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The effect of second generation rent controls: New evidence from Catalonia

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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 those, only the units above a certain reference price. Using micro-data on rental units, we analyze the effect of the policy on both rental prices and rental supply. We find that the policy led to a reduction in rental prices of around 5 percent. Half of this price decline is due to changes in the composition of units available in the market, particularly in larger municipalities. The policy also led to a decline in the amount of rental units available in the market. Using variation from the policy change, we compute a rental housing supply elasticity of around 4.

Keywords: *Rent control, reference price, housing supply, event study*

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 is 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 Great Britain, and it was replicated by several countries around Europe. During the last 40 years some cities in the US have also had various forms of rent control including New York City, Washington D.C., San Francisco, Boston, Brookline, Cambridge, or Los Angeles. In contrast, there are 36 US states that forbid rent control laws entirely, although this seems to be changing. The state of Oregon, for example, approved in 2019 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.

The recent experiences in the US and many other countries use what is often referred as "third generation" rent controls, which cap rent increases within tenancies. However, in some European cases there have been recent experiences of "second generation" rent controls[2], which limit rent increases within and between tenancies. That was the case of the *Mietpreisbremse* in Germany which was enacted in 2015. In 2020, the law was modified to be even more restrictive prohibiting any rent increase during a period of 5 years and moving the facto to a "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 highlight the negative consequences on economic efficiency of keeping rents below market prices.[3] As we discuss in more detail below, arguments against rent controls in general include fear of distortions in the rental and selling housing markets, mismatch between types of housing units and types of tenants, and inefficient levels of consumption of housing services. On the plus side, rent caps may be an effective tool to preserve neighborhood's social capital.

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

1. Two monumental sociological studies, (Desmond, 2017; Osman, 2022) show the problems of housing accessibility in the US. Osman (2022), in a very original sociological analysis, argues 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. Notice that Arnott (1995) define as 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 deals with the issue of rent freezes. Arnott (1995) argues that there is not much dispute about the harmful nature of first-generation rent controls since empirical evidence is quite robust.

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

In this paper, we analyze the effects of the approval of a "second generation" rent control regulation that was introduced in Catalonia in September 2020. The policy change established a maximum rental price (reference price) for each housing unit in the market, which depended on the mean rental price of the 25 closest units. The policy affected only a group of municipalities, something that gives us extra sources of variation, for instance, to compare various groups of treated and non-treated municipalities. Moreover, and better than in other studies, we have detailed housing unit data on the rental housing in Catalonia. In particular, we have information on each rental contract signed, its starting date, its ending date (in case the contract terminated), the rental price and some house unit characteristics, such as surface. We also have a unique identifier for each unit, which allow us to track the price associated to different contracts, and hence, control for unit time invariant characteristics.

Using these data we report two main results. First, we document that the policy led to price declines in the rental market. On average, rental prices dropped by around 4-7 percent 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 between treated and non-treated municipalities – even when we compute pre-trends using more than 50 months of data. We obtain these results using both non-parametric approaches that rely on comparing histograms of prices above and below the reference price and relying on standard difference-in-difference comparisons.

As we discuss in more detail below, the (mostly theoretical) literature has argued that price caps may result in changes in the supply of units into the rental market. These changes may be driven by both a change in the composition of the units and a change in the overall number of units available in the market. Interestingly, we find that around 50% of the price decline can be explained by composition effects. The point estimate drops by half when we include unit fixed effects in our price estimates. Furthermore, we corroborate this evidence by showing that the size of the units in the market changes as a result of the

4. Arnott (1995) argues that rent control should be judge 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.

policy, a clear indication that composition effects are behind part of the price decline. We also find that the overall supply of units in the market declines by around 10 - 14 percent, which is mostly explained by a large decline in units available above reference prices that is not compensated by the increase in units below the index. These estimates allow us to compute an elasticity of rental unit supply between 3 and 5.

Overall, these results provide empirical support for the notion that "second-generation" rental caps are effective at reducing prices but also affect supply. Changes in the supply materialize both through changes in the composition and through the overall amount of units available in the rental market.

Related Literature

Rent controls were a popular policy tool in the 1920s and 1970s. However, data to analyze the effects of this policy were not good or even available. Hence, early literature on this topic is predominantly theoretical. It is only more recently that the availability of reliable micro data sets at the level of individual rental units has allowed to study rent control in quite some detail. The most relevant and credible recent studies (Sims, 2007; Autor et al., 2014; Diamond et al., 2019) conclude that rent control reduces rental prices, but also the stock of rental units.

The main (intended) effect of rent controls is to reduce rental prices. However, the theoretical literature has identified several channels through which the policy can lead to unintended consequences. For instance, rent controls can also lead to "mismatches" between tenants and rental housing. Once a tenant has obtained a rent-controlled apartment, the household may decide to reside in that dwelling longer than would have been optimal based on their needs for housing services (Suen, 1989; Glaeser & Luttmer, 2003; Bulow & Klemperer, 2012). Examples of this misallocation of resources are abundant. Households with children that have already emancipated may decide to stay in family-size apartments, or, conversely, young families may end up living in crowded and small studios. Likewise, if rental prices are below market prices, renters may choose to consume excessive amounts of housing (Olsen, 1972; Gyourko & Linneman, 1989). Rental price caps can also cause rental housing to deteriorate. Owners may decide not to invest in maintenance because they cannot recoup this investment once rental price growth is limited (Sims, 2007; Downs, 1988; Moon & Stotsky, 1993). All these mechanisms translate into a change in the composition and 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 may develop specific social capital, such as a network of friends and family, proximity to a job, or children enrolled in local schools, and, hence, the risk of excessive rental price appreciation may be very costly for some households. Unlike people with little connection to a specific area, who can easily insure themselves against local rental price appreciation by moving to a cheaper location, renters who have made a substantial investment in the local community cannot use this type of insurance (moving) so easily, since they must give up some or all of the capital specific to their neighborhood.

