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Housing Market Responses to Transfer Taxes: Evidence from a French Reform

Mathilde POULHES[1]

[1] CREST, SDES (Ministry of Housing), Insee and SciencesPo (Paris)

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Mathilde Poulhes†

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Abstract

This paper estimates the impact of the rise in the housing transfer tax (the *régime de droit commun*) in France in 2014. It exploits both time and geographical discontinuities in the implementation of this reform that gave the right to local authorities to raise their housing transfer tax, an entitlement that most *départements* have chosen to exercise. In the short term, I provide evidence that buyers anticipated the reform to avoid the additional tax burden. I then use an event-study design to examine whether there was any lock-in effect in the volume of dwelling sales. I show there is evidence of a long-term negative effect of the tax increase on the number of transactions, but only in markets where supply was high relative to demand. Finally, I find no effect on pre-tax sale prices, meaning that the burden of the transfer tax rests now on the buyer. My findings highlight the price rigidity of the French housing market and suggest that lowering housing transfer taxes could be used as a fiscal stimulus in the short term.

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†CREST, SDES (Ministry of Housing), Insee and SciencesPo (Paris)

1 Introduction

The influence of transaction taxes on markets is an important issue in the tax policy debate. Transaction taxes refer to the taxes placed on legal documents, usually in the transfer of assets or property. Besides housing markets, financial markets are also subject to transaction taxes. From the stamp duty implemented in 1694 at the London Stock Exchange to the recent Chinese proposal to apply a Tobin tax in March 2016, financial transaction taxes have been applied in many countries to different types of transactions. The goal has always been to discourage excessive speculation, and to stabilize markets without discouraging any other activity. On the other hand, transaction taxes on housing markets have never been introduced to achieve a market goal. They stem from a time when few other potential taxes were as straightforward to implement and have always been a common means of raising revenues. Until the time of the French revolution, there had been different transaction taxes both at local and national levels. The first unified nationwide stamp duty on real estate was introduced only in 1790[1]. In the modern era of broadly based taxation, stamp duties have been maintained despite the generated inefficiencies highlighted by most economists. Many papers show that transaction taxes contribute to illiquidity of the real estate market (see Grossman and Laroque [1990] or Ioannides and Kan [1996]). More generally, the economic literature rather highlights the negative impact of transaction taxes on household mobility. To my knowledge, only one paper, Chen [2017], analyses the link between housing transfer taxes and housing price volatility and finds no effect of higher transfer tax rates on price volatility in the different states of the US.

However, for tax collectors, housing transfer taxes remain above all a reliable source of revenue. In view of this, the French government decided in 2014 to allow departments[2] to raise the rate of their housing transfer tax. This reform was implemented in order to increase the tax revenue of local authorities in a context of decreasing State funding without any *ex ante* analysis having been done beforehand to measure the possible impacts on the real estate market. However, economic theory predicts that an increase of transaction costs should decrease the number of sales and that this additional cost should be shared between buyers and sellers. As a consequence, this should impact negatively the pre-tax sale prices. Both of these effects could lead to an adverse negative effect on local tax revenue.

The short term effect of changes in housing transfer taxes has been little studied and historically, only a few countries have lowered their transfer tax rates to stimulate their real estate market. One exception is the UK which decided in 2008 to temporarily reduce its tax rate on certain properties which resulted in a corresponding increase in property transactions of around 8% (see Besley et al. [2014]). In case of anticipated changes (which was the case in 2014 for the French reform), the behavioral response of buyers can indeed be significant. The crucial question to be asked in the assessment of the effectiveness of reducing transfer taxes as a fiscal stimulus in France is whether buyers and sellers adjust the date of sale or adjust the sale price.

The aim of this paper is therefore to estimate both the extensive margins (whether or not to buy a dwelling) and the timing responses (when to buy). More precisely, I evaluate the impact of the

[1] In 1694 for the British tax system.

[2] Hereafter, I use the English term of department for the French *département*. It is the intermediary administrative unit to which some of the State's power is delegated. Notably, departments are allowed to raise taxes. In 2014, the 36,681 municipalities were grouped into 101 departments.

2014 tax reform on volumes and prices, as well as on tax revenues, in both the short- and the long-term. This tax reform gave departments the right to raise their transfer tax from 5.09% to 5.81% and to fix the date of implementation. These tax increases were announced by the local authorities well before their effective implementation. Considering the differences in the implementation dates between departments, I exploit time and geographical discontinuities to construct a convincing identification strategy using a linear panel event-study design. As tax incidence theoretically depends on market elasticities, I distinguish between the levels of tightness in real estate markets.

I obtained during the course of my research a unique dataset recording all the property transactions in France between 2012 and 2017 from French notaries. Each transaction is geocoded and many dwelling characteristics are provided. First, using this database, I counted the number of transactions by month and by municipality and ran a separate event study for tight and less tight markets. Second, to control for local shocks in the real estate market, I selected municipalities close to a border of identification (*i.e.* a border that separates a department that raised its tax rate in 2014 from one that did not). The results of these two methods are close: the market behavior in terms of the number of transactions in anticipation of the reform was very significant and a lock-in effect over the long term was only observed in less tight markets. For price analysis, I ran hedonic regressions on transactions directly. I find very limited short-term effects and no long-term effects on both tight and less tight markets. The very precise dataset and the design of the reform allowed me to properly estimate the causal impact of the reform on volumes and on prices. Moreover, to my knowledge, this is the first paper to analyze the heterogeneity of the impact of a change in real estate transfer taxes according to market tightness. My findings confirm the importance of this distinction.

With these results, I contribute to the growing literature on real estate transaction costs. Theoretically, it has been shown that an increase of real estate transaction costs should decrease mobility (see Mirrlees [2011]). Indeed, if transaction costs are higher than utility gains from mobility, households do not adjust the quantity of housing services they consume. Therefore, households may accept living in inadequate housing compared to what they would choose in a world without markets frictions. On the other hand, economic theory on the influence of transfer taxes on housing prices demonstrates the particular importance of the market conditions and the relative market powers of buyers and sellers. For example, Slemrod et al. [2016] or Besley et al. [2014] use a Nash bargaining model with perfect information to show that the transfer tax is paid by sellers and buyers proportionally to their bargaining power.

Empirically, some papers explore the impact of housing transfer taxes on the volume of transactions. Ommeren and Leuvensteijn [2005] assess the impact of a rise of transaction costs in the Netherlands. By using duration models, they find that a 1% increase of these costs implies a 8% decrease in household mobility. Fritzsche and Vandrei [2016] exploit the fact that the German tax rates have been regionally differentiated over ten years. They look at single-family home transactions and their estimates suggest that a one-percentage-point increase in the transfer tax rates reduces transactions by 6%.

Other papers focus on prices. Benjamin et al. [1993] find that the rise of housing transfer taxes from 3.5% to 5% in Philadelphia in May 1988 caused a decrease of pre-tax prices by an amount equal to the tax increase in the short term.

Most papers dealing with transfer taxes are interested in both prices and volumes. Dachis et al. [2012] exploit the 2008 housing transfer tax reform in Toronto to estimate with a difference-in-differences method that a 1.1% increase of the housing transfer tax caused both a 15% decrease in the number of transactions and a decrease in the pre-tax prices equal to the amount of the tax. Davidoff and Leigh [2013] observe negative impacts of stamp duty on housing turnover in the Australian real estate market. Instrumenting the endogenous real estate transfer tax rates, they estimate that a rise of transfer taxes lowers housing prices, suggesting that economic incidence falls on the seller. Petkova and Weichenrieder [2017] distinguish between single family houses and apartments in Germany. They find an elasticity of the volume of transactions of -0.23, but no significant effect on prices for houses. For apartments, they find no significant effects on the transaction volume, but the price effect tends to be negative.

The market seems to react as well to a decrease in transfer tax rates. Besley et al. [2014] exploit the 2008-2009 stamp duty holiday in the UK to estimate the incidence of a transaction tax on housing. The increase in transactions of properties affected by the tax holiday was found to be around 8%, but only in the short term. The estimates are used as inputs of a simple bargaining model and show that about sixty percent of the surplus generated by the tax holiday accrued to buyers.

Other papers use discontinuities in the tax liability rather than changes in the tax rate to analyse the impact of transfer taxes on the real estate market. Using the same reform as Besley et al. [2014], Best and Kleven [2016] exploit the notches due to the progressive real estate transfer taxes. They conclude that the tax creates some distortion of the housing market across marginal tax rates. In the US, Kopczuk and Munroe [2015] estimate the effect of the "mansion tax" in New-York that concerns only the high value properties (above $1 million). This tax imposes an additional 1% tax on the full value of the transaction. This policy generates a discontinuity (a notch) in the overall tax liability. They show that this tax impacts negatively on the market near the notch, and is inefficient in terms of tax revenue.

Finally, Slemrod et al. [2016] use a reform that creates both a time and a price discontinuity for identification. They find no evidence of a lock-in effect, but they observe a slight timing effect, which corresponds to an anticipation effect to avoid the tax increase. They conclude that buyers and sellers are more able to adjust the sale price in response to the tax increase, than to anticipate the sale date.

To my knowledge Berard and Trannoy [2016] is the only paper focusing on the French case. They refer to the same 2014 reform using data on transfer tax revenue aggregated at the departmental level, which prevent them from disentangling the effect on prices from the effect on volumes. By exploiting the invaluable database mentioned above, my paper effectively does so.

It proceeds as follows. Section 2 describes the institutional context and the data. Section 3 presents the empirical method. Section 4 reports the results. Section 5 discusses the theoretical mechanisms and the heterogeneous effects and Section 6 concludes.

