Rent Control, Market Segmentation, and Misallocation: Causal Evidence from a Large-Scale Policy Intervention*

Andreas Mense[a], Claus Michelsen[b], Konstantin A. Kholodilin[b]

[a] *University of Erlangen-Nuremberg & London School of Economics*
[b] *German Institute for Economic Research (DIW Berlin)*

Abstract

This paper studies market segmentation that arises from the introduction of rent control. When a part of the market remains unregulated, theory predicts an increase of free-market rents due to the misallocation of households to dwellings. To document this mechanism empirically, we study a large-scale policy intervention in the German housing market. We isolate the misallocation mechanism by exploiting temporal variation in treatment dates in an event study design. We find a robust positive spillover effect of rent control on free-market rents. Moreover, housing services consumption of households living in rent-controlled units increased. [93 words]

Keywords: Misallocation; price controls; rent control; spillovers.

JEL classification: D4, R31, R21.

*Andreas Mense: University of Erlangen-Nuremberg, Findelgasse 7, 90402 Nuremberg, Germany, `andreas.mense@fau.de`. We thank seminar participants at the Spatial Dimensions of Inequality Workshop at ZEW Mannheim, the Economic Geography Workshop at the University of Jena, the V Workshop on Urban Economics at IEB, the SERC workshop at LSE, the annual meetings of Verein für Socialpolitik 2017, Urban Economics Association 2017, Royal Economic Society 2018, European Urban Economics Association 2018, AEA at ASSA 2019, the ESCP-TAU-UCLA Housing Conference Madrid 2019, and the Affordable Housing Workshop at Bank of Spain 2019 . We are grateful for valuable comments and suggestions from Gabriel Ahlfeldt, Guillaume Chapelle, Jeffrey Cohen, Paul Cheshire, Richard Green, Christian Hilber, Mathias Hoffmann, Tim McQuade, Henry Overman, Christopher Palmer, Gary Painter, Johannes Rincke, Andrés Rodríguez-Pose, Tuukka Saarimaa, Kurt Schmidheiny, Olmo Silva, Nitzan Tzur, and Matthias Wrede. Part of this research was conducted while Andreas Mense was a Visiting Fellow at LSE. Andreas Mense thanks the London School of Economics for its hospitality. An earlier version of this paper circulated under the name *Empirics on the Causal Effects of Rent Control in Germany.*

1. Introduction

Regulators frequently intervene in the price mechanism of markets, for example in form of minimum wages or price controls for fuels, agricultural products, and pharmaceuticals. In particular, housing markets are many times subject to rent controls, often accompanied by long-standing, emotional debates amongst scholars and policymakers, centered on distributional issues. If not at the national level, rent controls exist at the local level in many countries, e.g., in the United States. Nearly every textbook on housing and real estate economics covers this topic (see, e.g. McDonald and McMillen, 2010; O'Sullivan and Irwin, 2007). Affordable housing is also a major issue in election campaigns. For example, in 1948, U.S. President Harry S. Truman won the White House by campaigning for the *Fair Deal*, which included a promise to resolve housing shortages (Von Hoffman, 2000). Even in recent years, affordable housing remains a vibrantly discussed topic: In light of sharply increasing rents in urban areas in Germany, the Social Democrats succeeded in launching a debate around the need for stricter rent controls in the 2013 German Bundestag elections (Knaup et al., 2013) and eventually managed to introduce rent controls 2015. In the 2021 Bundestag election campaign, several parties – including the Social Democrats – advocate tighter rent control measures. In the UK, increases in rents and rising shares of privately owned rental housing fuel the ongoing debate on rent control (Wilson, 2017), and housing played a major role in the 2015 UK general elections (Kelly, 2015). Inspired by the German rent control regime, Lille and Paris[1] (France) introduced similar regulations in 2014 and 2015, and in May 2019, the government of Catalonia (Spain) proposed to introduce a literal copy of German 2015 rent control regulations, although the law never came into force.

In this paper, we study market segmentation that arises from the introduction of a price ceiling in the market for rental housing in Germany in 2015. This policy intervention can be considered to be a poster-child of so-called "second-generation rent control".[2] The regulation establishes a rent ceiling only for a part of the market,

[1] The Paris ordinance was repealed by court in 2017.

[2] There are various types of rent controls that differ in their rules, scope and restrictiveness. A simple taxonomy distinguishes between first- and second-generation rent controls (Turner and

leaving the price mechanism in the rest intact. Moreover, in contrast to other rent control policies studied previously, the German rent cap is a pure price control. It did not impose other constraints on landlords that could also influence landlord or renter behavior. This puts us in the unique position to study important theoretical mechanisms triggered by the introduction of a price cap. As such, the German rent control policy represents an excellent test-case to empirically evaluate the causal effects of rent control on regulated and free-market rents, housing service consumption, and housing supply.

We base our empirical analysis on a standard comparative-static model of a divided (controlled/free) housing market (see, e.g. McDonald and McMillen, 2010; Skak and Bloze, 2013). We formalize and generalize the graphical representation to establish that an increase in free-market rents in response to rent control indicates misallocation of households to housing units. Furthermore, the size of the welfare loss related to misallocation increases with the strength of the spillover. The mechanism is as follows: Rent control allows some households, which otherwise would have been unwilling to rent a unit in the market, to compete for rent-controlled units, thereby replacing other households with higher willingness to pay. The latter households move to the free-market segment and bid up the price there.

For a number of reasons, many economists oppose rent regulations.[3] This paper

Malpezzi, 2003; Arnott, 1995): First-generation rent controls target the entire market, while second-generation controls, implemented since the mid 1960s, regulate only parts of the market. Supporters of rent controls argue that second-generation regulations, if adequately designed, can increase welfare, e.g. by stimulating additional construction activity in the uncontrolled part of the housing market (Arnott, 1995; Skak and Bloze, 2013).

[3]The extensive economic literature on rent controls almost unanimously opposes regulations—even the more flexible forms—finding them to be inefficient instruments at fighting the effects of housing market shortages (Arnott, 1995; Glaeser and Luttmer, 2003). Available studies suggest that rent controls cause immediate reductions to the market value of rental housing (Early and Phelps, 1999; Fallis and Smith, 1985; Marks, 1984), depress refurbishment, reduce maintenance (Kutty, 1996; Andersen, 1998; Olsen, 1988b; Moon and Stotsky, 1993), slow construction activity (McFarlane, 2003; Glaeser and Luttmer, 2003), and induce inefficient allocation of units (Glaeser and Luttmer, 2003; Arnott and Igarashi, 2000), while—in the short run—having ambiguous effects on rents (Nagy, 1997; Early, 2000; Fallis and Smith, 1984; Smith, 1988). Furthermore, the targeted groups only partially benefit (Linneman, 1987; Ault and Saba, 1990; Glaeser, 2003). Most of these results are derived from theoretical models that—depending on the viability of the assumptions—provide, at best, ambiguous predictions on the effects of rent controls, as some authors criticize in this context (Arnott, 1995; Olsen, 1988a,b; Kutty, 1996).

sheds light on two important aspects from the economic debate revolving around price controls in general and the effects of rent controls specifically.

First, based on an excellent housing market example, we provide quasi-experimental evidence on the distorting effects of price controls on the allocation of goods.(Glaeser and Luttmer, 2003; Davis and Kilian, 2011; Wang, 2011). Glaeser and Luttmer (2003) argue that traditional welfare analysis often ignores welfare losses from misallocation, i.e., from the allocation of goods to buyers who do not value these goods the most. So far, the literature on price controls focuses mainly on quantity responses, e.g., in the labor market in response to minimum wages (Card and Krueger, 1994; Stewart, 2004; Dube et al., 2010, 2016; Monras, 2019), a point noted already by Glaeser and Luttmer (2003). We demonstrate theoretically and empirically, that market segmentation—induced by price regulation—causes substantial misallocation. Yet, quasi-experimental evidence for misallocation remains scarce, particularly in housing market context. The findings of Glaeser and Luttmer (2003) and Skak and Bloze (2013) are based on cross-sectional variation between regulated and free local markets, requiring relatively strong, non-testable assumptions for identification. For example, there may be income-based sorting into locations with and without rent control, and between controlled and free-market segments in the same location (Autor et al., 2014). Housing demand and rental housing supply may also differ between locations for reasons that are not captured by the data. In such settings, it is hard to disentangle misallocation from other determinants of housing services consumption. This paper is the first to establish a causal link between rent regulation and misallocation.[4]

Second, we contribute to the small, but growing literature that analyzes the causal effects of rent controls. In a recent paper, Diamond et al. (2019) finds that tenancy rent control reduces household mobility and the size of the rental housing stock, and leads to city-wide increases of rents. Results from Sims (2007) and Autor et al.

[4]Skak and Bloze (2013) estimate a demand function of housing service consumption from a cross-section of locations without rent control, using lagged vacancy rates and the urbanization rate as supply shifters, and apply this demand function to locations with rent control to calculate an implied 'unconstrained demand'. Their rent data is also cross-sectional, and rents are measured with a five-year lag. Similarly, Glaeser and Luttmer (2003) use estimates for housing service consumption by socio-demographic groups from free markets to predict housing services consumption in rent-controlled markets. They do not consider rents in their analysis.

(2014) point in the direction that there is only a small effect of rent control on construction activity, but a shift of dwellings from rental to owner-occupied status and a deterioration in the quality of existing rental units (Sims, 2007). Moreover, the impact on the price of the non-controlled housing stock is negative and substantial (Autor et al., 2014).[5] Spillovers from the regulated part of the market to other parts work through various channels. Autor et al. (2014) propose two channels that may give rise to these same-sign spillovers: externalities through (i) higher maintenance and (ii) spatial sorting by income. The analysis of the 25 years lasting Cambridge rent control suggests that the maintenance and household sorting channels dominate in the long-run. In contrast, we isolate an opposite-sign spillover effect. Arguably, our short-run perspective allows us to shut down the more inert maintenance and sorting channels, and it minimizes the (negative) response of the supply of rental housing (as in Diamond et al., 2019). Moreover, the paper provides first evidence on the effects of rent control in a European housing market with a high share of rental housing.

We exploit the spatial, temporal, and within-market variation generated by the law to identify the effects of regulation, employing both an event study and a difference-in-differences strategy. In the analysis, we focus on three aspects: first, we analyze the impact of the rent cap on regulated and unregulated rents within a housing market. We focus on the short-run effects and find that regulated rents decreased immediately after the rent cap became effective, while rents in the free-market segment *increased* after a lag of one to two months. Second, we assess the demand responses to the regulation and analyze how households adjusted housing service consumption. We use data from the German Socio-Economic Panel (GSOEP) to evaluate the effects of the regulation on the share of income spent on housing services as well as on the consumption of living area and number of rooms. The results suggest that households responded to the rent cap by increasing housing service consumption. Third, we focus on the supply side effects of the rent cap. We do not find evidence for negative short-run impacts on rental housing supply in the regulated segment, thereby discounting alternative explanations for the estimated spillover. Our analysis further reveals that

[5] Relatedly, Autor et al. (2017) use rent de-control to show that gentrification reduces (or crowds out) crime.

a larger number of small residential buildings were demolished in 2016 in order to be replaced with a new residential building. In an accompanying study (Mense et al., 2019), we find evidence that prices for building lots increased in regulated markets. Both findings are consistent with positive revenue expectations for new, unregulated residential buildings. Finally, the numbers of housing units built in 2016 or 2017 were not affected significantly.

Our results are important for the following reasons. First, price controls are ubiquitous in housing markets. They typically only apply to part of the market. We provide clean evidence on the misallocation mechanism that is triggered by such market interventions. Second, we extend the analysis in Glaeser and Luttmer (2003) to partly controlled markets and provide a much simpler test of misallocation that relies exclusively on the price reaction. This is important for settings where other buyer and goods characteristics are unobservable. For instance, our strategy can also be applied to labor markets. Minimum wage regimes do not restrict self-employed persons, which may cause the same misallocation mechanism that we uncover for the case of second-generation rent controls. Whereas it is sufficient for our test to observe wage rates in the two market segments, the approach suggested by Glaeser and Luttmer (2003) also requires data on job and worker characteristics. Third, we provide evidence on the short-run supply response to the rent cap. The spillover to free-market rents represents an incentive for developers to demolish old units and to build new rental housing. This channel has not been considered in previous work, but it likely affects the future size of the rent-controlled stock, the uncontrolled segment, and the overall size of the local rental market – working in opposition directions as other channels documented by the literature.

The remainder of this paper is structured as follows: In the next section, we present a simple theoretical framework that helps to interpret the effect of rent control on free-market rents. Then, we outline the institutional background and stylized facts regarding recent developments in the German housing market in Section 3. In Section 4, we present our empirical strategy and the results. In the final section, we discuss our findings.

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

The standard prediction of a comparative-static model of the housing market is that a cap on rents reduces revenues for landlords, house prices, and incentives to invest. In the long-run, the housing stock declines. As some authors argue, this result is not straight-forward for second-generation rent controls (Arnott, 1995; Olsen, 1988a,b; Kutty, 1996). In particular, settings that divide the market into a regulated and a free segment (e.g., dwellings built before/after a specific date) can generate a positive effect on free-market rents, representing an incentive to invest. This section establishes that misallocation induces a positive effect on free-market rents. The spillover can thus be used as the basis for an empirical test of misallocation.

