Housing solution or Beggar the neighbours?:

An analysis of neighbourhood price effects of joint tenancy rental regulation

Master Thesis
Msc Spatial Transport & Environmental Economics(STREEM)
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Date: 27.06.2024
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Word count: 13494

Abstract

This thesis set out the price developments of neighbourhoods in 10 major Dutch cities subjected to a ban in new conversions to joint tenancy of three or more people. A hotly debated topic for municipal policy makers as they try to alleviate a growing sense of housing affordability. I found that there are is a strong coefficient that shows that the prices in the neighbourhoods subjected to a joint tenancy regulation have around 7.2 % higher prices than the non-treatment group, when I controlled for spatial and temporal fixed effects. However, this effect diminished to 3.2% over when we controlled for more specific spatial fixed effects, This suggests that there is a strong unobserved correlation between the observations in the treated neighbourhoods, which caused the prices to rise. When looking only at the more recent years we see that prices are mostly indistinguishable from the general market trend. I posit several possible explanations for these findings, including the result of non-enforcement of the regulation by the municipalities, the possibility that the negative effects are compensated due to a higher willingness to pay of owner occupiers, or that the period of time between the implementation and the latest observations in the data is simply too short to capture the full effect. Most importantly I theorise that the divergence before the treatment date is caused by a gentrification effect and that the joint tenancy regulation helps bringing the trajectory of price development in line with general market growth rates instead of leading to a price decrease as expected.

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1. Introduction:

1.1 Situation

Many countries around the world struggle with a housing crisis. Rents in major western cities seem to rise to new record heights every year and for many people buying a home seems out of reach. In the case of the Netherlands most, if not all major cities, have seen dramatic increases in the price levels of their housing stock and accompanying rent levels. To qualify for a mortgage that would enable a person to buy the average Dutch house, around 452,000 euro, you would need an income that is twice that of the modal income (jan modaal). The average price for a home in the city of Amsterdam is now 1.5 times the average home in the rest of the country while it was only slightly above average a decade ago. The price of an apartment is over 500.000 euro and the average single family home broke 950.000 euro in 2022 before the price drops to "only" 800.000 euro after the 2023 interest rate hikes. The city calculates that it has a shortage of around 200.000 dwellings, on a total of 460.000(Dignum, 2023). While Amsterdam is an extreme case in the Netherlands as both the largest city and home to some of the country's most important economic engines of the country. Similar rises have been observed for most major cities in the country. Prices for single family homes in the Hague breached 700,000 euro in 2022, while the same category sells for over 600,000 euro in Utrecht(De Hypotheker, 2024). While prices in Rotterdam seem to keep to the average, all four of the big 4 largest cities in the Netherland, that they are in the top 10 of most expensive European cities to rent in with Amsterdam being top of the list, with 3 being in the top 5 (Housing Anywhere, 2024). If looking only at room rentals Amsterdam Utrecht and the Hague make up the top 3.

The current crisis is a result of a slump in real estate development resulting from the Great Recession in 2008, exacerbated by a decrease in the average household size and higher than expected immigration rates in the 2010's. Causing demand for space to rise as development slowed down and population continued to grow. This change has especially been felt in the urban cores as changing demand and shifting economic patterns, household size reduction and environmental concerns have shifted the focus of both development and demand for space into the more dense old urban cores of the cities.

Buy to Let investors saw opportunity in this environment of high urban demand. As interest rates were low in the 2010's. They could borrow relatively cheaply in the high demand centres and rent them out to younger people or immigrants, who did not have enough capital built up to afford their own homes and needed places to live in the economically important cities.In this way the housing supply in the hands of private landlords has passed the 20% mark in most major cities and has surpassed 30% in Amsterdam and is nearing percentage that in the Hague and Groningen. At the same time a shrinking in the average household size meant a large increase in single person households.

Because they would buy the house and rent it out to several single household renters; Landlords could outbid families who would use these homes as primary residence. Making it increasingly unaffordable for many (middle class) families to remain living in the centres of major cities. Families started to see net emigration rates from the cities as it became increasingly unaffordable to buy or rent dwellings with a sufficient amount of space. Reversing a decade long trend of them returning to the cities. Raising concern within many major cities as to the social consequences as many middle class jobs like teacher, firemen, and police were at risk of being forced out of the city. In addition, as the primary target group for these rental units predominantly consists of consumers who have a desire/need for city living and access to limited wealth, many of these rentals were sold to students and people just entering the labour market. A group that is vulnerable against exploitation by and to landlord abuse and notorious for noise pollution and other negative externalities. Combined cities feared a city devoid of families and deteriorating neighbourhood and housing quality as landlords and (short term) renters are thought to neglect their properties. In response to this, municipalities decided to curtail the practice.

In an attempt to salvage the situation, many of the larger cities in the Netherlands have adopted regulations aimed to inhibit the ability to freely buy up homes and rent them. This was done in three ways. The first and most widespread requires a permit for the renting out of homes to non-family members above a certain number of people (usually two). This regulation generally also involves a limit of the number of houses that can be "verkamerd", or have joint tenancy, in a given neighbourhood. The second was the requirement of another permit, or in some cases a complete ban, on dividing up homes into several independent units. And finally municipalities have adopted a so called "opkoopbescherming" or buy up protection, which forbids any renting out of a residential unit for a certain amount of years after purchase. As this investment option would subsequently become unappealing for investors. The stated objective being that prices would remain more affordable for middle class families and reduce negative externalities stemming, such as noise complaints.

Recently, however, voices have become vocal in opposition against these regulations. The interest hikes of the last several years have made borrowing money for real estate development more expensive and limited the borrowing capacity of both buyers and developers. With the housing development goals set by the government ministry and the municipalities now under threat, opponents have raised concerns that the regulation inhibits the profitability and even viability of real estate developments and even proposed promoting these options instead of discouraging them. In an effort to increase housing supply. Opponents claim furthermore that it artificially lowers density and housing supply in the in demand city centres and thus add to the housing supply issue. They argue that this would raise prices in the cities instead of making it more affordable and could even spread unaffordability to other areas. Finally they argue that such a restriction would only benefit the more well-off, who can afford to buy a home while the less well-off will be unable to afford these homes reducing their supply to the remaining stock and possibly driving up prices. In this case, while arguably making it more affordable for homebuyers to own a house in the urban centre it could inadvertently lead to more demand in a broader area of the city. In response to the rising unaffordability and persistent shortages some municipalities, like Rotterdam, have started discussing the effectiveness of their regulations.

While this debate is currently becoming quite heated, research on the effects of anti-home division regulations is scarce. With discussion renewing about whether to implement repeal such regulations in some municipalities. Understanding whether these regulations reduce prices or raise them is a crucial element for implementing the correct policies.

2.1 Historical development

The current buy to let market and the reemergence of a significant private sector on the housing market in many Western countries (Ronald *et al,* 2018; Hochstenbach & Ronald, 2018;2020; Benjamin, 2016) have their origin in the great recession. Austerity measures that reduced funding for social housing programmes, collapse of easy access to mortgages after the crisis, a change in government policy regarding housing(Scanlon *et al,* 2015) and cheap debt as a consequence of the quantitative easing programmes of the 2010's; created a perfect storm for wealthy private or institutional investors to retrench itself in the housing market. This came after decades of private renting being on the decline in most western countries(Ronald *et al,* 2018; Hochtenbach & Ronald, 2018*)*. After the great financial crisis limited access to mortgages and devastated the demand for and construction of privately owned homes, younger people had less options to choose from and were relegated to private renting(Van Duijne & Ronald, 2018).Wealthy individuals and institutional investors started buying up residential real estate using the low mortgage rates of the last decade, their entry made possible by an increase in expected excess returns driven by declines in long-term interest rates and declining competition from buyers in and environment of binding household borrowing constraints(Hanson, 2022;Conijn et al, 2019;Mills *et al*., 2016;Thiel & Zaumbrecher, 2023)Their access to higher earnings and a preexisting access to capital enabled them to start outbidding regular homebuyers and buy up available homes and snowball into increasingly larger portfolios of rental properties (Soiata et al. 2017;Conijn et al, 2019). Furthermore, the introduction of institutional investors into the market coincided with a noticeable decline in private homeownership, especially in the urban cores. As several cities started to see large shares of homes being bought up by private investors(Haughwout *et al,* 2011)

Urban cores have a traditional pull for students, migrants and young professionals, due to the economic, educational and lifestyle benefits of living in the cities. While globally cities had seen a decline in population and density as cheaper car transport enabled more spread out cities in the later half of the 20th century the tide was reversing as young people desire the economic opportunities, amenities of city life and further abated as migration picked up speed again in the 2010's(Glaeser *&* Gottlieb, 2006; Savini & Boterman, 2016;Broitman & Koomen, 2020). With young urban people even willing to pay premiums for living on a smaller square footage if the location was well located near historic areas with a variety of urban amenities. Leading to large concentrations and subsequent densification in desirable neighbourhoods near the (historical) city centres as young people crowded near Universities, job opportunities and cultural amenities (Broitman & Koomen, 2019;Glaeser *et al,* 2006;Folmer, 2014).
As borrowing constraints were raised after the GFC, younger people generally lacked the means to buy their own homes in the more economically active urban cores(Thiel & Zaumbrecher, 2023). In addition as waiting lists for social housing placements rose, topping 10+ years in most places, they became generally inaccessible for people moving into the cities. As such recent arrivals, younger people, less wealthy people, and temporary residents, such as immigrants, are reliant on private rentals and the main target group for private landlords. While demand for housing grew in the urban cores, and low interest rates and high prospects of rents vs floor space made it attractive for buy-to-let investors to invest in properties in the cities(Bo, 2020). As a consequence urban cores saw high rises in private rental ownership with Utrecht, Groningen, Eindhoven, The Hague, Tilburg, Amsterdam and Rotterdam all seeing the relative share of private rental properties rise with more than 5% with some as high as 8%, compared to 3% nationally. With all of those now featuring shares

of private rental ownership higher than 20% vs 14% nationally, with a selection of those now reaching 30%. (CBS, 2024a).

Several studies have found a link between house prices/affordability and the rise of private investors in the real estate markets. Found was that the introduction of private buy to let investors drove price recovery after the great recession and continued to drive up house prices in the years following the recovery.(Garriga *et al*, 2023). However, in this way private investors have had a special tendency to outbid credit constrained owner occupiers and raise their housing unaffordability(Garriga *et al,* 2023; Austin, 2022). However, as long as investors believe that these houses have more investment value than owner occupiers are able to finance, investors will be able to outbid owner occupiers for the in demand areas of the cities(Conijn, et al 2019;Hanson, 2022) [1]

2.2 Municipal and planning reasons for implementation

In response to the rapid increases in housing prices and rents, that followed from the influx of residential real estate investors. The government and larger municipalities expressed concern for the affordability of living in the cities. Particular concern was raised for middle class families, as they often perform critical professions, such as teachers and firefighters(Hochstenbach, 2023;Schilder & Conijn, 2017; Middelkoop & Schilder, 2017). As these middle class homebuyers are limited by borrowing constraints such as income and regulations they have been outpriced by investors, and started to leave the more expensive cores or the municipality as a whole(Boterman & van Gent, 2023; Savini *et al,* 2016*;* Manting, & Majoor, 2024).In addition political parties and municipalities cite a loss of social cohesion as the tenants in the subdivided dwellings decrease liveability in the neighbourhood by increasing noise complaints, as they are usually younger, and neglecting to take care of both their housing and the neighbourhood due to high turnover rate(Gemeente Rotterdam*, n.d.*;Meurs *et al*., 2022*;*Socialistische Partij Amsterdam*,* 2020; Gemeente Utrecht*, n.d.*).

In an attempt to curb the spread of buy to let transformations major municipalities have adopted a bundle of regulations. This bundle is aimed at regaining control of rapidly increasing house prices in the city centres, stabilise prices and keep housing affordable for owner occupiers. First several municipalities restricted the ability of owners to subdivide established legal dwellings into several independent units, called “kadastraal splitsen” . Secondly a permit has to be granted based for the renting out of a dwelling to 3 or more individual renters (from now on called joint tenancy). Several of the largest municipalities have even adopted caps on the maximum number of joint tenancy units in a neighbourhood, such as Amsterdam, Rotterdam, The Hague and Eindhoven or banned/regulated the practice citywide (Almere & Utrecht). Finally, most of larger municipalities have recently issued a “opkoopbescherming” or buy up protection and banned buying newly built homes for several years after the date of construction.

In principle these regulations are all designed to reduce the investment value of the dwellings and to enable (middle class) families to keep affording homes in the cities. The regulations

[1] At the time of writing the market is or has recently gone through a period of uncertainty as interest rates hiked and inflation grew. This period is outside the research period, but investor buying power might have significantly changed

limit the number of tenants an investor can have for a property; thus it effectively reduces the total rent an investor can extract as it is presumed that several lower incomes put together will have a higher willingness to pay/income than a single family household would have (Conijn *et al,* 2019;Hanson, 2022). By restricting their profit potential, the municipalities hope to reduce house price growth or have it reach an equilibrium for middle class homebuyers. That such measures might have the potential to achieve price decreases is illustrated by a study from Brandenburg where rent controls could lead to 20-30% lower prices as investors had less appetite for the lower yields(Vandrei, 2018).

2.3 Traditional regulatory effects

While regulations against investors can achieve their goal of lowering house prices they risk restricting supply, especially for people dependent on rentals(Ater *et al,* 2021). Much research has been dedicated to the impact of regulations of housing as regulations seem to be the single most impactful influence on the supply of housing(Gyourko & Molloy, 2015). For years the price between construction costs and the price of homes has been rising; indicating that the price of land has been steadily rising(Gyourko & Molloy, 2015;Glaeser & Gyourko, 2003), while there are several reasons for this increase, such as geographic constraints, there is evidence that a " regulatory tax" could add a significant increase in house prices (Anthony, 2017; Gyourko & Molloy, 2015) and even restrict density(Glaeser & Gyourko, 2003). In general, most papers find that increasing regulations will raise prices and lower construction (Gyourko & Molloy, 2015; Anthony, 2017). Generally regulations affect prices in several ways: by directly increasing costs to pay for permits, by increasing waiting times and uncertainties, by artificially limiting supply in the form of minimum lot size, implementing maximum quota's or limiting available developable land at the edges(Anthony, 2017). A potent illustration of the price effects of regulation are Turner, et al (2014) who found that raising the regulation by one standard deviation would raise prices by 36%. However, as Gyourko & Molloy (2015) point out, regulations raising prices does not necessarily mean a welfare loss as the negative externalities they are trying to solve could weigh up against the price increase. For example, regulations can also raise prices due to the preservation of desirable amenities, such as greenbelts or parks(Koster, 2023) or historic heritage(Koster & Rouwendal, 2015).

While both Anthony (2017) and Gyourko & Molloy (2015) offer a comprehensive overview of research into regulatory effects; Anthony focusses on growth regulations and Gyourko & Molloy indicate that there are several gaps in the research as regulations and data are very diverse and differ substantially across even local jurisdictions. Moreover both texts indicate that due to historical data limitations most research was done on cross-sectional research. Furthermore, while consensus is that regulations increase prices and lower supply, studies into the regulation of housing divisions are scarce.

These effects are larger for smaller and older units, in which investors account for a larger share(Ater *et al,* 2016;Ihlanfeldt & Yang, 2021). The results suggest that policies that deter investors can achieve their stated objective of reducing house prices, but are also exposed to the peril of restricting supply of rental housing units, and thus adversely affecting renters.. Echoing this concern, Glaeser and Gottlieb (2008) conclude in a review that "*any government spatial policy is as likely to reduce as to increase welfare*."

2.4 Urban form effects

The implemented regulations primarily affect so called *'soft densification*' as they primarily restrict small scale incremental changes in densification of pre-existing housing stock, by preventing housing stock to be bought up and shared by several households, as well as limiting the viability of plot level developments with a dense amount of units(Dunning *et al,* 2020;Touati-Morel, 2015). This is opposed to traditional 'hard densification' policies that are more actively managed large-scale (re)development projects on sizable green- or brownfield sites.

Densification is a core part of traditional models of new urban economics(Muth,1969;Ogawa & Fujita, 1980;Lucas & Rossi-Hansberg, 2003)Although it does not distinguish between hard or soft densification, it allows us some room for analysis. New urban economics states that the higher land prices in the city centres cause people to trade in space for better access to desirable amenities or jobs. Similarly in response to rising ground and house prices cities will start to divide pre-existing housing units to create additional supply on less used space. Recent international evidence comes from Hong Kong and London, where soft-densification policies were implemented as a solution for rising house prices(Infranca, 2014; Edwards, 2016). Several studies have found that incremental soft densification can provide quite a significant addition in housing supply(Geuting *et al.,* 2023; Bertaud, 2018;Touati-Morel, 2015; Bibby *et al.*, 2021). Studies on implementation in the UK found that it is balanced by negative externalities such as overcrowding and competition for space(Bibby *et al*., 2021). This is best characterised by the examples that historical urban cores used to house as much or more population as their much expanded urban structure does currently(Ekamper *et al*, 2003).In case of the Netherlands; Broitman and Koomen (2016) have found that Dutch cities have seen a return to rising density gradients since the early 2000's, as the inner cities became in demand again. Even as the urban areas kept expanding at the same time.