2 Institutional background and data

2.1 Housing transfer tax in France

In France, the housing transfer tax is collected by the notary on behalf of the Treasury. This tax is paid by the buyer in full and is imposed according to the transaction value of land and of any construction on that land, and is proportional to its sale price. These elements are by nature on the public record and cannot be altered. Consequently, it is impossible to cheat on the imposable amount of tax or on the date of transaction.

Three tax regimes currently exist that determine the applicable rate. The standard regime (or *régime de droit commun*) applies to the flats, houses and lands not subject to the payment of value added tax (VAT). These cases include older properties (more than 5 years), new constructions (less than 5 years) if sold privately (except if the seller bought before completion), as well as lands if sold privately. The total amount paid is shared out between three parties, namely the department (the rate before the reform of 2014 was 3.80%), the local municipalities (or *communes* with a rate of 1.20%), and the state (0.09%). From 1^{st} March 2014, the departments were given the right to increase their rate from 3.80% to 4.50%, bringing the total rate from 5.09% to 5.80% of the sale price. The second tax regime is called the derogation system (or *régime dérogatoire*). This applies to new constructions or lands sold by property developers, to new constructions (less than 5 years) sold privately but which were bought before completion, as well as to property donations. The applicable rate was not affected by the 2014 tax reform and represents a lower rate than that of the standard regime. The third regime applies to the tax exemption status of acquisitions made by the State or by local authorities.

In addition to the housing transfer tax mentioned above, buyers must also pay the notary fees (about 1% of the sale price), and sundry administrative charges which are of the same order of magnitude. These mandatory transaction costs represent around 8% of the sale price. They must be paid in full once the bill of sale has been signed but before the payment of the property itself. For this reason, in almost all cases, these transaction costs aren't in practise financed through mortgages. Hence, as it will be demonstrated in this paper, potential buyers are highly sensitive to any changes in the transfer tax rates, which do have a significant impact on their purchasing decisions.

When a property agent arranges the sale, buyers also have to pay agency costs or negotiation fees. Transaction fees, both mandatory and non-mandatory, represent on average 12% of the sale price. This figure is significantly higher than that of most OECD countries (see Andrews et al. [2011][3]); for instance, the average German rate is equal to 8% and the American is equal to 5%.

2.2 Reform

The 2014 housing transfer tax reform took place in a context of decreasing grants for, and higher social spending by, the departments. During the 2000s, the average annual transfer tax revenue for all the departments added up to approximately 9 billion euros (almost 13% of their annual total budget, source: DGFiP). After a period of strong growth between 2000 and 2007 due to a dynamic real estate market, the revenues generated by the transfer taxes suffered subsequently from the collapse of

[3]However, the average rate of 14.2%,for fees paid in France found by these authors, seems overestimated, according to the figures.

property prices and the drop in the number of transactions following the 2007 financial crash. Another factor that negatively impacted on departmental budgets was the 2013 decision to reduce the transfer amounts from central to local authorities, by 1.5 billion euros. To compensate for this decrease, the government allowed the departments to temporarily raise their rate of housing transfer tax from 3.8% to 4.5% between 1 March 2014 and 29 February 2016. This reform was written into law in the 2014 Finance Bill, presented in September 2013 and voted on 19 November 2013.

In November 2015, with a view to helping departments balance their budgets, the government announced a time extension. The 2016 Finance Bill subsequently fixed the tax rate at 4.5% to be applied by all departments from 1 March 2016, unless they notified the fiscal administration that a lower rate should be applied.

Indeed between March 2014 and January 2016, 93 of the 96 departments of metropolitan France took advantage of the increased tax rate (in 2014, 60 departments raised their rates on 1 March, 19 on 1 April, 2 on 1 May, and 7 on 1 June. Three departments raised their rate on 1 January 2015, and two more on 1 January 2016). To date[4], only three departments in metropolitan France have chosen not to raise their rates: Indre, Isère and Morbihan (see Figure 1).

The authorisation given to the departments to raise their rates was a budgetary measure intended to help departments in a context of a sharp decrease in their funding. Like Berard and Trannoy [2016], I argue that the corresponding decision was almost a random one. In the next section, some descriptive statistics will show that the different treatment[5] groups (7 treatment groups corresponding to the 6 different dates of implementation and one group of "non-treated") have comparable real estate markets. Table 1 shows that these treatment groups are quite similar in terms of many dimensions as well: unemployment rates, poverty rates, average income, etc. Some differences exist in terms of population and housing stock variables because some departments like Paris (treated in January) are very particular. However the only variable that seems relevant in explaining the date of treatment is their debt ratios in 2014; the departments with lower debt ratios raised their tax rates later or have yet to do so.

Other local taxes have changed substantially during this period. Therefore I control for the annual property tax rate at the department and at the municipality levels and for annual housing tax rate at the municipality level (the departmental share of housing tax was removed in 2011).

2.3 Data

I focus on the flats and houses subject to the standard tax regime (see 2.1 for details). The data source is the *Perval* and *Bien* databases[6] produced by French notaries. For each transaction, the database provides information on the date of transfer, sale price, address, surface area, number and type of room, date of construction and other characteristics of the property. This database is available for the years 2000 to 2016. It covers around 60% of real estate transactions. The coverage rate of this database varies by departments, but is not correlated with the individual department's decision to

[4] updated in June 2020

[5] The treatment refers here and herafter to the rise of the transfer tax rate to 4.5%.

[6] The access to these data is governed by agreements between the Ministry of Housing (SDES), Paris Notaires and Min. Not.

raise their housing transfer tax rate or not. In the language of public policy evaluation, the coverage rate is independent of the treatment (see Appendix A).

I use the zoning A/B/C of the Ministry of Housing (Figure 2) to determine whether the transaction is located in an area where the housing market is tight or not. This zoning identifies the real estate markets where housing supply is insufficient to cover demand. It divides the French territory into 5 types of zones from the tightest (zone A bis) to the least tight (zone C). The matching with the *Perval* and *Bien* databases is made at the municipality level.

I use the REI (*Recensement des Éléments d'Imposition*) databases from the fiscal administration (DGFiP) to control for the changes in local tax rates for the property tax on built-up properties and for the housing tax, see Appendix B for descriptive statistics on this database.

2.4 Sample definition and descriptive statistics

For the purposes of this study, I restrict the sample to the period between January 2013 and December 2016. The reform took place during 2014 for most departments so this choice yields a sufficient number of months before and after the tax changes, which is necessary in order to set up the event study design with a window running from 12 months prior to 12 months after the reform. In the sample, I select all the municipalities of mainland France (excluding Corsica). Most municipalities are located in zone C (least tight markets) and are treated in March 2014 (Table 2). However the date of the tax rise does not seem to be linked with the zoning; for every treatment date, some municipalities are located in tight markets while others are located in zone C. Nevertheless, because the different groups of municipalities could have different turnover rates, I include municipality fixed effects in the regressions.

To construct the estimation sample, I select all the transactions between January 2013 and December 2016 located in the selected municipalities (Table 3). The average price per square meter in the municipalities treated in May 2014 is slightly lower than the average price per square meter in the other groups. On the contrary, the average surface area is higher. This is due to a high proportion of Zone C municipalities (Table 2). On the contrary, the average price per square meter in the municipalities treated in January 2016 is 6,500 euros, which is around three times the average price in the other groups. Only two departments raised their rate at this date, Paris (in the tightest zone Abis) and Mayenne (mostly in zone C). The high average price per square meter is driven by the large number of transactions in Paris where prices are extremely high. More generally, Table 2 and Table 3 show that the number of transactions varies strongly from one municipality to another, that strengthens the case for introducing municipality fixed effects.

Moreover, turnover rates and price levels differ between zones. The housing market in zone C is likely to behave differently from housing markets in tighter zones. In the analysis, I run different regressions for tight and less tight markets.

The effect of the tax rise is estimated by municipality. It applies to rural municipalities with few transactions as well as big cities with thousands of transactions by month. Thus, the global effect of the reform on the tax revenue depends on the number of transactions and on the price levels in each municipality. This cannot be directly assessed by the estimated coefficients. I compute the global impact of the reform in section 5.2.

The date of the tax increase was announced well before the implementation of the reform, allowing

buyers to anticipate their purchase. If buyers are able to change their purchasing dates, an increase in the number of transactions can be expected just before the date of the reform and a subsequent negative impact immediately after the reform. To estimate these short-term effects and measure whether buyers and sellers adjust the date of sale or adjust the sale price, I always add six dummy variables in the regressions indicating that the reform took place one, two or three months before or after the transaction. This anticipation behavior may have indeed begun as early as one month after the announcement due to the legal delay of property sales[7].

3 Empirical method

3.1 Research design and identification

I use different empirical methods to estimate the causal effect of the tax rise. The first method is an event study on all French municipalities. It takes advantage of differences in the timing chosen by local authorities in the policy implementation. The treatment effect is identified by the differentiated evolution of volumes and prices across departments.

To assess the impact on volumes, I construct a panel of municipalities that counts the number of transactions in each municipality every month. I include in the analysis the municipalities of mainland France (excluding Corsica). The number of transactions in each municipality is normalized by the average monthly number of sales in the same municipality in 2012 (before the estimation period). Consequently I exclude from the sample the municipalities where no transaction was registered during the normalization period (2012). This normalization allows me to directly interpret the estimated coefficients in terms of percentage change.

