The Impact of a Rise in the Real Estate Transfer Taxes on the French Housing Market

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Abstract

This paper estimates the effects of an increase in the real estate transfer taxes (RETT) rate from 3.80% to 4.50%, following an optional reform implemented in March 2014 by French *départements*. Not all the *départements* implemented the RETT increase, which is the starting point for a natural experiment: using a difference-in-differences design, we estimate two main effects. (1) An anticipation effect a month before the implementation of the reform in order to avoid the RETT increase (timing response). The total tax base increased by 28% just the month before. (2) The classic depressing effect of a tax on the equilibrium quantity (extensive margin response) is estimated to be 7% on average from March 2014 to October 2015. All in all, the average net effect corresponds to a drop of the transactions of 4.6% over a period of ten months following the implementation date. Furthermore, we estimate that the elasticity of the tax revenue to the tax increase is about 0.65, meaning that *départements'* tax revenues are still on the increasing side of the Laffer curve.

Keywords Local government, Real estate market, Transfer taxes, Natural experiment, Anticipation

JEL Classification H71, R21, R31, R51

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Introduction

The 2014 reform of the French real estate transfer taxes (RETT), allowing an increase in *départements*'[1] tax rate of 0.7 percentage point (from 3.80% to 4.50% of the tax base), aimed to raise their tax revenue, in a context of state grants reductions and increasing social spending. Not all *départements* implemented the RETT increase, and not at the same time, which is the starting point for a natural experiment. Even though it is not a purely random experiment, we show in the course of the paper that there was no departmental selection bias in choosing the tax increase: this choice was not correlated with the local housing market or political features.

The RETT, also called stamp duties land taxes, or *droits de mutations* in French[2], are taxes levied on all transfers of ownership of real estate or land. The RETT are an important source of revenue for the French *départements*: they represent around 10 billion per year. However, when the reform was implemented, the possible negative impact on the housing market was not evaluated or even discussed. In the following study, we use open access data on the monthly number of transactions and tax bases of real estate by *départements* and implement a difference-in-differences framework using a quasi-myopic model[3] as developed by Malani and Reif (2015). We assume that there were two main effects due to this reform, (1) an anticipation effect from the buyers and sellers to avoid the tax increase (timing response), and (2) a retention effect in post-reform period: a classic depressing effect of a tax on the equilibrium quantity (extensive margin response). What we are looking for is the behavioural response in terms of timing and extensive margin of the agents (i.e. buyers and sellers). Finally, we evaluate the elasticities of the outcome variables to the RETT, with particular attention to the tax revenues.

We estimate that (a) there was an anticipation effect of 28% on the volume of transactions, meaning that buyers and sellers reacted to the RETT increase, the month just before the implementation of the tax increase, by bringing forward their sale date. (b) We also estimate the mean retention effect for the regressed period at around 7% of the volume of transactions (assuming no effect on the sale price), meaning that the tax increase had a negative impact on the housing market. All in all, the average net effect corresponds to a drop of the transactions of 4.6% over a period of ten months following the implementation date. The estimates on the tax revenues confirm these results, and the tax revenue elasticity to the RETT, estimated at 0.65, shows that there was a loss from the total tax base through the volume of transactions, the sale price, or both, and that the *départements* are in the upward part of the Laffer curve. Finally, we perform a series of robustness checks such as a placebo test, a self-selection test and controlling for possible changes in local economic conditions, confirming that our results are unbiased and robust.

[1] Intermediary administrative unit.
[2] In France, they are also referred as *droits d'enregistrement et taxe de publicité foncière*.
[3] Econometric model with anticipation (see box 4).

Literature Review

Previous literature on the impact of the increase in the rate of the RETT is relatively recent, as the first empirical estimation was published in 1993, and other main theoretical and empirical evaluations were mostly done over the past four years. As this stream of papers follows a natural development, we present them in chronological order for a better understanding.

The first in-depth research on RETT was done by Benjamin et al. (1993), who analyze the effect of an increase of 45% in Philadelphia's transfer taxes in 1988. The particularity of the transfer taxes of this city compared to French ones, is that the payment of the tax is shared equally between the seller and the buyer. The authors focus on the effect of the RETT's increase on the sale price of residential property, using a hedonic model and micro data (around 350 transactions). Unfortunately, they could not estimate the impact on the volume of transactions. Nevertheless, they find a decrease in post-reform prices equal to the tax increase, meaning that the burden of the tax increase rests on the seller, at least in the short-run.

The next paper, (Ioannides & Kan, 1996), is not directly related to the RETT's impact, but more generally to residential mobility, and to the decision of moving, and that of whether to rent or to own. This article develops a theoretical model of housing tenure choice and residential mobility which is used as a basis in many following papers. The authors find that home-owners are responsive to housing market conditions by adjusting their stock. Their empirical estimates suggest that proportional monetary transaction costs are not worse than the lump-sum transaction costs in households' mobility decisions. Using the same theoretical framework, Van Ommeren and Van Leuvensteijn (2005) assess the impact of an increase in transaction costs in the Netherlands that are close to the transfer taxes in France, as they are *ad valorem* transaction costs, mostly paid by the buyer. Using duration models, they find that a 1% increase in the transaction costs decreases mobility by 8%, which is quite significant. They deduce that transaction costs could imply lock-in effect, leading to a negative impact on the housing market and the labour market. Their conclusion is that a decrease or an abolition of the buyer's transaction costs would improve the mobility of home-owners.

A more recent study by Dachis et al. (2012) estimates the effect of the implementation of a progressive transfer taxes in Toronto (1.1% on average), paid by the buyer. Unfortunately, they did not consider a potential anticipation effect. Combining differences-in-differences and regression discontinuity designs on a large sample, they estimate that the new tax decreased the volume of transactions by 15%, and the sale price by a proportional amount to the transfer taxes. Their theoretical model predicts a welfare loss of about $1 for every $8 in tax revenue raised. They conclude that the RETT should be removed in favour of the property tax.

Other papers are more focused on RETT empirical estimations, rather than theory. The first one by Davidoff and Leigh (2013) assesses the Australian's progressive RETT reform. Instrumenting the endogenous RETT variable, they obtain similar results to Benjamin et al. (1993): increases in transfer taxes lower housing prices, suggesting that economic incidence falls on sellers. They also evaluate that such increases have a negative impact on owners' mobility, and that this effect increases over time.

Besley et al. (2014) evaluate the impact on the housing market of a RETT holiday in the United Kingdom, using data on sale price and number of transactions. They develop detailed and convincing empirical evaluations as well as a bargaining model. They find a significant increase of around 8% in the volume of transactions following the tax holiday, but only in the short-run. Their theoretical model allows them to estimate that 60% of the tax decrease accrues to the buyer.

Kopczuk and Monroe (2015) estimate the effect of a specific RETT on high value housing in New-York, called the mansion tax. They assess that this tax creates a notch (i.e. a discontinuity in the tax liability), with a surplus of selling below, and a large gap above the threshold. They estimate that the volume of missing transactions above the threshold is greater than the volume of transactions below. They conclude that this observation is due to the bargaining of buyers and sellers, and that this particular tax impacts negatively the search market around the notch, and is inefficient in terms of tax revenue.

Best and Kleven (2016) also analyze some notches in the housing market in the United Kingdom, due to the progressive RETT. Their findings are similar to those of the previous article: there is some distortion of the housing market across marginal tax rates. Analyzing the same tax holiday as Besley et al. (2014), they find similar results regarding the volume of transactions: the elimination of 1% transfer taxes increased housing market activity by 20%. Therefore, there are large timing and extensive margin responses from buyers and sellers due to the RETT modification.

Finally, Slemrod et al. (2016) estimate the behavioural responses to a change in the RETT's notch in Washington D.C. They provide a useful model of bargaining between sellers and buyers, with progressive tax. Using a difference-in-differences design, they find no evidence of a lock-in effect, but they estimate a slight timing effect, which corresponds to an anticipation effect to avoid the tax increase. Furthermore, they conclude that buyers and sellers are more able to adjust the sale price in response to the tax increase, than to modify the sale date (which works only with progressive RETT).

Institutional Background

Box 1

The Real Estate Transfer Taxes System in France

The RETT are levied on all transfers of ownership of real estate and land. The French law distinguishes between two types of transfers, (1) the *droits de mutation à titre onéreux* (DMTO), which are based on transfers of ownership further to a sale, and (2) the *droits de mutation à titre gratuit*, which are based on transfers of ownership further to a donation or inheritance. Unlike in the United Kingdom or some counties in the United States, the RETT in France are proportional and not progressive. However, different rates exist; they depend on the characteristics of the real estate, the denomination of the buyer and seller (i.e. individual or professional) and the type of transfer. The RETT are calculated on the tax base after abatements, which are very scarce and small; therefore, the tax base reflects the real estates' sale price in almost all cases.

Three tax regimes of RETT exist in France, with different applicable rates:

(1) *Régime de Droit Commun*. It applies to the DMTO on real estate exempted from Value Added Tax (VAT): established properties (more than 5 years old), new constructions (less than 5 years old) sold between individuals (except if the seller has bought it in off-plan (*Véfa*)), and the buildable lands sold between individuals. The applicable rate for this tax system is decomposed as follows: **3.80% goes to the *départements*** (rate before the 2014 reform which we are interested in), 1.20% goes to the municipalities, and finally 2.37% applied to the *départements*' tax rate goes to the central government (for tax base and collection fees) (i.e. 0.09% of the tax base). Thus, the total rate for this regime was 5.09% before the reform, and is at 5.81% now for the *départements* which have implemented the reform.

(2) *Régime Dérogatoire*. First, it applies to the DMTO subject to VAT: the new constructions and lands sold by a professional or the new constructions bought in off-plan and resold between individuals, at the rate of 0.715%. Secondly, it applies to all the *droits de mutation à titre gratuit*, at the rate of 0.60%. Within this tax regime, the part of transactions of *droits de mutations à titre gratuit* is roughly 40%.

(3) *Exonération des Droits de Mutation*. This tax system applies only to the acquisition done by the State or local authorities, so there is a total tax exemption.

The average period between the signature of the preliminary sale agreement (between the seller and the buyer), and the bill of sale is 3 months. The minimum is 1 month due to the legal period of withdrawal.

The transaction costs (i.e. the RETT, the notary and experts' fees) of a house or land sale are **paid by the buyer, and must be paid in full when the bill of sale is signed**. Before the reform, the average rate of the transaction costs for real estate subject to the *Régime de Droit Commun* was around 7%. These transaction costs are collected by the notary on behalf of the Treasury Department (*Direction Générale des Finances Publiques* or DGFiP).

Following this, all the transfers of ownership and their details (e.g. number of transactions, sale price, tax revenue, locality, owners' identities) are registered by the *Service de Publicité Foncière*, which depends on the Treasury Department; except for the Alsace-Moselle region, which is composed of the *départements* of Moselle (57), Bas-Rhin (67) and Haut-Rhin (68), and has its own registration utility: the *Livre Foncier*. This situation is due to the particular legal status of this region, inherited from the German annexation of 1870.

It is important to notice that, in almost any case, the transaction costs cannot be financed through mortgages. In other words, the transaction costs must be paid first and in addition to the downpayment. Therefore, even a slight increase of the RETT could have a large impact on the behaviour of **the buyers, because it increases out-of-pocket contributions**, and thus may have large impacts on the housing market.

Sources: DGFiP and Légifrance, Bulletin Officiel des Finances Publiques – Impôts 2017.

Context of the Reform

As explained by all the official documents and newspapers, there are two main reasons why the government and the *départements* wanted to increase the RETT. (1) For several decades, a process of decentralization and fiscal autonomy of local authorities has been engaged. As a result, the grants of the State decreased drastically (by €1.5 billion in 2013). Moreover, in 2010, the State abolished the business tax, one of the main sources of tax revenue for local authorities. (2) The growth in the real estate market between 2000 and 2007 enabled *départements* to follow the pace of growth of local public expenditures until the financial crisis of 2007. Afterwards due to the economic downturn, the revenues generated by the transfer tax dropped. Simultaneously, the amount of social spending of the *départements* (RSA, APA and PCH[4] especially) increased sharply, and both factors resulted in a financial stranglehold. Thus, in the framework of the *Pacte de Confiance et de Responsabilité entre l'Etat et les Collectivités Territoriales*, the Prime Minister and the local councilors discussed the possibility of an RETT's increase, to help the *départements* which were struggling with their finances.

