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Does Regulation Discourage Investors? Sales Price Effects of Rent Controls in Germany*

Abstract

We analyze the extent to which sales prices for residential housing react to rent-price regulation. To this end, we exploit changes in apartment prices across the regulation treatment threshold. We examine a quasi-natural design in the German federal state of Brandenburg using transaction price data provided by the committee of evaluation experts. Brandenburg introduced both a capping limit for existing rental contracts as well as a price ceiling for new contracts for municipalities with tight housing markets in 2014. Whether or not a municipality falls under this classification is based upon a municipality's housing market characteristics, which are translated into a specific score. This allows us to employ a regression discontinuity design with a sharp cutoff point. We compare sales prices in municipalities that are located marginally above the assignment threshold with the prices in those slightly below. Our results suggest that the regulations reduced sales prices for affected apartments by 20–30 %.

JEL Code: D04, R31, R52
Keywords: Housing rent controls, sales prices

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1 Introduction

Recently, the German government empowered its federal states to determine areas that should be subject to stronger rent price regulation. We examine these two regulations for the German state of Brandenburg. We use a regression discontinuity design (RDD) on a dataset with actual transaction prices. We are able to identify the causal effect of rent regulations on the sales prices of rental objects. Dwellings that are subject to the regulations sell for 20–30 % less than they would have in the absence of regulations.

With this paper, we add to the very scarce literature on rent controls in Germany. In comparison to the existing literature, our analysis exhibits some crucial advantages. First, we use a unique micro dataset with official transaction prices as well as apartment characteristics in Brandenburg. Second, we are able to exploit a discontinuous treatment that allows us to employ a sharp RDD analysis. This gives us the benefit of not having to rely on time-series data, as we believe that it is impossible to pinpoint the exact time when expectations were being built. We base our definition of treatment and control groups on an official report for regional housing markets in Brandenburg and measure the treatment effect as the difference in the pooled cross section between these groups. Lastly, we focus on sales prices. Anticipation effects in the form of rent price increases right before the introduction of regulations do not pose a threat to our analysis. The effect that we measure reflects the lowered expectation regarding future rental revenue, which is formed initially during the introduction of the regulations.

State governments can only utilize the regulations in regions with a tight housing market. When determining such regions, the governments need to justify their decision. Therefore, Brandenburg commissioned a report quantifying the degree to which a region's market might be "tight." The contractor translated the housing data of the municipalities into index-score points according to a fixed scheme. Brandenburg implemented the so-called "Kappungsgrenzenverordnung" (loosely translated: "capping limit regulation", henceforth: KGV) in September 2014 in 30 municipalities with scores higher than a specific threshold. In affected municipalities, the highest possible rent increase for contracts with sitting tenants was lowered from 20 % to 15 % within three years. In the exact same municipalities and one additional region, the "Mietpreisbremse" (loosely translated: rent price brake, henceforth: MPB) was introduced in January 2016. The regulation curtails prices of new contracts to a maximum of 10 % above the local rent index. The quantification of a market's tightness that determines whether or not a regulation was implemented, provides us with the opportunity to compare those regions that have only just been selected for regulation with those that have only just not. We employ RD estimates across the regulation treatment threshold to identify the causal price effect of these rent price controls.

These regulations pertain to the so-called second-generation rent controls, which are rather common in industrial countries. The first generation of rent controls imposed strict rent ceilings and has mostly been replaced by more flexible forms. However, scientific discussion agrees on the negative side effects of these first-generation rent controls. They are unanimously regarded as an unsuitable measure to support the provision of adequate housing at affordable costs in the long run. Even second-generation rent controls usually cause undesired side effects. According to Arnott (1995),

> *"There has been widespread agreement that rent controls discourage new construction, cause abandonment, retard maintenance, reduce mobility, generate mismatch between housing units and tenants, exacerbate discrimination in rental housing, create black markets, encourage the conversion of rental to owner-occupied housing, and generally short-circuit the market mechanism for housing."* – *Arnott (1995), p. 99.*

Indeed, empirical studies identify a misallocation of apartments due to rent controls (see Arnott and Igarashi, 2000; Early, 2000; Glaeser and Luttmer, 2003; Skak and Bloze, 2013). They further observe a reduction in building activity (Glaeser and Luttmer, 2003; McFarlane, 2003) as well as maintenance (Olsen, 1988; Moon and Stotsky, 1993; Andersen, 1998; Sims, 2007), and that the effect on prices only partially benefits the main target groups (Linneman, 1987; Ault and Saba, 1990; Glaeser, 2003). In addition, even the short-run effects on rents for regulated apartments are ambiguous. Indeed, rent controls could mitigate price effects that arise from rigid supply in a scenario of drastic demand shocks (Arnott, 1995). Nagy (1997) finds a reduced rent growth for sitting tenants, but higher asking prices for rent-controlled apartments in New York City, compared to uncontrolled ones. Fallis and Smith (1984) and Early (2000) observe that in the Los Angeles area and New York City, prices for uncontrolled objects rise due to rent controls in a neighboring area. In Ontario, Smith (1988) finds rent prices for older housing units to actually be lower due to rent controls, whereas newer apartments are more expensive.

The effects of the MPB and KGV on rents are difficult to determine at the present moment for two reasons: First, the regulations have only been in place for a rather short period of time. Therefore, the long-term effect of interest is overshadowed by anticipation effects. Landlords could have set high prices prior to the regulations without being obligated to reduce prices later. Second, the MPB is barely enforceable in its present design, since, apart from other exceptions from the regulation, landlords do not have to reduce rents when re-letting an apartment. They are also not obligated to share the information on the former rent price with the new tenant. The tenant would have to rent the apartment and then attempt to sue his landlord. This rarely happens in reality.

Nevertheless, the recently introduced rent regulation, the MPB, has already drawn a lot of attention in media as well as in scientific discussion. Deschermeier et al. (2016) show that prior to the introduction on rent control, a high proportion of apartments is potentially affected in Berlin and Cologne. Thomschke and Hein (2015) conclude that at the time of their analysis, the causal relation between regulation and rent price evolution could not yet be identified. However, the authors suggest that the rent control actually curtailed price increases in Berlin. However, these early results are questioned by subsequent observations, where prices did increase (Hein and Thomschke, 2016). In a sophisticated causal analysis for the Berlin rental market, Thomschke (2016) estimates both the average effect on rent prices as well as effects on the rent price distribution. He concludes that a significant rent price effect can only be measured for the upper price segment. The second study focusing on causal effects of the MPB is Kholodilin et al. (2016). The authors find that the MPB does not decelerate price increases; if anything, it rather increases prices for both controlled and uncontrolled apartments. In a recent analysis, Kholodilin et al. (2017) emphasize that the MPB is only effective if the market rent level is sufficiently high compared to the local rent index. They find that in those regions, the MPB is actually effective in curtailing rent prices. In other regions, it is ineffective by design.

The situation is rather different when we look at *sales* prices. Even at the point of announcement, the regulations might have already posed a threat for landlords and investors. With an unknown development of future market rents, rent regulations are likely to affect expected returns negatively and will, therefore, decrease the value of rental objects (see Marks, 1984; Fallis and Smith, 1984; Early and Phelps, 1999). Landlords of apartments in rent-controlled areas presumably expected to be restricted in their price setting in the near future. Therefore, current sales prices should already account for these future restrictions. Sales prices in affected regions should be lower right upon the announcement of the implementation of rent controls. This setting allows a causal analysis even at this early stage of being in place.

For Germany, however, Kholodilin et al. (2016) find no significant effects on sales prices. This is quite surprising, given that intuition clearly suggests that prices are likely to fall. The authors use asking price data and compare neighboring postal-code regions in a difference-in-differences analysis. Their results might be distorted due to diverse price trends of the treatment and control groups. A further distortion could stem from the spread between asking and sales price which depends on market characteristics.[1]

In their new study, Kholodilin et al. (2017) find price drops at different points in time for regulated apartments, combining a difference-in-differences estimation with a discontinuity in time. The authors allow for three different points in time where price expectations could have changed and find two price drops of 2.1 % and 2.7 %. However, it is impossible to pinpoint the exact time when expectations regarding the regulations were being built. We argue that expectations have rather spread monotonously in the time frame between the announcement and the regulation coming into effect. Moreover, sellers might cling to

[1] Han and Strange (2016) show that there are non-trivial shares of sales with transaction prices above, below, and exactly at the list price in the USA. The size of these shares changes with the market condition: in a boom, the portion of below-list sales drops, while at-list and above-list sales are relatively more frequent. Henger and Voigtländer (2014) find similar results for Hamburg in Germany: the average gap between asking and transaction price of 6.7 % changes considerably over time—from around 8 % in 2007 and 2008 to 3.4 % in 2012. The data even indicate above-list sales for the year 2013.

market values that were being calculated prior to the regulations. Therefore, the drop in asking prices might be considerably less severe than for actual transaction prices. For these reasons, we believe our findings of 20–30 % to be perfectly conceivable.

2 Institutional background

As a basic human need, housing has always been on the political agenda and has, therefore, often been subject to regulation. For over half a century, owner occupancy was subsidized in one way or another. The focus on ownership took a turn in 2006, when the biggest nationwide home owner allowance ("Eigenheimzulage") was abolished and state governments were given the power to increase real estate transfer taxes.[2] In the past few years, strong migration to cities spurred increasing rent prices. Policymakers now focus on rent controls in order to provide affordable housing.

2.1 Brandenburg's housing market

Brandenburg is closely related to Berlin. This is reflected in the development of building land prices (see Figure 1). Prices in Brandenburg were relatively stable for about fifteen years. However, in the wake of rising prices in Berlin since 2010, developed building land in Brandenburg became more expensive as well. Prices rose by over 35 % within five years (and this is on state average). Examining the regions surrounding Berlin in comparison to those further away, both regions show different characteristics relevant to the housing market.

