Return on Investment for Promoting Homeownership: The Case of the French Interest-Free Loan Policy

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Abstract

We evaluate whether subsidies to first-time homebuyers expand homeownership, distort housing choices, or are capitalized into housing prices. For France's interest-free loan program between 2015 and 2019, we combine temporal and spatial variation in policy parameters with a doubly robust estimator to jointly assess the different causal effects of the policy. We find limited effects on homeownership, while purchase prices increase strongly with the intensity of the subsidy, indicating substantial price capitalization. Policy simulations show that the marginal return on public spending is low and often negative. These results highlight how policy design and market conditions shape the effectiveness of homeownership subsidies.

JEL classification: H81, R21, R38

Keywords: Housing policy ; subsidized mortgage ; unconfoundedness ; generalized additive model ; spatial smoothing.

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1 Introduction

Access to homeownership is becoming increasingly out of reach amid the ongoing affordability crisis. The widening gap between housing prices and household income is the primary obstacle, and the fall in interest rates during the 2010s did little to offset it (Barone et al., 2021). Difficulty entering ownership raises concerns about wealth inequality (Sodini et al., 2023) and may harm social welfare. Although ownership can reduce mobility (Green and Hendershott, 2001) and expose households to capital-loss risk (Cunningham and Reed, 2013), its positive externalities, such as better housing maintenance (Harding, Miceli and Sirmans, 2000), higher school performance (Green and White, 1997; Harkness and Newman, 2003), and greater self-employment opportunities (Harding and Rosenthal, 2017) often dominate, and are shown to generate positive overall social benefits (Coulson and Li, 2013). A decline in ownership therefore reduces these externalities, prompting most developed countries to subsidize first home purchases in the hope of raising welfare and meeting tenants' aspirations.

To foster homeownership development, countries have implemented various policy schemes to ease households' financial burden. These policies have mixed results on homeownership development (Gobillon and Le Blanc, 2008; Hilber and Turner, 2014; Hembre, 2018; Carozzi, Hilber and Yu, 2024). Whereas they aim to primarily affect tenure choice decisions, they may unintentionally distort housing choices and local housing markets. Depending on policy design, potential first-time owners can use the subsidy to enlarge their housing (Hanson, 2012; Benetton et al., 2022), leading to unintended effects of public expenditure. Moreover, as the housing supply is mostly inelastic, especially in urban areas (Accetturo et al., 2021), the subsidy stimulates housing demand, leading to a partial capitalization into housing prices (Martin and Hanson, 2016; Kunovac and Zilic, 2022; Coste, 2024). In return, it reinforces the main barrier to homeownership development, moving backwards from the initial policy objective (Sommer and Sullivan, 2018). Consequently, the effect on the number of additional first-time owners (commonly defined as the *extensive margin*) is not the only dimension to consider when discussing policy efficiency. It also depends on the distortion of housing choices (commonly defined as the *intensive margin*) and the capitalization of the subsidy into housing prices. These potentially offsetting effects raise a natural question: under what conditions do subsidies to first-time buyers actually achieve their stated objectives, and when do they instead generate inefficiencies?

In this paper, we refer to inefficiency in a policy-relevant sense. From the perspective of a policymaker, a subsidy aimed at promoting homeownership is efficient if public spending indeed translates into additional first-time owners, thereby generating the positive externalities associated with ownership, rather than being absorbed by inframarginal households or capitalized into housing prices. Conversely, the policy is inefficient if a substantial share of public resources is captured by sellers through higher prices, or if it primarily distorts housing choices without affecting tenure decisions. In that case, the subsidy fails both allocatively, by not increasing the number of homeowners, and redistributively, by transferring resources from taxpayers to incumbent owners.

This perspective motivates our use of the return-on-investment (ROI) framework. Rather than evaluating each outcome in isolation, the ROI aggregates the policy's effects along these three dimensions: the extensive margin (new homeowners), the intensive margin (housing choices), and price capitalization. In line with the policy's stated objectives, we treat increases in the number of first-time homeowners as the primary benefit of public spending. Changes in housing choices are valued only insofar as they contribute to this objective, while capitalization into prices is treated as a cost, as it reflects a transfer to sellers and raises barriers to entry for future buyers. Through the specification of monetary values for the three dimensions, the ROI therefore provides a transparent metric to assess whether additional public spending delivers benefits that outweigh its budget cost.

Two mechanisms are then central. First, when housing supply is inelastic, subsidies may primarily relax economic constraints for inframarginal households rather than enabling marginal renters to enter homeownership, leading to larger or higher-quality purchases and stronger bidding for the given housing stock. Second, policy design matters: increasing the coverage share (the proportion of the main mortgage to be free of interest cost) raises the subsidy proportionally for all recipients, fostering broad-based demand pressure and expected price capitalization, whereas increasing the ceiling value (the maximum amount to be subsidized) primarily benefits transactions at the upper end of the price distribution, potentially encouraging reallocation toward subsidized segments and amplifying opportunistic behavior without necessarily affecting tenure decisions. These two mechanisms imply that the policy's efficiency and its ROI depend critically on how subsidy intensity is adjusted.

The French Interest-Free Loan (IFL) policy provides a particularly well-suited setting to evaluate these mechanisms. Unlike the well-studied Mortgage Interest Deduction in the US, it provides sharp spatial variations of the intensity of the subsidy (it can be up to 40% of the mortgage free of interest) that we leverage to estimate policy effects. The subsidy amount also varied over time between 2015 and 2019, offering multiple differences to be leveraged to estimate the policy ROI. We estimate that the policy decreases the accession cost by 7% on average, i.e., an equivalent of a €15,000 cut in the homeownership cost. This subsidy intensity is comparable to policies being implemented in other countries, such as Canada (Coste, 2024), or European countries (Bäckman and Lutz, 2020; Kunovac and Zilic, 2022; Carozzi, Hilber and Yu, 2024). Finally, the subsidy amount depends both on a covering share and a ceiling value that changes during the period under consideration. Operations above the ceiling value will still benefit from the subsidy, although the base will be calculated on the ceiling value, not the operation price. We use this two-policy-parameter approach to disentangle the policy effect of a variation of the covering share or the ceiling value, two parameters of interest for alternative policies.

Our identification strategy builds on two complementary sources of variation in the policy. First, we exploit the 2018 reform, which sharply reduced the coverage share of the IFL in a subset of municipalities while leaving others unaffected. This reform generates plausibly exogenous variation in subsidy intensity that can be credibly exploited in a difference-in-differences framework. The DiD design allows us to identify the causal effect of a discrete change in the coverage share on homeownership outcomes, housing choices, and prices, under the standard parallel-trend assumption. This reform-based approach is internally valid but is inherently partial. In particular, the 2018 reform affects only one policy lever while leaving the ceiling value unchanged, despite the latter being the most frequently binding parameter and the main determinant of public spending per recipient. As a result, the DiD design cannot inform the overall ROI of the policy, nor can it disentangle the relative roles of proportional subsidy intensity and bindingness at the top of the price distribution. To overcome this limitation, we complement the DiD analysis with a selection-on-observables approach that exploits the full spatio-temporal variation of policy parameters induced by the treatment assignment. This second strategy leads to similar results to the DiD for the 2018 change, while it allows us to generalize beyond a single reform episode, identify the effects of both policy levers, credit market conditions, and compute policy-relevant ROI measures.

To compute the ROI empirically, we proceed as follows. First, we estimate the probability of receiving each treatment level for each French municipality. We exploit a rich set of pre-treatment covariates and a smooth bivariate function of geographical coordinates, using a logistic regression that respects the ordered nature of treatment intensity and reflects the latent character of market tightness that shapes policy design. Next, we use the Generalized Propensity Score (hereafter GPS, Imbens, 2000) as weighted least squares in regressions with different outcomes: new homeowners for the extensive margin, recipients' housing choices for the intensive margin, and average housing prices capitalization effects. An important distinction is that we are able to compute these three outcomes not only at the level of the municipality as a whole, but also specifically for policy beneficiaries within each municipality, thanks to access to individual-level data on recipients. Our causal interpretation rests on the assumption that, conditional on observed covariates and location, treatment variation is exogenous to municipal characteristics. Hence, we regress in a last stage the reduced-form effects on the policy levers (covering share and ceiling value), and on mortgage conditions (interest rate and loan maturity) to disentangle the individual contribution of each policy variable to homeownership support. For the extensive and intensive margins, we also account for the indirect effects arising from price capitalization. Finally, we simulate the ROI of additional public spending, gauging the marginal benefit of each extra euro relative to policymakers' objectives.

The validity of our results relies on the specification of the treatment assignment. Our approach is fourfold to ensure a well-specified GPS. First, we collect a large set of pre-treatment variables expected to relate to the tightness of housing markets, covering both demand-side factors (e.g. population density, income, socio-economic status) and supply-side factors (e.g. housing construction, past housing prices, past neighbourhood prices). Second, we address spatial confounding by adding smoothing functions of municipal coordinates in a semiparametric Generalized Additive Model (hereafter GAM, Wood, 2017). Geographical constraints (Saiz, 2010), housing-supply elasticity (Accetturo et al., 2021), or demand for amenities (Bayer et al., 2016) may vary smoothly over space and are likely to shape the treatment assignment. The main residual threat to GPS specification is omitted variables that change at very fine scales—such as school quality or road-traffic pollution—and correlate with treatment intensity. Third, we use a doubly robust estimator to mitigate these possible GPS misspecifications, including omitted-variable bias: the variables from the first step are also included in the outcome regression. Consistency of the policy-relevant treatment effects is ensured if either the first or the second step is correctly specified (Robins and Rotnitzky, 1995; Słoczyński and Wooldridge, 2018). Throughout, the DiD design based on the 2018 reform serves as a quasi-experimental benchmark against which our main findings are compared. Nevertheless, our strategy is more general: it disentangles policy effects by their source of variation (ceiling value, covering share, mortgage conditions).

Despite the large volume of data used and the rich temporal and spatial variation in the policy over the period considered, we are unable to examine the full range of effects of the IFL policy because of data limitations. First, we cannot assess interactions with the rental market. Although it is well established that policies supporting homeownership are likely to affect rental markets, recent general-equilibrium analyses (Rotberg and Steinberg, 2024) further stress spillovers across tenure markets that complicate welfare assessment of policies promoting homeownership. The available data in France do not allow us to investigate these spillovers.[1] Second, we are unable to distinguish between effects on new construction and on the existing (second-hand) housing market that can have differential capitalization rates (Chareyron, Ly and Trouvé-Sargison, 2025), depending on market tightness or credit market (Hilber and Turner, 2014; Bäckman and Lutz, 2020). The price data we rely on includes both newly built and existing dwellings. Yet, these segments are not fully segmented and do not necessarily follow the same price dynamics. Unfortunately,

[1] With respect to rental markets, unfortunately, no database at the municipal level for the period 2015–2019 is available for France. The network of *Observatoires Locaux des Loyers* (Local rental Observatories) gathers lease data in 37 major metropolitan areas but does not provide a national overview of the rental market. Furthermore, a database of rental indicators by municipality constructed using platform data is only available from 2018 onward and at a biennial frequency.

the inability to differentiate new from existing properties in our dataset prevents us from addressing this issue directly.

Our contribution to the literature is threefold. First, we estimate the efficiency of public spending on homeownership while accounting jointly for extensive- and intensive-margin effects. Although earlier studies evaluate each margin separately (Hanson, 2012; Hilber and Turner, 2014; Hembre, 2018), none measure both together in line with policymakers' objectives through an explicit ROI metric. We also incorporate an indirect effect to capture the negative impacts of price capitalization on tenure decisions and housing choices, and we show that expanding the IFL budget is, at best, inefficient in the sense that unintended effects tend to outweigh the targeted ones in plausible monetary values for extensive and intensive margins, and the cost of price capitalization. However, we cannot rule out that subsidizing transition to homeownership is positive for renters – especially the poorest ones, leading to a lower bound for the ROI estimation. Second, we disentangle the impact of two policy parameters (the covering share and the ceiling value) on extensive and intensive margins. Whereas the subsidy bindingness chiefly amplifies opportunistic effects on housing choices (shifting demand from non-subsidized to subsidized segments), the proportional channel is mainly responsible for capitalization, probably triggered by demand shock. Third, we alleviate standard external validity concerns using a selection-on-observables approach. Our results not only align with evidence from our benchmark DiD quasi-experiment but also extend its applicability, making the policy implications more relevant for other policy designs (e.g., in other countries).

The remainder of the paper is structured as follows. Section 2 describes the institutional context of the French IFL policy and the datasets we compile. In Section 3, we introduce the ROI function that incorporates both extensive- and intensive- margins effects. Section 4 sets out the identification strategy to tackle endogenous treatment intensity and details the doubly robust estimation procedure. Section 5 reports the empirical results obtained from the two-step approach, and Section 6 concludes.

2 Institutional Context and Data

2.1 The French IFL Policy

The Interest-Free Loan (IFL) policy was introduced in France in 1995 to foster first-time homeownership. Eligible purchasers[2] receive an interest-free top-up loan that covers a predetermined share of their total mortgage (the *covering share*) up to a maximum amount (the *ceiling value*). The budget cost corresponds to the foregone interest, which

[2] The French housing policy defined potential first-time owners as individuals who rent their main residence for at least two years. This eligibility condition remained stable since the policy introduction.

the government reimburses to lenders through tax deductions. Since inception, roughly 3.2 million loans have been issued at an aggregate cost of about 26.1 billion—approximately. Since 2015, the average cost per recipient amounts to €15,000, representing on average 7% of the total accession cost. Calculations for a 20-year amortisation duration, we estimate that it lowers the monthly payment by €70. The IFL policy excludes a small share of high-income tenants (around the top decile; Sotura, 2020) and, until 2016, was restricted to newly built dwellings (existing units became eligible only if substantial renovation was planned). The number of IFL in the existing segment is marginal according to the size of the housing market. We estimate at the country level an average of 27,000 contracts in the existing segment, while we observe an average of 900,000 transactions.

Every IFL must be paired with a standard, interest-bearing mortgage, so borrowers still have to satisfy conventional credit-worthiness criteria.[3] The total loan, therefore, equals the sum of the commercial mortgage and the IFL tranche. Policymakers adjust the key parameters annually, but borrowers cannot refinance into a new IFL if subsequent terms become more or less generous; they remain bound by the conditions prevailing at the date of purchase. Likewise, if they sell the property, they forfeit the subsidy and must repay the outstanding IFL principal. However, the repayment process does not account for inflation and housing appreciation.

The determination of the intensity of the IFL subsidy has been broadly stable since its introduction, according to two varying parameters about policy design and two parameters about credit market conditions. For every period and zoning tier (see Section 2.2), policymakers fix (i) a covering share s, between 10% and 40%, representing the maximum fraction of the overall mortgage that may be interest-free, and (ii) a ceiling V, between €100,000 and €150,000 for a single borrower. These are the two parameters at the government's disposal to implement the IFL policy, considering that the income cap for eligibility is not a binding condition. These two variables are not expected to have the same impact on the subsidy, with a potential differentiated effect on extensive and intensive margins. On the one hand, increasing the covering share s increases the subsidy for all policy recipients, regardless of the amount they borrow (left panel, Fig. 1). On the other hand, increasing the ceiling value $\overline{V}$ raises the subsidy for the most expensive housing units, while leaving the least expensive ones unaffected (right panel, Fig. 1).[4]

[3] For the French market, a common condition is that reimbursement payments cannot be higher than one-third of income. Consequently, this condition also restricts access to the IFL policy. Note that this condition applies to the total borrowed amount, not only the conventional loan.

