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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 real-time data available 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 effect of the policy is heterogeneous depending on dwelling characteristics, with a stronger effect on small apartments. We also estimate the upper bound of the effectiveness of the policy and show that if every dwelling respected the rent control, rents would have decreased by 8.2% to 8.7%. We confirm the effectiveness of the rent control policy by extending the analysis to five additional regulated cities using a staggered difference-in-differences strategy, which reinforces the external validity of our findings. Finally, we examine whether the policy affected the supply of rental housing, proxied by the number of new listings published by agencies. We find no evidence of a decline in supply attributable to the rent control.

1. Introduction

Between 2015 and 2023, nominal rental prices increased by 13% in the EU zone and by 42% in the United States (from the OECD data explorer). Among the various measures adopted to maintain affordable housing, rent control has come back into force since the 2010s in numerous cities or countries (e.g., Catalonia and California since 2020; Berlin with the Berlin Rent Cap Act "Mietendeckel", which was in force between February 2020 and March 2021, in addition to the rent cap "Mietpreisbremse" laid down in federal law since 2015) after being widely withdrawn (Kholodilin, 2020).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 affects the willingness to pay for dwelling's attributes (Van Ommeren and 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 1149 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

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[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.

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annual evolution of the "Indice de référence des loyers" since 2014. Second, cities in 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 (the date of 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, nearly 70 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 analyzed 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, whether on rents or on supply, 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. We also construct a panel at the agency level, covering 513 agencies operating in Paris and 342 agencies in the control cities to track changes in the flow of new listings over time. Our work then contributes to the growing literature using internet-based datasets to study the rental market in France and more widely in Europe: Mense et al. (2023), Breidenbach et al. (2022), 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 on 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, under the standard assumptions of parallel trends in rents, no anticipation, and no spillover effects.

Third, we study the heterogeneous effects of the policy. Rent ceilings 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. Dwellings with a high rent constraint intensity are the ones that have seen their rents decrease the most. On the other hand, a low rent compared to the maximum rent could encourage landlords to raise their rent closer to the cap.

Finally, we estimate the upper bound of the rent control policy's effectiveness. Indeed, around 40% of our sample's observations do not comply with the rent control. 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 to evaluate the impact on rents 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 chose all rental ads published in eight major French cities in which the rental market is particularly tense, but that did not choose to set up a rent control in their own rental market during the study period. Since the Parallel Trends Assumption (PTA) cannot be directly tested, we investigate pre-treatment trends to provide supporting evidence for its plausibility in identifying the causal effect of the rent control policy. 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. In parallel, we apply a similar difference-in-differences strategy to estimate the effect of rent control on rental supply. The identification strategy relies on comparing changes in the number of rental listings posted by the panel of agencies before and after the policy implementation, controlling for agency and year-by-quarter fixed effects. To reinforce the external validity of our evaluation of the effectiveness of the Parisian regulation, we extend our analysis using a staggered difference-in-differences framework covering six cities that implemented rent control at different points in time.

Our results show that the rent control policy caused a rent decrease of 3.7% to 4.2%. We also estimate the upper bound of the effectiveness of the policy: if all dwellings respected the rent control, the actual rent decrease would have been between 8.2% and 8.7%. The extension of the analysis to five additional regulated cities confirms the relevance of rent control as a tool to moderate rent increases. 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 least 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. As for rental supply, we find no evidence that the policy led to a structural decline in the number of listings. A temporary increase is observed during the COVID-19 period—likely driven by a shift from short-term to long-term rentals, but once this period is excluded, the estimated effect, while slightly negative, is not significant.

The rest of the paper is organized 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 concludes.

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

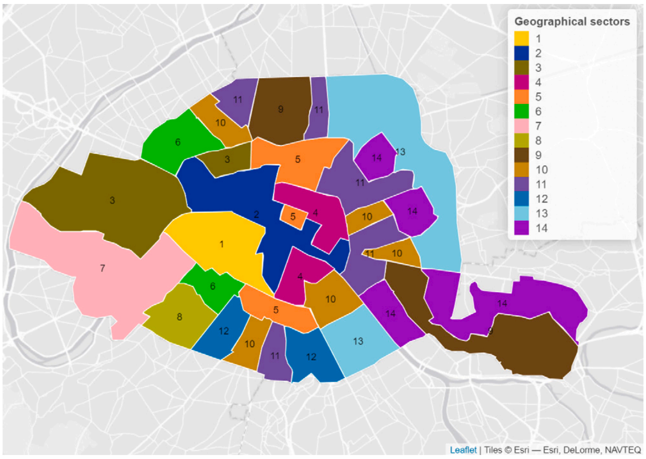

Fig. 1. Geographical sectors of the rent control policy in Paris.

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 nearly 70 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 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/EIMC, 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 Fig. 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 below plus 20% of this median ("loyer majoré").[7] 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)",[8] introduced in 1989, initially only regulated the rental market in the Parisian agglomeration by constraining the maximum yearly increase that the owner could apply[9] 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.[10]

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. Several recent empirical studies have shown that the bias between posted rents and lease rentals is minimal and even insignificant for French data in major cities. Chapelle and Eyméoud (2022) compared lease rentals collected by French Local Observatories (including OLAP) or Clameur to rental ads collected through web-scraping on the two largest French real estate websites (SeLoger, where our data comes from as part of a contractual agreement, and leboncoin) between December 2015 and January 2018 and across 35 urban areas. Specifically, they estimate the effect of the data source (Clameur or online platforms) on average and median rents using aggregated data at the urban area level by room category and data source. They show that online ads provide a non-biased picture of rental markets since they "do not find any significant differences between the rent measured from online ads and the average rent measured with surveys". Regarding other countries, Livingston et al. (2021) demonstrated that Zoopla rents in the UK are very similar to those estimated by the Valuation Office Agency, while Mense et al. (2023) concluded that

[4] The court found that the limited application to Paris violated the article 17 of the "loi pour l'Accès au Logement et un Urbanisme Rénové" (ALUR law) of March 24, 2014. See https://www.legifrance.gouv.fr/jorf/id/JORFTEXT000028772256 for the content of the ALUR law, 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 "loi portant Evolution du Logement, de l'Aménagement et du Numérique" (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. See https://www.legifrance.gouv.fr/loda/id/JORFTEXT000037639478 for the content of the ELAN law.

[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] A "loyer minoré" (minus 30% of this median) is also defined. This value is not a minimum threshold, as the landlord is free to set a lower rent.

[8] The Indice de Référence des Loyers (IRL) is calculated each year by INSEE (French National Institute for Statistical and Economic Studies) 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.

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

[10] 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 nearly 70 cities regulates rents both within and between tenancies; hence, it can be considered a second-generation rent control policy.