I use this month (t)-municipality (j) panel to run an event study with time (ξ_t) and zone (δ_j) fixed effects, which formally reads:

$$n_{j,t} = \delta_j + \xi_t + \sum_{-K \leq k \leq K} \chi_k D^k_{j,t} + \epsilon_{j,t} \tag{1}$$

The variables of interest are a set of dummies $D^k_{j,t}$ indicating the tax rise happening k periods away. $D^0_{j,t} = 1$ the month of the tax rise in the municipality j. I set a baseline event window for the graphs running from 12 months prior to the tax change to 12 months after[8]. This corresponds to the equation (1) with K equal to 12. To estimate a pure effect of the tax rise (purged of the short-term effects), I run an event study with $K = 3$. In this case, I interpret the end point estimate χ_3 (corresponding to $k \geq 3$) as the long-term impact of the tax rise.

To estimate the impact on sale prices, I use all the transactions (i) located in the selected municipalities and run an event study with hedonic controls and month (ξ_t) and municipality (δ_j) fixed effects, which formally reads:

[7] The period between the preliminary sale agreement and the actual of sale cannot be legally less than one month due to the legal period of withdrawal. In practice, the time necessary for the buyer to raise the funds or take out a loan, and the time to complete administrative formalities is on average 3 months.

[8] As commonly done, I bin up event dummies at the endpoints of the event window (for $k \geq 13$ and $k \leq 13$). I do not plot these end point estimates in the event study graphs.

$$\log(p_{i,j,t}) = \delta_j + \xi_t + \sum_{-K \leq k \leq K} \chi_k D^k_{j,t} + \sum_{l=1}^{L} \alpha_l z_{lit} + \nu_{i,t} \quad (2)$$

The interest variable is the logarithm of the pre-tax price ($p_{i,t}$). In most of the specifications, I include the characteristics of the dwelling (z_{lit}) in the equation to control for potential variations of the dwelling quality (in practice, z includes the logarithm of the surface area (interacted with the type of dwelling), a dummy for the type of dwelling (flat or house), for the tenure status (under a lease), for the number of rooms, for the number of bathrooms and for the period of construction. Again, I choose $K = 12$ for the event study graph and $K = 3$ for the estimation of the long-term impact of the tax rise (identified by χ_3 which corresponds to $k \geq 3$).

3.1.1 Robustness

Real estate markets are sensitive to local shocks. The research design would be invalid if these shocks were concomitant with the tax rise. To test the robustness of the results, I estimate the same equations, (1) and (2), on a new sample of municipalities. I select the municipalities that are located near the borders between the departments that have raised their rates in 2014 and those that have not yet done so or have raised their rates since 2014 (hereafter sample 2 in opposition to sample 1 that includes all the municipalities). This research design is based on the assumption that the real estate markets distant of 40 km at the most are exposed to common shocks. The identification directly comes from the comparison of the evolution of treated municipalities with "close" untreated municipalities. This sensitivity check is therefore an event study combined with a regression discontinuity design on departments borders. It takes advantage of both regional discontinuities in the policy implementation as well as differences in the timing chosen by local authorities.

But potential buyers of houses in treated departments close to the boundary could choose to buy in contiguous non-treated departments. This effect of the treatment on non-treated zones, or spillover effect, could lead to an over-estimation of the effect. Thus, I restrict the observations to the areas located within 15 km on each side of these regional boundaries, excluding the 5 km strip on either side (see Figure 3). I include in the analysis all the municipalities whose centroid is located within the strips (5 to 15 km) on both sides of the boundaries of the 8 departments not treated in 2014.

To estimate the impact on the sale prices, I use all the transactions located in the selected municipalities and estimate an event study with hedonic controls.

3.1.2 Pre-trend tests

Identification of causal effects in such models requires common trends in the absence of treatment. The common trend assumption is impossible to verify but I use pre-treatment data to show that before the reform the trends in volumes and in prices are common for treated and non-treated municipalities. These common trends appear in the event study graphs but I provide some additional tests.

I report in appendix C the pre-trend tests for volumes and prices. I select a sample of observations before October 2013, *i.e.* before any discussion of the reform in the National Assembly. Placebo dummies are equal to 1 for a given month in the municipalities treated in March 2014.

Table 10 reports the results of the tests of the common trend assumption on the volumes of transactions for 2013. In the first three columns I include all the municipalities (sample 1), in the last three columns I only include the municipalities within the strips between 5 and 15 km on either side of the selected borders (sample 2).

For sample 1, the placebo controls show nothing statistically significant. The F-tests of the joint significance of the coefficients have a p-value greater than 40%. For sample 2, estimated coefficients are of greater magnitude but never significant at 1%, except the coefficient for July 2013 which is significant at 5%. This placebo effect may reveal some reactions to the early rumors of reform. Other coefficients are insignificant. The F-test of the joint significance of coefficients (except July 2013) has a p-value of 34% in line with the common trend assumption.

Table 11 reports the results of the tests of the common trend assumption on prices for 2013. In the first three columns I include all transactions (sample 1), in the last three columns I only include the transactions located in municipalities within the strips between 5 and 15 km on either side of the selected borders (sample 2). Except for the March 2013 dummy for the sample 2 in tight markets in sample 2, all coefficients are close to zero and insignificant. The p-values of F tests of joint significance are greater than 38% for sample 1 indicating parallel trends between treatment and control groups.

3.1.3 Inference

In my baseline approach, I cluster standard errors at the municipal level which is the level of identification. Given the well-known problem of biased standard errors in difference-in-differences models (see Bertrand et al. [2004]), I test the sensitivity of the results by clustering the standard errors at a higher level of aggregation, *i.e.* the urban area. This is a way to take into account spatial correlations. As will be shown below, the standard errors of estimates are hardly affected.

4 Baseline results

4.1 Effect on the volume of transactions

Figure 4 shows the difference, month by month, in the evolution of the volume of transactions between the departments that have raised their tax rate and the ones that have not. Because departments have not raised their rate at the same date, the date of the treatment can be different across treated units.

As already mentioned, I estimate different parameters for tight and less tight markets to take into account that tax incidence depends on the relative elasticities of supply and demand according to the standard economic theory. More precisely, I split the sample into two groups, municipalities in zone C and municipalities in zones A or B and compare the evolution of volumes between treated and control groups within each sub-sample.

First, I observe no significant difference between the evolution of the volumes before the treatment confirming the test of parallel trends. Second, I observe large short-term effects: a huge positive anticipation effect the month before the reform and a comparable negative correlative effect the month of the reform. Finally, it seems that the long-term impact after the reform depends strongly on the

tightness of the market. I observe a negative effect for zone C but a zero effect or even a slightly positive effect for tight markets.

Table 4 presents the results of the estimation of equation (1) on sample 1 of all municipalities (26,062 municipalities). Dummy variables M-3 to M+2 evaluate short-term effects of the reform and correspond to the n^{th} month before or after the reform and $M \geq 3$ estimates the long-term effect (estimation of the endpoint χ_3). Econometric analysis confirms the results of the event study graphs: huge short-term effects and a negative long-term effect only in less tight markets. The interest variable is the number of transactions in the municipality by month normalized by the average monthly number in 2012. Then, I can interpret the coefficient as a percentage change.

In the first column, I estimate equation (1) without municipality fixed effects. Short-term effects are huge: +39% the month before the reform and -40% the first month of implementation. The decrease of the number of transactions three months after the reform date is estimated at -7%. Results are close to those obtained in the second column with municipality fixed effects indicating that the selection in treatment is weak. The long-term effect is slightly weaker and less significant with the fixed effects but still negative indicating a lock-in effect due to higher tax transfer.

In this second column, the coefficient corresponding to the month before the implementation of the reform in the treated departments is approximately equal to 40% and is highly significant, meaning that buyers anticipated purchases in order to avoid the additional tax burden. The coefficients for two or three months before the reform are negative and close to zero. Buyers mainly anticipated the month before the reform. For the first month following the implementation (i.e. the month M), I observe a large negative correlative effect -40%. For the two following months, the coefficients are still negative and significant but weaker. The anticipation effect is very concentrated the month before the reform but the negative subsequent effect of the tax rise lasts at least three months after the implementation.

The coefficient of the long-term effect measures the impact for all French municipalities. The estimated coefficient is negative meaning that for this French reform, an increase of the tax transfer of 0.72 pp caused a negative impact of 4.8%, significant at 10%. This result is in line with the results of the literature. The rise of transaction costs is linked with a decrease of the mobility rate. Dachis et al. [2012] find that a 1.1 pp increase causes a 15% decrease in the number of transactions and Ommeren and Leuvensteijn [2005] find that 1 pp causes a 8% decrease.

In columns 3-6, I split the sample into two groups: tight markets and less tight markets. Only 4,980 municipalities are located in tight markets whereas 21,082 are located in less tight markets. As expected, the results are different between the two groups. First, short-term effects seem to be larger and last longer in tight markets: three months after the reform, the impact is still significant at 5% and its magnitude is around 9% while the impact is less significant and around 6% for the less tight municipalities. Second, I find a negative long-term effect of 8% only for less tight markets. The coefficient for the tight markets is positive, of weaker magnitude and insignificant. In column 5 and 6, I control for annual property and housing tax rates and the results remain unchanged. To sum-up, in both tight and less tight markets, buyers have a high propensity to change the date of the sale in order to avoid the additional tax burden. These results suggest that lowering transfer taxes could be effective to stimulate activity.

In the end, the increase in the housing transfer tax caused a large change in the timing of house

sales around the day of the reform implementation and a decrease in the number of sales in the long term only in less tight markets.

Table 12 in appendix D reports the same results but with standard errors clustered by urban area. Results remain unchanged.

4.2 Effect on prices

Figure 5 shows the difference, month by month, in the evolution of prices between the departments that have raised their tax rate and the ones that have not raised their rate. The reference date corresponds to six months before treatment. Again, I distinguish between tight and less tight markets but the results are similar in both markets. The magnitude of the effect on prices is far weaker than the one of the effect on volumes. I observe small short-term effects: no significant impact the month before the reform and a very small negative impact the month of the reform. Finally, it seems that the average pre-tax price after the reform is approximately the same as before.

