IEB Working Paper 2022/02

EFFECTIVENESS AND SUPPLY EFFECTS OF HIGH-COVERAGE RENT CONTROL POLICIES

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

February 2022

Cities

IEB Working Paper 2022/02

EFFECTIVENESS AND SUPPLY EFFECTS OF HIGH-COVERAGE RENT CONTROL POLICIES

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

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

IEB Working Paper 2022/02

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

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

ABSTRACT: Concerns related to housing affordability are widespread in cities worldwide, and discussions about adopting rent control policies abound. This paper studies the effects of a rent control policy adopted in Catalonia in September 2020 that applies to some but not all municipalities. The policy virtually covers all the rental market and forces ads and tenancy agreements to specify the applicable rent cap to ensure enforcement. In order to identify the causal effect of the rent control regulation, we implement difference-in-differences regressions and event-study designs and analyze average rents and the number of tenancy agreements signed. Our results indicate that the regulation reduced average rents paid by about 6%. However, this price drop did not lead to a reduction in the supply of housing units in the rental market. We implement several robustness tests to address several identification concerns related to Covid-19. Our results suggest that rent control policies can be effective in reducing rental prices and do not necessarily shrink the rental market.

JEL Codes: R52, R31, H70

Keywords: Rent Control, Housing, Public Policy Evaluation, Event Study

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

Rodrigo Martínez-Mazza
Institute for Urban and Housing Research, Uppsala Unversity and IEB
E-mail: rodrigo.martinez@ibf.uu.se

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

[*] We are grateful to attendants at the Urban Lab seminar series at Uppsala University for their insightful comments.

1 Introduction

Concerns related to housing affordability are widespread in cities around the world. These concerns have raised the 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 given that 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 supply housing shortages and misallocation of housing units (Glaeser and Luttmer, 2003). However, the theory provides little guidance about the economic size of the policy effects. 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 introduction of a rent control system in Catalonia in the fall of 2020 that applies to some but not all municipalities. The regulation applies to municipalities exceeding 20,000 inhabitants with a tight rental market. In rented-controlled municipalities, rental prices must be below a nominal cap (which is dwelling and area-specific) and can not exceed the previous rent of that housing unit. The policy covers virtually all the rental market, with higher nominal caps for units built during the last five years. Ads and tenancy agreements must include the applicable rent cap, and fines are stipulated to ensure further enforcement.

We examine changes in average rents and the number of tenancy agreements signed in regulated vs. 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.

The results of our preferred specification, which includes municipality-specific linear time trends, indicate that rents decreased by 6% in regulated municipalities relative to non-regulated municipalities. In contrast, we do not find evidence that the regulation reduced the tenancy agreements signed, suggesting that supply shortages in the short run are not necessarily substantial.

Given that the Covid-19 shock occurred only two quarters before the implementation of the rent control, 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 suspended contracts, and the number of new employment contracts. Second, we run our baseline regression on different sub-samples: i) municipalities between 10,000 and 60,000 inhabitants to obtain treatment and control groups that are more balanced in terms of population, ii) a sample that excludes municipalities highly specialized in the tourism industry, and iii) a sample that only contains municipalities with a tight rental market, that is, we restrict our control group to municipalities with high past growth in rents but with a population below 20,000 inhabitants. Third, to account for possible "donut" effects of Covid-19 on housing

markets (Bloom and Ramani, 2021; Gupta et al., 2021), we introduce a Covid-19 dummy interacted with different measures of distance to Barcelona and with different measures of municipality size. Finally, 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 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) 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.

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 unregulated markets. More specifically, there is evidence that rent control policies displace housing units from the rental market to the home-ownership market (Sims, 2007; Diamond et al., 2019; Mense et al., 2019) 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 as it only exempts newly built housing units for a period of three years. This feature of the policy might limit the supply responses caused by the rent control. 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 the rent control on rents and the number of tenancy agreements signed. 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 (14.7% of all households) 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 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 in housing. As we will see below, between 2016 and 2019, prior to 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[2], which establishes a minimum contract duration of 5 years (up from 3 years in the previous law). There are no price restrictions in these 5-year tenancy agreements, regardless of whether the agreement is new or renews an expired contract. The law only restricts annual rent changes within the fixed-term 5-year agreements, which can only be changed to reflect inflation.

In September of 2020, the Catalan parliament passed a rent control system to be applied in the region. The regulation only applies to municipalities with "tight" housing markets. The regulation limits the rental price of 5-year rental contracts and applies to both new agreements and renewals. The regulation establishes both a nominal rent cap and a growth cap that anchors the new rental price to the price of the previous tenancy agreement.

Nominal rent caps are computed by the Catalan Housing Agency for each housing unit considering its characteristics and previous tenancy agreements in the area[3]. In regulated municipalities, the rental price of a new tenancy agreement cannot exceed the rental price

[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 rental index that sets the caps is an average of the price of tenancy agreements signed in a given area for the last three years. The study area has at least a 50 meters radius, with at least 25 observations in the sample. If within the first 50 meters there are less than 25 observations, then the radius is increased by 50 meters up to a maximum of 1050 meters, until at least 25 observations are found. The average is calculated using units of similar size, using a 10 square meter margin. For example, for a 50 square meter flat located in Barcelona city center, the index will 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 detailed methodology can be found here: `http://agenciahabitatge.gencat.cat/wps/wcm/connect/f0cb4ce4-5c0c-4a52-aee0-97d880442a9f/METODOLOGIA.pdf?MOD=AJPERES&useDefaultText=0&useDefaultDesc=0`

of the previous agreement for that specific unit. Therefore, the rental price of a new agreement is 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 is €800, then the new rental price cannot exceed €700. Given the aggregate increase in rents in the years prior to the regulation, we expect the previous rent to be binding more often than the nominal cap.

The regulation includes several exceptions. First, if the housing unit satisfies three out of eight specific criteria the rental cap can be increased by 5%[4]. Second, units built during the last five years and those that have undergone substantial renovation have more favorable conditions and are subject to a substantially higher rent cap. Last, if the landlord income is below a given threshold and the tenant income is above a certain level, the regulation of rents is relaxed. In practice, this last condition is hardly met.

The law includes several enforcement measures. These measures regulate the advertisement for rentals and tenancy agreements. Both rents and tenancy agreements must contain the rental cap associated with the unit and, if applicable, the rent paid by the previous tenant. There are penalties (ranging from 9,000 to 90,000 euros) associated with non-compliance.

