Discussion Papers in Economics

No. 24/2017

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ISSN 1867-6707

Friedrich-Alexander-Universität Erlangen-Nürnberg
Institute for Economics
https://www.iwf.rw.fau.de/research/iwf-discussion-paper-series/

Empirics on the causal effects of rent control in Germany*

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Abstract

This paper empirically analyzes the effects of a second generation rent control. We make use of an uncommon policy intervention in the German housing market and translate the generated variation into a difference-and-differences setup, augmented with elements of a discontinuity design, to identify the causal impact of rent controls. We exploit the spatial and temporal differences in the regulation, finding significant effects on *de facto* regulated and unregulated rents and house prices. Our results suggest that the regulation benefits low/medium income households. Further, we provide evidence that rent regulations alter land values and depress maintenance activities. Overall, these results fit the predictions of a standard comparative-static representation of a second-generation rent control, which sheds a more favorable light on housing market interventions.

Keywords: Housing policy; rent control; rental housing; Germany.

JEL classification: D2; D4; R31.

*We thank seminar participants at the Spatial Dimensions of Inequality Workshop at ZEW Mannheim, the Economic Geography Workshop of University of Jena, and the annual meeting of the Verein für Socialpolitik. We are grateful for valuable comments and suggestions to Jeffrey Cohen, Mathias Hoffmann, Johannes Rincke, Andrés Rodríguez-Pose, and Matthias Wrede.

1. Introduction

Housing market regulation and, in particular, rent controls are subject to long-standing debates amongst scholars and policy makers. Nearly every textbook on housing and real estate economics covers these issues (see, e.g. McDonald and McMillen, 2010; O'Sullivan and Irwin, 2007)—most likely because housing markets are substantially regulated in market economies around the world. In situations of tight housing markets, affordability of housing is a major concern of politicians. For example, In 1948, U.S. President Harry S. Truman won the White House by campaigning for the *Fair Deal*, which included a promise to resolve housing shortages (Von Hoffman, 2000). Even in recent years, affordable housing remains a vibrantly discussed topic: in the light of sharply increasing rents in urban areas in Germany, the Social Democrats succeeded in launching a debate around the need for stricter rent controls in the 2013 German Bundestag elections (Knaup et al., 2013). Housing also played a major role in the 2015 UK general elections, with every party platform promising to slow down rent increases and to stimulate the construction of affordable homes (Kelly, 2015).

There are various types of rent controls, e.g. for sitting tenants or new rental contracts. Most regulators intend to mitigate the consequences of an inelastic housing supply and cyclical construction activity by cutting the resulting short run peaks of rents. The goal is to minimize transaction costs, to protect tenants from monopolistic power of landlords, and to avoid segregation processes (Arnott, 1995). So-called *first-generation* rent controls imposed strict rent ceilings or even froze rents temporarily. *Second-generation* controls, implemented since the mid 1960s, are more flexible by allowing rents to increase, for example, in line with the consumer price index or to only regulate parts of the market (Turner and Malpezzi, 2003). Supporters of rent controls further argue that *second-generation* regulations, if adequately designed, increase welfare, e.g. by stimulating additional construction activity in the uncontrolled part of the housing market (Arnott, 1995; Skak and Bloze, 2013).

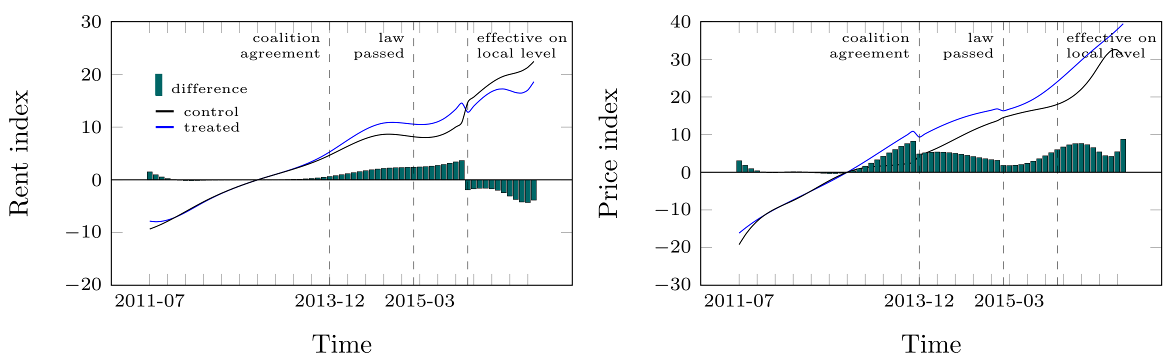
The extensive economic literature almost unanimously opposes regulations—even the more flexible forms—finding them to be inefficient instruments at fighting the effects of housing market shortages (Arnott, 1995; Glaeser and Luttmer, 2003). Available studies suggest that rent controls cause immediate reductions to the market value of rental housing (Early and Phelps, 1999; Fallis and Smith, 1985; Marks, 1984), depress refurbishment, reduce maintenance (Kutty, 1996; Andersen, 1998; Olsen, 1988b; Moon and Stotsky, 1993), slow construction activity (McFarlane, 2003; Glaeser and Luttmer, 2003), and induce inefficient allocation of units (Glaeser and Luttmer, 2003; Arnott and Igarashi, 2000), while—in the short run—having ambiguous effects on rents (Nagy, 1997; Early, 2000; Fallis and Smith, 1984; Smith, 1988). Furthermore, the targeted groups only partially benefit (Linneman, 1987; Ault and Saba, 1990; Glaeser, 2003). Most of these results are derived from theoretical models that—depending on the viability of the assumptions—provide, at best, ambiguous predictions on the effects of rent controls, as some authors criticize in this context (Arnott, 1995; Olsen, 1988a,b; Kutty, 1996). The only two papers that present causal empirical evidence point in the direction that there is only a small effect of rent control on construction activity, but a shift of dwellings from rental to owner-occupied status and a deterioration in the quality of existing rental units (Sims, 2007), while the impact on the price of the non-controlled housing stock is substantial (Autor et al., 2014). Both studies analyze the end of rent control in Cambridge, Massachusetts in 1995. In a contemporary working paper, Diamond et al. (2017) find that tenancy rent control reduces household mobility, the size of the rental housing stock, and leads to city-wide increases of rents.

In the present paper, we add empirical evidence on the effects of *second-generation* rent control to the debate. To date, there are no studies on the introduction of rent control in well identified empirical settings. With the exception of Diamond et al. (2017), previous causal evidence stems from rent de-control. In the de-control setting, it is less clear which of the differences between long-time rent controlled and uncontrolled buildings are *direct consequences* of rent control. We are the first to provide such evidence for a European housing market with a high share of rental housing. Moreover, the policy we study is much more flexible than the U.S. counterparts considered in the literature.

We analyze the short-run intended and unintended impacts of a rent ceiling imposed for new rental contracts in Germany. This regulation (the so-called *Mietpreisbremse*, lit.: *rent brake*) divides the housing market in multiple dimensions: first, the regulation is valid only in markets that are characterized by a substantial excess of housing demand. Within these regions, rent increases are tied to the development of a local reference rent. This setting separates *de jure* regulated regions into *de facto* regulated and unregulated parts: the cap on rents is only binding in areas where previous annual rent growth exceeded the threshold of 3.9% p.a. (for a detailed discussion, see Section 2.2). Further, the regulations exempts new dwellings. Finally, if the rent in the previous contract was above the limit of the rent cap, landlords may conclude this level in all subsequent contracts. Between June 2015 and April 2016, 294 municipalities across eleven federal states put the regulation into force at various points in time (see Table 1 and Figure 10 in the Appendix).

Overall, this policy intervention can be considered as a poster-child of a *second-generation* rent control regime, and, moreover, represents an excellent test-case to empirically evaluate the causal effects on rents, prices, land values, and housing quality. We exploit the spatial, temporal, and within market variation generated by the law and combine a difference-in-differences strategy with a discontinuity design that identifies sharp drops (or hikes) in the continuous trend of rents and prices (see Hausman and Rapson, 2017). We contrast the empirical findings with the implications of a standard comparative-static model of a divided (partly controlled/uncontrolled) housing market (see, e.g. McDonald and McMillen, 2010; Skak and Bloze, 2013).

Figure 1: The effect of the regulation on rents (see column (1) of Table 2) and house prices (see column (3) of Table 4)



Note: The dates of implementation on the local level are city-specific. In all graphs presented in this paper, we set this date to Dec. 2015.

The empirical results verify the expectations generated by the theoretical representation: we find that rents and house prices immediately drop in *de facto* regulated, high rent growth markets, while—at the same time—rents and prices of unregulated new dwellings rise. Figure 1 shows the development of rents and prices of treated and untreated dwellings in *de facto* regulated markets. All trends were normalized to zero in January 2013. Since then, the debate on rent controls gained momentum in the general public debate: likely, this had an effect on regulated and free rents as well as the prices of dwelling, which then started to deviate after close co-movement. One explanation could be that the rent cap, which stipulates that landlords do not need to lower rents from one contract to the next, encourages homeowners to trade higher vacancy risk for higher rents before the rent cap became effective.

For building lots, we find evidence that prices increased in regulated markets, which is consistent with positive revenue expectations for new (unregulated) residential buildings. Our analysis further reveals that refurbishment and renovation efforts are substantially dampened in regulated regions. These regions are characterized by high population density, relatively low house prices in 2011, and an above average share of foreign households.

The remainder of this paper is structured as follows: In the next section, we outline the institutional background and stylized facts about recent developments of the housing market in Germany. We then discuss briefly the potential impact of the new rent regulation in Section 3. In Section 4, we present our empirical strategy and the results. In the final section, we discuss our findings.

2. The German housing market: stylized facts and institutional setting

Before outlining our empirical strategy in detail, we briefly introduce key figures about the German housing market, discuss the institutional setting with the specific mechanics of the "*Mietpreisbremse*," and how this setting generates variation in the data that allows us to identify the causal effects of the rent control.

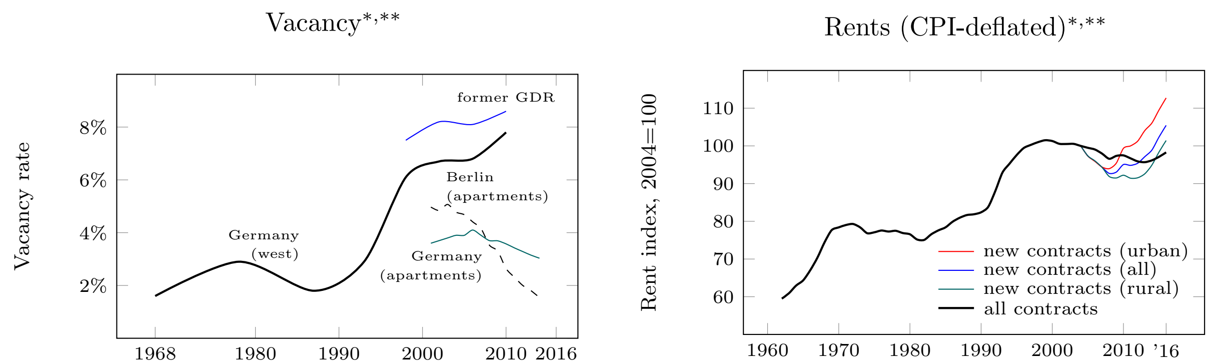
2.1. The German rental housing market

The German housing market is characterized by a relatively low homeownership rate: approximately 45% of all dwellings are occupied by their owner. According to official data (Federal Statistical Office, 2013), housing expenses—including rental payments, heating, and maintenance—of German households account for approximately 34% of their total expenditures. The net rent (27% of all expenses) is the largest component of private consumption, the next being transportation at just 14%. Thus, frictions on the housing market have immediate impact on the well-being of a large proportion of the German population, especially in urban areas.

Between 1995 and 2010, German housing market situation was relaxed. Low birth rates, outmigration from city centers to the periphery, and high construction activity in the 1990s contributed to this development. However, since 2010, urban agglomerations have become more attractive. Thanks to an inflow of migrants from smaller settlements and from abroad, the population of large German cities began to expand quickly. The result was a housing shortage, particularly putting pressure on rents for new contracts (see Figure 2).

This development is well reflected in figures on rents and vacancy rates. After 15 years of stagnation, rents started to increase rapidly, while vacancy rates fell, particularly in the urban housing stock. In 2016, rents were on average 23% above the level observed in 2010, in urban areas about 27%. However,

Figure 2: Rents and vacancy rates in Germany



Source: *Federal Statistical Office (*Statistisches Bundesamt*), Statistical Office for Berlin-Brandenburg (*Amt für Statistik Berlin-Brandenburg*); calculations by the authors; Index 2010=100; **empirica ag.

according to the Federal Statistical Office, tenant mobility is quite low: the length of a rental contract exceeds 10 years on average. Therefore, rents across all contracts (new and current) increased only slightly (see Figure 2) since 2010.

2.2. The rent cap for new contracts

Rent controls in Germany have a long history. First introduced in the early 1920s, many regulations, often rudimentary, were in place for decades. Particularly in times of extremely tight housing markets, regulation of rental housing was a preferred policy option. So-called *second-generation* rent controls were initially put into force in 1972.

Table 1: rent cap ordinances by federal states

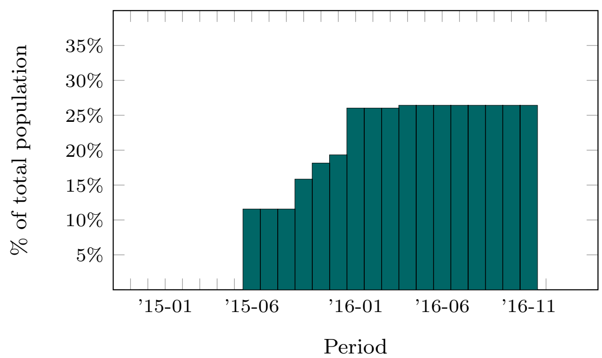
Federal state	Ordinance	Validty period	Regulated/all	Cumulative
Berlin	MietenbegrenzungsVO	2015/06-2020/05	1/1	1
Hamburg	MietpreisbegrenzungsVO	2015/07-2020/06	1/1	2
North Rhine-Westphalia	MietpreisbegrenzungsVO	2015/07-2020/06	22/396	24
Bavaria	MietpreisbremseVO	2015/08-2020/07	144/2056	168
Baden-Württemberg	MietpreisbegrenzungsVO	2015/10-2020/09	68/1101	236
Rhineland Palatinate	MietpreisbegrenzungsVO	2015/10-2020/10	3/2306	239
Hesse	MietenbegrenzungsVO	2015/11-2019/06	16/426	255
Bremen	Mietenbegrenzungs-VO	2015/12-2020/11	1/2	256
Schleswig-Holstein	MietpreisVO	2015/12-2020/11	12/1116	268
Bavaria	MieterschutzVO[a]	2016/01-2020/07	137/2056	261
Brandenburg	MietpreisbegrenzungsVO	2016/01-2020/12	31/419	292
Thuringia	MietpreisbegrenzungsVO	2016/04-2021/01	2/913	294

[a] 16 municipalities listed in the Bavarian MietpreisbremseVO were removed, while nine new municipalities were added.

