LUND UNIVERSITY
School of Economics and Management

Bachelor's Programme in Economy & Society

Firm Inequality and the Employment Tax

A Quantitative Study of Northern Sweden

by

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This paper quantitatively studies how firm inequality (society-wide firm concentration) in Northern Sweden was impacted by a 2002 deduction to the employment tax. The paper constructs Gini and Theil's T indices as measurements of firm inequality for 29 Swedish municipalities for the years 1998-2007, where the net sales and number of employees are used as two measurements of firm size. This results in four different indices used as panel data in a difference-in-differences approach to establish causality. Through econometric evaluation of all four tests, no evidence is found of the employment tax deduction having an impact on firm inequality.

Key words: Northern Sweden, Firm Inequality, Firm Concentration, Gini Index, Theil's T Index, Difference-in-differences

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Table of Contents

List of Tables

List of Figures

List of Maps and Equations

1 Introduction

Firm inequality, or how equally economic activity is distributed between firms in a marketplace, is at the very heart of economics as a discipline. As one of its most central tenets, the study of how dominant, monopolistic firms create adverse outcomes for consumers goes back at least to Adam Smith (1776: Book 1, Chapter 7). Consequently, understanding the forces that keep marketplace firms on equal footing can be considered one of the oldest interests of economics, to ensure the healthy competition of the marketplace. In a time of increasing divide between "average" firms and frontier firms (Frick, 2016), the need to continuously map and understand these forces is as apparent as ever, whether they are forces of market, taxation, or policy.

Sweden as a country has a worldwide reputation for its high-tax welfare state approach to levelling economic inequality. The country has an almost equally long history of using its tax code to forward marketplace competition. It has long been fine tuned to minimize distortions caused by it, and to maintain competitive markets. One example is the 1969 transition from a turnover tax to the VAT system (Ekonomifakta, 2024), which has been frequently argued to incentivize vertical integration in a distortive manner (Ross, 2016). Another is the abolishment of the inheritance tax in 2005 (Ydstedt, 2021), partially motivated as facilitating changes of ownership of small-and-medium sized enterprises (Berggren et al., 1999).

Nonetheless, the need for tax code adjustment always remains in public academic discussion. For instance, the employment organization Svenskt Näringsliv and individuals associated with it have across the last few years attempted to illuminate the issues of distorted market competition (Nelvin 2020; Sagebro, 2023). A 2009 study by Swedish Institutet för Näringslivsforskning provided a thorough analysis of current taxes they perceived as distortionary, alongside suggestions to change them. There appears to be a low-intensive but persistent debate on the subject in Swedish political and economic discourse.

This discussion is lacking empirical evidence from a national macro perspective. Individuals in the discussion often use localized case studies (see Nelvin, 2020) to study competition. However, taxation often being a national matter emphasizes a need for a national perspective on competition. The studies taking a national perspective, such as Institutet för

Näringslivsforskning (2009), mainly employ theoretical frameworks. This highlights a gap in the research. At present, there is a noticeable lack of empirical evidence on how the tax code impacts the concentration of Swedish firms' economic activity from a macro perspective.

1.1 Research Problem

One specific tax requiring more empirical evidence is the employment tax – a payroll tax levied against Swedish employers, calculated from the employee's salary (Bennmarker, Mellander, & Öckert, 2009). Previous research on it has proven difficult due to the national and universal nature of the tax (Edmark et al., 2012a). Its impact on firm inequality specifically has, to my knowledge, received very little to no empirical evidence.

There are theoretical frameworks surrounding it. These are centralized around the various distortive aspects of the tax, or the way in which its presence shifts market behaviors. Institutet för Näringslivsforskning argued that, since the tax increases contracting costs, consumers could for instance elect to perform services themselves rather than through the marketplace (2009: pp. 38-48), an analysis very similar to Ronald Coase's Coaseian view of the firm (1937). These frameworks can act as a springboard for informing an empirical analysis.

1.1.1 The 2002 Employment Tax Deduction

A rare opportunity to quantitively investigate the tax arose in northern Sweden (Norrland) in 2002. A 2009 study by Helge Bennmarker, Erik Mellander, and Björn Öckert investigated the impacts of a local deduction to the employment tax, performing a difference-in-differences study to establish the causal effects of changes to the employment tax. Norrland is divided into two "regional support areas" (stödområden, "RSAs"), one of which received a 10% deduction in the employment tax in 2002, capped at wage bills of 852,000 SEK (Bennmarker, Mellander, & Öckert, 2009). This example is one of seemingly few in recent Swedish history that lends itself well to a causal study on the impact of employment taxation.

The 2002 deduction being capped arguably makes it a form of progressive employment tax. As such, it could be expected that the improved strength of small firms relative to larger firms could have led to them expanding, potentially reducing firm inequality. This is an opportunity to provide some quantitative evidence, to fill the gap in the discussion on distorting taxes.

1.2 Aim, Scope, and Research Question

This paper seeks to investigate the impact of a 2002 reduction in the employment tax in Sweden's regional support area A ("RSA A"). It employs a difference-in-differences approach, comparing RSA A to the neighboring RSA B, which did not receive the same tax deduction. This quantitative study is primarily based on microdata from the Serrano Database (Weidenman, 2023). From this data, Gini and Theil's T indices of economic inequality are constructed from firm net sales and number of firm employees figures, between the years 1998-2007. It plots changes in these indices, with a focus on changes following the introduction of the 2002 deduction in RSA A. In doing so, it hopes to answer the research question: **By how much did firm inequality change in Norrland following the introduction of the 2002 employment tax deduction?**

This study is highly specific in scope, detailing a specific outcome of a specific policy. It also acts as a stress test of the Coaseian theory of the firm (1937), and its predictions for the results of an employment tax deduction. As such the research question is relevant to both policymakers and academics. Both finding an effect and failing to do so would be an interesting result, as it either implicates the employment tax as an instrument for strengthening market competition, or shows that the employment tax can be introduced without distorting this aspect of the market.

1.3 Outline of the Thesis

This paper starts by providing a brief review on modern literature on firm inequality. It then outlines theoretical frameworks for the impact of employment taxation on firm boundaries, one of which being from a Coaseian perspective. A review of the data used in the study follows. The paper discusses methods, where the fundamentals of difference-in-differences approaches are discussed, and the case of the 2002 deduction is put in context of the approach. A methodological discussion then follows on the construction of sample areas, and how inequality is best measured. Panel data is constructed for four different inequality indices for 29 municipalities split between RSAs A and B, for the period 1998-2007. The methods section ends with a description of the full difference-in-differences analysis using the constructed panel data, including a review of the so-called difference-in-differences assumptions. The empirical results are then presented, interpreted, and discussed in both descriptive and inferential analysis.

2 Theory and previous research

Studies investigating the direct link between the employment tax and firm inequality, or firm concentration, are seemingly very rare, making a literature review hard to perform. Instead, this section will discuss the role of the concept "firm inequality" in a modern academic context and point to some previous studies on the impact of the employment tax on the labor market. From this, a theoretical framework for the interaction of these variables is constructed.

2.1 Previous Research

2.1.1 Firm Inequality in Modern Academia

The terms "firm inequality" can loosely be likened to "firm concentration". At its most basic, it can be described as a measurement of how much economic activity is centered around how small a number of firms. It is usually measured using market shares of specific sectors, using proxies like firm sales to represent the presence of a firm in the marketplace (see for instance Grullon et al., 2019).

Interest in the concept goes back to the earliest days of economics. As a crucial part of price competition, it is arguably discussed already in Adam Smith's 1776 *The Wealth of Nations* (Book 1, Chapter 7). Describing this concentration in terms of "firm inequality" appears to be somewhat of a modern trend. A 2016 Harvard Business Review article (Frick, 2016) discussed firm concentration as a matter of firm inequality, in the face of a widening productivity gap between frontier firms and "average" firms in the United States.

Overall, this study will consider firm inequality and firm concentration as being different facets of the same phenomenon of market centralization. However, considering the intended macro perspective rather than focus on specific sectors, the term firm inequality will be used to capture a broader perspective.

2.1.1.1 Theories on Firm Inequality

Diversity of firms is generally considered an important part of well-functioning efficient markets. Any introductory level economics textbook will talk of the issues associated with monopolistic behavior, whether from individual firms (Cowen and Tabarrok, 2018: pp. 244-246) or cartel structures (2018: pp. 292-294). The reasoning is simple – the emergence of dominant firms through standard economic mechanisms like economics of scale (2018: p.253) results in a lower consumer surplus. Consequently, a diverse market of equal firms is desirable as it results in the best outcome for the consumer.

Beyond the elementary level, however, the discussion on the relationship between competition and growth is extensive. One highlight is Joseph Schumpeter's view that dominant firms are better for technological progress, and that they are as such overall beneficial to society at large (Nicholas, 2004). Another approach suggested that the relationship between competition and innovation is very case specific. Competition might encourage innovation between neck-in-neck firms but discourage if for laggard firms. This suggests an inverted-U model between innovation and supernatural profits (Aghion et al., 2005).

Although a full review is beyond the scope of this paper, the existence of this nuanced picture of the wider connection between inequality, consumer surplus, and technological innovation must be acknowledged. Furthermore, these ideas might be applicable to wider society rather than individual industries. This would require broader measurements of inequality in wider society.

2.1.1.2 Attempts to Measure Firm Inequality

Attempts to measure firm inequality, as a society-wide form of firm concentration, is common. However, is is mostly done in the context of the US. Grullon, Larkin, and Michaely (2017) measured it using the Herfindahl-Hirschman Index (HHI) of market share concentration, finding that across the preceding two decades, 75% of US industries has seen an increased concentration. Martin, Parenti, and Toubal (2020), also using the HHI, went one step further and directly included taxation policy in their analysis. Using a difference-in-difference approach, they found significantly increased US firm concentration following increased corporate tax avoidance.

Bighelli, di Mauro, Melitz, and Mertens. (2023) remark that studies most studies have been done in a US context, and seldom in European countries. This is potentially problematic, as the

market forces behind changes in firm inequality should not be assumed to be present in other context, such as Europe or Sweden. In response, Bighelli et al. (2023) created a variation on the standard HHI from European microdata. This association between this measure and several factors were investigated, including productivity, allocative efficiency, and markups. This is an example of a study society-wide firm concentration (or what this paper refers to as firm inequality), and its direct impact on the marketplace. However, the study did not include taxation in the analysis.

2.1.2 The Employment Tax and Labor Structures

Overall, modern academia seems to have somewhat of a fascination with firm inequality, and some studies do link taxation economics to the concept. To my knowledge, however, no study has been published that considers the effects of the employment tax on firm concentration, through the labor market. Instead, one must start by reviewing the documented impact of employment taxes on labor market structures, to link the tax and the inequality.

