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Housing for all and Construction for Profit: A Swedish Oxymoron

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Abstract: Municipal housing companies have historically been an integral part in the development of the Swedish welfare state and are still an important tool for securing the housing supply. This thesis examines the effect of *nya Allbolagen* on the quantity of rental dwellings built by municipal housing companies. *Nya Allbolagen* is a law that was enacted in 2011 with the goal of aligning with EU competition law by leveling the playing field in the Swedish housing market. The law revoked preferential financing conditions granted to municipal housing companies and coerces them to act in a business-like way. We conduct multiple fixed effects difference-in-differences regressions, with and without accounting for time trends, for a variety of specifications. Using debt-to-equity ratios, we argue that many of the municipal housing companies acted in a business-like way prior to the law and use them as a control group. Using a theory of mixed oligopolies we predict that the supply of rental dwellings provided by municipal housing companies should decrease as a consequence of the law. Evidence of compliance with the law is found, but the resulting effect on the quantity supplied is evident only in the first two years after implementation, and disappears when extending the post-treatment period. The results we find are robust, significant, and in line with our theoretical predictions. Further research and more data is needed to better capture the mechanisms municipal housing companies used to adapt to the law.

Keywords: Housing policy, Sweden, Municipalities, Rental Dwellings, Competition law, nya Allbolagen.

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1 Introduction

Housing has historically been an integral part of the Swedish welfare state, alongside matters of education and health care. The view on housing as a state responsibility has gradually shifted since the Second World War, and today it is incumbent upon the municipalities to provide adequate housing for all. At their disposal, municipalities have their own municipal housing companies (MHCs), or *Allmännyttan*, which predominantly build and operate rental dwellings. Its importance for the overall housing supply has diminished, especially since people own their housing to a larger extent than before, but a third of the population still lives in a rental dwelling and around half of those are owned and operated by a municipal housing company (Sveriges allmännytta, n.d.-a).

According to Boverket (2021a) Sweden needs to build 600 000 dwellings in the coming ten years to meet predicted demand. That is partly because of population growth but also because there has been a consistent deficit of dwellings since 2006. The lack of construction has accumulated the housing shortage, which amounted to 187 000 houses and apartments in 2020. The housing shortage is spread equally across the country, and between 2016 and 2020, 83 percent of the municipalities answered in a survey from Boverket (2021b) that they were facing a lack of housing. Meanwhile, only 1.5 percent said that they had an excess of housing[1].

Housing matters for all aspects of life and a large and varied housing supply allows people to move for work, studies, and love. The flexibility associated with rental dwellings is of particular importance - if they are attainable. That is unfortunately not the case. More than one million people were registered in a queue for a rental apartment in the three largest regions, Stockholm, Göteborg and Malmö in 2019. In Stockholm the average waiting time for an apartment was over ten years (Karlsson, 2019). This particularly hurts young people and people born abroad, who have not lived here long enough to get an apartment through the queue. The situation is even more problematic for people in less favourable socio-economic situations. They are more likely to be born abroad, or have parents that were, and the parents are less likely to have a university degree (SCB, 2020). These factors make it less likely that they will get access to a rental apartment, and that they will have the capital to buy their own housing. To make matters worse, since the year 2000, the supply of rental dwellings has decreased by close to 11 percent, during a period where the overall population has increased by almost 17 percent (SCB, 2022a).

Housing is a larger question than solely MHCs, but they play an important role and

[1] Own calculations using survey results

when their circumstances change, it propagates to society at large. This thesis looks at the implementation of a law - *nya Allbolagen* (2011) - which changed the conditions according to which MHCs were allowed to operate. After 2011, every MHC must act in a business-like way and the guiding principle for each decision should essentially be: "What would a long-term private owner do?" (Riksdagen, 2022). Since municipalities also have an obligation to provide housing for all (SOU, 2015:58), one might think that this is a bit contradictory. Even if multiple studies(see Westerdahl, 2019; Grander, 2017; Grander, 2020) have been conducted on the subject, all of them take on a qualitative perspective. No study has yet tried to quantify the effect *nya Allbolagen* has had on the number of newly built dwellings, which is what we aim to do. *Allmännyttan* provides home to almost 1,5 million Swedes, and has historically been a foundation in the development of the Swedish welfare state. It is important to research what implications the law might have had on such an institution.

This thesis aims to answer the following research question:

Did nya Alloblagen have an impact on the amount of newly constructed rental dwellings by municipal housing companies?

In order to answer the question, we are using panel data on 241 municipalities in Sweden to conduct a Difference-in-Differences (DiD) analysis with Fixed Effects (FE). The DiD setup allows us to identify the counterfactual outcome of the law, i.e., how much would have been built if the law had not been implemented. We argue that, prior to the law, many MHCs already acted in a business-like way. Those MHCs should consequentially not have been affected by the law, or at least not to the same extent. We identify those MHCs that were acting in accordance with the objective of the law prior to 2011 by using their Debt-to-Equity (D/E) ratios and a sample of 185 private companies, active in the same sector, as a reference group, to make the distinction valid. We compare MHCs in bottom percentiles to those in the top ones. The idea is that the lower the D/E, the less affected the municipalities are by the law. To avoid discontinuity and arbitrariness in our threshold, but also to make the results more robust, we conduct the same regressions for several thresholds. Further robustness checks are pursued by relaxing the parallel trend assumption, removing the largest cities and their surrounding municipalities and by changing the distribution from normal to a negative binomial.

We use the mixed oligopoly model of De Fraja & Delbono (1989) to predict what effects we are likely to see when a market shifts from having both public and private firms being active to only having firms acting (and being) private. We chose this paper given the fit of the theoretical assumptions with the characteristics of the Swedish housing market. The model predicts that a public company would supply more quantity under

a mixed oligopoly than under a private one. When a public firm starts behaving as a private company, the market should resemble a regular oligopoly more, and the public (now acting as a private) firm should consequently supply a lower quantity.

We find a consistent negative effect on the supply of newly constructed dwellings for those municipalities affected by the law. A negative effect translates to the treated group producing less than they would have, was the law not implemented. Under the assumption of parallel trends, we find results statistically significant on the 1% level. Being conservative, we relax said assumption and introduce linear time-trends, which yield no significant results, unless we end the post-treatment period at 2014. With the linear trends and post-treatment period ended at 2014, we find negative effects, significant at the 5% level for our main specification. We present a wide variety of alternative specifications which confirm the robustness of the results. This thesis argues that *nya Allbolagen* initially had a negative effect on the supply of housing for municipalities that prior to the law did not act in a business-like way. Further research is needed to understand why the effects faded and if it is possible to combine business practices and a societal objective.

The structure of the thesis will be as follows: Section 2 will brush over the historical background of Allmännyttan, expand on *nya Allbolagen*, and show the extent to which municipalities have complied to the law since its implementation. Section 3 discusses previous and related literature and introduces the theoretical framework. Section 4 summarizes the data and the variables used in the regressions. Section 5 outlines the methodological approach. Section 6 summarizes the results. Section 7 provides further analysis, sensitivity analysis and robustness checks. Section 8 provides a discussion about our results and section 9 concludes.

2 Background

2.1 What is a municipality?

Sweden is divided into 21 regions and 290 municipalities. Both regions and municipalities are rather autonomous and are governed by elected politicians (Regeringen, 2022). Each municipality is responsible to supply its citizens with, inter alia, schooling, elderly care, and housing. The size of municipalities varies and the largest, Stockholm, has almost one million inhabitants while Bjurholm, the smallest, has 2 500. The idea behind municipal self-government is that people should be able to impact the supply of services which they themselves consume (SKR, 2021). It is supposed to enable municipalities to

provide solutions and services more suited to local demand. It is important, however, to understand that even if the principle of self-government is outlined in the Swedish constitution, it has many limitations. For example, the government can influence the design and supply of municipal services through both law and earmarked grants. With regards to housing, the responsibility lays on the municipality to provide good housing for its citizens (Riksdagen, 2022). The state, however, bears the responsibility to provide the municipalities with adequate financial and judicial conditions to achieve the objective. Both the government and the parliament have concluded that the municipal housing companies play a central role in the matter of housing (SOU, 2015:58).

2.2 History of Allmännyttan

To understand the scope of this thesis, it is important to understand whose responsibility the provision of housing has historically been, what the historical ambitions was, and how housing policies have changed over the years.

After the second world war, the Swedish state was concerned about the lack of adequate housing, especially for families with lower incomes. The Social Democrats, who headed the government from 1933 to 1976, believed that housing was as much of an integral part of the welfare state as education and health care (Grander, 2020). To encourage construction, the state understood that they needed to absorb some of the risks associated with building. Generous government loans and rent allowances to municipalities encouraged them to build more. State subsidies were conditioned on the company being a non-profit and either directly owned by a municipality or under municipal control. MHCs were therefore an integral part of the state's ambition to expand the housing supply by operating as sustainable long-term owners and developers (Hedman, 2008).

During this period, rent regulations enacted during the second world war were still in place, however, that was never the intention. The idea behind a large housing supply was that tenants would be in such a good bargaining position that there would be no need for rent regulations (Lind, 2013). Rent regulations were slowly removed and mostly replaced by *bruksvärdessystemet*, which was introduced in 1969 (Grander, 2020). *Bruksvärdessystemet* essentially states that rents should be equal for apartments of similar standards, with a maximum difference in rents of five percent. In this setting, similar standards refers to geographical location, where in the building the apartment is located, the quality and also the size(Sveriges Domstolar, 2019) . The rent itself is decided through negotiation between the local tenant association and the MHC, and the guiding principle was that rent increases should be limited to the amount allowing

MHCs to cover their planned maintenance costs (SOU, 2015:58). The rent increases negotiated between an MHC and the tenant association would then be considered normative and a private landlord could not increase rents by more than what the MHC did. The fundamental logic behind this system was that tenants have a right of possession. Absent *bruksvärdessystemet*, private landlords could increase rents until a point where it would become untenable for tenants to remain (Hyresgästsföreningen, 2013).

The core idea behind this push for housing and the development of MHCs was to provide good housing to everyone. Poorer households should then be compensated through housing grants and other financial additions, rather than having designated housing. The belief that earmarking housing for the poor would lead to segmented segregation and limited personal and economic development is important for understanding the situation today.

This political ambition culminated in 1970, which marked the peak of an expansive period known as *miljonprogrammet* which stretched over the years 1965 to 1974, and during which Sweden built 1 005 578 apartments and houses (SOU, 2015:58). During this period, MHCs could be granted loans up to 100 percent of the building costs, in addition to interest subsidies and tax advantages. Housing policy was integrated with economic policy and used as a means for increased employment and to counteract bad business cycles (Hedman, 2008). However, in the middle of the 1970s the housing shortage had disappeared and there was an excess of housing instead. MHCs in particular had issues finding tenants. Many of the projects were financed solely through state lending and MHCs had little capital to compensate for the lack of renters.

Miljonprogrammet was a success in terms of construction and the sheer number of apartments and houses being built. However, the program later faced heavy criticism for contributing to and deepening segregation. An additional goal, to create diversity, both ethnic and economical was formulated and once again the state used municipalities and their housing companies as the means to achieve those goals. Prior to this, the main purpose of MHCs was production, now maintenance and tenant satisfaction also became their responsibility (Hedman, 2008, Grander, 2020). In the beginning of the 1990s, government Bildt, a right-centre coalition, came to power and implemented some large structural changes to the housing market in general and to MHCs in particular. State housing loans were removed, and interest subsidies abolished. The ambition of government Bildt was to create a situation more similar to a regular market and to minimise the state's exposure to financial risk. In the beginning of 1990s, rent subsidies accounted for almost two percent of GDP (SOU, 2015). For MHCs, this meant that they were not treated as favourably and needed to begin to act more in a business-like

way. Despite the brought about with government Bildt, MHCs still enjoyed advantages compared to their private counterparts, which became apparent when Sweden became members of the European Union.

In 1995, Sweden became a member of the European Union and as a result they had to adapt to the rules of the single market. European competition law prohibits state aid to public companies since it distorts competition (EU Commission, 2022). Year 2002, The association for property owners, an organisation to promote the interest of private property owners (Fastighetsägarna, 2022), reported the Swedish state and *Allmännyttan* to the European Commission and claimed that *Allmännyttan* did not fulfil the conditions of European competition policy (Grander, 2020). Municipal support to MHCs and the normative role in rent negotiations were the main arguments used in denouncing why MHCs did not comply with the state aid rules. In 2005, The European Property Federation also reported Sweden to the European Commission for not complying with the rules on state aid. They claimed that municipalities were not demanding a market rate of return from their MHCs (SOU, 2008:38). Private companies aim for a certain return given the amount of risk associated with an investment, something that MHCs did not do to the same extent according to the claim. This was viewed as distorting competition since MHCs could pursue projects that would not make any financial sense for a private company. These claims were never tried in court. Sweden started an inquiry in 2005 to "review the possibilities for pursuing a national housing policy within the framework of EU membership, with particular regard to the circumstances and conditions for municipal housing companies" (Hedman, 2008). The result of this inquiry was a law called *nya Allbolagen*, enacted in 2011. It set new terms for MHCs and new rules for how rents are determined. Since then, municipalities have to demand that their housing companies pursue a required rate of return which aligns with the market rate. In addition, the interest rates on loans from municipalities to MHCs also had to reflect market rate. Another large change is that municipalities could no longer cover the financial deficits of their MHCs (SKR, 2021b). The rules for rent setting also changed. The previously normative role of MHCs disappeared, thereby enabling private companies to increase rents by more than what *Allmännyttan* does (Sveriges allmännytta, n.d.-b).

What is worth emphasising about rents is that this law change only applies to dwellings in buildings which are a part of the *bruksvärdessystem*. Since 2006, a parallel system for newly built dwellings is in place. This system is known as *presumtionshyra*. It was created to incentivize private companies to build rental dwellings. The system enables real estate developers to set the rent independently of *bruksvärdet* by adjusting it to a level that corresponds to covering the company's expenses plus making a reasonable profit. The *presumtionshyra* last for a minimum of 15 years and then becomes a part of

the *bruksvärdessystem*.

There have been dramatic changes in Swedish housing policies from the Second World War until today. Originally, there was a broad state funded ambition to supply anyone, no matter their income, with adequate housing. The notion that social housing cements segregation and that housing is integral to the functioning of the welfare state are historical remains that still influence policy to this day. Simultaneously MHCs have been forced, by internal and external pressures, to change and behave in a business-like way. This stands in contrast to countries such as France, Austria, Germany and the Netherlands which have a system for social housing and are thereby exempted from the EU rules on state aid (Elsinga & Lind, 2013).

2.3 Nya Allbolagen

Sweden could choose one of three ways to align with the EU rules on state aid and comply with European competition law. One was to level the playing field between the non-profit and profit housing companies by offering the same type of support to both types of companies; another was to target support to lower income households and renounce to non-profit companies; the last solution was to abolish support targeted to non-profit companies and have them act as profit/private companies (Elsinga & Lind, 2013). Sweden chose the latter solution to align with the European demands. The objective of *nya Allbolagen* is clear: MHCs should start acting in a business-like way and finance themselves in accordance with market terms and conditions. Sveriges Allmännytta (n.d.-c) helps us understand the concrete implications of this law change.

First, the required rate of return set by MHCs should be that of the market. This implies that it must correspond to the required rate of return pursued by similar housing companies, be based on the local market conditions, conform to the chosen risk level, and it must work towards the long-term benefit of the housing company. Exceptions are allowed only if company financials have deteriorated, and need to be cleared with the European Commission. To understand this better, think of Stockholm and Sorsele (a small municipality in the Northern part of Sweden). If you build rental dwellings in Stockholm, it is almost certain that demand will be high, and the risk associated with the investment is small. Since the risk is close to nothing, you would require a lower return. In Sorsele, the risk of the investment becoming unprofitable is larger, and you would thereby require a higher return to compensate for said risk (Lind & Lundström 2011). Second, municipalities are not allowed to cover deficits of the MHCs anymore. Third, guarantee fees must be paid on existing and future loans. MHCs use the municipality as a guarantor, which ensures lower interest rate costs. This is an advantage private

companies do not have. To level the playing field, MHCs are supposed to pay as much in interest as they would have done if they did not have a municipal guarantee. That difference is called the guarantee fee. Using guarantee fees instead of changing the interest rates ensures a swift implementation of the law, especially for loan contracts with long term fixed interest rates.

As a result of these changes, MHCs are not allowed to make unprofitable investments on the grounds of providing a public service anymore (Elsinga & Lind, 2013). However, the new law maintains that MHCs have a public purpose as well. How then, are MHCs supposed to act in a business-like way and yet fulfil their public purpose?