Our paper contributes to recent empirical studies that analyze the impact of the limit on rental prices on tenants, owners, and the real estate market using "natural" experiments. Most of this literature, however, studies the removal of rental caps. For instance, Boston (1969), Brookline (1970), and Cambridge (1971) all adopted rent controls in the late 60s and early 70s. However, in November 1994, Massachusetts approved the elimination of rent control in a very close vote. Shortly before the removal of rent controls in 1994, controlled housing rents were 40 percent below the rental price of nearby uncontrolled housing. This change directly affected properties previously subject to rent control, allowing their owners to charge market rents.

As mentioned before, there is no good data to analyze the enactment of the policy. Instead, available data has allowed various researchers to analyze the removal of this rent control. In a seminal contribution, Sims (2007) analyzes the case of the end of rent control in Boston MSA using data from the American Housing Survey. He finds that rent control induced owners to remove units from the rental market and decreased rents. In addition, Sims (2007) shows that there was no effect on construction, but rent control reduced maintenance in controlled units and the spillover effect reduced the rent of non-controlled units. The dead weight loss from the reduction in the number of rental units was significant even without considering the missallocation of rental units. In addition, it was unclear that the transfer of the surplus from the tenants benefited poor households. Only 26% of rent controlled apartments were occupied by tenants in the bottom quartile of the household income distribution.

In a similar vein, Autor et al. (2014) analyze the impact of this unexpected elimination of rent control in Cambridge, finding that the market value of properties no longer subject to rent control increased by 45 percent. In addition to the direct effect of removing rent caps, Autor et al. (2014) show that removing rent control had significant indirect effects on neighboring properties, also increasing their values. In short, the rent control policy imposed $2 trillion in costs on local landlords, but only $300 million was passed on to tenants in rent-controlled apartments: existing tenants benefit from the insurance provided by rental caps, but the total cost of providing this insurance is very high.

More recently, Diamond et al. (2019) discuss 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 construction and smaller buildings were exempt from control. However, in 1994 the exemption for buildings with less than

5 apartments was eliminated. Diamond et al. (2019) show that while rent control appears to help ordinary tenants in the short term, in the long term it decreases affordability. Rental units are sold to very high income buyers increasing gentrification. The probability of transforming the buildings that were rent-controlled into condominiums was 8% higher than the probability in the control group. Consistent with this fact, the cap on rental prices decreased the number of tenants living in controlled buildings by 15 percentage points, and the number of tenants in controlled units by 25 percentage points from the 1994 levels.

We differ from Sims (2007) and Autor et al. (2014) in that we analyze the implementation, instead of the removal, of a rental cap. We differ from Diamond et al. (2019) in that we analyze a second, rather than a third, generation rental cap policy.[5] In principle second generation rent controls have a higher cost in terms of economic efficiency since it is not possible to reset rent to market rents even between tenancies. Our paper also contributes to the recent literature on second generation rent control in Europe. For the case of Germany. Breidenbach et al. (2022) uses a triple differences event study to show that the initial success of rent caps on reducing rental prices vanishes after one year. Mense et al. (2019a) shows that land prices grow faster in municipalities where rent caps were implemented than in the control group. Mense et al. (2019b) find a positive spillover effect of rent control on markets without rent caps. Our data, however, allows us to analyze both within unit and average effects of the policy, which is crucial to study both the intended and unintended consequences of these type of policies.

II The 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%[6]. 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 factors explain this change. First, banks reduced lending and increased the requirement to obtain a loan. Second, young people likely realized that getting a mortgage to buy a house before getting 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 the supply and rental prices increased. Other factors, such as flat rental platforms may have also played a role, at least in some specific neighborhoods (Garcia-Lopez

5. In all California 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.

6. These two numbers do no add to 100% because there is a fraction of tenants with reduced rents and there are units that are free of rent.

et al., 2020; Almagro & Dominguez-Iino, 2022). For instance, pressure on rental prices was higher in cities where there was already a high proportion of tenants. That is the case of Barcelona (which experienced a 38.4% rental price increase over the period 2014-2019) or the whole Metropolitan Area of Barcelona (23.4% rental price increase).

To try to contain the increase in prices, and following the experiences of some European cities like Berlin and Paris, the Parliament of Catalonia approved a system of rent ceilings that was implemented at the end of September of 2020.[7] The law considered two types of areas: tight housing markets and the rest of the territory. "Stressed" municipalities were initially defined as the ones where rental prices had grown by more than 20% during the period 2014-2019, which, as we show below, comprise many municipalities around Barcelona plus some others further dispersed over the territory.

The Catalan law imposed second generation rent caps, which limit rent increases between tenancies, following the regulations approved in Berlin.[8] After the approval of the law reference prices become, at least on paper, the highest price that the rental price per square meter can reach. The reference rental price is calculated using the mean rental price of the 25 closest rental units.[9] An interesting feature of these reference prices is that they were calculated not only for the areas that were declared "stressed" markets but for a total of 137 municipalities, many not affected by the policy change. To construct the reference price for each unit, the pool of comparison units is stratified by size: units above and below 90 square meters. This is, if the size of the unit is larger than 90 m2 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 meters, then the reference prices is calculated as the mean of the 25 closest units with a size plus/minus 10 square meters. Hence, detailed unit level data is needed to fully evaluate the effects of the policy.

III Data

Our data consists of more than half a million rental contracts reported to INCASOL from 2016 until June of 2021. In Catalonia there is a legal obligation to report rental contracts to this institution. We merged the information available in INCASOL with the information at the AHC (Housing Agency of Catalonia). The AHC was responsible of setting the methodology to calculate the reference price for each rental unit. After merging both data sources we have information on the the closing price, the starting date of the contract,

7. Act 11/2020 of September 18, 2020.
8. The Spanish regulation already caped rents within tenancies using CPI inflation.
9. The smallest radium is 50 meters.

the ending date – if it has already ended –, the square meters of the unit[10] and the reference price per square meter.