Therefore, we can argue that the implementation or the non-implementation of the treatment was not due to a willingness to stimulate the housing market, or to help buyers and sellers through fiscal policy. This policy change was mainly driven by reasons entrenched in the financial turmoil of *départements*.

The draft Finance Act for 2014 was publicly announced on September 25, 2013, and relayed the information of a first agreement between the *départements* and the French Government about an increase in RETT's *Régime de Droit Commun* (cf. box 1). Most of the *départements* announced whether they would increase the RETT and when during the first semester of 2014.

At this stage, we can argue that both buyers and sellers of property were aware of the reform and its date of implementation, and whether the *département* where they intended to buy or sell would increase the tax[5]. They then could anticipated the reform by bringing forward the sale date, in order to avoid the tax increase.

The RETT reform was enacted on December 29, 2013 by the article 77 of the Finance Act for 2014, and allows *départements* that are willing to do so (i.e. the implementation of a tax rise remains optional), to increase their part of RETT's *Régime de Droit Commun* by a maximum of 0.7 percentage point. It means that the rate of the RETT going to the *départements* can rise from 3.80% to 4.50% (i.e. an increase of 18.42% of the RETT departmental's part).

Furthermore, at this time, the reform was enacted as temporary and should have been implemented only on the agreements finalized between March 2014 and February 2016; afterwards, the RETT should have come back to 3.80% maximum. However, on December 29, 2014, the article 116 of the Finance Act for 2015 made permanent the possibility for the *départements* to rise their part of the RETT up to 4.50%. The choice to increase the tax or not and the level, falls to the local councilors. The 4.50% rate is an upper limit, and the *départements* can set whatever rate suits them between 1.20% and 4.50%. However,

[4] *Revenu de Solidarité Active, Allocation Personnalisée d'Autonomie* and *Prestation de Compensation du Handicap*.

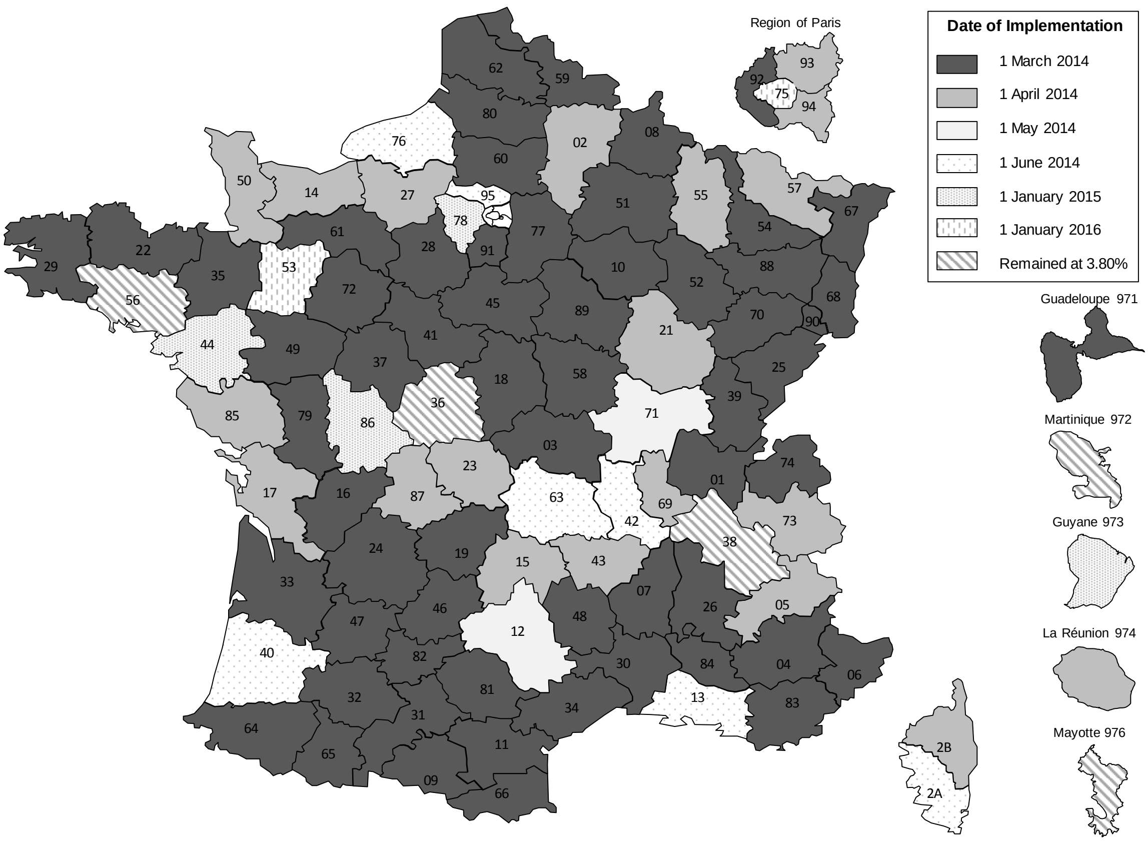
[5] An additional argument to demonstrate that they were aware of the reform, is that buyers and sellers usually mandate a real estate agent and/or a notary for the matching process and afterwards some counsels about the documents (e.g. expert diagnostics, property tax notice), needed for the preliminary agreement. These brokers and experts are well-informed about the changes of the real estate legal context.

in practice, every *département* that chose to raise the RETT has increased them by the maximum amount (i.e. 4.50%)[6].

A first group of 61 *départements* implemented the reform on March 1, 2014, a second group of 20 *départements* on April 1, 2014, a third group of 2 *départements* on May 1, 2014, a fourth group of 7 *départements* on June 1, 2014, a fifth group of 4 *départements* on January 1, 2015 and finally a group composed of 2 *départements* on January 1, 2016. However, this last group is not considered as treated in our estimates, as we stop the period of estimation in October 2015. Finally, 5 *départements* are still currently at 3.80%.

Figure I shows a departmental map of the implementation schedule of the reform.

Figure I - Map of the RETT Increase Implementation Schedule by *Département*



Notes: map updated May 2017.
Sources: Authors' drawing and DGFiP, Droits d'enregistrement : taux, abattements et exonérations 2017.

[6] Except the Côte d'Or (21) which increased them until 4.45%; thus, we consider it as being at 4.50% in the estimates.

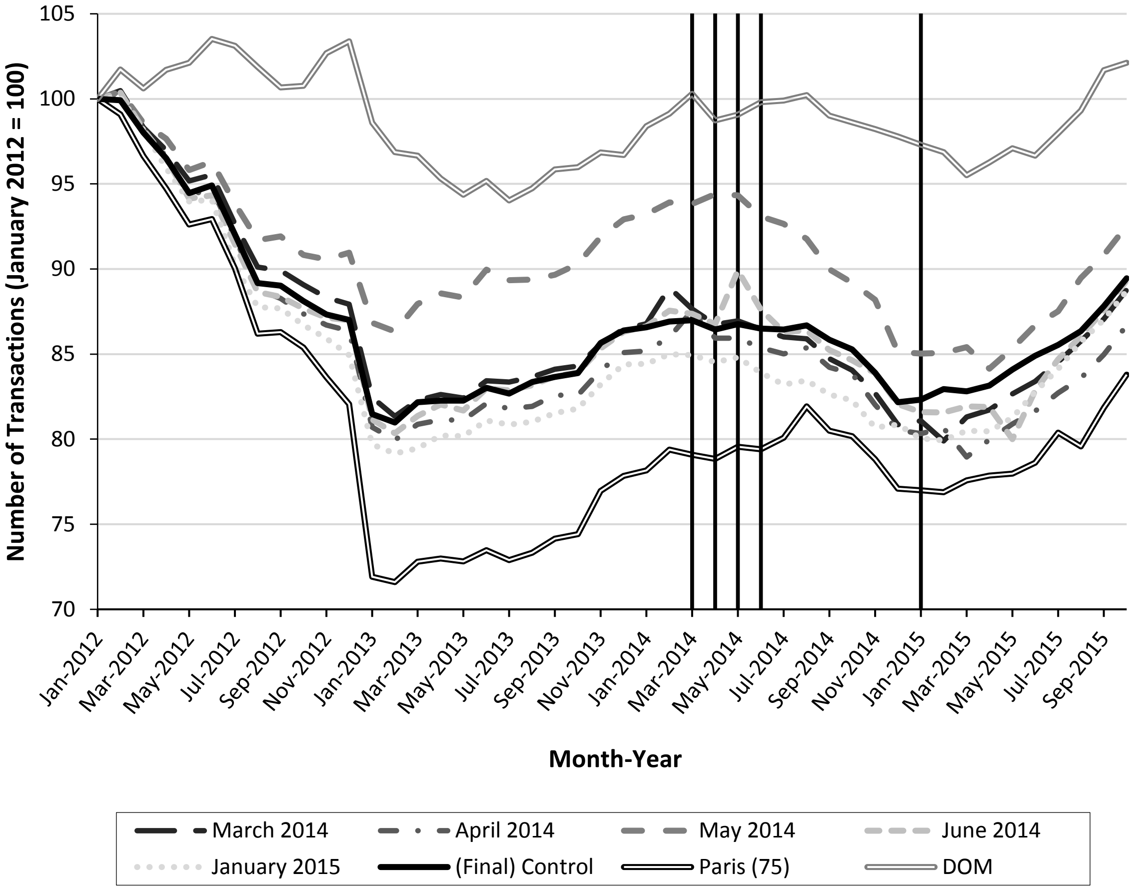
Purpose of the Evaluation

In the following evaluation, we focus on two main potential effects, although three effects may be distinguished:

(1) **Anticipation Effect**. As the reform was publicly announced far ahead, we assume that the buyers and sellers were not caught off guard, and thus many of them may have chosen **to bring forward the sale date in order to avoid the tax increase** in their *départements*. This behaviour can be referred to as a dynamic optimisation effect, or timing response. **This period of anticipation should precede the implementation month**. This assumption seems plausible when observing the trends in the number of transactions and the total tax base of the *Régime de Droit Commun* from January 2012 to October 2015 (see figure II). Indeed, some distinct peaks appear just before the date of implementation. We expect no effect on prices during the anticipation period. Two reasons can vindicate this guess. In the first place, as said in the previous sections, the sale price is set during the preliminary agreement, which is signed around 3 months before the sale date, and thus people who anticipated could have changed only the sale date and not the sale price set by agreement. Furthermore, as both the seller and the buyer are interested in avoiding the tax increase, no bargaining on price should have occurred.

(2) **Retention Effect**. We expect the housing market to be impacted durably by the increase in the RETT, **preventing some buyers from moving and accessing to ownership**. Thus, a decrease in the volume of transactions should be observed, that is the extensive margin response. In such a case, **we may observe a lock-in effect** (e.g. buyers could have chosen to renounce to buy, to postpone their purchase, or to rent rather than to become an owner). The retention effect should begin at the implementation date of the reform, and should have lasted a long time after. Indeed, one can observe in Figure II that the trends of the treated groups remained below the trend of the (final) control group from the date of implementation, and joined it in September/October 2015.

(3) **Price effect**. Theory also suggests a slight effect on sale prices: due to the extensive margin response, the demand must have decreased while the supply must have remained the same; therefore, the bargaining power of the buyers must be higher, the competition between sellers must increase, and some would be willing to decrease their selling price. **However, it is a strong assumption, knowing that the French housing market is sticky in terms of price**. In France, RETT must be paid by the buyers, and knowing that the housing market is rigid, sellers have a greater bargaining power. Then, unlike Philadelphia's RETT reform where RETT's payment is divided in half between buyer and seller, as studied by Benjamin et al., (1993) who estimated that housing prices decreased, we should not observe the same phenomenon in France. Moreover, the **RETT in France are proportional** and not progressive, hence **the agents have less interest in changing the sale price, compared to progressive RETT**, and more in changing the sale date (see Davidoff and Leigh, 2013; Slemrod et al, 2016). Furthermore, the data we use are not very suited to test this price effect. We hence focus on the first two effects.