There was little room for anticipation effects for this policy. First, the rent control legislation followed a rapid legislative procedure, and it was debated and passed in the first parliamentary session after the August summer break. Second, the result of the vote was very uncertain. Finally, the time window between the law's approval (September 9) and the date the regulation became effective (September 19) is very narrow. These views are supported by the data, as we do not see an increase in the number of tenancy agreements signed before the implementation of the rent control system.

The law states that the regulation should be applied to municipalities with over 20,000 inhabitants with a tight housing market. A tight housing market is defined as one where at least one of the following conditions applies: i) in the last five years, the yearly rent growth is 3 points higher than inflation; ii) the average rent has increased at a higher rate than the regional average; iii) households spend more than 30% of the income on rents or iv) young households - under 35 years old - spend more than 30% of their income on rents.

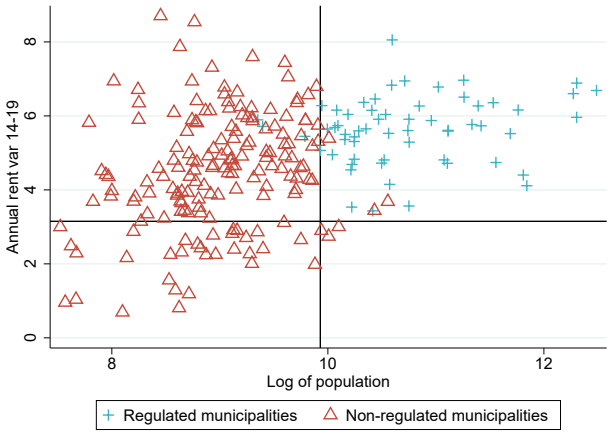
Panel a) in Figure 1 plots population size and annual rent variation in the 2015-2019 period for regulated and unregulated municipalities. The horizontal line represents the first market tightness criteria (annual rent growth 3 points higher than inflation, that is 3.15%), while the vertical line marks the 20,000 inhabitants threshold. Only 6 out of the 67 Catalan municipalities with more than 20,000 inhabitants are not regulated, indicating that the vast majority of municipalities with 20,000 inhabitants or more had a tight housing market according to the law. Compliance with the law's criteria is almost complete; the exception comes from a few municipalities that were regulated despite being below the 20,000 inhabitants threshold. The regulation was finally implemented in 61 Catalan municipalities, which account for 70% of the region's population.

Panel b of Figure 1 shows the geographical distribution of treated and control municipalities in Catalonia. Although treated municipalities are over-represented in the Barcelona

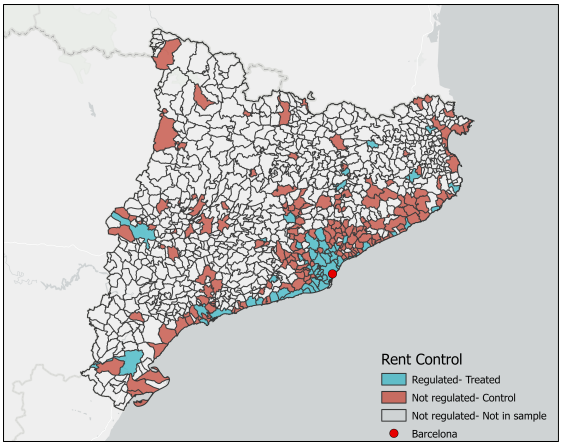
[4]The eight criteria are having an elevator, a parking space, a heating/cooling system, a pool, other building shared facilities, a janitor, a nice special view, or if it is already furnished.

metropolitan area, controlled and uncontrolled municipalities can be found in different parts of the region.

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 for the period 2014 to 2019. The horizontal line is the annual growth in rents needed to qualify as a tight housing market according to the first tightness criteria, that is, i) annual rent growth that exceeds inflation by 3%). The vertical line is the 20.000 inhabitants threshold. This graph excludes the city of Barcelona and small non-regulated municipalities with insufficient data as explained in the text. (b) shows the geography of regulated vs non regulated municipalities.

3 Data

We use rental market outcomes data from the Catalan Housing Agency. According to regional law, the tenant must give the landlord a security deposit equivalent to a monthly rent payment. The law stipulates that this deposit must be transferred to INCASOL, a regional public agency, within two months of the agreement's signature date. The deposit is kept at INCASOL until the rental agreement expires and the deposit can be returned to the tenant.

These data enables us to track changes in rental prices and in the number of agreements signed. The data is available at the municipality level for each quarter between 2016 and

2021. We work with average rental prices and the number of agreements signed. The data covers all municipalities with at least five tenancy agreements signed per quarter. The 230 municipalities that meet this criterion account for approximately 85% of the population living in rental units in Catalonia.

We complement this primary data set with other data sources at the municipality level. 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 people affected by Covid-19 temporally suspended contracts. This scheme, so-called ERTO in Spain, provides workers unemployment benefits while their contract is temporally suspended. We label this variable *Covid-suspended contracts*. Finally, we also include yearly local population inflows and outflows from the *Encuesta de Variaciones Residenciales*. Table 1, displays descriptive statistics of all the relevant variables.

Table 1: Descriptive statistics (fourth quarter of 2019)

Housing market	Mean	Std. Dev.	Min	Max
Rent (€/month)	613	197.4	302.7	1644.7
Tenancy agreement per 1,000 inh.	4.66	1.68	1.17	12.38
Labor market				
Unemployment per 100 inh.	4.97	1.16	2.09	8.19
New employ. contracts per 100 inh.	3.23	2.30	.53	18.80
Demographics				
Population	23172.9	36521.48	1850	264923
Population density	1527.8	2627.0	19.80	19451.03
Distance to Barcelona	57.10	40.09	6.38	185.64
Income ranking	35.49	7.26	25.46	79.63
Net migration	.015	.009	-.006	.052
In migration	.067	.016	.040	.136
Out migration	.052	.012	.025	.098

Notes: Descriptive statistics for our sample of 230 municipalities. Variables are measured in the third quarter of 2019.

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:

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

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.

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. 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 workers whose contracts are temporally suspended due to Covid-19, and the number of new employment contracts. All three variables are expressed relative to its population (i.e., per 100 inhabitants).

As explained in Section 2, we expect regulated municipalities to show higher rent growth in the years prior to the regulation, as this is one of the criteria used to select municipalities into treatment. Therefore, we allow for heterogeneous growth trajectories in our outcomes of interest in some specifications by including 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 will exploit is then limited to deviations from this trend.

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.