The first question is whether markets are responsive at all to employment tax alterations. For at least some cases, the answer appears to be a general yes. In the case of the 2002 deduction specifically, the Bennmarker, Mellander, and Öckert study (2009) found a positive effect on the number of firms with the deduction. Another study by Donald Bruce (2000) found that US payroll taxes had a significant, if multifaceted impact on the decisions by US workers to go into self-employment, simply concluding that "*Workers considering a switch to self-employment are apparently aware of their individual-level tax situations*" (2000: p.570). Although a comprehensive review is beyond the scope of this paper, this evidence suggests that employment taxes do impact the labor market to some extent.

However, the exact form of this impact is somewhat more up for debate. Traditionally, the employment tax is viewed as being borne by the employee, making it a de-facto income tax. This argument was used by the Swedish Reinfeldt government when elaborating their 2007 employment tax deduction (Finansdepartementet, 2012: p.8), alongside the argument that higher salaries could make individuals come out of unemployment (Finansdepartementet, 2012: p.11). In this view, deductions are passed on to the employee. There has however been some empirical evidence that the tax burden is split between more factors of production including capital, rather than all being borne by the employee (Carloni, 2021). Either way, in this view the employment tax can be viewed as hampering the general productive ability of a firm.

Another type impact is presented in a 2009 report by Institutet för Näringslivsforskning (pp. 38-48). It gives a specific theoretical model of distortion because of the employment tax. Their argument is exemplified by services provided to an end consumer. In a model with a buyer and a seller, the buyer will purchase a service if it is more advantageous than performing the service himself. If an employment tax is levied against the seller, it becomes relatively more advantageous for the buyer to perform the service himself, which disincentivizes a market transaction.

2.2 Theoretical Approach

Given the documented impact the employment tax on the labor market, it is a reasonable assumption that a deduction of such for small companies would strengthen their position in the wider economy, simply by providing tax savings for them. However, the exact way firms interact with the employment tax would change the way in which this impact would be noticeable in a quantitative study. This section will consider two such theoretical interactions.

Firstly, the savings could be passed on to employees, either fully (if the traditional view is to be believed), or partially (if the findings of Carloni (2021) are applicable in the 2002 case). In this view the deduction raises the relative bargaining power of small firms, allowing them to hire more competent individuals, which would make an impact on overall corporate performance. Alternatively, it would free up resources for the company, potentially to expand operations. In short, ability of the firm to service its market would be improved.

The second view is that the firm would expand its number of employees. The theoretical framework here is based on the logic of the 2009 (pp. 38-48) report by Institutet för Näringslivsforskning. This approach does indeed also predict that the amount of sales for service companies would reduce, as the end consumer could find it more profitable to perform the service themselves.

However, as it is essentially an analysis from transaction and contracting costs, it is spiritually alike an analysis from Ronald Coase's 1937 *The Nature of the Firm*. Through this lens, the deduction has other consequences. In the Coaseian view, the purpose of firms and employment contracts is to sidestep the transaction costs associated with negotiation in the free marketplace, saving resources for employer and employee. As the firm size increases however, the costs

associated with a larger firm (coordination costs, et cetera) start to increase. The natural boundary of the firm is thus found at the point where the gains from sidestepping the market mechanism offsets the rising costs of a larger firm.

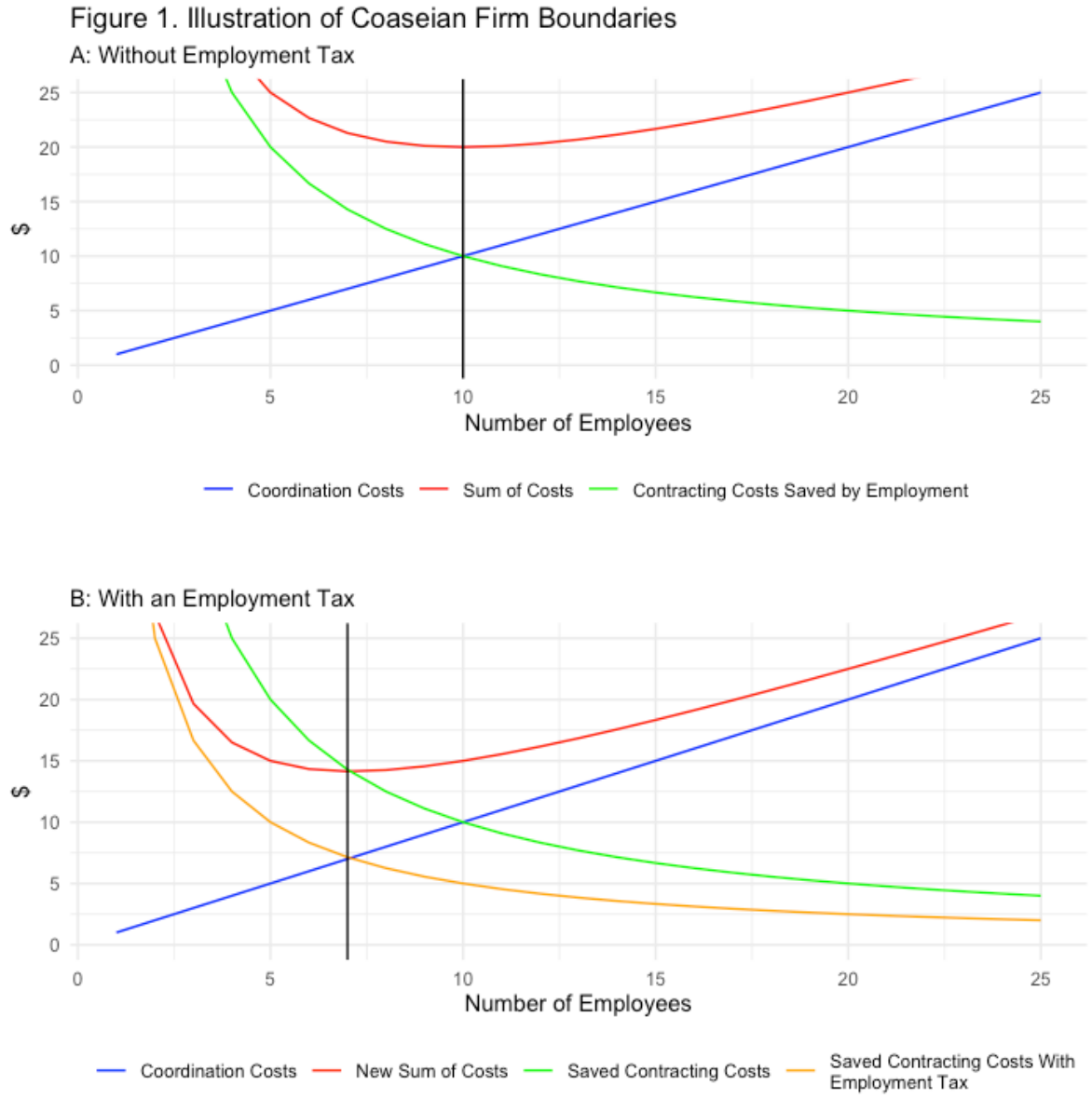
As an example, consider a firm that requires occasional legal consultation from an external law firm. For each specific case, the two parties agree on the contract for consultation on a specific matter. This process requires negotiations and resources, amounting to de facto contracting costs. If the firm requires very frequent legal consultation, these costs will eventually surpass the costs associated with hiring an in-house lawyer, which eliminates the need for contract negotiation.

An employment tax reduces the gains of employment contracts relative to free market coordination, which incentivizes outsourcing and makes the natural boundary of the firm smaller. For instance, a firm might no longer find it profitable to hire an in-house lawyer, instead turning to an external law firm. Similarly, a reduction in the employment tax expands the natural boundary of the firm. The dynamic is illustrated in figure 1.

This consideration lends itself to two specific measurements of firm size, suitable for different facets of interaction with the employment tax. The number of employees most directly goes in line with the Coaseian framework laid out above. Net sales, on the other hand, can work as shorthand for the overall ability of a firm to service a market. If small firms see an increase in their number of sales compared to larger firms following the deduction, it can ceteris paribus be seen as the firm being able to exploit the deduction savings to expand their operations, by hiring more employees, more high-quality employees, putting more resources back into the company, et cetera.

In brief, the consideration of what form firm expansion would take is contingent on whether the money saved by the deduction goes into hiring more individuals, or whether it goes back to the existing company and employees. Irrespective of what mechanism the tax works through, however, the fundamental dynamics in either case would strengthen the position of small firms, which could reduce firm inequality.

Figure 1. Illustration of Coaseian Firm Boundaries



Notes: in the theoretical example above, contracting costs saved by employment is defined as C1(n) = 100/n, where n is the number of employees in the firm, assumed to be an integer. The firm coordination cost is defined as C2(n) = n. The total cost function becomes C1+C2 = 100/n + n, at the minimum point of which the firm equilibrium border lie (n = 10). In B, a large employment tax is introduced, which halves the contract cost savings [0.5*C1(n) = 50/n]. The new equilibrium thus shifts to n=7. An increased employment tax as such makes the firm smaller. Source: author's calculations

3 Data

3.1 Source Material

This paper employs panel microdata from the Serrano Database (Weidenman, 2023) for the years 1998 through 2007, establishing a ten year period, encompassing the 2002 deduction (Bennmarker, Mellander, & Öckert, 2009), and stopping just before the 2008 financial crisis. This data includes information from individual firms in the based on ”*data from the Swedish Companies Registration Office* […] *Statistics Sweden* […] *and group data from Bisnodes group register.*” (Weidenman, 2023).

This makes the data quantitative. It is compiled from a number of other sources,making it technically second hand data. However, assuming no mistakes were made in the constructions of the database, there should be no difference between the numbers in the primary sources and in the Serrano Database. As such the data should be very reliable. Based on the frameworks in section 2.2., the figures for net sales and number of employees are collected, alongside some company specific information.

The variables used include:

- Net sales(“rr01_ntoms”), reported in thousands of SEK
- Number of employees (“bslov_antanst”), reported in number of employees
- Organisational number (“ORGNR”), an identification code assigned to every firm in Sweden
- Municipality (ser_kommunx), a four digit code for identifying the municipality the firm is registered in

Source: Weidenman, 2023

Furthermore, population density data from the Swedish Central Bureau of Statistics’ (“SCB”) Statistical Database (SCB, 2024a) is used for each municipality as a controlling variable, reported as a numerical value for each separate municipality by year. SCBs population data is quantitative, and comes from the Swedish tax authority (SCB, 2024b). It adds the variable:

- Population density (“popdens”) reported in number of inhabitants per square kilometer

Summary statistics for these numbers are provided in 1.A. Source: SCB, 2024a

Finally, the exact information on what municipality is in what RSA is found by consulting the specific government regulation that enacted them (SFS, 1999), as well as by the original Bennmarker, Mellander, and Öckert paper (2009).