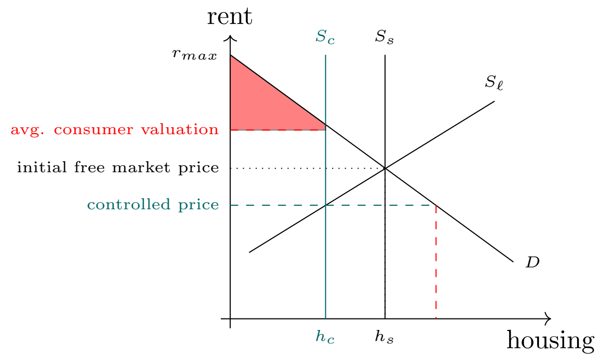
2.1. A comparative static representation of second-generation rent controls

This can be illustrated in a standard comparative-static framework (see, for example McDonald and McMillen, 2010; Skak and Bloze, 2013), as depicted in Figure 1: consider an unregulated market where demand (D) for housing decreases with the rent level (vertical axis). At the intersection with the perfectly inelastic short-run housing supply (S_s), the market is in equilibrium, providing h_s units of housing services (a function of housing quantity and quality). Ideally, the short-run equilibrium is identical with the long-run balance of demand and supply (S_l). The slope of the long-run supply curve is determined by the costs of new development, maintenance, and refurbishment. The housing stock expands as long as rental income exceeds the costs of housing service production, while deterioration may reduce the housing stock over time. Here, we abstract from such supply effects in order to focus the discussion.[6]

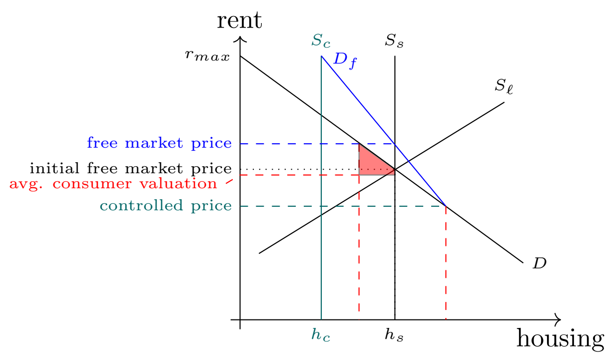
Consider the introduction of rent control. Under *first-generation* rent control, the controlled price applies to the whole market (Figure 1a). As for some units, marginal costs of renting out the unit exceed the controlled price, supply declines to S_c, implying a loss of (rental) housing units equal to $h_s - h_c$. The controlled price allows households that would have been forced to leave the market in absence of rent

[6] Arguably, deterioration effects are quantitatively much more important over a longer horizon.

Figure 1: Misallocation under *first-* and *second-generation* rent controls



a. *First-generation rent control*



b. *Second-generation rent control*

control to compete for dwellings in the city. Dwellings are allocated to households by mechanisms other than willingness to pay, e.g. queuing, lottery, or nepotism. Under random allocation, the welfare loss resulting from misallocation is equal to the red tetragon (see e.g. Glaeser and Luttmer, 2003).

In second-generation rent control, the controlled price applies only to part of the market (Figure 1b). This divides the housing stock h_s into a regulated segment (h_c) and a free-market segment ($h_s - h_c$). In the free-market segment, housing units are allocated by the willingness to pay. To keep the representation simple, we assume that allocation in the regulated segment is random. This implies that households unable to benefit from rent control are a random subset of all households whose willingness to pay exceeds the controlled price. Thus, the demand curve in the free-market segment,

D_f, connects the intersection of S_c and the maximum willingness to pay r_{max} with the intersection of the controlled price and D_f. In this setting, the introduction of the rent cap pushes up the price of new dwellings from the initial to the new free-market price. The driving force behind this result is the random allocation of households to rent-controlled units.[7]

The red tetragon depicts the welfare loss due to misallocation. The relevant part of the demand curve lies between the two vertical dashed red lines; these households have a willingness to pay that is below the price in the free-market segment but above the controlled price. Because these households are allocated randomly to rent-controlled units, what matters for welfare is the average consumer valuation in this group (depicted by the horizontal dashed red line).

The simple graphical representation has several implications: As the introduction of rent control leads to a decline of rents in the controlled sector, this triggers an increase of free-market rents. This represents an incentive to build new rental housing.[8] Since house prices are determined by future rental income, prices of building lots where new, unregulated housing can be developed should also increase. The previous literature documented negative impacts on maintenance and neighborhood sorting. However, these latter effects likely need more time to unravel, so that we can isolate the misallocation-induced rent spillover by taking a short-run perspective.

2.2. A simple formal model

To generalize the previous discussion and to derive more precise propositions, we develop a simple formal model. The model shows that, under partial price control, there is misallocation if at least some households with a willingness to pay for a

[7]An alternative explanation for increased prices of uncontrolled units could be a reduction of supply of controlled units, due to decreased maintenance effort (resulting in accelerated depreciation) or the conversion to owner-occupied status. In that case, a positive spillover to free-market rents would result even if there is no misallocation: The reduced supply would lead to a move up the original demand curve. We investigate this possibility below in the empirical analysis. In our setting, there are no signs for a reduction of the supply of rental housing units, neither at the time the coalition agreement on the rent control law was reached, nor at the dates when the rent control regime became effective at the local level.

[8]However, the introduction of rent controls may also affect long-term expectations regarding the regulatory environment, reducing the expected profitability of new rental housing.

dwelling below the initial equilibrium price consume housing services at the controlled price. This outcome is associated with an opposite-sign spillover to the free-market price.

Baseline. Assume households living in a dwelling receive utility $u = \varepsilon - r$, where ε has distribution F, F^{-1} exists, and r is the rent. The supply of housing units is costless and fixed at quantity 1, and there are $N > 1$ potential renters. Through competition, households bid up rents, and the marginal renter determines the equilibrium rent level. Households that do not get to rent a unit receive utility 0 from living somewhere else.

It must hold that

$$1 = N(1 - F(\varepsilon_0)), \tag{1}$$

where ε_0 is the willingness to pay of the marginal renter. Letting r_0 denote the equilibrium rent level prior to rent control, we have

$$r_0 = F^{-1}\left(1 - \frac{1}{N}\right). \tag{2}$$

Rent control. Suppose that rent control is introduced for a share $0 < \rho \leq 1$ of units. These units must not be rented out at a price higher than $\bar{r} < r_0$. Letting $J \subseteq [\bar{r}, \infty)$ denote households that get to rent one of these units, the equilibrium conditions are

$$\rho = N \int_J dF(\varepsilon), \tag{3}$$

$$1 - \rho = N \int_K dF(\varepsilon) \tag{4}$$

where $K \subseteq [r_0, \infty) \setminus J$. For given ρ and J, this determines the free market rent r_1,

$$r_1 = \inf K. \tag{5}$$

In the polar case where $J = [F^{-1}(1 - \rho/N), \infty)$, households with the highest willingness to pay get to live in rent-controlled units. Hence, only households with a willingness to pay below $F^{-1}(1-\rho/N)$ compete on the free market. Note that, because units are allocated to the highest bidders on the free market, $K = [r_1, F^{-1}(1 - \rho/N))$ must hold for some r_1 (i.e., K must not have any holes). In this case, the price on the free market does not change upon imposition of rent control:

$$J = \left[F^{-1}\left(1 - \frac{\rho}{N}\right), \infty\right) \Rightarrow 1 - \rho = N \int_{[r_1, F^{-1}(1-\rho/N))} dF(\varepsilon)$$
$$\Rightarrow \frac{1-\rho}{N} = 1 - \frac{\rho}{N} - F(r_1) \Rightarrow F(r_1) = 1 - \frac{1}{N} \Rightarrow r_1 = r_0.$$

Hence, there is no spillover to free market rents in this case.

In the supplemental material, we show that the reverse is also true: If some households with a willingness to pay below the initial free-market rent get to rent a rent-controlled housing unit, the rent cap leads to a positive spillover on rents in the free-market segment.[9] Moreover, the associated welfare loss increases weakly with the size of the spillover effect. Based on this representation, we can derive the following propositions:

Proposition. *Consider the simple model described above.*

(i) A test of the null hypothesis $r_1 = r_0$ *is a test of no misallocation.*

(ii) For a given initial equilibrium price r_0 *and a rent ceiling* $\bar{r}$*, the welfare loss due to misallocation weakly increases in* r_1*.*

Proof. See Section OC. □

3. Institutional setting

Before outlining our empirical strategy in detail, we briefly describe the German housing market, its institutional setting and the rent regulation studied in this paper.[10]

After 15 years of stagnation, rents in Germany started to increase rapidly in 2010, while vacancy rates fell, particularly in the urban housing stock. In 2016, rents in newly concluded contracts were on average 23% above the level observed

[9] Subject to minor technical qualifications.
[10] A more more extensive description of the German (rental) housing market and rent regulation is given in the Online Appendices OA and OB

in 2010. This development triggered debates about growing housing costs in urban areas, which ultimately led to the introduction of second-generation rent control in 2015. In contrast to the existing body of rent regulation in Germany, the 2015 rent control applies only to newly concluded contracts.[11]

There are two key differences that allow us to disentangle the effects of the new regime from the effects of the pre-existing rent controls. First, the existing rules applied uniformly over space in all regions of Germany. Second, the new regime introduced a rent ceiling for newly concluded contracts, whereas the pre-existing regime did not regulate rents in new contracts.

In 2015, the German parliament passed a law that empowered state governments to introduce a rent cap in municipalities characterized by "tight housing markets", which is e.g. the case if local rents grew faster than at the national average. This rent cap introduces a rent ceiling for new rental contracts that depends on past local rent growth. In new contracts, rents are not allowed to exceed the typical local rent by more than 10%.[12] There are four major exceptions from the law: First, rents are freely negotiable for contracts of newly built dwellings (housing completed after October 1, 2014) and all contracts that follow, making the rent cap an example of second-generation rent control. Second, units that are rented out temporarily are exempted. Third, there is no limit on the rent in the first contract after a substantial refurbishment of an existing dwelling (worth at least one-third of today's reconstruction costs of the dwelling). Fourth, if the rent of the previous contract was above the limit of the rent cap, landlords may conclude this level in all subsequent contracts.[13]

[11] In Germany, a "tenancy rent control-decontrol" regime was introduced in 1972. The regime allowed landlords to set rents freely when renting out to a new tenant, but rent changes in existing contracts were regulated. The rules for admissible rent changes and the rights of tenants as well as protection from evictions changed over time, but the basic regime is still in place today.

[12] The local reference rent is documented in so-called *Mietspiegel*, that is, a survey of rents in the municipality. If a *Mietspiegel* does not exist, the local reference rent can also be determined by a sworn expert on a by-case basis or by taking the average rent for at least three comparable housing units.

[13] The 2015 rent regulation falls under civil law, which makes it more difficult for renters to ensure law enforcement. In the period under consideration, there were no direct consequences for non-compliant landlords, as it was not possible for local, state, or federal governments to impose fees or other judicial measures. If a tenant successfully litigates a case, landlords have to refund the overpaid rent and bear all legal costs. This setting led to the development of a well-organized

Table 1: Rent cap ordinances by federal states

Federal State	Validity period	Regulated/all municipalities	Cumulative # of municipalities	Cumulative population share
Berlin	2015/06-2020/05	1/1	1	0.043
Hamburg	2015/07-2020/06	1/1	2	0.065
North Rhine-Westphalia	2015/07-2020/06	22/396	24	0.116
Bavaria	2015/08-2020/07	144/2056	168	0.174
Baden-Württemberg	2015/10-2020/09	68/1101	236	0.212
Rhineland Palatinate	2015/10-2020/10	3/2306	239	0.217
Hesse	2015/11-2019/06	16/426	255	0.240
Bremen	2015/12-2020/11	1/2	256	0.246
Schleswig-Holstein	2015/12-2020/11	12/1116	268	0.251
Bavaria[a]	2016/01-2020/07	137/2056	261	0.251
Brandenburg	2016/01-2020/12	31/419	292	0.260
Thuringia	2016/04-2021/01	2/913	294	0.264

[a] 16 municipalities listed in the first Bavarian rent cap ordinance were removed, while nine new municipalities were added.

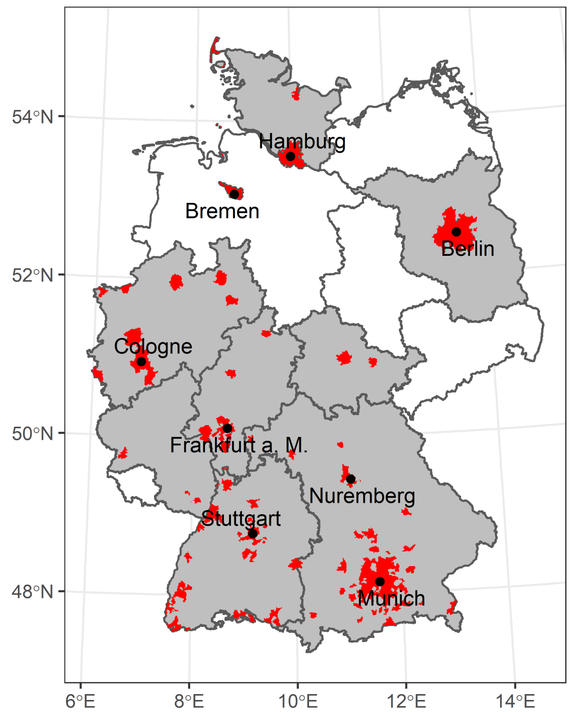
Eleven federal states implemented the rent cap at the local level between June 2015 and April 2016—at various points in time (see Table 1). Two years after their introduction, the rent cap was adopted in 294 municipalities. These municipalities have a population of about 21.5 million, and represent a quarter of the German housing stock. The regulation concentrates on urban areas, where rent and house price increases have gained strong momentum since 2010. Figure 2 displays a map of Germany, with municipalities that had adopted the rent cap in June 2016 in red.

4. Empirical analysis

In the empirical analysis, we investigate the short-run effects of the German rent cap. Primarily, we seek to establish the opposite-sign spillover to free-market rents as a test of misallocation. To disentangle general market dynamics from the effects of the rent cap, we start with an event study design that identifies both the effect on controlled rents, and the spillover to free-market rents, exploiting the state-specific start dates of the rent cap (see Table 1). As an alternative strategy, we

and efficient legal industry, geared towards helping tenants to ensure their rights, see e.g. https://www.wenigermiete.de/. Moreover, many renters in Germany are organized in renter associations that provide full legal coverage in renter-landlord disputes as a service for its members, e.g. https://www.berliner-mieterverein.de/. In this setting, when ignoring the rent regulation, landlords face a real risk of legal argument. The main financial risk in this setting is due to legal and process costs, which typically have to be paid by the losing party.

Figure 2: Municipalities with rent cap, as of June 2016



Source: Federal Agency for Cartography and Geodesy; own representation. The federal states in grey had adopted the regulation until June 2016. Areas in red represent municipalities where the rent cap was active in June 2016.

employ propensity score-weighting and -trimming to construct two — arguably similar — groups of municipalities with and without rent cap, which we then compare in a difference-in-differences estimation. This strategy relies on a completely distinct source of identifying variation and therefore helps to evaluate the sensitivity of the results to the identifying assumptions of the event study design. Finally, we consider short-run demand and supply reactions, to provide direct evidence for misallocation and to discount alternative explanations for the observed spillover.