[4] More precisely, it affects home purchases that are censored, i.e. home purchases with purchase price higher than the previous ceiling value (from a reform perspective).

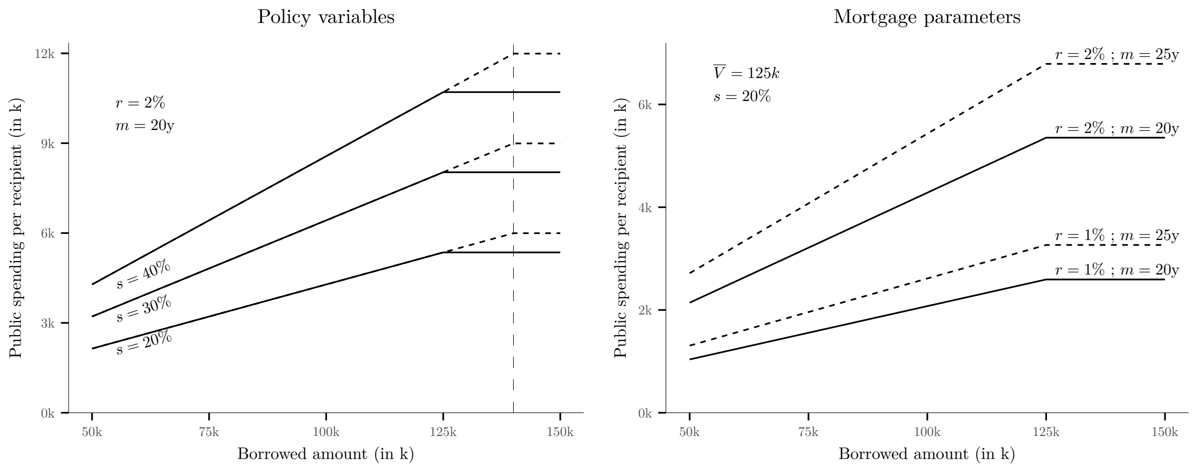

Figure 1. Cost of homeownership according to main policy variables

Notes: We simulate the cost of homeownership per recipient from the policymaker's perspective according to the four policy variables. The left panel represents cost variation according to parameters controlled by policymakers (covering share s and ceiling value $\overline{V}$), while the right panel considers variations in mortgage conditions (interest rates r and maturity m). The x-axis corresponds to the overall amount being borrowed by households, whereas the y-axis is the cost for the administration per recipient (and hence, the gain for purchasers). The overall budget cost also depends on the number of recipients.

Section A demonstrates that the budget cost of an IFL contract is:

$$c = \left[\frac{m \times r}{1 - (1 + r)^{-m}} - 1 \right] \times s \times \min(\widetilde{V}, \overline{V}). \tag{1}$$

Equation 1 reports the effects of the two credit market conditions: r is the market interest rate and m the loan maturity (in months). Moreover, the cost (and therefore the subsidy for recipients) is bound by the right term. If the housing price of the subsidized operations ($\tilde{V}$) is above the ceiling value ($\overline{V}$), the ceiling value is used as the main base to estimate the subsidy. The budget cost of an IFL contract weakly increases with the four varying parameters, which indicates that increasing one of them is equivalent to increasing the financial support for recipients. This allows us to define the treatment intensity of the policy as an increase in one of the four considered parameters or an increase in the budget cost for the government, although we focus more specifically on policy-controlled variables to be aligned with the ROI approach. Interest rate and maturity variations come from macroprudential decisions with regional singularities.

We restrict our study period to the last three IFL reforms of the 2015–2019 period, as eligibility conditions remain unaffected and the ABC classification of municipalities (i.e., the zoning for the spatial distribution of the two parameters under policy control) does not change. We decompose our study period into three distinct periods (2015, 2016-2017, 2018-2019) to account for changes in treatment intensity between years for zones, although the ABC zoning does not change. We consider operations as bounded when either the operation price is above the ceiling value or if the covering share equals the maximum set by the policy. Remark that the share of operations jointly bound by both parameters is marginal and correspond to operations equalling perfectly the ceiling value. Empirically, for our study period, Fig. 2 shows that the ceiling value is the parameter that binds the subsidy the most. While 69.7% of IFL contracts between 2015 and 2019 concerned operations with prices above the ceiling value, the covering share is binding for 15.2%. The bindingness of the ceiling value increased between 2015 and 2019, fuelled by the underlying trend of housing price increases. On the contrary, the bindingness of the covering share has decreased. Hence, the ceiling value is the main binding parameter for most IFL contracts.

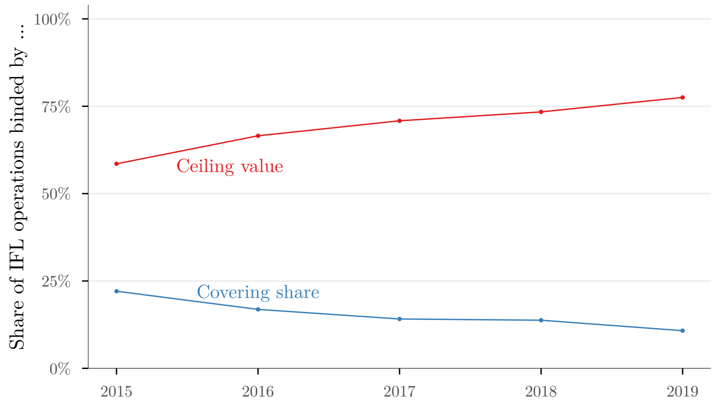

Figure 2. Binding parameter of the IFL subsidy over time

Notes: We report the share of IFL contracts being bound by either the ceiling value (red) or the covering share (blue). We consider an operation as bounded when either the operation price is above the ceiling value or the covering share equals the maximum imposed by the policy setting.
Sources: Authors' Calculation based on SGFGAS database.

2.2 The ABC Zoning

The two policy variables—covering share and ceiling value—are determined by the location of IFL contracts, according to an exhaustive and mutually exclusive classification of the 34,970 French municipalities that is designed to reflect housing-market tightness.[5] Consequently, treatment intensity is set by the ABC zoning. The practical implementation of the tightness measure comprises four tiers, from C, the lowest level, to A, the highest level, with B_2 and B_1 as intermediate levels.[6] This official zoning was updated four times since its introduction in 2003, the latest update of October 2014 being stable for the 2015–2019 period under study. Most French municipalities are rural and belong to zone C, the lowest level of the zoning. Panel A of Table 1 shows that the distribution of the ABC classification is consistent with general expectations, as municipalities with higher

[5]According to official documents, tightness is defined as "the imbalance between the housing supply and the housing demand" (French Ministry of Ecological Transition).

[6]The spatial distribution of municipalities is displayed in Fig. B.1 in the Online Appendix (OA).

population densities and higher housing prices per living area (unitary prices hereafter) are higher in the housing tightness hierarchy.

Despite strong correlations between ABC and observable pre-treatment variables, the practical implementation of the tightness measure is not mechanistically related to a subset of variables (Cour des Comptes, 2012). The zoning does not rely exclusively on objective criteria, suggesting potential subjectivity in the assignment, such as political discussions or variability in criteria. Hence, municipalities with similar latent tightness can be placed in different tiers. Many quasi-experimental studies addressing the endogeneity of French housing policies exploit the arbitrariness of this zoning to identify causal effects (Labonne and Welter-Nicol, 2015; Beaubrun-Diant and Maury, 2021). We also follow such a quasi-experimental design; additionally, we develop an identification strategy that leverages zoning discrepancies within a selection-on-observables framework to estimate policy impacts.

2.3 Data

We aggregate three exhaustive individual data sources at the municipal level ($N = 34{,}970$) and match them with demographic variables.[7] We remove municipalities located on the island of Corsica because our identification strategy requires spatial contiguity (360 observations) and those in the *Alsace-Moselle* region (1,605 observations), as housing price data are missing for administrative reasons. After discarding further observations with missing values or inconsistencies,[8] we retain a final sample of 26,819 municipalities. These municipalities lie mainly in zone C and differ significantly from B_2 municipalities (Table B.1 in OA), so any resulting selection bias should be negligible: their probability weight in the estimations is small.[9]

IFL data. Our first exhaustive dataset, provided by the SGFGAS, covers every household subsidized under the IFL scheme. Each record is geo-located at the municipality of the newly purchased dwelling and details the loan contract (total amount of the main and subsidized tranches, total subsidy, interest rate, and maturity). The file also contains household information—annual income, household size, marital status, previous place of

[7] The data sources to which we have access are either at the recipient level (IFL database), the housing transaction level (the transaction database), or the housing unit for first-time owners (the fiscal database). However, we cannot match these data at the lowest disaggregation level due to the lack of common identifiers and differences in spatial granularity.

[8] Most missing values come from the median income price (3,576 observations), which is unavailable for secrecy reasons.

[9] Considering a selection model within municipalities C with a binary variable as the dependent one that indicates whether variables are filled, we find that municipalities with missing variables on median income have lower density and lower housing prices. Thus, they are likely to have a low weight in bilateral estimations as they have a low probability of belonging to another group.

residence—and dwelling characteristics such as year of construction, floor area, purchase price, and purchase date. We aggregate these variables to the municipal level, computing each year the number of IFL contracts as well as average loan, housing, and household attributes.

Tax data. Because the IFL database records only subsidized buyers, we rely on exhaustive property-tax files (*Fichiers Fonciers*) to compute the total number of new homeowners, irrespective of subsidy status. This measure is crucial for extensive-margin analysis: a rise in IFL recipients with no change in total first-time owners would indicate pure opportunistic behaviour. Exploiting the panel dimension of the tax files, we tag first-time owners according to the IFL definition—households absent from the file during the preceding two years. We then count, for each municipality-year, the number of such owners. As expected, this figure always exceeds the number of IFL contracts (except in two municipalities that we dropped), confirming that some first-time buyers do not receive the subsidy. Finally, we recover the total number of newly-built housing over the 2010–2013 period based on the construction year reported in the tax files.

Transaction data. A third exhaustive dataset, DV3F, records every residential transaction. From it, we compute, for each municipality, the average unitary price over the pretreatment years 2010–2013. To attenuate border effects when measuring market tightness, we also calculate the corresponding average for contiguous neighbouring municipalities. DV3F further delivers post-treatment outcome variables—average transaction price, floor area, and unitary price—for each of the three policy periods. Finally, we derive the stock of newly built dwellings from the construction year reported in the tax files for the years 2010–2013.

Socio-demographic data. For each municipality under consideration, the *French National Institute of Statistics and Economic Studies* (INSEE) (year 2013) provides data before the ABC reform in 2014 on population density, median income, and socio-professional categories (see Table B.2 in OA).

Sample. We retain all French municipalities after the exclusions mentioned above and classify each municipality into its ABC tier. Regardless of the location or the period, at least 80% of the IFL contracts reach the maximum IFL amount, either bound by the ceiling value or the covering share. Table 1 summarises IFL intensity (panel B), mortgage conditions (panel C), and key pre-treatment covariates (panel A).

Table 1. Main variables for municipalities along the ABC zoning

Variable	Period	Country	*ABC Zoning Areas* A	B_1	B_2	C
			A - Pre-treatment variables			
Number of Municipalities	2013	34.970	0.724	1.535	3.828	28.883
(*thousand of units*)		100%	2.07%	4.39%	10.95%	82.59%
Housing Price	2010–2013	153.1	284.0	234.9	188.6	139.0
(*thousand euros*)		*(68.0)*	*(124.8)*	*(74.8)*	*(59.6)*	*(54.9)*
Unit. Housing Price	2010–2013	1,608.7	3,558.7	2,597.0	2,003.5	1,430.9
(*euros by squared meter*)		*(691.8)*	*(1,054.5)*	*(557.5)*	*(569.3)*	*(502.6)*
Unit. Price of Neighbors	2010–2013	1,561.0	3,654.1	2,618.6	1,975.2	1,371.6
(*euro per squared meter*)		*(724.0)*	*(1,099.1)*	*(587.2)*	*(582.8)*	*(517.1)*
Population Density	2013	1.9	26.4	6.8	3.1	0.7
(*inhabitants by hectare*)		*(8.1)*	*(38.2)*	*(9.4)*	*(4.6)*	*(1.0)*
Median Household Income	2013	20.0	25.3	24.0	22.1	19.2
(*thousand euros by year*)		*(3.4)*	*(6.0)*	*(4.8)*	*(3.7)*	*(2.6)*
Number of New Housings	2010–2013	41.9	364.5	226.8	72.4	15.9
(*number of units*)		*(226.7)*	*(918.7)*	*(648.3)*	*(151.7)*	*(28.7)*
			B - IFL Policy Values			
Maximum Ceiling Value	2015	-	150.0	135.0	110.0	100.0
(*thousand of euros*)	2016–2017	-	150.0	135.0	110.0	100.0
	2018–2019	-	150.0	135.0	110.0	100.0
Maximum Coverage Share	2015	-	26.0	26.0	21.0	18.0
(*percent*)	2016–2017	-	40.0	40.0	40.0	40.0
	2018–2019	-	40.0	40.0	20.0	20.0
Maximum IFL Amount	2015	-	39.0	35.1	23.1	18.0
(*thousand of euros*)	2016–2017	-	60.0	54.0	44.0	40.0
	2018–2019	-	60.0	54.0	22.0	20.0
Average Subsidy	2015	5.21	10.31	9.41	5.81	4.02
(*thousand euros*)	2016–2017	9.63	13.96	12.35	10.45	8.99
	2018–2019	5.03	12.44	10.89	4.64	3.98
			C - Mortgage Market Conditions			
Mortgage Maturity	2015	228	244	254	238	221
(*percent*)	2016–2017	260	268	269	265	258
	2018–2019	258	267	267	262	255
Annual Interest Rate	2015	2.51	2.46	2.45	2.52	2.52
(*number of months*)	2016–2017	1.87	1.77	1.77	1.89	1.88
	2018–2019	1.62	1.53	1.52	1.65	1.63

Notes: French municipalities are classified according to the ABC zoning in columns. Panel A reports the average and standard deviation of pre-treatment variables used to control the endogenous treatment assignment. The first three variables of panel B correspond to the IFL parameters for each period (constant between municipalities), with a Maximum IFL Amount that equals the maximum ceiling value times the Maximum Covering Share. The Average Subsidy is computed from IFL data and Equation 1. Panel C reports the average of loan maturities and interest rates, also extracted from IFL files.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.

3 Empirical Framework

We frame the objectives of the IFL policy from a policymaker's standpoint, distinguishing between the extensive and intensive margins. These objectives are proxied by the marginal value of, respectively, one additional homeowner and one euro more in the subsidized housing price, conditionally on the average housing price (subsidized or not). The intensive margin, therefore, measures the ability of subsidized households to purchase more expensive housing than they would have in the absence of the subsidy. We therefore formalize both objectives within an ROI function that we link to the budget costs of the policy.

