Multiple Missions and Academic Entrepreneurship*

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Abstract

This paper analyzes the choice of academic scientists to commercially exploit their research. I build a model of the timing of entry into commercial activities by an academic research team, and analyze the returns and costs of these activities. In order to focus on the peculiarities of academic entrepreneurship as opposed to industrial entrepreneurship, I compare the behavior and performance of the academic team to an industrial research team. The two teams are assumed to differ in their objectives, governance modes and incentive systems. I show that, while in some cases academic scientists are more reluctant to commercialize research, in other cases they may commercialize faster than profit-seeking firms would — and perform less basic research. I also derive that academic scientists tend to enter commercial projects with higher returns than industrial actors, and therefore a self-selection mechanism may explain the success of 'academic entrepreneurs'. This study helps interpreting the mixed evidence on the success of, and the arguments in favor and against the involvement of universities into business-related research activities. I also identify and discuss a series of implications for empirical analyses of the commercialization of academic research.

Keywords: Academic Entrepreneurship, Research and Development, Economics of Science, Science and Technology Policy.

JEL classification: I23, L21, L31, M13, O31.

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1 Introduction

The issue of commercialization of academic activities or 'academic entrepreneurship' – intended as the involvement of academic scientists and organizations into commercially relevant activities, in different forms[1] – has received great attention over the past 30 years. Several observers have pointed to academic research as an underutilized resource for a country's competitiveness, because academic research was too distant from practical applications, and of not easy transferability and applicability (see Slaughter and Rhoades 1996 for an account of these claims). Policy makers intervened with several provisions, such as the 1980 Bayh-Dole Act and the 1986 Federal Technology Transfer Act, in order to stimulate universities to undertake more industrially relevant research. While originally confined to the United States, more recently the role of universities for industrial success has received increasing attention also in Europe and Japan (Henrekson and Rosenberg 2000, Geuna et al. 2003, and David 2005), for both policy and managerial implications.

In the scholarly debate, the increasing interest in academic entrepreneurship has stimulated a broad and lively debate, and data from numerous sources have been collected to empirically analyze the process of academic involvement into commercialization and its economic and social consequences. Positions on the economic impact of the commercialization of academic research vary greatly. On the one hand, some scholars argue that the direct involvement of academic scientists into industrially relevant activities would solve some imperfections in the transmission of knowledge, and will also motivate researchers to undertake projects with greater economic and social relevance (Gibbons et al. 1994, Zucker and Darby 1995, Ezkowitz 2004)[2]. On the other hand, some observers are skeptical about the ability of academics to manage commercial activities, while still abiding by the rules and missions of academia and more generally of the scientific community. These rules are seen as in marked contrast with the profit seeking approach that characterizes commercial activities (Dasgupta and David 1994, Stern 1995, Nelson 2004).

Just as the theoretical and policy debates are controversial, so is the available empirical evidence. A few studies document the success of academic entrepreneurs, both when they start their own companies and when they collaborate with existing firms (Zucker and Darby 1995, Cockburn and Henderson 1998, Nerkar and Shane 2003, Rothaermel and Thursby 2005, Agrawal 2006, among others). Other evidence shows that commercial ventures involving academic scientists are often not successful (Kenney 1986, Lerner 2004, Lowe and Ziedonis 2005).

Despite the vast attention directed toward the issue, the state of the debate is still incomplete. In particular, very little is known about whether academic entrepreneurship is *different* from private-firm entrepreneurship. A deeper understanding of this point is not

[1] Industry-university collaborations, university-based venture funds, university-based incubator firms, start-up founding by academicians, double appointments of faculty members in firms and academic departments, etc. The same definition of academic entrepreneurship is used by other authors, such as Henrekson and Rosenberg (2000) and Franklin et al. (2001)..

[2] As reported by David (2005), the European Commission has repeatedly stated that universities have the potential to be more effective than European industry in high tech sectors.

only of intellectual interest but also of relevance for policy makers and managers. In order to evaluate the role of universities for the successful commercialization of research, and for an 'appropriate' balance between research activities and commercially oriented activities, we need to understand to what extent universities and academic scientists offer something that other actors, e.g. 'pure' firms, cannot replicate[3]. If the commercialization of research takes place in universities the same way it does in firms, there would be no reason to involve academic organizations into entrepreneurial activities. Moreover, since some forms of academic entrepreneurship occur through collaborations with companies, managers need to understand the organizational and institutional peculiarities of universities in order to properly evaluate the returns from collaborations, and to anticipate the behavior of their academic partners.

This paper analyzes the differences between academic and industrial entrepreneurs through the study of two key decisions of academic scientists when they have the opportunity to undertake commercially relevant work: the decision whether to undertake the commercial opportunity, and the timing of entry into the commercial venture. I build an economic model of the decision to commercialize research and of the timing of entry into commercially oriented activities by an academic research team, and of the returns and costs associated with these activities. In order to identify the peculiarities of academic entrepreneurship, I compare the outcomes obtained by the academic team with those of an industrial research team facing the same choices. The model is based on three key ingredients that characterize the research process: the assumption of the cumulative nature of knowledge (i.e. current knowledge production builds on previous knowledge), the presence of different forms of scientifically valuable knowledge, and the consideration of the institutional differences among the organizations performing research. The simultaneous consideration of these three characteristics of the research process is novel in the Economics and Management literature on Science and Innovation. I model cumulativeness by assuming that the cost of performing commercially oriented activities (development, commercialization, etc.) is lower if a team has previously performed some pre-commercial (or fundamental) research. Fundamental research has therefore an investment value. I also assume that the research teams choose among different types of fundamental research, more or less applicable to practical problems. In accordance with an institutional approach to the analysis of science (as introduced in Sociology by Merton (1957, 1973) and in Economics by Dasgupta and David 1994), the academic and the industrial teams are assumed to differ in the objectives they pursue, in the incentive systems they respond to, and in the organizational structure in which they perform research activities. In particular, I assume that academic scientists derive direct benefit from the performance of fundamental research with no direct economic value, for example in the form of publications and peer recognition. The benefit, in turn, may depend on the type of fundamental research that is performed, if some types of research are more consistent with the way the reward and

[3]These considerations are relevant also in relation to the internal organization of firms and to the provision of 'academic' incentives to company scientists, e.g. bonuses and promotions based on their recognition in the community of peers (see Henderson and Cockburn 1994, Stern 2004): to what extent, and under which circumstances, are these incentives beneficial? What kinds of behavior do they induce, that standard, 'monetary' incentives are not able to induce?

recognition system works in the scientific community. Moreover, just like industrial actors, academic scientists respond to economic incentives. Academic entrepreneurs are therefore characterized as entrepreneurs with multiple affiliations and missions. Research activities have both an investment value, and an immediate consumption value for academics.

The results of the model can be summarized as follows. While in some cases academic scientists are more reluctant to move to commercially relevant activities, in other cases they even move faster than profit-seeking firms would. On the one hand, academic scientists derive direct benefits from the performance of pre-commercial research; this reduces the likelihood that academic scientists will move to commercially oriented research. On the other hand, if the kind of basic research that scientists are more motivated to perform in academia is not easily applicable to commercially relevant research, then academic scientists, despite the consumption value they derive from performing basic research, may find the investment value too low and prefer to move to commercially relevant research soon. On the contrary, industrial researchers have incentives to perform fundamental research more easily applicable to commercial problems (for example, multidisciplinary and/or idiosyncratic to a particular problem); this would make the cost-reducing investment in fundamental research more profitable. The timing of entry, moreover, determines also the costs, and therefore the commercial profitability of the research effort: the later the entry, the lower the costs of transition from fundamental to commercial research. Two implications emerge from these findings. First, a trade off between timing of entry into commercialization and cost effectiveness exists, and different organizations solve it differently. Second, academic scientists tend to enter commercial projects with higher returns than industrial actors, because they derive positive utility also from not entering commercial activities and continuing to do fundamental research, or their opportunity cost of undertaking commercial activities is higher. Therefore, a self-selection mechanism is present.

This analysis helps reconciling the contrasting evidence on the outcomes of the commercialization of academic research, as well as the arguments in favor and against the academic involvement into commercial activities. The model identifies, for example, the environmental conditions and project types such that an academic research team would produce a greater (and possibly socially desirable) amount of research before moving to commercialization, and the cases in which an academic research team would be too slow or too fast in moving to commercialization, and more or less effective in performing both fundamental research and commercially oriented work. Moreover, the results of my analysis imply that some of the existing evidence on the success of academic entrepreneurs needs to be taken with caution, because of the self-selection mechanism mentioned above: the commercial ventures academicians enter are different from those undertaken by companies.

In Section 2, I review the literature on the commercialization of academic research, and point out the limits of the existing debate and evidence. In Section 3 I offer an informal description of the main features of the model. I also position my work within a recent tradition of theoretical analyses of the organization of research in academia. Section 4 is dedicated to the formal description and analysis of the model. Section 5 discusses the managerial and policy insights of my findings. Section 6 outlines the theoretical extensions of the analysis and proposes empirical strategies and settings for assessing the plausibility of the assumptions and for testing the results of the model. Section 7 offers a concluding summary.

2 Current debate and literature

2.1 Review

Several authors claim that since universities perform fundamental science and this basic knowledge is increasingly important in high-technology sectors and more generally in the knowledge economy, and because knowledge may be hard to transfer, it is desirable to directly involve academic organizations and scientists into commercially oriented activities. Moreover, these scientists would be 'disciplined' by such commercial involvement, since they would choose research projects still of scientific value, but also with practical applications. Academic scientists will therefore strike a virtuous compromise between the production of scientifically relevant knowledge, and the translation of this knowledge into economic and social value (see for example Gibbons et al. 1994, Zucker and Darby 1995, Stokes 1997, Ezkowitz 2004, and Agrawal 2006).

A vast empirical literature has provided evidence consistent with these claims. Several studies have shown that the presence of academic scientists in start-up and young, science-intensive companies (e.g. in biotech and semiconductors) has a positive impact on the innovative and financial performance of these firms (Zucker and Darby 1995, Zucker et al. 1998, Torero et al. 2001, Nerkar and Shane 2003, Shane 2004, Stephan et al. 2004, Rothaermel and Thursby 2005, Toole and Czarnitzki 2005). Other works have found that the direct involvement of academicians positively affects the innovativeness and profitability also of large, established firms (Zucker and Darby 1997, Cockburn and Henderson 1998). Some scholars showed that the 'reproduction' of academic incentives within the firm, e.g. by tying company scientists' bonuses and promotions to their standing in the scientific community, positively affects the innovative performance of firms (see for example Henderson and Cockburn 1994). Taken together, these results and claims imply that there is a special role for academic scientists and academic incentives in the performance of commercially relevant research that builds on the fundamental discoveries that these scientists have already achieved.

Other scholars are skeptical about the ability of academic scientists (and more broadly academic organizations) to manage commercial activities efficiently. These scholars claim that academic scientists are part of a peculiar institutional environment, the scientific community. The scientific community's mission is the production and timely diffusion of scientifically relevant knowledge. The priority rule for rewards and recognition, the disciplinary organization of research and discipline-based evaluation, and the open diffusion of the results of research are key instruments to efficiently achieve the mission. The simultaneous presence of multiple missions (i.e. the addition of commercial incentives to the academic incentives) would eventually generate tensions and cannot be sustainable. Academic scientists, for example, would need to give priority to one or the other environment they are affiliated to. One the one hand, they might delay or forego commercial opportunities. On the other hand, they may instead focus on commercialization, 'rushing' to commercial activities and neglecting their academic duties. Academic scientists would therefore be unable to balance the performance of scientifically relevant research and commercially oriented activities (among others, see Dasgupta and David 1994, Stern 1995, Heller and Eisenberg 1998, and Nelson 2004).

In fact, we see these tensions in some historical examples of research with commercial potential conducted by universities, as well as in several more recent cases and in large-sample studies. Historical cases of important discoveries show that, even when academic researchers had accumulated a good deal of the relevant knowledge required to obtain economically profitable results, and there was awareness of the economic relevance of these results, commercial research labs working 'in parallel' reached the results faster. Examples include the discovery of the transistor and the synthesis of human insulin[4].

Regarding more recent case studies, Kenney (1986) provides examples in which commercial activities performed by academic researchers produced poor results. Argyres and Liebeskind (1998) document that several attempts by universities to generate companies have been received with diffidence by private investors, because the institutional and organizational arrangements were not deemed as economically promising. Lerner (2004) reports the difficulties that academic organizations encountered when they directly engaged in sponsoring industrial research activities (see also Bok 2003).

As for large-sample evidence, Doutriaux (1987) finds that companies involving academics are likely to grow more if the academics give up on their commitments with the university (in the language of Franklin et al. 2001, they are not academic entrepreneurs any longer, and they are instead 'surrogate' entrepreneurs). Audretsch (2000) argues, and empirically shows that academic researchers tend to undertake entrepreneurial activities in later stages of their lives as compared to non-academics, since they respond to different incentives and have different priorities. Academic entrepreneurs would therefore delay or forgo the introduction of some innovations. A survey by Hall et al. (2000) reveals that the involvement of university partners in research projects tends to delay commercialization. Lowe (2002) finds that academic researchers start their companies around early stage discoveries, when still basic research has to be performed and extra-work is needed in order to make the discoveries profitable. Calderini et al. (2004) find that academic scientists with very high quality publications are less likely to appear as inventors in patents assigned to firms. The authors hint at an 'adverse selection' process, in which firms generally collaborate with academic scientists of lower quality. Lowe and Ziedonis (2005), while showing that university start-ups tend to perform no worse (and possibly better) than new entrepreneurial activities started by established firms, also show that the presence of the academics inventors among the founders of a company negatively affects some performance indicators. Interestingly, Rothaermel and Thursby (2005), while finding that incubator firms with an active involvement of academicians have lower rates of failure, also find that these firms take longer to be 'promoted', i.e. to exit

[4]See for example Nelson (1962), Braun and Macdonald (1978), Hoddeson (1980), Bray (1997) for the invention of the transistor; and Hall (1987), Stern (1995), and McKelvey (1996) for synthetic insulin.

from the incubator and become independent companies. With reference to licensing activities, Jensen and Thursby (2001) find that the academic research disclosed to Technology Transfer Offices (and that will be commercialized in the form of licenses) is most often in very early stages. At the same time, many potentially profitable discoveries are never disclosed and stay 'shelved' in the labs of academic professors.

2.2 Discussion

The positions and the evidence previously described witness the richness of the debate and the research on academic entrepreneurship. However, there are some questions that the existing literature has not satisfactorily dealt with. Let me focus on three issues.

