The impact of the 2014 increase in the real estate transfer taxes on the French housing market

Guillaume Bérard* and Alain Trannoy*

Abstract – This paper estimates the effects of an increase in the share of the real estate transfer taxes (RETT) rates going to the French *départements* from 3.80% to 4.50%. Not all the *départements* voted the RETT increase on the same date, which is the starting point of a natural experiment. Using a difference-in-differences design, we estimate two main effects. (1) An anticipation effect, one month before the implementation of the reform, in order to avoid the RETT increase. (2) A retention effect in the post-reform period. In the end, the net effect (retention minus anticipation) corresponds to an average drop in transactions of around 6% over the first three months after the reform, that is, approximately 15,000 transactions lost at national level. If we find a short term effect of the reform, we do not find evidence of a medium- or long-term effect.

JEL Classification: H71, R21, R31, R51
Keywords: local government, real estate market, transfer taxes, natural experiment

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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 ownership transfers of real estate or land (Box 1). The RETT are an important source of revenue for the French *départements*: they represent around €10 billion per year. However, while the RETT as the other transaction costs (notary and experts' fees) cannot be financed through mortgages and must be paid first by the buyer and in addition to the downpayment, the possible negative impact on the housing market was not evaluated or even discussed when the reform was implemented. In this study, we use open access data on the monthly number of transactions and real estate tax bases by *département* 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. Note that in the very short-run, this effect is composed of re-timing due to the anticipated transactions (intertemporal substitution by those who would have purchased a real estate property anyway), and of extensive margin responses (those who would have purchased a property in the absence of the reform).What we are looking for is the behavioral response in terms of timing and extensive margin of the agents (i.e. buyers and sellers). Finally, we evaluate the tax elasticity of the tax bases to the RETT.

We estimate that there was an anticipation effect of 26% 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. We also estimate the average monthly retention effect for the three months following the tax increase at around 14% of the volume of transactions (assuming no effect on the sale price) – with 49% of this loss due to a pure extensive margin effect (and not to re-timing) – meaning that the tax increase had a negative impact on the housing market. All in all, the average monthly net effect corresponds to a drop in transactions of around 6% over a period of three months following the implementation date. None of the estimates after these three months are significantly different from zero. The corresponding rough estimate of the lost transactions is around 15,000. Therefore, there is some compelling evidence of a strong short-term effect, but no medium- or long-term effect. Furthermore, we estimate that the elasticity of the tax bases to the tax increase is about - 0.42, meaning that tax bases decreased by 0.42% for a 1% increase in the RETT rate (i.e. there is a loss in the tax bases which reduces the gains of tax revenues for the local budgets). Computing the Laffer rate, we conclude that *départements'* tax revenues are still on the increasing side of the Laffer curve. Note that our results are valid in a partial equilibrium framework: we do not estimate the possible general equilibrium resulting from the distortion of the housing market. Finally, we run a series of robustness checks such as a placebo test, a self-selection test and control for possible changes in local economic conditions, confirming that our results are unbiased and robust.

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 specificity of the

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 3).*

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 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 and Kan (1996), is not directly related to the RETT's impact, but more generally to residential mobility, and to the decision of moving, and 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 lump-sum transaction costs in households' mobility decisions, and that housing price increases seem to discourage renters from moving and from owning after moving. Then, housing price appreciation is likely to have

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 (*VEFA*)), 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* – 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 territory. Composed of the *départements* of Moselle (57), Bas-Rhin (67) and Haut-Rhin (68), this territory has its own registration utility: the *Livre foncier*. This situation is due to the particular legal status of this territory, inherited from the German annexation of 1870.

It is important to note 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 behavior 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)

strong effects on the housing rental market. 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, but they did not conclude about the time length of this effect. They deduce that transaction costs could imply lock-in effects, leading to a negative impact on the housing market and the labor market. Their conclusion is that a decrease or an abolition of the buyer's transaction costs would improve home-owners' mobility.

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 could not consider a potential anticipation effect. Combining difference-in-differences and regression discontinuity designs on a large sample, they estimate that the new tax decreased the volume of transactions by 14%, 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.

Davidoff and Leigh (2013) assess 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 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.

Slemrod *et al.* (2017) estimate the behavioral 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). Finally, Best and Kleven (2018) 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.

To summarize, transaction costs have a negative impact on residential mobility. They lead to suboptimal equilibrium on the housing market: they distort owners and renters' choices between staying or moving and renting or owning. Empirical literature on RETT mainly resorts on difference-in-differences and regression discontinuity designs using the features of quasi-natural experiments. They proved that transfer taxes are highly distortionary in the short-run, in terms of volume, price and timing of transactions. Medium- and long-run effects are more ambiguous, and depend on the specificity of each legal system and local conditions. The specificity of the French case is that the RETT are proportional, they accrue to the buyer only, and it is a potential widespread reform that impacted the whole country.

Context of the reform in France

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, State grants decreased drastically. 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. Then, with 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'État 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 the *départements*, then it is as random. 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 the 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 anticipate 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, allowing the *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 gone 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 a rate of 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, in practice, every *département* that chose to raise the RETT has increased them by the maximum amount (i.e. up to 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*[7] are still currently[8] at 3.80%.

From the full sample of *départements* (i.e. 101), we remove 9 *départements*, because of 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. Those *départements* are the 3 *départements* of Alsace-Moselle territory for the reasons already defined above (cf. also Box 1), the 5 overseas *départements*, and finally the *département* of Paris (75). Figure I

4. Revenu de solidarité active (RSA), 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.*
6. *Except Côte d'Or (21) which increased them until 4.45%; thus, we consider it as being at 4.50% in the estimates.*
7. *Indre (36), Isère (38), Morbihan (56), and two overseas* départements *Martinique (972) and Mayotte (976).*
8. *May 2017.*

shows a map of the implementation schedule of the reform.