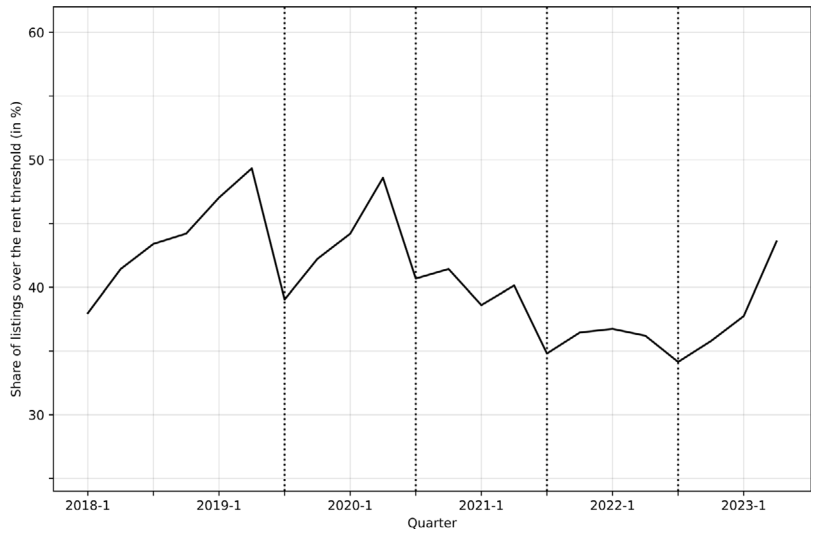

Fig. 2. Trends in exceeding the maximum reference rent threshold in %.

over extended periods, posted rents and final signed rents in Germany converge, and use platform data to evaluate the impact of rent control. These findings, along with the highly competitive nature of the selected cities, all located in tense areas, are very reassuring in confirming that rent negotiation is minimal, if not nonexistent. As a result, real estate listings on online platforms serve as reliable indicators of actual rental prices in such areas. To analyze the impact of the rent control policy, we rely on two complementary datasets provided by SeLoger.

3.1.1. Rental price data

The first dataset gathers all rent listings of apartments posted on SeLoger's portal between January 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 subsequent causal inference analysis since only the absolute rent level (rather than the relative level compared to the reference rent) is required.

We show in Fig. 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 ceilings 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 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. This recurrent increase (of a few percentage points) in the share of listings exceeding the rent threshold, before each new decree comes into effect on July 1st, can be explained by the fact that our data are based on listings. Landlords tend to anticipate that the lease will be signed after July 1st, adjusting their prices accordingly when posting the listing online. Moreover, this cannot be interpreted as an anticipation effect, because it does not account for the rent variation in the control group. Thus, these variations could be the global rent trend. We will further investigate this in the event study in Section 4. 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 18 sqm, 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,[11] 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[12] 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 upper bound of the effect of the policy.

3.1.2. Ad volume data

The second dataset records the number of listings published on the SeLoger platform and is aggregated at the real estate agency level on a quarterly basis, providing a proxy for the flow of rental housing supply over time. To mitigate the risk of capturing variations in the platform's popularity rather than genuine changes in supply, we restrict the sample to agencies that consistently posted listings in every quarter of the observation period.

3.2. Method

This paper employs a difference-in-differences (DiD) design to estimate the effects of Parisian rent control on both rent levels and the flow of rental supply. This empirical strategy allows us to measure the treatment effects across different segments of the rental market and examine how the impact of the policy evolves over time.

3.2.1. Treatment and control groups for rent-level estimation

When analyzing rent levels, 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.

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

12 https://www.legifrance.gouv.fr/loda/article_lc/LEGIARTI000037642425/.

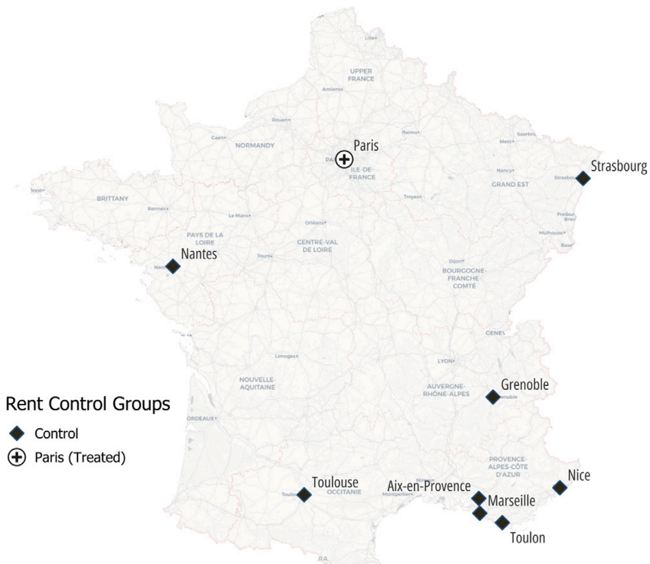

Fig. 3. Control and treatment cities.

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 control groups to comply with the SUTVA hypothesis,[13] (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.[14]

Fig. 4 shows that the aggregate rent (in log) 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).

3.2.2. Treatment and control groups for rental ad volume estimation

When analyzing the number of ads, the unit of observation is the real estate agency.[15] The treatment group includes 513 agencies operating in Paris, which we track over 22 quarters, resulting in a total of 11,286 observations. As for the control group, it consists of 342 agencies located in the eight selected control cities, also observed over 22 quarters, yielding an equivalent number of 7524 observations.

Fig. 5 represents the number of ads per quarter for both groups over the full period. Prior to the implementation of rent control, trends in the number of ads in Paris closely follow those of the control group, suggesting similar dynamics across the two markets. Following the introduction of rent control, we observe a decline in the number of ads that is similar across both groups, followed by a noticeable increase in the number of ads in Paris relative to the control group between the second quarter of 2020 and the first quarter of 2021. This divergence may reflect the impact of the COVID-19 pandemic: faced with a sharp decline in tourism, some landlords may have redirected properties from the short-term rental market to the long-term rental segment. Interestingly, the sharp increase in the number of ads observed during the COVID-19 period is even more marked in the furnished rental segment. This trend may reflect landlords transitioning their properties from the seasonal rental market to the traditional rental market due to the sudden decline in tourism. From the second quarter of 2022 onward, the number of ads in Paris began to decline, yet it continues to follow a trend that remains broadly aligned with that of the control group.

Fig. 6 focuses exclusively on unfurnished dwellings (a similar graph for furnished dwellings is provided in appendix H, figure H1).

To account for the potential distortions introduced by this transitory exogenous shock, we perform all our estimations – both on rent levels and on the flow of supply – by including and excluding the COVID-19 period, which is defined as the quarters from 2020Q2 to 2022Q4, i.e., the period most directly affected by the pandemic and lockdowns in France. This ensures that our results are not driven by the pandemic-related disruptions.

3.2.3. Difference-in-difference base model for rents

Under the 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[16] and μ_t is a year-by-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.

3.2.4. Difference-in-difference model with three dimensions of heterogeneity for rent-level estimation

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

[13] 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.

[14] Missing build years are treated as a distinct category within the construction period variable. Similarly, all missing values for categorical variables are handled by introducing a separate 'missing' category, except for the number of rooms for which we do not have missing values.

[15] Each real-estate agency operates in only one city, ensuring that agency-level activity can be uniquely attributed to a single local market.

[16] IRIS are small French statistical units created by the INSEE, with a population of around 2000 inhabitants and, as much as possible, homogeneous in terms of its housing characteristics. These units are designed to facilitate detailed urban and demographic analysis.

