39	***
Observations	351		351		351	

Robust standard errors are in parentheses. " $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

6.5. Robustness checks

The results of our analyses have been validated by some robustness checks. The first entailed conducting the same analyses using another survival model. A Cox semi-parametric model with a proportional hazard specification has been estimated using the same control and independent variables. In this kind of model the final failure event is idea acceptance and no other competing event is present. Also in this case, three models have been estimated, one for each selection gate. The obtained results are very similar both in terms of sign and significance level to those derived by estimating the competing risk models. Moreover, in

most cases, also the coefficients are very similar. These results confirm the robustness of the competing risk models used to run our analyses.

The second robustness check entailed the estimation of an alternative competing risk model where no intermediate selection gates were considered, but just a final failure event, acceptance at the validating gate, competing with the rejection one. Some of the variables included in the analysis had to be re-adjusted accordingly. This was the case for budget, sponsor and proponent success experience, and sponsor tenure, which were recalculated considering the whole time period.

The results of the estimation of this one-stage model show significant differences compared with those of the competing risk model estimated in three stages. For instance, novelty is significant in the one-stage model, but not significant in the regression model referred specifically at the validating gate. Moreover, some variables, such as sponsor tenure and strategic alignment, do not have significant coefficients in the one-stage model. However, the results obtained running three separate regressions confirmed that these variables are significantly influencing the sub-hazard rate in at least two of the three separate models. This implies that limiting the analysis just to those initiatives that survived the final gate of the selection process would lead to overlook some variables that are instead relevant to the initiative acceptance decisions at intermediate gates of the selection process. This consideration supports the appropriateness of conducting a multi-stage regression analysis which leads to more fine-grained results. The main result of our three-stage competing risk analysis was that those factors influencing initiative acceptance decisions seem to differ according to their stage of development. Estimating a single overall model would overlooks these differences, and it would not be capable of capturing the value of a multistage approach.

6.6. Concluding remarks

This chapter presented the results of three competing-risk regression models performed at each gate of the selection process, estimating the effect of several control and independent variables on the sub-hazard rates for each gate.

The main finding of the analysis was that the rate of initiative acceptance at each of the three selection gates seems to be affected by a different set of factors. In particular, the initiative acceptance at the screening gate seems to be mostly dependent on the characteristics of the project, with novelty, and strategic alignment (together with proponent success experience) showing positive and significant coefficients. The second gate seems to be the most complex, with all the independent variables, but strategic alignment, having a significant influence on the rate of acceptance. Conversely, at the last gate, only a few independent variables, mostly related to human capital characteristics, significantly influence the initiative rate of acceptance. In fact, besides time to deployment, all significant variables are related to the network involving proponents, sponsor, and deployment partners.

Nearly all the hypotheses are supported by the estimated models. With the only exception of the variable *novelty* at the executing gate (hypothesis 2b), all the hypotheses referred to initiative-related variables are confirmed (hypotheses 1a/b/c, 2a/c, and 3a/b/c). Also as concerns proponents' and sponsors' characteristics, most hypothesized relationships are confirmed. This applies both to proponent success experience and sponsor success experience (but at the screening gate), supporting hypotheses 4a/b/c, and 5b/c. Also the last hypothesis (7), predicting a positive influence of the early involvement of deployment partners on initiative acceptance at last gate of the decision making process is supported. The only truly unexpected result refers to the sponsor tenure variable. That was expected to have a positive influence on the initiative acceptance rate at all selection gates. However, our findings reveal that sponsor tenure has a negative effect on acceptance at the first two gates, and no effect at the final gate, contradicting the hypotheses 6a/b/c. One potential explanation for this unexpected result might be that a longer tenure is associated with a higher number of initiatives to support. In fact, sponsor tenure is measured in terms of number of years the sponsor has been spending working for the corporate venture unit. Based on our direct observation, sponsors that have been working longer for the unit have many ventures at different stage of development to take care of. This sort of overload may limit the amount of time and efforts they are able to devote to every single venture, hampering its chances of successfully surviving the selection gates.

