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

Joan Monras, Federal Reserve Bank of SF, UPF-BSE-CREI, and CEPR
Jose G. Montalvo, UPF-BSE-IPEG

This version: April 2023

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. As intended, 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 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 rationalizes the empirical facts. It also predicts that reference prices solve search friction inefficiencies, but lead to a decline in welfare of tenants in "low-price" units and a welfare improvement of tenants in "high-price" units.

Keywords: *rent control, reference price, housing supply, event study, directed search*

JEL classification numbers: D4, R21, R28, R31

*We are grateful to Antoni Sierra and his team of rental property security deposits for their assistance with the INCASOL data and to Lali Junyent at the Agència de l'Habitatge de Catalunya (AHC) for her guidance and invaluable help in obtaining the official data of the AHC. We are also grateful to Dani Villanueva (INCASOL) and David Fabian Rossi (AHC) for their technical assistance with the data. We would also like to thank Gregor Jarosch and Ezra Oberfield for very insightful discussions. We acknowledge the generous support of the Fundación BBVA (Scientific Research Projects in Social Science, 2022 Call) and the Spanish Ministry of Economy and Competitiveness, through the Severo Ochoa Programme for Centres of Excellence in RD (CEX2019-000915-S). Monras acknowledges the generous support of the Ramon y Cajal grant (AEI - RYC-2017-23682). Daniele Alimonti, Celeste Liu, Pablo de Llanos, and Schuyler Louie provided excellent research support. Corresponding author: Jose G. Montalvo, Universitat Pompeu Fabra, C/ Ramon Trias Fargas 25-27, Barcelona, 08005, Spain (email: jose.garcia-montalvo@upf.edu). The views expressed are those of the authors and do not necessarily reflect the views of the Federal Reserve Bank of San Francisco or the Federal Reserve System.

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 U.S. cities, including New York City, Washington, San Francisco, Boston, and Los Angeles, have also introduced various forms of rent control. In contrast, 36 U.S. states forbid rent control laws entirely.

Most commonly, the United States 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 countries have 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. As we discuss in more detail later, arguments against rent controls include the fear of distortions in the housing rental 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 and to weaken landlords' bargaining positions.

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 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.[3] Recent empirical evidence in the United States has either

1. This is a highly debated topic, both in economics and beyond. For example, sociological studies such as Desmond (2017) and Osman (2022) have discussed the problems of housing accessibility in the U.S.. Multiple views have emerged. Osman (2022), for instance, 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. Arnott (1995) defined second-generation rent controls as any mechanism that is not a rent freeze.

3. Arnott (1995) argued that rent control should be judged by the analysis of empirical evidence and, since programs are

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 prevailing rental price of the 25 closest units. The policy affected only some municipalities, providing us with additional sources of variation, as it allows us to compare various groups of treated and non treated municipalities. Moreover, unlike other studies, we have detailed housing unit microdata on rental housing in Catalonia. That is, we have information about each rental contract signed, its start date, its end date (in cases where the contract 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 several empirical results. First, we document that the policy led to price decreases in the rental market. On average, rental prices dropped 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 "excess prices" – i.e., the price relative to the reference price set by the government – 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 excess 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.[4]

As we discuss in more detail later, 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 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 around 10%, mostly explained by a large decrease in units available at prices above the reference price, which is not compensated by the increase in units below. These estimates allow us to compute an elasticity of rental unit supply, which we define as

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.

4. We apply some of the insights in Autor et al. (2015) and Cengiz et al. (2019), who study the effect of minimum wages along the wage distribution, to study the effect of rental price caps along the house price distribution.

$\frac{\Delta \ln(\text{New rental contracts})}{\Delta \ln(\text{Rental Price})}$, of around 2.[5]

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 the standard model, with homogeneous units, 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, can be viewed as the cost of redistributing welfare from landlords to renters.

Some of the evidence we find, however, does not fit well with this perfect competition framework. For instance, in perfect competition models with homogeneous housing, 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. Instead, we argue that the facts we document are best understood through the lens of a directed search model adapted to the particularities of the rental housing market.[6] 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-price and low-price units. As shown in the context of labor markets in Galenianos & Kircher (2009), in equilibrium this generates the coexistence of multiple submarkets in the rental market. Some landlords choose to post a high price, understanding that the chance of getting a tenant is lower. Some other landlords 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 submarkets. 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.

5. We discuss in detail below why we think that our measure of supply in the rental market accurately reflects changes in the total supply of housing units in the rental market. For instance, we document that there doesn't seem to be a change in the duration of contracts or in the time between contracts, suggesting that there is a fraction of rental units that leave the rental market, and hence, that new contracts signed is a good approximation of changes in the overall supply of housing in the rental market.

6. Directed search models deliver price dispersion in equilibrium, even with homogeneous housing. An alternative way to generate price dispersion is to assume that there are housing units of various qualities. We discuss this possibility at great length, both by introducing quality differences in our directed search framework and by discussing a perfect competition model with differentiated housing units in Appendix C. One empirical fact that points towards the directed search framework is that contract length is declining in excess prices, something that arises naturally in the directed search framework, and would be counterintuitive in perfect competition with quality differences.

The directed search model 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, as we observe in the data, even among units that are located very close to each other and share many common characteristics. Second, the policy leads to price convergence – one of the key empirical patterns that follows from the policy change. 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 with the perfect competition framework. We show that a rental reference price cap that collapses the various submarkets into a single one can be welfare improving. Intuitively, it prevents sellers from applying to multiple submarkets, which alleviates the externality caused by the fact that, with multiple submarkets, renters contribute to the queue length in submarkets where they may not end up participating. Furthermore, we characterize the range of reference prices that lead to welfare gains for both renters and landlords. The model suggests, moreover, that if we observe overall exit from the rental market (but still some units in the market) then the reference price set by the policymaker is at a price level that redistributes welfare from landlords to renters. We show that a policymaker concerned with alleviating the negative effects of search frictions and who wants both renters and landlords to benefit from the policy should set a reference price at intermediate levels of the price distribution. This policy is welfare improving ex ante; however, it hurts renters in already low-price matches and benefits renters already in high-price matches. We also show that the range of prices that allows welfare-enhancing policy interventions is determined by prevailing landlord costs of having units in the rental market and the overall demand for housing. Hence, while a reference price policy can alleviate search frictions in the short run, it cannot counteract long-run upward trends in prices driven by either low supply or strong demand for rental units.

Finally, we study whether quality differences can explain the patterns observed in the data and how they can be incorporated into our directed search framework. We argue that quality differences in perfect competition can explain various empirical facts reported in the paper, but they have a harder time explaining why we find, in the data, a strong negative correlation between excess price and contract duration – something that emerges naturally in directed search frameworks. With two qualities, a rental cap policy that is binding only for high-quality units reduces the number of landlords who supply high-quality units into the rental market. Some of the demand that would normally be satisfied within high-quality units then moves to the low-quality market, raising demand and, hence, leading to low-quality price increases. This framework results, again, in welfare improvements for tenants in high-price units, at the expense of high-quality landlords, with

ambiguous effects on the welfare of tenants in the low-quality market. These forces can be incorporated into our directed search framework in the exact same way as the perfect competition framework. The benefit of the directed search framework is that it can explain the patterns of contract duration observed in the data. While a binding reference price results in overall welfare losses in the perfect competitive framework, it can instead lead to ex ante welfare gains in search frameworks.

Overall, these results provide empirical support for the notion that reference prices are effective at reducing prices but also affect supply and welfare, potentially in richer ways than what the standard perfect competition with homogeneous housing framework suggests. In this paper, we contribute to the literature in two ways. First, we provide well-identified short run evidence on the effect of second-generation price controls using microdata. Second, we provide a novel framework to interpret the empirical evidence and to think about the welfare consequences of this type of policy.

Related literature

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

While there is little doubt that these types of policies help 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 units. 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) and Bulow & Klemperer (2012) among others.[7] 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. Landlords might decide not to invest in maintenance because they cannot recoup the investment when rental price growth is limited (Sims, 2007; Downs, 1988; Moon & Stotsky, 1993). Rent controls can also affect the relative size of the rental and home-ownership markets (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 and the neighborhood. For example, longer-term tenants

7. 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.

might develop specific social capital, such as a network of friends and family or proximity to a job or a local school, which may result in positive spillovers for the neighborhood. Hence, the risk of excessive rental price appreciation might be very costly for some households, which might be forced to leave the neighborhood, causing negative externalities. Most of these mechanisms, however, are likely to operate over the longer term. In the short term, rental caps probably affect housing supply, but the also have the presumably positive effect of weakening landlords' bargaining position, moving economic surplus from home owners to renters.

We contribute to this theoretical 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 homeownership 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 coexistence of various submarkets. We think that this relates well to the price dispersion observed in rental markets, even when one tries to account for many observable characteristics. Moreover, many of the empirical facts that we document align with either the assumptions or the implications of our model. We use our model to argue that standard results in the literature, such as the deadweight loss generated by rent control policies predicted by competitive models, may not apply in more realistic setups. Hence, our approach challenges some of the conventional wisdom in this literature and hopefully provides a novel and fruitful framework to think about rental policies.

More recently, the availability of reliable microdata at the level of individual rental units has allowed researchers to study rent control policies empirically in some more detail. In a seminal contribution, Sims (2007) analyzed the end of rent control in the Boston metropolitan area using data from the American Housing Survey. He found that rent control induced landlords to remove units from the rental market and lowered 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 rental price of noncontrolled units. In a similar vein, Autor et al. (2014) analyzed the impact of the unexpected elimination of rent control in Cambridge, Massachusetts, finding that the market value of properties no longer subject to rent control increased 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. Moreover, they study a third- rather than second-generation rent control policy.

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. This policy included regulated increases indexed to the Consumer Price Index (CPI) within tenancies, but 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 five 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 we tie our empirical findings to a novel model of the rental housing market.[8]

Our paper also contributes to the recent literature on 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 price distribution, and we provide a framework with which to think about our, and their, empirical results. Mense et al. (2019) and Mense et al. (2023) also analyze the introduction of second-generation rental caps in Germany, but in a setting where the policy does not apply to new construction. Their emphasis is slightly different. Mense et al. (2019) show that land prices grow faster in municipalities where rent caps are implemented than in the control group, while Mense et al. (2023) document a positive spillover effect of rent control on units not subject to rent caps within municipalities. Relative to these two papers, we provide evidence from a new policy change that affected all units within each municipality 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 use a purely empirical approach and aggregate instead of micro unit-level 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 this type of policy.

8. 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.

II Rent control in Catalonia

The Spanish housing market is characterized by a large proportion of homeowners. In 2011, homeowners represented 79% of the market, while renters amounted to 11%.[9] 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 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. However, the large increase in the demand for rental housing was 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 & Domiinguez-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 2014-2019) and the whole Barcelona metropolitan area (23.4% rental price increase).

To contain the increase in prices and following the experiences of some European cities, such as Berlin and Paris, the Parliament of Catalonia approved, quite unexpectedly, a system of rent ceilings that was implemented at the end of September 2020.[10] 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 more than 20% during the period 2014-2019, which, as we show below, comprise many municipalities around Barcelona plus others dispersed over the territory. It is worth mentioning, however, that despite these considerations, there don't seem to be large differences in price trends between stressed and non stressed municipalities, especially when we compute prices relative to the reference price, as we show later.

The Catalan law imposed second-generation rent caps, limiting rent increases within and between tenancies.[11] After the approval of the law, reference prices became the highest prices that the rental price per square meter could reach, at least on paper. To construct the reference price for each unit, the pool of comparison units is stratified by size in two groups: units above and below 90 square meters. That is, if the size of a unit is larger than 90 square meters, then the reference price was calculated as the mean of the 25

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

10. Act 11/2020 of September 18, 2020. The law passed unexpectedly on September 8, 2020, after the center-right party Junts per Catalunya, which traditionally opposed these type of policies, voted in favor. See https://elpais.com/cat/2020/09/09/catalunya/1599663147_337105.html.

11. The Spanish regulation already capped rents within tenancies using CPI inflation.

closest units between 90 and 250 square meters. For units of less than 90 square meters, the reference price is calculated as the mean of the 25 closest units with a size plus/minus 10 square meters. 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 were not affected by the policy change. It is worth noting that an important difference between this policy and the one studied in Mense et al. (2023) is that, in their case, new units were not subject to controls. This allows the authors to study a situation where part of the market is affected by a rental cap policy and part is not, even within municipalities. Hence, their focus is on spillovers.

III Data

Our data cover more than half a million rental contracts reported to INCASOL from the beginning of 2016 until June 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 for setting the methodology to calculate the reference price for each rental unit. Our merged data has information on the closing price of each rental contract, the start date of the contract, the end date (if it had already ended), the area in square meters of the unit,[12] and the reference price per square meter.

One key feature of these data is that we have information about rental market prices, rather than the asking price or an imperfect measure of the agreed price. Hence, we can measure potential price effects more accurately. 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., 2019, 2023). These data are also more accurate than the online price data used sometimes in other research that 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.[13] 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 the rental contracts of units in municipalities that were subject to the rent cap "treated" and call our control group, the ones that were not subject to the

12. We have the habitable surface, which is a better indicator than the built surface.
13. Online prices correspond to Idealista, which is the leading online real estate intermediary in Catalonia.

rent control cap, as "non-treated". 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. The major thread to our identification is systematic differences in the trends of the outcome variables of interest. To mitigate these concerns, as we discuss in what follows, we show data many months prior to the policy change.

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 explained before, the rental cap implied that rents could not exceed the reference price computed for each particular unit. Hence, we can define the *excess price* as:

$$\text{Excess price}_{i(m),t} = \ln\left(\frac{p_{i(m),t}}{\overline{p}_{i(m),t}}\right)$$

where $p_{i(m),t}$ denotes the rental price per square meter in unit i, located in municipality m, at time t, and where $\overline{p}_{i(m),t}$ is the price cap calculated for this housing unit based on the surrounding units in that time period. The excess price measures 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.[14] Using this information we can construct excess price 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 of the figure shows the same histograms for the control group.

