Success and Failure of a Zero-Interest Green Loan program: Evidence from France

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March 3, 2024

- First state-of-the-art evaluation of a zero-interest green loan program, implemented in France in 2009 to encourage home energy retrofits.
- Event study using a panel survey and exploiting an eligibility restriction to homes built before 1990.
- Impact is limited to the first two years, with a +22% effect on the extensive margin of investment, +3% on the intensive margin and −3% on electricity consumption.
- Effects are more pronounced for low-income homeowners.
- Complementary analysis using banking data suggests banks nudge consumers away from the program.

Abstract

Zero-interest green loan programs (ZIGL) are gaining traction to address the tremendous financing needs implied by net-zero emission targets. We provide the first evaluation of such a program, the *Éco-Prêt à Taux Zéro*, introduced in France in 2009 to encourage home energy retrofits. Using an event-study design applied to a panel survey of 10,000 households, we find evidence that the program had a substantial, yet short-lived, effect. Eligibility to the program increased investment by 20-22% on the extensive margin and 2-3% on the intensive one, thereby generating 3% electricity savings. The effects are however limited to the first two years, after which they turn non-significant. They are primarily driven by low-income homeowners, suggesting the program effectively alleviates credit constraints. These results are robust to a range of robustness checks, including placebo regressions and propensity score weighting. They lead to leverage estimates in the 1.3-1.7 range in the 'successful' period and below 1 thereafter. Using additional banking data to investigate the post-2011 failure, we find suggestive evidence that banks exploited prospective borrowers' incomplete information to sell them their own loan products in lieu of a ZIGL.

Keywords: household finance, home energy retrofit, green loan, energy efficiency.
JEL classification: G51, Q48, Q55, Q58.

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1 Introduction

Improving energy efficiency is celebrated as a key strategy to mitigating climate change. This is especially the case in the building sector, which contributes 31% of global CO_2 emissions, 70% of which stem from housing (IPCC, 2022). With unit costs ranging from several thousands to several tens of thousands dollars, comprehensive home energy retrofits are challenging to undertake and often require credit. In France, for instance, 20% to 30% of the households undertaking a retrofit take a loan, and this share exceeds 40% in the case of deep retrofits (ADEME, 2011, 2018). Subsidized loan programs are increasingly implemented around the world to close the energy efficiency financing gap (Berry, 1984; Guertler et al., 2013), including the KfW's Energy-efficient refurbishment program in Germany, the Property Assessed Clean Energy Financing program (PACE) in the United States (Rose and Wei, 2020) and a zero-interest green loan (hereafter ZIGL) program called *Éco-Prêt à Taux Zéro* in France.[1]

From a public economics perspective, the rationale for subsidized loans for home energy retrofits is two-fold. On the one hand, they can be seen as an energy efficiency subsidy – perhaps the most widespread Pigovian instrument for internalizing energy-use externalities (Kerr and Winskel, 2020). In the specific case of ZIGLs, the implicit benefit is equal to the interests that would be paid on a regular loan financing the same investment. Participation in the program is therefore expected to move together with the market interest rate. On the other hand, subsidized loans are also a policy remedy to information asymmetries excluding risky borrowers from credit markets (Stiglitz and Weiss, 1981), which tends to mostly affect low-income households (Zinman, 2015). This two-fold rationale therefore raises important questions: How does the performance of subsidized loan programs for home energy retrofits vary over time? Are they effective at expanding access to credit for low-income households?

In this paper, we provide the first causal evaluation of a subsidized loan program for home energy retrofits. We focus on the French *Éco-Prêt à Taux Zéro*, which is a good candidate for studying the research questions above. First, since its implementation in 2009, the program has been operating in a steadily declining interest rate environment. In this context, after a steady ramp-up in the first two years, the number of loans plummeted in 2011 and remained below 40,000 thereafter – an order of magnitude below the 400,000 annual ZIGLs expected by the government. The question arises whether this fall was due to the macroeconomic situation or some more specific supply and demand factors. Second, despite imposing restrictions on the type of works that can be conducted and the type of building in which they can be conducted, the program allows eligible households to apply without income restriction. This feature suggests that credit rationing was not a central concern in the government's motivation for the program, rather rooted in Pigovian concerns. The question still arises whether the program benefited most those households most in need of financing.

[1] Another program worth mentioning is the 'Green Deal' scheme launched in the United Kingdom in January 2013. A pay-as-you-go mechanism with fairly high interest rates (typically 8%), this program was inherently less appealing than the subsidized counterparts mentioned above. With only 6,000 loans issued every year, it was deemed a 'failure' by the House of Commons and terminated in July 2015 (Rosenow and Eyre, 2016).

To examine these questions, we proceed in two steps – first carefully eliciting the dynamic effects of the program then investigating candidate drivers in a more exploratory way. The first stage of our analysis estimates the causal impact of eligibility to the program on three margins – the extensive margin of retrofit investment, the intensive margin, and the induced energy savings. Specifically, we exploit a restriction of eligibility to the buildings built before 1990 to implement an event-study design on a panel dataset of nearly 10,000 French households surveyed from 2005 to 2013. Importantly, the pre-1990 eligibility restriction is unique to the ZIGL program, thus allowing us to isolate its impact. In addition to using household fixed effects, we use time-varying household controls to account for imbalances between the eligible and non-eligible groups and year fixed effects to account for the macroeconomic environment.

We find the impact to be significant, yet short-lived, on the extensive margin. ZIGL eligibility significantly increases the estimated probability of renovation by 3-4 percentage points (p.p.) in the first two years, equivalent to a 20-22% increase for the eligible group. The effect is lower, and no longer statistically significant, in subsequent years. Heterogeneity analysis points to stronger effects among low-income homeowners (11 p.p.). Intensive margin estimates confirm the non-persistence of the effect, however with a lower magnitude. Using two different metrics, we find a €127-175 increase in the amount spent on average on renovation, equivalent to a 3-5% increase for the eligible group, and a 36% increase in the number of renovation actions taken. As for induced energy savings, after restricting our attention to electricity due to measurement issues, we find a 2.7% consumption reduction in 2009, equivalent to 154 kWh, or €18. Here again, the effect is most pronounced among low-income homeowners (11% reduction).

Robustness checks focusing on the more pronounced extensive-margin results confirm our key findings. In placebo event studies, we find no effect of falsified eligibility criteria based on alternative cut-off construction years, except for year 1949. Re-running the baseline regression without the pre-1949 homes confirms their role is significant, yet not crucial. In an alternative event-study regression using propensity score weighting to rebalance the eligible and non-eligible groups, we find remarkably similar effects – even stronger in one of the specifications.

From an evaluation perspective, our estimates lead to a leverage effect of 1.3 in 2009 and 1.8 in 2010, but below 1 (based on non-significant estimates) in subsequent years. In other words, each euro granted by the government to a bank for issuing a ZIGL induced up to 1.8 euro additional spending on the extensive and intensive margins combined in 'high' times, but had hardly any detectable effect in 'low' times. More tentatively, given several measurement issues, the induced investment saved electricity at a lifetime discounted cost of about €55/MWh and carbon dioxide at about €700/tCO_2. These estimates, however, would arguably be significantly lower if we could include other heating fuels in that part of the analysis. Interestingly, when effective, the program benefited most the category of homeowners that are otherwise the most subject to credit rationing, despite not specifically targeting them.

In the second stage of the analysis, we set out to sort out the causes of the 2011 drop and the low overall participation. Taking a broad perspective, we examine both the demand and supply sides of the market for household loans.

On the demand side, we leverage a series of questions included in the survey to examine four candidate barriers to ZIGL application. First, the parallel trends we observe between the eligible and non-eligible groups pre-2009 allow us to rule out that homeowners strategically delayed or moved forward planned retrofits in response to the program. Second, we find no evidence of often-invoked debt aversion and financial distress. Third, we find suggestive evidence that the program weakly interfered with CITE, a tax credit program for home energy retrofit. Fourth, we find suggestive evidence that consumer information about the program decreased from 2011 onward.

On the supply side, a potentially important barrier is the opportunity cost incurred by banks if the compensation they receive from the government for issuing a ZIGL is lower than the margin they could earn from their best outside option, which consists of selling a regular consumption loan. Leveraging data on loan origination from Bank of France over the 2012-2018 period, we find support for this hypothesis and quantify the associated opportunity cost. We find that, for a given bank in a given local market, a 1 p.p. increase in the weighted average interest rate it charges on other loans is associated with a 5% decrease in its ZIGL origination.

Confronting this supply-side result with the dynamics of consumer information leads us to the conclusion that, from 2011 onward, banks have engaged in nudging prospective borrowers away from ZIGLs and into their more profitable own loan products. Imperfect information indeed is a necessary condition to enable this mechanism – otherwise, homeowners would never take a costly loan when they are entitled a free one for the same investment. Why this only started in 2011 remains unclear. Interviews with a few key stakeholders suggest that loan applications were tedious for the banks to put together, which created a lot of ex post rejection and dampened their interest in the program from late 2010 onward. It was not until 2019 that banks and consumers alike gained renewed interest – a phenomenon we briefly discuss in the conclusion yet which remains out of the scope of our analysis.

Given the hybrid nature of ZIGLs as a policy instrument, our analysis contributes to the literature on energy efficiency programs on the one hand, subsidized loans on the other, and the literature at their intersection. We elaborate on these three contributions below.

The literature on energy efficiency programs is mainly concerned with evaluating their effectiveness in terms of additional participation and induced energy savings (Metcalf and Hassett, 1999; Boomhower and Davis, 2014; Graff Zivin and Novan, 2016; Fowlie et al., 2018; Giraudet et al., 2018; Christensen et al., 2021). Most of the focus has been on subsidy programs – see Giraudet (2020) and Chlond et al. (2023) for a review. In the French context, several studies have estimated take-up of the tax credit program (CITE), including Nauleau (2014) and Risch (2020) using the same dataset as in this paper, Mauroux (2014) using fiscal data and Chlond et al. (2023) using a cross-sectional survey. In contrast, the analysis of ZIGL programs has been limited to a few studies that do not meet the standards of state-of-the-art evaluation (Berry, 1984; Guertler et al., 2013; Rose and Wei, 2020). We fill this gap by providing the first state-of-the-art evaluation of a ZIGL program. We do so within an empirical framework that is both more credible than that used in existing evaluations of the French CITE program, which do not exploit as exogenous a variation as our eligibility criterion, and more comprehensive, in that none considers three margins of impact as we do. Incidentally, our study generates novel insights, namely that the French ZIGL program has a strong effect on the extensive margin of investment, whereas evaluations of the CITE program find a most pronounced effect on the intensive margin. These findings together suggest that direct subsidies and low-interest loans aptly complement each other to stimulate all investment channels.

The literature on subsidized loans spans several economic sectors. The most studied applications are student loans (Cadena and Keys, 2012) and housing loans (Martins and Villanueva, 2006; Gruber et al., 2021; Labonne and Welter-Nicol, 2017; Gobillon et al., 2022). Here again, the main line of inquiry is about participation in the program, consistently found to be low, either in terms of the fraction of the eligible population taking a loan (Cadena and Keys, 2012) or the absence of a statistically significant effect of the program on investment in the associated asset (Gruber et al., 2021). The effect is found to be more pronounced on the intensive margin, with subsidized loans inducing people to buy more expensive property in Denmark (Gruber et al., 2021) and in France (Labonne and Welter-Nicol, 2017; Gobillon et al., 2022). In other words, low interest rates are capitalized into higher property value. Here again, we find a reversed pattern with the ZIGL program – a stronger effect on the extensive margin. Our additional finding that this is primarily driven by low-income homeowners is noteworthy considering that, unlike most of its subsidized loan counterparts, the program does not specifically target this category of households. It implies that subsidized loan programs may address credit constraints by design, and that income restrictions may not be systematically needed.

Lastly, one key question at the intersection of environmental economics and household finance is to what extent credit constraints contribute to explaining under-investment in energy efficiency – a phenomenon known as the energy efficiency gap (Gillingham et al., 2009; Allcott and Greenstone, 2012; Gerarden et al., 2017). Information asymmetries in credit markets have indeed been pointed out as one of the most understudied barriers to energy efficiency (Berry, 1984; Giraudet, 2020). Our focus on a ZIGL program allows us to assess their significance more effectively than do evaluations of energy efficiency subsidy programs, which do not observe borrowing behavior. Our complementary investigation of mechanisms additionally allows us to pinpoint their origin. On the demand side, debt aversion and financial illiteracy are often invoked in relation to energy efficiency investment (Schleich et al., 2021; Schueftan et al., 2021). We find little evidence of them. On the supply side, banks have been found to offer particularly high interest rates for home energy retrofits (Giraudet et al., 2021b), which suggests they might incur significant opportunity cost upon issuing ZIGLs. We find support for this hypothesis. To lower the opportunity cost, the government could either raise the compensation offered to banks or increase information provision so as to induce prospective borrowers not to accept a costly loan in lieu of a ZIGL. While the cost-effectiveness of these two options remains to be fully assessed, we see the latter as a fairer use of public funds, benefiting the demand side that is the target of the program instead of the supply side.

The remainder of this paper is organized as follows. Section 2 describes the zero-interest green loan program in greater detail. Section 3 presents the dataset and the empirical strategy. Section 4 presents the results, including the heterogeneity effects. Section 5 discusses the robustness checks. Section 6 examines the demand-side drivers of ZIGL activity. Section 7 examines the supply side. Section 8 concludes.

2 The ZIGL program

The French ZIGL program (*Éco-prêt à taux zéro*, or Eco-PTZ) was implemented in April 2009, following a large public consultation that had identified home energy retrofits as a key priority for action. Meanwhile, the banking industry had been severely hit by the 2008 financial crisis. In this context, offering ZIGLs to households was seen as a means to increase public support for residential energy efficiency while providing a stimulus for the recovery of the banking industry. The government therefore had high expectations for the program, targeting 400,000 annual ZIGLs at full operation.

For the 2009-2018 period that is the focus of our analyses,[2] the program granted interest-free loans for investments in comprehensive retrofit works that met certain performance requirements or combined several measures on the building envelope and/or the heating system. The amount eligible for interest discharge was capped at €30,000 and the repayment period at 15 years. Homeowners could apply without income restriction. Crucially for our analysis, eligibility to the program was restricted to housing units built before 1990.

The loans have been issued by government-approved credit institutions – hereafter the banks. In return for each ZIGL issued, the banks have been granted a tax credit on their corporate taxes, the rate of which depended on the interest rate on government bonds plus a fixed spread of 1.35 percentage points during the period covered in our analysis. The program has been part of a rich portfolio of incentive programs for home energy retrofits in France, chiefly including *crédit d'impôt pour la transition énergétique* (hereafter CITE), an income tax credit program introduced in 2005 (Nauleau, 2014; Mauroux, 2014; Risch, 2020).[3] Generally speaking, households have been allowed to benefit from all programs to finance the same investment. The rules for jointly benefiting from the ZIGL and the CITE programs have however changed over time – an issue we will go back to in Section 6.

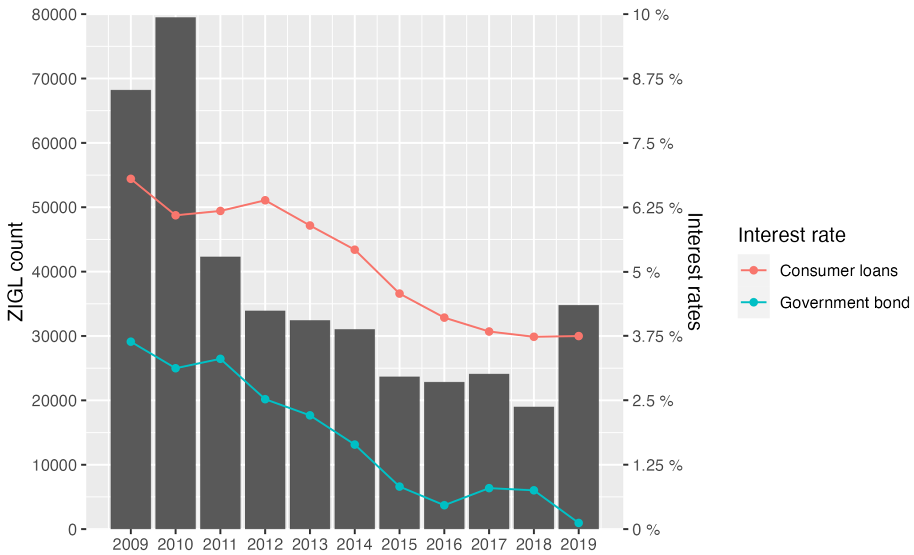
The dynamics of ZIGL provision is depicted in Figure 1, together with the rate on government bonds and the average interest rate charged by banks on consumer loans.[4] After steadily increasing in the first two years, up to 79,508 ZIGLs in 2010, the number of loans fell

[2] Important changes have occurred since 2019, which we briefly discuss in the Conclusion section.

[3] Other programs include a reduced value-added tax program introduced in 1999 (Carbonnier, 2007), subsidies to low-income households since 2010 (*Aides de l'ANAH*), and utility-sponsored subsidies since 2006, also known as the French white certificate program (Giraudet et al., 2012). See Giraudet et al. (2021a) and Chlond et al. (2023) for a comparative analysis of these policies.