Notes: the number of transactions of the *départements* in each group are cumulated over the previous 12 months, and correspond to the number of transactions in the *régime de droit commun* registered by the DGFiP in each *département*. The *départements* of Moselle (57), Bas-Rhin (67), Haut-Rhin (68) and Mayotte (976), are excluded. Vertical lines correspond to the implementation dates.
Sources: CGEDD and DGFiP, Nombre de ventes immobilières taxées au taux de droits commun par département from 2012 to 2015.

Figure II - Monthly (12-month cumulative) number of transactions trends from January 2012 to October 2015, by sample and implementation groups

Data

To undertake this evaluation, we use two main variables, which are: the number of transactions and the tax bases, both by *département* and month. The source of these variables is the *Conseil Général de l'Environnement et du Développement Durable* (CGEDD). The raw data on the RETT come from the *Service de Publicité Foncière* (datasets MEDOC and Fidji) and are compiled and modified by the CGEDD, before providing them at the departmental level in open access.

MEDOC provides the tax revenue by *département* and month, and is exhaustive. Fidji provides the tax revenue and the number of transactions also by *département* and month, but it presents the inconvenience to be not completely exhaustive (1% of the transactions are missing). To solve this problem, the CGEDD uses both databases, and applies a correction coefficient[7], in order to recover an estimation of the total number of transactions by *département*[8].

Next, the CGEDD computes the monthly total tax bases of each *département*, by dividing the tax revenue by the corresponding RETT rate, and publishes in open access two datasets.

(1) The first one provides monthly data on the number of transactions of the *Régime de Droit Commun* by *département*, for the period from April 2004 up to now (from MEDOC + Fidji), but those data are computed on a 12-month cumulative basis.

(2) The second one provides monthly total tax base (raw and 12-month cumulative) of the *Régime de Droit Commun* and the *Régime Dérogatoire* separately and by *département*, for the period from January 2000 up to now (from MEDOC). The data on the *Régime de Droit Commun* (whereby the reform is implemented) are composed approximately of 95% of established properties (whose 15% of non-residential premises) and around 5% of lands.

We only use the second dataset because it is impossible at this stage to recover the simple monthly data for the first dataset.

Following this, we applied some correction to these raw datasets, in order to make them match to the months when the bill of sale is signed (and not to the months of tax revenue collection).

Using those corrected data, we compute the total tax revenue of the *Régime de Droit Commun* as follows:

$$Total\ Tax\ Revenue_{dt} = Total\ Tax\ Base_{dt} \times \tau_{dt}$$

where d corresponds to the *département*, t to the month and τ to the corresponding *département*'s RETT rate (i.e. either 3.80% or 4.50%). The same procedure is applied to the data of the *Régime Dérogatoire*, used in the robustness checks.

Our control variables are composed of the unemployment rates and of the number of new residential construction. (a) Data on the unemployment rates come from INSEE[9] and are quarterly data by *département* for metropolitan France over the period 2000 to 2015, and yearly data for the overseas *départements*, over the period 2000 to 2014, both seasonally adjusted. In order to estimate monthly data for the period January 2000 to December 2015, we made linear interpolation and extrapolation. (b) Data on the new residential construction (monthly building permits by *département*) come from the database Sit@del2, and are compiled by INSEE, from January 2001 to February 2016.

[7] $Number\ of\ Transactions_{dt} = Number\ of\ Transactions_{dt}\ (Fidji) \times \frac{Tax\ Bases_{dt}\ (MEDOC)}{Tax\ Bases_{dt}(Fidji)}$.

[8] Data on 4 *départements* are missing: the 3 *départements* of the Alsace-Moselle because we have no precise data due to its specific registration case (cf. box 1); and data on Mayotte (976), because it is a French *département* only since 2011.

[9] Institut National de la Statistique et des Etudes Economiques.

Finally, we also use other variables, not included as covariates because they do not fit to our panel data, in order to check for possible unobservables that could affect the sample groups differently over time. (1) The mortgage rate corresponds to the monthly rates in France for the new mortgage contracts of more than 1 year of the households, in the loan institutions and other financial institutions, from the *Banque de France*. (2) The estimated population on January 1 of each year in each *département*, using the INSEE annual census. (3) The property tax rates voted each year by the *départements*, from the DGFiP[10]. (4) Three local variables from the DGCL[11] in order to compute an index of "good administration" of the local governments, and compare the groups of treated to control. These variables are the salary cost, the operating revenue and the social spending, all per capita, by *département*. (5) Two local variables from INSEE, in order to make a comparison of the treated and control groups from their inherent housing market, which are: the share of social housing and secondary residence, within the total number of housings, by *département*.

Empirical Strategy

In order to estimate the effects of the RETT increase, we use a difference-in-differences framework (see Donald and Lang, 2007). To undertake our difference-in-differences design, we divide our sample in two groups, which are (1) the treatment group, composed of the *départements* that implemented the reform during the period from March 2014 to January 2015, (2) the control group, composed of the *départements* which had not implemented the reform at the estimated period[12] and of the 4 *départements* which remained at a RETT rate of 3.80% during our regressed time period: the (final) control group.

The specificity of our difference-in-differences framework, is that there is **an attrition of the control group** over the regressed period, and an increase of the treatment group (cf. table 1 and figure III).

Table 1 - Size of the treatment and control groups over the estimated period, by date of implementation

Period		Group		
		Treatment	Control	Total
Control	January 2012 - January 2014	0	92	92
Treatment	February 2014	58	34	92
	March 2014	76	16	92
	April 2014	78	14	92
	May 2014 - November 2014	85	7	92
	December 2014 - October 2015	88	4	92

Notes: numbers correspond to the number of *départements*.

[10] It corresponds exactly to the property tax rates on built real estate.
[11] Direction Générale des Collectivités Locales.
[12] Then from the treatment group, but when they are still at 3.80%, and not in their anticipation or retention periods.

We also subdivide the treatment group in five subgroups (cf. table A2-1), where the treated *départements* are clustered by date of implementation (i.e. March 2014, April 2014, May 2014, June 2014 and January 2015), in order to estimate whether there have been some different effects and heterogeneous shocks between all groups and subgroups. Finally, from the full sample of *départements* (i.e. 101), we remove 9 *départements*, because of a lack of data, or because we strongly suspect them to have heterogeneous housing market and/or unobservables that affect their housing market differently over time (see figure II). Those *départements* are the 3 *départements* of Alsace-Moselle Region, for the reasons already defined in previous sections, the 5 overseas *départements*, and finally the *département* of Paris (75).

Figure III - Maps of the Treatment and Control *Départements*

Figure III-A: February 2014

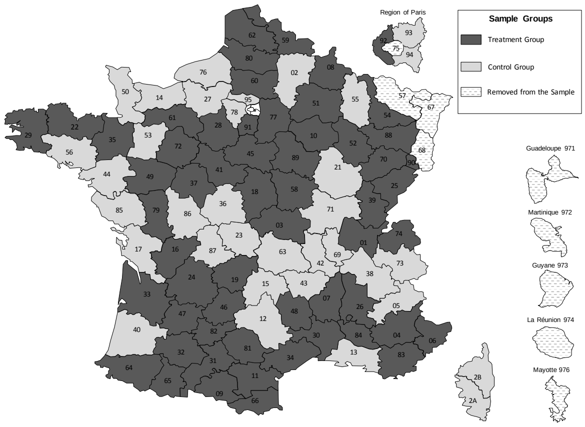


Figure III-B: March 2014

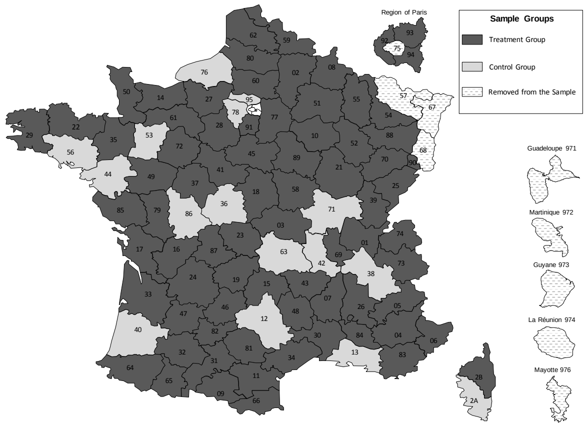


Figure III-C: April 2014

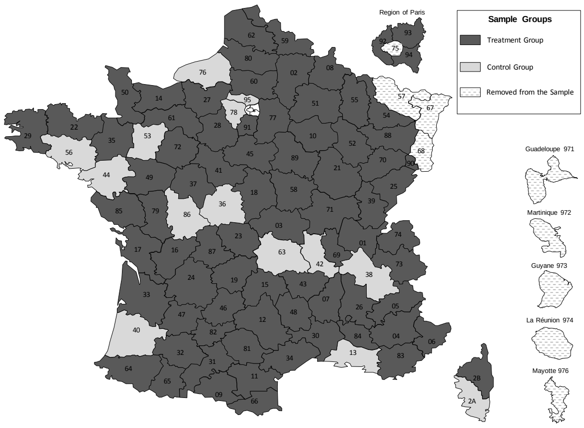


Figure III-D: May 2014 to November 2014

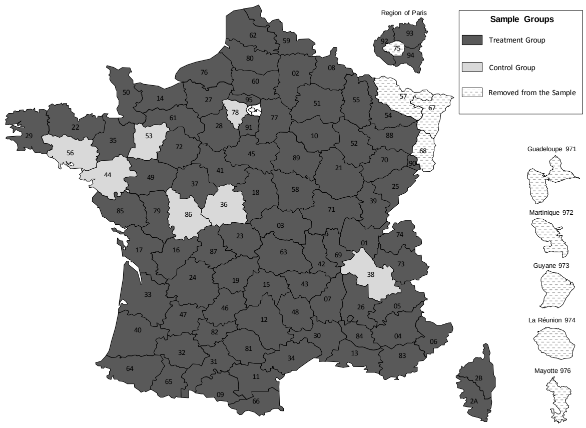
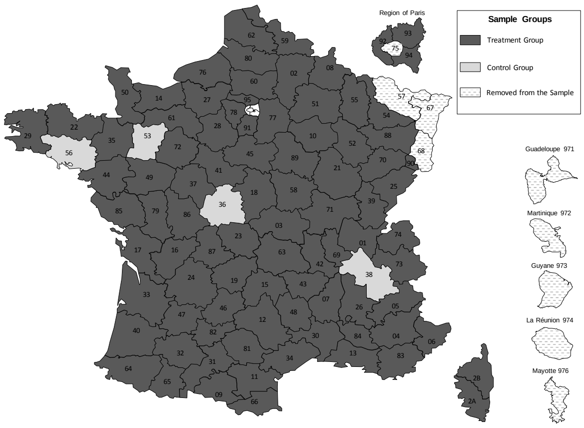


Figure III-E: December 2014 to October 2015

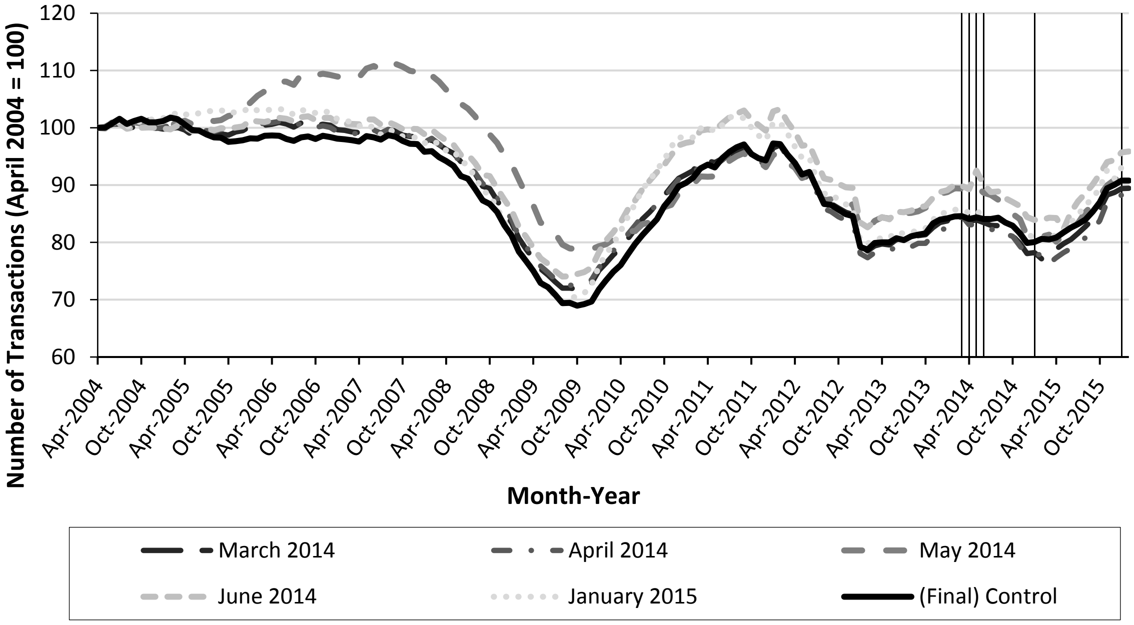


Source: authors' drawing.

