Capitalisation of Demand-side Policies with Heterogeneous Purchasers and Market Segmentation

Rémi Lei[*,†]
remi.leiuniv.acad@gmail.com

November 24, 2023
Work in progress

Abstract

I consider the price capitalisation of demand-side policies in the context of segmented housing market with heterogeneous purchasers according to information level on the housing market. Empirically, I exploit the French policy which subsidise buy-to-let investments through tax cuts. To account for endogenous treatment assignment following a place-based policy design, I apply a difference-in-difference approach for successive policy removal and introduction. The segmentation definition I consider follows the one assumed by policymakers, i.e. according to tenure status and building type. Exploiting micro-data available from the tax register, I distinguish the price effect experienced by local purchasers (who are assumed to be perfectly informed and thus subject only to the demand-shifting channel) from that experienced by Out-Of-Town purchasers (with lower level of knowledge of the housing market). The findings indicate that i) subsidy is at least partly capitalised on the targeted segments ii) there is no effect on segment defined by owner-occupation iii) information asymmetries of OOT purchasers play a minor role to explain price increase (only 1 point).

JEL classification: R31 ; R38 ; C21

Keywords: Difference-in-differences ; rental investment policies ; housing market segments ; information asymmetries.

[*]ESPACE, Université d'Avignon, F-84000 Avignon
[†]CESAER, L'Institut Agro Dijon, INRAE, Université de Bourgogne Franche-Comté, F-21000 Dijon.

1 Introduction

Housing represents the primary spending category for households and has increased in significance over the past few decades. For example, while housing expenses constituted 20.5% of French households' budgets in 1985, it now accounts for 28.5% [Sources: INSEE[1]]. This assertion concerns most developed countries. Even if households smooth their housing consumption to mitigate the increase in housing values (Ben-Shahar, Gabriel, and Golan, 2019), affordability issues affect low-income households more significantly (Quigley and Raphael, 2004). Consequently, it lower savings likely to raise inequality on the long term (Dustmann, Fitzenberger, and Zimmermann, 2022). In addition, the rise in housing costs within households' budgets lead to a decrease in non-durable consumption (Newman and Holupka, 2016), contributing to a reduction of the economic activity especially on metropolitan areas (Gabriel and Painter, 2020).

Whereas the issue of affordability has been addressed by the public response of housing vouchers in the short term with limited effect (Fack, 2006; Ellen, 2020), a long term response has been introduced which seeks to increase the supply of housing. This expectation appears to be empirically validated for New York (Li, 2022). This response is implemented in two ways: development of social housing and stimulating demand for housing investments. First, policymakers support affordable housing by implementing social housing programs. These programmes provide subsidies to housing developers and social landlords in exchange for capped rents. These policies have significantly impacted the housing supply in France (Gobillon and Vignolles, 2016), with significant effect on recipients' consumption (Le Blanc and Laferrère, 2001) and mitigation of wealth inequality (Goffette-Nagot and Sidibé, 2016). Second, policymakers encourage buy-to-rent investments through income tax cuts, stimulating demand for rental properties expected to result in a supply response (Baum-Snow and Marion, 2009). France is no exception, as the government promotes buy-to-rent among households through tax incentives. Recipients receive a reduction in income tax (ranging from 12% to 22% of the purchase price

[1]French National Institute of Statistics and Economics Studies.

spread over 9 years) in return for committing to renting the housing with capped rents. These policies are place based to concentrate public expenditure in the area with the tenser housing markets.

Considering the inelastic nature of supply (at least in the short term), reinforced by regulations on land consumption (Turner, Haughwout, and Van der Klauw, 2014; Hilber and Vermeulen, 2016; Molloy, Nathanson, and Paciorek, 2022), geographical constraints (Saiz, 2010), or construction costs (Glaeser and Gyourko, 2002), public support channelled through demand-side policies might be capitalised into prices. These effects are however mixed for the French case, as Bono and Trannoy (2019) find a capitalisation for land value, whereas Chapelle, Vignolles, and Wolf (2018) do not find any positive effect on housing value. In an international perspective, the LIHTC. These results are paradoxical in two ways. First, we expect the increase of land value to transit into housing value. Second, the housing supply in the US is less constrained than in European countries (Caldera and Johansson, 2013). However, as these policies are restricted to specific segments (newly built units for rental purposes), questions arise as to whether the price effect is heterogeneous within local housing market segments (Chareyron, Ly, and Trouvé-Sargison, 2021).

Beyond direct implications on housing price, demand-side policies that favour rental investments have additional effects such as crowd-out effects from non-eligible to eligible areas (Eriksen and Rosenthal, 2010) with limited effect on the overall housing stock (Malpezzi and Vandell, 2002). More recently, Levy (2021) provides insights that rental investment enhance Out-of-Town (OOT in the remaining of the paper) investments. These investors are expected to have a lower knowledge of housing markets they invest in as they face higher search costs (Turnbull and Sirmans, 1993; Ihlanfeldt and Mayock, 2012). It results that they are more likely to behave as misinformed speculators (Chinco and Mayer, 2016), leading to lower wealth return (Ling, Naranjo, and Scheick, 2021). Beyond their behaviour resulting with heterogeneous level of information, enhancing OOT investment is likely to raise housing price through additional demand, leading to negative impact on city welfare (Favilukis and Van Nieuwerburgh, 2021).[2] Hence, OOT investors can affect

[2]In fact, the impact on city welfare mainly depends on whether OOT purchasers place their units on

the average price in two ways: additional demand and overpricing resulting from higher information asymmetries. Accounting for both channels is crucial to understand how rental investment scheme affects local housing price.

I improve the results on price capitalisation of demand-side policies in two ways. First, I relax the assumption of a locally homogeneous housing market and estimate spillover effects on non-subsidised housing market segments. Housing market segmentation definition I adopt comply with those assumed by policymakers, i.e. according to tenure status and building type. Second, I disentangle the price effects of these policies according to whether they arise from an overall shift in demand or change in purchaser structure. I use micro-data to identify the local purchasers from OOT purchasers. The main distinction between both populations is assumed to be the level of knowledge about the housing market they invest in. Local purchasers are assumed to be perfectly informed while OOT purchasers face higher information asymmetries, regardless the tenure status (Li and Chau, 2023). Empirically, it is not clearcut how to delineate housing markets which is a necessary condition to define local and OOT purchasers. However, we assume that housing and labour markets are closely related (e.g., see Charles, Hurst, and Notowidigdo, 2019) and use commuting zones provided by the French Institute of Statistics (INSEE) to delineate housing markets. Note that our main conclusions are robust to our definition using euclidean distance as alternative criteria.

To handle the endogenous feature of the treatment assignment following the place based policy design, I rely on natural experiments. Using repeated cross-sections of housing transaction in eligible and non-eligible municipalities, I adopt a difference-in-difference framework. I make the best use of two control groups (never treated and always treated) and adopt a two-way fixed effects estimator. Our identification strategy is based on the successive removals and introductions of the rental investment scheme from July 2013 to January 2018. To allow for heterogeneity in the magnitude of the price effect within the population of purchasers, I introduce an interaction between the treatment dummy and the category of purchaser. It enables the distinction between the price effect observed by

the rental market (no effect on city welfare) or keep their units for own consumption (negative impact).

local purchasers (assumed to be only subject to demand shift) and the price effect observed by OOT purchasers (subject to information asymmetries). Therefore, if the impact of prices on the rental investment scheme is homogeneous within purchaser categories, I can conclude that the information asymmetries channel is marginal with respect to the demand shift in an inelastic supply context.

Findings indicate that assuming a locally homogeneous market based on tenure and housing type leads to imprecise results. Conclusions with local homogeneous housing market according to tenure status and building type are consistent with Chapelle, Vignolles, and Wolf (2018) ones. Yet, considering that transactions in the subsidised segments represents 3.6% of the overall market, it leads to imprecise results. The price effect on the subsidised segment is significant both statistically and economically, as the policy introduction raises the price per unit of surface by at least 9.9% (-13.3% for the repeal). More importantly, there is evidence of a smaller, yet significant, price impact on existing rental properties following policy implementation (+3.3%), assumed to be a spillover effect resulting from a shift in demand from the subsidised to the non-subsidised segment. Conversely, either the introduction or the removal of the policy has no effect in the owner-occupier segment. We interpret these results as a local segmentation according to tenure status. Beyond the measurement of the price effect, we estimate that most of the effect is channelled through the demand shift. Consistent results are obtained on information asymmetries for OOT purchasers (ranging from 6.0% to 7.1%, depending on the housing segments) but local purchasers experience a significant price increase after the introduction of the policy as well. Therefore, the capitalisation effect associated with the rental investment program is mainly driven by the demand effect channel rather than the information asymmetry one.

I contribute to three literature strands in housing economics. Firstly, I present more detailed results on the price capitalisation resulting from the rental investment scheme. I lay out that considering local housing markets as perfectly integrated across tenure and building type lowers the price effect. Consistent with Chareyron, Ly, and Trouvé-Sargison (2021), I challenge the notion that housing markets are homogenous. However, I broaden the definition of segmentation to include tenure status as well as building type and consider purchaser heterogeneity with respect to information asymmetries. Secondly, I make a contribution to the literature on housing market segmentation. While many papers addressing segmentation issues adopt a spatial approach (Goodman and Thibodeau, 1998; Bhattacharjee et al., 2016; Bourassa, Dröes, and Hoesli, 2021; Coën, Pourcelot, and Malle, 2022), our findings suggest that a lack of spillover effects is likely to correspond to a lack of substitutability between the owner-occupied and rental segments. Therefore, despite the fact that the spatial definition captures different sources of heterogeneity (Brueckner and Rosenthal, 2009; Duranton and Puga, 2015; Diamond and Gaubert, 2022) including tenure status, it opens new research questions on whether segmentation arises from spatial proximity or housing characteristics. Thirdly, I make contributions to the literature concerning location of purchasers and information asymmetries (Chinco and Mayer, 2016; Favilukis and Van Nieuwerburgh, 2021; Ha, Hilber, and Schöni, 2021; Ling, Naranjo, and Scheick, 2021). I obtain consistent results with previous research (OOT purchasers overprice housing) and illustrate that the process of overpricing is heterogeneous within segments of the housing market. These results demonstrate that overpayments are notably more prominent for the purpose of owner-occupation (ranging from 7.0% to 12.0%) as opposed to rental investments (ranging from 2.1% to 7.1%). I assume it results from the fact that rental investments are more closely related to profitability which incite investors to gather more information to appraise more precisely their profitability returns.

Yet, following efforts must be achieved to extend the current results. Among all, it appears necessary to add a third channel to explain housing price. Whereas we focus on the overall demand shift and information asymmetries, it would be informative to distinguish the demand shift caused by OOT from the demand shift caused by local purchasers. This represents an uncover dimension of rental investment scheme assessment.

The paper is structured as follows. Firstly, the conceptual framework that inspires our identification strategy is presented (Section 2). This is followed by a description of our data sources and how they have been adapted to provide relevant information to relax the homogeneous assumption (Section 3). Our identification strategy is then presented, together with a summary of the housing policies considered to induce demand shocks (Section 4). The results of our estimation are then presented (Section 5) and the conclusion is set out (Section 6).

2 Conceptual Framework

I assume a finite housing market at the equilibrium with two types of units, for instance rented and owner-occupied ones. For sellers, I assume the existence of homogeneous reservation price within the two housing segments (defined according to the type of units) denoted $\widehat{p}_R$ and $\widehat{p}_O$.

For the purchaser side, I consider two types of purchasers that differ according to their knowledge of the housing market. First, I consider local purchasers which are perfectly informed about the housing market. Hence, they observe the reservation price of sellers. Second, I consider Out-of-Town purchasers[3] (OOT) that have incomplete information about the housing market, and consequently, misprice housing units according to sellers reservation price. I assume that their price beliefs follow a normal distribution with seller reservation price as mean and σ, the dispersion. The distribution of purchaser within the housing segments is such as $\mu_\ell + \mu_{out} = 1$.

Consequently, I can recover the effective average price within housing segments such as

$$\forall s \in \{R, O\},\ p_s^0 = \mu_{\ell s}^0 \widehat{p}_s^0 + \int_{\widehat{p}_s^0}^{\infty} \mu_{OOT,s}^0(p) \cdot p \tag{1}$$

The average observed price p_s^0 is the combination of two terms, the price of local purchaser that pay the seller reservation price (first term of the right side), and OOT purchaser that have pricing process above the sellers' reservation price (second term of the right side). Remark that the average housing price in presence of misinformed purchasers is greater or equal to the reservation price of sellers.

I now introduce financial incentive specific to demand for one type of housing units. I consider two channels in which it can affects the average price: change in the purchaser

[3] This definition follows the definition provided by Favilukis and Van Nieuwerburgh (2021).

structure and demand shift.

Purchaser Structure I first consider the case in which the financial incentive favours OOT purchasers as empirically demonstrated by Levy (2021). In such case, we consider that the aggregate number of OOT purchaser is greater after the introduction of financial incentive. Consequently, the population which misprice housing is higher consecutive to the introduction of financial incentive. Then, even if the reservation price for seller remains unchanged (considering an elastic supply), the aggregate observe housing price increases consecutive to the policy introduction as

$$\int_{\widehat{p}_s^0}^{\infty} \mu_{OOT,s}^1(p) \cdot p > \int_{\widehat{p}_s^0}^{\infty} \mu_{OOT,s}^0(p) \cdot p \tag{2}$$

Consequently, the policy can affect the average price even if the supply is elastic. Indeed, the composition of purcahsers, and more importantly the share of misinformed purchasers are central.

Demand Shifts The second case we consider is the demand shift. Yet, the price response depends on the substitutability between housing units within the local housing market (the segmentation definition). Firstly, let consider that there is no substitution between housing segments. In such case, there is no price adjustment in the non-subsidised markets, as the available supply and aggregate demand remain similar. Consequently, the price adjustment would occur on the subsidised segments only, being reflected in seller reservation price.

Secondly, let consider that housing are not perfectly segmented. I then consider two cases. Firstly, the supply is substitutable between housing segments. Then, consecutive to the introduction of financial incentive, the demand shock is at least partly absorbed by increasing the number of exchanged units within the subsidised segment. However, as the supply is assumed to be inelastic, this decreases the available supply in the non-subsidised segment, yielding to price increase.

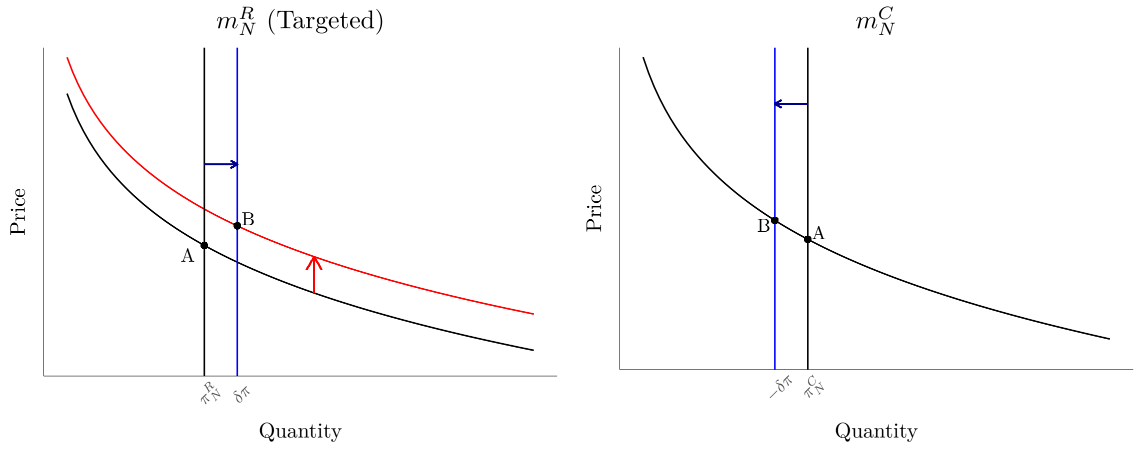

Figure 1: Expected Price Adjustment with Imperfect Segmentation (Supply Mechanism)

Notes: We report the price response in each segment (subsidised is the left one) considering a demand shift. The demand shift only concerns one segment. Yet, the demand shift can affect price in the non-subsidised if available quantities in each segment can vary (substitutability assumption). We consider the case where supply in the overall housing market is inelastic.

