Rent Control and Rental Prices: High Expectations, High Effectiveness?

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Abstract

This paper evaluates the rent control policy implemented in Germany in 2015. Like many countries around the world, German cities and metropolitan areas have experienced a strong increase in rental prices during the last decade. In response, the politicians aimed to dampen the rise in rental prices by limiting the landlords' freedom to increase rents for new contracts. To that end, the rent control was introduced. To evaluate the effectiveness of the rent control with respect to rental prices, we take advantage of its restricted scope of application. First, it is applied only in a selected number of municipalities, thereby generating regional variation. Second, the condition of rental objects generates an additional dimension of variation since new and modernised objects are exempt from rent control. Based on data for rental offersin Germany, we apply a triple-difference framework with region-specific time trends as well as flat type-specific ones. Despite the high political expectations, our estimates indicate that the German rent control dampens rental price by only 2.5 %. This effect varies across object characteristics and seems to be larger for lower-quality, smaller-sized dwellings and in the lower price segment. Nevertheless, the application of an event-study indicates that these effects are not persistent over time.

JEL Codes: C23, R31
Keywords: Rental prices, rent control, regional variation, regulation, diff-in-diff-in-diff, event study

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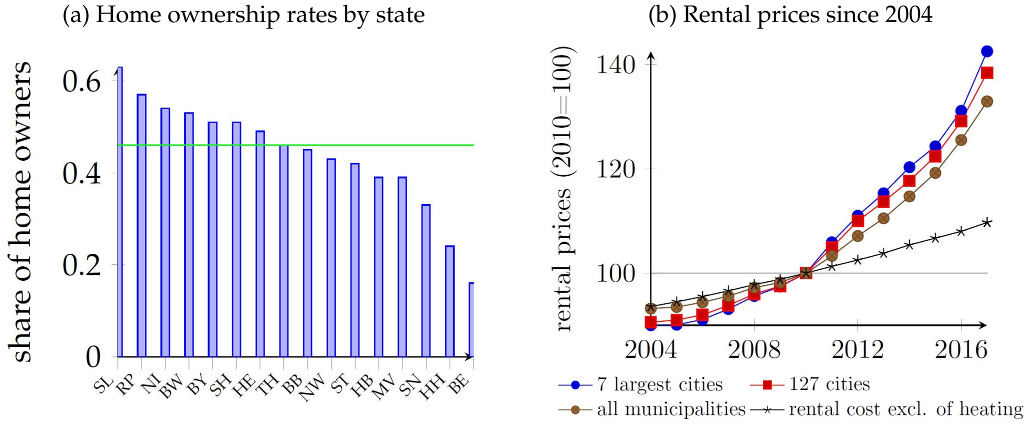
1 Introduction

In the present decade, residential property and rental prices have been on the rise in Germany, entailing vivid political and public debates. While rental prices have been very stable throughout the 1990s and the 2000s, the overall rental price index increased by 33 % in the present decade and even more so in agglomeration areas such as Berlin, Munich, Frankfurt and Hamburg (SVR [2018]). Due to the high proportion of tenants in Germany, this increase in rental prices led to large public discussion on affordable housing. Therefore, the Federal Government designed a bill to restrict this "excessive" increase. In March 2015, the law allowing a so-called rent control (*Mietpreisbremse*) passed the German parliament. By this, the German Federal States (*Bundesländer*) have the legal authority to introduce a regulation for rents on new contracts in municipalities that are characterised by a tight rental market. In these regions, rental price increases are then restricted by a ceiling of 10 % above the local comparative rent index (*ortsübliche Vergleichsmiete*). The Federal States determine to which municipalities this applies.

Evidence on the impact of rental price regulation is scarce. The US implemented rent controls in the 1920s and 1970s (Rajasekaran, Treskon and Greene [2019]), but there is no good micro data on rental prices for these time periods. One strand of literature focuses on city- or state-specific changes in regulations, e.g. on policies in San Francisco, New York or Cambridge. Several studies conclude that rent control policies lower rents with some findings for negative effects on the stock of rental objects (Autor, Palmer and Pathak [2014], Diamond, McQuade and Qian [2018]; Heskin, Levine and Garrett [2000] and Sims [2007]).

In contrast to the Unites States, the German rent control offers a rigorous (nationwide) regulation approach for new contracts, which is covered by data of sufficient quality before and after the introduction of the regulation. Furthermore, the structure of tenants and the design of the regulation offer highly interesting aspects for the analyses of this intervention and its impact on prices. Figure 1 (a) shows the home ownership rates by states in Germany. On average, 52 % of German households live in rented dwellings, while this is true for about 37 % of households in the United Kingdom and in the United States, and for less than 20 % in Norway. Moreover, renting is common within big cities which faced the strongest price increases. The lowest shares of home owners (highest shares of renters) are observed in the three city states of Berlin, Hamburg and Bremen.

FIGURE 1
HOME OWNERSHIP AND RENTAL PRICES IN GERMANY



NOTES.—SL: Saarland, RP: Rhineland-Palatinate, NI: Lower Saxony, BW: Baden-Wurttemberg, BY: Bavaria, SH: Schleswig-Holstein, HE: Hesse, TH: Thuriniga, BB: Brandenburg, NW: Northrhine-Westphalia, ST: Saxony-Anhalt, HB: Bremen, MV: Mecklenburg-Western Pomerania, SN: Saxony, HH: Hamburg, BE: Berlin.
SOURCE.—Authors' calculations based Deutsche Bundesbank, Federal Statistical Office.

Beside the newly introduced rent control, tenants are already protected by law in case of inventory rents (*Bestandsmieten*) which are already strictly regulated. Landlords are not allowed to increase inventory rents by more than 20 % within three years. Combined with the low turnover rate of rental units in Germany, this forms a high pressure on price adoption for new contracts since it is the only channel for landlords to increase rents without substantial investment. Moreover, under rent regulation, tenants also tend to move out less frequently than tenants in unregulated units (Diamond, McQuade and Qian [2018], Glaeser and Luttmer [2003], Gyourko and Linneman [1989], Heskin, Levine and Garrett [2000] and Sims [2007]). Consequently, apartment-seekers are exposed to heavy price increases (illustrated by rental price indexes in Figure 1 (b)). While seekers today have to pay 30 % higher prices than in 2010 over all cities (the price increase in large cities is even higher), rental payments for tenants in existing contracts only increased by about 10 %.

Several characteristics in the design of the legislation are relevant for our analysis. Firstly, the law on the rent control requires the definition of municipalities which fulfil the criteria of tight markets, thereby generating regional variation. Secondly, within these municipalities, not all dwellings are affected by the rent control, since condition-specific exceptions are prevalent: *New* buildings (firstly occupied or erected after October 2014) and thoroughly renovated buildings are exempted from the regulation. These exceptions were introduced to prevent negative incentives on investment which decrease the number of supplied dwellings or the quality of the supplied objects as observed in other countries (see e.g. Sims [2007] and Diamond, McQuade and Qian [2018] for evaluations of reforms in US Federal States). And thirdly, the local implementation of the rent control varies on state level, offering a time-specific variation. This specific design of the German rent control allows for a very flexible identification framework.

For identification, we combine these different sources of variation. First, we make use of the regional variation by which some regions are treated and others are not. However, simply relying on that variation, even over time in a diff-in-diff set-up, will not result in causal estimates. The identification of diff-in-diff approaches is based on the assumption that the price increase would have been the same in both groups in the absence of the rent regulation. Given that treated municipalities are denominated by the price development (*tight market*), the endogeneity problem is evident. Second, within these municipalities, not all dwellings are affected by the rent control and flat type-specific variation allows evaluation as proposed by the literature. Mense, Michelsen and Kholodilin [2018] and Deschermeier, Seipelt and Voigtländer [2017] make use of these condition-specific exceptions but do not focus on regional variation. Within the universe of treated municipalities, they compare the affected dwellings against the unaffected ones. Nevertheless, in both approaches - disregarding regional or disregarding condition-specific variation - available information is not used for identification.

Hence, our approach goes beyond and combines the variation of both standard diff-in-diff setups. We establish a framework of *triple differences* ("diff-in-diff-in-diff"), combining two different control groups. Our estimated effects are therefore based on the development of treated objects in treated regions. We are able to derive treatment effects while allowing treated municipalities to have higher price levels and - more importantly - to have stronger price increases over time. Furthermore, we can derive unbiased effects even if treated condition types have stronger price increases irrespective of the implementation of the rent control. This set-up allows us to make causal statements on the effectiveness even if the treatment of municipalities is endogenous. Last but not least, we make use of the time variation that is generated by the stepwise introduction of rent control over the Federal States. Applying an event-study design in the *triple differences* framework allows us to precisely distinguish between effects in the absolute time (possibly caused by various trends in treated municipalities) and effects in the relative time related to the respective introduction of the rent control (caused by the introduction of the rent control).

Therefore, our paper contributes to the literature by first, taking into account differential price developments on regions and second, differences in the condition of the apartments. In addition, our contribution is on effect heterogeneity. Arguably, rent control should improve the situation of low-income households which are affected by the increase of rents (Dustmann, Fitzenberger and Zimmermann [2018]) the most. Although we do not have information on household characteristics directly, we can exploit the rich data set we have at hand to shed further light on effect heterogeneity over several dimensions of dwelling characteristics.