Answering this question will naturally lead to emphasising one of the two conflicting interests of the new law, namely profit and public purpose. Herein lies the difficulty in assessing the impact of the new law on the Swedish rental market: it gives room for interpretation. Shortly after the law was passed, two conflicting interpretations were competing. One possible interpretation is brought forward by Lind and Lundström (2011) who say that MHCs should act in the same way as a long-term private owner would, i.e., require a market rate of return and dispose of unprofitable investments. This interpretation obviously emphasizes the profit aspect of the rental business at the expense of the public purpose. The opposing view is that of the Swedish Tenants' Union. According to them, the public purpose of MHCs should be central in the interpretation of the law. The argument is, among others, that since property values are expanding in cities, owners would not need to act in a business-like way to get a reasonable rate of return (SABO, 2011). However, this argumentation is lacking when it comes to parts of Sweden that are not expanding. As a matter of fact, it is not uncommon for MHCs in more rural areas to be facing construction costs which are higher than the immediate valuation of the property being built.

2.4 Have MHCs complied with nya Allbolagen?

Fortunately, we benefit from a longer time-frame and can interpret the changes brought about by the law with a bit more perspective. There are clear indicators that MHCs have begun to act more in a business-like way, whether they intended to or not.

We start by looking at ownership directives (ODs). ODs are the primary tool, besides electing the board of the company, that municipalities use to enact control over their housing companies (Sveriges Allmännytta, n.d.-d). ODs are legally binding once they are enacted (Lindberg & Wilson, 2015). The OD specify, inter alia, financial metrics such as return on equity, solvency and dividends. Lindberg & Wilson (2015) was one of the

first papers that tried to estimate how well MHCs were complying with the new law. They compare ODs in 2004 and 2013 and found that financial citations had increased by 77%. The increase was stastistically significant at the 0.001% level, indicating that the law had an impact at an early stage. Lindberg & Wilson (2015) end their paper by noting that there is a difference between "talk the talk" and "walk the walk", meaning that objectives stated in the directives do not necessarily translate into new business practices. We find similar indications when using *Bostadsmarknadsenkäten*, a survey that Boverket has conducted among all municipalities in every year since 2012. In 2012, 144 municipalities, which corresponds to 55 percent of all municipalities with an MHC, had enacted a new ownership directive since the law was implemented. In 2015 the numbers were 220 and 85 percent. In 2020, only 11 municipalities had not updated their ownership directive since 2011.

In addition to the change in the required rate of return of the MHCs, the new law was supposed to have an impact on existing and future debt. Starting 2011, all MHCs are supposed to pay the difference in interest expenses resulting from the preferential rates they receive with municipal guarantees and the market rates private actors pay. Most municipalities, and by extension MHCs, use a service called Kommuninvest, which is a collaborative service where all municipalities guarantee each other's debt (Kommuninvest, 2022). The collective guarantees result in an AAA rating which are essentially considered risk free loans. Figure 1 illustrates the share of municipalities receiving guarantee fees for the period 2011-2020. Unfortunately, data is missing for the years 2011-2013. It is apparent that the guarantee fee has been implemented broadly throughout Sweden and that most municipalities have adhered to this aspect of *nya Allbolagen*. Out of the 241 municipalities in our sample, 71 percent had received a guarantee fee by 2014 and 95 percent by 2020.

Finally, the law prohibits municipalities from covering the deficit of their MHCs. While we could not find any data showing whether this has been implemented or not, there are some indications pointing towards compliance. Lundmark (2011) writes about the MHC in the municipality of Sorsele, where, following the implementation of the law, the housing company could not cover its existing deficit without municipal support and had to cease existing in its current form. This happened the same year as the law was implemented and is, once again, indicative of the swift implementation.

In this section we have provided descriptive statistics and other sources in support of the law gaining traction and being widely implemented. Even if it is not entirely clear whether new ownership directives have been translated into corporate behaviour, there is no doubt that *nya Allbolagen* has been swiftly implemented as intended. Boverket (2017) present the results from a survey asking MHCs to rate how the law impacted their

ability to supply housing. The majority claimed that it had not made any difference, almost no one believed it was easier and around a third and a fifth of the companies, in weak respectively medium markets, felt that it had become more difficult[2]. The development towards business-like principles has changed gradually, already before the law, and it is likely that many MHCs did not experience any big changes. A survey could always have shortcomings, like lack of respondents and subjective interpretation. In the case of newly built rental dwellings, it is more unbiased to control for actual construction. This highlights why our research is important, both as a contribution to the existing body of literature and also because it provides a quantitative perspective on a quantitative matter.

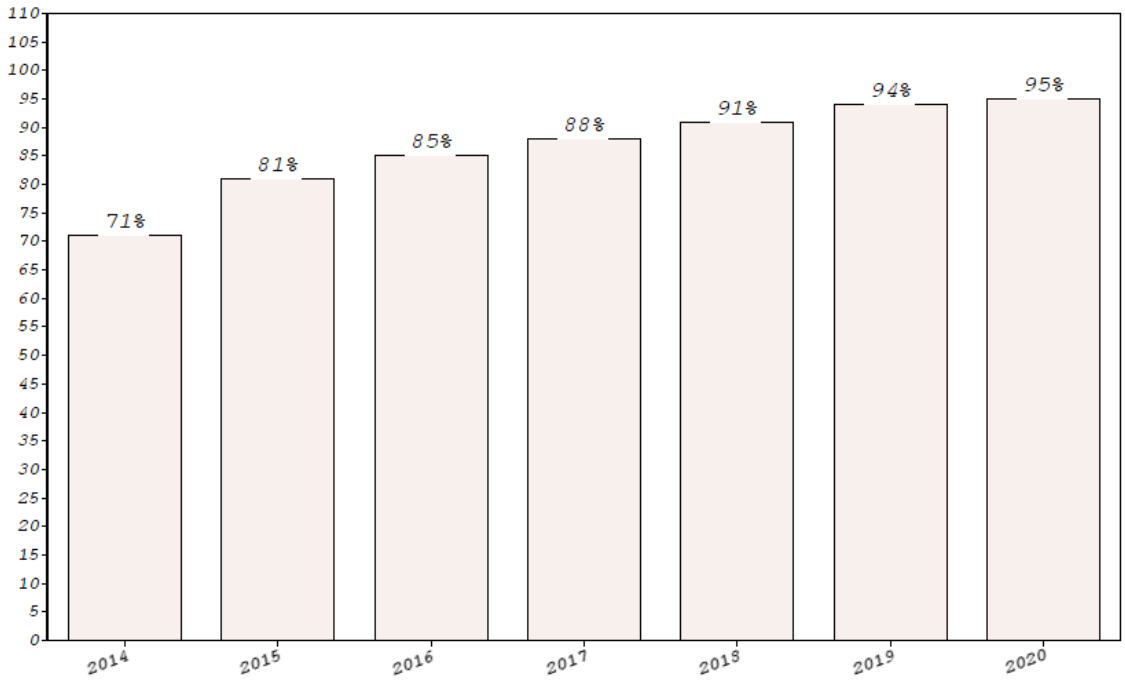

Figure 1: The share of municipalities in our sample receiving a guarantee fee.

3 Literature review

Literature on the consequences of *nya Allbolagen* on the Swedish housing market specifically has not gathered much attention so far. In addition, most of the analyses conducted on the topic lack quantification as well as an economic dimension. Finally, the research on the subject has overlooked the consequences on the supply of housing, favouring aspects such as rents and segregation instead.

[2]A medium market is defined as having an average house price between 1 and 2 million SEK in 2015. A weak market has an average price below 1 million SEK in the same year (Boverket, 2017)

3.1 Previous literature

In an early paper on the subject, Elsinga & Lind (2013) analyse the short-term consequences of the law and speculate on its longer-term potential effects. They argue that, theoretically, the law change should lead to an increase in rents across the market and an end to non-profit housing. They come to this conclusion using a theory by Kemeny (2005) who describes the Swedish rental housing market as a unitary rental market. A unitary rental market can be described as a market where non-profit and profit housing providers are competing against each other for the supply of housing. Kemeny (2005) argues that in a unitary market, profit companies need to set a rent which covers the market rate of return on equity as well as any interest on outstanding debt. While non-profit companies must also meet their debt obligations, they must not necessarily set rents which cover the market rate of return on equity (Kemeny, 2005). As a result, the financial costs of the non-profit companies are lower, and they can set lower rents than the profit companies. Vickers & Yarrow (1988) argues for the same conclusion when assessing the outcomes in a duopoly market with homogeneous goods, where one is a welfare-maximising public firm and the other is a profit maximising firm. If the public firms maximise a social welfare function, the Nash equilibrium in quantities is that the private firm operates where marginal revenue equals market price and the public firm where marginal cost equals market price. Pre-2011, not only did the MHCs not have to set the market required rate of return, but they also benefited from advantageous loan conditions both of which imply the capacity to set lower rents. However, the new law takes away both these advantages given to MHCs. As a result, it seems natural that the rents offered by MHCs should increase post-2011. These theoretical predictions are nonetheless refuted by Elsinga & Lind (2013) who argue against an increase in rents towards market prices mostly for political reasons. Gruis & Elsinga (2014) come to a similar conclusion and further mention the impact of the Swedish rent negotiation tradition as a barrier to the increase of rents in the Swedish housing market. Elsinga & Lind (2013) also posit that the law change has been detrimental to the lower-income households and contributed to a growing black market. Indeed, with less support from the government, MHCs in Sweden have started having more stringent demands with regards to the financial situation of the households whom they let to. Lind & Lundström (2007) also argue that from a game theoretical perspective, every company, MHCs and private ones alike, have an incentive to limit who can rent an apartment in order to increase the attractiveness of their housing and thereby manage to increase profits. This works directly against socially mixed neighbourhoods as lower-income groups are increasingly shifted to different forms of temporary lodgings (Elsinga & Lind, 2013).

The issue of segregation in the housing market has also been a topic in the Netherlands.

The Dutch housing market has also recently aligned itself with the EU competition policy. While the Netherlands implemented a different than Sweden, many papers have pointed towards the Dutch housing market becoming increasingly segregated (see Elsinga et al., 2008; Tasan-Kok et al. 2010; Priemus & Gruis, 2011; Van Duijne & Richard, 2018). Priemus & Gruis (2011) analyses the agreement between the European Commission and the Dutch government. In order to level the playing field between public and private housing providers, the Dutch government agreed to that MHCa allocate a minimum of 90 percent of their dwellings towards households with an annual income of less than EUR 33,000. This removed competition from the more lucrative, high-income, segment of the market and strongly reduced the scope of social housing in the Netherlands thus impeding social mix and tenure diversification in Dutch neighbourhoods.

Grander (2017) argues that the Swedish system after the law change is contradictory as it claims to be universal but its practices effectively exclude certain social groups. Grander uses surveys and in-depth case studies to identify what classifies as social responsibility among MHCs. There are three levels of social responsibilities: a good relationship between landlord and tenant; area-projects like neighbour activities; and the task of supplying housing for the general public. This last task is the main task since MHCs have an obligation to meet universal housing needs. Most MHCs claim that the law has not impacted their ability to act with social responsibility. However, around 50 percent of the companies state that the financial requirements have increased, which Grander finds conflicting with the third level of social responsibility. Westerdahl (2019) find similar results after conducting interviews with CEO:s and CFO:s at multiple MHCs. Westerdahl focuses on accounting practices and how they have limited the possibility to supply housing for all. The yield requirement (rate of return) that resulted from the law has forced the MHCs to primarily focus new production in more attractive areas, with higher rents and higher valuation. Affordable housing is suffering as a consequence of the law.

Blackwell & Bengtsson (2021) evaluates the resilience of social rental housing in the UK, Sweden and Denmark. They look at meeting housing needs, quality and security of tenure. They find that all three nations have moved from universalism towards selectivity. In the case of Sweden and the UK, they highlight that MHCs (since 2011 in Sweden), have preferred wealthier households by imposing minimum income requirements that undermine the universal approach. They find no difference between private and public rental sectors in terms of quality in any of the countries. In the UK and Denmark, security of tenure is strong whilst in Sweden it could be argued that the possibility to perform major renovations, under *bruksvärdessystemet*, can undermine tenure security.

3.2 Theoretical framework

The structure of the housing market in Sweden – whereby private companies compete with public ones – is particular but far from unique. Such markets can be analysed using mixed oligopoly models, which were first introduced by Merrill & Schneider in 1966. Since then, numerous papers have built upon this seminal work (Beato & Mas-Colell, 1984; De Fraja & Delbono, 1987; De Fraja & Delbono, 1989; White, 1996) but the original premise remains the same: public companies aim at maximizing social welfare while private companies only care about maximization of their own profits. Mixed oligopoly models typically try to figure out whether – and under what conditions – the presence of a public company in a private oligopoly would benefit social welfare. This is interesting as one could argue that the law change of 2011 led to a privatization of the housing market in Sweden, and hence a shift from a mixed oligopoly to a private oligopoly market. While the public firms still exist after the law change, their behaviour should have shifted toward increased profit-maximizing. According to De Fraja & Delbono (1989), who analyse a market where public and private companies compete using nothing but market instruments, the difference between a private and a public firm lies in the goals they pursue, and by extension, which utility function they try to maximize. In addition to the suppression of government help, the key order for the MHCs in Sweden following the law of 2011 was to act in a more "business-like" way. As previously mentioned, MHCs still have a public purpose mandate. However, this mandate has become harder to fulfil given the directives of *nya Allbolagen*. Hence, while the housing market in Sweden cannot be coined as a private oligopoly following the law change, we argue that it has certainly moved closer to one as the public mandate has taken the backseat.

We use the model of De Fraja & Delbono (1989) to analyse the dynamics of the law implementation on the Swedish housing market. We chose this paper given the fit of the theoretical assumptions with the characteristics of the Swedish housing market. First, De Fraja & Delbono (1989) solve for a Cournot competition problem with one public firm and n private ones. This also reflects the Swedish case very well since there is often only one MHC per municipality. In addition, MHCs are only allowed to operate within their municipality. Competition over prices would imply the possibility to flexibly adjust rents. However, this is not the case in the Swedish market which is characterized by rent controls and rent negotiations *(see figure 5 in Appendix)*. As a result, competition in quantities seems more fitting as it is the main lever which housing companies have access to maximize their utility function. Second, De Fraja & Delbono (1989) assume that all firms provide a homogeneous good. While there might be some differences in rental dwelling standards between private and public housing companies, Swedish law

imposes minimum quality standards to housing companies (Andersson, Magnusson, Holmqvist, 2010). Finally, the perspective of potential entrants is ruled out (De Fraja & Delbono, 1989). While this is a strong assumption, we think it is justified by high fixed costs to enter the housing construction market which act as a strong barrier to entry.

One aspect of the model which needs to be addressed is the fact that it assumes that public and private firms compete using only market instruments (De Fraja & Delbono, 1989). While this might be the case following the levelling of the competitive playing field through the law change, it certainly was not the case pre-2011 when MHCs were granted financing terms and conditions which were not available to private actors. White (1996), who studies the effect of subsidization in a mixed oligopoly setting, assumes that subsidies to public companies will not affect welfare directly as they should be included into the social welfare expression as a component of both profits of public firms and state expenditure. Under this assumption and given that private companies receive no subsidies, the subsidy does not enter the reaction function of any firm and the resulting net effect of the subsidy is zero.[3] Assuming that the advantageous financing conditions (loss coverage, below market rates on loans,...) granted to MHCs before *nya Allbolagen* are equivalent to subsidies, they should not matter in the equilibrium quantities predicted by the model. Another way to account for the fact that MHCs had access to implicit subsidies pre-2011 would be to model a situation in which we have firms competing with asymmetric costs. In a paper which studies the effect of privatization in an international mixed oligopoly with asymmetric costs, Pal & White (1998) conclude that their results would not differ substantially if they had assumed symmetric costs instead.

3.3 Theoretical predictions for the Swedish market

Under a mixed oligopoly market – which we argue was the situation of the Swedish housing market pre-2011 – De Fraja & Delbono (1989) show that, in a Cournot-Nash equilibrium, the quantities offered by a public company would be higher than those offered by a company in an oligopoly made of profit-maximizing firms only. Meanwhile, the quantities supplied by a private firm under mixed oligopoly are lower than those offered by a private firm in a private oligopoly. Since we argue that the law change of 2011 contributed to shifting the market away from a mixed oligopoly towards something

[3]We derived this result ourselves. White (1996) studies the case where both private and public firms receive a subsidy and not the case where one type of firm but not the other is subsidized. Note that if both the private and the public firm receive a subsidy, then the subsidy matters as it would enter the profit function of the private firm and thus the reaction function of the private firm. In equilibrium, the subsidy would then obviously impact the quantity produced by the public firm through the impact the subsidy has on the output of the private firm.

that resembles more to a private one, the law change should consequently mean that MHCs in Sweden build less, while private companies start building more following the law change. The net effect on quantity should be a decrease in total quantity. As far as prices are concerned, a move towards privatization should imply higher prices offered by all companies in the market.

