

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

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Abstract

We provide the first evaluation of a Zero-Interest Green Loan (ZIGL) program for home energy retrofits. Applying an event-study design to a program introduced in France in 2009, we find that eligibility for the program increased retrofit investment by 22% on the extensive margin, by 4% on the intensive margin and induced 1.5% of eligible households to switch away from fossil fuel-powered heating systems. The effects however become non-significant after two years. They are primarily driven by low-income homeowners, suggesting the program effectively alleviated credit constraints. These results are robust to a range of robustness checks, including deviations from pretrends. They lead to a marginal value of public funds of 0.98-1.03 in the early period. Using additional banking data to investigate the post-2010 drop, 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 seen as a key strategy to mitigate climate change. This is especially the case in the building sector, which contributes 31% of global CO₂ emissions, 70% of which stem from housing (IPCC, 2022). With unit costs ranging from several thousand to several tens of thousand dollars, comprehensive home energy retrofits often require credit. In France, for instance, 25% of households undertaking a retrofit take out a loan, and this figure rises above 40% in the case of deep retrofits (ADEME, 2011, 2018). Subsidized loan programs are increasingly implemented around the world to close this gap in financing (Berry, 1984; Guertler et al., 2013). This includes Germany’s Energy-Efficient Refurbishment Program, the United States’ Property Assessed Clean Energy Financing program (PACE) (Rose and Wei, 2020; Millar and White, 2024) and France’s *Éco-Prêt à Taux Zéro* (Zero-Interest Green Loan).

From a public economics perspective, the rationale for subsidized green loans is twofold. On the one hand, they can be viewed as a CO₂ abatement subsidy, the rate of which depends on the interest rate that would prevail for a conventional loan financing the same investment. The implicit subsidy amount therefore fluctuates over time with interest rates. On the other hand, such loans can help address information asymmetries that exclude low-income borrowers from credit markets (Stiglitz and Weiss, 1981; Zinman, 2015). This dual rationale raises two key questions: How does the effectiveness of subsidized green loans evolve over time, and to what extent do they improve access to credit for low-income households?

In this paper, we provide the first evaluation of a zero-interest green loan (ZIGL) program for home energy retrofits. We focus on the French *Éco-Prêt à Taux Zéro*, which provides a good case study for at least two reasons. Introduced in 2009 amid falling interest rates, the program expanded rapidly before experiencing a sharp and persistent decline, which raises concerns about its overall effectiveness. Moreover, the program imposes no income limits on applicants. While this suggests that alleviating credit constraints was not as strong a motivation as the Pigouvian principle, the question remains as to whether the program provided the greatest benefit to those households most in need of financing.

We start by estimating the effect of eligibility for the program on three outcomes – the extensive margin of renovation investment, the intensive margin and energy-use patterns. Building on a unique panel of nearly 10,000 French households surveyed on their renovation behavior from 2005 to 2013, we implement an event-study design exploiting a restriction of eligibility to buildings constructed before 1990. We find that eligibility to the program increased the probability of undertaking a renovation by 4 percentage points (p.p.) in the first two years, corresponding to a 22% increase among eligible households. The magnitude and significance of the effect fades thereafter. Heterogeneity analysis reveals stronger responses among low-income homeowners (10–13 p.p.). On the intensive margin, the average number of actions taken increases from 1.4 to 1.9 and spending increases by €127–175 (+4–5%). Both effects are similarly short-lived. Finally, eligibility increased the proportion of electricity-powered heating systems by 1.8 p.p., alongside a 1.3 p.p. decline in fossil fuel systems. We confirm these findings in robustness checks, including using alternative building vintage cut-offs as placebo, allowing for deviations from parallel pre-trends, balancing the eligible and

non-eligible groups through propensity score weighting and correcting for attrition. Valued at a social cost of carbon of €115/tCO₂, these effects imply a marginal cost of public funds of 0.98, increasing to 1.03 when additionally accounting for health benefits.