The most recent regulation was introduced in 2015: the German parliament passed a law that empowered state governments to introduce a rent cap in municipalities with a "tight housing market." This rent cap introduces a rent ceiling for new rental contracts that depend on past local rent growth. For a maximum of five years, a municipality, or part of it, can be declared as a tight housing market if at least one of the following four criteria is met: First local rents grow faster than at the national average; Second the local average rent-to-income ratio is significantly higher than the national average; Third population grows while new housing construction does not create enough dwellings; or Fourth the vacancy rate is low, while demand is high. In new contracts, rents are not allowed to exceed the typical local rent by more than 10%.[1]

There are four major exceptions from the law: First, rents are freely negotiable for contracts of newly built dwellings (housing completed after October 1, 2014) and all contracts that follow. Second, units that are rented out temporarily are exempted. Third, there is no limit on the rent in the first contract after a substantial refurbishment of an existing dwelling (worth at least one-third of today's reconstruction costs of the dwelling). Fourth if the rent of the previous contract was above the limit of the rent cap, landlords may conclude this level in all subsequent contracts. Eleven federal states implemented the rent cap on

Figure 3: Population subject to rent regulation



the local level between June 2015 and April 2016—at various points in time (see Table 1). Two years after their introduction, it covers about one-fourth of the housing stock in 294 municipalities, covering about 21.5 million inhabitants (see Figure 3). The regulation concentrates on urban areas, where rent and house price increases have gained strong momentum since 2011 (see section 2.1).

3. The effects of rent controls on rents and prices in regulated and unregulated markets

The standard prediction of a comparative-static model of the housing market is that a cap on rents reduces revenues for landlords, house prices, and incentives to invest. In the long run, the housing stock declines. As some authors argue, this result is not straight forward for *second-generation* rent controls (Arnott, 1995; Olsen, 1988a,b; Kutty, 1996). In particular, settings that divide the market into a regulated and a free segment (e.g., existing dwellings are regulated while new buildings remain unregulated) can generate strong incentives to invest in the unregulated segment. In the following, we briefly describe these general effects of such regulation, and the specific mechanics of the German rent cap.

[1] As the value of the typical local rent is not observable, it can only be approximated using one of three methods: First a so-called *Mietspiegel*, that is, a survey of typical rents in the region or similar region conducted or recognized by the municipality or by representatives of landlords' and tenants' associations, which should be updated at least every two years; Second a report by a sworn expert; or third rents in three dwellings of comparable size, quality, and location of other landlords. The *Mietspiegel* is considered to be the most objective and affordable way of determining the typical local rent. However, apart from many methodological drawbacks (for a detailed discussion, see, Lerbs and Sebastian, 2015), the major pitfall is that a *Mietspiegel* is simply not available for many municipalities subject to the rent cap. Further, *Mietspiegel* are typically calculated as an average for the entire city.

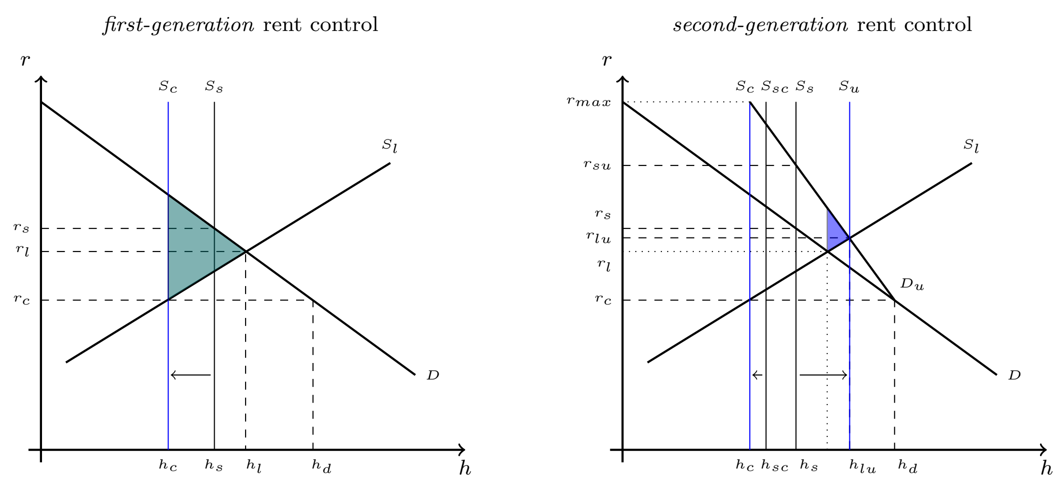
3.1. A comparative static representation of second-generation rent controls

As an immobile and durable good, it is in the nature of housing that there exist different regional market equilibria. Moreover, housing markets are spatially and qualitatively segmented (see, e.g., Goodman and Thibodeau, 1998). These market segments are typically interconnected: for example, if demand, and thus rents, in city centers rise, households substitute rental payments for commuting costs and move to the periphery of the market. On the one hand, the interconnectedness of the markets is ideal to find counterfactuals for market developments in regulated and unregulated areas. On the other hand, also the free segments of the market are affected by a regulation of a specific sub-segment.

This effect can be illustrated in a standard comparative-static framework (see, for example McDonald and McMillen, 2010; Skak and Bloze, 2013): consider an unregulated market where demand (D) for housing decreases with higher rents (r). At the intersection with the perfectly inelastic short run housing supply (S_s), the market is in equilibrium, providing h_s units of housing services (a function of housing quantity and quality) at rent r_s. Ideally, the short run equilibrium is identical with the long run balance of demand and supply (S_l). The slope of the long run supply curve is determined by the costs of new development, maintenance, and refurbishment. The housing stock expands as long as rental income exceeds costs of housing service production. If rents are limited to a level below the market equilibrium, refurbishment effort is reduced and the deterioration exceeds new housing supply (for a detailed discussion, see, Arnott et al., 1983).

Now consider a situation of housing shortage, for example due to a strong influx of immigrants. Because housing supply is rigid in the short run, rents sharply increase. In absence of rent controls, supply would also increase to the new equilibrium at the intersection of S_l and D. However, as it takes a while to reach the new equilibrium, households that are willing or forced to move have to bear substantially higher cost of living compared to a long run equilibrium situation.x This might raise concerns about

Figure 4: Long- and short-run housing supply under *first-* and *second-generation* rent controls



segregation and social inequality among policy makers who may be inclined to respond by imposing rent control. In *first-generation* rent controls, the maximum rent of r_c would be followed by less construction and refurbishment activity, thus decreasing housing stock over time to an amount of h_c. At rent level r_c, the price signals lower scarcity of housing. Households that would have been forced to leave the market in absence of rent controls still try to find a dwelling in the city. Dwellings are allocated to households by alternative mechanisms than willingness to pay, e.g. queuing, lottery, or nepotism. Overall, the result would be excess demand of the amount $h_d - h_c$ as well as a loss of welfare compared to an unregulated situation, represented by the green triangle.

In many *second generation* rent controls, rent caps are introduced for all existing buildings, while rents for new or recently constructed dwellings are determined on the free market—a setting quite similar to the German regulation. Thus, such regulation divides the housing stock (h_s) into a large regulated part (h_{sc} with a maximum rent of r_c; in the German case dwellings built before 2014/10), and a small unregulated part ($h_s - h_{sc}$, built 2014/10 or later). As the rent level r_c is below the rent required to maintain the entire regulated housing stock h_{sc}, refurbishment and maintenance effort can be expected to suffer, which over time leads to a declining regulated housing stock (h_c)

In the unregulated market segment, homes are allocated by willingness to pay; in the regulated part, let us assume that allocation is random. Further, we assume that households unable to benefit from rent control ($h_{rc} - h_{sc}$) are a randomly drawn subsample from total demand. Then, their maximum willingness to pay still falls into the interval $[r_c, r_{max}]$. Thus, the new demand curve D_u connects the intersection of S_c and the maximum willingness to pay r_{max} with the intersection of r_c and D_u. In our setting, the introduction of the rent cap pushes up the price of new dwellings from r_s to r_{su}, while the rent for existing dwellings drops from r_s to r_c.

In the long run, supply of unregulated dwellings increases to the amount $h_{lu} - h_c$ and rents of unregulated units equal r_{lu}. Overall, the introduction of rent control in a distinct segment of the market can be expected to produce a spread with controlled rents below and uncontrolled rents above the free market's equilibrium rent. A second finding from this representation is that, in the long run, rent control triggers an expansion of the housing stock, increasing welfare in the amount of the blue triangle.[2]

Overall, this representation has several empirically testable implications: With the introduction of rent control, we would expect to observe an immediate decline of rents in the regulated sector, while rents in the free market should increase. In the standard model, prices are determined by future rental income. Thus, we would expect to observe a similar pattern for house prices and increasing prices for building lots, where new unregulated housing can be developed. Finally, we would expect to observe lower refurbishment and maintenance effort, if the rents are capped to a too low level.

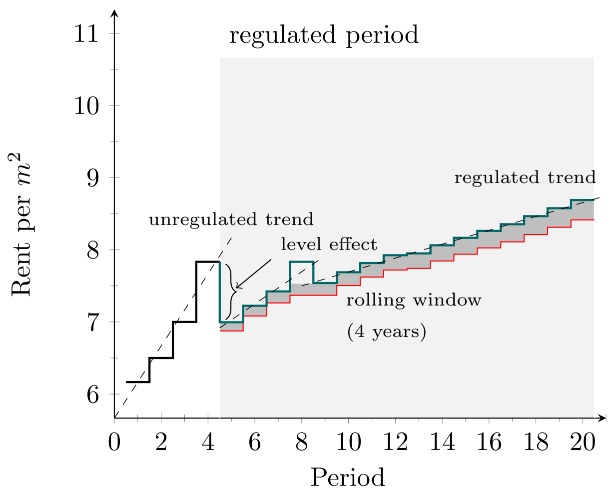
3.2. The specific effects of the German rent cap on rents in new rental contracts

In addition to the general effects of a *second-generation* rent control, there are some specific features of the German rent cap that must be addressed in the empirical strategy: To illustrate how the rent cap affects the development of rents in new contracts, consider a situation with four representative rental contracts that are consecutively signed in periods $t = 1...4$. Each contract is concluded for four periods. Further, we assume an increasing trend of rents in new rental contracts in the periods prior to the introduction of the regulation. In period $t = 5$, the rent cap is imposed and rents in all subsequently

[2] Qualitatively similar results can be expected for spatially neighboring regulated and unregulated sub-markets as long as dwellings provided by these markets are close substitutes.

concluded contracts are capped by the *typical local rent*, which is calculated as an average of the contracts concluded in the previous four periods plus 10%, unless the dwelling was rented out at a higher rate before. In this case, landlords are allowed to charge a rent equal to the rent in the past contract. The *typical local rent* is adjusted in each period.

Figure 5: Stylized representation of the effects of the rent cap over time



Source: own representation.

In our example, the rents concluded in periods one to three are below the average of the first four periods and are adjusted to the legal upper bound in periods five to seven. However, as the rent in the contract signed in period four was already above the rent cap, the landlord is allowed to charge this rent in all subsequent contracts. While the introduction of the rent cap leads to an immediate drop of rents in the first regulated period in our example, it becomes obvious from the graphical representation that in the short run, there still might be considerable rent increases in period–to–period comparison. However, it also becomes apparent that the dynamics are clearly decelerated (as indicated by the dashed trend lines in Figure 5). In the long run, rent increases are tied to the dynamics of the *typical local rent*.

The short run effects of the rent regulation—i.e. the drop of rents and a decelerated dynamic of rent increases after the introduction of the rent cap—depend on the dynamics prior to the introduction: this can be evaluated numerically. Given the short run rigidity of housing supply, an exponential growth model for rents seems plausible to describe the rent dynamics in a limited window around the introduction of a new regulation. This model is essentially the basis of a log-linear regression of rents on a time trend. Let

$$R_t = ae^{\gamma t}, \quad t \in \mathbb{N}_0, \quad a > 0 \tag{1}$$

where R_t is the rent level at t, γ is the constant growth rate, and a is a catch-all term. When time is measured in months, $0 \leq t \leq 47$ is the time period relevant for the calculation of the reference rent in $t = 48$. Under the assumption that the same number of dwellings is traded each month, the rent ceiling at $t = 48$ is defined as

$$\overline{R}_{48} = \frac{1.1}{48} \sum_{t=0}^{47} a e^{\gamma t}. \tag{2}$$

If binding, this ceiling leads to an initial drop in rents upon implementation when it is lower than the rent level at the end of the 4-year-period, R_{47}, i.e. when $\bar{R}_{48} < ae^{47\gamma}$. The γ that equates this expression is approximately 0.00413, i.e. a growth rate of 0.413% per month or approximately 4.8% per year. Below this growth rate, the rent cap will not lead to an immediate drop in the rent level. However, rent increases would be decelerated, from a 4.8% growth rate to approximately 4.5% annually on average in the first year after the introduction of the regulation. For $\gamma \approx 0.00395$ (monthly growth rate of 0.395%) and less, neither an initial drop, nor a decelerated rent dynamic would be the outcome.

Again, this has implications for the empirical strategy. *De facto*, markets are only regulated if the previous monthly rent growth exceeds 0.395%. Only in areas that experienced an average rent growth of at least 0.413%, we expect to observe an immediate drop in rents. As depicted in Figure 10 in the Appendix, this adds additional spatial variation to the data.

4. Empirical analysis

In the empirical analysis, we address the short-run effects of rent regulation. We concentrate on four aspects that should immediately kick in once the rent cap becomes effective. First, as outlined in the previous section, we expect rents to decline with the introduction of the rent cap, then, later, to increase at a decelerated rate. Second, we expect house prices—as a reflection of future rental income—to drop when rent regulations are credibly announced. Third, the change in house prices should capitalize fully in land values. Fourth, as rent controls reduce future returns from investment in the regulated market segment, this should result reduced maintenance effort in the regulated market. Other effects, like for example the allocation of dwellings among household groups, can only be studied in the long run.

To disentangle the general market dynamics from the effects of the regulation, we follow a difference-in-differences approach, as proposed, for example, by Sims (2007) and Autor et al. (2014). In the rent and house price regressions, we exploit the fine-grained temporal information in the data and augment the difference-in-differences approach by a discontinuity design that relies on cubic B-splines.

4.1. Effects on rents

The starting point for the empirical analysis is the assessment of rent dynamics in regulated and unregulated markets. We present two alternatives approaches to assess the impact of the rent cap on the rent level. We begin with the analysis of unregulated new vs. regulated young dwellings. We then proceed with the results for an alternative identification strategy that exploits spatial variation in the regulation. Additional identifying variation stems from the state- and in some cases city-specific start dates of the rent cap (see Table 1).