Box 2

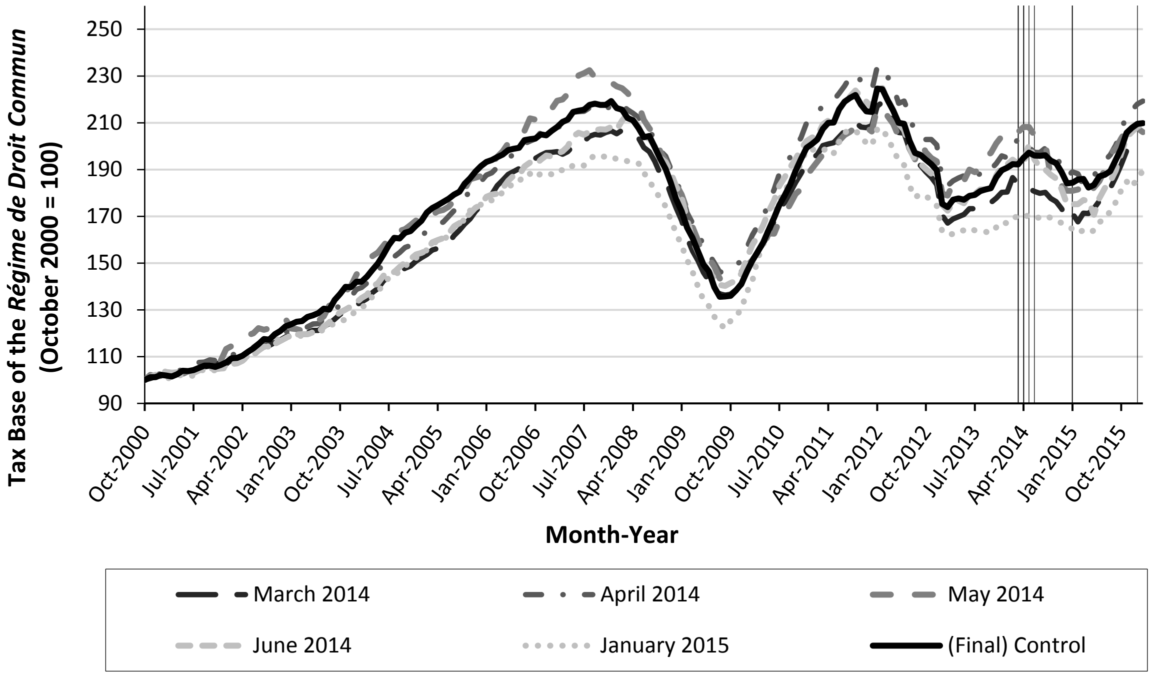
Difference-in-Differences Design

The most important hypothesis in the difference-in-differences framework is **the common trend assumption**, which assumes the evolution of the variable of interests would have been the same for the treatment and the control groups, without the reform. This assumption could be violated if there are some exogenous shocks or unobservables, which affect differently the groups over time. However, the trends of the outcome variables over the full sample period and the estimated period, show that they followed exactly the same trend and level until the reform, except for the *département* of Paris (75) and the overseas *départements* (DOM). There is also sometimes a slight difference in the trend of the May 2014 group (cf. figures II, A and B). Those observations deserve further enquiry, which we undertake in the robustness checks section. Nonetheless, our common trend assumption seems strongly plausible, and our difference-in-differences strategy can be implemented.



Notes: the number of transactions of the *départements* in each group are cumulated over the previous 12 months, and correspond to the number of transactions in the *régime de droit commun* registered by the DGFiP in each *département*. The *départements* of Moselle (57), Bas-Rhin (67), Haut-Rhin (68), Paris (75) and overseas *départements* (DOM), are excluded. Vertical lines correspond to the implementation dates.
Sources: CGEDD and DGFiP, Nombre de ventes immobilières taxées au taux de droits commun par département from 2004 to 2016.

Figure A - Monthly (12-month cumulative) number of transactions trends from April 2004 to February 2016, by sample groups



Notes: the total tax base of the *départements* in each group are cumulated over the previous 12 months, and corresponds to the tax revenue of the *régime de droit commun* registered by the DGFiP in each *département*, divided by the RETT's rates. The *départements* of Moselle (57), Bas-Rhin (67), Haut-Rhin (68), Paris (75) and overseas *départements* (DOM), are excluded. Vertical lines correspond to the implementation dates.
Sources: CGEDD and DGFiP, Assiettes des droits de mutation immobiliers par département from 2000 to 2016.

Figure B - Monthly (12-month cumulative) tax base of the *Régime de Droit Commun* trends from October 2000 to February 2016, by sample groups

We limit our estimations to the period from January 2012 to October 2015, for two reasons. First, we choose to start from January 2012 to avoid a possible noise from the reduction of the period of transmission of the bill of sale by the notaries from 2 months to 1 month (which occurred in 2011); even with the correction, it is an approximation. Second, we stop the study in October 2015 because on January 1, 2016, the Mayenne (53), one of the *département* of the (final) control group, implemented the tax increase. Consequently, its anticipation period should begin in November 2015 (date of its public announcement).

Box 3

Econometric Model with Anticipation

To estimate properly the anticipation and retention effects, we use a model proposed in Malani and Reif (2015) that allows to estimate properly effects of a treatment, when there is expectation and anticipation from the treated population, as it is the case in our evaluation. They propose two models: (1) the quasi-myopic model, and (2) the exponential discounting model. In this paper, our preferred model is the quasi-myopic model for two main reasons. First, the quasi-myopic model is easier to implement than the exponential discounting model and provides equal or better estimates when there is a finite and known period of anticipation, as in this natural experiment. Secondly, the exponential discounting model is harder to implement as it requires imposing structure on the error term, and most problematic, it assumes that people discount the future exponentially and have rational expectations, which is a strong assumption. Thus, this last model is more adapted when there are no restrictions on the time horizon for expectation.

Notice that if we assume endogeneity of the treatment, the exponential discounting model should be preferred for a long period of anticipation even known, as it requires only one instrument.

Month-Based Model

The aim of this model is to see the dynamics of the anticipation and the retention effects in the pre-treatment and post-treatment periods. We attempt to estimate how quickly the outcome variables react to the reform of the RETT, and how they evolve over time (e.g. how long the retention effect lasts). In order to perform these estimations, we use monthly leads and lags: 6 month leads for the anticipation effect, and 20 month lags for the retention effect (keeping in mind that 20 months is the full period of retention: March 2014 to October 2015). Another interesting feature of this model, is that it allows us to calibrate our regressions, in order to keep only the "true" months of anticipation, then to improve our estimations. The model with monthly regressors is shown in the following equation:

$$logY_{dt} = \alpha_d + \lambda_t + \sum_{j=1}^{6} \beta_{Aj}\, Anticipation_{d,t=T_d-j} + \sum_{k=0}^{19} \beta_{Rk}\, Retention_{d,t=T_d+k} + \rho X_{dt} + \epsilon_{dt} \quad (1)$$

Where T_d is equal to the implementation month of the reform in a *département* d.

$Anticipation_{d,t=T_d-j}$ is a dummy variable equal to 1 if the observation in a *département* d occurs during one of the first 6 months preceding the implementation month for that *département*, 0 otherwise. For instance, in the *départements* that implemented the reform in March 2014, the variable $Anticipation_{d,t=T_d-1} = 1$ in February 2014, $Anticipation_{d,t=T_d-2} = 1$ in January 2014, and so on. $Retention_{d,t=T_d+k}$ is a dummy variable equal to 1 if the observation in *département* d occurs during one of the first 20 months following the implementation month for that *département*, including that month, 0 otherwise. For instance, in the *départements* that implemented the reform in March 2014, the variable $Retention_{d,t=T_d+0} = 1$ in March 2014, $Retention_{d,t=T_d+1} = 1$ in April 2014, and so on. The anticipation effect in T_d - j is estimated by $\hat{\beta}_{Aj}$ and the retention effect in T_d + k is estimated by $\hat{\beta}_{Rk}$.

In addition, the models include (a) X_{dt}, a vector of time-variant control variables that could affect the outcome variable Y_{dt}, (b) α_d, which controls for *département* time-invariant characteristics (*département* fixed effects), and (c) λ_t, which controls for differences across months shared by the sample groups (month fixed effects). Finally, the error term ϵ_{dt}, clustered by *département*, and captures the $département \times month$ shocks to the variable Y_{dt} (see Wooldridge, 2005). This error term is assumed to be uncorrelated with the regressors, and problems could occur using a within estimator in a difference-in-differences framework, especially in the case of time-variant omitted variables that affect differently the sample groups.

Parsimonious Model

The following model is our "preferred one", because it must provide the most accurate estimations. Indeed, as developed in former sections, we assume that the anticipation effect is the largest the month just before the one of implementation, and the retention effect should last until the end of the period of estimation (i.e. October 2015). Regarding the coefficients of the estimations by month, as it will be developed latter in the result section, our assumption appears valid. That is why we develop a model with a dummy variable of one month of anticipation in T_d - 1. Actually, removing the months of anticipation with non-significant coefficients reduces the noise, and improves the estimates.

$$logY_{dt} = \alpha_d + \lambda_t + \beta_{A1}Anticipation_{d,t=T_d-1} + \beta_2 Retention_{d,t\in[T_d,T_d+19]} + \rho X_{dt} + \epsilon_{dt} \quad (2)$$

Where T_d is equal to the implementation month of the reform in a *département* d.

$Anticipation_{d,t=T_d-1}$ is a dummy variable equal to 1 only the month preceding the implementation month (i.e. T_d) in a *département* d, 0 otherwise. For instance, in the *départements* that increased the RETT in March 2014, $Anticipation_{d,t=T_d-1} = 1$ in February 2014; in the *départements* that implemented the reform in April 2014, $Anticipation_{d,t=T_d-1} = 1$ in March 2014. $Retention_{d,t\in[\tau_d,\tau_d+19]}$ is equal to 1 if a RETT increase is implemented in a *département* d, and that the month t belongs to its period of retention, 0 otherwise. The retention period lasts from the month of implementation of the reform, until the end of the sample period (i.e. October 2015). The anticipation effect in T_d - 1 is estimated by $\hat{\beta}_{A1}$ and the mean retention effect is estimated by $\hat{\beta}_2$.

Results

Box 4

Interpreting Results from a Log-Level Model

As all the models are estimated in log-level, and as our independent variables displayed in the tables of results are dummies, $(\exp(\beta) - 1) \times 100$ can be interpreted as: by how many percent the dependent variable Y has evolved in the situation where D = 1, compared to D = 0, (D represents the dummy variable of the treatment). An admissible approximation is $\beta \times 100\%$ when the coefficient is lower than 0.10.