Table 5 presents the results of the estimation of the impact on prices (equation (2)). I use all the transactions between 2013 and 2016 (1,501,274 transactions in sample 1). In column 1, I only include in the linear regression month fixed-effects and the treatment variables (long-term and short-term dummies). I find a huge positive and significant effect the month before the reform. But without municipality fixed effects, this can be explained by the higher propensity to anticipate purchases in tighter markets (Table 4) where the average price is higher than in less tight markets (232,607 euros versus 139,875 euros on average in 2012).

In column 2, I add municipality fixed-effects to take into account price level differences between municipalities. I find small short-term effects, positive and around 2% the month before the reform (significant at 5%) and negative and around 2% as well the two months after the implementation of the reform (significant at 1%). In the long term, the impact is insignificant (and equal to -1.5%). In column 3, I control for the characteristics of the property (surface area, type of dwelling, period of construction, number of rooms, etc.). The explained variance is equal to 77%. The magnitude of the effects decreases compared with column 2. The coefficient of the month before the reform is now close to zero and insignificant, meaning that the buyers who anticipated their purchase bought dwellings with "better characteristics". The negative short-term effects after the reform remain significant even if their magnitude are weaker. This means that the evolution of the characteristics of purchased houses partly explains the decrease in prices the months after the reform. In other terms, buyers chose to buy smaller houses in the two months following the reform.

Then, I split the sample between tight and less tight markets (columns 4-5) and I still control for the characteristics of the properties. The long-term impact is close in the two groups, roughly speaking a zero effect. In the short term, I find no anticipation effect but negative effects after. The magnitude of these effects is larger for less tight markets than for tight markets.

Based on the one-sided hypothesis test $H_0 : \hat{\chi}_3 < 0$, I am able to rule out at a 5 percent significance level the hypothesis that the incidence of the tax on the seller is higher than 0.36% for the tight markets and higher than 0.76% for less tight markets. Notice that these numbers refer to the one-sided hypothesis test (see Dustmann and Schönberg [2012] for similar tests), which is the appropriate test since the theory predicts the existence of incidence (on either the buyer or seller or on both). At a 10

percent significance level, I can rule out that the incidence is higher than 0.21% for the tight markets and higher than 0.61% for less tight markets. Hence, I can statistically rule out a full incidence on the seller at 5% for the tight markets and at 10% for the less tight markets.

In the end, the increase of the housing transfer tax caused a decrease in pre-tax prices in the three months after the reform, especially in less tight markets. These negative effects are higher than the increase of the tax: around 2% when the increase of the tax is equal to 0.72%. This can be due to an overreaction of the buyers or to unobserved heterogeneity of the purchased dwellings (weaker quality of the properties).

4.3 Sensitivity checks

4.3.1 Estimation on borders

Table 6 presents the results of the estimation of equation (1) when I select the municipalities close to an identifying border (sample 2). Although the sample size is divided by 15, the results are very close to those obtained with sample 1. I find similar short-term effects, between 40% and 50% and positive the month before the reform and around 35% and negative the month of the reform. In the long term, the magnitude is slightly larger. The tax rise caused a 11% decrease on the number of transactions. When I run different regressions for tight and less tight markets, again, I find a significant negative long-term effect only for less tight markets (around 12%). For the tight markets (550 municipalities), the effect is close to zero and insignificant.

Table 7 presents the results of the estimation of the impact on prices for sample 2. I run the same regressions than in Table 5 but on the transactions selected for the regression discontinuity design (156,116 transactions in the selected municipalities). The short-term effects are now insignificant for the less tight markets. I find a negative impact of the tax rise of 5% the month of the reform for tight markets. This impact is divided by two when I include property characteristics. Again, the buyers who purchased immediately after the tax rise bought properties of weaker quality (difference between column 2 and 3) and the coefficient is again higher than the increase of the tax. Overreaction or unobserved heterogeneity of the purchased dwellings could explain this negative impact. The long-term impact is close to zero and insignificant.

4.3.2 Estimation on a different zoning

In appendix D.2, I present results obtained with a different zoning. The zoning A/B/C from the Ministry of Housing is indeed a "black box" even if the principles are known. This new zoning is obtained by combining the vacancy rate at the level of the municipality. More precisely, I compute the growth rate of the vacancy rate between 2006 and 2012 using the Census. To avoid excessive volatility, I exclude the municipalities with less than 227 dwellings, which correspond approximately to municipalities of less than 500 people (the average person by dwelling is estimated at 2.2 in France in 2012). Then, municipalities with a growth rate weaker than the median (which is equal to 1.9%) are considered as tight markets.

Table 13 presents the comparison between the two zonings for the municipalities in the estimation sample (there are 26,043 municipalities in the comparison but 26,062 in the estimation tables because

Paris counts for 20 fixed effects for each *arrondissement* but is considered as a unique municipality in this comparison). I find some consistency between the two classifications (A and B are more often tight and C is more often less tight) but there are significant differences between the two (5426 municipalities in the C zone are considered as tight for instance).

Figure 6 shows the geographical repartition of this new zoning. Ile de France and the Mediterranean coastline concentrate many municipalities with tight markets as well as for the zoning A/B/C. But this new zoning identifies new tight municipalities, even in rural areas.

Figure 7 shows the same estimation than Figure 4 but with the new zoning. Results are very close: the long-term effect differs between tight and less tight markets. A lock-in effect is observed only in less tight markets.

Figure 8 presents the results of the event-study for prices. Again, the results are close to the ones obtain with the first zoning. There is a short-term negative effect the month of the reform but no effect on the long term for both tight and less tight markets.

These results obtained with a different zoning confirm the robustness of the main results of the paper. The lock-in effect is only observed in less tight markets.

5 Results for tax policy

5.1 Tax revenue

The Observatory of the Finances of Local Authorities publishes real estate transfer tax revenue each year. After a decrease between 2011 and 2013, from 12 to 10 billion euros, the total revenue increased again after 2013 and reached 12 billion euros again in 2015. These figures suggest that the tax increase did not have the negative impact on tax revenue feared by some economists at the time of the reform. But they are not conclusive about the global impact of the reform because the counterfactual evolution of volumes and prices in absence of the reform are unknown.

The estimation of the causal impacts on tight and less tight markets allows me to measure the pure effect of the tax rise. To compute the results in terms of tax revenue, I combine the effects on tight and less tight markets with the corresponding tax bases. For these calculations, I assume a zero effect on prices.

In the long term, the effect on tax revenue for tight markets is then a pure increase due to the tax rise. The gain in tax revenue is 0.7 pp. For less tight markets, the estimated decrease in the volumes is estimated to be 8%. The variation in tax revenue for these markets is equal to $92\% * 4.5\% - 3.8\% = 0.34$ pp, and is therefore positive. For 2013, tight markets represented 80% of the tax base. *In fine*, the effect of the reform for France is positive and has been about 0.63 pp.

5.2 Short-term stimulus

An other important lesson from this reform is the higher propensity of French buyers and sellers to adjust the date of sale rather than the sale price. Because of price rigidity, the change in tax rate causes a huge behavioral response (anticipation of sales in the case of this reform). The month before

the reform, the number of transactions increased by 40%. In such a situation, reducing transfer taxes to stimulate the market could by very effective.

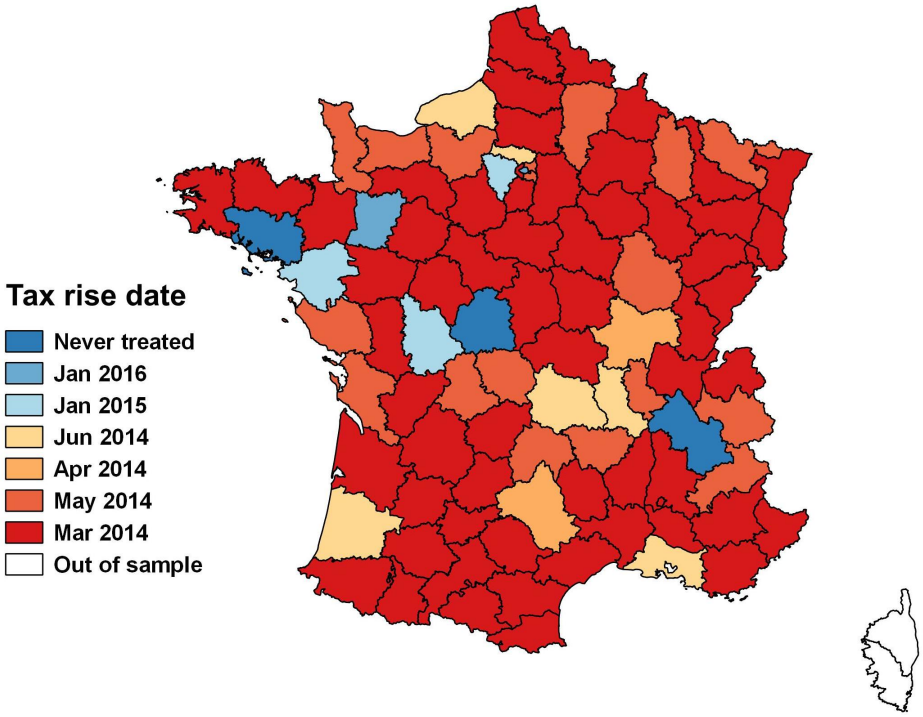
This confirms the results obtained by Best and Kleven [2016] indicating that the "temporary transaction tax cuts are an enormously effective form of fiscal stimulus" based on the evaluation of the 2008 British stamp duty holiday. Besley et al. [2014] estimate that for the same period the increase in transactions of properties affected by the stamp duty holiday was about 8%, and that incidence of the tax was both on buyers and sellers. In case of price rigidity, the incentive to adjust the date of sale for the sellers could be even higher.

