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Can fiscal policy reduce vacancy?

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Taxing Vacant Apartments: Can fiscal policy reduce vacancy?

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Abstract

In this paper, we focus on the empirical evaluation of a supply-sided fiscal policy: taxation of vacancy. We use the quasi-experimental setting of the implementation of a tax on vacancy in France in 1999 to identify the causal direct effect of the tax on the vacancy rate. Exploiting an exhaustive fiscal dataset, which contains information on every dwelling in France from 1995 to 2013, we implement a matching Difference-in-Difference approach. The results we obtain suggest that the tax was responsible of a 13% decrease on vacancy between 1997 and 2001. This impact is twice as big for high populated municipalities. Results are robust to the introduction of controls, sample reduction and choice of control group. Results also suggest that most of the vacant apartments moved to primary residences. In terms of policy implications, these results indicate that a municipal tax on vacancy can play a role in shaping the incentives of the owners in the housing market.[1]

Keywords: Housing, Vacancy Rate, Fiscal Policy

JEL Classification: R31, E62

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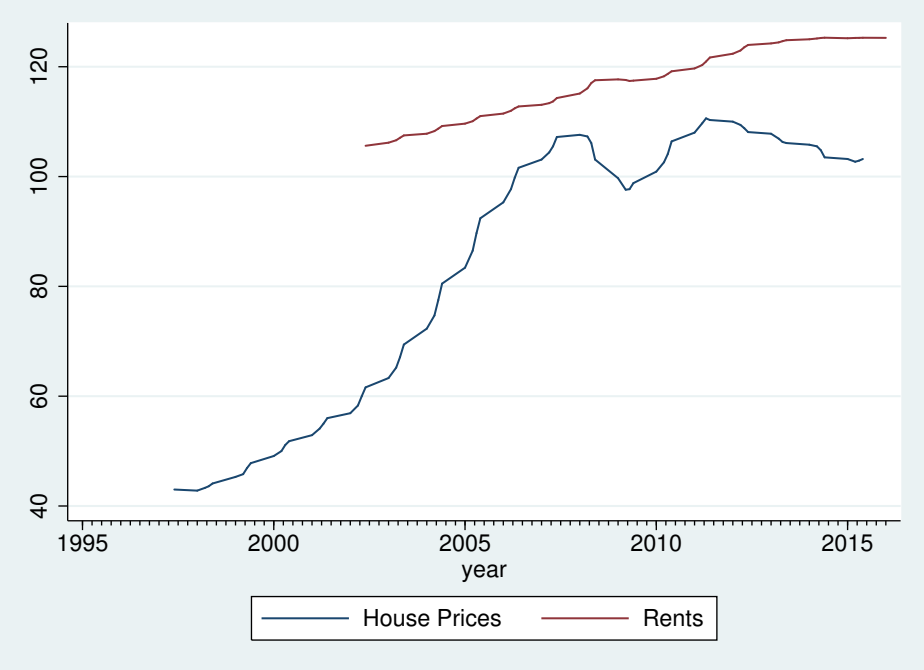
1 Introduction

Vacant dwellings are a relatively common phenomenon across European countries, where vacancy rates for the Euro Area amounted 10% in 2005 (Eiglsperger and Haine, 2009). While housing is a prime necessity good representing 21% of households' disposable income (Boulhol, 2011), it is also an investment good, which can result in speculative withholding of dwellings and hence in an inefficient use of housing stock. Politicians and activists have criticized this situation arguing that vacancy is a sign of a market failure that needs to be tackled. Various interventions have been proposed to deal with vacancy which range from a tax on vacant apartments to expropriation.

Housing vacancy is undesirable as long as demand for housing as a first necessity good is not satisfied. According to Andrews (2010) demand for housing has not ceased to increase in recent years due to aging, increase in human capital and financial deregulation (Debelle, 2004). Similarly, other demographic aspects have also been pressuring housing demand such as net migration, reduction of average household size[2] and the increase in number of separations. Even if housing supply has increased as well in France (at an average rate of

Figure 1: Evolution of housing and renting prices in continental France



Notes: Reference year for the house price index and for the rent prices is 2002 T4. Data comes from INSEE sources.

1.1% annually since 1988 according to Soes (2011)), it has remained rigid with respect to demand increases, which could be one of the causes of the significant increase in housing prices observed during the 2000s (see Figure1). Hence, the rise of prices, rents and the increasing number of demands for social housing (1.8 millions in 2014) suggest that a part of the demand for housing is not satisfied.

Mills and Nijkamp (1987) characterize the housing market as peculiar by identifying three particular features not present in other kind of markets. They define housing as

[2]In France, it has decreased from 2.41 to 2.09 for women born in 1940 compared to women born in 1960 (Pearce et al., 1999)

immobile, very durable (a house usually lasts longer than a person's life) and very expensive (representing the largest expenditure of a household's budget[3]). The combination of these features can result in a situation where the housing market fails to satisfy the need of the residents or to produce an efficient number of housing. The consequences of these failures include households living in sub-optimal conditions, youth delaying moving out of the family house, workers unable to afford to live near their workplace and, in more extreme cases, house evictions and homelessness. Furthermore, the elasticity of supply of the housing market is very low, which means that the supply reacts very slowly to changes in prices. In particular, Caldera and Johansson (2013) estimate the housing supply elasticity for various OECD countries and found France to be among the least responsive markets, with an elasticity of 0.363.

In such a context of increasing demand and inelastic supply it is paradoxical that, in many European countries, this situation coexists with considerably high levels of vacancy. Specifically, as estimated by Eiglsperger and Haine (2009) the average vacancy rate in Europe in 1999 was around 11.9%, and in France it was 8.95% (FILOCOM, 1999). Two different factors appear as possible explanation for the high vacancy rates: a mismatch problem between supply and demand and a strategic withholding of apartments. On the one hand, it is likely that, even if there is enough supply, it fails to meet demand in the sense that it does not meet the desired quality or location characteristics of the demand. Certainly, the durability, stability of occupancy and cost make the housing market heavily dependent on the current stock of houses, which may not be suitable to fulfill the current unmet demand. On the other hand, vacancy can also be a result of the willingness of the owner to keep it vacant. In the French context where there is a medium level of tenancy protection regulation (Seshimo, 2014), this second reason could indeed be important. Particularly, an owner may prefer to keep an apartment vacant than to rent it if he expects that he will not be able to use it at the chosen moment.

Nevertheless, the willingness to reduce vacancy rate goes beyond the expected increase in housing supply that the reintroduction of empty apartments to the market can entail. There have been some recent empirical evidence showing other social consequences of empty housing in the shape of negative externalities. First of all, concentrations of vacant apartments tend to reduce the value of the properties in the area (Lee, 2008; Fitzpatrick, 2012). Secondly, the increase in the perception of insecurity has also been identified as a consequence of the concentration of vacant houses, which can derive in an increase in the delinquency rate and damage social cohesion (Immergluck and Smith, 2006; Mummolo and Brubaker, 2008). Therefore, the justification for government intervention to reduce vacancy comes both from the potential redistributive effect of improving the matching process and from the reduction of the negatives externalities entail by vacancy.

In order to correct the mismatch between supply and demand and ensure equitable access to housing, governments have historically intervened in the housing market. However, few have directly tackled the reduction of vacancy, one of the reasons being the limited set of tools available. Similarly, the difficulty to define and measure vacancy – where there is no consensus so far (de La Morvonnais and Chentouf, 2000) – is another important challenge. Nevertheless, there have been some initiatives across OECD countries aiming to decrease vacancy rates. The Netherlands has historically opted for decriminalizing squat-

[3]According to INSEE, the average French household spent in housing expenditures an 18% of his total income in 2009

ting. The Vacant Property Act in 1981 stated that squatting was only a criminal offense if the building had been vacant for less than six month. This law was then revoked in 2007 but, according to Priemus (2011), enforcement by the municipalities of the prohibition of squatting has been low.