[4] The program data are provided by *Société de gestion foncière et de garantie de l'accession à la propriété* (SGFGAS). The dataset consists of the exhaustive record of ZIGL successful applications, described through 200 variables. A non-exhaustive version of the dataset is publicly available here: https://www2.sgfgas.fr/statistiques.

Figure 1: ZIGL provision and trends in market interest rates



Notes: The consumer loan series depicts average market interest rate. The government bond series depicts the 10-year constant maturity rate (*Taux de l'Écheance Constante*) for French government bonds. Data sources: program administrator (SGFGAS) for ZIGL, Bank of France's Webstat platform for interest rates.

dramatically to 42,324 in 2011 – an order of magnitude below the government's expectations.[5] It continued to fall thereafter until reaching a historic low of 19,010 in 2018. Table A2 provides further descriptive statistics, pointing in particular to a slight but steady increase in the average loan size (€16,000 to €18,000) and loan duration (8.9 to 10.5 years) over the period.

3 Empirical approach

3.1 Data

Our core analysis relies on the dataset *Maîtrise de l'Energie*, a representative survey carried out by a polling company, TNS-SOFRES, on behalf of the French Energy Management Agency (ADEME) – henceforth the ADEME Survey. The dataset consists of a self-administered panel survey about home energy consumption and energy efficiency improvements, covering the 2000-2013 period. Participation in the survey was incentivized with a customer points system, offering respondents a barbecue set as the largest possible reward. Respondents could then enter and exit the sample on a voluntary basis.

We add several restrictions to the sample. In an attempt to avoid capturing the effect of CITE introduction in 2005 (see Section 2), we exclude the years 2000 to 2004.[6] We

[5] The target was 200,000 by 2010, 240,000 in 2011, 320,000 in 2012 and 400,000 from 2013 onward (see `https://www.planbatimentdurable.developpement-durable.gouv.fr/IMG/pdf/convention_ecoptz_26-02-09-2.pdf`). These projections were not supported by any known ex ante assessment at the time. Recently, however, a micro-simulation assessment uncovered a potential for annual eligible retrofits that remarkably matches the 400,000 mark (Giraudet et al., 2021a). In retrospect, the government's target could therefore be deemed credible, at least assuming full participation of the eligible households.

[6] Introduction of the CEE program in 2006 could be another disturbance. The scale of that program in its early years was however quite small (Giraudet et al., 2012) so it is unlikely to have entailed significant

focus on homeowners, who account for more than 90% of ZIGL applications according to the program data, and thus exclude tenants. In order to harness the panel dimension of the data, we ignore those households that were present in the sample for one period only. These restrictions, along with dropping observations with crucially missing variables, leave us with 9,657 respondents observed for at least 2 periods, hence an unbalanced panel of 45,418 observations with 29% of respondents observed for 2 periods and 10% for the whole 9 periods.

We use three outcome variables to measure renovation works, defined in the questionnaire as "works aiming at reducing your energy consumption or improve your comfort (heating, hot water, isolation, ventilation, etc.)". To investigate the extensive margin of investment, we use a binary variable that takes the value of 1 if the household did renovate in a given year. To capture the intensive margin, we use two variables – the total euro amount spent on renovation and the number of renovation actions taken.

To investigate energy consumption, we use energy expenditure data reported in the survey and divide them by national average energy prices taken from two sources – Enerdata for 2005 and Pegase for 2006-2013.[7] The energy expenditure data are however subject to two measurement issues. First, they are self-reported, and, while 60% of the participants declare to have checked their electricity bill upon responding, only 25% did so for natural gas, 11% for fuel oil, and even fewer for fuel wood and coal. Second, the data are incoherent in 2007, with all fuel bills experiencing a dramatic drop. The phenomenon, illustrated in Figure A4 for electricity, cannot be explained by any macroeconomic factor. Rather, it is an artifact due to a surge in null spending reported that year in the data. To address these measurement issues, we take a conservative approach by restricting our analysis to electricity and dropping observations for 2007. After further removing null electricity spending (3% of the subsample) and trimming the top 0.1% outliers (22 observations), we are left with 22,672 observations. The distribution of electricity consumption in the resulting sub-sample is reported in Figure A5.

We use information on the year of dwelling construction to identify a household's eligibility to the ZIGL program. This piece of information is reported as a categorical variable with the following cutoff years: 1949, 1975, 1982, 1989 and a moving one defined as the "year before [the survey year]". We consider the first four categories – i.e., those dwellings built in 1988 or before – as eligible for ZIGL. Note that our eligibility measure entails a 1-year inaccuracy (1989 instead of 1990), which we think is small enough not to substantially bias our results.

We use a range of control variables, most of them provided as categorical variables. This includes the age of the household head, their occupation, income, the surface area of their house, the type of heating system and the fuel they use, the type of settlement (proxied by a population size indicator) and the region of France. These variables are described in Table A1. The income categories – an important control in our estimation – are not reported with stable cut-offs across years. We consolidate as much as possible the different

interactions with the ZIGL program.

[7] We thus ignore variation in household energy prices across France, which is very limited anyway.

categories, without eliminating all time inconsistencies – some overlap remains between the [€19,000, €23,000] and [€22,800, €27,600] categories, which we think only implies negligible measurement error. Lastly, we impute a few missing values – 1,792 observations for income (4% of the sample) and 1,357 for surface area (3% of the sample) – through an ordered logit procedure relying on a chained equations algorithm (van Buuren and Groothuis-Oudshoorn, 2011).

Summary statistics are presented in Table A3 for the years 2008 (one year before the introduction of the program) and 2013 (four years after). The share of renovating households is 17% in 2008 and 15% in 2013. A vast majority of the households (around 80% in both years) are eligible to the program. Most people live in single-family houses and one quarter live in multi-family units. Households are rather evenly distributed across income categories, the most frequent one being below €19,000 in 2008 and [€27,200-€36,600] in 2013 (both with 23%). Finally, most respondents (around 40%) heat their dwelling with natural gas, using individual heating systems. For the reduced sample with reliable electricity spending data, the average consumption is 6,270 kWh in 2008 and 5,590 kWh in 2013.

3.2 Empirical strategy

We implement an event-study strategy to identify the causal impact of having access to a ZIGL on three margins – the probability of undertaking renovation works, their amount, and the induced energy savings. We first use the eligibility criteria associated with the age of the house to identify a treatment and a control group – respectively, those units built before and after 1990. We then compare renovation outcomes for the two groups for different years before and after implementation of the program in 2009. We use the sampling weights built by the data provider to increase the representativeness of our results for the population of French homeowners.

We estimate the following regression model:

$$R_{i,t} = \alpha \text{ Eligible}_{i,t} + \sum_{t \neq 2008} \beta_t \left(\text{Eligible}_{i,t} \times \tau_t\right) + X'_{i,t}\gamma + \tau_t + \mu_i + \epsilon_{i,t} \tag{1}$$

where $R_{i,t}$ is our outcome variable, $\text{Eligible}_{i,t}$ is equal to one if the housing was built before 1990, $X_{i,t}$ is a vector of time-varying controls, τ_t are time dummies and $\text{Eligible}_{i,t} \times \tau_t$ is the interaction of the treatment variable with time dummies. In our preferred specifications, we further include respondent fixed effects μ_i. Our parameters of interest are the β_t, representing the impact of being eligible to the program at every point in time. We therefore estimate the impact of the intention to treat rather than the direct impact of the program.[8]

We use different models to estimate the different margins of renovation $R_{i,t}$. The extensive margin of investment, captured by a binary indicator, is estimated through a Linear Probability Model. The intensive margin is captured by two different variables – the number

[8]To estimate the treatment effect, we would need to observe participation in the program and instrument it, for instance with our event-study strategy. However, with only 160 declared ZIGL beneficiaries out of over 4,000 renovations reported in 2009-2013, participation is too limited in our sample to yield statistical power in an instrumental-variable strategy. The low variability of the treatment causes a weak instrument problem (F-statistic of 4.34), preventing us from estimating the treatment on the treated effect.

of renovation actions, estimated through a Poisson model, and the euro amount spent on renovation, estimated through a linear regression model. The energy savings are estimated through a Linear Probability Model. In all regressions, we cluster standard errors at the household level.

Vector $X_{i,t}$ includes the covariates listed in Table A1. In addition, we control for whether a household did renovate in the past through a dummy variable equal to 1 if the household undertook at least one renovation in the past 9 years. The 9-year window is given by the interval between the initial year of our dataset – 2000 – and ZIGL introduction – 2009. For the majority of our sample that is not observed for the full nine years, we assume households did not renovate in the missing years.

Specification (1) allows us to test the hypothesis of parallel trends between the two groups before implementation of the program. In Figure 2, we display the evolution of the renovation rate for each group. While the share of renovating households remains constant at an average of 10% for the control group, it surges for the eligible group right after the program was implemented. The trends are parallel before 2009 and only diverge thereafter, which suggests that our control group is adequate, at least for the investment decision. We test the parallel trends assumption more formally in the different regressions below.

Figure 2: Evolution of renovation rates by treatment group, 2005-2013

Notes: The blue and red lines plot the share of households who renovate in a given year, by treatment status. Survey weights are applied to mean calculation. The black vertical line represents the year before implementation of the ZIGL program. Data source: ADEME Survey.

Next, we run a balancing test to compare the demographic and housing characteristics of the two groups. Table A4 reports the average values and standard deviation for the key control variables, as well as the t-stat and p-value of a means differences t-test. We observe that most variables statistically differ between the two groups. While this is not a challenge for our identification strategy, which only relies on the common-trend assumption, it suggests that these differences are important to control for. We therefore do so in our main specification and additionally perform inverse probability weighting in robustness checks.

Finally, we also run the simple two-period difference-in-differences strategy to evaluate the average effect over the post-implementation period. In this regression, we interact the variable of interest with Post_t, an indicator of the post-2008 period.

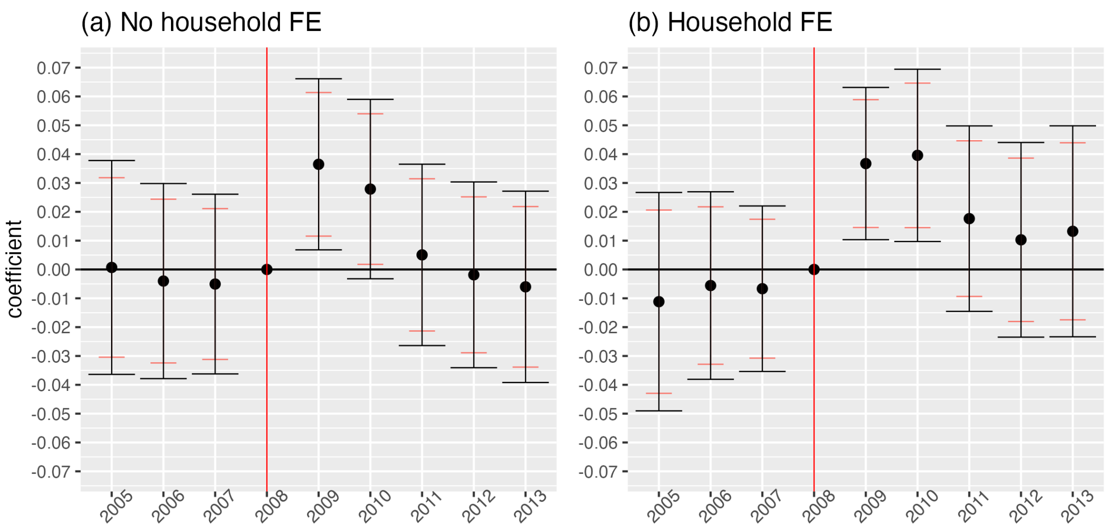
$$R_{i,t} = \alpha \ \text{Eligible}_{i,t} + \beta \ (\text{Eligible}_{i,t} \times \text{Post}_t) + X'_{i,t}\gamma + \tau_t + \mu_i + \epsilon_{i,t} \tag{2}$$

4 Baseline results

4.1 Extensive margin

Figure 3 presents the event-study coefficients for the main regression (Equation 1), with and without household fixed effects, taking 2008 as the baseline year. The results confirm the graphical evidence of Figure 2 in a more comprehensive specification that includes controls and fixed effects.

Figure 3: Effects of eligibility on the renovation decision



Notes: Estimates for the event study of Equation (1), with the renovation dummy as the dependent variable. Confidence intervals: 95% in black, 90% in red. Specification: (a) with household controls (both constant and time-varying), but no household FE; (b) with household FE and time-varying controls. Time FE used in both specifications. Standard errors clustered at the household level. See Table A1 for description of controls and Table A5 for regression results. Data source: ADEME survey.

No coefficient is found to be significant between 2005 and 2007, which confirms that the two groups followed parallel trends before the treatment was introduced. The rate of renovation significantly increases, for the eligible relative to non-eligible, in the two years following implementation, after which the effect is again non-significant. In the specification with controls, without fixed effects, the effect amounts to 3.7 percentage points (p.p.) in 2009 (5% statistical significance) and 2.8 p.p. in 2010 (10 % significance). With respondent fixed effects and time-varying controls, the effect is the same in 2009 (3.7 p.p., 1% significance) but more pronounced in 2010 (4 p.p., 1% significance). After 2010, all statistically significant differences disappear, meaning that the gap in renovation rates between eligible and non-eligible households is no longer different from that of the pre-ZIGL period. Altogether, these effects are economically substantial, amounting to a 20-22% increase in the renovation rate of the eligible group.

The dynamics elicited here are consistent with the program statistics reported in Figure 1 and Table A2. After steadily increasing in the first two years, the ZIGL count sharply dropped to 42,324 in 2011 and then continued to gradually decrease until it reached a historic 19,010 low in 2018. The ZIGLs taken after 2011 might have had an effect, but it is apparently too small for us to detect it. Even though our data only go as far as 2013, the continued decline in participation observed until at least 2018 makes it highly likely that the lack of a significant effect we find from 2011 onward extends to the 2013-2018 period as well. Finally, both a visual inspection of the co-movement between market interest rates and ZIGL participation and our use of time fixed effects in the regressions allow us to rule out the macroeconomic environment as an important determinant of the high-and-low profile.

The identified effect is confirmed in the two-period regression (Equation 2) – albeit smaller, as is expected since this specification averages the effect over the entire post-implementation period (Table A6). The estimated coefficient is 3.2 p.p. (significant at 1%) with household fixed-effects and 1.4 p.p. (non-significant) without fixed-effects. Table A6 also discloses the coefficients associated with the control variables. One noticeable outcome is that, in the fixed-effects model, households appear less likely to renovate if they have renovated in the past, as should be expected.

4.2 Heterogeneity by household income

The main rationale behind subsidized loan programs is to grant low-income households access to credit. In practice, this motivation was not salient in pre-implementation policy discourses, which placed more emphasis on the Pigovian rationale, and, to a lesser extent, on the recovery of the banking industry. The fact that eligibility criteria did not include income ceilings somewhat confirmed this relative lack of concern for mitigating credit rationing. Heterogeneous households, however, might have benefited from the program in different ways. To check whether this was the case, we perform a heterogeneity test by homeowner income.