The results of our *triple differences-approach* suggest that the rent control reduces the rental price trend for regulated dwellings within regulated regions by about 2.5 %. This result can be interpreted in a way that rent control actually works in the intended direction, but on a smaller scale than might have been expected. Nevertheless, the effectiveness is stronger for those dwelling types which are typically occupied by lower-income households. Apartments of lower quality in the lower price segment show higher effects by the introduction of the rent control (up to 4.4 %). However, the effect seems not to be long-lasting as results from the event-study show a strong fading-out of the effect after about twelve months, potentially caused by missing sanctions and transparency in case of violations against the rent control.

The remainder of this paper is organized as follows. Section 2 presents the applied methods, discussing the identification strategy in detail. Section 3 describes the dataset and some descriptive insights on rent price development. The results from the different estimation strategies are presented in Section 4, and Section 5 concludes.

2 Empirical Strategy

2.1 Difference-in-Differences Approach

To compare developments in rental prices in a regulated market (treatment group) to a non-regulated market (control group), one econometric solution is a difference-in-differences framework (diff-in-diff). The basic identifying assumption to estimate the treatment effect of rent control is that the development of rents in the regulated market would have been the same as in the non-regulated market in the absence of the introduction of rent control. This identifying assumption is unlikely to hold in the context of the German rent control, as municipalities are assigned to rent control because of their strong price increases. A potential solution to compare similar groups is presented by Kholodilin, Mense and Michelsen [2016], who shrink the control group to those postal code areas which are directly adjoining a regulated market. Vice versa they shrink the treatment group to those postal codes within the regulated market but directly adjoin an unregulated market. However, in this setup the considered groups tend to influence each other and therefore lead to biased results (*neighbourhood effects*). However, results stemming from such an approach can hardly convey representative evidence, and the considered groups tend to influence each other. Such potential spillover effects violate the SUTVA (Stable Unit Treatment Value Assumption) as the treatment applied to the regulated municipalities may effect the outcome for other municipalities which leads to biased estimates in a diff-in-diff framework.

In order to be able to compare our results to the existing literature, we start our analysis by a diff-in-diff model making use of the time variation in rental prices over regulated and non-regulated regions. The setup is given in Equation (1).

$$y_{irtc} = \alpha_R + \alpha_T + \alpha\mathbf{C} + \beta_{TR} + \gamma\mathbf{X}_{irtc} + v_{irtc}. \tag{1}$$

Thereby, y_{irtc} marks the log price per square meter for a rental object i in region r at calendar month t with condition c. α_R is a binary indicator variable for the region covering local level effects on postal-code level, $\mathbf{C}$ a vector of binary variables indicating apartment-specific conditions. α_T is a set of monthly time dummies based on the end date of the respective offer which can precisely control for time specific effects regarding the overall rental market. Due to the implementation of α_T, an aggregated before/after dummy, regarding the introduction of the rent control, cannot be included as it is a perfect linear combination of α_T. Moreover, the setup does not allow to define a uniform before/after dummy as the introduction differs among the Federal States.

The coefficient of interest is β_{TR}, which turns on for municipalities treated by the rent control after the respective introduction of the rent control on local level. Therefore, it gives the price effect in the regulated region after the introduction of rent control. Moreover, $\mathbf{X}_{irtc}$ is a set of characteristics of the rental object i in calendar month t and region r. The error term, v_{irtc}, is expected to be *i.i.d.*.

Another diff-in-diff setup is taken by Thomschke [2016] and Deschermeier, Seipelt and Voigtländer [2017], who compare rental prices in Berlin between regulated (treatment group) and non-regulated (control-group) objects, exploiting the fact that newly built and modernised objects are not covered by rent control. The most extensive study yet is the one by Mense, Michelsen and Kholodilin [2018], which builds on a diff-in-diff framework over regulated and non-regulated objects combined with a regression discontinuity design. The study estimates the effect on rental prices in regulated *new* buildings relative to non-regulated new buildings directly after the introduction of rent control. As a first step, they derive a theoretical model to identify those postal code areas which faced such a strong price increase (in the local rent index) that they became subject to rent control.[1]

This approach is vulnerable if those objects that are excluded from the rent control follow a different time trend than the regulated objects. This seems not unlikely, since the exempted objects present a more exclusive level of dwellings. However, in order to link our approach to the existing literature, the second estimated model in our study is presented in Equation (2) and compares rental price levels between regulated and non-regulated object conditions

$$y_{irtc} = \alpha_R + \alpha_T + \alpha_C + \beta_{TC} + \gamma \mathbf{X}_{irtc} + v_{irtc}. \tag{2}$$

All parameters are defined as above and α_C is a dummy indicating the condition of an apartment, separating between *new buildings* (non-treated) and *old buildings* (treated). β_{TC} gives the price effect in the regulated condition after the introduction of rent control (interaction of T and C). Note that this model is truncated to observations in regulated municipalities. To sum up, both approaches are prone to suffer from endogeneity issues: the introduction of the regulation is self-selected making regional identification less credible, and the non-regulated objects in regulated areas may be on a different time trend.[2]

[1] From a theoretical perspective, this is a necessary and meaningful step. However, local rent indices are primarily based on existing rent contracts which i) differ substantially from new rent offers and ii) are not covered by any existing dataset for research in Germany. Although this approach of identifying effectively treated areas initially looks very promising, we do not pursue this idea since the mentioned problems do not seem solvable.

[2] Price developments of the non-regulated objects may also be affected by the regulation. A demand surplus for regulated objects generated by the regulation may shift demand towards the non-regulated objects.

2.2 Difference-in-Difference-in-Differences Approach

To overcome the potential endogeneity issues of the rent control as mentioned above, we apply a *triple differences* setup. This framework combines the diff-in-diff estimation comparing regulated and non-regulated regions (Equation 1) with the diff-in-diff estimation comparing between regulated and non-regulated conditions of an object (Equation 2). Thus, exploit temporal variation, regional differentiation and heterogeneous object conditions to identify the causal effect of rent control on rental prices. The treatment can therefore be defined precisely by regulated conditions in regulated regions after the introduction of rent control. This methodology allows to identify the causal effect in the presence of non-random introduction of the rent control, as different time trends between regulated and non-regulated regions are estimated separately. Likewise, it also allows for new and modernised dwellings (exempted from the rent regulation) to follow another price. The model is given in Equation (3).

$$y_{irtc} = \alpha_R + \alpha_T + \alpha_C + \beta_{RT} + \beta_{RC} + \beta_{TC} + \delta_{RTC} + \gamma \mathbf{X}_{irtc} + v_{irtc}. \tag{3}$$

$y_{irtc}, \alpha_R, \alpha_T, \alpha_C, \beta_{RT}, \beta_{RC}, \beta_{TC}$ and X_{irtc} are defined as above. The advantages of this approach can be illustrated by summarizing the potential sources which this approach can control for without a potential bias of our key parameter of interest δ_{RTC}. Price levels may vary in its regional distribution (via α_R). Moreover, regulated and non-regulated regions are allowed to follow different trends in rental prices, as these trends are observable in β_{RT}. Therefore, we are able to control for stronger price increases in the treated cities and moreover, we can control for different price levels on the level of postal codes. Both effects do not affect our identifying assumption. Consequently, regional endogeneity does not mark a substantial problem in our *triple differences* approach.

By including α_C we allow price variation in levels between *new* buildings - not covered by regulation - and *old* buildings. Additionally, we also allow for separate time trends of the *new* and *old* buildings by including the binary indicator variable β_{TC}. Moreover, the β_{RC} dummy describes different rental object conditions over the regions and allows to have different price premiums for *new* buildings in regulated and non-regulated markets. Finally, δ_{RTC} marks our *triple differences* specification, which indicates rental prices after implementation of the rent control in regions where it was introduced and for object conditions that are affected by the rent control. Hence, this approach enables us to identify a rent control effect while still allowing for broad heterogeneity of rental price levels and trends over the location and the condition of a dwelling.

2.3 Disentangling temporal dynamics: Event-Study Approach

Yet, we have mainly focused on preventing endogeneity problems in our identification strategy. This is done by implementing a *triple differences* estimation which allows to derive causal effects of the rent control after the respective introduction. In addition, we further implement an event study which accounts for temporal dynamics in the post-implementation period and sheds light on pre-treatment trends. Especially by distinguishing between effects in the absolute time and effects in the relative time (related to the respective introduction of the rent control), the event study also improves causal inferences. The corresponding event-study model is given in Equation (4)

$$y_{irtc} = \alpha_R + \alpha_T + \alpha_C + \sum_{-J}^{J} \beta_j RT_{t\tau}^{j} C_j + \sum_{-J}^{J} \phi_j RT_{t\tau}^{j} + \gamma \mathbf{X}_{irtc} + v_{irtc}. \tag{4}$$

$y_{irtc}, \alpha_R, \alpha_T, \alpha_C$ and X_{irtc} are defined as above. Moreover, $RT_{t\tau}^{j} C_j\{t = \tau + j\}$ is a binary indicator that measures the time relative to the actual introduction of the rent control (τ). In period t, $RT_{t\tau}^{j}$ equals one if there are either j more months to the rent control introduction or if j months have already passed since the introduction with $j = -J, ..., 1, ..., J$. Hence, τ denotes the time period relative to the rent control introduction. t and τ differ because rent control introduction differs on the level of Federal States between June 2015 (Berlin) and December 2016 (Lower Saxony), while the dataset contains information on the years 2013 to 2017. The event fixed effect ϕ is measured relative to the introduction of rent control. For municipalities, without a rent control introduction (regional control group), all ϕ_j and β_j dummies remain 0. Controlling for both calendar month fixed effects (α_T) and event period fixed effects, (ϕ_j), ensures that we compare outcomes within the treatment and control groups in the same calendar month and in the same period after the introduction. The coefficients of interest in Equation (4) are those β_j which refer to treated objects in the post-introduction periods.