In the US, local governments have enacted vacant property registration ordinances in order to keep a better account of vacant apartments. They ask the owners for multiple notifications and for property maintenance requirements and they usually charge a fee at the time of registration. Such fees vary across municipalities, from $70 per year in Chula Vista, California to $1680 in San Jose, California or $5,000 in Wilmington, Delaware and they are increasing over time (Schilling, 2009).

Finally, there are a few countries that have implemented a tax on vacant apartments so far: the UK, Israel and France. In the UK, the government abolished in April 2013 the exemption of the Council Tax, a tax on housing properties, for vacant apartments. Now, it is the local authority who decides which level of discount to apply. Moreover, if the apartment has been vacant for more than two years, the authority can decide to charge up to a 50% surcharge of the standard Council Tax. The reform is still very recent and, to our knowledge, no evaluation has taken place yet. Jerusalem has also implemented a kind of vacancy tax in the form of doubling the Arnona tax (a municipality tax on property) for those properties that have been empty for more than six months. The measure, in place since 2013, aims to reduce the number of "ghost apartments" and eventually decrease rental prices.

In France, where 3,6 million people are housed under unsatisfactory conditions (*Fondation Abbé-Pierre*, 2014), housing is a sensitive topic. The intervention of the government on the housing market is substantial, corresponding to a 2% of the budget in 2010, twice the EU25 average, 0.9% (EUROSTAT, 2010). In 1999, the French government decided to implement a vacancy tax on municipalities with a tight housing market, the *Taxe sur les Logement Vacants* or TLV.

In this paper, we use an exhaustive fiscal dataset to measure vacancy rate and we apply a matching difference-in-difference strategy to compare cities that were concerned by the tax to those where the tax was not applicable. We find a negative and significant impact of the implementation of TLV equivalent to a decrease of 0.8 percentage points of the vacancy rate. This result is robust to the use of different specifications and identification method including different sets of controls and it is 50% higher in highly populated municipalities.

The paper is organized as follows, in Section 2 we review the literature on the housing market and on the impact of public intervention. Section 3 describes the institutional setting and the particularities of the tax implementation. In Section 4 we present the data, in Section 5 the empirical strategy used. Section 6 reports the main results, Section 7, the robustness tests and Section 8 provides the results on other outcome exploring the existing mechanisms behind the effect. Section 9 concludes.

2 Literature Review

This paper relates to two different branches of the economic literature: the theoretical papers that model the matching process in the housing market (together with the equivalent

and much more extended literature on the labor market) and the empirical branch that evaluates the impact of public policies in the housing market.

Traditionally, several sources of vacancy can be identified on the housing market (Blossier, 2012). The state of vacancy for a housing unit can come from a lack of demand for it (due to quality or location mismatch, for example) or be the consequence of a rational choice of the owner. According to the French National survey on Housing Conditions (Insee, 2001), owners of vacant housing justify vacancy either because they cannot find a buyer or a renter or due to the timing and delays of reconstruction or quality improvement operations. Nearly half of them keep it to sell or rent later. It is important to distinguish between involuntary and voluntary vacancies because ultimately, taxing vacant housing might essentially have an impact through the second type of vacancies.

Desgranges and Wasmer (2000) propose a model in which equilibrium rents are determined as the result of a Nash bargaining equilibrium between workers and houses owners on a housing market with stochastic search frictions on both sides. In their setting, vacancy can result from a stochastic mismatch or from stock retention by the owners, who can in this way maintain a bargaining power to fix higher rents levels by rationing rentable stocks. In this context, taxing vacant housing units increases the incentive of the owners to rent their housing unit at a lower rent because it increases their opportunity cost to keep it vacant. Hence, such policy should increase the workers bargaining power on the housing market by increasing the stock of rental housing and their ability to extract some of the surplus by exerting downward pressures on rent levels. However, in open cities, lower rents would attract new workers in the long run, which would diminish their market power and lead to rents increases, provided housing stock remains constant. Moreover, in this model, the vacancy due to market frictions can only be reduced by an improvement in searching technologies on the housing market, which is not achieved with a tax on vacancy.

More complex forms of speculative vacancy choices could also play a role as supplementary incentives to keep a housing unit vacant. One could expect in the housing market a similar mechanism than the one observed in the second generation of job search models in which unemployed workers searching for a job can decide not to accept some offers, even above their reservation wage, expecting to find better ones by continuing to search the market (see, for example, Pissarides (1986). Indeed, vacancy can be an opportunity to restore or improve the quality of a depreciated housing unit or to better search the demand side to extract more surplus of a rent or a sell. In this case, the opportunity cost of vacancy can be compensated by an anticipated increase in housing prices or rents if the probability to go out vacancy is high enough compared to the odds of entering vacancy, that is, provided there is an equilibrium stationary vacancy rate in cities. In such a framework, a tax on vacant housing could reduce such opportunities of intertemporal arbitrages by increasing the present opportunity cost of holding a housing unit out of the market, for any given sequence of present and anticipated rents or prices levels

Finally, in the short run where the housing stock is fixed, a vacancy tax should act as a positive supply shock on the housing supply and lead to a reduction of prices and/or rents. In the long run, however, it could act as a negative incentive to new construction in taxed cities, which could lead to higher equilibrium housing prices and rates, provided that initial housing demand is elastic or rationed enough. Similarly, there can also be positive effect on equilibrium prices or rate if we consider that high vacancy rate comes with some negative amenities (urban crimes, increased fire risks, reduced social interactions

at a local level, etc.) which are capitalized diminished housing prices and rents. But those inflationary long run price effects become more ambiguous if we consider the impact of vacancy periods on housing quality. By reducing the frequency and the length of vacancy, this type of policy could reduce the speed of regeneration or quality improvement of the actual housing stock, which could lead to lower long run equilibrium prices.

As per the second related branch of literature, there are the papers evaluating the impact of housing policies. On the one hand, we have the evaluation of demand-side policies, typically housing allowances. While demand side policies are theoretically superior in terms of consumer surplus, since they allow consumer choice, they entail some negative consequences. In fact, recent evidence has shown that intervention in the demand side can have inflationary effects due to the inelasticity of the supply (Laferrère and Le Blanc, 2004; Fack, 2005; Grislain-Letrémy and Trevien, 2014; Gibbons and Manning, 2006; Kangasharju, 2003; Susin, 2002). In particular, due to the short-term rigidity of the supply, part of the benefit from the government is directly shifted to the owner through an increase in price.

On the other hand, there is a group of papers that focused on the evaluation of supply-side policies, such as subsidized construction. Several authors in the US have assessed the spillover effects of the Low-Income Housing Tax Credits (LIHTC) on property value. For example, Ellen et al. (2007) shed light on the inconclusive evidence by showing that subsidized rental housing can have positive effects on property value. Similarly, Nguyen (2005) examines seventeen studies on the impact of subsidized housing and three crucial determinants: the design and management of affordable housing, the compatibility between affordable housing and host neighborhood, and the concentration of affordable housing. Finally, a more recent paper evaluated the impact of LIHTC on school quality and found no robust evidence of a negative impact of LIHTC on performance of nearby schools (Di and Murdoch, 2013).

Nevertheless, the only attempt to asses the impact of a tax on vacant housing is the one conducted by Felix Blossier in the context of France. He uses the extension of the tax in 2006 to other cities to identify the effect through a Propensity Score Matching strategy Blossier (2012). He uses data from the national census which provides data on vacancy only every 9 years and finds that the tax is ineffective in reducing the vacancy rate. Nonetheless, the spaced frequency of the census data challenges the identification strategy.

In this paper, we extend the analysis by using an exhaustive fiscal dataset with higher frequency and focusing on the implementation of the tax in 1999, instead of on the extension. We show that, contrary to what the results in (Blossier, 2012) show, the tax did had an effect on reducing vacancy rate.

