The Perverse Effects of Rent Control: Evidence From a Large-scale Stringent Regulation in Catalonia*

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Abstract

Using a stringent large-scale policy intervention that capped rental prices in more than 60 municipalities in Catalonia, Spain, we find that rent control initially reduces average rents, but this effect vanishes after one year due to a 30%-32% decline in rental housing supply. House sales increase by 13%-18% while house prices decrease by 2.3%-3.7%. The reduction in both rental and house prices stems from effects at the bottom of the respective distributions, with no significant effects on rents at the top. Conversely, expensive houses experience significant price increases. We estimate that working class properties lost 1 billion euros in value as a consequence of rent control, a significant larger amount than the 8 million euros gains from reduced rents for low- and medium-income tenants. Inequality also widened because house values at the top quartile of the distribution increased by almost 1.1 billion euros.

Keywords: rent control, house prices, inequality, housing affordability.

JEL Classification: R31, R38, R52.

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1 Introduction

The contemporary debate over policy responses to the pressing housing affordability crisis has gained traction, with rent control measures attracting significant attention across the globe. Various forms of rent control policies, all of which aim to limit rent increases in some way, have been adopted in the developed economies across the North Atlantic, including the United States, Canada, Germany, the Netherlands, France, the United Kingdom, and Spain. Although the provisions of these policies are varied, most commonly they regulate rent increases during the tenancy and impose restrictions on rents for new contracts. Furthermore, many policies include provisions that limit increases after evictions or restrict the eviction process itself.

Despite some existing theoretical and empirical analysis (see e.g., Arnott, 1995; Glaeser and Luttmer, 2003; O'Sullivan, 2007; McDonald and McMillen, 2010; Favilukis, Mabille and Van Nieuwerburgh, 2022; Mense, Michelsen and Kholodilin, 2023), evidence on the effects of rent control remains limited, primarily due to data availability and the specificity of the settings examined. Existing studies have primarily focused on the effects of rent control regulations in specific American municipalities, including Cambridge, Massachusetts (Sims, 2007, 2011; Autor, Palmer and Pathak, 2014, 2019), San Francisco, California (Diamond, McQuade and Qian, 2019; Asquith, 2019; Geddes and Holz, 2022), and St. Paul, Minnesota (Ahern and Giacoletti, 2022), with the latter being the only study investigating the impact on surrounding property prices following the imposition of new rent control legislation. Conversely, to the best of our knowledge, only Mense, Michelsen and Kholodilin (2023) has analyzed the introduction of a large-scale rent control regulation across multiple municipalities, but gaps remain in understanding spillovers between rental and owner-occupied markets, the effects on house prices, and the heterogeneity in the impacts of rent control adoption.

In this paper, we examine the impact of a stringent rent control policy implemented in over sixty municipalities in Catalonia, Spain, from September 2020. We assess the effects of the new regulation on the dynamics of rental and sales prices, with a focus on the differential impacts across the respective price distributions. The policy, designed to curb rent increases, caps the initial rent for new contracts at the reference price for similar properties in the same area and limits rent increases on renewals if the property was rented in the last five years. The new rent control regulation applies to all rental contracts signed on or after September 22, 2020, in areas declared as tense housing markets, including Barcelona and other major municipalities with over 20,000 inhabitants.[1] However, in March 2022, the Spanish Supreme Court declared the entire law void. This legal enactment provides a unique opportunity to study the effects of rent control across a wide range of municipalities over a specific period.

First, to rationalize our empirical findings on rent control, we introduce a theoretical model of rent control that builds on Favilukis, Mabille and Van Nieuwerburgh (2022). Our model consists of two regions, each featuring both rental and owner-occupied housing markets. However, in contrast to the lottery scheme in Favilukis, Mabille and Van Nieuwerburgh (2022), only one region is subject to rent control in our model and all rental units within this region are regulated ("stringent rent control"). Within both the controlled and uncontrolled regions,

[1] After one year, two cities ceased to be subject to the rent control law and nine municipalities were added to the list of affected municipalities.

there is a continuum of households and one investor who allocates his wealth among a risk-free bond, owner-occupied housing, and rental housing. The investor also faces a short sale constraint that limits his ability to use debt for purchasing housing. The participation constraint for households differs between regions due to variations in the relative price of rental housing compared to owner-occupied housing. We consider a dynamic recursive contract between households and the investor, incorporating the participation constraint and the market feasibility conditions into the investor's optimality problem to derive the efficient allocation for this economy.

Having introduced the theoretical framework, we then proceed to our empirical analysis of the Catalan rent control regulation, drawing on rich data from Fotocasa, one of the leading real estate online marketplaces in Spain. We have access to all posted rental and sales advertisements active between June 2020 and January 2022. This data set provides us with detailed information on housing rents and sales prices, as well as property characteristics and exact location. To identify the effects of rent control, we combine Difference-in-Differences estimation and event study designs. Leveraging the spatial and temporal variations generated by the rent control law, we isolate the causal impact of rent control by comparing trends in rental and house prices between 'always treated municipalities'—those that remained subject to rent control throughout the entire period of application of the law—and the selected group of neighboring municipalities that did not adopt the new regulation. This comparison, focused on similar housing markets, allows us to attribute observed differences to the effects of rent control.

Our estimates indicate that the rent control policy fails to achieve its goal to permanently reduce rents, as average treatment effects are not statistically different from zero at conventional levels. Initially, rents in controlled municipalities drop by about 5%, thus temporarily alleviating rent affordability issues. Within a year, however, rental housing supply declines dramatically by about 30%, and rents start rising again. As a result, the difference in rents between controlled and uncontrolled municipalities narrows, making the rent control policy ineffective in the long run. Furthermore, we document substantial spillovers to the owner-occupied housing market. We observe a 13% to 18% increase in sales volumes and a 3% decrease in sales prices in controlled areas relative to uncontrolled ones, indicating a substantial shift from rental to owner-occupied units. This trend highlights the policy's unintended consequence of reducing rental housing availability, primarily due to the conversion of rental properties into for-sale units, thus exacerbating the rental housing shortage.

Our theoretical model provides a comprehensive explanation for the observed dynamics of rent control's impacts. As in Favilukis, Mabille and Van Nieuwerburgh (2022), the cap on rents effectively reduces renting costs in the regulated region, making it more affordable than the uncontrolled region. These findings align with our empirical results in the short run, indicating that rent control leads to a significant decrease in rents in regulated areas, while the relative prices of owner-occupied units remain largely unaffected. This initial impact is attributed by our theoretical model to a more responsive (elastic) housing supply in the unregulated region, influenced by the larger non-housing market and by migration patterns. Specifically, in the model, migration from the regulated to the unregulated region expands the market for non-housing goods in the unregulated region, altering consumption patterns and marginal utilities across regions. Consequently, our theory predicts a more substantial drop in house prices in the unregulated region for any given decrease in rents.

However, the effectiveness of rent control in sustaining reduced rent levels diminishes over time. Our theoretical model provides a framework to understand this long-term ineffectiveness by rationalizing the reversal and subsequent increase in rents in the regulated region. The model highlights a significant reduction in the rental housing supply in regulated areas as a central factor. This reduction is primarily driven by decreased income from rental properties for investors, who consequently find less utility in maintaining or offering rental units. As a direct consequence, there is a notable shift towards owner-occupied housing, compounded by the lower elasticity of the housing supply in regulated versus unregulated regions. Additionally, the model suggests that broader market spillovers—where improved household welfare under rent control boosts consumption of non-housing goods—further reduce the relative utility of housing. The interplay of decreased rental supply, changes in investor behavior, and variations in consumption patterns neutralizes the policy's initial benefits, illustrating the key trade-offs between immediate affordability goals and long-term housing market stability.

In the second step of our empirical analysis, we assess the distributional effects of rent control by examining its impact across different segments of the rent and price distributions. Our findings show that rent control reduces rents at the bottom of the distribution, while leaving rents at the top essentially unaffected. For instance, rents in the bottom decile decrease by about 8%, while there is no significant effect on the top decile. Similarly, sales prices at the lower end of the distribution also decline, whereas sales prices at the higher end experience an increase, ranging from 3.7% for the top quartile to 5.7% in the top decile. Therefore, our findings show that improving rent affordability for units at the lower end of the distribution results in a loss of market value for these same units.

Conversely, the fact that top rents remain essentially unaffected raises market values of controlled properties at the upper end of the distribution. As a result, asset inequality among homeowners increases, which is particularly relevant given that home values represent the primary asset for many low-income owners. Consistent with this, we find that the decline in rental and especially sales prices is especially marked in cities exhibiting high population density and proportion of rentals. Conversely, less densely populated municipalities with lower shares of rentals see no significant loss in house market values.

The overall picture that emerges is that rent control is effective in reducing rents in less affluent areas, but also depresses house market values in these same areas. At the opposite tail of the price distribution, more valuable residential properties gain in assessed value following rent control. As a result, the new regulation benefits low- and medium-income tenants by decreasing both rental and house prices at the bottom of the respective distributions, but also widens the gap in real estate values between low- and high-income owners, in that the latter gain from a price appreciation of their properties. This finding is consistent with recent evidence on endogenous segmentation and gentrification in local markets (see Guerrieri, Hartley and Hurst, 2013; Diamond, McQuade and Qian, 2019).

Finally, to get a sense of the quantitative impact of rent control, we conduct a thought experiment estimating hypothetical changes in the aggregate pre-policy values of rentals and sales in treated municipalities, had rent control been in place. By computing these values over the pre-policy period and imputing the percentage change from our preferred models, we quantify the policy's effects on the rental and housing markets. Our estimates reveal that renters in the bottom three quartiles are projected to save almost 8 million euros over ten years due to lower rental costs, highlighting considerable benefits for lower- and medium-income tenants. In contrast, property owners in these segments face an estimated loss of nearly 1 billion euros in sales prices, pointing to significant costs on their end. At the same time, owners in the top quartile experience an approximate gain of 1.1 billion euros, indicating a substantial benefit for property owners in higher-income brackets. This differential impact emphasizes rent control's redistributive effects and its implications for wealth inequality across different income brackets.

Our work contributes to three different strands of literature. First, our study contributes to the growing body of literature on the effects of rent control legislation (see Jenkins, 2009, for an excellent review). A first set of results concerns the impact of rent control on tenants, with several studies finding that rent control is generally successful at reducing rents—or preventing them from rising—for controlled dwellings (Sims, 2007; Autor, Palmer and Pathak, 2014; Mense, Michelsen and Kholodilin, 2023; Diamond, McQuade and Qian, 2019). There is also evidence that rent control legislation lowers tenants' displacement for controlled units, making tenants less likely to leave their homes due to rising rents (Glaeser and Luttmer, 2003; Sims, 2007; Diamond, McQuade and Qian, 2019), with the possible exception of high-income households (Mense, Michelsen and Kholodilin, 2023). Consistently with these studies, we confirm that the rent control law introduced in several Catalan cities is effective in reducing rents in the controlled cities in the short-run but this effect virtually disappears in the long-run due to the induced scarcity of rental housing. Besides, we also examine how the treatment effect changes across the rent distribution. Therefore, we document how the reduction in rental prices stems from negative effects at the bottom of the rent distribution. Accordingly, we find that rents decrease especially in neighborhoods featuring higher population density and a larger share of renters.

Despite the benefits for tenants in controlled units, the literature also highlights potential negative effects for the rest of the rental market, as well as for property values. For instance, studying the introduction of rent control protections in 1994 in San Francisco, which regulated price increases *within* the duration of a tenancy, Diamond, McQuade and Qian (2019) show that rent control decreased landlords' rental housing supply by 15%, causing a 5.1% city-wide rent increase. With regards to property values, Ahern and Giacoletti (2022) find that the introduction of rent control in November 2021 in St. Paul, Minnesota, benefits average and above average income tenants at a rate that is greater than low income tenants.[2] Further, they show the loss in property aggregates in such a way that impacts all property owners virtually equally, although aggregate negative price impacts are felt more significantly by lower income landlords than higher income landlords. Similarly, our results from the study of the stringent large-scale rent control policy in Catalonia show that rent control benefits low-income tenants, while lower-income owners are damaged by rent control, because housing prices fall most significantly in the lowest quartiles of the sales price distribution. This is consistent with the

[2] This result assumes that all households are weighted equally, irrespective of their respective marginal utilities of consumption.

prediction that the market value of properties at the bottom end of the distribution fall because property prices capitalize the expectations of lower rents in the future. However, we also show owners at the top of the sales price distribution actually benefit from rent control, in that average prices in the top quantiles significantly increase following the passage of the law.

Our study is related to Mense, Michelsen and Kholodilin (2023) in that both papers are the first two studies to examine the impact of a large-scale rent control intervention in the rental market, although their case is focused on the German market. The most significant policy difference between the two cases is that the initial rent control regulations in Catalonia were more stringent and far reaching than the German case, which only applied to new rental contracts. However, in the case of Catalonia, rents in existing contracts were also implicated by the new policy. In other words, in the German case, the imposition of rent control unevenly across a municipality creates both market-rate and rent-controlled markets within the same municipality. Thus, our case is advantageous precisely because the Catalan policies were so broad sweeping. They allow us to truly isolate municipalities where rent control was not applied from those where it was, in a fashion that was not possible in Mense, Michelsen and Kholodilin (2023)'s important case study. A second important distinction is that our paper examines the spillover impacts into the owner occupied housing market, which is a subject not addressed by Mense, Michelsen and Kholodilin (2023). A third distinction in the differences between the policies is that the German rent control was adopted by municipalities in a much more stuccato fashion, in that rent control began to be adopted in 2015 in Berlin and was gradually expanded to other municipalities through April 2016. By contrast, the Catalan rent control was imposed virtually at the same time, across an entire single region. Relatedly, Mense, Michelsen and Kholodilin (2023) only study the short-term impacts of rent control, as they do not segment their impacts by time. Finally, Mense, Michelsen and Kholodilin (2023) do not consider the relationship between controlled rents and housing prices, although Ahern and Giacoletti (2022) have previously indicated market spillover impacts on housing prices are a critical element of understanding how rent control impacts wealth transfers. Thus, we are a providing a substantial contribution to the literature, in that ours is the first study to examine a large-scale impact for rent control on both the rental and owner occupied markets in both the short and long-term.

More broadly, we contribute to the growing debate on housing affordability, which is one of the most vital policy challenges for every large municipality in the world. The housing affordability crisis has worsened in recent years in the United States. There has been a sharp divergence in housing costs and household incomes, fueling a long-term increase in cost-burdened renters from 1980 through the present. Further, despite a sharp rise in income-eligible households, the number of renters with housing assistance has essentially been flat for two decades (Joint Center for Housing Studies of Harvard University, 2018). Indeed, as the affordability crisis has worsened globally, rent control has re-emerged as one of the main instruments that policy makers have employed to combat the affordability crisis. Favilukis, Mabille and Van Nieuwerburgh (2022) highlight that rent stabilization can operate as form of social insurance, resulting in a substantial welfare gain and benefiting households at the bottom of the income distribution most, but at the same time can lead to aggregate and spatial misallocation in both housing and labor markets, thus potentially exacerbating the affordability problem for non-controlled renters and potential future home owners. Our results are consistent with these findings in the long run. Therefore, while Diamond, McQuade and Qian (2019) argue that rent control can help affordability in the short-run, in the long-run it worsens the affordability crisis and exacerbates gentrification. We contribute to this literature by studying the impacts of rent control on rental and house prices, as well as on the supply of rental and housing units. Indeed, whether affordable housing exists depends both on whether rents are affordable and on the number of housing units on the market. Additionally, we study the heterogeneity in the impact of rent control at different segments of the rent and price distributions. Our goal is twofold. First, this will shed light on whether rent control benefits mostly renters and home owners at the bottom of the respective distributions, which is often the stated goal of rent control legislation. Second, this allows to assess how rent control can affect the gap in value between the lower and upper ends of the distributions, with possible consequences on wealth inequality.

The rest of the article is organized as follows. Section 2 describes the institutional framework and the exact content of the rent control law. Section 3 builds a two-region theoretical model that incorporates important elements of the Catalan rent control law described in Section 2 and provides theoretical results that rationalize the observed changes in quantities and prices described in the empirical part that follows. Section 4 describes the data. Section 5 discusses the empirical strategy and presents the main results. Section 6 provides evidence on the heterogeneity in the policy impact. Section 7 discusses our conclusions and potentials for future research.

2 The Catalan Rent Control Law 11/2020

On September 18 2020, the Catalan Parliament passed a Law on Urgent Measures for Rent Control (Law 11/2020), which came into force a few days later on September 22 2020.[3] The purpose of this Law is to regulate contractual rent increments of residential rental properties located in newly regulated areas declared as "tense housing markets". The Law explicitly describes what a regulated area is and how the upper bound on the growth of rents is defined.

Newly regulated areas. The Rent Control Law only applies to residential properties located in areas declared as "tense" ("*area con mercado de vivienda tenso*"). According to Article 2 of the Law, the declaration of a tense area has a maximum duration of five years and can be extended multiple times if properly justified. The declaration of a tense area depends on the well-known 30 per cent housing affordability rule of thumb as well as the comparison between the area's rental price reference index with the rest of Catalonia and its five year history of price growth.[4] In particular, an area is declared a tense housing market if any of the following conditions hold:

[3] http://justicia.gencat.cat/ca/departament/Normativa/normativasectorial/legislacio_civil_catalana/llei-11-2020-mesures-urgents-rendes-habitatge/index.html.

[4] The reference index of rental prices (*IRP*) is established by the Generalitat de Catalunya (Catalan Government) and, more specifically, by the Catalan Housing Agency (*Agència de l'Habitatge*). The IRP can be consulted on the Generalitat de Catalunya's website, at the link http://agenciahabitatge.gencat.cat/indexdelloguer/. To determine the rent limit, the index is taken into account and not the upper and lower price bounds.

(i) the ratio between the average rent burden and the average household's income exceeds 30 per cent;

(ii) the growth rate of the average rent in this area is higher than the average rent in Catalonia;

(iii) in the five years prior to the declaration of the area as tense, the annual growth rate of the average rent is at least three percentage points higher than the annual growth rate of the Catalan Consumer Price Index.

The Annex of the Law provisionally identifies sixty-one Catalan municipalities with more than 20,000 inhabitants as tense housing markets. These municipalities include Barcelona, Badalona, L'Hospitalet de Llobregat, Lleida, Sabadell, Terrassa, and Tarragona. We include the complete list in Appendix Table A1.After one year from the introduction of the Law, another nine municipalities were added to this list, while two were removed.[5]

Figure 1 displays the map of Catalonia with municipalities colored according to their treatment status based on their adoption of rent control. Each municipality has a circle with radius proportional to the number of rent ads (Panel a) and sales ads (Panel b) that were posted in the corresponding municipality during our sample period. Fifty-nine municipalities (colored in violet) belong to the group of always-treated-municipalities since September 20, 2020 and remained subject to the regulation since then. Two municipalities (in brown) were treated only in the first year after the passage of rent control, from September 2020 when the law was passed until August 2021 when they were no longer classified as tense areas. Nine municipalities adopted the regulation only in the following year: September 2021 (green, five municipalities), October 2021 (light blue, two municipalities) and December 2021 (violet, two municipalities). The remaining municipalities, colored in pink, were never subject to the rent control ordinance and thus we assign them to the control group. No ad was posted in the (unregulated) municipalities colored in light gray during our sample period.

Upper bound on the growth of rents. Once a property belongs to a tense area, it must comply with the Law, which establishes that the rent of those rental contracts signed on or after 22 September 2020 in a regulated area cannot exceed:

(a) the reference rent of a similar property in the same urban area;[6]

(b) the rent stated in the last rental contract if the lease of the property was signed between 22 September 2015 and 22 September 2020.[7]

[5]In particular, the nine municipalities of Begues, Canet de Mar, Cardedeu, La Garriga, La Palma de Cervello, Llagostera, Parets del Valls, Sant Fruitos de Bages, Sant Sarduni d'Anoia were declared as tense housing markets in September 2021, and thus started being concerned by the new rent control regulation only since then. Conversely, the municipalities of Tortosa and Martorell were subject to the rent control law only in the first year of application of the new law, namely from September 2020 to August 2021.

[6]The law permits the property owner to increase the rent by at most 5 percent if the property has at least three facilities of a list that includes lift, parking, and heating/cooling system. Alternatively, if the residential property has undergone renovation works in the last year to improve its habitability, safety, comfort or energy efficiency, the owner can also increase the rent by an amount that is calculated based on the total rehabilitation investment in the property.

[7]For the sake of transparency, the landlord is obliged to inform the tenant about the rent charged in the previous rental contract of his property. The landlord can also authorize the future tenant to obtain this information from the Registry of Legal Deposits (*Registro de Fianzas*) before signing the new rental contract. Once the contract is signed, the tenant can request this information without the landlord's authorization. All contracts signed before September 22 2020 remain subject to the previous legislation, with the exception of those contracts whose duration has been extended or whose rent has been modified after the Law was passed which will immediately become subject to the new rent control law.

Condition 2 eliminates the possibility of any rent growth above the previous rent signed before the Law was implemented. Only when the rent of the previous contract is below to reference rent of a similar unit in the area, a rent higher than the one stipulated in the previous contract can be signed. Generalized inflation or housing scarcity can increase the reference rent in the area, but never higher than the rent of the reference unit's lease signed between 22 September 2015 and 22 September 2020. Some exceptions to the rule apply, including those rental contracts signed before 1995, properties with a surface area exceeding 150 square meters,

3 Theory

In this section, we present our theoretical model to help us rationalize the mechanisms underlying our empirical findings. This model explores the dynamics of rent control, offering insights into the observed effects on rental and owner-occupied housing markets.

We consider an economy with two regions: a region C subject to rent control and an unregulated region N. Each region is populated by a continuum of mass 1 households. There is also a real estate investor in each region. We denote the measure of households that choose to rent (buy) by η ($1-\eta$, respectively). η is endogenous in our model. If households to migrate from one jurisdiction to another, the measure of households per jurisdiction can differ from 1. In the regulated region, households can only rent housing at the regulated price. The measure of rent controlled (non-rent controlled) rental housing units available in the C-region is κ_0 ($1-\kappa_0$, respectively). Because in region C all rental contracts must comply with the rent control by analogy with the case of Catalonia, Spain, we set $\kappa_0 = 1$.

