Rent control policies in Berlin: Role model or chilling example?

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Rent control policies in Berlin.
Role model or chilling example?

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Thanks to all the flats and houses
which sheltered me throughout the years

and more importantly to those who enriched my
life by living there with me.

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I. Introduction

From New York, London, The Hague to Berlin, in the last years increasingly desperate calls from tenants across the world for rent control policy have emerged due to a steep surge in rents (Cox, 2022; Das, 2022; West, 2021). The bigger the population living in rental housing the more urgent those calls are. Germany has the greatest number of housing tenants in the European Union with 50% of the population renting their homes compared to 30% in the Netherlands or the European average (Eurostat, 2020). In Germany itself, Berlin has by far the largest rent share with over 80% of the Berlin population living as tenants in their flats (Destatis, 2018; Local rent index Berlin, 2019). In recent years ever-increasing rents in the city have pushed the federal and national governments towards market interventions taking the form of new rent control measurements to secure affordable housing. Like no other city, Berlin has experimented with policies in this regard in the last years and continues to debate new policy proposals. Berlin therefore can be regarded as a petri dish for rent control policies in the last years. The German rent market and Berlin's rent policy experiments are being followed internationally as they could serve either as a role model or as chilling examples of how to relieve or how to burden tenants in a tight housing market (Cox, 2022; Voigtländer, 2017; Kurakin, 2021).

The two latest interventions in Berlin were the *Mietpreisbremse* ('MPB', translating approximately to 'rent brake') implemented in 2015 and the *Mietendeckel* ('MD', translating to 'rent cap') implemented in 2020. To create longer-lasting effects of policies so that not every year or two new policies are needed, a better understanding of already existing policies and how they (have) work(ed) is important. Therefore, those two policies will be analysed in this thesis.

The first policy, the MPB, has been evaluated empirically already a couple of times for Germany using the differences-in-differences method. I will use the differences-in-differences method throughout the thesis as well and contribute to the existing MPB literature in three ways. First, by extending the time frame. The literature covers up to one and a half years after the introduction of the policy. I will cover six years. Secondly, I will run additional regressions differentiating between socio-economic neighbourhoods in Berlin to establish who benefited the most from the MPB. This adds a greater understanding about the heterogeneity of benefits

regarding the MPB. Thirdly, I test for indicators of noncompliance by landlords, which was not done before.

The second policy, the MD, has so far not greatly been discussed econometrically in the academic literature. I will contribute to this gap in a methodological sense and with my results. One group of authors have published three papers on the MD. They seem to have utilised a differences-in-differences method, but the precise methodology is unclear. I will contribute to the methodology an alternative control group to analyse the MD effects. Moreover, as with the MPB policy, I will run a regional analysis to identify the differences between neighbourhoods.

Furthermore, this work contributes to the field of studying rent control econometrically. Empirical analysis and evidence on rent control policies are scarce.[1] Most research on rent controls is theoretical, establishing empirical ways of analysing rent control policies leads to new insights and testing opportunities for hypotheses. Drawing from the academic gap regarding the MD and further research regarding the MPB my research question is:

> *What effects did the Mietpreisbremse (MPB) and the Mietendeckel (MD) have on rents and other housing indicators in Berlin from 2015-2022 and did the effects differ between neighbourhoods?*

To answer this research question four hypotheses are formulated. They are being generated using a conceptual framework consisting of the welfare state economics theory combined with rational choice theory. The dataset which is used for the analyses consists of 600,000 observations of renting adverts. This pooled cross-sectional data regarding long term private renting contracts in Berlin and Hamburg from 2012 until 2022 is enriched with data on socio-economic neighbourhoods.

The MPB policy lowered rents by about 3%, but the effect ceased after two years. The MPB effect was longer detectable and stronger in socio-economic worse-off neighbourhoods. In contrast to the MPB, the MD policy had a more drastic effect of lowering the rents by over 15%. Contrary to the MPB the MD showed no differentiation between neighbourhoods. A differentiation between neighbourhoods for the MD is only detectable after the annulment of the MD. The effect was still detectable in lower socio-economic neighbourhoods whereas it vanished immediately in better-off neighbourhoods. A trade-off appeared for the MD policy. This policy led to a drastic decrease of supplied flats in the treated segment which ranges between 50%-70%.

[1] Academically receptions received only the studies of Cambridge (Massachusetts) by (Sims, 2007; Autor, Palmer, & Pathak, 2014) and of San Francisco (Diamond, McQuade, & Qian, 2019).

My thesis is structured in the following way. In the next section (section II), I will discuss the institutional context by introducing the two rent control policies and their legal mechanisms. Afterwards, section III deals with the theory and the conceptual framework used to derive the hypotheses. Section IV introduces the data used for this thesis and the methodology to empirically test the hypotheses. This section is followed by the analysis, section V, which contains results and discussion. Lastly, section VI closes the thesis with the conclusion and by identifying avenues for further research.

II. Rent control policies in Berlin

The degree of tenant rent protection in Germany was already quite high before the two policies were introduced (Voigtländer, 2017; Thomschke, 2016). Some regard Germany as a tenant's paradise (Fabricius, 2015) due to high protection for example against eviction or rent increases (Haffner, Elsinga, & Hoekstra, 2008). This existing high protection of tenants concerned only sitting tenants (Roßmann, 2015), i.e. only those who have a contract already. The rent price of newly rented out flats was on the other hand not regulated. The asking rent was determined by the market.

The MPB and the MD are breaking with this fundamental approach in the German rent regulation and regulate the asking price of new rents. Both policies aimed at securing the possibilities for poor and middle-income households to find affordable housing (BT-Dr. 18/3121, 2014; AGH-Dr. 18/2347, 2019). The MD furthermore had the goal to secure social peace in Berlin which is seen as threatened by displacement due to high rents (AGH-Dr. 18/2347, 2019).

First, the earlier MPB will be introduced followed by the MD.

1. Mietpreisbremse ('MPB')

In the national election in Germany in 2013, the Social Democratic Party (SPD) ran with one policy proposal which already encompasses the cornerstones of the latter policy. The possible rent increase of existing contracts was already regulated in paragraph 557 ff. of the German civil code (BGB). The policy proposal extended the legislation on new lettings. They should be capped at 10% over the local average. Newly built houses are exempted from that rule (SPD, 2013). This proposal found its way into the coalition agreement between the Christian Democratic Union (CDU), Christian Social Union (CSU) and SPD in November 2013 with some amendments (CDU, CSU, & SPD, 2013). A general rule for Germany favoured by the SPD was watered down to a policy that enabled federal states to implement the policy on a local

level (Schuler, et al., 2013). On 21 April 2015, the MPB was passed into national law (BGBl 2015 I 610, 2015). It came into effect on 1 June 2015. The MPB consist mainly of four newly added paragraphs 556d-g to the German civil code (BGB).

The most important provision states that if a flat is rented out in a tight housing market '*angespannter Wohnungsmarkt*' the rent can only be 10% above the local average rent (paragraph 556d I BGB). What can be regarded as a tight housing market is decided by federal governments. They can designate certain areas as being tight housing markets if the supply of flats under appropriate conditions is endangered (paragraph 556d II BGB). Berlin was the first federal state to use this policy and declared still in April 2015 with effect towards the first of June 2015 the entirety of Berlin as a tight housing market (Mietenbegrenzungsverordnung, 2015).

Initially, it was intended that a declaration concerning a tight housing market cannot exceed five years but due to a change in national law, this was extended to ten years (Schindler, 2020). Berlin used this change and renewed its declaration of a tight housing market in 2020 which will cede to exist on 31 May 2025 (GVBl. 2020, 343, 2020).

The average local rent is being computed by an estimation of comparable rents (location, size, endowments like heating system or bathroom) in the last four years in an area (Haffner, Elsinga, & Hoekstra, 2008). In Berlin, this average local rent is estimated every two years using scientific methods. The average local rent is then published as the Local Rent Index (LRI). The LRI points out the average cold rent per m^2. Cold rent means without utilities. The cold rent is the standard reference point in the German rent market and is also the one used by the MPB and MD as a reference. Before the MPB the LRI was used to limit the amount by which landlords could increase the rents of sitting contracts.

The MPB does not apply universally. Three types of newly rented out flats are exempted. The first exemption applies to flats that are being rented out for the first time after construction on 1 October 2014. Secondly, flats rented out for the first time after extensive modernisation (paragraph 556f BGB) are exempted. A renovation qualifies as extensive when the modernisation is comparable to having built a new house. This is considered to be the case if the modernisation costs approximately one-third of a new comparable house (Artz, 2020). These two exemptions can be differentiated in the regard that the first exemption applies generally while the second one only applies the first time after modernisation. The third and last exemption applies to flats that have been rented out above the 10% margin over the LRI

before the MPB came into effect (paragraph 556e I BGB). Those flats can be rented out at the same price as before, but a further rent increase is not allowed.

2. Mietendeckel (‘MD’)

On top of the MPB, the federal Government of Berlin implemented in February 2020 the so-called ‘Mietendeckel’ in Berlin. It was announced on 18 June 2019 and came into effect on 22 February 2020 (GVBl. 2020, 50.) It was perceived as the most drastic market intervention in the housing market since the end of socialism in Berlin (Kröger, 2021). The MD utilises different instruments for sitting tenants and newly agreed contracts. The first is the rent stop which came into effect immediately. With the rent stop, it was unlawful to ask for any rent that was higher than the one asked on the 19th of June 2019 (8 months earlier) paragraph 3 I MietenWoGBln. For new contracts, the policy postulates the maximum amount the landlord could ask for a m^2, paragraph 4 I MietenWoGBln. The maximum amount possible was set out in a table, paragraph 6 I MietenWoGBln. This table considered the year the house was built and the endowment but only very broadly. Differentiation between neighbourhoods was only slightly accounted for in paragraph 5 I MietenWoGBln. Furthermore, any rent increase was forbidden until 1 January 2022. Afterwards, rent increases were tied to the inflation rate but only to a maximum of 1.3% a year. If the inflation rate exceeds 1.3% the rent could still only be increased by a maximum of 1.3% paragraph 3IV1 MietenWoGBln.

As with the MPB, the MD exempted certain flats. Those exemptions are however significantly narrower than those contained within the MPB. Like the MPB the MD exempts newly build flats. In contrast to the MPB, the MD extends the date threshold for this criterion from the 1st of October to the 1st of January 2014. The second exemption possibility for modernisation with respect to the MPB is drastically reduced for the MD. Although the MD still mentions renovations and modernisations as possible reasons for exemption, the exemptions can now only be issued on a single case basis and if the flat was uninhabitable before and made again liveable through extensive renovation paragraph 1 Nr. 3 MietenWoGBln. This change was due to the perceived problem that people are being pushed away through renovating and modernising the houses (Artz & Börstinghaus, 2019). The last exemption of the MPB, previous higher rents, was with the MD completely scrapped.

Noncompliance like asking for too high rents by the landlords could be punished with a fine of up to 500.000 € paragraph 11 II MietenWoGBln.

The MD policy was judicially contested from the beginning. Different constitutional uncertainties arose during the legislative process but were disregarded by Berlin’s legislative

body (Fröhlich & Zawatka-Gerlach, 2020). In April 2021, the Constitutional Court of Germany ruled that the policy should be regarded as if it has never existed. They argued that the national legislator had by means of the MPB of 2015 reserved the power to regulate the rent market to itself. As a result, the legislative power of the federal states thereafter ceased to exist (Kaufmann, 2021).

As the court decision was based on formal and not material reasoning the possibility to enact an MD policy on the national level has not been ruled out. This topic was then hotly debated in the national elections of 2021 and led the MD policy to be adopted into different party manifestos for the national election in September 2021 (Hackenbruch, 2021; Joswig, 2021). So, even though the MD is not existing any longer its story is not over yet and is being discussed for example as well in Austria (Kurakin, 2021).

III. Theory and Conceptual Framework

I will use the welfare state economics theory and the rational choice theory to build my conceptual framework which I am going to use to analyse the MPB and the MD.

1. Welfare State Economics

Welfare state economics looks for justifications to intervene in the market. Those interventions should improve the welfare of society. A welfare improvement can be achieved by correcting for two things which can be differentiated to legitimise market intervention. The first one is to correct for market failures and the second one is a redistribution aspect (Barr, 2020). This mirrors the two fundamental welfare theorems. The first states that a competitive market is Pareto efficient. The second welfare theorem deals with the allocation without diminishing efficiency. The second theorem resembles the redistribution aspect of welfare state economics. The first welfare theorem applies to competitive markets but a market with market failures is not a competitive market. Market failures could exist for various reasons. Some are rooted in the type of the market like power asymmetries between the consumers and the producers or in the type of good (public good vs private good) or could be due to the type of exchange where information asymmetry exists (Parkin, Powell, & Matthews, 2017).

a) Market Failures

Two types of market failures are important in the case of rent control policies: information asymmetry and market power.

Information asymmetry appears if not both sides of a transaction have full knowledge about important aspects of the exchange (Stevens, Bowles, & Sethi, 2017). Information asymmetry

in the rental market appears in various forms. For example, the options for a tenant to check statements by the landlord e.g., when a renovation took place are limited. On the other hand, the landlord has restricted ways to verify the information e.g., on the financial credibility of the tenant. In the case of the MPB, a very important factor is the previous rent as that can serve as an exemption for the policy if the previous rent was higher than the price set out by the Local Rent Index + 10%. The previous rent is only known to the old tenant (who is in most cases not known to the new tenant) and to the landlord.

Information asymmetry can then lead to moral hazard. Moral hazard means behaving in a way that might be harmful to the other transaction partner but is not controllable due to a lack of information

Regarding the market failure due to market powers this occurs if not all requirements for a competitive market are met (Pleatsikas, 2018). Those are among others that the supply side are price takers (Landes & Posner, 1981; Parkin, Powell, & Matthews, 2017) and no exit and entry barriers exist (Harbord & Hoehn, 1994).

Those requirements are not met in the case of the renting market. For the supply side to be price takers they would have to face an elastic demand curve (Syverson, 2019). That would require the demand side to have a choice. No choice exists if the demand curve is inelastic. If a resource is scarce and substitutes are not available this could lead to inelastic demand curves (see e.g. Olmstead, 2010 for water) the same holds true for food and owner-occupied housing (Parkin, Powell, & Matthews, 2017). The demand side for housing in Berlin exceeds by far the supply. 137 inquiries faced an online ad for a flat on average in Berlin in 2020 (Wille, 2021). As people need a place to sleep and housing substitutes are not available, this leads to an inelastic demand curve. Therefore, the market is not competitive and market power asymmetries arise. A shortage of a commodity can be addressed by more suppliers entering the market. For that to happen the market needs to have low entry barriers (Karakaya & Stahl, 1989) but the entry barriers into the Berlin rent market are high. First, there is the barrier of time which hinders fast entries and second the barrier of cost. The process of planning and building a house spans years. Not regarding the planning and permits obtaining, it still takes on average three years in Berlin from the moment of permission to the finished house. The gap between approved and finished houses has increased drastically in the last few years (Investitionsbank Berlin, 2021). Secondly building a house is very costly. Especially in Berlin where the space is limited and the average price per m^2 of building soil ten folded in some areas in the last few years (Hörz, 2018). This makes it almost impossible for new actors to join the housing market. Therefore, landlords

already in the market can exercise market power and level the price above what a competitive market would render (Arnott, 1995).

b) Redistribution

The second legitimisation for market intervention concerns the redistribution aspect (Barr, 2020). A redistribution can occur from the supplier side to the demand side. In this case from the landlords to the renters. As written above, the great demand and the high market power of landlords enables the landlords to ask for higher rent prices. This price is not necessarily inefficient but could render solely gains on the landlord's side. This can be seen as unfair. The MPB specifically stated to transfer gains of the landlords back to tenants, especially for those in need of affordable housing (BT-Dr. 18/3121, 2014).

The MD was justified to keep the social peace in the city. Inequality rises if money is distributed unequally. Inequality affects negatively various aspects of living (Wilkinson & Pickett, 2019) but redistribution does not come without obstacles and can lead to less efficiency (Okun, 1975).

2. Rational Choice

The rational choice theory in contrast to the welfare state economics theory does not look at states and their behaviour but centres around individuals. This theory offers insights into why and how people act. The assumption is that people behave rationally and choose the options that give them the most utility after evaluating the costs and benefits of a certain policy (Peters, 2019). This might then not be in line with the demands of society at large. Institutions can influence how individuals react (Shepsle, 2008; Peters, 2019).

3. Conceptual Framework and Hypotheses

The conceptual framework I am going to use will combine aspects of these two theories. From welfare state economics I draw the redistribution aspect and the market failures of information asymmetry and market power. From rational choice theory I will use the utility maximising nature of individuals. The conceptual framework is then used to derive my hypotheses.

a) Market Power and Hypotheses 1 & 2

I will focus first on the market power aspect. As seen above, the burden of the market shortage might disproportionally be carried by the tenants in comparison to the landlords. As landlords behave rationally, they can seek to exploit this market failure and ask for too high prices for a flat (Arnott, 1995). This leads to welfare losses. According to the welfare state economics theory, this inefficiency can be dealt with a market intervention. In the case of the rental market, this occurs by lowering the rents. The MPB and the MD both aimed at exactly that.

