Forward to the Past: Short-Term Effects of the Rent Freeze in Berlin

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Abstract. In 2020, Berlin introduced a rigorous rent-control policy responding to soaring prices by capping rents: the *Mietendeckel* (rent freeze). The German Constitutional Court revoked the policy only one year later. Although successful in lowering rents during its duration, the consequences for Berlin's rental market and close-by markets are per se not clear. This article evaluates the short-term causal supply-side effects in terms of prices, quantities, and landlords' strategic behavior. We develop a theoretical framework capturing the key features of first-generation rent control policies and Berlin-specific aspects. Using a rich pool of detailed rent advertisements, predictions are tested, and further empirical causal inference techniques are applied for comparing price trajectories of dwellings inside and outside the policy's scope. Mechanically, advertised rents drop significantly upon the policy's enactment. A substantial rent gap along Berlin's administrative border emerges, and rapidly growing rents in Berlin's (unregulated) adjacent municipalities are observed. Landlords started adopting a hedging strategy insuring themselves against the risk of con-tractually long-term fixed low rents following a potentially unconstitutional law. Whereas this hedge was beneficial for landlords, the risk was completely borne by tenants. Moreover, the number of available properties for rent dropped significantly, a share of which appears to be permanently lost for the rental sector. This hampers a successful housing search for first-time renters and people moving within the city. Overall, negative consequences for renters appear to outweigh positive ones.

In many cases rent control appears to be the most effi-cient technique presently known to destroy a city—except for bombing (Assar Lindbeck, 1972, as cited in Rydenfelt 1981, p. 213).

1. Introduction

In 2020, Germany's capital, Berlin, introduced and soon after revoked a rigorous and old-fashionably designed rent control policy. For decades, Germany—where rent-ing is the dominant tenure type in cities—had in place relatively moderate rent control policies. Yet, as rents started to rise rapidly in the 2010s, Germany began to expand rent control again until, in February 2020, a more radical additional policy came into force: the rent freeze (in German *Mietendeckel*), a policy responding to soaring rents by basically switching off fundamental market economy mechanisms. Globally speaking, such old-fashioned policies had by then already been largely substituted by less strict regulations combining market economy features with tenant-protective measures. Yet the Berlin rent freeze meant a step back in time, poten-tially marking a third phase in the history of rent control: currently, lawmakers in, for example, Germany, France, Spain, and the United States are debating introducing

similarly strict policies detrimental to market economy principals.

The rent freeze was successful in temporarily lowering the overall price level for newly advertised rents within Berlin. However, it is not clear what the nuanced additional consequences for Berlin's rental housing market and neighboring areas would be. To our knowledge, this article is first in jointly exploring the immediate causal effects of the rent freeze on rent prices within and around Berlin and the quantity of flats to rent on the market and investigating landlords' reactions to rent regulations standing on shaky legal ground. Therefore, we focus on supply-side effects by assessing changes in landlords' decisions to advertise properties for rent upon vacancy.

We analyze these participation decisions in a simple theoretical framework of the rental property market that captures the key features of the rent freeze in Berlin and, subsequently, test its predictions by relying on microdata on rent advertisements. In addition, we employ causal inference techniques to measure the size of immediate price effects within and around Berlin. We find a substantial decline in the number of available rental properties in Berlin combined with an immediate drop in advertised rent prices. Prices decrease for regulated flats as compared with exempt ones within Berlin. Assessing the border region, we find a significant enlargement of the gap between advertised rents for properties targeted by the policy along the border with Berlin's surrounding federal state Brandenburg as a consequence of the rent freeze.

During its existence, Berlin's rent freeze determined a maximum rent price per square meter. To a certain extent, it was permitted to account for usual price-driving attributes, such as location and extraordinary provisions. In such cases, the rent freeze allowed strictly predefined markups to the basic rent. Yet the result was still an unambiguous maximum price ranging between 3.92 and 9.80 €/m^2. Landlords could undercut this price, but exceeding it could have been sanctioned. Because of these features, Berlin's rent freeze can be labeled a first-generation rent control policy as opposed to today's standard stabilizing second-generation policies tailored around limiting rent increases for sitting tenants (see Arnott 2003).

In general, economic theory does not support the use of first-generation rent control.[1] Whereas it may be beneficial for existing tenants, the consensus is that a ceiling on rents reduces both the quantity and quality of housing available in the market (Arnott 1995). For instance, owners seeing the value of renting to fall retard maintenance or convert their rental unit into owner-occupied housing. The shortage in supply may also force new residents to live in suboptimal locations and pay relatively higher uncontrolled rents (Early 2000). Glaeser and Luttmer (2003) show that the welfare costs of misallocation may be even greater than the efficiency loss

stemming from supply shortages. More recently, Borck and Gohl (2021) show that a rental cap, such as the rent freeze in Berlin, reduces welfare across all income groups with poorer households being affected worst.

The theoretical framework used in this article adds to this list by analyzing a number of other channels through which the rent freeze affects the supply of rental units in the short run. First, we show that forward-looking owners might exit the rental market even upon announcement of such a rent control policy. Moreover, this response is reinforced by the incentive to keep a unit vacant for refurbishment and modernization, which gives an opportunity to be exempt from rent control. Such a reaction reduces the quantity of more affordable housing units.

Finally, we analyze the implications of the occurrence of double-pricing rents, a practice adopted by owners in Berlin to hedge the expected foregone rents, as the constitutional basis for the rent freeze was shaky: advertisements would state a rent following the rent freeze rules as well as a (substantially higher) one payable in case the law was declared unconstitutional. We show that the adoption of double-pricing mitigates the negative impact of the rent freeze on the supply of rental units, but it does not offset it.

We make use of a comprehensive pool of rent advertisements to empirically test for model predictions and further price effects. Using a hedonic difference-in-differences (DiD) approach, we document a remarkable immediate aggregate drop of 7%–11% in advertised rent prices as compared with prices of unregulated units. Whereas comovements between sales and rent prices had been rather the norm, the two indices follow opposing trends ever since the rent freeze's enactment, hinting at a substitution effect between sectors, which is also supported by the exceptionally large number of units converted from rental to owner-occupied dwellings following the introduction of this policy. We document a leakage and likely second substitution effect for Berlin's neighboring city Potsdam as well as for further small surrounding municipalities, where asking rents were surging at an accelerated pace ever since the rent freeze came into force. We further estimate spatial difference-in-discontinuity models to assess price effects directly along the administrative border with Berlin's surrounding federal state, Brandenburg, which was not covered by the policy. Indeed, we detect an enlarged discontinuity in rents along the city border.

Next to price effects, we document a substantial and likely lasting decline in the number of rental units in Berlin. The incentives set by the rent freeze encourage modernization of the housing stock. This comes at the expense of a loss of affordable older units and encourages the conversion of rental units into owner-occupied properties. We find empirical evidence supporting that three channels led to this decline: increased conversions of rental to

owner-occupied units, a reduction in newly built dwellings, and a drop in property advertised for rent.

Because of reduced supply, the housing search within the rental segment is increasingly challenging for both established households in Berlin aiming for life cycle adaptations and would-be renters. These include newcomers and young people facing a double burden: low (initial) income and reduced availability of suitable housing options. The latter is quite problematic as people aged between 18 and 35 years are the largest group moving into German cities (Kholodilin 2017b).

Thus, our study shows that plain rent freezes bring more harm than good. Therefore, alternative policies may be preferable. These include established second-generation rent control policies and also completely different regulatory attempts theoretically and empirically found to be effective in tackling issues concerning housing supply shortage or affordability: for instance, Segú (2020) finds a substantial long-term causal increase in available housing units because of vacancy taxes in France, Agarwal et al. (2019) document how increases in minimum wages translate into over-proportional drops in rental defaults in the United States, and Curry and Gensch (1975) demonstrate how incentives for renovation in older residential neighborhoods can increase the quality and quantity of the local rental housing stock for low-income families.

The remainder of this article is organized as follows: Section 2 not only discusses the international, historic, and regulatory context of the rent freeze, but also describes its key distinctive features. In Section 3, we develop the theoretical framework, and in Section 4, we describe the data used for the empirical assessment. Next, we examine price and quantity effects in Sections 5 and 6, respectively. Finally, Section 7 conducts a variety of robustness checks, and Section 8 concludes. A comprehensive online appendix provides further details.

2. Historic, International, and Regulatory Context

This section discusses the historical and regulatory context surrounding the rent freeze in Berlin. After an overview of rent regulation in Germany in comparison with other countries, the nation-wide regulations in place in Germany in 2022 are discussed. The section concludes by describing the additional specific rules of the rent freeze.

2.1. A Visual History of Rent Control in Germany
Rent control in Germany has a long tradition dating back to 1919 (see Kholodilin 2017a), which likely contributes to the fact that, until now, renting is the dominant tenure status in German cities: in 2018, the rental housing rate in Berlin amounted to almost 83% of which the

Figure 1. Rent Control Regulation Intensity, 1910–2021
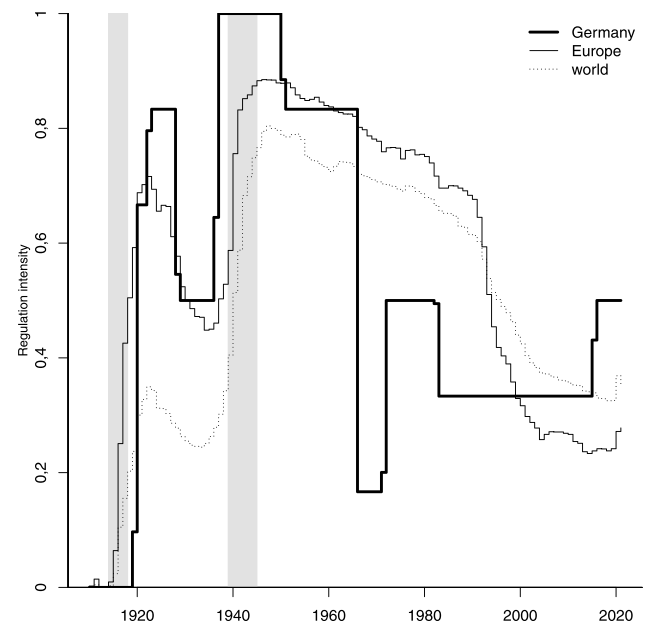
Source. Updated calculations based on Kholodilin (2020b).
Notes. The figure depicts the intensity of rent control policies in Germany and compares it to the situation in Europe (40 countries) and the rest of the world (125 countries and subnational regions). The gray shaded bars indicate World Wars I and II, respectively. The regulation intensity is computed as a simple average of six binary indices, each reflecting an aspect of rent control (e.g., real and nominal freeze, setting of the initial level of rent, and various exceptions).

vast majority were regular units (social housing units amounting to at most 5% of the total housing stock).[2]

Additional regulatory measures are usually put in place in extraordinary times, for example, world wars (see Kholodilin et al. 2021) and, most recently, in response to the global economic crisis triggered by the COVID-19 pandemic (see Kholodilin 2020a, Francke and Korevaar 2021). After historical high intensities during extreme events, a decades-long deregulation trend followed. Since 2015, the intensity of rental housing market regulations has again been increasing. This is shown in Figure 1, depicting the intensity of rent control measures in Germany between 1910 and 2021 and comparing it to Europe and the rest of the world.

2.2. National and International Resonance
Within Germany, the rent freeze received a lot of public attention: Figure 2 plots the number of occurrences of the word *Mietendeckel* (rent freeze) in German media between January 2018 and December 2021.[3] The rent freeze was mentioned on a couple of occasions first in 2018. However, in early 2019, the number of occurrences became nonnegligible. The topic was most prominently discussed between the policy's announcement and enactment and then, subsequently, upon its revocation and during the federal election campaign in September 2021

Figure 2. Coverage of the Rent Freeze in German Media
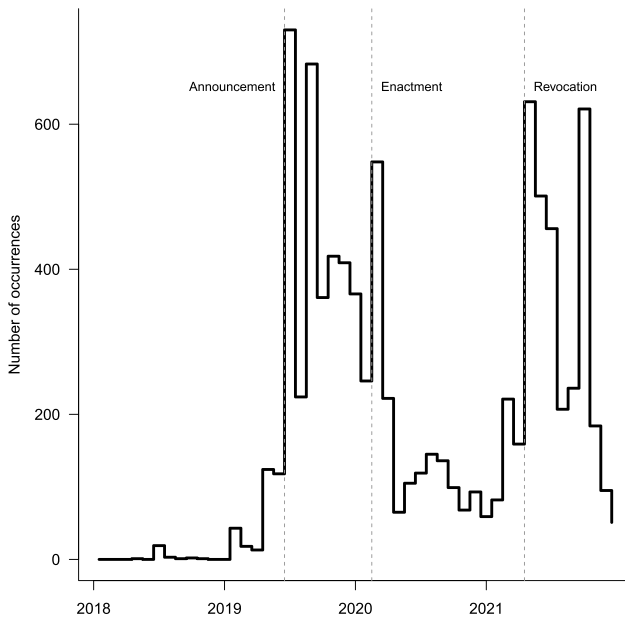
Source. Author's visualization based on data provided by *GENIOS*.
Notes. The figure shows the monthly number of occurrences of the word *Mietendeckel* (rent freeze) in the database of German print media GENIOS. The data are obtained through an automatic search for this keyword in the GENIOS database across all media items published between January 2018 and December 2021.

when the issue of introducing a nationwide rent freeze was raised.

