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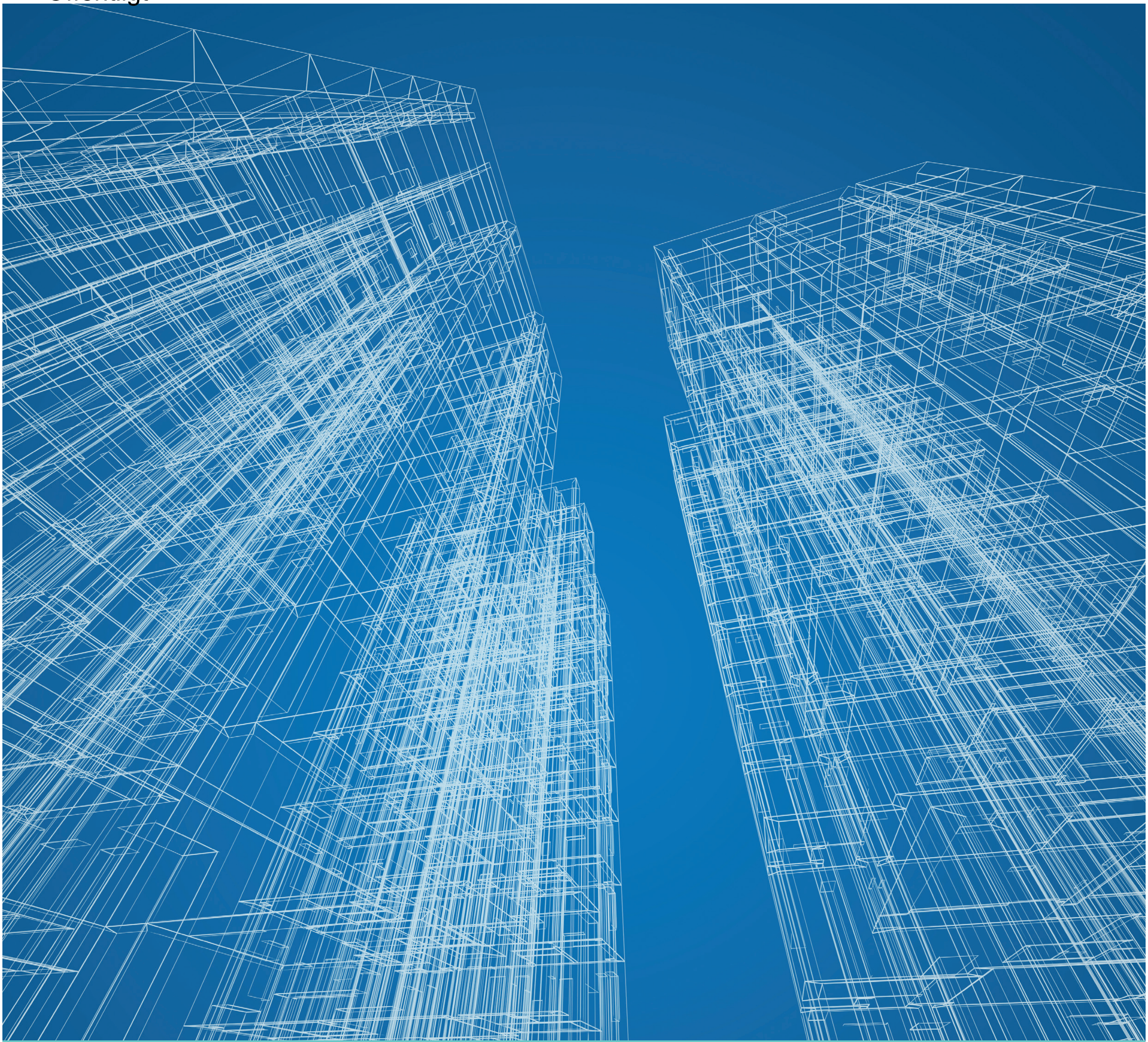

Analyses of Danish Innovation Programmes

– a compendium of excellent econometric impact analyses

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– a compendium of excellent econometric impact analyses

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Analyses of Danish Innovation Programmes

– a compendium of excellent econometric impact analyses

Copenhagen, November 2013

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>

FOREWORD

Progress depends on knowing what works and what does not. The primary tool for attaining knowledge is analysis based on proper scientific principles and methods. It is my hope that this compendium contributes to the advancement of sound knowledge about what works in innovation policy, which is a notoriously difficult subject ripe for methodological improvement.

The compendium consists of impact analyses of three Danish national innovation programmes funded by the Danish Council for Technology and Innovation:

– *The Innovation Assistant scheme*, which subsidises employment of highly educated individuals in small and medium-sized enterprises,

– *The Industrial PhD Programme,* where the PhD fellow is simultaneously employed by an enterprise and enrolled at a university, and

– *Innovation Consortia*, which are large scale innovation project collaborations between enterprises and public sector knowledge institutions.

Within the field of quantitative analysis of innovation policy, these three analyses are unrivalled in scope, detail and accuracy for two main reasons:

The first is the comprehensive data sets collected by Danish authorities that are available for research. They go far beyond what is generally offered in other countries.

The second reason is the methods used for these analyses. These are in accordance with the current state-of-the-art of econometric research, using the highest care in the selection of comparison groups and subsequent mathematical processing.

In combination with each other, these two factors provide for analyses that should be considered international best practice, and which provide a template for quantitative evaluations of other types of interventions in industry and society.

For this purpose, this compendium also includes a manual for carrying out such high-quality analyses.

I hope and encourage other agencies, in Denmark and elsewhere, to use this manual to measure the impacts of their own initiatives, and, through a dialogue with the Danish Ministry of Science, Innovation and Higher Education, to improve its methods of attaining sound knowledge. This is how progress is made.

Thomas Alslev Christensen
Head of Department
Danish Agency for Science, Technology and Innovation

Fremskridt afhænger af, at man ved, hvad der fungerer, og hvad der ikke gør. Det primære redskab til at opnå viden er analyse funderet på sunde, videnskabelige principper og metoder. Jeg håber, at dette kompendium bidrager til fremme af velfunderet viden om hvilken innovationspolitik, der fungerer – et overordentligt svært emne med potentiale for metodologiske forbedringer.

I kompendiet er effektmålinger af tre danske, landsdækkende innovationsordninger, som finansieres af Rådet for Teknologi og Innovation:

– *Videnpilotordningen*, som giver tilskud til ansættelse af højtuddannede i små og mellemstore virksomheder,

– *ErhvervsPhD-ordningen*, hvor den ph.d.-studerende er ansat i en virksomhed og samtidig indskrevet på et universitet, og

– *Innovationskonsortier*, som er større samarbejdsprojekter om innovation mellem virksomheder og offentlige videninstitutioner.

Indenfor kvantitativ analyse af innovationspolitik er disse tre analyser uovertrufne med hensyn til omfang, detaljegrad og præcision af to grunde:

Den ene er de omfangsrige datasæt, som danske myndigheder indsamler og stiller til rådighed for forskning. De overgår langt det, man sædvanligvis kan tilbyde i andre lande.

Den anden grund er metoderne, der anvendes i analyserne. De er i overensstemmelse med state-of-the-art indenfor økonometrisk forskning, hvor man anvender den største omhu i udvælgelsen af sammenligningsgrundlaget og den efterfølgende matematiske behandling.

Tilsammen muliggør disse to faktorer analyser, som bør anses for højeste internationale klasse, og som udgør en skabelon for kvantitativ evaluering af andre former for indgriben i erhverv og samfund.

Til dette formål indeholder kompendiet også en manual om, hvordan man udfører sådanne højkvalitetsanalyser.

Jeg håber på og opfordrer til, at andre institutioner, i Danmark og i udlandet, bruger manualen til at måle effekterne af deres egne indsatser, og i dialog med Styrelsen for Forskning og Innovation medvirker til at forbedre dens metoder til at opnå velfunderet viden. Sådan gør man fremskridt.

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An evaluation of the Danish Innovation Assistant Programme

En effektmåling af Videnpilotordningen

Copenhagen, August 2013

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SAMMENFATNING PÅ DANSK

>

Videnpilotordningen under *Rådet for Teknologi og Innovation* blev lanceret som en del af *'Viden flytter ud'*-tiltaget under regeringen i 2004. Ordningen har eksisteret siden 2005 og har som formål at øge små og mellemstore virksomheders vækst ved at øge incitamentet til og nedbryde barrierer for ansættelsen af akademikere i disse virksomheder.

På baggrund af den danske vækstudfordring generelt og den økonomiske afmatning i kølvandet på finanskrisen indtager Videnpilotordningen en central rolle blandt de politikinstrumenter, der sigter at skabe vækst og øge virksomheders kompetencer i forhold til innovation og nytænkning. Interessen for ordningen skyldes også, at en række tidligere analyser (f.eks. Junge og Skaksen, 2010, CEBR, 2011[2]) har vist positive sammenhænge mellem virksomheders andel af højtuddannede medarbejdere og deres produktivitet, og at udbygningen af ordningen kan argumenteres for at kunne reducere den for tiden høje arbejdsløshed blandt akademikere i Danmark.

Som led i sin løbende evalueringsstrategi har *Styrelsen for Forskning og Innovation*, der administrerer ordningen, bedt *Centre for Economics and Business Research (CEBR)* om at belyse, hvorvidt det kan vises, at ordningen lever op til sin målsætning. Til dette formål har CEBR fulgt både deltagende personer og virksomheder i et omfattende datamateriale. Denne rapport beskriver tilhørende analyse.

Med hensyn til metodologi, analysevariation samt hvilke indikatorer, der vurderes, er denne effektmåling af Videnpilotordningen i international sammenhæng 'best practice'. Den kan tjene som målestok for evaluering af effekten af en specifik indgriben i erhvervslivet, der kan udføres, hvis behandlingsgruppens etablerede datakvalitet er ganske høj, og der findes højt detaljerede landsdækkende registre med dataserier over tid for virksomheder og individer.

Analysen sammenligner løn- og beskæftigelsesudvikling for en stikprøve af individer, der deltager i ordningen (videnpiloter) med andre, sammenlignelige personer, der ikke deltager. Analysen sammenligner også vækst og produktivitetsudviklingen i en stikprøve af virksomheder, der deltager i ordningen, med andre (meget) sammenlignelige virksomheder, der ikke deltager.

[2] Junge og Skaksen, 2010, Produktivitet og videregående uddannelse, CEBR, 2011, Ansættelse af Ph.D.er og produktivitet.

Analysens resultater kan sammenfattes som følger:

Personer, der deltager i ordningen, øger deres beskæftigelsesrate i forbindelse med deltagelsen i ordningen. Dette er ikke overraskende, da ansættelse er en definerende karakteristik af selve ordningen. Efter mere end et år efter begyndelsen af deltagelsen kan det dog ikke længere vises, at beskæftigelsesraten blandt deltagerne er højere end i en referencegruppe af højt sammenlignelige individer – men det kan nævnes, at analysens observationsperiode delvist ligger i en højkonjunktur med lav arbejdsløshed blandt højtuddannede.

Personer, der deltager i ordningen, øger deres lønindkomst i forbindelse med deltagelsen i ordningen. Lønindkomsten forbliver højere end i referencegruppen i årene efter begyndelsen af deltagelsen, men konvergerer herefter.

Virksomheder, der deltager i ordningen, øger deres årlige vækst i antallet af højtuddannede medarbejdere i forbindelse med deltagelsen. Det kan dog ikke vises, at virksomheder, der deltager i ordningen, bliver ved med at ansætte flere højtuddannede i årene efter deltagelsen i ordningen.

Virksomheder, der deltager i ordningen, er også kendetegnet ved et midlertidigt forhøjet antal medarbejdere i årene efter deltagelsen, men det viser sig at være svært at finde robuste sammenhænge for finansielle succesparametre som værditilvækst, profit eller arbejdsproduktivitet. Dette skyldes ret stor variation i nogle virksomheders udvikling i disse variable, som ikke er relateret til, hvorvidt de deltager i ordningen.

For delstikprøver af mindre virksomheder, som ikke er kendetegnet ved større ændringer i deres succesvariable, findes, at deltagelsen i ordningen korrelerer positivt med stigende værditilvækst og profit. Således forøger deltagende virksomheder deres værditilvækst i gennemsnit med op til ca. 800.000 kr. og profitten med op til ca. 400.000 kr. i årene efter deltagelsen.

Disse resultater peger i retning af eventuelle positive effekter af ordningen og er i tråd med en tidligere analyses[3] resultater, men er behæftede med en betydelig statistisk usikkerhed. Så selvom datamaterialet er blevet betydelig udvidet i forhold til den tidligere analyse, er det på baggrund af de nye resultater stadig ikke muligt at træffe sikre udsagn om, i hvilket omfang deltagelsen i videnpilotordningen forøger værdiskabelsen eller profitten i virksomheden.

Det er ikke muligt at påvise positive sammenhænge mellem deltagelsen i programmet og arbejdsproduktivitet, lønniveau og afkastningsgraden (return-on-assets).

[3] DASTI, 2010, "Effektmåling af videnpilotordningens betydning for små og mellemstore virksomheder Innovation: Analyse og evaluering 4/2010"

Som sammenfatning kan det siges, at eventuelle positive effekter af ordningen kommer til udtryk i, at videnpiloter kommer hurtigere i arbejde, hvilket er forbundet med, at de kommer på et højere lønniveau i de første år efter deltagelsen end andre, sammenlignelige personer, der ikke deltager. Disse potentielle effekter kan forventes at være højere i de nuværende år, som i modsætning til en stor del af analyseperioden er kendetegnet ved en lavkonjunktur.

Resultater for virksomhedsdelen peger i retningen af, at virksomheder, som deltager i ordningen, oplever højere vækst i værditilvækst og profit, men en betydelig statistisk usikkerhed medfører, at disse resultater skal fortolkes med forsigtighed.

EXECUTIVE SUMMARY

The Innovation Assistant Programme under the Danish Council for Technology and Innovation was launched as part of the *"Knowledge is moving out"*-initiative by the Danish government in 2004. The programme has existed since 2005 and has the purpose of increasing the growth of small and medium-sized enterprises by increasing incentives and breaking down barriers to employment of highly educated individuals in these enterprises.

Because of Denmark's growth problems in general and the economic downturn in the wake of the financial crisis, the Innovation Assistant Programme plays a central part among the policy instruments aiming at creating growth and increasing the competences of enterprises on innovation and creative thinking. The interest in the programme is also due to a number of previous analyses (ie. Junge og Skaksen, 2010, CEBR, 2011[4]) that have shown positive correlations between the share of highly educated employees in enterprises and their productivity, and that the expansion of the programme can be argued to reduce the presently high unemployment rate among the highly educated in Denmark.

As part of its ongoing evaluation strategy, the Danish Agency for Science, Technology and Innovation (DASTI), which administers the programme, has asked the Centre for Economics and Business Research (CEBR) to cast light on whether it can be shown that the programme fulfils its objectives. For this purpose, CEBR has followed both participating individuals and enterprises in an extensive set of data. This report describes the corresponding analysis.

With regard to methodology, variation of the analysis and the indicators taken into consideration, this impact analysis of the Innovation Assistant Programme is international best practice. It may serve as a standard for intervention evaluations that can be carried out if the established data quality of the treatment group is quite high, and highly detailed national registers with data time series for enterprises and individuals are available.

The analysis compares salary and employment developments for a sample of participating individuals (innovation assistants) with other comparable individuals not participating. The analysis also compares growth and productivity developments for a sample of participating companies with other (highly) comparable companies not participating.

The results of the analysis can be summarised as follows:

Individuals who participate in the programme increase their employment rate in association with participating in the programme. This is not surprising, since employment is a defining characteristic of the programme itself. It cannot be shown that the employment rate among participants is higher than for a reference group of highly comparable individuals more than a year after starting to participate.

[4] Junge og Skaksen, 2010, Produktivitet og videregående uddannelse, CEBR, 2011, Ansættelse af Ph.D.er og produktivitet.

However, it should be noted that the observation period of the analysis falls partly within an economic boom period with low unemployment among the highly educated.

Individuals who participate in the programme increase their salary income in association with participation. Salary income remains higher than for the reference group in the years after starting to participate, but then converges.

Companies that participate in the programme increase their yearly growth of the number of highly educated employees in association with participation. However, it cannot be shown that companies that participate in the programme continue to employ more highly educated individuals in the years after participation.

Companies that participate in the programme are also characterised by a temporary increase in the number of employees in the years after participation, but it turns out to be difficult to find robust associations for financial success parameters such as value added, profits or labour productivity. This is due to a quite large variation in certain companies' developments for these variables, which is unrelated to their participation in the programme.

For subsamples of smaller companies that are not characterised by large changes in their success variables, it is found that participation in the programme is positively correlated to increasing value added and profits. Thus, participating companies on average increase their value added by up to approx. DKK 800,000 (EUR 106,000) and their profits by up to approx. DKK 400,000 (EUR 53,000) in the years after participation.

These results point to possible positive effects of the programme and correspond with the results of a previous analysis,[5] but are subject to a significant statistical uncertainty. So even though the data material has been expanded significantly compared to the previous analysis, it is still not possible to make any certain claims about the extent that companies' value added and profits are increased by participating in the programme on the background of the new results.

It is not possible to show positive correlations between programme participation and labour productivity, salary levels and return on assets.

In conclusion, it can be said that any positive programme effects are expressed by innovation assistants finding employment quicker, which is associated with a higher salary level in the first years after participating than other comparable individuals who do not participate. These potential effects can be expected to be higher in the present years, which unlike a large part of the analysis period are characterised by an economic downturn.

[5] DASTI, 2010, "Effektmåling af videnpilotordningens betydning for små og mellemstore virksomheder Innovation: Analyse og evaluering 4/2010"

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For the company part of the analysis, results indicate that participating companies experience higher growth in value added and profits, but a significant statistical uncertainty means that these results must be interpreted with care.

DEUTSCHSPRACHIGE ZUSAMMENFASSUNG >

Die vorliegende Studie wurde vom *Centre for Economics and Business Research (CEBR)* an der Handelshochschule Kopenhagen (CBS) für die *Styrelsen for Forskning og Innovation (DASTI)* des Ministeriums für Forschung, Innovation und weiterführende Bildung erstellt.

Sie betrachtet das Wissenspilotprogramm („*Videnpilotordning“, VP-Programm*), ein vom *DASTI* geführtes Innovationsprogramm. Dieses Programm existiert seit 2005 und subventioniert die Neuanstellung von Akademikern in kleinen und mittelständischen Unternehmen mit geringem Anteil hochqualifizierter Fachkräfte durch Gehaltszuschüsse. Ziel des Programms ist es, die Kompetenzen teilnehmender Unternehmen zu erhöhen und deren Wettbewerbsfähigkeit zu steigern.

Die Studie folgt ca. 360 teilnehmenden Personen und ca. 320 teilnehmenden Firmen in dänischen Registerdaten. Diese erlauben es, Aussagen über den Berufserfolg der am Programm teilnehmenden Personen zu machen, sowie das Wachstum teilnehmender Unternehmen zu analysieren.

Der Berufserfolg wird dabei anhand der Entwicklung des Beschäftigungsgrades und Jahresgehaltes gemessen. Auf Unternehmensniveau betrachtet die Studie Entwicklungen in der Anzahl hochausgebildeter Mitarbeiter, der Beschäftigung, der Lohnkosten, sowie der finanziellen Variablen Wertschöpfung, Gewinn und Arbeitsproduktivität.

Um den Berufserfolg der teilnehmenden Personen und das Wachstum der Unternehmen beurteilen zu können, werden aus den umfangreichen vorliegenden Registerdaten Kontrollgruppen von Personen oder Unternehmen ausgewählt, die die gleichen oder sehr ähnliche äussere Merkmale aufweisen wie die Teilnehmer im Jahr vor deren Teilnahme im VP-Programm. Die statistischen Methoden der Studie bestehen aus Vergleichen der verschiedenen Erfolgsvariablen zwischen den Teilnehmer- und den Kontrollgruppen. Zusätzlich dazu erlauben die Daten, für teilnehmende Unternehmen die Entwicklungen von Erfolgsvariablen nach Teilnahme im Programm mit den entsprechenden Entwicklungen vor der Teilnahme zu vergleichen. Ein ähnlicher Vergleich für Unternehmen in der Kontrollgruppe erlaubt es, auch unbeobachtbare Faktoren aus dem statistischen Modell herauszufiltern.

Die Ergebnisse der Studie lassen sich wie folgt zusammenfassen:

Personen, die am VP-Programm teilnehmen, weisen im ersten Jahr nach Beginn der Teilnahme am Programm eine höhere Beschäftigungsquote als Personen der Vergleichsgruppe auf. Nach zwei und mehr Jahren haben sich die Beschäftigungsquoten beider Gruppen jedoch weitgehend angeglichen, womit es nicht möglich ist, einen langfristigen Beschäftigungseffekt des VP-Programms auf individueller Ebene nachzuweisen. An dieser Stelle sei jedoch darauf hingewiesen, dass ein grosser Teil der Beobachtungsperiode der Analyse in eine Zeit guter Konjunktur mit allgemein geringer Akademikerarbeitslosigkeit fällt.

Personen, die am Programm teilnehmen, weisen eine bessere Gehaltsentwicklung als Personen, die nicht teilnehmen, auf. Dieser Unterschied ist statistisch signifikant für die ersten Jahre nach Beginn der Teilnahme.

Unternehmen, die am Programm teilnehmen, erhöhen die Beschäftigung hochqualifizierter Mitarbeiter im Vergleich zu Unternehmen in der Kontrollgruppe, sowie die Beschäftigung generell mit, im Durchschnitt, ca. einem zusätzlichen Mitarbeiter in Verbindung mit der Teilnahme am Programm.

In Bezug auf die finanziellen Erfolgsvariablen lässt sich feststellen, dass es grundsätzlich schwierig ist, potentielle Teilnahmeeffekte in den Daten zu isolieren: erhebliche Heterogenität der Firmen in Bezug auf die Entwicklung der Erfolgsvariablen relativ zu der Grösse der Stichprobe und der Grösse der potentiellen Effekte führt dazu, dass die Ergebnisse der jeweiligen Analyse von der Wahl des ökonometrischen Modells sowie der Stichprobenauswahl abhängen.

In Stichproben kleinerer teilnehmender Unternehmen mit geringer Heterogenität in den Erfolgsvariablen und der Entwicklung dieser Variablen, sind teilnehmende Unternehmen durch, im Durchschnitt, höheres Wachstum in der Wertschöpfung sowie des Unternehmensgewinns gekennzeichnet. Hier liegen für teilnehmende Unternehmen die potentiellen geschätzten Teilnehmereffekte bei bis zu ca. 800,000 Dänischer Kronen (ca. 106.000€) in Bezug auf die die jährliche Wertschöpfung und 400,000 Kronen (53.000€) für Unternehmensgewinn in den Jahren nach Programmteilnahme.

Diese Ergebnisse ähneln den Ergebnissen einer früheren Studie, die auf weniger umfangreichem Datenmaterial beruht[6], lassen sich jedoch aufgrund eines Mangels an statistischer Signifikanz und fehlender Robustheit in Bezug auf die Stichprobenauswahl nicht verallgemeinern.

Für die Erfolgsvariablen Rendite (return on assets), Lohnkosten (als Mass für das Lohnniveau des Unternehmens) sowie Arbeitsproduktivität lassen sich keine positiven potentiellen Teilnehmereffekte ermitteln. Auch in Bezug auf diese Variablen lassen die Ergebnisse den Schluss zu, dass die Bedeutung der Anstellung von Wissenspiloten in vielen Unternehmen von anderen Entwicklungen überlagert wird, und dass auch das im Vergleich zu einer früheren Studie ausgeweitete Datenmaterial noch nicht ausreicht, um gesicherte Aussagen über den Erfolg des Programms treffen zu können.

[6] DASTI, 2010, "Effektmåling af videnpilotordningens betydning for små og mellemstore virksomheder Innovation: Analyse og evaluering 4/2010"

1. INTRODUCTION >

This report presents the data, methodology, and results of an evaluation of the Danish Innovation Assistant Programme (*'Videnpilotordningen' - VP programme* in the following). The analysis was completed by CEBR for DASTI in 2012. It contributes to DASTI's strategy to continuously monitor and evaluate its innovation support programmes, to develop and improve the designs of its initiatives, and to improve programme evaluation techniques.

The VP programme was launched in 2005 and aims at increasing the growth and productivity of small and medium-sized enterprises (*SMEs*) by increasing the share of their employees with a higher education.[7] It is supposed to overcome any mutual reservations between SME managers and university graduates and increase academic knowledge in SMEs. To achieve this goal, the VP programme subsidizes the employment of university graduates in small and medium-sized companies.

Although the programme is small-scale, especially when compared to e.g. U.S. or European-level knowledge transfer programmes, schemes similar to the VP programme are currently being discussed or implemented in other countries as well, for example in a couple of local states in Germany and Austria. For this reason, the present study might also have an interest outside Denmark. From an academic point of view, the study furthermore contributes to our understanding of employment subsidies for highly skilled employees and the effects of knowledge transfers to SMEs.

The present analysis was supposed to address two questions: First, how do individuals who participate in the programme perform with regard to their employment and income developments? Second, how do participating companies perform in terms of employment and productivity growth? For this purpose, individuals and companies are followed in large-scale register data, and the success of programme participants is compared to highly similar individuals and companies that do not participate in the programme.

The two different questions imply that the present report is divided into two parts. The first part addresses the question of the extent to which individuals benefit from participating in the programme. This question has recently gained increasing public attention in Denmark, as unemployment among especially young university graduates is soaring in the aftermath of the recent financial crisis and the current Danish economic slowdown. This part looks at employment and salary developments of programme participants in association with programme participation.

[7] The education classifications of this study follow the International Standard Classification of Educations (ISCED). In the following, employees with at least a post-secondary education (ISCED classifications 4,5, and 6) are referred to as 'highly educated employees'.

>

The second part of the analysis looks at whether companies benefit from participating in the VP programme. This company-level analysis is again based on large-scale register data. It might be considered of primary interest, since the purpose of the VP programme is to increase company performance, whereas any individual employment effects are secondary.

The success parameters of interest in this part of the company-level analysis are employment growth, the number of highly educated employees, and the growth in value added, profits, return on assets, average wages, and labour productivity.

2. THE INNOVATION ASSISTANT PROGRAMME (VIDENPILOTORDNINGEN) >

An Innovation Assistant (‘videnpilot’, VP) is an academic employee with a post-secondary or tertiary-level education. In Danish educational terminology, this corresponds to respectively a medium-length (bachelor level) and a long higher education (postgraduate level). The employee has to be employed in an SME to solve one or more specific development tasks.

A VP-project is subsidised by DASTI and is supposed to contribute to the company’s innovation, growth and productivity. The subsidy pays up to half of the VP’s salary, with a maximum of DKK 12,500 (€1,700) a month for 6-12 months.

Privately owned small or medium-sized companies with at least 2 and at most 100 employees can apply for funding if there are at most two highly educated employees in the company, it has existed for at least a year, and its yearly revenues surpass DKK 1 million (€130,000).

The programme was launched in the beginning of 2005. Until 2012, approximately 500 projects have been completed.[8]

For the following analysis, it is relevant to have an idea of just how VP-projects come into life to better understand what kind of individuals and companies participate in the programme. However, it needs to be acknowledged that there is little if any general knowledge about how VP-company collaborations are initiated. Anecdotal evidence suggests that it is often the VP who contacts the company and suggests an employment relationship under the VP programme. And yet, it might also be presumed that companies hiring new employees might exploit the opportunity of saving wage costs in the beginning of the employment relationship.

[8] The analysis can only consider projects for which there is information in the data after they have been started, so the most recent projects are not part of the analysis.

3. DATA >

The data for the analysis is from three sources:

1. DASTI supplied information on individual VP-projects. Information includes individual identification numbers of participating individuals, company identifiers, and the start date of the project. These data will henceforth be called the *DASTI data*.

2. Data from companies' financial reports from *Experian A/S*, a credit rating agency. These data will be referred to as the *Experian data* in the following sections.

3. Register information from *Statistics Denmark*. This is matched employer-employee data including information on individuals (demographic information, information on education, wage and occupation) and companies (e.g. size, turnover). These data will be referred to as the *Statistics Denmark data*.

DASTI data

Since the start of the programme in 2005, DASTI has continuously collected information such as individual IDs of VPs, the start-up time of VP-projects, hosting company IDs (VP-companies in the following) and whether or not projects were completed or aborted before schedule. Individual IDs are social security numbers (CPR numbers) while company IDs are the numbers by which companies are registered by the public authorities (CVR numbers).

The Statistics Denmark data

Characteristics for individuals are drawn from Statistics Denmark's register. Data is available up to 2010, implying that there is no information on the most recent projects. Statistics Denmark data is typically available on an annual basis, with census date in mid-November. It allows associating individuals with their companies using the unique company and individual IDs.[9] Over the last decades, the data resources of Statistics Denmark have been continuously extended, as all Danish data with an associated individual or company ID can be merged with the existing data. For example, the present analysis benefits from Statistics Denmark's individual-level information on education (degrees, focus of electives, grades) and company-level information on turnover.

[9] Timmermans B. The Danish Integrated Database for Labor Market Research: Towards Demystification for the English Speaking Audience. Aalborg. 2010

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The Experian data

The Experian data consists of approximately 1.7 million financial records in the period from 2000 to 2010. The timing of the records is based on the closing dates of the financial report periods. In case of companies filing multiple reports in a calendar year, only one of these is selected for the analysis. The closing date of the financial reports sets the time structure of the company-level analysis (which is relevant to before-after comparisons). When merging information from Statistics Denmark with the Experian data (such as information on the number of highly skilled employees), it is the latest available information in the Statistics Denmark registers before a given financial report's closing date which is used in association with the financial report in question.[10]

A first look at the data

As a point of departure, there are 416 VPs in the DASTI data. Six of these cannot be found in the registers that form the basis of the analysis, and there is no information on the highest educational degree of 16 individuals. Since education is a control variable of key importance for the analysis, these individuals are not included, leaving us with 394 individuals for the individual-level analysis. For 30 of these individuals, it has proven impossible to find highly similar controls. This implies that the individual-level analysis is based on 364 individuals who participated in the VP programme.

370 companies which have hosted VP projects can be found in the Experian database the year before the start of programme participation. The remaining companies not in the Experian data must be presumed to be unincorporated and thus not obliged to submit financial reports to the authorities. Companies can be followed until 2009 in the Statistics Denmark data and until 2011 in the Experian data. In the sample of companies employed for the subsequent analysis, the companies are observed over an average time span of 6.7 years.

The results of this report are based on DASTI's information on the company-VP matches. This is important to note, because the identification of hosting companies is not always straightforward: Single companies may have several CVR numbers, and there might be an element of randomness or selection regarding which CVR number hosting companies use to register their VP-projects. In approximately 30 percent of the projects, the Statistics Denmark data (described in greater detail below) suggest that the VP is employed at a company with a different CVR number than the one stated in the DASTI data.[11]

[10] Most companies have their closing date at the end of December, which implies a short time lag between the Statistics Denmark information (of end-November) and the financial report information. However, there are also companies that have chosen other dates, e.g. end of March, to close books. For these companies, the information from the Statistics Denmark registers comes with a time lag of up to one year.

[11] This will of course govern robustness checks of later findings. It might be noted that some of the companies that the Statistics Denmark data suggests are the 'real' hosts of the VP-projects do not fulfill the conditions for programme participation.

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For one of the extensions of the analysis, DASTI provided data on companies that have participated in the so-called *Innovation Networks*. These networks are collaborations of typically small and medium-sized companies with the purpose of increasing knowledge transfer and innovation. The data on Innovation Networks consist of 1923 observations belonging to 1158 companies, the discrepancy owing to the fact that a number of companies participate in these networks more than once. We only consider the earliest participation in any of these networks for the following analysis.

Of the 1158 firms that participated in any of the networks, 1121 are found in the Experian data. The discrepancy must again be assumed to be a result of non-incorporated firms.

General methodological issues

The empirical analysis addresses the basic evaluation problem: What is the causal effect of participation in the programme on given outcome variables?

In accordance with the relevant econometrical literature, which again borrows from the biometrics and epidemiological literature, programme participants will subsequently also be referred to as treatments. Also, starting to participate in the programme will also be referred to as receiving a treatment. Non-participants who act as a control group for the statistical comparisons will be referred to as controls.[12]

There are different ways of addressing the evaluation problem. One way is using a linear regression model. This model is estimated on a sample of both participating and non-participating individuals. The linear regression model includes a set of conditioning variables which hold constant a set of observable characteristics and identifies causal effects under a *conditional independence assumption*, by which participants do – on average – not differ from non-participants in characteristics that (a) have an impact on the outcome variables and (b) are not controlled for in the regression model.

These characteristics, sometimes called 'omitted variables', prohibit interpreting treatment-control differences in outcome variables as causal programme effects. Instead, they offer alternative interpretations of latter results. And the above 'identifying' conditional independence assumption is equivalent to assuming that there exists no other explanation for treatment-control differences in the outcome variables than the fact that treatments have participated in the programme.

Obviously, any empirical model supposed to isolate programme effects needs to maximise the validity of this assumption. A first step in this direction is to carefully select a control group for the analysis by a *matching procedure*. These procedures are explained in greater detail in the following sections. The procedures select one (or more than one) 'twin' or 'match' for each treatment. They imply that controls are highly similar to treatments in their observable characteristics, which also increases the likelihood that treatments and controls are highly similar in their unobservable characteristics.

Also, the way the dependent 'outcome' variable enters the model has implications for the validity of the conditional independence assumption. For example, statistical comparisons of individual-specific before-after developments over time or fixed effects models will typically be preferred to cross-sectional comparisons.

And as noted earlier, a set of conditioning variables can control for any systematic differences between the treatment and control group which might remain even if the controls were selected in a way to make them as similar as possible to the treatments in their observable characteristics.

[12] The term 'controls' is also sometimes used for the conditioning variables in statistical models. In this report, 'controls' refers to subjects in a reference group and not conditioning variables.

Selection of controls

Obviously, the validity of any statistical comparison can be questioned if treatments (individuals or companies) systematically differ from the controls in characteristics that the subsequent regression model is unable to take fully into account. We want to select individuals into the control groups that are as similar as possible to the treatments in the most dimensions possible. The problem of finding 'good' matches is that there are no two absolutely identical individuals, so it should be acknowledged that any analysis that identifies controls on the basis of a matching procedure is nothing but a sophisticated comparison that requires additional all-else-equal assumptions for causal interpretation.

The controls can be selected by a host of different matching procedures developed over the last decades. Overviews of these procedures are found in *Caliendo and Kopeinig, 2008*, and *Blundell and Costa Dias, 2009*.[13] The basic idea is to find for each participant one or more 'twins' that are as similar to the given participant as possible, and to use these matches as the analysis' control group.

The specific matching procedure depends on the nature of the data. The modeller typically chooses between *matching on observables* and *propensity score matching*, or some combination of the two.

Matching on observables simply means that for each treatment, one or more 'twins' (referred to as matches in the following) are selected from the group of potential controls that have the same observable characteristics in a number of dimensions. For example, one could choose for each participating VP one control individual with the same education, gender, and stays in the same geographic region. For companies, one could select controls on the basis of industry, size, financial performance measures, and other characteristics.

When treatments are not particularly unique and there are a lot of potential candidates in the pool of potential controls, matching on observables might be the preferred choice. But, matching on observables runs into a *multidimensionality problem* when one uses too many observable characteristics as conditions in the procedure: It becomes impossible to find controls for all participants when they are required to be equal in too many dimensions.

Of course, one way of "solving" this problem would be to disregard a lot of information in the data and only require equality in a few observable characteristics. In this case one could, for each treatment, select one or more controls from the pool of potential controls that are equal in a few dimensions (or use the entire population of potential controls as controls and weigh them in the subsequent regressions).

[13] Blundell, R., Costa Dias, M., 2009. Alternative Approaches to Evaluation in Empirical Microeconomics. Journal of Human Resources 44(3., 565-640.

Caliendo, M., S. Kopeinig, 2008. Some Practical Guidance for the Implementation of Propensity Score Matching, Journal of Economic Surveys (2008) Vol. 22, No. 1, pp. 31–72.

Yet another option is to combine the benefits of the matching-on-observables-procedure with the benefits of the *propensity-score-matching-procedure*. The latter method has the benefit of allowing the use of vastly more information than the matching-on-observables method: It condensates all variables which might be considered relevant for the choice of programme participation into one single metric. This is simply the estimated predicted probability of programme participation, called the *propensity score*.[14] This way, it is possible to find matches that are most similar in terms of the propensity score instead of a set of observable characteristics.

The number of matches selected for each participant is set by the modeller, who faces a trade-off between bias and efficiency: By including many matches for each participant into the control group, the sample size is increased and the variance of the subsequent estimators is reduced. However, increasing the number of matches for each participant might lead to selecting subjects into the control group that are not very similar to the treatment. This decreases the validity of the conditional independence assumption. So there is a trade-off between the precision of the statistical estimates and minimizing the risk of matching participants with controls that differ in observed and unobserved characteristics.

Empirical specification

The empirical implementation is done in the following steps: (i) select a group of controls, (ii) specify the regression model.

For the individual-level analysis, the selection of controls is from the registers of Statistics Denmark, which contain information on the entire Danish population, and is carried out in three steps:

First, we adjust the sample of potential controls. This is achieved by deleting individuals with characteristics not found for any VP. For example, we drop individuals with educations that no single VP has taken, and younger than the youngest VP in our sample. The resulting data is referred to as the *adjusted individual-level sample*.

Second, we calculate a probability model for the likelihood of VP programme participation for any given individual. This model provides evidence of which individual characteristics are associated with programme participation, which might be interesting in its own right. It is also used to calculate the propensity score for each individual in the data and for each year, which is simply the predicted participation probability for the given individual in the given year.

[14] Rosenbaum, P.R., and D. Rubin (1983). The central role of the propensity score in observational studies for causal effects. Biometrika (1983) 70(1): 41-55.

The conditioning variables of the propensity score matching procedure are selected in cooperation with DASTI and include all variables potentially important for programme participation and available in the data. The list consists of factors such as demographic information (age, gender, marital status), information on education, including 15 education categories, whether the individual is currently in any education programme, the average grade of the final secondary education examination, and focus of secondary education electives (math, language). There are also occupational codes (17 categories including unemployment and leave), wage income (9 categories), labour market experience (5 categories), and geographical location of residence (9 categories).

For programme participants, these characteristics are collected for the year before treatment, called '*year 0*' or *t=0* in the following. This ensures that no information affected by treatment enters the propensity score model.

Finally, we apply a single nearest-matching procedure (by employing STATA Corp.'s psmatch2-command) on the basis of the probability model's predicted propensity scores (participation probabilities). In this procedure we also impose the condition that twins are exactly equal in terms of education (approximately 2,200 different categories in total and approximately 175 different categories for VPs), gender, occupation (11 categories) and highly similar in age. Again, all information entering the matching procedure is from the year prior to programme participation. We strive for minimum bias of the later estimators and choose only one control (instead of several controls) for each treatment.

For the following treatment-control analysis, it is necessary to define a year 0 (t=0) for controls just as has been done for treatments. This allows modelling the dynamics of potential treatment effects in association with programme participation. For controls, year 0 or t=0 is simply the year in which a given control is selected into the control group. This is the year in which the given individual is most similar to its twin in terms of observable characteristics and propensity score.

The following comparisons over time will be relative to year 0 instead of calendar time. E.g. for treatments, t=2 is two years after the year before treatment (i.e., one year after the start of programme treatment). For controls, t=2 is two years after being selected into the control group.

Individual-level analysis: the regression model

The individual-level analysis is carried out using separate multivariate regressions. We consider the following success parameters:

(a) Whether or not the individual is employed in *t=1, t=2, ..., t=5*, implemented by indicator (dummy) variables.

(b) The increase in wage income (salary) between year 0 and year *t=1, t=2, ..., t=5*.

The success parameters are regressed on a treatment dummy (taking the value 1 for treatments and 0 for controls) and the following conditioning variables: age, gender, experience, average grade of final secondary education (high school) examination, occupation in year 0, the sum of the Statistics Denmarks unemployment index (measuring the aggregated time an individual has been registered as being unemployed).

Individual-level analysis: descriptive statistics

394 individuals who have participated in the VP programme can be found in the Statistics Denmark registers. Of these, 364 can be associated with controls equal or similar in the dimensions described in the previous section. These 364 individuals form the basis for the subsequent analysis. TABLE 4.1 describes the adjusted individual-level sample (the total pool of available controls), the sample of VPs, and the samples of VPs and controls used for the subsequent analysis.[15]

[15] The variable on whether or not a person is in education at a given point in time is from Statistics Denmark's education registers, while the variable of having education as one's occupation is from Statistics Denmark's education occupation classifications (pstill). Individuals who work while studying are classified as under education in the education registers and as working in the occupation information.

TABLE 4.1: Individual-level characteristics								
	Adjusted sample excluding VPs (N = 1.018.245)		Treatment group (N=394)		Analysis sample, Treatments (N=364)		Analysis sample, Controls (N=364)	
Variable	Mean	Std. dev	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
General information								
Age (years)	37.176	11.358	34.226	9.380	34.162	9.426	34.110	9.627
Female	0.401	0.490	0.419	0.494	0.409	0.492	0.409	0.492
Experience (years, since 1980)	10.483	8.169	6.104	6.669	6.203	6.811	6.332	6.685
Average grade, secondary education (high school)	84.304	9.173	84.265	8.304	84.354	8.279	84.511	8.833
Average wage (DKK)	300574	199968	171721	207544	178437	212787	183930	197212
Years of registered unemployment	1.149	2.005	1.241	1.979	1.170	1.898	1.163	2.076
Married	0.487	0.500	0.411	0.493	0.429	0.496	0.412	0.493
In education	0.137	0.344	0.398	0.490	0.401	0.491	0.393	0.489
Post-secondary or tertiary education	0.588	0.492	0.807	0.395	0.805	0.397	0.805	0.397
Education: arts and humanities	0.142	0.349	0.183	0.387	0.181	0.386	0.181	0.386
Education: social sciences	0.273	0.445	0.274	0.447	0.288	0.454	0.288	0.454
Education: technical sciences	0.253	0.434	0.355	0.479	0.346	0.476	0.346	0.476
Secondary education, elective direction: no information	0.606	0.489	0.330	0.471	0.332	0.472	0.363	0.481
Secondary education, elective direction: general	0.193	0.395	0.231	0.422	0.231	0.422	0.187	0.390
Secondary education, elective direction: math	0.125	0.331	0.226	0.419	0.223	0.417	0.245	0.430
Secondary education, elective direction: languages	0.041	0.198	0.157	0.365	0.157	0.364	0.151	0.359

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Region of residence								
Copenhagen	0.312	0.463	0.223	0.417	0.217	0.413	0.247	0.432
Zealand N	0.112	0.315	0.056	0.230	0.049	0.217	0.055	0.228
Zealand S	0.076	0.264	0.079	0.270	0.082	0.275	0.077	0.267
Funen, Bornholm	0.079	0.269	0.157	0.365	0.162	0.369	0.135	0.342
Jutland S	0.065	0.247	0.046	0.209	0.049	0.217	0.052	0.223
Jutland W	0.095	0.294	0.063	0.244	0.063	0.244	0.058	0.233
Jutland E	0.177	0.381	0.231	0.422	0.234	0.424	0.277	0.448
Jutland N	0.080	0.271	0.142	0.350	0.140	0.348	0.099	0.299
Region not specified	0.005	0.071	0.003	0.050	0.003	0.052		
Occupation (from Statistics Denmark's variable 'pstill')								
Self-employed	0.000	0.015	0.003	0.050				
Manager	0.031	0.173	0.025	0.157	0.025	0.156	0.025	0.156
Employee, high level	0.324	0.468	0.259	0.439	0.277	0.448	0.277	0.448
Employee, medium level	0.123	0.328	0.074	0.261	0.077	0.267	0.077	0.267
Employee, basis level	0.227	0.419	0.099	0.299	0.102	0.303	0.102	0.303
Employee, other	0.055	0.228	0.030	0.172	0.022	0.147	0.022	0.147
Employee, no further information	0.103	0.305	0.063	0.244	0.069	0.253	0.069	0.253

Unemployed	0.036	0.187	0.241	0.428	0.228	0.420	0.228	0.420
On parental leave	0.003	0.051	0.013	0.112	0.008	0.091	0.014	0.117
On sickness pay	0.001	0.036	0.005	0.071	0.005	0.074	0.003	0.052
Non-salaried worker	0.003	0.056	0.018	0.132	0.019	0.138	0.005	0.074
Education measure	0.007	0.082	0.030	0.172	0.027	0.164	0.038	0.193
In job market training	0.003	0.055	0.005	0.071	0.003	0.052	0.003	0.052
On social benefits ("revalidering")	0.001	0.035	0.003	0.050	0.003	0.052	0.005	0.074
Unknown	0.000	0.020	0.008	0.087	0.008	0.091	0.003	0.052
Outside labour force, other	0.015	0.122	0.033	0.179	0.036	0.186	0.038	0.193
In education	0.031	0.173	0.058	0.235	0.060	0.239	0.060	0.239
Year								
2005	0.436	0.496	0.234	0.424	0.225	0.418	0.225	0.418
2006	0.080	0.272	0.142	0.350	0.137	0.345	0.137	0.345
2007	0.082	0.274	0.152	0.360	0.148	0.356	0.148	0.356
2008	0.077	0.266	0.140	0.347	0.143	0.350	0.143	0.350
2009	0.122	0.327	0.152	0.360	0.162	0.369	0.162	0.369

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We find that individuals who participate in the scheme are represented among all occupations, age groups and income levels. There is no gender bias in programme participation. However, many VPs are relatively young, are unemployed or recent higher education graduates, have sparse labour market experience and low income.

A more systematic way of describing the propensity of programme participation is to estimate a binary choice model. The results of this model (specified as a logit model) are shown in Table 4.2 which displays a selection of coefficient estimates. Note that this model is also the backbone of the matching procedure used to identify one matched control for each programme participant.

A look at the estimates of the individual-level logit model reveals that they by and large corroborate the findings of the mean comparisons of Table 4.2: Individuals participating in the programme are often relatively young, there are regional differences, they are not characterised by high or low secondary education grades, and they have high unemployment rates and low salary incomes. When controlling for these characteristics, labour market experience (as long as it is positive) does not come out as an important explanatory factor with regard to programme participation.

The matching procedure finds controls for 364 of the total 394 participants in the adjusted individual sample. The remaining 30 participants remain unmatched because no other individual in the adjusted individual sample (the total pool of available controls) could be found who was equal to these individuals in the dimensions of education, gender, occupation and age.

The matched sample of treatments and controls can be compared by referring to the right hand side columns of TABLE 4.1 and 4.2. We conclude that the matching procedure succeeded in finding a group of controls highly similar to the group of participants. This allows us to analyse treatment-control differences in the success factors associated with programme participation in the following section.

Tabel 4.2: Individual-level analysis. Logit estimation results. Dependent variable: Individual participates in the VP-programme in the following year. Selected coefficients.

	Adjusted sample		Sample of treatments and controls	
	N=1,018,245, LR chi2(78) =1129.19, Pseudo R2 = 0.1618		N=728, LR chi2(76)= 25,57, R2=0.026	
	Coeff.	Ste.	Coeff.	Ste.
General information				
Female	-0.122	0.114	-0.065	0.192
Married	-0.004	0.111	0.078	0.173
In education	-0.050	0.181	0.009	0.310
Age (in years, omitted: <25 years)				
(25-29)	0.750***	0.216	0.187	0.351
(30-34)	0.717***	0.256	0.304	0.427
(35-39)	0.611**	0.301	0.316	0.503
(40-44)	0.441	0.339	0.456	0.609
(45-49)	0.758**	0.352	0.548	0.617
(50+)	0.024	0.359	0.688	0.610
Region of residence (omitted: Copenhagen)				
Zealand N	0.325	0.243	0.142	0.406
Zealand S	1.446***	0.216	0.242	0.344
Funen, Bornholm	1.436***	0.171	0.372	0.273
Jutland S	1.018***	0.265	0.176	0.402
Jutland W	0.784***	0.233	0.298	0.386
Jutland E	0.775***	0.151	-0.063	0.237
Jutland N	1.269***	0.176	0.516	0.302
Region not specified	-0.079	1.007		

Secondary education final grade average (omitted group: unknown)				
(0-75)	0.064	0.205	0.281	0.324
(76-85)	0.242	0.163	0.308	0.257
(86-90)	-0.227	0.210	0.502	0.350
(90+)	0.011	0.190	0.332	0.301
Occupation (from Statistics Denmark's 'pstill' variable, omitted: pstill-category 12 ('VAT-payer'))				
Self-employed (pstill=14)	1.976*	1.051		
Manager	0.774	0.475	-0.047	0.794
Employee, high level	0.270	0.348	-0.090	0.557
Employee, medium level	0.159	0.384	-0.063	0.602
Employee, basis level	0.192	0.366	-0.040	0.597
Employee, other	0.702	0.438	-0.058	0.771
Employee, no further information	0.372	0.379	-0.016	0.621
Unemployed	1.998***	0.321	-0.160	0.541
On parental leave	1.219**	0.543	-0.604	0.923
On sickness pay	1.681**	0.773	0.474	1.367
Non-salaried worker	2.551***	0.487	0.804	0.981
Undergoing education measure	1.909***	0.418	-0.540	0.678
In job market training	2.808***	0.785	-0.265	1.582
On social benefits ("revalidering")	1.292	1.063	-0.949	1.410
Unknown (pstill=57)	2.645***	0.663	0.842	1.298
Outside labour force, other	0.876**	0.398	-0.311	0.633
In education	0.415	0.378	-0.095	0.606

Salary (omitted: no information)				
0-0.15% of sample mean	0.249	0.183	0.087	0.298
15-25% of sample mean	0.472**	0.219	0.265	0.352
25-50% of sample mean	0.275	0.201	-0.260	0.317
50-75% of sample mean	-0.828***	0.287	-0.033	0.450
75-100% of sample mean	-0.784***	0.275	0.105	0.446
100-125% of sample mean	-1.193***	0.287	-0.244	0.430
125-150% of sample mean	-1.379***	0.302	-0.115	0.444
150-200% of sample mean	-1.527***	0.313	0.027	0.480
200%+ of sample mean	-1.741***	0.408	-0.398	0.604

Notes: *, **, *** denote statistical significance at the 10, 5, and 1% level. Additional variables included in the regressions, but not presented in this table are: education (15 categories), experience (five categories), high school average grade (five categories), unemployment experience index (variable 'sumgrad', six categories).

Individual-level analysis: Results

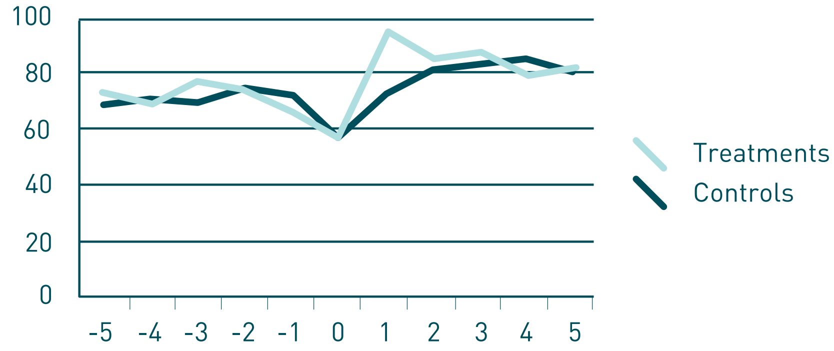
This section presents treatment-control differences in the outcome variables wage income (Statistics Denmark variable *slon*) and employment (Statistics Denmark variable *pstill* with a value of less than *40*). Results are based on descriptive graphs and estimations with conditioning variables taking any remaining treatment-control differences into account. All conditioning variables are from t=0, i.e. they are collected in the year before treatment or, in the case of controls, the year of selection into the control group.