The results presented in this section rely on advertised rents for dwellings offered on three large online market places between July 2011 and November 2016: *Immonet*, *Immowelt*, and *Immobilienscout24*. Each dwelling's month of offer and postal code is available in the data, together with a long list of housing characteristics. A detailed description of the data can be found in the Appendix. Although concluded rents would be preferable, the alternative of surveyed rents has shortcomings as well: sample sizes are typically small, there might be reporting error and selection issues, and the spatio-temporal information in the data typically is much lower. Moreover, such surveys do not always include the (exact) date the household moved into the dwelling, so that they do not necessarily represent current market rents.[3]

4.1.1. Rents in young and new units

First, we compare the time trends of rents within regulated postal code districts for dwellings that were recently (re-)built—these units are unregulated and serve as control group—and regulated units that are at least two and at most ten years old (treatment group). Two variables indicate whether a dwelling is new: the year of construction and a "first time use" dummy. We define a unit as "new" if it was offered on the market in its year of construction. Observations with building ages between two and ten years that are reported as "first time use" were excluded to reduce measurement error. We estimate the effects separately for postal code districts where the *de facto* regulation (rents increase at a monthly rate of at least 0.0413%) should be followed by an immediate drop in rents, and those regions where the annual rent increases are below the lower threshold of 0.395% monthly rent growth, see section 3.2. The latter districts are *de jure* subject to regulation but remain *de facto* unregulated.

In a first step, we identify regions where the rent cap is *de facto* binding. Therefore, we calculate average (constant quality) growth rates on the postal code district level, by running regressions of the form

$$\log R_i = x_i\beta + \sum_z \left[\rho_z d_{z,i} + \gamma_z t_i \times d_{z,i}\right] + \eta_i, \tag{3}$$

on the sub-sample of regulated units that ends before the rent cap became effective on the local level (May 2015). $\log R_i$ is the logged net rent per square meter of dwelling i and x_i are covariates. t_i is the month of observation for i, and $d_{z,i} = 1$ if i is from postal code z. The regression yields (estimated) postal code-specific growth rates for rents, γ_z, that can be contrasted with the critical values derived in Section 3.2.[4] Figure 10 in the Appendix plots the spatial distribution of past rent growth; the *de facto*-regulated areas colored in red are distributed over a large number of cities all over Germany. Figure 11 in the Appendix plots the distributions of past growth rates in the sample of regulated and unregulated dwellings. The regulation is not binding for observations to the left of the dashed vertical lines. Less than 25% of the observations are *de facto* regulated: only in these districts we would expect an immediate drop in rents.

We exclude postal code districts for which we expect only a reduction in the growth rate of rents (monthly growth rates between 0.395 and 0.413%). This group consists of 3.7% of the sample observations. We also drop 631 observations for which we were not able to calculate postal code district-level rent growth rates due to an insufficiently small number of observations. Furthermore, we focus on city regions[5] in which observations of at least one postal code district are *de facto* regulated. This reduces the sample size substantially, from 316,606 to 224,176 observations.

We then compare the monthly growth rates of rents for regulated units—the treatment group—and new units—the control group—around the introduction of the rent cap. This is a difference–in–differences

[3] Basten et al. (2017) use similar data.

[4] Equation (3) was estimated separately for four German regions (south-east, south-west, north-east, north-west) for computational practicability. Results are available on request.

[5] These city regions are plotted in Figure 12 in the Appendix.

strategy that asks how the rent cap affects the level of rents in treated relative to similar non-treated units. In a regression framework, the strategy translates into the following estimating equations:

$$\log R_i = x_i\beta + \rho_{z_i} + f(t_i;\mathrm{tr}_i) + \delta_0\mathrm{tr}_i + \delta_1\text{rent cap}_{t_i} + \delta_2(\mathrm{tr}_i \times \text{rent cap}_{t_i}) + \eta_i, \tag{4}$$

where ρ_{z_i} is a postal code district-fixed effect. $\mathrm{tr}_i = 1$ if the observation belongs to the treatment group and rent $\mathrm{cap}_{t_i} = 1$ if the rent cap was effective in month t_i. $f(t_i;\mathrm{tr}_i)$ is a cubic B-spline with six equidistant "knots" (interval boundaries) located at months $10, 20, ..., 60$ of the sample period (months 1–65) that is estimated separately for treatment and control observations.[6] B-splines are "piecewise" polynomials fitted on a near-orthogonal base that allow coefficients to change at each knot. To avoid unrealistic spikes in the fit, splines impose additional restrictions on the first (quadratic splines) and second derivative (cubic splines) at the interval boundaries, resulting in a differentiable fitted line that resembles a higher-order polynomial, but consumes considerably fewer degrees of freedom. Because of their flexibility in a (small) window around the treatment date, where splines capture continuous changes in the trends of treatment and control groups quite well, they help to identify δ_1 and δ_2 in a way similar to a regression discontinuity design. A caveat is the behavior of the fitted line at the end-points where the fit often increases or decreases sharply. The reason is that there are no restrictions on the derivatives at these points, meaning that very few data points can have considerable impact on the fit. Arguably, this also shows up in some of the results presented below (see Berk, 2008, chapter 2 for an introduction to splines).

The (gross) treatment effect is given by δ_2. It allows for a sudden drop in rents of regulated units at the activation date, relative to the general change in rents at that date, δ_1. This gross effect consists of a net effect on regulated units, $\delta_2 - \delta_1$, and potential spillovers from regulated to unregulated market segments, δ_1 (as outlined in Section 3.1). The flexible B-splines will also capture more casual spillovers between the groups that are not necessarily related to the rent cap, without identifying these directly. To gauge whether the effects are permanent, one needs to take into account the behavior of the cubic splines over time.

The estimated treatment effects are presented in Table 2, covariate results are in Table 10 in the Appendix. Additionally, we provide graphical illustrations of the trend of rents in treatment and control groups (see Figure 6). Standard errors are cluster-robust on the level of postal code districts.

Model (1) focusses on *de facto* regulated postal code districts. The left graph in Figure 6 shows that the trends in treatment and control group are highly correlated prior to the date of the coalition agreement. Remarkably, the lines deviate increasingly after that date, with a stronger rent increase in the treatment group. It seems that landlords of regulated dwellings tried to secure a higher baseline rent before the introduction of the rent cap—the rent cap never requires the reduction of rent from one tenant to the next. This would be in line with a theoretical model of search and matching where landlords trade higher vacancy risk for higher future rental income streams. At the treatment date, there is a slight increase of 2.9% in both the treatment and the control group upon activation, but a much larger and highly significant decrease for regulated units once the rent cap was effective in the respective

[6] We also considered quadratic splines, twelve equidistant knots, and polynomials, with similar results (see below). We chose this cubic spline specification because it scored lowest on the Bayes Information Criterion.

Table 2: Regression results: effects on rents, young vs new units

	Dependent variable: log rent	
	de facto regulated regions	*de facto unregulated regions*
	(1)	(2)
a) Baseline		
regulated	0.007	−0.009
	(0.012)	(0.008)
rent cap effective	0.029^{*}	−0.009
	(0.013)	(0.009)
b) Treatment Effects		
regulated × rent cap effective	-0.058^{***}	0.012
	(0.015)	(0.010)
adj. R^2	0.881	0.882
Observations	79847	144329

Both models use cubic splines with six knots at months 10, 20, ..., 60. Model 1 (2) is estimated for the sub-sample of postal codes with high (low) rent growth.
Clustered standard errors in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

municipality (-5.8%). Consequently, the net effect on regulated units is about 2.9%. As expected, this difference increases slightly over time.

So far, the analysis focuses on postal code districts that experienced high rent growth prior to the introduction of the rent cap. Model (2) is similar to model (1), but was estimated for postal code districts that were *de facto* unregulated. Remarkably, the treatment effect disappears entirely. The second graph in Figure 6 confirms that—at best—there is a slight smooth reduction in the rent trend around the time the law passed the Bundestag, but there is no level effect at the time the regulation became effective.

Figure 6: Effects of the rent cap on regulated rents

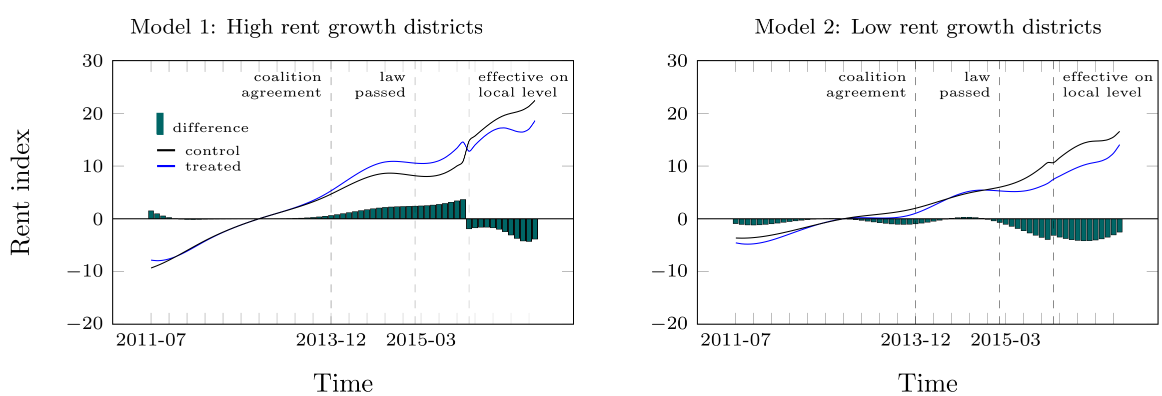


Table 13 in the Appendix reports several alternative specifications. Generally, the results are highly robust. The corresponding rent indices can be found in Figure 13. Models (R1) to (R4) vary the parameters of the B-splines (degree; number and placement of knots). Although the effect on the control group turns insignificant in these specifications, it remains relatively stable. The interaction effect ("regulated × rent cap effective") is highly significant and almost constant across the four models. Model (R5) controls for the trend in rents via a (less flexible) 4^{th}-order polynomial, with similar results. (R6) allows for adjustments prior to the activation of the rent control; this does not change the results qualitatively, but standard errors increase substantially. Furthermore, there does not seem to be a sharp announcement effect for rents.

There are a substantial number of renovated and retrofitted dwellings in the treatment group. In case retrofitting or renovation expenses exceed one-third of the construction costs of a comparable new dwelling, these dwellings are exempt from the regulation temporarily. Thus, our results might be biased downwards through false assignments. Therefore, we exclude all units that were offered as renovated or retrofitted in model (R7). The treatment effect remains stable and highly significant.

Finally, there might be sample composition effects. If the share of observations from a postal code district with relatively high rent levels is extraordinarily high in some months—as might be the case if a large construction project is finalized—this would lead to spikes in the growth rate of rents in these postal code districts. As a solution, the sample in model (R8) is weighted so that the share of observations from a certain postal code district is stable across all months and, simultaneously, the relative size of the treatment group remains stable in each postal code district. This weighting decreases the treatment effect slightly, to -4.5%, while the effect on the control group is marginally significant in this specification (-2.4%).

4.1.2. Effects on rents across space

This section presents results from an alternative identification strategy that relies on variation of the regulation across space. According to our main model from the previous section, rents of regulated units experienced a net decrease of approximately 2.9% whenever the regulation was binding, while the rent level for unregulated units in these areas jumped by 2.9% (although this latter result is insignificant in some robustness checks). This is in line with the theoretical discussion in section 3.2. The net effect on regulated units should also show up in a comparison across space. Whether or not this is accompanied by an increase in the rent level of unregulated areas depends on the degree to which dwellings from unregulated and regulated areas are substitutes. In any case, this second approach is an important robustness check because it exploits a different type of variation and relies on a different part of the sample.

We again drop observations from the sample if the city-region did not contain a postal code district with a *de facto* binding regulation or if we were unable to calculate the past rent growth rate. The estimating equation is similar to eq. (4), but tr_{t_i} now refers to spatial variation in the regulation (see below). As a cautionary note, the time trends in treatment and control groups do not move in parallel. However, this is not necessary for identification because the flexible cubic splines are able to handle any smooth deviations from common trends. For that reason, it is sufficient to rule out unobserved discrete changes in the trend around the treatment dates. The results are presented in Table 3 and Figure 7.

Model (1) is estimated based on the same treatment group as the regressions from Table 2, but the control group consists of regulated units from postal code districts where the rent cap is *de facto* not binding. While there is a zero common effect on the activation date, the treatment effect is significant and negative. The estimated effect of -2.7% approximately equals the net effects found in the previous section. Model (2) re-estimates model (1) for the sample of dwellings older than ten years. The treatment effect becomes only slightly stronger. Thus, the effects seem to be quite homogeneous within the group of *de facto*-regulated units.

Thus far, the analysis excludes postal code districts where only a reduction in the growth rate of

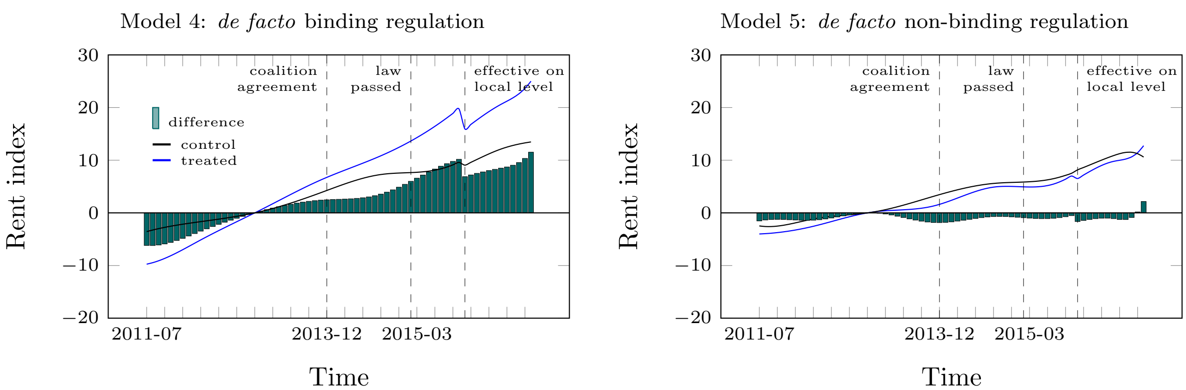
Table 3: Regression results: effects on rents across space

	Dependent variable: log rent					
	within rent cap municipalities			*rent cap and adjacent munic.*		
	(1)	(2)	(3)	(4)	(5)	(6)
a) Baseline						
rent cap effective	0.001	-0.011^{**}		-0.010^{*}	0.002	-0.012^{**}
	(0.005)	(0.004)		(0.004)	(0.007)	(0.004)
b) Treatment (drop)						
regulated–treated (drop)	-0.027^{*}	-0.033^{***}		-0.037^{***}		
$\times$ rent cap effective	(0.011)	(0.008)		(0.008)		
c) Treatment (kink)						
trend $\times$ rent cap effective			-0.00007			
			(0.00007)			
regulated–treated (kink) $\times$ trend			-0.00047			
$\times$ rent cap effective			(0.00024)			
d) Pseudo-treatment						
rent cap municipality $\times$ rent cap effective					-0.013	0.003
					(0.008)	(0.006)
adj. R^2	0.854	0.780	0.807	0.790	0.851	0.806
Observations	98420	470148	389920	364725	138220	336097

All models include cubic splines in the time trend with ten knots at months 10, 20, ..., 60, estimated separately for treatment and control groups. Models (1) to (3) focus on rent cap municipalities. The sample in model (1) consists of dwellings from rent cap municipalities that are 2–10 years old, model (2) uses dwellings older than ten years. The treatment group in both models are units that experienced monthly rent growth above 0.413%, the control group experienced rent growth below 0.395% per month. Model (3) does not restrict building age. The control group is similar to models (1) and (2), the treatment group experienced rent growth between 0.395 and 0.413% per month. Models (4) to (6) compare rent cap postal codes with unregulated postal codes. Control units are from unregulated postal codes that neighbor directly with a regulated postal code; the treatment group of model (4) comes from postal codes where the regulation is binding. In models (5) and (6), the regulation is non-binding in treated postal codes. Model (5) focusses on buildings not older than ten years, model (6) on postal codes that border directly with a postal code from the control group.