First, those studies that express concerns about the ability of academic organizations to successfully undertake entrepreneurial activities offer a very stylized view of the activities of companies and universities. A purely institutional approach, which typically characterizes the skeptical views, implies that firms never perform fundamental research, and universities never perform commercially-related activities. Neither is true. In particular, profit-seeking companies perform scientifically valuable research within their boundaries and through their own scientists. What makes this fundamental research more applicable to practical problems? A more detailed analysis of the role of fundamental research, of the various types of scientifically relevant research that organizations perform, and more generally of the nature of scientific knowledge, is in order.

Second, most of those who see the commercialization of academic activities with favor basically exclude a peculiar role for universities in the commercialization process. The 'academic entrepreneur' these works refer to, both in theoretical and quali-quantitative analyses, rarely has any connotation specific to the academic environment, and is represented as an individual (or a team) with some ideas or scientific discoveries that can be marketed. However, why do we need to assume that this inventor is an academician? Why can't these ideas emerge outside of the academic environment? A better understanding of the differential impact of academic entrepreneurship, as opposed to other forms of entrepreneurship, requires some precise characterization of the institutional and organizational features of academia, which university scientists are subject to. We also need an institutional benchmark or counterfactual against which to evaluate the commercial activities of academic organizations. What are the objectives of academic organizations, and more generally of the scientific community? How are universities organized, and how are academics rewarded? The institutional approach outlined in the previous section offers important insights for the characterization of the *academic* entrepreneur. Moreover, if university involvement into commercially relevant research were so crucial, we should not have observed so many failures, or diffidence by investors, nor should we see company labs developing their own relevant fundamental research and 'outperform' academic labs working on similar topics, and without necessarily replicating exactly the incentives and rules of academia.

The third reason of dissatisfaction concerns the state of the empirical research. The existing studies lack a common definition of performance, and it is therefore difficult to compare potentially contrasting results. Most likely, the impact of academic entrepreneurs should be evaluated along a range of performance measures rather than with respect to one-dimensional measures. Another problem in existing empirical studies is a more conceptual one. From the point of view of the single scientist or research lab, undertaking commercially related activities (founding a firm, licensing research results, keeping a stable relation with a company, etc.) is a *choice* to be weighed against alternatives, e.g. spending more time in other academic activities. Inferring any causal relation between scientist involvement and performance, in absence of these corrections for selectivity (indeed missing in the existing studies), would be misleading, both for descriptive and normative purposes[5]. This selection problem is related to the importance of properly characterizing *academic* entrepreneurship, i.e. including in the analysis considerations on the peculiarities of the academic institutional environment.

3 Modeling academic entrepreneurship

3.1 Informal description of the model

The study I propose is meant to address the issues raised in the previous section. The objective is to characterize and analyze the behavior of academic organizations when they have the option to undertake commercially relevant activities. The analysis is conducted through a model of the decision to undertake commercially oriented activities by an academic research team, of the timing of entry into commercialization, and of the returns and costs related to these activities. I then compare the behavior and performance of the academic team to an industrial research team. A summary of the basic features of the model follows.

1. I model three key aspects of the research process. First, I consider knowledge as cumulative, i.e. current knowledge production builds on previous knowledge. This is captured by assuming that the cost of performing commercially oriented activities (development, commercialization, etc.) is lower if a team has previously performed some pre-commercial (or fundamental) research. Fundamental research has therefore an investment value. Second, I allow for the presence of different forms of scientifically valuable knowledge, more or less applicable to practical problems. Third, the academic and the industrial team are assumed to differ in the objectives they pursue, in the incentive systems they respond to, and in the organizational structure in which they perform research activities. In particular, I assume that academic scientists derive direct benefit from the performance of fundamental research

[5] Consider the following examples. Lenoir (1997) describes the creation of Varian Associates in the late 1940s by some Stanford physicists and engineers, for the development of Nuclear Magnetic Resonance instrumentation. He reports that Felix Bloch, a leading theoretical physicist at Stanford, decided to get involved with the company only a few years after its foundation, when the company was already growing and in good health. Murray (2004) reports the case of an academic scientist who decided to join a firm which had developed some of his research, only after the firm was able to raise money from a range of sources. Was the direct involvement of Bloch in Varian Associates causing the good financial performance? Or did Bloch joined the company once its prospects began to look good? Was the anonymous scientist described by Murray causing the firm to be able to raise money, or did the scientist made a commitment to the firm only after learning the quality of the firm and its optimistic prospects?

with no direct economic value, for example in the form of publications and peer recognition. The benefit, in turn, may depend on the type of fundamental research that is performed, since some types of research are more consistent with the way the reward and recognition system works in the scientific community. By modeling these aspects of the research process, I integrate institutional views and considerations on the nature of scientific knowledge.

2. The model compares the performance of academic entrepreneurs to the performance of other types of entrepreneurs or companies who do not formally rely on university-based knowledge and scientists. I perform the comparison by analyzing the same problem as solved by an academic team and by an industrial team. I take the two teams (and more generally the academic and industrial environment) as differing in the objectives and incentives the scientists respond to. For example, in the industrial environment the scientists respond to (or are rewarded on the basis of) commercial incentives only, and do not attribute any consumption value to performing fundamental research. Academic scientists, as mentioned in the previous point, also receive a direct benefit from the performance of research[6]. Apart from this difference, I keep all of the other characteristics of the problem as being the same, regardless of the institutional environment. The driver of the different behavior and outcomes of academic and non-academic teams is therefore in the multiplicity of missions and incentives academics respond to, when they get involved into projects with commercial value.

3. I analyze the returns from commercialization and the costs of these activities. Moreover, the model has a dynamic structure that allows me to study another relevant dimension: the timing of commercialization. Therefore, I consider multiple measures of performance.

3.2 Relation to the existing theoretical literature

The model builds on a recent tradition of theoretical works that have focused on the performance of commercially relevant research by universities. Some of these works (Jensen and Thursby 2001, Jensen et al. 2003, Dechenaux et al. 2003, and Mazzoleni 2005) study university licensing, and focus on such issues as the agency relationships between the single scientist, the university and the Technology Transfer Office and the relation between appropriability of research and the different types of licenses (exclusive or non-exclusive) arranged by universities. Jensen et al. (2003) model the positive impact that the performance of additional research can have on the expected commercial returns, and my model shares this aspect with them. However, a limit of these works is that the scientists-inventors in the models have no clear institutional and organizational connotations and their behavior is not easily distinguishable from the behavior of non-academic scientists faced with the same research problem. If this is the case, then it is not clear why we need universities to be involved in such commercialization activities[7]. My framework is different in that it models some peculiar

[6] To a large extent, the model can also be applied to, say, only industrial research teams, responding to different incentive structures to which a firm can in some way commit. However, also for its greater policy relevance, I will focus on the case of a university-based team, and will compare it to a company-based team, thus identifying the organizational location with different incentive structures.

[7] Moreover, while the analysis of the relations between scientists and the Technology Transfer Office is important, it is not the only relevant one. For example, Jong (2005) points out that, while Stanford University

characteristics of the academic environment, such as the peculiar mission and incentive system. Other works, such as Lacetera (2005) and Aghion et al. (2005), do model the peculiar characteristics of agents belonging to the scientific community, but assume that there is no response, by academic scientists, to other forms of incentives. My model considers the simultaneous presence of multiple missions and institutional rules in academia, as a consequence of the possibility for academicians to undertake commercial activities, as in Beath et al. (2003). Finally, I apply the model both to an academic team and to a company team, thus making a comparison with the 'benchmark' actor (a firm) possible.

4 The model

4.1 Set up

4.1.1 The academic team

Environment An academic team has the opportunity to complete an economically valuable research project, given the amount of knowledge available and the amount of research performed up to that moment, which we call period 0. There are two periods, $t = 0$ and $t = 1$[8]. In period 0 the team faces the following choice set: it can perform some additional fundamental research (with no direct economic applications, but with novel scientific content), and possibly move to completion and commercial application in the following period, or it can move to commercially-relevant activities right away in period 0. Commercial activities include the time spent writing a business plan to market the product, and the performance or supervision of development and marketing activities[9]. These activities are supposed to be directly performed, at least to some extent, by the scientists themselves. More precisely, what will be relevant is that the academic scientists have authority over the kind of activities (commercial versus scientific) that are performed. Unlike research activities, commercial activities have no scientific value, i.e. they do not get any recognition or attention in the scientific community. The team can also stay idle. If the team chooses to perform additional fundamental research, it also chooses how 'applicable' to the commercial project the fundamental research will be. For example, and according to a number of studies, fundamental research is more applicable if it is multidisciplinary[10]. One could also think of applicability as being related to the degree of specificity and tacitness of the fundamental research: the more tailored the research is to a given project, the higher the applicability to that issue. In Stokes'

had a sophisticated technology transfer infrastructure, its scientists were much less entrepreneurial, at least in the biotechnology sector, than their colleagues at UC San Francisco, where there was not a comparable technology transfer infrastructure. It turns out, moreover, that some of the characteristics that Jong underlines as potentially explaining the entrepreneurial success of UCSF scientists, are also present in my model – in particular the attitude toward interdisciplinary research.

[8] Appendix B at the end of the paper begins to sketch an infinite (discrete) time version of the model.

[9] Kelvin Gee (2001), a pharmacologist at UC Irvine, offers some examples of these commercially related activities, which he has directly performed while keeping his academic position.

[10] See, among others, Rosenberg (1994), Stern (1995), Brewer (1999), Llerena and Meyer-Kramer (2003), Rinia et al. (2001), Carayol and Thi (2003), Boardman and Bozeman (2004). Several practitioners I interviewed stressed the importance of multidisciplinary research for industrial application of basic knowledge.

(1997) terminology, the team decides whether the fundamental research is going to be more into the Bohr's quadrant, i.e. it is performed without any interest for practical applications, or more into the Pasteur's quadrant, i.e. it is aimed at producing both scientifically valuable and practically useful results.

Examples Consider a case where the current state of knowledge can lead to the development and the commercialization of a particular technical device. Developing the device is plausibly more effective if knowledge from several disciplines is brought together in order to complete the project. For example, investing in this multidisciplinary, pre-commercial knowledge may prevent or solve problems that can emerge in the development and in the use of the innovation. However, scientists can also opt for proceeding along well-defined disciplinary paths, for example focus on the properties of a given material. Researchers at Purdue University in the 1940s, for example, struggled with this dilemma, when they had to decide how to proceed in their research effort on semiconductors: proceed through single-disciplinary paths, for example explore in more detail the properties of materials like germanium (see Bray 1997), or try to converge several lines of research and explore the practical implications of the available knowledge, as researchers at Bell Labs eventually did with the discovery of the transistor. Or, consider research in biology and the possibility to bring some findings to pharmacological applications. Again, this is typically going to be easier if a research team has accumulated, through pre-commercial research, also knowledge from other disciplines, such as chemistry and physiology. Alternatively, scientists may just explore biological properties through their single-disciplinary lenses. Academic scientists engaged in the research on synthetic insulin in the 1970s faced similar choices when they could move their research from the labs to pharmacological applications (Stern 1995). I will return on the transistor and the synthetic insulin cases (as well as on other examples) in several points of this paper.

Commercial returns If the team moves to commercial activities, there is a probability $p \in (0,1)$ that the project will be completed (economic returns are earned at completion). If the team performs research in period 0 and the completion is successful, the team earns a return $R > 0$ and there are no more choices to be made. The academic team, therefore, cares about the completion of the project and about receiving extra-revenues from commercialization of their research[11]. Let us define

$$pR = \text{Expected (gross) return from commercial activities} \tag{1}$$

Choice set in period 1 If the team does not move to commercial research in period 0, or does move but fails to complete, in period 1 it has the same choice set as in the previous period. Notice that in period 1 the team has a probability of commercial success equal to p, but no extra-attempts if the project is not successfully completed.

[11] Financial rewards for academic scientists can be substantial. See Stephan and Everhart (1998).

To summarize, the academic team u, in period 0, chooses $a_0^u \in \{s, c, \emptyset\}$. The superscript u stands for 'university'; s stands for fundamental research (or 'science'); and c for commercially-related research activities. $\emptyset$ indicates that the team stays idle for that period. The choice set a_1^u in period 1 is the same, unless the team has chosen c in period 0 and the project has been successfully completed[12]. There is no discounting between the two periods.

Commercialization costs and applicability of research The cost of commercial activities is borne only once, when the team enters commercialization (i.e. chooses c for the first time). Define

$$C_c^u = \begin{cases} K \text{ if } a_0^u = c \text{ or } \emptyset \\ K - \gamma^u \text{ if } a_0^u = s. \end{cases} \tag{2}$$

If the team enters commercialization in the second period ($t = 1$) after having performed fundamental research in the first period, the cost decreases to $K - \gamma^u$, where γ^u measures the level of applicability of the fundamental research to commercial research. K is given, while γ^u is chosen by the team each time the team undertakes fundamental research. Therefore, in each period $t = 0, 1$ the complete choice set is $\{a_t^u, \gamma_t^u\}$. By entering commercialization in the second period after having performed 'applicable' fundamental research in the first period, the team gives up the option of a second try. However, it incurs in lower costs through the spillover of knowledge from fundamental research to commercially related activities.

Through this parametrization of the cost function I introduce cumulativeness of knowledge, since previous knowledge produces spillovers on current activities and makes them less costly to perform. For example, a deeper knowledge of some basic properties facilitates the solution of more practical problems that can arise during the development phase, by guiding the search for solutions toward specific directions. Or, the performance of research increases the absorptive capacity of a team, i.e. its ability to exploit the publicly available knowledge and also to commercially profit from it (Cohen and Levinthal 1990)[13].

[12] The assumption that there are no actions in the last period, if commercialization is undertaken in $t = 0$ and is successful, is a restrictive one. We could expect, for example, the academic team to perform some additional research after the project is completed, if doing this brings extra-utility, or to receive extra commercial returns from one additional period of commercialization. Jensen et al. (2003) make an assumption similar to mine in their model: if the academic inventor discloses her invention in the first stage of the game, and the Technology Transfer Office finds an acquirer, then the game ends and there are no more periods of research activities. The game has indeed potentially a further stage, to which the parties end up if the academic scientist performs extra-research *before* disclosing. In my model, just as in theirs, the unit of analysis is a single project (apart from the presence of an alternative project in their model, and of the choice to stay idle, and earn zero utility, in mine), and once the project is completed, no other projects are available. We can imagine that the project has no additional commercial value after the first date in which it is successfully commercialized, say because others can imitate it in the immediately subsequent period, nor it has any additional scientific novelty content after commercialization of the final product. For example, after a drug successfully passes all clinical tests, basic research on that chemical entity has a much lower impact in the scientific community. Moreover, the academic team has also the choice no to enter commercial activities at all, and to perform instead pure basic research, with no level of applicability to the commercial venture, in both periods – see the following description and analysis of the model for further detail. In some sense, we can interpret this option as the performance of an alternative project.