It is here where opponents of the regulations argue that the inhibition of densification can have unintended or even counterproductive economic consequences. According to the New Urban economics theory, limiting the supply in the city centre below demand, by creating minimum floor sizes, height restrictions or other density restricting measures, will push out the demand to other areas of the city(Lin *et al,* 2020;), as well as spread out the city and create urban sprawl(Bertaud & Brueckner, 2005;Mills, 2005) Here welfare is lost as people have to travel further distances and pay higher transport costs to reach their jobs and the desired amenities(Brueckner & Sridhar,2012). Contrastingly many municipalities have adopted anti-sprawl policies and expansion in peripheral greenfields seems unlikely. This combination can potentially restrict supply growth quite severely as many studies have found that urban growth boundaries and growth regulations can increase house prices substantially(Anthony, 2017; Gyourko & Molloy, 2015; Gyourko *et al., 2013*). This is supported by recent research that indicates significant price effects on real estate when neighbourhoods are subject to building heights, where regulated neighbourhoods saw significant price increases compared to the surrounding neighbourhoods(Buitrago-Mora & Garcia-López, 2023).

However, in the Dutch case, most urban centres already have regulations that aim to protect historical cores, by limiting building heights and other forms of "radical" development, as cultural heritage has been shown to have positive effects on surrounding property values(Rouwendal & Koster, 2015; Franco & Macdonald, 2018). It is possible that in the context of already existing regulation further limits on densification could be negligible and the investor effect would thus cool the market down and is outweighed by the externalities. On the other hand, the effect of restricting or capping subdivisions would limit one of the few remaining options in which extra supply can be realised in the urban cores and exacerbate the supply issue. In this way policy makers could inadvertently drive prices up and potentially push price pressures to other areas of their cities as the demand of high demand neighbourhoods spills over to nearby neighbourhoods(Lin & Wachter, 2020).

2.5 Demographic effects:

One of the stated aims of the package of regulations is to preserve housing supply for middle class families in larger cities(Meurs, *et al.* 2022). Anti-density regulations, like those that discourage subdivisions, could have significant effects on the demographic composition of the neighbourhoods where they are implemented. When there are obstructions to densification, it restricts the development of, for example, denser multi-family units where less wealthy people can trade in space for capital(Bertaud, 2018;Austin, 2022). Similarly, restrictions on subdivisions can prevent poorer people from pooling resources together and living in divided houses. Buy to let investors, while playing a significant role in gentrification, seem to fill in rent gaps in these neighbourhoods and offer options for renters(Paccoud, 2016;Paccoud *et al,* 2020;Thiel & Zaunbrecher, 2023). High levels of restrictions in adding supply such as land shortage or regulations can lead to concentration of incomes in certain cities as demand is not matched by supply and concentrate higher incomes in the cities(Gyourko *et al,* 2013;Ganong & Shoag, 2017). Furthermore regulations can lead to segregation by income sorting and of minorities (Rothwell & Massey, 2009; Pendall, 2007), something which municipalities usually aim to avoid, as buy-to-investors transform larger units into smaller more affordable units in these neighbourhoods. In this way they're providing a service for people who otherwise would be forced out. While overcrowding of cities and fear of "slums" are significant externalities; over regulating subdivisions and other soft density measures municipalities in the Netherlands could inadvertently remove or reduce living options for poorer immigrants, students and others dependent on private rental housing in the more desirable city cores and preserve these for more affluent middle class families that can afford high mortgages. This is especially the case in the Netherlands as many inner cities feature quite strict regulations that aim to preserve the historical cores, which already put significant limitations on densification and redevelopment.

2.6 Dutch context

As municipalities transition to densification as the preferred for of housing supply expansion, of which shared tenancies is an important one in regulated areas, they have to maintain a careful balancing act, as residents are resistant to developments that cause densification and have a large presence in the surrounding debate, while potential future residents have not yet(Herdt & Jonkman, 2023). In Rotterdam this becomes clear as the municipality has recently repealed some of its regulations targeting joint tenancies as they have to balance between homeowners and a student housing shortage[2]. Similarly the municipality of Amsterdam seems in part to implement the regulations to attempt to retain a growing number of middle class families that are leaving the city for surrounding areas(Meurs, *et al*., 2022;Savini, *et al,* 2016*)*. While research on the effects of general regulatory effects are well studied. The effects of regulations on more incremental forms of adding housing supply is fairly limited, however as governments have started looking into these soft densification policies as a way to start adding housing supply the implications of such policies start to

[2] See the news article for an example: https://www.rijnmond.nl/nieuws/1783805/rotterdam-gaat-kamerverhuur-versoepelen-maar-overlast-studenten-mag-niet-erger-worden

matter as municipalities have to navigate between residential opposition, housing shortages and heritage preservation.

Buy to let investors get a lot of blame for the rising house prices in the media and an often stated goal of the implementation of regulation targeting buy to let rentals is to control the growth of housing prices, however several studies could not yet pin point price increases or decreases due to buy to let investors(Francke *et al,* 2023; Aalbers, *et al, 2018*). However tentative evidence exists that a price premium is paid in Amsterdam and the Hague(Jellesma, 2020), and concentrations of buy to let are concentrated in cities with a lot of expats, immigrants and students(Jellesma, 2020). The non-significant price effect might be caused by weak enforcement, as landlords continue to (illegally) rent out their rentals as before(Bouwmeester *et al*., 2023;Meurs, *et al.* 2022).While no accurate numbers for this practice exist, enforcement of the permits does not seem to be a high enforcement priority for municipalities(Bouwmeester *et al*., 2023;Meurs, *et al.* 2022), although it seems a substantial portion is deterred by the regulation(Bouwmeester *et al*., 2023). Another cause could be that an investors approach is able to find better deals and lower costs(Francke *et al,* 2023). Finally it could be that the investment value of investors is in a sort of equilibrium with the willingness to pay of homebuyers. In a recent study that focussed on the short term effects of the buy up ban on new construction in several Rotterdam neighbourhoods, it was found that there were no significant effects on the transaction price, but that supply of rentals declined, rents increased and average income of the neighbourhoods increased(Francke *et al,* 2023). However this could be affected by a significant worsening of the investment climate and the mortgage rates in the period after the introduction, as well as the fact that the ban only applied to houses with an estimated tax value below the €355.000 national mortgage guarantee, which was significantly below the median selling price of €429.000. And the study did find statistically significant evidence for a change in the average resident income after the ban as owner occupiers were able to move in and buy up homes, suggesting that restricting the practice negatively affects income mix.

Some evidence from Utrecht has also been found that landlords indeed shifted or intended to shift their buy to let activities to other jurisdictions when limitations were implemented, however no quantitative research was given(Bouwmeester *et al*., 2023). This suggests that it is possible that the regulations just push the buy to let investors to other areas.

While theoretical research on the effects of housing division regulation are well founded and fall under the broader category of planning. Soft densification policy has recently become more studied in the literature, and are seen as but studies on actual economic effects proves to be scarce. Furthermore research into the effects of these spatial regulations is limited, but incredibly relevant for further attempts to intervene in the housing market. Research suggests that the limitation of incremental densification would prevent the densification of cities, raise prices and spread-out cities. All three of which are things that the municipalities aim to avoid, however residential resistance might be high.

Within a wider context it is unclear whether these regulations will have effect within the context of more restrictive pre-existing regulations, or if they can indeed reduce investor value by making housing more affordable for owner occupiers. At the same time there is now a significant push from government institutions to re-regulate real estate development, with a special focus on limiting landlord excess and negative externalities of private joint tenancy rentals. While there is some research towards the soft densification regulations on housing prices; no conclusive evidence has been given and research into one of the most widespread regulations affecting soft densification, namely anti housing division and joint tenancy regulations, is absent. Even though a significant group of urban residents are dependent on these kinds of housing. For municipal regulators it is crucial to know about the effects of the policies within the current debate on their usefulness.

With larger national regulations, such as the Affordable rent act, poised to take effect; a better understanding of the effects that such regulations have is highly relevant for the debate and key to gain a vision of the reorganisation of the real estate market that might follow from large scale regulatory implementation.

3. Descriptives

3.1 The joint tenancy permit and neighbourhood caps

As stated before most of the Municipalities have broadly adopted three forms of regulations in an attempt to reduce negative externalities resulting from buy-to-let rentals. A ban on the buying and renting of newly built homes for up to four years, the tightening of requirements for dividing up a home into independent units, and finally, the implementation of or tightening of the permit requirements on conversion to rentals that feature more than two independent renters or roommates(*joint tenancy* from now on)[3]. This research will focus on the effects of the last regulation. Specifically the effects the regulation has on prices in the neighbourhoods affected by neighbourhood wide bans on renting to more than two independent renters in the same dwelling.

The first regulation has recently been studied. As Francke *et al.* (2023) have recently published research on the effects of the Buy up ban in the city of Rotterdam. Their research has added valuable insight into the effects of the ban and how the regulation of buy to let sales can have quite substantial implications for a neighbourhood. This thesis aims to gain insights on the complete package of regulations by researching one of the other two regulations.

While the other two regulations seem similar in their long term effects, as it reduces density of residents and prevents smaller living space, research on joint tenancy has several advantages over the other. Firstly the practice it aims to control is more widespread, as many of the largest cities in the Netherlands feature universities, where a significant percentage of students will share dwellings with roommates, and/or have large young professional populations. On the other hand, research on the benefits and possibility of judicial splitting has only recently started gaining traction as a solution to housing shortages. Furthermore the buying up of a home and renting it out to several roommates is much less costly and administratively complicated than the administrative process around judicial splitting of homes is. It requires less investment and does require much less transformative construction work. Hence I expect its effects to translate into data more quickly. The final and most crucial benefit that the regulation on *roommate* rentals has is that some municipalities have issued bans on the practice in specific neighbourhoods. This allows us to compare the effects of the regulations on a treatment and non-treatment group

[3] This is called "verkamering" in Dutch and does not have a perfect English equivalent. I have chosen the form joint tenancy, as it involves multiple independent renters who rent in the same dwelling.

3.2 Data set: NVM or the Dutch association of realtors and appraisers

The dataset that is used for this thesis is the data set of the Koninklijke Nederlandse Coöperatieve Vereniging van Makelaars en Taxateurs/Nederlandse Vereniging van Makelaars or NVM. This is the largest association of realtors and appraisers in the Netherlands and around 75% of all housing transactions are handled by them. Their database is an incredibly rich panel database that provides ample data for a comprehensive overview of the Dutch housing market. It's an incredibly detailed dataset that offers, among others, variables of the houses, house specific coordinates, its 4 and 6 digit postal code, its neighbourhood, district, municipality and several important housing characteristics such as its building type, year of construction and size.

Due to the conditions set by the NVM for acquiring access to the database, I have access to the transaction data of the 10 largest Dutch municipalities (as of 2024) over the years 2000-2022. These are the following cities:

Table 1: Municipalities

Municipality	**CBS code**	**Population 2022**	**Average WOZ 1 jan 2024**[4][5]
Amsterdam	GM0363	882 633	€499,000
Rotterdam	GM0599	655 468	€334,000
The Hague	GM0518	553 417	€363,000
Utrecht	GM0344	361 699	€450,000
Eindhoven	GM0772	238 326	€363,000
Groningen	GM0014	234 950	€301,000
Tilburg	GM0855	224 459	€326,000
Almere	GM0034	217 828	€375,000
Breda	GM0758	184 702	€390,000
Nijmegen	GM0268	179 100	€373,000

[4] WOZ or the property valuation according to the municipal tax authority, determines the property value of the house in the year before it. The valuation on the 1st of January 2024 is the effective valuation on the 1st of January 2023. Or the first date we have when 2022 is finished. Numbers are rounded to a clean thousand.

Furter note: WOZ valuations tend to be somewhat lower than actual market prices as the tax authorities have to value somewhat conservatively to avoid the legal results that would result from over valuation of the property.

[5] All data drawn from CBS statline, CBS(2024b):

There are over 730,000 transactions in the major cities over this time span. With around 25,000 to 42,000 transactions per year. The transactions peak right before the Great Recession and in the recovery period of 2015-2018 with a sever slump in transactions around the Great Recession. The years from 2020 onwards show a decline as housing shortages on the market start to become noticeable.[6]

Table 2: year frequencies

Year of observation	Freq.	Percent	Cum.
2000	25129	3.43	3.43
2001	28163	3.85	7.28
2002	29422	4.02	11.30
2003	30885	4.22	15.52
2004	32760	4.47	19.99
2005	35780	4.89	24.88
2006	37251	5.09	29.97
2007	37486	5.12	35.09
2008	33124	4.52	39.61
2009	26394	3.60	43.21
2010	26693	3.65	46.86
2011	24400	3.33	50.19
2012	24145	3.30	53.49
2013	22979	3.14	56.63
2014	32848	4.49	61.12
2015	39286	5.37	66.48
2016	42064	5.75	72.23
2017	38576	5.27	77.50
2018	34461	4.71	82.20
2019	34808	4.75	86.96
2020	36471	4.98	91.94
2021	30911	4.22	96.16
2022	28116	3.84	100.00
Total	732152	100.00	

3.3 Descriptives:

The dataset had already been cleaned of extreme outliers and missing values, however for this research not all the data in the set is relevant. Hence I have dropped several observations, besides the normal cleaning of the data. First of all, in all of the municipalities which I observed the need for the permit is required the moment the dwelling is slated to house three independent roommates. Hence have dropped all observations that featured less than three rooms, as this would make the dwelling unsuitable for housing 3 independent renters. This

[6] Full table in the appendix

removed about 105.000 observations. Similarly I have dropped all observations below 40 square metres as I expect this to be too small for the existence of enough space for 3 independent renters. Although this dropped few observations, since most were already dropped by the room requirement. I have furthermore dropped all observations below €75.000,- as this amount is very low for current house sales and will not be representative of the free market housing transactions we are looking for. Going higher than this number is going to risk eating into the bell curve as many earlier observations do feature sales in the 100.000's. Similarly I have dropped all observations of transactions above €1.250.000,- as these sales will most likely be dwellings that are not meant for buy to let exploitation. Instead these are most likely used for private use or as high-end speculation or investment objects. Hence these sales will not be affected by the regulation and will only skew the data upwards. While this bar coul For the same reason I have dropped observations with sizes of more than 350 square metres. Additionally I have dropped observations that featured the property types: Bungalow, Estate(Landgoed), Penthouse and Residential farm(Woonboerderij). These are all types in which I do not suspect large scale buy to let of more than 3 renters to take place. This results in a dataset of around 604.000 observations.

