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Do Consumer Subsidies Increase Productivity? Evidence from Swedish Service Firms

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Abstract

We suggest that *ROT-avdraget* has had a positive effect on industry productivity in the business sector affected by the policy and that this effect is divided into two statistically proven parts: the average firm is becoming more productive and the most productive firms gain market share. Industry and firm level productivity is estimated on a large panel data sample of firms using Levinsohn and Petrin's semi parametric model. By utilizing a natural experimental design where the companies affected by the subsidy is evaluated against a control group operating in a closely related business sector, we are able to perform several difference in difference regressions to test the relationship between a demand increasing ad valorem subsidy and productivity in the business sector providing these subsidized services. Testing for robustness using a second control group yields a positive point estimate of the true effect on productivity of the policy as well, but this is not statistically significant at a reasonable level. Finally we correct the standard errors for biases related to potential serial correlation in the dependent variable as well as possible heteroskedasticity and conclude that these corrections do not change our results.

Key Words: Productivity, Consumer Subsidies, Demand-Pull Effects

JEL-Classifications: C14, D24, H24, H3, L11

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Table of Contents

List of Tables

List of Figures

1 Introduction

Productivity has been attributed the key independent variable in explaining the great dispersion of average GDP per capita across the world and accounts for a substantial fraction of the difference in average per capita income between countries. In light of these findings, it comes as no surprise that an important goal when designing the institutional framework is to make way for sustainable improvement in this factor.

Economic policies are introduced by governments to change the behavior of actors in the market. In May 2009 the Swedish government made it possible for consumers to deduct half the cost of certain services on their tax bill. This ad valorem consumer subsidy, called *ROT-avdraget*, was introduced in order to deal with the problems of an illicit labor market and increase employment as well as demand in the business sectors concerned (Regeringen 2005). However, in the proposition by the government the issue of a potential change in productivity in the companies affected by this policy was never explicitly discussed. Evidence has been provided suggesting that demand for these services has increased heavily after the subsidy was introduced (Skatteverket 2011). As far as we are concerned, the matter of a possible effect on productivity has still not been investigated.

The relationship of growth in demand and productivity growth was first established in 1949 with the introduction of "Verdoorns's law" which concluded that an increase in demand leads to a growth in productivity through economies of scale, increased investment and technology innovation (Verdoorn 1949).

As aggregate productivity in an economy carries such tremendous weight for national as well as average individual prosperity, it would be of interest to investigate how an ad valorem consumer subsidy directed to a specific sector, will affect productivity in the companies within that sector. By evaluating a natural experiment using unbalanced panel data on Swedish companies affected by *ROT-avdraget*, the treatment group, and companies in a related sector not affected by the policy, the control group, we hope to establish a causal connection between the subsidy and effects on productivity in the sector in which the subsidy is present. We estimate firm-level total factor productivity (henceforth TFP) as well as industry TFP using the Levinsohn-Petrin method for our treatment and control group respectively (Levinsohn, Petrin 2003). Three difference-in-difference (henceforth DID) regressions are conducted in order to test if there is an increase in productivity due to the policy change and investigate through which channels this potential increase is generated. Robustness tests are carried out by including robust standard errors in the regressions and substituting the first control group with a group of firms from another Scandinavian country, operating in the same industry code as the treatment group.

Results from this research will be useful for evaluating the effects of an ad valorem consumer subsidy and will hopefully induce policy makers to broaden their scope to include this parameter when conducting these evaluations. It will also hopefully be beneficial when evaluating other types of economic policies, which ultimately results in a higher demand. In general, increased knowledge of productivity effects related to this type of demand increasing subsidy may help legislators to produce a more efficient institutional framework. The growth in productivity stimulated by a high demand is a field of research that has been neglected during a large part of modern history. An ambition of this thesis is to generalize the results of studying the effects of ROT-avdraget to shed light on and remind of the strong effect demand may have on productivity growth and also investigate the channels through which this growth is generated.

The remainder of this paper comprises the following sections. Section 2 contains a short background on the subsidy called *ROT-avdraget* as well as some documented effects of it. In section 3 we present a brief review of the previous research on the relationship of consumer subsidies, increased demand and productivity. This section will also present some theories on how to properly measure productivity. Section 4 will present our detailed research focus along with hypotheses. Section 5 describes the method applied, while section 6 presents the data used. The empirical results will be presented and discussed in section 7 and section 8 concludes.

2 *ROT-avdraget* – A brief review

In 2007, the issue of whether to launch the economic policy, which is now known as *ROT-avdraget*, was discussed by the Swedish government. At the time, the market for the services, which was later affected by this policy, was booming. That in combination with the budget constraint of the government back then, was ultimately the deciding factors when the Swedish parliament decided to postpone the implementation of the policy. When the financial crisis struck the world about a year later, the previously thriving industry faced a severe recession. This gave legislators the strong incentive and perfect timing they had been waiting for to finally implement *ROT-avdraget* the 13th of May in 2009, following the proposition submitted by the government 26th of March, 2009.

This was not the first time a measure like this was discussed. In 1993 a very similar policy was introduced. It enabled property owners to deduct labor costs associated with repair, maintenance, refurbishment and extensions to a house. Back then the deduction was 30 percent of the labor costs and the subsidy was linked to the property. For private homes the maximum deduction amounted to 35,000 SEK in a year and for rentals it was capped at 20,000 SEK or three times the property taxes in the year before. The policy was in effect between the 15th of February 1993 and the 31st of December 1994. After a short interruption it was implemented again in 1996 with a new end date, 31st of December 1997, which was later extended to the 31st of March 1999. Finally it was introduced for the third time in April 2004 applying basically the same rules as before and ceased to exist in June 2005.

When introduced again in May 2009 with a retroactive effect dating back to the 8th of December in 2008, the arguments in favor of the policy was practically identical to the ones presented prior to the implementation of the policy in the past. The legislators wished to increase demand in the industry in order to save jobs and at the same time reduce the presence of undeclared labor in the market. By making it cheaper for consumers to buy these types of services, they will also be less inclined to do these tasks on their own. Hence there is an increased supply of labor in other parts of the economy. This promotes efficiency through specialization. A skilled carpenter will, in most cases, be able to perform activities related to this field of work in a more time and quality efficient manner, than for instance an accountant would.

In the economic policy *ROT-avdraget* that is applicable today, the tax reduction amounts to 50% of the labor costs associated with these services. The maximum deductible amount is 100,000 SEK per person and year, which implies that the maximum tax reduction is 50,000 SEK. This time the deduction is linked to the property owners, rather than the property itself. Another important difference is that the policy is not available for rental owners. Consumers can benefit from the so-called "In-voice model", which means that they only pay half the amount of labor cost upfront. The contractor notifies the Swedish tax authority to claim the remaining amount. Prior to this the customer paid the full amount and then had to reclaim the deduction from the Swedish tax authority. All in all, the services affected are the same as described in the previous section. As before, it is only the labor costs associated with these services that can be deducted. The policy does not cover labor costs associated with building new houses and is restricted in the areas of building an extension or refurbishment on newly built properties. This restriction is based on the logic, that consumers would want to exploit the benefits of the tax reduction when building a new house. By imposing the condition that only property owners who pay property fees are eligible to use the deduction when refurbishing or building extensions, the legislator effectively limits the possibility of building only partially finished houses that are later finished through refurbishments and extensions. Since newly built houses are exempt from property fees for five years following completion, the perception is that a majority of new homeowners will probably not use this exploitation strategy (Regeringen 2009).

There have been several studies documenting the effects of *ROT-avdraget*. In 2001, the government agency responsible for investigating the efficiency of public spending called *"Riksdagens Revisorer"* (now known as *"Riksrevisionen"*), published a report on the effects of *ROT-avdraget* during the nineties (Riksdagens Revisorer 2001). By retrieving data from a questionnaire sent out to property owners and contractors, they were able to conclude the following. The deduction seemed to have induced property owners to use these services more extensively. The policy was also responsible for alleviating some of the illicit labor present in this industry. They also stated that it is hard to conclude anything regarding to the potential effects on employment in the industry. The Swedish tax authority, on behalf of the Swedish government, published a more comprehensive and up to date report on the effects of *ROT-avraget* and in 2011, focusing on what has happened since *ROT-avdraget* was introduced in 2009 (Skatteverket 2011). The primary question of interest to this study is how the subsidy has affected the labor market in terms of the presence of illicit work. The study of the illicit labor market poses a major problem, since there exists no completely reliable statistics accounting for this phenomenon. In this study the tax authority has conducted interviews with many respondents on their attitudes towards illicit labor before and after *ROT-avdraget* had implemented. The general conclusion is that the acceptance of illicit labor has decreased and that there are a group of respondents who would have contracted illicit labor, had the new policy not been in effect. These two reasons lead to the final conclusion that the illicit labor present, probably has decreased. This view is complemented by another questionnaire study directed to business owners, where 78% states that the amount of assignments involving illicit labor has decreased. The study does however acknowledge a major limitation. The question of how they would have contracted the services if the policy had not been in effect is hypothetical and many of the respondents may have a bias towards the belief that they would use legal alternatives.

The Swedish tax authority has also provided statistical information, which has been put together by Statistics Sweden, in order to review the extent of the adoption of *ROT-avdraget* (Statistics Sweden 2012). In 2010 11.9 % of the Swedish population aged 20 and over utilized this subsidy, compared to 9.2 % in 2009. The preliminary numbers from the tax authority is pointing towards an even greater fraction in 2011. This report also shows that the subsidy is utilized more often by citizens between the ages of 35 and 70, a fact that can be explained by the condition that you have to own a property in order to reap the benefits associated with the subsidy. This is less common for younger and elderly citizens. Another finding is that people with higher incomes tend to use the subsidy more often.

3 Previous Research and Theory

Theories of How to Measure Productivity

Since Solow published his seminal paper on productivity (Solow 1957), the research relating to how to measure it has occupied a large body of empirical and theoretical researchers. There have been substantial contributions to this subject as late as during the 21st century. Since productivity is the main driver explaining the difference in GDP per capita, the factor and what can be made to improve it is of very high interest to economists all over the world. The applications for the measurement are numerous, but a very important usage of it is applied when evaluating the effects of raising the quality of an institution or implementing a change in economic policy. Business owners and managers all over the world are also interested in improving and measuring the efficiency of their processes and production techniques, as a high productivity is essential in order for businesses to survive and thrive in the globalized economy of today.