[13] Or, the performance of more applicable basic research can increase the probability of successful completion of the project. Indeed, an alternative way to introduce cumulativeness of knowledge would be to assume an

Costs and benefits from basic research The academic team receives a direct benefit from performing fundamental research, e.g. through publications or peer recognition. Define

$$B^u = \text{Expected (gross) return from fundamental research activities} \tag{3}$$

The performance of fundamental research has a cost of

$$C_s^u = \frac{(\gamma^u)^2}{2\alpha} + \lambda^u \gamma^u, \tag{4}$$

where $\alpha \in (0, K]$, $\lambda^u \in (0, 1)$. The first part of the cost represents the disutility of organizing (applicable) fundamental research; α is a scaling parameter, and the upper bound to its value, as will be clear below, ensures that commercialization costs (plausibly) do not become negative for any amount of fundamental research performed[14]. The second part of the cost is related to the loss in peer recognition from performing 'applicable' fundamental research. For example, multidisciplinary research may not be consistent with how the peer review system works, since the system is highly discipline-based (therefore it would be more difficult to publish one's multidisciplinary work in prestigious journals), and/or multidisciplinarity is difficult to achieve because of the departmentalized organizational structure of universities. Brewer (1999) offers a typology of obstacles to interdisciplinary research. Some of these costs, e.g. the differences in methods and language across disciplines, can be said as referring to the nature itself of interdisciplinary research. Other sources of costs reported by Brewer, however, depends on the institutional rules and incentive systems of the environment in which the research is performed. These costs include the funding rules (and whether they give priority to disciplinary research), and scientists' concerns about their status and careers. Consider also the 'specificity-tacitness' interpretation of the level of applicability: academic scientists are penalized by their peers if they produce fundamental research which is too idiosyncractic and/or is kept tacit and not codified, say, in journal articles. Academicians find it therefore costly to move their basic research agenda out of the 'ivory tower' or the Bohr's quadrant. The negative impact on recognition depends on the parameter λ^u. In fact, one could think of $\lambda^u \gamma^u$ as a (negative) component of the direct benefit that academics receive from basic research. The direct benefit from fundamental research can be expressed as $(B^u - \lambda^u \gamma^u)$, and the cost as $\left[\frac{(\gamma^u)^2}{2\alpha}\right]$.

Notice that, when the team chooses activity c, no scientific benefit is obtained, while choosing applicable basic research may entail a positive net scientific benefit $B^u - \lambda^u \gamma^u$ (here I interpret $\lambda^u \gamma^u$ as a part of the private benefit from performing fundamental research). The team's choice of $\{a_t^u, \gamma_t^u\}$ can therefore be seen as a series of multitask problems: in each period the team has to choose between different activities. There is an implicit constraint in that the team cannot perform both s and c in the same period, and there are tradeoffs involved in

impact of the performance of research on p, the probability of completion and commercialization, rather than on the costs. Results are similar if this alternative modeling strategy is followed.

[14] I take this part of the cost function to be increasing and convex in γ^u: for example, it is increasingly difficult to organize a very heterogeneous team. Or, a too high level of specificity of the produced knowledge would reduce the ability to absorb knowledge from the external world.

this choice: scientific versus commercial rewards, early (but costly) commercialization versus late (but cheaper) commercialization, and choice of the type of pre-commercial research (or balance between the consumption value and the investment value of research). Figure 1 summarizes the choices and payoffs of the team.

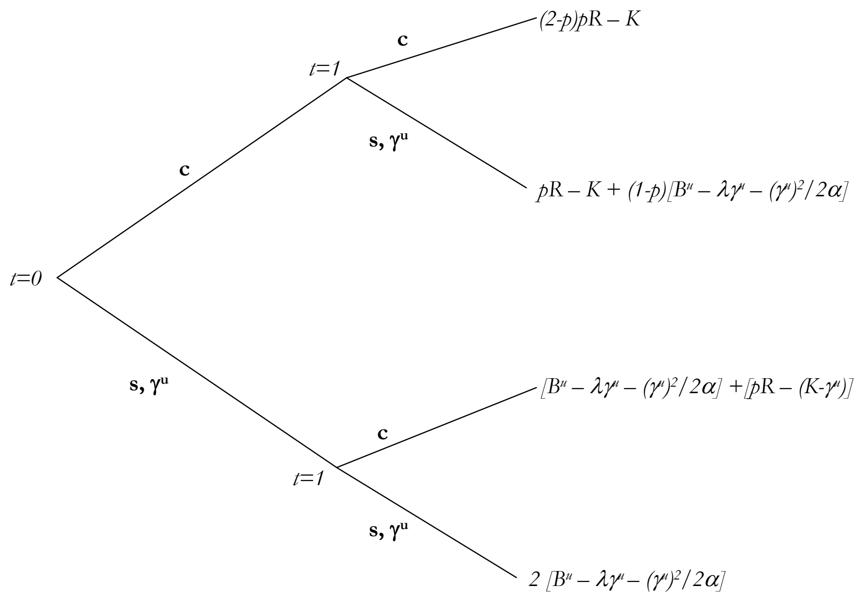

Figure 1: Decision tree for the academic team. The actions are reported in bold types. Ex ante payoffs are reported at the end of each branch.

4.1.2 The company team

> '[...] private firms feel no obligation to advance the frontiers of science as such. [...] they are always asking themselves how they can make the most profitable rate of return on their investment'. (N Rosenberg 1990, p. 169).
>
> 'In academia you probably wouldn't go to lunch with someone in a different department – says Maciewicz, a biochemist – but because the company's success depends on a group effort, you get to interact with people who have a really different skill base'. (Urquhart 2000).

I compare the timing of entry into commercially relevant activities, as well as the returns and the costs of entry for the academic team, to a company research team. This comparison allows me to identify some peculiarities of academic research teams when they have the option of engaging in commercially related activities. The problem for the company lab and the parameter values are the same as above, except for two modifications (see also figure 2):

1. The company team cares only about the completion of the project, which is when potential economic returns occur.

2. The company team does not bear a 'recognition' cost from performing more applicable fundamental research, but only an organizational cost. There is no 'stigma', for example, for a company lab to perform highly interdisciplinary research, or to invest in tacit knowledge.

These two assumptions imply $B^f = \lambda^f = 0$ (the superscript f stands for 'firm')[15]. I call $\{a_t^f, \gamma_t^f\}$ the choice set of the firm team in each period. The parameters B^i and λ^i therefore operationalize the differences among the two institutional environments[16]. Since $\lambda^f = B^f = 0$, I will write λ and B in place of λ^u and B^u, hereinafter, without loss of clarity.

The two teams do not interact, and I analyze the behavior of each team 'in isolation'. In fact I am performing comparative statics along a parameterized family of single-institution models rather than proposing a model of interactions among organizations. In Section 6, I outline some directions for future research that include several forms of interactions.

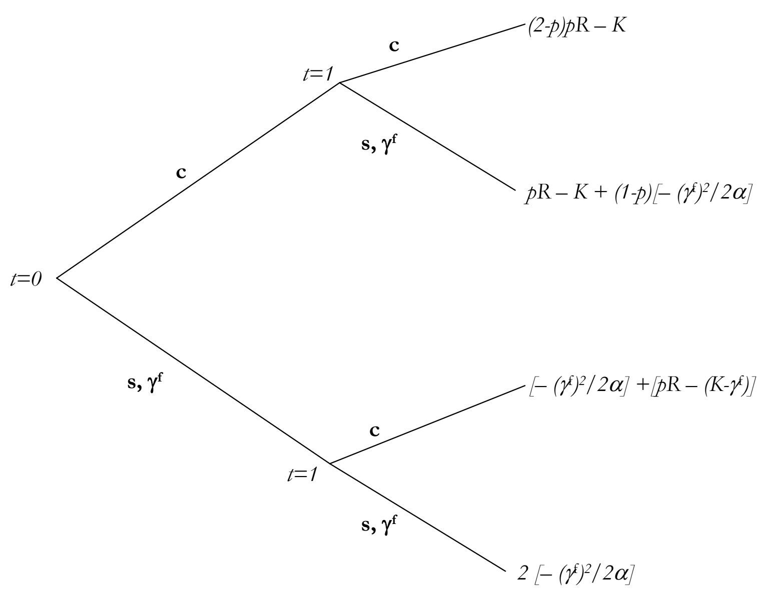

Figure 2: Decision tree for the company team. The actions are reported in bold types. Ex ante payoffs are reported at the end of each branch.

Comment On the one hand, the way I model the differences between an academic and an industrial team is very stark, since I assume that industrial scientists do not receive

[15] A possibly less arbitrary way to capture the lower cost for the company team (for a given γ) is to exclude the linear term $\lambda\gamma$ from the academic team's cost function, and assume that the parameter α takes two different values: α^u for the academic team and α^f for the company team, with $\alpha^u < \alpha^f$. This parameterization conveys qualitatively the same results and intuitions as the 'linear-quadratic' form I use here.

[16] These parameters can vary also *within* an institutional environment. Several studies report the differences between universities or specific departments along dimensions that can be expressed in terms of the parameters B and λ, in particular the attitude toward multidisciplinary research. See for example Louis et al. (1989), Bercovitz et al. (2001), Feldman and Desrochers (2004), Shane (2004), and Jong (2005). The differences within a given institutional environment may also depend on higher-level institutional constraints, e.g. at the national level. Gittelman (2001) analyzes the differences in the organization of academic research between France and the US, and Henrekson and Rosenberg (2000) describe the different incentives in the US and the Swedish academic systems.

any direct benefit from performing fundamental research. More precisely, I am assuming that, in the industrial environment, both research and commercial activities are evaluated by a common set of criteria; in the academic environment, by contrast, research activities, when performed, are subject to 'peer evaluation', while commercial activities are subject to market-based rewards (hence the idea of academia as having *multiple missions*). These differences, however, are to be seen as extreme versions of some largely plausible facts. On the other hand, the differences are minimal, i.e. limited only to the response to scientific incentives. I am assuming that the academic team and the industrial team have the same commercial capabilities, given the same amount and type of research performed, and are equally rewarded when they perform commercial activities. I am therefore confining all of the sources of heterogeneity into the sphere of pre-commercial research. While extreme, this choice is consistent with the institutional literature on science I mentioned above (Merton 1973, Dasgupta and David 1994, and others). The focus of this literature is on the analysis of research activities, and not on commercialization activities. It is at the level of research that differences between academia and business may emerge. In particular, the criteria that govern the evaluation of research in academia do not depend on the commercial value. Notice that this implies that the extra 'recognition costs' for academics from investing in more applicable research (i.e. from choosing some value of $\gamma > 0$) is not due to the type of activity being more 'commercial', but from the fact that the type of research is just not following in full the rules of the scientific community. This observation clarifies also why I do not assume any stigma or extra disutility to emerge from commercialization for academics (except for the foregone private benefit from performing s instead): once commercialization is chosen, the rules of the scientific community do not apply any longer[17]. I capture these differences in a simple way, and also explore the consequences of these differences on an otherwise homogeneous set of activities, those concerning development and commercialization.

4.2 Analysis

The model generates results that I group in two propositions. The first proposition focuses on the decision to enter commercially oriented research. The second proposition considers the timing of commercialization. In the next two subsections, I state the propositions, both in informal and in formal terms (proofs are in Appendix A). Then, in the following section I offer the intuitions behind the results, as well as comments and implications.

4.2.1 Academic reluctance and project selection

One effect of the different institutional rules in business and academia, as modeled here, is that, when deciding whether to move from fundamental to commercial research, industrial

[17] This characterization is also consistent with the characteristics of the modern university, especially in the IS, where the quest for commercial success is more and more considered as part of the academic mission (also because it brings extra funds for further research projects). Moreover, recall that the model can also be applied to the provision of both academic and commercial incentives to *company* scientists. In this case, also, it is plausible to assume that the incentives scheme includes peer evaluation only when research, and not commercialization activities (development, production, etc.) are performed.

and academic teams have different outside options and opportunity costs. As a consequence, they have different incentives to undertake a given commercial opportunity, and enter different types of projects. More specifically, there is a set of projects with positive profitability that the firm would undertake, and the university team would not. The university team is more 'selective' the higher is B, i.e. the consumption value of basic research, and more so if λ, i.e. the parameter affecting the 'recognition costs' from performing applicable fundamental research, is high.

Proposition 1 *The condition for the academic team to enter commercially oriented activities in period 0 or 1 is*

$$Max\left\{[p(2-p)R-K],\ \left[B+pR-K+\frac{\alpha}{2}(1-\lambda)^2\right]\right\} > 2B, \tag{5}$$

and for the firm is

$$Max\left\{[p(2-p)R-K],\ \left[pR-K+\frac{\alpha}{2}\right]\right\} > 0. \tag{6}$$

Condition (5) for the university team is more restrictive than condition (6).

Re-arranging the terms of expressions (5) and (6), we obtain that the company team enters commercialization (at some period) if

$$pR > K - \frac{\alpha}{2} \quad or \tag{7}$$

$$pR > \frac{K}{2-p}; \tag{8}$$

the academic team enters commercialization if

$$pR > \frac{2B+K}{2-p} \quad or \tag{9}$$

$$pR > K - \frac{\alpha(1-\lambda)^2}{2} + B. \tag{10}$$

The ex ante profitability conditions for the academic team to enter commercially relevant activities are stricter than for the company team.

Proof. See Appendix A. ■

4.2.2 Academic slowness and academic rush

The model implies not only that academic and industrial actors tend to undertake different sets of projects, but also that they can move to commercialization in different periods. The first part of the following proposition states an 'expected' result, i.e. that academic teams are slower than industrial teams in moving research to commercialization. In the logic of the model, this means that the academic team undertakes commercially relevant activities after having performed some additional fundamental research, while the company team would commercialize at period 0 with no additional fundamental research. The university team, moreover, invests in applicable fundamental research, thus reducing commercialization costs in date 1. The second part of the proposition considers a less obvious implication of the model: if performing applicable fundamental research is very costly for the academic team, and if the return from commercialization is sufficiently (but not excessively) high, then an academic team will commercialize earlier than an industrial team. The company team finds it optimal to perform some extra-research before moving to commercialization, while the university scientists do not perform any additional research. The industrial team performs pre-commercial research with a high level of applicability. This scenario is more likely to occur when λ, the parameter affecting the recognition costs (or negative benefits) from applicable basic research is large, i.e. close to 1.