3.2 Data Cleaning

For the Serrano Database, since the analysis concerns number of employees and net sales, the analysis is restricted to firms within the above defined sample area that reported a positive number for net sales or number of employees, depending on what inequality is calculated. This is to exclude inactive firms and firms that do not act as a traditional employer, and as such would be less interesting for the question about how firms respond to changes in the employment tax. Upon evaluation of the data set after the cleaning, it became clear that this restriction limited the analysis to only include joint stock firms (“Aktiebolag”). Finally, based on the ser_kommunx variable, firms are sorted by their municipality into either RSA A or B, depending on what category SFS (1999) places them in.

Unlike the study that inspired it (Bennmarker, Mellander, and Öckert, 2009), this thesis does not limit the analysis to small firms, as the purpose of the study is to investigate whether these firms were strengthened in relation to larger firms. This puts the total sample sizes for the calculations of firm inequality by net sales at 167,579 and by number of employees at 140,846, each split over 29 municipalities (see table 2 for a full list) and 10 years 1998-2007. The sample sizes for net sales and number of employees differ, due to not all firms reporting positive net sales necessarily reported a positive number of employees.

As such, the Serrano Database was split into two datasets – one used for calculating each category of inequality. Across tables 1-3 these two different datasets will both be displayed. From these datasets (Weidenman, 2023; SCB, 2024a), panel data is constructed for each municipality and year. The methods section outlines how this data was constructed, and section 4.4.4 describes the structure of the panel data.

3.3 Evaluation of Data

All the datasets come from reputable sources. The Serrano Database is published through the Stockholm School of Economics (Weidenman, 2023), SCB is the Swedish Government's statistical agency, and RSA borders are checked both against government regulation (SFS, 1999) and another study (Bennmarker, Mellander, & Öckert, 2009). The data should be evaluated for reliability, representativity, and validity.

All sources are reliable and valid. The Serrano Database gets it data from the Swedish Companies Registration Office among others (Weidenman, 2023), which means they are not estimates but very specific numbers. The SCB figures are originally from Skatteverket (SCB, 2024b) meaning the figures are reliable. As such, the figures are likely not estimates, but precise measurements. Finally, the RSA borders are (mostly) constructed by a simple list of municipalities, leaving little room for unreliability. As for the validity, the Serrano figures used are intuitive measures of firm size. The population density data captures both how many individuals are in a municipality, and how spread out they are. The figures are thus reliable and valid.

Representativity of samples should not be an issue. The population density figures match 1-to-1 with the municipalities and year, and there is no omitted data that could skew the result. The Serrano Database's source website (Weidenman, 2023) mentions nothing about sampling, simply that it is based on "data from the Swedish Companies Registration Office" and other sources. I interpret this as the Serrano database being a more-or-less comprehensive database, in which case sampling errors would not be a factor.

However, the cleaned datasets could have a representativity issue. When filtering for positive values in the dataset, only joint stock firms ("Aktiebolag") are left. This makes the analysis de facto only about such companies and excludes non-listed companies. This is potentially an issue when interpreting the results, as it could skew results against sectors that do not have many joint stock companies. Nonetheless, there is no reason to believe that joint stock firms interact fundamentally differently in response to the theoretical framework laid out above, and the sample is still large and diverse for conclusions to be drawn (Weidenman, 2023). Finally, there is an issue with the lack of more geodata in the Serrano dataset, leading to some issues in the construction of RSA borders. This issue is elaborated upon further in section 4.3.

4 Methods

The methodological approach of this paper is heavily influenced by Bennmarker, Mellander, and Öckert's 2009 study, and their constructed difference-in-differences approach ("DiD"). They attempted to exploit the 2002 tax deduction to investigate the impact of a deduction in the employment tax. My article mirrors theirs, using the same case to investigate another phenomenon. In this article the dependent variable is firm inequality, measured in Gini and Theil's T indices, based on figures on net sales and number of employees in firms.

This section details the theory behind DiD, and how the 2002 deduction fits into it. A discussion then follows on how to measure firm inequality, including choice of metric, and on what level of agglomeration (municipal or wider). The eventual result of this discussion is a panel dataset used for the final DiD method. The panel dataset is described in section 4.4.4. The subsequent analytical method is described in 4.5-4.6.

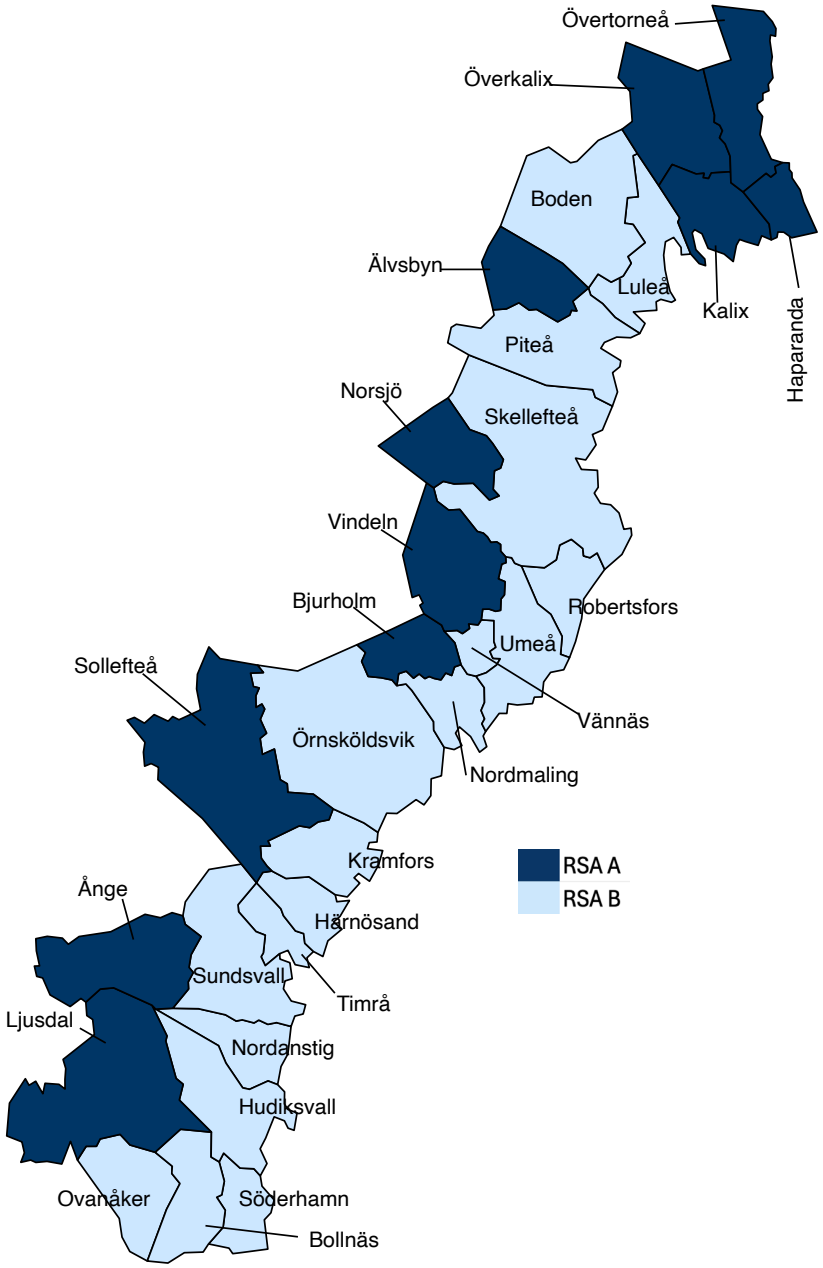
4.1 Difference-in-Differences Approach

A DiD approach is an econometric method of attempting to establish the causal effects – or lack thereof – of a specific event, such as a policy. The method considers two comparable samples of individuals, firms, municipalities, or similar, where only one of them is exposed to a specific "treatment", for instance a policy, and investment, or similar. The effects observed in the group exposed to the treatment (the "treatment group") is then compared to the effects observed in the group not exposed to the treatment (the "control group"). Any difference in effect can then, in theory, be argued to be a result of the treatment (Mailman School of Public Health, n.d.).

To establish causality, several assumptions about the underlying samples must hold. The Columbia Mailman School of Public Health (n.d.) summarize these assumptions as exchangeability (the effects of the treatment is not dependent on which group it is applied to), positivity (all individuals are at chance of receiving the treatment or not (Igelström et al., 2022)), allocation of treatment is not determined by baseline outcome, stable unit treatment value assumption (SUTVA), and parallel outcome trends. These are elaborated upon in section 4.5.

4.2 The Case of Swedish RSAs in 2002

Map 1. RSAs A and B, Coastal Norrland, 1999



Notes: Submunicipal areas in five municipalities (Boden, Piteå, Skellefteå, Örnsköldsvik and Sundsvall) that are part of RSA A are not represented as such in the map or in the method of the study (see 4.3 for details). RSA information borders are from SFS (1999), the map is made by the author, the shapefile for the map comes from SCB (n.d.)

Large parts of Sweden are sorted into a so called “regional support area” (“stödområde”, “RSA”). These are large chunks of the country that receive tax cuts and special investments as a tool for spurring local economic growth (Bennmarker, Mellander, & Öckert, 2009). In northern Sweden (Norrland), coastal areas are sorted into RSA B, while inland areas are part of RSA A. The border between the areas is drawn by municipalities (mostly), and map 1 illustrates this.

RSAs A and B have historically enjoyed deductions to their employment taxes. In 2002 the national employment tax was 32.82% of the employee’s salary. From this, RSA A had previously enjoyed a universal deduction of 10 and later 8%, which was removed fully in 1999. Starting in 1997, both RSAs had a 5% deduction to the employment tax up to a total wage bill ceiling of 852,000 SEK. Finally, in 2002 RSA A received an additional 10% deduction on top of the preexisting 5%, for a total of 15% up to the wage bill ceiling of 852,000 SEK (Bennmarker, Mellander, & Öckert, 2009).

In the 2009 study, Bennmarker, Mellander, and Öckert (2009) exploited this taxation discrepancy to perform a difference-in-differences analysis. They narrowed the analysis to more comparable parts of RSAs A and B, to attempt to construct two samples serving roughly the same markets. Here, the deduction is considered the treatment, RSA A takes the function of treatment group, and RSA B of control group. Through this approach, the authors were able to establish a positive causal effect of the deduction on number of firms.

Other employment tax reforms, like the 2007 Reinfeldt government deduction, have been applied nationally and universally, leaving no control group (Edmark et al., 2012a). Although quasiexperimental approaches can be employed in such instances (see Edmark et al., 2012b), it is hard to establish causality without a control group. This leaves the 2002 deduction as arguably the best possible policy instance in a Swedish context to peform a study mirroring the DiD approaches in the mold of Grullon et al. (2017), Martin et al. (2020), or Bighelli et al. (2023).