Productivity or total factor productivity (TFP) is a measure of the efficiency of production and is typically defined as a ratio of production output to inputs (Katayama, Lu & Tybout 2009). Although the notion of productivity is rather easy to understand, actually measuring it properly is pretty difficult. Depending on the data available productivity is measured using either non-parametric, semiparametric or parametric methods (Van Beveren 2012). Numerous methodological measurement problems arise due to a simultaneity or endogeneity problem (Marschak, Andrews 1945), a selection bias (Wedervang 1965), using industry-level deflators to proxy for-firm level prices (Katayama, Lu & Tybout 2009) and the fact that firm's product choices are likely to be related to their productivity (Bernard, Redding & Schott 2005).

Prior research has suggested several methods to mitigate the issues mentioned above and rendered some of the earlier applied methods, such as fixed effects instrumental variables and Generalized Methods of Moments, non-favorable. The underlying assumptions necessary in order to use any of these are often unrealistic or difficult to validate (Van Beveren 2012).

In the mid-nineties Olley and Pakes (1996) published their paper on a semi parametric model, which explicitly deals with the selection bias and mitigates the simultaneity problem to give more consistent estimators. The simultaneity problem is solved by using the firm's investment decision, where investments have to be positive, to proxy for unobserved productivity shocks and the selection bias are addressed by incorporating an exit rule into the model. The model relies on the assumptions that there is only one state variable, productivity, that is unobserved at the firm level and the model requires monotonicity on the investment variable to ensure invertibility of the investment demand function.

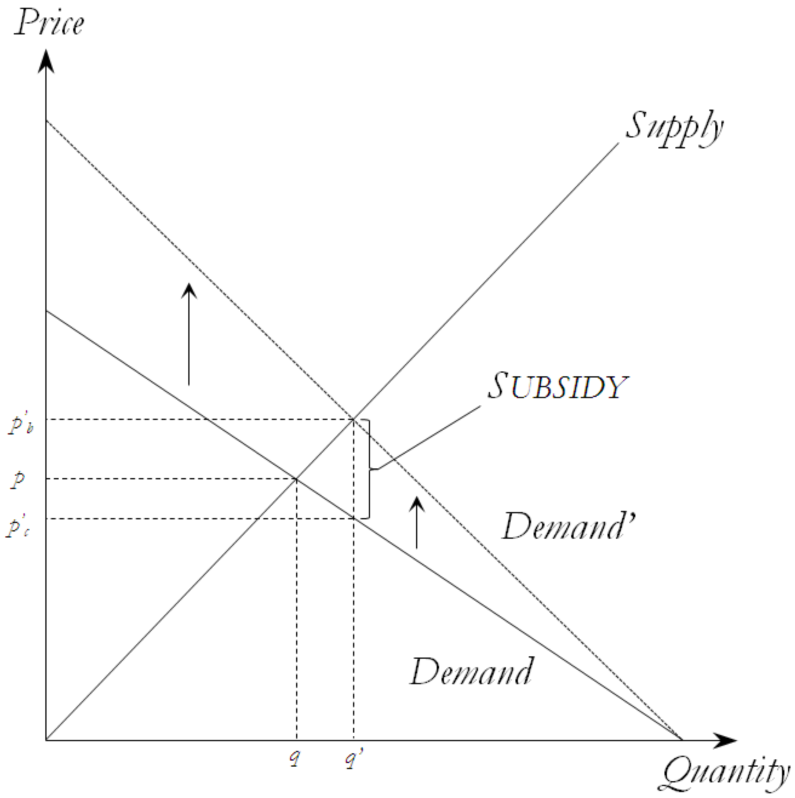
Levinsohn and Petrin (2003) introduced another semi parametric model, which also corrects for the simultaneity problem by using positive intermediate inputs as a proxy for unobserved productivity shocks (Levinsohn, Petrin 2003). Depending on what kind of data is available it might actually be preferred to use the Levinsohn-Petrin approach as it is often more likely that firms' have a positive use of intermediate inputs than that they would have positive investment. The fewer observations the estimation method must reject, the more likely is the monotonicity condition to hold. The Levinsohn Petrin model does not solve the selection bias problem, as it does not have an exit rule. The efficiency gains by accounting for this problem were very small when an unbalanced panel was used, as shown by the empirical results in the study of Olley and Pakes (Van Beveren 2012).

A few years ago De Loecker (2007) extended the Olley-Pakes methodology to correct for multi-product firms as well as the omitted output price bias relating to the use of industry, rather than firm-specific deflators.

The Effects of an Ad Valorem Consumer Subsidy

The research field of microeconomics provides a clear view of the effects of an ad valorem consumer subsidy. Ad valorem subsidies are different from the specific ones in the sense that they are based on a proportionate amount rather than a fixed one. Since the subsidy increases with price, the demand curve rotates upwards, as is illustrated in Figure I, if one were to assume a classic downward facing demand curve. Thus at any given price the consumer will demand more of a service or good as they only pay a fixed proportion of the price, in the case of *ROT-avdraget* 50%.

FIGURE I: THE EFFECTS OF AN AD VALOREM CONSUMER SUBSIDY ON THE MARKET EQUILIBRIUM



Notes: Assuming perfect competition, the ad valorem consumer subsidy rotates the demand curve upwards. Thus at any given price the consumer will demand more of a service or a good as they only pay a fixed proportion of the price. Price levels in equilibrium shift from p to p'_c *and* p'_b, where the former is the price the consumers receive and the latter the price the suppliers receive. The difference between the two is the consumer subsidy. Quantity shifts from q to q', in the new equilibrium.

Empirical Studies on Demand-Pull Effects and Productivity

The research related to productivity and the very important determinants of productivity growth have been extensive during the 20th and 21st century. Prior research has, however, mainly focused on supply side sources as drivers of growth in productivity. A few of these sources include, technology, innovation and capital. Less attention has been devoted to potential demand side factors and their effect on productivity.

An early paper by Verdoorn (1949) relates productivity performances to output, which in turn is highly dependent on demand. His results are concluded in the famous Verdoorn's law, which establishes a relationship between a growth in output and a growth in productivity in manufacturing companies, resting on theories of increasing returns to scale. Kaldor (2002) further develops the relationship specified by this law by adding the contribution of capital stock growth and presents this modification in his inaugural lecture at Cambridge. The arguments stated by Kaldor and Verdoorn are that increased demand induces increased specialization, resulting in enhanced skills and know-how of the workers as well as a boost in the introduction of technological innovation, which lead to increasing returns to scale and ultimately productivity growth. While most studies confirming this relationship has been conducted on manufacturing industries at least two studies during the 21st century has concluded that the law is applicable in service industries as well (Leon-Ledesma 2000; Seiter 2005).

In some of the latest research, Crespi and Pianta (2008) conclude that demand does have a positive effect on labor productivity, where they identify household consumption as the most important factor of demand with the ability to stimulate a greater efficiency in all industries. Cornwall and Cornwall (2002) states that a strong aggregate demand stimulates investment and technological change, leading to a higher productivity.

4 Delimitation and Hypothesis

Prior research has validated the pull effects of demand. The results are however based on aggregated data where demand and productivity is measured over industry levels in all industries or on a national level. Furthermore they do not link a single causal event in one industry, such as a policy change for instance, to the increase in productivity and explicitly address how the change is generated utilizing a natural experimental design with firm-level data. With their insights serving as a foundation, it would be interesting to account for what happens at the average firm level and industry aggregate level in a specific part of an economy; a service sector where demand is suddenly increased. The shortage of studies on demand-pull effects in service industries serves as another argument to why this study is important.

Based on this previous research in the field we formulate our main hypothesis, which reads as follows:

1. *The ad valorem consumer subsidy directed at this service sector will lead to increased demand and thus enhance TFP in the firms affected, ultimately increasing industry TFP.*

Moreover, our study is also trying to investigate how this expected increase in industry TFP is generated. As industry TFP is calculated as the weighted average firm-level TFP it is of high interest to the legislators how firms of different sizes are affected. This research goal is formalized in the question below:

2. *Will the resulting increased demand from the ad valorem consumer subsidy generate a uniform, proportionate increase in firm-level TFP or will there be a heterogeneous effect causing larger firms to benefit more from the subsidy?*

Finally, our last investigation concerns the importance of the reallocation effect contribution in the development of industry TFP, that is, we intend to answer the following question:

3. *Do the more productive firms, either through market share shifts among incumbents or through entry and exit, also increase their market share at the expense of less productive firms?*

Further research supporting our main hypothesis and guiding our subsequent investigations is to be found in the field of international economics. In a recent empirical study Lileeva and Trefler (2010) concludes that trade liberalization through a reduction of tariffs on imports between Canada and the United states lead to higher industry TFP in the sectors affected. They relate the gain in average firm-level TFP to a greater market created by the tariff cuts. They found that a larger market share induces firms to innovate their products and processes and improve their manufacturing information systems. Their results are in line with a theory developed by Schmookler (1954), which states that a larger market makes it more profitable for firms to invest in productivity-enhancing activities. The same argument can be applied to within market shares, making it more profitable for firms who already have a large market share to invest in productivity enhancing activities. Following this theory the larger firms would be more likely to invest and gain a higher productivity, provided that their market share remain constant while the overall market is increasing. As our case is somewhat analogous to the one studied by Lileeva and Trefler, in the sense *ROT-avdraget* has made way for a larger overall market for these services, we would probably expect that the policy has a heterogeneous effect on firms, benefitting larger ones.

There are some evidence against our main hypothesis, which was published in a paper by Aghion and Saint-Paul (1998). They argue that certain productivity improving activities are disruptive, meaning that firms must forego some output and profits in order to implement them. The notion is that a lower output, which is highly correlated with demand for the firm's products, will enable firm's to devote human resources to experiment with new technologies, managerial reorganizations and training programs. The opportunity cost of engaging in such productivity improving activities rises with a higher temporary demand and decreases with a lower temporary demand. The key issue is whether this demand change is perceived as temporary or permanent. If it is perceived as permanent the perceived opportunity cost will not change. As *ROT-avdraget* is a highly debated fiscal policy, which has been temporary in the past, the question of whether it is permanent this time is hard to answer. It is probably viewed as transitory by some and more permanent by others. If viewed as transitory there is a component present in the increased demand, which would actually work against our hypothesis.

5 Method

The Experimental Design

By introducing an ad valorem consumer subsidy directed at a restricted set of services, the government has enabled us to utilize a natural experimental approach in evaluating the effect of the subsidy. In a natural experimental design the participants in the treatment and control group are not selected at random as in a randomized controlled experiment. Instead they arise from the particular policy change (Wooldrigde 2009). Due to this, the general characteristics of the treatment and control group may differ, which in turn may cause them to react differently to macroeconomic conditions implying that causality is somewhat harder to prove. Choosing a suitable control group is key in order to obtain internal validity (Meyer 1995).