When interpreting results, it might be kept in mind that the available data suggest that long-term employment relationships for VPs in their hosting companies are not very common. For example, 69 VPs were hired in 2005 with the VP-company match confirmed by the Statistics Denmark data. Of these employment relationships, 53 (77 percent) were terminated within three years. For the employment relationships started in 2005 and 2006, 71 percent were terminated within two years.[16]

Potential employment effects

In the following, employment rates of VPs are compared with the employment rates of individuals in the control group. Employment is measured by the Statistics Denmark variable ‘pstill’ assuming a value of less than 40.[17] Note that this variable is conditioned on when controls were selected into the control group. As a consequence, employment rates are exactly equal for the two groups of individuals in year 0 (t=0).

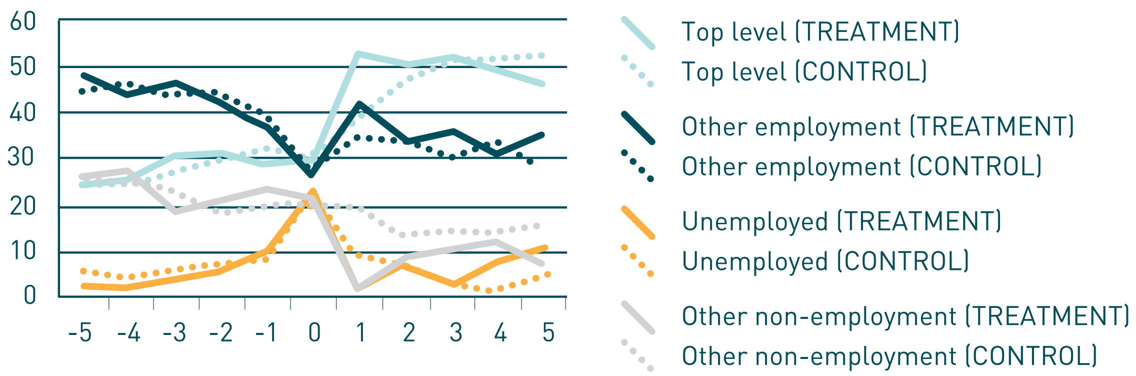
FIGURE 4.1: Share of treatments and controls in an employment relationship (’pstill’<40). By year after year 0 (on horizontal axis)



[16] In this project’s vintage of the Statistics Denmark data, the individual-company-match can only be followed until the year 2008, preventing us from following individual-company relationships over longer time periods or in more recent VP-projects.

[17] Individuals on leave are not counted as employed.

FIGURE 4.2: Share of treatments and controls, by occupation. By year after year 0 (on horizontal axis)



A first look at the data, see FIGURE 4.1, suggests that VPs are characterised by decreasing employment rates in the years before treatment. But in association with treatment, the employment rate increases to almost 100 percent. This is not surprising, since employment is a defining characteristic of the programme. This increase is not matched by the control group's development in year t=1. However, employment rates of the two groups converge over time and are at the same level two years after treatment.

FIGURE 4.2 splits up developments in occupation status by *top level employment (pstill<33), other employment (33<pstill<39), unemployment (pstill=40)*, and other *non-employment (pstill>40)*. Here, it is found that treatments and controls are characterised by highly similar developments in these variables in the years before year 0, suggesting that the matching procedure has been successful. The graph further suggests that in year 0, a number of individuals in the two groups are finishing education or have left employment in the year prior to treatment or being selected into the control group. After treatment, a large share of treatments are categorised as top level employees, while controls pick up and have the same shares of individuals in this category after approximately two to three years.

Employment probabilities are more formally analysed by means of simple binary choice logit models, with 'the individual is employed' at t=x, x=1,2...5 being the dependent variable, where t=0 is the year before treatment, t=1 is the year in which treatment takes place, etc. Estimation is by separate binary choice models for each t=x, x=1,2...5.

Table 4.3 displays the results. The coefficient of interest is the one associated with the treatment dummy 'Treatment=1".

We find a substantial potential programme effect on employment, as a coefficient of 2.361 implies an increase in the odds ratio of being employed by factor (exp(2.18)=) 10.5. This large increase comes as no surprise, given that employment is a defining characteristic of the programme, and given that we already had seen that employment is close to 100 percent for treatments in the year after the start of programme participation.[18]

Potential programme effects for the years following programme participation are a (exp(0.271)=) factor 1.3 increase in employment probability in year t=2, which is not significantly different from zero, and a factor 1.9 increase in year t=3, which just fails to be significant at the ten percent level. After more than three years after the start of participation, the signs of the coefficients switch around zero and become insignificant.

For the most part, the remaining variables come out as insignificant. The exception is low-wage individuals and individuals unemployed in year 0, who have the lowest probability of being employed in subsequent years.

We conclude that overall results indicate a presence of potential short-run employment effects and an absence of potential long-term effects of the programme. However, it should be noted that most of the observation period is from a boom period with high labour demand in the Danish economy. This implies that non-participants cannot be assumed to catch up to the same extent in current years compared to the analysis period.

[18] The numbers of observations of the estimations are reduced by the fact that some of the explanatory variables completely determine the outcome variables. As a robustness check, the models for employment and salary developments were estimated without explanatory variables. This did not change the overall results in any significant way.

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TABLE 4.3: Logit binary choice model results. Dependent variable: The individual is employed in t=x.

	Dependent variable: The individual is employed in t=1		Dependent variable: The individual is employed in t=2	
	Coeff.	Ste.	Coeff.	Ste.
Treatment=1	2.365***	0.350	0.271	0.293
Age (years)	-0.0669**	0.030	-0.011	0.029
Female	-0.360	0.300	-0.399	0.308
Annual wage (DKK 1000)	0.004***	0.001	0.006***	0.002
(Years of unemployment up to t=0)*1000	50.480	74.080	-174.5**	78.900
Year of experience since 1980	0.000	0.037	-0.012	0.040
Married	0.300	0.318	0.257	0.349
Secondary education, no information	1.785	2.156	0.145	2.069
Secondary education, elective direction: math	-0.580	0.508	-0.390	0.605
Secondary education, elective direction: languages	-0.461	0.590	-1.380**	0.586
Secondary education: hf ("higher preparation")	0.309	0.706	-1.492**	0.682
Secondary education: average grade	0.028	0.026	0.017	0.025

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Dependent variable: The individual is employed in t=3		Dependent variable: The individual is employed in t=4		Dependent variable: The individual is employed in t=5	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
0.577	0.354	-0.423	0.408	0.281	0.716
-0.031	0.036	-0.033	0.035	-0.104	0.120
-0.179	0.392	0.744	0.538	0.961	1.078
0.002	0.002	-0.001	0.002	0.007	0.004
-147.100	101.000	-114.500	96.880	-343.500	317.200
0.017	0.045	0.035	0.047	0.229*	0.135
-0.304	0.418	0.384	0.438	0.696	0.881
3.091	2.413	5.429*	2.910	7.224	6.034
0.837	0.753	0.010	0.782	2.769*	1.507
-0.977	0.667	-1.491*	0.837	1.392	1.025
0.725	1.207	-0.451	1.012	-0.051	1.343
0.042	0.029	0.0759**	0.038	0.043	0.071

Occupation				
Top level (pstill=31, 32, omitted category)				
Employee, medium level (pstill=34)	0.854	1.000	-1.268*	0.753
Employee, basic level (pstill 35)	-0.676	0.627	-0.788	0.578
Employee, other (pstill=36)				
Salaried employee, no further information (pstill=37)	-0.581	0.683	-0.429	0.647
Unemployed	-1.480***	0.531	-0.382	0.559
In education	-1.295*	0.696	-1.040	0.802
Self-employed				
On leave, and other non-employed	-1.137*	0.601	-0.415	0.655
Immigrant status: not an immigrant (omitted category)				
Immigrant status: first generation	-0.702	0.639	-0.099	0.616
Year: 2005	-0.071	0.397	0.396	0.398
Year: 2006	-0.375	0.489	0.766	0.513
Year: 2007	-0.655	0.477	-0.447	0.395
Year: 2008	-0.722	0.491		
Region: North Jutland	0.302	0.476	-0.149	0.414
Constant	1.967	2.369	1.701	2.341
Number of observations	568		486	
	596		492	
R-squared	0.28		0.37	

0.732	1.268	-0.348	0.888		
-0.466	0.662	-1.219	0.835	-2.046*	1.102
-0.110	0.765	0.094	1.078	0.476	1.728
-0.380	0.638	-0.805	0.743	-1.621	1.104
-0.392	1.233	-1.738	1.166		
-1.106	0.745	-0.892	0.915	0.618	1.723
-0.771	0.614	-0.101	0.904	-0.393	1.149
-0.065	0.461	-0.552	0.458		
-0.295	0.456				
-0.202	0.585	-1.178**	0.515	0.936	1.017
0.109	2.576	-1.334	2.900	-1.156	7.844
383		286		119	
386		293		129	
0.38		0.48		0.57	

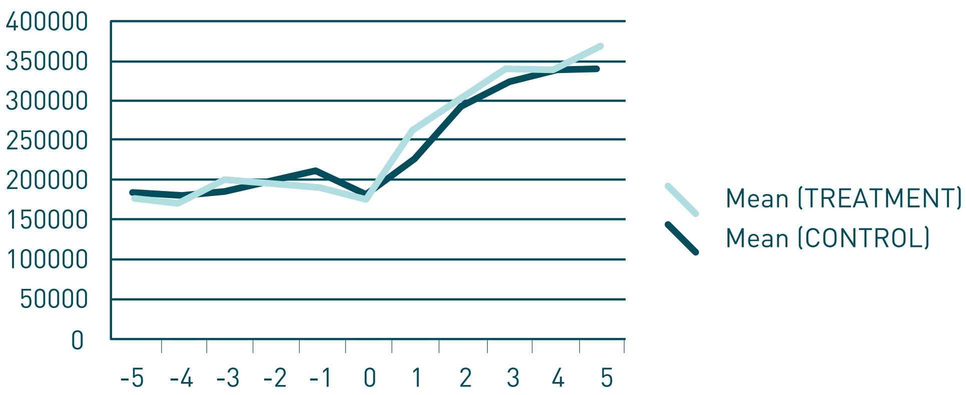
Notes: *, **, *** denote statistical significance at the 10%, 5%, 1% significance level. All monetary values are CPI-adjusted to base year 2009.

Potential earnings effects

FIGURE 4.3 looks at the average salary developments (measured by the Statistics Denmark variable 'slon') of treatments and controls. We find that earnings profiles are highly similar for treatments and controls before year 0, and that VPs on average experience increasing salaries in association with programme participation. These increases are higher for treatments than controls. However, after two to three years after year 0, developments converge and individuals in the control group are doing as well as participants.[19]

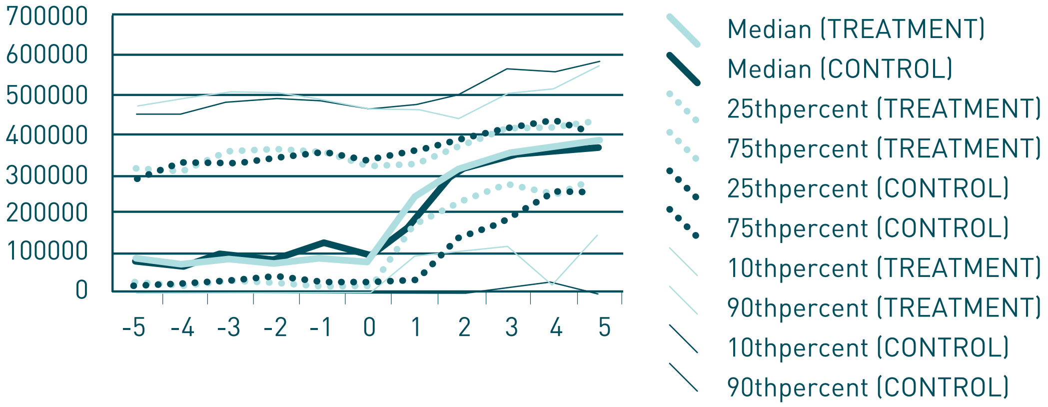
A look at the dynamics of the salary distributions (instead of the means) in FIGURE 4.4 suggests that this increase is driven by VPs with low salaries in year 0. VPs in the bottom 25th percentile of the salary distribution in year 0 experience the largest salary increases in association with programme participation, which might be presumed to be a result of these individuals entering an employment relationship in association with the programme. On the other hand, there are fewer VPs with very high salaries after year 0 than is the case for controls.

FIGURE 4.3: Salary developments of treatments and controls, in DKK. Means. By years after year 0 (on horizontal axis)



[19] The estimations behind TABLE 4.3 are based on the total sample of treatments and controls except for individuals who experience extreme changes in their annual salaries (e.g. increases of more than DKK400,000 between year 0 and year 1 or more than DKK1,000,000 between year 0 and t=5). See TABLE 4.6 for results on a sample including these individuals.

FIGURE 4.4: Salary developments of treatments and controls, in DKK. Distribution parameters. By year after year 0 (on horizontal axis)



The graphs suggest positive potential programme effects on salary in the years after treatment and an absence of long-run effects. TABLE 4.4 considers these potential effects in a more stringent way by means of a *conditional diff-in-diff* model. The parameters of interest are again those associated with the variable 'Treatment=1' that measures the potential programme effect on income for participating individuals.

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TABLE 4.4: Linear regression results. Dependent variable: Salary ('slon') increase between t=0 and t=x, in DKK.

	Dependent variable: salary increase between t= 0 and t=1		Dependent variable: salary increase between t=0 and t=2	
	Coeff.	Ste.	Coeff.	Ste.
Treatment=1	56456***	8534	21773*	12193
Age	-2319**	942	-3131***	1156
Female	-18141*	10183	-16614	12261
Annual wage (DKK 1000)	-0.258***	0.04	-0.516***	0.05
(Years of unemployment before t=0)*1000	0.55	2.63	-10.52**	4.19
Years of experience since 1980	303	1308	4966***	1510
Married	2766	9953	11503	13036
Secondary education, no information	35814	53968	-38180	70676
Secondary education, elective direction: math	-9863	14459	-1468	20176
Secondary education, elective direction: languages	-17159	16908	-20279	21190
Secondary education: hf ("higher preparation")	-33721*	19976	-43647	28839
Secondary education: average grade	500	638	-329	855
Occupation				
Top level management (pstill=31, omitted category)				
Employee, high level (pstill=32)	-28418	35165	-88562**	37002
Employee, medium level (pstill=34)	-20162	36887	-120011***	41621

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Dependent variable: salary increase between t=0 and t=3		Dependent variable: salary increase between t=0 and t=4		Dependent variable: salary increase between t=0 and t=5	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
25310*	14264	4721	18928	42096	34214
-5543***	1516	-6457***	2207	-8382**	3882
-33214**	15574	-41379**	19401	-43810	35452
-0.541***	0.06	-0.734***	0.10	-0.748***	0.19
-9.339*	5.61	-18.16**	7.54	-23.13*	12.00
5848***	2092	7111**	3105	6018	6024
489	15552	-34760	21773	-32646	32643
-106950	90991	36331	129246	-224476	224830
6464	22935	14275	28555	-10233	59345
-14886	29109	-31307	33005	-73838	67547
-16760	27138	-8739	24743	-6107	95472
-1126	1080	365	1447	-3405	2768
-58624	57540	-17487	58814	-46003	51369
-104792*	59370	-88445	67119	-334935***	57211

Employee, basic level (pstill=35)	-34889	38840	-111962***	41296
Employee, other (pstill=36)	-53589	40962	-179786***	49799
Salaried employee, no further information (pstill=37)	-59833	38815	-140168***	41893
Unemployed	-35792	38724	-91996**	43113
In education	-52257	38788	-157389***	47033
Self-employed	-98106**	43751	-216754***	60192
On leave, and other non-employed	-25577	40412	-64130	46789
Immigrant status: not an immigrant (omitted category)				
Immigrant status: first generation	-24764	20393	13383	25690
Immigrant status: second generation	18391	24869	106801***	20603
Year: 2005	28159**	12322	1196	13384
Year: 2006	24741*	14035	16645	16282
Year: 2007	41843***	14572	-8490	16329
Year: 2008	-15582	16977		
Region: North Jutland	-4876	14430	5762	15799
Constant	160822**	66487	432927***	86265
Number of observations	596		492	
R-squared	0.28		0.37	

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-96960	61138	-93042	65682	-135173*	75067
-141922*	73440	-62744	73003	-116249*	61842
-98519	63464	-58705	70012	-82539	95957
-62057	62101	-76378	68055	-94949	73276
-77928	66937	-82241	73711	-92145	78694
-247578***	78454	-264535***	93911	-75841	78306
-15111	66071	28299	71073	-61067	87232
34296	28970	21333	42702	-16168	57123
39432	72979	-34498	99428	-141338*	73137
12203	16679	-23334	19795		
15943	19477				
-14781	22487	-2496	28343	-2139	43334
576595***	115808	552475***	148468	963247***	266452
386		293		129	
0.38		0.48		0.57	

Notes: *, **, *** denote statistical significance at the 10%, 5%, 1% significance level. All monetary values are CPI-adjusted to base year 2009.

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Although potential employment effects are restricted to earlier employment for VPs, we find that potential salary effects are slightly more persistent, as coefficients come out statistically significant (albeit only at the ten percent level) for time leads of up to three years. TABLE 4.4 also allows calculating the total potential programme effect as the sum of the coefficient estimates. This is approximately DKK150,000 for the total sample of all treatments and controls, a number which might be related to the average cost of the programme.

Individual-level potential effects for different subsamples

As an extension of the previous analysis, the sample of VPs and associated controls is split up by a number of project-specific and VP-specific background characteristics. In particular, the following distinguishes between whether or not the VP-project was completed or terminated before schedule. The sample is also split up by the industrial sector of the companies that hire the VPs or the associated controls, and the education and gender of the VP and the associated controls.

Findings of the estimations on the subsamples are found in TABLE 4.5 for employment and 4.6 for salary increases. These tables are based on the same models that were estimated earlier, but only report the relevant coefficients associated with the treatment dummy variables.

It is found that that there is little heterogeneity in the estimated potential effects of the programme.[20] Only completed projects are associated with larger increases in employment. This indicates that uncompleted projects are not just aborted because of the VP moving to another employment relationship, but becoming unemployed. This is also reflected in the absence of any measurable potential salary effect for this group of individuals.

It is only possible to detect statistically significant potential employment effects in the year after treatment (t=2) for VPs with a technical sciences education. It is possible to detect positive potential salary effects in the years after treatment only for female VPs, VP-projects in ‘other industries’, and completed projects.

Although single coefficient estimates are in most cases not statistically significantly different from zero, the sum of the estimates of TABLE 4.6 are still the best guesses of any potential salary effects over the first five years after treatment. These potential effects are largest for female VPs and VPs who are employed in service industries, and lowest for VPs with degrees in arts and humanities or technical sciences, and VPs with a tertiary education.

[20] For a couple of estimations, not all coefficients could be estimated because of low variation in the data relative to the number of observations and the number of conditioning variables.

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TABLE 4.5: Linear regression results. Dependent variable: The individual is employed in t=x. By subsamples. Results for treatment dummy variables				
	Dependent variable: the individual is employed in t=1		Dependent variable: the individual is employed in t=2	
	Coeff.	Ste.	Coeff.	Ste.
All projects	2.365***	0.350	0.271	0.293
N	568		486	
Only completed projects	3.138***	0.471	0.507	0.346
N	449		377	
Only not completed projects	1.435	0.942	-0.358	0.810
N	99		87	
Manufacturing and construction			1.08	0.81
N			128	
Services			0.874	1.044
N			88	
Other industries			3.079*	1.864
N			122	
Males	2.371***	0.498	0.438	0.468
N	328		182	
Females	2.635***	0.551	0.072	0.439
N	212		213	
Tertiary-level education	2.264***	0.395	0.252	0.342
N	405		387	
Education in arts & humanities	2.008**	0.870	-0.086	0.786
N	116		98	
Education in social sciences	3.960**	1.935	-0.438	0.809
N	70		79	
Education in technical sciences	2.948***	0.717	1.182*	0.627
N	183		160	

Dependent variable: the individual is employed in t=3		Dependent variable: the individual is employed in t=4		Dependent variable: the individual is employed in t=5	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
0.577	0.354	-0.423	0.408	0.281	0.716
383		286		119	
1.019**	0.416	-0.423	0.460	-1.603	1.686
309		246		94	
1.45	0.98	-0.34	0.65		
115		94			
6.551	4.068	6.029***	2.109		
77		36			
0.796	1.150	-2.387*	1.390		
115		67			
0.160	0.502	-0.619	0.525	0.393	0.926
223		177		68	
1.406**	0.717	0.343	0.732		
131		89			
0.280	0.424	-0.451	0.485	-0.528	0.927
308		206		95	
1.108	1.292				
44					
-0.641	0.783	-1.234*	0.728		
108		99			

Notes: *, **, *** denote statistical significance at the 10%, 5%, 1% significance level.

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TABLE 4.6: Linear regression results. Dependent variable: Salary ('slon') increase between t=0 and t=x, in DKK. By subsamples. Results for treatment dummy variables

	Dependent variable: salary increase between t=0 and t=1		Dependent variable: salary increase between t=0 and =2	
	Coeff.	Ste.	Coeff.	Ste.
All projects	56456***	8534	21773*	12193
N	596		492	
All projects, including outliers	48137***	9685	11877	13440
N	605		501	
Only completed projects	69542***	9659	33476**	13573
N	467		381	
Only not completed projects	8866	19561	-8478	29591
N	129		111	
Manufacturing and construction	23180	18829	14164	30675
N	136		125	
Services	67198***	17814	47811	30202
N	170		90	
Other industries	79337***	16158	69038***	22550
N	156		149	
Males	49065***	11794	11342	17030
N	349		277	
Females	63790***	13465	34185*	18891
N	247		215	
Tertiary-level education	55809***	9855	16325	14205
N	468		391	
Education in arts & humanities	43389*	22664	13979	30402
N	116		98	
Education in social sciences	44390**	18527	10731	30264
N	161		137	
Education in technical sciences	59315***	15017	5266	21677
N	205		168	

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Dependent variable: salary increase between t=0 and t=3		Dependent variable: salary increase between t=0 and t=4		Dependent variable: salary increase between t=0 and t=5		Aggregated differences from t=1 to t=5
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	
25310*	14264	4721	18928	42096	34214	150356
386		293		129		
24982	15445	4721	18928	42096	34214	131813
393		293		129		
29125*	16547	9146	-21466	25853	39825	167142
311		248		106		
22334	33461	24120	56060	123197	103104	170039
75		45		23		
44672*	26317	11914	41021	19104	90804	113034
126		97		40		
-10601	38463	48873	39917	123624	135240	276905
89		64		26		
26789	25844	-7511	32362	13542	47681	181195
149		118		56		
19414	19759	-6218	26843	60274	53426	133877
232		181		79		
36790	22989	29105	28294	66325	44998	230195
154		112		50		
14992	16568	-7062	22172	20373	39581	100437
314		233		107		
-15664	43284	-53186	43560	-30421	169878	-41903
72		41		27		
3897	30019	4058	42777	61546	67346	124622
108		87		40		
12788	26287	-18634	34162	51668	87173	110403
144		115		45		

Notes: *, **, *** denote statistical significance at the 10%, 5%, 1% significance level. All monetary values are CPI-adjusted to base year 2009.

5. COMPANY-LEVEL ANALYSIS >

In the following, the setup and results of the company-level analysis are described. We briefly describe the model which aims at removing as much unobserved heterogeneity as possible from the statistical comparisons. We then take a look at the company-level data and inspect the sample for the subsequent analysis. Finally, we compare companies that participate (receive a *treatment*) in the programme ('*treatments*' or '*participants*' in the following) with highly similar companies that act as a control group. In particular, we compare developments in:

1. the number of highly educated employees
2. the number of employees
3. value added
4. net income (profit) and return on assets
5. average wage cost
6. labour productivity, measured as turnover per employee

The analysis addresses the question of how VP-companies perform in terms of these variables. This is answered by looking at the developments in these variables over time and comparing them to developments in a control group comprised of other, similar companies that do not participate in the VP programme.

It should be noted that the analysis of the number of (highly educated) employees, value added and net income gives highest weight to companies experiencing the largest changes in these variables. These are typically larger companies. For average wage cost, return on assets and labour productivity, companies are treated equally and, thus, higher weight is given to smaller companies.

Empirical specification

Company-level analysis: selection of controls

For the company-level analysis, the selection of controls is carried out in two steps. First, select a pool of potential controls in the Experian data. Second, apply a matching procedure.

Before applying the matching procedure, we go through the Experian data and exclude observations of companies in industries without participant companies, with ownership classifications where there are no participant companies, companies larger than 150 employees, and companies for which a set of additional conditions is not fulfilled.[21] The remaining sample is denoted the 'adjusted Experian sample'.

[21] These conditions are: equity being between DKK-20mio and 150mio., net income between DKK-20mio and 20mio., total assets between zero and DKK250 mio., short term debt between DKK15,000 and 70mio., an equity share between -2.5 and 0.9, return on assets between -1.2 and 1, the number of employees with at least a post-secondary education less than or equal to 25, the number of employees with a tertiary education less than or equal to 5, and firm age less than 150 years. Imposing these conditions does not affect the number of participants in the sample.

As a last step before the matching procedure, we exclude all observations of participants that do not belong to the last financial report before starting participation in a VP-project. We then estimate a binary choice model on the adjusted Experian sample which is used to predict a participation probability (a propensity score) for any given company for any given year in the reduced Experian population.

The population is then grouped by year and industry. Within each group, a matched twin is found for each participant company on the basis of the propensity score. This procedure ensures equality between the participants and controls in terms of the highly detailed industrial sector classification 'Dansk Branchekode' and timing.[22]

This procedure implies that we identify 316 control firms for 316 participants. These define the *analysis sample* of the study. The year in which a control company is selected is this company's year 0 (base year, t=0), which is the cut-off year for later before-after comparisons. For VP-companies, year 0 is simply the last year before participating in the programme.[23]

Company-level analysis: the empirical model

We chose a model with fully specified dynamics, which is highly similar to Kaiser and Kuhn, 2012.[24] This model is formulated as follows:

$$y_{i,t} - y_{i,t-1} = x_t + \sum_{n=1}^{5} \left(\alpha_n D(t_i = n) + \beta_n \left(D(treat_i = 1) \times D(t_i = n) \right) \right) + u_i + \varepsilon_{i,t}$$

where $y_{i,t}$ is the dependent variable, i is firm index, t is a time index, where $t=0$ is year 0, and x_t are year dummies to account for business cycle effects. The D are dummy variables assuming the value of 1 if the logical conditions in their brackets are fulfilled. This model is estimated subject to company-level fixed effects u_i and has statistical errors $\varepsilon_{i,t}$.

The α and β are estimation coefficients, where the β measures the potential treatment effects. Note that this model extends Kaiser and Kuhn's analysis by estimating post-year zero effects not just for participants but controls as well. These are measured by the coefficient vector α, while the vector β collects the conditional difference-in-difference estimators.[25]

[22] The observation period is characterised by considerable business cycle movements, which implies the need to match controls as exactly as possible with regard to the time when they are selected.

[23] To be specific, the base year of participants is defined by the closing date of the last financial report before the start of participation. This means the base year of participants is not necessarily the calender year before starting to participate in the programme.

[24] Kaiser, U., Kuhn, J.M., Long-run effects of public–private research joint ventures: The case of the Danish Innovation Consortia support scheme. Res. Policy (2012).

[25] Another minor extension is the clustering of statistical errors $\varepsilon_{i,t}$ within treatment-control twin pairs.

The fixed effects setup implies that all time-invariant factors drop out of the model, thus making the model robust to any omitted time-constant factors which might be correlated with the decision to participate in the programme. The set of dummy variables generates a difference-in-difference model setup, and the coefficients of the dummy variables in the vector β estimate the potential programme effect. Separate dummy variables for each year after the base year allow estimating the dynamics of the potential programme effect.[26]

Company-level analysis: descriptive statistics

Out of the 434 companies that have hosted VP-projects in the DASTI data, 370 can be found in the Experian data. The remaining 64 firms that cannot be found in these data are probably non-incorporated firms that are not obliged to publish their financial reports by submitting them to the Danish Business Authority. Of the firms found in the Experian data, 338 filed a report in the year prior to programme participation. Only these firms will be considered in the subsequent analysis comparing performance both before and after the start of participation.

When setting the sampling criteria for this analysis, we need to decide how to treat outliers (extreme observations). This decision trades off robustness of later results with their representativeness. In the following, we choose to describe results for 'typical' VP-companies and to not consider companies in the financial sector (reducing the sample by eleven companies) nor companies with ownership codes that only occur very rarely in the sample of VP-companies (reducing the sample by five companies).[27]

After deleting financial sector companies and companies with atypical ownership codes, we are left with 319 observations. Of these, 318 have started their project before 2011 and can be followed for at least one year in the Experian data.

The controls for the latter analysis are found in the adjusted Experian sample. In these data, there are 296,000 company-level observations in the period from 2004 onwards that are roughly similar to the participants in a few dimensions, e.g. industrial sector and number of employees. For 316 of the 318 VP-companies, the matching procedure succeeds in finding controls for the analysis.

Means and standard deviations of a set of characteristics of these companies are described in the first columns of TABLE 5.1. This table also shows the characteristics of the adjusted Experian sample – which was selected in order to roughly resemble the group of participants, and used for the estimation of propensity scores for the matching procedure. TABLE 5.1 allows comparing the 316 programme participants with the two Experian samples and the control group of companies selected by the matching procedure.

[26] Also note that taking first-differences in the outcome variables addresses any potential problems of serially correlated unobserved characteristics.

[27] For example, we drop co-operations (two occurrences), funds (one occurrence), companies with limited liability (one occurrence), and one company with an unidentified ownership code.

A look at the raw figures shows that VP-companies are distributed over most industries, with relatively large shares in trade (21 percent), consultancies (12 percent) and IT services (9 percent). These shares follow the industry distribution of the total sample of companies in the Experian database. However, VP-companies are underrepresented in construction and overrepresented in manufacturing, metal, construction, advertising and cleaning.

At first sight, the VP-companies look healthy: On average, they are slightly larger (mean 15 employees) than the average company in the Experian database (mean 11 employees) and have survived longer (15 years vs. 10 years). Many (42 percent) are registered as exporters in the Experian database, and almost 50 to 100 percent are owned by other companies, e.g. holding companies (compared to 34 percent for all companies in the Experian data). Also, 11 percent own other companies (compared to 5-6 percent of all companies).

When it comes to employee characteristics, it is found that VP companies have a relatively large share of employees with at least a secondary education and also an above-average share of employees with a post-secondary or tertiary-level education. They have a relatively low share of technically trained employees.

The fact that VP-companies are not fully representative companies implies that, if one aims at comparing these companies with other companies, one must carefully construct a control group of similar companies for the comparison.

A first step in this process is the estimation of a binary choice model to estimate propensity scores. This model is based on the 239,000 company observations in the adjusted Experian sample and the 318 participants in the year before treatment.

The results of the binary choice model (formulated as a logit model) are displayed in the left hand side columns of TABLE 5.2. Findings largely agree with what was seen in the mean comparisons: Companies are most likely to participate if they are not in the construction industry, are incorporated as joint stock companies, are relatively large, have high returns on assets and a relatively low equity share, a low average employee age, a high share of highly educated employees, and a low share of employees with primary school as their highest level of education. The VP programme is relatively popular in rural districts, with high propensity on the island of Funen and both Southern and Northern Jutland.

The results of the logit model allow us to calculate predicted participation probabilities (propensity scores). These are used to select a control group of companies for the subsequent treatment-control analysis.

TABLE 5.1: Means and standard deviations of key characteristics of company-level samples								
	Summary of all firms, N =296,087		Summary of adjusted sample, N = 238.375		Summary of treatments in analysis sample, N = 316		Summary of controls in analysis sample, N = 316	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Industry								
Construction	0.13	0.34	0.15	0.36	0.06	0.23	0.05	0.22
Trade	0.18	0.39	0.19	0.39	0.21	0.41	0.21	0.41
IT, services	0.07	0.26	0.07	0.25	0.09	0.29	0.09	0.28
Manufacturing	0.01	0.10	0.01	0.11	0.06	0.24	0.06	0.24
Metal industries	0.02	0.14	0.02	0.15	0.05	0.22	0.03	0.18
Furniture and related industries	0.02	0.12	0.02	0.13	0.06	0.23	0.08	0.27
Travel agencies, cleaning services	0.02	0.13	0.02	0.13	0.03	0.18	0.04	0.20
Advertisement	0.03	0.16	0.03	0.16	0.06	0.24	0.07	0.25
Consulting, business services	0.13	0.34	0.13	0.34	0.12	0.32	0.12	0.33
Paper&publishing	0.01	0.11	0.01	0.11	0.03	0.18	0.02	0.15
Other	0.38	0.48	0.35	0.48	0.22	0.42	0.23	0.42
Key figures								
Number of employees	11.21	64.13	7.02	12.80	14.75	18.39	13.96	17.46
No number of employees information	0.28	0.45	0.23	0.42	0.03	0.18	0.02	0.15
Number of employees=0	0.13	0.34	0.11	0.31	0.01	0.11	0.02	0.14
Number of highly educated employees[1]	0.19	0.31	0.17	0.30	0.22	0.27	0.22	0.29
Value added (DKK1,000)	4713	39920	2903	5941	6483	8425	6279	8304
No value added information	0.12	0.32	0.08	0.27	0.01	0.11	0.03	0.16
Net income (profit, DKK1,000)	676	25560	302	1654	457	2165	567	2070
Return on assets	-0.41	42.66	0.02	0.23	0.03	0.21	0.04	0.22

Notes: 1: "highly educated" refers to post-secondary education and tertiary-level education.

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Wage cost per employee (DKK1,000)	410	1540	400	660	395	217	377	163
No wage cost per employee info.	0.43	0.49	0.37	0.48	0.09	0.29	0.09	0.28
Labour productivity (DKK1,000)	3096	97103	2623	65175	2056	5479	1867	2627
No labour prod. Info.	0.44	0.50	0.37	0.48	0.08	0.27	0.09	0.29
Total assets (DKK1 mio.)	17.07	219.76	7.79	16.29	13.06	20.31	13.05	21.51
Equity share	-1.23	99.71	0.28	0.38	0.22	0.35	0.23	0.34
Short term debt (DKK1,000)	7008	86627	3428	6928	6532	9240	6579	9931
Development in selected key figures (average annual increase in t=-3 to t=0)								
Number of employees	0.34	9.25	0.24	2.09	0.88	3.01	0.85	3.12
Number of highly educated employees	0.12	2.64	0.04	0.54	0.19	0.91	0.11	0.82
Value added (DKK1,000)	269	7602	154	1233	448	1876	506	1870
Net income (DKK1,000)	33.9	9435.8	2.1	860.6	-1.4	1412.6	89.0	995.0
Wage cost per employee (DKK1,000)	-4.2	1567.3	-4.2	1529.4	2.6	161.3	-17.3	239.7
Labour productivity (DKK1,000)	94.0	40919.0	74.4	22814.4	-114.0	2694.0	-721.3	11055.9
Year								
2005	0.11	0.31	0.11	0.32	0.24	0.43	0.24	0.43
2006	0.16	0.36	0.16	0.37	0.15	0.35	0.15	0.35
2007	0.18	0.39	0.18	0.39	0.15	0.36	0.15	0.36
2008	0.21	0.41	0.20	0.40	0.15	0.36	0.15	0.36
2009	0.23	0.42	0.22	0.41	0.16	0.37	0.16	0.37
Company age and ownership information								
Ownership code: joint stock	0.27	0.44	0.44	0.44	0.52	0.50	0.53	0.50
Company age	10.45	21.80	21.80	13.45	15.10	19.87	13.94	16.32
Company has mother company	0.34	0.47	0.47	0.48	0.48	0.50	0.49	0.50
Company is mother company	0.06	0.24	0.24	0.23	0.11	0.31	0.09	0.29
Company is exporter	0.12	0.32	0.32	0.32	0.42	0.49	0.39	0.49

Region								
Zealand N, Copenhagen	0.24	0.43	0.23	0.42	0.14	0.35	0.19	0.39
Zealand S	0.09	0.28	0.09	0.28	0.04	0.19	0.04	0.19
Funen, Bornholm	0.11	0.31	0.12	0.32	0.15	0.35	0.16	0.37
Jutland S	0.07	0.26	0.07	0.26	0.11	0.32	0.07	0.25
Jutland W	0.09	0.29	0.10	0.30	0.11	0.32	0.11	0.32
Jutland E	0.09	0.28	0.09	0.29	0.10	0.30	0.07	0.26
Jutland N	0.16	0.37	0.16	0.37	0.18	0.38	0.18	0.38
Region not specified, overseas departments	0.08	0.27	0.08	0.27	0.11	0.31	0.10	0.31
Employee characteristics								
Company: mean employee age (years)	40.1	9.6	40.0	9.5	37.5	6.6	37.6	7.1
Company: share of employees that is female	0.26	0.29	0.25	0.29	0.30	0.26	0.27	0.26
Company: share with a secondary education	0.26	0.34	0.24	0.33	0.31	0.28	0.30	0.32
Company: share with a post-secondary education	0.19	0.31	0.17	0.30	0.22	0.27	0.22	0.29
Company: share with a tertiary education	0.08	0.21	0.07	0.20				
Company: share social sciences	0.26	0.32	0.26	0.32	0.29	0.25	0.29	0.29
Company: share arts & humanities	0.03	0.12	0.03	0.12	0.05	0.14	0.05	0.14
Company: share technical sciences	0.35	0.35	0.35	0.35	0.30	0.28	0.32	0.33

TABLE 5.2: Company-level analysis. Logit estimation results. Dependent variable: The company participates in the VP-programme in the following year

	Adjusted sample N = 238,693		Treatments and controls sample N = 632	
	Mean	Std. Dev	Mean	Std. Dev
Industry				
Construction	-0.85***	0.28	0.46	0.46
Trade	-0.31*	0.19	0.02	0.28
IT, services	-0.16	0.25	0.28	0.40
Manufacturing	0.90***	0.28	0.23	0.41
Metal industries	0.16	0.30	0.66	0.47
Furniture and related industries	0.55*	0.28	-0.27	0.40
Travel agencies, cleaning services	0.48	0.34	-0.31	0.51
Advertisement	0.28	0.28	-0.08	0.42
Consulting, business services	-0.19	0.24	0.13	0.36
Paper&publishing	0.19	0.36	0.34	0.58
Other (omitted category)				

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Key figures				
Number of employees	0.04***	0.01	0.00	0.02
Number of employees^2	0.00***	0.00	0.00	0.00
No employees information	-0.79	0.74	1.37	1.22
Number of employees=0	-1.18*	0.69	0.40	1.04
Value added (DKK 1 mio)	-0.01	0.02	0.02	0.03
No value added information	-0.70	0.63	-0.81	0.95
Net income (DKK 1 mio)	-0.04	0.04	-0.04	0.07
Return on assets	0.64**	0.32	0.32	0.53
Wage cost per employee (DKK1,000)	0.00	0.00	0.00	0.00
No wage cost per employee info.	0.74	0.48	0.02	0.93
Labour productivity (DKK1,000)	0.00	0.00	0.00	0.00
No labour prod. info.	-0.41	0.50	-1.16	0.78
Total assets (DKK 1 mio)	0.01	0.01	0.01	0.02
Total assets (DKK1,000)^2	0.00	0.00	0.00	0.00
Equity share	-0.57***	0.17	-0.35	0.33
Short term debt (DKK1,000)	0.00	0.00	0.00	0.00
Development in selected key figures (average annual increase in t=-3 to t=0)				
Number of employees	0.03	0.03	0.03	0.05
Number of employees, missing obs.	0.15	0.61	0.14	1.04
Number of highly educated employees	0.09	0.08	0.12	0.12
Number of highly educated employees, missing obs.	0.04	0.85	0.47	1.26
Value added (DKK 1 mio)	0.03	0.05	-0.06	0.09
Value added, missing obs.	0.17	0.42	-0.02	0.76
Net income (DKK 1 mio)	-0.03	0.08	0.00	0.11
Wage cost per employee (DKK1,000)	0.00	0.00	0.00	0.00
Wage cost per employee, missing obs.	0.74	0.48	0.02	0.93

Labour productivity (DKK 1 mio)	0.00	0.01	0.01	0.02
Labour productivity, missing obs.	1.49***	0.45	0.56	0.71
Year				
2005	0.53***	0.19	-0.06	0.29
2006	-0.27	0.21	-0.19	0.33
2007	-0.33	0.21	-0.10	0.32
2008	-0.34*	0.21	-0.22	0.34
2009	-0.23	0.20	-0.25	0.32
Company age and ownership information				
Ownership code: joint stock	0.30**	0.14	-0.13	0.21
Company age	0.00	0.01	-0.01	0.01
Company age^2	0.00	0.00	0.00	0.00
Company has mother company	0.06	0.12	-0.07	0.19
Company is mother company	0.18	0.19	0.23	0.31
Company is exporter	0.99***	0.14	0.12	0.20
Region (omitted category: Copenhagen)				
Zealand N	-0.24	0.27	-0.42	0.40
Zealand S	-0.44	0.38	-0.05	0.56
Funen, Bornholm	0.79***	0.28	-0.06	0.43
Jutland S	0.72**	0.29	0.64	0.46
Jutland W	0.41	0.29	0.14	0.44
Jutland E	0.23	0.30	0.44	0.46
Jutland N	0.34	0.27	0.06	0.40
Region not specified, overseas departments	0.67**	0.29	0.26	0.45

Employee characteristics				
Company: mean employee age (years)	-0.04***	0.01	0.00	0.01
Company: share of employees that is female	0.00	0.24	0.25	0.42
Company: share with a secondary education	0.16	0.38	0.03	0.64
Company: share with a post-secondary education	-0.53	0.41	-0.21	0.69
Company: share with a tertiary education	-0.67	0.41	0.49	0.70
Company: share social sciences	-0.08	0.31	0.04	0.55
Company: share technical sciences	-0.73**	0.35	-0.45	0.60

Before turning to the analysis, we need to establish an idea of just 'how similar' the groups of matched treatments and controls really are. Accordingly, we will compare the two groups of companies as follows:

First, we run a very simple test of the similarity of observable characteristics of the two groups of companies and estimate the same logit model as earlier, but this time on the matched treatment-control sample. The results of this exercise are displayed in the right hand side columns of TABLE 5.2. We find that all coefficients have decreased in absolute size and come out as insignificant, indicating an absence of considerable differences in these variables across the two groups of companies.

Second, we look at the similarity of the two groups of companies in the matched treatments-controls sample by simply comparing the means of observable characteristics of the two groups, displayed in the two right hand side columns of TABLE 5.1.

Inspection of TABLE 5.1 suggests that the matching procedure succeeded in finding matched twin companies that highly resemble the group of treatments in the year before treatment. Differences between the groups are typically one order of magnitude smaller than the corresponding standard deviations, implying that none of the differences are statistically different from zero.

So: If the VP programme significantly increases the performance variables of the analysis, we should be able to see this by higher growth in the performance variables after treatment than before treatment, and a greater growth increase around year 0 for treatments than for controls. This will be tested in the next section.

Company-level analysis: Results

In the following, developments in a number of performance variables for companies that have participated in the VP programme are compared with the group of controls selected by the matching procedure. These variables are: the number of highly educated employees (i.e. employees with an education at a post-secondary or tertiary level), the number of employees, value added, profits, return on assets, wage costs per employee, and labour productivity.

TABLE 5.4 displays the results of the *conditional diff-in-diff* model with company *fixed effects*. The coefficients *'TREAT=1 & t=1', 'TREAT=1 & t=2',..., 'TREAT=1 & t=5'* correspond to the potential treatment effect estimates β_n while the coefficients of 't+1', 't+2', etc. correspond to the α_n of the conditional diff-in-diff model described in the previous section. The results are based on the approximately 300 programme participants and the same number of associated control companies. But only companies that participated early in the programme can be observed after the very first years after treatment, so results for more than a few years after year 0 are based on a substantially reduced number of observations.

Before we look at the specific findings, it is necessary to consider how to treat outliers. We have to do with company level data which by its very nature is highly heterogenous, and the treatment of outliers is important to later results.[28]

TABLE 5.4 is based on VP-companies and companies in the control group with at most 50 employees that do not experience large year-to-year changes in their numbers of employees, as well as regression-specific conditions imposed to further reduce unobserved heterogeneity. Obviously, the results of the analysis depend on these sampling conditions, and when interpreting later results one must be aware that the results are only valid for companies that fulfil the conditions. In subsequent robustness checks, these conditions are relaxed.

The results of TABLE 5.4 are summarized in the following sections.

[28] Although there is a lot of background information in the data, we are unable to offer explanations (and, thus, cannot control for) for a large amount of heterogeneity in the data. Clearly, we do not want to base overall results of the analysis on single observations with extreme values - especially when it cannot be ruled out that these values are statistical noise (e.g. due to company mergers or organisational restructuring).

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TABLE 5.4: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.458***	0.12	0.596**	0.30	219.3	217.2
Treat=1 & t=2	0.318**	0.14	0.00	0.34	374.1	239.6
Treat=1 & t=3	0.01	0.17	0.33	0.40	165.2	324.4
Treat=1 & t=4	-0.14	0.21	-0.45	0.60	124.0	448.2
Treat=1 & t=5	-0.22	0.26	-0.69	0.65	-563.1	580.3
t=1	-0.03	0.10	0.00	0.24	-20.5	194.5
t=2	-0.05	0.13	-0.15	0.32	-268.9	233.5
t=3	-0.14	0.17	-0.33	0.41	-10.5	324.3
t=4	-0.09	0.20	-0.11	0.56	243.0	393.0
t=5	0.02	0.25	0.86	0.59	468.9	528.6
Year dummies						
2003	0.01	0.12	0.00	0.24	-298.1	217.1
2004	0.06	0.11	0.36	0.28	308.3	221.5
2005	0.01	0.10	0.36	0.29	156.1	210.2
2006	0.01	0.12	0.43	0.34	348.7	243.1
2007	0.01	0.15	0.37	0.36	277.8	280.0
2008	-0.13	0.17	0.24	0.43	-285.7	309.8
2009	-0.14	0.20	-1.477***	0.50	-810.8**	370.5
Constant	0.12	0.09	0.34	0.25	240.8	187.6
Number of observations:	2609		2727		2611	
Number of companies:	535		546		533	
R-squared	0.03		0.08		0.04	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
-48.5	95.2	-0.03	0.02	8.28	11.26	-27.92	93.59
136.5	111.4	-0.04	0.03	5.25	10.27	57.28	107.90
133.3	122.1	0.00	0.03	-21.34	13.79	-137.30	91.84
205.5	218.1	-0.04	0.04	16.06	17.18	-39.23	134.00
-103.5	189.2	-0.04	0.06	-19.32	29.92	-159.70	215.30
24.2	87.2	-0.01	0.02	-13.08	11.55	-36.00	87.17
-125.6	104.2	-0.03	0.03	1.67	11.32	88.66	91.22
38.2	129.0	-0.03	0.03	10.89	15.10	108.40	97.51
14.1	199.6	-0.02	0.04	-0.61	20.35	24.21	117.90
190.1	220.3	-0.07	0.05	2.41	27.05	97.65	203.20
-96.4	90.3	-0.0426*	0.02	16.90***	6.23	-141.0*	72.66
44.5	86.6	0.01	0.02	8.99	7.21	-34.30	58.79
65.2	84.0	0.01	0.02	8.00	7.97	-28.89	74.78
2.2	95.6	0.01	0.03	10.54	8.97	48.53	90.79
24.5	113.5	0.02	0.03	13.41	13.39	-65.07	96.66
-191.8	130.1	-0.02	0.03	4.35	15.66	-180.4*	108.80
-362.4**	159.9	-0.01	0.04	8.81	18.74	-90.63	122.50
78.7	70.7	0.01	0.02	-1.77	5.88	60.65	59.57
2553		2669		1494		1693	
542		544		346		323	
0.03		0.02		0.01		0.02	

2. Only observations with annual change in the value added by less than DKK 10 mio.
3. Only observations with annual change in net income by less than DKK 3 mio.
4. Only observations with annual change in return on assets by less than 1, and total assets > DKK100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

Potential employment effects

A first question addressed in the empirical analysis is whether companies participating in the programme do indeed increase the number of highly educated employees (employees with an education level categorised as at least 'post-secondary-non-tertiary and tertiary', ISCED 4-8) relative to companies in the control group.

TABLE 5.5.a: Potential effects on the number of highly educated employees. Further results

	Ordinary least squares regression		Firm fixed-effects model		Conditional diff-in-diff model	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.445***	0.109	0.436***	0.146	0.458***	0.115
Treat=1 & t=2	0.286**	0.108	0.413**	0.176	0.318**	0.143
Treat=1 & t=3	-0.118	0.113	0.097	0.233	0.005	0.169
Treat=1 & t=4	-0.241	0.124	0.092	0.300	-0.143	0.205
Treat=1 & t=5	-0.269	0.201	0.214	0.394	-0.221	0.257
Includes firm-fixed effects	no		yes		yes	
Includes year dummy variables	no		yes		yes	
Includes information from before year zero	no		yes		yes	
Includes observations of the control group	no		no		yes	
Number of observations:	631		1354		2609	
Number of companies:	274		274		535	
R2:	0.05		0.02		0.03	

Notes: Highly educated employees are employees with a post-secondary or tertiary-level education. Only observations with annual changes in the number of employees with a post-secondary and tertiary education < 5. Only observations with annual changes in the number of employees of less than 12.

*, **, *** denote statistical significance at the 10%, 5%, and 1% level.

The coefficients of a simple ordinary least squares regression, which are equivalent to the population means and found in the leftmost columns of TABLE 5.5.a, imply that participating companies increase their number of highly educated employees by (0.445+0.286=) 0.7 employees in the first two years after start of participation.

The results of a company fixed effects model, which implements a before-after comparison for programme participants, are presented in the middle columns of TABLE 5.5.a. The similarity of this model's results and the results of the simple ordinary least squares regression implies that the earlier finding of an increase in the number of highly educated employees in association with programme participation (the results of the full-fledged model of TABLE 5.4 are replicated on the right of TABLE 5.5.a) is not to be interpreted as a continuation of any before-participation growth trend.

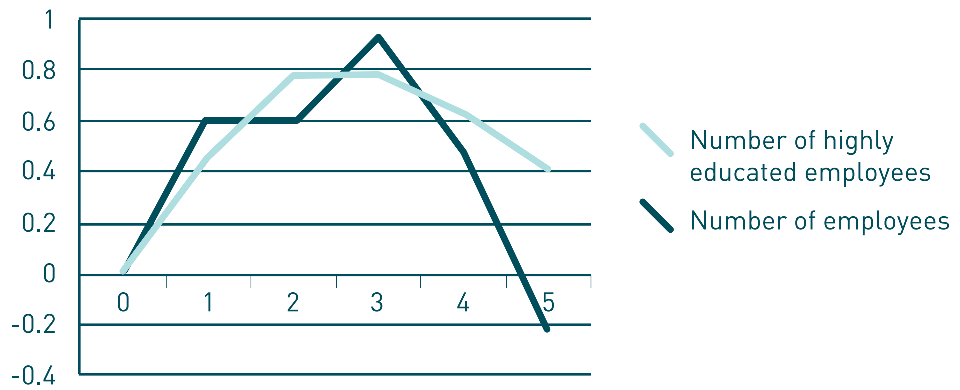
This allows the conclusion that the finding of positive potential programme effects with regard to highly educated employees is not just the result of the developments in (or the choice of) the group of control companies in the fully specified model behind TABLE 5.4. This observation, and non-positive coefficient estimates of the α_n-coefficient associated with *'t+1'*, *'t+2'* indicate an absence of behavioural additivity: Companies in the control group do not experience increases in the number of highly educated employees in the years after the selection into the control group.