Proposition 2 ***a***. *If the parameter values are such that*

$$pR > \frac{\alpha}{2(1-p)}, \tag{11}$$

$$pR > \frac{2B + 2K - \alpha(1-\lambda)^2}{2}, \tag{12}$$

and

$$0 \leq \lambda \leq 1 - \sqrt{\frac{2(1-p)pR - 2B}{\alpha}}, \tag{13}$$

then

$$\{(a_0^u, a_1^u), (\gamma_0^u, \gamma_1^u)\} = \{(s, c), (\alpha(1-\lambda), 0)\}, \tag{14}$$

and

$$\{(a_0^f, a_1^f), (\gamma_0^f, \gamma_1^f)\} = \{(c, c \textit{ if fail at } t = 0), (0, 0)\}. \tag{15}$$

The costs of entry for the company team will be equal to K, and therefore the expected return at period 0 will be $p(2-p)R - K$. The costs of entry for the university team will be equal to $K - \alpha(1-\lambda)$, and the expected commercial return at period 1 will be $pR - K + \alpha(1-\lambda)$.

b. *If the following two conditions hold:*

$$\frac{2B + K}{2 - p} < pR < \frac{\alpha}{2(1-p)}, \tag{16}$$

$$1 - \sqrt{\frac{2(1-p)pR - 2B}{\alpha}} < \lambda \leq 1, \tag{17}$$

then

$$\{(a_0^u, a_1^u), (\gamma_0^u, \gamma_1^u)\} = \{(c, c \textit{ if fail at } t = 0), (0, 0)\} \tag{18}$$

and

$$\{(a_0^f, a_1^f), (\gamma_0^f, \gamma_1^f)\} = \{(s, c), (\alpha, 0)\}. \tag{19}$$

The costs of entry for the academic team will be equal to K, and therefore the return will be $p(2-p)R - K$. The expected return for the company team will be, at period 1, $pR - K + \alpha$.

Proof. See Appendix A. ■

Figures 3 and 4 represent qualitatively the different cases emerging from the two propositions[18].

[18] Figure 4 is drawn under the following additional assumption: $\frac{B}{p(1-p)} < \frac{K}{p} - \frac{\alpha}{2p} < \frac{B+K}{p} - \frac{\alpha}{2p} < \frac{2B+K}{p(2-p)} < \frac{\alpha}{2p(1-p)} < \frac{B+K}{p}$. This implies $K > \alpha > 2B$ (so that $\frac{B}{p(1-p)} < \frac{\alpha}{2p(1-p)}$); $K > \frac{\alpha}{2} + \frac{B}{(1-p)}$ (so that $\frac{K}{p} - \frac{\alpha}{2p} >$

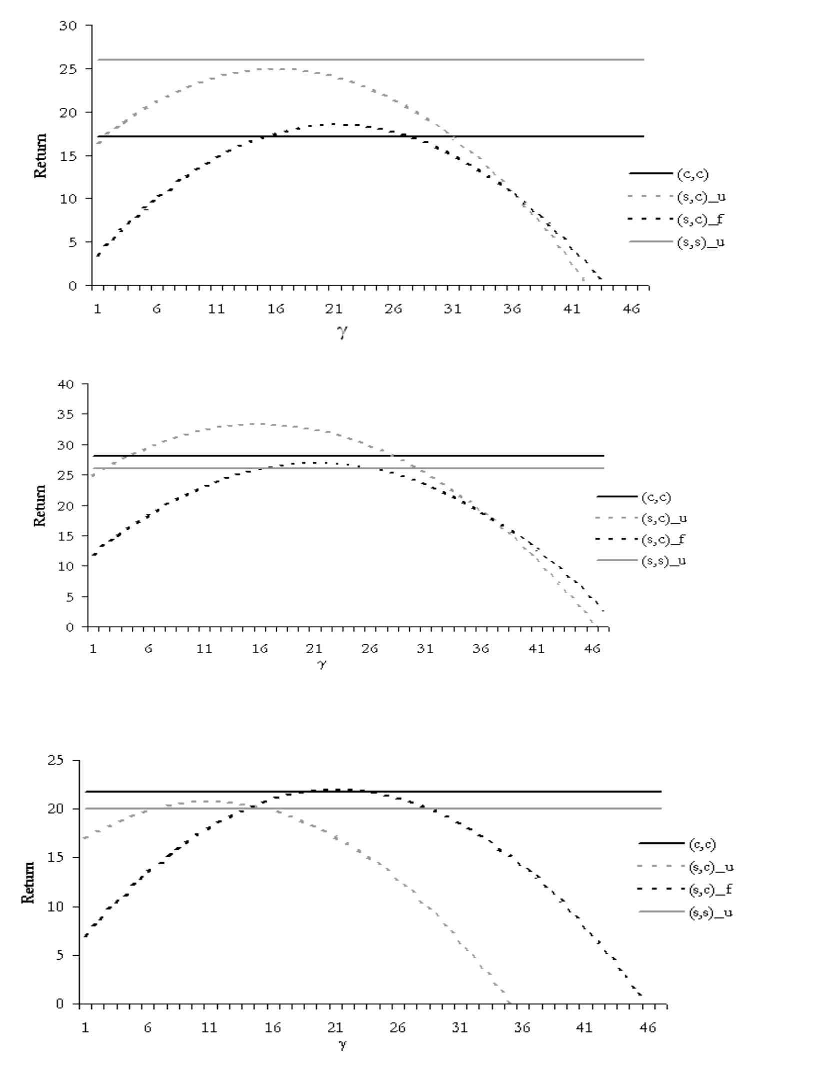

Figure 3: Qualitative representation of the different cases described in Propositions 1 and 2, in the (γ, [expected] return) space. The continuous black line (c,c) gives the expected payoff from trying commercialization in both periods. The dotted gray curve (s,c)_u represents the expected return for the academic team from choosing s in period 0 and c in period 1. The dotted black curve (s,c)_f represents the expected return for the industrial team from choosing s in period and c in period 1. The continuous gray line (s,s)_u gives the return for the academic team from choosing s in both periods. All of the functions are drawn for different levels of γ. The options described by these curves are the only rational ones the teams will choose. Moreover, the academic team chooses $\gamma = 0$ if it plans not to enter commercialization at any period (the (s,s) line is drawn considering this remark). See Appendix A for further details. The top diagram shows a case in which the academic team never enters commercialization, while the company team enters. The industrial team enters at date 1, since there are levels of γ such that it is preferable to wait before trying commercialization, and invest in cost-reducing research. The middle diagram is related to Proposition 2a. The company team chooses c from period 0, while the academic team invests in applicable research and then tries commercialization. The bottom diagram represents the opposite situation, as in Proposition 2b. The academic team 'rushes' to commercialization, while the company team invests in research before moving to commercially related activities.

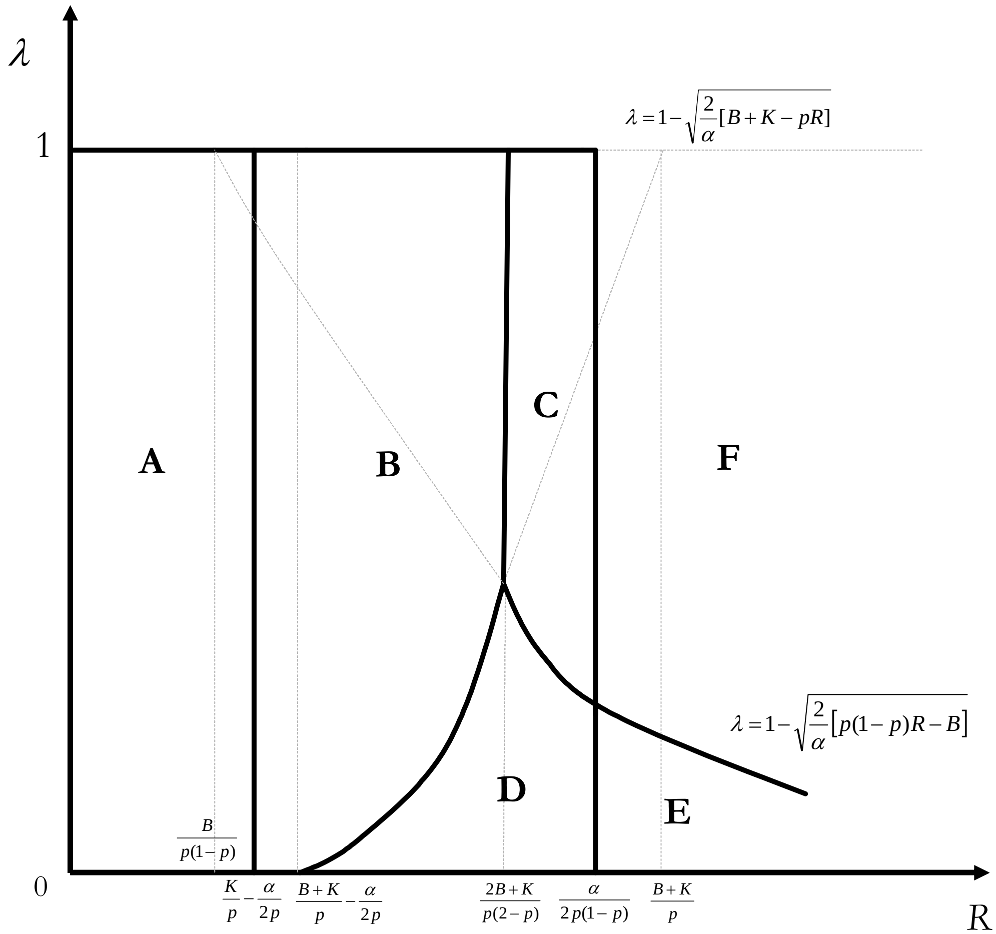

Figure 4: Qualitative representation of the cases in Propositions 1 and 2, in the (R, λ) space (see footnote at page 18 for some extra-assumptions made in drawing the figure). In region A neither the company team nor the university team enters commercialization in any period, as the returns would be negative. Region B represents the parameter space in which the academic team does not enter commercial activities, and undertakes fundamental research (with $\gamma^u = 0$) in both periods 0 and 1. This region is obtained from expressions (9) and (10) in Proposition 1 above (see page 17), expression (12) expression (13). In regions D and E, the academic team performs applicable basic research in period 0, and enters commercialization in $t = 1$. This is obtained from inequalities (12) and (13) at page 18. In region D also the firm perform research in $t = 0$ before moving to commercialization, while in region E the firm has incentives to enter commercialization in period 0 with no additional research. In regions C and F the academic team enters commercialization in period 0 without performing any additional basic research. In region C the academic team enters commercialization earlier than the industrial team would – see inequalities (16) and (17) in Proposition 2-b.

5 Intuitions, implications and relation to the existing literature

Let us now analyze each of the results described above. I consider the conceptual, empirical, managerial and policy contributions of the analysis.

5.1 Academic reluctance (and industrial focus)

Proposition 1 tells us that academic research teams have strong incentives not to enter commercially relevant activities at all. Academic scientists derive a higher benefit from performing fundamental research without direct economic value, since there is also a 'consumption-like' dimension in performing fundamental research. This creates a conflict between the pursuit of economic and scientifically relevant activities, and will delay or exclude the movement toward more applied, commercially-oriented research. Therefore, academic entrepreneurs would rationally forsake commercial opportunities with positive economic and social value. Moreover, academic teams will generally opt for a lower level of applicability of the content of fundamental research, because of the extra cost they derive from it as compared to 'pure' basic research[19]. In figure 4 at page 20, region A corresponds to a case in which the firm does not find it profitable to enter at any period, and neither the academic team moves to commercialization. In region B, however, the firm has incentives to undertake commercially relevant activities, but the academic team does not (see also the top diagram in figure 3 at page 19).

This result formalizes the previously described arguments and evidence which cast doubts on the viability of academic entrepreneurship on a large scale and as a solution to problems of lack of innovativeness. The reluctance I derive comes from the conflicts and tensions those analyses point to.

Also the historical cases mentioned above showed dynamics very similar to the 'reluc-

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$\frac{B}{p(1-p)}$); $K < \frac{\alpha(2-p)}{2(1-p)} + \frac{Bp}{1-p}$ (so that $\frac{B+K}{p} - \frac{\alpha}{2p} < \frac{2B+K}{p(2-p)}$); $K < \frac{\alpha(2-p)}{2(1-p)} - 2B$ (so that $\frac{2B+K}{p(2-p)} < \frac{\alpha}{2p(1-p)}$); and $K > \frac{\alpha}{2(1-p)} - B$ (so that $\frac{\alpha}{2p(1-p)} < \frac{B+K}{p}$). The condition $K < \frac{\alpha(2-p)}{2(1-p)} - 2B$, if respected, also automatically verifies that $K - \frac{\alpha}{2} < \frac{K}{2-p}$, and therefore condition (7) is less restrictive (in terms of R) than condition (8) (see page 17). In other words, if $R < \frac{K}{p} - \frac{\alpha}{2p}$, then a fortiori $R < \frac{K}{p(2-p)}$ and the company team will not enter commercial research at any period. A necessary condition for the previous inequalities to hold is that $max\left\{2B, \frac{4B(1-p)}{p}\right\} < \alpha$, given that we also have to satisfy $K > \alpha$. $\frac{4B(1-p)}{p} < \alpha$ is necessary for $\frac{2B+K}{p(2-p)} < \frac{\alpha}{2p(1-p)}$ (or $K < \frac{\alpha(2-p)}{2(1-p)} - 2B$) and $K > \alpha$ to hold. Values of $K = 7B$, $\alpha = 6.5B$ and $p = .5$ satisfy the condition above. Notice that the condition excludes some scenarios, for example the case in which $\{(a_0^u, a_1^u), (\gamma_0^u, \gamma_1^u)\}$=$\{(s, s), (0, 0)\}$. and $\{(a_0^f, a_1^f), (\gamma_0^f, \gamma_1^f)\}$=$\{(c, c \text{ if fail at } t = 0), (0, 0)\}$.