The 2002 tax reform is noteworthy, since it being capped would have led to it being proportionally more beneficial for small firms. The 852,000 SEK cap is about equal to the combined salaries of three industrial workers at the time (Bennmarker, Mellander, & Öckert, 2009: p.481). A ten percent deduction could, for instance, mean a small firm could afford a new employee. By the law of diminishing marginal utility, this would be a disproportionately beneficial policy to small firms, in comparison to a ten percent non-capped deduction. The

deduction functionally created two brackets for the employment tax, turning it progressive in much the same way as regular income taxes often are. In this way, the 2002 deduction finalized the transition from a universal employment tax deduction in RSA A, to a progressive employment tax deduction that was substantially higher in RSA A than it was in RSA B.

4.3 Constructing the RSAs

The analysis is restricted to the municipalities on the border region between RSAs A and B in the four northernmost coastal counties ("län") of Sweden – Norrbottens län, Västerbottens län, Västernorrlands län, and Gävleborgs län. This limits the analysis to municipalities that are physically close to each other and can be expected to somewhat share marketplace with their immediate neighbours on the other side of the RSA border. To make the two RSAs more comparable in number of observations, Haparanda and Övertorneå were included despite not being on the RSA border. This is motivated by Haparanda being the only coastal area in RSA A, and Övertorneå by its physical proximity (see map 1).

This narrows the analysis down to 11 municipalities in RSA A and 18 in RSA B. Every observation, without exception, was assigned an RSA value of "A" or "B" corresponding to their geographical location, where RSA A acts as treatment group in a difference-in-differences analysis. Table 2 contains a full list of municipalities included in each sample.

One issue is that municipal borders do not always align with RSA borders. In five municipalities (Boden, Piteå, Skellefteå, Örnsköldsvik, and Sundsvall), certain areas had been placed in RSA A while the rest of the municipalities were in RSA B. These areas were determined by church congregation or DeSO-codes (SFS, 1999). The underlying Serrano dataset had neither congregation nor DeSO codes. As such these "split municipalities" were all considered completely RSA B, as that is where the major population centers are in each case. This does however lead to a slight imperfection in the RSA borders in the analysis.

4.4 Measuring Inequality

4.4.1 Inequality of Sales and Inequality of Number of Employees

To investigate firm inequality, a proxy for the size of firms is needed. The literature review illustrated two good proxies – net sales, and number of employees. These two measures could be used to capture two different facets of corporate size – one facing the consumer market, and one facing the labor market. Together, these can form a two-pronged approach to measuring firm inequality, increasing both robustness and versatility of the results.

Net sales as a proxy for size is meant to capture the consumer market facing side of the firm. This is standard in economics of antitrust, where market shares are often calculated by using total sales when analysing specific sectors (see for instance Bighelli et al., 2023). However, the use of firm inequality in this paper should not be considered equal to a measure of sectoral firm concentration, as it is normally used (as in Grullon et al., 2019). Analyzing the entire marketplace entails aggregating sectors of different sizes, and with fundamentally different characteristics. Because of this, the figures involved will not be useful direct measurements of the levels of competition in the underlying sample. Instead, the measurement can be seen as gauging how much of the consumer market is centralized around how small a number of firms.

Similarly, inequality measures for number of employees provides a descriptive answer for how centralized the labor market is, by gauging how much of the labor market is centralized around how small a number of firms. As the phenomenon being investigated in the research question at hand is the employment tax, using number of employees as a proxy for corporate size is intuitive. It also more neatly aligns with the employment contract-approach in the aforementioned Coaseian analysis (Coase, 1937).

Tillväxtverket (2024) provides a comprehensive understanding of market distributions in Sweden for these variables. 97% of Sweden's 1.2 million firms have fewer than ten employees. However, firms with fewer than 50 employees make up roughly 40% of both total labor force and turnover among all firms. Although there is certainly room for interpretation among what firms should be included in such a breakdown of Sweden's market distributions, it is clearly a very top-heavy marketplace.

In brief, inequality will be measured using number of employees and total net sales by firm. This is intended to capture the presence of firms in the labor and consumer markets respectively, and how much economic activity in each market surrounds how small a number of firms. On a national level these markets are both top heavy, which would imply a high level of inequality.

4.4.2 Inequality Measurements

4.4.2.1 HHI and Gini

Furthermore, one needs to consider what index to use in measuring inequality. As seen in Bighelli et al. (2023), Martin et al. (2020), and Grullon et al. (2017), firm concentration is commonly measured using the Herfindahl-Hirschman Index (HHI), or a variation upon it. With N defined as the total number of firms in the market and index i being an individual firm in {1, 2, 3… N}, it is defined as:

$$Equation\ 1.\ HHI = \sum_{i}^{N} (Market\ share_i)^2$$

However, the HHI has many issues as a measurement, several of which were highlighted by Stephen Rhoades (1995). Rhoades shows that the HHI's has shortcomings in industries with several large actors and a large gap between top and average firms. He also illustrated in calculations that different markets with an increasing number of firms and increasingly extreme outliers can nonetheless have the same HHI (1995, p.660).

It is thus likely the HHI measures are misleading in a top-heavy marketplace like Sweden (Tillväxtverket 2024), and other measures should be considered. One slightly less used option to HHI in the economics of firm concentration and inequality is the standard Gini measurement, commonly associated with degrees of economic inequality between individuals in a nation (Tanusondjaja et al., 2021). It is calculated as in figure 2. The index has previously been used by Tanusondjaja, Dunn, and Miari (2021) to analyze market concentration in packaged goods (something the authors thought themselves the first to do).

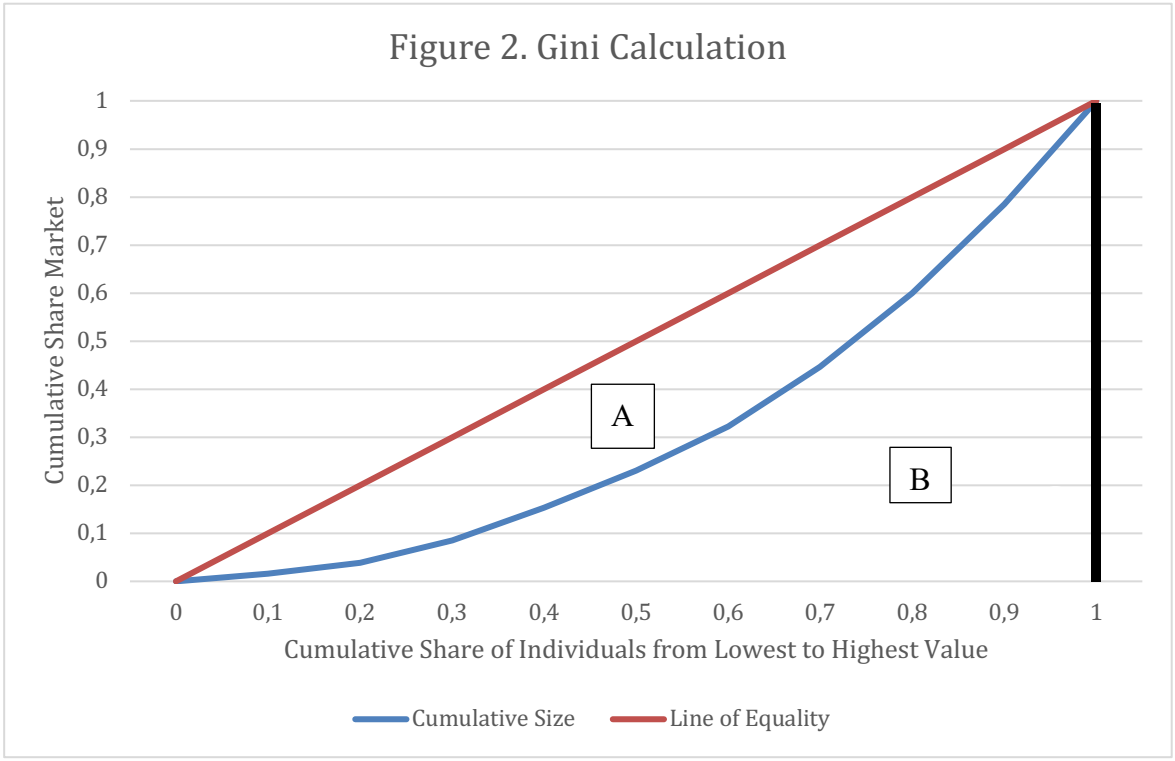


Figure: Illustration of calculating a Gini score using a Lorenz Curve (Cumulative Company Size), alongside a line of perfect equality. A is the area between the line of equality and the cumulative size curve, B is the area under the cumulative size curve. A Gini score would be calculated as: GINI = A / (A + B) (Source: Hasell, 2023) (figure made by author)

The Gini measurement does however not take the number of underlying observations into account (2021: p.476). With larger samples, however, Gini captures some nuances in a top-heavy market landscape that the HHI does not. This dynamic is exemplified in Appendix A. In the words of Tanusondjaja et al. (2021): "*HHI—are sales concentrated in a small number of manufacturers?* […] *Gini—are sales distributed equally across manufacturers?*" (2021: p.476).

4.4.2.2 Municipal Analysis, Theil's T and Gini Indices

However, as suggested by the Tanusondjaja et al. quote (2021: p.476), the Gini index might be misleading when comparing different samples of different sizes. In Sweden, Gini statistics are commonly used for gauging income inequality at a broader, national level (SCB, 2024c). There

are major limitations to this approach. One such is that different counties in Sweden have markedly different GDP levels (Enflo, Henning, & Schön, 2019), which could skew Gini calculations. For instance, if one region in the country has a significantly smaller economic output, a comparison between it and a larger region might not be a fair comparison. (see Cowell, 2000: pp.57-59 for an example where discrepancies in economic level can significantly alter Gini results). As Gini does not weigh results after local economic conditions, the measure is not necessarily decomposable.

One potential solution to this is to consider the Gini value of each municipality separate from each other, rather than for the entire country at large. This instead leads to each firm's size being benchmarked against its own municipality. This type of disaggregation of inequality is common. It is used in the context of the United States by Panizza (2002), and although the comparison of Swedish municipalities to US states is misleading, the basic concept of disaggregating a cohesive inequality to a subdivided panel data approach is nonetheless useful in a municipal context. Constructing separate Gini measurement is functionally to consider each municipality its own marketplace, which removes the issues of economic size discrepancies.

However, when considering municipalities separately, the obvious downside to the Gini index approach is that it does not take the size municipals into account in its calculation (figure 2). A Thiel's T index is more useful if one wants a measurement that weighs the size of the underlying sample area. It is defined as:

$$Equation\ 2. Thiel_{m,t} = \frac{1}{N_{m,t}} \sum_{i_{m,t}=1}^{N_{m,t}} \left[\frac{val_{i,t}}{avg.val_{m,t}} \times \ln\left(\frac{val_{i,t}}{avg.val_{m,t}} \right) \right]$$

Where t is a given time, m is an area (e.g. municipality), $N_{m,t}$ is the number of individuals in a subdivision m in time t (e.g. firms in a municipality during a specific year), index $i_{m,t}$ is for an individual firm i in $\{1, 2, 3\ldots N_{m,t}\}$ in an area m and time t, and $val_{i,t}$ and $avg.val_{m,t}$ is the value and average value of a variable of interest (for instance, net sales for a firm) for an individual and the full subdivision, respectively, in an area m and time t (Conceição & Ferreira, 2000).