3.1 Outcome variables

We evaluate the IFL through a cost–benefit lens that balances two goals: creating new homeowners and improving recipients' housing. On the extensive margin, the key variable is the total number of new homeowners with or without subsidies (denoted N). We also consider the number of IFL recipients $\widetilde{N}$, although it is a biased assessment of the extensive margin, as increasing $\widetilde{N}$ without changing N cannot be considered as favouring first-time homeownership. On the other hand, the intensive margin captures the changes in recipients' housing choices. The recipients could use the subsidy to increase their purchase price (noted $\widetilde{V}$), either by increasing floor area (noted $\widetilde{S}$) or the price per floor area (noted $\widetilde{P}$). Finally, policy-induced demand can also affect the average housing price from capitalization effects V, especially in the context of low supply elasticity. We then consider that the potential capitalization effect, measured by the housing price of subsidized and non-subsidized housing transactions, could affect both extensive and intensive margins.

Beyond these effects, changes in IFL intensity directly affect budget costs. The cost for the administration depends on the number of recipients (denoted $\widetilde{N}$) and the average cost per recipient (denoted $\widetilde{C}$). Hence, the effect of treatment intensity variation is ambiguous, as it depends both on first-time owners' take-up (whether they benefit from the policy) and the treatment intensity per recipient. We assume that policymakers aim to achieve their policy objectives while minimizing the overall cost of the policy.

3.2 Policy objectives

Policymakers are assumed to care about two outcomes: (i) additional homeowners and (ii) better housing for recipients, in balance with the budget cost of the policy. We integrate them in an efficiency index and define the policy ROI, denoted E, as:

$$E = \phi N + \psi \widetilde{V} - \widetilde{N}\widetilde{C} \tag{2}$$

with ϕ and ψ standing for, respectively, the policymaker's valuation of an additional homeowner and the valuation of the change in recipients' housing choice. E represents the monetary valuation of the IFL policy effects, taking into account the total costs defined by the product of the number of recipients ($\widetilde{N}$) and the average cost per recipient ($\widetilde{C}$). We consider the case of an increase of one of the four policy variables a, both from the policy design (covering share and ceiling value), or the mortgage conditions (interest rate and loan maturity). Derivating Equation 2 with respect to a, such as $e_a = \partial E/\partial a$ yields:

$$e_a = \phi \left(\frac{\partial N}{\partial a} + \frac{\partial N}{\partial V} \cdot \frac{\partial V}{\partial a} \right) + \psi \left(\frac{\partial \widetilde{V}}{\partial a} + \frac{\partial \widetilde{V}}{\partial V} \cdot \frac{\partial V}{\partial a} \right) - \left(\widetilde{N} \frac{d\widetilde{C}}{da} + \widetilde{C} \frac{d\widetilde{N}}{da} \right) \tag{3}$$

The ROI function combines direct and indirect effects at both margins, given that extensive and intensive margin outcomes depend on both the intensity of the subsidy and local housing prices, such as $N = N[a, V(a)]$ and $\widetilde{V} = \widetilde{V}[a, V(a)]$. Direct effects stem from how treatment intensity changes the number of homeowners or recipients' housing choices; indirect effects arise when price capitalization, itself affected by a, feeds back into those outcomes. Hence, the ROI for additional subsidy intensity depends at both margins on the direct (treatment response, denoted $\frac{\partial \cdot}{\partial a}$) and the indirect (adaptation to potential price capitalization, denoted $\frac{\partial \cdot}{\partial V} \cdot \frac{\partial V}{\partial a}$) effects. Finally, we assume policy cost only varies through the variation in average cost per recipient and the number of recipients, which justifies this simple derivative-based framework. The sign of e_a is the main criterion to determine whether increasing public spending by the treatment variable a is positive according to the policy objectives.

Two ingredients are needed to operationalize Equation 3. First, we have to specify credible monetary values for ϕ and ψ. It requires knowing the implicit monetary values set by policymakers to reflect their policy objectives. While the literature offers estimates of externalities from ownership (Coulson and Li, 2013), policymakers' relative valuation of intensive versus extensive effects is unclear. We therefore explore alternative scenarios, always assigning a positive value to an extra homeowner—consistent with the policy's stated goal—and varying the valuation of housing-choice distortions from negative through zero to positive. The valuation of modification of recipients' housing choices is less straightforward, leading us to introduce more variation in the credible valuation range.

Second, first-order responses to treatment variable variation for main outcomes such as the number of homeowners, housing market price, or recipients' purchase price are needed. It constitutes our building blocks to estimate the ROI of raising the IFL subsidy through each treatment variable. Moreover, we must estimate a similar quantity for average spending per recipient and the number of recipients to assess the impact on policy cost. To estimate these first-order responses, we leverage differences in treatment intensity following the ABC zoning to obtain dose-response functions presented below.

3.3 Dose-response functions

We recover the marginal effects at both the extensive and intensive margins for the IFL policy from the counterfactual framework (Rubin, 1974), via dose-response functions relating policy-relevant treatment effects to the four policy variables. Variations in IFL policy across the four ABC zones and the three periods define a multivalued treatment taking $G = 12$ levels. Let $g = 1...G$ denote a level of treatment and T_g a dummy variable that indicates whether the municipality receives this level. Then, we have:

$$Y = \sum_{g=1}^{G} T_g Y_g, \tag{4}$$

so that the observed outcome Y equals its potential value Y_g only if a municipality receives treatment g. We study $Y = N$ for the extensive margin, $Y = \widetilde{V}$ traces purchase of more desirable housing at the intensive margin, and $Y = \widetilde{C}$ for budget costs. Each bilateral combination of different treatment levels g and g' modifies at least one policy treatment variable, enabling us to map treatment effects to specific parameter changes. Considering the requirement of first-order derivates to estimate Equation 3, we adopt linear dose-response functions for every outcome Y with:

$$\mathbb{E}(Y_g - Y_{g'}) = \beta_0^Y + \sum\nolimits_a \beta_a^Y (a_g - a_{g'}) + \xi. \tag{5}$$

with mean-independent errors ξ. The Ordinary Least Squares (OLS) coefficients β_a^Y summarise how differenced in the policy levers a explain the heterogeneity of treatment effects $\mathbb{E}(Y_g - Y_{g'})$ across g and g'; in particular, $\beta_a^Y \equiv \partial Y / \partial a$ feeds directly into Equation 3. For outcomes defined on the whole population, $Y \in \{N, V, P, S\}$, we use the average treatment effects (ATEs). They are policy relevant as they appear in the left-hand side of Equation 5 as they represent the change of Y caused by the policy g relatively to g' for the whole population and β_a^Y summarizes how these changes can be attributed to differences between a_g and $a_{g'}$ (the policy variables and mortgage conditions) that we observe. For outcomes affecting only recipients, $Y \in \{\widetilde{N}, \widetilde{V}, \widetilde{P}, \widetilde{S}, \widetilde{C}\}$, the policy-relevant treatment effects concern recipients (ATT).[10] The left-hand side of the dose-response function (Equation 5) is then $\mathbb{E}(Y_g - Y_{g'} \mid T = g)$. As we study bilateral combinations

[10] All our statistical analyses are conducted at the municipal level; however, we estimate ATTs when variables are constructed using only policy recipients, and ATEs when recipients and non-recipients are included.

within three distinct periods, this gives $4 \times (4-1) \times 3 = 36$ policy-relevant treatment effects. Therefore, each set of dose-response functions is estimated based on 36 observations for each of the nine outcomes. These estimates deliver the marginal effects required at both margins and, ultimately, the ROI of an additional euro of public spending. To obtain the left-hand side of the Equation 5, we first compute counter-factual outcome differences, e.g., switching treatment intensity from B_1, and then regress those differences on the associated changes in policy variables. To operationalize the estimations of the dose-response functions, we must therefore estimate the causal statistics to be used as the outcome variable, that is, the left-hand side of Equation 5.

4 Identification strategy

We combine a generalized propensity score (GPS) design with a difference-in-differences (DiD) benchmark. Our main results come from the GPS, which exploits the continuous variation in treatment intensity generated by the ABC zoning system to estimate dose–response and ROI effects. The 2018 IFL reform is used in a DiD framework as a quasi-experimental benchmark. Because DiD relies on weaker identifying assumptions, its role is to show that our results are not driven by the additional restrictions imposed by the GPS. The consistency of the results obtained from the two approaches supports the credibility of the other results based on the GPS estimates.

4.1 DiD Strategy

We first exploit the 2018 reform of the IFL policy, which sharply reduced the coverage share in a subset of municipalities (namely the municipalities belonging to the B_2 and C tiers) while leaving others unaffected (namely the municipalities belonging to the A and B_1 tiers). This reform generates plausibly exogenous variation in the intensity of the subsidy across space and time, which we exploit in a canonical DiD strategy based on parallel trend. Because the reform was centrally decided and unrelated to contemporaneous local shocks, it provides a transparent and credible source of internal validity. The functional form is the following:

$$Y_{it} = \alpha + \beta \cdot \text{Treated}_i \times \text{Post}_t + \gamma_i + \lambda_t + \epsilon_{it} \tag{6}$$

with Y_{it} outcome for municipality i at time t, β the parameter of interest being the policy effect, γ_i municipality fixed effects, λ_t time fixed effects and ε_{it} the error term.

The reform-based DiD approach is informative but addresses only a narrow dimension of the policy, as the 2018 reform decreases the covering share from 40% to 20%, but leaves the ceiling values unchanged. Consequently, although it provides a quasi-natural experiment to estimate how the covering share affects policy objectives, it cannot address a variation for the ceiling value, which is the main binding parameter. Moreover, a single discrete reform cannot capture the continuous variation in treatment intensity that is central to evaluating the policy's return on investment. For this reason, we complement the DiD analysis with a second identification strategy that exploits the full spatio-temporal variation in policy parameters induced by the ABC zoning system. To increase the comparability between groups on the dynamic dimension, we restrict our sample to municipalities with a municipality being classified in an alternative tier in the ABC classification in their direct proximity. Although it harms the external validity, it provides a more credible estimation using the DiD approach.

4.2 ROI Estimation: Identifying Assumptions

The challenge raised by the GPS strategy is that treatment intensity is endogenous by construction: more generous subsidies are targeted toward tighter housing markets. To address this issue, we explicitly model the zoning assignment process and implement a generalized propensity score with spatial smoothing, combined with a doubly robust estimator. This approach allows us to recover policy-relevant treatment effects across the full distribution of subsidy intensity. We maintain two assumptions to recover causal treatment effects. The first is that, conditionally on pre-treatment variables, the g treatments are weakly unconfounded.

Assumption 1 *Weak Unconfoundedness.*

$$\forall(g, \mathbf{X})\, Y_g \perp T \,|\, \mathbf{X}$$

According to this assumption, the set of pre-treatment variables $\mathbf{X}$ ensures conditional randomization of the IFL policy between municipalities. This selection-on-observables restriction considers that pre-treatment variables control all the structural differences between municipalities and that the differences between the conditional outcomes can be attributed to policy changes. Because g combines spatial and time variations, the restriction applies both across ABC zones and across policy periods.

The well-known property of dimension reduction of well-specified propensity scores (Hahn, 1998) allows us to parsimoniously model the conditional expectation of the outcomes, as long as we have $Y_g \perp T \,|\, p_g(\mathbf{X})$ with $p_g(\mathbf{X}) \equiv \mathbb{P}(T = g \mid \mathbf{X})$ from Assumption 1. This is the definition of the GPS (Imbens, 2000) as the probability of receiving a level of treatment, knowing the pre-treatment variables. As Crump et al. (2009) show, the propensity to receive treatment should not be too close to zero or one to ensure precise and robust estimates. This leads to the following overlap assumption:

Assumption 2 *Overlap*

$$\forall(g, \mathbf{X}),\, p_g(\mathbf{X}) > 0$$

Under the two previous assumptions, Słoczyński and Wooldridge (2018, Lemma 3.2) show that counterfactual treatment effects can be identified from the probability weighting. The average outcome $Y_{g'}$ for a counterfactual treatment level g', respectively for the whole population and for municipalities that receive the treatment level g, are respectively:

$$\mathbb{E}(Y_{g'}) = \mathbb{E}\left[\frac{T_{g'}}{p_{g'}(\mathbf{X})}Y\right] \quad \text{and} \quad \mathbb{E}(Y_{g'}\,|\,T = g) = \frac{1}{\mathbb{P}(T = g)} \cdot \mathbb{E}\left[\frac{p_g(\mathbf{X})}{p_{g'}(\mathbf{X})}T_{g'}\,Y\right]. \tag{7}$$

These statistics concern, respectively, the full population of homeowners impacted by the externalities at both margins and the recipients targeted by the policy support. They are the building blocks of the policy-relevant treatment effects under consideration, as the ATE of g instead of g' on the outcome Y is $\mathbb{E}(Y_g - Y_{g'})$ and the related ATT is $\mathbb{E}(Y_g - Y_{g'} \mid T = g)$. These counterfactual statistics are used to build policy-relevant treatment effects as they are related to different populations.

4.3 Specification of the Propensity Score

The propensity score allows us to estimate $p_g(\mathbf{X})$, and, hence, to recover treatment effects from Equation 7. Consistent with the notion of housing market tightness, we posit an unobserved latent variable η_i^* whose position relative to unknown thresholds classifies municipalities. The propensity that a municipality i sits high in this hierarchy depends on the J pre-treatment variables x_{ji} that proxy the political decision, a bivariate smooth function of the geographical coordinates of its centroid $\mathbf{z}_i$ (longitude and latitude, Gilbert et al., 2024), and a random term ε_i capturing the arbitrary part of the zoning explained in the Section 2.2. Assuming ε_i follows a logistic distribution yields an ordered logit model. The latent variable describing the tightness of the housing market η_i^* is then:

$$\eta_i^* = \alpha + \sum_{j=1}^{J} f_j(x_{ji}) + h(\mathbf{z}_i) + \varepsilon_i. \tag{8}$$

The J univariate functions f_j are specified as additive spline transformations of pre-treatment variables within a generalized additive model framework (GAM, Wood, 2017). The spline coefficients are shrunk endogenously by penalized iterated weighted least squares, while the smoothing parameters are estimated using a separate criterion from the restricted maximum likelihood (Wood, Pya and Säfken, 2016). The same estimation procedure is used simultaneously for the bivariate smooth function h of coordinates. The main difference is the *a priori* specification of the spline, which is a bivariate thin plate. Whereas geographic regression discontinuity designs restrict attention to border municipalities to approximate quasi-random assignment, our approach explicitly models the full spatial dimension of treatment assignment and retrieves its stochastic component to exploit the entire sample of municipalities.

