PARIS2019: The impact of rent control on the Parisian rental market

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Abstract

We evaluate the impact of the rent control regulation implemented by the city of Paris in July 2019 on the Parisian rental market. We take advantage of the large amount of data available in real-time on the SeLoger platform containing the ads published by professional realtors. Using a database of 559,300 observations from January 2018 to June 2023, we apply a difference-in-differences model, where control units are located in eight major French cities in which the rental market is particularly tense but not regulated during the analysis period. We show that the rent control policy decreased rents by 3.7% to 4.2% in Paris on average. Yet, the policy is heterogeneous depending on dwelling characteristics with a stronger effect on small apartments. We also estimate the higher bound of the effectiveness of the policy and show that if every dwelling respected the rent caps, rents would have decreased by 8.2% to 8.7%.

JEL Classification: C31, O18, R38

Keywords: rent control, rental market, Paris, real-estate market platform, difference-in-differences

1 Introduction

Between 2015 and 2023, rental prices increased by 13% in the EU zone and by 42% in the United States (OECD, 2024). Among the various measures adopted to maintain affordable housing, rent control has come back into force since the 2010s in numerous countries (e.g., Catalonia and California since 2020, Berlin between 2020 and 2021) after being widely withdrawn (Kholodilin, 2020). Introduced in 1915 with the Increase of Rent and Mortgage Interest Act in Great Britain, the first generation of rent control that froze rents above a given ceiling was replicated in the United States and several European countries until the end of World War II. The more flexible second-generation of rent control, which limited rent increases within and between tenancies, was frequently used in Europe in the 1970s. The current third generation consists of controlling rent increases only within a tenancy (but not between tenancies).[1]

Rent control is theoretically expected to make housing more affordable for tenants, to help them stay in their homes, and to prevent gentrification. Yet, critics argue that rent control can destabilize the housing market with i) a decrease in the supply of rental housing (i.e., quantity) by discouraging landlords from entering the rental market or investing in additional rental properties, and/or ii) an excessive demand for rental housing due to artificially low rental prices. The quality of rental housing can also be impacted, as landlords may lack funds or incentives to maintain decent housing, leading to a deterioration of properties. Rent control also affect the willingness to pay for dwelling's attributes Van Ommeren and Graaf-de Zijl, 2013. Finally, rent controls likely introduce more market distortions (e.g., misallocation of housing resources and black market) and reduce mobility (Kholodilin, 2024). In general, the effectiveness of a rent control policy is highly dependent on local housing market conditions, design, and enforcement.

In France, a dual rent control system can coexist in tense areas consisting of 1,149 municipalities spread over 28 metropolitan areas[2]. First, the maximal yearly revision of the rent of the dwelling or the increase following a change of tenant is limited by the value of the annual evolution of the "Indice de référence des loyers" since 2014. Second, cities in

[1]See Kettunen and Ruonavaara, 2021 for a review on rent regulation.

[2]Tense areas ("zones tendues" in French) are continuous urbanization areas with more than 50,000 inhabitants characterized by a significant imbalance between the supply and demand for housing, thus creating high tension in the market.

tense areas can also impose a ceiling on rents with reference rents per square meter that must not be exceeded. The ALUR law of 2014 allowed rent control, which Paris took up between August 2015 and November 2017 (following the cancellation by the administrative court). Since then, the vote of the ELAN law in 2018 has allowed a 5-year experimental mechanism, which has been applied in Paris since July 1, 2019. Following on, 23 other cities belonging to "tense areas" have introduced the experimental rent control scheme as well. In these cities, rents are restricted by an upper limit (except for dwellings with exceptional characteristics) based on reference rents, which depend on the number of rooms, the building period, whether the dwelling is furnished or not, and the geographical sector.

The consequences of the first rent control system implemented in Paris were analysed by Malard and Poulhes (2020) based on data collected by the Observatoire des Loyers de l'Agglomération Parisienne (OLAP). Using a logistic model, they predicted a theoretical rate of exceeding the maximum base rent ("loyer de référence majoré") over the framework period and compared it with the observed rate. However, to the best of our knowledge, no study has yet evaluated the impact of the French rent control using an identification strategy able to identify the causal effect of the rent control scheme on rents, isolating this effect from the other factors likely to have an influence.

In this paper, we evaluate the effects of the second rent control scheme introduced in 2019 on the Parisian rental market. Our contributions are as follows. First, we exploit 559,300 ads published from January 1, 2018, to June 30, 2023, by the SeLoger group, which gathers the major French real estate websites. These ads are created by professional realtors and do not include any ads posted by private individuals. After cleaning the data, our database consists of 559,300 observations on Paris and our selected control group. Our work then contributes to the growing literature using internet-based datasets to study the rental market in France and more widely in Europe: Breidenbach et al. (2022), Mense et al. (2023), Sagner and Voigtländer (2022), and Thomschke (2019) on German data, Livingston et al. (2021) on UK data, Chapelle and Eyméoud (2022) on French data.

Second, in addition to fueling the public debate, we also contribute to the academic literature that evaluates the impact of "second-generation" rent controls with causal inference methods. Mense et al. (2023) measure positive spillovers on the unregulated market in Germany after the introduction of partial rent control on the territory by exploiting temporal variation in the implementation of treatment. Diamond et al. (2019) also use a quasi-experiment based on the introduction of rent control in San Francisco. Although this measure prevented the move of incumbent renters, they show detrimental effects with a reduction of the rental housing supply[3]. Monràs and Montalvo (2022) and Jofre-Monseny et al. (2023) studied the effects of the rent control policy in Catalonia. Both studies find a significant decrease in rents caused by the policy. However, their results differ in terms of the effect of the supply of rental housing. Although Monràs and Montalvo (2022) find a significant decrease in supply, this effect is found to be insignificant by Jofre-Monseny et al. (2023). However, Kholodilin et al. (2022) find that the policy's revocation in 2022 caused a significant increase in rent in previously regulated areas. Our paper is the first to provide a causal evaluation of rent control in the rental market in the French case.

Third, we study the heterogeneous effects of the policy. Rent caps are computed as the median over the past two years increased by 20% for different categories of dwellings. Therefore, the effect of the policy could be heterogeneous depending on the characteristics of each dwelling. Moreover, we also study the heterogeneity of the treatment effect depending on the level of constraint that the policy applies to rent. The higher the rent would have been without the policy, the higher the constraint enforced with rent caps. On the other hand, a low rent compared to the rent cap could encourage landlords to raise their rent closer to the cap.

Finally, we estimate the higher bound of the rent control policy's effectiveness. Indeed, around 40% of our sample's observations do not comply with the rent caps. Thus, the policy's actual effect may be less than expected. We estimate the policy's effect if every dwelling had a rent lower than or equal to the rent cap. To our knowledge, none of the previous studies provided such an estimate.

Our identification strategy is based on a difference-in-differences model (Roth et al., 2023) that we apply to our database of 559,300 observations from January 2018 to June 2023. We select the groups given the characteristics of our institutional context and data. Our treated group includes all rental ads published in Paris during the period from January 1, 2018, to June 30, 2023. For the control group, we choose all rental ads published in eight major French cities in which the rental market is particularly tense, but who did not choose

[3]see also Zheng et al. (2007) for a study of the effects of rent control on mobile home prices in California.

to set up a rent control in their own rental market during the study period. We verify that the Parallel Trend Assumption (PTA) is satisfied. In addition, since the observations in the control group are located outside the Paris region, we are also able to maintain the Stable Unit Treatment Value Assumption (SUTVA) as it is unlikely to have spillover effects between Paris and remote cities.

Our results show that the rent control policy caused a rent decrease of 3.7% to 4.2%. We also estimate the higher bound of the effectiveness of the policy: if all dwellings respected the rent caps, the actual rent decrease would have been between 8.2% and 8.7%. In addition, we show this policy has highly heterogeneous effects. First, the effect increases over time. The decrease in rent was 2.5% between July 2019 and June 2020, while it decreased to 5.9% between July 2022 and June 2023. The effect of the policy is also highly heterogeneous depending on the size of the residence. Smaller dwellings, with an area between 8 and 18 sqm, had a rent decrease of 10.2%, while the rent decrease is around 2% for dwellings of 60 sqm and more. We also evaluate the effect of the rent control policy depending on the level of constraint that the policy enforces on rent, we find that 5% of the dwellings that are the less constrained by the rent actually had their rent increased by 9%. Yet, the effect of the policy is a decrease in rents for most of the dwellings in our sample. Because the Paris housing market has a higher share of small dwellings than the control group, we test the robustness of the estimated treatment effect for this subset of dwellings. We find similar results as in the heterogeneity analysis, and also find that PTA cannot be rejected on this sub-sample. Using an adaptation of the synthetic difference-in-differences estimate, we also show that our results are robust to PTA. We compare our DID estimates of the rent control policy effects with synthetic difference-in-differences estimates and find the results to be similar.