Throughout the analysis, we exclude the city of Barcelona because it is too different from the rest of the municipalities in several dimensions. 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). 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 (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. (2021) show that this phenomenon was quantitatively important in Lisbon, where the increased supply of housing units in the residential market reduced rental prices. Therefore, to deal with this and other confounding factors, we leave the municipality of Barcelona out of the estimation sample.

5 Main results

5.1 Graphical evidence

Before moving to the econometric results of equations 1 and 2, in Figure 2 we plot the evolution of rents and number of agreements signed in regulated versus non-regulated municipalities. Figure 2a shows that municipalities with a regulated rental market have higher rents before and after the adoption of the regulation. Nevertheless, both groups of municipalities experienced rent increases before the implementation of rent controls, with the highest prices occurring in the third quarter of 2020. Perhaps surprisingly, rents increased despite the pandemic in the second and third quarters of 2020. After the adoption of the rent control system, rents drop in the regulated group while they stay constant in the non-regulated group. This suggests that rent control was effective in reducing rental prices in treated municipalities.

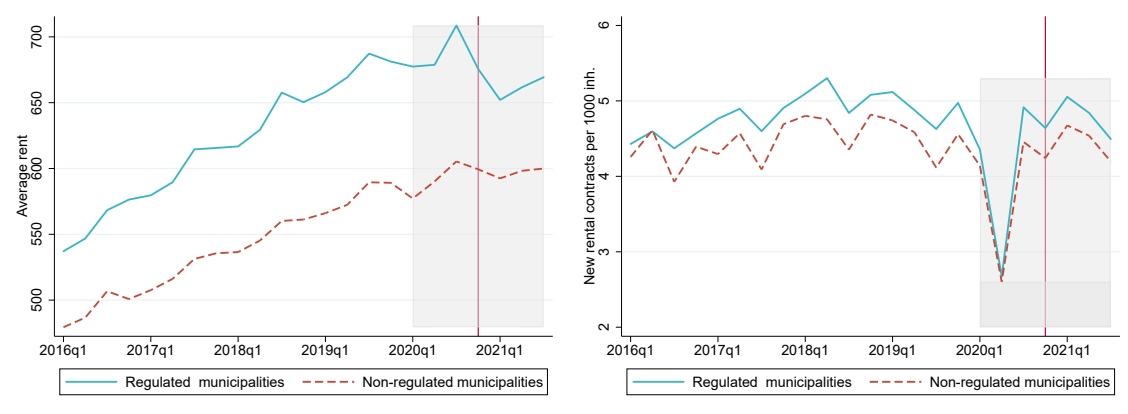
As explained above, the impact of the Covid-19 crisis complicates our analysis as it partly overlaps in time with the application of the rent control system. In this respect, it is interesting to note that the rent price gap between regulated and non-regulated municipalities widens after the lockdown in the third quarter of 2020 and is only reduced in the last quarter of 2020, coinciding with the introduction of the regulation.

Figure 2a seems to indicate that, in the years that precede regulation, rental price growth was slightly higher in regulated municipalities. This differential time trend coincides with expectations. As shown in Figure 1a, past growth in rental prices is one of the variables used to select which municipalities are affected by the rent control system. This differential growth between regulated and non-regulated municipalities suggests that allowing for heterogeneous time trends in Equation 1 will matter for our econometric estimates.

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 is also remarkably similar for the two groups despite the massive reduction in tenancy agreements signed 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. There is no visual indication that the rent control's introduction system in the fall of 2020 has widened or reduced the gap in tenancy agreements between regulated and non-regulated municipalities. Overall, this suggests that the rent control's introduction

did not lead to a sizable reduction in the supply of rental housing, at least in the short run.

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 (61 municipalities, excluding Barcelona) and non-regulated municipalities (169 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.

5.2 Main 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 our outcome of interest, controlling only for time and municipality fixed effects. Column 2 further includes the time-varying control variables (e.g. $X_{m,t}$). The results obtained when we additionally include municipality-specific linear time trends are shown in column 3 (no control variables) and column 4, which is the more complete and our preferred specification.

The results of all specifications in Table 2 suggest that the rent control system decreased average rents. Allowing for different rental growth paths through municipality-specific linear time trends increases the estimated effect of the rent control policy. This result is in line with our prior expectations. As discussed above, Figure 2 shows some indication that rental price growth was higher in regulated municipalities prior to the passage of the rent control system. Therefore, omitting the linear time trends in the specification will underestimate the treatment effect.

In contrast, the inclusion of variables reflecting the local labor market dynamics (columns 2 and 4) does not affect our estimates of interest. In Figure A1, deferred to the Annex, we plot the evolution of these labor market performance measures for regulated and non-regulated municipalities. The analysis shows no significant differences between the two groups of municipalities, suggesting that labor markets of regulated and non-regulated municipalities fared similarly before and after the Covid-19 shock. In Section 6, we will discuss additional robustness tests that deal with potential biases caused by the Covid-19 shock.

The results of our preferred specification (column 4) indicate that average rental prices decreased by 6.2% in regulated municipalities compared to non-regulated municipalities.

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

	Panel A: (Log) average rents			
	(1)	(2)	(3)	(4)
RentControl × *Post*	-0.038***	-0.038***	-0.062***	-0.062***
	(0.005)	(0.005)	(0.006)	(0.006)
Unemployment		-0.002		0.003
		(0.004)		(0.004)
Covid suspended contracts		0.000		0.000
		(0.000)		(0.000)
New employ. contracts		-0.000		0.000
		(0.001)		(0.001)
	Panel B: (Log) Tenancy agreements per 1000 inhabitants			
RentControl × *Post*	0.009	0.012	0.007	-0.004
	(0.020)	(0.020)	(0.021)	(0.021)
Unemployment		0.046***		0.068***
		(0.012)		(0.012)
Covid suspended contracts		-0.000		-0.001
		(0.002)		(0.002)
New employ. contracts		-0.007*		-0.005
		(0.004)		(0.004)
Observations	5,222	5,222	5,222	5,222
Time FE	X	X	X	X
Mun FE	X	X	X	X
Linear time trend	-	-	Mun	Mun
Sample	All municipalities excluding Barcelona			

Notes: Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parenthesis, are clustered at the municipality level.