3 Data Description

The empirical analysis is based on a unique dataset. We use object level rental price data from the RWI-GEO-RED, which is combined with self-collected data on the rent control introduction in Germany. The RWI-GEO-RED is a systematic collection of all German apartments and houses for sale and rent that were advertised on the internet platform *ImmobilienScout24* during the years 2013 to 2017.[3] According to its website, *ImmobilienScout24* receives about 1.5 million different properties either for rent or for sale per month. It has more than 2 billion page views per month, and covers over 100,000 property sellers. The platform covers about 35.7 percent of all new rental contracts in Germany.[4] The dependent variable of our analysis is the rental price measured in Euro[5] per square meter and enters the regression as its log.[6]

Figure 2 gives an overview on the averaged rental price on the municipality level. It can be seen that rental prices are lower in rural municipalities in Northern and Central Germany. Urban areas in North Rhine-Westphalia and many suburban municipalities throughout Germany are medium priced, while metropolis like Berlin, Munich, Hamburg or Frankfurt are the highest priced regions in Germany.

The RWI-GEO-RED data are combined with self-collected data on the rent control introduced in Germany. The data are obtained from Federal State laws containing information on the exact timing of introduction and a regional identifier on the municipality level. Hence, these data can be merged to the real estate data on the municipality. The regional distribution and the timing (on a quarterly basis) are reported in Figure 3.

Figure 4 gives information on the development of rental prices over the course of time around the introduction of rent control. Rental prices are grouped by treatment and control cities and by new and old buildings. Only old buildings in treatment cities are covered by rent control. Treatment cities are defined by those municipalities that have applied rent control by the end of our sample.

The blue solid line in Panel (a) indicates that rental prices of *new* buildings in treatment

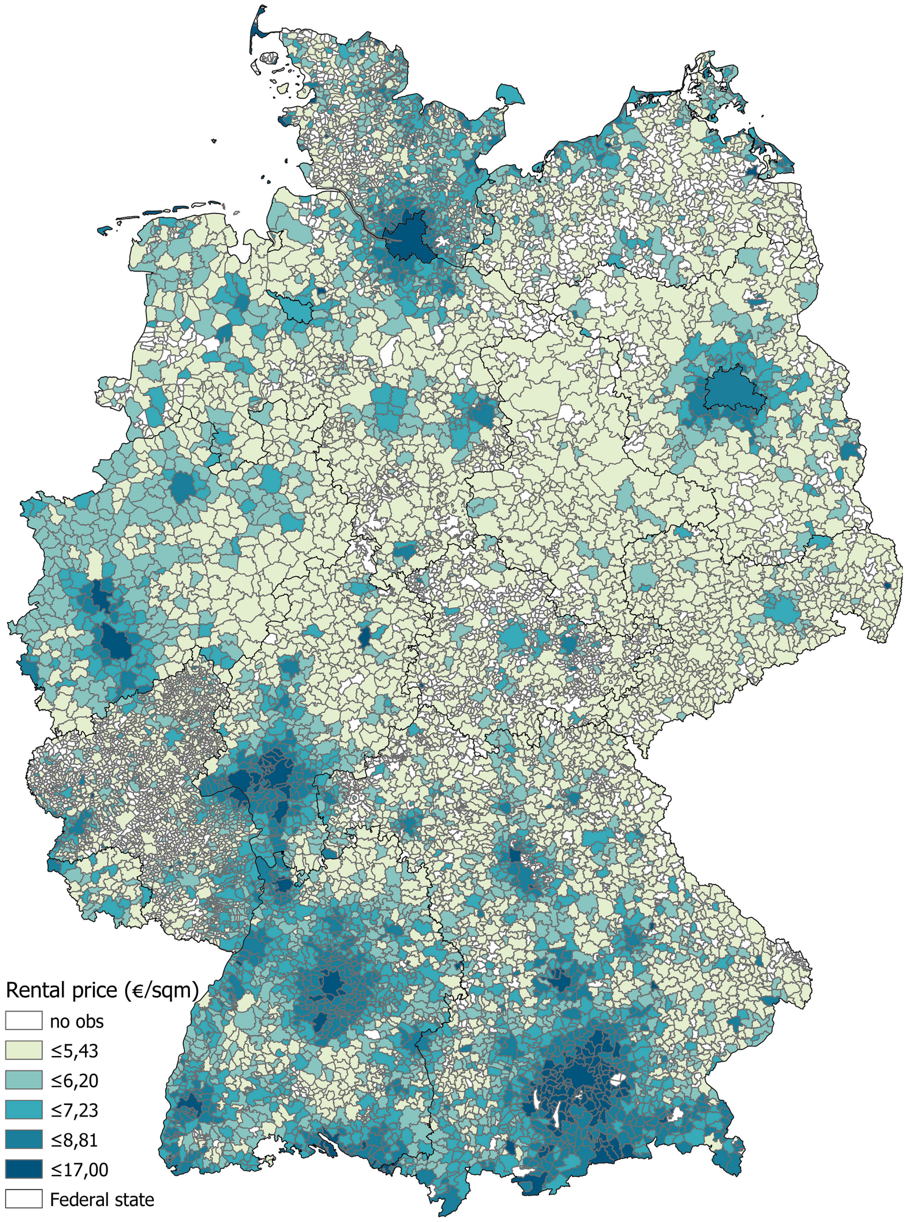
[3] For a documentation of this dataset, see Schaffner and Boelmann [2018].

[4] This information is available on the website of *ImmobilienScout24*: <https://www.immobilienscout24.de/>. Accessed 27 November 2018.

[5] non-deflated

[6] The sample is trimmed by dropping the observation at the highest and lowest one percent concerning the rental price, the number of rooms, the overall living area and the age of the apartment. The study relies on offer prices which reflect the latest adjustments in the rental market.

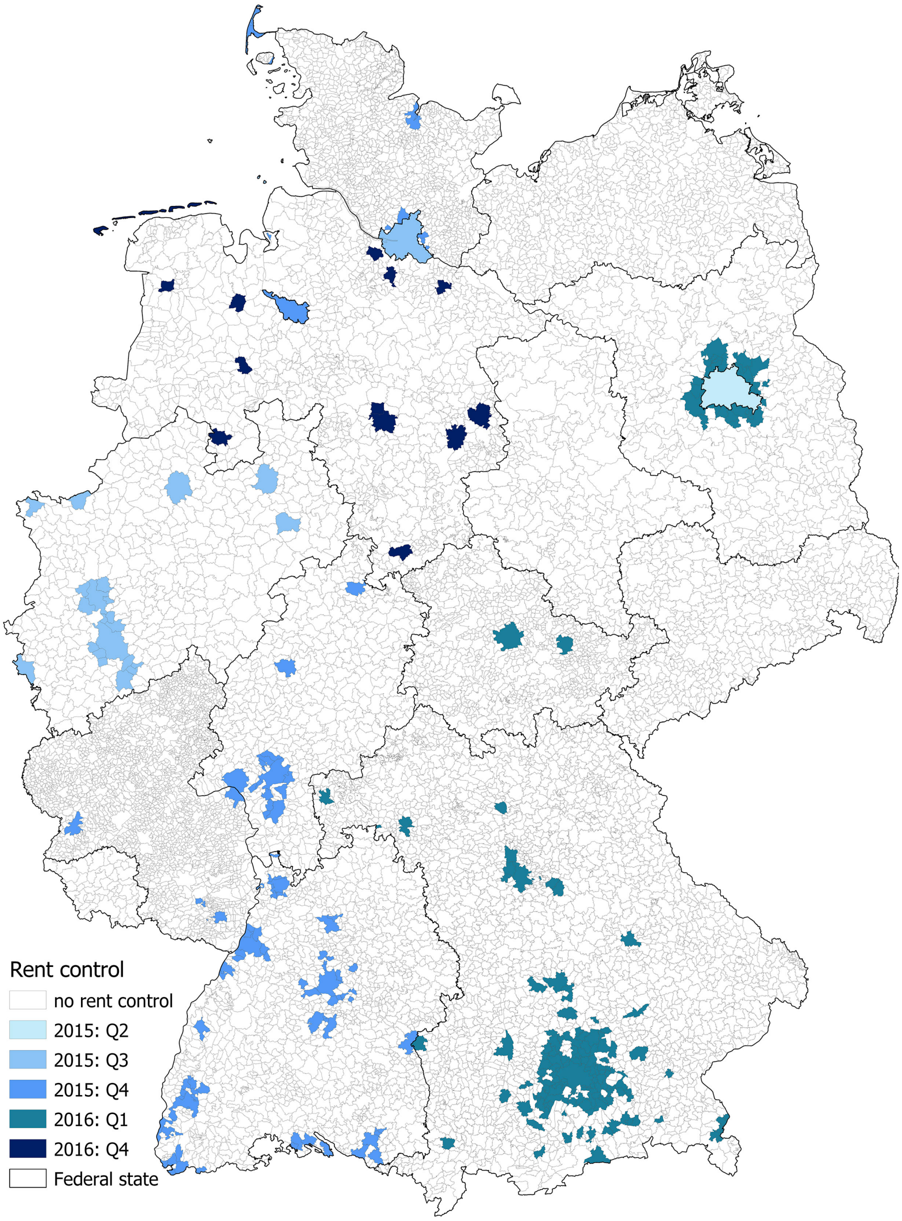
FIGURE 2
RENTAL PRICES: REGIONAL DISTRIBUTION



SOURCE: AUTHORS' CALCULATIONS BASED ON RWI-GEO-RED.