Secondly, the introduction of financial incentive can affect demand within segments. Indeed, purchasers can shift from non-subsidised to subsidised segments. If housing supply remains unaffected, this leads to a price decrease in the non-subsidised segments. However, the opposite is possible as well. Purchasers in the subsidised segments can be outpriced from the subsidised segment (due to more intense bargaining process) and shift in the non-subsidised segments. Hence, in this particular case of outpricing process, it leads to price increase in the non-subsidised segment.

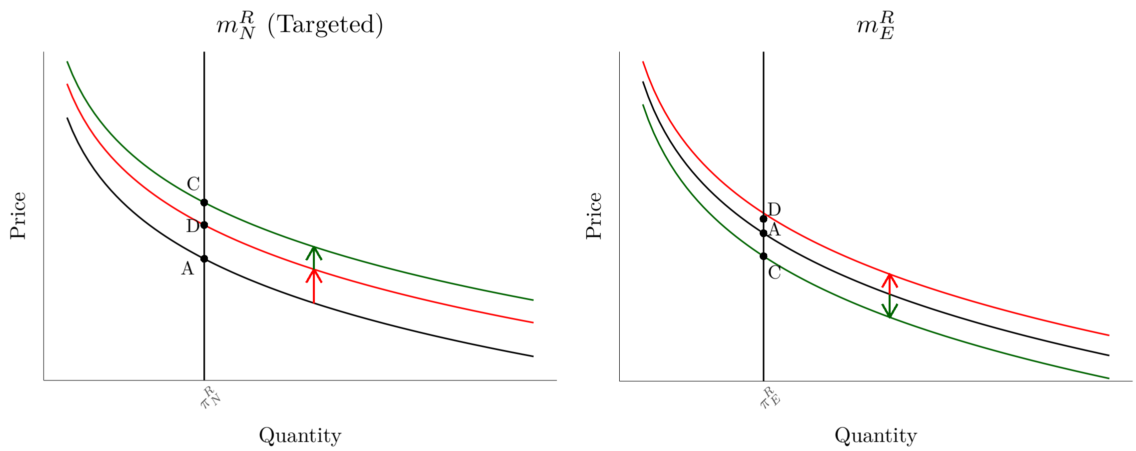

Figure 2: Expected Price Adjustment with Imperfect Segmentation (Demand Mechanism)

Notes: We report the price response in each segment (subsidised is the left one) considering a demand shift. The demand shift only concerns one segment. Yet, the demand shift can affect price in the non-subsidised if the introduction of financial incentive introduce shift from non-subsidised to subsidised segments (green line) or the reverse (red line). We consider the case where supply in the overall housing market is inelastic.

In a corollary approach, the adjustment price – or the lack of adjustment – reveals the substitutability degree between the two housing segments. A lack of price adjustment of the non-subsidised market would correspond to a perfectly segmented market, while an homogeneous price effect for housing segments would correspond to a perfectly integrated market.

In this simple framework, I demonstrate that the price effect resulting from the introduction of demand-side policies is likely to depend jointly on the modification of purchaser structure and the housing market segmentation. We empirically assess the magnitude of both channels, based on the French policy.

3 Data

The data we use in this paper originates from two fiscal sources. We detail how we combine these two data sources to extend results to individual housing segments and disentangle price effects resulting from information asymmetries and demand shift.

3.1 Transaction Data

Transactions data about premises, including housing, are available in France. Originating from 2010 to 2022, housing transactions are registered and includes detailed information about the transaction and the unit being purchased. This data source derived from fiscal source, which ensure variable to be consistent and precise (as it defines for instance property tax). From the housing characteristics side, it includes common information such as surface, whether it is a detached house or an apartment and building period, and more detailed information such as the presence of particular facilities (cellar, parking lot, swimming pool, etc.) or the dependencies surface that relate to the housing.

The advantage of this data source is twofold. Firstly, housing transactions are registered in a comprehensive manner.[4] Hence, these data do not suffer from sampling issues. Secondly,

[4]The only exception is the Alsace–Moselle *départements* for administrative and historical reasons.

housing transactions are georeferenced using the centroid of the parcel. Considering the average parcel size, the precision must be viewed as the best we can obtain with marginal error. Yet, it enables to precisely select observations based on their location. Nonetheless, the main default of these data lies on the lack of information about purchaser and seller.

3.2 Tax Property Files

The second source of data we leverage also originates from fiscal source. While we have detailed information about housing transaction, this second data source provides information about housing ownership every 01st January from 2009 to 2022.[5] Tax property files provide information about both housing and homeowners. Following previous works of Lei (2023), we can observe the residence place of homeowners over the 2011–2022 period. In addition, from the housing side, tenure status using a simple definition according to owner-occupied, rented or vacant is provided.

From the unit of observation, it is important to remark that observation unit is restricted to homeowners. Unlike other exhaustive sources, we do not observe the entire population, only current homeowners every 01st January.

3.3 Data Improvement

Our objective is to improve the transaction dataset with tax property files to i) observe the tenure status *after* the purchase to assign transaction to housing market segments ii) observe the previous residence place of purchaser. The data improvement is made possible as we observe a housing identifier common to both database.

Housing Market Segmentation Definition We leverage the tenure status yearly available in tax property files to define the purchase purpose based on tenure status. Our main assumption is that the purchase purpose can be inferred from the observation of tenure status *after* the purchase. Consequently, we merge tax property files to housing

[5] The only exception is the 2010 year which is missing.

transaction dataset which enable to observe the tenure status years after the purchase.

Hence, we use tenure status in the two years period after the purchase. For instance, if a housing transactions occurred in 2012, we use the tenure status of the 01st January of both 2013 and 2014. By doing so, it enables to precisely identify tenure status, especially for vacant housing after the purchase (i.e. in 2013 in our example) with the observation of the tenure status of the following year (i.e. in 2014 in our example). Nonetheless, if the tenure status is vacant in both years, we cannot precisely identify the purchase purpose and remove the transaction from our sample.[6]

Finally, we identify the building type as it defines whether the transaction belongs to segment subsidised by the rental investment scheme. We adopt the French definition of newly built units, i.e. never been occupied. In addition, rental investment scheme imposes the sell to take place at most two years after the end of the construction. We construct the existing units category by opposition.

Purchasers' Location The second data improvement we make is about the purchasers' location. While the merging process between transaction dataset and tax property files enable to observe the exhaustive list of purchaser, we make the best of Lei (2023) to replenish the list of residence place (using the municipality level) every 01st January. The residence place of individuals is the key criterion to infer whether purchasers are OOT or local ones.

An exception occurs for purchasers that enter homeownership consecutive to the purchase. Indeed, the first residence place we observe for new homeowners is after the purchase as they were not previously homeowners. However, some delays appears in the fiscal data about residence place consecutive to housing change, and we consider that the first residence place is likely to be prior to the purchase. In addition, considering that first housing purchase is related to stability, including spatial one, we can assume that most housing purchase made by homeowners are local ones.

[6]While we expect vacant housing to belong to the rental market, we do not reject the possibility for this housing to be a second home. Hence, the purchase purpose associated to vacant units appears as ambiguous.

From the observation of the previous municipalities, we infer the associated commuting zone in 2010. We also obtain the commuting zone for the housing to determine whether the purchaser is a local (i.e. similar commuting zone for residence place and housing) or a out-of-town (OOT) one (the opposite). For missing commuting zones (it represents 5.3% of the sample), I consider by default these purchasers as OOT ones. These observations mainly correspond to purchasers that live abroad (and by nature OOT purchasers). Note that our conclusions are robust to alternative specifications of the local – OOT definition, including urban areas (rather than commuting zones) and distance approaches.

Descriptive Statistics We combine the tenure segmentation definition with the purchasers location. Yet, we consider the case for which purchasers' category are different within the same transaction (e.g. one purchaser is a local one, while the second is an OOT). Rather than imposing an assumption to aggregate information at the transaction level, I keep unnested transaction – purchaser dataset. To account for heterogeneity in the number of purchaser between transaction, I weight observation with the inverse of number of purchasers.

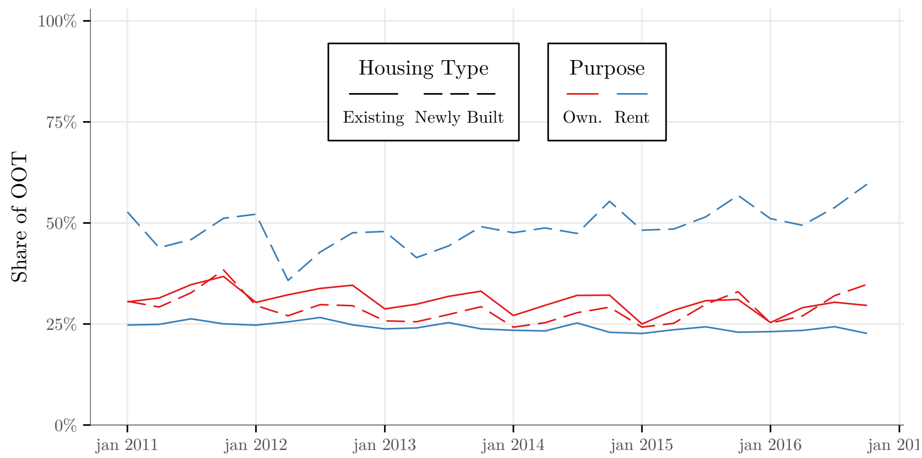

Figure 3: Average Country Share of OOT within Housing Market Segments

Notes: I report the share of OOT purchaser per housing segment according to tenure status and building type. OOT purchasers are defined as individuals that live outside the housing markets they invest in. Commuting zones are used to delineate housing markets. I aggregate the OOT share quarterly.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

OOT individuals are more likely to purchase newly built units in order to rent it (Figure 3). The share of OOT is indeed quite stable (despite a slight increase since January

2013) and close to 50% of purchaser in this segment. Conversely, OOT purchasers are less represented in the existing segment, despite similar purchase purpose. Finally, for owner–occupier purpose (including second homes), OOT purchaser represent nearly one third of purchasers. Consequently, considering the importance of OOT purchasers, the information asymmetry channel to explain price effect triggered by demand side policy is credible.

4 Identification Strategy

As most public policies, the assignation of rental investment scheme is not random, leading to endogeneity issues for treatment effect assessment. I first detail the practical implementation of the Pinel/Duflot scheme, and precise source of endogeneity that I need to deal with. I then present the identification strategy we set on to recover causal treatment effect on housing price. I leverage two specific policy shocks: policy introduction and repeal. I finally present our empirical specification.

4.1 Policy Design

French policy-makers introduced various demand-side policies to increase the rental supply in addition to social housing. Originated in 2003, rental investment schemes have evolved over time while the channel remains similar: boosting demand for rental investment. The last stable scheme was introduced in January 2013 (names as Duflot-Pinel[7]) and is scheduled to be abolished in December 2024. In the rest of the paper, I refer to Pinel to define the Duflot-Pinel scheme.

The Pinel scheme stimulates demand for rental investment through income tax cuts. As the main objective is to increase rental supply, the tax cut is available for newly built units only.[8] Recipients can thus deduct a share of their purchase in exchange for the

[7]In practice, Duflot and Pinel schemes are defined as different policies, but Pinel scheme is only a more flexible version of the Duflot scheme. The Duflot scheme was introduced in January 2013, while the Pinel scheme was introduced in July 2013.

[8]Only existing unhealthy units are eligible to the scheme in exchange of massive renovation works,

commitment to rent the unit with capped rent for period (three options are available, 6 years, 9 years or 12 years). The tax deductions increase with the commitment period, ranging from 12% of the purchase price for 6 years to 22% for 12 years. For example, a household that invests 100k in a house under the Pinel scheme over a period of 9 years will benefit from an annual tax reduction of 2k, giving a total subsidy of 18k over a period of 9 years. Remark that the financial incentive is attractive for households with important income tax. In addition, rental investment companies (*Société Civile Immobilière*, SCI) are eligible as well to the subsidy. The tax subsidy then apply to income collected from rent.

As rental markets are heterogeneous, policymakers target specific areas in which excess local demand is considered as important. This introduces spatial discontinuity commonly exploited to assess the policy effect (Chapelle, Vignolles, and Wolf, 2018; Bono and Trannoy, 2019) in line with the spatial continuity assumption (Keele and Titiunik, 2015). Using the ABC perimeter, a French housing zoning that defines housing market tenseness at the municipality level, it excludes from the policy municipalities with the lowest level of tenseness, representing 82.6% of French municipality (28,883 observations). On the opposite, municipalities that belong to two top levels of the ABC perimeter (namely A and B_1) have access to the policy. Finally, municipalities that constitute the intermediate level of the classification (named as B_2), experience major changes in the eligibility process over the last decade. Whereas they were previously eligible to the scheme prior to July 2013, they must obtain an exemption to be eligible for the Duflot Policy between July 2013 and January 2018. Since January 2018, they are no longer eligible for the Pinel scheme, regardless they previously benefit from an exemption.

The exemption process introduces a singularity within the B_2 municipalities that is fully attributable to rental investment scheme. Hence, focusing on these municipalities tackle the issues of compound treatment, as the ABC perimeter also determines intensity of other housing policies (including the *Prêt à Taux Zéro*). However, the exemption process is not

but it does not exist information about the importance of subsidised operations in existing segment. I assume that it is marginal and make robustness checks.

random. Municipalities must meet at least five out of ten criteria,[9] such as demographic dynamics, housing price, rent level or construction activity. In total, 1,251 municipalities benefited from a exemption, representing 29.7% of B_2 municipalities.