3 Institutional setting

The French government approved, on July 29th 1998, a tax on vacant apartments (Taxe sur les Logements Vacants or TLV), which was then implemented on January 1st 1999. The rationale behind the introduction of TLV was to encourage owners of empty apartments to re-introduce their properties into the market in order to increase housing supply in areas with a high demand for housing. At the beginning, the tax concerned only

those municipalities belonging to *urban units* [4] of more than 200,000 inhabitants and with a "substantial disequilibrium" between supply and demand at the expense of low-income people. The list of the concerned municipalities was published in the Décret Numéro 981249 on December 29th 1998. It included 680 municipalities in eight urban units (Paris, Lyon, Lille, Bordeaux, Toulouse, Montpellier, Nice and Cannes-Grasse-Antibes). Table 1 reports some demographic characteristics of the urban units as well as the average vacancy rate in 1999.

Table 1: Descriptive Statistics of Taxed Urban Units in 1999

Urban Unit	Vacancy Rate (99)	Average Population (99)	Average Population Growth (90-99)	Urban Unit Population	N° of Municipalities
Lyon	6.54%	10,411	10.08%	1,348,832	83
Paris	7.56%	19,614	8.00%	9,644,507	371
Nice	8.06%	18,817	9.99%	888,784	47
Toulouse	4.36%	12,778	26.87%	761,090	58
Bordeaux	5.49%	16,712	7.61%	753,931	44
Montpellier	5.32%	33,461	22.77%	287,981	8
Lille	5.02%	16,619	4.37%	1,000,900	59

Source: Data comes from FILOCOM dataset for year 1999 plus INSEE datasets on demographic characteristics of the urban units for the same year.

The tax concerned all those apartments with a minimum level of comfort that had been vacant, i.e. empty of furniture, for more than 2 years. A vacant apartment is defined as a unit that has been inhabited for less than 30 consecutive days during the previous two years. The TLV has an increasing tax rate based on the rental value, this is, the potential annual rent that the property could produce had it been rented. In particular, the tax rate of TLV is equal to the 10% of the rental value during the first year when the tax is due, 12,5% for the second year and 15% for longer periods.

Public housing, apartments requiring important reconstruction works and involuntarily vacant units are exempted from the tax. Involuntary vacant units include apartments that are in the market but cannot find a renter or a buyer and those affected by urban plans of rehabilitation or demolition.

In 2006 a new tax was implemented, the *Taxe d'Habitation sur les Logement Vacants* or THLV that allowed the rest of the municipalities to vote the implementation of the tax in the municipal council. Contrary to the TLV, the THLV only concerned those apartments that have been vacant for more than five years (instead of two) and the tax rate is decided at the municipality level; it is usually around 10%. Given that the THLV concerns fewer dwellings and charges a lower tax rate, only municipalities where TLV was not in place could discuss the introduction of THLV.

Finally, in 2013, the TLV was reinforced with a raise of the tax rate (to 12,5% of the rental value for the first year and 25% for the second year) and the reduction of the threshold for compulsory implementation from 200,000 to 50,000 inhabitants. A second decree was published with the list of the municipalities where the implementation of the TLV was compulsory. It included 1151 municipalities from 28 urban units. The period of vacancy accepted before paying the tax was as well reduced from two to one year.

Apart from the TLV and the THLV, in France there exists also a tax on housing

[4]INSEE defines "urban unit" as an area of continuous construction (without a separation of more than 200 meters between buildings) with at least 2,000 inhabitants.

(*Taxe d'Habitation* or TH) to be paid by every individual having an apartment at his disposal, either in ownership or renting regime and both for the primary and for the secondary residence. Therefore, this tax is charged in all apartment but vacant ones. It is one of the four fiscal tools that are set at the local level; hence tax rates are decided by local authorities, this includes city halls, regional or departmental authorities and other communities with special regimes. The average tax rate for all France is 8.6% while for the municipalities in our group of analysis is 13.6%. All primary residences enjoy a reduction of the TH. Moreover, there are also exemptions for low-income households, tax caps according to revenue and temporary reductions. Hence, the TH is only fully paid by the owner of secondary residences.

Lastly, there is the *Taxe Foncière*, a property tax charge on owners of habitation, commercial or industrial dwelling. It is as well a municipal tax whose base is the rental value and whose rates are decided at the local level, with an average of 29% at the national level in 2004.

4 Data

We exploit the fiscal dataset FILOCOM (FIchier des Logements par COMmune) containing information on the payments of the *Taxe d'Habitation* (TH) for every dwelling in metropolitan France from 1995 to 2005. The dataset consists of around 30 million observations per year distributed in 36,170 municipalities. For most of our analysis we aggregate the data at the municipality level for our municipalities of interest, which are 971.

FILOCOM is available from 1995 and until 2013, every odd year. However, we only use it up to 2005 because in 2006 there was an extension of the tax (THLV) and in 2007 the beginning of the financial crisis which could both blur our results. This dataset, created by the General Direction of Public Finances (DGFIP), contains information on the characteristics of the housing stock (surface, level of comfort, building characteristics), on household's characteristics (income, household's size, age of the members) and on the status, the mode and the length of occupation. The variable status of occupation classifies dwellings into three categories according to usage: inhabited by the owners, inhabited by renters — either in private, collective or social housing regime — or others uses — such as free occupancy or rural lease —. There is also a variable of the mode of occupation that distinguishes three[5] categories according to the usage (primary residency, secondary residency or vacant). Even if only primary and secondary residences apartments pay the TH, vacant apartments are still kept in the dataset. Other interesting variables are the length of occupancy (or vacancy) and the rental value, which is the tax base on which both the TH and the vacancy tax (TLV) are charged. Nevertheless, one needs to be cautious when evaluating the rental value since there is a widening gap between rental value and real renting prices.[6]

The status of vacancy is measured in FILOCOM according to the situation of the

[5]There is actually a forth category as well that includes part of commercial properties which are partially accommodated to live, they represent less than 0.5% of the sample and are therefore excluded from our sample.

[6]This is due to the fact that the methodology applied to compute it uses a fixed rate to actualize the value of the market price in 1970. While the law that established such methodology in 1974 expected an actualization every two years and a general revision every six years, neither of them have taken place.

dwelling on the 1st of January of a given year.[7] This is, if an apartment was non-occupied and empty of furniture on the 1st of January 1997 it did not pay the TH in 1997 and hence is considered as vacant. Hence, the vacancy rate is computed by dividing the total number of vacant dwellings in a municipality over the total number of dwellings in such municipality. It is important to notice that not all vacant apartments are concerned by the tax, only those that have been vacant for at least two years. Given that social housing institutions (HLM and SEM in the French government's terminology) are not concerned by the TLV, we do not consider the vacancy on the social sector. Hence, the outcome of interest will be the private vacancy rate.

We also use other datasets from INSEE for the basic characteristics of a municipality. Namely, population census, growth and density are going to be used as controls for the regression models. Data on prices comes from the Chamber of Notaries.

5 Empirical Strategy

We implement a Difference-in-Difference (DID) estimation strategy exploiting the fact that not all the municipalities implemented the tax in 1999. The DID strategy is a mean comparison design that implies, de facto, comparing four different groups, three of which are not affected by the treatment. In other words, we use the mean changes of the outcome variables for the non-treated and we add it to the initial level of outcome of the treated group to obtain the level that the treated group would have had without the treatment, what is commonly known as the counterfactual value (Lechner et al., 2011).

The main advantage of the DID strategy is that it allows for the two groups to start from a different level of outcome as long as they experienced a similar change over time. The DID strategy relies on one very important assumption: the common trend assumption. It implies that both groups, conditional on observables, should have experienced the same trend on the outcome variables had there been no treatment. Hence, if the common trend assumption holds, any deviation of the trend of the treated group from the trend of the non-treated can be directly attributed to the effect of the treatment.

$$E[V_{0t'} - V_{0t} \mid TLV = 0] = E[V_{0t'} - V_{0t} \mid TLV = 1] \tag{1}$$

In other words, the evolution of the vacancy rate in the treatment group – right part of equation (1) – would have been the same than the one in the control group had it not been treated. Moreover, we also need to assume that the treatment had no effect whatsoever before its introduction (or announcement). This is indeed plausible in this case, since we would not expect the owners of vacant apartments to behave in a different way because of the tax before its announcement in 1998.