6 Conclusion

In this paper, I exploit the 2014 French reform to analyze the incidence of transfer tax on the real estate market. I use transaction data between 2013 and 2017 to estimate the causal effect of transfer tax on the volume of transactions and on real estate prices.

I present two main findings. First, the short term response of the market is huge. The buyers anticipated the reform by purchasing properties the month before the tax rise, increasing the number of transactions of around 40% the month before the reform. This suggests that lowering housing transfer taxes could be used as a fiscal stimulus in the short term. Second, I show that the distinction between tight and less tight markets is crucial to analyse the market behavior. In tight markets, the volume of transactions is unchanged in the long term when I observe a negative impact of the reform on the number of transactions In less tight markets, of about 8%. In both types of markets, the tax burden seems to rest on the buyer, suggesting an important price rigidity of the French housing market

Figure 1: Implementation of the tax rise reform by date



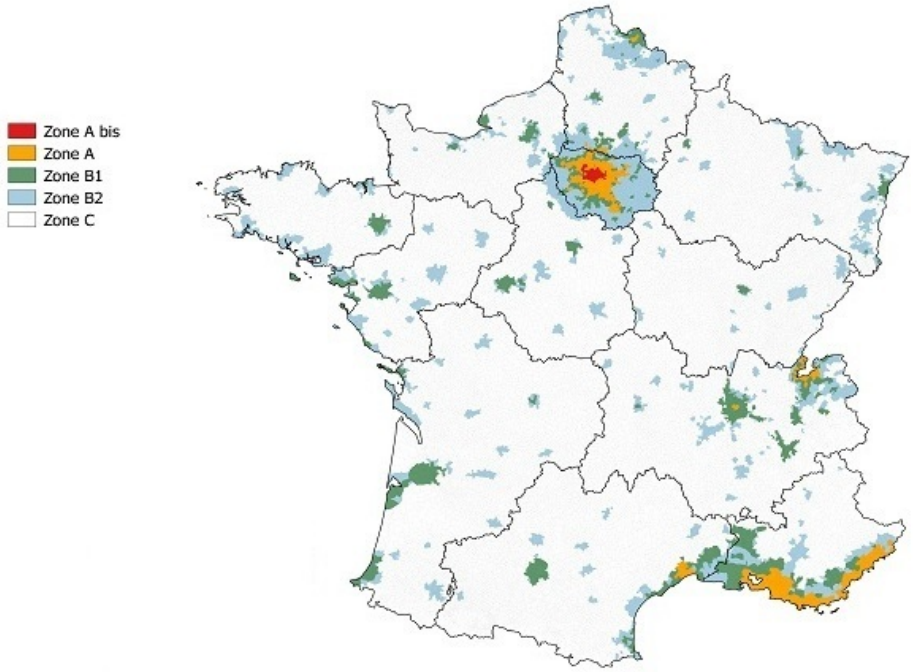
Notes: updated in May 2018.
Sources: DGFiP, 2018.

Table 1: Descriptive statistics of departments by treatment group

Treatment group	March 2014	April 2014	May 2014	June 2014	January 2015	January 2016	Never treated
Interdecile ratio of standard of living	3.2050	3.1389	2.9000	3.2833	3.2667	4.5000	2.9333
Standard of living median	20,406	20,209	19,924	20,576	22,483	23,341	20,626
Poverty rate	14.775	14.706	13.350	14.950	11.333	13.750	12.533
Part of main residence	80.427	77.406	77.000	84.533	86.600	84.350	78.333
Part of houses	65.872	61.300	71.600	54.383	59.400	40.750	68.633
Unemployment rate	8.9717	8.3333	7.3000	9.2500	7.0667	6.3500	8.0000
Average net hourly wage	13.239	13.063	12.455	13.863	15.810	17.690	13.127
Population	623,995	650,200	416,401	1,055,214	1,090,017	1,247,486	743,939
Population density	265.88	782.40	48.30	310.42	296.70	10,420.30	103.70
Debt ratio	54.448	62.234	59.672	57.494	38.452	11.481	15.987

Notes: Averages are computed at the level of the departments. Interdecile, Standard of living median, poverty rate, part of main residence, part of houses, average net income and population density are computed for 2016. Unemployment rate is the 2018 annual rate. Population corresponds to official population in 2017. Debt ratio is the one of 2014. Paris is treated in January 2016. Standard of living and net hourly wage are in euros.
Sources: Insee and Department of Local Studies and Statistics - DGCL

Figure 2: Tightness degrees in French market



Notes: Zones A and B are identified as tight markets and zone C as less tight market.

Table 2: Number of municipalities by treatment date and zone

Group	Date of the tax rise	A	Abis	B1	B2	C
1	March 2014	289	34	742	2014	13,555
2	April 2014	60	29	242	541	4,357
3	May 2014	0	0	0	23	593
4	June 2014	124	1	149	205	1,290
5	January 2016	0	20	0	6	207
6	January 2015	83	12	106	137	367
7	Never treated	0	0	73	90	713

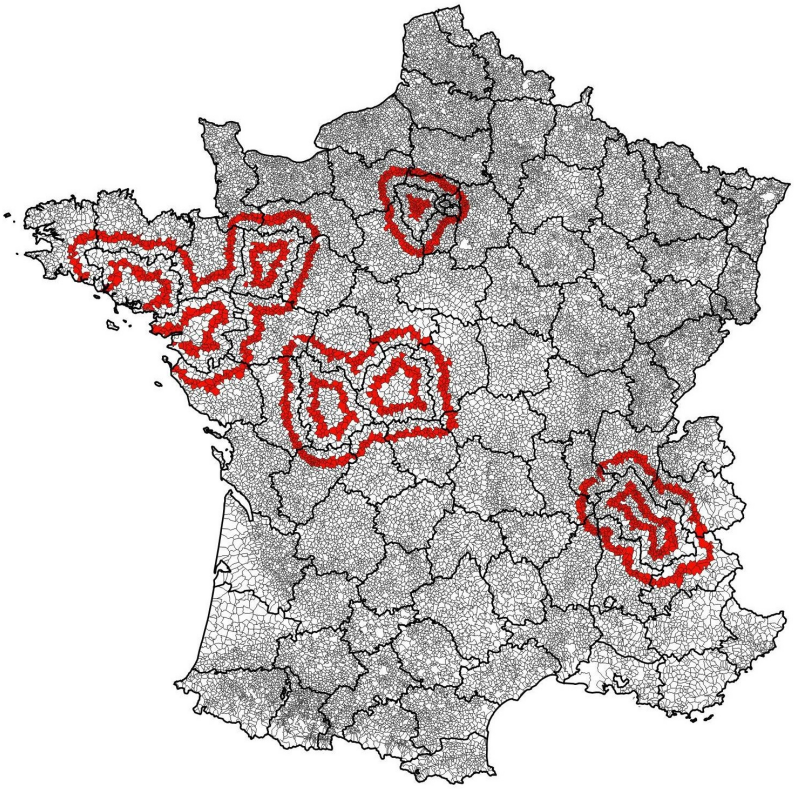
Notes: Less tight markets correspond to the municipalities in zone C and tight markets to municipalities in zones A or B.
Sources: Bien and Perval databases. Zoning of the Ministry of Housing.

Table 3: Characteristics of the transactions by treatment group

Group	Date of tax rise	Mean price per sq meter	Mean surface	Number of transactions	Frequency %
1	March 2014	2,153.83	89.81	1,329,655	59.65
2	April 2014	2,253.11	86.47	403,572	18.10
3	May 2014	1,175.82	99.59	24,792	1.11
4	June 2014	2,191.92	85.41	199,597	8.95
5	January 2015	2,497.85	88.22	110,955	4.98
6	January 2016	6,598.20	58.88	57,244	2.57
7	Never treated	1,959.76	90.10	103,386	4.64

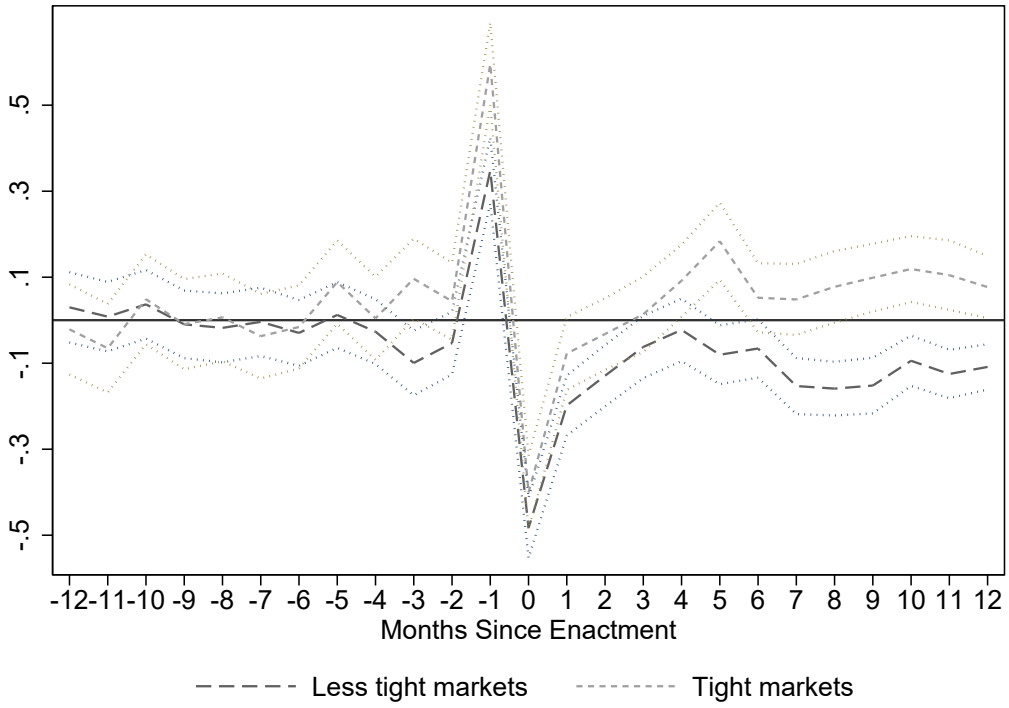
Sources: Bien and Perval databases, 2013-2016.