Aggregated coefficients of the fully specified model are shown graphically in Figure 5.1.[29] Findings suggest that a participating company increases the number of highly educated employees by 0.46 additional individuals in the year of the treatment. The reason this number is not equal to 1.0 is that some of the projects (and associated employment relationships) last less than one year and have already been terminated before the census date of year 1. Also, as noted earlier, in some cases the information on highly educated employees is registered with time lags, if the data is from different sources (for instance, VP projects starting between the end of November and the closing date of the company's financial report). In these cases, potential effects occur between *t=0* and *t=2* instead of between *t=0* and *t=1*.[30]

[29] Figure 5.1 (just like the figures to follow in the next subsections) presents aggregated estimated treatment coefficients β_n. These measure the average deviation of the developments of treatment companies after treatment from the developments of the control group and the (company-specific) developments before treatment.

[30] The variable 'number of highly educated employees' is constructed from information from Statistics Denmark. This information can be a couple of months older than the closing date of the given company's financial report, which sets the time structure of the analysis. For example, VPs hired between Statistics Denmark's closing date at the end of November and the end of March will, in companies closing their books at the end of March, first occur in the data in the following year.

FIGURE 5.1: Number of employees. Aggregated estimated model coefficients. Years after treatment on horisontal axis.



As with the individual-level analysis, the coefficient estimates *'TREAT=1 & t=1', 'TREAT=1 & t=2',..., 'TREAT=1 & t=5'* can be summed up to calculate the total potential effect up to five years after treatment. This potential effect is an additional *(0.46+0.32=) 0.78* individuals in the first two years and an additional *(0.46+0.32+0.00-0.14-0.22 =) 0.42* individuals in the first five years after treatment.[31]

Accordingly, a first conclusion is that VP-companies on average increase the number of employees with a post-secondary education and above by an additional *0.8* employees in association with programme participation. However, there are no indications that participating companies continue to increase their number of employees in the years after programme participation: They have, on average, lower increases (greater declines) in the number of highly educated employees than companies in the reference group in year four and five after year zero, but this finding is not statistically significant.

Results for employment (independent of educational level) indicate that there is an immediate potential effect of *0.6* additional employees in the year of treatment, which is slightly larger than the potential effect found for highly educated employees. This indicates that VPs are often hired in association with company growth, or that some of the VPs are categorised as having an education below ISCED 5 or 6 in the Statistics Denmark education registers.

[31] These numbers are high in comparison with the previous finding that long-term relationships between VPs and their hosting companies are relatively uncommon, suggesting that VPs are replaced by other highly educated individuals after the end of their projects.

As is the case for highly educated employees, there is no sign that participating companies continue to increase the number of employees in the years after programme participation, with negative coefficients for year 4 and 5 after treatment resulting in an aggregate potential treatment effect over the first five years of -0.2 additional employees. Even though this number is not statistically different from zero, it is still the best guess of any long-run treatment effect of the programme.

TABLE 5.5.b: Potential effects on the number of employees. Further results

	Ordinary least squares regression		Firm fixed-effects model		Conditional diff-in-diff model		Conditional diff-in-diff model, dependent variable: annual employment growth in percent[1]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	1.164***	0.175	0.642**	0.278	0.596*	0.296	8.834***	2.929
Treat=1 & t=2	0.237	0.241	-0.118	0.411	0.001	0.335	1.517	2.934
Treat=1 & t=3	0.215	0.280	0.004	0.520	0.331	0.400	0.812	3.152
Treat=1 & t=4	-0.896**	0.383	-0.448	0.651	-0.446	0.596	-1.618	4.058
Treat=1 & t=5	-1.168***	0.421	0.436	0.789	-0.694	0.646	-5.425	6.459
Includes firm-fixed effects	no		yes		yes		yes	
Includes year dummy variables	no		yes		yes		yes	
Includes information from before year zero	no		yes		yes		yes	
Includes observations of the control group	no		no		yes		yes	
Number of observations:	650		1399		2727		2520	
Number of companies:	274		278		546		525	
R2:	0.07		0.08		0.08		0.07	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% signficance level.

1: Only observations with annual growth between -50 and 100 percent.

We are again interested in whether or not the results regarding the potential employment effects are because of higher growth in treatment companies after participation relative to before participation, or if the results are due to control companies having lower growth after year zero relative to before when compared to the treatment companies. For employment developments, we find again that the overall results do not depend on the choice of the control group, as the before-after comparison of the fixed effects model (on the subpopulation of treatment companies) gives estimators that are highly similar to the fully specified model.

Also, we are interested in learning how much the previous results depend on measuring employment growth as either absolute increases or percentage-point growth. Investigating absolute annual increases is the first choice for simple-to-implement cost-benefit calculations, but this also implies that smaller companies with small absolute changes in the performance parameters are given low weight in the statistical estimations.

When considering percentage-point employment growth, we find again a statistically highly significant positive potential employment effect in the years around treatment, suggesting that treatment companies grow by an additional 10 percent in the first two years after treatment. But also in this alternative model, there is no indication that treatment companies continue to increase their number of employees in year 4 and 5 after treatment.[32]

Potential effects on value added, net income (profits) and return on assets

We now turn to the financial performance variables. The results for these variables need to be interpreted with care, since they depend critically on the treatment of data - first and foremost the definition and treatment of outliers, i.e. companies experiencing large changes in the performance variables.

For the specific treatments of outliers and the given modelling choices, we find mostly positive, albeit statistically insignificant potential treatment effects for both value added[33] and net income (profits). Findings of TABLE 5.4 are depicted in FIGURE 5.2 and show that participating companies gained up to an additional DKK800,000 (EUR106,000) in annual value added and DKK400,000 (EUR53,000) in net income. But given the lack of statistical significance, these results should be interpreted as highly tentative.

[32] We will also present results for percentage-point growth rates for some of the other success parameters: gross profit, average wages, and labour productivity. There will be no such regressions for the performance measures number of highly educated employees, net income and return on assets, because these measures often assume the value zero or negative values – which implies that growth rates cannot be calculated.

[33] This variable is from the financial statements that companies file with the public authority, where it is called dækningsbidrag/bruttofortjeneste.

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FIGURE 5.2: Gross profit and net income (DKK1,000) developments in small steady-going companies. Aggregated estimated model coefficients. Years after treatment on horisontal axis.

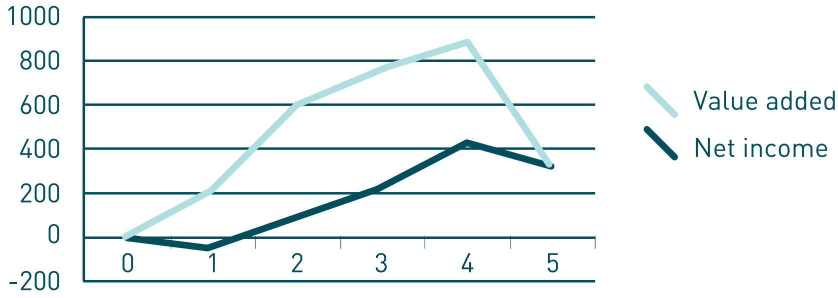
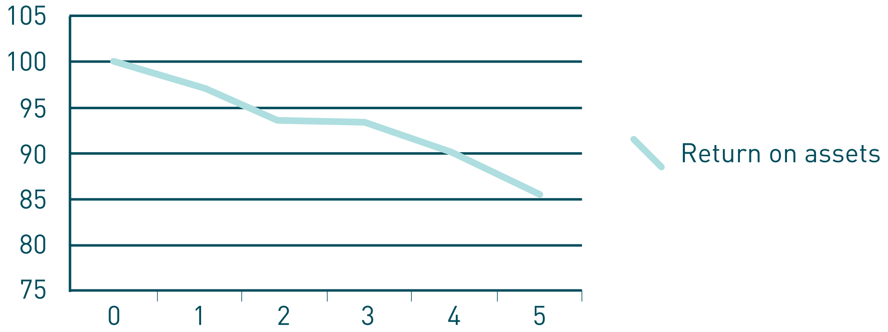


FIGURE 5.3: Return on assets developments (in percent) in small steady-going companies. Aggregated estimated model coefficients. Years after treatment on horisontal axis.



We also take a look at developments in return on assets, calculated as net income over total assets. The reasoning is that we have already looked at company growth variables, such as the number of employees and increases in value added, and that return on assets is largely independent of company size (which is obviously not the case for net income).

Cf. FIGURE 5.3, we find that companies that hire VPs on average do worse in terms of return on assets relative to companies in the control group of highly similar companies, but that coefficients are statistically insignificant.

>

TABLES 5.5.c-d further expand on the findings for value added, net income, and return on assets.

A look at the left hand side coefficients of TABLE 5.5.c suggests that value added developments are on average positive for treatment companies in the first three years after treatment, and negative more than three years after treatment. Part of the increases in the first years after treatment can be interpreted as a continuation of pre-treatment growth developments, as coefficients drop from DKK 421,702 to DKK 260,056 when controlling for company fixed effects. Controlling for developments in highly similar control companies, on the other hand, does not change the general picture, so the selection of the control group does not appear to be important to the overall result.

Also, for given sampling criteria, the previous (statistically insignificant) finding that treatment companies on average have higher value added growth is confirmed by the regression of percentage point value added growth. This regression even suggests the presence of positive and statistically significant potential effects for year two and four after treatment. The findings of a lack of significance for the model of absolute value added increases and the presence of significance for the growth rate model lends itself to the interpretation that companies with initially low value added gain the most in association with programme participation.

Turning to net income increases, we find that there is large heterogeneity in this variable, and as a consequence no statistically significant potential treatment effects can be detected for any of the different models. On average, absolute net income growth is negative for treatment companies after treatment. This can be explained by generally adverse business developments and company-specific time trends, as controlling with year dummies and for company-fixed effects in the regressions reverses the sign of the point estimates, making them positive. Again, taking into account the developments in the control group does not have any major impact on the overall results.

With regard to return on assets, it can be noted that the estimated coefficients are typically significantly negative in the pure before-after comparison of the company-fixed effects model: Treatment companies experience lower increases in return-on-assets after treatment relative to before treatment. This finding is not replicated in the fully specified conditional diff-in-diff model, where coefficients get closer to zero and are no longer statistically significant. This indicates that companies in the control group also experience adverse return-on-assets developments in the years after being chosen into the control group.

TABLE 5.5.c: Potential effects on value added (DKK1,000). Further results								
	Ordinary least squares regression[1]		Firm fixed-effects model[1]		Conditional diff-in-diff model[1]		Conditional diff-in-diff model, dependent variable: annual value added growth in percent[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	421.702***	113.743	260.056	214.888	219.298	217.152	4.07	3.58
Treat=1 & t=2	239.746*	134.653	181.428	281.134	374.106	239.569	10.10**	4.30
Treat=1 & t=3	57.844	186.946	237.428	384.165	165.199	324.385	5.59	4.27
Treat=1 & t=4	-90.041	305.445	396.622	514.582	124.020	448.158	12.12**	5.18
Treat=1 & t=5	-884.891**	353.632	-72.243	630.232	-563.130	580.313	-2.81	7.20
Includes firm-fixed effects	no		yes		yes		yes	
Includes year dummy variables	no		yes		yes		yes	
Includes information from before year zero	no		yes		yes		yes	
Includes observations of the control group	no		no		yes		yes	
Number of observations:	620		1346		2611		2223	
Number of companies:	272		272		533		451	
R2:	0.03		0.02		0.03		0.03	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% level.

1: Only observations with annual change in value added of less than DKK 10 mio.
2: Only observations with annual growth between -50 and 100 percent.

>

TABLE 5.5.d: Potential effects on net income (DKK1,000). Further results						
	Ordinary least squares regression		Firm fixed-effects model		Conditional diff-in-diff model	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	-48.481	48.225	6.965	98.949	-48.457	95.159
Treat=1 & t=2	-41.199	67.556	69.773	130.681	136.506	111.445
Treat=1 & t=3	51.065	81.233	229.337	166.055	133.285	122.101
Treat=1 & t=4	-36.775	126.978	262.090	227.975	205.536	218.128
Treat=1 & t=5	-277.485	152.530	171.135	267.157	-103.538	189.155
Includes firm-fixed effects	no		yes		yes	
Includes year dummy variables	no		yes		yes	
Includes information from before year zero	no		yes		yes	
Includes observations of the control group	no		no		yes	
Number of observations:	600		1322		2553	
Number of companies:	276		276		542	
R^2:	0.03		0.02		0.02	

Notes: Only observations with annual changes in the number of employees of less than 12. Only observations with annual change in net income of less than DKK 3 mio.

TABLE 5.5.e: Potential effects on return on assets (profits over total assets). Further results						
	Ordinary least squares regression		Firm fixed-effects model		Conditional diff-in-diff model	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	-0.027*	0.013	-0.038*	0.023	-0.029	0.023
Treat=1 & t=2	-0.039***	0.015	-0.066**	0.030	-0.036	0.026
Treat=1 & t=3	-0.017	0.021	-0.047	0.036	-0.001	0.028
Treat=1 & t=4	-0.036*	0.021	-0.082*	0.045	-0.036	0.039
Treat=1 & t=5	-0.099**	0.046	-0.141**	0.069	-0.042	0.060
Includes firm-fixed effects	no		yes		yes	
Includes year dummy variables	no		yes		yes	
Includes information from before year zero	no		yes		yes	
Includes observations of the control group	no		no		yes	
Number of observations:	630		1361		2669	
Number of companies:	277		277		544	
R2:	0.04		0.01		0.01	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% level.

Only observations with annual change in return on assets of less than 1, and total assets > DKK 100,000.

Potential effects on average wage costs and labour productivity

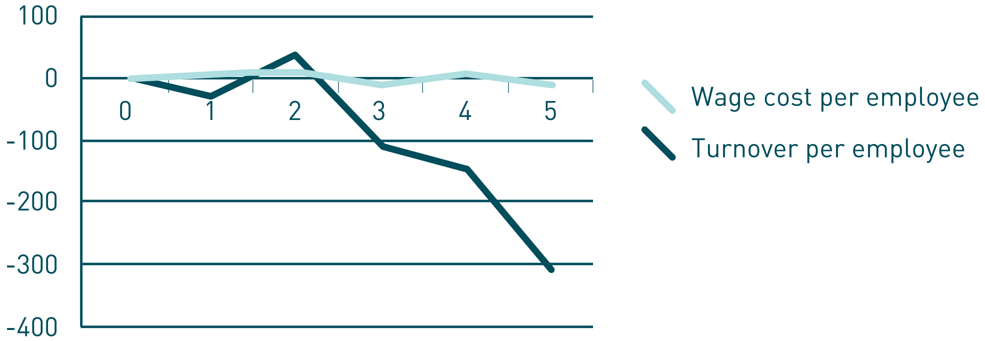
Results for average wage costs and labour productivity (measured as turnover per employee) are in the rightmost columns of TABLE 5.4, and illustrated in FIGURE 5.4.[34] With regard to the average wage costs per employee, it appears that any potential treatment effects are too small relative to the variation in the data and the number of observations. TABLE 5.5.f suggests that on average there are no substantial changes in wage cost per employee after treatment, a finding which is unaltered by the before-after comparisons for the subsample of treatment companies, or when considering growth rates rather than absolute changes.

[34] The variable 'wage cost per employee' is from the balance sheet information of the KOB/Experian database, and is characterised by a share of missing observations.

Labour productivity is measured as turnover per employee.[35] For absolute changes in labour productivity, it is not possible to demonstrate that VP-companies have higher productivity increases than the highly similar companies in the control group: Negative signs for t>2 indicate that VP-companies have lower increases than their counterparts in the reference group. However, this finding is not statistically significant and thus highly tentative. The picture also changes when we consider annual percentage-point growth in labour productivity rather than absolute annual increases: In this model specification, treatment companies generally outperform control companies in terms of labour productivity growth.

This finding – that treatment companies on average perform better than controls in terms of percentage-point growth and not significantly better in terms of absolute increases – implies that results are not robust with regard to model reformulation. This should advise us against drawing too strong conclusions on the basis of the statistical results. However, the fact that treatment companies seem to perform best when the performance is measured in percentage-point growth rather than absolute increases is an indication that it is in particular small companies that gain the most from programme participation.

FIGURE 5.4: Wage and labour productivity developments (DKK1,000). Aggregated estimated model coefficients. Years after treatment on horisontal axis.



[35] Turnover is from the Statistics Denmark registers instead of the Experian data. This is because (a) only companies above certain size thresholds are obliged to report this variable to the public authorities (which is why it is often missing in the Experian database) and (b) turnover is found for almost all companies in the Statistics Denmark registers (because VAT is registered for almost all companies).

TABLE 5.5.f: Potential effects on wage cost (DKK1,000) per employee. Further results

	Ordinary least squares regression[1]		Firm fixed-effects model[1]		Conditional diff-in-diff model[1]		Conditional diff-in-diff model, dependent variable: growth of wage cost per employee in percent[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	3.20	5.94	-8.96	10.86	8.28	11.26	-3.90	2.94
Treat=1 & t=2	14.98*	7.57	0.41	15.59	5.24	10.27	-0.43	2.41
Treat=1 & t=3	-6.34	9.74	-17.40	21.95	-21.34	13.79	-4.95	3.52
Treat=1 & t=4	16.04	12.27	7.09	24.88	16.06	17.18	1.36	4.50
Treat=1 & t=5	5.94	13.15	-26.35	35.12	-19.32	29.92	-5.15	6.70
Includes firm-fixed effects	no		yes		yes		yes	
Includes year dummy variables	no		yes		yes		yes	
Includes information from before year zero	no		yes		yes		yes	
Includes observations of the control group	no		no		yes		yes	
Number of observations:	355		794		1494		1474	
Number of companies:	190		190		346		343	
R2:	0.02		0.02		0.01		0.01	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1: Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
2: Only observations with annual growth between -50 and 100 percent.

>

TABLE 5.5.g: Potential effects on labour productivity (DKK 1,000). Further results

	Ordinary least squares regression[1]		Firm fixed-effects model[1]		Conditional diff-in-diff model[1]		Conditional diff-in-diff model, dependent variable: annual labour productivity growth in percent[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	-67.67	53.38	-78.40	85.83	-27.92	93.59	2.50	3.71
Treat=1 & t=2	146.02*	78.44	127.63	128.59	57.28	107.95	6.64*	3.72
Treat=1 & t=3	-70.54	69.29	-45.92	124.43	-137.30	91.84	2.90	4.07
Treat=1 & t=4	-43.03	103.33	-46.98	176.48	-39.23	134.02	4.26	5.41
Treat=1 & t=5	-133.16	83.80	-115.44	202.26	-159.68	215.28	-7.57	11.12
Includes firm-fixed effects	no		yes		yes		yes	
Includes year dummy variables	no		yes		yes		yes	
Includes information from before year zero	no		yes		yes		yes	
Includes observations of the control group	no		no		yes		yes	
Number of observations:	369		898		1693		2186	
Number of companies:	171		171		323		483	
R2:	0.02		0.02		0.02		0.02	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.
2: Only observations with annual growth between -50 and 100 percent.

Results for subsamples

In the following, we look at whether previous findings are different for different industries, VP- and project-specific characteristics.

This functions as a robustness check of the previous results, but it also offers an opportunity to see under what circumstances the programme might be considered to be most successful. In particular, the previous regression models will be applied on the following samples:

1. All companies, with no outliers removed.
2. Only companies where the DASTI and Statistics Denmark data are in accordance with regard to the company-VP match.
3 Only VP-projects that were not aborted before schedule.
4. Only companies without any tertiary-level educated employees in the year prior to programme participation.
5. Only VP-projects in, respectively, manufacturing, services, and other industries.
6. Only male VPs, only female VPs.
7. Only VPs with a tertiary education.
8. Only VPs with education degrees in, respectively, arts and humanities, social sciences, and technical sciences subjects.

For ease of reading, the results can be found in the appendix of this report. Aggregated regression coefficients, which measure potential treatment effects, are for most of the subsamples illustrated graphically and discussed below.

Let us first turn our attention to the results for the sample of all companies, with no outliers removed. For this sample, estimated standard errors are often much larger than the absolute sizes of the coefficient estimates (TABLE A.1). Thus, for all participant companies (including the larger ones), it is not possible to make statements on the potential treatment effects with any degree of accuracy, with the exception of the employment of highly educated employees.

Potential effects on the number of highly educated employees

We find the largest potential treatment effects for companies without tertiary-level educated employees in the year prior to treatment, and for companies hiring male VPs, and for those hiring VPs with a technical sciences education. The lowest potential effects are found for those hiring VPs with an education in arts and humanities, and, especially over a time horizon beyond the very first years after treatment, female VPs. There is only a small immediate potential effect for service industries. However, companies in these industries increase the number of highly educated employees in the years after treatment.

FIGURE 5.5.a: Number of highly educated employees. Estimated potential treatment effects. Years after year zero on horisontal axis.

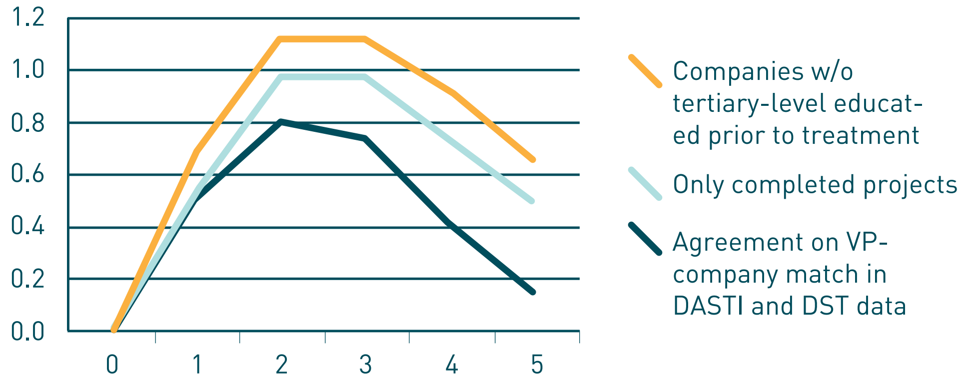


FIGURE 5.5.b: Number of highly educated employees. Estimated potential treatment effects. Years after year zero on horisontal axis.

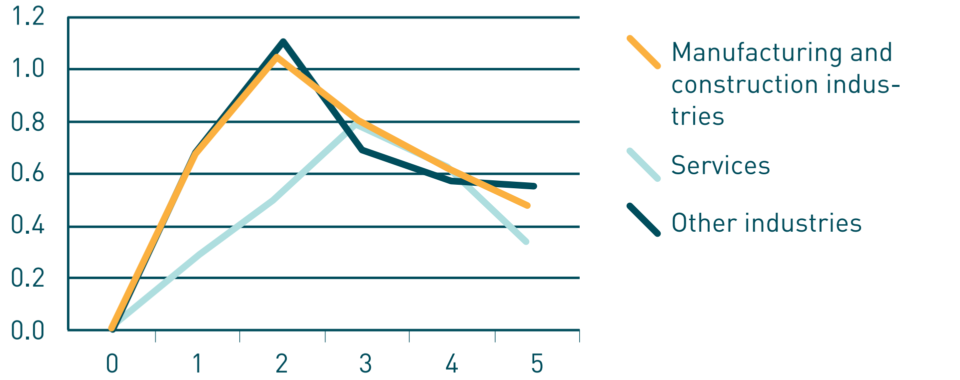


FIGURE 5.5.c: Number of highly educated employees. Estimated potential treatment effects. Years after year zero on horisontal axis.

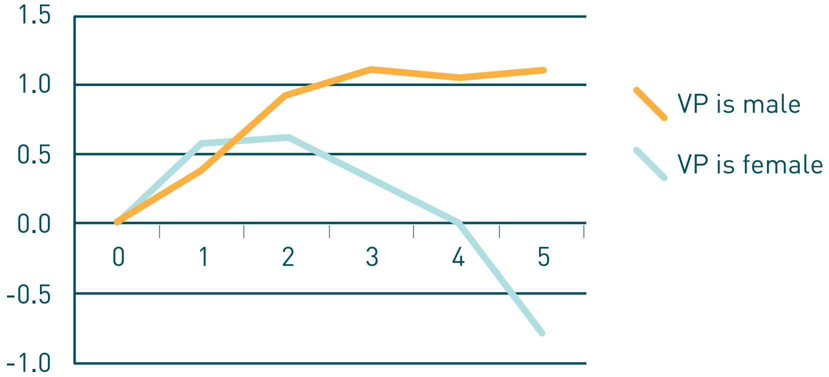
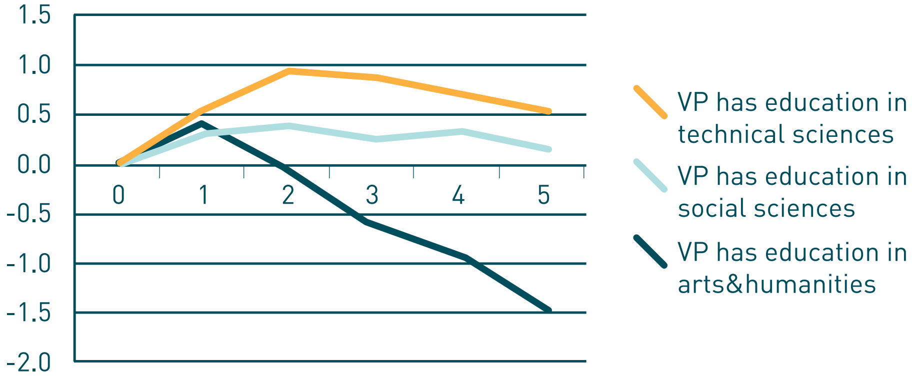


FIGURE 5.5.d: Number of highly educated employees. Estimated potential treatment effects. Years after year zero on horisontal axis.



Potential effects on the number of employees

For total employment, it proves to be important that the VP-project is completed and not aborted before schedule. Again, it is companies that hire female VPs and VPs with an education in arts and humanities that have the poorest growth performance. For VPs with a technical sciences education, a positive potential programme effect for highly educated employees and the absence of any detectable potential effect for employees of all educations indicate that companies that hire these VPs would have employed other individuals with lower educations in the counterfactual case of non-participation.

FIGURE 5.6.a: Number of employees. Estimated potential treatment effects. Years after year zero on horisontal axis.

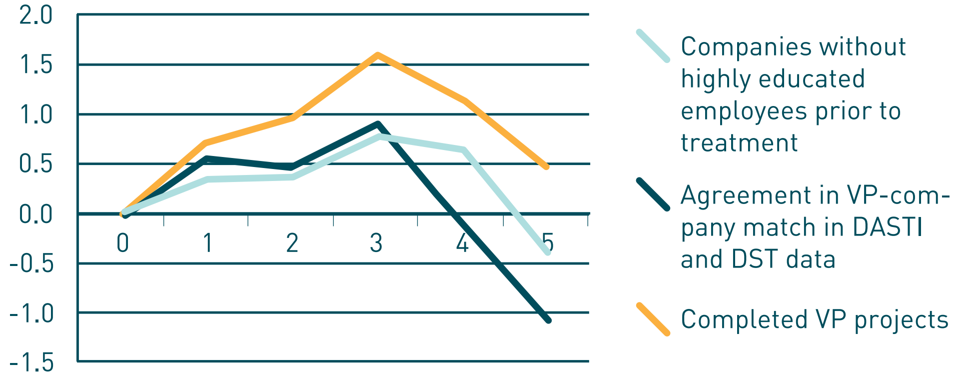


FIGURE 5.6.b: Number of employees. Estimated potential treatment effects. Years after year zero on horisontal axis.

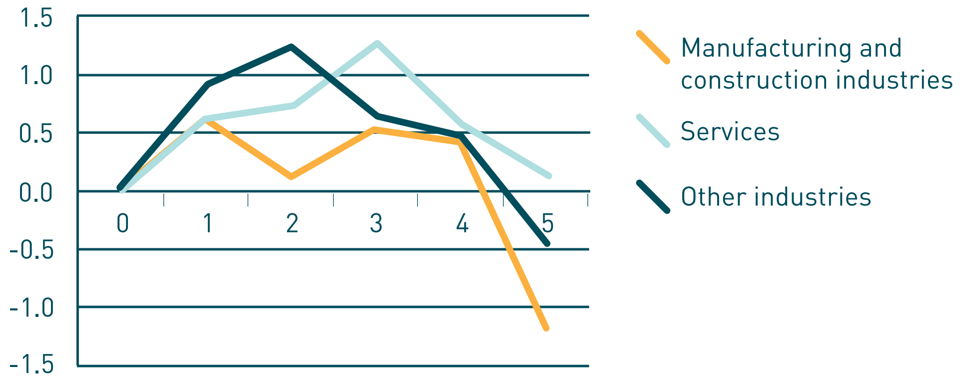


FIGURE 5.6.c: Number of employees. Estimated potential treatment effects. Years after year zero on horisontal axis.

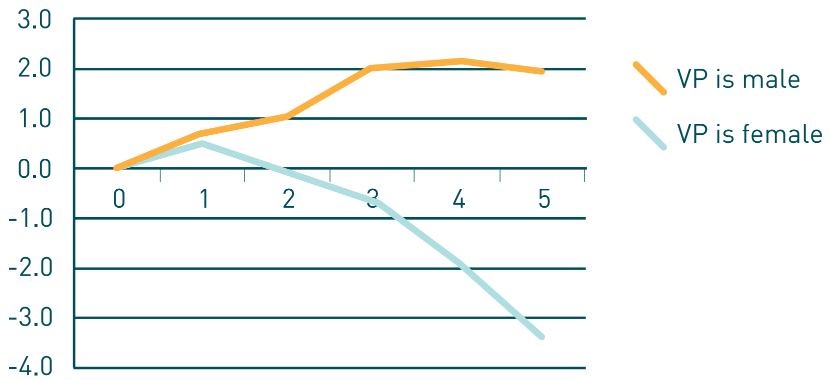
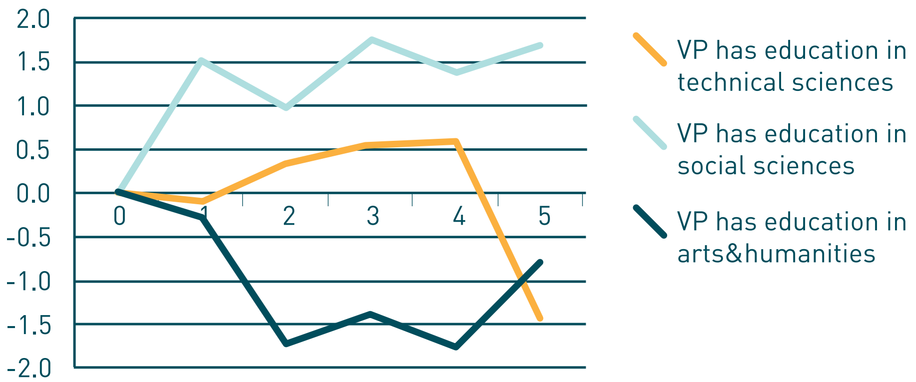


FIGURE 5.6.d: Number of employees. Estimated potential treatment effects. Years after year zero on horisontal axis.



Potential effects on value added

The comparison of potential value added effects agrees to large extent with the findings for employment: Subgroups of companies that are characterised by low average increases in the number of (highly educated) employees in association with programme participation are also characterised by low increases in value added. This is notably the case for companies hiring female VPs and VPs with an education categorised as within arts and humanities. The highest average increases are found in the manufacturing industries and for VPs with a social sciences-related education. With regard to value added, it is again important that the project was completed, while there is no indication that companies without tertiary educated employees prior to treatment gain the most in terms of value added.

FIGURE 5.7.a: Value added (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

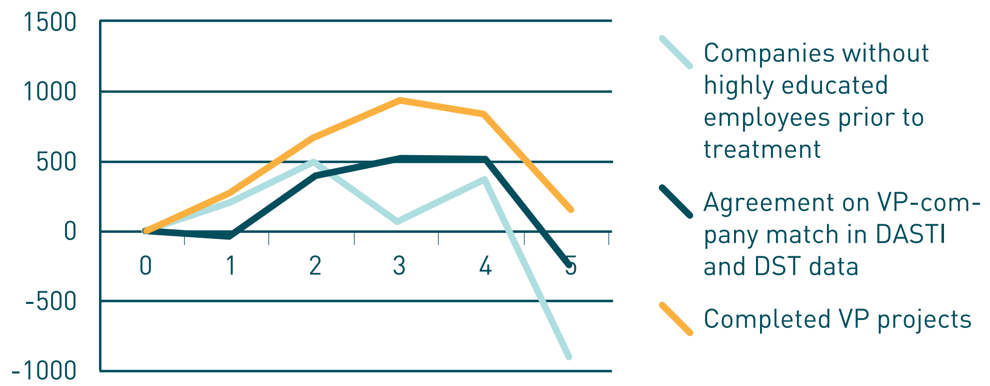


FIGURE 5.7.b: Value added (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

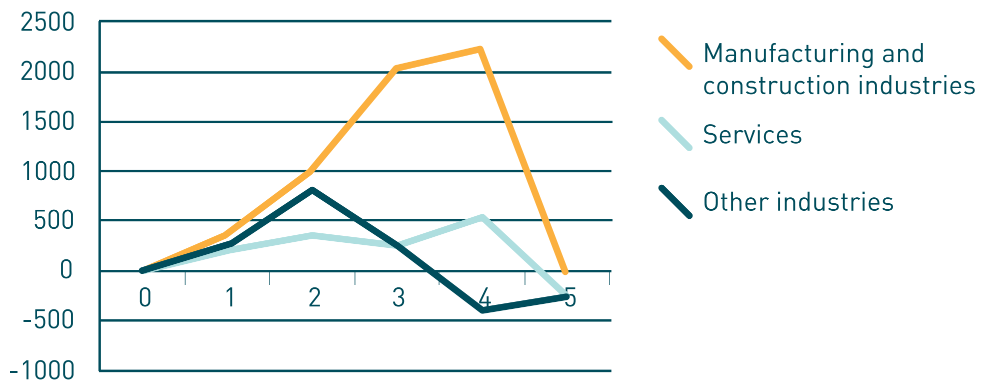


FIGURE 5.7.c: Value added (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

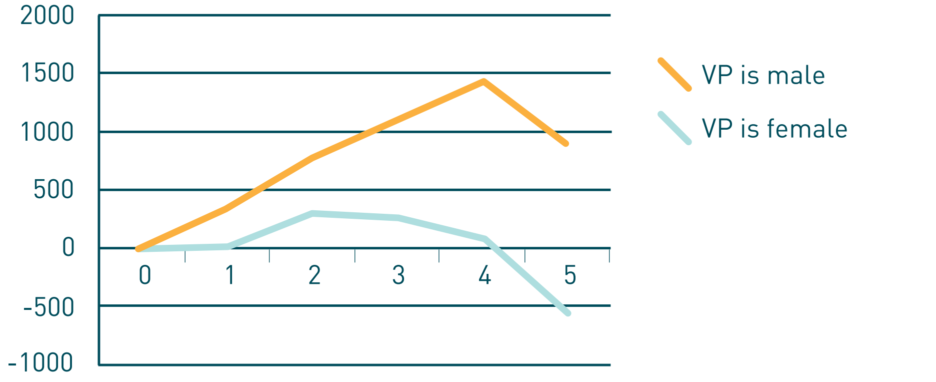
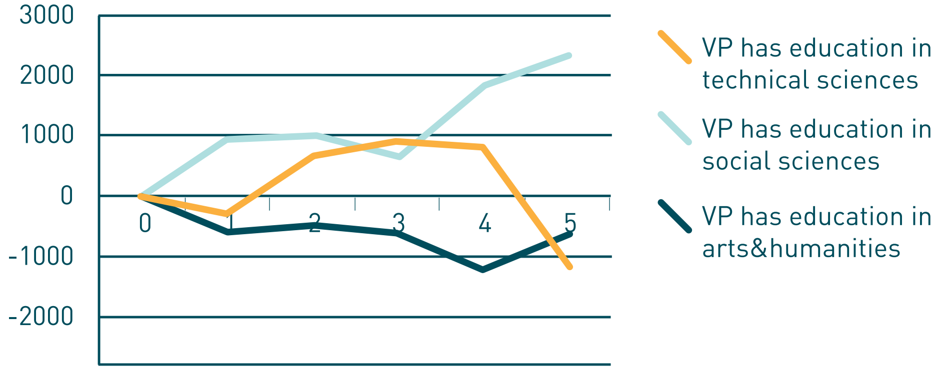


FIGURE 5.7.d: Value added (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis (year zero=100).



Potential effects on net income (profits) and return on assets

What was true for the developments of value added does not necessarily hold true for net income. For example, companies hiring female VPs are on average not characterised by less favourable net income developments. It can be noted that companies without tertiary-level educated employees prior to treatment and companies hiring VPs with a technical sciences education do best in terms of return-on-assets developments.

FIGURE 5.8.a: Net income (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

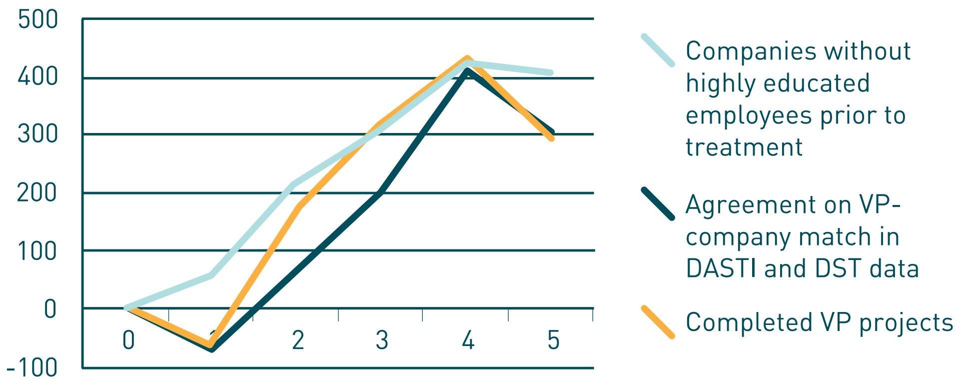


FIGURE 5.8.b: Net income (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

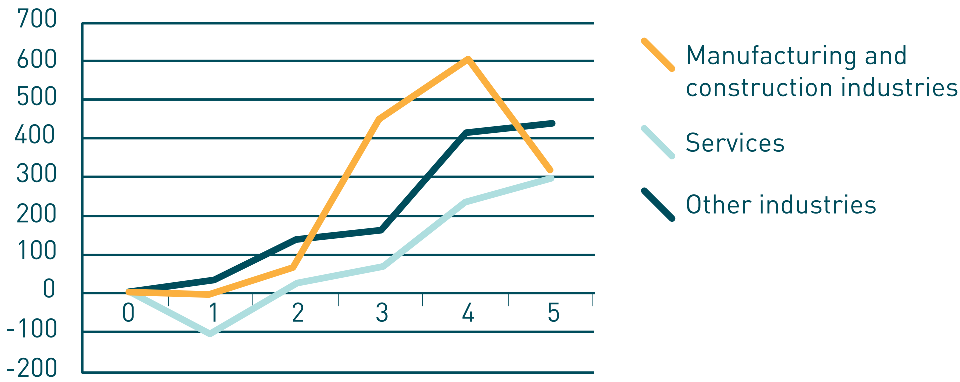


FIGURE 5.8.c: Net income (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

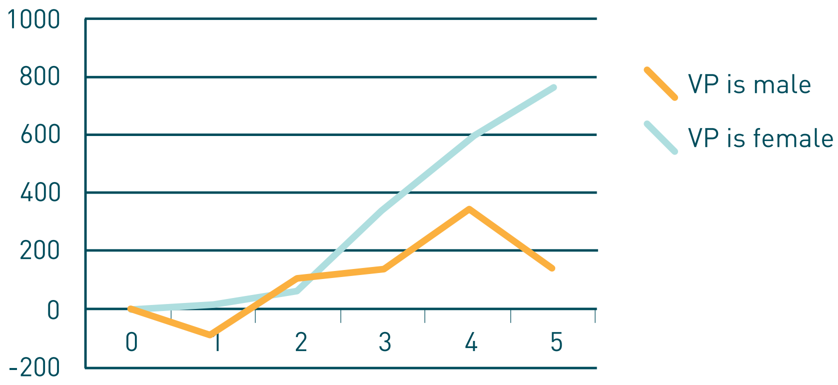


FIGURE 5.8.d: Net income (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

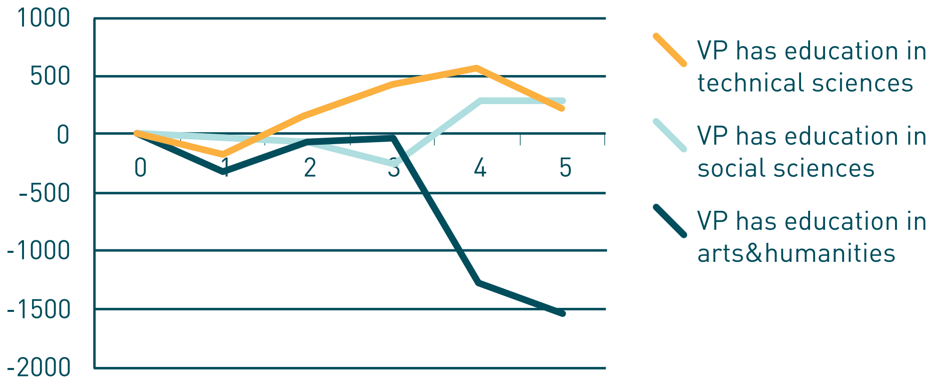


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FIGURE 5.9.a: Return on assets. Estimated potential treatment effects. Years after year zero on horisontal axis.

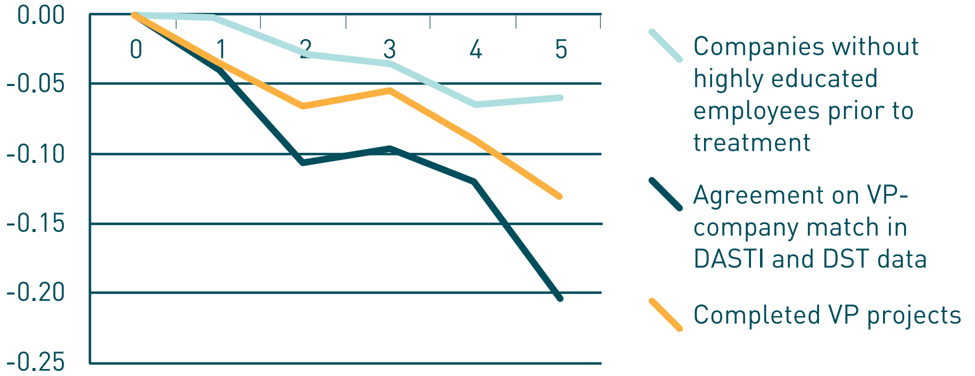


FIGURE 5.9.b: Return on assets. Estimated potential treatment effects. Years after year zero on horisontal axis.

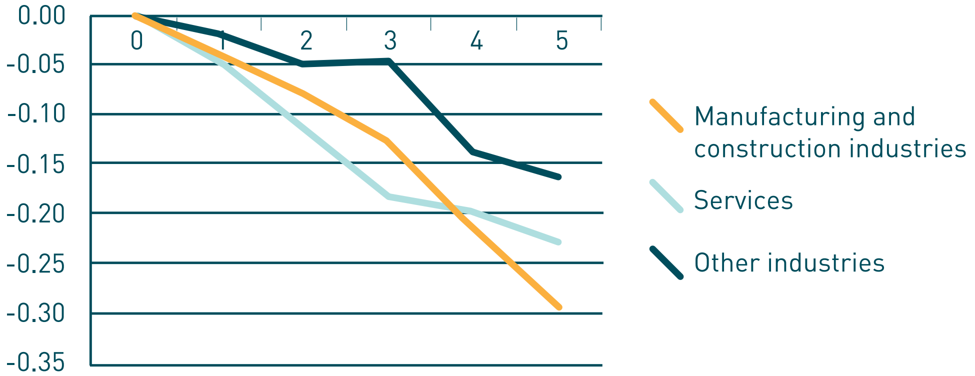


FIGURE 5.9.c: Return on assets. Estimated potential treatment effects. Years after year zero on horisontal axis.

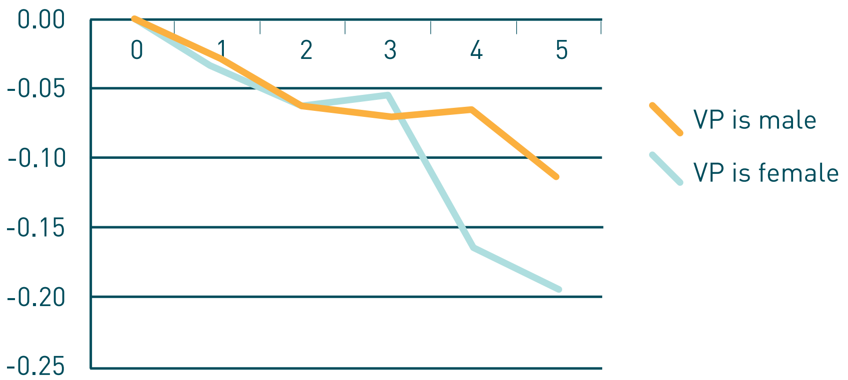
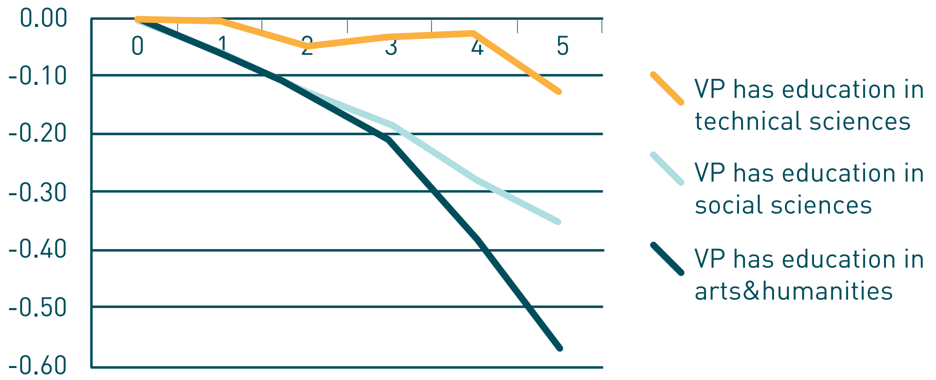


FIGURE 5.9.d: Return on assets. Estimated potential treatment effects. Years after year zero on horisontal axis.



Potential effects on wages and labour productivity

The comparison of wage costs per employee leaves us with no clear results. Instead, erratic movements in the estimates over time suggest that these are mostly due to statistical noise rather than underlying trends.

For labour productivity, we find that companies in other industries than manufacturing and services, and companies that hire VPs with technical educational degrees, do well relative to other companies. Those that hire VPs with an educational background in arts and humanities, and those in the service industry, are characterised by the most negative estimates.

FIGURE 5.10.a: Average wage cost per employee (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

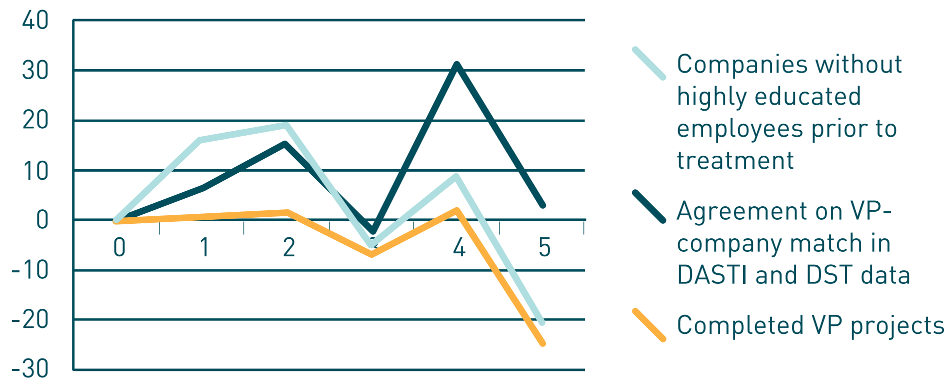


FIGURE 5.10.b: Average wage cost per employee (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

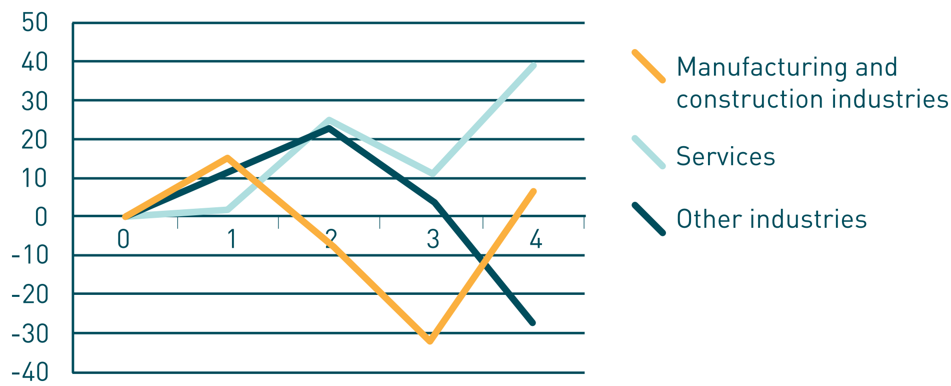


FIGURE 5.10.c: Average wage cost per employee (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

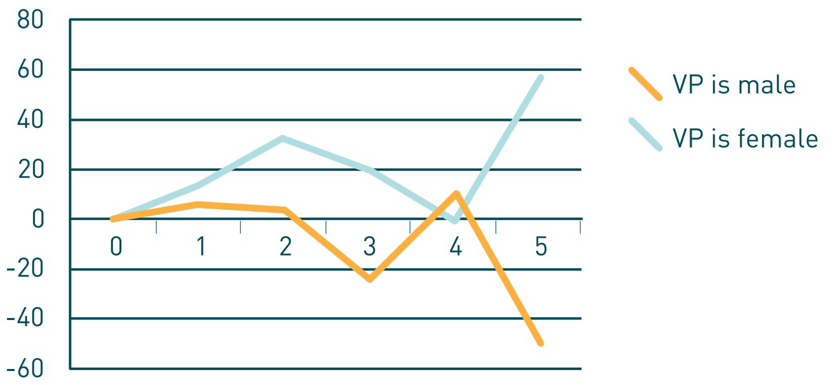


FIGURE 5.10.d: Average wage cost per employee (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

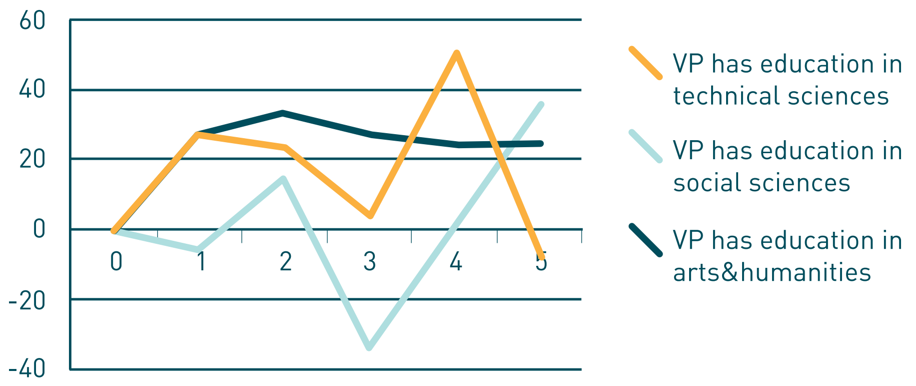


FIGURE 5.11.a: Labour productivity (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

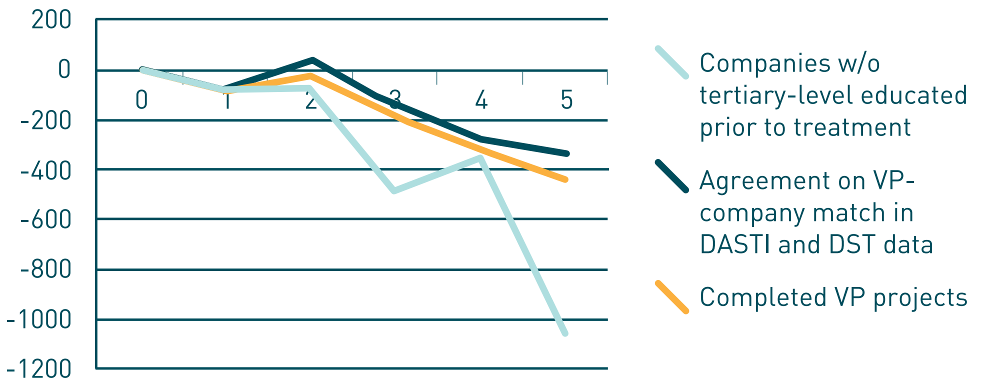


FIGURE 5.11.b: Labour productivity (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

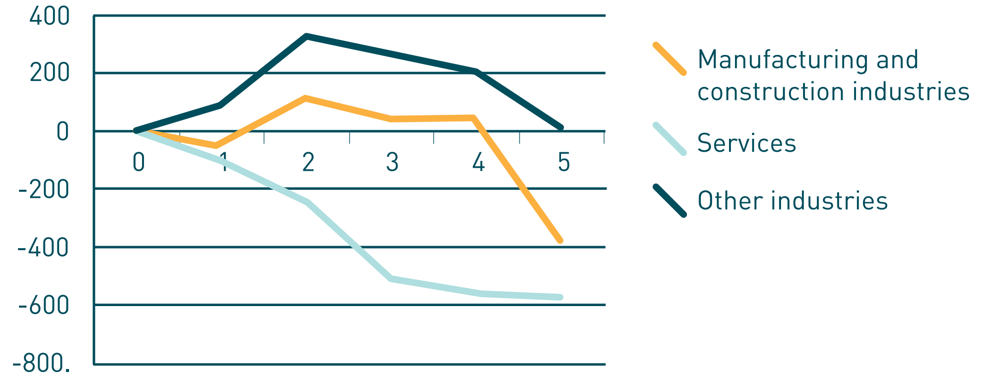


FIGURE 5.11.c: Labour productivity (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.