To interpret the economic magnitude of this effect, we refer to the average rent in regulated markets in the third quarter of 2019. A 6.2% reduction amounts to €42 in monthly rent or €506 annually. This decrease in rents represents a considerably high reduction if we take into account that the average household annual income for households in Catalunya is €33,321[5]. In particular, the reduction represents 2% of the annual income of households living in rental units in the region[6]

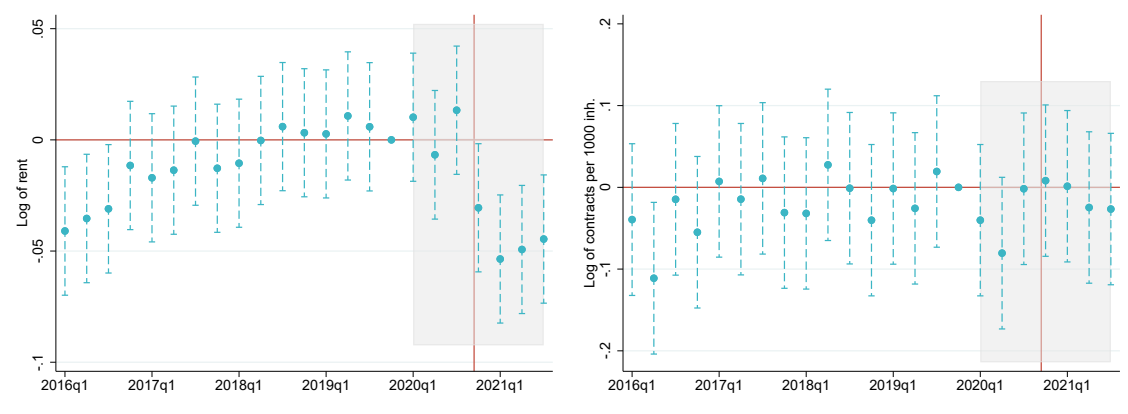
The results of panel B of Table 2 do not indicate that the introduction of the rent control system has affected the number of tenancy agreements signed in regulated municipalities. Coefficients remain close to zero in all specifications and are not statistically significant. This is in line with the graphical evidence shown in Figure 2. In column 4, the coefficient indicates that the implementation of the rent control reduced the number of tenancy agreements by only 0.4 percent. The standard errors in panel B are larger than those in panel A, indicating that quantity effects of rent control are less precisely estimated than price effects. The 95% confidence interval ranges from -4.5% to 3.6%. Therefore, we can rule out large supply reductions caused by the policy.

[5]Data from Idescat for 2019.

[6]The median income for rental households in the NUTS1 region that compromises Catalunya and Valencia is of €24,666. Data from the EU-SILC for 2019.

Additionally, we show the results of the event study regressions specified in Equation 2. Figure 3 plots the coefficients of the interaction terms between the treatment indicator and a set of quarter dummies. Results are shown for rents (Panel a) and new tenancy agreements (Panel b). The shaded area 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: Outcome variables are (log of) average rents and log 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.

Panel a) in Figure 3 confirms that rents grew slightly more in regulated than in non-regulated markets prior to the adoption of the rent control policy. This reinforces the view that municipality-specific time trends are needed in equation 1 in order to identify the causal effect of the rent-control system. Looking at the arrival of the Covid-19 crisis (first and second quarters of 2020), we see that regulated and non-regulated municipalities are not statistically different from each other. This suggests that the Covid-19 shock did not have a heterogeneous impact on the housing markets of the two groups of municipalities. In contrast, the coefficients start to become negative and statistically significant right after the fourth quarter of 2020, when the regulation is effectively in place. These results indicate that the implementation of rent control led to an immediate drop in rents in regulated municipalities of approximately 5%. The drop remains roughly stable over the four subsequent quarters.

The results reported in Panel (b) imply 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. This is also true in the second quarter of 2020, despite the significant drop in agreements caused by the lockdown (see Figure 2b). In contrast to the results found for rental prices, the regulation, which came into place in the fourth quarter of 2020, does not seem to have affected the number of agreements signed.

The event-study framework can be used to assess if there is evidence of anticipatory policy effects. As discussed in Section 2, these effects are likely to be small or nonexistent given the 3-week window of time in which the policy was discussed, approved, and came into effect. Consistent with this, the results of panel b) in Figure 3 do not show any increase in tenancy agreements signed in the third quarter of 2020. This indicates that there was not a large surge in new agreements signed in September 2020 to avoid the new

rent control system.

6 Robustness checks and the potential confounding effects of Covid-19

In this section, we run several robustness tests to address additional identification concerns. In the first set of robustness checks, we select alternative samples of regulated and non-regulated municipalities that are more similar to each other. More specifically, we first restrict our sample to municipalities between 10,000 and 60,000 inhabitants. Second, we exclude touristic municipalities from our sample by removing those with a high number of short-term rental apartments. Third, we restrict our sample to municipalities that experience similar pre-regulation rent increases.

In a second set of checks, we turn to potential confounding effects due to the Covid-19 pandemic. First, to assess the effects of working from home during the pandemic, we allow for potential differential impacts of Covid-19 on municipalities depending on its distance to Barcelona and its population size. Finally, we include annual migration inflows and outflows to assess if migration is confounding our findings.

Population size — Rent controlled municipalities are larger than non-regulated ones (see Figure 1a in Section 2). One might worry that, despite municipality-specific fixed effects and linear time trends, shocks to large municipalities might be different from shocks to smaller ones. Even if we are already excluding the city of Barcelona, here we follow this path further by restricting the sample to municipalities between 10,000 and 60,000 inhabitants. This restriction leaves us with 40 municipalities in the treatment group and 59 in the control group.

We report the results of running our preferred estimation (column 4 of Table 2) on this sub-sample in column 2 of Table 3. The results are qualitatively and quantitatively similar to those of our baseline strategy. The coefficient indicates that rents in regulated municipalities were reduced by 5.2% in relation to non-regulated municipalities. Panel B, column 2, shows no significant effects of implementing the rent control system on the number of tenancy agreements signed.

Tourism — The pandemic has had a significant effect on the tourism sector, causing the number of worldwide tourists to drop abruptly. The region of Catalonia was no exception to this: the number of tourists in the region in 2020 was 80% lower compared to 2019[7]. As we argued above, tourism can impact the residential housing market through short-term rental housing (Batalha et al., 2021). 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[8]. Therefore, we drop all municipalities with more than 1,000 short-term rental licenses as a robustness exercise. This leaves us with 53 treated municipalities and 153 municipalities in the control group[9].

[7]Source: Movimientos Turísticos en Fronteras, INE

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

[9]In particular, this excludes from the treatment group the municipalities of Blanes, Calafell, Palagrugelll, Salou, Sant Feliu de Guixols, Sitges, and Tarragona. Barcelona was already excluded from the initial

The results reported in column 3 of Table 3 are in line with our baseline results and show a decrease in average rents of 6.0% 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 rent control.