Estimating Productivity

As discussed briefly in the previous research section above, estimating productivity using OLS will lead to biased results due to the simultaneity problem and the selection bias. Using either the Olley-Pakes or Levinsohn-Petrin method will generally lead to less biased results. We opted for the Olley-Pakes method using the Stata command "opreg" developed by Yasar, Raciborski and Poi (2008), but our data lost too many observations to make this estimation feasible and efficient. The "opreg" command could not even be performed on the control group. Instead we estimate the production function and firm-level productivity using the Levinsohn-Petrin method and the "levpet" command provided by Petrin (2004). We also estimate the production function of the treatment group using each one of the three methods in order to compare the results. Although using the Levinsohn-Petrin model corrects for the bias caused by the simultaneity problem there are however still biases present due to the selection problem (Wedervang 1965) and imperfect competition in output and input markets causing omitted output and input price bias (Levinsohn and Melitz 2002). Recent research has also presented a bias stemming from the endogenous product choices of multi-product firms (Loecker 2007). Data restricts us from controlling for these biases.

The methods described above use different approaches in order to get an unbiased estimator of firm-level productivity. All of them do however rely on the basic relationship outlined below. At the establishment-level, productivity can be obtained through calculating the residual in the functional relationship where output depends on the input a firm employs and its productivity (Katayama, Lu & Tybout 2009). This relationship is usually written in the form of a Cobb Douglas function, as illustrated below:

$$Y_{it} = A_{it}K_{it}^{\beta_k}L_{it}^{\beta_l}M_{it}^{\beta_m} \tag{1}$$

where Y_{it} represents physical output of firm i in period t, K_{it}, L_{it}, and M_{it}, are inputs of capital, labor and materials respectively and; A_{it}, is the Hicksian neutral efficiency level of firm i in period t. Taking natural logs of (1) results in the linear production function:

$$y_{it} = \beta_0 + \beta_k k_{it} + \beta_l l_{it} + \beta_m m_{it} + \varepsilon_{it}$$

where lower case letters refer to natural logarithms and

$$\ln(A_{it}) = \beta_0 + \varepsilon_{it}$$

While β_0 measures the mean efficiency level across firms and over time; ε_{it} is the time- and producer-specific deviation from that mean, which can then be further decomposed into an observable (or at least predictable) and unobservable component. This results in the following equation,

$$y_{it} = \beta_0 + \beta_k k_{it} + \beta_l l_{it} + \beta_m m_{it} + w_{it} + u_{it}^q \tag{2}$$

Where w_{it} represents firm-level productivity and u_{it}^q is an i.i.d. component, representing unexpected deviations from the mean due to measurement error, unexpected delays or other external circumstances.

Typically empirical researchers estimate (2) and solve for $\widehat{w}_{it}$. Estimated productivity can then be calculated as follows:

$$\widehat{w}_{it} = y_{it} - \hat{\beta}_k k_{it} - \hat{\beta}_l l_{it} - \hat{\beta}_m m_{it} \tag{3}$$

and productivity in levels can be obtained as the exponential of $\widehat{w}_{it}$, shown in (4).

$$\widehat{\Omega}_{it} = \exp\,(\widehat{w}_{it}) \tag{4}$$

The productivity measure resulting from (4) can be used to evaluate the influence and impact of various policy variables directly at the firm level; or alternatively, firm-level TFP can be aggregated to the industry level by calculating the share-weighted average of $\widehat{\Omega}_{it}$. Weights used to aggregate firm-level TFP can be firm-level output shares as is applied using the Olley and Pakes method. Equation (4) shows how industry TFP can be calculated for each year:

$$\hat{P}_{Jt} = \sum_{i=1}^{Nt} s_{it}\hat{\Omega}_{it} \tag{5}$$

where $\hat{P}_{Jt}$ is predicted industry productivity at year t, s_{it} is firm-level output share and $\hat{\Omega}_{it}$ is predicted firm-level productivity.

Industry productivity can then be further decomposed into an unweighted average and a sample covariance term as shown in equation (5). While differences in the unweighted average over time refer to within-firm changes in TFP; changes in the sample covariance term signal reallocation of market shares as the driver of productivity shifts (Olley and Pakes, 1996).

$$\hat{P}_{Jt} = \sum_{i=1}^{Nt} s_{it}\hat{\Omega}_{it}$$

$$\hat{P}_{Jt} = \sum_{i=1}^{Nt} (\bar{s}_t + \Delta s_{it})(\hat{\Omega}_t + \Delta\hat{\Omega}_{it})$$

$$\hat{P}_{Jt} = \left(N_t \bar{s}\bar{\hat{\Omega}}_t\right) + \sum_{i=1}^{Nt} (\Delta s_{it}\Delta\hat{\Omega}_{it})$$

$$\hat{P}_{Jt} = \bar{\hat{P}}_{it} + \sum_{i=1}^{Nt} \left(\Delta s_{it}\Delta\hat{\Omega}_{it}\right) \tag{6}$$

A major issue of measuring productivity arises from the procyclicality of productivity due to capital and labor utilization swings. Economists have long been trying to explain the procyclicality of productivity and one of the explanations stems from a systematic mismeasurement in changes of capital and labor utilization (Basu 1996). A fall in output is captured when estimating productivity but a potential fall in capital and labor utilization is not captured to the same extent, causing the measured productivity to fall if output falls (Hulten 1986). This is in some sense a measurement error and does not reflect a fall in productivity or a loss of knowledge (Griliches 1990). A fall in measured productivity due to a low labor utilization rate is called the labor hoarding hypothesis, which occurs when a firm employs more labor than the minimum level required to produce a given good or service (Sbordone 1996; Aizcorbe 1992).

Choosing the Control Group

Careful consideration has been given to the selection of the control group in our study. The control group should be as similar as possible to the treatment group, but not affected by the treatment (Meyer 1995). We have used both qualitative and quantitative methods to find a suitable one, such as pre-treatment descriptive statistics and productivity trends.

Three Difference in Difference Approaches

The difference-in-difference (DID) estimator is a common instrument for evaluating the effects of a natural experiment (Wooldrigde 2009). Angrist and Pischke (2009) further states that the method is a best practice in estimating and measuring the effects of a policy at any given time. By using three DID estimators, capturing various effects of the *ROT-avdraget* policy, we hope to establish a comprehensive evaluation of this policy with regards to its potential effects on productivity.

The DID approach relies on data availability for the treatment and control group for the pre and post treatment period in order to be able to control for systematic differences between the two groups. The equation for estimating the DID coefficient is presented below:

$$y = \beta_0 + \delta d2 + \beta_1 dT + \delta_1 d2 * dT + other\ factors$$

where y is the dependent variable of interest, $d2$ is a dummy variable indicating the second period and dT is a dummy variable equaling unity for observations in the treatment group. Without other variables the DID estimator will be:

$$\hat{\delta}_1 = \left(\bar{y}_{2,T} - \bar{y}_{2,C}\right) - \left(\bar{y}_{1,T} - \bar{y}_{1,C}\right)$$

Where the bar denotes the average, the first subscript denotes the year, and the second subscript denotes the group.

By differencing the differences of the treatment and control group, one effectively isolates the treatment effect. The vital assumption necessary is that the differences between the control and treatment group remain constant over time, that is, that they react similarly to macroeconomic shocks and that there are no interactions affecting one of the groups exclusively other than the treatment effect on the treatment group (Meyer 1995).

We begin by estimating a DID-estimator using firm-level TFP as our dependent variable. The equation is as follows.

$$\widehat{\Omega}_{it} = \beta_0 + \delta Y10 + \beta TREATG + \delta Y10 * TREATG + u \tag{7}$$

where $\widehat{\Omega}_{it}$is the total factor productivity (TFP) at the firm-level in level form, $Y10$ is a dummy variable equaling unity if the year is 2010 and zero if the year is 2008 and $TREATG$ is a dummy variable equaling unity if the observation belongs to the treatment group and zero if it belongs in the control group. The DID estimator is presented in the equation below which follows the pattern described above.

$$\hat{\delta}_1 = \left(\bar{\bar{\Omega}}_{2010,T} - \bar{\bar{\Omega}}_{2010,C}\right) - \left(\bar{\bar{\Omega}}_{2008,T} - \bar{\bar{\Omega}}_{2008,C}\right)$$

where $\bar{\bar{\Omega}}$ corresponds to the average firm-level TFP.

In the next regression we introduce weights to the firm TFP, $\widehat{\Omega}_{it}$, observations. Each weight is based on the following equation:

$$w_i = Treat * \frac{Y_{i,2008}}{\sum_{i=1}^{n} Y_{i,2008,T}} * n_{2008,T} + Cont * \frac{Y_{i,2008}}{\sum_{i=1}^{n} Y_{i2008,C}} * n_{2008,C}$$

where w is the weight assigned to observation i in 2008 and 2010, $Treat$ is a dummy variable equaling unity when observation i belongs to the treatment group and zero otherwise, and $Cont$ is a dummy variable equaling unity when observation i belongs to the control group and zero otherwise. $Y_{i,2008,T}$ and $Y_{i,2008,C}$ represents real output for firm i in year 2008 for the treatment group, denotation T, and control group, denotation C, respectively. $n_{2008,T}$ and $n_{2008,C}$ represents the number of observations in year 2008 for the treatment and control group respectively. The weight equation for each group is basically the share equation Olley and Pakes advocated when decomposing productivity, but in order to use the weights in the DID-regression framework we multiply each weight with the corresponding total number of observations. This will result in the total number of observations staying unaffected, but each observation will have the same relative weight to the others as when calculating a weighted average TFP.

By doing this we allow firms with different sizes to influence average industry productivity in a proportional matter. At the same time, by holding the firm weight constant we will control for the market share of firms and isolate the productivity gain due to differences in firm sizes. We drop observations that are not present in both time periods in order to be able to use the weights of 2008 in 2010, thus creating a balanced panel. The resulting equation is:

$$w_i\widehat{\Omega}_{it} = \beta_0 + \delta Y10 + \beta TREATG + \delta Y10 * TREAG + u \tag{8}$$

The new DID estimator is given by the following equation:

$$\hat{\delta}_1 = (WTFP_{2010,T} - WTFP_{2010,C}) - (WTFP_{2008,T} - WTFP_{2008,C})$$

where $WTFP$ corresponds to a weighted average firm TFP using 2008 weights. As observations has been dropped to create a balanced panel, this DID estimator is not comparable with the first regression. In order to be able to conclude whether some productivity gain can be attributed to size, we perform the first regression (7) again on this balanced panel to use as a comparison group. A larger value in the second DID estimator than the comparison group would indicate that larger firms have gained more due to the policy.