CHAPTER 7
Wrap-up and conclusion

Heeding the call for more process-related studies of corporate entrepreneurial activities (Dess et al., 2003; Phan et al., 2009; Narayanan et al., 2009), this thesis aimed at extending research on the internal corporate venturing process by providing a more nuanced approach to understanding selection dynamics in the development of internal corporate ventures.

Inductive and deductive methods were combined to capture unexplored factors influencing selection decisions throughout the venture development process, overcoming limitations characterizing most studies in the field.

Following the overview of the thesis structure and underlying motivations provided in the introductory *Chapter 1*, the review of the relevant literature on internal corporate venturing reported in *Chapter 2* highlighted a substantial lack of knowledge of the process and mechanisms firms use to evaluate and select competing venture proposals. Research, so far, has mostly focused on issues related to the generation of steady flows of innovative ideas and to venture implementation, leaving aspects related to venture selection rather unexplored.

A thorough understanding of the process through which entrepreneurial initiatives evolve in the internal corporate selection environment, seems to be hindered, on the one hand, by its common conceptualization as a single-stage phenomenon and, on the other hand, by the severe limitations associated with the research methodologies used by most studies in the field to identify venture evaluation criteria and critical success factors.

Our literature review set the stage for subsequent empirical investigations. To explore how actual venture selection choices are made in a real decision-making environment a qualitative study was conducted, as described in *Chapter 3*. A two-stage procedure entailing first a small sample case-based analysis, and then the examination of a large set of longitudinal archival data was followed. In the first part, by tracking the development of eight

corporate entrepreneurial initiatives, the evaluation and selection interfaces through which they evolve were identified, unveiling the multistage nature of the underlying decision-making process. Besides empirically validating critical selection criteria such as novelty, strategic alignment and proponent and sponsor experience, already highlighted in prior contributions, our exploratory study provided evidence also on the relevance of deployment-related factors. The second part of our investigation, which entailed the analysis of a comprehensive set of archived venture files related to the evaluation process of 1,527 ideas, revealed how the importance of the identified criteria varied between different stages of venture development. While novelty and strategy-related considerations were more central to the evaluation process in the early stage of the venture development, the opposite applied to deployment-related factors, which instead were more critical in the later stages. By integrating the evidence from our case-based analysis with that provided by our archival data, a multistage model for venture evaluation and selection was inductively derived.

This model set the basis for the deductive study described in *Chapter 4*, which aimed at uncovering the determinants of initiative survival across different selection interfaces. By waving existing theories and evidence from the qualitative study, a set of testable hypotheses predicting the influence of specific venture-related and human capital factors on the rate of initiative acceptance at difference selection gates was derived. Based on a competing risk regression analysis performed on a sample of 1,527 new venture ideas, our study showed how a multistage perspective could be helpful in providing a more fine-grained understanding of the factors influencing the way initiatives evolve within the internal selection environment.

Our findings revealed that predictors of venture survival vary between different initiative development stages. The past success experience of the initiative's proponent, the initiative's degree of novelty, and its alignment with the corporate priorities enhance the initiative rate of acceptance at the initial screening gate. Consistently with the real options reasoning, decision-makers are willing to accept small bets on very novel ideas that can potentially result in huge value-creating opportunities for their parent firms. Experienced proponents, with a good track record and proven ability to support their ideas, can significantly enhance initiatives' chances to be selected in at this initial stage of development.

The most articulated phase of the selection process seems to be the executing one.

Technical and business-related aspects are much more defined at this stage, and decisions about initiative acceptance are influenced also by its time-to-deployment horizon. Contrary to our expectations, the initiative's degree of novelty enhances its rate acceptance also at this gate, while a long time-to-deployment lowers it. In addition to the proponent's success experience, also the sponsor's one is very critical, at this stage, to secure resource commitment. However, projects supported by sponsors with a long organizational tenure show higher rejection rates, probably because issues of time allocation among different initiatives are usually more relevant to sponsors with those characteristics.

