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Postal Address:
Institut d'Economia de Barcelona
Facultat d'Economia i Empresa
Universitat de Barcelona
C/ John M. Keynes, 1-11
(08034) Barcelona, Spain
Tel.: + 34 93 403 46 46
ieb@ub.edu
http://www.ieb.ub.edu

EFFECTIVENESS AND SUPPLY EFFECTS OF HIGH-COVERAGE RENT CONTROL POLICIES *

Jordi Jofre-Monseny, Rodrigo Martínez-Mazza, Mariona Segú

ABSTRACT: Concerns about housing affordability are widespread in cities worldwide, prompting discussions about rent control policies. This paper studies the effects of a rent control policy adopted in Catalonia in 2020 that applied to some but not all municipalities. The policy virtually covered all the rental market and forced ads and tenancy agreements to specify the applicable rent cap to ensure enforcement. To identify the causal effect of the rent control regulation on the rental market, we exploit register microdata of tenancy agreements and implement difference-in-differences regressions and event-study designs. Our results indicate that the regulation reduced average rents paid by about 4% to 6%. We do not find evidence of a reduction in the supply of rental units, as measured by the number of signed and ended agreements or the active stock of rental units. We implement several robustness tests to address identification concerns related to Covid-19. Our results suggest that rent control policies can effectively reduce rental prices without necessarily shrinking the rental market.

JEL Codes: R52, R31, H70

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Jordi Jofre-Monseny
Universitat de Barcelona & IEB
E-mail: jordi.jofre@ub.edu

Rodrigo Martínez-Mazza
University College London, Uppsala University & IEB
E-mail: rodrigo.martinez@ibf.uu.se

Mariona Segú
CY Cergy Paris Université
E-mail: mariona.segu@cyu.fr

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1 Introduction

Concerns related to housing affordability are widespread in cities around the world. These concerns have raised interest among citizens, policy-makers, and scholars for policies that aim at improving housing affordability in urban areas. One of the star measures of this debate is the adoption of rent control policies. More and more European cities, such as Paris and Berlin, have chosen this path and have recently adopted such policies. Despite being rather unpopular among economists, rent controls are unlikely to vanish from the political sphere because they are often popular among voters, and their adoption does not entail direct government expenditures.

From a theory perspective, the case for rent control policies is weak, as caps on rents can lead to housing supply shortages and misallocation of housing units (Glaeser and Luttmer, 2003). Yet, rent control policies can create net welfare gains (especially for low-income families) as they act as an insurance device in a context of incomplete markets and risk aversion (Favilukis et al., 2023). Given the salience of this policy, it is surprising that the empirical literature studying the effects of rent controls is relatively scarce.

In this paper, we study the effects of a rent control system in Catalonia introduced in the fall of 2020 that applied to some but not all municipalities. The regulation applied to municipalities exceeding 20,000 inhabitants with a tight rental market. In rent-controlled municipalities, rental prices had to be below a dwelling and area-specific nominal cap and could not exceed the previous rent of that housing unit. The policy covered the entire rental market virtually, with higher nominal caps for units built during the last five years. Ads and tenancy agreements had to include the applicable rent cap, and fines were stipulated to ensure further enforcement. The policy ended in March 2022, when it was declared unconstitutional.

This paper uses a novel administrative dataset containing the universe of tenancy agreements signed and ended in Catalonia between 2016 and 2022. In contrast to posted rental data used in earlier studies, the register data allow us to more accurately measure rent prices and perform an in-depth analysis of the rental market dynamics after a rent control regulation.

In order to identify the causal effect of the rent control regulation, we exploit the fact that only a subset of municipalities is subject to rent control. We aggregate the data at the municipality-quarter level and implement difference-in-differences regressions and event-study designs. In particular, we compare regulated municipalities to a group of non-regulated municipalities that also experienced a tight housing market but did not validate the population criteria. This allows us to compare two groups with similar rental market pre-trends. We examine changes in average rents, the number of tenancy agreements signed and ended, and the active stock of rental units in regulated vs. non-regulated municipalities.

Our findings indicate that rents decreased between 4% and 6% in regulated municipalities relative to non-regulated municipalities. In contrast, we do not find evidence that the regulation reduced the number of tenancy agreements signed, suggesting that supply shortages in the short run are not necessarily substantial. However, we identify an anticipation effect in the number of signed agreements, which increased two weeks before the start of the rent control. We further explore the supply effects of rent control by looking at the number of ended agreements and at the stock of rented units. Both outcomes seem unaffected by the rent control, confirming that the number of rented units did not shrink as a consequence of the rent regulation. Moreover, we do not find that the policy changed the quality of rented units, which minimizes the concerns that changes in the composition of units drive the estimated price effects. Finally, while sales prices are not affected by the rent control, the number of sales seems to decrease, although this result is less robust across specifications.

We undertake a comprehensive set of robustness checks. First, we check that our results are robust to alternative econometric specifications (including municipality-specific linear time trends) and alternative samples with smaller population differences between regulated and non-regulated municipalities. We also check that the results are robust to excluding touristic municipalities or including Barcelona in the sample.

We address potential spillover effects on non-regulated municipalities. One could expect a displacement effect, by which displaced demand increases rents in non-regulated neighboring municipalities, or a contagion effect, which could create a downward price pressure in non-regulated municipalities. When excluding neighboring non-regulated municipalities from our sample, the coefficient for rents becomes more negative, which is consistent with a contagion effect. We further explore spillovers by comparing non-regulated neighbor municipalities to non-regulated non-neighbor ones. We show that neighboring non-regulated municipalities experience a rent reduction half the size of that of regulated municipalities. In contrast, the number of tenancy agreements in neighboring municipalities is unaffected by rent control.

Second, we implement several strategies that address the potential confounding effect of Covid-19 on housing markets. First, in our baseline regression, we account for the dynamics of local labor markets by controlling for unemployment, Covid-19 furloughs and the number of new employment contracts. Second, we account for possible "donut" effects of Covid-19 on housing markets (Ramani and Bloom, 2021; Gupta et al., 2021) by introducing a Covid-19 dummy interacted with distance dummies to the CBD or, alternatively, the Covid-19 dummy interacted with municipality size dummies. Third, we include direct measures of in-migration and out-migration rates at the municipality level as control variables. All these empirical analyses suggest that the differential effects of Covid-19 on regulated and non-regulated markets do not drive our findings.

The end of the rent control in March 2022 provides an additional robustness exercise. We find that the price effects of the policy were approximately constant between the first and the last quarter of the regulation, yet, once the regulation stopped, the price difference between regulated and non-regulated municipalities moved back to pre-regulation levels.

The empirical literature on the effects of rent control is scarce. Sims (2007) and Autor et al. (2014) study the elimination of rent controls in Massachusetts in the mid-nineties. More recently, Diamond et al. (2019) analyzed the 1994 reform in San Francisco that extended the rent control regime to a segment of the market (buildings of four housing units or less built before 1980) that had been exempted from the regulation until that point. Mense et al. (2019), Mense et al. (2023) and Breidenbach et al. (2021) focus on the effects of the German Federal law of 2015 (not to be confused with the recently abolished municipal rent control law in Berlin) that controls rental prices in German municipalities with tight housing markets.

The literature is inconclusive concerning the effectiveness of rent control policies. Sims (2007) finds that, after deregulation, rents increased more in neighborhoods with a higher proportion of rent-controlled units. However, this effect might partially capture processes of gentrification triggered by the end of rent controls, as shown by Autor et al. (2014). In turn, Diamond et al. (2019) finds that the extension of rent control slowed down the displacement of low-income households, suggesting that rent controls were effective in shielding some low-income households from rent increases. For the German case, Mense et al. (2019) find that rent controls slowed down rental growth in treated municipalities, but the effect is small (around 3%). Breidenbach et al. (2021) explore the temporal dynamics of the policy and find an immediate effect of around 5%, which vanishes after one year, suggesting that rent controls are not effective in the medium run. Finally, Monràs et al. (2022) assess the same rent control policy as this paper and find that the policy led to a convergence in rents toward the reference price. They rationalize this finding with a model with search friction inefficiencies and find that tenant welfare depends on their preferences for low or high-price units.

One unintended effect of rent control policies is supply distortions. However, large responses in overall housing supply seem implausible in cities with tight housing markets. In fact, Sims (2007) and Mense et al. (2019) do not find that rent control policies affect new construction. However, there is evidence that rent control policies can displace housing units from the regulated to non-regulated markets. More specifically, there is evidence that rent control policies displace housing units from the rental market to the homeownership market (Sims, 2007; Diamond et al., 2019; Mense et al., 2019, 2023) or to segments of the rental markets where regulation does not apply, such as renovated units in Germany or condos in San Francisco.