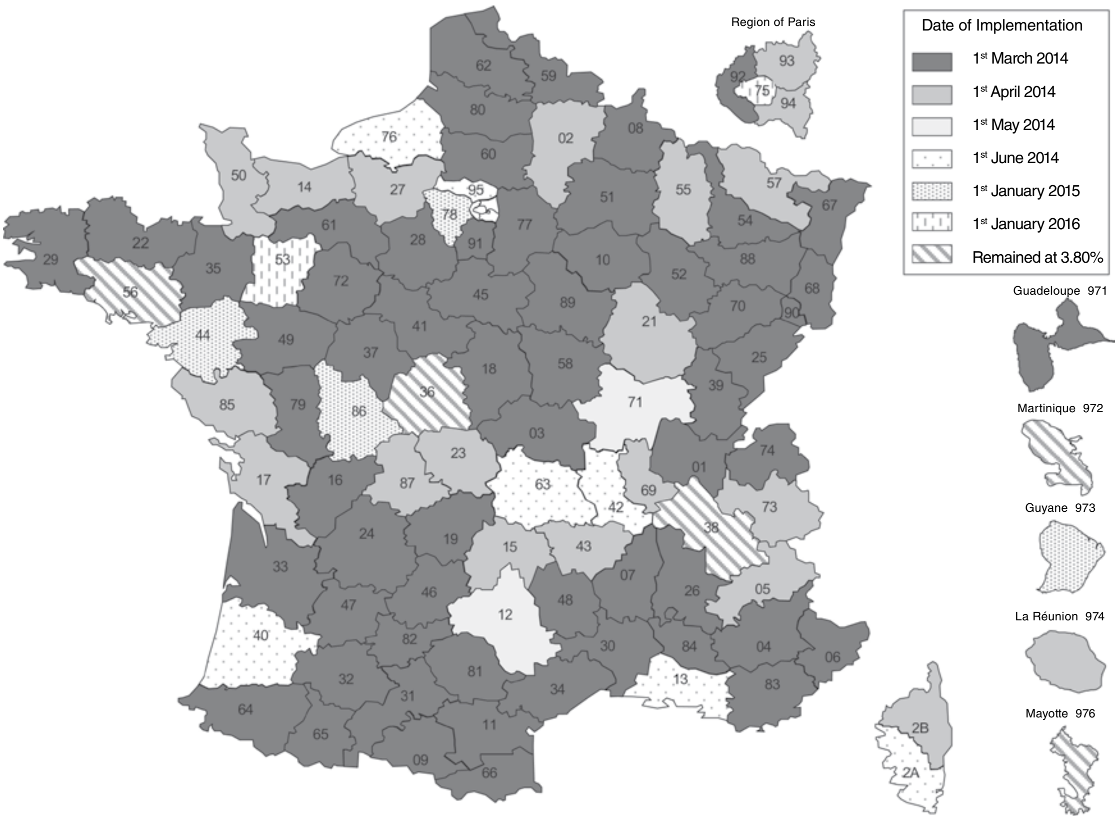
Purpose of the evaluation

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 behavior 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 (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.

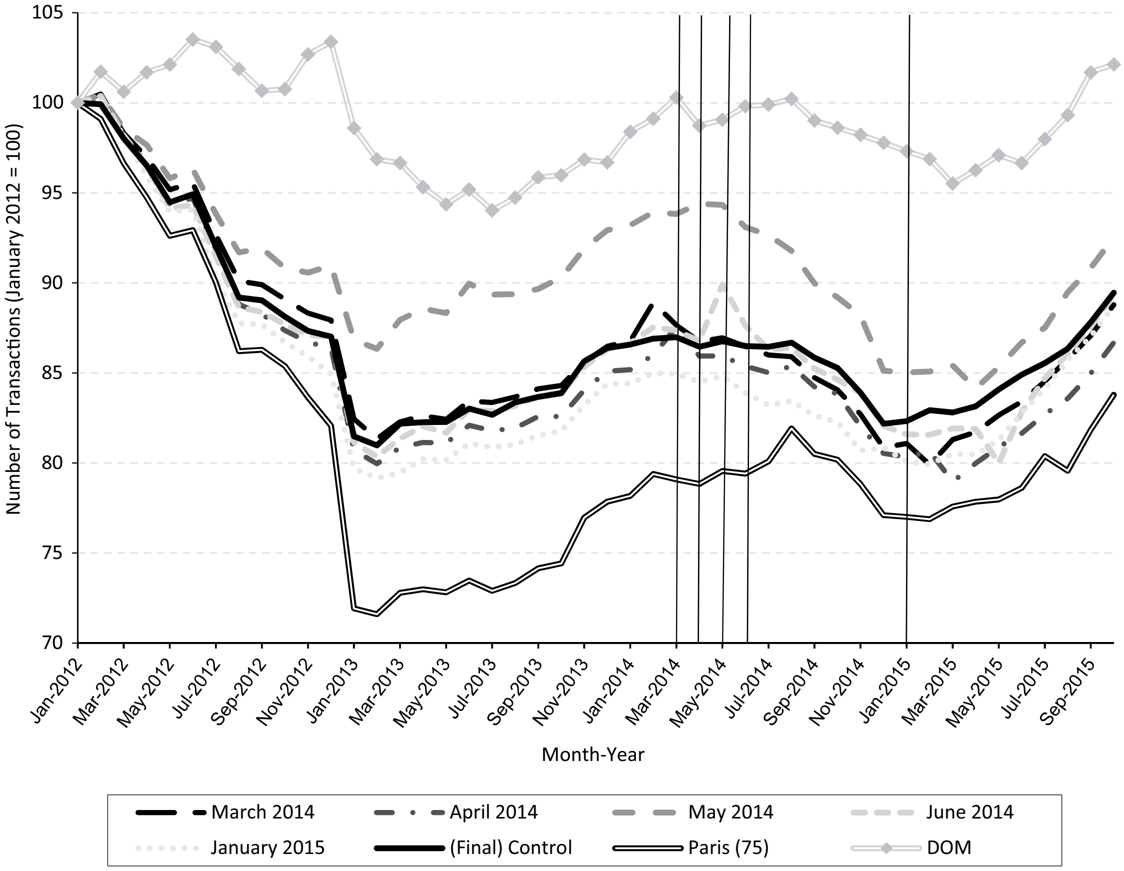
(2) Retention effect. We expect the housing market to be impacted durably by the increase in the RETT, preventing some buyers from moving and accessing to ownership. Thus, a decrease in the volume of transactions should be observed. The retention effect should begin at the implementation date of the reform, and could have either persisted or diminished over time ("resilience of the market"). Note that in the very short-run, this effect is composed of re-timing due to the anticipated transactions (intertemporal substitution by those who would

Figure I
Map of the RETT-increase implementation by *Département*



Notes: Map updated May 2017.
Sources: DGFiP, Droits d'enregistrement : taux, abattements et exonérations 2017; authors' drawing.

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



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 from DGFiP *(MEDOC + Fidji)*, *Nombre de ventes immobilières taxées au taux de droits commun par département* from 2012 to 2015.

have purchased a real estate anyway), and of extensive margin responses (those who would have purchased a real estate in the absence of the reform). If the extensive margin effect is dominating the timing effect, 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 owners).

(3) Price effect. Theory also suggests a slight effect on sales 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, this is a strong assumption knowing that the French housing market is price-sticky. 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 – studied by Benjamin *et al.* (1993) who estimated that housing prices decreased, it is less likely to 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 (Davidoff & Leigh, 2013; Slemrod *et al.*, 2017). Furthermore, the data we use are not very suited to test this price effect. We hence focus on the first two effects.

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 being made available 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[9], in order to recover an estimation of the total number of transactions by *département*[10]. 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.

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. The second one provides monthly total tax bases (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 this 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).

