# THE EFFECT OF SECOND-GENERATION RENT CONTROLS: NEW EVIDENCE FROM CATALONIA*

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**Abstract**

Catalonia enacted a second-generation rental cap policy in late September 2020. The policy affected some municipalities but not others and, within the former, the policy only affected the units with prices above the "reference" price. Using microdata on rental units, we analyze the effect of the policy on both rental prices and the composition of rental supply. We find that the policy led to a reduction in rental prices of approximately 5%. The policy also led to a convergence of prices toward the reference price, a decline in the overall supply of rental units, and to a significant shift in the composition of units available in the market. We argue that the evidence can be understood through the lenses of a directed search model. The model predicts that the policy leads to a decline in welfare of homeowners and tenants in "low-price" units, and a welfare improvement for tenants in "high-price" units.

**Keywords:** *rent control, reference price, housing supply, event study, directed search*
**JEL classification numbers: D4, R21, R28, R31**

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# I Introduction

The increase in rental prices has reduced the accessibility of housing in many large cities and prompted much political debate.[1] One increasingly popular measure has been the introduction of rent controls. This type of policy has a long history. The Increase of Rent and Mortgage Interest Act of 1915 introduced rent controls in the United Kingdom, and it was replicated by several countries around Europe. Over the last 40 years, some cities in the US have also introduced various forms of rent control, including New York City, Washington, D.C., San Francisco, Boston, Brookline, Cambridge, and Los Angeles. In contrast, there are 36 US states that forbid rent control laws entirely, although this seems to be changing. In 2019, for example, the state of Oregon approved the first US statewide rent control act (Senate Bill 608), overturning the previous ban on rent control. Rent control was also enacted at the state level in California in 2020 under the California Tenant Protection Act.

In recent years, the US and many other countries have used what is often referred to as "third-generation" rent controls, which cap rent increases within tenancies. However, some European cases have been recently used "second-generation" rent controls,[2] limiting rent increases within and between tenancies. This was the case for the *Mietpreisbremse* in Germany, which was enacted in 2015. The *Mietpreisbremse* was further modified in 2020, prohibiting any rent increase during a period of five years and moving to a de facto "first-generation" rent control.

There is also a long history of studying the effects of rent controls among economists. Since Friedman & Stigler (1946), much economic research has used theoretical arguments to emphasize the negative consequences of keeping rents below market prices on economic efficiency.[3] As we discuss in more detail later, arguments against rent controls include fears of distortions in the housing rentals and sales markets, mismatches between types of housing units and types of tenants, and inefficient levels of consumption of housing services. On the plus side, rent caps might be an effective tool to preserve a neighborhood's social capital.

In contrast, the empirical literature on the effects of rental price controls has been scarce. As noted in Diamond et al. (2019), "we have little well-identified empirical evidence evaluating how introducing local rent

1. Two monumental sociological studies, (Desmond, 2017; Osman, 2022), have shown the problems of housing accessibility in the US. Osman (2022), in a very original sociological analysis, argued that gentrification in Brooklyn "was the work not of banks, developers, and speculators but of grassroots movements waging war against those very forces. The movement began as a neo-romantic quest for authenticity".

2. Note that Arnott (1995) defined second-generation rent controls as any mechanism that is not a rent freeze. Following the recent literature, we differentiate between second- and third-generation rent controls.

3. Most of this theoretical literature has addressed the issue of rent freezes. Arnott (1995) argued that there is not much dispute about the harmful nature of first-generation rent controls since the empirical evidence is quite robust.

controls affects tenants, landlords, and the broader housing market". The reason for this lack of evidence is twofold. First, microlevel data on house prices and rents are mostly available for recent periods, whereas sources of potentially exogenous variation are available for earlier times. Second, there are various ways in which rental cap policies are implemented; hence, we have only a partial understanding of how certain types of rental cap policies can affect the market.[4] Recent empirical evidence in the US has either evaluated the removal of price caps or, as in the case of Diamond et al. (2019), evaluated "third-generation" rent control policies.

In this paper, we analyze the effects of a "second-generation" rent control regulation that was introduced in Catalonia in September 2020. This policy established a maximum rental price (reference price) for each housing unit in the market, which depended on the rental price of the 25 closest units. The policy affected only some municipalities, providing us with additional sources of variation, for instance, to compare various groups of treated and non-treated municipalities. Moreover, unlike other studies, we have detailed housing unit data on rental housing in Catalonia. In particular, we have information about each rental contract signed, its start date, its end date (in cases where the contract has terminated), the rental price, and some house unit characteristics, such as size. We also have a unique identifier for each unit, allowing us to track the prices associated with different contracts and, hence, to control for unit time-invariant characteristics.

Using these data, we report two main results. First, we document that the policy led to price decreases in the rental market. On average, rental prices dropped by approximately 5% in treated municipalities relative to non-treated ones. The estimates are precise, and we find no evidence of systematic differences in the trends leading to the policy change – even when we compute pre-trends using more than 50 months of data. We obtain these results using both nonparametric approaches that rely on comparing histograms of prices relative to the reference price, and those that rely on standard difference-in-differences comparisons. Interestingly, we also find a strong convergence of prices toward the reference price in treated municipalities. While price increases are uniform across the entire price distribution in non-treated municipalities, we find a strong gradient in price changes along the distribution in treated municipalities. "Low-price" units experience price increases above those seen in control municipalities, while "high-price" units experience strong price declines.

As we discuss in more detail below, the (mostly theoretical) literature has argued that price caps can result in changes in the supply of units to the rental market. These changes might be driven by both a

4. Arnott (1995) argued that rent control should be judged by the analysis of empirical evidence and, since programs are so different across countries and cities (rent control within tenancies or also across tenancies, exemptions, level of vacancy decontrol, tenant protection, condominium conversion, etc.) on a case-by-case basis.

change in the composition of the units and a change in the overall number of units available in the market. We document that the overall supply of units in the market declines by around 15%, mostly explained by a large decrease in units available at prices above the reference prices, which is not compensated by the increase in units below. These estimates allow us to compute an elasticity of rental unit supply of around three.

From the perspective of a standard perfect competition model of the (rental) housing market, evaluating the welfare consequences of rental cap policies is relatively straightforward. In this model, as long as the supply of rental units is not perfectly inelastic, rental caps lead to a reduction in rental prices and reduced welfare among owners. Rent caps also generate a gap between supply and demand. All renters who participate in the market, however, are better off. Overall, the perfect competition framework also predicts a deadweight loss which, from a policy perspective, is the price to pay to redistribute welfare from owners to renters. Some of the evidence we find, however, does not fit well with a perfect competition model. For instance, in perfect competition models, there is only one rental price. Hence, it is difficult to interpret the meaning of a policy that leads to "price convergence to the reference price". It is also difficult, in perfect competition, to fully understand what is really meant by changes in the composition of rental housing supply.

We argue instead that the facts that we document are best understood through the lens of a directed search model, adapted to the particularities of the rental housing market. In our model, owners post rental prices. Potential tenants observe these prices and decide to apply for rental units. We assume that renters apply to two types of units: high- and low-price units. As shown in the context of labor markets in Galenianos & Kircher (2009), this generates, in equilibrium, the coexistence of two sub-markets in the rental market. Some owners choose to post a high price, understanding that the chance of getting a tenant is lower. Some other owners choose to post a low-price, ensuring a higher chance of matching with a tenant but receiving a lower payoff. Renters apply to the two types of sub-markets. If they are lucky, they find a low-price unit, which they always prefer. If unlucky, they settle for a high-price one. We show that, as in Galenianos & Kircher (2009), the market does not lead to a constrained efficient allocation, contrary to what happens in the perfect competition framework.

The directed search model that we propose has a number of predictions that help us understand the empirical evidence better than the perfect competition framework. First, without a rental price cap policy, the model generates price dispersion. Second, the policy leads to price convergence. Third, as long as the supply in the rental market is not fully inelastic, the model predicts that the units leaving the rental market will be the "high-price" units, exactly as we see in the data.

Welfare consequences are more nuanced, and likely more realistic, in our model compared to the perfect competition framework. We show that, a rental price cap that collapses the various sub-markets into a single one is efficient. Intuitively, it prevents sellers from applying to multiple sub-markets, which alleviates the externality caused by the fact that with multiple sub-markets, renters contribute to the "queue length" in sub-markets where they may not end up participating. If the rental cap is set at "intermediate" prices, i.e., somewhere in between the high- and the low-prices in the multiple sub-markets equilibrium, the policy hurts renters in low-price units and is beneficial to renters in high-price ones. Depending on the level at which the rental cap is set, the policy can be beneficial or harmful for owners. On the plus side, the policy prevents multiple sub-markets which is good for sellers, since it reduces the amount of competition they face, as renters apply only to one instead of multiple sub-markets. The policy, however, can set prices low enough to hurt sellers and still not destroy the rental market, which results in a welfare decline among owners.

Overall, these results provide empirical support for the notion that "second-generation" rental caps are effective at reducing prices but also affect supply and welfare. In this paper, we contribute to the literature in two ways. First, we provide well identified evidence on the effect of "second-generation" price controls using micro-data. Second, we provide a novel framework to interpret the empirical evidence and to think about the welfare consequences of this type of policies.

### *Related literature*

Rent controls were a popular policy tool in the 1920s and 1970s, and they seem to have been returning to the policy arena in the last few years. However, early data for analyzing the effects of these types of policies are scarce or even unavailable. Hence, the literature on this topic is still predominantly theoretical.

While there is little doubt that these types of policies help to reduce rental prices, a large theoretical literature has identified several channels through which the policy can lead to unintended consequences. For instance, rent controls can lead to mismatches between tenants and rental housing. Once a tenant has obtained a rent-controlled apartment, the household might decide to reside in that dwelling longer than would have been optimal based on their needs for housing services, a point made by Suen (1989); Glaeser & Luttmer (2003); Bulow & Klemperer (2012) among others.[5] Similarly, if rental prices are below market prices, renters might choose to consume excessive amounts of housing (Olsen, 1972; Gyourko & Linneman, 1989). Rental price caps can also cause rental housing to deteriorate. Owners might decide not to invest in maintenance because they cannot recoup this investment when rental price growth is limited (Sims, 2007; Downs, 1988; Moon & Stotsky, 1993). Rent controls can also shift the relative size of the rental and home-

5. One example of these mismatches is households with adult children who might decide to stay in family-size apartments, and, hence, displace families with young children into crowded accommodation and small studios.

ownership market(Fetter, 2016). All of these papers explore mechanisms that may result in a change in the composition and, potentially, the overall number of units available in the rental market. On the positive side, rent control also offers potential benefits for tenants. For example, longer-term tenants might develop specific social capital, such as a network of friends and family, or proximity to a job or a local school; hence, the risk of excessive rental price appreciation might be very costly for some households. Most of these mechanisms, however, are likely to operate over the longer term.

We contribute to this literature by exploring the shorter term effects of the introduction of rental caps in a directed search model of the rental market, something that has not been done before. Directed search models have been developed mainly to study the labor market (see the recent review of the literature in Wright et al. (2021). There are a handful of papers that use *random* search models to study the home ownership market, such as Diaz & Jerez (2013), but our paper is the first to use a *directed* search model to study the rental market and to explore a rental cap policy. We argue that directed search models are particularly well suited to studying rental markets. One of the key features in these models is that prices are posted and that potential renters apply to various types of housing units, which leads to the co-existence of various sub-markets, something that, we think, characterizes the rental market well. Many of the empirical facts that we document match align with either the assumptions or the implications of the model. Moreover, we show that standard results in the literature, such as the deadweight losses generated by rent control policies predicted by competitive models, may not apply in more realistic models. Hence, our approach challenges some of the conventional wisdom in this literature.

It is only more recently that the availability of reliable microdata sets at the level of individual rental units has allowed researchers to study rent control in detail. In a seminal contribution, Sims (2007) analyzed the case of the end of rent control in the Boston metropolitan area using data from the American Housing Survey. He found that rent control induced owners to remove units from the rental market and decreased rents. In addition, Sims (2007) showed that there was no effect on construction, but that rent control reduced maintenance in controlled units, and the spillover effect reduced the rent of non-controlled units. In a similar vein, Autor et al. (2014) analyzed the impact of the unexpected elimination of rent control in Cambridge, finding that the market value of properties no longer subject to rent control increased by 45%. In addition to the direct effect of removing rent caps, Autor et al. (2014) showed that removing rent control had significant, indirect effects on neighboring properties, also increasing their value. Unlike our paper, these two papers analyze the removal of rental price controls, rather than its introduction. Their focus is also on the long-term consequences of price controls, rather than the shorter-term impact on the rental market.

More recently, Diamond et al. (2019) discussed the consequences of an expansion of rent control on tenants, landlords, and the overall San Francisco housing market. In 1979, San Francisco imposed third-generation rent control on buildings with five or more apartments. It was a control with regulated increases in income, at the rate of the CPI, within tenancies. The regulation did not apply between tenancies. New and smaller buildings were exempt from control. However, in 1994, the exemption for buildings with fewer than 5 apartments was eliminated. Diamond et al. (2019) showed that, while rent control appears to help ordinary tenants in the short term, it decreases affordability in the long term. Rental units are sold to very high-income buyers, increasing gentrification. We differ from Diamond et al. (2019) in that we analyze a second- rather than a third- generation rental cap policy and that we tie our empirical findings to a novel model of the rental housing market.[6]

Our paper also contributes to the recent literature on second-generation rent control in Europe. Breidenbach et al. (2022) uses detailed rental price microdata and a triple differences event study to show that the initial success of rent caps in reducing rental prices vanished after one year. Some of the empirical findings in our paper resonate with theirs; however, we provide a more complete study of how the policy affects the rental market along the entire distribution and we provide a framework with which to think about our, and their, empirical results. Mense et al. (2019a) and Mense et al. (2019b) also analyze the introduction of second-generation rental caps in Germany. Their emphasis is slightly different. Mense et al. (2019a) show that land prices grow faster in municipalities where rent caps are implemented than in the control group, while Mense et al. (2019b) document a positive spillover effect of rent control on markets without rent caps. Relative to these two papers, we provide evidence from a new policy change and a new framework to analyze these types of policies. In a contemporaneous paper, Jofre-Monseny et al. (2022) study the same policy that we analyze in our paper but using a purely empirical approach and aggregate, instead of microlevel, data. As a result, they cannot analyze changes in the composition of units in the rental market, something that we argue is crucial for thinking about the effects of these types of policies.

## II Rent control in Catalonia

The Spanish housing market is characterized by a large proportion of homeowners and a small proportion of renters. In 2011, homeowners represented 79% of the market, while renters amounted to 11%[7]. After the financial crisis, the demand for rental housing increased significantly in a very short period of time. In 2018, tenants at market price represented 16% of the market, while homeowners decreased to 75%. Several

6. In all Californian cities with rent control, there is vacancy decontrol, which means that rents can be set at the market rate when new households move in. This is the difference between third- and second-generation rent control.