[19] The case of Varian Associates in the late 1940s, as described by Lenoir (1997), offers again some insights. The development of Nuclear Magnetic Resonance (NMR) instrumentation required the performance of research that was a 'disciplinary hybrid between engineering and physics' (p. 247). However, the Stanford scientists interested in NMR found it hard to conduct interdisciplinary research in their university. Somewhat paradoxically, they felt less constrained in a company environment. Interestingly, such strict disciplinary organization of research at Stanford is confirmed by Jong (2004) in a study of the biochemistry departments in the San Francisco Bay Area in the 1970s and 1980s. Also, Hall et al. (2000) find that in collaborative projects with universities, firms experience difficulties in assimilating fundamental knowledge useful for the completion of the project. This can be due to the fact that university researchers have incentive to generate less applicable knowledge.

tance' result. Consider, as an illustration, the invention of the transistor in the late 1940s. A company research team at Bell Labs, and an academic team at Purdue, where performing very similar research on solid state physics. It can be argued, from the existing accounts, that both groups had the knowledge and the abilities to reach the invention. For example, Bardeen, Brattain and Shockley, who led the project at Bell Labs, shared the Nobel prize in 1956, and Karl Lark-Horovitz, who led solid state research at Purdue, was an authority in solid-state physics in the 1940s. Moreover, the academic scientists at Purdue were also aware of the economic and social impacts of their research, and of the possibility to profit from it (universities could file for patents in the 1940s, and in fact Purdue had already obtained some patents before entering semiconductor research). However, the academic team focused on single-disciplinary research paths with high 'pure' scientific value, but no immediate applicability. Research at Bell Labs, while having undoubtedly high scientific content, was multidisciplinary, and there was more intense communication between scientists with different backgrounds. Research at Bell Labs was also secretive, and could be diffused only several months after patent applications. There also were clear priorities in the direction of the research, and top R&D management had to approve any research program (see for example Braun and Macdonald 1978, and Bray 1997). This gave the sense of a common, practical goal to be achieved (see for example Shockley 1956, Nelson 1962, Braun and Macdonald 1978, Hoddeson 1980, Bray (1982, 1997))[20]. Contrary to an alleged uniqueness and diversity of the research at Bell Labs if compared to other industrial settings, and its alleged similarity to a university environment as claimed by many observers, a careful reading of the available accounts, and the comparison to what was simultaneously (and independently) happening in a 'real' academic laboratory, reveal that the organization and the rules of Bell Labs were not so different from what one would expect from profit-seeking, economically-focused agents, and were different from an academic setting. It is this 'normality', I claim, rather than any kind of diversity and uniqueness, that explains the greater success of Bell Labs, and the anticipation of the discovery of the transistor by as much as a decade (according to Riordan and Hoddeson 1997). Similar considerations apply to the case of the synthesis of human insulin, and of the differences in the objectives and organization of research at Genentech[21] and in the university laboratories engaged in insulin research, at Harvard and at UC - San Francisco (see Hall 1987, Stern 1995, McKelvey 1996).

5.2 Project selection

Proposition 1 also reveals that a selection mechanism is in place: academic researchers tend to choose among projects with higher expected returns (or equivalently, lower costs)[22]. The

[20] I am very grateful to Professor Ralph Bray, who was a doctoral student in the Lark-Horovitz's group at Purdue in the late 1940s, for agreeing to be interviewed.

[21] Some would consider Genentech as a form of academic entrepreneurship. For my purposes, Genentech represents a 'pure' firm as opposed to the university-based teams that were working on very similar research. See Hall (1987) and Stern (1995) for interpretations consistent with mine.

[22] Rewriting (and reinterpreting) condition (10) at page 17 in terms of λ, i.e. the parameter affecting the recognition cost of performing applicable fundamental research, we obtain: $\lambda < 1 - \sqrt{\frac{2}{\alpha}(K + B - pR)}$.

model therefore offers an alternative (or additional) explanation for 'success stories' of the involvement of academics into commercially related activities in such studies as Zucker and Darby (1995), Cockburn and Henderson (1998), Torero et al. (2001), Nerkar and Shane (2003), Shane (2004), Stephan et al. (2004), Kumaramangalam (2005), Rothaermel and Thursby (2005), Toole and Czarnitzki (2005), and Agrawal 2006. The positive impact of the direct involvement of academics into commercially relevant research may be driven by the fact that academics *choose* to participate only in those commercial projects which make it worthwhile for them to forego valuable academic activities, and not necessarily by the superiority of their knowledge and capabilities[23,24]. The selection effect would be stronger for more skilled scientists: the private benefit they give up (e.g. in terms of the expected number and quality of publications they would achieve, if they keep on a pure basic research path) is indeed higher. Some of the previously mentioned empirical studies focus on highly skilled academic scientists. Calderini et al. (2004) find that the professors with highest quality publications are less likely to be listed among the inventors of a patent assigned to a firm. My selection result offers an explanation for this finding[25].

While somewhat intuitive, this result has never been considered before[26]. From a policy perspective, any causal inference and normative implication from empirical tests of the impact of academic entrepreneurs on the viability and success of a commercial activity (and in bringing fundamental research to market successfully) should be taken with caution because of this possible selection mechanism, unless the endogeneity problem is appropriately corrected for. A further potential concern is that an academic team would not undertake a research projects with positive (albeit small) profits and therefore of positive social value

Academic teams tend to enter projects for which the recognition cost from performing applicable fundamental research is not too high. The projects selected by academic and company teams may also differ in this respect (the industrial team's decisions do not depend on λ).

[23] While Lowe and Ziedonis (2005) find that spinoffs from the University of California (UC) are more successful than non-university generated spinoffs, Shane (2002) finds contrasting results for MIT spinoffs. The result discussed in this section could contribute to explain this difference. UC has shown to have a more 'conservative' approach to the involvement of academicians in commercial activities than MIT (see UC presidential Retreat 1997). This can be translated in my model as a high opportunity cost for scientists to move to commercial activities, and therefore to a higher selectivity in the undertaken activities. MIT professors may instead be attracted also by less profitable ventures.

[24] Capability and selection can coexist. Very skilled scientist may have a stronger impact on the profitability of a commercial venture both because they pick more profitable ventures, and because they have higher knowledge that can be applied to the venture. Moreover, if we look at status and academic position rather than ability, it can be argued that scientists with *higher* status would be more less selective in undertaking commercial activities, since they do not need to perform additional research to achieve peer recognition.

[25] Calderini et al. add that a further selection may occur: firms may not find academic prestige as a good proxy for the ability to perform 'applicable' research. This view is consistent with the presence of several types of basic research and of different incentives for academic and industrial teams to perform each type.

[26] A partial exception is Witt and Zellner (2005). Toole and Czarnitzki (2005) acknowledge this potential endogeneity problem in their empirical analysis. So does Agrawal (2006), who also lists a few arguments in support of the causal direction from scientists' involvement to commercial performance. However, none of these papers proposes empirical tests (e.g. through the use of instrumental variables techniques) to solve the problem. Kumaramangalam (2005) studies the impact of collaboration with academic scientists on the quality of biotech articles. He accounts for the fact that the sample of articles he considers may not be a random sample. However, he does not consider that also the subsample of biotech papers with an academician among the authors may be a self-selected sample.

(see Audretsch 2000). On the other hand, the presence of a 'positive-utility' alternative to the commercial path represents a sort of disciplining device for an academic team, which will undertake only commercial opportunities of high quality. Therefore, we would have both 'success stories' of academic entrepreneurs, and 'missed opportunities'. From a managerial standpoint, this result implies that attracting talented academic entrepreneurs may be very costly.

5.3 Academic slowness and the timing-performance tradeoff

The second set of results, in Proposition 2, focus on the timing of entry into commercially oriented research. I first obtain (part a of the proposition) an expected result: the institutional and organizational features of universities make academic researchers slower than company scientists in undertaking research with commercial potential. The argument is similar to what said above regarding the reluctance case. However, in this case the academic team has incentives to undertake commercial activities 'not too late'. In figure 4 at page 20, this case corresponds to regions D and E (in region D, both teams would wait until period 1 before entering). The survey of Franklin et al. (2001) shows that one of the major concerns of Technology Transfer official in universities, about the direct involvement of academic inventors in commercially valuable projects, is that academics tend to focus on the scientific and technical aspects of a project, thus neglecting or delaying commercially-related activities. Rothaermel and Thursby (2005), while finding that incubator firms with an active involvement of academicians have lower rates of failure, also find that these firms take longer to be 'promoted', i.e. to exit from the incubator and become independent companies. These findings are consistent with my result.

The model implies a few other considerations. First, when academics take longer to move to commercialization, they accumulate more knowledge through the performance of additional research, and therefore the performance of commercially oriented activities will be less costly than for a firm entering earlier (see for example Audretsch 2000 for an argument consistent with this). There is, therefore, a trade-off between time effectiveness and cost effectiveness, and firms and universities may position on different points of this tradeoff. Empirical studies that analyze the performance of academic ventures should use multiple performance measures, and should appropriately account for the timing of commercialization as well as for the costs. Second, and in comparison with the reluctance case, notice that not only does the academic team enter commercialization activities at some point, but it also undertakes a different type of basic research. There are incentives to 'sacrifice' some private benefits from fundamental science and perform more applicable research, with greater investment value. However, the level of applicability of basic science will not be as high as what a firm would choose (see below).

Social implications can also be conjectured from this result. If, at a given point in time and for a given amount of knowledge in the system, the performance of some additional basic research has a higher social value than the delay of commercialization, then a university team will have the 'right' incentives to perform it, while a company team cannot commit to strike a compromise between the performance of additional research and the performance of commercially oriented activities.

5.4 Academic rush

The second part of Proposition 2 defines the parameter space where a less intuitive scenario emerges, one in which a university research team is more eager to bring its research to the market than a company would be. The model shows that in certain circumstances academic scientists gain less than company scientists from performing additional fundamental research before moving to commercial research, if the applicable content of fundamental research is very low. Recall that the level of applicability of basic research is endogenously determined, and the more applicable the research, the higher the cost reduction. If the academic reward from the research project (the parameter B in the model) is not very high, and if the loss in recognition from performing applicable fundamental research is substantial (λ is high), then it turns out that the academic team is more eager to move to commercialization. In figure 3 at page 19, this case is shown in the bottom diagram. In figure 4 at page 20, this case corresponds to region C.

Compared to the slowness case, now the consumption incentive and the investment incentive collide. By performing fundamental research before entering commercialization, the team receives a small consumption value from the research; moreover, since the recognition cost is high, the investment in applicability will be small (recall that the level of γ^u, the degree of applicability of pre-commercial science or cost reduction, is negatively correlated to λ, the parameter affecting the recognition costs from applicable basic research – see Proposition 2 at page 18). Moreover, performing additional pre-commercial research delays the achievement of (uncertain) economic returns. Therefore, the academic team would prefer to move to commercially oriented activities right at the outset, giving up the private benefit from basic research.

The absence of consumption motives and recognition issues for a firm eliminates this contrast, and makes the investment in additional research, with no immediate economic value, still optimal. An exclusive orientation to economic profit leads a company to fully appreciate the investment value of fundamental research, while the simultaneous presence of multiple motives inhibits the investment in research by the academic team. The exclusive orientation toward economic profits from the project also leads the industrial team to choose a highly applicable type of fundamental research, more applicable than the type chosen by the academic team when it performs some fundamental research before moving to commercialization. This more applicable pre-commercial research, while of great economic potential (in terms of investment value) and potentially also scientifically novel, does not completely respond, however, to the rewarding rules of the scientific community.

Two issues can be raised regarding this result. First, isn't the academic team behaving like a 'pure firm' since it is not performing any additional research? Recall that I characterize the academic and the industrial teams as responding to different incentives when they perform research activities, e.g. activities with some level of scientific novelty. The reward system in the scientific community is concerned with this kind of activities. Development and commercialization activities are activities for which there are no 'academic' rewards, e.g. rewards in the form of recognition, publications, promotions and the like. This does not mean that universities (or individual scientists) do not care about commercialization, since they can get monetary returns out of it. Commercialization activities, *per se*, do not imply that universities are not behaving as universities, since the differences between the academic and the industrial environment, in my setting, are confined to the research phase. As pointed out above (see in particular the comment at page 15), the peculiarity of the academic environment is the pursuit of *multiple missions*, with different activities, research and commercialization, been rewarded by peer recognition and market-based mechanisms, respectively. In the industrial setting, any activity is subject only to market-based rules. This implies that behavioral differences between the two teams, if any, will be in the amount and type of research. This will have an impact possibly on the timing of commercialization, but not in the way commercialization is performed. Recall, finally, that we look at the performance of *additional* research for a single project. Therefore, it may well be that some 'academic' research has already been performed, and that scientists are performing research for other projects. By commercializing early, an academic team gives itself one more shot to be successful on the market, but also gives up any potential 'private' benefit from the performance of additional research.

A second issue can be stated as follows: isn't the academic team, by rushing to commercialization, getting a lower payoff than the industrial team (see the bottom diagram in figure 3 at page 19)? Indeed, one could argue that the academic team might instead prefer to 'behave like a firm' and not care about the rules of the scientific community. This would guarantee the team a higher payoff by choosing to do some pre-commercial research in the first period, and therefore we would not observe academic rushing. However, this is precisely the case in which we are treating the academic team as just a company team. What we are interested in is instead the analysis of the behavior and performance of a team when it responds to the rules and incentives of the scientific community. This is what characterizes the team (and the entrepreneurial activity it engages in) as *academic*. If we, in fact, do not observe academic entrepreneurship in a certain area, according to the model this may be due not to the fact that an academic team would be (possibly inefficiently) too slow, but it would be (possibly inefficiently) too *eager* to commercialize.

Some managerial implications derive from the rushing result. There are cases in which, if a firm wants to commit to a higher effort in research, partnering with organizations responding to the incentives of the scientific community, or providing 'academic' incentives to its own scientists (e.g. by tying monetary bonuses or promotions to the reputation of the scientists in their community of peers) are not the right ways to go: researchers who respond to academic incentives, as defined above, may be even more eager that their industrial partners to bring their research to the market, potentially at high costs given the state of knowledge.

From a policy perspective, if the aim of promoting academic entrepreneurship is to increase both the scientific and the commercial value of research, then in some cases academicians are not the appropriate agents of such policy. A university team in some cases has no incentives to perform additional explorations before commercialization, while a company team would. Having *academic* researchers involved in commercial research implies that these researchers will be exposed to heterogeneous sets of incentives, and will have multiple missions. It is important to analyze whether and how these missions will reinforce each other, or whether they will collide. A clear (albeit simple) characterization of the academic environment, and on how academic scientists are rewarded by their peers, helps detecting these consistencies and conflicts. For example, I obtain that exposing scientists to strong economic incentives, without modifying the reward structure and the organization of research in academia, may not be sufficient and could actually generate the 'wrong' results. Consider, as an example, a profitable economic opportunity emerging from a project in a very young and still not well defined area of research. Since the area is new, the benefit from peer recognition may be low. Or, the new area requires an organization of research which is not rewarded in the scientific community, e.g. a high level of multidisciplinarity or tacitness. Further scientific explorations could be valuable for society, if they, say, are expected to branch into related results before the original research is 'privatized' through commercial applications[27]. Reforms of reward criteria for academic scientists, the promotion of multidisciplinary research, and incentives for keeping knowledge more tacit and idiosyncratic would be important, for example, to avoid too early commercialization. The benefits of these changes, however, need to be weighed against the potential costs, given the multiplicity of tasks that universities are called to perform. Furthermore, these changes would generate an institutional transformation of universities in organizations very similar to business companies. Given that we already have business companies, the question is what would be the incremental benefit of modifying universities to make them similar to already existing organizations[28].