Table 3: Observations by year filtered

Year of observation	Freq.	Percent	Cum.
2000	19291	3.19	3.19
2001	22607	3.74	6.94
2002	23856	3.95	10.89
2003	25034	4.15	15.03
2004	26115	4.32	19.36
2005	28244	4.68	24.03
2006	29200	4.84	28.87
2007	30249	5.01	33.88
2008	27655	4.58	38.46
2009	22012	3.64	42.10
2010	22312	3.69	45.80
2011	20324	3.37	49.16
2012	20411	3.38	52.54
2013	19183	3.18	55.72
2014	27321	4.52	60.24
2015	32453	5.37	65.62
2016	35105	5.81	71.43
2017	32833	5.44	76.87
2018	29419	4.87	81.74
2019	29818	4.94	86.67
2020	31314	5.19	91.86
2021	25645	4.25	96.11
2022	23515	3.89	100.00
Total	603916	100.00	

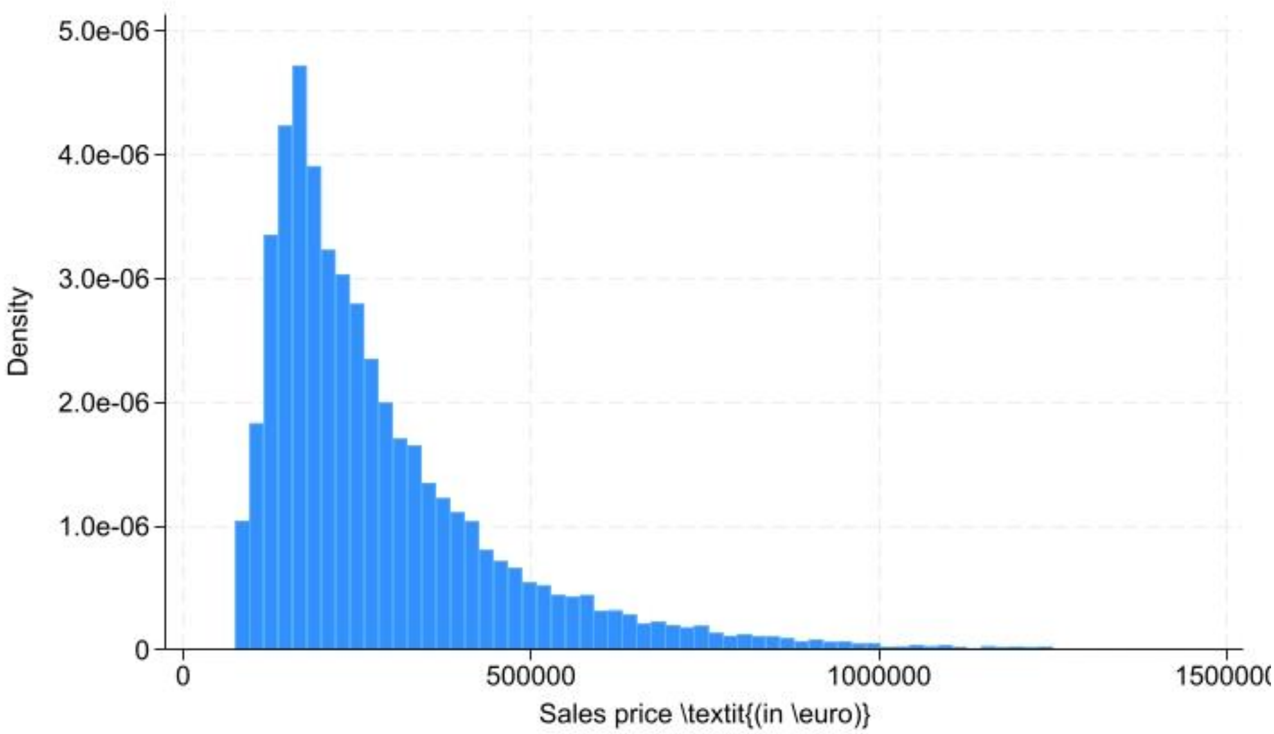
As we observe we see that the dropped data has been fairly consistent along the run time with some slightly more severe drops in the earlier years as houses in this time are slightly cheaper and a larger share of the sales in those years were probably below the €75.000 threshold. This is okay as we are mostly interested in the last decade or so. Data is still relatively evenly spread between roughly 20.000 and 35.000 or between 3.2% and 5.8%, but we do see peak percentage of sales in the years leading up to the 2008 crash and in the recovery years in the later 2010's with a slight decline in more recent years. The mean of the entire dataset is €286.000 euros, however the standard deviation is very high at €178.000. This is not too surprising, because, as we will see later, price levels across the whole timeline have risen significantly and can differ quite substantially between cities, so large standard deviations are expected.

Table 4: Frequency by municipality and year

Year of observation	Municipality identifier										
	Groningen	Almere	Nijmegen	Utrecht	Amsterdam	Rotterdam	Den Haag	Breda	Eindhoven	Tilburg	Total
2000	1526	1966	727	2661	3190	3718	2016	1244	1268	975	19291
2001	2004	2301	946	2945	3331	4534	2506	1390	1387	1263	22607
2002	2151	2019	1089	2888	3632	5121	2595	1537	1409	1415	23856
2003	2348	1777	1187	3033	3877	5212	3074	1630	1429	1467	25034
2004	2384	1976	1178	3098	4347	5077	3159	1763	1600	1533	26115
2005	2335	2084	1391	3411	4927	5253	3330	1903	1809	1801	28244
2006	2559	1959	1267	3406	5402	5369	3817	1777	1779	1865	29200
2007	2669	1876	1277	3433	5852	5500	4182	1798	1759	1903	30249
2008	2315	1664	1256	3295	5447	4826	3924	1630	1611	1687	27655
2009	1976	1314	1050	2538	4809	3667	3083	1218	1124	1233	22012
2010	2002	1269	1121	2743	4842	3538	2961	1291	1296	1249	22312
2011	1785	1181	1003	2401	4606	3118	2742	1139	1239	1110	20324
2012	1750	1312	1043	2417	4750	2886	2602	1132	1359	1160	20411
2013	1685	1128	940	2298	4665	2723	2300	1144	1232	1068	19183
2014	2245	1531	1229	3568	6866	3725	3390	1621	1763	1383	27321
2015	2583	1892	1480	3984	7895	4729	4139	2033	1980	1738	32453
2016	2729	2300	1740	4088	7631	5283	4720	2143	2278	2193	35105
2017	2653	2360	1693	3800	6565	4902	4244	2047	2244	2325	32833
2018	2391	2180	1524	3555	5834	4352	3585	1857	2194	1947	29419
2019	2402	2301	1584	3624	6203	4344	3492	1892	2072	1904	29818
2020	2487	2146	1584	3847	6782	4462	3812	2058	2147	1989	31314
2021	1902	1680	1229	3149	5955	3759	3070	1652	1757	1492	25645
2022	2026	1520	1248	3134	4987	3307	2787	1463	1587	1456	23515
Total	50907	41736	28786	73316	122395	99405	75530	37362	38323	36156	603916

If we look at the distribution per municipality and by year we can see some obvious differences between cities. Amsterdam has about 3 times as many transactions as Nijmegen, Eindhoven, Tilburg or Breda have. This is largely expected as Amsterdam also has a population that is three to four times greater than these cities. Utrecht has an outsized number of transactions that is as large as the number of transactions in Rotterdam, which can explain the high price level in the city. Interestingly Groningen has quite a significant amount more transactions than the similarly sized cities of Eindhoven, Almere and Tilburg. If we look at the sales over the last 5 years in the dataset around 30,000 sales in Amsterdam, around 9000 sales in Almere, Breda, Eindhoven and Tilburg, around 17.000 in Utrecht and Rotterdam, 20,000 in the Hague and 11,000 in Groningen.

Figure 1: Price distribution curve

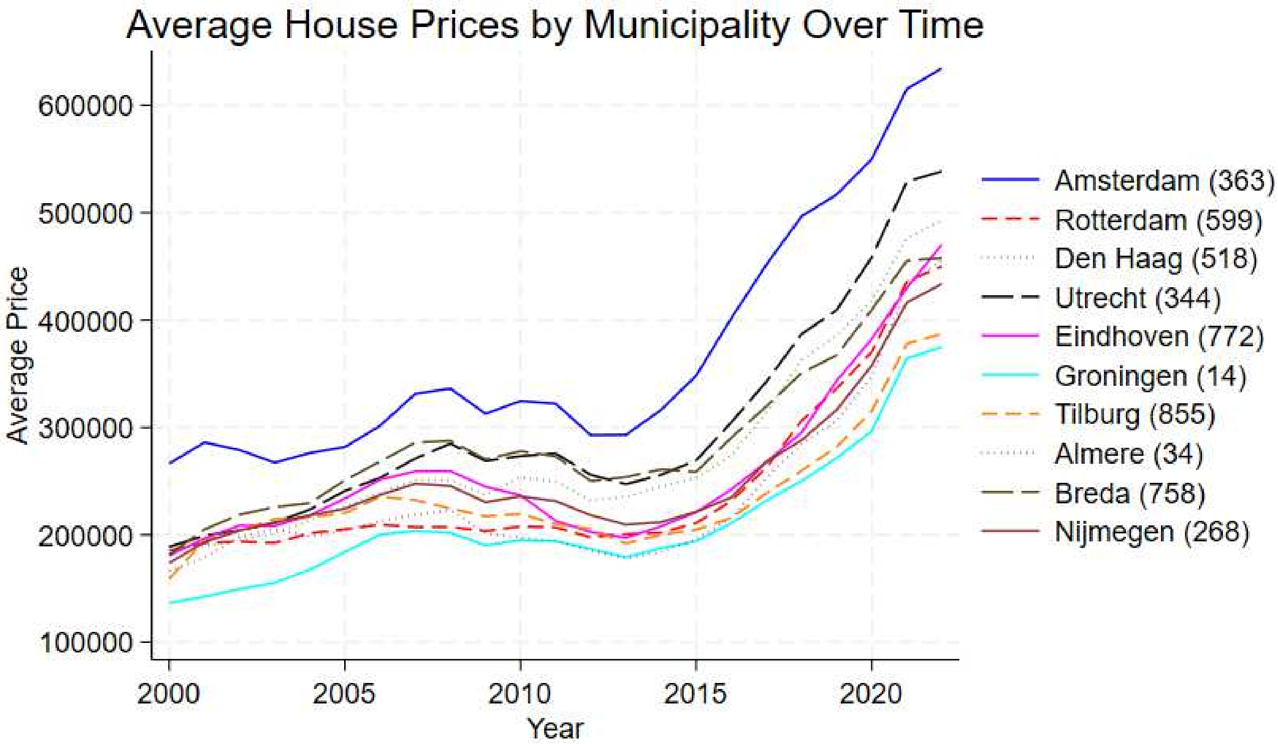


If we take a look at the remaining distribution we find a bell curve with a tail to the right. This is expected as the sales prices rose in the latter part of the time frame. While the frequency at the end of the tail seem to become quite insignificant these frequencies are primarily in the later years when the regulation is in effect. For example: Amsterdam, has an average sales price of around 600.000[7] in the later years and many areas will have a square metre price of more than €10.000 (Gemeente Amsterdam, 2024), and the city as a whole had an average price of more than €9.000 in 2022 and while the average house size has been steadily shrinking houses over 100 square metres are generally quite standard[8]. Furthermore,

[7] See graph 2: Average price
[8] See graph 4: Average size

single family houses reached an average price of €954.000 in 2022 (De Hypotheker,2024). And as our observations of interest have to feature 3 or more rooms there is a likelihood that the observations of interest have higher surface area's. This means that there is a significant chance that several observations we want to observe are being sold for prices above €1.000.000. Hence I have capped the upper boundary at €1.250.000.

Figure 2: Average price[9]

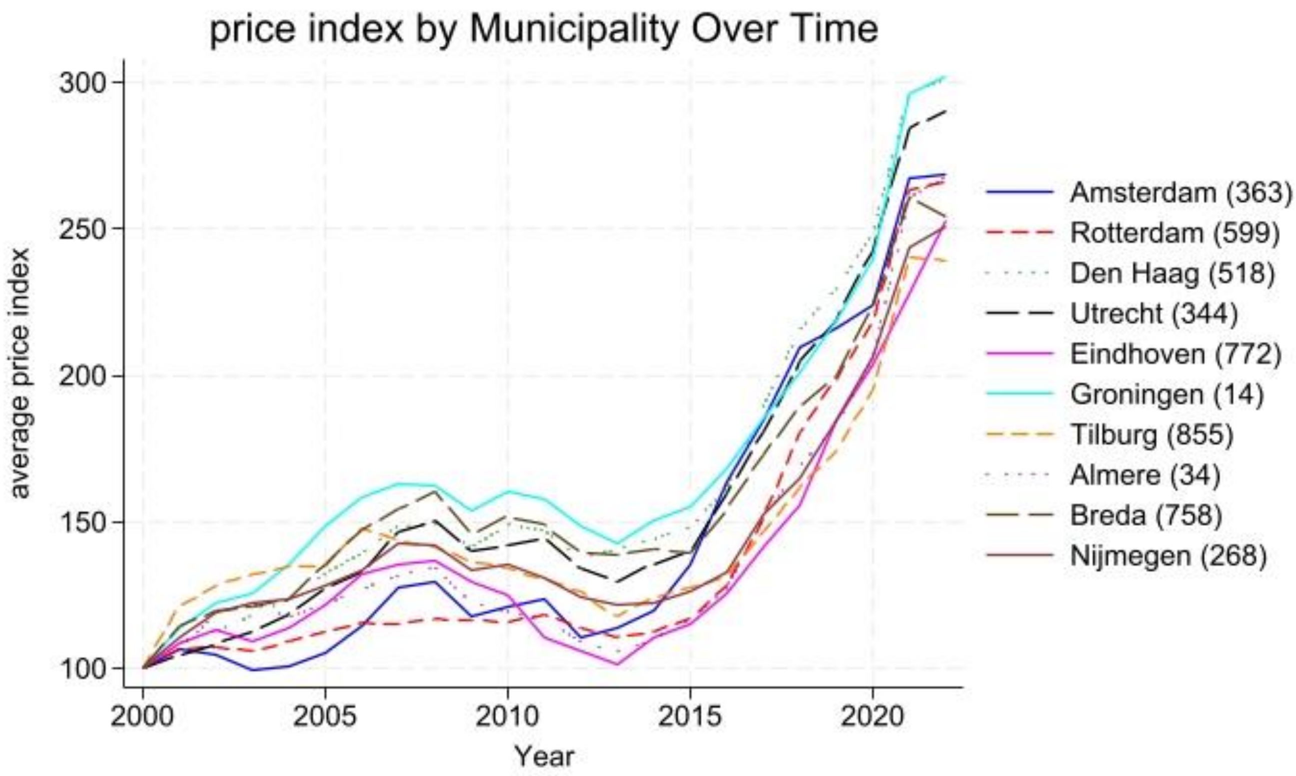


If we take a look at the average price development of the remaining data we see a price trend that is in line with what we would expect of market developments the last two decades, with steep price increases the last decade or so following a slump that started in the 2008 financial crisis and resulting housing market crash. Clear is that the houses in Amsterdam are significantly higher than in most other cities and Groningen mostly rounds out the bottom. The data thus does not seem to be edited in a way that deviates strongly from general market trends. Interesting is that according to this graph the municipality of Utrecht has the second highest average house sales at the end and has made a significant divergence from the rest of the pack in the last 7 years of the dataset. In addition, Rotterdam features between the bottom and the middle of the pack and is even ranked lower than Breda and Almere at the end. This is unexpected as larger cities generally have higher house prices. However, if we look at the average price per square metre of the sales we see Rotterdam crawl back to the top with only the other 3 big four cities with higher average square metre prices. As Rotterdams higher m2

[9] This graph is based on the edited version of the data, so it will give a more average view of the house sales as a significant amount of high and low outliers were dropped. I have a larger version of the graph in the appendix

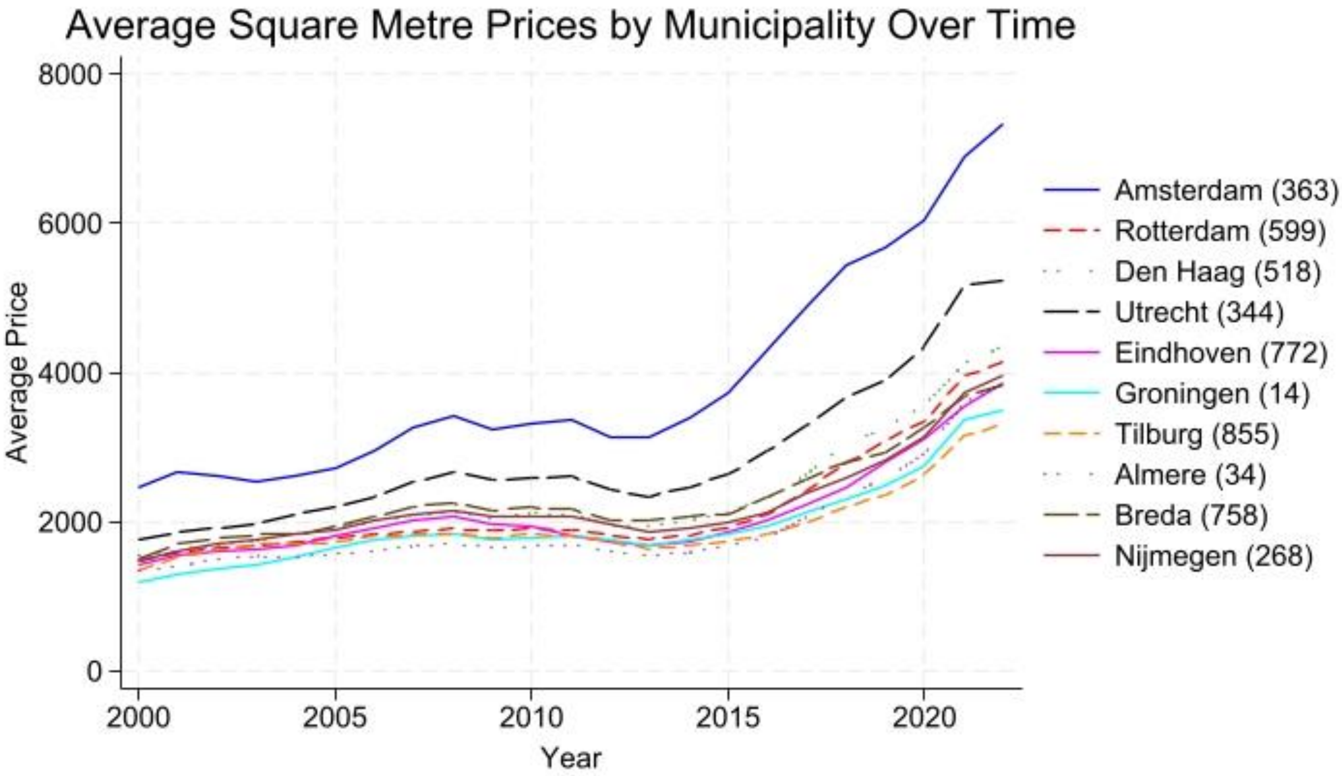
prices are apparently more than compensated by living on less space.This graph more than any other shows the big gap between Amsterdam and, to a lesser extent, Utrecht, and the rest of the biggest cities. Of note for us is that if we look at Eindhoven and Rotterdam together; we can see them as quite representative of a more average of the big 10 cities, while Amsterdam is an exceptional case. The treatment areas thus include a spread over different price levels.