Tight market — We have previously discussed the potential bias driven by the existing differences in pre-regulation rent growth. An alternative way to address this issue (besides municipality-specific time trends) is to restrict the sample to municipalities with a more similar pre-regulation rent growth. In particular, we restrict our control group to municipalities with an average rent growth higher than the threshold of 3.15% (the first of the criteria specified in the law to qualify as a tight market). This is, we restrict our control sample to municipalities in the top half of Figure 1a. Therefore, in this exercise, we compare two groups of municipalities similar in previous rent growth but that differ in population size (above or below the 20,000 inhabitants threshold). This restriction leaves us with 56 regulated municipalities and 100 non-regulated municipalities.

Results of this exercise are reported in column 4 of Table 3. Yet again, the coefficient for rents remains negative and strongly significant, while the coefficient of the number of tenancy agreements is not significant and very close to zero.

Table 3: Impact of rent control on rents and tenancy agreements: Robustness tests on different sub-samples.

	Panel A: (Log) Rents			
	(1)	(2)	(3)	(4)
$RentControl \times Post$	-0.062***	-0.052***	-0.060***	-0.050***
	(0.006)	(0.008)	(0.007)	(0.007)
	Panel B: (Log) Tenancy agreements per 1000 inhabitants			
$RentControl \times Post$	-0.004	-0.036	-0.002	-0.007
	(0.021)	(0.027)	(0.023)	(0.023)
Sample	Baseline	10,000 to 60,000	Vacation homes	Tight market
Observations	5,222	2,178	4,513	3,557
Time FE	X	X	X	X
Mun FE	X	X	X	X
LM controls	X	X	X	X
Linear time trend	Mun	Mun	Mun	Mun

Notes: Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parenthesis, are clustered at the municipality level. Results are based on Equation 1. "10,000 to 60,000" refers to sample that only includes cities between 10,000 and 60,000 inhabitants. "Vacation homes" refers to sample with cities with less than 1,000 vacation homes."Tight market" refers to those cities that did comply with the tight market criterion (that rents in the last five years increased more than 3.15% annually).

Working from home and "donut" effects— One way through which the Covid-19 pandemic could have affected the housing market is through the increase of 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 Bloom

sample.

and Ramani (2021) show that, in US cities, Covid-19 has reduced housing rents and prices in central city locations relative to the suburbs[10]. This effect has been labeled the "donut" effect, and it could confound our findings if the geography of regulated and non-regulated partly overlaps with this effect.

We deal with this identification threat by augmenting our preferred specification (column 4 in Table 2 reproduced here in column 1 to ease comparability). We include interaction terms between distance to Barcelona and the Covid-19 dummy, which takes the value of one for pandemic quarters (first quarter of 2020 and subsequent quarters). Also, we interact the Covid-19 dummy with the linear distance to Barcelona (column 2) and with a quadratic polynomial of this distance (column 3). Finally, we interact the Covid-19 dummy with categorical distance variables (0-20, 20-35, 35-55, 55-91, >92 km) in column 4. The results are presented in Table A1.

The results are very similar to our baseline findings. In particular, the drop in rents caused by rent control is 6.1%, and there is no significant effect on the number of tenancy agreements signed. This suggests that the "donut" effect, that is, the flattening of the city center - suburbs rent gradient, does not seem to be driving our results.

Similarly, one might also be worried that the Covid-19 impact on local housing markets could vary by municipality size if WFH increases the demand for larger 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 interaction terms between a Covid-19 indicator and different measures of population size. More specifically, we interact the Covid-19 dummy with population (column 2 in Table A2), with a quadratic polynomial of population (column 3) and with categorical variables in column 4 (0-5000, 5001-10,000, 10,001-20,000, 20,001-50,000, 50,0001-10,000, and over 100,000 inhabitants). These augmented regressions leave our coefficients of interest virtually unchanged.

Accounting for local migration flows — 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)[11].

One limitation of this exercise is that population inflows and outflows are measured annually. Moreover, the data is not available for 2021, so we have replaced the 2021 missing values with 2020 values. The results are reported in the second column of Table A3, while column 1 reproduces the results of our preferred specification of Table 2. In column 3, we do the same exercise, but we allow the impacts of population inflows and outflows to differ between pre-and Covid-19 times. Our findings remain largely unaffected, suggesting

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

[11] Figure A2 plots yearly in-migration, out-migration and net-migration rates for regulated and non-regulated municipalities in our period of study. The Covid-19 shock has caused a drop in in-migration and out-migration rates in regulated and non-regulated municipalities. In particular, the drop in the in-migration rate is larger in the regulated group. However, this differential shock in in-migration does not seem to confound our estimates, as can be seen in Table A3.

again 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 findings.

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 the number of tenancy agreements signed in regulated vs. 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 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 tenancy agreements signed, suggesting that supply shortages in the short run are not necessarily substantial.

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.

References

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, 661–717.

Batalha, M., D. Gonçalves, S. Peralta, and J. P. dos Santos (2021): "The virus that devastated tourism : the impact of Covid-19 on the housing market," Working paper, CEPR.

Bloom, N. and A. Ramani (2021): "The donut effect: How covid-19 shapes real estate," Working paper, SIEPR Policy Brief.

Breidenbach, P., L. Eilers, and J. Fries (2021): "Temporal dynamics of rent regulations – The case of the German rent control," *Regional Science and Urban Economics*, 103737.

Delventhal, M. J., E. Kwon, and A. Parkhomenko (2021): "JUE Insight: How do cities change when we work from home?" *Journal of Urban Economics*, 103331.

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, 3365–94.

Garcia-López, M.-À., J. Jofre-Monseny, R. Martínez-Mazza, and M. Segú (2020): "Do short-term rental platforms affect housing markets? Evidence from Airbnb in Barcelona," *Journal of Urban Economics*, 119, 103278.

Glaeser, E. L. and E. F. P. Luttmer (2003): "The Misallocation of Housing Under Rent Control," *American Economic Review*, 93, 1027–1046.

Gupta, A., V. Mittal, J. Peeters, and S. Van Nieuwerburgh (2021): "Flattening the curve: Pandemic-Induced revaluation of urban real estate," *Journal of Financial Economics*.

Mense, A., C. Michelsen, and K. A. Kholodilin (2019): "The effects of second-generation rent control on land values," in *AEA Papers and Proceedings*, vol. 109, 385–88.