This rushing result is novel. It represents a warning for the definition of research alliances and policy interventions. This result also matches some empirical evidence on the behavior of academic organizations when they move to commercialization, such as Jensen and Thursby (2001) and Lowe (2002). Jensen and Thursby (2001) and Jensen et al. (2003) propose a different argument for the survey results of Jensen and Thursby (2001), and focus on the principal-agent relationship between scientists and the Technology Transfer Office of the University. Lowe (2002) motivates his findings with an argument based on contractual incompleteness and information asymmetries. My focus is less on contractual and informational issues, and more on the differences among institutional environments and their effect on the production of knowledge and on the performance of commercially relevant research. The issues I point to are complementary to the ones these authors focus on. Kogut and Gittelman (2003), in an analysis of the biotech industry, find that there is a tension between the production of highly rewarded science and commercially viable research, with company

[27] This consideration is similar to Heller and Eisenberg's (1998) discussion of the 'Tragedy of Anticommons' from the introduction of Intellectual Property Rights (and therefore stronger economic incentives) in academic research, and has been considered also by Aghion et al. (2005) in what the authors call 'early privatization'.

[28] David (2005) proposes to create 'bridge institutions' with rules different from both the industrial and the academic environments.

scientists 'polarizing' toward the production of one type of activity or the other. The authors hints at the presence of different institutional logics – the scientific one and the commercial one – in order to explain the observed behaviors and outcomes. My study can be seen as an attempt to formalize the idea of the presence of multiple missions and commitments in the performance of research, and to explore the implications of this. Finally, the result that firms have incentives to do fundamental research is also consistent with the evidence of outstanding research performed in industrial labs through history. Companies have low 'static' incentives to perform basic research, say in a one-period world; however they can have strong incentives to perform fundamental research in a multi-period, dynamic setting.

5.5 Summary of insights and implications

The model offers a variety of results that contribute to explaining the diversity in the existing evidence (historical, case-based, and large sample) on the role and success of academic entrepreneurship. The analysis defines the environmental conditions and project types such that an academic research team would be expected to balance the production of scientifically relevant research and the commercialization of economically valuable results, the cases in which an academic research team would be too slow or too fast in moving to commercialization (as compared to an industrial actor), and the cases in which we should not expect academicians to have the incentives to bring their research to the market. This is the first theoretical study that considers these aspects in the context of the debate on the role of academic entrepreneurship. The findings have managerial as well as policy implications.

As for managerial implications, this study underlines the tensions a firm encounters when trying to collaborate with individuals and organizations belonging to different institutional environments. For example, the involvement of academic scientists may excessively delay the research process (as several practitioners I have interviewed actually lamented), or, on the contrary, there are conditions under which academics are expected to push for early commercialization. Stories of 'greedy' professors and universities, such as those described in Wysocki (2004), represent suggestive evidence for my findings. Finally, any organizational and strategic implications from empirical analyses that show a positive impact of the involvement of academics have to be taken with caution as a strategy suggestion, unless those empirical analyses properly correct for the endogeneity of the scientists' choice of whether to participate in the collaboration.

In terms of policy insights, my analysis addresses several issues related to the role of academic organizations in facilitating the translation of basic research into commercial applications, and in balancing research and entrepreneurial effort. In addition to appropriately considering both the returns and the costs from commercially oriented research, the model studies how the entry and timing decisions affect both costs and return, and, in turn, how the entry and timing decisions depend on the cumulative nature of knowledge and on the institutional differences between organizations. I determine the cases in which the entry and timing decisions of different organization differ, and this informs us about the ability of a given organization to balance research and commercialization. For example, in the two extreme cases of no (or very late) entry and immediate entry of academic teams, there may be no positive impact of relying on academic organizations for the commercialization of the research they produce: profit seeking firms would better fulfill the role of bridging research to market, and have strong incentives to perform their own fundamental research. The analysis also warns against too simple interpretations of the existing studies on the importance of academic involvement into commercialization, as long as these studies do not account for the endogeneity of a scientist's choice about whether and when to be involved into commercially relate activities. The type of commercial ventures academics enter would generally be different from those undertaken by companies, thus rendering any comparison (and consequent policy implications) between innovative and economic outcomes of ventures that involve of academic researchers and ventures that do not is potentially misleading.

6 Extensions and directions for future research

6.1 Model extensions

A first, natural extension of the analysis would be to consider the academic and the company team as interacting among each other, rather than operating individually and separately. The interaction would take place, for example, in the form of knowledge spillovers among the parties. In particular, the level of tacitness of the fundamental research chosen by a research group determines to what extent another group is able to exploit the knowledge produced externally. The presence of knowledge spillovers would modify the incentive of an academic team to enter commercially oriented research. Given the structure of the model, we could expect asymmetric effects of the presence of spillovers on an academic and an industrial team. For the industrial team, on the one hand, knowledge spillovers would generate a typical free-riding response, with a reduction of the fundamental research performed internally. The academic team, on the other hand, has stronger incentives to perform fundamental research, and, in addition, knowledge spillovers from the firm would further reduce the costs of commercially oriented activities, thus making them more appealing than the performance of 'ivory tower' fundamental research (i.e. research with no applicability and pure consumption value). Some openness of research and free flow of knowledge would therefore stimulate academic entrepreneurship.

The academic and the company team can be influenced in their entry and timing decisions also by another form of interaction, namely competition for priority in the discovery of the commercializable results. This possibility is not an abstract one, since this type of competition actually occurs in science based sectors. For example, in the previously mentioned case of the synthesis of human insulin, there was some degree of competition between Genentech and the two academic teams (at Harvard and at the UC San Francisco) engaged in insulin research (see Hall 1987). Such competition was even more evident in the research on the human genome mapping (see Davies 2001), with Celera Genomics, a private firm, on the one hand and a public consortium (including the NIH and Whitehead Institute in the US, and the Wellcome Foundation in the UK) on the other. Several other stories of industry-university competition the bio-pharmaceutical and bio-agricultural sectors are described in Werth (1995) and in Evans (2004). Again, given the basic structure of the model developed in this paper, we might expect asymmetric effects of the presence of competition. Both the university and the industrial team will have incentive to preempt the rival and anticipate entry into commercialization activities. However, since the academic team has the positive utility option to keep performing basic research with no commercial applications, the reduction in the expected returns from commercialization (because of competition and, let us assume, the presence of a single prize) would make the 'ivory tower' option more appealing. This incentive would contrast the tendency to anticipate entry. Interestingly, while in the human insulin case this competition did not seem to have changed the behavior of the parties, with the academic teams still preferring a longer, scientifically more relevant and commercially less applicable path of research, in the human genome case the entry of Celera into the 'race' caused the public consortium to change their path of research and to opt for shorter, less scientifically relevant methods.

Both types of interactions between industry and academia in the performance of commercially relevant research projects (through knowledge flows and through direct competition) seem also to characterize emergent sectors like Nanotechnology. In future work, I plan to extend my analysis in the directions I just described, and I hope to be able to capture the foundations of the different behaviors observed in actual cases.

6.2 Testing the model

An even more important extension would be to move the analysis from theoretical to empirical investigation, by testing the results of the model. A first direction of empirical analysis would extend the references to the cases of 'parallel' research by industrial and academic laboratories mentioned before, toward more detailed and informed historical case studies. In addition, one could think of detailed, case-based comparisons of other contexts, such as company-based and university-based business incubators (think of Xerox's PARC, for example, as opposed to university-based incubators, possibly in that same area, e.g. at Stanford). These case studies would be interesting because we could see how university-based and company-based labs behave when faced with similar research projects, with economic potential. This comparison helps identifying the specific role (if any) of academic scientists in the commercialization of research. It would be interesting to explore whether and how the organization of research differs in these settings, and whether different amounts (and different types) of fundamental research are performed before a project is moved to the development and commercialization phases. Case studies, more generally, will be of particular relevance since my study is based on comparisons between academic and industrial research, and it is difficult to gather a large number of observations on such pairs. Moreover, while very simple, the model parameterizes issues that are difficult to measure for a large sample and without detailed knowledge of specific cases. Detailed case studies, finally, also offer insights for further elaborations to make the model closer to reality, such as the inclusion of several forms of interactions as mentioned before.

A second direction of empirical research would be an econometric assessment of the existence of the selection effect regarding the involvement of academics in commercial ventures, as described above. In absence of any consideration about the multiple institutional affiliations of academic entrepreneurs, one would expect, as several studies find, that the involvement of academic scientists in commercially oriented research *causes* this research to be more profitable. However, my theoretical analysis shows that academic scientists will tend to enter commercial projects with higher expected returns than industrial scientists, since the alternative option, i.e. performing fundamental research, is more valuable for academic scientists than for company researchers. In order to assess this selection effect empirically, a first step is to define more narrowly the type of academic involvement into business we are interested in, so as to collect consistent and comparable data. One case to focus on would be the participation of individual academicians in joint projects with commercial entities, while keeping their academic position, similar to such studies as Zucker and Darby (1995), Cockburn and Henderson (1998) and Calderini et al. (2004). Another case would be the study of the impact of the presence of an academic professor in the founding team of a firm, similarly to Nerkar and Shane (2003) and Lowe and Ziedonis (2005). As a starting point, one might re-run some of the regressions performed in these papers with the appropriate endogeneity and self-selection corrections, such as Heckman 2-stage and instrumental variable procedures. A plausible instrumental variable for the selection equation is represented by changes in a university's guidelines regarding conflict of interests and of commitment for professors. These guidelines vary across universities, and many universities have modified their guidelines over time. From the point of view of the *single* scientist, this change is exogenous, and would influence the propensity to participate in commercially oriented research regardless of the expected return or the cost from a particular venture. Another instrumental variable is given by sudden changes (e.g. cuts) in the funding coming from a university to some specific research groups. The analysis of the determinants of a scientist's choice of whether to join a commercial venture is of interest in itself in order to test the presence of tradeoffs to be solved, given the multiple institutional affiliations and the different rules in the different institutional environments. In addition to econometric tests, more fine-grained, case based evidence from interviews and qualitative research, e.g. along the lines of Murray (2004), would help detecting the presence of self-selection and reverse causation issues. For example, it would be interesting to collect information about the determinants of the choice of an academic scientists to join (in various forms) a commercial venture. If academicians are driven also by an evaluation of the expected economic returns from the venture, then the concerns about endogeneity, highlighted in my model, would find some foundations. More generally, evidence showing the existence of a self-selection effect would confirm the importance of considering the multiple affiliations of academic entrepreneurs, and of considering the role of incentives in addition to the role of the skills and knowledge of academic scientists.

Finally, we could define empirical analyses to study the other major result of the model, the one regarding slowness and rushing by academic and industrial research team. The model identifies some key parameters that drive firms and universities to choose different transition times for a given project, given the assumptions on the different missions and governance modes. 'Ideal' data to be collected would concern a large number of industrial and academic research laboratories, and would give information about the timing of transition to development and commercialization phases. Similarly, data on business incubators offer a good empirical setting. We could assess, for example, whether and when university-led incubator firms tend to move to commercialization slower than commercial firms do, or whether and when they move faster, thus counteracting the common wisdom about the slowness and reluctance of academics toward applied work regardless of the environmental, institutional and organizational features. It would also be interesting to see if higher profitability coincides with slow completion. The recent study of Rothaermel and Thursby (2005) on the performance of incubator firms with and without connections to academicians is a reference setting. The authors, as mentioned above, find that incubator firms who actively involve academicians take longer to exit the incubator. The authors attribute this result to the fact that scientists get involved in early phase projects. However, the study does not seem to control for the phase of the project. Such control would be important in order to disentangle the impact of the phase of the project from the behavior of the academic scientists: we could observe whether academic scientists tend to be slower, keeping the stage of the project constant. Hall et al. (2000) had previously found a similar result regarding projects performed jointly by companies and universities: the commercialization seems to be delayed, as compared to projects developed with no collaborations form universities. The authors, again, conjecture that this can be due to the different phases of projects involving universities, and also in this case it would be interesting to control for the phase in order to isolate the impact (if any) of the different institutional environment on the time to commercialization. Also, one could collect information about research agreements between companies and universities, and analyze which phases of a given project are done in the university and which phases are done by the firm directly. The common wisdom would predict that early phases, or those with a high content of 'basicness', will be performed by the university scientists. However, an implication of my model is that, in certain circumstances, it is the firm to have better incentives to perform certain types of basic research, while the university may prefer to perform different types or to move too early to commercialization. Therefore, in some situations we would assist to less 'conventional' divisions of labor. Major challenges for these analyses would be to find appropriate proxies for some of the parameters of the model, such as the private benefit from basic research for academic scientists, the degree of uncertainty, and the different types of basic research; and to gather information about hard-to-observe events, like to movement of a project from fundamental research to more applied and commercially relevant investigations. Variations across sectors and scientific disciplines, variations in the maturity of disciplines, as well as structural and organizational differences between different universities in a given countries, or between academic organizations in different countries, could be explored, as a first cut, as the factors that would drive the decisions on the amount of fundamental research to perform and consequently on the timing of entry into commercialization.

7 Summary and conclusion

What are the peculiarities of academic entrepreneurship? How are the behavior and performance of academic entrepreneurs different from that of other entrepreneurs? Can the direct involvement of academic researchers into the commercialization of research resolve some difficulties of the research commercialization process? Despite the vast attention toward these questions in the scholarly and policy debate over the past 30 years, still very little is known about whether academic entrepreneurship is different from private-firm entrepreneurship. Clarifying this issue is of key importance in order to identify the specific role of universities in the commercialization of research.

In this paper, I have analyzed the choice of academic scientists to undertake commercially relevant activities. I built an economic model of the decision to enter and the timing of entry into commercially oriented activities by an academic research team, and of the returns and costs associated with these activities. In order to identify the peculiarities of academic entrepreneurship, I compared the behavior of the academic team with that of an industrial research team faced with the same choice set. In the model, academic and non-academic teams differ in the objectives they pursue and in the organizational rules they follow. Knowledge is assumed to be cumulative. The cost-reducing impact of fundamental research depends on the type of research a team chooses to perform, and each type is more or less applicable to commercially oriented research. The choice of the type of fundamental research, in turn, depends on the rules and incentives which agents in different institutional environment respond to. The model therefore includes considerations on the nature of knowledge, allows for the presence of several types of scientifically relevant research, and accounts for the institutional differences between the actors and organizations performing research. The inclusion of these differences allows the identification of some peculiarities of academic entrepreneurs, as opposed to other entrepreneurs. This is the first theoretical framework, in the Economics and Management literature on Science and Innovation, that includes all of these aspects.