However, we believe that not all MHCs were affected by *nya Allbolagen* to a similar extent. Ownership directives and other operational circumstances are likely to differ between municipalities so it would be unrealistic to believe that the law would impact everyone equally. As we will argue for under the *Methodology* section, some MHCs already acted in a business-like way prior to the introduction of *nya Allbolagen*. Our analysis is based on a difference-in-differences approach whereby the treated group is made up of those municipalities where MHCs did not act in a business-like way prior to 2011. We expect to see a decrease in the quantity of newly produced dwellings by MHCs in the treated municipalities following the law change. In those same municipalities, the quantity produced by private housing companies should increase. Meanwhile, municipalities in our control group are defined as already having acted in a business-like way before 2011. In this case, our theoretical model does not help us predict the effect of *nya Allbolagen*, since the market structure will virtually remain unchanged after 2011. However, this is of little importance since we are mainly interested on studying the effect of *nya Allbolagen* on those municipalities which were affected by it.

4 Data

In this thesis, we aim to analyse the effect of *nya Allbolagen* (2011), on the quantity of rental housing supplied by MHCs. The outcome variable is newly constructed rental dwellings by MHCs. The independent variable of interest is an indicator capturing whether a municipality has been affected by *nya Allbolagen* or not. Furthermore, we use up to four control variables depending on the model specification: population, house prices, unemployment rate and the share of people with an aggregated gross salary lower than 1.5 base amounts. We use yearly panel data at the municipality level for 241 municipalities in Sweden, which corresponds to 91% of the municipalities that had an MHC and 83% of all Swedish municipalities. The data was retrieved from Statistics Sweden (SCB) for all our variables except for the treatment variable which was constructed using the Serrano database provided by the Swedish House of Finance (SHoF). All data from SCB is measured yearly at the municipality level while the data from SHoF is measured yearly at company level. Given that MHCs can only conduct their operations in their respective municipality, we were able to use company level

data while retaining a panel format at municipality level. Company level data includes information on 261 MHCs and 185 private housing companies over the period 1998-2020. Municipality level data was available for the years 1991-2020, except for unemployment rate and share of people with an aggregated gross salary lower than 1.5 base amounts, where the data starts in 1997 and ends in 2020.

4.1 Dependent variable

The dependent variable is the number of newly constructed rental dwellings built by MHCs in each municipality for each year. It is discrete (or count) data. As emphasized in the introduction of this thesis, we are focusing on rental dwellings. The statistics offered by SCB does not allow to extract data on the amount of newly built *rental* dwellings by MHCs, only the total number of dwellings built by MHCs. However, MHCs almost exclusively build rental dwellings. Hence, we use the total quantity of newly built dwellings by MHCs as a proxy for the number of rental dwellings built by MHCs. This is particularly important for the choice of some of our control variables, since we believe that demand for rental dwellings could be a influencing factor.

Given that there can be substantial differences in the quantity of newly built dwellings between big and small municipalities for any given year, we decided to log transform this variable. The transformation facilitates comparison across municipalities and eases the interpretation of the results. In addition, absent transformation, the residuals are heteroscedastic. The log transformation thus provides the added benefit of helping reduce heteroscedasticity. Finally, OLS regression assumes that the dependent variable is continuous. The logarithmic transformation changes our discrete outcome variable to a continuous one and further helps fulfil the OLS assumptions. Given that the number of new rental dwellings built by MHCs for a given year can take on the value zero and that this value is a normal count value, it is important not to lose those zero values. We thus transformed the dependent variable using a log(y+1) transformation as suggested by MaCurdy & Pencavel (1986). We argue that whether we assume the municipality to build an additional dwelling per year (y+1) should not substantially change the interpretation of the result. In particular, there should be little difference between a municipality which does not build at all in a given year and one that builds just one dwelling. Furthermore, given that the logarithm of 1 equals zero, the nil values will remain coded as such.[4]

[4]Assume a dependent variable which is the number of visits a person has paid to the doctor in a given year. In that case the difference between zero and one visit could indicate whether a person has been sick or not. Transforming zeros to ones could be problematic as one would be assuming that some perfectly healthy people were not. We don't believe there to be such a stringent difference between an MHC which, in a given year, has not built at all and one who has built just one dwelling.

4.2 The Difference-in-differences estimator

The independent variable of interest is the difference-in-differences estimator. In typical DiD settings with pooled cross-sectional data, it is the result of the interaction of two dummy variables. A first dummy which captures treatment and second one which captures time periods after the policy/law change. Given that we are using panel data, we do not need to interact terms and can instead use a straight dummy which takes on a positive value if the municipality observed was affected by *nya Allbolagen* and the year is 2013 or after. We call this variable the DiD indicator. Even though *nya Allbolagen* was enacted as of January 1st, 2011, the time dummy takes a positive value from the year 2013 onward. Discussions with professionals from SCB and Motala´s MHC were unanimous about the fact that a building project takes on average 18-24 months to complete. To accommodate for this, we choose 2013 as the intervention year. This assumption is furthermore tested under the next section.

Given that *nya Allbolagen* was implemented nationwide, all MHCs are theoretically affected by the law. We thus had to construct our DiD indicator using an alternative measurement of treatment rather than simply assigning treatment to municipalities where the law was implemented. [5] We used financial data - more specifically the debt-to-equity ratios - to identify treated municipalities. We expand more on the choice of treatment and control group in the next section.

Using company level data to construct our independent variable could be problematic for our municipality level panel data set. However, only one MHC exists per municipality in most cases, so that the observed output per municipality corresponds to the output produced by the MHC we had financial data on. In a few municipalities, more than one MHC exists. In those cases, we used the median debt-to-equity ratio of those MHCs to assign treatment at the municipality level. MHCs in the same municipality all share the same owner and have the same or similar ownership directives, making them very alike. Finally, the operations of MHCs are restricted to their respective municipalities. Hence, data at the MHC level is representative of what is happening at the municipality level.

4.3 Control variables

Our control variables were chosen so as to control for possible systematic differences between the treated and the control group that could explain differences in the quantity

[5] This would have led to all municipalities in our sample being treated

of rental dwellings being built. The amount of newly built dwellings we observe for one year stems from a decision taken years prior. As previously argued, the lag amounts to around 2 years. We therefore lagged all our control variables so that the output in e.g. 2010 would correspond to the values of a control variables in 2008. The idea is also to stay consistent with the choice of 2013 as treatment year.

4.3.1 Population

We control for population since population size and population changes are likely to explain the quantity of new housing being built. It is especially important to lag this variable as population levels in a given year could be, to some extent, explained by the number of newly built rental dwellings in that same year (assuming people move as a result of the newly available housing supply). However, populations levels from two years ago cannot be explained by the current number of newly built dwellings. The variable was, as our dependent, log transformed to account for the large variations between the municipalities.

4.3.2 House prices

House prices were included for three reasons. First, higher house prices in a municipality increase the value of buildings owned by the MHCs and make their debt-to-equity ratio lower since equity (buildings) appreciates and debt is nominal. Being less leveraged, i.e., having a lower D/E makes it is easier to fund new housing projects without increasing your risk exposure. This variable thus captures a potential explanation of our independent variable. Second, higher housing prices indicate a more attractive market, which people are also more likely to move to thus increasing demand for housing. Third, the higher house prices could also act as financial incentive for the MHCs to build more. Data on house prices was only available for independent houses, but the idea is to capture the general price level so that should not be a concern.

4.3.3 Unemployment rate

The unemployment level in a municipality indicates if there is a strong labour market, in either the municipality itself or in proximity to it. Demand for personnel tend to be higher with lower unemployment and people are thereby more likely to relocate to municipalities with strong labour markets. The opposite is true for high unemployment. An additional feature of low unemployment is the potential increase in the share of

people with income coming from labour as opposed to unemployment benefits. The risk associated with construction is lower when more of the potential demand is fiscally solvent. A higher supply of rental dwellings could, however, also be driven by a higher unemployment rate. More unemployment means lower financial means and, consequently, less demand for more expensive alternatives to rental dwellings, such as houses, dwelling ownership or the black market.

4.3.4 Share of people with low income

Low income in this case is defined by having an aggregated gross salary lower than 1.5 base amounts. The share is measured among the population aged 20 to 64. The base amount is an income index which is used by the government to decide different financial metrics, like transfers and pensions (Regeringen, 2022). The median salary, in terms of base amounts, was around 5 during the period. This variable was preferred to a median income variable. Median income and house prices had such a strong correlation that we would be exposed to multicollinearity if both had been included. We also believe house prices to be a better control as it accounts for more than income.

The trend for the last decades has been that rental dwellings are primarily occupied by people with lower incomes (Hyresgästföreningen, 2022). As argued for unemployment, this is possibly due to lack of capital to pursue alternative housing solutions. MHCs have the mandate to build for all income classes but demand for their housing solutions is likely driven by people with lower incomes. A shift in the share of people with lower incomes in a municipality could increase or decrease the demand for rental dwellings and by extension, construction. This is a particularly important metric to consider if one assumes that the MHCs still serve their societal objective.

A higher share of low income people could also lead to fewer rental dwellings being built. Having tenants with lower income makes the risk associated with the investment bigger because the likelihood of personal default increases. A developer could also more easily build apartments with higher standards according to bruksvärdessystemet (see Historical background) and introduce higher rents if the income is higher.

4.4 Descriptive statistics

Table 1 shows the group means for the treated and the control group, before and after the law. In RCTs, it is important that the two groups are similar, indicating a successful randomization. In a DiD setup, this is not the case. However, if the structural differences

are not stable over time it hurts the validity of the regression. It becomes less likely that the difference in outcomes stems from the treatment when the covariates, that could potentially explain the outcome, diverge from their previous, similar, trajectory (Simon, Wing, & Bello-Gomez, 2018).

Table 1: Balancing Table

	2010		2020		%	
Group:	Treated	Control	Treated	Control	Diff(T)	Diff(C)
Population	16900	74700	17750	84600	13	5
HS.P	48	47	50	47	4	0
S.LI	11.4	11.6	8.0	8.4	-30	-27
HP	1007	2002	1674	3237	66	62
Unemployed	16.5	15.4	13.5	15.0	-18	-3
D/E	14.7	3.03	6.57	2.33	-55	-23
Dwellings	386	1761	320	2753	-17	55
Threshold	20	20	20	20	20	20

Notes: Diff = % change
S.LI = Share of population (%) with low income.
HS.P = Housing Stock/Population.
HP = House prices in KSEK.
D/E = Debt-to-equity ratio
Dwellings show the average for the period 1991-2011 & 2012-2020

We can observe that the treated and the control group have mostly developed in the same direction but with a different pace between 2010 and 2020. This could potentially be an issue for our difference-in-differences interpretation since it is obvious that the two groups have not developed equally. Noteworthy is that D/E and the number of newly built dwellings have developed in line with our theoretical predictions.

Figure 2 graphically displays where in Sweden the control and treated municipalities are situated. From the map it is easy to infer that the different groups are spread out across the country, even if the control group is more concentrated around the Stockholm area.

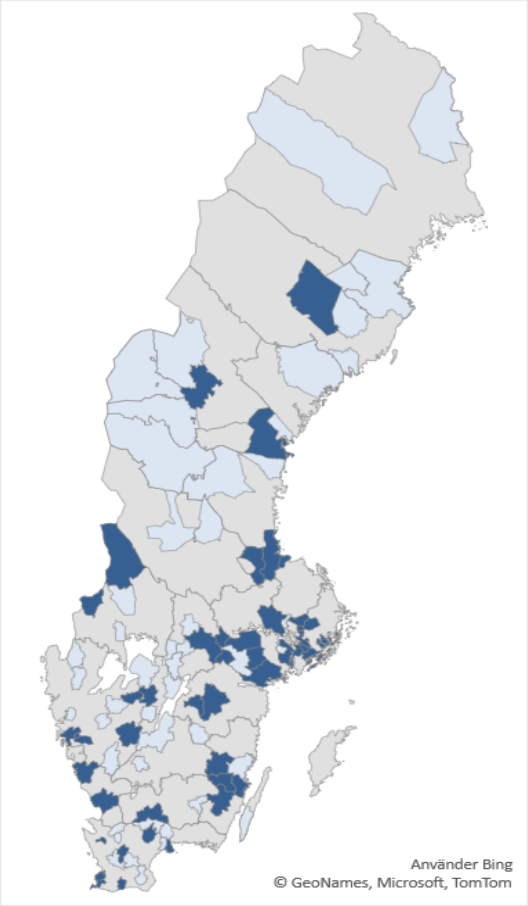

Figure 2: Spatial representation of treatment and control groups. Dark blue shows municipalities in the control group and light blue shows those in the treated group

5 Methodology

To analyse the impact of *nya Allbolagen* on the supply of rental dwellings constructed by MHCs, we conducted a fixed effects difference-in-differences analysis. The difference-in-differences analysis has been a popular choice across the literature studying the effect of policy changes (see Kruger & Card, 1994; Card, 1990; Galiani et al., 2005). In its most basic and widespread version, the DiD usually makes use of two time periods and pooled cross-sectional data. In our case however, we have data in the panel form, i.e., the unit of observation (which is the municipality in our case) does not change over time. This data format allows for more flexibility when choosing our model. In particular, panel data allows us to control for municipality fixed effects which might otherwise not have been captured and would help to alleviate omitted variable bias. In other words, using panel data, we can control for unobserved municipality-level effects which are time-invariant and impact our outcome variable. More detailed information

about the dependent, core independent and control variables used in this section can be found in the previous section.

5.1 A fixed effects difference-in-differences model

Our model consists of two parts. The difference-in-differences implies a comparison of the outcome variable between our treatment group, which is affected by the law, and our control group, which we argue is not. The DiD tries to capture changes in the differences in quantity of new dwellings built by the two groups by comparing them before and after the law change. The fixed effects part simply means that we are controlling for panel i.d. (municipality) unobserved effects which do not vary over time. The baseline model looks as follows:

$$y_{mt} = \beta_0 + \beta_1 Treat_{mt} + \theta_t + a_m + \varepsilon_{mt} \tag{1}$$

where y_{mt} is the outcome variable of interest, i.e., the number of new rental dwellings built by a given MHC in a year. This variable has undergone logarithmic transformation such that $y_{mt} = log(y_{mt} + 1)$. $Treat_m$ is the dummy which indicates whether, for each year, the municipality observed was treated or not. This is our DiD indicator as it captures the effect of the law on our outcome variable. Note that this dummy will take a positive value if the associated observation is linked to a treated municipality *and* the year is 2013 or later. a_m captures the municipality fixed effects. Finally, θ_t is a vector of yearly time dummies which captures yearly fixed effects. These coefficients will help capture effects which vary over time within municipalities.

This model is then expanded to include a first set of control variables (the logarithm of municipality population and the average house prices in a municipality). The resulting model, including controls, is defined as follows:

$$y_{mt} = \beta_0 + \beta_1 Treat_{mt} + \theta_t + a_m + \gamma_1 \mathbf{x}_{mt-2} + \varepsilon_{mt} \tag{2}$$

where $\mathbf{x}_{mt-2}$ is our vector of control variables (small set).

The models specified in equation (1) and (2) are analysed over the time period stretching from 1991 to 2020. We chose 1991 as our starting year since that was the first year we had data on and we wanted to take advantage of a longer period and resulting higher number of observations.

Two more control variables were defined in the previous section: the share of population with a low income as well as the unemployment rate. Given that data for these two controls was only available from 1997 onward, we specify a third model for which the analysis will be restricted to the time period 1997-2020:

$$y_{mt} = \beta_0 + \beta_1 Treat_{mt} + \theta_t + a_m + \gamma_1 \mathbf{X}_{mt-2} + \varepsilon_{mt} \tag{3}$$

where $\mathbf{X}_{mt-2}$ is a vector including our full set of control variables (logarithm of population, house prices, share of population with low income, and unemployment).

5.2 Identification strategy

Two key prerequisites need to be met by any DiD model for it to yield unbiased estimates. First, a good control group needs to be defined. The control group should share similar characteristics with the treated group and be unaffected by *nya Allbolagen*. Second, the control and treatment groups should share parallel trends in the outcome variable during the pre-treatment periods.