The rest of the paper is organised as follows. Section 2 discusses the institutional context. Section 3 describes the data and the identification strategy. Section 4 presents the results and section 5 performs several robustness checks. Section 6 concludes.

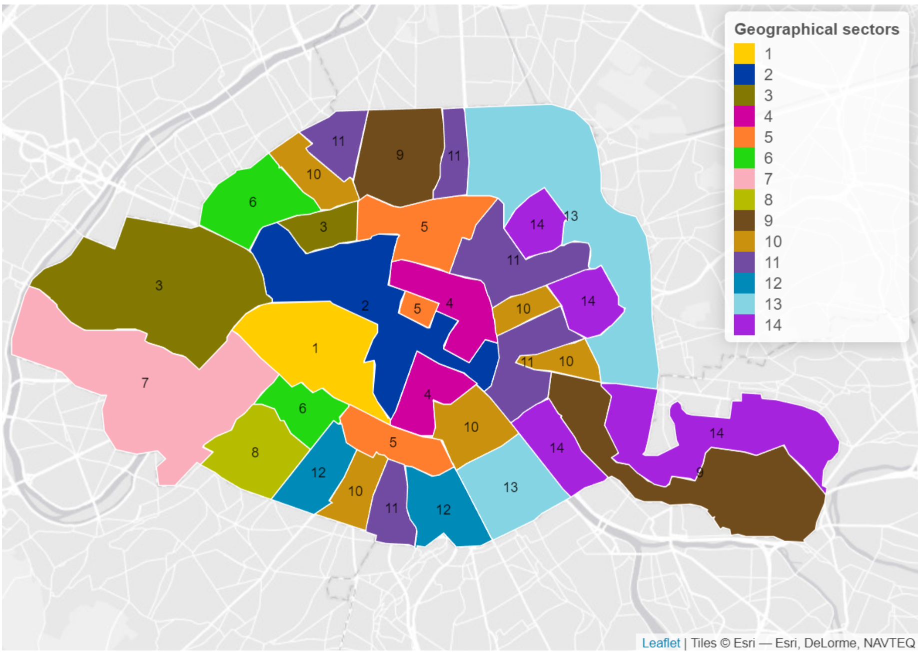


Figure 1: Geographical sectors of the rent control policy in Paris

2 Background

Paris is the first French city to have implemented rent control measures since the postwar years and has done so twice. Parisian rent control first came into effect from August 1, 2015, until November 28, 2017, when it was canceled by the decision of the Paris administrative court (confirmed on appeal in June 2018)[4]. Since July 1, 2019, a new Parisian rent control measure has taken the form of an experiment[5]. Following Paris, other cities and establishments for inter-municipal cooperation (EIMC) belonging to "tense areas" have implemented the experimental rent control: Lille-Hellemmes-Lomme since March 1, 2020, the EIMC of Plaine Commune since June 1, 2021, Lyon and Villeurbanne since November 1, 2021, the EIMC of Est Ensemble since December 1, 2021, Montpellier since July 1, 2022, and finally Bordeaux since July 15, 2022, making a total of 24 cities.

Current Parisian rent control applies to all leases signed as of July 1, 2019, to both unfurnished and furnished rentals for first-time rentals (including shared apartments), lease renewals, and change of tenants. Only social housing (HLM, APL and ANAH subsidized

[4]The court found that the limited application to Paris violated the article 17 of the ALUR law of March 24, 2014, according to which it should have been implemented in a broader area "of continuous urbanization with more than 50,000 inhabitants where there is a marked imbalance between housing supply and demand"

[5]Initially scheduled for 5 years under the ELAN law of November 23, 2018, the experiment was extended by the "3DS" law of February 21, 2022, by an additional 3 years, until November 23, 2026.

housing), housing subject to the law of 1948[6], secondary residence, company accommodation and short-term rentals are not subject to rent control. In each regulated city/EPCI, the prefect sets each year the level of "reference rents" applicable to the private housing market. In Paris, the reference rents are based on data from the Parisian Agglomeration Rent Observatory (OLAP) and defined according to 4 criteria: i) the type of housing (unfurnished or furnished), ii) the construction date (before 1946, from 1946 to 1970, from 1971 to 1990, after 1990), iii) the number of rooms (1 room, 2 rooms, 3 rooms, 4 rooms and more) and iv) the geographical sector, with Paris being divided into 14 geographical sectors (and 80 administrative neighborhoods), as shown by Figure 1. The median reference between July of year t and June of year t+1 is computed for each geographical sector and each category using the data from $t-1$ and $t-2$. Then, landlords must set their rent per square meter excluding charges between minus 30% of this median ("loyer minoré") and plus 20% of this median ("loyer majoré"). However, a rent supplement ("complément de loyer") can be levied on top of the maximum rent ("loyer majoré") when the housing includes features related to the location and/or can be considered luxurious or rare, compared to similar housing in the same geographical area. Unfortunately, our data do not allow us to distinguish between rent supplements justified by exceptional characteristics and those that are not.

This regulation of the rent level adds to an ongoing regulation of rent increases. The "indice de référence des loyers (IRL)"[7], introduced in 1989, initially only regulated the rental market in the Parisian agglomeration by constraining the maximum yearly increase that the owner could apply[8] to the rent in case of lease renewal. In August 2012, it was geographically extended to new agglomerations and expanded to a change of tenants. With the ALUR law of 2014, this rent increase regulation was extended to all tense areas in France[9].

[6]To be subject to the 1948 law, which confers the tenant very broad benefits, a dwelling must have been built before September 1, 1948, in certain municipalities with more than 10,000 inhabitants or in areas adjacent to these municipalities, and the tenant must have moved into the premises before December 23, 1986.

[7]The Indice de Référence des Loyers (IRL) is calculated each year by INSEE based on the average change in consumer prices, excluding tobacco and rents, over the past 12 months. See https://www.insee.fr/fr/statistiques/serie/001515333

[8]This yearly increase can be applied once per year within a tenancy but is also enforced when the tenant changes.

[9]Because the IRL only regulates the rent increases within tenancies, it can be considered as a third-generation policy (Arnott, 1995, 2003). On the other hand, the rent control policy implemented by the 24 cities regulates rents both within and between tenancies; hence, it can be considered a second-generation

3 Empirical strategy

3.1 Data

Real estate platforms have become a must-have tool for all real estate projects of French households. Among them, Groupe SeLoger, whose data we process, is the leading portal for realtors' listings, gathering around 18'000 realtors and offering an average of 1'000'000 listings of properties for rent or sale continuously on its websites. Two issues may arise using listing data rather than lease agreement data. The first is that the rent paid by the tenant can be different from that listed on the online portal. However, rent negotiations are unlikely to occur in our sample of tense rental markets. The second issue is the representativeness of the sample. However, Chapelle and Eyméoud (2022) compared data from online ads with other datasets and confirms that they represent the French housing market well.

Our dataset gathers all rent listings of apartments posted on SeLoger's portal between 2018 and June 2023. For descriptive analysis purposes, we first consider a restricted sample of 164,384 Parisian listings for which we can calculate the maximum reference rent applicable to each dwelling based on its — fully completed — characteristics (number of rooms, construction period, "furnished/unfurnished" status), and the geographical sector. Parisian dwellings with at least one missing value in terms of characteristics are excluded from this descriptive analysis (42% of the data are excluded). However, they will be part of the larger sample used for the causal inference analysis since only the absolute rent level (rather than the relative level compared to the reference rent) is required.

We show in Figure 2 the evolution of the percentage of dwellings above the maximum reference rent over our analysis period based on the restricted sample. The vertical dotted lines represent the date that new rent caps are applied yearly. For observations before the implementation of the rent control mechanism, i.e., between January 1, 2018, and June 30, 2019, we apply the rent control thresholds applicable as of July 1, 2019.

The proportion of listings with rents (excluding charges) above the reference rent (which would be set starting July 1, 2019) reaches a peak of 49% before the implementation of the

rent control policy.