OECD (2021): "OECD Affordable Housing Database," `https://www.oecd.org/housing/data/affordable-housing-database/housing-policies.htm`, [Online; accessed 1-November-2021].

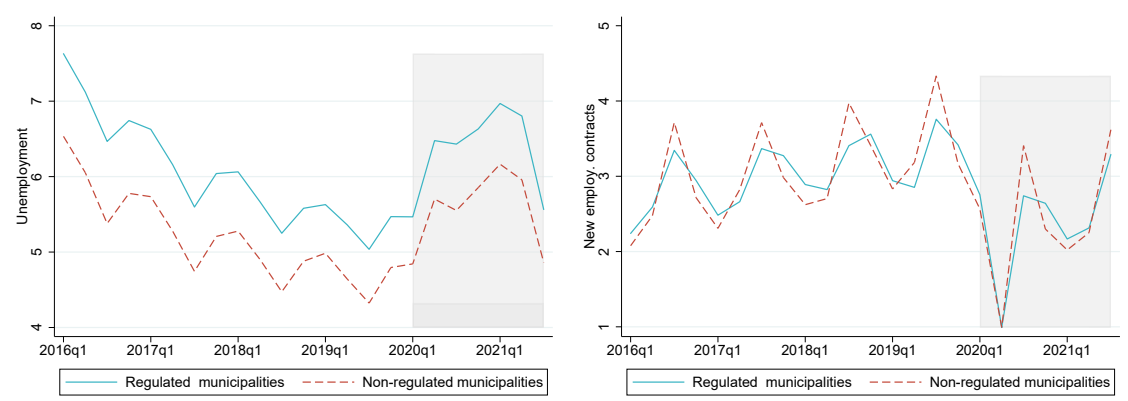
Rosenthal, S. S., W. C. Strange, and J. A. Urrego (2021): "JUE insight: Are city centers losing their appeal? Commercial real estate, urban spatial structure, and COVID-19," *Journal of Urban Economics*, 103381.

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.

8 Appendix

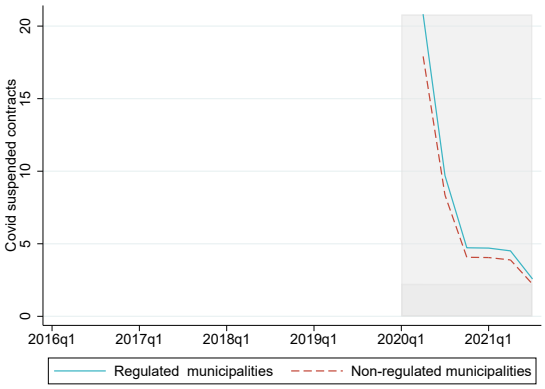
A Local labor market dynamics and migration for regulated and non-regulated municipalities.

Figure A1: Evolution of labor market variables



(a) Unemployment per 100 inhabitants.

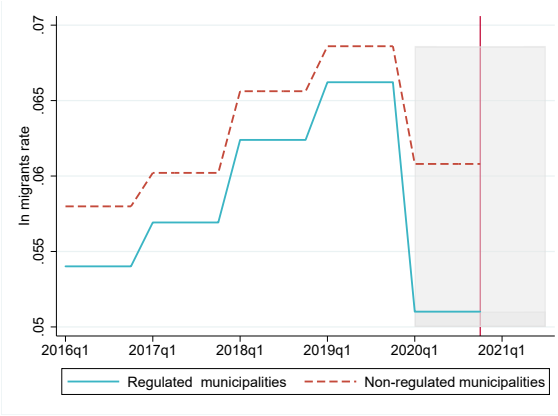
(b) Employment contracts per 100 inhabitants.



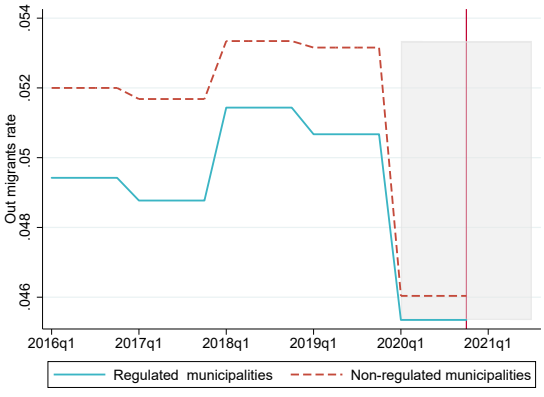
(c) Covid-suspended contracts per 100 inhabitants.

Notes: Variables as defined in the text. The shaded area indicates the beginning of the pandemic in the first quarter of 2020.

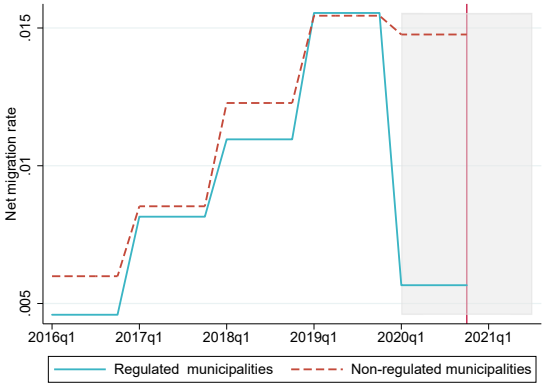
Figure A2: Migration patterns at the municipality level



(a) In migration rate- No bcn



(b) Out migration rate- No bcn



(c) Net migration rate- No bcn

Notes: In both cases the vertical line indicates the implementation of rent control. The shaded area indicates the beginning of the pandemic in the first quarter of 2020.

B Robustness checks

Table A1: Impact of rent control on rents and tenancy agreements: Robustness test with distance to Barcelona and Covid-19 interactions.