7. These two numbers do not total 100% because some tenants have reduced rents and some units are rented free of charge.

factors explain this change. First, banks reduced lending and increased the requirements for obtaining a loan. Second, young people likely realized that obtaining a mortgage to buy a house before obtaining financial stability could lead to financial distress and a high risk of foreclosure. The large increase in the demand for rental housing was, however, not met with a sufficient increase in supply, and rental prices increased. Other factors, such as rental platforms, might have also played a role, at least in some specific neighborhoods (Garcia-Lopez et al., 2020; Almagro & Domiınguez-Iino, 2022). For instance, pressure on rental prices was greater in cities where there was already a large proportion of tenants. This is the case for Barcelona (which experienced a 38.4% rental price increase over the period of 2014-2019) and the whole Barcelona metropolitan area (23.4% rental price increase).

To attempt to contain the increase in prices and following the experiences of some European cities, such as Berlin and Paris, the Parliament of Catalonia approved a system of rent ceilings that was implemented at the end of September 2020.[8] The law considered two types of areas: tight housing markets and the rest of the territory. "Stressed" municipalities were initially defined as those where rental prices had grown by more than 20% during the period of 2014-2019, which, as we show below, comprise many municipalities around Barcelona plus others dispersed over the territory.

The Catalan law imposed second-generation rent caps, limiting rent increases between tenancies, following the regulations approved in Berlin.[9] After the approval of the law, reference prices became, at least on paper, the highest prices that the rental price per square meter could reach. To construct the reference price for each unit, the pool of comparison units is stratified by size: units greater and less than 90 square meters. That is, if the size of the unit is larger than 90 square meters, then the reference price is calculated as the mean of the 25 closest units between 90 and 250 square meters. If the size is smaller than 90 square meters, then the reference price is calculated as the mean of the 25 closest units with a size plus/minus 10 square meters. Hence, detailed unit-level data are needed to fully evaluate the effects of the policy. An interesting feature of these reference prices is that they were calculated not only for the areas that were declared "stressed" markets but also for a total of 137 municipalities, many of which not affected by the policy change.

# III Data

Our data consist of more than half a million rental contracts reported to INCASOL from 2016 until June 2021. In Catalonia, there is a legal obligation to report rental contracts to this institution. We merged the

8. Act 11/2020 of September 18, 2020
9. The Spanish regulation already capped rents within tenancies using CPI inflation.

information available in INCASOL with the information at the AHC (Housing Agency of Catalonia). The AHC was responsible for setting the methodology to calculate the reference price for each rental unit. After merging both data sources, we had information about the closing price, the start date of the contract, the end date (if it had already ended), the area in square meters of the unit,[10] and the reference price per square meter.

The main advantage of these data is that we have information about rental market prices, instead of the asking price or an imperfect measure of the agreed price. This difference, for example, is important with respect to the research that evaluated the impact of rent control in Germany, which used posted rents (Mense et al., 2019a,b; Borusyak et al., 2021). These data are also more accurate than the online price data used in other research and are widely available through scrapping. As seen in Figure I, prices posted by online intermediaries are much more volatile, with faster declines in downturns and faster growth in upturns.[11] Finally, it is worth emphasizing that for each unit on the market, we have data on both rental price and the size of the unit. This information allows us to investigate composition effects and prices per square meter – something that cannot be investigated with aggregate price data used in an existing analysis of this policy change (Jofre-Monseny et al., 2022).

Our data cover two types of municipalities. We denote as "treated" the rental contracts for units in municipalities that were subject to the rent cap and as "non-treated" or control group the ones that were not. It is worth emphasizing that only contracts signed after September 22, 2020, were subject to the new regulation. Figure II shows a map of the distribution of the municipalities subject to rent caps. As anticipated before, many of the treated municipalities are in the metropolitan area of Barcelona, although some are in other areas of Catalonia.

Treated and non-treated municipalities are markedly different in a number of dimensions, beyond their proximity to Barcelona. Table I reports a number of summary statistics. Treated or "regulated" municipalities tend to be larger, more expensive, and denser. Since our empirical design depends on comparisons in the trends across municipalities, systematic differences in the level of outcomes of interest do not necessarily pose a threat to our identification strategy.

10. We have the habitable surface, which is a better indicator than the built surface.

11. Online prices correspond to Idealista, which is the leading online real estate intermediary in Catalonia.

# IV Empirical Evidence

In this section, we evaluate the impact of the policy change that took effect in late September 2020. We analyze various dimensions through which the policy affected the rental housing market. First, we study how the policy affected rental prices using a number of empirical strategies. Second, we study whether the policy also affected the supply of housing, in terms of both the composition and the overall number of units available in the rental market.

## *IVA. The effect of the policy on rental prices*

### IVA..1 Nonparametric evidence

We start our analysis of how the policy change affected rental prices by providing nonparametric evidence. As we discussed before, the rental cap implied that rents could not exceed the "reference" price, or index, for each particular unit. Hence, we can define the *excess price* as:

$$\text{Excess price}_{i(m),t} = \ln\left(\frac{P_{i(m),t}}{\bar{P}_{i(m),t}}\right)$$

where $P_{i(m),t}$ denotes the rental price per square meter in unit $i$, located in municipality $m$, at time $t$, and where $\bar{P}_{i(m),t}$ is the price cap calculated for this housing unit based on the surrounding units, as explained earlier. This 'excess price' measure captures the percentage deviation of the actual rental price from the cap. That is, if the rental price is at the cap, then this measure is equal to 0. Any negative number means that the rental price is less than the cap. If the excess price is equal to -0.1, it indicates that the rental price is approximately 10% less than the rental cap.

It is worth noting that the reference price is also calculated for the control area – something that was justified for transparency purposes;[12] hence, we can construct histograms for both treated and non-treated areas. In Figure III we show histograms of excess prices for both treated and control municipalities. On the left-hand side of the figure, we display the histogram of excess prices before and after the rental cap policy in treated municipalities. The right-hand side figure shows the same histograms for the control group. There are several things to note in Figure III. On the one hand, we see that the histogram of prices among treated municipalities is "distorted". There is a much greater mass of units with prices immediately below the cutoff and an even greater mass greater than, but close to, the cutoff. This outcome contrasts with the histogram for the control municipalities. The histogram in control municipalities does not have any bunching around

12. The reference cannot be computed for some units in control areas, as they are in areas that are too isolated.

0. Moreover, during the post-policy change period, excess prices are shifted to the right, something we do not observe in treated municipalities.

Figure III is the first indication that the policy change might have had some effect on rental prices, although it seems clear that not all the new contracts complied with the legislation. It is also difficult to assess whether the rightward shift in the distribution of rental prices in control municipalities is the natural evolution of rental prices in those locations or whether it is some sort of spillover from treatment to control locations.

One of the aspects that makes Figure III difficult to interpret is that it pools all contracts signed before and all those signed after the policy change. When pooling all of the data, it is difficult to determine what part of the observed changes is due to differential trends and what part might be a consequence of the policy change. To advance in this dimension, it is worth showing the histogram of prices for a narrower set of new contracts.

Figure IV shows four histograms for two specific periods around the months of the policy change and around the same months in the previous year. Specifically, the graphs on the left show histograms for September and October 2019, and the graphs on the right show September and October 2020. Panel A includes the contracts in the treated municipalities, while Panel B, at the bottom, shows the distributions of rental prices for the contracts in the control areas.

The comparison of the figures on the left and right indicates that in the control municipalities do not show much difference in rental prices before and after the policy change, whereas in treated areas there is a clear movement of the distribution of excess rents to the left, producing a mitigating effect on rents. Again, it is also interesting to note that, after the policy change, almost half of the rental prices are greater than the corresponding reference price, which seems to suggest that owners found ways to charge renters a price greater than the reference prices by finding loopholes in the regulation or informal agreements with tenants.

While the histograms presented in this section are indicative of the effects of the policy, it might be useful to quantify the results using various identification strategies. We turn to this point in what follows.

#### IVA..2 Two-way fixed effects estimates

A second way to look at how the policy affected rental prices is to compare treated and non-treated municipalities, following a standard two-way fixed effects difference-in-differences approach. These types of research designs crucially depend on whether the trends in the outcomes of interest between the treated and control groups are parallel prior to the treatment. This condition is usually checked visually by running dynamic difference-in-differences specifications such as the following:

$$y_{i(m),t} = \delta_m + \lambda_t + \sum_k \beta_k \mathbf{1}_{t=k} \times \textit{Rent Cap}_m + \varepsilon_{i(m),t} \quad (1)$$

where $y_{i(m),t}$ is an outcome of interest, such as the "excess price" defined before, and where $\textit{Rent Cap}_m$ is a dummy variable taking the value of 1 if municipality $m$ ever faces a rent cap. The coefficients $\beta_k$ estimate the differential effect of the treatment at various points in time. In what follows, $i$ indicates units, $m$ municipalities, and $t$ monthly frequency.

Figure V plots these $\beta_k$ coefficients for three different outcomes of interest for each month around the policy change, with a window of more than 50 months prior to the change and 9 afterward. The three outcome variables that we consider are the excess price (i.e., the log ratio of rental price divided by the price index or reference price), the price per square meter, and the proportion of rental prices greater than the reference price. In each of these three cases, we consider the baseline specification (1) and a second specification that includes flexible province trends.

Using the baseline specification, we see in Panel A that the average excess price drops immediately after the introduction of the rent cap. It is also worth emphasizing that all of the coefficients prior to the policy change fluctuate around and are never significantly different from zero. We get very similar patterns when including provincial trends. If anything, including province-specific flexible time trends makes the trends prior to the policy change even more parallel. When we include these flexible trends, the drop in prices after the implementation of the rental cap is of the same magnitude.

We can quantify the results observed in Figure V by running the following difference-in-differences specification:

$$y_{i(m),t} = \delta_m + \lambda_t + \beta \textit{Rent Cap}_{m,t} + \varepsilon_{i(m),t} \quad (2)$$

where "Rent $\text{Cap}_{m,t}$" is a dummy variable taking the value of 1 if municipality $m$ has a rent cap in place at time $t$.

Table II shows the results using the same three outcome variables: excess prices, price per square meter, and the fraction of prices greater than the index or reference price. Across the three panels, Column 1 shows the baseline specification, which only includes municipality and month fixed effects. Column 2 includes province times month fixed effects, which allows for each of the four Catalan provinces to evolve in different ways over time.[13] Column 3 includes rental unit fixed effects. Hence, this column identifies the effect of the

13. We can also include flexible county-specific flexible trends, and the results are almost identical. We prefer province flexible trends because there are counties that consist of only one municipality.

policy by comparing the outcome of interest for the same unit before and after the policy change. Columns 4 and 5 repeat the specifications in Columns 1 and 3 but with only those municipalities that have fewer than 40,000 inhabitants. This specification should mitigate, to some extent, concerns about the treatment and control groups being markedly different, as explained above. Standard errors reported in Table II and in the rest of the paper are always clustered at the municipality level.

Panel A presents the results for the specification on the excess of prices. All of the specifications show that rent caps reduce the excess of rental prices. Excess prices decrease by between 4% and 8%. Several things are worth noting. First, when we include flexible province trends, the point estimate is only slightly less than in our baseline specification, suggesting that differential trends across provinces are not particularly pronounced. The point estimate is somewhat smaller (in absolute terms) when restricting the sample to comparable municipalities (at least in terms of size). This outcome can be explained by somewhat differential trends between Barcelona and the other smaller municipalities in our sample, or, most likely, by a smaller effect of the policy in smaller municipalities. It is interesting to explore how point estimates change once we include unit fixed effects. When we compare Columns 1 and 2 to Column 3, we see that the point estimate increases somewhat in absolute terms. This outcome indicates that the policy change seems to have a disproportionate effect on the units that stay in the market.

Panel B shows the same specifications as Panel A but using rental prices as the outcome variable.[14] Estimates fluctuate between -0.08 and -0.037.[15] Moving from Column 1 to Column 2 shows that differential province-specific trends are a larger concern when studying rental prices than deviations of those from the index price, as one could likely expect. Moving to Column 3, we again observe that the point estimate is substantially larger when conditioned on unit fixed effects. Panel C shows another way to look at the data. In this panel, for each municipality-month we compute the fraction of housing units that is greater than the index price. The estimates corroborate the evidence presented in Panels A and B. The decline in the fraction of units greater than the index price, which, prior to the policy was around 50%, fluctuates between 13 and 22 percentage points – both statistically significantly different than 0 and than 50% –, depending on the types of comparisons of each specification. Hence, noncompliance with the policy is estimated to be as

14. It is worth mentioning that we have a number of units in control for which reference prices are not computed, hence, we have a smaller number of observations in Panel A than in Panel B.

15. The results are also robust to using the estimator proposed in de Chaisemartin & D'Haultfoeuille (2020) (see Table A.1 in the Appendix), which delivers a main estimate of -4.3%, and other estimators that allow for dynamic effects, such as Callaway & Sant'Anna (2021). In this case, the estimation is -4.8%. Using the recently proposed synthetic diff-in-diffs estimator (Arkhangelsky et al., 2021), the estimation is -4.1%. All the coefficients are statistically significant. In the latter case, if the synthetic group is constructed to consider the treatment in the city of Barcelona, the estimator is not statistically significant.

high as 74% $((50-13)/50)$.

## *IVB. The effect of rent caps on the supply of rental units*

The previous section shows that the policy change led to a decline in rental prices, although likely smaller than intended, since a substantial fraction of new rentals still charged prices greater than the reference price. As mentioned before, past, mainly theoretical, papers have expressed concerns about these types of policies for at least a few reasons. First, rental price caps can affect the overall supply of housing units in the market. At the binding price cap, some homeowners might decide not to rent their housing units. Second, rental price caps can also affect the composition of units available in the rental market. We empirically investigate whether there is any evidence for either of these two side effects.

### IVB..1 Overall supply of rental units

We start our empirical investigation of the effect of the policy on the overall supply of housing by running, following equations (1) and (2), an OLS and a Poisson TWFE model, using a pseudo-maximum likelihood algorithm for multiple high-dimensional fixed effects. Our dependent variable is the (log) number of new contracts signed in each municipality, each month. Hence, this measure of volume needs to be interpreted as a flow measure. It is worth noting that when we use OLS, we miss roughly 50%t of the observations: the municipality - month pairs with zero new contracts. PPML estimates take into account this potential concern by including the zeros in the estimation.[16]

Figure VI shows the dynamic difference-in-differences specifications, where we omit the three covid lockdown months when the market almost disappeared, as can be seen in Appendix Figure A.1, and where we remove municipality-specific linear time trends and seasonality.[17] Figure VI shows a remarkable drop in the overall volume of transactions right after the implementation of the policy. Moreover, the drop is potentially substantial: estimates prior to the policy change fluctuate around 0, while the point estimates after the policy drop to almost -20%.

