An Analysis of Firm Growth Effects of the Danish Innovation Consortium Scheme

Innovation: Analyse og evaluering 3/2010

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by
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Johan M. Kuhn, Ph.D.

Danish Agency for Science, Technology and Innovation, 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.

[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.

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.

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.

6. Estimation set-up >

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'.

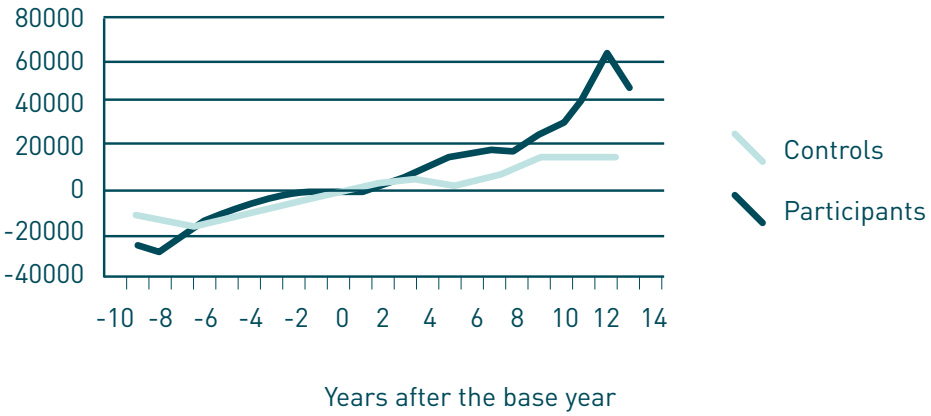
Still, as mentioned already, the chosen cut-offs are, of course, arbitrary. Hence, the robustness of findings with respect to changing the thresholds will be discussed in section 9.

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.

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.

FIGURE 2: Number of employees. Mean difference compared to base year. Firms with less than 300 employees in base year. 3-year moving averages

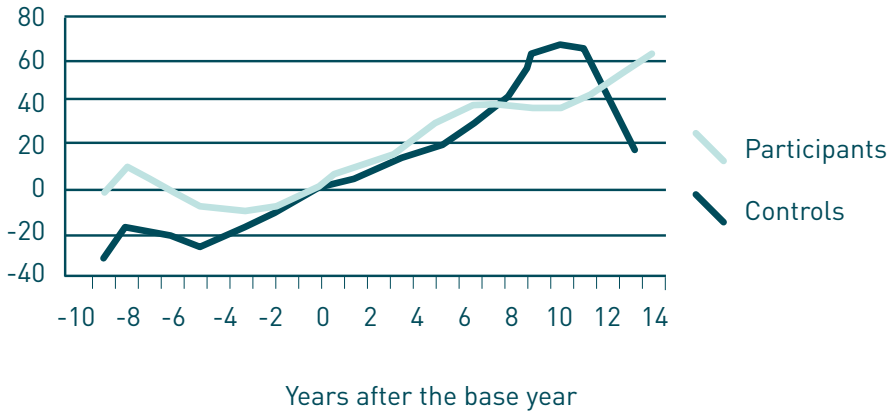


FIGURE 2 does not suggest robust positive IC programme effects, although one might notice that growth accelerates for participants but not controls in the years close to the base year.

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.

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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:

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.

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.

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%).

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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%).

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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 $\beta 4$ to be estimated. $\beta 1$, $\beta 2$, and $\beta 3$ 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.

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.

TABLE A3.2: Results of logit model regressions				
	Model 1: Dependent variable: exit before 2008		Model 2: Dependent variable: firm closure (before 2008)	
	Coefficient	Standard error	Coefficient	Standard error
Firm is participant firm	-0,856***	0,209	-0,291	0,299
Constant term	-3,303***	0,093	-4,469	0,163
	Pseudo R2=0.014 5,238 observations		Pseudo R2=0.0016 5,238 observations	

*** significant at 1%. ** significant at 5%, * significant at 10%; both models are estimated on all firm-year observation after the base year and before 2008.

Finally, we investigate whether the relationships between the growth in the success parameters and exit probability are different for participants and controls.

If for example fast-growing controls have a disproportionally low propensity of leaving the data, any effects of the programme would be underestimated – because there would be disproportionally many fast-growing controls which are observed five or ten years after the base year compared to relatively fewer fast-growing participants.

TABLE A3.3: Results of logit model regressions. Dependent variable: the firm exits the data. Gross profit measured in million DKK

		Coefficient	Standard error
Firm is participant firm	(1)	-0,9124***	0,2654
#employees(t)-#employees(base year)	(2)	-0,0004	0,0006
gross profit (t)-gross profit (base year)	(3)	-0,0025**	0,0013
{#employees(t)-#employees(base year)} *(Firm is participant firm)	(4)	0,0006	0,0009
{gross profit (t)-gross profit (base year)}*(Firm is participant firm)	(5)	0,0025*	0,0013
Years after base year	(6)	-0,0172	0,0356
Constant term	(7)	-3,4500***	0,2087
		Pseudo R2=0.014 5,238 observations	

*** significant at 1%. ** significant at 5%, * significant at 10%; the model is estimated on all firm-year observation after the base year and before 2008.

The results of this model are presented in TABLE A3.3. Coefficient (5) suggests that participants with high growth in gross profit have higher risk of leaving the data compared to controls. A participant that increases gross profit by 10 million DKK increases the probability of leaving the data by approx. 3 percent. This implies an absolute percentage point increase of approx. 0.09 percent. Put different, differences are negligible, and, if anything, the measured increases in growth in association with programme participation would have been larger if not a number of high growth participants would have left the data.

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Virksomheder oplever store gevinster ved at samarbejde med videninstitutioner om forskning og udvikling >

En virksomhed, der har deltaget i et forsknings- og udviklingssamarbejde med universiteter og GTS-institutter, oplever i løbet af de næste 10 år en merværditilvækst, der er ca. 20 mio. kroner højere end for lignende virksomheder, som ikke har været med i et samarbejde. Der er endvidere signifikant positive beskæftigelseseffekter for virksomheder, der havde mindre end 150 medarbejdere året før de indgik i samarbejde.

Det dokumenterer denne analyse som Centre for Economic and Business Research (CEBR) på Copenhagen Business School (CBS) har lavet for Forsknings- og Innovationsstyrelsen. Analysen er baseret på registerdata fra 220 virksomheder, der i perioden 1995-2003 har deltaget i et innovationskonsortium med statslig medfinansiering.