To explain the post-2010 decline, we then investigate mechanisms on both the demand and supply sides of the loan market. On the demand side, we find the downturn unlikely to stem from an exhaustion of the pool of eligible participants, but rather to be due to a decline in program awareness after 2010 and, to a lesser extent, from interactions with overlapping policies. On the supply side, using data from Banque de France, we find that a 1 p.p. increase in the average interest rate charged by a bank on non-subsidized loans is associated with a 5% drop in its ZIGL production. This pattern is consistent with banks steering borrowers away from ZIGLs and into their own loan products, which have been found to be particularly highly profitable (Giraudet et al., 2021b). Such an arbitrage is only possible under imperfect information, however, since fully informed households would never choose a costly loan when eligible for a zero-interest equivalent.

We contribute to the literature at the intersection of environmental economics and household finance, and incidentally to the literature on two types of policies – energy efficiency programs and subsidized loans.¹ The literature on the former has primarily focused on assessing infra-marginal participation (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). In the French context, this includes Nauleau (2014), Risch (2020), Mauroux (2014) and Chlond et al. (2023), all focusing on a tax credit program called CITE, whose effect they find to have been stronger on the intensive than on the extensive margin. The literature on subsidized loans, in turn, has primarily focused on student loans (Cadena and Keys, 2012) and housing loans (Martins and Villanueva, 2006; Gruber et al., 2021; Gobillon et al., 2022). Their effect has similarly been found to be stronger on the intensive margin, for instance with borrowers purchasing more expensive property. We find the opposite with the French ZIGL – a stronger effect on the extensive margin, even more pronounced among low-income households, despite them not being particularly targeted. This indicates that credit constraints, a commonly-cited (Gillingham et al., 2009; Allcott and Greenstone, 2012; Gerarden et al., 2017) but under-studied (Berry, 1984; Giraudet, 2020) barrier to energy efficiency investment, are significant and effectively overcome by ZIGLs. It additionally suggests that direct subsidies and low-interest loans may aptly complement each other to support energy efficiency investment.

Our findings have important implications for managing ZIGLs. The French program is implemented through commercial banks in an effort to harness their extensive retail network. The opportunity cost they face however creates an incentive problem, which is not compensated by the benefits they might expect from cross-selling other products (Basten and Juelsrud, 2023) or strengthening customer relationships (Agarwal et al., 2018). One solution could be to increase the compensation they receive on each ZIGL. The implied transfers

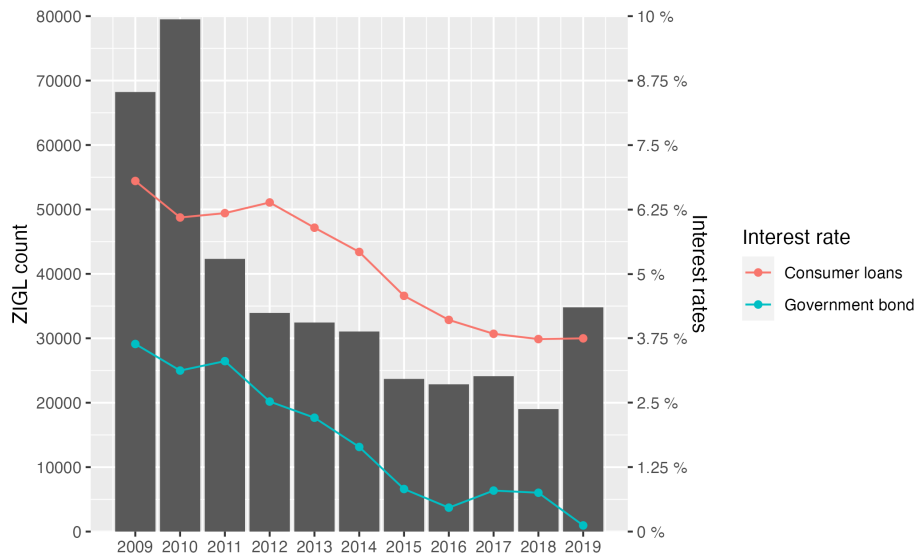
¹ZIGL programs are the very intersection of these two tools. Their analysis however remains scarce and largely descriptive (Berry, 1984; Guertler et al., 2013; Rose and Wei, 2020). One exception is Millar and White (2024), who evaluate the PACE program with a focus on real estate market outcomes. In contrast, we assess the impact of ZIGLs on renovation investment.

would nevertheless raise acceptability concerns. Another solution would be to assign the program to a dedicated public bank that would pool ZIGLs, thus generating economies of scale. In any case, increasing consumer awareness is a key factor of success.