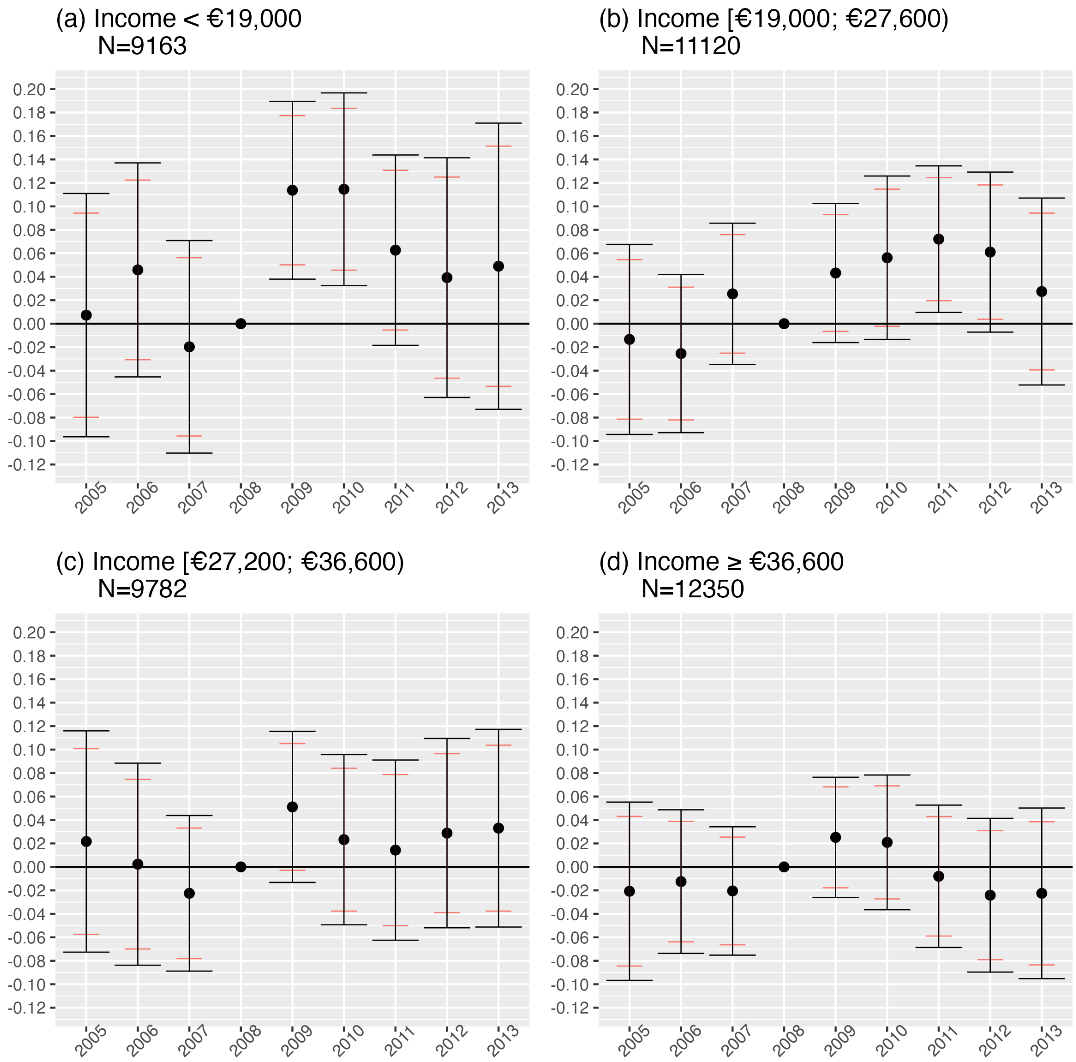
In order to increase statistical power and deal with groups of similar sizes, we group the six available income categories into four: below €19,000, €19,000 to €27,600, €27,200 to €36,600 and above €36,600.[9] We then run the event-study regression (1) on the four sub-samples. The results are presented in Figure 4 and Table A7. Low-income homeowners by far experience the strongest effect of ZIGL eligibility on the decision to renovate – a 11.4 p.p. increase in 2009 and 11.5 p.p. in 2010, both significant at 1%. The lower-middle income group also experiences a smaller, slightly delayed and statistically significant effect – a 7.2 p.p. increase in 2011 (5% significance) and 6.1 p.p. in 2012 (10% significance). For the remaining two groups of wealthier homeowners, we fail to detect any statistically significant effect.[10] The results are confirmed in a triple difference-in-differences approach applied to

[9]As pointed out in Section 3.1, the [€22,800, €27,600] and [€27,200, €36,600] slightly overlap.

[10]The split-sample analysis is mildly sensitive to the income imputation procedure described in Section 3.1. The most conservative approach of excluding all imputed income values yields a 5.8 p.p. effect (5% significance) for the lowest income group in 2009 and no statistically significant effect in 2010.

the 2005-2010 subsample, in which we interact the four income categories with the eligibility variable and a $Post_t$ variable taking the value of 1 for 2009-2010. As reported in Table A8, the lowest income group is 6.3 p.p. more likely to renovate (10% significance) than the next income group, which in turn is 4.4 p.p. more likely to renovate after 2009 (10% significance).[11]

Figure 4: Heterogeneous effects by income group



Notes: Estimates for the event study of Equation (1), for four income groups. Confidence intervals: 95% in black, 90% in red. Household fixed effects, time fixed effects and time-varying controls (controlling by income bins only in (b) and (d)) used in all regressions. Standard errors clustered at the household level. See Table A7 for detailed results and Table A1 for a description of controls. Data source: ADEME survey.

These results lead us to the important conclusion that credit constraints are significant among low-income homeowners, and that the ZIGL program was effective at lifting them – perhaps in an unintended way, since eligibility was not restricted by any income ceiling. One possible mechanism is that low-income homeowners responded to lower interest rates by substituting professional work for do-it-yourself (DIY) or undeclared work, as was documented

[11]The triple difference result is not sensitive to the imputation of income. If anything, the exclusion of imputed values makes the triple difference larger for the lowest income group.

in Luxembourg (Lindner et al., 2022). We provide supporting evidence for this hypothesis at the end of the next subsection.

4.3 Heterogeneity by type of renovation action

We now examine the differential effect of eligibility across different dimensions of renovation actions. We start with the technical parts of a renovation project, which we group into two broad categories (from 32 provided in the dataset) – reduction in heat leakage[12] and upgrades in the heating, water heating or ventilation systems. We then construct a binary variable for each category (equal to 1 if at least one action from that category was taken) and use it as our outcome variable in the event-study regression (1). As displayed in Figure A1, we find heat leakage results to be similar to our baseline results – 3.2 to 3.6 p.p. in 2009 and 3.5 to 4.0 p.p. in 2010, all statistically significant at 5% (panel a) – while heating, water heating or ventilation systems results are non-significant (panel b). This suggests that our baseline effect on aggregate renovation is mainly driven by an uptake of heat leakage-reducing actions.

We then turn to professional versus DIY or undeclared work, which is documented in the dataset as a binary variable. We find a relative increase in professional renovations, especially in 2010 (panel c), suggesting that eligibility to ZIGL has induced households to substitute professional work for more informal work. In a split-sample analysis, we find that this increase is most significant for low-income households (see Figure A2), thus lending support to the hypothesis made in the previous section that substitution was an important margin of adjustment for this category of households.

4.4 Intensive margin

Renovation expenditure is a natural candidate to capture the intensive margin of investment. This variable is reported in the dataset as a categorical variable with fairly large intervals on the right tail of the distribution. Among the 11 categories, the top category – €6,098 or larger – is particularly imprecise, considering that the median amount for ZIGL-backed projects is €17,355. To address these measurement issues, we complement the renovation expenditure analysis with another measure of the intensive margin – the number of renovation actions, equal to 1.5 on average for the eligible group in 2008. This variable is indeed likely to vary in response to ZIGL eligibility, since combining several actions is a requirement of the program.

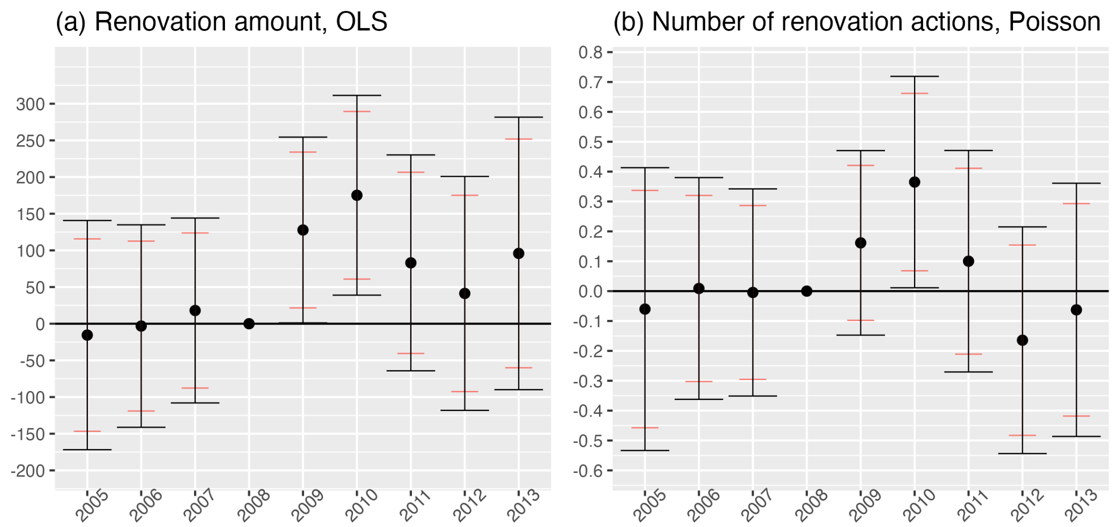
Our renovation amount variable is a continuous variable taking the central value of the interval it belongs to, €6,098 as the highest value, and zero in the absence of a renovation. We estimate Equation (1) with an ordinary least-square (OLS) linear regression model, best suited for continuous variables in event-studies (although the share of zeroes in the outcome variable is high). As reported in Figure 5 (panel a), we find positive effects in 2009 in 2010 (both significant at 5%). The magnitude of the effects – €127 additional spending in 2009 and €175 in 2010 – is rather modest, representing 3.3% and 4.6% of the €3,816 spent on average by the eligible group in 2008.

[12] This category includes: Internal wall insulation; external wall insulation; roof, attic, floor or ceiling insulation; duct sealing; window insulation; double glazing window; window shutter installation or replacement.

Turning to the count of actions, we set it to zero in the absence of a renovation in a given year, use a Poisson regression to address the non-normality of the count and include the same set of explanatory variables as in the baseline regressions, with fixed effects. Figure 5 (panel b) displays the estimated percentage changes in the number of renovation actions due to ZIGL eligibility. Here again, we find a positive, statistically significant effect in 2010, equivalent to a 36% increase in the number of renovation actions. As reported in Table A9 of the regression results, the Poisson regression with respondent fixed effects excludes around 55% of observations – these are from respondents who never renovate and for whom the Poisson estimate is not identified.

Taking both metrics together, the effect of ZIGL eligibility was therefore weaker, and more limited in time, on the intensive margin of investment than it was on the extensive one.

Figure 5: Effect of eligibility on the intensive margins of renovation



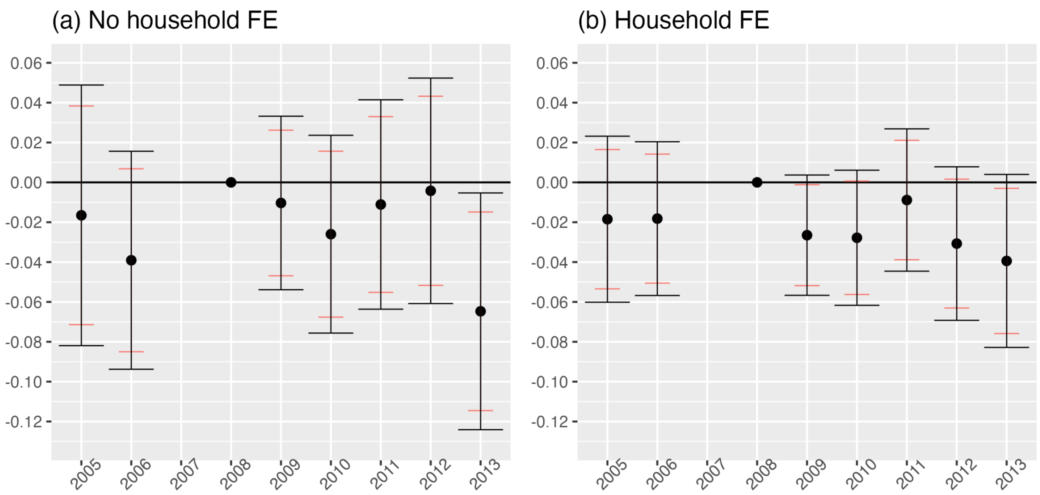
Notes: Event-study estimates of renovation amounts and the number of renovation actions. Renovation amount: OLS regression with household fixed effects and time-varying controls. Number of actions: quasi-Poisson regression with the same explanatory variables. Survey weights are applied. See results in Table A10, see controls description in Table A1. Standard errors clustered at the household level. Data source: ADEME Survey.

4.5 Electricity consumption

To estimate the energy savings generated by eligibility-induced renovation, we focus on the electricity consumption variable described in Section 3.1 and use its log transformation to account for the skweness of its distribution. Ignoring year 2007 for the reasons discussed above, we find no pre-trends, either with or without fixed effects, as illustrated in Figure 6. In the specification without fixed effects, we find a 6.5% reduction of electricity consumption in 2013 only (5% significance). With household fixed effects, the estimated impact is -2.7% (10% significance) in 2009 and -3.9% (10% significance) in 2013. With an average electricity consumption of 5,693 kWh for eligible households in 2008, the -2.7% effect estimated in 2009 translates into around 154 kWh, or €18, saved annually.[13]

The weak significance of these effects and their absence in 2010-2012 is likely due to low statistical power, the sample being half the size of that used to study investment. Moreover, their magnitude is likely smaller than the one we would find for energy consumption as a whole, since only 31% of households use electricity for heating – the use specifically targeted by renovation – while virtually all of them use it for other uses unaffected by renovation.

Figure 6: Effect of eligibility on electricity consumption



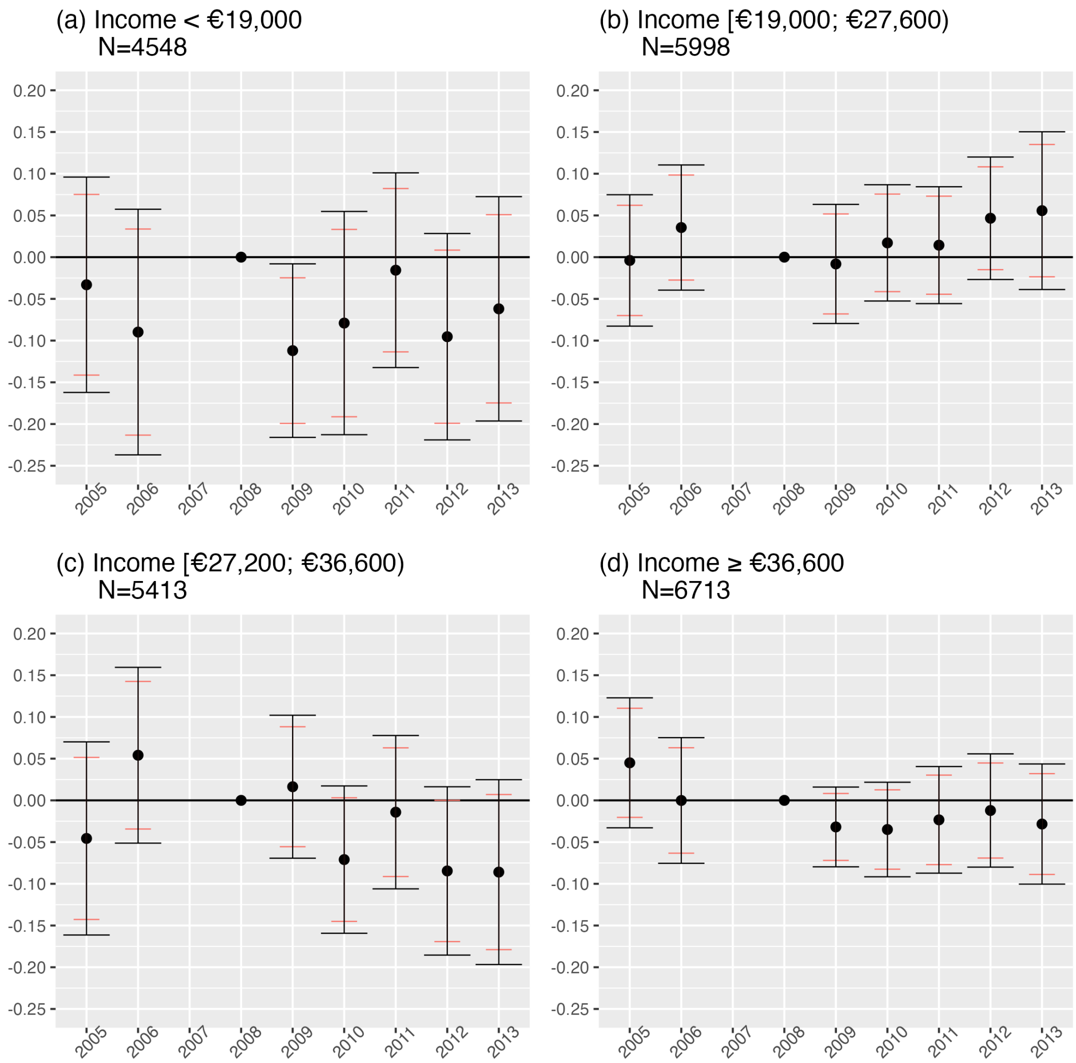
Notes: Estimates for the event study of Equation (1), log of electricity consumption as the dependent variable. Confidence intervals: 95% in black, 90% in red. Specification: (a) with household controls (both constant and time-varying), but no household FE; (b) with household FE and time-varying controls. Time FE used in both specifications. Standard errors clustered at the household level. See Table A1 for description of controls and Table A5 for regression results. Data sources: ADEME survey for electricity spending and controls, Enerdata and Pegase for electricity prices.

Turning to heterogeneity analysis, we divide the sample as before into four income categories and repeat the analysis for the obtained sub-samples. The results, reported in Figure 7, are only significant for the lowest income group, with a 11% ZIGL-induced reduction in electricity consumption in 2009 (5% significance). Albeit sizeable, the effect is short-lived among low-income households – a result that echoes (Peñasco and Anadón, 2023)'s recent findings in England and Wales. One reason commonly advanced to explain such a rebound is that low-income households are now enjoying comfort they were holding back prior to investment (Aydin et al., 2017).

