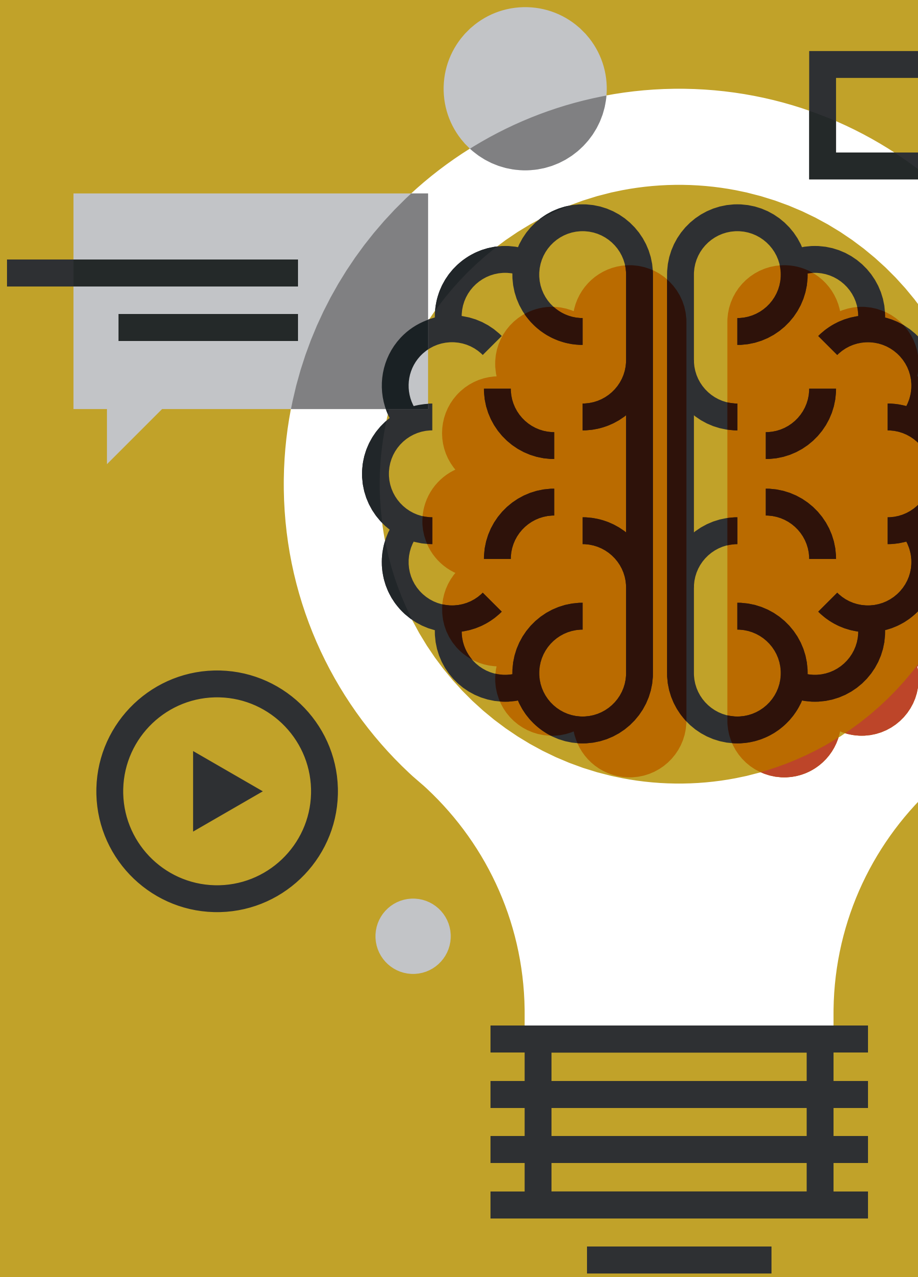
tillväxtanalys

En del av ramprojektet
"Hur kan staten bidra till innovation i nya och små företag genom inkubatorer?"



PM 2018:20

Does incubation lead to innovation?

Evidence from the Swedish incubation program

THIS REPORT ADDRESSES a potentially important effect of incubators, namely their impact on innovation. It uses a rich individual-level dataset with CEOs and employees and information about their patenting activity, which has been linked to registry data on the full population of Swedish firms and individuals. Such an analysis has, to the best of our knowledge, not previously been possible.

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Förord

Frågeställningarna inom tillväxtpolitiken är komplexa och kräver en djuplodande och mångsidig belysning för att ge kunskap om vad staten kan och bör göra. Tillväxtanalys arbetar därför med vad vi benämner som ramprojekt. Ett ramprojekt består av flera delprojekt som bidrar till att belysa en viss frågeställning och löper i upp till två år. Den här studien bildar ett kunskapsunderlag för ramprojektet *Hur kan staten bidra till innovation i nya och små företag genom inkubation*, som avrapporteras i december 2018.

Delrapporten undersöker en potentiellt viktig effekt av inkubatorer, nämligen deras effekt på innovation. Rapporten använder ett rikt individbaserat material över företagsledare och sysselsatta och information om de är uppfinnare som är länkade till hela befolkningen och svenska företag. Såvitt vi känner till har detta inte tidigare kunnat göras. Det viktigaste resultatet är att operativa företagsledare ökat sin patentering, i skattningarna med så mycket som 300 procent i genomsnitt, jämfört med en jämförelsegrupp av liknande individer.

Rapporten har skrivits av Docent Olof Ejermo, Lunds Universitet. Den har kvalitetsgranskats externt av Dr. Daniel Halvarsson, Ratio och Professor Patrik Tingvall, Kommerskollegium.

Stockholm, november 2018

Enrico Deiaco
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Foreword

Growth policy is a complex topic which demands a thorough and multifaceted analysis in order to generate knowledge about what the state can and should do. Growth Analysis therefore works in what we call framework projects. A framework project consists of several sub-studies which contribute to illuminating a particular issue in growth policy and runs for about two years. This sub-study is part of the framework project *How can the state foster innovation in young and small firms through incubators*, which will be presented in December 2018.

The sub-study investigates a potentially important effect of incubators, namely their impact on innovation. The report uses a rich individual-level dataset with CEOs and employees and information about their patenting activity, which has been linked to registry data on the full population of Swedish firms and individuals. Such an analysis has, to the best of our knowledge, not previously been possible. The main result is that CEOs of incubated companies increase their patenting activity, in some estimates with as much as 300 per cent on average, compared to a control group of similar individuals.

The report has been written by Associate Professor Olof Ejermo, Lund University. It has been externally reviewed by Dr. Daniel Halvarsson, The Ratio Institute and Professor Patrik Tingvall, National Board of Trade of Sweden.

Stockholm, November 2018

Enrico Deiaco
Director, Innovation and green transition
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Sammanfattning

Denna rapport undersöker en potentiellt viktig effekt av inkubatorer, nämligen deras påverkan på innovation. Leder inkubatorverksamheten till ökad innovation hos de företag som inkuberats och som examinerats från inkubatorer?

För att analysera denna fråga har vi använt unika data med information om specifika individer (uppfinnare) och deras patentering före och efter att de inkuberats. Rapporten använder ett individbaserat material över företagsledare och sysselsatta, information om de är uppfinnare (har patent) och som sedan länkats till hela befolkningen och svenska företag. Datamaterialet har flera fördelar. För det första går det att följa individernas patentering över tid, före och efter inkubation. En annan fördel, jämfört med många andra datakällor som innehåller uppgifter om innovation, är att de allra minsta företagens innovationsförmåga kan analyseras då de ofta utesluts i enkätbaserade undersökningar. En metodologisk innovation i rapporten, i förhållande till tidigare studier om inkubatorer, är att vi kunnat matcha egenskaper före inkubatorbehandling på *individnivå*, snarare än på företagsnivå.

En jämförelsegrupp av sådana uppfinnare har skapats och som är så lika de ”inkubator-behandlade” individerna som möjligt. Detta gör att effekterna av att ha deltagit i en inkubator kan studeras. Ett matchat urval av ”inkubatorbehandlade” individer har skapats som kontrasteras mot en jämförbar grupp av individer. Denna jämförelsegruppansats är viktig då den gör det möjligt att identifiera huruvida det finns en kausal effekt på innovationsförmågan efter att ha vistats i en inkubator.

Det viktigaste resultatet är att operativa företagsledare ökat sin patentering med så mycket som 300 procent i genomsnitt efter inkubation jämfört med jämförelsegruppen. För sysselsatta i inkubatorer finner vi en ökning av patenteringen men vi kan inte i regressionsanalyserna hitta statistiskt signifikanta effekter, det vill säga vi kan inte utesluta att någon effekt föreligger. Effekten på företagsledare är både starkare och ser ut att sträcka sig över en längre tid efter avslutad inkubation. De positiva effekterna återfinns huvudsakligen bland individer som inte tidigare var uppfinnare, snarare än de som registrerats som uppfinnare före inkubation.

När resultat i föreliggande delrapport jämförs med observationerna i en tidigare rapport om de ekonomiska effekterna av att ha vistats i en inkubator finner vi två huvudresultat: 1) inkubation leder inte till ekonomiska resultat mätt i tillväxt av omsättning och förädlingsvärde, åtminstone inte på kort sikt efter inkubationen (Tillväxtanalys, PM 2018:02), och 2) inkubation leder till innovation, mätt med patent.

Summary

This report addresses a potentially important effect of incubators, namely their impact on innovation. Using rich individual-level data on CEOs and employees and whether they are inventors (has patented), we can link these individuals to incubator firms and the general population of individuals and firms. In this way, we are able to create a matched sample of treated and controls allowing us to contrast the effects on treated individuals with those in the matched control group, and derive a plausibly causal effect on treated individuals comparing their performance after incubation with that before incubation in so-called difference-in-differences regressions.