2 The ZIGL program

The French Zero-Interest Green Loan (*Éco-prêt à taux zéro* or Eco-PTZ, hereafter ZIGL) was launched in April 2009. In the wake of the 2008 financial crisis, it was designed to boost retrofit investment while stimulating the recovery of the banking sector. The program offers interest-free loans for comprehensive home energy retrofits of up to €30,000, with no income restrictions and a maximum repayment term of 15 years. Retrofits have to combine multiple improvements to the building envelope and/or heating system. They are restricted to homes built before 1990 — a criterion absent from overlapping programs.² Loans are issued by government-approved credit institutions (hereafter, banks), who in return receive a corporate tax credit equal to the government bond rate plus a fixed spread of 1.35 percentage points. Since 2019, some of these features have changed – single renovation actions are now allowed and eligibility is extended to homes that are at least two years old. Due to data availability constraints, we leave this later period outside the scope of our analysis.

Figure 1: ZIGL provision and trends in market interest rates



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

The government envisioned 400,000 loans per year at full capacity ([Ministère de l'Écologie et al., 2009](#)), a goal later found consistent with naive micro-simulations ignoring banks' strategic behavior ([Giraudet et al., 2021a](#)). As shown in Figure 1, however, after steadily

²These include an income tax credit program called CITE, a reduction on value-added tax ([Carbonnier, 2007](#)), a program called *Habiter Mieux* for low-income households, and utility-sponsored subsidies ([Giraudet et al., 2012](#)). See [Giraudet et al. \(2021a\)](#) and [Chlond et al. \(2023\)](#) for comparative analyses.

increasing to 80,000 in 2010, the number of loans fell sharply to 42,000 in 2011. It continued to decline thereafter, down to a historic low of 19,000 in 2018. Overall, 256,440 ZIGLs were issued during the first five years of the program. With 14.7 million owner-occupier households recorded by INSEE in 2009, this is equivalent to a 1.8% participation rate. As detailed in Table A2, the average loan size increased from €16,000 to €18,000 during this period and the average loan duration rose from 8.9 to 10.5 years.

3 Empirical approach

3.1 Data

We use a self-administered panel on home energy use surveyed by the French Energy Management Agency (ADEME) between 2000 and 2013. This survey is unique in tracking home renovation decisions over time. Participation was encouraged through a customer points system, with a barbecue set as the highest-value reward. Respondents could enter or leave the panel voluntarily.

We restrict the sample to the post-2005 period to make up for reporting inconsistencies before that year³ (while keeping the full period for robustness checks). We moreover focus on homeowners, who represent over 90% of ZIGL applicants. We also exclude households that were observed only once to leverage the panel dimension of the data. After finally excluding observations with missing key variables, we end up with an unbalanced panel of 9,657 households and 45,418 observations, in which 29% of respondents appear twice and 10% are present across all nine waves.

We identify eligibility for the program using the year of dwelling construction, reported in five categories – built before 1949, 1949–1974, 1975–1981, 1982–1988, 1989 to “the year before [the survey year].” We classify the first four categories as eligible for ZIGL. We expect the bias implied by the one-year inaccuracy in the eligibility cut-off (1989 instead of 1990) to be negligible (downward if anything). The distribution of home vintages otherwise remains stable over time.

Table A1 describes the variables used in the analysis. Income categories are not reported with stable cut-offs across years. We consolidate them but cannot avoid minor overlap between the €23,000/€22,800 and €27,600/€27,200 cut-offs. We impute missing income for 4% of the sample and missing surface area for 3% of the sample through a chained equations ordered logit procedure (van Buuren and Groothuis-Oudshoorn, 2011).