Figure 3: Selection of the municipalities for the sensitivity check



Notes: I include in the analysis all municipalities whose centroid is located in the 15 km strip on both sides of the boundaries of departments which have not raised their transfer rate in 2014, excluding the 5 km zone to get rid of spillover effects.

Figure 4: Event study on the volumes for tight and less tight markets



Notes: I plot the difference, month by month, in the evolution of volumes between the departments that have raised their tax rates and the ones that have not raised their tax rate. Municipality and month fixed effects are introduced in the estimation. Less tight markets correspond to the municipalities in zone C and tight markets to municipalities in zones A or B.

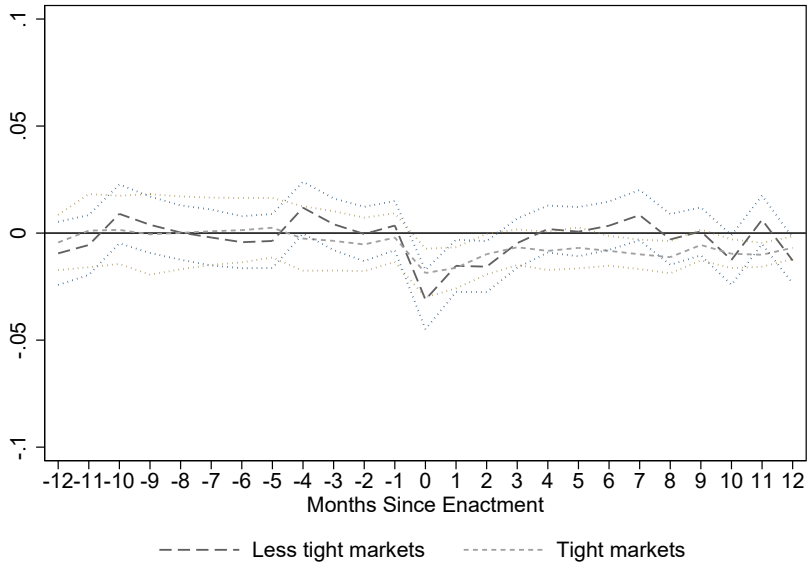
Table 4: Effect on the volume of transactions for whole France

	(1) Baseline	(2) With Muni FE	(3) Tight market	(4) Less tight market	(5) Tight market	(6) Less tight market
Short-term effects						
M-3	-0.065**	-0.053*	0.086*	-0.090**	0.086*	-0.090***
	(0.029)	(0.029)	(0.044)	(0.035)	(0.045)	(0.035)
M-2	-0.043	-0.028	0.024	-0.050	0.023	-0.050
	(0.028)	(0.029)	(0.038)	(0.035)	(0.038)	(0.035)
M-1	0.388***	0.406***	0.585***	0.350***	0.583***	0.349***
	(0.032)	(0.033)	(0.046)	(0.041)	(0.046)	(0.041)
M	-0.400***	-0.408***	-0.431***	-0.406***	-0.429***	-0.405***
	(0.027)	(0.027)	(0.039)	(0.034)	(0.039)	(0.034)
M+1	-0.113***	-0.120***	-0.112***	-0.127***	-0.110***	-0.125***
	(0.028)	(0.028)	(0.039)	(0.034)	(0.039)	(0.034)
M+2	-0.057**	-0.062**	-0.085**	-0.059*	-0.084**	-0.059*
	(0.028)	(0.027)	(0.039)	(0.033)	(0.039)	(0.033)
Long-term effect M>= 3	-0.077***	-0.048*	0.034	-0.080***	0.030	-0.082***
	(0.015)	(0.025)	(0.034)	(0.031)	(0.034)	(0.031)
Muni. Housing Tax rate					0.010	-0.002
					(0.007)	(0.005)
Muni. Property Tax rate					0.007	-0.002
					(0.016)	(0.008)
Dep. Property Tax rate					0.019	0.034
					(0.017)	(0.024)
Municipality fixed effects		x	x	x	x	x
Month fixed effects	x	x	x	x	x	x
R2	0.01	0.01	0.02	0.01	0.02	0.01
Number of observations	1,241,856	1,241,856	229,920	1,011,936	229,920	1,011,936
Number of municipalities	26,062	26,062	4,980	21,082	4,980	21,082

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Notes: Less tight markets correspond to the municipalities in zone C and tight markets to municipalities in zones A or B. Standard-errors are clustered at the municipality level.

Figure 5: Effect on the prices month by month



Notes: I plot the difference, month by month, in the evolution of prices between the departments that have raised their tax rates and the ones that have not raised their tax rate. Municipality and month fixed effects are introduced in the estimation. Less tight markets correspond to the municipalities in zone C and tight markets to municipalities in zones A or B.

Table 5: Effect on the prices of transactions for France

	(1) Baseline	(2) With Muni FE	(3) Muni FE and Char.	(4) Tight market	(5) Less tight market
Short-term effects					
M-3	-0.060	0.001	-0.004	-0.005	-0.000
	(0.057)	(0.005)	(0.004)	(0.004)	(0.005)
M-2	-0.098	-0.005	-0.004	-0.004	-0.002
	(0.080)	(0.005)	(0.004)	(0.005)	(0.006)
M-1	0.216**	0.019**	0.001	-0.000	0.005
	(0.109)	(0.009)	(0.004)	(0.004)	(0.006)
M	0.063	-0.025***	-0.018***	-0.015***	-0.023***
	(0.052)	(0.008)	(0.004)	(0.005)	(0.007)
M+1	0.057*	-0.016***	-0.013***	-0.012***	-0.015**
	(0.032)	(0.006)	(0.004)	(0.004)	(0.006)
M+2	0.044*	-0.007	-0.007**	-0.005	-0.014**
	(0.026)	(0.006)	(0.003)	(0.004)	(0.006)
Long-term effect M>= 3	-0.312	-0.015	0.001	0.003	-0.001
	(0.196)	(0.010)	(0.003)	(0.004)	(0.004)
Municipality fixed effect		x	x	x	x
Month fixed effect	x	x	x	x	x
Characteristics of the house			x	x	x
R2	0.02	0.36	0.77	0.78	0.71
Number of observations	1,501,274	1,501,274	1,288,001	848,832	439,169

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Notes: Prices are in log and correspond to pre-tax amounts. Less tight markets correspond to the municipalities in zone C and tight markets to municipalities in zones A or B. Standard-errors are clustered at the municipality level.

Table 6: Effect on the volume of transactions around the borders

	(1) Baseline	(2) With Muni FE	(3) Tight market	(4) Less tight market	(5) Tight market	(6) Less tight market
Short-term effects						
M-3	0.156*	0.072	0.136	0.062	0.135	0.066
	(0.094)	(0.089)	(0.125)	(0.124)	(0.125)	(0.124)
M-2	0.042	-0.053	0.081	-0.089	0.083	-0.079
	(0.074)	(0.074)	(0.095)	(0.107)	(0.096)	(0.108)
M-1	0.543***	0.445***	0.388***	0.516***	0.391***	0.527***
	(0.103)	(0.100)	(0.111)	(0.149)	(0.112)	(0.150)
M	-0.439***	-0.350***	-0.373***	-0.328***	-0.373***	-0.339***
	(0.069)	(0.069)	(0.082)	(0.102)	(0.082)	(0.103)
M+1	-0.077	0.009	-0.012	0.011	-0.012	0.001
	(0.081)	(0.079)	(0.096)	(0.112)	(0.096)	(0.113)
M+2	-0.147*	-0.070	-0.083	-0.060	-0.083	-0.069
	(0.080)	(0.073)	(0.090)	(0.103)	(0.090)	(0.105)
Long-term effect $M \geq 3$	0.081***	-0.110**	-0.023	-0.140**	-0.019	-0.121**
	(0.029)	(0.046)	(0.063)	(0.057)	(0.065)	(0.061)
Muni. Housing Tax rate					0.020	0.030
					(0.014)	(0.019)
Muni. Property Tax rate					0.069	-0.021
					(0.049)	(0.035)
Dep. Property Tax rate					-0.184	-0.137
					(0.280)	(0.201)
Municipality fixed effect		x	x	x	x	x
Month fixed effect	x	x	x	x	x	x
R2	0.01	0.01	0.02	0.01	0.02	0.01
Number of observations	85,728	85,728	22,512	63,216	22,512	63,216
Number of municipalities		1,867	550	1,317	550	1,317

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Notes: Selection of municipalities is shown in Figure 3. Standard-errors are clustered at the municipality level.