The main advantage of these data is that we have information on rental market prices, instead of the ask price, or an imperfect measure of the agreed price. This is, for example, an important difference with respect to the research that evaluated the impact of rent control in Germany which uses posted rents (Mense et al., 2019a,b; Borusyak et al., 2021). These data are also better than online price data used in other research, and widely available through scrapping. As can be 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 we have data for each unit in the market, both on the rental price and the size of the unit. This 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 covers two types of municipalities. We denote as treated the rental contracts for units in municipalities that faced the rent cap, and as the control group the ones that did not. It is worth highlighting 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 there are some cases that are in other areas of Catalonia.

Treated and non-treated municipalities are markedly different in a number of dimensions, beyond its proximity to Barcelona. Table A.1 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 trend across municipalities, systematic differences in the level of outcomes of interest does not necessarily pose a thread to our identification strategy. It is important, however, to keep in mind that there is a substantial difference between the two groups, and hence a thorough check on pre-trends is an important step in the analysis or use synthetic groups.

IV Results

In this section we evaluate the effect of the policy change that took effect in October 2020. We analyze various dimensions through which the policy affected the housing market for rentals. First, we study how the

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.

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.

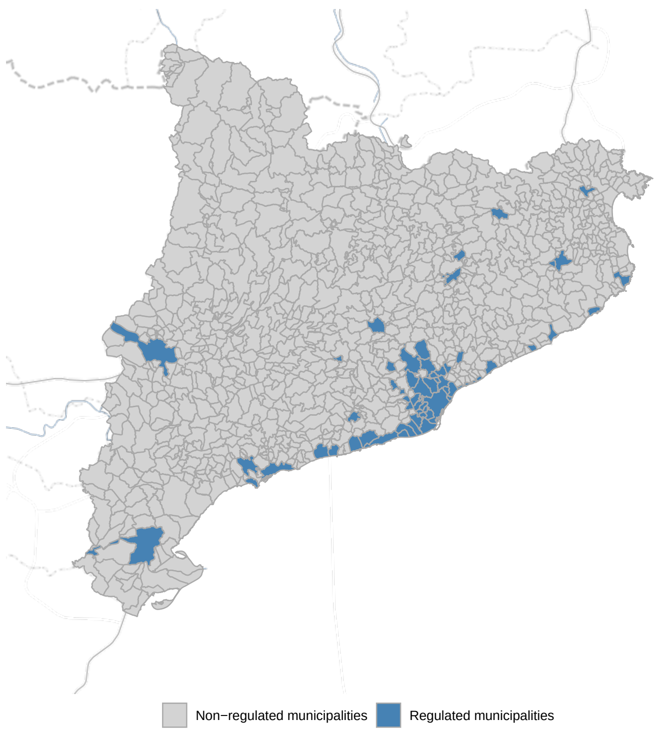
policy affected rental prices using a number of empirical strategies. Second, we study whether the policy also affected the supply of housing, both in terms of the composition and the overall number of units available in the rental market.

IVA. The effect of the policy on rental prices

IVA..1 Non-parametric evidence

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

Figure II: Map of municipalities affected by rental price caps



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

$$\text{Excess price}_{i(m),t} = \ln\left(\frac{r_{i(m),t}}{\text{index}_{i(m),t}}\right)$$

where $r_{i(m),t}$ denotes the rental price per square meters in unit i, located in municipality m, at time t, and where $\text{index}_{i(m),t}$ is the price cap calculated for this housing unit based on the surrounding units, as explained before. This 'Excess price' measure captures the percent deviation of the actual rental price from the cap. This 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 below the cap. If the Excess price is equal to -0.1 it means that the rental price is around 10% below the rental cap.

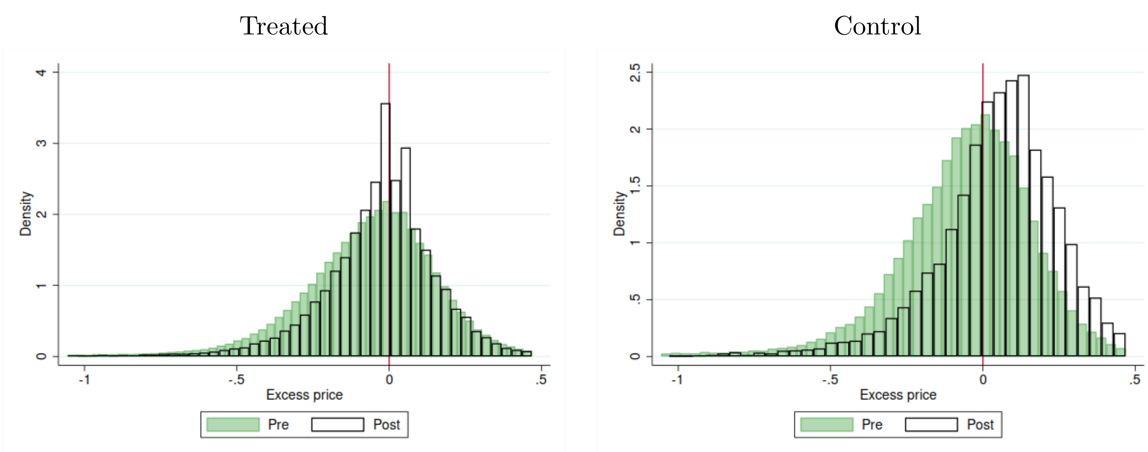
Figure III presents the histogram of excess of prices for treated and control municipalities. It is worth noting that the reference price is also calculated for the control area, something that was justified for transparency purposes, and, hence, we can construct histograms for both treated and non-treated areas. On the left hand side of the figure, we see 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 higher mass of units with prices just below the cut-off, and even a higher mass above, but close to, the cut-off. This contrasts with the histogram for the control municipalities. The histogram in control municipalities does not have any bunching around 0. Moreover, during the post- policy change period, excess prices are shifted to the right, something that 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 new contracts complied with the legislation. It is also hard 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, instead, is some sort of spillover from treatment to control, or the direct effect of the policy on the treated locations.