	Panel A: (Log) Rents			
	(1)	(2)	(3)	(4)
$RentControl \times Post$	-0.062***	-0.059***	-0.060***	-0.061***
	(0.007)	(0.006)	(0.006)	(0.006)
$DistBCN \times Covid$		0.000***	-0.000	
		(0.000)	(0.000)	
$DistBCN^2 \times Covid$			0.000	
			(0.000)	
$DistBCN_1 \times Covid$				-0.016
				(0.011)
$DistBCN_2 \times Covid$				-0.001
				(0.006)
$DistBCN_3 \times Covid$				0.002
				(0.004)
$DistBCN_4 \times Covid$				0.007**
				(0.003)
	Panel B: (Log) Tenancy agreements per 1000 inhabitants			
	(1)	(2)	(3)	(4)
$RentControl \times Post$	0.000	0.005	0.007	0.008
	(0.021)	(0.021)	(0.021)	(0.021)
$DistBCN \times Covid$		0.001	0.002	
		(0.000)	(0.001)	
$DistBCN^2 \times Covid$			-0.000	
			(0.000)	
$DistBCN_1 \times Covid$				0.030
				(0.032)
$DistBCN_2 \times Covid$				0.039**
				(0.016)
$DistBCN_3 \times Covid$				0.010
				(0.010)
$DistBCN_4 \times Covid$				0.020**
				(0.009)
Observations	4,996	4,996	4,996	4,996
LM Controls	X	X	X	X
Time FE	X	X	X	X
Mun FE	X	X	X	X
Trend	Mun	Mun	Mun	Mun
Sample	All cities excluding Barcelona			

Notes: Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parenthesis, are clustered at the municipality level. Use equally sized distances bins: 0-20, 20-35, 35-55, 55-91, 92-185. Base group 0-20.

Table A2: Impact of rent control on rents and tenancy agreements: Robustness test with municipality size and Covid-19 interactions.

Panel A: (Log) Rents				
	(1)	(2)	(3)	
$RentControl \times Post$	-0.062***	-0.060***	-0.058***	-0.059***
	(0.006)	(0.007)	(0.007)	(0.007)
$MunSize \times Covid$		-0.000***	-0.000**	
		(0.000)	(0.000)	
$MunSize^2 \times Covid$			0.000*	
			(0.000)	
$MunSize_1 \times Covid$				0.029
				(0.019)
$MunSize_2 \times Covid$				0.005
				(0.012)
$MunSize_3 \times Covid$				0.002
				(0.012)
$MunSize_4 \times Covid$				0.004
				(0.011)
$MunSize_5 \times Covid$				-0.003
				(0.011)
Panel B: (Log) Tenancy agreements per 1000 inhabitants				
	(1)	(2)	(3)	
$RentControl \times Post$	-0.004	0.008	0.014	0.018
	(0.021)	(0.021)	(0.021)	(0.021)
$MunSize \times Covid$		-0.000***	-0.000**	
		(0.000)	(0.000)	
$MunSize^2 \times Covid$			0.000*	
			(0.000)	
$MunSize_1 \times Covid$				0.079*
				(0.045)
$MunSize_2 \times Covid$				0.080**
				(0.033)
$MunSize_3 \times Covid$				0.074**
				(0.031)
$MunSize_4 \times Covid$				0.017
				(0.027)
$MunSize_5 \times Covid$				-0.008
				(0.034)
Observations	5,244	5,244	5,244	5,244
Time FE	X	X	X	X
Mun FE	X	X	X	X
LM controls	X	X	X	X
Trend	Mun	Mun	Mun	Mun
Sample	All cities excluding Barcelona			

Notes: Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parenthesis, are clustered at the municipality level. Bin for muncipality 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.

Table A3: Impact of rent control on rents and tenancy agreements: Controlling for migration.

	Panel A: (Log) Rents		
	(1)	(2)	(3)
$RentControl \times Post$	-0.062***	-0.061***	-0.060***
	(0.006)	(0.007)	(0.007)
$In - migration$		0.491	0.379
		(0.318)	(0.295)
$Out - migration$		-0.284	-0.232
		(0.258)	(0.271)
		(0.742)	(0.764)
$In - migration \times Covid$			0.456
			(0.433)
$Out - migration \times Covid$			-0.298
			(0.554)
	Panel B: (Log) Tenancy agreements per 1000 inhabitants		
	(1)	(2)	(3)
$RentControl \times Post$	-0.004	0.004	0.004
	(0.021)	(0.021)	(0.021)
$In - migration$		2.861***	2.660***
		(0.756)	(0.771)
$Out - migration$		-0.829	-0.763
		(0.742)	(0.764)
$In - migration \times Covid$			0.314
			(0.901)
$Out - migration \times Covid$			-1.422
			(1.346)
Observations	5,222	5,222	5,222
Time FE	X	X	X
Mun FE	X	X	X
LM controls	X	X	X
Trend	Mun	Mun	Mun
Sample	All cities excluding Barcelona		

Notes: Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parenthesis, are clustered at the municipality level.

2018

2018/1, Boadway, R.; Pestieau, P.: "The tenuous case for an annual wealth tax"
2018/2, Garcia-López, M.À.: "All roads lead to Rome ... and to sprawl? Evidence from European cities"
2018/3, Daniele, G.; Galletta, S.; Geys, B.: "Abandon ship? Party brands and politicians' responses to a political scandal"
2018/4, Cavalcanti, F.; Daniele, G.; Galletta, S.: "Popularity shocks and political selection"
2018/5, Naval, J.; Silva, J. I.; Vázquez-Grenno, J.: "Employment effects of on-the-job human capital acquisition"
2018/6, Agrawal, D. R.; Foremny, D.: "Relocation of the rich: migration in response to top tax rate changes from spanish reforms"
2018/7, García-Quevedo, J.; Kesidou, E.; Martínez-Ros, E.: "Inter-industry differences in organisational eco-innovation: a panel data study"
2018/8, Aastveit, K. A.; Anundsen, A. K.: "Asymmetric effects of monetary policy in regional housing markets"
2018/9, Curci, F.; Masera, F.: "Flight from urban blight: lead poisoning, crime and suburbanization"
2018/10, Grossi, L.; Nan, F.: "The influence of renewables on electricity price forecasting: a robust approach"
2018/11, Fleckinger, P.; Glachant, M.; Tamokoué Kamga, P.-H.: "Energy performance certificates and investments in building energy efficiency: a theoretical analysis"
2018/12, van den Bergh, J. C.J.M.; Angelsen, A.; Baranzini, A.; Botzen, W.J. W.; Carattini, S.; Drews, S.; Dunlop, T.; Galbraith, E.; Gsottbauer, E.; Howarth, R. B.; Padilla, E.; Roca, J.; Schmidt, R.: "Parallel tracks towards a global treaty on carbon pricing"
2018/13, Ayllón, S.; Nollenberger, N.: "The unequal opportunity for skills acquisition during the Great Recession in Europe"
2018/14, Firmino, J.: "Class composition effects and school welfare: evidence from Portugal using panel data"
2018/15, Durán-Cabré, J. M.; Esteller-Moré, A.; Mas-Montserrat, M.; Salvadori, L.: "La brecha fiscal: estudio y aplicación a los impuestos sobre la riqueza"
2018/16, Montolio, D.; Tur-Prats, A.: "Long-lasting social capital and its impact on economic development: the legacy of the commons"
2018/17, Garcia-López, M. À.; Moreno-Monroy, A. I.: "Income segregation in monocentric and polycentric cities: does urban form really matter?"
2018/18, Di Cosmo, V.; Trujillo-Baute, E.: "From forward to spot prices: producers, retailers and loss averse consumers in electricity markets"
2018/19, Brachowicz Quintanilla, N.; Vall Castelló, J.: "Is changing the minimum legal drinking age an effective policy tool?"
2018/20, Nerea Gómez-Fernández, Mauro Mediavilla: "Do information and communication technologies (ICT) improve educational outcomes? Evidence for Spain in PISA 2015"
2018/21, Montolio, D.; Taberner, P. A.: "Gender differences under test pressure and their impact on academic performance: a quasi-experimental design"
2018/22, Rice, C.; Vall Castelló, J.: "Hit where it hurts – healthcare access and intimate partner violence"
2018/23, Ramos, R.; Sanromá, E.; Simón, H.: "Wage differentials by bargaining regime in Spain (2002-2014). An analysis using matched employer-employee data"