There are several things to note from Figure III. On the one hand, we see that the histogram of prices among treated municipalities is "distorted." In treated municipalities, there is a much greater mass of units with prices immediately below the cutoff and even close to but just above it. This outcome 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 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 not necessarily the exact consequences that were intended by the policy. For instance, it seems clear that not all the new contracts complied with the legislation. Many new contracts were signed at prices above the reference price. 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 between treated and control municipalities and what part might be a consequence of the policy change. To examine this point further, 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, when the policy was still not in place, and the graphs on the right show September and October 2020, the first month with the new rental cap policy. Panel A includes the contracts in the treated municipalities, while Panel B, shows the distributions of rental prices for the contracts in the control areas.

Comparing figures on the left and on the right indicates that the control municipalities do not show

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

much difference in rental prices before and after the policy change. However, in treated areas there is a clear movement of the distribution of excess rents to the left. It is also interesting that, after the policy change, almost half of the rental prices are greater than the corresponding reference price, which seems to suggest that landlords found ways to charge renters a price greater than the reference price by finding loopholes in the regulation or through informal agreements with tenants. Alternatively, some tenants may not have been aware of the policy change. The narrower time window makes the treatment and control graphs in the control group much more similar.

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 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 or 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 before the treatment. This condition is usually checked visually by running dynamic difference-in-differences specifications such as the following – ideally with multiple time periods prior to the treatment:

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

where $y_{i(m),t}$ is an outcome of interest, such as the excess price defined earlier, 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 β_k estimate the differential effect of the treatment at various points in time. In this and subsequent equations, i indicates units, m municipalities, and t time – usually at a monthly frequency.

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 afterward. The three outcome variables that we consider are the excess price (i.e., the log ratio of the rental price divided by the 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 – i.e. non parametric trends common for a larger geographic unit that groups various municipalities.

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 zero and are never significantly different from zero. We get very similar patterns when we include 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 province trends, the drop in prices after the implementation of the rental cap is of the same magnitude.

Panel B of Figure V shows the results for the rental price per square meter. In this case, we observe a slight upward trend in the price per square meter in treated relative to control municipalities. This is what we would have expected from a policy that tries to reduce housing prices in places where rental prices are increasing. Still, the differential trend is small, and even smaller when we control for province flexible trends. In contrast, the drop after the policy change is sharp and substantial. Comparing these estimates to the ones in panel A suggests that a benefit of using excess price rather than price per square meter as the outcome variable is that the computation of the reference price effectively removes the differential trends between treatment and control.

Panel C of Figure V shows yet another way to measure whether the policy was effective at reducing rental prices. In this case, the dependent variable is the fraction of rentals that are above reference prices in each municipality. Again, trends leading to the policy change are remarkably parallel between treatment and control municipalities, and the effect of the policy is clearly visible.

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

$$y_{i(m),t} = \delta_m + \lambda_t + \beta Rent\ Cap_{m,t} + \varepsilon_{i(m),t} \tag{2}$$

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 includes only municipality and month fixed effects. Column 2 includes province times month fixed effects, which allows for each of the four Catalan provinces to evolve flexibly over time.[15] Column 3 includes rental unit fixed effects. Hence, this column identifies the effect of the policy by comparing the outcome of interest for the same unit before and after the policy change. Columns 4 and

15. 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.

5 repeat the specifications in columns 1 and 3 but with only 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. Finally, column 6 repeats the estimation of column 2 but using quarterly instead of monthly fixed effects.[16] Standard errors reported in Table II and in the rest of the paper are always clustered at the municipality level to account for serial correlation.

Panel A presents the results for the specification on excess of prices. All of the specifications show that rent caps reduced excess rental prices. Excess prices are estimated to decrease by between 4% and 8%. Several things are worth noting. First, when we include flexible province trends, the point estimate is only slightly lower 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.[17] Estimates fluctuate between -0.08 and -0.037.[18] Moving from column 1 to column 2 shows that differential province-specific trends may be a larger concern when studying rental prices than deviations of those from the reference price, as one could expect from the fact that the reference price removes part of the differential trend. Moving to column 3, we again observe that the point estimate is substantially larger when conditioning on unit fixed effects.

Panel C shows another way to look at the data, akin to panel C of Figure V. For each municipality-month pair, in this panel we compute the fraction of housing units that is above the reference price. The estimates corroborate the evidence presented in Panels A and B. The decline in the fraction of units with prices above the reference price, which was around 50% before the policy, fluctuates between 13 and 22 percentage points

16. Appendix AA. shows additional graphs for quarterly frequency.

17. 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.

18. 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%.

– both statistically significantly different from 0 and from 50% –, depending on the types of comparisons in each specification. Hence, again, the policy was followed to some extent but not fully. Noncompliance with the policy is estimated to be as 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 perhaps smaller than intended, since a substantial fraction of new rentals still charged prices above the reference price. As mentioned before, earlier theoretical papers have expressed concerns about these types of policies for 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 housing supply by running an ordinary least squares (OLS) and a Poisson model (PPML), using a pseudo-maximum likelihood algorithm for multiple high-dimensional fixed effects, following equations (1) and (2). Our dependent variable is the (log) number of new contracts signed in each municipality each month.[19] Hence, this measure of volume is a flow measure. The main reason for using this variable rather than the stock of rentals in the market is that we have good data on the new contracts signed, but we lack data on the initial stock in the market at the beginning of our sample.

Before presenting our main estimates of the effect of the policy on the housing supply, we investigate whether using a flow measure for the supply of rental units into the market is, potentially, a concern. Figure VI reports the number of months between one rental contract and the following one in treatment and control municipalities both before and after the policy change, as a function of the excess price. The figure shows that there is a slightly positive correlation between the excess price and the time elapsed that the unit was vacant. The difference between the cheapest units and the most expensive ones is around one month. This declines somewhat in the post-treatment period (mostly because we have fewer months of data), but this decline is not different in treatment and control municipalities. We present various formal regressions on this

19. It is worth noting that when we use OLS, we miss roughly 50% of the observations, namely, the municipality - month pairs with zero new contracts. PPML estimates take into account this potential concern by including the zeros in the estimation. Montalvo (1997) discussed the use of pseudo-maximum estimation procedures for dynamic count data models.

point in Table A.2, in the Appendix. This analysis suggests that using a flow rather than a stock measure for the supply of rental units in the market is fine, since we do not observe big differential changes in the contract duration between treatment and control municipalities around the policy change.

In Figure VII we show the evolution of the volume of new contracts for rentals, both in total and 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 20%. The graph that shows the evolution of rental contracts above reference prices declines by substantially more than this 20%. In contrast, the series for the new contracts below reference prices experiences a substantial increase after the policy change in treated relative to control municipalities. This increase is smaller, and hence does not compensate, for the drop in the volume of new contracts above the reference price, explaining the overall drop in the graph showing the effects on the overall volume of new contracts.

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 subperiods. 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. That 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 instrumental variables (IV) estimates. Given the results shown so far, it should not be surprising that we obtain a positive estimate when using the policy change as an IV 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.

IVC. Distributional changes

While the previous section suggests that the supply of rental units is upward sloping, it only hints at the idea that the overall distribution of housing units in the market might have changed with the rental cap policy. We perform two additional exercises to investigate this point in more detail.

First, we group all the contracts in our data 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 excess prices fully comparable across time and space.[20]

Once we have these six groups, or sextiles, which summarize the entire distribution of excess prices, we plot in Figure VIII the change in prices and quantities, before and after the policy change, for each of these six groups. The price change results are shown in the graph on the left of the figure, Panel A. 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.

20. In practice, it doesn't matter much whether we condition on time and location fixed effects, because we are essentially normalizing the rental prices by the reference price, hence excess prices are comparable across time and space. This approach is inspired in the work on minimum wages by Autor et al. (2015) and Cengiz et al. (2019).

In the right-hand graph of Figure VIII, in Panel A, we investigate whether the policy induced changes in the overall number of monthly contracts along the excess price 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 VII and Table III). These results indicate that high-price units "disappear from the market" in treated relative to control municipalities.

Panel B of Figure VIII repeats the exact same exercise as panel A, but assumes that the policy took place one year prior to the true implementation of the policy (and using the same post-treatment period length, but one year earlier). Hence, it provides placebo estimates that should reassure that the effects estimated in Panel A are a consequence of the policy and are not driven by existing pre-trends in different parts of the distribution. Both the left and the right hand side graph show that price and quantity changes were similar in treatment and control municipalities along the entire excess price distribution prior to the implementation of the policy.

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.

IVD. Lack of spillovers

Arguably, the most important result in Mense et al. (2023) is the evidence of spillovers from units facing price caps to those that did not. In this section, we investigate whether there are also spillovers in our context, although in our case across rather than within municipalities. We do so using two strategies. First, we examine whether there is a gap in excess prices before and after the reform in rental units in treated municipalities that share a border with non treated ones. Figure A.7 shows that there is no gap in excess prices at the border prior to the policy reform. After the policy, a gap opens for units on each side of the border. With spillovers from treated to non treated municipalities, one would expect the gap around the border to be larger than suggested by the difference-in-differences estimates (as long as demand moves to non treated municipalities). We estimate a gap of similar magnitude than with our difference-in-differences estimates, which indicates an absence of spillovers. Our second strategy is to compare the price and volume changes for units in non treated municipalities but close to treated ones, relative to non treated

municipalities that are far from the treatment. Figure A.8 provides evidence which is, again, consistent with minimal spillovers between treatment and control.[21]

V Model

Up to this point, we have presented 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 (excess) prices and supply. Prices converged to reference prices in treated municipalities, 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 with homogeneous housing, 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 many units landlords 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 landlords to renters (who stay in the rental market), as shown graphically in the Appendix BA..[22]

While the standard neoclassical perfect competition model with homogeneous housing may provide a useful benchmark, there are several aspects of the rental market and of the evidence presented above that do not fit well into the framework. First, we see substantial heterogeneity in prices across units in the market, whereas in the neoclassical framework there is a unique price. This has been observed even when conditioning on a large host of observable characteristics (as can be observed, for example, in Bierdel et al. (2022)), or when deflating prices by rental prices of nearby units as we do with our measure of excess price, which controls in great detail for location and other characteristics at a granular level. Second, the results indicate interesting effects of the policy along the entire distribution, both in terms of price and supply changes. A

21. In Figure A.4 in the Appendix, we investigate whether there is any heterogeneity in our results for units where we observe multiple contracts and for units where we only observe one contract in our data. We show no meaningful difference in the estimates, in line with the idea that there was no scope for spillover effects in the case of our policy given that reference prices were applied to all units in treated municipalities.

22. We show a graphical representation of the perfect competition model in Appendix BA.. 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.

simple 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 this simple neoclassical benchmark. When people search for rental units, it is common to observe the prices posted online. It is also common to look for several options: low price units that are attractive but where the 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 meet the landlords of a unit or go through an application process.

This search process resonates strongly with the assumptions behind directed search models used mainly to study labor markets. Motivated by these facts, 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. One key feature of these models is that, as we will see, they generate price dispersion in equilibrium, which is a prevalent empirical regularity in the data.

An alternative way to generate price dispersion is to incorporate unobserved housing characteristics. We address this point in two ways. First, in section VE. we expand the baseline model to include quality differences and we discuss how our main results would change.[23] Second, and to help gain intuition on the results discussed in Section VE., we discuss in Appendix C, a perfect competition model with two qualities.

Ultimately the question is what generates price dispersion. In the data we see that contract length is strongly negatively correlated with excess price while the time between contracts is strongly positively correlated with excess price, as shown in Figures VI and A.5. These two facts emerge naturally in the directed search framework and it would be harder to rationalize them in perfect competition. This is why we present the directed search model in the main text and we relegate to the Appendix C the discussion of a perfect competition model with quality differences.

23. Rekkas et al. (2021) also introduces unobserved heterogeneity into a directed search model. The main difference between their approach and ours is whether quality is observed or unobserved when buyers apply to units. They show that when quality is completely unobserved, this does not change the fundamental insights of directed search models. In that case, the submarkets are defined by quality price pairs. We study instead the case when quality is observed by the applicants ex-ante, for instance because there are high- and low-quality online portals.

VA. Model setup

There are two types of agents in the model: unit owners or landlords 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, so $N = N_b/N_s$. All buyers and sellers are ex ante homogeneous.

We denote by $\alpha(n)$ the probability that a mass n_s of sellers 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 multiple prices are posted in equilibrium. 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 (ex post) payoff for buyers to rent at price p is given by $u - p$, while the payoff for sellers is $p - c$.

Matches are determined by a constant return to scale (CRS) matching function $\mu(n_b, n_s)$. We can define the probability that a seller meets a buyer, $\alpha(n)$, using the matching function: $\alpha(n) = \frac{\mu(n_b,n_s)}{n_s} = \mu(n, 1)$. We can also define $\frac{\alpha(n)}{n} = \frac{\mu(n_b,n_s)}{n_b} = \frac{\mu(n,1)}{n}$, which 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 and sellers.

VB. Multiple submarkets

We assume that buyers apply to two different types of units.[24] Sellers, instead, can only post one price. We denote by $\underline{p}$ and $\overline{p}$ the prices associated with each of the different submarkets and by $\underline{n}$ and $\overline{n}$ the ratio of buyers to sellers in each submarket.[25] Hence, the number of buyers in each submarket is equal to the total number of buyers, i.e. $\underline{n}_b = \bar{n}_b = N_b$, whereas the number of sellers in each submarket is determined in equilibrium.

With multiple submarkets, the value of the buyers can be expressed as:

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

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

24. More generally, we could consider buyers applying to an arbitrary number of different types of units, but using two types simplifies the model, the exposition, and the intuitions that we describe in what follows.

25. Galenianos & Kircher (2009) show that there are as many submarkets as buyer applications.

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 meets neither a low- nor a high-price seller, and hence obtains her outside option, which we assume to be equal to 0.