The announcement of the rent freeze in Berlin also triggered broad international reactions with leading newspapers, such as *Financial Times* (United Kingdom), *Le Monde* (France), and *The New York Times* (United States) covering it. The example of Berlin also inspired other actors in different parts of Germany and abroad to request similar regulations. For instance, in October 2019, Munich's tenants' association launched an initiative to organize a referendum concerning the introduction of a similar rent freeze in the German federal state of Bavaria. In September 2020, Sadiq Khan, the mayor of London, suggested freezing private housing rents in the British capital for two years, alluding to the case of Berlin.[4]

2.3. Regulatory Context in Berlin

2.3.1. The Rent Brake. The new rent freeze regulation was preceded by the so-called rent brake introduced in June 2015.[5] According to this policy, the rent for a dwelling located in an area classified as a tight housing market (*angespannter Wohnungsmarkt*) may be at most 10% higher than the typical local rent.[6]

Thus, the rent brake is a stricter form of second-generation rent control because, in contrast to a standard version, it imposes limitations not only on price increases,

but also on the initial rent. By 2022, 13 out of 16 federal states had adopted the rent brake.[7]

Though initially set for five years, the law was prolonged for another five years in March 2020.[8]

2.3.2. The Rent Freeze. The idea of a rent freeze was publicly announced on June 4, 2019, by Berlin's then-minister of construction Katrin Lompscher (a member of the leftist political party *Die Linke*).[9] As an immediate reaction, on June 9, 2019, the landlords' and home-owners' association *Haus und Grund* called upon landlords to raise rents before June 18, 2019, hoping that, by this action, the rent determining the basis for rent setting would rise.[10] Initially, it was unclear whether the controversial law would indeed be enacted because the constitutional basis for law making in the domain of housing markets at the federal state level was shaky. Nevertheless, in late 2019, it became clear that the law would pass. In February 2020, Berlin then enacted the preannounced rent freeze,[11] only to be declared unconstitutional and overturned 13 months later on March 25, 2021, by the German constitutional court.

What are the main features of the short-lived law? Rents (excluding running costs) within the administrative borders of Berlin were ex post frozen at the June 18, 2019, level for five consecutive years. By default, all residential premises were covered. A number of exceptions were specified: most importantly those housing units that became ready for occupation for the first time on or after January 1, 2014; residential premises that were uninhabitable (and indeed vacant) for an extended period of time; or premises that were remodeled with efforts comparable to new construction (hence, new to the rental market).[12]

In addition to these features, the law defined the so-called valid rent (*zulässige Miete*) to range between 3.92 and 9.80 €/m^2 per month. The exact amount depended on the building's construction year and equipment (heating and bath; see Table 1). A somewhat higher rent is allowed for dwellings in two-family houses (+10%) or with modern equipment (+1 €/m^2).[13] In addition, the location was factored in: −0.28 €/m^2 for simple locations (*einfache Wohnlage*), −0.09 €/m^2 for average locations (*mittlere Wohnlage*), and +0.74 €/m^2 for good locations (*gute Wohnlage*).[14] Hence, the valid rent could have been at most 11.54 €/m^2 corresponding to a house built between 2003 and 2013, having central heating and a bathroom as well as general modern equipment, and located in a good neighborhood.

If, in June 2019, the rent paid in existing contracts was in excess of the valid rent by more than 20%, it would then be classified as excessive rent (*überhöhte Miete*). Starting from November 23, 2020, excessive rents must have been reduced to reach the maximal allowed level. Non-compliance was classified as an administrative offense and could lead to substantial fines up to €500,000.[15]

Table 1. Valid Rent Under the Rent Freeze

		Equipment			Valid rent [EUR/m^2]
		$CH \wedge B$	$CH \vee B$	$\neg CH \wedge \neg B$	
1.	Before 1918	✓			6.45
2.			✓		5.00
3.				✓	3.92
4.	1919–1949	✓			6.27
5.			✓		5.22
6.				✓	4.59
7.	1950–1964	✓			6.08
8.			✓		5.62
9.	1965–1972	✓			5.95
10.	1973–1990	✓			6.04
11.	1991–2002	✓			8.13
12.	2003–2013	✓			9.80

Source. "Gesetz zur Mietenbegrenzung im Wohnungswesen in Berlin (MietenWoG Bln)," section 6, as of February 11, 2020.
Note. The table reports the (unadjusted) valid rent per square meter depending on the year of first-time availability for rent and the provision of basic equipment ($CH \wedge B$ central heating and bathroom, $CH \vee B$ central heating or bathroom, $\neg CH \wedge \neg B$ neither central heating nor bathroom).

From January 1, 2022, onward, rents could only be increased in line with the growth rate of the Germany-wide consumer price index subject to a general cap of 1.3% but only if they would still be below the valid rent. Thus, rents equal to or exceeding the valid rent were effectively frozen. Finally, monthly rents may have been increased by no more than 1 €/m^2 in properties that underwent modernization and must have nevertheless followed the general guidelines.

2.3.3. Rent Brake vs. Rent Freeze. Under the rent freeze regime, valid rents were generally lower than those only following the rent brake guidelines. This is shown for 2019 in Figure 3, where the horizontal axis corresponds to the valid rent set by the rent freeze, whereas the vertical axis depicts the prices according to the rent brake. The different shades of gray denote different floor areas of dwellings, whereas the shape of the dots corresponds to the year of completion of the buildings. Rents refer to dwellings located in average zones.[16]

Most dots in Figure 3 lie above the 45° line, indicating that prices following the rent brake regime are generally higher than those stipulated by the rent freeze. We observe particularly large deviations between rent brake and freeze prices for small dwellings (living surface of 40 m^2 or less). The lowest rents per square meter are set for dwellings in buildings completed between 1965 and 1972. Moreover, for older dwellings, the rent per square meter is higher for smaller dwellings, whereas we do not detect such a monotonicity for dwellings completed after 1990.

In general, we conclude that prices set under the rent freeze regime lag behind the already low rental prices set under the rent brake regulation. However, this difference

disappears when taking into account the excessive rent threshold: 10% above the valid rental price for the rent brake and 20% for the rent freeze.

2.3.4. Further Fostering Policies. Policies directly targeting people in need are often considered as being a substitute to supply-side affordability interventions, such as the rent and rent freeze (see Olsen 2003, Eriksen and Ross 2015). During the period covered here, there were two such targeted fostering policies in place that could, at least in part, support residents facing housing affordability constraints:[17] social housing construction (*sozialer Wohnungsbau*) and housing allowances (*Wohngeld*). The former covers subsidized private or public construction of affordable housing units. In such dwellings, rents are substantially lower than regular market rents, yet tenants need to fulfill certain eligibility criteria mainly related to income.

Housing allowances are directly paid to tenants whenever their household income falls short of a certain minimum, depending on the number and age of household members. Social housing makes up just about 4% of the total housing stock across Germany (Pittini et al. 2019).

Figure 3. Valid Rent: Rent Freeze vs. Brake
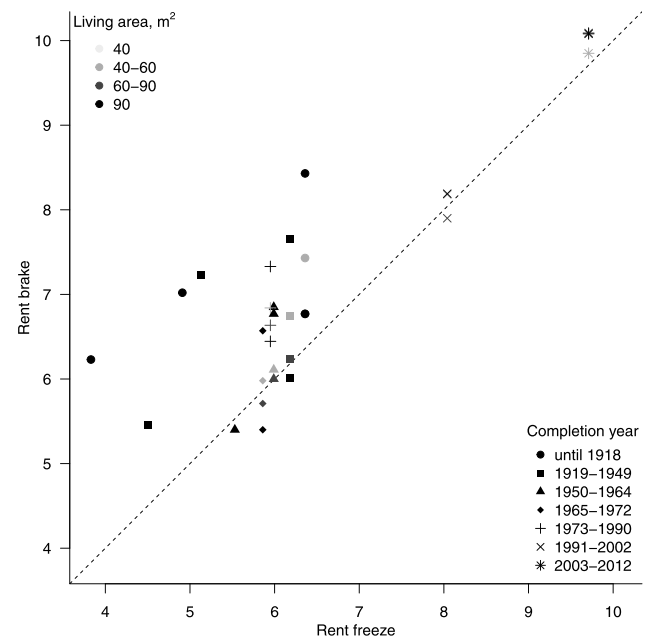
Sources. (1) *Mietendeckel*—"Gesetz zur Mietenbegrenzung im Wohnungswesen in Berlin (MietenWoG Bln)" as of February 11, 2020; (2) *Mietspiegel* of 2019—Senatsverwaltung für Stadtentwicklung und Wohnen, https://www.stadtentwicklung.berlin.de/wohnen/mietspiegel/de/downloads.shtml; and (3) own representation.
Notes. The figure shows the valid rental prices per square meter per month, according to the rent freeze law (horizontal axis) and rent brake law as defined in the *Mietspiegel* of 2019 (vertical axis). The diagonal dotted line has a slope of 45° and, thus, shows the points at which the values of both rent freeze and rent brake coincide.

Some measures to increase the financing of social housing were adopted since 2015.[18] Moreover, in 2021, the federal government announced plans to build up to 100,000 social housing units a year.[19] Housing allowances have also been reformed recently: amounts were increased, and more importantly, an automatic indexation mechanism was added. The latter links the amount granted to the nationwide official rent price index.[20]

3. Theoretical Framework

In this section, we characterize the economic incentives governing owners' decisions on how to use a currently rented property unit upon vacancy, that is, whether to readvertise it for rent or not, and subsequently analyze the impact of the rent freeze on these considerations. Thus, we focus on owners' participation decisions.[21] To do so, we develop a partial equilibrium model of the rental market and focus on the supply of rental units, taking the demand side as given.[22]

We proceed by setting up the environment and owners' incentives. Subsequently, we derive market participation conditions in the absence of rent control. Next, we extend this environment by introducing the rent freeze and analyze its impact on market participation, including the occurrence of double-pricing advertisements. Online Appendix A contains a more exhaustive exposition of the model.

3.1. A Partial Equilibrium Model of the Supply of Rental Units

3.1.1. Environment. We consider a mass of measure one of property units uniformly distributed in a city and owned by the same mass of infinitely lived owners. We define an owner $i \in \mathbb{N}$ as an economic agent owning a unit suitable in which to live. Time is discrete, and each period $t \in \mathbb{N}$ represents a month. We denote owners' discount factor with $\delta > 0$. We assume the ongoing rental price $R > 0$ at which units can be advertised and rented to be exogenous and constant over time. We also abstract from maintenance costs: as these are paid regardless of how units are used, they do not matter for participation decisions. Because we focus on a steady state in which all variables are time-constant, we refrain from time subscripts to ease the notational burden.

3.1.2. The Use of a Property Unit. In each period, a unit can be in one of four states: occupied and rented, vacant and employed for personal use, vacant and under refurbishment, or vacant and advertised in the rental market.

3.1.2.1. Rented. Owners of rented units enjoy the monetary benefits of receiving a monthly rent R. Further, for simplicity, we abstract from the possibility of end-of-tenancy separations: once a property is occupied and rented, it remains in this state forever.[23]

3.1.2.2. Personal Use. Units in this state are kept out of the rental market. This is to capture both owner occupation and its use for different purposes (e.g., as vacation house, for storage, or for sale in the housing market). We denote owner i's per-period benefit of keeping a unit for personal use by $b_i \geq 0$, where b is distributed according to the cumulative distribution function F. Hence, owners are heterogeneous along this dimension: the value of keeping a unit for personal use could depend on personal needs and preferences.

3.1.2.3. Under Refurbishment. Owners may decide to keep their property temporarily out of the rental market while they invest in refurbishment and modernization. We assume that these owners are able to advertise and rent their unit at a higher price $\hat{R} > R$ once refurbishment is completed. Hence, there are some expected future gains in investing in refurbishment, during which owners give up the benefits from personal use as well as the possibility of earning a monthly rent.