In Figure VI we also display the volume of contracts for rentals below and above the reference prices. The number of new contracts fluctuates around zero during the months prior to the policy. This is so when we allow for municipality linear time trends, otherwise there is a small upward trend in treatment municipalities relative to control. After the policy is implemented, however, we observe a very substantial drop of around

16. Montalvo (1997) discussed the use of pseudo-maximum estimation procedures for dynamic count data models.

17. As we will see later, none of the results depend on these choices, but graphs are easier to read when not showing these three months, and when allowing for linear time trends and seasonality.

50%. In contrast, the series for the new contracts below reference prices experience a substantial increase with the policy in treated relative to control municipalities. This increase is smaller and hence does not compensate the drop in the volume of new contracts above the reference prices.

We quantify these estimates in Table III, using OLS (in Panel A) and PPML (in Panel B). The effect of rent caps on the number of new contracts is negative and statistically significant. The reduction is large for contracts above the reference price. There is also a significant increase in contracts below the reference price, but this effect does not compensate for the decrease in contracts above it. The estimates are stable to the inclusion of flexible province trends and to restricting the estimation sample to municipalities with fewer than 40,000 inhabitants. When comparing across panels, we see that taking the 0s, in Panel B, into account reduces the point estimates somewhat, but the estimates are still precise and always statistically different than zero.

Using the results reported so far, we can obtain estimates of the elasticity of new contracts with respect to price changes. We show these results in Table IV. Panel A of Table IV displays the "naive" least square estimates, for various specifications and sub-periods. In general, however, when we regress the (log) number of new contracts on (log) rental prices we obtain small point estimates. The small estimates likely reflect the standard endogeneity problem when estimating demand or supply curves. For example, when demand increases, the supply may react and increase as well, resulting in small equilibrium price changes. Our setting, however, allows us to estimate the supply of rental units by exploiting the policy change. This is, the policy changes the price of housing. By tracking how this change affects the overall number of new contracts, we can estimate the rental market housing supply curve.

Panel B of Table IV presents the IV estimates. Given the results shown so far, it should not be surprising to see that we obtain a positive estimate when using the policy change as an instrumental variables strategy. In the baseline specification with the full sample, a decrease of 1% in rental prices implies a decrease of around 2% in the supply of rental housing. We obtain a similar point estimate when we allow for flexible province-specific time trends (Column 2) and, when we restrict the sample to municipalities with fewer than 40,000 inhabitants (Column 3). In Columns 4 to 6, we present estimates of this key elasticity when removing the lockdown period from the sample. Point estimates are, if anything, slightly larger than in the full sample, reaching an estimate of approximately three.

Overall, the evidence presented in this section suggests that the supply of housing units reacts to price changes in the market; i.e., it suggests that the supply of housing units in the rental market is upward sloping.

### IVB..2 Distributional changes in the supply of housing

While the previous section suggests that the supply of rental units is upward sloping, it does not tell us as much about how the overall distribution of housing units in the market changes with the rental cap policy. We perform two additional exercises to investigate this point in more detail.

First, we take all the contracts in our data and group them by sextiles of "excess price" within treated and control municipalities, once we have removed municipality and time fixed effects. This creates six *equally* sized groups of housing units for both the treated and control municipalities as a function of its price relative to the reference price. The municipality and time fixed effects make these prices comparable across time and space.

Once we have these six groups, or "sextiles", which summarize the entire distribution of excess prices, we plot in Figure VII the change in prices and quantities, pre- to post-policy change, for each of these six groups. The price change results are shown in the graph on the left of the figure. The results suggest something that we could have perhaps anticipated from the histograms shown in Figure III: prices in the control municipalities increase uniformly along the entire distribution, by around 7%. In contrast, there is a clear gradient in rental price changes along the distribution in the treatment municipalities. Prices decline at the top of the distribution by as much as 4% in absolute terms and by close to 10% when we compare them to the control group. In contrast, prices increase above the control group at the bottom of the distribution. This suggests that the policy managed to compress the prices towards the reference price, perhaps making the reference price a "focal" point.

On the right-hand graph of Figure VII, we investigate whether the policy induced changes in the overall number of monthly contracts along the distribution. The figure suggests a movement towards higher priced units in control municipalities. This movement is also observable in treatment municipalities up until the third sextile. After this, i.e., at the top of the distribution, we see a clear decline in the number of new contracts signed. Since all the groups are roughly similar, we can add up the different estimates to check that the overall supply in treatment municipalities declined relative to control ones. This graph shows very clearly the missing mass of new contracts that we estimated in the previous section above and below the reference price (see Figure VI and Table III). These results indicate that high-price units "disappear from the market" in treated relative to control municipalities.

Overall, the evidence in this section suggests that the introduction of reference prices becomes a focal point. Some units above this focal point disappear from the market, and those that stay, experience significant price drops. Moreover, in treated municipalities we see a convergence of prices towards reference prices.

# V Model

Up to this point, we have presented the empirical evidence on how the policy affected equilibrium prices and supply. We have shown that both prices and supply declined with the policy. At the same time, we have documented interesting patterns along the entire distribution of both prices and supply. Prices converged to reference prices, whereas supply declined mainly as a result of changes at the top of the distribution.

To think about the welfare consequences of this policy, we need some theoretical framework. From the perspective of a standard neoclassical perfect competition model, introducing a binding price cap has several consequences. A cap on prices generates a gap between the amount of units that potential tenants would demand and how much unit owners would be willing to offer in the rental market. As a result, at the price cap some potential renters would want to rent, but the supply does not satisfy this demand. Moreover, as long as the supply of rental units is not perfectly inelastic, there is a deadweight loss associated with the policy. Hence, a rental price cap policy can be understood as a trade-off between efficiency and redistribution from unit owners to renters.[18]

While the standard neoclassical perfect competition model may provide a useful benchmark, there are several aspects of the rental market and of the evidence presented above that do not fit well with the framework. First, we see substantial heterogeneity in prices across units in the market, whereas in the neoclassical framework there is a unique price. Second, the results indicate interesting effects of the policy along the entire distribution, both in terms of price and supply changes. The neoclassical framework is ill-suited to speak to these findings, since again, there is a unique price in the market.

For these and potentially other reasons, it may be useful to depart from the neoclassical benchmark. When people search for rental units, it is standard to observe the prices posted online. It is also standard to look for several options: low price units that are attractive but where likelihood of getting them is probably low, and some others at higher prices, which may serve as backup options. Existing websites make the search simpler, in that posted prices are easily observable, although then one needs to actually find and meet the particular owners of the unit of interest.

This search process resonates strongly with the assumptions behind directed search models used mainly to study labor markets. Hence, in this section, we develop a directed search model to study how rental price caps affect the rental market. The model is inspired by the many insights in Galenianos & Kircher (2009) and Wright et al. (2021) but adapted to the rental market. We study the model under two alternative assumptions: exogenous number of rental units in the market and free entry. These two extremes encapsulate

18. We show a graphical representation of the perfect competition model in Appendix AB.. The graph shows the initial equilibrium $\{p^0, Q^0\}$ and how a cap on price moves the equilibrium to $\{\underline{p}, \underline{Q}\}$, leaving an unsatisfied demand from $\underline{Q}$ to $Q^D$.

the main insights that we obtain from this framework, and help us to understand the welfare implications of the empirical facts documented.

It is worth noting that in the model we abstract from modeling explicitly unobserved characteristics that can explain the price dispersion observed in the data. Rekkas et al. (2021) show that unobserved heterogeneity does not change the fundamental insights of directed search models. Moreover, a posted "high price" can always be interpreted as a landlord who values (unobservable) characteristics above the market, and is ready to wait longer to find someone who shares this view, and conversely for low-price posting.

## VA. Model set-up

There are two types of agents in the model: unit owners and tenants. Following the terminology in the directed search literature, we denote by "sellers" the owners of rental units, and by "buyers" the potential tenants. Sellers post (and commit to) a single price. Posted prices are observed by buyers, who can apply to many rental units. There is a mass $N_b$ of buyers and a mass $N_s$ of sellers. We denote by $N$ the ratio of buyers to sellers. All buyers and sellers are ex-ante homogeneous.

We denote by $\alpha(n)$ the probability that a mass $n_s$ seller meets a mass $n_b$ of buyers for a posted rental price $p$, with $n = n_b/n_s$ – note that this may or may not coincide with $N$, depending on whether whether multiple prices are posted in equilibrium or not. This ratio is also referred to in the literature as market tightness. We denote the value that renters derive from renting a unit by $u$ and the cost that sellers incur when having a unit in the rental market by $c$. Hence, the payoff for buyers to rent at price $p$ is given by $u-p$, while the payoff for sellers is $p - c$. We assume $\alpha(n)$ is increasing in $n$. It is worth noting that $\frac{\alpha(n)}{n}$ is the probability that a buyer meets a seller. We denote by $\varepsilon(n) = \frac{n\alpha'(n)}{\alpha(n)}$ the elasticity of the meeting probability with respect to the ratio of buyers to sellers.

## VB. Multiple sub-markets

We assume that buyers apply to two different types of units. Sellers, instead, can only post one price. We can denote by $\underline{p}$ and $\overline{p}$ the prices associated with each of the different sub-markets and by $\underline{n}$ and $\overline{n}$ the ratio of buyers to sellers in each sub-market.[19]

With multiple sub-markets, the value of the buyers can be expressed as:

19. Galenianos & Kircher (2009) show that there are as many sub-markets as buyer applications. We study the case of two applications to keep the algebra simple, but the insights carry over to the case of an arbitrary number of sub-markets.

(3) $$V_b = \frac{\alpha(\underline{n})}{\underline{n}}(u - \underline{p}) + (1 - \frac{\alpha(\underline{n})}{\underline{n}})\frac{\alpha(\overline{n})}{\overline{n}}(u - \overline{p})$$

This expression says that with probability $\frac{\alpha(\underline{n})}{\underline{n}}$, the buyer meets a low-price seller, in which case she accepts the price and derives utility $(u - \underline{p})$. With probability $(1 - \frac{\alpha(\underline{n})}{\underline{n}})$, she does not meet a low-price unit, and hence is "forced" to consider high-price options. In the high-price market, with probability $\frac{\alpha(\overline{n})}{\overline{n}}$, she meets a high-price seller and derives utility $(u - \overline{p})$. With probability $(1 - \frac{\alpha(\underline{n})}{\underline{n}})(1 - \frac{\alpha(\overline{n})}{\overline{n}})$ she does not meet either a low- or a high-price seller, and hence obtains her outside option, which we assume to be equal to 0.

With multiple sub-markets, we can start the analysis by looking into the optimization problem in the high-price sub-market. This maximization problem can be written as follows:

$$V_{\overline{s}} = \max_{\overline{n},\overline{p}}\{(\alpha(\overline{n})(1 - \frac{\alpha(\underline{n})}{\underline{n}})(\overline{p} - c))\} \text{ subject to } \frac{\alpha(\underline{n})}{\underline{n}}(u - \underline{p}) + (1 - \frac{\alpha(\underline{n})}{\underline{n}})\frac{\alpha(\overline{n})}{\overline{n}}(u - \overline{p}) = V_b$$

This equation says that sellers in the high-price market meet a buyer who did not find a low-price unit with probability $\alpha(\overline{n})(1 - \frac{\alpha(\underline{n})}{\underline{n}})$, in which case they obtain a payoff equal to $(\overline{p} - c)$.

We can solve this maximization problem by substituting the constraint into the objective function and taking first-order conditions. Doing so, we obtain that:

(4) $$\overline{p} = (1 - \varepsilon(\overline{n}))u + \varepsilon(\overline{n})c$$

Equation (4) is easy to interpret. It says that sellers who decide to post in the high-price sub-market, post a price that is a weighted average between their cost of posting and buyers' utility.

Using the equilibrium price, we obtain that the sellers' value to posting in the high-price sub-market is given by:

(5) $$V_{\overline{s}} = \alpha(\overline{n})\left(1 - \frac{\alpha(\underline{n})}{\underline{n}}\right)(1 - \varepsilon(\overline{n}))(u - c)$$

We can now turn to the "low-price" sub-market. Sellers in the low-price market also need to decide what price to post given the demand they face. This can be written as:

$$V_{\underline{s}} = \max_{\underline{n},\underline{p}}\{\alpha(\underline{n})(\underline{p} - c)\} \text{ subject to } \frac{\alpha(\underline{n})}{\underline{n}}(u - \underline{p}) + (1 - \frac{\alpha(\underline{n})}{\underline{n}})\frac{\alpha(\overline{n})}{\overline{n}}(u - \overline{p}) = V_b$$

This equation says that sellers in the low-price market meet a buyer with probability $\alpha(\underline{n})$, in which case, given that buyers always prefer low prices, they match, and hence, sellers obtain a payoff equal to $(\underline{p} - c)$.

We can also solve this maximization problem by substituting the constraint into the objective function and taking first-order conditions. When we do so, we obtain:

$$\underline{p} = (1 - \varepsilon(\underline{n}))\underline{u} + \varepsilon(\underline{n})c \tag{6}$$

where $\underline{u} = u - \frac{\alpha(\overline{n})}{\overline{n}}(u - \overline{p})$.

Equation (6) is also easy to interpret. It says that sellers who decide to post in the low-price sub-market post a price that is a weighted average between their cost of posting and buyers' utility, in this case, relative to the one obtained in the high-price sub-market.

As before, we can obtain the payoff of posting in the low-price sub-market by introducing the equilibrium price into the objective function:

$$V_{\underline{s}} = \alpha(\underline{n})(1 - \varepsilon(\underline{n}))(\underline{u} - c) \tag{7}$$

## *VC. Exogenous number of sellers vs. free entry*

We consider two alternative solutions to this model. First, we explain how to solve the model when we assume an exogenous overall number of sellers. Second, we consider the case when there is free entry.