Let Λ denote the logistic cumulative distribution function and $\mu_0 < \mu_1 < \cdots < \mu_5$ the unknown ordered thresholds associated with the four ABC tiers. The resulting GPS for treatment level g is as follows, with $\eta_i \equiv \eta_i^* - \varepsilon_i$ the deterministic parts of the latent variable:

$$p_g(\eta_i) = \Lambda(\mu_g - \eta_i) - \Lambda(\mu_{g-1} - \eta_i). \tag{9}$$

Because tier A municipalities are, by definition, tighter than those in B_1, B_2 and C, η_i lies between μ_4 and μ_5. As the ABC zoning was unchanged over the 2015–2019 period, the probability of belonging to a given tier is time-invariant. Thanks to this ordered structure, conditioning on η_i alone is sufficient to reach weak unconfoundedness when the GPS is correctly specified, obviating the need to control for the full vector X. Remaining threats to specification stem from very local omitted factors that vary at sub-municipal scales and correlate with treatment intensity; we judge these risks to be minor. Nevertheless, to guard against potential GPS misspecification, we employ a doubly robust strategy: outcome equations include the same covariates, ensuring consistency if either the first-stage GPS or the second-stage outcome model is correctly specified (Robins and Rotnitzky, 1995; Słoczyński and Wooldridge, 2018).

4.4 Specification of the Outcomes

The outcomes are specified within the same semi-parametric GAM framework as the GPS. All pre-treatment variables used in the first stage re-enter the outcome specification as controls, thereby delivering a doubly robust estimator that reinforces credibility. The only distinction is that each outcome Y is modelled separately for each subsample defined from the treatments g received by the municipality. Consequently, the smooth functions f_j and h are now indexed by the outcome y and the treatment g:

$$y_{gi} = \alpha_g^y + \sum_{j=1}^{J} f_{gj}^y(x_{ji}) + h_g^y(\mathbf{z}_i) + \varepsilon_{gi}^y. \tag{10}$$

The same pre-treatment variables and geographical coordinates are retained, but their smoothing parameters are free to vary across outcomes and treatments. With nine outcomes, four treatment levels, and three periods, Equation 8 entails 108 GAM estimations to obtain the full set of functions f^y_{gj} and h^y_g for a given GPS. The set of y_{gj} allows us to estimate counterfactual outcomes if a set of municipalities had received another treatment level, which would be needed as the dependent variable to compute Equation 5. Following Słoczyński and Wooldridge (2018), the doubly robustness property requires weighting each regression by the GPS ratios from Equation 7. To obtain the average counter-factual outcome for the treatment g' for the municipalities receiving g, each municipality is weighted by $\widehat{p}_g(\eta_i)/\widehat{p}_{g'}(\eta_i)$ predicted from the first stage. As is standard, these weights were normalised with each treatment subsample.

We conclude by setting out the formulas used to assess the ROI of the IFL. Counter-factual components of Equation 7 come from regressing Y on the subset of municipalities with treatment g' using weights $1/p_{g'}(\eta_i)$ and $p_g(\eta_i)/p_{g'}(\eta_i)$ respectively. Under assumptions 1 and 2, and writing $\mu_g \equiv \mathbb{P}(T = g)$ for the share of municipalities at level g, averaging the fitted values provides a consistent estimation as:

$$\mathbb{E}(Y_{g'}) = N^{-1} \times \sum_{\ell=1}^{N} \hat{y}_\ell(g') \quad \text{and} \quad \mathbb{E}(Y_{g'} \,|\, T = g) = \mu_g^{-1} \times \sum_{\ell=1}^{N} T_{g\ell} \times \hat{y}_\ell(g') \tag{11}$$

where $\hat{y}_\ell(g') \equiv \hat{\alpha}^Y_{g'} + \sum_{j=1}^{J} \hat{f}^Y_{g'j}(x_{j\ell}) + \hat{h}^Y_{g'}(z_\ell)$ comes from the estimation of the outcome Y for the subset of municipalities that receive treatment g'. The predicted outcome values for the entire population of municipalities with $\ell = 1, \ldots, N$, will be used in Equation 5 as the ATE or ATT dependent variables of Dose-Response functions.

5 Results

We first present our DiD estimations as a benchmark for our identification restrictions. Then, we present our dose-response functions using the selection-on-observables approach. We conclude the section by reporting the resulting ROI measures.

5.1 DiD Results

The results from the DiD estimation provide an overview of the effects of the 2018 policy reform on the extensive margin, intensive margin, and price capitalization. Table 2 reports the estimation of Equation 6 with standard errors clustered at the municipal level.

The 20 percent points (pp) cut in the covering share in some municipalities in 2018 is shown to have a non-significant effect on their total number of first-time owners (column 1). On the contrary, it significantly affects both the number of recipients (column 5). The response is close to a 1:1 relationship, highlighting that the subsidy intensity has

Table 2. DiD estimation for policy effects following the 2018 reform for border municipalities

	Population of interest:								
	Full population				Recipients				
	N	V	S	P	$\widetilde{N}$	$\widetilde{V}$	$\widetilde{S}$	$\widetilde{P}$	$\widetilde{C}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Time	-0.087***	0.069***	0.011***	0.076***	-0.262***	0.038***	-0.019***	0.057***	-0.133***
	(0.011)	(0.016)	(0.003)	(0.018)	(0.026)	(0.005)	(0.007)	(0.005)	(0.008)
Time × Group	0.017	-0.018	-0.004	-0.029	-0.173***	-0.004	0.013	-0.017**	-0.704***
	(0.013)	(0.017)	(0.003)	(0.020)	(0.030)	(0.006)	(0.008)	(0.007)	(0.009)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,596	8,596	8,596	8,596	8,596	8,596	8,596	8,596	8,596
R^2	0.967	0.787	0.897	0.791	0.878	0.822	0.713	0.858	0.920
Within R^2	0.031	0.019	0.005	0.013	0.172	0.036	0.002	0.043	0.861

*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$
Reading:
N accounts for the number of new homeowners; $\tilde{N}$ accounts for the number of IFL beneficiaries
V accounts for the average housing value; $\tilde{V}$ accounts for the average subsidized housing value
S accounts for the average housing surface; $\tilde{S}$ accounts for the average subsidized housing surface
P accounts for the average housing price per m^2; $\tilde{P}$ accounts for the average subsidized housing price per m^2
$\tilde{C}$ accounts for the average cost per recipient
Notes: We report the OLS coefficients from a Difference-in-Difference estimation. We leverage the 2016 reform, lowering the covering share for some municipalities by 20 pp. The observation unit is a municipality per period of interest. 'Times' is a dummy variable equal to 1 if the period under consideration is after 2016, 0 otherwise. We estimate that following the 20 pp cut, the number of recipients has decreased by 17.3% compared to non-treated municipalities. All outcome variables are transformed through a log transformation. We restrict our sample to border municipalities to increase the credibility of the approach. As we introduce municipality fixed effects, we do not add group fixed effects. We report clustered standard errors at the urban area level in parentheses.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.

a strong effect on the subsidy take-up, as we expected. Considering both the effect on the number of recipients and the absence of effect on the number of first-time owners, we interpret that a covering share variation generates opportunistic behaviour from the subsidized (i.e., newly built sectors) to the non-subsidized (i.e., the existing market). Yet, due to data limitations, we cannot estimate whether this behavior arises from households' preferences or from the reduction of housing supply. Moreover, the reduction by 20 pp of the covering share has significantly decreased the cost per recipient.

Moreover, the 20 pp cut in the covering share has decreased the average price for subsidized operations by 1.7% (column 8). Although the effect is small, it means that the covering share is likely to distort housing choices in the subsidized segment. We lay out this effect by some potential recipients downgrading their housing choices, either through alternative locations or by lowering housing floor area. Finally, the cut by 20 pp of the covering seems not to affect the local housing market dynamics. As we focus on border municipalities, potential first-time owners may have changed their location choices, thus affecting other markets that are spatially close.

5.2 Treatment Effects and Dose-Response Functions

The results from the estimation of the GPS are reported in the Section C.2 for varying specifications of the effects of pre-treatment variables and geographic coordinates. Our preferred model has a maximum degree of freedom of 200 (Fig. C.3). For this specification, 90.1% of municipalities are correctly classified in ABC according to the maximum probability rule. The substantial flexibility offered by the model nevertheless allows for good overlap (particularly for adjacent categories such as A and B1, B1 and B2, and B2 and C, Fig. C.1), thereby ensuring a good balance between the control and treatment groups, while also enabling a fine-grained account of the effects of pre-treatment variables and geographic coordinates (Fig. C.2 and Fig. C.3).

The second stage of our estimation procedure is then to use the GPS predictions as weights in the outcome equations that will bring all the predictions required to estimate dose-response functions (Equation 11). The joint F-tests of each covariate block are reported in Table C.3. The pre-treatment variables explain more than 74% of the variance in the number of first-time owners. Housing-supply growth, proxied by the number of newly built dwellings, is strongly significant for both the number of first-time owners and the number of recipients. Likewise, local transaction prices and median income significantly predict ownership transitions, underscoring the role of affordability. These findings confirm the usefulness of incorporating rich pre-treatment data to absorb potential heterogeneity across municipalities in our doubly robust estimation.

We then consider all combinations of bilateral effects (g, g') using the weights from Słoczyński and Wooldridge (2018) to compute the efficiency derivatives defined in (Equation 3). The estimated effects and their bootstrap standard errors (500 iterations) are reported in Table C.4 for ATEs and Table C.5 for ATTs. They serve as the inputs for the dose-response regressions that attribute each effect to the corresponding variation in the policy parameters. Hence, we estimate each β_a^Y from Equation 5 by regressing the bilateral effects on the differences in treatment variable values between treatment levels g and g'. Table 3 presents the ATE and ATT coefficients from the dose-response functions. Dose response plots are available from Fig. C.4 to Fig. C.7.

An increase in the *covering* share has a sizeable and highly significant impact on policy cost, yet it does not translate into a significant rise in the number of new homeowners. Hence, expanding the subsidy rate fails to achieve the policy's primary objective at the extensive margin. Instead, a 1pp increase in the covering share raises transaction prices across the entire local market (+3.2%), pointing to substantial price capitalization. The covering share appears to operate as a broad demand shifter: by uniformly increasing subsidy intensity across eligible operations, it stimulates aggregate demand without altering recipients' housing choices (size, price per square meter). In a context of relatively in-

Table 3. Regression Results from Dose-Response Functions

	Population of interest:								
	Full population				IFL recipients only				
	N	V	S	P	$\widetilde{N}$	$\widetilde{V}$	$\widetilde{S}$	$\widetilde{P}$	$\widetilde{C}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Covering Share	-0.007	0.032**	0.004	0.007	-0.033	0.001	0.010	-0.000	0.041***
	(0.025)	(0.015)	(0.006)	(0.015)	(0.046)	(0.007)	(0.011)	(0.011)	(0.011)
Ceiling Value	-0.024	-0.003	-0.006	-0.004	0.091**	-0.013	-0.047***	0.026***	-0.006
	(0.026)	(0.015)	(0.005)	(0.014)	(0.035)	(0.009)	(0.010)	(0.008)	(0.010)
Interest Rate	-0.004	0.001	-0.001	-0.003	0.021**	-0.005***	-0.009***	0.004	-0.001
	(0.007)	(0.004)	(0.001)	(0.004)	(0.010)	(0.002)	(0.003)	(0.002)	(0.003)
Loan Maturity	0.009	-0.002	-0.001	-0.004	-0.025	0.004	0.025***	-0.016***	0.021***
	(0.012)	(0.011)	(0.004)	(0.009)	(0.021)	(0.003)	(0.005)	(0.004)	(0.006)
Price	0.003					-0.001			
	(0.005)					(0.003)			
Constant	-0.038	-0.000	-0.000	-0.000	-0.130	0.009	-0.039	0.035*	-0.014
	(0.052)	(0.000)	(0.000)	(0.000)	(0.077)	(0.021)	(0.023)	(0.020)	(0.023)
R^2	0.162	0.569	0.505	0.649	0.522	0.334	0.456	0.434	0.906
Adj. R^2	0.022	0.513	0.441	0.603	0.460	0.223	0.386	0.361	0.894
N	36	36	36	36	36	36	36	36	36

Reading:
N accounts for the number of new homeowners; $\tilde{N}$ accounts for the number of IFL beneficiaries
V accounts for the average housing value; $\tilde{V}$ accounts for the average subsidized housing value
S accounts for the average housing surface; $\tilde{S}$ accounts for the average subsidized housing surface
P accounts for the average housing price per m^2; $\tilde{P}$ accounts for the average subsidized housing price per m^2
$\tilde{C}$ accounts for the average cost per recipient
Notes: For the nine outcomes Y in columns, the table reports the β_a^Y coefficients associated to each treatment variable in rows. They are estimated from dose-response functions of Equation 5. The variables with a $\tilde{}$ are the same variables computed for IFL recipients, and $\widetilde{C}$ is the IFL cost per recipient. The interest rate is expressed in a hundredth of a percent. The unit of observation is the bilateral combination of four ABC zones for the three periods of interest; the full set of policy-relevant treatment effects is reported in the Table C.4 and Table C.5 of OA. Standard errors in parentheses are estimated using a bootstrap with 500 iterations, accounting for the uncertainty of treatment effects. ATEs for tax and transaction data are unweighted, and ATT for IFL variables are weighted according to the number of municipalities receiving the considered treatment levels.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.
*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

elastic short-run supply, the additional demand is largely absorbed through higher prices rather than additional transitions into homeownership.

By contrast, increasing the *ceiling value*, thereby relaxing the binding constraint for the most expensive operations, strongly expands the number of recipients (+9.1% for a €1,000 increase) while leaving average policy cost per recipient and overall local prices largely unaffected. However, its effects concentrate on the intensive margin. Higher ceilings reduce average housing size but increase the price per square meter, suggesting an upgrading effect toward higher-quality locations or constructions. Because the ceiling only raises treatment intensity for high-end purchases, it primarily reshuffles demand within the market, fostering opportunistic behavior and reallocating buyers toward more expensive or more recent properties. While this shift enlarges the pool of beneficiaries, it does not generate additional transitions in the existing market, nor does it significantly affect the overall number of homeowners.

Mortgage conditions also shape recipients' housing choices. An increase in interest rates leads beneficiaries to downscale housing size, indicating that the additional subsidy associated with higher rates is more than offset by the higher borrowing cost. Conversely, longer maturities allow households to purchase larger dwellings, albeit at a lower price per square meter. Overall, the limited response at the extensive margin likely stems from two structural factors: first, the subsidy does not sufficiently bridge the price gap between existing and newly built housing to relax affordability constraints; second, housing supply responds weakly to demand shocks. As a result, policy-induced demand increases are largely capitalized into prices rather than translating into higher homeownership rates.

5.3 Policy Simulation and Return on Investment

Armed with the estimated dose-response functions, we now provide counterfactual simulations that quantify the ROI of raising public spending through each treatment variable (Equation 3). To ensure comparability, we simulate variations in the ceiling value, covering share, and mortgage conditions that yield a similar increase in total programme cost, thus normalizing the measures to "one extra euro" of expenditure.

The impact of a change in any treatment variable a on the total cost of the IFL policy combines the effect on the cost per recipient ($\partial\widetilde{C}/\partial a$) and the number of recipients ($\partial\widetilde{N}/\partial a$). Consequently, even though raising the ceiling value has no significant effect on the average cost per recipient (Table 3), it still raises total cost because it markedly enlarges the pool of beneficiaries, which could lead to a lower ROI.