The dataset offers a number of advantages. First and foremost, the individuals can be followed longitudinally before and after incubation. This allows us to understand how their inventive capacity changes during incubation, both compared to themselves and to the control group. Another advantage of these data, in contrast to many other sources of innovation data, is that there is no firm size threshold set. Surveys to collect innovation data are typically sent to firms with a certain size threshold in terms of employees or sales value, with the idea of creating representative samples. By contrast, the inventor dataset employed here make it no less likely that small firm inventors are in the sample, and this is advantageous because incubator firms are typically quite small.

A methodological innovation in the context of incubation for the purposes of this report is that we are able to match on pre-treatment characteristics on the level of *individual* rather than on the firm. This also differs from most other studies that use patent data, which tend to use patents linked to firms. As is known patent data have various caveats but is the only option in the current case. Alternative innovation indicators tend to be survey-based and small firms and/or firms with low turnover are not covered, a particularly disturbing fact when the interest is on small firms coming from incubators.

The report shows that incubation has a positive innovation effect on some individuals that participate in incubation. The main finding is that CEOs, plausibly causally, increase their patenting by 300 per cent compared to the control group, on average. This effect is very clear for CEOs, who increase their patenting by 300 per cent compared to a carefully matched control group. The effect on CEOs is strong and seems to last long after incubation. Positive effects mainly pertain to the subpopulation of previous non-inventors, rather than previous inventors. For employees, although simple descriptive statistics suggest that there is an effect, matched sample regressions cannot systematically find significant effects.

Putting these results into context, and together with the earlier report (Tillväxtanalys, PM 2018:02), we have two key results: 1) incubation does not foster economic performance measured as growth in turnover and value added, at least not in the short run following incubation and 2) incubation does foster innovativeness, measured with patents

1 Introduction

This report evaluates innovation measured through patenting resulting from incubation. To do this, it exploits a comprehensive inventor-individual database, where the link between individuals and incubator firms is created from registry data at Statistics Sweden. Individuals are linked to firms either through being a CEO or an employee. At the same time, an individual control group is created of individuals that share characteristics with those of incubator CEOs/employees.

The dataset offers a number of advantages. First and foremost, the individuals can be followed longitudinally before and after incubation. This allows us to understand how their inventive capacity changes during incubation, both compared to themselves and to the control group. These comparisons further make it possible to make causal interpretations answering whether incubation is likely to lead to more innovation.

The empirical results answer this question with a clear yes, but with the qualification that the effect is primarily found among CEOs and not necessarily among employees. The most relevant regressions show an increase in patenting activity of close to 300 per cent for CEOs compared to control group CEOs. For employees the effect is not significantly different from zero, although also here point estimates indicate a positive effect. It could be that this result would have been significant had we had a longer timer series for patenting at our disposal. In number terms, 11 CEOs are inventors after the firm has graduated from incubation and actually as many as 56 employee inventors can be observed. The corresponding numbers in the control group are just 3 and 25, respectively.

In an earlier report on the economic performance of incubator (Tillväxtanalys, 2018), it was found that on many indicators, incubator firms performed worse than the control group of firms. A combined reading therefore suggest that, rather than promoting short-term growth measured through regular indicators such as sales and value added, this report instead finds that incubation stimulate innovation, which is arguably more long-term. In this sense, and in line with theory, the incubator programs seem to complement the short-sighted nature of markets, which may have little patience in fostering innovativeness. However, official documents emphasize the growth-enhancing capabilities. While the earlier results do not rule out such long-term effects, this report in combination with the earlier suggest a mismatch between the incubator programs and their actual effects. A clear conclusion is therefore that 1) either program theory needs to be adapted to outcomes, or 2) the programs need to be drastically modified to deliver short-term growth. The first approach makes more sense from both a theoretical and implementation perspective. This report gives credence to the view that innovation effects are more likely to be obtained.

Chapter 2 gives a short review of the aims of the incubator programs. This is mainly a summary of earlier documents produced by the agency as part of the incubator evaluation program. Chapter 3 discusses theoretical motives for why incubation can be expected to have innovation effects.

2 Theoretical rationales and the Swedish incubator program

Science parks, incubators and other programs run by government to stimulate new firm formation share certain theoretical rationales for their existence.

On a basic level the existence of a “liability of newness” should be recognized (Freeman et al., 1983, Stinchcomb, 1965). According to this perspective, new innovative (or technology-based) firms suffer from the double challenge of convincing markets to take on their firm’s products. It is a double challenge because any firm needs to convince markets, but innovative firms not only have a problem of legitimacy in themselves, they also need substantially more capacity to convince about the need for their unusual offer to the market than ordinary firms. Incubators, or other programs, could shelter such firms from immediate pressure to enter the market. Programs to stimulate innovation can also complement incubator firms’ in-house competence by providing coaching, management and legal advice. They can also provide access to networks, both within the incubator, but primarily to other actors, such as venture capitalists or other financiers, or look for successful exit opportunities, according to which an outside firm acquires and commercialize their product.

Such an outcome is in line with the idea of Baumol’s “David-Goliath symbiosis”, according to which small innovative firms can successfully be integrated by large firms who possess the capacity to market and host products within their existing logistical infrastructure (Baumol, 2002).

A third perspective for why the government should intervene to support new technology comes from Jaffe (1998), who in his analysis of the Advanced Technology Program (ATP) suggested to divide the returns of firms into private and social. Private returns include those we commonly observe and are well-proxied for by indicators such as sales, profits and value added. Social returns include those that accrue to society and are of two kinds. First, there are unmeasured potential large social returns directly involving the firm in question that go beyond the directly attributable private returns.

A good example is Trajtenberg (1990), who in his analysis of citations in patent data on the computerized tomography (CT) scanner unravels huge social returns. Second, social returns of highly innovative firms extend across the boundaries of the firm where they originate. While new products are often championed by new firms (Christensen, 2000), the role of developing the product further is often taken over by other firms. If a firm can successfully protect the intellectual property of its development, the future returns on its innovative product act as an incentive to its development. However, as many small firms have painfully experienced, innovative products are hard to protect. This creates an incentive problem and motivates public support, i.e., products such as lighthouses and knowledge-embedded goods belong to the category of public goods (Jones, 2002).

A final related point is that new firms and new products and services that serve certain policy objectives may be more desirable to support. One such area pointed out in Swedish policy is sustainable development, which is visible in programs at e.g., the Swedish Energy Agency and Vinnova. Areas prioritized by policy or areas with the potential of outlining new directions for growth, i.e., with large potential spillovers in new areas, constitute yet another rationale (Bresnahan and Trajtenberg, 1995).

As discussed in an earlier report (Tillväxtanalys, 2018), there are reasons to expect that incubator firms perform worse than other firms, based on how the incubator selection process works. First of all, incubation entails the addition of some sort of complementary efforts to boost the competitiveness of the tenants of incubators. In other words, there must somehow be a role for them to fill, otherwise such prospective firms would not seek to be incubated. Second, incubators generally see some potential in the firms that they select for incubation. If new firms are distributed along a simple parameter called performance, the best performers would probably seek not to be incubated. Among the remaining firms, incubators try to select the best. They may seek firms of a certain technological level or type.

Thus, while the standard selection argument may be fruitful for general economic performance, it may need qualification when discussing innovation. As discussed above, innovative firms could first of all find it difficult to directly enter into the market, because of their double liability of newness. Thus, while the above selection argument may hold for more 'standard' firms and standard indicators, innovative firms may seek the signal of a credibility stamp from an outside evaluator such as an incubator. They may also need and seek complementary skills from incubation. Innovative firms may certainly possess the capability to come up with great ideas with the skill set of high levels of human capital to back it up but have little or no market competence to enter the market. This suggests that incubator firms are both selected differently, and that incubator firms may self-select themselves differently to incubation than 'standard' firms.

The Swedish national incubator program took off in the early 2000's thanks to the launch of national funding channelled through Vinnova from the Swedish government (Tillväxtanalys, 2017). This led to a large increase in the number of incubators and eventually to a large increase in the number of incubated firms. The early programs emphasized a bridging role between academia and industry and their potential to deliver new technology-based growth. This stemmed from a perceived need to increase efforts to commercialize Swedish research as some observers had suggested that the Swedish system was underperforming as formulated in the notion of 'the Swedish paradox' (Edquist and McKelvey, 1998). Later empirical research and theoretical arguments has criticized the interpretation of earlier evidence and added empirical evidence to demonstrate that the Swedish performance is much more positive than those based on the original claims (Jacobsson et al., 2013, Ejermo and Andersson, 2013, Ejermo and Kander, 2009, Ejermo and Kander, 2011, Ejermo and Källström, 2016). This evidence, however, does not invalidate the basic theoretical rationale that there may be an important role for public policy in promoting new innovative firms.

The early program, though not strongly theoretically founded, was therefore not mis-guided. But what has later followed does not conform well to theory. In the late 2000's the program expanded by allowing for a much larger variety of incubators. Such expansion risk moving the rationale for strong innovativeness to a more diffuse objective, and the complementary role of incubators to the market may therefore have become less obvious.

3 Earlier literature

There is certainly a voluminous literature on incubators especially if we add the closely related literature on science parks. Both instruments could be described as providing somewhat of a sheltered existence for its tenants with the potential for internal spillovers and varying intensity of interaction. Incubators, however, are more intense and incubation is supposedly short-lived. Science parks[1] as well as incubators are often in the vicinity of universities, following an understanding that academic institutions can contribute to development of its tenants, e.g., through research or an educated workforce. An even more pronounced version of incubators is the accelerator, where the incubation is counted in months.