We contribute to this literature by analyzing a rent control regulation that, unlike other policies studied, virtually covers the entire rental market. This feature of the policy might limit the supply responses caused by rent control, since landlords cannot displace units to non-regulated market segments by renovating units or converting units to condos. We show that such a policy does not necessarily reduce the size of the rental market, at least in the short run. Another characteristic of the rent control policy in Catalonia is that its enforcement is likely to be high compared to the German regulation. This is because the Catalan regulation includes fines for noncompliance (contrary to the German case), and awareness of the regulation should be high since ads and tenancy agreements must include the applicable rent cap. Our results indicate that rent control policies can be enforced and, thus, effective in reducing rent growth in cities.

The paper is structured as follows. Section 2 provides the institutional setting for the rent control in Catalonia, as well as a description of the rent control measures implemented. In section 3, we present the data used, while the empirical strategy is detailed in section 4. In section 5, we discuss the effects of rent control on rents and the number of tenancy agreements signed and the other housing market outcomes. In section 6, we adopt several strategies to address the potential confounding effects of Covid-19 on our results. Finally, in section 7, we discuss some implications of our findings.

2 The rent control system in Catalonia

Spain is a country of homeowners. In 2020, 75% of households were homeowners, while only 25% were renters. Most tenants rent in the private market, as social rental housing is very limited in Spain (3.3% of all households)[1]. Despite the low figures, the rental market share has considerably increased in recent years, since it only represented 14% of all households in 2004.

Rental affordability is a concern for many Spanish households. In fact, Spain rates among the worst OECD countries in housing affordability statistics (OECD, 2021). About 20% of tenant households are overburdened by housing costs, as they spend over 40% of their income on housing. Between 2016 and 2019, right before the rent control regulation, rents increased by 30% in those soon-to-be-treated municipalities.

The Spanish rental market is regulated by the Law of Urban Rentals of 2019, which establishes a minimum contract duration of 5 years[2]. There are no price restrictions between 5-year tenancy agreements, regardless of whether the agreement is new or renews an ended contract. The law only restricts annual rent changes within agreements, which can only be changed to reflect inflation. Prior to the 2019 law, the minimum contract duration was three years. This implies that in our study period, an ended contract had been typically signed three years before.

In September 2020, the Catalan parliament passed a rent control system to be applied in the region. The regulation, which was only applied to some municipalities, limited the rental price of 5-year rental contracts, and it was applied to both new agreements and renewals. The system was in place until March 2022 when it was declared unconstitutional by the Spanish Constitutional Court.

The regulation established a nominal rent cap and anchored the new rental price to that of the previous tenancy agreement if there was one. Nominal rent caps were computed as reference prices using unit characteristics and the tenancy agreements signed in the local area during the previous three years and were annually updated[3]. Therefore, the rental price of a new agreement was the minimum rent resulting from this two-part rule. For example, if a flat was previously rented at €700 and the nominal rent cap for that flat was €800, then the new rental price could not exceed €700. In contrast, a unit that enters the market for the first time is only subject to the nominal cap.

Notice that the rent cap is specific to the area and varies according to dwelling characteristics. Thus, a priory, we do not expect a large heterogeneity, either spatial nor by housing characteristics, in the extent to which the regulation constrained rental prices. For previously rented units, given the average annual increase in rents in the years prior to the regulation (which is 5.66% as can be seen in Table 1), we expect the previous rent to be binding more often than the nominal cap.

[1] Source: Housing Conditions Survey, INE.

[2] *Ley de Arrendamientos Urbanos* or *Real Decreto-ley 7/2019, de 1 de Marzo, de medidas urgentes en materia de vivienda y alquiler.*

[3] The study area had at least a 50 meters radius, with at least 25 observations in the sample. If within the first 50 meters, there were fewer than 25 observations, then the radius was increased by 50 meters up to a maximum of 1050 meters, until at least 25 observations were found. The average was calculated using units of similar size, using a 10 square meter margin. For example, for a 50 square meter flat in Barcelona city center, the index would reflect the average square meter price of rental units signed during the last three years, within a 50 meters radius and a surface between 40 and 60 square meters.

The regulation included several exceptions. First, if the housing unit had three out of eight specific amenities, the rental cap could be increased by 5%[4]. Second, units built during the last five years and those that had undergone a complete renovation had more favorable conditions and were subject to a substantially higher rent cap. Thirdly, if the landlord's income was below a given threshold and the tenant's income was above a certain level, the regulation of rents was relaxed. In practice, this last condition was hardly met. Lastly, units over 150 square meters are exempted from the rent control. These units are marginal in the rental market, representing only 0.59% of the agreements in our sample.

The law included several enforcement measures that regulated the advertisement for rentals and the content of tenancy agreements. Both rental ads and tenancy agreements had to contain the rental cap associated with the unit and, if applicable, the rent paid by the previous tenant. The penalties associated with non-compliance ranged between 9,000 and 90,000 euros.

The time window in which there might be anticipation effects is very short because the rent control legislation followed a rapid legislative procedure and the initiative was not salient until the first week of September[5]. The legislation was debated and passed on September 9, the first parliamentary session after the August summer break, and it became effective on September 21. Thus, anticipation effects should be restricted to this two-week period.

The law stated that the regulation applied to municipalities with over 20,000 inhabitants with a tight housing market. The list of the 61 municipalities subject to the control was made public when the law was passed. Eight additional municipalities entered the system in January 2022 and two exited it in 2021. We exclude these few partially treated municipalities from the empirical analysis. A tight housing market was defined as one in which the past 5-year annual rent growth was three percentage points higher than inflation. In practice, this implied an annual rent growth higher than 4.15% between 2014 and 2019.

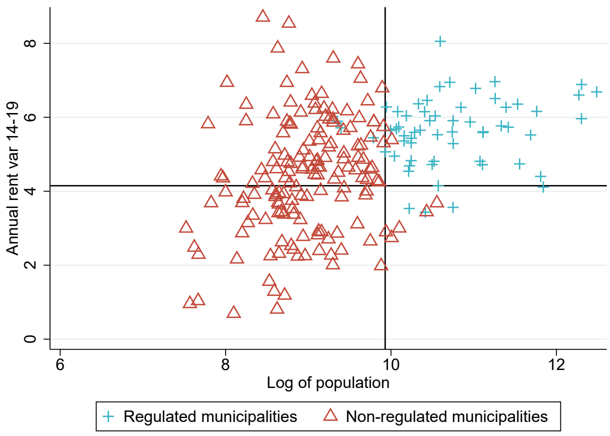
Panel a) in Figure 1 plots population size and annual rent variation in the 2014-2019 period for the regulated and the non-regulated municipalities[6]. The horizontal line represents the market tightness criteria (annual rent growth 3 points higher than inflation), while the vertical line marks the 20,000 inhabitants threshold. Compliance with the law's criteria is almost complete, with just a few exceptions on both criteria. Panel b of Figure 1 shows the geographical distribution of municipalities with a tight housing market by regulation status, which is our sample of analysis, as we explain in section 3. Although regulated municipalities are over-represented in the Barcelona metropolitan area, both regulated and non-regulated municipalities can be found in different parts of the region.

[4]The eight specific amenities were having an elevator, a parking space, a heating/cooling system, a pool, other building shared facilities, a janitor, a nice special view, or being already furnished.

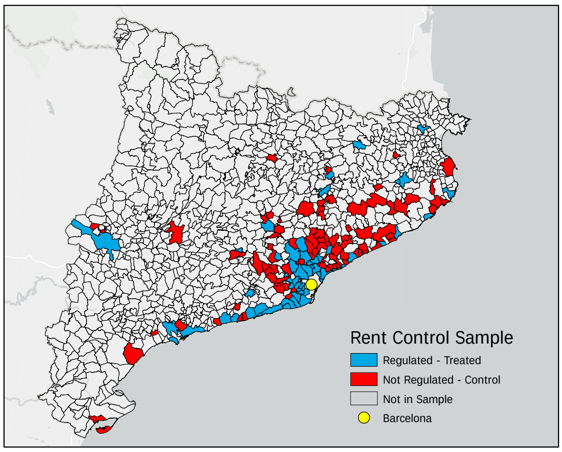
[5]In La Vanguardia, the newspaper with the most online and on-paper readers in Catalonia, the issue was featured on September the 7th for the first time (https://www.lavanguardia.com/hemeroteca).

[6]The Figure excludes municipalities that do not have at least one agreement signed in each quarter.

Figure 1: Regulated and non-regulated municipalities.



(a) Population size and rental price growth thresholds



(b) Map of regulated and non-regulated municipalities

Notes: (a) plots the (log of) population and the yearly rent variation from 2014 to 2019. The horizontal line is the minimum annual rent growth needed to qualify as a tight housing market according to the tightness criteria, which requires annual rent growth to exceed inflation by three p.p. over a five-year period. The vertical line is the 20.000 inhabitants threshold. The graph excludes small non-regulated municipalities with insufficient data as explained in the text. (b) shows the geography of regulated vs non-regulated municipalities in our final sample as detailed in Section 3.

3 Data

We use microdata on rental market agreements from the Catalan Housing Agency and INCASOL[7]. We have the universe of agreements signed and ended, with information on the exact location of the dwelling, its price, the square footage of the unit and a measure of quality. The data enables us to track rental price changes and the number of agreements signed and ended. Similarly, we are able to build a measure of the active stock of rental units at each point in time. The data is available between 2016 and 2022, and, in most analyses, we aggregate it at the municipality and quarter level.