**Exogenous number of sellers**

With an exogenous number of sellers, it must be the case that sellers are indifferent between posting in the high- and the low-price sub-markets. In that case, we solve the model by considering the following indifference condition, which results from equating Equations (5) and (7):

$$\alpha(\underline{n})(1 - \varepsilon(\underline{n}))(\underline{u} - c) = \alpha(\overline{n})\left(1 - \frac{\alpha(\underline{n})}{\underline{n}}\right)(1 - \varepsilon(\overline{n})) \tag{8}$$

Moreover, with an exogenous number of sellers, it must also be the case that:

$$\frac{1}{\underline{n}} + \frac{1}{\overline{n}} = \frac{\underline{n}_s}{N_b} + \frac{\overline{n}_s}{N_b} = \frac{N_s}{N_b} = \frac{1}{N} \tag{9}$$

We can now state the following proposition, which we prove with the, from now on maintained, simplifying assumption that $\alpha(n)$ is obtained from a Cobb-Douglass matching function:[20]

20. Not surprisingly, proofs of the main propositions are sometimes simpler when we assume that $\alpha(n) = \mu(n, 1)/n =$

PROPOSITION 1. Equations (8) and (9) define a system of two equations and two unknowns, $\underline{n}$ and $\overline{n}$, that has a unique solution.

*Proof.*See Appendix BA..1 for a detailed proof. Sketch of the proof: We show that Equation (8) (resp. Equation (9)) defines a decreasing function (resp. increasing) of $\underline{n}$ as a function of $\overline{n}$. Hence the two equations must cross in the space $\underline{n}$, $\overline{n}$. ■

With a solution to this system, we can then obtain all the endogenous variables of the model, such as prices in each sub-market, sellers' and buyers' payoffs.

**Free entry**

An alternative assumption is to allow free entry in the rental market. One way to model this is to assume that there is an outside option to owners, for instance to sell the unit they have, with payoff $k_s(N_s)$. This payoff may be a function that depends on the number of sellers ($N_s$) in the rental market. This captures the idea that, when the rental market is attractive, even owners for whom it may be very costly to offer their unit in the rental market may consider doing so. To capture this, we assume that $k_s(N_s) > 0, \forall N_s \in [0, \infty)$ and, that $k_s(N_s)$ is (weakly) increasing. In this case, the indifference condition between posting in the high- and in the low-price sub-markets needs to be equal to the outside option $k_s(N_s)$:

$$\alpha(\underline{n})(1-\varepsilon(\underline{n}))(\underline{u}-c) = \alpha(\overline{n})\left(1-\frac{\alpha(\underline{n})}{\underline{n}}\right)(1-\varepsilon(\overline{n}))(u-c) = k_s(N_s) \tag{10}$$

We can now state the following proposition.

PROPOSITION 2. Equation (10) defines a system of two equations and two unknowns, $\underline{n}$ and $\overline{n}$, that has a unique solution.

*Proof.*See Appendix BA..2 for a detailed proof. Sketch of the proof: The procedure is almost identical to Proposition 1, except that the second equation is replaced by a non-decreasing free entry condition. ■

As before, with a solution to this system, we can then obtain all the endogenous variables of the model, such as prices in each sub-market, sellers' and buyers' payoffs.

$\mu(n_s, n_b)/n_s$, with $\mu(n_s, n_b) = n_s^{\theta} n_b^{1-\theta}$. While we have not formally proved it, we expect all the results to hold for arbitrary constant return to scale $\mu(.,.)$ functions.

## *VD. Welfare implications of a reference price rule*

In this section, we turn to study the implications of introducing a reference price rule, similar, in spirit, to the one introduced in Catalonia in late September 2020, in the context of our model. We study the implications of this policy both when we assume an exogenous number of sellers and when we assume free entry. In principle, the free entry scenario could capture best outcomes in the long term, although in the real world this "long term" can come around quickly, if it is easy for owners to switch to options other than the rental market.

PROPOSITION 3.

1. A policy that forces a unique price $P \in [c, u]$ results in the following:
   (a) Prices converge to $P$.
   (b) The policy increases total welfare.
   (c) The policy has the following consequences for buyers:
   - if $P > \underline{p}$ welfare of all buyers decreases
   - if $\underline{p} < P < \overline{p}$ welfare of high-price buyers increases, while that of low-price ones decreases
   - if $P < \underline{p}$ welfare of all buyers increase

   (d) Absent transfers, the policy has the following consequences for sellers:
   - if $P > \overline{p}$ welfare of sellers increases
   - There $\exists P^* < \overline{p}$ such that if $P < P^*$ welfare of sellers declines, and some sellers leave the market
2. A policy that forces a unique price $P \notin [c, u]$ destroys the rental market
3. A policy that forces a unique market, decided by the market, is efficient, but beneficial to sellers, since it leads the market to converge to $P^N = (1 - \varepsilon(N))u + \varepsilon(N)c$, which with Cobb-Douglass matching is equal to $\overline{p}$.

*Proof.*See Appendix BA..3, for a detailed proof. ■

This proposition has several implications. When the number of sellers is exogenous, forcing the market to converge to a single price is "on average" beneficial. The policy effectively destroys the multiplicity of sub-markets and this is a good thing because neither buyers nor sellers internalize the fact that buyers who end up in the high-price market also apply to the low-price market, which has the externality of increasing the "queue length" in that market (Galenianos & Kircher, 2009). In that sense, the policy is analogous to forcing buyers to only apply to one type of rental unit. This means that, if lump-sum transfers between the different agents are allowed, the policy leads to a constrained Pareto Optimal. It is worth emphasizing, however, that this result depends critically on assuming an exogenous number of sellers.

Free entry erodes the policy's effectiveness. This is so because the policy hurts sellers. If allowed to, sellers would like to leave the market, decreasing the overall supply of units in the market and, hence, increasing competition among buyers, albeit at a unique price, rather than in two separate sub-markets. As a result of increased competition among buyers, with free entry the average price can be higher than with a fixed number of sellers, something that is in line with the dynamics documented in Breidenbach et al. (2022) using German data.

In either case, the model has substantially richer predictions on welfare than the perfect competition model. Welfare of homeowners declines. This is so because the policy removes the option to post high prices. Unlike the perfect competition model, welfare among renters is not uniformly improved by the policy change. Without the policy, there are some "lucky" renters that mange to be in a low-price unit, which makes them, ex-post, have high levels of welfare. The policy hurts these renters in low-price units. In contrast, there are also renters in high-price units who gain substantially from the policy change.

# VI Conclusions

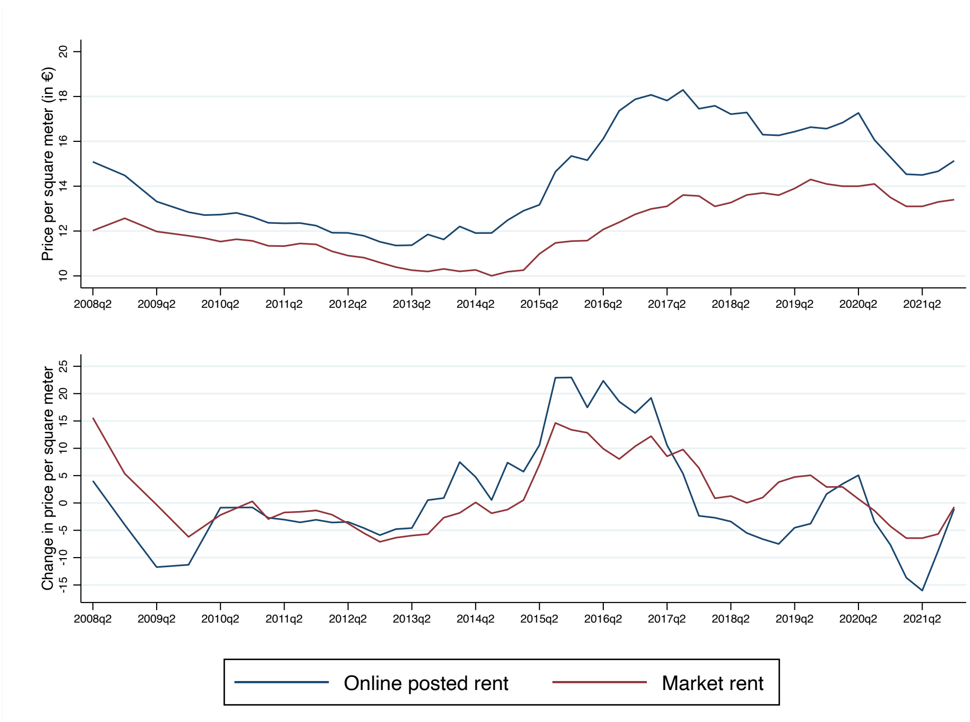
In this paper, we analyze the impact of the second-generation rent control enacted in Catalonia in September 2020. We find that the introduction of rental price controls led to a reduction in both prices and the supply of rental units. We estimate that the policy change led to price reductions of around 5%, and that the fraction of units above the reference price, which was around 50% prior to the policy, was reduced by between 13 and 22 percentage points. This evidence suggests that the policy was effective in reducing rental prices, but also that there was a high degree of non-compliance.

The supply of rental units in the market also changed as a consequence of the policy. We estimate that the overall supply of rental units declined by around 15 percent. This number masks substantial shifts in the composition of rental units in the market. We document a very pronounced decline in the units above the reference price that was not compensated by an increase in units below.

Finally, we argue that a directed search model is useful to interpret the evidence. The model suggests that the policy change introduced in Catalonia in late September 2020, may have had negative welfare consequences for homeowners will to rent their units and for renters in low-price units, while it is beneficial to renters in high-price units. Moreover, when supply is sufficiently inelastic, the policy may increase efficiency.

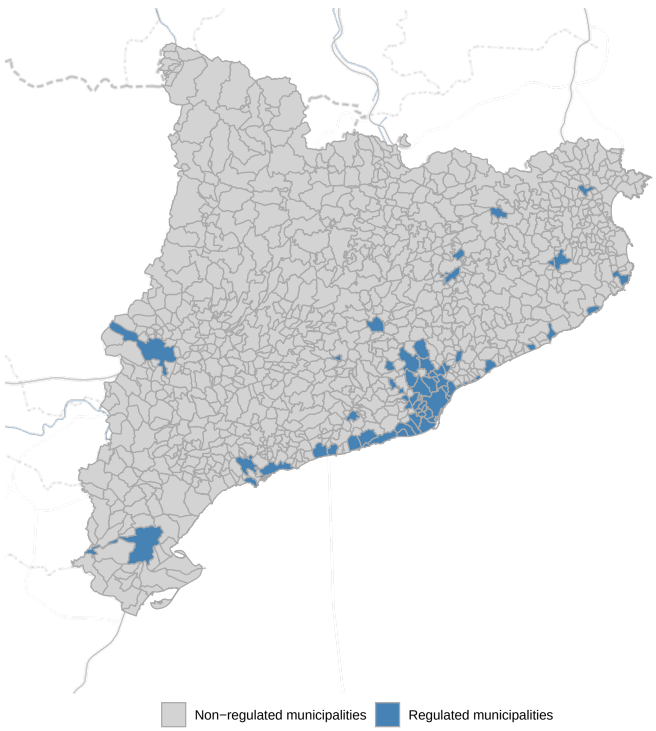
# VII Figures

Figure I: Comparison between online posted prices and market prices



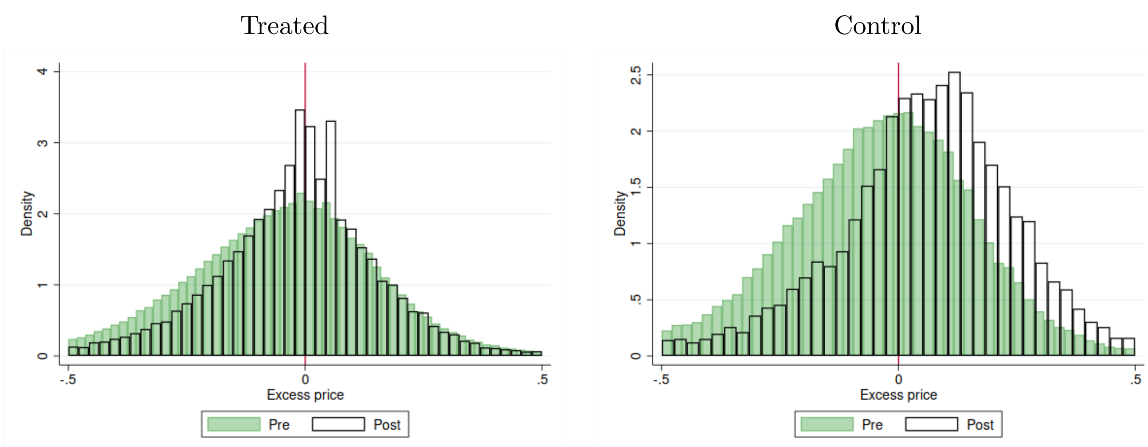

Notes: The graph above (below) shows the level (growth rate) of rental market prices and online posted prices. Sources: INCASOL and Idealista.

Figure II: Map of municipalities affected by rental price caps




Notes: These maps show treated and control municipalities. Treated municipalities are defined as municipalities that were subject to a price cap.

Figure III: Histograms of excess prices before and after rent caps

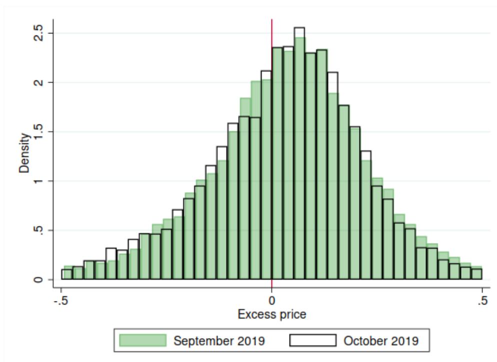



Notes: These graphs show the histograms of 'excess price' in treatment and control municipalities. 'Excess price' is defined as the log difference between the rental price and the index calculated for that unit.