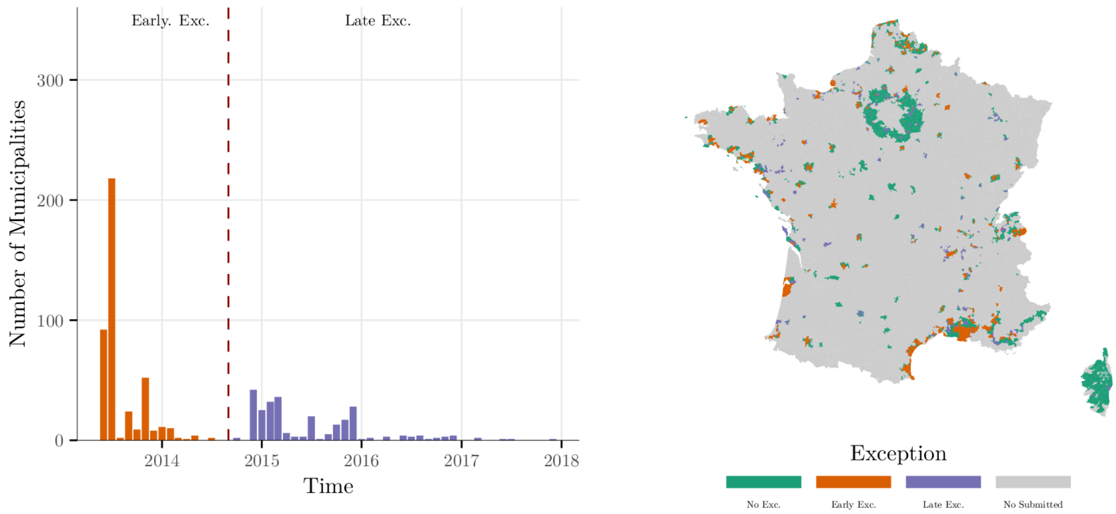

Figure 4: Timing of Introduction and Spatial Distribution of Exemption

Notes: The left panel represents the number of exemptions for B_2 according to the timing of the acceptance at the country level. The dotted red line represents the timing of the reshuffling of the ABC zoning that occurred in October 2014. The right panel represents the spatial distribution of exemptions according to the exemption timing. Early exemptions (prior the 2014 reform) are reported in orange, while early ones are reported in purple. Municipalities that do not belong to the B_2 tier in the ABC classification are reported in grey.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

However, exemptions are not granted at a similar time (Figure 4), even if most occurred in July-August 2013 (45.6% of accepted exceptions, 571 municipalities). These municipalities thus remain eligible for the rental investment scheme without interruption, as all B_2 municipalities were eligible for the Pinel scheme before July 2013. After the 2014 reform of the ABC classification, the number of exemptions decrease. These later exemptions mainly concerned municipalities affected by the ABC reform and reclassified as B_2 (whereas they were previously classified as C or B_1). Nevertheless, the exemption process generates multiple changes in the eligibility status for the rental investment scheme within the B_2 tier. I consider three major changes (Figure 4). Firstly, most municipalities experienced a policy repeal in July 2013, while early exempted ones remains eligible to the policy. Secondly, the staggered adoption of exemption over the 2014–2018 period leads to differentiated policy introduction. Finally, the policy was repealed for all municipalities

[9] I report the full list on Table A.1 based on French *décret*.

that benefit from the exemption in January 2018.[10]

Consequently, the process is endogenous to the characteristics of the municipalities. Although I cannot accurately collect individual criteria for each municipality due to data unavailability,[11] municipalities benefiting from an exemption have higher construction activity, higher density and a higher number of transactions (see detailed results in Figure A.1). However, our groups do not differ according to the unitary housing price (F-test = 1.4). In addition, the timing of the characteristics of exempted municipality differs according to the timing of the introduction. Municipalities benefiting from an early exemptions have higher density, number of buildings and number of transactions. Differences may be explained by the fact that late exemptions are granted to municipalities classified as C prior to the 2014 reform, which is the lowest level according to the ABC classification. Yet, the exemption process cannot be considered as random.

4.2 Strategy

Our identification strategy exploits change of eligibility status resulting from the exemption process within the B_2 tier. I adopt a difference-in-difference to deal with the endogeneity issues. However, I do not consider specific reforms. Firstly, the 2014 reform of the ABC zoning that indirectly affects the eligibility for the rental investment scheme (it affects the classification of some municipalities) also affects alternative housing policies leading to compound treatment issues. Secondly, the repeal of the Pinel scheme for all B_2 municipalities in 2018 was effectively introduced in March 2019. Early announcements are likely to affect both individual and developers behaviour.

I finally consider two major policy breakdowns. Firstly, I exploit the repeal of the rental investment policy in July 2013 for most B_2 municipalities, while municipalities benefiting from an exemption remain eligible. Secondly, I use the wave of exemptions that occurred in early 2015, which affected a sufficient number of municipalities to have consistent samples.

[10]However, the repeal was a two-stage process. Households can still apply for the tax cuts until March 2019 if they purchase a unit for which the permit was deposited prior January 2018.

[11]Most data used in the exemption process are unavailable to researchers.

These two dates enables us to assure that there is no simultaneous policy changes which could affect the housing price in specific segments. Consequently, I consider that any price change in housing market segments would be a consequence of either the repeal or the introduction of the rental investment scheme.

I leverage in both experiments the presence of two control groups: municipalities that never received the rental investment scheme and those that always received the treatment. In this design, municipalities that experience a change in their eligibility status composed the switcher group. Always treated groups is composed of municipalities classified as B_2 and which benefit from the early exemption. Hence, they always benefit from the rental investment scheme until January 2018. On the opposite, our never treated groups are composed of municipalities promoted from C to B_2 following the ABC reform and do not receive an exemption. Hence, considering that C municipalities do not benefit from rental investment scheme, we can assure that these municipalities are not eligible to the Pinel/Duflot scheme. Naturally, shifter groups are composed of municipalities that either experience a policy repeal in July 2013 or a policy re-introduction in early 2015. Figure 5 summarises group composition for our two experiments.

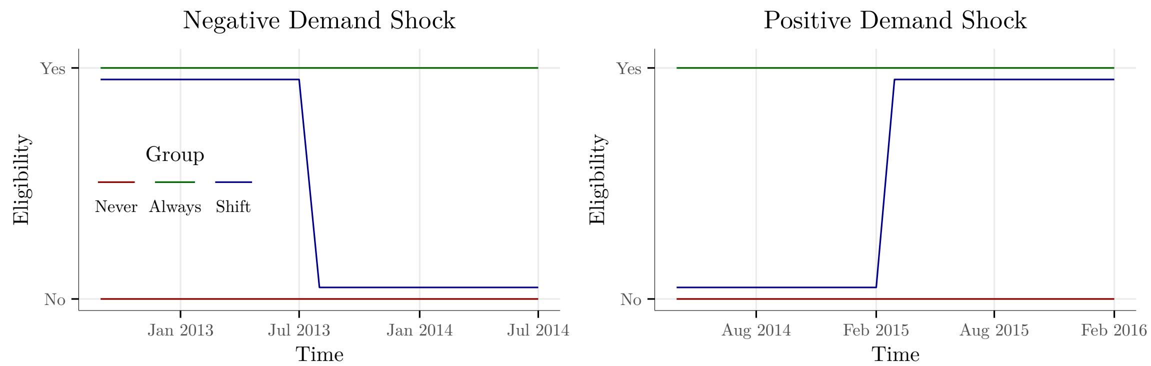

Figure 5: Group Composition According to the Two Natural Experiments Under Consideration

Notes: I report the two natural experiments under consideration. The left panel represents the negative demand shock, with municipalities that experience policy repeal in July 2013. The right panel represents the positive demand shock, with policy re-introduction for some municipalities in February 2015. In both cases, it exists municipalities that always (respectively never) benefit to the policy and constitute the always treated group (respectively never treated group).
Sources: Author's representation based on French minister of Ecological Transition data.

Whereas it is tempting to adopt a pooled models to increase sample size (and thus estimator precision) following the recent developments of DiD estimators (de Chaisemartin and D'Haultfœuille, 2018; Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2022), I rather adopt distinct models for policy repeal and introduction. My motivations for this approach are twofold. Firstly, increasing the period would increase the likelihood to have compound treatment issues, especially resulting from the 2014 reform. Secondly, it imposes to have monotonous effect of the policy, i.e. that the policy repeal have the same impact on housing price in absolute value that the policy introduction. Yet, we have no insight that support such assumption, price effect triggered by the policy introduction might persist over time, despite the policy repeal. Individuals models would be informative about the monotone aspect of the price effect. Finally, supply is likely to adapt to potential demand shift. By restricting the period close to the breakdowns, I can reveal whether housing markets are integrated according to tenure status and building type. Nonetheless, pooled models over the 2013–2018 period are informative about mid-term effects about the policy. We thus estimate such model as robustness checks to discuss whether recovered effects are likely to be transitory or persistent.

4.3 Empirical Specification

The model specification is similar for the two policy changes I consider. To make the best use of the presence of two potential groups (namely the always treated and never treated groups), I adopt a two-way fixed effects estimator. The model specification is

$$Y_{ijt}^{k\ell} = \alpha_t^{k\ell} + \beta_j^{k\ell} + \gamma_i^{k\ell} PC_i + \tau_1^{k\ell} W_{jt} + \tau_2^{k\ell} W_{jt} \cdot PC_i + \eta \mathbf{X}^\top + \varepsilon_{ijt}^{k\ell} \tag{3}$$

with $Y_{ijt}^{k\ell}$ transaction price for individuals i purchasing housing j at time t in market segment $k\ell$, $\alpha_t^{k\ell}$ time fixed effects, $\beta_j^{k\ell}$ group fixed effects, PC_i purchaser category of individual i, W_{jt} dummies that indicates whether municipality the transaction j is located in municipalities that is eligible to the rental investment scheme at time t and $\varepsilon_{ijt}^{k\ell}$ error term. The set of control variables $\mathbf{X}$ includes commuting zone areas (to account for cross-sectional heterogeneity). We add as robustness checks control variables about housing characteristics in different functional forms, which lower treatment effect but do not affect our conclusions.

I choose to select as time range from the policy breakdown a symmetric 12-months period. This choice is motivated by the balance between consistent sample with sufficient observations, and time restrictions to ensure supply inelasticity (requires to study the integration of housing market segments) and avoid compound treatment issues. Our conclusions are nonetheless robust to the specification of the time range. Finally, I choose to cluster standard error at the commuting zone level.

5 Results

I organise our results section as follows. Firstly, I provide results assuming that the housing market as homogeneous according to tenure and construction type. Secondly, I relax the homogeneous assumption by running independent regressions on each housing market segment defined *a priori*. I provide pre-trend comparisons to discuss the credibility of the parallel trends assumption and additional robustness checks to conclude this section.

5.1 Accounting for Information Asymmetries with Homogeneous Housing Markets

I first present results about pooled models, i.e. assuming that the potential price effect resulting from demand-side policies would affect the entire local market. In addition, I also control for housing characteristics and disentangle price effect experience by each population of interest. Table 1 presents results from the OLS estimation of Equation 3.

Under the local homogeneous assumption, the introduction of rental investment scheme causes price increase by 2.1%. We thus retrieve consistent results with Chapelle, Vignolles, and Wolf (2018). Nonetheless, we remark that the price effect is not monotone, as the policy repeal (i.e. negative shock for demand, columns 5 to 8) does not affect transaction price.

More interestingly, while we retrieve a significant price effect for demand shock, statistical

Table 1: Results for Price Adjustment (OLS Estimation) Under Homogeneous Local Housing Market Assumption

	Dependent Variable: Unit. Price (*log*)							
	Positive Shock		Negative Shock		Positive Shock		Negative Shock	
Covariate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Group (Shift)	0.007	-0.009	0.010	0.022	0.001	-0.009	0.007	0.016
	(0.033)	(0.035)	(0.059)	(0.061)	(0.033)	(0.034)	(0.059)	(0.057)
Group (Always)	-0.046*	-0.062***	0.003	-0.004	-0.043*	-0.055**	0.005	0.018
	(0.025)	(0.023)	(0.063)	(0.064)	(0.025)	(0.023)	(0.062)	(0.057)
OOT	-	-	-	-	0.065***	0.058***	0.071***	0.062***
	-	-	-	-	(0.009)	(0.008)	(0.010)	(0.009)
SCI	-	-	-	-	-0.036***	-0.038***	-0.036***	-0.043***
	-	-	-	-	(0.012)	(0.012)	(0.014)	(0.014)
Treatment	0.021**	0.022**	-0.003	-0.003	0.013	0.019	-0.012*	-0.011*
	(0.010)	(0.009)	(0.004)	(0.004)	(0.013)	(0.012)	(0.006)	(0.006)
Treatment × OOT	-	-	-	-	0.032*	0.019	0.029**	0.019*
	-	-	-	-	(0.019)	(0.017)	(0.012)	(0.010)
Treatment × SCI	-	-	-	-	-0.012	-0.013	0.012	0.011
	-	-	-	-	(0.018)	(0.017)	(0.016)	(0.016)
Monthly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.248	0.257	0.237	0.248	0.255	0.295	0.247	0.298
Adj. R^2	0.248	0.256	0.237	0.248	0.255	0.294	0.246	0.298
N	279,565	279,565	323,434	323,434	279,565	279,565	323,434	323,434

Notes: I report results from the estimation through OLS of Equation 3 to explain unitary housing price. Housing market are considered as homogeneous according to tenure status and building group. The first four columns also consider purchaser as homogeneous, whereas the last four introduces purchasers category in interaction with the treatment dummy. The variable `treatment` corresponds to the price effect associated with either the introduction or the removal of the policy. Reference group for municipality is never treated, while it is local purchaser for purchaser category. Standard errors are reported between parentheses, and clustered to commuting area using the `fixest` R package. Observation unit is transaction. I report time fixed effects and commuting areas fixed effects for clarity reasons in Figure B.1 to Figure B.4.

Sources: Data derived from DV3F and property tax files. Authors' calculation.
*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

significance fall when accounting for the information asymmetries channel. Indeed, OOT purchasers experience a higher transaction price by 6.0% (when we control for housing characteristics) in comparison to local ones. In this design, the price effect faced by local purchasers is no longer significant, thus reinforcing the need to account for purchaser category.

5.2 Accounting for Housing Market Segmentation with Homogeneous Purchasers

While we first consider local housing markets as perfectly integrated, we relax this assumption along the tenure decision and the housing type (i.e. whether it is newly built or existing unit). Hence, we estimate Equation 3 in the four segments that define local housing markets in our framework. In the first place, we do not account for the information asymmetries channel. Table 2 reports results from the OLS estimation.

Table 2: Results from TWFE Regressions According to Demand Shock Nature and Housing Market Segments

	Dependent Variable: Unit. Housing Price (*log*)							
	Positive Shock				Negative Shock			
	Existing		New Construction		Existing		New Construction	
Covariates	Own.	Rent	Own.	Rent	Own.	Rent	Own.	Rent
Group (Ref. Never Treated)								
Group (Shift)	0.006	-0.012	0.175***	0.089	0.021	0.018	-0.097	-0.099*
	(0.025)	(0.043)	(0.053)	(0.074)	(0.053)	(0.059)	(0.060)	(0.053)
Group (Always)	-0.047**	-0.090***	0.171***	0.177**	-0.005	-0.006	0.082	0.036
	(0.023)	(0.030)	(0.043)	(0.069)	(0.055)	(0.060)	(0.060)	(0.057)
Treatment	0.012	0.033*	-0.010	0.099**	-0.005	-0.002	-0.019	-0.133***
	(0.008)	(0.019)	(0.026)	(0.050)	(0.004)	(0.008)	(0.025)	(0.036)
Adj. R^2	0.242	0.261	0.247	0.360	0.236	0.245	0.259	0.301
Monthly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	208,929	53,815	9,929	6,892	229,723	68,860	14,423	10,428

Notes: I report results from the estimation through OLS of Equation 3 to explain unitary housing price. Housing market are considered as heterogeneous according to tenure status and building group. The first four columns consider the positive demand shock, whereas the last four consider the negative demand shock. Purchasers are considered as homogeneous. The variable `treatment` corresponds to the price effect associated with either the introduction or the removal of the policy. Reference group for municipality is never treated. Standard errors are reported between parentheses, and clustered to commuting area using the `fixest` R package. Observation unit is transaction. I report time fixed effects and commuting areas fixed effects for clarity reasons in Figure B.5 to Figure B.8.
Sources: Data derived from DV3F and property tax files. Authors' calculation.
*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

Relaxing the assumption about local homogeneous housing markets affects substantially conclusion about price capitalisation of demand-side policies. Firstly, while we do not find any significant effect after the policy repeal, we estimate a sizeable and significant price effect in the targeted segment by the policy (column 8, Table 2). Consecutive to the policy repeal, we estimate that price falls by 13.3%. In the meantime, we do not find any significant price effect in other segments, especially for owner-occupied ones. Hence, the lack of significance for pooled models is likely to results from the low representation of purchase in newly built units for rental purpose in the entire housing market.

Moreover, we retrieve more precise results fro price effect resulting from positive demand shock. While the aggregated effect is estimated to 2.0%, it recovers heterogeneity within housing market segments. Firstly, the effect is sizeable in the targeted segment and comparable in magnitude to those recover for the negative shock (9.9%, column 4). Secondly, transactions for owner-occupier are unaffected by the rental investment scheme. Thirdly, we estimate spillovers in segments defined by rental purpose and existing units. The transaction price indeed increase by 3.3% albeit a lower statistical significance ($p-value$

= 0.081). Hence, assuming that purchasers are perfectly informed, the price effect in this segment is likely to result from demand shift that originated from the subsidised segments (newly built units) to unsubsidised segments (existing units) due to higher activity.