Notice that all the results displayed in the tables are the raw estimated coefficients.

Anticipation Effect

The month-based model shows an increase of around 25% the month just before the implementation of the reform (i.e. T_d - 1), significant at the 1% level (cf. table 2 – A). None of the other anticipation periods coefficients are significantly different from zero, meaning that our assumption that the anticipation effect is concentrated over the month just before the date of implementation is empirically supported.

In the parsimonious model, we find that there was an anticipation in T_d – 1, of around 28%, significant at the 1% level (cf. table 3 – A).

To conclude, our estimates show an increase of approximately 28% in the volume of transactions during the month just before the implementation month (i.e. T_d - 1), meaning that buyers and sellers really agreed to escape the tax increase, and consequently, they brought forward the sale date of one month.

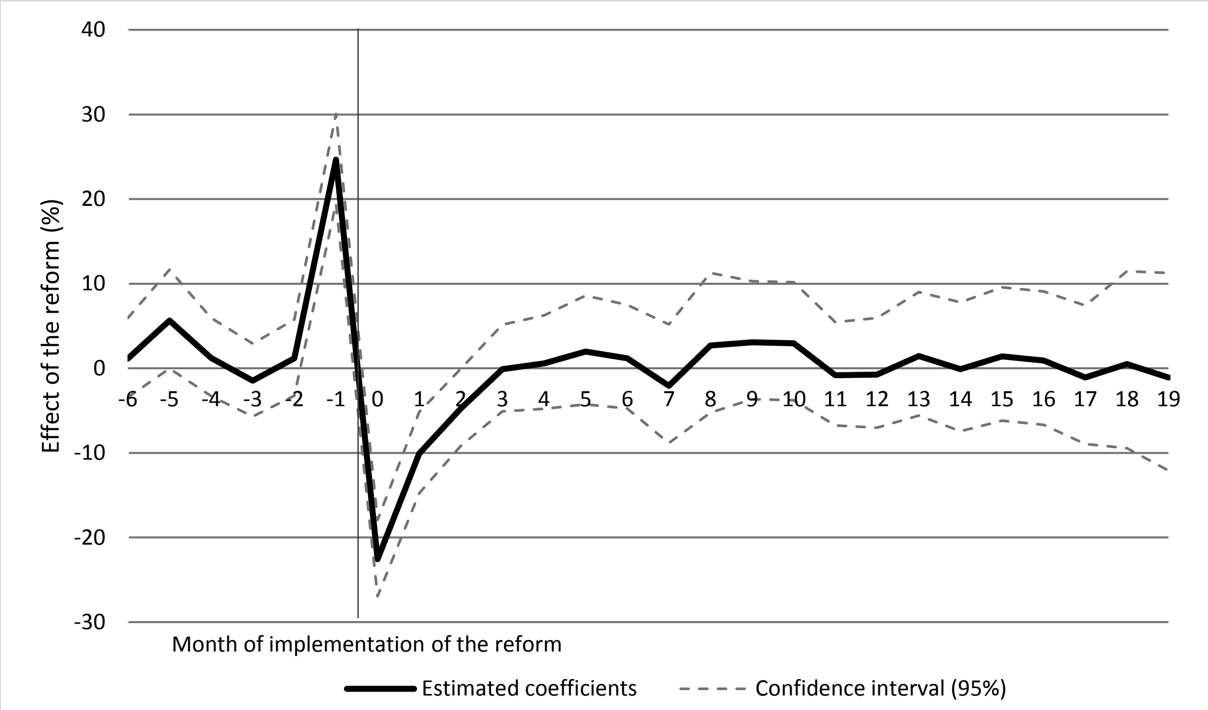
Retention Effect

The estimates with the month-based model show a large decrease in the tax bases the first month of the RETT increase (i.e. T_d), of around 23%, 10% the second month after reform (i.e. T_d + 1), and 5% the third month (i.e. T_d + 2), all significant at the 1% level (cf. table 2 – A). It proves that most of the retention effect took place the first months after the reform, and the effect mitigated later on. The cumulated decrease in the months following the reform, that is higher than the increase of 25% in T_d – 1, proves that it is not only due to the brought forward "missing" transactions.

The estimations with the parsimonious model shows that the average decrease during the estimated period is around 7%, and significant at the 1% level (cf. table 2 – B). By the way, the R^2 with the parsimonious model is almost as good as the extended model.

To conclude, most of the decrease in the total tax bases took place during the first months after the RETT increase, and is around 7% on average during the following year.

Figure IV – Effect of the reform on the volume of transactions, month by month before and after the implementation



Notes: month 0 corresponds to the month of implementation of the reform in a given *département*. As the month-based model is log-level, the "gross" coefficients should be calculated using the following method in order to be interpreted as a percentage, as shown in the graph above: (exp(β) – 1) × 100. These effects are estimated from monthly total tax bases by *département*, thus represent the change in the volume of transactions assuming that prices was unchanged.
Note for the reader: the number 1 on the abscissa axis means that one month after the implementation of the reform, the volume of transactions decreased by 10% in the *départements* which increased the RETT.
Sources: CGEDD and DGFiP, Assiettes des droits de mutation immobiliers par département, from 2000 to 2016; authors' computation.

Table 2 – A: Model (1) Estimates: Tax Bases

	Total Tax Bases of the *Régime de Droit Commun*
Anticipation Effect (T_d - 3) ($\hat{\beta}_{A3}$)	- 0.015 (0.022)
Anticipation Effect (T_d - 2) ($\hat{\beta}_{A2}$)	0.012 (0.022)
Anticipation Effect (T_d - 1) ($\hat{\beta}_{A1}$)	0.22*** (0.021)
Retention Effect (T_d) ($\hat{\beta}_{R0}$)	- 0.26*** (0.029)
Retention Effect (T_d + 1) ($\hat{\beta}_{R1}$)	- 0.11*** (0.027)

Retention Effect (T_d + 1) ($\hat{\beta}_{R2}$)	- 0.049*** (0.024)
Adjusted R²	0.65
Observations	4,232

Notes: for a better understanding, we present only estimates for the 3 months before and the 2 months after reform. This table reports estimates of equation 1, using within estimator. Outcome variable is in log in the estimation. In this table T_d corresponds to the month of implementation of the reform in a *département* d. Standard errors, given in brackets, are clustered by *département*. Stars indicate significance level: * p<0.1, ** p<0.05 and *** p<0.01.
Sources: CGEDD and DGFiP, Assiettes des droits de mutation immobiliers par département, INSEE, Construction de logements (Sit@del2) and Taux de chômage localisés, from 2012 to 2015.

Table 2 – B: Model (2) Estimates: Tax Bases

	Total Tax Bases of the *Régime de Droit Commun*
Anticipation Effect (T_d - 1) ($\hat{\beta}_{A1}$)	0.25*** (0.022)
Mean Retention Effect ($\hat{\beta}_2$)	- 0.077*** (0.026)
Adjusted R²	0.63
Observations	4,232

Notes: this table reports estimates of equation 2, using within estimator. Outcome variable is in log in the estimation. In this table T_d corresponds to the month of implementation of the reform in a *département* d. Standard errors, given in brackets, are clustered by *département*. Stars indicate significance level: * p<0.1, ** p<0.05 and *** p<0.01.
Sources: cf. table 2 - A.

Tax Revenue

If the volume of transactions and the sale price had not changed in response to the reform and that our common trend assumption were valid, we would have observed an increase in the tax revenue of the *Régime de Droit Commun* of the *départements,* by the same proportion as the RETT departmental's part rise (i.e. 18.42%), during the retention period. It is not what the estimated results reveal.

Both models show exactly the same increase in the anticipation period, compared to previous results obtained in tables 2 (cf. tables 3 – A and 3 - B). More interestingly, the month-based model shows a large decrease the implementation month of the reform (i.e. T_d), of around 8%, before an increase less than proportional to the RETT rise: 6% in T_d + 1 and 13% in T_d + 2, respectively significant at the 1%, 5% and 1% level (cf. table 3 – A). This large decrease in tax revenue in T_d, proves that there was a strong loss in the tax base.

The parsimonious model points out an average increase of tax revenue during the retention period of around 10%, significant at the 1% level (cf. table 3 – B). This increase is lower than the RETT increase (i.e. 18.42%).

To conclude, the estimates on the tax revenue confirm our previous results: there was both anticipation and retention effects, with a decrease either in the volume of transactions, the sale price, or both, during the post-reform period.

Table 3 – A: Model (1) Estimates: Tax Revenue

	Total Tax Revenue of the *Régime de Droit Commun*
Anticipation Effect (T_d - 3) ($\hat{\beta}_{A3}$)	- 0.015 (0.022)
Anticipation Effect (T_d - 2) ($\hat{\beta}_{A2}$)	0.012 (0.022)
Anticipation Effect (T_d - 1) ($\hat{\beta}_{A1}$)	0.22*** (0.021)
Retention Effect (T_d) ($\hat{\beta}_{R0}$)	- 0.087*** (0.029)
Retention Effect (T_d + 1) ($\hat{\beta}_{R1}$)	0.063** (0.027)
Retention Effect (T_d + 2) ($\hat{\beta}_{R2}$)	0.12*** (0.024)
Adjusted R^2	0.69
Observations	4,232

Notes: cf. table 2 - A.
Sources: cf. table 2 - A.

Table 3 – B: Model (2) Estimates: Tax Revenue

	Total Tax Revenue of the *Régime de Droit Commun*
Anticipation Effect (T_d - 1) ($\hat{\beta}_{A1}$)	0.25*** (0.022)
Mean Retention Effect ($\hat{\beta}_2$)	0.092*** (0.026)
Adjusted R^2	0.68
Observations	4,232

Notes: cf. table 2 - B.
Sources: cf. table 2 - A.

Tax Elasticities

In this section we are interested in quantifying the response of the outcome variables Y (i.e. total tax bases and total tax revenue) to a one-percent increase in the tax. To undertake this evaluation, we choose to estimate precisely the elasticities of the outcome variables to the RETT, using log-log models. We estimate the elasticity of the tax base to the total RETT rates, and the elasticity of the tax revenue to the RETT rates departmental's part. Therefore, in the following equations, τ corresponds either to the total RETT rates of the *Régime de Droit Commun* (i.e. 5.09% or 5.81%) or to the RETT departmental's part rates (i.e. 3.80% or 4.50%). This difference is done to disentangle the effect of the RETT on the tax bases, from the effect of the RETT on the *départements*' tax revenue, which depends only on their RETT's part.

The two models are estimated using the following equations:

$$logTax\ Bases_{dt} = \alpha_d + \lambda_t + \varepsilon.\, log\tau_{dt} + \rho X_{dt} + \epsilon_{dt} \quad (3)$$

In the equation 3, the coefficient ε corresponds to the elasticity of the tax bases to a 1% increase in the RETT of the *Régime de Droit Commun*, and can be defined as:

$$\varepsilon_{TB}^{\tau} = \frac{\partial logY}{\partial log\tau} \cong \frac{\Delta Y/Y}{\Delta\tau/\tau} \cong \frac{\Delta Y/Y}{0.1415} \quad (4)$$

(RETT of the *Régime de Droit Commun* increased by 0.72 percentage point (due to the increase of the departmental's part, see box 1), jumping from 5.09% to 5.81%, thus a rise of 14.15%).

$$logTax\ Revenue_{dt} = \alpha_d + \lambda_t + \varepsilon.\, log\tau_{dt} + \rho X_{dt} + \epsilon_{dt} \quad (5)$$

In equation 5, the coefficient ε corresponds to the elasticity of the tax revenue to a 1% increase in the **RETT departmental's part**, and can be defined as:

$$\varepsilon_{TR}^{\tau} = \frac{\partial logY}{\partial log\tau} \cong \frac{\Delta Y/Y}{\Delta\tau/\tau} \cong \frac{\Delta Y/Y}{0.1842} \quad (6)$$

(the *départements*' RETT increased by 0.7 percentage point, jumping from 3.80% to 4.50%, thus a rise of 18.42%).