An initial asking rent decrease would occur if the asking rents before the MPB were above the Local Rent Index +10% because the MPB only allows asking rents to exceed the LRI by 10%. That this was the case before the reform took place has been established by Held et al (2013) who found in Berlin that asking rents exceeded the price by 30% before the MPB (Held, Nielsen, Schürt, & Waltersbache, 2013). Thus, the MPB was set out to correct for the market failure of market power on the landlord's side. My first hypothesis is therefore:

1. The MPB lead to a decrease in asking rents

My second hypothesis concerns the MD. The construction of the MD is different compared to the one of the MPB, as it sets out a maximum allowed asking rent. If the set-out price by the MD was below the price achievable by the MPB I expect the MD policy to have lowered the rents. The price achievable by the MPB is the price from the LRI +10%. The list of the MD does not regard the size of the flat as an indicator like the LRI but otherwise applies the same criteria as the Local Rent Index. Taking then only the cheapest m^2 price[2] in every given category of the LRI of 2019[3] for every given building year category and applying the 10% margin to exceed the LRI allowed by the MPB (paragraph 556d I BGB) and comparing that to the price table set out by the MD in paragraph 6 MietenWoGBln and applying the local discounts, renders the A. Table 1. In Table 1 only the differences between minimum MPB prices and MD prices are shown. A negative sign indicates that the price set out by the MD table is below the price achievable with the MPB.

Except for the flats from 1973-1990 which can be explained through the lack of differentiation between former East and West Berlin in the MD in every case the MD price was below the price achievable by the MPB. Therefore, I expect that the MD policy corrected the market failure of market power on the landlord's side by lowering rents. Furthermore, as the MD included no possibility of further increasing the prices, I would expect this effect to hold stable over the existence of the policy. My second hypothesis is consequently:

2. The MD led to an immediate and stable asking rent decrease

[2] The LRI differentiates between the sizes of flats which the MD table does not. The prizes vary substantially between the sizes of the flats. For example, taking a simple neighbourhood the flats under 40 m^2 are almost 30% more expensive per m^2 than the flats in the same neighbourhood which exceed 90m^2. But if even the cheapest m^2 price +10% is above the price the table of the MD assigns then it will defiantly hold for all flat sizes.
[3] The LRI of 2019 was the most recent one before the MD applied.

Table 1 Difference between the rents possible under the MPB and the MD policy

	Building Year						
	Till 1918	1919-1949	1950-1964	1965-1972	1973-1990 East Berlin	1991-2002	2003-2017[4]
Difference LRI good and MD good	=-0,91€	=-0,35€	=-0,35€	=-0,63€	=+0,92€	=-1,13€	=-0,33€
Difference LRI middle and MD middle	=-1,09€	=-0,43€	=-0,61€	=-0,08€	=+0,17€	=-0,65€	=-1,13€
Difference LRI simple and MD simple	-0.68€	-0,46€	=-0,34€	=-0,33€	=+0,01€	=-0,64€	=-3,25€

Note: LRI is the cheapest LRI price for a building year plus the 10% margin. MD is the price set out by the MD policy with the applied reduction or plus regarding the area of the LRI. This table shows only the difference between the two values for the full table see A. Table 1.

b) Redistribution and Hypothesis 3

The aim of the MPB and the MD was to help secure affordable housing. The main assigned beneficiaries of the policies to keep housing affordable were low-income groups (BT-Dr. 18/3121, 2014; AGH-Dr. 18/2347, 2019). Based on the set-out goals by the policy, I expect the MPB to have a stronger effect in worse-off neighbourhoods than in better-off ones. For the MD Table 1 gives unclear results. Every neighbourhood sees twice[5] its rent the most reduced compared to the other neighbourhoods. Hence, I expect in my third hypothesis:

3. The MPB had stronger effects in worse-off neighbourhoods and the MD was indifferent between neighbourhoods

c) Non-compliance and Hypothesis 4

As pointed out by the rational choice theory, people will try to behave in a way that maximises their utility. As shown by the welfare state economics in a market with imperfect information people might act in a way that benefits them but harms others and is due to imperfect information not preventable. Here, the theories of welfare state economics and rational choice

[4] MD table goes only until 2013 because flats from 2014 onwards are exempt.
[5] Not considering the outlier 1973-1990 flats.

theory overlap (Baker, 1996). In the case of the MPB, the landlord´s information of an exemption was not accessible to tenants. That can be regarded as non-compliance by the landlords. This is further incentivised if the policies are not enforced. The MPB had weak enforcing mechanisms. Another non-compliance could take place by using furnishing to circumvent the policies or to withdraw from the market entirely. Drawing from the moral hazard due to information asymmetry and the rational behaviour of landlords I expect in my hypothesis 4 that:

4. Landlords showed non-compliance

IV. Methods & Data

In the following section, I will first give an overview of the datasets I used. Afterwards, I will introduce the differences-in-differences method followed by the operationalisation for each hypothesis.

1. Data

    a) Main Dataset

The dataset concerns long term private renting contracts in Berlin and Hamburg. For Berlin, the years 2012 until 2022 are available for Hamburg from 2017 until 2022.[6] The data is a sample from online platforms and news outlets over which in Germany flats are being rented out and includes over 600.000 observations. It is pooled cross-sectional data, meaning observations are taken over time but each observation is an independent flat. The dataset contains the cold (without utilities) and warm (with utilities like water and heating) rent,[7] the flat size, the furnishing, the location by postal codes and various other variables.

The dataset only consists of the asking rents of the advertised flats and not the transaction rent. Transaction rent is the rent actually paid and the asking rent is rent the one advertised. This is not a problem as in contrast to buying houses where negotiations happen in Germany,[8] tenants in Berlin do not have the negotiation power to individually agree on another rent then advertised. A bidding competition between tenants to secure the apartment does not take place. Applicants are only informed if they are chosen or not.

[6] The dataset was made available by Value Marktdaten AG. It is the biggest dataset of this kind in Germany.
[7] The cold rent is the amount which can be set by the landlord.
[8] Dinkel and Kurzrock (2012) found a significant difference between asking and transaction house prices in six counties of Germany.

Although the sample is drawn from over 100 sources, not every flat is advertised publicly. Anecdotal evidence points in the direction that some flats are being mediated through personal channels and are therefore not advertised online or in print media. Additionally, housing cooperatives have internal waiting lists and are consequently not part of the dataset either. The dataset does therefore not cover the entirety of the market. This is however just a minor limitation as with the 100 sources of the Dataset almost all advertised flats can be detected.

b) Local Rent Index

The Local Rent Index is being published every 2 years. I used the one from 2015 as it was the year the MPB was introduced and the one from 2019 as it was the most recent one before the MD. The LRI covers the estimated median rent of newly agreed contracts in the last four years. The LRI separates Berlin into three living areas, from good to middle to simple neighbourhoods. This separation is done by accounting for various factors like access to public transport, green areas and socio-economic factors (Local rent index Berlin, 2019).

I aligned those neighbourhoods to the postal codes of Berlin. Then I added this classification to the main dataset.

c) Household Income and Unemployment

I further enrich the dataset with the average household income by district and the long-term unemployment rates by "*Lebensweltlichen orientierte Räume*" (LOR). The data on the average household income on district level is taken from the Mikrozensus in Berlin compiled by the statistical office of Berlin and Brandenburg. The unemployment rates are available on a more granularly level then the household income. Separating Berlin into 61 neighbourhoods (LORs). The data on unemployment by LOR is also collected by the statistical office of Berlin and Brandenburg. I aligned the LORs with the postal codes and afterwards added the unemployment numbers to the dataset

The data regarding household income is from the year 2018. The data regarding unemployment rates is from 31.12.2017. I have chosen those timepoints because they are in the middle between the MPB and MD policies.

Table 2 and Table 3 show some descriptive statistics of the dataset used to analyse the hypotheses.

Table 2 Descriptive statistics concerning rent

Variables	Observations	Mean rent per m^2 in €	Standard deviation
Treatment Group MPB Berlin	368,690	8.96	2.91
Control Group MPB Berlin	91,043	11.85	3.87
Treatment Group Berlin MD	335,771	9.1	2.92
Control Group MD Hamburg	122,903	11.54	3.09
Lift	192,067	10.23	3.92
Barrier free	27,811	10.96	4.07
Shower	133,440	10.86	3.61
Furnished	12,483	11.42	4.32
Neighbourhoods			
LRI good	61,466	10.81	3.45
LRI middle	82,131	8.88	2.94
LRI simple	110,824	9.20	3.25
Income high	211,497	9.86	3.11
Income medium	136,179	8.58	2.85
Income poor	170,206	9.83	3.70
Unemployment low	236,932	10.45	3.44
Unemployment medium	95,930	8.77	3.01
Unemployment high	109,440	8.35	2.80

Note: Except for the Control Group MD Hamburg every variable only includes the values for Berlin observations. That includes 459,733 observations. The price is always per m^2 and always without utilities. LRI is the Local Rent Index. The number of observations indicates the number of observations fulfilling the requirement. Numbers for the regional analyses varies in total amount as the matching with the postal codes worked differently and led to some overlapping or not.

Table 3 Descriptive statistics concerning general characteristics

Variables	Mean	S D	Min	Max
Living space in m^2	72.49m^2	33.09	8m^2	570m^2
Rent per m^2	9.53€	3.33	2.36€	42.86€
Building Year	1953.19	40.96	1205	2023
Lift	0.42	0.49	0	1
Barrier free	0.06	0.24	0	1
Shower	0.29	0.45	0	1
Furnished	0.03	0.16	0	1

Note: Only the 459,733 values for Berlin are included.

2. Differences-in-Differences Method

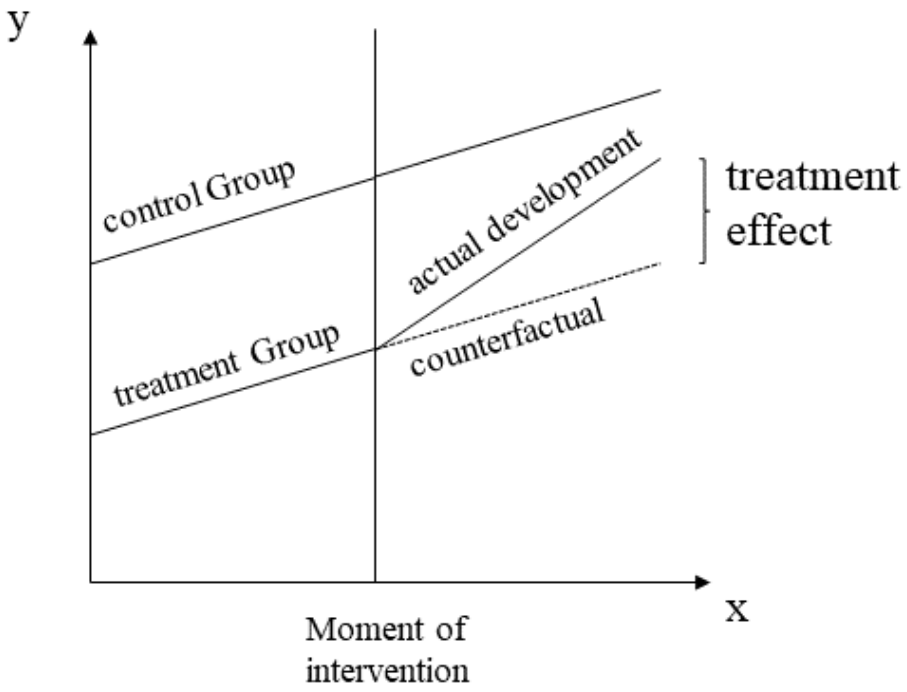
Establishing causal relationships is challenging. The best option is to have a large sample and assign treatment and non-treatment randomly. Furthermore, only those assigned treatment are actually treated and there is no problem with non-compliance. This “gold standard” is called randomized control trials (RCT). If the treatment and control group are the same and differ only in the assigned treatment, then any difference in the outcome between these two groups is due

to the treatment and the causal relationship has been established (Angrist & Pischke, 2015). Although RCTs are the best option to establish the effects of one variable on another they are not always doable. There might be ethical, legal or feasibility issues (Toshkov, 2016). In Germany, all powers of the state (Legislative, Executive and Judicative) must follow the primacy of the constitution the basic law (Ossenbühl, 2007).

Part of the Basic Law is Art. 3 which establishes the principle of equal treatment. Arbitrary differentiations are unconstitutional if no proper reason to differentiate exists (Kischel, 2020). Conducting a randomised control trial on a big policy scale does not qualify as such a legitimate reason as it would have to assign people with the same characteristics to the treatment and control group. Econometrics has established ways to approximate the RCT without the rigid requirements of experiments.

Looking at the requirement of an experiment of having a treatment and a control group it becomes clear that this does not only occur planned but can happen unintentionally as well. These are called natural experiments (Jäger & Pischke, 2021). They could happen for example due to a natural disaster like bacteria affecting only certain water suppliers (Snow, 1855) or a different applied jurisdiction in neighbouring counties (Card & Krueger, 1994). Another option is that a policy affects only certain observations and not others. This can mirror a treatment and a control group and open the path for the differences-in-differences method. This method approximates an RCT and establishes a causal relationship between two aspects (Angrist & Pischke, 2015). It works in the following way. Before a treatment occurs, the treatment and control group behave similarly. This is the core assumption of the method. This similar behaviour is then used to calculate the development of the treatment group after the intervention assuming no treatment would have taken place. This is done by transferring the line of the control group to the treatment group from the moment of intervention onwards. This is the counterfactual line but this only works if the common trend assumption holds before. The difference between the calculated counterfactual line and the observed line is then the causal effect of treatment (Angrist & Pischke, 2015). This is represented in Figure 1.

Figure 1 Difference in difference



Source: Own representation based on Angrist & Pischke, 2015.

The aim of this method is not to have a before and after implementation evaluation but a with and without evaluation with estimating the unobservable counterfactual in which the intervention would not have taken place (Toshkov, 2016).

In the case of rent control policy, this method has been applied for the first time by (Sims, 2007) who studied the sudden end of rent control in Massachusetts in 1995 and used by others since then to establish the effects of the MBB in Germany.

3. Operationalisation

In the following, all hypotheses will be operationalised individually.

To analyse the effects of the MPB the differences-in-differences method was first used by Thomschke and Hein (2015) and later by others like Kholodilin, Mense and Michelsen (2016). Different operationalisations of the treatment group and control group exist. Thomschke compared in his research flats in the scope of the MPB as a treatment group to houses exempted in the same area as a control group. Another composition of treatment and control group was used by Kholodilin, Mense and Michelsen (2016). They used as a treatment group flats under the scope of the MPB which are in a county bordering a county not under the scope of the MPB. In this unregulated county, they took similar groups as the control group.

For my analysis of Berlin, this latter approach has certain limitations. First, since Berlin in its entirety was declared to be under the scope of the MPB, the counties bordering the closest

federal state, Brandenburg, are limited. It would exclude central districts. This would limit the relevance of the study as the centre districts are the most urban and experiencing the most pressure in the housing market (Thomschke, 2016). Additionally, as in the case of Berlin, it would mean crossing federal states lines and therefore a different applied jurisdiction which could lead to less comparable groups. Therefore, I will follow Thomschke's approach in the construction of the treatment and control group. Three exemptions existed for MPB. Flats exempted will be my control group. The others will be my treatment group. Reasons for exemption from the policy were:

1. Newly constructed. First time rented out after 1.10.2014
2. First time rented out after extensive renovation
3. Previous rent exceeded the MPB

The first exemption, the year of construction, will be used in the following way. Every house built after 2014 will be in the control group. Additionally, I include in the control group every house which was built in 2014 or 2013 and was rented out in the same year. This mirrors the policy before the implementation. Otherwise, observations for the control group start only half a year before the policy implementation. The second aspect can be approximated. The dataset contains the year of renovation and modernisation. It does not include how extensive the renovation was. My assumption is that only when an extensive renovation took place it will be advertised as such online. In my control group is therefore every flat if it was renovated and rented out for the first time. The third aspect, having agreed on a higher rent on a previous contract, cannot be considered in the compiling of the treatment group as there is no data available showing the previous rent. Consequently, some exempted flats will end up in my treatment group. To account for exemption 3, I extended the first time rented out criterion of exemption 2 to having been renovated in the last three years. The second exemption led to a price freely chosen above the LRI +10%. This would then qualify the flat for an exemption under exemption 3 due to previous agreed higher rents but as that rent cannot be increased afterwards above the level of the first time rented out, this exemption fades out over time. I set this limit to three years. This composition of treatment and control group will be used for every regression if not stated otherwise.

Control and Treatment group therefore do not consist of the same houses. As the control group includes all the newly built houses and the treatment group the older ones. Regardless of the common trend assumption this would be a problem if newly built houses differ greatly for example in the neighbourhoods where they are offered compared to existing flats. This is not

the case. Treatment and control group are part of the same housing market and although they differ in the characteristic of building year are otherwise comparable.

Regarding the inclusion of the control variables, I followed existing research on the MPB.[9]

My differences-in-differences model has the following equation:

$$\ln(r_{ot}) = \alpha + \beta' \cdot X_{ot} + \gamma_0 G_0 + \delta_1 T_t + \tau(G \cdot T) + \epsilon_{ot}$$

ln represents the logarithmic changes in rent prices. r represents the rents over an observation o at time t. α is the intercept X are the control variables. β captures the effect of those control variables over time. G is a binary variable 0 & 1 depicting the control and treatment group, where the treatment group is 1. γ is the group effect. The next term captures the time trend. T is the time variable also binary 0 & 1 and one after the treatment. δ the effect of the time trend. In my computation T is considered as half years after the implementation of the MPB and individually computed for every half year. τ is the treatment effect as it captures the interaction term between the group and time variable and only offers a result if both the dummy group and the time variable are 1. ϵ is an error term over the observation and time.