Our analysis of energy savings, while more restrictive and lesser-powered, exhibits the same timing and heterogeneity as that of investment. It therefore confirms our main result, at least qualitatively.

[13]Using the same dataset, Glachant and Blaise (2019) find a 0.64% reduction in energy use for each €1,000 of retrofit investment. Their analysis focuses on energy savings from all types of investment, regardless of any policy stimulus. Incidentally, their sample is broader – in particular, it is not restricted to homeowners nor electricity use. In turn, their identification does not rely on such an exogenous variation as our eligibility criterion.

Figure 7: Electricity consumption effect: heterogeneity by income



Notes: Estimates for the event study of Equation (1) with log of electricity consumption, for four income groups. Confidence intervals: 95% in black, 90% in red. Household fixed effects, time fixed effects and time-varying controls (controlling by income bins only in (b) and (d)) used in all regressions. Standard errors clustered at the household level. See Table A7 for detailed results and Table A1 for a description of controls. Data sources: ADEME survey for electricity spending and controls, Enerdata and Pegase for electricity prices.

4.6 Leverage and cost-effectiveness

Our estimated effects on investment can be compared to some measure of public cost to compute leverage – the extra amount of private investment induced by one euro of public spending on ZIGLs. To do so, we start with computing the total effect of ZIGL eligibility as the sum of our extensive and intensive margin estimates, both expressed in percentage increase of the 2008 baseline for the eligible group.[14] We then divide this term by the euro

[14]This simple sum implicitly assumes that additional participants match the pre-2008 investment amount of non-additional participants. Making the alternative assumption that they match the post-2008 amount would add a product term (% intensive * % extensive) at the numerator. This term is negligible given our estimates.

amount the bank receives from the government on each loan (as tax credit – see how it is calculated in Section 2), here again expressed in percentage of the underlying investment, as reported in the program data. The approach is summarized in Equation 3. We apply this formula to our yearly event-study coefficients.

$$\text{Leverage}_t = \frac{\%\text{ extensive margin effect}_t + \%\text{ intensive margin effect}_t}{\%\text{ rate of public cost}_t} \tag{3}$$

The results, displayed in Table 1 (together with the calculation inputs), suggest that leverage was in the 1.3-1.8 range in the 'high' times when the program was found to be effective, but rather in the 0.5-0.8 range in subsequent 'low' times, although this estimate is less reliable since impact was no longer significant. In other words, every euro granted to banks for issuing ZIGLs induced at best a €1.8 increase in retrofit investment.

To put these numbers in perspective, the 'high-times' estimates fall within the same range as those estimated for other loan programs and energy efficiency subsidy programs in micro-simulation works. Gobillon and le Blanc (2005) for instance, found a 1.1-1.3 leverage for the *Prêt à taux zéro* (PTZ) program, a zero-interest loan program for first-time home purchase targeting low- and middle-income households in France. As for energy efficiency subsidies, Giraudet et al. (2021a) exhibit a leverage of 1.2-1.5 for the ZIGL program in micro-simulation work, against 0.9-1.1 for other incentive programs – reduced VAT, CITE and white certificates (see Section 2). The authors attribute ZIGL's higher leverage to the stronger performance requirements the program includes, which imposes higher spending on participants.

Our estimates lead us to the conclusion that, in its 'prime,' the program performed pretty much as well as predicted in micro-simulation works. However different from expectations the total number of participants was, at least public investment induced more-than-proportional private investment, suggesting public money – €215 million in 2010 – was spent wisely. In subsequent times, however, with hardly any detectable effect and leverage below 1, the social benefit from the program became questionable.

Next, we can compare investment figures with energy consumption figures to derive cost-effectiveness estimates. This part of the evaluation is however less internally-consistent and more tentative, due to the restriction of our analysis to electricity use and the other measurement issues discussed in Section 3.1. Focusing on the intensive margin of investment, the €129 additional spending estimated in 2009 induced 154 kWh, or €18, annual savings. These figures imply a private payback time of 129/18 = 7.2 years (undiscounted). In terms of private cost-effectiveness, they imply a €43 to €69 cost per lifetime discounted megawatt-hour (MWh) savings, assuming a 4% discount rate and a conventional lifetime of 16 to 35 years. These private "negawatt-hour" costs in turn translate into social carbon abatement costs of €538/tCO_2 to €863/tCO_2, assuming a 80gCO_2/kWh carbon content of electricity in France, where nuclear power contributes over 70% of electricity generation. Lastly, dividing these indicators by the 1.3 leverage estimated for 2009 implies that public intervention induced private benefits at a cost of €33-55/MWh and public benefits at a cost of €414-664/tCO_2.

While the private negawatt-hour costs compare favorably with retail electricity prices (€115/MWh in 2009), the social cost values are significantly higher than the €250/tCO_2

estimated for France in 2030 (France Strategie, 2019). However, our social cost estimates are likely overestimated, for several reasons. Again, our focus on electricity underestimates renovation-induced energy savings. The low carbon content of electricity in France further undermines the associated carbon abatement. Lastly, our figures are based on average effects. They thus ignore ancillary health benefits specifically accruing to low-income households, who are the main beneficiaries of the program.[15]

Table 1: Leverage calculation

Year	Extensive margin effect	Intensive margin effect	Public cost	Leverage
2009	20.0%***	3.3%**	17.1%	1.4
2010	21.5%***	4.6%**	14.9%	1.8
2011	9.6%	2.2%	16.0%	0.7
2012	5.6%	1.1%	14.3%	0.5
2013	7.2%	2.5%	12.3%	0.8

Notes: The extensive and margin effects are calculated as the coefficients of the event-study regressions, divided by the average value of the associated outcome for eligible households in 2008. Significance codes: ***0.01, **0.05, * 0.1. Public cost estimates are provided by the ZIGL administrators. Leverage is calculated by equation (3). Data sources: ADEME Survey, program data.

5 Robustness checks

The robustness checks focus on the extensive margin of investment, found to entail the most significant effects.

5.1 Placebo regressions

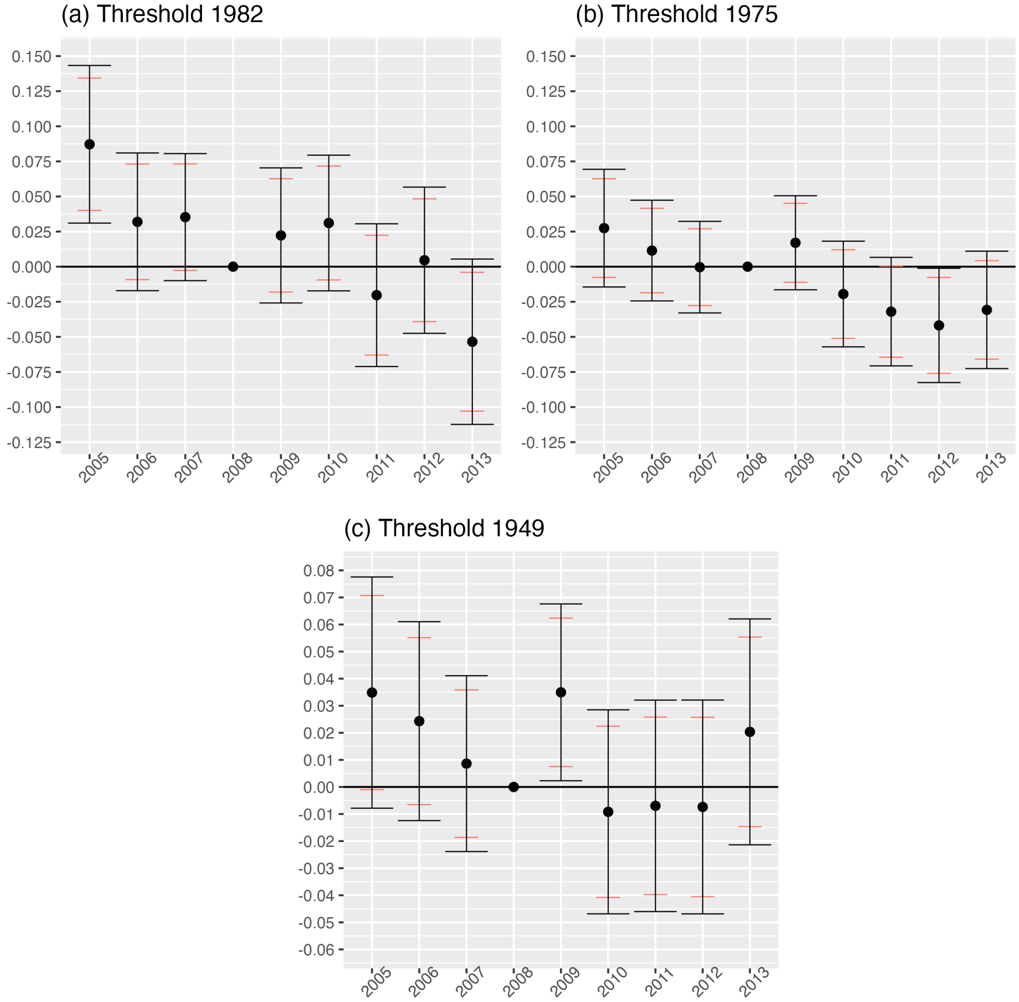
To assess the validity of our treatment variable, we test several fictional measures of eligibility to the program in placebo regressions. The six construction periods in which the data are framed (see Table A1) provide us with three fictional partitions of the treatment and control groups: pre-/post-1982, pre-/post-1975, and pre-/post-1949. In all placebo regressions, we exclude the post-1989 units from the fictional control group in an attempt to avoid capturing the true effect of ZIGL eligibility.

Figure 8 presents the event-study estimates of Equation (1), for each of the three placebo eligibility measures, with household fixed effects.[16] The 1982 eligibility cut-off (panel a) results in significant difference in differences in 2005 (i.e., before the program was implemented) and in 2013. Yet none of the coefficients around the date of implementation are statistically significant, suggesting that the two groups did not follow different renovation trends. The 1975 eligibility cut-off (panel b) results in no significant difference in coefficients, except for 2012, when it is negative and statistically significant at the 10% level. Here again, we observe no different pre-trends in renovation rates. Finally, the 1949 eligibility

[15] A governmental task force recently estimated that upgrading the least energy-efficient dwellings occupied by low-income households yielded €7,500 per dwelling in social benefits, due to reduced care (€400), morbidity (€1,400) and mortality (€5,700) (Domergue et al., 2022).

[16] The results without fixed effects are qualitatively equivalent.

Figure 8: Placebo differences-in-differences, extensive margin



Notes: Placebo tests for the event study of Equation (1) for the binary renovation decision, with 95% (black) and 90% (red) confidence intervals. All regressions done after removing the true control group (year of construction after 1990). Placebo eligibility criterion: (a) houses constructed before 1982; (b) houses constructed before 1975; (c) houses constructed before 1949. All regressions include household FE. Data source: ADEME survey.

cut-off (panel c) results in a statistically significant coefficient for 2009, of the same order of magnitude as in the main result in Figure 3 – around 3.5 p.p.. The effect is not significant in subsequent years. This suggests that the 2009 effect we identify in our main regression might be primarily driven by older houses. In Figure A3, we check whether this is the case by running our main specification with a sample that excludes pre-1949 units. We find that this exclusion preserves our 2010 effect but challenges the 2009 effect. We therefore conclude that the oldest homes may have played an important role in 2009 and a lesser one in 2010.

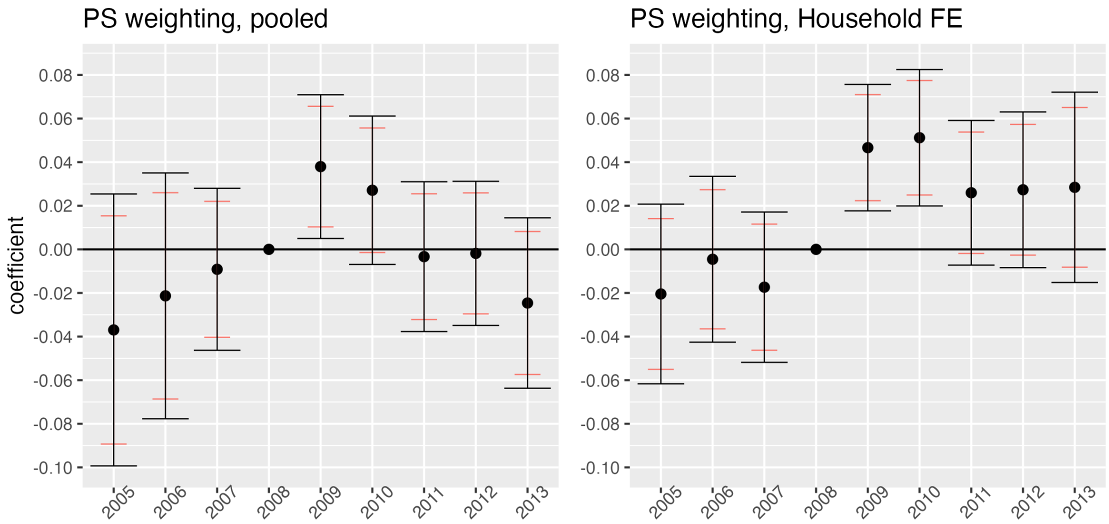
5.2 Event Study with Propensity Score Weighting

As discussed in Section 3.2, the eligible and non-eligible groups differ along several important dimensions, such as household age and income. Such imbalance does not threaten the credibility of our approach as long as the parallel-trends assumption holds. We nevertheless investigate its broader implications, using inverse probability weighting with propensity scores.

We estimate a standard logit model explaining eligibility to ZIGL with the covariates of Table A1 (except region) and use the fitted values as propensity scores. The estimated coefficients are reported in Table A11. Following Hirano and Imbens (2001), we then apply the inverse probability weighting to the data, combined with the survey weights used before. As depicted in Figure A6, all the observations in our sample fall within the common support area, implying they can all be used. To check the effectiveness of the approach, we perform a balancing test with the new weights. The results reported in Table A12 of the Appendix show that half of the variables are now balanced between the two groups in 2008. The largest discrepancies are still observed in relation to age (higher in the eligible group), income (more frequently lowest among the eligible) and heating systems (fuel oil much more frequently used among the eligible). Based on these observations, we keep as matching variables all the covariates included in the baseline regression, except regional dummies.

Figure 9 presents the estimates of regression (1) with the inverse probability weighting based on propensity scores. The 2009 effect increases to 3.8 p.p. (5% significance) and to 4.6 p.p. (1% significance) in the pooled and household fixed effects regressions, from 3.7 p.p. in both specifications without propensity scores. For 2010, the coefficients are 2.7 without fixed effects and 5.1 p.p. with fixed effects, only the latter being significant at 1%. Table A13 in Appendix 5 provides a detailed account of the results. This test shows that accounting for covariates imbalance in a more flexible way generates remarkably similar results to those of the baseline estimation, making the effects even stronger when combined with households fixed effects.

Figure 9: Extensive-margin regression with propensity score weighting



Notes: Estimates for the event study of equation (1) for the binary renovation decision, using inverse probability weighting with propensity scores. 95% (black) and 90% (red) confidence intervals. Specification: (left) with household controls (both constant and time-varying), but no household FE; (right) with household FE and time-varying controls. Time FE used in both specifications. Standard errors clustered at household level. See Table A13 for regression results. Data source: ADEME survey

6 Demand-side mechanisms

Previous analysis has showed that effective participation in the ZIGL program was an order of magnitude below the potential one generated in a micro-simulation exercise (Giraudet et al., 2021a), which in turn was consistent with the French government's expectations. Our empirical analysis complements this static result with a dynamic one, namely that the ZIGL program had a significant, yet short-lived, effect on energy-efficient renovation.

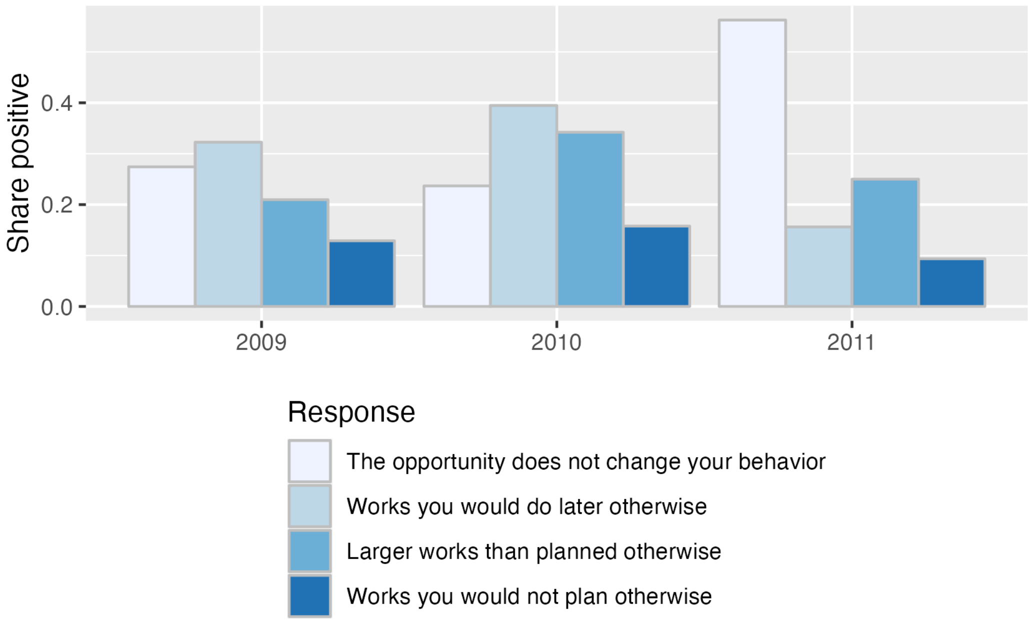
We now discuss candidate mechanisms for both the low level and downward trend of ZIGL participation. We start by discussing a range of demand-side mechanisms – strategic, behavioral, contextual and informational, most of them documented with additional data from the ADEME survey.