Denote by $T_i^r \in \mathcal{T}^r \subset \mathbb{N}$ the number of periods covering owner i's refurbishment duration. To preserve tractability, we define a mapping $\rho : \mathcal{T}^r \rightarrow [0,1]$, strictly decreasing on $\mathcal{T}^r$ with $\rho(0) = 1$ and $\lim_{T^r \rightarrow \infty} \rho(T^r) = 0$. One way to interpret ρ is to think of it as the per-period, constant probability that a refurbishment lasting T^r periods is successful; another interpretation is to think of it as an owner's discount factor for a payoff that comes T^r periods ahead in time. Henceforth, we treat ρ as a parameter and assume it is distributed according to the cumulative distribution function G. Further, we assume that all owners have access to credit and they use this channel to fund their investment in refurbishment.[24]

3.1.2.4. Advertised. Owners deciding to participate in the rental market advertise their unit at the ongoing rental price R in search of a suitable tenant. By doing so, owners give up the benefits of using the property for their personal use and pay an additional search cost denoted by $k > 0$. The per-period probability that an advertised unit is matched with a resident searching for a unit is exogenous and denoted by $q > 0$. We assume the present value of advertising a unit to be strictly positive and that matches are mutually advantageous: once a match is formed, the property becomes occupied, and the owner starts receiving the monetary benefit, that is, the monthly rent R.

3.1.3. Participation Conditions. Next, we derive the steady-state conditions under which it is optimal for owners to participate in the rental market, that is, we seek to find under which configuration of b_i and ρ_i are owners better off by advertising their unit for rent at the current rental price R rather than keeping it for personal use or investing in refurbishment.

To begin, denote the expected present discounted value of advertising a unit by V^a and the analog value of investing in refurbishment by V^r. An owner i finds it optimal to advertise the unit at the current rental price if $V^a_i > V^r_i$, that is, if

$$\frac{\delta}{1-\delta} R > \rho_i \frac{\delta}{1-\delta} \hat{R} + [1-\rho_i] \frac{k}{q}, \tag{1}$$

which implies that the present value of renting at price R must be larger than its opportunity cost in terms of the present value of renting after T^r_i periods at a higher price $\hat{R}$, net of the expected cost of search. Clearly, whether this condition is satisfied for owner i crucially depends on the duration of refurbishment: the longer the refurbishment period, the smaller the opportunity cost of advertising the unit at the current rental price. In other words, the right-hand side of (1) is decreasing in T^r_i.

Lemma 3.1. *There exists a threshold value $\tilde{\rho} = \tilde{\rho}(R, \hat{R})$ such that, for all $\rho_i < \tilde{\rho}$, $V^a_i > V^r_i$: advertising is always preferred to refurbishing. Moreover, $\tilde{\rho}$ is strictly increasing in R and strictly decreasing in $\hat{R}$.*

Next, consider an owner i's choice between advertising a unit or keeping it for personal use with corresponding expected present discounted value V^u_i. Advertising is optimal if $V^a_i > V^u_i$, that is, if

$$\frac{\delta}{1-\delta} R > \frac{1-\delta[1-q]}{q[1-\delta]} b_i + \frac{k}{q}. \tag{2}$$

An owner finds it optimal to advertise the unit if the present value of renting at the price R is larger than its opportunity cost in terms of the expected present value of using the unit for personal use plus the expected search cost. Hence, the greater the benefit b_i perceived by the owner of using the unit for personal use, the larger is the opportunity cost of advertising.

Lemma 3.2. *There exists a threshold value $\tilde{b} = \tilde{b}(R)$ such that, for $b_i < \tilde{b}$, $V^a_i > V^u_i$: advertising is always preferred to using the unit for personal use. Moreover, $\tilde{b}$ is strictly increasing in R.*

Thus, owners participating in the rental market are those for whom investing in refurbishing is not profitable as they would be better off in advertising their property for rent immediately and for whom the current rental price is high enough such that the present value of the income flow from renting outweighs the present value of the benefits that they perceive from keeping the unit vacant for themselves. These considerations enable us to establish the following result.

Proposition 3.1. *The steady-state share of units advertised in the rental market is given by*

$$\alpha(R, \hat{R}) = F(\tilde{b})G(\tilde{\rho}), \tag{3}$$

where α is strictly increasing in R and strictly decreasing in $\hat{R}$.

The higher the current rental price R, the larger the proportion of owners who prefer to advertise their unit in the market. Note, this effect works on both participation margins.

On the other hand, a higher expected gain from refurbishing a property, all else equal, decreases the share of owners participating in the rental market as they would rather invest in refurbishment and enjoy a higher stream of income in the future.

3.2. Rent Freeze, Double-Pricing, and Participation

Next, we use the theoretical framework to analyze the impact of the rent freeze on owners' participation decisions. Our assumptions and definitions capture the specific environment of Berlin's rent freeze. Hence, we explicitly consider the announcement, enactment, and subsequent abolishment of the rent freeze. Further, we also model and analyze the implications of owners adopting double-pricing advertisements, that is, the observed occurrence of advertisements listing two alternative prices: one for the duration of the rent freeze and a higher one contingent on the policy being abolished.

3.2.1. The Rent Freeze Environment. We define the rent freeze as a rent control policy under which the absolute monthly rental price is exogenously set below the current market price: $\overline{R} < R$. This feature is common to all first-generation rent control policies. Further, we assume that, once such a policy is announced, in each subsequent period, there is a constant probability $p \in [0, 1]$ that the policy is enacted and a probability $1 - p$ that the policy is not enacted.

Further, denote by $\overline{T} \in \overline{\mathcal{T}} \subset \mathbb{N}$ the expected number of periods covering the lifetime of the rent freeze and define the mapping $\gamma : \overline{\mathcal{T}} \to [0, 1]$, which is assumed to be strictly decreasing on $\overline{\mathcal{T}}$ with $\gamma(0) = 1$ and $\lim_{\overline{T} \to \infty} \gamma(\overline{T}) = 0$. The interpretation of γ is analogous to the interpretation of ρ and can be thought as the probability that the rent freeze policy is abolished after $\overline{T}$ periods or as the owners' discount factor for a payoff $\overline{T}$ periods ahead.

For simplicity, p and γ are common to all owners.

3.2.1.1. Double-Price Advertising. Owners adopting a double-pricing strategy advertise their unit with a clause: if the rent freeze policy is abolished, the matched tenant has to retrospectively reimburse the foregone monthly rent under the rent freeze $D = R - \overline{R}$ for the number of months the reduced rent freeze price was paid. If double-pricing is not adopted, $D = 0$.

Hence, conditional on the rent freeze policy being enacted, owners adopting double-pricing expect to receive a monthly rent of $\overline{R}$ for $\overline{T}$ periods, the expected duration of the rent freeze; an augmented monthly rent of $R + D$ for the subsequent $\overline{T}$ periods after the rent

freeze is abolished; and the market rental price R forever afterward.

3.2.2. The Impact of the Rent Freeze on Participation.
Analyzing the effect of the rent freeze on the supply of advertised units in the rental market requires analyzing the impact of a lower expected rental price on owners' participation decision captured by the steady-state share of advertised units α derived in Proposition 3.1.

Our framework is also suitable to derive predictions on the impact of the announcement of the policy, of following the double-price strategy, and of owners' expectations about the rent freeze lifetime.

To begin, considering the owners' choice of adopting double-price advertising in the environment of the rent freeze, denote by $\overline{V}^a$ and $\overline{V}^d$ the expected present discounted values of single- and double-price advertising, respectively, from the policy's announcement onward.

Lemma 3.3. *For all $p > 0$ and $0 < \gamma < 1$, $\overline{V}_i^d > \overline{V}_i^a$: double-price advertising is always preferred to single-price advertising.*

Lemma 3.3 implies that, as long as there is a positive probability that the rent freeze is enacted and, subsequently but not immediately, abolished, it is optimal for all owners to switch to double-pricing advertisements once the policy is announced. Indeed, the intuition is straightforward: double-pricing itself is free of cost yet increases the expected rent at which units can be advertised and rented once and if the rent freeze is abolished.

Next, denote the share of advertised units in the environment of the rent freeze by $\overline{\alpha}$ and consider the following proposition.

Proposition 3.2. *For all $p > 0$, $\gamma < 1$, and $D = \{0, R - \overline{R}\}$, the steady-state share of advertised units in the rental market after the rent freeze is announced decreases, that is,*

$$\overline{\alpha}(R, \hat{R}, \overline{R}, D, p, \gamma) < \alpha(R, \hat{R}),$$

where $\overline{\alpha}$ is strictly decreasing in p and strictly increasing in γ.

Proposition 3.2 establishes that the rent freeze negatively affects the supply of advertised units in the rental market. Importantly, this effect takes place as soon as the policy is announced and works on both participation margins as the enactment of the rent freeze also increases the expected gain from refurbishing. Perhaps surprisingly, the incentive to refurbish, which ideally aims to counteract the long-term negative effects of under-maintenance, exacerbates the negative impact of the rent freeze in the short run.

Moreover, the higher the probability of enactment (i.e., a higher p), the smaller the share of advertised units in the market, which implies that, once the policy is enacted (i.e., $p = 1$), the share of advertised units in the market is even smaller than at the time of the policy's announcement. Proposition 3.2 also establishes that, as owners expect the rent freeze to remain in place for longer (i.e., a lower γ), the drop in the share of advertised units in the market is greater.

The intuition behind these results resides in the effect of the rent freeze on owners' expectations of the rental price at which units can be advertised and rented in the future. In fact, the expectation of renting a unit at a lower price for the duration of the rent freeze lowers the present value of advertising and increases the opportunity cost of doing so: the expected gains from investing in refurbishment or from keeping the unit for personal use are then higher.

Finally, note that Proposition 3.2 holds even in the case of double-pricing. Although, under double-pricing, owners expect to be paid the foregone rent differential D, because these foregone rents are collected at a later date in the future once and only if the policy is abolished, their value is discounted: the present value of advertising with double prices in the environment of the rent freeze is smaller than if the rent freeze was never announced. Hence, whereas the double-pricing strategy dampens the negative effects of the rent freeze— as Lemma 3.3 implies—its adoption is not enough to entirely offset them.

4. Data
To assess the immediate impact of the rent freeze on advertised rents and test our theory, we use two data providers collecting rent advertisements: VALUE Marktdaten and Immobilienscout24. Immobilienscout24 is the most popular online real estate portal in Germany, whereas VALUE Marktdaten is a real estate data company pooling and harmonizing several real estate platforms, also including Immobilienscout24. For a fine-grained analysis of owners' applied hedging strategies, information on alternative rents (double pricing) not systematically collected by VALUE Marktdaten is needed.

Both sources are further complemented by the *Mietspiegel*, an official repository reporting the typical current rent paid in different parts of Berlin.

4.1. VALUE Marktdaten
We assess the policy's immediate impact on advertised asking rents. For this purpose, we primarily use online sale and rent advertisements collected and processed by VALUE Marktdaten (formerly Empirica Systeme).[25] This platform gathers ample information on all types of apartments and houses on the market by pooling a rich set of real estate information providers, including asking prices as well as various dwelling characteristics.[26] Importantly, it allows us to obtain precise information on location provided in the form of geocoded exact addresses mentioned in the advertisements.[27]

For our estimation sample, we exclude statistical outliers (properties older than 300 years) and units that, even if already advertised, were not built yet. In total, we exclude only eight observations, leaving 74,657 in the full estimation sample, covering the period between January 2018 and June 2021. Summary statistics compiled for all advertisements included in our sample are shown in Online Table C1.

More detailed statistics, compiled to assess the comparability of types of units advertised before and after the announcement and enactment of the rent freeze, are reported as a part of comprehensive robustness and plausibility checks in Online Appendix B.

4.2. Immobilienscout24

To obtain additional information on advertised rents, we contacted Immobilienscout24, the largest real estate advertisement platform. Doing so enables us to obtain information from advertisements' free-text fields (unfortunately not collected by VALUE Markdaten), which were frequently used to provide clarifications on the demanded rent in case the rent freeze was abolished. As the Immobilienscout24 data are, in fact, a subset of the data pooled by VALUE Markdaten, we exclusively rely on the Immobilienscout24 data for assessing the double-pricing strategy and use VALUE Markdaten for all other purposes.

Because of data-protection concerns, we were not provided with the full content of the free-text entries (which sometimes also contain owners' names and contact information). Thus, we asked Immobilienscout24 to provide us with results from a keyword search query within the free-text fields, next to the standard housing characteristics and asking prices. We summarize keywords used for this query in Table 2. We use only keywords for identification that were mentioned at least in 2% of all advertisements.[28]

Overall, we obtained more than 323,000 advertisements covering the period from January 2018 through June 2021. Online Table C5 reports summary statistics for the Immobilienscout24 data.

As the Immobilienscout24 data are a subset of the VALUE Marktdaten sample, we generally do not expect significant differences between the two. Still, we check for a systematic selection bias by comparing the respective summary statistics. Reassuringly, we do not detect any substantial difference. We also compile spatially stratified summary analyses (reported in Online Table C7) and do not find significant deviations between the two data sources.[29]

4.3. The *Mietspiegel*

The so-called *Mietspiegel*[30] is an official source of information on local rents paid for comparable dwellings that are used for determining the allowed rents. It is based on rent price data collected in the course of a representative survey among renters. The survey covers rental contracts concluded within the last six years and should be updated every two years.