Postal code-clustered standard errors in parentheses; $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Figure 7: Regulated and *de facto* unregulated areas: effects on rents (see Table 3)



rents (a "kink" instead of a "drop") can be expected. Such a reduction should follow if the pre-activation monthly growth rate of rents falls within the (small) interval [0.395%, 0.413%]. In this regression, we use dwellings of all vintages and the same definition for the control group as in models (1) and (2). The treatment group consists of postal code districts where the monthly growth rate of rents fell into this interval. Instead of a treatment dummy, we include an interaction of t_i with the date the rent cap became effective, and an interaction of this term with the treatment group indicator. Further, we drop all spline knots after June 2015, the very first treatment date in the sample. The coefficient on the latter term measures whether the trend in the treatment group changed relative to the trend in the control group. The results suggest a reduction of the monthly growth rate at the treatment dates in the treatment group, of -0.047 percentage points (marginally significant). Over the course of a year, this implies an effect of approximately -0.6 percentage points. Note, however, that there likely is a considerable error in the allocation of postal codes to treatment and control groups because there is no "buffer" between the two groups—in other regressions, this buffer is the interval [0.395%, 0.413%]. This likely biases the estimate towards zero.

Models (4) to (6) compare rents from regulated and unregulated (control group) postal code districts. In model (4), the regulation in the treated postal code district is binding. Again, there is a highly significant reduction of -3.7% in the treatment group. The graphical output (see the first graph of Figure 7) confirms that the growth rate of rents was much higher in areas with a binding rent ceiling. However, the difference to the control group seems to follow a linear trend that is interrupted only by the treatment effects. Models (5) and (6) serve as placebo checks. The treated postal code districts have a non-binding regulation. In model (5), the sample is restricted to buildings not older than ten years; in model (6) the treatment group is restricted to postal codes directly neighboring a control postal code district. The treatment effect is insignificant and small in both cases. This is also confirmed by the second graph of Figure 7.

In summary, the results from Tables 2 and 3 suggests that the rent cap had a substantial and intended effect on rents. This effect, however, is restricted to areas where the regulation was binding. In terms of size, the results are also in line with expectations: The mean expected reduction in the rent level in areas with a binding regulation is 3.2%.[7] This implies that, on average, landlords and real estate agents were well-informed about past growth rates.

4.2. The effects on house prices

A binding rent regulation immediately reduces the value of future rental income streams and should be capitalized in house prices. The uncertainty inherent in the announcement of a rent control regime alone should reduce the price of regulated dwellings. In this section, we evaluate the short run effects of the rent cap on investors' expectations by considering the regulation's effect on prices. We rely on a sample of listing prices that stem from the same data source and share the same characteristics as the rent data described in section 4.1.

Transacted prices are superior to listing prices, but we are confident that the results are not biased by this choice, for the following reasons: While there can be significant differences between the transaction price and a first offer, the literature points out that systematic mis-pricing can be very costly to sellers of real estate (Knight et al., 1994; Knight, 2002; Merlo and Ortalo-Magné, 2004). Existing empirical evidence suggests that trends and turning points are captured quite reliably by listing price indices (Lyons, 2013). House prices in the German cities under consideration grew constantly, at least since 2011. Therefore, it is likely that listing prices and offered rents are reliable proxies for transaction prices in our case.

As in the previous section, we follow a two-pronged strategy, exploiting both variation between regulated young and unregulated new dwellings as well as variation between regulated and unregulated areas. A potential threat to the validity of the first strategy are investor expectations with respect to future rent control. The introduction of rent control might lead investors to expect even stricter regulation in the future, which in turn would influence prices of unregulated new dwellings already today. The second

[7] According to the arguments presented in section 3.2, the expected effect can be calculated as $\Delta = 1.1/48 \times (exp(-48\gamma) - 1)/(1 - exp(\gamma))$, see equation (2).

strategy faces the problem that expected rental income streams differ across space (see Figure 7). With these obstacles in mind, we focus on the general picture.

We concentrate the analysis on regulated buildings that are between two and 40 years old. On the one hand, very old buildings have a propensity to be re-built in the near future and, thus, are less likely to be affected by the rent cap. On the other hand, we chose not to use the 2–10 years bracket from the rent regressions because a dwelling sold shortly after its construction might have serious defects.

4.2.1. Prices of regulated and new dwellings

The model to be estimated in this section takes on the following form:

$$\log P_i = x_i\beta + \rho_{z_i} + f(t_i;\mathrm{tr}_i) + \delta_0\mathrm{tr}_i + \delta_1\text{rent cap}_{t_i} + \delta_2(\mathrm{tr}_i \times \text{rent cap}_{t_i}) + \eta_i, \tag{5}$$

where P_i is the listing price of dwelling i. To account for potential anticipation effects, we estimate the impact of the rent cap on house prices at different points in time. The first time investors might have seriously considered the rent regulation to become reality was in December 2013 when the Social Democrats and the conservative Christian Democrats signed a coalition agreement. Moreover, the agreement clarified that only existing buildings would fall under the regulation. 14 month later, on March 11$^{\text{th}}$ 2015, the regulation passed the German Bundestag. This removed uncertainty that stemmed from the fact that the Christian Democrats had only accepted the rent cap as a concession to its coalition partner. Since June 2015, the federal states are empowered to implement the rent cap in municipalities with tight housing markets—the introduction on the local level again reduced uncertainty for investors.

The results are reported in Table 4. Figure 8 displays the corresponding price trends.

Table 4: Regression results: effects on prices, regulated vs new units

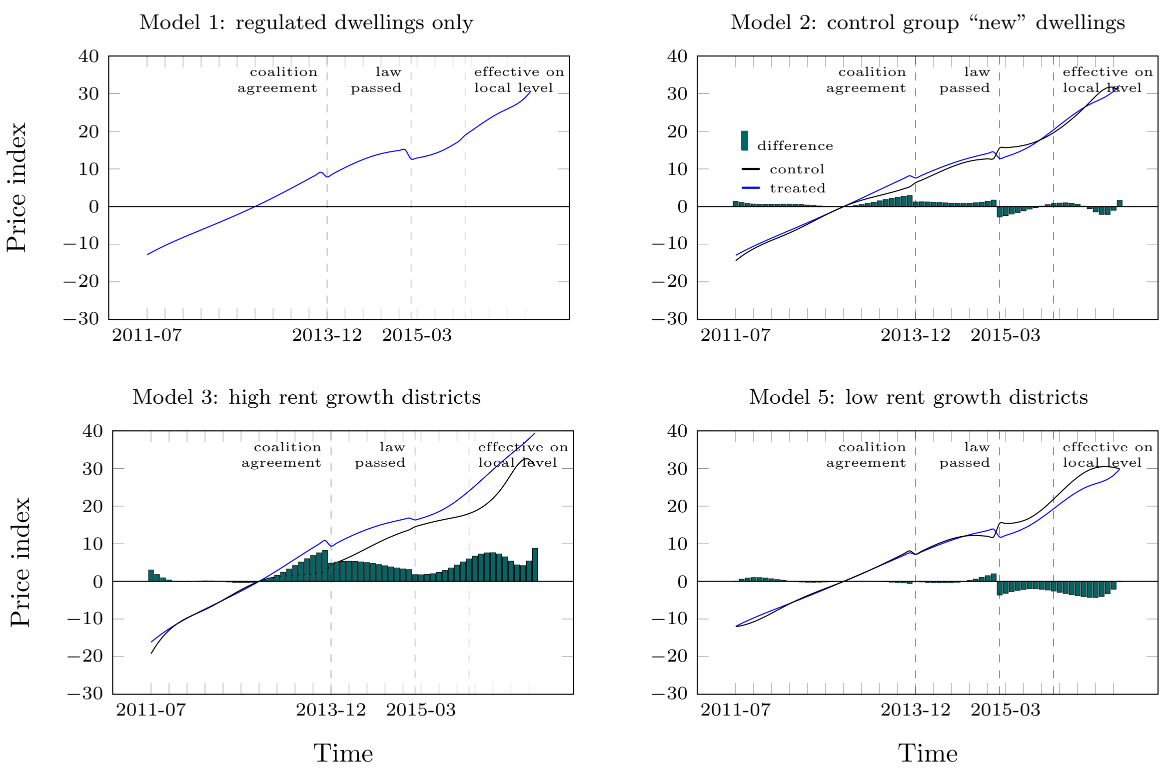
	Dependent variable: log listing price			
	all postal codes		*high rent growth postal codes*	*low rent growth postal codes*
	(1)	(2)	(3)	(4)
a) Baseline				
regulated	-0.284^{***}	-0.070^{***}	-0.046^{*}	-0.086^{***}
	(0.013)	(0.014)	(0.023)	(0.016)
after coal. agr., before law passed		0.005	0.014	−0.017
		(0.015)	(0.024)	(0.017)
after law passed, before rent cap effective				
rent cap effective				
after law passed		0.032	0.017	0.020
		(0.018)	(0.027)	(0.020)
b) Treatment				
regulated × after coal. agr., before law passed	-0.021^{**}	−0.018	−0.038	0.006
	(0.007)	(0.017)	(0.031)	(0.019)
regulated × after law passed, before rent cap effective	-0.048^{***}			
	(0.010)			
regulated × rent cap effective	-0.043^{***}			
	(0.013)			
regulated × after law passed		-0.066^{**}	−0.050	-0.056^{*}
		(0.020)	(0.036)	(0.023)
adj. R^2	0.803	0.820	0.829	0.819
Observations	305710	305710	91161	203679

All models include cubic splines in the time trend with ten knots at months 10, 20, ..., 60, estimated separately for treatment and control groups. The control group consists of dwellings offered for sale in the year of construction. The treated units are at least two and at most 40 years old. Postal-code clustered standard errors in parentheses; $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Model (1) is estimated based on regulated units from regulated postal code districts only. Our results indicate that there are two significant drops in the price trend in regulated areas. Directly after the coalition agreement, prices fell by approximately 2.1%. An additional drop of 2.7% occurred once the law passed the Bundestag. By contrast, there is no additional effect visible once the rent cap became effective. This suggests that investors anticipated the implementation of the regulation quite well.

Model (2) adds newly constructed dwellings as a control group. Further, we combined the two dummies "after law passed, before rent cap effective" and "after rent cap effective" into a single dummy that is equal to one for the entire period. Qualitatively, the results are similar, although the initial drop upon the announcement is insignificant in this regression. The corresponding graph shows that the identified short-term drop in prices of regulated dwellings *relative to prices of new dwellings* might not be permanent—at least, it is reduced temporarily several months later. Interestingly, the prices in treatment and control group increased by 3.2% (marginally significant) when the law passed. This matches the corresponding results for rents quite closely. Note, however, that this latter effect is only identified through the cubic splines in the trend.

Figure 8: Effects of the rent cap on housing prices, new vs. young dwellings



The other two models in Table 4 focus on the two subgroups of postal code districts where the regulation was *de facto* binding in June 2016 and where it was not (excluding postal code districts where monthly rent growth was between 0.395 and 0.413%). They show that the results from model (2) are not driven by areas where the regulation is binding. The results remain stable, but estimation precision is much lower in the case of high rent growth districts.

There are remarkable differences in the behavior of the price trends. The somewhat erratic development of prices in the control group from model (3), starting in 2013, suggests that the rent cap altered expectations about the political feasibility and probability of stricter rent regulation in Germany. In other words, there might be a time inconsistency problem if investors are uncertain that future governments will stick to the promise to exempt dwellings built in 2015 or 2016 from the regulation. An additional interesting feature is the slow-down of price growth in the control group before the coalition agreement. At that time, the rent cap was part of the Social Democrats' election campaign. When the plans were clarified in the coalition agreement, treatment and control group moved closer together.

Several alternative specifications are reported in Table 13 in the Appendix. Models P1 to P4 focus on high growth postal code districts. P1–P3 vary the degree and the number and placement of knots for the B-splines. The most important coefficients remain stable, although the statistical precision is low in all of these models. P4 considers a composition-constant weighting scheme, analogous to the one used in rent regression R8, see Table 12. It ensures constant postal code district–month shares and constant treatment group shares within each postal code–month combination. In this regression, the (short-run) effect is stronger and highly significant, but there are very large swings in the price trends. Models P5 to P8 repeat the exercise for postal code districts where the regulation was *de facto* not binding in June 2016. Compared to the main results from Table 4, the treatment effects become stronger.

Taken as a whole, the price regressions are compatible with the results found for rents, both qualitatively and quantitatively. However, part of the house price behavior might be related to long-run expectations about stricter rent regulation that would reduce returns on investments in housing even further. In this interpretation, the fact that the law passed the Bundestag is a signal that tighter rent control is politically feasible. Thus, from the perspective of landlords, the hazard of rent regulation increased with the introduction of the rent cap. Crucially for our analysis, this likely influenced investor expectations with respect to regulated, old dwellings *and* new dwellings that—at the moment—are exempt from the regulation. Therefore, we now turn to the second identification strategy that exploits spatial variation in the regulation.

4.2.2. Effects on prices across space

We again focus on the effect of the coalition agreement and of the legislation in parliament. We do not consider additional effects on the dates rent control came into force. The model is similar to eq. (5); tr_{t_i} now refers to areas where the regulation is effective. The results can be found in Table 5 and Figure 9.