6.1 Strategic participation

One natural hypothesis to explain the 2009-2010 peak in participation is that households anticipated the ZIGL program and postponed their renovation works until it came into force. Our baseline event-study design shows that this was not the case, since, prior to 2009, the trends in renovation rates for eligible and non-eligible were parallel. Had the peak been due to strategic participation, renovation rates would have been lower for the eligible group right before the start of the program. We can thus discard strategic participation as a driver of the effect of ZIGL eligibility.

Instead, the 2009-2010 peak is either due to a genuine increase in the demand for renovation or some pulling ahead of renovation works that would have taken place anyway. These

Figure 10: Stated motivation for taking a ZIGL



Notes: Share of approval of a range of statements about the motivation for renovation. Number of responses: 62 in 2009, 38 in 2010, 23 in 2011). Data source: ADEME survey.

two channels can be investigated through additional questions from the ADEME dataset surveying households' motivation for taking a ZIGL. Since ZIGL beneficiaries are very few in the sample (N=160 cumulative respondents over five years), however, the results should be interpreted cautiously.

Figure 10 shows that the most common motivation in 2009 and 2010 is to undertake works that would otherwise have been done later (32% and 40%, respectively). In contrast, the 'newly created demand' of beneficiaries who state that the program was instrumental in their decision represents 12% and in 2009 and 16% in 2010. This suggests that the renovation peak essentially consisted of an acceleration of already planned renovation. It is however unlikely that this moving forward exhausted the full potential for renovation of the French housing stock, which, according to a recent study, was always far larger than the yearly number of works (Giraudet et al., 2021a).

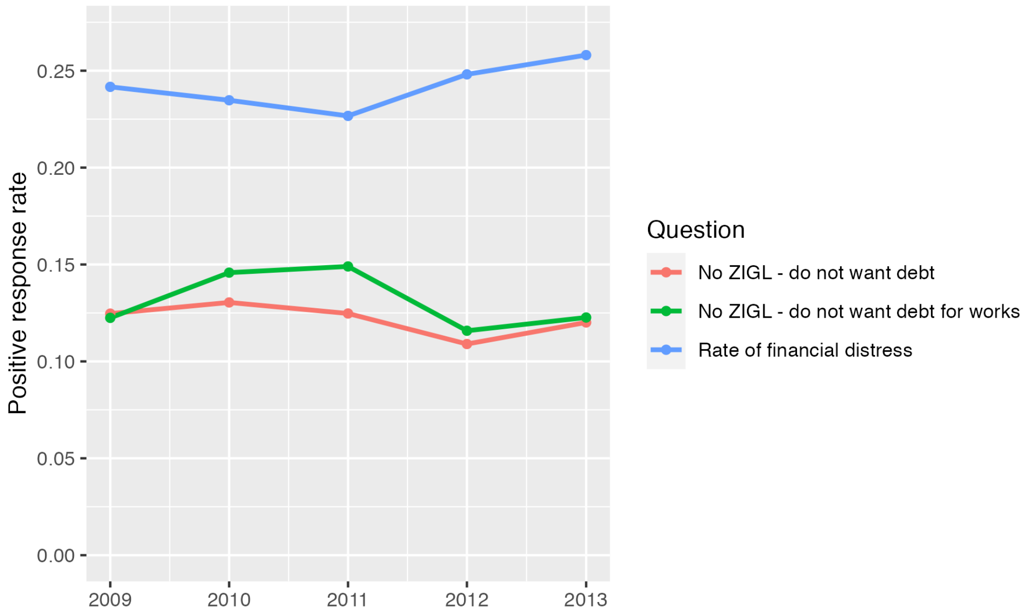
Figure 10 is also informative about the intensive margin of renovation. In 2010, 34% of beneficiaries declare to have undertaken larger works than otherwise planned. In line with our empirical analysis, this share is highest in 2010. We therefore conclude that the ZIGL program had an impact on both the volume and timing of renovation.

Finally, the share of beneficiaries declaring that the program did not change their behavior significantly increases in 2011. This increase coincides with the loss of strength of the eligibility effect we estimated in the previous sections.

6.2 Debt aversion and financial distress

An increasingly discussed explanation for under-investment in energy efficiency is debt aversion and financial illiteracy (Schleich et al., 2021; Blasch et al., 2019).[17] The ADEME survey includes one question that sheds light on the issue. The question inquires about the reasons for not taking a ZIGL (response rate around 50%). As depicted in Figure 11, about 11-15% of respondents invoke not wanting debt, neither in general nor for renovation works. This rate is fairly stable over time. In particular, it does not increase in 2011, when the ZIGL effect ceases to be significant, and even tends to decline thereafter. This suggests that, even if debt aversion cannot be ruled out, it is unlikely to be a major driver of the downward trend in ZIGL participation.

Figure 11: Stated financial distress and debt aversion



Notes: Share of approval of a range of statements: (red) "I did not take a ZIGL because I did not want to be indebted at all"; (green): "I did not take a ZIGL because I did not want to be indebted for renovation works"; (blue): "my current interest payments are too high" or "far too high". Data source: ADEME survey.

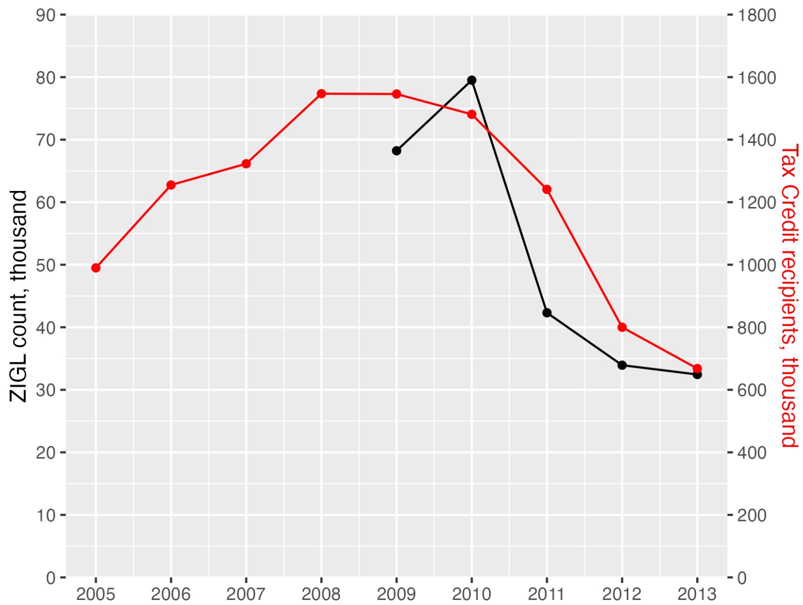
Behavioral explanations apart, one might simply argue that financial distress induced by the adverse macroeconomic context of 2009 dampened the households' willingness to undertake renovation works and/or take a loan. To investigate this, we look at yet another question in the ADEME survey surveying the whole sample about interest payments on their debts (response rate of 70%). The share of respondents declaring high or too high interest payments is depicted with debt aversion responses in Figure 11. The share of financially distressed households starts increasing in 2012, suggesting it might contribute to the decline in ZIGL participation. The relationship between financial distress and debt aversion, however, seems relatively weak.

[17] In a broader perspective, other theories have been advanced to explain low participation in subsidized loan programs, such as self-control against over-spending (Cadena and Keys, 2012) and mental accounting when faced with non-trivial computation problems such as debt (Wonder et al., 2008; Thaler, 1985; Herrmann and Wricke, 1998; Dynarski and Scott-Clayton, 2006).

6.3 Policy interference

As discussed in Section 2, the ZIGL program is part of a rich portfolio of incentives for home energy retrofit. As a general rule, benefits from different programs can be jointly claimed to finance the same investment. However, the rules for combining ZIGL and the tax credit program (CITE) have varied over time. Only those households earning less than €45,000 per year were allowed to jointly benefit from the two programs in 2009 and 2010. The overlap was then forbidden in 2011, before being permitted again in 2012 and 2013 with a €40,000 income ceiling.[18]

Figure 12: Co-evolution of ZIGL (black) and CITE (red) beneficiaries.



Notes: Data source: program administrator (SGFGAS) for ZIGL, Waysand et al. (2017) for CITE.

The CITE program has been the most widespread energy efficiency tool since 2005. In 2009, it benefited 1.5 million households, for a total cost of €2.6 billion (Waysand et al., 2017). One might therefore expect the changing overlapping rules to affect the ZIGL dynamics in important ways. In particular, if the CITE program were a strong driver of the ZIGL program, we would expect a drop in ZIGL participation in 2011 when overlap was forbidden. Our empirical analysis clearly shows that renovation among ZIGL-eligible households did plummet in 2011. Since the program overlap was allowed again in 2012, we could have expected a new ZIGL uptake, yet our analysis fails to uncover a positive counter-shock in 2012. This can be due to the fact participation in the CITE program shrank in 2012, as depicted in Figure 12.

[18] From 2014 to February 2016, it was permitted with differentiated income ceilings (€25,000 for a single person and €35,000 for a couple, plus €7,500 per child) and since March 2016 it is permitted without income restrictions.

6.4 Imperfect information

Imperfect information about the program is yet another natural candidate explanation for low uptake. Table 2 shows that the share of respondents claiming to know about the ZIGL program in the ADEME survey significantly declined from 56-57% in 2009-2010 to 42-44% in 2011-2013. Looking more closely at the sub-sample of renovating households, overall awareness is higher, but the 2011 drop similarly occurs. Finally, looking at the even smaller sub-sample of renovating households who report to have taken a loan, general awareness is higher still and the drop still occurs. In particular, the difference in awareness between 2010 (78%) and 2011 (64%) is statistically significant at the 5% level, despite small sample sizes.

The decline in awareness about the program, observed even among the presumably best informed households, suggests that banks may have reduced their publicity effort about the program. This lower effort in turn could be motivated by a declining interest in issuing ZIGLs. In the next section, we explore whether this is the case and discuss possible motives behind it.

Table 2: Knowledge of ZIGL

Sample	All		Renovators		Renovators with loan	
	Know ZIGL	N	Know ZIGL	N	Know ZIGL	N
2009	57%	5,596	67%	1,117	76%	187
2010	56%	5,139	67%	944	78%	129
2011	44%	4,646	54%	792	64%	122
2012	42%	4,708	50%	739	67%	111
2013	43%	4,295	65%	637	76%	83

7 Supply-side mechanisms

One important aspect of the ZIGL program is its reliance on commercial banks. This is in contrast with the German counterpart program, which relies on a public bank, the KfW. The rationale for relying on commercial banks is to harness their retail network and thus presumably gain better access to prospective lenders. The resulting incentives for banks are however ambiguous. On the one hand, banks may see ZIGLs as a convenient vehicle for cross-selling other types of loans (Basten and Juelsrud, 2022) or build a stronger relationship with their clients (Agarwal et al., 2018). On the other hand, issuing ZIGLs implies significant opportunity cost, as illustrated in Figure 1. The difference between the average consumption loan interest and the 10-year government bond rate exceeded 2 p.p. throughout 2009-2018, a gap not covered by the 1.35 p.p. spread received from the government for compensation. The opportunity cost is all the more significant that banks have been found to charge particularly high interest rates on loans for home energy retrofits, compared to household assets of a comparable size (Giraudet et al., 2021b).[19] Offering a ZIGL instead to finance the same

[19]One reason might be that credit institutions perceive home energy retrofits as a risky investment. Indeed, there is growing evidence of a discrepancy between predicted and realized energy savings, also known as the energy performance gap (Christensen et al., 2021) This is partly due to the credence-good nature of energy

investment would thus imply significant opportunity cost. It is therefore unclear whether own-loans and ZIGLs are complements or substitutes. In this section, we explore this issue by estimating banks' opportunity cost at the local market level.

7.1 Data

We use interest rate data from a sample of new contracts collected by Bank of France (M_CONTRAN dataset). The dataset provides all loan origination of a given bank branch, for a sample of branches of the majority of French banks, at a quarterly frequency, starting in 2012. For a given loan, we observe its type (consumption or housing loan),[20] its interest rate, its amount and some other characteristics. We match these data with Bank of France's census of bank branches at the municipal level (FEGA dataset). We further match the data with the administrative data containing the universe of ZIGL. Finally, we use the National Statistical Office (INSEE)'s census data to get information on local demographics and economic conditions. The census data being available up to 2018, we focus on the 2012-2018 period. This results in 14,726 observations for 49 banks in 704 catchment areas,[21] observed during at least two quarters and at most 26 quarters.

7.2 Empirical strategy

We focus on local banking markets, considered to be catchment areas. For a given bank in a given catchment area in a certain period, we compute the opportunity cost as the average interest rate of all consumption and housing loans, weighted by loan amounts:

$$\text{Opportunity cost}_{b,a,t} = \frac{\sum_i (\text{Interest rate}_{i,b,a,t} \cdot \text{Loan amount}_{i,b,a,t})}{\sum_i \text{Loan amount}_{i,b,a,t}} \tag{4}$$

where b is the bank identifier, a is the catchment area identifier, t is a quarter and i is a loan identifier.

Constructed in this way, our variable captures various opportunity cost channels. First, local market conditions might allow a bank to charge higher interest on household loans, for example due to softer competition. Second, a bank might specialize in consumption loans in some areas and in housing loans in others. Since consumption loans typically entail higher interest rates, a specialization in this type of loan products results in a higher opportunity cost of ZIGL provision.

We relate our measure of opportunity cost to the number of ZIGLs of the corresponding bank, in a corresponding area, originated in a given period. We regress the ZIGL count variable in a Poisson model that includes time-varying controls and multiple fixed effects:

$$\ln\left(\mathbb{E}[\#\text{ZIGL}_{b,a,t}]\right) = \beta \cdot \text{Opportunity cost}_{b,a,t} + X_{a,t} + \varphi_b + \gamma_a + \tau_t \tag{5}$$

efficiency assets, which gives rise to a range of information asymmetries (Giraudet, 2020).

[20] The consumption loans considered here are "personal loans" (*prêt personnel* in French), a product associated with no specific asset, a fixed interest rate and a fixed repayment schedule defined at the onset of the contract. The housing loans (*Prêt immobilier* in French) include loans for house purchase and for renovation.

[21] The catchment area (*bassin de vie* in French) is a geographical unit defined by INSEE as the area that allows households to get all essential services and amenities. There are 1,666 catchment areas in France.

The vector of controls for a catchment area a in a period t, $X_{a,t}$, includes variables on population, the demographic structure, local labor market dynamics and characteristics of the local housing stock. Time fixed effects (τ_t) allow us to capture factors that influence all banks and catchment areas equally, e.g., the uniform rate of bank compensation for ZIGL origination that depends on government bond rates. Bank and catchment area fixed effects (φ_b and γ_a) allow us to capture all time-invariant determinants of ZIGL production on a bank and on a local level. In order to strengthen the identification of the opportunity cost effect, we also run a specification with bank-time fixed effects ($\varphi_{b,t}$). Our coefficient of interest, β, is interpreted as a semi-elasticity of ZIGL production to opportunity cost.

7.3 Results

The regression results are reported in Table 3. Absent any cross-sectional fixed effects, the estimate of the semi-elasticity is negative and significant at the 1% level, with a magnitude of -0.22. In our preferred specification with bank and area fixed effects, the estimate is -0.05, significant at the 10% level. The effect is substantial – a 1 p.p. increase in the opportunity cost is associated with a 5% drop in ZIGL provision for a given bank in a given area. The effect is unchanged in the regression with bank-time fixed effects.

Table 3: Effect of banks' opportunity cost on ZIGL activity

Dependent Variable:		Nb of ZIGL	
Model:	(1)	(2)	(3)
Variables			
Opportunity cost	-0.2177***	-0.0463*	-0.0458*
	(0.0461)	(0.0251)	(0.0268)
Controls	Yes	Yes	Yes
Fixed-effects			
Time	Yes	Yes	
Bank		Yes	
Catchment Area		Yes	Yes
Bank × time			Yes
Fit statistics			
Observations	14,726	14,726	14,726
Squared Correlation	0.244	0.74	0.79
Pseudo R^2	0.206	0.475	0.496
BIC	64,673	50,098	56,708

Notes: Estimates of the Poisson regression (5) with different sets of fixed effects. Opportunity cost is measured as a weighted average of consumption and housing loans interest rates of a bank in a catchment area, in a given quarter (see Equation 4). Significance codes: ***0.01, **0.05, *0.1. Bank×Catchment Area standard-errors in parentheses. Data sources: Banque de France (M_CONTRAN and FEGA datasets), INSEE (census results), ZIGL administrator.