There is also a numeraire good available for consumption. We denote the household l's consumption of the numeraire good by c^l and his endowment of this good by ω^l. For the sake of brevity, we focus on the C-region in the main text and leave for the Appendix the notation and optimization problems corresponding to the unregulated region. We assume that the rent effectively paid by households in the rent controlled region is proportional to the level of rents R in absence of rent control, that is, the regulated rent is κR. This differs from Favilukis, Mabille and Van Nieuwerburgh (2022) who instead assume that rent control affects both rents and sale prices proportionally. In our model, the house price p_κ is a function of κ but not necessary a linear function. The regulated rent κR and the house price p_k are both expressed in terms of the numerarie good. κ is a real number defined such that $\kappa \equiv \kappa_0 \eta$. Because $\kappa_0 = 1$ in equilibrium, κ is effectively equal to η in our model.

The preference function of households is quasi-linear in the consumption of the numeraire good c^l and housing h^l. Housing is a convex combination of the consumption of rental housing $\hat{h}$ and owner-occupied housing h with corresponding weights are $\alpha^{R,l}$ and $\alpha^{o,l}$, i.e., $h^l \equiv \alpha^{R,l}\hat{h}^l + \alpha^{o,l}h^l$. The household's utility function is

$$U(c^l, h^l) \equiv (c^l)^{1-\sigma}/(1-\sigma) + h^l \tag{1}$$

The household l' choice variables are c^l, $\hat{h}$ and h. $U(c^l, h^l)$ must satisfy the following participation constraint:

$$U(c^l, h^l) \geq \bar{U} \tag{2}$$

We characterize the economy by means of a (centralized) recursive problem solved by the investor (described below), so it is enough to restrict the household behavior by this participation constraint. If the utility function U has a curvature σ big enough, the consumption of the numeraire is positive $c^l > 0$. To avoid $h^l < 0$, we set $\bar{U}$ sufficiently high. $\sigma > 1$ and $\bar{U} > 0$ ensure that the household consumes a positive amount of housing which can rental or owner-occupied. The ratio R/p_κ drives the nested tenure decision between renting and owning. Households live for only one period, so here p_κ is understood as the price of buying one housing unit for a one period consumption.

The participation constraint of household l in region C is the following:

$$\frac{(\omega+\omega^l-c)^{1-\sigma}}{1-\sigma}+\mathbb{I}\left(\alpha^{o,l}/\alpha^{R,l}\geq p_\kappa/(\kappa R)\right)\alpha^{o,l}h^l+(1-\mathbb{I}(\alpha^{o,l}/\alpha^{R,l}\geq p_\kappa/(\kappa R)))\alpha^{R,l}\hat{h}^l\geq\bar{U} \quad (3)$$

The first term on the left hand side of equation (3) is the household l's utility associated with the numerarie consumption. We replace c^l by its equilibrium value (feasibility is incorporated in the optimization problem). The term $\omega+\omega^l-c$ in the numerator follows from the feasibility condition $c^l=\omega+\omega^l-c$, where c and ω are the investor's consumption of the numeraire good and the household l's endowment, respectively.

The second term on the left of the equation (3) follows from the quasi-linear nature of preferences which makes the housing decision dichotomous. If the utility of buying a house is sufficiently higher than the rental option (i.e., $\alpha^{o,l}/\alpha^{R,l}$ high) or if the cost of buying a house is sufficiently lower than the rental price ($p_\kappa/\kappa R$ low), households in region C choose to buy a house, i.e., $\mathbb{I}(.)=1$, and hence $\hat{h}^l=0$. A fraction $1-\eta$ of households choose to buy. The rest of households in region C have preferences such that $(\alpha^{o,l}/\alpha^{R,l})<(p_\kappa/\kappa R)$. So a measure η of households choose to rent and thus $h^l=0$. The households' heterogeneity in preferences, characterized by a ratio $\alpha^{o,l}/\alpha^{R,l}$ that is different for each agent $l\in[0,1]$, is critical to endogenously determine η. Below we will allow for households' heterogeneity in income to match a novel stylized fact characterizing low-income and high-income households' decisions.

The solution of the model depends on a dynamic recursive contract between an investor and the households. For the sake of brevity and following Favilukis, Mabille and Van Nieuwerburgh (2022), we assume that households live for only one period.[8] This guarantees that the investor's recursive problem has a minimal state space $[\omega,h,b]$, where $b\in\mathbb{R}$ is the net asset position, h is the housing stock of owner occupied units and ω is the endowment. Endowments are positive and represent income generated by activities not related to the real estate market, which are assumed to be exogenous. Shocks to ω follow a Markov process.

The investor solves a dynamic problem subject to a static participation constraint related to equation (2) that will be described below. Given an income level, the investor decides how much to consume c of the numeraire, his asset position b' at price q, and the amount $h'+\hat{h}$ of housing units today. The amount h' of owner-occupied housing units bought today is put for sale tomorrow, whereas the rest ($\hat{h}$ rental units) is put for rent today. We make these assumptions to keep the model tractable with a reduced state space in the recursive problem

[8]In the Appendix B.1, we characterize this economy based on a recursive version of this contract. Dealing with long-lived households would require keeping track of the promised utility, which would add an additional endogenous state variable to the recursive problem.

as in Favilukis, Mabille and Van Nieuwerburgh (2022). The investor's budget constraint is

$$c + p_\kappa(h' + \hat{h}) + qb' = \omega + \kappa R\hat{h} + p_\kappa h + b \tag{4}$$

When borrowing ($b' < 0$), the investor must satisfy the following short sale constraint:

$$-b' \leq \theta h \tag{5}$$

where $0 < \theta < 1$. The upper bound is given by a fraction θ of the total housing stock h that is available for sale. The higher is the stock of housing h purchased, the more debt the investor can issue.

The investor solves the following optimization problem:

$$V(\omega, x) \equiv Max_{h',\hat{h},b',c} \quad u(c) + \beta E_\omega(V(\omega', x'))$$

subject to equations (3) for all l, (4) and (5)

where u is a strictly concave and increasing instantaneous[9] utility function. The concavity of the value function $V(\omega, x)$ is needed to get an appropriate responsiveness of the housing supply. $V(\omega, x)$ depends on the aforementioned properties of utility function u. We solve constraint (3) intra-temporally in each period. Thus, the optimization problem is not actually restricted by a feasible set formed by a continuum of equations but by only four participation constraints: $\eta\times$(3), $(1-\eta)\times$(3), (4) and (5). We characterize the problem in the Appendix using a finite set of well-defined multipliers. In this sense, the degree of heterogeneity is sufficiently rich to match the stylized facts while keeping the problem tractable.

We denote by $x \equiv [b, h]$ the vector of endogenous state variables[10], which consists of bonds and owner-occupied housing assets. Because the investor can rent in the same period the housing stock that he buys, $\hat{h}$ is not a state variable.

The investor solves a dynamic contract as in Marcet and Marimon (2019), where households internalize their incentives given the participation constraint and the feasibility requirements for the numerarie good, i.e., $\Omega \equiv \omega + \omega^l = c + c^l$. The feasibility conditions in the housing market follow from the participation constraint. The investor is the only supplier of housing services, so the following housing market clearing conditions must hold: $\hat{h} = \eta\hat{h}^l$ and $h = (1-\eta)h^l$.[11]

There is no optimization and budget constraint in the household's problem. Households' individual rationality follows from the participation constraints, the aggregate feasibility conditions for the three active markets (the numeraire good market, the rental housing market, and the owner-occupied housing market), and the condition that requires that the household's utility is not smaller than the minimum utility $\bar{U}$ (see also Kehoe and Levine, 1993).

[9]In an infinite horizon optimization problem the objective function is $U(c_\infty) \equiv E_0 \sum \beta^t u(c_t)$, where U is the utility function and u is the instantaneous utility function which in turn are functions of the sequence of bounded future consumption bundles c_∞ and current consumption c_t respectively.

[10]An endogenous state is a state variable determined inside the model. Shocks are exogenous states as they evolve according to a low of motion determined outside the model.

[11]Purchasing a house is always preferred for a measure $(1-\eta)$ of households because their relative utility $\alpha^{o,l}/\alpha^{R,l}$ is above the relative price $p_\kappa/\kappa R$.

We obtain a non-arbitrage equation for the housing market that balances the two options available: renting and selling. We report this equation in the Appendix (eq. (12)). This equation determines the investor's housing portfolio. Housing has the role of a financial asset in our model. This is not the case in Favilukis, Mabille and Van Nieuwerburgh (2022) where individuals buy housing for living and thus housing units are assigned a positive marginal utility. In our model the investor only buys housing if he expects its value to appreciate.

Before presenting the theoretical results derived from the model, we point out two additional differences between our model and the model in Favilukis, Mabille and Van Nieuwerburgh (2022). First, in Favilukis, Mabille and Van Nieuwerburgh (2022) developers must produce a mandatory minimum amount of rent subsidized units while households are assigned to these units randomly. In our model, there is no distortion created by the rent control policy because the investor can freely choose and diversify between rental and owner-occupied housing. In this sense, contrary to what is claimed in Favilukis, Mabille and Van Nieuwerburgh (2022), rent control does not generate a misallocated housing stock. Moreover, in order to capture the specificities of the rent control regulation in Catalonia, Spain, all rentals are regulated in the rent control region and all households choosing to rent in this region must do so at the regulated rent. Secondly, Favilukis, Mabille and Van Nieuwerburgh (2022) evaluate the effects on welfare only for households. However, in their model rent control generates (negative) spillover effects on other markets including the owner-occupied housing and goods markets. Thus, in their framework, it is not clear what the overall effect of rent control is on a given municipality or region as a whole. In our model, the investor buys a stock of housing given the households' preferences and optimally chooses how much to sell and how much to rent of this housing stock. This setting allows us to derive a marginal utility based welfare measure for the entire economy and not only for households.

Next we present three theoretical results that explain the effects of the adoption of a large-scale stringent rent control policy in one of the two regions. We are interested in the behavior of rents, house prices, housing supply and households' migration between regions. For this, we take the path of rents and sale prices from the data to rationalize the mechanism behind the observed changes in the quantities of rental housing, owner-occupied housing and the numeraire good. We first present the results for the benchmark economy described above where each region has homogeneous households. Afterwards we will present the results for an extension of the benchmark economy that instead considers regions with heterogeneous households.

Proposition 1 (Rent control region): *In the rent control, the housing rent R and the house price p_κ decrease in the short run. However, the rent R' reverses and starts increasing in the long run, while the house price p'_κ keeps falling. For this pattern of prices to occur the supply of rental housing must decrease and the supply of owner-occupied housing must increase.*

For the sake of brevity, we leave the formal proof for the Appendix. Here we elaborate on the intuition behind the mechanisms of the recursive contract. The regulation on rents reduces the revenue that the investor gets from its stock of rental housing (see right hand side of equation (12)). The investor responds by consuming less of the numeraire good. Households absorb this additional consumption of the numeraire for the market to clear. Because households' preferences are risk averse, the household's marginal utility of consumption decreases. This effect dominates the solution to the recursive problem because households are larger in size than the investor by assumption. The marginal value of rents decreases as a result (see equation (12) in the Appendix). Absence of arbitrage requires the investor's housing portfolio in the rent control region to rebalance from rental to owner-occupied housing. Thus, the supply of rental housing decreases and the supply of owner-occupied housing increases. This behavior is consistent with Table 4 and Figure 3. In the long run, the aforementioned change in quantities induce house prices to decrease and rents to reverse and start increasing, consistent with Figure 2.

To account for the reduction in the supply of rental housing we must use the Euler equation for bonds (see equation (11) in the Appendix). We can rewrite the investor's budget constraint (4) as $c + p_\kappa h' + (p_\kappa - \kappa R)\hat{h} + qb' = y(p_\kappa) \equiv \omega + p_\kappa h + b$.The fall in house prices reduces the investor's net income $y(p_\kappa)$. The investor's consumption must then decrease since it is risk averse, $p_\kappa - \kappa R > 0$ (equation (10)), and equation (11) holds. The investor smooths consumption by means of trading the risk-free bond b'. We consider three cases:

1. *Rent control is permanent*: If the investor believes that rent control is permanent, consumption c decreases by the same amount than $y(p_\kappa)$. Since $p_\kappa - \kappa R > 0$, the investor's budget constraint implies that h' increases and $\hat{h}$ decreases.

2. *Rent control is transitory and the investor has capacity to increase leverage*: If the investor believes that rent control is transitory, then the reduction in c is smaller than the change in the net financial income. This in turn implies that the investor issues debt to smooth consumption provided that the investor is not collateral constrained. Because the change in debt and consumption equal the reduction in income after p_κ and h' decrease, the supply of rental housing $\hat{h}$ must decrease.

3. *Rent control is transitory and the investor is collateral constrained*: If the investor is collateral constrained, then the investor cannot issue debt to smooth consumption and therefore the investor chooses to reduce even further supply of rental housing $\hat{h}$ as compared to the transitory non-binding case.

Proposition 2 (Spillover effects on unregulated region): *In the short run, the decrease in rents R' is stronger in the rent control region than in the unregulated region ($\kappa R_C/R_N$ decreases), while house prices p in the unregulated region experience a similar fall than in the rent control region. In the long run, the ratio of rents between the regulated versus unregulated regions ($\kappa R_C/R_N$) increases because the investor's strong reduction of rental supply in the rent control region.*

Even though the elasticity of owner-occupied housing is smaller in the regulated region, the investor's income reduction dominates in the long run (as κ directly affects this investor) and the investor effectively reduces more the supply of rental units in the rent control region than in the non-regulated region.[12] This in turn generates an increase in the ratio of relative rents $\kappa R_C/R_N$, consistent with the long run behavior of relative rents in Figure 2.

[12]The reduction in the supply of rental units is induced by the rigidity of consumption through the budget constraint.

For house prices to exhibit a similar fall in the two regions, we need the owner-occupied house price to be more sensitive in the uncontrolled region than in the rent-control region. Migration and the spillovers between the housing and numeraire good markets rationalize this. Scarcity of rental housing in the rent control region induces some households to migrate from the regulated region to the unregulated region, increasing the market of the numeraire good in the unregulated region and thus increasing more the difference in the marginal utilities between the investor and the households in the unregulated region than in the regulated region. This explains a more responsive owner-occupied housing price in the unregulated region than in the regulated region.

4 The Data

Having introduced the theoretical framework, we now turn to our empirical analysis of the rent control regulation in Catalonia. This section will detail our primary dataset on housing rents and sales prices, alongside additional data sources (Section 4.1), and will also provide summary statistics (Section 4.2).

4.1 Data Sources

Atlas Real Estate Analytics facilitated making a large database on rental and sale posts available to us. The data covers all the posts made public on the online real estate portal Fotocasa, an easy to use website advertising for sale and rental postings free of cost to the poster.[13] Together with Idealista, Fotocasa dominates the long-term search and sale search online market in Spain, with more than 1.5 million advertisements and an average of 15 million visits per month. Thus, Fotoscasa's data provides a reliable pulse to better understand the conditions of real estate markets in Spain.[14]

Our dataset includes all the advertisements (hereinafter, ads) that were active for the whole country of Spain between June 2020 and January 2022 on a monthly basis. The dataset includes the exact location of the property unit, the posted rental or sale price, and a set of property characteristics, such as the type of unit (e.g., apartment, duplex, studio, etc.), the size in squared meters, the number of bedrooms and bathrooms, and other property attributes such as garage, terrace, and air conditioning. Properties are also ranked according to their status on a scale from 1 to 4. We exclude properties if they are missing any of the following key data points: location, price, number of bedrooms, or number of bathrooms. We also exclude properties with more than ten bedrooms, more than eight bathrooms, or with no bathrooms. We also drop properties classified as new constructions, as these are not affected by the rent control regime.

We stress that our analysis is based on *posted* rents and sale prices. This approach is generally referred as a good proxy of actual price behaviour (see Lyons, 2013; Chapelle and Eyméoud, 2022), but is subject to the caveat that posted rents and sale prices may not coincide with the final ones agreed in the transaction by the two parties. Departure between the two may occur for different reasons including a bargaining process. Of course, bargaining is

[13]This data was kindly provided by Atlas Real Estate Analytics.
[14]https://www.similarweb.com/it/website/fotocasa.es/#overview.

less likely to result in price changes in the rental market, as compared to the owner occupied market.

We further complement our dataset with a comprehensive set of socio-economic characteristics at the zip-code and municipal level in Spain, recorded before the introduction of rent control, such as population density, median household income, median age, the percentage of people living in rental schemes, and the percentage of the population who have completed tertiary education. This allows us to study the heterogeneity in the impact of rent control on average rents and sales prices across different areas. Additionally, we collect information on the number of hotels in each municipality using Google Maps APIs, which serves as a proxy for touristic activity.

Finally, to evaluate how rent control's impact varies with property proximity to city centers, we calculate two distance measures for each property in our sample: the air (straight-line) distance and the travel distance by car. Properties within a 2-kilometer radius of the city center, identified as the highest density point in satellite images, are considered central. The air distance is derived from latitude-longitude coordinates, whereas the travel distance by car is obtained using the Google Maps API.

4.2 Summary statistics

Table 1 reports the descriptive statistics for rent ads (Panel A) and sales ads (Panel B) for the period of study. From Panel (A), we can see that the average asked rent is just above €1,300 and the median is €1,050. This corresponds to an average rent per squared meter of €16.02 and a median rent of €14. About 73 percent of the total number of rent ads belong to treated municipalities in the period when rent control was in place. Most ads are specific to flats or apartments. We also find that rental units have an average size of 85 squared meters, with 2.5 bedrooms and 1.5 bathrooms. The average status of advertised rental units has a score of 3 (good status) on a scale from 1 to 4, while about 30 percent of rental units are renovated, corresponding to the highest score of 4.

Table 1 (Panel B) reports an average sale price over the sample period of €257,207, which corresponds to an average sale price per squared meter of €2,650. The corresponding median prices are €180,000 and €2,238. On average, sales units have a size of about 110 squared meters, with 2.8 bedrooms and 1.5 bathrooms. Most of the properties for sale are in good condition and do not require substantial repairs.

In Appendix Table A2, we present the main characteristics of properties listed for rent and sale in the pre-rent control period, separately by treatment status. Beginning with Panel A, unsurprisingly, we can see that rental units in municipalities that later introduced rent control are, on average, more costly than those in the the non-treated group. Specifically, the difference in rent amounts to approximately €400, or €6 per square meter. This discrepancy persists despite the larger average size of units in the latter group, which exceed those in treated municipalities by nearly 20 square meters. However, the number of bedrooms and bathrooms shows no significant difference between these groups.

Conversely, as shown in Panel B, the characteristics of properties listed for sale are relatively more balanced between areas with and without rent control. The average sale price in treated municipalities is slightly higher, at just over €275,000, compared to €180,000 in control areas, but this difference is not statistically significant at conventional levels. This also applies to the average sale price per square meter, which is given by approximately €2,700 in treated areas versus nearly €2,000 in untreated ones. Yet, properties in treated municipalities are, on average, larger and feature more bedrooms, though the number of bathrooms does not significantly differ between the two groups.

5 Empirical Analysis

In this section, we first outline our identification strategy (Section 5.1). We then present our main estimates of the impact of rent control on average housing rents and sales prices (Sections 5.2.1 and 5.2.2), and assess the dynamics of rent control's effect through "event study" models (Section 5.2.3). Next, we present results on the availability of rental and for-sale housing units (Section 5.3). Finally, we conduct several robustness checks to demonstrate the reliability of our findings (Section 5.4).

5.1 Identification Strategy

Our identification strategy is based on a Difference-in-Differences (DiD) approach, where we compare the dynamics of the considered outcomes before and after the introduction of a rent control policy in regulated *versus* unregulated municipalities. Our outcomes of interest are both housing rents and sales prices.

In our main analysis, the treatment group includes properties located in municipalities that introduced rent control as of September 2020 and kept the regulation since then ('always treated'). Our control sample is restricted to properties located in cities that share a border with at least one treated city and never adopted rent control legislation ('neighboring control'). Restricting the control sample to neighboring municipalities that did not introduce rent control increases our confidence that these properties are more likely to be comparable to those located in treatment municipalities. In Section 5.4 on robustness tests, we will show that our main results are not sensitive to the choice of the control group.

Our main specification takes the following form:

$$Y_{ijt} = \alpha + \beta \, Treat_i \times Post_t + \mathbf{X}_i'\zeta + \gamma_j + \delta_t + \varepsilon_{ijt} \tag{6}$$

where Y_{ijt} is either the logged advertised rental price or sale price per squared meter of a property i located in zip-code j at year-month t. The indicator variable $Treat_i$ equals 1 for the ads of properties located in a rent control municipality, and 0 otherwise. $Post_t$ is another indicator that equals 1 if the ad is active after the introduction of rent control, and 0 otherwise. $\mathbf{X}_i$ is a vector of property characteristics, including the size of the unit in square meters, the status (on a scale from 1 to 4), dummies for different property sub-types (e.g., apartment, duplex, studio), and other characteristics.[15] The terms γ_j and δ_t denote the location (zip-code) and time

[15]Other characteristics that are available and that we use in our analysis are the number of bedrooms and bathrooms, the presence of the lift, garage, storage, terrace, air-conditioning, swimming pool, garden, and place for sports.

(year-month) fixed effects, respectively.

The coefficient of interest is β, which measures the estimated average effect of the rent control regulation on rents or sales prices in regulated cities. More precisely, because the dependent variable is in log terms, β measures the percentage change in advertised (rental and sales) prices in treated municipalities that is due to the introduction of the rent control law, relative to the control group.

We cluster standard errors at the zip-code level. In Section 5.4 on robustness, we also consider specifications that instead include municipality fixed effects and that cluster standard errors at the municipal level. We will show that our main results are robust to these alternative specifications.