4.1. Effects on rents

4.1.1. Data

The results presented in this section rely on posted rents for dwellings offered on the three largest online market-places in Germany (*Immonet*, *Immowelt*, and *Immobilienscout24*) between July 2011 and November 2016. Each dwelling's month of offer and postal code is available in the data, supplemented by a long list of housing characteristics. We provide a more detailed description of the data in Appendix A. Table A1 presents summary statistics for the samples used in the analysis below.

Although concluded rents would be preferable, it seems unlikely that posted rents deviate systematically from concluded rents over longer horizons, suggesting that differences between concluded and posted rents cannot explain stable differences emerging only after the treatment.[14] The alternative of surveyed rents have shortcomings as well: sample sizes are typically small, there may be reporting errors and selection issues, and spatio-temporal information in these data are substantially less precise. Moreover, surveyed rents capture rental prices in the stock of rental contracts, which may not correspond to current market rents.

Two variables indicate whether a dwelling is new (and thus unregulated): the building age, and a 'first time use' dummy. We define a unit as "regulated" if building age is greater than zero. To reduce measurement error, we drop observations with building age greater than zero that are reported as 'first time use', and observations with building age equal to zero that are not 'first time use'.[15] Moreover, we exclude units that are reported to be one or two years old because such units would be exempt when observed late in the sample (in 2015 or 2016).

4.1.2. Market segmentation and spillover to free-market rents

In order to show that the rent cap produced two distinct market segments, we consider separately controlled units and the free-market segment in an event study design. The identifying variation comes from the different start dates at the local level. We focus on a small window around the introduction of the rent cap. Moreover, we restrict attention to the years 2015 and 2016, because new units appearing on the market in 2014 could be subject to the rent cap. The sample is restricted to municipalities where the rent cap was introduced eventually.

[14]This implies that discrepancies between concluded and offered rents could at most explain short-lived effects. We show below that the effects are stable over time, suggesting that concluded rents also shifted in a similar way as offered rents. Moreover, we provide auxiliary evidence consistent with the observed effects, relying on other data sources.

[15]Note that a non-housing unit converted to rental housing would fall under the rent control regime if it had been used for a non-housing purpose before October 2014.

Following Schmidheiny and Siegloch (2020), we specify the following event study regression:

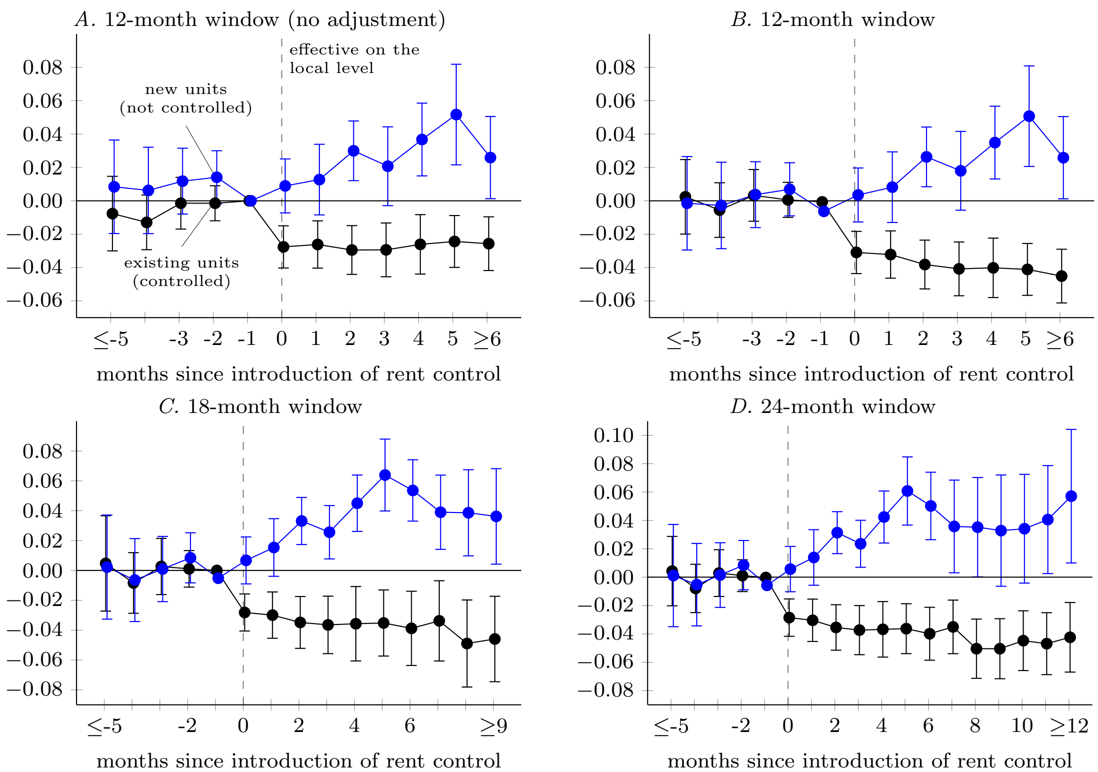
$$\ln r_{it} = \bar{\delta}^{-} \bar{I}_{it}^{-} + \sum_{\tau=-4}^{T} \delta^{\tau} I_{it}^{\tau} + \bar{\delta}^{+} \bar{I}_{it}^{+} + x_{it}^{\prime}\beta + \text{postcode FE}_{i} + \text{month FE}_{t} + \eta_{it}. \quad (6)$$

The indicators I_{it}^{τ} are equal to one if unit i observed in period t is τ periods apart from the introduction of the rent cap at the local level (with $\tau = 0$ representing the month when the rent cap became effective). The last month before the introduction of the rent cap, $\tau = -1$, is the baseline period, which we impose by setting $\delta^{-1} = 0$. That is, we define the event time indicators symmetrically around the baseline period. Following the suggestions by Schmidheiny and Siegloch (2020), $\bar{I}_{it}^{-}$ and $\bar{I}_{it}^{+}$ are 'binned' indicators, capturing units observed outside of the treatment window (ranging from -4 to T). These two binned indicators capture long-run differences before and after the treatment. The regression also controls for housing characteristics, and postcode and month fixed effects. In the baseline regression, we start with $T = 5$ and a sample window covering $+/-12$ months around the introduction of the rent cap. In further regressions, we extend the sample window and increase T.

We run the regression separately for the rent-controlled and free-market segments and plot the δ^{τ}'s and 90% confidence intervals in Figure 3. Panel *A* displays the baseline specification, for the smallest sample window ($+/-12$ months). Prior to the treatment date, the trends in both lines are flat, and the two lines diverge after the introduction of the rent cap. In the month when the rent cap was introduced, rents of regulated units decreased sharply, by about 0.025 log points. At this point, rents of unregulated units started to trend upwards, with significant effects two months after the introduction. The lagged effect on unregulated units is consistent with the spillover mechanism, which runs from regulated to unregulated units.

To rule out that pre-trends contribute to the post-treatment pattern observed in Panel *A*, we apply the pre-trend adjustment suggested in Monras (2019) in Panel *B*. The results are virtually identical. Panels *C* and *D* extend the sample window to $+/-$ 18 and $+/-24$ months and estimate a higher number of post-treatment indicators. The results are highly robust to these changes, indicating that the treatment effects were stable in both groups in the first 12 months after the rent cap became effective at the local level.

Figure 3: Impact of the rent cap on controlled and uncontrolled units (event study design)



Notes: The black and blue lines display the δ^τ-coefficients for existing units (subject to rent control) and new units (not subject to rent control), see equation (6). The vertical bars depict 90%-confidence intervals (cluster-robust at municipality level). Covariates for Panel *A* are displayed in Table B2.

After having established that the event time coefficients of neither of the two groups exhibit significant pre-trends, we next turn to regressions with pooled treatment indicators. This helps to assess whether the average rents in the two groups differed systematically before and after the treatment (conditional on fixed effects and covariates), without having to rely on a single baseline period. Because Figure 3 suggests that there was only a small effect on unregulated units shortly after the introduction of the rent cap, we define two indicators that capture periods $\tau = 0, 1$ and $\tau > 1$.

Table 2 summarizes the results. Columns (1) to (3) show the results for uncontrolled units. Consistent with Figure 3, the indicator capturing the first two months

Table 2: Impact of the rent cap on controlled and uncontrolled units (event study design)

Dependent variable:	Log rent					
	Free market segment			Rent-controlled units		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
rent cap effective since this or last month	0.004 (0.009)	0.002 (0.010)	0.003 (0.010)	-0.025*** (0.007)	-0.024*** (0.006)	-0.024*** (0.006)
rent cap effective for two or more months	0.025** (0.010)	0.022* (0.010)	0.021* (0.010)	-0.027** (0.009)	-0.026** (0.008)	-0.029*** (0.008)
Observations	26,782	26,782	33,069	229,689	229,689	262,590
Adj. R^2	0.878	0.879	0.873	0.849	0.849	0.846
Controls	yes	yes	yes	yes	yes	yes
Month FE	yes	yes	yes	yes	yes	yes
Postcode FE	yes	yes	yes	yes	yes	yes
State-specific trend	no	yes	yes	no	yes	yes
12-month window	yes	yes	no	yes	yes	no

Notes: The sample is restricted to 2015 and 2016. Municipality-clustered standard errors in parentheses, $^{***}: p < .001, ^{**}: p < .01, ^{*}: p < .05$.

of the rent cap is insignificant in all specifications. However, the treatment effect is significantly positive thereafter. It is very stable across specifications: Adding a State-specific time trend, and extending the sample beyond twelve months after the introduction of the rent cap does not affect much the two coefficients of interest. In columns (4) to (6), we replicate the same set of regressions for the group of rent-controlled units. Here, both indicators are highly significant and negative. Moreover, they hardly differ across specifications.

4.1.3. Robustness: Propensity score weighted difference-in-difference design

As a robustness check, we employ a spatial difference-in-differences design. We follow a propensity score-weighting and -trimming strategy to construct a sample of — arguably comparable — treatment and control municipalities.

Propensity score model. Rent control was introduced in the first place to combat rising rents. This represents a challenge to the estimation because municipalities without rent control had relatively lower rent growth prior to the policy. State governments selected municipalities that met the criteria provided by the law (a tight housing market). However, the distributions of local rent growth overlap, suggesting that some similar locations at the margin were selected, while others were not. The empirical strategy focuses on these marginal locations, which are arguably much more comparable.

The propensity score model captures the dimensions local housing demand and supply growth, (population) density, and the political economy. We report estimation results, covariate balance, and pre-trends in potential confounders in Appendix C. Generally, weighting and trimming at the 10%–90% interval improves greatly the comparability of the two groups, although some differences remain. The main difference is that rent-cap municipalities are more urbanized. Nonetheless, variables capturing housing demand and local labor market dynamics move in parallel at least since 2002 in the two groups (see Figure C2).

Pre-trends in new and existing rental housing. In contrast to the propensity score model, the rent regressions implicitly place more weight on larger municipalities, where the number of observations is larger. This could result in misaligned pre-trends, despite the fact that the sample of municipalities is balanced with respect to average rent growth in the pre-treatment period (see Table C4).

In order to asses pre-trends, we estimate the following regression:

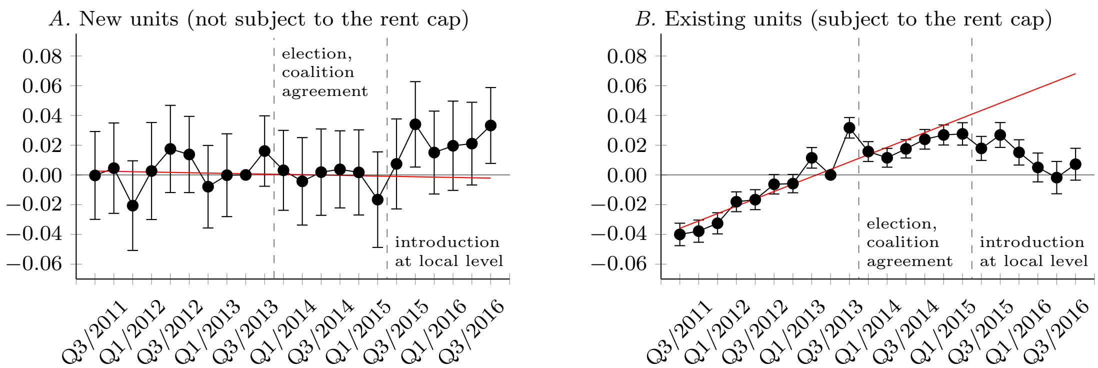
$$\ln r_{it} = x_{it}\beta + \text{postcode FE}_i + \text{quarter FE}_t + \text{quarter FE}_t \times \text{rent cap}_i + \eta_{it}. \qquad (7)$$

Through the quarter $\text{FE}_t \times \text{rent cap}_i$ interaction term, the regression recovers the differential trend, and the impact of the rent cap on this differential trend. By specifying the regression in terms of calendar quarters (instead of event quarters), the identifying variation comes purely from the temporal variation between locations.

In Figure 4, we plot the quarter $\text{FE}_t \times \text{rent cap}_i$-interaction terms for both types of housing units. We add a linear trend line to the graphs that we estimate by fitting the interaction terms against the calendar quarters, using the period up to the passage of the law in Q1/2015.

Four things are noteworthy: First, pre-trends are almost perfectly aligned in the group of newly built housing units (Panel A). Second, there is a positive relative trend in the group of existing housing, (Panel B). Third, this pre-trend appears to be very stable over time, up to the point when the rent cap was introduced. Apart from one or two outliers, the black line in Panel B hovers around the red trend line in the first 16 quarters of the sample period, without longer periods of systematic deviation. Fourth, in both panels, the black line deviates substantially from the trend line starting in Q3/2015, indicating that the relative trend between the treatment and control municipalities changed with the introduction of the rent cap. Rents of new units in municipalities subject to the rent cap increase above the trend line, while the reverse is true for rents of existing units.