Very little research has used a control group approach trying to assess whether incubators give rise to effects and as such can be regarded as mainly qualitative (for an overview, see Diez-Vial and Montoro-Sanchez, 2017). A proper evaluation that distinguishes causal from correlation and effects from selection requires the setup of a quasi-natural experiment for evaluation and is typically developed by economists using econometric tools. The gold standard is the "experimental ideal" (Angrist and Pischke, 2009).[2] In experiments, participants are randomized into treatment and control groups, respectively. Randomization assures that there is no systematic difference between the two groups *prior* to treatment. This ensures that selection – an important confounder in almost any evaluation exercise – plays no role.

The risk of confusing treatment and selection is clear once we know that incubator managers actively try to select their participants: they are not interested in helping just about anyone with business ideas. A similar analogy is where patients seeking hospital treatment are in worse shape than individuals not seeking the help of doctors. Whereas this is an example of negative selection, the case of incubators is actually not unequivocally positive. Prospective incubates are positively selected by managers among a pool of willing participants, but may be negatively selected from a market perspective, where potentially the "best" ideas may not need incubation. Thus, while positively selected from the pool, they may be negatively selected compared to other firms.

Absent the possibility to randomly allocate incubation treatment, the most common approach in this literature is to construct an adequate control group. The control group idea is based on the creation of a counterfactual. Say that we can observe an identical twin who is not treated, then the outcome of one of the twins could be compared with the non-treated twin. This approach is referred to as matching. The selection of parameters chosen for matching should be governed by theory on the factors that influence the performance of firms. It is only if matching succeeds in removing such systematic influences that treatment group and control groups can be considered to be twins in the terminology above. There could of course be unmeasurable parameters that influence outcome.

Let us now review the scant literature that adopts control group approaches.[3]

[1] "Science park" and "technology parks" are often used synonymously, so also here.
[2] Ejermo (2016) discusses this approach extensively in the context of incubators in Swedish.
[3] This section draws partially from Tillväxtanalys (2018).

3.1 Science parks and innovation

The control group approach that we discuss in this report emerges from a literature on science parks. In the 1990s, several studies on British parks emerged (Westhead, 1997, Westhead and Storey, 1994, Westhead et al., 1995). These studies used the same data material to compare on park firms with 'similar' off park firms, in essence a matching approach. They matched on sector characteristics, firm age, judicial form and region (Westhead, 1997). These four characteristics established a standard for matching in this literature.

Focusing on results related to innovation, Westhead (1997) finds that science park firms neither have higher levels of research and development (R&D), nor have higher patent levels. These are the two most commonly used indicators of innovation in the literature. In addition, they also do not find higher levels of copyright or new products or service, as compared to the control group.

There are two major issues with the data material used. First, even in the presence of matching there could be selection biases into treatment. This means that firms of certain characteristics select or self-select into incubation, and introduces a confounding effect between selection and treatment: i.e., is it really the incubation that is responsible for the differences with the control group, or is it the initial selection process? Siegel et al. (2003) tried to separate those effects, using again the same data material, by using an instrument variable to explain park selection. He used a dummy variable for "radical technology". This clearly changed the result and they were able to discern a positive, albeit small, effects on innovativeness measured by new products and processes and patents. However, it may clearly be debated if technology is a valid instrument as technology is a firm choice variable, and thus not exogenous to park participation.

The second main issue with the data used in these papers is that it is based on survey information. Thus, even without selection issues into treatment, non-responses or other types of survey biases such as those associated with imperfect recall etc. could be systematically related to the willingness to answer the survey. More successful firms could be more willing to answer or be more or less time-constrained to answer or not. The dataset is also quite small, which further aggravates any biases.

3.2 Incubator and accelerator studies

The quantitative literature draws from an earlier science park literature, which used a simplistic form of matched sample approach, whereby science park firms from the same sector, birth year and region were matched against control firm with similar characteristics. Schwartz (2013) used these match variables drawing from a large set of control firms, adding legal form of the enterprise as matching characteristic, to examine survivability among German incubator firms. He found a substantially lower rate of survivability among incubator firms.

Amezcua and McKelvie (2011) study a large sample of US incubators. They use a matched sample approach with 950 incubators and 18 909 incubated firms and 28 600 non-incubated firms. Amezcua and McKelvie (2011) examine sales and employment growth differentials for male and female entrepreneurs. Different from most other studies they use General Method of Moments (GMM) to address revesrse causality, whereby the dependent variable is instrumented by its own lag(s). They find that the effects of incubation on growth is larger for female than for male entrepreneurs. They also find that female incubators perform better than non-incubators businesses run by females.

Lasrado et al. (2016) looks at performance differences for firms incubated at university incubators compared to firms from other incubators. Using a sample of American firms 1999–2009 matched on start year, sector and number of employees, they find that university incubated firms have higher employment and sales growth than firms from other incubators.

McShane (2017) examines the performance of ICT-incubated firms in south Swedish Malmoe (Minc) and Lund (Ideon Innovation). His dataset of firms are observed before, during and after incubation and is taken from Retriever, a commercial database. He finds that return on capital and sales drops during incubation and continue to be significantly lower afterwards compared to a matched sample of non-incubated firms, while employment and assets are not significantly affected. A potential issue of the study is whether all incubated firms can really be observed in the database which might induce selection.

Tillväxtanalys (2018) followed Schwartz (2013) approach by matching incubated firms at the year of examination with a control group of firms and followed them post-incubation using regression analysis, taking the panel structure of the firms and censoring because of exit into account. The report found that incubator firms as a whole performed worse than control group firms. They have a lower turnover performance. But when making a distinction between incubator firms that graduated from incubators supported by the national incubator program and non-supported incubators, it was found that firms that graduated from the incubator program generally performed economically better than other incubator firms, but still perform somewhat worse than the control group. In line with Schwartz (2013), incubator firms were found to have lower survival rates.

Tillväxtanalys (2018) made two further sub-analyses. The direction of incubator programs has previously been debated regarding whether they should be oriented towards more research-based ideas or of being more growth-oriented, adopting ideas originating from the business sector (Ejermo, 2016). Tillväxtanalys (2018) finds that ideas from individuals with background in academia or the public sector are driving the main results. The weak performance is thus largely explained by incubator firm with ideas from academic/public background. But the same group also largely explains a better growth in employees among incubator firms. This means that incubator firms with ideas from the business sector perform more like firms in the control group. The second dimension examined concerns differences in effects for incubators that have been supported or not by the so-called BIG-program (Business Incubation for Growth), 2011–2014. The analysis showed that firms from incubators supported by BIG, if anything perform worse than firms from incubators without BIG-support, regardless of whether BIG-supported incubators are among the most supported (”BIG6”) incubators or among those supported through operating expenses.

In a recent report on the Finnish innovation programs, an econometric control group approach was adopted to evaluate several innovation programs (Halme et al., 2018). These include the Young Innovative Company (NIY) funding scheme and the accelerator program (VIGO). It seems that participating firms in the Finnish accelerator program are allowed to take longer time than in the Swedish system and therefore that the accelerators are more like Swedish incubators. The method used for evaluation is quite similar to that in Tillväxtanalys (2018). But firms are matched on the first year of treatment and the treatment time period is included in the evaluation period.[4] The econometric evaluation matches innovation program treated firms with a control group taken from Finnish register data, which is combined with differences-in-differences (see the methods section). The report matches on firm age (bins of 0, 2, 5 and above), sector (one of 20 dummies), employment size (bins of 0, 5, 10 and 20 workers). Halme et al. (2018) find that Finnish innovation participant firms perform clearly better than the control group, basically across all different programs.

The econometric approach is used to look at employment, turnover, and labour productivity and are generally positive and significant, or near-significant, on the 10 per cent level for all programs and indicators. Specifically, for the accelerator program employment is significant at least until four years after the match year, turnover is higher and significant, also until the fourth year, whereas while the coefficient for labor productivity is positive it is not significant. Additional descriptive analyses suggest that Tekes-supported firms, including the accelerator program, do not have higher survivability, but score higher on digitalization. Although comparability to the Swedish case is not perfect, it is probably good enough to allow for a comparison. The results from Finland are quite remarkable, where on most indicators Swedish incubators scored worse than in the Finnish case on regular economic indicators.

[4] It seems from the report that program participants must have a firm registered before they are eligible for the programs. By contrast, for Swedish incubators, firms are often registered during incubation which is why in Tillväxtanalys (2018) matching was done for the time of examination in order not to lose too many observations.

4 Data, basic methodological considerations

Three sources of data are used to construct the database for this report:

1. Information on the identity of incubated firms
2. Statistics Sweden data on individuals and firms
3. Inventor information

Data on firms participating in incubators are from Vinnova and cover the period 2005–2014.[5] This material was linked to Statistics Sweden (henceforth: SCB) on firm characteristics through their MONA-interface (www.scb.se/mona). This generally follows the process of an earlier report Tillväxtanalys (2018), where firm data were organized and arranged in a panel structure. Information on firms from SCB were used to construct a matched sample. Treated, i.e., incubated, firms and the created control group of firms were followed after incubation in terms of economic performance and survivability.

For this report, instead of using economic indicators as outcome, we look at inventors. From SCB-data, we are also able to obtain a link between firms and individuals. This demands a third set of data to be added to the main database discussed above. Inventor data were originally compiled in Tillväxtanalys (2011) to examine inventors. This database was further developed in January 2015 with an updated panel data covering about 83 per cent of Swedish inventors 1978–2010 (i.e., Swedish address) listed on European Patent Office (EPO) applications. Several published articles use these data. Because patents take a long time until they are granted, the main period of analysis in this report is 2005–2012. In robustness analyses, we use the period 2005–2010 to address potential censoring of patent data, i.e., that not all patents in Sweden applied for at the EPO in 2011–2012 are in the database. The observed outcome in this report is thus an inventor's patenting, where the main outcome period is what follows after incubation. A strength of these data, in contrast to many other sources of innovation data, is that there is no firm size threshold set. Surveys to collect innovation data typically sent to firms with a certain size threshold in terms of employees or sales value, with the idea of creating representative samples. By contrast, the inventor dataset employed here make it no less likely that small firm inventors are in the sample, and this is advantageous because incubator firms are typically quite small.