The federal statistical office of Berlin-Brandenburg makes a distinction between the Berlin-neighboring region and the further metropolitan area of Brandenburg.[3] The former comprises an area of almost 3,000 square kilometers with over 900,000 inhabitants, whereas the peripheral area is almost 27,000 square kilometers large, with more than 1.5 million people living there. For the surrounding area of Berlin, the federal statistical office expects population numbers to increase in the next few years, followed by a constant development (see Figure 7 and the supplement information in the appendix as well as Amt für Statistik Berlin-Brandenburg (Hrsg.), 2015).

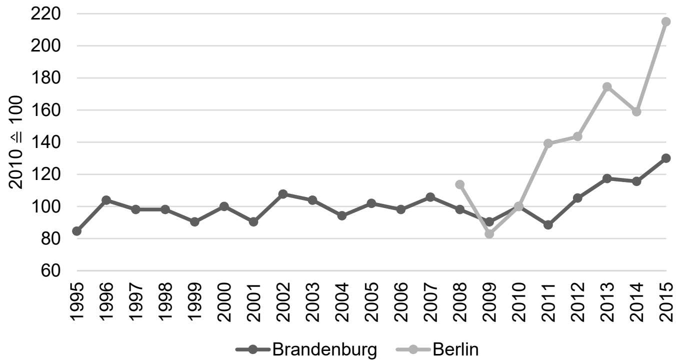
Table 1 displays housing market characteristics that distinguish between regions close to Berlin and more peripheral ones.[4] As it is common in Germany, the majority of apartments

[2] On the effects of the German real estate transfer taxes, see Fritzsche and Vandrei (2016), Buettner (2017), as well as Petkova and Weichenrieder (2017).

[3] This official differentiation is made since 2010 and reflects the interdependency between Berlin and its neighboring regions in Brandenburg. The so-called further metropolitan area is therefore less structurally connected to Berlin.

[4] Among two other factors, these indicators were used to determine which municipalities in Brandenburg should be subject to the KGV (see F+B, 2014). The additional indicators are the number of housing

Figure 1: Price evolution for developed building land in Brandenburg and Berlin



Notes: The price decreases in recent years could be explained by increased real estate transfer taxes. The increases were from 3.5 % to 5 % on January 1, 2011 and again to 6.5 % on June 1, 2015 in Brandenburg, and from 4.5 % to 5 % on April 1, 2012 and again to 6 % on January 1, 2014 in Berlin (see Fritzsche and Vandrei, 2016). Data for Berlin is not available before 2008. *Source*: Amt für Statistik Berlin-Brandenburg.

Table 1: Housing market indicators for Brandenburg

Indicator	Berlin-surrounding area	Further metropolitan area
Current housing demand		
Share of households receiving social benefit (SGB II) 2012	8.30%	14.40%
Share of students 2012	3.20%	1.40%
Unemployment rate 2012	6.00%	10.30%
Share of long-term unemployed persons on all unemployed persons 2012	33.00%	41.70%
Population development 2007 to 2012	4.90%	-6.00%
Household development 2007 to 2012	6.30%	-2.80%
Current market situation		
Vacancy rate 2011	3.00%	7.30%
Share of rental apartments on all apartments 2011	53.70%	51.90%
Level of comparable rent 2013 in EUR/sqm	5.59	4.79
Level of asking rent 2013 in EUR/sqm	7.11	5.19
Development of asking rents 2007/08–2012/13	11.60%	2.30%
Spread of comparable rents and asking rents	27.30%	8.30%
Expected market situation		
Population forecast to 2030	4.90%	-18.50%
Share of apartments subject to rent- or tenant-control agreements 2013–2018	4.70%	6.80%

Notes: The table shows housing market indicators that were used to define “tight housing markets” in Brandenburg. *Source*: Own representation based on F+B (2014).

in Brandenburg are rented out as well.[5] Regions close to Berlin exhibit population growth and a healthier labor market, thereby resulting in a higher housing demand than those regions of the further metropolitan area of Brandenburg. This is also reflected in a tighter housing market situation, particularly in terms of a lower vacancy rate as well as higher rent prices. These regions also show a higher spread between asking prices and comparable rents, thereby indicating a stronger increase in the local rent index in the near future (see F+B, 2014).

2.2 Rent controls in Germany and Brandenburg

The high share of tenant households can partially be explained by a well-established tenant protection in Germany, which began during the First World War. In the early 1920s, heavy rent regulations were introduced, fixing both the rent prices for existing and new contracts. However, after the Second World War, rent controls were gradually relaxed or abolished. In 1972, only so-called second-generation regulations were in place, which tied price setting to the local rent level. The nationwide capping limit was introduced in 1982, prohibiting the increase of rents by more than 30 % within three years or above the comparable rent level ("Mietspiegel"). A decade later, the capping limit was lowered to 20 %. Initially, this regulation only addressed apartments with relatively high rents. In 2001, it applied to all apartments (see Kholodilin, 2015).

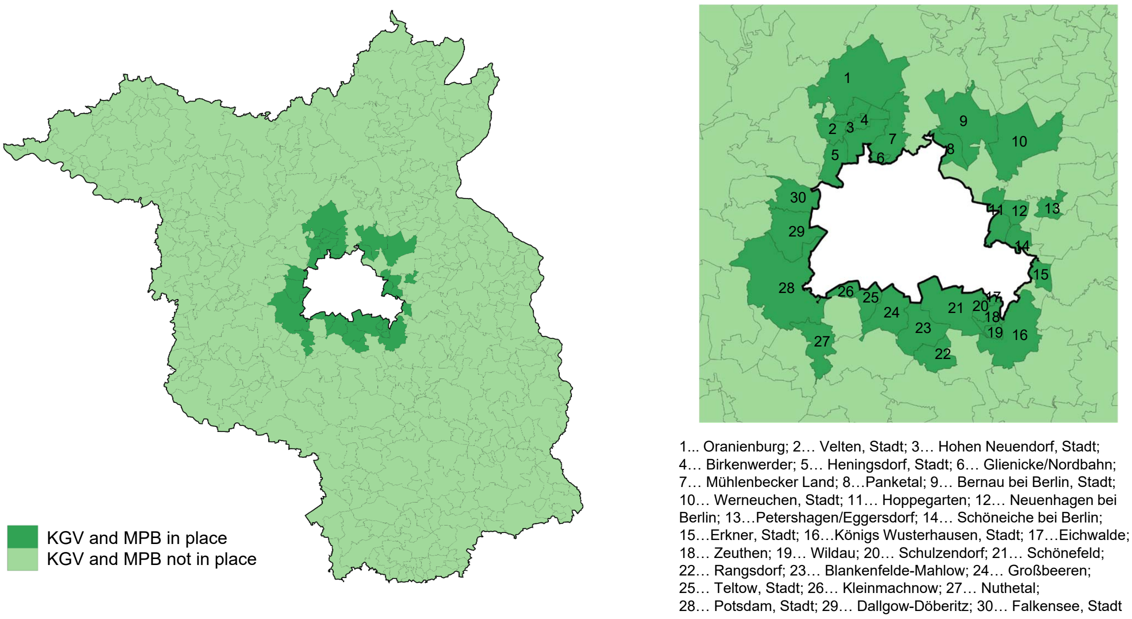
In many markets, prices for current tenants were substantially lower than market prices for new contracts, giving landlords an incentive to use eviction as a measure to increase profits. However, there was (and still is) only a small set of reasons that legitimately justified tenant eviction, one of which being that the landlord needs the apartment for himself or for close relatives. Rather than looking for a new tenant, he could thus be looking for a buyer, who would then evict the current tenants in order to claim the apartment for himself. Although this was only possible after a waiting period of three years after converting the rental apartment to an owner-occupied apartment, this behavior was observed in many urban areas. This gave cause for the state government to tighten eviction laws in 1990 by expanding the waiting period to five years. The regulation only affected areas where state governments regarded the sufficient provision with affordable rental housing as endangered. In 1993, this period was extended to ten years. With this tenant protection law, for the first time, regions in Germany were discriminated based on their housing market situation.

At that time, state governments were not obliged to give any justification regarding why they chose specific regions where the law should apply. This is also the case for the possibility to reduce the capping limit from 20 % to 15 % (KGV) in specific regions, which

allowance recipients and recipients of social benefits according to the Social Insurance Code (SGB) XII. These indicators are only available at a district level.

[5] Only 44 % of households in Germany owned their primary residence in 2014 (see Deutsche Bundesbank, 2016).

Figure 2: Regions with lowered capping limit (KGV) and rent price regulation (MPB) in Brandenburg



Source: Own representation. *Geodata*: ©GeoBasis-DE / BKG 2014.

was introduced in 2013. Only with the latest rent control, prohibiting landlords to set rent prices for new contracts that exceed the local rent level by more than 10 % (MPB), the national government specified formal requirements. At least one of the following conditions had to be fulfilled: (1) Rent increases considerably exceed the national average; (2) the average rent burden[6] of households considerably exceeds the national average; (3) there is population growth without new construction covering the additional demand; (4) there are few vacancies and a high demand.

When Brandenburg implemented the KGV in certain regions in 2014, the MPB was already being debated upon. Only for the MPB, state governments were required to base their decision on where to implement the regulation on housing market evidence. However, Brandenburg already did so for the introduction of the KGV. In March 2014, Brandenburg commissioned the external contractor F+B[7] to evaluate which municipalities would exhibit housing markets, where the provision of affordable rental housing is endangered. Among a few other factors (see above), F+B analyzed the indicators displayed in Table 1. The values for each indicator are then put into one of five categories, corresponding to scores of 0.00, 0.25, 0.50, 0.75, and 1. The boundaries for the lowest and highest categories represent the extreme values for the respective indicator, while curtailing the distribution at the average plus three standard deviations. The other categories are determined by equidistant step lengths between these extreme values. The authors apply weights of 20 % on the current

[6]The rent burden is defined as the share of a household's gross basic rent on its disposable net income.
[7]F+B Forschung und Beratung für Wohnen, Immobilien und Umwelt GmbH.

housing demand, 75 % on the current market situation and 5 % on the expected market situation. As a result, the authors calculate scores that can accept values between 0 and 100. The municipalities in Brandenburg range between a score of 16.0 (Steinreich) and 88.4 (Potsdam). The classification as a municipality where the sufficient provision with rental housing under appropriate conditions is endangered corresponds to an average score plus two standard deviations, which is 70.3 points (F+B (2014), pp. 26 ff.). This is the case for 30 municipalities in Brandenburg, which are all located in close proximity to Berlin (see Figure 2).[8] In these regions, the capping limit was lowered from 20 % to 15 %, effective from September 1, 2014.