Figure 4: Relative rent trends in treatment and control groups (propensity-score weighted and trimmed difference-in-differences design)



Notes: The black lines display the coefficients of the quarterly dummies, δ_τ for existing units (subject to rent control) and new units (not subject to rent control), see equation (6). The vertical bars depict 95%-cluster-robust confidence intervals.

Difference-in-differences estimates. For the difference-in-difference estimation, we compare average rents (conditional on covariates) from the period before the rent cap had become effective in the respective state, to the period after this date. To account for the difference in relative pre-trends, we adjust the log rent for the trend in the pre-period and consider as the outcome variable the deviation from this trend. The identifying assumption is that, absent the rent cap, this relative trend difference between the treatment and the control group would have been stable in 2015 and 2016. We make this assumption based on Figure 4, where it appears that the relative trends are very stable from 2011 to 2014. Moreover, the local real estate markets under consideration experienced a sustained period of persistent rent growth, starting in 2010 and continuing well beyond 2016.

Table 3 displays the treatment effects, as captured by the interaction of the post-treatment dummy with the rent cap status of the municipality. In columns (1) to (3), the sample consists of newly built units that are exempt from the regulation if first-time use was October 2014 or later. In column (1), we use the full sample without propensity score weights, adjusting for the relative trend in this sample.[16] The estimated treatment effect is significantly positive and comparable in magnitude to the estimates obtained from the event study design. When employing the propensity score weights in column (2) and when additionally trimming the sample in column (3), it remains fairly stable.

In columns (4) to (6), we estimate the same three specifications as in columns (1) to (3), but for the sample of existing units. The treatment effects in all regressions are significantly negative and of the same order of magnitude as those obtained from the event study design.

Table 3: Impact of the rent cap on controlled and uncontrolled units (propensity-score weighted and trimmed difference-in-differences design)

Dependent variable:	Log rent					
	Free market segment			Rent-controlled units		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
rent cap effective × treatment group	0.034*** (0.005)	0.023*** (0.007)	0.023** (0.008)	-0.030*** (0.003)	-0.037*** (0.004)	-0.039*** (0.004)
Observations	153,548	120,089	58,885	1,309,423	1,309,423	695,839
Treated municipalities	286	286	179	292	292	183
Control municipalities	4,292	4,292	356	8,110	8,110	365
Adj. R^2	0.903	0.882	0.884	0.877	0.868	0.869
Controls	yes	yes	yes	yes	yes	yes
Quarter FE	yes	yes	yes	yes	yes	yes
Postcode FE	yes	yes	yes	yes	yes	yes
Prop. score-weighted	no	yes	yes	no	yes	yes
Prop. score-trimmed	no	no	yes	no	no	yes

Notes: The sample is restricted to the period before the election 2013 (Q3/2011–Q2/2013, pre-treatment period), and to the period when the rent cap was effective at the local level (post-treatment period). Postcode clustered standard errors in parentheses, $^{***}: p < .001, ^{**}: p < .01, ^{*}: p < .05$.

Very importantly, the two sets of estimates from the event study design in Section 4.1.2 and from this section rely on two completely distinct sources of identifying variation, but lead to very similar results. Overall, we take these estimates as evidence for the hypothesized opposite-sign spillover effect from the rent controlled to the free market segment.[17]

[16] For each regression, we estimate the relative pre-trend in the respective sample. The pre-trend adjustment is negligible in the trimmed sample, consistent with Panel *A* of Figure 4.

[17] In an earlier version of the paper, we employ a regression discontinuity in time-design (Hausman

4.2. Demand response

If misallocation is present in the market, this should not only be reflected in rents of regulated and unregulated housing units. The introduction of rent control should also be observable in housing service consumption patterns, as argued by Glaeser and Luttmer (2003). Glaeser and Luttmer (2003) compare housing consumption by socio-demographic subgroups in a cross-section, finding that households living in rent controlled apartments in New York City consumed substantially more housing services, as compared to similar households living in other cities. In a first step, we consider an alternative, more direct measure of misallocation, the share of income spent on housing services. At a very basic level, as the rental price of housing decreases, mover households should respond by consuming more housing services, limiting the degree to which the income share spent on housing decreases.[18] Turning this argument around, a constant income share of housing consumption despite rent control indicates increased housing service consumption, which implies misallocation of households to housing units due to rent control. The analysis in this section is based on regionally disaggregated micro-data from the German Socio-Economic Panel (GSOEP). GSOEP is a representative longitudinal study for Germany, surveying approximately 15,000 households annually since 1984. The sample is restricted to renters moving house in the years 2002 to 2018. Summary statistics are displayed in Table D5 in the Appendix.

In order to analyze the income share spent on housing services, we follow two distinct identification strategies. First, we compare the income shares spent on housing between movers in regulated and unregulated markets in a difference-in-differences design, using the propensity-score-trimmed and -weighted sample. We regress income shares spent on housing of households moving into housing units built before 2015, on individual characteristics, spatial controls, year fixed effects, a treatment dummy that captures whether the location is subject to rent control after 2015, and a treatment

and Rapson, 2017), finding very similar effects. The regression discontinuity in time-design relies only on time-series variation and thus seems less robust to potential confounders. We therefore decided to focus on the event study and difference-in-differences estimates instead.

[18]Under Cobb-Douglas preferences, the income share spent on housing services would even remain constant.

$\times$ year fixed effects interaction:

$$\begin{aligned}\text{income share spent on housing}_{it} = & x'_{it}\beta + \text{location FE}_i + \text{year FE}_t + \\ & \psi_{\text{year}_i} + (\text{year FE}_t \times \text{rent cap municipality}_i) + \eta_{it}.\end{aligned} \tag{8}$$

x_{it} is a set of individual, household, and building characteristics of household i observed in year t, rent cap municipality$_i$ indicates whether the rent cap was introduced in the municipality, and η_i is the error term. The sample is restricted to years in which the household moves house.

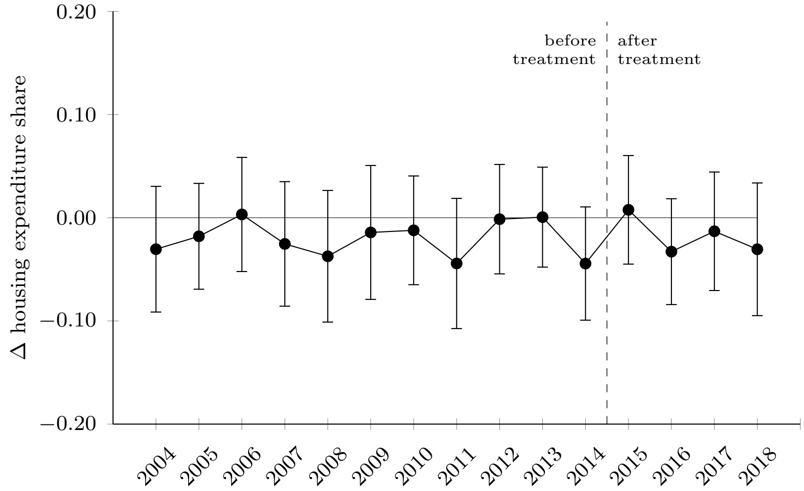
The regression sample is based on the weighted and trimmed sample of regulated and unregulated municipalities. We restrict the sample to households moving into a housing unit built before 2015.[19] As control variables, we use the lagged change of household size, the lagged income, lagged changes in employment status, and the building age at the previous address. As spatial controls, we include federal state fixed effects and a city size classification. Figure 5 displays the interaction effects of the treatment indicator and the year fixed effects. Detailed regression results are presented in Table D6.

The results show that the share of income spent on housing services among movers does not differ systematically between municipalities with and without rent control, neither before nor after the introduction of the rent cap. Since the rent cap successfully lowered rents, as documented in Section 4.1, this result suggests that housing service consumption at the household-level increased in municipalities under rent control. This lends support to the view that the observed spillovers indicate misallocation.

As the second identification strategy, we concentrate on residential moves inside municipalities with rent control. Second-generation rent control divides the market also within regulated cities. We therefore compare the income share spent on housing services of households moving into housing units subject to the rent cap (i.e. built

[19]These units are subject to rent control. There is a temporary exception for units that were substantially refurbished (if landlords spent more than 25% of the replacement value of the housing unit on refurbishments), but this is not reported in the GSOEP. Arguably, such larger refurbishments make up at best a small share of the total number of rent controlled units.

Figure 5: Income share spent on housing services (difference in percentage points)



Notes: The figure shows coefficients of the interactions of an indicator for rent cap municipalities and the year fixed effects, see equation (8). Standard errors are clustered on municipality level. Confidence bands indicate the 90-percent level of significance. Table D6 in the Online Appendix contains results for control variables.

before 2015) and new units that are exempt permanently from the regulation.[20] The results are presented in column (1) of Table 4. Again, we find an insignificant and slightly positive coefficient of the rent control treatment indicator. This suggests that the introduction of the rent cap did not affect differentially the income share spent on housing between households moving into rent controlled housing units and households renting a unit on the free-market segment – despite the difference in rental prices due to the rent cap, as depicted, e.g., in Figure 3.

A constant expenditure share of housing—despite lower rents—should also be reflected in the quantities of housing services consumed, which constitutes the main idea behind Glaeser and Luttmer (2003). Although this quantity is difficult to measure, the number of rooms is a central component of housing services. In columns (2) and (3) of Table 4, we therefore present results from two ordered probit models to assess the effect of rent control on the choice of the number of rooms among mover households. As outcomes, we consider the total number of rooms in the new unit and the change in the number of rooms compared to the previous unit. As in

[20] When observing moves before 2015, we treat new units as exempt from rent control and assign them to the control group.

Table 4: Effects on housing services consumption within rent controlled municipalities

	OLS	Ordered Probit	
	income share spent on housing (1)	total number of rooms (2)	Δ number of rooms (3)
treatment group	-0.01	-0.05	-0.22
	(0.02)	(0.30)	(0.28)
rent cap effective × treatment group	0.03	1.04*	0.83*
	(0.08)	(0.62)	(0.45)
lagged Δ household size	-0.00	0.13	0.07
	(0.01)	(0.09)	(0.08)
lagged household income	-0.00***	0.00***	-0.00***
	(0.00)	(0.00)	(0.00)
lagged job status change	-0.02*	-0.30***	-0.13*
	(0.01)	(0.10)	(0.08)
lagged building age/100	-0.05**	0.20	0.24
	(0.02)	(0.22)	(0.16)
Year FE	yes	yes	yes
Federal states FE	yes	yes	yes
Municipal size FE	yes	yes	yes
Observations	981	981	981
(Pseudo) Adj. R^2	0.061	0.048	0.035

Notes: The treatment group is defined as dwellings built before 2015; the control group comprises new dwellings for which the year of construction is identical to the survey year. Detailed results are available upon request. Municipality-level clustered standard errors in parentheses, $^{***}: p < .01, ^{**}: p < .05, ^{*}: p < .1$.

column (1), we compare households moving into rent-controlled units to households that move into a newly constructed unit, in municipalities where rent control was introduced in 2015 or 2016. The results lend further support to the view that housing consumption increased in response to rent control. Both in columns (2) and (3), the treatment effect is marginally significant and positive, suggesting that households benefiting from reduced rents chose a higher number of rooms. Around the sample mean (3.0 rooms), the probability to consume one additional room increased with the introduction of rent control, by approximately 15.3%.[21] In other words, about one out of seven households opted for increasing the size of the dwelling by one room. For a three-room unit, this represents an increase in the number of rooms by 33.3%. Combining the two margins, the average impact amounts to 15.3% × 33.3% = 5.1%,

[21] Marginal effects of treatment are computed at the sample mean. The marginal impact of rent control on the probability to increase housing service consumption by one room is significant at the 10%-level of confidence.

a similar magnitude as the estimated relative difference in rental prices due to rent control in the full sample (see Sections 4.1.2 and 4.1.3).

Unfortunately, the GSOEP panel stopped surveying detailed housing characteristics in 2014. Therefore, we are not able to elaborate further on the consequences of rent control on housing service consumption and housing quality choice. Overall, we interpret the result as a supporting piece of evidence that the price spillovers indeed measure misallocation of households to housing units.

4.3. Supply response

The preceding sections documented a spillover effect of the rent cap on the free-market segment and an increase in individual housing demand, as suggested by the theoretical framework. Both the artificial reduction of rents in the controlled sector, and the spillover effect can give rise to supply responses, such as conversions of rent-controlled housing to condominiums or commercial use, the demolition of existing residential buildings, and new housing supply.[22]

4.3.1. Supply of rent-controlled housing units

Apart from being interesting in its own right, supply effects represent an alternative explanation for the observed positive spillover on free-market rents. If part of the rent-controlled housing stock is converted to owner-occupied housing shortly after the introduction of the rent cap at the local level, free-market rents would have to increase to accommodate overall housing demand.[23] In theory, this is possible also in the short-run.

To tackle this issue, we consider the flow of rental housing units on the market as the outcome variable. Specifically, we use the rental listings data to calculate

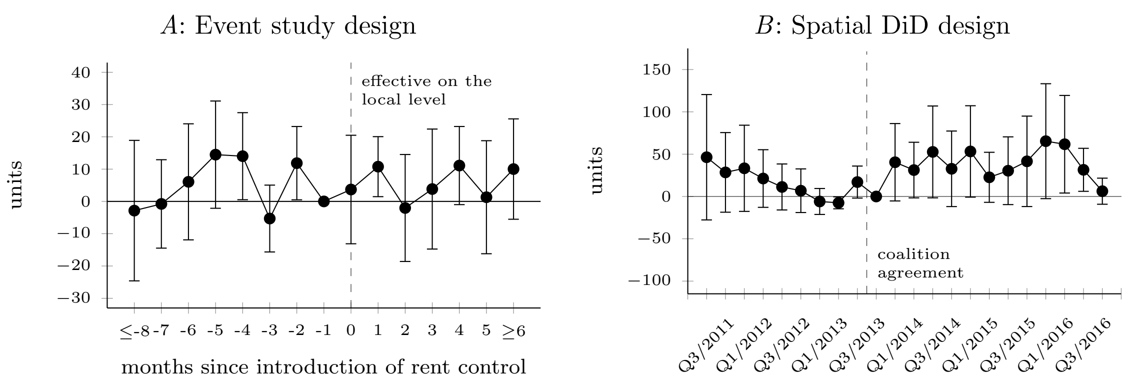
[22] There can also be effects on maintenance and unit quality (Sims, 2007). Under the German rent cap, the individual unit's rent ceiling increases with its quality, its furnishing, and its condition (because the rent ceiling depends on average rents paid for similar units). Thus, there still is an incentive to invest in existing units, while the marginal benefit of a renovation might even increase due to the rent cap. Having said this, we do not find measurable effects on maintenance and unit quality in the period covered by our sample (not reported, in the interest of space). Arguably, (negative) maintenance effects become more important over a longer horizon.