cities do not seem to react to the introduction and application of rent control. This is an important indication, that these objects are a good control group for our identification. These objects do not seem to have price increases do to spillover effects from the regulated part of the market. The orange solid line, in contrast, shows a distinct dampening of rental prices for old buildings in treatment cities. This is exactly the group of dwellings that rent control is supposed to

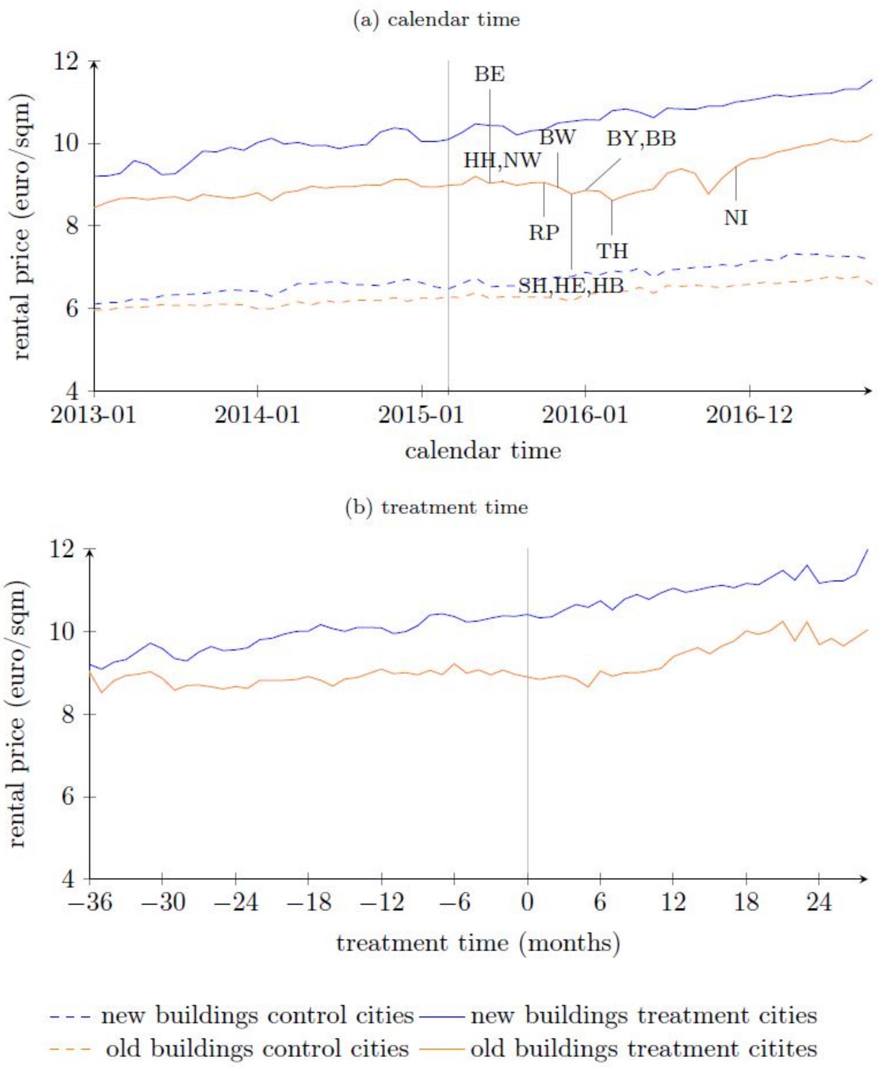
FIGURE 3
RENT CONTROL: REGIONAL DISTRIBUTION AND TIME OF INTRODUCTION



SOURCE: AUTHORS' CALCULATIONS BASED ON MUNICIPALITIES WITH RENT CONTROLS OBTAINED FROM RESPECTIVE LAWS ON FEDERAL STATE LEVEL.

affect. Price development is steady on a comparatively low level in control cities that have not applied rent control so far. Panel (b) depicts developments of rental prices against treatment time, that is calendar time relative to the application of rent control by Federal States. In this way, prices can be depicted for treatment cities only. In those cities, prices for new buildings increase steadily before and after treatment time = 0, when rent control is applied. Prices for

FIGURE 4
RENTAL PRICES AND INTRODUCTION OF RENT CONTROL



SOURCE: AUTHORS' CALCULATIONS BASED ON RWI-GEO-RED.

old buildings show a distinct lower increase of prices about 30 months before rent control is applied. In the event study which includes monthly dummies for each dwelling condition, such different trends in the pre-treatment period can be controlled for. About 18 months after the implementation of rent control, prices seem to have returned to their long-time trend, paralleling prices for new buildings in the same cities. Also, as seen from Panel (b), prices of new buildings do not seem to be affected by rent control. In addition, the descriptive time series Figure 4 in Panel (b) hints at a negative anticipation effect, as the price trend for regulated old buildings deviates from the price trend for non-regulated new buildings about 12 to 6 months before the introduction of rent control by the Federal States. This could be linked to rent control when owners refrain from charging higher rents in anticipation of the rent control legislation. An explanation for this behaviour could be fear of sanctions from vague specifications of the policy measure at this early time. Mense, Michelsen and Kholodilin [2018] indicate that high rent-growth cities with rent control (”de facto regulated”) positively anticipate rent control by excessively increasing rents prior to the regulation. However, this phenomenon only applies to a small fraction of regulated markets.

The explanatory variables capture apartment characteristics, such as the age of the apartment in years, its size in sqm and equipment variables such as balcony, fitted kitchen, garden, elevator and cellar. These variables constitute the vector $\mathbf{X}_{irtc}$ and are used as covariates in Equations (1) to (4) to explain apartment prices.[7]

Newly built and completely modernised apartments are not subject to rent control, apartment condition is one of the most important flat characteristics and enters the regression equation as α_C. This variable differentiates apartments into ten groups presented in Table 1. Table 1 is divided into two panels. The left panel presents a *t*-test comparing apartments located in regions treated by rent control against those apartments that are located in non-treated regions. The right panel presents a *t*-test on the subgroup of apartments located in regions treated by rent control and differentiates between before and after the implementation of the rent control.

TABLE 1
T-TESTS

	Municipality with rent control				Rent control applied			
	CT	TG	Difference	Std. Err.	Before	After	Difference	Std. Err.
First occupancy	0.03	0.06	-0.025	0.000***	0.06	0.06	-0.007	0.000***
First occupancy after reconstruction	0.04	0.06	-0.018	0.000***	0.06	0.06	0.000	0.000
Like new	0.06	0.08	-0.014	0.000***	0.08	0.08	0.003	0.000***
Reconstructed	0.10	0.05	0.044	0.000***	0.05	0.05	0.007	0.000***
Modernised	0.06	0.07	-0.001	0.000***	0.07	0.06	0.001	0.000***
Completely renovated	0.09	0.11	-0.018	0.000***	0.12	0.09	0.027	0.000***
Well kempt	0.27	0.26	0.011	0.000***	0.28	0.23	0.048	0.001***
Needs renovation	0.01	0.01	-0.000	0.000***	0.01	0.00	0.002	0.000***
Dilapidated but negotiable	0.01	0.01	0.004	0.000***	0.01	0.00	0.002	0.000***
Dilapidated	0.00	0.00	-0.000	0.000	0.00	0.00	-0.000	0.000***
Number of observation	3,227,121	2,168,425			1,413,057	755,368		

NOTES.—T-test for apartments located in ’municipality with rent control’ is based on 5,395,546 observations in total while the t-test for ’Rent control applied’ is based on 2,168,425 observations in total. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. TG: Treatment group; CG: Control group.
SOURCE.—Authors’ calculations based on RWI-GEO-RED.