Table 1
Descriptive statistics for the control and treated groups.

		Control group (N = 275 079)		Paris (N =284 221)		p-value
		Mean	Std. Dev.	Mean	Std. Dev.	
Rent per sqm		15.0	5.3	32.5	8.6	<0.001
Area		48.9	24.8	48.0	32.8	<0.001
		Number	%	Number	%	
Number of rooms	1	77 017	28.0	95 278	33.5	<0.001
	2	99 330	36.1	105 553	37.1	<0.001
	3	70 523	25.6	49 662	17.5	<0.001
	4	22 443	8.2	19 581	6.9	<0.001
	5 and more	5766	2.1	14 147	5.0	<0.001
Furnished	Yes	63 571	23.1	119 571	42.1	<0.001
	Missing	20 352	7.4	17 617	6.2	<0.001
	No	191 156	69.5	147 033	51.7	<0.001
Building period	Before 1945	20 501	7.5	150 618	53.0	<0.001
	1946–1970	22 012	8.0	26 368	9.3	<0.001
	1971–1990	14 899	5.4	17 082	6.0	<0.001
	After 1990	48 963	17.8	11 764	4.1	<0.001
	Missing	168 704	61.3	78 389	27.6	<0.001

The p-value column reports the results of the test of the difference in mean/proportion of the modality between the control and treated group.

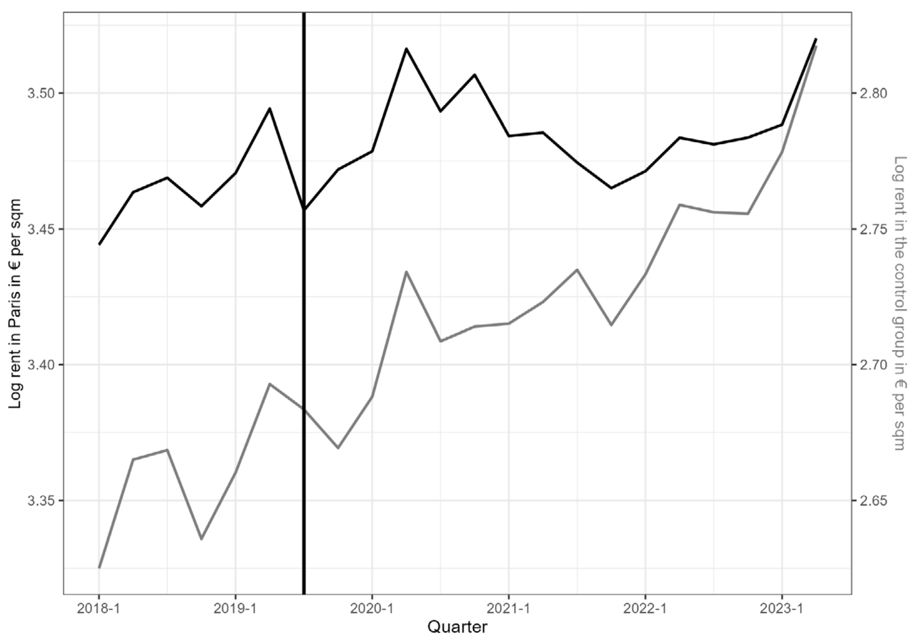

Fig. 4. Rent trends of control and treated groups (in log).

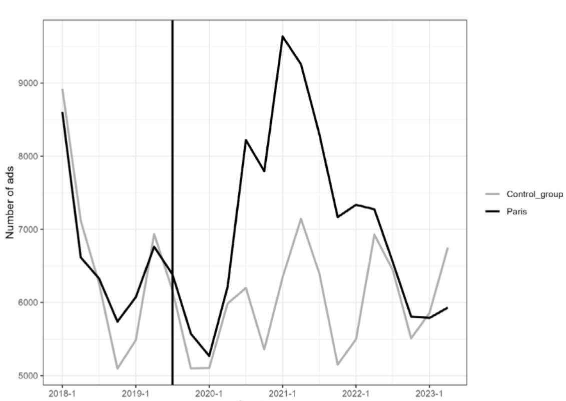

Fig. 5. Number of ads for all dwellings.

Fig. 6. Number of ads for unfurnished dwellings.

variable M_{ict}, which can be either period (at the quarter level), 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.[17] 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). 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. This specification for analyzing the heterogeneous effects of the policy has several advantages. Typically, there are two main approaches to conducting a heterogeneity analysis. First, we can use the entire sample and estimate a treatment effect for each modality of the variable where we expect treatment heterogeneity. Second, we can restrict the sample to a specific subset that includes only the modality of interest and estimate the treatment effect on this subset. In the second approach, the specification allows for individual and time-fixed effects specific to the modality for which we estimate a treatment effect. Additionally, since the sample is limited to a subset with one specific modality, the coefficient associated with that modality is absorbed by the individual fixed effect. This second approach is more general because fixed effect parameters are specific to the modality. However, in our case, we do not have enough observations to estimate the second model for all modalities, as we would not be able to identify individual fixed effects at the IRIS level. By using a specification that includes heterogeneous coefficients for the treated/control group and time-fixed effects by modality, we achieve a setup closer to this second method while still maintaining enough observations for reliable estimation.

The model we estimate for the heterogeneity analysis is thus:

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

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.,

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

to compare its results to the model using homogeneous coefficients.

In models (1)–(3), standard errors are clustered at the municipality level.

3.2.5. Difference-in-difference base model for rental ad volume

Under the PTA, we estimate the following model comparing the changes in the number of rental ads between treatment and control cities between January 2018 and June 2023:

$$Y_{act} = \tau T_{ct} + \overline{X}_{ict}\theta + \mu_a + \mu_t + \varepsilon_{act} \quad (4)$$

where Y_{act} is the log of the number of ads published by agency a in year-by-quarter t in city c, and T_{ct} is the binary treatment variable equal to 1 if rent control is in place in city c during year-by-quarter t, i.e., from the third quarter of 2019 in Paris. The parameter τ captures the average treatment effect of rent control on ad counts. $\overline{X}_{ict}$ is the vector of average dwelling characteristics of rental ads published by agency a in year-by-quarter t in city c,[18] θ is the vector of estimated parameters associated with $\overline{X}_{ict}$. μ_a represents agency fixed effects, which control for time-invariant characteristics of each agency (including its location, size, and publication behavior), and μ_t denotes year-by-quarter fixed effects, which absorb national or seasonal shocks that affect all cities simultaneously. ε_{act} is the error term. This simplified specification omits housing characteristics since the unit of observation is no longer the dwelling but the agency.

4. Results on the effects of rent control

4.1. Causal impact of rent control on rent dynamics

4.1.1. Causal impact of rent control in Paris

The estimated treatment effects for the base model (1) with homogeneous coefficients are reported in column (1), and those for model (3) with heterogeneous coefficients and differentiated time-fixed effects are reported in column (3) of Table 2. The table with all estimated coefficients can be found in Appendix F. The treatment is significant, negative, and of comparable magnitude for both specifications.[19] 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.[20, 21] In appendix I we also estimate the model using a modified version of the synthetic difference-in-differences (Arkhangelsky et al., 2021; Kranz, 2021) that accounts for the repeated cross-sections of our dataset. It relies on an assumption that is weaker than the PTA. The estimates we obtain are similar to the DiD results.