One of the aspects that makes Figure III hard to interpret is that it pools all contracts signed before and all those signed after the policy change, both among treated and control groups. When pooling all the data, it is hard to know what part of the observed changes is due to differential trends and what part may 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 but in the previous year. Specifically, graphs on the left show histograms for September and October of 2019 and graphs on the right show September and October of 2020. Panel A

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 indexed calculated for that unit.

includes the contracts in the treated municipalities while Panel B in the bottom shows the distributions of rental prices for the contracts in the control area.

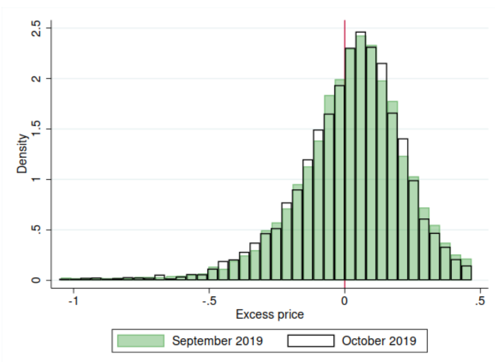
The comparison of the figures in the left and the right indicates that, while in the control municipalities there is not much difference in rental prices before and after the policy change, in the treated area there is a clear movement of the distribution of excess rents to the left producing a mitigating effect on rents. Again, it is also interesting to notice that, after the policy change, almost half of the rental prices are above the corresponding reference price. This seems to suggest that owners found ways to charge renters a price above the reference prices by finding loopholes in the regulation.

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

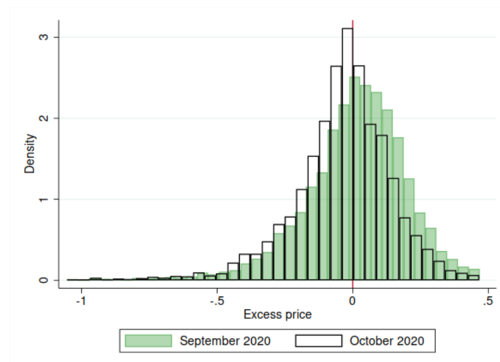
Figure IV: Histograms of excess prices around the policy change date versus previous year

Panel A: Treated municipalities

September/October 2019

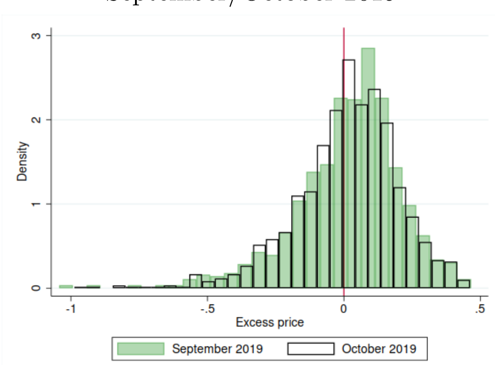


September/October 2020

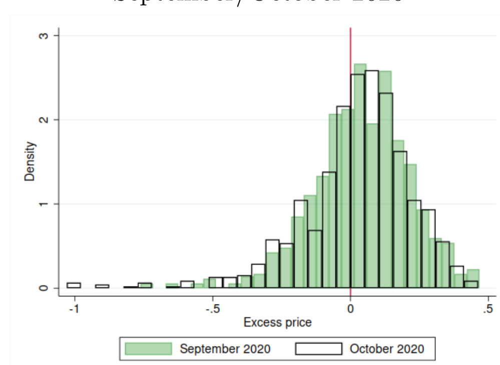


Panel B: Control municipalities

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 indexed calculated for that unit.

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 difference approach. These type of research designs crucially depend on whether the trends in the outcomes of interest between treated and control group are parallel prior to the treatment. This is usually checked visually by running dynamic difference-in-difference specifications such as the following one:

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

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 value 1 if municipality m ever faces a rent cap. The coefficients β_k estimate the differential effect on the treatment at various points in time.

Figure V plots these β_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 afterwards. 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 above the reference price. In the 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 right after the introduction of the rent cap. It is also worth highlighting that all the coefficients prior to the policy change fluctuate around, and are never statistically different than, zero. Including province trends shows very similar patterns. If anything, including province specific flexible time trends makes the trends prior to the policy change even more parallel. The drop in prices after the implementation of the rental cap is of the same magnitude when we include these flexible trends.[12]

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

12. Results are also robust to using the estimator proposed in de Chaisemartin & D'Haultfoeuille (2020) (see Table A.2 in the Appendix) which delivers a main estimate of -4.3%, and other estimators that allow for dynamic effects like 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 of them are statistically significant. In the latter case if the synthetic group is constructed to considers the treatment in the city of Barcelona the estimator is not statistically significant.

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

where 'Rent $\text{Cap}_{m,t}$' is a dummy variable taking value 1 if municipality m has a rent cap in place at time t.

Table I shows the results using three outcome variables: excess prices, price per square meter and the fraction of prices above 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 allow for the fact that each of the four Catalan provinces evolve in different ways over time.[13] Column 3 shows the estimates of the baseline specification using municipalities with less than 40,000 inhabitants. This specification should mitigate, to some extent, concerns about the fact that the treatment and control group are markedly different, as explained above. Columns 4 and 5 repeat the specifications of Columns 1 and 3 but using unit fixed effects, and hence, only the sample of units which we observe in multiple rental contracts. Being able to perform the estimation of Columns 4 and 5 is one of the main advantages of having micro level observations on the universe of rental units. Standard errors reported in Table I and in the rest of the paper are always clustered at the level of municipality.

Panel A presents the results for the specification on the excess of prices. It shows that rent caps reduce the excess of rental prices in all the specifications. Excess prices prices decline by between 3 to 7.5 percent. The differences in the point estimates are indicative of various aspects that can only be taken into account with our data, instead of more aggregate data. First, when we include flexible province trends, the point estimate is only slightly lower than in our baseline specification. This suggests that differential trends across provinces are not particularly pronounced. The point estimate is somewhat larger (in absolute terms) when restricting the sample to comparable municipalities (at least in terms of size). This can be explained by somewhat differential trends between Barcelona and the other smaller municipalities in our sample.