I will evaluate the MPB policy at half-year intervals. As robustness checks, I will conduct two placebo tests. One a year and the other two years prior to the policy implementation.

As the reference half-year, I selected 2015h1 as it was the most immediate half-year before the policy was implemented. As the MPB took effect on the first of June 2015 one month is included in this group although already treated.

For hypothesis 2 the sudden end of the policy due to the Constitutional Court ruling in April 2021 which changed the environment from one day to another would enable a regression discontinuity approach at the end of the policy. This however would not grasp the full effects of the policy as due to the MD in the previous year the rental market in Berlin has already been changed to such a degree that the jump in prices after the court decision does not reflect the initial decrease. To estimate the full effect, I will therefore use a differences-in-differences model as well. Dolls, Fuest, Krolage and Neumeier (2020); Dolls, Fuest, Neumeier and Stöhlker (2021) and Arlia, Dolls, Fuest, Gstrein and Krolage (2022) have analysed the MD empirically every year since 2020 but are not clear about their methodology or full results. Their research

[9] Control variables are building year, living size, lift, barrier-free, in need of renovation, like new, recently build and shower. Following among others Deschermeier, Seipelt and Voigtländer (2017) and Thomschke II (2019) building year is included squared as well and the living size as an ln.

concerns renting prices, buying prices and offerings. Their research reads like they have utilised a differences-in-differences model as well. They used every city in Germany with 500.000 or more inhabitants as a control group. This is not convincing for three reasons. Firstly, the 500.000 inhabitants criterion led them to include 12 cities as a control group. A quarter of those cities never enacted the MPB policy as they do not have a tight housing market. A comparison to the MPB is therefore not too fruitful. Secondly, the cities included have seen stark differences in the last years between rent and inhabitants' development, rendering a comparison less useful. Lastly, the time when the MD was in place was during the first year of COVID-19. This affected cities differently. Especially between metropoles and smaller cities. Comparing therefore for example Nürnberg in Bavaria with 500tsd inhabitants to Berlin with 3.6 Mio inhabitants is not plausible. I will therefore pursue a different path and will use the treated group of the MPB in Hamburg as a control group. As a treatment group, I use flats regulated by the MD policy in Berlin. The original composition of MPB regulated and non-regulated flats in Berlin is not useful for the MD policy, because with the introduction of the MPB the groups have differentiated. To base the counterfactual of the MD treatment group in 2020 on the common trend of the treatment and control group of the MPB before 2015 is not convincing. As with progressing years, the common trend assumption weakens. Hamburg applied the MPB too but did not implement the MD. As the MPB in contrast to the MD was a national, not a federal law the same MPB rules apply to Hamburg as they did to Berlin. Therefore, policy-wise the control and treatment group only differ in the implementation of the MD policy treatment. Regarding other housing and social indicators, Hamburg gets closest to Berlin. The cities differ only 0.2% in inhabitants' developments from 2016-2020 (Investitionsbank Berlin, 2021) and are regarding city size, a renting market under pressure closest to each other.

My control group for hypothesis 2 will therefore consist of the flats in Hamburg which are subject to the MPB. Thus, they will be constructed in the same way as the treatment group for hypothesis 1 was compiled and differ only in the federal state. The treatment group will be flats in Berlin. This treatment group differs from the treatment group for the MPB as with the stricter regulation renovated flats are now not considered exempt anymore. Furthermore, now all flats having been built after 2013 are removed from the treatment group as the MD changed the qualifying date for exemption from October to January of 2014.

I constructed an alternative model to mirror the treatment group of Berlin in Hamburg to see if its effects are due to the different composition of treatment and control group.

I conduct this analysis in contrast to hypothesis 1 in quarter time frames and not half-year time frames because my time span is now five years instead of ten and the policy was only in place

for one year allowing a more granular overview. This is as well useful because the end of the policy falls on a quarter and not a half year. The formula stays the same only that now the time variable and group variable have been modified.

As a moment of reference, I have chosen 2019q2. This was the last quarter before the policy was announced on 19 June. For this, I follow Dolls, Fuest, Krolage, and Neumeier´s (2020) reasoning as they have shown immediate anticipation effects after the policy was announced.

As robustness checks, I apply two placebo tests. One pre-placebo test two quarters before the policy implementation and one post-placebo test two quarters after the policy was ruled unconstitutional.

To test for hypothesis 3 a differentiation between richer and poorer neighbourhoods is necessary. Afterwards, the same regression as for hypotheses 1& 2 will be run but with the differentiation into neighbourhoods. I will differentiate between three neighbourhood indicators: the Local Rent Index, mean household income on the district level and the unemployment rate on the LOR level.

Firstly, I will use the differentiation of the LRI of Berlin into good middle and simple areas to check which neighbourhood the MPB and the MD affected the strongest. The LRI calculation of good middle and simple areas serves as a proxy for richer and poorer neighbourhoods. Thomschke (2016) tried to establish for each flat individually which classification of the Local Rent Index in Berlin applies. In contrast to that I follow a more macro-level approach using the assignment on the level of postal codes. I am looking less at the individual characteristics of a flat but the overall neighbourhood of a postal code because the socio-economic distribution in a city is not flat by flat but rather block by block (Kessl & Reutlinger, 2022). The classification of the LRI is focused on housing characteristics and has been criticized for not capturing the real developments (Rendtel, Sebastian, & Frink, 2021). This can also be seen in Table 2. The mean rent for new contracts in the last four years in LRI simple neighbourhoods is above those in the medium well-off neighbourhoods. I use the LRI in my analysis because it is used by the MPB and MD to differentiate between locations in Berlin and to achieve the redistribution aspect set-out by the policies.

Next two the LRI differentiation I use mean household income on a district level. I differentiate between wealthy, middle and poorer districts. From 1775€-1900€ household income, I regarded them as poorer. From 1900€-2025€ as medium and from 2025€-2325€ as wealthy districts. These values are chosen to have an equal number of districts in each third.

To account for inner district discrepancies I use a third Model. This third model takes long-term unemployment numbers at the LOR level into account. LORs are administrative units used by the Berlin administration. They are compiled to represent building and socio-economic homogeneity (Senatsverwaltung; Amt für Statistik Berlin-Brandenburg, 2020). With a greater emphasis on the socio-economic aspect, they are more suitable for my analysis than the classification by the LRI. Unemployment numbers vary greatly across the city from below 8% to above 38% of the population below 65. The amount of money received as a recipient of unemployment benefits was in 2017 409€. A high share of long-term unemployed people with low income due to the low amount of benefits serves as a proxy for better and worse-off neighbourhoods. I consider neighbourhoods with below 16% of recipients as well-off between 16%-25% as median-well-off and above 25% as poor. This differentiation is chosen to have an equal distribution of inhabitants between the three groups. As can be seen in Table 2 in this category the mean of new asking rents still mirrors the separation between well-off middle and worse-off neighbourhoods. In the two other indicators, the new asking rent is different compared to the assigned groups.

For hypothesis 3 the formula stays the same as for hypotheses one and two with the difference that the regressions are now only conducted region by region.

Hypothesis 4, non-compliance, opens a lot of indicators to look at. I will look at three. Firstly, a path described to be used to circumvent the MPB was by pretending to be qualified for an exemption, so for example by having asked higher rents before or done extensive modernisations. Unfortunately, no data exist for the rents of the flats before. To account for the information asymmetry, the MPB law was changed with effect on the 1.1. January 2019. A newly added paragraph 1a to §556g BGB established that to qualify for an exemption for the MPB you need to pass on the relevant information to the new tenant and the complaint burden for the tenant has been decreased. Although the burden of proof for the landlord is very low (a pure statement is enough) (Joppe, 2018), I expect existing non-compliance to have decreased after the introduction of the new policy. I will test this by seeing if there was a change in the effectiveness of the MPB at the year change 2018/2019 using the differences-in-differences model of hypothesis 1.

Secondly, another way described to circumvent the MPB and MD was by renting out ‘furnished’ apartments and ask a steep surcharge for this (Peter, 2020). This surcharge could though not be included in the cold rent which only gives the price per m^2 but would have to be

included (if the landlords comply with the law) in the extra costs charged (Sarikaya, Kühlberg, & Sprotte, 2022). The extra costs should not change between treated and non-treated groups as they are mostly constructed to include costs for water and gas. Taking the non-compliance into account I do expect that extra costs have increased in the treatment groups of MPB and MD. I will run the same differences-in-difference model of hypothesis 1 only exchanging the cold rent for extra costs. Additionally, I will compare the results of hypothesis 1 with and without the inclusion of furnished flats.

Thirdly, due to reduced profit margins, it was expected that landlords under the scope of the rent control policies withdraw from the rental market and seek profits elsewhere for example by selling the flat (Haffner, Elsinga, & Hoekstra, 2008; Voigtländer, 2017). This negative incentive only applies to the controlled and not the uncontrolled segment. I will therefore look if the number of observations has changed after the implementation of the MD policy using a differences-in-differences model again. The dependent variable is the ln of observations to account for percentage changes in observations. As a treatment group I use the MD treatment group of Berlin and as a control group, I use non-treated flats of the MD policy in Berlin and the hypothetical treatment group of an MD policy in Hamburg. No control variables will be applied as I will just look at the observations.

The formula is then:

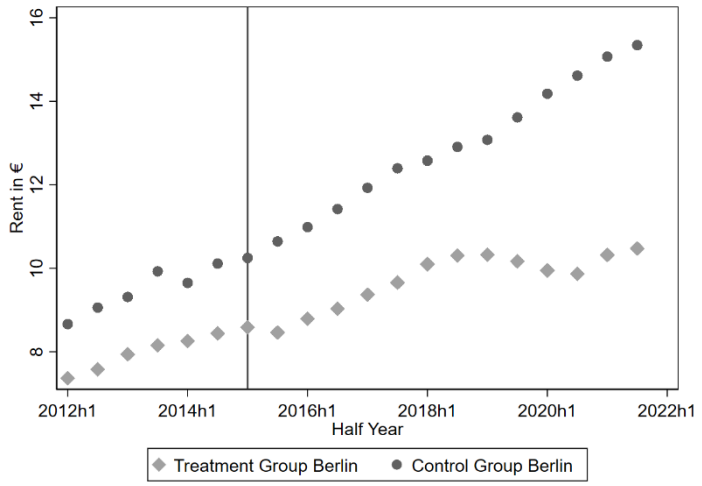
$$\ln(O_{ot}) = \alpha + \gamma_0 G_0 + \delta_1 T_t + \tau(G \cdot T) + \epsilon_{ot}$$

V. Analysis

My Analysis is organised the following way. The results are shown for each hypothesis individually. In the individual analysis of each hypothesis, the discussion and potential limitations, as well as robustness checks, are included.

1. The Effects of the MPB

Graph 1 Rent prices per m^2 for the Treatment and Control Group in Berlin under the MPB



Note: The vertical line indicates 2015h1. The reference half year. The policy was implemented at the end of 2015h1.

Graph 1 depicts the rent development between the treatment and control group in Berlin in the last decade. The MPB policy was implemented at the end of 2015h1. Visually a small decrease can be identified. The magnitude of this decrease can be seen in the results presented in Table 4 Model 1.

The MPB lowered asking rents by 3% for two years. I find for the second half of 2015 that the MPB lowered the rent by around 3.7% and for the half years to follow until 2018 by around 3%. Those results are statistically significant. Afterwards, the results cease to be statistically significant for one year until 2019. In the first half-year of 2019, I find a small negative effect of 1.5% which then increases to about 5% in the next half year and triples from there on onwards to a negative effect of 15%.

For the following analysis I will separate the time since the MPB into three time frames. The first time frame is from the implementation 2015h2 until 2018h1. The second time frame covers 2018 and the first half of 2019 and the third time frame 2019h2-2020h2. The first time frame shows the immediate significant effects of the MPB. The second time frame is characterized by no significant effect. The third time frame is defined by a drastic decrease in rents. Other empirical results do exist for the first time frame.

Table 4 The Effects of the MPB on Rents in Berlin over half years

VARIABLES	(1) Model 1	(2) Model 2	(3) Placebo 2014h1	(4) Placebo 2013h1
Treatment Group	-0.116***	-0.159***		
	(0.00431)	(0.00476)		
Interaction Terms				
2021 H2	-0.154***	-0.121***		
	(0.00821)	(0.00896)		
2021 H1	-0.139***	-0.115***		
	(0.00829)	(0.00896)		
2020 H2	-0.143***	-0.130***		
	(0.00832)	(0.00881)		
2020 H1	-0.107***	-0.0894***		
	(0.00773)	(0.00821)		
2019 H2	-0.0506***	-0.0358***		
	(0.00622)	(0.00667)		
2019 H1	-0.0145**	0.0153**		
	(0.00608)	(0.00658)		
2018 H2	0.00239	0.0296***		
	(0.00594)	(0.00649)		
2018 H1	-0.00153	0.0271***		
	(0.00580)	(0.00632)		
2017 H2	-0.0341***	-0.00513		
	(0.00581)	(0.00630)		
2017 H1	-0.0297***	-0.00540		
	(0.00568)	(0.00610)		
2016 H2	-0.0334***	-0.00899		
	(0.00574)	(0.00625)		
2016 H1	-0.0259***	-0.0248***		
	(0.00569)	(0.00608)		
2015 H2	-0.0365***	-0.0244***		
	(0.00577)	(0.00627)		
2015 H1	0	0	-0.00716	0.00231
	0	0	(0.00546)	(0.00626)
2014 H2	0.00802	0.00749	0.000859	0.0103*
	(0.00565)	(0.00697)	(0.00499)	(0.00586)
2014 H1	0.00716	0.0236***	0	0.00947*
	(0.00546)	(0.00690)	0	(0.00567)
2013 H2	-0.0119*	0.0122	-0.0191***	-0.00958
	(0.00679)	(0.00809)	(0.00626)	(0.00697)
2013 H1	-0.00231	0.00513	-0.00947*	0
	(0.00626)	(0.00830)	(0.00567)	0
2012 H1	-0.00458	-0.0160*	-0.0117**	-0.00227
	(0.00627)	(0.00872)	(0.00568)	(0.00645)
Constant	24.52***	24.02***	24.46***	24.43***
	(7.423)	(7.416)	(7.423)	(7.423)
Observations	363,429	359,261	363,429	363,429
R-squared	0.419	0.416	0.419	0.419
Furnished flats	yes	no	yes	yes

*Note: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The table shows the effect of the MPB by half year. 2015h1 is the reference point. For the full table including control variables see A. Table 2. Models 3 and 4 are the placebo tests. Which use 2014 and 2013 H1 as reference points. The observation drop in Model 2 two can be explained by the exclusion of furnished flats. All columns include control variables.*

My findings for the first time frame is an approximate decrease of around 3%. This finding is in line with existing research. Thomschke II (2019) finds an effect of -2.7% for the MPB. Around 3% has also been found by Deschermeier, Seipelt and Voigtländer (2017). Kholodilin, Mense and Michelsen (2016) find in contrast to that no effect but utilize a different methodology and aggregate the effect over various cities. Therefore, their findings do not contradict my results.

Surprising are the findings for the second time frame. The MPB effect ceases to be significant. The policy was already effectless after two years. Its extension from originally five to ten years is therefore questionable. The drastic reduction in time frame three can be explained by the MD policy. That the MPB has a significant effect after the MD policy was annulled is an indicator that the MD policy changed the market.

Regarding the plausibility of the results the common trend assumption is confirmed. Before the implementation of the treatment six out of seven half years show no significant result for Model 1. This indicates that the control group and the treatment group indeed behaved similarly before the policy implementation. Thus, the assumption that they would have continued to develop identical if the policy would not have been implemented is supported.

As robustness checks I conduct two placebo tests. Both placebo tests support the plausibility of the results. The first placebo test has as reference point 2014h1 and the second 2013h1, so one and two years before policy implementation. The placebo test pretends that the policy was not implemented in 2015 but in 2014 or 2013. The treatment group therefore gets a “placebo” as no real change between the two groups took place. If the results after the placebo treatment are significant something apart from the policy influenced the two groups differently before policy implementation. The results are shown in columns 3 and 4 of Table 4. The results are not significant for the first placebo and only in two cases slightly significant for the second placebo two years prior. This further strengthens the assumption that the treatment and control group developed similar before the policy was implemented and would have continued to develop similar in the absence of the policy. The significant effects of Model 1 can therefore be attributed to the change that the MPB brought to the treatment and control group.