When creating a balanced panel by dropping observations from firms that are not present in both years, a sampling bias is occurs. Firms with observations in only one of the two years have either entered or exited the market during the period of study, which may lead to a selection bias. Theoretical models developed by Jovanovic (1982) and Hopenhayn (1992) predict that the entry and exit patterns of firms are motivated by productivity differences at the firm level. In a more recent study by Fariñas and Ruano (2005) they find that a higher firm level productivity will lower the exit probability of the firm. This sampling bias will affect the results when calculating the mean TFP as well as the weighted average TFP. As we are only interested in establishing a proof for a heterogenous effect by comparing the DID-estimator of this regression with the one performed on the comparison group, we expect that this bias will be of minor importance. After all, both regressions are performed on the exact same sample and the selection bias will probably affect the calculations in a similar way, unless the size of the firms entering and exiting is also correlated with productivity. In this case, the comparison has to be interpreted with extra caution.

In our final regression we introduce different weights in 2010 and 2008 for the treatment and control group respectively. The weights are calculated as follows:

$$w_{it} = Treat * \frac{Y_{it}}{\sum_{i=1}^{Nt} Y_{it,T}} * n_{t,T} + Cont * \frac{Y_{it}}{\sum_{i=1}^{Nt} Y_{it,C}} * n_{t,C}$$

where w is the weight assigned to observation i in time t. $Treat$, $Cont$ and Y along with denotations are defined above. $n_{t,T}$ and $n_{t,C}$ represents the number of observations in time *t* for the treatment and control group respectively. The resulting equation is:

$$w_{it}\hat{\Omega}_{it} = \beta_0 + \delta Y10 + \beta TREATG + \delta Y10 * TREATG + u \qquad (9)$$

The last DID estimator is given by the following equation:

$$\hat{\delta}_1 = (\hat{P}_{J,2010,T} - \hat{P}_{J,2010,C}) - (\hat{P}_{J,2008,T} - \hat{P}_{J,2008,C})$$

where $\hat{P}_J$ in this equation is the weighted average productivity using weights corresponding to each time period and each group. This measurement captures all the proportionate effects of different firm sizes and market shares on industry TFP.

Threats to Internal Validity Using the Difference in Difference Approach

The difference in difference method completely relies on the fact that the control and treatment groups react in a similar way to other common time-varying shocks and events. If that is not the case, then the true effect might be over or understated and we will get a biased difference in difference estimator. There is of course also a possibility that one of the two groups has experienced another interaction at the time of the treatment, which is not controlled for, resulting in an understatement or overstatement of the true effect of the treatment. By choosing a control group in a related business sector, one minimizes the risk for these types of interactions (Meyer 1995).

Testing for Robustness

To test our main results for robustness we perform a fourth DID regression using a new control group. This group will consist of firms from another Nordic country operating in the same two-digit NACE code, 43, as our treatment group. This is done to reduce the importance of biases or random variation in a single control group (Meyer 1995).

As there is a possibility that our difference in difference regressions might suffer from heteroskedasticity and serial correlation we plan to include fully robust standard errors that are heteroskedasticity and autocorrelation consistent in our four DID equations in order to be able to determine how sensitive our conclusions are to the standard errors in use. This is done using the "cluster" command in STATA with the panel identifier, in our case the firm name, as the cluster variable. The issue of the standard errors is of extra importance if we have t-statistics around 2, as not correcting for the potential problem may lead us to falsely reject our null hypothesis at the 5% significant level. Bertrand et al. (2004) state that not controlling for serial correlation could result in overestimations of the t-statistics due to underestimation of the standard errors. The severity of the underestimation depends on the length of the time series used and the serial correlation of the most commonly used dependent variable. Moreover, if the assumption of homoscedasticity does not hold, the standard errors could be larger or smaller than if robust standard errors were calculated (White 1980).

6 Data

Underlying Data for Treatment Group and Main Control Group

The method of estimating the production functions explained in the previous section is applied to firm-level data from the Swedish database Retriever; a Swedish comprehensive database containing detailed firm level financial statement data. The data are a sample of Swedish firms operating within the two-digit NACE industry code 43, "Specialized construction activities". The data set includes approximately 153,428 observations during the period from 2001 to 2010, of which 123,269 and 30,159 observations belong to the treatment group and control group respectively. Subsectors that are not affected by the subsidy or are not belonging to the control group are excluded from the data set.[1]

To catch the effects of entry and exit dynamics on industry productivity development, an unbalanced data set is desirable. Also, when using the Olley and Pakes method unbalanced data are needed to construct the dichotomous variable, which takes the value unity for each observation if the firm exits in the current year. In some sense, the data set includes the entry and exit dynamics. Firms that have exited the market but still not liquidated the company, meaning that they still have a company code, are included in the data set. However, data for liquidated firms without a company code is not available and our data are thus not fully unbalanced. Using fully unbalanced data is preferred when estimating productivity in order to avoid the problem of a selection bias (Van Beveren 2012).

The main variables necessary to estimate the production function with the Levinsohn and Petrin method include output, labor, materials and capital. Estimating the production function parameters with the Olley and Pakes method requires an additional variable, investment, which serves as a proxy to control for unobserved productivity shocks. Real firm output is proxied by deflating firm level sales to year 2000 price levels by the *"Entreprenad index E84"*, a price index for the Swedish construction industry produced by Statistics Sweden in cooperation with The Swedish Construction Federation. This index is also used when deflating materials, capital and investment. A sector specific producer price index for NACE code 43 would be desirable but is unfortunately not available, which is why the more general index described above, is used instead. Regarding the labour input variable, Olley and Pakes (1996) as well as Levinsohn Petrin (2003) advocate the use of actual hours worked as the proper measure. However, such detailed data is not available. Therefore, the average number of full-time equivalent employees is used as

[1] The following five digit NACE subsectors are included for the treatment group: 43210, 43221, 43222, 43223, 43229, 43290, 43310, 43320, 43330, 43341, 43342, 43911 and 43912. The control group consists of only one five digit NACE subsector, 43120.

a proxy. The materials variable is constructed by adding materials, consumables and traded goods from the financial statement together and subsequently deflated to year 2000 values. The capital variable is proxied by deflating the book value of fixed material assets. To obtain the investment variable, the following calculation based on financial statement figures has been done:

$$i_{j,t} = K_{j,t} - K_{j,t-1} + \delta_{j,t}$$

where $i_{j,t}$ represents investments by firm *j* in year *t*, $K_{j,t}$ is the book value of material fixed assets and $\delta_{j,t}$ represents depreciation.

An important caveat with the data set emerges due to the large amount of data and the absence of an effective method to distinguish between business-to-consumer and business-to-business firms. As a result of this, the data set contains firms focusing on businesses even though they are not supposed to be included our study. This is the case for the treatment group as well as the control group. It is hard to understand the magnitude of this caveat or to quantify the effects of it on our results, but is nevertheless important to acknowledge and keep in mind when interpreting our results. We will further discuss the implications of this caveat in the final discussion.

Definitions, descriptions and sources on the data set collected for estimating the production function as well as our regression analysis are presented in Table I. Descriptive statistics of the main variables is presented below in Table II and yearly descriptive statistics of the same variables can be found in Appendix A, Table A1.

TABLE I: DEFINITIONS OF EXPLANATORY VARIABLES

Variable	Variable name	Definition	Source
Y	Real output	Deflated net sales (SEK x1,000)	Retriever / Orbis
L	Employment	Number of employees (full-time equivalent employees)	Retriever / Orbis
M	Real materials	Deflated materials, consumables and traded goods (SEK x1,000)	Retriever / Orbis
K	Real capital	Deflated total material fixed assets (SEK x1,000)	Retriever / Orbis
I	Real (pos.) investment	Deflated values of calculations based on firm-level capital and depreciation (SEK x1,000)	Retriever
EXIT	Exit current year	Dichotomous variable taking the value unity if the firm exits the current year	-
Y10	Year 2010	Dichotomous variable taking the value unity if current year equals 2010, zero otherwise	-
TREATG	Treatment group	Dichotomous variable taking the value unity for observations belonging to the treatment group, zero otherwise	-

TABLE II: DESCRIPTIVE STATISTICS - MAIN EPLANATORY VARIABLES

		Treatment group						Control group						Mean Difference
Variable name	Year	# Obs.	Mean	% of real output	S.D.	Min	Max	# Obs.	Mean	% of real output	S.D.	Min	Max	(%)
Real output (SEK x1,000)	2001-2010	123,269	6,705	*N.M.*	57,651	0	4,085,992	30,159	5,097	*N.M.*	14,416	0	892,823	31.57
Employment (FTE)	2001-2010	123,269	7.48	*N.M.*	58.24	0	4,644	30,159	4.72	*N.M.*	10.35	0	477	58.42
Real materials (SEK x1,000)	2001-2010	123,269	2,276	34%	15,097	0	1,867,547	30,159	1,593	31%	7,359	0	382,018	42.83
Real capital (SEK x1,000)	2001-2010	123,269	423	6%	3,533	0	386,963	30,159	1,807	35%	4,461	0	170,033	-76.59

Notes: The time period 2001-2010 includes both pre-treatment observations, 2001-2008, and post-treatment, 2009-2010. Real values are in price levels of year 2000 and are obtained by deflating monetary values with *"Entreprenad index E84"* collected from Statistics Sweden. Output is defined as the turnover of the firm. Employment is measured in full-time equivalent employees (FTE). The materials variable includes raw materials, consumables and traded goods. Capital is defined as the book value of fixed material assets.

Underlying Data for Robustness Test

As mentioned in the description of the method above, to strengthen the internal validity of our results we conduct a robustness test by using a second control group. This will consist of Finnish, Danish or Norwegian firms from the exact same industry as the treatment group, selected subsectors from NACE industry code 43.[2] We will now present the underlying data for this second control group. Firm-level data have been collected from the Bureau Van Dijk Orbis database, a global database containing public and private company information. The data include 41,301, 54,450 and 56,682 observations for Finland, Denmark and Norway respectively during the period 2002 to 2010. Detailed descriptive statistics is presented below in Table III, IV and V. As one notices when comparing data from Orbis in Table III, IV and V with the data from Retriever in Table II, the number of observations of each variable of the data collected from Orbis differs from each other, which is not the case for the data collected from Retiever. The explanation is simply that the data collected from Retriever is more comprehensive than the data from Orbis.