It is particularly interesting to explore how point estimates change once we include unit fixed effects. When we compare Column 1 and 2 to Column 4 we see that the point estimate drops by half. This indicates that the selection of units in the market explains 50% of the estimate average excess price decline. Instead, when we compare Column 3 to column 5, which both restrict the sample of municipalities to those below 40,000 inhabitants, we see that the estimated excess price decline is almost the same across the two columns. This suggests that composition effects are entirely concentrated in the bigger municipalities. We return to this important point later, in Section IVB..1.

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.

Table I: Effect of rent cap on rental prices

Panel A

	Excess price				
	(1)	(2)	(3)	(4)	(5)
Effect of Rent Cap	-0.061	-0.052	-0.076	-0.029	-0.067
	(0.012)	(0.008)	(0.012)	(0.007)	(0.014)
Observations	513559	513451	253466	110783	50277

Panel B

	(ln) Price				
	(1)	(2)	(3)	(4)	(5)
Effect of Rent Cap	-0.049	-0.038	-0.074	-0.030	-0.065
	(0.011)	(0.008)	(0.011)	(0.011)	(0.013)
Observations	513559	513451	253466	110783	50277

Panel C

	Fraction above index				
	(1)	(2)	(3)	(4)	(5)
Effect of Rent Cap	-0.143	-0.133	-0.185	-0.158	-0.224
	(0.012)	(0.011)	(0.016)	(0.024)	(0.033)
Observations	105049	105049	26086	58476	8459

Notes: Columns (1) is our baseline specification. Columns (2) adds province $\times$ month fixed effects. Columns (3) limit our baseline specification to municipalities with fewer than 40,000 inhabitants. Column (4) adds unit fixed effects. Column (5) adds unit fixed effects but limits the sample to municipalities with fewer than 40,000 inhabitants. Robust standard errors clustered at the municipality level are reported.

Panel B shows the same specifications than panel A, but using rental prices as outcome variable. Estimates fluctuate between -0.074 and -0.030. Moving from Column 1 to Column 2 shows that differential province specific trends are a bigger concern when studying rental prices than deviations of those from the index price, as one could probably expect. Moving to Column 3, we observe, again, that the point estimate is substantially larger when conditioning on municipalities of less than 40,000 inhabitants, once again, reflecting the presumably differential trend between larger municipalities such as Barcelona and smaller ones.

Once more, perhaps the most interesting facts emerge when looking at the estimates in column 4 and 5. Those corroborate the idea that there is a change in the composition of the units available in the market, which is particularly prevalent in larger municipalities.

Panel C shows another way to look at the data. In this panel we compute for each municipality - month, the fraction of housing units that is above the index price. The estimates corroborate the evidence presented in panels A and B. The decline in the fraction of units above the index price fluctuates between 13 and 23 percent, depending on the types of comparisons of each specification.

IVA..3 Price growth

The results in Section IVA..2 suggest that composition effects are likely to be important, and hence, there is much to learn from studying price changes within units that we observe in the market several times. We study this point further by looking at the change in rental prices of renewed contracts that were initially above or below the reference price using the following specification:

$$\begin{aligned}\Delta lnP_{it} &= \alpha + \beta_1 I(P_{it-1} < \bar{P}) ln(\bar{P}/P_{it-1}) + \beta_2 I(P_{it-1} > \bar{P}) ln(P_{it-1}/\bar{P}) \\ &+ \sum \gamma_t \lambda_t + \pi_x X_{it} + \delta_g + \epsilon_{it}\end{aligned} \tag{3}$$

where ΔlnP_{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; $I(P_{it-1} < \bar{P})$ ($I(P_{it-1} > \bar{P})$) is an indicator variable that takes value 1 if the reference prices is above (below) rental prices of the contract before the approval of price caps; $ln(\bar{P}/P_{it-1})$ is the distance of the previous price to the reference price[14]; λ_t is a monthly time dummy for the starting month of the contract before rent caps[15]; δ_g is a dummy variable that takes value 1 if the contract belongs to a municipality in the treatment area, and the variables in X can include indicator variables for previous price being above or below the reference price, the duration of the initial contract or the size of the house.[16]

In principle, the policy change has the intention to affect rental prices that are above the reference price, especially those with prices well above the index of reference. In contrast, in the absence of market spillovers, rental units with prices below the reference price should experience no differential change in prices, since, in

14. For $I(P_{it-1} > \bar{P})$ the variable is defined as $ln(P_{it-1})/\bar{P}$.
15. We also include a dummy for the month of the signature of the contract after the rent caps were introduced.
16. The specifications that include duration do not include the time dummies

principle those units are not directly affected by the policy. This means, that, without spillovers, we would estimate β_1 to be around 0 and β_2 to be negative and statistically significant.

We can extend that extend that exercise to an asymmetric effect in the treatment and the control area.

$$\begin{aligned}\Delta lnP_{it} &= \beta_1 I(P_{it-1} < \bar{P}) ln(\bar{P}/P_{it-1})\delta_g + \beta_2 I(P_{it-1} > \bar{P}) ln(P_{it-1}/\bar{P})\delta_g \\ &+ \beta_3 I(P_{it-1} < \bar{P}) ln(\bar{P}/P_{it-1})(1-\delta_g) + \beta_4 I(P_{it-1} > \bar{P}) ln(P_{it-1}/\bar{P})(1-\delta_g) \\ &+ \sum \gamma_t \lambda_t + \pi_x X_{it} + \epsilon_{it}\end{aligned} \tag{4}$$

Table II shows the results of these specifications. The first column shows that the higher is the rental price above the cap the faster it is reduced in the next contract after the approval of the new regulation. This is expected from the mechanism that regulates rent caps. Every 10 percentage points that the rental price in the previous contract is above the reference price, the change in price to the next contract is reduced by 3.8 percentage points. It is interesting to notice that if the previous price was below the reference price then the price increases the lower was the original rental price with respect to the reference price. This means that the reference price becomes a focal point for owners that increase prices to get close to that reference price independently of the original price. This result is consistent with the non parametric evidence that we showed previously.