The very same regions in which the capping limit was lowered were subject to the MPB, which was introduced in January 2016. Since then, a landlord was restricted in his price setting by two regulations: First, due to the KGV, he could only raise prices for current tenants for a maximum of 15 % within three years.[9] Second, with the MPB, new tenants could not be charged a price that exceeds the local rent level by more than 10 %.[10]

3 Conceptual framework

Both price regulations are linked to the local rent index, but in different ways. The MPB restricts landlords from asking for rents that are more than ten percent above the local rent index. Therefore, a low index goes along with a stronger restriction. However, the KGV is only binding if the local rent level is over 15 % higher than the current rent that a tenant pays his landlord. In this case, a landlord could only raise the rent by 15 % and not by a maximum of 20 % within three years, as would be the case without the KGV. Mostly, the KGV is relevant for cases where the landlord did not raise the rent for a long period of time. This can particularly be the case if he has had a price-binding agreement as part of a social housing program.[11] After the price-binding period, the local rent index will probably be above the rent he currently charges and the KGV might be binding. The MPB is presumably the regulation with the higher impact for the residential housing market. We illustrate this in the following model:

[8] Approximately half of Brandenburg's municipalities received scores from 30 to 40 points. Over 50 points are quite equally represented and can particularly be found in the Berlin-surrounding area (see also Figure 4 in Section 5.1).

[9] Presumably, for many cases, this regulation is ineffective. Even without the lowered capping limit, the price increase is restricted to the local rent level. There are some exceptions that allow a landlord to raise the rent for a current tenant above the local rent index: If he conducted modernizations, he can raise the yearly rent by 11 % of the incurred costs. Moreover, the KGV does not apply for graduated rental agreements, where price increases are agreed upon within the rental contract. The same applies for index-linked rents which are connected to the consumer price index.

[10] Apartments built or extensively reconstructed after January 1, 2014 as well as furnished apartments are exemptions. Moreover, the landlord is not obliged to reduce the rent price he received from the previous tenants.

[11] In such a program, the landlord usually receives subsidies in the form of a cheap loan or a monthly grant. The rent is often fixed for over ten years.

Potential investors are willing to pay the present value of the expected rental income for the remaining useful life of L years for an apartment:[12]

$$PV_t = \sum_{t=1}^{L} p_t \,, \tag{1}$$

where p_t is the yearly (net) rental income the investor/landlord receives in year t. He expects the market rent to increase by a yearly rate of r. Furthermore, he expects a tenant to rent the apartment for a time of C years. p_t depends on whether the landlord forms a contract with (a) a new tenant or (b) already has a tenant and can just decide to raise the current rent.

$$p_t = \begin{cases} p_t^a & \text{if} \quad t = 1 + nC, \ \forall n \in \mathbb{N} \\ p_t^b & \text{else} \end{cases} \,, \tag{2}$$

with

$$\begin{aligned} p_t^a &= \min\left\{p_t^m, 1.1 I_t\right\} \,, \\ p_t^m &= (1+r) p_{t-1}^m \,, \end{aligned} \tag{3}$$

and

$$p_t^b = \max\left\{p_{t-1}, \min\{1.15 p_{t-3}, I_t\}\right\} \,.$$

p_t^a is therefore only charged in the first period with a new tenant. This is the case every C years. With the MPB in place, p_t^a is either the market rent, p_t^m, or the local rent index, I_t, plus ten percent. In all other periods, the landlord can only increase the rent for his current tenant if I_t is higher than his current price. In this case, he can increase his current rent by a maximum of 15 % in relation to the price he charged three years ago, due to the KGV. However, the new rent must not exceed the rent index.

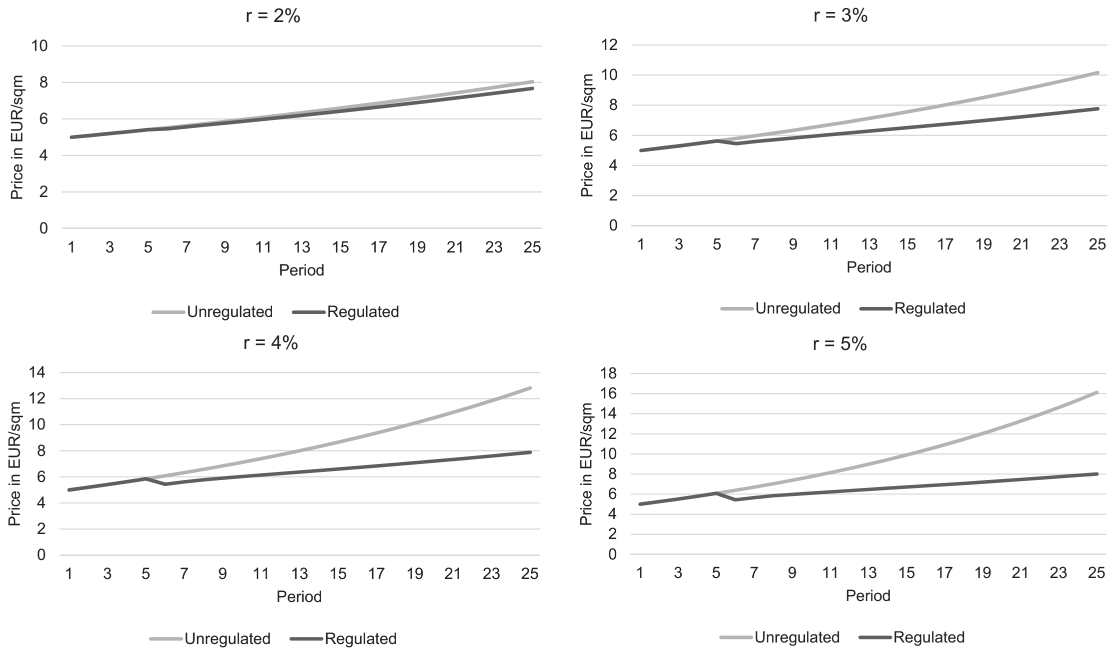
The rent index is the arithmetic mean of newly agreed upon rents as well as changes in existing contracts over the last four years. We assume a constant market-price growth rate and further assume that the number of new contracts in each period is constant. Since the rent index is calculated with prices of previous periods, it is always lower than the current market rent (due to positive growth). For this reason, a landlord who just agreed upon a contract with a new tenant, cannot raise the rent in the following few years.[13] From the moment the rent index surpasses the current rent price, a rational landlord would increase the rent for a sitting tenant for every following year. These increases, in turn, are also considered in the local rent indices in the following four years. Therefore, the local rent index is indirectly influenced by market prices from even more than four years ago.

To illustrate, we compare two apartments, of which one is subject to the regulations and the other is not. We assume $C = 10$, $L = 20$, an exogenously given market price, p_1^m, of

[12] For facilitation, we disregard maintenance costs, transaction costs, search costs, search duration, interest rates, and time preferences and we assume risk neutrality. Policymakers are aware of potential side effects due to the reforms. For example, rent price increases due to specific maintenance activities are therefore exempt from the regulations.

[13] Therefore, the KGV would not be binding.

Figure 3: Simulation of regulated and unregulated asking prices.



Notes: Simulation results for regulated and unregulated market rents. The MPB and the KGV are introduced after period 5. The KGV is not binding. *Source*: Own representation.

€ 5.00 with an annual growth of $r = 3\,\%$. Even without the new regulations, the rent for sitting tenants cannot be increased above the rent index. Initially, landlords will charge the market rent. For the given parameters, they will not be able to raise prices due to a lower rent index for the following five years. Thereafter, they increase prices to meet the index for the remaining four years that the tenant is present. Given that the number of contracts per year is constant, ten percent of the apartments get new tenants with new prices, whereas 40 % of the apartments become more expensive for sitting tenants (and in 40 % of the apartments, prices remain unchanged). Therefore, new contracts only have a weight of 20 % in the local rent index and prices for existing contracts have 80 %.[14] The market price with these parameters is 17.03 % higher, without the regulations.

Figure 3 shows that the gap between the unregulated and regulated asking price increases in the rent's growth rate. For the abovementioned case of 3 % growth, the unregulated market rent would be € 10.16 per square meter. With the MPB in place, € 7.77 would be the highest legitimate asking price. This is reflected in the landlord's present value. If he invested at the beginning of period 6, the regulation would lower his present value by 11.8 %. If he expected prices to grow by less than 1.8 % per year, the regulation would not be binding. He expects to lose 20.3 % at $r = 4\,\%$ and 28.0 % at $r = 5\,\%$ due to the regulation.

[14]Note that these populations and the local rent index are interdependent and the mentioned sizes are at equilibrium.

4 Data

Fortunately, we are able to rely on a complete survey of micro data with actual transaction prices provided by the Superior Property Valuation Committee of Brandenburg. There are sixteen county-level Property Valuation Committees in Brandenburg that receive copies of every sales contract for real estate transactions that are conducted in the respective administrative area. Therefore the committees hold information on the transaction price, the exact location of the real estate, as well as the date the contract was finalized. Through surveys, the committees collect data on the estates' characteristics, such as living space, year of construction, number of housing units in the building, or even the shape of the roof. In addition, they enrich the data with information regarding the location, including a calculated land value. The Superior Property Valuation Committee collects and administrates the data at the state level.

Table 2 displays the summary statistics for the main variables of our data. The sample covers all apartment sales between January 2011 and August 2017. Altogether, the data comprise 11,111 individual sales that include information on the living space. Approximately a third of the transactions are first sales, that is, they were not occupied prior to being sold and would thus not be subject to the MPB. For the majority of the transacted apartments, we have information on the renting situation. Unfortunately, the rent price is only known for about 2,000 observations. However, we know that at the time of sale, approximately half of the apartments came with an active rental contract.