Real output is proxied by the operating turnover of the firms and subsequently deflated to year 2000 price levels by country specific Eurostat Construction Cost Index. Employment input is measured in full-time equivalent employees. To develop the real materials variable, income statement data on cost of materials is used, which is deflated by a country specific Eurostat price index of input prices for materials in the construction sector. Balance sheet data on material fixed assets deflated by the country specific Eurostat Construction Cost Index serves as a proxy for real capital input. Real output, materials and capital is also converted into Swedish Kronor (SEK). Since the real values of the variables have a base year at 2000, average exchange rates for Euro (EUR), Danish Kroner (DKK) and Norwegian Kronor (NOK) for year 2000 is used. The exchange rate for EUR/SEK, 8.4459, is collected from Eurostat, DKK/SEK, 1.1331, is collected from Danmarks Nationalbank and NOK/SEK, 1.0412, is collected from Norges Bank.

Existing caveats and limitations with this data set is essentially similar to the ones present in the main data set from Retriever with the important exception that the data from Orbis are balanced.

[2] The following NACE four-digit subsectors are included: 4321, 4322, 4329, 4331, 4332, 4333, 4334 and 4391.

TABLE III: DESCRIPTIVE STATISTICS - CONTROL GROUP FINLAND

		Control group Finland					
Variable name	Year	# Obs.	Mean	% of real output	S.D.	Min	Max
Real output (SEK x1,000)	2002-2010	30,623	7,162	*N.M.*	51,601	0	2,983,729
Employment (FTE)	2002-2010	20,939	9.99	*N.M.*	61.79	1	3,516
Real materials (SEK x1,000)	2002-2010	29,368	2,905	41%	20,189	0	1,392,169
Real capital (SEK x1,000)	2002-2010	30,417	532	7%	2,026	0	148,843

Notes: Employment is measured in full-time equivalent employees (FTE). Real values are in price levels of year 2000. Values in EUR has been converted into SEK with an EUR/SEK exchange rate of 8.44593, which is the average exchange rate for year 2000.

TABLE IV: DESCRIPTIVE STATISTICS - CONTROL GROUP DENMARK

		Control group Denmark					
Variable name	Year	# Obs.	Mean	% of real output	S.D.	Min	Max
Real output (SEK x1,000)	2002-2010	4,039	16,920	*N.M.*	87,577	0	1,446,651
Employment (FTE)	2002-2010	18,014	12.50	*N.M.*	50.02	0	1,707
Real materials (SEK x1,000)	2002-2010	3,904	11,820	70%	59,436	0	1,114,873
Real capital (SEK x1,000)	2002-2010	24,516	1,057	15%	6,296	0	405,705

Notes: Employment is measured in full-time equivalent employees (FTE). Real values are in price levels of year 2000. Values in DKK has been converted into SEK with an DKK/SEK exchange rate of 1.1331, which is the average exchange rate for year 2000.

TABLE V: DESCRIPTIVE STATISTICS - CONTROL GROUP NORWAY

		Control group Norway					
Variable name	Year	# Obs.	Mean	% of real output	S.D.	Min	Max
Real output (SEK x1,000)	2002-2010	40,813	8,570	*N.M.*	43,818	0	3,004,021
Employment (FTE)	2002-2010	15,418	9.20	*N.M.*	48.83	0	3,531
Real materials (SEK x1,000)	2002-2010	38,450	4,021	24%	17,914	0	1,148,832
Real capital (SEK x1,000)	2002-2010	41,188	423	6%	2,266	0	378,108

Notes: Employment is measured in full-time equivalent employees (FTE). Real values are in price levels of year 2000. Values in NOK has been converted into SEK with an NOK/SEK exchange rate of 1.0412, which is the average exchange rate for year 2000.

7 Analysis

Selecting the Control Group

The first step towards answering our hypothesis by conducting a difference in difference regression is to determine a suitable control group. As stated above we base our selection on both qualitative and quantitative criteria. Fortunately, we can identify a somewhat natural control group for our difference in difference approach. Our chosen control group operates in an adjacent business-sector to the treatment group, which is within the same two-digit NACE industry classification, 43. The sub-sector in which the firms of the treatment group operate in is categorized as “site preparation” with the four-digit NACE classification 4312. It includes firms offering site preparation services to consumers. Examples of these services are modifying gardens, constructing driveways and most importantly laying foundations of new houses. The sub-sector also includes firms that serve businesses and governments in the construction of infrastructure. However, as described in the data section, we try to adjust this by excluding firms with secondary NACE classifications relating to services regarding infrastructure construction. Site preparation services for consumers has actually been listed by the Swedish government as an exception to the policy in order to avoid confusion regarding whether it is included or not, due to its proximity to the services deductible in accordance with the policy. It also shares similar characteristics with regards to average firm size, in terms of real sales and employment, and average real materials used. As can be seen in Table II in the data section, the average firm real sales in over the entire period from 2001 to 2010 in the treatment group, 6.705 million SEK, is approximately 32 percent larger than the corresponding figure for the control group, 5.097 million SEK. The difference in average employment during the same period is approximately 58 percent, the treatment group average, 7.48, being larger than the control group average, 4.72. Regarding real materials used, the control group and the treatment group are quite similar in relative terms of sales with the former using 31% in terms of sales and the latter 34%.

There is however a rather large discrepancy between the two groups in terms of average real capital employed. The capital employed in terms of real sales in 2008 for the control group is 35% compared 6% for the treatment group.

We further intend to examine the industry TFP trends of both groups during the period 2001 to 2008 to understand if their industry productivity develop in a similar way over time and thus further reinforce the selection of our control group as an appropriate one. Therefore, we proceed by estimating the production function parameters to measure firm level productivity and consequently aggregated industry productivity.

Estimating Firm and Industry Productivity

To show how the Olley and Pakes, OLS and Levinsohn Petrin methods for estimating the production function parameters differ we present the outcome of the production function parameters for the treatment group in Table VI below.

TABLE VI: COMPARISON OF PRODUCTION FUNCTION ESTIMATION METHODS

			Employment		Materials		Capital	
Method	# Obs.	# Firms	β_l	*SE*	β_m	*SE*	β_k	*SE*
OLS	93,855	17,230	.543***	.001	.437***	.001	.038***	.001
Olley-Pakes	61,218	14,028	.538***	.005	.424***	.006	.031***	.003
Levinsohn-Petrin	93,855	17,230	.489***	.004	.436***	.033	.050***	.007

Notes: Values are coefficients. * inducates that the estimate is significant on a 10% level, ** indicates that the estimate is significant on a 5% level, *** indicates that the estimate is significant on a 1% level.

According to theory, the simultaneity and the selection bias cause the capital coefficient to be downward biased, while the materials and labor coefficients are expected to show an upward bias (Van Beveren 2012). The Levinsohn-petrin and Olley-Pakes methods are used to mitigate these biases and one would therefore expect the coefficients on labor and material to be lower and the capital coefficient higher using these. The results with the Levinsohn Petrin model is in line with expectations, but estimating the production function with the Olley and Pakes method yields a lower capital coefficient than using OLS. A potential explanation to this unexpected result may be that due to the significant loss of observations when using the Olley and Pakes method, 32,637, the monotonicity condition is less likely to hold, leading to inconsistent estimators. When trying to estimate the control group production function in STATA, the Olley and Pakes method was not feasible. We suspect again that this problem arises due to the lack of positive investment observations in the data. As the Olley and Pakes method is likely to suffer from bias in the estimated coefficients in the production function of the treatment group and is not applicable to the control group, the Levinsohn Petrin method is deemed a better alternative.

The result using the Levinsohn-Petrin method to estimate the production function parameters for the control group and the treatment group are presented in Table VII. A few interesting observations can be made from Table VII. The employment parameters of the treatment and control group, .489 and .550, are similar in magnitude. However, the parameter estimates of materials, .436 and .282, is more than one and a half times as high for the treatment group compared to the control group. The capital parameter of the treatment group, .05, differs substantially from the corresponding parameter for the control group, .165. It should also be noted that the number of observations of the control group is considerably lower than that of the treatment group. This may lead to more accurate parameter estimates for the treatment group compared to the control group. In this case, the numbers of observations for both groups are more than enough to get highly significant estimates of the parameters in the production functions.

TABLE VII: PRODUCTION FUNCTION PARAMETER ESTIMATES

			Employment		Materials		Capital	
Group	# Obs.	# Firms	β_l	*SE*	β_m	*SE*	β_k	*SE*
Treatment group	93,855	17,230	.489***	.004	.436***	.033	.050***	.007
Control group	17,068	4,250	.550***	.134	.282***	.054	.165***	.022

Notes: Values are coefficients. * inducates that the estimate is significant on a 10% level, ** indicates that the estimate is significant on a 5% level, *** indicates that the estimate is significant on a 1% level.

Given the estimated production function with the Levinsohn-Petrin method we can obtain estimates of firm-level productivity by applying (3) and (4) to the sample. From the firm-level productivity it is possible to calculate the industry TFP using (5). Average firm-level TFP can be extracted from (6). Table VIII presents average firm-level TFP, industry TFP and normalized values of the latter. Figure II, III, and IV presents the same results graphically.

TABLE VIII: OBTAINED TOTAL FACTOR PRODUCTIVITY

	Treatment group			Control group			
Year	Average firm-level TFP	Industry TFP	Normalized industry TFP	Average firm-level TFP	Industry TFP	Normalized industry TFP	Absolute difference
2001	61.898	69.387	1.000	91.299	113.784	1.000	44.396
2002	61.989	69.439	1.001	89.549	109.635	0.964	40.196
2003	61.525	69.111	0.996	88.741	118.014	1.037	48.903
2004	61.256	69.609	1.003	86.933	114.905	1.010	45.296
2005	61.093	69.727	1.005	86.961	111.383	0.979	41.656
2006	61.193	70.240	1.012	89.427	117.093	1.029	46.853
2007	61.691	71.094	1.025	90.723	121.096	1.064	50.002
2008	61.498	71.085	1.024	87.853	123.079	1.082	51.994
2009	61.134	70.477	1.016	84.086	110.188	0.968	39.711
2010	60.574	69.656	1.004	83.678	110.286	0.969	40.630
Average	61.385	69.982	1.009	87.925	114.946	1.010	44.964

Notes: Average firm-level TFP is the arithmetic mean of firm level TFP.