Model (1) in Table 4 relies on the whole sample of regulated units with building ages between two and 40 years. It confirms the result from Table 4. The results are fairly robust to restricting the treatment area to postal code districts at the borders of the regulated municipalities (model (2), marginally significant) or to postal code districts with a binding regulation (model (3)). The corresponding graphs of the time trend all show similar patterns, so we concentrate on the graphical representation of model (1) (see Figure 9). Prices in the control group follow a continuous trend, whereas there are two sharp jumps in the price trend of the treatment group. It is also clear that prices increased much faster in the treatment than in the control group. Despite this fact, the pattern of the price development was very similar in the two groups at least up to the date of the coalition agreement. This suggests that the identifying assumptions hold.

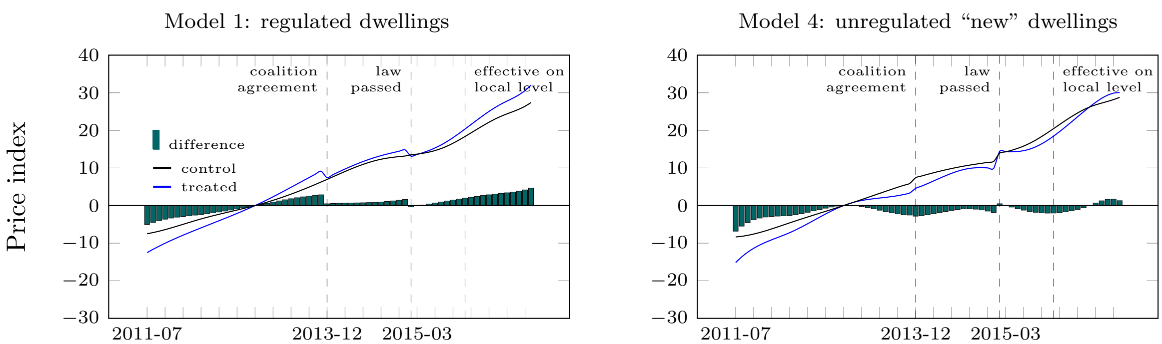
Table 5: Regression results: effects on prices across space

	Dependent variable: log listing price					
	regulated units			*unregulated units*		
	all areas (1)	edge (2)	high growth (3)	all areas (4)	edge (5)	high growth (6)
a) Baseline						
after coal. agr., before law passed	−0.000	−0.001	−0.001	0.010	0.008	0.011
	(0.009)	(0.009)	(0.009)	(0.014)	(0.015)	(0.015)
after law passed	0.001	0.001	−0.001	0.029	0.028	0.033
	(0.014)	(0.014)	(0.014)	(0.022)	(0.023)	(0.023)
b) Treatment						
rent cap municipality	-0.025^{*}	−0.012	-0.051^{**}	−0.003	0.010	0.005
× after coal. agr., before law passed	(0.011)	(0.013)	(0.019)	(0.019)	(0.024)	(0.025)
rent cap municipality	-0.047^{**}	−0.035	-0.060^{*}	0.024	0.022	0.008
× after law passed	(0.017)	(0.019)	(0.025)	(0.027)	(0.032)	(0.032)
adj. R^2	0.885	0.856	0.872	0.860	0.840	0.862
Observations	245875	137027	104496	161770	55864	78242

All models include cubic splines in the time trend with ten knots at months 10, 20, ..., 60, estimated separately for treatment and control groups. Regulated units are at least two and at most 40 years old, unregulated units were offered in the year of construction. Models (1) and (4) use the whole sample, models (2) and (5) restrict the sample to regulated postal code areas that directly border a postal code area of the control group, and models (3) and (6) restrict the treatment group to postal code areas with a binding regulation.

Postal-code clustered standard errors in parentheses; $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Figure 9: Effects of the rent cap on housing prices across space



The other models repeat the above exercise for the sample of unregulated new dwellings. Overall, we do not find any significant effects related to the introduction of the rent cap. As visible in the graph of model (4) (see Figure 9), prices in the treatment group show more volatile behavior beginning in 2013, while prices in the control group followed a relatively steady trend. This points to the sensitivity of real estate developers to the political and judicial uncertainty induced by the regulation, although the results do not allow to reach a more definite conclusion on this issue.

4.3. Effects on land values

To complement the results from the previous section, we investigate whether land values in regulated municipalities increased in response to the rent cap, as suggested by the stylized model in Section 3.1. According to theory, undeveloped land becomes more attractive in rent cap municipalities because units in new buildings do not fall under rent control and c.p. yield higher returns—see Section 4.1.1.

Unfortunately, there are only aggregate transaction data available. We rely on sales of developed vacant plots of land in Bavarian municipalities in the years 2010–2016, provided by the Statistical Office of Bavaria. The rent cap was implemented in 137 of 2056 Bavarian municipalities on August 1, 2015 and January 1, 2016.[8] Unfortunately, municipality-level data from other German states are not available. The data contain information on the number of sales, the total area sold, and the average price per square meter by year and municipality. We construct land price indices on the municipal level by dividing the yearly land prices in each municipality by the municipality's average land price in the period 2010–2012.[9] This municipality-level index is our dependent variable. The regression equation reads

$$y_{it} = \psi_i + \phi_t + \gamma \text{size}_{it} + \delta_1(\text{tr}_i \times \mathbf{1}(t = 2015)) + \delta_2(\text{tr}_i \times \mathbf{1}(t = 2016)) + \eta_{it}. \tag{6}$$

ψ_i and ϕ_t are municipality and year fixed effects. size_{it} is the average number of square meters per sale. This variable captures two aspects: First, large plots of land tend to be located at the outskirts of the city, while smaller plots are more common in central places. Secondly, there might be gains to selling multiple lots at once (lower per-unit administrative costs, returns to scale of land development), which also likely reduces the selling price per square meter. As before, tr_i is a treatment group dummy. $\mathbf{1}(\cdot)$ is the indicator function, so that δ_1 and δ_2 capture potential effects of the rent cap in the years 2015 and 2016. We run that regression on the sub-sample from the years 2014–2016.

On average, land prices in Bavarian municipalities without a rent cap were almost flat throughout the entire period, while they increased steadily in rent cap municipalities during that time. Therefore, we use trimming and propensity score weighting to construct more comparable treatment and control groups in terms of covariates and pre-treatment price trends. To that end, we estimate a logit model with tr_i as the dependent variable and the number of unemployed per resident, population density, the residential area share, the number of jobs per resident (all in 2011), the average land price in 2010–2012, and the change from that average to the land price in 2014 as explanatory variables. The results are presented in Table 14 in the Appendix. We then drop all observations with predicted probabilities p_i of treatment below 10 or above 90% and use $p_i(1 - p_i)$ as propensity score weights.

Table 6 summarizes the estimation results for equation (6). In model (1), we use the whole sample for the years 2014–2016. C.p., larger plots of land sell at a lower price per square meter, as expected. Furthermore, prices increased in 2015 and 2016 in both treatment and control group. Prices in rent cap municipalities increased by an additional ten (22) percentage points between the years 2014 and 2015 (2016). In model (2), we drop all municipalities with estimated probabilities for treatment below 10 or above 90%, leaving us with 59 treated and 77 control municipalities. The coefficients shrink slightly and turn insignificant. In model (3), we weigh the trimmed sample by propensity score weights. Model (4) further adds covariates (business starts per resident, children in childcare divided by total childcare capacity, unemployed per resident). The treatment coefficients are very similar to the respective coefficients from model (1). Figure 15 in the Appendix shows that the pre-treatment trends in treatment and control groups of the trimmed and weighted sample are fairly similar, while a distinct difference emerges in the years 2015 and 2016. Models (5)–(7) consider the log price per square meter of buildable land as dependent variables. Model (5) uses the whole sample, (6) the trimmed and weighted sample, and (7) pools the two coefficients of main interest. Model (8) uses the raw price per square meter on the

[8] The rent cap was introduced in 144 municipalities on August 1, 2015. Nine municipalities were added on January 1, 2016, and 16 were removed, see Table 1.

[9] We use a period of three years as a base period because there are many municipality-years without sufficiently many sales—the price is not reported because of data protection in these cases.

Table 6: Effects on Bavarian land prices, 2014–2016

	Dependent variable							
	Index				*log Land Price per* m^2			*Land Price/*m^2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
sqm per sale	-0.09**	-0.29***	-0.32***	-0.31***	-0.14***	-0.28***	-0.28***	-97.21***
	(0.03)	(0.05)	(0.06)	(0.05)	(0.02)	(0.04)	(0.04)	(20.41)
business starts per resident				10.03		9.83	9.13	2926.00
				(12.08)		(8.46)	(8.41)	(4213.87)
childcare capacity				-0.53		-0.39	-0.33	-185.11
				(0.41)		(0.29)	(0.26)	(129.76)
unemployed per resident				-0.95		7.08	5.43	-1120.05
				(21.27)		(14.51)	(14.84)	(7446.35)
year 2015 dummy	0.07***	0.03	0.03	0.04	0.05***	0.03	0.02	16.25
	(0.02)	(0.05)	(0.06)	(0.06)	(0.01)	(0.05)	(0.04)	(18.39)
year 2016 dummy	0.16***	0.22***	0.22***	0.22*	0.13***	0.14*	0.16**	71.49*
	(0.02)	(0.06)	(0.06)	(0.09)	(0.01)	(0.06)	(0.06)	(28.71)
rent cap × year 2015	0.10*	0.06	0.10	0.09	0.09**	0.09		23.36
	(0.05)	(0.08)	(0.08)	(0.08)	(0.03)	(0.06)		(27.61)
rent cap × year 2016	0.22***	0.18	0.21*	0.23*	0.14**	0.14		72.02*
	(0.06)	(0.10)	(0.09)	(0.10)	(0.05)	(0.08)		(31.18)
rent cap × year 2015–16							0.11*	
							(0.06)	
Observations	2417	263	263	263	3003	263	263	263
Trimming	no	yes	yes	yes	no	yes	yes	yes
Propensity score weights	no	no	yes	yes	no	yes	yes	yes
Treated municipalities	137	59	59	59	137	59	59	59
Control municipalities	1919	77	77	77	1919	77	77	77

***: p < .001, **: p < .01, *: p < .05.

trimmed and weighted sample. Generally, these robustness checks agree with the results found for the land price index, although effect sizes are slightly smaller. Regressions that drop the year 2015 produce very similar results that can be obtained from the authors—we do not report them here to save space.

The average index value in rent cap municipalities was 1.3 in 2014. Thus, according to the point estimates from Table 6, land values in these municipalities increased by 11–18% on average. Table 2 suggests that future income streams from newly built rental units increased by approx. 2.5% due to the introduction of the rent cap. To make sense of these numbers, denote the total house value by p and the value of the land by θp. At face value, these numbers are sensible if θ is in the range 0.11–0.18. Because the land sales also include plots of land designated for commercial or industrial use, the estimated change in land values should be even larger when estimated from sales of land for residential use only. As a proxy for the share of residential land sales, we calculate the change in land use from 2010 to 2014 according to three ATKIS classifications (residential, commercial, industrial).[10] The average of the 59 treated observations is 70%.[11] Thus, we would expect that the true θ of multi-family buildings in these municipalities lies somewhat below (or at the lower end of) the 0.11–0.18 range.

To obtain a comparable estimate for θ in rent cap municipalities in the trimmed sample, we calculate the price per square meter of living space for dwellings built and offered for sale in 2013 and 2014 from the listing price data.[12] We then divide the municipality's average land price per square meter in 2013 and 2014 by the average price per square meter of living area and average over all municipalities in the treatment group. The resulting share is 0.11.[13] The average building height is 2.76 floors. Unfortunately,

[10]Municipal-level ATKIS land use in square meter is provided by the Bavarian Statistical Office.

[11]This does not account for the possibility that the rent cap made residential land more attractive relative to other uses of land, which would increase the share.

[12]We have listing price data for 24 of the 59 treated municipalities.

[13]Most likely, the land of buildings completed in 2013/14 was bought by the developer in 2010/11/12 rather than in the year of construction completion. If we account for the fact that land values increased substantially between 2010/11/12

we do not have information about the typical lot and ground floor size. Nevertheless, it seems plausible that lot sizes of typical apartment buildings are not much larger than 2.76 × ground floor size. Furthermore, buildings might have additional living space under the roof or below ground floor. Overall, this suggests that the estimated effect on land values is of reasonable magnitude.

4.4. Effects on landlords' maintenance decisions

The preceding sections show that the rent cap had the intended effects on rents and corresponding effects on prices. This section deals with the unintended consequences of the rent cap. We consider all municipalities subject to the rent cap, and do not restrict the sample to municipalities that contain at least one postal code district where the regulation is *de facto* binding.

In particular, we estimate linear probability models[14] for all regulated dwellings (not new or substantially retro-fitted) to evaluate whether the rent regulation influenced landlords in their maintenance decisions, as it is often argued in the literature. Specifically, we model the likelihood that a dwelling was advertised as refurbished or retrofitted. The rent control regime discussed in this paper allows free negotiation of rents for the first contract after a substantial refurbishment, if expenses exceed one-third of today's reconstruction costs of the dwelling. However, there is no such rule for smaller renovations, which are important to keep the quality of buildings constant over time.

Letting y_i denote the outcome for observation i, we estimate

$$Pr(y_i = 1) = x_i\beta + \rho_{d_i} + \kappa_{q_i} + \delta_1 \text{rent cap}_{t_i} + \delta_2(\text{tr}_i \times \text{rent cap}_{t_i}) + \eta_i. \tag{7}$$

x_i is a vector of housing and location characteristics that is described in Table 9. ρ_{d_i} is a (district× treatment group) fixed effect, and κ_{q_i} is a quarterly fixed effect. As before, tr_i takes on the value 1 if observation i is located in a regulated municipality (treatment group). rent cap_{t_i} is equal to 1 if the regulation is in force in month t_i. This can either be in the ith municipality, if i is a treated observation, or in an adjacent municipality, if i is a control observation. Technically, δ_1 is identified because the treatment date varies over municipalities. The coefficient δ_2 of the interaction of both dummies captures the effect of the rent cap. As in the price regressions, we also allow for anticipation effects at the dates of the coalition agreement and adoption of the law.

Models (1) and (2) in Table 7 test whether landlords adjusted refurbishment effort. The announcement of the coalition agreement reduced the probability that a dwelling was offered as refurbished by 1.5 percentage points; a stable result until the date the rent control came into force. Our estimations indicate a slightly lower level effect after this date (-0.8 pp.). These effects are substantial, given that only 7.3% of all dwellings were offered as refurbished on average. Model (2) exploits the variation within controlled municipalities across postal code districts with low and high rent growth between July 2011 and May 2015. All three treatment effects are insignificant and small, suggesting that, in the short run, there is only little difference in the maintenance effort of landlords in postal code districts where the regulation *de facto* binding and other regulated postal code districts.

and 2013/14 (see Figure 15), the share drops to ∼0.08–0.09.

[14] We estimate linear models because we are interested in average effects and because of the lower computational burden of fixed effects estimation.