With multiple submarkets, sellers need to decide whether to post a high or a low price. We can start the analysis by looking into the optimization problem of sellers considering the high-price submarket. 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 sellers obtain a payoff equal to $(\overline{p}-c)$. We also assume that sellers can internalize the demand they face given the price they decide to post.

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

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

Equation (4) is easy to interpret. It says that sellers who decide to post in the high-price submarket, 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 when posting in the high-price submarket is given by:

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

We can now turn to sellers considering the low-price submarket. 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 submarket 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 submarket.

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

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

It is worth noting that $\underline{p} < \overline{p}$, since we can use the fact that $V_{\underline{s}} = V_{\overline{s}}$ to obtain $\frac{\overline{p}}{\underline{p}} = \frac{\alpha(\underline{n})}{\alpha(\overline{n})(1-\frac{\alpha(\underline{n})}{\underline{n}})}$. The numerator on the right-hand side, which is the probability a seller meets 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 one in the low-price market. Otherwise, there would be more entry in the high-price market, which delivers a higher payoff to sellers.

VC. Free entry

Entry conditions have two margins. First, landlords who participate in the rental market need to be indifferent between posting in the high- and the low-price submarkets. This leads to the following indifference condition, which results from equating Equations (5) and (7):

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

The second margin of the entry decision is whether landlords want to participate in the 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 or to enter short-term rental contracts, with payoff $k_s(N_s)$. We assume that this payoff is 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 submarkets needs to be equal to the outside option $k_s(N_s)$:

$$\alpha(\underline{n})(1-\varepsilon(\underline{n}))(\underline{u}-c) = \alpha(\overline{n})\left(1-\frac{\alpha(\underline{n})}{\underline{n}}\right)(1-\varepsilon(\overline{n}))(u-c) = k_s(N_s) \tag{9}$$

Equation (9) defines a system of two equations and two unknowns, $\underline{n}$ and $\overline{n}$, that has a unique solution, as shown in Galenianos & Kircher (2009).

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 can prove the following proposition.

Proposition 1.

1. A policy that forces a unique reference price $P \in [c, u]$ results in the following:
   (a) Prices converge to P.
   (b) The policy increases total welfare.
   (c) The policy is ex ante welfare-improving for buyers and sellers if and only if:
   $$(1-\gamma)c + \gamma u < P < (1-\eta)u + \eta c$$
   for $\gamma > 0$ and $\eta > 0$, defined in the appendix BB..
   (d) Ex post the policy is welfare improving or welfare declining depending on whether the reference price is above or below existing prices in each submarket.
2. A policy that forces a unique price $P \notin [c, u]$ destroys the rental market.
3. Overall supply in the rental market is affected by the policy as follows:
   (a) If $P < c$ then all landlords exit the rental market.
   (b) If $c < P < (1-\gamma)c + \gamma u$ then some landlords exit the rental market.
   (c) If $P > (1-\gamma)c + \gamma u$ then more landlords enter the rental market.

Proof.

See Appendix BB., for a detailed proof. ■

This proposition's implications allow us to speak to several policy debates. First, when a reference price is introduced, prices converge to the reference price, as we observed in the data. There is a range of prices, determined by the utility derived from housing and the cost of having a unit in the market, where the policy is (ex ante') welfare improving. This is so because the policy removes the inefficiency created by the fact that renters who end up in high-price units also queue in low-price ones, making matching more costly. Furthermore, there is a range of prices where the policy is welfare improving for both renter and landlords. Proposition 1 characterizes the (smaller) range of prices where this happens.

The proposition shows that, if the reference price P is lower than $(1-\gamma)c+\gamma u$ but still above c, then landlords lose, while if the reference price is higher than $(1-\eta)u+\eta c$ but still below u, renters lose. Hence, there is a range of prices that does no destroy the market and that favors either landlords or renters.

The reference price set by the policymaker has effects on the overall size of the market. A policy that sets the price P in the range $(c,(1-\gamma)c+\gamma u)$, leads to lower entry in the rental market since it lowers the value of having units in the rental market. In that case, high-price matches are the ones more likely to break. A reference price above $(1-\gamma)c+\gamma u$ encourages entry.

Proposition 1 can be useful to guide how we can think about rent cap policies more broadly. It says, for example, that rental cap policies need to adjust to the conditions in the market. That is, if demand in the market is very high, which can be thought of as high u, then welfare-improving price caps for both renters and landlords cannot be set too low. Similarly, if it is costly for landlords to have their units in the market, then price cap policies cannot be set too low. This also means that, if either the cost of supplying units to the rental market increases (i.e., higher c) or the demand for rental prices increases (higher u) – both potential drivers of higher prices – then a policy that fixes rental prices will lead to short-run price convergence and (ex ante) welfare gains that will erode over time. For example, if c increases, the policy would first lead to landlord welfare losses (as P leaves the range defined in the proposition) and will then destroy the market (when P leaves the range $[c,u]$). Similarly, if u increases, then there will be a point where landlords will start to lose in terms of welfare and will begin exiting the market.

Finally, the proposition also says that ex post, renters who were paying low prices will be worse off while renters in high price matches will be better off (and the reverse for landlords). Ex-post, i.e. after matches are formed, landlords who managed to form a match at a high-price, and renters in low-price units do not have incentives to change the outcome of their matches. Or, to put it differently, upon the introduction of the policy, renters who ended up in high-price units will have strong incentives to look for new rental units.

All these results can be summarized in a graph, as shown in Figure IX. This graph provides an example where $\overline{p}$ is above $(1-\eta)u+\eta c$, and where $\underline{p}$ is below $(1-\gamma)c+\gamma u$ (something that does not necessarily need to be the case, but is the most plausible ordering).[26] In this case, we define six regions where the policymaker could set the price P (without destroying the market). A reference price P set in region A would be good for landlords, who value high prices, but would be detrimental to renters (both ex ante and ex post), even for those initially in the high-price submarket. Region B is similar to region A, except that in this region the price P is lower than the prevailing price in the high-price market, so renters matched in the high-price submarket would have incentives to find a new match. Region C is ex ante welfare improving for both landlords and renters. In this region the post-policy average price would be higher than prior to the policy. Region D is like region C except that the policy leads to a lower average price, something that is in line with the data. In this region, however, we would observe new entrants into the rental market, something that we did not observe in the data. Region E is ex ante welfare improving for renters, but ex ante detrimental to landlords. In this region we should observe overall exit in the rental market. In this case, however, renters in the low-price units would have incentives to stay in the match, and landlords in the low-price submarket would have strong incentives to stay in the market. This means that we would see matches in the high-price submarket disappear. Hence, this region is in line with all the empirical results discussed above. Finally, Region F is similar to region E, except that even matched landlords in low-price units would lose and hence would have incentives to leave the market, something that we do not observe.

Overall, we think that this model provides a simple framework that can explain several empirical facts that a perfect competition model cannot explain. First, the model generates price dispersion in equilibrium. Second, introducing a reference price leads to price convergence and a reduction in the supply of high-price units. This model also suggests more nuanced welfare consequences than the standard perfect competition model with homogeneous housing.

VE. Quality differences

For simplicity, we assumed thus far that housing units are homogeneous. This assumption simplifies the exposition of the model and is adequate if reference prices correctly account for any unobserved heterogeneity. In practice, however, there may be quality differences that are hard to take into account when computing reference prices. For example, within one block of apartments, one unit may have a nicer view than another one, or may have been maintained in better condition. This may matter for the rental price and will not

26. Figure A.9 in the Appendix explores a reversed ordering. The empirical evidence provided in the paper rules out this situation.

be taken into account when computing the reference price for each unit. For this reason, in this section we introduce quality differences into the model and discuss the implications that this would have for our main results.

For simplicity we assume that there are two qualities, high and low. We index quality by $q \in \{High, Low\}$. Using this notation, we denote the payoff of living in a high-quality unit by u^h, while the payoff for a low-quality unit is u^l. We use similar notation for all the other variables in the model. We assume $u^h > u^l$ and $c^h > c^l$.

One option would be to have two types of buyers, one that is only interested in high-quality units and the other only interested in low-quality ones. This would result in equations that mimic the ones in the previous sections. This assumption, however, would break any relationship between the demand in the high- and low-quality markets. It is conceivable that someone would want to search in the high-quality market only if prices are not too high. This is why we consider heterogeneous preferences over quality, parameterized by an idiosyncratic taste shock, denoted by ϵ. In particular we assume that the utility of an individual i for looking into the market of quality q is given by:

$$\ln V_b^q + \epsilon_{iq} = \ln[\frac{\alpha(\underline{n}^q)}{\underline{n}^q}(u^q - \underline{p}^q) + (1 - \frac{\alpha(\underline{n}^q)}{\underline{n}^q})\frac{\alpha(\overline{n}^q)}{\overline{n}^q}(u^q - \overline{p}^q)] + \epsilon_{iq} \tag{10}$$

We further assume that ϵ is drawn from an extreme value distribution such that a fraction of the buyers search in the high-quality market and the rest in the low-quality market (imagine there are two online platforms where people apply). Then:

$$N^q = \pi^q N = (\frac{V_b^q}{V_b})^{\frac{1}{\lambda}} \tag{11}$$

where $V_b = [(V_b^l)^{\frac{1}{\lambda}} + (V_b^h)^{\frac{1}{\lambda}}]^{\lambda}$, in a slight abuse of notation relative to the previous sections.

Finally, we assume that sellers either have high- or low-quality units, i.e., we abstract from investments that may allow for upgrading the quality of a unit, and that sellers take the number of buyers in their market N^q as given. Under these assumptions, we have the same equations as in the previous section, but all depending on each quality market. On top, we have equation (11) that governs the overall size of each quality market. In particular, there will be four equilibrium prices in the market:

$$\underline{p}^q = (1 - \varepsilon(\underline{n}^q))\underline{u}^q + \varepsilon(\underline{n}^q)c^q \tag{12}$$

and,

$$\overline{p}^q = (1 - \varepsilon(\overline{n})^q)u^q + \varepsilon(\overline{n}^q)c^q \tag{13}$$

for $q \in \{High, Low\}$.

The logic of Proposition 1 applies in this model with quality differences. First, in equilibrium, each quality market can be divided in two submarkets. Depending on the values of u^q and c^q, we may have overlap across markets. That is, it may happen that the high-quality market has uniformly higher prices than the low-quality market, or that the low-price submarket of the high-quality market has in equilibrium lower prices than the high-price submarket of the low-quality market. In equations:

$$\text{Either } \underbrace{\overline{p}^h > \underline{p}^h > \overline{p}^l > \underline{p}^l}_{\text{Case 1}} \text{ or } \underbrace{\overline{p}^h > \overline{p}^l > \underline{p}^h > \underline{p}^l}_{\text{Case 2}}$$

With quality differences, a reference price P that does not distinguish between qualities will have similar effects as the ones described in Proposition 1, except for the fact that the policy is more likely to be welfare enhancing for buyers (and more likely to be welfare declining to sellers) in the high-quality market. This moves demand across qualities according to equation (11). We provide more intuition about these forces in a simpler model in Appendix C. Overall, however, adding quality differences means that if a policy makes one particular quality market more attractive, there will be some demand that moves toward that market, and conversely, if a market becomes less attractive, demand will move away from that market towards the other one and towards the outside options.

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 10%. 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.

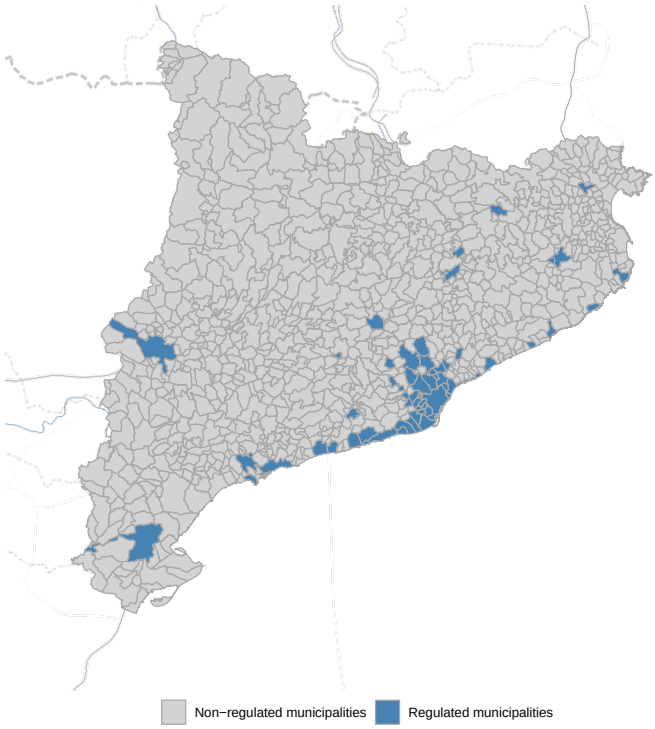
Finally, we argue that a directed search model is useful to interpret the evidence and provides a useful framework to study welfare consequences. Overall, we think that this paper can inform current policy debates. Many cities in developed countries are experiencing high rental prices. High rental prices can be due to either high demand for living in these cities or market inefficiencies - including limited entry, market power, or information frictions. We think that the evidence provided in this paper suggests that while search frictions can explain price dispersion, high prices are probably related to either high demand for rental units or high costs for landlords to put their units into the rental market.