The reader may wonder to what extent patents really represent innovation. This is something which has been extensively debated in the literature. It can be noted that a) patents do not necessarily translate into innovation, b) can be used for other purposes than innovation (e.g., protection), c) not all inventions are patented. Still, a vast literature finds that firms which patent are more competitive, export more and are more productive, but the role of patents vary by industry. Despite these caveats, patent data remain highly useful and is the only option in the current case. Alternative innovation indicators tend to be survey-based and small firms and/or firms with low turnover are not covered, a particularly disturbing facet when the interest is on small firms. The often hailed community innovation survey is only collected every second year, and similarly to research and development data risk missing many of the newest and smallest firms.[6]

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[5] Incubator data prior to 2005 is not used due to incomplete information on firms accepted for incubation.

[6] Key references on innovation indicators include Levin et al. (1987), Griliches (1990), Trajtenberg (1990) and Smith (2005).

A methodological innovation in the context of incubation for the purposes of this report is that we are able to match on pre-treatment characteristics on the level of *individual* rather than on the firm. This also differs from most other studies that use patent data, which tend to use patents linked to firms. On patent documents, applicants are often firms, while inventors are individuals. In some cases, these may be the same if e.g., a solo inventor applies for a patent, but this is becoming increasingly less common.

Using individuals has several advantages over the use of firm level data. First, individual data provides many more observations on outcomes than firms, because several individuals may be associated with a single firm. Second, by examining individuals, we can utilize rich information in Statistics Sweden databases to construct an adequate control group, based on similar individuals. Moreover, and in contrast to firms, individuals can be observed before the actual incubation takes place.[7] As will be further discussed below, such before-after comparisons allow for causal inferences to be made.

Using inventors implies that we somehow need to link them to incubator firms. Two options exist in SCB data. Individual information exists on both the CEO (*operativ företagsledare*) and the main firm of employment. Separate analyses examine the effects of incubation on CEOs and employees respectively.

Using one of these two links comes with the assumption that the inventive activity is related to the firm. This need not necessarily be the case an individual could patent outside the context of the incubated firm. However, we do know that the individuals investigated are linked to the incubated firms. Our outcome variable is therefore whether individuals associated with incubator firms are more likely to invent after incubation. We will use the term innovative for such an outcome. It can be argued that this is a quite relevant indicator as policy may primarily be interested in whether innovative outcomes increase, not necessarily where it takes place.

4.1 Matching

The point of matching is to construct a quasi-natural experiment according to which a group subject to the treatment (here: incubation) is contrasted against a similar control group. Under the assumption that all relevant characteristics that determine assignment into treatment and the success of incubation can be captured by measurable variables (the so-called conditional independence assumption, CIA; a strong assumption), matching removes selection effects, and a causal effect of treatment can be inferred.

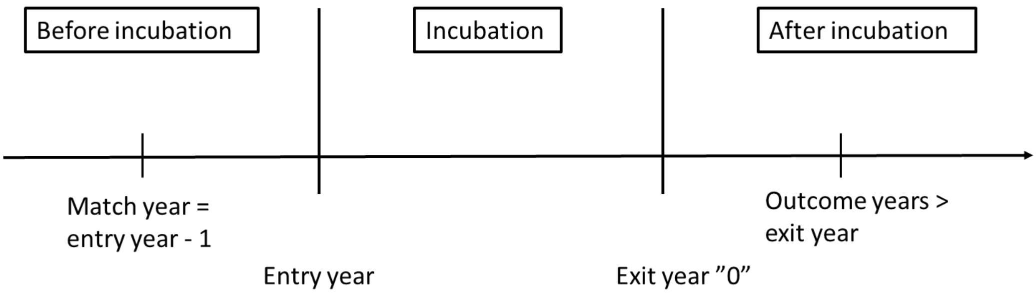
By virtue of their link with SCB data, rich individual level characteristics can be used for matching. In Tillväxtanalys (2018), incubated firms could often not be observed in SCB data until they had started their incubation process, because a firm had not been registered. In that report therefore, firms were chosen to be matched in the year of examination. This implies that there was always a risk that the effects of incubation were part of the matching and therefore could confound the results. Of course, steps were taken to alleviate these potential issues. In particular this influenced the choice of matching parameters, chosen not to be influenced by treatment.

In this report the constraint that the unit of observation cannot be observed before incubation is not an issue as individuals are generally observed in SCB databases from 16 years of age. Moreover, because individuals along with their earlier patenting can be

[7] In the earlier report on economic outcomes, most incubator firms were found to be created sometime during incubation making a before-after comparison not possible.

observed prior to incubation, we can match on outcome characteristics. Specifically, we match on cumulative inventor productivity as observed from the inventor database up to the year before incubation. Figure 1 relates and illustrates the incubation process period and how it relates to our matching and the evalution (outcome) period.

Figure 1 Basic outline of the incubation process and its relation to matching and outcome



It is not entirely clear what type of observable characteristics determine whether an individual is selected for treatment. In the case of incubator located close to universities, factors such as education type and length, age, gender and sector are likely to be important. These same factors have a strong backing from studies on the main characteristics that determine inventor productivity (Jung and Ejermo, 2014).

In matching, a trade-off always exists between the desire to obtain many matches, which increases the validity of the matched sample across the whole sample of treated individuals, and the precision in the matching, where more precision, e.g., on more variables or in more fine-grained categories, reduces the number of matches. Fortunately, quite a few potential controls exist because we can draw controls from the entire population and for this reason we can choose many variables for our matching.

The following variables were chosen for matching:

1. Birth year
2. Female
3. Match year
4. School type
5. School years
6. Cumulative patenting and patent rate

Birth year was chosen because age and life-cycle effects have been found to be an important factor explaining both scientific and technical productivity (Levin and Stephan, 1991, Jung and Ejermo, 2014).

Female is a variable taking the value 0 or 1. Gender has been shown by many studies to be highly influential in explaining inventive performance (Jung and Ejermo, 2014, Huang et al., 2011, Ding et al., 2006).

Match year is defined as the year before entry (time *t*). Control individuals are sought in the same year, since we want to remove potential effects stemming from the business cycle or technological opportunities that change over time.

School type has a strong impact on the possibility to invent, where most inventors have education in technical sciences (e.g., engineering), followed by medicine and natural sciences (Tillväxtanalys, 2011). The variable school type used for matching is constructed from the first digit of SUN2000 type codes, where it represents general education type (technical, medical, natural sciences, social sciences/humanities, other).

School years is also used as it has a strong influence on patenting (Toivanen and Väänänen, 2015), where number of years in education typically increases the propensity to patent. It is for instance much more common among university educated and academic researchers to patent (Tillväxtanalys, 2011).

Cumulative patenting t-2 and *patent rate t-1* are highly important factors. Although most individuals only invent once, a seasoned inventor is much more likely to invent than someone without inventive experience. This has to do with familiarity of the patent system and other cumulative learning effects of invention. Following Azoulay et al. (2009), we recognize that not only cumulative patenting is important, but also to some extent the distribution such that a person with a high rate of patenting just prior to incubation may have a different propensity to patent compared to a person with the same cumulative patenting, but whose patenting activity took place further back in time. The differentiation into two variables takes this into account.

Sector of work may be an important factor determining the potential for relevant patent experience. We use the division by Schwartz (2013) into nine sectors of the economy.

We employ the method Coarsened exact matching (CEM), a flexible approach that allows the researcher to vary matching between coarsened or exact. In contrast to propensity score matching, the matched sample will by construction have common support and neither needs to be tested post matching or estimation (Iacus et al., 2012). We match 1:1, i.e., each treated individual has one control individual. There is thus no need to create weights to allow for different number of controls for each treated.

From the pool of potential controls there is an order of magnitude more potential matches for non-inventors than for inventors as being an inventor is highly unusual among the whole population among which we try to find matches. Therefore, it was expected that it would be more difficult to find appropriate matches among earlier inventors than among non-inventors. The basic ambition was to use the same matching principle for inventors and non-inventors alike. Thus, we match exactly on Female, Match year, School type and School years. As life cycle effects are more approximate than exact, the Birth year variable is coarsened and determined by the built-in algorithm. For the two remaining variables, the matching process was slightly different for earlier inventors and non-inventors.

For inventors, *Cumulative patenting* and *patent rate* was considered much more important matching variables than *Sector of work.* Using both for inventors was found to lower the number of matches severely. Hence, only cumulative patenting and patent rate were used, while for earlier non-inventors and *Sector of work* was included as well. By definition, cumulative patenting and patent rate were zero for earlier non-inventors. For inventors, categories of cumulative patenting and patent rate now had to be decided. First, the distribution of the two variables among treated and control was tabulated, and then the categories chosen. It was clear that many more potential controls in the lower end of the patenting distribution existed. Theoretically, it could be argued that the potential for increasing patenting with experience matters more when an individual is at her early stages of being an inventor, i.e., at low productivity levels. Therefore, the binning ranges were chosen thinner at the lower end of the patenting distribution. Moreover, patent distributions were found to differ between CEOs and employees, with CEOs generally being more prolific at the higher end of the distribution. For CEOs the following binning structure was used for cumulative patenting in *t-2*: (0.5 1.5 2.5 3.5 11 19.5 30 70). For employees, the following binning structure was used: (0 1.5 3.5 6.5 10.5 20).

CEM constructs strata based on the matching and binning structure chosen. In practice this involves creating a large number of cells in M dimensions, M being the number of parameters. All units are put in the cells and then the best match is picked within a stratum. Sometimes more than one treated and control unit end up in the same stratum. While treated units are unique (and therefore only appear once), control units are observed in multiple years and are therefore potential candidates more than once. Therefore, after matching a check was undertaken to make sure that controls do not pop up in multiple strata. This happened in one case for CEOs and this strata was simply dropped and the units put in the unmatched group.