Importantly, the *Mietspiegel* also contains information of the quality of each address in Berlin, and these are classified into three categories: simple (*einfache Wohnlage*), average (*mittlere Wohnlage*), and good (*gute Wohnlage*) locations. These classifications are used in determining rent caps in accordance with the rent freeze (see Section 2.3 for the detailed rules).

5. Immediate Price Effects

In this section, we empirically evaluate the immediate effects of the rent freeze on advertised rental prices. After descriptive analyses, including comparisons to sales and rent prices in other German cities, we measure causal price effects. To do so, we use two hedonic models designed as a difference-in-differences approach for measuring changes within Berlin and as a spatial difference-in-discontinuity model for measuring effects along the border with the surrounding federal state of Brandenburg.

5.1. Descriptive Analysis

To illustrate the general trends in Berlin's housing market, Figure 4 shows hedonic rent price indices (see Rosen 1974). To put movements into perspective, we also show a residential house sales price index based again on advertisements. Since roughly mid-2018, sales prices were increasing at a much more rapid pace than rents. Sales prices, though more volatile, also left an ever-increasing price trajectory, yet we do not observe declining prices. Whereas comovements between sales and rent prices were rather the norm before 2019, ever since the enactment of the rent freeze, the two indices follow opposing trends.

We identify increases in rent prices up until June 2019, after which advertised rents stagnate and then,

Table 2. Keywords for Double Rent Identification

	Keyword	Frequency of appearance, %
German	English	
MietenWoG	Abbreviation of the rent freeze law (technical term)	4.4
Mietendeckel	Rent ceiling	2.6
nichtig	Void, invalid (technical term)	2.2
verfassungswidrig	Unconstitutional	2.2

Source. Author's calculations based on data provided by Immobilienscout24.

Notes. The table summarizes the queries used to identify double rent prices in the context of the rent freeze. In addition to these keywords, we searched for numerical values and currency symbols that were retrieved whenever our search for keywords was successful.

Figure 4. Nominal Sales and Rent Price Indices: Berlin
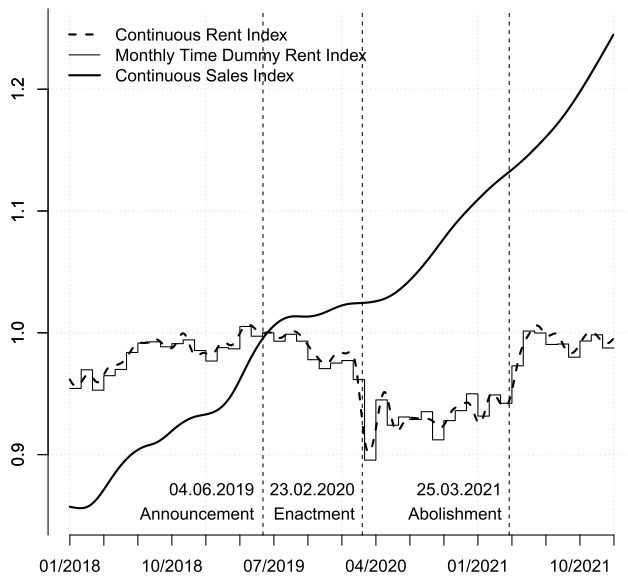
Source. Author's visualization based on data provided by VALUE Marktdaten.

Notes. The indices unveil the general trend in the sales and rental market between 2018 and 2021. Indices are normalized to their respective average index number in June 2019 when the announcement took place. The time-continuous indices follow the methodology developed in Waltl (2016) based on adaptive smoothing techniques. The continuous trend in the rental market is compared with a standard monthly time dummy index (see de Haan and Diewert 2013).

subsequently, start falling. The continuing decline in rents starting in February 2020 until revocation of the rent freeze already hints at substantial price effects related to the policy.

The exceptional disruptions in Berlin's rental market are even more visible when comparing changes in asking rents in Berlin to those in other major German cities as well as in Berlin's satellite city Potsdam and adjacent municipalities (see Figure 5 and Table 3). In 2020, whereas asking rents kept increasing at a similar pace in all other cities, asking rents in Berlin fell instead. Importantly, the adjacent areas as well as the satellite city Potsdam are—though part of the urban conglomerate—not covered by the rent freeze as they are located outside of the administrative boundaries of the German capital.

Particularly remarkable is the sharp rise in rents in Potsdam as reported in Table 3. The cumulative change since the announcement of the rent freeze amounts to roughly 5%, 9%, and 12% in the first three quarters of 2020, respectively. Comparing these increases to Germany-wide changes or other major cities, Berlin truly stands out. Smaller adjacent municipalities also experienced substantial increases comparable to those in Potsdam. These findings indicate a substitution effect likely triggered by the rent freeze, which exclusively applies to dwellings strictly located within Berlin's administrative boundaries.

Why are owners substantially increasing asking rents outside of Berlin? A possible explanation could rely on the one-time opportunity for owners of vacant apartments located just across Berlin's border, which are exempted from the rent freeze regulation: as we show that the supply of rental units dropped significantly within Berlin upon enactment of the policy (see Figure 9), the excess demand from home-seekers needing to rent immediately might have leaked across the border, resulting in those owners taking advantage of the situation by asking for relatively higher rental prices.

5.2. Measuring Causal Price Effects

5.2.1. Price Effects Within Berlin. We formally test the causal relationship between changes in asking rents within and around Berlin. Therefore, we use the rent freeze announcement and enactment dates, together with flat characteristics, to distinguish covered and exempt units to causally identify corresponding supply-side reactions. Specifically, we select advertised rents for this purpose in order to avoid timing ambiguity because of the common lengthy time gap between first advertisement (and price setting) and the subsequent signing of rent contracts.

The rent freeze was first communicated on June 4, 2019, and became effective on February 23, 2020. These two dates delineate the three clearly distinguishable time periods as depicted in Figure 6.

Figure 5. Rent Price Indices for Selected German Cities
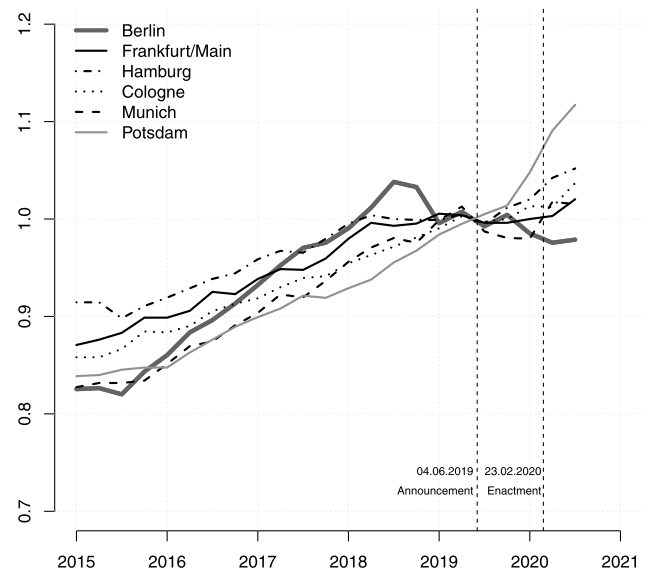
Source. Indices compiled by VALUE Marktdaten.

Notes. The figure shows quarterly asking rent price indices for existing property units ("Bestandswohnungen") for several large German cities (Cologne, Frankfurt/Main, Hamburg, and Munich) as well as for Berlin's satellite city Potsdam. Indices are normalized to the average index number in the second and third quarters 2019.

Table 3. Berlin vs. Other German Locations

	Aggregation level	Change since announcement: Δ		
		Q1:2020	Q2:2020	Q3:2020
Germany	Whole country	0.019	0.028	0.035
Berlin	Major city	−0.015	−0.024	−0.021
Hamburg	Major city	0.020	0.042	0.052
Cologne	Major city	0.013	0.012	0.037
Frankfurt/Main	Major city	0.000	0.003	0.020
Munich	Major city	−0.020	0.018	0.015
Potsdam	Satellite city	0.048	0.091	0.117
Barnim	Adjacent municipality	0.023	0.053	0.084
Dahme-Spreewald	Adjacent municipality	0.028	0.060	0.081
Havelland	Adjacent municipality	0.015	0.017	0.055
Märkisch-Oderland	Adjacent municipality	0.075	0.067	0.090
Oberhavel	Adjacent municipality	0.040	0.039	0.075
Oder-Spree	Adjacent municipality	0.027	0.011	0.034
Potsdam-Mittelmark	Adjacent municipality	0.012	0.005	0.013

Source. Author's calculations based on indices provided by VALUE Marktdaten.
Notes. The table reports changes in various hedonic rent price indices. Nearby municipalities are adjacent municipalities bordering Berlin. $I(Q_t)$, the change in index numbers between quarter Q_t and the reference period, is computed via $\Delta(Q_t) = I(Q_t)/Mean(I(Q2:2019), I(Q3:2019)) - 1$.

To put a clear focus on the immediate price effects, we here look at the shorter periods of 28 days before and after each event. The length of 28 days was chosen in an attempt to exclude potential confounding effects of the COVID-19 pandemic: on March 22, 2020, that is, 28 days after the enactment, the second regulation on pandemic containment measures became effective in Berlin.[31] The strict health measures aimed at combating the pandemic led to a (shortly lived) deep economic downturn.[32]

The resulting subperiods are (1) preannouncement, *Pre-A* between May 7, 2019, and June 3, 2019; (2) postannouncement, *Post-A* between June 4, 2019, and July 1, 2019, that is, after the announcement of the new law; (3) pre-enactment, *Pre-E* between January 26, 2020, and February 22, 2020; and (4) postenactment, *Post-E* between February 23, 2020, and March 21, 2020, that is, after the rent freeze became legally binding.

We estimate hedonic DiD models to identify the immediate price effects within Berlin upon announcement and enactment of the rent freeze. Therefore, we use in a first step dwellings explicitly excluded from the policy as control group. As a robustness check, Online Section B.2 relaxes the strict selection criteria to advertised units that are not for sure yet likely exempt.

The explicit control group comprises newly constructed buildings ready for occupancy for the first time starting from January 1, 2014, or, in particular cases, apartments that have been modernized to a substantial degree after a prolonged period of nonoccupancy (*Neubauaufwand*). To identify those objects, we use the label "first-time occupancy" collected by VALUE Marktdaten. This yields a subset comprising all apartments that were ready for first-time occupancy starting from January 1, 2014, covering both units in newly built properties and substantially refurbished ones. Moreover, apartments that were built after January 1, 2014, are generally included in the control group as they are explicitly exempt from the rent freeze.

To increase precision—similarly to Mense et al. (2023)—we exclude relatively old units (built before

Figure 6. (Color online) Timeline

Note. The timeline visualizes the sequence of relevant events as well as the definition of periods.

2013), which are offered under the label "first-time occupancy" even if they have not undergone any modernization.[33] Accordingly, we also exclude all units that were renovated before 2013.

One could be concerned that dwellings in the control and treatment groups may be different with regard to their features and, thus, may attract a distinct audience. We tackle this potential issue by including a rich set of hedonic control variables, including, most importantly, the exact location of a dwelling.

We rely on two complementary strategies to precisely measure sudden shifts in rent prices upon announcement and enactment. The scheme is depicted in Figure 6. Strategy A relies on a single model covering the entire time span (see Model (A)), whereas strategy B estimates separate models for the events "announcement" (Model (B.1)) and "enactment" (Model (B.2)):

$$
\begin{aligned}
\log(R_{it}) = {} & \beta_0 + \beta_1 C_i + \beta_3 \text{Transition}_t + \beta_4 \text{Post-E}_t \\
& + \beta_6 C_i \times \text{Transition}_t + \beta_7 C_i \times \text{Post-E}_t \\
& + \xi' X_i + \varepsilon_{it},
\end{aligned} \tag{A}
$$

$$
\begin{aligned}
\log(R_{it}) = {} & \beta_0 + \beta_1 C_i + \beta_2 \text{Post-A}_t + \beta_5 C_i \times \text{Post-A}_t \\
& + \xi' X_i + \varepsilon_{it},
\end{aligned} \tag{B.1}
$$

$$
\begin{aligned}
\log(R_{it}) = {} & \beta_0 + \beta_1 C_i + \beta_4 \text{Post-E}_t + \beta_7 C_i \\
& \times \text{Post-E}_t + \xi' X_i + \varepsilon_{it},
\end{aligned} \tag{B.2}
$$

where R_{it} denotes the monthly rent of unit i as advertised at time t. The dummy variable C_i equals one if the apartment is covered by the rent freeze and zero otherwise. The dummies Transition_t, Post-A_t, and Post-E_t indicate in which time period the advert was observed. Further, β_j for $j \in \{0, \ldots, 7\}$ are parameters associated with the steering variables, and X denotes a matrix containing various time-invariant hedonic characteristics, including a smooth locational spline with associated parameter vector ξ. Finally, ε_{it} is a normally and independently distributed error term. As the set of covariates includes smooth regressors, the models are consequently estimated via penalized least squares.