Apartments located in cities treated by rent control seem to be significantly different from

[7] Descriptive statistics of apartment characteristics are reported in Table A.1.

apartments in cities without rent control in terms of condition. Apartments in treated cities are more often first occupancy, first occupancy after reconstruction, like new or completely renovated while apartments in non-treated cities are significantly more often reconstructed, modernised or well kept (see left hand site panel). Comparing apartments in treated regions over time, apartments with an applied rent control are significantly more often first occupancy or like new. Our overall sample consists of 5,395,546 observations, whereupon 2,168,425 apartments are treated by an active rent control.

4 Estimating the effects of rent control

In line with the methodology described in Section 2, the results are presented stepwise in Table 2.[8] Thereby, the diff-in-diff results exploiting only regional variation are presented in Column (1). Column (2) presents the diff-in-diff results obtained using the variation in the condition of dwellings (newly built dwellings are exempted from rent control) within regulated municipalities. Our main results obtained from the application of a *triple differences* model are shown in Column (3), making use of both variations by municipality and by the condition of an object to identify potential effects of rent control. All estimations include a set of object specific control variables like indicators for calendar month, postal code fixed effects, a dummy indicating whether a municipality is subject to the rent control, and a dummy variable indicating the effectiveness of the rent control (interaction term of introduction and regional application of rent control).

Within the setup using variation over treated and non-treated municipalities, rental prices are 2.3 % higher than in those that never apply the regulation (see Column (1)).[9] This estimator is plausible as the legislative possibility to introduce a rent control is linked to a tight rental market. The treatment indicator in this setup indicating municipalities that adopted rent control after its introduction, has a positive sign and a size of 0.4 %, but remains insignificant even on the 10 % level. The rental price trend in treatment municipalities does not significantly differ from the trend in non-treated municipalities after the introduction of rent control. But, as discussed before, this result is prone to be plagued by the endogenous decision on the

[8]The full regression Table A.2 is in the appendix.

[9]Note that these differences might be much higher actually since a fraction of the local differences is already captured by postal code fixed effects in the regression.

implementation of a rent control.

TABLE 2
MAIN REGRESSION RESULTS

	DiD Region (1)	DiD Condition (2)	DiDiD (3)
Municipality with rent control (α_R)	0.023**		0.034***
	(0.0111)		(0.0112)
Rent control applied (β_{RT})	0.003	0.005	0.021***
	(0.0024)	(0.0042)	(0.0044)
Old building (α_C)		-0.058***	-0.068***
		(0.0026)	(0.0042)
Municipality with rent control × old building (β_{RC})			-0.015***
			(0.0031)
Rent control applied × old building (β_{TC})		-0.026***	
		(0.0031)	
Municipality with rent control × rent control applied × old building (δ_{RTC})			-0.025***
			(0.0044)
Set of control variables ($\mathbf{X}_{irtc}$)	YES	YES	YES
Indicator for calendar month of rental offer (α_T)	YES	YES	YES
Postal code fixed effects (α_R)	YES	YES	YES
Condition ($\alpha\mathbf{C}$)	YES		
Old building × calendar month of rental offer			YES
Observations	5,395,546	2,168,403	5,395,546
R^2	0.724	0.552	0.714

NOTES.—The set of control variables include age, age square, living space in square meter, floor of object, number of floors, elevator, balcony, kitchenette, garden, cellar, heating type (cogeneration/combined heat and power plant, electric heating, self-contained central heating, district heating, floor heating, gas heating, wood pellet heating, night storage heaters, heating by stove, oil heating, solar heating, thermal heat pump, central heating, type of heating (unknown) with central heating being the reference class) and equipment characteristics (simple, normal, sophisticated with deluxe being the reference class). The class condition contains first occupancy after reconstruction, like new, reconstructed, modernised, completely renovated, well kempt, needs renovation, dilapidated but negotiable, dilapidated and unknown with first occupancy being the reference class. The constant is not reported. Standard errors are robust to clustering at postal code level and are presented in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
SOURCE.—Authors' calculations based on RWI-GEO-GRID and municipalities with rent controls obtained from respective laws on Federal State level.

Focusing on offers in municipalities which eventually introduce rent control we identify the effect via the condition of dwellings (Column (2).[10] Objects being subject to rent control by their characteristics generally have a 5.8 % lower rental price compared to new or modernised objects, which are not regulated by rent control. This makes sense since the excluded objects are defined by an above average condition (apartment in new building or modernised apartment). The price effect of the rent control introduction for those objects covered by the rent control is negative. The implementation of the rent control has dampened the price of

[10]The focus on municipalities which introduced the rent control truncates the number of observations to 2.2 million rental objects. During the observed period of our sample, 313 out of 11,012 (number of municipalities in 2018) municipalities introduced a rent control.

these dwellings by 2.6 % compared to objects that are exempt from rent control. These results are in line with Thomschke [2016] who obtain a short-run dampening effect of 4.3 % on rental prices in a similar setup for Berlin. This effect is, however, smaller than what one would expect from the relation of pre-tenant rental prices or the rent index. Deschermeier, Seipelt and Voigtländer [2017] confirm the small negative effect by the end of 2016, which takes on the value of 2.7 % in their study. Nevertheless, this way of identification is solely based on the condition of the object (newly built or modernised) that also has some shortcomings since it cannot control for potential different price trends of these (generally higher-class) dwellings. As a result the effects may be biased since exempted object conditions might be on another trend than non-excluded dwellings.

Therefore, we apply the *triple differences* approach to evaluate whether the German rent control regulation meets the political goal of stopping excessive rental price growth. The rent control is specified to regulate *old* dwellings that had been erected before 2014, which generates variation over buildings within regions that are subject to rent control. Column (3) presents the corresponding results. Similar to the results obtained from the diff-in-diff approaches presented in Columns (1) and (2), rental prices in municipalities with an applied rent control are characterised by a higher price level, which seems to be even higher in the *triple differences* framework (3.4 % higher price level compared to 2.3 % in the diff-in-diff framework). *Old* buildings that fulfil the requirements to be covered by a rent control have a lower price by about 6.8 %, but within rent control municipalities, this effect is reduced by 1.5 %. Estimating the third difference reveals an overall negative effect of the rent control on the price trend of -2.5 % which is significant at the 1 % level. This effect is in line with the finding by Mense, Michelsen and Kholodilin [2018] who find an effect of -2.9 % on the rental price trend.

4.1 Effect heterogeneity

The results stemming from the *triple differences* results seem quite plausible as the relevant coefficient show the expected sign. However, the obtained average treatment effect of -2.5 % on the treated is rather small as compared to the high political expectations for the rent control. Since the comparative local rent – which is usually calculated based on the actual location, the size and the age of a dwelling – marks the base of compliant price increases, the regulation of the rent control may have heterogeneous effects for different types of dwellings. Such heterogeneous effects may also bring important insights for politicians as the rent control is an instrument of governmental social policy. In this setup, a rent control which mainly benefits high price-dwellings and has no effects or substantially smaller effects for cheaper dwellings misses its goal in the social policy context. Thus, a sound effect of this policy can only be evaluated by further insights on the effects for concrete types of dwellings. We split our database in subsamples by (i) categories for the quality of a dwelling, (ii) the number of rooms, (iii) size categories, and (iv) price categories (designed for each year and each district) in a broader sense. Table 3 lists the estimated treatment effects in all these subsamples.

The quality of a dwelling (i) is split into two categories, low quality (named ”simple”, ”normal” or ”unknown” in the dataset) and high quality (named ”sophisticated” and ”deluxe”). Regression outputs show that the implementation of the rent control has a clearly stronger effect (-4.3 %) for apartments characterised by low or unknown quality. In the case of higher quality dwellings, the effect is very close to zero and statistically insignificant.

Three categories are defined by the number of rooms: dwellings with two rooms or less (typical for singles), those with two or three rooms (typical for two person household) and three or more rooms (suggesting that dwellings are occupied by families).[11] As the respective results shows, we are not able to gain further knowledge from this separation. The treatment effect for all of these three subgroups lies around -2.8 %, which is very much in line with the initial effect of the full sample estimates.

In contrast, size effects are present when size is defined by square meters (instead of the number of rooms). Separating the size in four categories (<50 sqm, 50 to 80 sqm, 80 to 120 sqm and 120 and more sqm) reveals that rent control is most effective for dwellings with a size of more than 120 square meters (-4.4 %) while the effect is the smallest for small dwellings (-1.9 %). The effects are statistically significant for all categories. Yet, it remains unclear whether large dwellings rather hint at larger family households (which need larger dwellings) or whether they hint at better-off households (which can afford larger dwellings).

In terms of the social policy goals of the rent control, such a classification is crucial for the success of the policy. To gain further insights for the interpretation of the size-effect, we divide our sample into three subgroups by the price per square meter. We generate the distribution of

[11] Categories for two and three rooms are overlapping.