Figure 3: Price index development by municipality



If we look at the relative development we do see more clustering of the municipalities around an similar trends overall, however we do still see large differences between municipalities that can reach up to 50 index points between the highest and lowest municipality. Interestingly we do not see the same pattern in the total growth over the total period as we saw with the average prices. Amsterdam does not seem to be the fastest grower despite the large gap in average house prices as the Hague, Groningen and Utrecht all over take it. Rotterdam even drops to last place.

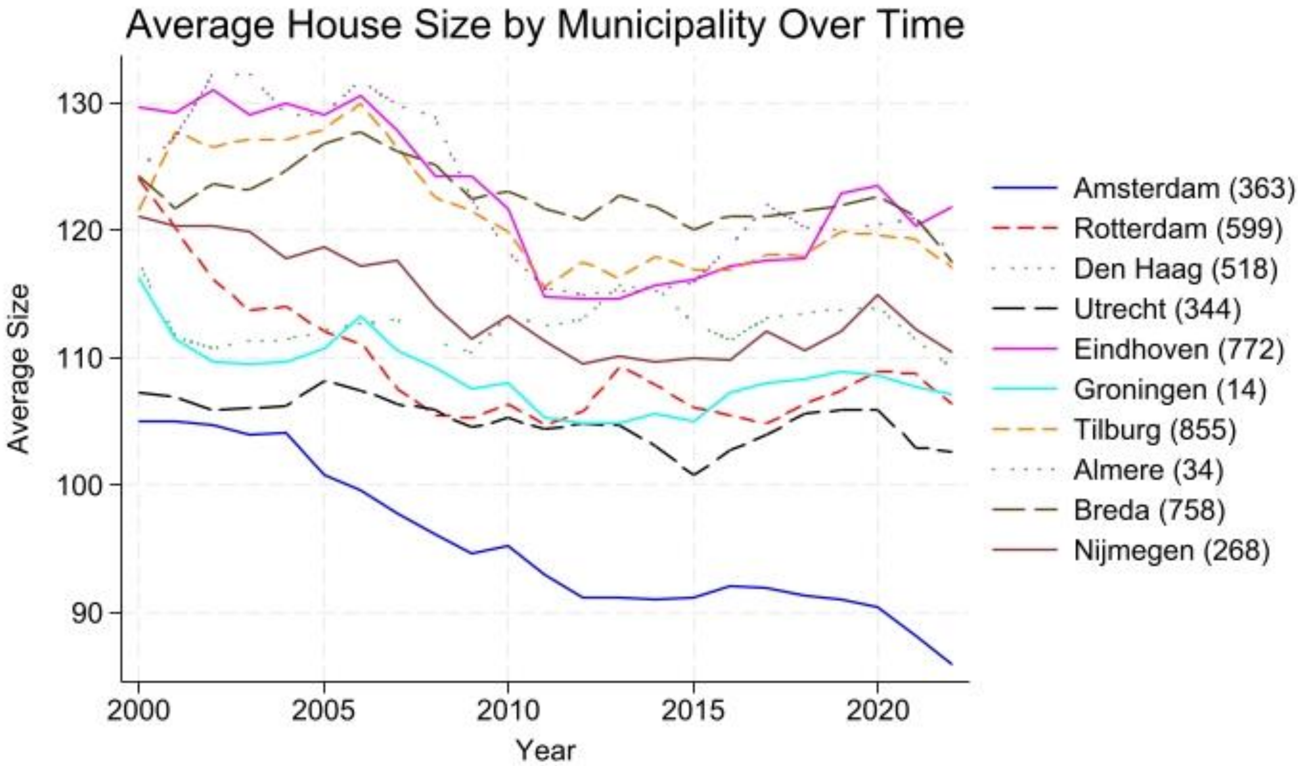
Figure 4: Average square metre price[10]



Furthermore, as expected, if we look at the average size of the sold houses we do generally see a reduction in size over time as houses become more expensive. This trend is especially noticeable in Amsterdam, but the trend looks largely an inverse of the average square metres price.This is in line with the theories stemming from the Monocentric city model as people trade in space for other benefits and higher prices.

[10] This graph is also drawn from the edited data

Figure 5: Average surface area[11]



In general, the dataset does not show anything unexpected and since it encompasses around 3/4ths of all residential real estate transactions in the Netherlands it is unsurprising that its results do show the developments we would expect it to be relatively similar to the actual market over the last 20 years. Important to note is that choices made in editing the dataset do not seem to result in significant diversions from what we would expect from the market.

3.4 Treatment criteria

While most municipalities have implemented the *roommate* regulation. There are differences in how municipalities implemented them. In Almere for example the entire city was subjected to a ban on new joint tenancies. While in other cities, such as Tilburg the limit is based on a radius from already existing houses and in Utrecht and Nijmegen it is judged on a case-by-case basis. Several municipalities have designated several neighbourhoods where they will not grant permits for buy to let conversions for more than 2 persons. These are Amsterdam, Rotterdam, The Hague and Eindhoven. However, the neighbourhoods of the Hague are tied to the WOZ value being lower than 405.000 euros (national mean at the time), which would cause endogeneity issues as those neighbourhoods are chosen because they have a lower average price and a lower price level in those areas could be a result of those prices instead of. As a consequence, these neighbourhoods have not been selected in the treatment neighbourhoods.

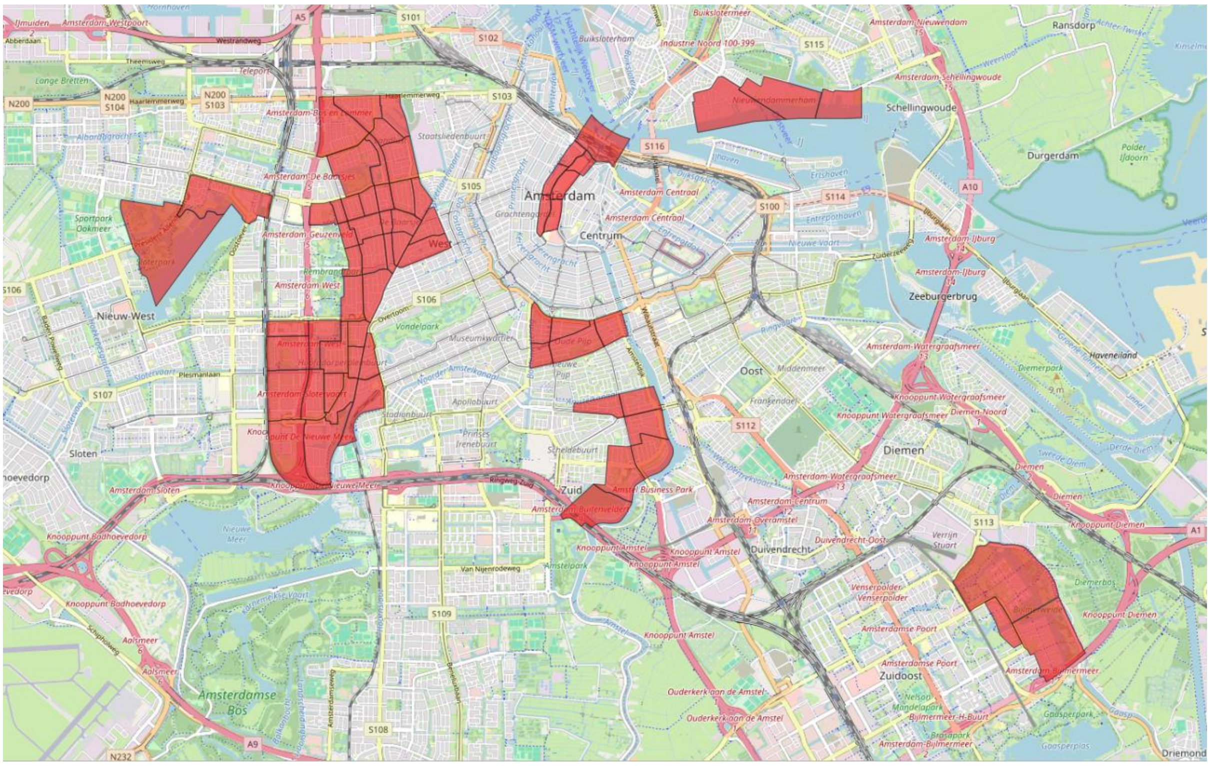
[11] This set is taken from the unedited data, to show a general trend. I assume its less severe in the edited data as we control for lower sizes and a minimum number of rooms

Due to the agency of each individual municipality in implementing this regulation, there is some difference in the implementation date of the roommate cap. In Eindhoven for example the regulation was implemented in 2019 for most neighbourhoods, but two more were added in 2020. In Amsterdam the general implementation took place in 2020, but with some leniency in 2020 and 2021 and some neighbourhoods were added as the 5% threshold was crossed at a later date.

3.4 Treatment area's

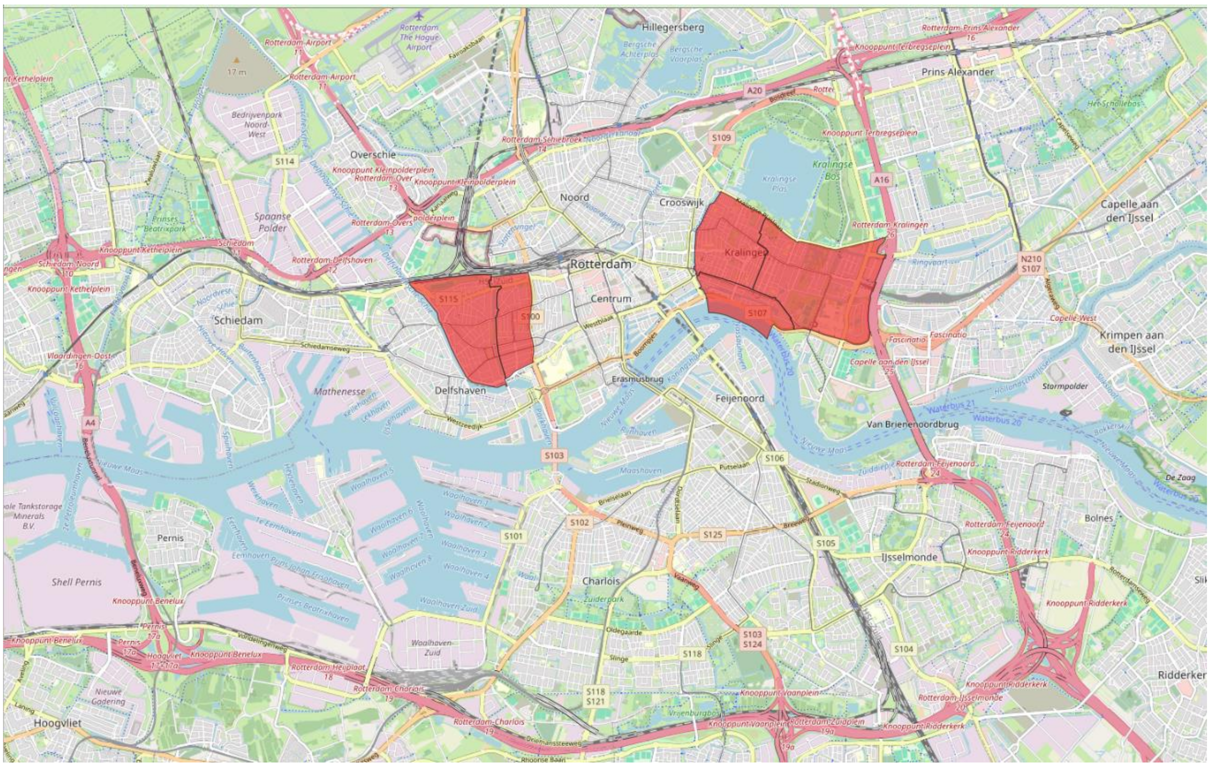
We identify neighbourhoods that have been selected based on their name and neighbourhood identification in the NVM data. This neighbourhood identification, or nid, is based on a classification by the Dutch Centraal Bureau voor de statistiek or CBS. From the names applied by the CBS and provided by the municipalities of Amsterdam, Rotterdam and Eindhoven I have drawn the neighbourhoods subjected to a neighbourhood wide ban on any new roommate rentals and tied those to the nid given in the NVM dataset. In the case of Amsterdam the provided neighbourhoods were classified a level higher(district id) than the neighbourhoods in the other cities. I have taken all the nid's of these "districts" and added them. The neighbourhoods chosen as the treatment neighbourhoods are the following:[12]

Amsterdam:

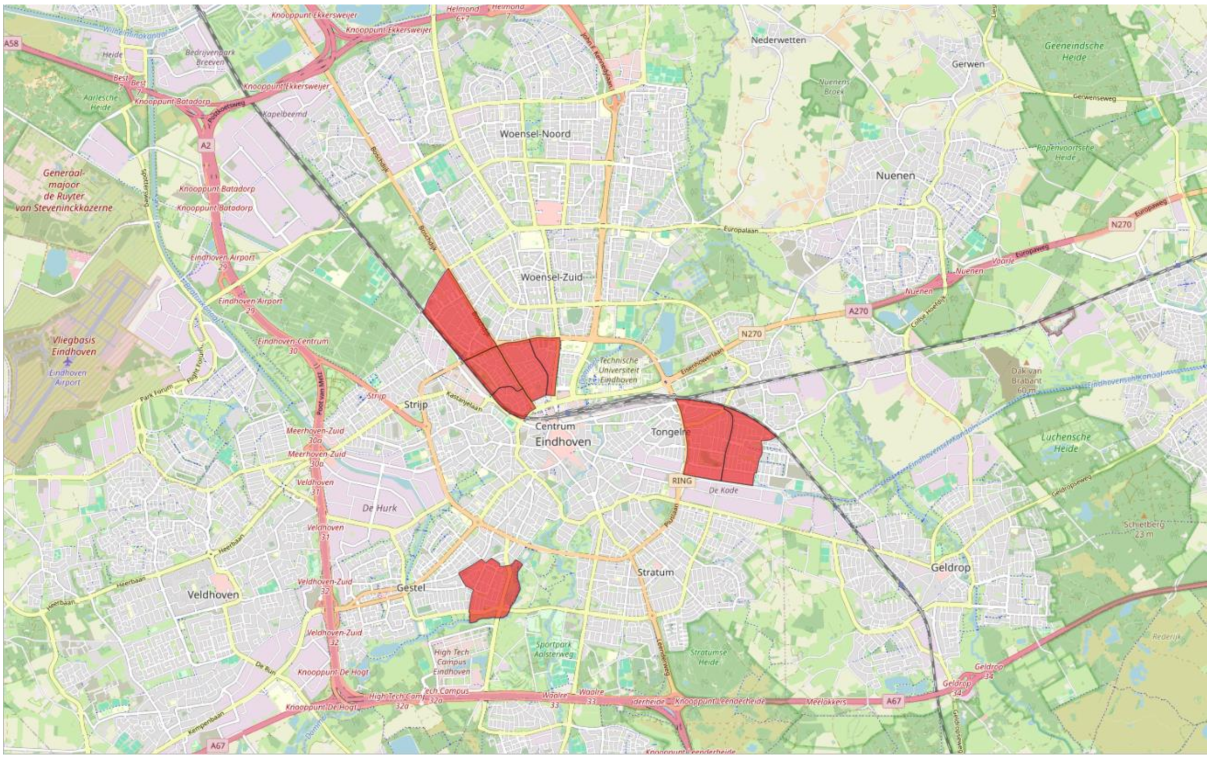


[12] The complete list with neighbourhood names and identifiers can be found in the appendix

Rotterdam[13]