Main identification assumption. In the above specification, zip-code and time fixed effects absorb any observable or unobservable average difference in the outcome variable across neighborhoods and time periods. Since most of the location-specific determinants of property values are highly persistent over time, the location fixed effects likely absorb many of the confounding factors affecting rental and sales prices.

The main DiD identifying assumption is that there is no omitted time-varying *and* zip-code specific factor that is correlated with both the introduction of the rent control law and the outcome variable. Since it is not possible to directly test this assumption, we also consider specifications that include an interaction term of zip-code level variables recorded before the introduction of the rent control law and that are plausibly correlated with its adoption, with the dummy for the treatment period, $Post_t$ (e.g., see Fouka, 2020). For example, for the case of the zip-code's median income, denoted by $MedIncome_j$, we estimate the following specification:

$$Y_{ijt} = \alpha + \beta\, Treat_i \times Post_t + \mathbf{X}_i'\zeta + \theta MedIncome_j \times Post_t + \gamma_j + \delta_t + \varepsilon_{ijt} \tag{7}$$

This alternative specification allows neighborhoods with different characteristics to have different time trends.

Anticipation. Another concern for the validity of the implementation of the DiD analysis is the possibility that market players anticipated the adoption of the rent control law, in which case it would be possible that rent prices reacted before the law was legally introduced in September 2020, thus biasing our main results. On the one hand, if we suppose that landlords in treated cities anticipated the new law adoption, we would expect them to ask disproportionately high rents to countervail the future constraint on rents. As a result, we would overestimate the effect of the law of on rents. On the other hand, the anticipation of rent control regulations may make tenants more motivated to try to negotiate a lower rent, as this would allow them to lock in a lower rent before the regulation takes effect. In this case, we would likely underestimate the effectiveness of rent control in reducing rents.

To show that this is not a serious concern in our setting, we collected data from Google Trends to check whether people's interest towards the rent control regulation started to differentially increase in treated *versus* control municipalities prior to the introduction of the law. Figure A1 in the Appendix suggests that the search trends in keywords 'rent control law' ('ley alquier') do not significantly differ between municipalities in the two groups, thus reinforcing our trust that anticipatory behavior is not a concern in the Catalonian rent control context.

5.2 Main Estimates

5.2.1 Effects on Housing Rents

Table 2 reports the estimated coefficients derived from the estimation of model (6) using as dependent variable the log advertised rent per squared meter. All specifications include year-month and zip-code fixed effects. Column (1) reports the estimates of the baseline specification. Column (2) adds controls for the property size and sub-type. Column (3) adds controls for the characteristics of the unit. Column (4) controls for the property status. Column (5) includes all the property-level controls. Column (6) adds an interaction term between the zip-level median income recorded before rent control was passed and the indicator for the post-treatment period. Column (7) adds a linear zip-code specific trend fitted to the treatment zip-codes.

Our results suggest that rent control did not cause any substantial reduction in average logged rents, as coefficient estimates are not statistically significant at conventional levels. In fact, in Column 1, when location and time fixed effects are included, we find that asked average rents increase by about 0.6% ($p > 0.10$) relative to uncontrolled municipalities after rent control was introduced. The estimated effect of rent control becomes negative as more controls are added, reaching about -3.5% when we include all property level and time-varying zip-code level controls (Column 7). However, the impact on average rents remains not statistically different from zero, suggesting that the rent control policy is ineffective in curbing rental prices.

5.2.2 Effects on House Sale Prices

Table 3 reports the estimates of the impact of rent control on sales prices which are derived from the estimation of the baseline model (6) and using as dependent variable the log advertised sale price per squared meter. Similarly to the analysis on average rents, we estimate seven different specifications (columns (1)—(7)), always including year-month and zip-code fixed effects. In contrast to the case of rents, we find that rent control caused a substantial decline in sales prices, statistically significant at the 1% level, across all of our specifications. Furthermore, the impact on sales prices increases in magnitude as more controls are added, with the effect ranging from -2.3% in the baseline specification (Column 1) to -3.7% when all property level and time-varying zip-code level controls are included (Column 7). The fact that rent control decreases property values is consistent with our theoretical model, in which property values internalize the negative effect of future reduced rents due to the new regulation.

5.2.3 The Time Path

Next, we examine the dynamic effects of rent control by estimating the average treatment effect across different lengths of exposure to the new regulation. To this end, we estimate the following event study model:

$$Y_{ijt} = Treat_i \times \sum_{\tau \neq -1} \beta_\tau I(t - t^* = \tau) + \mathbf{X}_i' \zeta + \gamma_j + \delta_t + \varepsilon_{ijt}. \quad (8)$$

As before, we denote by Y_{ijt} either the logged advertised rent or sale price per squared meter of a property i located in zip-code j at year-month t. γ_j and δ_t denote the zip-code and year-month fixed effects, respectively, which allow to control for fixed differences across locations and trends over time.

The indicator variable $Treat_i$ equals 1 if property i is located in a municipality that adopted rent control, and 0 otherwise. The indicator variables $I(t - t^* = \tau)$ measure the time difference with respect to the month when rent control was implemented (denoted by t^*). This indicator is equal to 0 for every month if the property is in an unregulated municipality. We posit that treatment occurs in period $\tau = 0$ and omit the event time dummy for the month immediately prior to the rent control adoption, $\tau = -1$. As a consequence, each estimate of the coefficients β_τ measures the treatment-control price differential change in period τ, relative to the month prior to the introduction of rent control.

Figure 2 plots the event study estimates β_τ from equation 8, associated with event times $\tau = -3$ to $\tau = 16$. Panel (a) of Figure 2 shows that relative rents in regulated municipalities start to decrease in the months following the introduction of the rent control law. However, this effect reverses soon afterwards and it completely fades out one year after the adoption of the law, meaning that rents in treatment municipalities level up with their original levels before rent control. At the same time, we note that the confidence intervals for the rent estimates are relatively large, encompassing zero for most observations, suggesting that most of the reported effects are not statistically significant. In Section 6, we will further investigate the heterogeneity by disentangling the effects at different quantiles of rents, allowing us to better understand the sources of variation in the effects of rent control.

In Panel (b) of Figure 2 we see that the time path of sale prices is different than the path of rents in Panel (a). In the short run, house prices exhibit little to no reaction to the adoption of the rent control law. However, about one year after the law adoption, house prices start decreasing continuously. Thirteen months after the law adoption, relative prices in regulated areas lost almost 5% with respect to the price level before the law was implemented. Thus, we observe a time lag between the introduction of the law when rents become capped from above and the moment when market prices start capitalizing the imposed reduction in rents.

Importantly, we confirm that the estimated coefficients in the pre-treatment leads (namely for periods associated with event times before $\tau = -1$) are statistically zero for both outcome variables (rents and house prices) with the only exception of one pre-treatment period for the outcome variable sale prices. This suggests that both rent and sales prices were trending similarly in regulated and unregulated areas prior to the law, which reinforces our confidence in the main DiD identification assumption.

5.3 Effects on Rent and Sales Volumes

In light of our findings on the behavior of rents and, even more remarkably, sales prices after the law adoption, we examine now the extent to which rent control also impacted the volumes of rental and for-sale housing units. To this end, we aggregate our data by month and municipality, and then regress the logged number of rent and sales ads on an indicator which takes value equal to 1 for rent controlled municipalities after the introduction of rent control law, and 0 otherwise. Table 4 reports the estimated effects on rental volumes (Columns 1 and 2) and sales volumes (Columns 3 and 4) when controlling for year-month and municipality fixed effects. Columns 2 and 4 also add control variables for average property characteristics.

For the case of rental ads, both columns (1) and (2) show that the volume of rent ads significantly declined in municipalities that adopted the rent control law relative to uncontrolled ones. We find that the fall in the logged number of rent ads is between 30% and 32% with a p-value $p < 0.01$.

In Columns (3) and (4), we find the opposite effect on sales volumes. In particular, rent control significantly increases sales ads by between 13% and 18% ($p < 0.01$). Coupled with the substantial decline in sales prices documented in Table 3, the large impact on sales volume suggests a net increase in the for-sale housing stock following the introduction of rent control.

These results are consistent with our theory, where market outcomes are supply-driven. The intuition is that some landlords who were renting their properties decide to sell their properties after realizing that renting became less profitable due to the control law on rents. Our findings on the increase in the for-sale housing stock and on the reduction of the rental stock due to a shift from rental to owner-occupied status of rent controlled housing units have also been documented in the literature (Sims, 2007; Mense, Michelsen and Kholodilin, 2023; Diamond, McQuade and Qian, 2019; Ahern and Giacoletti, 2022).

Finally, we illustrate the dynamic treatment effects of the law on rent and sales volumes in Figure 3. The graph for the dynamics of logged rental ads is in Panel (a). The graph for the logged for-sale house ads is in Panel (b). Our estimation model corresponds to an event-study, in the same spirit of the one outlined in equation 8. Here we also pool the data at the month-by-municipality level. In Figure 3 we can see that the magnitude of the effect is increasing in time for both outcome variables.

5.4 Robustness

The above estimated specifications have proved that the effects reported are robust to time-varying zip-code level differences and linear zip-code trends. In the rest of this section, we summarize the results of other important robustness checks. First, we verify the absence of pre-trends in our outcome variables. Secondly, we conduct a number of falsification tests considering placebo treatment dates and different control groups. Thirdly, we show that our results are robust to outliers on the top and bottom of the price distribution. Additionally, we test the sensitivity of our estimates to clustering at different geographical location levels. We then conduct an additional check to make sure that our estimates are robust to the inclusion of fixed effects defined at different geographic levels. Furthermore, we explore the robustness of our estimates to the choice of the control group. Finally, we check that the results are robust to excluding the city of Barcelona from the sample, as well as show that our main results are not driven by the effects of Covid-19.

Pre-treatment trends. We have already shown the absence of differential trends in average rents (Figure 2a) and sales prices (Figure 2b) for treatment *versus* control municipalities prior to the introduction of rent control. If average rents or prices for controlled and non-controlled municipalities exhibit parallel trends prior to rent control, the estimates of β_τ from equation (8) associated with event times prior to $\tau = -1$ should not be statistically different from zero. Figure 2 confirms that, before rent control was introduced, none of the outcomes was trending upwards nor downwards in the municipalities that later adopted rent control.

Placebo tests using pre-treatment periods. To provide further evidence on the absence of pre-trends in rental and housing prices, we conduct falsification tests using placebo treatment dates. We re-estimate the baseline DiD specification in equation (6) using only data from periods prior to the actual treatment, and estimate the treatment effect for two placebo treatment dates: July and August 2020. The analysis is conducted using only observations prior to September 2020, when the rent control law was actually enacted. As there was no actual policy change during these months, we expect the estimated treatment effect to be insignificant.

Reassuringly, results presented in the Appendix Table A3 (Columns 1 to 4) show no significant changes in advertised rents for treatment *versus* control municipalities for all of the aforementioned placebo treatment dates. The same holds true for sales prices (Columns 5 to 8), as can see that the difference in sales prices between treatment and control municipalities does not significantly change after any of the placebo treatment dates. This provides further evidence that the treatment effect is not driven by pre-existing trends in rent and housing markets prior to the implementation of rent control.

Robustness to outliers. For this robustness check, we trim the top and bottom 1 percent of the distribution of prices by month, for both data sets on advertised rents and sales. Appendix Table A4 shows that the results are virtually unchanged.

Robustness to measurement error. Since our identification strategy exploits within-municipality variation in outcomes over time, we wish to check that results are not driven by (presumably small) municipalities for which we do not actually observe price distributions across months, as well as by municipality-month observations with overly volatile supply. Much of this variability, in fact, is likely to reflect measurement error. Total supply (and, in turn, also average prices) can vary considerably across months in a municipality (and especially in small municipalities), which in turn creates substantial shifts in municipality-by-month outcomes (as obtained when collapsing the ad-level observations by month and municipality).

In this robustness check, we trim the data set by eliminating (*i*) municipalities that have ads only over less than five months, (*ii*) municipality-month observations with only less than six ads, (*iii*) municipality-month observations with overly volatile number of ads.[16] Finally, we drop municipalities with overly volatile supply over more than 1/6 of the monthly observations. Namely, if more than 1/6 of the monthly observations are excluded under criteria (*i*)-(*iii*), we also exclude the remaining ones.[17]

Appendix Table A5 presents results from the restricted sample excluding municipalities with low or unstable supply, for rents (Columns 1-3) and sales prices (Columns 4-6). In the case of rents, the estimated effect is not statistically significant from zero in any of the specifications presented. As for sales prices, the estimates confirm that rent control depresses property

[16] For the latter criterion, we exclude municipality-month observations with supply more than double the municipality's average supply, as well as municipality-month obs with supply more than 60% above or below the prior year's supply. We also identify municipality-month observations with supply more than 50% above or below the municipality's constant-growth-rate trend. That is, we remove a linear trend and drop the observation if the residual is too large.

[17] This sample selection is in the same spirit of Lafortune, Rothstein and Schanzenbach (2018), in which the authors select the sample to reduce the volatility of summary measures at the state-by-year level, obtained by collapsing the student-level measures.

values, mirroring the main analysis with magnitudes between -0.020 and -0.038, consistently significant at the 1 percent level.

Location fixed effects and clustering. Next, we conduct an additional check to make sure that our estimates are robust to the inclusion of fixed effects defined at different geographic levels. In particular, we estimate the baseline specification with location fixed effects defined at the level of the municipality, instead of the zip-code level. In this case, we also cluster standard errors at the municipality level. Since in this setup the unit of observation is more detailed than the level of variation of treatment, this more conservative clustering under consideration might be more effective in dealing with serial correlation issues across properties located in municipalities observed in many points in time (Bertrand, Duflo and Mullainathan, 2003). Results are presented in Appendix Table A6 for rents (Columns 1 to 3) and sales prices (Columns 4 to 6). As expected, while standard errors do increase, our coefficient estimates are virtually unaffected, nor do they miss statistical significance at conventional levels.

Alternative control groups. We also explore the robustness of our estimates to the choice of the control group. First, we extend our control group to include all municipalities in Catalonia, therefore including other municipalities without rent control but still located in the same region of our regulated municipalities. The estimated impacts on rental and sales prices are presented in Appendix Table A7, with corresponding volume effects in Appendix Table A8.

Next, we include in the control group properties throughout Spain to verify the consistency of our results. The outcomes for rents, sales prices, and volumes are respectively reported in Appendix Tables A9 and A10. We also generate dynamic treatment effects of the rent control law on rents, with Appendix Figure A2 illustrating these effects when the control group includes properties in the rest of Catalonia (Panel A) and Spain (Panel B).

These variations in control groups confirm that the direction and magnitude of the observed effects align closely with our primary analysis. The only exception emerges in rent estimates: when expanding the control to include municipalities in Catalonia and Spain that were never subject to rent control, the average effect becomes negative and statistically significant at the 1 percent level, ranging between -0.030 and -0.060. At the same time, however, the event studies when considering these alternative treatment groups (shown in Appendix Figure A2) reveal that these negative effects stem from short-term rent declines within the first nine months following rent control implementation, which then fade out after a year. We also note a visible downward trend in the treated group compared to never-treated municipalities in the rest of Spain, which raise some doubts on the reliability of these estimates.

Excluding Barcelona. We also check that the results are robust to excluding the city of Barcelona from the sample. In fact, as compared to other cities (both regulated and unregulated), Barcelona stands out as an outlier compared to other cities in Catalonia due to its larger population, highly developed tourism sector, and robust economy. With over 1.6 million inhabitants, it is by far the most populated city of Catalonia, considering that the second-largest city, L'Hospitalet de Llobregat, has just over 250,000 inhabitants. Additionally, the city's highly developed tourism sector attracts both domestic and international tourists. Finally, its highly developed service sector and a growing startup ecosystem make Barcelona a hub of innovation and entrepreneurship, making it differ from the rest of Catalonia also in terms of labor market structure.

Appendix Table A11 presents the results for rents (Columns 1–3) and sales prices (Columns 4–6). Excluding Barcelona from the group of regulated municipalities has limited impacts on the estimated effects on both rents and sales prices. Rent control has no statistically significant impact on rent, but significantly reduces sales prices, with estimates ranging from -0.018 to -0.069.

Addressing Covid-19 confounds. Our sample period starts in June 2020, as the months immediately prior to that coincide with the outbreak of the Covid-19 pandemic. Covid-19 affected many aspects of the global economy, including the rental and housing markets. The outbreak and subsequent lockdowns have disrupted the functioning of markets, leading to significant changes in supply and demand for housing. Therefore, it is essential to make sure that our main results are not driven by the effects of Covid-19 on the rental and housing markets.

First, we show that our results are robust to the Covid outbreak by adding a control for the number of reported Covid cases per capita, measured for each month and municipality.[18] This control variable can help to account for any changes in the rental and housing markets that may be directly related to the pandemic, such as changes in demand for housing due to Covid-related health concerns. As shown in Appendix Table A12, the estimated treatment effects of rent control remain robust to the inclusion of this control variable.

Second, we expect that municipalities with higher touristic activity and population have experienced a greater reduction in housing demand due to Covid-19. Touristic cities, characterized by a higher concentration of short-term rentals like Airbnb, experienced a significant decrease in demand for such accommodation during the outbreak. This oversupply of short-term rentals can put downward pressure on housing prices and rents, potentially biasing the estimates of the rent control policy. Moreover, anecdotal evidence suggests that some AirBnb hosts converted their short-term rentals into long-term rentals to hedge the risk of lower demand during the Covid-19 pandemic, thereby increasing the supply of rental units. In this case, our findings regarding the decrease in rental supply due to the introduction of rent control may actually be a lower bound of the true effect, rather than an overestimation. Furthermore, municipalities with greater touristic activity tend to have a higher population and, as such, may have been more affected by the spread of the virus, leading to a decline in housing demand due to health concerns.

To address these concerns, we augment our baseline DiD specification in equation (6) with interaction terms between the treatment indicator and population size, as well as the number of hotels per capita (as a proxy for touristic activity). The results, shown in Appendix Tables A13 and A14, respectively, reveal that the interaction term between treatment and population size is consistently positive and statistically significant, indicating that the effect of the rent control policy on reducing rents and house prices is actually smaller in larger municipalities. Furthermore, the treatment effect of rent control on rents and house prices remains robust to the inclusion of both interaction terms. This suggests that our main results are not driven by the impact of Covid on municipalities with more touristic activity or higher population size.

Overall, these tests provide evidence that the observed reductions in rents and house prices

[18]See `https://www.idescat.cat/dades/obertes/covid` for the data.

in treated municipalities are primarily due to the rent control policy, rather than the Covid outbreak or other factors that may be correlated with population size or touristic activity.

6 Policy Impact Heterogeneity

In this section, we extend our analysis by examining the heterogeneity in the impact of rent control. Section 6.1 reports our empirical findings on the impact that rent control has on the different quantiles of the advertised rents and prices distributions. Section 6.2 explores how the impact of rent control varies depending on selected municipality and neighborhood characteristics, namely the proximity to the center, population density and the share of renters. Finally, Section 6.3 assesses rent control's net impact, balancing its benefits for tenants against costs to property owners.

6.1 Rent Control and Municipal-level Prices Distributions

6.1.1 Empirical Results

In what follows, we study the impact of rent control on the distribution of rents and sale prices. We do this by estimating counterpart specifications to our baseline model (6), using as dependent variables Y_{jt} a set of municipality-specific moments of the advertised rents or prices distribution.[19] In particular, we study the effect of rent control on the standard deviation of the rent and sales price distributions, as well as on average prices below and above the median, below the first and above the tenth decile, and within each quartile of the distributions. To this end, we begin by computing the considered quantiles (the median, top and bottom deciles, and the four quartiles) at the municipality-by-month level. Then, we generate our dependent variables by averaging the rent and sales prices within each municipality-by-month quantile.

The key parameter of interest, β, from specification (6) measures the difference between each of these moments in municipalities that introduced rent control and those that did not. All regressions include municipality and year-month fixed-effects. We also add municipal-level month-specific controls for average characteristics of the advertised units and zip-level socio-economic indicators. In all regressions, standard errors are clustered at the municipality level.

Figure 4 graphically illustrates our main results, showing the heterogeneity effect of the rent control policy on the four quartiles of the municipality-level distribution of rents (Panel 4a) and sales prices (Panel 4b). These figures include controls for average unit characteristics and municipal demographics. In Figure A3 in the Appendix, we present the counterpart plot of the effects in the short-run—specifically the nine months following the adoption of rent control—confirming that the heterogenous effects of the policy that we document below already emerge in the short-term.

In Panel 4a we can see that rent control is effective in decreasing rents at the bottom of the distribution, while average rents in the top third and fourth quartiles are essentially unaffected. This suggests that tenants in relatively cheaper rental housing, presumably low-to-

[19] Note that we run this analysis at the municipality level—instead of considering zip-codes, as done in the main analysis—so that we have enough observations within the considered quantiles.

lower-income tenants, are the ones who benefit most from the new regulation. At the bottom quartiles of the rent distribution, rent control causes a fall in asked average rents by about 7-8% relative to uncontrolled municipalities.

Despite being smaller in magnitude, Panel 4b suggests that the effect of rent control on house prices is also negative for the bottom quartiles, causing a fall in sale prices of about 4-5%. Unsurprisingly, since rents fall for cheap rental properties at the bottom of the rent distribution, the expected value of future discounted cash flows is now lower, which in turn decreases the sale price of these bottom-quartiles properties. We find the opposite effect in the top quartile of the distribution of sale prices since the value of the most expensive properties increase by about 4% after the introduction of rent control.[20]

We conclude that the rent control policy adoption in Catalonia initially achieves one of the goals for which it was implemented: it makes relatively poorer households in the rental market better off in the short term, as the rents that these households pay decrease after the policy adoption. However, it is not clear how the gains made by poor renters because of rent control compare with the cost of lower housing valuations. We do this in Section 6.3 below.