TABLE 3
EFFECT HETEROGENEITY REGRESSION RESULTS

(i) Quality	Low	High		
Municipality with rent control × old building × rent control applied	-0.043*** (0.0060)	0.004 (0.0040)		
Observations	4,179,769	1,215,360		
R^2	0.690	0.708		
(ii) Number of rooms	**1-2 rooms***	**2-3 rooms***	**≥ 3 rooms**	
Municipality with rent control × old building × rent control applied	-0.027*** (0.0044)	-0.028*** (0.0045)	-0.028*** (0.0059)	
Observations	2,684,500	4,090,386	700,801	
R^2	0.710	0.729	0.750	
(iii) Size in square meters	**≤ 50 sqm**	**> 50 sqm to ≤ 80 sqm**	**> 80 sqm to ≤ 120 sqm**	**>120 sqm**
Municipality with rent control × old building× rent control applied	-0.019*** (0.0047)	-0.033*** (0.0042)	-0.022*** (0.0063)	-0.044*** (0.0081)
Observations	1,047,793	2,711,704	1,362,358	272,045
R^2	0.765	0.753	0.726	0.716
(iv) Price in euro per sqm	**Low**	**Medium**	**High**	
Municipality with rent control × old building × rent control applied	-0.035*** (0.0044)	-0.009*** (0.0019)	0.009** (0.0036)	
Observations	1,812,256	1,796,963	1,785,439	
R^2	0.800	0.958	0.849	

*—Apartments characterized by two rooms are present in both classifications.
NOTES.—The set of control variables include age, age square, living space in square meter, floor of object, number of floors, elevator, balcony, kitchenette, garden, cellar, heating type (cogeneration/combined heat and power plant, electric heating, self-contained central heating, district heating, floor heating, gas heating , wood pellet heating, night storage heaters, heating by stove, oil heating, solar heating, thermal heat pump, central heating, type of heating (unknown) with central heating being the reference class) and equipment characteristics (simple, normal, sohisticated with first occupancy being the reference class). The class condition contains first occupancy after reconstruction, like new, reconstructed, modernised, completely renovated, well kempt, needs renovation, dilapidated but negotiable, dilapidated and unknown with first occupancy being the reference class. Standard errors are robust to clustering at post code level and are presented in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
SOURCE.—Authors' calculations based on RWI-GEO-GRID and municipalities with rent controls obtained from respective laws on Federal State level.

the price per square meter for each year and each municipality and separate this distribution into three equal parts (terciles), which we name lower, medium and upper price segment. The estimates show that the rent control effect is mainly driven by the lower price segment. For these dwellings, the effect of the rent control is about -3.5 % The medium price segment also shows a negative estimate which is substantially smaller (around -0.9 %). For the upper price level, the treatment effect of rent control is positive but close to zero (+0.9 %) and only significant at the 5 % level. [12] Regarding these results, the regulation meets the goal to affect lower income households (associated with lower price segments). However, also in this subgroup, the effect is rather moderate.

4.2 Event study

The regressions above are specified to consider the endogeneity that occurs by the selective introduction of the rent control. Based on the same setup as the *triple differences* approach the event study allows further insights in the temporal dynamics of the treatment effect.[13] As regression output tables do not allow an easy interpretation of this effect, we switch to a visual inspection in Figure 5 plotting the time-varying effects setting the baseline to $\tau = -4$. [14] Note that the plotted outcome variable is still defined in a triple diff-in-diff style, i.e. it reflects the difference in rental price trends. The horizontal axis plots the treatment time, where $\tau = 0$ defines the calendar month when a Federal State applies the rent control, all positive points in time define months after the introduction and all negative points in time define months before the introduction of the rent control.

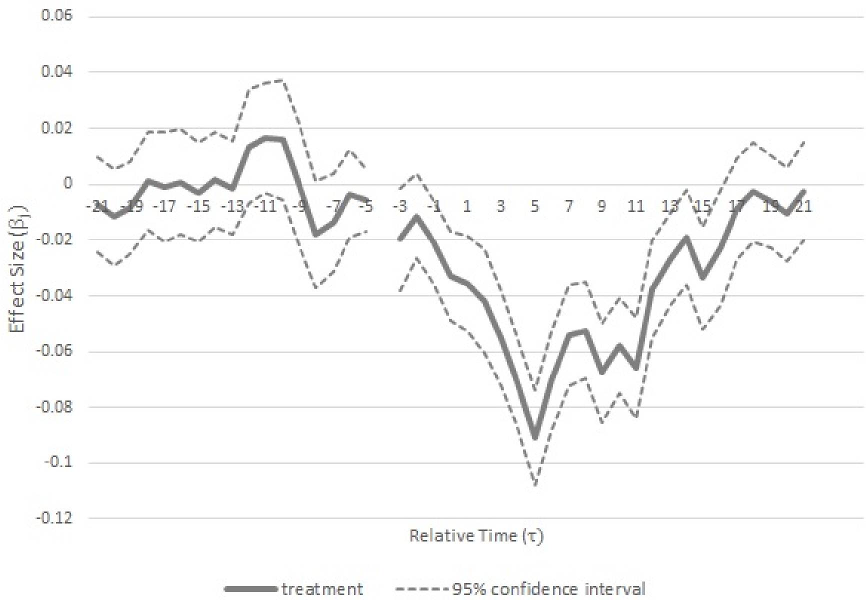
For periods before the omitted baseline category, the estimated coefficients are mostly positive, of small magnitude and not statistically significantly different from zero. This provides suggestive evidence that the common trend assumption in treated and control regions is likely to hold. A deeper inspection of the pre-trend shows that two jumps in the pre-trend cause the unsteady development – ten months and four months before the introduction of the rent control, respectively. To search for events which may have caused these dips, the relative time distances have to be translated into calendar months, which are different for each Federal State (at least if introduction dates differ between the States). This translation shows that the ten- and four-months dips indicate the calendar month "March 2015" for Northrhine-Westphalia (the largest Federal State of Germany) and Bavaria (representing about 40 % of all municipalities with a rent control). March 2015 has an important role for the rent control since the underlying

[12] This price segment may rather be affected by the construction of the rent control which allows new contracts above the maximum price level when the price in the previous contract already exceeded the maximum level.

[13] The event-study is balanced in the relative time (adjusted to the time-lead and time-lag to the respective introduction). Consequently, it is not balanced in calender month and the sample differs from the *triple differences* sample. Estimating the *triple differences* in the event-study sample does not change the results as shown in column (i) of Table A.4.

[14] Results from the underlying estimation can be found in Table A.4.

FIGURE 5
EVENT STUDY



SOURCE.—Authors' calculations based on RWI-GEO-RED and information on rent control obtained from federal state laws.
NOTES.—The treatment variable is defined as interaction between treatment group and the time difference between the offer and the actual application of the rent control. Standard errors are clustered at the post code level. Results from the underlying estimation can be found in Table A.4.

national law which allows the states to introduce a rent control passed the German parliament (*Deutsche Bundestag*) in March 2015. Before the further event study design is presented, we pursuit the possible effects stemming from the anticipation of rent control. Column (ii) of Table A.3 in the appendix is based on the estimation from the *triple differences* approach and additionally include an indicator for post-March-2015 observations. It reveals that the pure publication of the law already had a substantial negative effect on rental prices of dwellings which were affected by a later introduction of the rent control. Therefore, the observed dips in the pre-treatment period of the event study seem to be in line with the observation that the announcement of the law already had an effect on rental prices.

After the application of rent control, the difference in price trends becomes clearly negative, as prices increase slower in treated than in control dwellings and regions. The effect is strongest after five months with -6.9 %, and tending to -4.4 % after 11 months. After one year, i.e. 12 months, the effect of the rent control becomes statistically insignificant.

5 Conclusion

The implemented rent control for new rental contracts was intended to be a speedy solution against accelerating rental price increases. Initially, tenants in existing contracts were already well protected by strict price regulations, putting all pressure for price adaptions on new tenants. So, tenants and political actors alike had high expectations for the effectiveness of the new rent control to stop rental price growth (in non-modernised dwellings). We conduct a thorough empirical analysis, using data from rental offers and given an econometric approach to exploit variation generated by the implementation of rent control.

To prevent our approach from various endogeneity problems, the chosen empirical approach exploits both the regional variation in the application of rent control by the Federal States as well as variation over different dwellings, where new objects are exempt from regulation, in addition to variation over time. In combination with time variation, we set up a *triple differences* estimator to estimate the causal effect of rent control on rental prices. By taking all German municipalities into account, spillover effects caused by households who move to neighbouring municipalities in order to evade the regulation do not play a major role in our approach.

Based on this comprehensive approach, we find an average effect of rent control on treated dwellings in treated municipalities of -2.5 %. Moreover, our rich dataset allows to shed light on effect heterogeneity, showing that similar dwellings with a relatively low quality and in the lower price segment drive the price dampening effect. This is in accordance with the target group of the rent control, low- and medium-income households who are likely unable to pay continuously increasing rents.

However, given the high expectations, the estimated effect is very small. Taking the average effect of -2.5 %, we exploit an example for a 60 sqm dwelling with three rooms in Berlin. Given a standard dwelling observed under rent control in 2016, the monthly price per square meter is estimated to be 8.30 Euro. Consequently, without rent control implementation, the price would have increased to 8.51 Euro. For a dwelling sized 60 sqm, a tenant pays about 12.50 Euro less by the introduction of the rent control. The stronger affected subgroups show slightly higher effects of about 14 Euro (for dwellings in the lowest price category) and 21 Euro (for low-quality dwellings). Though these effects are robust, the tense situation of low-income tenants is not changed substantially by the rent control.