Notice that in the estimations of equation (3) and (5), we removed the observations corresponding to the anticipation effect in T_d - 1, in order to avoid a bias due to the fact that the outcome variables begin to vary before the date of implementation T_d when the RETT changed.

The elasticity of the tax base to the tax is estimated at – 0.45, significant at the 5% level (cf. table 4 – A). The elasticity of the tax revenue to the departmental tax is estimated at 0.65, significant at the 1% level (cf. table 4 – B).

To conclude, these tax elasticities point out that an increase in the RETT induces a loss in the tax base, either through the volume of transactions, the sale price, or both. Since the value of the revenue elasticity is lower than 1 (0.65), the tax increase is still a good deal for the *départements*, in terms of tax revenue; meaning that the *départements*' revenue are still in the upward part of the Laffer curve.

Table 4 –A: Elasticity of the Tax Bases to the RETT

	Total Tax Bases of the *Régime de Droit Commun*
Elasticity (ε_{TB}^{τ})	- 0.45** (0.15)
Observations	4,144

Notes: this table reports estimates of equation 3, using within estimator. Variables are all in log. Standard errors, given in brackets, are clustered by *département*. Stars indicate significance level: * p<0.1, ** p<0.05 and *** p<0.01. Sources: cf. table 2 – A.

Table 4 – B: Elasticity of the Tax Revenue to the RETT Departmental's Part

	Total Tax Revenue of the *Régime de Droit Commun*
Elasticity (ε_{TR}^{τ})	0.65*** (0.15)
Observations	4,144

Notes: this table reports estimates of equation 5, using within estimator. Variables are all in log. Standard errors, given in brackets, are clustered by *département*. Stars indicate significance level: * p<0.1, ** p<0.05 and *** p<0.01. Sources: cf. table 2 – A.

Net effect

One may want to compute the net retention effect. Indeed, the retention effect which has been evaluated so far is magnified by the strong anticipation effect in $T_d - 1$ which creates a "loss" of transactions the following month, a gap in the data. The upshot is an increase of the estimated negative effect.

We proceed as follows to estimate the net effect. We reorganize the data set by building up moving-average bimonthly data. Thus, the transactions of the month of anticipation $T_d - 1$ and the following month T_d are added up, and it should remove the bias from the anticipated transactions. In tune with this, we bring forward the implementation month, which is now $T_d - 1$ and we re-estimate the parsimonious model on the period 2012 to 2015.

Using this methodology, we estimated that the tax basis decreased on average by 4.6% over a period of ten months after the reform (i.e. $T_d + 9$) and it is significant at the 5% level, and that the tax revenues increase by 12.7% over the same period (significant at the 1% level.) The elasticities' estimate do not change using the same data and method.

Our estimates of the net retention effect are somewhat lower than the estimates of the gross retention effect but it confirms that there was a clear negative effect on the volume of transactions, assuming no effect on prices in the post-reform period.

Discussion

Our study faces three main limitations. (1) We cannot use the 12-month cumulative data because it smooths the trends, reducing variability for the estimations, and the probability to capture an effect (see McKinnish, 2000). (2) A possible spillover effect, due the fact that some buyers could have voted with their feet may introduce a negative bias. More precisely, some buyers that were willing to buy real estate in a treated *département* neighboring a controlled *département*, in an area close to the border, could have chosen to buy in the controlled *département* because of the reform. In future studies using the micro data, this spillover effect could be estimated with a regression discontinuity design (see Hahn et al., 2001; Imbens & Lemieux, 2008), by clustering the neighboring treated and controlled *départements*. Defining a band of a few kilometers around the border to make the difference between treated and controlled, and between the housing markets in the center of the treated *départements*, compared to their housing market at this border. Nonetheless, we guess that this effect is small in magnitude, as real estate are heterogeneous goods, including their localization. (3) A possible lack of control variables, because we could not get all the desired data (monthly and by *départements*), especially the rent by *département*. Nevertheless, we attempt below to check for possible unobservables or heterogeneity between *départements*, and we assume that most of the possibly omitted covariates are time-invariant, thus captured by the fixed effects estimator.

One could also argue that there is a selection bias, because the *départements* that did not implement the RETT increase, are different in some points to the others. Such assumption seems not true when looking at the trends of the outcome variables (cf. figures II, A and B). Furthermore, when looking at the distributions and trends of the other local variables between groups (see figures C1 – I, C1 – II and C2 – I in the online add-on), there is no marked differences between the treated and control groups. Population, property tax rates, index of "good administration" and their inherent housing market show no differences between groups, and between them and the national statistics. Ultimately, what we are interested in here is the elasticity of supply and demand of buyers and sellers in real estate, while the choice of the reform implementation falls to the local councillors. Those decision makers are elected, and thus one could think that there is a correlation between them and the population (composed of the buyers and sellers). However, the point in case is to know whether those elasticities are correlated with the choice to implement the reform or not. Such independence assumption is difficult to test. Nonetheless, we attempt below to test for a possible bias from the political color of the local governments.

The main selection problem in natural experiments including a local fiscal policy reform, is the political color of the local councillors that decided to implement (or not) the tax increase. Indeed, in our study one could argue that left-wing or right-wing *départements* might have implemented the reform differently. However, the proportion of left-wing and right-wing *départements* which implemented the tax increase (or not), is exactly the same as the distribution of left-wing and right-wing *départements* among the whole country (cf. table 5). Furthermore, in the 2015 departmental elections, 28 *départements* switched from the left-wing to the right-wing, and only one switched from the right-wing to the left-wing. The new political distribution of the local councils is: 34 for the left-wing and 67 for the right-wing, which represent 34% and 66% of the total respectively. Therefore, the distribution has shifted between political wings, but no *département* has decided to decrease the RETT, while they have had the possibility to do so.

Table 5 - Distribution of the *départements*' political color, by implementation or non-implementation of the RETT increase

	Party		
	Left-Wing	Right-Wing	Total
RETT = 4.50% (increased) (%)	60.4 58[(1)]	39.6 38[(1)]	100 96[(1)]
RETT = 3.80% (unchanged) (%)	60 3[(1)]	40 2[(1)]	100 5[(1)]
Whole country (%)	60.4 61[(1)]	39.6 40[(1)]	100 101[(1)]

[(1)] Figures correspond to the number of *département* used to compute the percentages.

Notes: the party of the local government corresponds to the political color when the RETT increase was voted. Then, it corresponds either to the 2011 or 2015 departmental elections.
Coverage: Whole France. This computation was made among all the *départements* (i.e. 101).
Sources: Ministère de l'Intérieur and France-Politique, résultats des élections cantonales 2011 et départementales 2015.

Another point introducing a possible confounder is the 2013 reform of the *Taxe sur les Logements Vacants* (TLV), a tax on unoccupied housing: in the urban area where this tax was implemented, the number of transactions is supposed to have slightly increased. Nonetheless, we consider that this increase had no effect on our estimations because there are 24 *départements* within the treatment group (i.e. 88) and 1 *département* within the (final) control group (i.e. 4) which include urban area subject to the TLV. The distribution between each group is almost equal: 27% for the treatment group and 25% for the control group; for a total of 25% of *départements* with urban area submitted to the TLV, in the whole country. Furthermore, if the TLV actually increased the number of transactions, the distribution of this increase should be distributed equally between the months of the years 2013, 2014 and 2015. Indeed, the TLV is collected in November on unoccupied housing on January 1; then, there should be no sharp increase in a particular month: if the common trend assumption of the difference-in-differences holds, every group should be affected identically, and the TLV should not bias our estimations downward.

Robustness Checks

As suggested in Meyer (1995), we multiply the tests of robustness, in order to check the validity of our results. The estimates are reported in additional tables in the appendix 1.

Test on Possible Self-Selection: Logit

The binary logit is used to test whether there is a selection bias in the *départements* which implemented the tax increase, compared to the *départements* which did not (i.e. (final) control group). We regressed the dependent and control variables in a binary logit over the period from January 2001 to December 2013.

$$Y_d = \sum_{x=1}^{11} \beta_x X_d + \epsilon \quad (7)$$

In this equation, Y_d is equal to 1 if the *Département* implemented the tax increase, 0 otherwise; X_d corresponds to one of the dependent variables or control variables in a *département* d.

Estimates of the table A1 - 1 show that the coefficients are close to zero, meaning that there is no selection bias of the treated *départements* (i.e. the *départements* which increased the RETT, did not do it because of a possible difference in the explaining variables, compared to the *départements* which chose to remain at 3.80%): the choice to increase the tax is not correlated with these variables.

Placebo Test

The placebo test is used to check empirically the validity of the common trend assumption (see box 2), by regressing our variable of interests in a pre-reform period, and prior the period used in the standard regressions (i.e. January 2012 to October 2015). To implement this test, we use the period from January 2008 to October 2011, and regress the parsimonious model on the outcome variables, using the same groups. We define our dummies for anticipation and retention as being the same than in the previous estimations, but the period are moved back of four years. For instance, for the *départements* which implemented the reform on March 1, 2014, the dummy for the anticipation effect (T_d - 1) is equal to 1 in February 2010, 0 otherwise, and the dummy for the mean retention effect is equal to 1 from March 2010 to October 2011, 0 otherwise.

Table A1 - 2 shows no coefficients significantly different from zero at the 10% level, in all the variables of interest; meaning that the trends of the treatment and control groups are the same before the implementation of the reform. Thus, the common trend assumption appears valid.

Alternative Dependent Variables

The alternative dependent variable test is used to test whether the results are biased because there was an exogenous shock affecting the housing markets of the two groups differently. To do so, we substitute the outcome variables with other variables, which are presumably not affected by the reform. In order to implement this test, we use the *Régime Dérogatoire* as the dependent variable. The real estate market subject to the *Régime Dérogatoire* is assumed to be not influenced by the reform, and are the closest data that we can compare to the *Régime de Droit Commun*.

Results of table A1 – 3 show no coefficient significantly different from zero at the 10% level, for the substitute outcomes. Then, it appears that our results are not biased: there was no shock affecting differently the housing markets of the two groups during the regressed period.

Estimations Using Different Period and Sample

This fourth test is done to check the validity of our results to the choice of the period and sample groups. In order to implement this test, we reduce the regressed period from January 2013 to October 2014. Doing this, we reduce the pre-reform period and we increase the (final) control group, as the January 2015 group is now never treated (its period of treatment begins in December 2014). Then, our (final) control group is now composed of 7 *départements*, against 4 in previous regressions.

Table A1 - 4 shows estimates close to the ones found in the main estimations. Indeed, regressing different period and sample, we see an anticipation effect in $T_d - 1$ of 27%, compared to 28% before, and a decrease in the tax base in the retention period of 13%, compared to 7% before. The tax revenue estimation shows no changes (0%), meaning a loss in the tax base, compared to a 10% increase in previous estimations. However, this decrease in the estimates can be anticipated as the retention effect decreases over time, as proved in the estimations by month. Then, reducing the regressed period, we increase mathematically the estimated retention effect. Therefore, our first estimates appear robust to the choice of the period and to different sample.

Changes in Local Economic Conditions

As the results that we find could be impacted by an exogenous economic shock, affecting the sample groups differently, we test for this kind of changes in the local economic conditions.

To implement this test, we use the same method as in Benzarti and Carloni (2015), with interaction variables between a dummy variable defining in which group belongs the *département* d, and the monthly unemployment rate of this *département* d.