Table A3 presents summary statistics for 2008 (before the program) and 2013 (four years after the program’s launch). 80% of households are eligible for the program, 75% live in multi-family housing and 40% use individual natural gas heating systems. Income is relatively evenly distributed. The proportion of renovating households slightly declined from 17% in 2008 to 15% in 2013.

³Renovation expenditure is missing in 2004 and the breakdown by type of works is unavailable in 2002 and 2003.

3.2 Empirical strategy

We implement an event-study design to identify the impact of access to a ZIGL on several dimensions of renovation investment. Our identification strategy exploits the eligibility restriction on home vintage – units built before 1990 form the treatment group, those built in 1990 or later form the control group. We then compare renovation outcomes for both groups before and after the program’s introduction in 2009. We factor in the sampling weights provided in the dataset to ensure our estimates are representative of the population of French homeowners.

We estimate the following regression model:

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

where $R_{i,t}$ is the renovation outcome and $\text{Eligible}_{i,t}$ equals one if the dwelling was built before 1990.⁴ $X_{i,t}$ is a vector of time-varying controls, τ_t are year dummies, and μ_i are respondent fixed effects. Our parameters of interest are the β_t , which capture the dynamic effects of eligibility on renovation outcomes. We thus estimate the impact of the *intention to treat* rather than the direct effect of the program.⁵ In all regressions, we cluster standard errors at the household level.

We use several outcome variables to measure renovation investment, defined in the survey as “works aimed at reducing energy consumption or improving comfort (heating, hot water, insulation, ventilation, etc.).” To capture the extensive margin, we construct a binary variable equal to 1 if the household undertook any renovation in a given year. To capture the intensive margin, we use three variables: the total amount spent on renovation, the probability of undertaking a *costly* renovation, and the number of renovation actions completed.

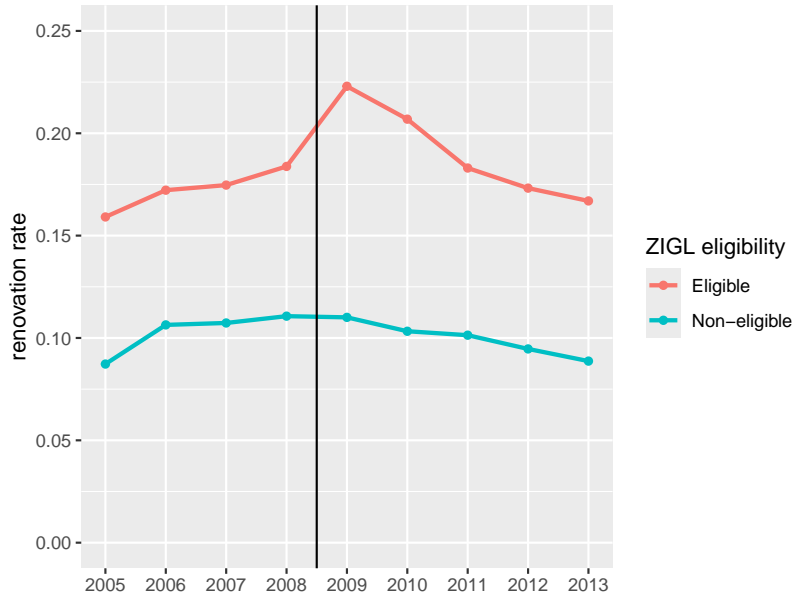
As shown in Figure 2, the proportion of renovating households remains stable at around 10% for the control group but sharply rises for the eligible group immediately after the program’s introduction. The trends are similar before 2009 and diverge thereafter, suggesting our control group provides a valid counterfactual. We test the parallel-trends assumption more formally in the regressions presented below. Figure A1 further shows renovation rates by house vintage, dividing the eligible group into four age brackets. Although pre-trends across vintage groups are somewhat noisier, the sample-size-weighted series indicate that the most noticeable deviations from parallel trends occur in the smaller subgroups.