[7]INCASOL is a public agency that collects the rent deposit on every rent agreement. Landlords are obliged by law to transfer the rent deposit on each agreement to INCASOL within two months of the agreement's signature date. The deposit is kept at INCASOL until the rental agreement ends, and the deposit can be returned to the tenant.

We exclude municipalities without at least one signed agreement each quarter to keep a balanced panel of municipalities. As explained in Section 2, for a municipality to be regulated, its housing market had to be considered tight and its population above 20,000 inhabitants. In order to make regulated and non-regulated municipalities more comparable in terms of pre-treatment housing market dynamics, we exclude from our main sample non-regulated municipalities that did not qualify for the tight housing market condition. More specifically, we exclude municipalities with annual rent growth in the 2014-2019 period below 4.15%. We also excluded eight municipalities that entered the system and two that exited it in 2021, as these are partially treated. Finally, we also exclude the city of Barcelona. Besides being much larger than the rest of the municipalities (its population is six times the size of the second-largest municipality), there are also significant differences concerning its economy (i.e., tourism, tradable business services)[8]. This leaves us with a sample of 58 regulated and 90 control municipalities.

To provide a more complete picture of the effects of rent control on housing markets, we complement our data with information on housing sales provided by IDESCAT. Specifically, we examine average sales prices and the number of housing sales at the municipality and quarter level.

We complement these primary data sets with other data sources to build control variables. These additional data include labor market outcomes from Social Security, such as quarterly unemployment data, the number of employment contracts signed, and the number of Covid-19 furloughs. This scheme, the so-called ERTO in Spain, provides workers unemployment benefits while their contract is temporally suspended. We label this variable Covid-19 furloughs. Finally, we include yearly local population inflows and outflows from the *Encuesta de Variaciones Residenciales.*

Table 1 displays descriptive statistics of all the relevant variables by regulation status for two points in time: one year before and one year after the start of the rent control. We note that the average rent slightly decreases in regulated municipalities, while it increases in non-regulated ones. The rent gap is twice as small in 2021 as before the rent control in 2019. We also observe that regulated municipalities are larger and closer to the nearest CBD than non-regulated municipalities.

[8]One specific example is related to the short-term rental sector. Prior to Covid-19, short-term rental accommodation was quantitatively important in the city center of Barcelona, as shown by (Garcia-López et al., 2020). One may be concerned that the collapse of tourism in 2020 due to Covid-19 might have reduced the demand for short-term rental accommodation, which, in turn, might have led to a supply increase of rental units in the residential market. Batalha et al. (2022) show that this phenomenon was quantitatively important in Lisbon, where the increased supply of housing units in the residential market reduced rental prices.

Table 1: Descriptive statistics pre- and post-rent control

	2019 4th quarter		2021 4th quarter	
	Mean Regulated	Mean Non-regulated	Mean Regulated	Mean Non-regulated
Rent (€/month)	688.11	646.19	681.83	665.07
Past Rent growth (%)	5.66	5.48	.	.
Agreements (p.1,000 inhab)	4.82	4.48	4.49	4.11
Surface (sq meters)	67.86	75.62	66.10	72.98
Good Condition (%)	38	49	40	50
Unemployed	5.46	4.80	5.24	4.63
New employment contracts	3.27	2.90	2.82	2.57
Population	62,237	10,167	62,903	10,388
Distance to CBD (km)	20.39	31.14	20.39	31.14
Net Migration (%)	1.55	1.57	0.46	1.15
In-Migration (%)	6.63	6.86	5.97	6.56
Out-Migration (%)	5.08	5.28	5.51	5.40
Observations	58	90	58	90

Notes: Descriptive statistics for our sample of 148 municipalities. Variables are measured one year before rent control (fourth quarter of 2019) and two years after (fourth quarter of 2021). Past rent growth is calculated as the annual rent change from December 2014 to December 2019 using aggregated data to match the regulation's tight market criteria. The number of unemployed people and new employment contracts are expressed by 100 inhabitants. Migration data is presented as the share of the population in the municipality.

4 Empirical strategy

In order to identify the causal effect of the rent control regulation, we implement difference-in-differences regressions and event-study designs, exploiting the fact that only a subset of municipalities is subject to the rent control. Formally, we estimate variants of the following regression:

$$Y_{m,t} = \alpha + \beta(RentControl_m \times Post_t) + \gamma_m + \delta_t + X_{m,t} + \varepsilon_{m,t} \tag{1}$$

where $Y_{m,t}$ is the outcome of interest in municipality m at quarter t, namely, the log of the average monthly rent or the log of the number of tenancy agreements signed per 1,000 inhabitants. We use a relative measure of the number of agreements to account for differences in population size across municipalities. We also examine other housing market outcomes including the number of ended contracts, the stock of active rented units, sales prices and the number of sales.

Our main explanatory variable is $RentControl_m$ and indicates whether the municipality was subject to rent control. The dummy variable $Post_t$ indicates that the rent control system was in place in that quarter. Since the regulation's approval is in September 2020, we consider the last quarter of 2020 as the first fully treated quarter. The coefficient associated with the interaction of these two variables, β, estimates the change in average rents (or the number of agreements) in regulated municipalities relative to non-regulated municipalities. In some specifications, we add an additional coefficient, $RentControl_m \times Anticipation_t$, that takes value one for the third quarter of 2020 when anticipation effects could potentially occur. Municipality (γ_m) and time (δ_t) fixed effects are included in all specifications. We cluster standard errors at the municipality level.

The Covid-19 shock represents a challenge in our estimation, as the pandemic and its associated policy measures started six months before the rent control's implementation. Even if we control for quarter fixed effects, the impacts of Covid-19 on housing markets could be different across municipalities. We are particularly worried that the Covid-19 shock has heterogeneous local impacts on the labor market, and these, in turn, translate into heterogeneous impacts on housing markets. Therefore, the vector $X_{m,t}$ includes variables reflecting the dynamics of local labor markets. More specifically, we include the number of people registered as unemployed, the number of Covid-19 furloughs and the number of new employment contracts. All three variables are expressed relative to its population (i.e., per 100 inhabitants).

In order to allow for richer dynamics of the effects of rent control on housing markets, we complement the difference-in-differences regressions with an event study approach. In this case, the specification that we estimate is the following:

$$Y_{m,t} = \alpha + \sum_{t \neq 2019q3} \beta_t (RentControl_m \times \delta_t) + \gamma_m + \delta_t + X_{m,t} + \varepsilon_{m,t} \tag{2}$$

As in the previous specification, we include municipality and time fixed effects (γ_m and δ_t), as well as time-varying variables at the municipality level (i.e., $X_{m,t}$). Here, we estimate several coefficients β_t that result from the interaction of the treatment indicator with a set of quarter dummies. We use the fourth quarter of 2019 to normalize the estimates and avoid using a quarter affected by the Covid-19 shock as a reference.

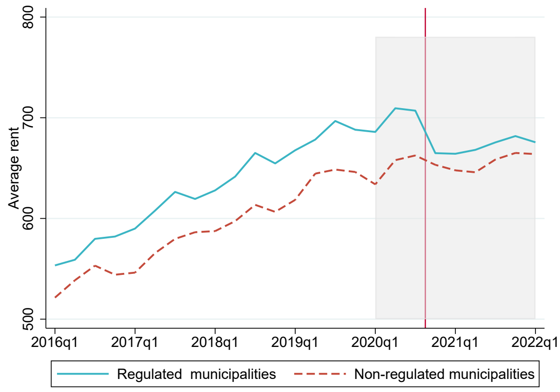
4.1 Graphical evidence

Before moving to the econometric results, in Figure 2 we plot the evolution of rents and the number of agreements signed in regulated versus non-regulated municipalities. Figure 2a shows that rents markedly drop in the regulated group in the fourth quarter of 2020 when the rent control system was adopted, suggesting that rent control was effective in reducing rental prices in treated municipalities. Prior to this, and despite a price difference in levels, both groups of municipalities had a similar rental price evolution before the adoption of the regulation. This is not particularly surprising given that our control group only includes municipalities with high rent growth in the 2014-2019 period.

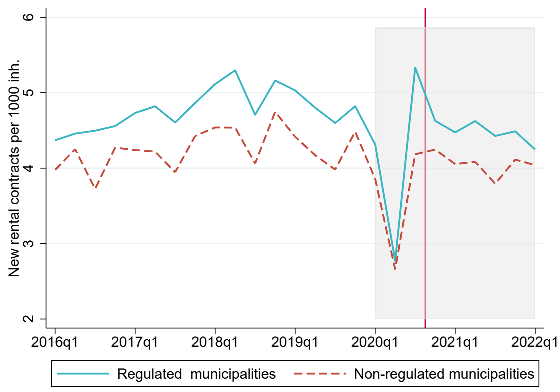
As explained above, the impact of the Covid-19 crisis could complicate our analysis as it partly overlaps in time with the application of the rent control system. In this respect, it is reassuring that the rent price gap between regulated and non-regulated municipalities remains constant with the arrival of the pandemic and is only reduced in the last quarter of 2020, coinciding with the introduction of the regulation.