Formally, the model we estimate is:

$$V_{mt} = \alpha + \beta Post + \gamma TLV_m + \delta TLV_m * Post + X_m + \mu_m + \varepsilon_{mt} \tag{2}$$

where V_{mt} is the ratio of vacant apartments over the total stock of housing defined in municipality m at time t, $Post$ is a dummy equal 1 if the the tax was already implemented

[7] There exists some discrepancies on the way vacancy is measured between the national census and the FILOCOM dataset. The vacancy rate estimated by FILOCOM is usually two percentage points higher. This discrepancy is basically due to a different definition of vacancy. In any case, this overestimation is likely to be constant over time, and hence it does not cause a bias.

and zero otherwise, TLV_m is a dummy equal 1 if the municipality was concerned by the tax and zero otherwise, X_m is a vector of housing, demographic and geographic characteristics at the municipality level . We include all those set of controls that could lead to a differential time trend. This is, we have selected the covariates X whose distributions differed between treatment and control. We include a group of demographic variables from 1999 comprising the population, population growth (from 1990 to 1999) and density; a group of housing controls with the average revenue, the rental value in 1997 and the average surface and, finally, geographic controls include latitude, longitude and its interaction. In some specifications we also include urban unit fixed effects (μ_m). Given that our observations are grouped into 30 different urban units, it could be reasonable to think that municipalities from one urban unit behave differently from those of another urban unit. With urban unit fixed effects we are left only with the variation *within* urban units.

In model of equation (3) we add year fixed effects (θ_t) and we interact them with the treatment variable (TLV_m) to see the yearly effect of belonging to the taxed group. In the last specification of this model we also include a specific time trend for each urban unit ($\theta_t * \mu_m$). This is a linear trend representing the hypothetical evolution of the outcome. Although it captures a significant part of the time variation of the outcome it allows to identify any deviation from the linear trend.

$$V_{mt} = \gamma TLV_m + \delta TLV_m * \theta_t + X_m + \theta_t + \mu_m + \theta_t * \mu_m + \varepsilon_{mt} \tag{3}$$

The treatment group for our DID approach includes those taxed municipalities that belonged to urban units of more than 200,000 inhabitants in 1999 and with a significant imbalance between housing supply and demand. This concerns 672 urban municipalities from 7 urban units, of which we use 299, from 6 urban units since we exclude the urban unit of Paris. Given that housing market in Île-de-France department is under much more pressure than the rest of the country, we exclude it from the sample. Such higher pressure can be noticed when looking at the average price per squared-meter, which is as high as 5,470€/m^2 for Île-de-France while roughly around 3,000€/m^2 for the rest of important cities (3,210€/m^2 in Lyon and Toulouse, 2,930€/m^2 in Bordeaux, 3,070€/m^2 in Lille and around 2,500€/m^2 in Marseilles, Nantes and Rennes).[8] Indeed, Île-de-France plays in another league as far as the housing market is concerned. Keeping it in the sample would make the common trend assumption less believable since the evolution of market prices and housing variables in Île-de-France can very well be affected differently by external factors and hence, experience a trend in outcomes different from the one in other French cities. Although removing Île-de-France implies a reduction of the treatment group by half (to 300 municipalities) results are still significant since the sample is big enough.[9]

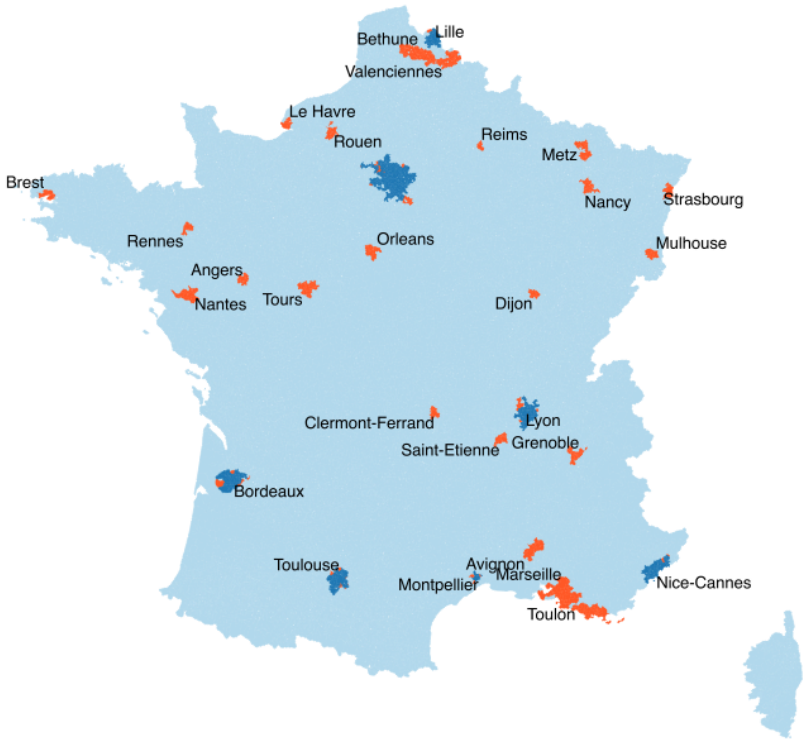
Figure 2, shows the distribution of treatment and control urban units across the country. It can be seen that our sample is made of urban units spread among all the country. Most regions have either treatment or control urban units and only three of them — Nord Pas de Calais, Rhône-Alpes and Provence-Alpes Côte-d'Azur — have both.

As per the control group we use the rest of the municipalities belonging as well to urban units of more than 200,000 inhabitants but that the government considered not to

[8] Chambre de Notaires de Paris, June 2013.

[9] This exclusion does not change the magnitude nor the significance of the coefficients in any of the main results tables. It only changes the Placebo test coefficients that become positive and significant, which indicates that indeed the treatment group with Paris behave differently than without it

Figure 2: Distribution of treatment and control urban areas across France



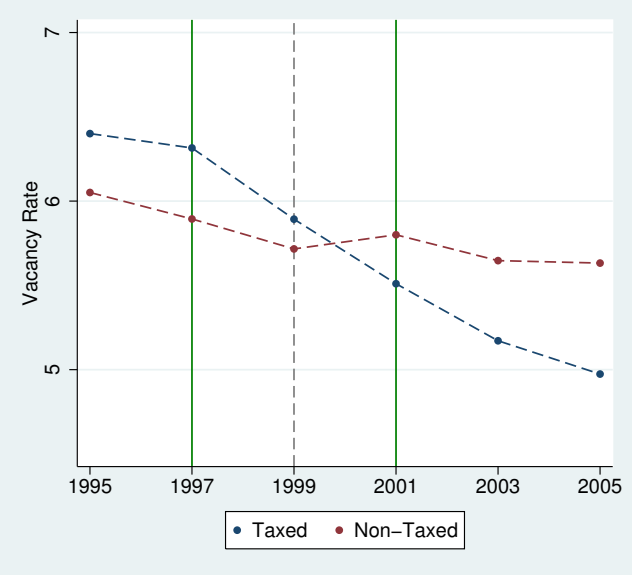
Notes: Blue areas represent urban units with the tax (treated) and red pins urban units without the tax (control). Shaded areas represent regions with at least one treated and one control urban unit.

have a substantially big enough "disequilibrium between supply and demand" to be taxed. In this group there are 697 municipalities belonging to the other 23 urban units of more than 200,000 inhabitants[10].