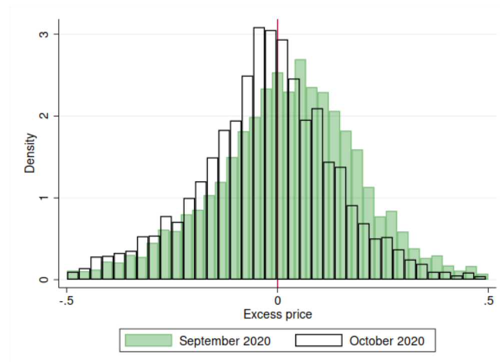
Figure IV: Histograms of excess prices around the policy change date versus the previous year

**Panel A:** Treated municipalities

September/October 2019

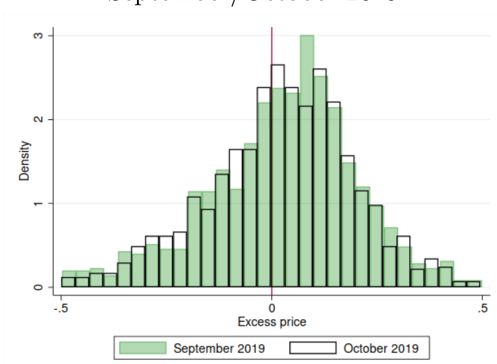



September/October 2020

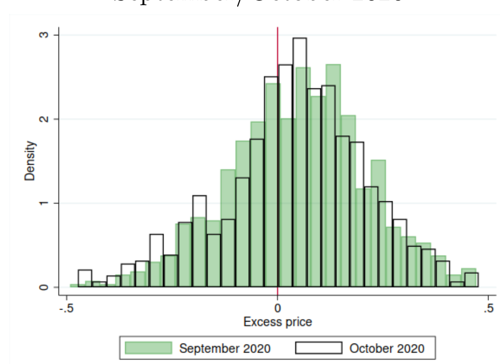



**Panel B:** Control municipalities

September/October 2019




September/October 2020

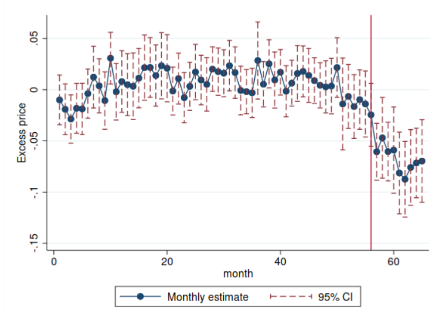



Notes: These graphs show the histograms of 'Excess price' in treatment and control municipalities during selected months around the policy change and in the preceding year. 'Excess price' is defined as the log difference between the rental price and the index calculated for that unit.

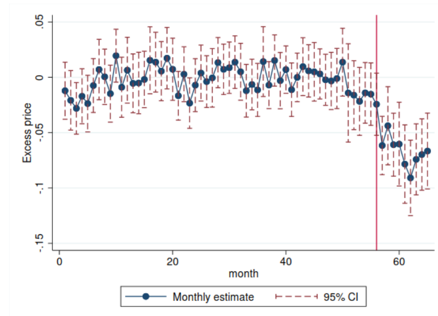
Figure V: Dynamic estimates of the effect of the policy on rental prices

**Panel A:** Outcome variable: Excess price

Baseline

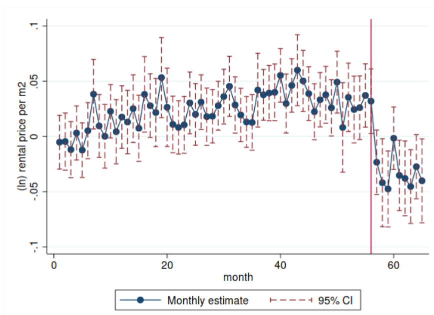



Controlling for province trends

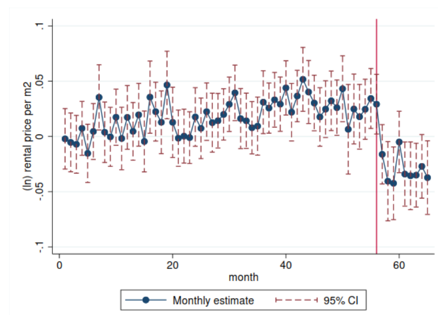



**Panel B:** Outcome variable: (ln) rental price per m2

Baseline

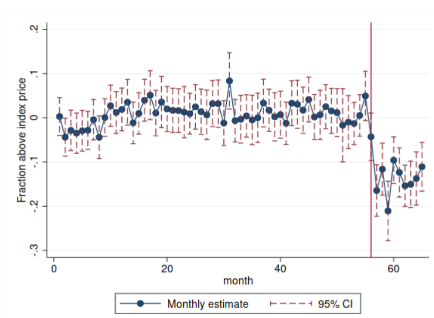



Controlling for province trends

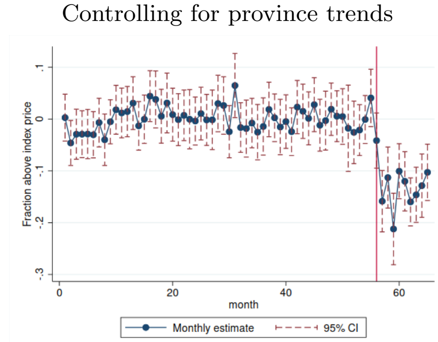



**Panel C:** Outcome variable: Fraction above the index price

Baseline

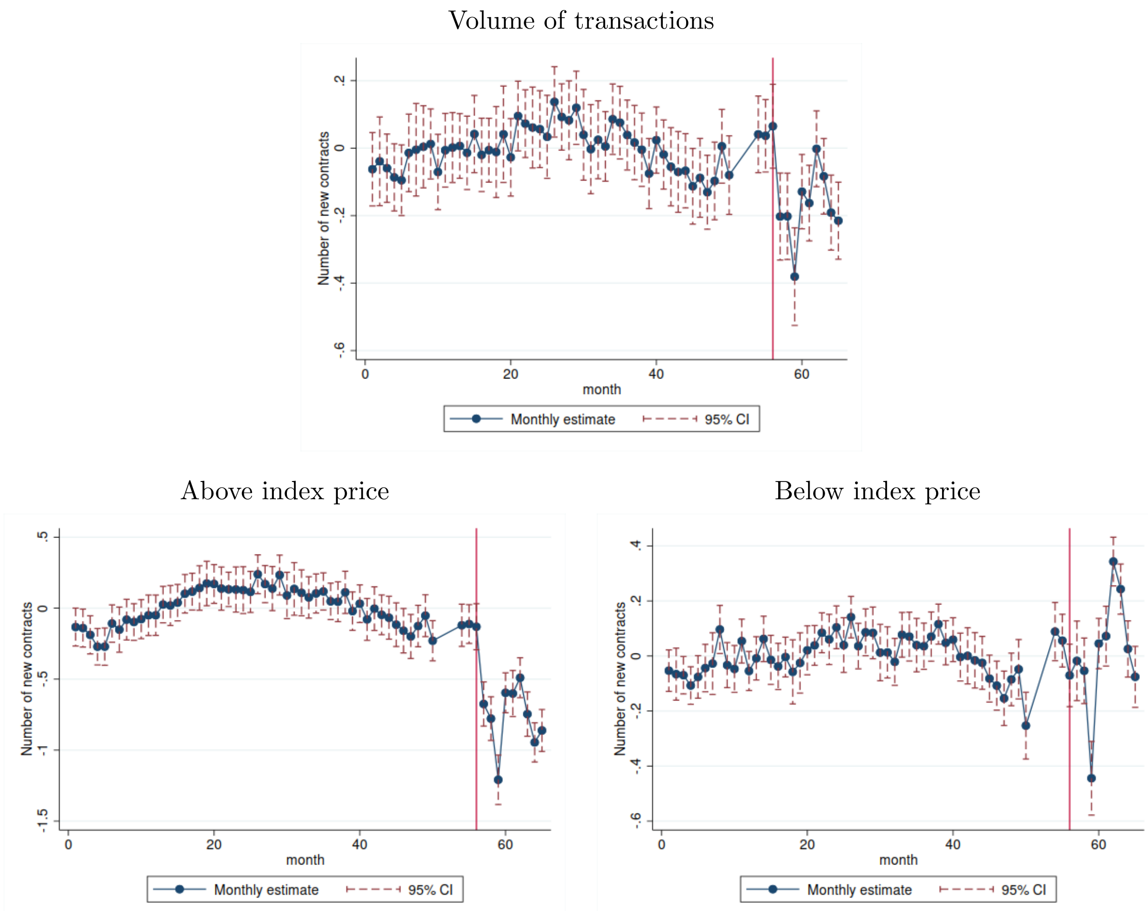



Controlling for province trends



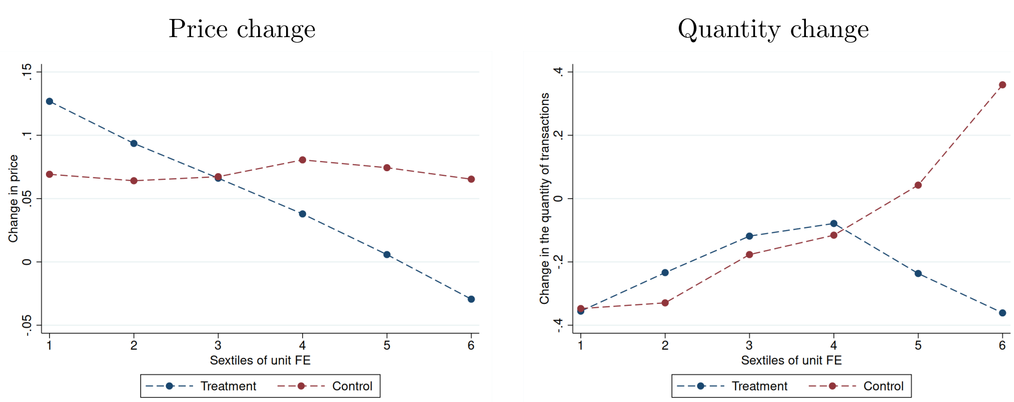

Notes: This figure shows two types of graphs and three different outcome variables. The graphs on the left show our baseline specification, while the graphs on the right show our baseline specification controlling for flexible province-specific time trends. Ninety-five percent confidence intervals of robust standard errors clustered at the municipality level are reported.

Figure VI: Dynamic estimates of the effect of the policy on the volume of transactions




Notes: This figures shows graphs of the (log) number of the overall number of transactions, and the overall (log) number above and below the reference price in the treatment relative to the control municipalities, using our baseline specification controlling by flexible province-specific time trends and allowing for municipality-specific linear time trends estimated prior to the policy. Ninety-five percent confidence intervals of robust standard errors clustered at the municipality level are reported.

Figure VII: PRICE AND QUANTITY CHANGES BY SEXTILE

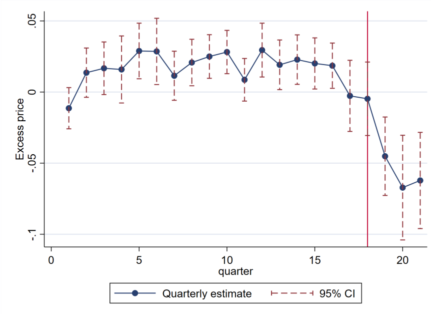



Notes: The figure shows the change in prices and quantity of monthly new contracts, between the pre- and the post-period for the entire sample of units. Sextiles are define six equally sized groups within treatment and control municipalities.

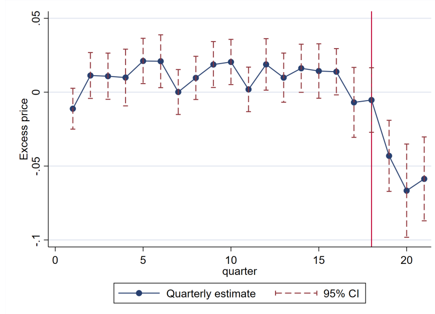
Figure VIII: Dynamic estimates of the effect of the policy on rental prices - Data grouped at quarterly frequency