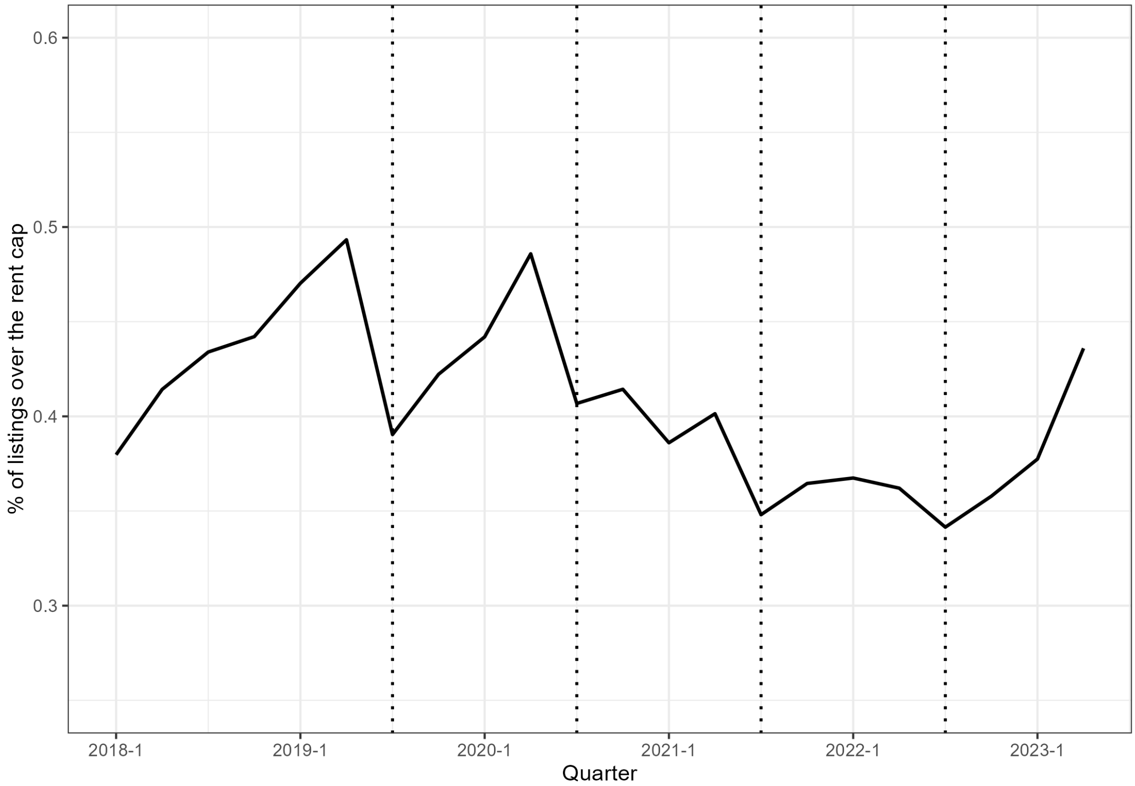


Figure 2: Trends in exceeding the maximum reference rent threshold in %

experimental mechanism. The introduction of rent control in July 2019 dropped the rate by almost 10 points in 1 quarter. Hence, nearly 4 out of 10 listings offer a rent higher than the threshold set by Parisian regulation. Although we always observe increases before the new thresholds are implemented, the trend is towards better compliance over the period. In appendix A, Figure A1 provides the decomposition depending on the number of rooms. A high heterogeneity in exceeding the reference threshold is observed for apartment sizes. In the second quarter of 2023, the proportion of dwellings with a rent (excluding charges) above the maximum limit is 60.6% for units less than 18sqm, compared to 41.4% for units between 18 and 24 sqm, and 32.4% for larger units. Our figures align with the rates observed by OLAP in their study on the private unfurnished rental market[10], when we limit our sample to the unfurnished rental sector. The overrun of the maximum reference rent threshold can be illegal or justified by exceptional characteristics[11] that lead to a rent supplement, but we are not able to distinguish them from our data. We further discuss this issue in section 4 when we study the higher bound of the effect of the policy.

3.2 Method

This paper employs a difference-in-difference (DD) design to estimate the effects of Parisian rent control on the level of Parisian rents. This model allows us to estimate the average effects of treatment across different segments of the rental market to compare compliance.

3.2.1 Treatment and control groups

The treatment group comprises 284,221 rental listings that are located in the area where rent control (treatment) is established, that is, the city of Paris.

The control group contains 275,079 listings in eight major French cities: Aix-en-Provence, Grenoble, Marseille, Nantes, Nice, Strasbourg, Toulon, and Toulouse. The selection of these eight cities results from the application of the following criteria: i) a population larger than 100,000 inhabitants (from INSEE census), ii) a location outside of the "Ile-de-France" region (see Fig.3) to ensure the absence of spatial proximity between the treatment and the

[10] https://www.observatoire-des-loyers.fr/sites/default/files/olap_documents/etudes_partenariats/Bilan%20Paris%20en%202022-resume-V2.pdf

[11] https://www.legifrance.gouv.fr/loda/article_lc/LEGIARTI000037642425/

Figure 3: Control and Treatment Cities

control groups to comply with the SUTVA hypothesis[12], iii) a tense area in the sense of the ALUR law, as defined in the background section, with rents capped in their evolution by the Rent Reference Index (IRL), and iv) the absence of a rent control policy between 2018 and 2023. These criteria ensure that our control group is composed of cities that display a housing rental market similar to the one in Paris but, as opposed to Paris, do not have a rent-control policy during the observation period.

Table (1) shows that apartments in control and treated groups have similar total area and mean area per room. Although the treated group shows a median rent twice as high as the control one, the proportion of furnished apartments is also twice the one in the control group. Apartments in the treated group are generally older and more likely to be either very small (1 room) or very large (more than 4 rooms). Eventually, the proportion of missing build years is close to 54% in both groups.

Figure 4 shows that the aggregate rent trend of these 8 cities belonging to the control group is very similar to that of Paris before the rent control was implemented in July 2019 (with an estimated correlation of 0.94), which tends to validate PTA.

[12] The Stable Unit Treatment Value Assumption stipulates that the rent asked for a property in the control group should not be affected by the rent control in Paris. Conversely, the fact that rents are not controlled in the cities of the control group should not affect rent levels in Paris.

		Control group (N=275,079)		Paris (N=284,221)	
		Mean	Std. Dev.	Mean	Std. Dev.
Rent per sqm		15.0	5.3	32.5	8.6
Area		48.9	24.8	48.0	32.8
		N	Pct.	N	Pct.
Number of rooms	1	77,017	28.0	95,278	33.5
	2	99,330	36.1	105,553	37.1
	3	70,523	25.6	49,662	17.5
	4	22,443	8.2	19,581	6.9
	5 and more	5,766	2.1	14,147	5.0
Furnished	Yes	63,571	23.1	119,571	42.1
	No	191,156	69.5	147,033	51.7
	Missing	20,352	7.4	17,617	6.2
Building period	Before 1945	20,501	7.5	150,618	53.0
	1946-1970	22,012	8.0	26,368	9.3
	1971-1990	14,899	5.4	17,082	6.0
	After 1990	48,963	17.8	11,764	4.1
	Missing	168,704	61.3	78,389	27.6

Table 1: Descriptive statistics for the control and treated groups

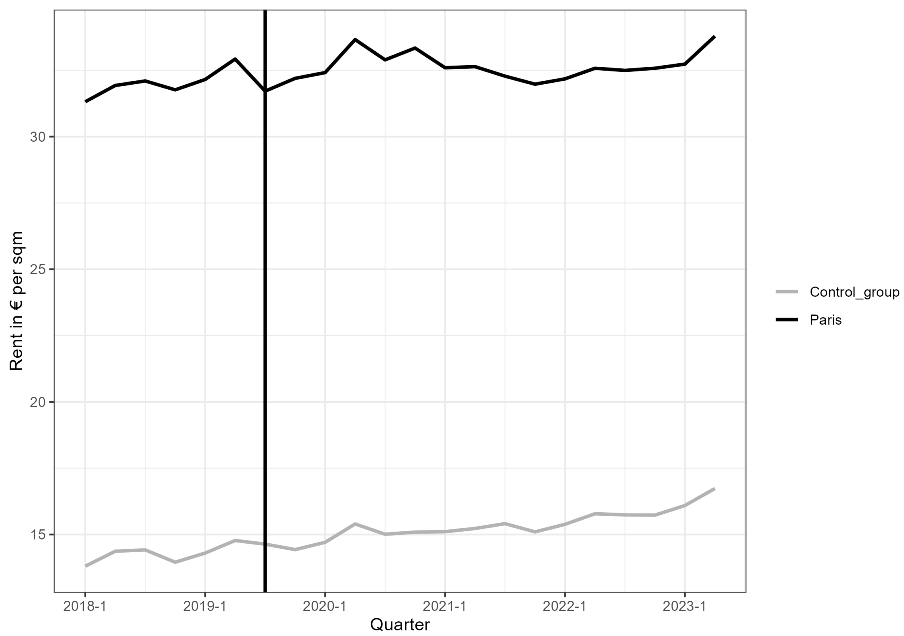


Figure 4: Rent trends of control and treated groups

3.2.2 Difference-in-difference base model

Under PTA, we estimate the following base model comparing changes in rents between treatment and control cities between January 2018 and June 2023:

$$Y_{ict} = \tau T_{ct} + X_{ict}\beta + \mu_{iris} + \mu_t + \varepsilon_{ict} \tag{1}$$

where Y_{ict} is the logarithm of rent (excluding charges) per square meter, T_{ct} is a binary variable equal to 1 if rent control is in place in city c in quarter t, i.e., from the third quarter 2019 in Paris, τ is the treatment effect, X_{ict} is a set of control variables with all housing characteristics, β is the vector of estimated parameters associated with X_{ict}, and ε_{ict} is an independent and identically distributed white noise. μ_{iris} is a spatial fixed effect accounting for the effects of local amenities on rents at the IRIS level[13] and μ_t is a quarter fixed effect. Our set of control variables X_{ict} includes the number of rooms, the log of the area, the building period, whether the dwelling is furnished or not, the floor, the number of bathrooms and toilets, the number of balconies and dummy variables for the presence of a garden, a private parking, a cellar and whether it is a split-level apartment.

3.2.3 Difference-in-difference model with three dimensions of heterogeneity

As rent control in Paris is segmented according to housing characteristics (period of construction, furnished housing, number of rooms) and geographical sectors, we perform a heterogeneity analysis according to three dimensions. First, we investigate whether the treatment effect of the rent control policy is heterogeneous regarding modalities of a variable M_{ict}, which can be either periods between 2 decrees (from July to June), the area of the apartment, the number of rooms, the building period, the type (furnished or not), the 20 Parisian administrative districts and the 14 geographical sectors of the rent control policy. We estimate a specific equation for each variable by interacting its modalities with the treatment variable T_{ct}. Second, we allow for heterogeneous coefficients depending on the group (treated group or control group) of a subset Z of control variables used to determine the reference rent (period of construction, furnished housing, number of rooms).

[13] IRIS are small French statistical units created by the INSEE, with a population of around 2,000 inhabitants and, as much as possible, homogeneous in terms of its housing characteristics. These units are designed to facilitate detailed urban and demographic analysis.

If the observation is in the control group, Z^{co} takes the value of the associated variables, otherwise 0, and vice versa for Z^{tr}. Therefore, these variables are no longer in matrix X. This specification with heterogeneous coefficients β allows us to estimate differentiated shocks regarding the group by dwelling category. Third, we allow for differentiated temporal fixed-effect for each modality of the variable Z, whatever the observation group. For example, regarding the variable number of rooms, this interaction allows for estimating a fixed temporal effect for one room, another for two rooms, etc. The specification also includes a fixed temporal effect for furnished and unfurnished and a fixed temporal effect for each construction period. The model we estimate for the heterogeneity analysis is thus:

$$Y_{ict} = (T_{ct} \times M_{ict})\tau + Z^{co}_{ict}\beta^{co} + Z^{tr}_{ict}\beta^{tr} + X_{ict}\beta + \mu_{iris} + \mu_t + Z_{ict} \times \mu_t + \varepsilon_{ict} \qquad (2)$$

where M_{ict} is the variable interacted with the treatment and τ is the vector associated with the estimated heterogeneous treatment effects for each modality of the variable M_{ict}. Z^{co}_{ict} is the subset of covariates with specific coefficients for the control group (equal to 0 for the treated group), and Z^{tr}_{ict} is the same but for the treated group. Finally, $Z_{ict} \times \mu_t$ is the time fixed-effect specific for each modality of the variables in Z_{ict}. Note that we are not able to estimate a specific regression for each categorical variable in our heterogeneity analysis because we do not have enough observations to cover all IRIS. Using differentiated temporal fixed-effects by category ($Z_{ict} \times \mu_t$) and heterogeneous coefficients by group ($Z^{co}_{ict}\beta^{co}$ and $Z^{tr}_{ict}\beta^{tr}$) allows us to approach a specification where the model would be estimated independently for each property category, e.g., an estimation for properties with 1 room, without proceeding with a separate specification by property category.

We also estimate this model without differentiated treatment effects, i.e.,

$$Y_{ict} = \tau T_{ct} + Z^{co}_{ict}\beta^{co} + Z^{tr}_{ict}\beta^{tr} + X_{ict}\beta + \mu_{iris} + \mu_t + Z_{ict} \times \mu_t + \varepsilon_{ict} \qquad (3)$$

to compare its results to the model using homogeneous coefficients.

In models (1)-(3), standard errors are clustered at the city-level.

4 Results

4.1 Main results

The estimation results of the base model (1) with homogenous coefficients are reported in column (1), and those of model (3) with heterogeneous coefficients and differentiated time-fixed effects are reported in column (2) of Table 2. The treatment is significant, negative, and of comparable magnitude for both specifications. As the "Treatment" variable indicates the impact of rent control on the rent level per square meter expressed in logarithms, we use an exponential transformation to interpret the coefficient in percentage, e.g., exp(-0.038) - 1 for column 1. Thus, the rent control policy caused a decrease in rents in Paris between 3.7% and 4.2%. We retain the more flexible model (3) with heterogeneous coefficients in the rest of the paper.

Table 2: Estimate of the ATT

	Homogeneous coefficients (1)	Heterogeneous coefficients (2)
Treatment (%)	−0.037	−0.042
Treatment	−0.038***	−0.043***
	(0.005)	(0.006)
Num.Obs.	559 300	559 300
R2	0.870	0.879
R2 Adj.	0.870	0.879

p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001
Each regression includes control variables, time fixed-effects and area fixed-effects.
Standard-errors are clustered by city.

We then interact the treatment with the period to obtain an event study, based on model (2). Instead of calendar years, we use periods of 12 months between two decrees of application of thresholds of rent control. For example, "2021" starts on July 1st, 2021, and ends on June 30th, 2022. The period "2018" from July 1st, 2018, to June 30th, 2019, is used as the reference because it is the period before the rent control was implemented. Figure 5 displays the coefficients of interest and 95% confidence intervals from the estimation of the model (2) on rents. Each point estimate represents the weighted-average difference in rents between treatment and control groups for each period relative to the same difference for

July 2018-June 2019, the last period before treatment. We do not find any evidence of pre-trend in the pre-treatment period. Thus, landlords do not seem to have anticipated the rent control policy by raising their rents. The event study also allows testing different pre-trends between the control and treated groups. Because the coefficient is not significant before the policy is applied, it implies that the trend is similar between the treated and control groups, and that PTA cannot be rejected in the pre-treatment period. Our identification strategy also relies on the assumption that the COVID-19 health crisis has similarly affected the Parisian real estate market and those of the control group, which is reasonable following recent studies quantifying mobility flows[14].

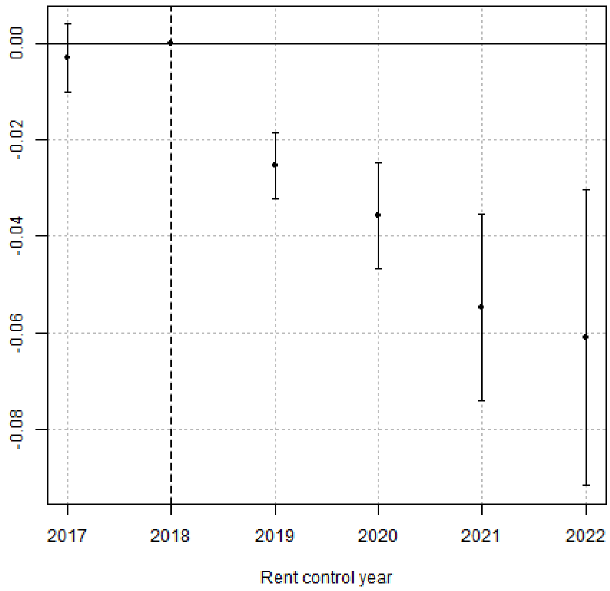


Figure 5: Event study

During the post-treatment period, the rent control policy has highly heterogeneous effects over time, with increasing impacts throughout the years[15]. We find similar results by quarter (Figure B1 of appendix B). Calculating rent reductions in euros attributable to the rent control policy, we show that during the first year of implementation (July 2019-June 2020), the average rent decreased by 39€ per month (or 463€ for the whole year).