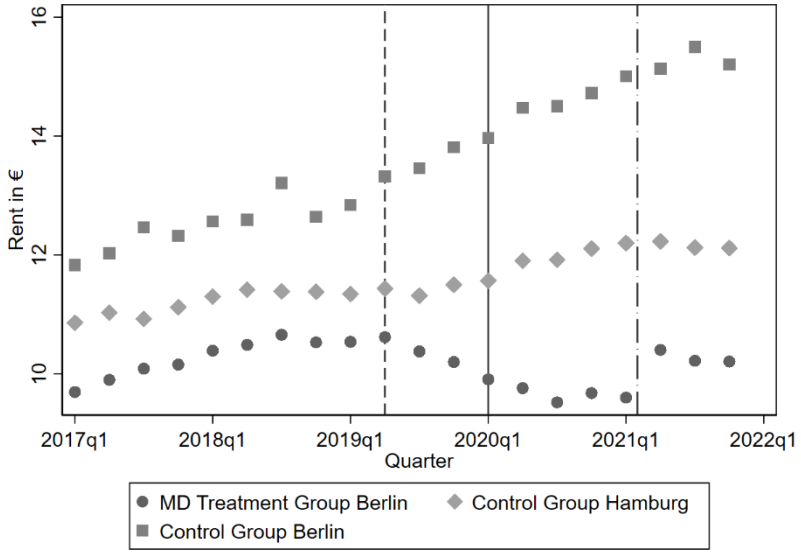
A limitation is that from approximately 460tsd Berlin observations only 360tsd are included in the regressions. This reduced sample size is due to observations where the variable of building year is missing. If the observations which have a missing building year are mostly old and therefore regulated flats this could alter the results. I tested for this by rerunning Model 1 with the inclusion of those flats in the treatment group. The results did change only slightly. This is

an indicator that they indeed mostly belong to the treatment group, but to what degree is unclear. As building year is a central variable around which the treatment and control groups are compiled leaving the variable out would not be an option. As there is no way of detecting the building year and no information exists to what precise degree those observations fall in the regulated or unregulated sector and the results if included do not change greatly, I have decided to exclude the observations where this variable is missing.

Hypothesis 1 which states that the MPB lowered rents can therefore be confirmed. The MPB lowered rents by around 3% for two years.

2. The Effects of the MD

Graph 2 Rent per m² before while and after the MD in Berlin and Hamburg in the Treatment and Control Groups



Note: The dashed line represents the quarter the policy was announced. The solid line is the moment the policy was implemented and the dash dotes line is when the policy was annulled by the constitutional court.

The MD policy lowered rents. Visually the drop in the treatment group of Berlin is identifiable in Graph 2. The effect could already be seen in Table 4 Model 1 in the third time frame. Observable is as well the different trend of the MD treatment group in Berlin and those not directly regulated by it in Berlin. The unregulated flats saw an increase in rents after the policy was adopted as can be seen in Graph 2. If this is due to the MD policy cannot be established as a viable control group is lacking.[10] The effect of the MD policy on the treated group can be

[10] One option could have been to compare the Berlin MD control group to Hamburg´s MPB control group. But those groups did not show a common trend. Additionally, the rent increases in the uncontrolled group in Berlin started already a quarter before the announcement of the MD policy. An explanation for the rise before the official policy announcement could be an early anticipation effect. Even before the precise policy was announced the policy idea was widely discussed. It could be that in the less regulated new building segment landlords reacted earlier and faster to compensate for anticipated losses, but this would need further research.

estimated by comparing the MD treatment group with the control Group of Hamburg[11], because of the common trend before. The results are presented in Table 5 Model 1.

The time around the MD can be separated into three time frames. The last two quarters of 2019 before the implementation are the first time frame. The second spans from 2020 until 2021Q2 while the MD was in effect and the third captures the post-MD time from 2021Q2 until 2022Q1. In the first time frame, the effect is rather small with a decrease of 1% and around 4%. In the second time frame, the effect triples to around 15%. And in the last time frame the effect halves to a stable of around 8%.

The small reduction in the first-time frame can be explained by anticipation effects. The policy was designed in a way that any rent higher than the one agreed before the announcement of the MD policy in June 2019 was unlawful. Therefore, in the quarters after the announcement, the policy effect can already be detected, as an increase in rents at that moment would have been unlawful a couple of months later. The full effect can be seen in the second time frame where a significant negative effect of 15% can be detected.

The MD policy, therefore, lowered the rents by around 15% compared to what would have been without the MD policy. Arlia, Dolls, Fuest, Gstrein, and Krolage (2022) found an effect of around 11%. My results show stronger effects. This can be explained by the different data source utilised and by the different composition of the control and treatment groups. The overall magnitude of the results is although the same.

The MD policy changed the market sustainable. The significant effect of -8% in the third time where the MD policy was already annulled can be explained by this. It could be argued that the effect in the third time frame is not anymore due to the MD but again caused by the MPB policy which was still in place. This is not convincing for two reasons. Firstly, the MPB did show results with a far smaller magnitude and already ceased to be significant before. Secondly, if the MPB needed the push of the MD to be effective again one would expect a different development between the treated MPB Berlin and Hamburg group after the annulment of the MD as in Hamburg this push did not occur. As can be seen in the post placebo test this is not the case. After the annulment of the MD policy both groups Hamburg and Berlin controlled by the MPB behaved similarly again. This supports the plausibility of the results.

[11] The control group of Hamburg for the MD policy is the treatment group of the MPB policy in Hamburg.

Table 5 The Effects of the MD on Rents in Berlin in quarters

VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3	(4) Placebo 2018q4	(5) Placebo 2021q3
Treatment Group	-0.0817***	-0.0831***	-0.0909***	-0.0883***	-0.168***
	(0.00472)	(0.00475)	(0.00457)	(0.00469)	(0.00600)
Interaction Terms Group x Time					
2021 Q4	-0.0788***	-0.0783***	-0.0843***		0.00796
	(0.00794)	(0.00796)	(0.00771)		(0.00875)
2021 Q3	-0.0868***	-0.0885***	-0.0902***		0
	(0.00762)	(0.00764)	(0.00742)		(0)
2021 Q2	-0.0876***	-0.0896***	-0.0931***		-0.000861
	(0.00750)	(0.00752)	(0.00727)		(0.00837)
2021 Q1	-0.151***	-0.151***	-0.153***		-0.0637***
	(0.00778)	(0.00782)	(0.00757)		(0.00861)
2020 Q4	-0.151***	-0.155***	-0.155***		-0.0642***
	(0.00825)	(0.00828)	(0.00806)		(0.00904)
2020 Q3	-0.150***	-0.152***	-0.149***		-0.0633***
	(0.00801)	(0.00803)	(0.00781)		(0.00882)
2020 Q2	-0.129***	-0.131***	-0.125***		
	(0.00810)	(0.00815)	(0.00792)	(0.00808)	
2020 Q1	-0.0733***	-0.0765***	-0.0744***	-0.0666***	
	(0.00728)	(0.00730)	(0.00707)	(0.00726)	
2019 Q4	-0.0379***	-0.0375***	-0.0398***	-0.0312***	
	(0.00710)	(0.00713)	(0.00685)	(0.00708)	
2019 Q3	-0.0132**	-0.0122*	-0.0145**	-0.00655	
	(0.00668)	(0.00671)	(0.00645)	(0.00666)	
2019 Q2	0	0	0	0.00670	
	(0)	(0)	(0)	(0.00664)	
2019 Q1	0.000328	0.000725	-0.00115	0.00702	
	(0.00661)	(0.00664)	(0.00639)	(0.00659)	
2018 Q4	-0.00670	-0.00432	-0.00848	0	
	(0.00664)	(0.00667)	(0.00638)	0	
2018 Q3	-0.00145	-0.000425	-0.000660	0.00524	
	(0.00659)	(0.00662)	(0.00634)	(0.00658)	
2018 Q2	-0.0184***	-0.0158**	-0.0180***	-0.0117*	
	(0.00651)	(0.00654)	(0.00629)	(0.00649)	
Constant	21.58***	21.46***	20.46***	21.58***	21.63***
	(6.616)	(6.635)	(6.378)	(6.616)	(6.616)
Observations	424,303	419,355	433,534	424,303	424,303
R-squared	0.356	0.357	0.365	0.356	0.356
Furnished flats	yes	no	yes	yes	yes
Control Group	1	1	2	1	1

*Note: Note: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The table shows the effect of the MD policy by quarter. 2019Q2 is the reference point. For the full table including control variables see A. Table 3. Model 1 is the reference Model. Model 2 excludes furnished flats (explaining the reduction in observations) and Model 3 uses a different control group in Hamburg. This control group is broader than the control group in the other models explaining the observation increase.*

Having a control group in a different city is a limitation as city-specific developments cannot be captured. On the other hand, this diminishes the danger that spill-over effects dilute the observed effects. The policy of the MD affected the housing market in Berlin in general. As can be seen in Graph 2 the uncontrolled sector in Berlin saw rent increases, potentially due to the MD policy. To test the validity of the Hamburg control group I look at the common trend and do two placebo tests.

The common trend can be identified. There is no significant effect in the three quarters preceding the reference point. In the quarters before, the common trend is not detectable. This means before 2018Q3 the treatment and control group behaved differently and the housing market in Berlin and Hamburg only recently showed parallel trends.[12] The results are hence less robust compared to hypothesis 1. Nevertheless, a common trend of the three preceding quarters is enough to be mildly confident that without the policy implementation they would have stayed on a similar path. To further test this I conduct two placebo tests.

The placebo tests strengthen the results. The pre-placebo tests pretends that the policy was announced two quarters before the MD was announced so in 2018Q4. The post-placebo test pretends that the policy was implemented two quarters after it was annulled so in 2021Q3. In both cases, no significant result should be detectable after the placebo as the differentiating force between control group and treatment group did not exist yet or ceded to exist. Columns 4 and 5 of Table 5 show the results. No significant effects can be detected for the pre-placebo test two quarters after the placebo treatment. The post placebo test shows no significant results for the quarter before and after the new reference point of 2021Q3. This shows that the control and treatment groups developed the same before and after the MD policy. Therefore, control and treatment group would also have developed the same if the MD policy would not have been implemented.

Additionally, I check if the results of Model 1 are due to the difference in composition of control and treatment group in Hamburg and Berlin. Model 3 uses a different control group and mirrors the treatment group of the MD policy as this group is slightly bigger than the MPB treatment group due to fewer exemptions. No significant difference can be observed between the two

[12] An explanation for the different development before 2018 can be found in the history of the cities. Berlins rent development was not comparable to Hamburg´s or other west German cities rent development as Berlin saw a more drastic increase in rents compared to west German cities. This is due to the low level from which Berlin rents due to the separation and east German regulation started to increase compared to Hamburg. It took some time for Berlin to catch up to west German cities.

models, showing that the difference is not due to two different compiled groups but the existence of the MD policy or not.

A limitation of the analysis of the MD policy is the moment the policy was implemented. At approximately the same time COVID-19 led to lockdowns and major disruptions into the market. This and not the MD might have influenced rents but due to the similarity of Hamburg and Berlin regarding overall city development and Covid-19 regulation, I have the assumption that this effect was similar in both cities. This assumption is supported by the post-placebo test as in 2021 Covid-19 was still a major disruption into the market, but the Berlin and Hamburg treatment and control group showed not differentiation anymore. Using the treatment and control group controls therefore for the overall circumstances and the observed effect can be attributed to the MD policy and not Covid-19 regulation.

Therefore hypothesis 2 can be confirmed. The MD policy led to a rent decrease which survived even its annulment.

3. Regional Heterogeneity of the MPB and MD

The MPB and MD policies affected neighbourhoods differently. After the general effect of the two policies has been established with the previous results it is time to look at the redistribution aspect and which areas benefitted from the MPB and MD policy. Table 6 shows the results for the MPB and Table 7 for the MD Policy. Every three rows a new proxy for socio-economic neighbourhoods starts. The first three rows are for the Local Rent Index (‘LRI’) the next three for household income (‘Income’) and the last three for unemployment. Each of those Tripel is organised in that the first column of each Tripel represents the well-off neighbourhoods the middle the medium and the last column the worse-off neighbourhoods. Meaning that the first, fourth and seventh column are comparable in socio-economic aspects. To see whom the policy benefited the most I then compare for example the effects in columns 3, 6 and 9 (worse-off neighbourhoods) with the effects in columns 1, 4 or 7 (well-off neighbourhoods). If they differ the neighbourhoods were affected differently.

For the MPB, the results show that the worse-off neighbourhoods benefited most from the policy. The indicators are higher and longer significant compared to the other neighbourhoods. For example, in column 4 of Table 6 so in the most affluent districts, the MPB effect was detectable with an effect between 2.2% and 2.9%. In the poorest districts column 6, the effect ranged in the same time frame from 4.2% to 5.5%. An even greater discrepancy can be identified by looking at the most fined grained-indicator: unemployment columns 7,8 and 9. The effect of the MPB is the strongest in neighbourhoods with the highest long term

unemployment rate with around -6% for two and half years (column 9 of Table 6) which is double the effect of the MPB in neighbourhoods with low unemployment rates (column 7 of Table 6) and the established Berlin average of -3%.

Hypothesis 3 regarding the MPB can therefore be accepted. Worse-off instead of better-off neighbourhoods benefited the most from the MPB.

For the MD policy results show a different picture. No differentiation between neighbourhoods can be detected while the MD policy was active so until 2021Q2. The interaction terms between the neighbourhoods differ only slightly and no clear benefiter compared to the others can be identified. The part of hypothesis 3 concerning the MD can therefore be confirmed. The MD affected the different socioeconomic neighbourhoods roughly in the same way. This can be explained through the universal city-wide setting of prices by the MD policy.

Interestingly, this changes completely after the annulment of the MD policy. The effect in good neighbourhoods is halved or even reduced to a third. For example, from -13.5% to -4.2% in neighbourhoods with low unemployment rates (column 6 of Table 7) or from -14.9% to -6.8% in neighbourhoods with a higher income (column 4 of Table 7). In contrast to that in medium and worse-off neighbourhoods, the effect decreased way less and was stable at around -10% (columns 3, 6 and 9 of Table 7). The results for the MPB and post-MD period are important for gentrification, a topic hotly debated in Berlin (Krajewski, 2022). Gentrification is defined as the process of upgrading a neighbourhood which comes then as well with replacement of the previous inhabitants with new more affluent people (Döring & Ulbricht, 2016). Areas where gentrification happens (Döring & Ulbricht, 2016) are concerning my dataset in the middle or worse-off categories. This means without intervention over time they would become richer and more affluent neighbourhoods, replacing the poorer inhabitants. This effect cannot be observed in already good neighbourhoods as the opportunity to upgrade those neighbourhoods even further is limited. If the effect of the MPB and post-MD is stronger in middle and worse-off areas the process of replacement is mitigated and reduced because people from those areas can still find affordable houses nearby and do not have to move to a different district once they want to change flats. Concerning the plausibility of the results, the common trend assumption holds true for the MD policy in the three quarters prior to the policy implementation. In 22 out of 27 quarters no significant effect can be detected see A. Table 5. For the MPB the common trend is less strong for middle and simple LRI neighbourhoods but otherwise observable see A. Table 4.

Hypothesis 3 which posits that the MPB had heterogenous effects on neighbourhoods and that the MD was indifferent can therefore be confirmed.

Table 6 Effects of the MPB on Rents in different Neighbourhoods in Berlin

VARIABLES	(1) LRI good	(2) LRI middle	(3) LRI simple	(4) Income wealthy	(5) Income medium	(6) Income poor	(7) Unemployment low	(8) Unemployment middle	(9) Unemployment high
Treatment Group	-0.101***	-0.0954***	-0.0981***	-0.0981***	-0.114***	-0.116***	-0.0957***	-0.133***	-0.121***
	(0.00944)	(0.00994)	(0.00943)	(0.00543)	(0.00912)	(0.00728)	(0.00508)	(0.00967)	(0.00941)
Interaction Terms									
Group x Time									
2018 H2	0.0420***	0.0336**	-0.0609***	-0.0157**	-0.00377	0.00374	0.00352	-0.0567***	-0.0169
	(0.0136)	(0.0144)	(0.0121)	(0.00779)	(0.0119)	(0.00972)	(0.00738)	(0.0128)	(0.0124)
2018 H1	0.00717	0.00371	-0.0501***	-0.00639	0.000554	-0.0101	-0.00436	-0.00272	-0.0233*
	(0.0132)	(0.0137)	(0.0120)	(0.00755)	(0.0120)	(0.00964)	(0.00714)	(0.0128)	(0.0121)
2017 H2	-0.0105	-0.0587***	-0.0617***	-0.0448***	-0.0364***	-0.0452***	-0.0400***	-0.0305**	-0.0512***
	(0.0136)	(0.0133)	(0.0121)	(0.00753)	(0.0121)	(0.00973)	(0.00713)	(0.0129)	(0.0122)
2017 H1	0.0423***	-0.0480***	-0.0795***	-0.0291***	-0.0167	-0.0426***	-0.0291***	-0.0229*	-0.0800***
	(0.0131)	(0.0132)	(0.0116)	(0.00731)	(0.0116)	(0.00960)	(0.00702)	(0.0126)	(0.0119)
2016 H2	0.0306**	-0.0504***	-0.0916***	-0.0249***	-0.0398***	-0.0548***	-0.0304***	-0.0417***	-0.0691***
	(0.0134)	(0.0134)	(0.0116)	(0.00740)	(0.0119)	(0.00964)	(0.00710)	(0.0126)	(0.0121)
2016 H1	0.0300**	-0.0339***	-0.0662***	-0.0221***	-0.0205*	-0.0442***	-0.0247***	-0.0299**	-0.0378***
	(0.0134)	(0.0129)	(0.0117)	(0.00736)	(0.0115)	(0.00954)	(0.00697)	(0.0127)	(0.0120)
2015 H2	-0.00314	-0.0619***	-0.0636***	-0.0276***	-0.0560***	-0.0403***	-0.0233***	-0.0325**	-0.0648***
	(0.0129)	(0.0137)	(0.0120)	(0.00735)	(0.0121)	(0.00985)	(0.00698)	(0.0130)	(0.0125)
2015 H1	0	0	0	0	0	0	0	0	0
2014 H1	0.0235*	-0.0349***	-0.0123	-0.00481	0.00398	0.0141	-0.000346	0.0314**	0.0124
	(0.0125)	(0.0127)	(0.0112)	(0.00704)	(0.0112)	(0.00906)	(0.00655)	(0.0122)	(0.0113)
Constant	83.68***	64.49***	34.04*	19.62**	20.27*	26.40**	16.29***	46.52	17.93*
	(9.851)	(16.04)	(18.32)	(9.630)	(11.06)	(10.34)	(6.063)	(30.67)	(10.53)
Observations	46,978	66,283	88,538	164,214	111,004	134,582	181,950	77,311	90,246
R-squared	0.431	0.438	0.491	0.419	0.411	0.445	0.422	0.462	0.470