Our control variables include the unemployment rates, the number of new residential construction, population, property tax rates, and three local variables on the *départements*' finances. Data on the unemployment rates come from Insee[11] and are quarterly data by *département* for metropolitan France, and yearly data for the overseas *départements*, both seasonally adjusted. In order to estimate monthly data, we made linear interpolation. Data on the new residential construction (monthly building permits by *département*) come from the database *Sit@del2*, and are compiled by Insee. The estimated population on January 1 of each year in each *département*, using the Insee annual census. The property tax rates voted each year by the *départements*, from the DGFiP[12]. Three local variables from the DGFiP-DGCL[13] in order to "compute an index of good administration" of the local governments. These variables are the salary cost, the operating revenue (which the total local taxes revenues) and the social spending, all per capita, by *département*.

Finally, we also use other variables in order to check for possible unobservables that could affect the sample groups differently over time, not included as covariates in the estimates because they do not fit to our panel data. Indeed, these variables do not vary across the regressed period, then their effect ought to be captured by the *département* and month fixed effects. These data are 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 (Donald & Lang, 2007). To undertake our difference-in-differences design (Box 2), we divide our sample in two groups: (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 yet implemented the reform at the estimated month (i.e. these *départements* were still on their pre-reform period) and of the 4 *départements* which remained at a RETT rate of 3.80% during our regressed time period: the (final) control group.

9. $$Number\ of\ Transactions_{dt} = Number\ of\ Transactions_{dt}\ (\text{Fidji}) \times \frac{Tax\ Bases_{dt}\ (\text{MEDOC})}{Tax\ Bases_{dt}\ (\text{Fidji})}.$$

10. *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.*

11. *Institut national de la statistique et des études économiques.*

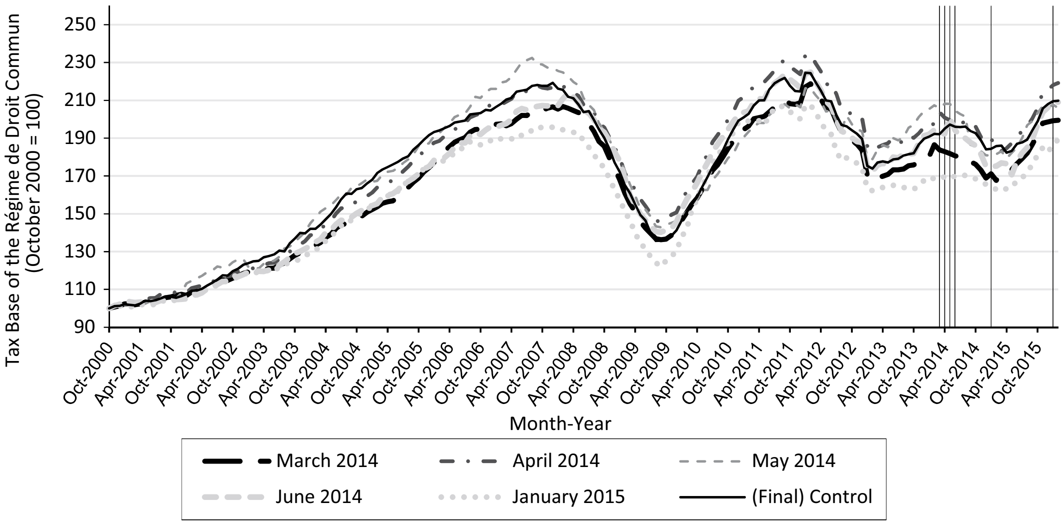
12. *It corresponds exactly to the property tax rates on built real estate.*

13. *Direction générale des collectivités locales.*

Box 2 – **Validity of the 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 overseas *départements* (DOM). There is sometimes a slight difference in the trend of the May 2014 group (cf. Figures II and A). Those observations deserve further enquiry, which we perform below, and later in the robustness checks section.

Figure A
Monthly (12-month cumulative) tax base of the *Régime de droit commun* trends from October 2000 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 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 from DGFiP *(MEDOC)*, *Assiettes des droits de mutation immobiliers par département* from 2000 to 2016.

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 use a binary logit over the period from January 2008 to December 2013.

$$Y_{dt} = \sum_{x=1}^{9} \beta_x X_{dt} + \varepsilon \qquad (1)$$

In this equation, Y_{dt} is equal to 1 if the *département* implemented the tax increase, 0 otherwise; X_{dt} corresponds to one of the variables of interest or control, in a *département* d, in period t.

Estimates are reported in the online add-on C5. Estimates of the Table C5-1 show that the coefficients are close to zero, even if they are statistically significant. The choice to increase the tax is almost not correlated with these variables. It means that there is no selection bias of the treated *départements.* They did not do it because of a possible difference in the explaining variables, compared to the *départements* which chose to remain at 3.80%.

Placebo test

The placebo test is used to check empirically the validity of the common trend assumption, 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 month-based model (see below) on the outcome variables, using the same groups. We define our dummies for anticipation and retention as being the same than equation (2) below, but the periods 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.

Estimates are reported in the Online complement C5. Table C5-2 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, and our difference-in-differences design can be implemented.

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 (Table 1 and Figure III). We subdivide the treatment group in five subgroups (Table A1 in Appendix), 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.

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). 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).

We estimate two models, respectively termed month-based model and parsimonious model.

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 maximum retention effect (keeping in mind that 20 months is the full period of retention: March 2014 to October 2015). This model can be seen as a sensitivity check against the parsimonious model. The model with monthly regressors is shown in the following equation:

$$\log Y_{dt} = \alpha_d + \lambda_t + \sum_{j=1}^{6} \beta_{Aj} Anticipation_{d,t=T_d-j} + \sum_{k=0}^{Oct.2015} \beta_{Rk} Retention_{d,t=T_d+k} + \rho X_{dt} + \varepsilon_{dt} \quad (2)$$