[17] The modalities of the variable M_{ict} (e.g., 1 room, 2 rooms, furnished, built between 1971–1999) do not change between decrees. Only the rent ceilings associated with the combination of the three housing characteristics (building period, furnished status, number of rooms) and geographical sectors can change with each new decree.

[18] We use the same variables as we did at the dwelling level, taking the average for continuous variables and the proportion for categorical variables.

[19] We compared the standard errors at different geographical levels (city level versus Grand Quartier), and it confirms the significance of the estimated effect of the rent control policy.

[20] We also conducted an F-test to test if the heterogeneous coefficients (unrestricted model) are significantly different from the homogeneous ones (restricted model). The value of the F-test is 714.620, which leads us to reject the equality of the coefficients at a significance level below 0.001.

[21] To ensure that the earlier rent control policy did not have lingering effects on our estimates, we re-estimated the model using alternative pre-treatment periods that exclude the immediate aftermath of the first policy episode. The findings are consistent with institutional reports suggesting no lasting impact. In particular, the 2019 annual report on 2018 rent levels published by OLAP (Observatoire des Loyers de l'Agglomération Parisienne) states that "the rent control effect [was] forgotten as soon as the policy ended" (L'Observatoire des Loyers l'Agglomération Parisienne, 2019).

Table 2
Estimates of the ATT.

	Hom. coeff. (1)	Hom. coeff. no COVID (2)	Heter. coeff. (3)	Heter. coeff. no COVID (4)
Treatment (%)	−3.7	−2.9	−4.2	−3.8
Treatment	−0.038***	−0.030***	−0.043***	−0.038***
	(0.005)	(0.005)	(0.006)	(0.006)
Num.Obs.	559 300	264 757	559 300	264 757
R2	0.870	0.879	0.879	0.887
R2 Within	0.510	0.522	0.545	0.554

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

To ensure that our estimates of the rent control policy are not confounded by the effects of the COVID-19 pandemic, we complement our main specifications with an additional analysis excluding the COVID-19 period (from 2020Q2 to 2022Q4, that are characterized by several lockdowns and significant changes in mobility patterns.[22]) since it may have altered landlord behavior independently of the policy. Therefore, we replicate our two difference-in-differences models, both with homogeneous and heterogeneous coefficients, on a sample restricted to pre-pandemic and post-pandemic quarters. The corresponding results are reported in the additional columns (2) and (4) of Table 2. The persistence and statistical significance of the treatment effect (albeit slightly smaller in magnitude) in this restricted sample confirm the robustness of our main findings.

We then interact the treatment with the period to obtain an event study by year-by-quarter, based on model (2). Fig. 7 displays the coefficients of interest and 95% confidence intervals (the table with the detailed results can be found in appendix B, table B1). Each point estimate represents the weighted-average difference in rents between treatment and control groups for each period relative to the same difference for 2019q2, the last period before treatment. We do not find any evidence of an anticipation effect in the first four quarter of the pre-treatment period. The first quarter of 2019 is significant, but only at 5%. Considering the low significance of this estimate and the low value of the coefficient (0.6%), this potential anticipation of landlords raising their rent before the policy should not affect our estimates. If anything, its positive sign implies that the magnitude of the post-treatment negative effect would be even larger in absolute value when measured relative to this quarter, making our estimates a conservative measure of the true effect.

During the post-treatment period, the rent control policy has highly heterogeneous effects over time, with increasing impacts throughout the years.[23] Specifically, the impact corresponds to a reduction of around 3% during the first seven quarters after the implementation of rent control, approximately −5% for the following four quarters, and stabilizes around −6% in the later quarters. We find similar results by year (Figure C1 of appendix C). 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. Calculating rent reductions

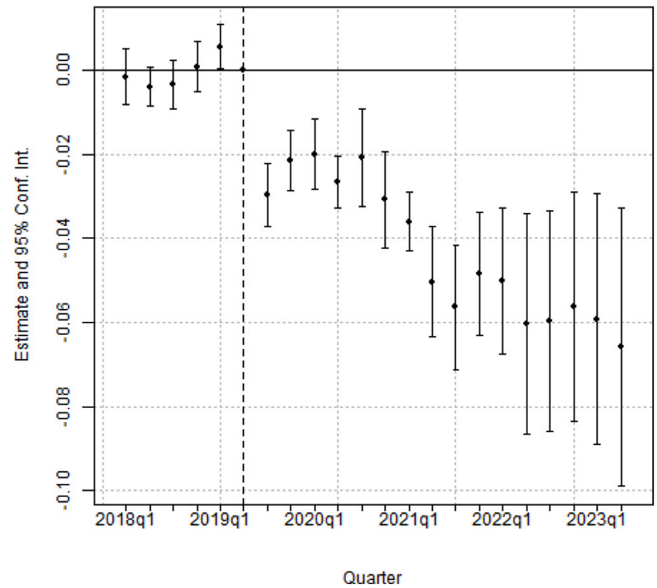

Fig. 7. Event study by quarter.

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). The effect increased until 2022 to reach a decrease of 97€ per month (or 1165€ for the whole year). For each 12-month period, the annual and monthly variations in rents due to rent control are provided in Table E1 in appendix E, on the last two lines. This pattern of rent control being more effective for smaller dwellings is consistent with the descriptive evidence presented in 3.1.1. In 2023q2, 60.6% of units below 18 m^2 exceeded the ceiling, compared to 41.4% for units between 18–24 m^2 and only 32.4% for the largest dwellings. These figures suggest that the regulation binds more strongly for small apartments, where demand pressures are intense and the ceilings are more constraining. Statistics show that small dwellings are much more frequently above the legal ceiling, and therefore the most constrained by the regulation. In other words, the stronger treatment effect observed for studios and very small apartments is directly explained by their higher probability of being affected by the rent cap. This descriptive evidence provides the basis for the following section, where we move beyond size categories and analyze heterogeneity in terms of the intensity of constraint. This approach allows us to measure more precisely how the strength of the policy effect varies with the degree to which individual dwellings are bound by the regulation.

4.1.2. Heterogeneity analysis

The rent control policy sets a maximum rent for each dwelling depending on the area, building period, furnished status, and number of rooms. The upper rent ceiling is the median of the past two years for each subcategory plus 20%. However, the rent distribution is not the same for each category of dwelling. In appendix D, figure D1, we compute the distribution of the rent by number of rooms before the rent control policy was applied (from January 1st 2018 to June 30th 2019). The black vertical bar represents the mean of the distribution and the blue one its median. The upper tail of this distribution for dwellings with 1 room is thicker than the others. On the one hand, those will have a rent further away from the rent ceiling and are less likely to comply with the policy. On the other hand, if they comply, it implies a higher rent decrease in percentage. Thus the effect of this policy could be heterogeneous depending on the characteristics of the dwellings.