[23] Landlords need permission from the local planning authority in order to convert housing units into commercial space, making such conversions less likely in the short run.

the number of rental housing units offered on the market in each municipality. For identification, we again rely on the event study design and the propensity-score-weighted and trimmed difference-in-differences design.

Figure 6 displays the results. For the event study design in Panel *A*, we use the same specification as for Figure 3 above, restricting the sample to a 12-month window around the introduction of the rent cap at the local level, to achieve comparability. There is no clear pattern in the graph, suggesting that the supply of rent-controlled units was not affected shortly after the introduction of the rent cap. In Panel *B*, we regress the number of rental units offered by municipality and quarter, on municipality and quarter fixed effects and an interaction of the quarter fixed effects with the treatment status, while allowing for State-specific trends. This graph also suggests that the number of units offered on the market remained stable after the rent cap became effective, lending further support to the view that the misallocation mechanism is the main driver behind the observed spillovers.

Figure 6: Effects of the rent cap on the number of regulated rental housing units on the market



Notes: Panel *A* displays the coefficients of the 'event time' indicators, with the month before the introduction of the rent cap at the local level as the baseline (analogous to the definition in equation (6)). The sample is restricted the same way as for Figure 3 above. Panel *B* displays the coefficients of the interaction terms of the treatment status with quarter dummies, with the number of rental units on the market relative to Q2/2013 as the dependent variable. The vertical bars indicate cluster-robust 90% confidence intervals. Both regressions include municipality-FE and control State-specific trends, as well as time-FE (Panel *A*: months; Panel *B*: quarters).

4.3.2. Demolitions of residential buildings and new supply

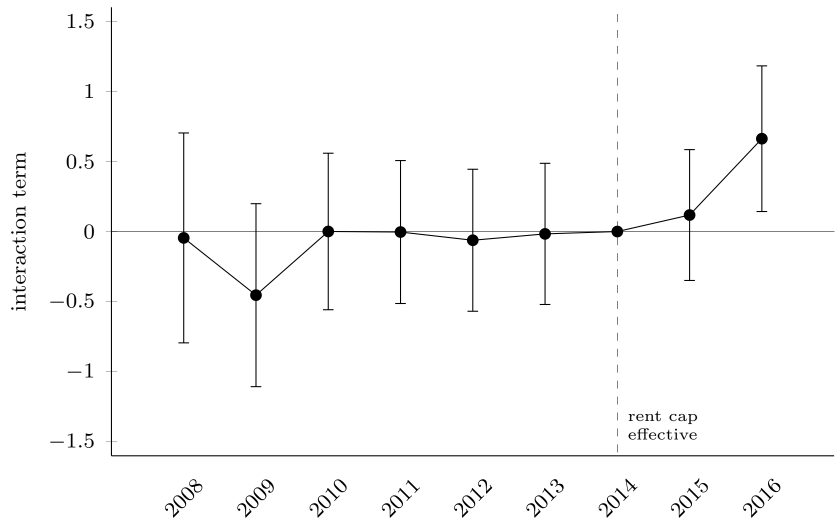
The rent cap reduces returns to investments in existing rental housing. At the same time, the spillover makes new units more profitable. *Ceteris paribus*, these two aspects should, at the margin, increase the likelihood that old buildings ripe for destruction are demolished in order to make room for a new residential building. To investigate this channel, we draw on the administrative Demolition and Conversion Statistic (DCS) for 2008–2016. It contains all demolitions in German municipalities recorded by the building authorities. The DCS is provided by the Statistical Offices of the German Länder. In the sample period, 64,529 (91,547) residential (non-residential) buildings were demolished fully or partly in Germany. 35,346 of the lost residential buildings were demolished to make room for new residential buildings, in 3,996 distinct municipalities. From here on, we refer only to these demolished residential buildings. We focus on this group because it is of particular interest in the present case, and because no change in local land-use regulation is required in order to rebuild a residential building. In the regressions, we restrict the sample to municipalities where at least one demolished building was reported between 2008 and 2016.

We also plot the trends in treatment and control groups for different building sizes in Figure OD3. When considering all buildings (Panel *A*), the trends are comparable before the treatment year (which we set to 2014, as before), although the two lines deviate somewhat shortly before the treatment date. After the treatment date, there are considerably more demolitions in rent cap municipalities than in the control group. Panels *B*–*D* show that this is entirely due to single-family homes (detached and semi-detached).

We investigate further the effect on small buildings in several regressions. First, we regress the number of demolished units per municipality and year, on municipality fixed effects, and a full set of year-treatment group interactions, using the propensity-score weighted and trimmed sample. Standard errors are clustered on the municipality level. Figure 7 plots the interaction terms along with 90% confidence intervals. The year 2016 coefficient is significant (relative to 2014), and the pre-trends are almost perfectly aligned.

In Table 5, we present results based on just two indicators for 2015 and 2016, using the full pre-treatment period as baseline. Without weighting (column 1 of Table 5), about 0.71 additional small buildings were demolished in treated municipalities in 2016 (relative to the control group). There is no significant effect in 2015. Both coefficients are very stable when employing propensity-score-weighting in column (2).

Figure 7: Event study design: demolitions of small residential buildings



Notes: The figure is based on a regression of the number of demolished buildings on year and municipality fixed effects, and an interaction of the year fixed effects with a treatment group indicator. It displays the year interaction × treatment group terms and 90% confidence intervals based on municipality-clustered standard errors. The sample is restricted to the propensity-score-trimmed sample (see Appendix Section C).

Overall, these results suggest that developers reacted to the rent cap by demolishing more small buildings than they would have done otherwise. In the trimmed sample, about three small buildings were demolished, on average, in a rent cap-municipality in 2014. Relative to this number, the effect is large. However, the total mean (median) housing stock in 2011 was roughly 11,100 (3,900) buildings in these municipalities. This means that the negative short-run effects on the supply of housing through demolitions are small. Because single- and two-family homes are typically owner-occupied, the rental stock is even less affected.

On the other hand, these results represent another piece of evidence consistent with the positive spillover effect on free-market rents.[24] They could be a first sign of positive supply effects resulting from the market segmentation.

In the supplemental material, we provide further results on new housing supply. We consider housing completions in rent regulated municipalities and a control group between 2008 and 2017, but find no evidence in the data that new housing supply was

[24] This is also consistent with positive effects of the rent cap on land values of buildable land, as documented in Mense et al. (2019).

Table 5: Effects on demolitions

Dependent variable:	Demolished (semi-)detached SFH	
	(1) OLS	(2) OLS
year 2015 × rent cap	0.20	0.20
	(0.26)	(0.29)
year 2016 × rent cap	0.71**	0.75*
	(0.27)	(0.30)
Observations	4,716	4,716
municipalities	524	524
rent cap municipalities	181	181
weighting	no	yes
trimming	yes	yes
Municipality FE	yes	yes
Year FE	yes	yes

Notes: The sample is restricted to the propensity-score trimmed sample, see Appendix Section C. Municipality-clustered standard errors in parentheses, $^{***}: p < .001,^{**}: p < .01,^{*}: p < .05$.

affected by the regulation. Due to planning and construction lags, it seems likely that there was just not enough time for new housing supply to react to the rent regulation.

5. Conclusions

Rent controls are still subject to intense debates among scholars and policy makers. We add to this debate by providing causal empirical evidence for the short-run effects of rent controls. Results based on an event study design and a difference-in-differences strategy, show consistently that a differentiated, second-generation rent control regime reduces rents in the controlled sector, but triggers rent increases on the free market. Additionally, we document that the the artificially lowered rents led to significant increases of housing service consumption of renters moving into regulated units. This indicates misallocation, which indirectly pushes up demand on the free market—hence the spillover on free-market rents. The spillover brings forward demolitions of old, ramshackle buildings. However, we were not able to document additional (or less) construction activity in the short run, potentially a consequence of the time lag between demolition and new construction. The empirical findings are robust to various alternative specifications.

The theoretical model suggests that, absent a short-run supply effect of rent control, the observed spillover between the regulated and unregulated markets indicates misallocation of households to housing units. Rent control allows households with a lower willingness to pay compared to the equilibrium rent in a free market setting to enter the market, thereby crowding out households with higher willingness to pay. These latter households bid up rents in the free market. An opposite-sign spillover from the regulated to the free segment of the housing market can thus be interpreted as an indication of misallocation. More generally the theoretical model and the empirical test strategies are also applicable to other price controls, such as pharmaceuticals (where alternatives to a price-controlled drug may exists[25]), or minimum wages (with reversed signs[26]).

While economists frequently mention misallocation of housing as an important argument against rent control, we are the first to provide causal empirical evidence for this type of demand response. Our results also shed light on competing interpretations of misallocation: In our interpretation, rent control benefits households with a low willingness to pay for housing by crowding out groups that have a higher willingness to pay. If willingness to pay and income are correlated, this implies that a millionaire with very high willingness to pay living in a rent-controlled unit does not represent misallocation. Thus, misallocation arising from rent control does not necessarily benefit high-income households, as suggested, for instance, in Glaeser and Luttmer (2003).

This study has strong implications for policy makers and housing market economists. In the short-run, rent controls primarily benefit low income groups from *within* regulated markets by lowering rents, which allows local renters to increase their housing consumption. This might explain why such policies are popular among policy makers in urban areas. However, these potential benefits come at a substantial cost: Welfare losses arise due to misallocation of households to housing units. The misallocation cost adds to the negative long-run impact on new construction and

[25] Many countries regulate the price of newly approved pharmaceutical drugs, see Kyle (2007).

[26] On the labor market, misallocation may arise from workers willing to work for a wage between the initial free-market wage and the minimum wage, thereby crowding out workers with lower reservation wages. These latter workers would then compete on the informal market or become self-employed. Wages in these markets would decrease as a consequence of the increased labor supply.

maintenance documented in the literature.

References

Andersen, H. S. (1998). Motives for investments in housing rehabilitation among private landlords under rent control. *Housing Studies 13*(2), 177–200.

Arnott, R. (1995). Time for revisionism on rent control. *Journal of Economic Perspectives 9*(1), 99–120.

Arnott, R. and M. Igarashi (2000). Rent control, mismatch costs and search efficiency. *Regional Science and Urban Economics 30*(3), 249–288.

Ault, R. and R. Saba (1990). The economic effects of long-term rent control: The case of New York City. *The Journal of Real Estate Finance and Economics 3*(1), 25–41.

Autor, D. H., C. J. Palmer, and P. A. Pathak (2014). Housing market spillovers: Evidence from the end of rent control in cambridge, massachusetts. *Journal of Political Economy 122*(3), 661–717.

Autor, D. H., C. J. Palmer, and P. A. Pathak (2017). Gentrification and the Amenity Value of Crime Reductions: Evidence from Rent Deregulation. Working Paper No. 23914, NBER.

Caliendo, M. and S. Kopeinig (2008). Some Practical Guidance for the Implementation of Propensity Score Matching. *Journal of Economic Surveys 22*(1), 31–72.

Card, D. and A. B. Krueger (1994). Minimum wages and employment: A case study of the fast-food industry in new jersey and pennsylvania. *American Economic Review 84*, 772–793.

Cheshire, P. C. and S. Sheppard (1995). On the price of land and the value of amenities. *Economica 62*(246), 247–267.

Davis, L. W. and L. Kilian (2011). The allocative cost of price ceilings in the US residential market for natural gas. *Journal of Political Economy 119*(2), 212–241.

Diamond, R., T. McQuade, and F. Qian (2019). The effects of rent control expansion on tenants, landlords, and inequality: Evidence from san francisco. *American Economic Review 109*(9), 3365–94.

Dube, A., T. W. Lester, and M. Reich (2010). Minimum wage effects across state borders: Estimates using contiguous counties. *The Review of Economics and Statistics 92*(4), 945–964.

Dube, A., T. W. Lester, and M. Reich (2016). Minimum Wage Shocks, Employment Flows, and Labor Market Frictions. *Journal of Labor Economics 34*(3), 663–704.

Early, D. and J. Phelps (1999). Rent regulations' pricing effect in the uncontrolled sector: An empirical investigation. *Journal of Housing Research 10*(2), 267–285.

Early, D. W. (2000). Rent control, rental housing supply, and the distribution of tenant benefits. *Journal of Urban Economics 48*(2), 185–204.

Fallis, G. and L. B. Smith (1984). Uncontrolled prices in a controlled market: The case of rent controls. *The American Economic Review 74*(1), 193–200.

Fallis, G. and L. B. Smith (1985). Price effects of rent control on controlled and uncontrolled rental housing in Toronto: A hedonic index approach. *The Canadian Journal of Economics / Revue canadienne d'Economique 18*(3), 652–659.

Federal Statistical Office (2013). *Wirtschaftsrechnungen: Einkommens- und Verbrauchsstichprobe – Wohnverhältnisse privater Haushalte*. Fachserie 15, Sonderheft 1. Wiesbaden: Federal Statistical Office.

Glaeser, E. L. (2003). Does rent control reduce segregation? *Swedish Economic Policy Review 10*, 179–202.

Glaeser, E. L. and E. F. Luttmer (2003). The misallocation of housing under rent control. *American Economic Review 93*, 1027–1046.

Hausman, C. and D. S. Rapson (2017). Regression discontinuity in time: Considerations for empirical applications. Technical report, National Bureau of Economic Research Working Paper 23602.

Kelly, L. (2015). Renters, first-time buyers and owners — how will the election affect you? *The Guardian* (April 17, 2015).

Knaup, H., A. Neubacher, and A.-K. Nezik (2013). Squeezed out: Rocketing rents become election issue in Germany. *Spiegel Online International* (January 2, 2013).

Knight, J. R. (2002). Listing price, time on market, and ultimate selling price: Causes and effects of listing price changes. *Real Estate Economics 30*(2), 213–237.