Table 4 presents a set of simulations designed to identify the effects of higher subsidies on policy ROI, given that policymakers' marginal valuations of extensive- and intensive-margin effects are unknown and implicit in policy choices. We assume throughout that an additional homeowner is valued positively, in line with the programme's stated objective. The valuation of intensive-margin effects is more ambiguous: policymakers may either favour larger dwellings (implying a positive valuation) or seek to limit housing market effects (implying a negative valuation). We therefore consider three alternative scenarios: positive, zero, and negative monetary values.

Recalling that any increase in a mechanically raises the programme's budget cost, Table 4 reports the effects of higher subsidy intensity on the ROI. Depending on which parameter (in rows) is modified and on the assumed valuations of the extensive and intensive margins (in columns), the impact of the reform on policy efficiency differs substantially.

For an extensive-margin valuation of $\phi =$€5,000 per additional homeowner, increasing

Table 4. ROI Measures According to Policy Objectives

	$\phi =$ €5,000				$\phi =$ €1,000			
ψ	€-2,000	€-1,000	€0	€1,000	€-2,000	€-1,000	€0	€1,000
Covering Share	-0.62	2.23	5.08	7.93	-5.49	-2.64	0.22	3.07
	(26.1)	(22.0)	(20.7)	(22.8)	(17.2)	(9.2)	(4.1)	(9.6)
Ceiling Value	-1.16	-2.33	-3.50	-4.67	0.84	-0.33	-1.50***	-2.67*
	(4.2)	(3.2)	(2.7)	(3.1)	(3.2)	(1.7)	(0.5)	(1.6)
Interest Rate	0.29	-0.72	-1.74	-2.75*	0.88	-0.13	-1.15***	-2.16***
	(2.1)	(1.7)	(1.5)	(1.6)	(1.3)	(0.7)	(0.3)	(0.7)
Maturity	-0.95	-0.48	-0.02	0.44	-1.73	-1.27*	-0.80***	-0.34
	(1.9)	(1.5)	(1.3)	(1.4)	(1.4)	(0.8)	(0.3)	(0.7)

Notes: Exploiting coefficients derived from the dose-response function (Table 3), we calculate ROI for a cost-normalised increase of the overall IFL budget using Equation 3 for the four sources of treatment variables. Our results can be interpreted as the monetary benefits from a €1 increase in the IFL budget cost from a policymaker perspective. As our ROI measure depends on the marginal valuation of extensive margin effects (ϕ), distortion of housing choices (ψ), and price capitalization, we simulate different scenarios. The left panel (respectively bottom panel) corresponds to the situation in which an additional homeowner is valued at €10,000 (respectively €1,000). We report in parentheses the standard errors using the 500-iteration bootstrap procedure.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.
*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

either the ceiling value or the covering share does not lead to a significant improvement in ROI, regardless of the valuation assigned to the intensive margin. The only statistically significant change arises from an increase in the interest rate, which raises the programme's cost without generating a sufficient number of additional homeowners to offset the higher expenditure. Our estimates even predict a significant decline in ROI when the interest rate increases under this valuation scenario. Even when assigning this high value to the extensive margin, thereby placing substantial weight on this policy objective, the positive effects of increasing the covering share (for intensive-margin valuations above –€2,000) are not estimated with sufficient precision to reject the null of no impact on the programme's budgetary efficiency.

For lower valuations of the extensive margin ($\phi =$€1,000, closer to those commonly retained in the literature), some statistically significant effects emerge, yet they do not point toward an improvement in policy efficiency. Overall, increasing the budget allocated to the IFL policy does not appear to be an effective way to enhance its efficiency. Although a higher covering share may raise ROI when the intensive margin is positively valued, our estimates remain too imprecise to rule out the absence of any effect. By contrast, increases in the ceiling value, the interest rate, or loan maturity significantly reduce policy efficiency when the intensive margin is weakly positively valued.

These seemingly counterintuitive findings can be explained by the dominance of indirect over direct effects. While beneficiaries may use more generous subsidies to purchase more desirable dwellings, the resulting price effects operating through V offset these gains. Consequently, the capitalization of subsidies into housing prices counterbalances the positively valued improvements in housing choices (see Equation 3). In summary, raising the subsidy, regardless of the lever used, is at best inefficient and may even be counter-productive. The policy mainly reshaped recipients' housing choices and encouraged opportunistic switching from the non-subsidized to the subsidized segment, yet it failed to expand homeownership.

6 Conclusion

The French IFL policy is intended to raise the number of new homeowners by cutting the interest burden for eligible households. Exploiting spatial and temporal variation in treatment, we apply a selection-on-observables design to evaluate how changes in subsidy intensity affect objectives on both the extensive margin and the intensive margin, and price capitalization. Our GPS specification and the accompanying regression adjustment include, inter alia, spatial coordinates to mitigate omitted-variable spatial bias from local contexts. Using linear dose-response functions, we perform simulations under alternative valuations of extensive- and intensive-margin effects and derive estimated values for the policy's ROI.

We cannot rule out the possibility that higher IFL spending leaves the number of new homeowners unchanged. Although our identification strategy cannot completely dismiss extensive-margin responses, our estimates show that intensive-margin effects outweigh any plausible extensive-margin gains—the latter being the policymakers' primary target. Additional subsidy mainly shifts demand from existing to new housing, reflecting opportunistic behaviour. Consequently, unless intensive-margin benefits are valued very highly, the ROI is non-positive and may even be negative. In short, whichever ceiling value or covering share is used, expanding the IFL budget appears economically inefficient. Importantly, the consistency between the quasi-experimental estimates obtained from the 2018 reform in a DiD framework and the broader selection-on-observables approach strengthens the credibility of our conclusions. Our reform-based DiD estimates provide a credible causal benchmark, while the richer spatio-temporal variation allows us to assess policy design and compute policy-relevant return-on-investment measures.

These findings are consistent with the economic mechanisms highlighted throughout the paper. In supply-constrained housing markets, subsidies to first-time buyers are expected to primarily benefit inframarginal households and are largely capitalized into housing prices, limiting their ability to expand homeownership. The absence of significant effects on the extensive margin should therefore be interpreted as an equilibrium outcome of the policy, rather than as a lack of statistical power or identification.

From a policy perspective, our results caution against expanding demand-side subsidies to homeownership in supply-constrained markets without accounting for price capitalization and intensive-margin distortions. Increasing the intensity of subsidies raises public budget cost while delivering limited—or even negative—returns in terms of new homeowners. More generally, the framework developed in this paper provides a consistent way to evaluate housing subsidies beyond the French context and highlights the importance of policy design when targeting homeownership development.

Several issues warrant further research. Because price capitalization depends on housing-supply elasticity and land availability, the externalities generated by interest cuts are likely to vary across areas. Mapping this heterogeneity more precisely would help tailor the IFL to places policymakers wish to favour. Moreover, as the scheme alters recipients' housing choices, it raises concerns about land consumption and interactions with land-development objectives—a dimension that future work could explore.

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Online Appendix (not for publication)

A Budget Cost of the policy

The budget cost of IFL for the subsidized first-time owner is equal to the implicit subsidy in decreasing mortgage cost. Consider a loan of total value V_b for a loan duration d at a yearly interest rate of r. For each due date, t, the new homeowner reimburses a fixed payment m. The remaining capital to reimburse at the end of the year is:

$$X_t = X_{t-1} - m + rX_{t-1} = (1+r)\ X_{t-1} - m \tag{12}$$

Then, we obtain from the condition $X_0 = V_b$:

$$X_t = (1+r)^t \left[V_b - \frac{m}{r} \right] + \frac{m}{r} \tag{13}$$

Thus, we estimate the monthly payment using $X_D = 0$, corresponding to the loan maturity. Hence, we obtain:

$$m = \frac{rX_0}{1-(1+r)^{-D}} \tag{14}$$

yielding an overall cost for the household to

$$C = \sum_{k=1}^{D} m - V_b = \left[\frac{D\,r}{1-(1+r)^{-D}} - 1 \right] V_b \tag{15}$$

B Additional Descriptive Statistics

B.1 ABC Perimeter

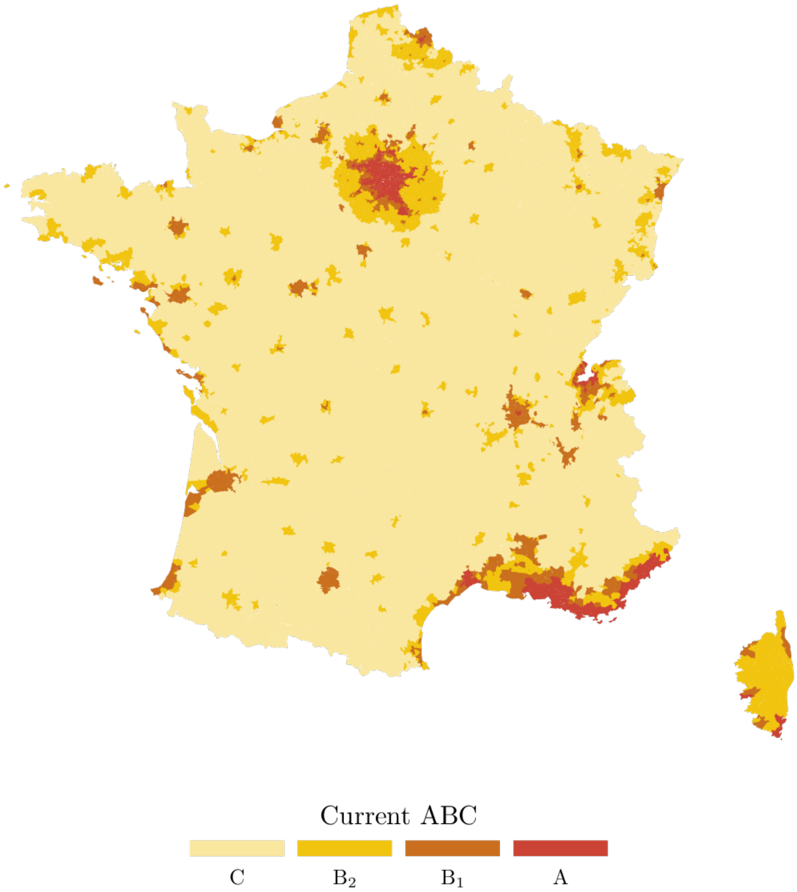


Figure B.1. ABC Zoning of French Municipalities

B.2 Outcome Variables

Table B.1. The Policy Outcomes tabulated with the ABC Zoning

	N	Mean	Std Dev	Median	Q1	Q3	Min	Max
A								
FTO	1,874	258.7	614.6	118.0	48.0	48.0	1.0	12,760.0
Price (Transaction)	1,874	443,572	426,877	320,763	261,133	261,133	107,760	5,999,507
Surface (Transaction)	1,874	80	17	78	68	68	39	177
Unit. Price (Transaction)	1,874	7,279	11,407	4,416	3,470	3,470	1,197	238,899
Recipients	1,874	46.8	102.6	17.0	5.0	5.0	1.0	2,182.0
Price (IFL)	1,874	281,426	67,118	273,749	232,633	232,633	107,000	660,000
Surface (IFL)	1,874	81	25	75	62	62	22	271
Unit. Price (IFL)	1,874	3,662	1,096	3,407	3,003	3,003	641	10,897
Cost	1,874	19,068	5,295	18,568	15,694	15,694	2,264	40,806
B_1								
FTO	3,295	122.4	348.7	48.0	24.0	24.0	1.0	8,268.0
Price (Transaction)	3,295	309,657	263,593	254,787	208,695	208,695	111,964	7,386,864
Surface (Transaction)	3,295	90	17	89	79	79	39	175
Unit. Price (Transaction)	3,295	4,174	5,056	3,095	2,536	2,536	1,442	119,469
Recipients	3,295	18.7	38.4	8.0	3.0	3.0	1.0	848.0
Price (IFL)	3,295	238,583	53,601	232,025	202,959	202,959	68,441	620,610
Surface (IFL)	3,295	95	23	95	80	80	30	280
Unit. Price (IFL)	3,295	2,595	613	2,499	2,197	2,197	615	10,753
Cost	3,295	17,445	4,734	17,189	14,408	14,408	2,422	41,496
B_2								
FTO	6,200	55.2	129.4	25.0	12.0	12.0	1.0	2,572.0
Price (Transaction)	6,200	213,863	201,699	186,010	154,125	154,125	20,000	6,297,033
Surface (Transaction)	6,200	96	16	95	86	86	38	191
Unit. Price (Transaction)	6,200	2,630	3,528	2,117	1,713	1,713	345	125,116
Recipients	6,200	8.2	11.5	4.0	2.0	2.0	1.0	127.0
Price (IFL)	6,200	207,007	41,762	200,911	179,365	179,365	60,691	479,954
Surface (IFL)	6,200	103	21	101	93	93	1	500
Unit. Price (IFL)	6,200	2,117	3,142	1,978	1,766	1,766	372	172,197
Cost	6,200	11,906	5,642	10,576	7,370	7,370	995	47,635
C								
FTO	29,463	17.5	23.1	11.0	6.0	6.0	1.0	424.0
Price (Transaction)	29,463	161,697	137,386	144,000	114,000	114,000	12,000	6,258,743
Surface (Transaction)	29,463	100	18	99	90	90	20	400
Unit. Price (Transaction)	29,463	1,849	2,728	1,534	1,229	1,229	138	169,770
Recipients	29,463	3.7	5.7	2.0	1.0	1.0	1.0	169.0
Price (IFL)	29,463	181,554	37,411	178,500	157,928	157,928	40,000	492,888
Surface (IFL)	29,463	108	22	104	95	95	1	700
Unit. Price (IFL)	29,463	1,769	2,769	1,706	1,504	1,504	165	243,577
Cost	29,463	9,851	5,117	8,489	5,729	5,729	322	41,061

B.3 Pre-Treatment Variables

Table B.2. Pre-treatment Variables included in the Double Robust Analysis

	N	Mean	Std Dev	Median	Q1	Q3	Min	Max
Density	26,819	1.926	8.058	0.497	0.244	1.142	0.007	259.982
CS1	26,819	2.91	3.86	1.59	0.18	4.00	0.00	55.00
CS2	26,819	4.34	2.82	3.93	2.57	5.65	0.00	31.25
CS3	26,819	5.81	4.47	4.88	2.79	7.84	0.00	38.46
CS4	26,819	13.50	5.60	13.33	9.62	17.13	0.00	45.00
CS5	26,819	15.87	4.83	15.87	12.98	18.64	0.00	60.14
CS6	26,819	15.43	6.23	15.00	11.11	19.21	0.00	55.00
CS7	26,819	29.92	9.14	29.07	23.65	35.38	0.00	87.50
Price	26,819	153,108	68,002	141,975	110,323	181,032	20,518	2,261,166
Price per m^2 (2010–2013)	26,819	1,608.7	691.8	1,471.0	1,181.4	1,855.4	159.3	19,306.5
Neigh Price per m^2	26,819	1,561.0	724.0	1,420.2	1,123.6	1,807.3	0.0	35,686.1
New Housing (2010–2013)	26,819	42	227	9	4	25	1	15,748
Median Income (2013)	26,819	19,954	3,399	19,432	17,774	21,546	8,774	47,316
Longitude (WGS 84)	26,819	653,319	187,946	653,382	511,822	802,857	124,073	1,072,432
Latitude (WGS 84)	26,819	6,651,138	243,230	6,677,060	6,448,774	6,858,734	6,139,677	7,108,696

Notes: Our sample is composed of 26,819 French municipalities. The average density is in inhabitants per kilometer square, CS variables correspond to the percent of socio-professional categories with CS1 corresponds to farmers, CS2 to artisans and merchants, CS3 to managers, CS4 to intermediate professions, CS5 to employees, CS6 to labor works, CS7 to retired. Prices and incomes are in current values.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.