We then perform a heterogeneity analysis by estimating model (2) to estimate the differentiated effect of rent control on rents for

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

[23] The increase of confidence interval over the rent control periods cannot 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.

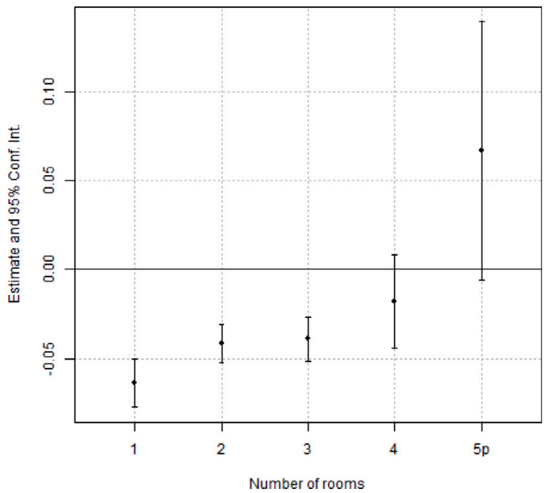

Fig. 8. Heterogeneity by number of rooms.

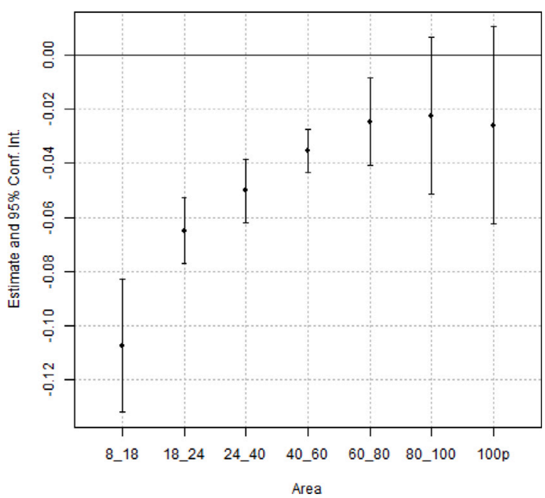

Fig. 9. Heterogeneity by area.

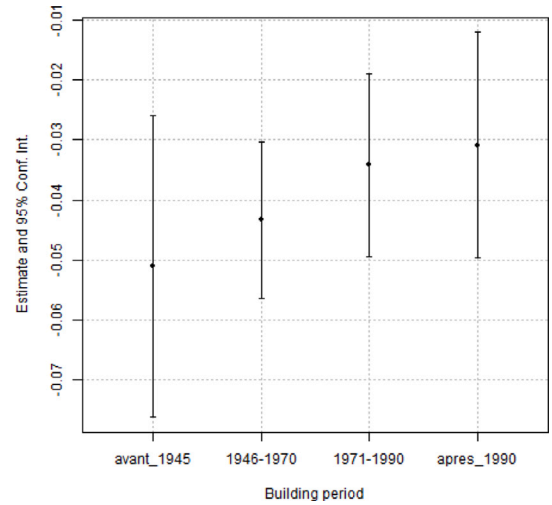

Fig. 10. Heterogeneity by building period.

various housing characteristics (event studies for each modality of each variable used in this heterogeneity analysis can be found in appendix G). We first assess the rent control policy's effects depending on the dwellings' size. 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.

Fig. 8 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 rents of apartments with 2 or 3 rooms show a decrease of around 4%. The effect of the policy for 4-room dwellings is not significant. For dwellings with 5 rooms or more, the confidence interval is much higher, and the overall effect of the policy is also not significant: such dwellings being rarer in our sample may also have highly heterogeneous characteristics. Then we consider the heterogeneity of the treatment using dwelling area (Fig. 9). In the main specification, we account for size effects by including both the number of rooms as a categorical variable and the logarithm of the area. To accurately capture the heterogeneous effects of the area (treated as a categorical variable), we also need a second variable to capture all size effects. Using the number of rooms as a categorical variable would be too correlated with area, that is also a categorical variable in this case. Therefore, we include the number of rooms as a continuous variable, consistent with the earlier specification that used both variables to capture size effects. 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. The stronger effect of rent control on smaller dwellings is consistent with the fact that, prior to its enforcement in July 2019, a much larger share of small units had rents exceeding the legal reference that was to be introduced in July 2019 compared to larger dwellings (as shown in Appendix A1). In other words, before regulation took effect, studios and very small apartments were more frequently overpriced relative to the forthcoming benchmark, leaving greater scope for downward adjustment once rent control was implemented. 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. For each number of rooms or housing surface category, the annual and monthly variations in rents due to rent control are provided in Table E2 and Table E3 in appendix E, on the last two lines.

Dwellings being furnished or not does not seem to affect the efficiency of the rent-control policy (see Fig. 11). 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. Fig. 10 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.

Fig. 12 shows the heterogeneity of the rent control policy for each Paris district. Our results are strongly related to the socio-spatial heterogeneity of Parisian districts. Specifically, districts with the highest concentration of wealth,[24] i.e., the 6th, 7th, 8th, 9th, 16th, and 17th arrondissements are also those in which the impact of the rent control has been weakest, while those with lower concentration of wealth experience more pronounced effects. Consequently, wealthier districts face less pressure from rent controls, whereas lower-income districts

[24] See the Figure plotting the degree of segregation and income in the various districts of Paris in 2019 on https://www.insee.fr/fr/statistiques/6680439#barfigure2_radio3.

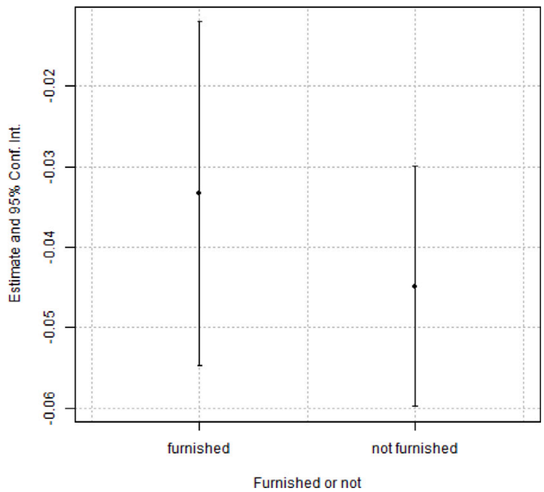

Fig. 11. Heterogeneity: furnished or not.

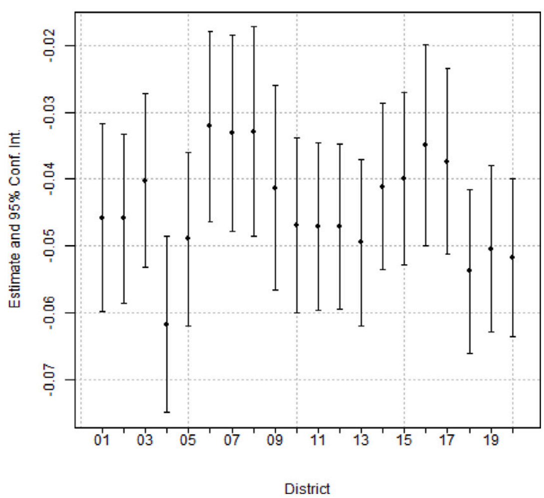

Fig. 12. Heterogeneity by district.