The number of individuals being included in Theil's T index gives it the upside of weighing the size of the underlying group. On the downside, unlike Gini (figure 2), the index is not bounded between 0 and 1, and can take any positive value. Because of this, a Theil index can only be used to understand changes in an inequality, relative to itself.

4.4.3 How Inequality Will Be Measured

Considering the varying GDP levels between counties in the study (Enflo, Henning, & Schön, 2019), calculating an overarching index for the full RSAs would be skewed. Therefore, indices will be calculated for each municipality and year separately. Gini is used rather than HHI to deal with the potential measurement issues associated with the top-heavy nature of the Swedish economy (Tillväxtverket, 2024). Theil's T index will also be calculated as a supplement to the Gini index. The use of two indices is meant to partially increase the robustness of the results, as well as use the respective strengths of each index. The Gini index will allow for an intuitive score, bounded by $0 \leq \text{Gini} \leq 1$. Theil's T index will allow for inequality measurements to be weighted by number of firms in the municipality.

Gini is calculated by the method outlined in figure 2. Theil's T indices is calculated according to equation 2. Here, t is a year, m is municipality, $N_{m,t}$ is the number of firms in that municipality, $i_{m,t} = \{1, 2, \ldots, N_{m,t}\}$ and $val_{i,t}$ and $avg.val_{m,t}$ is the average value of either total sales or number of employees for a given municipality in time t.

4.4.4 The New Panel Data Set

The above discussion on inequality results in a panel dataset being constructed for 29 individual municipalities, each of which has an RSA value of A or B. The set contains Gini and Theil indices calculated by municipality and year. Inequality is measured both based on net sales of the underlying companies, and number of employees. This results in four new variables constructed from the Serrano Database (Weidenman, 2023):

- Inequality of net sales as measured by a Gini index
- Inequality of net sales as measured by a Theil's T index
- Inequality of number of employees as measured by a Gini index
- Inequality of number of employees as measured by a Theil's T index

One of each variable will be calculated for each year 1998-2007. Summary statistics for these variables are found in table 1.B. The municipal Gini and Theil's T indices are compiled into panel data, separated by RSA, and analyzed in comparison to each other. A excerpt of the panel data from 2002 is shown in Appendix B.

4.5 DiD Approach: Pre-Regression Analysis

With panel data constructed, a DiD approach can be employed. This will require two steps – ensuring the fundamental assumptions of the DiD approach are satisfied, followed by a DiD regression test. This section details the pre-regression work, and section 4.6. details the DiD method peformed on top of these assumptions. All the results are then presented jointly in section 5.1, and discussed in section 5.2.

First, the assumptions of the DiD approach must be considered (Columbia Mailman School of Public Health, n.d.). There are two levels of analysis to consider in this example – the firms in the RSAs, and the municipalities in the RSAs, both of which can be important to the assumptions.

4.5.1 Exchangeability, Positivity, Allocation of Treatment

Exchangeability (in this case that the effects of the deduction – is not dependent on which RSA it is applied to), positivity (all municipalities were at chance of receiving the treatment or not (Igelström et al., 2022)), and allocation of treatment not being determined by baseline outcome are all in a similar position of being tentatively true. RSAs A and B do have some structural differences, like RSA B being more on the coast and having larger population centers (Umeå, Luleå, et cetera). On the one hand, there are differences in how population density impacts hiring practices (Karahan, 2013). If there is a major difference in population density between the two RSAs, it could mean that the ways in which firms in each RSA interact with the tax deduction could be fundamentally different. On the other hand, the allocation of treatment might have been indirectly impacted by the same underlying characteristics that influence which RSA the municipality is allocated to.

Although this problem cannot be fixed, it can be mitigated by introducing controlling variables in an analysis. For instance, controlling for population density in the linear regression can account for some of the variability. Furthermore, accounting for the presence of institutions of higher learning can capture major differences in human capital levels and identify population centers large enough to support a university. Even so, with controls included, the issues with the assumptions makes a good case for caution in interpreting results.

4.5.2 SUTVA

The stable unit treatment value assumption states that *"[c]omposition of intervention and comparison groups is stable for repeated cross-sectional design*" (Columbia Mailman School of Public Health, n.d.). On a municipal level this holds true, as the number of municipalities is the same in all years in the sample (Weidenman, 2023). However, on the submunicipal level, the exact number of firms in a municipality being stagnant across an entire ten-year span is a wholly unrealistic assumption. Furthermore, it is possible that some firms could have crossed the border from RSA B to RSA A after 2002, to take advantage of the tax cut. Finally, as Bennmarker, Mellander, and Öckert (2009) mentioned, it is not guaranteed that all firms in RSA A took advantage of the tax deduction, or that they all did it at once.

Whether a substantial amount of people in RSA A did not take advantage of the deduction is likely impossible to establish with certainty. If the findings of Bruce (2000) – that individuals are aware of and respond to changes in their tax situation – is generalizable and applicable in the case of Norrland, we would not expect many firms to go without the deduction. However, definitive evidence either way likely does not exist for this specific case.

To the other points, I investigate the number of firms in each municipality in 1998 and 2007, to investigate the respective growth in number of firms, and see if there are any major asymmetries between municipalities. This difference is then plotted in table 2 to identify the potential asymmetries. Furthermore, I use the individual firm identifiers (ORGNR) in the Serrano database (Weidenman, 2023) to see whether any individual firm is observed in two different RSAs during the sample period. The number of such is presented in table 3.

4.5.3 Parallel Outcome Trends

Finally, parallel outcome trends must be established. This condition is satisfied when "*in the absence of treatment, the difference between the 'treatment' and 'control' group is constant over time*" (The Columbia Mailman School of Public Health, n.d.). Statistical software Stata provides an econometric test for establishing parallel trends in panel data ("ptrends" (Stata, n.d)), outlined in equation 3. This equation is used and reported in table 4. The null hypothesis is that there are parallel trends. The average observed values will also be seen in figures 3-6, however, these visually displayed average values cannot take any controlling variables into account

$$Equation\ 3.\ y_{ist} = DID_{ist} + \omega_i d_{t,0} t\zeta_1 + \omega_i d_{t,1} t\zeta_2 + \varepsilon_{ist}$$

Where DID_{ist} is a difference-in-difference model (see equation 4), ζ_1 and ζ_2 is the treatment effect in the pre- and post-treatment time respectively, $\omega_i = 1$ if the individual belongs to the treatment group and $\omega_i = 0$ otherwise, and $d_{t,0}$ and $d_{t,1}$being indicators of pre- and post-treatment time respectively. A Wald test establishes whether $\zeta_1 = 0$, or whether the treatment effect is non-existent before the administration of the treatment. The null hypothesis is that the trends are parallel before treatment. Source: Stata, n.d.

4.6 DiD Approach: Establishing Causality

The Gini and Theil's T indices are used in a difference-in-differences regression approach. The indices constitute panel data, with 29 municipalities across ten years (1998-2007). Each municipality and year has a population density value ("popdens"). A dummy variable ("university_status") is created to denote the municipalities with an institute of higher learning present. These include Umeå University (Umeå Universitet, 2022), Luleå Technical University in Luleå and Skellefteå (Luleå Technichal University, 2023), and Mittuniversitetet (previously Mitthögskolan) in Sundsvall (Mittuniversitetet, 2022) and Örnsköldsvik (Mittuniversitetet, 2019). The full regression model is highlighted in equation 4, and results are shown in table 5.

$$Equation\ 4.\ y_{m,t} = \beta_1 + \beta_2 \times RSA\_A_m \times after\_treat_t + \beta_3 \times RSA\_A_t + \beta_4 \times after\_treat_m + \beta_5 \times popdens_{m,t} + \beta_6 \times university\_status_m + \varepsilon_{t,m}$$

Where the dependent variable $y_{m,t}$ is inequality in any municipality and year as measured by any of the indices, RSA_A = 1 if the municipality is in RSA A and 0 if it is not, after_treat =1 if t >= 2002, popdens is population density as a numerical continuous variable, and university_status = 1 if the municipality has a university. β_2 is the difference-in-differences effect estimator.

5 Empirical Analysis

5.1 Results

5.1.1 Summary Statistics

Table 1. Summary Statistics for Serrano Datasets and Constructed Panel Data, split by RSA						
A. Serrano Datasets						
	N	Mean	Median	St. Dev	Min	Max
Num. Employees, RSA A	3,533	6.70	3	19.81	1	517
Net Sales, RSA A	4,006	9,858.387	2,119.5	57,794.23	1	2,030,331
Num. Employees, RSA B	20,877	8.82	3	61.56	1	6,490
Net sales, RSA B	24,609	15,571.26	1,820	221,476.7	1	35,879,000
Num. Employees, Total	24,151	8.51	3	57.33	1	6,490
Net Sales, Total	28,243	14,743.68	1,864	205,995.1	1	35,879,000
B. Constructed Panel Data						
RSA A	N	Mean	Median	St. Dev	Min	Max
Gini, Net Sales	110	0.76	0.77	0.08	0.56	0.92
Gini, Num. Employees	110	0.60	0.61	0.08	0.39	0.72
Theil's T, Net Sales	110	1.53	1.48	0.57	0.55	2.81
Theil's T, Num. Employees	110	0.90	0.88	0.34	0.26	1.72
Population Density	110	5.48	3.8	3.67	1.4	13.2
RSA B						
Gini, Net Sales	180	0.82	0.82	0.06	0.71	0.95
Gini, Num. Employees	180	0.68	0.68	0.07	0.53	0.83
Theil's T, Net Sales	180	2.09	1.86	0.77	1.09	4.65
Theil's T, Num Employess	180	1.28	1.18	0.51	0.57	3.05
Population Density	180	16.91	13.8	11.05	5.3	47.9

Notes: Rounded to two digits. Total number of firms is smaller than the sum of RSA A and B due to firms moving across borders, see table 3. Net sales reported in thousands, population density in people per square kilometer. In the constructed panel data, there are 29 municipalities across 10 years of observations, for 290 total observations. Data sources: Weidenman, 2023 (Serrano); SCB, 2024a (Population density); SFS, 1999 (RSA borders)