In addition, this policy also has unintended consequences, consequences that compound as time goes on. First, low-to-middle income households who own properties in rent control municipalities become worse off with the policy adoption since property prices fall at the bottom quartiles of the housing price distribution. This is particularly harmful for this part of the population, who represent a significant portion of the working class, because the value of their home typically represents their most significant financial asset. Second, high-income households who presumably own the most expensive houses benefit from the regulation by seeing the value of their properties increase. Third, owners of the most expensive properties benefit from the long-term increase in rents after the adoption of rent control in treated municipalities, as sales prices increase and their ability to profit off of housing as an asset class also increases. Therefore, we argue that the policy seems to have unintended effects on increasing the difference in property values between the bottom and the top of the price distribution, which might potentially lead to increased wealth inequality.

6.1.2 Mechanism

To better understand the mechanism behind the empirical results reported above in Section 6.1.1, we extend our benchmark theoretical model of Section 3 to the case of household income heterogeneity. For the sake of brevity, we leave the details of this exercise and the associated formal results for the Appendix (see Section B.2).

The extension of our model rationalizes the empirical findings reported in Section 6.1.1. Roughly speaking, Proposition 3 in the Appendix claims that when there is a low elasticity of for-sale cheap housing supply with respect to rents in the rent-controlled and a high elasticity of for-sale expensive housing supply with respect to rents in the unregulated region, then

- both the ratio of cheap housing rents between the regulated and the unregulated regions

[20] This result may justify a progressive taxation on home sales to recapture the increase in funding following the adoption of rent control.

and the ratio of cheap housing prices between the rent controlled and unregulated regions decrease after the adoption of rent control;[21]

- the ratio of rents for expensive housing between the regulated and the unregulated regions remains roughly constant;[22]
- the ratio of expensive housing prices between the regulated and the unregulated regions increases.[23]

The mechanism behind the results in Proposition 3 in the Appendix is similar to the mechanism outlined in Proposition 1. In a nutshell, rent control affects the supply of housing units through investor's non-arbitrage condition. The only difference with Proposition 1 is that we now allow for migration to generate the changes observed in figure 4. We refer to Section B.2 in the Appendix for further discussion.

6.1.3 Robustness

In order to show the robustness of these results, we document the effects of rent control on all the above-mentioned moments of the advertised rents distribution in Table 5 and on that of sales prices in Table 6. In both tables, Panel (a) considers the baseline specification including only municipality and year-month fixed effects. Panels (b) and (c) additionally control for the time-varying mean characteristics of the advertised units, and socio-economic indicators, respectively. Finally, Panel (d) pulls all the controls together.

In column (1) we see that rent control increases the standard deviation of the rent distribution by about 5 percent. Also, the variance of the house price distribution increases, by about 1.6 - 2.2 percent. This is consistent with the effects by quartile, presented in Figure 4, which increase as we move from the bottom to the top of both the rent and price distributions.

The fact that rent control has opposite effects at the tails of the rent distribution and, especially, at the tails of the house price distribution is confirmed by considering alternative moments. Columns (2) and (4) report average effects for rents and prices below the 50^{th} and 10^{th} percentiles, while columns (3) and (5) for those in the top 50^{th} and 10^{th} percentiles, respectively. Starting from rents, Table 5 shows that the adoption of rent control decreases rents by about 7% ($p < 0.01$) for rents below the median (column 2), with no effect for those above (column 3). These effects are even larger in magnitude when considering the bottom and top 10 percentiles (columns 4 and 5), with rents decrease by about 8% at the left end of the rent distribution ($p < 0.01$), and slightly increase (though the effect is not statistically significant) at the right end.

The impact of rent control on house prices (Table 6) has similar effects for house prices at the bottom of the price distribution, ranging from about -3.3% for units below the median to -5.0% for those in the bottom decile. By contrast, the opposite holds true for house prices at the top of the price distribution. In fact, the effect of rent control on house prices is positive and statistically significant at the right end of the house prices distribution. House prices in the

[21] In terms of the new notation of the extended model in the Appendix, R_L^C/R_L^N and p_L^C/p_L^N decrease.
[22] i.e., R_H^C/R_H^N is constant.
[23] i.e., p_H^C/p_H^N increases.

top decile are on average about 5.7% higher in municipalities with rent control as compared to uncontrolled ones (column 5). The effect is similar (around 3.7%) for sales prices in the top quartile (column 9).

Taken together, these results suggest that the variance of both the rent and the sales prices distribution seem to go up after the introduction of rent control. This increased polarization is actually favoring the lower segment in the rental market, as rents at the bottom become more affordable. At the same time, in the owner-occupied housing market, inequality between the top and the bottom of the sales price distribution also increases. Thus, the owners of relatively cheaper houses are now worse off, in that they see the value of their houses fall further, compared to the value of houses in the opposite tail of the distribution.

6.2 Rent control and neighborhood characteristics

To further corroborate these findings, we investigate whether the impact of rent control varies based on the proximity of properties to the central areas of the respective cities. We should note that in this calculation the most expensive properties are likely located near the center, with prices decreasing as the distance from the center increases.

To assess the impact of proximity to the center, we employ two measures of distance: air (or straight-line) distance and travel distance by car. Central properties are defined as those located within a 2-kilometer distance from the city center, which is identified as the point of highest density in satellite images. We then interact the treatment indicator from equation 6 with indicators denoting whether a property is located in the central versus the surrounding districts.

Table 7 presents the results where central properties are identified using air distance from the center. Appendix Table A15 further validates the robustness of our findings using travel distance by car. Although not significantly different from zero, the estimated effects on rents are observed and suggest a negative impact only in less central municipal areas (Columns 1–4), showing a decrease in rents of approximately 2.0–2.5% following the implementation of rent control. Consistently with this, sales prices in these areas experience a statistically significant decline of about 10-12 percentage points ($p < 0.01$). Conversely, sales prices in central areas increase by about 3% (although the latter effect is not significantly different from zero).

These findings provide evidence that rent control primarily affects housing markets in peripheral areas, while having limited impact on the central districts of municipalities. These results are consistent with the previous findings of rent and sales price reductions at the bottom of the respective distributions, as discussed earlier in this section.

Finally, we study how the effects of rent control vary with population density and amount of rental housing in a municipality. Since we have documented a decrease in rents at the bottom of the rent distribution in municipalities that introduced rent control, we hypothesize that rent control was most effective in decreasing rents in areas that have the highest population density and the highest share of renters. Indeed, these areas are expected to feature typically lower rents. Therefore, we ask whether the difference-in-differences effect of introducing rent control differs in more versus less densely populated municipalities, or in municipalities with a higher or lower proportion of renters.

In order to study the heterogeneity in the treatment effect of rent control, we first divide treated municipalities by quartiles of the distributions of the considered characteristics. We then estimate our baseline specification (6) separately for the treated municipalities falling in each of the four quartile groups based on the population density and share of renters distributions. As dependent variables, we consider both the logged rent and sales price of the advertised unit. All regressions include municipality and year-month fixed-effects. We also consider model counterparts where we add the set of controls for property and municipal characteristics used in our main analysis. In all regressions, standard errors are clustered at the municipality level.

Figure 5 illustrates the heterogeneity results for population density. Panel (a) considers the rent control impact on rents, while panel (b) considers the impact on sales prices. Looking at the chart on rents, we see that the effect of rent control appears to decrease as we move from less to more densely populated areas, suggesting that the policy decreases rents more significantly in less densely populated municipalities. Admittedly, the point estimate for relatively highly populated treated municipalities—those in the third quartile—is slightly higher than the estimate for less populated areas. However, these differences are not statistically significant. By contrast, panel (b) of Figure 5 reveals a marked heterogeneity in the effect of rent control on sales prices. The decrease in sales prices following rent control, in fact, seem to be driven by the most densely populated municipalities, where sale prices decrease by about 5-6% ($p < 0.01$) relative to the control group.

Similar findings are documented in Figure 6 where we illustrate how the effect of rent control changes with the municipal share of individuals in rental schemes. While it is somewhat true that rents decrease more markedly in areas with many renters (Panel a), the effect on sales prices is clearly monotonically decreasing as we consider municipalities with a higher concentration of rents. In the bottom quartile, the effect of rent control on sales prices is given by a statistically insignificant -1.5%, while in the top quartile, it is given by -5%, significant at the 1% level.

The finding that sales prices went down especially in the more densely populated municipalities with a higher share of households living in rental schemes corroborate our hypothesis that rent control makes house prices decrease in the municipalities where income inequality is more significant. Thus, the concentration of sales price impacts in these urban areas likely further exacerbates wealth inequality, especially among families where housing is their primary asset.

6.3 Discussion

In summary, we have documented the impact of the rent control policy on rental prices and housing values across different quantiles of the respective distributions. Our findings indicate that rent control led to a reduction in rents for properties located in the lower quantiles of the distribution, while leaving rents in the top quantiles unaffected. Consequently, tenants residing in the lower end of the distribution experienced greater gains from the policy. These findings differ from Ahern and Giacoletti (2022), who found that middle-to-upper income tenants benefit more from rent control than lower income tenants, but are consistent with the majority of the literature, including Olsen (1972)'s classic study, as well as more contemporary empirical studies, such as Mense, Michelsen and Kholodilin (2023), and theoretical studies, such as Favilukis, Mabille and Van Nieuwerburgh (2022).

Concurrently, house prices in the bottom quantiles of the price distribution experienced a decline, whereas prices in the top quantiles exhibited an increase. As a result, homeowners situated in the lower end of the distribution incurred losses, while those at the top are actually benefiting from the policy. This finding is consistent with the initial finding of Ahern and Giacoletti (2022) and Mense, Michelsen and Kholodilin (2023), in that all three studies found property values decline. However, our study differs from these other results, in that we are able to show how the price decline is actually concentrated in its impact, hurting the lowest income homeowners more greatly, while it actually seems to benefit upper income home-owners.

To get a sense of the quantitative magnitude of the estimated effects, we conduct a thought experiment. We explore by how much the aggregate pre-policy values of rentals and sales advertised in treated municipalities would have shifted if rent control had been in place. We explore how these values would have been impacted by rent control across different quartiles to capture the differential effects across segments of the housing market. We then use our back-of-the-envelope calculation to assess whether the benefits outweigh the costs associated with the implementation of rent control.

Specifically, we first compute the aggregate values of rentals and sales advertised in treated municipalities over the pre-policy period (June-August 2020) for the different quartiles. We then use the estimated average percentage change in rents and sales prices following the introduction of rent control to compute the corresponding estimated change of rentals and sales price values in each quartile. Finally, we convert the rent values to yearly figures for better comparability.

Results are shown in Table 8, which presents the aggregate gains and losses in rents (Panel A) and sales prices (Panel B) resulting from the implementation of rent control. Column 1 represents the total euro value of rentals (Panel A) and sales prices (Panel B) in each quartile, aggregated over the three-month pre-policy period in treated municipalities. Column 2 reports the percentage change in rents and sales prices, from our preferred specification from Panel D in Tables 5 (for rents) and 6 (for sales), which incorporate all property- and municipal-level controls. Finally, to provide a comprehensive analysis of the gains and losses, we calculate the corresponding change in euro values in Column 3. These values represent the estimated change in rental and sales price values based on the percentage change estimates from Column 2. Finally, Columns 4 and 5 project the change in rental values over longer time frames (one and ten years, respectively).[24]

In Panel A on rents, we observe a significant decrease in rental values in the bottom quartile, with a corresponding decrease of -€2,014,467 (corresponding to a percentage change of -0.079). This represents the lower rental cost that renters in the lower end of the distribution benefit from. On the other hand, rental values in the top quartile increase by €383,965 (corresponding to a percentage change of 0.014), suggesting that renters in higher-income brackets are relatively unaffected by the policy. This differential impact highlights that the gains expe-

[24]For the ten-year projection, we use the European Central Bank deposit facility interest rate as of June 2023, set at 3.50%.

rienced by the lower-income individuals and families exceed the increase in rents for the most expensive units.

In Panel B on sales prices, we can see that the bottom quartile experiences a significant decrease in sales prices, with a corresponding decrease of -€460,193,817 (corresponding to a percentage change of -0.042). This implies that property owners in the lower quartile bear the brunt of the policy, facing a loss in the value of their properties. In contrast, the top quartile sees an increase in sales prices by €1,088,737,029 (corresponding to a percentage change of 0.037), indicating that property owners in higher-income brackets benefit from the policy. Once again, this differential impact reflects the redistributive consequences of rent control, with gains and losses distributed across different segments of the housing market.

These results emphasize the trade-offs and challenges associated with rent control policies. While they can provide relief to lower-income renters by reducing their housing costs, they may also lead to losses for property owners in the lower income segments of the owner occupied market. Additionally, given that the average share of renters across the 59 treated municipalities is 12.7% (with a median of 12.2%), the proportion of households benefiting from the policy is relatively low, when compared to the proportion of households that will experience impacts from changes in home sale prices.[25]

In conclusion, our analysis suggests that the new rent control regulation benefited to some extent low- and medium-income tenants (up to the 75% income percentile) by reducing annual rental housing costs by about €8 million in total. Even if discounting this benefit during a 10-year window, total benefits of rent control for low- and medium-income tenants would amount to €68 million approximately, still an insignificant amount when compared to the €1 billion loss in property values for the working-class. Other redistributive and housing policies may prove more efficient.

7 Conclusion

As the housing affordability crisis escalates, rent control policies are experiencing revived popularity, placing them on the agendas of policy makers all around the world. However, to date the bulk of the empirical evidence concerns the introduction or removal of rent control policies in single cities in the U.S., such as Cambridge, Massachusetts, San Francisco, California, and, more recently, St. Paul, Minnesota (see Autor, Palmer and Pathak, 2014; Diamond, McQuade and Qian, 2019; Ahern and Giacoletti, 2022). While these studies suggest tenants benefit from the insurance provided by rent control, they also document several unintended consequences of such regulatory policies. For instance, evidence suggests that rent control leads to a deterioration in housing quality (see Andersen, 1998; Glaeser and Luttmer, 2003), reductions in mobility(see Diamond, McQuade and Qian, 2019; Krol and Svorny, 2005; Mense, Michelsen and Kholodilin, 2023), and debatable impacts on segregation (see Sims, 2011; Glaeser and Luttmer, 2003; Enström Öst, Söderberg and Wilhelmsson, 2014).

In this paper, we add to this literature by assessing the consequences of the introduction of

[25] At the same time, we note that the share of renters is notably higher in specific cities such as Barcelona and Figueres, reaching the maximum values of 24.9% and 27.1%, respectively. For the distribution of the share of renters across always treated municipalities, please refer to Figure 7.

large-scale rent control in more than sixty cities in the region of Catalonia, Spain, as of September 2020. The new regulation imposed a rent cap on all new rental contracts in the targeted cities that are identified as tight housing markets. We evaluate its impact on the dynamics of rental and sales prices in controlled *vs.* uncontrolled areas. In our main analysis, we include all cities neighboring regulated municipalities in the control group. Importantly, because the policy was introduced in several cities at the same time, we explore the heterogeneity in the policy impact across the rental and sales price spectrum.

Our first set of results suggest that the rent control policy is ineffective in curbing rental prices, as average treatment effects are not significantly different from zero. Despite a moderate decrease in rents in the short-term of about 5% in regulated areas as compared to the unregulated neighboring areas, in the long-run—namely after one year from the introduction of rent control—rental housing supply significantly decreases by about 30%-32% and, at the same time, rents start rising again. The results on rental prices are mirrored by a significant reduction in rental housing supply, thus pointing to the unintended side effect of reducing rental housing availability for low-income segments of the population.

By contrast, we document large and statistically significant negative effects on house prices, as controlled municipalities see house prices fall by an average of about 2.3%-3.7%. Accordingly, we document a large increase in sales volumes, which increase by 13%-18% in treated cities following rent control, relative to neighboring uncontrolled municipalities. This suggests that rent control, by decreasing the expected return of rental housing, causes a substantial reduction in the size of the rental housing stock, as well as in real estate property values. Our findings thus complement Ahern and Giacoletti (2022) in that home prices fall after the introduction of rent control and Diamond, McQuade and Qian (2019) in that there is an additional resulting reduction in housing supply.

These estimated effects are robust to the inclusion of a wide range of controls, including property characteristics, as well as year-month and location fixed effects. We also perform several sanity checks to demonstrate the robustness of our estimates. These include the absence of pre-trends in the considered outcome variables, clustering at different geographic location levels, and changing the estimation sample as well as the control group considered. Moreover, supporting a causal interpretation of our results, our estimation strategy yields coefficients that are statistically indistinguishable from zero when using placebo treatment dates.

In a second step, we investigate the differential impact of the new regulation across the rental and sales prices distributions. We find that the impact of rent control on the rent distribution has opposite signs at different points of the distribution: negative at the bottom and virtually zero effect at the top of the distribution. Therefore, we can conclude that the new policy is effective in limiting rent increases for rental segments with the lowest rents, which are plausibly the ones where rental distress is disproportionately concentrated. Additionally, these effects are evident in the short- and in the long-run, respectively nine months and almost two years after rent control implementation.

At the same time, however, our heterogeneity results highlight that in rent controlled areas properties at the bottom of the price distribution lose value, while properties at the top gain value, as compared to the unregulated areas. As a consequence, in the owner-occupied housing market, the gap between the bottom and the top of the sales prices distribution increases, which makes owners of the cheaper housing units, and thus owners who are also plausibly relatively lower income, now worse off. Here, our results contrast with those of Ahern and Giacoletti (2022) in that while they saw virtually no distinguishing between lower and upper income owners in terms of absorbing the costs of rent control, our study shows the results are much more likely disproportionately concentrated on lower income segments of the owner-occupied population.

These results are confirmed by the evidence that rents decrease relatively more in peripheral areas, while the central parts of the cities show minimal changes in rental prices after the introduction of rent control. Similarly, rent decreases are more marked in more densely populated cities where the share of rental housing is relatively higher. Consistently, these are also the areas where sales prices experience the most marked reduction, while remaining virtually unchanged or slightly increasing in presumably richer areas.

To conclude, in spite of reduced rent increases for the lower-income segments of the population, which are possibly only temporary, the decrease in the rental stock and the widening gap in property values highlight important unintended consequences of rent control. To the extent that lower rents lead to a reduction of housing availability for low-income households, preventing future increases will become more and more difficult. Moreover, the reduction in house prices, disproportionately concentrated at the bottom, likely contribute to exacerbating the financial distress for lower-income owners. Such side effects reduce the overall benefits of the rent control policy, with potentially far reaching consequences for wealth inequality across the income spectrum.

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8 Tables and Figures

Table 1: Summary statistics for rent and sales ads

	N	Mean	S.D.	Median
	Panel A: Rent Ads			
Price	216,240	1,305.31	988.83	1,050.00
Price per squared meter	216,240	16.02	8.03	14.00
Treated	216,240	0.73	0.44	1.00
Property type				
Apartment	216,240	0.11	0.32	0.00
Attic	216,240	0.04	0.21	0.00
Duplex	216,240	0.02	0.15	0.00
Flat	216,240	0.78	0.41	1.00
Ground	216,240	0.01	0.10	0.00
Loft	216,240	0.01	0.12	0.00
Studio	216,240	0.01	0.11	0.00
Size	216,240	85.82	51.81	75.00
Bedrooms	216,240	2.46	1.10	2.00
Bathrooms	216,240	1.51	0.70	1.00
Lift	216,240	0.69	0.46	1.00
Garage	216,240	0.10	0.30	0.00
Storage	216,240	0.09	0.28	0.00
Terrace	216,240	0.31	0.46	0.00
Air conditioning	216,240	0.60	0.49	1.00
Swimming pool	216,240	0.08	0.28	0.00
Garden	216,240	0.03	0.16	0.00
Sports	216,240	0.01	0.10	0.00
Status	98,908	3.08	0.82	3.00
	Panel B: Sales Ads			
Price	1,102,839	257,206.84	274,353.35	180,000.00
Price per squared meter	1,102,839	2,650.03	1,643.96	2,238.00
Treated	1,102,839	0.69	0.46	1.00
Property type				
Apartment	1,102,839	0.07	0.25	0.00
Attic	1,102,839	0.05	0.21	0.00
Duplex	1,102,839	0.04	0.21	0.00
Flat	1,102,839	0.81	0.39	1.00
Ground	1,102,839	0.03	0.16	0.00
Loft	1,102,839	0.00	0.07	0.00
Studio	1,102,839	0.00	0.06	0.00
Size	1,102,839	112.38	1,874.98	84.00
Bedrooms	1,102,839	2.85	0.97	3.00
Bathrooms	1,102,839	1.52	0.67	1.00
Lift	1,102,839	0.62	0.48	1.00
Garage	1,102,839	0.16	0.36	0.00
Storage	1,102,839	0.17	0.38	0.00
Terrace	1,102,839	0.39	0.49	0.00
Air conditioning	1,102,839	0.41	0.49	0.00
Swimming pool	1,102,839	0.12	0.32	0.00
Garden	1,102,839	0.04	0.19	0.00
Sports	1,102,839	0.02	0.13	0.00
Status	395,065	2.76	0.94	3.00

Notes: This table presents summary statistics for our sample, which includes rental ads (Panel A) and sales ads (Panel B) active on Fotocasa website over the time period spanning from June 2020 to January 2022. The sample is restricted to properties located in always treated and never treated neighboring municipalities.