The average values for living space and the number of rooms for the transacted apartments in our data are smaller than the average values of the census on buildings and houses in 2011: 84 square meters living space and 4.2 rooms. This reflects the fact that, first, apartments are usually smaller than single- and two-family houses. Second, smaller housings are overrepresented in transaction data compared to stock data, as they tend to be transacted more frequently. The age variable corresponds to the year of construction or to the year of constructional changes to the apartment. Therefore, it indicates the remaining period of use. In about 475 cases, construction was only completed the next year, or in the year after in a few cases. On average, an apartment in our dataset is situated in a house with a total of 26 units. This stems from the fact that, again, we only regard apartments that are not in single- or two-family houses.

The land value (German: Bodenrichtwert) represents the monetary per square-meter value of a building lot, apart from any structures that might be on that lot. The Property Valuation Committees calculate land values for a group of lots with similar characteristics. Therefore, these land-value zones allow for far finer analyses than the municipality level. However, there are still differences in actual land values within the land-value zones. The micro-location quality provides additional information. The variable considers character-

Table 2: Descriptive statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
General					
Sales price (EUR/sqm)	11,111	1,844.83	1,011.26	43	8,201
First sale (dummy)	11,111	0.32	0.47	0	1
Let (dummy)	8,395	0.49	0.50	0	1
Rent price (EUR/sqm)	2,034	6.77	1.96	0	18
Property characteristics					
Living space (sqm)	11,111	78.32	31.55	17	917
Number of bedrooms	8,402	2.79	1.01	1	24
Age (years)	11,111	12.43	11.76	-2*	236
Number of units	7,613	25.56	32.29	1	524
Location characteristics					
Land value	10,757	139.95	108.33	0	650
Location quality (1–9)	6,342	5.83	1.18	2	9
No special location (dummy)	8,845	0.64	0.48	0	1
Near water (dummy)	8,845	0.11	0.31	0	1
Second row (dummy)	8,845	0.25	0.43	0	1
Noisy or near landfill (dummy)	8,845	0.01	0.08	0	1
Municipality score	11,111	74.06	15.67	22	88

Notes: * The age of an apartment represents the time difference of the finalization of the sales contract and the year of construction or, if applicable, the year of constructional changes. A negative age indicates that an apartment is sold prior to constructions being completed.

istics that explicitly influence the land value within a land-value zone. Examples of such characteristics are close proximity to water or being exposed to noise.

In addition to the data provided by the Superior Property Valuation Committee in Brandenburg, we allocate municipality-scores, calculated in F+B (2014), to each transaction. High scores indicate a so-called tightness of a housing market in the sense that a high demand for rental housing might not be met at affordable prices. Municipalities in Berlin's neighboring areas and/or with a good infrastructural connection typically show higher scores. Both are true for the capital and largest city of Brandenburg, Potsdam, which has the highest score of 88.4 points. Overall, 169 of a total 418 municipalities are represented in the data. In particular small municipalities and those with a higher share of single-family homes are less likely to host an apartment transaction during the regarded time frame. This is also the case for the lowest-scoring municipalities: Steinreich (16.0), Drahnsdorf (20.0), and Heideblick (20.5). However, all municipalities with a score greater than 60 are included in the data.

In the following analysis, we exclude first sales, as these are most likely not subject to the reforms. We also exclude the municipality of Ahrensfelde, since it constitutes the one exception of being subject to the MPB, but not to the KGV.[15] We have a few cases in the

[15]While the KGV was not introduced due to a score of 67.9, Brandenburg decided to introduce the MPB,

data, where entire houses and therefore multiple apartments are transacted simultaneously. Treating entire house sales as multiple observations of apartment sales would distort our data. To prevent this, we compute these cases as single apartment transactions, using the mean apartment price and characteristics.[16]

5 Empirical analysis

5.1 Identification strategy

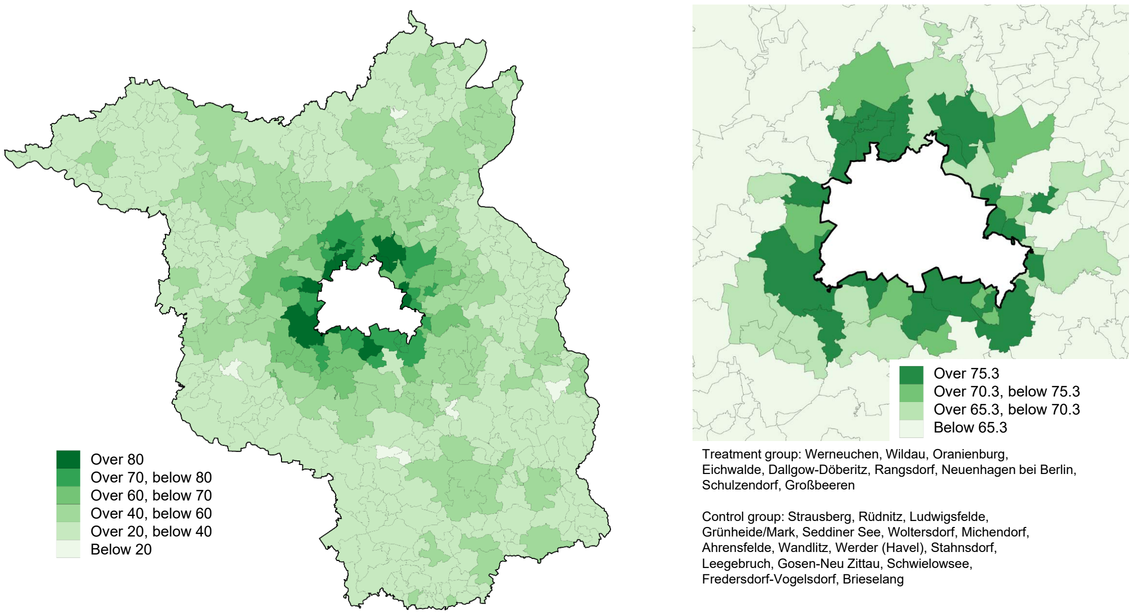
We test whether rent price regulation has an impact on transaction prices of affected apartments. To identify this effect, we employ a regression discontinuity approach (see Thistlethwaite and Campbell, 1960; Hahn et al., 2001; Imbens and Lemieux, 2008; Lee and Lemieux, 2010). The basic idea is that around the treatment threshold (70.3 points), municipalities are so similar in those characteristics that determine whether or not they receive the treatment, that the actual assignment can be regarded as being random. Since the apartments in those municipalities are unalterably located there, they also receive treatment randomly. Therefore, in the best-case scenario, these apartments show the same characteristics on average, with the only difference being whether or not they are subject to rent regulations. With this design, we are able to estimate the causal price effect of the rent regulations on those apartments.

Figure 4 depicts the regional distribution of the municipality scores calculated in F+B (2014). The right-hand side illustrates the control and treatment groups within a bandwidth of five score points to either side of the cutoff point of 70.3, above which the treatment is given. In many cases, treated regions are neighboring non-treated ones. Since score levels reflect the local housing market, in the absence of regulations, investors who would have invested in a market slightly above the cutoff point, might now have an incentive to select a neighboring location with a slightly lower score. In this regard, our estimation can be considered optimistic. The coefficients that we find might partially be driven not only by lower prices in treated municipalities, but also by higher prices due to spillover effects to non-treated ones. The data we use for the calculations include the living space, age, land value, and the transaction date of the apartments. Further control variables would either restrict our sample size too heavily, as we do not have this information for all observations (e. g. the "let" dummy variable), or are strongly correlated with other covariates (e. g. number of rooms with living space). The sample we use comprises 6,303 observations for the entire time frame.

since demand picked up rapidly in the recent past.

[16] As a robustness check, we exclude all sales, where multiple apartments of the same house were transacted simultaneously (see Section 6.2).

Figure 4: Municipality score distribution in Brandenburg (left) and assignment to treatment and control groups (right)



Data: F+B (2014). *Geodata*: ©GeoBasis-DE / BKG 2014. *Notes*: The assignment to treatment and control groups is based on a bandwidth of five score points above and below the cutoff of 70.3 points.

We define the treatment as receiving both the KGV and the MPB. Therefore, our calculations are more optimistic in comparison with Kholodilin et al. (2017), who concentrate on the MPB. The treatment is assigned to all municipalities with a score higher than 70.3 points. All regions below that threshold are untreated. Therefore, we have a sharp discontinuity at the cutoff. To identify a causal effect, we must ensure that there is no additional discontinuity at the cutoff that would confound our interpretation. This is not the case since the municipality scores were used for the sole purpose of identifying regions that should receive rent regulation. Therefore, the cutoff has no further meaning other than allocating the treatment. We conclude that there is no discontinuity in *potential* outcomes.

Further, we need to check whether there might be manipulation into treatment or non-treatment. Observations of our sample might have influenced their score in order to receive or to be exempt from rent price regulations. Our data comprise single transactions, whose assigned score depends solely on the municipality they are located in. If at all existent, the influence of one single apartment (respectively of its owner) on the municipality's score is vanishingly small. Thus, the concern of manipulation does not lie on the level of single observations but rather on the level of the corresponding municipalities. The strongest argument against manipulation on this level is that the scores are based on a report that utilizes actual housing market data. Moreover, it was commissioned not by the local but the state government. Thus, it does not seem plausible that a municipality was able to actively influence, for example, its vacancy rate in order to lower (or increase) its score. Further, the cutoff point itself appears to not be politically influenced, since it is fixed at exactly the distribution's mean value plus two standard deviations. However, the report by F+B (2014) includes a municipality survey that might raise concern regarding manipulation. Ultimately, the survey was not used to calculate the municipality scores. We do not know for sure, though, that the indicator weights that were used are free from manipulation. To tackle this issue, we conduct a density test and find no significant bunching of scores around the cutoff point of 70.3 (see Table 10 and Figure 8 in the appendix for details).