FIGURE II: AVERAGE FIRM-LEVEL TFP

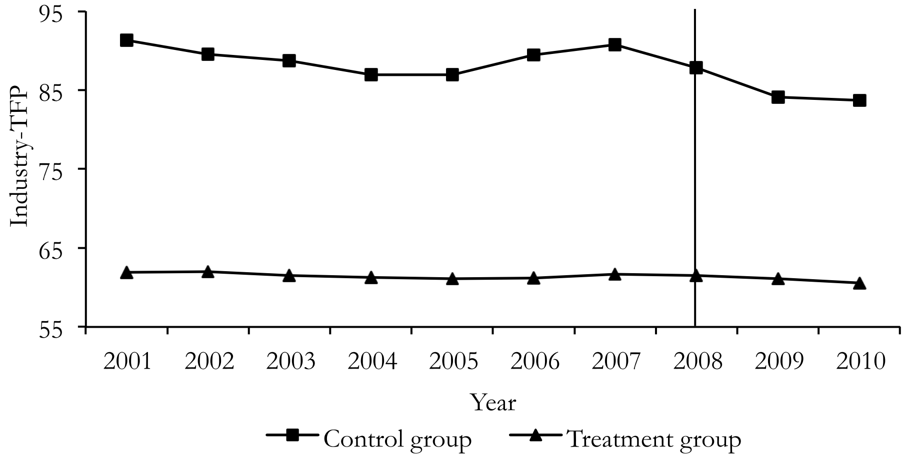


FIGURE III: INDUSTRY-TFP

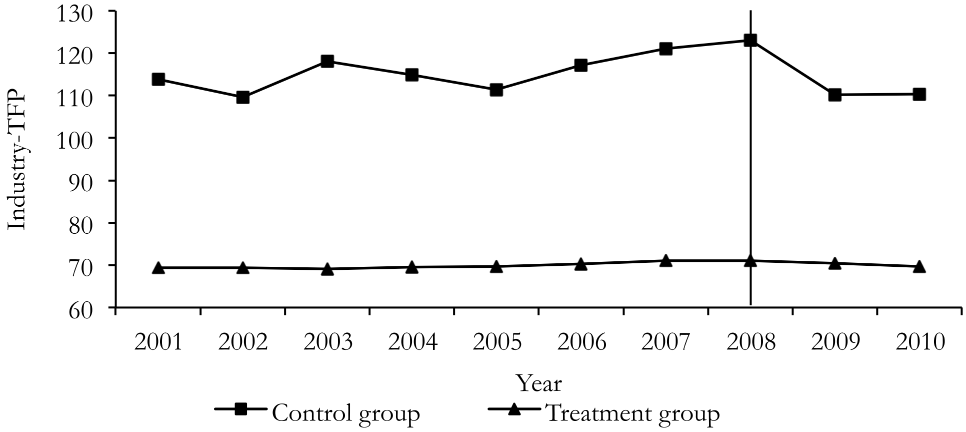
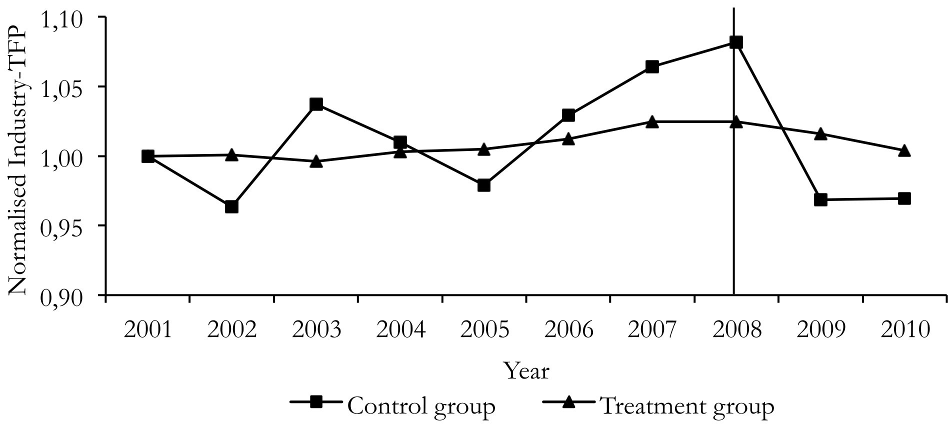


FIGURE IV: NORMALIZED INDUSTRY-TFP

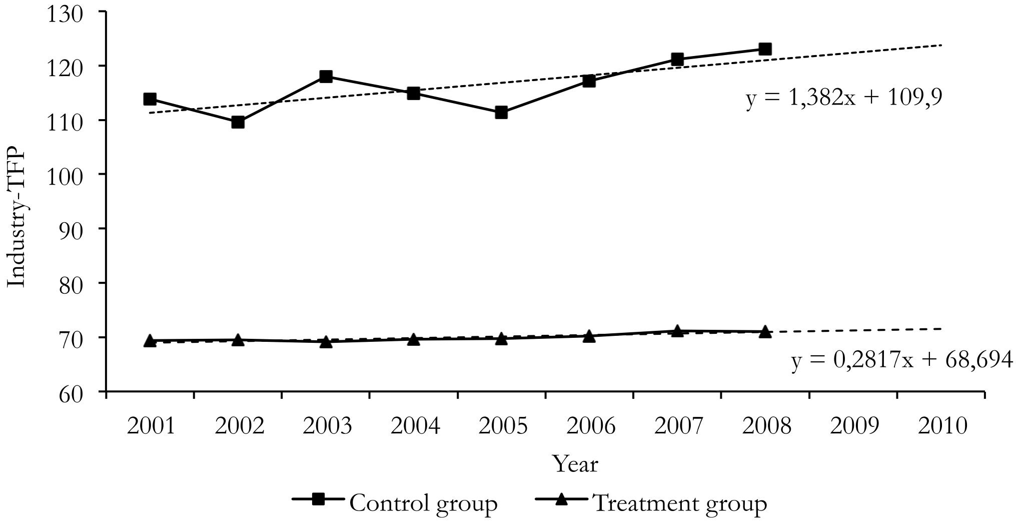


Notes: The figures II, III, IV present average firm-level TFP, industry-TFP and normalized values of the latter respectively for the treatment and control group over the period 2001 to 2010. The pre-treatment period, 2001 to 2008, is separated by a horizontal line from the post-treatment period, 2009 to 2010.

Comparing the results in Table VIII and the corresponding Figures II, III and IV a few interesting conclusions can be drawn. The average firm-level TFP is lower than the industry-TFP for both the control group and the treatment group. In the control group the average firm-level TFP is even declining while at the same time industry-TFP in the group up until 2008 is rising. This can be explained by the fact that the larger firms are more productive than the average firm and over time they, together with other above-average-TFP firms, gain market share, at the expense of less productive firms, and/or experience higher growth in productivity than the average firm. This effect is known as the reallocation effect. Looking at the graph of normalized industry-TFP one will notice that there is a positive accumulated growth in TFP for both groups up until 2008. After 2008, there is a dramatic drop in industry-TFP for the control group, but there is only a slight decline in the treatment group. Our hypothesis is that the introduction of the policy *ROT-avdraget* may play a part in this evolvement and this is tested using a difference in difference approach.

As stated earlier, the inclusion of a suitable control group in the DID estimator is key in order to try to ensure internal validity. We present another quantitative determinant for choosing this group illustrated in Figure V below. As can be seen, the control group also shows a rising trend in weighted average industry TFP up until 2008, which is right before the implementation of *ROT-avdraget*. It is steeper than the treatment group trend and the yearly observations show more variation. This variation may be due to the fact that the control group has fewer observations than the treatment group. Although the trend is somewhat different in absolute slope value we still believe that this could be a suitable control group following the arguments presented earlier and considering that both groups have experienced a positive trend in industry TFP. The differential trends and differences in yearly variation around the trends do however present a few discussion topics regarding the interpretation of the magnitude of the DID estimators.

FIGURE V: PRE-TREATMENT INDUSTRY-TFP



Notes: The figure presents the estimated industry-TFP over the period 2001 to 2010. The dashed lines represent estimated trends for each group and the corresponding trend function is presented below each line.

Difference in Difference Regression Results

Our results from the DID regressions are presented in Table IX. The first DID regression (7) estimates the effect on average firm-level TFP due to the implementation of *ROT-avdraget*. With a t-statistic of 2.52 and a p-value of .012 this is a highly, statistically significant result. The interpretation is that firms in the treatment group have gained a TFP of 3.25 on average due to the policy.

The next regression (8) introduces the 2008 weights to the estimation equation. The estimated average effect of *ROT-avdraget* on average weighted firm level TFP calculated using 2008 weights, WTFP, is 3.54 and has a p-value of 0.014. A higher result than in our first regression would indicate that not only has the policy change induced an increase in TFP on the treatment group, but also firms with a larger market share have gained proportionally more than the average firm. Since we have dropped observations to create a balanced panel in order to use the 2008 weights, we have to do the first regression again on this balanced panel to get comparable results. Our point estimate of 3.54 in (8) compared to 2.15 in (8.1) using the new balanced panel supports this heterogeneous effect benefitting the larger firms. The confidence intervals are however overlapping, implying that it is not possible to verify this effect statistically. It should also be noted that the p-values for the two regressions differ substantially with 0.014 in (8) compared to 0.071 in (8.1). The DID estimator of 3.54 should be interpreted with extra caution as the regression has been conducted using a balanced sample, created by dropping observations of firms that have either exited or entered the market during the period from 2008 to 2010. All the firms which operate in only one of the two years has been dropped in order to be able to use the 2008 weights in 2010. As decisions regarding whether to exit or enter a market are not random and are likely correlated to the productivity of firms, creating a balanced panel dropping these observations may lead to biased results. The estimator is calculated for the sole purpose of comparing the heterogeneous effect with the average firm level effect and should be limited to discussions around this.

TABLE IX: DIFFERENCE IN DIFFERENCE REGRESSIONS

(STANDARD ERRORS IN PARENTHESES AND P-VALUES IN BRACKETS)

Model	(7) D-i-D unweighted tfp	(8) D-i-D weighted tfp (2008)	(8.1) D-i-D unweighted tfp (2008)	(9) D-i-D weighted tfp (2008 & 2010)
Y10	-4.176***	-5.933***	-2.814***	-12.792***
	(1.181)	(1.325)	(1.096)	(1.731)
	[.000]	[.000]	[.010]	[.000]
TREATG	-26.355***	-42.453***	-24.306***	-51.994***
	(.942)	(1.020)	(.844)	(1.380)
	[.000]	[.000]	[.000]	[.000]
Y10 * TREATG	3.252**	3.546**	2.154*	11.363***
	(1.290)	(1.443)	(1.193)	(1.891)
	[.012]	[.014]	[.071]	[.000]
Cons.	87.853***	112.733***	85.242***	123.079***
	(.865)	(.937)	(.775)	(1.267)
	[.000]	[.000]	[.000]	[.000]
R^2	.053	.127	.065	.083
Adjusted-R^2	.052	.127	.064	.083
# Obs.	26,612	22,116	22,116	26,612

Notes: * inducates that the estimate is significant on a 10% level, ** indicates that the estimate is significant on a 5% level, *** indicates that the estimate is significant on a 1% level.