Once the two groups have been selected, the DID strategy consists in comparing the evolution of the outcome before and after the implementation of the tax for the two groups. Hence, the choice of the time period is as well crucial. Ideally, we would like to have information from the point immediately before any sort of treatment (in this case, the tax) and then compare it with a point in time when the tax is already in place. In our dataset, we have three points in time before the implementation: 1995, 1997 and 1999. Technically, data from 1999 contains information on the very first day of the implementation of the tax, January 1st, 1999. Consequently, it is ambiguous whether the point 1999 should be considered as pre- or post- treatment. Moreover, given that the tax was already announced in July 1998, it would be reasonable to expect some anticipation from the households. This is, some households might have been affected by the tax even before its implementation and might have changed their behavior already during the second half of 1998. Ideally, we would like to use 1998 as a pre-treatment point but since data is only available every odd year, we will use 1997 instead. This way, we make sure we are capturing the entire effect of the tax (anticipation + direct effect) while we still have two points in time to check the common trend assumption.

[10]This includes Avignon, Bèthune, Saint-Etienne, Metz, Douai-Lens, Toulon, Marseille-Aix-en-Provence, Dijon, Brest, Rennes, Tours, Grenoble, Nantes, Orléans, Angers, Reims, Nancy, Valenciennes (French part), Clermont-Ferrand, Strasbourg (French part), Mulhouse, Le Havre and Rouen.

Figure 3: Evolution of vacancy rate



Notes: This graph displays the mean of the vacancy rate for taxed and non-taxed municipalities. Taxed municipalities include all taxed urban units except Paris (299 municipalities in 6 urban units). Control municipalities include 671 municipalities in 23 urban units. Data comes from FILOCOM dataset for years 1995 to 2005.

Figure 3 shows the evolution of vacancy rate for taxed municipalities (in blue) and for non-taxed ones (in red). The first thing that can be observed is that the starting level of vacancy in 1995 for the treatment group is higher than in the control group, which could justify the choice made by the French administration when determining the municipalities with a "higher disequilibrium between supply and demand". Secondly, it can be seen that the vacancy rate decreases only slightly for non-taxed municipalities, less than 0.5 percentage points in a 10-years period whereas it decreases significantly in taxed municipalities, going from 6.5% to 5%, and reaching a level below the control group. The gray dashed line indicates when the tax was officially implemented and the green vertical lines, the pre and the post time points compared, 1997 versus 2001.

Finally, Figure 3 also allows to visually check the common trend assumption by looking at the slope of the outcome between 1995 and 1997. The trends for the treated and the control group are both slightly decreasing and almost parallel, which makes the common trend assumption fairly plausible. At worst, the fact that the control group decreases slightly more than the treatment group could lead to an underestimation of the true effect, which would make our estimates a lower bound.

Table 2 displays the descriptive statistics for the main outcome and explicative variables for 1997. It can be seen that most of the variable means (except private vacancy rate and population census) are statistically different in the two groups. This is a bit problematic since it shows that the two groups were not similar before the implementation of the tax and hence, even if it is not a required assumption for the DiD model, it makes it less likely that the two groups would have behaved similarly with respect to the vacancy rate. This is why we implement as well a Matching DiD strategy that weights observations in the treatment and the control group having more similar characteristics.

For that purpose, we first estimate the probability of having been assigned to the tax as a function of descriptive statistics with a Probit Model (equation 4) and obtain a

Table 2: Descriptive statistics 1997

	Treatment		Control				
	Mean	Std Dev.	Mean	Std Dev.	Difference	t-value	p-value
Vacancy Rate	6.32	3.18	5.89	2.88	-0.43	2.04	0.04
Private Vacancy Rate	6.39	3.25	6.07	2.98	-0.32	1.47	0.14
Av. Income	22462.51	5586.52	19479.42	5887.00	-2983.09	7.41	0.00
Population 99	16395.51	45937.25	12745.87	41082.51	-3649.64	1.23	0.22
Population Growth	12.15	15.88	6.55	11.88	-5.60	6.09	0.00
Density 99	1234.77	1552.90	885.35	988.53	-349.42	4.22	0.00
Rental Value	18199.80	5055.44	15378.54	4640.35	-2821.26	8.51	0.00
Surface	90.24	13.71	85.95	12.80	-4.29	4.72	0.00
Primary Residence	89.21	9.81	91.45	6.04	2.24	-4.34	0.00
Social Housing	10.36	12.40	12.20	11.90	1.84	-2.20	0.03

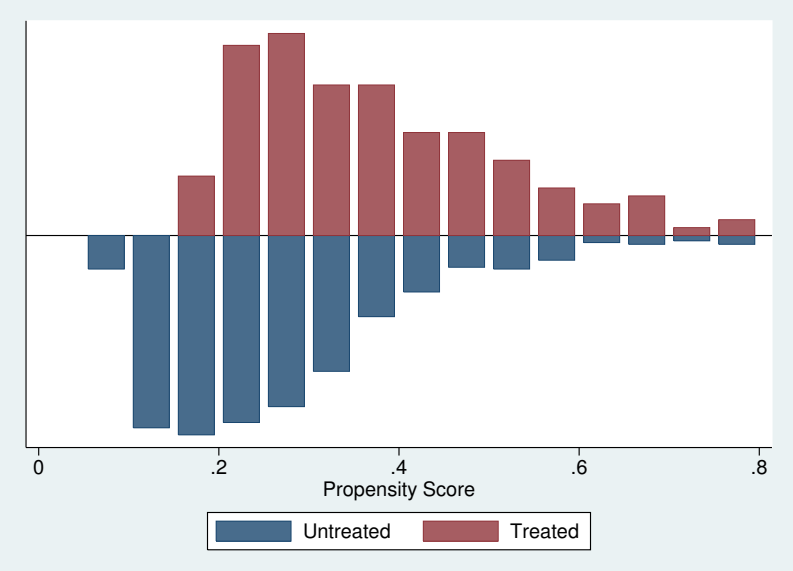
Notes: Data comes from FILOCOM dataset for year 1997 plus INSEE datasets on demographic characteristics of the municipalities for 1999. Treatment group has 299 observations while Control group has 671.

propensity score for each observation. The variables that we use to compute said probability are the same variables used for regression (2) and (3) plus the initial level of vacancy in 1997. The geographical variables are excluded in one of the specifications.

$$PS(TLV_m) = \alpha + X_m + \varepsilon_{mt} \tag{4}$$

For the matching strategy to be valid we need to check that there is Common Support, meaning that for each level of X there is a positive probability of being assigned to treatment.

Figure 4: Common Support



Notes: This graph plots the frequency of municipalities according to the estimated propensity score. Data comes from FILOCOM dataset for years 1997 to 2001

(5) $$0 < PS(TLV_m) < 1$$

Figure 4 shows that for each observation in the treatment group there is at least one in the control group with a similar level of propensity score. Hence, we do not have to remove any observation of the sample to ensure a common support.

Then we match observations according to their propensity score using the Kernel method, which matches observations to a weighted average of the other observations. Weights depend on the distance between observations with respect to propensity score, the closer, the higher the weight. The main advantage of using a Kernel method is that all observations are used which reduces the variance of the estimators.

Finally, we perform another test to check that the matching strategy has been appropriately computed. In particular, we want to make sure that the Kernel algorithm has weighted observations in a way that descriptive statistics of the resulting groups are similar to each other. For that purpose, we report in Table 3 the means and the descriptive statistics by different levels of propensity score. We report as well the p-value resulting from a means different t-test between the two groups. It can be seen, that once the matching is performed, the treatment and control group are much more similar in terms of descriptive statistics. All variables appear to be not statistically significant between treatment and control for the three groups of propensity score, except from average income and population growth in the first group.