Table 7: Linear probability models:

Dependent variable	*refurbished*		*renovated*	
	(1)	(2)	(3)	(4)
rent cap municipality × after coal. agr., before law passed	-0.015^{***}		-0.002	
	(0.003)		(0.004)	
rent cap municipality × after law passed, before rent cap effective	-0.016^{***}		-0.010	
	(0.004)		(0.006)	
rent cap municipality × after rent cap effective	-0.008^{**}		-0.011^{*}	
	(0.003)		(0.005)	
high rent growth × after coal. agr., before law passed		-0.001		-0.002
		(0.006)		(0.005)
high rent growth × after law passed, before rent cap effective		-0.001		-0.007
		(0.007)		(0.007)
high rent growth × after rent cap effective		-0.004		0.002
		(0.007)		(0.006)
adj. R^2	0.111	0.115	0.069	0.071
Observations	800003	645030	911679	735608
Mean dep. var	0.073	0.080	0.187	0.194

Standard errors are clustered at the postal code level.
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Renovations are an even cheaper way of prettying up a dwelling. They are much more common than retrofitting (18.7% of the dwellings were offered as renovated). Models (3) and (4) are specified similarly to models (1) and (2), but use a "renovated" dummy as dependent variable. The sample excludes units in need of renovation and units that were refurbished. In model (3), there is no effect visible at the date of the coalition agreement, but a marginally significant effect of -1.0 percentage point when the law passed. This coefficient remains stable and turns significant when the regulation came into focre (-1.1 percentage points). Hence, the effect on renovations is smaller in relative terms, and it materialized only at a later point in time—in line with a shorter investment horizon of renovations as compared to refurbishments. Again, there is no heterogeneity of the effect within rent cap municipalities, see model (4).

In summary, landlords adjusted their maintenance decisions considerably in response to the regulation. Furthermore, our estimations reveal that these effects materialized relatively quickly and were not restricted to areas where the regulation is binding. A potential explanation for this latter point could be uncertainty about the development of the rent ceiling.

4.5. Distributional effects: Which areas are de facto-regulated?

In this section, we characterize postal code districts that faced a binding rent ceiling, according to the results presented in section 4.1. Rent regulation has been criticized for a poor targeting of vulnerable groups (Sims, 2007). Because the ceiling is *de facto* binding in some areas, but not in others, its distributional consequences can be inferred from looking at the socio-economic characteristics of these areas. Table 8 shows results of a linear probability model (column (1)) and the corresponding logit model (column (2)) of regressions of treatment status (binding/not binding) on the median house price per m^2 (as a proxy for local wealth), the vacancy rate, population density, the share of the population under 18, above 65, and the share of foreigners. House prices are calculated from listing prices of dwellings offered for sale in 2011. The other variables were aggregated to the postal code level from $1km^2$ grids (Census 2011). The regressions also include city-region fixed effects, so that the results represent within-city differences.

The regressions show that rent growth was particularly strong in neighborhoods with low 2011 house prices and higher vacancy rates (marginally significant in the logit regression). Specifically, this suggests

Table 8: Which postal codes face a binding regulation?

Dependent variable:	*indicator: above-threshold rent growth*	
	OLS	Logit
median house price/m^2	-0.057^{***}	-0.532^{***}
	(0.015)	(0.148)
vacancy rate	0.031^{*}	0.242
	(0.014)	(0.124)
pop density	0.022^{***}	0.132^{**}
	(0.006)	(0.049)
(pop density)^2	-0.001^{**}	-0.009^{*}
	(0.000)	(0.004)
share under 18	0.005	0.023
	(0.005)	(0.048)
share above 65	0.003	0.005
	(0.003)	(0.024)
share foreign	0.011^{***}	0.088^{***}
	(0.002)	(0.023)
adj. R^2	0.375	
Observations	974	974
Mean dep. var	0.252	0.252

The unit of observation is the postal code. Both models include sub-region fixed effects. The unit of median house prices/m^2 is 1000 Euro. It is calculated from listing prices that were online between July and December 2011, for all postal codes with at least six observations. All other variables were taken from the Census 2011 and were aggregated to postal codes from a 1km × 1km grid. Population density is demeaned. The unit is 1000 inh./km^2.)
Heteroskedasticity-corrected standard errors in parentheses.
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

that the pressure on low-income neighborhoods increased substantially in the years before the regulation. These areas also seem to be somewhat more densely populated (with a negative square term) and the share of the foreign population is higher. The other two indicators, the share of residents under 18 or above 65, are not correlated with the indicator for above-threshold rent growth. The overall picture that emerges from these regressions is that, on average, the rent cap reduced rent growth in less affluent areas that served as an overpressure valve for the local rental markets. This is in line with recent evidence on gentrification, see Guerrieri et al. (2013).

5. Conclusions

In this paper, we empirically evaluate the short run effects of *second-generation* rent control. We assess how the German rent cap, introduced for parts of the housing market in June 2015, affected rents, house prices, land values, and maintenance effort by landlords in the regulated and unregulated segments of the housing market. Building on a difference-in-differences approach augmented with a discontinuity in time design, we found that rents and house prices in the regulated sector drop, while they sharply increase in the free market. These findings are robust to various alternative specifications. We further find that the regulation is also capitalized in higher land values, which is consistent with positive revenue expectations for new, unregulated dwellings. As expected, we also find that the regulation led to lower maintenance effort directly after the rent cap was announced in the coalition agreement.

These findings are consistent with the predictions of a standard comparative-static assessment of *second generation* rent control. Additionally, the model predicts an increase of housing starts. While we observe the expected short-run effects on rents in *de facto* regulated markets, on house prices and on refurbishments, we have no empirical evidence on the behavior of real estate developers of new rental housing. Because new housing supply involves planning and administrative lags, this effect can only be evaluated over a longer period of time. In that direction, a first indication is the significant effect of the regulation on land values. This supports advocates of rent control as an instrument to increase welfare. However, we also found negative consequences on maintenance, which might initially offset a positive supply effect. In the long run model equilibrium, however, the decline of the regulated housing stock is compensated by new, unregulated dwellings.

Our results for house prices suggest that investors anticipated the introduction of the regulation in advance: Already in December 2013, when the coalition agreement was signed, prices responded to the announcement of a rent cap. This is an indication for the efficiency of housing markets. A notable difference compared to the results for rents is that the effects for prices are more widespread. In particular, they are not confined to areas where the regulation is *de facto* binding. This latter finding could be explained by the potentially curtailing effect a stochastic, dynamic rent ceiling has on potential revenue in the future. Furthermore, landlords in all regulated areas face a higher risk of lawsuits when there is disagreement about the correct rent ceiling.

There are other factors we do not address in the analysis: Potentially, the increase of unregulated rents induced by the rent cap might hamper immigration into regulated cities. Although–according to the evidence–regulated areas seem less affluent, mostly located close to the centers, accommodating medium/low income households, we do not know which households effectively have access to regulated units. Finally, a positive supply effect due to higher unregulated rents might, in the short run, be offset by a greater number of conversions of rental to owner-occupied or commercial units. Again, such effects can only be studied in the medium or long run.

For the German case, our results stand in contrast to previous work on the effects of this specific rent control. Available studies indicate that the regulation had only little effects on rents, see e.g. Deschermeier et al. (2016); Thomschke and Hein (2015); Hein and Thomschke (2016); Deschermeier et al. (2017). In the public debate, the rent cap is perceived as an ineffective housing policy. The evidence presented in this paper clearly shows that this is not the case.

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6. Appendix

6.1. Appendix: Rent and house price data

Here, we describe in detail the rent and house price data.

Data sources and data quality. The rent and house price data used in this study are advertised rents and prices for dwellings from three large online real estate market places: *Immonet*, *Immowelt*, and *Immobilienscout24*. Although not a perfect substitute for transaction data, listing price data are shown to reliably capture price trends (Lyons, 2013; Dinkel and Kurzrock, 2012). There can be significant differences between the transaction price and a first offer, but the literature points out that systematic mis-pricing can be very costly to sellers of real estate (Knight et al., 1994; Knight, 2002; Merlo and Ortalo-Magné, 2004). Landlords face a higher vacancy risk when the initial price is too high. The gap is larger in declining and smaller in increasing markets (Henger and Voigtländer, 2014). While this suggests that turning points of the market cannot be described adequately by indices based on listing prices, this cannot be observed in empirical applications (Lyons, 2013). House prices in the German cities under consideration grew constantly, at least since 2011. Importantly, there are no turning points of the market in the sample period. It is therefore likely that listing prices and offered rents are reliable proxies for transaction values in our case.

Sample and covariates. The sample covers the period from July 2011 to November 2016, allowing us to examine the initial phase of introduction of the rent cap throughout 2015 and 2016. A long list of housing characteristics (type and size of the dwelling, number of bathrooms, balcony, fitted kitchen, etc.) and their quality (e.g., past refurbishments etc.) are included, as well as information on the postal code of the dwelling. These variables are often-used controls in hedonic studies, see, e.g., Malpezzi (2003) and Cheshire and Sheppard (1995).

Summary statistics for treatment and control groups for the sub-samples used in the regressions discussed in Sections 4.1.1 and 4.2.1 are presented in Table 9 in the Appendix. Many of the new dwellings offered for rent are rehabilitations of older dwellings. This does not seem to be the case for new dwellings offered for sale. Further, a substantial share of regulated and new dwellings is advertised as "refurbished" or "renovated"—even if the building age is reported to be zero. In order to minimize measurement error with respect to the treatment status, we excluded all refurbished dwellings from the sales sample and run robustness checks that exclude refurbished or renovated units for the rent sample.

In addition to the characteristics of the dwellings, the postal code information was used to map observations to regulated and unregulated regions. This mapping is in some cases ambiguous. Because postal code districts cover about 40,000 inhabitants, a postal code district contains several municipalities in rural areas, whereas larger cities constitute a municipality, but there may be many postal codes within these cities. In the first case, we completely exclude the postal code whenever there were municipalities with *and* without a rent cap among the matches. Furthermore, we added population density on the level of postal codes, based on rastered population data provided by the Federal Statistical Office (based on the census 2011).

6.2. Appendix: Tables

Table 9: Variable means for treatment and control groups

	rents		*prices*		*Description*
	regulated	*control*	*regulated*	*control*	
rent	866.973	918.999			*monthly net rent*
listing price			263380.990	427528.532	*sales price*
yc	1975.098	1984.016	1984.542	2013.928	*year of construction*
building age	4.936	0.000	19.184	0.000	*time since (re-)construction*
rooms	2.663	2.682	2.841	3.204	*Number of rooms*
floor NA	0.118	0.140	0.296	0.267	*floor number not indicated*
floor	1.677	1.689	1.343	1.410	*floor number*
elevator	0.372	0.423	0.317	0.731	*elevator access*
second bathroom	0.296	0.297	0.292	0.461	*two or more bathrooms*
garden use	0.217	0.196	0.196	0.250	*access to garden*
built in kitchen	0.470	0.354	0.500	0.131	*equipped w/ built-in kitchen*
floor heating	0.171	0.239	0.126	0.570	*dwelling has floor heating*
self cont heating	0.100	0.098	0.044	0.009	*dwelling has self-contained heating*
central heating	0.648	0.588	0.631	0.491	*dwelling has central heating*
heating gas	0.366	0.294	0.352	0.168	*heating fuel is natural gas*
heating fluid gas	0.000	0.000	0.000	0.000	*heating fuel is fluid gas*
heating oil	0.066	0.053	0.065	0.001	*heating fuel is light oil*
heating electricity	0.018	0.012	0.015	0.006	*heating by electricity w/o night storage*
heating district	0.139	0.148	0.099	0.198	*district heating*
heating coal	0.000	0.000	0.000	0.000	*heating by coal combustion*
qual luxury	0.055	0.076	0.027	0.077	*very high quality*
qual high	0.408	0.443	0.171	0.386	*high quality*
qual low	0.004	0.002	0.004	0.000	*low quality*
cond renovated	0.210	0.156	0.112	0.001	*renovated dwelling*
cond refurbished	0.106	0.288			*refurbished dwelling*
type regular	0.534	0.564	0.543	0.576	*regular dwelling*
tpye top floor	0.119	0.106	0.092	0.086	*dwelling is on top floor*
type ground floor	0.152	0.155	0.140	0.143	*dwelling is on ground floor*
type terraced	0.033	0.031	0.032	0.053	*dwelling has a terrace*
type souterrain	0.008	0.006	0.003	0.001	*dwelling is below ground floor*
type maisonette	0.055	0.040	0.085	0.053	*dwelling spans two floors*
type loft studio	0.008	0.007	0.004	0.006	*dwelling is loft or studio*
type penthouse	0.018	0.019	0.022	0.066	*dwelling is a penthouse apartment*
type apartment	0.003	0.003	0.006	0.002	*dwelling is an apartment*
parquet flooring	0.046	0.053	0.052	0.131	*dwelling has parquet flooring*
air condition	0.004	0.004	0.006	0.007	*dwelling has air conditioning*
garage	0.074	0.067	0.106	0.080	*garage parking available*
carport	0.011	0.009	0.013	0.014	*carport parking available*
duplex	0.019	0.014	0.025	0.013	*duplex parking available*
undergr parking	0.212	0.207	0.275	0.447	*underground parking available*
any parking	0.237	0.216	0.270	0.204	*any parking available*
rooftop terrace	0.034	0.036	0.038	0.099	*dwelling has a rooftop terrace*
balcony	0.589	0.577	0.526	0.563	*dwelling has a balcony*
terrace	0.407	0.421	0.368	0.543	*dwelling has a terrace*
winter garden	0.010	0.009	0.018	0.008	*dwelling has a winter garden*
loggia	0.034	0.034	0.046	0.061	*dwelling has a loggia*
commission	0.156	0.197	0.697	0.362	*commission payment required*
pop density	528.337	622.543	441.378	565.885	*population density*
after rb active	0.328	0.291	0.254	0.314	*dummy: rental brake is active*
Observations	96104	128072	181319	126046	