Figure 2b plots the analogous graph for the average number of tenancy agreements signed per 1,000 inhabitants. Notice that regulated municipalities always show a slightly larger number of tenancy agreements signed per 1,000 inhabitants. This difference in levels is remarkably constant over time, and seasonal effects seem to affect regulated and non-regulated markets similarly. The effect of the first Covid-19 lockdown between February and June 2020 seems to have hit slightly harder the regulated group, yet both groups experienced a massive reduction in tenancy agreements in the second quarter of 2020. The market quickly recovered in the third quarter of 2020, when the number of tenancy agreements increased to pre-pandemic levels. The gap in the third quarter of 2020 is particularly large and is consistent with an anticipation effect to avoid the regulation. There is no strong visual indication that the rent control introduction in the fall of 2020 has widened or reduced the gap in tenancy agreements between regulated and non-regulated municipalities.

Figure 2: Evolution of rental markets in regulated and non-regulated municipalities.



(a) Average rent



(b) Contracts per 1000 inhabitants

Notes: (a) plots the evolution of the average rent for regulated (58 municipalities) and non-regulated (90 municipalities) while (b) shows the evolution of the number of tenancy agreements signed in each quarter per 1000 inhabitants. The vertical line indicates the implementation of rent control while pandemic quarters are shaded in gray.

5 Main results

5.1 Baseline results

In Table 2, we present our baseline results for the impact of rent control on average rents and the number of tenancy agreements signed. In column 1, we regress the log of rents against the difference-in-differences interaction term, controlling only for time and municipality fixed effects. Column 2 further includes the time-varying control variables (e.g. $X_{m,t}$). In column 3, which is our preferred specification, we allow for anticipation effects of the rent control system in the third quarter of 2020. We then reproduce the same three regressions using, as an outcome, the log of the number of tenancy agreements signed per 1000 inhabitants.

Starting with the effects on rental prices, the results of all specifications in Table 2 suggest that the rent control system decreased average rents. The inclusion of variables reflecting the local labor market dynamics (column 2) or allowing for anticipation effects (column 3) does not affect our estimates of interest. The results of our preferred spec-

Table 2: Impact of rent control on rents and tenancy agreements: Baseline results

	(Log) average rents			(Log) Tenancy agreements per 1000 inhabitants		
	(1)	(2)	(3)			
RentControl × Post	-0.044***	-0.044***	-0.045***	-0.011	-0.010	-0.003
	(0.006)	(0.006)	(0.006)	(0.021)	(0.020)	(0.021)
RentControl×			-0.004			0.130***
Anticipation			(0.009)			(0.029)
Unemployed		-0.007	-0.007		0.058***	0.059***
		(0.004)	(0.004)		(0.015)	(0.015)
Covid-19 furloughs		-0.001	-0.001		0.004	0.004
		(0.001)	(0.001)		(0.003)	(0.003)
New employ. contracts		-0.000	-0.000		-0.013*	-0.013*
		(0.001)	(0.001)		(0.008)	(0.007)
Observations	3,698	3,698	3,698	3,698	3,698	3,698
Municipalities	148	148	148	148	148	148
Time FE	X	X	X	X	X	X
Mun FE	X	X	X	X	X	X

Notes: Estimates of equation 1. Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parentheses, are clustered at the municipality level. The number of unemployed people and the number of new employment contracts are measured per 100 inhabitants.

ification (column 3) indicate that average rental prices decreased by 4.5% in regulated municipalities compared to non-regulated municipalities. To interpret the economic magnitude of this effect, we refer to the average rent in regulated markets in the third quarter of 2019. A 4.5% reduction amounts to €30 in monthly rent or €358 annually. This decrease in rents represents a significant reduction if we take into account that the average household annual income in Catalonia is €33,321[9]. In particular, the reduction represents 1.4% of the annual income of households living in rental units in the region[10].

Next, the results of Table 2 do not indicate that the introduction of the rent control system has affected the number of tenancy agreements signed in regulated municipalities. Coefficients remain close to zero in all specifications and are not statistically significant. This is in line with the graphical evidence shown in Figure 2b. In column 6, the results indicate that in regulated municipalities, there was a 13% increase in the number of signed agreements in the third quarter of 2020. This large increase is also consistent with Figure 2b and indicates that more agreements than usual were signed just before the implementation of the system. Below, we will turn to weekly data to provide further evidence of these anticipation effects.

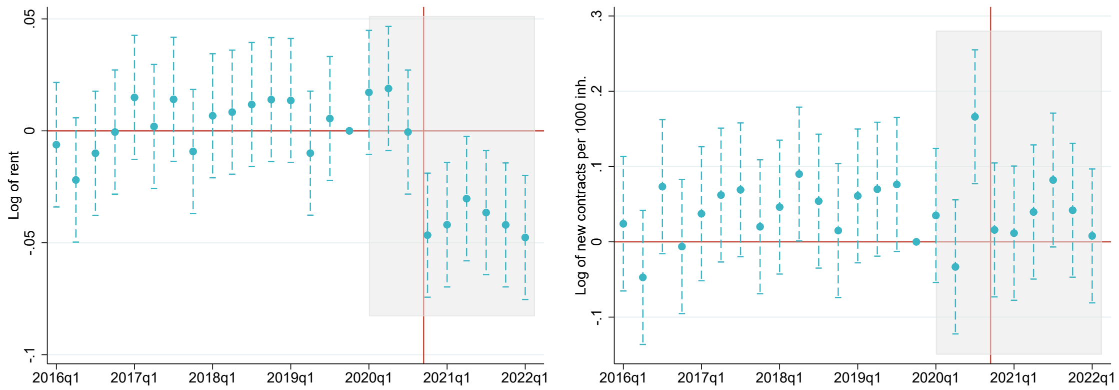
The results of the event study regressions specified in Equation 2 are presented in Figure 3 that plots the coefficients of the interaction terms between the treatment indicator and a set of quarter dummies and their 95% confidence intervals. Results are shown for rents (Panel a) and tenancy agreements per 1000 inhabitants (Panel b). The shaded area

[9]Data comes from Idescat for 2019.

[10]The median income for rental households in the NUTS1 region that comprises Catalonia and Valencia is €24,666. Data from the EU-SILC for 2019.

indicates the start of the Covid-19 pandemic.

Figure 3: Event study for rents and new tenancy agreements



(a) Effect on rents

(b) Effect on tenancy agreements

Notes: Graphs plot the interaction terms between the treatment indicator and a set of quarter dummies and their 95% confidence intervals (see equation 2). Outcome variables are (log of) average rents and log of tenancy agreements per 1,000 inhabitants. In both cases, the vertical line indicates the implementation of rent control. The beginning of the shaded area indicates the start of the pandemic.

Panel a) confirms that rents were reduced by 4-5% in regulated municipalities when the system came into place in the last quarter of 2020. The figure also shows that, prior to the adoption of the rent control system, the growth in rental prices was similar in the regulated and non-regulated municipalities of our sample, particularly so from 2017 onward. Again, this is not surprising since our control group only includes municipalities with tight housing markets. Reassuringly, the Covid pandemic did not seem to have a statistically different effect on rents.

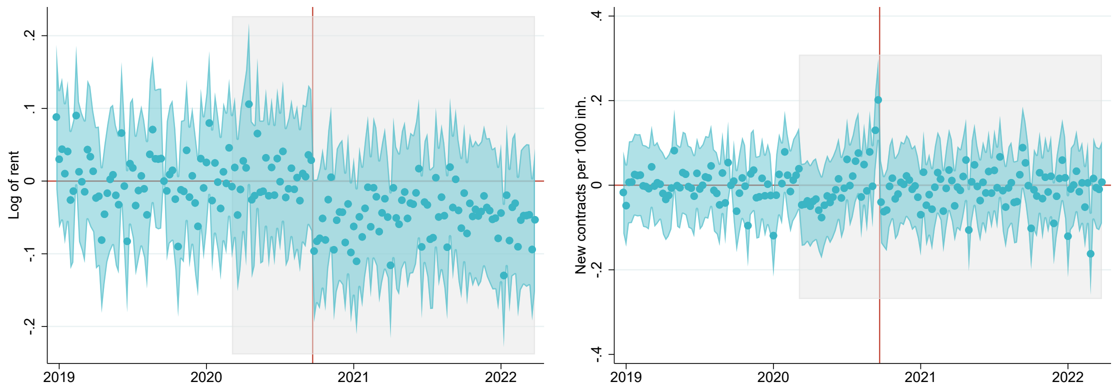
Similarly, there is no evidence of pre-trends in Panel (b), implying that before the rent control system was adopted, the two groups of municipalities evolved similarly regarding the number of tenancy agreements signed per 1000 inhabitants. In contrast to the results on rental prices, the regulation does not seem to have affected the number of agreements signed beyond the anticipation effect that took place in the third quarter of 2020.