To further exclude the possibility of manipulation, the covariates need to be balanced around the threshold. This is most certainly the case for the apartments' age as well as the date the transactions took place (see Figure 9 in the appendix). Moreover, the living space appears rather continuous; however, land values suggest a discontinuity at the cutoff. This does raise a concern regarding whether transactions are indeed distributed randomly. Examining the earlier time frame, though, we can see that the distribution in land values was balanced before the treatment. This gives reason to believe that the current land values already, at least partly, reflect the negative impact of regulation on property prices. Since the committees of evaluation experts calculate land values on a yearly basis, this is indeed possible. Therefore, we conduct our regression analysis both with and without inclusion of land values.

The time frame in our data spans from 2011 to 2017. In Brandenburg, the KGV came into effect in September 2014 and the MPB in January 2016. Since media coverage on the topic was very strong, it seems plausible that potential investors were aware of the regulations by January 2016.[17] It makes sense to also examine a time frame where there had been no treatment. The treatment in our context is that investors know of the rent price regulation that might affect future rental income. Although both instruments were already discussed even before 2014, it remained unclear, which regions, if any, would be subject to the regulation. If investors anticipated the introduction of a rent price regulation in any of Brandenburg's regions, their expectations should have been similar for those regions around the cutoff point. For this reason, we can span our pre-treatment time frame to April 2014, since the technical report evaluating the municipalities' housing markets was not published before May 2014. Therefore, the pre-treatment period stretches from January 2011 to April 2014, while the post-treatment period is from January 2016 to November 2017.[18]

In our baseline regression, we estimate the local average treatment effect in a pooled

[17] The KGV is generally less known compared to the MPB. However, for our analysis, it is more important that the information distribution did not change in the regarded time frame, rather than everybody being fully informed.

[18] Potentially, changes in real estate transfer taxes could have had a great impact on apartment prices during this time. However, Brandenburg raised transfer taxes on two occasions: in January 2011 and in July 2015. Therefore, transfer taxes were constant within the pre- and post-treatment time frame, respectively.

cross-section RD approach. We use a local polynomial to smooth the functional form of the underlying relationship between sales price and the municipality score. Since this relationship might be stronger for very high and very low values of the municipality score, we conduct the estimation with a quadratic smoothing function. Our baseline regression function takes the following form:

$$\begin{aligned} p_i = {} & \alpha + \beta_1 T_i \\ & + \beta_2(x_i - x_c) + \beta_3(x_i - x_c)^2 + \beta_4 T_i(x_i - x_c) + \beta_5 T_i(x_i - x_c)^2 \\ & + \delta X_i' + \epsilon_i \,. \end{aligned} \tag{4}$$

p_i denotes the log sales price of an apartment i. The coefficient of interest is β_1, which measures the treatment effect around the threshold. T_i is a dummy variable that equals one if a transaction takes place in a treated municipality and zero otherwise. x_i represents the position on the running variable, whereas x_c is fixed at the cutoff of 70.3. Thus, the difference indicates the municipality score distance to the threshold. This difference and the squared difference control for the smooth function of the relationship between running and the outcome variable, which we assume to be quadratic at this point.[19] We also include an interaction term of the score distance with the treatment dummy to allow for different reactions of treated and non-treated units to the treatment. In the regression, we use a triangular kernel function. Hereby, observations that are closer to the threshold receive greater weights. X_i depicts a vector of control variables, such as living space and the apartment's age, with its corresponding coefficients δ. α is the intercept and ϵ_i is the standard error, clustered for municipalities.

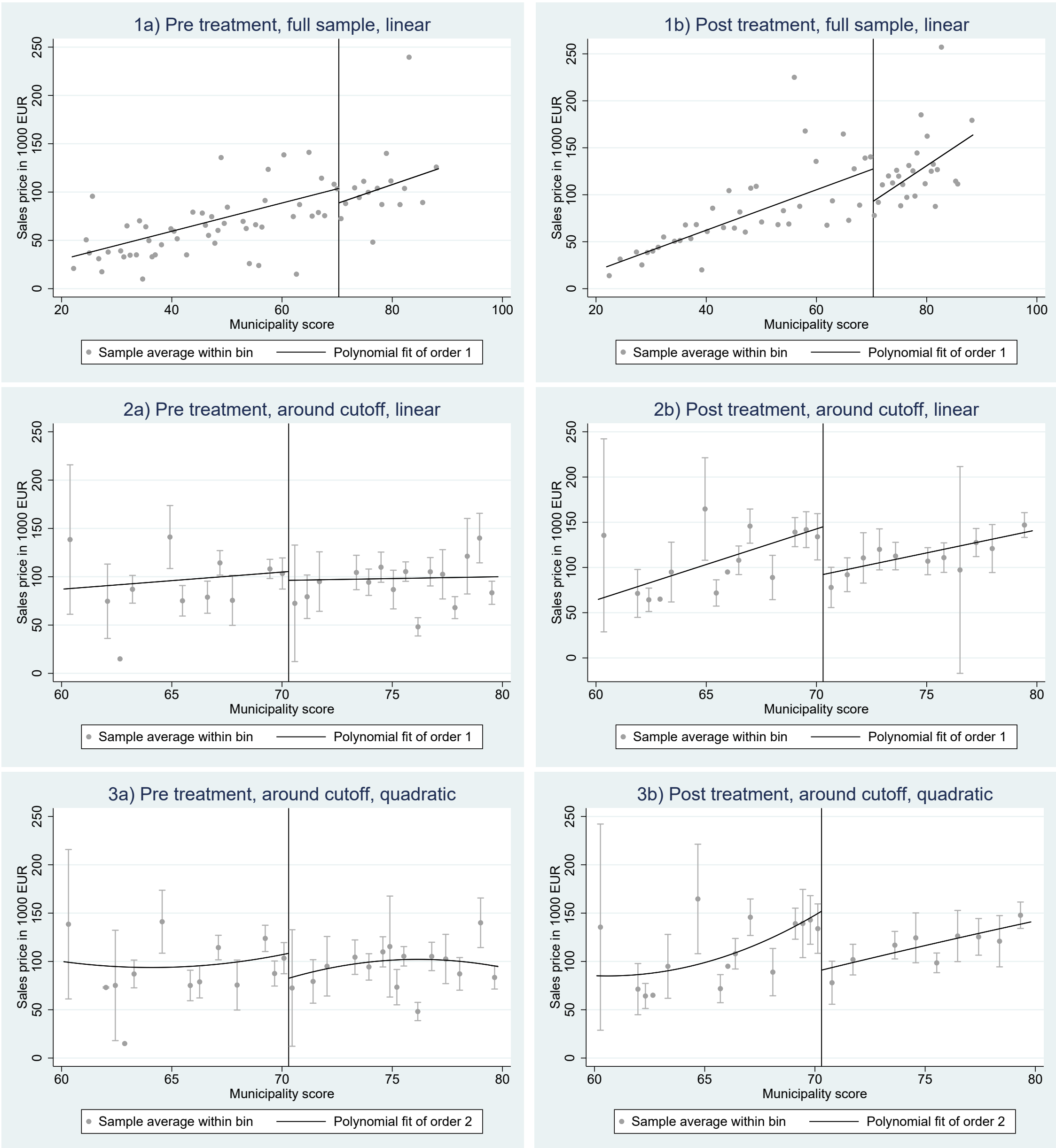
5.2 Results

Figure 5 illustrates the discontinuity of prices per square meter at the threshold of 70.3 score points. The first thing to notice is that for the entire range of the sample, the relationship appears to be rather linear and continuous, both for the pre- and the after-treatment period (Figure 5 plots 1a and 1b). Further, for the effect around the threshold, prices are quite clearly continuous for the pooled cross section from 2011 to 2014 (plot 2a and 3a). However, in the time frame after the treatment, observations that are located in municipalities with scores just slightly above the cutoff do indeed appear to sell for smaller prices, as plots 2b and 3b suggest. This significance of this effect is illustrated by the fact that the 95 % confidence intervals of the close-to-cutoff observations do not overlap—in contrast to the pre-treatment period. This result does not change when we add control variables.

Table 3 displays the baseline regression results. In a nutshell, we find that the rent regulations reduce sales prices by approximately 27 %. The first three columns show that in

[19] In section 6.3, we also use polynomial orders 0, 1, and 3 for the (interacted) smoothing function.

Figure 5: Distribution of mean prices per square meter for municipality scores



Notes: The graphs show the municipality mean prices per square meter for municipality scores before (left) and after (right) the treatment for the entire sample (top) and around the treatment cutoff of 70.3 points (bottom). The mid and bottom figures display 95 % confidence intervals with standard errors clustered at municipality level. The vertical lines indicates the cutoff at 70.3 points, above which the treatment is given. The other lines show the best weighted least-squares fit of a first- (top and mid) and second- (bottom) order polynomial. The pre-treatment time frame is January 2011 to April 2014; the post-treatment time frame is January 2016 to November 2017. The displayed bins are calculated to be evenly spaced and to mimic the variance of the raw data (see Cattaneo et al. (2017a) for details).