The vector $X_{i,t}$ includes the covariates listed in Table A1 including household head’s age and occupation, income of the household; dwelling surface area; heating system type and fuel used; settlement type (proxied by population size); and region of residence. We also control for pre-treatment renovations – arguably a strong predictor of future investment – using a dummy for renovation in the nine years prior to the survey, with 2001 as the earliest and

⁴Eligible_{*i,t*} can vary over time because some households move and can thus become ineligible. Movers represent about 6% of households. We verify the absence of strategic moves by confirming that the probability of moving to an eligible dwelling remains stable over time.

⁵Estimating the actual treatment effect would require observing program participation and instrumenting it, for example using our event-study setup. As detailed in the Online Appendix (Section A), the data on participation are subject to measurement error and too scarce to allow for a reliable IV strategy. Notwithstanding this, our estimates are consistent with the program dynamics shown in Figure 1.

Figure 2: Renovation rates by treatment group, 2005-2013



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

2008 as the latest year.⁶ We run a balancing test to compare the demographic and housing characteristics of the eligible and non-eligible groups. As reported in Table A4, most variables differ statistically between the two groups. While this is not a challenge for our identification strategy, which only relies on the common-trend assumption, it nevertheless 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.

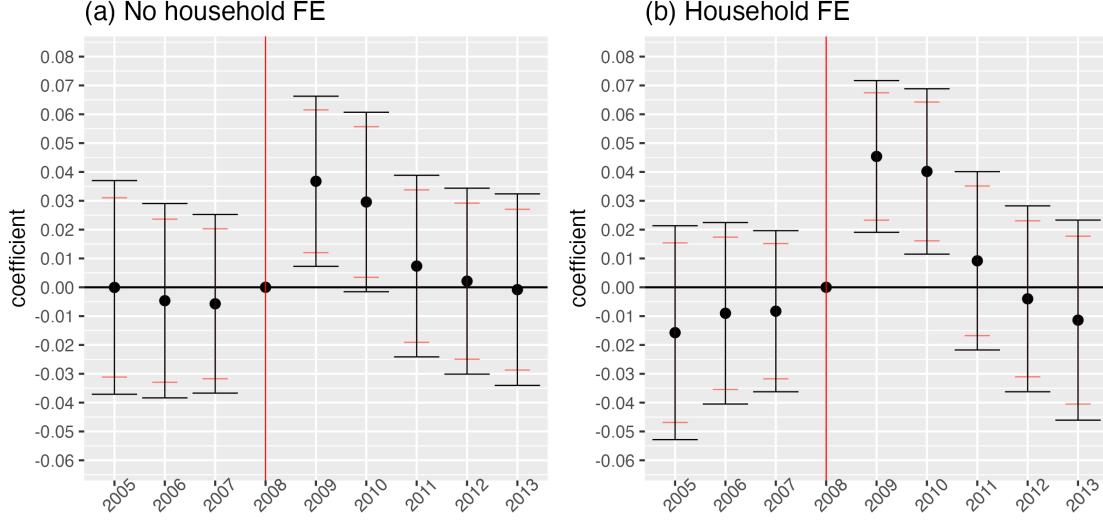
4 Results

Extensive margin As shown in Figure 3, eligibility for the program has no statistically significant effect between 2005 and 2007, confirming the parallel pre-trends assumption. In the fixed-effects specification, the renovation rate then increases by 4.5 percentage points (p.p.) in 2009 and 4 p.p. in 2010 for eligible households relative to non-eligible ones, before returning to non-significant levels in subsequent years. The early effects amount to a 22% increase in the renovation rate among eligible households.⁷ They are slightly lower without fixed effects – 3.7 p.p. in 2009 and 3 p.p. in 2010. Importantly, Table A5 confirms that accounting for past renovations is crucial, since previous works strongly reduce the likelihood of new renovations.

⁶Holding this variable constant after 2008 is meant to avoid using a “bad control” that could be affected by the treatment.

⁷A robustness test extending the sample to 2001–2013 yields identical results, save for non-systematically parallel pre-trends before 2005 (see Figure A2).