VII Figures

Figure I: Comparison between online posted prices and market prices

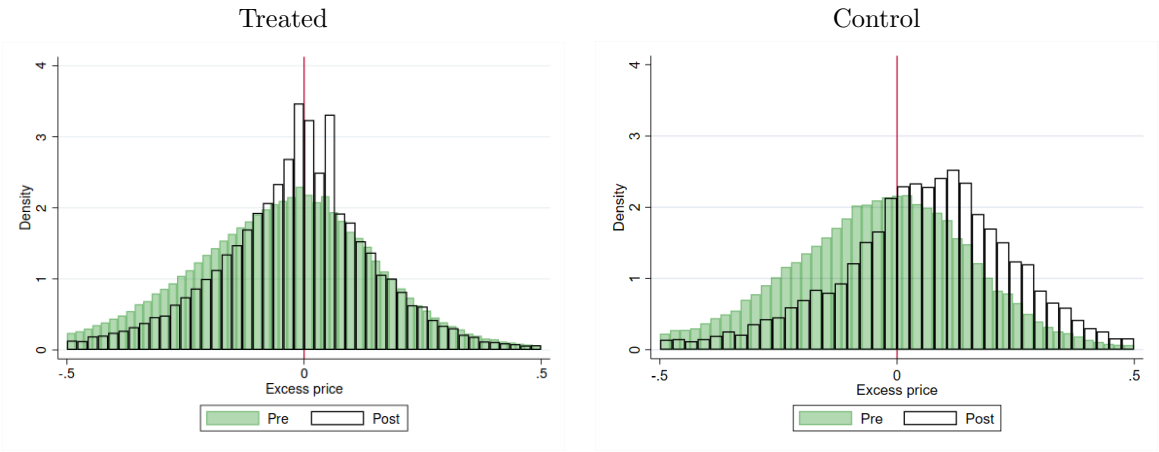
Notes: The top graph shows the level of rental market prices and online posted prices. The bottom graph shows the growth rate of rental market prices and online posted prices. Sources: INCASOL and Idealista.

Figure II: Map of municipalities affected by rental price caps



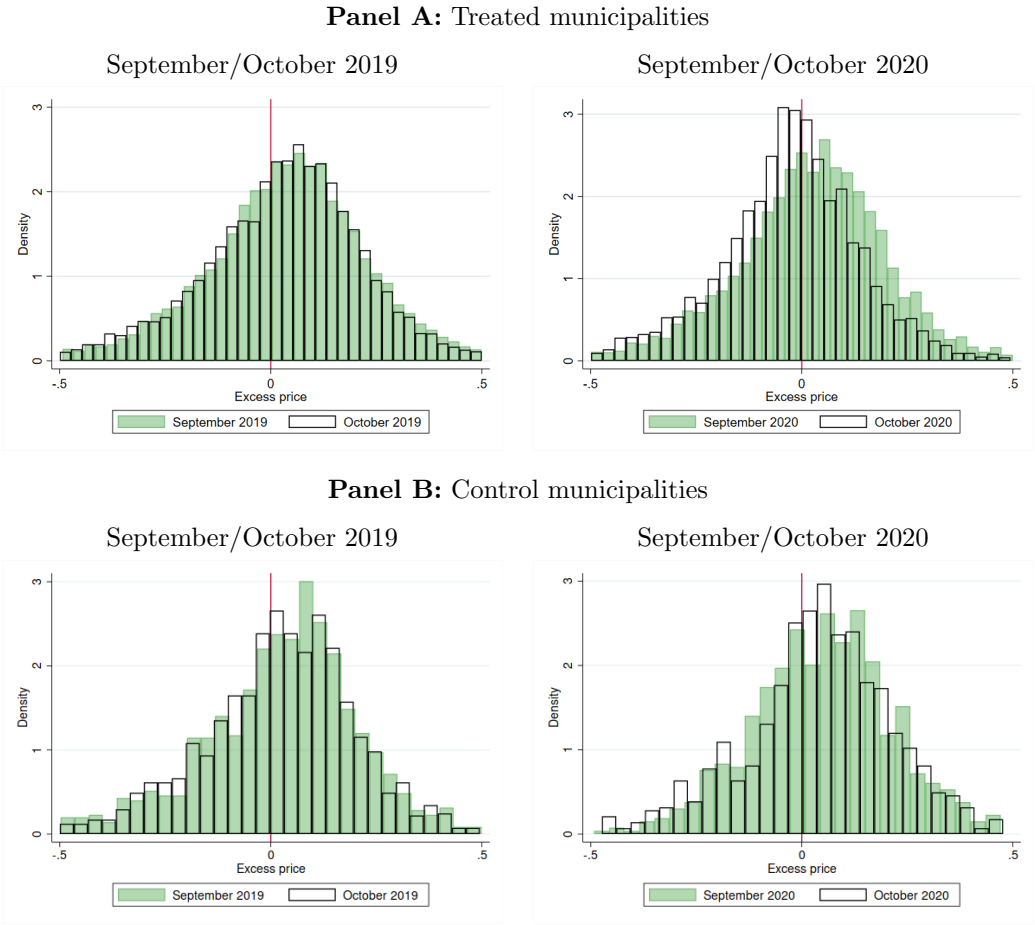
Notes: These maps show treated, in blue, and control municipalities, in gray. Treated municipalities are defined as municipalities that were subject to a rental price cap.

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



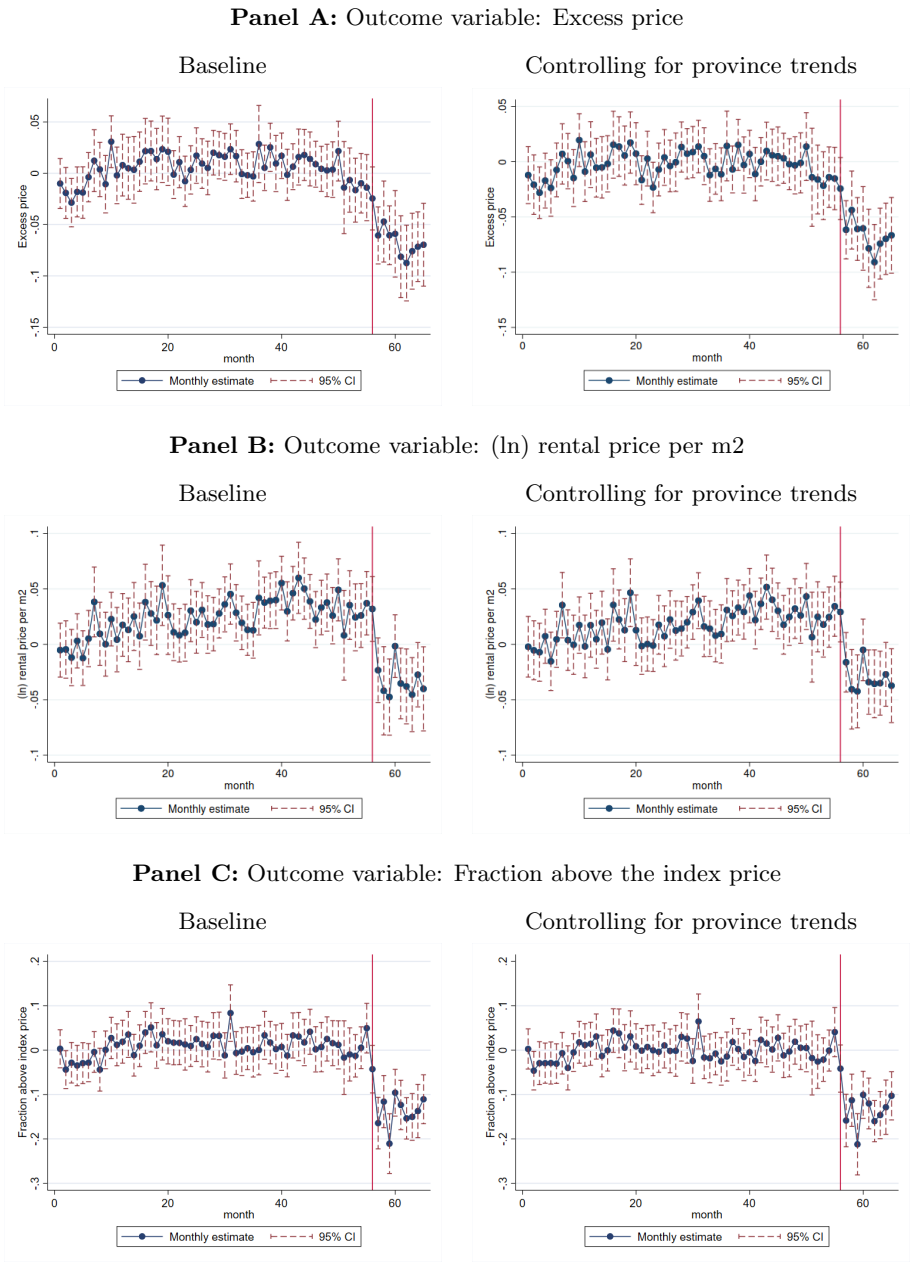
Notes: These graphs show the histograms of excess price in treatment and control municipalities. Excess price is defined as the log difference between the rental price and the reference price calculated for each unit.

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



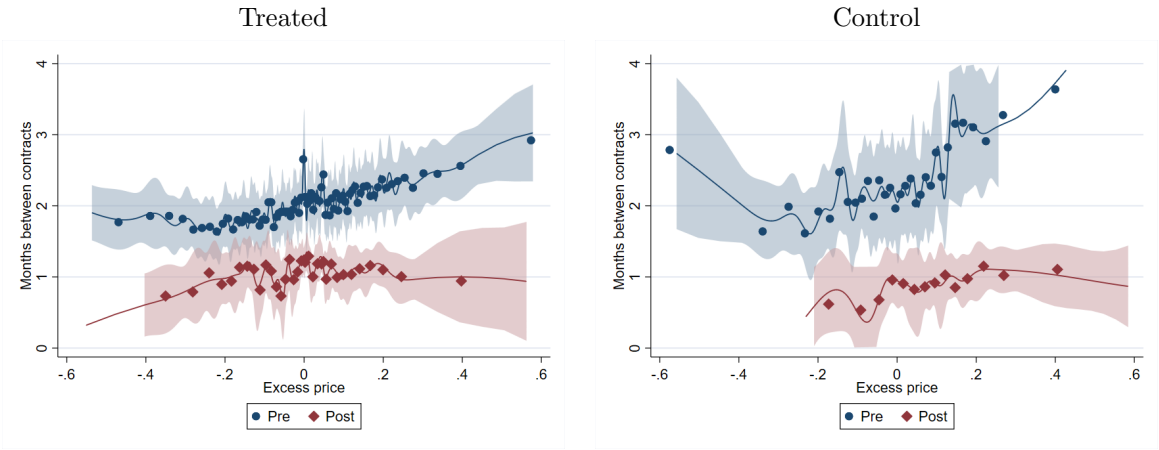
Notes: These graphs show the histograms of Excess price in treatment and control municipalities during selected months around the policy change and in the preceding year. Excess price is defined as the log difference between the rental price and the reference price calculated for each unit.

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



Notes: This figure shows two types of graphs and three different outcome variables, as denoted in the titles. 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: Relationship between time between contracts and excess price



Notes: This figure shows binned scatter plots for time in-between contracts and excess price. The data points are grouped into equal-sized bins, and the selected number of bins is optimal in minimizing the (asymptotic) integrated mean-squared error (IMSE). Each data point corresponds to the mean of the two variables within each bin, controlling for municipality. We add a fitted line and a confidence band based on cubic B-splines.

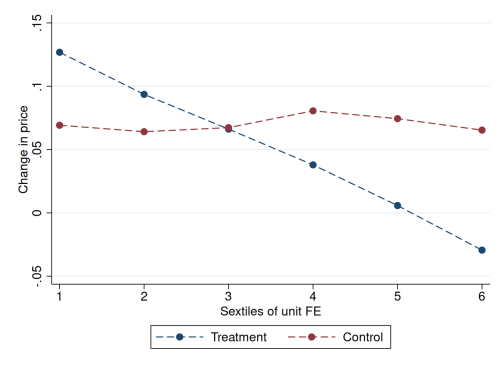
Figure VII: Dynamic estimates of the effect of the policy on the volume of transactions

Notes: These figures show graphs of the (log) number of the overall number of transactions and the overall (log) number of transactions above and below the reference price in the treatment relative to the control municipalities. These graphs use 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.

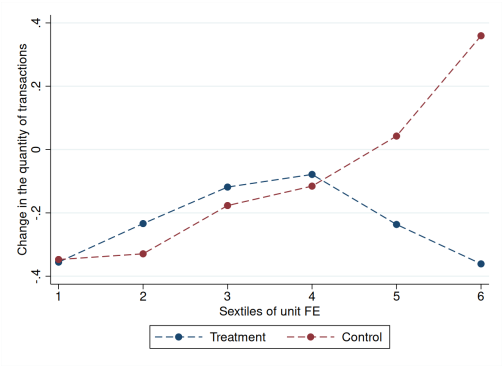
Figure VIII: Price and quantity changes by sextile

Panel A: Price and quantity change around the policy

Price change

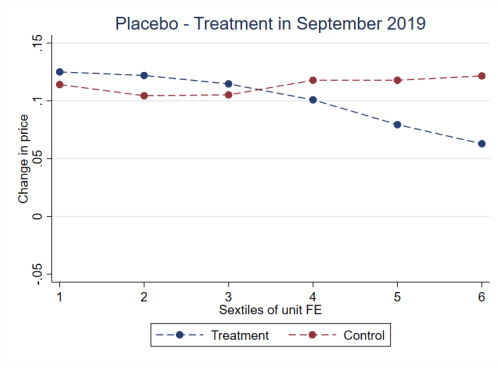


Quantity change

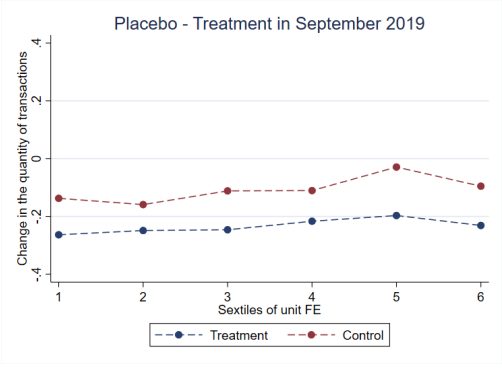


Panel B: Placebo assuming policy change took place in the preceding year

Price change

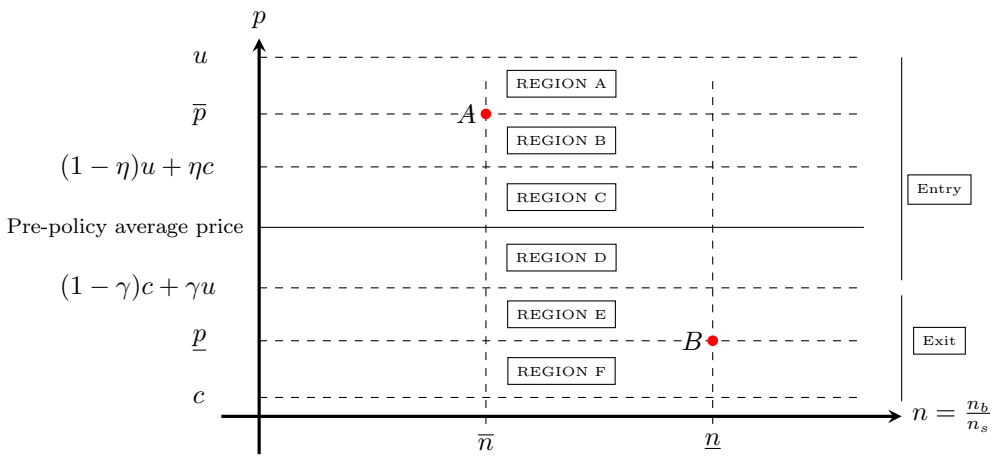


Quantity change



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 along the excess price distribution. Sextiles define six equally sized groups within treatment and control municipalities. Panel A shows the change before and after the policy change, while Panel B shows a placebo that assumes that the policy was implemented one year before its actual implementation.