Knight, J. R., C. Sirmans, and G. K. Turnbull (1994). List price signaling and buyer behavior in the housing market. *The Journal of Real Estate Finance and Economics 9*(3), 177–192.

Kutty, N. K. (1996). The impact of rent control on housing maintenance: A dynamic analysis incorporating European and North American rent regulations. *Housing Studies 11*(1), 69–88.

Kyle, M. K. (2007). Pharmaceutical Price Controls and Entry Strategies. *The Review of Economics and Statistics 89*(1), 88–99.

Linneman, P. (1987). The effect of rent control on the distribution of income among New York City renters. *Journal of Urban Economics 22*(1), 14–34.

Malpezzi, S. (2003). Hedonic pricing models: A selective and applied review. In T. O. Sullivan and K. Gibbs (Eds.), *Housing Economics: Essays in Honour of Duncan Maclennan*. Blackwell.

Marks, D. (1984). The effect of rent control on the price of rental housing: an hedonic approach. *Land Economics 60*(1), 81–94.

McDonald, J. F. and D. P. McMillen (2010). *Urban economics and real estate: theory and policy*. John Wiley & Sons.

McFarlane, A. (2003). Rent stabilization and the long-run supply of housing. *Regional Science and Urban Economics 33*(3), 305–333.

Mense, A., C. Michelsen, and K. Kholodilin (2019). The effects of second-generation rent control on land values. *American Economic Association Papers and Proceedings 109*.

Merlo, A. and F. Ortalo-Magné (2004). Bargaining over residential real estate: evidence from England. *Journal of Urban Economics 56*(2), 192–216.

Monras, J. (2019). Minimum Wages and SpatialEquilibrium: Theory and Evidence. *Journal of Labor Economics 37*(3), 853–904.

Moon, C.-G. and J. G. Stotsky (1993). The effect of rent control on housing quality change: A longitudinal analysis. *Journal of Political Economy*, 1114–1148.

Nagy, J. (1997). Do vacancy decontrol provisions undo rent control? *Journal of Urban Economics 42*(1), 64–78.

Olsen, E. (1988a). Economics of rent control. *Journal of Real Estate Finance and Economics 28*, 673–678.

Olsen, E. O. (1988b). What do economists know about the effect of rent control on housing maintenance? *The Journal of Real Estate Finance and Economics 1*(3), 295–307.

O'Sullivan, A. and R. D. Irwin (2007). *Urban Economics*. McGraw-Hill/Irwin.

Schmidheiny, K. and S. Siegloch (2020). On event studies and distributed-lags in two-way fixed effects models: Identification, equivalence, and generalization. ZEW Discussion Paper 20-017.

Sims, D. P. (2007). Out of control: What can we learn from the end of Massachusetts rent control? *Journal of Urban Economics 61*, 129–151.

Skak, M. and G. Bloze (2013). Rent control and misallocation. *Urban Studies 50*(10), 1988–2005.

Smith, L. B. (1988). An economic assessment of rent controls: The Ontario experience. *Journal of Real Estate Finance and Economics 1*, 217–231.

Stewart, M. B. (2004). The employment effects of the national minimum wage. *The Economic Journal 114*(494), C110–C116.

Turner, B. and S. Malpezzi (2003). A review of empirical evidence on the costs and benefits of rent control. *Swedish Economic Policy Review 10*, 11–56.

Von Hoffman, A. (2000). A study in contradictions: The origins and legacy of the housing act of 1949. *Housing policy debate 11*(2), 299–326.

Wang, S.-Y. (2011). State Misallocation and Housing Prices: Theory and Evidence from China. *American Economic Review 101*(5), 2081–2107.

Wilson, W. (2017). Private rented housing: the rent control debate. Technical report, Commons Library Briefing, 3 April 2017.

Appendix

A. Rent data

Data sources and data quality. The rent and house price data used in this study are advertised rents and prices for dwellings from three large online real estate market places: *Immonet*, *Immowelt*, and *Immobilienscout24*. There can be significant differences between the transaction price and a first offer, but the literature points out that systematic mis-pricing can be very costly for sellers of real estate (Knight et al., 1994; Knight, 2002; Merlo and Ortalo-Magné, 2004). It is thus unlikely that the dynamics of offered rents deviates systematically from the dynamics of concluded rents over an extended period.

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

In addition to the characteristics of the dwellings, coordinates from the map snapshots that are displayed with every offer on the real estate marketing platforms were used to map observations to regulated and unregulated regions. Wherever the exact address is given, the coordinates refer to this address. In those cases where the address is hidden, these refer to the centroid of a neighborhood the dwelling is located in. Although there is the possibility that individual observations are misclassified as being subject (not being subject) to rent control, we believe that this probability is very small: Often, municipal boundaries are also postal code boundaries and the neighborhoods used by the online real estate/rents marketplaces usually are contained in a single municipality.

We added population density on the level of postcodes, based on the latest census (2011) and aggregated from grid-level population data provided by the Federal Statistical Office.

Table A1 contains summary statistics for the sub-samples used in the regressions discussed in Section 4.1.

Table A1: Variable means for regulated and unregulated units in the rental housing data

	Unregulated				Regulated			
Building age	zero				3+			
	all rent cap	all r.c. 2015/16	trimmed rent cap	trimmed control	all rent cap	all r.c. 2015/16	trimmed rent cap	trimmed control
monthly net rent	1,035.92	1,034.39	981.37	808.13	656.12	676.39	604.13	456.96
living area (m^2)	85.24	81.58	85.88	86.91	71.49	69.44	71.26	68.67
avg. room size (m^2)	31.15	30.62	31.24	30.31	29.22	29.07	29.05	26.41
# rooms	2.78	2.71	2.79	2.92	2.52	2.47	2.53	2.69
year of construction	2007.19	2002.96	2006.38	2008.87	1967.47	1967.21	1965.61	1967.70
building age	0.00	0.00	0.00	0.00	42.57	43.63	44.77	43.15
floor	1.55	1.59	1.53	1.05	1.64	1.48	1.66	1.22
floor is NA	0.27	0.27	0.31	0.32	0.29	0.36	0.30	0.35
elevator	0.62	0.59	0.60	0.49	0.25	0.26	0.23	0.11
second bathroom	0.36	0.34	0.35	0.38	0.16	0.15	0.14	0.14
garden use	0.20	0.20	0.20	0.21	0.13	0.13	0.12	0.12
built-in kitchen	0.43	0.46	0.48	0.29	0.38	0.42	0.38	0.23
floor heating	0.44	0.51	0.41	0.43	0.06	0.08	0.05	0.04
self cont'd heating	0.04	0.05	0.04	0.04	0.08	0.09	0.08	0.08
central heating	0.51	0.40	0.52	0.50	0.61	0.60	0.60	0.57
quality: luxury	0.09	0.10	0.08	0.08	0.01	0.01	0.01	0.00
quality: high	0.43	0.48	0.39	0.46	0.12	0.12	0.09	0.09
quality: low	0.00	0.00	0.00	0.00	0.01	0.01	0.01	0.00
type: roof storey	0.10	0.10	0.11	0.11	0.09	0.09	0.09	0.11
type: ground floor	0.15	0.16	0.14	0.20	0.12	0.12	0.12	0.14
type: souterrain	0.01	0.01	0.00	0.01	0.01	0.01	0.01	0.01
type: maisonette	0.04	0.04	0.05	0.05	0.04	0.03	0.03	0.03
type: NA	0.10	0.07	0.09	0.10	0.11	0.10	0.11	0.12
parquet flooring	0.08	0.08	0.07	0.04	0.03	0.04	0.03	0.01
air conditioning	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
parking available	0.66	0.66	0.60	0.76	0.37	0.39	0.32	0.34
balcony or terrace	0.74	0.78	0.73	0.74	0.63	0.66	0.63	0.58
Observations	71,038	33,069	40,355	18,530	594,408	262,590	392,763	303,076

Notes: The missing categories of the quality and type variables are 'average quality' and 'regular type'. 'Trimmed' refers to the propensity-score trimmed sample used in Section 4.1.3, where we construct more comparable treatment and control groups of municipalities with and without a rent cap, by using propensity score weighting and trimming.

B. Event study design: Additional table

Table B2: Covariate results for Figure 3, Panel *A*

Dependent variable:	Log rent			
	(1) Not subject to rent cap (first use, building age zero)		(2) Subject to rent cap (building age 3+ years)	
	Coef.	SE	Coef.	SE
log area	0.562052***	(0.030878)	0.497868***	(0.017404)
log area × pop. density	0.000002*	(0.000001)	0.000006***	(0.000001)
average room size	0.005291***	(0.000927)	0.002916***	(0.000404)
# rooms	0.119295***	(0.011980)	0.120796***	(0.006304)
constructed 1800-1918	0.003132	(0.011936)	-0.080225***	(0.014725)
constructed 1919-1945	-0.026758*	(0.010884)	-0.146236***	(0.014922)
constructed 1946-1965	-0.078502***	(0.007533)	-0.181821***	(0.014838)
constructed 1966-1975	-0.107476***	(0.012179)	-0.185521***	(0.014987)
constructed 1976-1985	-0.082373***	(0.009811)	-0.180004***	(0.016811)
constructed 1986-1990	-0.074641*	(0.035153)	-0.154879***	(0.012416)
constructed 1991-2000	-0.059214***	(0.015033)	-0.128469***	(0.011231)
constructed 2001-2005	-0.006943	(0.029330)	-0.065917***	(0.010265)
constructed 2006-2010	-0.007632	(0.025325)	-0.029540***	(0.005856)
floor	0.002888	(0.001682)	-0.000224	(0.000814)
floor is NA	0.025276***	(0.003533)	0.020892***	(0.004707)
elevator	0.019592***	(0.005499)	0.001384	(0.003842)
floor × elevator	0.008225***	(0.002425)	0.001796*	(0.000843)
second bathroom	0.015319***	(0.004579)	0.046311***	(0.004098)
garden use	0.014103	(0.010176)	0.019214***	(0.003738)
built-in kitchen	0.027989***	(0.004922)	0.067503***	(0.007993)
floor heating	0.040091***	(0.007012)	0.053718***	(0.005056)
self cont'd heating	-0.037650***	(0.009271)	-0.027567***	(0.005356)
central heating	-0.006154	(0.003335)	-0.009936**	(0.003658)
quality: luxury	0.074616***	(0.010715)	0.155429***	(0.006678)
quality: high	0.014519*	(0.006493)	0.041712***	(0.003987)
quality: low	0.111795	(0.079066)	-0.103376***	(0.007074)
type: top story	0.011583	(0.007473)	0.001441	(0.007349)
type: ground floor	-0.013161*	(0.006043)	-0.015955***	(0.003874)
type: souterrain	-0.098442***	(0.018424)	-0.096165***	(0.006811)
type: maisonette	0.033412	(0.018308)	0.032435***	(0.004023)
type: NA	-0.001069	(0.006035)	0.024807	(0.019048)
parquet flooring	0.038733***	(0.008212)	0.065734***	(0.005787)
air conditioning	0.033995	(0.049483)	0.095120***	(0.018085)
parking available	0.022367**	(0.007320)	0.041448***	(0.003948)
balcony or terrace	0.023311***	(0.004163)	0.024890***	(0.002364)
Postcode fixed effects	yes		yes	
Month fixed effects	yes		yes	
Observations	26,782		229,689	
Adj. R^2	0.879		0.849	

Notes: Event study dummies omitted; they are displayed in Figure 3. The omitted categories are buildings constructed after 2010, average quality, regular dwelling type. The sample is restricted as in Panel *A* of Figure 3 (+/- 12 months around the treatment date). Municipality-clustered standard errors in parentheses, $^{***}: p < .001, ^{**}: p < .01, ^{*}: p < .05$.

C. Propensity score model

The propensity score model relies on measures of local housing demand and supply growth, (urban) density, and the political economy. The sample is restricted to municipalities from federal states that adopted the rent cap, giving us 8664 valid cases and 294 rent cap municipalities. The variables capturing local housing demand and supply are the growth rates of rents and population between 2011 and 2013, the change in land values between 1995–97 and 2010–12, the number of new housing units per capita prior to the introduction of the rent cap (2008-2013), the vacancy rate (2011), and the number of unemployed per capita (2011). Since the rent cap is designed to make renters of existing units better off (relative to owners and renters of new units), we expect that a low homeownership rate makes adoption more likely. We capture density by the residential area share, population density, and the average number of housing units per residential building (all in 2011).[27] The model is specified as a logistic regression.

Table C3: Logit model: German rent cap municipalities

Dependent variable: rent cap municipality dummy	Coef.	robust SE
population growth rate, 2011–2013	10.046	(5.809)
median rent per m^2, growth rate, 2011–2013	2.006*	(0.803)
new housing units per capita, 2008–2013	167.651***	(31.014)
avg. land price 2010–2012 (district-level)	0.009***	(0.001)
change of land price, (1995–1997) vs (2010–2012)	0.005*	(0.002)
unemployed per resident	-47.913***	(11.501)
homeownership rate	-11.031***	(1.915)
vacancy rate	-40.028***	(6.313)
units per residential building	0.366	(0.409)
population density	-0.122*	(0.053)
residential area share	7.348***	(1.861)
dummy: population growth rate NA	14.553***	(2.256)
dummy: rent growth rate NA	-0.752**	(0.270)
dummy: land prices NA	0.576	(0.444)
dummy: unemployed per resident NA	-17.221***	(1.125)
dummy: homeownership rate NA	-21.080***	(1.266)
dummy: vacancy rate NA	-23.055***	(2.558)
dummy: units per residential building NA	-12.458***	(0.890)

Notes: Unless noted otherwise, variables refer to the Census 2011. Robust standard errors in parentheses, $^{***}: p < .001, ^{**}: p < .01, ^{*}: p < .05$.