The first equation uses two sample groups: Treated and (Final) Control;

$$\begin{aligned} logY_{dt} = \alpha_d + \lambda_t + \beta_{A1} Anticipation_{d,t=T_d-1} + \beta_2 Retention_{d,t\in[T_d,T_d+19]} \\ + \gamma_1(Treated \times URate_{dt}) + \gamma_2(Control \times URate_{dt}) + \rho X_{dt} \\ + \epsilon_{dt} \qquad (8) \end{aligned}$$

and the second one uses the decomposition of the treated *départements* clustered by subgroups according to the implementation date (see table A2 – 1):

$$\begin{aligned} logY_{dt} = \alpha_d + \lambda_t + \beta_{A1} Anticipation_{d,t=T_d-1} + \beta_2 Retention_{d,t\in[T_d,T_d+19]} \\ + \sum_{Subgroup=March}^{G} \gamma_{Subgroup}(Subgroup \times URate_{dt}) \\ + \gamma_{Control}(Control \times URate_{dt}) + \rho X_{dt} \\ + \epsilon_{dt} \qquad (9) \end{aligned}$$

where G = [March, April, May, June, January] is the set of treated subgroups.

Results for both models presented in tables A1 – 5 and A1 – 6 show no differences between the estimates and our main results for the anticipation effect. Estimates of the retention effect are slightly different, they show a decrease of around 10% in the tax base, and an increase of around 7% in the tax revenue. We can therefore conclude that our estimates are robust, and that no exogenous local economic shock affected differently our groups.

Removing Possibly Heterogeneous Groups

When looking at the trends on the outcome variables (cf. figures II, A and B), we see some different trends or levels in the January 2015 and May 2014 groups. Thus, we may suspect a possible heterogeneity or unobservables that affect them differently over time. In order to test this hypothesis, we estimate our coefficients removing either January 2015 or May 2014 group or both, from the estimated sample.

Removing May 2014 group increases slightly the estimates of the anticipation effect, estimated at 30%, but does not change our estimates of the retention effect (see table A1 – 7). Removing January 2015 group shows the same results for the anticipation effect as removing May 2014. For the retention effect (see table A1 – 8), it increases the coefficients of the tax base, estimated at - 9%, and decreases the coefficient of the tax revenue, estimated at + 8% (compared to – 7% and + 10% in the main results). Finally, removing both groups from the estimated sample increases the coefficients of the anticipation effect, estimated at 31%, and shows the same results as removing January 2015 group for the retention period (see table A1 – 9). We can conclude that our findings are robust to the choice of the sample, and to a possible bias from heterogeneous *départements*.

Conclusion

To conclude, in line with the economic literature, we find evidence that the RETT increase had an impact on the housing market. Buyers and sellers anticipated the tax rise, and brought forward their sale date to avoid the increase. We prove that they had a behavioural response, and that this anticipation increased the volume of transactions by 28%, the month preceding the one of the implementation of the reform. Furthermore, we estimate another negative effect of the tax increase: the retention effect, of around 7% on average on the volume of transactions, and we estimate the average net effect to a drop of the transactions of 4.6% over a period of ten months following the implementation date, assuming no sale price changes. Such assumption appears realistic, as the RETT system in France is proportional and the RETT's payment accrues to the buyer. Then, buyers and sellers have more interest in changing the sale date rather than the sale price (see Benjamin et al., 1993; Davidoff and Leigh, 2013; Slemrod et al, 2016); which seems confirmed when looking at the large anticipation effect. Nonetheless, it is difficult without non-cumulative monthly data on the number of transactions and hedonic estimation, to disentangle the effect on the volume of transactions, from the effect on price. Moreover, we find that the elasticity of the tax revenue to the RETT is 0.65, meaning that there is a loss of the tax base, which reduces the gains for the local budgets.

This evaluation can be extended through three ways: (1) using the non-cumulative monthly data on the number of transactions, (2) doing a precise estimation of the price effect through hedonic model (using BIEN and Perval databases), and (3) implementing a regression discontinuity design to estimate the possibility that buyers could have voted with their feet (i.e. spillover effect).

Finally, our results might be used to discuss the impact of future RETT's reforms, and anticipated the effect on the housing market, in particular on buyers and sellers' behaviour. Even if the RETT rise was a "good" deal for the *départements* in terms of tax revenue, the distorting effect of the tax' reform was proved: some people who could have become owners or moved from a place to another, did not because of the reform (i.e. lock-in effect). Consequently, in line with the findings of Van Ommeren and Van Leuvensteijn (2005), we conclude that the RETT increase has an important negative impact on mobility and well-being.

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Appendix A1: Robustness Checks Results

Table A1 - 1: Test on Possible Self-Selection: Logit, January 2001 to December 2013

Variables	Marginal Effects
Total Tax Bases of the *Régime de Droit Commun*	9.08e - 11*** (2.36e - 11)
Total Tax Bases of the *Régime de Dérogatoire*	1.62e - 10 *** (5.64e - 11)
Unemployment Rate	0.0138147 *** (0.0007083)
Number of New Residential Construction	- 5.32e - 06 *** (4.94e - 07)
Observations	14,352

Notes: this table reports estimates of equation 7, using binary logit. Treated *départements* are equal to 1, and controls to 0. Stars indicate significance level: * p<0.1, ** p<0.05 and *** p<0.01. Coefficients represents marginal effects and not odd ratios, then they can be interpreted in term of magnitude. Standard errors are given in brackets.
Sources: CGEDD and DGFiP, Assiettes des droits de mutation immobiliers par département, INSEE, Construction de logements (Sit@del2) and Taux de chômage localisés, from 2001 to 2013.

Table A1 - 2: Placebo Test: Period January 2008 to October 2011

	Total Tax Bases of the *Régime de Droit Commun*	Total Tax Revenue of the *Régime de Droit Commun*
Anticipation Effect (T_d - 1) ($\hat{\beta}_{A1}$)	- 0.013 (0.023)	- 0.013 (0.023)
Mean Retention Effect ($\hat{\beta}_2$)	- 0.030 (0.030)	- 0.030 (0.030)
Adjusted R^2	0.73	0.73
Observations	4,232	4,232

Notes: cf. table 2 - B.
Sources: CGEDD and DGFiP, Assiettes des droits de mutation immobiliers par département, INSEE, Construction de logements (Sit@del2) and Taux de chômage localisés, from 2008 to 2011.

Table A1 - 3: Alternative Dependent Variable: *Régime Dérogatoire*

	Total Tax Bases of the *Régime Dérogatoire*	Total Tax Revenue of the *Régime Dérogatoire*
Anticipation Effect (T_d - 1) ($\hat{\beta}_{A1}$)	0.045 (0.057)	0.045 (0.057)
Mean Retention Effect ($\hat{\beta}_2$)	- 0.021 (0.046)	- 0.021 (0.046)
Adjusted R^2	0.42	0.42
Observations	4,232	4,232

Notes: cf. table 2 - B.
Sources: cf. table 2 – A.

Table A1 - 4: Estimations Using Different Period and Sample: January 2013 to October 2014

	Total Tax Bases of the *Régime de Droit Commun*	Total Tax Revenue of the *Régime de Droit Commun*
Anticipation Effect (T_d - 1) ($\hat{\beta}_{A1}$)	0.24*** (0.020)	0.24*** (0.020)
Mean Retention Effect ($\hat{\beta}_2$)	- 0.14*** (0.022)	0.033 (0.022)
Adjusted R²	0.63	0.63
Observations	2,024	2,024

Notes: cf. table 2 - B.
Sources: CGEDD and DGFiP, Assiettes des droits de mutation immobiliers par département, INSEE, Construction de logements (Sit@del2) and Taux de chômage localisés, from 2013 to 2014.

Table A1 - 5: Change in Local Economic Conditions: Controlling for the Local Unemployment Rate

	Total Tax Bases of the *Régime de Droit Commun*	Total Tax Revenue of the *Régime de Droit Commun*
Anticipation Effect (T_d - 1) ($\hat{\beta}_{A1}$)	0.24*** (0.021)	0.24*** (0.020)
Mean Retention Effect ($\hat{\beta}_2$)	- 0.10*** (0.027)	0.066** (0.027)
Treated × URate ($\hat{\gamma}_1$)	0.0082 (0.012)	0.0082 (0.012)
Control × URate ($\hat{\gamma}_2$)	- 0.11*** (0.041)	- 0.11*** (0.041)
Adjusted R²	0.64	0.68
Observations	4,232	4,232

Notes: this table reports estimates of equation 8, using within estimator. Outcome variable is in log in the estimation. In this table T_d corresponds to the month of implementation of the reform in a *département* d. Standard errors, given in brackets, are clustered by *département*. Stars indicate significance level: * p<0.1, ** p<0.05 and *** p<0.01.
Sources: cf. table 2 – A.

Table A1 - 6: Change in Local Economic Conditions: Controlling for the Local Unemployment Rate, by Subgroups

	Total Tax Bases of the *Régime de Droit Commun*	Total Tax Revenue of the *Régime de Droit Commun*
Anticipation Effect (T_d - 1) ($\hat{\beta}_{A1}$)	0.24*** (0.021)	0.24*** (0.020)
Mean Retention Effect ($\hat{\beta}_2$)	- 0.11*** (0.026)	0.063** (0.026)
March × URate ($\hat{\gamma}_{March}$)	0.0056 (0.013)	0.0056 (0.013)
April × URate ($\hat{\gamma}_{April}$)	0.019 (0.017)	0.019 (0.017)
May × URate ($\hat{\gamma}_{May}$)	0.062* (0.036)	0.062* (0.036)
June × URate ($\hat{\gamma}_{June}$)	- 0.00095 (0.017)	- 0.00095 (0.017)
January × URate ($\hat{\gamma}_{January}$)	- 0.042 (0.023)	- 0.042 (0.023)
Control × URate ($\hat{\gamma}_{Control}$)	- 0.12*** (0.041)	- 0.12*** (0.041)
Adjusted R^2	0.64	0.68
Observations	4,232	4,232

Notes: this table reports estimates of equation 9, using within estimator. Outcome variable is in log in the estimation. In this table T_d corresponds to the month of implementation of the reform in a *département* d. Standard errors, given in brackets, are clustered by *département*. Stars indicate significance level: * p<0.1, ** p<0.05 and *** p<0.01.
Sources: cf. table 2 – A.

Table A1 - 7: Removing Possibly Heterogeneous Groups: Removing May 2014 Group

	Total Tax Bases of the *Régime de Droit Commun*	Total Tax Revenue of the *Régime de Droit Commun*
Anticipation Effect (T_d - 1) ($\hat{\beta}_{A1}$)	0.26*** (0.022)	0.26*** (0.022)
Mean Retention Effect ($\hat{\beta}_2$)	- 0.074*** (0.026)	0.095*** (0.026)
Adjusted R^2	0.63	0.68
Observations	4,140	4,140
January 2015 Group	Yes	Yes
May 2014 Group	No	No

Notes: cf. table 2 - B.
Sources: cf. table 2 – A.

Table A1 - 8: Removing Possibly Heterogeneous Groups: Removing January 2015 Group

	Total Tax Bases of the *Régime de Droit Commun*	Total Tax Revenue of the *Régime de Droit Commun*
Anticipation Effect (T_d - 1) ($\hat{\beta}_{A1}$)	0.26*** (0.021)	0.26*** (0.021)
Mean Retention Effect ($\hat{\beta}_2$)	- 0.094*** (0.030)	0.075** (0.030)
Adjusted R^2	0.63	0.68
Observations	4,094	4,094
January 2015 Group	No	No
May 2014 Group	Yes	Yes

Notes: cf. table 2 - B.
Sources: cf. table 2 – A.

Table A1 - 9: Removing Possibly Heterogeneous Groups: Removing both May 2014 and January 2015 Groups

	Total Tax Bases of the *Régime de Droit Commun*	Total Tax Revenue of the *Régime de Droit Commun*
Anticipation Effect (T_d - 1) ($\hat{\beta}_{A1}$)	0.27*** (0.022)	0.27*** (0.022)
Mean Retention Effect ($\hat{\beta}_2$)	- 0.091*** (0.030)	0.078** (0.030)
Adjusted R^2	0.63	0.68
Observations	4,002	4,002
January 2015 Group	No	No
May 2014 Group	No	No

Notes: cf. table 2 - B.
Sources: cf. table 2 – A.