We interpret these results as evidence that a higher opportunity cost – measured as the weighted average interest rate on other household loans – reduces ZIGL provision. This may be due to loan officers pushing consumers towards traditional, interest-bearing products to finance renovation. Note that, for this mechanism to be effective, consumers must be imperfectly informed about ZIGLs. Otherwise, they would never take a costly loan when they are entitled a ZIGL instead.

7.4 Other possible mechanisms

One common critique made by the banking industry and government representatives alike about the ZIGL program was that it entailed significant transaction costs. To investigate this hypothesis, we conducted interviews with three high-profile stakeholders who have been in office ever since the introduction of the program – one with the program administrator and two with the banking industry. Their accounts concur to build the following narrative. The program indeed featured highly demanding administrative requirements which the program administrator took time to learn to check. After an initial phase in which banks issued ZIGLs unabatedly, the first control checks completed in mid-2010 identified a high prevalence of non-conformity. In particular, many retrofit works had been performed prior to the year of loan application, at odds with the requirement that the two be contemporaneous. This plausibly made the banks realize how demanding the program truly was, urging them to pause ZIGL production, never to take over again. In 2015, some simplifications were implemented when the burden of technically appraising the project was transferred from banks to retrofit contractors. However, this change was not followed by any noticeable increase in the ZIGL count.[22]

8 Conclusion

The ZIGL program for home energy retrofits was introduced in France in 2009 with high expectations. Amidst the shock wave of the 2008 financial crisis and growing concerns for climate action, it was meant to address the energy efficiency financing gap in a comprehensive demand-and-supply approach.

As our empirical analysis reveals, based on a state-of-the-art event-study design applied to a rich panel data set, the program successfully delivered on its two main objectives – at least in the first two years. First, it significantly increased investment in home energy retrofits on the extensive margin (+20-22%) and, to a lesser extent, on the intensive margin (+3-5%), thereby reducing electricity consumption (-3%), in 2009 and 2010. These estimates together imply leverage in the 1.3-1.8 range, suggesting that public money – over €200 million per year at the time – was spent effectively on reducing the negative environmental externalities associated with energy use. Second, with much stronger effects for low-income homeowners than for the average eligible participant, the program succeeded in benefiting borrowers that are otherwise excluded from credit markets. This achievement is all the more remarkable that no targeted provision was built into the program, unlike in other low-interest loan programs. While the external validity of our first-of-its-kind analysis still needs to be corroborated in other contexts, we think it already generates an important insight: by

[22] Our interviewees also claimed that the compensation – a fixed spread of 1.35 percentage point on top of the rate on government bonds – was too low to cover the bank's private cost of issuing a ZIGL. Without access to banks' private information, we were not able to investigate this hypothesis.

stimulating the extensive margin of energy efficiency investment, low-interest loan programs can aptly complement direct subsidy programs, seemingly more effective on the intensive margin.

Then, in 2011, the number of new ZIGLs collapsed, to the point that we detect no statistically significant impact of the program from then on. This outcome is only weakly related to the downward trending interest rate environment. Instead, the best explanation we can propose, based on a broad review of candidate problems, is that, from 2011 onward, banks reduced their information provision in an effort to divert prospective borrowers away from taking ZIGLs and sell them their own loan products instead. This is jointly substantiated by a drop in households' stated awareness of the program in 2011 and a carefully estimated -0.05 semi-elasticity of ZIGL provision with respect to the weighted average interest rate charged on other loans in 2012-2018. Why this regime shift occurred in 2011 remains unclear. Interviews with a few key stakeholders suggest it was due to ex post rejection of a substantial number of ZIGL applications, which in turn induced the banks to perceive the program as risky. The program then remained at too low a participation level to induce significant learning and economies of scale, considering that, with about 40,000 bank branches across the French territory, each branch produced on average 0.5 to one ZIGL every year over the 2011-2018 period.

Our findings raise important questions for the justification of the program and its design. First, are private banks the best-suited agents for providing low-interest loans for energy efficiency investment? France took this road based on the premise that banks' retail networks offer unparalleled access to prospective borrowers. In contrast, the German and U.S. programs rely on public lenders. Our finding that banks may lack interest suggests that pooling ZIGL applications instead as in the latter approach may be more effective at generating economies of scale. Second, should the government increase the compensation given to banks in order to reduce the opportunity cost of issuing ZIGLs? The answer depends on how the benefits would be shared between increased consumer participation and increased banks' profit, which requires further analysis. In any case, increasing information provision may be a more cost-effective, and fairer, way to increase participation in the program, since, again, were households fully aware of the program, they would never accept a costly loan to finance the same investment. These insights are important to bear in mind if ZIGL programs are to be extended to the financing of other green assets, such as electric vehicles, as recommended by the French Citizens' Convention for Climate (Convention Citoyenne pour le Climat, 2020).

While our analysis only goes as far as 2018, recent changes have created a whole new environment for the program. The requirement to combine several measures was lifted in the last quarter of 2019. Participation has been on the rise ever since, with 35,574 loans issued in 2019, 42,107 in 2020 and 61,034 in 2021. Meanwhile, as one would expect from such an adjustment, the average amount borrowed has decreased from around €18,000 in 2016-2018 to €13,000 in 2019-2021. While this new momentum creates prospects of economies of scale, the reduction in spending lessens one of the key merits of the previous version of the program – targeting comprehensive retrofits, which is key to induce significant leverage.

Furthermore, the eligibility restriction to those dwellings built before 1990 has been eased off to those buildings built at least two years ago. Lastly, the macroeconomic environment has dramatically changed recently, with rising energy prices increasing the profitability of home energy retrofits while rising interest rates increase the implicit benefit of ZIGLs. Further analysis is therefore needed to assess how these new settings have affected the performance of the program.

Acknowledgements

This research was supported by funding from Banque de France (Prix jeune chercheur pour la finance verte), ADEME (Projet FRITE, convention 20ESC0034) and CGDD (Convention pluriannuelle de recherche, convention 2201247634). We are extremely grateful to Maryam Vahid Dastgerdi for invaluable preliminary work with the program dataset that paved the way for using the ADEME dataset in the core analysis. We thank Paul Lévi, Marie-Laure Nauleau, Selma Geraut, Roxane Rousseau, Silvano Domergue and Sonia Snoussi for support with the data. We thank Brice Welti, Nicole Chavier and Christine Passeman for their first-hand account of the history of the program. We thank Nicolas Astier, Ondine Berland, Bettina Chlond, Sébastien Houde, Marion Leroutier, Brendon McConnell and Philippe Quirion for useful comments on an earlier draft. We thank seminar participants at CIRED, University College Dublin, Toulouse School of Economics, Université de Rennes' CREM, Paris-Saclay Applied Economics and the audience in various conferences where the paper was presented (AFSE-DG Tresor 2021, FAERE 2021, ZEW Mannheim Energy Conference 2022, Atlantic Workshop on Energy and Environmental Economics 2022) for their feedback.

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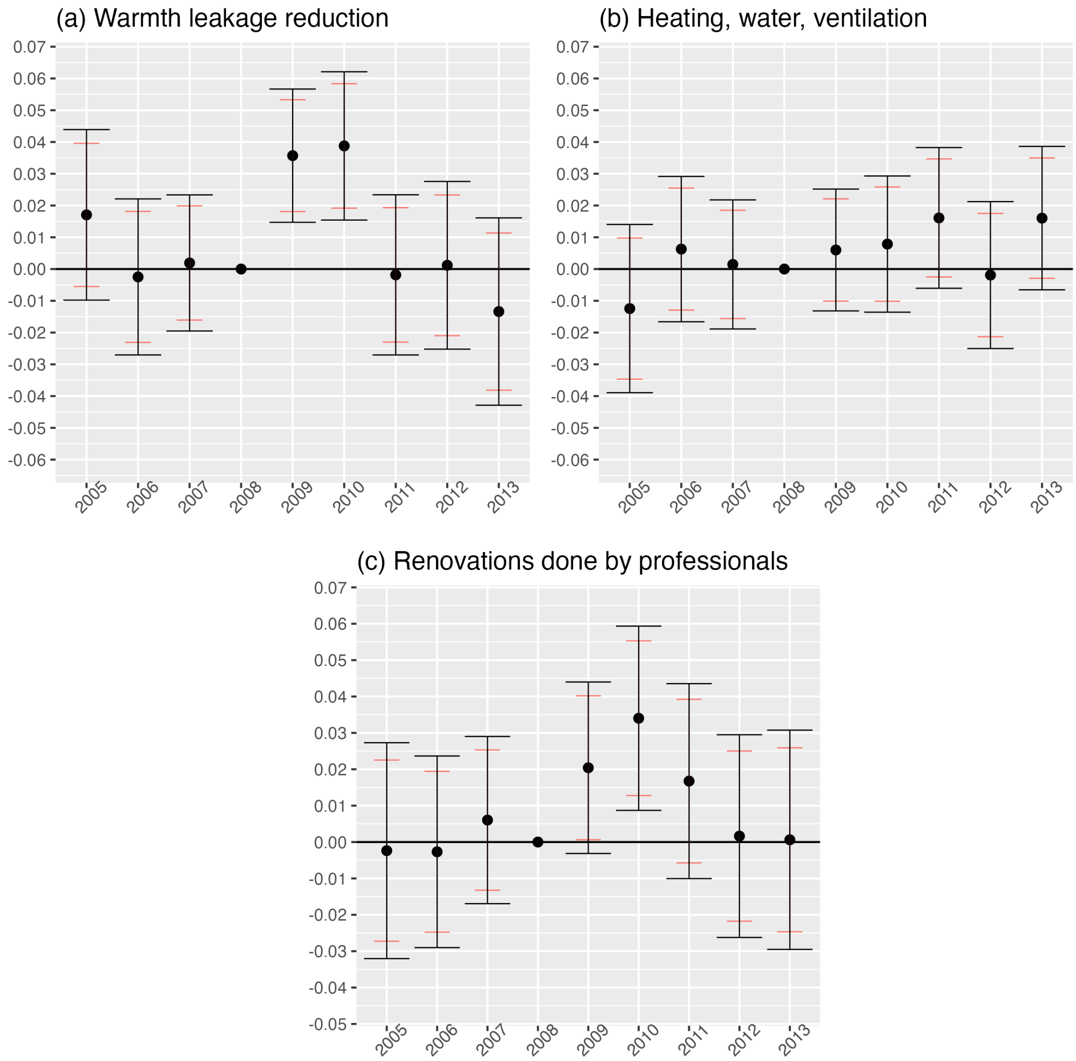
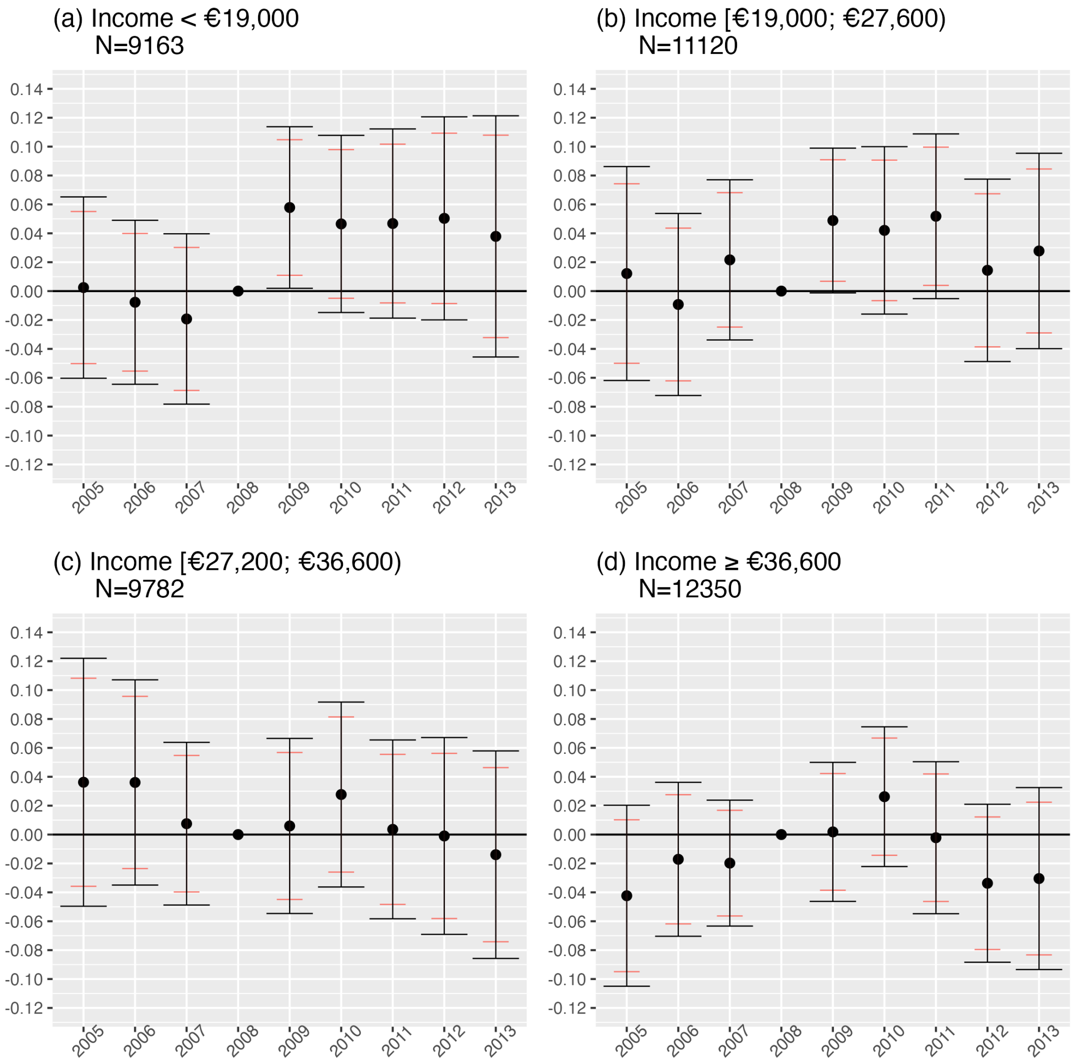
A Figures

Figure A1: Effects of eligibility on various types of renovation actions



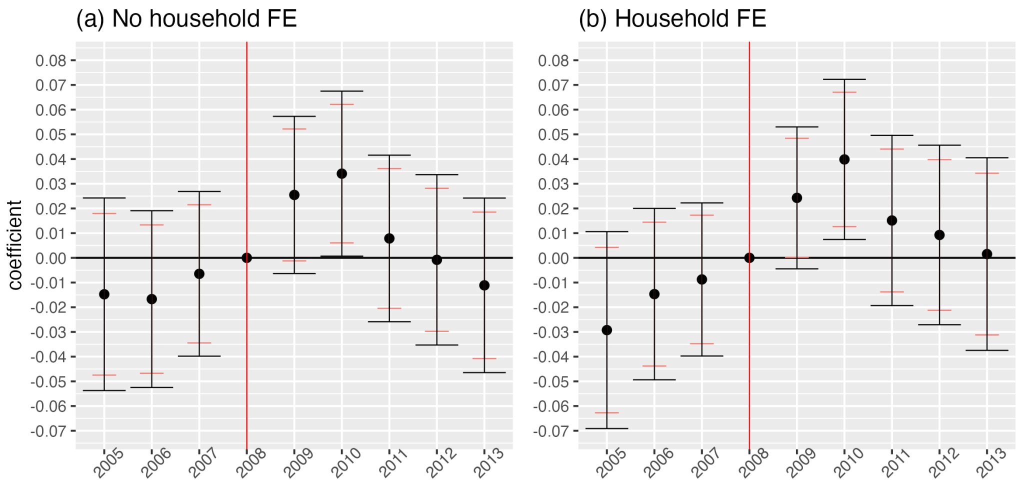
Notes: Estimates of for the event study of Equation (1), with various definitions of renovation as dependent variable. Confidence intervals: 95% in black, 90% in red. Household FE, time FE and time-varying controls used in all regressions. Standard errors clustered at the household level. See Table A1 for description of controls. Data source: ADEME survey. Back to section 4.3