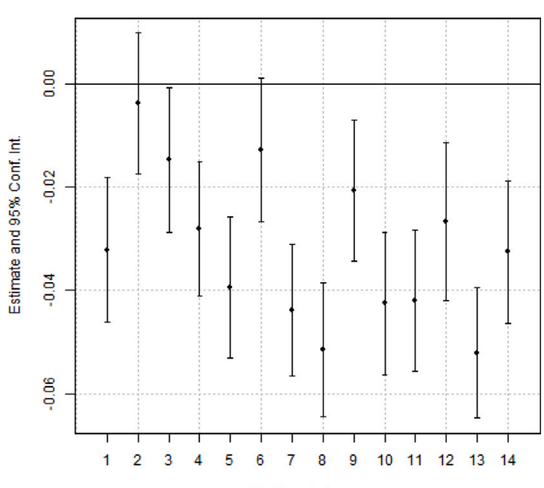

Fig. 13. Heterogeneity by geographical sector.

benefit more from such policies. The 4th district, which is located in the center of Paris, and has a lower number of dwellings compared to other areas, stands out due to its highly pronounced effect.

Table 3
Comparison of the treated effects for different samples.

	All obs Homogeneous coefficients (1)	All obs Heterogeneous coefficients (2)	Restricted sample Homogeneous coefficients (3)	Restricted sample Heterogeneous coefficients (4)
Treatment (%)	−3.7	−4.2	−3.0	−3.5
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 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, year-by-quarter fixed-effects, and area-fixed effects.
Standard-errors are clustered by city.

The results from the analysis with the heterogeneity by geographical sectors (see Fig. 1) are reported in Fig. 13. For most areas, the results appear 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). However, the event studies in appendix G show significant differential pre-trends between Paris and the control group. Unlike heterogeneity analyses based on unit characteristics (e.g. the number of rooms, where we can identify one-bedroom units in the control group to ensure comparability), the geographical sector breakdown or the district breakdown does not allow for such equivalence. Thus, those results are not reliable enough to conclude on the effects of the rent control policy by geographical sectors or by districts.

4.1.3. 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 increased 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).[25] 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:

$$RC_{ict} = \frac{\hat{Y}_{ict}(0) - RT_{ict}}{\hat{Y}_{ict}(0)} \tag{5}$$

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

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

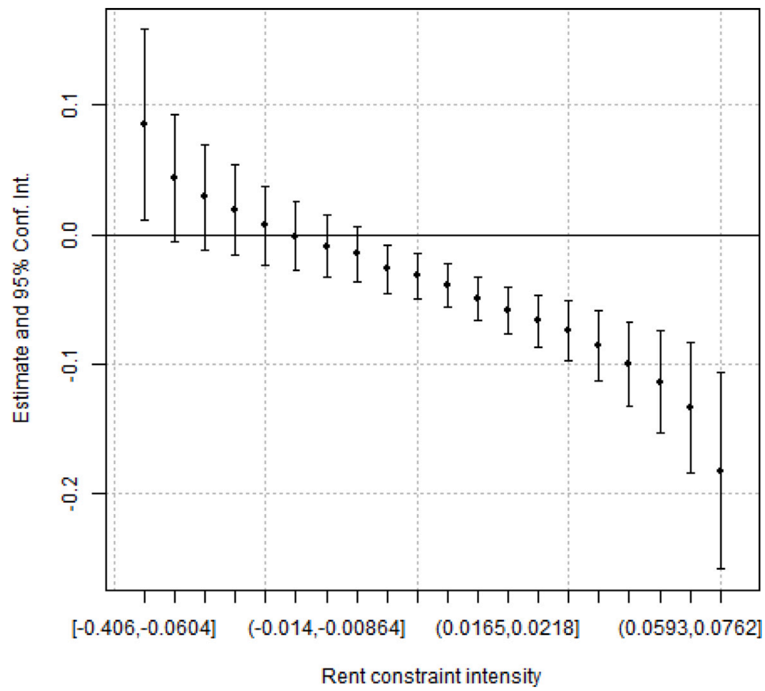

Fig. 14. Heterogeneity analysis by level of rent constraint.

exact address and building period for all observations. In the post-treatment period, we can identify the rent threshold for 124,688 out of 210,544 observations in the treated group. Table 3 displays the estimates of the treatment effect for both 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 results on the sample with only observations with an identified rent control threshold. The estimates of the treatment effect are similar for all columns. For both 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 Fig. 14. In general, the rent control policy has been more efficient at reducing rents that would have been over the threshold. The higher the rent constraint intensity, 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 landlords to adjust their rents based on this new information. However, we cannot disentangle between these two mechanisms because it would require information about the number of people interested in each dwelling to proxy for the actual demand.

Table 4
Upper bound of the effect of the rent-control policy.

	Homogeneous coefficients (1)	Heterogeneous coefficients (2)
Treatment (%)	−8.7	−8.2
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.

4.1.4. 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 neighborhood. 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.

Estimating the effects of the policy if everyone respected the rent threshold is equivalent to estimating the upper 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 restricted sample with only observations for which we can identify the applicable rent threshold (the sample used to produce the results in columns (3) and (4) of Table 3). 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 Eq. (1)) and column (2) containing the results obtained with heterogeneous coefficients (see Eq. (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.[26] The stronger communication organized by the city of Paris should allow the policy to have a higher effect on rent decrease, for which we now know the upper bound of the effect.

4.1.5. Causal impact of rent control in Paris and other regulated cities using a staggered difference-in-difference approach

As other cities have decided to implement rent control following Paris, we also carry out a more comprehensive analysis by expanding our treatment group. We incorporated only a subset of these regulated municipalities rather than all regulated municipalities for two reasons. First, we excluded all municipalities from the two inter-municipal groups located in the Ile-de-France region, which could have threatened the Stable Unit Treatment Value Assumption (SUTVA), which requires that the treatment in one unit does not affect outcomes in other units. Specifically, some of the ten cities of the inter-municipal group "Plaine Commune" adjoin the northern arrondissements of Paris (17th, 18th, and 19th), and some of the nine cities of the inter-municipal group "Est ensemble" adjoin the northern-eastern arrondissements of Paris (19th and 20th). The proximity and economic integration between Paris and neighboring cities may have led to spillover effects (for example, people may prefer to locate in Paris instead of the close suburb if the rent is closer because of the rent control policy. This would lead to a decrease in rental price in surrounding cities, violating the SUTVA assumption). Second, we excluded the two municipalities below the 100,000 inhabitants threshold (i.e., the two neighboring cities of Lille, Hellemmes with 17 633 inhabitants, and Lomme with 28 012 inhabitants) to ensure better comparability with the control group, whose selection is also subject to this population threshold. In all, we thus included five cities in addition to Paris, that are Lille (regulated from March 2020), Lyon and Villeurbanne (November 2021), and Bordeaux and Montpellier (July 2022).