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

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

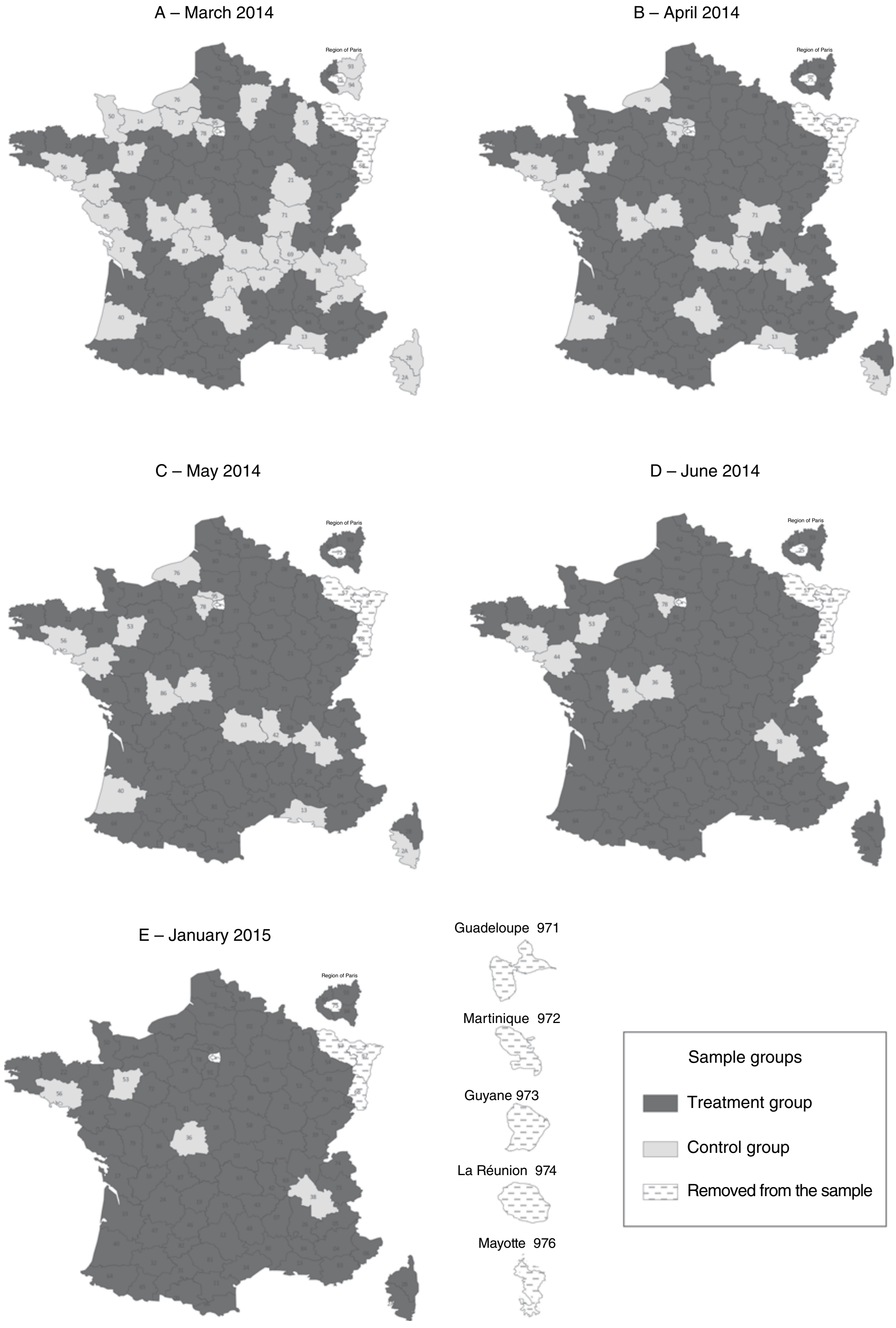
Period (from)		Group		Total
		Treatment	Control	
Treatment	March 2014	58	34	92
	April 2014	76	16	92
	May 2014	78	14	92
	June 2014	85	7	92
	January 2015	88	4	92

Notes: Numbers correspond to the number of *départements*. Dates correspond to the month of implementation, and are different from the treatment subgroups.

Box 3 – Standard event study model with anticipation

To estimate properly the anticipation and retention effects, we use a standard event study model (see for instance Jacobson *et al*., 1993) as proposed in Malani and Reif (2015) that allows to estimate properly effects of a treatment, when there are expectations and anticipations from the treated population, as it is the case in our evaluation. Indeed, as explained in the previous cited papers, when there are anticipations, the full treatment effect depends on both the ex-ante and the ex-post effects. Therefore, they must be estimated simultaneously to avoid a bias in the estimations. They propose two models: (1) the quasi-myopic model which is based on a standard event study specification, 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 requires a structure on the error term, and in addition it assumes that people discount the future exponentially and have rational expectations, which is a strong assumption.

Figure III
Maps of the treatment and control *départements*



Notes*:* Dates correspond to the month of implementation, and are different from the treatment subgroups.
Source*:* Authors' drawing.

$Anticipation_{d,t=T_d-j}$ 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 X_{dt}, a vector of 7 time-variant control variables that could affect the outcome variable Y_{dt}, α_d, which controls for *département* time-invariant characteristics (*département* fixed effects), and 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* × *month* shocks to the variable Y_{dt} (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 similar to the regression developed by Best and Kleven (2018). It is our benchmark because it is parsimonious. Indeed, as shown in the estimates of the month-based model, the anticipation effect only occurs the month before the implementation, while the retention effect seems to last only in a short-term of 3 months after reform. After this short-term effect, the coefficients are non-different from zero, meaning that the housing market should have reached a new steady state. Then, we developed the following model in order to estimate the average effects of these three periods.

$$\begin{aligned}\log Y_{dt} = {} & \alpha_d + \lambda_t + \beta_{A1} Anticipation_{d,t=T_d-1} \\ & + \beta_2 Retention_{d,t\in[T_d,\,T_d+1,\,T_d+2]} \\ & + \beta_3 Post.Retention_{d,t\in[T_d+3,\,Oct.\,2015]} \\ & + \rho X_{dt} + \varepsilon_{dt} \qquad (3)\end{aligned}$$

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.

$Retention_{d,t\in[T_d,\,T_d+1,\,T_d+2]}$ is equal to 1 if a RETT increase is implemented in a *département* d, and the month *t* belongs to its 3 first months following the implementation date, 0 otherwise.

$Post.Retention_{d,t\in[T_d+3,\,Oct.\,2015]}$ is equal to 1 if a RETT increase is implemented in a *département d*, and the month *t* belongs to the period after the 3 first months following the implementation date, 0 otherwise.

The anticipation effect in T_d-1 is estimated by $\hat{\beta}_{A1}$ (positive timing effect), the mean retention effect is estimated by $\hat{\beta}_2$ (negative effect due to re-timing + extensive margin response) and the mean effect post retention is estimated by $\hat{\beta}_3$. To interpret the raw coefficients, see Box 4.

Results

Month-based model

Table 2-A shows the estimates of the month-based model, where the dependent variable is the total tax base of the *Régime de droit commun*, and are illustrated by Figure IV which shows a plot of the coefficient and confidence intervals (Table C-2 in the Online complement shows the detailed coefficients).