The third regression (9) is using the same weights as used when calculating industry-TFP for the control group and treatment group in 2008 and 2010 respectively. Running this regression we get a very high estimated average effect, an increase in industry-TFP of 11.36, due to *ROT-avdraget*. The t-statistic of 6.01 with a p-value of 0.000 renders this effect statistically significant at any reasonable significance level. Comparing the point estimate to the results from (8) and (7) we can conclude that the largest and most productive firms have also gained market share. In this case there is no overlapping of the confidence intervals, which implies that this effect is statistically proven.

Our three DID estimations strongly support our hypothesis that *ROT-avdraget* has had a positive effect on productivity in the firms affected. They also provide a view of how this change is generated, that is, by increasing average firm-level TFP and by increasing the TFP proportionally more on the larger and more productive firms. This second effect is however not statistically proven. The fourth estimation shows that the more productive firms also gain market share, enhancing industry-TFP even more.

The analysis rests on the assumption that besides the treatment the control group and treatment group does not experience any other group-specific shock and that they react similarly to macroeconomic shocks. In 2008 a huge macroeconomic shock, known as the financial crisis, struck all industries. If the two groups have reacted differently to this our result will be biased. Looking at the trend before the treatment, one notices that the control group trend is steeper. If we would expect this trend to continue in the absence of the financial crisis, then we have an understatement of our DID estimation. If we were to expect our control group to react stronger to the financial crisis, then we would have another effect moving in the opposite direction of the first one. We can only speculate on how these opposite effects would affect our results. We can however conclude that the second effect would have to be large in magnitude in order to render our third and final DID estimator negative or statistically insignificant at any reasonable level.

Another potential caveat of the analysis is the fact that the data include firms selling services to businesses rather than consumers. These firms are not affected by *ROT-avdraget* and should consequently be excluded from the data. As pointed out in section 4, data, they are included because we lack an efficient method to distinguish between business-to-business and business-to-consumer firms. Since we are not able to isolate the firms affected by the subsidy, we suspect that we are understating the true effect.

The potential mismeasurement errors relating to the procyclical property of productivity may also be of importance in our analysis. As concluded before and as can be seen in Table 2, the relative capital intensity of our control group is considerable larger than that of our treatment group, which, according to theories presented, may be an explanation to why the pre-treatment productivity swings are larger in our control group. Resting on the same theories, this would also imply that the measured fall in productivity of our control group post-treatment is not only capturing a fall in productivity but also a lower capital and labor utilization rate due to the financial crisis and its negative consequences for aggregate demand. This could imply that the increased demand created by *ROT-avdraget* is merely resulting in a higher utilization rate of capital and labor for the treatment group compared to the control group rather than an actual productivity increase or resilience.

Since there are a few caveats to our analysis the results presented should be interpreted not as exact numbers or intervals, but rather as causal indicators of the positive effect a consumer subsidy has on productivity.

Testing for Robustness

As our results are sensitive to the choice of an appropriate control group, we use a second one in order to limit the potential biases and random variation from just using one group. Data was retrieved for firms in all the Scandinavian countries, but only the companies in Finland had provided sufficient data for our analysis. The fourth DID regression will be performed in an exactly similar way as regression (9) with the important exception that instead of using Swedish site preparation companies as a control group, Finnish companies operating in the same industry as the treatment group will be used.

The potential problems by using firms operating in Finland as a control group are of similar character as when using site preparation companies in Sweden. It is hard to tell whether the companies in the control group react similarly to macroeconomic shocks. Due to the fact that the groups belong to different legislations, there is also a larger risk that the groups are subject to changing country-specific conditions that occur simultaneously with the treatment. As far as we are concerned the companies in Finland have not been subject to any major environmental changes specific to them in the years during the treatment. Moreover, as the economies of Sweden and Finland are at a similar stage of development we do not expect the conditions that firms face in their respective markets to differ substantially. We have also found evidence that the financial crises affected the economies in a very similar way, which is presented in Figure VI. For example, real GDP between the first quarter 2008 and the first quarter 2009 fell 6.3 percentage points in Sweden compared to 6.0 percentage points in Finland. Another issue with using Finland as a control group is that it has in fact received treatment. In 1997 the Finnish legislative body implemented a similar policy to the one introduced by the Swedish parliament in 2009.[3] We do however expect that the effects of this treatment has been fully exhausted by the Finnish companies, enabling us to use this control group in spite of this fact.

The production function estimates for the new control group is presented together with the function for the treatment group in Table X below. The estimated coefficients of the production function parameters of the two groups are very similar. The industry TFP in Finland is however on a significantly lower level than in Sweden for the entire period as can be seen in Table XI and in Figure VI. This raises some concern as the Swedish and Finnish economies are at a similar development stage and we would not expect the productivities in corresponding business sectors to differ that much. As the productivity in the Finnish companies is estimated using data from the Orbis database instead of Retriever from where we collected the data for the treatment group, the difference might actually stem from the use of different data sources.

FIGURE VI: DIFFERENCE IN GDP, UNEMPLOYMENT AND YOUTH UNEMPLOYMENT - SCANDINAVIAN COUNTRIES

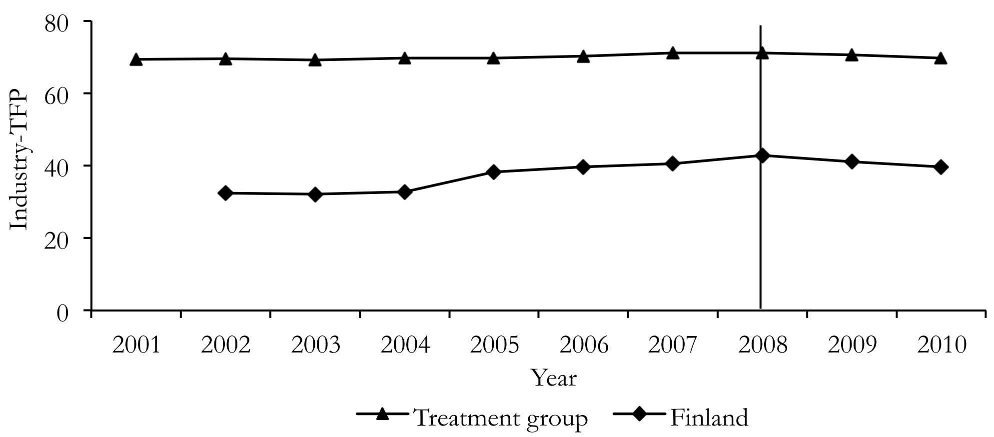
Notes: The figures represent GDP percentage change, unemployment and youth unemployment percentage point changes between first quarter 2008 and first quarter 2009. GDP development is calculated in real prices in the local currency of each country. Data have been collected from Eurostat.

TABLE X: PRODUCTION FUNCTION PARAMETER ESTIMATES - CONTROL GROUP FINLAND

			Employment		Materials		Capital	
Group	# Obs.	# Firms	β_l	*SE*	β_m	*SE*	β_k	*SE*
Treatment group	93,855	17,230	.489***	.004	.436***	.033	.050***	.007
Control group Finland	19,449	4,589	.499***	.009	.499***	.117	.054***	.019

Notes: Values are coefficients. * indicates that the estimate is significant on a 10% level, ** indicates that the estimate is significant on a 5% level, *** indicates that the estimate is significant on a 1% level.

FIGURE VI: INDUSTRY-TFP - CONTROL GROUP FINLAND



Notes: The figure presents industry-TFP for the treatment group and the control group with Finnish firms over the period from 2001 to 2010.

The result from our fourth DID regression is presented in table XI. The DID estimator is substantially lower in this case, 1.736, compared to our third regression, 11.363, when we used site preparation companies as the control group. We can almost reject the null hypothesis that the estimator is different from zero at the 10% significant level as the p-value is 0.107.

TABLE XI: DIFFERENCE IN DIFFERENCE REGRESSION - CONTROL GROUP FINLAND

(STANDARD ERRORS IN PARENTHESES AND P-VALUES IN BRACKETS)

Model	D-i-D weighted tfp (2008 & 2010)
Y10	-3.165***
	(.993)
	[.001]
TREATG	-28.296***
	(.778)
	[.000]
Y10 * TREATG	1.736
	(1.078)
	[.107]
Cons.	42.789***
	(.717)
	[.000]
R^2	.102
Adjusted-R^2	.102
# Obs.	26,235

Notes: * indicates that the estimate is significant on a 10% level, ** indicates that the estimate is significant on a 5% level, *** indicates that the estimate is significant on a 1% level.

The robustness test using another control group does create some concern regarding the validity of our other DID regressions. It should be noted however that the new control group data is from another source, which might cause problems when comparing the two different groups, as the measurement techniques in obtaining the data may differ. Perhaps that could be the explanation to why companies in Finland have a lower productivity through the entire period.

Even though we get a DID estimator smaller in magnitude and less statistically significant than our third DID estimator, we still obtain a positive result, which lends some support to the conclusion that there is an effect of the subsidy.

Following the recommendations of Bertrand et al. (2004) we also correct our standard errors. As the "cluster" command is not compatible with the "iweight" command used to assign weights to our observations, we only perform this correction on our first regression. The effect on the standard errors is presented in Table XII. The previously calculated standard errors were understated due to the presence of serial correlation in the dependent variable and/or heteroskedasticity. Looking at the parameter of interest, the DID-estimator, we see that the unadjusted standard error with a value of 1.290 is 18% lower than the corresponding robust standard error with a value of 1.576. Provided that this result is applicable to the other regressions as well, we will expect the p-values to rise in every single regression. This may cause problems in the comparison group in the second regression as it may not even be possible to reject the null hypothesis at the 10% significant level if the standard errors increase. The rejection or non-rejection of the other DID estimators are likely not affected by this increase in standard errors.