Table 7: Effect on the prices of transactions around the borders

	(1) Baseline	(2) With Muni FE	(3) Muni FE and Char.	(4) Tight market	(5) Less tight market
Short-term effects					
M-3	0.061	0.024*	0.015**	0.015	0.010
	(0.055)	(0.012)	(0.007)	(0.011)	(0.016)
M-2	0.062	-0.006	-0.001	0.017	-0.019
	(0.072)	(0.015)	(0.012)	(0.014)	(0.013)
M-1	0.014	-0.004	-0.007	-0.021*	-0.001
	(0.051)	(0.009)	(0.010)	(0.011)	(0.016)
M	-0.030	-0.049***	-0.022*	-0.021**	-0.013
	(0.040)	(0.017)	(0.011)	(0.008)	(0.022)
M+1	0.004	-0.030**	-0.004	0.004	-0.019
	(0.036)	(0.013)	(0.010)	(0.013)	(0.017)
M+2	-0.003	-0.024**	-0.007	-0.009	-0.015
	(0.036)	(0.011)	(0.009)	(0.015)	(0.019)
Long-term effect M>= 3	0.090	0.004	0.010	0.024	-0.006
	(0.105)	(0.007)	(0.009)	(0.014)	(0.008)
Municipality fixed effect		x	x	x	x
Month fixed effect	x	x	x	x	x
Characteristics of the house			x	x	x
R2	0.02	0.36	0.78	0.78	0.74
Number of observations	156,116	156,116	101,815	58,499	43,316

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Notes: The selection of municipalities is shown in Figure 3. Standard-errors are clustered at the department level.

A Coverage rate

To measure the impact of the tax rise on the volume of transactions, I use non-exhaustive data. One source of bias is the potential link between the coverage rate and the rise of the transfer tax. In table 8 I estimate the correlation between both variables and I find an insignificant coefficient close to zero in line with a coverage rate independent of the treatment.

The coverage rates by month and department are obtained by the comparison between the sum of the prices of the transactions in the database and the tax base computed by the local fiscal administration (Source: CGEDD).

Table 8: Test for coverage rate endogeneity

	(1)
	All departements
Treatment	0.012
	(0.012)
Department fixed effect	x
Month fixed effect	x
R2	0.88
Number of observations	4,464
Number of departments	94

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Notes: The coverage rates by month and department are obtained by the comparison between the sum of the prices of the transactions in the database and the tax base computed by the local fiscal administration.

B Local taxation

The property tax on built-up properties is due annually by the owner of a property, whether it is occupied or not. The housing tax has to be paid annually by the occupants of a housing, whether they are owners, tenants or occupants free of charge. The amount of these taxes is calculating using a rental value that depends on the housing characteristics (surface area, level of comfort, etc.), which is multiplied by a rate fixed each year by local authorities (the municipality for the housing tax, the municipality and the department for the property tax). In the case of the housing tax, the rental value is weighted according to the composition of the household and the income received by all the occupants of the dwelling. The tax rates voted by the local authorities significantly vary geographically, and thus the total amount of these taxes paid by the owners and/or the occupants varies more depending on the city they inhabit than the quality of their housing. These tax rates also vary over time. In Table 9, I report some descriptive statistics for these three taxes.

Table 9: Evolution of rates of housing and property taxes

	2013	2014	2015
Property Tax- Municipality			
Mean	14.07	14.13	14.24
Standard deviation	6.64	6.65	6.68
Min	0.01	0.05	0.05
Max	60.5	60.5	63.59
Property Tax- Department			
Mean	19.76	19.79	19.85
Standard deviation	4.95	4.93	4.88
Min	5.13	5.13	5.13
Max	32.92	32.92	32.92
Housing Tax			
Mean	13.02	12.83	12.92
Standard deviation	5	4.98	4.99
Min	0.01	0.01	0
Max	51.3	45.97	49.88
Number of municipalities	36,645	36,645	36,645
Number of departments	94	94	94

Sources: DGFiP

C Pre-trend tests

Table 10: Common Trend for volumes

	(1)	(2)	(3)	(4)	(5)	(6)
	All of France 2013	Fr. Tight	Fr. Less Tight	Boundaries 2013	Bound. Tight	Bound. Less Tight
Feb 2013	0.033	0.006	0.040	-0.168	-0.185	-0.138
	(0.040)	(0.064)	(0.049)	(0.196)	(0.277)	(0.255)
Mar 2013	0.080*	0.095	0.078	0.073	0.314	0.029
	(0.044)	(0.078)	(0.049)	(0.209)	(0.259)	(0.261)
Apr 2013	0.064	0.111*	0.055	0.069	-0.043	0.114
	(0.042)	(0.064)	(0.053)	(0.211)	(0.265)	(0.272)
May 2013	0.060	0.079	0.057	-0.151	-0.203	-0.150
	(0.054)	(0.070)	(0.068)	(0.174)	(0.239)	(0.219)
Jun 2013	-0.023	0.011	-0.027	0.094	0.059	0.121
	(0.047)	(0.091)	(0.052)	(0.169)	(0.281)	(0.210)
Jul 2013	0.029	0.132	0.008	-0.413*	0.186	-0.585**
	(0.069)	(0.093)	(0.075)	(0.221)	(0.325)	(0.264)
Aug 2013	0.051	0.043	0.054	-0.142	0.103	-0.229
	(0.048)	(0.077)	(0.056)	(0.213)	(0.258)	(0.270)
Sep 2013	0.006	0.105	-0.015	0.029	0.684*	-0.134
	(0.055)	(0.129)	(0.060)	(0.228)	(0.396)	(0.259)
Municipality fixed effect	x	x	x	x	x	x
Month fixed effect	x	x	x	x	x	x
R2	0.01	0.02	0.01	0.01	0.02	0.01
F test	1.00	0.79	0.79	3.39	1.59	3.30
pvalue	0.44	0.61	0.61	0.01	0.12	0.01
Number of observations	312,744	59,760	252,984	22,404	6,600	15,804
Number of municipalities	26,062	4,980	21,082	1,867	550	1,317

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Notes: in the first three columns I include all the municipalities, in the last three columns I only include the municipalities in the strip between 5 and 15 km on either side of the border. Less tight market corresponds to the municipalities in zone C and tight market to municipalities in zones A or B. Placebo dummies are equal to 1 for a given month in the municipalities treated in March 2014.

D Robustness checks

D.1 Clustering by urban areas

D.2 Zoning

Table 11: Common Trend for prices

	(1) All of France 2013	(2) Fr. Tight	(3) Fr. Less Tight	(4) Boundaries 2013	(5) Bound. Tight	(6) Bound. Less Tight
Feb 2013	0.003	-0.003	0.017	0.010	0.003	-0.009
	(0.007)	(0.008)	(0.015)	(0.030)	(0.045)	(0.042)
Mar 2013	-0.002	0.002	-0.009	0.039	0.082*	-0.005
	(0.006)	(0.007)	(0.014)	(0.026)	(0.046)	(0.040)
Apr 2013	0.001	0.005	-0.010	0.002	-0.002	-0.023
	(0.007)	(0.007)	(0.014)	(0.026)	(0.042)	(0.041)
May 2013	-0.003	0.002	-0.018	0.007	0.017	-0.010
	(0.006)	(0.007)	(0.014)	(0.026)	(0.047)	(0.038)
Jun 2013	-0.001	0.001	-0.008	-0.005	0.019	-0.035
	(0.006)	(0.007)	(0.014)	(0.022)	(0.038)	(0.034)
Jul 2013	0.001	0.002	-0.000	0.010	0.042	-0.009
	(0.006)	(0.007)	(0.013)	(0.024)	(0.037)	(0.034)
Aug 2013	-0.001	-0.004	0.005	0.006	-0.003	-0.020
	(0.007)	(0.009)	(0.014)	(0.024)	(0.039)	(0.037)
Sep 2013	0.004	0.006	-0.002	-0.002	0.056	-0.062*
	(0.006)	(0.007)	(0.013)	(0.026)	(0.035)	(0.037)
Surface (log)	x	x	x	x	x	x
Municipality fixed effect	x	x	x	x	x	x
Month fixed effect	x	x	x	x	x	x
R2	0.78	0.78	0.75	0.80	0.78	0.77
F test	0.29	0.34	1.07	0.56	2.08	0.63
p-value	0.97	0.95	0.38	0.81	0.04	0.75
Number of observations	315,773	210,443	105,330	26,035	15,279	10,756
Number of municipalities	25,073	4,289	20,784	1,596	227	1,369

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Notes: in the first three columns I include all the municipalities, in the last three columns I only include the municipalities in the strip between 5 and 15 km on either side of the border. Less tight market corresponds to the municipalities in zone C and tight market to municipalities in zones A or B. Placebo dummies are equal to 1 for a given month in the municipalities treated in March 2014.