5.2 Evidence from weekly data

As explained in Section 2, the time period in which anticipation effects could occur is a two-week period between September 7, when the parliamentary discussions of the law became salient, and September 21 when the law became effective. To zoom in on the anticipation effects, we reproduce the main analysis at the weekly level.

One complication that appears when working at the weekly level is that many municipalities have zero contracts signed in many weeks. Thus, the rental price regressions are carried out in an inevitably unbalanced sample. As for the number of contracts signed, we will work with the variable in levels instead of logs (i.e. contracts per 1000 inhabitants) in order to keep a balanced sample. The difference-in-differences estimates of equation 1 with weekly data are reported in Table A1, where the *Anticipation* period is restricted to the two-week period before the policy became effective on September 21. The results of the analogous event-study regressions (see equation 2) are shown in Figure 4. Although the regressions are run with data for the entire time period (e.g. 2016-2022), for illustrative purposes this figure only reports the interaction terms between *RentControl* and the weekly dummies from 2019 onward.

Figure 4: Event study for rents and new tenancy agreements



(a) Effect on rents **(b)** Effect on tenancy agreements

Notes: Graphs plot the interaction terms between the treatment indicator and a set of weekly dummies and their 95% confidence intervals (see equation 2). The equation is estimated with the full 2016-2022 sample. Outcome variables are: (the log of) average rents and (the level of) new tenancy agreements per 1,000 inhabitants. In both cases, the vertical line indicates the implementation of rent control. The beginning of the shaded area indicates the start of the pandemic.

The estimates in Table A1 and Figure 4a are very similar to the baseline results estimated with quarterly data, although the estimated price effects caused by the rent control are slightly larger (5%). The results of columns 4 to 6 in Table A1 confirm that the policy did not reduce the number of new agreements signed. As for anticipation effects, the results indicate that in the two weeks prior to rent regulation, the number of agreements signed increased by 17%. The short-time span in which anticipation effects took place is visible Figure 4b where only the coefficients for the two weeks before the adoption of the policy are positive and statistically significant.

5.3 Results on other housing market outcomes

To have a more complete picture of the way landlords and tenants react to rent control, we look at other housing market outcomes. Even if the number of new contracts does not change, there could still be a decrease in supply if the rent control encouraged incumbent tenants to move in order to find a better deal in the rent-controlled scenario. If this is the case, we should observe an increase in the number of ended contracts in regulated municipalities.

As explained in Section 3, we also have data on the universe of agreements that have come to an end, which is an interesting outcome per se as it allows us to assess potential effects on turnover. Moreover, the combination of signed and ended agreements allows us to compute a measure of the stock of rented units at each point in time, this is, the number of housing units that are active in the rental market. Since we do not observe the agreements signed prior to 2016, the number of ended agreements and the stock of active dwellings are reliable measures only from 2019 onward, which is when the first three-year agreements signed in 2016 start coming to an end[11]. Thus, this exercise is carried out for the period 2019-2022.

Columns 3 and 4 in Table 3 report the results of our baseline specification, where the dependent variables are ended agreements and the active stock of rented units. Both variables are also expressed relative to 1000 inhabitants and logged. For comparability, in columns 1 and 2 we re-run the baseline specification for our two main outcomes (rental prices and tenancy agreements signed) on the shorter 2019-2022 period. The results are very similar to those of the baseline sample of Table 2. The results in column 3 indicate that the rent control system did not change the number of ended agreements. This suggests that the policy did not encourage tenants to move to other units as a response to the regulation, suggesting that the policy did not significantly affect turnover. Since the rent control system did not affect the number of signed agreements nor the number of ended agreements, the stock of rented units should also be unaffected by the policy. The results in column 4 confirm that this is the case. The effect is very close to zero and reinforces the view that the rent control system did not reduce the stock of housing units in the rental market.

Table 3: Impact of rent control on other housing market outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Rents	Tenancy Agreements	Ended Agreements	Active Stock	Sales prices	Sales number
$RentControl \times$	-0.049***	0.001	-0.006	0.002	0.015	-0.071**
$Post$	(0.006)	(0.021)	(0.023)	(0.007)	(0.013)	(0.028)
$RentControl \times$	-0.008	0.134***	0.090**	-0.000	0.031	0.039
$Anticipation$	(0.010)	(0.028)	(0.038)	(0.004)	(0.028)	(0.041)
Observations	1,922	1,922	1,922	1,922	3,439	3,444
Municipalities	148	148	148	148	138	138
Controls	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Mun FE	X	X	X	X	X	X
Period	19-22	19-22	19-22	19-22	16-22	16-22

Notes: Estimates of equation 1. Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parentheses, are clustered at the municipality level. In columns 1 and 5 the outcomes are the log of the average rent and sales prices. In columns 2, 3, 4 and 6 the outcomes are expressed relative to 1,000 inhabitants and logged. Controls include the number of unemployed people, the number of Covid-19 furloughs and the number of new employment contracts.

In the last two columns of Table 3 we turn to sales in the housing market. Being unable to rent their units at a market price level, landlords may choose to sell their units instead. If this is a general behavior, we should observe an increase in the number of sales and potentially a decrease in sales prices. To assess this question, we examine average sales prices and the number of sales per 1000 inhabitants. The two outcomes are logged and the data covers the entire time period 2016-2022. According to column 5, there is no indication that the adoption of the rent control system reduced house prices in regulated municipalities. The fact that the reduction in rents did not translate into a reduction

[11] As detailed in Section 2, until 2019 the minimum length of rental contracts was 3 years.

in house prices could be explained by the uncertainty regarding the permanent versus temporary nature of the policy. In contrast, the results in column 6 indicate that the rent control reduced the number of sales by about 7%. This result suggests that the policy did not induce landlords to sell units that were previously in the rental market. This is in line with our previous findings showing no reduction in the stock of active rented units. However, this result would be consistent with the rent control discouraging buy-to-let purchases, which could affect the stock of rented units in the medium run. Yet, we will show in the robustness checks that the negative impact of the policy on the number of sales is not a particularly robust result, which calls for caution in its interpretation.

Finally, we evaluate a last supply margin by looking at the entry of new units into the rental market. We do so by differentiating between signed agreements in units that were previously rented and agreements in units that are rented for the first time. The sample is restricted to the period from the third quarter of 2019 onward, which corresponds to the period where we can distinguish between these two types of units.[12] The results are reported in Table 4 where columns 1 and 2 report the findings for rental prices while results for the number of signed agreements are shown in columns 3 and 4.

Table 4: Impact of rent control on rents and tenancy agreements: Previously rented versus new units.

	(1)	(2)	(3)	(4)
	Rents		Tenancy agreements	
Outcome	New units	Old units	New Units	Old Units
$RentControl \times Post$	-0.047***	-0.056***	0.010	0.013
	(0.008)	(0.009)	(0.027)	(0.039)
$RentControl \times Anticipation$	-0.009	-0.007	0.138***	0.145***
	(0.014)	(0.014)	(0.036)	(0.051)
Observations	1,626	1,625	1,626	1,625
Municipalities	148	148	148	148
Controls	X	X	X	X
Time FE	X	X	X	X
Mun FE	X	X	X	X
Period	19q3-22	19q3-22	19q3-22	19q3-22

Notes: Estimates of equation 1. Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parentheses, are clustered at the municipality level. Rents is the logged average rent and tenancy agreements are expressed relative to 1,000 inhabitants and logged. Controls include the number of unemployed people, the number of Covid-19 furloughs and the number of new employment contracts.

The effect of the regulation on rental prices is slightly larger for units that were previously rented (-0.056) than for units that were rented for the first time (-0.047). This conforms to expectations, since previously rented units are subject to both a nominal cap and the previous rent, while newly rented units are only subject to the nominal cap.

The results in columns 3 and 4 indicate that the policy did not reduce the number of agreements signed for any of the two groups of units considered. These results indicate that the policy did not cause an exit of units from the rental market, as there is not a reduction of signed agreements of units that had been previously rented. Analogously, there is no evidence that the policy reduced the entry of new units into the rental market.

[12] Agreements signed in 2015 which we do not observe should end by 2018. Hence, if a unit first appears in a signed agreement on June 1st 2019 we know that it has not been rented during the last six months (since December 2018) and we consider it as a new unit.

These results indicate again that the rent control in Catalonia did not significantly reduce the supply of rental units.

5.4 The effects on the composition of rented units

Although the data are not particularly rich regarding dwelling characteristics, we have two variables that reflect quality. The first one is the squared footage of the unit and the second one is a binary variable that indicates if the unit is in a good condition. In Table A2 we use equation 1 to estimate the effect of the rent control system on the (logged) average values of these outcomes at the municipality level. The results indicate that neither the squared footage of units nor the proportion of units in good condition change after the introduction of the rent control system. These findings suggest that the policy did not generate a change in the composition of units being rented.