TABLE XII: STANDARD ERRORS COMPARISON

Model	Unadjusted standard errors	Robust standard errors
Y10	1.180	1.532
TREATG	.942	1.504
Y10 * TREATG	1.290	1.576
Cons.	.865	1.468

Notes: Figures above are derived from the first DID regression (7). Robust standard errors are created by clustering using the panel identifier as the cluster variable.

8 Conclusion

The results from our DID estimations suggests that there is a rather large positive effect on productivity related to *ROT-avdraget*. We find that the effect is divided into two statistically proven parts: the average is firm becoming more productive, and the more productive firms are gaining market share due to the policy implementation. Our point estimates also suggest that the larger firms become more productive than the average firm. This effect is however not statistically proven due to overlapping of confidence intervals.

The results are in line with the previous research on demand-pull effects on productivity. They indicate that an ad valorem consumer subsidy stimulating demand in a single industry causes an increase in productivity within that industry. The results contribute to existing research by providing evidence to how this increase is generated using firm-level data to support the conclusions. Our third DID-estimator strongly implies that the demand-pull effects are stronger than the effect of the opportunity cost theory presented as an argument against our hypothesis.

The robustness test using Finnish firms in the control group yields a small positive DID estimator with a value of 1.736, which is not statistically significant. The correction of the standard errors lowers the p-value of all DID estimators but the effect does not change the decision whether to reject or not, except for a possible change in the comparison group in our second regression.

The results from the third regression, a point estimate of 11.36 increase in TFP due to the policy, can be related to the level of productivity that had prevailed, had the policy change not been implemented. *ROT-avdraget* has increased productivity by 19% in the sectors affected, which has increased the values of services provided given the inputs used by around 24 Billion SEK for the year of 2010. This figure can be related to the costs associated with the policy in 2010, which amounted to 13.5 billion SEK. The underlying calculations can be found in Appendix B. Provided that the third DID estimator is a reasonable estimate of the productivity gain, the result may act as another very strong argument for the introduction of this policy.

The problem with mismeasurement due to the procyclictal properties of productivity remains even after testing for robustness. There are also possible biases present when estimating the production function and the DID estimation relies heavily on that the control group reacts similarly, which is very hard to prove. The magnitude of our third DID estimation is however very large, indicating that there is a positive effect. One probably should not rely completely on the magnitude of the estimated DID-results, but should merely recognize that there is a causal positive effect on productivity due to the implementation of the policy.

The implication for this causal link stretches far beyond the effects of consumer subsidies. All economic policies that stimulate a higher demand, will in fact also drive productivity growth. It indicates that the productivity aspect should be evaluated when implementing all types of demand-increasing policies.

It would be interesting to see a study that corrects for the biases in the production function and include several control groups in order to strengthen the probability of obtaining more reliable results. It would also be interesting to mimic the study on several business sectors in manufacturing and service industries in order to conclude if the effects through which productivity increases are sector specific or if they follow a general pattern.

9 References

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Appendix A

TABLE A.1: DESCRIPTIVE STATISTICS - MAIN EPLANATORY VARIABLES (2010-2001)

		Treatment group						Control group						Mean
Variable name	Year	# Obs.	Mean	% of real output	S.D.	Min	Max	# Obs.	Mean	% of real output	S.D.	Min	Max	Difference (%)
Real output (SEK x1,000)	2010	16 074	5 950	*N.M.*	52 915	0	3 624 168	3 986	5 331	*N.M.*	20 175	0	892 823	11,61
Employment (FTE)	2010	16 074	7,09	*N.M.*	58,93	0	4 458	3 986	5,06	*N.M.*	12,77	0	477	40,16
Real materials (SEK x1,000)	2010	16 074	2 198	37%	17 002	0	1 585 537	3 986	1 758	33%	10 057	0	382 018	25,04
Real capital (SEK x1,000)	2010	16 074	367	6%	2 595	0	150 902	3 986	1 859	35%	5 266	0	159 850	-80,26
Real output (SEK x1,000)	2009	15 166	6 357	*N.M.*	59 392	0	3 767 575	3 750	5 119	*N.M.*	15 695	0	521 770	24,18
Employment (FTE)	2009	15 166	7,50	*N.M.*	65,31	0	4 578	3 750	4,96	*N.M.*	11,61	0	313	51,12
Real materials (SEK x1,000)	2009	15 166	2 318	36%	18 626	0	1 606 654	3 750	1 640	32%	7 985	0	314 642	41,39
Real capital (SEK x1,000)	2009	15 166	390	6%	2 789	0	183 026	3 750	1 923	38%	4 953	0	170 033	-79,73
Real output (SEK x1,000)	2008	14 315	6 884	*N.M.*	64 611	0	4 085 992	3 548	5 549	*N.M.*	15 461	0	386 045	24,06
Employment (FTE)	2008	14 315	7,77	*N.M.*	66,44	0	4 644	3 548	5,03	*N.M.*	10,48	0	256	54,42
Real materials (SEK x1,000)	2008	14 315	2 548	37%	20 641	0	1 803 343	3 548	1 849	33%	8 204	0	297 898	37,82
Real capital (SEK x1,000)	2008	14 315	399	6%	3 179	0	259 673	3 548	1 964	35%	4 916	0	168 242	-79,69
Real output (SEK x1,000)	2007	13 525	6 950	*N.M.*	65 382	0	4 059 263	3 298	5 591	*N.M.*	15 616	0	321 180	24,31
Employment (FTE)	2007	13 525	7,74	*N.M.*	64,45	0	4 376	3 298	5,08	*N.M.*	12,48	0	446	52,25
Real materials (SEK x1,000)	2007	13 525	2 515	36%	21 083	0	1 867 547	3 298	1 861	33%	7 965	0	261 456	35,15
Real capital (SEK x1,000)	2007	13 525	402	6%	3 396	0	291 518	3 298	1 882	34%	4 709	0	169 082	-78,64
Real output (SEK x1,000)	2006	12 701	6 927	*N.M.*	64 946	0	3 931 402	3 100	5 285	*N.M.*	13 832	0	270 735	31,09
Employment (FTE)	2006	12 701	7,62	*N.M.*	63,91	0	4 146	3 100	4,81	*N.M.*	10,80	0	313	58,22
Real materials (SEK x1,000)	2006	12 701	2 406	35%	12 978	0	1 177 798	3 100	1 746	33%	7 646	0	252 729	37,79
Real capital (SEK x1,000)	2006	12 701	418	6%	3 886	0	351 201	3 100	1 823	35%	4 571	0	167 680	-77,08

Real output (SEK x1,000)	2005	12 031	6 811	*N.M.*	65 661	0	3 297 425	2 919	4 916	*N.M.*	11 499	0	223 288	38,54
Employment (FTE)	2005	12 031	7,48	*N.M.*	63,20	0	4 162	2 919	4,51	*N.M.*	8,64	0	158	65,84
Real materials (SEK x1,000)	2005	12 031	2 253	33%	13 905	0	1 326 837	2 919	1 534	31%	6 292	0	210 505	46,84
Real capital (SEK x1,000)	2005	12 031	429	6%	3 963	0	354 320	2 919	1 757	36%	4 132	0	131 255	-75,60
Real output (SEK x1,000)	2004	11 333	6 751	*N.M.*	52 610	0	3 493 573	2 763	4 784	*N.M.*	11 443	0	224 012	41,13
Employment (FTE)	2004	11 333	7,35	*N.M.*	47,53	0	2 215	2 763	4,41	*N.M.*	8,37	0	153	66,76
Real materials (SEK x1,000)	2004	11 333	2 049	30%	6 036	0	190 313	2 763	1 356	28%	5 855	0	209 050	51,10
Real capital (SEK x1,000)	2004	11 333	466	7%	4 566	0	386 963	2 763	1 708	36%	3 842	0	111 874	-72,71
Real output (SEK x1,000)	2003	10 823	6 641	*N.M.*	42 592	0	1 991 718	2 626	4 599	*N.M.*	10 342	0	187 864	44,41
Employment (FTE)	2003	10 823	7,22	*N.M.*	41,62	0	2 086	2 626	4,30	*N.M.*	7,96	0	140	67,97
Real materials (SEK x1,000)	2003	10 823	2 021	30%	5 981	0	194 517	2 626	1 325	29%	5 424	0	174 022	52,58
Real capital (SEK x1,000)	2003	10 823	476	7%	3 841	0	299 680	2 626	1 709	37%	3 788	0	106 327	-72,17
Real output (SEK x1,000)	2002	10 323	6 927	*N.M.*	47 410	0	2 090 931	2 525	4 602	*N.M.*	10 501	0	244 308	50,52
Employment (FTE)	2002	10 323	7,32	*N.M.*	45,32	0	2 132	2 525	4,22	*N.M.*	7,43	0	96	73,49
Real materials (SEK x1,000)	2002	10 323	2 058	30%	6 290	0	213 750	2 525	1 185	26%	3 806	0	42 341	73,69
Real capital (SEK x1,000)	2002	10 323	477	7%	3 978	0	315 268	2 525	1 656	36%	3 483	0	89 757	-71,19
Real output (SEK x1,000)	2001	6 978	7 472	*N.M.*	44 760	0	1 926 108	1 644	4 554	*N.M.*	10 061	0	118 568	64,07
Employment (FTE)	2001	6 978	7,88	*N.M.*	45,32	0	1 944	1 644	4,16	*N.M.*	7,40	0	96	89,41
Real materials (SEK x1,000)	2001	6 978	2 229	30%	7 591	0	235 242	1 644	1 273	28%	4 706	0	78 350	75,06
Real capital (SEK x1,000)	2001	6 978	482	6%	3 200	0	175 536	1 644	1 532	34%	3 015	0	45 267	-68,54

Notes: The time period includes both pre-treatment observations, 2001-2008, and post-treatment, 2009-2010. Real values are in price levels of year 2000 and are obtained by deflating monetary values with *"Entreprenad index E84"* collected from Statistics Sweden. Output is defined as the turnover of the firm. Employment is measured in full-time equivalent employees (FTE). The materials variable includes raw materials, consumables and traded goods. Capital is defined as the book value of fixed material assets.

Appendix B

TABLE B.1: CALUCALTIONS FOR MONETARY GAIN FROM "*ROT-AVDRAGET*"

Treatment group TFP 2010 (inc. treatment)	69.66
Lower drop in TFP because of "*ROT-avdraget*"	11.36
% of 2010 TFP level	16.31%
Total industry revenue 2010 (SEKt)	150,795,205
Industry revenue 2010 without "*ROT-avdraget*"	126,203,854
Net gain	**24,591,351**

Source: Retriever