Table 3: Baseline results

	Dependent variable: sales price (logged)							
Time frame	Pre-treatment				Post-treatment			
RDD estimator								
Coefficient	-0.207	-0.048	0.092	0.055	-0.565***	-0.259	-0.293*	-0.269**
Std. error	0.192	0.178	0.153	0.122	0.193	0.171	0.154	0.133
p-value	0.281	0.788	0.549	0.653	0.003	0.130	0.056	0.043
Control variables								
Living space	NO	YES	YES	YES	NO	YES	YES	YES
Living space squared	NO	NO	YES	YES	NO	NO	YES	YES
Age	NO	NO	YES	YES	NO	NO	YES	YES
Age squared	NO	NO	YES	YES	NO	NO	YES	YES
Land value	NO	NO	NO	YES	NO	NO	NO	YES
Date	NO	NO	YES	YES	NO	NO	YES	YES
Obs.	419	419	419	417	366	366	366	364
Obs.–	262	262	262	260	242	242	242	241
Obs.+	157	157	157	157	124	124	124	123

Notes: Significance levels: *** 0.01, ** 0.05, and * 0.10. Standard errors are clustered at municipality level. Pre-treatment time frame: January 2011 to April 2014; post-treatment time frame: January 2016 to November 2017. Quadratic smoothing function. Bandwidth: 5 points to either side of the cutoff of 70.3. Kernel type: triangular. Obs.– (obs.+) denotes the number of observations below (above) the cutoff within the respective bandwidth.

the pre-treatment period, between January 2011 and April 2014, the RDD estimator is insignificant. When controlling for the living space, the smaller standard errors give us confidence that the true coefficient is in fact close to zero. Put simply, we do not find a treatment effect when there was no treatment. This is the case for both regressing with just the living space as a control variable as well as other controls, such as the age of an apartment and its land value. We also include the date variable to account for a supposedly linear trend of increasing prices. For the post-treatment frame, the effect is considerably larger. Simply regressing on the sales price overestimates the effect, since the treatment group shows lower prices even in the pre-treatment frame, when not controlling for any covariates. Adding the living space as the only control variable does not reduce the variance by much. However, it does reduce the coefficient to a level that does not differ from zero at a significant level. The last column is the most interesting one. Adding further controls, such as the age, land value, and the date of transaction, the coefficient remains unchanged, but gains significance at the 5 % level. Apartments that are similar in terms of living space, age, land value, and the transaction date, realize sales prices that are significantly lower if the apartment is located in a municipality that only just received treatment. On average, such an apartment sells for a price of approximately 27 % below an object that is not treated.

As suggested earlier, our estimation can be regarded as optimistic. The external validity of our results could be limited due to the fact that we cannot exclude spillover effects to neighboring municipalities. Therefore, it could be possible that owners in unregulated municipalities even profit from the regulations in the form of price increases. Thus, the effect that we find addresses the price effect differences between treated and non-treated apartments, which might have higher prices due to the regulations. When regarding the scenario of no introduction of any treatment at all as the counterfactual world to our setting, we expect the coefficients to be slightly lower. In most cases, the introduction of the regarded regulations will always evoke spillover effects, since treated municipalities are almost always neighboring non-treated ones. Moreover, municipalities are typically fairly small. For this reason, we expect spillovers not only at the edges of a municipality. Particularly for investors, choosing the neighboring municipality will not be much of a problem.

However, the results above show lower sales prices of almost 30 % when we factor in that the land value also decreases. Taking possible spillovers and the following sensitivity analyses into account, we are confident that the effect of the regarded rent price regulations on sales prices is in the range of 20 % to 30 %.[20] In comparison with the study by Kholodilin et al. (2017), our findings suggest much greater effects of rent price regulation on sales prices. This can be explained by the fact that, first, we do assume a sharp treatment timing. The reforms should affect market prices when people are aware of the reforms. The proportion of informed market participants does not switch from zero to one from one day to the next. Rather, information spreads monotonously over time. Therefore, the effect we find does not only include the 2.1 % and the 2.7 % drop in prices that Kholodilin et al. (2017) found. It also includes the (presumably smaller, but many) price drops at other dates between announcement and introduction of the reforms. Second, we measure not only the MPB but also the KGV. The latter is arguably not binding (see Section 3). However, if it has any effect on prices, it is in the same direction as the MPB. Third, we use transaction data rather than asking prices. Therefore, our effect does not only include lowered price expectations by sellers but also a discouragement of investors. We argue that it is primarily potential buyers who drive down the price due to market restrictions. Owners, however, might orient asking prices on past sales or price assessments that took place before the regulations were announced.

6 Robustness exercises

In this section, we conduct a number of robustness exercises to check the sensitivity of the measured treatment effect toward different model specifications.

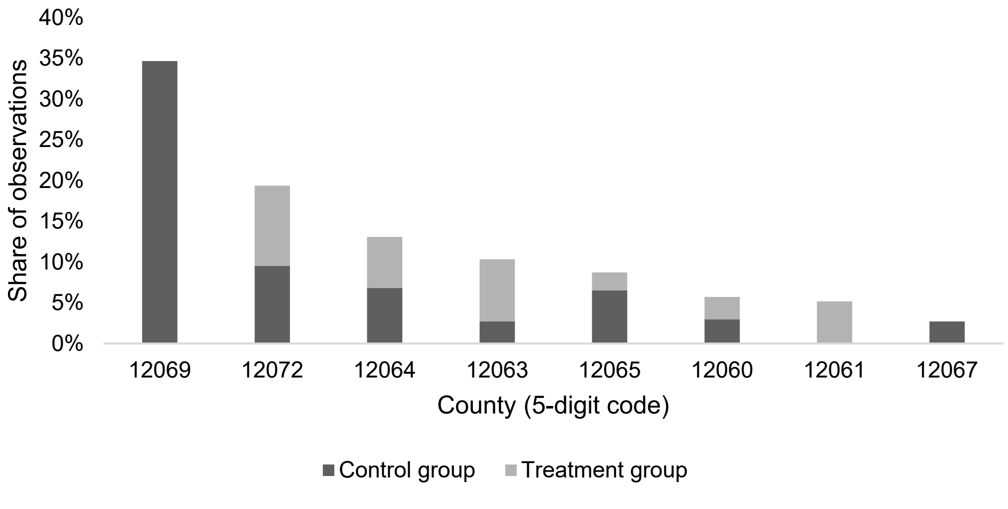
[20] Section 6.5 suggests that sales prices below the cutoff are decreasing compared to apartments in municipalities with even lower scores. Therefore, the difference that we measure in the baseline regression is dominantly driven by comparably decreasing prices in the treatment group.

6.1 Clusters

When we look at the map on the right-hand side of Figure 4, we might be concerned that the distribution of control and treatment observations is geographically clustered. Indeed, there are four municipalities southwest of Berlin that are not neighboring Berlin and that all belong to the control group. In addition, they are all located in the county Potsdam-Mittelmark. Figure 6 shows that the observations in this county constitute more than a third of all observations used in the baseline regression. The concern is that there might be a price trend in the cluster that does not show in the other areas we examine. More specifically, our main coefficient might be driven not by an actual drop in prices of treated observations but rather by price increases of non-treated ones (in the cluster). To test for this, we conduct the same exercise as we did for the baseline regression, while omitting the data of Potsdam-Mittelmark.

The results are displayed in Table 4. Compared to the baseline results in Section 5.2, we lose 127 observations in our treatment group. This provides us with an almost perfectly balanced sample. The results support our previous findings. What is worth noticing is that the treatment effect of 25 % is significant at the highest significance level, while the effect is measured at almost exactly zero for the period prior to treatment. This gives us confidence that our baseline results are not driven by geographical clusters.

Figure 6: Distribution of observations in the treatment and control groups by county



Notes: The figure shows the distribution of observations in the post-treatment time frame (January 2016 to November 2017). The treatment (control) group includes observations with municipality scores from 70.3 to 75.3 (65.3 to 70.3) points. The represented counties include: 12069 Potsdam-Mittelmark, 12072 Teltow-Fläming, 12064 Märkisch-Oderland, 12063 Havelland, 12065 Oberhavel, 12060 Barnim, 12061 Dahme-Spreewald, and 12067 Oder-Spree.

Table 4: Results without Potsdam-Mittelmark

	Dependent variable: sales price (logged)							
Time frame	Pre-treatment				Post-treatment			
RDD estimator								
Coefficient	-0.269**	-0.131	0.069	0.001	-0.547***	-0.236*	-0.270***	-0.245***
Std. error	0.126	0.123	0.120	0.094	0.104	0.124	0.102	0.082
p-value	0.032	0.287	0.570	0.923	0.000	0.057	0.008	0.003
Control variables								
Living space	YES	YES	YES	YES	NO	YES	YES	YES
Living space squared	NO	NO	YES	YES	NO	NO	YES	YES
Age	NO	NO	YES	YES	NO	NO	YES	YES
Age squared	NO	NO	YES	YES	NO	NO	YES	YES
Land value	NO	NO	NO	YES	NO	NO	NO	YES
Date	NO	NO	YES	YES	NO	NO	YES	YES
Obs.	311	311	311	310	239	239	239	237
Obs.–	154	154	154	153	115	115	115	114
Obs.+	157	157	157	157	124	124	124	123

Notes: Significance levels: *** 0.01, ** 0.05, and * 0.10. Standard errors are clustered at municipality level. Pre-treatment time frame: January 2011 to April 2014; post-treatment time frame: January 2016 to November 2017. Quadratic smoothing function. Bandwidth: 5 points to either side of the cutoff of 70.3. Kernel type: triangular. Obs.– (obs.+) denotes the number of observations below (above) the cutoff within the respective bandwidth. Observations in Potsdam-Mittelmark are excluded from the regression.

6.2 Entire house sales

The prices for entire houses might follow a different logic compared to apartment prices. Typically, the buyer is an institutional investor as opposed to the private investor who aims to inhabit or rent out a single apartment. These sales should not affect our baseline results too much. We treated the simultaneous sales of apartments in the same house as a single transaction, using mean values. Moreover, our baseline results would only be distorted if these transactions systematically appeared in our control or treatment groups. We have no reason to believe that this should be the case. However, almost 25 % of all the transactions in our data were part of the entire house being sold. For that reason, we conduct a robustness check here: we re-estimate the baseline regressions while omitting transactions from entire house sales.

For the sample that we use in the regression, we lose fewer than twenty observations for both the pre- and the post-treatment time frames, respectively (compare Table 3 and 5). Accordingly, the results are very similar. We can conclude that entire house sales do not distort our baseline results.