The second column of Table II shows the results of the specification that distinguishes between treatment and control areas. In control areas there is an interesting effect: for prices above the reference price the distance to the reference price is not statistically significant. This is reasonable since this are rental units in the control area. Interestingly, for rental prices that in the previous contract were below the reference prices, the increase as a function of the difference of the previous price with respect to the reference prices is even larger than in treatment areas.

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 probably smaller than intended, since a substantial fraction of new rentals still charged prices above the reference price. As mentioned before, past, mainly theoretical, papers have expressed concerns on these type of policies for at least to reasons. First, rental price caps may affect the composition of units available in the rental market, which may have efficiency consequences. Second, rental price caps may affect the overall supply of housing units in the market. At the binding price cap, some homeowners may decide not to put their housing unit for rent. We investigate empirically whether there is any evidence for either of these two side effects.

Table II: Effect of rent cap on rental price growth

	Growth rate of rental price between t-1 and t					
	(1)	(2)	(3)	(4)	(5)	(6)
	Symmetric	Symmetric	Symmetric	Asymmetric	Asymmetric	Asymmetric
β_1	0.322^{***}	0.330^{***}	0.336^{***}	0.302^{***}	0.309^{***}	0.309^{***}
	(0.028)	(0.032)	(0.034)	(0.034)	(0.038)	(0.035)
β_2	-0.383^{***}	-0.396^{***}	-0.396^{***}	-0.437^{***}	-0.449^{***}	-0.441^{***}
	(0.015)	(0.020)	(0.022)	(0.018)	(0.016)	(0.016)
β_3				0.491^{***}	0.498^{***}	0.500^{***}
				(0.063)	(0.068)	(0.067)
β_4				0.062	0.050	0.067
				(0.075)	(0.069)	(0.079)
δ	-0.086^{***}	-0.086^{***}	-0.088^{***}			
	(0.013)	(0.013)	(0.014)			
$I(P_{t-1} < \bar{P})$		-0.024	-0.035		-0.037	
		(0.023)	(0.024)		(0.024)	
$I(P_{t-1} \geq \bar{P})$		-0.017	-0.030		-0.032	
		(0.021)	(0.021)		(0.022)	
(log) Size in m^2	-0.011^{***}	-0.011^{***}	-0.011^{***}	-0.009^{***}	-0.009^{***}	-0.009^{***}
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Duration			0.001^{***}			0.001^{***}
			(0.000)			(0.000)
Constant	0.201^{***}	0.222^{***}	0.109^{***}	0.117^{***}	0.152^{***}	-0.011
	(0.021)	(0.036)	(0.030)	(0.020)	(0.026)	(0.012)
Time dummies pre	Yes	Yes	No	Yes	Yes	No
Time dummies post	Yes	Yes	No	Yes	Yes	No
Observations	26934	26934	26934	26934	26934	26934

Cluster standard errors at municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

IVB..1 Composition effects

The results where we estimate the effect of rental price caps on rental prices (or excess prices) suggest that composition effects might explain half of the price decline caused by the policy, particularly in larger municipalities. We have some data that can help us to directly look into whether the characteristics of units in the market changed in response to the policy.

We can investigate this point by using as dependent variable the size of the unit in Equation (2). We show these results in Table III. The first two columns show that the policy lead to a small decline of around 1 percent in the size of the units in the rental marked. This is consistent with the idea that with the policy, bigger apartments were removed from the rental market, which can be indicative of composition effects. The price results that control for unit fixed effects suggest that this effect is driven mostly by larger municipalities. We investigate this point in Columns 3 and 4. Column 3 restricts attention to municipalities with fewer than 40,000 inhabitants. The point estimate drop to zero. The fact that the policy does not seem to affect one of the characteristics of the units in the market for rentals is consistent with the finding that the price effect is similar with and without unit fixed effects, among these municipalities. In Column 4, instead, we allow restrict to municipalities in the treated group with more than 100,000 inhabitants. In this case the point estimate is larger and more precisely estimate, which is, again, direct evidence that the composition of the units in the market changed in larger municipalities.

Table III: Effect of rent cap on the average size of rental units in the market

	(log) squared meters			
	(1)	(2)	(3)	(4)
Effect of Rent Cap	-0.008	-0.011	-0.002	-0.012
	(0.005)	(0.005)	(0.007)	(0.005)
Observations	513476	513368	123007	380224

Notes: This table estimates the effect of the policy change on the average size on units in the rental market. Column 1 uses the baseline specification. Column 2 includes flexible province trends. Column 3 restricts the regression to municipalities with fewer than 40,000 inhabitants. Column 4 restricts the sample to municipalities above 100,000 inhabitants among the treatment group. Robust standard errors clustered at the municipality level are reported.

These results point to some heterogeneity in how important composition effects are across municipalities. They suggest, in line with the evidence presented in Table I, that there is a substantial change in the composition of the units available before and after the policy, particularly in some segments of the market.

IVB..2 Overall supply

The literature argues that another potential impact of rent caps is the reduction of the overall supply of rental units in the market. Since the number of rental units are non-negative count data we run a Poisson TWFE model using a pseudo-maximum likelihood algorithm for multiple high dimensional fixed effects.[17] The specification for the number of contracts takes the following form:

$$P(y_{gt}|x_g, \mu_g) = \gamma_{gt}^{y_{gt}} exp(-\gamma_{gt}))/y_{gt}!$$
$$\gamma_{gt} = exp(\alpha_g + \lambda_t + \beta x_{gt}) = \mu_g exp(\lambda_t + \beta x_{gt}) \tag{5}$$

where y is the number of contracts, α_g are the fixed effects, λ_t are monthly time dummies and x are potentially explanatory variables.

Table IV presents the results of the estimation. 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 seems to be an increase in contracts below the reference price but this effect does not compensate for the drop in contracts above the reference price. The estimates are stable to the inclusion of flexible province trends and to restricting the estimation sample to municipalities below 40,000 inhabitants. The point estimates for new contracts below the price index becomes insignificant for this latter sample.