5.2.1 Defining the control group

For our analysis to be consistent, the control and treatment group should share similar characteristics. In addition, it is necessary that the control group remains unaffected by the treatment. In this specific case, finding a good control group proved to be a challenge since every MHC was, in theory, affected by the law. While private companies are not directly affected by the law, using them as a control group was not an option since they differ substantially from MHCs. Not only do they have different goals than MHCs, but they also faced different baseline conditions in the housing market, notably in terms of how they finance their housing projects. In addition, since the initial complaint to the European Commission came from the Property Owners Foundation, it seems reasonable to assume that private housing companies felt that a law change would create competitive advantages for them. It thus becomes difficult to argue that they would, in fact, be unaffected by the law. Hence, our solution was to identify those MHCs which, prior to the enaction of *nya Allbolagen*, acted as if they were private companies already. Those MHCs could then be considered essentially unaffected by the law and constitute a control group which would share many of the similar characteristics and market conditions of the treated group. The key difference between the treated and the control group is that the MHCs in the control group acted in a business-like way before

they were obliged to. This is also in line with the survey results from Boverket [6] where a majority of the MHCs claimed that the law did not greatly impact their operations. One might think that it is odd to dismiss private companies as a control group and instead choose public companies that were acting as private. It is important to remember that MHCs are restricted to one geographical market and the competitive landscape does not change depending on how other MHCs choose to act in their respective markets.

5.2.2 Using Debt-to-equity ratios to identify companies acting as private

We identified the MHCs that acted in a business-like way before the implementation of *nya Albollagen*, i.e., the MHCs that would be part of the control group, using their debt-to-equity ratios (D/E). The D/E ratio is calculated by dividing the total debt of the company with the equity, which is found by subtracting total liabilities from total assets. This ratio tells you how much financial debt a company has in relation to its equity and is a good indicator of how much exposure to risk a company takes on, whereby a higher D/E means you are more leveraged. A change in business cycle, an interest rate hike or anything similar would make it increasingly harder to service your debt. D/E is highly relevant for all housing companies, and mostly irrelevant of size. How much risk a business decides to take on is an operational question which is faced by businesses of any size everywhere. Furthermore, real estate projects are cash intensive, which implies that debt financing is highly relevant in this sector. While more leverage implies more risk, it also leads to higher potential profit since it allows a company to expand its business operations in a cheap way. In a stylized example where risk considerations are completely absent, a company should thus take on as much debt as possible to finance its operations.

Prior to *nya Allbolagen*, MHCs could operate in an environment which resembles the previously mentioned stylized example. Not only were the loans granted to MHCs guaranteed by the municipalities, but the municipalities also covered losses realized by MHCs. This meant that MHCs did not have to make appropriate risk-considerations regarding their debt-to-equity ratio, incentivizing them to take on excessive amounts of debt. In addition, MHCs also benefited from preferential interest rates on loans, thus increasing the attractiveness of debt even further. This led to a classic moral hazard situation where the MHCs were incited to increase their exposure to risk because the municipalities ended up bearing most of the risk associated with debt financing. As a case in point, Haninge municipality had to borrow one billion SEK from the government to prevent its municipal housing company from defaulting on existing loans after the

[6] See introduction

Swedish housing bubble burst in the mid 1990s (Malmborg, 1993). On the other hand, a private housing company is less likely to take on such excessive amounts of debts since it is accountable to itself. It follows that MHCs should, on average, have a higher D/E than private housing providers. This is confirmed by the data. Over the period stretching from 1998 to 2010, the 261 MHCs in our sample had a median D/E of 8.23 which contrasts with the median D/E of 5.69 among the 185 private housing companies. These private companies had the same SNI-code as our MHCs, which is the Swedish Standard Industrial Classification, and tells you which type of industry the business primarily operates in (SCB, 2022b). [7]

The enaction of *nya Allbolagen* led to a swift ending of the advantageous financial conditions which MHCs benefited from, pushing them to act as if they were privately run companies.[8] This implies that MHCs had to readjust their risk exposure and, by extension, their D/E. However, not all MHCs took on excessive amounts of debt before the implementation of *nya Allbolagen*. We argue that companies with lower D/E ratios prior to the implementation of the law already acted in line with what can be expected from private companies and hence in accordance with the objective of the law. Consequently, MHCs with low D/E levels prior to the implementation of the law serve as our control group. Even though the control group was theoretically also subject to the law change, we argue that this was not really the case in practice or at least to a much smaller extent than the treated group. For example, the payment of guarantee fees - which MHCs are subject to in order to compensate for lower interest rates - disproportionately affects the more indebted MHCs. Furthermore, D/E also captures how an MHCs chooses to operate. If you cater to your societal objective and build for people with lower incomes and in less attractive areas, your holdings, i.e., your equity is effectively going to be worth less and your D/E ratio is going to be higher – under the assumption that debt is constant. MHCs that acted in a business-like way prior to the law are thereby more likely to have made risk-adjusted considerations, which should be reflected by lower D/E-ratios.

5.2.3 Allocation of municipalities to the control group

As previously shown and argued, private housing companies do have a lower D/E than MHCs, on average. However, a private housing company is free to take on high amounts of debt if it wishes to and there is no law or rule enforcing an absolute D/E on private companies. Deciding which MHCs, and by extension which municipalities, to allocate to the control group proved to be a tricky exercise: what D/E level is considered

[7] The code is 68.201 and the group is labelled "Renting and operating of own or leased dwellings.
[8] See section 2.4 for a more detailed discussion of the implementation of *nya Allbolagen*.

in line with what one could expect to see among private companies and where is the limit?

Answering this question naturally comes with endogeneity issues. To try and mitigate this, we followed a practice that is commonly used in papers which compare outcomes between subjects stemming from poor as opposed to rich backgrounds (see Burton, Phipps & Zhang, 2013). These studies face a similar issue in that they need to define who is poor and who is rich. The proposed solution consists of comparing top and bottom percentiles of the group that is subject to segmentation. In our case, it can safely and reasonably be assumed that those municipalities with D/E in bottom percentiles are likely to follow business practices of private businesses while the top percentiles are not. We conduct our main analysis by comparing the top and bottom *quintiles* [9]. The sensitivity analysis in section 7 repeats the analysis for different percentile splits. This method allows us to avoid an arbitrary division where we would end up comparing municipalities which are very similar with regards to D/E levels but yet would not belong to the same group.

The D/E distribution from which the percentiles were drawn was constructed using the median D/E over the period 1998-2010 for each MHC. The choice of the median was motivated by the fact that D/E is uncapped and sometimes yielded unreasonably high values. At this point, it should be kept in mind that we are identifying our control group based on company level data while our analysis is conducted at the municipality level. In most cases, municipalities only have one MHC so the company level data of the MHC is fully representative of the municipality level data on newly constructed rental dwellings. In addition, MHCs can only conduct business in their municipalities so no overlaps are possible. However, there are a few examples of municipalities with multiple MHCs, for example Stockholm. In those cases, we chose to remain consistent and to take the median of the medians (of the different MHCs) as our final value for the municipal D/E. Given that municipalities prescribe operational ownership directives which apply to all MHCs in their municipality, MHCs in a given municipality should have similar debt-to-equity ratios.

5.2.4 The parallel trend assumption

The parallel trend assumption is necessary in a difference-in-differences since it enables you to construct a counterfactual outcome which assumes that, in the absence of treatment, the treated group would have been on the same growth trajectory as the control

[9] A quintile represent 20% of a given population. Municipalities belonging to the top and bottom quintiles are represented spatially in figure 2 and listed in Appendix A.3.

group. The trends for the control and treated groups are presented graphically below for the period 1991-2020. Prior to the implementation of the law, we observe trends which are somewhat parallel. After the implementation of *nya Allbolagen* however, the trends in newly built rental dwellings diverge. It might be surprising to observe that the trends were parallel for companies with such different D/E, i.e., operating in different ways. However, in a weaker market, you could produce similar, trend-wise, output as MHCs in stronger markets by not accounting for risk and taking on excessive debt. This is a possible explanation for the very similar trends observed prior to the implementation of the law.

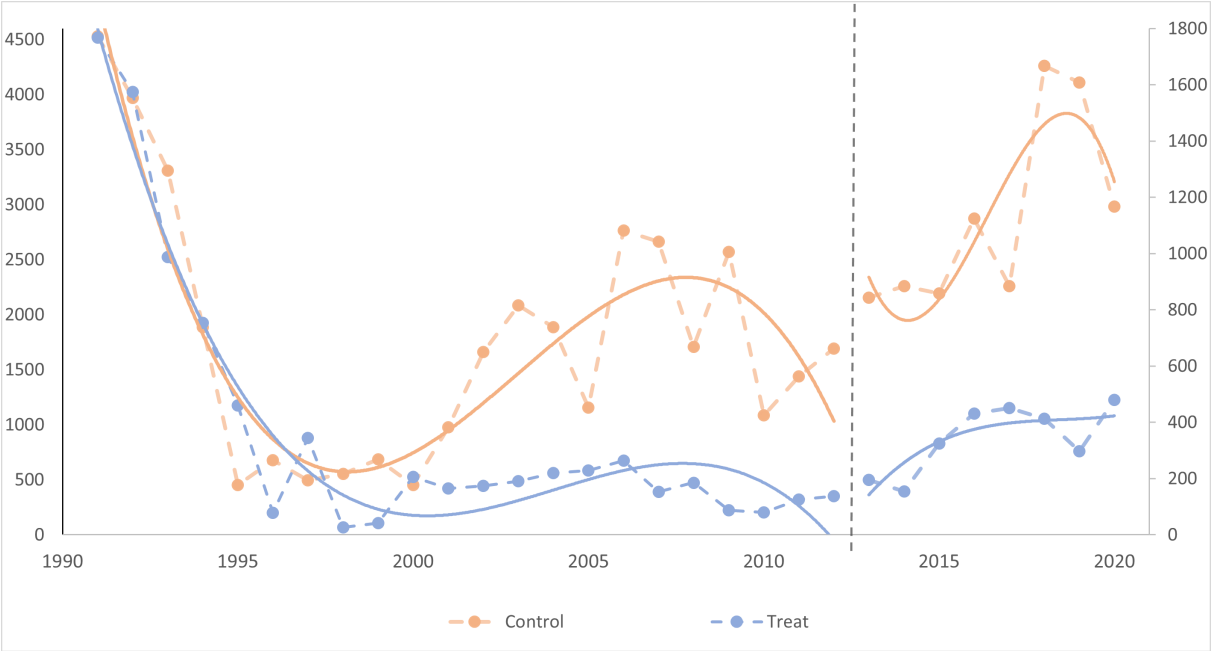

Figure 3: Pre-treatment trends in the outcome variable for the 20/20 split. The scale of the treated and control group are found on the right respectively the left side on the graph. This is done to better capture the trends despite the difference in magnitude.

Parallel trends remain a strong assumption which can easily shift from holding to breaking down based on, for example, the reference period which is being used. The parallel trends displayed in Figure 3 are a perfect example of this. Up until 1996, the trends between control and treatment group follow each other almost perfectly. However, starting 1997, the parallel trends are much murkier whereby they seem to hold for certain periods but not for others. A graphical analysis is one of multiple diagnostic checks which can be run to ensure that the parallel trends assumption holds. Another commonly used diagnostic check is known as the Granger causality test. The idea is to test whether the law change had anticipatory or lagged effects before by allowing for hypothetical treatment in every single year separately. This can be done using the following regression:

$$
\begin{aligned}
y_{mt} = &\beta_0 + \beta_1(Treat_m \times t1991_t) + \beta_2(Treat_m \times t1992_t) + \ldots + \\
&\beta_{19}(Treat_m \times t2009_t) + \beta_{20}(Treat_m \times t2011_t) + \ldots + \\
&\beta_{28}(Treat_m \times t2019_t) + \beta_{29}(Treat_m \times t2020_t) + \theta_t + a_m + \varepsilon_{mt}
\end{aligned} \tag{4}
$$

Note that the $Treat_m$ variable is slightly different from before as it does not incorporate any time indicator. The time specification now instead comes from the interaction with the yearly dummies. The year 2010 was left out since it serves as base/reference year. This specification can then easily be modified to include controls. Note that since the Granger test allows us to capture anticipatory as well as lagged effects of the law, the equation specified in 4 assumes treatment in 2011 unlike the specifications 1-3 which assume treatment in 2013. Summary results of the regression model specified in 4, as well as its extensions including control variables are displayed in Table 2. The full table including all time periods as well as the coefficient for controls can be found in the appendix.

Looking at the results, an anticipatory effect of treatment cannot be excluded as shown by the significant coefficients for treatment in 2007 (DiD07) across all three models. These results are not a good sign for our parallel trends assumption. As expected, the law does not have any immediate effect; none of the 2011 coefficients are significant. As a matter of fact, we only start seeing significant effects appear by the year 2013, with the effects being strongest in 2014. This comforts our choice of 2013 as the first treatment year, which was motivated by the average time it takes to see a construction project through. The observed effect, which lags the law implementation by two years seems to fade away over time before picking up again for the years 2018 and 2019. Even though not all the coefficients after the law implementation have significance, they have the expected negative sign across all three models.

Table 2: Granger Causality Test

Controls:	None	2	4
DID07	-0.799**	-0.811**	-0.824**
	(0.354)	(0.352)	(0.354)
DID08	-0.163	-0.171	-0.215
	(0.333)	(0.333)	(0.334)
DID09	-0.298	-0.302	-0.336
	(0.322)	(0.323)	(0.327)
DID11	-0.186	-0.179	-0.217
	(0.330)	(0.329)	(0.332)
DID12	-0.396	-0.386	-0.407
	(0.390)	(0.391)	(0.396)
DID13	-0.903**	-0.888**	-0.874**
	(0.395)	(0.394)	(0.399)
DID14	-1.124***	-1.104***	-1.09***
	(0.329)	(0.327)	(0.331)
DID15	-0.517	-0.493	-0.482
	(0.356)	(0.350)	(0.352)
DID16	-0.559	-0.532	-0.511
	(0.395)	(0.396)	(0.398)
DID17	-0.172	-0.143	-0.109
	(0.355)	(0.358)	(0.356)
DID18	-0.888**	-0.856*	-0.839*
	(0.432)	(0.442)	(0.435)
DID19	-1.03***	-0.996***	-0.977**
	(0.374)	(0.380)	(0.382)
DID20	-0.375	-0.335	-0.286
	(0.412)	(0.412)	(0.413)
Observations:	2880	2688	2112
Group Split:	20&20	20&20	20&20

Notes: Robust standard errors are presented in parenthesis below the Difference-in-differences estimator.
*** p<0.01, ** p<0.05, * p<0.1

Taking the graphical and Granger analysis together, our initial model specification does not appear to fulfill the key requirement of parallel trends. Hence, we further enhance our model to include linear time trends for every municipality. The goal of the linear time trends is to capture the slope differences in the outcome variable between our treatment and control groups, or in our case, between every individual municipality. The baseline model - which is equivalent to the model specified in equation (1) but accounts for linear time trends at the municipality level - looks as follows:

$$y_{mt} = \beta_0 + \sum_{n=1}^{m-1} \lambda_n (a_n \times t) + \beta_1 Treat_{mt} + \theta_t + a_m + \varepsilon_{mt} \tag{5}$$

where the interaction term $(a_n \times t)$ adds a linear time trend to every municipality. It does so by multiplying the time trend with municipality dummies. The λ_n are then the corresponding municipality specific trend coefficients. Given that a baseline municipality needs to be excluded, there will be $(m-1)$ coefficients, where m is the number of municipalities included in the regression. The choice of the baseline group does not impact the coefficient of interest β_1. The rest of the specification is unchanged from our original model presented in equation (1). We apply the same time trends to the models specified in equations (2) and (3) which include controls:

$$y_{mt} = \beta_0 + \sum_{n=1}^{m-1} \lambda_n (a_n \times t) + \beta_1 Treat_{mt} + \theta_t + a_m + \gamma_1 \mathbf{x}_{mt-2} + \varepsilon_{mt} \tag{6}$$

$$y_{mt} = \beta_0 + \sum_{n=1}^{m-1} \lambda_n (a_n \times t) + \beta_1 Treat_{mt} + \theta_t + a_m + \gamma_1 \mathbf{X}_{mt-2} + \varepsilon_{mt} \tag{7}$$

The parallel trends for the model specified in equation 5 are displayed in figure 4. Allowing for municipality specific time-trends has substantially improved the trends which now move in a parallel fashion for every pre-treatment period. In addition, running a parallel-trends test with null hypothesis of parallel linear trends for the pre-treatment period cannot be rejected at any conventional significant level across all specifications. Note that these parallel trends are computed using the logarithm of y and that the control and treatment groups correspond to the municipalities belonging to the bottom, respectively top quintiles of the D/E distribution. The same graphical analysis conducted for the models specified by equation 6 and 7 can be found in the appendix. While we will still present results using the specification of equations 1-3 which do not include linear trends, the specifications including linear time trends presented in equation 5-7 will serve as our workhorses.