Because the treatment starts at different dates for each location, usual difference-in-differences estimates are biased and do not identify the usual treatment effect (Goodman-Bacon, 2021). We thus use the staggered difference-in-difference approach of Gardner (2021) and estimate the model with heterogeneous coefficients.

The estimation results from the staggered difference-in-difference where the treatment rolled out at different times across six cities (including Paris) are reported in columns (2) of Table 5. We have also included, as a reminder, the column (1) corresponding to previous estimate reported in Table 2 for Paris only. The estimated effect of the rent control policy in the staggered model suggests a rent reduction of 4.3%. This effect is highly significant and closely aligns with the estimated effect for Paris only (4.2%). We also estimate the staggered DiD model excluding Paris from the set of treated cities. The aim is to test whether the observed policy effects are driven primarily by the Parisian rental market or whether they also hold in other cities that implemented rent control. The result confirms that the estimated treatment effect remains statistically significant, although slightly reduced in magnitude when Paris is excluded. The estimated effect decreases from −4.3% to −2.4% when Paris is excluded, indicating that Paris accounts for a substantial share of the overall impact. This difference can be driven by two main reasons. First, because the rental market is larger in Paris, we have more observations, which drives the effect toward the value estimated in Paris when including all cities. Second, the policy was implemented earlier in Paris, and because its effect increases through time, the ATT might be equivalent for similar exposure time. We further investigate this effect using an event study in the next paragraph. Overall, the persistence of a significant effect in the remaining cities confirms that the rent control policy produced measurable impacts beyond Paris, albeit of lower intensity, reinforcing the external validity of our findings. This difference in magnitude may also reflect variations in local enforcement, market structure, or compliance levels across cities.

Table 5
Staggered DiD results with and without Paris.

	Paris (1)	All cities (2)	All cities but Paris (3)
Treatment (%)	−4.2	−4.3	−2.4
Treatment	−0.043***	−0.044***	−0.024**
	(0.006)	(0.007)	(0.009)
Num.Obs.	559 300	757 273	473 135
R2	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.

In Fig. 15, we compare the event study by quarter for the estimate where Paris is the only treated city (right panel) with the staggered estimate that includes five additional rent-controlled cities (left panel) and with the other cities without Paris (center panel).[27] These results are also reported in a table in appendix K table K1. Including these other rent-controlled cities slightly widens the confidence intervals, but it does not alter our results. The rent decrease intensifies over time, and there is no evidence of a pre-trend in the five periods preceding the implementation of the rent control policy. In the earlier pre-treatment periods, 3 out of 12 periods show a significant pre-trend, and the first three have wider confidence intervals. This could be explained by the smaller number of observations for old pre-treatment periods, making the results more sensitive to temporary, city-specific shocks. When looking at the pre-treatment period without Paris (center panel), the three periods before the implementation of the rent control policy has significant pre-trends, However, this effect remains low compared to the effect of the policy in the next periods. In the post-treatment periods, the effect of the rent control policy increases through time. The point estimate is higher than Paris for the two last periods. This indicates that the lower estimate of the ATT in Table 5 column (3) is mostly driven by a shorter observation period rather than a lower effect of the policy in other rent controlled cities.

To summarize, while the estimated ATT is higher for Paris than for the other cities, this difference appears to stem from the longer duration of policy implementation in Paris. Event study analyses for both Paris and the remaining cities (excluding Paris) show that the effect of the policy increases over time. Given that the policy has been in place longer in Paris, a higher ATT is to be expected. Interestingly, the slightly higher overall ATT when pooling all six cities suggests that the policy may perform marginally better in other cities; however, this difference is not statistically significant, given how close the estimates are.

[26] Until the end of 2022, the authority to which tenants could report illegal rents, and that could impose administrative penalties was the prefect of the Paris "département". However, starting in January 2023, the City of Paris assumed this role, which was authorized under request by the 3DS Act of February 21, 2022, which amended Article 140 VII of the ELAN Act. Despite the delegation of enforcement powers to the city of Paris, the penalties themselves have not changed in 2023. An administrative fine of up to €5000 for individuals and €15,000 for legal entities may still be imposed.

[27] We also show the side-by-side comparison of Paris vs. all other cities in appendix J figure J1.

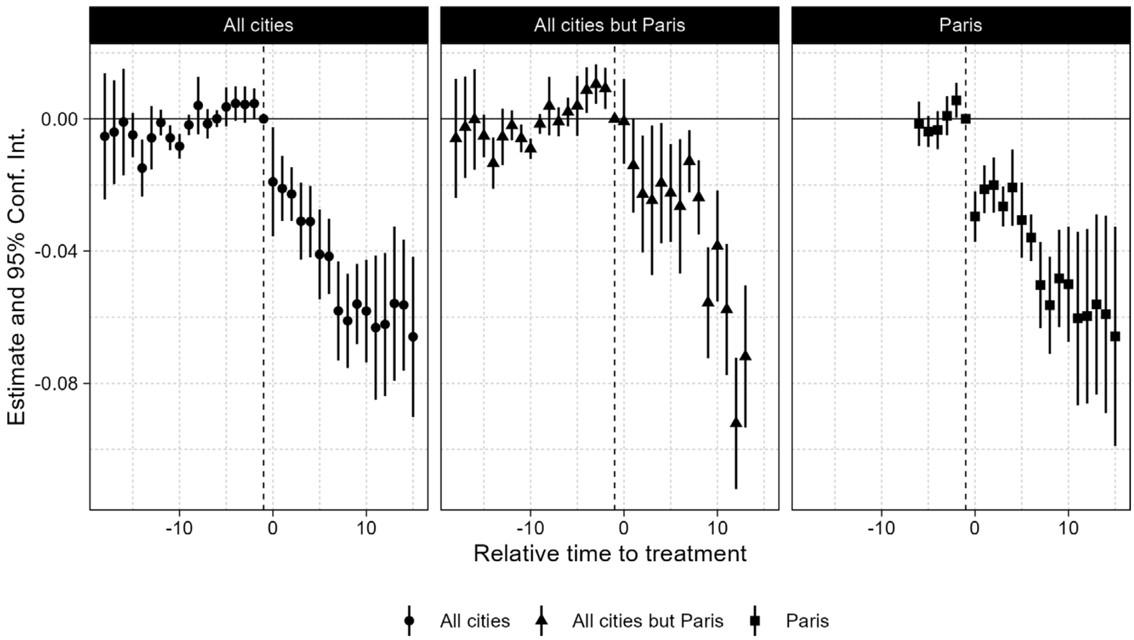

Fig. 15. Event study comparison.

Table 6
Supply effect at the real estate agency level.

	All periods with covariates	No COVID-19 period with covariates
Treatment (%)	11.8	−3.5
Treatment	0.111**	−0.036
	(0.027)	(0.020)
Num.Obs.	18 810	9405
R2	0.750	0.765
R2 Within	0.027	0.029

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001.
Each regression includes control variables, year-by-quarter fixed-effects and real estate agency fixed-effects. Standard errors are clustered by city.