Figure IX: Graphical representation of the model



Notes: This graph shows the original equilibrium with two submarkets, denoted by A and B in a diagram with prices on the y-axis and market tightness in the x-axis. It also depicts the various regions where the policymaker can set the reference price P. Entry and Exit denote whether in those regions the model predicts a higher number of rental contracts or a lower one, after the implementation of a reference price. Region A: Welfare improving for all landlords, detrimental to renters even ex post. Region B: Welfare improving for all landlords, detrimental to renters. Region C: Welfare improving for landlords and renters, higher post-policy average prices. Region D: Welfare improving for landlords and renters, lower post-policy average prices. Region E: Welfare improving for all renters, detrimental to landlords. Region F: Welfare improving for renters, detrimental to landlords even ex post.

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	-0.067
	(0.016)	(0.012)	(0.015)	(0.010)	(0.012)	(0.012)
Observations	523954	523844	257461	112763	50857	523844
Fixed_Effects	yes	yes	yes	yes	yes	yes
Province_trends	no	yes	yes	no	no	yes
Unit_FE	no	no	yes	no	yes	no
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	-0.048
	(0.014)	(0.010)	(0.014)	(0.011)	(0.012)	(0.010)
Observations	591618	591568	286899	164337	72193	591568
Fixed_Effects	yes	yes	yes	yes	yes	yes
Province_trends	no	yes	yes	no	no	yes
Unit_FE	no	no	yes	no	yes	no
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	-0.135
	(0.012)	(0.011)	(0.015)	(0.025)	(0.033)	(0.011)
Observations	106149	106149	26957	59264	8911	106149
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-differences 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. Column (2) adds province × 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. Column (6) uses the same specification as (3), but considers the data at quarterly frequency. In Panel C, we compute the fraction of contracts above the reference price in each municipality 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. 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	-0.196	-0.093	-0.577	-0.545	-0.546	-0.449	0.166	0.146	0.171	0.233
	(0.021)	(0.023)	(0.030)	(0.025)	(0.042)	(0.042)	(0.052)	(0.048)	(0.038)	(0.038)	(0.047)	(0.049)
Observations	26176	26176	24273	26176	26176	26176	24273	26176	26176	26176	24273	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	-0.111	-0.106	-0.069	-0.086	-0.401	-0.343	-0.286	-0.359	0.238	0.213	0.259	0.210
	(0.018)	(0.021)	(0.031)	(0.021)	(0.044)	(0.035)	(0.048)	(0.036)	(0.044)	(0.041)	(0.053)	(0.043)
Observations	56007	56007	53545	19558	56007	56007	53545	18669	56007	56007	53545	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 new rentals in the market

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	0.068	0.064	0.068	0.064	0.067	0.066
	(0.012)	(0.013)	(0.012)	(0.021)	(0.013)	(0.013)	(0.013)	(0.022)
Observations	27302	27302	25303	12168	26176	26176	24273	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	2.139	1.605	3.069	3.195	3.110	2.206
	(0.395)	(0.517)	(0.599)	(0.512)	(0.459)	(0.613)	(0.646)	(0.571)
Observations	27302	27302	25303	12168	26176	26176	24273	11617
fstat	74.572	45.646	43.819	33.478	73.136	44.729	43.543	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 uses 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.

References

Almagro, M., & Dominguez-Iino, T. (2022). Location sorting and endogenous amenities: Evidence from Amsterdam. *mimeo*.

Arkhangelsky, D., Athey, S., Hirshberg, D., Imbens, G., & Wager, S. (2021). Synthetic difference-in-differences. *American Economic Review*, *111*(12), 4088-4118.

Arnott, R. (1995). Time for revisionism on rent control? *Journal of Economic Perspectives*, *9*(1), 99-120.

Autor, D., Manning, A., & Smith, C. (2015). The contribution of the minimum wage to u.s. wage inequality over three decades: A reassessment. *American Economic Journal: Applied Economics*.

Autor, D., Palmer, C., & Pathak, A. (2014). Housing market spillovers: evidence from the end of rent controls in Cambridge, Massachusetts. *Journal of Political Economy*, *122*(2), 661-717.

Bierdel, A., Drenik, H. J., A., & Ottonello, P. (2022). Asymmetric information and capital accumulation. *mimeo*.

Breidenbach, P., Eilers, L., & Fries, J. (2022). Temporal dynamics of rent regulations - the case of german rent control. *Regional Science and Urban Economics*, *92*, 103737-.

Bulow, J., & Klemperer, P. (2012). Regulated prices, rent seeking, and consumer surplus. *Journal of Political Economy*, *120*(1), 160-186.

Callaway, B., & Sant'Anna, P. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, *225*, 200-230.

Cengiz, D., Dube, A., Lindner, A., & Zipperer, B. (2019). The effect of minimum wages on low-wage jobs. *Quarterly Journal of Economics*.

de Chaisemartin, C., & D'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, *110*(9), 2964-2996.

Desmond, M. (2017). *Evicted*. Penguin Random House.

Diamond, R., McQuade, T., & Quian, F. (2019). The effect of rent control expansion on tenants, landlords and inequality: evidence from San Francisco. *American Economic Review*, *109*(9), 3365-3394.

Diaz, A., & Jerez, B. (2013). House prices, sales, and time on the market: A search-theoretic framework. *International Economic Review*.

Downs, A. (1988). Residential rent controls: An evaluation. *DC: Urban Land Institute*.

Fetter, D. (2016). The home front: Rent control and the rapid wartime increase in home ownership. *Journal of Economic History*.

Friedman, M., & Stigler, G. (1946). *Roofs or ceilings?* Foundation for Economic Education.

Galenianos, M., & Kircher, P. (2009). Directed search with multiple job applications. *Journal of Economic Theory*, *144*(2), 445-471.

Garcia-Lopez, M.-A., Jofre-Monseny, J., Martínez-Mazza, R., & Segú, M. (2020). Do short-term rent platforms affect housing markets? evidence from airbnb in barcelona. *Journal of Urban Economics*, *119*.

Glaeser, E. L., & Luttmer, E. F. P. (2003). The misallocation of housing under rent control. *American Economic Review*, *93*(4), 1027-1046.

Gyourko, J., & Linneman, P. (1989). Equity and efficiency aspects of rent control: An empirical study of new york city. *Journal of Urban Economics*, *26*, 54–74.

Jofre-Monseny, J., Martinez, R., & Segu, M. (2022). Effectiveness and supply effect of high-coverage rent control policies. *IEB Working Paper 2022/02*.

Mense, A., Michelsen, C., & Kholodilin, K. (2019). The effect of second-generation rent control on land values. *American Economic Review Paper and Proceedings*, *109*(3), 385-388.

Mense, A., Michelsen, C., & Kholodilin, K. (2023). Rent control, market segmentation, and missallocation: Causal evidence from a large-scale policy intervention. *Journal of Urban Economics*.

Montalvo, J. G. (1997). GMM estimation of count panel data models with fixed effects and predetermined instruments. *Journal of Business and Economic Statistics*, *15*(1), 82-89.

Moon, C.-G., & Stotsky, J. G. (1993). The effect of rent control on housing quality change: A longitudinal analysis. *Journal of Political Economy*, *101*(6), 1114-1148.

Olsen, E. (1972). An econometric analysis of rent control. *Journal of Political Economy*, *80*(6).

Osman, S. (2022). *The invention of brownstone brooklyn: gentrification and the search for authenticity in postwar new york*. Oxford University Press.

Rekkas, M., Wright, R., & Zhu, Y. (2021). How well does search theory explain housing prices? *miemeo*.

Sims, D. (2007). Our of control: what can we learn from the end of massachusetts rent control? *Journal of Urban Economics*, *61*(1), 129-151.

Suen, W. (1989). Rationing and rent dissipation in the presence of heterogeneous individuals. *Journal of Political Economy*, *97*(6), 1384-94.

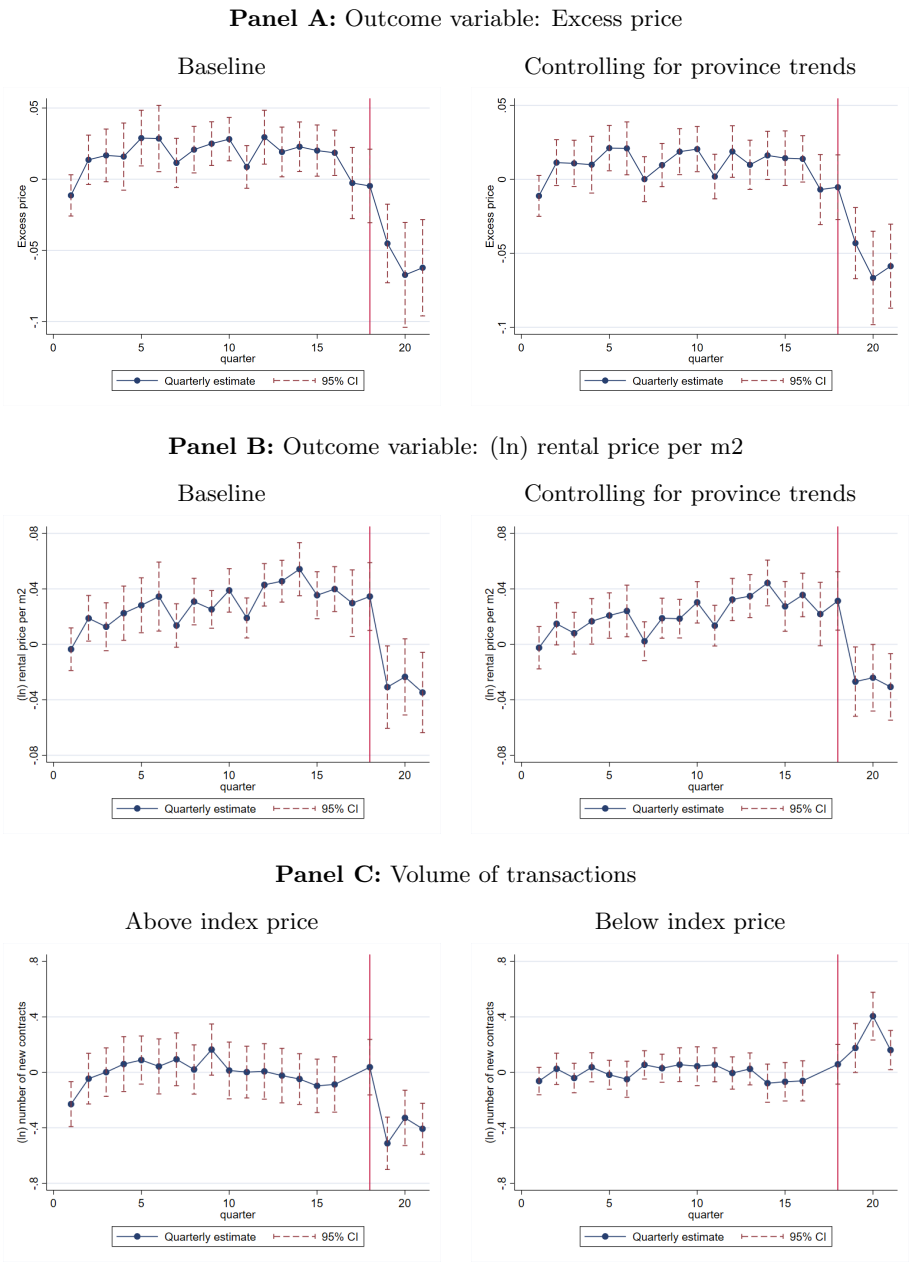
Wright, R., Kircher, P., Julien, B., & Guerrieri, V. (2021). Directed search and competitive search: A guided tour. *Journal of Economic Perspectives*, *59*(1), 90-148.

FOR ONLINE PUBLICATION

A Empirical Appendix

AA. Quarterly frequency

Figure A.1: Dynamic estimates of the effect of the policy - quarterly frequency



Notes: This figure repeats some of the main graphs shown in Figures V and VII using data grouped at the quarterly frequency. Details on each of these graphs are provided in the notes to Figures V and VII.