5.3 Accounting Jointly for Housing Market Segmentation and Information Asymmetries

We now disentangle the price effect in each housing market segments discussed in the previous section according to whether it is channelled by demand shift or information asymmetries. Recall that we assume that local purchasers (i.e. that live within the housing market in which they invest) have better knowledge about the housing market than OOT purchaser. Hence, any price increase faced by local purchaser would be cause by overall demand shift. We then estimate Equation 3 in each housing market segment and Table 3 reports estimated results.

The heterogeneity in price according to purchaser category is consistent with our expectations. OOT purchasers do face a higher transaction price. However, the price difference is heterogeneous according to segments under consideration. The main structure is according to the tenure status. For owner-occupier purpose, the difference is more pronounced as the difference ranges from 7.0% to 12.0%. The difference falls between 2.1% and 7.1% for rental purpose. In addition, the price difference between OOT and local purchasers is greater for newly built units than for existing ones. While reasons that might explain these differences are out of scope of the paper, it provides stylized facts that support heterogeneity in information asymmetries according to purchase purpose and construction type.

Despite accounting for the information asymmetries channel, the price effect resulting either for the introduction or the repeal of the demand-side policies remains. However, the magnitude is lowered. We estimate that accounting for purchaser type lowers by 10% the effect, regardless on whether it is a positive or a negative demand shock. Consequently, assuming that purchasers have a similar knowledge of the housing market overestimate

Table 3: Results from TWFE Regressions According to Demand Shock Nature, Housing Market Segments and Purchaser Category

	Dependent Variable: Unit. Housing Price (*log*)							
	Positive Shock				Negative Shock			
	Existing		New Construction		Existing		New Construction	
Covariates	Own.	Rent	Own.	Rent	Own.	Rent	Own.	Rent
Group (Ref. Never Treated)								
Group (Shift)	-0.001	-0.013	0.160***	0.091	0.018	0.017	-0.092	-0.093*
	(0.025)	(0.043)	(0.053)	(0.072)	(0.052)	(0.059)	(0.059)	(0.054)
Group (Always)	-0.043*	-0.091***	0.169***	0.168**	-0.000	-0.007	0.092	0.040
	(0.023)	(0.031)	(0.041)	(0.068)	(0.055)	(0.060)	(0.059)	(0.058)
Treatment	0.007	0.033*	-0.006	0.091*	0.006	0.004	-0.005	-0.120***
	(0.011)	(0.019)	(0.029)	(0.051)	(0.006)	(0.009)	(0.027)	(0.040)
Purchaser Category (Ref. Local)								
OOT	0.070***	0.021***	0.119***	0.071***	0.103***	0.043***	0.120***	0.060***
	(0.009)	(0.008)	(0.021)	(0.024)	(0.012)	(0.010)	(0.013)	(0.010)
SCI	-	0.012	-	0.019	-	0.034***	-	-0.027
	-	(0.012)	-	(0.041)	-	(0.012)	-	(0.017)
Treatment × OOT	0.021	-0.002	0.007	0.006	-0.030**	-0.013	-0.038	-0.027
	(0.021)	(0.012)	(0.030)	(0.032)	(0.013)	(0.012)	(0.035)	(0.041)
Treatment × SCI	-	0.005	-	-0.021	-	-0.019	-	0.070
	-	(0.017)	-	(0.050)	-	(0.016)	-	(0.053)
R^2	0.250	0.264	0.280	0.390	0.247	0.249	0.287	0.323
Adj. R^2	0.249	0.261	0.266	0.375	0.246	0.246	0.278	0.312
Monthly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	208,929	53,815	9,929	6,892	229,723	68,860	14,423	10,428

Notes: I report results from the estimation through OLS of Equation 3 to explain unitary housing price. Housing market are considered as heterogeneous according to tenure status and building group. The first four columns consider the positive demand shock, whereas the last four consider the negative demand shock. I introduce the interaction between purchasers category and treatment dummy to disentangle price effect within populations. The variable `treatment` corresponds to the price effect associated with either the introduction or the removal of the policy. Reference group for municipality is never treated. Standard errors are reported between parentheses, and clustered to commuting area using the `fixest` R package. Observation unit is transaction.
Sources: Data derived from DV3F and property tax files. Authors' calculation.
*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

the price effect associated to rental investment scheme by 10%. Nonetheless, despite a lower statistical power of treatment effect for positive shock, the demand shift remains the main channel to explain the capitalisation of demand-side policies.

Finally, there is no specific information asymmetries based on proximity specific to rental investment scheme. Interaction terms between purchaser category and treatment dummy are not statistically significant. Hence, the overpayments of OOT purchasers within each housing market segment is constant over time, regardless the presence of the policy.

5.4 Falsification Test

The internal validity of our identification strategy pertains on the parallel trends assumption. While by nature it is not possible to test empirically whether the assumption holds, we perform a falsification test to discuss whether parallel trends holds *prior* to the introduction. We then arbitrary pre-empt the timing of either the introduction or the repeal of the policy from three months to twelve. We report the average of individual treatment effects and the number of effect with $p-value$ lowered than 0.05 with falsified breakdown timing. Results are reported in Figure 6.

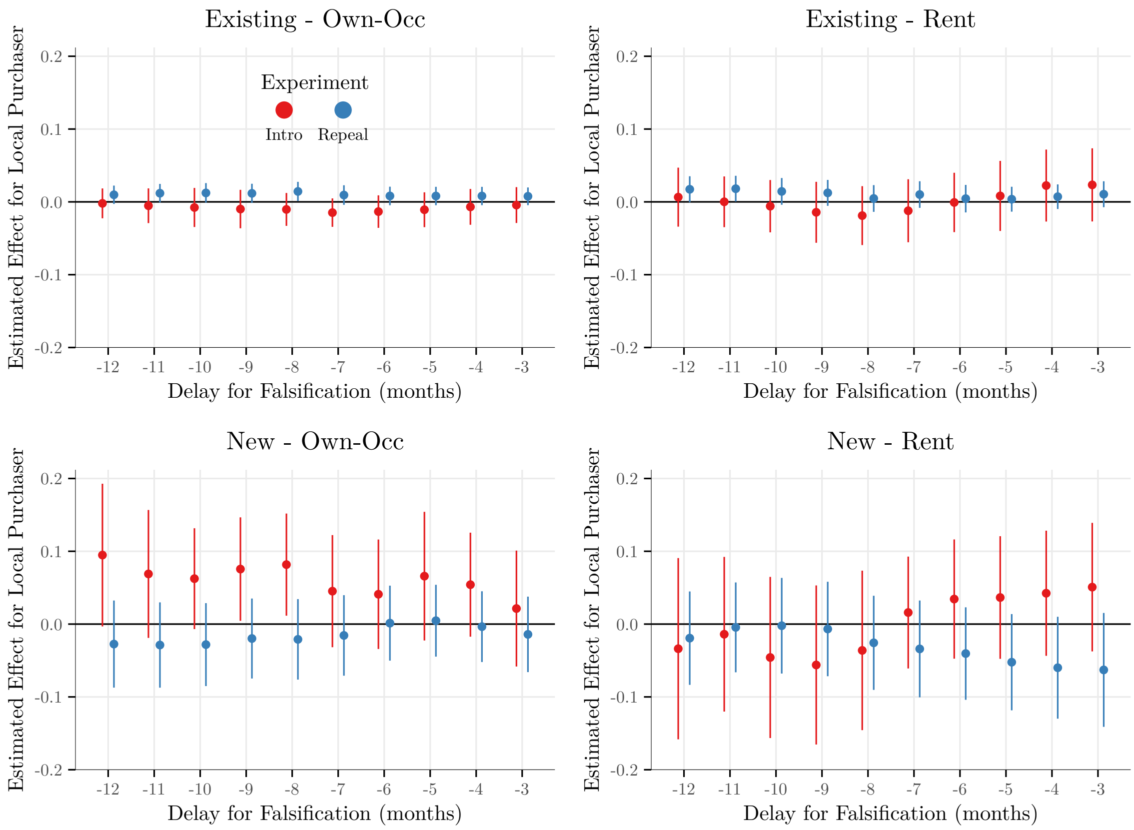

Figure 6: Treatment Effects for Local Purchasers with Falsified Timing in Eligibility Status Change

Notes: I report treatment results based on Equation 3 with falsified introduction timing. The range of falsified introduction is between 3 months and 12 months. Red dots corresponds to results for the introduction of the policy, while the blue one corresponds to the removal of the policy. Results are presented for each housing segments under consideration. I also report for each estimation the 95% confidence interval.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

Results with falsified timing for either policy removal or introduction are non-significant. Hence, we cannot reject the parallel trends assumption which ensure the internal validity of the identification strategy. This reinforces the credibility of the identification strategy, assuming that measured effects are imputable to the policy breakdown, especially for non-subsidised housing markets.

5.5 Robustness Checks

We discuss the robustness of our conclusions to three methodological choices we make: local – OOT definition, introduction of housing characteristics as control variables, and selected time range from the policy breakdowns.

Local – OOT Purchaser Definition The main assumption to disentangle the price effect channelled through information asymmetries pertains on the local – OOT purchaser definition. While we adapt the common definition provided by Chinco and Mayer (2016) based on the MSAs, we test whether our results are sensitive to this definition. We test for other definitions, three being defined according to the distance between the housing units and the purchasers (ranging from 5 to 20 kilometres), and one being based on equivalent MSA classification for France (Chapelle, Eyméoud, and Wolf, 2023). For brevity purpose, we only report coefficients associated to treatment effects and cross-sectional differences between purchaser categories (Table 4). Full results are reported in Appendix (Appendix C.2 to Appendix C.4).

Our conclusions are robust to the local – OOT purchaser definition. Although it affects the statistical significance of treatment effect for the smallest sample, it appears that local purchaser still experience price increase (respectively decrease) consecutive to policy introduction (respectively increase). The stricter the definition of local purchaser, the lower the price effect is. Hence, our results are likely to correspond to the lower bound for the price effect channelled through information asymmetries, and consequently the upper bound for the demand side channel. However, the latter channel is still the main channel for the price effect caused by rental investment scheme.

In addition, remark that regardless the definition of local – OOT purchaser, segments defined by transactions achieved for owner–occupier purposes remains unaffected by the policy. Hence, it reinforces our results about the lack of integration within housing markets according to tenure status.

Table 4: Main Results with Alternative Definition for Local and OOT Purchasers

	Dependent Variable: Unit. Housing Price (*log*)							
	Positive Shock				Negative Shock			
	Existing		New Construction		Existing		New Construction	
Covariates	Own.	Rent	Own.	Rent	Own.	Rent	Own.	Rent
	OOT Criterion: Distance > 5km							
Treatment	0.015	0.039**	-0.011	0.079	0.005	0.002	-0.002	-0.084*
	(0.010)	(0.019)	(0.030)	(0.051)	(0.007)	(0.010)	(0.031)	(0.043)
Out Purchaser	0.058***	0.007	0.087***	0.084***	0.076***	0.025***	0.081***	0.087***
	(0.005)	(0.007)	(0.017)	(0.025)	(0.009)	(0.007)	(0.013)	(0.011)
SCI	-	0.010	-	0.049	-	0.035***	-	0.008
		(0.012)		(0.040)		(0.012)		(0.018)
Treatment × Out Purchaser	-0.003	-0.014	0.001	0.008	-0.015	-0.003	-0.033	-0.072**
	(0.012)	(0.010)	(0.026)	(0.031)	(0.010)	(0.010)	(0.028)	(0.029)
Treatment × SCI	-	-0.002	-	-0.019	-	-0.017	-	0.036
		(0.017)		(0.049)		(0.016)		(0.048)
	OOT Criterion: Distance > 10km							
Treatment	0.011	0.032*	-0.016	0.090*	0.004	0.004	-0.002	-0.116***
	(0.011)	(0.019)	(0.028)	(0.053)	(0.006)	(0.009)	(0.028)	(0.042)
Out Purchaser	0.054***	0.005	0.090***	0.087***	0.078***	0.027***	0.101***	0.073***
	(0.007)	(0.007)	(0.019)	(0.026)	(0.011)	(0.009)	(0.012)	(0.009)
SCI	-	0.008	-	0.034	-	0.033**	-	-0.010
		(0.012)		(0.040)		(0.013)		(0.017)
Treatment × Out Purchaser	0.005	-0.000	0.024	-0.006	-0.018	-0.009	-0.041	-0.021
	(0.017)	(0.011)	(0.029)	(0.033)	(0.011)	(0.010)	(0.029)	(0.031)
Treatment × SCI	-	0.006	-	-0.022	-	-0.020	-	0.066
		(0.017)		(0.049)		(0.016)		(0.052)
	OOT Criterion: Distance > 20km							
Treatment	0.007	0.031	-0.006	0.091*	0.005	0.005	-0.006	-0.118***
	(0.012)	(0.019)	(0.029)	(0.051)	(0.006)	(0.009)	(0.028)	(0.040)
Out Purchaser	0.066***	0.017**	0.127***	0.075***	0.098***	0.042***	0.122***	0.064***
	(0.010)	(0.008)	(0.022)	(0.026)	(0.012)	(0.010)	(0.014)	(0.009)
SCI	-	0.011	-	0.019	-	0.034***	-	-0.023
		(0.012)		(0.040)		(0.012)		(0.017)
Treatment × Out Purchaser	0.018	0.003	-0.002	0.001	-0.030**	-0.015	-0.037	-0.034
	(0.021)	(0.012)	(0.030)	(0.033)	(0.014)	(0.012)	(0.036)	(0.040)
Treatment × SCI	-	0.007	-	-0.018	-	-0.021	-	0.067
		(0.017)		(0.049)		(0.016)		(0.052)

Notes: I report results from the estimation through OLS of Equation 3 to explain unitary housing price with alternative definitions for OOT purchasers. The top (respectively middle and bottom) panel considers OOT as purchasers that live 5km (respectively 10km and 20km) away from their purchase. Housing market are considered as heterogeneous according to tenure status and building group. The first four columns consider the positive demand shock, whereas the last four consider the negative demand shock. I introduce the interaction between purchasers category and treatment dummy to disentangle price effect within populations. The variable `treatment` corresponds to the price effect associated with either the introduction or the removal of the policy. Reference group for municipality is never treated. Standard errors are reported between parentheses, and clustered to commuting area using the `fixest` R package. Observation unit is transaction.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

Control for Housing Characteristics We also test whether our results are robust to the introduction of housing characteristics as control variables and the choice of time period from the breakdown (set to 12 months in our preferred specification). To characterise housing units, we introduce the surface in square metres, the distance from the centre of the commuting zones, and the presence of particular facilities such as balcony, cellar or parking lot. For numerical variables, we test two different specifications: linear and endogenous smoothing transformation (Wood, 2017). We report the average estimated effect by functional form over the Table 5.