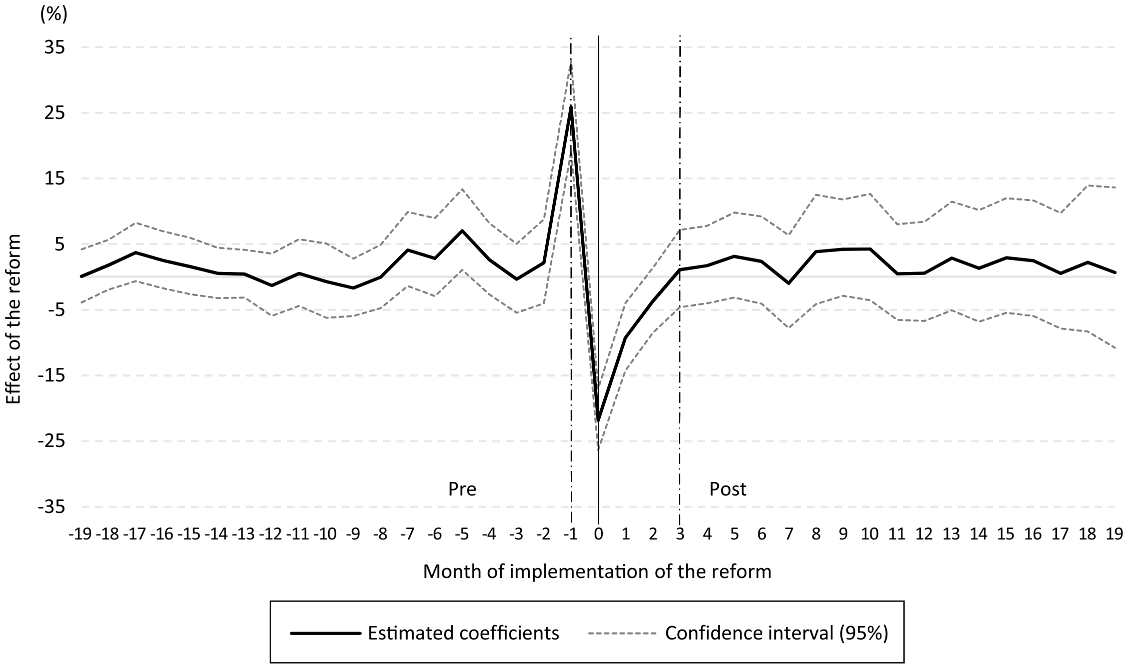
The estimates show an increase of around 25% the month just before the implementation

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.

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

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



Notes*:* Month 0 (solid vertical line) is the month of implementation of the reform in a given *département*; vertical dotted lines indicate statistically significant months 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.
Reading note*:* One month after the implementation of the reform, the volume of transactions decreased by around 10% in the *départements* which increased the RETT.
Sources*:* CGEDD from DGFiP *(MEDOC)*, *Assiettes des droits de mutation immobiliers par département,* Insee, *Construction de logements (Sit@del2)*, *Taux de chômage localisés, Estimation de population au 1er janvier,* DGFiP, *Taux de fiscalité directe locale* (TFPB), DGFiP-DGCL, *Les budgets primitifs des départements,* from 2012 to 2015; authors' computation.

of the reform (i.e. $T_d - 1$), significant at the 1% level. None of the other anticipation-period coefficients are significantly different from zero (except the coefficient for $T_d - 5$, equal to 5.6%[14]), meaning the anticipation effect is concentrated over the month just before the date of implementation. Both specifications suggest that buyers and sellers really agreed to escape the tax increase, and consequently, they brought forward the sale date of one month.

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 22%, 9.5% the second month after reform (i.e. $T_d + 1$), and 4.6% the third month (i.e. $T_d + 2$), all significant at the 1% and 5% level (Table 2-A). None of the other

14. This coefficient is quite puzzling and we speculate that it might correspond to a possible first anticipation during the last quarter of 2013, following the draft Finance Act.

coefficients are significantly different from zero. It proves that most of the retention effect took place the first three months after the reform, and the effect vanished later on as the plot of estimated coefficients of the monthly effects shows (cf. Figure IV). The cumulated decrease in the months following the reform is higher than the increase of 25% in $T_d - 1$ (see Figure V). This proves that the estimated fall in the number of transactions is not only due to re-timing (i.e. anticipated transactions that already occurred in $T_d - 1$).

Parsimonious model

Table 2-B shows the estimates of the parsimonious model for different specifications, introducing one by one the monthly-fixed effect, the *département* fixed effect and the control variables. For the anticipation effect, once we introduce monthly fixed effects, that is, we are really adopting the difference-in-differences estimation strategy, neither the coefficients nor the standard errors really change with or without covariates (columns (4) to (6)). We find that there was an anticipation in $T_d - 1$, of around 26%, significant at the 1% level. The average monthly retention effect during the three months following the implementation is around -14%, and significant at the 1% level (columns (4) to (6)), while we see no effect significantly different from zero in the period post retention when introducing the monthly fixed effects (columns (4) to (6)).

Net effect

One may want to compute the net retention effect (Mian & Sufi[15], 2012; Best & Kleven, 2018). 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

15. We cannot implement the very same method proposed in this paper, because of the different waves of the implementation process.

Table 2-A
Estimates for the month-based model

	Total tax bases of the *Régime de droit commun*
Anticipation effect ($T_d - 5$) ($\hat{\beta}_{A5}$)	0.055** (0.027)
Anticipation effect ($T_d - 4$) ($\hat{\beta}_{A4}$)	0.013 (0.022)
Anticipation effect ($T_d - 3$) ($\hat{\beta}_{A3}$)	-0.013 (0.021)
Anticipation effect ($T_d - 2$) ($\hat{\beta}_{A2}$)	0.013 (0.022)
Anticipation effect ($T_d - 1$) ($\hat{\beta}_{A1}$)	0.22*** (0.021)
Retention effect (T_d) ($\hat{\beta}_{R0}$)	-0.25*** (0.030)
Retention effect ($T_d + 1$) ($\hat{\beta}_{R1}$)	-0.10*** (0.026)
Retention effect ($T_d + 2$) ($\hat{\beta}_{R2}$)	-0.047** (0.023)
Retention effect ($T_d + 3$) ($\hat{\beta}_{R3}$)	0.00085 (0.029)
Retention effect ($T_d + 4$) ($\hat{\beta}_{R4}$)	0.0076 (0.027)
Adjusted R^2	0.65
Observations	4,232

Notes: For a better understanding, we present only estimates for the 5 months before and after reform. All coefficients are available in the Online Complement C2.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: CGEDD from DGFiP *(MEDOC), Assiettes des droits de mutation immobiliers par département,* Insee, *Construction de logements (Sit@del2), Taux de chômage localisés, Estimation de population au 1er janvier,* DGFiP, *Taux de fiscalité directe locale* (TFPB), DGFiP-DGCL, *Les budgets primitifs des départements,* from 2012 to 2015.