Before estimating these models precisely focusing on immediate effects, we test the common trend assumption crucial when employing a DiD estimator. We do so in three ways: First, we visually inspect pretrends in Figure 7 for both the treatment and control groups by comparing identically compiled hedonic indices but separately evaluated for dwellings in the control and treatment groups. This means a quality-adjusted test. Indeed, both types of apartments appear to follow a common trend up until the announcement of the new law. Thereafter, trends diverge. We next formally test this relationship prior to the announcement date by comparing monthly and weekly growth rates computed

Figure 7. Price Trends in the Treatment and Control Groups
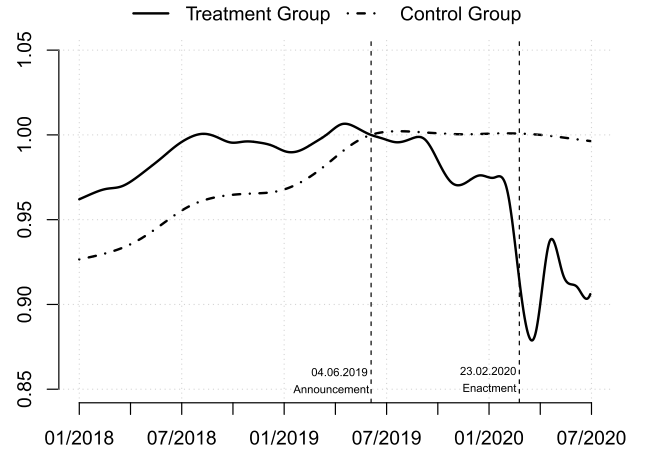
Source. Author's visualization based on data provided by VALUE Marktdaten.
Notes. The indices show the general trend of prices in the treatment versus control group between 2018 and the end of the second quarter 2020. Indices control for observable hedonic features and are normalized to the announcement date (June 4, 2019). The time-continuous indices follow the methodology developed in Waltl (2016) based on adaptive smoothing techniques.

from these indices:

$$
g_t^T = 100 \cdot \left(\frac{I_t^T}{I_{t-1}^T} - 1 \right) \quad \text{and} \quad g_t^C = 100 \cdot \left(\frac{I_t^C}{I_{t-1}^C} - 1 \right). \tag{4}
$$

We then calculate the paired Pearson correlation coefficients $\rho_{g_t^T, g_t^C}$ between the growth rates related to rent price indices for the treatment T and control C groups, subsequently testing whether the resulting paired correlations are statistically significantly different from zero using a Pearson product-moment correlation test. As we used a paired test, growth rates from the same time periods are matched, thus truly testing comovement over time and, as we use quality-adjusted indices for our test, differences due to a change in the mix of dwellings on the market is controlled for. We find statistically highly significant correlations as reported in Table 4.

Third, we estimate a hedonic model, including monthly time dummies (similar to the model used for constructing the indices) and interact them with a treatment group identifier to test directly within this model for significant differences in price trends. Again, we add hedonic controls. Detailed results are reported in Table 5.

Until shortly before June 2019, interaction terms were largely insignificant, thus suggesting no systematic deviation of the time trends for covered and exempt advertisements. From announcement onward, interaction terms separating the two groups gradually turn statistically significant. Assessing effect sizes, Table 5 reveals that monthly treatment effects gradually increased in size

Table 4. Correlation of Rent Index Growth Rates

				$H_1: \rho_{g_t^T, g_t^C} \neq 0$		$H_1: \rho_{g_t^T, g_t^C} > 0$	
Frequency (period length)		Number of periods	$\rho_{g_t^T, g_t^C}$	p-value	Significance	p-value	Significance
Months	(30 days)	17	0.730	0.001	**	0.001	***
Weeks	(7 days)	75	0.641	0.000	***	0.000	***

Notes. The table reports Pearson correlation coefficients $\rho_{g_t^T, g_t^C}$ between monthly and weekly growth rates of rent indices separately compiled for the control C and treatment T groups between 2018 and the announcement date (June 4, 2019) to check the common trend assumption. Results refer to the hypothesis $H_0: \rho_{g_t^T, g_t^C} = 0$ tested against two alternative hypotheses H_1.

Significance is indicated using standard notation: $\cdot p$-value < 0.1, *p-value < 0.05, **p-value < 0.01, ***p-value < 0.001.

following the announcement. A large jump occurred between February and March 2020, coinciding with the rent freeze's enactment on February 23, 2020.

Following these pretests, Table 6 reports key estimation results for Models (A), (B.1), and (B.2), focusing precisely on the 28-day periods before and after the rent freeze's announcement and its legally binding enactment as well as the in-between transition period as shown in Figure 6. Online Table C8 reports the full set of results.

As already suggested by findings in Table 5, the mere announcement has only a weak immediate impact on

Table 5. Monthly Treatment Effects

		Response: *Monthly rent (log)*			
Intercept	5.551***	(0.010)	Treatment (covered units)	−0.040***	(0.011)
$T_{2018-02}$	0.011	(0.013)	Treatment $\times T_{2018-02}$	0.003	(0.015)
$T_{2018-03}$	−0.005	(0.013)	Treatment $\times T_{2018-03}$	0.002	(0.014)
$T_{2018-04}$	−0.004	(0.013)	Treatment $\times T_{2018-04}$	0.018	(0.015)
$T_{2018-05}$	0.004	(0.013)	Treatment $\times T_{2018-05}$	0.011	(0.015)
$T_{2018-06}$	0.008	(0.014)	Treatment $\times T_{2018-06}$	0.025	(0.015)
$T_{2018-07}$	0.044**	(0.014)	Treatment $\times T_{2018-07}$	−0.009	(0.015)
$T_{2018-08}$	0.036*	(0.014)	Treatment $\times T_{2018-08}$	0.004	(0.015)
$T_{2018-09}$	0.023.	(0.013)	Treatment $\times T_{2018-09}$	0.008	(0.015)
$T_{2018-10}$	0.030*	(0.013)	Treatment $\times T_{2018-10}$	0.001	(0.014)
$T_{2018-11}$	0.047***	(0.012)	Treatment $\times T_{2018-11}$	−0.015	(0.014)
$T_{2018-12}$	−0.003	(0.013)	Treatment $\times T_{2018-12}$	0.040**	(0.015)
$T_{2019-01}$	0.019	(0.013)	Treatment $\times T_{2019-01}$	0.002	(0.014)
$T_{2019-02}$	0.047***	(0.013)	Treatment $\times T_{2019-02}$	−0.015	(0.014)
$T_{2019-03}$	0.036**	(0.013)	Treatment $\times T_{2019-03}$	0.001	(0.014)
$T_{2019-04}$	0.070***	(0.013)	Treatment $\times T_{2019-04}$	−0.024.	(0.014)
$T_{2019-05}$	0.077***	(0.013)	Treatment $\times T_{2019-05}$	−0.040**	(0.014)
$T_{2019-06}$	0.079***	(0.013)	Treatment $\times T_{2019-06}$	−0.040**	(0.014)
$T_{2019-07}$	0.072***	(0.013)	Treatment $\times T_{2019-07}$	−0.042**	(0.014)
$T_{2019-08}$	0.078***	(0.013)	Treatment $\times T_{2019-08}$	−0.041**	(0.015)
$T_{2019-09}$	0.068***	(0.014)	Treatment $\times T_{2019-09}$	−0.037*	(0.015)
$T_{2019-10}$	0.086***	(0.014)	Treatment $\times T_{2019-10}$	−0.073***	(0.015)
$T_{2019-11}$	0.064***	(0.014)	Treatment $\times T_{2019-11}$	−0.056***	(0.015)
$T_{2019-12}$	0.071***	(0.015)	Treatment $\times T_{2019-12}$	−0.057***	(0.016)
$T_{2020-01}$	0.086***	(0.014)	Treatment $\times T_{2020-01}$	−0.076***	(0.015)
$T_{2020-02}$	0.071***	(0.014)	Treatment $\times T_{2020-02}$	−0.081***	(0.016)
$T_{2020-03}$	0.074***	(0.016)	Treatment $\times T_{2020-03}$	−0.170***	(0.018)
$T_{2020-04}$	0.105***	(0.017)	Treatment $\times T_{2020-04}$	−0.144***	(0.019)
$T_{2020-05}$	0.045*	(0.018)	Treatment $\times T_{2020-05}$	−0.090***	(0.019)
$T_{2020-06}$	0.069***	(0.016)	Treatment $\times T_{2020-06}$	−0.131***	(0.018)
Housing characteristics					✓
Adjusted R^2					0.825
GCV					0.045
Number of observations					74,657

Source. Author's calculations based on data provided by VALUE Marktdaten.
Notes. The model regresses the logged monthly rent on monthly time dummies interacted with treatment group identifiers. The entire time span assessed in this article is considered (January 2018–June 2020). GCV stands for the Generalized Cross Validation criterion. Standard errors shown in parentheses.

Significance is indicated using standard notation: $\cdot p$-value < 0.1, *p-value < 0.05, **p-value < 0.01, ***p-value < 0.001.

Table 6. Main Results

	Response: *Monthly rent (log)*		
	Strategy A	Strategy B	
Treatment	−0.063***	−0.075***	−0.073**
(units covered by rent freeze)	(0.012)	(0.017)	(0.024)
Post-A		−0.009	
		(0.013)	
Transition	−0.007		
	(0.010)		
Post-E	−0.018		−0.018
	(0.017)		(0.020)
Treatment × Post-A		0.007	
		(0.014)	
Treatment × Transition	−0.008		
	(0.011)		
Treatment × Post-E	−0.113***		−0.075***
	(0.019)		(0.022)
Housing characteristics	✓	✓	✓
Number of observations	26,842	5,311	3,314
Adjusted R^2	0.832	0.843	0.830

Notes. Standard errors shown in parentheses. The Generalized Additive Models (GAMs) estimated include the variables *location* (smooth term), *age* (smooth term), *first time occupation, living area, number of rooms, garden, balcony/terrace, fitted kitchen, parking, elevator, separate toilet.* The full set of results are reported in Online Table C8.

Significance is indicated using standard notation: ·*p*-value < 0.1, **p*-value < 0.05, **p-value < 0.01, ***p-value < 0.001.

advertised rent prices when contrasting the regulated sector against the unregulated one. Apparently, owners did not immediately and consistently follow the call by the landlords' and homeowners' association *Haus und Grund* to raise rents before the rent freeze would come into force.[34] In contrast, the legal enactment of the rent freeze led to a sharp and statistically highly significant decrease in asking rents within the treatment group (−0.075***) as compared with the nonregulated control group.

We find that units covered by the rent freeze (treatment group) are generally less expensive than those exempt (control group). Given the fact that the control group comprises new and renovated property units, this result is in line with what we expect. Reassuringly, the size of the estimated coefficient associated with the classification into covered and exempt units remains practically identical for all specifications as reported in the first line in Table 6. Additionally, a placebo test performed on the control group in Online Table B6 suggests that declines in asking rents drive results rather than increases in the unregulated sector.

5.2.2. The Role of Administrative Borders
As shown in Figure 8, Berlin is entirely enclosed in the federal state of Brandenburg. Berlin and the adjacent part of Brandenburg form the German capital region and can be considered as a single metropolitan agglomerate. Being part of a common urban zone is particularly

true for the area directly neighboring the border between the two federal states. The rent freeze, however, is exclusively applicable to the administrative territory of Berlin. Units just across the border are not covered by the regulations.

We supplement the descriptive evidence on effects on rent prices in municipalities in Berlin's neighborhood provided in Table 3 by a strictly causal assessment here. Hence, we compare how advertised prices changed between the pre-enactment (*Pre-E*) and postenactment (*Post-E*) periods for apartments that, given their characteristics, would have been covered by the rent freeze if they were located within Berlin's administrative boundaries. Yet apartments with such features located just across the border are not covered. We exploit this spatial discontinuity to assess how advertised rents are affected by the rent freeze on both sides of the city border.