Moreover, in-depth analyses from an event-study design reveal that the effect has its maximum magnitude after about six month and decreases thereafter. About one to one and a half year after the implementation, the effect vanishes. Even the government admits that the effectiveness of rent control lags behind its high expectations. One reason why the effectiveness of rent control does not meet these expectations might be incomplete control of tenants to prove the existence of too-highly-raised rents and missing sanctions against owners that do not obey the rent control.

The Federal Government adjusted the rent control law to improve effectiveness of the regulation. Thereby, an obligation for the landlord to disclosure concerning the pre-tenant rent should increase transparency and security of tenants. Sanctions of violations of the rent control and reduced requirements for objections could additionally strengthen the position of tenants to enforce the effectiveness of the regulation.

To conclude, rent control cannot be the single solution for housing shortage. It is a fast but short-lived answer to the problem of rising rental prices in the cities and the congested areas. The regulation is effective, but on a small scale and only in the short run. In the long run, rental price growth for regulated objects returns to the overall trend. Moreover, rent control does not set incentives to promote additional housing supply.

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Appendix A Appendix

A.1 Tables

TABLE A.1
SUMMARY STATISTICS FROM RWI-GEO-RED

Variable	Mean	Std. Dev.	Min.	Max.
Rent (per sqm)	7.55	2.6	3.79	17.65
Age	33.71	38.66	0	517
Age unknown	0.33	0.47	0	1
Living space (sqm)	71.86	25.77	16.17	179.98
Floor of object	1.52	1.61	0	14
Number of floors	1.94	2.06	0	14
Number of rooms	2.54	0.92	1	10
Year	2014.76	1.38	2013	2017
Elevator	0.18	0.39	0	1
Balcony	0.62	0.48	0	1
Kitchenette	0.36	0.48	0	1
(Shared) garden	0.19	0.39	0	1
Heating costs covered by inclusive rent	0.57	0.49	0	1
Cellar	0.62	0.49	0	1
Condition				
First occupancy	0.04	0.2	0	1
First occupancy after reconstruction	0.05	0.21	0	1
Like new	0.07	0.26	0	1
Reconstructed	0.08	0.27	0	1
Modernised	0.06	0.25	0	1
Completely renovated	0.1	0.3	0	1
Well kempt	0.27	0.44	0	1
Needs renovation	0.01	0.08	0	1
Dilapidated but negotiable	0.01	0.09	0	1
Heating type				
Heating Type (not selected)	0.23	0.42	0	1
Cogeneration/combined heat and power plant	0	0.05	0	1
Electric heating	0	0.05	0	1
Self-contained central heating	0.1	0.3	0	1
District heating	0.04	0.19	0	1
Floor heating	0.02	0.15	0	1
Gas heating	0.04	0.2	0	1
Wood pellet heating	0	0.04	0	1
Night storage heaters	0	0.06	0	1
Heating by stove	0	0.06	0	1
Oil heating	0.01	0.11	0	1
Solar heating	0	0.02	0	1
Thermal heat pump	0	0.05	0	1
Central heating	0.55	0.5	0	1
Unknown	0.32	0.47	0	1

NOTES.—The number of observations is 5,395,546.
SOURCE.—Authors' calculations based on RWI-GEO-RED.

TABLE A.2
MAIN REGRESSION RESULTS – FULL

	DiD Region (1)	DiD Condition (2)	DiDiD (3)
Age	-0.001***	-0.001***	-0.001***
	(0.0001)	(0.0001)	(0.0001)
Age unknown	-0.034***	-0.076***	-0.060***
	(0.0024)	(0.0042)	(0.0023)
Age squared	0.000***	0.000***	0.000***
	(0.0000)	(0.0000)	(0.0000)
Living space (sqm)	-0.002***	-0.002***	-0.001***
	(0.0000)	(0.0001)	(0.0000)
Floor of object	-0.003***	0.001	-0.003***
	(0.0005)	(0.0007)	(0.0005)
Floor unknown	0.000	-0.017***	0.001
	(0.0014)	(0.0028)	(0.0013)
Number of floors	-0.006***	-0.005***	-0.007***
	(0.0007)	(0.0010)	(0.0007)
Number of floors unknown	-0.027***	-0.036***	-0.034***
	(0.0018)	(0.0031)	(0.0019)
Number of rooms	-0.001	0.009***	-0.003***
	(0.0010)	(0.0019)	(0.0010)
Elevator	0.037***	0.035***	0.049***
	(0.0025)	(0.0031)	(0.0025)
Elevator unknown	-0.005***	-0.001	-0.005***
	(0.0014)	(0.0023)	(0.0014)
Balcony	0.020***	0.008***	0.020***
	(0.0013)	(0.0023)	(0.0014)
Balcony unknown	0.014***	0.028***	0.014***
	(0.0014)	(0.0024)	(0.0015)
Kitchenette	0.058***	0.067***	0.055***
	(0.0015)	(0.0027)	(0.0015)
Kitchenette unknown	-0.004***	0.002	-0.006***
	(0.0013)	(0.0024)	(0.0013)
(Shared) garden	0.020***	0.027***	0.022***
	(0.0012)	(0.0023)	(0.0012)
(Shared) garden unknown	0.020***	0.031***	0.020***
	(0.0015)	(0.0025)	(0.0016)
Cellar	-0.012***	-0.018***	-0.013***
	(0.0012)	(0.0022)	(0.0013)
Cellar unknown	0.015***	0.020***	0.013***
	(0.0016)	(0.0028)	(0.0015)
Cogeneration/combined heat and power plant	0.003	-0.008	0.006
	(0.0087)	(0.0172)	(0.0097)
Electric heating	-0.056***	-0.095***	-0.083***
	(0.0072)	(0.0162)	(0.0079)
Self-contained central heating	-0.032***	-0.059***	-0.055***
	(0.0063)	(0.0139)	(0.0071)
District heating	-0.021***	-0.037***	-0.044***
	(0.0065)	(0.0139)	(0.0073)
Floor heating	0.020***	0.016	0.023***
	(0.0066)	(0.0143)	(0.0074)
Gas heating	-0.041***	-0.095***	-0.066***
	(0.0069)	(0.0150)	(0.0077)
Night storage heaters	-0.084***	-0.099***	-0.108***
	(0.0069)	(0.0144)	(0.0076)
Heating by stove	-0.086***	-0.127***	-0.112***
	(0.0072)	(0.0145)	(0.0079)

Oil heating	-0.040***	-0.067***	-0.067***
	(0.0062)	(0.0137)	(0.0070)
Solar heating	0.029***	0.010	0.029***
	(0.0089)	(0.0179)	(0.0096)
Thermal heat pump	0.019***	0.023	0.038***
	(0.0066)	(0.0146)	(0.0075)
Central heating	-0.022***	-0.049***	-0.046***
	(0.0061)	(0.0136)	(0.0069)
Type of heating (unknown)	-0.031***	-0.087***	-0.059***
	(0.0065)	(0.0139)	(0.0074)
Simple equipment	-0.046***	-0.059***	-0.064***
	(0.0034)	(0.0057)	(0.0033)
Normal equipment	-0.007***	-0.013***	-0.018***
	(0.0024)	(0.0038)	(0.0022)
Sophisticated equipment	0.074***	0.100***	0.087***
	(0.0025)	(0.0043)	(0.0025)
Equipment unknown	0.013***	0.021***	0.002
	(0.0023)	(0.0040)	(0.0023)
First occupancy after reconstruction	-0.076***		
	(0.0039)		
Like new	-0.107***		
	(0.0029)		
Reconstructed	-0.164***		
	(0.0038)		
Modernised	-0.190***		
	(0.0038)		
Completely renovated	-0.183***		
	(0.0039)		
Well kempt	-0.185***		
	(0.0033)		
Needs renovation	-0.258***		
	(0.0042)		
Dilapidated but negotiable	-0.219***		
	(0.0060)		
Dilapidated	-0.238***		
	(0.0323)		
Unknown	-0.202***		
	(0.0038)		
Municipality with rent control	0.023**		0.034***
	(0.0111)		(0.0112)
Rent control applied (β_{RT})	0.003	0.005	0.021***
	(0.0024)	(0.0042)	(0.0044)
Old building		-0.058***	-0.068***
		(0.0026)	(0.0042)
Municipality with rent control × old building (β_{RC})			-0.015***
			(0.0031)
Rent control applied × old building (β_{TC})		-0.026***	
		(0.0031)	
Municipality with rent control × rent control applied × old building (α_{RCT})			-0.025***
			(0.0044)
Indicator for calendar month of rental offer	YES	YES	YES
Postal code fixed effects	YES	YES	YES
Old building × calendar month of rental offer			YES
Observations	5,395,546	2,168,403	5,395,546
R^2	0.724	0.552	0.714

NOTES.—The constant is not reported. Standard errors are robust to clustering at postal code level and are presented in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. SOURCE.—Authors' calculations based on RWI-GEO-RED and municipalities with rent controls obtained from respective laws on Federal State level.