5.1.2 SUTVA and Parallel Trends

Table 2. Change in Number of Firms in Datasets by Municipality, 1998 to 2007

		Employment Dataset			Net Sales Dataset		
		Number of firms in			Number of firms in		
Municipality	RSA	1998	2007	Growth	1998	2007	Growth
Haparanda	A	129	180	40%	160	216	35%
Ljusdal	A	382	490	28%	435	562	29%
Älvsbyn	A	172	199	16%	201	227	13%
Kalix	A	235	263	12%	272	301	11%
Överkalix	A	70	74	6%	81	89	10%
Övertorneå	A	104	107	3%	117	122	4%
Ånge	A	215	221	3%	253	259	2%
Sollefteå	A	358	364	2%	405	425	5%
Norsjö	A	150	143	-5%	173	162	-6%
Bjurholm	A	56	52	-7%	71	61	-14%
Vindeln	A	136	125	-8%	154	155	1%
AVERAGE	**A**	**183**	**202**	**8%**	**211**	**235**	**8%**
Umeå	B	1719	2168	26%	2089	2728	31%
Nordanstig	B	138	167	21%	162	194	20%
Vännäs	B	99	119	20%	118	146	24%
Sundsvall	B	1671	1946	16%	2036	2455	21%
Piteå	B	666	774	16%	790	952	21%
Luleå	B	1190	1382	16%	1410	1663	18%
Bollnäs	B	498	577	16%	593	664	12%
Örnsköldsvik	B	940	1081	15%	1122	1308	17%
Söderhamn	B	465	521	12%	553	620	12%
Timrå	B	219	242	11%	254	277	9%
Boden	B	378	409	8%	429	486	13%
Härnösand	B	371	399	8%	424	472	11%
Hudiksvall	B	607	652	7%	717	787	10%
Skellefteå	B	1457	1527	5%	1668	1893	13%
Ovanåker	B	257	267	4%	287	301	5%
Robertsfors	B	126	128	2%	139	140	1%
Kramfors	B	327	330	1%	372	391	5%
Nordmaling	B	167	159	-5%	196	190	-3%
AVERAGE	**B**	**628**	**714**	**11%**	**742**	**870**	**13%**

Notes: Figures have been rounded to closest integer. Data sources: Weidenman, 2023 (Serrano); SCB, 2024a (Population density); SFS, 1999 (RSA borders)

Table 3. Number of Firms to Change RSA, 1998-2007		
	Dataset	
	Employees	Net Sales
Number	259	372
N	24151	28243
As % of Sample	1,1%	1,3%
Notes: A firm is considered to have crossed a border if it has observations in two different RSAs during 1998-2007 (firm data: Weidenman, 2023; border data: SFS, 1999)		

Table 4. Parallel Trends Test in Four Different Conditions		
	Theil's T	Gini
Net Sales	F(1, 28) = 0.38 P > F = 0.5431	F(1, 28) = 0.31 P > F = 0.5842
Number of Employees	F(1, 28) = 1.46 P > F = 0.2365	F(1, 28) = 1.95 P > F = 0.1737
Notes: H0=Lines Are Parallel. N = 290, strongly balanced across 1998-2007. Calculated from panel data, controls include population density and university status. Underlying data sources: Weidenman, 2023; SCB, 2024a; SFS, 1999		

5.1.3 Regression Results and Graphs

Table 5. Difference-in-Differences Regression Specifications Tests				
	Gini, Net Sales	Gini, Number of Employees	Theil's T, Net Sales	Theil's T, Number of Employees
Coefficient	-0.007	-0.004	-0.021	0.049
Rob. St. Err	0.012	0.011	0.109	0.060
t	-0.60	-0.38	-0.19	0.82
P>|t|	0.552	0.704	0.852	0.417
95% conf. min	-0.031	-0.028	-0.243	-0.073
95% conf. max	0.017	0.019	0.202	0.171
Notes: controls include population density and university status. Figures have been rounded to three decimal points. Data sources: Weidenman, 2023; SCB, 2024a; SFS, 1999; Umeå Universitet, 2022; Luleå Technichal University, 2023; Mittuniversitetet, 2022; Mittuniversitetet, 2019				

Figure 3. Mean Gini of Net Sales by RSA and Year

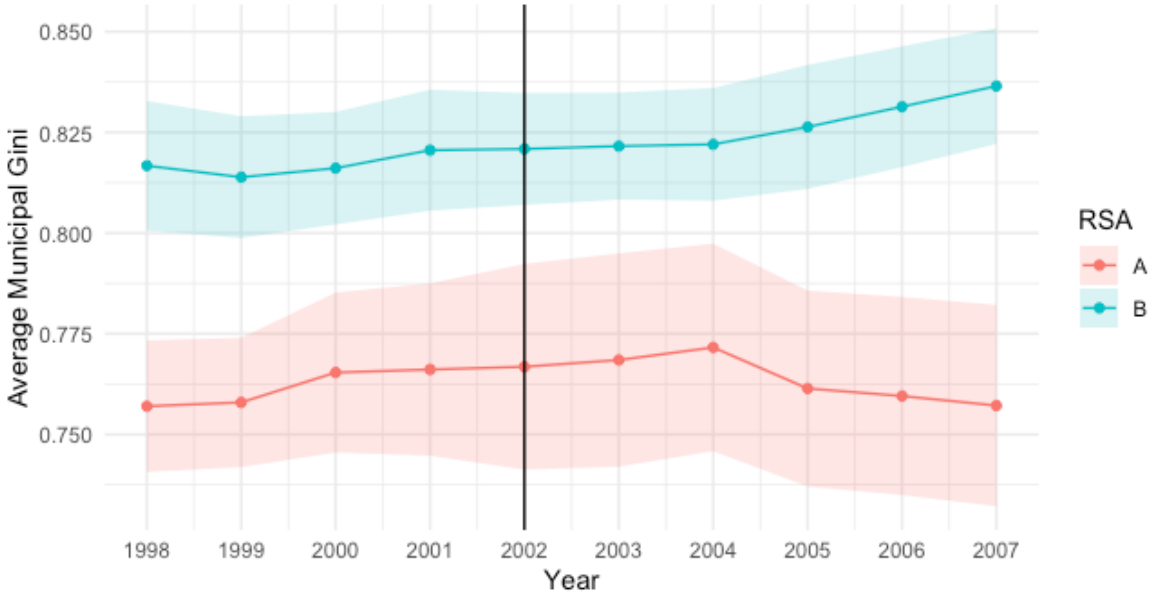
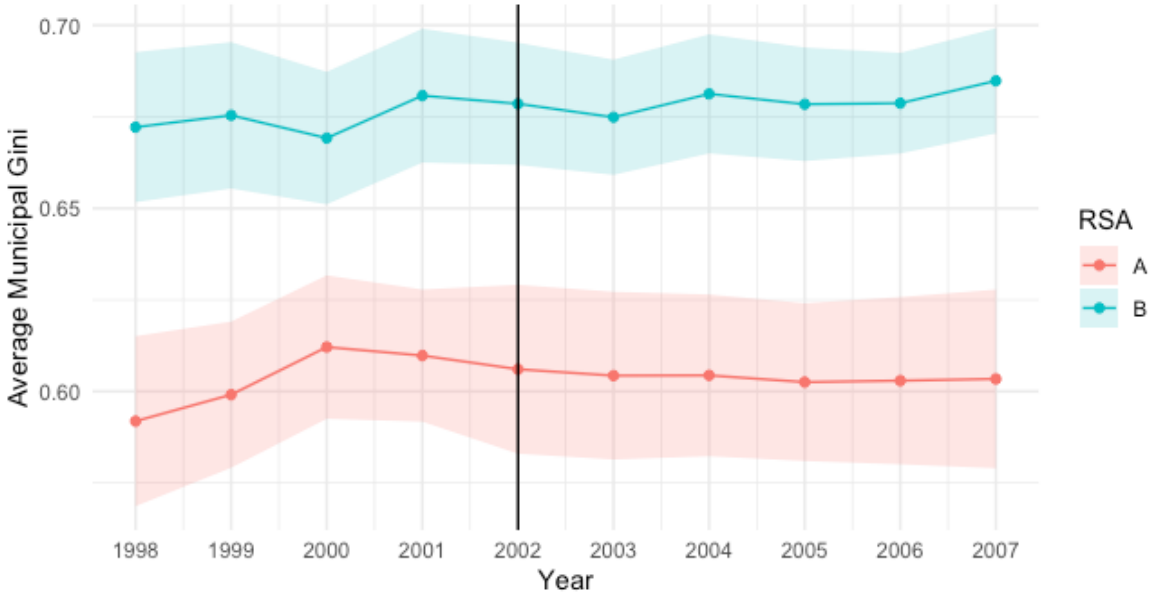


Figure 4. Mean Gini Number of Employees by RSA and Year



Notes for figures 3 and 4: Standard errors of means in shaded areas. Tax deduction introduced in 2002. This graph does not control for population density or university status. Y-axis limits are not consistent between the graphs. This is to facilitate descriptive comparison with figures 5 and 6. Graph by author. Data source: Weidenman, 2023; RSA borders: SFS, 1999

Figure 5. Mean Theil's T Index of Net Sales by RSA and Year

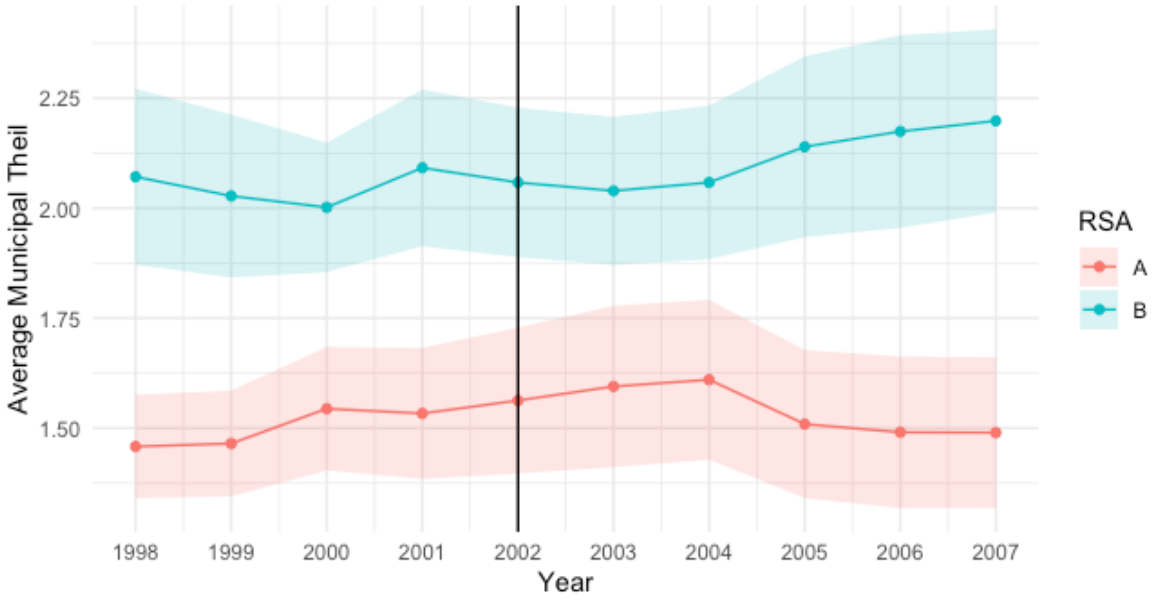
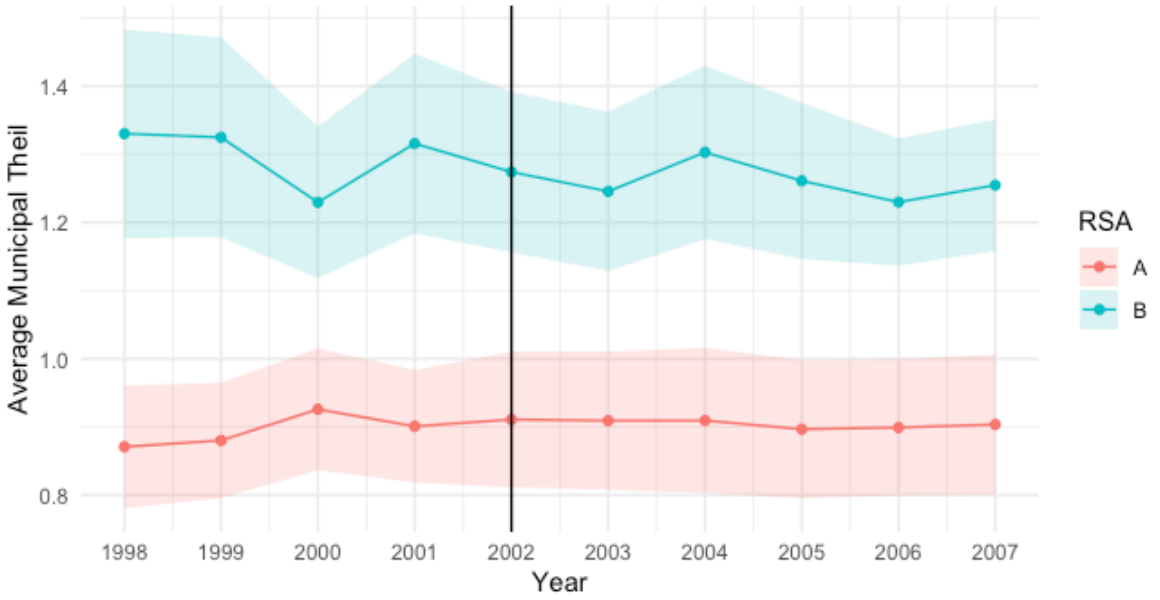