*Note: Note: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The table shows the effect of the MPB divided by neighbourhoods. Reference point is 2015h2. For the full table including control variables see A. Table 4. LRI is the neighbourhood based on the Local Rent Index. The income proxy looks at mean household income on a district level. Unemployment looks at average long-term unemployment rates at a small administrative unit in Berlin the LORs.*

Table 7 Effects of the MD on Rents in different Neighbourhoods in Berlin

VARIABLES	(1) LRI good	(2) LRI middle	(3) LRI simple	(4) Income wealthy	(5) Income medium	(6) Income poor	(7) Unemployment low	(8) Unemployment middle	(9) Unemployment high
Treatment Group	0.0587***	-0.135***	-0.0964***	-0.0290***	-0.157***	-0.0585***	0.00615	-0.143***	-0.161***
	(0.00845)	(0.00710)	(0.00702)	(0.00567)	(0.00609)	(0.00623)	(0.00565)	(0.00692)	(0.00631)
Interaction Terms									
2021 Q4	-0.0116	-0.0372***	-0.0542***	-0.0367***	-0.0950***	-0.0917***	-0.0169*	-0.102***	-0.104***
	(0.0166)	(0.0133)	(0.0119)	(0.0100)	(0.0100)	(0.0108)	(0.0101)	(0.0115)	(0.0108)
2021 Q3	-0.0413***	-0.0199	-0.0986***	-0.0668***	-0.0798***	-0.120***	-0.0521***	-0.0977***	-0.115***
	(0.0156)	(0.0129)	(0.0118)	(0.00966)	(0.00991)	(0.0106)	(0.00978)	(0.0115)	(0.0103)
2021 Q2	-0.0260*	-0.0419***	-0.119***	-0.0678***	-0.0885***	-0.109***	-0.0421***	-0.116***	-0.129***
	(0.0153)	(0.0123)	(0.0113)	(0.00952)	(0.00966)	(0.0106)	(0.00945)	(0.0113)	(0.0101)
2021 Q1	-0.133***	-0.109***	-0.151***	-0.149***	-0.164***	-0.153***	-0.135***	-0.188***	-0.159***
	(0.0174)	(0.0130)	(0.0124)	(0.00995)	(0.0102)	(0.0110)	(0.0102)	(0.0118)	(0.0108)
2020 Q4	-0.0873***	-0.119***	-0.162***	-0.149***	-0.140***	-0.150***	-0.141***	-0.158***	-0.160***
	(0.0171)	(0.0134)	(0.0136)	(0.0107)	(0.0108)	(0.0118)	(0.0110)	(0.0130)	(0.0116)
2020 Q3	-0.0932***	-0.103***	-0.169***	-0.137***	-0.139***	-0.167***	-0.131***	-0.177***	-0.163***
	(0.0173)	(0.0136)	(0.0127)	(0.0104)	(0.0107)	(0.0115)	(0.0106)	(0.0123)	(0.0111)
2020 Q2	-0.132***	-0.0820***	-0.125***	-0.152***	-0.129***	-0.132***	-0.128***	-0.152***	-0.146***
	(0.0165)	(0.0138)	(0.0130)	(0.0102)	(0.0108)	(0.0119)	(0.0104)	(0.0128)	(0.0117)
2020 Q1	-0.105***	-0.0330***	-0.0679***	-0.0962***	-0.0693***	-0.0672***	-0.0846***	-0.0811***	-0.0565***
	(0.0143)	(0.0111)	(0.0111)	(0.00880)	(0.00933)	(0.00997)	(0.00890)	(0.0105)	(0.0101)
2019 Q4	-0.0420***	-0.0340***	-0.0263***	-0.0506***	-0.0359***	-0.0254***	-0.0528***	-0.0536***	-0.00708
	(0.0133)	(0.0105)	(0.0102)	(0.00847)	(0.00887)	(0.00924)	(0.00852)	(0.00994)	(0.00946)
2019 Q3	-0.0118	0.000210	-0.0133	-0.0262***	-0.0111	-0.00455	-0.0133*	-0.0274***	0.00121
	(0.0122)	(0.00993)	(0.00967)	(0.00794)	(0.00838)	(0.00880)	(0.00795)	(0.00949)	(0.00893)
2019 Q2	0	0	0	0	0	0	0	0	0
Constant	27.59*	30.11*	30.28**	19.36**	23.08**	23.48***	15.74***	29.62*	26.79**
	(15.98)	(17.42)	(14.82)	(9.065)	(10.95)	(8.534)	(5.876)	(16.51)	(12.73)
Observations	132,549	150,246	173,088	240,852	193,043	212,534	252,930	160,301	176,355
R-squared	0.200	0.398	0.395	0.319	0.439	0.340	0.290	0.418	0.462

*Note: Note: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The table shows the effect of the MD policy by quarter. 2019Q2 is the reference point. For the full table including control variables see A. Table 5. LRI is the neighbourhood based on the Local Rent Index. The income proxy looks at mean household income on a district level. Unemployment looks at average long-term unemployment rates at a small administrative unit in Berlin the LORs.*

4. Non-Compliance of Landlords

    a) Policy Change 1.1.2019

Comparing the results between 2018h2 and 2019h1 using Model 1 of Table 4 shows indeed a jump from an insignificant result to a slightly negative one at 1.45%. The change is however very subtle and is not greater than the normal fluctuation observed. An inference that huge non-compliance took place prior the policy change is therefore not possible but it can serve as an indicator for future research.

    b) Furnished Flats

That furnished flats were used extensively to circumvent the MPB or MD policy cannot be established. Model 2 of Table 4 and Table 5 exclude all furnished flats from the dataset. If they were substantially used to circumvent the MPB I would expect higher values in Model 2 than in Model 1. The opposite is the case. The values for the MPB in Model 1 of Table 4 are higher compared to those in Model 2 of Table 4. Roughly the same holds true for the MD policy. No great difference can be observed between Model 1 and Model 2 in Table 5 but if the higher rent due to furnishing is not shown in the cold rent but in the utilities, this would not be too surprising as my dataset looks only at the cold rent. Using not the rent but the utilities render the results of Table 8. If utilities were used to circumvent the MPB policy I would expect the utility prices to rise faster in the treatment group, than in the control group and therefore a positive sign. Secondly, I would expect the sign to be smaller once furnished flats are excluded from the dataset.

Both expectations do not hold. The utilities develop the same for the first year between treatment and control group of the MPB and are afterwards even negative.

Regarding Model 1 and 2 of Table 8, the expected effect cannot be observed. On the contrary, the effect between the utilities is even stronger once furnished flats are excluded indicating that in the uncontrolled sector furnishing was more used to increase the rents than in the treated group. That the MPB lead to non-compliance by furnishing can therefore not be concluded. The utilities costs rose both in the treatment and control group over the years but more drastically in the control group, this can be seen in Graph 3 too.

Furthermore Model 1 of Table 8 shows that the MPB policy indeed like set out targeted only the cold rent and left the utilities unaffected as the terms after policy implementation are insignificant.

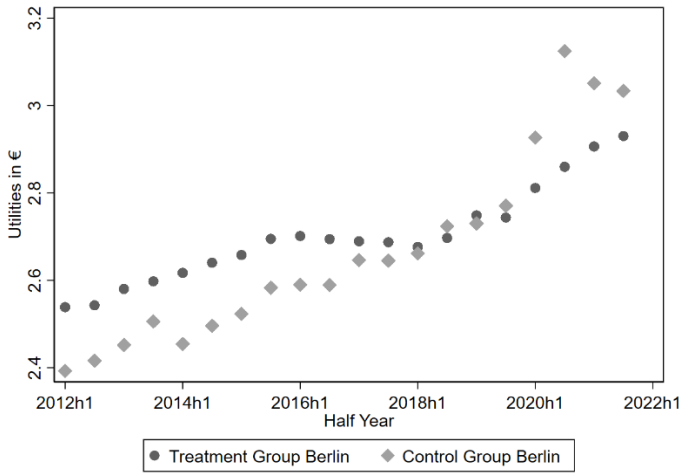
Regarding non-compliance and furnished flats hypothesis 4 can not be confirmed.

Table 8 Effect of the MPB on warm rent. In and excluding furnished flats.

VARIABLES	(1) Model 1	(2) Model 2
Treatment Group	0.0297***	0.0437***
	(0.00619)	(0.00646)
Interaction Terms Group x Time		
2017 H2	-0.0165**	-0.0459***
	(0.00790)	(0.00827)
2017 H1	-0.0253***	-0.0408***
	(0.00774)	(0.00797)
2016 H2	-0.0150*	-0.0352***
	(0.00788)	(0.00828)
2016 H1	-0.000368	-0.00188
	(0.00806)	(0.00827)
2015 H2	-0.00747	-0.00302
	(0.00806)	(0.00860)
2015 H1	0	0
	(0)	(0)
2014 H1	0.00420	0.00741
	(0.00763)	(0.00903)
2013 H1	-0.0140*	-0.0191**
	(0.00830)	(0.00975)
2012 H1	0.00361	-0.00583
	(0.00863)	(0.0110)
Constant	-8.439*	-8.527*
	(4.634)	(4.710)
Observations	354,544	350,735
R-squared	0.150	0.153
Furnishing	yes	no

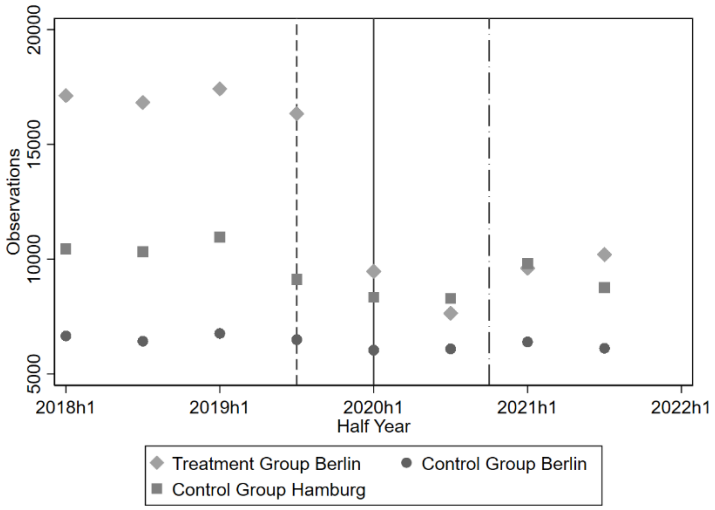
*Note: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The table shows the effect of the MPB on utilities in the MPB treatment group compared to the MPB control group of Berlin by half year. 2015h1 is the reference point. Model 1 includes Model 2 excludes furnished flats from the dataset.*

Graph 3 Utilities per m^2 for MPB control and treatment Group in Berlin



c) Flat Supply

Graph 4 Number of flat observations in Berlin and Hamburg from 2018 until 2022 with the MD Treatment and Control Groups



Note: The dashed line represents the quarter the policy was announced, the solid line the moment the policy was implemented and the dash-dotted line the moment the policy was annulled by the constitutional court.

The MD policy led to a drastic decrease in flat offerings in the treated segment. Graph 4 gives already a good indication of the magnitude of the effects. A small decrease in the supply of flats can be seen in Graph 4 for both control groups in the second half of 2019 and during 2020.

The most drastic decrease can be observed in the treatment group of Berlin. The results of the regressions using the mirror group of Hamburg and the uncontrolled group in Berlin can be seen in Table 9. The supply side of treated flats collapsed. Taking the uncontrolled flats of Berlin (Model 1) as a reference point the supply decreased by 70% while the MD was active compared to around 50% if Hamburg's similar flats are taken as a reference group (Model 2). After the annulment, the coefficient lowers to -40% (Model 1) or -30% (Model 2). This means that flat offerings still below what would have been if the MD policy would not have been enacted.

Arlia, Dolls, Fuest, Gstrein and Krolage (2022) find a value exactly in the middle at -60%.[13] That landlords reacted to rent control policies by shifting the units away from renting has been theorised (Haffner, Elsinga, & Hoekstra, 2008) and has been observed as well in Massachusetts (Sims, 2007) and San Francisco (Diamond, McQuade, & Qian, 2019) although to a smaller degree. This poses a trade-off in rent regulation. Decreased rents benefit those being able to

[13] The genesis of this value is although unclear. It is only mentioned in the abstract. The text body only mentions 70% once but that is a comparison between the moment of the announcement and the moment of evaluation and not between the moment of evaluation and the counterfactual line.

secure contracts. For those unable the market supply worsened. This mirrors an Insider Outsider conflict observed in other areas like labour markets (Rueda, 2006).

Table 9 Effect of the MD on the number of observations in Berlin

VARIABLES	(1) Berlin	(2) Hamburg
Treatment Group	1.003***	0.461***
	(0.00500)	(0.00506)
Interaction Terms Group x Time		
2021 H2	-0.430***	-0.300***
	(0.00798)	(0.00809)
2021 H1	-0.504***	-0.448***
	(0.00774)	(0.00802)
2020 H2	-0.715***	-0.537***
	(0.00850)	(0.00858)
2020 H1	-0.400***	-0.260***
	(0.00882)	(0.00935)
2019 H2	0.0125*	0.141***
	(0.00755)	(0.00785)
2019 H1	0	0
	(0)	(0)
2018 H2	0.0213***	0.0242***
	(0.00726)	(0.00737)
2018 H1	0.00906	0.0274***
	(0.00674)	(0.00720)
2017 H2	-0.0412***	0.00220
	(0.00706)	(0.00737)
2017 H1	0.0576***	0.00937
	(0.00701)	(0.00718)
2016 H2	0.224***	
	(0.00758)	
2016 H1	0.387***	
	(0.00753)	
2015 H2	0.333***	
	(0.00797)	
2015 H1	0.0939***	
	(0.00734)	
Constant	3.675***	4.217***
	(0.00388)	(0.00396)
Observations	456,157	434,253
R-squared	0.572	0.397

*Note: Note: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The table shows the effect of the MD on observations. Column 1 shows the effect of the MD on observations if the unregulated flats of Berlin are used as a control Group and column 2 if similar flats like those treated in Berlin in Hamburg are used as a control group.*

Some of the flats withdrawn from the renting market were converted into property flats and then sold instead of rented out. This number of converted and sold flats was the highest in Berlin in 2020 (Rbb 24, 2021; Investitionsbank Berlin, 2021).

To reduce this flat drain the process of flat conversion needs since August of 2021 approval by public authorities.

Possible other explanations for what has happened to these flats are that they have been kept empty in the hope that the MD policy would be ruled unconstitutionally or that people just moved less leaving fewer flats open (Arlia, Dolls, Fuest, Gstrein, & Krolage, 2022).

There are some limitations to this finding as no clear common trend can be identified beforehand, but the coefficients before the MD policy are small single-digit numbers and therefore from a completely different magnitude compared to the effect seen after the introduction of the MD. A prediction that treatment and control group would have behaved completely similar without the MD is therefore not possible. Still detectable is that the magnitude of development between treatment and control group changed.

The COVID-19 limitation for the MD policy applies to this analysis as well. Moving into Berlin and in Berlin was reduced but the same

holds for Hamburg and the control group in Berlin. So, although the exact percentage of reduction in supply cannot be stated, its number is approximately between the values of Hamburg and Berlin estimates.

Additional limitations come in form of the dataset. Although the dataset is huge, it is only a sample. Due to changes in the scrapping algorithm which is unknown to me the number of observed flats might have changed but this change would have affected the whole market or at least the same type of flats. Comparing therefore the number of observations to those in Berlin and Hamburg leaves room to control for this.

Hypothesis 4 non-compliance in the form of withdrawing from the market altogether can therefore be confirmed. For the other two indicators, a clear statement cannot be issued. There was no or only a small indication that non-compliance took place using furnished flats or pretended high rents before.

VI. Conclusion

Hypothesis 1 and 2 were confirmed. Both policies lowered the rents in Berlin. They were successful in correcting for the market failure of the market power on the landlords' side compared to the tenants´ side. The degree of lowering was different between the MPB with an effect of around 3% and the MD with an effect five times as high at about 15%.

Hypothesis 3 was confirmed for the MD policy and the MPB policy. In the MPB policy, the effect was the strongest in medium or worse-off neighbourhoods. Interestingly, the same was detected after the abolishing of the MD. Regarding the issue of redistribution as legitimisation for market intervention after the welfare state economics theory, the results are therefore mixed. The MPB differentiated between neighbourhoods the MD did not. Only after the annulment heterogeneous effects regarding neighbourhoods could be identified.