Table 2: The effect of rent control on average rent

Dep.Var.:	*Log Rent*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.006	0.003	0.006	-0.010	0.001	-0.029	-0.035
	(0.016)	(0.016)	(0.014)	(0.023)	(0.022)	(0.024)	(0.024)
Size		-0.002***			-0.002***	-0.002***	-0.002***
		(0.000)			(0.000)	(0.000)	(0.000)
N. Bedrooms			-0.116***		-0.071***	-0.071***	-0.071***
			(0.003)		(0.005)	(0.005)	(0.005)
N. Bathrooms			0.018***		0.076***	0.076***	0.076***
			(0.007)		(0.011)	(0.011)	(0.011)
Status: Regular				0.206***	0.078**	0.078**	0.078**
				(0.056)	(0.036)	(0.036)	(0.036)
Status: Good				0.214***	0.070**	0.069**	0.069**
				(0.055)	(0.034)	(0.034)	(0.034)
Status: Renovated				0.300***	0.117***	0.116***	0.116***
				(0.054)	(0.033)	(0.033)	(0.033)
Controls							
Property subtype		Yes			Yes	Yes	Yes
Property char.			Yes		Yes	Yes	Yes
Property status				Yes	Yes	Yes	Yes
$MedIncome_j \times Post_t$						Yes	Yes
Linear trends							Yes
Fixed effects							
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip-code	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	2.673	2.673	2.673	2.705	2.705	2.705	2.705
Adj. R-squared	0.41	0.55	0.53	0.45	0.60	0.60	0.60
Observations	216,222	216,222	216,222	98,894	98,894	98,894	98,894
Municipalities	Always treated + Neighboring controls						
Period	Jun20—Jan22						

Notes: The dependent variable is the logged rent per squared meter. Column (1) includes only the treatment indicator. Column (2) adds indicators for the property sub-type (apartment, loft, studio, etc.) and size. Column (3) includes housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area). Column (4) includes an indicator for the property status. Column (5) includes all of the controls for property characteristics. Column (6) adds the zip-level median income recorded before the introduction of rent control, interacted with an indicator for the post-treatment period. Column (7) adds a linear zip-code specific trend fitted to the treatment zip-codes. The sample is restricted to all advertised rents for dwellings posted on Fotocasa website for Catalan municipalities that were always treated from September 2020 and for never treated neighboring municipalities. The period ranges between June 2020 and January 2022. All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the zip-code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: The effect of rent control on average sales prices

Dep.Var.:	*Log Price*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.023***	-0.024***	-0.017***	-0.023***	-0.023***	-0.028**	-0.037***
	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)	(0.012)	(0.013)
Size		-0.000***			-0.000***	-0.000***	-0.000***
		(0.000)			(0.000)	(0.000)	(0.000)
N. Bedrooms			-0.078***		-0.071***	-0.071***	-0.071***
			(0.003)		(0.003)	(0.003)	(0.003)
N. Bathrooms			0.085***		0.073***	0.073***	0.073***
			(0.006)		(0.006)	(0.006)	(0.006)
Status: Regular				0.158***	0.094***	0.094***	0.094***
				(0.010)	(0.009)	(0.009)	(0.009)
Status: Good				0.244***	0.150***	0.150***	0.150***
				(0.012)	(0.010)	(0.010)	(0.010)
Status: Renovated				0.345***	0.201***	0.201***	0.201***
				(0.014)	(0.010)	(0.010)	(0.010)
Controls							
Property subtype		Yes			Yes	Yes	Yes
Property char.			Yes		Yes	Yes	Yes
Property status				Yes	Yes	Yes	Yes
$MedIncome_j \times Post_t$						Yes	Yes
Linear trends							Yes
Fixed effects							
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip-code	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	7.709	7.709	7.709	7.784	7.784	7.784	7.784
Adj. R-squared	0.67	0.69	0.74	0.71	0.78	0.78	0.78
Observations	1,102,819	1,102,819	1,102,819	395,048	395,048	394,974	394,974
Municipalities	Always treated + Neighboring controls						
Period	Jun20—Jan22						

Notes: The dependent variable is the logged price per squared meter. Column (1) includes only the treatment indicator. Column (2) adds indicators for the property type (apartment, loft, studio, etc.) and size. Column (3) includes housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area). Column (4) includes an indicator for the property status. Column (5) includes all of the controls for property characteristics. Column (6) adds the zip-level median income recorded before the introduction of rent control, interacted with an indicator for the post-treatment period. Column (7) adds a linear zip-code specific trend fitted to the treatment zip-codes. The sample is restricted to all advertised sales prices for dwellings posted on Fotocasa website for Catalan municipalities that were always treated from September 2020 and for never treated neighboring municipalities. The period ranges between June 2020 and January 2022. All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the zip-code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: The effect of rent control on rent and sales volumes

Dep.Var.:	*Log Number of Rent Ads*		*Log Number of Sales Ads*	
	(1)	(2)	(3)	(4)
Treatment	-0.324***	-0.300***	0.178***	0.133***
	(0.068)	(0.078)	(0.040)	(0.042)
Fixed effects				
Time	Year-Month	Year-Month	Year-Month	Year-Month
Location	Municipality	Municipality	Municipality	Municipality
Controls	No	Yes	No	Yes
Adj. R-squared	0.92	0.92	0.97	0.98
Observations	2,567	1,995	3,779	3,308
Municipalities	Always treated + Neighboring controls		Always treated + Neighboring controls	
Period	Jun20—Jan22		Jun20—Jan22	

Notes: In Columns (1) and (2), the dependent variable is the logged number of rental ads per municipality and month, while in Columns (3) and (4), the dependent variable is the logged number of sales ads by municipality and month. All columns include year-month and municipality fixed effects. Columns (2) and (4) add control variables for average property characteristics. These include the share of each property sub-type (apartment, loft, studio, etc.), average size, and housing features (e.g., average number of rooms, bathrooms, and the presence of amenities such as a lift, garage, storage area, terrace, air conditioning, swimming pool, garden, and sports area), along with the average property status. In the first (last) two columns the sample is restricted to all advertised rent (sales) ads for dwellings posted on Fotocasa website for Catalan municipalities that were always treated from September 2020 and for never treated neighboring municipalities. The period ranges between June 2020 and January 2022. Standard errors clustered at the municipality level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Impact of rent control on moments of the municipality-level rent distribution

	SD rents	Average rents							
		Median		*Decile*		*Quartiles*			
Dep. var.:		Below	Above	1^{st}	10^{th}	1^{st}	2^{nd}	3^{th}	4^{th}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Panel A. No controls					
Coefficient	0.050*	-0.077***	-0.028	-0.090**	0.004	-0.091***	-0.071***	-0.041	-0.006
S.E.	(0.027)	(0.027)	(0.029)	(0.044)	(0.040)	(0.033)	(0.023)	(0.026)	(0.034)
				Panel B. Controlling for (average) property characteristics					
Coefficient	0.050*	-0.071***	-0.007	-0.078*	0.033	-0.078***	-0.067***	-0.029	0.017
S.E.	(0.027)	(0.023)	(0.032)	(0.039)	(0.048)	(0.028)	(0.021)	(0.027)	(0.040)
				Panel C. Controlling for (average) municipality characteristics					
Coefficient	0.048*	-0.080***	-0.035	-0.093**	-0.002	-0.093***	-0.074***	-0.049*	-0.013
S.E.	(0.026)	(0.026)	(0.028)	(0.043)	(0.040)	(0.033)	(0.022)	(0.025)	(0.034)
				Panel D. Controlling for (average) property and municipality characteristics					
Coefficient	0.050*	-0.072***	-0.011	-0.079**	0.030	-0.079***	-0.068***	-0.032	0.014
S.E.	(0.027)	(0.022)	(0.031)	(0.039)	(0.047)	(0.027)	(0.021)	(0.026)	(0.039)

Notes: This table presents the effects of rent control on the distribution of advertised rents across municipalities, by considering impacts on different municipality-specific moments. The dependent variables considered are the standard deviation of rental prices (Column 1), average rents below and above the median (Columns 2 and 3), rents below the first decile and above the tenth decile (Columns 4 and 5), and rents within each quartile of the distribution (Columns 6 to 9). Please refer to Section 6.1.1 for further details on the construction of these variables. Panel (a) considers the baseline specification including only municipality and year-month fixed effects. Panels (b) and (c) additionally control for the time-varying mean characteristics of the advertised units, and municipal socio-economic indicators, respectively. Panel (d) pulls all the controls together. Standard errors are clustered at the municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Impact of rent control on moments of the municipality-level sales prices distribution

	SD prices	Average prices							
		Median		*Decile*		*Quartiles*			
Dep. var.:		Below	Above	1^{st}	10^{th}	1^{st}	2^{nd}	3^{th}	4^{th}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel A. No controls				
Coefficient	0.022	-0.061***	-0.009	-0.076**	0.030	-0.071***	-0.053***	-0.031*	0.016
S.E.	(0.016)	(0.022)	(0.019)	(0.031)	(0.024)	(0.025)	(0.020)	(0.019)	(0.021)
					Panel B. Controlling for (average) property characteristics				
Coefficient	0.019*	-0.033**	0.022	-0.052**	0.063***	-0.042***	-0.024*	-0.001	0.044***
S.E.	(0.011)	(0.013)	(0.014)	(0.020)	(0.021)	(0.015)	(0.013)	(0.013)	(0.017)
					Panel C. Controlling for (average) municipality characteristics				
Coefficient	0.018	-0.060***	-0.015	-0.074**	0.026	-0.069***	-0.053***	-0.037**	0.010
S.E.	(0.016)	(0.021)	(0.018)	(0.030)	(0.024)	(0.025)	(0.020)	(0.017)	(0.020)
					Panel D. Controlling for (average) property and municipality characteristics				
Coefficient	0.016	-0.033***	0.015	-0.050**	0.057***	-0.042***	-0.026**	-0.008	0.037**
S.E.	(0.011)	(0.013)	(0.013)	(0.020)	(0.021)	(0.015)	(0.013)	(0.012)	(0.016)

Notes: This table presents the effects of rent control on the distribution of advertised sales prices across municipalities, by considering impacts on different municipality-specific moments. The dependent variables considered are the standard deviation of sales prices (Column 1), average prices below and above the median (Columns 2 and 3), prices below the first decile and above the tenth decile (Columns 4 and 5), and prices within each quartile of the distribution (Columns 6 to 9). Please refer to Section 6.1.1 for further details on the construction of these variables. Panel (a) considers the baseline specification including only municipality and year-month fixed effects. Panels (b) and (c) additionally control for the time-varying mean characteristics of the advertised units, and municipal socio-economic indicators, respectively. Panel (d) pulls all the controls together. Standard errors are clustered at the municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: The effect of rent control on rents and sales prices—By air distance from the center

Dep.Var.:	*Log Rent*				*Log Sales Price*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment*Center	0.032	0.030	0.032	0.027	0.027	0.027	0.026	0.027
	(0.020)	(0.019)	(0.019)	(0.028)	(0.028)	(0.027)	(0.021)	(0.025)
Treatment*Suburbs	-0.020	-0.024	-0.019	-0.025	-0.118***	-0.121***	-0.090***	-0.100***
	(0.016)	(0.018)	(0.015)	(0.027)	(0.029)	(0.027)	(0.022)	(0.017)
Size		-0.002***		-0.002***		-0.000***		-0.000***
		(0.000)		(0.000)		(0.000)		(0.000)
N. Bedrooms			-0.126***	-0.085***			-0.086***	-0.084***
			(0.002)	(0.003)			(0.003)	(0.004)
N. Bathrooms			0.045***	0.101***			0.130***	0.132***
			(0.009)	(0.003)			(0.018)	(0.021)
Status: Regular				0.059***				0.060*
				(0.019)				(0.034)
Status: Good				0.052***				0.116***
				(0.018)				(0.044)
Status: Renovated				0.102***				0.172***
				(0.017)				(0.039)
Controls								
Property subtype		Yes		Yes		Yes		Yes
Property char.			Yes	Yes			Yes	Yes
Property status				Yes				Yes
Fixed effects								
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	2.673	2.673	2.673	2.706	7.709	7.709	7.709	7.784
Adj. R-squared	0.35	0.49	0.49	0.56	0.59	0.61	0.69	0.72
Observations	216,222	216,222	216,222	98,896	1,102,674	1,102,674	1,102,674	395,002
Municipalities	Always treated + Neighboring							
Period	Jun20—Jan22							

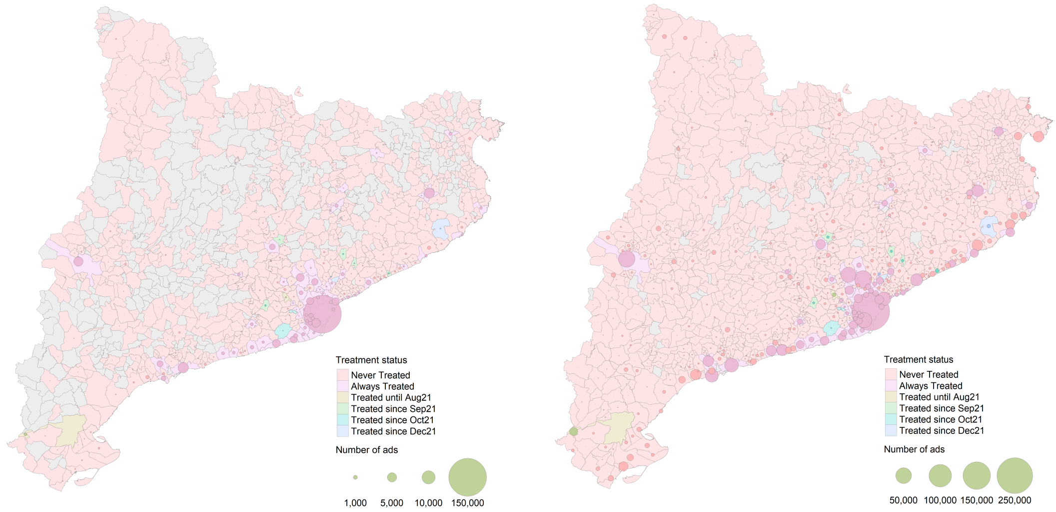
Notes: The dependent variable is the logged rent per squared meter (Columns 1 to 4) and the price per squared meter (Columns 5 to 8). Columns (1) and (5) include only the treatment indicator interacted with dummy variables denoting whether a property is located in the central *vs.* surrounding districts. Columns (2) and (6) add indicators for the property type (apartment, loft, studio, etc.) and size. Columns (3) and (7) include housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area). Columns (4) and (8) include all the controls for property characteristics and an indicator for the property status. Central properties are defined as those located within a 2-kilometer radius of the center, which is identified as the point of highest density in satellite images. The sample includes all advertised rental ads (Columns 1 to 4) and sales ads (Columns 5 to 8) for dwellings posted on the Fotocasa website in the always treated municipalities and never treated neighboring municipalities. The time period spans between June 2020 and January 2022. All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Aggregate gains and losses in rents and sales prices

	Total value (€)	Estimated change (p.p.)	Estimated value change (€)		
			6-month window	1-year projection	10-year projection
Quartile	(1)	(2)	(3)	(4)	(5)
			Panel A: Rents		
First	25,499,581	-0.079***	-2,014,467	-4,028,934	-35,364,814
Second	20,444,808	-0.068***	-1,390,247	-2,780,494	-23,725,494
Third	20,407,945	-0.032	-653,054	-1,306,108	-9,583,984
Fourth	27,426,056	0.014	383,965	767,930	5,620,779
			Panel B: Sales Prices		
First	10,956,995,647	-0.042***	-460,193,817	–	–
Second	14,809,885,706	-0.026**	-385,057,028	–	–
Third	18,445,744,059	-0.008	-147,565,952	–	–
Fourth	29,425,325,113	0.037**	1,088,737,029	–	–

Notes: This table presents the aggregate gains and losses in rents (Panel A) and sales prices (Panel B) resulting from the implementation of rent control in treated municipalities, categorized by quartiles of the respective distributions. Quartiles are defined based on the price per square meter. Column 1 depicts the total euro value of rentals/sales in each quartile, aggregated over the three-month pre-policy period (June-August 2020). Column 2 corresponds to the estimated percentage change in rents/sales prices, based on the preferred specification from Panel D in Tables 5 and 6, respectively, where all property- and municipal-level controls are included. Column 3 reports the corresponding estimated change in euro values. Finally, Columns 4 and 5 project the change in rental values from Column 3 over longer time frames (one and ten years, respectively). For the ten-year projection, we use the European Central Bank deposit facility interest rate as of June 2023, set at 3.50%.

Figure 1: Municipalities subject to rent control and sample coverage

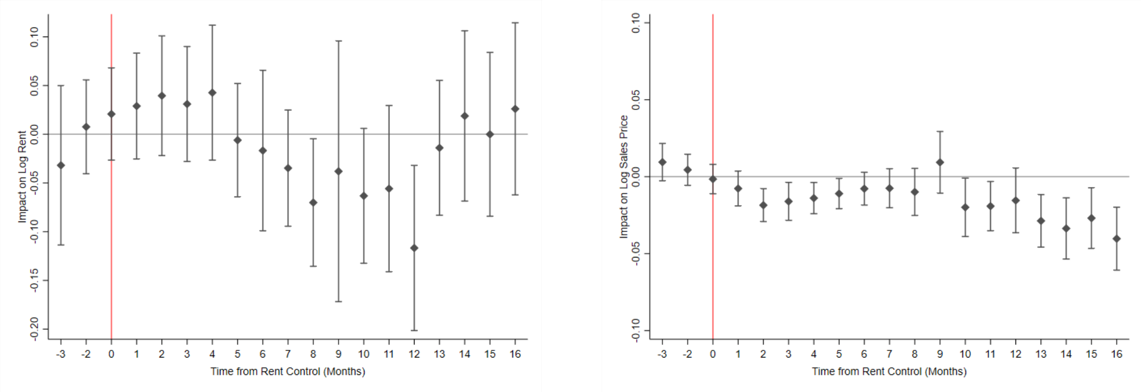


(a) Rental ads

(b) Sales ads

Notes: The figures represent the map of Catalonia, in which municipalities are colored according to their treatment status, as based on their adoption of the rent control law. The size of each circle represents the number of rent ads (Panel a) and sales ads (Panel b) that were posted in the corresponding municipality during our sample period. Fifty-nine municipalities (colored in violet) belong to the group of always treated municipalities in that they adopted the rent control law when it was first introduced as of September 2020 and remained subject to the regulation since then. Two municipalities (in brown) were treated only in the first year after the passage of the rent control law, namely from September 2020 until August 2021, when they stopped being considered as tight housing markets. Nine municipalities adopted the regulation only towards the end of 2021. Of these, five municipalities (in green) introduced rent control only as of September 2021, two of them (in light blue) introduced rent control in October 2021, and another two (in violet) introduced rent control in December 2021. The remaining municipalities (in pink) belong to the control group, in that they were never subject to the rent control ordinance. No ad was posted in the (unregulated) municipalities colored in light gray.

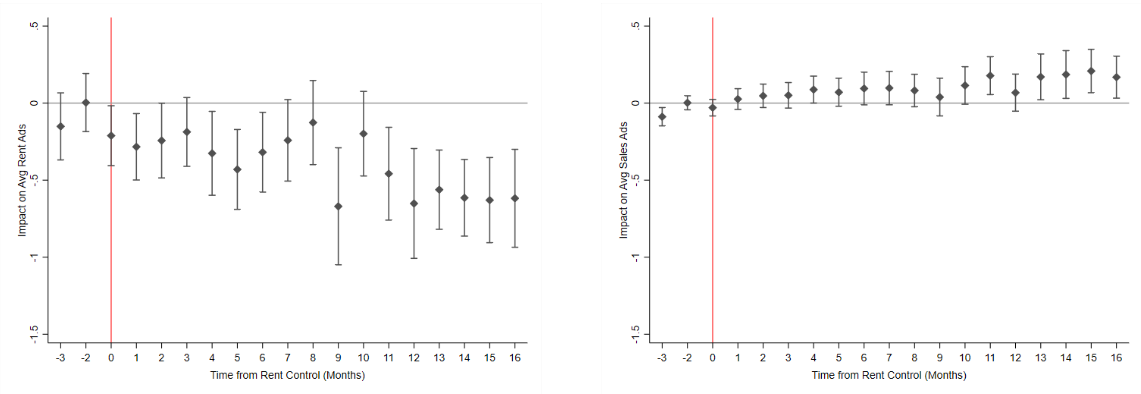
Figure 2: Dynamic effects of the rent control law



(a) Rent (b) Sales price

Notes: The figure shows the estimated effects of the introduction of the rent control law on the dynamics of the logged rent per squared meter (Panel a) and the logged house price per squared meter (Panel b). Specifically, we report the estimates of β_τ from equation 8, which correspond to the percentage change in log price per square meter at event time τ for properties that are located in municipalities subject to the rent control law during the whole period of study ('always treated') relative to never treated neighboring ones. The red vertical lines at event time 0 depict the month in which rent control was introduced (September 2020). The bounds correspond to 95% confidence intervals.

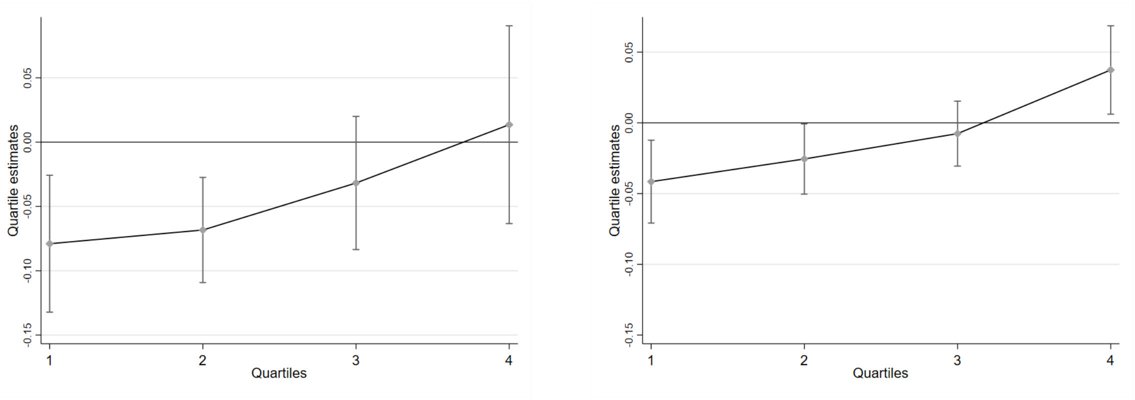
Figure 3: Dynamic effects of the rent control law on volumes



(a) Rent volumes (b) Sales volumes

Notes: The figure shows the estimated effects of the introduction of the rent control law on the dynamics of the supply of rental ads (Panel a) and sales ads (Panel b). Specifically, we report the estimates of β_τ from equation 8, which correspond to the percentage change in the log number of ads at event time τ for properties that are located in municipalities subject to the rent control law during the whole period of study ('always treated') relative to never treated neighboring ones. The red vertical lines at event time 0 depict the month in which rent control was introduced (September 2020). The bounds correspond to 95% confidence intervals.