4.2 Period of investigation and treatment variables

In principle, a control could have been allowed more than once in the dataset, had it not been for the fact that we need to define a unique “pseudoincubation” period. Our interest is in the period following incubation, because the incubation period is part of the treatment and it is the long-term success of incubation that is of interest. While it is clear that a postincubation period exists for treated individuals, for control units the timing of postincubation was transferred from the treated individual in the same matched stratum. For example, if the match year was 2004 and the exit year for a treated individual was in 2006 for a stratum in question, and hence postincubation started in 2007, the year 2007 was assigned to the control individual. In a very few cases, more than one treated-control pair existed in the same strata, and therefore multiple exit years existed. These exit years were then randomly allocated among the controls.

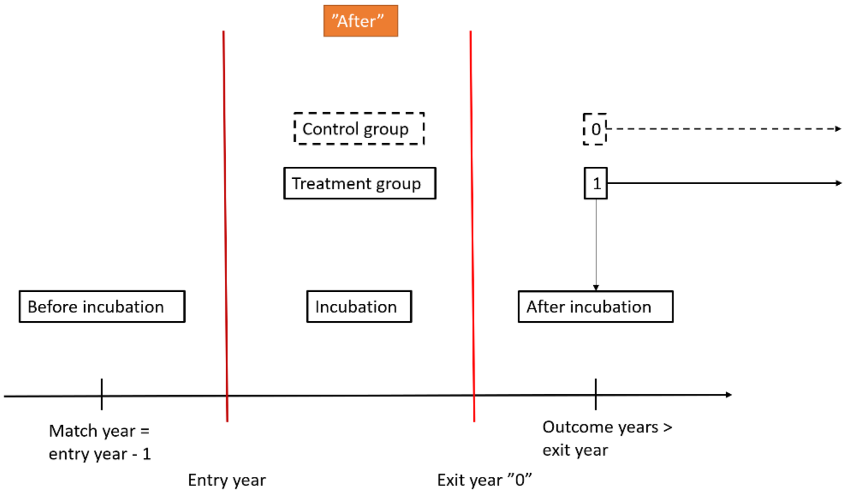
An important aspect of the analyses is that all postincubation years were used in the analysis. While the treated CEOs and employees must have been subject to incubation, we do not require that they continue as CEOs/employees in the incubated firm. It is the change in total inventive activity for the individual, not just at the incubated firm, which is used. The argument for this is that this is what is socially most relevant. The alternative would have been to ensure that all individuals stay with the firm, but there could be turbulence among both treated and control units which would mix firm and individual data in a complicated, and non-transparent manner. The current setup is simpler and more transparent, and arguably makes intuitive sense.

While the postincubation period is our evaluation period, the period before incubation is also used in the main estimations as it makes it possible to compare an individual’s patenting before and after incubation. Moreover, what goes on during incubation is of interest. In the regression analysis, we can therefore distinguish between three different ways of comparing treated with control units, see Figure 2. The indicator (dummy) variable *After*, panel (a) in the figure, uses only post-incubation and pseudo-incubation observations. It takes the value 1 for treated firms and 0 for control firms in our matched sample. The estimator therefore highlights differences in the two groups after incubation. Implicitly, this indicator also assumes that matching removes all selection effects. The second indicator (b in the figure) is labeled *Treatment* and in addition to the observations from *After*, uses all observations during incubation, i.e., from entry year to examination. Treatment is problematic from an evaluation perspective as changes may take place at different points in time for incubated objects and may not manifest themselves until after incubation. Its main role is to descriptively highlight if treatment subjects start to change already during incubation and compare results with the other indicators.

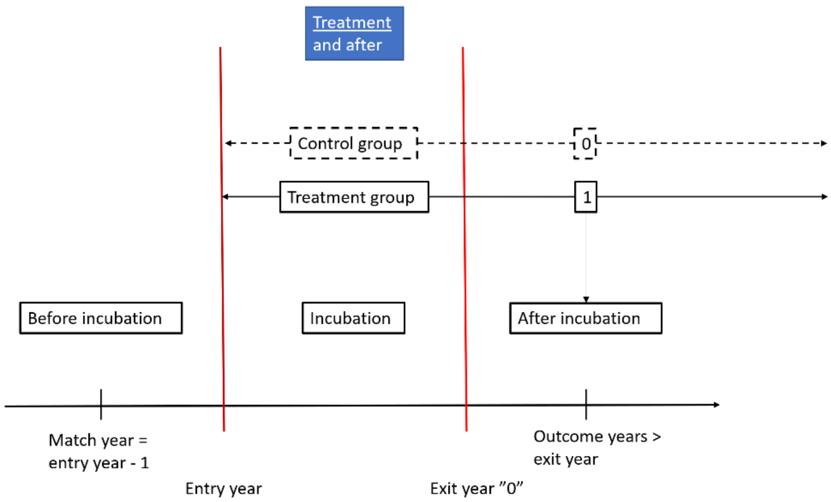
Finally, *Difference-in-differences* (*DiD,* panel c) uses observations from both before incubation and after incubation. The DiD-indicator takes the value 0 for <u>both</u> treated and control groups before the treatment (incubation) period and the value 1 for just the treatment group after treatment, i.e., once they have left the incubator. DiD makes treated subject use their own observations as control observations before treatment as well as the control group. This effectively leads to baseline observations that net out any underlying trend existing before incubation. DiD is considered the main evaluation parameter in the regressions that follow. In addition, DiD can be estimated using for each period (year) a dummy variable whose estimated coefficient can be used to test whether pre-trends exist. If so, our matching has not succeeded in eliminating pre-trends a problem which would violate a key assumption that our matching aims at addressing.

Figure 2 Illustration of After, Treatment and Difference-in-differences indicators

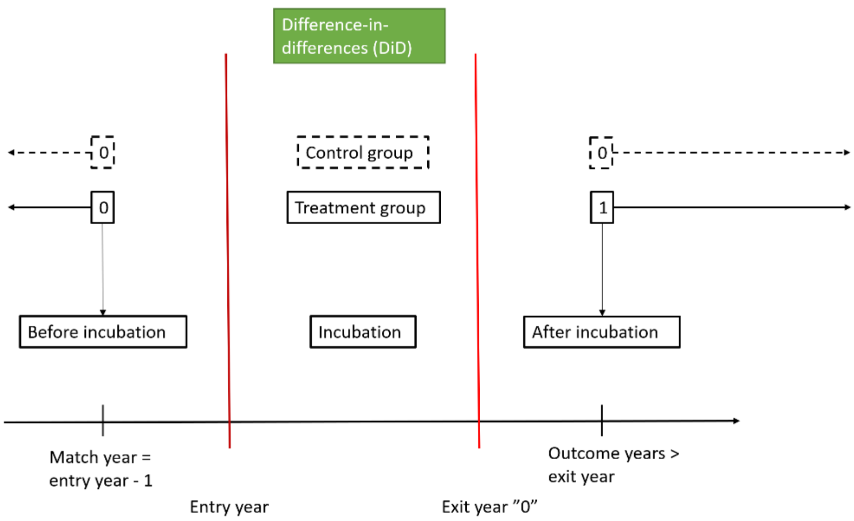
(a)



(b)



(c)



5 Matching results and descriptive information

Table 1 (CEOs) and Table 2 (employees) show descriptive characteristics of the two matched samples and the population from which controls are drawn. These tables make it abundantly clear that treated individuals (column 2) are substantially different from the pool of controls (column 1 and 6). A major difference concerns the number of observations, where there are in total 296 treated CEOs and 2585 treated employees in incubator firms, almost a ten-fold difference. However, the populations from which we can draw controls also exhibit large differences, with CEO potential control x year observations amounting to more than 900 000 and employee potential control observations being more than 1.5 million. This should improve our possibilities to find appropriate controls among employees.

CEOs in incubator firms are significantly less frequently female, only 19 per cent compared with 32 per cent among all CEOs. They are born later and have different schooling from other CEOs, involving in particular more natural sciences and technology/engineering-based education (in total 62 per cent among treated CEOs vs. 41 per cent among all) and they have 2.5 more school years than for all CEOs. Treated CEOs have substantially more patents on average. Their wage income is also higher, earning about 270 000 SEK per year, compared to 167 000 SEK for all CEOs. Columns 3 and 4 show the matched treated and control group individuals. The matched control group is by construction very similar to that of the treated matched group. A certain validity of the construction of the sample is given by the fact that the wage level is quite similar, although significant the difference is just about 10,000 SEK per annum, despite not having been matched on. In total, only 10 CEOs, or about 3 per cent, were not matched. It is clear that they are somewhat different from the treatment group, in having had substantially more patenting and earning higher incomes, differences that, are not statistically significantly different. Closer inspection of the data show that earlier inventors are common among the unmatched treated CEOs, as out of 10 unmatched individuals, 6 had earlier inventors experience distributed as 2 persons with 1 earlier patent, 1 person with 5 patents, 1 person with 7, 1 with 8 and 1 unmatched star inventor with as many as 45 previous patents. Despite varying the bin size for earlier inventors, this matching could hardly be improved without imposing the substantial risk that the matched individuals are not sufficiently similar.

Similar patterns are found for the matched sample of employees. Here, 34 per cent are female, compared to 53 per cent in the general population. Treated are born later and have more school years, they have even more pronounced differences in in terms of natural sciences and technology/engineering-based education (54 per cent for incubator employees vs just 15 per cent for all) and more patent experience, though far from the high share as found for treated CEOs. The wage income is dramatically higher, primarily because the full sample includes many individuals with very low incomes. Matched treated and controls this time have larger wage differences, with controls having about 24 400 SEK *higher* income per year. This difference is significantly different from zero but is not huge in economic terms.