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

	Number of new contracts, PPMLE estimates								
		All			Above			Below	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Effect of Rent Cap	-0.102	-0.139	-0.145	-0.449	-0.393	-0.385	0.342	0.191	0.074
	(0.034)	(0.028)	(0.050)	(0.070)	(0.051)	(0.058)	(0.105)	(0.075)	(0.077)
Observations	24684	24684	8405	19008	19008	7106	21054	21054	7696

Notes: Columns (1), (4) and (7) is our baseline specification. Columns (2), (5), and (8) add province × month fixed effects. Columns (3), (6), and (9) limit our baseline specification to municipalities with fewer than 40,000. Robust standard errors clustered at the municipality level are reported.

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

Using the results reported so far, we can obtain estimates of the elasticity of new contract with respect to price changes. This is, Table I estimates by how much rental prices drop, while Table IV estimates by how much the amount of new contracts changes. Hence, we can estimate the rental housing supply elasticity using and instrumental variable estimates that takes the effect of the policy change on prices as the first-stage.

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

	(ln) Number of new contracts								
	Full Sample			Recent Sample			No lockdown		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(ln) Rental price	2.993	3.291	4.748	4.292	4.242	5.429	3.170	3.485	4.887
	(0.659)	(0.716)	(1.583)	(1.479)	(1.480)	(2.524)	(0.673)	(0.732)	(1.586)
Observations	11102	11102	8405	1936	1936	782	10601	10601	7998
fstat	46.277	41.367	16.720	13.504	12.790	7.138	48.086	43.056	17.352

Notes: Columns (1), (4) and (7) is our baseline specification. Columns (2), (5), and (8) add province × month fixed effects. Columns (3), (6), and (9) limit our baseline specification to municipalities with fewer than 40,000.

We report various estimates of the elasticity of the supply of rental units to rental prices in Table V. Given the results shown so far it should not be surprising to see that we obtain a positive estimate. In the baseline specification with the full sample, a decrease of 1% in rental prices implies a decrease of 2.99% of the supply of rental housing. We obtain a slightly larger point estimate when we allow for flexible province specific time trends and when we restrict the sample to municipalities below 40,000 inhabitants (Column 3).

In Columns 4 to 9 we present several alternative estimates of this elasticity that summarizes the effect of the policy change on both prices and the supply of housing. Columns 4 to 6 we remove from the estimation sample new contracts signed early in our sample period. Point estimates do not change substantially. In columns 7 to 9, we remove the three months where there was a lockdown in Spain (a period that starts in March 2019). Again, point estimates are similar when we remove these months.[18]

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

18. The lockdown reduced the volume of transactions as can bee seen in Appendix Figure A.1, but not differentially so between treated and control municipalities.

V Conclusions

In this paper we analyze the impact of the second generation rent control enacted in Catalonia in September of 2020. In contrast with previous research on second generation rent controls, which relies mostly on aggregated data or Internet platforms, we are using individual data of all the rental contracts during the period 2016-2021. Having detailed information allows dissecting the mechanisms that explain the results. We find that rent control reduce slightly prices and the supply of rental units despite the short period of time observed. First, we find that many contracts are signed with prices above the reference price, which denotes a clear inefficiency in the application of the regulation. Despite the sanctions included in the act there have been almost no penalties. This is not uncommon for this type of regulation, which leaves to the judicial system the enforcement of the legislation. For instance, the Californian system does not enforce the rules and tenants have to go to the court if they believe their rights are being violated. In Cambridge the ban that forbade landlords for leaving controlled units vacant for more than three months was never actively enforced.[19]

Second, the reference rental price is being used by owners who had rental prices below it to adjust their prices up jeopardizing the mitigating effect of the rental caps on high rents. The further away is a rent below the reference rent, the faster it increases. In fact, this effect is stronger in control areas than in treated municipalities. Third, rental prices seem to have gone down in the treatment area versus the control municipalities although the reason maybe associated with the fast increase in rents that were below the reference rent in control areas. Finally, the estimation shows that the number of contracts went down as a result of the rent control. The selection of units in the market explains 50% of the estimate average excess price decline but these composition effects are entirely concentrated in the large municipalities.

In a companion research we check the results of this paper using alternative methodologies, in particular synthetic difference-in-differences and spatial regression discontinuity designs. The results presented in this paper are very similar to those obtained using these alternative methodologies. We also analyze the duration of contracts and the effect of the rent control. Future research will also analyze the probability of rental unit to be sold before and after the application of rent controls as a potential mechanism to explain the reduction in the supply of rental units. We will also investigate quantile treatment effects in the difference-in-differences model. In addition, the Constitutional Court declared unconstitutional the rent control of Catalonia in a ruling of March 11, 2022. We plan to update our dataset to consider the impact of the end of the limitation on rental prices and supply of rental housing.

19. Landlords can also challenge the legislation producing a high degree of litigation. In the case of Berlin landlords went to court very often on disagreement about the calculation of the reference prices, collapsing the judicial system.

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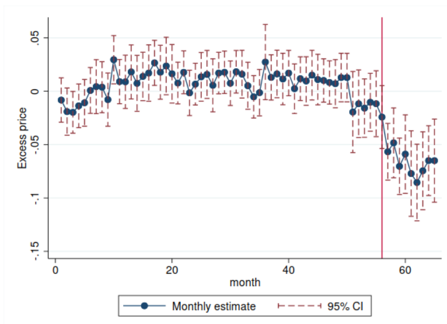
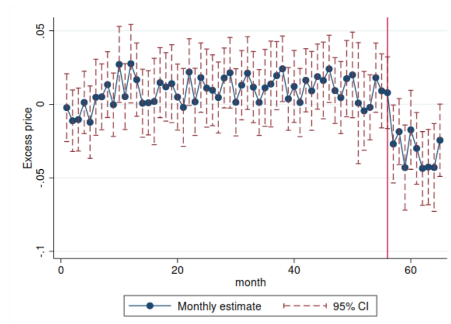
Figure V: Dynamic diff-in-diffs estimates on the effect of the policy on rental prices