Table 5: Robustness Checks From TWFE Results

	Dependent Variable: Unit. Housing Price (*log*)							
	Positive Shock				Negative Shock			
	Existing		New Construction		Existing		New Construction	
Covariates	Own.	Rent	Own.	Rent	Own.	Rent	Own.	Rent
				Linear Control				
Treatment	0.008	0.042**	-0.008	0.075	0.002	0.003	-0.007	-0.100***
	(0.010)	(0.017)	(0.029)	(0.047)	(0.005)	(0.008)	(0.026)	(0.033)
OOT	0.066***	0.017**	0.095***	0.048**	0.091***	0.025**	0.112***	0.035***
	(0.008)	(0.008)	(0.016)	(0.024)	(0.009)	(0.010)	(0.012)	(0.008)
SCI		0.008		0.049		0.036***		0.005
		(0.012)		(0.039)		(0.012)		(0.018)
Treatment × OOT	0.013	-0.010	0.004	-0.001	-0.023**	0.002	-0.040	-0.029
	(0.019)	(0.012)	(0.028)	(0.028)	(0.011)	(0.012)	(0.034)	(0.042)
Treatment × SCI		0.013		-0.005		-0.026		0.061
		(0.016)		(0.048)		(0.016)		(0.049)
				Smoothing Transformation Control				
Treatment	0.011	0.044***	-0.009	0.075***	0.000	-0.000	-0.008	-0.092***
	(0.007)	(0.014)	(0.028)	(0.027)	(0.003)	(0.007)	(0.012)	(0.015)
OOT	0.077***	0.024***	0.094***	0.044***	0.092***	0.023***	0.115***	0.034***
	(0.002)	(0.006)	(0.010)	(0.013)	(0.002)	(0.004)	(0.006)	(0.005)
SCI		0.016**		0.045***		0.034***		0.005
		(0.006)		(0.017)		(0.004)		(0.010)
Treatment × OOT	0.006	-0.016**	0.010	0.002	-0.014***	0.007	-0.042***	-0.030*
	(0.004)	(0.008)	(0.014)	(0.014)	(0.003)	(0.008)	(0.013)	(0.016)
Treatment × SCI		0.006		-0.001		-0.019**		0.053**
		(0.009)		(0.022)		(0.009)		(0.023)

Notes: I report results from the estimation through OLS of Equation 3 to explain unitary housing price. We add control variables such as surface, distance to the centre, housing type or presence of equipments in the Equation 3. The top panel adopts a linear specification for numerical control variables, whereas the bottom considers splines transformation using semi-parametric GAM (Wood, 2017). Housing market are considered as heterogeneous according to tenure status and building group. The first four columns consider the positive demand shock, whereas the last four consider the negative demand shock. I introduce the interaction between purchasers category and treatment dummy to disentangle price effect within populations. The variable `treatment` corresponds to the price effect associated with either the introduction or the removal of the policy. Reference group for municipality is never treated. Standard errors are reported between parentheses, and clustered to commuting area using the `fixest` R package. Observation unit is transaction.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

The introduction of control variables, regardless the functional form, slightly affects the price effect experienced by local purchasers. It does lower the price fall for the policy repeal (respectively -10.5% and -9.6% while our main results are -12.0%). Hence, while housing characteristics might slightly differs across time and space, it appears to have a marginal impact on our results.

Time Range Finally, we close the robustness checks section by assessing whether the time range from the policy breakdowns drives our results. We then range the time range from 6 months to 15 months, while recall that we set it to 12 months in our preferred specification. For brevity purpose, we only report summary results about relevant statistics, i.e. the price effect experienced by each population (local and OOT purchasers). We report detailed results in Appendix C.1.

Table 6: Summary about Treatment Effects Introducing Time Range Variation in DiD Experiments

	Positive Shock				Negative Shock			
	Existing		New Construction		Existing		New Construction	
	Own.	Rent	Own.	Rent	Own.	Rent	Own.	Rent
Average Effect	0.007	0.052	0.016	0.088	0.004	0.006	0.000	-0.115
Significance	0/10	8/10	0/10	6/10	0/10	0/10	0/10	10/10

Notes: I report the aggregate treatment effect based on Equation 3 with range in the time window used in the natural experiment specification. The time window ranges from 6 months to 15 months. The bottom line reports the number of significant estimation using a 95% confidence interval.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

The time range does not affect our conclusion about either housing market integration or information asymmetries channel. On the one hand, regardless the time range used for the policy repeal experiment, local purchaser experience an increase of the purchase price higher than 10%. On the other hand, it affects the significance of the relevant statistics for the policy introduction experiments. Nonetheless, considering that most non-significant estimations concerns the smallest time range, we expect attrition of sample size to drive the loss of significance.

6 Discussion and Concluding Remarks

Whereas previous studies have focused specifically on the potential inflationary effects of demand-side policies in the housing market, I extend the assessment of price effects in two directions. Firstly, I enhance the present findings by introducing heterogeneity based on tenure status and construction type, the two criteria used by policymakers to target particular housing segments. I demonstrate that the rental investment scheme has no effect on owner-occupied segments, at least in the short-term. More interestingly, there is a positive and significant price effect for buy-to-rent purchases of existing units. Although I cannot rule out the possibility that the increased demand is caused by the presence of subsidised operations in existing units (existing units are eligible for the rental investment scheme subject to massive renovation),[12] I interpret these results as a shift in demand

[12] Robustness checks are in progress.

from subsidised to non-subsidised segments. The existing segments then absorb part of the positive demand shock, leading to a price increase. Secondly, I distinguish the price effect that is influenced by the shift in demand from the impact of information asymmetry. By differentiating between local and out-of-town (OOT) buyers, I demonstrate that the majority of the price effect arises from the shift in demand rather than from fluctuations in the population that is exposed to information asymmetries.

These findings provide valuable information for policymakers. Firstly, they confirm that demand-side policies have a significant impact on housing prices in the short term. The estimated effect is significantly larger than in previous papers dealing with these policies (Chapelle, Vignolles, and Wolf, 2018). The impact on the subsidised segment is so pronounced that I estimate the subsidy to be beneficial for recipients if the commitment period is at least nine years. In the 6-year design, the subsidy is equal to the price increase. However, if we introduce a discount rate factor (which smooths the subsidy over the commitment period), it appears that the subsidy may actually be counter-productive for the recipients. Unfortunately, I cannot precisely estimate the share of recipients for which the subsidy is counter-productive as I do not observe the distribution according to the commitment period.

However, price capitalisation is unlikely to persist over time. Our findings illustrate that there is an immediate decrease in price following the repeal of the policy. Therefore, inflation is closely associated with the existence of the policy. Decreasing information asymmetry for OOT purchasers would slightly alleviate the price capitalisation. Finally, the joint implementation of demand-side policies supporting buy-to-let and owner-occupying purposes (the *Prêt à Taux Zéro*) appears to have no adverse effects. As it does not affect the price for owner-occupier purposes, I conclude that these policies have an independent effect on the housing market. Nonetheless, this conclusion is only valid for the short-term as this joint implementation might support competition for land (Bono and Trannoy, 2019), leading to adverse effects in the long-term. However, despite its relevance, this dimension is outside the scope of this paper.

Despite their direct impact on the measurement of the price effect, the difference between

OOT purchasers and local purchasers is heterogeneous. Specifically, the difference is more significant for purchases made for own occupation rather than buy-to-rent. Buy-to-rent purchases are likely to be driven by profitability rates, whereas owner-occupation purchases are more likely to meet consumption needs. Hence, buy-to-let investors are more likely to acquire additional information regarding the housing market, including OOT purchasers, to reduce any uncertainty about the profitability rates. Conversely, owner-occupiers purchases, which are less likely to generate income through this investment, are motivated purely by consumption choices. This fundamental difference in tenure status may explain the price heterogeneity between local and OOT purchasers, and further investigations are needed to explain these differences.

From the housing segmentation literature approach, these results reveal new dimensions of the housing market segmentation based on different methodological approach. While most previous work on this topic has taken a spatial approach, these results reveal a lack of substitutability (the main criterion to identify segmentation according to Bhattacharjee et al., 2016) between rented and owner-occupied units. It is important to objectively investigate the relationship between spatial and tenure dimensions, exploring any potential overlap. Methodologically, a new approach utilising natural experiments instead of hedonic models is presented.

Finally, in consideration of our conclusions, I suggest three directions for future research. First, it seems relevant to extend the short-term effects recovered in this paper to the medium-term perspective. Although our conclusions are robust to the chosen time period, I observe variation in the recovered effects. Understanding the medium-term effects and highlighting the role of housing market integration seems to be an attractive direction. Second, I suggest unveiling the underlying determinants of spatial segmentation of housing market. For example, disentangling segmentation caused by spatial proximity from that caused by structural housing characteristics seems to be an appealing direction. Finally, as our results confirm that at least part of the subsidy is captured by developers (while it does not seem to affect the capital gains of current homeowners), the question arises whether these gains captured by developers distort their behaviour, especially in the land market.

References

Baum-Snow, Nathaniel and Justin Marion (2009), "The Effects of Low Income Housing Tax Credit Developments on Neighborhoods". *Journal of Public Economics* 93.5-6, pp. 654–666.

Ben-Shahar, Danny, Stuart Gabriel, and Roni Golan (2019), "Housing Affordability and Inequality:A Consumption-Adjusted Approach". *Journal of Housing Economics* 45, p. 101567.

Bhattacharjee, Arnab, Eduardo Castro, Taps Maiti, and João Marques (2016), "Endogenous Spatial Regression and Delineation of Submarkets: A New Framework with Application to Housing Markets". *Journal of Applied Econometrics* 31.1, pp. 32–57.

Bono, Pierre-Henri and Alain Trannoy (2019), "The Impact of the 'Scellier' Income Tax Relief on Building Land Prices in France". *Economie et Statistique / Economics and Statistics* 507d, pp. 91–114.

Bourassa, Steven C., Martijn Dröes, and Martin Hoesli (2021), "Hedonic Models and Market Segmentation". *SSRN Electronic Journal* 21-62.

Brueckner, Jan K and Stuart S Rosenthal (2009), "Gentrification and Neighborhood Housing Cycles: Will America's Future Downtowns Be Rich?" *Review of Economics and Statistics* 91.4, pp. 725–743.

Caldera, Aida and Åsa Johansson (2013), "The Price Responsiveness of Housing Supply in OECD Countries". *Journal of Housing Economics* 22.3, pp. 231–249.

Chapelle, Guillaume, Jean-Baptiste Eyméoud, and Clara Wolf (2023), "Land-Use Regulation and Housing Supply Elasticity: Evidence from France". *THEMA Working Paper* 08, p. 48.

Chapelle, Guillaume, Benjamin Vignolles, and Clara Wolf (2018), "Impact of a Housing Tax Credit on Local Housing Markets: Evidence from France". *Annals of Economics and Statistics* 130, pp. 101–131.

Chareyron, Sylvain, Tidiane Ly, and Yohann Trouvé-Sargison (2021), "Ownership Incentives and Housing Affordability: Evidence from France". *SSRN Electronic Journal*.

Charles, Kerwin Kofi, Erik Hurst, and Matthew J Notowidigdo (2019), “Housing Booms, Manufacturing Decline and Labour Market Outcomes”. *The Economic Journal* 129.617, pp. 209–248.

Chinco, Alex and Christopher Mayer (2016), “Misinformed Speculators and Mispricing in the Housing Market”. *Review of Financial Studies* 29.2, pp. 486–522.

Coën, Alain, Alexis Pourcelot, and Richard Malle (2022), “Macroeconomic Shocks and Ripple Effects in the Greater Paris Metropolis”. *Journal of Housing Economics* 56.101823.

de Chaisemartin, Clément and Xavier D’Haultfœuille (2018), “Fuzzy Differences-in-Differences”. *The Review of Economic Studies* 85.2, pp. 999–1028.

de Chaisemartin, Clément and Xavier D’Haultfœuille (2022), “Two-Way Fixed Effects and Differences-in-Differences with Heterogeneous Treatment Effects: A Survey”. *The Econometrics Journal.*

Diamond, Rebecca and Cecile Gaubert (2022), “Spatial Sorting and Inequality”. *Annual Review of Economics* 14.1, pp. 795–819.

Duranton, Gilles and Diego Puga (2015), “Urban Land Use”. *Handbook of Regional and Urban Economics*. Vol. 5A. Boston, MA: Elsevier, p. 698.

Dustmann, Christian, Bernd Fitzenberger, and Markus Zimmermann (2022), “Housing Expenditure and Income Inequality”. *The Economic Journal* 132.645, pp. 1709–1736.

Ellen, Ingrid Gould (2020), “What Do We Know about Housing Choice Vouchers?” *Regional Science and Urban Economics* 80, p. 103380.

Eriksen, Michael D. and Stuart S. Rosenthal (2010), “Crowd out Effects of Place-Based Subsidized Rental Housing: New Evidence from the LIHTC Program”. *Journal of Public Economics* 94.11-12, pp. 953–966.

Fack, Gabrielle (2006), “Are Housing Benefit an Effective Way to Redistribute Income? Evidence from a Natural Experiment in France”. *Labour Economics* 13.6, pp. 747–771.

Favilukis, Jack and Stijn Van Nieuwerburgh (2021), “Out-of-Town Home Buyers and City Welfare”. *The Journal of Finance* 76.5, pp. 2577–2638.

Gabriel, Stuart and Gary Painter (2020), “Why Affordability Matters”. *Regional Science and Urban Economics* 80.103378.

Glaeser, Edward and Joseph Gyourko (2002), "The Impact of Zoning on Housing Affordability". *NBER Working Paper* 8835, p. 37.

Gobillon, Laurent and Benjamin Vignolles (2016), "Evaluation de l'effet d'une politique spatialisée d'accès au logement: la loi Solidarité et Renouvellement Urbain (SRU)". *Revue Économique* 67.3, pp. 615–637.

Goffette-Nagot, Florence and Modibo Sidibé (2016), "Housing Wealth Accumulation: The Role of Public Housing". *Regional Science and Urban Economics* 57, pp. 12–22.

Goodman, Allen C. and Thomas G. Thibodeau (1998), "Housing Market Segmentation". *Journal of Housing Economics* 7.2, pp. 121–143.

Goodman-Bacon, Andrew (2021), "Difference-in-Differences with Variation in Treatment Timing". *Journal of Econometrics* 225.2, pp. 254–277.

Ha, Sejeong, Christian A.L. Hilber, and Olivier Schöni (2021), "Do Long-Distance Moves Discourage Homeownership? Evidence from England". *Journal of Housing Economics* 53.101766.

Hilber, Christian A. L. and Wouter Vermeulen (2016), "The Impact of Supply Constraints on House Prices in England". *The Economic Journal* 126.591, pp. 358–405.

Ihlanfeldt, Keith and Tom Mayock (2012), "Information, Search, and House Prices: Revisited". *The Journal of Real Estate Finance and Economics* 44.1-2, pp. 90–115.

Keele, Luke and Rocío Titiunik (2015), "Geographic Boundaries as Regression Discontinuities". *Political Analysis* 23.1, pp. 127–155.

Le Blanc, David and Anne Laferrère (2001), "The Effect of Public Social Housing on Households' Consumption in France". *Journal of Housing Economics* 10.4, pp. 429–455.

Lei, Rémi (2023), "The Dynamics of Property Ownership with Spatial Markets and Policies". PhD thesis. Dijon: Bourgogne France-Comté.

Levy, Antoine (2021), "Housing Policy with Home-Biased Landlords: Evidence from French Rental Markets". *Working Paper*, p. 86.

Li, L. and K. W. Chau (2023), "Information Asymmetry with Heterogeneous Buyers and Sellers in the Housing Market". *The Journal of Real Estate Finance and Economics*.

Li, Xiaodi (2022), "Do New Housing Units in Your Backyard Raise Your Rents?" *Journal of Economic Geography* 22.6, pp. 1309–1352.

Ling, David C., Andy Naranjo, and Benjamin Scheick (2021), "There Is No Place like Home: Information Asymmetries, Local Asset Concentration, and Portfolio Returns". *Real Estate Economics* 49.1, pp. 36–74.

Malpezzi, Stephen and Kerry Vandell (2002), "Does the Low-Income Housing Tax Credit Increase the Supply of Housing?" *Journal of Housing Economics* 11.4, pp. 360–380.

Molloy, Raven, Charles G. Nathanson, and Andrew Paciorek (2022), "Housing Supply and Affordability: Evidence from Rents, Housing Consumption and Household Location". *Journal of Urban Economics* 129, p. 103427.

Newman, Sandra and C. Scott Holupka (2016), "Housing Affordability And Children's Cognitive Achievement". *Health Affairs* 35.11, pp. 2092–2099.