Table 2-B
Estimates for the parsimonious model

	Total tax bases of the *Régime de droit commun*					
	(1)	(2)	(3)	(4)	(5)	(6)
Anticipation effect (T_d - 1) ($\hat{\beta}_{A1}$)	0.19*** (0.014)	0.19*** (0.016)	0.18*** (0.016)	0.23*** (0.021)	0.23*** (0.021)	0.23*** (0.021)
Mean retention effect ($\hat{\beta}_2$)	-0.16*** (0.011)	-0.15*** (0.014)	-0.17*** (0.013)	-0.14*** (0.022)	-0.15*** (0.021)	-0.15*** (0.021)
Mean effect post retention ($\hat{\beta}_3$)	0.031*** (0.0050)	0.049*** (0.010)	0.036*** (0.0099)	-0.0099 (0.026)	-0.018 (0.025)	-0.016 (0.024)
Adjusted R²	0.055	0.067	0.070	0.64	0.64	0.65
Observations	4,232	4,232	4,232	4,232	4,232	4,232
Monthly FE	No	No	No	Yes	Yes	Yes
Département FE	No	No	Yes	No	Yes	Yes
Control Variables	No	Yes	Yes	Yes	No	Yes

Notes: This table reports estimates of equation 3, using within estimator. Outcome variable is in log in the estimations. 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*. FE indicates fixed effects. Stars indicate significance level: * p<0.1, ** p<0.05 and *** p<0.01.
Sources: cf. Table 2-A.

(i.e. re-timing). The upshot is an increase of the estimated negative effect.

The coefficient $\hat{\beta}_{A1}$ = 0.23 (0.021) from equation (3) implies that the anticipation of the reform increase the volume of transactions by 26% the month just before the implementation, and the coefficient $\hat{\beta}_2$ = -0.15 (0.021) implies that the average monthly activity was 14% lower in the treated *départements* the 3 months following the implementation. These estimates together imply that $-\hat{\beta}_{A1}/(3\hat{\beta}_2)$ = 51% of the retention effect was a re-timing effect due to the anticipated transactions (intertemporal substitution by those who would have purchased a real estate anyway), and the remaining 49% was an extensive margin effect (those who would have purchased a real estate in the absence of the reform).

A new piece of evidence is brought by Figure V, which plots the cumulative sum of the coefficients from the month-based model starting from one month before the implementation month (i.e. $T_d - 1$). It shows that the magnitude of the retention effect is higher than the one of the anticipation effect, and that in the months following the implementation date, the cumulative sum is always negative. Performing a Wald test on the sum of the coefficients from $T_d - 1$ to $T_d + 2$, we can reject at the 5% level the hypothesis that this add-up to zero (H_0). In fact, it is even true for a period of 5 months from $T_d - 1$ to $T_d + 3$ (Figure V and the double arrow), except for the month of implementation of the reform (i.e. T_d), which implies that the re-timing is almost completely absorbed in the first month of implementation. Beyond four months following the implementation date, we cannot reject that the evolution of the treatment and the control groups are similar.

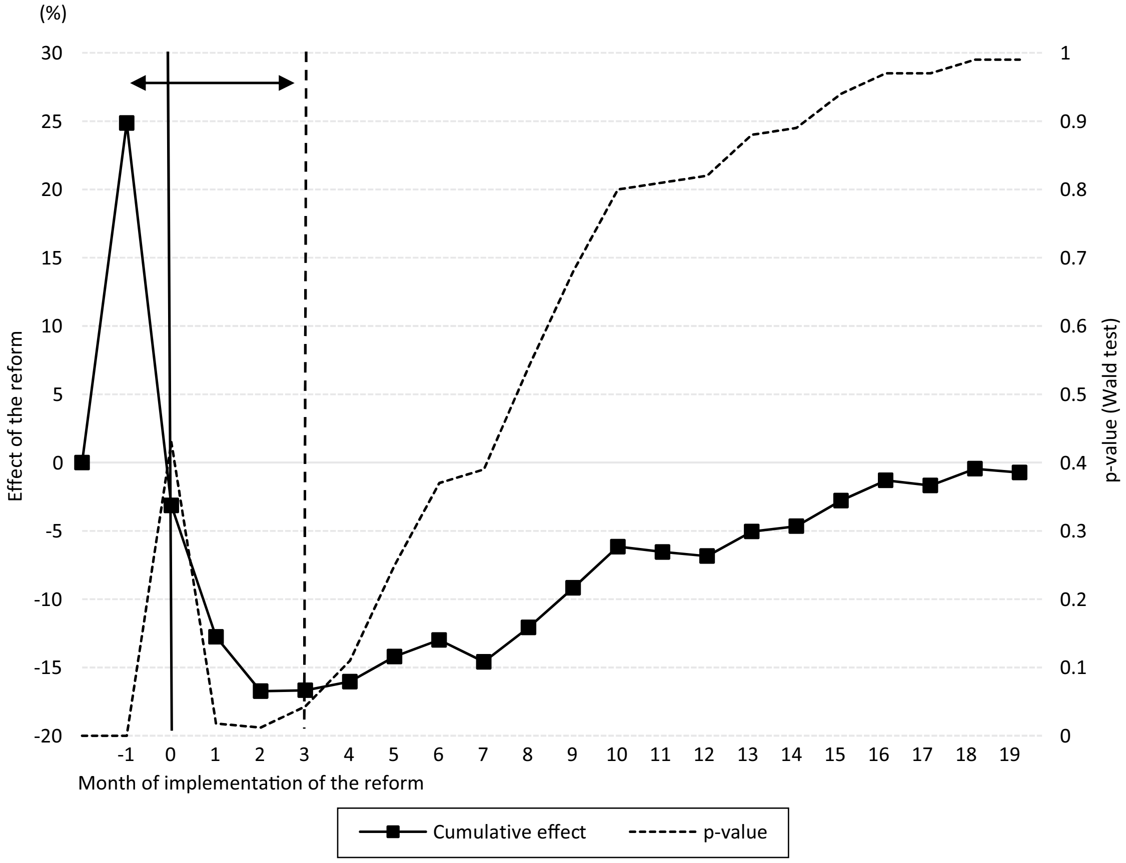
The cumulative sum of the coefficients up to $T_d + 2$ is equal to -0.18. Dividing it by 3 (i.e. number of months of the retention effect), we find an average monthly net effect of -5.8% over three months. The same computation up to $T_d + 3$ (-0.17) gives an average monthly net effect of -5.5% during four months. We have then strong evidence of a short-term effect.

Using the coefficients from the parsimonious model (cf. Table 2-B) and applying a similar computation[16], we find an average monthly net proportional change of -7% during the three initial months following the implementation date.

Taking advantage of both estimations and giving more weight to the monthly estimation, we then conclude to a short-term drop between 5.5% and 7% per month during three months after the implementation date (i.e. approximatively 6%), and no medium- or long-term response afterwards.

16. (0.23 + 3 x (– 0.15)) / 3 = -0.073.