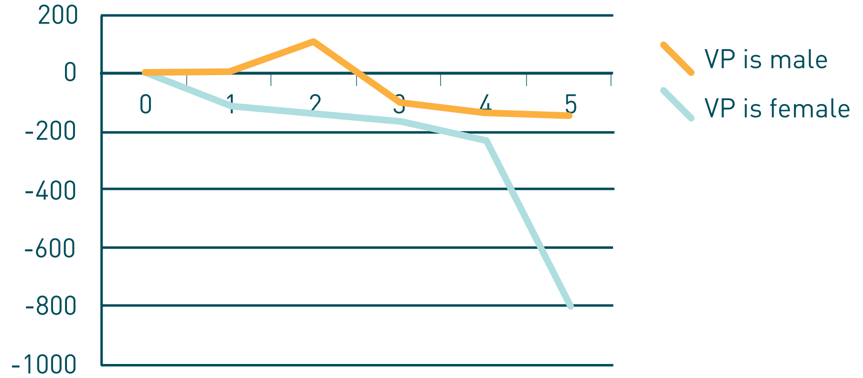
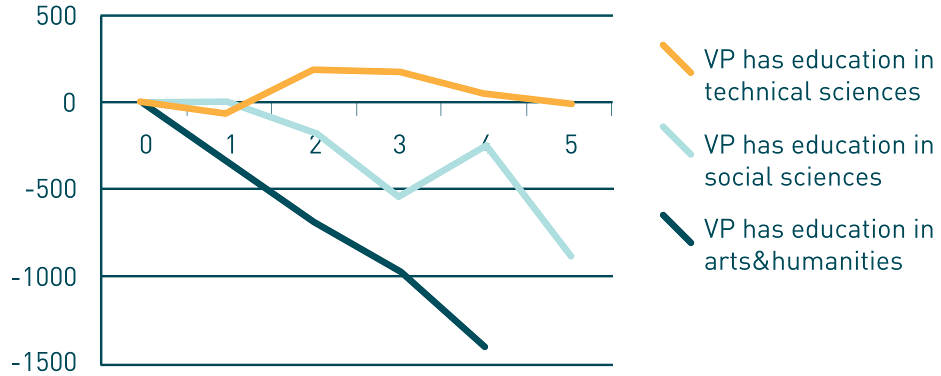


FIGURE 5.11.d: Labour productivity (DKK1,000). Estimated potential treatment effects. Years after year zero on horisontal axis.



6. EXTENSIONS

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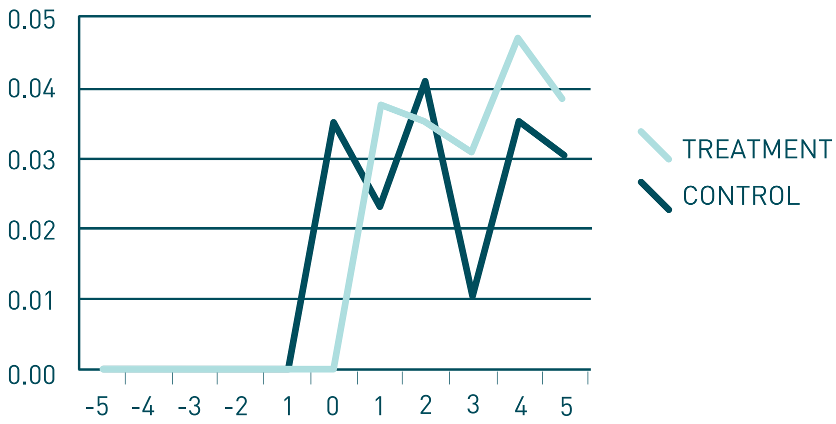
The survival of VP-companies

As a first extension of the analysis, we look at the survival/closure rate of VP-companies in comparison with the reference group of control companies. This is achieved by simply comparing closure rates as depicted in Figure 6.1 and an estimation of a binary choice model which has company closure in a given year as its dependent variable.[36] The results of this regression are displayed in TABLE 6.1 and corroborate the finding that there are no significant differences between companies that hire VPs and other similar companies that do not participate in the programme.

FIGURE 6.1: Company closure rates, by year after year 0 (horizontal axis).



[36] Closure is measured between year t and year t+1, where year t is the last year in which the company is found in the Experian database. The Experian database has information on the status of companies that allow distinguishing company closures from, for example, company sales or mergers.

TABLE 6.1: Comparison of company closure probabilities of VP-companies and companies in the reference group. Logit binary choice regression results. Dependent variable: bankruptcy after t=x.				
Dependent variables (in first differences):	All companies in treatment and control group		Companies with less than 50 employees	
	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.54	0.49	0.67	0.51
Treat=1 & t=2	-0.15	0.47	-0.20	0.50
Treat=1 & t=3	1.11	0.83	1.09	0.83
Treat=1 & t=4	0.30	0.61	0.41	0.66
Treat=1 & t=5	0.25	0.78	0.29	0.78
t=1 (omitted category)				
t=2	0.43	0.51	0.48	0.55
t=3	-1.33	0.82	-1.17	0.83
t=4	0.12	0.61	0.01	0.67
t=5	0.93	0.76	1.05	0.78
Year dummies				
2005 (omitted category)				
2006	0.13	1.17	0.13	1.17
2007	1.32	1.07	1.24	1.08
2008	2.24	1.05	2.16	1.05
2009	0.98	1.07	0.99	1.08
2010	-0.04	1.13	-0.26	1.15
Constant	-4.82	1.05	-4.816	1.054
Number of observations:	1987		1876	
Pseudo-r-squared	0.08		0.08	

A comparison of *VP-companies* and companies participating in *Innovation Networks*

For one of the extensions of the analysis, DASTI provided data on companies that have participated in the so-called *Innovation Networks*. These networks or clusters are financially supported by DASTI and have the purpose of increasing knowledge diffusion by providing a platform for collaborations between companies, knowledge institutions and other cluster participants.

These data consist of 1923 observations belonging to 1158 companies, the discrepancy owing itself to the fact that some firms participate in these networks more than once. In the following, these companies' *(IN-companies)* performance is compared with the performance of the VP-companies.

First, we compare developments in some of the performance variables between IN- and VP-companies. This comparison is highly informal since the two groups of companies differ in observable characteristics and must be assumed to differ in unobservable characteristics as well.

The left hand side columns of TABLE 6.2 compare VP-companies with all IN-companies present in the Experian data that participated in the clusters after 2004. We initially find that IN companies are on average significantly larger and have more highly educated employees than VP-companies. Also, a larger share of the IN-companies are in the IT industry.

To increase the comparability of the two groups of companies for the subsequent comparisons, only companies with a net income between DKK -7 million and DKK 7 million and a maximum size of 50 employees in the year before treatment (which is roughly the 99% percentile of the VP-companies' distribution of this variable) are considered.

Summary statistics of the adjusted sample used for the statistical comparison are in the right hand side columns of TABLE 6.2. The adjustments in terms of company size and profit have made the two groups of companies surprisingly similar in their observable characteristics in the year before treatment, with the exception that IN-companies are characterised by a higher share of highly educated employees.

The results of the new comparison are shown in TABLE 6.3 and are in concordance with earlier findings based on the comparison of VP-companies with a reference group of highly similar companies: VP-companies increase their numbers of highly educated employees in the year of treatment and sometimes in the first years after treatment.

However, it cannot be shown that VP-companies grow faster than IN-companies in the number of employees. On the contrary, they appear to have lower growth, i.e. shrink faster, than IN-companies more than three years after treatment. Additional regressions (not shown) further indicate that this finding becomes even more accentuated when considering percentage point employment growth rather than absolute increases in the number of employees.

Findings also suggest that VP-companies have a lower growth in value added and net income, but these findings are generally not statistically significant. VP-companies have wage developments and labour productivity (turnover/employees) developments approximately equal to the group of IN-companies in most years after treatment and higher in single years.

In sum, earlier findings that VP-companies do not have statistically significant higher increases in the set of financial success variables relative to the reference group of highly similar companies are replicated in the comparison with a sample of small companies that have participated in an Innovation Network.

TABLE 6.2: Summary statistics of companies participating in Innovation Networks (IN) vs. VP-companies, in year t=0.

	Raw data				Comparison sample[1]			
	VP-companies N=314		IN-companies N=828		VP-companies N=297		IN-companies N=479	
	Mean	Std.	Mean	Std.	Mean	Std.	Mean	Std.
Number of highly educated employees	2.42	2.42	58.98	239.96	1.72	2.19	4.34	6.54
Number of employees	17.45	17.45	246.88	1043.84	11.22	11.07	14.12	13.71
Turnover (DKK1,000)	43682.70	43682.70	598268.60	3897574.00	19600.08	40120.67	24178.86	29720.16
Value added (DKK1,000)	7896.05	7896.05	130868.60	793678.70	5234.49	5547.89	7304.33	8648.45
Net income (DKK,1,000)	2129.82	2129.82	26234.95	591004.40	357.50	1311.80	213.33	1871.56
Return on assets	0.28	0.28	-0.06	0.52	0.03	0.24	-0.07	0.55
Labour productivity (DKK1,000)	4898.19	4898.19	2063.51	2676.78	2175.00	5010.00	1821.75	1793.93
Wage cost per employee (DKK1,000)	218.67	218.67	477.04	387.54	396.27	220.95	447.00	205.25
Industry: Construction	0.23	0.23	0.02	0.14	0.06	0.23	0.01	0.11
Industry: Trade	0.41	0.41	0.18	0.38	0.23	0.42	0.20	0.40
Industry: IT	0.29	0.29	0.13	0.33	0.10	0.30	0.15	0.35
Industry: Manufacturing	0.25	0.25	0.06	0.24	0.07	0.26	0.05	0.21
Industry: Metal	0.22	0.22	0.02	0.15	0.05	0.21	0.02	0.15
Industry: Furniture	0.21	0.21	0.06	0.23	0.04	0.20	0.05	0.21
Industry: Service	0.19	0.19	0.02	0.15	0.04	0.19	0.02	0.14
Industry: Business service	0.23	0.23	0.03	0.16	0.06	0.23	0.04	0.20
Industry: Consulting	0.32	0.32	0.11	0.31	0.12	0.33	0.13	0.34
Industry: Wood/paper	0.18	0.18	0.03	0.18	0.03	0.16	0.04	0.19
Industry: Other	0.42	0.42	0.35	0.48	0.22	0.41	0.29	0.45

Notes: The comparison sample consists of companies with maximum 50 employees and net income between DKK 7 million and DKK 7 million in year zero.

TABLE 6.3: Diff-in-diff fixed effects regression results for VP- and IN-companies. Companies with up to 50 employees in year zero.

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.508***	0.17	0.24	0.29	174.4	214.2
Treat=1 & t=2	0.33	0.22	-0.19	0.35	-455.1*	255.5
Treat=1 & t=3	0.502*	0.28	-0.53	0.44	-55.7	315.7
Treat=1 & t=4	-0.34	0.35	-1.285**	0.55	-156.5	386.7
Treat=1 & t=5	0.96	0.87	-0.91	0.80	-15.4	491.2
t=1	-0.05	0.13	0.02	0.22	255.2	165.7
t=2	0.07	0.18	0.29	0.29	522.9**	218.3
t=3	-0.506**	0.25	0.31	0.40	328.4	290.7
t=4	0.41	0.32	0.873*	0.51	855.2**	373.0
t=5	-0.68	0.84	0.57	0.76	786.4	487.8
Year dummies						
2005	0.15	0.10	0.07	0.18	-211.1	143.1
2006	0.08	0.10	0.347*	0.18	50.5	148.6
2007	0.173*	0.10	0.372**	0.19	-191.7	151.5
2008	-0.07	0.11	0.01	0.20	-662.9***	162.3
2009	-0.391***	0.13	-1.342***	0.24	-1652***	189.8
2010			-1.115***	0.33	-1252***	239.2
2011			-0.72	0.99	498.1	663.0
Constant	0.052	0.07	0.572***	0.13	731.9***	104.6
Number of observations:	3208		3706		4127	
R-squared	0.03		0.06		0.05	
Number of companies:	698		743		754	

Notes: Only observations with annual changes in the number of employees by less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
-433.8	760.0	-0.0575**	0.03	-17.41	16.00	92.24	234.40
-110.1	904.9	0.00	0.03	-8.09	18.91	127.60	298.60
-279.2	1116.0	0.01	0.04	14.72	23.58	987.1***	375.40
-137.9	1365.0	-0.123**	0.05	11.02	28.84	-108.80	465.40
-580.6	1730.0	-0.09	0.06	10.40	41.93	425.60	1855.00
687.5	581.8	0.02	0.02	-9.66	11.81	13.37	171.60
413.5	767.8	-0.01	0.03	5.65	15.32	21.82	239.80
486.4	1025.0	0.02	0.04	-21.34	21.11	-1073***	326.80
352.7	1313.0	0.04	0.05	19.84	26.66	281.20	416.30
844.3	1717.0	0.05	0.06	-11.45	39.18	-661.30	1819.00
-41.2	506.2	-0.01	0.02	1.93	9.26	-138.40	124.40
-883.1*	523.9	-0.03	0.02	-6.76	9.64	-172.50	129.00
-413.5	533.2	-0.0569***	0.02	8.95	9.91	-331.2**	132.00
-944.5*	571.1	-0.0567***	0.02	4.42	10.62	-236.2*	141.90
-980.4	665.2	-0.0993***	0.02	-1.30	12.38	-202.40	166.60
-663.4	840.9	-0.04	0.03	12.71	16.92		
574.5	2357.0	0.04	0.08	28.23	49.73		
46.950	368.7	0.0216*	0.01	3.75	6.81	116.20	89.61
4224		4222		1917		2301	
0.0		0.02		0.02		0.01	
769		764		515		459	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

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A comparison of VP-companies and an extended sample of control companies

Earlier findings were characterised by a large heterogeneity in the companies' financial success variables. This argues for a test of the robustness of earlier findings by running the same regressions on a larger control group of companies. This reduces the variance of the estimators but comes at the cost of lower similarity between the group of treatments and the group of controls.

In the following, we depart from the propensity scores calculated earlier and select five controls (instead of one) for each treatment into the control group. This time, matching is based purely on the propensity score, without additional conditions on industry etc.

This procedure selects 1,596 companies into the control group for the 318 participant companies. The similarity of the two groups can be assessed by inspecting TABLE 6.4. As expected, the two groups are not as similar as the sample of the earlier analysis, with e.g. slightly larger companies in the extended control group. However, the conditional diff-in-diff model still allows for a meaningful comparison between the two groups of companies, and its estimates should be less affected by statistical noise thanks to an increase in the sample size.

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TABLE 6.4: Summary statistics of VP-companies and companies in the extended control group (5 controls per treatment). Companies with up to 50 employees.				
	VP-companies N=300		Companies in the extended control group N=1,488	
	Mean	Std.	Mean	Std.
Number of highly educated employees	1.80	2.26	1.78	2.62
Number of employees	11.65	11.36	11.16	11.86
Turnover (DKK1,000)	18437.84	27033.64	18103.18	25721.50
Value added (DKK1,000)	5540.32	5739.63	5249.33	6234.30
Net income (DKK,1,000)	296.99	981.43	236.41	1139.21
Return on assets	0.04	0.21	0.03	0.23
Labour productivity (DKK1,000)	2165.72	4973.50	2037.00	3856.43
Wage cost per employee (DKK1,000)	395.80	220.14	397.12	390.80
Industry: Construction	0.06	0.23	0.04	0.20
Industry: Trade	0.22	0.41	0.22	0.41
Industry: IT	0.10	0.30	0.09	0.29
Industry: Manufacturing	0.07	0.25	0.05	0.21
Industry: Metal	0.05	0.21	0.04	0.20
Industry: Furniture	0.06	0.23	0.05	0.22
Industry: Service	0.03	0.18	0.04	0.19
Industry: Business service	0.06	0.24	0.07	0.26
Industry: Consulting	0.12	0.33	0.13	0.34
Industry: Wood/paper	0.03	0.16	0.03	0.18
Industry: Other	0.21	0.41	0.23	0.42

The results of the new comparisons are in TABLE 6.5. We find that increasing the sample size only marginally reduced the standard errors of the estimates, and that the results of this model do not alter the previous findings that VP-companies do in general not experience statistically significant positive developments in the financial success variables. However, for return on assets, positive (though insignificant) signs of the relevant coefficient estimates imply that the previous finding of treatment companies that experience lower growth in this variable in association with treatment is not robust to changes in how the control group is selected. Also, there are weak signs that participants experience higher growth in wage costs and value added, and lower growth in labour productivity.

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TABLE 6.5: Diff-in-diff fixed effects regression results for VP-companies and companies in the extended control group (5 controls per treatment). Companies with up to 50 employees in year zero.

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.510***	0.10	0.621***	0.21	235.8	145.6
Treat=1 & t=2	0.451***	0.13	0.25	0.27	33.7	172.7
Treat=1 & t=3	0.06	0.15	0.11	0.28	-47.5	190.2
Treat=1 & t=4	-0.01	0.16	-0.16	0.41	497.2**	251.8
Treat=1 & t=5	0.19	0.26	-0.975*	0.57	-325.1	331.9
t=1	-0.03	0.05	-0.17	0.12	63.3	82.1
t=2	-0.10	0.07	-0.23	0.16	51.2	108.8
t=3	-0.179**	0.09	-0.29	0.21	159.6	142.4
t=4	-0.09	0.12	-0.31	0.25	-11.5	174.6
t=5	-0.27	0.18	0.20	0.31	164.0	221.6
Year dummies						
2005	0.182***	0.05	0.455***	0.17	-11.5	87.3
2006	0.173***	0.05	0.781***	0.17	302.9***	97.1
2007	0.180***	0.07	0.645***	0.20	87.2	112.2
2008	0.02	0.08	0.614***	0.22	-343.7***	131.7
2009	0.00	0.10	-0.632**	0.26	-925.4***	168.1
2010			-0.51	0.34	-633.2***	189.9
2011			-2.452***	0.93	661.4	577.3
Constant	-0.045	0.04	0.036	0.15	304.5***	77.4
Number of observations:	7130		8088		8945	
R-squared	0.03		0.07		0.05	
Number of companies:	1633		1706		1709	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
-85.9	85.2	-0.02	0.03	3.64	10.79	-301.50	248.00
-2.1	103.3	0.06	0.07	16.17	12.21	-251.70	304.70
-100.0	123.6	0.06	0.04	24.43*	13.01	-555.7**	272.90
29.7	152.0	0.00	0.05	29.97**	14.87	-126.40	205.40
-199.6	204.0	0.01	0.06	28.57	21.65	-282.50	493.90
13.0	51.4	-0.0248*	0.02	6.08	7.36	334.80	248.30
13.9	67.6	-0.08	0.06	0.49	8.82	414.90	349.10
28.8	83.4	-0.0555**	0.02	2.75	11.57	491.6**	222.50
-72.2	93.5	-0.0770***	0.03	5.91	14.76	272.50	188.70
152.1	108.8	-0.07	0.04	11.66	18.71	66.82	430.30
114.7	69.8	0.0444**	0.02	-7.87	6.26	-189.0*	105.80
129.1*	74.9	0.0513**	0.02	-4.08	6.95	-183.60	152.80
7.5	74.8	0.03	0.02	1.10	8.42	-314.50	259.40
-174.4**	86.0	0.01	0.02	-16.05	11.75	-365.6***	140.70
-221.3**	94.5	-0.0436*	0.03	-5.68	13.48	-272.3*	149.80
23.5	109.4	0.04	0.03	-17.47	18.33		
870.7***	289.8	0.11	0.08	82.72**	33.91		
-26.130	63.0	-0.0326***	0.01	3.15	4.84	67.26	77.79
7609		9094		3106		4500	
0.0		0.01		0.02		0.00	
1494.0		1748.00		1051.00		960.00	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

7. CONCLUSIONS

This study has taken a look at the potential effects of the Danish Innovation Assistant Programme ('Videnpilotordningen', VP programme) on the individual and company level. For this purpose, the analysis considers the employment probabilities and salary developments of individuals participating in the programme (VPs) and follows a number of performance variables for participating companies.

To form an understanding of the absolute potential effects of the programme, we compare participating individuals and companies with highly similar individuals and companies that do not participate. These comparisons indicate that:

(a) Individuals who participate in the programme have higher employment probability than similar control individuals in the year after starting to participate. This is no surprise, since employment is a defining element of the programme.

(b) Individuals who participate in the programme do not have higher employment probability than controls more than one year after starting to participate in the programme, but earn higher wages in the first years. Here it should be noted that the observation period falls within an economic boom period with low unemployment. It might be assumed that the wage and employment developments of programme participants and non-participants do not converge at the same speed in the current economic slow-down.

(c) Participating companies increase their numbers of highly educated employees in association with programme participation. The analysis finds no signs of behavioural additivity of the programme, i.e. non-participants increasing their number of highly educated employees. There are no indications that companies continue to increase the number of highly educated employees in the years after programme participation.

(d) Participating companies increase the number of employees in association with programme participation. However, in this case there are also no indications that the companies continue to increase their employment in the years after programme participation.

(e) It is difficult to detect statistically significant positive potential effects of the programme on participating companies' financial performance variables. For subsamples of small companies that do not experience large year-to-year changes in employment or financial measures, participant companies on average increase their gross profit and net income in association with programme participation. Findings are again not statistically significant, and need to be interpreted with care.

(f) Participating companies do not experience increases in return on assets, wage costs per employee, or labour productivity in association with programme participation.

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There are no strong findings about which particular projects are more successful than others, but it appears that VPs with a tertiary-level education gain less from participation than VPs with a post-secondary education, while females and VPs finding employment in service industries gain the most.

For companies, it is important to note that results related to specific characteristics of the VP or the hosting company are tentative, due to the presence of substantial statistical uncertainties. This said, one can note that the largest potential effect on the number of highly educated employees is estimated for small companies that hire VPs with a technical sciences education as well as male VPs, and for companies that had no tertiary-level educated employees before treatment.

Also, small companies in manufacturing do well in terms of value added and net income (profits) developments in association with programme participation, while participant companies that hire female VPs do relatively poorly in terms of value added and employment, but not net income. Companies that hire VPs with an educational background within arts and humanities are characterised by low growth in association with programme participation, while those hiring VPs with a technical sciences education do the best, not just in terms of increasing the number of highly educated employees, but also with regard to net income, return on assets and labour productivity developments.

The general finding that it is difficult to measure statistically significant potential effects of the programme proved to be robust to comparing participant companies with other companies that participated in a similar programme administered by DASTI (the *Innovation Network* programme) as well as an alternative control group consisting of several highly comparable control companies for each participant company.

The VP programme has been analysed earlier on the basis of less extensive data. This earlier study found potential effects of similar size to the present study. However, it also found large unexplained year-to-year variation in the performance variables, leading to statistically insignificant coefficient estimates.

The current analysis supports the earlier analysis' findings. But the fact that it is still difficult or impossible to establish statistical significance for most of the relevant financial variables implies that we still cannot be certain that increased company performance is a general feature of the programme.

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So while there are indications of positive potential programme effects for restricted subsamples in our data, the general lack of statistical significance implies that any positive effects of hiring a VP on company performance are small in the face of the high data demands of our econometric model, a still very limited number of observations in our data, and the large variation in the companies' performance measures. The latter observation also suggests that other company developments, for example initiated by product developments, must be assumed often to be of major importance relative to the presence of a VP in the company.[37]

[37] Fox, J.T., V. Smeets, 2011, Does Input Quality Drive Measured Differences In Firm Productivity?, International Economic Review, vol. 52(4), 961-989.

TABLE A.1: Comparison between VP-companies and companies in the reference group. All companies irrespective of outliers. Diff-in-diff fixed effects regression results

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.289*	0.17	0.14	0.74	48.9	302.4
Treat=1 & t=2	0.16	0.15	0.25	0.52	73.5	244.8
Treat=1 & t=3	-0.17	0.20	0.63	0.55	66.4	313.4
Treat=1 & t=4	-0.380*	0.23	-0.30	0.86	-43.8	473.8
Treat=1 & t=5	0.29	0.70	-0.88	1.41	-558.7	452.6
t=1	-0.09	0.12	-0.41	0.45	78.8	246.0
t=2	-0.07	0.15	-0.51	0.63	-394.6	288.3
t=3	-0.23	0.20	-0.60	0.80	-103.4	418.1
t=4	-0.05	0.25	-0.99	1.09	-13.9	451.8
t=5	-0.59	0.64	0.68	1.35	109.0	521.9
Year dummies						
2003	0.08	0.15	-0.33	0.53	-382.7	494.3
2004	0.02	0.11	0.49	0.52	219.7	492.3
2005	0.02	0.12	0.47	0.55	361.8	506.6
2006	0.17	0.16	1.185*	0.61	446.4	522.5
2007	0.14	0.18	0.56	0.74	251.4	571.0
2008	0.00	0.23	0.22	0.89	-207.5	592.5
2009	-0.10	0.22	-2.647**	1.07	-1024.0	654.6
Constant	0.12	0.10	0.53	0.50	338.2	481.1
Number of observations:	3046		2989		3664	
Number of companies:	596		580		611	
R-squared	0.02		0.08		0.03	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
-52.7	124.2	-0.0580*	0.03	44.45	53.58	-664.00	680.20
12.3	162.3	0.05	0.04	-20.82	26.08	436.50	318.80
-53.7	221.0	0.00	0.04	16.87	23.68	125.50	315.90
-476.7	336.1	-0.04	0.06	30.80	25.34	-62.61	310.20
185.5	267.5	-2.39	2.38	44.40*	26.05	-71.96	593.80
35.8	132.2	0.02	0.04	3.72	16.63	821.50	814.70
57.8	169.7	-0.01	0.06	38.96	28.42	172.30	481.80
69.6	228.6	0.02	0.08	25.52	28.06	273.40	588.60
299.2	276.1	0.04	0.06	-16.65	24.65	209.60	614.10
10.5	269.5	0.09	0.17	-25.41	31.03	178.50	833.40
-333.5	259.2	0.19	0.13	10.05	12.95	-159.60	660.90
-29.1	253.6	0.16	0.11	-1.59	13.24	-53.96	524.50
54.4	257.4	0.14	0.09	-17.10	19.86	-240.10	508.60
0.3	278.0	0.12	0.08	-28.85	28.64	117.90	601.20
-152.3	291.4	0.147**	0.07	-32.51	39.48	-142.80	663.30
-468.2	308.2	0.05	0.07	-39.87	42.35	-331.50	813.00
-620.8*	326.0	0.22	0.19	9.14	25.26	227.80	673.60
177.9	246.3	-0.120***	0.04	8.19	12.37	-267.80	477.70
3799		3867		3107		2856	
626		627		588		567	
0.02		0.01		0.01		0.01	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

TABLE A.2: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results. Only companies with agreement on the VP-company-match in the DASTI and DST data.

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.516***	0.15	0.57	0.37	-23.2	234.1
Treat=1 & t=2	0.283*	0.16	-0.10	0.40	413.1	257.1
Treat=1 & t=3	-0.06	0.20	0.42	0.42	127.0	366.9
Treat=1 & t=4	-0.34	0.24	-0.981*	0.54	-0.2	521.9
Treat=1 & t=5	-0.26	0.29	-0.96	0.77	-768.1	682.9
t=1	0.00	0.11	0.14	0.31	102.3	216.2
t=2	0.02	0.15	0.16	0.42	-248.9	275.6
t=3	-0.16	0.21	0.08	0.53	171.8	412.0
t=4	-0.07	0.24	0.64	0.71	443.6	453.0
t=5	-0.02	0.32	1.549*	0.90	475.8	663.0
Year dummies						
2003	-0.01	0.15	-0.18	0.26	-371.0	257.6
2004	-0.01	0.15	-0.18	0.26	-371.0	257.6
2005	0.02	0.13	0.15	0.31	401.6	257.2
2006	0.05	0.12	0.08	0.33	98.9	239.8
2007	-0.02	0.15	0.15	0.40	343.1	283.5
2008	0.01	0.18	0.15	0.47	272.3	354.3
2009	-0.15	0.22	-0.14	0.61	-270.2	413.8
Constant	0.10	0.10	0.34	0.25	95.9	203.6
Number of observations:	1632		1697		1627	
Number of companies:	289		294		290	
R-squared	0.05		0.09		0.04	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
-67.7	114.2	-0.04	0.03	6.42	13.33	-80.61	124.30
208.0	131.1	-0.0656**	0.03	8.99	12.07	125.30	130.80
191.4	140.0	0.01	0.03	-17.89	15.11	-178.3*	105.30
258.7	256.9	-0.02	0.05	34.01*	19.80	-144.90	161.80
-104.8	222.2	-0.08	0.07	-28.68	34.35	-54.19	241.70
-29.0	113.3	-0.02	0.03	-12.53	13.70	-95.96	112.50
-277.0**	137.1	-0.04	0.03	-1.81	12.21	114.80	120.10
-81.9	177.0	-0.0709*	0.04	3.28	16.46	141.00	128.00
-177.5	252.0	-0.07	0.05	-26.61	22.88	48.66	152.70
-32.8	292.2	-0.113*	0.06	-12.68	31.07	122.60	262.00
-129.6	107.5	-0.0597**	0.03	13.46*	7.06	-175.6**	87.28
-129.6	107.5	-0.0597**	0.03	13.46*	7.06	-175.6**	87.28
99.0	101.3	0.02	0.02	13.24	8.06	-35.98	71.15
5.3	94.2	-0.01	0.02	13.22	9.19	-8.17	90.36
50.3	117.5	0.01	0.03	10.87	10.59	21.71	111.20
104.4	147.7	0.03	0.03	20.12	14.66	-77.02	117.50
-71.4	185.9	0.01	0.04	4.24	17.45	-154.30	145.40
66.0	74.9	0.02	0.02	-3.65	5.68	63.03	65.53
1579		1658		978		1052	
291		292		202		178	
0.02		0.03		0.03		0.04	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

TABLE A.3: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results. Only companies with completed VP-projects.

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.555***	0.13	0.708**	0.33	273.2	245.3
Treat=1 & t=2	0.429***	0.16	0.26	0.34	374.5	255.4
Treat=1 & t=3	0.00	0.18	0.63	0.42	278.5	364.8
Treat=1 & t=4	-0.24	0.22	-0.45	0.65	-85.2	489.9
Treat=1 & t=5	-0.25	0.28	-0.68	0.61	-678.6	626.9
t=1	-0.13	0.11	0.06	0.26	92.7	207.0
t=2	-0.15	0.15	-0.16	0.35	-210.3	242.4
t=3	-0.25	0.19	-0.51	0.43	11.4	372.8
t=4	-0.24	0.23	-0.31	0.58	441.7	419.4
t=5	-0.16	0.29	0.84	0.58	571.3	581.9
Year dummies						
2003	-0.04	0.14	-0.04	0.23	-255.3	245.7
2004	0.02	0.12	0.22	0.27	390.3	242.5
2005	0.05	0.11	0.26	0.29	103.4	234.2
2006	0.06	0.14	0.21	0.34	279.2	260.6
2007	0.12	0.18	0.44	0.36	269.8	302.5
2008	-0.03	0.20	0.22	0.45	-226.3	347.1
2009	-0.01	0.24	-1.304**	0.52	-749.3*	429.8
Constant	0.12	0.10	0.38	0.23	204.9	207.3
Number of observations:	2122		2217		2120	
Number of companies:	431		440		359	
R-squared	0.04		0.08		0.04	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

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Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
-58.8	107.2	-0.03	0.03	0.71	11.04	-72.72	114.10
228.1*	122.4	-0.03	0.03	0.47	11.92	52.31	140.10
148.0	124.9	0.01	0.03	-7.51	14.10	-163.7*	97.86
112.0	236.1	-0.04	0.04	8.34	19.07	-134.50	145.80
-134.4	195.4	-0.04	0.07	-26.68	31.65	-121.00	258.60
25.5	97.8	-0.02	0.02	3.99	11.48	-68.50	107.60
-142.4	116.1	-0.04	0.03	21.19*	12.35	73.53	116.40
45.2	146.5	-0.05	0.03	34.80**	16.25	88.85	115.30
123.6	216.8	-0.03	0.04	27.78	21.93	-7.39	144.10
271.5	240.7	-0.08	0.06	25.04	30.60	13.19	242.40
-129.2	101.3	-0.0530**	0.03	15.23**	6.89	-60.82	55.73
36.0	95.8	0.00	0.02	7.18	7.80	8.70	57.82
49.0	92.5	0.01	0.02	3.37	7.01	80.78	62.20
-15.1	106.1	0.00	0.03	-2.78	9.59	166.4*	95.99
-8.6	126.9	0.02	0.03	-7.74	13.91	3.10	104.70
-239.4	149.1	-0.02	0.04	-23.69	16.81	-82.71	116.70
-392.2**	184.5	-0.01	0.04	-16.50	20.86	39.04	133.70
105.5	78.6	0.01	0.02	3.29	6.02	-19.71	49.68
2084		2174		1175		1307	
438		439		274		386	
0.03		0.02		0.02		0.03	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

TABLE A.4: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results. VPs in companies without employees with a tertiary education prior to programme participation.

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.690***	0.12	0.35	0.37	52.5	255.3
Treat=1 & t=2	0.426***	0.16	0.02	0.41	449.0	316.4
Treat=1 & t=3	0.00	0.19	0.41	0.47	79.6	332.1
Treat=1 & t=4	-0.19	0.24	-0.13	0.73	181.5	594.6
Treat=1 & t=5	-0.27	0.34	-1.05	0.91	-478.5	729.5
t=1	0.02	0.10	0.27	0.31	132.5	218.4
t=2	0.03	0.13	0.03	0.43	-251.3	293.7
t=3	0.08	0.15	0.17	0.54	296.5	359.3
t=4	0.07	0.20	0.50	0.74	485.9	498.9
t=5	0.10	0.28	1.512*	0.85	112.8	625.3
Year dummies						
2003	0.07	0.10	-0.16	0.38	-475.2*	286.9
2004	0.09	0.10	0.33	0.41	117.2	270.9
2005	0.07	0.08	0.42	0.43	34.4	263.7
2006	0.05	0.11	0.46	0.49	307.5	321.8
2007	0.00	0.13	0.33	0.51	181.1	351.2
2008	-0.11	0.15	-0.08	0.61	-509.3	379.6
2009	-0.26	0.17	-2.371***	0.68	-1222***	453.6
Constant	0.03	0.07	0.33	0.36	327.3	240.6
Number of observations:	1711		1716		1671	
Number of companies:	347		348		342	
R-squared	0.07		0.12		0.07	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
58.0	126.6	0.00	0.03	15.72	13.69	9.63	91.46
155.5	145.1	-0.02	0.03	3.22	12.19	170.70	115.10
95.1	148.7	-0.01	0.03	-24.15	16.50	-58.23	95.90
114.6	284.5	-0.03	0.05	14.08	21.63	-37.22	140.10
-17.5	310.1	0.00	0.07	-29.47	36.69	-74.40	226.50
16.3	113.6	0.00	0.03	-15.93	15.31	-80.51	91.12
-124.6	121.6	-0.03	0.03	0.61	14.47	76.28	103.30
141.5	155.4	-0.02	0.03	19.23	18.95	101.50	108.50
161.9	253.2	0.00	0.04	-3.87	25.53	-108.10	129.00
126.8	271.7	-0.135**	0.06	0.94	34.50	51.62	236.80
-124.8	97.9	0.01	0.02	14.56*	7.53	-167.1*	89.11
-34.4	106.1	0.01	0.02	2.25	8.26	-60.30	66.77
-9.4	88.2	0.01	0.02	0.58	10.18	-73.07	92.82
-92.3	113.0	0.02	0.03	6.07	12.99	13.41	99.39
-86.0	127.4	-0.03	0.03	4.48	16.76	-107.60	116.80
-372.3**	149.0	-0.01	0.03	-4.13	20.49	-168.70	132.30
-551.5***	191.3	0.00	0.04	7.49	23.38	-81.89	134.90
161.7**	76.6	0.00	0.02	3.07	7.30	83.71	71.79
1620		1686		1010		1168	
345		347		230		215	
0.04		0.03		0.02		0.04	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

TABLE A.5: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results. Companies in manufacturing industries and contruction.

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.675***	0.20	0.61	0.85	383.2	506.1
Treat=1 & t=2	0.37	0.29	-0.47	0.79	623.6	610.9
Treat=1 & t=3	-0.25	0.27	0.40	0.90	1010.0	760.4
Treat=1 & t=4	-0.18	0.43	-0.10	1.29	220.3	1454.0
Treat=1 & t=5	-0.14	0.49	-1.61	1.36	-2228.0	3169.0
t=1	-0.07	0.15	0.39	0.64	161.8	433.3
t=2	-0.22	0.22	0.29	0.86	-117.0	530.1
t=3	-0.03	0.21	1.09	1.05	385.7	709.5
t=4	-0.28	0.37	0.95	1.37	958.8	1008.0
t=5	0.02	0.23	3.974***	1.23	2767.0	2760.0
Year dummies						
2003	-0.11	0.27	0.45	0.72	-438.8	614.6
2004	0.09	0.25	0.91	0.82	480.5	598.5
2005	-0.05	0.21	1.43	0.88	212.1	617.4
2006	-0.07	0.22	1.40	0.99	987.6	660.3
2007	-0.04	0.27	1.06	0.97	491.2	686.3
2008	-0.07	0.27	0.14	1.24	-530.1	688.6
2009	-0.22	0.29	-3.382**	1.40	-1705**	845.5
Constant	0.15	0.20	-0.32	0.72	100.1	527.1
Number of observations:	643		667		640	
Number of companies:	125		128		126	
R-squared	0.07		0.17		0.11	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

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Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
0.2	200.4	-0.04	0.04	15.16	15.91	-48.80	102.20
67.4	226.3	-0.04	0.04	-21.46	14.91	165.80	101.60
378.3	257.3	-0.05	0.05	-25.78	22.17	-76.95	135.60
159.7	695.7	-0.09	0.09	38.30	29.98	-5.01	240.00
-289.4	393.7	-0.08	0.08	-160.2*	82.57	-417.10	409.70
176.8	172.5	-0.03	0.03	-19.52	15.82	-22.15	107.00
65.6	197.9	-0.04	0.04	-13.16	16.90	75.43	130.50
178.8	266.5	-0.05	0.05	7.90	23.95	26.12	184.40
274.7	560.0	-0.07	0.07	-42.36	27.50	39.84	244.60
0.0	0.0	-0.14	0.14	76.77	67.54	405.70	481.60
110.2	215.9	-0.03	0.03	14.69*	8.25	-45.30	68.40
100.8	231.5	-0.02	0.02	19.67**	9.03	49.33	69.99
139.5	173.5	-0.02	0.02	4.48	5.95	6.06	67.65
121.7	217.0	-0.03	0.03	29.13**	11.75	120.40	121.30
0.7	228.7	-0.03	0.03	34.80*	20.13	20.63	145.40
-294.3	272.4	-0.04	0.04	32.23	20.59	-74.83	194.10
-516.3	368.1	-0.05	0.05	31.22	24.77	-66.23	246.80
3.5	159.2	0.01	0.01	-7.64	5.32	24.76	64.36
612		660		463		532	
128		128		98		98	
0.04		0.05		0.07		0.05	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

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TABLE A.6: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results. Companies in service industries

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.27	0.17	0.619**	0.28	178.0	271.4
Treat=1 & t=2	0.24	0.20	0.11	0.41	157.0	267.0
Treat=1 & t=3	0.30	0.25	0.55	0.45	-75.1	413.6
Treat=1 & t=4	-0.16	0.29	-0.69	0.65	269.1	407.7
Treat=1 & t=5	-0.30	0.30	-0.45	0.69	-758.7	565.8
t=1	0.03	0.15	-0.18	0.25	-95.7	250.1
t=2	0.00	0.18	-0.35	0.34	-134.4	254.9
t=3	-0.34	0.25	-0.775*	0.43	115.0	416.6
t=4	-0.01	0.26	-0.46	0.62	511.8	469.1
t=5	0.00	0.29	-0.07	0.64	878.6*	448.3
Year dummies						
2003	0.15	0.15	0.07	0.18	-288.5	243.6
2004	0.23	0.14	0.23	0.24	307.4	225.5
2005	0.22	0.14	0.02	0.24	65.0	206.1
2006	0.18	0.16	0.23	0.30	163.7	243.8
2007	0.23	0.21	0.22	0.36	83.9	291.3
2008	0.13	0.23	0.43	0.38	-373.1	365.6
2009	0.03	0.26	-0.49	0.45	-796.4**	399.1
Constant	-0.04	0.12	0.433**	0.21	340.2*	199.4
Number of observations:	1434		1492		1430	
Number of companies:	300		304		293	
R-squared	0.02		0.06		0.04	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

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Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
-107.4	118.2	-0.04	0.03	1.60	22.15	-99.96	184.50
133.6	130.3	-0.0702*	0.04	23.14	19.69	-148.10	221.60
40.3	145.6	-0.0692*	0.04	-13.65	25.40	-255.60	172.20
161.7	224.9	-0.02	0.05	27.89	30.68	-53.12	255.70
65.6	236.3	-0.03	0.10	-5.02	41.22	-15.26	466.20
-76.4	111.0	-0.03	0.03	-18.10	22.37	-75.44	169.40
-178.6	131.9	-0.02	0.04	-2.07	21.77	162.60	177.30
-22.0	165.4	-0.03	0.04	-14.01	27.33	181.50	179.50
56.4	236.5	-0.05	0.05	-6.00	41.05	2.36	220.80
271.1	270.5	-0.10	0.08	-15.28	39.36	-113.40	356.90
-197.0*	118.3	-0.0709*	0.04	15.75	10.89	-176.80	116.90
179.0*	92.0	0.03	0.03	2.12	15.19	-32.71	121.00
65.3	103.4	0.01	0.04	14.59	16.26	49.76	124.00
100.1	118.0	0.03	0.04	4.45	16.37	126.20	157.40
32.1	134.5	0.02	0.04	18.34	23.56	-16.60	164.20
-105.6	156.8	0.00	0.05	2.49	29.14	-185.10	179.40
-335.3*	191.1	0.01	0.06	13.96	33.24	-5.86	187.10
59.4	86.5	0.01	0.03	1.98	12.72	13.71	107.70
1420		1444		634		722	
300		302		159		142	
0.05		0.03		0.03		0.03	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

TABLE A.7: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results. Companies in 'other' industries

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.670***	0.23	0.93	0.74	253.8	451.0
Treat=1 & t=2	0.44	0.33	0.30	0.64	541.6	546.5
Treat=1 & t=3	-0.42	0.38	-0.57	0.94	-548.7	581.5
Treat=1 & t=4	-0.11	0.37	-0.18	1.57	-656.4	839.1
Treat=1 & t=5	-0.03	0.65	-0.92	1.58	150.0	733.3
t=1	-0.04	0.23	-0.02	0.60	-70.8	416.4
t=2	0.07	0.27	-0.06	0.71	-467.1	616.7
t=3	0.28	0.39	-0.18	0.92	-210.0	640.7
t=4	0.06	0.53	0.43	1.21	-651.9	799.8
t=5	0.19	0.72	0.91	1.25	-1383.0	1119.0
Year dummies						
2003	-0.08	0.26	-0.61	0.47	-182.5	335.3
2004	-0.25	0.20	-0.01	0.53	57.0	434.0
2005	-0.35	0.23	-0.13	0.51	282.4	393.3
2006	-0.28	0.30	-0.44	0.63	-85.5	485.6
2007	-0.45	0.37	-0.24	0.82	324.9	612.0
2008	-0.806*	0.46	-0.61	0.96	-105.5	660.1
2009	-0.45	0.55	-2.684***	0.97	-257.5	906.3
Constant	0.374*	0.19	0.914**	0.44	233.3	298.5
Number of observations:	532		568		541	
Number of companies:	110		114		114	
R-squared	0.07		0.12		0.06	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
33.9	251.8	-0.02	0.05	10.97	14.59	91.45	142.50
101.2	298.5	-0.03	0.05	12.11	14.07	238.20	175.10
26.8	336.2	0.00	0.05	-18.98	19.72	-61.10	177.20
248.8	413.3	-0.09	0.09	-31.56	32.56	-63.79	152.20
24.6	241.0	-0.03	0.03	40.93	34.04	-188.00	264.70
154.2	237.0	0.01	0.05	2.13	19.13	59.35	144.00
-44.4	282.5	-0.06	0.07	18.42	17.63	21.04	162.50
140.6	318.4	-0.03	0.06	41.87	26.48	186.10	161.50
-266.0	432.8	-0.02	0.08	49.64	32.94	89.03	170.50
-259.3	459.0	-0.08	0.07	8.49	39.20	265.60	287.80
-108.7	174.2	-0.03	0.04	21.23*	12.66	-201.00	189.30
-354.1**	171.2	-0.04	0.05	7.11	11.21	-124.40	107.80
6.3	227.6	0.02	0.04	-2.79	14.73	-218.50	201.00
-401.5*	240.2	-0.01	0.05	-6.87	16.59	-158.40	215.50
26.0	344.1	0.03	0.06	-23.19	24.68	-279.20	204.20
-307.0	357.7	-0.02	0.06	-27.97	27.68	-311.00	226.30
-273.9	409.5	0.02	0.07	-23.52	36.09	-315.10	258.40
189.3	166.5	0.00	0.03	2.66	10.41	173.80	141.40
521		565		405		439	
114		114		89		83	
0.06		0.03		0.04		0.03	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

TABLE A.8: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results. Male VPs.

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.390**	0.15	0.665*	0.40	354.9	310.2
Treat=1 & t=2	0.523***	0.18	0.39	0.44	429.9	301.0
Treat=1 & t=3	0.18	0.24	0.933*	0.52	308.0	436.8
Treat=1 & t=4	-0.05	0.26	0.13	0.68	333.1	605.4
Treat=1 & t=5	0.05	0.28	-0.19	0.78	-532.8	635.7
t=1	-0.09	0.13	-0.07	0.32	-215.0	280.9
t=2	-0.21	0.18	-0.41	0.44	-391.1	316.6
t=3	-0.31	0.23	-0.68	0.54	-281.8	456.2
t=4	-0.17	0.25	-0.32	0.71	329.6	516.9
t=5	-0.16	0.32	0.32	0.75	254.0	628.9
Year dummies						
2003	0.02	0.18	0.11	0.32	-284.2	315.5
2004	0.16	0.15	0.55	0.38	545.6*	311.3
2005	0.06	0.14	0.50	0.41	247.3	309.1
2006	0.07	0.17	0.66	0.48	723.6**	351.6
2007	0.24	0.20	0.71	0.48	513.9	401.7
2008	-0.04	0.23	0.61	0.57	-48.9	429.1
2009	0.02	0.26	-0.89	0.65	-631.3	529.3
Constant	0.05	0.13	0.08	0.34	81.5	269.0
Number of observations:	1605		1666		1585	
Number of companies:	331		336		327	
R-squared	0.03		0.06		0.05	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
-90.9	130.5	-0.03	0.03	5.67	17.47	3.25	122.90
192.3	144.9	-0.04	0.04	-2.21	14.71	104.10	139.50
39.8	153.4	-0.01	0.04	-27.79	18.29	-211.8*	117.10
198.4	283.4	0.00	0.05	33.80	21.22	-28.19	181.00
-208.6	233.9	-0.05	0.08	-59.65	36.94	-8.74	276.50
96.0	119.4	-0.02	0.03	-22.36	18.49	3.28	114.80
-108.7	146.8	-0.06	0.04	3.95	17.31	140.50	134.70
188.9	169.9	-0.04	0.04	13.10	22.52	165.40	136.30
263.3	253.6	-0.03	0.05	-14.17	28.77	51.63	162.10
552.6*	290.8	-0.08	0.06	13.29	36.89	84.58	251.20
-115.0	126.4	-0.03	0.03	16.51**	7.78	-97.60	66.09
78.4	121.8	0.02	0.02	6.10	10.08	-44.83	71.81
-12.9	112.2	0.01	0.03	11.56	12.43	-18.77	78.32
19.6	133.4	0.05	0.03	15.95	13.89	120.10	105.90
-79.9	152.2	0.04	0.04	13.88	19.70	-135.60	125.40
-295.5*	171.3	-0.01	0.04	10.00	23.55	-218.40	139.40
-608.3***	214.6	0.00	0.05	2.84	28.28	-135.40	154.00
113.9	95.1	-0.01	0.02	-1.53	8.62	46.79	63.64
1547		1628		898		1032	
334		336		214		199	
0.04		0.03		0.03		0.05	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

TABLE A.9: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results. Female VPs.