Figure 3: Effects of eligibility on the renovation decision



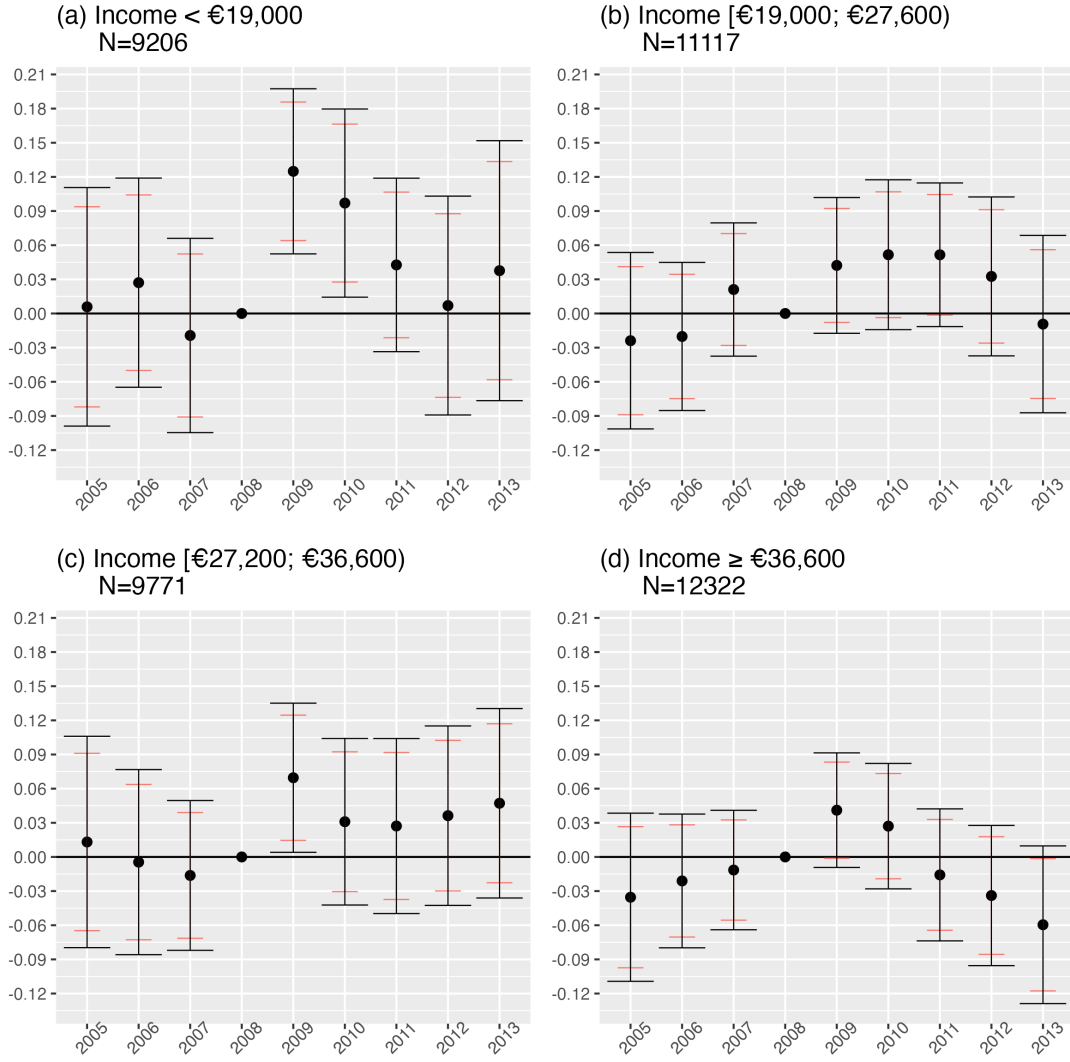
Notes: Estimates from the event study of Equation 1, with the renovation dummy as the dependent variable. Confidence intervals: 95% in black and 90% in red. Specification: (a) with household controls, without fixed effects; (b) with household fixed effects and time-varying controls. Time fixed effects are included in both specifications. Controls include all variables in Table A1 and an indicator for past renovations before the ZIGL program. Standard errors are clustered at the household level. Source: ADEME survey.