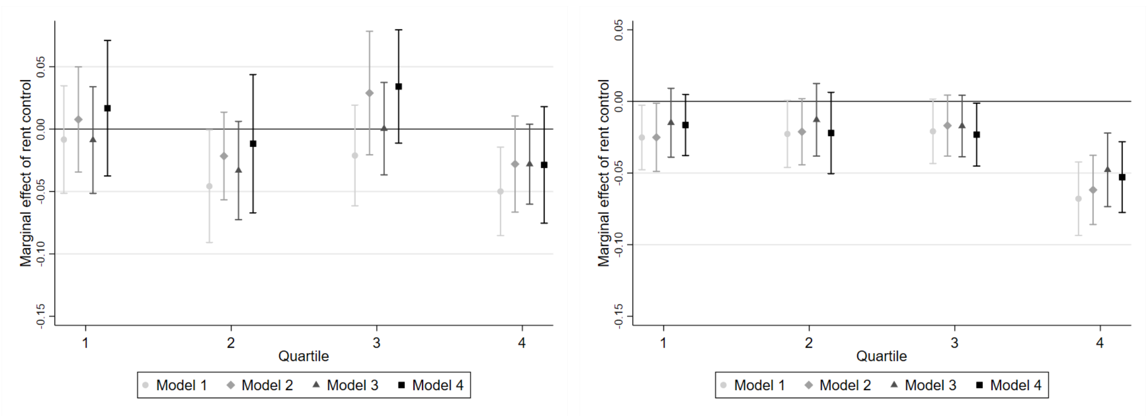
Figure 4: The impact of rent control on average rental and sales prices by quartile



(a) Rental prices (b) Sales prices

Notes: The figure shows the estimated effects of the introduction of the rent control law on the four quartiles of the municipality-level distribution of rents (Panel a) and sales prices (Panel b). The bounds correspond to 95% confidence intervals.

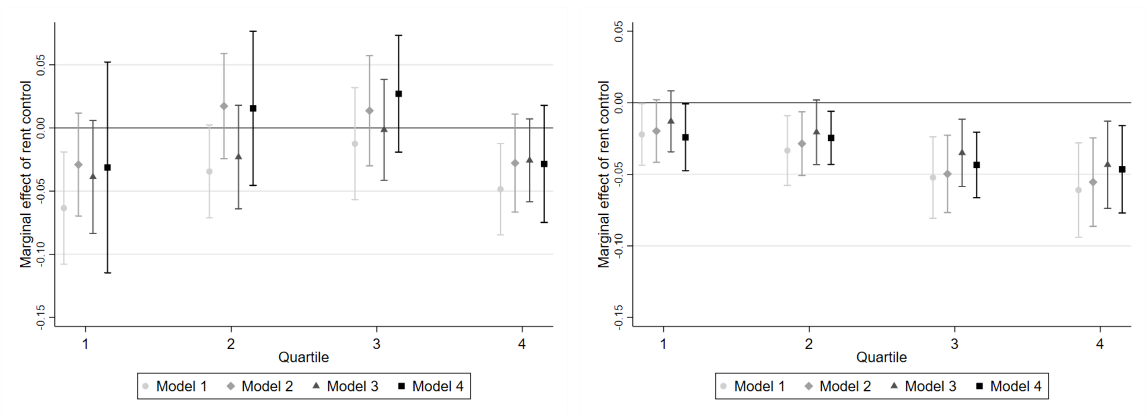
Figure 5: Rent control and the municipal population density



(a) Rent (b) Sales prices

Notes: The figure shows the estimated effects of the introduction of the rent control law on the four quartiles of the municipality-level distribution of rents (Panel a) and sales prices (Panel b). The bounds correspond to 95% confidence intervals.

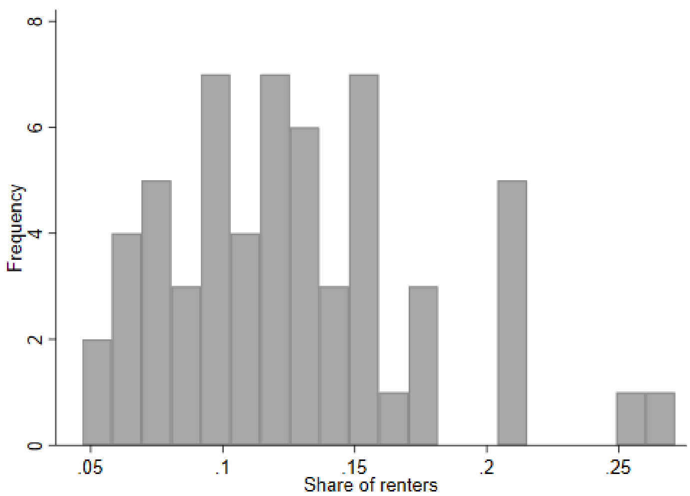
Figure 6: Rent control and the municipal share of renters



(a) Rent (b) Sales prices

Notes: The figure shows the estimated effects of the introduction of the rent control law on the four quartiles of the municipality-level distribution of rents (Panel a) and sales prices (Panel b). The bounds correspond to 95% confidence intervals.

Figure 7: Share of renters in always treated municipalities



Notes: The figure shows the histogram illustrating the distribution of the share of renters in always treated municipalities, which refer to the subset of 59 municipalities that adopted rent control and maintained it throughout the entire sample period. The x-axis represents the share of renters in these municipalities, while the y-axis represents the count of such municipalities within each share range.

A Appendix – Empirics

Table A1: Municipalities subject to the law on rent control

	Municipality		Municipality
1.	Badalona	32.	Prat de Llobregat, el
2.	Barberà del Vallès	33.	Premià de Mar
3.	Barcelona	34.	Reus
4.	Blanes	35.	Ripollet
5.	Calafell	36.	Rubí
6.	Castellar del Vallès	37.	Sabadell
7.	Castelldefels	38.	Salou
8.	Cerdanyola del Vallès	39.	Salt
9.	Cornellà de Llobregat	40.	Sant Adrià de Besos
10.	Esplugues de Llobregat	41.	Sant Andreu de la Barca
11.	Figueres	42.	Sant Boi de Llobregat
12.	Gavà	43.	Sant Cugat del Vallès
13.	Girona	44.	Sant Feliu de Guíxols
14.	Granollers	45.	Sant Feliu de Llobregat
15.	Hospitalet de Llobregat, l’	46.	Sant Joan Despí
16.	Igualada	47.	Sant Just Desvern
17.	Lleida	48.	Sant Pere de Ribes
18.	Manlleu	49.	Sant Vicenç dels Horts
19.	Manresa	50.	Santa Coloma de Gramenet
20.	Martorell	51.	Santa Perpètua de la Mogoda
21.	Masnou, el	52.	Sitges
22.	Mataró	53.	Tarragona
23.	Molins de Rei	54.	Terrassa
24.	Mollet del Vallès	55.	Tortosa
25.	Montcada i Reixac	56.	Vendrell, el
26.	Montgat	57.	Vic
27.	Olesa de Montserrat	58.	Viladecans
28.	Olot	59.	Vilafranca del Penedès
29.	Palafrugell	60.	Vilanova i la Geltrú
30.	Pallejà	61.	Vilassar de Mar
31.	Pineda		

Notes: This table lists the municipalities included in the Annex of the Law 11/2020 by the Catalan Parliament declared as ‘tense housing markets’.

Table A2: Summary statistics for rent and sales ads by treatment status pre-rent control

	Always treated			Neighboring never treated			Difference
	N	Mean	S.D.	N	Mean	S.D.	p-value
	Panel A: Rent Ads						
Price	66,176	1,416.90	1,051.30	2,151	1,025.80	1,028.70	0.020
Price per squared meter	66,176	16.12	6.80	2,151	10.31	7.59	0.000
Property type							
Apartment	66,176	0.230	0.420	2,151	0.350	0.480	0.000
Attic	66,176	0.040	0.210	2,151	0.020	0.150	0.000
Duplex	66,176	0.030	0.160	2,151	0.040	0.190	0.210
Flat	66,176	0.670	0.470	2,151	0.530	0.500	0.000
Ground	66,176	0.010	0.080	2,151	0.040	0.190	0.000
Loft	66,176	0.010	0.110	2,151	0.010	0.100	0.550
Studio	66,176	0.010	0.090	2,151	0.010	0.100	0.670
Size	66,176	91.9	56.0	2,151	110.6	105.6	0.010
Bedrooms	66,176	2.540	1.100	2,151	2.730	1.150	0.090
Bathrooms	66,176	1.560	0.730	2,151	1.640	0.850	0.210
Status	38,248	3.100	0.820	1,098	2.900	0.830	0.000
	Panel B: Sales Ads						
Price	123,801	276,630.90	291,821.30	23,986	175,405.40	125,784.00	0.110
Price per squared meter	123,801	2,770.70	1719.50	23,986	1968.90	995.40	0.130
Property type							
Apartment	123,801	0.040	0.210	23,986	0.160	0.360	0.010
Attic	123,801	0.050	0.220	23,986	0.040	0.190	0.020
Duplex	123,801	0.050	0.210	23,986	0.070	0.250	0.070
Flat	123,801	0.830	0.380	23,986	0.690	0.460	0.000
Ground	123,801	0.020	0.150	23,986	0.030	0.180	0.130
Loft	123,801	0.000	0.070	23,986	0.000	0.040	0.080
Studio	123,801	0.000	0.050	23,986	0.010	0.090	0.080
Size	123,801	120.850	2701.280	23,986	93.830	81.380	0.010
Bedrooms	123,801	2.930	0.950	23,986	2.670	0.930	0.000
Bathrooms	123,801	1.560	0.680	23,986	1.540	0.640	0.780
Status	52,651	2.760	0.940	9,418	2.810	0.890	0.270

Notes: This table presents summary statistics by treatment status for rental ads (Panel A) and sales ads (Panel B) active on Fotocasa website over the pre-treatment period (from June 2020 to August 2020). The sample is restricted to ads concerning properties located in always treated and never treated neighboring municipalities. Standard errors are clustered at the municipality level.

Table A3: Placebo test on average rent and sales prices using pre-treatment periods

Dep.Var.:	*Log Rent*				*Log Sales Price*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Placebo Treatment	-0.014	0.040	-0.011	0.015	-0.002	-0.005	-0.004	-0.005
	(0.016)	(0.035)	(0.013)	(0.024)	(0.004)	(0.004)	(0.004)	(0.005)
Controls								
Property subtype	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property status		Yes		Yes		Yes		Yes
Fixed effects								
Month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip-code	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.666	0.657	0.666	0.657	0.762	0.788	0.762	0.788
Observations	39,279	19,802	39,279	19,802	147,772	62,051	147,772	62,051
Placebo Treatment Date	Jul20	Jul20	Aug20	Aug20	Jul20	Jul20	Aug20	Aug20
Municipalities	Always treated + Neighboring							
Period	Jun20-Aug20							

Notes: The dependent variable is the logged rent per squared meter (Columns 1 to 3) and the price per squared meter (Columns 4 to 6). The sample is restricted to all advertised rents for dwellings posted in the pre-treatment period (between June 2020 and August 2020) on Fotocasa website for Catalan municipalities that were always treated from September 2020 and for neighboring municipalities. The regressor of interest is a placebo treatment variable equal to 1 for ever treated observations after July 2020 (Columns 1 and 3) and August 2020 (Columns 2 and 4), and 0 otherwise. All specifications include all control variables, that are indicators for the property type (apartment, loft, studio, etc.), housing characteristics (size, number of bedrooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area), and indicators for the property status. All specifications include month and zip-code fixed effects. Standard errors are clustered at the zip-code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Robustness of price effects—Outliers

Dep.Var.:	*Log Rent*			*Log Sales Price*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.010	-0.022	-0.051**	-0.027***	-0.025***	-0.033**
	(0.013)	(0.017)	(0.020)	(0.007)	(0.006)	(0.013)
Size		-0.003***	-0.003***		-0.002***	-0.002***
		(0.000)	(0.000)		(0.000)	(0.000)
N. Bedrooms		-0.060***	-0.060***		-0.034***	-0.034***
		(0.006)	(0.006)		(0.005)	(0.005)
N. Bathrooms		0.089***	0.089***		0.111***	0.111***
		(0.006)	(0.006)		(0.008)	(0.008)
Status: Regular		0.069*	0.069*		0.078***	0.078***
		(0.037)	(0.037)		(0.007)	(0.007)
Status: Good		0.059*	0.058		0.134***	0.134***
		(0.036)	(0.036)		(0.008)	(0.008)
Status: Renovated		0.106***	0.105***		0.184***	0.184***
		(0.035)	(0.035)		(0.009)	(0.009)
Controls						
Property subtype		Yes	Yes		Yes	Yes
Property char.		Yes	Yes		Yes	Yes
Property status		Yes	Yes		Yes	Yes
$MedIncome_j \times Post_t$			Yes			Yes
Linear trends			Yes			Yes
Fixed effects						
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes
Zip-code	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	2.677	2.706	2.706 7.712	7.774	7.774	
Adj. R-squared	0.41	0.60	0.60	0.68	0.79	0.79
Observations	210,696	97,235	97,235	1,080,755	388,564	388,490
Municipalities	Always treated + Neighboring					
Period	Jun20—Jan22					

Notes: The dependent variable is the logged rent per squared meter (Columns 1 to 3) and the price per squared meter (Columns 4 to 6). Columns (1) and (4) include only the treatment indicator. Columns (2) and (5) add indicators for the property type (apartment, loft, studio, etc.), the size, housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area), as well as an indicator for the property status and for whether it is a new construction. Columns (3) and (6) add the zip-level median income recorded before the introduction of rent control, interacted with an indicator for the post-treatment period, as well as a linear zip-code specific trend fitted to the treatment zip-codes. The sample includes all rental and sales ads for dwellings posted on the Fotocasa website in always treated and never treated neighboring municipalities. The time period spans between June 2020 and January 2022. We further trim the top and bottom 1 percent of the distribution of rent prices (Columns 1 to 3) and sales prices (Columns 4 to 6) by month. All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the zip-code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Robustness of price effects—Measurement error

Dep.Var.:	*Log Rent*			*Log Sales Price*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.021	0.028	-0.008	-0.020***	-0.024***	-0.038***
	(0.018)	(0.033)	(0.035)	(0.006)	(0.006)	(0.013)
Size		-0.002***	-0.002***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
N. Bedrooms		-0.071***	-0.071***		-0.071***	-0.071***
		(0.005)	(0.005)		(0.003)	(0.003)
N. Bathrooms		0.076***	0.076***		0.073***	0.073***
		(0.011)	(0.011)		(0.006)	(0.006)
Status: Regular		0.083**	0.083**		0.094***	0.094***
		(0.036)	(0.036)		(0.009)	(0.009)
Status: Good		0.074**	0.073**		0.151***	0.151***
		(0.034)	(0.035)		(0.010)	(0.010)
Status: Renovated		0.121***	0.121***		0.202***	0.202***
		(0.033)	(0.034)		(0.010)	(0.010)
Controls						
Property subtype		Yes	Yes		Yes	Yes
Property char.		Yes	Yes		Yes	Yes
Property status		Yes	Yes		Yes	Yes
$MedIncome_j \times Post_t$			Yes			Yes
Linear trends			Yes			Yes
Fixed effects						
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes
Zip-code	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	2.682	2.712	2.712	7.710	7.785	7.785
Adj. R-squared	0.40	0.59	0.59	0.67	0.78	0.78
Observations	204,455	94,625	94,625	1,073,157	387,051	386,987
Municipalities	Always treated + Neighboring based on supply					
Period	Jun20—Jan22					

Notes: The dependent variable is the logged rent per squared meter (Columns 1 to 3) and the price per squared meter (Columns 4 to 6). Columns (1) and (4) include only the treatment indicator. Columns (2) and (5) add indicators for the property type (apartment, loft, studio, etc.), the size, housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area), as well as an indicator for the property status and for whether it is a new construction. Columns (3) and (6) add the zip-level median income recorded before the introduction of rent control, interacted with an indicator for the post-treatment period, as well as a linear zip-code specific trend fitted to the treatment zip-codes. The sample includes all rental and sales ads for dwellings posted on the Fotocasa website in always treated and never treated neighboring municipalities. The time period spans between June 2020 and January 2022. We further exclude municipalities with relatively low or unstable supply of rental ads across months (see Section 5.4 for further details on sample selection). All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the zip-code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Robustness of price effects—Clustering at the municipality level

Dep.Var.:	*Log Rent*			*Log Sales Price*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.005	-0.002	-0.133***	-0.025***	-0.021***	-0.369***
	(0.016)	(0.027)	(0.029)	(0.009)	(0.006)	(0.036)
Size		-0.002***	-0.002***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
N. Bedrooms		-0.086***	-0.086***		-0.086***	-0.088***
		(0.004)	(0.004)		(0.005)	(0.006)
N. Bathrooms		0.103***	0.097***		0.140***	0.120***
		(0.004)	(0.004)		(0.025)	(0.020)
Status: Regular		0.059***	0.064***		0.054	0.069**
		(0.019)	(0.018)		(0.037)	(0.033)
Status: Good		0.052***	0.057***		0.109**	0.126***
		(0.018)	(0.018)		(0.048)	(0.043)
Status: Renovated		0.102***	0.108***		0.167***	0.187***
		(0.017)	(0.017)		(0.042)	(0.036)
Controls						
Property subtype		Yes	Yes		Yes	Yes
Property char.		Yes	Yes		Yes	Yes
Property status		Yes	Yes		Yes	Yes
$MedIncome_j \times Post_t$			Yes			Yes
Linear trends			Yes			Yes
Fixed effects						
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes
Zip-code	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	2.673	2.705	2.705	7.709	7.784	7.784
Adj. R-squared	0.35	0.55	0.56	0.58	0.72	0.73
Observations	216,232	98,902	98,900	1,102,839	395,063	394,988
Municipalities	Always treated + Neighboring					
Period	Jun20—Jan22					

Notes: The dependent variable is the logged rent per squared meter (Columns 1 to 3) and the price per squared meter (Columns 4 to 6). Columns (1) and (4) include only the treatment indicator. Columns (2) and (5) add indicators for the property type (apartment, loft, studio, etc.), the size, housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area), as well as an indicator for the property status and for whether it is a new construction. Columns (3) and (6) add the zip-level median income recorded before the introduction of rent control, interacted with an indicator for the post-treatment period, as well as a linear zip-code specific trend fitted to the treatment zip-codes. The sample includes all rental and sales ads for dwellings posted on the Fotocasa website in always treated and never treated neighboring municipalities. The time period spans between June 2020 and January 2022. All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Robustness of price effects—Alternative control with never treated municipalities in Catalonia

Dep.Var.:	*Log Rent*			*Log Sales Price*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.012	-0.031**	-0.067***	-0.014***	-0.024***	-0.035***
	(0.012)	(0.013)	(0.017)	(0.005)	(0.004)	(0.012)
Size		-0.002***	-0.002***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
N. Bedrooms		-0.071***	-0.071***		-0.073***	-0.073***
		(0.006)	(0.006)		(0.003)	(0.003)
N. Bathrooms		0.077***	0.077***		0.071***	0.071***
		(0.011)	(0.011)		(0.005)	(0.005)
Status: Regular		0.089**	0.088**		0.108***	0.108***
		(0.035)	(0.035)		(0.009)	(0.009)
Status: Good		0.082**	0.081**		0.167***	0.168***
		(0.034)	(0.034)		(0.010)	(0.010)
Status: Renovated		0.128***	0.127***		0.217***	0.217***
		(0.033)	(0.033)		(0.010)	(0.010)
Controls						
Property subtype		Yes	Yes		Yes	Yes
Property char.		Yes	Yes		Yes	Yes
Property status		Yes	Yes		Yes	Yes
$MedIncome_j \times Post_t$			Yes			Yes
Linear trends			Yes			Yes
Fixed effects						
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes
Zip-code	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	2.649	2.685	2.685	7.583	7.695	7.696
Adj. R-squared	0.46	0.63	0.64	0.70	0.79	0.79
Observations	225,548	102,684	102,678	1,379,356	474,454	474,304
Municipalities	Always treated + Never treated across Catalonia					
Period	Jun20—Jan22					

Notes: The dependent variable is the logged rent per squared meter (Columns 1 to 3) and the price per squared meter (Columns 4 to 6). Columns (1) and (4) include only the treatment indicator. Columns (2) and (5) add indicators for the property type (apartment, loft, studio, etc.), the size, housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area), as well as an indicator for the property status and for whether it is a new construction. Columns (3) and (6) add the zip-level median income recorded before the introduction of rent control, interacted with an indicator for the post-treatment period, as well as a linear zip-code specific trend fitted to the treatment zip-codes. The sample includes all rental and sales ads for dwellings posted on the Fotocasa website in always treated and never treated municipalities in the rest of Catalonia. The time period spans between June 2020 and January 2022. All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the zip-code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Robustness of volume effects—Alternative control with never treated municipalities in Catalonia

Dep.Var.:	*Log Number of Rent Ads*		*Log Number of Sales Ads*	
	(1)	(2)	(3)	(4)
Treatment	-0.361***	-0.320***	0.195***	0.187***
	(0.053)	(0.055)	(0.028)	(0.028)
Fixed effects				
Time	Year-Month	Year-Month	Year-Month	Year-Month
Location	Municipality	Municipality	Municipality	Municipality
Controls	No	Yes	No	Yes
Adj. R-squared	0.89	0.91	0.96	0.97
Observations	5,600	3,761	12,750	9,272
Municipalities	Always treated + Never treated across Catalonia			
Period	Jun20—Jan22			