Therefore, we estimate suitably designed hedonic-style spatial difference-in-discontinuity models following Grembi et al. (2016). Similar to the hedonic DiDs adopted

Figure 8. (Color online) Apartments Close to the Border with Brandenburg
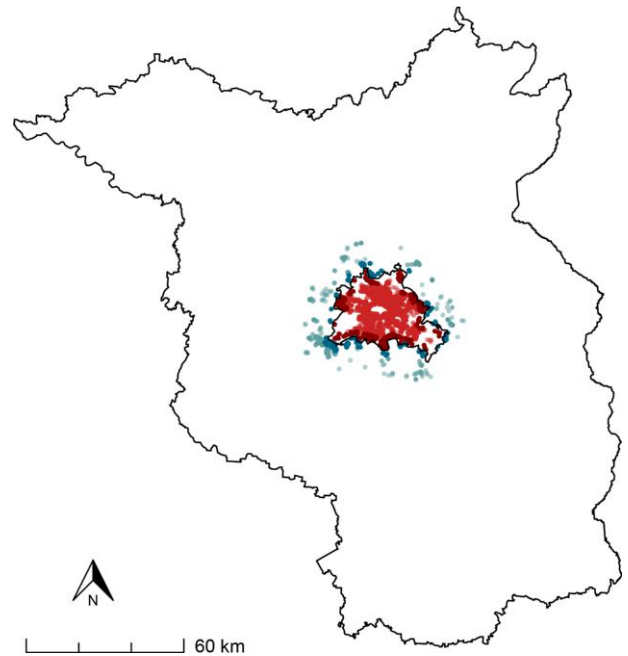
Source. Author's visualization based on data provided by VALUE Marktdaten.
Notes. The figure shows the shapes of Berlin (small shape) and Brandenburg (large shape). Each dot represents the location of a property unit advertised in Berlin or Brandenburg. We use observations assigned to the treatment group within Berlin and observations in Brandenburg, that, given their characteristics, would have been covered by the rent freeze if they were located in Berlin. The dots in darker shades lie within 2 km of the border, whereas those in lighter shades are located within a 10-km band. Advertised units are scattered around the entire city except local recreation areas (e.g., in the south/southeast the "Müggelspree-Löcknitzer Wald-und Seengebiet," see also Table 7).

earlier, we regress the logged monthly advertised rent on a set of steering variables and hedonic controls. These steering variables are a temporal dummy (*Post-E*) distinguishing advertisements posted before or after enactment, a locational dummy (*Brandenburg*) indicating on which side of the border the property unit is located, and a continuous variable d measuring the distance to the border between the two federal states.[35]

As we explicitly measure a spatial discontinuity, we substitute the locational spline used previously by a dummy indicating whether the unit is located within or outside the administrative border of Berlin. By doing so, we project two-dimensional coordinates into a one-dimensional unit and miss the modeling of price variations across different border areas. Thus, we reintroduce a second dimension via an additional categorical variable *CD*, which indicates compass directions. We distinguish eight directions: north (N), northeast (NE), east (E), southeast (SE), south (S), southwest (SW), west (W), and northwest (NW). We allocate each apartment to its closest compass direction, which we compute using the exact geolocation of the property unit.

Three geographic elements together uniquely approximate the exact geolocation of advertised units, just as longitudes and latitudes, but now in the spirit of a polar coordinate system rather than the Cartesian system we used before: compass directions *CD*, the distance to the city-border polygon d (topologically equivalent to a circle), and the indicator discriminating between locations inside and outside of the city-border polygon.

We define a negative distance $d_i < 0$ for units within Berlin and a positive one $d_i > 0$ for observations in Brandenburg. The special case $d = 0$ means an apartment located within 100 m from the border. We estimate the distance again as either a continuous linear variable or a smoothly estimated function relying on polynomials of order up to four but finally identify $f(d_i) = \delta d_i$ as the best approximation.

An additional dummy distinguishes apartments located in Berlin (*Brandenburg* = 0) and those in Brandenburg (*Brandenburg* = 1).

This yields the model equation

$$\log(R_{it}) = \beta_0 + f(d_i) + \beta_1 \text{Post-E}_t + \beta_2 \text{Brandenburg}_i$$
$$+ \beta_3 \text{Post-E}_t \times \text{Brandenburg}_i$$
$$+ \beta_4' CD_i + \xi' X_i + \varepsilon_{it}. \quad (5)$$

Again, β_j for $j = 0, \ldots, k$ denote parameters associated with steering variables and X a matrix containing time-invariant hedonic controls (but this time excluding location) with associated parameters ξ. Again, ε_{it} is a normally and independently distributed error term.

Our main effect of interest is β_3 measuring the change in the spatial discontinuity after the introduction of the rent freeze.

Table 7. Apartments at the Border: Compass Directions

	N	NE	E	SE	S	SW	W	NW	Total
Brandenburg	24	27	35	27	13	128	58	38	350
Berlin	358	380	352	332	132	228	384	344	2,510
Total	382	407	387	359	145	356	442	382	2,860

Source. Author's calculations based on data provided by VALUE Marktdaten.
Notes. The table reports the number of advertised property units for rent in *Pre-E* and *Post-E* within 10 km of the administrative border between the federal states Berlin and Brandenburg by compass direction. Compass directions are abbreviated as follows: north (N), northeast (NE), east (E), southeast (SE), south (S), southwest (SW), west (W) and northwest (NW). We show apartments covered by the rent freeze within Berlin and apartments in Brandenburg that, given their characteristics, would have been covered by the rent freeze if they were located in Berlin.

We use a local linear modeling approach and set the bandwidths to 2, 5, and 10 kilometers, respectively.[36] A weighting scheme based on a triangular kernel ensures that observations close to the border ($d_i \approx 0$) receive more weight than those further away.

Table 8 reports our main estimation results. We find a stark spatial price effect along the border with Brandenburg after the enactment of the rent freeze as measured by the interaction between the *Brandenburg* and *Post-E* dummies. Comparable units located just across the border are advertised with significantly higher rents than their counterparts within Berlin. The total gap (of size 0.106) is driven by both an overall drop in advertised prices on the Berlin side of the border (−0.077 for the 2-km bandwidth) and an increase on the Brandenburg side (0.037 + 0.146 = 0.183 for the 2-km bandwidth) among units with rent freeze–relevant characteristics. The effect is most pronounced (and most precisely estimated) when assessing a very narrow bandwidth covering the direct border area only. The effects remain significant even when moving further away from the border.

Thus, the rent freeze has indeed significantly enlarged the gap between asking rents along this border between German federal states. Such a price gap is instead usually observed along national borders when substantially different housing markets and regulation regimes meet (see, for instance, Micheli et al. 2019).

Our findings are robust to varying the exact specifications: we tested including interaction terms between the *Brandenburg* dummy and distance d, between the time dummy *Post-E* and the distance d, and between those three variables. The findings are also robust toward technical details, such as the exclusion of kernel weights and extending the time period considered to four months prior to and after the enactment of the rent freeze. Regarding the latter, estimated coefficients increase in precision in all model specifications when considering a longer time span because of the larger number of observations. This yields more stable results

Table 8. Spatial Regression Results

Bandwidth	Response: *Monthly rent (log)*					
	2 km		5 km		10 km	
Brandenburg	0.037	−0.005	0.004	0.009	0.015	0.036
	(0.039)	(0.038)	(0.026)	(0.025)	(0.020)	(0.019)
Post-E	−0.077***	−0.072***	−0.076***	−0.076***	−0.083***	−0.084***
	(0.018)	(0.019)	(0.012)	(0.013)	(0.009)	(0.010)
Post-E × Brandenburg	0.146***	0.141***	0.093***	0.100***	0.104***	0.103***
	(0.038)	(0.039)	(0.026)	(0.027)	(0.023)	(0.023)
Distance *d*	−0.004*	−0.001	−0.001	−0.000	−0.001***	−0.001***
	(0.002)	(0.002)	(0.001)	(0.000)	(0.000)	(0.000)
Housing characteristics	✓	✓	✓	✓	✓	✓
Compass directions	✓	✗	✓	✗	✓	✗
Number of observations	546	546	1,386	1,386	2,860	2,860
Adjusted R^2	0.797	0.782	0.783	0.774	0.745	0.741

Source. Author's calculations based on data provided by VALUE Marktdaten.
Notes. The table reports estimation results of model 5. Models differ in the set of rent advertisements included (maximum distance of 2, 5, or 10 km away from the administrative city boundaries) and the inclusion of compass directions. Kernel weights (triangular) are respected. Physical housing characteristics comprise the same variables as in the DiD specification reported in Table 6. Full results are found in Online Tables C9–C11.

along the different model specifications. However, we do not include them as here we explicitly aim to cleanly exclude possible effects related to the COVID-19 pandemic.

6. Quantity Effects

In this section, we empirically evaluate the short-run effect of the rent freeze on the supplied quantity of property units in Berlin. We first test the predictions of our theoretical framework in terms of the number of advertised dwellings and then assess the impact of the double-pricing strategy.

6.1. The Fluctuating Number of Advertisements

Our theoretical framework predicts that both the announcement and enactment of the rent freeze adversely affect owners' participation in the rental market, meaning a decline in properties for rent in the market. This prediction is in line with empirical findings by Diamond et al. (2019) analyzing the impact of a rent control policy in San Francisco. They document that "[l]andlords treated by rent control reduce rental housing supplies by 15 percent by selling to owner-occupants and redeveloping buildings." From a macroeconomic perspective, Leamer (2007) even argues in favor of generally focusing on the housing volume cycle to detect and predict systematic fluctuations and adverse developments rather than assessing price movements only.

Thus, we empirically evaluate predicted quantity effects on the supply side in three dimensions: changes in the use of dwellings, changes in new construction activities, and changes in the number of posted advertisements (volume).

Regarding, first, the use of dwellings, it is insightful assessing property units in Berlin that were converted from rental to owner-occupied apartments. Their number

sharply increased from 12,700 in 2019 to 19,200 in 2020 (+51%) indicating that a substantial number of dwellings seems to be permanently lost for renters. The 2020 number is the highest ever measured since the beginning of this time series in 2005.[37]

Second, the number of completed new dwellings can be used to predict the medium- to long-term effects of the rent freeze. According to our theoretical framework, the rental market is expected to be affected in the longer term even though the policy was shortly lived: a lower expected stream of income from renting out a flat is expected to reduce the number of participating actors in this market. Indeed, in 2020, this number had declined sharply by 14% as compared with 2019, whereas in Germany as a whole this indicator increased by almost 5% (see Destatis 2021).

Finally, we assess the immediate effects on units directly targeted by the rent freeze. Therefore, we measure the number of newly posted advertisements per week. Table 9 and Figures 9 and 11 report the results: whereas there are hardly any noticeable differences between the preannouncement (*Pre-A*) and transition (*Transition*) periods, the ultimate enactment of the policy led to remarkable disruptions. The number of weekly advertised units to rent halved from more than 600 per week before the announcement to less than 300 following implementation of the policy.[38] The changes are statistically significant as indicated by the nonoverlapping whiskers (95% coverage intervals) in Figure 9 and as formally confirmed by Tukey post hoc tests (Tukey 1949). Full results are reported in Online Table C12.

We also repeat the same exercise for sales advertisements and observe a small drop in the number of advertisements in the *Post-E* period even though changes are less pronounced and not significant.

Table 9. Volume Effects

		Number of new advertisements per week		
		Mean	Median	Interquartile range
Rent advertisements				
Pre	January 1, 2018–June 3, 2019	628.55	639.50	152.75
	...thereof in treatment group	510.04	517.00	98.25
	...thereof in control group	118.51	115.50	29.50
Trans	June 4, 2019–February 22, 2020	614.97	619.00	105.00
	...thereof in treatment group	509.14	522.00	79.00
	...thereof in control group	105.84	106.00	36.00
Post	February 23, 2020–June 30, 2020	276.28	262.50	79.75
	...thereof in treatment group	224.00	223.50	50.50
	...thereof in control group	52.28	54.00	19.50
2018-Post	February 23, 2018–June 30, 2018	598.18	587.00	89.00
2019-Post	February 23, 2019–June 30, 2019	651.41	644.00	92.00
Sales advertisements				
Pre	January 1, 2018–June 3, 2019	143.85	140.50	41.00
Trans	June 4, 2019–February 22, 2020	126.41	126.00	31.00
Post	February 23, 2020–June 30, 2020	105.83	107.50	19.25
2018-Post	February 23, 2018–June 30, 2018	144.53	141.00	23.00
2019-Post	February 23, 2019–June 30, 2019	140.24	133.00	19.00

Source. Author's calculations based on data provided by VALUE Marktdaten.
Notes. The number of new advertisements for apartments to rent or sale per week measures the volume changes on the supply side. Numbers are reported for all types of property units as well as for rental advertisements separately for those falling into the treatment and control groups, respectively. For comparability across seasons, the trade volume for usual activity between February 23 and June 30 are included for the two preceding years 2018 and 2019. For the calculations, all advertisements fulfilling our selection procedure described in Section 4 are considered. The data are visualized in Figure 9.

The number of advertised properties for rent usually also varies across seasons. To rule out that the measured effects are seasonal, we additionally compare the volume in the 2020 posttreatment period to the exact same time span in 2018 and 2019. The observed volumes in the 2018-post and 2019-post are indistinguishable from that in the 2020-pre and 2020-trans periods.