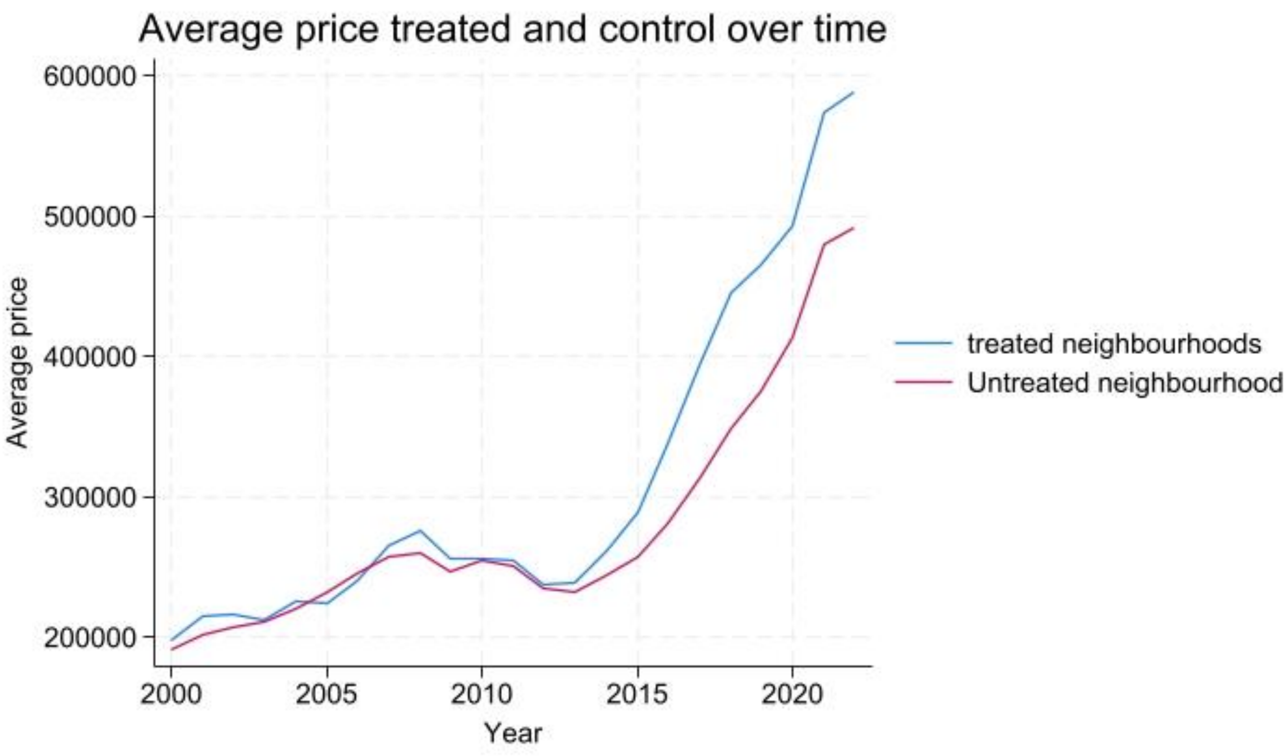
Eindhoven



13 Op de site van de gemeente Rotterdam zijn nog twee gebieden in Charlois aangekaart als nulquotum gebied. Deze zijn pas in 2023 ingevoerd

In total there are 65 neighbourhoods where a ban has been implemented[14]. Out of these 65, 51 are located in Amsterdam, 6 in Rotterdam and 8 in Eindhoven. This significant imbalance is partly caused by the aforementioned classification effect as Amsterdams neighbourhoods were selected on a district level and a result of Amsterdam's higher price level and more widespread occurrence of buy to let. However, the significant imbalance towards the neighbourhoods in Amsterdam has to be taken into account in the research on treatment effects. I have furthermore dropped two additional large areas in Amsterdam: The Coenhaven/Minervahaven and IJburg-Oost as these areas are currently large-scale developments and any price changes here are because of those developments instead of any treatment effects. Amsterdam furthermore has 10 neighbourhoods that have been dedicated as "limited availability". Which have area's available, but it is very uncertain if your permit will be granted. I have chosen not to include these, as these will have had space left in 2020 onwards and I assume that their prices are not affected.

Figure 6[15]: Average price treated vs untreated

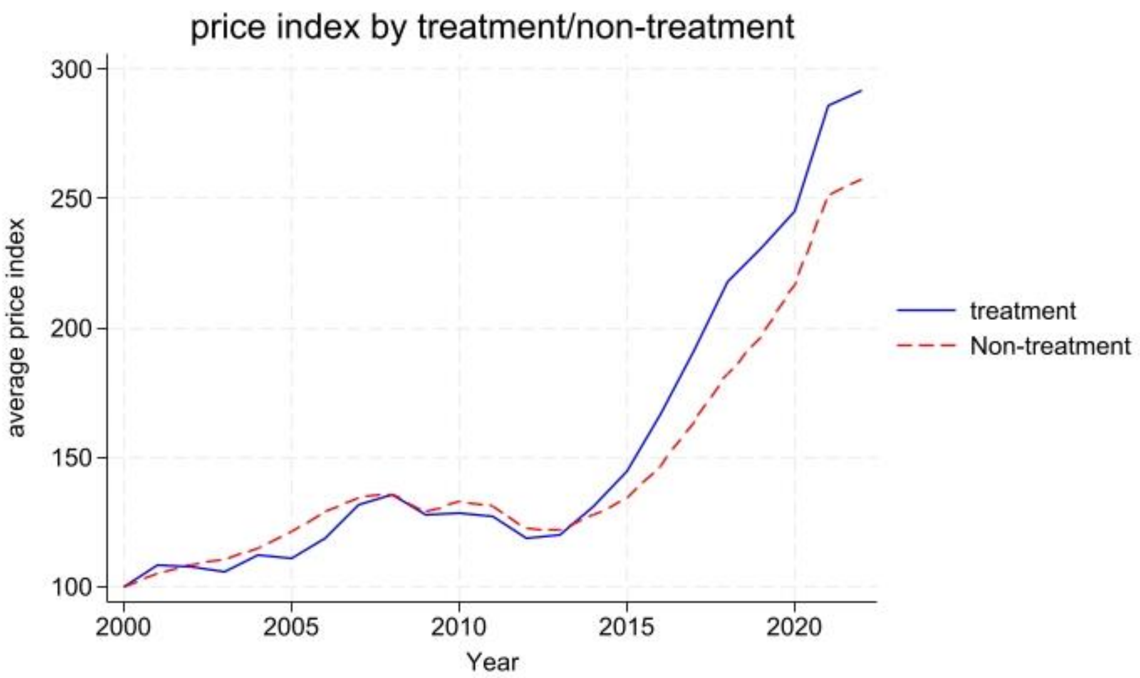


If we look at the development of average prices over these years in the treatment neighbourhoods, we see that the price levels were at roughly the same level until the start of the recovery after the great financial crash. After that we see that the prices in the treated neighbourhoods started to grow much faster, although the rest was rising quickly as well.

[14] See appendix 1

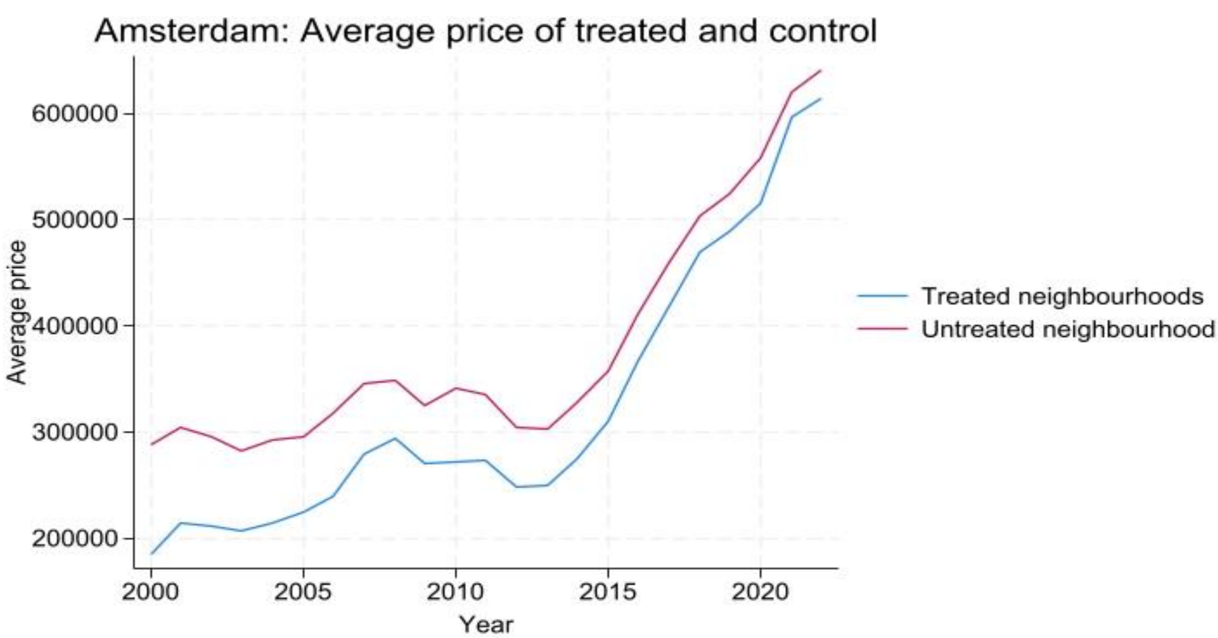
[15] Does not include "Limited availability" neighbourhoods

Figure 7: Price index treatment vs non treatment areas



In the index we can clearly see that the average prices in the treatment areas start to diverge from the non-treated areas around 2013. As this is relative increase to the base value, we can see that the prices in the treatment area have clearly risen faster relative to their starting value than prices in non-treated areas.

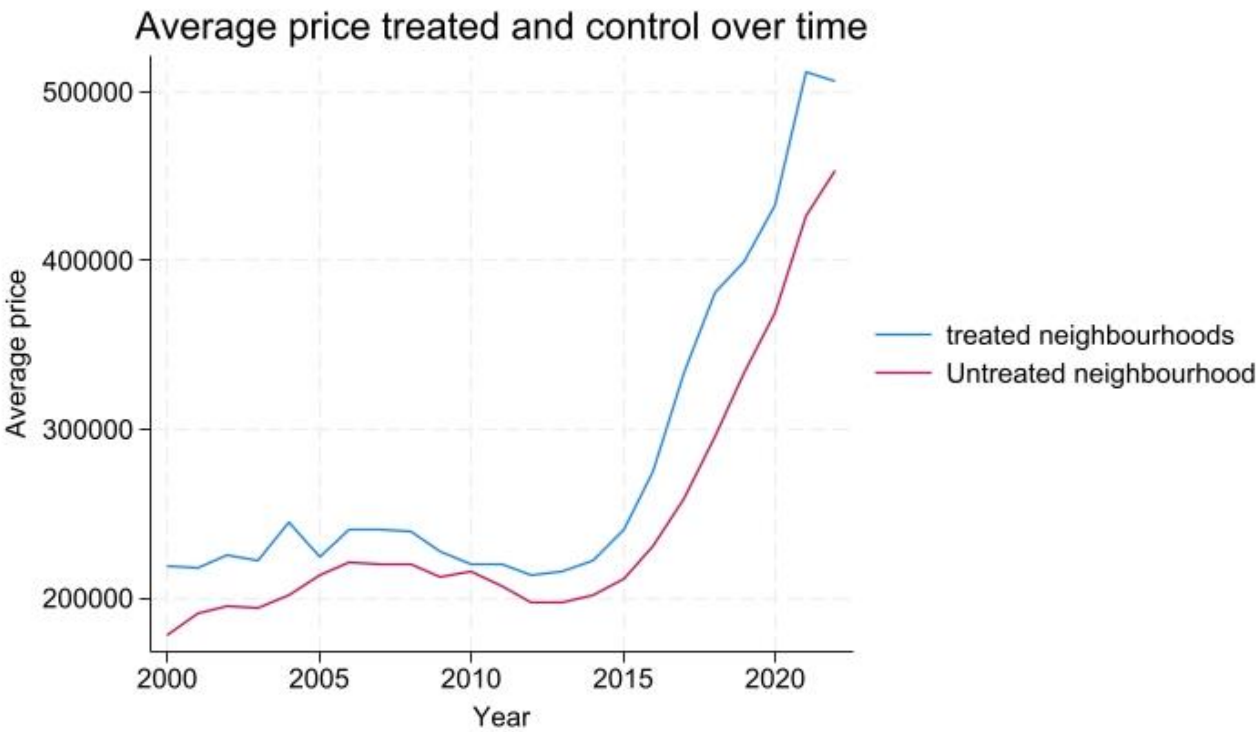
Figure 8[16]: Average price Amsterdam treatment



[16] Does not include "Limited availability" neighbourhoods

As Amsterdam makes up >75% of the treatment neighbourhoods it could cause the higher price levels of the treated neighbourhoods in comparison to their non-treated counterparts. If we look at the price development of the treated neighbourhoods, we see that the treated neighbourhoods are generally less valuable than other neighbourhoods, but have been rising more quickly since 2013e and start to diverge slightly after 2020, as the treated neighbourhoods growth starts to slow. This is coincidentally when the regulation was introduced and could indicate a treatment effects. In general developments seem to follow the same trends as the complete picture, although with differing speeds.

Figure 9[17]: Treatment neighbourhoods excluding Amsterdam



If we look at the graph of the neighbourhoods in Rotterdam and Eindhoven together, we do still see that the treated areas have higher average prices over time, but the difference is not as large, furthermore the average prices in the treatment seem to decline after 2020. Whereas the untreated neighbourhoods continue to rise. This could point to an effect of the treatment. Furthermore we see that the price rises start a couple of years later than they did in Amsterdam,

While we see that there is a gap between the treatment neighbourhoods and the untreated neighbourhoods and significant differences between the neighbourhoods in cities, this is not necessarily a problem, but attention must be paid to control for area specific differences.

[17] Further splits are in the appendix. But the higher average price in the treatment is due to Rotterdams treatments being more expensive than other area's

The “normal” treatment area features about 6.2% of all the observations in the dataset[18], while the treatment neighbourhoods make up 4.65% of all neighbourhoods in the dataset. It is not too unsurprising that the treatment areas have an outsized effect as they can be expected to be areas with more investor activity, otherwise the municipalities would not have been perceived the neighbourhoods in need of regulations. The treatment area had between 1800 and 1400 observations each year of the last 5 years in the set. Over these same years the share of the transactions stemming from the treatment areas stays relatively stable between 5.65% and 6.2%.

4. Model

As we have access to a very rich database of around 3 quarters of all housing transactions in the Netherlands and the availability of clear areas where new roommate rentals are no longer allowed, thus thesis will use a quasi-experimental hedonic pricing, specifically a difference in difference set up to study possible price effects of the new regulations of this implementation. Model is based on traditional hedonic pricing models, although most hedonic pricing models were cross sectional. As our dataset allows for a longitudinal set up as, we will extend this by implementing a difference in difference set up. This way we can indicate between pre- and post-treatment. The basic difference in difference model looks as follows:

$$(1) \quad Y_{ijt} = a + \beta X_{ijt} + \beta X_{jt} + a_i + a_j + \delta\, T_{ijt} + u_{ijt} + \varepsilon$$

where:
Y = is the dependent variable, most frequently the log price.
i = indicates the object specific effects, these are the specifics of the dwellings and not the transactions. These include having a garden or the number of rooms.
j = indicates the effects of development specific to the area. This could include municipal developments, neighbourhood and even on the 4- or 6-character postal code level development.
t = is a time specific development like a general housing market growth effect between two years.
$X_{ijt} =$ indicate all effects that are affected by time, location, and object, these
$X_{jt} =$ Indicate the effects that are affected by time and area.
$a_i + a_j$ = indicate all effects specific to time and area respectively.
T_{jt} = indicates the effect of the Treatment. This can be affected by both area and by the time period.
u_{ijt} = indicates all the unobserved effects of of area, time, and individual objects outside of the dataset
ε = indicates the noise in the set

[18] Full table in appendix

We indicate two areas: one equals the neighbourhoods where new permits to for shared or roommate rentals were no longer granted and the neighbourhoods where permits are still available. This provides for a clear treatment and non-treatment group which is designated by a dummy, which we will call $Treatment$.
To control for the post treatment effect I have added year dummy's for every year in the dataset and feature time fixed effects. I then created a new variable to measure the effects of the treatment in the post.
As the dataset does not provide data on amenities and other effects that might have spatial spillovers in house prices we control for spatial and time fixed effects.

Finally, as hedonic literature stresses, house prices can vary wildly based on the characteristics of the house that is up for transaction. Hence I control for almost all the available housing characteristics in the dataset, such as size, maintenance , construction period, type of residential dwelling, and if it is a newly built residence.

As it is a longitudinal dataset the fixed effects require an additional interaction to capture the interaction for area*year. As our coefficients of interest are based on the interaction between the spatial unit of a neighbourhood and the year, the model will conduct this effect on a municipality level instead of a neighbourhood level. Putting it on the neighbourhood level would cause collinearity with the main interaction of this research. Since real estate markets are relatively related on a city level I believe the municipality is a good alternative to control for market related price developments, but retain inner city price fluctuations. In this way the model can still arrive at useful coefficients for our objects of interest.

Because we are interested in neighbourhoods all the regressions will be clustered on the nid, as we are interested in the neighbourhood effects of the treatment and less in the individual differences.