**Panel A:** Outcome variable: Excess price

Baseline

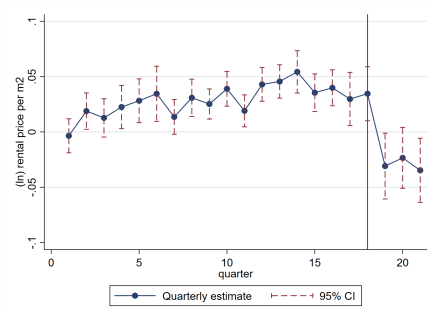



Controlling for province trends

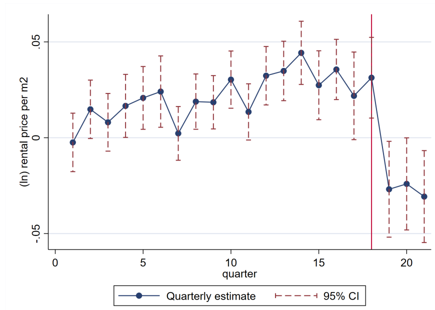



**Panel B:** Outcome variable: (ln) rental price per m2

Baseline

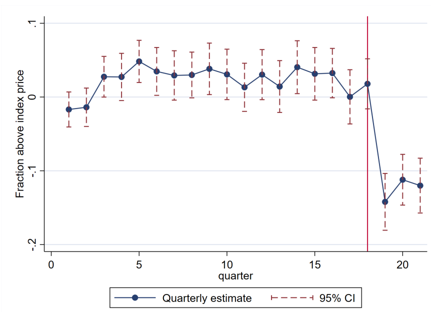



Controlling for province trends

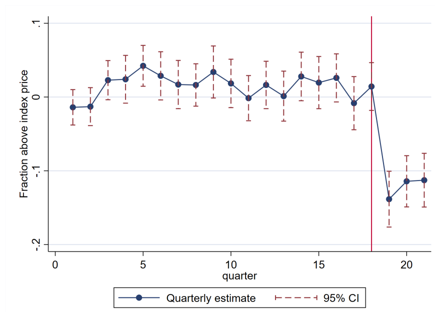



**Panel C:** Outcome variable: Fraction above the index price

Baseline

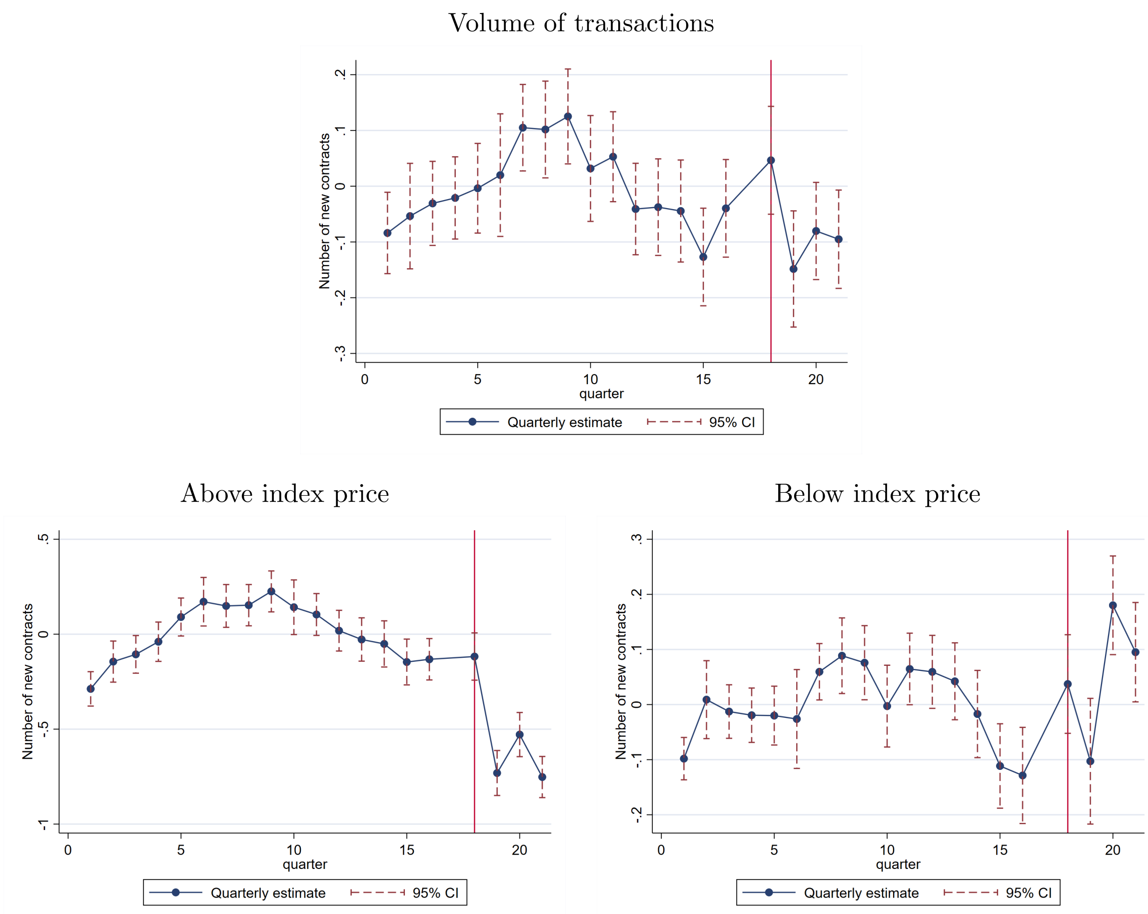



Controlling for province trends



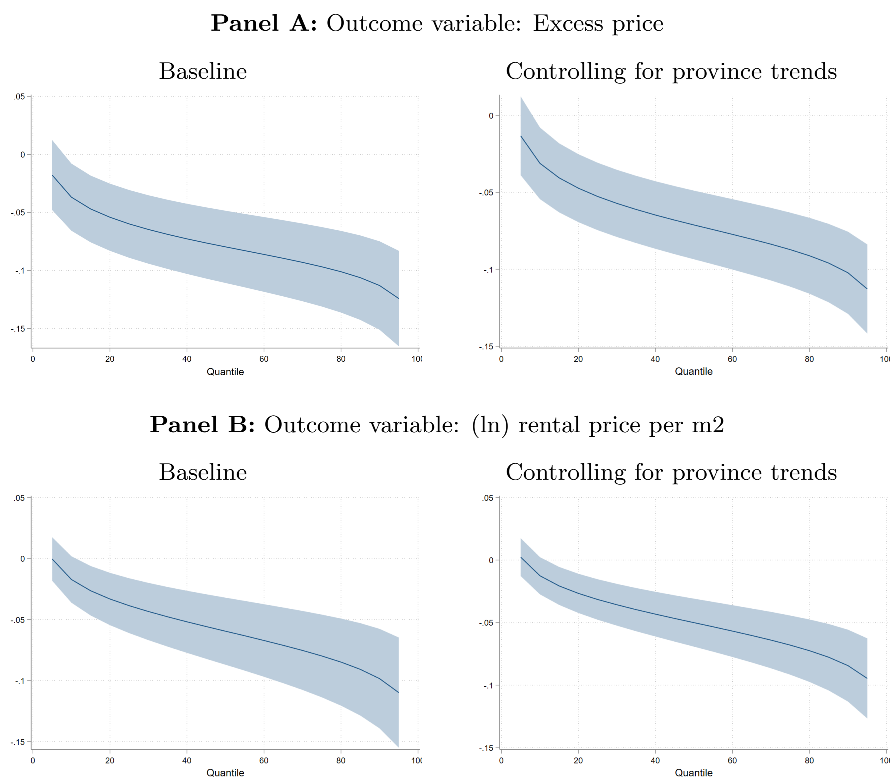

Notes: This figure shows two types of graphs and three different outcome variables. The graphs on the left show our baseline specification, while the graphs on the right show our baseline specification controlling for flexible province-specific time trends. Ninety-five percent confidence intervals of robust standard errors clustered at the municipality level are reported.

Figure IX: Dynamic estimates of the effect of the policy on the volume of transactions - Data grouped at quarterly frequency




Notes: This figures shows graphs of the (log) number of the overall number of transactions, and the overall (log) number above and below the reference price in the treatment relative to the control municipalities, using our baseline specification controlling by flexible province-specific time trends and allowing for municipality-specific linear time trends estimated prior to the policy. Ninety-five percent confidence intervals of robust standard errors clustered at the municipality level are reported.

Figure X: Point estimates of the effect of the policy on rental prices as quantile varies




Notes: This figure shows two types of graphs and two different outcome variables. The graphs on the left show our baseline specification, while the graphs on the right show our baseline specification controlling for flexible province-specific time trends. Ninety-five percent confidence intervals of robust standard errors clustered at the municipality level are reported.

# VIII Tables

Table I: Descriptive statistics

| | **Regulated municipalities** | | | **Non-regulated municipalities** | | |
|---|---|---|---|---|---|---|
| | Mean | Std. Dev. | Obs. | Mean | Std. Dev. | Obs. |
| Rent (€/month) | 745.44 | 355.92 | 474,477 | 525.41 | 236.10 | 118,758 |
| Square meters | 64.55 | 32.14 | 474,477 | 71.82 | 31.41 | 118,758 |
| Price per sq.meter | 12.20 | 5.14 | 474,477 | 7.75 | 3.26 | 118,758 |

Notes: This table shows summary statistics for treated and non-treated municipalities. Sources: INCASOL and ACH.

Table II: Effect of rent cap on rental prices

**Panel A**

| | Excess price | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Effect of Rent Cap | -0.076 | -0.068 | -0.082 | -0.042 | -0.067 | -0.081 |
| | (0.016) | (0.012) | (0.015) | (0.010) | (0.012) | (0.015) |
| Observations | 523954 | 523844 | 257461 | 112763 | 50857 | 257461 |
| Fixed_Effects | yes | yes | yes | yes | yes | yes |
| Province_trends | no | yes | yes | no | no | yes |
| Unit_FE | no | no | yes | no | yes | yes |
| Condition | no | no | no | <40k | <40k | no |
| Quarterly_freq | no | no | no | no | no | yes |

**Panel B**

| | (ln) Price | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Effect of Rent Cap | -0.058 | -0.048 | -0.080 | -0.037 | -0.062 | -0.080 |
| | (0.014) | (0.010) | (0.014) | (0.011) | (0.012) | (0.014) |
| Observations | 591618 | 591568 | 286899 | 164337 | 72193 | 286899 |
| Fixed_Effects | yes | yes | yes | yes | yes | yes |
| Province_trends | no | yes | yes | no | no | yes |
| Unit_FE | no | no | yes | no | yes | yes |
| Condition | no | no | no | <40k | <40k | no |
| Quarterly_freq | no | no | no | no | no | yes |

**Panel C**

| | Fraction above index | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Effect of Rent Cap | -0.146 | -0.135 | -0.186 | -0.160 | -0.216 | -0.186 |
| | (0.012) | (0.011) | (0.015) | (0.025) | (0.033) | (0.015) |
| Observations | 106149 | 106149 | 26957 | 59264 | 8911 | 26957 |
| Fixed_Effects | yes | yes | yes | yes | yes | yes |
| Province_trends | no | yes | yes | no | no | yes |
| Unit_FE | no | no | yes | no | yes | yes |
| Condition | no | no | no | <40k | <40k | no |
| Quarterly_freq | no | no | no | no | no | yes |

Notes: This table shows the difference-in-difference estimates of the effect of the policy on 'Excess price' – defined as the percentage difference between the contract price and the reference price –, the rental price, and the fraction of contracts above the reference price. Column (1) is our baseline specification. Columns (2) adds province $\times$ month fixed effects. Column (3) adds unit fixed effects. Column (4) limits our baseline specification to municipalities with fewer than 40,000 inhabitants. Column (5) adds unit fixed effects and limits the sample to municipalities with fewer than 40,000 inhabitants. In Panel C, we compute the municipality level fraction of contracts above the reference price conditioning, in Columns (3) and (5), on the sample of units with multiple contracts, and in Columns (4) and (5) on the sample of municipalities with fewer than 40,000 inhabitants. Column (6) uses the same specification as (3), but considering the data at quarterly frequency. Fixed effects include municipality and time fixed effects. Robust standard errors clustered at the municipality level are reported.

Table III: Effect of rent cap on number of new contracts signed

**Panel A:** OLS

| | Number of new contracts, OLS estimates | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | All | | | | Above | | | | Below | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Effect of Rent Cap | -0.207 | -0.181 | -0.192 | -0.093 | -0.786 | -0.740 | -0.745 | -0.660 | 0.166 | 0.146 | 0.184 | -0.660 |
| | (0.021) | (0.023) | (0.031) | (0.025) | (0.034) | (0.033) | (0.050) | (0.035) | (0.038) | (0.038) | (0.049) | (0.035) |
| Observations | 26176 | 26176 | 24267 | 26176 | 26176 | 26176 | 24267 | 26176 | 26176 | 26176 | 24267 | 26176 |
| Fixed_Effects | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Province_trends | no | yes | no | yes | no | yes | no | yes | no | yes | no | yes |
| Condition | no | no | <40k | no | no | no | <40k | no | no | no | <40k | no |
| Quarterly_freq | no | no | no | yes | no | no | no | yes | no | no | no | yes |

**Panel B:** PPML

| | Number of new contracts, PPMLE estimates | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | All | | | | Above | | | | Below | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Effect of Rent Cap | -0.113 | -0.107 | -0.065 | -0.086 | -0.561 | -0.529 | -0.441 | -0.541 | 0.236 | 0.211 | 0.260 | -0.541 |
| | (0.018) | (0.021) | (0.031) | (0.021) | (0.027) | (0.029) | (0.045) | (0.029) | (0.044) | (0.041) | (0.053) | (0.029) |
| Observations | 56007 | 56007 | 53541 | 19558 | 56007 | 56007 | 53541 | 18669 | 56007 | 56007 | 53541 | 18669 |
| Fixed_Effects | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Province_trends | no | yes | no | yes | no | yes | no | yes | no | yes | no | yes |
| Condition | no | no | <40k | no | no | no | <40k | no | no | no | <40k | no |
| Quarterly_freq | no | no | no | yes | no | no | no | yes | no | no | no | yes |

Notes: This table estimates the effect of the policy change on the volume of new contracts at the monthly-municipality level. Columns (1), (5) and (9) are our baseline specifications. Columns (2), (6), and (10) add province × month fixed effects. Columns (3), (7), and (11) limit our baseline specifications to municipalities with fewer than 40,000 people. Columns (4), (8) and (12) consider the data at quarterly frequency. All the regressions exclude the lockdown period. Fixed effects include municipality, time fixed effects, and municipality specific linear time trends. Robust standard errors clustered at the municipality level are reported.

Table IV: Estimates of the elasticity of rental market housing supply

**Panel A:** OLS estimates

| | (ln) Number of new contracts | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Full Sample | | | | No lockdown | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| (ln) Rental price | 0.068 | 0.065 | 0.070 | 0.064 | 0.068 | 0.064 | 0.069 | 0.066 |
| | (0.012) | (0.013) | (0.012) | (0.021) | (0.013) | (0.013) | (0.013) | (0.022) |
| Observations | 27302 | 27302 | 25299 | 12168 | 26176 | 26176 | 24267 | 11617 |
| Fixed_Effects | yes | yes | yes | yes | yes | yes | yes | yes |
| Province_trends | no | yes | no | yes | no | yes | no | yes |
| Condition | no | no | <40k | no | no | no | <40k | no |
| Quarterly_freq | no | no | no | yes | no | no | no | yes |

**Panel B:** IV estimates

| | (ln) Number of new contracts | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Full Sample | | | | No lockdown | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| (ln) Rental price | 1.987 | 2.046 | 2.103 | 1.605 | 3.069 | 3.195 | 3.109 | 2.206 |
| | (0.395) | (0.517) | (0.618) | (0.512) | (0.459) | (0.613) | (0.669) | (0.571) |
| Observations | 27302 | 27302 | 25299 | 12168 | 26176 | 26176 | 24267 | 11617 |
| fstat | 74.572 | 45.646 | 42.295 | 33.478 | 73.136 | 44.729 | 41.541 | 34.646 |
| Fixed_Effects | yes | yes | yes | yes | yes | yes | yes | yes |
| Province_trends | no | yes | no | yes | no | yes | no | yes |
| Condition | no | no | <40k | no | no | no | <40k | no |
| Quarterly_freq | no | no | no | yes | no | no | no | yes |

Notes: This table estimates uses the the policy change to estimate the elasticity of new contracts to price changes. Columns (1) and (5) are our baseline specifications. Columns (2) and (6) add province × month fixed effects. Columns (3) and (7) limit our baseline specifications to municipalities with fewer than 40,000 people. Columns (4) and (8) consider the data at quarterly frequency.Fixed effects include municipality, time fixed effects, and municipality specific linear time trends. Panel A presents OLS estimates, while panel B presents IV estimates based on the policy change. Robust standard errors clustered at the municipality level are reported.

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FOR ONLINE PUBLICATION

# A Empirical Appendix

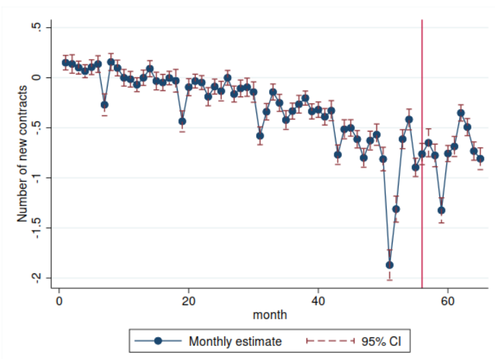
## *AA. Additional evidence*

### AA..1 Volume of new contracts

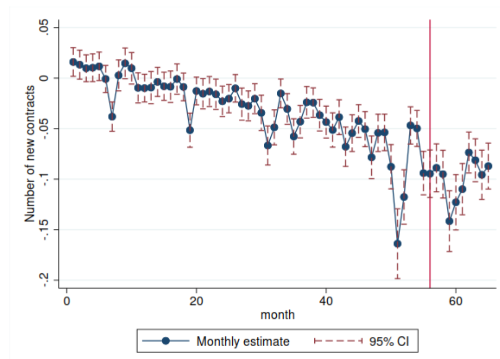
Figure A.1: Event graphs, second stage, separate treatment and control.