Hypothesis 4 concerning the non-compliance due to rational behaviour of the landlords and moral hazard due to information asymmetry showed only regarding the number of flats offered clear results. The landlords withdrew the flats from the renting market completely. Regarding the non-compliance, by using furnished flats no indication of this behaviour could be identified in my dataset. Regarding the noncompliance by pretending to have asked for higher rents previously only a small indicator was observable.

Thus, my research question which effects had the MPB and MD policies on the Berlin rent market can be answered in the following way. The policies lowered the rents. The MPB did so

by 3% the MD by 15%. The MPB policy did differentiate between neighbourhoods the MPB only after its annulment. The MD furthermore led to a drastic decline in offerings.

This study faces some limitations. Those limitations do open possibilities for further research. The differentiation between neighbourhoods in this study can serve only as a broad proxy. A more holistic classification of neighbourhoods is needed. Furthermore, not aggregated but individual data on tenants is needed to answer the question precisely. Data on with whom landlords under the MPB and MD agreed on contracts is necessary to open this pathway. Theory suggests that misallocations could happen (Glaeser & Luttmer, 2003). This is also argued for the MPB and MD (Leuschner, 2014; Artz & Börstinghaus, 2019; Voigtländer, 2017). Data collection would also be needed to solve another limitation of this and similar research. To know the real effect of the MPB the previous rent needs to be known as it poses an exemption. This data is not collected yet. Furthermore, this study concerns mainly the housing market of Berlin. The MPB effects can therefore not necessarily be scaled to Germany as a whole. To establish the overall MPB effect similar studies in other cities are needed.

This research has shown that politicians still struggle to strike the right balance between the effectiveness of lowering rents and pushing landlords out of the market. This trade-off is crucial for the future design of rent control policies. The MD policy created an insider-outsider conflict. For those in a contract, the lowering of rents was beneficial but for the outsiders seeking their way into the market the situation worsened due to a lowered supply of flats.

Additionally, it serves as an example that judicial contested market interventions should be applied cautiously. Judicially it was possible to annul the MD from its beginning, but the market intervention was still observable afterwards as the market has been altered. This market change cannot be undone by a judicial court ruling.

Since the demand for a policy solution is not over as rents are still increasing (Schönball I, 2022), new ideas for market interventions are being discussed[14] the trade-off is important to keep in mind. Either by watering down the effectiveness of a policy so landlords are not withdrawing from the market or circumventing this non-compliance with an additional policy, which prohibits for example the selling or empty keeping of flats.

[14] For example, a tax for landlords with too high rents (Manzel, 2022) or a proposition to limit rent to a maximum of 30% of a person's income (Schönball II, 2022).

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C. Appendix

A. Table 1 Possible rent under the MPB and the MD policy

	Building Year						
	Till 1918	1919-1949	1950-1964	1965-1972	1973-1990 East Berlin	1991-2002	2003-2017[15]
Local Rent Index value for a building year LRI good	7,33€ +10% =8,1€	6,69€ +10% =7,36€	6,52€ +10% =7,17€	6,65€ +10% =7,32€	5,32€ +10% =5,86€	9,09€ +10% =10,00€	9,88€ +10% =10,87€
Price set by the MD policy MD good	6,45€ +0,74€ =7,19€	6,27€ +0,74€ =7,01€	6,08€ +0,74€ =6,82€	5,95€ +0,74€ =6,69€	6,04€ +0,74€ =6,78€	8,13€ +0,74€ =8,87€	9,80€ +0,74€ =10,54€
Difference LRI good and MD good	=-0,91€	=-0,35€	=-0,35€	=-0,63€	=+0,92€	=-1,13€	=-0,33€
LRI middle	6,77€ +10% =7,45€	6,01€ +10% =6,61€	6,00€ +10% =6,60€	5,40€ +10% =5,94€	5,25€ +10% =5,78€	7,90€ +10% =8,69€	9,85€ +10% =10,84€
MD middle	6,45€-0,09 € =6,36€	6,27€ -0,09€ =6,18€	6,08€ -0,09€ =5,99€	5,95€ -0,09€ =5,86€	6,04€ -0,09€ =5,95€	8,13€ -0,09€ =8,04€	9,80€ -0,09€ =9,71€

[15] MD table goes only until 2013 because flats from 2014 onwards are exempt.

Difference LRI middle and MD middle	=-1,09€	=-0,43€	=-0,61€	=-0,08€	=+0,17€	=-0,65€	=-1,13€
LRI simple	6,23€ +10% =6,85€	5,87€ +10% =6,45€	5,58€ +10% =6,14€	5,45€ +10% =6,00€	5,23€ +10% =5,75€	7,72€ +10% =8,49€	11,61€ +10% =12,77€
MD simple	6,45 €-0,28€ =6,17€	6,27€ -0,28€ =5,99€	6,08€ -0,28€ =5,8€	5,95€ -0,28€ =5,67€	6,04€ -0,28€ =5,76€	8,13€ -0,28€ =7,85€	9,80€ -0,28€ =9,52€
Difference CLRI simple MD simple	-0.68€	-0,46€	=-0,34€	=-0,33€	=+0,01€	=-0,64€	=-3,25€

Note: This table shows the cheapest local average rent price for a building year in the LRI and the 10% margin on top. Additionally, the tables portraits the price set out by the MD policy with the applied reduction or plus regarding the area of the local rent index. Lastly the difference between the two is shown.

A. Table 2 The Effects of the MPB on Rents in Berlin over half years. Full Table.

VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3
Year of construction	-0.0211***	-0.0204***	
	(0.00763)	(0.00762)	
Year of construction^2	4.92e-06**	4.74e-06**	
	(1.96e-06)	(1.96e-06)	
Ln living size	-7.62e-05	0.00283*	
	(0.00151)	(0.00152)	
Lift	0.0692***	0.0680***	
	(0.000982)	(0.000985)	
Barrier free	0.0240***	0.0230***	
	(0.00202)	(0.00202)	
In need of rennovation	-0.173***	-0.173***	
	(0.00206)	(0.00207)	
Like new	0.195***	0.210***	
	(0.00234)	(0.00235)	
Recently build	0.264***	0.229***	
	(0.00637)	(0.00646)	
Shower	0.109***	0.108***	
	(0.00109)	(0.00109)	

2021 H2	0.298***	0.278***	0.370***
	(0.00725)	(0.00812)	(0.00712)
2021 H1	0.276***	0.257***	0.352***
	(0.00733)	(0.00811)	(0.00725)
2020 H2	0.234***	0.225***	0.327***
	(0.00713)	(0.00775)	(0.00719)
2020 H1	0.230***	0.216***	0.304***
	(0.00677)	(0.00735)	(0.00682)
2019 H2	0.218***	0.207***	0.270***
	(0.00546)	(0.00601)	(0.00578)
2019 H1	0.192***	0.170***	0.233***
	(0.00530)	(0.00590)	(0.00542)
2018 H2	0.183***	0.162***	0.224***
	(0.00515)	(0.00584)	(0.00540)
2018 H1	0.171***	0.151***	0.201***
	(0.00504)	(0.00568)	(0.00533)
2017 H2	0.165***	0.147***	0.188***
	(0.00507)	(0.00566)	(0.00535)
2017 H1	0.132***	0.116***	0.152***
	(0.00494)	(0.00547)	(0.00525)
2016 H2	0.0982***	0.0829***	0.107***
	(0.00503)	(0.00565)	(0.00537)
2016 H1	0.0645***	0.0635***	0.0670***
	(0.00502)	(0.00550)	(0.00536)
2015 H2	0.0317***	0.0248***	0.0370***
	(0.00510)	(0.00569)	(0.00543)
2014 H2	-0.0281***	-0.0172***	-0.0121**
	(0.00498)	(0.00650)	(0.00540)
2014 H1	-0.0565***	-0.0602***	-0.0597***
	(0.00485)	(0.00647)	(0.00518)
2013 H2	-0.0539***	-0.0694***	-0.0366***
	(0.00616)	(0.00759)	(0.00668)
2013 H1	-0.0911***	-0.0892***	-0.0997***
	(0.00565)	(0.00788)	(0.00604)
2012 H2	-0.131***	-0.133***	-0.127***
	(0.00546)	(0.00765)	(0.00572)
2012 H1	-0.159***	-0.142***	-0.171***
	(0.00572)	(0.00836)	(0.00607)
2011 H2	-0.155***	-0.133***	-0.133***
	(0.0198)	(0.0186)	(0.0223)
Treatment Group	-0.116***	-0.159***	-0.184***
	(0.00431)	(0.00476)	(0.00455)
Interaction Terms Group x Time			
2021 H2	-0.154***	-0.121***	-0.201***
	(0.00821)	(0.00896)	(0.00805)
2021 H1	-0.139***	-0.115***	-0.198***
	(0.00829)	(0.00896)	(0.00817)
2020 H2	-0.143***	-0.130***	-0.218***
	(0.00832)	(0.00881)	(0.00824)
2020 H1	-0.107***	-0.0894***	-0.177***

	(0.00773)	(0.00821)	(0.00771)
2019 H2	-0.0506***	-0.0358***	-0.112***
	(0.00622)	(0.00667)	(0.00652)
2019 H1	-0.0145**	0.0153**	-0.0597***
	(0.00608)	(0.00658)	(0.00621)
2018 H2	0.00239	0.0296***	-0.0476***
	(0.00594)	(0.00649)	(0.00616)
2018 H1	-0.00153	0.0271***	-0.0416***
	(0.00580)	(0.00632)	(0.00605)
2017 H2	-0.0341***	-0.00513	-0.0695***
	(0.00581)	(0.00630)	(0.00606)
2017 H1	-0.0297***	-0.00540	-0.0628***
	(0.00568)	(0.00610)	(0.00594)
2016 H2	-0.0334***	-0.00899	-0.0545***
	(0.00574)	(0.00625)	(0.00605)
2016 H1	-0.0259***	-0.0248***	-0.0415***
	(0.00569)	(0.00608)	(0.00600)
2015 H2	-0.0365***	-0.0244***	-0.0506***
	(0.00577)	(0.00627)	(0.00609)
2015 H1	0	0	0
	(0)	(0)	(0)
2014 H2	0.00802	0.00749	-0.00468
	(0.00565)	(0.00697)	(0.00603)
2014 H1	0.00716	0.0236***	0.0225***
	(0.00546)	(0.00690)	(0.00576)
2013 H2	-0.0119*	0.0122	-0.0155**
	(0.00679)	(0.00809)	(0.00729)
2013 H1	-0.00231	0.00513	0.0215***
	(0.00626)	(0.00830)	(0.00661)
2012 H2	-0.00478	0.00903	0.00308
	(0.00604)	(0.00804)	(0.00628)
2012 H1	-0.00458	-0.0160*	0.0166**
	(0.00627)	(0.00872)	(0.00659)
2011 H2	-0.0174	-0.0417**	-0.0274
	(0.0205)	(0.0192)	(0.0229)
Constant	24.52***	24.02***	2.298***
	(7.423)	(7.416)	(0.00409)
Observations	363,429	359,261	459,733
R-squared	0.419	0.416	0.252
Control variables	yes	yes	no
Furnished flats	yes	no	yes

*Note: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1 The observation rise in column 3 compared to column 1 and 2 can be explained by missing building year variables. The different coefficient between the in and exclusion of control variables is not due to the different sample but due to the other control variables. No great difference in the outcome was observed if they were kept in dataset by including them in the treatment group.*

A. Table 3 The Effects of the MD on Rents in Berlin in quarters. Full Table.

VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3	(4) Placebo 2018q4	(5) Placebo 2021q3
Year of construction	-0.0177***	-0.0176***	-0.0165**	-0.0177***	-0.0177***
	(0.00680)	(0.00682)	(0.00655)	(0.00680)	(0.00680)
Year of construction^2	4.00e-06**	3.96e-06**	3.67e-06**	4.00e-06**	4.00e-06**
	(1.75e-06)	(1.75e-06)	(1.68e-06)	(1.75e-06)	(1.75e-06)
Ln living size	0.00561***	0.00751***	0.00360**	0.00561***	0.00561***
	(0.00151)	(0.00152)	(0.00147)	(0.00151)	(0.00151)
Lift	0.0705***	0.0698***	0.0702***	0.0705***	0.0705***
	(0.000976)	(0.000978)	(0.000959)	(0.000976)	(0.000976)
Barrier free	0.0405***	0.0403***	0.0386***	0.0405***	0.0405***
	(0.00216)	(0.00217)	(0.00215)	(0.00216)	(0.00216)
In need of rennovation	-0.181***	-0.180***	-0.181***	-0.181***	-0.181***
	(0.00200)	(0.00200)	(0.00199)	(0.00200)	(0.00200)
Like new	0.201***	0.201***	0.199***	0.201***	0.201***
	(0.00235)	(0.00237)	(0.00225)	(0.00235)	(0.00235)
Recently build	0.392***	0.393***	0.415***	0.392***	0.392***
	(0.00571)	(0.00572)	(0.00572)	(0.00571)	(0.00571)
Shower	0.108***	0.107***	0.107***	0.108***	0.108***
	(0.000986)	(0.000989)	(0.000957)	(0.000986)	(0.000986)
2021 Q4	0.0387***	0.0371***	0.0441***	0.0395***	-0.0133**
	(0.00582)	(0.00584)	(0.00551)	(0.00588)	(0.00604)
2021 Q3	0.0520***	0.0499***	0.0553***	0.0528***	0
	(0.00548)	(0.00551)	(0.00521)	(0.00555)	0
2021 Q2	0.0632***	0.0616***	0.0686***	0.0641***	0.0112**
	(0.00539)	(0.00541)	(0.00506)	(0.00545)	(0.00563)
2021 Q1	0.0548***	0.0539***	0.0575***	0.0556***	0.00280
	(0.00544)	(0.00547)	(0.00513)	(0.00551)	(0.00568)
2020 Q4	0.0572***	0.0571***	0.0618***	0.0581***	0.00524
	(0.00565)	(0.00567)	(0.00536)	(0.00571)	(0.00588)
2020 Q3	0.0417***	0.0416***	0.0402***	0.0425***	-0.0103*
	(0.00551)	(0.00551)	(0.00520)	(0.00557)	(0.00574)
2020 Q2	0.0448***	0.0457***	0.0403***	0.0457***	-0.00715
	(0.00551)	(0.00552)	(0.00524)	(0.00557)	(0.00574)
2020 Q1	0.0129**	0.0139**	0.0139***	0.0138**	-0.0391***
	(0.00558)	(0.00560)	(0.00530)	(0.00564)	(0.00581)
2019 Q4	0.00703	0.00712	0.00877	0.00787	-0.0449***
	(0.00569)	(0.00572)	(0.00538)	(0.00575)	(0.00592)
2019 Q3	-0.00371	-0.00432	-0.00252	-0.00287	-0.0557***
	(0.00533)	(0.00536)	(0.00504)	(0.00540)	(0.00558)
2019 Q2	0	0	0	0.000842	-0.0520***
	0	0	0	(0.00530)	(0.00548)
2019 Q1	-0.00815	-0.00792	-0.00672	-0.00731	-0.0601***

	(0.00520)	(0.00522)	(0.00492)	(0.00526)	(0.00545)
2018 Q4	-0.000842	-0.00166	0.000827	0	-0.0528***
	(0.00530)	(0.00533)	(0.00498)	0	(0.00555)
2018 Q3	0.00358	0.00286	0.00273	0.00442	-0.0484***
	(0.00520)	(0.00523)	(0.00487)	(0.00527)	(0.00545)
2018 Q2	0.00828	0.00799	0.00777	0.00912*	-0.0437***
	(0.00510)	(0.00513)	(0.00482)	(0.00517)	(0.00535)
2018 Q1	0.00446	0.00407	0.00350	0.00530	-0.0475***
	(0.00511)	(0.00513)	(0.00482)	(0.00518)	(0.00536)
2017 Q4	-0.0191***	-0.0200***	-0.0102**	-0.0182***	-0.0710***
	(0.00530)	(0.00532)	(0.00498)	(0.00536)	(0.00554)
2017 Q3	-0.0308***	-0.0315***	-0.0261***	-0.0300***	-0.0828***
	(0.00503)	(0.00505)	(0.00477)	(0.00510)	(0.00529)
2017 Q2	-0.0249***	-0.0247***	-0.0183***	-0.0241***	-0.0769***
	(0.00505)	(0.00507)	(0.00476)	(0.00512)	(0.00530)
2017 Q1	-0.0396***	-0.0394***	-0.0333***	-0.0388***	-0.0916***
	(0.00498)	(0.00500)	(0.00472)	(0.00505)	(0.00524)
Treatment Group	-0.0817***	-0.0831***	-0.0909***	-0.0883***	-0.168***
	(0.00472)	(0.00475)	(0.00457)	(0.00469)	(0.00600)
Interaction Terms Group x Time					
2021 Q4	-0.0788***	-0.0783***	-0.0843***	-0.0721***	0.00796
	(0.00794)	(0.00796)	(0.00771)	(0.00792)	(0.00875)
2021 Q3	-0.0868***	-0.0885***	-0.0902***	-0.0801***	0
	(0.00762)	(0.00764)	(0.00742)	(0.00760)	(0)
2021 Q2	-0.0876***	-0.0896***	-0.0931***	-0.0809***	-0.000861
	(0.00750)	(0.00752)	(0.00727)	(0.00749)	(0.00837)
2021 Q1	-0.151***	-0.151***	-0.153***	-0.144***	-0.0637***
	(0.00778)	(0.00782)	(0.00757)	(0.00776)	(0.00861)
2020 Q4	-0.151***	-0.155***	-0.155***	-0.144***	-0.0642***
	(0.00825)	(0.00828)	(0.00806)	(0.00823)	(0.00904)
2020 Q3	-0.150***	-0.152***	-0.149***	-0.143***	-0.0633***
	(0.00801)	(0.00803)	(0.00781)	(0.00800)	(0.00882)
2020 Q2	-0.129***	-0.131***	-0.125***	-0.123***	
	(0.00810)	(0.00815)	(0.00792)	(0.00808)	
2020 Q1	-0.0733***	-0.0765***	-0.0744***	-0.0666***	
	(0.00728)	(0.00730)	(0.00707)	(0.00726)	
2019 Q4	-0.0379***	-0.0375***	-0.0398***	-0.0312***	
	(0.00710)	(0.00713)	(0.00685)	(0.00708)	
2019 Q3	-0.0132**	-0.0122*	-0.0145**	-0.00655	
	(0.00668)	(0.00671)	(0.00645)	(0.00666)	
2019 Q2	0	0	0	0.00670	