6 Robustness checks

6.1 Alternative specifications and samples

In a first set of robustness checks, we assess if the results are robust to alternative econometric specifications and samples. The results are presented in Table 5. As shown in the raw data (Figure 2) and more formally in the event-study graphs (Figure 3), the hypothesis that regulated and non-regulated municipalities show parallel trends prior to the adoption of the policy seems to hold. Despite this, in column 2 of Table 5, we re-estimate the baseline specification allowing for heterogeneous time trends. More specifically, we include municipality-specific linear time trends (i.e., $\eta_m \times t$). In doing so, each municipality is allowed to have its own linear time trend, and the variation that we exploit is then limited to deviations from this trend. The results obtained are similar to those of the baseline specification (column 3 in Table 2 reproduced here in column 1), although the price effect is slightly larger (-0.051 vs. -0.045) and the effect on the number of signed agreements becomes more negative, but still is statistically insignificant (-0.028 vs -0.003).

In order to obtain treated and control groups with more similar pre-treatment dynamics in the housing market, our control group only includes municipalities that met one of the two policy eligibility criteria, namely, to have a tight rental market. Yet, as can be seen in Figure 1a and in Table 1, regulated municipalities are larger than non-regulated ones. To assess to what extent this might be driving our results, we restrict our sample to less dissimilar population levels. More specifically, columns 3 to 5 report our baseline regressions when we restrict the sample to 5,000-150,000, 7,500-100,000 and 10,000-60,000 inhabitants. The number of municipalities in each sub-sample decreases accordingly. The results are qualitatively and quantitatively similar to those of our baseline strategy. Although the effect on the number of signed agreements becomes more negative, it remains statistically insignificant.

The pandemic has had a significant effect on the tourism sector, and the region of Catalonia was no exception to this: the number of tourists in the region in 2020 was 80% lower compared to 2019[13]. Tourism can impact the residential housing market through

[13]Source: Movimientos Turísticos en Fronteras, INE.

short-term rental housing (Batalha et al., 2022). Although we drop Barcelona from the main analysis partly due to this identification concern, there are other municipalities in Catalonia where the tourism industry is one of its main economic activities[14]. Therefore, we drop all municipalities with more than 500 short-term rental licenses as a robustness exercise. The results reported in column 6 of Table 5 are in line with our baseline results and show a decrease in average rents of 5.0% in regulated municipalities. As in the main analysis, there is no significant change in the number of tenancy agreements signed due to the implementation of the rent control.

Next, in column 7 and for the sake of completeness, we show the results obtained when we include the city of Barcelona in the sample. The inclusion of the capital and largest city in the region changes our estimates very little.

One caveat of our approach is that non-regulated municipalities in our control group might also be affected by the policy. This could occur through two mechanisms. On the one hand, the regulation could increase the demand in non-regulated municipalities if households can not find a suitable unit in the rent-controlled market (i.e. a displacement effect). On the other hand, the lower prices in regulated municipalities might create a downward price pressure in non-regulated municipalities (i.e. a contagion effect). In both cases, we expect spillover effects to be larger in control municipalities that are direct neighbors of rent-controlled municipalities. Thus, we re-run the main specification excluding control municipalities that are direct neighbors of regulated municipalities (i.e. that share a border with a treated municipality). This represents an exclusion of 45 control municipalities. The results are reported in column 8. The coefficient for rents becomes more negative (-0.058 instead of -0.045 in the baseline), while the coefficient for tenancy agreements remains insignificant. This is consistent with a contagion effect, by which the reduction in rents in regulated municipalities disciplined rents in neighboring but non-regulated municipalities.

To further explore these spillover effects, we conduct a difference-in-differences exercise where we compare non-regulated municipalities that are neighbors of regulated municipalities to non-regulated municipalities that are not direct neighbors. The results, presented in Table A3, indicate that among the non-regulated municipalities, the rent control reduced prices by 2.7% in municipalities that are direct neighbors of regulated municipalities. This effect is sizable and is consistent with a contagion effect by which the reduction of rental prices in regulated municipalities causes a price reduction in neighboring and non-treated municipalities. This suggests that our baseline results on prices are actually a lower bound of the effect of the rent control on prices. In contrast, we find no evidence of spillovers when it comes to the number of tenancy agreements signed.

For completeness, Tables A4 and A5 show the same robustness tests for the other housing market outcomes analyzed in Table 3. The results remain largely unaltered across the alternative specifications and samples, with one exception. The negative effect of the policy on the number of sales becomes closer to zero and statistically insignificant in column 2 (which includes municipality-specific time trends) and in columns 4 and 5 (that restricts the sample to 7,500-100,000 and to 10,000-60,000 inhabitants).

[14] In the region, there are 80,000 licenses to operate short-term rentals.

Table 5: Impact of rent control on rents and tenancy agreements: Alternative specifications and samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: (Log) Average rents								
RentControl × *Post*	-0.045***	-0.051***	-0.044***	-0.038***	-0.047***	-0.050***	-0.046***	-0.058***
	(0.006)	(0.007)	(0.006)	(0.007)	(0.008)	(0.006)	(0.006)	(0.007)
RentControl×	-0.004	-0.008	-0.001	0.004	-0.002	-0.008	-0.005	-0.019
Anticipation	(0.009)	(0.010)	(0.009)	(0.010)	(0.012)	(0.010)	(0.009)	(0.012)
Panel B: (Log) Tenancy agreements per 1000 inhabitants								
RentControl × *Post*	-0.003	-0.028	-0.014	-0.015	-0.037	0.008	-0.000	-0.013
	(0.021)	(0.025)	(0.022)	(0.024)	(0.029)	(0.023)	(0.021)	(0.027)
RentControl×	0.130***	0.111***	0.134***	0.140***	0.119***	0.138***	0.129***	0.137***
Anticipation	(0.029)	(0.030)	(0.029)	(0.032)	(0.034)	(0.030)	(0.028)	(0.038)
Sample	Baseline	Municipality Time trends	5,000 to 150,000	7,500 to 100,000	10,000 to 60,000	Vacation homes	With Barcelona	Without neighbors
Observations	3,698	3,698	3,325	2,675	1,875	3,298	3,723	2,574
Muncipalities	148	148	133	107	75	132	149	103
Controls	X	X	X	X	X	X	X	X
Time FE	X	X	X	X	X	X	X	X
Mun FE	X	X	X	X	X	X	X	X

Notes: Estimates of equation 1. Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parentheses, are clustered at the municipality level. Column 2 is the baseline sample with municipality-specific linear time trends. "5,000 to 150,000" refers to a sample that only includes cities between 5,000 and 150,000 inhabitants. The same logic applies to columns 3 and 4. "Vacation homes" refers to a sample of municipalities with less than 500 vacation homes. "Without neighbors" is a sample that excludes control municipalities that are immediate neighbors of regulated municipalities. Controls include the number of unemployed people, the number of Covid-19 furloughs and the number of new employment contracts.

6.2 The potentially confounding effects of Covid-19

One way through which Covid-19 could have affected the housing market is through the increase in Working From Home (WFH). Commuting costs are significantly reduced with WFH, which makes moving to the suburbs more attractive (Delventhal et al., 2021). Gupta et al. (2021) and Ramani and Bloom (2021) show that, in US cities, Covid-19 has reduced housing rents and prices in central city locations relative to the suburbs[15]. This effect has been labelled the "donut" effect, and it could confound our findings if the geography of regulated and non-regulated municipalities partly overlaps with this effect.

We deal with this identification threat by augmenting our preferred specification by including a set of interaction terms between a Covid-19 dummy, which takes the value of one for pandemic quarters (first quarter of 2020 and subsequent quarters) and a set of dummies (i.e. 0-14, 15-20, 21-29, 30-39, 40-82 km) that reflect the distance to the CBD, measured here by the distance to the provincial capital. The results, which are presented in Table A6, remain largely unaltered for all outcomes, suggesting that our results are not driven by a correlation between treatment status and distance to the CBD.

Similarly, one might be worried that the Covid-19 impact on local housing markets could vary by municipality size if WFH increases the demand for larger dwellings and dwellings with outdoor space (Delventhal et al., 2021). Suppose dwellings in larger municipalities tend to be smaller, and these are less likely to have terraces, patios, or gardens. In that case, Covid-19 could particularly reduce housing demand in larger municipalities which, in turn, could bias our estimates. To address this concern, we augment our preferred specification with a set of interaction terms between the Covid-19 indicator and a set of dummies reflecting different municipality sizes (i.e. 0-5000, 5001-10,000, 10,001-20,000, 20,001-50,000, 50,0001-10,000, and over 100,000 inhabitants). The results are reported in Table A7. Although most results remain largely unchanged, there are two exceptions. First, the results for the number of tenancy agreements become positive (0.049) and statistically significant at the 10 per cent level. Taken together with the results of the other specifications, the estimated effect of the policy on the number of signed agreements seems to be centered around zero. Second, the negative result for the number of sales (column 6 in Table 3) becomes smaller in absolute value and becomes statistically insignificant. Combined with the results of Table A5, we conclude that the negative effect of the policy on the number of sales is not robust across specifications and samples.