An even higher share than for CEOs, 99.3 per cent, of treated employees are matched. Non-matched have different school types and are significantly older. They earn substantially more than the matched group, with a yearly income 200 000 SEK higher on average. They also patent more. We find 4 unmatched individuals with 1 earlier patent, 4 with 2, 1 with 3, 1 with 4 patents, 1 with 5, 1 with 6, 1 with 7, and 1 with 8 earlier patents. This distribution is less extreme than for CEOs, but patenting is still significantly higher among non-matched.

Table 1 Descriptive information of CEOs (full sample, treated and controls) and their statistical differences

	(1) Full sample mean/sd	(2) All treated mean/sd	(3) Control matched mean/sd	(4) Treated matched mean/sd	(5) Treated non-matched mean/sd	(6) Difference (1)-(2) b/se	(7) Difference (3)-(4) b/se	(8) Difference (4)-(5) b/se
Female	0.32	0.19	0.19	0.19	0.30	0.10^{***}	0.00	-0.11
	0.47	0.39	0.39	0.39	0.48	0.03	0.03	0.15
School type	3.37	4.00	3.98	3.98	4.60	-0.57^{***}	0.00	-0.62
	2.75	1.82	1.82	1.82	1.65	0.12	0.15	0.53
Birth year	1960.02	1969.95	1969.87	1969.93	1970.50	-10.61^{***}	-0.06	-0.57
	13.54	10.62	10.69	10.70	8.17	0.70	0.89	2.66
School years	11.83	14.39	14.37	14.37	14.90	-2.56^{***}	0.00	-0.53
	2.39	2.14	2.13	2.13	2.47	0.14	0.18	0.79
Applied patents, cum.	0.01	0.35	0.13	0.13	6.70	-0.15^{***}	0.01	-6.57
	0.33	2.76	0.67	0.61	13.81	0.05	0.05	4.37
Wage income	166.73	270.23	274.34	264.03	447.66	-103.20^{***}	10.31	-183.63
	235.65	280.21	305.43	274.05	398.98	18.27	24.26	127.21
Observations	914655	296	286	286	10	914887	572	296

* p < 0.1, ** p < 0.05, *** p < 0.01. t-tests in (6)-(8) use Welch's approximation for unequal number of observations. For treated and controls, observations are from match year, for full sample CEOs from 2004. Wage income is in 1000 SEK's.

Table 2 Descriptive information of employees (full sample, treated, controls) and their statistical differences

	(1) Full sample mean/sd	(2) All treated mean/sd	(3) Control matched mean/sd	(4) Treated matched mean/sd	(5) Treated non-matched mean/sd	(6) Difference (1)-(2) b/se	(7) Difference (3)-(4) b/se	(8) Difference (4)-(5) b/se
Female	0.53	0.34	0.34	0.34	0.26	0.18^{***}	0.00	0.08
	0.50	0.48	0.48	0.48	0.45	0.01	0.01	0.10
School type	2.80	3.70	3.69	3.69	4.89	-0.93^{***}	0.00	-1.20^{**}
	3.19	2.38	2.38	2.38	2.35	0.05	0.07	0.54
Birth year	1955.80	1974.64	1974.64	1974.68	1969.47	-18.75^{***}	-0.04	5.21^{*}
	19.16	10.68	10.67	10.66	11.52	0.22	0.30	2.65
School years	10.61	13.27	13.26	13.26	14.44	-2.67^{***}	0.00	-1.19^{**}
	2.38	2.42	2.42	2.42	2.23	0.05	0.07	0.53
Applied patents, cum.	0.03	0.10	0.08	0.08	2.37	-0.05^{***}	-0.00	-2.28^{***}
	0.37	0.69	0.59	0.62	2.50	0.01	0.02	0.57
Wage income	8.98	202.63	225.56	201.16	401.05	-200.40^{***}	24.40^{***}	-199.89^{**}
	64.99	220.78	277.46	219.37	313.74	4.42	6.98	72.11
Observations	1556456	2585	2566	2566	19	1558891	5132	2585

* p < 0.1, ** p < 0.05, *** p < 0.01. t-tests in (6)-(8) use Welch's approximation for unequal number of observations. Wage income is in 1000 SEK's.

Still, even though among both CEOs and employees non-matched there are more inventors among treated that are not matched, this does not necessarily imply that results become biased in our regressions, as appropriately matched individuals among the controls should also have patented earlier. On the other hand, there is some risk that the results based on the matched samples do not appropriately generalize to the population of inventors. How frequent are inventors before, during and after (pseudo-)incubation?

Table 3 Count of inventors, and their patents applied within brackets, before, during and after incubation for CEOs and employees in the matched sample

	Before (in match year)	**During**	**After (t > 0)**
CEOs			
Treated	1 [1]	20 [37]	11 [27]
Control	1 [1]	6 [14]	4 [6]
Employees			
Treated	17 [20]	65 [131]	57 [104]
Control	18 [27]	27 [88]	21 [48]

Table 3 lists the count of unique inventors and their patents applied for (not their cumulative patenting) in the year of matching, before, during and after incubation. Note that neither here, nor in the regressions do we adjust for double-counting, i.e., two persons listed on the same patent are counted twice. This follows common practice in the literature.

It is clear that both the number of inventors and their patents increase drastically for both CEOs and employees both during and after incubation compared to both before incubation (i.e., the match year) and compared to controls. We find only 1 inventor in the treatment group and the control group alike among CEOs before in the match year. Both applied for 1 patent. During incubation, the number of inventors among CEOs goes up to 20, applying for 37 patents. Among controls, there are 6 inventors with 14 patents. After incubation, there are 11 inventors in the treatment group, applying for 27 patents. The control group has 6 patents by 4 inventors. Even though not many, the increase in inventive activity among treated CEOs is substantial after incubation but much smaller in the control group.

Among employees, we have many more inventors, in part because we have more observations of employees. Among treated, we have 17 inventors in the match year applying for 20 patents. During incubation, the number of inventors increase to 65 in the treatment group, applying for 131 patents. After incubation the number of inventors are 57, applying for 104 patents. This means that the number of inventors has increased by more than 200 per cent and that there are more than 400 per cent more patents in the treatment group compared to the match year. In the control group we start with 18 inventors in the match year responsible for 27 patents, 27 inventors and 88 patents during the constructed incubation interval period and 21 inventors and 48 patents after incubation. The rate of increase in patenting (less than 80 per cent) is not as large as for the treatment group.

6 Regression methodology[8]

Regressions represent more advanced methods to investigate the differences in inventive output, that net out effects that could arise from e.g., specific years, due to business cycle effects or otherwise. This report uses the Poisson estimator in a panel (longitudinal) setting with individual effects and robust standard errors clustered on the individual level as the preferred estimator (Hausman et al., 1984). This estimator addresses several econometric issues that could potentially arise in the current research setting.

Fixed effects (FE) can be estimated using the *DiD*-estimator described above. Individual FE:s have the great advantage that unmeasured effects that are constant on the individual level which influence patenting are absorbed by the FE. These could be unknown ability effects or random influential effects, such as an attorney father specializing in patent law who greatly contribute to the capacity to invent for an individual. It should be noted that FE estimations require us to use panel data, in order to be able estimate any other parameters of interest.[9]

The choice of the Poisson is motivated by the fact the dependent variable is applied patent counts for an individual. We thus estimate the probability to observe the dependent variable conditional on some parameters of interest (Cameron and Trivedi, 1998). The Poisson estimates a parameter, λ, based on the following probability density function (with a time interval set to 1):

$$\Pr(Y = y_i) = \frac{e^{-\lambda_i}\lambda_i^{\,y_i}}{y_i!} \qquad (1)$$

Most commonly, λ_i is specified in loglinear form as $\ln \lambda_i = \boldsymbol{x}_i'\boldsymbol{\beta}$. $\boldsymbol{x}_i$ is the vector of explanatory variables and $\boldsymbol{\beta}$ the estimated parameter vector (Greene, 2008).

Combined with robust standard errors, the Poisson estimator addresses another important issue. If the dependent variable, patent applications, is strongly correlated with an individual's past applications and the cumulative patent stock, matching on the dependent variable would give rise to serial correlation in the error term (Bertrand et al., 2004). However, conditional Quasi Maximum Likelihood (QML, "robust standard errors") Poisson is robust to arbitrary patterns of serial correlations. Moreover, this estimator is robust to distribution misspecification, i.e., when the underlying data-generating process is not Poisson (Wooldridge, 2002, Silva and Tenreyro, 2006, Azoulay et al., 2010). This last point addresses a critique often levied against the Poisson, that of its implicitly assumed equality of variance and mean. The advocated estimator does not rely on the Poisson variance assumption (Azoulay et al., 2010). Finally, Wooldridge (Wooldridge, 2002, p. 672) points out that the random effects Poisson estimator is sensitive to several maintained assumptions.

But *After* and *Treatment* are time-invariant (in contrast to DiD) and can therefore not be estimated using FE. For *Treatment*, we use the pooled Poisson QML estimator. Wooldridge (2002, p. 669) notes that this estimator is consistent and again robust to distributional misspecification, given that the model is correctly specified. This could of

[8] The author would like to thank Daniel Halvarsson and Patrik Gustavsson Tingvall for valuable discussions and input to this section.