For measuring quantity effects—unlike price effects—we look at a period overlapping the first COVID restrictions in

Figure 9. Number of Newly Posted Rent and Sales Advertisements per Week
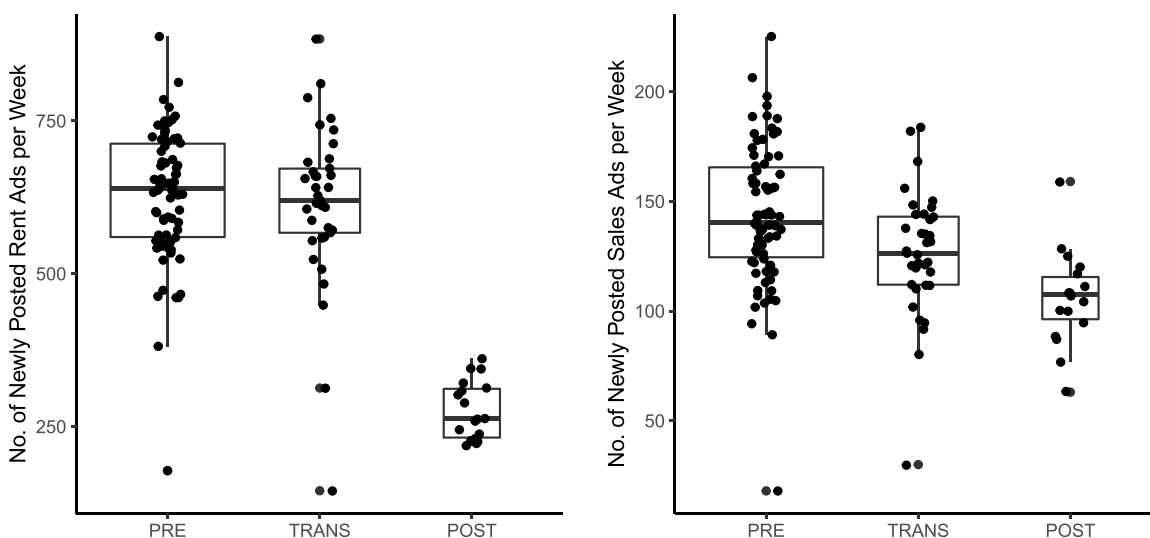
Source. Author's visualization based on data provided by VALUE Marktdaten.
Notes. The boxplots depict the number of newly posted rent (left) and sales (right) advertisements per week separately for the pre, trans, and post periods as defined in Table 9. Each dot represents a unique full week. Corresponding numeric results are presented in Table 9.

Figure 10. Asking vs. Allowed Rents
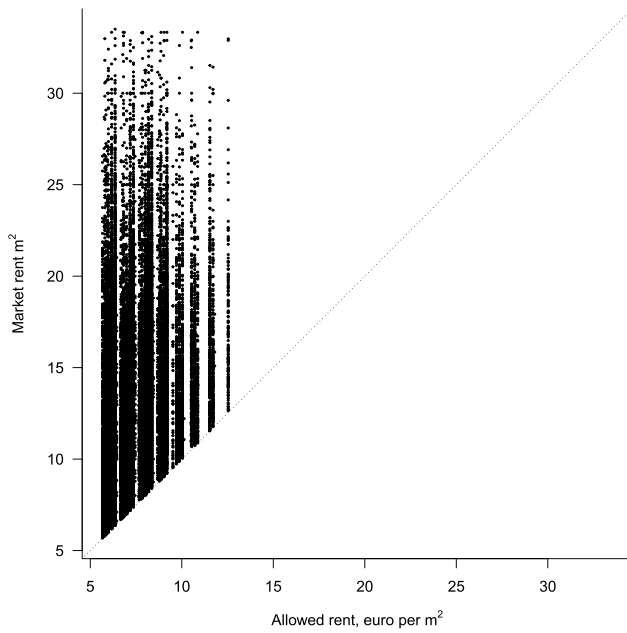
Figure 11. (Color online) Number of Advertisements with and Without Double Rents

Berlin. This seems valid as supply has not decreased in other parts of Germany.[39]

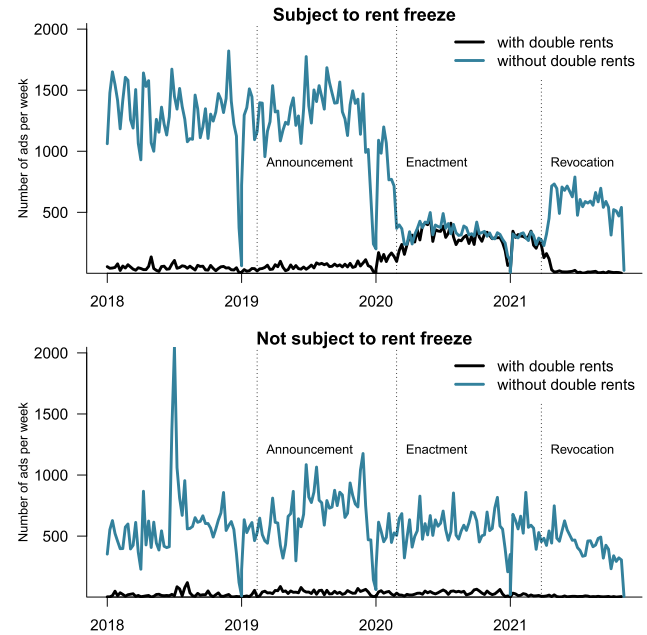Hence, the volume effect we document here is a feature unambiguously linked to the rent freeze.

However, which types of flat drive this drop in advertisements? In fact, the volume of advertised property units in both the treatment and control groups falls by roughly the same rate (see splitups in Table 9): those being directly captured by the rent freeze as well as those exempt. This finding hints—together with those for new construction and transformation of tenure status—at a substantial and likely lasting sharp decline in available rental units of all kinds in Berlin. In other words, the tightness of the Berlin rental market will likely continue.

6.2. A Double-Pricing Strategy

After enactment, there was a high degree of uncertainty about the constitutionality of the rent freeze law. In response, a double-pricing strategy evolved, allowing landlords to hedge the risk of renting out a vacant unit at the lower price following the rules of the rent freeze that could not easily be adjusted upward in case the law was overturned. This would have meant substantial losses in expected future revenues over an extended period of time as laid out in Section 3.

Landlords could hedge this risk by advertising two rent prices clarifying the rent due in different scenarios:

the market price and the capped rent following the rent freeze rules. Typically, the authors of advertisements would primarily state the lower rent freeze price and mention in the section "miscellaneous" that, in case the law was overturned, tenants would have to pay the market price from then onward and additionally refund the foregone difference accumulated during the validity of the rent freeze law. This strategy is free of charge for landlords but potentially provides them with extra future income. Thus, rationality and profit-maximization arguments mean expecting all newly established contracts to include such a clause. Usually, advertisements already mentioned it.

Figure 10 compares the two rent prices: stated alternative rents in advertisements are considerably higher than rents following the rent freeze regulations. This confirms landlords' large possible economic benefit from hedging. The alternative current market rent landlords expect to achieve is higher than the stated allowed one. This is indicated by all alternative rents exceeding the allowed one. In fact, the median difference between the two types of prices amounts to 51%. This difference is even higher for dwellings in good locations (63%), but even for dwellings in simple and average locations, the difference amounts to about 47%–48%.

As consequence of the free-of-charge economic benefit, Lemma 3.3 in our theoretical framework predicts that all landlords should adopt this strategy.

To empirically investigate this issue, we use the Immobilienscout24 data set. Using a different primary data source was necessary as the alternative rents were stated in the unsystematic free-text field, which is not collected by VALUE Marktdaten.[40]

Among the Immobilienscout24 advertisements, 51% were subject to the rent freeze following the same classification procedure as for the VALUE Markdaten advertisements. Among covered advertisements, the share mentioning the rent freeze law varies across time as shown in Figure 11, displaying weekly dynamics of the number of advertisements with and without double rents. The upper (lower) panel depicts the dynamics for dwellings (not) subject to the rent freeze.

The share of advertisements with double rents started off at 11% in February 2020,[41] increased to 34% in March 2020, and further stabilized at 45%–50% through March 2021. After the law was overturned, the share of such advertisements plummeted. Before enactment, as well as after revocation, the phenomenon of advertisements with double rents is very rare.

Whereas the majority of posted advertisements subject to the rent freeze did indeed apply the hedge, a substantial portion did not.[42] As the rent freeze was heavily debated in Berlin (see Section 2), it appears unlikely that this observation is due to a lack of information.[43] Even if such terms are not stated in advertisements, landlord and renter may still agree upon such terms when signing the contract. As contracts do not need to be officially recorded in Germany, we cannot rule out that many more of them, in fact, were subject to such terms. Thus, what we measure here is the lower bound of the prevalence of this phenomenon. Together with landlords potentially having different beliefs about whether and when the rent freeze would be abolished, these reasons could explain a more heterogeneous response in the adoption of the hedging strategy already upon posting an advert.

7. Robustness Analyses

We perform a variety of robustness checks and sensitivity analyses to increase confidence in our empirical findings. We sketch out results here; details are presented in Online Appendix B. Our main results thoroughly survive all these tests.

We first perform four placebo tests in Online Appendix B.1, all ruling out statistical artifacts. First, we use sales instead of rent advertisements and test shifts in prices upon announcement and enactment of the rent freeze. Therefore, we allocate sale advertisements to the treatment and control groups given their characteristics as if they were rent advertisements. As reported

in Online Table B1, we do not find an effect, thus ruling out that what we interpret as rent freeze effects may, in fact, be a general housing market feature.

In the same spirit, we estimate regressions on rent data but for previous years. The announcement and enactment dates did not trigger any changes in 2018/2019 as reported in Online Table B2. Regarding spatial effects, it turns out that—though there has always been a certain "border effect"—the rent freeze appears to have strongly widened the gap in rent levels along the administrative border between Berlin and Brandenburg. Results for previous years do not indicate a systematic change along the border as reported in Online Tables B3 and B4.

Second, another potential concern may be that the effect sizes we estimate could depend on—possibly adverse—changes in asking rents for unregulated units. This may be particularly worrisome as we document such effects for adjacent municipalities in Table 3. Reassuringly, though, the quality-adjusted trends shown in Figure 7 do not indicate such a reciprocal effect: rents in the unregulated sector remained stable ever since the announcement of the rent freeze. Additionally, a placebo test for the control group reported in Online Table B5 finds only vaguely significant or even insignificant effects when estimating a spatial regression model on exempt advertisements, that is, the control group. Thus, our models predominantly estimate the size of rent drop among regulated units and not a composite effect of rent drop in the treatment group and a rent increase in the control group.

Third, we relax the strict selection criteria to delineate treated and nontreated rent advertisements by relying on a single indicator available in the data set. These changes led to some reclassifications with more advertisements allocated to the control group than when relying on the detailed classification procedure making use of dwelling characteristics. Nonetheless, as reported in Online Table B6, estimation results are very similar to the main regressions, and in terms of interpretation, no changes emerge.

Following the placebo tests, Online Appendix B.3 analyzes whether the types of property units offered vary over the three constructed periods under consideration by assessing the characteristics of advertised flats. If so, our regression results may be confounded. However, Online Tables B7–B9 suggest no such systematic differences.

In this spirit, Online Table B10 directly tests the importance of including hedonic controls in our main regressions. Additional controls should increase precision, yet general trends are expected to still be visible without them. It turns out that keeping hedonic controls indeed strongly improves the models' goodness of fit, yet main conclusions are also visible in models without these controls.

Finally, we test whether technical choices affect regression results. The data by VALUE Marktdaten contain

geolocations for advertisements reporting an exact address, that is, street name and number, and the main results presented in the article exclusively rely on these exact geocodes. Nevertheless, we reestimate results on the full data also including advertisements that contain truncated address information. These pieces of information result in quasi locations, which indicate an approximate position of the property unit. In addition, we use confidence circles constructed by VALUE Marktdaten, which reports the level of insecurity depending on the amount of spatial information provided. For instance, if an advertisement only contains information on the street name but no number, it implies that the unit could be located anywhere along that street: the longer the street, the greater the insecurity about the exact location. The radius of the insecurity circle is then larger for longer streets. Thus, we use quasi-locations and the radius of the insecurity circle to elicit as much additional information as possible. Again, Online Table B11 reports that main results do not change when including advertisements with quasi-locations.

8. Summary and Conclusions

This article discusses the immediate effects of the rent freeze, a rigorous rent control policy implemented in 2020 in Berlin, which was subsequently declared unconstitutional by the German constitutional court. The policy was designed in a quite outdated fashion, making use of stringent elements of—for good reasons—rarely used first-generation rent control policies. The rent freeze itself was short-lived, yet the resulting shrinking supply of units for rent is likely to have long-lasting adverse consequences for renters in Berlin.

To rigorously analyze its effects, we develop a theoretical framework of the rental market suitable for modeling first-generation rent control regimes and add features specific to the Berlin rent freeze. We make use of observable rent advertisements to estimate the magnitudes of price and quantity effects upon the policy's announcement and enactment. As the rent freeze covered only property units within the administrative border of the city of Berlin but not the surrounding federal state of Brandenburg, we additionally assess the border region that is still part of the same metropolitan agglomeration. Along this border, two rental markets meet, which suddenly had quite different underlying regulations.

The supply side reacted to the rent freeze upon enactment as intended by policy makers: within Berlin, advertised rent prices indeed dropped substantially. These decreases are highly significant—in both economic and statistical terms—and causally linkable to the rent freeze policy. At the same time, asking rents surged in Berlin's satellite city Potsdam and further smaller municipalities close to Berlin, indicating a substitution effect. We detect a general stark border effect: although advertised rents

for property units just across the administrative border had been higher before the rent freeze, upon enactment of the rent freeze, this gap increased significantly, thus creating an artificial price gap within a unique metropolitan area.