The model for use in the regressions will then look as follows:

$$LogPrice = \alpha + \beta_1 Post + \beta_2 Treatment + \delta(Treatment * Post) + \theta H + \tau + \tau\varphi + \varphi + \epsilon$$

Where LogPrice indicates the log of the transaction prices of the neighbourhoods. α is a constant β_1, β_2, θ & δ are coefficients to be estimated, where δ is our coefficient of interest. H is a representative of object specific characteristics. And finally τ & φ represent time and spatial fixed effects respectively

5. Analysis

As the implementation of the joint tenancy regulation was mostly implemented in 2019 and 2020 and the dataset features observations until 2022, I do not expect the long term supply issues to materialize yet. Hence, I expect that the difference in difference regression will yield a negative effect on the prices post treatment.

I will first estimate the baseline difference in difference, by estimating a simple version of the model. In this version I estimate the coefficients of the treatment area, the year 2022, and the interaction between the two. The year 2022 was chosen as this is the both the first year past a transitory period for some municipalities and the final year in the dataset. The first regression is without any control variables or fixed effects. Next, I extend the amount of controls by including house specific controls by including various house specific variables, such as: if it's newly built, the construction year, and dwelling type. Finally, I add fixed effects of both the years and the neighbourhoods and the interaction of year and municipality, so the spatial and time specific effects are taken into account.

5.1 The basic model

The simplest form of the model gives us a coefficient of 0,09. This means that the treatment group neighbourhoods would have 9.4% higher transaction prices than the non-treatment group neighbourhoods in 2022. This is significant at the 1% level. The treatment area price change itself is only significant at the 5% level, but this only features as a control variable. This is quite a large effect; however we have not controlled for anything yet. If we control for house or location specific qualities, we see a rise in the coefficient to 0.134 or a difference in treatment and non-treatment price levels of 14.3%. This is even larger, however as there is a significant difference in price levels between cities and Amsterdam with the highest price and average prices makes up a significant number of the treated neighbourhoods, controlling for house size would increase the difference significantly. So, when we implement the fixed effects and control for time and area differences we see that the coefficient decreases to 0.0681 for a price difference of 7%. The first results of the basic model would imply that the treatment areas are positively correlated with higher prices.

Table 5: Basic model results

VARIABLES	(1) simple	(2) house specific	(3) fixed effects
interactie_all_22	0.0900***	0.134***	0.0681***
	(0.0271)	(0.0194)	(0.0118)
treatment_all	0.0825*	0.187***	
	(0.0483)	(0.0562)	
dummy_2022	0.649***	0.671***	
	(0.00797)	(0.00550)	
logsize		0.838***	0.791***
		(0.0222)	(0.00756)
maintgood		0.165***	0.131***
		(0.00687)	(0.00232)
newbuilt		0.0529***	0.102***
		(0.0178)	(0.00437)
apartment		-0.249***	-0.411***
		(0.0253)	(0.00872)
terraced		-0.354***	-0.326***
		(0.0186)	(0.00684)
semidetached		-0.263***	-0.238***
		(0.0160)	(0.00605)
garden		-0.00251	0.0196***
		(0.0105)	(0.00374)
constrlt1905		0.447***	0.0362***
		(0.0249)	(0.00738)
constr19061930		0.187***	-0.00821
		(0.0258)	(0.00726)
constr19311944		0.0144	-0.0244***
		(0.0318)	(0.00840)
constr19451959		-0.0215	-0.0675***
		(0.0261)	(0.00760)
constr19601970		-0.135***	-0.108***
		(0.0228)	(0.00633)
constr19711980		-0.130***	-0.0742***
		(0.0196)	(0.00701)
constr19912000		0.0408**	0.0603***
		(0.0198)	(0.00661)
constr20012010		0.187***	0.0831***
		(0.0196)	(0.00752)
Time Fixed Effects	No	No	Yes (year)
Spatial Fixed Effects	No	No	Yes (nid)
Year * Municipality FE	Yes	Yes	Yes
Constant	12.38***	8.552***	8.965***
	(0.0151)	(0.115)	(0.0402)
Observations	603,768	603,768	603,742
R-squared	0.057	0.460	0.904

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

As the fixed effects on the neighbourhood level are significant at the 1% level. I have run several regressions on different spatial levels to check if the significance or the outcome would change. The significance and coefficient do not really change when the fixed effects are put on the 4-digit postal code, but this area largely overlaps with neighbourhood. When looking at the 6-digit postal code we see a slight decrease in the coefficient of around 0,05. However, the coefficient more than halves when you fix for observations of individual dwellings indicating that there might be some variation in the house characteristics, which we have not accounted for within the neighbourhoods which push the numbers up. This suggest that the larger spatial coefficients might be biased. In addition, the effect of the size of the home starts to diminish as the spatial effect becomes smaller. Furthermore, the significance is only significant at the 5%, but this is higher than I expected due to the nature of individual transactions.

Table 6: Fixed Effect robustness

	(1)	(2)	(3)	(4)
VARIABLES	neighbourhood	postcode 4	postcode 6	house specific
interactie_all_22	0.0695***	0.0706***	0.0655***	0.0317**
	(0.0118)	(0.0118)	(0.0111)	(0.0155)
treatment_all		0.00248		
		(0.0175)		
logsize	0.789***	0.821***	0.657***	0.252***
	(0.00753)	(0.00732)	(0.00736)	(0.0127)
maintgood	0.130***	0.131***	0.123***	0.128***
	(0.00230)	(0.00237)	(0.00207)	(0.00299)
newbuilt	0.0870***	0.0916***	0.0711***	0.0697***
	(0.00420)	(0.00439)	(0.00381)	(0.00478)
apartment	-0.413***	-0.420***	-0.345***	
	(0.00869)	(0.00862)	(0.00732)	
terraced	-0.327***	-0.344***	-0.226***	
	(0.00687)	(0.00686)	(0.00515)	
semidetached	-0.239***	-0.251***	-0.170***	
	(0.00608)	(0.00637)	(0.00447)	
garden	0.0197***	0.0181***	0.0190***	
	(0.00375)	(0.00390)	(0.00308)	
constrlt1905	-0.0420***	-0.0113	-0.0408***	
	(0.00721)	(0.00769)	(0.00524)	
constr19061930	-0.0865***	-0.0726***	-0.0677***	
	(0.00684)	(0.00683)	(0.00518)	
constr19311944	-0.103***	-0.0896***	-0.0764***	
	(0.00805)	(0.00777)	(0.00558)	
constr19451959	-0.146***	-0.142***	-0.110***	
	(0.00788)	(0.00758)	(0.00639)	
constr19601970	-0.187***	-0.194***	-0.116***	
	(0.00671)	(0.00668)	(0.00597)	
constr19711980	-0.158***	-0.152***	-0.0971***	
	(0.00816)	(0.00814)	(0.00600)	
constr19811990	-0.111***	-0.110***	-0.0784***	
	(0.00704)	(0.00726)	(0.00568)	
Year Fixed Effects	Yes	Yes	Yes	Yes

Spatial Fixed Effects	Yes (nid)	Yes (pc4)	Yes (postcode)	Yes (house id)
Year * Municipality FE	Yes	Yes	Yes	Yes
Constant	9.058***	8.913***	9.577***	11.05***
	(0.0403)	(0.0391)	(0.0384)	(0.0595)
Observations	603,742	603,764	599,513	216,863
R-squared	0.905	0.895	0.943	0.976

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The previous fixed effects analysis would suggest a increase of between 3.2% to 7.3% compared to the non-treatment group. However, there are a couple of things to notice in the outcome. The first is that the House specific fixed effects regression remains significant to the 5%. While this is not impossible it is interesting to note as it would need multiple observations of the same house, both outside of and inside of the treatment year of 2022. This is remarkable, because houses typically do not get sold very frequently, as one tends to buy a home when you intend to stay somewhere more long term, due to transaction costs and inflexibility. It is possible that enough houses have been sold in the treatment areas in 2022 to achieve significant results. However, as the treatment area features only around 1500, or ~5%, of the transactions in a given year. Divided over 65 neighbourhoods, it gives an average of around 23 transactions per neighbourhood per year. It is questionable if repeated transactions have been frequent enough in the treatment to establish clear significance. I expect this to be caused by taking into account all the sales over the preceding 22 years in which it is plausible that most houses sold in 2022 have been sold at least once before in the dataset. It looks like this is the case as the housed has around 217.000 observations, which is around a third of the observations, however the number is probably less accurate than it semes. However important for u is that the effect is diminishing with lower spatial units.

I have chosen the year 2022 for the year to measure the effects of the regulation. Since this is both the most recent year available in the dataset and the first year I would say the transitionary period of several municipalities had passed. However, the model compares the effects of the treatment area and year interaction to all the years before it from 2000 to 2021. Hence it could be that some earlier effect is captured. Looking back at graphs 5-7, it is possible that the rapid price increases in the 2010's have been stronger in the treatment areas than in the non-treatment neighbourhoods and some residual price differences are captured. In the table below I have added all interactions for the years 2017 to 2022. I have chosen 2017 as a start date, since this was 2 years before the ban was put in place in the first neighbourhoods. Here I assume a anticipation effect to be absent as it might not have been in 2018.

If we look at the interactions below we see that even in 2017 the transaction prices were around 9% higher than their non-treatment. This is noticeably higher than only taking account of the year 2022, Although this is probably explained by the fact that several higher price years are now separately accounted for. After this we see some fluctuation in the treatment effect, but this is within one standard error of the previous neighbourhood effects. This would suggest that the effect we capture in the post treatment interactions Is a residual effect in the treatment neighbourhoods.

Table 7: Interactions

VARIABLES	(1) neighbourhood	(2) postcode 4	(3) postcode 6	(4) house id
interactie_all_22	0.0940***	0.0943***	0.0918***	0.0548***
	(0.0147)	(0.0146)	(0.0140)	(0.0178)
interactie_all_21	0.111***	0.110***	0.105***	0.0706***
	(0.0155)	(0.0154)	(0.0147)	(0.0171)
interactie_all_20	0.0969***	0.0917***	0.0944***	0.0618***
	(0.0142)	(0.0143)	(0.0130)	(0.0162)
interactie_all_19	0.0885***	0.0894***	0.0889***	0.0618***
	(0.0126)	(0.0124)	(0.0120)	(0.0142)
interactie_all_18	0.0881***	0.0880***	0.0965***	0.0696***
	(0.0152)	(0.0154)	(0.0134)	(0.0159)
interactie_all_17	0.0916***	0.0920***	0.0873***	0.0890***
	(0.0184)	(0.0181)	(0.0166)	(0.0193)
treatment_all		-0.0209		
		(0.0180)		
logsize	0.789***	0.821***	0.657***	0.255***
	(0.00751)	(0.00730)	(0.00733)	(0.0126)
House specific controls[19]	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Spatial Fixed Effects	Yes (nid)	Yes (pc4)	Yes (postcode)	Yes (house id)
Year * Municipality FE	Yes	Yes	Yes	Yes
Constant	9.057***	8.914***	9.576***	11.04***
	(0.0402)	(0.0390)	(0.0383)	(0.0588)
Observations	603,742	603,764	599,513	216,863
R-squared	0.905	0.895	0.943	0.977

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

To see if this is due to I will run another version of the model where the treatment variable is structured slightly differently. Instead of taking an interaction I select the treatment on the neighbourhood and already select by year. By selecting all observations in the treatment neighbourhoods in and after the year of introduction into that neighbourhood. I can see the general effect of the treatment before and after introduction. This differs of the interaction by taking all the years into account at the same time. Here we find that the treatment coefficient still points to a correlation between the treatment areas and the nontreatment areas of 8,8%.

[19] These include all the house specific controls from the previous regressions

This coefficient seems very similar to the coefficients in the previous table and noticeably has roughly the same effect as the treatment area dummy had in the very first regression.

Table 8: Pooled treatment

	(1)	(2)	(3)	(4)
VARIABLES	nid	pc4	postcode	house specific
treatment_toh	0.0856***	0.0809***	0.0805***	0.0499***
	(0.0124)	(0.0125)	(0.0114)	(0.0131)
logsize	0.789***	0.821***	0.657***	0.253***
	(0.00752)	(0.00732)	(0.00734)	(0.0127)
maintgood	0.130***	0.130***	0.123***	0.128***
	(0.00230)	(0.00237)	(0.00207)	(0.00299)
newbuilt	0.0867***	0.0915***	0.0706***	0.0694***
	(0.00419)	(0.00437)	(0.00379)	(0.00479)
apartment	-0.413***	-0.420***	-0.345***	
	(0.00869)	(0.00862)	(0.00732)	
terraced	-0.327***	-0.344***	-0.226***	
	(0.00687)	(0.00686)	(0.00515)	
semidetached	-0.239***	-0.251***	-0.170***	
	(0.00608)	(0.00637)	(0.00446)	
garden	0.0197***	0.0182***	0.0190***	0.00379***
	(0.00375)	(0.00390)	(0.00308)	(0.00127)
constrlt1905	-0.0420***	-0.0114	-0.0407***	
	(0.00721)	(0.00768)	(0.00524)	
constr19061930	-0.0865***	-0.0727***	-0.0676***	
	(0.00683)	(0.00683)	(0.00517)	
constr19311944	-0.103***	-0.0898***	-0.0765***	
	(0.00805)	(0.00776)	(0.00556)	
constr19451959	-0.146***	-0.142***	-0.110***	
	(0.00788)	(0.00756)	(0.00640)	
constr19601970	-0.187***	-0.194***	-0.116***	
	(0.00670)	(0.00666)	(0.00598)	
constr19711980	-0.158***	-0.152***	-0.0970***	
	(0.00817)	(0.00812)	(0.00599)	
constr19811990	-0.111***	-0.110***	-0.0783***	
	(0.00703)	(0.00725)	(0.00564)	
Constant	9.057***	8.912***	9.576***	11.05***
	(0.0402)	(0.0391)	(0.0384)	(0.0593)
Observations	603,742	603,764	599,513	216,863
R-squared	0.905	0.895	0.943	0.976

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

To examine the possible hold over I have extended the interactions to the year 2001 to see the development of the residual over time. We can see that the difference stays about the same until the recovery of the financial crisis. After which the difference starts to become quite large. We have seen that the average price of the treatment and non-treatment have started to diverge around this point in graph. However, we see that after explosive growth in the mid-decade the difference starts to become more steady. This seems to confirm that the measured effect seems to be a residual of this boom. Interestingly here the difference in coefficients between the house fixed effects and the neighbourhood fixed effects seems to be much smaller and the house specific effects seem to be larger than the neighbourhood effects for much of the period.

Table 10[20]: interactions over time

VARIABLES	(1) Extended interactions	(2) house id
interactie_all_22	0.126***	0.108***
	(0.0201)	(0.0242)
interactie_all_21	0.144***	0.124***
	(0.0213)	(0.0247)
interactie_all_20	0.129***	0.114***
	(0.0192)	(0.0203)
interactie_all_19	0.121***	0.112***
	(0.0179)	(0.0196)
interactie_all_18	0.121***	0.119***
	(0.0200)	(0.0215)
interactie_all_17	0.124***	0.136***
	(0.0233)	(0.0252)
interactie_all_16	0.106***	0.117***
	(0.0202)	(0.0212)
interactie_all_15	0.0936***	0.0940***
	(0.0191)	(0.0203)
interactie_all_14	0.0559***	0.0887***
	(0.0156)	(0.0152)
interactie_all_13	0.0406***	0.0591***
	(0.0143)	(0.0171)
interactie_all_12	0.0325**	0.0614***
	(0.0155)	(0.0160)
interactie_all_11	0.0226	0.0443***
	(0.0159)	(0.0140)
interactie_all_10	0.0144	0.0465***
	(0.0140)	(0.0136)
Spatial Fixed Effects	Yes(nid)	Yes(housed)
Year Fixed Effects	Yes	Yes
Mun*year Fixed Effects	Yes	Yes
Constant	8.963***	11.02***
	(0.0399)	(0.0586)
Observations	603,742	216,863
R-squared	0.905	0.977

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

To attempt to account for the holdovers I have dropped all the observations stemming from the years up to, but excluding, 2018. When I run the fixed effects regressions again, the

[20] Full table Can be found in the appendix

regression shows no discernible significance in most of the interactions. The coefficients as a whole suggest that the previously found effect was, indeed, due to earlier price divergence.

Figure 11: 2018+ data

VARIABLES	(1) neighbourhood	(2) postcode 4	(3) postcode 6	(4) house id
interactie_all_22	-0.00282	-0.00185	-0.00932	0.0109
	(0.00663)	(0.00664)	(0.00665)	(0.0163)
treatment_all		0.0145		
		(0.0143)		
logsize	0.745***	0.765***	0.685***	0.400***
	(0.00588)	(0.00589)	(0.00587)	(0.0589)
House specific controls[21]	Yes	Yes	Yes	No
Year Fixed Effects	Yes	Yes	Yes	YES
Spatial Fixed Effects	Yes (nid)	Yes (pc4)	Yes (postcode)	Yes (house id)
Year * Municipality FE	Yes	Yes	Yes	Yes
Constant	9.633***	9.546***	9.829***	10.80***
	(0.0313)	(0.0315)	(0.0312)	(0.271)
Observations	139,680	139,705	127,028	10,366
R-squared	0.909	0.899	0.957	0.980

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

If we extend this to the show the interactions of all the years we see that all the years have no significant change compared to the non-treatment group. Only the interaction in 2021 shows any significant increase compared to 2018. Remarkably the house specific fixed effects are significant at the 1% in 2021. Showing a positive effect of around 4.7% on the housing price. This is interesting as 2021 was the year directly after the introduction in Amsterdam and could point to a transitory development, as investors sold of property to pre-empt the regulation.