Figure A2: Heterogeneous effects of professional renovations



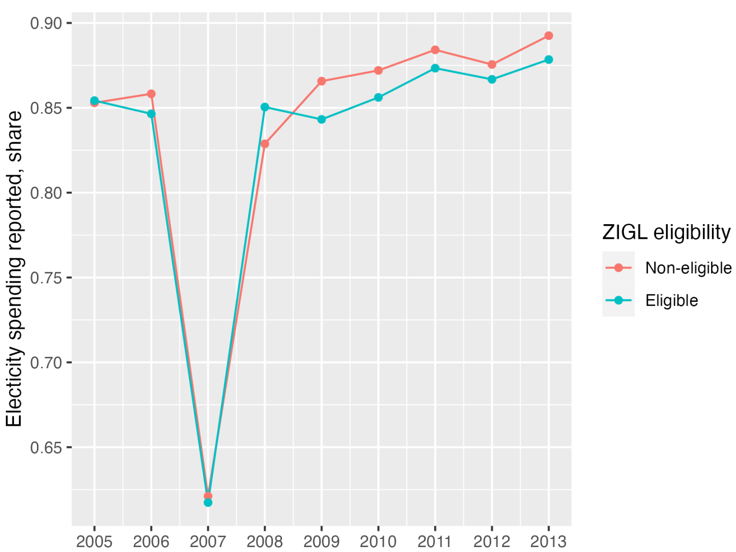
Notes: Estimates for the event study of Equation (1), with professional renovations as the dependent variable. Confidence intervals: 95% in black, 90% in red. Household FE, time FE and time-varying controls (controlling by income bins only in (b) and (d)) used in all regressions. Standard errors clustered at the household level. See Table A1 for description of controls. Data source: ADEME survey. Back to section 4.3

Figure A3: Effects of eligibility on renovation decision, excluding houses built before 1949



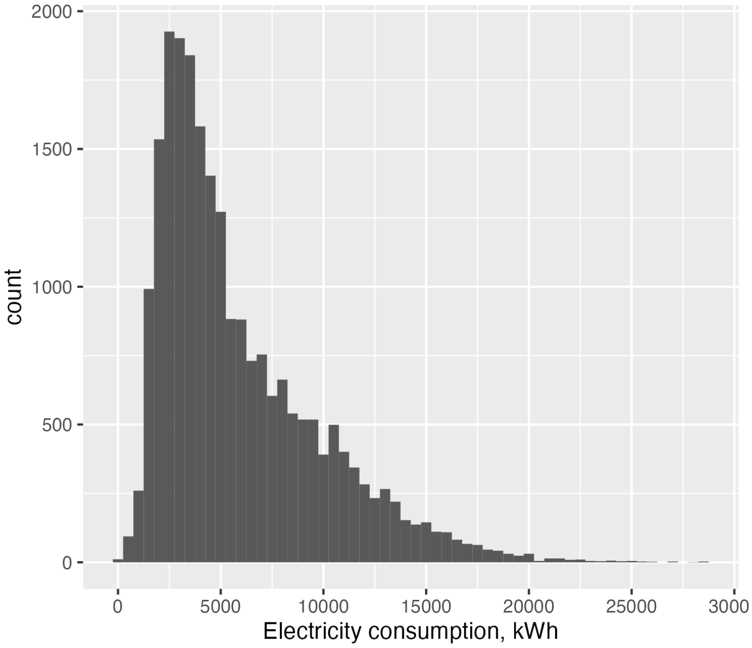
Notes: Estimates for the event study of Equation (1) for renovation decision (binary), excluding houses built before 1949. Confidence intervals: 95% in black, 90% in red. Specification: (a) with household controls (both constant and time-varying), but no household FE; (b) with household FE and time-varying controls. Time FE used in both specifications. Standard errors clustered at the household level. Data source: ADEME Survey. Back to Section 5.

Figure A4: Share of households reporting electricity spending



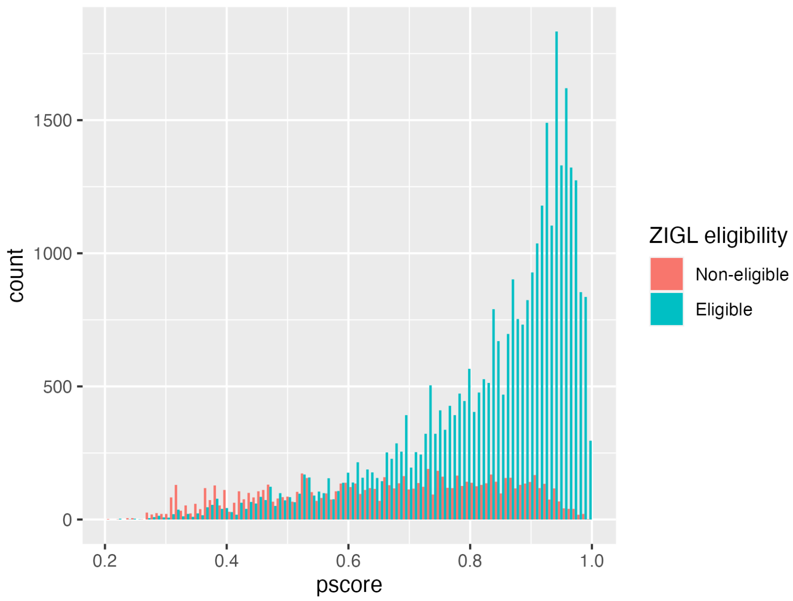
Notes: Share of households that report (nonzero) electricity spending, by year. Data source: ADEME survey. Back to Section 4.5.

Figure A5: Distribution of electricity consumption



Notes: Distribution of electricity consumption from reduced sample described in Section 3.1. Data source: ADEME Survey for electircity spending, Enerdata and Pegase for electricity prices. Back to Section 3.1.

Figure A6: Common support of the treatment and control groups



Notes: Propensity scores obtained from a logit regression of ZIGL eligibility on the covariates of Table A1, except region. Data source: ADEME Survey. Back to Section 5.2.

B Tables

Table A1: Description of categorical variables.

Variable	Values
ZIGL eligibility	
Dwelling construction period	Before 1949; 1949 to 1974; 1975 to 1981; 1982 to 1988; 1989 to survey year−1; survey year
Control variables	
Age of household head	Less than 25 years old; 25 to 34 ; 35 to 44 ; 45 to 54 ; 55 to 64 ; 65 years old and more*
Occupation of household head (*PCS*)	Agricultural; Trade/entrepreneur; Independent/management; Intermediary; Employee; Worker; Non-employed*
Income	Less than 19k; 19 to 23k; 22.8 to 27.6k; 27.2 to 36.6k*; 36.6 to 45.6k; 45.6k € and more
Population size indicator	Paris agglomeration; More than 100,000 inhabitants*; From 20,000 to 100,000; From 2,000 to 20,000 ; Rural
Region	22 INSEE regions
Surface area	Less than 50 m^2; 50 to 74; 75 to 99 ; 100 to 149* ; 150 m^2 and more
Main heating fuel	Natural Gas*, Electricity, Fuel Oil, Other
Heating system type	Individual non-electric*, Individual electric, Central
Dwelling type	Single-family*, Multi-family

Notes: * signals the omitted category in all regressions. Data source: ADEME Survey. Back to Section 3.1.

Table A2: ZIGL summary statistics

	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019
Descriptive statistics											
Number of loans	68,225	79,508	42,324	33,936	32,448	31,053	23,692	22,858	24,118	19,010	34,806
Number of registered lenders	99	104	101	102	99	102	99	98	96	92	93
Number of municipalities with ≥ 1 ZIGL	15,823	17,497	12,633	11,238	11,330	11,255	9,580	9,537	9,857	8,653	13,653
Average amount borrowed, euros	16,318	16,798	17,020	17,119	17,297	17,293	17,140	17,569	17,916	17,850	13,408
Average retrofit size, euros	18,518	19,091	19,383	19,556	20,003	20,782	20,514	21,238	22,223	22,701	16,795
Average loan-to-value ratio	0.89	0.87	0.87	0.86	0.85	0.83	0.82	0.82	0.81	0.80	0.79
Average loan duration, months	107	109	110	116	122	123	123	125	125	126	116
Share of secured loans	0.30	0.32	0.31	0.30	0.31	0.30	0.29	0.30	0.32	0.31	0.20
Years studied in the different analyses											
Baseline analysis (Sections 4 and 5)	X	X	X	X	X						
Demand-side analysis (Section 6)	X	X	X	X	X						
Supply-side analysis (Section 7)				X	X	X	X	X	X	X	

Notes: Data source: program administrator (SGFGAS). Back to Section 2.

Table A3: Summary statistics for 2008 and 2013

		2008		2013	
Variable	Category	Mean	Std.Dev.	Mean	Std.Dev.
Renovate	Yes/No	0.17	0.38	0.15	0.36
Eligible	Yes/No	0.81	0.40	0.77	0.42
Construction period	Before 1949	0.28	0.45	0.26	0.44
	1949 to 1974	0.29	0.45	0.29	0.45
	1975 to 1981	0.14	0.34	0.13	0.34
	1982 to 1988	0.10	0.30	0.09	0.29
	After 1988	0.19	0.40	0.23	0.42
Agglomeration type	Paris Area	0.13	0.34	0.13	0.33
	Pop. $>$ 100k	0.26	0.44	0.27	0.44
	Pop. 20k to 100k	0.13	0.33	0.12	0.33
	Pop. $<$ 2k	0.18	0.39	0.19	0.40
	Rural	0.30	0.46	0.29	0.45
Age of head	$<$ 25 y.o.	0.01	0.07	0.00	0.04
	25 to 34 y.o.	0.09	0.29	0.07	0.25
	35 to 44 y.o.	0.17	0.38	0.17	0.38
	45 to 54 y.o.	0.19	0.39	0.19	0.39
	55 to 64 y.o.	0.20	0.40	0.19	0.40
	$>$ 65 y.o.	0.34	0.47	0.37	0.48
Occupation of head	Agriculture	0.02	0.14	0.02	0.13
	Blue-collar worker	0.15	0.36	0.15	0.35
	Independent/Mngmnt	0.12	0.32	0.12	0.32
	Intermediary	0.14	0.35	0.13	0.34
	Non-employed	0.46	0.50	0.45	0.50
	Trade/Entrepreneur	0.04	0.19	0.05	0.22
	White-collar worker	0.07	0.26	0.08	0.27
Income	$<$ 19k €	0.22	0.42	0.20	0.40
	19k to 22.8k €	0.13	0.34	0.13	0.33
	22.8k to 27.6k €	0.15	0.36	0.13	0.33
	27.2k to 36.6k €	0.20	0.40	0.23	0.42
	36.6k to 45.6k €	0.16	0.36	0.13	0.33
	$>$ 45.6k €	0.14	0.35	0.18	0.38
Surface	$<$ 50 sq.m.	0.03	0.18	0.04	0.19
	50 to 74 sq.m.	0.14	0.35	0.15	0.36
	75 to 99 sq.m.	0.26	0.44	0.31	0.46
	100 to 149 sq.m.	0.40	0.49	0.37	0.48
	$>$ 150 sq.m.	0.17	0.38	0.14	0.34
Heating main energy	Electricity	0.31	0.46	0.32	0.47
	Fuel Oil	0.20	0.40	0.17	0.38
	Gas	0.42	0.49	0.39	0.49
Heating type	Central	0.10	0.31	0.10	0.30
	Individual non-electric	0.52	0.50	0.47	0.50
	Individual electric	0.28	0.45	0.27	0.45
Multi-family unit	Yes/No	0.25	0.43	0.26	0.44
N		5406		4295	
Electricity consumption	’000 kWh	6.27	7.53	5.59	6.69
N		3203		2671	

Notes: Survey weights are applied. Data source: ADEME Survey. Back to Section 3.1.

Table A4: Balancing test: covariates in eligible vs. non-eligible households in 2008

Variable	Category	Eligible (T) Mean	SD	Non-Eligible (C) Mean	SD	Diff	t-stat	p-value
Agglomeration	Paris Area	0.14	0.35	0.08	0.28	0.06	5.15	0***
	Pop. > 100k	0.27	0.45	0.21	0.41	0.07	4.36	0***
	Pop. 20k to 100k	0.13	0.34	0.10	0.30	0.04	3.08	0.002***
	Pop. < 2k	0.17	0.38	0.22	0.41	-0.04	-3.23	0.001***
	Rural	0.28	0.45	0.39	0.49	-0.12	-7.54	0***
Age	< 25 y.o.	0.01	0.08	0.00	0.04	0.00	1.61	0.107
	25 to 34 y.o.	0.07	0.26	0.17	0.37	-0.10	-9.77	0***
	35 to 44 y.o.	0.13	0.34	0.34	0.48	-0.21	-16.88	0***
	45 to 54 y.o.	0.18	0.39	0.22	0.41	-0.03	-2.28	0.022**
	55 to 64 y.o.	0.21	0.41	0.13	0.34	0.08	6.16	0***
	> 65 y.o.	0.39	0.49	0.14	0.35	0.25	15.80	0***
Occupation	Agriculture	0.02	0.13	0.03	0.16	-0.01	-1.84	0.065*
	Blue-col. worker	0.12	0.33	0.26	0.44	-0.14	-11.79	0***
	Indep./Mngmnt	0.11	0.31	0.15	0.36	-0.05	-4.21	0***
	Intermediary	0.13	0.33	0.20	0.40	-0.08	-6.55	0***
	Non-employed	0.52	0.50	0.22	0.42	0.30	17.79	0***
	Trade/Entrepr.	0.04	0.19	0.04	0.19	-0.00	-0.16	0.869
	White-col. worker	0.07	0.26	0.09	0.29	-0.02	-2.13	0.034**
Income	< 19k €	0.25	0.43	0.12	0.32	0.13	9.35	0***
	19k to 22.8k €	0.14	0.35	0.10	0.31	0.04	3.01	0.003***
	22.8k to 27.6k €	0.14	0.35	0.16	0.37	-0.02	-1.61	0.106
	27.2k to 36.6k €	0.19	0.40	0.23	0.42	-0.04	-2.97	0.003***
	36.6k to 45.6k €	0.14	0.35	0.20	0.40	-0.06	-5.18	0***
	> 45.6k €	0.13	0.34	0.17	0.38	-0.04	-3.71	0***
Surface area	< 50 sq.m.	0.04	0.19	0.03	0.17	0.01	0.97	0.332
	50 to 74 sq.m.	0.15	0.36	0.08	0.28	0.07	5.75	0***
	100 to 149 sq.m.	0.37	0.48	0.49	0.50	-0.12	-7.33	0***
	> 150 sq.m.	0.18	0.38	0.17	0.38	0.01	0.70	0.484
Main heating fuel	Electricity	0.26	0.44	0.51	0.50	-0.26	-16.56	0***
	Fuel Oil	0.23	0.42	0.10	0.30	0.13	9.63	0***
	Natural Gas	0.45	0.50	0.30	0.46	0.15	8.91	0***
Heating type	Central	0.12	0.33	0.02	0.13	0.11	10.24	0***
	Individ. non-elec.	0.55	0.50	0.38	0.49	0.17	10.13	0***
	Individual elec.	0.23	0.42	0.47	0.50	-0.24	-16.08	0***
Multi-family unit	Yes/No	0.27	0.44	0.19	0.39	0.07	4.99	0***
N		4273		1133				

Notes: t-stats and p-values come from t-tests of covariate mean equality between eligibility groups. Survey weights are used. Data source: ADEME Survey.

Table A5: Effect of eligibility on renovation decision — extensive margin

Dependent Variable:	Renovation this year	
Model:	(1)	(2)
Variables		
Eligible	0.0976***	0.0378*
	(0.0118)	(0.0206)
Eligible × 2005	0.0007	-0.0112
	(0.0189)	(0.0193)
Eligible × 2006	-0.0040	-0.0056
	(0.0173)	(0.0166)
Eligible × 2007	-0.0051	-0.0067
	(0.0159)	(0.0146)
Eligible × 2009	0.0365**	0.0367***
	(0.0151)	(0.0135)
Eligible × 2010	0.0279*	0.0396***
	(0.0159)	(0.0152)
Eligible × 2011	0.0051	0.0176
	(0.0160)	(0.0164)
Eligible × 2012	-0.0019	0.0103
	(0.0164)	(0.0172)
Eligible × 2013	-0.0060	0.0132
	(0.0169)	(0.0187)
Fixed-effects		
Year	Yes	Yes
Household		Yes
Fit statistics		
Observations	42,415	42,415
R^2	0.04171	0.43748
Within R^2	0.03996	0.10541

Notes: Estimates of equation (1). Survey weights are applied. See controls description in Table A1. Clustered (Household) standard errors in parentheses. Significance codes: ***0.01, **0.05, *0.1. Data source: ADEME Survey. Back to Figure 3.