In the last stage of the selection process, when a proof-of-concept has to be achieved, initiative acceptance decisions seems to be mostly dependent on deployment-related factors. A timely involvement of deployment counterparts enhances ventures' rates of acceptance, as well as having proponents and sponsors with successful venturing experiences. However, a long time-to-deployment lowers, as at the previous gate, initiative acceptance rates.

Although addressing different aspects of the internal venture selection process, the studies proposed in this thesis complement each other in providing an encompassing view of the decision-making dynamics underlying firms' venture investment choices. The longitudinal nature of our investigations and the integration of inductive and deductive methods to uncover drivers and dynamics of internal venture selection, contribute to further extant research on corporate venturing by overcoming the severe limitations posed by the research methodologies employed by most studies in the field.

The identification of stage-based differences in the relevance of specific evaluation criteria to venture selection decisions extends prior conceptualization of the related decision-making process, suggesting the adoption of a multi-stage perspective on venture selection. Disentangling the effects of venture-related and human-capital factors on the transition of entrepreneurial initiatives across different selection stages, allows to capture the micro-processes shaping venturing dynamics in the corporate context. It enables also to overcome a traditional bias of most empirical studies in this research stream, which solely focus on surviving initiatives, omitting from consideration early-stage ventures discontinued along the way. By acknowledging the contributory role those play in the creation of the internal variety enabling firms to escape inertial biases and respond to shifts in their external environments,

our study sets the stage for subsequent, and more elaborate theorizing, on internal venturing.

7.1. Theoretical and empirical contributions

Based on a longitudinal investigation of the selection and development of early-stage initiatives in a corporate venture unit, this study advances prior research on internal venturing unveiling the staged nature of venture selection decisions and the existence of stage-based differences in their determinants. Its main theoretical contributions are easier to grasp if we consider the relevance of these specific selection decisions to two broader phenomena. First, the process through which corporations adapt and change. Second, the nature of strategic decision making and resource allocation within companies.

As far as the former is concerned, our study challenges the structural inertia hypothesis characterizing models of organizational ecologies (Hannan & Freeman, 1984), showing that corporate venturing units contribute to create internal variety that enables their parent firms to adapt to changing environments and evolve over time.

Ecological perspectives on intra-organizational processes view organizations as entities evolving through variation, selection and retention dynamics (Wholey & Brittain, 1986; Nelson & Winter, 1982). They provide useful theoretical lenses to interpret internal corporate venturing phenomena, depicting firms as ecologies of innovative initiatives competing for limited resources in an internal selection environment (Burgelman, 1984; 1991). However, selection mechanisms have been often dealt with in abstract terms in these perspectives (Betton& Dess, 1985; Johnson et al., 2006; Baum & Shipilov, 2006) leaving many important aspects of the micro-processes underlying firms' venturing dynamics unexplored. Our study adds to these perspectives showing how selection operates in corporate venture units and identifying key factors influencing initiative survival in that context.

A further contribution of our study relates to the real options perspective. Despite its appropriateness to explain strategic investments under high uncertainty has been widely acknowledged (McGrath, 1999; MacMillan & McGrath, 2000; Alvarez & Stenbacka, 2001), this approach has been only limitedly applied to the entrepreneurship context. Critics centered

on the hurdles firms experience in exiting or discontinuing their strategic investments have challenged the value of the real options reasoning in explaining high-uncertain investment choices, such as the venturing ones (Bowman & Moscowitz, 2001; Adner & Levinthal, 2004). Our findings, however, show that when structured staged investment systems are adopted, so that potential losses can be limited to the amount of the initial investments, and dedicated funds are available, as in the case of corporate venture units, also high-variance investment opportunities are pursued. These low-loss experiments enable firms to explore domains that are distant from those they are currently in, broadening the scope of their search trajectories.

Finally, the results of our study suggest the need to extend conclusions drawn from traditional behavioural and resource-based arguments, used to explain corporate resource allocation processes, by integrating neglected mechanisms that may allow to account for firms' decisions to engage in high-risk and uncertain initiatives. Such mechanisms refer, for instance, to strategies employed by venture proponents and sponsors to influence decision-makers' judgements about their proposed initiatives. By framing them in a way that lowers their perceived risk, also highly uncertain projects may end up being selected.