[21] These include all the house specific controls added in previous FE regressions

Table 12: 2018+ interactions

VARIABLES	(1) neighbourhood	(2) postcode 4	(3) postcode 6	(4) house id
interactie_all_22	0.00572	0.00680	-0.00694	0.0268
	(0.0128)	(0.0126)	(0.0105)	(0.0187)
interactie_all_21	0.0229*	0.0239*	0.0119	0.0462***
	(0.0123)	(0.0123)	(0.00918)	(0.0161)
interactie_all_20	0.00984	0.00718	0.00331	0.0303
	(0.0108)	(0.0108)	(0.00728)	(0.0230)
interactie_all_19	0.00198	0.00443	-0.00522	0.0207
	(0.00929)	(0.00932)	(0.00570)	(0.0201)
treatment_all		0.00568		
		(0.0172)		
logsize	0.745***	0.765***	0.685***	0.400***
	(0.00588)	(0.00589)	(0.00587)	(0.0589)
House specific controls[22]	Yes	Yes	Yes	No
Year Fixed Effects	Yes	Yes	Yes	Yes
Spatial Fixed Effects	Yes (nid)	Yes (pc4)	Yes (postcode)	Yes (house id)
Year * Municipality FE	Yes	Yes	Yes	Yes
Constant	9.632***	9.546***	9.828***	10.80***
	(0.0313)	(0.0315)	(0.0312)	(0.271)
Observations	139,680	139,705	127,028	10,366
R-squared	0.909	0.899	0.957	0.980

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

I have run another regressions with the same specifications as table 6, but with the log of the square metre price as the dependent, to see if something irregular would occur, but outcomes showed basically the same results as table 6. [23]The effect is somewhat smaller, but not by much. This is to be expected as we already controlled for the log of the size of the homes up for transaction.

Another area the regulation might possibly affect is the size of the homes put up for transaction. It is possible that it has become lucrative to buy larger homes and have several renters pay for them. Hence I have also run a regression with the log of the house size on the dependent variable[24]. This does not yield significant results, although all the signs are negative.

[22] These include all the house specific controls from the previous regressions

[23] The results are in table 18 in the appendix

[24] Table 19 in the appendix

5.2. Individual municipal analysis

While the treatment area as a whole does not show any significant deviation from the non-treatment area, there is s substantial imbalance in the number of neighbourhoods between the cities, while this should be accounted for by the spatial fixed effects there were large differences in the average price development history as seen in figures 6 and 7. Furthermore, many municipalities which did not implement any restrictions on specific neighbourhoods that were banned from adding new roommate rentals did implement a permit. Utrecht has one for example but based on a case to case by case basis. Additionally, as mentioned above the Hague has implemented it on many neighbourhoods, but its implementation criteria would have caused endogeneity issues. While the dataset features the ten largest cities of the Netherlands the regulations in other cities might have had an effect that would be similar to the effect of the treatment in specific neighbourhoods. To see if the results for the total dataset are representative, I will analyse the effects on a city level. For this I will run the model for the three cities of which I have included treatment neighbourhoods and compare them only to the rest of their respective cities.

5.2.1 Amsterdam

Amsterdam shows the same basic pattern as we have seen before, however coefficients are slightly less strong than they were in the complete dataset. As Amsterdam has a large share of the total observations and the treatment units it is not too out of the ordinary to see a similar set of results. However it seems that the price trend in the price development seems to be more significant leading up to 2021, before the level drops again. Indicating that 2021 might indeed have seen a spike in prices, before it dropped down again.

When running the fixed effects variations in Amsterdam we see that the coefficients of the effect become smaller when they are applied to more local spatial levels. This would point to unobserved spatial variables that influence the price development. If we look at the the house fixed effects the growth in prices does become more noticeable as 2021 has twice as big an effect as 2022 and 2020.

Table 13: A'dam Fixed Effect Variations

VARIABLES	(1) nid	(2) pc4	(3) pc6	(4) houseid
interactie_ams_22	0.0747***	0.0712***	0.0738***	0.0346**
	(0.0127)	(0.0136)	(0.0125)	(0.0139)
interactie_ams_21	0.0992***	0.0940***	0.0974***	0.0770***
	(0.0135)	(0.0141)	(0.0129)	(0.0154)
interactie_ams_20	0.0730***	0.0647***	0.0725***	0.0405***
	(0.0114)	(0.0126)	(0.0112)	(0.0149)
interactie_ams_19	0.0666***	0.0642***	0.0655***	0.0458***
	(0.0104)	(0.0112)	(0.0101)	(0.0142)
logsize	0.833***	0.861***	0.797***	0.375***
	(0.00626)	(0.00762)	(0.00616)	(0.0383)
House specific controls[25]	Yes	Yes	Yes	No
Year Fixed Effects	Yes	Yes	Yes	Yes
Spatial Fixed Effects	Yes (nid)	Yes (pc4)	Yes (postcode)	Yes (house id)
Year * Municipality FE	Yes	Yes	Yes	Yes
Constant	9.190***	9.113***	9.234***	10.82***
	(0.0431)	(0.0508)	(0.0382)	(0.172)
Observations	122,317	122,323	121,483	31,992
R-squared	0.924	0.913	0.946	0.976

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0

Finally I will drop all observations before 2018 again to remove the residual of previous growth. Here we see that the effects in 2022 are again insignificant, but that the price effect in 2021 is significant across all spatial levels and only begins to weaken on the house id level. This is interesting as it suggests either a transitionary period of the regulations or that prices did see an rise directly after the regulation. A transitory period could ould be possible as the regulation in Amsterdam was implemented halfway through 2020 and had a transitionary period into 2021 to enable landlords to apply for the permit.

[25] These include all the house specific controls from the previous regressions

Table 14: A'dam 18+ FE

VARIABLES	(1) nid	(2) pc4	(3) pc6	(4) houseid
interactie_ams_22	0.00254	0.00638	-0.000781	0.0215
	(0.00887)	(0.0104)	(0.00830)	(0.0199)
interactie_ams_21	0.0229***	0.0266***	0.0207***	0.0340*
	(0.00734)	(0.00993)	(0.00663)	(0.0187)
interactie_ams_20	0.000281	0.00110	0.00146	0.0177
	(0.00562)	(0.00891)	(0.00538)	(0.0260)
interactie_ams_19	-0.00752	-0.00144	-0.0108*	-0.0126
	(0.00637)	(0.00987)	(0.00554)	(0.0223)
logsize	0.762***	0.772***	0.758***	0.716***
	(0.00662)	(0.00713)	(0.00749)	(0.196)
House specific controls[26]	Yes	Yes	Yes	No
Year Fixed Effects	Yes	Yes	Yes	Yes
Spatial Fixed Effects	Yes (nid)	Yes (pc4)	Yes (postcode)	Yes (house id)
Year * Municipality FE	Yes	Yes	Yes	Yes
	(0.0390)	(0.0489)	(0.0275)	
Constant	9.921***	9.929***	9.860***	9.812***
	(0.0529)	(0.0623)	(0.0500)	(0.870)
Observations	29,756	29,760	27,146	1,487
R-squared	0.905	0.887	0.945	0.977

- Robust standard errors in parentheses
- *** p<0.01, ** p<0.05, * p<0

Overall the outcome from Amsterdam is not too dissimilar from the outcome of the total set. The strength of the coefficients is slightly weaker overall, except in the year 2021 when a clear price increase in the treatment area can be established. This outcome is in line with the expectations as Amsterdam makes up around 20% of the observations in the dataset and more than 75% of the treatment neighbourhoods.

5.2.2 Rotterdam

Rotterdam is unique in that its treatment areas had a higher average price level than its non treated areas. In Amsterdam and Eindhoven this was the reverse. It also has the least designated neighbourhoods(6), although it still has a decent share of the transactions (12.4%). The model from Rotterdam is less accurate as the significance in the last two years drops and its house observations are not significant at all. We do see a stronger effect of the treatment area, but with 2020 as a high point year. Although the standard deviations are larger here. The house specific spatial fix becomes insignificant due to large standard errors but is

[26] These include all the house specific controls from the previous regressions

surprisingly large in 2019 and 2020 where it is still significant at the 5% level and shows a 8-9% correlation.

Table 15: R'dam FE variations

VARIABLES	(1) nid	(2) pc4	(3) pc6	(4) houseid
interactie_rot_22	0.106**	0.108***	0.100**	0.0383
	(0.0401)	(0.0390)	(0.0392)	(0.0470)
interactie_rot_21	0.100**	0.104**	0.0825*	0.0299
	(0.0435)	(0.0425)	(0.0424)	(0.0397)
interactie_rot_20	0.122***	0.124***	0.119***	0.0899**
	(0.0394)	(0.0383)	(0.0368)	(0.0378)
interactie_rot_19	0.106***	0.108***	0.114***	0.0801**
	(0.0348)	(0.0336)	(0.0322)	(0.0314)
logsize	0.839***	0.843***	0.647***	0.253***
	(0.0263)	(0.0264)	(0.0186)	(0.0381)
House specific controls[27]	Yes	Yes	Yes	No
Year Fixed Effects	Yes	Yes	Yes	No
Spatial Fixed Effects	Yes (nid)	Yes (pc4)	Yes (postcode)	Yes (house id)
Year * Municipality FE	Yes	Yes	Yes	Yes
	(0.0183)	(0.0181)	(0.0167)	
Constant	8.762***	8.746***	9.546***	10.98***
	(0.140)	(0.139)	(0.0957)	(0.177)
Observations	75,523	75,525	74,518	22,677
R-squared	0.872	0.871	0.932	0.971

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

If we drop the all observations before 2018 we see almost everything becomes insignificant again. Most coefficients drop back to close to zero with large standard errors. Although the house fixed effects seem to show large effects their standard errors match their size.

[27] These include all the house specific controls from the previous regressions

Figure 16; R'dam 18+ FE

VARIABLES	(1) nid	(2) pc4	(3) pc6	(4) houseid
interactie_rot_22	-0.0243	0.00655	-0.0338	0.0390
	(0.0392)	(0.0374)	(0.0393)	(0.0485)
interactie_rot_21	-0.0128	0.0204	-0.0252	0.0383
	(0.0392)	(0.0388)	(0.0326)	(0.0284)
interactie_rot_20	0.00184	0.0346	0.00290	0.0933
	(0.0370)	(0.0369)	(0.0295)	(0.0680)
interactie_rot_19	-0.00552	0.0274	0.00324	0.0896*
	(0.0272)	(0.0301)	(0.0163)	(0.0531)
logsize	0.767***	0.776***	0.668***	-0.228
	(0.0198)	(0.0201)	(0.0170)	(0.215)
House specific controls[28]	Yes	Yes	Yes	No
Year Fixed Effects	Yes	Yes	Yes	No
Spatial Fixed Effects	Yes (nid)	Yes (pc4)	Yes (postcode)	Yes (house id)
Year * Municipality FE	Yes	Yes	Yes	Yes
Constant	9.481***	9.435***	9.832***	13.69***
	(0.106)	(0.107)	(0.0960)	(0.996)
Observations	16,744	16,746	14,772	958
R-squared	0.897	0.895	0.958	0.981

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Rotterdam shows some divergence from the model for the complete dataset, although much of this suffers from low significance. Interesting here is that 2019 and 2020 show relatively strong effects on the house fixed effects. In Rotterdam too we can see that the strength of the coefficient diminishes when accounting for lower level spatial effects.

5.2.3 Eindhoven

Eindhoven has a larger share of neighbourhoods in the treatment than Rotterdam with 8. Since Eindhoven has about 1/3 of the population of Rotterdam this is quite significant. It also has around half as many observations in the dataset(6.6%). If we look at the spatial levels of Eindhoven we see it follow roughly the same pattern as the normal version does although the drop of is more gradual over the different spatial levels. The standard errors are more substantial as expected, but the house fixed effects retain the same level of significance as in Amsterdam and the broad dataset.

[28] These include all the house specific controls from the previous regressions

Table 17: Eindhoven FE robustness

VARIABLES	(1) nid	(2) pc4	(3) pc6	(4) houseid
interactie_eind_22	0.0882***	0.0614**	0.0414	0.0576**
	(0.0310)	(0.0244)	(0.0315)	(0.0260)
interactie_eind_21	0.0963***	0.0639**	0.0656**	0.0410**
	(0.0323)	(0.0300)	(0.0282)	(0.0199)
interactie_eind_20	0.0938***	0.0581***	0.0574***	0.0454**
	(0.0237)	(0.0207)	(0.0153)	(0.0192)
interactie_eind_19	0.0858***	0.0570***	0.0648***	0.0437
	(0.0162)	(0.0147)	(0.0186)	(0.0292)
logsize	0.738***	0.847***	0.462***	0.225***
	(0.0282)	(0.0379)	(0.0197)	(0.0361)
House specific controls[29]	Yes	Yes	Yes	No
Year Fixed Effects	Yes	Yes	Yes	No
Spatial Fixed Effects	Yes (nid)	Yes (pc4)	Yes (postcode)	Yes (house id)
Year * Municipality FE	Yes	Yes	Yes	Yes
Constant	9.133***	8.630***	10.40***	11.14***
	(0.153)	(0.201)	(0.106)	(0.169)
Observations	38,319	38,320	38,070	13,987
R-squared	0.864	0.838	0.937	0.975

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

However If we look at the data corrected for the later years we see that the neighbourhood level of fixed effects retains an incredibly strong growth, which is significant at the 1% level. Interestingly enough this is immediately wiped out from the 4 digit postal code onwards. This indicates a strong spatial bias that is only visible at the neighbourhood level and disappears at the 4 digit postal code level that is about the same size.

[29] These include all the house specific controls from the previous regressions

Table 18: Eindhoven 2018+ FE

VARIABLES	(1) nid	(2) pc4	(3) pc6	(4) houseid
interactie_eind_22	0.102***	0.00855	0.00173	-0.00524
	(0.0384)	(0.0311)	(0.0376)	(0.0347)
interactie_eind_21	0.103***	0.00720	0.0214	0.0769
	(0.0374)	(0.0320)	(0.0288)	(0.0507)
interactie_eind_20	0.105***	0.00442	0.0205	0.0306
	(0.0291)	(0.0228)	(0.0241)	(0.0469)
interactie_eind_19	0.0993***	0.00242	0.0269	0.0146
	(0.0244)	(0.0251)	(0.0214)	(0.0472)
logsize	0.744***	0.826***	0.593***	0.307
	(0.0274)	(0.0355)	(0.0186)	(0.234)
House specific controls[30]	Yes	Yes	Yes	No
Year Fixed Effects	Yes	Yes	Yes	No
Spatial Fixed Effects	Yes (nid)	Yes (pc4)	Yes (postcode)	Yes (house id)
Year * Municipality FE	Yes	Yes	Yes	Yes
	(0.0390)	(0.0489)	(0.0275)	
Constant	9.417***	9.040***	10.09***	11.11***
	(0.145)	(0.188)	(0.0957)	(1.108)
Observations	9,756	9,757	9,029	819
R-squared	0.857	0.832	0.943	0.968

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.

In general the results of the different municipalities do not diverge radically from the coefficients stemming from the general dataset. While Eindhoven and Rotterdam suffer from a lack of observations in the treatment as they have less observations and fewer treatment neighbourhoods, there is a significant spatial effect in the first regressions that becomes smaller as we account for more precise spatial units. This is nullified in almost all cases when observations before 2018 are dropped from the dataset.

[30] These include all the house specific controls from the previous regressions

6. Discussion

The finding of the main research objective if this paper, to see if the implementation of the roommate regulation in spatial areas can be tied to an increase or decrease in prices, is that there is no discernible effect of the regulation on the price relative to the non-treatment areas once we look only at the years post 2017. The fact that there is no significant or strong correlation is noticeable, as a motivator for the implementation of the regulation given by many municipalities was to try to keep house prices affordable and is contrary to what much of the literature would predict. As discussed, a significant body of literature exists that suggests that regulations lead to higher prices as it is hard to add new living space as demand grows(Bertaud, 2018; Gyourko & Molloy, 2015:Anthony, 2017). However, as the data available to us is only extending a few years past the implementation of the regulation, we would expect a negative effect on prices; as an extra layer of regulations would reduce the profit of investors, since they have less to gain from investing in new property in these areas.