[14] https://www.urbanisme-puca.gouv.fr/l-exode-urbain-petits-flux-grands-effets-les-a2388.html

[15] The increase of confidence interval over the rent control periods could be explained by a widening gap between properties that comply with the control and those that do not. On the one hand, properties that comply see their rent gradually decrease compared to the control group. On the other hand, properties that do not comply reduce the effectiveness of the control and thus increase the variance of the estimate.

The effect increased until 2022 to reach a decrease of 97€ per month (or 1165€ for the whole year). Details are given in table C1 of appendix C.

4.2 Heterogeneity analysis

We then perform a heterogeneity analysis by estimating model (2) to estimate the differentiated effect of rent control on rents for various housing characteristics. We first assess the rent control policy's effects depending on the dwellings' size, i.e., 1 room, 2 rooms, 3 rooms, 4 rooms, and 5 rooms and more. Because the price per square meter of dwellings usually decreases with size, the policy effects may be highly dependent on the number of rooms, as well as the area, since they are highly correlated.

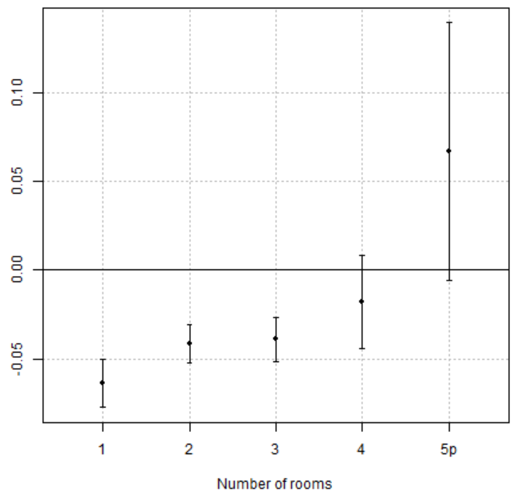


Figure 6: Heterogeneity by number of rooms

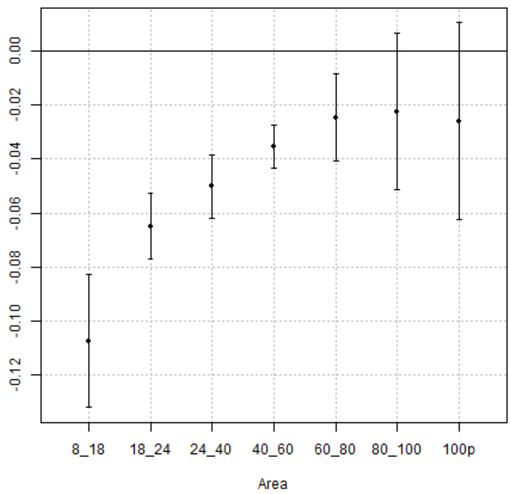


Figure 7: Heterogeneity by area

Figure 6 shows that the effects of the rent control policy indeed differ greatly depending on the number of rooms in the dwelling. 1-room apartments have seen their rents decrease slightly more than 6% while apartments with 2 or 3 rooms show a decrease of around 4%. The effect of the policy for 4-room dwellings is still negative, but it is not significant. For dwellings with 5 rooms or more, the confidence interval is much higher, and the overall effect of the policy is not significant: such dwellings being rarer in our sample, they may also have highly heterogeneous characteristics. Then we consider the heterogeneity of the treatment using dwelling area (figure 7). Because the dwelling area is not included as a categorical variable in our main specification, we slightly change it for this estimation. We replace the number of rooms as a categorical variable by a continuous variable. Instead of having the log of the area, we use the categorical variable reported in figure 7. The effect of the rent-control policy is even more visible for the smallest apartments with an area between 8 and 18 sqm. Their rents decreased by 10.2%. As with the number of rooms, the effect of the policy is lower for medium size apartments, and exhibits a high confidence interval for the larger ones. The ability of rent control to reduce rents thus decreases as the surface area/number of rooms increases. Between July 1st 2019 and June 30th 2023, the average rent for a one-room dwelling is 808€ and the treatment effect -6.2%, implying a counterfactual rent 861€ per month. Thus, the rent decrease caused by the rent control policy amounts to 53€ per month or 641€ per year. Detailed results for all categories are provided in appendix C.

Dwellings being furnished or not does not seem to affect the efficiency of the rent-control policy (see figure 9). While the effect seems a bit more pronounced for furnished dwellings, the confidence intervals of both categories highly overlap. We then assess the effects of the rent control policy depending on the building periods based on the rent control policy thresholds. Figure 8 shows that the results are homogeneous for the building periods. The standard errors of dwellings built before 1945 are higher, which may be explained by a mix of highly valued Haussmann-style buildings and less valued old, dilapidated buildings in this category.

Figure 10 shows the heterogeneity of the rent control policy for each Paris district. The results are highly homogeneous across districts except for the 4th district. The 4th district is located in the centre of Paris, and has a lower number of dwellings compared to other areas. Being at the banks of the River Seine with the Notre-Dame de Paris cathedral, this area is likely to have its rent highly constrained by the rent control policy, explaining why the rent decrease is higher in this area than in others. Taking into account the heterogeneity of treatment by geographical sectors (see Figure 1), the results are reported in figure 11. For most areas, the results are highly homogeneous across the rent control zones. This is consistent with those areas defined as homogeneous housing markets by the OLAP (Rent observatory of the Paris Agglomeration).

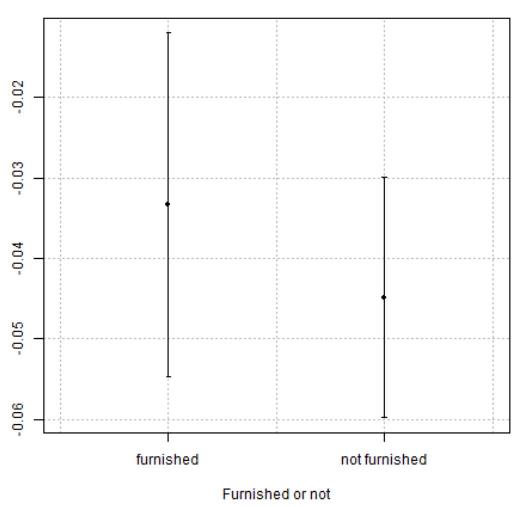
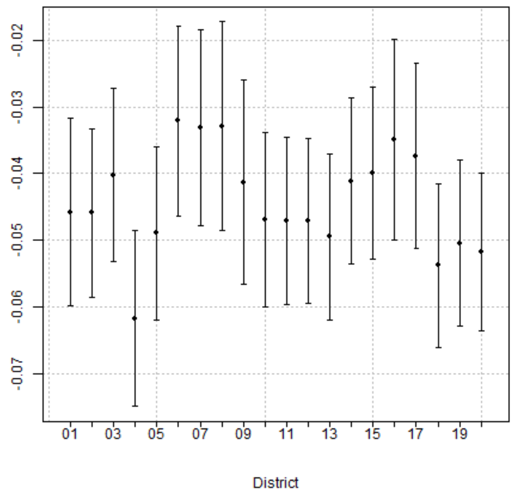


Figure 8: Heterogeneity by building period

Figure 9: Heterogeneity: furnished or not



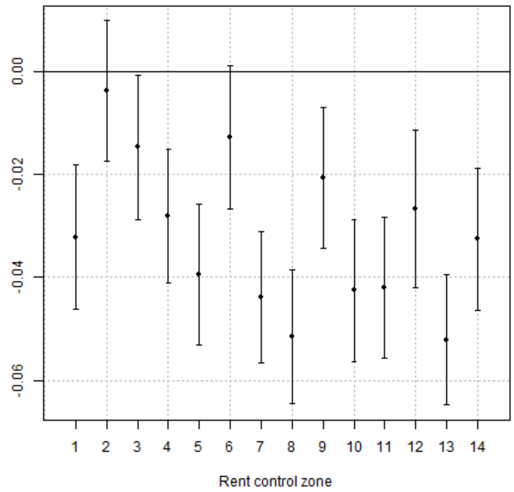


Figure 10: Heterogeneity by district

Figure 11: Heterogeneity by geographical sector

4.3 Heterogeneity by constraint intensity

Previous studies in countries where a rent-controlled sector and a free sector coexist found that the policy succeeded in reducing the rent in the controlled sector but that it increases in the free one (Skak and Bloze, 2013; Chapelle et al., 2019; Mense et al., 2023). The Paris rent control policy affects all dwellings in our dataset, so that there is no sector unaffected by the rent control policy in our data for the treatment group. However, we can define different levels of rent constraint using the difference between the rent threshold and the counterfactual rent.