AB. Heterogeneity-robust estimates

Table A.1: 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

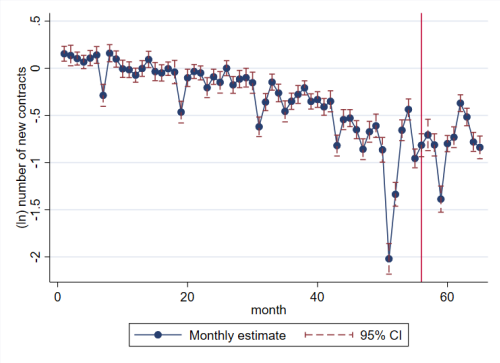
Notes: This table reports the same results reported in Table II but using the estimator proposed in de Chaisemartin & D'Haultfoeuille (2020).

AC. Additional results on volume of new contracts

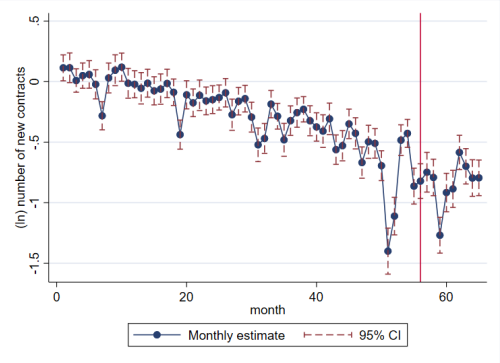
Figure A.2: Event graphs, second stage, separate treatment and control.

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

Treatment

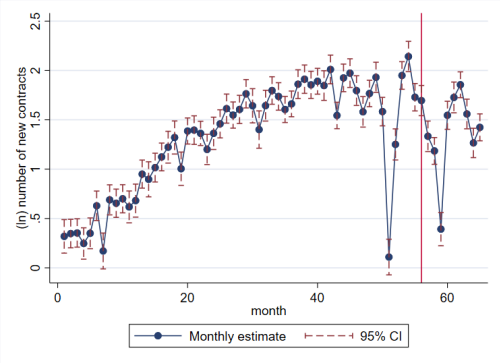


Control

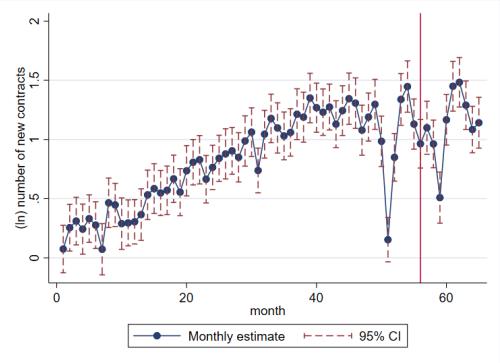


Panel B: Outcome variable: Volume of transactions above threshold

Treatment

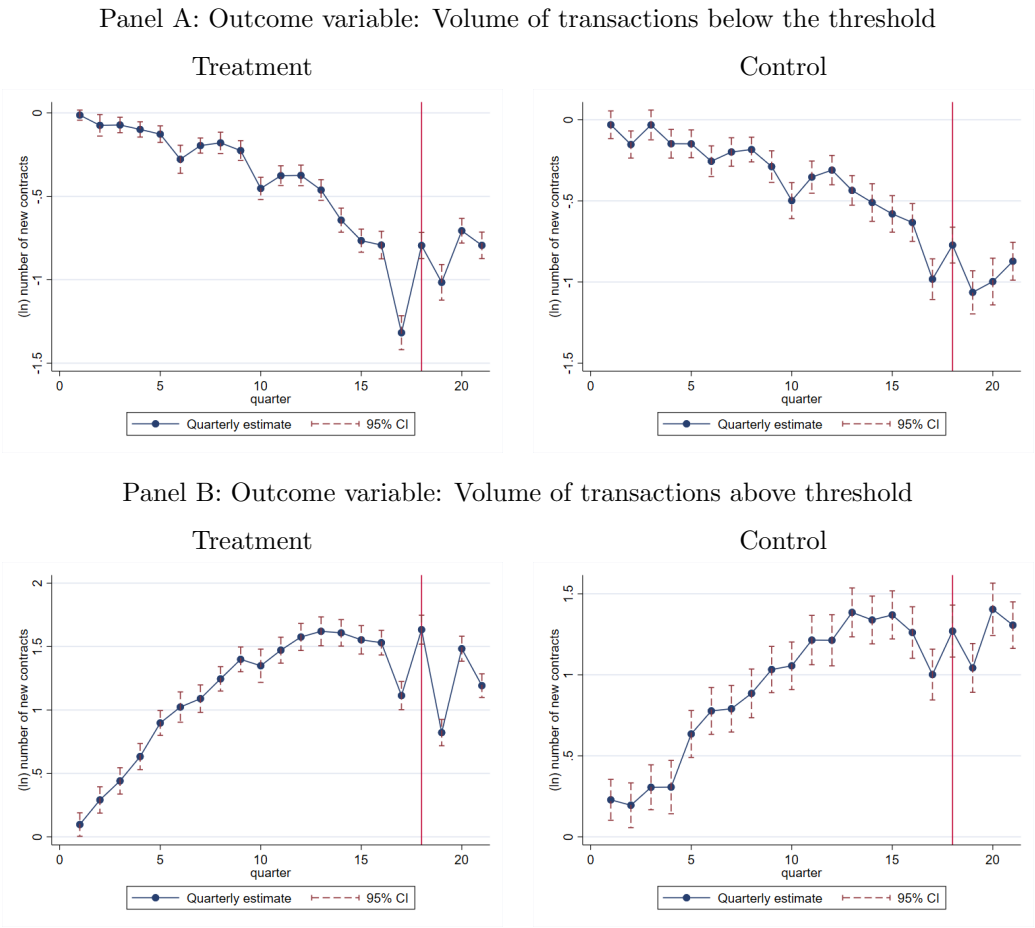


Control



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

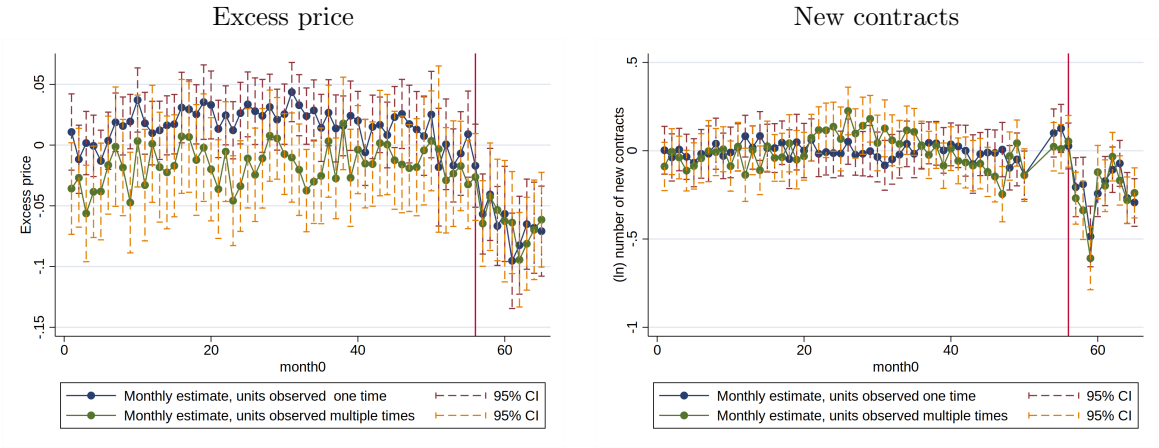
Figure A.3: Event graphs, second stage, separate treatment and control - Data grouped at quarterly frequency.



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

AD. Heterogeneity by units in the rental market once versus multiple times

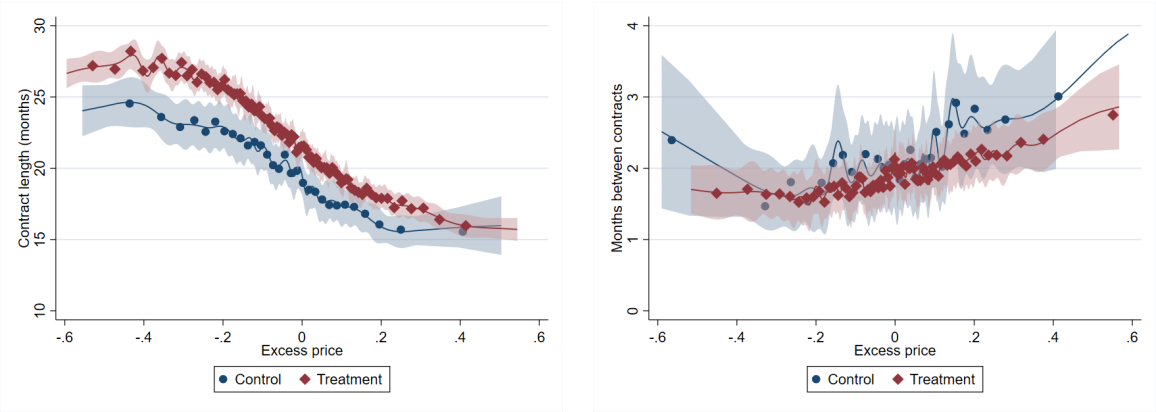
Figure A.4: Price and quantity results by type of unit



Notes: These graphs show the main results on excess prices and new contracts signed separating by units for which we observe multiple contracts in our data and units for which we only observe one contract.

AE. Contract duration

Figure A.5: Relationship between contract duration and excess price



Notes: This figure shows two different binned scatter plots. The data points are grouped into equal-sized bins, and the selected number of bins is optimal in minimizing the (asymptotic) integrated mean-squared error (IMSE). Each data point corresponds to the mean of the two variables within each bin, controlling for municipality. We add a fitted line and a confidence band based on cubic B-splines.

Table A.2: Effect of rent cap on the percentage of short contracts among the newly signed contracts

OLS estimates						
	Share of short contracts					
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of Rent Cap	0.004	0.001	0.005	0.009	0.015	0.019
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
Observations	26278	26278	26278	26278	26278	26278
Fixed_Effects	yes	yes	yes	yes	yes	yes
Province_trends	no	no	no	no	no	no

Notes: This table estimates the effect of the policy change on the share of contracts among the newly signed contracts at the monthly-municipality level. We calculate the percentage of short contracts using different definitions for short. Column (1) defines a short contract as lasting less than 1 month, column (2) defines a short contract as lasting less than 2 months, and so on. All the regressions exclude the lockdown period. Fixed effects include municipality and time fixed effects. Robust standard errors clustered at the municipality level are reported.

AF. Within-unit price and supply changes

We investigate within-unit variation in Figure A.6. 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 highlighting 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.[27] 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 had a very substantial effect along this margin.

We quantify the results in Figure A.6 and explore mean reversion more thoroughly by running the following regression:

$$\Delta \ln P_{i(m)t} = +\beta_1 I(P_{it-1} < \overline{p}) + \beta_2 I(P_{it-1} > \overline{p}) + \delta_m + \lambda_t + \pi X_{it} + \epsilon_{i(m)t} \tag{14}$$

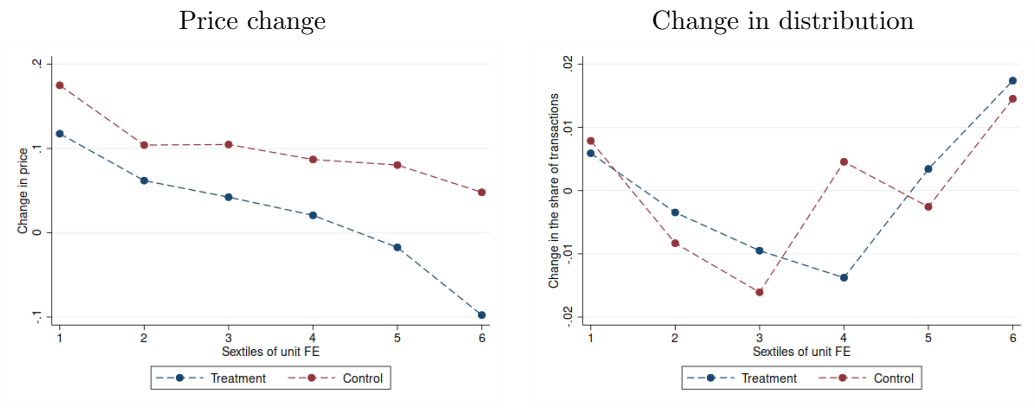
where $\Delta \ln P_{it}$ is the rate of growth of the rental price from the last contract before the beginning of the approval of the control cap to the first contract after the approval of the new regulation; $\overline{p}$ is the reference price; $I(P_{it-1} < \overline{p})$ (and respectively $I(P_{it-1} > \overline{p})$) 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 δ_m and δ_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

27. 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.

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

Table A.3 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 1) and control (column 2) municipalities. This result suggests that there is mean reversion, as we also observe visually in Figure A.6. 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.

Figure A.6: 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.

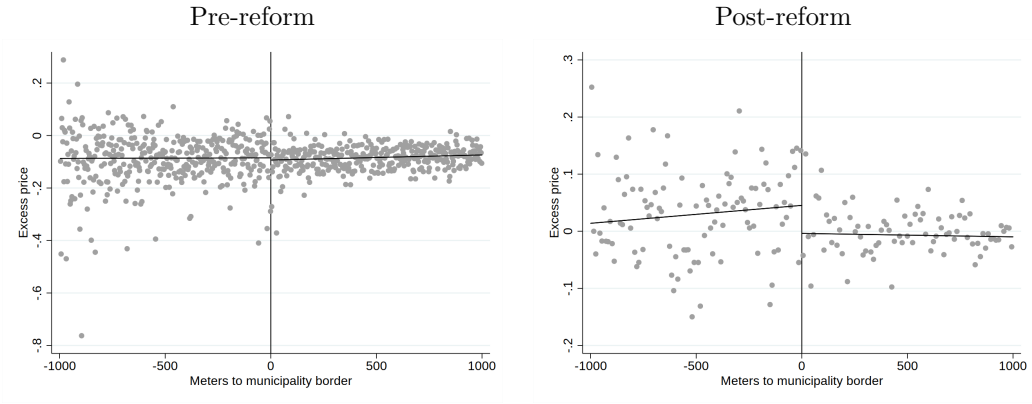
Table A.3: Estimates of the elasticity of rental market housing supply

	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.