While the result are mostly undiscernible in the 2018 to 2022 version of the model. There is a significant effect of around 2.3% to 4.7% in the year 2021 before it returns to no discernible effect in 2022. This could be a transitory development, since it is possible that buy to let investors scrambled to gain a few remaining homes in the treatment neighbourhoods before the regulation went into effect. This effect stems mostly from Amsterdam, as Amsterdam grew 2% to3.4% in 2021 before it dipped. In Amsterdam the the treated neighbourhoods had almost reached the average price level of the non-treated areas after years of strong growth in the average price set. While I assume that the effects of 2021 are a transitory effect as it directly follows the implementation of joint tenancy restrictions in Amsterdam, it could be that the regulation had a price upward effect and that another effect in 2022 reduces the growth rate. When looking at a general price trend in Amsterdam we see that the highest price level is reached mid-2022, before prices start to drop. It could be that an external effect causes the growth to stop and prices to fall as hard as in the rest of the market.

My findings could be caused by the issue of non-enforcement. Buy to let Investors could simply continue business as usual if the chance of being caught is very small. A study on the effectiveness of the policy in Amsterdam showed that enforcement was not a high priority of the municipality and relatively few fines were doled out, (Meurs, *et al.* 2022), however this does not mean that the policy would have no impact on investor behaviour. As Bouwmeester *et al.* (2023) found in a case study of a similar policy in Utrecht, that a significant portion of roughly two thirds of the interviewed investors was found to adhere to- or be dissuaded by the policy. Even as they found cases where the municipality was knowingly ignoring violations of the regulation. This would suggest that even if not all investors would adhere to the regulation, a significant share would be deterred. If there is a price effect of the policy, it seems unlikely that this would be completely nullified by non-enforcement.

Furthermore the spatial nature of the regulation could just shift the buy to let investors towards nearby non-regulated neighbourhood and dilute the effect. Bouwmeester *et al.* (2023) reported that a third of the investors was looking in municipalities surrounding

Utrecht, where the regulation of buy to let was less ambiguous and stringent. Meurs *et al.* (2022) found that taken over the entire city roughly only half of all permits were granted, which means that investors could shift their attention elsewhere in the city, however one still would expect a relatively lower prices rise compared to the other neighbourhoods.

On the other hand a non-discernible impact is not completely unexpected, as recent studies of the Dutch housing market have also been unable to find a discernible price effect of buy to let investors in a Dutch contest (Francke *et al.*,2023; Aalbers *et al.,* 2018). My findings taken together with the findings of Francke *et al.* (2023) would suggest that the price effects of two of the three major housing market regulations implemented by municipalities are non-discernible.

Francke *et al.* (2023) in a study on the related " buy up protection" found significant changes in demographic composition following the implementation of this anti-buy to let regulation, especially of income. According to them the introduction of the buy up protection cause supply of rentals to decline, rents to increase and average income of the neighbourhoods to increase, due to a change in demographic make-up and not increase in income of the residents Similarly as buy to let investors fill in a certain rent gap for less wealthy residents (Paccoud *et al.,* 2020) effectively limiting the number of houses available for joint tenancy in a neighbourhood would prevent these groups from obtaining residence in these neighbourhoods. While research on the demographic changes of the neighbourhoods was outside the scope of this research, I expect that this regulation could have impacted demographic composition of the neighbourhoods. This could indicate that the regulation has more of an effect on demographic composition of the neighbourhoods than they are able to lower prices. Further research into possible changes in demographic composition of income age, and migratory background of the neighbourhoods would be valuable to see if these regulations can cause a sorting or exclusionary effect. In addition, further research into rental supply and rental price developments in these neighbourhoods is valuable to better understand the consequences of the joint tenancy regulation.

The findings Francke *et al.* (2023) raise the possibility that a possible drop in value due to the loss of profit for buy to let investors is compensated by owner occupier buyers. This could be twofold. The first is the possibility that owner occupiers place higher value on the certainty that their peace will not be disturbed by the conversion of their neighbouring residence into a joint tenancy residence, where the turnover is high(er) and there is generally more noise pollution or other negative external effects. It is possible that this effectively cancels out the negative price effects of the disappearance of buy to let investors. Furthermore, it is possible that buy to let investors were already in an equilibrium with owner occupiers instead of being able to outprice them. If this is the case, then the large demand for owner occupied housing could easily fill the gap left by buy to let investors without causing a significant drop in prices. Research into the willingness to pay of owner occupiers for the reduction of these negative externalities could add valuable information into the evaluation of such policies to provide for better . Furthermore deeper research into the equilibrium between owner occupier in neighbourhoods with high concentrations of free market rentals is valuable to see if the costs of reducing housing supply is outweighed by the positive effects for owner occupiers.

While the findings did not support evidence of a significant price divergence after the introduction of the regulation. I did find a large divergence if we look at the years from 2000 to 2022, which seems to emerge in the years of the housing recovery in the 2010's. The treatment areas as a whole had a lower average price level than the non-treatment areas [31]and showed higher growth in the years leading up to the implementation of the regulations. Which then returns to equal growth. This held if we looked at the municipalities individually. This could suggest that the neighbourhoods do not suffer a price decrease but return to the regular growth pattern of the market. As the coefficients in the complete data set show a strong and significant effect on all spatial levels. While this effect becomes smaller if we account for more precises spatial levels, it does point to a an spatial effect in these areas. I theorise that this pre-treatment effect is contributable to gentrification. Many of the neighbourhoods do seem to be areas that have undergone recent gentrification, like the Pijp and Oud-West in Amsterdam, Woensel in Eindhoven and Kralingen in Rotterdam.

Buy to let investors play a significant role in gentrification(Paccoud, 2015), by investing in property and outpricing "native" residents, while they fill in rent gaps in these neighbourhoods and offer options for renters(Paccoud, 2016;Paccoud *et al,* 2020;Thiel & Zaunbrecher, 2023). By doing so they buy up less well maintained property and renovate them for new renters and bring in younger people like students. I suspect that the growth in these areas prior to the introduction is a gentrification effect correlated with the emergence of a widespread private rental sector in the selected neighbourhoods. If the growth prior to the implementation of the treatment is due to gentrification, it could have correlated with a concentration of buy-to-let investors, which would have made them more likely to be designated for a ban on new joint tenancy residences. I That the cheaper areas are interesting for buy to let investors can also be seen from the implementation of cities like the Hague which restrict buy to let investors from buying in neighbourhoods with a lower-than-average house.

I posit that this role for buy to let investors as catalysators of gentrification and rising house prices could explain why the divergence in prices of the treatment areas stops around the time of the implementation of anti-buy to let regulations. As investors pull out the relative price changes in the neighbourhood would be more in line with general macroeconomic developments of income and mortgage rent, that are determinant of the value for owner occupiers. If this is true then the regulations will have had an effect, but they act to bring the development of the prices in line with the owner occupier market instead of reducing prices. This possibility would suggest that owner occupiers were in some kind of equilibrium with buy to let investors. Instead of the investment value of investors.

[31] See graphs 5 and 6, table 8.

This is all rather speculative; hence I propose that future research focus' their attention on the development of buy to let investors in the treatment neighbourhoods before and after the introduction of such regulations.

7. Conclusion

This thesis set out the price developments of neighbourhoods in 10 major Dutch cities subjected to a ban in new conversions to joint tenancy of three or more people. A hotly debated topic for municipal policy makers as they try to alleviate a growing sense of housing affordability.

First I found that there are is a strong coefficient that shows that the prices in the neighbourhoods subjected to a joint tenancy regulation have substantially higher prices when I controlled for spatial and temporal fixed effects. This is remarkable as I had expected a decrease in prices following the regulation. However, this effect diminished over when we controlled for more specific spatial fixed effects, such as repeated sales. This suggests that there is a strong unobserved correlation between the observations in the treated neighbourhoods, which caused the prices to rise. I theorise that this is due to gentrification

Furthermore, while strong price level differences were observed on several spatial levels at first, it was found that these differences stemmed mostly from divergence in the price development in the years leading up to the implementation of the joint tenancy regulation. The effect all but disappeared when only considering the more recent years in the dataset. Suggesting that post treatment the price developments are mostly indistinguishable from the general market trend.

While there are similar findings in a Dutch context. I posit several possible explanations for these findings, including the result of non-enforcement of the regulation by the municipalities, the possibility that the negative effects are compensated due to a higher willingness to pay of owner occupiers, or that the period of time between the implementation and the latest observations in the data is simply too short to capture the full effect. A last possibility is that the buy to let investors spread out towards other neighbourhoods and cause other neighbourhoods to start experiencing similar price growth.

Finally, as the findings do suggested a strong unobserved spatial effect I theorise that the measurement of a strong pre-treatment effect and a non-significant post treatment effect could indicate that the pre-treatment effect is largely due to gentrification, but that the introduction of several anti-buy to let regulations, including the joint tenancy regulation pushes the strong growth of these regulations back towards the general market trend as owner occupiers no longer compete with buy to let investors in the neighbourhoods, however this needs significantly more research.

If there is indeed no significant effect of the regulation it implies that the joint tenancy regulation does not necessarily make house prices more affordable, but rather serves to curb negative external effects of joint tenancy rentals and preserve housing for owner occupiers. However, as the regulation primarily affects housing supply used by youth, immigrants and other less well of groups it could influence the demographic make-up of these neighbourhoods and inadvertently cause income segregation. A finding that is supported by research on an associated regulation. If no discernible price effects can be found in price levels, then the policies main effect is in redistributing housing supply between renters and owner occupiers. While the decision is ultimately a political one it would imply that by regulating the rental market in this fashion the municipality is restricting supply for residents with lower incomes, and instead preserving it for residents who do have the means to buy their own houses.

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Appendix:

by name and neighbourhood identifier codes

Gemeente	Naam wijk	Nid('s)
Amsterdam (51)		
	Landlust (5)	3633702 3633703 3633704 3633705 3633706
	Erasmus park (2)	3633802 3633803
	Geuzenbuurt (3)	3634000 3634001 3634002
	Chassébuurt (4)	3637500 3637501 3637502 3637503
	Van Galenbuurt (3)	3634101 3634102 3634100
	Hoofdweg e.o. (4)	3634200 3634201 3634202 3634203
	Bellamybuurt/Kinkerbuurt (2)	3631801 3631800
	Westindische buurt (2)	3634300 3634301
	Hoofdorpleinbuurt (5)	3634400 3634401 3634402 3634403

		3634404
	Schinkelbuurt (2)	3634500 3634501
	Slotermeer-Zuidoost (3)	3637704 3637701 3637700
	Oude Pijp (5)	3632400 3632401 3632402 3632403
	IJselbuurt(2)	3635301 3635300
	Rijnbuurt (4)	3635400 3635401 3635402 3635403
	Noordelijke IJ-oevers-oost (2)	3637200 3637202
	Geerdinkhof/Kantershof (3)	3639410 3639409 3639408
Rotterdam (6)		
	Het Nieuwe Westen	5990324
	Middelland	5990325
	Oud Mathenesse	5990327
	Kralingen West	5990841
	Kralingen Oost	5990842
	Struisenburg	5990847
Eindhoven (8)		
	Doornakkers west	7723210
	Doornakkers-Oost	7723220
	Limbeek zuid	7724100
	Limbeek noord	7724110
	Hemelrijken	7724120
	Gildebuurt	7724130
	Woensel-West	7724210

Table 10 extension: Total interactions table

VARIABLES	(1) Extended interactions	(2) house id
interactie_all_22	0.126***	0.108***
	(0.0201)	(0.0242)
interactie_all_21	0.144***	0.124***
	(0.0213)	(0.0247)
interactie_all_20	0.129***	0.114***
	(0.0192)	(0.0203)
interactie_all_19	0.121***	0.112***
	(0.0179)	(0.0196)
interactie_all_18	0.121***	0.119***
	(0.0200)	(0.0215)
interactie_all_17	0.124***	0.136***
	(0.0233)	(0.0252)
interactie_all_16	0.106***	0.117***
	(0.0202)	(0.0212)
interactie_all_15	0.0936***	0.0940***
	(0.0191)	(0.0203)
interactie_all_14	0.0559***	0.0887***
	(0.0156)	(0.0152)
interactie_all_13	0.0406***	0.0591***
	(0.0143)	(0.0171)
interactie_all_12	0.0325**	0.0614***
	(0.0155)	(0.0160)
interactie_all_11	0.0226	0.0443***
	(0.0159)	(0.0140)
interactie_all_10	0.0144	0.0465***
	(0.0140)	(0.0136)
interactie_all_09	0.0214	0.0340***
	(0.0137)	(0.0127)
interactie_all_08	0.0349**	0.0587***
	(0.0155)	(0.0142)
interactie_all_07	0.0363***	0.0269**
	(0.0137)	(0.0112)
interactie_all_06	0.00828	0.0361***
	(0.0101)	(0.00922)
interactie_all_05	-0.00150	0.0165**
	(0.00920)	(0.00755)
interactie_all_03	-0.00553	-0.00598
	(0.0103)	(0.0117)
interactie_all_02	0.00597	0.00947
	(0.00835)	(0.0104)
interactie_all_01	-0.00752	0.0146
	(0.00935)	(0.00983)

House controls[32]	Yes	Yes
Spatial Fixed Effects	Yes(nid)	Yes(housed)
Year Fixed Effects	Yes	Yes
Mun*year Fixed Effects	Yes	Ye
Constant	8.963***	11.02***
	(0.0399)	(0.0586)
Observations	603,742	216,863
R-squared	0.905	0.977

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 18: Size development

VARIABLES	(1) logsize
interactie_all_22	-0.0106
	(0.0129)
interactie_all_21	-0.00855
	(0.0120)
interactie_all_20	-0.00887
	(0.00988)
interactie_all_19	-0.00348
	(0.00970)
o.interactie_all_18	-
maintgood	0.0474***
	(0.00312)
newbuilt	0.0523***
	(0.0126)
apartment	-0.618***
	(0.0105)
terraced	-0.307***
	(0.00851)
semidetached	-0.239***
	(0.00834)
garden	-0.0383***
	(0.00361)
Constant	5.072***
	(0.00913)
Observations	139,680
R-squared	0.517

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[32] Includes all the specific controls from the previous regressions. Removed due to table size

Table 19: square metre price

VARIABLES	(1) nid	(2) pc4	(3) pc6	(4) house id
interactie_all_22	0.0865***	0.0852***	0.0850***	0.0448**
	(0.0141)	(0.0141)	(0.0135)	(0.0174)
interactie_all_21	0.105***	0.102***	0.0988***	0.0625***
	(0.0150)	(0.0149)	(0.0142)	(0.0165)
interactie_all_20	0.0902***	0.0841***	0.0884***	0.0561***
	(0.0138)	(0.0139)	(0.0125)	(0.0158)
interactie_all_19	0.0822***	0.0822***	0.0828***	0.0576***
	(0.0122)	(0.0120)	(0.0114)	(0.0135)
interactie_all_18	0.0815***	0.0802***	0.0900***	0.0662***
	(0.0145)	(0.0147)	(0.0126)	(0.0154)
treatment_all	-	-	-	-
logsize	-0.209***	-0.175***	-0.343***	-0.746***
	(0.00754)	(0.00730)	(0.00734)	(0.0126)
newbuilt	0.102***	0.111***	0.0737***	0.0692***
	(0.00434)	(0.00450)	(0.00377)	(0.00478)
maintgood	0.131***	0.132***	0.123***	0.128***
	(0.00231)	(0.00237)	(0.00207)	(0.00299)
apartment	-0.412***	-0.418***	-0.344***	
	(0.00872)	(0.00864)	(0.00733)	
terraced	-0.326***	-0.343***	-0.226***	
	(0.00684)	(0.00683)	(0.00515)	
semidetached	-0.238***	-0.250***	-0.170***	
	(0.00605)	(0.00634)	(0.00446)	
garden	0.0196***	0.0180***	0.0190***	0.00389***
	(0.00374)	(0.00390)	(0.00307)	(0.00127)
constrlt1905	0.0360***	0.0678***	0.0167***	
	(0.00737)	(0.00814)	(0.00556)	
constr19061930	-0.00824	0.00697	-0.0100*	
	(0.00724)	(0.00744)	(0.00546)	
constr19311944	-0.0249***	-0.0103	-0.0193***	
	(0.00837)	(0.00826)	(0.00582)	
constr19451959	-0.0677***	-0.0616***	-0.0509***	
	(0.00758)	(0.00787)	(0.00597)	
constr19601970	-0.108***	-0.113***	-0.0555***	
	(0.00631)	(0.00705)	(0.00536)	
constr19711980	-0.0744***	-0.0667***	-0.0303***	
	(0.00701)	(0.00773)	(0.00491)	
constr19912000	0.0600***	0.0665***	0.0532***	

	(0.00661)	(0.00696)	(0.00484)	
constr20012010	0.0830***	0.0814***	0.0516***	
	(0.00753)	(0.00760)	(0.00559)	
treatment_all		-0.0148		
		(0.0180)		
Constant	8.965***	8.814***	9.514***	11.04***
	(0.0401)	(0.0387)	(0.0384)	(0.0591)
Observations	603,742	603,764	599,513	216,863
R-squared	0.869	0.856	0.923	0.968

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1