Next to price effects, we identify a considerable decline in the number of advertised rental units. This sizable—yet unintended—side effect hampers renters' flexibility and adaptability. In particular, newcomers, (young) first-time renters, and households moving within Berlin face hurdles in finding a suitable place to live. The drop in supply may be transitory, yet we report first evidence supporting a permanent decline because a positive fraction of rental properties appears to have left the market in line with predictions of our theoretical framework. Thus, this decrease is likely a prelude to even harsher search conditions for housing in Berlin.

The overall conclusions are rather pessimistic. In general, rigorous price restrictions limiting owners' potential return appear to be shortsighted if they come without supplemental strategies to increase the supply of rental units. The rent freeze did not incentivize the creation of further housing units available to renters, but rather has contributed to shrinking their number. Consequently, an overall welfare-increasing effect is very doubtful in line with findings by Borck and Gohl (2021).

The rent freeze was short-lived but did still create substantial turbulence in Berlin's rental market. This "experiment" provides a glimpse of the likely side effects of introducing overly strict but not well thought through and legally shaky policies in a rental market facing distress.

and the Department of Land Economy at the University of Cambridge. First results were published as DIW Weekly Report (Hahn et al. 2021). This article uses data kindly provided by VALUE Marktdaten (formerly Empirica Systeme) and Immobilienscout24. The results presented do not necessarily reflect the opinion of the data providers. All remaining errors are the authors' own.

Endnotes

[1] See Kholodilin (2022) for a thorough review of published empirical studies on all possible effects of rent control.

[2] See Statistisches Bundesamt "Eigentümerquote nach Bundesländern im Zeitvergleich," https://www.destatis.de/DE/Themen/Gesellschaft-Umwelt/Wohnen/Tabellen/eigentuemerquote-nach-bundeslaender.html, last accessed in May 2022.

[3] The data are taken from the database GENIOS, including about 2,200 high-quality German-speaking media with the total number of documents exceeding 500 million. See https://www.genios.de, last accessed in January 2022.

[4] "If Berlin can freeze rents for five years, there's no reason London shouldn't be able to freeze rents for two years in these extraordinary times." See https://www.london.gov.uk/press-releases/mayoral/mayor-calls-for-two-year-rent-freeze, last accessed in December 2020.

[5] The title of the original law is "Gesetz zur Dämpfung des Mietanstiegs auf angespannten Wohnungsmärkten und zur Stärkung des Bestellerprinzips bei der Wohnungsvermittlung (MietNovG)" as of April 21, 2015. See the "Bundesgesetzblatt Jahrgang 2015 Teil I Nr. 16, ausgegeben zu Bonn am 27. April 2015" for juridical details.

[6] To identify a tight market, at least one of the following four conditions must be met: (1) local rents grow faster than at the national level; (2) the local average rent-to-income ratio is significantly higher than the national average; (3) population grows, whereas new housing construction does not create enough dwellings; or (4) the vacancy rate is low, whereas demand is high. Nonetheless, even in areas witnessing tight housing market conditions, not all dwellings are subject to the rent brake. There are two explicit exceptions: (1) dwellings used and let for the first time since October 1, 2014, or (2) dwellings let for the first time after an extensive modernization.

[7] These states are Baden-Württemberg, Bavaria, Berlin, Brandenburg, Bremen, Hamburg, Hesse, Mecklenburg-West Pomerania, Lower Saxony, North Rhine-Westphalia, Rhineland-Palatinate, Saxony, and Thuringia. Note that each of the 16 German federal states is empowered to determine which areas are tight housing markets.

[8] "Gesetz zur Verlängerung und Verbesserung der Regelungen über die zulässige Miethöhe bei Mietbeginn" as of March 19, 2020.

[9] "Information der Koalition zu einem Berliner Mietengesetz Eckpunkte für ein Berliner Mietengesetz," https://haus-und-grund-berlin.de/wp-content/uploads/2019/06/Eckpunkte_Berliner_Mietengesetz.pdf, last accessed in December 2020.

[10] "Erhöhen Sie bis zum 17. Juni 2019 die Miete!" See https://haus-und-grund-berlin.de/wichtig-erhoehen-sie-vor-dem-18-juni-2019-die-miete/, last accessed in December 2020.

[11] The title of the original law is "Gesetz zur Mietenbegrenzung im Wohnungswesen in Berlin (MietenWoG Bln)" as of February 11, 2020. The law was enacted on the February 23, 2020, and abolished on March 25, 2021.

[12] Further premises excluded from the regulation's scope are units fulfilling at least one of the following criteria: (1) housing units built under state support schemes, (2) residential premises modernized and refurbished using public aid and which are already subject to rent price restrictions, and (3) dormitories and similar accommodation facilities.

[13] Modern equipment (*moderne Ausstattung*) means that at least three of the following features are available: an elevator (accessible without steps), fitted kitchen, high-value sanitary equipment, high-value flooring in most rooms, or energy consumption below 120 kWh/m^2.

[14] See, therefore, the official classification of locations in Berlin (*Mietspiegel*) described in Section 4.3.

[15] The Berlin city council offered support to tenants to check their rental agreement and give a complaint in case of landlords refusing to provide sufficient information needed to compute the valid rent.

[16] Therefore, we subtract €0.09 to obtain rents following the rent freeze rules. The *Mietspiegel* is an official repository of local market-based rent prices serving as the basis for setting initial rents according to the rent brake. The *Mietspiegel* further differentiates between rents in East and West Berlin as well as by completion years (between 1973 and 1990). As the rent freeze does not include such distinctions, we computed a simple average of East and West Berlin's rental prices. In addition, the rent freeze does not distinguish between different dwelling sizes.

[17] See Online Table C13 for related demographic statistics per district.

[18] For example, in 2015, annual federal support for the social housing construction budget was increased from €518.2 to €1,018.2 million. ("Asylverfahrensbeschleunigungsgesetz" as of October 20, 2015, BGBl. I S. 1722, enacted on October 24, 2015).

[19] The 2021–2025 Coalition agreement between the Sozialdemokratische Partei Deutschlands (SPD), BÜNDNIS 90/DIE GRÜNEN, and the Freien Demokraten (FDP) is "Mehr Fortschritt wagen: Bündnis für Freiheit, Gerechtigkeit und Nachhaltigkeit."

[20] "Gesetz zur Stärkung des Wohngeldes" as of November 30, 2019, BGBl. I S. 1877 (Nr. 44), enacted on January 1, 2020.

[21] We abstract from the role of owners as sellers in the housing market. The decision of owners to sell their property could nevertheless fall under the category of using a property for personal use—in this case, selling it—the features of which are explained as follows.

[22] As our model is designed to capture the effect of a price cap on the short-run supply of rental units, this assumption is innocuous. Indeed, this is not the case in a model of the rental property market aiming to characterize how the rent (or its distribution) is determined in equilibrium, absent policy interventions. As such, our approach can be thought as considering an exogenous change in the rental price of property units, taking as given the market price determined in a general equilibrium model. Moreover, as we focus on the short-run effects of the rent freeze, it is unlikely that population growth or trends in urbanization have substantial effects on the demand for rental properties.

[23] This assumption is for tractability and does not affect the main conclusions of the model.

[24] This assumption purposely abstracts from borrowers' wealth and borrowing constraints, which imply an interesting but unnecessary complication to the model. In fact, following this assumption and given the infinite-horizon environment, we can then abstract from the extent of the investment in refurbishing, and we can focus on its expected duration as the key factor influencing its profitability.

[25] See https://www.value-marktdaten.de/ (last accessed in April 2021) for a description of sources as well as quality checks applied.

[26] Thus, privately concluded contracts—common, for example, among students living in shared apartments—are not covered. However, these cases do not typically involve a new rental agreement with landlords. Therefore, they fall outside the scope of our analysis.

[27] In the case of missing exact address information (e.g., street name but no street number), geographic coordinates are estimated as well as

a confidence circle. We use this circle as a measure of trustworthiness into the constructed pseudo-address and check the impact on results. However, these technicalities do not affect the overall results (see Online Appendix B).

[28] For this reason, further tested keywords have been discarded, namely *abschlag* (reduction), *alternativ* (alternative), *aufheben* (revoke), *nutzungentgelt* (usage fee), *senat* (senate), and *wohnrecht* (housing legislation).

[29] Online Table C7 compares the number and proportion of advertisements of rental dwellings by Berlin's 12 districts, according to Immobilienscout24 and VALUE Marktdaten. There is a very strong positive relationship: the larger the share of observations in one district, according to the Immobilienscout24, the larger the share, according the VALUE Marktdaten. The differences in the shares vary between −1.6 percentage points for Charlottenburg-Wilmersdorf to +2.9 percentage points for Marzahn-Hellersdorf. This is relatively small given an average share of about 8.3%.

[30] See https://www.stadtentwicklung.berlin.de/wohnen/mietspiegel/de/wohnlagenkarte.shtml, last accessed in December 2020.

[31] See "Zweite Verordnung zur Änderung der SARS-CoV-2-Eindämmungsmaßnahmenverordnung, vom 21. März 2020," available online in German https://www.berlin.de/sen/justiz/service/gesetze-und-verordnungen/2020/ausgabe-nr-12-vom-27-3-2020-s-217-224.pdf, last accessed in December 2020.

[32] In Germany, the government reacted to the strained financial situation of tenants caused by the pandemic by imposing stronger protection from eviction. In late March 2020, a law was enacted that prohibited eviction of tenants who did not pay their rent between April 1 and June 30, 2020 (Gesetz zur Abmilderung der Folgen der COVID-19-Pandemie im Zivil-, Insolvenz- und Strafverfahrensrecht of March 27, 2020). Unlike some other countries, no rent freezes were introduced. The purchasing power of households was supported by different subsidies that allowed them, among other things, to pay rent on time. For example, the ratio of rent debts to the total rent revenue computed for housing companies belonging to the Federal Association of German Housing and Real Estate Companies (GdW Bundesverband deutscher Wohnungs- und Immobilienunternehmen e.V.) remained in 2020 at 2.1%, even lower than the average over the 2011–2019 period.

[33] In their investigation regarding the rent brake, Mense et al. (2023) exclude dwellings with building ages ranging between 2 and 10 years although they were reported as first-time use in order to mitigate measurement error.

[34] It is still possible that the rents were raised for already concluded contracts, which cannot be observed from asking prices. However, given rather strict regulations concerning rent adjustments within existing contracts, this is quite improbable.

[35] We express the distance in 100-m steps and define it as the minimum Cartesian distance between an apartment's geolocation and the border polygon. We apply the universal transverse mercator projection, zone 33, where Brandenburg and Berlin are located.

[36] For selecting a minimum bandwidth, we rely on optimally criteria developed by Imbens and Kalyanaraman (2012): for a triangular kernel, this yields a bandwidth of 1.54 km and, for a rectangular kernel, 2.42 km. The upper bound results from Berlin's geographic setting. The maximum distance between any advertised flats within Berlin and the city boundaries in the *Pre-E* and *Post-E* period was 11.8 km. Thus, a distance of more than 10 km would introduce ambiguities of how to allocate advertisements to unique border segments.

[37] See "Monitoring zur Anwendung der Umwandlungsverordnung. Jahresbericht 2020" by the IfS Institut für Stadtforschung und Strukturpolitik GmbH, Section 3.3.1: Berlin, p. 72; available at https://www.stadtentwicklung.berlin.de/aktuell/pressebox/archiv_volltext.shtml?arch_2112/nachricht7242.html, last accessed in January 2022.

[38] Incomplete weeks are excluded from the analysis.

[39] See figures reported by Immobilienscout24: https://www.immobilienscout24.de/lp/covid19/immobilienmarkt-in-zeiten-von-corona.html#angebot, last accessed in February 2022.

[40] Reassuringly, however, we show in Online Table C7 that the subsample of units advertised on the Immobilienscout24 platform does not systematically deviate from the full set of online advertisements collected by Value Marktdaten.

[41] Apparently, some authors of advertisements were informed about the coming law and included the corresponding provisions in their advertisements. Another explanation is that such provisions were included later in the advertisements that were published prior to the enactment of the rent freeze law.

[42] We can rule out that mentioning alternative rents is systematically driven by any observable physical or locational property characteristics (see Online Table C6).

[43] Another rational strategy could be to agree upon a limited rent duration for new contracts. Unfortunately, we cannot directly test for an increase in short-term rent agreements as such information is not provided in real estate advertisements. Nonetheless, the strict rent law generally foresees an indefinite contract duration (see Holm et al. 2018). This fact translates into a rather long observed average tenure duration in German cities: 2017 microcensus data reveal for Germany's major cities a median tenure length of about nine years, whereas 23% of renters report having rented their apartment for 24 years or longer (see Holm et al. 2018).

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