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.580***	0.17	0.51	0.45	24.4	279.2
Treat=1 & t=2	0.03	0.22	-0.54	0.52	277.6	393.4
Treat=1 & t=3	-0.29	0.21	-0.58	0.61	-31.2	464.4
Treat=1 & t=4	-0.30	0.35	-1.27	1.12	-192.4	658.6
Treat=1 & t=5	-0.80	0.49	-1.53	1.02	-642.9	1242.0
t=1	0.07	0.14	0.10	0.36	248.3	240.5
t=2	0.16	0.19	0.27	0.45	-140.2	344.3
t=3	0.11	0.23	0.22	0.64	330.3	440.7
t=4	0.01	0.33	0.13	0.94	-39.9	601.3
t=5	0.31	0.32	1.674*	0.86	827.2	1072.0
Year dummies						
2003	-0.03	0.14	-0.25	0.36	-381.7	233.4
2004	-0.09	0.14	0.04	0.37	-72.9	249.8
2005	-0.06	0.14	0.12	0.38	7.5	210.1
2006	-0.06	0.18	0.06	0.43	-216.8	270.9
2007	-0.361*	0.21	-0.21	0.57	-60.3	331.7
2008	-0.25	0.25	-0.38	0.65	-645.1	396.8
2009	-0.36	0.32	-2.392***	0.77	-1034**	434.4
Constant	0.229*	0.12	0.747**	0.35	498.4**	192.7
Number of observations:	1004		1061		1026	
Number of companies:	204		210		206	
R-squared	0.06		0.14		0.05	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
15.0	134.2	-0.03	0.03	13.67	12.56	-117.00	147.30
51.2	175.9	-0.03	0.04	18.14	13.81	-23.48	165.90
279.7	200.8	0.01	0.04	-12.73	19.98	-23.92	148.50
241.2	347.2	-0.108*	0.06	-20.79	29.18	-63.13	194.40
176.7	297.4	-0.03	0.11	58.27	36.94	-571.6*	313.80
-74.9	119.5	0.01	0.03	0.98	11.76	-108.10	134.00
-173.5	143.0	0.01	0.03	-4.02	13.02	-14.89	110.10
-217.4	191.9	-0.03	0.04	7.51	17.31	-3.76	133.50
-418.2	324.6	0.01	0.06	24.47	26.46	-33.94	164.80
-497.3*	270.9	-0.05	0.10	-35.51	32.44	249.60	323.70
-63.7	125.3	-0.06	0.04	19.05*	9.62	-209.30	163.20
11.3	115.4	0.00	0.04	14.57	8.94	-17.98	107.20
183.9	125.5	0.00	0.03	3.99	8.03	-48.35	155.80
-28.7	131.5	-0.04	0.04	3.92	10.40	-63.25	169.10
192.6	168.6	-0.01	0.04	11.08	16.59	61.72	161.40
-28.4	188.8	-0.04	0.05	-5.40	17.29	-93.17	180.10
21.2	218.8	-0.01	0.05	19.74	20.54	-8.21	206.20
23.1	102.8	0.02	0.03	-2.26	6.70	84.41	124.00
1006		1039		597		661	
208		208		132		124	
0.03		0.03		0.04		0.03	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

TABLE A.10: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results. VPs with a tertiary education.

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.533***	0.18	0.48	0.43	206.2	287.9
Treat=1 & t=2	-0.06	0.21	-0.63	0.52	281.6	376.1
Treat=1 & t=3	-0.28	0.29	0.27	0.51	-432.1	399.5
Treat=1 & t=4	-0.15	0.31	-1.577**	0.64	313.6	397.5
Treat=1 & t=5	-0.570**	0.29	-1.20	1.02	-1277*	711.0
t=1	-0.07	0.15	-0.16	0.31	-211.0	259.5
t=2	-0.05	0.18	-0.37	0.37	-440.3	332.4
t=3	-0.21	0.25	-0.62	0.49	383.1	436.5
t=4	-0.16	0.25	0.50	0.60	294.4	489.5
t=5	0.04	0.28	0.55	0.67	1023**	498.8
Year dummies						
2003	0.26	0.16	-0.42	0.37	-87.3	272.5
2004	0.10	0.17	0.02	0.40	328.3	264.6
2005	0.26	0.18	-0.05	0.35	179.6	240.2
2006	0.23	0.20	-0.01	0.41	198.6	279.3
2007	0.30	0.23	0.19	0.51	313.0	365.7
2008	0.02	0.27	0.24	0.52	-320.2	404.2
2009	0.14	0.30	-1.182*	0.63	-696.4	473.5
Constant	-0.06	0.16	0.577*	0.34	219.8	214.3
Number of observations:	1177		1239		1186	
Number of companies:	251		257		250	
R-squared	0.05		0.08		0.05	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

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Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
146.6	146.6	-0.04	0.04	20.51	21.29	-79.42	162.80
164.1	164.1	-0.06	0.05	6.89	20.56	7.22	184.90
197.4	197.4	-0.01	0.05	-35.96	30.15	-413.1**	175.80
296.2	296.2	-0.03	0.06	13.84	31.59	140.30	237.30
236.7	236.7	-0.167*	0.09	-22.74	60.93	-714.2**	309.20
140.3	140.3	-0.03	0.03	-20.35	21.33	-0.79	155.20
148.6	148.6	-0.06	0.04	-6.48	19.92	239.30	154.70
199.4	199.4	-0.05	0.04	-3.69	23.96	301.7*	164.50
274.7	274.7	-0.04	0.06	-9.57	36.18	-6.43	194.90
343.0	343.0	-0.10	0.06	4.62	29.02	679.1**	299.60
164.9	164.9	-0.06	0.04	10.16	12.16	-220.4*	127.40
130.6	130.6	0.00	0.04	-1.77	15.68	-203.5*	116.70
126.1	126.1	0.00	0.04	6.54	16.66	-116.30	132.30
142.1	142.1	0.02	0.04	0.26	16.46	-90.73	151.20
167.1	167.1	0.02	0.05	26.97	22.13	-204.00	147.50
189.6	189.6	0.01	0.05	-3.31	25.36	-343.4*	178.30
234.3	234.3	0.03	0.06	15.84	26.69	-216.70	182.00
111.3	111.3	0.01	0.03	-0.83	12.81	172.20	117.50
1179		1208		624		732	
256		256		157		146	
0.04		0.03		0.04		0.05	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

TABLE A.11: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results. VPs with degrees in art&humanities

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.40	0.33	-0.24	0.74	-585.4	479.1
Treat=1 & t=2	-0.473*	0.26	-1.486*	0.76	90.0	532.2
Treat=1 & t=3	-0.53	0.32	0.33	0.68	-118.4	734.4
Treat=1 & t=4	-0.32	0.69	-0.36	1.15	-588.1	1190.0
Treat=1 & t=5	-0.53	0.71	0.96	1.17	541.7	952.4
t=1	-0.13	0.21	0.15	0.42	220.7	396.2
t=2	-0.11	0.23	-0.24	0.52	-519.9	485.4
t=3	-0.04	0.29	-0.69	0.64	-241.8	629.0
t=4	-0.22	0.49	-1.830*	0.98	619.9	795.6
t=5	-0.11	0.36	-0.13	0.98	316.3	759.0
Year dummies						
2003	0.16	0.26	0.90	0.61	78.5	370.6
2004	0.22	0.23	0.87	0.63	175.4	307.9
2005	0.580**	0.22	0.81	0.68	-59.8	302.1
2006	0.27	0.32	0.84	0.68	-98.9	369.8
2007	0.38	0.32	1.20	0.83	128.9	439.7
2008	0.18	0.36	1.38	0.89	-171.3	557.0
2009	0.43	0.39	0.00	0.96	-285.9	464.9
Constant	-0.16	0.21	-0.18	0.62	349.3	270.5
Number of observations:	366		377		374	
Number of companies:	79		80		78	
R-squared	0.06		0.14		0.04	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
-310.9	241.5	-0.06	0.06	26.45	25.89	-332.90	304.30
225.6	252.0	-0.07	0.07	6.66	27.80	-339.00	432.00
56.4	356.6	-0.08	0.08	-5.97	54.10	-293.00	323.30
-1233.0	849.7	-0.17	0.17	-2.70	67.91	-435.9*	255.10
-250.1	847.8	-0.19	0.19	0.00	0.00	0.00	0.00
-130.5	243.6	-0.05	0.05	-12.28	24.17	31.47	376.00
-423.7*	215.5	-0.07	0.07	-24.61	27.57	392.70	409.80
-304.1	338.8	-0.07	0.07	-15.68	43.30	156.80	361.60
909.4	674.0	-0.18	0.18	4.38	39.65	63.65	232.50
341.1	703.5	-0.10	0.10	-16.95	45.84	495.30	327.90
18.3	176.9	-0.08	0.08	-3.70	26.77	-2.09	351.50
47.2	134.7	-0.08	0.08	-12.41	32.46	-11.39	312.40
99.2	147.3	-0.08	0.08	-19.93	21.77	95.89	316.00
143.3	166.6	-0.09	0.09	-17.82	27.03	194.20	393.80
102.2	221.8	-0.09	0.09	6.56	33.61	195.40	316.70
145.3	260.5	-0.10	0.10	-16.19	42.94	14.77	379.90
76.8	294.6	-0.11	0.11	29.41	49.11	56.61	392.80
18.2	113.1	0.08	0.08	6.67	21.30	-87.24	320.30
370		365		183		224	
79		79		44		44	
0.06		0.08		0.07		0.06	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

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TABLE A.12: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results. VPs with degrees in social sciences.

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.32	0.26	1.512***	0.52	896.0**	389.2
Treat=1 & t=2	0.05	0.30	-0.55	0.78	89.0	490.5
Treat=1 & t=3	-0.12	0.39	0.79	0.78	-333.9	435.6
Treat=1 & t=4	0.07	0.43	-0.38	1.01	1194*	661.3
Treat=1 & t=5	-0.16	0.33	0.31	1.55	449.3	899.2
t=1	-0.11	0.22	-0.997**	0.41	-805.9**	350.7
t=2	-0.04	0.28	-0.828*	0.45	-582.5	447.8
t=3	-0.67	0.41	-1.286**	0.53	353.1	461.1
t=4	-0.46	0.39	-0.59	0.85	-487.7	753.3
t=5	-0.18	0.45	-1.65	1.17	-22.4	921.3
Year dummies						
2003	0.01	0.23	-0.45	0.39	-506.4	362.3
2004	0.07	0.22	-0.02	0.49	32.3	283.6
2005	0.13	0.21	0.07	0.42	137.4	259.4
2006	0.18	0.24	0.26	0.50	228.8	342.6
2007	0.12	0.32	0.37	0.56	249.2	461.9
2008	0.22	0.36	0.67	0.57	-470.2	523.0
2009	0.12	0.43	-0.61	0.67	-610.7	668.2
Constant	0.10	0.18	0.65	0.40	444.6*	239.0
Number of observations:	630		658		621	
Number of companies:	127		130		124	
R-squared	0.04		0.09		0.07	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

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Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
-31.8	206.1	-0.06	0.04	-6.09	27.41	-2.87	213.40
-32.2	204.4	-0.07	0.06	20.51	30.54	-155.00	242.20
-213.2	259.8	-0.05	0.04	-48.35	32.39	-387.40	239.20
541.7	364.3	-0.09	0.08	35.91	29.57	297.70	301.30
24.8	390.6	-0.07	0.05	33.74	21.08	-636.4*	323.10
-176.0	204.1	-0.02	0.04	-21.91	26.86	-25.13	148.10
-169.5	223.3	-0.04	0.05	9.09	25.73	321.1**	141.70
369.5	261.0	-0.01	0.05	-5.14	31.11	395.6**	196.90
-25.2	373.0	-0.03	0.07	-13.95	43.11	-126.50	183.50
273.3	452.1	-0.09	0.10	13.19	38.30	581.0*	339.10
32.6	160.9	0.0578*	0.02	-1.98	15.25	-191.30	131.00
172.8	148.7	0.0863**	0.03	-5.47	18.16	-73.31	145.50
257.8*	145.2	0.0983**	0.04	11.68	22.17	-90.01	162.80
152.4	168.3	0.07	0.04	-3.27	20.85	-154.90	157.80
117.9	226.0	0.05	0.05	22.46	28.50	-257.90	177.00
-212.0	237.9	0.11	0.05	-4.65	33.52	-421.0*	224.20
-364.7	317.6	-0.0649***	0.07	5.39	36.33	-215.60	218.40
12.3	117.2	-0.0649***	0.02	0.76	15.95	172.10	125.10
626		638		368		416	
129		130		85		79	
0.07		0.03		0.09		0.06	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

TABLE A.13: Comparison between VP-companies and companies in the reference group. Companies with up to 50 employees in year zero. Diff-in-diff fixed effects regression results. VPs with degrees in technical sciences.

Dependent variables (in first differences):	Number of highly educated employees[1]		Number of employees		Value added (DKK1,000)[2]	
	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
Treat=1 & t=1	0.516**	0.21	-0.11	0.58	-282.6	383.9
Treat=1 & t=2	0.417*	0.24	0.44	0.53	915.7*	466.0
Treat=1 & t=3	-0.08	0.30	0.19	0.72	283.7	645.6
Treat=1 & t=4	-0.17	0.34	0.05	0.93	-103.5	904.3
Treat=1 & t=5	-0.17	0.39	-2.006***	0.70	-1962*	991.6
t=1	0.09	0.17	0.921*	0.51	745.2**	355.2
t=2	0.19	0.23	0.35	0.72	-154.7	441.8
t=3	0.09	0.25	0.89	0.88	517.3	673.9
t=4	0.11	0.31	0.96	1.06	1157*	692.2
t=5	0.08	0.40	3.272***	1.00	2082**	858.6
Year dummies						
2003	-0.10	0.23	-0.01	0.45	-441.7	393.0
2004	0.06	0.19	0.31	0.52	579.8	443.9
2005	-0.14	0.18	0.47	0.57	104.6	419.3
2006	-0.20	0.22	0.32	0.73	552.6	489.5
2007	-0.16	0.26	-0.06	0.79	207.8	538.8
2008	-0.41	0.29	-0.58	0.90	-625.7	566.5
2009	-0.31	0.30	-2.893***	1.05	-1549**	666.5
Constant	0.14	0.17	0.30	0.45	129.1	352.2
Number of observations:	932		985		926	
Number of companies:	187		193		190	
R-squared	0.05		0.10		0.09	

Notes: Only observations with annual changes in the number of employees of less than 12. *, **, *** denote statistical significance at the 10%, 5%, and 1% significance level.

1. Employees with post-secondary or tertiary education. Only observations with annual changes in the number of employees with post-secondary and tertiary education < 5.

Net income (DKK1,000)[3]		Return on assets[4]		Wage per employee (DKK1,000)[5]		Labour productivity (DKK1,000)[6]	
Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.	Coeff.	Ste.
-168.3	172.7	-0.01	0.04	27.09	19.75	-55.06	130.90
337.1	217.5	-0.04	0.05	-3.78	14.09	230.80	148.80
260.3	206.9	0.01	0.05	-19.81	23.65	-12.40	97.78
134.9	414.8	0.01	0.06	47.24*	27.10	-104.30	205.20
-333.1	230.7	-0.10	0.10	-58.75	49.85	-63.98	370.80
413.9***	150.8	0.00	0.04	-29.64	20.67	50.05	137.80
-6.6	186.6	-0.01	0.05	-23.62	19.63	-8.47	160.20
339.9	208.4	0.00	0.05	-3.06	26.29	-10.69	176.70
403.0	339.2	0.04	0.06	-57.71*	32.57	63.38	235.40
701.6**	276.1	0.03	0.07	-20.32	52.61	82.76	352.10
-218.3	167.4	-0.0717*	0.04	21.43***	7.97	-203.60	123.00
35.5	166.6	0.00	0.03	23.56***	8.29	-31.67	84.93
-169.1	144.1	-0.05	0.04	15.60*	8.73	-155.00	125.70
-171.8	174.6	-0.03	0.05	40.25***	15.14	111.30	154.60
-260.2	176.9	0.00	0.05	36.60	22.51	-105.00	192.80
-497.4**	220.2	-0.09	0.05	39.29	28.01	-143.90	204.90
-874.0***	264.0	-0.09	0.07	39.78	31.08	-130.50	246.20
188.7	118.9	0.04	0.03	-11.02*	6.52	76.85	95.41
894		977		557		647	
192		193		125		119	
0.06		0.03		0.04		0.05	

2. Only observations with annual change in the value added of less than DKK 10 mio.
3. Only observations with annual change in net income of less than DKK 3 mio.
4. Only observations with annual change in roa of less than 1, and total assets > DKK 100,000.
5. Only observations with number of employees > 5. Only observations with change in average wage < DKK 500,000.
6. Only observations with number of employees > 5 and change in labour productivity < DKK 3 mio.

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Analysis of the Industrial PhD Programme

Copenhagen, February 2011

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EXECUTIVE SUMMARY

This report has been prepared by the Centre for Economic and Business Research (CEBR). It presents an analysis of the economic impact of the Danish Industrial PhD Programme on participating companies and on wage and career characteristics of Industrial PhD graduates.

The Industrial PhD Programme is funded by the Danish Council for Technology and Innovation and is administered by the Danish Agency for Science, Technology and Innovation (DASTI). The programme subsidises PhD studies where the student is employed in a private sector company and simultaneously enrolled as a PhD student at a university.

The analysis follows approx. 430 individuals and approx. 270 companies that have participated in the programme and for whom relevant data is available in the selected registers.

On the individual level, we compare wage income and occupation of Industrial PhDs with regular PhDs and other university level graduates.

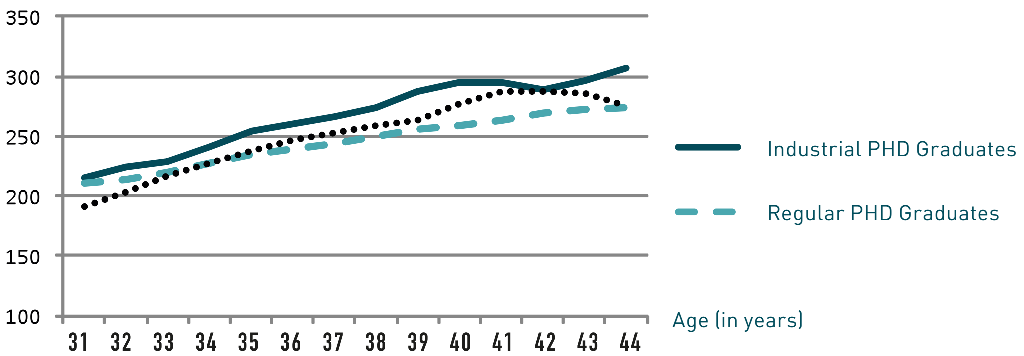
On the company level, we analyse company level developments within four success parameters:

- the number of patents applications,
- gross profit growth,
- total factor productivity, and
- employment growth.

For a sample of companies which have hosted a maximum of three Industrial PhD projects, we identify a control group of highly similar companies which have not hosted any Industrial PhD projects. We then compare developments in the success parameters in these two groups. Under identifying assumptions, the difference between the sample group and the control group isolate the causal impact of the programme on companies hosting Industrial PhD projects.

The results of the analysis can be summarised as follows: Industrial PhDs earn approx. 7-10 percent higher wages than both regular PhDs and comparable university graduates. This comparison is illustrated in FIGURE 1.

FIGURE 1: Hourly wage (DKK) in 2006, by Individual age

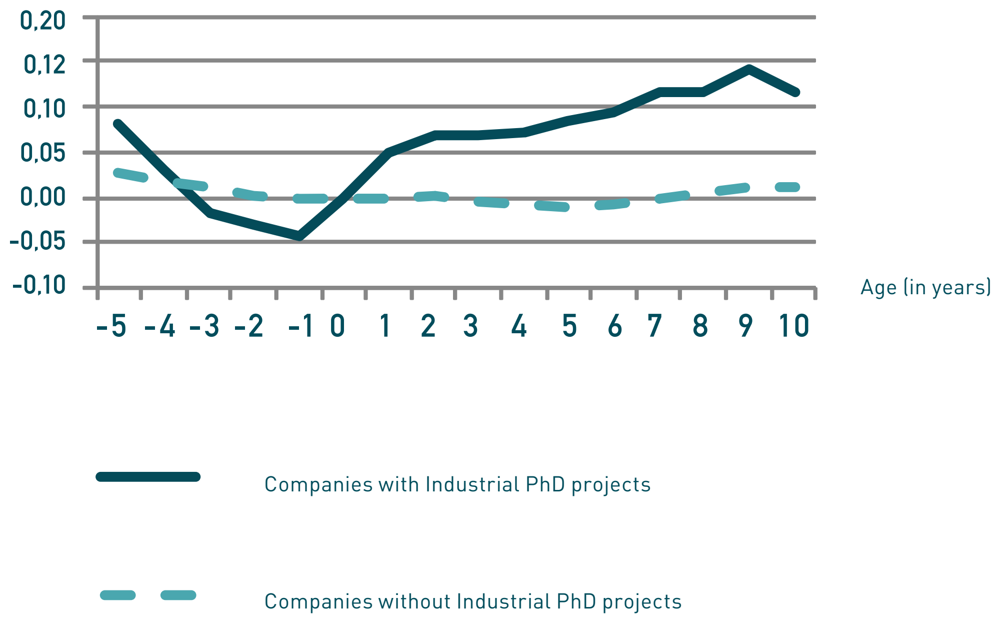


They are also more likely to be found at the top levels of their organisations' hierarchies compared to normal PhDs and more likely to be found in positions requiring high-level specialist knowledge than regular university graduates.

Companies that host Industrial PhDs see on average increasing patenting activity, illustrated by FIGURE 2. They are characterised by high growth in gross profit, and more positive developments in gross profit and employment growth than companies in the control group. We are not able to identify robust relationships between hosting Industrial PhD projects and total factor productivity developments.

FIGURE 2: Number of patent applications, high-quality matches.

Average number of patent applications per company, change relative to year before first initiating an Industrial PhD project



Denne rapport er skrevet af Centre for Economic and Business Research (CEBR). Den beskriver en analyse af ErhvervsPhD-ordningens potentielle effekter på udviklingen i de deltagende virksomheder og løn- og karrieremønstre for personer, som har erhvervet deres ph.d.-grad gennem ordningen.

Et ErhvervsPhD-projekt er et treårigt erhvervsrettet ph.d.-projekt, hvor den studerende ansættes i en privat virksomhed og samtidig indskrives på et universitet.

Ved hjælp af registerdata følger analysen ca. 430 individer og 270 virksomheder, som har deltaget i ordningen. På individniveau studeres væksten i ErhvervsPhD'ernes lønindkomst i forhold til almindelige ph.d.'ere og sammenlignelige kandidater.

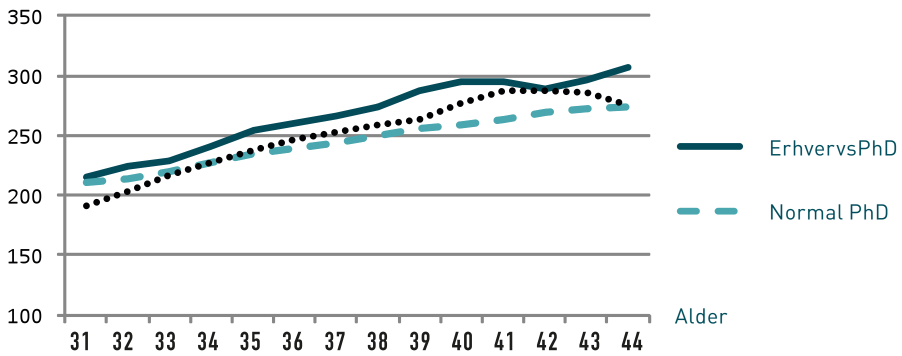
For virksomheder studeres udviklingen i patentering, bruttofortjeneste, totalfaktorproduktivitet og beskæftigelse. Hertil identificerer vi en gruppe af kontrolvirksomheder, som ikke ansætter en ErhvervsPhD, men som ellers ligner de ansættende virksomheder i størrelse, branche, alder og region.

Dermed kan vi besvare spørgsmålet om, hvorvidt de virksomheder, som ansatte en ErhvervsPhD, har haft en mere positiv udvikling i succesparametrene, end man ville have forventet på basis af udviklingen for kontrolvirksomhederne.

Analysens resultater kan sammenfattes som følger:

Efter uddannelsens afslutning har ErhvervsPhD'er i gennemsnit mellem 7 og 10 procent højere lønindkomst end normale ph.d.'er og personer med en afsluttet universitetsuddannelse. Dette er illustreret i FIGUR 1.
ErhvervsPhD'ere har endvidere en væsentligt højere sandsynlighed for at blive ansat i lederstillinger end almindelige ph.d.'ere og er stærkere repræsenteret i

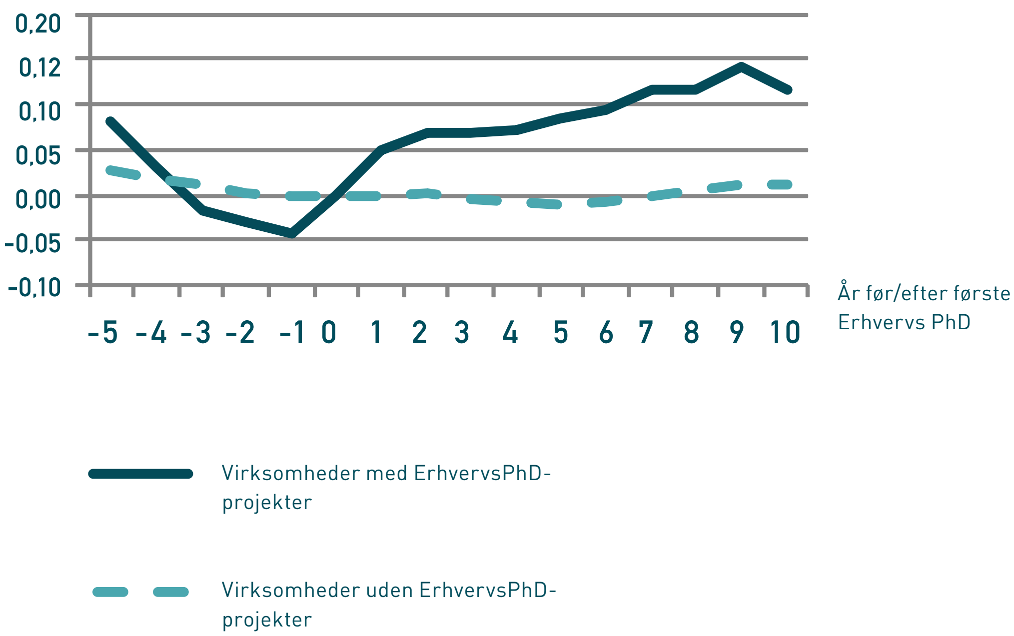
FIGURE 1: Timeløn (i kr.) in 2006, efter alder



gruppen af medarbejdere med jobfunktioner, som kræver specialviden på højeste niveau. Virksomheder, som ansætter ErhvervsPhD'ere, har i gennemsnit højere patenteringsaktivitet efter ansættelsen end før. Dette er illustreret i FIGUR 2.

FIGURE 2: Antal patentansøgniner, højkvalitetssammenligning

Gennemsnitlig antal patentansøgninger pr. virksomhed
(i afvigelser ift. året før første ErhvervsPhD-projekt)



De er også kendetegnet ved højere vækst i bruttofortjenesten/værdiskabelsen og har en mere positiv udvikling i væksten i bruttofortjenesten og medarbejderantallet end virksomhederne i kontrolgruppen.

Det er på nuværende tidspunkt ikke muligt at påvise, at ErhvervsPhD-ordningen bidrager til højere vækst i virksomhedernes totalfaktorproduktivitet.

1 INTRODUCTION >

This report has been prepared by the Centre for Economic and Business Research (CEBR). It presents an analysis of the economic impact of companies participating in the Danish Industrial PhD Programme in terms of growth and value creation, and on wage income and career patterns of Industrial PhD graduates.

Even though this analysis is an evaluation of a specific Industrial PhD subsidy programme, its results might be of general interest, as programmes similar to the Danish Industrial PhD Programme have been implemented or are considered for implementation in a number of countries. However, general knowledge of their effects which can be integrated into cost-benefit analyses of these programmes is still rare.

The Industrial PhD Programme aims at increasing knowledge sharing between universities and private sector companies, promoting research with commercial perspectives, and taking advantage of competences and research facilities in private business to increase the number of PhDs.

For this purpose, the Industrial PhD students typically spend 50 percent of their time in a company and 50 percent of their time at a university while taking the degree. The Danish Agency for Science, Technology and Innovation (DASTI) subsidises the Industrial PhD's salary with a fixed monthly amount, roughly corresponding to 30-50 percent of the Industrial PhD's total salary.

The Industrial PhD programme was initiated in 1971 under the name "The Industrial Researcher Programme". In 1988 it was made possible to qualify for a PhD degree when graduating. The programme was subsequently reformed to comply with Danish PhD regulations, making every graduate a formal PhD graduate. Until 2009, approx. 1,200 projects have been started. As part of its evaluation policy, DASTI has asked CEBR to analyse the company and individual level effects of the Industrial PhD Programme. The main questions of the evaluation are whether and how participating in the Industrial PhD Programme is associated with company performance and, with regard to individuals, to what extent an Industrial PhD degree is associated with future career developments, measured by wage income and occupation.

To answer the questions outlined above, this analysis considers 430 individuals and approx. 270 companies that have participated in the programme using a matched employer-employer register dataset.

On the individual level, we compare wage income developments of Industrial PhD graduates with regular PhD graduates and individuals with a university level degree (and who have graduated at approximately the same time as the Industrial PhD graduates).

>

On the company level, we analyse company level developments within four success parameters:

- number of patent applications,
- gross profit growth,
- total factor productivity (TFP), and
- employment growth

Gross profit is defined as annual net sales subtracted annual costs of variable inputs (raw materials, energy, intermediate goods purchases, etc.), except labour costs. Thus, gross profit is a measure of the company's value creation.[1]

Total factor productivity is gross profit corrected for the company's use of capital and the number of employees. It is measured as the percentage-wise deviation of a company's gross profit from the gross profit that would have been expected on basis of the company's number of employees and its capital stock.[2]

To identify innovation, growth and productivity effects of hosting an Industrial PhD, we analyse increases in the number of patent applications, gross profit growth, total factor productivity and employment growth for a sample of companies which have participated in the Industrial PhD Programme. By using a control group of highly similar companies which have not participated in the programme, we can compare the developments of the success parameters of the two groups of companies to each other.

1 Gross profit is the most precise measure of the company's value creation, but one should, of course, keep in mind that a part of the company's total value creation may be passed on to consumers, may be retained in the company and increase its value (for which there is no data available for this analysis), or may take the form of positive externalities, such as knowledge and/or innovations which benefit other companies or society in general.

2 For this analysis, we measure TFP as the residuals of a Cobb-Douglas-production function estimation with total assets and the number of employees as right hand side variables.

2 DESCRIPTION OF THE INDUSTRIAL PHD PROGRAMME >

An Industrial PhD project is a three-year industrially focused PhD project where the student is hired by a company and enrolled in a university at the same time.[3]

The company receives a monthly wage subsidy of (currently) DKK 14,500 (approx. €2,000) while the university has its expenses for supervising etc. covered. The PhD student works full time on the project and divides his or her time equally between the company and the university. There are additional subsidies available for project-relevant stays abroad.

Currently, there are allocated annually approx. DKK 100-150 million (€15-20 million) for new projects. Approval rates for applications are currently above 60 percent.

Different aspects of the Danish Industrial PhD programme were addressed in earlier evaluations. DASTI (2007a)[4] concludes that Industrial PhDs are characterised by earning higher wages and are more likely to be a part of their organisation's management compared to regular PhDs. Companies hosting Industrial PhD projects expect increased patenting activity and growth.

DASTI (2007b)[5] lists several positive benefits for the participating companies. Among other things, companies may gain new knowledge, patents and licenses, growth and new market opportunities, and an increased network inside the academic world.

A similar conclusion is reached in a report by Right, Kjaer and Kjerulf from 2003. Based on interviews with participating candidates and companies in 2002, they find that a majority of companies expect the Industrial PhD to contribute to patents, while close to half of all companies expect increased earnings.

International evaluations include a report from the European University Association,[6] which concludes that participating candidates enjoy better employment opportunities due to improved skills. Two studies for the Swedish agency KK-stiftelsen[7] have also been carried out. These conclude (a) that certain conditions need to be met for projects to be successful, and (b) that the different stakeholders of Industrial PhD projects report that the programme is achieving its goals.

3 This section draws extensively on the information published by DASTI.
4DASTI, 2007a: "ErhvervsPhD - Et effektivt redskab for innovation og vidensspredning".
5DASTI, 2007b: "ErhvervsPhD - Ny viden til erhvervslivet og universiteterne".
6European University Association, 2009: "Collaborative Doctoral Education - University-Industry partnerships for enhancing knowledge exchange".
7(a) KK-stiftelsen, 2003: "KK-stiftelsens företagsforskarskolor - utvärdering av ett koncept för ökat samarbete mellan akademi och näringsliv".
(b) KK-stiftelsen, 2006: "Småföretags- och institutsdoktorander för kunskaps- och kompetensutveckling".

3 INDIVIDUAL LEVEL ANALYSIS >

For the individual level analysis, information gathered by DASTI on participating individuals was merged with public register information typically referred to as the "Integrated Database of Labour Market Research (IDA)". These data cover the period from 1980 onward and contain information on a multitude of individual demographic background characteristics, like education, gender and age.

The IDA data have – for the period 1997 to 2006 – been merged with information from the "Wage Statistics Database", which includes detailed information on wages and occupation, including hierarchical levels.

Also, information from education-related registers has been added to the data, to make it possible to control for inherent human capital endowments – pproximated by the grades of secondary education certificates – in the regressions.

The following analysis compares wages and careers of Industrial PhD graduates with:
(a) individuals with a university degree, but no PhD degree, and
(b) regular PhD graduates.

The validity of these comparisons depends on how similar the two groups are with the Industrial PhD graduates and the potential to control for observable factors presumably related to educational choices and, later, income and career developments. Both objectives raise some issues regarding the optimal sampling strategy, which is presented in the following:

When selecting the sample for the analysis, we obviously include all individuals who have completed an Industrial PhD education. Individuals who have completed a regular PhD education form the first control group.

With regard to university graduates, who form the second control group of individuals for comparison, there is an issue which needs to be resolved: There is a large number of secondary educations where it is not entirely clear whether they should be defined as university level educations or not.

We choose to address this issue by identifying the highest educational degrees of the Industrial PhD graduates before obtaining their Industrial PhD degrees. As a first step in the sampling procedure, we only select individuals with the same set of educations for the control group.

But without further conditions on sampling, the educational fields of the Industrial PhDs and the university graduates would be very different. For example, there would be a large share of individuals with university degrees in arts and Humanities in the control group, while these degrees are relatively uncommon in the group of Industrial PhDs. This would bias any comparison between the two groups.

For this reason, we also align the composition of the educational fields of Industrial PhDs prior to obtaining the Industrial PhD degree and the educational fields of the control group by selecting a fixed number of individuals into the control group for each Industrial PhD graduate.

Specifically, we select ten individuals into the control group for each Industrial PhD. The number ten is a compromise between being able to find individuals with the same educational degrees and a sample size large enough to isolate relationships in the data.

These individuals, referred to as the group of ‘university graduates’ are randomly selected, but must correspond to the educational field of the given Industrial PhD graduate (before he/she obtains her PhD degree). In the selection process, we also prefer persons of the same gender and origin (Danish vs. non-Danish), and persons who are of similar age. This way, we base the comparisons on groups of individuals similar not only in terms of their educational field, but also age, gender and origin.

The individual level analysis is based primarily on information for the year 2006, which is the last year where the data provides detailed information on wages and occupation.

3.1 Results of the individual level analysis

At present there are approx. 1,200 individuals who have participated or are participating in the Industrial PhD Programme. In year 2006, which is the last year for which all relevant data is available, 999 Industrial PhDs can be identified in the register data.

Of these, the register data shows 442 completed their projects, i.e. obtained their Industrial PhD degree, before 2006.

Additionally, there is wage information for 430 of these 442 individuals.

The wage concept used in the following analysis is Statistics Denmark’s ‘nw’-variable of the Wage Statistics Database. This variable is a description of the person’s hourly wage income excluding pension contributions and cleaned for peculiarities such as overtime, dirty work premiums, etc.

Career developments are measured by Statistics Denmark’s ‘disco’-variable, also from the Wage Statistics Database. This variable categorises occupation by different hierarchical levels and work functions. The question to be considered is whether Industrial PhDs are over- or underrepresented in leadership positions (disco code 1000-1999) or positions which require high-skilled specialist knowledge – these will be denoted as specialist positions in the following (disco code 2000-2999).

Descriptive statistics

In this subsection, we describe the gross sample of all individuals associated with the Industrial PhD Programme – with or without completed Industrial PhD degrees - and of all individuals with a PhD degree in the last year in which they are observed, and all individuals selected for the group of university graduates. This ensures the most comprehensive description of these groups. However, when we turn to the comparison of wages and career patterns, we will concentrate on those individuals with completed educations (either university or PhD) in 2006.

When taking a first look at the data (TABLE 3.2.1), we find that in 2006, the last year for which data is available for this analysis, Industrial PhDs earn approx. 10 percent lower wages than regular PhDs, but are, in the current sample, almost eight years younger on average.

Industrial PhDs have slightly lower grades than normal PhDs in their secondary education examinations, but this difference is negligible relative to this variable's variation.

About four percent of the Industrial PhDs are represented at the top of their organisation's hierarchy, which is very similar to the two control groups. Approx. 60 percent of Industrial PhDs work in specialist positions, a share which locates them between regular PhDs and university graduates, where this is the case for 74 and 44 percent, respectively. Obviously, we can expect differences in both wages and positions to increase when focusing only on individuals with completed educations in the next subsection.

TABLE 3.2.1: Descriptive statistics of the individual level data (2006), mean values

	Industrial PhD students and graduates	Regular PhD students and graduates	University graduates	All
Hourly wage (DKK)	228,61	223,44	223,44	243,06
Female	0,35	0,34	0,35	0,34
Age (year)	34,55	42,66	34,72	38,98
Grade of university-entrance diploma (standard deviation: 8.9)	91,83	92,63	87,33	90,07
Non-Danish origin	0,06	0,08	0,05	0,07
Leadership position	0,04	0,04	0,03	0,04
Specialist position	0,58	0,74	0,44	0,61
Number of observations	999	12369	9625	22993

Cf. TABLE 3.2.2, we find that approx. 38 percent of all Industrial PhDs had a degree in engineering, and another approx. 23 percent had degrees in chemistry or electronics engineering before receiving their Industrial PhD degree. [8]

[8] Obviously, the group of university graduates is supposed to only consist of individuals who actually have graduated. Thus, when we formally compare the different groups of individuals in the results subsection, please note that we will not consider individuals registered as having a university-entrance diploma as their highest educational degree in 2006.

>

TABLE 3.2.2: Highest educational degree in 2006 (for Industrial PhD and regular PhD graduates: highest degree before receiving the PhD degree), in percent				
	Industrial PhDs	Regular PhDs	University Graduates	All
Master's in engineering	37,99	12,38	32,95	20,99
Unknown	3,89	30,24	0,08	18,08
Master's in medical science	2,52	25,42	4,01	16,61
Master's in biology	9,84	14,26	11,16	12,94
Master's in chemical engineering	10,53	5,3	11,42	7,76
Master's in electronics engineering	12,81	3,46	12,02	6,99
University-entrance diploma	9,38	0,6	15,79	6,54
Master's in pharmaceutics	6,41	4,04	6,73	5,13
Master's in biochemistry	6,64	4,3	5,84	4,96

Before turning to wage and career comparisons, we take a brief look at the kind of PhD degrees of Industrial and regular PhDs - see TABLE 3.2.3 for the most popular subjects. We find these two groups to be quite different in their compositions of the specific degrees. Consequentially, later comparisons of wages and careers between Industrial and regular PhDs will have to take these differences into account.

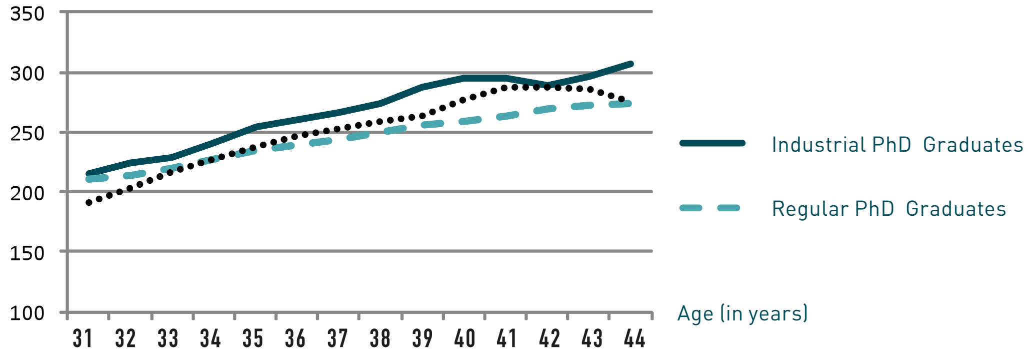
Interestingly, there are a number of Industrial PhDs in medical sciences, yet only relatively few of these individuals had medical science university degrees before taking their PhD.

TABLE 3.2.3: Type of PhD degrees, in percent			
	Industrial PhDs	Regular PhDs	All
Technical sciences	59,5	24,5	25,9
Natural sciences	10,6	22,5	22,0
Other disciplines	3,4	20,8	20,1
Medical sciences	14,3	19,3	19,1
Veterinarian/agricultural	6,3	8,9	8,8
Pharmaceutical sciences	3,2	2,0	2,1
Social sciences	2,7	1,9	2,0

Results

As a first step, we compare average hourly wages of the different groups of individuals under consideration in 2006 and graph the averages as a function of age in FIGURE 3.2.1.

FIGURE 3.2.1: Hourly wage (DKK) in 2006, by Individual age



We find that wages of Industrial PhD graduates are higher than those of both regular PhD graduates and university graduates. Differences are largest in the early and mid-forties. Regular PhDs have lower wages relative to both Industrial PhDs and university graduates.

An obvious explanation of these differences may be sought in different employment patterns of the different groups of employees, with regular PhDs being overrepresented in public sector research institutions, which are generally characterised by lower wages than private sector employers.

Comparisons between Industrial PhDs and regular PhDs

In this section, we compare wages between Industrial and regular PhD graduates by using a linear regression, holding constant a set of (pre-determined) background characteristics (age, gender, etc.).

These comparisons are based on the 430 Industrial and approx. 5,850 regular PhD graduates. We choose a logarithmic specification of the wage variable, implying that regression coefficients are the expected (approximately) percentage-wise changes in the wage when the condition of the associated explanatory variable is fulfilled.

TABLE 3.2.4: Hourly wage of Industrial and regular PhD graduates, linear regression results, dependent variable: log (hourly wage), sample: Industrial and regular PhD graduates (2006)

Variables	Coefficient		Standard error	Coefficient		Standard error
The person is an Industrial PhD graduate	0.090	***	0.014	0.063	***	0.014
The person is female	-0.060	***	0.007	-0.065	***	0.007
The person is an immigrant (or descendant)	0.028		0.027	0.005		0.027
Grade of secondary education diploma (normalised)	0.026	***	0.004	0.019	***	0.003
Age (in years)	0.016	***	0.001	0.021	***	0.001
Additional controls	Secondary education: elective courses (7 categories)			Secondary education: elective courses (7 categories); specific PhD degree (10 categories); age when receiving the PhD degree		
Number of observations	6.283			6.283		

Notes: ***: significant at the 1% level. Heteroscedasticity-consistent standard errors.

The regression results confirm the findings of FIGURE 3.2.1: Industrial PhDs earn approx. (exp(0.090)=1.094) 9 percent higher hourly wages compared to their counterparts who have taken a regular PhD degree. When including additional variables in the regressions (which control for the different compositions of the subjects of the PhD projects and for age differences when obtaining the PhD degree), the difference drops to approx. 6 percent, but remains statistically highly significant.

Here, it might be noted that the result of positive wage income differences is robust when considering gross hourly wages (i.e. total wage income including pensions divided by the number of working hours) or annual income instead of the current wage concept. In the first case, the relevant coefficient dropped to approx. 5 percent (instead of approx. 9 percent). In the second case, when considering annual income without correcting for working hours, the coefficient increased to between 10 (in the specification with additional controls) and 15 percent (in the more simple specification). This indicates that Industrial PhDs register more working hours than regular PhDs.

Comparing the career developments between the two types of PhDs, we first note that 6.3 percent of Industrial PhD graduates are employed in leadership positions, as opposed to 3.9 percent of regular PhD graduates. The formal comparison is by estimating a so-called binary choice model (assuming a logistic distribution). The coefficients of this model are displayed in TABLE 3.2.5.

TABLE 3.2.5: Occupation of Industrial and regular PhD graduates, binary choice (logit) model, sample: Industrial and regular PhD graduates (2006)

	Dependent variable: The person has a leadership position			Dependent variable: The person has a specialist position		
Variables	Coefficient		Standard error	Coefficient		Standard error
The person is an Industrial PhD graduate	1.087	***	0.217	-0.738	***	0.112
The person is female	-0.577	***	0.178	0.268	***	0.064
The person is an immigrant (or descendant)	0.985	**	0.403	-0.344		0.227
Grade of secondary education diploma (normalised)	0.108		0.078	0.077	**	0.033
Age (in years)	0.103	***	0.020	0.010		0.009
Additional controls	Secondary education: elective courses (7 categories)			Secondary education: elective courses (7 categories)		
Number of observations	7,214			7,214		

Notes: ***: significant at the 1% level, ** significant at the 5% level.

The exponents of the model's coefficients equal the increases in the probability that the individual is a leader or a specialist when the logical conditions of the corresponding variables are true. We find that Industrial PhDs are almost three times more likely (exp(1.087)=2.95) to hold a leadership position than regular PhDs when holding constant the set of background characteristics included in the regression.

Industrial PhDs have an approx. (exp(-0.738)=0.48) 50 percent lower probability of being employed in a specialist position than regular PhDs. We conclude that, while regular PhDs are almost entirely employed in specialist positions, Industrial PhDs are more evenly distributed across the different occupational levels.

Comparisons between Industrial PhDs and university graduates

In the following, we compare wages between Industrial PhD graduates and university graduates.

TABLE 3.2.6 summarises the results of the comparison of hourly wages. They suggest that Industrial PhDs earn a wage premium of approx. 7 percent relative to university graduates in similar fields of study while controlling for demographic factors, secondary education grades and course specialisation.

TABLE 3.2.6: Hourly wage of Industrial PhD and university graduates, linear regression results, de-pendent variable: log (hourly wage), sample: Industrial PhD graduates and university graduates (2006)

Variables	Coefficient		Standard error
The person is an Industrial PhD graduate	0.066	***	0.014
The person is female	-0.129	***	0.009
The person is an immigrant (or descendant)	-0.053		0.034
Grade of secondary education diploma (normalised)	0.043	***	0.005
Age (in years)	0.028	***	0.001
Additional controls	Secondary education: elective courses (7 categories)		
Number of observations	5,246		

Notes: ***: significant at the 1% level. Heteroscedasticity-consistent standard errors.

Again, it may be noted that the result of positive wage income differences is unaffected by considering gross hourly wages (i.e. total wage income including pensions divided by the number of working hours) or annual income instead of the current wage concept. In the first case, the relevant coefficient dropped to 0.056 (instead of 0.066). In the second case, when considering annual income without correcting for working hours, the coefficient increased to 0.11 - again indicating that Industrial PhDs register more working hours than other graduates.

The results of the career development comparisons are found in TABLE 3.2.7. In comparison with university graduates, Industrial PhDs are overrepresented in leadership positions and specialist positions. However, the difference regarding leadership positions is not statistically significant and must be regarded as tentative.

For specialist positions, the coefficient 0.397 corresponds to an approx. 50 percent higher probability that Industrial PhDs are employed in specialist positions than university graduates.

TABLE 3.2.7: Occupation of Industrial PhD graduates and university graduates, binary choice (logit) model, sample: Industrial PhD graduates and university graduates (2006)

	Dependent variable: The person has a leadership position			Dependent variable: The person has a specialist position		
Variables	Coefficient		Standard error	Coefficient		Standard error
The person is an Industrial PhD graduate	0.182		0.217	0.397	***	0.113
The person is female	-0.741	***	0.156	0.105	**	0.051
The person is immigrant (or descendant)	-0.572		0.710	-0.120		0.217
Grade of secondary education diploma (normalised)	0.139	**	0.064	0.151	***	0.025
Age (in years)	0.095	***	0.015	0.072	***	0.006
Additional controls	Secondary education: elective courses (7 categories)			Secondary education: elective courses (7 categories)		
Number of observations	7,465			7,465		

Notes: ***: significant at the 1% level, ** significant at the 5% level.

4 COMPANY LEVEL ANALYSIS >

4.1 Data and methodology of the company level analysis

Data

The data used for the company level analysis is from three sources:

- First, data from DASTI on the participation of companies and individuals in the Industrial PhD Programme.
- Second, information on financial reports that companies above certain size thresholds must file to a public authority.
- Third, information on patenting activity from the European patent office.

The data from DASTI on the participation of companies and individuals in the Industrial PhD Programme contain information on the year an individual was employed as an Industrial PhD student, and in many cases also the employing company's registration number ('cvr-number'), which is filed at the public authorities and which is also available in the other datasets used in this study.

Data on financial reports is from the private information provider company Købmandsstandens Oplysningsbureau, now Experian A/S. This dataset, henceforth denoted as the KOB data, contains information from the financial reports that companies with a certain size and ownership structure must file to the public authorities.

Data on patenting is from the CEBR patent database, which has information on all patent applications at the European Patent Office by at least one applicant residing in Denmark.

The sample

In the original data from DASTI, there are 1,224 Industrial PhD projects in 536 different companies; 47 projects are registered as abandoned. Excluding these projects from the sample (including one project which lacks information on when the project was started) leaves us with 1,177 projects and 514 different companies.

However, it should be noted that in the original sample, the 514 different companies are defined by their names. This number is partly due to registering the same company under slightly different names in the DASTI data.

For the following performance analysis, we have to merge the sample of 1,177 projects in 514 companies with the information from the KOB database.

To accomplish this, we first had to find company registration numbers ('cvr'-numbers) of companies with missing or erroneous registration numbers in the original DASTI data. We managed to find these registration numbers for 509 different companies as defined by their names (hosting 1,161 projects). These 509 different company names in the DASTI data correspond to 445 different companies as defined by their company registration numbers. This is the definition of companies we will use henceforth.

The first Industrial PhD projects were initiated in 1988. Up to 2003, the number of projects initiated each year was relatively stable at approx. 30 to 50. However, in recent years the number of projects initiated per year has increased steadily and is now in the range of 80 to 120 projects. It should be noted that approx. 30 percent of the companies in the sample have hosted more than one project, and that some companies have hosted a considerable number of projects (e.g. more than 20).

1,053 out of the 1,161 projects and 387 out of the 445 companies can be identified in the KOB database. A large share of the attrition is related to companies which have either been established too recently to be covered by the KOB database or closed down before the KOB database assumed full coverage.

For 383 companies, there is financial report information in the KOB data. These companies are observed on average for 15.6 years, which implies that there are a total of 5,018 annual financial reports for companies that have hosted at least one Industrial PhD project. However, it should be noted that any potential bottom-line effects of Industrial PhD projects may take a couple of years to materialise, and that a considerable share of Industrial PhD projects was initiated at the end of the observation period.