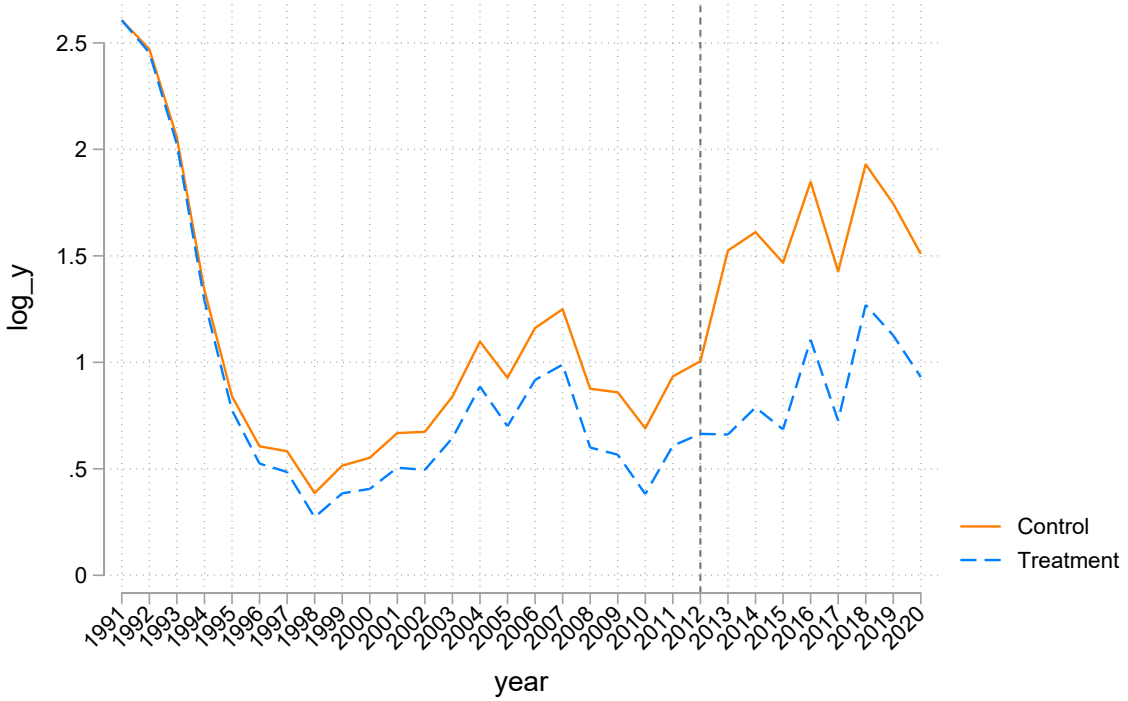

Figure 4: Parallel trends with linear time trends for the model without controls.

5.3 Standard errors

Even though the logarithmic transformation of the dependent variable substantially reduces the variance of the residuals in our OLS analyses, it still leads to a rejection of the null hypothesis of constant variance (homoscedasticity) when conducting a Breusch-Pagan/Cook-Weisberg test for heteroscedasticity. In the interest of being conservative, we report robust standard errors for all our model specifications since they are consistently bigger than non-robust ones in our analysis.

6 Main results

6.1 Believing the parallel trends assumption

The results of the models specified in equations 1-3 are summarized in Table 3.[10] All model specifications indicate that *nya Allbolagen* had an effect on the quantity of newly built rental dwellings supplied by MHCs in our treatment group at the 1% significance level. Increasing the number of control variables decreases the size of the effect but does not affect the significance. These results should nonetheless be taken with a grain of

[10]Detailed results tables for all regressions run hereafter can be found in the Appendix A.5.

salt since they assume parallel trends in the outcome variable between our control and treatment groups for the pre-treatment period.

Table 3: Effect of the law on newly built dwellings (no time trends)

Model: Years:	(1) 1991-2020	(2) 1991-2020	(3) 1997-2020
DiD	-0.550*** (0.136)	-0.470*** (0.138)	-0.403*** (0.142)
Controls	None	2	4
Observations	2880	2688	2112
Adj. R-squared	0.139	0.080	0.059
Mun. per group	48	48	48
Group split:	20&20	20&20	20&20
Time trends:	No	No	No

Notes: Robust Standard errors are presented in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Model 1 in table 3 includes no control variables at all and observations are made over the period 1991-2020. The coefficient of the DiD indicator, which indicates whether a municipality was treated and the observation made during a year post-treatment, is -0.55. This implies that a municipality which was in the top quintile with regards to its debt-to-equity ratio built 42%[11] less over the post-treatment period than it would have if it had been following the trend in construction set by the municipalities with D/E in the bottom quintile. The controls accounting for population and housing prices are included in Model 2. This specification leads to a smaller effect of *nya Allbolagen* on our outcome variable, whereby the treatment group built 37% fewer buildings than it would have absent treatment. Finally, further controlling for the share of population with low economic standard and for unemployment rate decreases the effect size to 33%.

In the previous section, we argued that the results observed over the entire post-treatment period were most likely to be driven by the effect of the law for the years 2013 and 2014. This is confirmed by the results displayed in Table 4 which reproduce the exact three model specifications which were presented in Table 3 but limit the

[11]The effect of a dummy switch from 0 to 1 in a log-linear model is calculated using 100[exp(DiD) - 1]

post-treatment period to the years 2013 and 2014.

Table 4: Effect of the law on newly built dwellings (no time trends)

Model: Years:	(1) 1991-2014	(2) 1991-2014	(3) 1997-2014
DiD	-0.868*** (0.218)	-0.738*** (0.220)	-0.709*** (0.220)
Controls	None	2	4
Observations	2304	2112	1536
Adj. R-squared	0.166	0.083	0.040
Mun. per group	48	48	48
Group split:	20&20	20&20	20&20
Time trends:	No	No	No

Notes: Robust Standard errors are presented in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Comparing the results from Table 4 and Table 3, we see that the coefficients for every specification have increased, in absolute terms, when limiting the post-treatment years. The increase is substantial as well. Over the period 2013-2014 only, a treated municipality built almost 58% fewer buildings than it would have if it had not been treated. For Model 2 and Model 3 the magnitude of the effect amounts to 52% respectively 51%.

6.2 Including linear time trends

We now move on to the models specified in equations 5-7. These model specifications include linear time trends which are important to make our parallel trends assumption believable. Again, all three models are presented for the entire post-treatment period, as well as the shorter (2013-2014) post-treatment period. The results are presented in Table 5.

None of our models which analyse the impact of *nya Allbolagen* for the entire post-treatment period are statistically significant. In addition, even though the coefficients still have the expected sign, their size has substantially dropped. This is especially

Table 5: Effect of the law when including linear time trends

Model:	(5)	(6)	(7)	(5)	(6)	(7)
Years:	91-20	91-20	97-20	91-14	91-14	97-14
DiD	-0.345	-0.268	-0.220	-0.671**	-0.620**	-0.489**
	(0.219)	(0.200)	(0.203)	(0.262)	(0.261)	(0.276)
Controls	None	2	4	None	2	4
Observations	2880	2688	2112	2304	2112	1536
Adj. R-squared	0.161	0.102	0.076	0.199	0.102	0.049
Mun. per group	48	48	48	48	48	48
Group split:	20&20	20&20	20&20	20&20	20&20	20&20
Time trends:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust Standard errors are presented in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1

true for Model 3 where the treatment indicator is almost half what it was in the model without linear time trends. This result is no surprise as we did not expect the parallel trends to hold for the model specifications presented earlier. A violation of parallel trends produces biased estimators. In our case, it seems that the impact of the new law on the decrease in quantity of newly built rental dwellings was overestimated by the models 1-3.

The analysis becomes interesting when looking at the last three columns of Table 5. These are the results we get when limiting the post-treatment period to the years 2013-2014. Compared to our analysis which did not include time trends, the size of coefficients as well as the significance have decreased. Still, all the coefficients are significant at the 5% level. The effects are still substantial too. The results tell us that, as a result of treatment, MHCs in the treated group built between 39% and 49% fewer dwellings, depending on the model specification, than they otherwise would have. This corresponds to a deficit of between 168 and 112 dwellings per year in the treatment group for the period 2013-2014. As we hypothesized, it thus seems that *nya Allbolagen* had an effect on the quantity of new dwellings built by MHCs with high D/E ratios. However, this effect seems to have lasted only for two years after the law started having an impact on the construction of rental dwellings by MHCs. In all likelihood, the effect then faded away as time passed, although as can be seen from the Granger analysis conducted in Section 5, the law might have had effects in individual years after 2014. Potential explanations for this fading effect will be suggested in our discussion section.

7 Further analysis

This further analysis section is divided into three parts. First, we conduct sensitivity analysis to test the robustness of our results, especially with regards to our identification strategy. Second, we model the relationship between our outcome variable and treatment variable using a generalized linear model, which is better suited for outcome variables in the count format. Finally, we try to capture the spillover effects which the law might have had on the supply of housing by private actors.

7.1 Sensitivity analysis

The goal of this section is to provide robustness to our identification strategy. More specifically, we focus on the assignment to treatment and control based on the D/E ratio which is not without its issues. In a first step, we vary the percentiles which define whether a municipality belongs to the treated or control group, or whether they are excluded completely from the analysis. In a second step, we exclude cities and metropolitan areas sequentially from our analysis.

7.1.1 Varying the threshold groups

In our baseline specification, we assigned municipalities belonging to the top and bottom quintiles in terms of D/E to, respectively, our treatment and control groups. Meanwhile, other municipalities were left out of the analysis. While we argued for why this method makes sense, there is still a possibility that the split we chose was not completely random with regards to other municipality characteristics which we do not account for. If that is the case, we would not be capturing the effects of the law change.

To account for this possibility, we ran the model specifications defined in equations (5), (6) and (7) again, but varying the percentile splits which assign a municipality to one of the groups.[12] First, we study the effect of *nya Allbolagen* when comparing municipalities in the bottom 10^{th} and top 10^{th} percentile. Once more, we conduct the analysis for the entire as well as the 2013-2014 post-treatment period. The results are shown in Table 6.

If our rationale was correct, i.e. the municipalities with the highest D/E should be the ones most affected by the law while those with the lowest D/E should be least affected, then this analysis should yield bigger effects than our baseline specification

[12] The municipalities belonging to the control and treatment groups according to the changed percentile splits can be found displayed spatially in Appendix A.2 and listed in Appendix A.3.

Table 6: Effect of the law for the 10/10 split

Model:	(5)	(6)	(7)	(5)	(6)	(7)
Years:	91-20	91-20	97-20	91-14	91-14	97-14
DiD	-0.400	-0.345	-0.452*	-0.752**	-0.737**	-0.820**
	(0.288)	(0.270)	(0.259)	(0.330)	(0.337)	(0.333)
Controls	None	2	4	None	2	4
Observations	1440	1344	1056	1152	1056	768
Adj. R-squared	0.175	0.109	0.096	0.207	0.108	0.076
Mun. per group:	24	24	24	24	24	24
Group split:	10&10	10&10	10&10	10&10	10&10	10&10
Time trends:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust Standard errors are presented in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1

with linear time trends presented in table 5. This is exactly what we observe. The coefficient of interest has increased in size in all our models. Once again, limiting the post-treatment period yields results which are significant at the 5% level. We even start observing significant results at the 10% level for Model 3 over the entire post-treatment period. However, this result should be assessed carefully. Adding the full set of control variables has increased the size of the effect. This is an effect known as suppression effect. According to Crede et al. (2016), suppression effects can be considered "statistical artifacts" in most cases.

Next, the same analysis was conducted but this time widening the percentile splits which define treatment and control assignment. The results for the comparison between treatment and control groups assigned based on the top, respectively bottom 30^{th} percentiles of D/E, are presented in Table 7.

We expect the effect of the law change to get smaller as we start including municipalities in the comparison which are increasingly similar in terms of D/E levels. This is precisely what we observe. Coefficients focused on the shorter post-treatment periods even start losing significance. These results provide robustness to our identification strategy.

Table 7: Effect of the law for the 30/30 split

Model:	(5)	(6)	(7)	(5)	(6)	(7)
Years:	91-20	91-20	97-20	91-14	91-14	97-14
DiD	-0.223	-0.247	-0.174	-0.340	-0.364*	-0.287
	(0.169)	(0.155)	(0.157)	(0.207)	(0.204)	(0.209)
Controls	None	2	4	None	2	4
Observations	4320	4032	3168	3456	3168	2304
Adj. R-squared	0.156	0.094	0.076	0.198	0.097	0.056
Mun. per group:	72	72	72	72	72	72
Group split:	30&30	30&30	30&30	30&30	30&30	30&30
Time trends:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust Standard errors are presented in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1

7.1.2 Excluding metropolitan areas

In this section, we run the regressions specified in equations 5-7 but we exclude municipalities which are in metropolitan areas. The intuition behind this test is to account for potential effects happening between clusters of municipalities which are not fully captured by our model. This is especially true with regards to the D/E levels which can be assumed to be smaller in metropolitan areas where housing prices are higher, thus driving equity levels up. First, we exlude the three largest municipalities (Stockholm, Gothenburg and Malmö) from the analysis. In a second step, we also exclude municipalities which are in the vicinity of the those three big cities using the classification provided by SCB (SCB, 2005). This last step reduces the sample size from 241 to 202 municipalities. In both cases, we reassigned the remaining municipalities to the control and treatment groups again using the top and bottom D/E quintiles.

The results when excluding only the three biggest municipalities from the analysis are displayed in Table 8. The results are very close to the specification which included all municipalities, both in terms of effect sizes and significance levels. It is important to point out that Stockholm, Gothenburg and Malmö all belonged to the bottom D/E quintile and were therefore part of the control group in the regressions including all municipalities. Hence, it seems that the effects previously observed were not driven by the largest municipalities in our sample.

Table 8: Excluding the three largest cities

Model:	(5)	(6)	(7)	(5)	(6)	(7)
Years:	91-20	91-20	97-20	91-14	91-14	97-14
DiD	-0.333	-0.288	-0.274	-0.647**	-0.603**	-0.512*
	(0.217)	(0.194)	(0.197)	(0.263)	(0.259)	(0.269)
Controls	None	2	4	None	2	4
Observations	2880	2688	2112	2304	2112	1536
Adj. R-squared	0.146	0.082	0.064	0.192	0.087	0.048
Mun. per group	48	48	48	48	48	48
Group split:	20&20	20&20	20&20	20&20	20&20	20&20
Time trends:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust Standard errors are presented in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1

Things get a bit more confusing when excluding all metropolitan municipalities. 39 municipalities were removed in the process. Eighteen of these municipalities originally belonged to the control group while only three of them belonged to the treated group. The remainder was not assigned to any group. The results can be found in Table 9. First, we observe effects significant at the 10% level across our three models when applied to the entire post-treatment period. Meanwhile, effects for the shorter post-treatment period lose all their significance. This seems to indicate that the effect observed in 2013 and 2014 is mostly driven by the metropolitan areas. There still seems to be an effect in municipalities which are not part of metropolitan areas. However, this effect does not seem to follow the same pattern as the one set by metropolitan areas.

Table 9: Excluding large cities and their surrounding municipalities

Model:	(5)	(6)	(7)	(5)	(6)	(7)
Years:	91-20	91-20	97-20	91-14	91-14	97-14
DiD	-0.418*	-0.372*	-0.360*	-0.427	-0.409	-0.376
	(0.229)	(0.210)	(0.208)	(0.266)	(0.252)	(0.244)
Controls	None	2	4	None	2	4
Observations	2460	2296	1804	1968	1804	1312
Adj. R-squared	0.167	0.101	0.079	0.205	0.101	0.057
Mun. per group	41	41	41	41	41	41
Group split:	20&20	20&20	20&20	20&20	20&20	20&20
Time trends:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust Standard errors are presented in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1

7.2 Generalized linear models

While ordinary least squares (OLS) presents the big advantage of being easier to set up, compute and interpret, outcome variables in the form of counts are typically not well modelled by linear least squares methods such as OLS. This is even more true when the data contains many zero values as is the case for our dependent variable. In addition, the logarithmic transformation applied to our outcome variable across the analysis is only partially satisfactory in dealing with this issue. Given that we would like our model to capture the relationship between our core independent and our outcome variables as truly as possible, we decided to conduct an analysis using a generalized linear model form which was suited to a dependent variable in the count format. Two regressions which are commonly applied to count outcome variables which include zero values are the Poisson regression and the negative binomial regression. The choice between the two is subject to ongoing debate in the current econometrics literature, especially when applied to a fixed effects model. Cameron & Trivadi (2009) argue that the negative binomial will likely lead to better estimation efficiency when facing overdispersed count data, i.e., when the conditional variance exceeds the conditional mean. Overdispersion is typically a concern when the count data includes a high number of zeros as is the case of our outcome variable. In addition, running the statistical test for overdispersion suggested by (Fávero et al., 2020) led to a strong rejection of the null hypothesis of equidispersion. However, Wooldridge (1999) neatly shows that the fixed effect Poisson estimator is robust to every failure of the Poisson assumptions, except for the baseline

assumption of correct mean specification. Nonetheless, we believe that the excessive number of zeros in our data strongly drags the conditional mean downwards, thus exacerbating the overdispersion issue. The mean number of newly built rental dwellings per year for the municipalities in our 20/20 split is 25.3. However, the variance is a staggering 9524. Furthermore, Green (2021) shows that the negative binomial model fitted data with over-abundant zeros better than the Poisson model. We thus deemed the negative binomial estimator as most appropriate in this setting.