Table 3: Descriptive statistics by different levels of propensity score

	PS < 0.24			$0.24 <$ PS < 0.41			PS > 0.41		
	T	C	p-value	T	C	p-value	T	C	p-value
Private Vacancy Rate	4.98	6.58	0.29	6.35	6.18	0.67	7.59	5.86	0.09
Av. Income	21028	17238	0.05**	20243	20588	0.48	25472	25427	0.97
Population 99	8281	9744	0.84	13223	16381	0.50	20818	14966	0.29
Population Growth	3.14	1.08	0.06*	8.17	9.19	0.35	17.39	16.76	0.78
Density 99	696	695	0.99	944	926	0.88	1585	1324	0.24
Rental Value	13744	13469	0.87	16498	16650	0.71	21450	19911	0.16
Surface	87.40	84.86	0.72	88.66	86.79	0.17	95.49	94.27	0.77
Primary Residence	93.70	96.35	0.79	91.62	90.85	0.19	90.66	88.61	0.68
Social Housing	11.99	14.91	0.30	13.06	10.99	0.17	8.87	8.67	0.91
N	53	372		110	208		137	91	

Notes: Data comes from FILOCOM dataset for year 1997 plus INSEE datasets on demographic characteristics of the municipalities for 1999. Each propensity score group gathers a third of the observations.

6 Results

Table 4 presents the main results of the DID strategy of the effect of the TLV on the private vacancy rate, two years after its implementation. It is the result of estimating equation (1). We compare the number of vacant dwellings over the total stock of housing for taxed and non-taxed municipalities in 1997 and 2001. Columns (1) to (4) report four different OLS specifications while columns (5) and (6) report the matching results.

Column (1) is the direct DiD estimator without any other controls, in column (2), we include the three sets of controls described above (housing, demographic and geographic controls). In columns (3) we add urban unit fixed effect and in column (4), the interaction

Table 4: Effect of TLV on vacancy rate, comparing 1997 *vs.* 2001

	OLS				Matching	
	(1)	(2)	(3)	(4)	(5)	(6)
TLV	0.314	0.435	-0.766***	-0.794**		
	(0.569)	(0.792)	(0.310)	(0.336)		
Post	-0.142*	-0.160	-0.313***	-0.340***		
	(0.085)	(0.118)	(0.095)	(0.123)		
ATT	-0.768***	-0.788***	-0.810***	-0.689***	-0.911***	-0.853 ***
	(0.159)	(0.139)	(0.123)	(0.141)	(0.164)	(0.198)
ATT*LargeCity				-0.419**		
				(0.195)		
Housing Controls		X	X	X	X	X
Demographic Controls		X	X	X	X	X
Geographic Controls		X	X	X		X
UU Fixed Effects			X	X		
N	1942	1942	1942	1942	971	971

*Notes:*Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parenthesis, for OLS they are block bootstrapped at the urban unit level (29 clusters), for the Matching, they are bootstrapped. Each column represents a different regression with vacancy rate as the dependent variable in columns (1) to (4) and first difference of the vacancy rate for (5) to (6). Last two columns use a propensity score matching strategy to weight control observations. Variable Post equal 1 for year 2001 and 0 for 1997. ATT is the interaction of TLV and Post for OLS and TLV for the Matching columns. Housing controls include rental value from 1997, the average surface, the average revenue and the proportion of social housing. Demographic controls include population in 1999, population growth 90-99 and density in 1999. Geographic controls are the corresponding latitude and longitude. Regressions with urban unit fixed effects have 29 groups. Data comes from FILOCOM dataset for years 1997 and 2001 plus INSEE datasets on demographic characteristics of the municipalities for 1999. Treatment group has 300 observations in 6 clusters while Control group has 671 in 23 clusters per year.

of a dummy for big cities and the treatment variable[11]. The coefficient ATT represents the average impact of the tax on those municipalities where TLV was implemented. The effect is negative and statistically significant in all specifications, both for the OLS and for matching. Standard errors are block bootstrapped at the urban unit level for the OLS regressions[12]. The resulting effect size is a reduction of 0.8 percentage points. Considering that the average vacancy rate for taxed municipalities in 1997 was 6.32% a decrease of 0.8 percentage points is equivalent to a reduction of the 13%. In absolute terms, there were 40,000 less vacant apartments in treated municipalities than there would have been without the implementation of the tax.

In terms of policy implications it is interesting to look at the heterogeneity of the effect according to the size of the municipality. In column (4) we check this by interacting the treatment with a dummy of the size which appears to be significant and negative. The magnitude of the interaction is -0.4 which means that the effect of the tax is 50% higher for cities of more than 10,000 inhabitants. Indeed, high populated treatment cities had a much higher vacancy rate at the beginning of the period (7.82%) which decreased to 6.68%

[11]LargeCity equals 1 if population is higher than 10,000 inhabitants, which corresponds to the percentile 75 of the distribution

[12]Given that the number of clusters is considerably low (30 urban units) a block bootstrapped treatment of the standard errors is preferred to the standard clustering method

in 2001, while the control group stayed constant (at a level of 6.63%).

As in the labor market there is a *natural rate* of unemployment, in the housing market there is, likewise, a *natural rate* of vacancy. This means that there is an in-compressible level of vacancy that will never disappear due to market frictions. When an apartment becomes vacant, it is likely that it will remain vacant for at least a specific period of time due to transaction costs. Although the existence of a *natural rate* is generally accepted in the housing market literature, few are the estimations of its magnitude. There is, however, a report by the Economics Affaires Commission of the French Senate that reports a "generally accepted" rate of vacancy ranging between 4% and 5% (Cleach, 2003). If we take this into account, the magnitude of our estimated coefficients is very relevant since such natural vacancy rate would have been reached by the treatment group in 2005.

A concern that can challenge our results is that in 2000 the French government approved and implemented another housing law: the "Loi relative à la solidarité et au renouvellement urbaine" or Law SRU. The most significant article of this law is article 55 that aims at increasing social mixing by requiring all municipalities of more than 3,500 inhabitants to have at least a 20% of social housing. If this law has been effective and the supply of social housing has increased, it may have had an effect on vacancy which would be higher in the municipalities where the supply of social housing has increased more. Given that this could bias our results, we have performed a test consisting in adding a variable for whether a municipality is concerned by the Law SRU and its interaction with the proportion of social housing. Results remain the same in terms of magnitude and significance and can be seen in Table A9. Hence, this law does not seem to affect our results.

Table 5 provides the yearly effect of TLV taking 1995 as a reference year. Here, instead of having a dummy for before and after the treatment, we include year fixed effects and we interact them with the tax. This allow us to see how the vacancy rate changed in treatment versus in control municipalities every period. Coefficients of the interaction terms for 1997 are not significant. These results indicate that the vacancy rate did not evolve in a statistically significant way for treated and non-treated municipalities before the implementation of the tax. Therefore it can be interpreted as, again, a test of the common trend assumption.

As per the interaction term with year 1999, coefficients are already negative and significant only for some specifications, the urban unit fixed effects model and the matching strategy. It suggests that there had been some anticipation effects of the tax. In Figure 3, the anticipation effect can already be seen since the vacancy rate starts to decrease after 1997. Interestingly enough, there is also a decrease in non-taxed municipalities, which could indicate an anticipation effect happened as well in the control group, given that the list of concerned municipalities was not public until December 30^{th} 1998.

Lastly, the coefficients of the interaction terms for 2001 to 2005 are negative and highly significant. The biggest decrease occurred in 2001 with coefficients slightly smaller than in Table 4. The long term effect of the tax can be observed with the interaction with 2005 and it is equivalent to a decrease of 1 percentage points of the vacancy rate.