Empirical contributions

The anti-failure bias characterizing most studies on corporate venturing, as effectively pointed out by McGrath (1999), besides limiting our understanding of how also discontinued ventures can contribute to the creation of valuable resources and capabilities, is reflected in their sampling strategy. Most of them have solely focused on ventures that were successfully implemented, omitting from consideration ventures that were initiated but discontinued over time, generally treated as failures. Our study overcomes this bias towards successful and mature initiatives, common to most research in the field, focusing on the whole population of ventures initiated and developed by a focal corporate venture unit since its creation to the end of the chosen study period. This enabled us to identify not just the factors that influenced the final venture selection decision, but also those that influenced the transition of the venture throughout different development phases. A further key empirical contribution of our study derives from its reliance on a unique large-scale project-level dataset. The objective nature of

our data, which were all extracted from the corporate venture unit's central database, allowed us to overcome the severe limitations, in terms of accuracy and validity of our results, posed by the experimental and post-hoc research methods used by most studies in the field.

7.2. Implications for managerial practice

Several implications for the management of internal corporate venture units can be drawn from the results of our study.

First, to improve the outcomes and effectiveness of their selection processes, and ensure that their venture portfolios reflect corporate needs to explore new options in new areas, internal venturing units have to employ selection mechanisms balancing the need of avoiding a detrimental escalation of commitment with that of preventing a premature rejection of early-stage ideas. Multistage selection models, entailing go/no-go decisions at different phases of the venture development, seem to address this need.

Second, some key factors influencing the outcomes of the initiative selection process are under management control, and if properly adjusted, may enhance the rate of initiative success. Based on our findings, sponsor role can be very critical to venture survival, especially in the later phases of development, when severe technical and commercial hurdles need to be overcome to secure resource commitment. Assigning expert people, able to understand the technical, market and financial aspects of a venture, and to deal with the rest of the organization, becomes crucial to move the venture forward in the selection process. Equally critical, although also at the early stage of a venture development it is the role of the initiative proponent. His ability to navigate the internal corporate selection environment is a function of his prior experience with the venture development process. However, to help also less experienced proponents effectively manage different venture development activities, corporate venture units may decide to set up specific training programs or learning schemes.

Third, involving potential deployment counterparts early in the venture development process enhances initiatives' chances to survive internal selection and find a suitable organizational home. By taking into account deployment partners' opinions in the

development of new products or technologies, or by involving them in venture evaluation panels, an early ownership of the developed solutions is encouraged. Corporate venture units can train venture proponents and sponsors to stimulate this early involvement of their potential deployment partners, thus lowering the risk of crossing "the valley of death"(Markham et al., 2010).

7.3. Limitations and avenues for future research

Although benefiting from a privileged access to a rich source of qualitative and quantitative data that allowed to capture unexplored dimensions of the corporate venture selection process, our study suffers from some limitations that open up opportunities for future research.

First, it focuses on selection decisions made by a specific organizational entity, a corporate venture unit, purposefully designed to pursue early-stage initiatives in a large firm. Therefore, although generalizable to other companies with internal venturing units or analogous organizational entities, we would be reluctant to assert that the initiative selection and development model described in our study is representative of what happens in firms lacking those structurally differentiated entrepreneurial vehicles. Future studies should, then, investigate to what extent our findings apply to initiatives developed within companies adopting a distributed approach to corporate entrepreneurship (Birkinshaw, 2000).

Second, no environmental influence is taken into consideration in our model. For instance, changes in governmental policies or restricted access to critical resources or capabilities may shape corporate venture selection decisions, and direct them towards specific target domains. Further studies should analyse the influence of these external sources of variance and link them to corporate venture selection patterns (Dietl et al., 2009).

Third, despite its longitudinal nature, our investigation doesn't account for possible variations in the relevance attributed by the corporate venture unit to the identified selection criteria over time, as a result of changes in the corporate-level strategy. The latter might lead to a re-orientation of the goals and type of innovative activities firms' internal venturing units pursue, thus influencing the weight assigned to the criteria guiding their initiative selection

decisions. This is a proposition that should be taken up by future research.