TABLE A.3
REGRESSION RESULTS: ROBUSTNESS TESTS

	DiDiD on Event-Study Period	Announcement Effect
Age	-0.001***	-0.001***
	(0.0001)	(0.0001)
Age unknown	-0.059***	-0.060***
	(0.0024)	(0.0023)
Age squared	0.000***	0.000***
	(0.0000)	(0.0000)
Living space (sqm)	-0.002***	-0.001***
	(0.0001)	(0.0000)
Floor of object	-0.004***	-0.003***
	(0.0005)	(0.0005)
Floor unknown	0.006***	0.001
	(0.0013)	(0.0013)
Number of floors	-0.009***	-0.007***
	(0.0006)	(0.0007)
Number of floors unknown	-0.037***	-0.034***
	(0.0018)	(0.0019)
Number of rooms	-0.002	-0.003***
	(0.0011)	(0.0010)
Elevator	0.060***	0.049***
	(0.0026)	(0.0025)
Elevator unknown	-0.004**	-0.005***
	(0.0016)	(0.0014)
Balcony	0.022***	0.020***
	(0.0014)	(0.0014)
Balcony unknown	0.013***	0.014***
	(0.0017)	(0.0015)
Kitchenette	0.056***	0.055***
	(0.0017)	(0.0015)
Kitchenette unknown	-0.004***	-0.006***
	(0.0015)	(0.0013)
(Shared) garden	0.023***	0.022***
	(0.0011)	(0.0012)
(Shared) garden unknown	0.014***	0.020***
	(0.0017)	(0.0016)
Cellar	-0.011***	-0.013***
	(0.0013)	(0.0013)
Cellar unknown	0.010***	0.013***
	(0.0016)	(0.0015)
Cogeneration/combined heat and power plant	0.004	0.006
	(0.0098)	(0.0097)
Electric heating	-0.086***	-0.083***
	(0.0080)	(0.0079)
Self-contained central heating	-0.060***	-0.055***
	(0.0072)	(0.0071)
District heating	-0.049***	-0.045***
	(0.0074)	(0.0073)
Floor heating	0.020***	0.023***
	(0.0074)	(0.0074)
Gas heating	-0.068***	-0.066***
	(0.0078)	(0.0077)
Night storage heaters	-0.110***	-0.108***
	(0.0076)	(0.0076)
Heating by stove	-0.114***	-0.112***
	(0.0081)	(0.0079)

Oil heating	-0.068***	-0.067***
	(0.0070)	(0.0070)
Solar heating	0.024***	0.029***
	(0.0095)	(0.0095)
Thermal heat pump	0.036***	0.038***
	(0.0075)	(0.0075)
Central heating	-0.049***	-0.046***
	(0.0069)	(0.0069)
Type of heating (unknown)	-0.059***	-0.059***
	(0.0075)	(0.0074)
Simple equipment	-0.063***	-0.064***
	(0.0033)	(0.0033)
Normal equipment	-0.018***	-0.018***
	(0.0023)	(0.0022)
Sophisticated equipment	0.084***	0.087***
	(0.0025)	(0.0025)
Equipment unknown	0.001	0.002
	(0.0023)	(0.0023)
Municipality with rent control	0.039***	0.030***
	(0.0114)	(0.0112)
Rent control applied (β_{RT})	0.009*	0.008
	(0.0048)	(0.0053)
Old building	-0.033***	-0.054***
	(0.0048)	(0.0034)
Municipality with rent control × old building (β_{RC})	-0.025***	-0.013**
	(0.0048)	(0.0056)
Municipality with rent control × rent control applied	-0.014***	-0.012***
× old building (α_{RCT})	(0.0039)	(0.0033)
Announcement		0.111***
		(0.0066)
Announcement × municipality with rent control		0.017***
		(0.0055)
Announcement × old building		0.007
		(0.0059)
Announcement × Municipality with rent control		-0.015**
× old building		(0.0058)
Indicator for calendar month of rental offer	YES	YES
Postal code fixed effects	YES	YES
Condition	YES	YES
Old building × calendar month of rental offer	YES	YES
Observations	4,363,759	5,395,546
R^2	0.717	0.714

NOTES.—The constant is not reported. Standard errors are robust to clustering at postal code level and are presented in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
SOURCE.—Authors' calculations based on RWI-GEO-RED and municipalities with rent controls obtained from respective laws on Federal State level.

TABLE A.4
REGRESSION RESULTS: UNDERLYING ESTIMATES FROM THE EVENT STUDY

	(1)
Age	-0.001***
	(0.0001)
Age unknown	-0.059***
	(0.0024)
Age squared	0.000***
	(0.0000)
Living space (sqm)	-0.002***
	(0.0001)
Floor of object	-0.004***
	(0.0005)
Floor unknown	0.006***
	(0.0013)
Number of floors	-0.009***
	(0.0006)
Number of floors unknown	-0.038***
	(0.0018)
Number of rooms	-0.002
	(0.0011)
Elevator	0.061***
	(0.0026)
Elevator unknown	-0.003*
	(0.0016)
Balcony	0.022***
	(0.0014)
Balcony unknown	0.013***
	(0.0016)
Kitchenette	0.056***
	(0.0017)
Kitchenette unknown	-0.003**
	(0.0015)
(Shared) garden	0.022***
	(0.0011)
(Shared) garden unknown	0.013***
	(0.0017)
Cellar	-0.011***
	(0.0013)
Cellar unknown	0.010***
	(0.0016)
Cogeneration/combined heat and power plant	0.004
	(0.0097)
Electric heating	-0.087***
	(0.0079)
Self-contained central heating	-0.060***
	(0.0071)
District heating	-0.049***
	(0.0073)
Floor heating	0.020***
	(0.0073)
Gas heating	-0.068***
	(0.0078)
Night storage heaters	-0.111***
	(0.0075)
Heating by stove	-0.115***
	(0.0081)
Oil heating	-0.068***

	(0.0069)
Solar heating	0.024***
	(0.0094)
Thermal heat pump	0.037***
	(0.0074)
Central heating	-0.050***
	(0.0068)
Type of heating (unknown)	-0.059***
	(0.0074)
Simple equipment	-0.061***
	(0.0033)
Normal equipment	-0.016***
	(0.0023)
Sophisticated equipment	0.085***
	(0.0025)
Equipment unknown	0.003
	(0.0023)
Municipality with rent control	0.030**
	(0.0135)
Old building	-0.050***
	(0.0017)
Municipality with rent control × old building	-0.009
	(0.0081)
Old building × time difference between calendar month	-0.007
of rental offer and introduction of rent control (β_j): $\tau = -21$	(0.0087)
$\tau = -20$	-0.012
	(0.0088)
$\tau = -19$	-0.009
	(0.0084)
$\tau = -18$	0.001
	(0.0090)
$\tau = -17$	-0.001
	(0.0100)
$\tau = -16$	0.001
	(0.0096)
$\tau = -15$	-0.003
	(0.0091)
$\tau = -14$	0.002
	(0.0086)
$\tau = -13$	-0.001
	(0.0085)
$\tau = -12$	0.013
	(0.0105)
$\tau = -11$	0.016*
	(0.0100)
$\tau = -10$	0.016
	(0.0110)
$\tau = -9$	-0.000
	(0.0111)
$\tau = -8$	-0.018*
	(0.0097)
$\tau = -7$	-0.014
	(0.0090)
$\tau = -6$	-0.003
	(0.0079)
$\tau = -5$	-0.006
	(0.0057)
$\tau = -3$	-0.020**

	(0.0095)
$\tau = -2$	-0.011
	(0.0077)
$\tau = -1$	-0.021***
	(0.0076)
$\tau = 0$	-0.033***
	(0.0082)
$\tau = +1$	-0.036***
	(0.0086)
$\tau = +2$	-0.042***
	(0.0095)
$\tau = +3$	-0.055***
	(0.0086)
$\tau = +4$	-0.071***
	(0.0081)
$\tau = +5$	-0.091***
	(0.0086)
$\tau = +6$	-0.070***
	(0.0091)
$\tau = +7$	-0.054***
	(0.0092)
$\tau = +8$	-0.052***
	(0.0089)
$\tau = +9$	-0.067***
	(0.0091)
$\tau = +10$	-0.058***
	(0.0087)
$\tau = +11$	-0.066***
	(0.0093)
$\tau = +12$	-0.038***
	(0.0089)
$\tau = +13$	-0.027***
	(0.0083)
$\tau = +14$	-0.019**
	(0.0087)
$\tau = +15$	-0.033***
	(0.0094)
$\tau = +16$	-0.023**
	(0.0106)
$\tau = +17$	-0.009
	(0.0093)
$\tau = +18$	-0.003
	(0.0091)
$\tau = +19$	-0.006
	(0.0085)
$\tau = +20$	-0.011
	(0.0085)
$\tau = +21$	-0.002
	(0.0089)
Indicator for calendar month of rental offer	YES
Indicator for time difference between calendar month of rental offer and introduction of rent control	YES
Postal code fixed effects	YES
Condition	YES
Old building $\times$ calendar month of rental offer	YES
Observations	4,363,759
R^2	0.718

NOTES.—The constant is not reported. Standard errors are robust to clustering at postal code level and are presented in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
SOURCE.—Authors' calculations based on RWI-GEO-RED and municipalities with rent controls obtained from respective laws on Federal State level.