[9] This comes from the two potential ways in which fixed effects are 'netted out', through first differences or within-transformations that deduct the average by individual, which requires more than one observation per individual.

course be disputed, and so interpretation of this estimator needs to be hedged with caution. For *After,* we proceed by using a method called the inverse probability of censoring weights (IPCW). Censoring issues arise after incubation if CEOs or employees leave the sample for endogenous reasons. It seems likely that individuals with a lower probability of patenting might exit the sample more quickly and this could in turn influence the parameter estimate. Note that retirement is not such a type of exit, but death (probably negatively related to patenting) or emigration (positively) would be. The basic idea of IPCW is to inversely weight observations that are more likely to be censored, i.e., give them a lower weight. Following Azoulay (2009) the censoring weights can be written as

$$\mathrm{sw}_{it}^{*} = \prod_{\tau=0}^{t} \frac{\Pr(Exit|X_{i\tau})}{\Pr(Exit|X_{i\tau}, Z_{i\tau})}, t > t_e \tag{2}$$

where t_e is the year of exit from incubation. That is, the weight in *t* is dependent on the product sum of earlier years' probability to exit. $X_{i\tau}$ works as a 'stabilizer', since the variables are modeled as estimated probabilities in both the denominator and the numerator. It contains the following variables: dummies for years since incubation, dummies for firm sector at t_e, and a dummy for if the individual is female. The denominator holds in addition a vector $Z_{i\tau}$ which includes individual age and age squared, wage income and lagged firm age. Both the numerator and the denominator is estimated using logit regressions and then the weights are calculated. The IPCW is estimated with Poisson, but does not use the panel structure other than through the weights, as panel Poisson estimation methods require weights to be constant.

In the estimations, in addition to the indicators *After, Treatment* and *DiD,* we include year fixed effects and a constant. In the pooled regressions we also include a dummy (intercept) for the treated individuals.

In the above matched DiD-model, an important assumption is that of parallel trends which requires that no clear trend pattern in the outcome variable can be discerned before incubation, because this would suggest that there remains negative/positive selection of the treated individuals relative to the control group that our matching does not take into account. To test this, we estimate a variant:

$$\ln \lambda_i = \sum_{\tau=0}^{m} \delta_{-\tau} D_{t-\tau, t-\tau<t_e} + \sum_{\tau=1}^{q} \delta_{+\tau} D_{t+\tau, t+\tau>0} + year_t + u_i + e_{it}, t < t_e \text{ or } t \geq 0, \tag{3}$$

which includes *m+1* lead effects and *q* lagged effects. Note that the trend dummies are only estimated for the pre-/postincubation periods (i.e., the incubation period is never included). If matching and the regression setup does not successfully remedy pre-incubation trend differences, we should observe that lead effects are present in the data for the incubated individuals. This will be graphically illustrated through plot of the estimated coefficients of the trend dummies along with their estimated confidence interval.

7 Regression results

7.1 Main regressions

Table 4 and Table 5 show the main results for CEOs and employees respectively. All coefficients are exponentiated, implying that they can be interpreted as percentage increases after deducting one and multiplying by 100. The different models make it possible to differentiate between changes during and after incubation to arrive at a causal implication. The main estimated model is the poisson FE model (4) in both tables.

Starting with CEOs, the percentage increases are really large, but not implausible given the descriptive counts given in Table 3. *After* shows that CEOs from incubator firms have 291 per cent more patenting than CEOs from non-incubators. This difference is significantly different on the 1 per cent level.

Consider instead the *Treatment* estimator. The coefficent is somewhat lower, and not strictly comparable. The estimated difference is 201 per cent. It is a smaller estimate (though not using the exact same method, see section 6), corroborating the descriptive difference that suggested that the main differences in patenting arise after incubation. The main results are obtained from the DiD-estimates as they net out underlying trends, account for FE (in model 4) and therefore have stronger causal interpretations. Similar to OLS and ordinary fixed effects regressions, it is usually expected that the true effect is in between the pooled and the FE, because some variation is invariably captured by the FE and the OLS overstates the true effect. Model 4's increase of almost 300 per cent is probably the most accurate. It is also remarkably close to the estimate in model 1. All models 2–4 are significant on the 5 per cent level.

Table 4 Main regressions. Sample: all CEOs

	(1) Poisson IPCW	(2) Poisson pooled	(3) Poisson pooled	(4) Poisson FE
After	3.91^{***}			
	(1.91)			
Treatment		3.01^{**}		
		(1.44)		
DiD			5.24^{**}	3.96^{**}
			(3.63)	(2.65)
Number of observations	1465	2907	6186	404
Number of individuals	494	500	502	31

Exponentiated coefficients; robust standard errors in parentheses.
FE = individual fixed effects. All regressions are panel models and include year fixed effects and a constant (not reported). Pooled models also include a dummy for the treatment group. Sample period is 2005-2012.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5 shows the same setup for employees. The number of observations are substantially larger, which raises the precision of the estimates. The table shows a smaller effect on patenting among employees from incubators than for CEOs. The models indicate that patenting increases by 45–73 per cent. However, despite the larger number of observations, the coefficients are only significant in model 1, at the 5 per cent level. A closer look at the regression output shows that in model 3 the p-value is 0.12 (in model 2 it is 0.27; in model 4, 0.30). In sum, we conclude that we cannot definitely say that there is a positive effect on employee patenting.

Table 5 Main regressions. Sample: all employees

	(1) Poisson IPCW	(2) Poisson pooled	(3) Poisson pooled	(4) Poisson FE
After	1.73**			
	(0.48)			
Treatment		1.51		
		(0.57)		
DiD			1.71	1.45
			(0.58)	(0.53)
Number of observations	15023	26295	56058	2478
Number of individuals	4667	4699	4718	191

Exponentiated coefficients; robust standard errors in parentheses
FE = individual fixed effects. All regressions are panel models and include year fixed effects and a constant (not reported). Pooled models also include a dummy for the treatment group. Sample period is 2005-2012.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The next step is to check whether pre-trends exist. This basically involves replacing the DiD indicator by trend dummies. The coefficients are drawn using unexponentiated coefficients in Figure 3, where panel a) shows the trend dummies for CEOs and panel b) for employees. Point estimates are indicated through bullet points and 95 per cent confidence intervals by the vertical lines intersecting the points at the center. Unfortunately, these regressions could only be estimated using random effects Poisson for the case of CEOs and pooled Poisson for employees. This is obviously far from ideal given the arguments presented earlier, but is still arguably indicative of trends in temporal patterns.

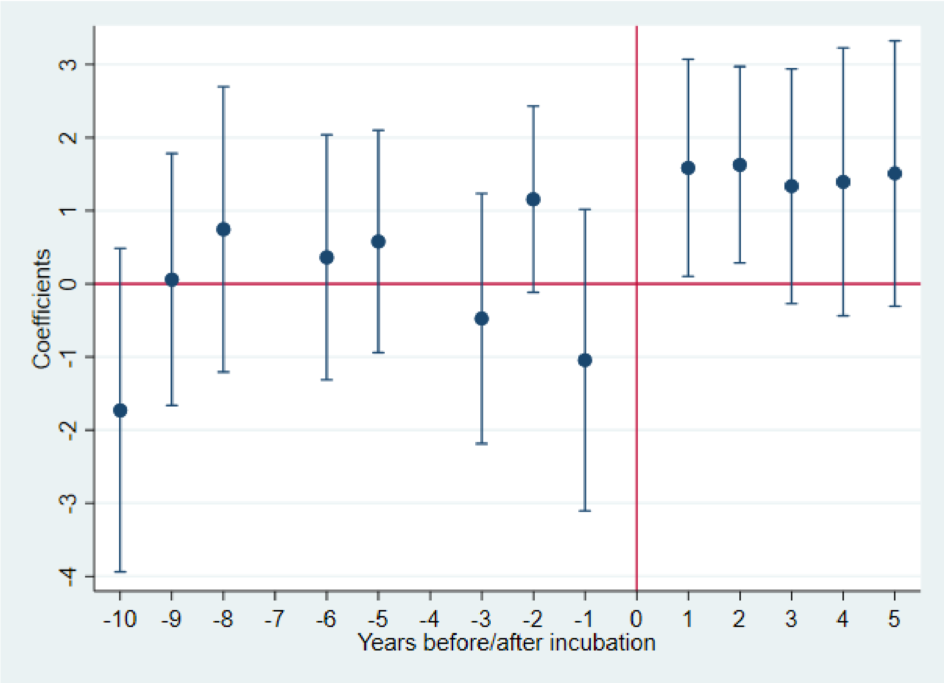
None of the CEO lead effects are statistically significantly different from zero on conventional significance levels, because the confidence intervals stretch over the zero line. All the lag effects are positive and roughly of the same size. Two are significant on the 5 per cent level, all five at least on the 15 per cent level.

For employees, most lead effects are insignificantly different from zero. For -7 and -8 there is a negative and significant effect on the 5 per cent level and a positive -2 effect on the 10 per cent level. But since this pattern is not systematic, there is little reason to think that selection effects abound. Besides this, the most important result is that -1 is very close to zero. When it comes to (lagged) post-incubation effects, all three years that directly follow after incubation are above zero but are not significant. The lead coefficients 1–3 (exponentiated values 1.34–1.93) are close to the main estimate (1.45) from model 4, Table 5.