Fourth, our analysis has been restricted to the identification of the determinants of initiative acceptance at different selection gates. Since discontinued ventures are an important complementary outcome of the selection process, extending the scope of the investigation also to factors affecting initiative termination rates might provide additional insights. In fact, as shown by Hoang and Rothaermel (2010) in their study on drug approval, factors increasing the rate of project acceptance don't necessarily decrease the rate of project termination.

Besides those directly stemming from its highlighted limitations, additional potentially fruitful research avenues are suggested by our study. In relation to discontinued ventures, the analysis of their contribution to the development of new capabilities through, for instance, the introduction of new patents during some intermediate development stages, would allow to shed further light on the value-creation role of venturing initiatives that don't result in commercial successes. Interesting insights might also be gained from the investigation of the retention phase of the internal venturing process, in conjunction with the selection one (Burgelman, 1991). With a focus, this time, on those ventures that survived the selection process and achieved a working proof-of-concept, factors explaining their subsequent successful or unsuccessful implementation would be worth exploring. Finally, our findings suggest that the real options perspective is particularly appropriate to explain investments in high-uncertain projects, such as early-stage ventures. Its combination with resource allocation and decision making arguments promise to be an interesting area of future research.

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Appendix

Table A.1

Corporate Archives	**Books** published by ENERCO between 1996 and 2010 1. *book on emerging technological areas* 2. *book on energy scenarios to 2050* 3. *booklets on GC activities and process* written by GC managing director and lead scientists 4. *booklets on GC success stories and selected ongoing venturing projects* written by GC managing director and lead scientists 5. *technology portfolio reviews* updated every 6 months by a team of senior research managers	The textual data we rely on, provide background information on the major trends in the energy industry and the related innovation trajectories followed by most players. They help us contextualize the role of GC in the ENERCO's innovative activities, and identify the critical steps of its venture development process.
	Internal presentations 1. GC process 2. GC project evaluation lenses 3. GC domains and portfolio overview 4. Innovation in the energy sector and strategic innovation projects 5. R&D portfolio management	Presentations used by GC's and ENERCO's managers to illustrate the strategic innovation areas and domains of interest. They provide additional support to our understanding of GC evaluation criteria and selection dynamics, and their implications for portfolio management decisions.
	Video-taped archival interviews recorded by ENERCO with former management (no longer available for interviews): JvdV (former CEO); JvdE (former group chief technology officer); DM (former technology implementation manager); JL (former VP for R&D).	This recorded material was used to triangulate observations and documental facts. It facilitated our understanding of how the GC process enables the development of new business initiatives, and of the critical issues that need to be addressed for their successful deployment.
	Database containing semi-structured minutes related to the evaluation process of 1,527 venturing ideas submitted between 1996 and 2009. It also includes presentations, project plans and budgets referred to each of the proposed ideas.	These venture-level data enhanced our efforts to grasp the GC project selection criteria and dynamics, and identify the key roles to the process
Interviews	**First round (Feb-Apr. 2008),** 14 interviews with 9 members: 1. VP for EP R&D 2. Group strategy executive 3. Managing director of ENERCO's Internal Venture Capital Fund 4. GC director, senior and junior scientists	Broad questions about the genesis, structure, goals and practices of the GC unit. Informants discussed the role played by GC in the ENERCO's new business creation and technology development activities, and described the standard GC venture selection process and the main underlying evaluation criteria.
	Second round (Mar-Jun 2009), 28 interviews with 17 members 1. Managing director of ENERCO's Internal Venture Capital Fund 2. Proponents and sponsors for GC ventures 3. Participants in GC review panel meetings	Timelines with key events in the development process of the selected ventures were defined with the informants' help. Critical issues and survival determinants were discussed, as well as the role of proponents and sponsors in moving the projects forward. The appropriateness of alternative deployment strategies to different types of ventures was also addressed to derive predictable patterns.
Other archival sources	1. **Publications** on GC written by management scholars (Hamel, 2000; Valikangas & Gibbert, 2005; Hansen & Birkinshaw, 2007; Birkinshaw & Hill, 2008) 2. **Reports** on the energy industry and the EP business (Datamonitor; Society of Petroleum Engineers)	This material was used to triangulate facts and observations about the GC unit. It enriched our understanding of how the GC process works and enables the identification and exploitation of new business opportunities in the ENERCO's EP business.