Of the 383 companies in the KOB data, 72 companies are not observed after first initiating an Industrial PhD project. These obviously cannot be used for the following analysis, leaving us with 311 different companies have hosted a total of 851 Industrial PhD projects. Out of these, 195 companies have hosted only one Industrial PhD project, 48 percent have hosted two projects, 27 companies three projects, 9 companies four projects, and 32 (approx. 10 percent) companies more than five projects. There are also a few companies which have hosted more than 20 projects.

The companies with many projects are typically large companies for which it is difficult, if not impossible, to find similar companies for the comparisons in the statistical analyses to follow. Also, the statistical model which is preferred by the precision of its estimates requires fixing a year before a company first participates in the Industrial PhD Programme. For companies with many projects, this year is not well-defined, and the year before hosting the first Industrial PhD is often before the KOB database assumes full coverage.

Accordingly, we will only consider companies that have hosted a maximum of three projects for the following analysis. These represent approx. 85 percent of all companies participating in the Industrial PhD Programme, which leaves us with 270 companies for the company level analysis.

Of the 270 companies, approx. 120 are observed five years before first initiating an Industrial PhD project, approx. 160 are observed five years after, and 86 are observed ten years after first initiating a project.[9]

[9] However, it should be noted that missing information for a number of observations means that the number of records which can be used for the analysis is reduced. For example, total factor productivity figures are available for 91 companies five years after first initiating an Industrial PhD project, and for 46 companies ten years after first initiating a project.

The characteristics of the companies in the sample used for analysis are described in greater detail in the leftmost column of TABLE 4.2.1. In this table, we also summarise the characteristics of two control groups of companies, which are identified by a matching procedure briefly presented in the next section and explained in greater detail in Appendix 1.

TABLE 4.2.1: Descriptive statistics of the matched treatment-control samples

		All companies with a maximum of three projects			High-quality matches		
		Companies that have hosted at least one Industrial PhD project	Control companies	All companies	Companies that have hosted at least one Industrial PhD project	Control companies	All companies
Number of companies		270	539	809	129	283	412
	Total factor productivity	-0.056	-0.006	-0.023	0.090	0.016	0.039
	Gross profit per employee (DKK1,000)	1529.4	689.5	971.9	445.5	466.6	460.0
	Patent applications	2.4	0.7	1.3	0.8	0.3	0.5
	Number of employees	520.0	212.0	314.8	28.8	31.8	30.9
	Gross profit (DKK1,000)	651460.8	154181.9	320482.4	14828.6	15841.2	15522.5
	Total assets (DKK1,000)	18800000	951008	6894683	22515.98	22661.74	22616.1
	Establishment year	1978.8	1977.8	1978.2	1988.6	1988.2	1988.4
Industries							
	Business services	18.52	18.55	18.54	26.87	24.09	24.94
	Research and development	9.26	9.09	9.15	14.93	12.54	13.27
	IT	8.89	8.91	8.90	11.94	11.22	11.44
	Medical equipment, instruments manufacturing	8.52	8.53	8.53	7.46	7.92	7.78
	Finance	8.52	8.53	8.53	5.97	6.60	6.41
	Wholesale trade	7.78	7.79	7.79	5.97	4.95	5.26
	Chemicals, pharmaceuticals	4.81	4.82	4.82	3.73	3.96	3.89
	Food production	4.44	4.45	4.45	0.00	2.97	2.06
	Manufacturing	3.33	3.34	3.34	2.99	2.31	2.52
	Other	25.93	25.97	25.96	0.00	0.00	0.00
Zip-codes							
	1000-1999	11.85	11.13	11.37	12.69	11.55	11.90
	2000-2999	41.85	39.89	40.54	44.03	38.61	40.27
	3000-3999	10.00	9.83	9.89	11.19	10.89	10.98
	4000-4999	4.07	4.27	4.20	3.73	4.62	4.35
	5000-5999	7.41	7.98	7.79	8.21	6.93	7.32
	6000-6999	3.33	5.38	4.70	1.49	4.29	3.43
	7000-7999	4.81	3.53	3.96	0.75	1.98	1.60
	8000-8999	9.63	11.13	10.63	8.96	10.23	9.84
	9000-9999	7.04	6.86	6.92	5.22	4.29	4.58

Unsurprisingly, we find that Industrial PhDs are typically hosted by companies in knowledge-intensive industries. Also, hosting companies are geographically concentrated in the Copenhagen area (zip-codes below 3000).

Companies hosting Industrial PhD projects are, on average, relatively large companies with sometimes very high capital intensities (which is mostly due to the presence of large financial sector companies).

Methodology of the company level analysis

Our statistical model compares two groups of companies:
(a) companies that have hosted at least one Industrial PhD project, and
(b) companies that have not hosted any Industrial PhD projects.

In accordance with the academic project evaluation literature, the group of companies which have hosted Industrial PhD projects will henceforth be called the 'treatment group', while the comparison group of companies which have not hosted any Industrial PhD projects will be denoted as the 'control group'.

When interpreting the results of the statistical comparisons, one must take into account the fact that it is not possible to include all relevant factors in the models because they are unobservable in the data. Examples include different kinds of company competences and other immeasurable company characteristics.

This implies that interpreting any systematic treatment-control differences in company performance developments as genuine causal effects of hosting an Industrial PhD project will have to rest on an 'all-else-equal' assumption, i.e. the assumption that factors omitted from the model are either irrelevant or, on average, equal for treatments and controls.

To maximise the validity of this 'all-else-equal' assumption, we identify the control group using a matching procedure which ensures that we compare the treatment group companies with a control group of highly similar companies.

The identification procedure is described in greater detail in Appendix 1. Here, it may be sufficient to note that in the analysis to follow, we will compare developments in the success parameters over time of two groups of companies highly similar in a number of observable characteristics.

Of interest in the following analysis is whether treatment group companies experience more positive developments in the success parameters in association with hosting Industrial PhD projects compared to control group companies.

The modelling setup was chosen to generate the most precise estimates possible. However, it should be noted that the associated before/after comparisons imply that this procedure is only applicable to analysing companies that have hosted one or very few projects, as otherwise the timing issue cannot be resolved.

As a compromise between the precision of the before/after time period definition and having a sufficient number of observations for the analysis, we consider companies with a maximum of three Industrial PhD projects. As noted earlier, these companies represent approx. 85 percent of the participating companies.

The year that separates a company's pre-participation period from its post-participation period will be denoted as "year 0" or the "base year". For companies hosting an Industrial PhD project, year 0 is defined as the year before initiating the first Industrial PhD project. For a company in the control group, year 0 is the year in which it most resembled one of the project hosting companies in its base year.

Using this method, we can measure participating companies' developments in the success parameters before and after their base year - the year before initiating the first Industrial PhD project - and compare these developments to the developments of the control group companies.

4.2 Results of the company level analysis

In the following sections, the results of the company level analysis will be described. Here, two introductory remarks should be made:

Firstly, it must be assumed that it is practically impossible to isolate any performance effects of hosting Industrial PhD projects on large companies, as any contribution of an Industrial PhD project on aggregate company performance would be small relative to the companies' considerable heterogeneity in the success measures. For this reason, we will also present results for an alternative sample where companies with more than 300 employees or total assets of at least DKK 100 million in year 0 are not considered. This sample will be denoted the 'sample of small companies'.

Secondly, it proved to be difficult to find highly similar control companies for a number of treatment companies. For this reason, we also consider a separate sample of companies with less than 300 employees and total assets of less than DKK 100 million where these low-quality matches are excluded. This results in a sample of highly similar treatment and control group companies, denoted as the sample of 'high-quality matches'.

Before turning to the comparisons of the company performance parameters, we will address the question of how successful the matching procedure is in finding highly similar groups of treatment and control companies. Turning back to TABLE 4.2.1, which is a snapshot of the companies in year 0, we can compare the observable characteristics of the treatment and control group companies – both for the sample of all companies with a maximum of three Industrial PhD projects and their corresponding control companies, and for the sample of high quality matches.[10]

[10] Note that the sampling procedure implies that the base years of the two groups of companies is distributed highly similarly over time.

While industry and geographical distributions are almost identical for treatment and control group companies in the two samples (implied by the matching procedure), some of the very large Industrial PhD companies in the sample of all companies lack counterparts in the control group. In this sample of all companies, treatment group companies have a lower total factor productivity and a higher gross profit (which is consistent with a higher capital intensity) than the control group companies. However, the large heterogeneity in these variables implies that these differences are not statistically significant.

Companies in the high quality match sample are on average considerably smaller, younger and, of course, generally more similar in their observable characteristics.

We conclude that it was possible to find highly similar matches in terms of geographic location and company age. For the sample of high-quality matches, controls are also highly similar in company size.

Patenting activity

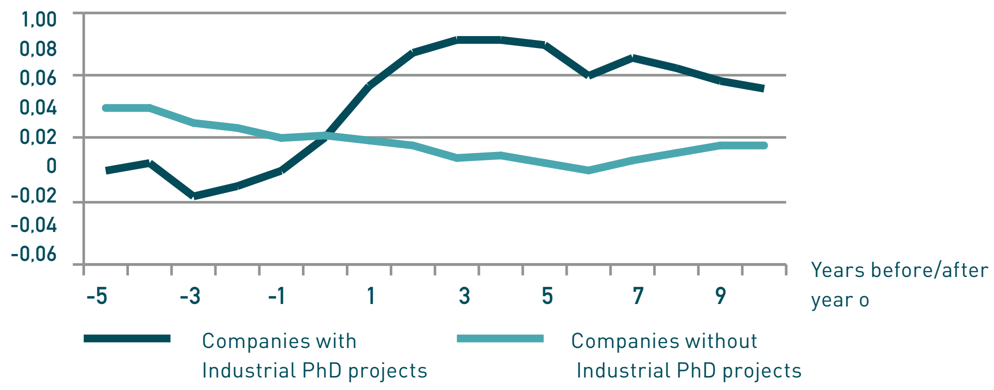
Patenting activity is measured by the company's number of patent applications per year.[11]

To isolate any Industrial PhD programme participation effects, we calculate for every company and year the difference between the number of patent applications in the given year and the number of patent applications filed in year 0.

FIGURES 4.2.1-3 display developments of these differences, i.e. current patenting activity relative to activity in year 0 for treatment and control group companies, respectively.

We find large movements over time for companies that host Industrial PhD projects relative to companies in the control group. This is likely to be a result of generally higher absolute patenting activity in treatment companies.

FIGURE 4.2.1: Number of patent applications, all companies



[11] An alternative measure would have been to consider granted patents. However, the long patent approval process renders it difficult to associate this variable to current innovation output.

FIGURE 4.2.2: Number of patent applications, small companies

Average number of patent applications per company relative to year 0

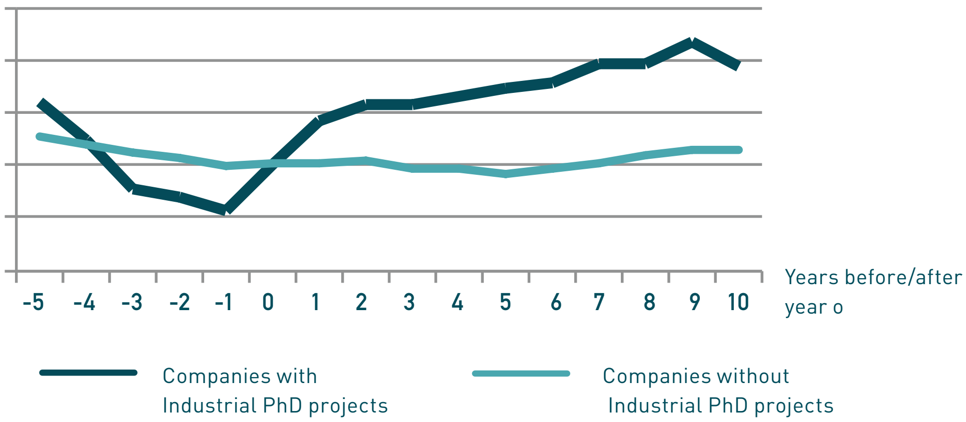
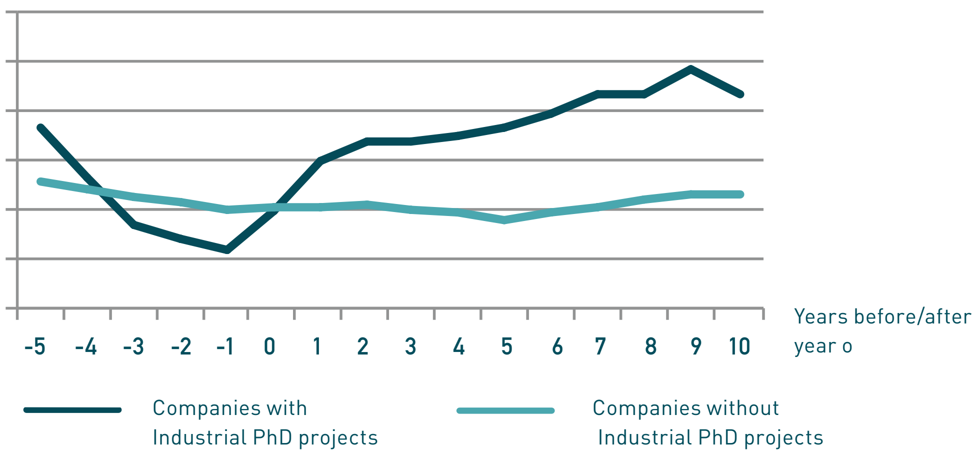


FIGURE 2: Number of patent applications, high-quality matches

Average number of patent applications per company, change relative to year before first initiating an Industrial PhD project



All graphs indicate that after year 0, the developments over time for treatment companies are equal to or larger than developments for control companies, indicating greater increases in patenting activity for the group of treatment companies compared to the group of control companies.

One could note that there are also differences between pre-base year trends in patenting activity depending on the sample under consideration, indicating the difficulties of finding control companies with patenting activities similar to the companies hiring Industrial PhDs.

Whether or not one is willing to interpret the graphs as evidence of positive effects of hosting Industrial PhD projects depends on one's underlying assumptions. E.g. in FIGURE 4.2.1, there is a positive trend of increasing patenting activity before hosting the first Industrial PhD project, but not in the years after. But over a longer time horizon, activity is higher after year 0 than before. So the interpretation of the results depends on whether one assumes that:
(a) trends would continue in the absence of programme participation, or,
(b) activity would stay at the same level in the long run in the absence of the programme.

The estimates of the statistical model presented below will be based on a pre-participation period specified as the five years up to year zero, and the post-participation period as the ten years after year zero. Obviously, the lengths of these time periods are computed are arbitrary, and the robustness of later results when choosing different before/after time intervals needs to be checked in the numerical analysis.

A look at the raw data reveals that participant firms in the sample of high-quality matches apply for on average 0.07 patents per year before year zero, and 0.18 after year zero (i.e., an increase of 0.11). Control firms have almost the same patenting activity both before and after year zero. Under the assumption that both groups of firms would have experienced the same developments in their patenting activity in the absence of the programme, the programme increases patenting activity with 0.11 patent applications per year.

To address the robustness of the graphs' suggestions and to quantify the strength of these associations in the data, we apply a model that estimates the expected percentage-point changes in the number of patent applications in a given year depending on whether the company is a treatment or a control company, and on whether the year under consideration is before or after the base year.

The results of this model are presented in TABLE 4.2.2. Of particular interest are the coefficients for the variable "The observation is after the base year and belongs to an Industrial PhD company". Under the assumption that patenting of treatments and controls would develop in similar ways in the absence of the programme, this variable identifies the genuine causal effect of hosting an Industrial PhD project on patenting activity.

TABLE 4.2.2: Count data regression results, dependent variable: number of patent applications in a given year. The table presents exponentiated coefficients, i.e. multiples of the number of patent applications when the logical conditions of the associated variable are fulfilled.

		Sample: all compa-nies with a maximum of three Industrial PhD projects		Sample: small companies		Sample: high-quality matches	
Variable							
	The observation is after year 0	0,88		0,76		0,84	
	The observation belongs to an Industrial PhD company	4,13	***	4,17	***	4,36	***
The observation is after year 0 and belongs to an Industrial PhD company		1,70	**	2,19	**	1,94	*
	Constant term	0,06	***	0,04	***	0,04	***

Notes: ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level. All regressions based on STATA Corp.'s 'xtpoisson' routine.

Findings of the statistical analysis of patenting activity can be summarised as follows: We find positive potential effects of hosting an Industrial PhD project for the sample of all companies with a maximum of three Industrial PhD projects. According to the estimates, hosting an Industrial PhD almost doubles (1.70) the number of patents per year in the years after year 0.

For the other samples, associations between hosting Industrial PhD projects and changes in patenting activity are also positive, and stay significant the ten-percent significance level also for the considerable reduced sample of high-quality matches.

In sum, one can conclude that there is evidence of positive associations between hosting Industrial PhD projects and changes in patenting activity in the data.[12]

[12] These relationships were robust when changing the lengths of the before- and after-base year periods considered for the estimations. Also, computing average numbers of patents of both participants and controls both before and after year zero, and estimating a linear model of the pre-post base-year differences revealed very similar (and also statistically significant) results.

Gross profit growth

The analyses of gross profit and TFP in the next subsection follow the same blueprint as the previous look at patenting activity. Recall that gross profit is the surplus of annual revenues over costs (excluding wages), and accordingly measures the value creation of a company in a given year.

First, for every year we calculate the difference between the year's gross profit and the gross profit in the base year (the year before the company first initiated an Industrial PhD project). Next, we calculate the average of these differences for both the group of treatment companies (which have hosted Industrial PhD projects) and the group of control companies (which have not hosted any Industrial PhD project).

FIGURES 4.2.4-6 show these averages for the treatment and control companies for the three different samples. They suggest that companies which host Industrial PhD projects are characterised by high growth in gross profit. While FIGURE 4.2.4, which compares all sampled companies both with and without Industrial PhD projects, show a decrease in the growth trend in association with hosting the first Industrial PhD project, FIGURE 4.2.5 and FIGURE 4.2.6, respectively comparing small companies and high quality matches, show a consistent gross profit growth which has no equivalent in the corresponding control group's gross profit growth pattern.

FIGURE 4.2.4: Gross profit developments (in DKK1,000), all companies

Average values relative to year 0

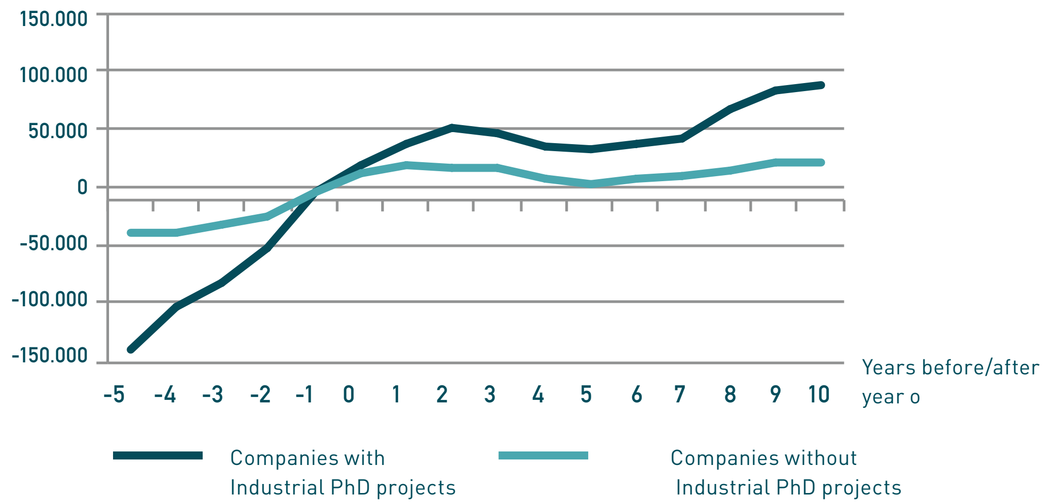


FIGURE 4.2.5: Gross profit developments (in DKK1,000), small companies

Average values relative to year 0

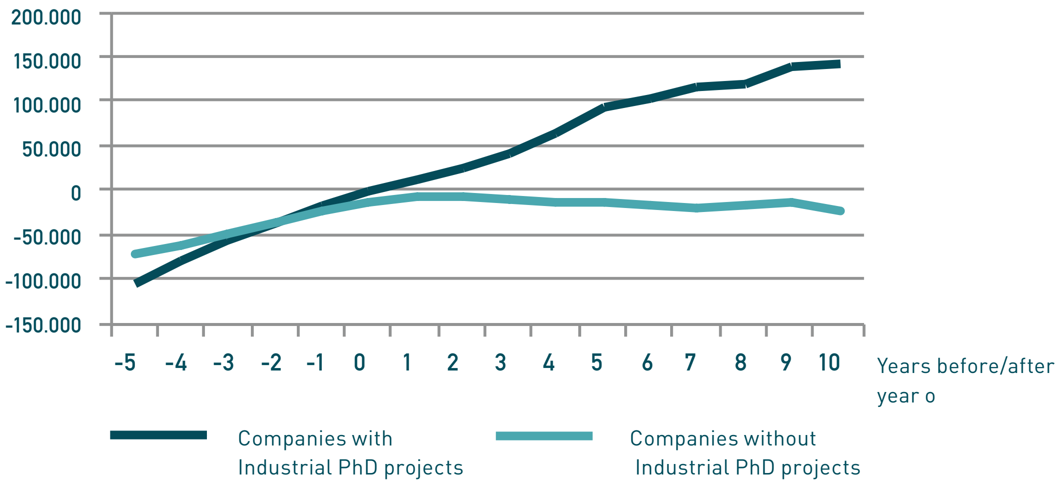
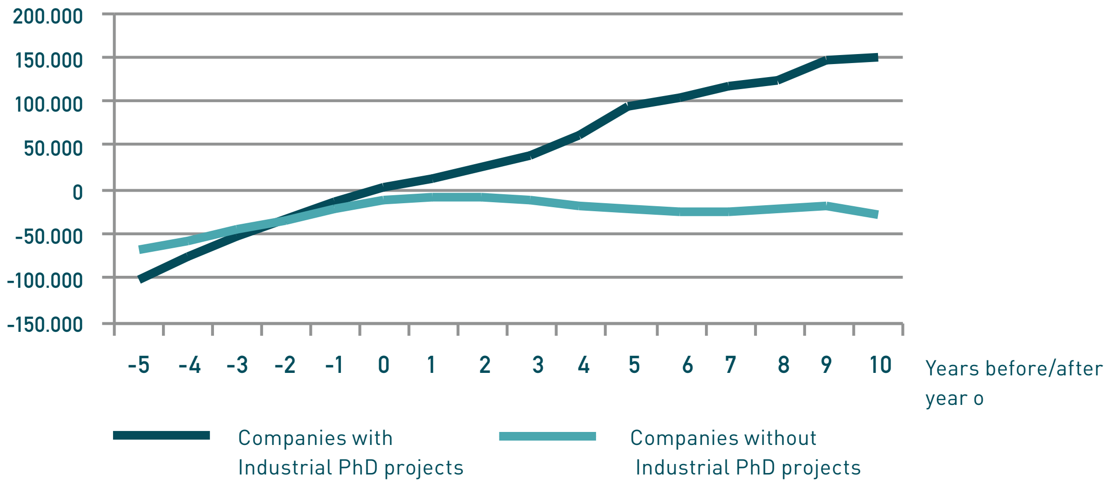


FIGURE 4.2.6: Gross profit developments (in DKK1,000), high-quality matches

Average values relative to year 0



Thus, assuming that treatment group companies in the absence of the programme would experience similar gross profit growth (or in this case: a similar decline in growth) as control group companies, the vertical distance between the graphs shows a considerable genuine (causal) effect on the gross profit growth of companies hosting Industrial PhD projects.

We turn now to formally estimating before/after year 0 differences in gross profit growth for treatment and control groups respectively. [13]

[13] We consider before/after differences in growth rather than levels, since gross profit levels show clear time trends which need to be taken into consideration in the estimations to avoid generating biased estimates.

Accordingly, we divide each company's observation period into two periods: one period before year 0, and one period after year 0. For every company and for both two periods, gross profit growth is measured by the average of the annual (absolute) increases in gross profit.

We can now compare these averages both over time and between treatment and control group companies. In the statistical model, we use the same variables as in the count data regression used of the patenting analysis as right-hand-side variables.

Hence, the difference between the developments in gross profit growth before/after year 0 for treatment group companies and the gross profit growth developments before/after year 0 for control group companies is measured by the coefficient associated with the variable: *"The observation is after the base year and belongs to an Industrial PhD company"* [14].

The results of this comparison, which is again carried out by using a simple linear regression model, are summarised in TABLE 4.2.3. The table shows the results for high-quality matches, i.e. the treatment and control group companies most similar to each other with regard to their observable characteristics, and for which the comparison accordingly has the highest validity.

TABLE 4.2.3: Linear regression results, dependent variable: annual increase in gross profit (in DKK1,000, in prices of 2007), sample: high-quality matches.

		Observation period: three years before to five years after year 0			Observation period: three years before to ten years after year 0		
Variable		Coefficient		Standard error	Coefficient		Standard error
	The observation is after year 0	-1792,35	**	764,36	-1422,68	*	797,83
	The observation belongs to an Industrial PhD company	-458,33		905,19	-458,33		906,09
The observation is after year 0 and belongs to an Industrial PhD company		2267,23	*	1259,42	1458,82		1457,88
	Constant term	1488,27	**	607,25	1488,27	**	607,86
	Number of observations	381			321		

Notes: ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level. Estimated with heteroscedasticity-consistent standard errors.

[14] E.g., if the increase in annual gross profit of treatment companies is on average DKK 5m before the base year and DKK 7m after year 0, and if gross profit for control firms increases on average DKK 3m before and DKK 4m after year 0, the coefficient associated with "The observation is after the base year and belongs to an Industrial PhD company", measured in DKK, is equal to (7m-5m)-(4m-3m)= 1m.

In the first model, which compares growth trends in the three-year period before and the five-year period after year 0, the coefficient of *"The observation is after year 0"* (-1,792.35) suggests that gross profit growth has slowed down by almost DKK 2m per year.

But the estimate of the coefficient for the variable "*The observation is after the base year and belongs to an Industrial PhD company"* of 2,267.23 implies that gross profit growth of Industrial PhD companies maintains its positive trend. So, for Industrial PhD companies, growth after first initiating an Industrial PhD project is approx. DKK 2m higher per year than would otherwise be expected if they had experienced a similar decline in gross profit growth as the control group companies.

So the approx. DKK 2m growth difference per year, implying an additional gross profit of (2+4+6+8+10) DKK 30m in the first five years of programme participation, is the genuine causal effect of programme participation, assuming that Industrial PhD companies' growth in gross profit would otherwise have followed the exact same pattern of the control companies if they had not participated.

It becomes clear that the programme might be considered successful even if only a part of this difference is because of a genuine causal effect of the Industrial PhD Programme.

When we compare the growth patterns of the two groups of companies between both the three-year time period before and the ten-year time period after year 0, the difference still suggests higher growth for participating companies, but becomes statistically insignificant (i.e. it becomes more likely that the finding is coincidental).

Total factor productivity

For this analysis, total factor productivity (TFP) was calculated on an annual basis for all companies in the entire KOB database in the given year.

Total factor productivity is gross profit 'corrected for' the number of employees and total assets. It is calculated as the residuals of a Cobb-Douglas-production function regression. In other words, TFP is the share of the company's value creation which cannot be explained by its number of employees or its capital stock.

Thus defined, TFP approximates the percentage-wise deviation in gross profit from the gross profit that we would have expected to observe, given the company's number of employees and its stock of assets.

For the analysis, we first take a look at the developments using a graphical depiction of the data. FIGURES 4.2.7-9 summarise.

FIGURE 4.2.7: Total factor productivity developments, all companies.

Average values relative to year 0

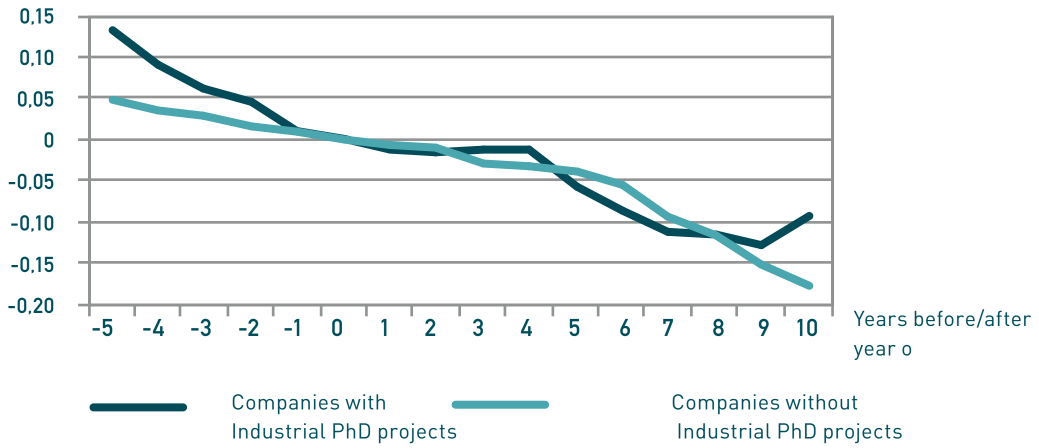


FIGURE 4.2.8: Total factor productivity developments, small companies

Average values relative to year 0

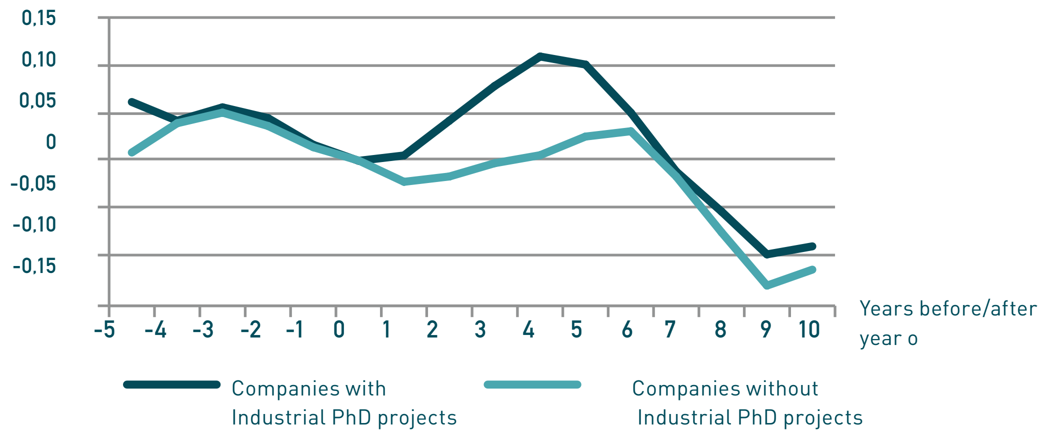
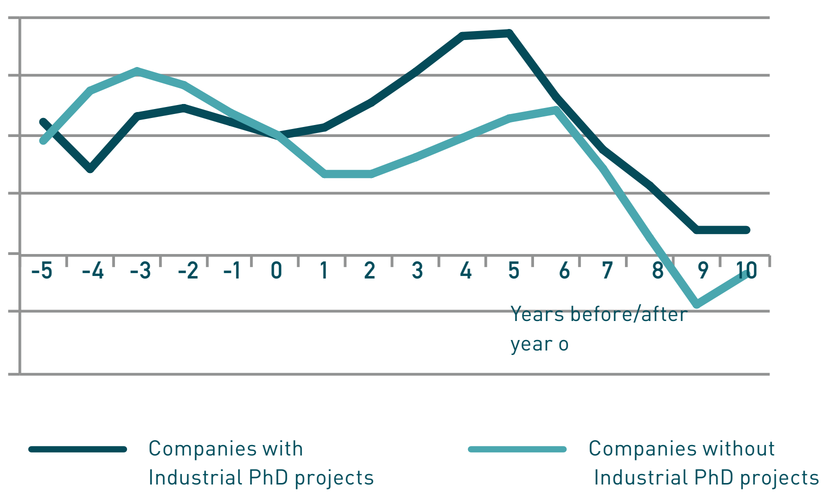


FIGURE 4.2.9: Total factor productivity developments, high-quality matches

Average values relative to year 0



The figures illustrate that developments are very different depending on whether or not large companies are excluded from the sample observed.

While there is a negative trend in TFP for the sample of all companies, there are no such trends for the subsamples. The erratic movements in the graphs (in spite of smoothing) suggest large heterogeneity in TFP over time and between companies.

For the subsamples, which are unaffected by the presence of very large companies, TFP is between 5 to 10 percentage points higher approx. two to six years after year 0 in the subsamples.
Again, we qualify the suggestions of the graphs by use of linear regression, the results of which are depicted in TABLE 4.2.4.

TABLE 4.2.4: Linear regression results, dependent variable: (TFP in a given year) - (TFP in year 0)

	Sample: all companies with a maximum of three Industrial PhD projects			Sample: small companies		Sample: high-quality matches		
Variable	Coefficient		Standard error	Coefficient	Standard error	Coefficient		Standard error
The observation is after year 0	-0,084	***	0,023	-0,050	0,036	-0,066	*	0,038
The observation belongs to an Industrial PhD company	0,057		0,028	0,008	0,043	-0,027		0,043
The observation is after year 0 and belongs to an Industrial PhD company	-0,002		0,040	0,042	0,064	0,068		0,067
Constant term	0,003		0,014	0,013	0,019	0,019		0,020

Notes: ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level. Estimated with heteroscedasticity-consistent standard errors

We see the negative TFP trends of companies hosting Industrial PhD projects and their counterparts in the high-quality match control group corroborated by the negative coefficients associated with the variable *"the observation is after year 0".*

Also, TFP has increased more (or decreased less) in the treatment group companies compared to the control group the samples of small companies and that of the high-quality matches.

This is indicated by the positive coefficients of the variable *"the observation is after year 0 and belongs to an Industrial PhD company",* which, for high-quality matches, show that companies which have hosted Industrial PhD projects have on average approx. 7 percentage points higher TFP than would otherwise be expected if they had experienced a TFP development similar to the control group companies.

Under the assumption that treatment group companies would experience TFP developments similar to those for control group companies in the absence of initiating Industrial PhD projects, this 7 percentage point difference is the most qualified assumption of the Industrial PhD Programme's causal total factor productivity effect. However, although positive, the TFP differences between treatment and control groups are too small compared to the large variations in TFP to interpret them as statistically significant, and must accordingly be interpreted tentatively. In conclusion, one cannot claim any strong association between hosting Industrial PhD projects and TFP development.[15]

Employment growth

We conclude the company level analysis by taking a look at employment growth. The finding of high growth in gross profit but not in total factor productivity might be an indication that companies hosting Industrial Phd projects are high-growth companies. This is strongly supported by a closer look at the data, illustrated by FIGURE 4.2.10, with companies hosting Industrial PhD projects being characterised by high growth in their number of employees both before and after first initiating a project.

To establish the statistical significance of this result, we formally test the growth difference by means of linear regression, the results of which (for high-quality matches) are presented in TABLE 4.2.5. The results of these regressions suggest that companies participating in the programme sustain an annual employment growth of approximately (-3.48-1.33+3.44+2.95=) 1.58 employees per year in the first five years after first initiating an Industrial PhD project, while companies in the control group decrease their number of employees by approximately -(2.95-3.48=) 0.5 employees per year. Qualitatively, this finding is independent of whether one follows the firms for five or ten years after the base year, and is statistically highly significant.

[15] This finding was robust to changes of the lengths of the time periods before and after the base year which were considered in the regressions. The findings was also robust to changing the regression model, e.g. using each firm's average total factor productivity in the time periods before and after the base year as the dependent variable, or using different specifications of the production function which was employed for the calculation of TFP.

FIGURE 4.2.10: Number of employees developments, high-quality matches

Average values relative to year 0

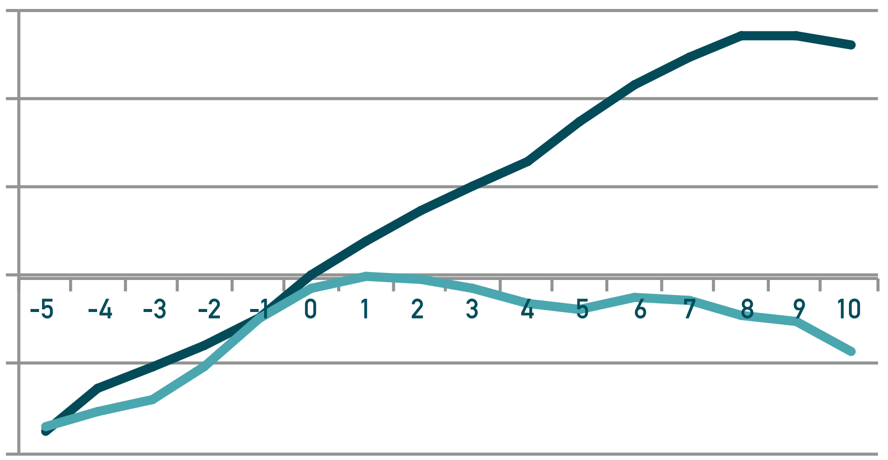


TABLE 4.2.5: Linear regression results, dependent variable: annual increase in number of employees. Sample: high-quality matches

	Observation period: three years before to five years after year 0			Observation period: three years before to ten years after year 0		
Variable	Coefficient		Standard error	Coefficient		Standard error
The observation is after year 0	-3.48	***	0.77	-3.13	***	0.73
The observation belongs to an Industrial PhD company	-1.33		1.00	-1.33		1.00
The observation is after year 0 and belongs to an Industrial PhD company	3.44	***	1.18	2.73	**	1.19
Constant term	2.95	***	0.65	2.95	***	0.65
Number of observations	349			267		
Notes: ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level. Estimated with heteroscedasticity-consistent standard errors.						

Notes: ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level. Estimated with heteroscedasticity-consistent standard errors

5 SUMMARY AND CONCLUSIONS

This analysis considers approx. 430 individuals and 270 companies which have participated in the Industrial PhD Programme and can be found in register data. On the individual level, we compare wage income and the occupations of Industrial PhD graduates with regular PhDs and individuals who have a university degree (and who are similar in terms of their fields of study, gender, etc.).

In the analysis, we take into account a set of demographic background characteristics, like age and gender, but also the average grade of the school-leaving examination, which to some extent controls for individual abilities.

On the company level, we analyse developments across four success parameters: the number of patents, gross profit and employment growth and total factor productivity. For a sample of companies which have hosted a maximum of three Industrial PhD projects before 2009, we identify a control group of highly similar companies which have not hosted any Industrial PhD projects, and compare developments in the success parameters between these two groups. Under identifying assumptions, these models isolate the causal impact of the programme on companies hosting Industrial PhD projects.

The results of the analysis can be summarised as follows: Industrial PhD earn approx. 7-10 percent higher wages than both regular PhDs and university graduates. They are more likely to be found at the top levels of their organisations' hierarchies compared to regular PhDs and more likely to be found in positions requiring high-level specialist knowledge than regular university graduates. Companies which host Industrial PhD projects see on average increasing patenting activity in association with hosting the projects. They are characterised by high growth in gross profit (value creation) and employment.

The comparison with a control group of highly similar control companies suggests that companies hosting Industrial PhD projects would have considerably less positive gross profit and employment developments if they did not participate in the programme.

We cannot find robust differences in total factor productivity developments between companies which have hosted Industrial PhD projects and companies which have not. This finding might be due to firm growth being negatively associated with productivity developments. [16] The relative high wages of Industrial PhD graduates, on the other hand, indicate that they have high individual productivity.

Summing up, earlier studies which found that Industrial PhDs are characterised by positive labour market outcomes have been corroborated. Findings on the company level indicate that the Danish Industrial PhD Programme also has positive effects for participating companies in terms of firm growth and patenting activity.

[16] This would be the case if there are decreasing returns to labour, which is one of economic theory's most standard arguments. Empirical support for this argument can be found in: Bingley, P., Westergaard-Nielsen N., 2004, "Personnel policy and profit." Journal of Business Research 2004; 57: 557-563.

6 APPENDIX 1: SELECTION OF CONTROLS

The KOB dataset is a panel dataset which has repeated observations for most of the companies - one for each annual account filed to the authorities. So for every company, there are typically multiple company-year observations (where a company-year observation refers to a record, i.e. a data-point of a given company in a given year). In the following, we will use the expression 'control observation' to describe a single company-year observation (record) of a control company.

Control companies are selected in the year in which they most closely resemble one of the companies participating in the programme, based on the participating company's characteristics in the year before hosting its first Industrial PhD project.

Note that the similarity between participating companies and potential control companies is determined by (a) the companies' region, size, age and industry, and (b) the expected probability of participation, which is derived as follows:

We run an auxiliary regression on the universe of approx. 370,000 company-year observations in KOB in the period from 1994 to 2008 which roughly resemble the group of participants (for example, we do not consider industries in which there is no single participating company).

The auxiliary regression is formulated as a simple probit model where the dependent variable is initiating an Industrial PhD project the following year, and company size, industry, region, productivity, total assets and time period as the model's right-hand-side variables. The regression's pseudo R squared, which is a measure of the model's goodness-of-fit, is 0.29, which we consider to be high.

The probit regression predicts how likely programme participation is for a given company. This allows us to find pairs or groups of companies for which this predicted probability is very similar. For two companies, A and B, with similar participation probability, the fact of company A participating and company B not participating can accordingly be interpreted as coincidental.

Under this interpretation, the identification setup resembles an experiment where programme participation is random, which would allow systematic differences in outcome variables between participants and controls to be interpreted as the programme's causal effect on participating companies.

Yet, even companies with similar predicted participation probabilities can be quite different, and to avoid systematic differences in industry affiliation, size, etc. between participants and controls, we also require that a number of observable characteristics are equal for a given participant and its matched control company(s).

To do this, we divide the total number of company-year observations into groups with the same industry affiliation, same geographic location, of similar size and observed in the same year.

For each participating company, we select the company-year observation of a non-participating company within the same group and with a participation probability closest to the participating company's participation probability. This selected company-year observation defines the participating company's control company, and the control company's 'base year' (or 'year 0') – which is the year in which it is most similar to one of the participating companies in its base year, and in which it is selected as a control company. For each of the control companies found by this procedure, the base year forms the basis for comparisons of given success parameters over time.

By repeating the matching procedure, we can find an arbitrary number of control observations for each participant. Here, a greater number of control observations increases the robustness of later results. However, increasing this number also makes it increasingly difficult to find highly similar control observations for some of the participants.

As a compromise between these two considerations, we choose to find two control observations (company-year observations of non-participants) for each participating company. The selection of the two control observations per participating company is made in two rounds. In each of the rounds we select one control observation for each participating company.

In the first round, we find 270 control observations of non-participating companies. In the second, we find another 269 control observations of non-participating companies (the reason for only 269 instead of 270 is that in a single case, one company-year observation is chosen as a control observation for two participants).

In each of the two rounds, we first require that many factors are highly similar when selecting control observations. This leaves a number of participating companies for which no control observations could be found. In subsequent steps, we reduce the number of factors and start choosing control observations which are increasingly less similar, until each round has identified one control observation for every participating company.

When control observations are equal in terms of industry (when distinguishing between at least 36 different categories), number of employees (at least 11 different categories), gross profit (at least 7 categories), time period (at least 7 different categories) and company age (at least 3 different categories), they are regarded as 'high-quality matches' in the analysis.

Note that in each of the rounds, we select only one control observation per participating company. This does not rule out selecting different control observations (belonging to different years) of the same control company. This implies that there are a number of control observations that occur more than once in the data forming the basis.

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The Innovation Consortium Scheme

– an analysis of firm growth effects

Copenhagen, April 2010

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EXECUTIVE SUMMARY >

This report has been prepared by the Centre for Economic and Business Research (CEBR). It presents an analysis of the economic impact of 'Innovationskonsortieordningen' (Innovation Consortium scheme, IC scheme) on participating firms.

The IC scheme is a Danish subsidy scheme granted by Rådet for Teknologi og Innovation (The Danish Council for Technology and Innovation, RTI) in cooperation with Forsknings- og Innovationsstyrelsen (The Danish Agency for Science, Technology and Innovation, FI).

This analysis follows 220 firms which have participated in at least one Innovation Consortium using a firm-register dataset. We primarily study firm level developments in two success parameters: gross profit and employment.

While employment is simply the number of employees in a given firm at a given point in time, gross profit is a measure of the firm's value creation.

In this study, we consider (absolute and percentage wise) growth in gross profit and the number of employees both before and after programme participation and analyse the changes in the growth patterns in association with participating in the programme. Moreover, we identify a control group of firms that do not participate (non-participants), but which are similar to the participants in terms of size, industry, and region.

Again, we can use firm-level data to calculate the changes in gross profit and employment for the non-participants, allowing us to address the question of whether participants have higher increases in growth than what would be expected on basis of the growth patterns of non-participants.

Under the assumption that gross profit and employment developments of participants and non-participants would be symmetric in the absence of programme participation, differences between the two groups of firms can be interpreted as the causal impact of the programme on participating firms.

The results of the analysis can be summarized as follows: It is possible to show that firms that participated in the IC scheme have experienced significant increases in the growth of gross profit and employment in association with programme participation. These results are robust to controlling for pre-participation growth and developments in the growth of firms in the control group.

Findings depend on the participant firms under consideration.[1] We, for example, find positive potential gross profit effects that are significant at the five percent significance level for firms with a gross profit below 150 million DKK (approx. €20) in the year before the programme. We also find potential employment effects for firms with less than 150 employees in the year before the programme.

For firms with gross profit less than DKK150 million in the year before participation, estimates show that, on average, annual gross profit in a participating firm has grown by an additional approx. DKK2 million per year relative to firms in the control group. This implied an on average approx. DKK20 million difference in annual gross profit after 10 years. It should be noted that one should be careful when interpreting this result, both because of statistical uncertainty and the possibility of participant and controls firms being different in unobserved factors potentially being important with regards to the observed differences. But when one relates the approx. DKK20 million difference to the programme's research subsidies – corresponding to approx. DKK3 million (approx. €370,000) per participant firm – it becomes clear that the programme is a success even in case of only a share of the observed gross profit differences owing itself to a genuine causal effect of the programme.

This result is robust to changing sampling conditions and using firms that applied for funding and got their application rejected as an alternative control group. Results for employment growth are not robust to using the alternative control group, and should thus be interpreted as being more tentative.

[1] For the largest participant firms, any effects of the programme are small relative to these firms' large variations in the success parameters, and inclusion of large firms in the sample renders impossible finding any potential positive programme effects.

SAMMENFATNING (DANISH SUMMARY) >

Denne rapport er skrevet af Centre for Economic and Business Research (CEBR). Den beskriver en analyse af Innovationskonsortie-ordningens potentielle effekter på udviklingen i de deltagende virksomheder.

Innovationskonsortie-ordningen er et virkemiddel under Rådet for Teknologi og Innovation (RTI). Rådet administrerer virkemidlet i samarbejde med Forsknings- og Innovationsstyrelsen (The Danish Agency for Science, Technology and Innovation, FI). Gennem Innovationskonsortier støtter RTI samarbejde mellem virksomheder og vidensinstitutionerne (f.eks. universiteter, GTS-institutter m.fl.).

Ved hjælp af registerdata følger analysen 220 virksomheder som har deltaget i ordningen. Vi studerer væksten i to succesmål: bruttofortjeneste og beskæftigelse.

Mens beskæftigelse er antallet af medarbejdere på et givet tidspunkt, er bruttofortjeneste et mål for virksomhedens værdiskabelse.

I dette studie betragter vi væksten i bruttofortjeneste og beskæftigelse både før og efter starten af programdeltagelsen. Yderligere identificerer vi en gruppe af kontrolvirksomheder som ikke deltager, men som ellers ligner de deltagende virksomheder i størrelse, branche, alder og region. Også for dem udregner vi vækst i bruttofortjeneste og beskæftigelse. Det betyder, at vi kan besvare spørgsmålet hvorvidt de deltagende virksomheder har haft højere vækst end man ville have forventet - ikke kun på basis af deres vækst før programdeltagelsen, men også på basis af udviklingen for kontrolvirksomhederne.

Ud fra antagelsen om at udviklingen i bruttofortjeneste og beskæftigelse ville være symmetrisk i fraværet af ordningen, kan differencen mellem de to gruppers udvikling fortolkes som ordningens direkte effekt for de deltagende virksomheder.

Analysens resultater kan sammenfattes som følger: Mindre virksomhederne, som har deltaget i Innovationskonsortie-ordningen, har oplevet større vækst i bruttofortjenesten og i antallet af medarbejdere end virksomhederne i kontrolgruppen, der ikke har deltaget. Disse resultater er robuste overfor at der korrigeres for væksten inden programdeltagelsen og korrigeres for udviklingen i væksten i kontrolgruppen.

Der skal dog lægges mærke til, at resultaterne afhænger af størrelsen af de virksomheder, som betragtes.

For eksempel er den potentielle effekt på bruttofortjenesten signifikant på et 5 % niveau for deltagervirksomheder, der havde under 150 millioner Kr. i bruttofortjeneste i året før programdeltagelsen. Differencen på bruttofortjenesten kan her estimeres til ca. 2 millioner kr. ekstra vækst i deltagervirksomhedernes årlige bruttofortjeneste om året. Dette betyder, at deltagervirksomhedernes årlige bruttofortjeneste er blevet forøget med ca. 20 millioner kr. over en ti års tidshorisont. Sådan en sammenligning skal fortolkes med en vis forsigtighed grundet statistisk usikkerhed, og det at forskellen sandsynligvis delvis skyldes faktorer, som analysen ikke kan tage højde for. Med programomkostninger svarende til ca. 3 millioner kr. pr virksomhed kan det dog konkluderes, at ordningen er en succes selv i tilfældet at kun en mindre del af differencen skyldes en kausal effekt.

Vi finder yderligere signifikant positive potentielle beskæftigelseseffekter for virksomheder, der havde mindre end 150 medarbejdere året før programdeltagelsen. Disse potentielle effekter svarer til ca. 50 ekstra ansatte over en fem til ti-års periode efter starten af programdeltagelsen.

Resultatet vedr. bruttofortjeneste er robust overfor ændringer i dataopsætning og overfor at man bruger virksomheder, hvis ansøgning om støtte til finansiering af deltagelsen i et Innovationskonsortium ikke blev imødekommet, som alternativ kontrolgruppe. Resultatet vedr. beskæftigelsesvæksten viser sig derimod ikke at være robust overfor at bruge denne alternative kontrolgruppe, og må dermed fortolkes med større forsigtighed.

1. INTRODUCTION >

This report has been prepared by the Centre for Economic and Business Research (CEBR). It presents an analysis of the economic impact of 'Innovationskonsortieordningen' (Innovation Consortium scheme, IC scheme) on participating firms in terms of growth and value creation.

The report is a follow-up to an earlier CEBR analysis (FI, 2007 and FI, 2008) and exploits the availability of more recent data, which allow following the participating firms for another 3 years.

Although this analysis is an evaluation of a specific subsidy scheme, its results might be of general interest, as schemes similar to the IC scheme have been implemented in a number of countries. However, general knowledge of their effects which can be integrated into cost-benefit analyses of these schemes is still rare.[2]

The IC scheme is a Danish subsidy scheme granted by Rådet for Teknologi og Innovation (The Danish Council for Technology and Innovation, RTI) in cooperation with Forsknings- og Innovationsstyrelsen (Danish Agency for Science, Technology and Innovation, FI).

ICs subsidise and facilitate cooperation between private firms and research and knowledge institutions (see next section 2 of the report for a more detailed description of the scheme). Cooperating institutions can apply for financial grants at the RTI/FI, and the grants subsequently finance the expenses incurred by the research and knowledge institutions whilst undertaking the cooperative project. Typically grants amount to DKK7-15 mio (approx. €1-2 million).