2019

2019/1, Mediavilla, M.; Mancebón, M. J.; Gómez-Sancho, J. M.; Pires Jiménez, L.: "Bilingual education and school choice: a case study of public secondary schools in the Spanish region of Madrid"
2019/2, Brutti, Z.; Montolio, D.: "Preventing criminal minds: early education access and adult offending behavior"
2019/3, Montalvo, J. G.; Piolatto, A.; Raya, J.: "Transaction-tax evasion in the housing market"
2019/4, Durán-Cabré, J.M.; Esteller-Moré, A.; Mas-Montserrat, M.: "Behavioural responses to the re)introduction of wealth taxes. Evidence from Spain"
2019/5, Garcia-López, M.A.; Jofre-Monseny, J.; Martínez Mazza, R.; Segú, M.: "Do short-term rental platforms affect housing markets? Evidence from Airbnb in Barcelona"
2019/6, Domínguez, M.; Montolio, D.: "Bolstering community ties as a means of reducing crime"
2019/7, García-Quevedo, J.; Massa-Camps, X.: "Why firms invest (or not) in energy efficiency? A review of the econometric evidence"
2019/8, Gómez-Fernández, N.; Mediavilla, M.: "What are the factors that influence the use of ICT in the classroom by teachers? Evidence from a census survey in Madrid"
2019/9, Arribas-Bel, D.; Garcia-López, M.A.; Viladecans-Marsal, E.: "The long-run redistributive power of the net wealth tax"
2019/10, Arribas-Bel, D.; Garcia-López, M.A.; Viladecans-Marsal, E.: "Building(s and) cities: delineating urban areas with a machine learning algorithm"

2019/11, Bordignon, M.; Gamalerio, M.; Slerca, E.; Turati, G.: "Stop invasion! The electoral tipping point in anti-immigrant voting"

2020

2020/01, Daniele, G.; Piolatto, A.; Sas, W.: "Does the winner take it all? Redistributive policies and political extremism"
2020/02, Sanz, C.; Solé-Ollé, A.; Sorribas-Navarro, P.: "Betrayed by the elites: how corruption amplifies the political effects of recessions"
2020/03, Farré, L.; Jofre-Monseny; J., Torrecillas, J.: "Commuting time and the gender gap in labor market participation"
2020/04, Romarri, A.: "Does the internet change attitudes towards immigrants? Evidence from Spain"
2020/05, Magontier, P.: "Does media coverage affect governments'preparation for natural disasters?"
2020/06, McDougal, T.L.; Montolio, D.; Brauer, J.: "Modeling the U.S. firearms market: the effects of civilian stocks, crime, legislation, and armed conflict"
2020/07, Veneri, P.; Comandon, A.; Garcia-López, M.A.; Daams, M.N.: "What do divided cities have in common? An international comparison of income segregation"
2020/08, Piolatto, A.: "'Information doesn't want to be free': informational shocks with anonymous online platforms"
2020/09, Marie, O.; Vall Castello, J.: "If sick-leave becomes more costly, will I go back to work? Could it be too soon?"
2020/10, Montolio, D.; Oliveira, C.: "Law incentives for juvenile recruiting by drug trafficking gangs: empirical evidence from Rio de Janeiro"
2020/11, Garcia-López, M.A.; Pasidis, I.; Viladecans-Marsal, E.: "Congestion in highways when tolls and railroads matter: evidence from European cities"
2020/12, Ferraresi, M.; Mazzanti, M.; Mazzarano, M.; Rizzo, L.; Secomandi, R.: "Political cycles and yardstick competition in the recycling of waste. evidence from Italian provinces"
2020/13, Beigelman, M.; Vall Castelló, J.: "COVID-19 and help-seeking behavior for intimate partner violence victims"
2020/14, Martínez-Mazza, R.: "Mom, Dad: I'm staying" initial labor market conditions, housing markets, and welfare"
2020/15, Agrawal, D.; Foremny, D.; Martínez-Toledano, C.: "*Paraísos fiscales*, wealth taxation, and mobility"
2020/16, Garcia-Pérez, J.I.; Serrano-Alarcón, M.; Vall Castelló, J.: "Long-term unemployment subsidies and middle-age disadvantaged workers' health"

2021

2021/01, Rusteholz, G.; Mediavilla, M.; Pires, L.: "Impact of bullying on academic performance. A case study for the community of Madrid"
2021/02, Amuedo-Dorantes, C.; Rivera-Garrido, N.; Vall Castelló, J.: "Reforming the provision of cross-border medical care evidence from Spain"
2021/03, Domínguez, M.: "Sweeping up gangs: The effects of tough-on-crime policies from a network approach"
2021/04, Arenas, A.; Calsamiglia, C.; Loviglio, A.: "What is at stake without high-stakes exams? Students' evaluation and admission to college at the time of COVID-19"
2021/05, Armijos Bravo, G.; Vall Castelló, J.: "Terrorist attacks, Islamophobia and newborns'health"
2021/06, Asensio, J.; Matas, A.: "The impact of 'competition for the market'regulatory designs on intercity bus prices"
2021/07, Boffa, F.; Cavalcanti, F.; Piolatto, A.: "Ignorance is bliss: voter education and alignment in distributive politics"

2022

2022/01, Montolio, D.; Piolatto, A.; Salvadori, L.: "Financing public education when altruistic agents have retirement concerns"

ieb@ub.edu
www.ieb.edu

Cities

IEB Working Paper