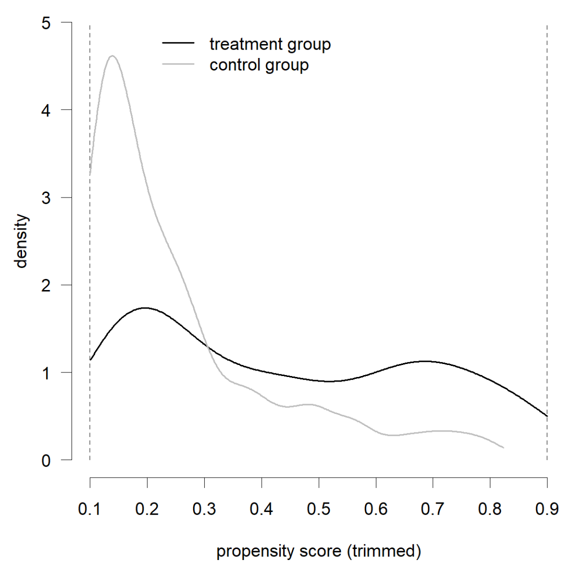
The estimation results are summarized in Table C3. A lower vacancy rate, higher and increasing land prices, higher rents, as well as a lower homeownership rate all

[27]We account for missings by adding NA dummies and setting missing values to zero.

make it more likely that the rent cap will be introduced in a municipality. Other important predictors are the share of unemployed per resident (with a negative sign) and the residential area share (positive sign). While the change in population is only marginally significant, new housing supply per capita is highly significant, suggesting that the rent cap was introduced in more active local housing markets.

Figure C1 displays kernel density estimates of predicted probabilities (capped at 10 and 90%), separately for the treatment and control groups. There is considerable overlap, with many treated units having low estimated probabilities, and a sizable share of control units that are classified as rent cap municipalities by the model. Thus, an important pre-requisite for the propensity score weighting and trimming approach is fulfilled.

Figure C1: Density of propensity scores from Table C3



Notes: The figure displays kernel density estimates of the predicted probabilities of belonging to the treatment group, trimmed at 0.1 and 0.9. The black (grey) line refers to municipalities with(out) rent control.

Our main analysis is based on a trimmed sample that restricts attention to municipalities with a predicted probability between 10 and 90%. Before turning to the main regressions, we assess the balance of the covariates used in the propensity score model, between treated and control municipalities in the full and trimmed samples. We report the 'standardized bias' criterion, see Caliendo and Kopeinig (2008). After

Table C4: Covariate balance of the trimmed and weighted treatment and control groups

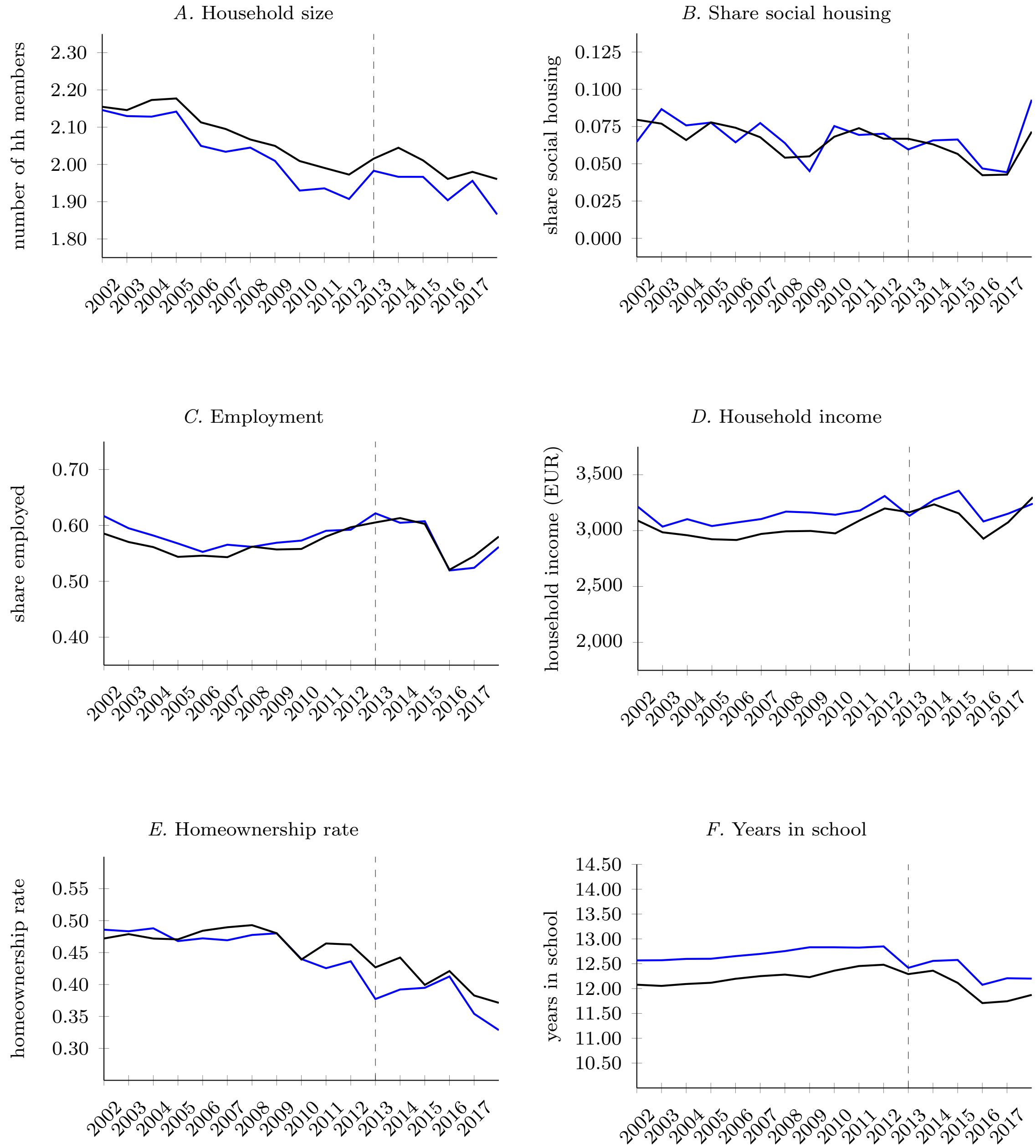
	Standardized bias		
	full	trimmed	trimmed +weighted
population growth rate, 2011–2013	0.960	0.129	0.129
median rent per m^2, growth rate, 2011–2013	0.174	0.002	0.030
new housing units per capita, 2008–2013	0.571	-0.010	-0.044
avg. land price 2010–2012 (district-level)	1.305	0.195	0.147
change of land price, (1995–1997) vs (2010–2012)	0.393	0.070	0.054
unemployed per resident	-0.088	-0.004	-0.029
homeownership rate	-1.523	-0.209	-0.170
vacancy rate	-0.920	-0.137	-0.144
units per residential building	1.286	0.286	0.252
population density	1.285	0.281	0.207
residential area share	1.328	0.173	0.109

Notes: The standardized bias variable X is the difference in means divided by $\sqrt{(\mathbb{V}(X_{\text{tr}}) + \mathbb{V}(X_{\text{tr}})/2}$ (values above 0.2 in bold). See Caliendo and Kopeinig (2008) for details.

trimming, the two groups are already much more balanced, especially with respect to the dynamics of the local housing market. They are less comparable with respect to the urban density (with a lower homeownership rate and higher density in the treatment group) — although trimming improves balance considerably also along these dimensions. Weighting further improves balance, but the difference is relatively subtle.

We assess further the balance of the trimmed samples with respect to pre-trends in important variables. This is arguably much more important than balance with respect to time-constant variables. Figure C2 displays trends of household size, the share of social housing, employment, household income, the homeownership rate, and years in school, based on GSOEP data. All six graphs show that the housing demand and labor market dynamics in the trimmed and weighted treatment and control groups are very similar prior to the election in 2013 and beyond. Figures 5 and 7, 4, and OD4 also suggest that the trimmed and weighted groups are comparable across a wide range of important variables prior to the introduction of the rent cap.

Figure C2: Pre-trends of potential confounders



Notes: The figures display trends in potential confounders in the trimmed and weighted sample. All variables are aggregated from the German Socio-Economic Panel, employing trimming and propensity score weighting.

D. Demand response: Additional tables

Table D5: Means of covariates for Figure 5 and Table 4: Income share spent on housing and housing service consumption

	Figure 5 weighted & trimmed		**Table 4** within-market variation	
	Mean	SD	Mean	SD
rent to income ratio	0.279	(0.126)	0.296	(0.149)
No. of rooms	—		3.007	(1.197)
Δ No. of rooms	—		-0.323	(1.824)
income	2,318.48	(1,394.15)	2,361.99	(1,498.86)
employment status change	0.179	(0.384)	0.183	(0.387)
building age	43.284	(24.58)	40.862	(24.804)
change household size	-0.265	(0.861)	-0.299	(0.889)
2003	—		0.069	(0.254)
2004	0.058	(0.234)	0.060	(0.238)
2005	0.081	(0.273)	0.085	(0.278)
2006	0.056	(0.229)	0.064	(0.245)
2007	0.078	(0.267)	0.064	(0.245)
2008	0.050	(0.217)	0.060	(0.238)
2009	0.061	(0.239)	0.047	(0.211)
2010	0.045	(0.207)	0.047	(0.211)
2011	0.039	(0.193)	0.048	(0.214)
2012	0.074	(0.263)	0.076	(0.265)
2013	0.081	(0.273)	0.077	(0.267)
2014	0.071	(0.257)	—	
2015	0.079	(0.270)	0.070	(0.256)
2016	0.049	(0.217)	0.054	(0.226)
2017	0.049	(0.216)	0.054	(0.226)
2018	0.061	(0.240)	—	
rent cap	0.459	(0.498)	—	
population 2000-5000	0.008	(0.089)	0.004	(0.063)
population 5000-10000	0.077	(0.266)	0.068	(0.252)
population 10000-50000	0.160	(0.367)	0.117	(0.321)
population 50000-100000	0.161	(0.367)	0.118	(0.323)
population 100000-500000	0.367	(0.482)	0.460	(0.498)
population $>$ 500000	0.225	(0.417)	0.229	(0.420)
Hamburg	0.091	(0.288)	0.164	(0.370)
Bremen	0.039	(0.193)	—	
Northrhine-Westfalia	0.365	(0.481)	0.226	(0.419)
Hesse	0.074	(0.261)	0.056	(0.230)
Phineland-Palatinite	0.055	(0.229)	0.060	(0.237)
Baden-Wuerttemberg	0.130	(0.336)	0.162	(0.368)
Bavaria	0.112	(0.315)	0.139	(0.347)
Brandenburg	0.023	(0.149)	0.234	(0.151)
Thuringia	0.033	(0.179)	0.053	(0.224)
Observations		2,106		981

Notes: Source: GSOEP; standard deviations in parentheses. The table displays summary statistics for the samples used in Figure 5 and Table 4.

Table D6: Coefficients of control variables for Figure 5: Income share spent on housing services

Dependent variable:	Income share spent on housing
Rent cap	0.037
	(0.02)
Δ household size (L1)	-0.005
	(0.01)
Income (L1)	-0.000***
	(0.00)
Job status change (L1)	-0.021***
	(0.01)
Building age (L1)	-0.000**
	(0.00)
population 2,000-5,000	-0.021
	(0.05)
population 5,000-10,000	-0.023
	(0.02)
population 10,000-50,000	-0.001
	(0.02)
population 50,000-100,000	-0.020
	(0.02)
population 100,000-500,000	-0.007
	(0.02)
population $>$ 500,000	0.006
	(0.02)
Hamburg	-0.018
	(0.01)
Bremen	-0.041***
	(0.01)
Northrhine-Westfalia	-0.028***
	(0.01)
Hesse	0.012
	(0.01)
Rhineland-Palatinate	-0.014
	(0.01)
Baden-Wuerttemberg	-0.002
	(0.01)
Bavaria	-0.008
	(0.01)
Brandenburg	-0.043*
	(0.02)
Thuringiga	-0.049***
	(0.01)
Year fixed effects	yes
Observations	2,106
R^2	0.074

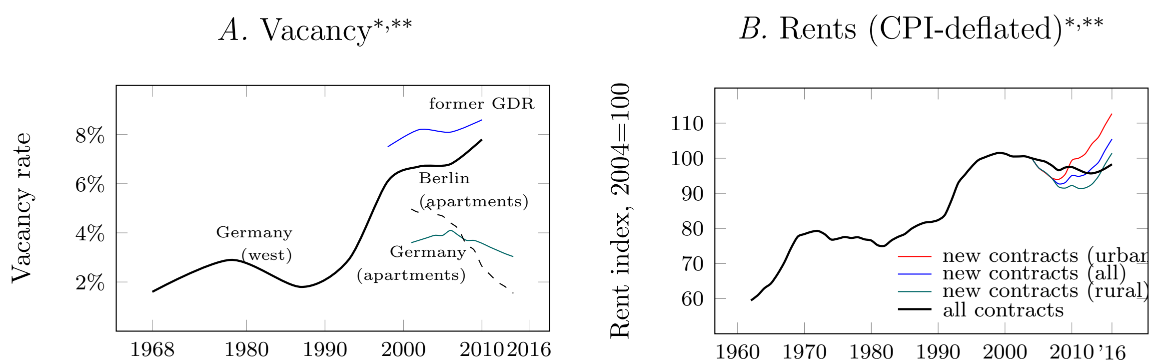
Notes: Municipality-clustered standard errors in parentheses; $^{***}: p < .001, ^{**}: p < .01, ^{*}: p < .05$.

For Online Publication

OA. The German rental housing market

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

Figure OA1: Rents and vacancy rates in Germany



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

Between 1995 and 2010, the German housing market was relaxed. Low birth rates, outmigration from city centers to the periphery, and high construction activity in the 1990s contributed to this development. However, since 2010, urban agglomerations have become more attractive. Thanks to an inflow of migrants from smaller cities and from abroad, the population of large German cities began to expand quickly. The result was a housing shortage, particularly putting pressure on rents for new contracts (see Figure OA1).

After 15 years of stagnation, rents started to increase rapidly, while vacancy rates fell, particularly in the urban housing stock. In 2016, rents were on average 23%

above the level observed in 2010, in urban areas about 27%. However, according to the Federal Statistical Office, tenant mobility is quite low: the length of a rental contract exceeds 10 years on average. Therefore, rents across all contracts (new and current) increased only slightly (see Figure OA1) since 2010.