Table B.1 Results of competing risk regression (sub-hazard rations) – screening gate

Variable	Model 1			Model 2			Model 3		
Idea source	1.30	(0.14)	*	1.11	(0.13)		1.22	(0.14)	"
Core domain	1.28	(0.09)	**	1.09	(0.08)		0.77	(0.06)	**
Co-proponents	1.22	(0.03)	***	1.19	(0.03)	***	1.14	(0.02)	***
Before 2002	1.32	(0.09)	***	1.54	(0.12)	***	1.76	(0.13)	***
Sponsor success screening				0.98	(0.00)	**	0.99	(0.00)	
Proponent success screening				1.22	(0.02)	***	1.17	(0.02)	***
Sponsor tenure				0.98	(0.03)		0.94	(0.02)	*
Novelty							1.68	(0.08)	***
Strategic alignment							1.26	(0.05)	***
Time to deployment							0.96	(0.03)	
Log pseudolikelihood	-5987.62			-5889.92			-5771.84		
Wald χ^2	130.99		***	264.28		***	647.40		***
Observations	1527			1527			1527		

Robust standard errors are in parentheses. " $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

Table B.2 Results of competing risk regression (sub-hazard rations) – executing gate

Variable	Model 1			Model 2			Model 3		
Idea source	0.40	(0.07)	***	0.36	(0.06)	***	0.39	(0.07)	***
Core domain	1.23	(0.13)	"	0.99	(0.12)		0.92	(0.11)	
Co-proponents	1.09	(0.03)	*	1.08	(0.03)	*	1.06	(0.03)	"
Before 2002	0.65	(0.07)	***	0.77	(0.10)	"	0.95	(0.13)	
Budget_time1	1.00	(5.68)	***	1.00	(6.66)	***	1.00	(5.98)	***
Sponsor success executing				1.02	(0.00)	***	1.02	(0.00)	***
Proponent success executing				1.52	(0.07)	***	1.48	(0.07)	***
Sponsor tenure				0.79	(0.02)	***	0.75	(0.02)	***
Novelty							1.83	(0.14)	***
Strategic alignment							1.05	(0.08)	
Time to deployment							0.77	(0.04)	***
Log pseudolikelihood	-2217.73			-2142.96			-2109.84		
Wald χ^2	172.10		***	231.74		***	347.12		***
Observations	869			869			869		

Robust standard errors are in parentheses. " $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

Table B.3 Results of competing risk regression (sub-hazard rations) – validating gate

Variable	Model 1		Model 2		Model 3	
Idea source	7.50 (4.50)	**	2.39 (1.40)		2.28 (1.28)	
Core domain	1.21 (0.22)		1.15 (0.24)		1.23 (0.25)	
Co-proponents	0.98 (0.06)		0.96 (0.07)		0.98 (0.07)	
Before 2002	1.00 (0.18)		2.04 (0.50)	**	2.18 (0.55)	**
Budget_time2	1.00 (2.03)	***	1.00 (3.06)	"	1.00 (3.20)	
Sponsor success validating			1.07 (0.03)	*	1.07 (0.03)	*
Proponent success validating			4.38 (0.62)	***	4.03 (0.62)	***
Sponsor tenure			0.94 (0.08)		0.97 (0.08)	
Novelty					0.17 (0.21)	
Strategic alignment					0.85 (0.12)	
Time to deployment					0.76 (0.09)	*
Early involvement of deployment partners					2.23 (0.48)	***
Log pseudolikelihood	-663.15		-596.46		-584.36	
Wald χ^2	48.93	***	182.70	***	233.39	***
Observations	351		351		351	

Robust standard errors are in parentheses. " $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$