Table 10: Covariate results for Table 2: rents

	Dependent variable: log rent	
	(1)	(2)
log(area)	0.727 (0.018)***	0.719 (0.013)***
log(area) × pop density	0.000 (0.000)	0.000 (0.000)*
rooms	0.053 (0.004)***	0.054 (0.003)***
yc 1918	−0.026 (0.007)***	−0.042 (0.005)***
yc 1919-1945	−0.083 (0.008)***	−0.089 (0.006)***
yc 1946-1955	−0.100 (0.012)***	−0.111 (0.006)***
yc 1956-1965	−0.109 (0.008)***	−0.124 (0.005)***
yc 1966-1975	−0.114 (0.007)***	−0.133 (0.005)***
yc 1976-1985	−0.126 (0.008)***	−0.121 (0.004)***
yc 1986-1990	−0.108 (0.011)***	−0.103 (0.005)***
yc 1991-1995	−0.092 (0.007)***	−0.095 (0.005)***
yc 1996-2000	−0.182 (0.031)***	−0.190 (0.025)***
yc 2001-2005	0.001 (0.017)	−0.037 (0.005)***
yc 2006-2010	0.007 (0.010)	−0.022 (0.005)***
building age	−0.008 (0.001)***	−0.004 (0.001)***
floor NA	0.049 (0.005)***	0.043 (0.005)***
floor	−0.002 (0.001)	−0.002 (0.002)
elevator	0.014 (0.005)**	0.019 (0.004)***
floor × elevator	0.008 (0.001)***	0.006 (0.001)***
second bathroom	0.052 (0.004)***	0.036 (0.002)***
garden use	0.014 (0.004)***	0.004 (0.002)
built in kitchen	0.054 (0.003)***	0.043 (0.003)***
floor heating	0.050 (0.005)***	0.045 (0.003)***
self cont heating	−0.041 (0.006)***	−0.030 (0.004)***
central heating	−0.020 (0.004)***	−0.010 (0.004)**
cond renovated	−0.052 (0.005)***	−0.041 (0.004)***
cond refurbished	−0.013 (0.004)***	−0.002 (0.002)
qual luxury	0.134 (0.009)***	0.127 (0.007)***
qual high	0.049 (0.004)***	0.043 (0.003)***
qual low	−0.112 (0.012)***	−0.105 (0.012)***
type regular	0.011 (0.004)**	−0.002 (0.003)
tpye top floor	0.055 (0.007)***	0.007 (0.004)
type ground floor	−0.006 (0.004)	0.002 (0.004)
type terraced	0.045 (0.008)***	0.034 (0.006)***
type souterrain	−0.063 (0.014)***	−0.092 (0.008)***
type maisonette	0.040 (0.008)***	0.031 (0.004)***
type loft studio	0.078 (0.013)***	0.080 (0.015)***
type penthouse	0.136 (0.012)***	0.115 (0.007)***
type apartment	0.026 (0.039)	−0.021 (0.026)
parquet flooring	0.058 (0.007)***	0.045 (0.005)***
air condition	0.069 (0.021)**	0.079 (0.023)***
garage	0.005 (0.012)	0.036 (0.006)***
carport	0.038 (0.023)	0.022 (0.008)**
duplex	0.013 (0.017)	0.019 (0.009)*
undergr parking	0.015 (0.008)	0.020 (0.006)***
any parking	0.014 (0.005)**	0.019 (0.005)***
pop density × garage	−0.000 (0.000)	−0.000 (0.000)***
pop density × carport	−0.000 (0.000)	−0.000 (0.000)
pop density × duplex	−0.000 (0.000)	−0.000 (0.000)
pop density × undergr parking	0.000 (0.000)	0.000 (0.000)
pop density × any parking	0.000 (0.000)	−0.000 (0.000)
rooftop terrace	0.049 (0.011)***	0.048 (0.005)***
balcony	0.000 (0.003)	−0.005 (0.002)*
terrace	0.027 (0.003)***	0.020 (0.002)***
winter garden	−0.003 (0.010)	0.020 (0.008)*
loggia	0.009 (0.007)	0.012 (0.005)*
heating gas	−0.019 (0.004)***	−0.023 (0.004)***
heating fluid gas	0.055 (0.035)	−0.027 (0.025)
heating oil	−0.032 (0.005)***	−0.025 (0.004)***
heating electricity	−0.019 (0.016)	−0.037 (0.006)***
heating district	−0.016 (0.004)***	−0.018 (0.004)***
heating coal	−0.197 (0.081)*	−0.150 (0.026)***
commission	0.026 (0.004)***	0.013 (0.003)***

Note: Postal code fixed effects and spline coefficients are omitted. The column numbers refer to Table 2. Covariate results for other rent models are available on request, but are omitted here to save space.

Postal code-clustered standard errors in parentheses; $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table 11: Covariate results for Table 4: prices

	Dependent variable: log prices		
	(2)	(3)	(4)
log(area)	0.806 (0.013)***	0.785 (0.034)***	0.798 (0.015)***
log(area) × pop density	−0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
rooms	0.069 (0.004)***	0.072 (0.009)***	0.069 (0.005)***
yc 1918	−0.189 (0.016)***	−0.231 (0.024)***	−0.170 (0.020)***
yc 1919-1945	−0.291 (0.018)***	−0.354 (0.029)***	−0.249 (0.022)***
yc 1946-1955	−0.352 (0.016)***	−0.405 (0.027)***	−0.325 (0.020)***
yc 1956-1965	−0.371 (0.014)***	−0.410 (0.023)***	−0.356 (0.018)***
yc 1966-1975	−0.385 (0.015)***	−0.419 (0.022)***	−0.367 (0.019)***
yc 1976-1985	−0.272 (0.014)***	−0.299 (0.022)***	−0.259 (0.017)***
yc 1986-1990	−0.216 (0.014)***	−0.258 (0.023)***	−0.197 (0.017)***
yc 1991-1995	−0.209 (0.014)***	−0.251 (0.025)***	−0.194 (0.017)***
yc 1996-2000	−0.158 (0.013)***	−0.200 (0.023)***	−0.146 (0.016)***
yc 2001-2005	−0.077 (0.013)***	−0.108 (0.022)***	−0.062 (0.016)***
yc 2006-2010	−0.037 (0.013)**	−0.039 (0.026)	−0.032 (0.016)*
building age	−0.002 (0.000)***	−0.002 (0.000)***	−0.002 (0.000)***
floor NA	0.019 (0.005)***	0.038 (0.010)***	0.008 (0.005)
floor	0.001 (0.002)	0.009 (0.003)**	−0.004 (0.002)
elevator	0.004 (0.004)	0.010 (0.011)	0.004 (0.004)
floor × elevator	0.007 (0.001)***	0.008 (0.002)**	0.004 (0.002)**
second bathroom	0.023 (0.003)***	0.013 (0.006)*	0.027 (0.003)***
garden use	0.005 (0.003)	0.002 (0.007)	0.006 (0.003)
built in kitchen	0.050 (0.004)***	0.059 (0.008)***	0.045 (0.003)***
floor heating	0.025 (0.004)***	0.011 (0.008)	0.034 (0.004)***
self cont heating	−0.042 (0.007)***	−0.064 (0.016)***	−0.030 (0.007)***
central heating	−0.016 (0.003)***	−0.028 (0.007)***	−0.009 (0.003)**
cond renovated	0.004 (0.004)	0.013 (0.009)	0.003 (0.004)
qual luxury	0.123 (0.007)***	0.105 (0.009)***	0.129 (0.009)***
qual high	0.011 (0.003)***	0.009 (0.007)	0.014 (0.003)***
qual low	−0.135 (0.011)***	−0.144 (0.018)***	−0.131 (0.013)***
type regular	0.025 (0.003)***	0.037 (0.006)***	0.021 (0.003)***
tpye top floor	0.058 (0.005)***	0.082 (0.011)***	0.046 (0.005)***
type ground floor	0.012 (0.005)*	0.018 (0.010)	0.004 (0.005)
type terraced	0.066 (0.008)***	0.071 (0.017)***	0.062 (0.007)***
type souterrain	−0.158 (0.018)***	−0.194 (0.051)***	−0.164 (0.019)***
type maisonette	0.024 (0.005)***	0.016 (0.014)	0.029 (0.005)***
type loft studio	0.116 (0.019)***	0.182 (0.038)***	0.075 (0.018)***
type penthouse	0.192 (0.008)***	0.235 (0.017)***	0.179 (0.008)***
type apartment	−0.031 (0.016)*	−0.018 (0.030)	−0.032 (0.020)
parquet flooring	0.045 (0.005)***	0.048 (0.011)***	0.042 (0.006)***
air condition	0.086 (0.013)***	0.134 (0.021)***	0.062 (0.018)***
garage	0.066 (0.005)***	0.051 (0.016)**	0.063 (0.006)***
carport	0.018 (0.009)	0.069 (0.027)*	0.023 (0.013)
duplex	0.011 (0.010)	−0.005 (0.025)	0.018 (0.011)
undergr parking	0.037 (0.005)***	0.053 (0.012)***	0.048 (0.006)***
any parking	0.026 (0.003)***	0.038 (0.010)***	0.026 (0.005)***
pop density × garage	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
pop density × carport	0.000 (0.000)*	−0.000 (0.000)	−0.000 (0.000)
pop density × duplex	0.000 (0.000)	0.000 (0.000)	−0.000 (0.000)
pop density × undergr parking	−0.000 (0.000)	−0.000 (0.000)	−0.000 (0.000)
pop density × any parking	−0.000 (0.000)	−0.000 (0.000)	−0.000 (0.000)
rooftop terrace	0.046 (0.008)***	0.063 (0.017)***	0.035 (0.008)***
balcony	−0.003 (0.002)	0.001 (0.005)	−0.003 (0.002)
terrace	0.022 (0.002)***	0.021 (0.004)***	0.023 (0.002)***
winter garden	0.020 (0.009)*	0.008 (0.021)	0.021 (0.010)*
loggia	−0.031 (0.009)***	−0.050 (0.017)**	−0.019 (0.006)**
heating gas	−0.002 (0.003)	−0.017 (0.008)*	0.003 (0.003)
heating fluid gas	−0.174 (0.083)*	0.043 (0.034)	−0.179 (0.083)*
heating oil	−0.009 (0.005)	−0.013 (0.011)	−0.005 (0.006)
heating electricity	−0.018 (0.011)	0.010 (0.026)	−0.029 (0.012)*
heating district	−0.013 (0.006)*	−0.011 (0.010)	−0.010 (0.007)
heating coal	−0.102 (0.042)*	−0.125 (0.073)	−0.096 (0.052)
commission	0.018 (0.004)***	−0.001 (0.008)	0.027 (0.003)***

Note: Postal code fixed effects and spline coefficients are omitted. The column numbers refer to Table 4. Covariate results for other models are available on request, but are omitted here to save space.
Postal code-clustered standard errors in parentheses; $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table 12: Robustness checks: effects on rents, young vs new units

	Dependent variable: log rent							
	(R1)	(R2)	(R3)	(R4)	(R5)	(R6)	(R7)	(R8)
a) Baseline								
regulated	0.005	0.011	0.010	0.010	0.005	0.008	0.024	0.001
	(0.011)	(0.012)	(0.012)	(0.012)	(0.007)	(0.012)	(0.017)	(0.012)
after coal. agr., before law passed						−0.008		
						(0.015)		
after law passed, before rent cap effective						−0.021		
						(0.022)		
rent cap effective	0.017	0.023	0.021	0.022	0.021^{*}	0.007	0.021	0.024
	(0.015)	(0.016)	(0.018)	(0.014)	(0.010)	(0.026)	(0.016)	(0.013)
b) Treatment Effects								
regulated × after coal. agr., before law passed						−0.002		
						(0.020)		
regulated × after law passed, before rent cap effective						−0.020		
						(0.023)		
regulated × rent cap effective	-0.060^{***}	-0.059^{**}	-0.058^{**}	-0.061^{***}	-0.047^{***}	-0.080^{**}	-0.053^{**}	-0.045^{**}
	(0.018)	(0.018)	(0.021)	(0.016)	(0.013)	(0.028)	(0.019)	(0.016)
adj. R^2	0.881	0.881	0.881	0.881	0.881	0.881	0.873	0.879
Observations	79847	79847	79847	79847	79847	79847	44960	79830

Model R1 uses quadratic splines with six knots, R2 uses cubic splines with six knots shifted by three months, R3 uses quadratic splines with twelve knows, R4 uses cubic splines with seven knots. R5 relies on 4^{th}-order polynomials, model R6 allows for jumps at the date of the coalition agreement and when the law passed. R7 restricts the sample to unrenovated, unrefurbished units. R8 uses a weighted sample so that the per month/ per postal code share of observations and the per month/ per postal code share of treatment and control groups are constant.

Clustered standard errors in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 13: Robustness checks: effects on prices

	Dependent variable: log rent							
	High rent growth districts				*Low rent growth districts*			
	(P1)	(P2)	(P3)	(P4)	(P5)	(P6)	(P7)	(P8)
a) Baseline								
regulated	-0.051^{*}	-0.045^{*}	-0.045^{*}	-0.060^{**}	-0.083^{***}	-0.087^{***}	-0.089^{***}	-0.092^{***}
	(0.023)	(0.023)	(0.023)	(0.021)	(0.016)	(0.016)	(0.016)	(0.012)
after coal. agr., before law passed	0.016	0.016	0.018	0.034^{*}	0.009	0.009	0.018	-0.027^{*}
	(0.027)	(0.027)	(0.024)	(0.016)	(0.020)	(0.019)	(0.020)	(0.012)
after law passed	0.021	0.023	0.015	0.062^{***}	0.015	0.022	0.051^{*}	0.018
	(0.038)	(0.036)	(0.027)	(0.018)	(0.022)	(0.022)	(0.025)	(0.014)
b) Treatment								
regulated × after coal. agr., before law passed	−0.031	−0.030	−0.041	-0.085^{**}	−0.033	−0.033	−0.039	0.014
	(0.037)	(0.036)	(0.032)	(0.027)	(0.023)	(0.022)	(0.023)	(0.013)
regulated × after law passed	−0.055	−0.054	−0.057	-0.102^{***}	-0.076^{**}	-0.080^{***}	-0.093^{**}	-0.065^{***}
	(0.047)	(0.045)	(0.036)	(0.028)	(0.025)	(0.024)	(0.028)	(0.017)
adj. R^2	0.829	0.829	0.829	0.883	0.819	0.819	0.819	0.892
Observations	91161	91161	91161	90964	203679	203679	203679	203317

Models P1 and P5 use quadratic splines with six knots; P2 and P6 use cubic splines with six knots shifted by three months; P3 and P7 use cubic splines with seven knots, and P4 and P8 use weighted samples so that the per month/ per postal code share of observations and the per month/ per postal code share of treatment and control groups are constant.