I derived that there are situations in which academic organizations have incentives to profitably enter commercially relevant activities, after having performed fundamental research that reduces the costs of transition to commercialization, while business companies are not be able to 'wait' for costs to reduce. In other situations the role of academic organizations and scientists in bringing research to market is more controversial. For example, academics find it too costly to 'abandon' (even if partially) the research activities that generate peer recognition in the scientific community. They may also find the type of fundamental research that is relevant for commercial application, e.g. multidisciplinary research or research with a high content of tacit knowledge, not consistent with the rewarding criteria in the community of peers. Academic scientists would therefore choose to invest little in this kind of research. This choice can generate two opposite outcomes: either academic research teams give up commercialization altogether, or they move very fast to commercialization, at potentially high transition costs and with too low production of fundamental knowledge. The timing of entry, indeed, determines also the costs, and therefore the commercial profitability of the research effort: the later the entry, the lower the costs of transition from fundamental to commercial research. A tradeoff between timing and cost effectiveness is therefore present, and different organizations solve it differently. Moreover, academic researchers will tend to forsake commercial projects with positive but small commercial value, and will pursue the purely scientific alternative. By contrast, company teams would be willing to undertake also these 'marginal' projects with economic and potentially social value. Therefore, a self-selection mechanism is present, and the observed success of academic entrepreneurs may therefore derive from the fact that, on average, university researchers move to commercialization only if the prospects are very good.

The analysis helps reconciling the contrasting evidence on the outcomes of the commercialization of academic research, and the arguments in favor and against the academic involvement into commercial activities. Based on the model, I also identified and discussed a series of empirical settings where the predictions of my findings can be tested, and highlight the challenges in performing the tests.

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A Proof of Propositions 1 and 2

I offer a combined proof for both Propositions 1 and 2. Consider the following remarks:

1. The academic team invests in 'applicable' fundamental research (i.e. in γ^u) in period 0 only if it plans to enter commercially relevant activities in period 1. The company team always invests in γ^f in period 0, if it plans to enter commercialization in period 1.

2. Neither the academic team nor the industrial team invests in γ in the second and last period, since there is no benefit from doing this, while there are costs. The university team does not invest in γ^u in period 0 either, if it plans to perform fundamental research in both periods (something that the firm will never do).

3. When the teams invest in applicable fundamental research, they choose

$$\gamma^u = \alpha(1-\lambda) \tag{20}$$

$$\gamma^f = \alpha. \tag{21}$$

Therefore, $\gamma^u < \gamma^f$. These values are obtained by maximizing, with respect to γ,

$$B - \lambda\gamma - \frac{(\gamma)^2}{2\alpha} + [pR - (K-\gamma)] \;\; s.t.\; \gamma \geq 0; \tag{22}$$

$$-\frac{(\gamma)^2}{2\alpha} + [pR - (K-\gamma)] \;\; s.t.\; \gamma \geq 0. \tag{23}$$

i.e. the ex ante expected returns for the academic and the industrial team from performing fundamental science in period 0, and entering commercialization in period 1.

4. If the academic or the company team enters commercialization in period 0, and they are not successful, they will both choose $a_1^i = c$. This choice is obvious for the firm. As for the university, the choice is between $a_1^u = c$ and $\{a_1^u = s, \gamma^u = 0\}$ (as for the choice of γ^u in period 0, see point 3 above). Now, the academic team chooses $a_1^u = s$ only if $B > pR$ (at this point the entry cost is sunk). If this is the case, then the team would have chosen s also in the first period, because, a fortiori, $B > pR - K$. Therefore, having chosen to go commercial in the first period implies that the parameter values are such that it is optimal to go commercial also in the second period.

5. No party stays idle in period 0 if it plans not to stay idle also in period 1. The company team would retard the payoffs by one period without enjoying reduction in entry costs. The academic team would also forsake the net benefit B. In fact, the academic team never stays idle, since it can always guarantee itself a benefit of $B > 0$ in each period. If $pR > K - \frac{\alpha}{2}$, the firm does not stay idle in the second period either.

Given these observations, the decision trees for the academic and company teams reduce to what reported in Figure 5.

Consider conditions (5) and (6) in Proposition 1 (page 17). If the academic team moves to commercialization, it means that either $p(2-p)R - K > 2B$ or $B + pR - K + \frac{\alpha}{2}(1-\lambda)^2 > 2B$ (or both). If $p(2-p)R - K > 2B$, then a fortiori $p(2-p)R - K > 0$, and also a company team would find it profitable to enter the project. If $B + pR - K + \frac{\alpha}{2}(1-\lambda)^2 > 2B$, then $pR - K + \frac{\alpha}{2}(1-\lambda)^2 > B > 0$. Now, since $\lambda \in (0,1)$, also $pR - K + \frac{\alpha}{2} > 0$. Any project that the academic team would enter (e.g.

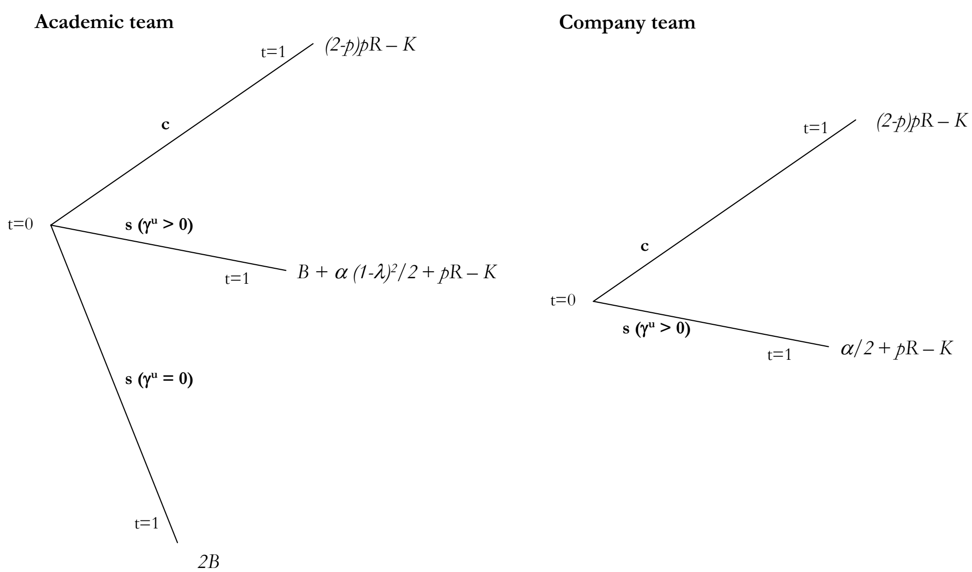

Figure 5: 'Relevant' decision trees for the university and the company research team.

would move to commercialization) would also be entered by the company team, while the opposite is not necessarily true.

As for Proposition 2-a, consider the problem of the academic team. Entering commercialization in period 1 is optimal if

$$B - \lambda\gamma^u - \frac{(\gamma^u)^2}{2\alpha} + [pR - (K - \gamma_u)] > 2B \tag{24}$$

and

$$B - \lambda\gamma^u - \frac{(\gamma^u)^2}{2\alpha} + [pR - (K - \gamma^u)] > pR + p(1-p)R = p(2-p)R - K \tag{25}$$

Similarly, for the firm, optimal entry into commercially oriented activities at period 1 requires

$$-\frac{(\gamma^f)^2}{2\alpha} + [pR - (K - \gamma^f)] < pR + p(1-p)R = p(2-p)R - K \tag{26}$$

Given the optimal determination of γ^u and γ^f from (20) and (21), we get the conditions (11), (12) and (13) – see page 18. By a similar procedure we obtain the conditions in Proposition 2-b.

B An infinite-time version of the model

(In Progress) In this appendix, I begin to build an infinite (discrete) time extension of the model that nests the two-period basic framework described in the paper. Some clarifications and modifications are necessary to adapt the model to the infinite period case. Consider first, as before, the academic team. In each period $t = 0, 1, 2, ...$, the team chooses $\{a_t^u, \gamma_t^u\}$, where $a_t^u \in \{s, c, \emptyset\}$ and γ_t^u, as before, is the level of applicability of fundamental research. Once the team enters commercially relevant activities (choice of c), then there is a probability p, in each period to receive an amount R, and occurrences are independent across periods. The investment in γ_t^u is separate in each period, and the impact on the reduction of commercialization costs is additive. So for example, if in time t the team invests an amount γ^*, and it enters commercialization in period $z > t$, the cost reduction in z will be equal to γ^*. Recall that the cost of commercially relevant research is paid only once, the first time the team tries commercialization. There is discounting across periods; the discount factor is $\delta \in (0, 1)$. We derive the following

Proposition 3 *Define*

$$\Pi_0 = pR + \delta(1-p)pR + \delta^2(1-p)^2 pR + \cdots = \frac{pR}{1-\delta(1-p)} - K. \tag{27}$$

$$SC^u(\tau) = \frac{1-\delta^\tau}{1-\delta}B + \frac{\alpha\lambda^2}{2}\frac{1-\delta^\tau}{1-\delta} - \tau\delta^\tau\alpha\lambda + \frac{\alpha\delta^\tau}{2}\frac{\left(\delta-\delta^{\tau+1}\right)}{(1-\delta)} + \delta^\tau\Pi_0 \tag{28}$$

and

$$\begin{aligned} NND^u(\tau,t) =& \frac{1-\delta^{\tau-t}}{1-\delta}B + \frac{\alpha\lambda^2}{2}\frac{1-\delta^{\tau-t}}{1-\delta} + \alpha\lambda\left(t-\tau\delta^{\tau-t}\right) \\ &+ \frac{\alpha\delta^{\tau-t}}{2}\frac{\left(2\delta^{t+1}+\delta^{\tau+1-t}-2\delta^{\tau+1}-\delta\right)}{(1-\delta)} - (1-\delta^{\tau-t})\Pi_0 \\ &\forall\, t = 1, 2, ..., \tau-1. \end{aligned} \tag{29}$$

i. *If* $\exists\, \tau^u \in (0, \frac{\ln\lambda}{\ln\delta})$ *such that*

$$\tau^u = \arg\max_{\{\tau\}} SC^u(\tau) \ s.t.\ 0 < \tau < \frac{\ln\lambda}{\ln\delta}, \tag{30}$$

$$SC^u(\tau^u) > Max\left\{\frac{B}{1-\delta}, \Pi_0\right\}, \tag{31}$$

and

$$NND^u(\tau^u) > 0\ \forall\, t = 1, 2, ..., \tau^u - 1, \tag{32}$$

then the academic team performs fundamental research for τ^u periods, from period 0 to period $\tau^u - 1$, enters commercially relevant activities in period τ^u, i.e. $a^u_{\tau^u} = c$, and keeps trying until success. In each period $t = 0, 1, ..., \tau^u - 1$, the team invests an amount $\gamma_t^u = \alpha(\delta^{\tau^u - t} - \lambda)$ in 'applicable' basic research: $\{a_t^u = s,\ \gamma_t^u = \alpha(\delta^{\tau^u-t} - \lambda)\}\ \forall\, t = 1, 2, ..., \tau^u - 1$; $a_t^u = c$ at $\forall\, t = \tau^u$ and in any further period, until success.

ii. *If* $\Pi_0 > Max\left\{\frac{B}{1-\delta}, SC^u(\tau^u)\right\}$, *then the team undertakes commercially relevant in the first period $t = 0$ and tries until success: $a_t^u = c$ (until success) $\forall t = 0, 1, ...$*

iii. *If* $\frac{B}{1-\delta} > Max\left\{\Pi_0, SC^u(\tau^u)\right\}$, *then the team never undertakes commercially relevant activities: $a_t^u = s\ \forall t = 0, 1, ...$*

Proof. I prove the proposition in three points.

1. The options reported in the previous proposition – performing s in each period with no investment in applicability, entering commercialization in the first period and trying c until success, and performing applicable research in the first x periods before entering commercialization – are the only rational ones. The reasoning is similar to the one offered for the proof of propositions 1 and 2 in Appendix A, and is expressed in the following remarks:

1-a. Once the team chooses c in some period z, there are no incentives to switch to any other activities thereafter. Conditional on having entered in a given period z and having failed to complete, there is no reason to invest in applicable research afterwards since the one-shot commercialization cost has already been paid, and further expenses in γ_t^u will not translate in cost reduction. Moreover, choosing c in a period z implies that the expected return from commercial research ($pR-(K - cost\ savings)$) is greater than the return from choosing pure basic research (i.e. $\{a_z^u = s,\ \gamma_z^u = 0\}$). Consider period $z + 1$. Suppose that, instead of trying c again, the team makes a one-time deviation to $\{a_{z+1}^u = s,\ \gamma_{z+1}^u = 0\}$, and gains B. From period $z + 2$, the team is back to the 'c path'. This deviation is profitable if $B + \delta pR + \delta^2(1 - p)pR + \cdots = B+\frac{\delta pR}{1-\delta(1-p)}>\frac{pR}{1-\delta(1-p)}$ or, rearranging, if $\frac{B}{1-\delta}>\frac{pR}{1-\delta(1-p)}$. If this is the case, then a fortiori $\frac{B}{1-\delta}>\frac{pR}{1-\delta(1-p)}-(K-\ cost\ savings)$, so never entering into commercialization dominates entry. This contradicts the assumption of entry into commercialization at a finite date z.

1-b. A path in which the team chooses c at some finite period, and has chosen inapplicable basic research in at least one previous period (i.e. $\{a_t^u = s,\ \gamma_t^u = 0\}$) is not an equilibrium path. Suppose that, in some period t, the team finds it optimal to choose $\{a_t^u = s,\ \gamma_t^u = 0\}$, and gets a payoff of B. Take the path (or plan) after t (i.e. from $t + 1$ to entry into commercialization) as given, and as yielding an expected sum of discounted payoffs of A_{t+1}. Now, at t, if the team chooses $\{a_t^u = s,\ \gamma_t^u = 0\}$, this means that $B + \delta A_{t+1} > A_{t+1}$: the team is better off retarding the payoff A from the established policy by one period, and getting B in the current period. A and B are time independent: choosing $\{a_t^u = s,\ \gamma_t^u = 0\}$ 'today' does not change the number of periods in which the team will perform applicable research from tomorrow on before moving to action c, and therefore retards entry into commercialization by one period. Hence, at each subsequent period, the team faces the choice between $B + \delta A$ on the one hand and A on the other hand. If $B + \delta A_{t+1} > A_{t+1}$ (or equivalently $\frac{B}{1-\delta} > A_{t+1}$), then in each period the team is better off doing inapplicable research in any subsequent stage, rather than undertaking the path that leads to commercialization at some point. This contradicts the assumption that the team would choose c at some finite time[29].

1-c. The team chooses $\gamma_t^u > 0$, at a given period t, only if the team chooses $a_z^u = c$ at some finite date $z > t$. If the team never chooses c, obviously it would be better off by performing $a_t^u = s$ with $\gamma_t^u = 0$ at any period t, since $\gamma_t^u > 0$ entails a cost and the benefit is enjoyed only if the team moves to commercialization at some finite time.