Quigley, John M and Steven Raphael (2004), "Is Housing Unaffordable? Why Isn't It More Affordable?" *Journal of Economic Perspectives* 18.1, pp. 191–214.

Saiz, Albert (2010), "The Geographic Determinants of Housing Supply". *Quarterly Journal of Economics* 125.3, pp. 1253–1296.

Turnbull, Geoffrey K. and C.F. Sirmans (1993), "Information, Search, and House Prices". *Regional Science and Urban Economics* 23.4, pp. 545–557.

Turner, Matthew, Andrew Haughwout, and Wilbert Van der Klauw (2014), "Land Use Regulation and Welfare". *Econometrica* 82.4, pp. 1341–1403.

Wood, Simon N. (2017), *Generalized Additive Models: An Introduction with R*. 2nd ed. Chapman and Hall/CRC.

A Additional Material

A.1 Criterion for Municipalities to Obtain Exception

Table A.1: Requirement for Municipalities to Obtain Exception

Topic	Data Sources	Require Criterion
Share of household with rent higher than 39% of income of	CAF*	Higher than 15%
Mobility rate in social housing	Housing minister survey*	Lower than 15%
Average unitary housing price for existing apartments	Perval Data+	Higher than 1.75k
Number of transactions	Perval Data+	Higher than 10.25 per 1k inhabitants
Average unitary housing price for newly built apartments	Commercialisation survey +	Higher than 2,075 euros
Average unitary rent	Clameur Data *	Higher than 7.3 euros per months
Population evolution	INSEE	Higher than 0.4%
Vacancy rate	INSEE	Lower than 9%
Average annual new construction over a three year period	SITADEL	Higher than 200 per year
Average of all criterion	Multi-data	Must be higher than 25/100

Notes: We report criterion municipalities must comply to obtain exception for rental investment scheme.

A.2 Descriptive Statistics about B_2 According to Eligibility Status

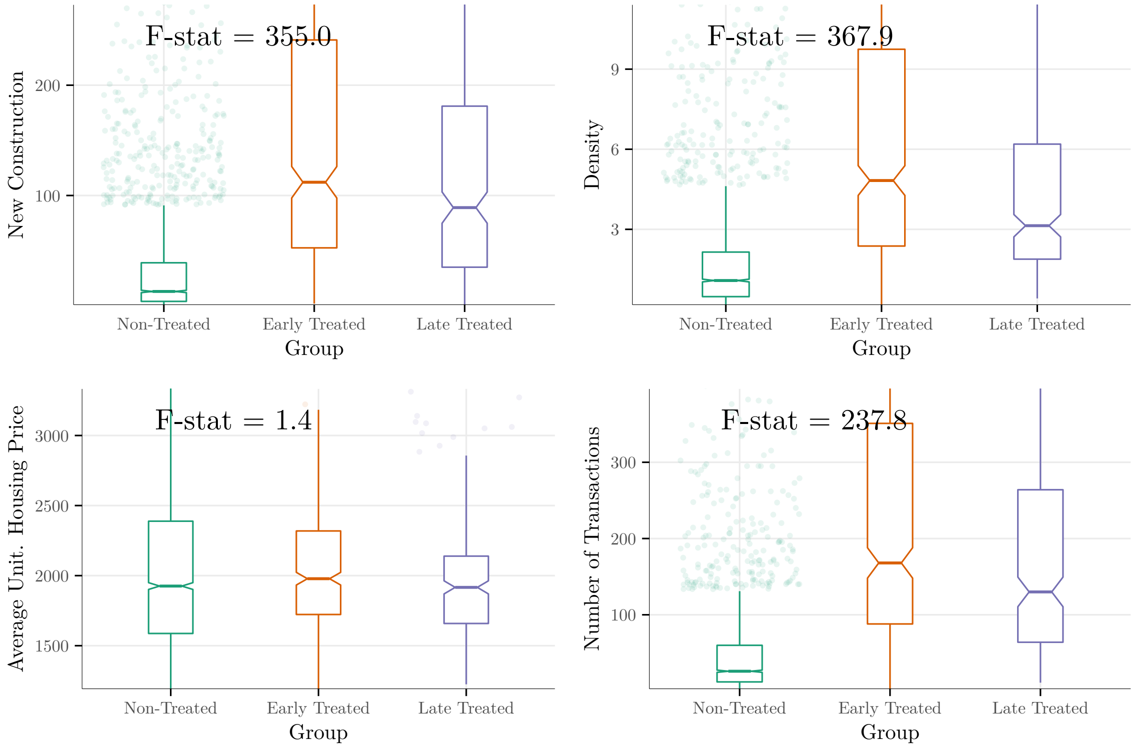

Figure A.1: Descriptive Statistics About B_2 Municipalities

Notes: I report the distribution of the average per municipalities for the number of new constructions (2010–2012), population density (2013), average unitary housing price (2010–2012) and number of transactions (2010–2012). I classify each group according to whether municipalities benefit from an exception. In addition, I introduce a subcategory to distinguish early exceptions from late ones. Finally, I report $F-value$ from ANOVA tests to test the equality of values between groups. *Sources:* Data derived from DV3F and property tax files. Authors' calculation.

A.3 Descriptive Statistics for Transactions Selected for Negative Demand Shock (12-months range)

Table A.2: Descriptive Statistics for Transactions Selected for Repeal Natural Experiments

			Existing		Newly Built	
Group	Variable	Metrics	Rent	Own.	Rent	Own.
Always Treated	N		5,117	14,887	479	1,168
	Unit. Price	Mean	1,642	1,816	2,538	2,398
		Median	1,618	1,783	2,607	2,375
		Std. Dev.	(525)	(569)	(667)	(668)
	Price	Mean	118,074	162,982	157,395	181,247
		Median	104,000	150,100	155,868	174,772
		Std. Dev.	(69,745)	(81,187)	(58,903)	(72,000)
Shifters	N		10,787	47,487	894	2,172
	Unit. Price	Mean	1,557	1,799	2,425	2,199
		Median	1,500	1,734	2,420	2,119
		Std. Dev.	(597)	(648)	(750)	(769)
	Price	Mean	129,743	172,664	157,172	188,008
		Median	118,000	160,000	149,980	179,000
		Std. Dev.	(80,207)	(83,312)	(73,803)	(77,983)
Never Treated	N		31,384	80,621	5,379	6,200
	Unit. Price	Mean	1,774	1,970	2,962	2,722
		Median	1,714	1,895	2,967	2,667
		Std. Dev.	(654)	(708)	(626)	(776)
	Price	Mean	111,947	168,415	157,202	201,488
		Median	90,000	150,000	150,620	182,150
		Std. Dev.	(78,506)	(94,799)	(55,725)	(87,135)

Notes: We report for each group (never treated, shifters, always treated) descriptive statistics about transactions. We select transactions six months from the policy repeal, i.e. from January 2013 to December 2013. We detail the number of transactions, the share of local purchasers, the average unitary housing price, and the average price. We also report for relevant metrics standard deviation between parentheses. We also introduce median. All these statistics are provided according to our segment definition (columns).
Sources: Data derived from DV3F and property tax files. Authors' calculation.

A.4 Descriptive Statistics for Transactions Selected for Positive Demand Shock (12-months range)

Table A.3: Descriptive Statistics for Transactions Selected for Introduction Natural Experiments

			Existing		Newly Built	
Group	Variable	Metrics	Rent	Own.	Rent	Own.
Always Treated	N		16,554	69,825	942	3,120
	Unit. Price	Mean	1,595	1,828	2,538	2,389
		Median	1,537	1,759	2,504	2,266
		Std. Dev.	(638)	(678)	(879)	(873)
	Price	Mean	132,916	174,822	169,136	200,223
		Median	120,000	163,000	158,000	185,000
		Std. Dev.	(82,816)	(86,642)	(79,385)	(90,816)
Shifters	N		1,826	6,893	190	381
	Unit. Price	Mean	1,531	1,854	2,604	2,474
		Median	1,479	1,794	2,606	2,390
		Std. Dev.	(551)	(667)	(551)	(700)
	Price	Mean	115,952	157,207	171,963	197,104
		Median	102,000	145,750	171,000	182,000
		Std. Dev.	(71,489)	(85,756)	(45,590)	(73,677)
Never Treated	N		18,393	52,214	3,265	2,980
	Unit. Price	Mean	1,548	1,775	2,932	2,608
		Median	1,500	1,722	2,942	2,611
		Std. Dev.	(565)	(628)	(553)	(781)
	Price	Mean	106,790	160,360	160,330	203,019
		Median	87,000	145,000	155,000	183,500
		Std. Dev.	(73,759)	(88,350)	(50,707)	(90,810)

Notes: We report for each group (never treated, shifters, always treated) descriptive statistics about transactions. We select transactions six months from the policy introduction, i.e. from June 2014 to November 2013. We detail the number of transactions, the share of local purchasers, the average unitary housing price, and the average price. We also report for relevant metrics standard deviation between parentheses. We also introduce median. All these statistics are provided according to our segment definition (columns).
Sources: Data derived from DV3F and property tax files. Authors' calculation.

A.5 Summary Statistics about Municipalities Groups Adopted in DiD Designs

Table A.4: Composition of Groups According to the Nature of the Demand Shock

	Positive Shock				Negative Shock			
	Existing		New Construction		Existing		New Construction	
Group	Own	Rent	Own	Rent	Own	Rent	Own	Rent
	Number of Transactions							
Shifter	47,487	10,787	2,172	894	69,825	16,554	3,120	942
Always Treated	80,621	31,384	6,200	5,379	6,893	1,826	381	190
	Number of Municipalities							
Shifter	1,955	1,955	1,955	1,955	2,371	2,371	2,371	2,371
Always Treated	457	457	457	457	72	72	72	72
	Transaction per Municipality (Average)							
Shifter	24.3	5.5	1.1	0.5	29.4	7.0	1.3	0.4
Always Treated	176.4	68.7	13.6	11.8	95.7	25.4	5.3	2.6

Notes: I report for each natural experiments I implement (either positive or negative demand shock), the composition of each group exploited in the two-way fixed effects specification. The top panel corresponds to the number of transactions, middle one to the number of municipalities with at least one transaction, while the bottom panel compute the average number of transactions per municipalities. For the positive shock, the time range from demand shock is set to 9 months (from February 2015), while it is 6 months for the negative shock (July 2013).
Sources: Data derived from DV3F and property tax files. Authors' calculation.

B Additional Empirical Results

B.1 Commuting Area Fixed Effects for Positive Demand Shock to Explain Unitary Housing Price with Homogeneous Housing Markets

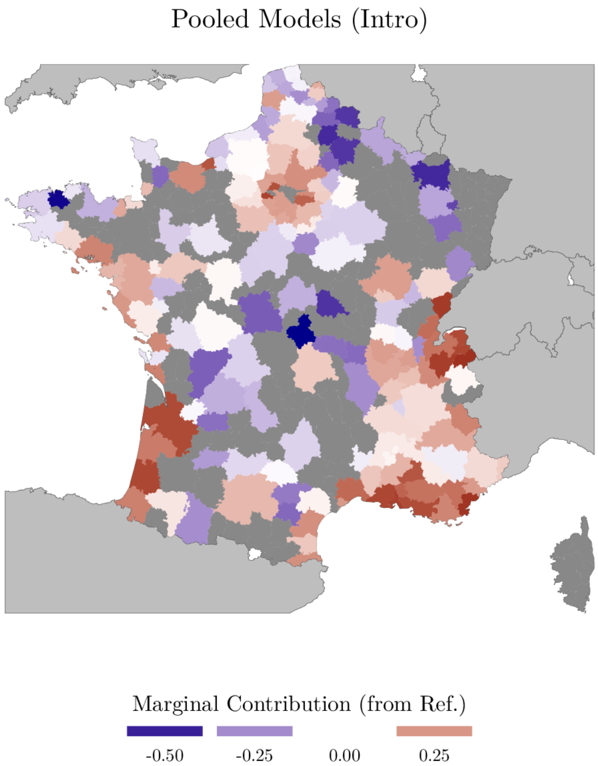

Figure B.1: Commuting Area Fixed Effects for Positive Demand Shock to Explain Unitary Housing Price

Notes: We report marginal contributions for commuting area fixed effects resulting from the estimation of Equation 3 for the positive demand shock experiment. Each map corresponds to the four estimations we make based on the *a priori* defined market segment. The model is estimated with OLS and standard errors are clustered to the commuting area.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

B.2 Time Fixed Effects for Positive Demand Shock to Explain Unitary Housing Price with Homogeneous Housing Markets

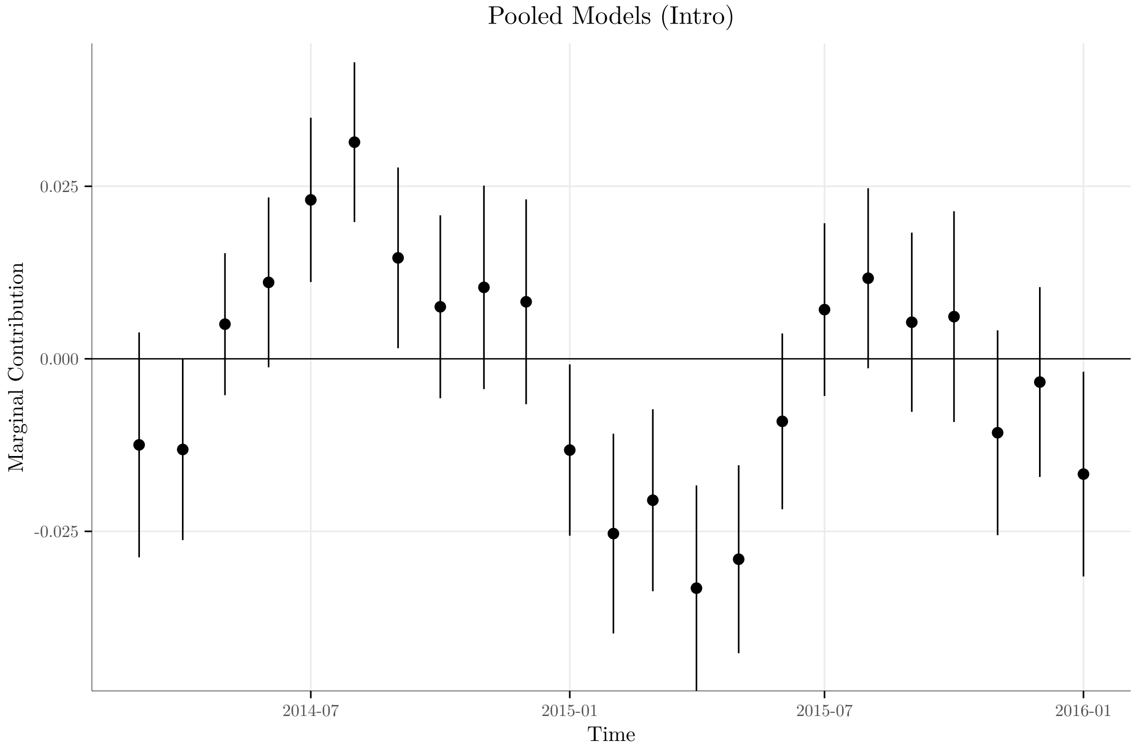

Figure B.2: Time Fixed Effects for Positive Demand Shock to Explain Unitary Housing Price

Notes: We report marginal contributions for time fixed effects resulting from the estimation of Equation 3 for the positive demand shock experiment. Each plot corresponds to the four estimations we make based on the *a priori* defined market segment. The model is estimated with OLS and standard errors are clustered to the commuting area. We report confidence intervals to 95% level. The model specification contains commuting areas fixed effects.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

B.3 Commuting Area Fixed Effects for Negative Demand Shock to Explain Unitary Housing Price with Homogeneous Housing Markets