First, we estimate the counterfactual rent using the DID imputation estimate considered by Borusyak et al. (2024)[16]. We estimate the model for non-treated observations. Thus, we keep the treated group in the pre-treatment period and the control group for all periods. We use the parameter estimates to impute the rent values for the treated group in the post-treatment period. By doing so, we obtain the counterfactual rent $\hat{Y}_{ict}(0)$, i.e., the rent for the treated group if the rent control policy was not introduced. Then, we build a measure of rent constraint such as:

$$RC_{ict} = \frac{\hat{Y}_{ict}(0) - RT_{ict}}{\hat{Y}_{ict}(0)} \tag{4}$$

where RT_{ict} is the rent threshold applicable for the dwelling i at time t in city c (here, in Paris). We divide the difference by the counterfactual rent to avoid size effects; dwellings with a higher rent also exceed the threshold by a larger amount. The measure obtained has negative values for rents that would have been below the rent threshold without implementing the rent control policy and positive in the other case. We also divide this RC_{ict} into 20 quantiles to make it more tractable. Using this variable as a moderator in a heterogeneity analysis, we can assess the effects of the rent control policy depending on the intensity of the constraint of the rent threshold.

As mentioned in the data section, we cannot recover the threshold that applies to each dwelling in our data because we do not have the exact address and building period for all observations. In the post-treatment period, we can identify the rent threshold for 124,688 observations of our 210,544 observations of the treated group. Table 3 displays the estimates of the treatment effect for this restricted sample and the full sample used previously. Columns (1) and (2) report the previous results on the whole sample, while columns (3) and (4) report the result on the sample with only observations with an identified rent control threshold. The estimates of the treatment effect are similar for all columns.

[16] In our case, we use it with a non-staggered treatment.

Table 3: Comparison of the treated effects for different samples

	All obs Homogenous coefficients	All obs Heterogeneous coefficients	Restricted sample Homogenous coefficients	Restricted sample Heterogeneous coefficients
Treatment (%)	0.037	0.042	0.030	0.035
Treatment	−0.038***	−0.043***	−0.031***	−0.035**
	(0.005)	(0.006)	(0.006)	(0.008)
Num.Obs.	559 300	559 300	473 444	473 444
R2	0.870	0.879	0.872	0.881
R2 Adj.	0.870	0.879	0.871	0.881
R2 Within	0.510	0.545	0.530	0.565

$p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Each regression includes control variables, time-fixed effects, and area-fixed effects.
Standard-errors are clustered by city.

For both the cases with homogeneous and heterogeneous coefficients, the estimates on the whole sample are within one standard error of the estimates on the restricted sample. Thus, this new sample displays a treatment effect similar to the one previously estimated and should not add bias to the following analysis.

The results of the heterogeneity analysis by the rent constraint level are reported in figure 12. In general, the rent control policy has been more efficient at reducing rents that would have been over the threshold. The higher the counterfactual rent, the greater the effect of the policy. The rents that are the less constrained (1st quantile), have seen their rent increase by close to 9% despite the rent control policy. The effect is close to 0 and not significant for rent constraints around the 2nd to 8th quantile. The policy significantly reduced rent for the 9th quantile and higher. Thus, rent can rise in the presence of a rent control policy not only for free-market rents but also for rents unaffected by the policy because they are too far away from the threshold set by the policy. Two mechanisms could explain this catch-up of prices for dwellings with the lowest rents. Due to the rent decrease caused by the rent control policy, the demand for regulated dwellings may rise. Dwellings with rents already over the threshold will not be able to increase, but they can be for rent under this threshold. The second mechanism is related to price information provided by the rent control policy. New rent thresholds are provided each year based on the median rent of the two previous years. Thus, having more precise information about rent levels may lead tenants to adjust their rents based on this new information. However, we cannot disentangle between these two mechanisms because it would require information about the

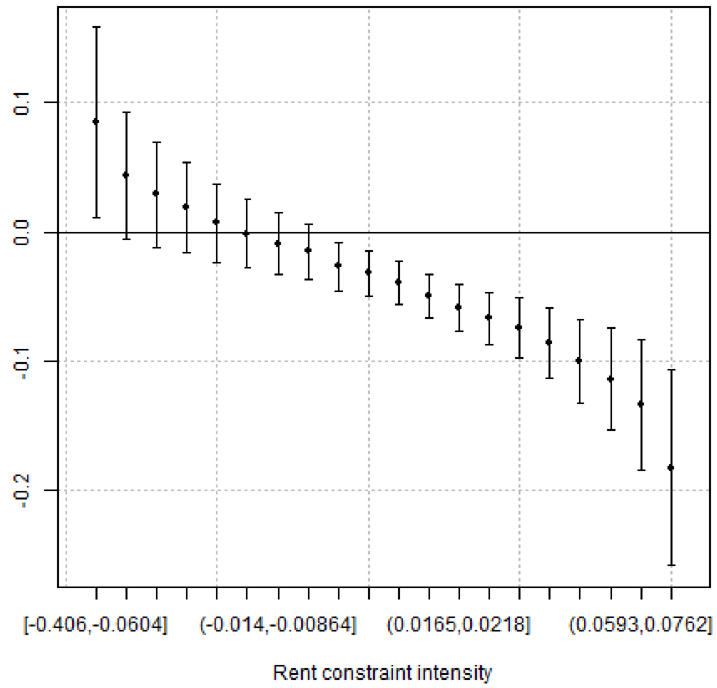


Figure 12: Heterogeneity analysis by level of rent constraint

number of people interested in each dwelling to proxy for the actual demand.

4.4 The potential impact of the rent-control policy if every dwelling respected the rent threshold

We now study the potential impact of the rent-control policy implemented in Paris, if each dwelling had a rent lower or equal to the rent threshold that applies in its case. As described in section 2, a dwelling can have a rent over the threshold if it has an exceptional characteristic distinguishing it from other dwellings in the neighbourhood. However, considering the high share of rents over the threshold, landlords may overestimate the characteristics of their dwellings as a means to have a higher rent than authorized by the policy. Because exceptional characteristics should be rare, so should dwellings with a higher rent than the threshold.

Table 4: Higher bound of the effect of the rent-control policy

	Homogeneous coefficients	Heterogeneous coefficients
Treatment (%)	-0.087	-0.082
Treatment	-0.091***	-0.085***
	(0.001)	(0.002)
Num.Obs.	473 444	473 444

$p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Each regression includes control variables, time fixed-effects and area fixed-effects.
Standard-errors are bootstrapped for 1000 replications.

Estimating the effects of the policy if everyone respected the rent threshold is equivalent to estimating the higher bound of the policy effect. To do so, we replace the rents higher than the maximum threshold with the value of this threshold. Then, we estimate the model using difference-in-differences on the sample with observations for which we can identify the applicable rent threshold. Because our procedure implies replacing the value of the rent by the maximum rent allowed if it is over the threshold, we do not use analytical standard errors, instead, we use Bootstrap with 1000 replications. The results are reported in table 4 with column (1) containing the results obtained with homogeneous coefficients (see equation 1) and column (2) containing the results obtained with heterogeneous coefficients (see equation 3). The estimated effects are highly significant for both cases and of similar magnitude. According to these estimates, if every dwelling respected the rent-control policy in Paris, rent would be 8.2 to 8.7% lower than in the control group. This effect is more than twice as large as the current effect of the rent-control policy. Since January 1st, 2023, the Paris City Hall of Paris has helped tenants who have rents higher than the maximum authorized rent threshold to sue landlords. It should allow the policy to have a higher effect on rent decrease, for which we now know the upper bound of the effect.