Table A6: Effect of eligibility on the renovation decision – two-period, with controls

	Dependent variable:	
	Renovation this year	
	(1)	(2)
Eligible	0.095*** (0.008)	0.030 (0.020)
Eligible×Post	0.014 (0.009)	0.032*** (0.011)
Renovated before	0.068*** (0.005)	−0.364*** (0.008)
Age < 25	0.069 (0.048)	−0.163** (0.081)
Age 25 to 34	0.155*** (0.013)	−0.022 (0.033)
Age 35 to 44	0.097*** (0.012)	−0.014 (0.027)
Age 45 to 54	0.055*** (0.011)	−0.028 (0.021)
Age 55 to 64	0.048*** (0.008)	−0.002 (0.014)
Occupation Agriculture	−0.061*** (0.021)	−0.140 (0.103)
Occupation Trade.Entrep.	−0.036** (0.017)	0.033 (0.043)
Occupation Indep.Mngmnt	−0.007 (0.012)	−0.056* (0.029)
Occupation Intermediary	−0.020* (0.011)	−0.010 (0.025)
Occupation White-collar worker	−0.026** (0.011)	−0.011 (0.027)
Occupation Blue-collar worker	−0.013 (0.011)	−0.013 (0.029)
Agglomeration Paris Area	−0.046** (0.019)	−0.161 (0.100)
Agglomeration > 100k inhab.	−0.002 (0.007)	−0.058* (0.035)
Agglomeration 20 to 100k inhab.	0.004 (0.008)	0.007 (0.031)
Agglomeration < 2k inhab.	0.0001 (0.007)	0.013 (0.021)
Multi-family unit	−0.044*** (0.007)	−0.063** (0.027)
Surface area < 50 sq.m	−0.036*** (0.013)	−0.079*** (0.027)
Surface area 50 to 74 sq.m	−0.027*** (0.008)	−0.053*** (0.014)
Surface area 75 to 99 sq.m	−0.017*** (0.005)	−0.024*** (0.009)
Surface area > 150 sq.m	0.012 (0.007)	−0.002 (0.011)
Income < 19k €	−0.023*** (0.007)	0.00000 (0.010)
Income 19 to 23k €	−0.003 (0.008)	−0.010 (0.010)
Income 22.8 to 27.6k €	0.001 (0.007)	−0.001 (0.008)
Income 36.6 to 45.6k €	0.001 (0.007)	0.001 (0.008)
Income > 46.6k €	0.005 (0.008)	0.021* (0.011)
Heating Electricity	0.052** (0.023)	−0.029 (0.032)
Heating Fuel Oil	0.005 (0.007)	−0.095*** (0.023)
Heating Other fuel	0.042* (0.024)	−0.013 (0.033)
Heating Central	0.011 (0.010)	0.029 (0.027)
Heating Indiv. Elec.	−0.055** (0.025)	0.009 (0.039)
Heating Other type	−0.033 (0.024)	0.032 (0.037)
Fixed-effects		
Year	Yes	Yes
Household		Yes
Fit statistics		
Observations	42,418	42,418
R^2	0.042	0.437
Adjusted R^2	0.040	0.270
Residual Std. Error	0.397	0.346

Notes: Estimates of Equation (2). Survey weights are applied. Clustered (Household) standard errors in parentheses. See Table A1 for the baseline (omitted) category of each categorical variable. Significance codes: ***0.01, **0.05, *0.1. Data source: ADEME Survey. Back to Section 4.

Table A7: Effect of eligibility on the renovation decision, income sub-samples

Dependent Variable:	Renovation this year				
Income	Full sample	< €19k	[€19k, €27.6k)	[€27.2k, €36.6k)	≥ €36.6k
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Eligible	0.0378*	0.0153	0.0339	0.0300	0.0761**
	(0.0206)	(0.0533)	(0.0447)	(0.0505)	(0.0385)
Eligible × 2005	-0.0112	0.0073	-0.0134	0.0217	-0.0207
	(0.0193)	(0.0529)	(0.0413)	(0.0481)	(0.0387)
Eligible × 2006	-0.0056	0.0459	-0.0254	0.0023	-0.0125
	(0.0166)	(0.0465)	(0.0344)	(0.0439)	(0.0312)
Eligible × 2007	-0.0067	-0.0197	0.0254	-0.0225	-0.0205
	(0.0146)	(0.0462)	(0.0307)	(0.0338)	(0.0279)
Eligible × 2009	0.0367***	0.1138***	0.0432	0.0511	0.0252
	(0.0135)	(0.0386)	(0.0302)	(0.0328)	(0.0261)
Eligible × 2010	0.0396***	0.1146***	0.0562	0.0232	0.0209
	(0.0152)	(0.0419)	(0.0355)	(0.0370)	(0.0293)
Eligible × 2011	0.0176	0.0627	0.0721**	0.0143	-0.0080
	(0.0164)	(0.0414)	(0.0319)	(0.0392)	(0.0309)
Eligible × 2012	0.0103	0.0393	0.0610*	0.0288	-0.0241
	(0.0172)	(0.0521)	(0.0348)	(0.0412)	(0.0334)
Eligible × 2013	0.0132	0.0490	0.0274	0.0330	-0.0225
	(0.0187)	(0.0622)	(0.0406)	(0.0430)	(0.0371)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed-effects					
Year	Yes	Yes	Yes	Yes	Yes
Household	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	42,415	9,163	11,120	9,782	12,350
R^2	0.43748	0.53942	0.56685	0.59327	0.51086
Within R^2	0.10541	0.10416	0.11426	0.12081	0.10232

Notes: Significance codes: ***0.01, **0.05, *0.1. Clustered (Household) standard errors in parentheses. Columns (2) to (5) are estimates of Equation (1) for four sub-samples based on income. Data source: ADEME Survey. Back to Section 4.2.

Table A8: Heterogeneity of extensive margin effect: triple differences

Dependent Variable:	Renovation this year	
Model:	(1)	(2)
Variables		
Eligible	0.1063***	0.0041
	(0.0144)	(0.0245)
Eligible × Post	0.0125	0.0436*
	(0.0231)	(0.0242)
Eligible × Post × Income < 19k	0.0769**	0.0632*
	(0.0329)	(0.0345)
Eligible × Post × Income [27.2k, 36.6k)	0.0167	-0.0173
	(0.0321)	(0.0321)
Eligible × Post × Income ≥ 36.6k	0.0188	0.0058
	(0.0297)	(0.0321)
Controls	Yes	Yes
Fixed-effects		
Year	Yes	Yes
Household		Yes
Fit statistics		
Observations	28,767	28,767
R^2	0.04455	0.50827
Within R^2	0.04264	0.13200

Notes: Estimates of triple differences in differences, obtained by interactions of the income variable with the Eligibility and Post variables in equation (2). The years 2011-2013 are excluded since the aggregate effect is not found for that period. Income category from €27,600 to €36,600 — the most frequent one — is the omitted category. Significance codes: ***0.01, **0.05, *0.1. Clustered (Household) standard errors in parentheses. Data source: ADEME Survey.

Table A9: Effect of eligibility on renovation – intensive margin

Dependent Variables:	Renovation amount	Nb of renovation actions
Model:	(1)	(2)
	OLS	Poisson
Variables		
Eligible	125.6	0.3724*
	(98.12)	(0.1996)
Eligible × 2005	-15.52	-0.0602
	(79.73)	(0.2415)
Eligible × 2006	-3.183	0.0089
	(70.43)	(0.1893)
Eligible × 2007	18.03	-0.0045
	(64.31)	(0.1769)
Eligible × 2009	127.8**	0.1615
	(64.62)	(0.1576)
Eligible × 2010	175.1**	0.3650**
	(69.49)	(0.1805)
Eligible × 2011	82.99	0.1001
	(75.11)	(0.1891)
Eligible × 2012	41.29	-0.1644
	(81.36)	(0.1935)
Eligible × 2013	95.87	-0.0626
	(94.80)	(0.2162)
Controls	Yes	Yes
Fixed-effects		
Year	Yes	Yes
Household	Yes	Yes
Fit statistics		
Observations	40,755	19,586

Notes: OLS estimates of equation (1) for amounts spent on renovation. Survey weights are applied. Clustered (Household) standard errors in parentheses. Significance codes: ***0.01, **0.05, *0.1. Data source: ADEME Survey. Back to Section 4.4.

Table A10: Effect of eligibility on electricity consumption – full sample, income sub-samples

Dependent Variable:	Log(Electriciy consumption,kWh)				
Income	Full sample	< €19k	[€19k, €27.6k)	[€27.2k, €36.6k)	≥ €36.6k
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Eligible	9.84×10^{-5}	0.0071	-0.0908	0.0819	0.0324
	(0.0309)	(0.0751)	(0.0735)	(0.0788)	(0.0452)
Eligible × 2005	-0.0185	-0.0331	-0.0039	-0.0456	0.0450
	(0.0213)	(0.0658)	(0.0402)	(0.0590)	(0.0397)
Eligible × 2006	-0.0182	-0.0898	0.0355	0.0541	-0.0001
	(0.0197)	(0.0751)	(0.0383)	(0.0537)	(0.0384)
Eligible × 2009	-0.0265*	-0.1120**	-0.0081	0.0163	-0.0318
	(0.0154)	(0.0530)	(0.0364)	(0.0437)	(0.0244)
Eligible × 2010	-0.0278	-0.0790	0.0171	-0.0710	-0.0349
	(0.0173)	(0.0682)	(0.0355)	(0.0450)	(0.0289)
Eligible × 2011	-0.0089	-0.0156	0.0143	-0.0142	-0.0233
	(0.0182)	(0.0595)	(0.0357)	(0.0469)	(0.0326)
Eligible × 2012	-0.0307	-0.0953	0.0466	-0.0846	-0.0121
	(0.0197)	(0.0631)	(0.0374)	(0.0515)	(0.0346)
Eligible × 2013	-0.0394*	-0.0619	0.0558	-0.0860	-0.0284
	(0.0221)	(0.0686)	(0.0482)	(0.0565)	(0.0367)
Fixed-effects					
Year	Yes	Yes	Yes	Yes	Yes
Household	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	22,672	4,548	5,998	5,413	6,713
R^2	0.92419	0.93284	0.94472	0.94945	0.94429
Within R^2	0.09334	0.08080	0.08868	0.08864	0.12296

Clustered (Household) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Column (1) is estimates of equation (1) for log of electricity consumption. Columns (2) to (5) are estimates of the same regression for four sub-samples based on income. Household fixed effects and time-varying controls used in all regressions. Clustered (Household) standard-errors in parentheses. Significance codes: ***0.01, **0.05, *0.1. Data sources: ADEME Survey, Enerdata, Pegase. Back to Section 4.5.

Table A11: Propensity scores — Logit regression

	Dependent variable:
	Eligible
Age < 25	−1.366*** (0.270)
Age 25 to 34	−1.410*** (0.269)
Age 35 to 44	−0.728*** (0.269)
Age 45 to 54	−0.132 (0.269)
Age 55 to 64	−0.047 (0.270)
Occupation Agriculture	0.514*** (0.094)
Occupation Trade.Entrep.	0.548*** (0.087)
Occupation Indep.Mngmnt	0.438*** (0.084)
Occupation Intermediary	0.441*** (0.088)
Occupation White-collar worker	0.184** (0.082)
Occupation Blue-collar worker	0.616*** (0.091)
Agglomeration Paris Area	−0.259*** (0.050)
Agglomeration > 100k inhab.	−0.352*** (0.058)
Agglomeration 20 to 100k inhab.	−0.708*** (0.055)
Agglomeration < 2k inhab.	−0.729*** (0.054)
Surface area < 50 sq.m	−0.073 (0.079)
Surface area 50 to 74 sq.m	−0.402*** (0.078)
Surface area 75 to 99 sq.m	−0.683*** (0.080)
Surface area > 150 sq.m	−0.366*** (0.086)
Income < 19k €	−0.302*** (0.048)
Income 19 to 23k €	−0.435*** (0.047)
Income 22.8 to 27.6k €	−0.643*** (0.041)
Income 36.6 to 45.6k €	−0.835*** (0.045)
Income > 46.6k €	−0.986*** (0.048)
Heating Electricity	−0.148 (0.138)
Heating Fuel Oil	0.606*** (0.044)
Heating Other Fuel	0.086 (0.146)
Heating Central	−1.539*** (0.085)
Heating Indiv. Elec.	−2.280*** (0.160)
Heating Other type	−1.941*** (0.161)
Multi-family unit	−0.459*** (0.042)
Constant	4.900*** (0.305)
Observations	42,418
Log Likelihood	−21,351.450
Akaike Inf. Crit.	42,766.910

Notes: Estimates of logit regression for propensity scores. Survey weights are applied. Figure A6 plots the predicted propensity scores for the eligible and non-eligible. Significance codes: ***0.01, **0.05, *0.1. Data source: ADEME Survey. Back to Section 5.2.

Table A12: Balancing test with propensity score weighting (2008)

Variable	Category	Eligible (T) Mean	SD	Non-Eligible (C) Mean	SD	Diff	T-stat	p-value
Multi-family unit	Yes/No	0.25	0.43	0.28	0.45	-0.03	-2.17	0.03**
Agglomeration	Paris Area	0.13	0.34	0.12	0.32	0.02	1.86	0.063*
	Pop. > 100k	0.26	0.44	0.27	0.44	-0.01	-0.97	0.33
	Pop. 20k to 100k	0.13	0.33	0.14	0.34	-0.01	-0.84	0.401
	Pop. < 2k	0.18	0.39	0.18	0.38	0.00	0.40	0.691
	Rural	0.30	0.46	0.30	0.46	-0.00	-0.13	0.899
Age	< 25 y.o.	0.01	0.07	0.00	0.05	0.00	1.47	0.141
	25 to 34 y.o.	0.09	0.29	0.09	0.28	0.00	0.17	0.868
	35 to 44 y.o.	0.17	0.37	0.19	0.39	-0.02	-2.33	0.02**
	45 to 54 y.o.	0.20	0.40	0.19	0.40	0.00	0.26	0.794
	55 to 64 y.o.	0.20	0.40	0.23	0.42	-0.03	-2.75	0.006***
	> 65 y.o.	0.34	0.48	0.30	0.46	0.05	3.78	0***
Occupation	Agriculture	0.02	0.15	0.02	0.15	-0.00	-0.27	0.79
	Blue-col. worker	0.14	0.35	0.16	0.37	-0.02	-1.81	0.071*
	Indep./Mngmnt	0.12	0.32	0.12	0.32	0.00	0.43	0.669
	Intermediary	0.15	0.35	0.16	0.36	-0.01	-1.27	0.205
	Non-employed	0.46	0.50	0.43	0.50	0.02	1.83	0.067*
	Trade/Entrepr.	0.04	0.19	0.03	0.18	0.00	0.75	0.455
	White-col. worker	0.07	0.26	0.07	0.26	-0.00	-0.19	0.852
Income	< 19k €	0.23	0.42	0.20	0.40	0.03	2.44	0.015**
	19k to 22.8k €	0.14	0.34	0.17	0.38	-0.04	-3.94	0***
	22.8k to 27.6k €	0.14	0.35	0.15	0.36	-0.01	-1.38	0.167
	27.2k to 36.6k €	0.20	0.40	0.19	0.40	0.01	0.55	0.583
	36.6k to 45.6k €	0.15	0.36	0.15	0.36	-0.00	-0.29	0.768
	> 45.6k €	0.14	0.35	0.12	0.33	0.02	2.32	0.02**
Surface area	< 50 sq.m.	0.03	0.18	0.04	0.20	-0.01	-1.44	0.149
	50 to 74 sq.m.	0.14	0.35	0.15	0.36	-0.02	-1.59	0.112
	100 to 149 sq.m.	0.40	0.49	0.36	0.48	0.03	2.30	0.022**
	> 150 sq.m.	0.18	0.38	0.19	0.39	-0.01	-0.82	0.412
Main heating fuel	Electricity	0.31	0.46	0.33	0.47	-0.02	-1.75	0.08*
	Fuel Oil	0.20	0.40	0.14	0.35	0.06	6.26	0***
	Natural Gas	0.42	0.49	0.46	0.50	-0.04	-3.25	0.001***
Heating type	Central	0.10	0.30	0.13	0.34	-0.03	-3.05	0.002***
	Individ. non-elec.	0.51	0.50	0.47	0.50	0.05	3.38	0.001***
	Individual elec.	0.28	0.45	0.30	0.46	-0.02	-1.83	0.067*
N		4273		1133				

Notes: t-stats and p-values come from t-tests of covariate mean equality between eligibility groups. All statistics calculated with inverse probability weighting using propensity scores, as well as the survey weights. Significance codes: ***0.01, **0.05, *0.1. Data source: ADEME Survey. Back to Section 5.2.

Table A13: Effect of eligibility on renovation decision, with propensity score weighting

Dependent Variable:	Renovation this year	
Model:	(1)	(2)
Variables		
Eligible	0.0885***	0.0140
	(0.0125)	(0.0232)
Eligible × 2005	-0.0369	-0.0205
	(0.0318)	(0.0210)
Eligible × 2006	-0.0213	-0.0045
	(0.0288)	(0.0194)
Eligible × 2007	-0.0091	-0.0174
	(0.0190)	(0.0176)
Eligible × 2009	0.0380**	0.0466***
	(0.0168)	(0.0148)
Eligible × 2010	0.0271	0.0512***
	(0.0174)	(0.0160)
Eligible × 2011	-0.0033	0.0260
	(0.0175)	(0.0169)
Eligible × 2012	-0.0018	0.0273
	(0.0169)	(0.0182)
Eligible × 2013	-0.0246	0.0284
	(0.0199)	(0.0223)
Controls	Yes	Yes
Fixed-effects		
Year	Yes	Yes
Household		Yes
Fit statistics		
Observations	42,415	42,415
R^2	0.05358	0.46475
Within R^2	0.05247	0.10654

Notes: Estimates of Equation (1), with inverse probability weighting using logit-estimated propensity scores, along with survey weights. Standard errors clustered at the household level. Significance codes: ***0.01, **0.05, *0.1. Data source: ADEME Survey. Back to Section 5.2.