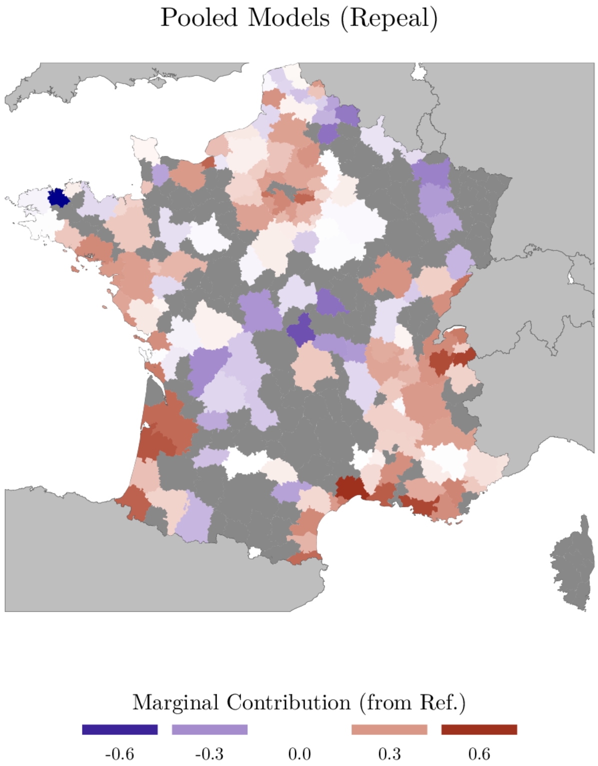

Figure B.3: Commuting Area Fixed Effects for Negative Demand Shock to Explain Unitary Housing Price

Notes: We report marginal contributions for commuting area fixed effects resulting from the estimation of Equation 3 for the negative demand shock experiment. Each map corresponds to the four estimations we make based on the *a priori* defined market segment. The model is estimated with OLS and standard errors are clustered to the commuting area.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

B.4 Time Fixed Effects for Negative Demand Shock to Explain Unitary Housing Price with Homogeneous Housing Markets

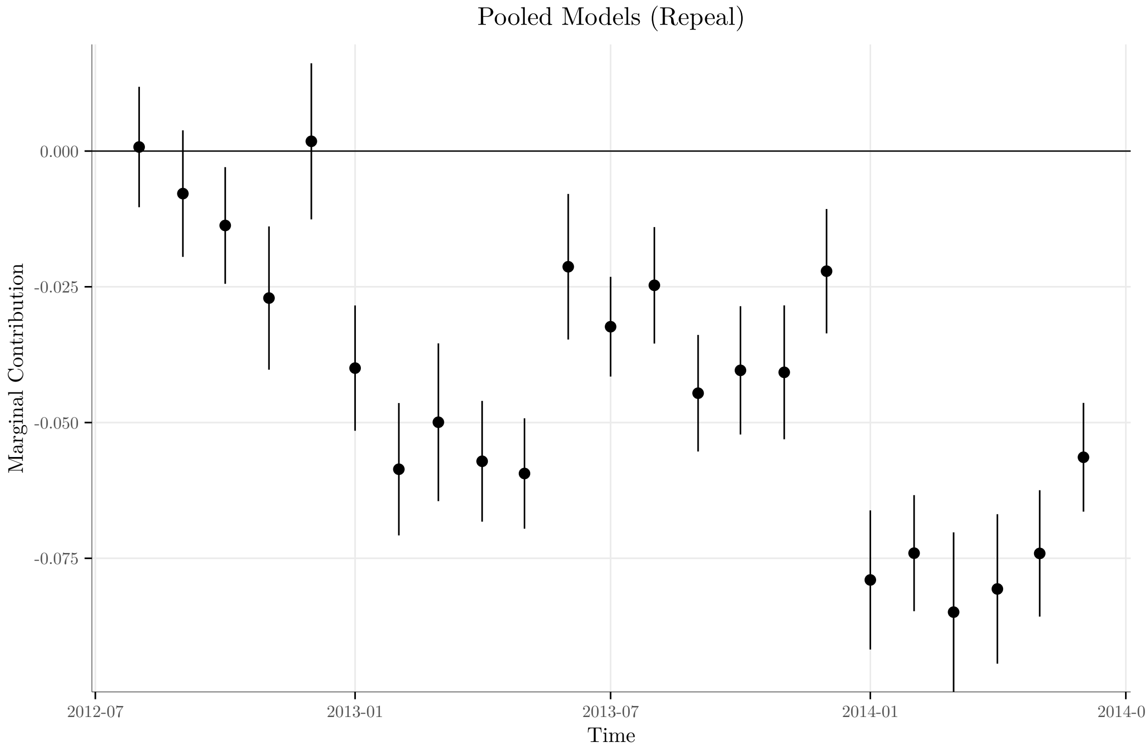

Figure B.4: Time Fixed Effects for Negative Demand Shock to Explain Unitary Housing Price

Notes: We report marginal contributions for time fixed effects resulting from the estimation of Equation 3 for the negative demand shock experiment. Each plot corresponds to the four estimations we make based on the *a priori* defined market segment. The model is estimated with OLS and standard errors are clustered to the commuting area. We report confidence intervals to 95% level. The model specification contains commuting areas fixed effects.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

B.5 Commuting Area Fixed Effects for Positive Demand Shock to Explain Unitary Housing Price with Heterogeneous Housing Markets

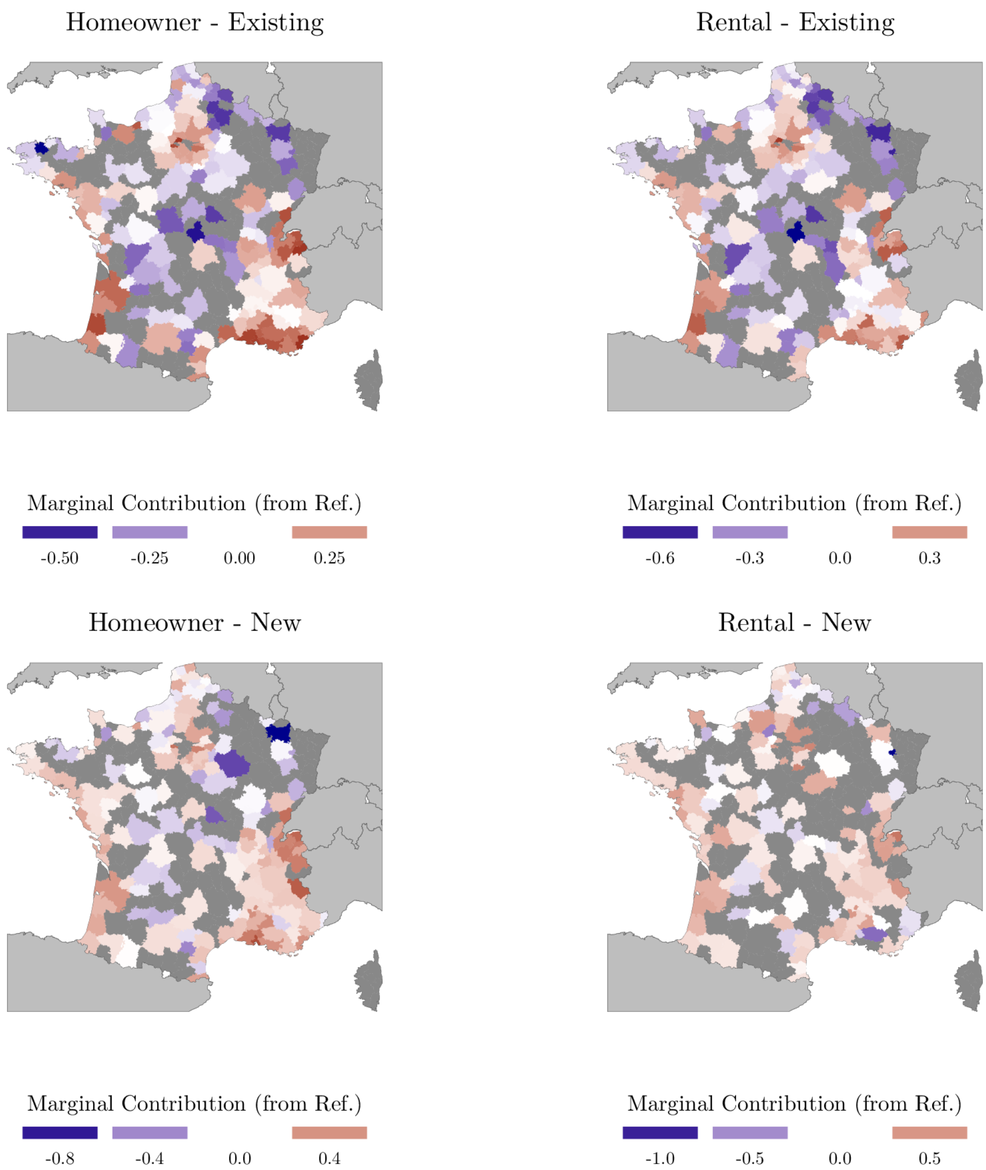

Figure B.5: Commuting Area Fixed Effects for Positive Demand Shock to Explain Unitary Housing Price

Notes: We report marginal contributions for commuting area fixed effects resulting from the estimation of Equation 3 for the positive demand shock experiment. Each map corresponds to the four estimations we make based on the *a priori* defined market segment. The model is estimated with OLS and standard errors are clustered to the commuting area.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

B.6 Time Fixed Effects for Positive Demand Shock to Explain Unitary Housing Price with Heterogeneous Housing Markets

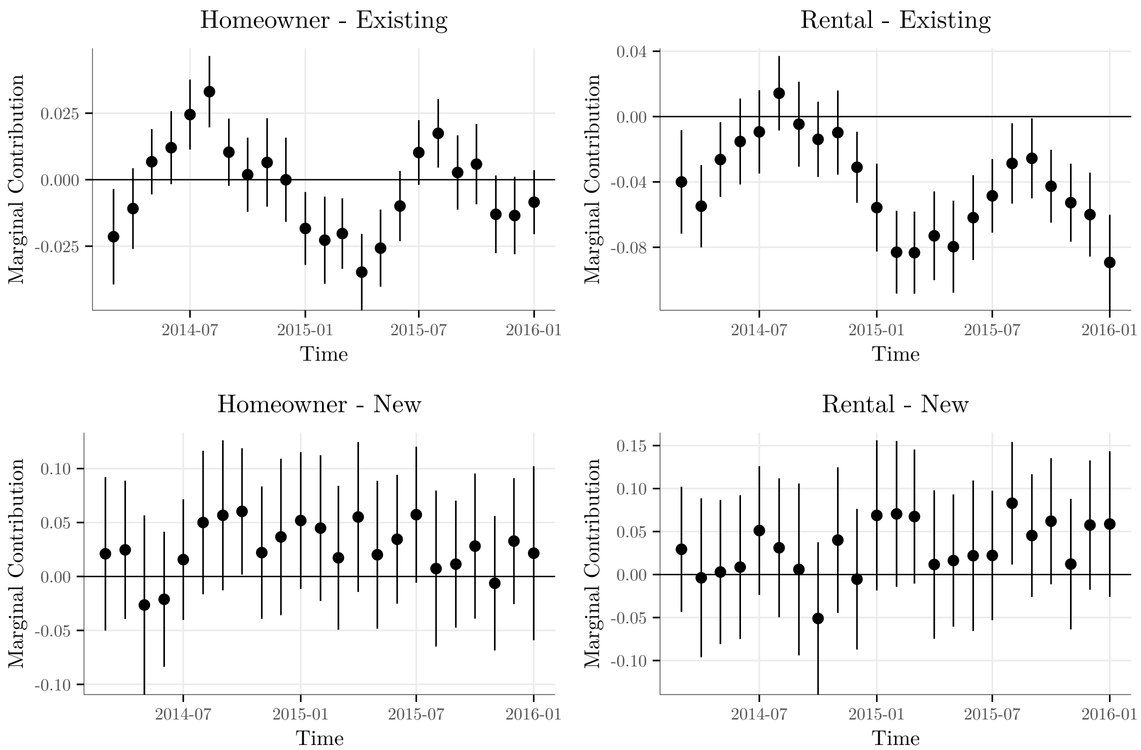

Figure B.6: Time Fixed Effects for Positive Demand Shock to Explain Unitary Housing Price

Notes: We report marginal contributions for time fixed effects resulting from the estimation of Equation 3 for the positive demand shock experiment. Each plot corresponds to the four estimations we make based on the *a priori* defined market segment. The model is estimated with OLS and standard errors are clustered to the commuting area. We report confidence intervals to 95% level. The model specification contains commuting areas fixed effects.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

B.7 Commuting Area Fixed Effects for Negative Demand Shock to Explain Unitary Housing Price with Heterogeneous Housing Markets

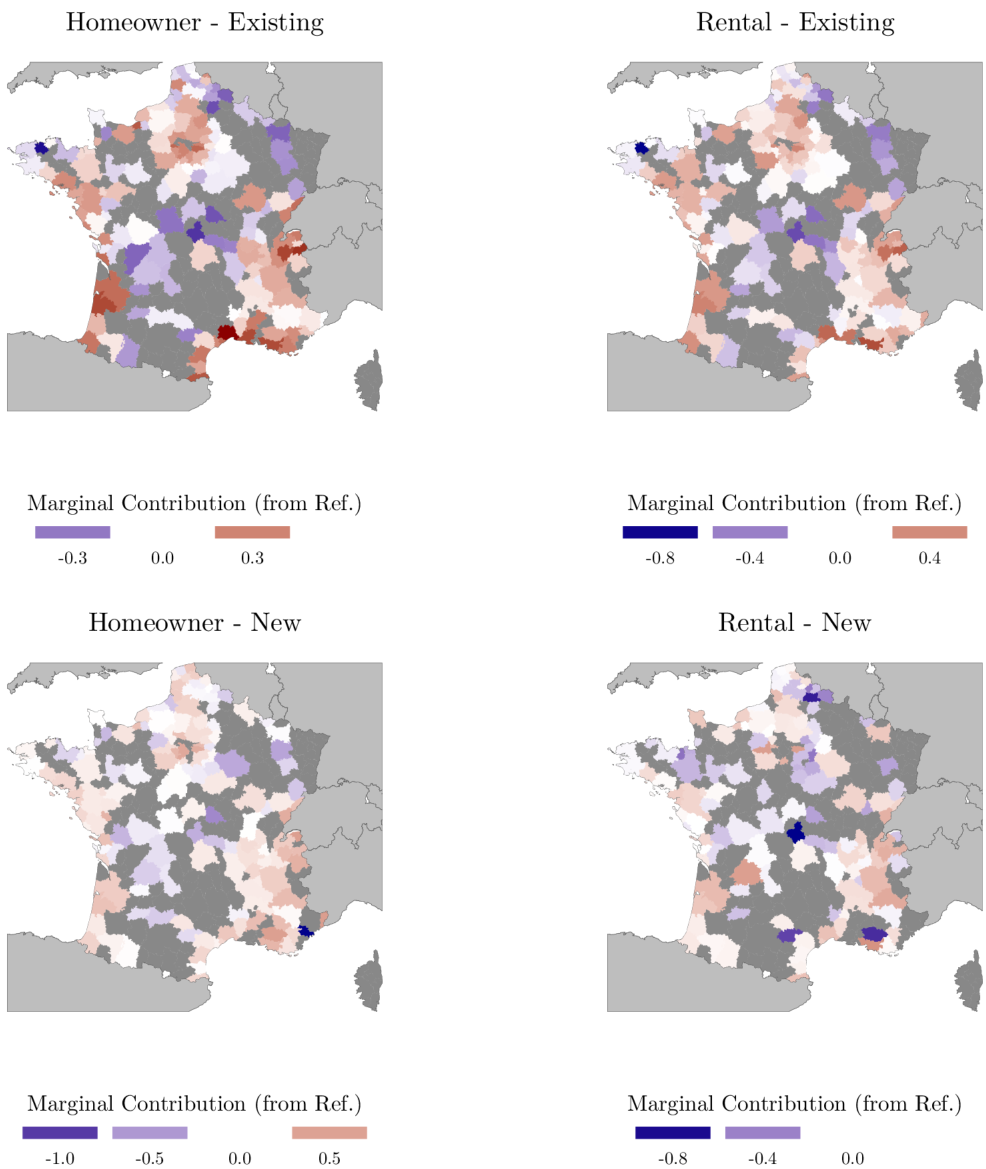

Figure B.7: Commuting Area Fixed Effects for Negative Demand Shock to Explain Unitary Housing Price