Figure V
Cumulative effect of the reform on the volume of transactions, month by month before and after the implementation date



Notes: Month 0 corresponds to the month of implementation of the reform in a given *département*. This graph shows the cumulative sum from $T_d - 1$ of the effects estimated in Table 2-A, and plotted in Figure IV (left axis), and the p-value of the Wald test (right axis). The double arrow figures out the period for which the Wald test rejects H_0 (i.e. the sum of the coefficients is null).
Reading note: The value -3.14% for month 0 is the percentage change from $T_d - 1$ to T_d (two months), and has the following interpretation: after two months since $T_d - 1$, there is a cumulative fall of 3.14% of the monthly number of transactions, and the p-value is 0.43 for the Wald test.
Sources: CGEDD from DGFiP *(MEDOC)*, *Assiettes des droits de mutation immobiliers par département*, Insee, *Construction de logements (Sit@del2)*, *Taux de chômage localisés*, *Estimation de population au 1er janvier,* DGFiP, *Taux de fiscalité directe locale* (TFPB), DGFiP-DGCL, *Les budgets primitifs des départements,* from 2012 to 2015; authors' computation.

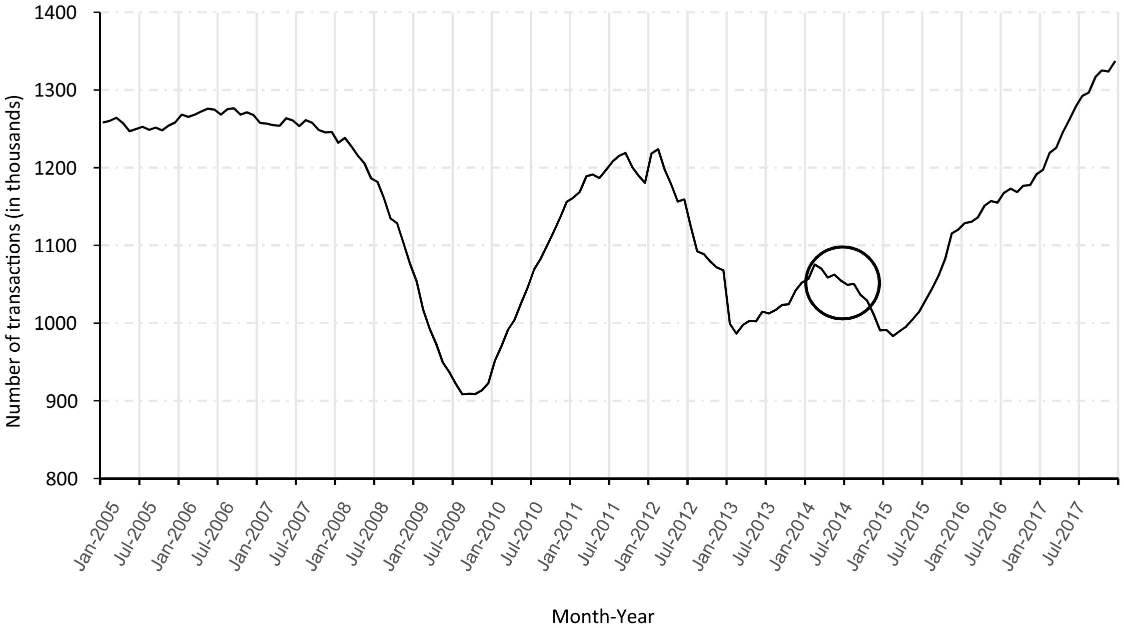
This net monthly effect helps to provide a rough estimate of the number of missed transactions in 2014 due to the rise of the RETT. As shown in the following graph (Figure VI), the yearly number of transactions in the *Régime de droit commun* at the national level were around 1,050,000 on the verge of 2014. We could offer a rough estimate of the drop that would have occurred, were the implementation nationwide. We should observe a drop of the number of transactions of around 18% at the end of the three month period, that is $16{,}142 \simeq (\text{-}18\%) \times \overline{\textit{Number of Transactions}_0}$, where $\overline{\textit{Number of Transactions}_0}$ is equal to the previous two years' average monthly number of transactions (i.e. 89,681).

In fact, only 93%[17] of the *départements* implemented the measure over the regressed period. Then, the true effect is closer to 15,000, which is approximatively 1/4 of the drop that we can detect on Figure VI (see the circle, which brings about the total number of real estate transactions[18]). This computation surely underestimates the true effect since we ignore any interdependence between local markets. It should be considered as a lower bound of the true effect.

Unfortunately, we cannot conclude on the issue whether this loss in transaction of the three (four) initial months is reversed (i.e. the entire response to the reform is a timing response). On the one hand, a piece of evidence in favor of no recovering is the fact that the monthly coefficients are not significant after $T_d + 2$ in the monthly regression

17. *94 / 101 = 0.93.*
18. *Houses + non-residential premises + lands.*

Figure VI
12 month-moving-average total number of real-estate transactions



Notes*:* The number of transactions is cumulated over the previous 12 months, and corresponds to the number of transactions in the *régime de droit commun* registered by the DGFiP.
Coverage*:* France (Mayotte excluded).
Sources*:* CGEDD from DGFiP (*MEDOC* + *Fidji*), *nombre de ventes immobilières taxées au taux du régime de droit commun par département* from 2005 to 2017.

(cf. Table 2-A), contrary to Mian and Sufi (2012) who got statistically significant reversal coefficients. On the other hand, a piece of evidence in the other direction is the fact that looking at the Wald test of the nullity of the sum of the coefficients, we cannot reject the hypothesis that these coefficients add up to zero beyond five months (i.e. after $T_d + 3$). However, we should not forget that the design of the implementation introduces a noise beyond $T_d + 3$ due to the attrition of the control group (cf. Table 1), in addition to the decrease in magnitude of the effect, which reduces the statistical power. Furthermore, the statistical power of the cumulative effect weakens also mechanically as we extend the horizon by adding an extra noise for each month added, as shown by the pattern of the Wald test's p-value. So, we choose to let this issue unsettled at this stage, and we conclude that this point needs further investigations.

Tax elasticity and the Laffer curve

In this section we are interested in quantifying the response of the total tax bases to a one-percent increase in the tax. Since we found that the net effect is estimated to be around -6% whereas the increase in tax rates is about 14.15%[19] the elasticity is:

$$\varepsilon_{TB}^{\tau} = \frac{\partial \log Y}{\partial \log \tau} \cong \frac{\frac{\Delta Y}{Y}}{\frac{\Delta \tau}{\tau}} \cong \frac{-0.06}{0.1415} = -0.42 \qquad (4)$$

This means that tax bases decreased by 0.42% for a 1% increase in the RETT's rate (i.e. there is a loss in the tax bases which reduces the gains of tax revenues for the local budgets). Following this, we want to compute $\overline{\tau}$ the rate from which the tax revenues of the *départements* would be maximum, then, begin to decline for each $\tau > \overline{\tau}$ (i.e. the maximum of the Laffer curve).