Clustered standard errors in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 14: Logit model: Bavarian municipalities

Dependent variable: rent cap municipality dummy	Coef.	robust SE
unemployed per resident	-82.376^{*}	(32.707)
population density	93.742^{***}	(12.605)
residential area share	-58.873^{***}	(7.697)
jobs per resident	-15.959^{***}	(1.254)
land price 2010–2012	0.018^{***}	(0.002)
change of land price, (2010–2012) vs. (2013–2014)	1.452^{***}	(0.338)
Observations	1505	
Rent cap municipalities	118	
Other municipalities	1387	
Sample year	2011	

***: $p < .001$, **: $p < .01$, *: $p < .05$.

6.3. Appendix: Figures

Figure 10: *de facto* regulated and unregulated postal code districts, as of June 2016

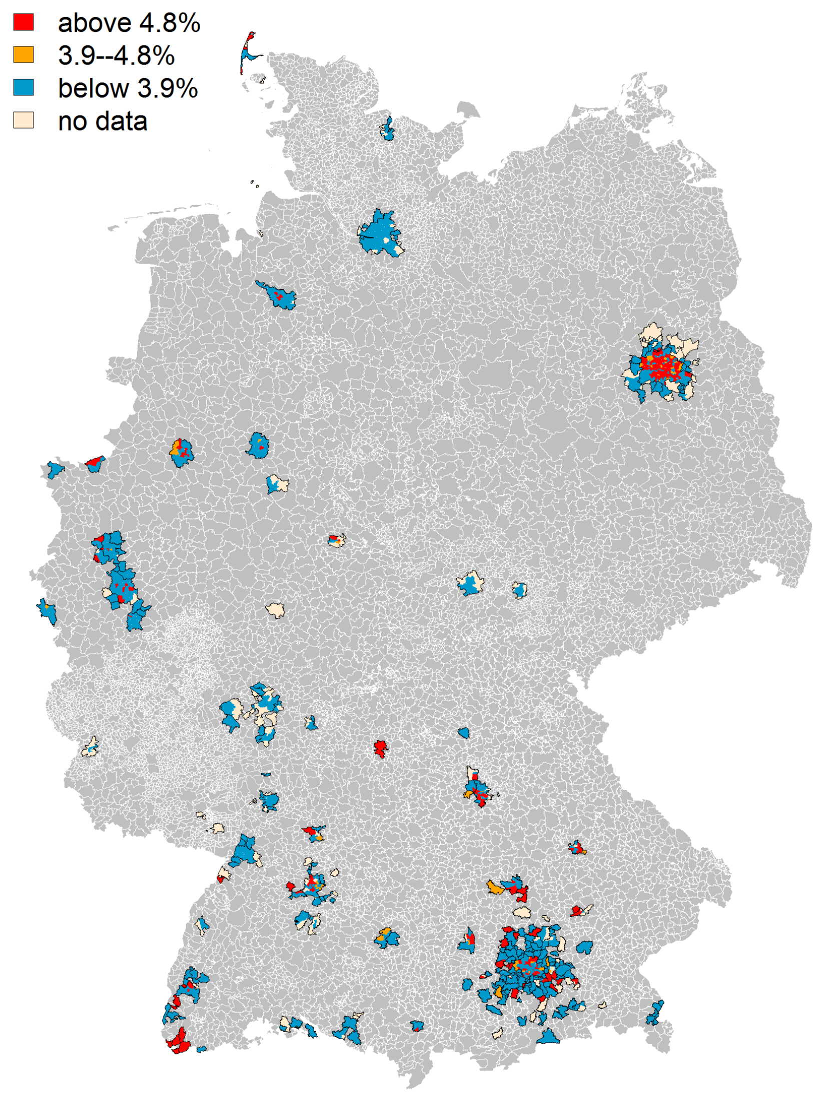

Figure 11: Past growth rates of rents in rent cap municipalities

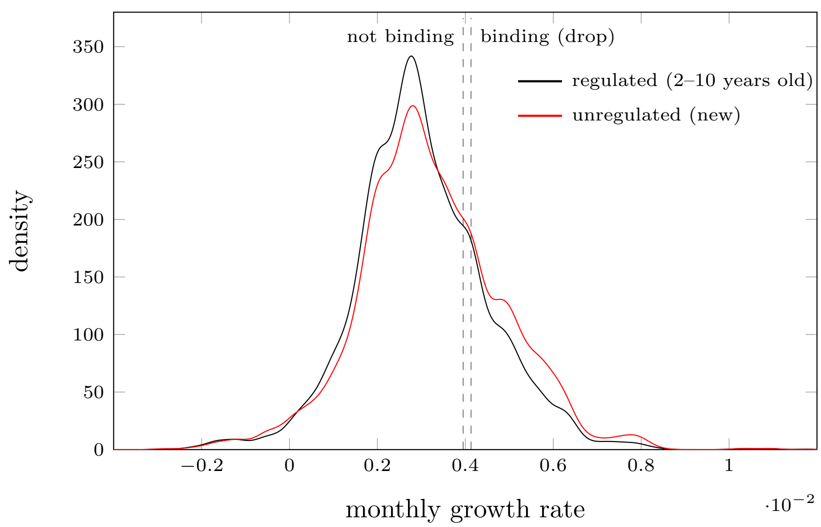

Figure 12: City-regions

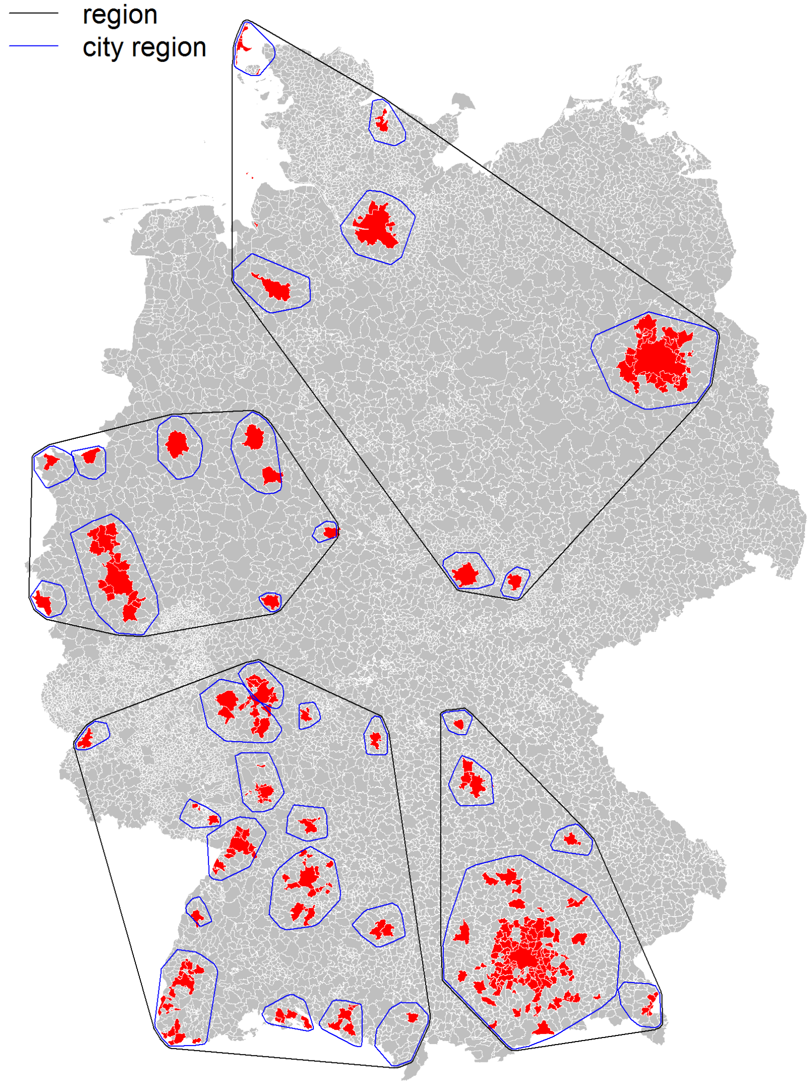

Figure 13: Robustness checks: effects of the rent cap on regulated rents, see Table 12

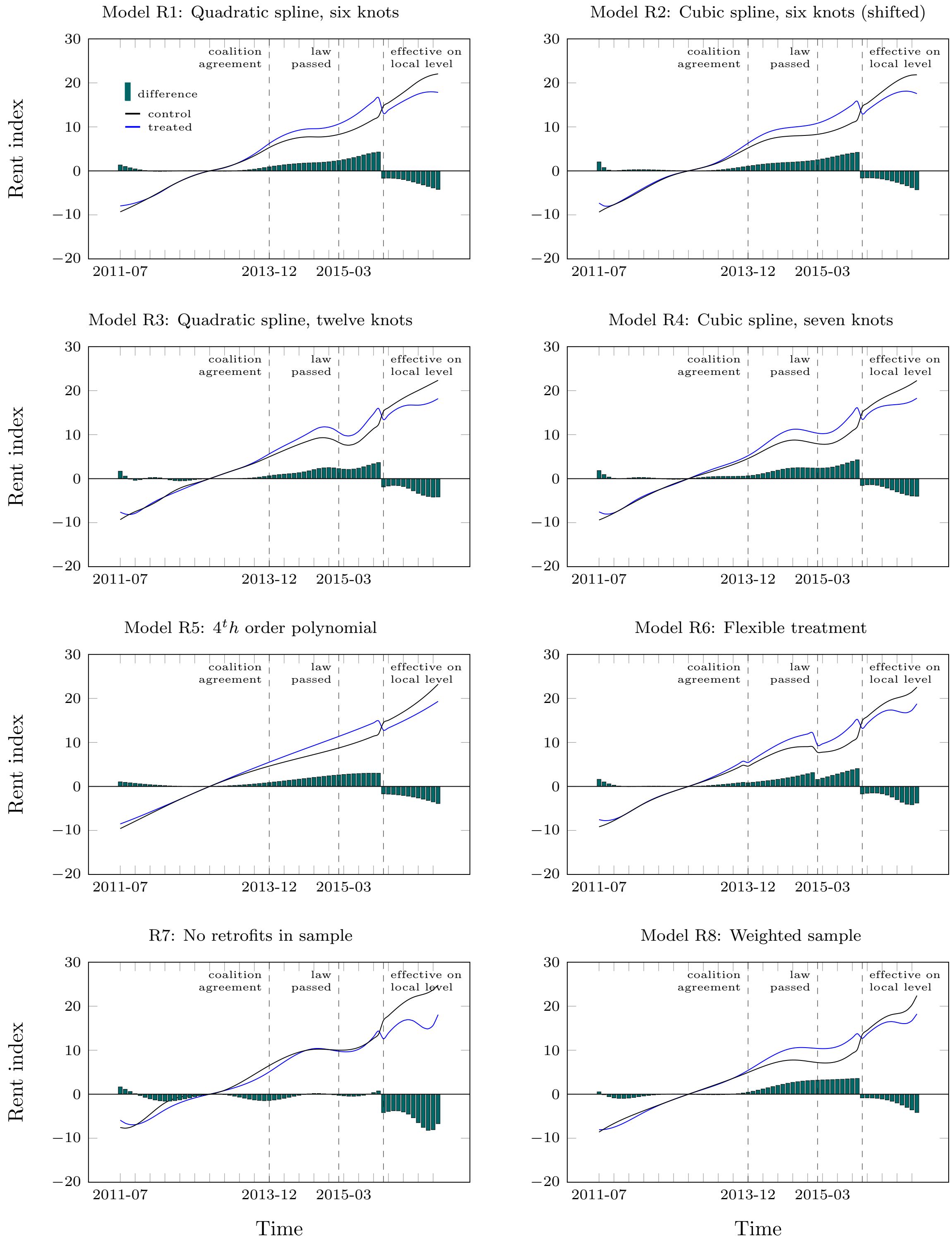

Figure 14: Robustness checks: effects of the rent cap on prices, see Table 13

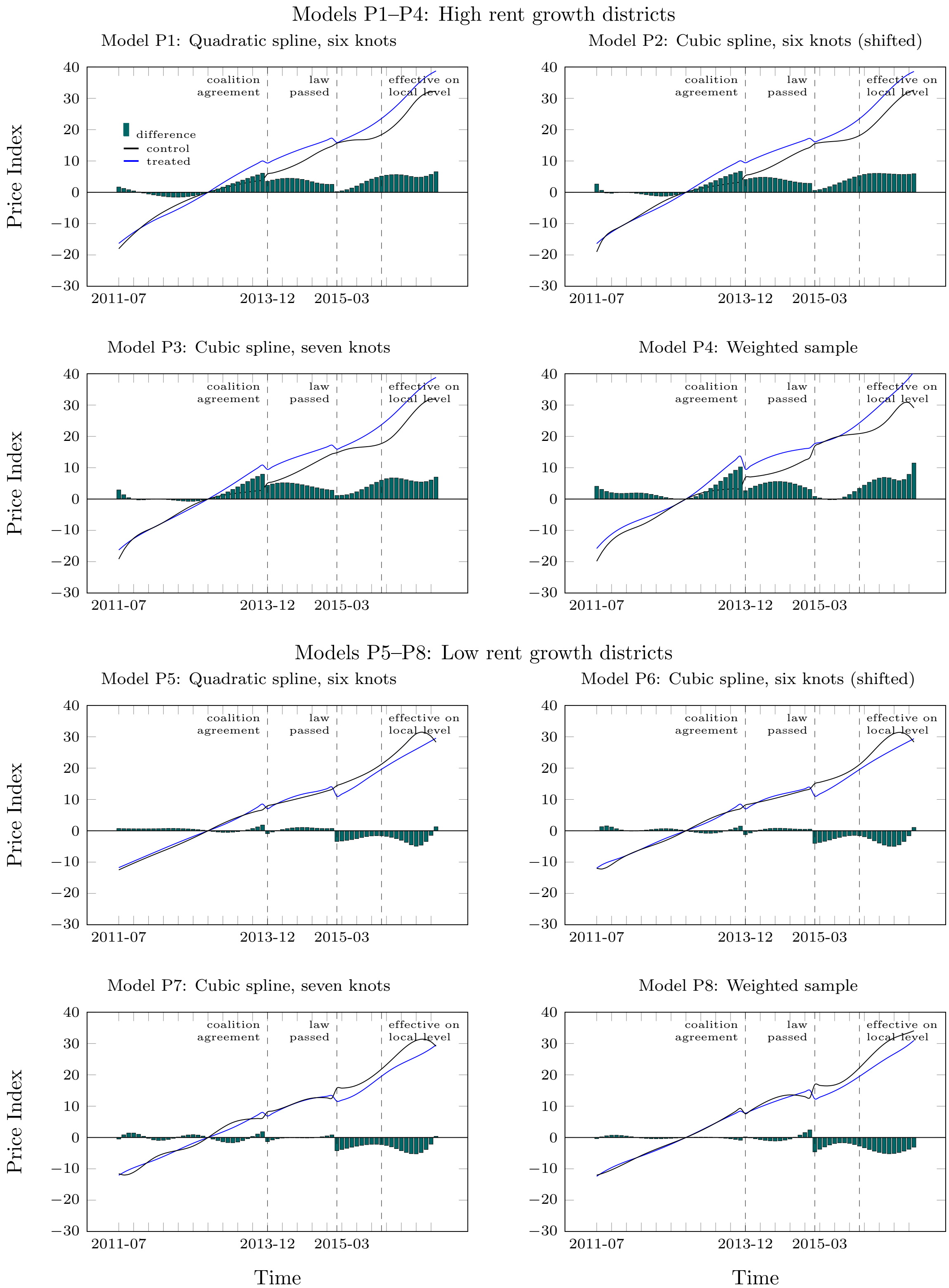

Figure 15: Land values in Bavaria: treatment and control groups in the trimmed and weighted sample