[29] Note that I am implicitly assuming that the path that leads to entry in a finite period includes some periods of applicable research (in fact, I just proved that all of the periods preceding entry will be spent in applicable basic research). Clearly, performing basic research with $\gamma = 0$ and then moving to commercialization is not optimal: if no applicable research is being performed, in each period the alternative is between getting $pR - K$ and getting B, independent of time. So if one is greater than the other, it is so in any period.

2. Consider the choice of the investment levels $\{\gamma_t^u\}$, $t = 0, 1, ..., \tau^u - 1$, taking τ^u, i.e. time in which c is first chosen, as given. Consider the first period $t = 0$ (see remarks 1-a, 1-b and 1-c above). The payoff function for the academic team, at period $t = 0$, can be expressed as[30]

$$SC^u(\tau^u) = B - \lambda^u \gamma_0^u - \frac{(\gamma_0^u)^2}{2\alpha} + \delta \left(B - \lambda^u \gamma_1^u - \frac{(\gamma_1^u)^2}{2\alpha} \right) + \cdots \\ + \delta^{\tau^u - 1} \left(B - \lambda^u \gamma_{\tau^u - 1}^u - \frac{(\gamma_{\tau^u - 1}^u)^2}{2\alpha} \right) + \delta^{\tau^u} \left(\Pi_0 + \sum_{t=0}^{\tau^u - 1} \gamma_t^u \right) \tag{33}$$

This means that, when the team has to choose the level of investment γ_0^u, it expects this investment to generate a cost reduction equal to γ_0^u in τ^u+1 periods from the present period. Therefore, while the cost $\lambda^u \gamma_0^u + \frac{(\gamma_0^u)^2}{2\alpha}$ is borne in the present period, the benefit is discounted by a factor δ^{τ^u}. When the team has to choose the level of investment γ_1^u, the cost $\lambda^u \gamma_1^u + \frac{(\gamma_1^u)^2}{2\alpha}$ is borne in the current period, while the benefit is discounted by a factor $\delta^{\tau^u - 1}$. And so on. Therefore, maximizing the present-valued intertemporal payoff in each period t with respect to γ_t^u yields a sequence $\{\gamma_t^u\} = \{\alpha(\delta^{\tau^u - t} - \lambda)\}$, $t = 0, 1, ..., \tau^u - 1$. Notice that $\gamma_t^u > 0$ if and only if $\delta^{\tau^u - t} - \lambda > 0$ or, equivalently, $t > \tau^u - \frac{\ln \lambda}{\ln \delta}$. From remarks 1-b and 1-c above, the team will perform at most $\frac{\ln \lambda}{\ln \delta}$ periods of applicable research, and, if it decides to do applicable research, it will start from $t = 0$.

3. Now, take the sequence $\{\gamma_t^u\} = \{\alpha(\delta^{\tau - t} - \lambda)\}$, $t = 0, 1, ..., \tau - 1$ as a function of τ, and consider the choice of the optimal τ, which we call τ^u. In point 2 of the proof, we took τ^u as given and found the optimal sequence $\{\gamma_t^u\}$ (given also remarks 1-a, 1-b, and 1-c). In this point 3, we instead consider the sequence $\{\gamma_t^u\}$ for any value of τ (the time of entry into activity c), and then find the optimal $\tau = \tau^u$. The team is choosing both $\{\gamma_t^u\}$ and τ^u, and the two choices have to be consistent. Substituting $\{\gamma_t^u\}$ into (33), we obtain

$$SC^u(\tau) = \frac{1 - \delta^\tau}{1 - \delta} B - \alpha\lambda \sum_{t=0}^{\tau - 1} \delta^t (\delta^{\tau - t} - \lambda) - \frac{\alpha}{2} \sum_{t=0}^{\tau - 1} \delta^t (\delta^{\tau - t} - \lambda)^2 + \delta^\tau \left[\Pi_0 + \alpha \sum_{t=0}^{\tau - 1} (\delta^{\tau - t} - \lambda) \right], \tag{34}$$

or equivalently

$$SC^u(\tau) = \frac{1 - \delta^\tau}{1 - \delta} B + \frac{\alpha\lambda^2}{2} \frac{1 - \delta^\tau}{1 - \delta} - \tau\delta^\tau \alpha\lambda + \frac{\alpha\delta^\tau}{2} \frac{\left(\delta - \delta^{\tau + 1}\right)}{(1 - \delta)} + \delta^\tau \Pi_0. \tag{35}$$

Consider $\tau^u = \arg\max_{\{\tau\}} SC^u(\tau)$ s.t. $0 < \tau < \frac{\ln \lambda}{\ln \delta}$. If τ^u maximizes (35) with respect to τ under the constraint that $0 < \tau < \frac{\ln \lambda}{\ln \delta}$, and condition (32) is satisfied (see page 42), then it is optimal to choose $\{a_t^u = s,\ \gamma_t^u = \alpha(\delta^{\tau^u - t} - \lambda)\}\ \forall\, t = 1, 2, ..., \tau^u - 1$, and $a_t^u = c$ at $t = \tau^u$ and in any further period, until success.

Condition (32) at page 42 ensures that, in each period before τ^u, entering commercialization (with the cost reduction accumulated up to that point) is not profitable if compared to staying on

[30] Assume that K is always greater than the sum of cost-reducing investments, in order to ensure that commercialization costs be non-negative, no matter how much (applicable) fundamental research is performed.

the path that implies investments in γ up to τ^u-1, and first attempt to commercialize at τ^u, given the path $\{\gamma_t^u\}=\{\alpha(\delta^{\tau-t}-\lambda)\}$, $t=0,1,...,\tau-1$. Suppose, for example, that $\tau^u>1$. Consider the choices available to the team at period 1, and recall we keep the sequence $\{\gamma_t^u\}$ constant. The team can choose between staying on the 'equilibrium path', i.e. investing a sequence $\{\gamma_t^u\}$ up to period τ^u-1, or entering commercialization $a_t^u=c$ in period 1. Notice that in period 1 the team has already sunk the cost of investing in γ_0^u, and expects to gain $\Pi_0+\alpha(\delta^{\tau^u}-\lambda)$ from 'deviating'. If instead the team stays on the path, the expected return is

$$ND^u(\tau^u,t)\,|_{t=1}=\frac{1-\delta^{\tau^u-1}}{1-\delta}B-\alpha\lambda\sum_{i=0}^{\tau^u-2}\delta^i(\delta^{\tau^u-1-i}-\lambda)$$
$$-\frac{\alpha}{2}\sum_{i=0}^{\tau^u-2}\delta^i(\delta^{\tau^u-1-i}-\lambda)^2+\delta^{\tau^u-1}\left[\Pi_0+\alpha\sum_{i=0}^{\tau^u-1}(\delta^{\tau^u-i}-\lambda)\right] \tag{36}$$

or equivalently

$$ND^u(\tau^u,t)\,|_{t=1}=\frac{1-\delta^{\tau^u-1}}{1-\delta}B+\frac{\alpha\lambda^2}{2}\frac{1-\delta^{\tau^u-1}}{1-\delta}-\tau\delta^{\tau^u-1}\alpha\lambda$$
$$-\frac{\alpha\delta^{\tau^u-1}}{2}\frac{\delta-\delta^{\tau}}{1-\delta}+\left(\alpha\delta^{\tau^u-1}\right)\frac{\delta-\delta^{\tau^u+1}}{1-\delta}+\delta^{\tau^u-1}\Pi_0. \tag{37}$$

More generally, the expected return from deviating at a given period $t<\tau^u$ is

$$D^u(\tau^u,t)=\Pi_0+\alpha\sum_{i=0}^{t-1}(\delta^{\tau^u-i}-\lambda)$$

and the expected return from staying on the path is

$$ND^u(\tau^u,t)=\frac{1-\delta^{\tau^u-t}}{1-\delta}B-\alpha\lambda\sum_{i=0}^{\tau^u-t-1}\delta^t(\delta^{\tau^u-t-i}-\lambda)$$
$$-\frac{\alpha}{2}\sum_{i=0}^{\tau^u-t-1}\delta^t(\delta^{\tau^u-t-i}-\lambda)^2+\delta^{\tau^u-t}\left[\Pi_0+\alpha\sum_{i=0}^{\tau^u-1}(\delta^{\tau^u-i}-\lambda)\right], \tag{38}$$

or equivalently

$$ND^u(\tau^u,t)=\frac{1-\delta^{\tau^u-t}}{1-\delta}B+\frac{\alpha\lambda^2}{2}\frac{1-\delta^{\tau^u-t}}{1-\delta}-\tau\delta^{\tau^u-t}\alpha\lambda$$
$$-\frac{\alpha\delta^{\tau^u-t}}{2}\frac{\delta-\delta^{\tau^u+1-t}}{1-\delta}+\left(\alpha\delta^{\tau^u-t}\right)\frac{\delta-\delta^{\tau^u+1}}{1-\delta}+\delta^{\tau^u-t}\Pi_0. \tag{39}$$

In order for any deviation to be not profitable, we need $ND^u(\tau^u,t)-D^u(\tau^u,t)=NND^u(\tau^u,t)>0$, $t=1,...,\tau^u-1$ (see condition (29) at page 42).

Notice that $SC^u(0)=\Pi_0$. Moreover, if $\tau^u>\frac{\ln\lambda}{\ln\delta}$, then this implies that there will be some periods of inapplicable basic research performed ($\gamma_t^u=0$). However, from the remarks above we know that either the team performs applicable research in any period before entering commercialization, starting from $t=0$, or the team always chooses $\gamma_t^u=0$ and does s in any period. Therefore we can write $SC^u(\tau)=\frac{1}{1-\delta}B$ for $\tau>\frac{\ln\lambda}{\ln\delta}$[31]. ■

As for the industrial team, we proceed in the same way, and obtain the following

Proposition 4 *Define*

$$\Pi_0=\frac{pR}{1-\delta(1-p)}-K. \tag{40}$$

$$SC^f(\tau)=\frac{\alpha\delta^\tau}{2}\frac{\left(\delta-\delta^{\tau+1}\right)}{(1-\delta)}+\delta^\tau\Pi_0 \tag{41}$$

and

$$NND^f(\tau,t)=\frac{\alpha\delta^{\tau-t}}{2}\frac{\left(2\delta^{t+1}+\delta^{\tau+1-t}-2\delta^{\tau+1}-\delta\right)}{(1-\delta)}-(1-\delta^{\tau-t})\Pi_0 \tag{42}$$

$$\forall t=1,2,...,\tau-1.$$

i. *If* $\exists\,\tau^f$ *such that*

$$\tau^f=\arg\max_{\{\tau\}}SC^f(\tau)$$

$$SC^f(\tau^f)>\Pi_0,$$

and

$$NND^f(\tau^f)>0\ \forall\, t=1,2,...,\tau^u-1, \tag{43}$$

then the company team performs fundamental research for τ^f *periods, from period* 0 *to period* τ^f-1*, enters commercially relevant activities in period* τ^u*, i.e.* $a^u_{\tau^f}=c$*, and keeps trying until success. In each period* $t=0,1,...,\tau^f-1$*, the team invest an amount* $\gamma_t^f=\alpha\delta^{\tau^u-t}$ *in 'applicable' basic research.*

ii. *If* $\Pi_0>SC^f(\tau^f)$*, then the team undertakes commercially relevant in the first period* $t=0$ *and tries until success.*

Proof. Follows from the proof of Proposition 3, once we recall that $B^f=\lambda^f=0$. ■

We see how the results derived and discussed in the paper can all be derived also from this more general formulation. The reluctance and selection results, which state that the parameter space for which the academic team enters commercialization at some finite time is a subset of the parameter space for which the company team enters, can be seen as follows. If $\Pi_0>\frac{1}{1-\delta}B$, then a fortiori $\Pi_0>0$, so for sure the company team does find it profitable to enter commercialization, at least a t=0. Suppose now that $\Pi_0<0$ and

$$SC^u(\tau)=\frac{1-\delta^\tau}{1-\delta}B+\frac{\alpha\lambda^2}{2}\frac{1-\delta^\tau}{1-\delta}-\tau\delta^\tau\alpha\lambda+\frac{\alpha\delta^\tau}{2}\frac{\left(\delta-\delta^{\tau+1}\right)}{(1-\delta)}+\delta^\tau\Pi_0>\frac{1}{1-\delta}B \tag{44}$$

or equivalently

[31] The problem can also be seen as one in which the team has a dichotomous choice: either enter commercially relevant activities, or not enter. If it does not enter it performs inapplicable basic research for ever. If it does enter, the team decides how many periods it will perform applicable basic research before attempting the commercial activities.

$$\frac{\alpha\lambda^2}{2}\frac{1-\delta^\tau}{1-\delta}-\tau\delta^\tau\alpha\lambda+\frac{\alpha\delta^\tau}{2}\frac{\left(\delta-\delta^{\tau+1}\right)}{(1-\delta)}+\delta^\tau\Pi_0>\frac{\delta^\tau}{1-\delta}B(>0) \text{ at some } \tau\in(0,\frac{\ln\lambda}{\ln\delta}). \tag{45}$$

This implies that the academic team will enter commercialization at some point. If assumption (45) is true, then the company team could always choose an investment level and entry time so as to achieve a positive return, and therefore will enter.

There also are parameter values such that the slowness and rush results holds, i.e. the academic team may enter commercialization later or earlier than a company team would[32]. The upper bound to the applicable research periods for the academic team introduces a 'bias' for the academic team not to spend too much time in applicable research, making the two 'extreme' options, i.e. entering commercialization at date 0 or staying on 'ivory tower research' for ever, more appealing. The upper bound to τ^u is negatively related to λ, the recognition cost: the closer λ is to 1, the smaller $\frac{\ln\lambda}{\ln\delta}$.

[32]For example, for $p=\delta=.5$, $R=6,000$, $B=500$, $\alpha=810$, $K=3,000$, $\lambda=.1$, we have $\tau^u=1$ and $\tau^f=0$. For $p=\delta=.5$, $R=5,000$, $B=500$, $\alpha=1,500$, $K=3,000$, $\lambda=.1$, we have $\tau^u=\infty$ (the university team never enters) and $\tau^f=1$.For $p=\delta=.5$, $R=7,000$, $B=250$, $\alpha=3,000$, $K=4,000$, $\lambda=.28$, we have $\tau^u=0$ and $\tau^f=1$. For $p=.7$, $\delta=.9$, $R=10,000$, $B=400$, $\alpha=1,900$, $K=6,000$, $\lambda=.4$, we have $\tau^u=5$ and $\tau^f=3$.