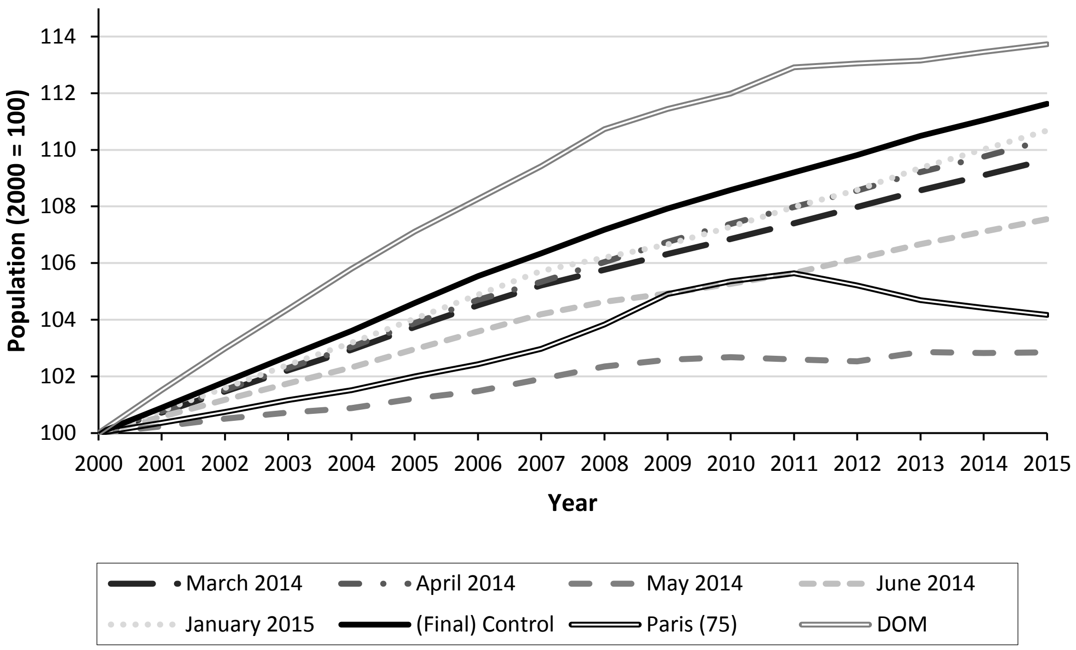
Appendix A2: Table A2 - 1: Sample Groups with Subdivision of the Treatment Group[(1)] in Subgroups by Date of Implementation

March 2014				April 2014		May 2014	
N°	*Département*	N°	*Département*	N°	*Département*	N°	*Département*
01	Ain	47	Lot-et-Garonne	02	Aisne	12	Aveyron
03	Allier	48	Lozère	05	Hautes-Alpes	71	Saône-et-Loire
04	Alpes-de-Haute-Provence	49	Maine-et-Loire	14	Calvados		
06	Alpes-Maritimes	51	Marne	15	Cantal		
07	Ardèche	52	Haute-Marne	17	Charente-Maritime		
08	Ardennes	54	Meurthe-et-Moselle	2B	Haute-Corse		
09	Ariège	58	Nièvre	21	Côte-d'Or		
10	Aube	59	Nord	23	Creuse		
11	Aude	60	Oise	27	Eure		
16	Charente	61	Orne	43	Haute-Loire		
18	Cher	62	Pas-de-Calais	50	Manche		
19	Corrèze	64	Pyrénées-Atlantiques	55	Meuse		
22	Côtes-du-Nord	65	Hautes-Pyrénées	69	Rhône		
24	Dordogne	66	Pyrénées-Orientales	73	Savoie		
25	Doubs	70	Haute-Saône	85	Vendée		
26	Drôme	72	Sarthe	87	Haute-Vienne		
28	Eure-et-Loir	74	Haute-Savoie	93	Seine-St-Denis		
29	Finistère	77	Seine-et-Marne	94	Val-de-Marne		
30	Gard	79	Deux-Sèvres				
31	Haute-Garonne	80	Somme				
32	Gers	81	Tarn				
33	Gironde	82	Tarn-et-Garonne				
34	Hérault	83	Var				
35	Ille-et-Vilaine	84	Vaucluse				
37	Indre-et-Loire	88	Vosges				
39	Jura	89	Yonne				
41	Loir-et-Cher	90	Territoire-de-Belfort				
45	Loiret	91	Essonne				
46	Lot	92	Hauts-de-Seine				
June 2014		January 2015		(Final) Control		Removed from the Sample	
N°	*Département*	N°	*Département*	N°	*Département*	N°	*Département*
13	Bouches-du-Rhône	44	Loire-Atlantique	36	Indre	57	Moselle
2A	Corse-du-Sud	78	Yvelines	38	Isère	67	Bas-Rhin
40	Landes	86	Vienne	53	Mayenne	68	Haut-Rhin
42	Loire			56	Morbihan	75	Paris
63	Puy-de-Dôme					971	Guadeloupe
76	Seine-Maritime					972	Martinique
95	Val-d'Oise					973	Guyane
						974	La Réunion
						976	Mayotte

[(1)] Treatment group is composed of the subgroups: March 2014, April 2014, May 2014, June 2014 and January 2015.

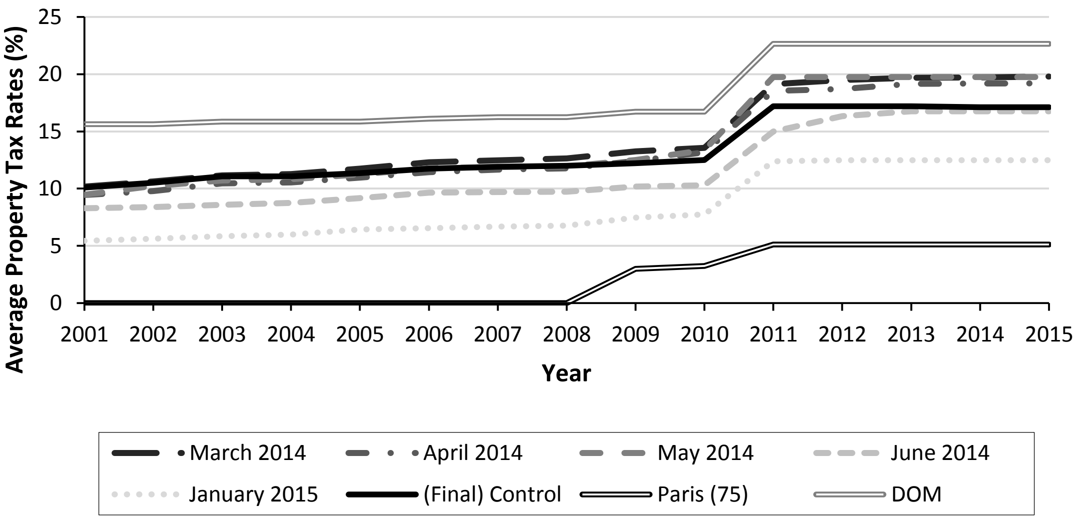
Online Add-on C1: Population and Property Tax Distributions

Figure C1 – I: Yearly Population Trends from 2000 to 2015, by Sample and Date of Implementation Groups



Notes: the population corresponds to the estimated population on 1 January of each year in each *département*. The *départements* of Moselle (57), Bas-Rhin (67), Haut-Rhin (68) and Mayotte (976), are excluded.
Source: INSEE, Estimations de la population, from 2000 to 2015.

Figure C1 – II: Yearly Departmental Property Tax from 2001 to 2015, by Sample and Date of Implementation Groups



Notes: the property tax rates corresponds to the property tax rates on built real estate, voted each year by the *départements*. The *départements* of Moselle (57), Bas-Rhin (67), Haut-Rhin (68) and Mayotte (976), are excluded. The sharp increase in 2011 corresponds to the abolition of the business tax.
Source: DGFiP, Impôts locaux, from 2001 to 2015.

Online Add-on C2: Figure C2 – I: Distribution of the Local Variables by Implementation or Non-Implementation of the RETT Increase

Figure C2-I, A: Salary Cost (Per Capita)

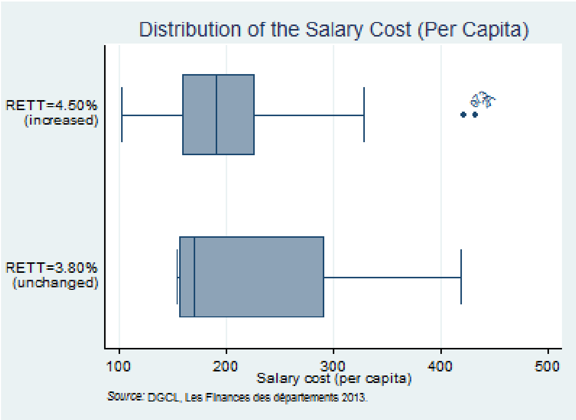


Figure C2-I, B: Operating Revenue (Per Capita)

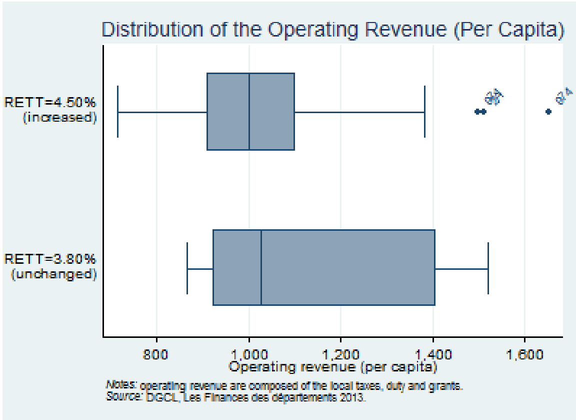


Figure C2-I, C: Social Spending (Per Capita)

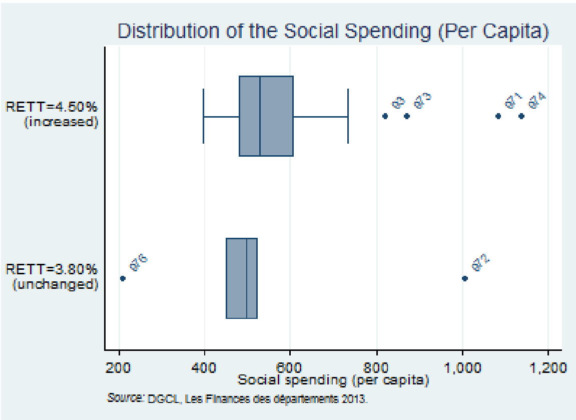


Figure C2-I, D: Share of Social Housing

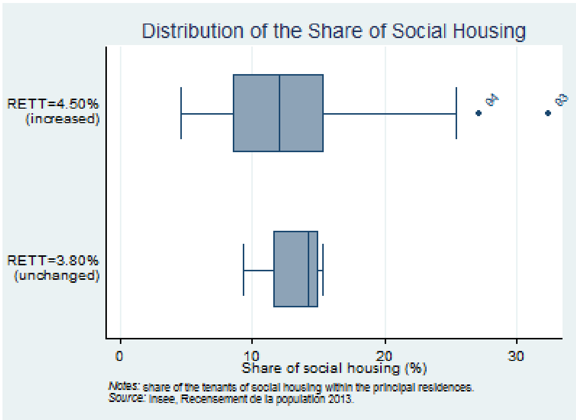
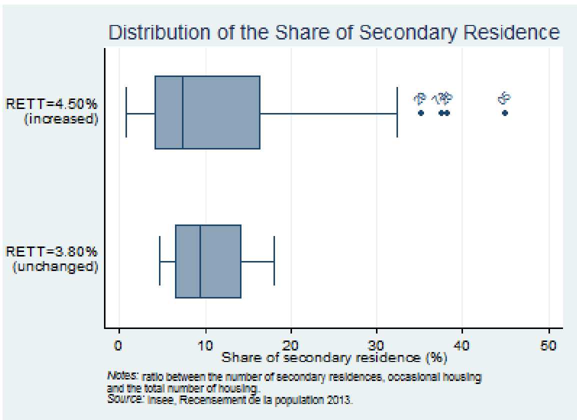


Figure C2-I, E: Share of Secondary Residence



Source: authors' drawing.