C Additional Results

C.1 Difference-in-Difference Results

Table C.1. DiD estimation for policy effects following the 2018 reform for full sample of municipalities

	Population of interest:								
	Full population				Recipients				
	N	V	S	P	$\widetilde{N}$	$\widetilde{V}$	$\widetilde{S}$	$\widetilde{P}$	$\widetilde{C}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Time	-0.088***	0.056***	0.011***	0.061***	-0.232***	0.038***	-0.017***	0.055***	-0.134***
	(0.009)	(0.014)	(0.002)	(0.016)	(0.021)	(0.004)	(0.005)	(0.004)	(0.006)
Time × Group	0.035***	-0.009	-0.005**	-0.017	-0.132***	-0.007	0.009	-0.016***	-0.702***
	(0.010)	(0.014)	(0.003)	(0.017)	(0.023)	(0.005)	(0.006)	(0.005)	(0.007)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,896	22,896	22,896	22,896	22,896	22,896	22,896	22,896	22,896
R^2	0.962	0.820	0.842	0.813	0.880	0.797	0.671	0.837	0.895
Within R^2	0.017	0.014	0.002	0.009	0.151	0.024	0.001	0.030	0.837

*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

Reading:

N accounts for the number of new homeowners; $\tilde{N}$ accounts for the number of IFL beneficiaries

V accounts for the average housing value; $\tilde{V}$ accounts for the average subsidised housing value

S accounts for the average housing surface; $\tilde{S}$ accounts for the average subsidised housing surface

P accounts for the average housing price per m^2; $\tilde{P}$ accounts for the average subsidised housing price per m^2

$\tilde{C}$ accounts for the average cost per recipient

Notes: We report the OLS coefficients from a Difference-in-Difference estimation. We leverage the 2016 reform, lowering the covering share for some municipalities by 20pp. The observation unit is a municipality per period of interest. 'Times' is a dummy variable equalling to 1 if the period under consideration is after 2016, 0 otherwise. We estimate that following the 20pp cut, the number of recipients has decreased by 17.0% compared to non-treated municipalities. All outcome variables are transformed through a log transformation. As we introduce municipality fixed effects, we do not add group fixed effects. We report clustered standard errors at the urban area level in parentheses.

Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.

C.2 GPS Estimation (first stage)

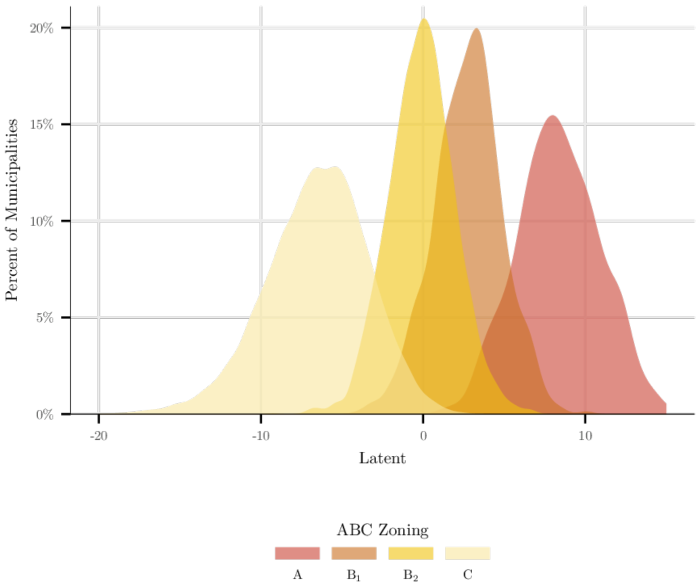
Table C.2 reports the joint significance of each variable according to the maximum degree allowed for spline smoothing. Overlap is assessed from the distribution of the latent tightness variable from ordered ABC modelling (Fig. C.1). Each continuous pre-treatment variable enter through univariate spline transformations reported in Fig. C.2 and geographic coordinates enter in a bivariate function reported in Fig. C.3.

Table C.2. Covariates' joint significance from ordered GAMs

	Outcome: Ordered ABC Zoning					
	No Spatial Smoothing		With Spatial Smoothing			
Max. degrees of freedom	df = 0	df = 0	df = 50	df = 50	df = 100	df = 200
Population Density	1,991.3***	1,723.1***	2,003.3***	1,656.4***	1,688.5***	1,479.1***
	[6.1]	[5.7]	[5.8]	[5.8]	[6.0]	[6.0]
New Housing Unit	468.7***	99.0***	295.2***	126.1***	127.4***	141.2***
	[6.0]	[5.3]	[5.3]	[5.0]	[4.9]	[4.8]
Median Annual Income	1,647.6***	353.5***	654.7***	208.4***	200.5***	182.7***
	[6.7]	[6.6]	[6.7]	[6.2]	[6.1]	[6.0]
Professional Ocupations	984.1***	819.4***	312.9***	317.3***	273.8***	267.8***
	[37.0]	[28.4]	[30.6]	[32.6]	[25.1]	[26.8]
Unitary Housing Price		214.8***		70.3***	67.7***	51.3***
		[6.6]		[5.5]	[5.2]	[1.0]
Neighboring Unitary Price		110.4***		37.1***	26.6***	23.1***
		[1.1]		[3.1]	[1.0]	[4.2]
Spatial Coordinates			4,018.4***	2,211.0***	2,575.9***	3,048.8***
			[47.9]	[47.3]	[90.2]	[165.2]
Number of Observations	26,818	26,818	26,818	26,818	26,818	26,818
McFadden R2	52.60	61.31	67.08	69.17	70.98	73.81
Percent of Good Predictions	85.88	87.31	88.94	89.29	89.70	90.13
Akaike Information Criterion	18,625.4	16,406.0	14,602.8	14,152.3	13,736.9	13,156.0

Notes: The unit of observation is the French municipality. The top panel reports χ^2 statistics of joint significance for each covariate of the first-stage GPS. Professional Occupations are coded as population shares of eight categories according to the one-digit French *Catégories Socio-Professionelles*. The effective degrees of freedom reported in brackets indicate the smoothing intensity, low values correspond to more smoothing. Estimations come from the `gam` function of the `mgcv` R package (Wood, Pya and Säfken, 2016). In our preferred specification (df = 200), the relationship between unitary price and treatment intensity is linear and increasing, consistent with the ABC perimeter definition.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.
*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$.

Figure C.1. Overlap between predictions of tightness between the different ABC zones



Notes: The distributions of the latent tightness variable (x-axis) are predicted from the first stage GPS with a maximum degree of freedom set to 200 (6^{th} column of Table C.2). The latent variable is unitless and is displayed between municipalities according to the ABC classification. We report the distribution *within* each classification level (rather than the distribution of the all population) for clarity.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.

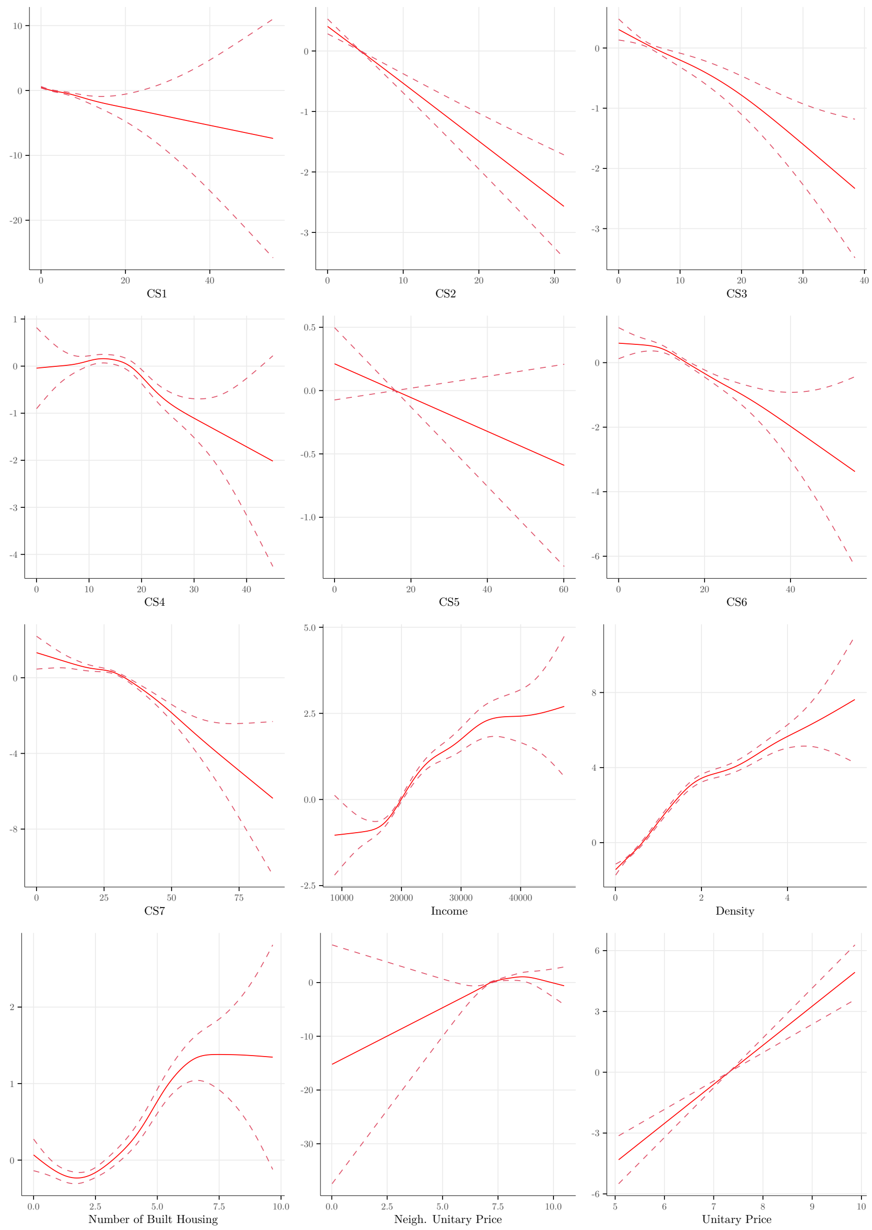


Figure C.2. Marginal Effects of Pre-Treatment Variables

Notes: For each pre-treatment variable, we draw its marginal effect in the GPS estimation, following the endogenous shrinkage procedure to set the effective degree of freedom. In addition, we report the confidence interval for a 95% level from the `gam` function of the `mgcv` R package.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.

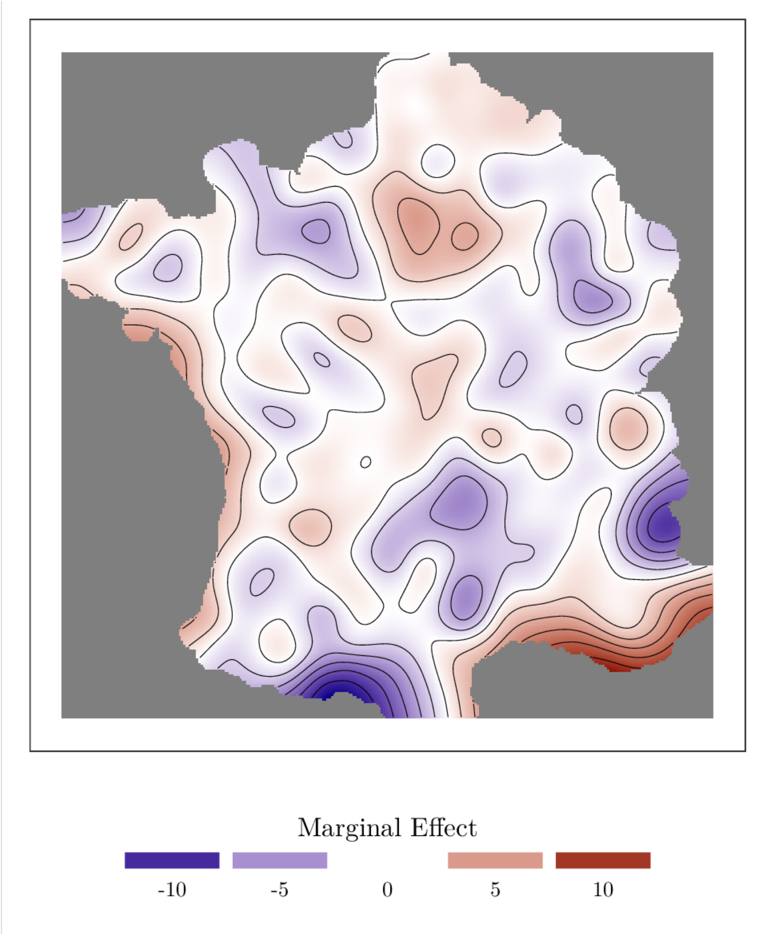


Figure C.3. Spatial Smoothing Function of Geographical Coordinates

Notes: We report the spatial smoothing function for the GPS estimation, using bi-variate additive splines. Spline parameters are endogenously shrunk using restricted maximum likelihood approach. The maximum degree of freedom is set to 200. Red (respectively blue) values indicate that the outcome is locally higher than the average. We exploit the `gam` function from the `mgcv` R package.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.