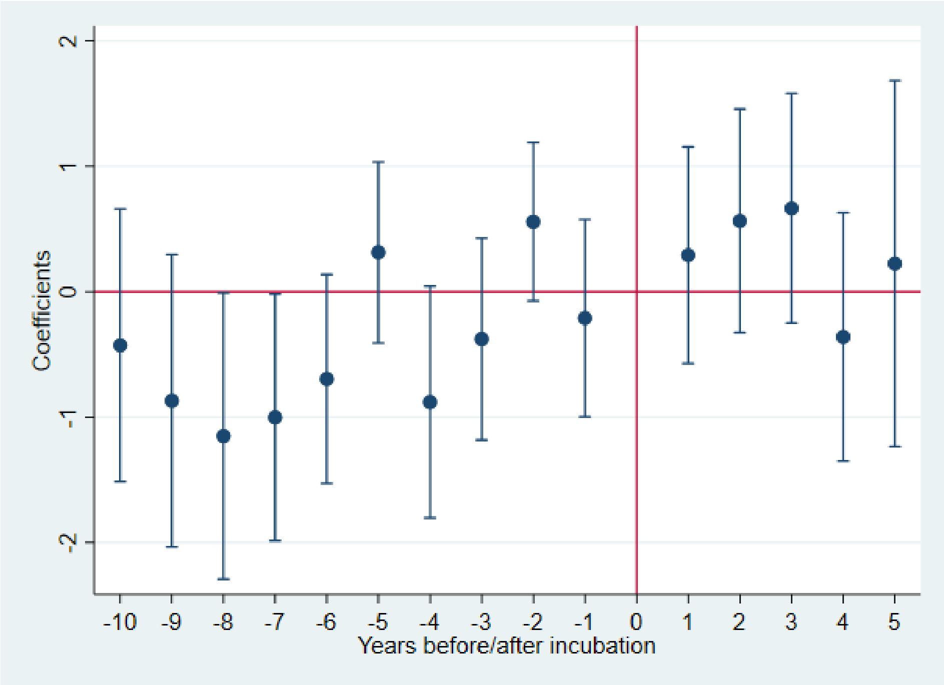
One interpretation of the results is as follows. Because we do not exclude observations of individuals to the incubated firm (they may have left it but we still use their inventive activity as outcome), one could expect a lower attachment of employees to the incubated firm than CEOs. Moreover, if CEO often are more closely linked to the development of innovation, it is natural to expect both a stronger innovative effect and that the effect lasts longer after incubation. Our results so far are clearly in line with this interpretation.

Figure 3 Trend dummy coefficients before/after incubation and associated 95 per cent confidence intervals

(a) CEOs



(b) Employees



Remark: -10 is a dummy that indicates ten years before incubation, -9 indicates nine years before, -8 indicates eight years before, ..., 1 indicates one year after, 2 two years after, ..., 5 indicates five years after incubation.

7.2 Early inventors vs. noninventors

An additional dimension according to which it would be interesting to analyse the results is to understand whether any rise in inventive activity primarily relate to individuals that have previously invented, or appear among new inventors. To do this, we create a dummy variable taking the value 1 for individuals with previous patent experience ("early inventors") and 0 for those without ("earlier noninventors").

Table 6 and Table 7 first include models 1–2 and then the estimated models 3–4 of Table 4–5 for ease of comparison.

Table 6 Regressions on earlier noninventors/inventors. Sample: CEOs. (1), (2) identical from main regression table, earlier inventors only in (3), (4).

	(1) Poisson pooled	(2) Poisson FE	(3) Poisson pooled, inventors	(4) Poisson FE, inventors
DiD	5.24**	3.96**	3.67	1.92
	(3.63)	(2.65)	(2.99)	(1.23)
Number of observations	6186	404	377	309
Number of individuals	502	31	30	24

Exponentiated coefficients; robust standard errors in parentheses
FE = individual fixed effects. All regressions are panel models and include year fixed effects and a constant (not reported). Pooled models also include a dummy for the treatment group. Sample period is 2005-2012.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7 Regressions on earlier noninventors/inventors. Sample: employees. (1), (2) identical from main regression table, earlier inventors in (3), (4).

	(1) Poisson pooled	(2) Poisson FE	(3) Poisson pooled, inventors	(4) Poisson FE, inventors
DiD	1.71	1.45	1.33	1.06
	(0.58)	(0.53)	(0.58)	(0.45)
Number of observations	56058	2478	2172	1895
Number of individuals	4718	191	168	146

Exponentiated coefficients; robust standard errors in parentheses
FE = fixed effects. All regressions are panel models and include year FE and a constant (not reported). Pooled models also include a dummy for the treatment group. Sample period is 2005-2012.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

7.3 Robustness check

Our inventor material is based on patent information until application year 2012. Obviously it would have been more satisfying to have more up-to-date material, but this is what has been available. But also this material is incomplete in the sense that not all patents applied for at the European Patent Office from 2011 and 2012 are there. Although we have no reason to suspect that biases exist in the data, if applications were systematically missing from one of the incubator/control groups, our estimations above could be biased. On the other hand, if those are not systematically missing from one group, losing observations would lower the precision of the estimated effects. A simple way of testing for this is to re-run the main estimated relationships without the years 2011 and 2012. This is done in Table 8 and Table 9. Starting with CEOs in Table 8, all models display somewhat smaller coefficients. Significance levels are also lower. In model 3 it is significant on the 10 per cent level and model 4 now on the 20 per cent level. This is hardly surprising as right censoring the data material leads to a substantial drop in number of observations (14–57 per cent of observations). In conclusion, the results are fairly robust to removing the observations after 2010 and the ensuing loss in significance can be attributed to lower number of observations.

Table 8 Regressions with 2010 as last observation year. Sample: all CEOs.

	(1) Poisson IPCW	(2) Poisson pooled	(3) Poisson pooled	(4) Poisson FE
After	3.20**			
	(1.84)			
Treatment		2.86**		
		(1.38)		
DiD			4.49*	2.89
			(3.64)	(2.42)
Number of observations	624	1950	5327	313
Number of individuals	285	500	502	28

Exponentiated coefficients; robust standard errors in parentheses
FE = individual fixed effects. All regressions are panel models and include year fixed effects and a constant (not reported). Pooled models also include a dummy for the treatment group. Sample period is 2005-2010.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

For employees, the estimated effects are somewhat larger for *After* and *Treatment* and is now significant also for Treatment, at least on the 10 per cent level. The DiD coefficients are somewhat smaller and remain insignificant. In sum, the results are not radically different and suggest again that the coefficients are fairly robust despite the fact that between 15–55 per cent of observations are dropped.

Table 9 Regressions with 2010 as last observation year. Sample: all employees.

	(1) Poisson IPCW	(2) Poisson pooled	(3) Poisson pooled	(4) Poisson FE
After	2.29**			
	(0.97)			
Treatment		1.88*		
		(0.62)		
DiD			1.51	1.17
			(0.64)	(0.50)
Number of observations	6780	17045	47771	1870
Number of individuals	1229	4690	4714	165

Exponentiated coefficients; robust standard errors in parentheses
FE = fixed effects. All regressions are panel models and include year FE and a constant (not reported). Pooled models also include a dummy for the treatment group. Sample period is 2005-2010.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

8 Conclusions

The report quite clearly shows that incubation has a positive innovation effect on some individuals that participate in incubation. This effect is very clear for CEOs who increase their patenting by 300 per cent compared to themselves and a carefully matched control group. The results for employees is somewhat less clear. Some of the estimates indicate that a positive effect exists. But this effect is not robust to change in estimation method, and in our preferred estimation we cannot rule out that the effect is zero.

Another result is that positive effects pertain to individuals who have not invented before. Finally, we should also mention that the inventiveness effect that we observe is not confined to the incubated firm. In fact, the analyses set no restriction that individuals need to stay at the firm. This is relevant from a policy perspective, which should look for an overall societal effect on innovativeness, whether at the incubated firm or in a new context. Our findings suggest that the effect on CEOs is both larger and lasts longer after incubation than for employees, possibly because CEOs may stay longer with the incubated firm.

Setting these results in context, and together with the earlier report (Tillväxtanalys, 2018), we have two key results: 1) incubation does not foster economic performance, at least not in the short run following incubation and 2) incubation does foster innovativeness. This contrasts to a large extent the ambitions of the programs, where the possibility to scale activities is the primary objective. The results in this report suggest that while this aim is not clearly fulfilled, another objective which could be recognized in the program construction, to raise innovativeness seems to be reached. Certainly, such an aim is much more in line with a possible ambition for policy to complement market efforts. For instance, stimulating short-term growth could possibly be an objective which venture capital funds more easily obtained. Long-term growth achieved through intermediate outcomes would be more in line with discussions how government could stimulate growth in a complementary role (Jaffe, 1998).

Certain limitations should make us somewhat cautious about strongly interpreting the results. These limitations are that a) the matched sample approach may not necessarily address the selection issues we face, b) the time period under investigation has incomplete data, c) patent data do not necessarily reflect innovativeness and d) we cannot separate between firms that participated in incubators in the national program and firms that incubated in incubators outside the program.

Beginning with the matched sample, we have done every effort possible to match individuals that take part in incubation, CEOs and employees, to find an appropriate control individual. This effort is aided by the fact that we have observations on the individual level, where data availability is extremely good. We can, uniquely for this report, match individuals before incubation, which makes it possible to match them on the outcome variable, and follows state-of-the-art econometric techniques (e.g., Azoulay et al., 2010). These data furthermore allows for difference-in-differences estimation to be undertaken, which support a causal interpretation of the results. The second caveat is that the data material ends in 2012. Ideally, we should have more recent data available. It may be that more recent data could have led to more precisely estimated coefficients, although robustness analyses mainly support the main results. The third issue is that patent data do not necessarily reflect innovativeness. This concern is somewhat non-constructive. Patent data, certainly in this context, represent the only possibility for which we can examine innovativeness. Thus, if we believe that patent information cannot be used, it is a pity that the reader has come this far! To the defence of patent information should be said that, even if not used in innovations, the incubation process at least led to something which was felt worthwhile to protect through intellectual property rights, and that in many areas patents are useful to attract additional venture capital funding. Moreover, there is usually a strong correlation between patents and other existing indicators, suggesting that had we had the possibility to collect them, it is likely that we would have found similar results. Of course, only future research could address this fully should ever alternative indicators be available. But it is hard to see such information being developed on the level of individual and over time. Finally, due to data accessibility, this report could not distinguish between firms that incubated as part of the national incubator program and other incubated firms, because Statistics Sweden would not allow us access to this information for reasons of anonymity.[10]

[10] This distinction was possible in an earlier report on economic effects of incubation, where data were housed at the agency (Tillväxtanalys, 2018)

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