The coefficients estimated thus far were computed using the OLS method. GLMs compute coefficients using maximum likelihood estimation (MLE). MLE methods typically require to be fed more information than OLS models, especially in the negative binomial case. Accounting for municipality and time fixed effects, as well as including time trends for every single municipality further decreases variation. As a result, we could only run the fixed effects regressions for the models including control variables since they provide more information. Note that the dependent variable is now left in its original count format and is not undergoing logarithmic transformation. The results for the negative binomial specification are displayed in Table 10. As before, we present the results based on the number of controls included as well as the length of the post-treatment period. All specifications include linear time trends.

Table 10: Negative Binomial regression

Model:	(6)	(7)	(6)	(7)
Years:	1991-2020	1997-2020	1991-2014	1997-2014
DiD	-0.244	-0.226	-0.780**	-0.688*
	(0.253)	(0.270)	(0.382)	(0.401)
Controls	2	4	2	4
Observations	2604	2002	1958	1216
Mun. per group	48	48	48	48
Group split:	20&20	20&20	20&20	20&20
Time trends:	Yes	Yes	Yes	Yes

Notes: Robust Standard errors are presented in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

The results are in line with what we observed in our OLS specifications. Taking the results for the entire post-treatment period, the coefficients are not statistically significant, but have the correct sign. Furthermore, including more controls reduces the size of the

effect. When looking at the short post-treatment period the treatment indicator becomes significant at the 5% level for model specification 6. In that case, the coefficient of -0.78 tells us that treatment led to building 0.46 times the amount that would have been built absent treatment. The coefficient for model specification 7 is statistically significant at the 10% level and indicates that treatment resulted in building about half the amount that would have been built absent treatment. These size of the effect is thus very close to the size of the effects measured in the main results for the corresponding specifications.

In summary, using a different estimation method does not substantially change our results which seem to keep pointing at *nya Allbolagen* having had an effect on the supply of new rental dwellings built by MHCs in our control group. These results are also in line with our interpretation which posits that the effects were strongest shortly after the law before they started fading over time. A word of caution should be expressed with regards to the negative binomial regression. The statistical software we used did not allow us to compute robust standard errors when running a fixed effects negative binomial analysis. Clustering for standard errors at the municipality level could end up having substantial impact on the significance level of the estimated coefficients.

7.3 Spill-over effects in the private sector

While our analysis was centered around the impact of *nya Allbolagen* on the quantity of new rental dwellings built by MHCs, our theoretical framework also made predictions for the quantity which would be produced by private housing companies as a result of the law and the change in market structure. We repeated the same analysis conducted using the model specifications defined in equations 5-7 but replaced the dependent variable with the quantity of dwellings built by private companies. The rationale is that private companies in the treated group should experience a shift from a mixed oligopoly market to a private oligopoly market as a result of the law, while private companies in the control group should not. The results are displayed in table 11.

It seems that *nya Allbolagen* barely had an effect on the supply of housing by private companies in treated municipalities. Only the second model specification yields a significant result and only for the entire post-treatment period. In addition, the coefficient has the opposite sign than the one predicted by our theoretical model.

Table 11: Effect of the law on the private sector

Model:	(5)	(6)	(7)	(5)	(6)	(7)
Years:	91-20	91-20	97-20	91-14	91-14	97-14
DiD	-0.234	-0.327*	-0.002	-0.105	-0.201	-0.003
	(0.196)	(0.195)	(0.208)	(0.197)	(0.199)	(0.206)
Controls	None	2	4	None	2	4
Observations	2879	2687	2111	2303	2111	1535
Adj. R-squared	0.242	0.141	0.110	0.281	0.133	0.026
Mun. per group	48	48	48	48	48	48
Group split:	20&20	20&20	20&20	20&20	20&20	20&20
Time trends:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust Standard errors are presented in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1

8 Discussion

8.1 Results and interpretation

In this thesis we tried to estimate if *nya Allbolagen* (2011) had an effect on the quantity of new rental dwellings built by those MHCs, which we argue were impacted by it. Assuming that the parallel trends assumption holds for the pre-treatment periods, we identify a negative and statistically significant effect, on the 1 % level, on the supply of housing across our model specifications. However, we have shown that the parallel trends do not hold across our entire pre-treatment period. This was to be expected given the length of the time period we observe. The longer the pre-treatment period, the less likely this assumption is to hold. Lacking parallel trends, it is difficult to assume that the treated group would have been on the same trajectory as the control group, in the absence of treatment.

Choosing the more conservative approach, we included linear time trends to account for slope differences in the outcome variable between the municipalities. The resulting pre-treatment trends between our control and treatment groups look close to perfect as a result. When including the linear trends and looking at the entire post-treatment period (2013-2020), the sign of the coefficients is aligned with the theoretical prediction but the results are not statistically significant. However, the coefficients become significant once we restrict the post-treatment period to the first two treatment years (2013-2014). These

conclusions are true regardless of the model specification, that is, whether controls are added or not. Furthermore, they are robust when excluding the three largest Swedish municipalities or when switching from an OLS fixed effects to a negative binomial regression. A potential explanation to the absence of statistically significant effects for the whole period is that there were no effects. The switch to business-like principles has happened gradually ever since the beginning of the 1990s and the law might have been redundant. The survey from *Boverket* which stated that almost a third and a fifth of all municipalities in weak, respectively medium markets had a harder time to secure the housing supply after the law speaks against that notion. Furthermore, arguing that the law did not have any effect makes it hard to explain the results observed over the short post-treatment period. Instead, we believe the law gained traction early, and that it had a strong but short effect on the quantity of rental dwellings supplied. The most probable scenario, in our view, is that the law resulted in caution amongst those MHCs that were most likely to be affected by it as they were aligning their business operations with the law requirements. During this transition period it could have been more difficult for them to finance their housing projects. For example, the introduction of the guarantee fee is likely to have limited the amount heavily indebted MHCs could borrow. On one hand, these results are encouraging as they indicate that MHCs resumed building at pre-treatment rates from 2015 onward. On the other hand, the question remains whether the deficit in rental dwellings from 2013-2014 has subsequently been compensated for or not. While we cannot answer this question given the insignificance of the coefficients over the entire post-treatment period, the sign of the coefficient does not point in an encouraging direction.

We were surprised to see the effects of the law fade over time given that the average D/E of the treated group decreased by more than the average D/E of the control group. It is nonetheless possible for MHCs to have decreased their D/E while starting to build at pre-treatment levels again from 2015 onward. Since house prices have developed equally across the two groups we can exclude the possibility that it is due to appreciation of their holdings. D/E becomes lower by either paying off debt without accruing more or by pursuing projects which individually yield a lower D/E than what the company currently has. If an MHC has a D/E of 10, then each new project that has a book value[13] smaller than 10 times the debt needed to finance the construction, will automatically lower the total D/E ratio. However, even if a company pursues projects that lowers the D/E ratio it would still increase its nominal amount of debt, which is always associated with more risk. Municipalities can also sell existing holdings in less attractive areas and then use parts of the proceeds to finance new construction. If the new construction is situated in a more attractive location or is of higher standard then the resulting book

[13]The assets value minus its associated liabilities

value will be higher. Higher standard dwellings allow higher rent setting - thanks to *brukvärdessystemet* - and higher rents increase the book value by discounting future cash flow from a higher level. Selling existing holdings to finance new construction could thus also explain why treated MHCs have managed to lower their D/E while starting to construct more again, so that the effects fade after 2014. If this was the case, it raises questions about the long-term sustainability of the approach as MHCs will eventually run out of dwellings to sell. This analysis also implies that MHCs might have started offering different types of dwellings than they were before. After 2015, it is possible that MHCs in treated municipalities started targeting different customer groups to ensure they would survive without municipality support. This would effectively mean that people in real need of non-profit housing would see the supply decrease. This remains a hypothesis, the answer to which goes beyond the scope of this thesis. Credit must be given to MHCs in the treated group which, after the initial confusion created by the law, managed to swiftly start building again. One could nonetheless question whether this was done with their societal objective in mind and whether it will be sustainable in the long-run.

Treated municipalities, i.e., municipalities which experience a shift in market structure thus experience a decrease in the quantity of new rental dwellings built by MHCs. As predicted, a stronger shift in market structure equals a larger effect size, as can be seen when we move from the 20^{th} to the 10^{th} percentile split. This is what we expected given that municipalities which are even more, respectively less likely, to have acted in a business-like way pre-2011 are being compared. The effects become smaller and less significant when we instead use the 30^{th} percentile split which is to be expected following the same reasoning.

When excluding Malmö, Stockholm, Göteborg and their surrounding municipalities from the analysis, the treatment coefficients are significant for the whole post-treatment period but not for the shortened one. Metropolitan municipalities were over-represented in the original control group and the composition of the control group consequently changed substantially when they were removed. The exact opposite is true of the treatment group. The fact that the effect disappears over the shorter post-treatment period makes sense since the new control group includes municipalities with a higher D/E than in the original control group. However, following this reasoning, we should not be observing any effect over the entire post-treatment period either. A more likely explanation is that the effect pattern observed in the main results is driven by the metropolitan areas. When replacing metropolitan municipalities by other municipalities in the control group, we believe there to also be an effect, although over the entire period and with no particularly big effect over the period 2013-2014. The fact that the results are only significant at the 10 percent level in this case also makes sense since we

are comparing municipalities with more similar D/E levels. However, this leaves the question of why we observe different effect patterns when excluding the metropolitan areas. We previously argued how the treated group might have started building more again after 2014. However, these results hint at another possible explanation whereby the fading of the effects might have been linked to metropolitan areas building less after 2014. Whether both hypotheses hold, or whether one is driving the results observed in our main specification will have to be addressed in future research.

Finally, we tried to assess the effects of *nya Allbolagen* on the quantity of housing supplied by private companies. The results were not in line with the theory which predicted an increase in supply by private companies in treated municipalities, i.e., where the market structure would shift from a mixed oligopoly to a private one. Although the results were mostly insignificant, they seem to point in the opposite direction of the theory. As previously mentioned, it is possible that, following the law, MHCs started cannibalizing the supply of private companies by targeting different customer groups and areas within their municipality. This would also explain why the (negative) effects become smaller and lose significance for the private sector when restricting the post-treatment period to 2013-2014. Those are exactly the years during which we argue the MHCs built less and were thus less likely to steal opportunities from the private sector. This reasoning would imply that private companies and MHCs were not competing over perfectly overlapping customer groups pre-2011. More importantly, it could mean that certain demand segments which were previously only supplied by MHCs are left unsupplied after the law change.

8.2 Limitations, validity and avenues for further research

The methodology proved to be the most challenging aspect of this paper. In particular, we were faced with the issue of defining a good control group. An alternative approach to the control group selection would have been to use a synthetic control. However, Sweden´s universal housing approach makes comparisons with other countries difficult so we were sceptical about using that method. Furthermore, we are convinced that *nya Allbolagen* did not affect all municipalities in Sweden. Considering all municipalities part of the treatment group would thus have been misleading. With parts of the sample essentially unaffected, the task instead became to identify which municipalities were treated and which were part of the control group. Acting in a business-like way is obviously not all black or white. However, we had to assume it was in the process of creating a treated and control group characterized by dummies indicating, well, black or white. We are confident that our control and treatment groups fulfil their role in

separating MHCs which definitely acted in a business-like way pre-2011 from those which definitely did not. However, we lost the ability to draw conclusions about a number of municipalities located in the grey zone in the process. Furthermore, our identification strategy is closely tied to the effects of *nya Allbolagen*, a law that was implemented in Sweden. We would thus not expect our results to generalize to other countries which might also need to align their housing markets with EU competition policy. We acknowledge that our methodology is a bit unconventional and creative. However, it seemed like the most reasonable way to answer this research question, which we feel touches upon an important topic with potentially large ramifications.

An issue directly related to the choice of control and treatment group is the parallel trends assumption. We have shown that parallel trends did not hold between our comparison groups, or at least not over the entire pre-treatment period. We believe that the inclusion of time trends at the municipality level has substantially reduced the bias induced by the lack of parallel trends. However, this procedure might have had the externality of absorbing some of the treatment effects in addition to absorbing pre-treatment trends.

The choice of time-lag could also be subject to discussion. It is unlikely that the time it takes to conduct a building project does not vary across municipalities. Additionally, the time it takes to build a five-story building is not comparable to that of a small house. Discussions with people in the industry led us to decide on the two year lag. The Granger causality test displayed that the effect of the law is first apparent after two years, which gave credibility to our decision. Our large sample, and long time-period, hopefully average out such variation. Another benefit of the large sample, is that it accounts for aspects like the type of dwellings which municipalities are building. If an MHC predominantly focuses on student housing compared to family sized apartments then it would be easier to supply a larger quantity, which in return would bias our results. The longer the period and the larger the sample the less likely our regressions are to suffer from such biases.

The large number of zeros in our sample is also a cause for concern. Out of the 2880 observations available for our main model specifications, 2069 had a value of zero. Nonetheless, we are confident that the negative binomial model, which confirmed our main results, is suited to handle such a high number of zero values. In addition, the aggregate output per group is never zero in our main model specifications. Finally, zero values should not be seen as a problem if they are to be expected. In our case, it is completely normal for MHCs in smaller municipalities not to build anything for multiple years in a row.

We believe that further research and more data is needed to capture the effect of the law and especially the mechanisms that municipalities use to accommodate with it. A closer examination of what type of apartments are being constructed, where they are being built, and if rents have changed as a result of the law, would be an important addition. Rents are unlikely to differ substantially in total, due to being decided in agreement with the tenant association. In municipalities that have been forced/nudged to build in more attractive areas, we might see an increase in rent levels due to the *bruksvärdessystem*. If MHCs sold dwellings to finance new construction that would also be visible in the average rent level, since newly constructed apartments tend to have higher rents.

9 Conclusion

This thesis aimed to answer the question of whether *nya Allbolagen* had affected the supply of rental dwellings built by municipal housing companies. We find that the law was implemented swiftly and that the Swedish housing market has aligned with the EU rules on state aid. Our results indicate that the law had a statistically significant negative effect on the treated group for the years 2013-2014. We present a number of alternative specifications which speak for the robustness of our results. Surprisingly, the effect disappears when the post-treatment period is extended to 2020. It is difficult to understand why the effect fades without more granular data that could show how municipalities accommodated with the law in practice. We find that the average debt-to-equity ratio decreased more in the treated group compared to the control group, indicating that the treated group has changed their operations to be more business-like. One possible explanation is that MHCs in the treated group have sold existing holdings and used the proceeds to finance new constructions that have an individually lower D/E than the company average. This hypothesis should cause some worry among policy makers about the long-term sustainability of those practices. Furthermore, as pointed out by the spillover effects, there are reasons to believe that MHCs affected by the law have changed their target groups in order to build higher valued dwellings. This could potentially lead to a lack in housing supply targeted at the lower socio-economic strata of the population.

This thesis has contributed to the literature by being the first study to quantitatively assess the effects of *nya Allbolagen* on the quantity of rental dwellings built by MHCs. This is also the first study to compile the D/E of every MHC and show that it was substantially different from their private counterparts, and that it has decreased since the law. Nothing about this thesis, however, indicates that is has become easier for our

treatment group to supply housing, even if we cannot conclude that it necessarily has become more difficult.

The universal housing approach is the result of a notion that public companies should not solely cater to the less endowed. There is merit to that notion. Equality should, in the authors' views, be desirable. The question, however, remains: is a public-private hybrid the solution to achieving a broad and adequate housing supply for all?