Notes: In Columns (1) and (2), the dependent variable is the logged number of rental ads per municipality and month, while in Columns (3) and (4), the dependent variable is the logged number of sales ads by municipality and month. All columns include year-month and municipality fixed effects. Columns (2) and (4) add control variables for average property characteristics. These include the share of each property sub-type (apartment, loft, studio, etc.), average size, and housing features (e.g., average number of rooms, bathrooms, and the presence of amenities such as a lift, garage, storage area, terrace, air conditioning, swimming pool, garden, and sports area), along with the average property status. In the first (last) two columns the sample includes all advertised rent (sales) ads for dwellings posted on Fotocasa website for Catalan municipalities between June 2020 and January 2022. Standard errors clustered at the municipality level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Robustness of price effects—Alternative control with never treated municipalities in Spain

Dep.Var.:	*Log Rent*			*Log Sales Price*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.008	-0.032***	-0.062***	-0.014***	-0.024***	-0.035***
	(0.005)	(0.005)	(0.011)	(0.005)	(0.004)	(0.012)
Size		-0.002***	-0.002***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
N. Bedrooms		-0.059***	-0.059***		-0.073***	-0.073***
		(0.004)	(0.004)		(0.003)	(0.003)
N. Bathrooms		0.070***	0.070***		0.071***	0.071***
		(0.007)	(0.007)		(0.005)	(0.005)
Status: Regular		0.029	0.030		0.108***	0.108***
		(0.030)	(0.030)		(0.009)	(0.009)
Status: Good		0.046	0.047		0.167***	0.168***
		(0.030)	(0.030)		(0.010)	(0.010)
Status: Renovated		0.095***	0.096***		0.217***	0.217***
		(0.030)	(0.030)		(0.010)	(0.010)
Controls						
Property subtype		Yes	Yes		Yes	Yes
Property char.		Yes	Yes		Yes	Yes
Property status		Yes	Yes		Yes	Yes
$MedIncome_j \times Post_t$			Yes			Yes
Linear trends			Yes			Yes
Fixed effects						
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes
Zip-code	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	2.311	2.354	2.354	7.583	7.695	7.696
Adj. R-squared	0.59	0.72	0.72	0.70	0.79	0.79
Observations	1,305,115	600,495	598,071	1,379,356	474,454	474,304
Municipalities	Always treated + Never treated across Spain					
Period	Jun20—Jan22					

Notes: The dependent variable is the logged rent per squared meter (Columns 1 to 3) and the price per squared meter (Columns 4 to 6). Columns (1) and (4) include only the treatment indicator. Columns (2) and (5) add indicators for the property type (apartment, loft, studio, etc.), the size, housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area), as well as an indicator for the property status and for whether it is a new construction. Columns (3) and (6) add the zip-level median income recorded before the introduction of rent control, interacted with an indicator for the post-treatment period, as well as a linear zip-code specific trend fitted to the treatment zip-codes. The sample includes all rental and sales ads for dwellings posted on the Fotocasa website in always treated and never treated municipalities in the rest of Spain. The time period spans between June 2020 and January 2022. All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the zip-code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: Robustness of volume effects—Alternative control with never treated municipalities in Spain

Dep.Var.:	*Log Number of Rent Ads*		*Log Number of Sales Ads*	
	(1)	(2)	(3)	(4)
Treatment	-0.349***	-0.307***	0.155***	0.151***
	(0.047)	(0.045)	(0.023)	(0.023)
Fixed effects				
Time	Year-Month	Year-Month	Year-Month	Year-Month
Location	Municipality	Municipality	Municipality	Municipality
Controls	No	Yes	No	Yes
Adj. R-squared	0.92	0.93	0.97	0.98
Observations	33,566	23,170	86,206	61,723
Municipalities	Always treated + Never treated across Spain		Always treated + Never treated across Spain	
Period	Jun20—Jan22		Jun20—Jan22	

Notes: In Columns (1) and (2), the dependent variable is the logged number of rental ads per municipality and month, while in Columns (3) and (4), the dependent variable is the logged number of sales ads by municipality and month. All columns include year-month and municipality fixed effects. Columns (2) and (4) add control variables for average property characteristics. These include the share of each property sub-type (apartment, loft, studio, etc.), average size, and housing features (e.g., average number of rooms, bathrooms, and the presence of amenities such as a lift, garage, storage area, terrace, air conditioning, swimming pool, garden, and sports area), along with the average property status. In the first (last) two columns the sample includes all advertised rent (sales) ads for dwellings posted on Fotocasa website for Catalan municipalities that were always treated from September 2020 and never treated municipalities across Spain. The period ranges between June 2020 and January 2022. Standard errors clustered at the municipality level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Robustness of price effects—Excluding Barcelona

Dep.Var.:	*Log Rent*			*Log Sales Price*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.005	0.012	-0.050	-0.018**	-0.019***	-0.069***
	(0.015)	(0.023)	(0.034)	(0.008)	(0.006)	(0.018)
Size		-0.003***	-0.003***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
N. Bedrooms		-0.058***	-0.058***		-0.075***	-0.075***
		(0.008)	(0.008)		(0.003)	(0.003)
N. Bathrooms		0.038***	0.038***		0.073***	0.073***
		(0.014)	(0.014)		(0.007)	(0.007)
Status: Regular		0.048	0.050		0.133***	0.133***
		(0.047)	(0.046)		(0.011)	(0.011)
Status: Good		0.059	0.061		0.201***	0.201***
		(0.047)	(0.047)		(0.011)	(0.011)
Status: Renovated		0.112**	0.113**		0.248***	0.248***
		(0.048)	(0.047)		(0.013)	(0.013)
Controls						
Property subtype		Yes	Yes		Yes	Yes
Property char.		Yes	Yes		Yes	Yes
Property status		Yes	Yes		Yes	Yes
$MedIncome_j \times Post_t$			Yes			Yes
Linear trends			Yes			Yes
Fixed effects						
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes
Zip-code	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	2.323	2.338	2.338	7.517	7.563	7.563
Adj. R-squared	0.41	0.66	0.66	0.53	0.67	0.66
Observations	61,653	25,132	25,132	839,444	282,380	282,306
Municipalities	Always treated (excluding Barcelona) + Neighboring					
Period	Jun20—Jan22					

Notes: The dependent variable is the logged rent per squared meter (Columns 1 to 3) and the price per squared meter (Columns 4 to 6). Columns (1) and (4) include only the treatment indicator. Columns (2) and (5) add indicators for the property type (apartment, loft, studio, etc.), the size, housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area), as well as an indicator for the property status and for whether it is a new construction. Columns (3) and (6) add the zip-level median income recorded before the introduction of rent control, interacted with an indicator for the post-treatment period, as well as a linear zip-code specific trend fitted to the treatment zip-codes. The sample includes all advertised rental and sales ads for dwellings posted on Fotocasa website for always treated municipalities (with the exception of Barcelona) and never treated neighboring municipalities. The time period spans between June 2020 and January 2022. All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the zip-code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: Robustness of price effects—With controls for Covid cases

Dep.Var.:	*Log Rent*			*Log Sales Price*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.015	0.005	-0.031	-0.022***	-0.022***	-0.037***
	(0.017)	(0.024)	(0.026)	(0.007)	(0.006)	(0.013)
Size		-0.002***	-0.002***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
N. Bedrooms		-0.071***	-0.071***		-0.071***	-0.071***
		(0.005)	(0.005)		(0.003)	(0.003)
N. Bathrooms		0.077***	0.077***		0.073***	0.073***
		(0.011)	(0.011)		(0.006)	(0.006)
Status: Regular		0.078**	0.078**		0.093***	0.093***
		(0.036)	(0.036)		(0.009)	(0.009)
Status: Good		0.069**	0.069**		0.150***	0.150***
		(0.034)	(0.034)		(0.010)	(0.010)
Status: Renovated		0.117***	0.116***		0.201***	0.201***
		(0.033)	(0.033)		(0.010)	(0.010)
Covid cases	-1.892***	-0.671*	-0.637*	0.104	0.251*	0.313**
	(0.485)	(0.358)	(0.368)	(0.140)	(0.137)	(0.138)
Controls						
Property subtype		Yes	Yes		Yes	Yes
Property char.		Yes	Yes		Yes	Yes
Property status		Yes	Yes		Yes	Yes
$MedIncome_j \times Post_t$			Yes			Yes
Linear trends			Yes			Yes
Fixed effects						
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes
Zip-code	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	2.675	2.707	2.707	7.717	7.792	7.792
Adj. R-squared	0.41	0.60	0.60	0.67	0.78	0.78
Observations	215,919	98,778	98,778	1,094,281	392,178	392,118
Municipalities	Always treated + Neighboring					
Period	Jun20—Jan22					

Notes: The dependent variable is the logged rent per squared meter (Columns 1 to 3) and the price per squared meter (Columns 4 to 6). All specifications include an additional control variable for the number of reported Covid cases per capita, measured for each month and municipality. The variable captures any changes in the rental and housing markets that may be directly related to the Covid-19 pandemic, and is adjusted for the size of the municipal population. Additionally, Columns (1) and (4) include only the treatment indicator. Columns (2) and (5) add indicators for the property type (apartment, loft, studio, etc.), the size, housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area), as well as an indicator for the property status and for whether it is a new construction. Columns (3) and (6) add the zip-level median income recorded before the introduction of rent control, interacted with an indicator for the post-treatment period, as well as a linear zip-code specific trend fitted to the treatment zip-codes. The sample includes all advertised rental and sales ads for dwellings posted on Fotocasa website for always treated municipalities and never treated neighboring municipalities. The time period spans between June 2020 and January 2022. All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the zip-code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A13: The effect of rent control on rents and sales prices—Interaction with population size

Dep.Var.:	*Log Rent*			*Log Sales Price*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.005	-0.001	-0.036	-0.024***	-0.023***	-0.038***
	(0.016)	(0.022)	(0.024)	(0.007)	(0.006)	(0.013)
Size		-0.002***	-0.002***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
N. Bedrooms		-0.071***	-0.071***		-0.071***	-0.071***
		(0.005)	(0.005)		(0.003)	(0.003)
N. Bathrooms		0.076***	0.076***		0.073***	0.073***
		(0.011)	(0.011)		(0.006)	(0.006)
Status: Regular		0.078**	0.078**		0.094***	0.094***
		(0.036)	(0.036)		(0.009)	(0.009)
Status: Good		0.070**	0.069**		0.150***	0.150***
		(0.034)	(0.034)		(0.010)	(0.010)
Status: Renovated		0.117***	0.116***		0.201***	0.201***
		(0.033)	(0.033)		(0.010)	(0.010)
Treated $\times$ Pop.	0.000**	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Controls						
Property subtype		Yes	Yes		Yes	Yes
Property char.		Yes	Yes		Yes	Yes
Property status		Yes	Yes		Yes	Yes
$MedIncome_j \times Post_t$			Yes			Yes
Linear trends			Yes			Yes
Fixed effects						
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes
Zip-code	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	2.673	2.705	2.705	7.709	7.784	7.784
Adj. R-squared	0.41	0.60	0.60	0.67	0.78	0.78
Observations	216,222	98,894	98,894	1,102,819	395,048	394,974
Municipalities	Always treated + Neighboring					
Period	Jun20—Jan22					

Notes: The dependent variable is the logged rent per squared meter (Columns 1 to 3) and the price per squared meter (Columns 4 to 6). All specifications include an interaction term between the treatment indicator and population size. Additionally, Columns (1) and (4) include only the treatment indicator. Columns (2) and (5) add indicators for the property type (apartment, loft, studio, etc.), the size, housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area), as well as an indicator for the property status and for whether it is a new construction. Columns (3) and (6) add the zip-level median income recorded before the introduction of rent control, interacted with an indicator for the post-treatment period, as well as a linear zip-code specific trend fitted to the treatment zip-codes. The sample includes all advertised rental and sales ads for dwellings posted on Fotocasa website for always treated municipalities and never treated neighboring municipalities. The time period spans between June 2020 and January 2022. All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the zip-code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A14: The effect of rent control on rents and sales prices—Interaction with touristic activity

Dep.Var.:	*Log Rent*			*Log Sales Price*		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.006	0.003	-0.033	-0.024***	-0.023***	-0.038***
	(0.016)	(0.022)	(0.024)	(0.007)	(0.006)	(0.013)
Size		-0.002***	-0.002***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
N. Bedrooms		-0.071***	-0.071***		-0.071***	-0.071***
		(0.005)	(0.005)		(0.003)	(0.003)
N. Bathrooms		0.076***	0.077***		0.073***	0.073***
		(0.011)	(0.011)		(0.006)	(0.006)
Status: Regular		0.074**	0.073**		0.094***	0.094***
		(0.036)	(0.036)		(0.009)	(0.009)
Status: Good		0.065*	0.065*		0.150***	0.150***
		(0.034)	(0.034)		(0.010)	(0.010)
Status: Renovated		0.112***	0.112***		0.201***	0.201***
		(0.033)	(0.033)		(0.010)	(0.010)
Treated $\times$ N. Hotels	23.924	-18.710	-18.465	24.679*	45.125**	44.874**
	(24.084)	(41.562)	(41.574)	(14.745)	(21.308)	(21.470)
Controls						
Property subtype		Yes	Yes		Yes	Yes
Property char.		Yes	Yes		Yes	Yes
Property status		Yes	Yes		Yes	Yes
$MedIncome_j \times Post_t$			Yes			Yes
Linear trends			Yes			Yes
Fixed effects						
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes
Zip-code	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	2.673	2.706	2.706	7.709	7.784	7.784
Adj. R-squared	0.41	0.60	0.60	0.67	0.78	0.78
Observations	216,131	98,839	98,839	1,102,106	394,859	394,785
Municipalities	Always treated + Neighboring					
Period	Jun20—Jan22					

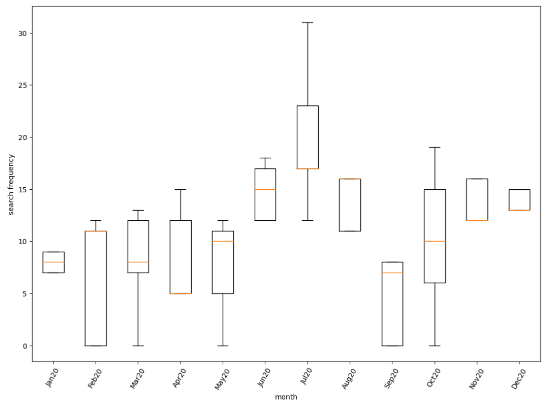
Notes: The dependent variable is the logged rent per squared meter (Columns 1 to 3) and the price per squared meter (Columns 4 to 6). All specifications include an interaction term between the treatment indicator and number of hotels per capita in each municipality, as a proxy for the level of touristic activity. Additionally, Columns (1) and (4) include only the treatment indicator. Columns (2) and (5) add indicators for the property type (apartment, loft, studio, etc.), the size, housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area), as well as an indicator for the property status and for whether it is a new construction. Columns (3) and (6) add the zip-level median income recorded before the introduction of rent control, interacted with an indicator for the post-treatment period, as well as a linear zip-code specific trend fitted to the treatment zip-codes. The sample includes all advertised rental and sales ads for dwellings posted on Fotocasa website for always treated municipalities and never treated neighboring municipalities. The time period spans between June 2020 and January 2022. All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the zip-code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A15: The effect of rent control on rents and sales prices—By travel distance from the center

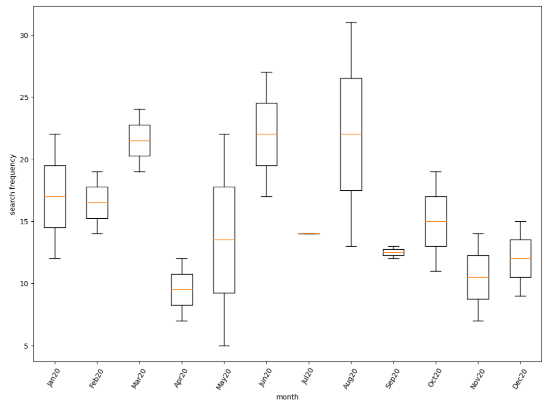
Dep.Var.:	*Log Rent*				*Log Sales Price*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment*Center	0.039*	0.043*	0.045*	0.044	0.043	0.042	0.040	0.042
	(0.023)	(0.023)	(0.025)	(0.029)	(0.036)	(0.035)	(0.026)	(0.030)
Treatment*Suburbs	-0.010	-0.017	-0.013	-0.020	-0.078***	-0.080***	-0.059***	-0.069***
	(0.016)	(0.018)	(0.015)	(0.027)	(0.022)	(0.021)	(0.016)	(0.014)
Size		-0.002***		-0.002***		-0.000***		-0.000***
		(0.000)		(0.000)		(0.000)		(0.000)
N. Bedrooms			-0.126***	-0.085***			-0.087***	-0.086***
			(0.002)	(0.003)			(0.003)	(0.005)
N. Bathrooms			0.045***	0.101***			0.132***	0.134***
			(0.009)	(0.003)			(0.019)	(0.022)
Status: Regular				0.062***				0.057
				(0.018)				(0.035)
Status: Good				0.055***				0.114**
				(0.018)				(0.046)
Status: Renovated				0.105***				0.170***
				(0.017)				(0.041)
Controls								
Property subtype		Yes		Yes		Yes		Yes
Property char.			Yes	Yes			Yes	Yes
Property status				Yes				Yes
Fixed effects								
Year-Month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean Outcome	2.673	2.673	2.673	2.706	7.709	7.709	7.709	7.784
Adj. R-squared	0.35	0.49	0.50	0.56	0.58	0.61	0.69	0.72
Observations	216,222	216,222	216,222	98,896	1,102,674	1,102,674	1,102,674	395,002
Municipalities	Always treated + Neighboring							
Period	Jun20—Jan22							

Notes: The dependent variable is the logged rent per squared meter (Columns 1 to 4) and the price per squared meter (Columns 5 to 8). Columns (1) and (5) include only the treatment indicator interacted with dummy variables denoting whether a property is located in the central *vs.* surrounding districts. Columns (2) and (6) add indicators for the property type (apartment, loft, studio, etc.) and size. Columns (3) and (7) include housing characteristics (number of rooms, number of bathrooms, presence of lift, garage, storage area, terrace, air conditioning, swimming pool, garden and a sports area). Columns (4) and (8) include all the controls for property characteristics and an indicator for the property status. In this specification, central properties are defined as those located within a 2-kilometer distance of the center, which is identified as the point of highest density in satellite images. The distance is calculated as the travel distance by car, utilizing the Google Maps API. The sample includes all advertised rental ads (Columns 1 to 4) and sales ads (Columns 5 to 8) for dwellings posted on the Fotocasa website in the always-treated municipalities and never treated neighboring municipalities. The time period spans between June 2020 and January 2022. All specifications include year-month and zip-code fixed effects. Standard errors are clustered at the municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A1: Google Trend searches for 'rent control law' in treatment and control municipalities



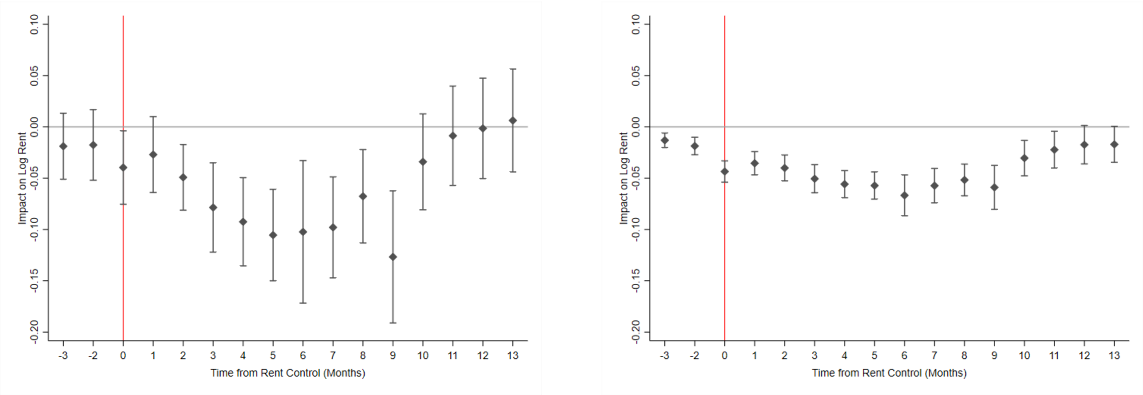
(a) Treated



(b) Control

Notes: The Figures displays the evolution of people's interests towards the rent control law, as measured by the time trend in Google searches for 'ley alquier'. The figure shows the monthly mean (connected line), median (horizontal bar), 25th and 75th percentiles (edges of the boxes), as well as 5th and 95th percentiles (whiskers), all computed at the municipality level, for selected municipalities in the treatment (Panel a) and control (Panel b) groups. The time period ranges between January 2020 to December 2020.

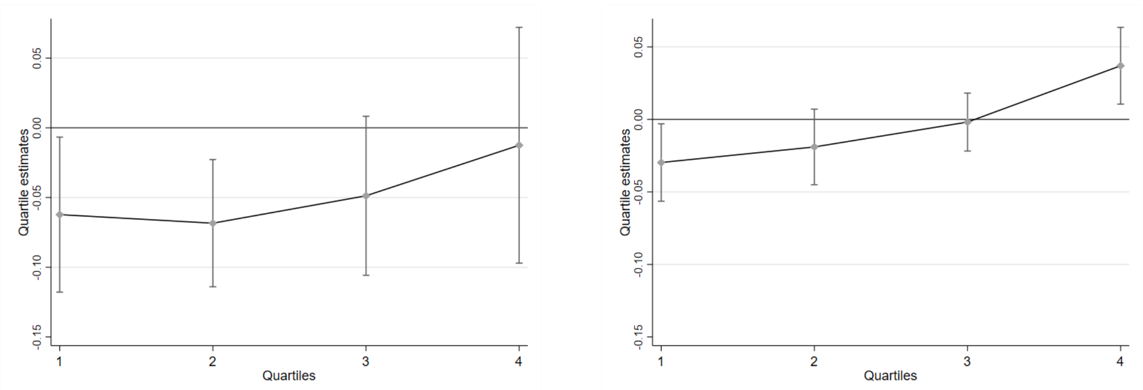
Figure A2: Robustness of dynamic effects on rents—Alternative control



(a) Catalonia (b) Spain

Notes: The figure shows the estimated effects of the introduction of the rent control law on the dynamics of the logged rent per squared meter. Specifically, we report the estimates of β_τ from equation 8, which correspond to the percentage change in log price per square meter at event time τ for properties that are located in municipalities subject to the rent control law during the whole period of study (always treated) relative to the control group (never treated). The control group includes never treated municipalities in the rest of Catalonia (Panel A) or in the rest of Spain (Panel B). The red vertical lines at event time 0 depict the month in which rent control was introduced (September 2020). The bounds correspond to 95% confidence intervals.