Figure 6. Mean Theil's T Index of Number of Employees by RSA and Year



Notes for figures 5 and 6: Standard errors of means in shaded areas. Tax deduction introduced in 2002. This graph does not control for population density or university status. Y-axis limits are not consistent between the graphs. This is to facilitate descriptive comparison with figures 5 and 6. Graph by author. Data source: Weidenman, 2023; RSA borders: SFS, 1999

5.2 Discussion

5.2.1 SUTVA and Parallel Trends

Table 1.A. gives an overview of the number of firms in RSAs A and B, and an overview of their similarities. The sample size in RSA A is significantly smaller than in RSA B. However, median and mean values are either matching or at least comparable. The significantly higher standard deviations in RSA B paints a picture of two differently sized areas that are somewhat comparable, but where RSA B has a more extreme distribution.

Tables 2 and 3 plot the stability of the underlying municipalities in the sample. According to table 3, 259 and 372 firms in the employees and net sales datasets respectively appear to have moved across RSA borders in the period, composing just over 1% of total firms in each dataset. As such, spillover effects seem extremely limited, to the point of being negligible.

As mentioned, the number of municipalities in the data is completely stable (Weidenman, 2023). However, as visible in table 2, some municipalities see major variation in number of firms within them, with most municipalities seeing an increase. There is a major difference in the average number of firms in the RSAs (table 2). This must be kept in mind when analyzing the results, especially when reading results from the Gini indices, which does not weigh the results by sample size (figure 2).

That being said, both RSAs had municipalities seeing significant growths (Haparanda and Umeå), stagnations (Robertsfors and Sollefteå) and recessions (Vindeln and Nordmaling) (table 2). Crucially, this suggests that the growth rate in the number of firms in a municipality is not necessarily dependent on its RSA. While this would need inferential analysis for a more definitive conclusion, the current evidence suggests that the municipalities in both RSAs in aggregate follow comparable trends, with municipalities in RSA A having fewer firms and more extreme growth trends (table 2).

The average municipality in RSA B saw its number of firms increase by 11% in the employment dataset and 13% in the sales dataset between 1998-2007. RSA A saw an increase with 8% in both sets (table 2). Bennmarker, Mellander, and Öckert (2009) found the 2002 deduction to give a positive effect for number of firms in RSA A. As such, the higher growth rate in RSA B should not be considered a result of the deduction. Instead, this could be a result of the generally

smaller amount of firms in RSA A municipalities (table 2) resulting in more extreme percentage swings from a smaller shift in number of firms. In that light, alongside the aforementioned diversity of growth patterns in both groups, the trends can be considered somewhat parallel.

In the parallel trends test, no null hypothesis can be rejected for any test (table 4), suggesting that the inequality trends are parallel across RSAs. However, the net sales inequalities are significantly closer to being perfect parallels than the employees inequalities. A visual inspection of figures 3-6 show as much. This appears to primary be due to a major shift in the inequality of number of employees in the year 2000, as visible in tables 4 and 6, in what is otherwise relatively horizontal plots.

Finally, analyses of Gini and Theil's T indices return similar results for similar size proxies. The p-values presented in table 4 are close to each other. Figures 3 and 5 for net sales and figures 4 and 6 for employees even show very similar trends in average values. This implies that the results are somewhat robust, and not subject to major swings by the exact variable being used. More variables of inequality, such as the HHI, could be used to enhance this robustness.

In conclusion, given the seemingly balanced growth in the underlying samples in municipalities, and the mostly-parallel trends of the two RSAs, the two areas are decent points of comparison in a difference-in-differences approach. This confirms Bennmarker, Mellander, and Öckert's use of the case for a DiD approach (2009). However, the size difference between the areas, as well as highlighted differences means that causal studies comparing the two should be done with some caution.

5.2.2 Descriptive Analysis

The mean Gini scores for municipalities (figures 3 and 4) are overall very high. Standard errors are at or below 0.08, suggesting only a moderately sized difference between municipalities across all years. This confirms that the Norrland coastal area is a top-heavy marketplace, just like the rest of Sweden. Similarly, the inequality of number of employees being larger than the net sales figures goes in line with wider Swedish trends (Tillväxtverket, 2024).

The measurements of inequality are remarkably stagnant, with no average Gini measurement changing by more than 0.02 points for any category of analysis (figures 3 and 4). The Theil index trends are similarly uneventful (figures 5 and 6). These results stand in contrast to the US

trends seen in Grullon, Larkin, and Michaely (2017) and Martin, Parenti, and Toubal (2020), where firm concentration has overall increased. Although the metrics being used are different, Norrland's municipalities do not seem to be following the US trend of economic power being consolidated in fewer and fewer companies.

Another remarkable feature is the similarity of trends across the two indices. The shapes of curves for net sales Theil and Gini (figures 3 and 5), and the same for number of employees (figures 4 and 6), appear from visual inspection to follow very similar trends. This suggests that these trends are consistent across different measuring methods.

Irrespective of metric and variable, RSA B consistently exhibits more inequality than RSA A. This is an interesting result that can serve as a springboard for further study on the underlying causes. One potential cause is that economic activity is much smaller in RSA A (as seen in the much lower number of firms in table 2). One could imagine a Kuznets curve-like structure (Kuznets, 1955) where RSA A is on the left hand side of peak inequality, and that an increase in economic activity would eventually translate into higher economic inequality between firms. RSA A's significantly lower population density (table 1.a.) could also provide an explanation. Firms in RSA A might have smaller local labor and goods markets, which would simply not allow for major inequalities in either variable.

5.2.3 Interpretation of Specification Test

None of the four tests provide evidence enough to reject the null hypothesis of the 2002 tax cut having an effect (table 5). The coefficients are all close to zero and have values both positive and negative within the 95% confidence interval. As such, none of the four different tests suggest that the 2002 tax deduction had any substantial impact on firm inequality. It is possible that changes in the labor market would not be visible on such a short time frame, with effects manifesting over time. However, the unanimously statistically insignificant results, as well as all results being either positive or negative within the 95% confidence interval (table 5), should be read as fairly decisive evidence that the deduction simply did not have an impact on inequality as measured by the Gini and Theil indices.

Interpretation of this depends on one's theoretical approach to the result. Three potential interpretations are highlighted in this section. First, the deduction might have been too small to generate an effect. Second, the deduction did have an effect, but it is not visible as a shift in

firm inequality by the metrics used. Third, it is possible that the employment tax actually does not have a distortionary effect on the labor market. These will be considered in turn.

5.2.3.1 Interpretation 1 – Small Effect Size

First, regarding the effect size, it is possible that a 10% deduction with a comparatively low cap is not a large enough reform to significantly impact the overall market balance. Although this is an appealing explanation, it must be remembered that Bennmarker, Mellander, and Öckert (2009) were able to find some small but statistically significant effects on wages, while the results in table 5 find nothing approximating such. Furthermore, although the cap of 852.000 SEK (Bennmarker, Mellander, & Öckert, 2009) can appear small, a ten point deduction is a major cut. Bruce (2000) established statistically significant results based on incremental increases in the US payroll tax amounting to over 12 points across 40 years. In this context, the 2002 deduction appears as a reform large enough to warrant a change.

5.2.3.2 Interpretation 2 – Invisible in the Metrics

Second, it is possible that the chosen metrics of inequality – Theil's T and Gini – were unable to capture the treatment effects. Both measurements include benchmarking individual values against average values among firms (figure 2 and equation 2). Since a change is expected to take place in small firms, it might be misleading to benchmark these against larger firms. Considering again the generally top-heavy structure of the Swedish marketplace (Tillväxtverket, 2024) and the observed large distance between median and top firms in table 1 for both number of employees and sales, small firms might increase their relative strength without significantly denting the dominance of the large firms enough to register in the indices.

This could also explain why the indices of inequality remained so stagnant despite the economic changes. All trendlines in figures 3-6 show a remarkably small shifts in the average inequality of municipalities, while table 2 shows a major increase in the number of firms in most municipalities. In future research, this potential issue could be solved by excluding the largest firms from the sample, and investigating specifically how small firms strengthened their positions relative to medium sized firms. However, where to set the cutoff point for what firms to include could easily become arbitrary. Instead, weighing the economic sizes of smaller firms in the analysis could be more advisable approach

However, this would only reduce the observed effect, not eliminate it completely. The regression results in table 5 does not just show a slight, non-significant effect, but an effect

whose sign is unclear. Even if Gini and Theil failed to capture the nuances in these changes, assuming the tax deduction was large enough to manifest change (as already argued that it was), we would expect a small, broad trend to be visible. As such, there are reasons to doubt this interpretation.