C.3 WLS Estimation (second stage)

Table C.3. Covariates' joint significance from second-stage pooled GAMs

	Outcome variables from...								
	Tax	Transaction Data			IFL Files				
	N	V	S	P	$\widetilde{N}$	$\widetilde{V}$	$\widetilde{S}$	$\widetilde{P}$	$\widetilde{C}$
Population Density	517.1***	9.6***	102.7***	50.8***	51.9***	17.6***	88.0***	58.9***	32.2***
	[8.7]	[6.6]	[8.6]	[7.8]	[8.7]	[6.8]	[7.7]	[7.9]	[4.2]
Number of New Housing	2,380***	38.7***	93.3***	120.1***	1,128***	8.8***	11.5***	10.6***	3.0**
	[8.2]	[5.6]	[4.6]	[5.6]	[7.1]	[4.3]	[4.3]	[4.1]	[4.1]
Median Income	53.4***	68.4***	198.2***	3.3**	16.9***	105.3***	27.3***	22.3***	1.5**
	[8.4]	[7.0]	[6.8]	[3.0]	[6.0]	[6.5]	[5.7]	[8.5]	[1.8]
Professional Occupations	771.3***	55.8**	21.3**	48.6**	357***	6.6**	6.6**	12.1**	34.0**
	[50.8]	[34.1]	[49.2]	[38.7]	[41.4]	[38.3]	[32.9]	[47.0]	[20.4]
Lagged Unitary Price	9.7***	21.7***	8.3***	31.9***	9.7***	4.7***	2.8***	3.8***	4.5***
	[8.2]	[4.5]	[8.2]	[3.5]	[9.0]	[3.1]	[6.4]	[8.6]	[7.6]
Lag. Neighbor. Unit. Price	6.7***	96.7***	11.7***	83.7***	9.1***	10.1***	18.3***	32.8***	7.1***
	[8.3]	[7.5]	[7.2]	[8.1]	[8.8]	[8.7]	[7.3]	[7.6]	[3.6]
Spatial Coordinates	33.7***	16.6***	37.6***	8.3***	22.0***	21.8***	8.5***	20.5***	7.0***
	[189]	[182]	[188]	[179]	[186]	[181]	[168]	[193]	[112]
Number of observations	54,993	54,993	54,993	54,993	54,993	54,993	54,993	54,993	54,991
McFadden R2	77.72	56.12	36.53	56.99	54.52	45.27	18.63	55.81	9.16

Notes: For the nine outcomes of interest (in columns), the table reports the F statistics for the joint significance of each covariate (in rows). N accounts for the number of new homeowners, V for housing value, S for surface, and P for unitary housing price. The variables with a $\tilde{}$ are the same variables computed for IFL recipients, and $\widetilde{C}$ is the IFL cost. We report pooled GAMs for all treatment levels for the sake of clarity, different GAMs are estimated for each treatment level in the policy-relevant treatment effects reported in the text. Professional Occupations are coded as population shares of eight categories according to the one-digit French *Catégories Socio-Professionelles*. The effective degrees of freedom reported in brackets indicate the smoothing intensity, low values correspond to more smoothing. The unit of observation is the French municipality and the maximum degree of freedom we allow for the spatial coordinates is 200.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.
*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

C.4 Dose-response Functions

Table C.4. Average Treatment Effects (ATE)

Area	*Treatement (ATE)* 2015 A	B$_1$	B$_2$	C	2016–2017 A	B$_1$	B$_2$	C	2018–2019 A	B$_1$	B$_2$	C
						N_g: Number of FTO						
A	-	0.242	0.451	0.469	-	-1.180	-0.359	-0.508	-	1.168	1.026	0.979
	-	(1.231)	(1.213)	(1.219)	-	(1.032)	(0.953)	(0.950)	-	(1.202)	(1.167)	(1.164)
B$_1$	-0.242	-	0.209	0.227	1.180	-	0.821**	0.672*	-1.168	-	-0.142	-0.189
	(1.231)	-	(0.185)	(0.174)	(1.032)	-	(0.393)	(0.389)	(1.202)	-	(0.297)	(0.295)
B$_2$	-0.451	-0.209	-	0.018	0.359	-0.821**	-	-0.149**	-1.026	0.142	-	-0.048
	(1.213)	(0.185)	-	(0.067)	(0.953)	(0.393)	-	(0.061)	(1.167)	(0.297)	-	(0.053)
C	-0.469	-0.227	-0.018	-	0.508	-0.672*	0.149**	-	-0.979	0.189	0.048	-
	(1.219)	(0.174)	(0.067)	-	(0.950)	(0.389)	(0.061)	-	(1.164)	(0.295)	(0.053)	-
						V_g: Average Housing Price (Overall Transaction)						
A	-	-0.431	-0.129	-0.199	-	0.446	0.488	0.456	-	0.294	-0.202	-0.248
	-	(0.784)	(0.730)	(0.729)	-	(0.583)	(0.564)	(0.562)	-	(1.195)	(1.184)	(1.187)
B$_1$	0.431	-	0.301	0.232	-0.446	-	0.043	0.010	-0.294	-	-0.496***	-0.542***
	(0.784)	-	(0.255)	(0.254)	(0.583)	-	(0.167)	(0.169)	(1.195)	-	(0.142)	(0.135)
B$_2$	0.129	-0.301	-	-0.070***	-0.488	-0.043	-	-0.033	0.202	0.496***	-	-0.046
	(0.730)	(0.255)	-	(0.021)	(0.564)	(0.167)	-	(0.021)	(1.184)	(0.142)	-	(0.036)
C	0.199	-0.232	0.070***	-	-0.456	-0.010	0.033	-	0.248	0.542***	0.046	-
	(0.729)	(0.254)	(0.021)	-	(0.562)	(0.169)	(0.021)	-	(1.187)	(0.135)	(0.036)	-
						Q_g: Average Housing Size (Overall Transaction)						
A	-	-0.081	0.132	0.113	-	0.177	0.147	0.168	-	0.270	0.128	0.158
	-	(0.294)	(0.286)	(0.284)	-	(0.324)	(0.321)	(0.321)	-	(0.293)	(0.289)	(0.290)
B$_1$	0.081	-	0.213***	0.194***	-0.177	-	-0.030	-0.010	-0.270	-	-0.142**	-0.112*
	(0.294)	-	(0.074)	(0.072)	(0.324)	-	(0.054)	(0.053)	(0.293)	-	(0.065)	(0.065)
B$_2$	-0.132	-0.213***	-	-0.019	-0.147	0.030	-	0.021	-0.128	0.142**	-	0.030**
	(0.286)	(0.074)	-	(0.019)	(0.321)	(0.054)	-	(0.015)	(0.289)	(0.065)	-	(0.014)
C	-0.113	-0.194***	0.019	-	-0.168	0.010	-0.021	-	-0.158	0.112*	-0.030**	-
	(0.284)	(0.072)	(0.019)	-	(0.321)	(0.053)	(0.015)	-	(0.290)	(0.065)	(0.014)	-
						P_g: Average Housing Price per m^2 (Overall Transaction)						
A	-	-0.116	0.092	0.061	-	0.209	-0.021	-0.103	-	0.259	-0.146	-0.227
	-	(0.867)	(0.852)	(0.850)	-	(0.588)	(0.562)	(0.561)	-	(1.087)	(1.080)	(1.081)
B$_1$	0.116	-	0.209	0.177	-0.209	-	-0.229	-0.312**	-0.259	-	-0.404***	-0.485***
	(0.867)	-	(0.208)	(0.203)	(0.588)	-	(0.154)	(0.156)	(1.087)	-	(0.137)	(0.133)
B$_2$	-0.092	-0.209	-	-0.031	0.021	0.229	-	-0.082***	0.146	0.404***	-	-0.081**
	(0.852)	(0.208)	-	(0.037)	(0.562)	(0.154)	-	(0.022)	(1.080)	(0.137)	-	(0.032)
C	-0.061	-0.177	0.031	-	0.103	0.312**	0.082***	-	0.227	0.485***	0.081**	-
	(0.850)	(0.203)	(0.037)	-	(0.561)	(0.156)	(0.022)	-	(1.081)	(0.133)	(0.032)	-

Notes: We report the bilateral combinations effect for ATE type estimator. The four panels correspond to the four outcome concerned by ATE estimation and derived from fiscal data. Then, we have three main columns that represent the stable period for the IFL scheme, with four subcolumns related to the ABC classification. In rows, we have again the levels contained in the ABC classification. Hence, the bilateral combinations are reported for each intersection, and must be understood as "if (rows) have received (cols), difference in outcome would be (results)". We also report in brackets the standard errors obtained with a bootstrap procedure with 500 iterations.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.*** $p < 0.01$, ** $p < 0.05$ * $p < 0.1$

Table C.5. Average Treatment Effects on the Treated (ATT)

	Treatment (ATT)											
	2015				2016–2017				2018–2019			
Area	A	B_1	B_2	C	A	B_1	B_2	C	A	B_1	B_2	C
$\widetilde{N}_g$: Number of IFL												
A	-	-0.999***	-0.804*	-0.938	-	-0.289	-0.871**	-2.548***	-	-0.095	-0.668	-1.933*
	-	(0.266)	(0.434)	(0.616)	-	(0.180)	(0.408)	(0.600)	-	(0.230)	(0.540)	(1.024)
B_1	3.446**	-	-0.196**	-0.290	0.275	-	-0.205**	-0.547***	-1.715	-	-0.210**	-0.576*
	(1.587)	-	(0.079)	(0.179)	(1.450)	-	(0.090)	(0.172)	(1.209)	-	(0.089)	(0.297)
B_2	3.809*	-0.145	-	-0.163***	0.185	-0.204	-	-0.233***	-0.279	-0.124	-	-0.261***
	(2.036)	(0.158)	-	(0.052)	(2.103)	(0.186)	-	(0.051)	(1.940)	(0.179)	-	(0.059)
C	5.385*	0.275	-0.009	-	2.255	-0.095	0.002	-	0.670	-1.472*	0.066	-
	(3.138)	(0.695)	(0.100)	-	(3.344)	(1.090)	(0.138)	-	(3.067)	(0.814)	(0.131)	-
$\widetilde{V}_g$: Average Housing Price (Subsidized Housing)												
A	-	-0.028	-0.191**	-0.076	-	-0.022	-0.011	-0.060	-	-0.069*	-0.161***	-0.046
	-	(0.054)	(0.082)	(0.103)	-	(0.027)	(0.054)	(0.096)	-	(0.036)	(0.060)	(0.085)
B_1	-0.801***	-	-0.043**	-0.034	-0.118	-	-0.050***	-0.036	-0.293	-	-0.048***	-0.048*
	(0.297)	-	(0.017)	(0.032)	(0.229)	-	(0.013)	(0.025)	(0.273)	-	(0.014)	(0.028)
B_2	-1.000**	0.096***	-	-0.030***	-0.163	0.054	-	-0.019**	-0.090	0.103***	-	-0.032***
	(0.391)	(0.030)	-	(0.011)	(0.313)	(0.035)	-	(0.008)	(0.378)	(0.028)	-	(0.009)
C	-1.508**	0.160	0.075***	-	0.222	0.100	0.033	-	0.207	0.307**	0.037	-
	(0.613)	(0.123)	(0.022)	-	(0.514)	(0.154)	(0.044)	-	(0.595)	(0.124)	(0.038)	-
$\widetilde{Q}_g$: Average Housing Size (Subsidized Housing)												
A	-	-0.109	0.167*	0.654***	-	-0.073*	0.077	0.242**	-	-0.083*	0.051	0.350**
	-	(0.087)	(0.090)	(0.160)	-	(0.041)	(0.066)	(0.098)	-	(0.049)	(0.069)	(0.144)
B_1	-1.027**	-	0.022	0.125***	-0.409	-	0.011	0.057**	-0.448	-	0.023	0.117***
	(0.457)	-	(0.027)	(0.045)	(0.290)	-	(0.018)	(0.027)	(0.319)	-	(0.017)	(0.045)
B_2	-1.062*	0.026	-	0.014	-0.996**	0.089*	-	0.010	-0.734	0.019	-	-0.001
	(0.582)	(0.039)	-	(0.013)	(0.408)	(0.053)	-	(0.008)	(0.469)	(0.036)	-	(0.014)
C	-1.997**	0.216	0.054**	-	-2.287***	0.473	0.012	-	-0.941	0.262*	-0.046	-
	(0.894)	(0.155)	(0.027)	-	(0.695)	(0.288)	(0.058)	-	(0.778)	(0.158)	(0.058)	-
$\widetilde{P}_g$: Average Housing Price per m^2 (Subsidized Housing)												
A	-	0.100	-0.355***	-0.460***	-	0.027	-0.094*	-0.302***	-	0.017	-0.214***	-0.396***
	-	(0.087)	(0.101)	(0.135)	-	(0.037)	(0.052)	(0.093)	-	(0.042)	(0.080)	(0.124)
B_1	0.231	-	-0.071***	-0.128***	0.382	-	-0.063***	-0.093***	0.212	-	-0.069***	-0.165***
	(0.370)	-	(0.022)	(0.039)	(0.263)	-	(0.014)	(0.023)	(0.233)	-	(0.018)	(0.039)
B_2	0.116	0.037	-	-0.044***	0.978**	-0.042	-	-0.026***	0.624*	0.077***	-	-0.032***
	(0.467)	(0.039)	-	(0.014)	(0.482)	(0.070)	-	(0.007)	(0.332)	(0.025)	-	(0.012)
C	0.484	-0.046	0.018	-	1.822**	-0.366	0.026	-	1.219**	0.079	0.084	-
	(0.705)	(0.149)	(0.030)	-	(0.836)	(0.340)	(0.026)	-	(0.548)	(0.119)	(0.067)	-
$\widetilde{C}_g$: Average Cost per IFL												
A	-	-0.125	-0.516***	-1.514***	-	-0.056	-0.226**	-0.173	-	-0.110**	-1.096***	-0.745***
	-	(0.079)	(0.162)	(0.393)	-	(0.043)	(0.096)	(0.179)	-	(0.049)	(0.115)	(0.148)
B_1	-0.096	-	-0.527***	-1.148***	0.155	-	-0.200***	-0.266***	-0.229	-	-0.874***	-0.922***
	(0.430)	-	(0.042)	(0.106)	(0.226)	-	(0.025)	(0.045)	(0.347)	-	(0.019)	(0.043)
B_2	0.245	0.509***	-	-0.490***	-0.151	0.144***	-	-0.110***	0.664	0.876***	-	-0.126***
	(0.560)	(0.056)	-	(0.030)	(0.364)	(0.037)	-	(0.014)	(0.580)	(0.059)	-	(0.017)
C	0.532	0.905***	0.555***	-	-0.432	0.508**	0.139***	-	1.197	0.976***	0.140***	-
	(0.908)	(0.224)	(0.086)	-	(0.628)	(0.222)	(0.033)	-	(0.979)	(0.232)	(0.039)	-

Notes: We report the bilateral combinations effect for ATT type estimator. The five panels correspond to the fives outcomes concerned by ATT estimation and derived from recipients' files. Then, we have three main columns that represent the stable period for the IFL scheme, with four subcolumns related to the ABC classification. In rows, we have again the levels contained in the ABC classification. Hence, the bilateral combinations are reported for each intersection, and must be understood as "if (rows) have received (cols), difference in outcome would be (results)". We also report in brackets the standard errors obtained with a bootstrap procedure with 500 iterations.