4.2. Causal impact of rent control on the supply dynamics

The estimation results of the model (4) over the full period are reported in column (1) of Table 6, while column (2) restricts the estimation to a period without the COVID effects that excludes the second quarter of 2020 through the fourth quarter of 2022, in order to account for the spike in ads during the pandemic period discussed earlier. We also report the full table with all the coefficients for the control variables in Appendix L table L1.

When using the full sample, the estimated treatment effect is positive (11.8%) and statistically significant at the 1% level. However, this effect is likely driven by the sharp and specific increase in the volume of ads observed in Paris during the COVID-19 period (see Fig. 5), and thus may not reflect a genuine causal effect of the rent control policy.

When the COVID-19 period is excluded, the estimated treatment effect becomes negative (−3.5%), although it is not statistically significant. Moreover, the within-R^2 remains close to zero in both specifications, highlighting that most of the variation is absorbed by the fixed effects. These results suggest that the rent control policy had at best a very limited impact on the flow of rental ads, and any apparent effect during the pandemic period is likely confounded by the exceptional circumstances of COVID-19.

Event studies are reported in Fig. 16 for all periods and in Fig. 17 when we remove the COVID-19 period. The table with these results can be found in Appendix M Table M1. In the three quarters following the introduction of rent control, treatment effects are not statistically significant, indicating no impact on ad volumes during this initial period. The subsequent eight quarters show a positive and significant effect of rent control on the number of ads. These quarters fall between Q2 2020 and Q4 2022, overlapping with the COVID-19 period. This likely reflects a Paris-specific phenomenon, not observed in the control group, consistent with previous findings showing a shift of properties from short-term to long-term rental markets during the pandemic. In the three quarters that follow, treatment effects are no longer statistically significant, suggesting the effect of rent control on listings had dissipated. In the last two quarters (2023Q1 and 2023Q2), we observe a negative effect. However, this effect is not significant at the 5% level for both of these estimates.

To sum up, the effect of the rent control policy on the number of ads is not significant for all periods when the COVID-19 period is excluded. These findings align with those of Jofre-Monseny et al. (2023), who found no effect of the rent control policy in Catalonia on the supply of dwellings. Nonetheless, considering that the decrease in the supply of dwellings is one of the major drawbacks of such policies (Diamond et al., 2019), and that we excluded a significant number of periods from the analysis because of the COVID, this effect should be further investigated as time passes and more recent data becomes available.

5. Conclusion

In this paper, we evaluate the impact of the rent-control policy implemented in Paris since July 2019 on the Parisian rental market. Our results show that the policy led to a reduction in rents of 3.7% to 4.2% on average. The effect is highly heterogeneous across dwelling sizes, with a stronger impact on small apartments (less than 18 sqm), which typically have higher prices per sqm than bigger ones and are more likely to be occupied by students or low-income tenants. Since the regulation applies differentiated thresholds based on the number of rooms, smaller dwellings are more constrained, resulting in stronger rent reductions. Using a counterfactual rent approach, we further document that rent reductions are concentrated among the most constrained dwellings (above the 9th quantile of rent constraints), while the least constrained (bottom 5%) experienced rent increases. We also find that, if all dwellings fully complied with the maximum rent threshold, the estimated rent reduction would range from 8.2% to 8.7%. These findings suggest that improved enforcement of the rent

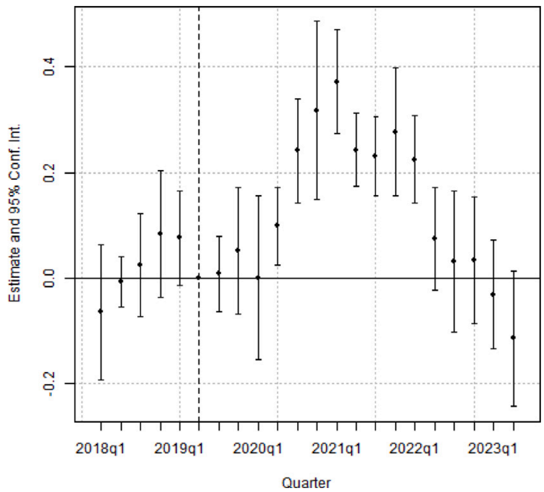

Fig. 16. Event study (agency level) for all periods.

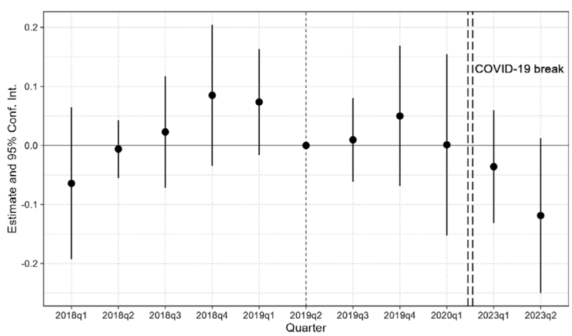

Fig. 17. Event study (agency level) without COVID-19 period.

cap could greatly enhance the effectiveness of the rent control policy on rent decreases. The staggered analysis reveals that the rent control policy also produced measurable – albeit smaller – impacts in other cities, likely due to their more recent implementation. As we have shown in the case of Paris, the effects may strengthen over time. The presence of significant effects outside the capital confirms the relevance of rent control as a tool to moderate rent increases.

From a policy point of view, the current rent control has the advantage of slowing the growth of rents – since rent thresholds are indexed on the median of the past two years – without fully freezing rents that would be too unfavorable for landlords. However, rent control policies also raise concerns regarding their potential negative effects on rental housing supply and quality (see Kholodilin, 2024 for a review). In this regard, we investigate whether the Parisian rent control policy affected the flow of available rental ads. While a naïve estimation over the full period suggests a positive effect on ad volumes, this result is clearly driven by the COVID-19 period, during which Paris experienced a spike in listings likely caused by temporary shifts from the short-term to the long-term rental market. When excluding the COVID-19 period from the analysis, the estimated treatment effect becomes negative but small (−3.3%) and not significant. Overall, the effect of the policy on rental supply appears very limited. Future research should explore potential long-term effects on the housing stock both in terms of quantity (the number of dwellings available) and quality (maintenance and renovation by landlords). Indeed, some landlords may choose to sell their apartment if they consider the rent too low to be profitable or stop renovating it. These risks could be exacerbated 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).

CRediT authorship contribution statement

Yoann Morin: Writing – review & editing, Writing – original draft, Validation, Software, Methodology, Formal analysis. **Martin Regnaud:** Writing – review & editing, Writing – original draft, Visualization, Software, Methodology, Data curation. **Marie-Laure Breuillé:** Writing – review & editing, Writing – original draft, Supervision, Project administration, Methodology, Funding acquisition, Conceptualization. **Julie Le Gallo:** Writing – review & editing, Writing – original draft, Supervision, Project administration, Methodology, Funding acquisition, Conceptualization.

Declaration of competing interest

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: Marie Breuille, Julie Le Gallo, Yoann Morin, Martin Regnaud reports financial support was provided by Atelier Parisien d'Urbanisme (APUR). If there are other authors, they declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jhe.2025.102101.

Data availability

The authors do not have permission to share data.

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