Finally, we augment our baseline specification with additional controls measuring population inflows and outflows at the municipality level. This would directly control for WFH-induced migration but would also account for other population shocks related or not to Covid-19 (i.e., less international migration or new housing developments). Here we need to rely on annual measures of population inflows and outflows[16]. More specifically, we allow the impacts of population inflows and outflows to differ between pre- and Covid-19 times. The results are reported in Table A8 and remain largely unaffected.

Overall, the results of this section suggest that the Covid-19 shock that partly overlaps in time with the application of the rent control system does not seem to be driving our

[15] Rosenthal et al. (2021) show that the same is true for commercial rents in US cities.

[16] Moreover, the data is not available for 2022 yet, so we have replaced the 2022 missing values with 2021 values.

findings.

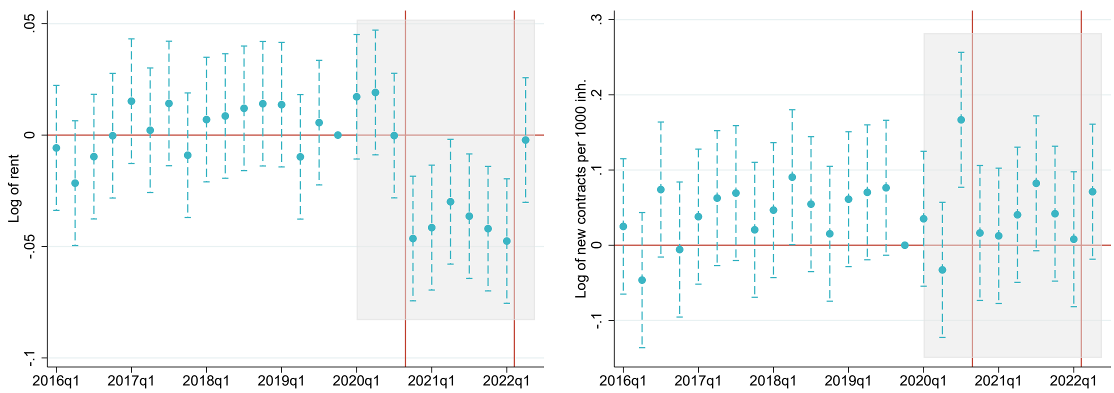
6.3 The end of rent control

As explained in Section 2, the rent control system was declared unconstitutional and suddenly stopped in March 2022. In Figure 5, we re-estimate equation 2 extending the time period until the second quarter of 2022 when the policy was not in place anymore.

This exercise shows that price effects were approximately constant between the last quarter of 2020 and the first quarter of 2022 and, once the regulation stopped, the price difference between regulated and non-regulated municipalities moved back to the pre-regulation period. Contrary to the German case (Breidenbach et al., 2021), this analysis indicates that the policy did not lose efficacy while it was in place. Instead, once it was abandoned, its effects immediately disappear. As for quantities, there was not a significant increase in agreements signed after the end of the rent control system, which is consistent with the notion that the rent control system did not reduce the number of signed agreements while it was in place.

Besides informing on the dynamic effects of the policy, this exercise also provides very strong evidence that the effects that we estimate are causal and are not driven by differential trends or asymmetric shocks between regulated and non-regulated municipalities.

Figure 5: Event study for rents and new tenancy agreements, including the end of the rent control



(a) Effect on rents

(b) Effect on tenancy agreements

Notes: Graphs plot the interaction terms between the treatment indicator and a set of quarter dummies and their 95% confidence intervals (see equation 2). Outcome variables are (log of) average rents and log of tenancy agreements per 1,000 inhabitants. The two vertical lines indicate the implementation and end of rent control. The beginning of the shaded area indicates the start of the pandemic.

7 Concluding remarks

Housing affordability raises concerns in urban areas worldwide. Despite the general unpopularity of rent control policies among economists, cities like Berlin or Paris have decided to implement them. Despite being such a salient policy, the empirical literature on its effects is scarce.

To study the impacts of these policies, we analyze the effects of a high-coverage rent control policy implemented in Catalonia. We examine changes in average rents and several supply measures of the rental market in regulated versus non-regulated municipalities. In order to identify the causal effect of the rent control regulation, we implement difference-in-differences regressions and event-study designs, exploiting the fact that only a subset of municipalities is subject to the rent control. Our results are robust to several robustness checks that address the potential confounding effects of Covid-19 on housing markets.

The results suggest that the rent control measure was effective in decreasing the rent paid in regulated municipalities. Rents in these municipalities decrease by around 4% to 6%, implying an annual decrease in rent payments of approximately 520€. In contrast, we do not find evidence that the regulation reduced the number of new tenancy agreements, the number of ended agreements, not the stock of rental units. Our findings suggest that supply shortages following a rent control regulation are not necessarily substantial, at least in the short run.

Rent control policies are likely to continue to be on the agenda of local and regional governments. Our findings contribute to a more informed debate regarding rent control policies and the design of policies aimed at improving housing affordability in urban areas.

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8 Appendix

Table A1: Impact of rent control on rents and tenancy agreements: Weekly results

	(Log) average rents			(Log) Tenancy agreements per 1000 inhabitants		
	(1)	(2)	(3)	(4)	(5)	(6)
RentControl × *Post*	-0.051***	-0.051***	-0.051***	-0.009	-0.008	-0.007
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
RentControl×			0.030			0.169***
Anticipation			(0.031)			(0.022)
Unemployed		-0.004	-0.004		0.013***	0.013***
		(0.004)	(0.004)		(0.005)	(0.005)
Covid-19 furloughs		-0.001	-0.001		0.002**	0.002**
		(0.001)	(0.001)		(0.001)	(0.001)
New employ. contracts		-0.001	-0.001		-0.002	-0.002
		(0.001)	(0.001)		(0.003)	(0.003)
Observations	43,762	43,762	43,762	49,580	49,580	49,580
Municipalities	148	148	148	148	148	148
Controls	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Mun FE	X	X	X	X	X	X

Notes: Estimates of equation 1 with weekly data. Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parentheses, are clustered at the municipality level. Controls include the number of unemployed people, the number of Covid-19 furloughs and the number of new employment contracts.

Table A2: Impact of rent control: Composition effects

	(1)	(2)
Outcome	Surface	Good condition
RentControl × *Post*	0.001	0.006
	(0.017)	(0.034)
RentControl×	-0.025**	-0.026
Anticipation	(0.011)	(0.043)
Observations	3,698	3,640
Controls	X	X
Time FE	X	X
Mun FE	X	X

Notes: Estimates of equation 1. Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parentheses, are clustered at the municipality level. Both outcome variables are defined as the log of the municipality average.Controls include the number of unemployed people, the number of Covid-19 furloughs and the number of new employment contracts.

Table A3: Impact of rent control on rents and tenancy agreements: Spillover effects

	(Log) average rents			(Log) Tenancy agreements per 1000 inhabitants		
	(1)	(2)	(3)	(4)	(5)	(6)
Neigh Non Regulated× *Post*	-0.025*** (0.008)	-0.025*** (0.008)	-0.027*** (0.008)	-0.027 (0.030)	-0.024 (0.031)	-0.023 (0.031)
Neigh Non Regulated× *Anticipation*			-0.030** (0.014)			0.008 (0.044)
Observations	2,248	2,248	2,248	2,248	2,248	2,248
Municipalities	90	90	90	90	90	90
Controls		X	X		X	X
Time FE	X	X	X	X	X	X
Mun FE	X	X	X	X	X	X

Notes: Estimates of equation 1 where we compare non-regulated municipalities that are neighbors of regulated municipalities to non-regulated municipalities that are not direct neighbors of regulated municipalities. Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parentheses, are clustered at the municipality level. Controls include the number of unemployed people, the number of Covid-19 furloughs and the number of new employment contracts.

Table A4: Alternative specifications and samples for other housing market outcomes: Part 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: (Log) Ended agreements per 1000 inhabitants							
RentControl $\times$ *Post*	-0.006	-0.009	-0.020	-0.015	-0.001	0.009	-0.006	-0.016
	(0.023)	(0.034)	(0.024)	(0.026)	(0.031)	(0.024)	(0.023)	(0.031)
RentControl $\times$	0.090**	0.087**	0.076**	0.088**	0.073*	0.112***	0.092**	0.085
Anticipation	(0.038)	(0.038)	(0.036)	(0.039)	(0.042)	(0.040)	(0.038)	(0.056)
	Panel B: (Log) Active stock of rented units per 1000 inhabitants							
RentControl $\times$ *Post*	0.002	-0.002	0.001	0.004	-0.002	0.003	0.002	-0.004
	(0.007)	(0.009)	(0.007)	(0.008)	(0.009)	(0.007)	(0.007)	(0.010)
RentControl $\times$	-0.000	-0.003	0.001	0.003	0.004	-0.001	-0.001	-0.003
Anticipation	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)
Sample	Baseline	Municipality Time trends	5,000 to 150,000	7,500 to 100,000	10,000 to 60,000	Vacation homes	With Barcelona	Without neighbors
Observations	1,922	1,922	1,729	1,391	975	1,714	1,935	1,338
Muncipalities	148	148	133	107	75	132	149	103
Controls	X	X	X	X	X	X	X	X
Time FE	X	X	X	X	X	X	X	X
Mun FE	X	X	X	X	X	X	X	X

Notes: Estimates of equation 1. Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parentheses, are clustered at the municipality level. Column 2 is the baseline sample with municipality-specific linear time trends. "5,000 to 150,000" refers to a sample that only includes cities between 5,000 and 150,000 inhabitants. The same logic applies to columns 3 and 4. "Vacation homes" refers to sample with municipalities with less than 500 vacation homes. "Without neighbors" is a sample that excludes control municipalities that are immediate neighbors of regulated municipalities. Controls include the number of unemployed people, the number of Covid-19 furloughs and the number of new employment contracts.