Table 12: Clustering by urban areas: effect on volumes

	(1) Baseline	(2) With Muni FE	(3) Tight market	(4) Less tight market	(5) Tight market	(6) Less tight market
Short-term effects						
M-3	-0.065*	-0.053	0.086	-0.090**	0.086	-0.090**
	(0.035)	(0.036)	(0.068)	(0.036)	(0.068)	(0.036)
M-2	-0.043	-0.028	0.024	-0.050	0.023	-0.050
	(0.027)	(0.031)	(0.036)	(0.037)	(0.036)	(0.037)
M-1	0.388***	0.406***	0.585***	0.350***	0.583***	0.349***
	(0.050)	(0.042)	(0.068)	(0.043)	(0.069)	(0.043)
M	-0.400***	-0.408***	-0.431***	-0.406***	-0.429***	-0.405***
	(0.041)	(0.033)	(0.061)	(0.040)	(0.061)	(0.040)
M+1	-0.113***	-0.120***	-0.112***	-0.127***	-0.110**	-0.125***
	(0.034)	(0.029)	(0.042)	(0.034)	(0.042)	(0.033)
M+2	-0.057**	-0.062**	-0.085**	-0.059**	-0.084**	-0.059**
	(0.023)	(0.024)	(0.041)	(0.028)	(0.042)	(0.028)
Long-term effect M>= 3	-0.077	-0.048	0.034	-0.080***	0.030	-0.082***
	(0.062)	(0.031)	(0.036)	(0.030)	(0.037)	(0.030)
Muni. Housing Tax rate					0.010	-0.002
					(0.010)	(0.007)
Muni. Property Tax rate					0.007	-0.002
					(0.015)	(0.008)
Dep. Property Tax rate					0.019	0.034
					(0.022)	(0.026)
Municipality fixed effects		x	x	x	x	x
Month fixed effects	x	x	x	x	x	x
R2	0.01	0.01	0.02	0.01	0.02	0.01
Number of observations	1,241,856	1,241,856	229,920	1,011,936	229,920	1,011,936
Number of municipalities		26,062	4,980	21,082	4,980	21,082

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

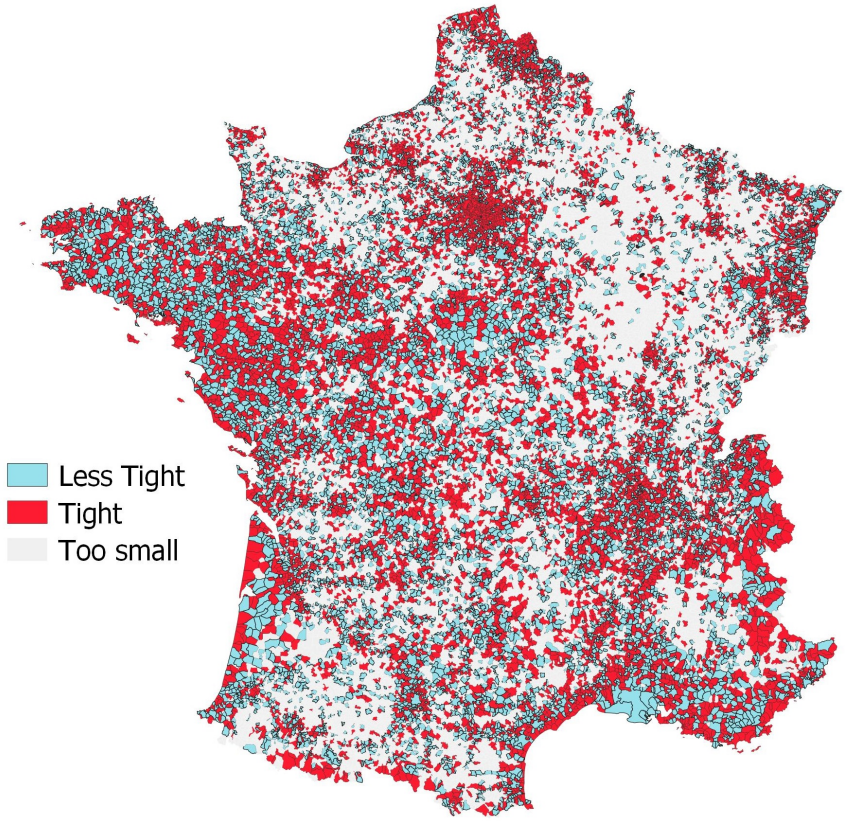
Notes: Standard errors are clustered by urban area.

Table 13: Zoning comparison

Zoning A/B/C	**Zoning robust**						**Total**	
	Less Tight		**Tight**		**Too small**			
	No.	Col %	No.	Col %	No.	Col %	No.	Col %
A	177	2.3	376	4.7	3	0.0	556	2.1
Abis	6	0.1	71	0.9	0	0.0	77	0.3
B1	528	6.8	712	9.0	72	0.7	1312	5.0
B2	1222	15.7	1355	17.1	439	4.3	3016	11.6
C	5860	75.2	5426	68.3	9796	95.0	21082	81.0
Total	7793	100.0	7940	100.0	10310	100.0	26043	100.0

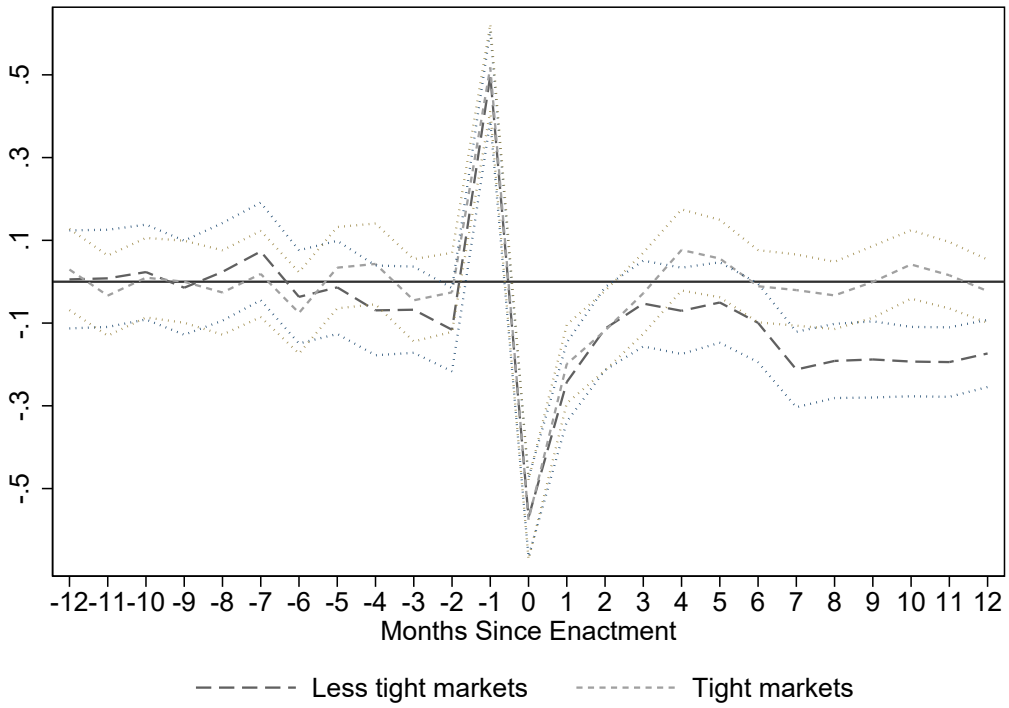
Municipalities with a growth rate weaker than the median (which is equal to 1.9%) are considered as tight markets.Municipalities with less than 227 dwellings (which correspond approximately to municipalities of less than 500 people) are excluded.

Figure 6: Second zoning



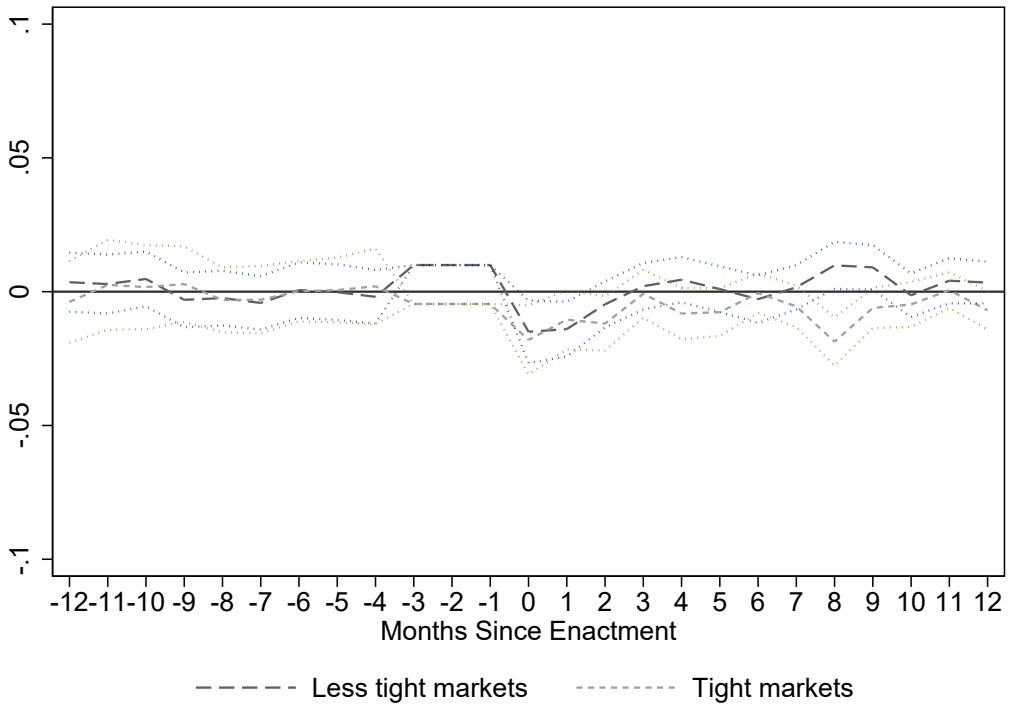
Notes: A market is considered tight if the municipality has at least 227 dwellings and the growth of the vacancy rate is below the median.

Figure 7: Event study on the volumes for tight and less tight markets determined by the second zoning



Notes: I plot the difference, month by month, in the evolution of volumes between the departments that have raised their tax rates and the ones that have not raised their tax rate. Municipality and month fixed effects are introduced in the estimation. Tightness is determined with the growth of the vacancy rate of the municipality. A market is considered tight if the municipality has at least 227 dwellings and the growth of the vacancy rate is below the median.

Figure 8: Effect on the prices for tight and less tight markets determined by the second zoning



Notes: I plot the difference, month by month, in the evolution of prices between the departments that have raised their tax rates and the ones that have not raised their tax rate. Municipality and month fixed effects are introduced in the estimation. Tightness is determined with the growth of the vacancy rate of the municipality. A market is considered tight if the municipality has at least 227 dwellings and the growth of the vacancy rate is below the median.

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