OB. Details on the German rent control regime

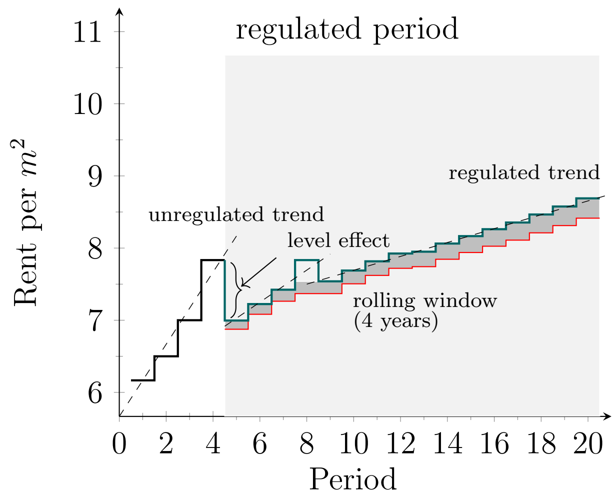
OB.1. Rental market regulations in Germany

Rent controls in Germany have a long history. First introduced in the early 1920s, many regulations, often rudimentary, were in place for decades. *First-generation* rent controls were introduced after World War I in response to the huge housing shortage and were kept in place until the late 1960s. These rules were meant to be temporary. Rents for existing dwellings were frozen at the levels of 1914. However, with the exception of a short period of loosened regulation in the early 1930s, rents were frozen until the late 1960s. In some major cities—like Berlin, Hamburg, and Munich—rents were frozen even longer. A second-generation "tenancy rent control-decontrol" regime was introduced in 1972. The regime allowed landlords to set rents freely when taking in a new tenant, but rent changes in existing contracts were regulated. The rules for admissible rent changes and the rights of tenants as well as protection from evictions changed over time, but the basic regime is still in place today. The new regime introduced in 2015 adds to these existing rules.

OB.2. The specific mechanism of the German rent cap

To illustrate how the rent cap affects the development of rents in new contracts, consider a situation with four representative rental contracts that are consecutively signed in periods $t = 1...4$. Each contract is concluded for four periods. Further, we assume an increasing trend of rents in new rental contracts in the periods prior to the introduction of the regulation. In period $t = 5$, the rent cap is imposed and rents in all subsequently concluded contracts are capped by the *local reference rent*, which is calculated as an average of the contracts concluded in the previous four periods plus 10%, unless the dwelling was rented out at a higher rate before. In this case, landlords are allowed to charge a rent equal to the rent in the past contract. The *local reference rent* is adjusted in each period.

Figure OB2: Stylized representation of the effects of the rent cap over time



Source: own representation.

In our example, the rents concluded in the first, second, and third periods are below the average of the first four periods and are adjusted to the legal upper bound in periods five to seven. However, as the rent in the contract signed in period four was already above the rent cap level, the landlord is allowed to charge this rent in all subsequent contracts. While the introduction of the rent cap leads to an immediate drop of rents in the first regulated period in our example, it becomes obvious from the graphical representation that, in the short-run, there still might be considerable rent increases in year-to-year comparison. However, it also becomes apparent that the dynamics are clearly decelerated (as indicated by the dashed trend lines in Figure OB2). In the long-run, rent increases are tied to the dynamics of the *local reference rent*.

OC. Proof of the Proposition in Section 2.2

Consider the model described in Section Section 2.2. Let B be a set of households with a willingness to pay above $F^{-1}(1-\rho/N)$, i.e. $B \subseteq [F^{-1}(1-\rho/N), \infty)$. Clearly, $NF(B) \leq \rho$. Let $X = [r_0, F^{-1}(1-\rho/N))$ and let A be a set that satisfies $A \subseteq [\bar{r}, F^{-1}(1-\rho/N))$ and $F(A) = F(B)$. Households in A get to live in a rent controlled unit, replacing households from B. Formally, the set of renters in the controlled segment is $A \cup [F^{-1}(1-\rho/N), \infty) \setminus B$.

Define $C := A \cap X$ and $D := A \setminus X$ as the subsets of A that are above and below the initial market price r_0. There are two relevant cases, $F(D) > 0$ and $F(D) = 0$.

First, consider the case $F(D) = 0$. There is no household with willingness to pay below r_0 that replaces a household with willingness above r_0 under rent control. Clearly, there is no welfare loss from misallocation in this case, but there is redistribution from landlords of rent controlled units to their renters, and from renters of free market units to their landlords. $F(D) = 0$ implies $F(C) = F(A) = F(B)$, so that

$$1 - \rho = F(X \setminus C) + F(C) = F(X \setminus C) + F(B). \tag{9}$$

This means that

$$r_1 = \inf X \setminus C = \begin{cases} r^* > r_0 & \text{if } \exists \delta > r_0 \text{ s.t. } [r_0, \delta) \subseteq C \\ r_0 & \text{otherwise.} \end{cases} \tag{10}$$

The first case is not relevant in practice, because it implies that virtually all renters below δ get to live in a rent-controlled unit. Furthermore, the spillover would likely be small, as there are no good reasons why *all* households with a willingness to pay above r_0 and below r^* would end up in the rent-control segment of the market for larger r^*, while at the same time, there are other regions in X where no household lives in a rent-controlled unit, although their willingness to pay exceeds r^*. Hence, empirically, we would expect that $r_1 = r_0$ under no misallocation.

Now let $F(D) > 0$. Since $F(D) + F(C) = F(A) = F(B)$, we have

$$\frac{1 - \rho}{N} = F(X) = F(X \setminus C) + F(C) < F(X \setminus C) + F(B). \tag{11}$$

Hence, it follows that $r_1 > \inf X \setminus C \geq \inf X = r_0$. The strict inequality follows from the fact that competition on the free market ensures an allocation of dwellings to the highest bidders in $X \setminus C$, while there are not enough dwellings on the free market to accommodate all households in $X \setminus C$. In other words, if there are households without a dwelling whose willingness to pay exceeds r_0, rent control pushes up rents in the free segment of the market.

The welfare loss due to misallocation is

$$\Delta = N\left(\int_E \varepsilon dF - \int_D \varepsilon dF\right), \tag{12}$$

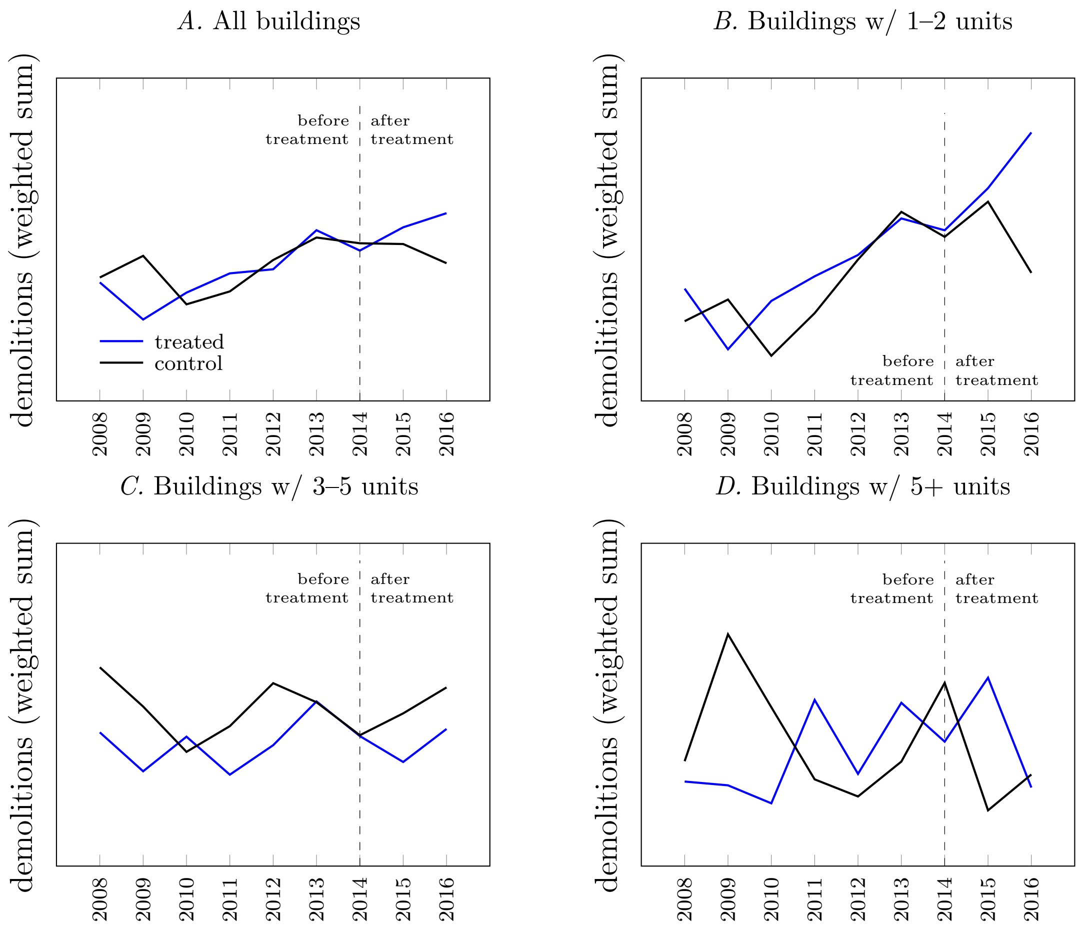
where $E = [r_0, r_1) \setminus C$, and it must hold that $F(E) = F(D)$. Households in D benefit since they received utility 0 prior to rent control but pay $\bar{r}$ under rent control to get utility $v > \bar{r}$. On the other hand, households in E had net utility $v - r_0 > 0$ prior to rent control, but get 0 afterwards. Other renters gain or lose as well, but these gains and losses are matched by equally sized losses and gains of landlords. Clearly, $\Delta < 0$, because $\sup D \leq \inf E$ and $\inf D < \sup E$.

For given r_0, $\bar{r}$, it is clear that $\int_E \varepsilon dF$ weakly increases with r_1, while $\int_D \varepsilon dF$ is constant. This implies that the welfare loss weakly increases in r_1. $\square$

OD. Supply response: additional results

OD.1. Demolitions of residential buildings

Figure OD3: Demolitions: Trends in propensity-score weighted and trimmed treatment and control groups



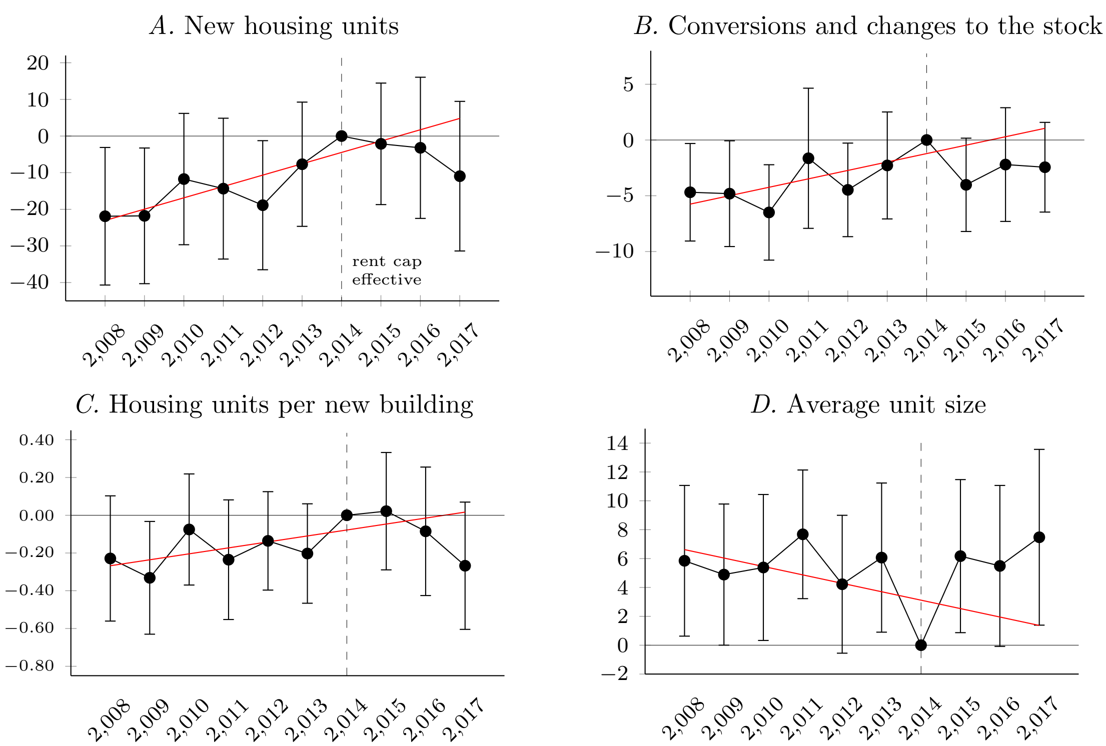
Source: Demolition and Conversion Statistic; own calculations based on the propensity-score weighted and trimmed samples.

OD.2. New housing supply

In case vacant, build-able sites are available, the positive effect on rents in newly constructed housing units should push up supply of new housing. To investigate this issue, we use the propensity score-weighted and -trimmed sample and administrative data on housing completions by municipality and year from 2008 to 2017. We consider the number of new housing units, new housing units from conversions and additions to existing buildings, the average number of housing units per new residential building, and the average unit size in new-builds.

In Figure OD4, we plot the coefficients of rent cap × year interaction terms for 2008 to 2017 for each of the four outcomes, conditional on year and municipality fixed effects. Some graphs reveal slight positive relative pre-trends between the two groups, as indicated by the red trend lines. Even if one takes into account the pre-trends, there are no significant effects on either outcome variable after the rent cap became effective. With regard to the interpretation of our main findings, there did not seem to be (negative) effects on overall housing supply in 2015 or 2016. At most, the graphs reveal a tendency of decreasing supply in 2017, with fewer, but larger units per building. One potential reason could be the ongoing public debate about stricter rent controls.

Figure OD4: New housing supply: Trends in propensity-score weighted and trimmed treatment and control groups



Notes: The four graphs display the year × treatment group interaction terms based on the propensity-score weighted and trimmed samples. The red lines depict the pre-trends (2008–2014) of the year × treatment group interaction terms (extrapolated to the years 2015 to 2017). The vertical bars represent cluster-robust 90% confidence intervals.