Panel A: Outcome variable: Excess price

Baseline

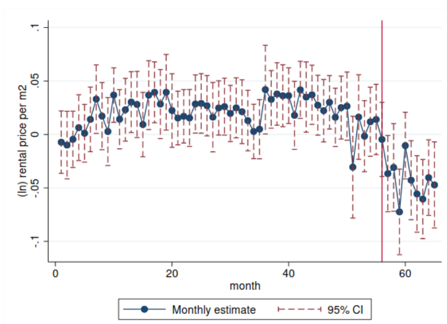


Controlling for province trends

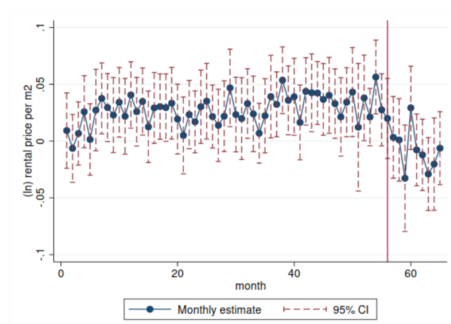


Panel B: Outcome variable: (ln) rental price per m2

Baseline

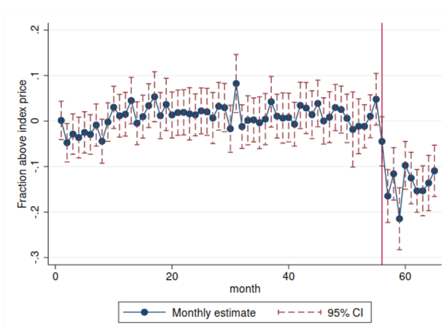


Controlling for province trends

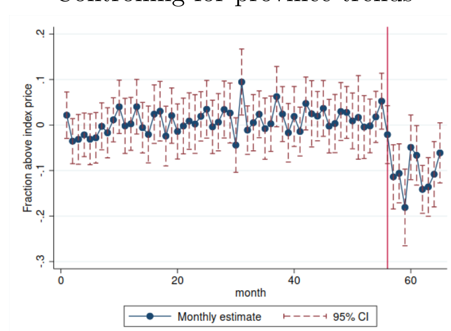


Panel C: Outcome variable: Fraction above index price

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 province specific flexible time trends. 95 percent confidence intervals of robust standard errors clustered at the municipality level are reported.

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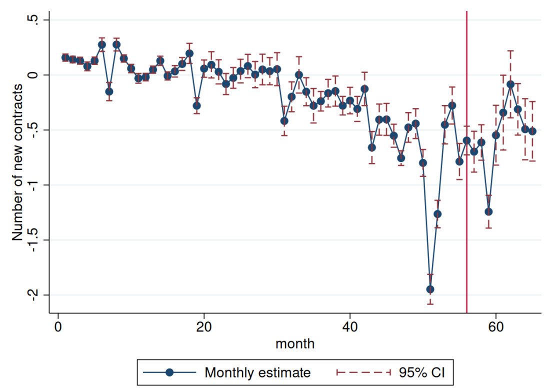
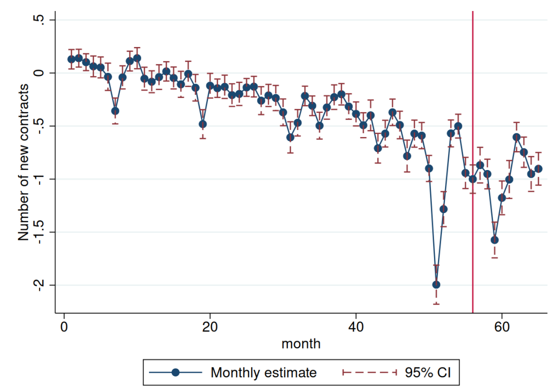
Figure A.1: Event graphs, second stage, separately treatment and control.

Panel A: Outcome variable: Volume of transactions below threshold

Treatment

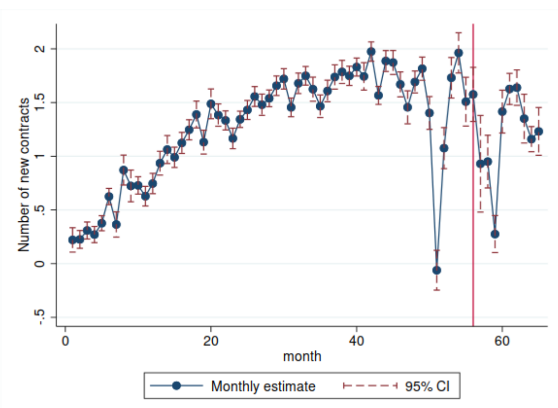


Control

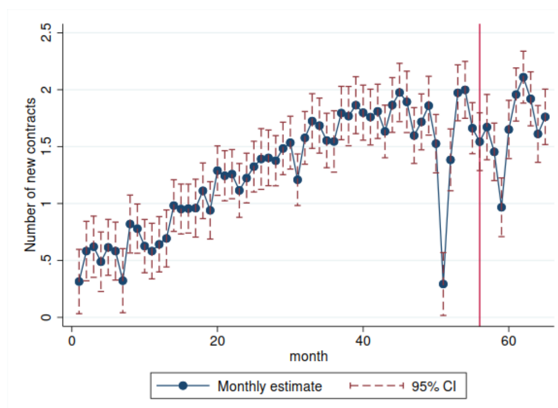


Panel B: Outcome variable: Volume of transactions above threshold

Treatment



Control



Notes:

Table A.1: 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 A.2: Heterogeneity-robust estimates of first-stage

	Excess price				(ln) rental price				Fraction above excess price			
	β	s.e.	β	s.e.	β	s.e.	β	s.e.	β	s.e.	β	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 the same results reported in Table I but using the estimator proposed in de Chaisemartin & D'Haultfoeuille (2020).