	(0)	(0)	(0)	(0.00664)	
2019 Q1	0.000328	0.000725	-0.00115	0.00702	
	(0.00661)	(0.00664)	(0.00639)	(0.00659)	
2018 Q4	-0.00670	-0.00432	-0.00848	0	
	(0.00664)	(0.00667)	(0.00638)	0	
2018 Q3	-0.00145	-0.000425	-0.000660	0.00524	
	(0.00659)	(0.00662)	(0.00634)	(0.00658)	
2018 Q2	-0.0184***	-0.0158**	-0.0180***	-0.0117*	
	(0.00651)	(0.00654)	(0.00629)	(0.00649)	
2018 Q1	-0.0268***	-0.0245***	-0.0259***	-0.0201***	
	(0.00640)	(0.00642)	(0.00617)	(0.00638)	
2017 Q4	-0.0226***	-0.0203***	-0.0316***	-0.0159**	
	(0.00662)	(0.00665)	(0.00636)	(0.00660)	
2017 Q3	-0.0178***	-0.0152**	-0.0226***	-0.0111*	
	(0.00638)	(0.00641)	(0.00618)	(0.00636)	
2017 Q2	-0.0437***	-0.0421***	-0.0503***	-0.0370***	
	(0.00643)	(0.00646)	(0.00620)	(0.00640)	
2017 Q1	-0.0451***	-0.0436***	-0.0514***	-0.0384***	
	(0.00628)	(0.00631)	(0.00608)	(0.00626)	
Constant	21.58***	21.46***	20.46***	21.58***	21.63***
	(6.616)	(6.635)	(6.378)	(6.616)	(6.616)
Observations	424,303	419,355	433,534	424,303	424,303
R-squared	0.356	0.357	0.365	0.356	0.356
Control variables	yes	yes	yes	yes	yes
Furnished flats	yes	no	yes	yes	yes
TG	1	2	3	1	1

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

A. Table 4 Effects of the MPB on Rents in different Neighbourhoods in Berlin. Full Table.

VARIABLES	(1) LRI good	(2) LRI middle	(3) LRI simple	(4) Income wealthy	(5) Income medium	(6) Income poor	(7) Unemployment low	(8) Unemployment middle	(9) Unemployment high
Year of construction	-0.0836***	-0.0617***	-0.0300	-0.0166*	-0.0161	-0.0233**	-0.0133**	-0.0433	-0.0134
	(0.0101)	(0.0164)	(0.0188)	(0.00989)	(0.0113)	(0.0106)	(0.00623)	(0.0315)	(0.0108)
Year of construction^2	2.14e-05***	1.53e-05***	7.11e-06	3.90e-06	3.55e-06	5.59e-06**	3.10e-06*	1.06e-05	2.90e-06
	(2.59e-06)	(4.22e-06)	(4.85e-06)	(2.54e-06)	(2.91e-06)	(2.74e-06)	(1.60e-06)	(8.09e-06)	(2.78e-06)
Ln living size	0.0373***	-0.0703***	-0.0901***	0.0243***	-0.0496***	-0.0338***	0.0216***	-0.0700***	-0.162***
	(0.00295)	(0.00363)	(0.00329)	(0.00185)	(0.00273)	(0.00275)	(0.00162)	(0.00539)	(0.00269)
Lift	0.0945***	0.0637***	0.0338***	0.0989***	0.0322***	0.0689***	0.114***	0.0357***	0.00774***
	(0.00225)	(0.00201)	(0.00205)	(0.00135)	(0.00160)	(0.00194)	(0.00130)	(0.00259)	(0.00174)
Barrier free	0.0181***	0.0229***	0.00660	-0.00515*	0.0415***	0.0380***	0.0116***	0.0249***	0.0198***
	(0.00453)	(0.00430)	(0.00412)	(0.00267)	(0.00367)	(0.00344)	(0.00253)	(0.00444)	(0.00398)
In need of rennovation	-0.189***	-0.130***	-0.154***	-0.188***	-0.127***	-0.189***	-0.202***	-0.140***	-0.121***
	(0.00694)	(0.00470)	(0.00364)	(0.00314)	(0.00296)	(0.00345)	(0.00331)	(0.00349)	(0.00376)
Like new	0.143***	0.135***	0.196***	0.112***	0.232***	0.251***	0.154***	0.166***	0.136***
	(0.00429)	(0.00475)	(0.00555)	(0.00269)	(0.00524)	(0.00416)	(0.00241)	(0.00599)	(0.00616)
Recently build	0.151***	0.219***	0.365***	0.189***	0.241***	0.312***	0.196***	0.212***	0.288***
	(0.00858)	(0.0119)	(0.0169)	(0.00786)	(0.00977)	(0.00985)	(0.00525)	(0.0258)	(0.0122)
Shower	0.0726***	0.104***	0.0626***	0.0936***	0.100***	0.116***	0.0877***	0.0896***	0.0629***
	(0.00235)	(0.00257)	(0.00223)	(0.00137)	(0.00199)	(0.00191)	(0.00127)	(0.00300)	(0.00211)
2021 H2	0.323***	0.299***	0.352***	0.376***	0.226***	0.266***	0.365***	0.236***	0.278***
	(0.0173)	(0.0165)	(0.0153)	(0.00888)	(0.0136)	(0.0125)	(0.00818)	(0.0160)	(0.0163)
2021 H1	0.259***	0.240***	0.308***	0.333***	0.198***	0.300***	0.322***	0.247***	0.206***
	(0.0166)	(0.0160)	(0.0179)	(0.00928)	(0.0142)	(0.0119)	(0.00832)	(0.0162)	(0.0176)
2020 H2	0.239***	0.208***	0.307***	0.300***	0.176***	0.224***	0.278***	0.209***	0.251***
	(0.0177)	(0.0171)	(0.0149)	(0.00947)	(0.0136)	(0.0112)	(0.00874)	(0.0150)	(0.0165)

2020 H1	0.229***	0.247***	0.262***	0.263***	0.214***	0.224***	0.254***	0.237***	0.233***
	(0.0147)	(0.0162)	(0.0156)	(0.00840)	(0.0140)	(0.0110)	(0.00781)	(0.0150)	(0.0167)
2019 H2	0.227***	0.171***	0.256***	0.236***	0.212***	0.225***	0.222***	0.243***	0.247***
	(0.0118)	(0.0128)	(0.0113)	(0.00729)	(0.0110)	(0.00859)	(0.00667)	(0.0123)	(0.0118)
2019 H1	0.221***	0.174***	0.284***	0.228***	0.186***	0.193***	0.199***	0.216***	0.223***
	(0.0117)	(0.0121)	(0.0109)	(0.00692)	(0.0108)	(0.00833)	(0.00651)	(0.0116)	(0.0110)
2018 H2	0.153***	0.162***	0.268***	0.200***	0.192***	0.186***	0.178***	0.240***	0.223***
	(0.0111)	(0.0131)	(0.0103)	(0.00667)	(0.0109)	(0.00816)	(0.00614)	(0.0116)	(0.0112)
2018 H1	0.150***	0.187***	0.237***	0.172***	0.186***	0.171***	0.162***	0.189***	0.215***
	(0.0109)	(0.0123)	(0.0103)	(0.00648)	(0.0111)	(0.00810)	(0.00593)	(0.0116)	(0.0109)
2017 H2	0.128***	0.207***	0.213***	0.167***	0.186***	0.166***	0.155***	0.174***	0.213***
	(0.0113)	(0.0120)	(0.0105)	(0.00648)	(0.0112)	(0.00826)	(0.00598)	(0.0117)	(0.0111)
2017 H1	0.0737***	0.170***	0.200***	0.129***	0.146***	0.130***	0.120***	0.132***	0.204***
	(0.0108)	(0.0120)	(0.00997)	(0.00626)	(0.0106)	(0.00809)	(0.00588)	(0.0113)	(0.0108)
2016 H2	0.0346***	0.132***	0.160***	0.0891***	0.121***	0.105***	0.0872***	0.107***	0.152***
	(0.0113)	(0.0122)	(0.0100)	(0.00638)	(0.0110)	(0.00819)	(0.00599)	(0.0115)	(0.0110)
2016 H1	0.00883	0.0880***	0.114***	0.0638***	0.0683***	0.0702***	0.0559***	0.0663***	0.106***
	(0.0113)	(0.0117)	(0.0102)	(0.00639)	(0.0107)	(0.00822)	(0.00592)	(0.0117)	(0.0110)
2015 H2	0.0125	0.0770***	0.0592***	0.0316***	0.0600***	0.0235***	0.0186***	0.0353***	0.0680***
	(0.0108)	(0.0126)	(0.0105)	(0.00638)	(0.0113)	(0.00851)	(0.00592)	(0.0120)	(0.0115)
2014 H2	-0.0248**	0.0163	-0.0141	-0.0181***	-0.0127	-0.0314***	-0.0417***	-0.0367***	0.0137
	(0.0108)	(0.0124)	(0.0103)	(0.00634)	(0.0106)	(0.00804)	(0.00574)	(0.0122)	(0.0114)
2014 H1	-0.0613***	-0.00796	-0.0532***	-0.0396***	-0.0482***	-0.0787***	-0.0480***	-0.0816***	-0.0637***
	(0.0107)	(0.0117)	(0.00977)	(0.00621)	(0.0104)	(0.00786)	(0.00564)	(0.0113)	(0.0104)
2013 H2	-0.0260**	-0.0204	-0.0624***	-0.0389***	-0.0637***	-0.0691***	-0.0350***	-0.126***	-0.0871***
	(0.0120)	(0.0137)	(0.0127)	(0.00779)	(0.0131)	(0.0100)	(0.00678)	(0.0162)	(0.0130)
2013 H1	-0.0670***	-0.0951***	-0.0921***	-0.0567***	-0.116***	-0.123***	-0.0605***	-0.153***	-0.136***
	(0.0117)	(0.0136)	(0.0110)	(0.00705)	(0.0116)	(0.00905)	(0.00635)	(0.0140)	(0.0113)
2012 H2	-0.117***	-0.108***	-0.123***	-0.109***	-0.125***	-0.153***	-0.116***	-0.147***	-0.157***
	(0.0132)	(0.0133)	(0.0108)	(0.00694)	(0.0115)	(0.00902)	(0.00642)	(0.0128)	(0.0111)
2012 H1	-0.156***	-0.160***	-0.170***	-0.132***	-0.149***	-0.201***	-0.139***	-0.193***	-0.186***
	(0.0120)	(0.0152)	(0.0112)	(0.00733)	(0.0119)	(0.00941)	(0.00667)	(0.0137)	(0.0117)

2011 H2	-0.129***	-0.253***	-0.183***	-0.0883***	-0.168***	-0.220***	-0.127***	-0.140**	-0.248***
	(0.0317)	(0.0480)	(0.0303)	(0.0238)	(0.0477)	(0.0272)	(0.0216)	(0.0553)	(0.0381)
Treatment Group	-0.101***	-0.0954***	-0.0981***	-0.0981***	-0.114***	-0.116***	-0.0957***	-0.133***	-0.121***
	(0.00944)	(0.00994)	(0.00943)	(0.00543)	(0.00912)	(0.00728)	(0.00508)	(0.00967)	(0.00941)
Interaction Terms									
Group x Time									
2021 H2	-0.111***	-0.0943***	-0.180***	-0.190***	-0.0937***	-0.141***	-0.170***	-0.113***	-0.137***
	(0.0203)	(0.0186)	(0.0171)	(0.0105)	(0.0148)	(0.0142)	(0.0100)	(0.0174)	(0.0175)
2021 H1	-0.0577***	-0.0515***	-0.167***	-0.172***	-0.0808***	-0.170***	-0.147***	-0.144***	-0.0739***
	(0.0202)	(0.0180)	(0.0193)	(0.0109)	(0.0154)	(0.0138)	(0.0102)	(0.0177)	(0.0188)
2020 H2	-0.0669***	-0.0642***	-0.219***	-0.190***	-0.0793***	-0.134***	-0.167***	-0.136***	-0.153***
	(0.0210)	(0.0193)	(0.0172)	(0.0114)	(0.0151)	(0.0136)	(0.0109)	(0.0169)	(0.0180)
2020 H1	-0.0938***	-0.0777***	-0.118***	-0.146***	-0.0971***	-0.0985***	-0.128***	-0.134***	-0.0889***
	(0.0179)	(0.0179)	(0.0174)	(0.00992)	(0.0152)	(0.0129)	(0.00953)	(0.0164)	(0.0180)
2019 H2	-0.0476***	0.0106	-0.0683***	-0.0710***	-0.0502***	-0.0444***	-0.0568***	-0.0928***	-0.0385***
	(0.0145)	(0.0141)	(0.0129)	(0.00842)	(0.0120)	(0.0102)	(0.00794)	(0.0134)	(0.0130)
2019 H1	-0.0126	0.00272	-0.0806***	-0.0476***	-0.00851	-0.0121	-0.0210***	-0.0360***	-0.0245**
	(0.0141)	(0.0135)	(0.0127)	(0.00806)	(0.0119)	(0.00994)	(0.00776)	(0.0129)	(0.0122)
2018 H2	0.0420***	0.0336**	-0.0609***	-0.0157**	-0.00377	0.00374	0.00352	-0.0567***	-0.0169
	(0.0136)	(0.0144)	(0.0121)	(0.00779)	(0.0119)	(0.00972)	(0.00738)	(0.0128)	(0.0124)
2018 H1	0.00717	0.00371	-0.0501***	-0.00639	0.000554	-0.0101	-0.00436	-0.00272	-0.0233*
	(0.0132)	(0.0137)	(0.0120)	(0.00755)	(0.0120)	(0.00964)	(0.00714)	(0.0128)	(0.0121)
2017 H2	-0.0105	-0.0587***	-0.0617***	-0.0448***	-0.0364***	-0.0452***	-0.0400***	-0.0305**	-0.0512***
	(0.0136)	(0.0133)	(0.0121)	(0.00753)	(0.0121)	(0.00973)	(0.00713)	(0.0129)	(0.0122)
2017 H1	0.0423***	-0.0480***	-0.0795***	-0.0291***	-0.0167	-0.0426***	-0.0291***	-0.0229*	-0.0800***
	(0.0131)	(0.0132)	(0.0116)	(0.00731)	(0.0116)	(0.00960)	(0.00702)	(0.0126)	(0.0119)
2016 H2	0.0306**	-0.0504***	-0.0916***	-0.0249***	-0.0398***	-0.0548***	-0.0304***	-0.0417***	-0.0691***
	(0.0134)	(0.0134)	(0.0116)	(0.00740)	(0.0119)	(0.00964)	(0.00710)	(0.0126)	(0.0121)
2016 H1	0.0300**	-0.0339***	-0.0662***	-0.0221***	-0.0205*	-0.0442***	-0.0247***	-0.0299**	-0.0378***
	(0.0134)	(0.0129)	(0.0117)	(0.00736)	(0.0115)	(0.00954)	(0.00697)	(0.0127)	(0.0120)