Table A5: Alternative specifications and samples for other housing market outcomes: Part 2

	Panel A: (Log) average sales price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RentControl × *Post*	0.015	-0.006	0.014	0.003	-0.007	0.018	0.013	0.020
	(0.013)	(0.018)	(0.013)	(0.013)	(0.016)	(0.014)	(0.013)	(0.018)
RentControl×	0.031	0.016	0.024	0.034	0.021	0.050*	0.030	0.031
Anticipation	(0.028)	(0.028)	(0.029)	(0.032)	(0.037)	(0.029)	(0.028)	(0.035)
	Panel B: (Log) Number of sales per 1000 inhabitants							
RentControl × *Post*	-0.071**	-0.053	-0.066**	-0.028	-0.011	-0.079***	-0.073**	-0.134***
	(0.028)	(0.033)	(0.029)	(0.030)	(0.036)	(0.029)	(0.028)	(0.030)
RentControl×	0.039	0.052	0.036	0.050	0.040	0.052	0.037	-0.019
Anticipation	(0.041)	(0.039)	(0.041)	(0.044)	(0.052)	(0.042)	(0.040)	(0.049)
Sample	Baseline	Municipality Time trends	5,000 to 150,000	7,500 to 100,000	10,000 to 60,000	Vacation homes	With Barcelona	Without neighbors
Observations	3,439	3,439	3,319	2,675	1,875	3,039	3,464	2,443
Muncipalities	148	148	133	107	75	132	149	103
Controls	X	X	X	X	X	X	X	X
Time FE	X	X	X	X	X	X	X	X
Mun FE	X	X	X	X	X	X	X	X

Notes: Estimates of equation 1. Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parentheses, are clustered at the municipality level. Column 2 is the baseline sample with municipality-specific linear time trends. "5,000 to 150,000" refers to a sample that only includes cities between 5,000 and 150,000 inhabitants. The same logic applies to columns 3 and 4. "Vacation homes" refers to sample with municipalities with less than 500 vacation homes. "Without neighbors" is a sample that excludes control municipalities that are immediate neighbors of regulated municipalities. Controls include the number of unemployed people, the number of Covid-19 furloughs and the number of new employment contracts.

Table A6: Impact of rent control: Robustness test with distance to nearest province capital and Covid-19 interactions.

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Rents	Tenancy Agreements	Ended Agreements	Active Stock	Sales price	Sales number
$RentControl \times Post$	-0.038***	-0.005	-0.022	0.003	0.010	-0.062**
	(0.006)	(0.020)	(0.023)	(0.007)	(0.013)	(0.030)
$RentControl \times Anticipation$	-0.003	0.122***	0.074*	0.000	0.030	0.033
	(0.009)	(0.028)	(0.038)	(0.004)	(0.028)	(0.041)
$DistCBD_1 \times Covid$	-0.002	0.016	0.018	0.003	-0.009	-0.064**
	(0.008)	(0.031)	(0.040)	(0.009)	(0.018)	(0.030)
$DistCBD_2 \times Covid$	0.001	-0.005	-0.012	-0.006	-0.005	-0.026
	(0.004)	(0.016)	(0.023)	(0.005)	(0.009)	(0.017)
$DistCBD_3 \times Covid$	-0.002	-0.017	-0.026*	-0.003	-0.000	-0.013
	(0.003)	(0.012)	(0.015)	(0.004)	(0.007)	(0.015)
$DistCBD_4 \times Covid$	0.004**	0.001	-0.014	0.006**	-0.001	0.008
	(0.002)	(0.008)	(0.011)	(0.002)	(0.005)	(0.012)
Observations	3,846	3,846	2,070	2,070	3,575	3,582
Controls	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Mun FE	X	X	X	X	X	X

Notes: Variants of equation 1. Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parentheses, are clustered at the municipality level. In columns 1 and 5 the outcomes are the log of the average rent and sales prices. In columns 2, 3, 4 and 6 the outcomes are expressed relative to 1,000 inhabitants and logged. Use of equally sized distances bins: 0-14, 14-20, 20-29, 29-39, 39-82. Base group 0-14. Controls include the number of unemployed people, the number of Covid-19 furloughs and the number of new employment contracts.

Table A7: Impact of rent control: Robustness test with municipality size and Covid-19 interactions.

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Rents	Tenancy Agreements	Ended Agreements	Active Stock	Sales price	Sales number
$RentControl \times Post$	-0.055***	0.049*	0.001	0.005	-0.025	-0.023
	(0.009)	(0.029)	(0.028)	(0.007)	(0.017)	(0.038)
$RentControl \times$	-0.015	0.181***	0.096**	0.002	-0.009	0.088**
$Anticipation$	(0.012)	(0.036)	(0.041)	(0.005)	(0.029)	(0.038)
$MunSize_1 \times Covid$	-0.023**	0.030	-0.126*	0.010	-0.096***	0.066*
	(0.011)	(0.058)	(0.065)	(0.017)	(0.021)	(0.034)
$MunSize_2 \times Covid$	-0.025**	0.083**	0.046	0.017*	-0.074***	0.074*
	(0.011)	(0.034)	(0.043)	(0.009)	(0.024)	(0.043)
$MunSize_3 \times Covid$	-0.022**	0.096***	0.031	0.024**	-0.046**	-0.008
	(0.010)	(0.032)	(0.040)	(0.009)	(0.022)	(0.040)
$MunSize_4 \times Covid$	-0.013*	0.011	-0.002	0.018**	-0.026	-0.026
	(0.007)	(0.023)	(0.028)	(0.007)	(0.021)	(0.033)
$MunSize_5 \times Covid$	-0.015*	0.054**	0.038	0.015**	0.005	-0.017
	(0.008)	(0.027)	(0.029)	(0.007)	(0.025)	(0.031)
Observations	3,698	3,698	1,922	1,922	3,439	3,444
Time FE	X	X	X	X	X	X
Mun FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X

Notes: Variants of equation 1. Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parentheses, are clustered at the municipality level. In columns 1 and 5 the outcomes are the log of the average rent and sales prices. In columns 2, 3, 4 and 6 the outcomes are expressed relative to 1,000 inhabitants and logged. Bin for municipality size: 0-5000; 5,001-10,000; 10,001-20,000; 20,001-50,000; 50,001-100,000; >100,000. Base group is those over 100,000. Controls include the number of unemployed people, the number of Covid-19 furloughs and the number of new employment contracts.

Table A8: Impact of rent control: Controlling for migration

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Rents	Tenancy Agreements	Ended Agreements	Active Stock	Sales price	Sales number
$RentControl \times Post$	-0.043***	0.005	-0.007	0.002	0.006	-0.066**
	(0.006)	(0.022)	(0.024)	(0.006)	(0.014)	(0.031)
$RentControl \times$	-0.005	0.142***	0.083**	-0.000	0.022	0.058
$Anticipation$	(0.009)	(0.029)	(0.040)	(0.004)	(0.028)	(0.040)
$In - migration$	0.212	4.107***	2.626	0.635	-1.013	4.687**
	(0.315)	(0.967)	(2.363)	(0.487)	(0.707)	(1.816)
$Out - migration$	-0.459	1.395	-3.516	-0.224	1.071	1.333
	(0.349)	(0.987)	(2.817)	(0.791)	(0.804)	(1.236)
$In - migration \times Covid$	-0.328	-0.870	-3.630**	-0.486	-0.307	-0.625
	(0.315)	(1.129)	(1.801)	(0.488)	(0.626)	(1.427)
$Out - migration \times Covid$	0.594	0.095	3.683	0.061	-0.130	-0.929
	(0.462)	(1.617)	(2.451)	(0.616)	(0.980)	(1.902)
Observations	3,546	3,546	1,770	1,770	3,297	3,302
Controls	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
Mun FE	X	X	X	X	X	X

Notes: Estimates of equation 1. Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parentheses, are clustered at the municipality level. In columns 1 and 5 the outcomes are the log of the average rent and sales prices. In columns 2, 3, 4 and 6 the outcomes are expressed relative to 1,000 inhabitants and logged. Controls include the number of unemployed people, the number of Covid-19 furloughs and the number of new employment contracts.

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