5 Robustness checks

5.1 Specificities of small dwellings

One of the specificities of the Parisian housing market is the number of small apartments. These are often only dwellings people with low income can afford in a rental market with high prices like Paris. Such dwellings are also more often furnished than the larger ones. Therefore, the issue is that such dwellings may have a different price trend in the treated and control group. To test whether PTA still holds for this category, we estimate the model with heterogeneous coefficients and heterogeneous time-fixed effects for dwellings with one room. Because we have a high number of such dwellings, we are still able to properly estimate the local fixed effects at the IRIS level that capture the effect of local amenities on rents.

Table 5: Comparison of the treatment effect for dwellings with 1 room

	All observations (1)	Dwellings with 1 room (2)
Treatment (%)	−0.062	−0.055
treatment	−0.064***	−0.056***
	(0.006)	(0.006)
Num.Obs.	559 300	172 295
R2	0.880	0.864
R2 Adj.	0.879	0.862

p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001
Each regression includes control variables, time fixed-effects and area fixed-effects.
Standard-errors are clustered by city.

Table 5 compares the estimated treatment effect for two estimations. In column (1), we report the same estimate as in the heterogeneity analysis in section 4 (we report only the treatment effect estimated for dwellings with 1 room) while in column (2) we estimate a model specific for dwellings with one room. The estimated treatment effects for both models are close. The implied decrease in rent is 6.2% for the model estimated on all observations and 5.5% for the model estimated only on dwellings with one room. Moreover, both estimates are just over one standard error apart, meaning that their confidence intervals widely overlap.

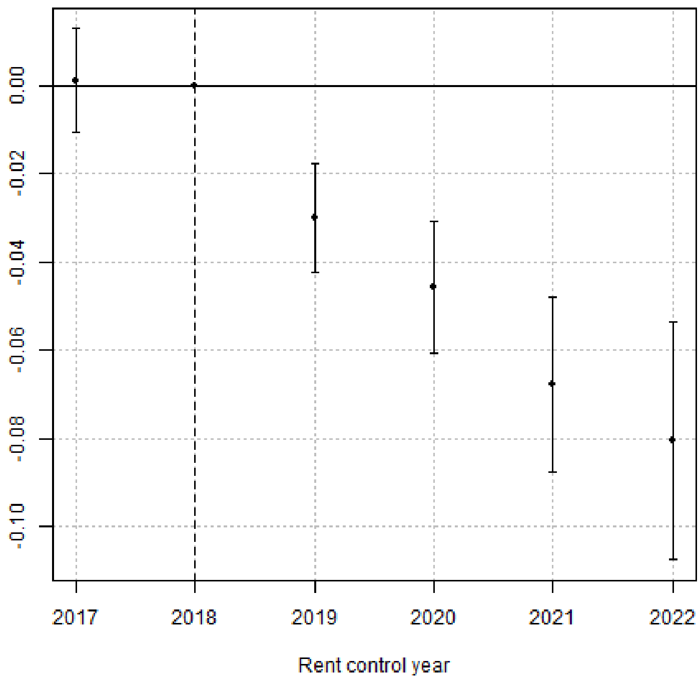


Figure 13: Event study for dwellings with 1 room

Figure 13 reports an event study specific for the model estimated only for dwellings with one room. In the pre-treatment period, the point estimate of the treatment effect is near 0 and its confidence interval crosses 0. Thus, there is no evidence of pre-trend before the treatment is applied and PTA cannot be rejected. Considering there is no significant difference between the treatment effect estimated on all observations and only on dwellings with one room, and that PTA holds in the pre-treatment period for single-room dwellings, we conclude that there is no difference that could bias our results between single-room dwellings in the treated and control group.

5.2 Robustness to PTA

The estimates we report in this paper rely on PTA. According to the event studies, we do not find any evidence of pre-trend, meaning that the parallel trend assumption is respected in the pre-treatment period. To further test the robustness of our results to this hypothesis, we use an estimate that does not rely on this assumption. Because the rents in Paris are the highest in our sample, we cannot use synthetic control methods. Indeed, they rely on building a counterfactual with a weighted average of other cities of our control group, with the weights in the interval $[0, 1]$. Instead, we use the synthetic difference-in-differences (SDiD hereafter).

SDiD (Arkhangelsky et al., 2021) proceeds in two steps. First, it computes individual weights ω_c similar to the synthetic control literature, but rather than matching the outcome of the treated group with a weighted combination of the control group, it matches its trend by allowing for an intercept in the weight computation. It also computes time weights λ_t for the pre-treatment period that matches the post-treatment period in the control group. In the second step, it estimates a weighted DID regression using the product of the two weighted described previously. The SDID method is only usable for panel data, but in this paper, we use repeated cross-sectional data. To use SDID on such data, we first aggregate our dataset at the city level. Then, we compute individual and time weights as described in Arkhangelsky et al. (2021). Because the number of observations differs in each city-period pair, the individual weights ω_c do not allow to match the trend of the outcome of the treated and control group. Thus, we add a third type of weights such as:

$$\nu_{c,t}^{RC} = \frac{1}{N_{c,t}} \tag{5}$$

where $N_{c,t}$ is the number of observations in the city c at time t. The weights $\nu_{c,t}^{RC}$ sum to 1 for each group-period pair. These weights also allow making each period equally weighted (as was the case when computing the weights λ_t on aggregated data). Finally, we can estimate the treatment effect by computing the following weighted DID regression on non-aggregated data:

$$\left(\hat{\tau}^{rc-sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{ict} - \mu - \alpha_k - \beta_t - W_{ct}\tau\right)^2 \omega_k^{\text{sdid}} \lambda_t^{\text{sdid}} \nu_{c,t}^{RC} \right\} \tag{6}$$

We use the method of Kranz (2021) to include covariates in the analysis. We adjust for covariates in the model before computing the weights. To do so, we remove the effect of the covariates on the variable Y_{ict} before computing the synthetic weights. We estimate the following regression on observations that are not treated (control group and not yet treated):

$$Y_{ict} = X_{ict}\theta + \alpha_c + \beta_t + \varepsilon_{ict} \tag{7}$$

Then, we compute the covariate-adjusted outcome for each observation such as:

$$Y_{ict}^{adj} = Y_{ict} - X_{ict}\hat{\theta} \tag{8}$$

Once Y_{ict}^{adj} is computed, we can applied the procedure describe previously to compute the synthetic weights.

Table 6: Comparison of the treatment effect using SDiD and RC-SDiD

	DID homogeneous coefficients (1)	RC-SDID homogeneous coefficients (2)	DID heterogeneous coefficients (3)	RC-SDID heterogeneous coefficients (4)
Treatment (%)	-0.037	-0.039	-0.042	-0.042
treatment	-0.038***	-0.040***	-0.043***	-0.043***
	(0.005)	(0.003)	(0.006)	(0.003)
Num.Obs.	559 300	425 044	559 300	450 575
R2	0.870	0.831	0.879	0.884
R2 Within	0.510	0.003	0.545	0.004

$p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Each regression includes control variables, time fixed-effects and area fixed-effects.
Standard-errors are clustered by city for columns (1) and (3) and are bootstrapped for columns (2) and (4).

In table 6 we compare the estimate we obtain using DiD (columns (1) and (3)) and SDiD for repeated cross-sectional data (RC-SDID heteafter) estimates (columns (2) and (4)). The results are presented both for the model with homogeneous coefficients (columns (1) and (2)) and with heterogeneous coefficients (columns (3) and (4)). For the model estimated with RC-SDID, standard-errors, and p-values are computed using bootstrap with 1000 repetitions. For both models, the estimated treatment effect of the rent control policy is quite similar. For homogeneous coefficients, we find a decrease of 3.7% with DiD and 3.9% for RC-SDiD, and the same effect is estimated with heterogeneous coefficients: a decrease of 4.2% of rents. Because of how close the results are for both methods, our results are robust to the PTA used in this paper.

6 Conclusion

In this paper, we evaluate the impact of the rent-control policy implemented in Paris since July 2019 on the Parisian rental market. We find that the policy decreased rents in Paris by 3.7% to 4.2% on average. We also find that the effect of the policy is highly heterogeneous across dwelling sizes. This effect is due to the small apartments often having higher prices per sqm than bigger ones. Because the policy separates dwellings based on the number of rooms, the effect is more pronounced for the small apartments (less than 18 sqm). This effect can be considered positive because such dwellings are occupied mainly by students or people with a low income. By studying the effect of the policy depending on the difference between the actual rent and the counterfactual rent (rent constraint), we show that the 5% dwellings the least constrained by the policy actually had their rent increase because of the policy while the rents decreased for the 9th quantile and higher. We also find that, if every dwelling respected the maximum rent threshold, the rent decrease would have been from 8.2% to 8.7%. Thus, enforcing the respect of the rent thresholds could greatly improve the effect of the rent control policy on rent decreases.