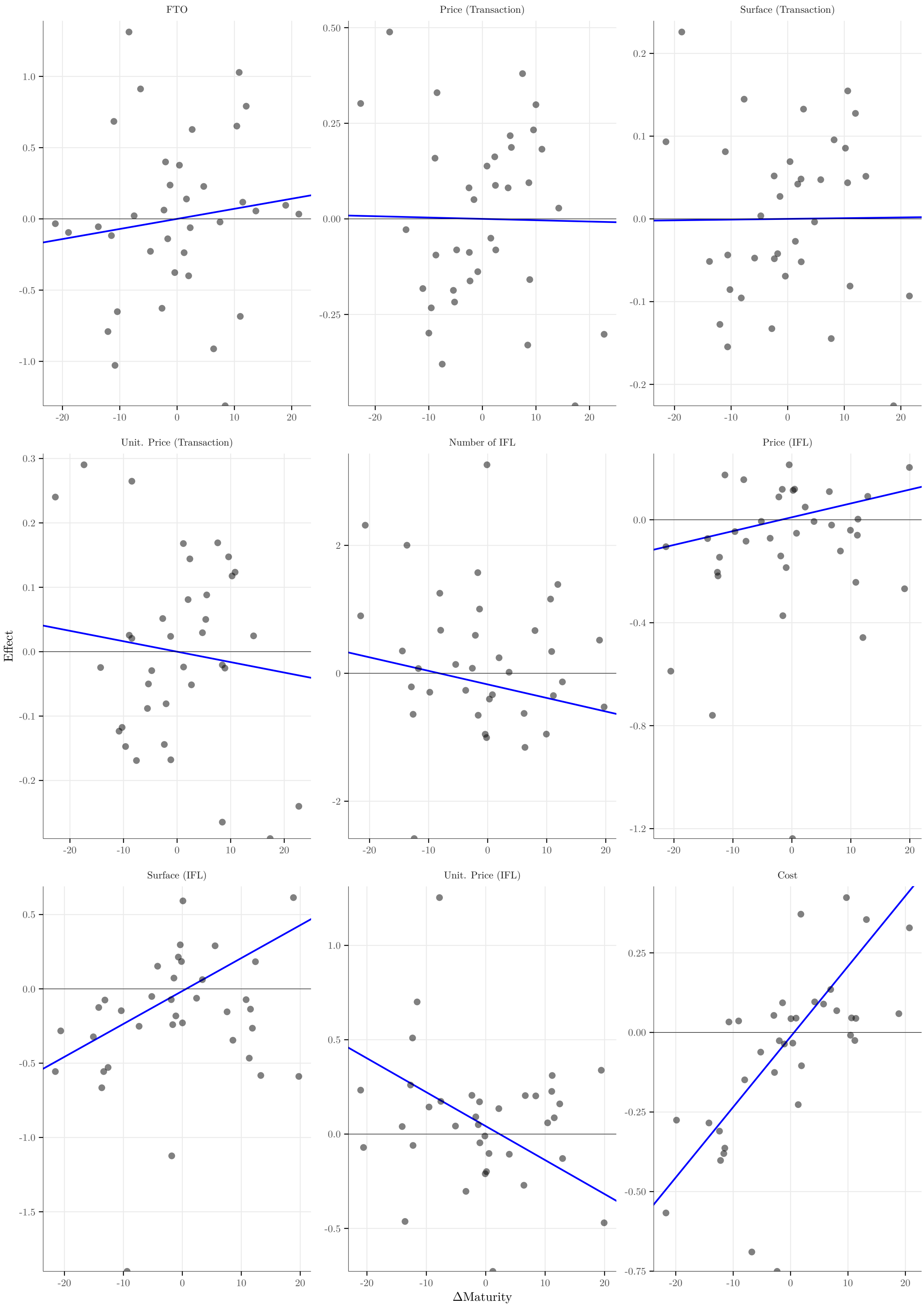


Figure C.4. Partial Plots for Loan Maturity variations

Notes: We report the partial plot for dose-response function. The nine partial plots correspond to our nine selected outcomes, while the x-axis represents a variation in primitive data sources. The observation unit is the bilateral combinations of treatment (either ATE or ATT). The regression is performed using OLS.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.

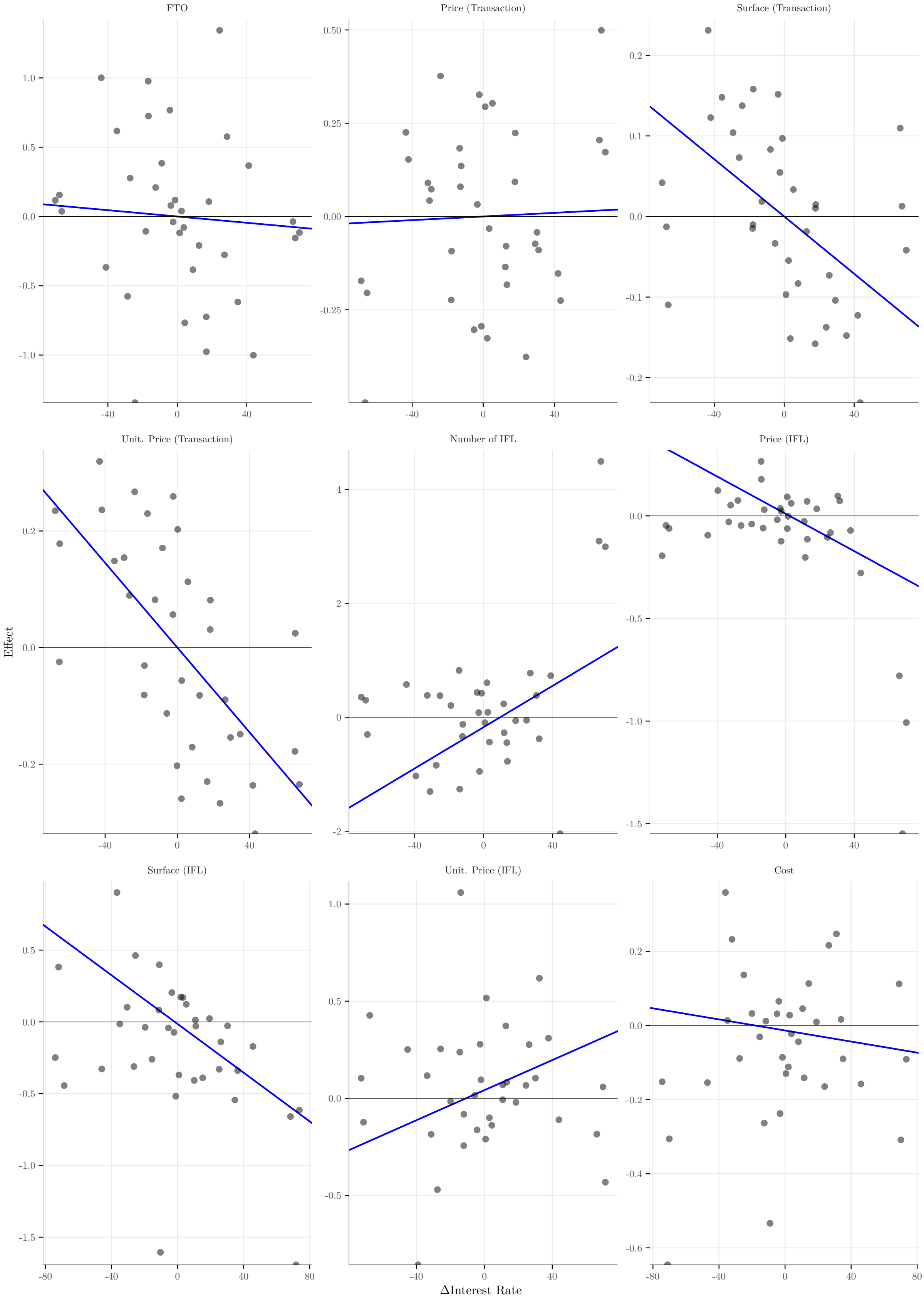


Figure C.5. Dose-Response Plots for Interest Rate Variations

Notes: We report the partial plot for dose-response function. The nine partial plots correspond to our nine selected outcomes, while the x-axis represents a variation in primitive data sources. The observation unit is the bilateral combinations of treatment (either ATE or ATT). The regression is performed using OLS.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.

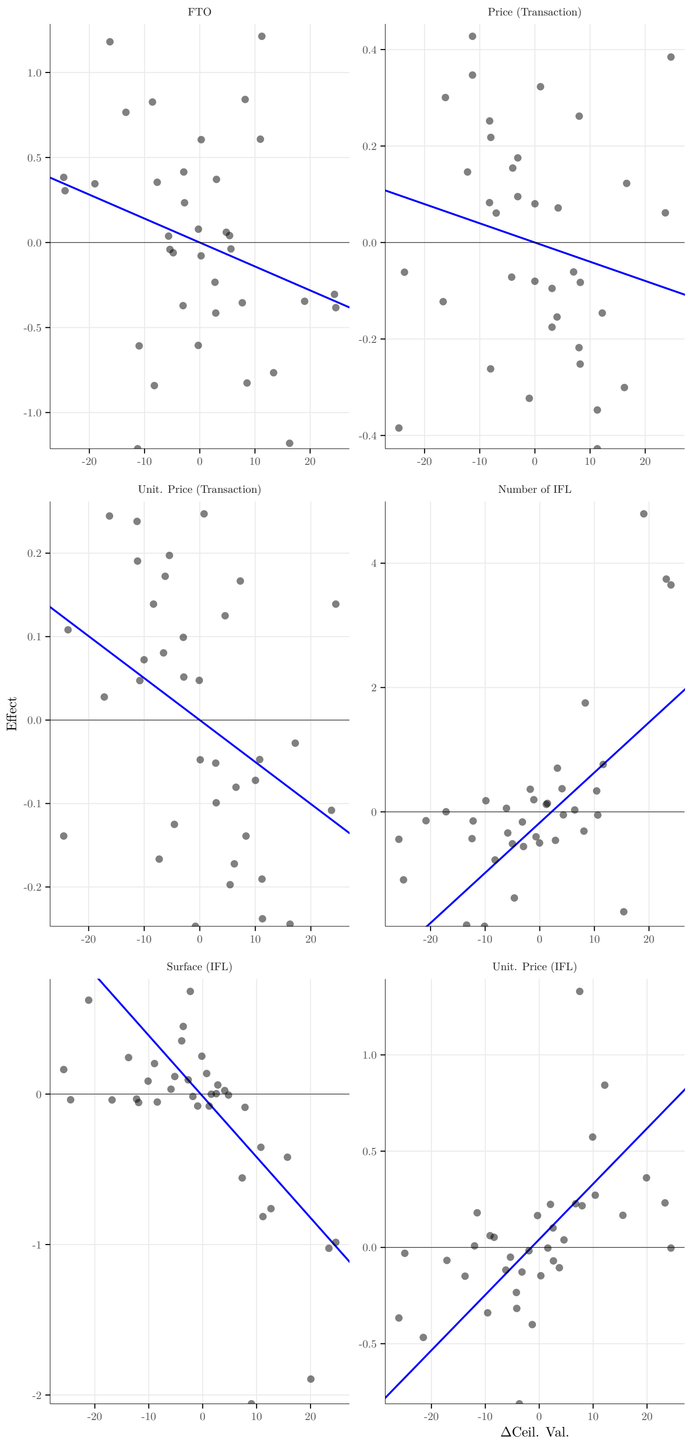


Figure C.6. Dose-Response Plots for Ceiling Value Variations

Notes: We report the partial plot for dose-response function. The nine partial plots correspond to our nine selected outcomes, while the x-axis represents a variation in primitive data sources. The observation unit is the bilateral combinations of treatment (either ATE or ATT). The regression is performed using OLS.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.

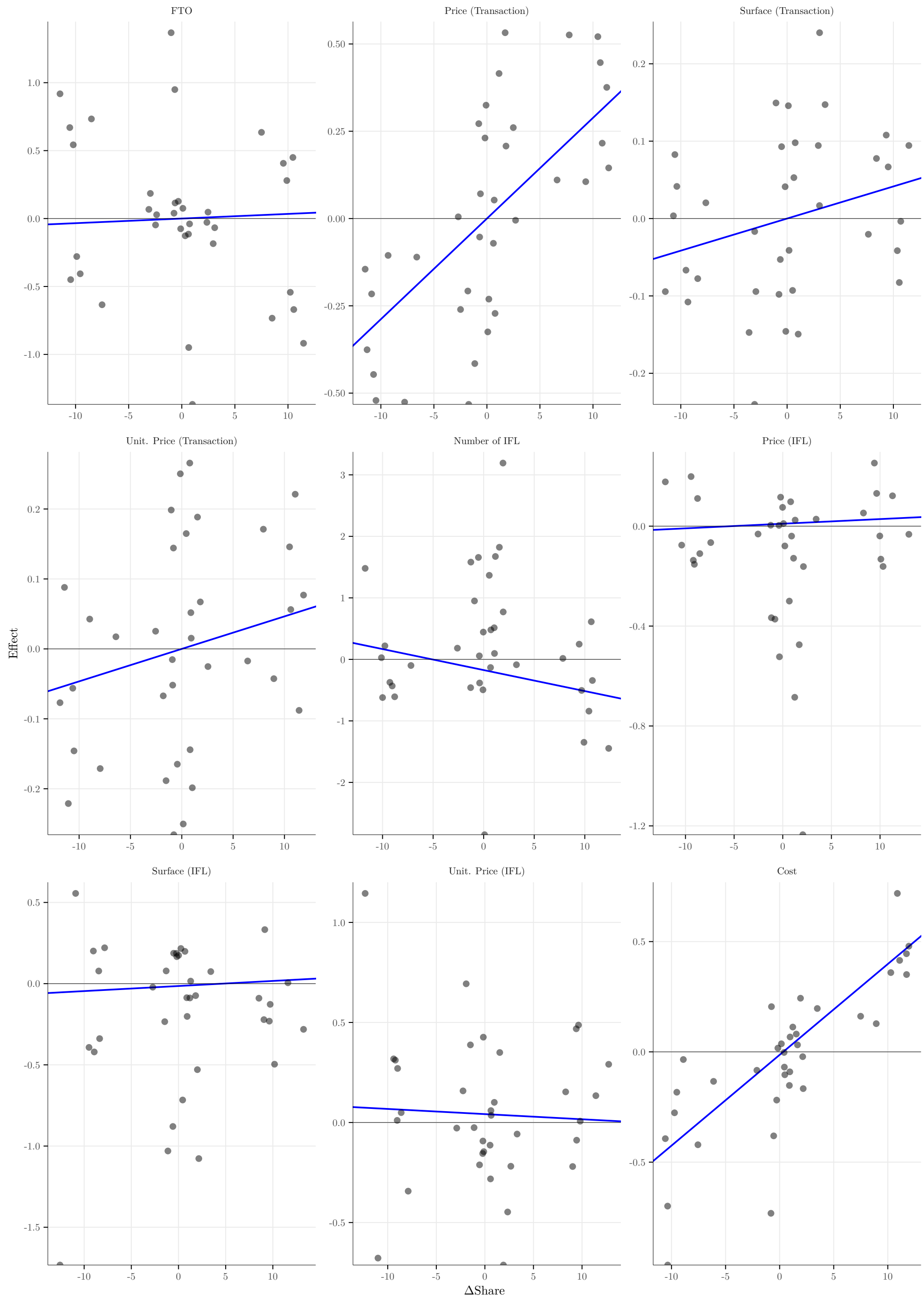


Figure C.7. Dose-Response Plots for Covering Share

Notes: We report the partial plot for dose-response function. The nine partial plots correspond to our nine selected outcomes, while the x-axis represents a variation in primitive data sources. The observation unit is the bilateral combinations of treatment (either ATE or ATT). The regression is performed using OLS.
Sources: Authors' Calculation based on SGFGAS, DV3F, *Fichiers Fonciers* and INSEE data.