Panel A: Outcome variable: Volume of transactions below the threshold

Treatment

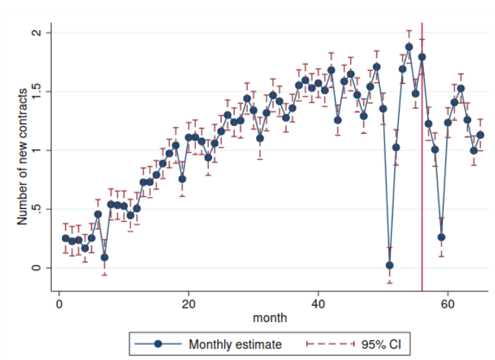



Control

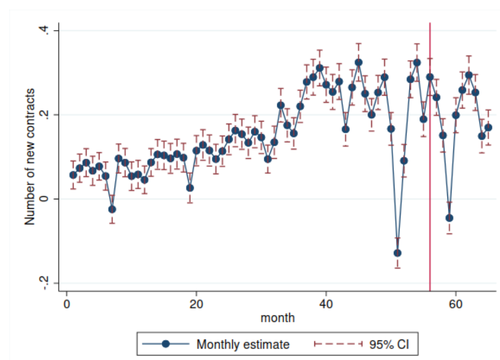



Panel B: Outcome variable: Volume of transactions above threshold

Treatment




Control

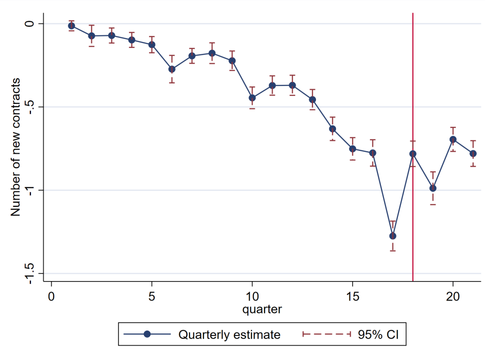



Notes: These graphs document the number of transaction above and below the reference price in the treatment and control municipalities.

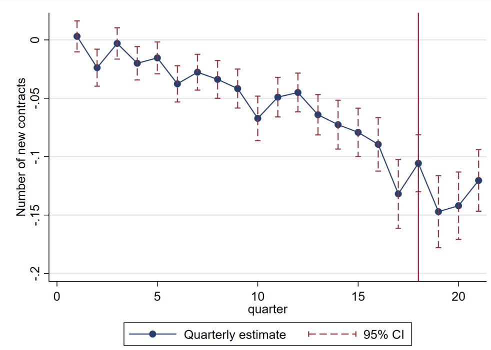
Figure A.2: Event graphs, second stage, separate treatment and control - Data grouped at quarterly frequency.

Panel A: Outcome variable: Volume of transactions below the threshold

Treatment

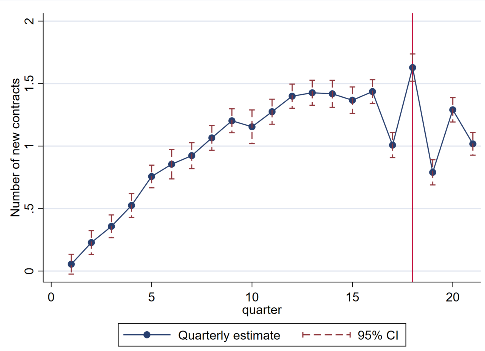



Control

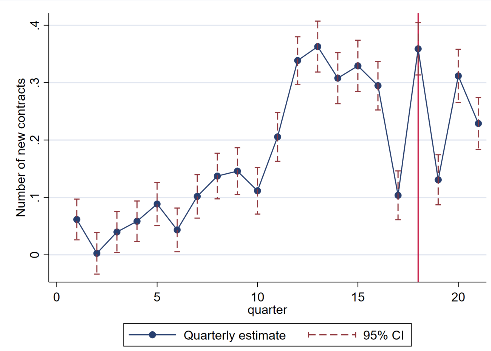



Panel B: Outcome variable: Volume of transactions above threshold

Treatment




Control




Notes: These graphs document the number of transaction above and below the reference price in the treatment and control municipalities.

## AA..2 Heterogeneity-robust estimates

Table A.1: HETEROGENEITY-ROBUST ESTIMATES OF FIRST STAGE

| | Excess price | | | | (ln) rental price | | | | Fraction above excess price | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | $\beta$ | s.e. | $\beta$ | s.e. | $\beta$ | s.e. | $\beta$ | s.e. | $\beta$ | s.e. | $\beta$ | s.e. |
| t-5 | | | -.0093 | .024 | | | .011 | .034 | | | -.064 | .053 |
| t-4 | | | -.0075 | .015 | | | -.012 | .021 | | | .0052 | .034 |
| t-3 | | | .0082 | .01 | | | .016 | .012 | | | .0092 | .031 |
| t-2 | | | .0097 | .011 | | | .0091 | .013 | | | .076 | .031 |
| t-1 | | | -.022 | .013 | | | -.03 | .016 | | | -.11 | .047 |
| Main estimate | -.051 | .019 | 0 | | -.043 | .028 | 0 | | -.13 | .043 | 0 | |
| t+1 | | | -.038 | .013 | | | -.032 | .021 | | | -.12 | .04 |
| t+2 | | | -.022 | .011 | | | -.019 | .02 | | | -.07 | .037 |
| t+3 | | | -.045 | .013 | | | -.058 | .022 | | | -.16 | .045 |
| t+4 | | | -.04 | .012 | | | -.0076 | .019 | | | -.073 | .04 |
| t+5 | | | -.059 | .013 | | | -.039 | .019 | | | -.1 | .033 |
| t+6 | | | -.062 | .015 | | | -.047 | .017 | | | -.13 | .04 |

Notes: This table reports the same results reported in Table II but using the estimator proposed in de Chaisemartin & D'Haultfoeuille (2020).

### AA..3 Within unit price and supply changes

We investigate within unit variation in Figure A.3. For this, we restrict the sample to units that we observe in multiple contracts. Using these units, we first run a regression to obtain the "unit fixed effect" price (removing municipality and time fixed effects), *prior* to the policy change. We use this to construct the distribution over excess price for these units. Once these units have been allocated to sextiles (in treatment and control groups), we plot, as before, the price and quantity changes in treated and control municipalities before and after the policy change. In this case, it is worth to highlight that price and quantity changes are for units that appear in our sample multiple times.

A number of interesting facts emerge from this exercise. First, both in treated and control municipalities we observe reversion to the mean. Units with low prices prior to the policy change tend to experience price increases and units with high prices tend to experience price declines. This is particularly pronounced in treated municipalities, which explains why the estimated within unit average effect of the policy change is negative. Another interesting aspect is that in both treated and control municipalities, we see that units in the extreme are much more likely to enter into a new contract.[21] This is, again, a sign of mean reversion. Contracts that were signed for a relatively high or low price are more likely to break, presumably because either the tenant or the owner can find better alternatives. The policy does not seem to have affected along this margin very substantially.

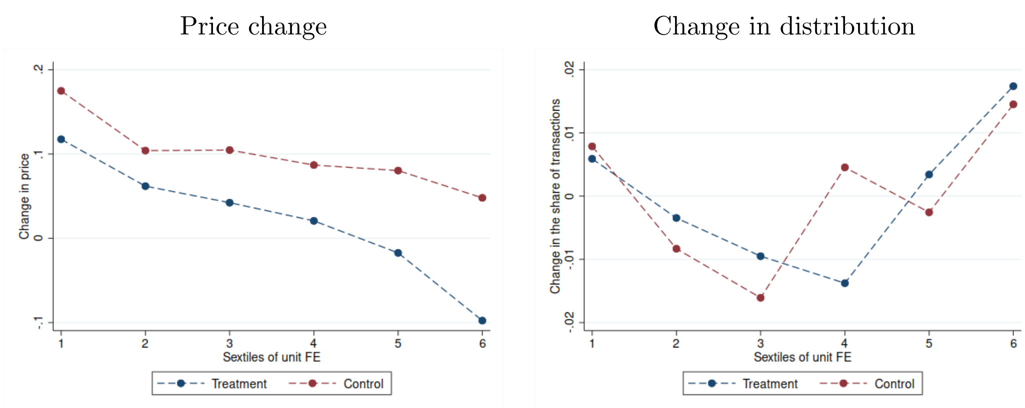
We quantify the results in Figure A.3 and explore mean reversion more thoroughly by running the following regression:

$$\Delta \ln P_{i(m)t} = +\beta_1 I(P_{it-1} < \bar{P}) + \beta_2 I(P_{it-1} > \bar{P}) + \delta_m + \lambda_t + \pi X_{it} + \epsilon_{i(m)t} \tag{11}$$

where $\Delta \ln P_{it}$ is the rate of growth of the rental price from the last contract before the beginning of the approval of the control cap to the first contract after the approval of the new regulation; $\bar{P}$ is the reference price; $I(P_{it-1} < \bar{P})$ (and respectively $I(P_{it-1} > \bar{P})$) is an indicator variable that takes the value of 1 if the reference prices are greater (or lower) than rental prices of the contract before the approval of price caps; and where $\delta_m$ and $\delta_t$ are municipality and time fixed effects. We run this regression only in the post-policy period. In some specifications we run the regression with units only in the treated municipalities, only in the control, or in both, but allowing different estimates for each group. This specification tests whether there

21. Note that in this graph we plot the change in the share of contracts rather than the absolute change, since the post-period is much shorter than the pre-period and hence the estimates for the absolute number are negative but not very meaningful.

Figure A.3: Price and quantity changes for repeated contract units




Notes: The figure shows the change in prices and quantity of monthly new contracts, between the pre- and the post-period for units with multiple rental contracts. The groups are defined prior to the policy change.

is mean reversion, and whether mean reversion is disproportionately strong in treated relative to control municipalities.

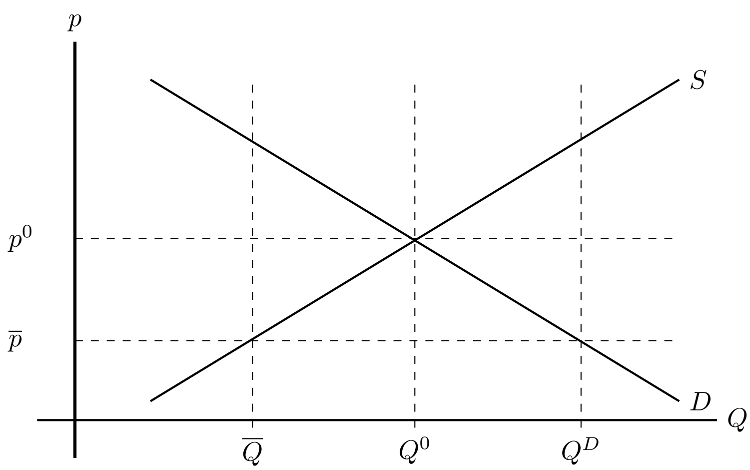
Table A.2 shows the results of these specifications. The first column shows that when the rental price is above the reference price, there is a price decline in the subsequent contract. Similarly, we obtain that if the rental price is below the reference price, then price increases substantially in the new contract. This true both in treated (Column 2) and control (Column 2) municipalities. This results suggests that there is mean reversion, as we also observe visually in Figure A.3. Columns 3 and 4 test whether mean reversion is significantly different between treated and control municipalities. We observe that only prices above the index price seem to converge more strongly to the index price in municipalities affected by the policy change, while there seems to be no differential effects on units with prices below the reference price.

Table A.2: Estimates of the elasticity of rental market housing supply

| | Post-period within unit price change ($\Delta \ln(P)$) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $\{P < \bar{P}\}$ | 0.043 | 0.035 | 0.035 | 0.022 |
| | (0.013) | (0.007) | (0.007) | (0.007) |
| $\{P > \bar{P}\}$ | -0.083 | -0.054 | -0.054 | -0.041 |
| | (0.017) | (0.007) | (0.007) | (0.007) |
| $\{P < \bar{P}\}\times$ treatment | | | 0.008 | 0.001 |
| | | | (0.015) | (0.014) |
| $\{P > \bar{P}\}\times$ treatment | | | -0.029 | -0.042 |
| | | | (0.018) | (0.018) |
| Observations | 25367 | 5779 | 31146 | 31146 |
| Fixed_Effects | yes | yes | yes | yes |
| Sample | Treatment | Control | All | All |
| Controls | no | no | no | yes |

Notes: This table estimates mean reversion for treatment and control municipalities after the policy change, for units above and below the reference price. Fixed effects include municipality and time fixed effects. Controls indicate whether the duration of the previous contract is added as a control. Robust standard errors clustered at the municipality level are reported.

### *AB. Perfect competition graph*




# B Theory appendix

### *BA. Proofs*

#### BA..1 Proof of Proposition 1

We need to proof that the system:

$$\alpha(\underline{n})(1-\varepsilon(\underline{n}))(\underline{u}-c) = \alpha(\overline{n})(1-\overline{\varepsilon}(\overline{n}))(u-c)$$

$$\frac{1}{\underline{n}} + \frac{1}{\overline{n}} = \frac{1}{N}$$

has a unique solution. First, it is worth noting that the second equation can be represented using a strictly increasing function $f_2(.)$ defined by:

$$\underline{n} = f_2(\overline{n}) = \frac{1}{\frac{1}{N} - \frac{1}{\overline{n}}}$$

Hence, we only need to show that the first equation defines an implicit function for $\underline{n}$ as a function of $\overline{n}$, which we can denote by $f_1()$. To show that $f_1()$ is decreasing we can take the total derivative with respect to $\overline{n}$ in the first equation.

$$\alpha'(\underline{n})\frac{\partial \underline{n}}{\partial \overline{n}}(1-\varepsilon(\underline{n}))(\underline{u}-c)-\alpha(\underline{n})\varepsilon'(\underline{n}))\frac{\partial \underline{n}}{\partial \overline{n}}(\underline{u}-c)+\alpha(\underline{n})(1-\varepsilon(\underline{n}))\alpha'(\overline{n}) = \alpha'(\overline{n})(1-\overline{\varepsilon}(\overline{n}))(u-c)-\alpha(\overline{n})\overline{\varepsilon}'(\overline{n})(u-c)$$

Since, $u - \frac{\alpha(\overline{n})}{\overline{n}}(u - \overline{p})$ and we assume, without loss of generality, that $\frac{\alpha(\overline{n})}{\overline{n}} = 1 - \alpha(\overline{n})$. Hence, we can write:

$$\frac{\partial \underline{n}}{\partial \overline{n}} = \frac{\alpha'(\overline{n})(1-\overline{\varepsilon}(\overline{n}))(u-c) - \alpha(\overline{n})\overline{\varepsilon}'(\overline{n})(u-c) - \alpha(\underline{n})(1-\varepsilon(\underline{n}))\alpha'(\overline{n})}{[\alpha'(\underline{n})(1-\varepsilon(\underline{n})) - \alpha(\underline{n})\varepsilon'(\underline{n}))](\underline{u}-c)}$$

To show that this is negative, the first step is to examine the sign of the denominator, which entirely depends on the sign of $[\alpha'(\underline{n})(1-\varepsilon(\underline{n})) - \alpha(\underline{n})\varepsilon'(\underline{n}))]$.