From a policy point of view, this policy has the advantage of slowing the growth of rents (because the rent threshold depends on the median of the past two years), without freezing rents that would be too unfavorable for landlords. However, rent control policies also have limits (see Kholodilin, 2024 for a review). Thus, a further research avenue is to evaluate the effects of this policy on the evolution of the dwellings, for both quantities (the number of dwellings available) and quality (how much landlords are maintaining their apartments). Indeed, some landlords may choose to sell their apartment if they consider the rent too low to be profitable or stop renovating it. This effect could have been worsened by the ongoing rise of interest rates and by the interdiction in Paris to apply extra charges over the rent threshold if the dwelling has poor energy performance (since January 2023).

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Appendices

Appendix A Rent control compliance statistics

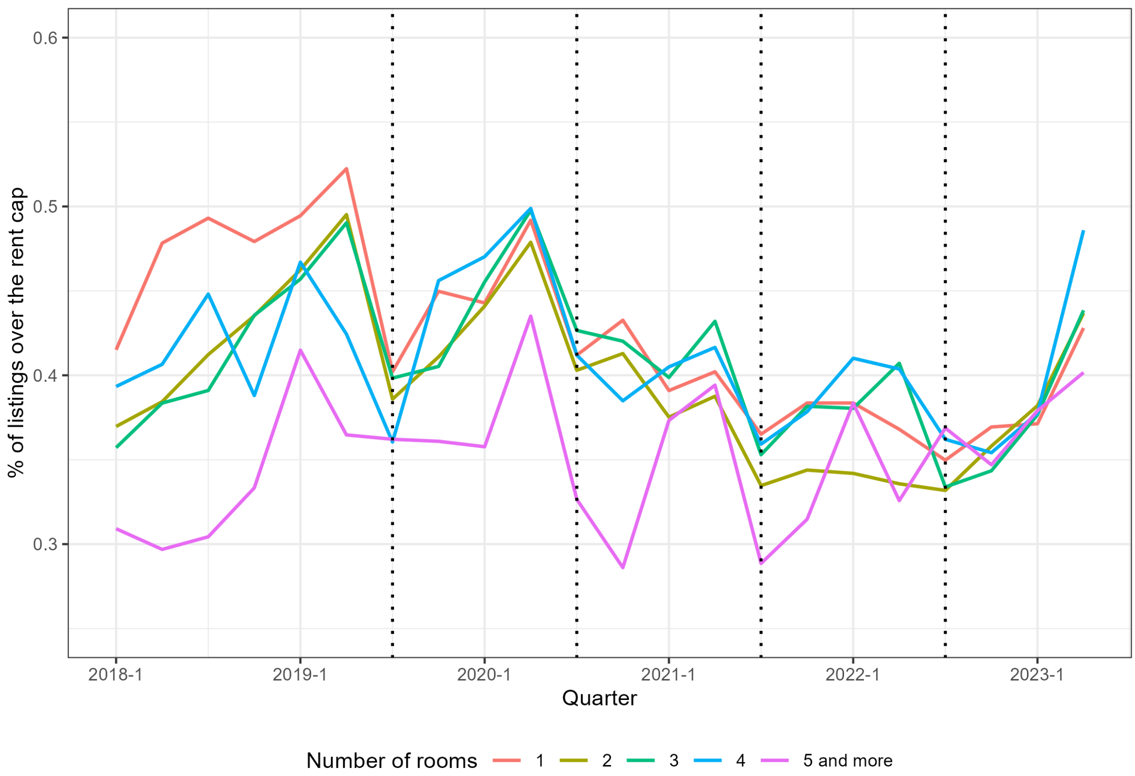

Figure A1: Percent of dwellings over the rent cap by number of rooms

Appendix B Event study by quarter

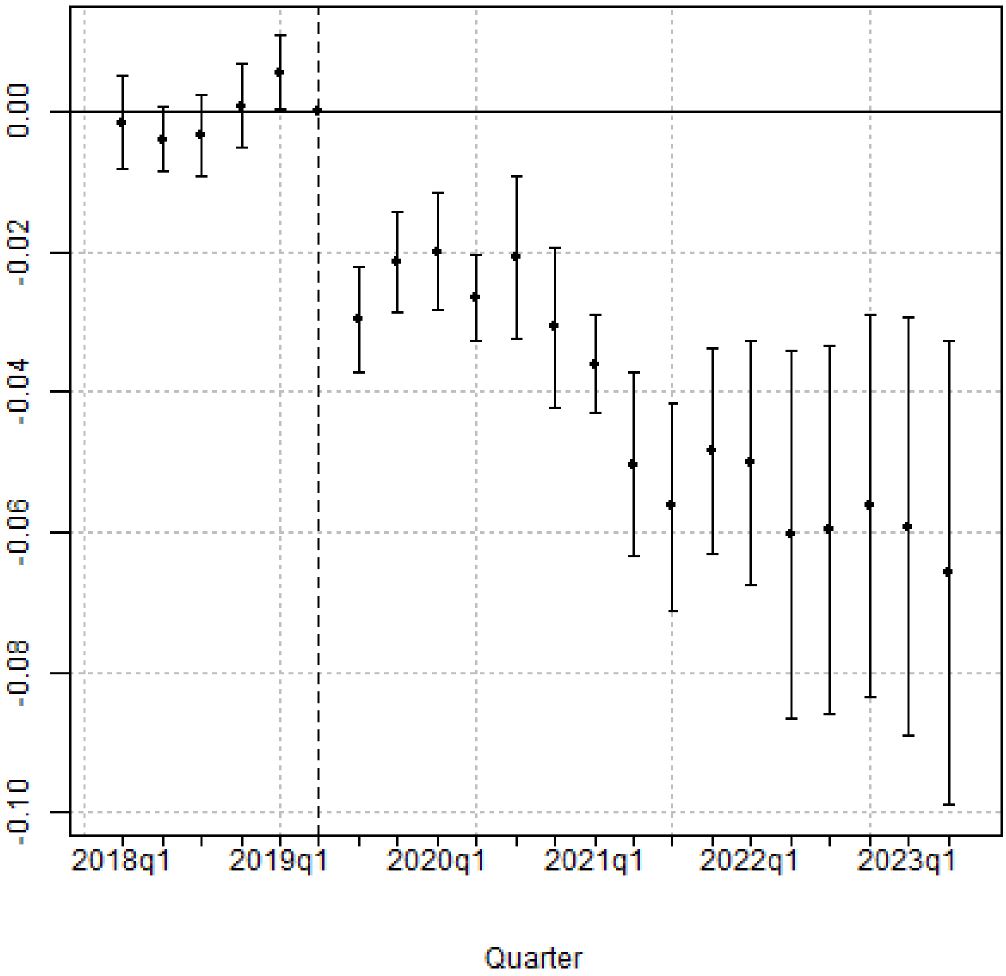

Figure B1: Event study by quarter

Appendix C Rent variations

Table C1: Rent variation per year

	2019	2020	2021	2022
Observed rent (€)	1504	1429	1427	1549
Treatment effect (%)	-2.5	-3.5	-5.3	-5.9
Counterfactual rent (€)	1543	1481	1507	1646
Rent variation (€/month)	-39	-52	-80	-97
Rent variation (€/year)	-463	-622	-958	-1165

Table C2: Rent variation per number of rooms

	1 room	2 rooms	3 rooms	4 rooms	5 and more rooms
Observed rent (€)	808	1282	1922	2783	4100
Treatment effect (%)	-6.2	-4.1	-3.8	-1.8	6.9
Counterfactual rent (€)	861	1337	1998	2834	3835
Rent variation (€/month)	-53	-55	-76	-51	265
Rent variation (€/year)	-641	-658	-911	-612	3176

Table C3: Rent variation per area

	8 to 18sqm	18 to 24sqm	24 to 40sqm	40 to 60sqm	60 to 80sqm	80 to 100sqm	$\geq$ 100 sqm
Observed rent (€)	612	790	1032	1471	1992	2624	4029
Treatment effect (%)	-10.2	-6.3	-4.9	-3.5	-2.4	-2.2	-2.6
Counterfactual rent (€)	682	843	1085	1524	2041	2683	4137
Rent variation (€/month)	-70	-53	-53	-53	-49	-59	-108
Rent variation (€/year)	-834	-637	-638	-640	-588	-708	-1291