5.2.3.3 Interpretation 3 – The Employment Tax is Non-Distortionary

The final interpretation, assuming the previous two do not hold fully, is to take the results of the specification tests (table 5) at face value, and conclude that the deduction simply did not have an effect measurable as firm inequality. In this case, the interpretation becomes that this facet of the market is agnostic the employment tax.

The data gives some reasons to believe this. As mentioned regarding the parallel trends test, the trends do not exhibit major differences across different measurements being used. Although it might be distortive to benchmark shifts in small firms against the larger firms, there does still seem to be a certain robustness to the results. The previous observations that the trends in Gini and Theil are very similar in shape suggests that these figures might not be subject to major distortions by inherent issues in the indices. Considering how robustly stagnant the trends appear to be and considering the unanimous results of four different tests (table 5), the conclusion is that the employment tax does not impact firm inequality.

If this is the case, it has ramifications for both academia and policymaking. In academia, the results contradict Coase's theory of the firm (1937), by disproving the prediction made in figure 1. While this does not mean the Coaseian dynamics are not a force in the marketplace, but that there is no evidence of these forces being strong enough to change it. Instead, this suggests the market structure is more determined by other factors. These could be anything from technological conditions to other policies. A comprehensive review is beyond the scope of this paper, and lends itself well to future research on the toic of firm inequality.

The results also have ramifications for policymakers, specifically those informed by Coase (1937), or arguments similar to it (such as in the report by Institutet för Näringslivsforskning 2009: pp. 38-48). These results highlight that while there might be good economic theory being these arguments, they require quantitative evidence behind them to be meaningful as policy recommendations. As it stands, the decision to adjust the employment tax should instead consider other distortions that have empirical evidence behind them, such as number of firms (Bennmarker, Mellander, & Öckert, 2009) or degrees of self-employment (Bruce, 2000).

5.2.4 Limitations of the Study

5.2.4.1 Data Limitations

There could limitations with the underlying dataset. The Serrano database (Weidenman, 2023) seems to report certain company financials, specifically net sales and number of employees, exclusively for joint stock companies, which de facto limits the analysis to containing only this corporate form. While this might not be greatly distortive, there are certain labor market behaviors this analysis would thus fail to capture. For instance, considering the findings of Bruce (2000) that changes in the employment tax impacts the decision to go into self-employment, it is reasonable to expect the 2002 deduction to have had an impact on this behavior.

However, one could argue that if this had a major effect, the sharp increase in number of firms seen in table 2 would have been tempered. Furthermore, this does not challenge the overall finding that firm inequality is relatively agnostic to changes in the employment tax. This critique would only be crucial if there was reason to believe that the way joint stock companies interact with changes to the employment tax is fundamentally different to that of other corporate forms, to the point of the results being generalizable. That being said, there are potential biases introduced with limiting the sample in this way. To navigate these dynamics, a similar study to this should be performed with a dataset containing the data of more firms than just joint stock firms.

5.2.4.1 Incorrect RSA Borders

Another potential issue is that, unlike what is shown map 1, RSA borders do not always follow municipal borders. This division is usually done by church congregation ("församlingar") or so called DeSO codes (SFS, 1999)).This leads to five municipalities – Boden, Piteå, Örnsköldsvik, Skellefteå, and Sundsvall – being split between the two RSAs. Because of this, there are some firms in RSA A that have been placed in RSA B. This is an issue that Bennmarker, Mellander, and Öckert (2009) solved by using other geographical encoding other than municipal data, but the Serrano database only contains municipal data and post codes (Weidenman, 2023).

That being said, if this "split municipality issue" was successfully solved, the underlying methodology would need adjustment. The Theil index was selected specifically because it weighs the size of each individual municipality (see equation 2). However, once this weighing

has been done, the datapoints would still be considered of equal weight to significantly more populated areas in both equation 3 and 4 irrespective of their population sizes.

This might be a problem, as these congregations in RSA A can be too small for fair comparison. For instance, Gunnarsbyn congregation, which in 2007 had just over 700 inhabitants (SCB, 2024d) and could not reasonably be considered as a market separate from its neighbors. As such, including Gunnarsbyns församling on the same terms as major population centers, this would have greatly skewed the analysis even after being weighed. In this light, although the border between RSAs A and B have imperfections, given the chosen method, it might be significantly less distortionary to maintain these imperfections.

5.2.4.3 Exogenous Shocks

The study also does not take exogenous shocks into account. The study ends with the 2007 wave specifically to avoid complications with the 2008 recession. There are however some trends that might have struck both RSAs, potentially in an asymmetric fashion, which would skew the analysis. Consider the sharp spikes and drops for inequalities in the year 2000, as visible graphs 4-6. This single-year discrepancy suggests there could be an event in that year this paper fails to consider.

6 Conclusions

This study finds no evidence of the 2002 employment tax deduction having had an impact on firm inequality. There are reasons to maintain skepticism about the results, including some imperfections to the underlying assumptions of the difference-in-differences model used, and potential issues with the measurements of inequality. Nonetheless the results are robust and consistent across four different methods of investigating inequality. As an answer to the research question, this suggests that firm inequality did not change as a result of the 2002 deduction. Nonetheless, the framework and analytical method presented in the article lends itself well to further study of the topic of firm inequality and can be used to investigate other factors that can hypothetically impact it.

The construction of Gini and Theil's T indices also provided interesting insights. In contrast to trends in the United States where firm concentration is increasing (Grullon, Larkin, & Michaely, 2017), firm inequality in the Norrland coastal area is remaining stagnant (figures 3-6), despite major economic changes (see the increase in number of firms of table 2).

Despite some minor imperfections to the stable unit treatment value assumption, this study also finds that the border region between RSA A and B at this time is similar enough to serve as valid control and treatment groups in difference-in-differences studies. As such, the RSAs could be used in other studies attempting to causally establish the result of specific tax reforms.

The findings from this study could be of interest to academics and policy makers alike. The results contradict Ronald Coase's 1937 theory of the firm framework by showing an instance of the relative benefits of employment contracts increasing did not result in expanded firm boundaries. It also illustrates to legislators that employment tax reform might in fact not decrease firm inequality.

6.1 Statement on Artificial Intelligence

Generative artificial intelligence (AI) has been used fully in accordance with the Lund University Department of Economic History's policies (LUSEM, 2024). All original ideas are the author's own, and all analysis and writing was performed by the author without AI aid.

AI was frequently used as an aide with the coding languages R Studio and Stata 18. The use of AI in the coding was purely mechanical and did not replace any analytical intellectual effort on part of the writer. As an example, including shaded areas illustrating standard errors in figures 3-6 was the idea of the writer. A generative AI was then asked to provide the specific lines of code for how to create the shaded areas. All generated code was quality controlled before use.

To a small extent, AI enhanced search engines were used as a search tool to find articles associated with specific key words. Three articles were brought to the attention of the author in this way, those being Bennmarker, Mellander, and Öckert (2009), Bruce (2000), and Karahan (2013). No article content was informed by AI.

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Appendix A. Outliers in HHI and Gini

Table 6. Gini and HHI Example												
	Firm and firm size in turnover											
Market	1	2	3	4	5	6	7	8	9	10	Gini	HHI
A	$10	$10	$10	$10	$10	$10	$10	$10	$10	$1190	0.83	0.86
B	$10	$10	$10	$10	$10	$10	$10	$10	$600	$600	0.74	0.44
Notes: Total turnover in both marketplaces is $1280. Calculations are done by the author.												

Table 6 contains two different markets, A and B. Both have a total turnover of $1280 split between 10 firms, where most firms have a turnover of $10. Market A has one dominant firm, market B has two. Calculated Gini and HHIs are shown. When the dominant firm of Market A is split into two equal sized firms in market B, the HHI almost halves. The Gini score remains high. This is an example of Gini being able to correctly identify a top-heavy market B, while HHI clearly struggles to do so.

Appendix B. Excerpt of Panel Data

The below is an excerpt of the panel data presented in section 4.4.4. A decimal comma rather than decimal point is used. The example data contains all values for the year 2002. Average values are shown. RSA B has consistently higher inequality figures and population density figures than RSA A (Data sources: Weidenman, 2023; SCB, 2024a; SFS 1999).

Table 7. Excerpt of Panel Data

year	municipality	mun_code	rsa	gini_emp	gini_sales	theil_sales	theil_emp	population_densit	university_status
2002	Kalix	2514	A	0,72	0,86	2,34	1,40	9,8	0
2002	Älvsbyn	2560	A	0,71	0,88	2,36	1,61	5,2	0
2002	Norsjö	2417	A	0,64	0,75	1,27	0,95	2,6	0
2002	Vindeln	2404	A	0,64	0,76	1,43	1,01	2,2	0
2002	Sollefteå	2283	A	0,63	0,87	2,37	1,00	3,9	0
2002	Överkalix	2513	A	0,61	0,80	1,70	0,90	1,5	0
2002	Haparanda	2583	A	0,60	0,79	1,68	0,79	11,1	0
2002	Ånge	2260	A	0,60	0,79	1,58	0,83	3,6	0
2002	Ljusdal	2161	A	0,60	0,73	1,17	0,81	3,7	0
2002	Övertorneå	2518	A	0,48	0,64	0,76	0,40	2,3	0
2002	Bjurholm	2403	A	0,43	0,56	0,55	0,31	2	0
2002	Örnsköldsvik	2284	B	0,81	0,92	3,69	2,51	8,6	1
2002	Luleå	2580	B	0,77	0,88	2,68	2,01	34,2	1
2002	Sundsvall	2281	B	0,75	0,89	2,81	1,72	29,1	1
2002	Skellefteå	2482	B	0,74	0,88	2,85	1,75	10,5	1
2002	Kramfors	2282	B	0,74	0,85	2,46	1,44	12,1	0
2002	Ovanåker	2121	B	0,73	0,80	1,59	1,42	6,5	0
2002	Hudiksvall	2184	B	0,72	0,84	2,08	1,54	14,8	0
2002	Piteå	2581	B	0,71	0,88	2,80	1,38	13	0
2002	Umeå	2480	B	0,70	0,87	2,54	1,29	45,7	1
2002	Härnösand	2280	B	0,64	0,79	1,56	1,00	23,7	0
2002	Vännäs	2460	B	0,64	0,78	1,36	0,94	15,9	0
2002	Nordanstig	2132	B	0,64	0,79	1,47	0,89	7,3	0
2002	Robertsfors	2409	B	0,63	0,74	1,50	1,07	5,5	0
2002	Söderhamn	2182	B	0,63	0,85	2,24	0,87	25,6	0
2002	Bollnäs	2183	B	0,63	0,76	1,50	0,96	14,4	0
2002	Timrå	2262	B	0,59	0,77	1,43	0,71	22,6	0
2002	Nordmaling	2401	B	0,59	0,75	1,27	0,81	6,1	0
2002	Boden	2582	B	0,56	0,74	1,24	0,60	7	0
	Averages, RSA A			0,61	0,77	1,56	0,91	4,35	
	Averages, RSA B			0,68	0,82	2,06	1,27	16,81	