A small change in the tax rate changes Y by:

$$\frac{\partial \tau . Y}{\partial \tau} = Y + \tau \frac{\partial Y}{\partial (1+\tau)} = Y\left(1 - \frac{\tau}{1+\tau}\varepsilon\right) \qquad (5)$$

where $\varepsilon \equiv \varepsilon_{TB}^{1+\tau} = -\frac{\partial \log Y}{\partial \log(1+\tau)}$ is the elasticity of the tax bases with respect to tax-inclusive

19. *RETT of the* Régime de droit commun *increased by 0.7 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%.*

prices. The Laffer rate sets the above expression to zero:

$$\bar{\tau} = \frac{1}{\varepsilon - 1} \tag{6}$$

To compute this rate, we use the following expression:

$$\varepsilon_{TB}^{1+\tau} = \varepsilon_{TB}^{\tau} \frac{1+\tau}{\tau} \cong 0.42 \frac{1+0.0509}{0.0509} \cong 8.7 \tag{7}$$

Then, replacing (7) in (6) implies that $\bar{\tau} \cong 13\%$, and that the *départements*' tax revenues are still on the increasing part of the Laffer curve[20].

Discussion

The main result – only a short-term effect of the reform – raises interesting issues. At first glance, when increasing tax, we should expect the market to be negatively impacted durably. Nevertheless, in our case, the extensive margin effect is estimated to last only 3 months, and we see no difference between treated and controlled *départements* beyond. We provide three possible explanations for this result. (1) It is possible to build theoretical model of housing investment where the long-term effect is ambiguous. (2) This short decrease could correspond to the shifting time of demand (people should buy anyway). Indeed, the average time for a housing contract in France is 3 months. So, perhaps that these three months of decrease correspond to the time spell for sellers to find new buyers, after that the first buyers gave up on buying when being informed of the tax increase. Moreover, those buyers could have decided to buy at lower price. They renounced to real estates that they were looking for, in order to buy some with lower characteristics and amenities a few months later. (3) It could be related to a cognitive bias from the agent. As developed by the Nobel Prize Richard Thaler, people do not feel price differences "equally" when prices are big. For instance, people are ready to pay a relatively important "cost" to save €10 for a small purchase (e.g. at a restaurant); at the same time, they think that a €200,000 and a €205,000 housing are almost of the same values, except the deviation is €5,000!

Nevertheless, our study faces two main limitations. One is a possible spillover effect, due the fact that some buyers could have "voted with their feet" may introduce a bias. More precisely, some buyers who 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 further studies using micro data, this spillover effect could be estimated with a regression discontinuity design (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. The other is 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. That does not seem likely when looking at the trends of the outcome variables (cf. Figures II and A). Furthermore, when looking at the distributions and trends of the other local variables between groups (see Online complement C1), there is no marked difference 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 national statistics. Ultimately, what we are interested in here is the elasticity of buyers' and sellers' supply and demand in real estate, while the choice of the reform implementation falls to the local councilors. 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

20. *Notice that the elasticity estimate would be higher using the gross estimates (rather than net estimates).*

check. 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 councilors 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 (Table 3). 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. 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.

More elements are discussed in the Online complement C4.

Robustness checks

As suggested in Meyer (1995), we multiply the tests of robustness, in order to check the validity of our results. Developments and estimates are reported in the Online complement C5.

(1) Alternative dependent variable. Alternative dependent variable (total tax bases of the *Régime dérogatoire*) is used to test whether the results are biased because there was an exogenous shock affecting the housing markets of the two groups differently. Results of Table C5-3 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.

(2) Estimations using different period. We check the validity of our results to the choice of the period and sample groups. Table C5-4 shows estimates close to the ones found in the main estimations. The main effect in which we are interest in being similar to our first estimates, they appear robust to the choice of the estimation period.

(3) Changes in local economic conditions. As the results that we obtain 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). Results for both models presented in Tables C5-5 and C5-6 show only slight differences between the estimates and our main results. We can therefore conclude that our estimates are robust, and that no exogenous local economic shocks affected differently our groups.

(4) Regressing by treatment subgroups. We re-estimate the parsimonious model where we allow for a possible heterogeneity for the different subsets of treated groups. Results of this regression are displayed in Table C5-7. The anticipation effect is non-significant for the January subgroup and for the other

Table 3
Distribution of the *départements*' political color, by implementation or non-implementation of the RETT increase

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

(a) 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, résultats des élections cantonales 2011 et départementales 2015.

subgroups spans a large range between 16% (May) and 45% (April). The retention effect is also non-significant for the January subgroup and is comprised between -10% (March) and -17% (May) for the other subgroups. None of the coefficients of the post retention period are significantly different from zero. It is not very surprising that there is some heterogeneity in the local-market responses.

(5) Removing possibly heterogeneous groups. We may suspect a possible heterogeneity or unobservables that affect differently May 2014 and January 2015 groups 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. Tables C5-8 shows only slight differences between the estimates and our main results. We can conclude that our findings are robust to the choice of the sample, and to a possible bias from heterogeneous *départements*.

* *
*

To conclude, we find evidence that the RETT increase had an impact on the housing market in line with the economic literature. We bring empirical evidence that two behavioral responses took place. We show extremely compelling estimates of a short-term timing response to an anticipated tax increase. People brought forward transactions to the month before the tax increase. The number of transactions rocketed by 26% the month preceding the implementation of the reform. Second, the volume of transactions fell by around 14% on average per month during the three months following the rate change, whose 51% of this loss is due to re-timing. The two effects do not cancel out. All in all, the average monthly net effect corresponds to a transaction drop of around 6% over the three 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. Buyers and sellers can more easily agree in changing the sale date rather than the sale price (Benjamin *et al.*, 1993; Davidoff & Leigh, 2013; Slemrod *et al.*, 2017), a behavior supported by 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 price effect. We find compelling evidence of a sizable short-term effect – but no medium or long-run effect – meaning that there is a strong "resilience" from the housing market (people should buy anyway). Moreover, we estimate that the short-term elasticity of the tax base to the tax rate is around -0.42, meaning that there is a loss of 42% in the tax revenues with respect to a situation of no behavioral response, the first quarter after the reform. Computing the Laffer tax rate, we conclude that *départements'* tax revenues are still on the increasing side of the Laffer curve. Note that our results are valid for partial equilibrium. We do not estimate the possible other general equilibrium aspects resulting from the distortion of the housing market, such as changes in investment from the local governments or impact in the labor market. Applied to national transactions data, our estimate means that around 15,000 transactions were missing because of the transfer tax increase. This estimate is likely a lower bound of the true impact.

This evaluation can be extended in three ways: using the non-cumulative monthly data on the number of transactions; doing a precise estimation of the price effect through hedonic model (using the notarial databases *BIEN – Base d'informations économiques notariales* – and *Perval*); 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 reforms, and anticipate the effect on the housing market, in particular on buyers and sellers behavior. 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 assessed: 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 a negative sizable (short-term) impact on mobility and well-being. □

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Table A1
Sample groups with subdivision of the treatment group[(a)] 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

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