The IC programme has existed since 1995 (until 2003 under the heading "Centre Contracts"). Until 2003, 80 ICs covering 274 different firms (denoted participants in the following) had been completed, representing total grant costs of DKK766 million (approx. €100million), which corresponds to DKK2.8million (approx. €370,000) per firm.

This analysis follows 220 of these firms in a firm-register dataset that covers the period up to (and including) the year 2008. We study firm level developments in two success parameters: gross profit and employment.[3]

[2] See Schibany et al. (2004) for a study based on a similar Austrian subsidy scheme. Branstetter og Sakakibara (2002) consider a similar Japanese scheme and Adams et al. (2003) analyse the effects of the cooperation between private and public R&D for firms in the U.S.

[3] We also take a look at firm closure as an additional success parameter. However, given that this is not central to the analysis, the results of this exercise are reported in Appendix 3.

While employment is simply defined as the number of employees in a given firm at a given point in time, gross profit is defined as annual net sales subtracted annual costs of variable inputs (raw materials, energy, intermediate goods purchases, etc.) except labour costs. Gross profit is the most precise measure of the firm's value creation, but one should, of course, keep in mind that part of the firm's total value creation may be passed on to consumers, may be retained in the firm and increase its value (of which there is no data available for this analysis), or may take the form of positive externalities, such as knowledge and/or innovations, that benefits other firms or society as such.[4]

In this study, we consider (absolute and percentage wise) growth in gross profit and the number of employees both before and after programme participation. In addition, we also analyse the changes in the growth patterns in association with participating in the programme.

Moreover, we identify a control group of firms that do not participate (non-participants), but which are similar to the participants in terms of size, industry, and region. Again, we can use firm-level data to calculate the changes in gross profit and employment for the non-participants, allowing us to address the question of whether participants have higher increases in growth than what would be expected on basis of the growth patterns of non-participants.

Under the assumption that growth in gross profit and employment of participants and non-participants would be equal in the absence of programme participation, differences between the two groups of firms can be interpreted as the causal impact of the programme on participating firms.

The results of this exercise can be summarized as follows: Of the firms that participated in the IC scheme it appears that relatively small firms have experienced a significant increase in (the growth of) gross profit and employment.

It is important to note, that the size and statistical significance of these potential effects depend on the size of the firms under consideration. We, for example, find positive potential gross profit effects that are significant at the five percent significance level for firms with a gross profit below 150 million DKK (approx. €20) the year before the programme. We also find potential employment effects for firms with less than 150 employees in the year before the programme.

Finally, we also look at the survival rates of participant firms and compare these with the survival rates of firms in the control group. Here, we find high survival rates (most likely due to IC participants and their control counterparts being relatively large) and no difference in the survival rates of participants and non-participants.

[4] As a measure of knowledge creation, we could in principle also have considered firm-level patenting activity. No actual data on patenting activities were, however, available for this analysis.

2. DESCRIPTION OF THE IC SCHEME >

An innovation consortium is a flexible framework for collaboration between companies, research institutions and non-profit advisory/knowledge dissemination parties. An innovation consortium must consist of at least two companies that participate throughout the entire project, one research institution and one advisory and knowledge dissemination party. Additionally, an innovation consortium may involve or attach other types of partners that are considered relevant to the project.

The consortiums' collaboration should be based on a joint project aimed at developing and bringing research based knowledge to maturity, so that it can form the foundation for Danish companies' innovation in the years to come.

The joint project should result in the completion of high-quality research relevant to Danish companies. Furthermore, the project should ensure that the new knowledge is converted into competences and services specifically aimed at companies, and that the acquired knowledge is subsequently spread widely to the Danish business community – including in particular small and medium-sized companies.

Any project initiated by the consortiums must comply with the following:

- The project should have generic content and the results must be of relevance to a wide group of companies.
- The project should be at a high level of innovation and research.
- The project should not have the character of product development for individual companies.
- The project should require close collaboration between the consortium parties.
- The project should have a duration of two to four years.

The role of companies in the consortiums is to ensure that the joint research and development project is based on relevant development needs within Danish companies. Consequently, the project theme should be of significance to the participating companies' business development. However, it should not take the form of actual product development.

The company participation is also to ensure that the business community's knowledge and competences are utilised in the project. Therefore, the participating companies should contribute knowledge and competences at a high level within the project field.

The companies may be Danish or foreign (or both).

Over the period 1995-2003, 274 different firms have participated in an IC, but a number of firms have participated more than once. On average there were approx. 40 firms starting to participate in an IC per year, but there are large differences across years, with the years 1998-2000 being characterised by the highest activity with on average almost 70 firms starting to participate.

Approx. 50 percent of all participating firms are in manufacturing, 25 percent are in financial or business services and 15 percent are in trade in services.

3. DATA >

The data for this analysis comes from three sources:

1. Data on program participants, which were assembled by CEBR based on FI's (paper) file records of the IC-programme for an earlier analysis (Forsknings- og Innovationsstyrelsen, 2007 and 2008). These data will in the following be called 'IC data'.

2. Data from the private information provider company Købmandsstandens Oplysningsbureau, now Experian A/S. This dataset, henceforth denoted as the KOB data, has information from the financial reports that firms of a certain size and ownership structure are obliged to file at the public authorities. Thus, there are typically a number of observations for a given firm (one for each annual account), denoted firm-year observations in the following.

3. Information on firm transitions (e.g., mergers, liquidations or bankruptcies) are included from the 'cvr-register' of the Danish Commerce and Companies Agency (Erhvervs- og Selskabsstyrelsen). These data will be put to use when we analyse survival probabilities of participating firms.

Note the KOB data provide information on a host of accounting-related variables, including employment and gross profit. Note that only large firms are obliged to file information about sales. This would make sales growth a potentially skewed indicator of the IC impact upon firms.

The KOB data include firm-level information about industry and geographical location, which will be exploited later when we identify a control group for the empirical analysis.

4. SAMPLING >

There are a total of 405 firm observations in the IC data over the period 1995-2003. These belong to firms that participated in one of the programmes which go under the umbrella 'Innovationskonsortier'. For 19 observations, it was not possible to identify the point in time when the project was started, for another 35 observations it was not possible to find firm-identification numbers which were necessary to match the IC data with the KOB data (leaving us with 351 observations).

A number of firms are registered more than once in the data, because they have participated more than once in the programme. We treat participation as a zero/one variable, independently of how many times a firm has participated, and consider the earliest time a firm is registered as participating as the starting point of programme participation. This leaves us with 274 firm observations.

For 20 of these firms, there is no information in the KOB database, which leaves us with 254 observations, and for 34, there is no accounting information in the KOB data before the start of the program. This information, however, is necessary for the before-after estimation set-up employed in the following. So we are left with 220 participant firms for the analysis.

As in any firm accounting database, considerable variation can be observed in the KOB-data, which owes itself to some firms being part of corporate groups, organizational changes and/or because firms change accounting policies and practices. We treat this issue differently depending on the stage of the analysis, which is, basically, divided in two steps:

As a first step, we identify a control group of comparison firms. In this step, we will exploit the total universe of firms available in the data, independent of missing observations or zero reporting.

As a second step, we compare the performance of participant firms with the performance of firms in the control group. In this step, there is a need to make decisions of how to treat the data in case of missing values in the data or when firms report zero activity. This will also direct our robustness checks of the results of the analyses. In this context, we will commit to one sampling strategy, and subsequently check the robustness of the results when changing the strategy.

In essence, we want to analyse samples that are as 'clean' as possible, i.e., concentrate on firms which report regularly, and which do not raise suspicions of significant organizational or accounting issues. By implication:

(a) When analysing gross profit we consider the 61 percent of firms that do not report zero gross profit in the KOB database. The argument being that, if there is any economic activity, zero gross profit is an event having (almost) zero probability, indicating non-reporting rather than gross profit being zero.

>

(b) When analyzing employment growth, we consider the 62 percent of all firms that report a strictly positive number of employees in the KOB data.

Our sampling scheme implies that we start the analysis with the cleanest data possible. Obviously, robustness checks will address whether these decisions are critical for results. By implication, the sensitivity of results with regard to the rather restrictive sampling scheme will be addressed subsequent to the presentation of the performance comparisons.

5. IDENTIFICATION OF THE CONTROL GROUP >

To identify the control group of this analysis, we use a 'matching-on-observables' technique, which can be seen as the workhorse of programme evaluation (see, for example, Woolridge, 2002).

According to this method a control is identified for each firm participating in the scheme. Except for not having participated in the programme, the controls are selected to be as similar as possible to the given participant firm before programme participation. In the following, these comparison firms will be called 'control firms' or just 'controls'.

The details of the identification process are described in Appendix 1 of this report. At this place it may be sufficient to note that, in the latter analysis, we will compare developments in gross profit and employment over time of two highly similar groups of firms, one which consists of the programme participants, the other of the controls (non-participants).

Note also that the selection of highly similar controls increases the realism of the assumption that participants and controls would have had similar developments in gross profit and employment in the absence of the programme. By this, differences in the developments can be interpreted as the programme's genuine causal effect. The selection of highly similar controls is an improvement of CEBR's earlier analysis (Forsknings- og Innovationsstyrelsen, 2007 and 2008), which simply uses private sector firms for comparison purposes.

For participants, we will also compare the growth in employment and gross profit in the time period before participating in the IC scheme with the growth in employment and gross profit in the years after having participated. The cut-off year which separates the pre-participation period from the after-participation period is the year just prior to participation. This year will in the following be denoted the 'base year'.

For controls, we also define a base year, which now refers to the year the given control was selected. This is the year in which it most closely resembled one of the participants in its base year. So we can also compare controls' growth in gross profit and employment between before and after the base year.
In the analysis, we will consider the growth of any of the two success parameters

(gross profit and employment) before and after the base year, where growth will be measured both as absolute and percentage wise annual increases. We will analyse changes in growth between before and after the base year, and will compare these changes between participants and controls.

E.g., when growth accelerates after the base year for participants, but not for controls, this indicates positive programme effects. The acceleration is interpreted as the programme's causal effect for participating firms under the ('identifying') assumption that participants' growth would accelerate by just as much as the controls in the absence of the programme.

Note this set-up further improves upon the method employed in CEBR's earlier evaluation (Forsknings- og Innovationsstyrelsen, 2007 and 2008). Here, the evaluation was based on a comparison of the levels of participant's and control's success parameters and, thus, addressed the question whether participants had grown faster than non-participants.

This methodology could not take into account the possibility that participant firms might generally have higher growth independent of whether they decide to participate in the programme or not. Any inherent growth difference between participants and controls, however, should show in the years before the base year and, thus, can be controlled for in the present analysis.

This is achieved by no longer comparing pre-participation levels of success parameters with post-participation levels. Instead, we compare pre-participation growth (or increases) with post-participation growth (or increases). So the evaluation is based on participant-control differences in the acceleration of growth, rather than just growth differences. This allows taking account of innate growth differences that can be measured before programme participation (for participants) or before the base year.

In analyzing growth developments, we will in the following consider both absolute and (approximately) percentage wise changes in the growth in gross profit and employment – the latter being measured by increases in the logarithms of these two success parameters.

There are good reasons for analyzing both absolute and relative changes in firm level growth. Considering absolute increases allows us to make statements in absolute terms, (e.g., 'ICs increase participants' gross profit by on average XYZ DKK') which can be integrated into cost-benefit analyses, whilst inclusion of relative (percentage-wise) growth gives greater weight to smaller firms in case of absolute programme effects being larger for larger firms. If, for example, the programme is assumed to have a proportional effect on growth rather than increasing gross profit by the same amount for all participants independent of their size, then the analysis of relative growth will allow us to estimate these proportional effects.
However, it should be noted at this point that percentage wise growth can only be measured for those firms that have positive gross profit (or nonzero employees) in

the base year and the years to follow. By implication, analyzing growth instead of increases necessarily restricts the sample to these firms.

The estimation set-up of potential programme effects is explained in greater detail in Appendix 2 of this report. At this point, it should, however, be noted that we only look at firms, which we can follow for at least three years before the base year and five (in a second iteration: ten) years after the base year.

When considering absolute increases instead of percentage wise growth, we calculate the average of each firm's annual increases in the success parameter in the three year period before the base year. This defines a firm's average annual increase in the pre-base-year period. Also, we calculate for each firm the average annual increase in the success parameter in the five (ten) year period after the base year, which defines the firm's average increase in the after-base year period.

This implies that we have two observations for each firm: one describing average increases before the base year, and one describing average increases after the base year. As a result, we can calculate for each firm, whether average annual increases have become larger or smaller in association with passing the base year. In other words, we can evaluate the development of average annual increases.

So this study's performance analysis takes a look at participants' average increases in the average annual increases in association with participating in an IC, and compares them with the average increases of the annual increases for controls following their assigned base year. If the increase in the average annual increases is larger for participants than controls, this implies that there is a more positive change in growth developments of participants than controls. Thus, the comparison estimates the potential effect of the IC programme on the participating firms.

In this case, any differences in the increase of annual growth can not be interpreted as the result of different pre-base year developments, nor can it be explained by reference to differences in the two group's characteristics given the similarity of participants and controls (and given that we additionally include some control variables in the models to take account of potentially remaining differences). In short, this approach makes it more likely that positive differences between participants and controls must be attributed their participation in IC schemes.

7. DESCRIPTIVE STATISTICS >

The above-mentioned identification procedure yields 439 control observations belonging to 334 different firms, implying that repeated observations occur for a number of control firms.

To interpret the results of the following analysis as measuring the impact of the programme, one needs to assume that participants and controls would experience the same changes in growth if not it was for the programme. It can be argued that this assumption becomes increasingly realistic the more similar the participants and controls are in terms of their observable characteristics. Hence, we have sought to identify a highly comparable group of controls.

TABLE 1 illustrates how successful we were at identifying a group of controls that is highly similar to the group of participants by describing both groups of firms in the base year.

We find that the distribution across industries is highly similar for the two groups, but that there are differences w.r.t. the mean size of the participants vs. controls. Also, participants are slightly more concentrated in the Copenhagen area (zip codes below 2999). The size difference between participants vs. controls owes itself to the fact that some participating firms belong to the biggest firms in Denmark, for which it is not possible to find controls of similar size.

Although we of course will test differences in the success parameters between all participants and controls, any effects of the programme might in this case be undetectable as they may be washed out by the large variations in the success parameters in large firms for reasons outside the statistical models.

As a consequence, we will analyse different samples distinguished by the maximum size of the firms under consideration. As a starting point, we consider small and medium size firms separately. More specifically, employment growth will be analysed separately for firms below 300 employees in the base year. Growth profit will be analysed separately for firms with gross profit less than 150 million DKK (approx. €20 million) in the base year. Although it may appear restrictive, these thresholds imply that the resulting samples still represent approx. 75 per cent of all observations, reflecting the large share of SMEs in Denmark.

For these subgroups of firms, expected unobserved heterogeneity is smaller and, thus, the power of the analysis' statistical tests (i.e., the probability of finding effects in case there are any) is larger compared to the sample where large firms are included. Also, participants and controls are more similar in their observable characteristics, which increases the realism of the 'identifying' assumption that, in the absence of the programme, growth developments would be similar for participants and controls.

Please note that the chosen size thresholds are completely arbitrary, and constitute a compromise between being representative for the entire population on the one hand and the desired robustness of findings and the realism of the identifying assumption on the other. Note, moreover, that the thresholds can be moved easily, and we will do so to test how this affects analyses and results.

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TABLE 1. Mean values of key variables for participants and controls in the base year

	All firms		Firms with less than 300 employees in the base year	
	Participants	Controls	Participants	Controls
Number of employees	612	279	83	87
Gross profit (1000DKK)	364.271	145.825	51.439	42.554
Industries (shares of total)				
Agriculture	0,005	0,005	0,000	0,003
Construction	0,036	0,039	0,030	0,027
Electricity	0,009	0,007	0,007	0,009
Finance, business service	0,255	0,260	0,289	0,247
Manufacturing	0,500	0,499	0,452	0,509
Trade, hotels, restaurants	0,155	0,155	0,178	0,165
Transport, telecom	0,014	0,009	0,015	0,012
Services	0,014	0,014	0,015	0,015
Not stated	0,014	0,014	0,015	0,012
Region (zip codes)				
1000-2999	0,455	0,380	0,444	0,363
3000-3999	0,068	0,068	0,089	0,061
4000-4999	0,064	0,064	0,059	0,064
5000-5999	0,041	0,046	0,037	0,046
6000-6999	0,077	0,109	0,074	0,091
7000-7999	0,077	0,093	0,096	0,110
8000-8999	0,159	0,169	0,141	0,189
9000-9999	0,059	0,071	0,059	0,076

For the sub-sample of firms with less than 300 employees in the base year, we find the difference in the number of employees in the base year between participants and controls to be within the 'natural' statistical variation, i.e., not significantly different from each other at any commonly used significance level. However, participants remain overrepresented in the Copenhagen area (a finding which is significant at the 10% significance level), and have higher gross profit in the base year (significant at the 10% level).

For the sub-sample of firms with gross profit less than 150 million DKK in the base year, we find gross profit (and the number of employees) in the base year to be not significantly different between participants and controls at any commonly used significance level. However, participants remain again overrepresented in the Copenhagen area (significant at the 10% significance level).

In total, there are 10,167 firm-year observations belonging to 554 different firms. Note here that the same control firm may occur repeatedly in the data, if more than one of its firm-year observations were selected by the procedure outlined in Appendix 1.

It should be noted here that there are only relatively few observations that enable us to follow firms long before and long after the base year: only firms that participated early in the programme or the controls associated with these firms can be followed over a long time period after having participated or selected as controls.

This is, for example, reflected in the fact that there are only 15 observations with employee information available ten years before base year. There are, however, 106 observations where data is available eight years before the base year, and 275 observations where data is available five years prior to the base year. Five years after base year we have information on 340 firms, whilst 178 firms remain in the database ten years after base year.

Only part of this attrition is due to firms leaving the data before the end of the observation period: Of the 554 firms in the final sample, approx. 25 per cent leave the data before 2008.

8. RESULTS >

The following considers developments of employment and gross profit over time, and compares these developments between participants and controls. This performance analysis is split up into two parts:

1. The first part of the performance analysis is based on two subsamples excluding large firms: one, in which gross profit is below DKK 150 million in the base year, and another one, in which the number of employees is below 300 employees in the base year.

2. The second part of the performance analysis is based on a set of alternative samples and extends (and checks the robustness of) the previous results. Results are reported in section 9 of this report.

Choosing sub-samples of relatively small firms as the point of departure for the performance analysis, instead of the total sample, is motivated by the following reasons:

First, we have difficulties finding highly similar controls for large participants such as, for example, multinationals in specific industries of which there are only a few in Denmark. As result, the assumption necessary to identify causal effects of the programme, which is that firms in both groups would change their growth patterns in the same way in the base year if not it was for the presence of the programme - can be argued to be more realistic for a sub-sample of small and medium size firms than in a sample that include the few, very large companies.

Second, we find that results for this subgroup are well-suited to illustrate the estimation technique employed to answer the question of whether findings should be interpreted as being the result of underlying processes (in which case they are 'statistically significant') or just 'coincidental'.

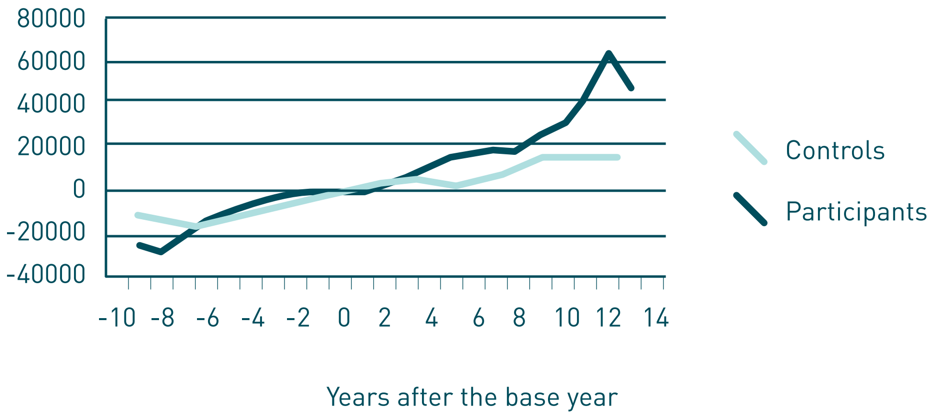
Still, as mentioned already, the chosen cut-offs are, of course, arbitrary. Hence, the robustness of findings with respect to changing the thresholds will be discussed in section 9.

A last point to mention here is that we will depart from only analyzing firms that always report nonzero and non-missing information. Again, we will subsequently check whether these strict sampling conditions are critical for the results.

8.1 Gross profit developments

After these introductory remarks, we are now ready to take a look at the numbers. A graphical depiction of the absolute differences in gross profit is displayed in FIGURE 1:

FIGURE 1: Gross profit (in DKK1,000). Mean differences compared to base year. Firms with gross profit less than DKK150 million in the base year. 3-year moving averages



We find similar increases in gross profit for participants and controls in the years before the base year. This suggests absence of any inherent differences in gross profit growth between the two groups of firms, which also indicates that the matching procedure succeeded in finding a group of controls of similar inherent growth compared to the group of participants.

After the base year, the gaps between the graphs widen, with participants having larger increases in gross profit compared to the controls. Under the assumption that participants and controls would have continued their pre-participation (pre-base-year) growth patterns in the absence of the programme or would have changed their growth patterns in the same fashion, the higher increase in the group of participants measure positive effects of the programme on participants' employment and gross profit.

We will have a closer look at the size of the differences between participants' and controls' growth patterns in a more formal treatment below. For now, we may note that, if pre-base-year trends are indeed equal, the graphs suggest participation in an ICs to have a gross profit effect of approx. DKK13,4 million five years and approx. DKK15,4 million ten years after the base year.

Obviously, a next step is to establish evidence on whether or not the finding of diverging growth trends is statistically significant, i.e., the result of underlying mechanisms, or just incidental and within the statistical variation which must be expected for firm data typically being characterized by large variations.

However, before addressing this issue, some minor remarks regarding FIGURE 1 (and those figures to follow) might be in place: Note that firms, to be observable long after the base year, need to have participated or have to be selected as controls at the time of the start of the programme in the mid-nineties, and must not have left the data before the end of the observation period. Also, to be observable long before the base year, firms need to have started to participate or been selected late in the observation period, and need to have existed long before the base year.

As a result, there exist only a limited number of observations long before and after the base year, implying that findings based on these observations get increasingly tentative at the left and the right sides of the figures.[5] Also, when determining (linear) growth trends before and after the base year, observations long before and after the base year are given a higher weight, so firms with high or low growth have a higher leverage on trend estimates when being observable for extended time periods.

Note also that observations long after the base year belong to the same cohort or nearby cohorts, and findings for these observations may be due to business cycle effects - which does not matter for the results of the analysis unless business cycles affect participants and controls in different ways.

To establish evidence on whether or not the above differences in the two groups' growth patterns are statistically significant, i.e., too large compared to the general variation in the data to be considered coincidental, we employ the regression model as described in section 6 and Appendix 2. Results for the changes in gross profit developments in association with programme participation relative to the changes in the group of controls are summarized in TABLE 2A and TABLE 2B:

[5] Of course, one could right-censor the graphs at, say, ten years after the base year to avoid that large variation at the end of the observation period steals the picture. This would, however, be highly arbitrary and even manipulating, leading us to present results for the entire observation period independently of the number of observations long before and long after the base year.

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TABLE 2A: Growth in gross profit up to five years after the base year: Diff-in-diff regression results

	Model 1: Dependent variable: Average annual increase in gross profit in either the three-period up to the base year or in the five-year period after the base year		Model 2: Dependent variable: Average annual growth in gross profit in either the three-period up to the base year or in the five-year period after the base year	
	Coefficient	Standard error	Coefficient	Standard error
Constant term k	-1914,5	2.496,4	0,100	0,206
Observation is after base year, d1	-2425,2***	681,9	-0,086**	0,035
Observation belongs to a participant, d2	-846,8	1.090,9	-0,066	0,055
Observation belongs to a participant and is after the base year, d1d2	3668,9**	1.738,3	0,145**	0,067
	R2=0.13 517 observations		R2=0.30 510 observations	

Notes: *** significant at 1%. ** significant at 5%, * significant at 10%; only firm observations with positive gross profit are used in Model 2; gross profit is measured in DKK1000. The following set of controls was included in the regressions: Seven industry dummy variables, eight dummy variables for the firms' geographical regions, three calendar time dummy variables for when the firm has its base-year, and six dummy variables describing the firm's gross profit in the base year. Base category: firms in manufacturing industries, with gross profit 0-500 million DKK in the base year and zip-code <3000.

TABLE 2B: Growth in gross profit up to ten years after the base year: Diff-in-diff regression results

	Model 1: Dependent variable: Average annual increase in gross profit in either the three-period up to the base year or in the ten-year period after the base year		Model 2: Dependent variable: Average annual growth in gross profit in either the three-period up to the base year or in the ten-year period after the base year	
	Coefficient	Standard error	Coefficient	Standard error
Constant term k	138,5	1.889,7	0,399***	0,093
Observation is after base year, d1	-696,5	827,5	-0,086***	0,018
Observation belongs to a participant, d2	-940,9	1.030,4	-0,030	0,038
Observation belongs to a participant and is after the base year, d1d2	1.981,7	1.916,7	0,121 **	0,058
	R2=0.17 399 observations		R2=0.38 390 observations	

Notes: *** significant at 1%. ** significant at 5%, * significant at 10%; only firm observations with positive gross profit are used in Model 2; gross profit is measured in DKK1000. The following set of controls was included in the regressions: Seven industry dummy variables, eight dummy variables for the firms' geographical regions, three calendar time dummy variables for when the firm has its base-year, and six dummy variables describing the firm's gross profit in the base year. Base category: firms in manufacturing industries, with gross profit 0-500 million DKK in the base year and zip-code <3000.

The coefficient estimates presented in the tables have the following interpretations:

- the constant term k estimates the average annual increases for a specific subgroup of controls (in this case controls in manufacturing, with gross profit between zero and 500 million DKK and with zip code less than 3000) before the base year,

- the coefficient associated with d1 estimates the difference in the average annual increases (or the increase in annual growth) for all controls between before and after the base year,

- the coefficient associated with d2 estimates the difference between the average annual increases (growth) between participants and controls before the base year.

- The coefficient associated with d1d2 estimates the difference in the increases of the average annual increases (growth) between participants and controls.

To illustrate, consider the case where we follow firms over ten years (TABLE 2B). After the base year, the average annual increase in the gross profit of controls is DKK 696,500 (approx. €95,000) lower than before the base year. Participants' average annual increase in the years before the base year is DKK 940,100 (approx. €130,000) lower than the controls'. Finally, the difference in the increases of the annual average increases between participants and controls is found to be 1,981,700 DKK (approx. €260,000). As a result, the average annual increase of the gross profit of participants in association with participating in the IC programme exceeds the controls' increases by almost two million DKK in the ten-year period after the base year.

Turning to TABLE 2A, we find that the average annual gross profit increase for participants over the first five years after the base year is approx. 3.6 million DKK higher (and statistically significant at the 5% level) compared to what would be expected in absence of participation in the IC scheme.

Looking at relative change (average annual logarithmic differences translating interpreted as average annual percentage wise growth), we find that the average annual growth in gross profit for participants over the first five years after the base year is approx. 15 per cent higher compared to what would be expected in absence of participation in the IC scheme.

Note the percentage-wise growth difference gets smaller when one only considers firms above a certain size in the base year. E.g. when only considering firms with gross profit above 50 million DKK in the base year, the estimated average annual growth difference goes down to approx. eight percent but remains to be statistically significant at the 10% level. Hence, we can conclude that the positive differences in the growth of gross profit is not (exclusively) driven by very small firms.

Over a ten-year period, the average annual increase in excess of what would be expected in absence of the programme for participation is (as noted earlier) approx. two million DKK, and growth is approx. 12 per cent higher than in the absence of the programme.

In summary, our findings agree with the presence of considerable effects of the IC programme on participants' increases on gross profit. Findings for both absolute and logarithmic differences are statistically significant at the 5% significance level for firms followed over the first five years after the base year and significant at the five per cent level for percentage-wise increases for those firms which are able to follow for at least ten years after the base year.

The finding of estimated differences in average increases being 3.6 million DKK when one follows firms over five, and approx. two million DKK when one follows them over ten years might indicate that differences in absolute increases are largest in the years directly following the start participation (the base year).

8.2 Employment developments

The analysis of employment developments follows the blueprint of the previous subsection. We, however, depart from focusing on firms with less than 300 employees in the base year, which always report nonzero and non-missing employment information, and leave the consideration of different samples to the next section of this report.

Firms under 300 employees in the base year represent approx. 75 percent of the total sample of firms, and approx. 71 percent of the present sample of firms which never report missing or zero employment information.

For each firm, we consider average annual increases and annual average growth rates for the three-year-period before the base year and the time period between the base year and five years later. As a second step, we follow the firms for ten years after the base year. Again, we only consider firms that always report nonzero employees, which considerably reduce the number of observations, and leave relaxing this strict sampling condition for later.

When taking a look at the average employment differences between a given year after the base year and the base year in FIGURE 2, we do find IC participants to have slightly higher growth in the first years after the base year. When following firms for more than eight years, the picture changes: participants have considerable lower growth eight to twelve years after the base year. But when following (a greatly reduced number of) firms for more than 12 years, we find that those controls which can be followed for so long have experienced considerably lower growth than the corresponding participants. Again, the end of the curves should be interpreted with caution.

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FIGURE 2: Number of employees. Mean difference compared to base year. Firms with less than 300 employees in base year. 3-year moving averages

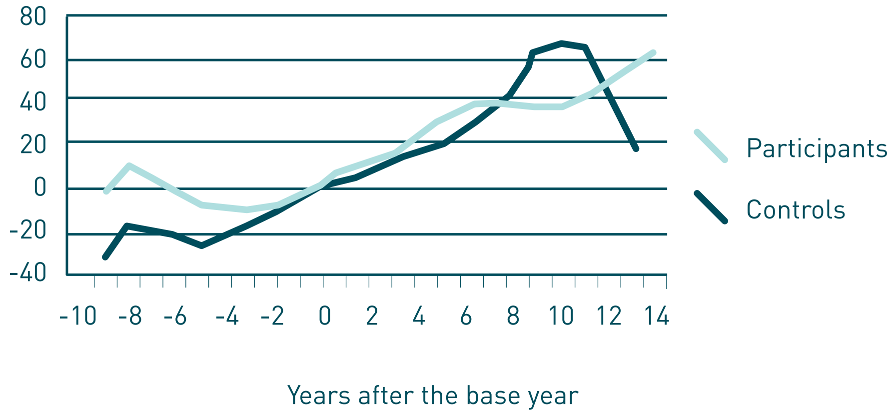

FIGURE 2 does not suggest robust positive IC programme effects, although one might notice that growth accelerates for participants but not controls in the years close to the base year.

We estimate the same statistical model to substantiate the findings suggested by FIGURE 2, and present the results of this exercise in TABLE 3A and 3B:

TABLE 3A: Employment growth up to five years after the base year: Diff-in-diff regression results

	Model 1: Dependent variable: Average annual employment increase in either the three-period up to the base year or in the five-year period after the base year		Model 2: Dependent variable: Average annual employment growth in either the three-period up to the base year or in the five-year period after the base year	
	Coefficient	Standard error	Coefficient	Standard error
Constant term k	-1,33	2,37	0,100*	0,051
Observation is after base year, d1	-1,96	1,48	-0,080***	0,019
Observation belongs to a participant, d2	-2,00	2,32	-0,019	0,032
Observation belongs to a participant and is after the base year, d1d2	5,12	3,70	0,061	0,039
	R2=0.067 495 observations		R2=0.088 495 observations	

Notes: *** significant at 1%. ** significant at 5%, * significant at 10%. The following set of controls was included in the regressions: Seven industry dummy variables, eight dummy variables for the firms' geographical regions, three calendar time dummy variables for when the firm has its base-year, and four dummy variables describing employment in the base year. Base category: firms in manufacturing industries, with 5-10 employees in the base year and zip-code <3000.

TABLE 3B: Employment growth up to ten years after the base year: Diff-in-diff regression results				
	Model 1: Dependent variable: Average annual employment increase in either the three-period up to the base year or in the ten-year period after the base year		Model 2: Dependent variable: Average annual employment growth in either the three-period up to the base year or in the ten-year period after the base year	
	Coefficient	Standard error	Coefficient	Standard error
Constant term k	3,7	2,6	0,071**	0,036
Observation is after base year, d1	1,3	3,3	-0,084***	0,022
Observation belongs to a participant, d2	-2,4	2,3	-0,021	0,032
Observation belongs to a participant and is after the base year, d1d2	-1,6	4,1	0,023	0,039
	R2=0.08 389 observations		R2=0.10 389 observations	

Notes: *** significant at 1%. ** significant at 5%, * significant at 10%. The following set of controls was included in the regressions: Seven industry dummy variables, eight dummy variables for the firms' geographical regions, three calendar time dummy variables for when the firm has its base-year, and four dummy variables describing employment in the base year. Base category: firms in manufacturing industries, with 5-10 employees in the base year and zip-code <3000.

Results of the statistical analysis confirm the findings of FIGURE 2: in the group of firms which can be followed over five years, participants increased employment by five additional employees per year, and had 6 percent (not to be confused with percentage points) higher growth. These results have t-probabilities of 17% (for absolute increases) and 13% (for percentual growth). This means that the probability of being wrong when stating that participation in an IC's generally increases employment growth is 17% and 13%, respectively.

We suggest interpreting this result as follows: there are positive relationships between growth and programme participation. However, the probability of these relationships being coincidental is too high to claim that there exist underlying mechanisms implying that these positive relationships are a general feature of participation in an IC.

Also, results are not robust to following firms for time periods of different lengths. For those firms which can be followed over at least ten years, participants have on average had lower increases in the number of employees than controls, which further advise us to be careful with regards to statements regarding general employment effects of ICs.

These results for the ten-year period are again not significant. We conclude that – at least for this sample of firms with up to 300 employees in the base year - it is not possible to find relationships which are strong enough to claim that ICs generally have positive employment growth effects.

9 ALTERNATIVE SAMPLES AND ROBUSTNESS >

In this section, we report the results of the above-described model for alternative samples distinguished by the size of the firms under consideration and their sampling criteria. Also, we will run separate regressions for firms in service industries. Finally, we use another group of firms as a control group than before, which consists of firms that have applied for funding of an IC, but found their applications being rejected by FI.

To take a look at firms in the service sector is motivated by FI's special interest in service industries as a potential growth industry. There are of course large inherent differences between firms in the service sector, and it is tempting to differentiate between knowledge intensive and less knowledge intensive industries. We, however, came to the conclusion that we will not distinguish firms along these lines. First, because we have too of few observations in the service sector, and second, because it is difficult to argue that knowledge is not relevant for the service firms that participate in a collaboration which aims to enhance innovation activity.

Thus, when considering firms in services industries, we analyse on all firms coded 65-97 in the Danish standard industry classification (db93), which covers firms which according to db93 are firms in "Financial and business services" and just "Services".

In this section, we also address the robustness of the results with regards to including firms that report irregularly. Especially when considering employment developments in the previous section, focusing on clean data implied that we lost a relatively large share of firms which report zero employees in single years.

Firms may grow by hiring new employees, or by integrating organizational units from other firms in the same corporate group, e.g., merging a holding company (with no employees) with its operating company (with employees), by acquisitions or organisational reshuffle within corporate groups. Focusing on clean data might be assumed to reduce the impact of the latter explanations, but it is still relevant to check whether this is critical with regards to the results of the analysis.

Finally, we exploit the data that CEBR has collected for the earlier study (FI, 2008) on 133 firms that applied for funding before 2003, but did not receive it (and did not receive funding later on). These firms, denoted 'rejected firms' in the following, are equal to the participants with respect to the fact that they have applied, but the fact that their project was declined funding indicates lower quality projects or lower quality applications (which again may be correlated to firm characteristics that also are related to the firm's growth potential).

These problems notwithstanding, using this alternative control group for an additional robustness check makes sense, as the potential finding of rejected firms doing just as well as participants would advise us not to interpret earlier finding as the programme's causal effect.

To implement the comparison of participants and rejected firms, we define the year of the application as the base year for comparisons. For participants, this is typically the year preceding the start of the project. The relatively low number of rejected firms implies that for this robustness check, we only follow firms over a five-year period.

In the exposition of the results of the various robustness checks, we only report the relevant coefficient estimate which is associated with the indicator (dummy) variable "Observation belongs to a participant and is after the base year, d1d2". Recall that this parameter estimates the deviation between actual post-participation average annual increases for participants and the increases which would be expected in the absence of participation. Under the identifying assumptions, this coefficient estimates the effect of the programme on participating firms.

Results of the different regressions are summarised in TABLE 4. In this table, we report t-statistics, which are the probabilities of being wrong when stating that there are non-zero underlying relationships in the data. E.g., the probability of being wrong with the claim "Firms that report gross profit less than 75 million DKK in the base year and always report nonzero gross profit experience a different average annual increase in gross profit (compared to controls) in the first five years after the base year" has a 6% estimated probability of being wrong.

The following sums up the result of the different regressions:

(a) We find that no potential programme effects can be identified when considering the total sample of all firms. This comes as no surprise, as there are large players among participants with gross profit (and large variations in gross profit) being orders of magnitude too large to potentially allow us to find any impact of the programme. The large variation in gross profit in the total sample superimposes any potential (in this case relatively small scale) effects of the programme.

(b) However, looking at smaller firms with gross profit below 75 million in the base year corroborates the picture of significant higher gross profit growth for participating firms after the programme and relative to pre-programme growth, relative to the developments of firms in the matched control group, and taking account of potential differences in observable factors between the participant and the control firms.

(c) We cannot find relationships for service sector firms, which might be because we have too few observations in this sector to allow identifying relationships of any degree of reliability.

(d) We find the strictness of the sampling conditions with regards to whether or not to sample firms that sometimes report zero activity or have missing values not having any effect on the general results.

TABLE 4: Regression estimates of the parameter of "Observation belongs to a participant and is after the base year, d1d2" for alternative samples. Dependent variable: Average annual increase in gross profit (in DKK1000).

Sample	Observation period (in years)	Parameter estimate	t-probability	Number of observations
Firms that report regularly (i.e., always report nonzero and non-missing gross profit):				
All firms	5 10	-2967 -3178	0,380 0,256	651 505
Firms that report gross profit less than 75 million DKK in the base year	5 10	6035*** 3491*	0,001 0,060	394 305
Firms that report gross profit less than 150 million DKK in the base year in the service sector	5 10	-576 -3302	0,820 0,383	86 57
Firms that occasionally report zero gross profit or have occasionally missing gross profit information:				
Firms that report gross profit less than 150 million DKK in base year	5 10	4888* 8331	0,050 0,173	696 552
Alternative control group: Rejected firms:				
Firms that report gross profit less than 150 million DKK in base year, and always nonzero and non-missing gross profit	5	4710**	0,036	233

*** significant at 1%. ** significant at 5%, * significant at 10%; the estimations include the same controls as specified in TABLE 2.

(e) Also, participants grow faster after the start of participation compared to the alternative control group consisting of firms the applications for funding were rejected. So the necessary condition for giving previous findings a causal interpretation is fulfilled. The size of the potential effect of this comparison is similar to the previous findings (an approx. DKK4.5 vs. DKK3.7 million difference in average annual increases in gross profit).

The general conclusion is that there are stable differences in gross profit developments between participants and controls after the base year for up to medium size firms – differences that cannot be easily explained by other factors than IC programme participation.

We turn now to employment developments, and summarise results in TABLE 5:

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TABLE 5: Regression estimates of the parameter of "Observation belongs to a participant and is after the base year, d1d2" for alternative samples. Dependent variable: Average annual increase in the number of employees.

Sample	Observation period (in years)	Parameter estimate	t-probability	Number of observations
Firms that report regularly (i.e., always report nonzero or nonmissing number of employees):				
All firms	5 10	-2,8 -11,0	0,794 0,288	703 560
Firms that have less than 150 employees in the base year	5 10	11,2** 4,6	0,016 0,179	325 274
Firms that have less than 75 employees in the base year	5 10	11,9** 9,2*	0,028 0,084	206 145
Firms that that have less than 300 employees in the base year in the service sector	5 10	22,0 5,2	0,104 0,635	95 66
Firms that that have less than 150 employees in the base year in the service sector	5 10	24,0* 11,2	0,392 0,392	87 58
Firms that occasionally report having zero employees or have occasionally missing employment information:				
Firms that have less than 300 employees in the base year	5 10	6,7** 2,0	0,023 0,540	693 529
Firms that have less than 150 employees in the base year	5 10	9,2*** 4,1*	0,007 0,076	554 356
Alternative control group: Rejected firms:				
Firms that have less than 150 employees in the base year and always report nonzero or nonmissing number of employees	5	1,4	0,873	165

*** significant at 1%. ** significant at 5%, * significant at 10%; the estimations include the same controls as listed in TABLE 3.

In the case of employment growth, changing the sampling conditions reveals new results: for participating firms of size below 150 employees in the base year, we find large and statistically significant potential employment effects of ICs of about eleven additional employees per year. One may note here that approx. 24 percent of all 220 participants have less than 50 employees and approx. 50 percent have less than 150 employees, so we find potential effects for the participants in the lower half of their size distribution. We also find statistically significant effects for firms of size below 300 employees in the base year, when we include firms that report zero employees in some years.

Again, we cannot find relationships when considering samples of all firms which have participated in an IC – some of which have several thousand employees.

We find weakly significant positive potential employment effects for firms in the service sector, which is remarkable given the relatively small size of this sample (less than 100). We advise not to take the large potential effect of annually 24 additional employees at face value. The combination of large heterogeneity and relative few observations implies that this result is associated with a high level of uncertainty.

Finally, we do not find potential employment effects when comparing participants with the group of firms the project applications were rejected. The absence of any significant result might be due to large variation in employment growth in the group or rejected firms - in association with a relatively small number of observations. However, it also implies that high employment growth in small participant firms after the base year might not be so much an effect of IC participation, but might instead be the result of strategic decisions correlated to applying to the programme, and shared by participants and controls.

TABLE 4 and 5 only present a small but representative share of the robustness tests undertaken for this analysis, but none of our alternative sampling or modelling[6] strategies have changed the general conclusion of there being positive potential effects for the firms at the lower half of the total sample's size distribution, which in some cases even can be shown to be significant when considering long-run averages over ten years after the base year.

[6] This includes, for example, estimating the models with the inclusion of firm random effects – which is possible because there are two observations per firm. This does, however, not change any of the previous results. Also, random effects estimations of annual increases (instead of average annual increases over a couple of years) on the panel of firm year observations give very similar results.

10 CONCLUSIONS >

This report summarises the results of an evaluation study of the IC programme. For this purpose, we follow firms which have participated in the programme before and after the start of participation, and analyse their developments with regards to gross profit and employment.

This is possible, because firm information from FI regarding programme participation has been merged with register data on key accounting variables that firms are obliged to file at public authorities. We can find 220 firms that participated out of 285 in total in the register data, and can follow 203 of these firms for at least five years after they have started to participate in an IC.

For our analysis, it is natural to distinguish firms by their size. Some of the firms that participate in the IC programme are very large, having gross profits of several billion DKK and several thousand of employees. It would be unrealistically optimistic to search for potential effects of the IC scheme in a group of firms in which these large firms are included.

Hence, for the analysis, we consider firms that represent roughly the smallest 75 percent of all firms in the sample, and find positive potential gross profit effects of programme participation – a finding which is based on a joint comparison of growth patterns of participant firms and a highly similar group of comparison firms, in which we correct for potential differences in inherent (pre-participation) growth trends before the start of programme participation.

We find that participants have annual increases in gross profit in the first five years after the start of participation, which are on average 3.7 million DKK above what would be expected in the absence of programme participation. Under the assumption that participants would have experienced the same developments in gross profit growth as the controls in the absence of the programme, the additional 3.7 million per year in the first five years after participation is the genuine effect of participating in an IC.

Over a ten year-period, the average potential effect gross profit effect is smaller and is approx. two million DKK per year, and is no longer statistical significant. An obvious explanation might be that potential effects of the programme are realised in the first years after starting to participate in the programme, so the average of the annual increases over a period of time becomes smaller the longer the time period under consideration.

If participants' counterfactual growth in the absence of participating in the programme is indeed appropriately measured by the growth of the controls, then the most qualified guess of the programme's effect is that it increases annual gross profit per year of smaller firms by approx. DKK20 million over a five to ten year time period after participation. It should, however, be noted that this number is associated with statistical uncertainty, which advises us to be careful when making predictions regarding future programme effects.

It is difficult (and has not been part of the present analysis) to estimate what the counterfactual behaviour of participants in the absence of the IC scheme might have been. Maybe ICs are a means of helping to implement firms' strategic decisions and innovations, which are the true reasons of the positive developments, maybe participants have higher growth than controls for reasons we could not observe in the data and did not control for in this analysis.

Still, the back-on-the-envelope calculation resulting in a DKK20 million difference in annual gross profit after five to ten years suggests that the programme is a success even in case of only a share of this difference owing itself to a genuine causal effect of the programme. Here, it could be noted that differences in annual gross profit accumulate over time, implying substantial differences between participants' and control firms' value creation when measured over several years.

We also consider employment developments and can again not find significant results for the sample of firms where we include large firms. It is, however, possible to demonstrate that smaller (in this case firms having less than 150 employees in the year before participating in the programme) participants have an additional annual employment growth of approx. eleven employees. This difference is statistically significant at the 5% significance level, but the sum of the evidence advises us to be careful to interpret it as a causal effect of the IC scheme.

We conclude that it comes as no surprise that we do not find potential programme effects for those samples which include large firms. Instead, we find positive potential effects of the programme on gross profit and employment for relatively small firms, where we expected to have a chance of finding them in case of their existence. The difficulty of finding potential effects for large firms is likely to be due to a measurement issue, and should not been taken as evidence of ICs having no effect for large firms.

Even though the present data at hand must be seen as favourable for this kind of analysis, regularly updating them might in the future allow analyzing which firms benefit more from participating in an IC than others, and which ICs work better than others.

In the current case, it was for example not possible to make statements of any reliability regarding the experiences of participant firms in the service sector.

Furthermore, we did not have data on the patenting activities of the participating firms, but we expect this type of data to become available for subsequent analyses. This additional information may be exploited for the identification of controls and may also be used to directly estimate the effects of IC programme participation on innovation output.

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APPENDIX 1: SELECTION OF CONTROLS >

The KOB dataset is a panel dataset which has repeated observations for most of the firms - one for each annual account filed at the authorities. So for each firm, there are typically multiple firm-year observations (where a firm-year observation refers to a data-point of a given firm in a given year). In the following, we will use the expression 'control observation' to describe a single firm-year observation of a control.

Control firms are chosen in the year in which they are most similar to one (in a single case: two) of the participants in the year before participation. This defines each control firm's 'base year' as the year in which it is selected as a control firm. For each control, the base year forms the basis for comparisons of given success parameters over time.

Note similarity between participants and potential controls is in terms of (a) the firms' industry, region, size and age and (b) the expected probability of participation, derived as follows:

We run an auxiliary regression on the universe of approx. 370,000 firm-year observations in KOB in the period 1994 to 2001 that roughly resemble the group of participants (we do for example not consider industries in which there is no single participant).

The auxiliary regression is formulated as a simple probit model, with starting to participate in the programme next year being the dependent variable, and 32 controls in total, covering firm size , industry, region and time period. The regressions' pseudo R2, which is a measure of the model's goodness-of-fit, is 0.22, which we consider as being high.

The probit regression allows making statements of how likely program participation is for a given firm. This allows finding pairs or groups of firms, in which this probability is very similar. For two firms A and B with similar participation probability, the fact of firm A participating and not firm B can now be interpreted as being coincidental.

Under this interpretation, the identification set-up resembles an experiment, in which programme participation was at random, and which would allow interpreting systematic differences in outcome variables between participants and controls as the programme's causal effect on participating firms.

Yet, even firms with similar predicted participation probabilities can be quite different, and to avoid systematic differences in industry affiliation, size, etc., between participants and controls, we also condition on a number of observable characteristics being equal for a given participant and its matched control firm(s).

For this purpose, we divide the total number of firm-year observations into groups having the same industry affiliation and being in the same region, of similar size and observed in the same year.

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For each participant, we select the firm-year observation of a non-participant firm being within the same group and having a participation probability which comes closest to the participant's. This selected firm-year observation defines the participant firm's control firm, and the control firm's base year.

By repeating this matching procedure, we can find an arbitrary number of control observations for each participant. Here, a greater number of control observations increases the robustness of later results, however, increasing this number also makes it increasingly difficult to find highly similar control observations for some of the participants.

As a compromise within this trade-off, we chose to find for each participant two control observations (firm-year observations of non-participants). The selection of the two control observations per participant is in two rounds. In each of the rounds we select one control observation for each participant.

In the first round we find 220 control observations of non-participants, in the second we find another 219 control observations of non-participants (the reason for only 219 instead of 220 being that in a single case one firm-year observation is chosen as a control observation for two participants).

In each of the two rounds, we first condition on many factors being highly similar when selecting control observations. This leaves a number of participants, for which no control observations could be found. In subsequent steps, we reduce the number of factors and start choosing control observations which are increasingly less similar, until each round has identified one control observation for each participant. This selection of control observations is described in greater detail in TABLES A1.1 and A1.2.

In each of the rounds we only select one control observation per participant. This does not rule out selection of different control observations (belonging to different years) of the same control firm, which implies that there are a number of control observations which occur repeatedly in the data which form the basis of the performance analysis.

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TABLE A1.1: Identification of first neighbours by balanced score matching procedure

Step 1:	Participants and controls are restricted to be equal in terms of …. Industry (143) categories Number of employees (11 categories) Gross profit (7 categories) Year in which they are observed (9 years) Region (8 regions) Firm age (3 categories) This identifies control firms for 61 participant firms (27.7%).
Step 2:	Participants and controls are restricted to be equal in terms of …. … industry (143) categories … number of employees (11 categories) … gross profit (7 categories) … time period in which in which they are observed (5 periods covering 2 years each) … region (8 regions) … firm age (3 categories) This identifies control firms for 67 participant firms (30.5%).
Step 3:	Participants and controls are restricted to be equal in terms of …. … industry (143) categories … number of employees (11 categories) … gross profit (7 categories) … time period in which in which they are observed (5 periods covering 2 years each) … firm age (3 categories) This identifies control firms for 103 participant firms (46.8%).
Step 4:	Participants and controls are restricted to be equal in terms of …. … industry (33) categories … number of employees (11 categories) … gross profit (7 categories) … time period in which in which they are observed (5 periods covering 2 years each) … firm age (3 categories) This identifies control firms for 165 participant firms (75.0%).
Step 5:	Participants and controls are restricted to be equal in terms of …. … industry (33) categories … number of employees (9 categories) … gross profit (6 categories) … time period in which in which they are observed (5 periods covering 2 years each) … firm age (3 categories) This identifies control firms for 169 participant firms (76.8%).
Step 6:	Participants and controls are restricted to be equal in terms of …. … industry (33) categories … number of employees (6 categories) … gross profit (5 categories) … time period in which in which they are observed (5 periods covering 2 years each) … firm age (3 categories) This identifies control firms for 184 participant firms (83.4%).

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Step 7: Participants and controls are restricted to be equal in terms of ….
… industry (9 categories)
… number of employees (6 categories)
… gross profit (5 categories)
… time period in which in which they are observed (5 periods covering 2 years each)
… firm age (3 categories)
This identifies control firms for 199 participant firms (90.5%).