Table 5: Vacancy rate, yearly effect

	OLS				Matching	
	(1)	(2)	(3)	(4)	(5)	(6)
TLV*1997	0.137	0.110	0.036	-0.040	-0.012	-0.140
	(0.144)	(0.142)	(0.122)	(0.282)	(0.104)	(0.119)
TLV*1999	-0.132	-0.172	-0.277*	-0.102	-0.410***	-0.513***
	(0.125)	(0.139)	(0.128)	(0.192)	(0.134)	(0.150)
TLV*2001	-0.632***	-0.673***	-0.768***	-0.346***	-0.924***	-0.994***
	(0.143)	(0.139)	(0.134)	(0.067)	(0.160)	(0.179)
TLV*2003	-0.846***	-0.920***	-1.104***	-0.616***	-1.110***	-1.225***
	(0.228)	(0.228)	(0.281)	(0.120)	(0.178)	(0.190)
TLV*2005	-0.943***	-1.020***	-1.213***	-0.662**	-1.364***	-1.376***
	(0.171)	(0.170)	(0.218)	(0.244)	(0.223)	(0.205)
Housing Controls		X	X	X	X	X
Demographic Controls		X	X	X	X	X
Geographic Controls		X	X	X		X
UU Fixed Effects			X	X		
Year Fixed Effects	X	X	X	X		
Linear Time Trend				X		
N	5826	5826	5826	5826	971	971

Notes: Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parenthesis, for OLS they are block bootstrapped at the urban unit level (29 clusters), for the Matching, they are bootstrapped. Each column represents a different regression with vacancy rate as the dependent variable in columns (1) to (4) and first difference of the vacancy rate for (5) to (6). Reference year is 1995. Last two columns use a propensity score matching strategy to weight control observations.. ATT is the interaction of TLV and Post for OLS and TLV for the Matching columns. Housing controls include rental value from 1997, the average surface, the average revenue and the proportion of social housing. Demographic controls include population in 1999, population growth 90-99 and density in 1999. Geographic controls are the corresponding latitude and longitude. Regressions with urban unit fixed effects have 29 groups. Data comes from FILOCOM dataset for years 1995 to 2005. Treatment group has 299 observations in 6 clusters while Control group has 671 in 23 clusters per year.

7 Robustness Test

Although the common trend assumption can never be really tested, we provide some intuition of its validity by computing a placebo test. This means, we have conducted the same DiD strategy on the period before the implementation to check whether the treatment and the control group were already different before the tax. Table 6 presents the results of the Placebo Test where none of the DiD coefficients are significant, even after the inclusion of controls, fixed effects and for the matching method. This results indicates that indeed the two groups evolved similarly before the ta and hence suggest that they might have evolve equally had the tax not been implemented.

On a second robustness test, we restrict the sample to only those regions in France that have both taxed and non taxed urban units. The reason of doing so responds to an effort to try to find two comparison groups that are more similar and on which the common trend assumption would be even more plausible. Indeed, the fact that treatment and control urban units belong to a same region makes it more likely that they will be affected in a similar way by exogenous factors. There are only three regions in France with treated and control urban units: Nord Pas de Calais, Rhone Alpes and Provence Alpes

Table 6: Placebo Test (comparing 1995 *vs.* 1997)

	OLS			Matching	
	(1)	(2)	(3)	(4)	(5)
TLV	0.177	-0.014	-0.364		
	(0.480)	(0.681)	(0.274)		
Post	-0.192**	-0.184	-0.375***		
	(0.084)	(0.164)	(0.139)		
ATT	0.137	0.127	0.053	-0.012	-0.140
	(0.144)	(0.095)	(0.075)	(0.105)	(0.109)
Housing Controls		X	X	X	X
Demographic Controls		X	X	X	X
Geographic Controls		X	X		X
UU Fixed Effects			X		
N	1942	1942	1942	971	971

*Notes:*Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parenthesis, for OLS they are block bootstrapped at the urban unit level (29 clusters), for the Matching, they are bootstrapped. Each column represents a different regression with vacancy rate as the dependent variable in columns (1) to (3) and first difference of the vacancy rate for (4) to (5). Last two columns use a propensity score matching strategy to weight control observations. Variable Post equal 1 for year 1997 and 0 for 1995. ATT is the interaction of TLV and Post for OLS and TLV for the Matching columns. Housing controls include rental value from 1997, the average surface, the average revenue and the proportion of social housing. Demographic controls include population in 1999, population growth 90-99 and density in 1999. Geographic controls are the corresponding latitude and longitude. Regressions with urban unit fixed effects have 29 groups. Data comes from FILOCOM dataset for years 1995 and 1995 plus INSEE datasets on demographic characteristics of the municipalities for 1999. Treatment group has 300 observations in 6 clusters while Control group has 671 in 23 clusters per year.

Cote d'Azur[13].

We provide the results in Table 7. Columns (1) and (2) report the OLS regressions, whose coefficients remain of the same magnitude and significance. Column (3) reports the results of the Matching strategy, with a much higher coefficient and the same significance. This indicates that our results are robust to a reduction of the sample.

Finally, we choose a different methodology to select the control group to check the validity and magnitude of our coefficients. In this case, we use those municipalities that implemented the tax after the first reform of the law in 2006. This reform allowed municipalities not concerned by the TLV to vote and approve the THLV (a local tax on vacancy). In 2008, there were 1,938 municipalities that had adopted the tax among which 608 were in urban areas.

The idea behind this second choice of control group is that municipalities that implemented the tax in 2008 may share some characteristics with the ones that adopted it in 1999. This is, it is possible that the politicians of the moment considered that they too were under a 'substantial disequilibrium between supply and demand'. In fact, those cities had as well a high level of vacancy in 1999 (7.96%), which may explain why they implemented the tax later on.

[13]In Nord Pas de Calais there is one treated urban unit (Lille) and three control (Béthune, Douai-Lens and Valenciennes). In Rhone Alpes there is also one treatment urban unit (Lyon) and two control (Saint-Etienne and Grenoble). And finally, in Provence Alpes Cote d'Azur there are two treatment urban units (one composed by Nice and Cannes altogether and Marseille) and three control urban units (Avignon, Aix-en-Provence and Toulon)

Table 7: Robustness Test: Testing the impact of TLV on different samples

	Regions with both T and C units			Adoption of THLV in 2008		
	OLS		Matching	OLS		Matching
	(1)	(2)	(3)	(4)	(5)	(6)
TLV	0.410	-0.592		-1.121**	-1.428**	
	(1.203)	(0.364)		(0.487)	(0.621)	
Post	-0.225*	-0.374***		-0.204**	-0.244***	
	(0.119)	(0.117)		(0.089)	(0.089)	
ATT	-0.762***	-0.803***	-1.179***	-0.727***	-0.826***	-0.841***
	(0.230)	(0.206)	(0.198)	(0.169)	(0.115)	(0.159)
Housing Controls	X	X	X	X	X	X
Demographic Controls	X	X	X	X	X	X
Geographic Controls	X	X	X	X	X	X
UU Fixed Effects		X			X	
N	1078	1078	539	1814	1814	906

*Notes:*Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parenthesis, for OLS they are block bootstrapped at the urban unit level (11 and 439 clusters per sample), for the Matching, they are bootstrapped. Each column represents a different regression with vacancy rate as the dependent variable in OLS and first difference of the vacancy rate for Matching. We use a propensity score matching strategy to weight control observations. Variable Post equal 1 for year 2001 and 0 for 1997. ATT is the interaction of TLV and Post for OLS and TLV for the Matching columns. Housing controls include rental value from 1997, the average surface, the average revenue and the proportion of social housing. Demographic controls include population in 1999, population growth 90-99 and density in 1999. Geographic controls are the corresponding latitude and longitude. Regressions with urban unit fixed effects have 29 groups. Data comes from FILOCOM dataset for years 1997 and 2001 plus INSEE datasets on demographic characteristics of the municipalities for 1999.

When we replicate the results with this new choice of control group (Table 7 columns 4 to 6), we see that the ATT coefficients is as well significant and of a similar magnitude than in our main estimation both for the OLS and the matching strategy. Hence, this alternative strategy shows that our results are not tied to the choice of the control group.

8 Impact on other outcomes

So far, we have attempted to assess the first direct effect of TLV after its implementation in 1999. We find that there is a strong and significant effect of the tax on the reduction of vacancy rate. The assessment of the direct effect is a first and necessary step to investigate the role of fiscal policies in housing outcomes. Now, we would like to know why this effect occurred and, notably, how. Which are the mechanisms through which the tax had an effect? What had happened with the apartments that were vacant before the implementation of the tax? It is clear that the owners of vacant apartments reacted to the tax by changing the status of their apartments, but can we understand the relevant elements of their decision process? In what follows, we provide a first plausible explanation of what could be the mechanisms underlying this effect.