10 References

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A Appendix

A.1 Rents

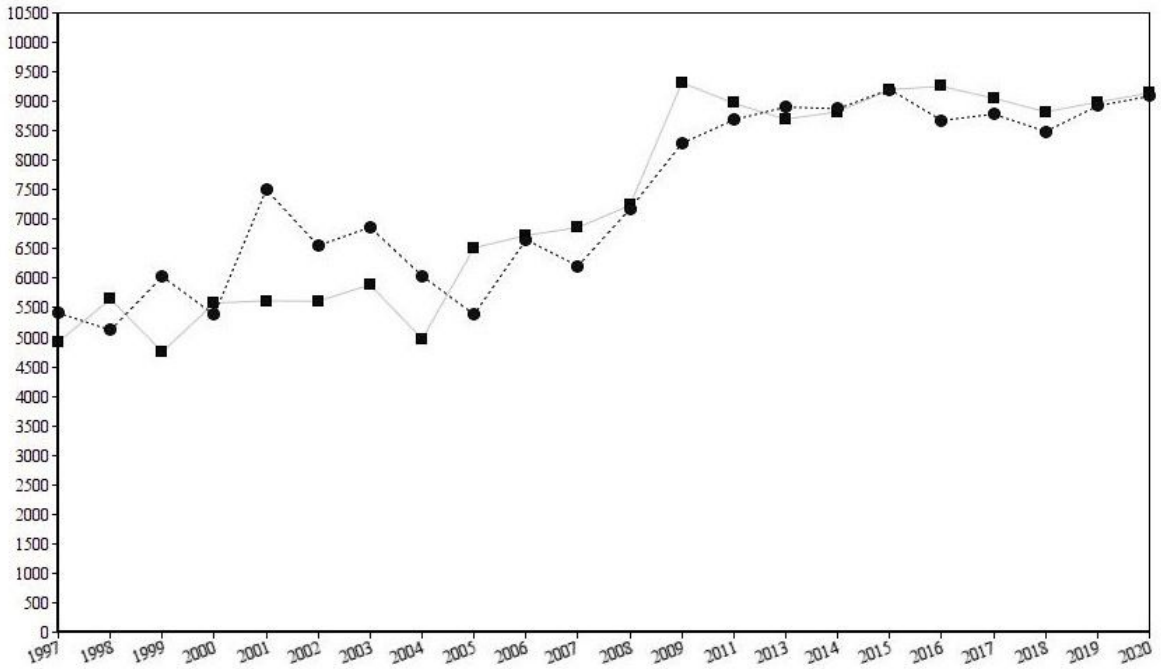

Figure 5: Average monthly rent in newly constructed dwellings. Circles represent MHCs and squares private developers.

A.2 Maps - Spatial representation of the treatment and control groups

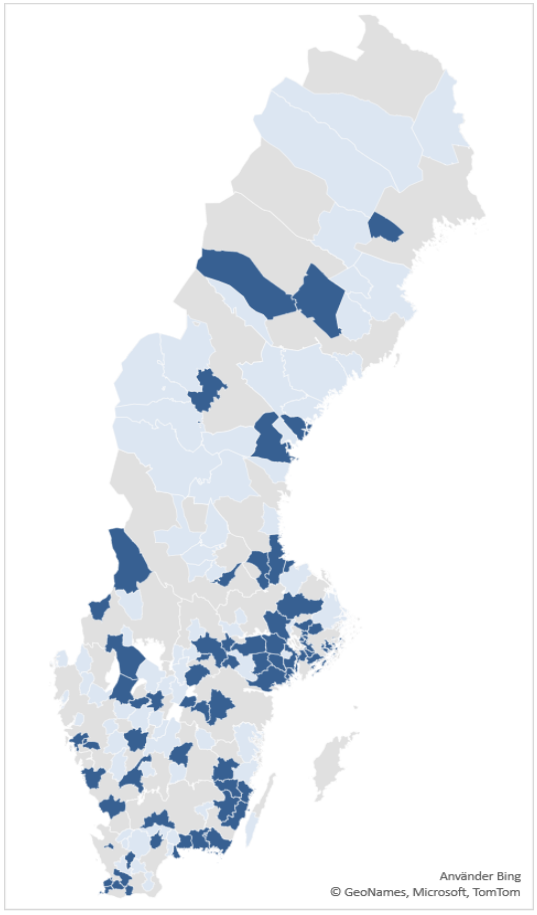

Figure 6: Spatial representation of treatment and control groups for the 30/30 percentile split. Dark blue shows municipalities in the control group and light blue shows those in the treated group

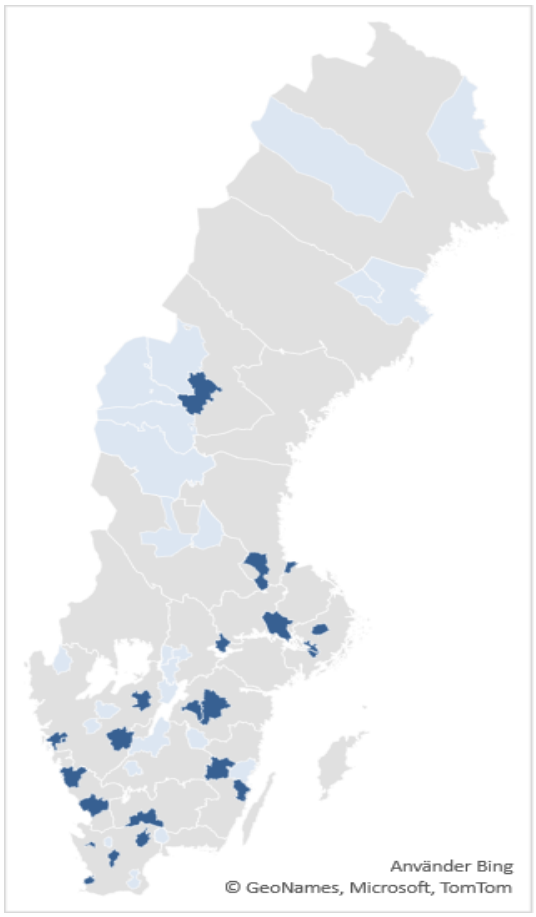

Figure 7: Spatial representation of treatment and control groups for the 10/10 percentile split. Dark blue shows municipalities in the control group and light blue shows those in the treated group

A.3 Municipalities used in the DiD analysis

Table 12: Municipalities included in the control and treatment groups.

(10)	(20)	(30)	(10)	(20)	(30)
Control	Control	Control	Treated	Treated	Treated
Upplands Väsby	Älvkarleby	Järfälla	Ydre	Nykvarn	Norrtälje
Vallentuna	Enköping	Sundbyberg	Gnosjö	Håbo	Tierp
Värmdö	Vingåker	Uppsala	Jönköping	Katrineholm	Habo
Huddinge	Nyköping	Gnesta	Oskarshamn	Lessebo	Sävsjö
Botkyrka	Flen	Strängnäs	Olofström	Mörbylånga	Västervik
Haninge	Eskilstuna	Trosa	Tomelilla	Örkelljunga	Hässleholm
Sollentuna	Linköping	Vadstena	Perstorp	Hörby	Töreboda
Stockholm	Mjölby	Gislaved	Dals-Ed	Bromölla	Lysekil
Södertälje	Älmhult	Nässjö	Vårgårda	Osby	Strömstad
Sigtuna	Högsby	Kalmar	Karlsborg	Eslöv	Vänersborg
Burlöv	Hultsfred	Karlskrona	Vara	Stenungsund	Borås
Östra Göinge	Mönsterås	Ronneby	Lekeberg	Färgelanda	Falköping
Höör	Nybro	Karlshamn	Laxå	Svenljunga	Surahammar
Åstorp	Sölvesborg	Svedala	Degerfors	Mariestad	Köping
Malmö	Vellinge	Lund	Rättvik	Tidaholm	Gagnef
Halmstad	Skurup	Mark	Mora	Grums	Leksand
Varberg	Härryda	Lidköping	Krokom	Sunne	Borlänge
Partille	Skara	Säffle	Åre	Nora	Söderhamn
Göteborg	Eda	Kumla	Berg	Nordanstig	Ånge
Ulricehamn	Torsby	Askersund	Härjedalen	Ljusdal	Kramfors
Skövde	Örebro	Säter	Norsjö	Timrå	Sollefteå
Arboga	Gävle	Härnösand	Skellefteå	Örnsköldsvik	Dorotea
Sandviken	Sundsvall	Vilhelmina	Jokkmokk	Nordmaling	Arvidsjaur
Östersund	Lycksele	Älvsbyn	Pajala	Vindeln	Gällivare

Notes: The number in parenthesis refers to the percentile. Municipalities belonging to the 20th percentile are obtained by aggregating the 10th and 20th percentile columns. Municipalities belonging to the 30th percentile are obtained by aggregating the 10th, 20th, and 30th percentile columns.

A.4 Graphical display of parallel trends

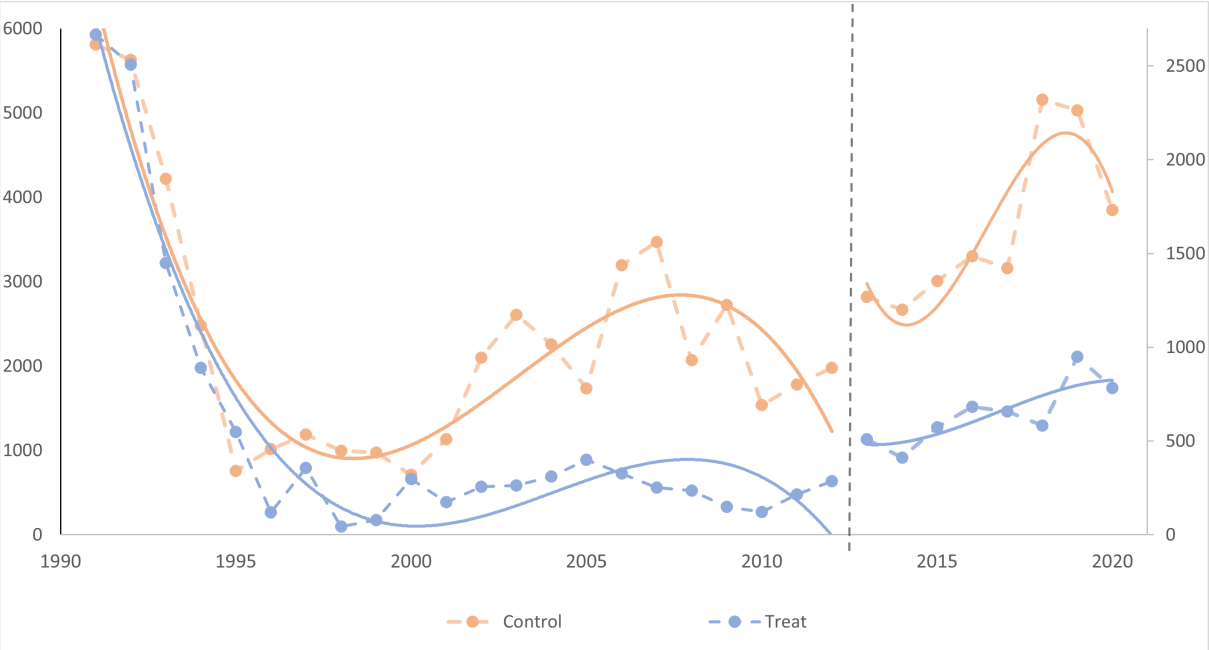

Figure 8: Displays the trends in the outcome variable for the 30/30 split.The scale of the treated and control group are found on the right respectively the left side on the graph. This is done to better capture the trends despite the difference in magnitude.

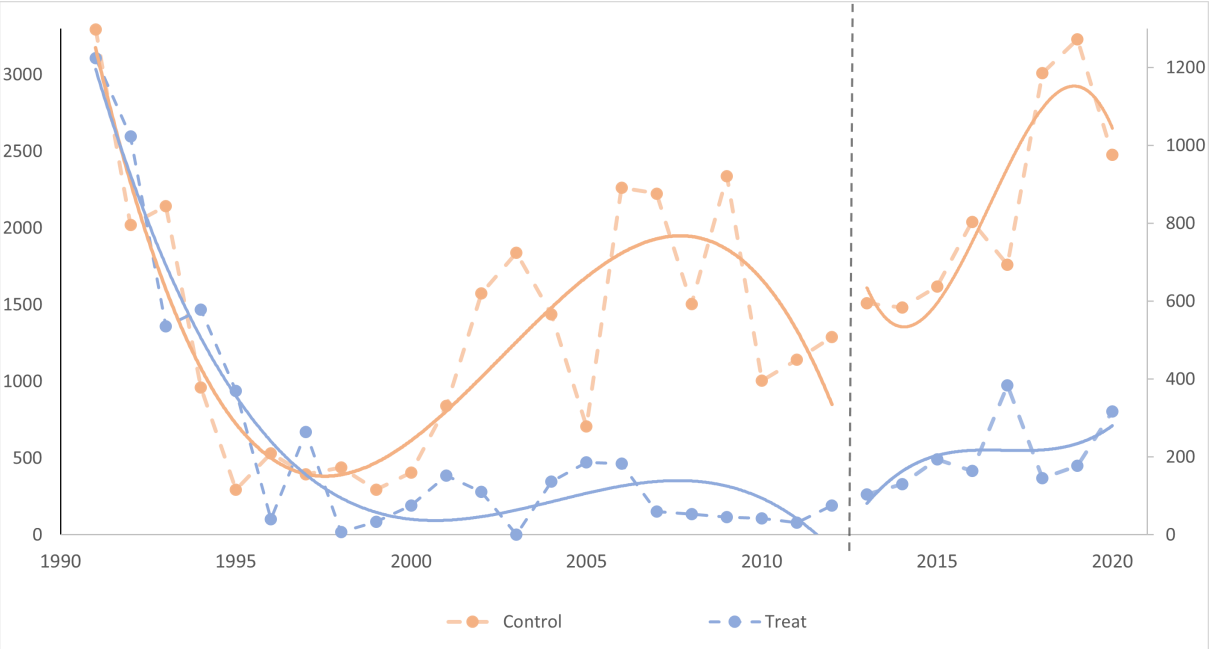

Figure 9: Displays the trends in the outcome variable for the 10/10 split. The scale of the treated and control group are found on the right respectively the left side on the graph. This is done to better capture the trends despite the difference in magnitude.

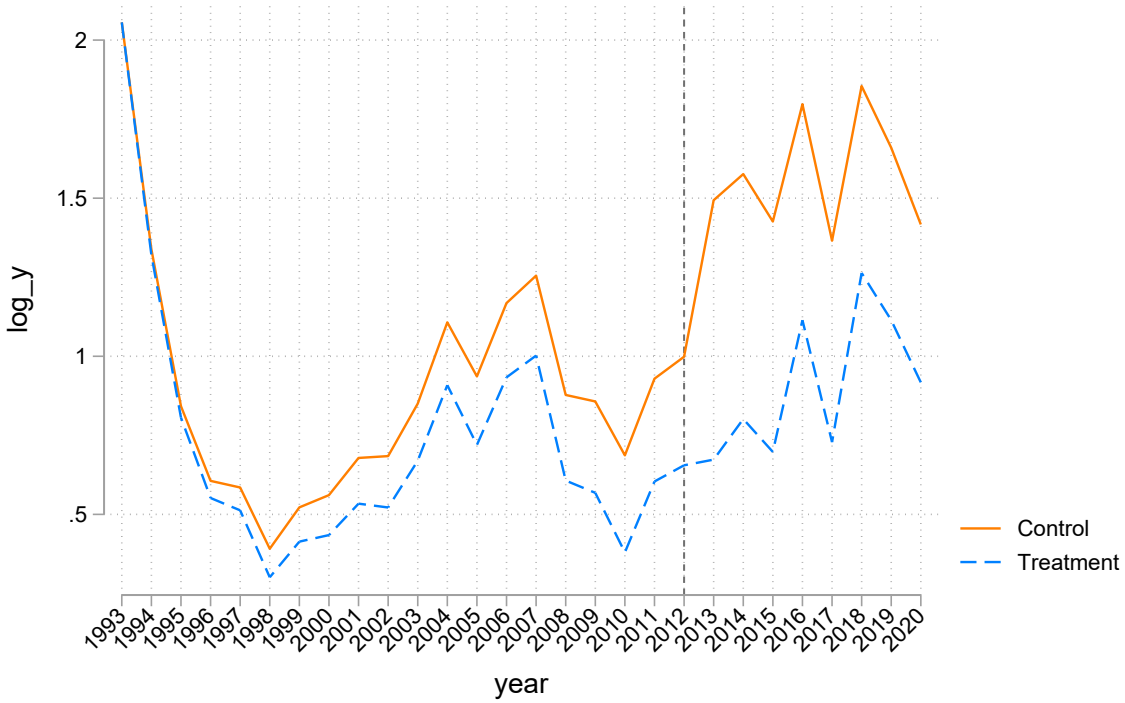

Figure 10: Linear time-trends. 30/30 split for the years 1991-2020 with two controls (population and house prices) included.