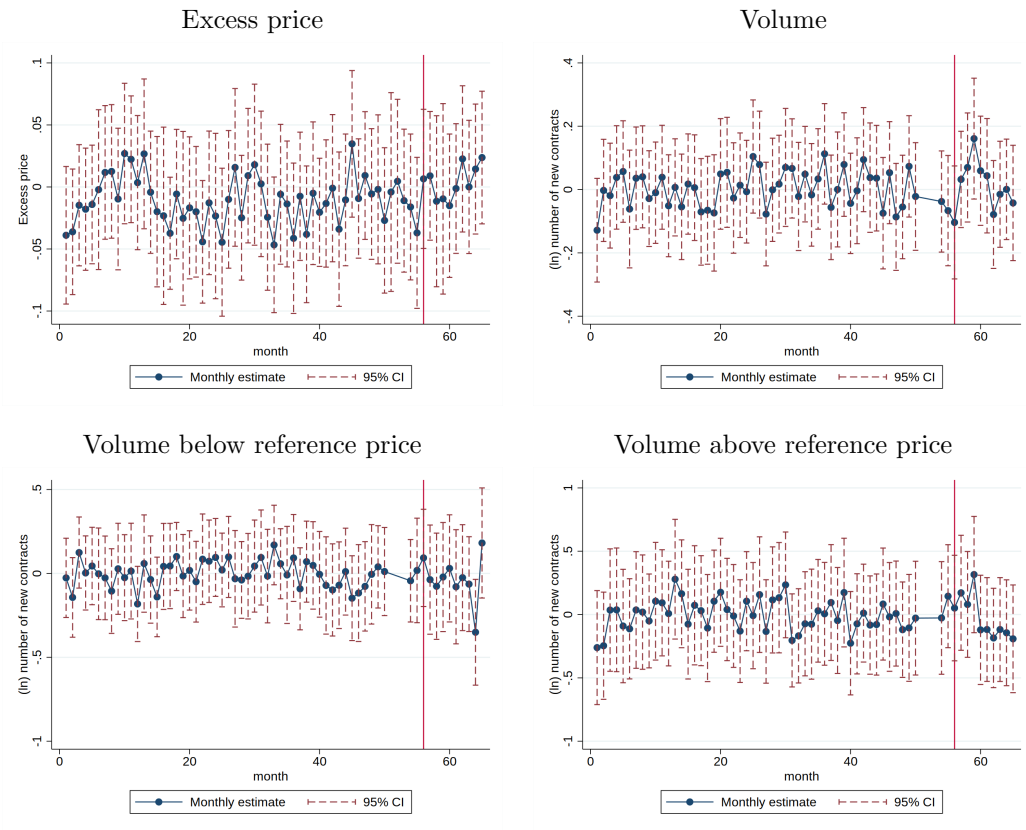
AG. Spillovers figures

Figure A.7: Border discontinuity



Notes: The graph on the left estimates a discontinuity along the border between non-treated (left) and treated (right) in terms of excess prices before the policy. The graph on the right repeats the exercise after the policy. Controls include province fixed effects.

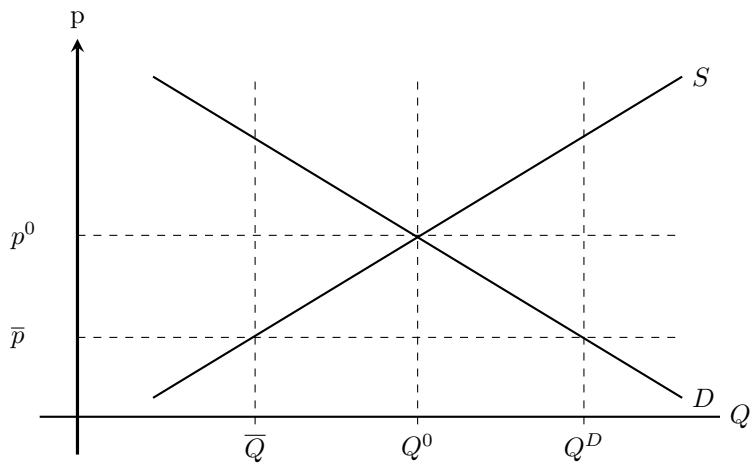
Figure A.8: Spillovers



Notes: The top left graph estimates the effect of the policy change on the excess price on units in non treated municipalities within 1,000 meters of treated ones relative to units in non treated municipalities. The top right graph estimates the effect of the policy on volumes in municipalities with units on average within 1,000 meters of treated municipalities, relative to non treated municipalities. The bottom graphs repeat the exercise on volumes for units with prices above and below the reference price.

B Theory appendix

BA. Perfect competition graph



BB. Proof of Proposition 1

Point a) is by assumption.

To prove point b) we need to show that welfare is higher with one submarket than with two submarkets. For this, with one submarket, when buyers are allowed to submit two applications, we have that total buyer welfare is:

$$N_b V_b = N_b \frac{\mu(N_s, 2N_b)}{N_b}(u - P) = \mu(N_s, 2N_b)(u - P)$$

Total seller welfare is given by

$$N_s V_s = N_s \frac{\mu(N_s, 2N_b)}{N_s}(P - c) = \mu(N_s, 2N_b)(P - c)$$

Hence:

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

In the two-market equilibrium, total buyer welfare is given by

$$N_b V_b = N_b(\frac{\mu(\underline{n}_s, N_b)}{N_b}(u - \underline{p}) + (1 - \frac{\mu(\underline{n}_s, N_b)}{N_b})\frac{\mu(\bar{n}_s, N_b)}{N_b}(u - \overline{p}))$$
$$= \mu(\underline{n}_s, N_b)(u - \underline{p}) + (1 - \frac{\mu(\underline{n}_s, N_b)}{N_b})\mu(\bar{n}_s, N_b)(u - \overline{p})$$

Likewise, total seller welfare is given by

$$N_s V_s = \underline{n}_s \frac{\mu(\underline{n}_s, N_b)}{\underline{n}_s}(\underline{p} - c) + \bar{n}_s \frac{\mu(\bar{n}_s, N_b)}{\bar{n}_s}(\overline{p} - c)$$

Hence:

$$N_s V_s = \mu(\underline{n}_s, N_b)(\underline{p} - c) + \mu(\bar{n}_s, N_b)(\overline{p} - c)$$

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

$$W = (\mu(\underline{n}_s, N_b) + \mu(\bar{n}_s, N_b))(u - c) - \frac{\mu(\underline{n}_s, N_b)}{N_b}\mu(\bar{n}_s, N_b)(u - \overline{p})$$

We can compare welfare:

$$\begin{aligned} W^* - W &= [\mu(N_s, 2N_b) - (\mu(\underline{n}_s, N_b) + \mu(\bar{n}_s, N_b))](u - c) \\ &+ \frac{\mu(\underline{n}_s, N_b)}{N_b}\mu(\bar{n}_s, N_b)(u - \overline{p}) = \\ &= [2\mu(\frac{1}{2}N_s, N_b) - (\mu(\underline{n}_s, N_b) + \mu(\bar{n}_s, N_b))](u - c) + \\ &+ \frac{\mu(\underline{n}_s, N_b)}{N_b}\mu(\bar{n}_s, N_b)(u - \overline{p}) \end{aligned}$$

And this is smaller than W^*, since $\mu(\underline{n}_s, N_b) \leq \mu(N_s, N_b)$ and $\mu(\bar{n}_s, N_b) \leq \mu(N_s, N_b)$ and because of the properties of CRS matching functions.

For point c), we have that welfare of buyers is:

$$V_b^* = \frac{\mu(N_s, 2N_b)}{N_b}(u - P)$$

whereas in the one market equilibrium is:

$$V_b = (\frac{\mu(\underline{n}_s, N_b)}{N_b}(u - \underline{p}) + (1 - \frac{\mu(\underline{n}_s, N_b)}{N_b})\frac{\mu(\bar{n}_s, N_b)}{N_b}(u - \overline{p}))$$

Before comparing the two, it is useful to realize that from $\underline{p} = (1-\varepsilon(\underline{n}))(u - \frac{\alpha(\overline{n})}{\overline{n}}(u-\overline{p})) + \varepsilon(\underline{n})c$ and $\overline{p} = (1-\varepsilon(\overline{n}))u + \varepsilon(\overline{n})c$ we have that:

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

and that:[28]

$$\underline{p} - u = \Omega(\underline{n}, \overline{n})\varepsilon(\overline{n})(c-u)$$

with $\Omega(\underline{n}, \overline{n}) \equiv (\frac{\varepsilon(\underline{n})}{\varepsilon(\overline{n})} - (1-\varepsilon(\underline{n}))\frac{\alpha(\overline{n})}{\overline{n}}) < 1$.

Using this, we can compare buyers' welfare in the one and two submarket equilibria:

$$V_b^* - V_b = \frac{\mu(N_s, 2N_b)}{N_b}(u-P) - \frac{\mu(\underline{n}_s, N_b)}{N_b}(u-\underline{p}) - (1 - \frac{\mu(\underline{n}_s, N_b)}{N_b})\frac{\mu(\bar{n}_s, N_b)}{N_b}(u-\overline{p})$$

Hence $V_b^* - V_b > 0$ if and only if:

$$P < u - \frac{\mu(\underline{n}_s, N_b)}{\mu(N_s, 2N_b)}(u-\underline{p}) - (1 - \frac{\mu(\underline{n}_s, N_b)}{N_b})\frac{\mu(\bar{n}_s, N_b)}{\mu(N_s, 2N_b)}(u-\overline{p})$$

Hence, using the expression for prices:

$$P < u - \frac{\mu(\underline{n}_s, N_b)}{\mu(N_s, 2N_b)}\Omega(\underline{n}, \overline{n})\varepsilon(\overline{n})(u-c) - (1 - \frac{\mu(\underline{n}_s, N_b)}{N_b})\frac{\mu(\bar{n}_s, N_b)}{\mu(N_s, 2N_b)}\varepsilon(\overline{n})(u-c)$$

Re-arranging we obtain:

28.

We have that

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

hence

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

thus

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

and so:

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

hence:

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

$$P < u(1 - \frac{\mu(\underline{n}_s, N_b)}{\mu(N_s, 2N_b)}\Omega(\underline{n}, \overline{n})\varepsilon(\overline{n}) - (1 - \frac{\mu(\underline{n}_s, N_b)}{N_b})\frac{\mu(\bar{n}_s, N_b)}{\mu(N_s, 2N_b)}\varepsilon(\overline{n}))+$$
$$+c(\frac{\mu(\underline{n}_s, N_b)}{\mu(N_s, 2N_b)}\Omega(\underline{n}, \overline{n})\varepsilon(\overline{n}) + (1 - \frac{\mu(\underline{n}_s, N_b)}{N_b})\frac{\mu(\bar{n}_s, N_b)}{\mu(N_s, 2N_b)}\varepsilon(\overline{n}))$$

And this defines $\eta \equiv (\frac{\mu(\underline{n}_s, N_b)}{\mu(N_s, 2N_b)}\Omega(\underline{n}, \overline{n})\varepsilon(\overline{n}) + (1 - \frac{\mu(\underline{n}_s, N_b)}{N_b})\frac{\mu(\bar{n}_s, N_b)}{\mu(N_s, 2N_b)}\varepsilon(\overline{n}))$.

For the second part, we first need to realize that:

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

and, after some algebra:[29]

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

which can be rewritten as:

$$\underline{p} - c = (1 - \varepsilon(\underline{n}))(u - c)\Gamma(\underline{n}, \overline{n})$$

with $\Gamma(\underline{n}, \overline{n}) \equiv [\frac{(1-\varepsilon(\underline{n}))}{(1-\varepsilon(\overline{n}))} - \frac{\alpha(\overline{n})}{\overline{n}}\frac{(1-\varepsilon(\underline{n}))}{(1-\varepsilon(\overline{n}))} - \frac{\alpha(\overline{n})}{\overline{n}}(1 - \varepsilon(\underline{n}))] < 1$.