2015 H2	-0.00314	-0.0619***	-0.0636***	-0.0276***	-0.0560***	-0.0403***	-0.0233***	-0.0325**	-0.0648***
	(0.0129)	(0.0137)	(0.0120)	(0.00735)	(0.0121)	(0.00985)	(0.00698)	(0.0130)	(0.0125)
2015 H1	0	0	0	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)
2014 H2	0.0104	-0.0376***	-0.0201*	0.00437	-0.00793	-0.00346	0.0236***	0.00880	-0.0332***
	(0.0127)	(0.0135)	(0.0117)	(0.00728)	(0.0114)	(0.00934)	(0.00675)	(0.0132)	(0.0123)
2014 H1	0.0235*	-0.0349***	-0.0123	-0.00481	0.00398	0.0141	-0.000346	0.0314**	0.0124
	(0.0125)	(0.0127)	(0.0112)	(0.00704)	(0.0112)	(0.00906)	(0.00655)	(0.0122)	(0.0113)
2013 H2	-0.0163	-0.0480***	-0.0169	-0.0152*	-0.00725	-0.00738	-0.0157**	0.0559***	0.00808
	(0.0140)	(0.0149)	(0.0140)	(0.00866)	(0.0139)	(0.0112)	(0.00775)	(0.0171)	(0.0140)
2013 H1	0.00238	0.01000	-0.0252**	-0.0180**	0.0268**	0.00113	-0.0160**	0.0485***	0.0250**
	(0.0136)	(0.0147)	(0.0123)	(0.00792)	(0.0125)	(0.0102)	(0.00729)	(0.0150)	(0.0122)
2012 H2	0.0125	-0.0239*	-0.0443***	-0.0105	-0.00510	-0.0102	-0.00880	0.00841	0.0108
	(0.0148)	(0.0143)	(0.0120)	(0.00774)	(0.0123)	(0.0101)	(0.00730)	(0.0137)	(0.0120)
2012 H1	0.0471***	-0.0139	-0.0269**	-0.00727	-0.0131	0.00557	-0.00270	0.0188	0.00148
	(0.0137)	(0.0161)	(0.0124)	(0.00807)	(0.0126)	(0.0105)	(0.00750)	(0.0145)	(0.0125)
2011 H2	0.0435	0.0685	-0.0266	-0.0410*	-0.0317	0.0338	0.0148	-0.0598	0.0436
	(0.0338)	(0.0493)	(0.0315)	(0.0247)	(0.0483)	(0.0287)	(0.0226)	(0.0559)	(0.0388)
Constant	83.68***	64.49***	34.04*	19.62**	20.27*	26.40**	16.29***	46.52	17.93*
	(9.851)	(16.04)	(18.32)	(9.630)	(11.06)	(10.34)	(6.063)	(30.67)	(10.53)
Observations	46,978	66,283	88,538	164,214	111,004	134,582	181,950	77,311	90,246
R-squared	0.431	0.438	0.491	0.419	0.411	0.445	0.422	0.462	0.470

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

A. Table 5 Effects of the MD on Rents in different Neighbourhoods in Berlin. Full Table.

VARIABLES	(1) LRI good	(2) LRI middle	(3) LRI simple	(4) Income wealthy	(5) Income medium	(6) Income poor	(7) Unemployment low	(8) Unemployment middle	(9) Unemployment high
Year of construction	-0.0250	-0.0266	-0.0264*	-0.0160*	-0.0192*	-0.0199**	-0.0124**	-0.0261	-0.0229*
	(0.0164)	(0.0179)	(0.0152)	(0.00930)	(0.0112)	(0.00877)	(0.00604)	(0.0169)	(0.0131)
Year of construction^2	6.14e-06	6.36e-06	6.22e-06	3.70e-06	4.39e-06	4.67e-06**	2.80e-06*	6.22e-06	5.36e-06
	(4.21e-06)	(4.58e-06)	(3.91e-06)	(2.39e-06)	(2.88e-06)	(2.26e-06)	(1.55e-06)	(4.35e-06)	(3.35e-06)
Ln living size	0.0246***	-0.0142***	-0.0304***	0.0262***	-0.0138***	-0.0118***	0.0235***	-0.0151***	-0.0555***
	(0.00373)	(0.00414)	(0.00342)	(0.00198)	(0.00276)	(0.00246)	(0.00160)	(0.00408)	(0.00299)
Lift	0.0877***	0.0720***	0.0522***	0.0957***	0.0495***	0.0725***	0.103***	0.0585***	0.0401***
	(0.00253)	(0.00221)	(0.00202)	(0.00137)	(0.00159)	(0.00164)	(0.00127)	(0.00219)	(0.00170)
Barrier free	0.0214***	0.0400***	0.0304***	0.00894***	0.0564***	0.0358***	0.0222***	0.0420***	0.0356***
	(0.00399)	(0.00401)	(0.00373)	(0.00281)	(0.00340)	(0.00331)	(0.00271)	(0.00396)	(0.00355)
In need of rennovation	-0.149***	-0.128***	-0.164***	-0.185***	-0.137***	-0.189***	-0.195***	-0.147***	-0.133***
	(0.00554)	(0.00412)	(0.00350)	(0.00298)	(0.00285)	(0.00320)	(0.00313)	(0.00304)	(0.00352)
Like new	0.170***	0.173***	0.201***	0.144***	0.215***	0.234***	0.169***	0.189***	0.193***
	(0.00444)	(0.00484)	(0.00480)	(0.00273)	(0.00389)	(0.00360)	(0.00236)	(0.00498)	(0.00452)
Recently build	0.253***	0.279***	0.381***	0.283***	0.327***	0.396***	0.282***	0.260***	0.273***
	(0.0106)	(0.0117)	(0.0123)	(0.00692)	(0.0101)	(0.00811)	(0.00526)	(0.0128)	(0.0128)
Shower	0.0862***	0.0949***	0.0811***	0.0959***	0.0933***	0.104***	0.0919***	0.0899***	0.0840***
	(0.00177)	(0.00191)	(0.00175)	(0.00117)	(0.00150)	(0.00146)	(0.00108)	(0.00190)	(0.00171)
2021 Q4	0.0388***	0.0374***	0.0370***	0.0388***	0.0377***	0.0380***	0.0385***	0.0375***	0.0356***
	(0.00581)	(0.00581)	(0.00584)	(0.00582)	(0.00583)	(0.00581)	(0.00582)	(0.00581)	(0.00587)
2021 Q3	0.0539***	0.0507***	0.0500***	0.0529***	0.0509***	0.0523***	0.0534***	0.0508***	0.0483***
	(0.00546)	(0.00546)	(0.00550)	(0.00546)	(0.00549)	(0.00548)	(0.00547)	(0.00547)	(0.00552)
2021 Q2	0.0651***	0.0620***	0.0617***	0.0643***	0.0623***	0.0632***	0.0646***	0.0623***	0.0599***

	(0.00537)	(0.00537)	(0.00539)	(0.00537)	(0.00539)	(0.00538)	(0.00537)	(0.00537)	(0.00542)
2021 Q1	0.0562***	0.0542***	0.0542***	0.0554***	0.0545***	0.0548***	0.0554***	0.0545***	0.0532***
	(0.00544)	(0.00543)	(0.00545)	(0.00543)	(0.00545)	(0.00544)	(0.00544)	(0.00543)	(0.00547)
2020 Q4	0.0575***	0.0567***	0.0574***	0.0570***	0.0573***	0.0576***	0.0572***	0.0570***	0.0568***
	(0.00562)	(0.00562)	(0.00566)	(0.00563)	(0.00565)	(0.00564)	(0.00563)	(0.00563)	(0.00569)
2020 Q3	0.0420***	0.0415***	0.0421***	0.0416***	0.0419***	0.0421***	0.0420***	0.0417***	0.0417***
	(0.00548)	(0.00549)	(0.00553)	(0.00548)	(0.00552)	(0.00550)	(0.00548)	(0.00550)	(0.00556)
2020 Q2	0.0451***	0.0449***	0.0459***	0.0446***	0.0455***	0.0453***	0.0447***	0.0453***	0.0459***
	(0.00547)	(0.00548)	(0.00553)	(0.00548)	(0.00552)	(0.00550)	(0.00548)	(0.00549)	(0.00556)
2020 Q1	0.0137**	0.0125**	0.0121**	0.0128**	0.0127**	0.0135**	0.0130**	0.0125**	0.0117**
	(0.00557)	(0.00557)	(0.00560)	(0.00557)	(0.00559)	(0.00558)	(0.00558)	(0.00557)	(0.00562)
2019 Q4	0.00660	0.00661	0.00690	0.00664	0.00690	0.00711	0.00667	0.00672	0.00660
	(0.00569)	(0.00568)	(0.00571)	(0.00569)	(0.00570)	(0.00569)	(0.00570)	(0.00569)	(0.00574)
2019 Q3	-0.00501	-0.00410	-0.00379	-0.00458	-0.00374	-0.00412	-0.00476	-0.00410	-0.00407
	(0.00532)	(0.00532)	(0.00535)	(0.00532)	(0.00535)	(0.00533)	(0.00533)	(0.00532)	(0.00538)
2019 Q2									
2019 Q1	-0.00953*	-0.00862*	-0.00823	-0.00868*	-0.00836	-0.00899*	-0.00903*	-0.00866*	-0.00879*
	(0.00519)	(0.00518)	(0.00522)	(0.00519)	(0.00521)	(0.00520)	(0.00519)	(0.00519)	(0.00525)
2018 Q4	-0.00206	-0.00132	-0.000857	-0.000979	-0.00116	-0.00192	-0.00162	-0.00133	-0.00142
	(0.00528)	(0.00528)	(0.00531)	(0.00529)	(0.00531)	(0.00529)	(0.00529)	(0.00528)	(0.00534)
2018 Q3	0.00431	0.00337	0.00292	0.00456	0.00326	0.00344	0.00469	0.00318	0.00280
	(0.00511)	(0.00512)	(0.00519)	(0.00513)	(0.00517)	(0.00519)	(0.00514)	(0.00513)	(0.00521)
2018 Q2	0.00905*	0.00866*	0.00898*	0.00881*	0.00871*	0.00836	0.00889*	0.00874*	0.00892*
	(0.00507)	(0.00508)	(0.00512)	(0.00507)	(0.00511)	(0.00509)	(0.00507)	(0.00509)	(0.00515)
2018 Q1	0.00359	0.00395	0.00453	0.00386	0.00453	0.00407	0.00387	0.00415	0.00396
	(0.00508)	(0.00509)	(0.00513)	(0.00508)	(0.00512)	(0.00510)	(0.00508)	(0.00510)	(0.00516)
2017 Q4	-	-	-	-0.0172***	-0.0173***	-	-0.0174***	-0.0162***	-0.0158***
	0.0173***	0.0168***	0.0178***			0.0196***			
	(0.00527)	(0.00527)	(0.00530)	(0.00527)	(0.00529)	(0.00530)	(0.00528)	(0.00527)	(0.00533)
2017 Q3	-	-	-	-0.0300***	-0.0295***	-	-0.0303***	-0.0288***	-0.0286***
	0.0301***	0.0293***	0.0302***			0.0314***			

	(0.00501)	(0.00501)	(0.00505)	(0.00500)	(0.00504)	(0.00503)	(0.00500)	(0.00502)	(0.00508)
2017 Q2	-	-	-	-0.0233***	-0.0227***	-	-0.0232***	-0.0216***	-0.0210***
	0.0225***	0.0222***	0.0239***			0.0246***			
	(0.00502)	(0.00502)	(0.00506)	(0.00501)	(0.00505)	(0.00505)	(0.00502)	(0.00503)	(0.00509)
2017 Q1	-	-	-	-0.0376***	-0.0380***	-	-0.0376***	-0.0368***	-0.0370***
	0.0371***	0.0373***	0.0391***			0.0400***			
	(0.00495)	(0.00496)	(0.00500)	(0.00495)	(0.00499)	(0.00498)	(0.00496)	(0.00496)	(0.00503)
Treatment	0.0587***	-0.135***	-	-0.0290***	-0.157***	-	0.00615	-0.143***	-0.161***
Group			0.0964***			0.0585***			
	(0.00845)	(0.00710)	(0.00702)	(0.00567)	(0.00609)	(0.00623)	(0.00565)	(0.00692)	(0.00631)
Interaction Terms									
Group x Time									
2021 Q4	-0.0116	-	-	-0.0367***	-0.0950***	-	-0.0169*	-0.102***	-0.104***
		0.0372***	0.0542***			0.0917***			
	(0.0166)	(0.0133)	(0.0119)	(0.0100)	(0.0100)	(0.0108)	(0.0101)	(0.0115)	(0.0108)
2021 Q3	-	-0.0199	-	-0.0668***	-0.0798***	-0.120***	-0.0521***	-0.0977***	-0.115***
	0.0413***		0.0986***						
	(0.0156)	(0.0129)	(0.0118)	(0.00966)	(0.00991)	(0.0106)	(0.00978)	(0.0115)	(0.0103)
2021 Q2	-0.0260*	-	-0.119***	-0.0678***	-0.0885***	-0.109***	-0.0421***	-0.116***	-0.129***
		0.0419***							
	(0.0153)	(0.0123)	(0.0113)	(0.00952)	(0.00966)	(0.0106)	(0.00945)	(0.0113)	(0.0101)
2021 Q1	-0.133***	-0.109***	-0.151***	-0.149***	-0.164***	-0.153***	-0.135***	-0.188***	-0.159***
	(0.0174)	(0.0130)	(0.0124)	(0.00995)	(0.0102)	(0.0110)	(0.0102)	(0.0118)	(0.0108)
2020 Q4	-	-0.119***	-0.162***	-0.149***	-0.140***	-0.150***	-0.141***	-0.158***	-0.160***
	0.0873***								
	(0.0171)	(0.0134)	(0.0136)	(0.0107)	(0.0108)	(0.0118)	(0.0110)	(0.0130)	(0.0116)
2020 Q3	-	-0.103***	-0.169***	-0.137***	-0.139***	-0.167***	-0.131***	-0.177***	-0.163***
	0.0932***								
	(0.0173)	(0.0136)	(0.0127)	(0.0104)	(0.0107)	(0.0115)	(0.0106)	(0.0123)	(0.0111)
2020 Q2	-0.132***	-	-0.125***	-0.152***	-0.129***	-0.132***	-0.128***	-0.152***	-0.146***
		0.0820***							

	(0.0165)	(0.0138)	(0.0130)	(0.0102)	(0.0108)	(0.0119)	(0.0104)	(0.0128)	(0.0117)
2020 Q1	-0.105***	-0.0330***	-0.0679***	-0.0962***	-0.0693***	-0.0672***	-0.0846***	-0.0811***	-0.0565***
	(0.0143)	(0.0111)	(0.0111)	(0.00880)	(0.00933)	(0.00997)	(0.00890)	(0.0105)	(0.0101)
2019 Q4	-0.0420***	-0.0340***	-0.0263***	-0.0506***	-0.0359***	-0.0254***	-0.0528***	-0.0536***	-0.00708
	(0.0133)	(0.0105)	(0.0102)	(0.00847)	(0.00887)	(0.00924)	(0.00852)	(0.00994)	(0.00946)
2019 Q3	-0.0118	0.000210	-0.0133	-0.0262***	-0.0111	-0.00455	-0.0133*	-0.0274***	0.00121
	(0.0122)	(0.00993)	(0.00967)	(0.00794)	(0.00838)	(0.00880)	(0.00795)	(0.00949)	(0.00893)
2019 Q2	0	0	0	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)	(0)
2019 Q1	0.0160	-0.00409	0.00140	-0.00711	0.0110	-0.00558	0.000687	0.00232	-0.0131
	(0.0120)	(0.0101)	(0.00979)	(0.00786)	(0.00856)	(0.00874)	(0.00788)	(0.00948)	(0.00892)
2018 Q4	-0.0139	-0.00878	0.00321	-0.0205***	0.0106	-0.00308	-0.0166**	0.00580	-0.00376
	(0.0117)	(0.0100)	(0.00953)	(0.00783)	(0.00851)	(0.00861)	(0.00783)	(0.00943)	(0.00890)
2018 Q3	-0.0287**	0.00668	-0.00188	-0.0181**	0.00638	0.00251	-0.0141*	0.00226	-0.00504
	(0.0117)	(0.0100)	(0.00966)	(0.00770)	(0.00843)	(0.00869)	(0.00772)	(0.00963)	(0.00889)
2018 Q2	-0.0593***	-0.00556	-0.00963	-0.0384***	0.00380	-0.0254***	-0.0337***	-0.00166	-0.0197**
	(0.0114)	(0.00997)	(0.00961)	(0.00770)	(0.00848)	(0.00862)	(0.00770)	(0.00946)	(0.00882)
2018 Q1	-0.0567***	-0.00937	-0.0363***	-0.0457***	-0.00671	-0.0312***	-0.0462***	-0.00640	-0.0234***
	(0.0112)	(0.00975)	(0.00920)	(0.00746)	(0.00823)	(0.00840)	(0.00748)	(0.00920)	(0.00862)
2017 Q4	-0.0621***	-0.0135	-0.0191**	-0.0480***	-0.000562	-0.0264***	-0.0460***	-0.0153	-0.0137
	(0.0118)	(0.00995)	(0.00944)	(0.00773)	(0.00852)	(0.00863)	(0.00774)	(0.00945)	(0.00887)
2017 Q3	-0.0554***	-0.000816	-0.0209**	-0.0424***	2.35e-05	-0.0250***	-0.0386***	-0.0123	-0.0160*
	(0.0114)	(0.00964)	(0.00932)	(0.00748)	(0.00839)	(0.00847)	(0.00747)	(0.00937)	(0.00872)
2017 Q2	-0.0656***	-0.0394***	-0.0321***	-0.0677***	-0.0170**	-0.0542***	-0.0669***	-0.0406***	-0.0385***
	(0.0113)	(0.00973)	(0.00926)	(0.00752)	(0.00836)	(0.00862)	(0.00755)	(0.00946)	(0.00866)

2017 Q1	-0.0766***	-0.0214**	-0.0495***	-0.0661***	-0.0212***	-0.0574***	-0.0700***	-0.0408***	-0.0394***
	(0.0112)	(0.00951)	(0.00917)	(0.00735)	(0.00811)	(0.00837)	(0.00734)	(0.00909)	(0.00857)
Constant	27.59*	30.11*	30.28**	19.36**	23.08**	23.48***	15.74***	29.62*	26.79**
	(15.98)	(17.42)	(14.82)	(9.065)	(10.95)	(8.534)	(5.876)	(16.51)	(12.73)
Observations	132,549	150,246	173,088	240,852	193,043	212,534	252,930	160,301	176,355
R-squared	0.200	0.398	0.395	0.319	0.439	0.340	0.290	0.418	0.462

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