Figure A3: The impact of rent control on average rental and sales prices by quartile—Short-term



(a) Rental prices (b) Sales prices

Notes: The figure shows the short-term estimated effects of the introduction of rent control on the four quartiles of the municipality-level distribution of rents (Panel a) and sales prices (Panel b). The time period is restricted between June 2020 and June 2021. The bounds correspond to 95% confidence intervals.

B Appendix – Theory

In this Appendix we first characterize the economy of our toy model. Then, we provide the proofs of Propositions 1 and 2. We also describe the households and investor's problems in the unregulated housing market N (control group). Finally, we extend the benchmark model to the case of heterogeneous households in each region.

B.1 Characterization of the Economy

Let us denote the Lagrangian multiplier of constraint (5) by λ_1 and the Lagrangian multipliers of the participation constraints by λ_2. The aggregate endowment is denoted by $\Omega \equiv \omega + \omega^l$.

The equations below characterize the investor's decision problem.

$$u'(c)p_\kappa = \beta E_\omega(V'_{h'} - \lambda'_1\theta + \lambda'_2(1-\eta)\alpha^{o,l}) + \lambda_2 U'_c(\Omega - c)p_\kappa \tag{9}$$

$$u'(c)(p_\kappa - \kappa R) = \lambda_2(U'_c(\Omega - c)(p_\kappa - \kappa R) + \alpha^{R,l}\eta) \tag{10}$$

$$u'(c)q = \beta E_\omega(V'_{h'} + \lambda'_1) + \lambda_2 U'_c(\Omega - c)q \tag{11}$$

where U'_c is the derivative of the concave part of the household's quasi-linear preference function. We replaced $\mathbb{I}(\alpha^{o,l}/\alpha^{R,l} \geq p'_\kappa/(\kappa R'))$ and $(1 - \mathbb{I}(\alpha^{o,l}/\alpha^{R,l} \geq p_\kappa/(\kappa R)))$ by $(1-\eta)$ and η, respectively. As mentioned in the description of the investor's problem in the main text, there are four constraints: the budget equation (4) that is embedded into the objective function, two participation constraints (3) (one for the measure η of households that rent a house and the other for the measure $1-\eta$ of households that buy a house) and the collateral constraint (5). Because the last three restrictions enter linearly in the Lagrangian problem, we weight λ_2 by the measure of households depending on whether they rent or purchase a house.

Equation (9) is the first order condition (FOC) with respect to h', where $u'(c)p_\kappa$ is the cost of purchasing the house today, $\beta E_\omega(V'_{h'})$ is the benefit of selling the house tomorrow, $-\beta E_\omega(\lambda'_1\theta)$ is the benefit of relaxing the collateral constraint tomorrow, $\beta E_\omega(\lambda'_2(1-\eta)\alpha^{o,l})$ is the household's marginal utility of consuming owner-occupied housing in the regulated market which is internalized by the investor, and $\lambda_2 U'_c(\Omega - c)p_\kappa$ is the marginal utility of an increase in the investor's housing stock that affects households through the numeraire good market.[26]

We now turn to equation (10), which is the FOC associated with the fraction of the housing portfolio associated with the stock of rental housing. The term $u'(c)(\kappa R - p_\kappa)$ is the opportunity cost of investing in a rental housing unit (this cost is positive provided when $\kappa R < p_\kappa$), $\lambda_2(U'_c(\Omega - c)(p_\kappa - \kappa R))$ is the increase in the household's utility due to market feasibility of the numerarie good after the increase in investment, and $\lambda_2(\alpha^{R,l}(1-\eta))$ is the household's utility derived from renting a house in the unregulated market which is internalized by the investor.

[26] A change in the investor's portfolio may modify the investor's consumption of the numerarie good, and in turn the households' consumption of the numerarie good, and in turn the dynamic contract through the household's participation constraint.

Equation (11) is the FOC with respect to b', which is a standard Euler equation subject to a short sale constraint, except for $\lambda_2 U'_c(\Omega - c)q$ which represents the increase in the household's utility associated with an increase in investment.

B.2 Proofs

Proof of Proposition 1: It follows from equations (9) and (10). We use the common term $u'(c)p_\kappa$ to obtain the following non-arbitrage condition between investor's variables $\hat{h}$ and h':

$$\begin{aligned} &\beta E_\omega(V'_{h'} - \lambda'_1\theta + \lambda'_2(1-\eta)\alpha^{o,l}) = \\ &u'_c\kappa R + \lambda_2(\alpha^{R,l}\eta - U'_c(\Omega - c)\kappa R) \end{aligned} \tag{12}$$

The left hand side of equation (12) captures the total value obtained by buying a house: its value as an asset (T1), the utility derived from easing the short sale constraint (T2), and the household's utility derived from consuming housing services (T3). Term T1 is the investor's marginal utility. Term T3 is the household's marginal utility internalized by the investor through the participation constraint. The right hand side of equation (12) captures the value of renting: the utility of the income generated by the rent (T1') and the marginal utility for the households associated with housing services, which can be split in two terms: the direct utility for the household who rent (T2'), and the marginal value of the consumption of the numerarie good for the household (T3'). Both T2' and T3' are internalized by the investor .

The fall of p_κ along the transition must be consistent with an increase in the supply of housing h'. As rents κR in the short run decrease, then *if* u'_c is sufficiently higher than U'_c, then the change in R reduces the right hand side of (12). As V is concave, in order for the left hand side to decrease, h' must increase as expected. The general equilibrium effects captured by the relationship between u'_c and U'_c are essential to generate the observed match. Since households are lareger in size than the investors, we have $c < c^l$, so marginal utilities behave as expected. ■

Proof of Proposition 2: It follows by comparing equation (12) with equation (14) in this Appendix. Equation (14) is analogous to equation (12), but specific to the N region and it will be described in the following subsection. The response of the supply of new houses h' in the treatment group is characterized by (12) and in the control group by (14). This is observed in the data because the direction of the change is the same for R, κR, p and p_κ. To account for the order of magnitude, we focus on the general equilibrium determinants of h'. As explained before, the difference between the investor and the household's consumption explains the supply response (i.e., $c < c^l$). In particular, if $0 < (c^l_C - c_C) < (c^l_N - c_N)$, the elasticity of h' with respect to a change in the rent R in the treatment group will be smaller than in the control group. Because there is only one investor and $\omega_C = \omega_N$, this follows if $c^l_C < c^l_N$ which in turn implies that households moved from the rent control (C) region to uncontrolled (N) region after the policy is implemented. We allow the mass of households in region C (N) to be smaller (bigger, respectively) than 1. ■

The N-region

We now present the optimization problem for the N region. There is also only one type of household in the N region who has the option to buy or rent, in this case in a non-regulated market. To highlight this fact we define $\hat{h}^{N,l}$ to be the rented unit, where N stands for "non-regulated". The participation constraint is:

$$\frac{(\omega + \omega^l - c)^{1-\sigma}}{1-\sigma} + \mathbb{I}(\alpha^{o,l}/\alpha^{R,l} \geq p/R)\alpha^{o,l}h^l + (1 - \mathbb{I}(\alpha^{o,l}/\alpha^{R,l} \geq p/R))\alpha^{R,l}\hat{h}^{N,l} \geq \bar{U} \tag{13}$$

Note that in equation (13) we assume that the household's preferences are the same across regions when markets are active. Thus, the problem of the investor is:

$$V(\omega, x) = Max_{h',\hat{h},b',c} \quad u(c) + \beta E_\omega(V(\omega', x'))$$
$$\text{subject to equations (4), (5) and (13)}$$

For simplicity we omit the subscript associated with the region for each control variable. After computing the first order conditions, we obtain the following equation that arbitrages the owner-occupied housing market and the rental market:

$$\begin{aligned} &\beta E_\omega(V'_{h'} - \lambda'_1\theta + \lambda'_{2,N}\alpha^{o,l}(1-\eta^N)) = \\ &u'_c R + \lambda_{2,N}(\eta^N\alpha^R - U'_c(\Omega - c)R) \end{aligned} \tag{14}$$

where $\lambda_{2,N}, \eta^N$ is the multiplier associated with (13) and the fraction of households renting in this market respectively. There is only one difference between equations (12) and (14). Prices and rents are not affected by the housing policy, which implies that κR and p_κ are replaced by R and p respectively. A reduction in R will generate a decrease in p' associated with an increase in h' under the same conditions as before (i.e., if c^l is sufficiently above c). However, the order of magnitude may differ with respect to the previous case as $\kappa < 1$ and $p_\kappa < p$. For instance, if $(1-\kappa) > (p - p_\kappa)$, then the same decrease in R will generate a stronger effect in the supply of housing in the unregulated region.

Extension of the Model to Allow for Household Income Heterogeneity

We introduce household income heterogeneity in the benchmark model of Section 3 to get intuition on the mechanisms behind the changes in prices and quantities reported in the next Section 6.1. We now consider that there are low-income (L) and high-income (H) households in each region. The vector of household incomes is $(\omega_L^{l,C}, \omega_H^{l,C}, \omega_L^{l,N}, \omega_H^{l,N})$. There are four types of housing markets: regulated cheap housing (1), regulated expensive housing (2), non-regulated cheap housing (3), and non-regulated expensive housing (4). Typically, cheap housing is consumed by low-income households and expensive housing by high-income households, although we allow for other configurations as well, e.g., a low-income household from the regulated region consuming expensive housing in the unregulated region.

Each type of housing market is split between renting and owner-occupied. η^i $(1-\eta^i)$ denotes the measure of households that rent (purchase, respectively) a housing unit in market $i = 1, 2, 3, 4$. Before the rent control regulation is adopted, there is a mass 1 of households in each category. This measure can change because of migration after the policy is adopted.

For simplicity, we assume segmented markets with one investor for each of the four types of housing market who decides on how much rental and for-sale housing supplies to his respective market. The investor's income ω is constant across investment categories.

We denote by p_j^m the price of an owner-occupied housing unit of housing type $j = L, H$ in region $m = C, N$. Similarly, R_j^m denotes the rent of housing type $j = L, H$ of region $m = C, N$. For the shake of simiplicy and by abuse of notation, the subindex L of p_L^m refers to the cheap housing type that has low-income households as typical consumers, while the subindex H of p_H^m refers to an expensive housing type that has high-income households as typical consumers. As mentioned before, it is possible that a low-income household in the C-region buys an expensive house in the N-region at a price p_H^N.

We impose the following assumptions motivated by our empirical analyses in Section 6.1 (see Figure 4):

- *Assumption 1:* (a) $\omega_L^{l,C}(1-\eta^1) < \omega_L^{l,N}(1-\eta^3)$ and (b) $\omega_H^{l,C}(1-\eta^2) < \omega_H^{l,N}(1-\eta^4)$.
- *Assumption 2:* (a) $\eta^1 + (1-\eta^1) < 1$ and (b) $\eta^4 + (1-\eta^4) > 1$.
- *Assumption 3:* (a) $\alpha_1^{O,l}/\alpha_1^{R,l} > p_L^C/R_L^C$ and (b) $p_L^C/R_L^C > p_H^N/R_H^N$.

Proposition 3 (Household income heterogeneity): *Let assumptions 1, 2 and 3 hold. Then, (i)* $\eta^1\kappa\Delta R_L^C < \eta^3\Delta R_L^N < 0$*, (ii)* $(1-\eta^1)\Delta p_L^C < (1-\eta^3)\Delta p_L^N < 0$*, (iii)* $\eta^2\kappa\Delta R_H^C = \eta^4\Delta R_H^N < 0$*, and (iv)* $(1-\eta^2)\Delta p_H^N < (1-\eta^4)\Delta p_H^C < 0$*, where* Δ *denotes the first difference with respect to time. Moreover, the ratios of rents* R_L^C/R_L^N *and house prices* p_L^C/p_L^N *for cheap housing decrease after the adoption of rent control, the ratio of rents for expensive housing* R_H^C/R_H^N *remains roughly constant, and the ratio of house prices for expensive housing* p_H^C/p_H^N *increases.*

Proof of Proposition 3: The proof relies on Assumptions 1, 2 and 3. In particular, the change in rent and price ratios follow from Assumptions 1(a) and 1(b) and the investor's non-arbitrage equations; migration is possible because of Assumption 2; and the households' participation constraints are satisfied by Assumption 3. Before we elaborate these steps, notice that we denote the aggregate income in each market by $\Omega_j^m \equiv \omega + \omega_j^m$, where $m \in \{C, N\}$ and $j \in \{L, H\}$. Given this notation we use equations (12) and (14) to characterize the regulated and non-regulated market by simply replacing Ω in each equation with Ω_j^C in equation (12) and Ω_j^N in equation (14). This results in four non-arbitrage equations between renting and selling a housing unit, one for each market.

We analyze statements (i)-(ii) and (iii)-(iv) by pairs since equations (12) and (14) imply that changes in rents generate price variations with a different supply elasticity across groups and regions.[27]

[27] This can also be seen by noting that the fraction of agents add up to 1 before allowing for migration.

Statements (i) and (ii) refer to low-income households. The investor's non-arbitrage equations (12) and (14) characterize (i) and (ii) by replacing the terms $-U'_c(\Omega - c)\kappa R$ and $-U'_c(\Omega - c)R$ by $-U'_c(\Omega^C_L - c)\kappa R$ and $-U'_c(\Omega^N_L - c)R$, respectively. The remaining terms in the two equations do not change because heterogeneity only affects income.

Notice that for statements (i) and (ii) to hold, we need a stronger response in prices in the unregulated region for rents in the regulated C-region to change more than rents in the unregulated N-region. [28] Notice also that the elasticity of for-sale cheap (L) housing supply with respect to rents is smaller in the controlled C-region than in the unregulated N-region: $0 < -\varepsilon^C_{h',L} < -\varepsilon^N_{h',L}$. Alternatively, we can also write $\left|\varepsilon^C_{h',L}\right| < \left|\varepsilon^N_{h',L}\right|$ for $\varepsilon^C_{h',L}, \varepsilon^N_{h',L} < 0$.[29] Assumption 1 (a) generates this difference in elasticities. To see this, notice that the only difference between equation (12) (adjusted to characterize the rent-controlled market of low-income households) and equation (14) (modified to represent the unregulated market of low-income households) is the difference between $-U'_c(\Omega^C_L - c)\kappa R$ and $-U'_c(\Omega^N_L - c)R$. Assumption 1(a) implies $\Omega^C_L < \Omega^N_L$, which in turn implies $-U'_c(\Omega^C_L - c)\kappa R < -U'_c(\Omega^N_L - c)R$. This last inequality and equations (12) and (14) imply that $\left|\varepsilon^C_{h',L}\right| < \left|\varepsilon^N_{h',L}\right|$.

Statements (iii) and (iv) are specific to high-income households. In this case, a similar change in rents in both regions ($\kappa\Delta R^C_H = \Delta R^N_H < 0$) implies a stronger response in prices in the unregulated municipality $\Delta p^N_H < \Delta p^C_H < 0$. Thus, the elasticity of for-sale expensive (H) housing supply with respect to rents in the unregulated region is larger than in the regulated region, i.e., $0 < -\varepsilon^C_{h',H} < -\varepsilon^N_{h',H}$. Alternatively, we can also write $\left|\varepsilon^C_{h',H}\right| < \left|\varepsilon^N_{h',H}\right|$ where $\varepsilon^C_{h',H}, \varepsilon^N_{h',H} < 0$. Following the same reasoning as for the cases (i) and (ii), Assumption 1 (b) implies that $-U'_c(\Omega^C_H - c)\kappa R < -U'_c(\Omega^N_H - c)R$.

Since $\left|\varepsilon^C_{h',H}\right| < \left|\varepsilon^N_{h',H}\right|$, a change in rents (statement (iii)) implies a stronger response in house prices (statement (iv)).

Low-income households in the regulated region (Assumption 2a) choose to migrate to the non-regulated region (Assumption 2b) because $-U'_c(\Omega^C_H - c)\kappa R < -U'_c(\Omega^N_H - c)R$ and $-U'_c(\Omega^C_L - c)\kappa R < -U'_c(\Omega^N_L - c)R$. Assumption 2 guarantees that these two inequalities hold.

Assumption 3 guarantees that there are at least *some* low-income households in the regulated region choose to migrate to the uncontrolled region after the policy is implemented and consume expensive housing. For this we assume that the price-rent ratio of purchasing an expensive house in the uncontrolled region is sufficiently low (i.e., $p^C_L/R^C_L > p^N_H/R^N_H$).[30]

[28] For example, let us consider that in market 1 (L-C) rents drop by 30% and in market 2 (L-N) rents drop by by 10%. As a result, prices in the former drop by 20% and in the latter by 10% because the supply of new housing h' is more responsive in the unregulated region ($20/30 < 10/10$). This implies that both ratios $\kappa R^C_L/R^N_L$ and p^C_L/p^N_L go down as in Figure 4.

[29] If the differences in elasticities is too big, we will not observe a drop in the ratio of prices when the ratio of rents goes down. Period 8 in figure 6 is representative of that possibility: when the ratio of rents significantly decreases, the ratio of prices does not.

[30] This may occur simply because renting in the uncontrolled region is too costly with respect to buying.. Notice that the new equilibrium is also compatible with other types of configurations. For example, a low-income household from the regulated region migrates and rents cheap housing in the unregulated region. This is the case when (a) $\alpha^{O,l}_1/\alpha^{R,l}_1 < p^C_L/R^C_L$ (households in the regulated C-region prefers to rent cheap L-houses) and (b) $p^N_L/R^N_L > p^C_L/R^C_L$ (renting cheap L-housing in the unregulated N-region is cheaper than in the regulated C-region). It is also possible that low-income households from the regulated C-region migrate and buy expensive housing in the unregulated N-region. This happens if (a) $\alpha^{O,l}_1/\alpha^{R,l}_1 > p^C_L/R^C_L$ (households in the regulated C-region prefer to buy cheap L-houses) and (b) $p^C_L/R^C_L > p^N_H/R^N_H$ (L-houses in the regulated C-region are relatively more expensive than H-houses in the unregulated N-region).

The last part of Proposition 3 follows from statements (i)-(iv). In particular, statements (i) and (ii) from Proposition 3 imply that the ratios of rents R_L^C/R_L^N and house prices p_L^C/p_L^N for cheap housing decrease after the adoption of rent control. Statements (iii) and (iv) from Proposition 3 imply that ratio of rents for expensive housing R_H^C/R_H^N remains roughly constant, while the ratio of house prices for expensive housing p_H^C/p_H^N increases. ■

Further discussion:

When the rents of cheap housing units in the regulated region (R_L^C) decrease after the policy is adopted, the overall value of the rental option in the investor's problem decreases. To reestablish the investors' non-arbitrage condition between renting today or selling tomorrow, the supply of owner-occupied cheap housing in the regulated region must increase while cheap rental housing supply must decrease.

Since we only observe changes in relative rents in our data, we cannot separate apriori the change in the numerator (regulated region) and denominator (unregulated region) of the rents ratio. We assume instead that rents in both regions decrease. Housing supply increases as a result and this triggers a reduction in house prices in both regions.

Recall that the responsiveness of house prices is determined by the difference in the marginal utility of numerarie consumption between the investor and the households. The bigger is the this difference, the stronger is the response of house prices.

Our data shows a reduction in the ratio of cheap units rents (R_L^C/R_L^N) and roughly no changes in the ratio of expensive units rents (R_H^C/R_H^N). Thus, the response of the cheap units and expensive units' house price ratios (p_L^C/p_L^N and p_H^C/p_H^N) must be different.

For a given elasticity of house prices to rents, a stronger reduction in rents in the regulated market of cheap units ($\kappa\Delta R_L^C < \Delta R_L^N < 0$) implies a stronger response in the price of owner-occuppied units in the regulated region ($\Delta p_L^C < \Delta p_L^N < 0$). Here we assume that the housing market is segmented.

In absence of migration, we would not be able to generate a change in the ratio of expensive house prices (p_H^C/p_H^N) as observed in the data when expensive rents exhibit a similar change in both regions (as observe in data $\kappa\Delta R_H^C = \Delta R_H^N < 0$) and the elasticities of for-sale owner-occupied expensive housing supply with respect to rents are similar to the ones in the cheap housing market.

By allowing for migration from the unregulated to the regulated market, the households' marginal utility of the numerarie consumption in the expensive unregulated market changes in such a way that the responsiveness of house prices increases, allowing for an increase in the ratio of expensive house prices (p_H^C/p_H^N). This follows because migration implies $\Delta p_H^N < \Delta p_H^C < 0$.

Finally, notice that for some households to migrate and buy expensive housing, we restrict the ratio of marginal utilities of renting and purchasing and the ratio of relative prices accordingly. Roughly speaking, low-income households in the regulated region that would prefer to live in a low-income owner-occupied unit in the controlled region if migration were not allowed choose to migrate to the uncontrolled region provided that the relative price of purchasing a high-income house in the uncontrolled region is sufficiently low. This can be simply because renting in the uncontrolled region is too costly with respect to buying. This migration increases (decreases) the size of the numeraire market in the non-regulated (regulated) region, in turn lowering (increasing) the marginal utility of the households' numeraire consumption and increasing (decreases) the elasticity of housing supply in the unregulated (regulated, respectively) market for both household types.