Now, we have that the welfare of sellers in the one market is:

$$V_s^* = \frac{\mu(N_s, 2N_b)}{N_s}(P - c)$$

whereas in the two submarket equilibrium is:

29. From

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

we have:

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

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

Hence:

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

$$V_s = \frac{\underline{n}_s}{N_s}\frac{\mu(\underline{n}_s, N_b)}{\underline{n}_s}(\underline{p} - c) + \frac{\bar{n}_s}{N_s}\frac{\mu(\bar{n}_s, N_b)}{\bar{n}_s}(\overline{p} - c)$$

Hence

$$V_s^* - V_s = \frac{\mu(N_s, 2N_b)}{N_s}(P - c) - \frac{\underline{n}_s}{N_s}\frac{\mu(\underline{n}_s, N_b)}{\underline{n}_s}(\underline{p} - c) - \frac{\bar{n}_s}{N_s}\frac{\mu(\bar{n}_s, N_b)}{\bar{n}_s}(\overline{p} - c)$$

and this is greater than zero if and only if:

$$\mu(N_s, 2N_b)(P - c) - \mu(\underline{n}_s, N_b)(\underline{p} - c) - \mu(\bar{n}_s, N_b)(\overline{p} - c) > 0$$

Hence, if and only if:

$$P > c + \frac{\mu(\underline{n}_s, N_b)}{\mu(N_s, 2N_b)}(\underline{p} - c) + \frac{\mu(\bar{n}_s, N_b)}{\mu(N_s, 2N_b)}(\overline{p} - c)$$

Now, using the definitions of prices:

$$P > c + \frac{\mu(\underline{n}_s, N_b)}{\mu(N_s, 2N_b)}(1 - \varepsilon(\underline{n}))(u - c)\Gamma(\underline{n}, \overline{n}) + \frac{\mu(\bar{n}_s, N_b)}{\mu(N_s, 2N_b)}(1 - \varepsilon(\overline{n}))(u - c)$$

And hence:

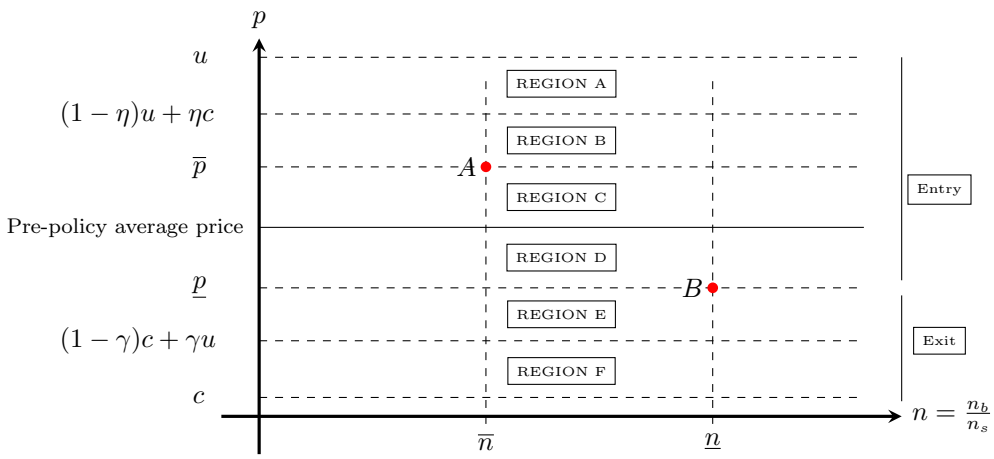
$$\begin{aligned} P > c[1 - \frac{\mu(\underline{n}_s, N_b)}{\mu(N_s, 2N_b)}(1 - \varepsilon(\underline{n}))\Gamma(\underline{n}, \overline{n}) - \frac{\mu(\bar{n}_s, N_b)}{\mu(N_s, 2N_b)}(1 - \varepsilon(\overline{n}))] + \\ + u[\frac{\mu(\underline{n}_s, N_b)}{\mu(N_s, 2N_b)}(1 - \varepsilon(\underline{n}))\Gamma(\underline{n}, \overline{n}) + \frac{\mu(\bar{n}_s, N_b)}{\mu(N_s, 2N_b))}(1 - \varepsilon(\overline{n}))] \end{aligned}$$

And this defines $\gamma \equiv [\frac{\mu(\underline{n}_s, N_b)}{\mu(N_s, 2N_b)}(1 - \varepsilon(\underline{n}))\Gamma(\underline{n}, \overline{n}) + \frac{\mu(\bar{n}_s, N_b)}{\mu(N_s, 2N_b))}(1 - \varepsilon(\overline{n}))]$.

The rest of the proposition follows from these price ranges and welfare comparisons.

BC. Appendix theory graph

Figure A.9: Alternative theory graph



Notes: This graph shows the original equilibrium with two submarkets, denoted by A and B in a diagram with prices on the y-axis and market tightness on the x-axis. It also depicts the various regions where the policymaker can set the reference price P. Entry and Exit denote whether in those regions the model predicts a higher number of rental contracts or a lower one, after the implementation of a reference price. Region A: Welfare improving for all landlords, detrimental to renters even ex post. Region B: Welfare improving for landlords and renters, ex post welfare improving for all landlords. Region C: Welfare improving for landlords and renters, higher post-policy average prices. Region D: Welfare improving for landlords and renters, ex post welfare detrimental for all landlords. Region E: Welfare improving for landlords and renters, even low-price landlords exit. Region F: Welfare improving for all renters, detrimental to landlords.

C Quality differences in perfect competition

CA. Demand

Suppose there are two types of units, high and low quality, and an outside option, such as buying or moving to another city. Tenants decide among high- and low-price units (q denotes quality) based on their utility, given by:

$$\ln u^q - \ln p^q + \varepsilon_{iq}$$

where ε_{iq} is an idiosyncratic taste shock for quality drawn from an Extreme Value distribution. By the law of large numbers, the share of buyers that choose quality q is given by:

$$D^q = (\frac{(u^q/p^q)^{1/\lambda}}{(u^l/p^l)^{1/\lambda} + (u^h/p^h)^{1/\lambda} + (b/p^l)^{1/\lambda}})N$$

where b indicates the value of the outside option – denominated in low-price metric.

CB. Supply

Landlords either own a high- or a low-quality unit and put their unit of quality q in the rental market according to an upward sloping supply function:

$$S^q = (\frac{p_q}{C})^{1/\gamma} L_q$$

where C is the cost of supplying a unit in the rental market and L_q is the mass of landlords who have a unit of quality q. To simplify, we assume $L_q = 1$.

CC. Equilibrium

The equilibrium is determined by equating the demand and supply equations in both the high- and low-quality markets:

$$(\frac{(u^l/p^l)^{1/\lambda}}{(u^l/p^l)^{1/\lambda} + (u^h/p^h)^{1/\lambda} + (b/p^l)^{1/\lambda}})N = (\frac{p^l}{C})^{1/\gamma}$$

$$(\frac{(u^h/p^h)^{1/\lambda}}{(u^l/p^l)^{1/\lambda} + (u^h/p^h)^{1/\lambda} + (b/p^l)^{1/\lambda}})N = (\frac{p^h}{C})^{1/\gamma}$$

From dividing the two equilibrium conditions we obtain:

$$(\frac{p^h}{p^l}) = (\frac{u^h}{u^l})^{\frac{\gamma}{\lambda+\gamma}}$$

Hence, $p^h = \Omega p^l$, with $\Omega > 1$ (as long as $u^h > u^l$). We can now substitute for p^h in the equilibrium condition for the low-quality market and obtain:

$$p^l = [(\frac{(u^l)^{\frac{1}{\lambda+\gamma}}}{(u^l)^{\frac{1}{\lambda+\gamma}} + (u^h)^{\frac{1}{\lambda+\gamma}} + (b(u^l)^{\frac{-\gamma}{\lambda+\gamma}})^{\frac{1}{\lambda}}})]^{\gamma} N^{\gamma} C$$

which can be expressed more succinctly as:

$$p^l = [(\frac{(u^l)^{\frac{1}{\rho}}}{(u^l)^{\frac{1}{\rho}} + (u^h)^{\frac{1}{\rho}} + (B)^{\frac{1}{\rho}}})]^{\gamma} N^{\gamma} C = (\frac{u^l}{U})^{\gamma/\rho} N^{\gamma} C$$

With this, we can obtain the high quality price:

$$p^h = (\frac{u^h}{U})^{\gamma/\rho} N^{\gamma} C$$

Equilibrium quantities are given by:

$$Q^l = (\frac{u^l}{U})^{1/\rho} N$$

$$Q^h = (\frac{u^h}{U})^{1/\rho} N$$

CD. A rental price cap

Suppose we introduce a price cap that affects only high price units. Then, the supply of high-quality units at price P is given by:

$$S^h = (\frac{P}{C})^{1/\gamma}$$

But given this supply, there will be excess demand. We can denote by p^h (in a slight abuse of notation) the price that buyers would be willing to pay if units of high quality have price P. This is determined by:

$$(\frac{(u^h/p^h)^{1/\lambda}}{(u^l/p^l)^{1/\lambda} + (u^h/p^h)^{1/\lambda} + (b/p^l)^{1/\lambda}})N = (\frac{P}{C})^{1/\gamma}$$

The equilibrium condition for the low-quality market is still:

$$(\frac{(u^l/p^l)^{1/\lambda}}{(u^l/p^l)^{1/\lambda}+(u^h/p^h)^{1/\lambda}+(b/p^l)^{1/\lambda}})N=(\frac{p^l}{C})^{1/\gamma}$$

Following the same steps as before we obtain that relative prices are given by:

$$p^h=(\frac{u^h}{u^l})(p^l)^{\frac{\gamma+\lambda}{\gamma}}P^{-\frac{\lambda}{\gamma}}$$

Notation simplifies if we assume that $P=\eta p^h$, $\eta<1$. That is, the rental cap price is set at a fraction η of the high price. In this case:

$$p^h=p^l(\frac{u^h}{u^l})^{\frac{\gamma}{\lambda+\gamma}}\eta^{-\frac{\lambda}{\gamma+\lambda}}$$

where this p^h is, again, the willingness to pay for high quality units at price P. Hence:

$$p^h=p^l\Omega\eta^{-\frac{\lambda}{\gamma+\lambda}}$$

Using this into the equilibrium condition for the low-quality market, we obtain:

$$p^l=[(\frac{(u^l)^{\frac{1}{\rho}}}{(u^l)^{\frac{1}{\rho}}+(u^h)^{\frac{1}{\rho}}\eta^{\frac{1}{\gamma+\lambda}}+(B)^{1/\rho}})]^{\gamma}N^{\gamma}C$$

Hence, we can express the equilibrium with rental price caps as:

$$p^l=\Gamma^{\gamma}(\frac{u^l}{U})^{\gamma/\rho}N^{\gamma}C$$

$$p^{h*}=P$$

and the quantities:

$$Q^l=\Gamma(\frac{u^l}{U})^{1/\rho}N$$

$$Q^h=(\frac{P}{C})^{1/\gamma}$$

with $\Gamma>1$.

Given these equations, it is easy to see that the rental cap decreases prices and quantities in the high-quality market and increases them in the low-quality one. With the outside option, the increase in the number of units in the low-quality market does not compensate the decline of units in the high-quality market. We provide further intuition in the following subsection.

CE. Graphical representation

The perfect competition model can be represented in a set of simple graphs, which we show in Figure A.10. The figure is divided into two panels. Panel A shows the equilibrium in the market when there is no rental cap or reference price. Panel B shows the new equilibrium once a rental cap is introduced that is binding for the high-quality units.

Initially, the equilibrium is at point A and B. At these points, prices for high-quality units are higher than for low-quality ones, reflecting either higher demand or lower supply (in the algebra above we assume that this is generated by higher demand). A reference price P is more likely to be binding in the high-quality market. At that price, as shown in Panel B, fewer owners of high quality units will be willing to put their unit into the market, hence the amount of high quality units in the market (Q^h) will be lower with the price cap.

The price cap will generate excess demand for high-quality units. This excess demand can be computed by looking at the location of p^h in panel B, which is the price that renters would be willing to pay at the limited supply. This excess demand moves to the low-quality market or to the outside option. This is shown in Panel B as a modest shift in the demand for low-quality units. This increase in the demand for low-quality units leads to increases in both prices and quantities in the low-quality market.

In terms of welfare, there is some deadweight loss in the high quality market. Renters with a high willingness to pay gain at the expense of landlords and some renters who, at price P, cannot find a high quality unit. This demand is transfered to the low-quality market. Hence, landlords in this market benefit, whereas the effect on renters is ambiguous: it depends on comparing the area below the old demand and the old prices that is lost at the new price, to the area below the new demand but above the old one, and above the new price.

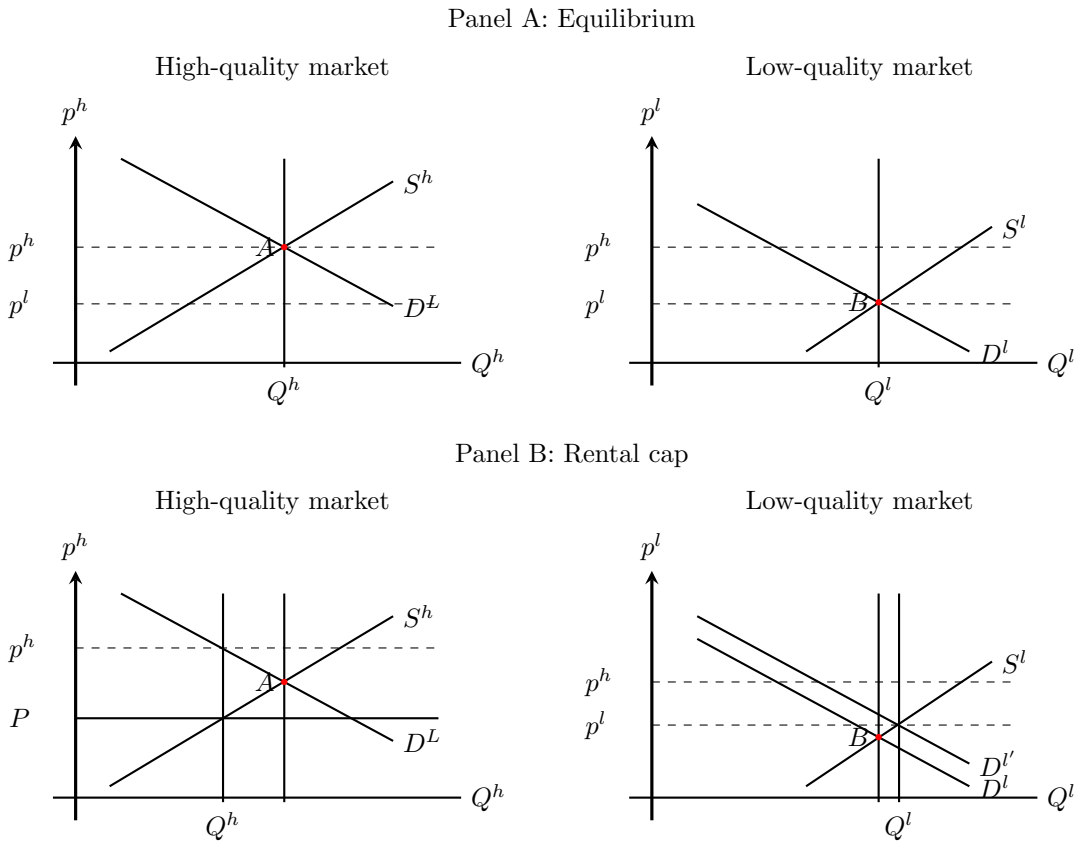



Figure A.10: Perfect competition model with hetergeneous quality