Table 5: Results without entire house sales

	Dependent variable: sales price (logged)							
Time frame	Pre-treatment				Post-treatment			
RDD estimator								
Coefficient	-0.209	-0.054	0.085	0.039	-0.559***	-0.244	-0.280*	-0.255*
Std. error	0.196	0.179	0.151	0.118	0.196	0.174	0.154	0.134
p-value	0.286	0.761	0.574	0.739	0.004	0.161	0.068	0.057
Control variables								
Living space	YES	YES	YES	YES	NO	YES	YES	YES
Living space squared	NO	NO	YES	YES	NO	NO	YES	YES
Age	NO	NO	YES	YES	NO	NO	YES	YES
Age squared	NO	NO	YES	YES	NO	NO	YES	YES
Land value	NO	NO	NO	YES	NO	NO	NO	YES
Date	NO	NO	YES	YES	NO	NO	YES	YES
Obs.	401	401	401	399	347	347	347	345
Obs.–	255	255	255	253	230	230	230	229
Obs.+	146	146	146	146	117	117	117	116

Notes: Significance levels: *** 0.01, ** 0.05, and * 0.10. Standard errors are clustered at municipality level. Pre-treatment time frame: January 2011 to April 2014; post-treatment time frame: January 2016 to November 2017. Quadratic smoothing function. Bandwidth: 5 points to either side of the cutoff of 70.3. Kernel type: triangular. Obs.– (obs.+) denotes the number of observations below (above) the cutoff within the respective bandwidth. Observations that are identical in district, transaction date, year of construction, and the number of units are excluded from the regression.

6.3 Polynomial

In the baseline regression, we use a second-order polynomial to control for the relationship between the running (municipality score) and the outcome variable (logarithmic sales price). Here, we test the sensitivity of our treatment coefficient with regard to different polynomial orders. By varying the polynomial order, we also change the bandwidth for the estimation. The reason for this is that with the bandwidth selection, there is a general trade-off between misspecification bias of the model and variance of the estimation coefficients, both of which depend on the polynomial order (Cattaneo et al., 2017a). We employ the data-driven mean-square error optimal bandwidth selector (mserd), developed by Calonico et al. (2014), Calonico et al. (2016), and Calonico et al. (2017).

It is evident from Table 6 that the treatment effect is quite robust against the variation of the smoothing polynomial and the corresponding estimator bandwidths. The estimator is not significantly different from zero, if we use polynomials of a higher order than quadratic. However, even with a cubic function, the point estimator still lies within the same magnitude.

Table 6: Results with polynomials of different orders

	Dependent variable: sales price (logged)			
Estimator polynomial order	0	1	2	3
Bias polynomial order	1	2	3	4
Estimator bandwidth	2.29	7.72	4.59	5.57
Bias bandwidth	4.82	11.20	6.78	7.09
RDD estimator				
Coefficient	-0.248***	-0.210*	-0.245*	-0.198
Std. error	0.068	0.109	0.138	0.152
p-value	0.000	0.055	0.076	0.192
Obs.	160	526	334	416
Obs.–	135	265	219	241
Obs.+	25	261	115	175

Notes: Significance levels: *** 0.01, ** 0.05, and * 0.10. Standard errors are clustered at municipality level. Kernel type: triangular. Time frame: January 2016 to November 2017. Obs.– (obs.+) denotes the number of observations below (above) the cutoff within the respective bandwidth. Covariates included: living space, living space squared, age, age squared, land value, date.

6.4 Bandwidth

The most common exercise to conduct in an RDD approach is to alternate the bandwidth of the regression and thereby use different sample sizes for estimating the treatment effect (Hahn et al., 2001; Lee and Lemieux, 2010). This is particularly important for our analysis, since only slightly different bandwidths can result in the inclusion (or exclusion) of a large number of observations at once, since the running variable refers to municipalities. Therefore, we conduct the regression with a broad sample of different windows, reaching from 2 to 8 score points both above and below the cutoff and also include a vastly larger bandwidth of 15 points.

For all specifications of the bandwidth, the RDD coefficient is somewhat in the same range as the baseline regression with a bandwidth of 5 score points. Interestingly, the smallest bandwidth shows the highest coefficient, while the effect is insignificant for a bandwidth of 3 and 4. This could imply that the effect is restricted to a very narrow window around the cutoff. However, the fact that the coefficient is very robust towards bigger window choices suggests that the effect stretches further. This is true even at the biggest window and, therefore, justifies the inclusion of a considerably larger number of observations.

Table 7: Results with different bandwidths

	Dependent variable: sales price (logged)			
Bandwidth	2	3	4	5
RDD estimator				
Coefficient	-0.311**	-0.144	-0.218	-0.269**
Std. error	0.123	0.138	0.141	0.133
p-value	0.012	0.294	0.121	0.043
Obs.	160	208	310	364
Obs.–	135	145	218	241
Obs.+	25	63	92	123
Bandwidth	6	7	8	15
RDD estimator				
Coefficient	-0.262**	-0.239**	-0.234**	-0.235**
Std. error	-2.156	0.116	0.111	0.114
p-value	0.031	0.038	0.034	0.039
Obs.	419	447	544	1037
Obs.–	244	244	265	333
Obs.+	175	203	279	704

Notes: Significance levels: *** 0.01, ** 0.05, and * 0.10. Standard errors are clustered at municipality level. Kernel type: triangular. Time frame: January 2016 to November 2017. Obs.– (obs.+) denotes the number of observations below (above) the cutoff within the respective bandwidth. Covariates included: living space, living space squared, age, age squared, land value, date.

6.5 Pseudo treatments

As a further robustness check, we conduct our RDD estimation with artificial cutoff points. Table 8 shows that the true cutoff of 70.3 points is the only one where we actually find a significant treatment effect. All other estimators are either very close to zero or exhibit very large standard errors. Note that there is a relatively big coefficient at the pseudo cutoff of 64.3 points. Despite being insignificant, the coefficient indicates that apartments just above the pseudo cutoff are relatively low-priced compared to those just below the pseudo cutoff. The majority of apartments right of the pseudo cutoff are within the bandwidth left to the true cutoff. This result suggests that the effect that we measure for the true cutoff does not stem from price increases of untreated units but rather from price decreases of treated ones.

Table 8: Results with different pseudo cutoffs

	Dependent variable: sales price (logged)						
Cutoff	60.3	64.3	67.3	70.3	73.3	76.3	80.3
RDD estimator							
Coefficient	-0.181	-0.515	-0.069	-0.269**	0.031	-0.035	-0.112
Std. error	0.588	0.371	0.238	0.133	0.086	0.170	0.252
p-value	0.758	0.166	0.773	0.043	0.720	0.835	0.657
Obs.	92	288	347	364	414	404	581
Obs.–	49	44	139	241	198	165	270
Obs.+	43	244	208	123	216	239	311

Notes: Significance levels: *** 0.01, ** 0.05, and * 0.10. Standard errors are clustered at municipality level. Kernel type: triangular. Time frame: January 2016 to November 2017. Bandwidth: 5 points to either side of the respective cutoff. Obs.– (obs.+) denotes the number of observations below (above) the cutoff within the respective bandwidth. Covariates included: living space, living space squared, age, age squared, land value, date.

6.6 Ordinary least squares regression

Table 9 displays the regression results of an ordinary least squares regression. The treatment effect is captured by a dummy variable that equals one if the respective municipality received a score greater than 70.3. It is zero otherwise. We show both the regression results for the pre-treatment period as well as for the time frame with active regulations. We estimate the treatment coefficient as a local effect, using the data-generated bandwidth for a zero-order polynomial (2.287 points both above and below the cutoff). In this manner, we treat the effect of the municipality score on house prices as locally constant. We also include a regression over the full range of municipality scores, assuming a linear form for the relationship between municipality score and logarithmic sales prices.

The results suggest that there is no significant effect before the regulations were active, whereas the coefficients for the treatment dummy are significant in the post-treatment period. Compared to the RDD estimations, they are slightly smaller. It appears that the local effect of the municipality score is positive, even at the small windows around the threshold. Further, when estimating with the full sample and without interaction terms, we pretend that the effect of the municipality score on sales prices is linear over the full range. However, due to the discontinuity, the true relationship should be steeper and the treatment effect should therefore be greater.

Considering that the sample sizes differ tremendously between specifications, the robustness of the coefficients is remarkable. This also applies for the covariates. As expected, bigger apartments are more expensive, whereas the square-meter price decreases with size. Older units sell for less, but very old ones have a higher value. Moreover, apartments

Table 9: Results of ordinary least squares regression

	Dependent variable: sales price (logged)			
Time frame	Pre-treatment		Post-treatment	
Sample	Local	Full	Local	Full
Treatment (dummy)	-0.0784	-0.1043	-0.2173*	-0.2176**
Municipality score	—	0.0128***	—	0.0197***
Living space (sqm)	0.0167***	0.0202***	0.0295***	0.0256***
Living space squared	-0.0000***	-0.0000***	-0.0001***	-0.0001***
Age (years)	-0.0197	-0.0237***	-0.0097	-0.0098**
Age squared	0.0001	0.0002***	0.0000	0.0001
Land value (EUR/sqm)	0.0031*	0.0019***	0.0016	0.0016***
Date (day)	0.0002**	0.0001***	0.0004***	0.0002***
Constant	6.6596***	6.3324***	2.3570	3.5503**
Obs.	197	2,761	160	1,669
R^2	0.5675	0.6856	0.5601	0.7153

Notes: Significance levels: *** 0.01, ** 0.05, and * 0.10. Standard errors are clustered at municipality level. Pre-treatment time frame: January 2011 to April 2014; post-treatment time frame: January 2016 to November 2017. The local sample is restricted to municipality scores from 68.013 to 72.587. The full sample includes all municipality scores. Standard errors are clustered at the municipality level.

achieve higher prices when the land is more valuable and when they are transacted at a later date.

7 Conclusion

We investigated the effect of rent controls on apartment sales prices. We were able to show that apartments that are subject to rent regulations sell for approximately 20–30 % less than comparable units that are not affected by the reform. The design of the study and the relatively high robustness of the results suggest that the drop in prices is indeed caused by the rent controls.

Our finding translates to two messages: First, rent price regulations cause massive wealth losses for apartment owners. Second, investors show strong reactions to rent price regulations. This might indicate that investors avoid regulated regions in general, which could lead to a lower supply of rental housing in the long run and, therefore, possibly higher rent prices. However, the MPB does not affect newly built apartments. Therefore, we cannot rule out that the reforms stimulate new construction and increase supply. We deem it plausible, though, that investors are also discouraged to invest in newly built homes that eventually might be subject to regulations.