We are interested in the effect of the tax on different outcomes. On one hand, we aim to assess how have the two complementary status of vacancy evolved due to this decrease in vacancy. This is, has it translated into an increase of the primary residence ratio or

in the secondary residence ratio[14]? One could think that the owner has interest to act strategically by transferring a vacant apartment into a secondary residence in order to keep it at their disposal and avoid renting or selling it. To do so, they need to furnish it and to pay the Habitation Tax (TH). Then, the decision of moving the apartment away from vacancy will depend on the relative cost of the TH compared to the TLV. We first look at what happened with the primary and the secondary residence ratio (columns 3 to 6 of Table 8), we see that most of the decrease in vacancy seems to be translated into an increase in primary residence ratio, at least for the short term effect (-0,9 in vacancy and 0.6 in primary ratio). However, in columns (5) and (6) there is, as well, a slight increase on the secondary residence ratio, of 0,17. Thus, even though this reaction does not seem to be the predominant one, there are some owners who move their apartments into secondary residences after the implementation of TLV. In any case we would not have expected a high increase in the secondary residence ratio since in 1999, the tax rate of the TLV was 10% for the first year of tax payment while the average tax rate of the Taxe d'Habitation (TH) was 14%, which indicates that such a strategic behavior was not optimal in average. This could explain why, when testing for heterogeneous effects according to the level of the TH we find no significant results.

Secondly, since theoretically a positive shock of supply is expected right after the implementation of the tax, it is interesting to see whether this shock has had a negative impact on prices. When an owner decides to take an apartment out of vacancy, she can decide to either sell it or rent it. Hence, a shock would happen in both the rental and the purchasing market of housing. We assess the impact on purchasing prices and we found no significant effect, although negative coefficients. Then, we cannot say that the shock in supply was strong enough to have an identifiable effect on prices.

As a measure of mobility, we use the proportion of ownership changes over all apartments. This measure includes apartments that have been sold but also any kind of transfer or donation implying a change in name on the ownership documents. We find a positive and significant coefficient which indicates that, as theory would predict, there is more mobility in the housing market after the implementation of the tax.

Finally, we also look at whether the TLV had an effect on new construction of housing. If the introduction of the TLV have increased the cost for owners of having an apartment empty, it may as well have changed the incentive for the investment in new housing. This effect, if it exists, we would expect it in the long term. Columns (11) and (12) report the results of the impact of the tax on new construction. They are not significant in any of the cases but they are negative for the long term specification. Hence, we can not state that the tax shrank the supply of housing in the long term even if the coefficients go in the right direction[15].

[14] It is also important to consider the possibility of demolition. The French Ministry of Housing has computed an approximate figure of the apartments that have been destroyed using the FILOCOM database. In a 10-years period, from 1999 to 2009 the rate of disappearance has been estimated to be around 1%, i.e. 0.1% per year in average. Hence, this possibility can be deemed negligible for our analysis.

[15] We also look at other variables such as household size, proportion of tenants, the average income level or the distribution and we find no effect

Table 8: Effect of the tax on different outcomes

PS Matching	Vacancy Rate		Primary Residence		Secondary Residence		Price		Ownership Changes		New Construction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
TLV	-0.897***	-1.139***	0.612***	0.726***	0.172 **	0.172 **	-5005	-1726	0.400*	0.557 **	0.32	-0.165
	(0.157)	(0.205)	(0.155)	(0.256)	(0.104)	(0.099)	(50619)	(100887)	(0.250)	(0.201)	(0.257)	(0.287)
Mean of C in 97	6.07		91.09		2.54		84755		8.37		2.18	
Post Period	2001	2005	2001	2005	2001	2005	2001	2005	2001	2005	2001	2005
Housing Controls	X	X	X	X	X	X	X	X	X	X	X	X
Demographic Controls	X	X	X	X	X	X	X	X	X	X	X	X
Geographic Controls	X	X	X	X	X	X	X	X	X	X		X
UU Fixed Effects	X	X	X	X	X	X	X	X	X	X	X	X
N	1942	1942	1942	1942	1942	1942	1703	1703	1942	1942	1942	1942

Notes: Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors, in parenthesis, are bootstrapped. Each column represents a different regression which dependent variable is indicated every two columns, the first difference of the outcome is used. We use a propensity score matching strategy to weight control observations. In the short term effect, variable Post equals 1 for year 2001 and 0 for 1997, for the long term it equals 1 for year 2005. Housing controls include rental value from 1997, the average surface, the average revenue and the proportion of social housing. Demographic controls include population in 1999, population growth 90-99 and density in 1999. Geographic controls are the corresponding latitude and longitude. Data comes from FILOCOM dataset for years 1997 and 2005 plus INSEE datasets on demographic characteristics of the municipalities for 1999.

9 Conclusion

In this paper, we evaluate the impact of the *Taxe sue les Logement Vacants*, implemented in 1999 in 680 municipalities in France, on the vacancy rate. The setting of the implementation of the tax in a subgroup of municipalities allows for a clean identification of a treatment and a control group, which are then compared using a Difference-in-Difference approach. We find that the introduction of the tax represented a decrease of 0.8 percentage points of the vacancy rate for treated municipalities with respect to control ones. In other words, vacancy rate was reduced by 13% in taxed municipalities with respect to control municipalities four years after the baseline level. The effect is higher in more populated municipalities. Results are robust across specifications, sample and econometric strategy. They suggest that most of the vacant apartments move to primary residences even though there is some evidence suggesting a strategic behavior of moving vacancy apartments into secondary residences.

In terms of policy implications, these results indicate that a municipal tax on vacancy can indeed influence the behavior of owners of vacant apartments. While it might not be the best instrument to collect public revenues, it does play a role in shaping the incentives in the housing market.

Our results contradict the ones from a previous evaluation that found no effect of the tax. Such evaluation focused on the first reform of the tax in 2006 while we have concentrated in the first implementation in 1999. What distinguish our analysis is the use of the FILOCOM dataset, which adds explicative power due to the higher frequency, exhaustivity and level of detail of the information. The availability of this data together with the special circumstances of the implementation allow us to estimate the impact of the tax with a simple DID strategy.

We contribute to the literature on the role of fiscal policies in the housing market by providing evidence of the direct effect of taxing vacancy. While we have managed to shed light on our main starting question, many new interesting questions have arisen during the course of the evaluation whose answers would provide a better understanding of the effect. There is, hence, still a need for further research to continue investigating about the implications of housing policies. To name a few issues, future investigation could help determine the mechanisms behind the behavioral reaction of the owners, understand the heterogeneous effects of the tax, such as which kinds of apartments that are more sensible to the tax or provide insights on the optimal tax rate.

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Appendices

Table A9: Testing the effect of Loi SRU

	OLS			
	(1)	(2)	(3)	(4)
Post*TLV	-0.739***	-0.762***	-0.786***	-0.652***
	(0.125)	(0.125)	(0.132)	(0.150)
Post*TLV*SRU	-0.073	-0.067	-0.064	-0.123
	(0.271)	(0.279)	(0.289)	(0.331)
Post*TLV*LargeCity*SRU				0.535
				(0.366)
Housing Controls		X	X	X
Demographic Controls		X	X	X
Geographic Controls		X	X	X
UU Fixed Effects			X	X
N	1942	1942	1942	1942

Notes: Significance is indicated by * p<0.1, ** p<0.05, *** p<0.01. Standard errors in parenthesis, they are block bootstrapped at the urban unit level (29 clusters). Each column represents a different regression with vacancy rate as the dependent variable. Variable Post equal 1 for year 2001 and 0 for 1997. Housing controls include rental value from 1997, the average surface, the average revenue and the proportion of social housing. Demographic controls include population in 1999, population growth 90-99 and density in 1999. Geographic controls are the corresponding latitude and longitude. Regressions with urban unit fixed effects have 29 groups. Data comes from FILOCOM dataset for years 1997 and 2001 plus INSEE datasets on demographic characteristics of the municipalities for 1999. Treatment group has 300 observations in 6 clusters while Control group has 671 in 23 clusters per year.