Notes: We report marginal contributions for commuting area fixed effects resulting from the estimation of Equation 3 for the negative demand shock experiment. Each map corresponds to the four estimations we make based on the *a priori* defined market segment. The model is estimated with OLS and standard errors are clustered to the commuting area.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

B.8 Time Fixed Effects for Positive Demand Shock to Explain Unitary Housing Price with Heterogeneous Housing Markets

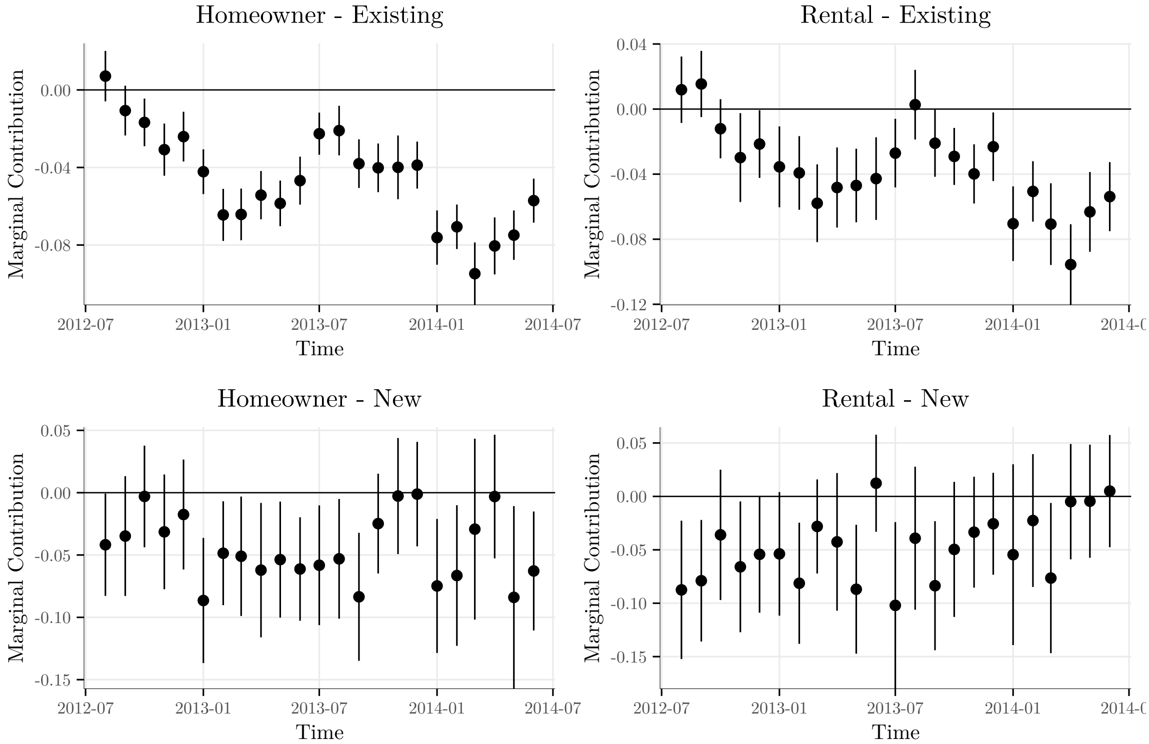

Figure B.8: Time Fixed Effects for Negative Demand Shock to Explain Unitary Housing Price

Notes: We report marginal contributions for time fixed effects resulting from the estimation of Equation 3 for the negative demand shock experiment. Each plot corresponds to the four estimations we make based on the *a priori* defined market segment. The model is estimated with OLS and standard errors are clustered to the commuting area. We report confidence intervals to 95% level. The model specification contains commuting areas fixed effects.
Sources: Data derived from DV3F and property tax files. Authors' calculation.

C Robustness Checks

C.1 Detailed Results for Time Range Varying for Policy Introduction Experiments

Table C.1: Treatment Effects According to Time Range From Policy Introduction

	Positive Shock				Negative Shock			
	Existing		New Construction		Existing		New Construction	
Time Range	Own.	Rent	Own.	Rent	Own.	Rent	Own.	Rent
6 months	0.012	0.084***	0.044	0.069	0.001	0.008	-0.004	-0.115***
	(0.02)	(0.03)	(0.05)	(0.06)	(0.01)	(0.01)	(0.03)	(0.04)
7 months	0.011	0.080***	0.050	0.068	0.004	0.007	0.008	-0.100**
	(0.01)	(0.02)	(0.04)	(0.06)	(0.01)	(0.01)	(0.03)	(0.04)
8 months	0.007	0.075***	0.028	0.060	0.002	0.011	0.012	-0.119***
	(0.01)	(0.02)	(0.04)	(0.06)	(0.01)	(0.01)	(0.03)	(0.04)
9 months	0.007	0.064***	0.015	0.107	0.003	0.005	0.005	-0.110**
	(0.01)	(0.02)	(0.04)	(0.07)	(0.01)	(0.01)	(0.03)	(0.04)
10 months	0.007	0.056***	0.035	0.132**	0.003	0.004	0.008	-0.135***
	(0.01)	(0.02)	(0.03)	(0.07)	(0.01)	(0.01)	(0.03)	(0.04)
11 months	0.008	0.046***	0.017	0.110*	0.005	0.001	-0.000	-0.123***
	(0.01)	(0.02)	(0.03)	(0.06)	(0.01)	(0.01)	(0.03)	(0.04)
12 months	0.007	0.033*	-0.006	0.091*	0.006	0.004	-0.005	-0.120***
	(0.01)	(0.02)	(0.03)	(0.05)	(0.01)	(0.01)	(0.03)	(0.04)
13 months	0.004	0.032*	-0.016	0.090*	0.005	0.006	-0.006	-0.113***
	(0.01)	(0.02)	(0.03)	(0.05)	(0.01)	(0.01)	(0.03)	(0.04)
14 months	0.003	0.028	-0.013	0.076*	0.004	0.005	-0.005	-0.110**
	(0.01)	(0.02)	(0.03)	(0.04)	(0.01)	(0.01)	(0.02)	(0.04)
15 months	0.002	0.020	0.003	0.077**	0.003	0.009	-0.011	-0.107**
	(0.01)	(0.02)	(0.03)	(0.04)	(0.01)	(0.01)	(0.02)	(0.04)

Notes: To do
Sources: Data derived from DV3F and property tax files. Authors' calculation.
*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

C.2 Main Results with Alternative Narrow / Long-Distance Purchaser (Distance < 5km)

Table C.2: Results from TWFE Regressions According to Demand Shock Nature, Housing Market Segments and Purchaser Category

	Dependent Variable: Unit. Housing Price (*log*)							
	Positive Shock				Negative Shock			
	Existing		New Construction		Existing		New Construction	
Covariates	Own.	Rent	Own.	Rent	Own.	Rent	Own.	Rent
Group (Ref. Never Treated)								
Group (Shift)	0.005	-0.012	0.175***	0.100	0.017	0.017	-0.099	-0.096*
	(0.024)	(0.043)	(0.056)	(0.068)	(0.052)	(0.059)	(0.060)	(0.053)
Group (Always)	-0.040*	-0.090***	0.178***	0.177***	0.005	-0.006	0.089	0.040
	(0.023)	(0.031)	(0.041)	(0.066)	(0.054)	(0.060)	(0.061)	(0.057)
Treatment	0.015	0.039**	-0.011	0.079	0.005	0.002	-0.002	-0.084*
	(0.010)	(0.019)	(0.030)	(0.051)	(0.007)	(0.010)	(0.031)	(0.043)
Purchaser Category (Ref. Local)								
Out Purchaser	0.058***	0.007	0.087***	0.084***	0.076***	0.025***	0.081***	0.087***
	(0.005)	(0.007)	(0.017)	(0.025)	(0.009)	(0.007)	(0.013)	(0.011)
SCI	-	0.010	-	0.049	-	0.035***	-	0.008
		(0.012)		(0.040)		(0.012)		(0.018)
Treatment × Out Purchaser	-0.003	-0.014	0.001	0.008	-0.015	-0.003	-0.033	-0.072**
	(0.012)	(0.010)	(0.026)	(0.031)	(0.010)	(0.010)	(0.028)	(0.029)
Treatment × SCI	-	-0.002	-	-0.019	-	-0.017	-	0.036
		(0.017)		(0.049)		(0.016)		(0.048)
R^2	0.248	0.264	0.274	0.392	0.244	0.248	0.278	0.328
Adj. R^2	0.247	0.261	0.259	0.377	0.243	0.246	0.269	0.317
Monthly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	208,929	53,815	9,929	6,892	229,723	68,860	14,423	10,428

Notes: I report results from the estimation through OLS of Equation 3 to explain unitary housing price. I relax homogeneous assumptions, and run independent regression according to nature of demand shock (columns 1 to 4 corresponds to positive demand shock, columns 5 to 8 corresponds to negative shock) and the housing market segment. Reference group is never treated. Standard errors are reported between parentheses, and clustered to commuting area using the `fixest` R package. Observation unit is transaction. For positive shock, I restrict transaction from June 2014 to Nov. 2015. For negative shock, I restrict transaction from Jan. 2013 to Dec. 2013. I do not report time fixed effects and commuting areas fixed effects for clarity reasons, but results are available on Figure B.6 and Figure B.8.
Sources: Data derived from DV3F and property tax files. Authors' calculation.
*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

C.3 Main Results with Alternative Narrow / Long-Distance Purchaser (Distance < 10km)

Table C.3: Results from TWFE Regressions According to Demand Shock Nature, Housing Market Segments and Purchaser Category

	Dependent Variable: Unit. Housing Price (*log*)							
	Positive Shock				Negative Shock			
	Existing		New Construction		Existing		New Construction	
Covariates	Own.	Rent	Own.	Rent	Own.	Rent	Own.	Rent
Group (Ref. Never Treated)								
Group (Shift)	0.002	-0.012	0.167***	0.094	0.021	0.018	-0.095	-0.096*
	(0.025)	(0.043)	(0.053)	(0.069)	(0.052)	(0.059)	(0.058)	(0.054)
Group (Always)	-0.043*	-0.090***	0.171***	0.172**	0.003	-0.006	0.092	0.037
	(0.023)	(0.031)	(0.040)	(0.068)	(0.055)	(0.060)	(0.059)	(0.058)
Treatment	0.011	0.032*	-0.016	0.090*	0.004	0.004	-0.002	-0.116***
	(0.011)	(0.019)	(0.028)	(0.053)	(0.006)	(0.009)	(0.028)	(0.042)
Purchaser Category (Ref. Local)								
Out Purchaser	0.054***	0.005	0.090***	0.087***	0.078***	0.027***	0.101***	0.073***
	(0.007)	(0.007)	(0.019)	(0.026)	(0.011)	(0.009)	(0.012)	(0.009)
SCI	-	0.008	-	0.034	-	0.033**	-	-0.010
		(0.012)		(0.040)		(0.013)		(0.017)
Treatment × Out Purchaser	0.005	-0.000	0.024	-0.006	-0.018	-0.009	-0.041	-0.021
	(0.017)	(0.011)	(0.029)	(0.033)	(0.011)	(0.010)	(0.029)	(0.031)
Treatment × SCI	-	0.006	-	-0.022	-	-0.020	-	0.066
		(0.017)		(0.049)		(0.016)		(0.052)
R^2	0.247	0.264	0.277	0.392	0.244	0.248	0.283	0.327
Adj. R^2	0.246	0.261	0.263	0.377	0.243	0.246	0.274	0.316
Monthly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	208,929	53,815	9,929	6,892	229,723	68,860	14,423	10,428

Notes: I report results from the estimation through OLS of Equation 3 to explain unitary housing price. I relax homogeneous assumptions, and run independent regression according to nature of demand shock (columns 1 to 4 corresponds to positive demand shock, columns 5 to 8 corresponds to negative shock) and the housing market segment. Reference group is never treated. Standard errors are reported between parentheses, and clustered to commuting area using the `fixest` R package. Observation unit is transaction. For positive shock, I restrict transaction from June 2014 to Nov. 2015. For negative shock, I restrict transaction from Jan. 2013 to Dec. 2013. I do not report time fixed effects and commuting areas fixed effects for clarity reasons, but results are available on Figure B.6 and Figure B.8.
Sources: Data derived from DV3F and property tax files. Authors' calculation.
*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

C.4 Main Results with Alternative Narrow / Long-Distance Purchaser (Distance < 20km)

Table C.4: Results from TWFE Regressions According to Demand Shock Nature, Housing Market Segments and Purchaser Category

	Dependent Variable: Unit. Housing Price (*log*)							
	Positive Shock				Negative Shock			
	Existing		New Construction		Existing		New Construction	
Covariates	Own.	Rent	Own.	Rent	Own.	Rent	Own.	Rent
Group (Ref. Never Treated)								
Group (Shift)	-0.001	-0.012	0.162***	0.093	0.020	0.017	-0.093	-0.093*
	(0.025)	(0.043)	(0.052)	(0.071)	(0.052)	(0.059)	(0.058)	(0.054)
Group (Always)	-0.044*	-0.091***	0.168***	0.168**	-0.000	-0.008	0.090	0.039
	(0.023)	(0.030)	(0.041)	(0.068)	(0.055)	(0.060)	(0.059)	(0.058)
Treatment	0.007	0.031	-0.006	0.091*	0.005	0.005	-0.006	-0.118***
	(0.012)	(0.019)	(0.029)	(0.051)	(0.006)	(0.009)	(0.028)	(0.040)
Purchaser Category (Ref. Local)								
Out Purchaser	0.066***	0.017**	0.127***	0.075***	0.098***	0.042***	0.122***	0.064***
	(0.010)	(0.008)	(0.022)	(0.026)	(0.012)	(0.010)	(0.014)	(0.009)
SCI	-	0.011	-	0.019	-	0.034***	-	-0.023
		(0.012)		(0.040)		(0.012)		(0.017)
Treatment × Out Purchaser	0.018	0.003	-0.002	0.001	-0.030**	-0.015	-0.037	-0.034
	(0.021)	(0.012)	(0.030)	(0.033)	(0.014)	(0.012)	(0.036)	(0.040)
Treatment × SCI	-	0.007	-	-0.018	-	-0.021	-	0.067
		(0.017)		(0.049)		(0.016)		(0.052)
R^2	0.249	0.264	0.282	0.390	0.246	0.248	0.288	0.324
Adj. R^2	0.248	0.261	0.268	0.375	0.245	0.246	0.279	0.313
Monthly FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	208,929	53,815	9,929	6,892	229,723	68,860	14,423	10,428

Notes: I report results from the estimation through OLS of Equation 3 to explain unitary housing price. I relax homogeneous assumptions, and run independent regression according to nature of demand shock (columns 1 to 4 corresponds to positive demand shock, columns 5 to 8 corresponds to negative shock) and the housing market segment. Reference group is never treated. Standard errors are reported between parentheses, and clustered to commuting area using the `fixest` R package. Observation unit is transaction. For positive shock, I restrict transaction from June 2014 to Nov. 2015. For negative shock, I restrict transaction from Jan. 2013 to Dec. 2013. I do not report time fixed effects and commuting areas fixed effects for clarity reasons, but results are available on Figure B.6 and Figure B.8.
Sources: Data derived from DV3F and property tax files. Authors' calculation.
*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$