Assuming Cobb-Douglass matching function we have that:

$$\alpha(n) = (n)^{1-\theta}, \alpha'(n) = (1-\theta)n^{-\theta}, \varepsilon(n) = (1-\theta), \overline{\varepsilon}(n) = \frac{1-\theta}{1-n^{-\theta}}, \overline{\varepsilon}'(n) = \frac{(1-\theta)\theta n^{-\theta-1}}{(1-n^{-\theta})^2}$$

Hence, in that case we have:

$$[\alpha'(\underline{n})(1-\varepsilon(\underline{n})) - \alpha(\underline{n})\varepsilon'(\underline{n}))] = \frac{(1-\theta)\theta}{\underline{n}^{\theta}}$$

Which is trivially positive. The numerator is given by:

$$\alpha'(\overline{n})(1-\overline{\varepsilon}(\overline{n}))(u-c) - \alpha(\overline{n})\overline{\varepsilon}'(\overline{n})(u-c) - \alpha(\underline{n})(1-\varepsilon(\underline{n}))\alpha'(\overline{n}) =$$

$$= (1-\theta)\overline{n}^{-\theta}(1 - \frac{1-\theta}{1-\overline{n}^{-\theta}})(u-c) - (\overline{n})^{1-\theta}\frac{(1-\theta)\theta\overline{n}^{-\theta-1}}{(1-\overline{n}^{-\theta})^2}(u-c) - (\overline{n})^{1-\theta}\theta(1-\theta)\overline{n}^{-\theta} =$$

$$= (1-\theta)\overline{n}^{-\theta}(\frac{\theta-\overline{n}^{-\theta}}{1-\overline{n}^{-\theta}})(u-c) - \frac{(1-\theta)\theta}{(1-\overline{n}^{-\theta})^2}(u-c) - (\overline{n})^{1-\theta}\theta(1-\theta)\overline{n}^{-\theta} <$$

$$< (1-\theta)(\frac{\theta}{1-\overline{n}^{-\theta}})(u-c) - \frac{(1-\theta)\theta}{(1-\overline{n}^{-\theta})^2}(u-c) = -\frac{(1-\theta)\theta\overline{n}^{-\theta}}{(1-\overline{n}^{-\theta})^2}(u-c) < 0$$

And this finishes the proof.

### BA..2 Proof of Proposition 2

We need to proof that the system:

$$\alpha(\underline{n})(1-\varepsilon(\underline{n}))(\underline{u}-c) = \alpha(\overline{n})(1-\overline{\varepsilon}(\overline{n}))(u-c)$$

$$\alpha(\overline{n})(1-\overline{\varepsilon}(\overline{n}))(u-c) = k_s(N_s)$$

has a unique solution. For this, we can rely on the proof shown in Appendix BA..1 and realize that the second equation is non-decreasing.

### BA..3 Proof of Proposition 3

#### a)

This is by assumption

#### b)

We need to show that welfare is higher with one sub-market than with two sub-markets. For this, with one sub-market, when buyers are allowed to submit two applications, total welfare is given by:

$$W^* = 2 * (N_s V_s + N_b V_b) = 2 * (N_s \alpha(N)(p-c) + N_b \frac{\alpha(N)}{N}(u-p))$$

Hence, total welfare in the one market equilibrium is given by:

$$W^* = 2\mu(N_s, N_b)(u-c)$$

Instead, in the two-market equilibrium a fraction $\frac{\mu(N_{\underline{s}},N_b)}{N_b}$ of buyers meet a low-price seller, while a fraction $\left(1-\frac{\mu(N_{\underline{s}},N_b)}{N_b}\right)\frac{\mu(N_{\overline{s}},N_b)}{N_b}$ of buyers meet a high-price seller, but not a low-price one. Hence, total buyer welfare is given by

$$\begin{aligned} N_b V_b &= N_b \left[\frac{\mu(N_{\underline{s}}, N_b)}{N_b}(u-\underline{p}) + \left(1-\frac{\mu(N_{\underline{s}}, N_b)}{N_b}\right)\frac{\mu(N_{\overline{s}}, N_b)}{N_b}(u-\overline{p})\right] \\ &= \mu(N_{\underline{s}}, N_b)(u-\underline{p}) + \left(1-\frac{\mu(N_{\underline{s}}, N_b)}{N_b}\right)\mu(N_{\overline{s}}, N_b)(u-\overline{p}) \end{aligned}$$

Likewise, total seller welfare is given by

$$N_s V_s = \mu(N_{\underline{s}}, N_b)(\underline{p}-c) + \left(1-\frac{\mu(N_{\underline{s}}, N_b)}{N_s}\right)\mu(N_{\overline{s}}, N_b)(\overline{p}-c)$$

Therefore, in the two-market equilibrium total welfare is given by:

$$W^{**} = \left(\mu(N_{\underline{s}}, N_b) + \mu(N_{\overline{s}}, N_b)\right)(u-c) - \frac{\mu(N_{\underline{s}}, N_b)}{N_b}\mu(N_{\overline{s}}, N_b)(u-\overline{p}) - \frac{\mu(N_{\underline{s}}, N_b)}{N_s}\mu(N_{\overline{s}}, N_b)(\overline{p}-c)$$

And this is smaller than $2\mu(N_s, N_b)(u-c)$, since both $\mu(N_{\underline{s}}, N_b)$ and $\mu(N_{\overline{s}}, N_b)$ are equal to a number smaller than one multiplied by $\mu(N_s, N_b)$.

**c)**

Although total welfare is higher, there may be winners and losers, depending on the level of chosen prices:

$$V_b(N,P) = \frac{\alpha(N)}{N}(u-P)$$

Whereas in the two sub-market equilibrium:

$$V_b = \frac{\alpha(\underline{n})}{\underline{n}}(u-\underline{p}) + (1-\frac{\alpha(\underline{n})}{\underline{n}})\frac{\alpha(\overline{n})}{\overline{n}}(u-\overline{p})$$

Without loss of generality we can assume $N_b = 1$, then:

$$V_b(N,P) = \mu(N_s, 1)(u-P)$$

$$V_b = \mu(1-\rho,1)\mu(N_s,1)(u-\underline{p}) + (1-\mu(1-\rho,1)\mu(N_s,1))\mu(\rho,1)\mu(N_s,1)(u-\overline{p})$$

Let's start with the first part of the proposition. Suppose a policy is put in place that forces the same price to each unit. Then, buyers only send one application and buyers only post one price.

**One sub-market equilibrium**

The maximization problem is then:

$$V_s = \max_{p,N} \alpha(N)(p-c) \text{ subject to } \frac{\alpha(N)}{N}(u-p) = V_b$$

From the constraint we have that $p = u - \frac{N}{\alpha(N)}V_b$, and hence we can substitute it into the maximization and obtain:

$$V_s = \max_{N} \alpha(N)(u - \frac{N}{\alpha(N)}V_b - c)$$

Taking first order conditions we obtain:

$$\alpha'(N)(u-c) = V_b$$

With this, we can replace $V_b$ in the constraint and obtain:

$$P^N = (1 - \varepsilon(N))u + \varepsilon(N)c$$

where as, again, $\varepsilon(N) = \frac{N\alpha'(N)}{\alpha(N)}$ is the elasticity of meeting probability with respect to the ratio of buyers to sellers.

With this, we can compute the welfare of sellers and buyers:

$$V_s = \alpha(N)(1 - \varepsilon(N))(u - c)$$

And the welfare of buyers is given by:

$$V_b = \frac{\alpha(N)}{N}\varepsilon(N)(u - c)$$

**Price comparisons**

First, it is worth noting that $\underline{p} < \overline{p}$. To see this, we can use the fact that $V_{\underline{s}} = V_{\underline{s}}$ to obtain:

$$\frac{\overline{p}}{\underline{p}} = \frac{\alpha(\underline{n})}{\alpha(\overline{n})(1 - \frac{\alpha(\underline{n})}{\underline{n}})}$$

And the numerator, which is the probability to find a buyer in the low-price market needs to be larger than the probability of finding a buyer in the high-price market conditional on not finding it in the low-price one. Otherwise, there would be more entry in the high price market, which delivers a higher payoff to sellers.

Now, we can also show that $\underline{p} < P^N$ (at least under Cobb-Douglass matching). To see this:

$$(1 - \varepsilon(\underline{n}))\underline{u} + \varepsilon(\underline{n})c < (1 - \varepsilon(N))u + \varepsilon(N)c$$

which can be re-written as:

$$(1 - \varepsilon(\underline{n}))u + \varepsilon(\underline{n})c - (1 - \varepsilon(\underline{n}))\frac{\alpha(\overline{n})}{\overline{n}}(u - \overline{p}) < (1 - \varepsilon(N))u + \varepsilon(N)c$$

And hence:

$$(1 - \varepsilon(\underline{n}))u + \varepsilon(\underline{n})c - (1 - \varepsilon(\underline{n}))\frac{\alpha(\overline{n})}{\overline{n}}(u - \overline{p}) < (1 - \varepsilon(N))u + \varepsilon(N)c$$

And hence, when the matching function is Cobb-Douglas, and $\varepsilon$ is a constant, then we have $\underline{p} < P^N$.

Finally, note that:

$$\overline{p} = (1 - \overline{\varepsilon}(\overline{n}))u + \overline{\varepsilon}(\overline{n})c = (1 - \varepsilon(\overline{n}))u + \varepsilon(\overline{n})c - (\overline{\varepsilon}(\overline{n}) - \varepsilon(\overline{n}))(u - c)$$

**Welfare comparisons**

**Planner's solution**

To show that the planners solution is the same as the one price market equilibrium, we need first, to realize that the one market equilibrium is superior to the two sub-markets equilibrium, hence the planner should only consider one sub-market equilibria. We assume that the planner maximizes:

Note that:

$$W^{Pl}(n) = N_s\alpha(n)(p-c) + n_b\frac{\alpha(n)}{n}(u-p) = N_s\alpha(n)(p-c) + N_s\alpha(n)(u-p) = N_s\alpha(n)(u-c)$$

But this is increasing in $n$, hence, the maximum welfare is when all the buyers are in the market. In that case $W^{Pl}(N) = W^*$.

**Part 2**

We now turn into the second part of the proposition, i.e., where we assume free entry. With free entry the number of buyers now depends on the free entry condition. When the policy forces a unique price, then, as before we obtain:

$$p = (1-\varepsilon(N^{FE}))u + \varepsilon(N^{FE})c$$

And with this, we obtain that:

$$V_s(N^{FE}) = \alpha(N^{FE})(1-\varepsilon(N^{FE}))(u-c)$$

Free entry means that we impose $V_s(N^{FE}) = k_s(N_s^{FE})$, hence:

$$\alpha(N^{FE})(1-\varepsilon(N^{FE}))(u-c) = k_s(N^{FE})$$

Which can be re-written as:

$$(\frac{N_b}{N_s^{FE}})^{1-\theta}\theta(u-c) = k_s(N_s^{FE})$$

Note that this has a solution since $k_s(N^{FE})$ is either a weakly increasing while $(\frac{N_b}{N_s^{FE}})^{1-\theta}\theta(u-c)$ is decreasing in $N_s$.

To see that with one price, the supply is lower, we need to compare to the seller's payoff under free entry and one sub-markets:

$$V_s^{FE} = \alpha(N^{FE})\varepsilon(N^{FE})(u-c) = k_s(N^{FE})$$

And the sellers' payoff with two sub-markets:

$$V_s^{**} = \alpha(\overline{n})(1 - \frac{\varepsilon(\overline{n})}{(1 - \frac{\alpha(\underline{n})}{\underline{n}})})(u-c) = k_s(N_s)$$

Which we can write as:

$$V_s^{**} = (\overline{n})^{1-\theta}(1 - \frac{1-\theta}{(1-(\underline{n})^{-\theta})})(u-c) = k_s(N_s)$$

$$V_s^{**} = (\overline{n})^{1-\theta}(\frac{\theta-(\underline{n})^{-\theta}}{(1-(\underline{n})^{-\theta})})(u-c) = k_s(N_s)$$

$$V_s^{**} = (\frac{N_b}{\overline{n}_s})^{1-\theta}(\frac{\theta-(\underline{n})^{-\theta}}{(1-(\underline{n})^{-\theta})})(u-c) = k_s(N_s)$$

Without loss of generality we can assume that a fraction $\rho$ of the sellers post high prices, and a fraction $(1-\rho)$ post low-prices:

$$V_s^{**} = (\frac{N_b}{\rho N_s})^{1-\theta}(\frac{\theta-(\underline{n})^{-\theta}}{(1-(\underline{n})^{-\theta})})(u-c) = k_s(N_s)$$

$$V_s^{**} = (\frac{N_b}{N_s})^{1-\theta}(\frac{1}{\rho})^{1-\theta}(\frac{\theta-(\underline{n})^{-\theta}}{(1-(\underline{n})^{-\theta})})(u-c) = k_s(N_s)$$

Now, note that $\underline{n} = \frac{N_b}{\underline{n}_s} = \frac{N_b}{(1-\rho)N_s}$

$$V_s^{**} = (\frac{N_b}{N_s})^{1-\theta}(\frac{1}{\rho})^{1-\theta}(\frac{\theta-(\frac{N_b}{(1-\rho)N_s})^{-\theta}}{(1-(\frac{N_b}{(1-\rho)N_s})^{-\theta})})(u-c) = k_s(N_s)$$

$$V_s^{**} = (\frac{N_b}{N_s})^{1-\theta}(\frac{1}{\rho})^{1-\theta}(\frac{\theta-(1-\rho)^{\theta}(\frac{N_b}{N_s})^{-\theta}}{(1-(1-\rho)^{\theta}(\frac{N_b}{N_s})^{-\theta})})(u-c) = k_s(N_s)$$

Hence, we need to check

$$(\frac{1}{\rho})^{1-\theta}(\frac{\theta-(1-\rho)^{\theta}(\frac{N_b}{N_s})^{-\theta}}{(1-(1-\rho)^{\theta}(\frac{N_b}{N_s})^{-\theta})}) > \theta$$

Now, this holds, as long as $\rho > 1 - \theta^{\frac{1}{\theta}}(\frac{N_s}{N_b})^{\theta}$. And this finishes the proof.