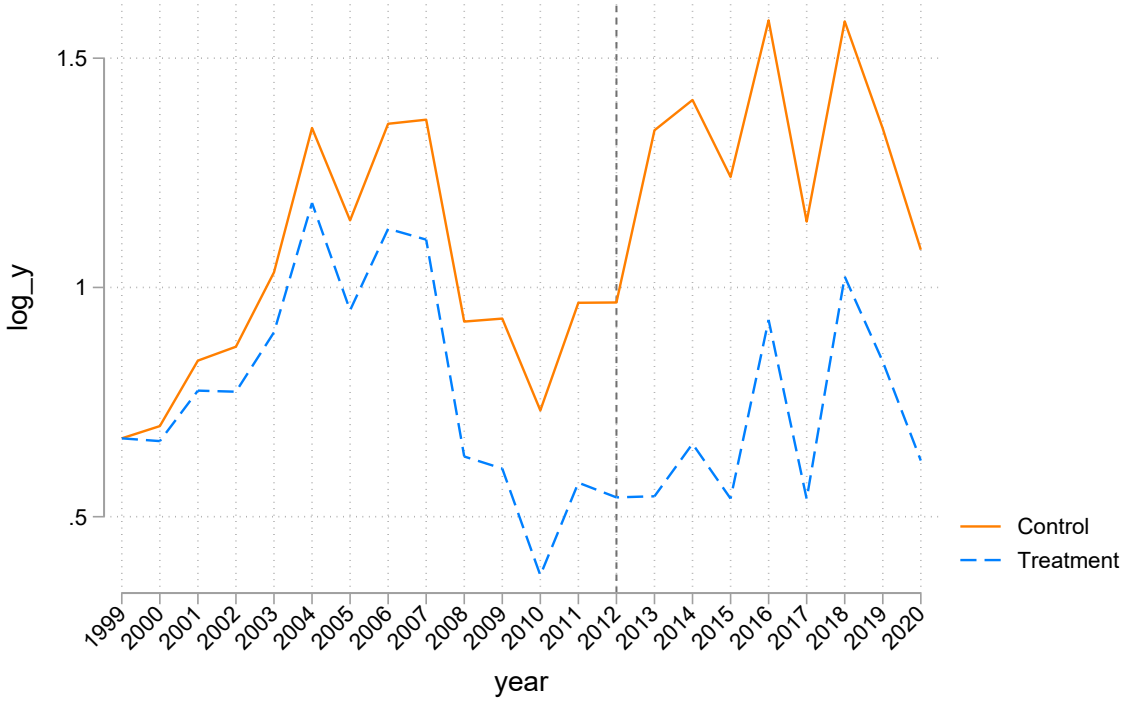

Figure 11: Linear time-trends. 10/10 split for the years 1997-2020 with all controls included.

A.5 Detailed results tables

Table 13: Granger Causality Test.

Controls:	None	2	4
Years:	1991-2020	1991-2020	1997-2020
DID91	-0.004		
	(0.458)		
DID92	-0.360		
	(0.463)		
DID93	-0.479	-0.547	
	(0.427)	(0.445)	
DID94	-0.164	-0.229	
	(0.435)	(0.437)	
DID95	0.451	0.388	
	(0.387)	(0.387)	
DID96	-0.107	-0.165	
	(0.310)	(0.307)	
DID97	0.349	0.296	
	(0.348)	(0.337)	
DID98	0.097	0.048	
	(0.346)	(0.326)	
DID99	-0.127	-0.170	-0.099
	(0.308)	(0.299)	(0.302)
DID00	0.473	0.433	0.474
	(0.299)	(0.292)	(0.294)
DID01	-0.147	-0.182	-0.156
	(0.317)	(0.320)	(0.320)
DID02	0.285	0.255	0.267
	(0.298)	(0.299)	(0.299)
DID03	-0.345	-0.370	-0.346
	(0.328)	(0.323)	(0.322)
DID04	-0.441	-0.462	-0.436
	(0.359)	(0.350)	(0.350)
DID05	-0.382	-0.399	-0.399
	(0.303)	(0.304)	(0.306)
DID06	-0.482	-0.497	-0.491
	(0.366)	(0.364)	(0.368)

Granger Causality Test (cont.)

DID07	-0.799**	-0.811**	-0.824**
	(0.354)	(0.352)	(0.354)
DID08	-0.163	-0.171	-0.215
	(0.333)	(0.333)	(0.334)
DID09	-0.298	-0.302	-0.336
	(0.322)	(0.323)	(0.327)
DID11	-0.186	-0.179	-0.217
	(0.330)	(0.329)	(0.332)
DID12	-0.396	-0.386	-0.407
	(0.390)	(0.391)	(0.396)
DID13	-0.903**	-0.888**	-0.874**
	(0.395)	(0.394)	(0.399)
DID14	-1.124***	-1.104***	-1.09***
	(0.329)	(0.327)	(0.331)
DID15	-0.517	-0.493	-0.482
	(0.356)	(0.350)	(0.352)
DID16	-0.559	-0.532	-0.511
	(0.395)	(0.396)	(0.398)
DID17	-0.172	-0.143	-0.109
	(0.355)	(0.358)	(0.356)
DID18	-0.888**	-0.856*	-0.839*
	(0.432)	(0.442)	(0.435)
DID19	-1.03***	-0.996***	-0.977**
	(0.374)	(0.380)	(0.382)
DID20	-0.375	-0.335	-0.286
	(0.412)	(0.412)	(0.413)
Observations:	2880	2880	2304
Group Split:	20&20	20&20	20&20

Notes: Robust standard errors are presented in parenthesis below the Difference-in-differences estimator.
*** p<0.01, ** p<0.05, * p<0.1
Municipality fixed effects and time fixed effects are included. Coefficients for time trends, time dummies and municipality dummies are not displayed for cosmetic reasons.

Table 15: Effect of the law on newly built dwellings (no time trends)

Model	(1)	(2)	(3)
Years	1991-2020	1991-2020	1997-2020
DiD	-0.550***	-0.470***	-0.403***
	(0.136)	(0.138)	(0.142)
Controls	None	2	4
Population(t-2)		0.693	1.951*
		(0.857)	(1.020)
House prices(t-2)		0.0000230	-0.000130
		(0.000160)	(0.000122)
Income Group(t-2)			-11.40***
			(3.967)
Unemployment(t-2)			3.502*
			(2.089)
Constant	2.606***	-4.874	-17.76*
	(0.180)	(8.471)	(10.04)
Observations	2880	2688	2112
Adjusted R^2	0.139	0.080	0.059
Mun.per group	48	48	48
Group split:	20&20	20&20	20&20
Time Trends:	No	No	No

Notes: Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Municipality fixed effects and time fixed effects are included. Coefficients for time trends, time dummies and municipality dummies are not displayed for cosmetic reasons.

Table 16: Effect of the law on newly built dwellings (no time trends)

Model	(1)	(2)	(3)
Years	1991-2014	1991-2014	1997-2014
DiD	-0.868***	-0.738***	-0.709***
Controls	None	2	4
	(0.218)	(0.220)	(0.220)
Controls	None	2	4
Population(t-2)		-0.737	1.465
		(1.368)	(1.397)
House prices(t-2)		0.000407	0.0000621
		(0.000262)	(0.000210)
Income Group(t-2)			-8.365**
			(3.854)
Unemployment(t-2)			1.228
			(2.367)
Constant	2.606***	9.118	-13.04
	(0.181)	(13.51)	(13.77)
Observations	2304	2112	1536
Adjusted R^2	0.166	0.083	0.040
Mun.per group	48	48	48
Group split:	20&20	20&20	20&20
Time Trends:	No	No	No

Notes: Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Municipality fixed effects and time fixed effects are included. Coefficients for time trends, time dummies and municipality dummies are not displayed for cosmetic reasons.

Table 17: Effect of the law when including linear time trends

Model	(5)	(6)	(7)	(5)	(6)	(7)
Years	91-20	91-20	97-20	91-14	91-14	97-14
DiD	-0.345	-0.268	-0.220	-0.671**	-0.620**	-0.489*
	(0.219)	(0.200)	(0.203)	(0.262)	(0.261)	(0.276)
Controls	None	2	4	None	2	4
Population(t-2)		3.952	1.680		5.129	1.889
		(2.826)	(3.041)		(3.095)	(5.352)
House prices(t-2)		0.000273	-0.000186		0.000669*	-0.000424
		(0.000225)	(0.000283)		(0.000354)	(0.000440)
Income Group(t-2)			-7.604*			-2.741
			(4.405)			(5.478)
Unemployment(t-2)			0.209			-1.376
			(2.648)			(3.283)
Constant	2.606***	-37.33	-15.23	2.606***	-49.11	-16.90
	(0.168)	(28.02)	(29.83)	(0.156)	(30.69)	(52.49)
Observations	2880	2688	2112	2304	2112	1536
Adjusted R^2	0.161	0.102	0.076	0.199	0.102	0.049
Mun.per group	48	48	48	48	48	48
Group split:	20&20	20&20	20&20	20&20	20&20	20&20
Time Trends:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Municipality fixed effects and time fixed effects are included. Coefficients for time trends, time dummies and municipality dummies are not displayed for cosmetic reasons.

Table 18: Effect of the law for the 10/10 split

Model	(5)	(6)	(7)	(5)	(6)	(7)
Years	91-20	91-20	97-20	91-14	91-14	97-14
DiD	-0.400	-0.345	-0.452*	-0.752**	-0.737**	-0.820**
	(0.288)	(0.270)	(0.259)	(0.330)	(0.337)	(0.333)
Controls	None	2	4	None	2	4
Population(t-2)		1.228	-0.174		1.060	-4.650
		(2.702)	(3.245)		(3.967)	(6.458)
House prices(t-2)		0.000264	-0.0000925		0.00122**	0.000485
		(0.000258)	(0.000399)		(0.000475)	(0.000587)
Income Group(t-2)			-0.385			7.666
			(5.141)			(6.072)
Unemployment(t-2)			-6.138			-6.753*
			(3.795)			(3.934)
Constant	2.889***	-10.41	3.108	2.889***	-9.132	46.89
	(0.244)	(26.84)	(31.68)	(0.227)	(39.43)	(63.47)
Observations	1440	1344	1056	1152	1056	768
Adjusted R^2	0.175	0.109	0.096	0.207	0.108	0.076
Mun.per group	24	24	24	24	24	24
Group split:	10&10	10&10	10&10	10&10	10&10	10&10
Time Trends:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Municipality fixed effects and time fixed effects are included. Coefficients for time trends, time dummies and municipality dummies are not displayed for cosmetic reasons.

Table 19: Effect of the law for the 30/30 split

Model	(5)	(6)	(7)	(6)	(6)	(7)
Years	91-20	91-20	97-20	91-14	91-14	97-14
DiD	-0.223	-0.247	-0.174	-0.340	-0.364*	-0.287
	(0.169)	(0.155)	(0.157)	(0.207)	(0.204)	(0.209)
Controls	None	2	4	None	2	4
Population(t-2)		3.167	1.355		4.558*	-1.107
		(2.303)	(2.484)		(2.641)	(3.853)
House prices(t-2)		0.0000525	-0.000396*		0.000425	-0.000342
		(0.000190)	(0.000226)		(0.000298)	(0.000325)
Income Group(t-2)			-5.447			-2.053
			(3.647)			(4.662)
Unemployment(t-2)			-1.564			-2.944
			(2.315)			(2.550)
Constant	2.593***	-29.64	-11.88	2.593***	-43.68*	12.14
	(0.133)	(22.91)	(24.64)	(0.125)	(26.30)	(38.21)
Observations	4320	4032	3168	3456	3168	2304
Adjusted R^2	0.156	0.094	0.076	0.198	0.097	0.056
Mun.per group	72	72	72	72	72	72
Group split:	30&30	30&30	30&30	30&30	30&30	30&30
Time Trends:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Municipality fixed effects and time fixed effects are included. Coefficients for time trends, time dummies and municipality dummies are not displayed for cosmetic reasons.

Table 20: Excluding the three largest cities

Model	(5)	(6)	(7)	(5)	(6)	(7)
Years	91-20	91-20	97-20	91-14	91-14	97-14
DiD	-0.333	-0.288	-0.274	-0.647**	-0.603**	-0.512*
	(0.217)	(0.194)	(0.197)	(0.263)	(0.259)	(0.269)
Controls	None	2	4	None	2	4
Population(t-2)		4.358	2.663		5.851*	3.129
		(2.779)	(3.040)		(3.063)	(5.304)
House prices(t-2)		0.000171	-0.000287		0.000292	-0.000508
		(0.000271)	(0.000333)		(0.000390)	(0.000438)
Income Group(t-2)			-6.075			-2.642
			(4.279)			(5.185)
Unemployment(t-2)			-1.109			-2.206
			(2.674)			(3.238)
Constant	2.491***	-41.03	-24.57	2.491***	-55.66*	-28.61
	(0.170)	(27.28)	(29.55)	(0.158)	(30.08)	(51.52)
Observations	2880	2688	2112	2304	2112	1536
Adjusted R^2	0.146	0.082	0.064	0.192	0.087	0.048
Mun.per group	48	48	48	48	48	48
Group split:	20&20	20&20	20&20	20&20	20&20	20&20
Time Trends:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Municipality fixed effects and time fixed effects are included. Coefficients for time trends, time dummies and municipality dummies are not displayed for cosmetic reasons.

Table 21: Excluding large cities and their surrounding municipalities

Model	(5)	(6)	(7)	(5)	(6)	(7)
Years	91-20	91-20	97-20	91-14	91-14	97-14
DiD	-0.418*	-0.372*	-0.360*	-0.427	-0.409	-0.376
	(0.229)	(0.210)	(0.208)	(0.266)	(0.252)	(0.244)
Controls	None	2	4	None	2	4
Population(t-2)		1.088	-0.733		3.965	-4.203
		(2.584)	(3.265)		(3.476)	(5.083)
House prices(t-2)		0.000850*	0.000135		0.000727	-0.0000620
		(0.000502)	(0.000531)		(0.000639)	(0.000604)
Income Group(t-2)			-7.266			-1.486
			(4.515)			(5.279)
Unemployment(t-2)			-2.250			-5.620*
			(3.101)			(3.301)
Constant	2.651***	-9.178	8.859	2.651***	-37.18	43.00
	(0.169)	(25.26)	(31.48)	(0.162)	(33.99)	(49.14)
Observations	2460	2296	1804	1968	1804	1312
Adjusted R^2	0.167	0.101	0.079	0.205	0.101	0.057
Mun.per group	41	41	41	41	41	41
Group split:	20&20	20&20	20&20	20&20	20&20	20&20
Time Trends:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Municipality fixed effects and time fixed effects are included. Coefficients for time trends, time dummies and municipality dummies are not displayed for cosmetic reasons.

Table 22: Negative Binomial regression

Model	(6)	(7)	(6)	(7)
Years	1991-2020	1991-2020	1991-2014	1997-2014
y				
DiD	-0.244	-0.226	-0.780**	-0.688*
	(0.253)	(0.270)	(0.382)	(0.401)
Controls	2	4	2	4
Population(t-2)	0.817***	1.148***	0.775***	1.064***
	(0.0733)	(0.152)	(0.0861)	(0.183)
House prices(t-2)	-0.000552***	-0.000428*	-0.000363	-0.000898
	(0.000200)	(0.000244)	(0.000402)	(0.000637)
Income Group(t-2)		-18.55***		-9.934
		(6.626)		(9.512)
Unemployment(t-2)		3.487		-0.832
		(3.414)		(5.126)
Constant	-9.312***	-12.63***	-9.075***	-11.78***
	(0.808)	(1.703)	(0.916)	(2.266)
Observations	2604	2002	1958	1216
Mun.per group	48	48	48	48
Group split:	20&20	20&20	20&20	20&20
Time Trends:	Yes	Yes	Yes	Yes

Notes: Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Municipality fixed effects and time fixed effects are included. Coefficients for time trends, time dummies and municipality dummies are not displayed for cosmetic reasons.

Table 23: Effect of the law on the private sector

Model	(5)	(6)	(7)	(5)	(6)	(7)
Years	91-20	91-20	97-20	91-14	91-14	97-14
DiD	-0.234	-0.327*	-0.00187	-0.105	-0.201	-0.00318
	(0.196)	(0.195)	(0.208)	(0.197)	(0.199)	(0.206)
Controls	None	2	4	None	2	4
Population(t-2)		-1.465	-2.011		-0.740	-2.334
		(2.212)	(3.252)		(2.550)	(5.044)
House prices(t-2)		0.000133	-0.000271		0.000253	-0.000244
		(0.000302)	(0.000418)		(0.000402)	(0.000403)
Income Group(t-2)			-1.828			4.347
			(4.343)			(5.373)
Unemployment(t-2)			-5.043*			-7.337**
			(2.706)			(2.838)
Constant	3.067***	16.61	21.70	3.067***	9.403	24.88
	(0.146)	(21.94)	(31.93)	(0.143)	(25.27)	(49.50)
Observations	2879	2687	2111	2303	2111	1535
Adjusted R^2	0.242	0.141	0.110	0.281	0.133	0.026
Mun. per group	48	48	48	48	48	48
Group split:	20&20	20&20	20&20	20&20	20&20	20&20
Time trends:	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Municipality fixed effects and time fixed effects are included. Coefficients for time trends, time dummies and municipality dummies are not displayed for cosmetic reasons.