Step 8: Participants and controls are restricted to be equal in terms of ….
… industry (9 categories)
… number of employees (4 categories)
… gross profit (4 categories)
… time period in which in which they are observed (4 periods covering 3 years each)
… firm age (3 categories)
This identifies control firms for 202 participant firms (91.8%).

Step 9: Participants and controls are restricted to be equal in terms of ….
… industry (9 categories)
… time period in which in which they are observed (5 periods covering 2 years each)
This identifies control firms for 220 participant firms (100.0%).

TABLE A1.2: Identification of second neighbours by balanced score matching procedure

Step 1:	Participants and controls are restricted to be equal in terms of …. Industry (143) categories Number of employees (11 categories) Gross profit (7 categories) Year in which they are observed (9 years) Region (8 regions) Firm age (3 categories) This identifies control firms for 33 participant firms (15.0%).
Step 2:	Participants and controls are restricted to be equal in terms of …. … industry (143) categories … number of employees (11 categories) … gross profit (7 categories) … time period in which in which they are observed (5 periods covering 2 years each) … region (8 regions) … firm age (3 categories) This identifies control firms for 54 participant firms (24.6%).
Step 3:	Participants and controls are restricted to be equal in terms of …. … industry (143) categories … number of employees (11 categories) … gross profit (7 categories) … time period in which in which they are observed (5 periods covering 2 years each) … firm age (3 categories) This identifies control firms for 87 participant firms (40.0%).
Step 4:	Participants and controls are restricted to be equal in terms of …. … industry (33) categories … number of employees (11 categories) … gross profit (7 categories) … time period in which in which they are observed (5 periods covering 2 years each) … firm age (3 categories) This identifies control firms for 144 participant firms (65.5%).
Step 5:	Participants and controls are restricted to be equal in terms of …. … industry (33) categories … number of employees (9 categories) … gross profit (6 categories) … time period in which in which they are observed (5 periods covering 2 years each) … firm age (3 categories) This identifies control firms for 151 participant firms (68.6%).

Step 6: Participants and controls are restricted to be equal in terms of ….
… industry (33) categories
… number of employees (6 categories)
… gross profit (5 categories)
… time period in which in which they are observed (5 periods covering 2 years each)
… firm age (3 categories)
This identifies control firms for 173 participant firms (78.6%).

Step 7: Participants and controls are restricted to be equal in terms of ….
… industry (9 categories)
… number of employees (6 categories)
… gross profit (5 categories)
… time period in which in which they are observed (5 periods covering 2 years each)
… firm age (3 categories)
This identifies control firms for 194 participant firms (88.2%).

Step 8: Participants and controls are restricted to be equal in terms of ….
… industry (9 categories)
… number of employees (4 categories)
… gross profit (4 categories)
… time period in which in which they are observed (3 periods covering 3 years each)
… firm age (3 categories)
This identifies control firms for 200 participant firms (90.0%).

Step 9: Participants and controls are restricted to be equal in terms of ….
… industry (9 categories)
… time period in which in which they are observed (5 periods covering 2 years each)
This identifies control firms for 220 participant firms (100.0%).

APPENDIX 2:

ILLUSTRATION OF THE DIFF-IN-DIFF ESTIMATION SET-UP >

Estimation of the programme's effects is by a difference-in-difference model: For both participants and controls, we calculate the average annual increases of the success parameters in the years before the base year. We also calculate the average annual increases of the success parameters in the years after the base year for both participants and controls.

Thus, we can compare the (a) average increases of participants before they start participating in an IC, (b) the average increases of participants after they have started to participate in an IC, (c) the average increases of controls before they were selected as controls (i.e. were most similar to one of the participants before it started to participate) and (d) the average increases of controls after they were selected.

So let a be a participant's pre-base-year average annual increase in either success parameter, b a participant's after-base-year average annual increase in either success parameter, c a control's pre-base-year average annual increase in either success parameter and d a control's after-base-year average annual increase in either success parameter.

Note b-a measures by how much a participant's average annual increase in the success parameter changes when the participants starts participating in the programme. For controls, the difference d-c measures the difference in the average annual increases between before and after the base year.

Under the assumption that participants would continue having average annual increases a in the absence of the programme, the average of the participant-specific differences b-a estimates the IC's causal average effect on participant firms.

However, this assumption is relatively strong, as b may be different from a for other reasons than programme participation (e.g., business cycle or firm age effects).

But given the similarity of participants and controls in the base year, one may argue that these 'other reasons' should have the same effect for both participants and controls, and assume that b-a would on average be equal to d-c in the absence of programme participation. Under this 'identifying' assumption, (b-a)-(d-c) is the change in participants' average annual increases between before and after the start of participating in the IC which can only be explained by the programme, in other words: the programme's causal effect on participating firms.

To the extent that there remain dissimilarities between participants and controls in observable factors such as industry, size or geographical region which potentially could generate differences in the growth patterns of participants and controls, these will be taken account of by including control variables in the regressions to follow.

When taking this model to the data, (b-a)-(d-c) is estimated by a simple linear regression (with heteroscedasticity-consistent standard errors).

Here, we need to make decisions regarding the length of the time periods over which pre-base-year and after-base-year average annual increases are computed. We made the following choices: average pre-base-year increases are computed over a three-year period before the base year. Average post-base-year increases are computed over a five year and, as a second step, over a ten year period.[7]

For the estimation of the model, we generate (typically) two observations per firm: First, the average increases of the success parameters in the three-year period before the base year. Second, the average increases of the success parameters in the five (ten) year period after the base year.

This implies that we will only consider firms that were observed three years before the base year, and at least five or ten years after the base year for the estimations.

The regression equation is taking the following form:

$$A = k + \beta 1 * d1 + \beta 2 * d2 + \beta 3 * d1d2 + \beta 4 * x + \varepsilon,$$

where A is the average increase in the success parameter in either the time period before or after the base year and k is the constant term. d1 is an indicator variable taking the value one (and zero otherwise) if the observation is after the base year, d2 is an indicator variable taking the value one (and zero otherwise) if the observation belongs to a participant. cd2 takes the value one if the observation belongs to a participant and is after the base year (and zero otherwise). x is a set of control variables with an associated set of coefficients β4 to be estimated. β1, β2, and β3 are also coefficients to be estimated, and ε is an error term assumed to satisfy standard specifications.

Note that inclusion of the vector x is redundant in the sense that the matching procedure implies high similarity in observable characteristics across participants and controls. Still, inclusion of x increases the explanatory power of the model, and might safeguard against potential differences between participants and controls.

[7] These choices reflect compromises between the wish not to lose too many firms for the analysis which only are observed for shorter time periods and the wish to being able to follow firms long enough to being able to detect any effects in case they exist. Also, the precision of the growth trend measures increases with the length over which the averages are calculated, which is relevant here because of considerable year-to-year volatility in the success parameters. Basing estimates of pre-base-year time trends on a three-year period is a compromise between not to lose too many firms for the analysis and the wish to generate reasonably stable estimates of pre-base-year growth patterns.

Note further that

- the constant term k estimates average c, i.e., the average annual increases for controls before the base year[8],
- k+β1 estimates average d, the average annual increases for controls after the base year,
- k+β2 estimates average a, i.e., the average annual increases for participants before the base year,
- k+β1+β2+β3 estimates average b, i.e., the average annual increases for participants after the base year.

Thus, β3 estimates average (b-a)-(d-c), which is, under the indentifying assumption, the programme's average causal effect for firms that participate in the programme. In the language of the evaluation literature, β3 estimates the 'average treatment effect on the treated (ATT)'.

[8] Strictly speaking does k estimate the average annual increases for controls with all variables in the vector x taking the value zero before the base year.

APPENDIX 3:

EXIT AND SURVIVAL OF PARTICIPANTS AND CONTROLS >

The last additional step of the analysis is comparing participants' and controls' exit and closure behaviour. In the following, exit will refer to a firm leaving the data before 2008 (which is the end of the observation period) – without making any distinctions between the potential reasons for doing so.

Closure on the other hand is defined as one of the following transitions: bankruptcy, liquidation, or forced exit. Information of these transitions is from the cvr-register of the ministerial body '*Erhvervs- og Selskabsstyrelsen*'.

There are 162 exit and 60 closure events in the data.

This appendix addresses two issues: first, whether fast growing participants have a higher probability of staying in the data compared to controls. This would imply that growth increase estimates for participants in association with the programme are biased upwards.

The second question is whether participants have lower closure probability compared to controls, which might be – given the similarity of participants and controls – interpreted as a positive effect of the programme on participants' survival.

TABLE A3.1 presents participants' and controls' exit status when leaving the data. We find that approx. 76 per cent all firms stay in the data until 2008, which is the end of the observation period. There is a higher share of participants that can be followed until 2008. Participants have a lower propensity to exit in general, and especially to exit by a merger/acquisition event.

There is a higher share of participants that can be followed until 2008. Participants have a lower propensity to exit in general, and especially to exit by a merger/acquisition event.

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TABLE A3.1: Firm transitions (in per cent of total)

	Participants	Controls	Both
Continued until at least 2008	88,18	69,93	76,03
Merger/acquisition	3,18	14,81	10,93
Bankruptcy	4,55	5,01	4,86
Liquidation	2,73	3,87	3,49
Dissolution	0	1,82	1,21
Split up	0,45	1,37	1,06
Restructured	0,91	1,14	1,06
Forced exit	0	1,14	0,76
Erased from register	0	0,91	0,61
Total	100	100	100

To test the statistical significance of this result, we employ a simply binary choice logit model, which allows making statements on the differences in the expected exit or closure probabilities of participants and controls. Results are presented in TABLE A3.2, and can be summarized as follows:

The results of Model 1 provide evidence of participants having a significantly lower probability of leaving the data as exits. The coefficient -0.856 implies that their probability of exiting in a given year is less than half of controls' exit probability. Given that 'exit' is by no means to be associated with 'failure', this finding is no indication of participants being more successful than controls.

The result of Model 2 implies that there is no significant difference in the probability to exit by a closure event (which might be interpreted as a success measure) between participants and controls.

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Central Innovation Manual on Excellent Econometric Evaluation of the Impact of Interventions on R&D and Innovation in Business (CIM)

February 2013

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An excellent econometric impact evaluation of innovation policy is defined as a performance measurement of an innovation policy instrument that has been implemented in accordance with state-of-the-art econometric research methods, and is of a quality on par with state-of-the-art research, facilitating publication of methods and results in the most respected international research journals in the relevant fields.[1]

The main target group of this *Central Innovation Manual on Excellent Econometric Evaluation of the Impact of Interventions on R&D and Innovation in Business (CIM)* is programme owners in the Danish Ministry of Science, Innovation and Higher Education and other government agencies who work with R&D, innovation and business instruments, and who require better information and guidance on the best methods for evaluating the impact of these instruments as well as wider innovation and business policy.

Other target groups are external expert stakeholders, evaluation experts and researchers who are interested in following and discussing how to conduct impact evaluation studies with the Danish Ministry of Science, Innovation and Higher Education. The manual also has the purpose of disseminating knowledge about the best methods for performance measurements of research, innovation and business policy in Denmark and elsewhere.

This manual (CIM) is not identical to similar work done in other countries[2] since the key objective is to establish a clear set of minimum requirements for so-called excellent econometric impact evaluations of innovation policy. CIM focuses on how to set up a framework for a "standard" impact assessment procedure that makes it possible to compare the impact of different instruments. CIM is not an attempt to establish a practical guide on a broader number of methods on how to evaluate the wider impact of R&D and innovation interventions. In this way, CIM complements existing documents and reports.[3]

[1] See e.g. Kaiser and Kuhn (2012), Long-run Effects of Public-private Research Joint Ventures: the Case of the Danish Innovation Consortia Support Scheme, Journal of Research Policy (forthcoming 2012).

[2] See Guidance on evaluating the impact of interventions on business, Department for Business, Innovation and Skills (BIS), august 2011

[3] E.g. The role of evaluation in evidence-based decision-making, Department for Business, Innovation and Skills (BIS), august 2010, and The Green Book – Appraisal and Evaluation in Central Government, Treasury Guidance, London, United Kingdom, and The Magenta Book: guidance notes for policy evaluation and analysis, Government Social Research Unit, HM Treasury, London, United Kingdom (October 2007)

1 OBJECTIVE, VISION AND DELIMITATION >

1.1 Objective

The objective of this manual is to establish a number of minimum requirements and standards for the implementation of excellent econometric impact evaluation of the innovation policy instruments of the Danish Ministry of Science, Innovation and Higher Education. However, anyone interested in econometric impact evaluation in ministries and agencies might find it useful and informative. Accordingly, the manual has been prepared in collaboration with Danish and non-Danish researchers. It has been discussed at seminars with researchers[4] and policy makers and has been presented for comments in the Danish Ministries of Finance, of Business and Growth, of Climate and Energy, of Food, Agriculture and Fisheries, of Foreign Affairs, and of the Environment. The manual is the result of the evaluation strategy of the Danish Agency of Science, Technology and Innovation (DASTI) and has been implemented as a 5-year research and innovation project about performance measurements in the innovation field.[5] CIM summarises some key methodical results, but the main elements of the 5-year project are 50+ evaluations that have been conducted from 2007 to 2011.

1.2 Focus and delimitation of the manual

As the manual focuses on minimum requirements on excellent econometric impact evaluations, it does not contain guidelines on other types of evaluation and performance measurements of research and innovation programmes, such as research, learning, organisational, internationalisation, equality or environment-related effects.

[4] In particular, I would like to thank PhD Johan Moritz Kuhn and Professor PhD Anders Sørensen at Center for Economic and Business Research (CEBR) at CBS (Copenhagen), and Michael Mark (DAMVAD Consulting) for their comments on CIM.

[5] Since 2007, the development of methods for performance measurements has been an ongoing work. See e.g. the report DASTI (2007), Data og metoder ved effektmåling af innovationskonsortier (Data and methods for performance measurements of innovation consortia) and DASTI (2009), Data og metoder ved effektmåling af videnpiloter (Data and methods for performance measurements of knowledge pilots). Also see DASTI (01/2011), which describes methods and data selection in relation to analyses of Industrial PhDs and Innovation Consortia, respectively. Further developments are to be found in, Kaiser and Kuhn (2012), Long-run Effects of Public-private Research Joint Ventures: the Case of the Danish Innovation Consortia Support Scheme, Journal of Research Policy (2012). Also see DASTI (01/2010) and DASTI (02/2011).

Although CIM lists standards for impact evaluations, the intention has been to do this in a way that makes room for flexibility. This is partly because the recommended *'propensity score matching method by nearest neighbour'* will not be the most relevant method for all policy instruments. For instance, this can be the case if the analysis has a different or wider focus than enterprise performance. For other instruments, impact evaluations of economic performance targets are less relevant if the main instrument purposes are non-economic activities. To provide an example, this is the case for impact evaluations of clusters where the main objectives are not just economic performance targets, but also include non-economic behaviour-regulating performance objectives.

It is thus important that specific impact evaluations take into consideration the objective of a given instrument. Accordingly, the manual also includes an overview of the non-economic performance objectives that are listed for the most important innovation instruments in the Danish Ministry of Science, Innovation and Higher Education.

Finally, for many instruments it is challenging to establish sufficiently consistent data series in terms of timeframe and number of observations. It is also challenging to identify a (high quality) qualified control group in accordance with the same conditions. So for new instruments, or instruments where only a relatively small number of businesses have participated, it may be necessary to show a certain amount of flexibility due to the data quality. Alternatively, it will be necessary when such limitations occur to insist that impact evaluations be implemented using methods that test the robustness of the results.

1.3 Vision

The vision of the Danish Agency of Science, Technology and Innovation (under the Danish Ministry of Science, Innovation and Higher Education) is that the excellent impact evaluations and analyses of R&D and innovation instruments carried out will be examples of international best practice over the coming decade.[6]

Internationally, there is an increasing interest in carrying out quantitative analyses of the effects of enterprises' activities in research, development and innovation. The increased focus has been encouraged, among others, by the OECD,[7] which has paid great attention to the subject through a coordinated effort among a majority of the 27 EU countries as well as Korea, Norway, Switzerland, Russia, Turkey, South Africa, and most of the countries in South America.

[6] In the reports 'Clusters Are Individuals – Benchmarking Insights from Cluster Management Organizations and Cluster Programs' by Kompetenznetze Deutschland (VDI/VDE Innovation + Teknik) and 'Service innovation: Impact analysis and assessment indicators' by the European Commission's Pro-Inno Net EPISIS, the Danish Ministry of Higher Education's econometric performance measurements are singled out as being international best practice.

[7] OECD (2008), Science, Technology and Industry Outlook.

Most of these countries do not have the same possibilities as Denmark (or e.g. Norway, Sweden and the Netherlands) due to limited access to quantitative micro data and very long time series. In most of these countries, it is difficult to establish the micro data basis needed to carry out solid and validated quantitative econometric analyses that can document and calculate the effects of R&D and innovation instruments historically.

In Denmark, the policy concerning evaluations and performance measurements is that:

- The effect must be ***documented*** consistently for all innovation instruments.

- ***Unambiguous key performance objectives*** must be listed for all instruments by the responsible authorities.

- Impact evaluations and performance measurements must be applied when making decisions on possible ***continuation, mergers or adjustments of innovation instruments.***

1.4 Establishing minimum requirements and standards

When establishing minimum requirements and standards, a number of issues must be taken into consideration:

- The purpose of impact evaluations is firstly to document, in the best way possible, economic effects and other key performance effects of existing innovation instruments.

- Secondly, in more general terms it is important to be able to document innovation policy effects in order to strengthen innovation policy as a policy and political discipline.

- Thirdly, it is important to be able to establish a better understanding of the different instruments in the innovation policy toolbox. This can be achieved, for instance, by ensuring comparability of results *across* analyses and *across* innovation instruments in a far better way than has been the case until now.

- Fourthly, there is a need for evidence-based development and renewal of the prioritisation tools for innovation policy.

The problem is that there are many degrees of freedom for impact evaluations, e.g. in the choice of key performance indicators, success variables, choice of data basis, treatment of outliers, choice of statistical analysis methods, interpretation of results achieved etc. This means that a whole string of choices have to be made in the course of carrying out excellent performance measurements.

The objective of this manual (CIM) is to create a framework for establishing performance objectives (key performance indicators) in order to ensure a common framework for the analysis methods and databases used for impact evaluations and performance objectives, and to make possible better comparisons of key performance indicators across instruments in Denmark and abroad.

1.5 Overview of the most important standards and minimum requirements

CIM establishes a number of standards and minimum requirements for impact analyses in order to illuminate the effects of innovation policy on key performance indicators.

The manual is aimed at R&D and innovation instruments that may involve both public and private participants. It is not aimed at instruments whose primary purpose is to further basic research at public research institutions, universities etc.

The CIM requirements for an excellent econometric impact evaluation are high data quality, use of the most recent research-based statistical methods, and a high control group quality. With this in mind, CIM sets out 12 principles as minimum requirements for an excellent impact evaluation.

>

12 principles: Minimum requirements for excellent econometric impact evaluations	
List of key performance indicators for object to be analysed	
1.	Unambiguous key performance indicators (based on ex ante evaluations of the instrument) formulated as indicators of effects (input variables), throughput variables and results (output variables) must be listed in instrument descriptions which are to be approved by the ministry's management.
Identification and harmonisation of data collection	
2.	Establish standards for data collection, including standards for input variables and registration in databases. Standards for data collection are to be harmonised across all R&D and innovation instruments in the Danish Ministry of Science, Innovation and Higher Education through a common electronic application system.
Data quality and long time series	
3.	Ensure high data quality with long time series of at least 6-15 years with minimal data gaps in the time series. Use of national registers for enterprise data and personal data as well as the ministry's databases for applications, appropriations, rejections and projects. Databases are to be established with time series of up to 20-25 years, depending on the instrument applied.
Treatment of data and quality requirements in identifying control groups	
4.	Use of the difference-in-difference method and balanced panel data.
5.	Use of the propensity score and nearest neighbour matching method for selecting the most comparable control group / comparison group.
6.	Use of alternative control groups / comparison groups with a clear and unambiguous interpretation option: e.g. propensity score matching group, group of participants in other innovation policy instruments, rejection group (group of enterprises and individuals whose applications have been rejected), group of enterprises within the same industrial sector, etc. This facilitates analyses of an instrument's additionality (additional effect) and comparison across instruments.
7.	Selection of comparable (control) enterprises must be based on matching as many relevant parameters as possible. The very highest requirements on quality and interpretation of data for comparison groups must be stipulated.
8.	Selection of comparable individuals (persons, researchers) must be based on matching as many relevant parameters as possible. The very highest requirements on quality and interpretation of data for comparison groups must be stipulated.
9.	Outliers must be handled in accordance with the most established international methods in the fields of economic research and econometric methods.
10.	The key impact indicators must be relative in order to avoid comparison of uneven entities, e.g. through differences in growth rates.
Robustness test	
11.	Robustness tests are recommended in analyses with long time series and many observations. In case of data limitations in the form of limited time series and observations, it is a requirement that impact evaluations be carried out using methods that thoroughly test the robustness of the results.
Interpretation and peer review of results	
12.	The quality and utility value of impact evaluations must be discussed with independent research organisations that are not affiliated with the analyses, e.g. through peer reviews, research seminars, policy maker workshops etc. Preferably, the results of the impact evaluations should be suitable for acceptance into the most reputable international research journals.

This manual does not contain standards for criteria and administration. For these, please refer to the evaluation and impact assessment strategy of the Danish Ministry of Science, Innovation and Higher Education, the Danish Council for Technology and Innovation, and to the action plans InnovationDenmark 2007-2010, InnovationDanmark 2009-2012 and InnovationDenmark 2010-2013, which describe the overall guidelines for administration, evaluation criteria and key performance indicators of the various innovation instruments.

The establishment of standards for administration and evaluation criteria is described in further detail for each innovation instrument in separate performance description. These also describe the correlation between the innovation instrument objectives and the key performance indicators for activities, effects and results alike.

1.6 Overview of the most important impact assessments and results

More than 13 impact evaluations of various R&D, innovation and education activities have been carried out since 2007. The impact evaluations have been carried out by independent researchers or organisations and were commissioned by the ministry or by independent institutions. 10 major impact assessments of innovation instruments were conducted between 2010 and 2013 alone.

>

Examples of impact evaluations	
Focus area	**Cluster and network policies**
Study no. 1	**The independent impact evaluation of Innovation Networks Denmark Programme (DASTI 18/2011):** The programme supports the establishment and running of cluster and network organisations. Among 1,200 non-innovative enterprises participating in the programme, the likelihood of being innovative increased 300 percent compared to 1,200 statistically identical enterprises not participating in the Innovation Networks Denmark infrastructure.[9] Among R&D-active or innovative enterprises participating in the programme, the likelihood of initiating their first R&D collaboration project with a research institution increased 300 percent compared to statistically identical enterprises not participating in the programme.
Focus area	**R&D collaboration projects between business and research**
Study no. 2-4	**Three independent impact evaluations (DASTI 06/2008, DASTI 03/2010, DASTI 01/2011 and Kaiser & Kuhn (2012)) of the Danish Innovation Consortium Programme** (public grants to large research collaboration projects between several enterprises, research institutions and technology institutes) show that there are statistically significant impacts for enterprises as well as for individual researchers depending on the key impact indicators analysed. Key performance indicators are for gross profits, individual employment, employment in enterprises, patenting activity, salary and total factor productivity. Some of the analyses show positive and statistically significant impact for small and medium-sized enterprises with respect to labour productivity, patenting activity and employment. None show similar impact on total factor productivity or for large enterprises. One study shows positive, statistically significant impact on the salary level of researchers at the research institutions. Gross profits increased on average EUR 2.7 million in an enterprise participating in an innovation consortium over a period of nine years after the innovation consortium started. Enterprises did not receive public grants.
Study no. 5	**An independent impact evaluation (DASTI 17/2011) of international research and development collaboration projects (EUREKA projects)** was conducted in 2010. The impact of EUREKA participation with respect to labour productivity, employment, turnover and exports was analysed. The analysis shows a positive, statistically significant impact on growth rates in labour productivity, employment, turnover and exports compared to statistically similar enterprises not participating in EUREKA projects. EUREKA participation also results in significantly higher growth rate in exports and employment compared to enterprises that only participate in the Innovation Consortium Programme (and not in international projects).
Study no. 6	**An independent impact evaluation (DASTI 02/2011) on national research and innovation collaboration projects between enterprises and universities or GTS-institutes** was conducted in 2010 and 2011. Both projects with or without grants from public research funding bodies were included. More than 1,500 R&D-active enterprises engaging in one or more R&D collaboration projects with research and technology institutions in the period 1999-2006 were compared to more than 1,500 statistically identical enterprises that did not collaborate, selected from 20,000 Danish R&D-active enterprises. The labour productivity is 9 per cent higher for the average enterprise with R&D collaboration compared to statistically identical R&D-active but non-collaborating enterprises in the analysed period. The analysis also looks at differences across different sectors, types of enterprises and research institutions. Impacts are higher in large enterprises than in small enterprises. Impacts are also higher in exporting enterprises than non-exporting enterprises. Finally, impacts increase with the skill level in the enterprises.

[9] http://fivu.dk/publikationer/2011/innovationsnetvaerk-skaber-vaekst
http://fivu.dk/publikationer/2011/innovationsnetvaerkenes-performanceregnskab-2011

Focus area	Education and academics (candidates and PhDs) in the business sector
Study no. 7-8	**Two independent impact studies of the Danish Industrial PhD Programme (DASTI 2007 and DASTI 01/2011)** show positive, statistically significant impacts. 200-300 participating enterprises and 400 participating Industrial PhD graduates, depending on the key impact indicators, are analysed. The programme provides a subsidy to enterprises hiring PhD students to work on a PhD project. Key performance indicators are labour productivity, individual employment, total employment in enterprises, patenting activity, individual salary and total factor productivity. The 01/2011-analysis shows positive and statistically significant impacts for small and medium-sized enterprises with respect to labour productivity, patenting activity and employment compared to statistically similar enterprises without Industrial PhD projects. Patenting activity nearly doubles and employment is nearly 2 persons higher per PhD project per year. Both analyses show positive impacts for individual employment and salaries in enterprises. Neither shows any impact on total factor productivity or on large enterprises.
Study no. 9-10	**An independent impact evaluation of the Danish Innovation Assistant Scheme (Knowledge Pilots) (DASTI 04/2010)** shows positive but statistically insignificant impacts for enterprises. The scheme provides a subsidy of up to EUR 20,000 to SMEs hiring university graduates. Key performance indicators analysed are gross profits, total employment and survival rates of enterprises. **An evaluation of the Danish Innovation Assistant Programme (Videnpilotordningen) (DASTI ?/ 2013)** shows that there are positive short-term employment effects for the innovation assistants, but no statistically significant impacts for enterprises. The scheme provides a subsidy of up to EUR 20,000 for SMEs hiring university graduates. Key performance indicators analysed are gross profits, value added, return on assets, labour productivity, total employment and survival rates of enterprises.
Study no. 11	**An independent study of the impact of PhD candidates on productivity in enterprises (DASTI 2012, prepared by CEBR – Centre for Economic and Business Research at CBS, Copenhagen, 23 September 2011)** shows that the average labour productivity in enterprises with at least one PhD candidate is approximately 34 percent higher compared to enterprises with the same mix of educations and skills but without a PhD candidate. The impact of PhD candidates seems to be smaller in small enterprises than in larger enterprises. The average labour productivity difference for small enterprises with and without PhD candidates is 11 percent. The salary of PhD candidates is approximately 10 percent higher than the salary of non-PhD individuals with same educational background, age and sex and working in the same type of enterprise and business sector.
Study no. 12	**The Report on 'Productivity and higher education' conducted by the Centre for Economic and Business Research (CEBR) for the Danish Business Research Academy (DEA) in 2010.** The effect of different types of highly-educated working capacities on productivity (added value) in 138,372 Danish enterprises over a nine year period (from 1999 to 2007) is analysed. The analysis shows that productivity for each individual increases with the length of the individual's educational background, regardless of the field of education. An education within social sciences results in the highest individual productivity. Technical and health sciences and scientific educations result in a slightly lower productivity than social sciences. An increase of one percentage point in the share of employees with an education at a master's degree level will cause an increase in GNP by approximately 1 per cent.
Focus area	**Commercial exploitation of public inventions**
Study no. 13	**An independent impact evaluation of the Incubator Programme (DASTI 01/2010)** shows that there are no statistically significant impacts for more than 300 enterprises and more than 300 entrepreneurs. The programme provides public risk capital for the establishment of new knowledge intensive enterprises. Key performance indicators analysed are individual salaries, total factor productivity, total employment and survival rates of enterprises. Because of the lack of sufficient data and observations, a new independent impact evaluation is to be conducted in 2014. The focus of the upcoming study is impacts at enterprise level and for individual entrepreneurs.

2 PERFORMANCE OBJECTIVES STANDARD: KEY PERFORMANCE INDICATORS

>

The Danish Ministry of Science, Innovation and Higher Education has headed an international effort on performance indicators in the EU Pro-INNO project called EPISIS. This collaboration had participants representing government agencies, ministries and researchers from countries including Denmark, Sweden, Germany, United Kingdom and Finland as well as the European Commission. Good practice on evaluations and performance measurements was exchanged, and a manual was elaborated with recommendations for indicators that can be used for setting out performance objectives and key performance indicators.

2.1. Independence and excellence

Decisive emphasis is placed on carrying out independent evaluations and performance measurements. The intention is to carry out external performance measurements based on the best and most widely accepted international research-based statistical methods. Evaluations are carried out by independent researchers and knowledge consultants. Efforts are undertaken to ensure the quality and utility value of all impact evaluations by having the external and independent parties discuss the evaluations with other independent research organisations that are not behind the analyses. This can be achieved for instance by establishing steering committees or conducting peer reviews, seminars etc. on a par with the procedures and processes that also apply to publishing in international research journals. Emphasis is placed on publishing the results of the completed impact evaluations in for example the most accepted international journals or at high-level international conferences.

2.2. Ex ante evaluation

The objectives and expected effects of each innovation instrument is stated in separate instrument descriptions approved by the ministry's management. This means the instrument description also includes an ex ante evaluation of the instrument. On this basis, the Danish Ministry of Science, Innovation and Higher Education sets out key performance indicators for each innovation instrument, which can be key performance objectives in the form of so-called output and input objectives. The assessment of indicators to be selected follows the EPISIS project work as well as national legislation.

In each instrument description, the ministry aims to document the choice of the listed performance objectives, the work to follow up on the performance objectives, and the plans for verifying the effects of the innovation instrument in question.

The overview below shows known key performance indicators for output (results), input (effects) and assessment criteria for each innovation instrument in the ministry.

2.3 Baseline measurement at ex post evaluation

Emphasis is placed on ensuring baseline measurements of the efforts in order to be able to document the situation before the launch of the innovation instrument and the situation if the instrument had not been implemented. This enables estimating the effects of the innovation instrument relative to a situation where the instrument did not exist.

To this end, the most recent research-based methods are applied by choosing advanced control groups that represent the situation if the instrument had not been implemented. If the analysis includes a sufficiently large number of observations, the propensity score matching method can be used for making baseline measurements, cf. below. On this basis, ex post evaluations can be carried out with estimations of instrument effects.

Key performance indicators (impact) for each instrument	Innovation voucher	Innovation consortia	Innovation Assistant
Individual employment			X
Effect on employment in enterprise	X	X	X
Added value growth in enterprise (gross product)	X	X	X
Productivity per employee in enterprise	X	X	X
Individual salary effect		X	X
Survival rate for enterprises	X		X
Key performance indicators (input and output) for each instrument	**Innovation voucher**	**Innovation consortia**	**Innovation Assistant**
Innovation ability	X	X	X
Investments in private research	X	X	
Investments in innovation	X	X	X
PhD production, patenting etc.		X	
Mobility of labour between public and private sector			X
Regional distribution of activities	X	X	X
Collaboration projects between enterprises and knowledge institutions	X	X	X
Gender distribution			X
Participation of small enterprises	X	X	X
Number of enterprises		X	
Number of newly established enterprises			

Euro-stars	Industrial PhD	Networks and clusters	GTS institutes	Innovation incubators
	X			X
X	X			X
X	X	X	X	X
X	X	X	X	X
	X			X
				X
Euro-stars	Industrial PhD	Networks and clusters	GTS institutes	Innovation incubators
X	X	X	X	
X	X	X	X	X
		X	X	
X	X		X	
	X			
X	X	X	X	X
X	X	X	X	X
	X			
X	X	X	X	
X	X	X	X	
				X

Assessment criteria	Innovation voucher	Innovation consortia	Innovation Assistant
Research height		X	
Innovation height	X	X	
Commercial utility		X	
Social utility		X	
Education			X
Employment			X
Project control and project management		X	
Knowledge dissemination		X	
Requirement on participation of small enterprises	X		X
Partner composition and enterprise participation	X	X	
Economy and private co-funding	X	X	X
Professional focus area	X	X	X

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Euro-stars	Industrial PhD	Networks and clusters	GTS institutes	
X	X		X	
X			X	
X		X	X	
X		X		
	X		(X)	
X	X			
		X	X	
X		X	X	
X	X	X		
X	X	X	X	
X	X	X	X	

2.4 Key performance indicators/objectives: Results of impact evaluations

Econometric impact evaluations have been carried out for most instruments. Tables 1-3 show whether there are significant effects of the innovation instrument relative to the control groups. The control groups may consist of either similar enterprises or individuals that did not participate in the instrument.

Table 1 looks at instruments that involve direct enterprise grants.

Table 2 looks at instruments that focus on R&D, but where there are no direct enterprise grants. In general, national business-research collaboration projects only receive indirect enterprise grants through R&D funding at knowledge institutions.

Table 3 looks at initiatives where patenting activities have been analysed.

TABLE 1. Direct enterprise grants: Status of performance measurements
(effect relative to control group)

Performance objective and documented effect in evaluations	Productivity per employee	Added value in enterprises	Employment in enterprises	Individual salary effect	Survival rate	Individual employment
Innovation assistant	Insignificant	Insignificant	Insignificant	Insignificant	Insignificant	Significant on a short-term basis
Industrial PhD	Significant	Significant	Significant	Significant	Not a performance objective	Significant
Innovation incubators	Insignificant	Insignificant	Not studied	Insignificant	Insignificant	Not a performance objective
Eureka	Significant	Significant	Significant	Insignificant	Not a performance objective	Not a performance objective

>

TABLE 2. No direct enterprise grants: Status of performance measurements
(effect relative to control group)

Performance objective and documented effect in evaluations	Productivity per employee	Added value in enterprises	Employment in enterprises	Individual salary effect	Export growth	Share of innovative enterprises in Denmark
Innovation consortia	Significant	Significant	Significant	Significant	Not a performance objective	Not a performance objective
Innovation networks and clusters	Significant*	Significant*	Not a performance objective*	Not a performance objective	Not a performance objective	Significant
GTS collaboration	Significant	Significant	Not a performance objective	Not a performance objective	Not a performance objective	Not a performance objective
University collaboration	Significant	Significant	Not a performance objective	Not a performance objective	Not a performance objective	Not a performance objective
Purchase of R&D at knowledge institution	Insignificant	Insignificant	Not a performance objective	Not a performance objective	Not a performance objective	Not a performance objective
Business sector's investments in R&D	Significant	Significant	Significant	Not a performance objective	Not a performance objective	Not a performance objective

* The innovation networks generate, among other things, collaboration projects with universities, GTS institutes and innovation consortia, and the results follow the performance measurements for innovation consortia, GTS institutes and universities.

TABLE 3. Patenting activity
(effect relative to control group)

Documented effect in impact evaluations	Patenting activity
Innovation consortia	Significant*
Industrial PhD	Significant*

* CEBR research projects and DASTI 01/2011

3 STANDARDS FOR COMPARISON GROUPS (CONTROL GROUPS) >

In order to assess the isolated results and effects of an innovation instrument or the difference in results and effects between two instruments, the development of key performance indicators for the enterprises or individuals participating in an innovation policy instrument must always include a comparison group (control group) of enterprises or individuals. The purpose is to study the difference in the results between two instruments, or whether there is an added effect from participating in an instrument as opposed to not participating.

3.1 Minimum requirements for the selection of comparable enterprises

When selecting enterprise control groups, it is important to consider that the enterprises that participate in an instrument are compared with other enterprises that are not participating, but are similar in as many relevant parameters as possible that may be of significance to the effect of the analysed instrument. The minimum requirements are that as many different factors as possible are taken into account, but this also depends on the instrument analysed. When selecting control groups, enterprises must be chosen that are more or less equally likely to participate, but have not. The probability model can be based on the following variables:

- Educational level of the enterprise's employees
- R&D intensity
- R&D department
- Export intensity
- R&D investments
- Profit, surplus or contribution margin
- Enterprise size
- Industrial sector

It is recommended that a propensity score matching be used. The point of the comparison group is to figure out what would have happened at participating enterprises if they had not participated. If the alternative to participation would be that the enterprises participated in a similar initiative, it makes good sense to compare with other enterprises that participated in a similar initiative - otherwise, it does not.

However, it is important to avoid including too many explanatory variables, which may give overlapping results, either individually or in combination. By including too many identical variables, there is a risk that multicollinearity will occur along with too great a correlation between the explanatory variables. This means that the parameters become insignificant and the result becomes biased. An example is if R&D intensity is included along with R&D investments, R&D department and enterprise size, as there is interdependency between these variables.

3.2 Minimum requirements for the selection of comparable individuals

When selecting control groups for individuals, individuals must be chosen who were as likely to participate as the participating individuals, yet did not. The probability model can be based on the following variables:

- Education
- Educational institution
- Enterprise size
- Industrial sector
- Gender and age
- Any other socioeconomic variables, such as salary, background etc.

It is recommended that a propensity score matching be used. When comparing with what would have happened if the individual had participated in another initiative, a control group can also be a group of individuals who participate in the other similar initiative.

3.3 Standard method for selection of comparable control groups

3.3.1 Control groups may be selected using a so-called 'propensity score matching' and 'nearest neighbour' method

The recommended standard method is the 'propensity score nearest neighbour matching method', which is used to establish and delimit, on a one-to-one scale, the group of enterprises that participate in an instrument, and a statistically comparable control group of enterprises that do not participate, but are as likely to do so.

It is impossible to find a control group that is completely identical.[11] The probability models for enterprises' participation in an instrument, which are used to identify factors that affect the likelihood of participation, are set out as logistic regressions and used in connection with the propensity score matching method.

In most cases, it will be an advantage to put together a control group that has as many control enterprises as possible – based on the law of large numbers. Accordingly, one-to-one is a minimum requirement, but the standard should be one-to-many. Furthermore, this should be supplemented by balance tests in order to analyse the difference between the treatment group and the control group.

[11] Examples of application of this method are found in DASTI (01/2010) and DASTI (02/2011).

The idea of the propensity score matching method is that for an enterprise T which participates in the instrument, an enterprise C is found among the other enterprises in the statistical material. For a number of statistical parameters, enterprise C should resemble enterprise T by having the same probability ('propensity score') of participating in the instrument, except that in actuality, enterprise C has not participated. In this way, enterprise T (designated as the 'treatment' or 'participating' enterprise) can be compared to a similar enterprise C (designated as the 'comparison' or 'control' enterprise) found in the statistical material. Statistically, enterprise C must resemble enterprise T with regard to industrial sector, enterprise size, export pattern, staff education, profit, contribution margin and composition as well as R&D or innovation activities.

It naturally follows that it is not possible to find a control group that is completely identical in all partially unobservable factors using this or other methods. Another selected control group may give different results. It is thus important to be able to interpret the characteristics found in the control group.

3.3.2 The control group may be selected through comparison with other innovation programmes

When comparing effects across instruments, the standard is that the comparison group is found among participating individuals or enterprises in the instruments to be compared. Here, it is important that observation sorting and data cleaning as a minimum is done the same way for all instruments.[12]

[12] Examples of a programme comparison is the comparison between ordinary PhDs and Industrial PhDs in DASTI (01/2011), and the comparison between enterprises participating in EUREKA projects and innovation consortia in DASTI (15/2011).

The possibilities depend on the design of the instruments. For some innovation instruments, considerably more precise estimation methods than the matching method described above and the difference-in-difference method described below are possible. This depends on, for instance, whether a regression-discontinuity design is possible.

4.1 The difference-in-difference method

One of the recommended central statistical methods that has been used until now is the difference-in-difference method. This method is used to calculate differences between developments of the treatment group and the control group.[13]

The difference-in-difference method is based on comparing output changes (the performance objective). The model looks as follows:

$$\delta = Y_1^T - Y_0^T - \left(Y_1^C - Y_0^C\right)$$

- in which δ is the effect of the instrument. This is calculated on basis of the difference between the performance indicator development, called Y, of the treatment group (T) - defined as the performance indicator at time 1 minus the performance indicator at time 0 - and the performance indicator development of the control group (C) - defined as the performance indicator in time 1 minus the performance indicator in time 0. Whether there is a significant difference between the two can be tested subsequently by e.g. standard t-tests or linear regression.

BOX 1. Central analysis method: Difference-in-difference

Difference-in-difference:

(a) before-after comparison for enterprises that participate in the instrument (participant)

(b) before-after comparison for enterprises that do not participate in the instrument (control)

See whether (a) is more positive than (b).

T1 – success parameter of participant before.
T2 – success parameter of participant after.
C1 – success parameter of non-participant before.
C2 – success parameter of non-participant after.

The difference (T2-T1)-(C2-C1) measures the difference between the increases.

[13] Examples of application of this method may be found in DASTI (1/2010) and DASTI (2/2011).

4.2 Balanced panel data

The effect of enterprises' R&D investments on added value and productivity per employee is a dynamic process which may vary over time. Cross-sectional analyses based on a single year are not adequate for analysis of the variation over time. In addition, there may be unobservable effects on the individual enterprise which the models are not able to take into consideration. The before-after comparison that results from applying the difference-in-difference method means that panel data (cross-sectional data over time) and methods are needed to check for these unobservable effects.

Large enterprises are included in R&D statistics every year, while samples of small and medium-sized enterprises are selected randomly. The result is a very 'unbalanced' panel. For some enterprises, observations are available for all years, while others only have data for one or a few years.

Because of this, it is recommended that the panel data set is put together as follows:

- Panel data analyses are only to be made for enterprises with at least two observations. In order to ensure that the analyses are as representative as possible, all enterprises with two or more observations are to be included. If the data basis allows, the requirements may be made more stringent, so only enterprises with three or more observations are included. Naturally, this will reduce the number of enterprises in the analysis.

- The following approach is recommended for missing observations in time series: If a single observation is missing in a time series, the single missing observation should be estimated. If two or more years are missing in the time series, the most recent continuous part of the time series should be kept.

- Extensive changes in the variables may indicate a merger or division of the enterprise. Such changes may have a disproportionately large effect on the results. It is recommended that enterprises with annual growth rates in added value, fixed assets, number of employees or R&D capital of less than - 50 % or more than 300 % be removed. This is in accordance with the standard set out in international literature.

- It is recommended that sensitivity analyses be carried out when basic data are changed.

The Cobb-Douglas productivity function is used as a standard for indicating the effects of a given instrument in pounds and pence in the form of increased productivity per employee, profits, etc. This is typically modelled as an OLS regression.[14]

Depending on the chosen key performance indicator (the analysed success variable), changes of levels over time may also be seen as might annual growth rate changes over time. An example of changes in levels would be changes in the number of employees and in the level of employment.

An example of relative changes would be the survival rate of enterprises or employment quotas. Examples of changes in growth rates are growth in productivity per employee, growth in turnover or growth in added value in enterprises.
In general, the standard for calculating economic effects depends on the key performance objectives that are assessed and estimated.

When selecting background factors, it is important to consider how the individual background factors affect both outcome and treatment. For instance, there may be a time-related challenge with background variables which might be affected by treatment in a model that includes lagged variables.

[14] Examples of application of this method may be found in DASTI (02/2011).

6.1 Causality and use of control groups

The effect of R&D investments and a particular instrument are often indirect and therefore difficult to measure and identify. It is difficult to isolate the actual effect that may be the result of many and varying external factors. It is also difficult to identify causality.

The selection of control groups is important for the question of causality. Accordingly, a standard is recommended for the establishment of control groups based on information about the enterprises' industry, export, size, internationalisation, R&D characteristics, employee composition and employee education. This way, a basis is established for making it probable whether there is a causal connection between the factor analysed and the performance objective, along with the basis for measuring the isolated effect.

6.2 Standards for analysis of R&D-active or innovative enterprises

In general, analyses of R&D instruments are based on employee productivity, employment, profits, survival rates, patent activity etc. at R&D-active enterprises only. If enterprises that do not conduct research and development, e.g. innovative or non-innovative enterprises, were to be included in the econometric analysis, it would be necessary to apply suitable methods to allow for differences between R&D-active and non-R&D-active enterprises. The methods are relatively complex and require an extensive analysis of the factors that make enterprises choose to invest in R&D.[15]

It should be assessed whether a control group should be established from R&D-active enterprises only or whether innovative enterprises and non-innovative enterprises should also be included.

If the control group consists of R&D-active enterprises only this must be justified, e.g. by how the analysed instrument is not an instrument that all enterprises can participate in overnight, but is restricted to R&D-active enterprises only.

This is a strict assumption which will undoubtedly exclude enterprises that were predisposed for the analysed activity. Conversely, it may also be a conservative assumption that helps ensure robustness of results, as it avoids a control group of enterprises where the probability of participation is very low.

[15] The methods first estimate the tendency to invest in R&D and then estimate what the enterprises' R&D activities would have been if the enterprises had chosen to invest in R&D. These estimated values can be used in productivity analyses or other performance measurements. The so-called CDM model (Crépon et al, 1998) applies a similar approach to analysing the relationship between innovation and e.g. productivity, albeit only in part. Crépon et al estimate the tendency to be innovative in order to check for selection bias, but only include R&D-active enterprises in the analysis.

6.3 Treatment of outliers

For results to be as representative as possible, econometric models should be able to measure effects in a wide range of enterprises. However, extreme values may distort the effects and reduce precision. In some cases, there may be good reasons for removing extreme values. An example is young enterprises where large and risky investments are made, which affect the enterprises' added value for a short period of time. Such enterprises will potentially experience extreme increases from one year to the next.

However, whether or not extreme values should be removed depends on the purpose of the analysis and the innovation instrument. Thus, a careful assessment of outliers should be carried out for each analysis and instrument before they are excluded.

Furthermore, data have been found to include extreme values measured against e.g. enterprises' average productivity per employee, added value, employment, etc. These are assumed to be incorrect registrations connected either to the enterprise's added value or to the number of full-year employee equivalents. Regardless of where the incorrect registration is found, it is recommended that such values are removed from the data.

There may however be other methods, for example to include or exclude extreme data to see whether this has any effect on the results, or to consider medians, etc.

6.4 Structure of output variable and valuation

It is not always easy to identify and delimit effects. Also, differences occur in valuation depending on players and stakeholders. An example is an enterprise's market value. One way is to use the market's valuation of the individual enterprise as a measure for the price or value of the total 'tangible' and 'intangible' assets. However, this would require the enterprises in the analysis to be quoted on the stock exchange. This means this method is not used, as most enterprises are not quoted on the stock exchange.

When effects in enterprises are analysed, it is recommended that a key performance indicator relative to labour input is used. This way, it is ensured that the effects cannot be attributed to an endless supply of labour.

6.5 Modelling of connection between instrument and effect

Effect measuring is complex, since a linear connection between the analysed instrument and a subsequent effect is hardly ever found. Accordingly, there are a number of conditions that may make it difficult to measure effects, such as potential time layers before the effects set in, different starting points for the enterprises, differences between the enterprises' characteristics and the enterprises' experience and competences with regard to the instrument.

As a standard, the econometric models must therefore be able to make allowance for:

- Time lag between the analysed instrument and its effects. The effects may set in with varying delays.
- Correction for enterprise differences. The enterprises in the analyses will vary in size, industry, market conditions, globalisation and other objective factors. It is important to check for these factors when isolating the effects. To avoid 'losing' some of the effects in the analyses because the data set includes many different enterprises where there will be different effects, the analyses should both treat data as a whole and include information for each enterprise/individual about their industrial sector and number of employees.
- It is also recommended to carry out enterprise analyses for different industrial sectors and enterprise sizes if the data basis allows.

6.6 Spillover effects

The transaction mechanisms between activities and their yield are complex, as there is no linear connection between activities and yield. Besides, there may be multiple gains that may be difficult to delimit and valuate.[16]

[16] In OECD contexts, the concept of behaviour additionality is used increasingly to measure and define the multiple gains from innovation instruments, among other things. However, it is still very difficult to attach a value on the additionalities.

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One of the challenges in measuring the effects of innovation instruments is that knowledge is a ‘non-competing’ advantage. This means that enterprises, individuals or public institutions may benefit from knowledge produced by others. And if such knowledge is transferred, it can be further developed through other innovation instruments. This becomes even more evident for innovation policy instruments that are meant to be combined with other instruments, e.g. if an Industrial PhD student takes part in an innovation consortium, or if innovation consortia or innovation voucher collaboration projects are facilitated through activities in the Danish Ministry of Higher Education’s innovation networks. In literature, some researchers argue that knowledge increases in value when it is shared and used by several different players and enterprises. The increase and dissemination of knowledge between the different players and enterprises is achieved by collecting knowledge and through the mobility of labour, as employees carry with them knowledge they have gained through other enterprises and research institutions’ investments in research, development and innovation.

Other enterprises than the one that has participated in the analysed activity will have higher marginal earnings on a product, either because manufacturing the product has become more efficient and thus cheaper, or because the production value has increased and the product can be sold at a higher price. However, the effect does not only benefit the manufacturer, but all links in the value chain, right through to the wholesaler or retailer.

The spillover effect from knowledge can also create so-called creative destruction. Here, innovation and development of new products and services will remove value from existing products and services. As a result, it has a negative impact on the effects for other enterprises. Hence, performance measurements should be supplemented by other types of economic models which may pick up transmission mechanisms and spillover effects better than microeconomic models, if the full effect of the analysed activity at a socio-economic level is to be exposed.

7 STATISTICS FOR PERFORMANCE MEASUREMENTST >

The following national statistics are used in connection with the impact evaluations:

- R&D statistics (Denmark's National Statistical Bureau)
- Accounts statistics (Denmark's National Statistical Bureau)
- Community Innovation Survey (CIS) (Denmark's National Statistical Bureau)
- Education statistics (Denmark's National Statistical Bureau)
- Project databases in ministries and funding agencies
- Patent statistics (Denmark's National Statistical Bureau)
- Labour market statistics (Denmark's National Statistical Bureau)
- Salary statistics (Denmark's National Statistical Bureau)
- The Danish Commerce and Companies Agency's Central Business Register / *Købmandsstandens Oplysningsbureau/Experian A/S* (Danish Business World's Information Agency)

PUBLICATIONS

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Publications from the Danish Agency for Science, Technology and Innovation in the series *Innovation: Analysis and evaluation*

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2011

20/2011	Access to Research and Technical Information in Denmark
19/2011	Universiteternes Iværksætterbarometer 2011
18/2011	Impact Study: The Innovation Network Programme
17/2011	Clusters are Individuals: Nordic-German-Polish Cluster Excellence Benchmarking
16/2011	24 ways to cluster excellence – successful case stories from clusters in Germany, Poland and the Nordic countries

15/2011 Impact Study of Eureka Projects
14/2011 Evaluering af GTS-instituttet Teknologisk Institut
13/2011 Evaluering af GTS-instituttet DBI
12/2011 Evaluering af GTS-instituttet DELTA
11/2011 Kommercialisering af forskningsresultater – Statistik 2010 (Public Research Commercialisation Survey – Denmark 2010)
10/2011 Performanceregnskab for Videnskabsministeriets GTS-net 2011
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2010

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