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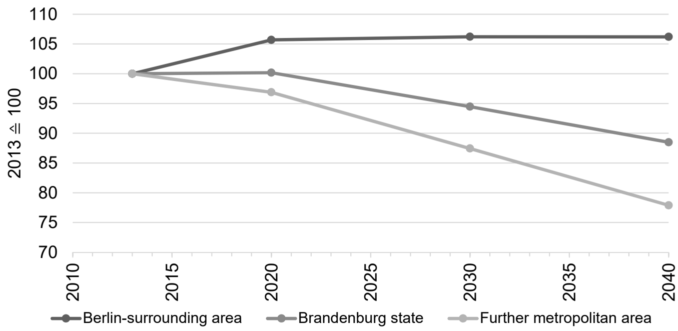
8 Appendix

Figure 7: Population forecast for Brandenburg



Notes: The forecast assumes a constant birth rate of 1.46 children per woman. Life expectancy rises for all cohorts. At the time of birth, it rises by over two years for girls and three years for boys until 2030. Net migration is expected to be a plus of 235,000 people between 2014 and 2040, of which 85 % migrate to the Berlin-surrounding area. *Source*: Own representation based on Amt für Statistik Berlin-Brandenburg (Hrsg.) (2015).

Supplemental Information

After the German reunification in 1989, many people migrated from East Germany, including Brandenburg, to the western part of Germany. In conjunction with low birth rates, the population in East Germany has been declining ever since. However, there are strong disparities on a less aggregated level. Due to internal migration from the countryside into or close to cities, there is a strong urban–rural gap in East Germany currently. This is particularly relevant for Brandenburg, as the state completely encircles the city state of Berlin and profits from migration into regions in close proximity. Berlin counts about 3.5 million people, whereas Brandenburg, despite being over 30 times bigger in area, has roughly 2.5 million inhabitants. This huge population concentration goes along with a great interdependency between the social and economic regions of Berlin and Brandenburg's area in close proximity to Berlin, which is also reflected in the housing market.

Table 10: Test results for manipulation of municipality scores

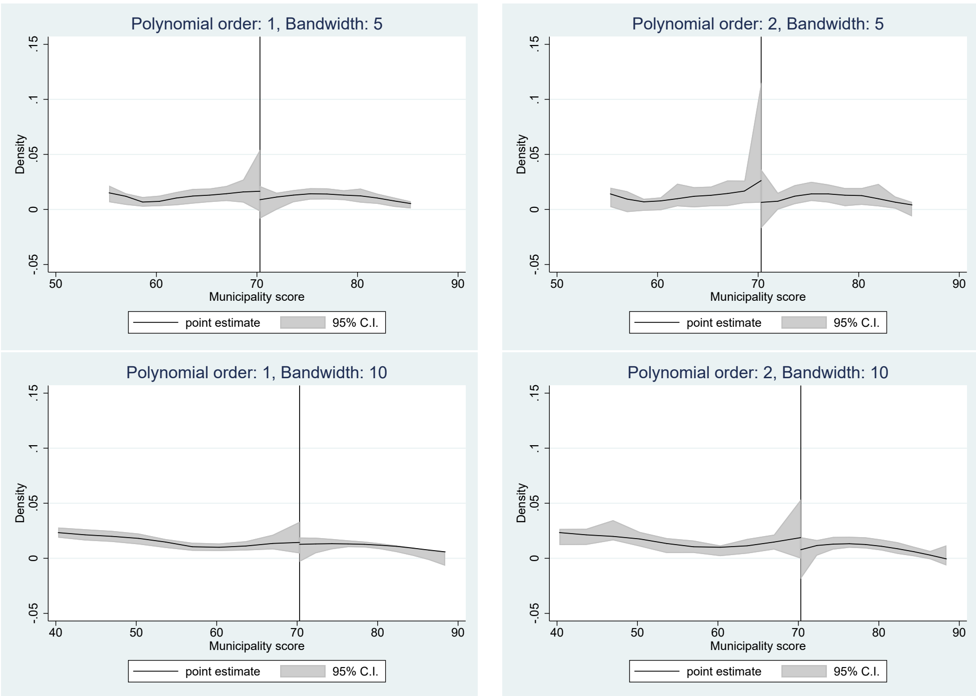
Order of polynomial	1	2	1	2
Bandwidth	5	5	10	10
T statistic	-1.187	-1.603	-1.122	-1.542
p-value	0.235	0.109	0.262	0.123
Obs.	24	24	44	44
Obs.–	15	15	23	23
Obs.+	9	9	21	21

Notes: Significance levels: *** 0.01, ** 0.05, and * 0.10. Kernel type: triangular. VCE method: jackknife. Obs.– (obs.+) denotes the number of observations below (above) the cutoff within the respective bandwidth.

Supplemental Information

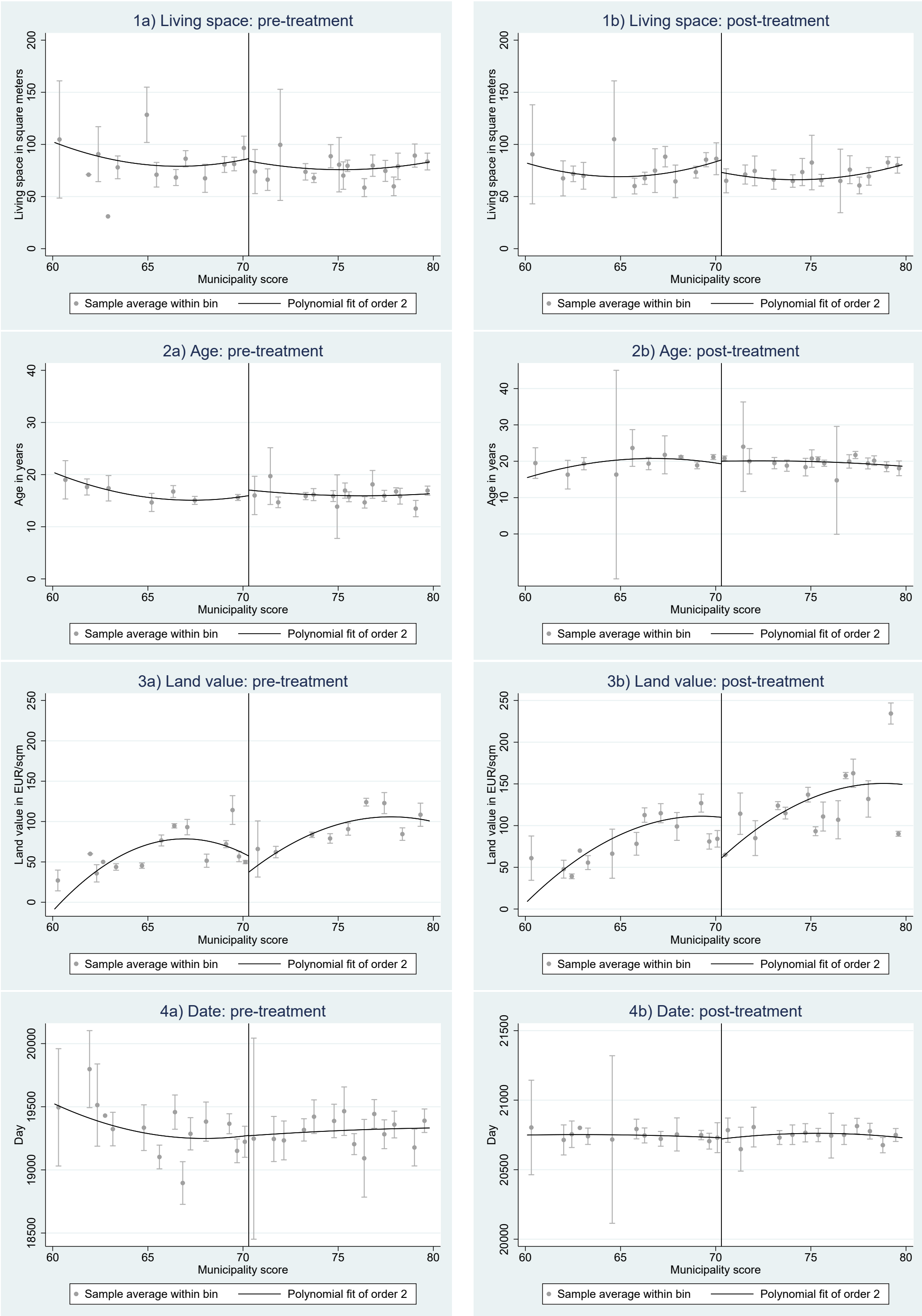
We test whether there is a significant bunching of municipality scores around the cut-off point of 70.3. We conduct a density test, as proposed by Cattaneo et al. (2017b). The null hypothesis of the test is that the density of the running variable, municipality score, is continuous. The results show that we cannot reject this hypothesis for significance levels of 0.10 or below (see Table 10). Figure 8 shows the corresponding density plots with great overlaps of the 95 % confidence bands at the cutoff of 70.3 points. Although insignificant, there might be a slight bunching just before the cutoff. This can be explained by the cluster of municipalities in Potsdam-Mittelmark, which are all in the control group. It makes perfect sense that these observations receive similar score points. They are located in close proximity to one another and have comparable housing markets. Therefore, the bunching that we find does not indicate a systematical manipulation of municipalities into treatment. In addition, we perform a robustness check without the cluster region in Section 6.1.

Figure 8: Density plots for municipality scores



Notes: Cutoff at 70.3 points.

Figure 9: Distribution of covariates around cutoff



Notes: Cutoff at 70.3 points. 95 % confidence intervals with standard errors clustered at municipality level. The day 20,500 is equivalent to February 16, 2016; 21,000 refers to June 30, 2017. The pre-treatment time frame is January 2011 to April 2014; the post-treatment time frame is January 2016 to November 2017. The displayed bins are calculated to be evenly spaced and to mimic the variance of the raw data (see Cattaneo et al. (2017a) for details).

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