Heterogeneity by household income Despite the absence of income ceilings in the eligibility criteria, heterogeneous households may have benefited differently from the program. To compare outcomes across income categories with sufficient statistical power, we aggregate the six income categories used in the regression (see Table A1) into four – below €19,000, €19,000–€27,600, €27,200–€36,600, and above €36,600. We then estimate Equation 1 separately for each income group. As shown in Figure 4, low-income homeowners exhibit by far the biggest increase in their probability of renovating — 12.5 p.p. in 2009 and 9.7 p.p. in 2010. The upper-middle income group exhibits a smaller but also statistically significant increase of about 7 p.p. in 2011. High-income households exhibit no effect. These results are robust to excluding observations with imputed income values. They are moreover confirmed in a triple-difference specification reported in Table OA1 where the four income categories are interacted with the eligibility variable and a $Post_t$ dummy equal to one for 2009–2010.

These results support the hypothesis that credit constraints are binding among low-income homeowners and that the ZIGL program effectively relaxed them—perhaps unintentionally, since eligibility was not conditioned on income. One possible mechanism is that low-income households responded to lower borrowing costs by substituting professional renovation work for do-it-yourself or informal labor, as previously documented by Lindner et al. (2022). We provide supporting evidence for this hypothesis in the Online Appendix, where we examine heterogeneity by renovation type (Figure OA3).

Renovation expenditure A natural way to capture the intensive margin of investment is through renovation expenditure. In our dataset, this variable is reported as categorical, with wide intervals at the upper end of the distribution. We make it continuous by assigning

Figure 4: Heterogeneous effects by income group



Notes: Estimates from the event study of Equation 1, for four income groups. Confidence intervals: 95% in black, 90% in red. All regressions include household fixed effects, time fixed effects, and time-varying controls. Controls comprise all variables listed in Table A1 and an indicator for past renovations before the ZIGL program. Standard errors are clustered at the household level. Source: ADEME survey.

each category its midpoint value, with a maximum of €6,098 and a minimum of zero in the absence of renovation. As shown in Figure A3a, eligibility for the program increases spending by €127 in 2009 and €175 in 2010, equivalent to a 3-5% increase relative to the €3,816 spent on average by eligible households in 2008. In the Online Appendix, we additionally find that “costly renovations” exceeding €4,693 were more strongly stimulated than those not categorized as costly in 2009 (Figure OA4).

Number of renovation actions A second proxy for the intensive margin is the number of renovation actions undertaken, which should also respond to eligibility since combining several actions was a program requirement. We estimate a Poisson regression of the count of actions, including the same set of controls and fixed effects as in the baseline specification. As shown in Figure A3b, the average number of actions increased significantly by 39 %, i.e.,

from 1.4 in 2008 to 1.9 in 2010. Note that the Poisson model with respondent fixed effects excludes households that never renovated, representing 55% of observations.

Fuel switch Eligible actions include replacing fossil fuel-based heating systems with more efficient electric ones. To examine the prevalence of this measure, we estimate Equation 1 using an indicator for the household’s main energy source for heating and including household fixed effects to ensure that we capture within-household switches across fuels rather than cross-sectional differences in energy sources. The prevalence of electricity shows no pre-trend and increases significantly by 1.8 percentage points (p.p.) in 2009 (Figure A4a). Meanwhile, the prevalence of oil and gas heating declines by 1.4 p.p. in 2009 and 0.8 p.p. in 2010 (A4b), with some minor pre-trends. This effect is substantial, given an unconditional probability of switching away from fossil fuels of only 3.6% over the 2009–2013 period. These trends together widen the gap between electric and fossil-based systems.

Electricity consumption Energy savings and CO₂ emissions reductions are a key outcome of interest for policy evaluation. Unfortunately, as detailed in the Online Appendix (Section E), data issues only allow us to examine electricity consumption, with some restrictions. With these caveats in mind, we find a marginally significant reduction in electricity spending of 2.7% in 2009 and 3.9% in 2010, even more pronounced among low-income households. This, together with the identified switch away from fossil fuels, suggests CO₂ emission reductions are meaningful.

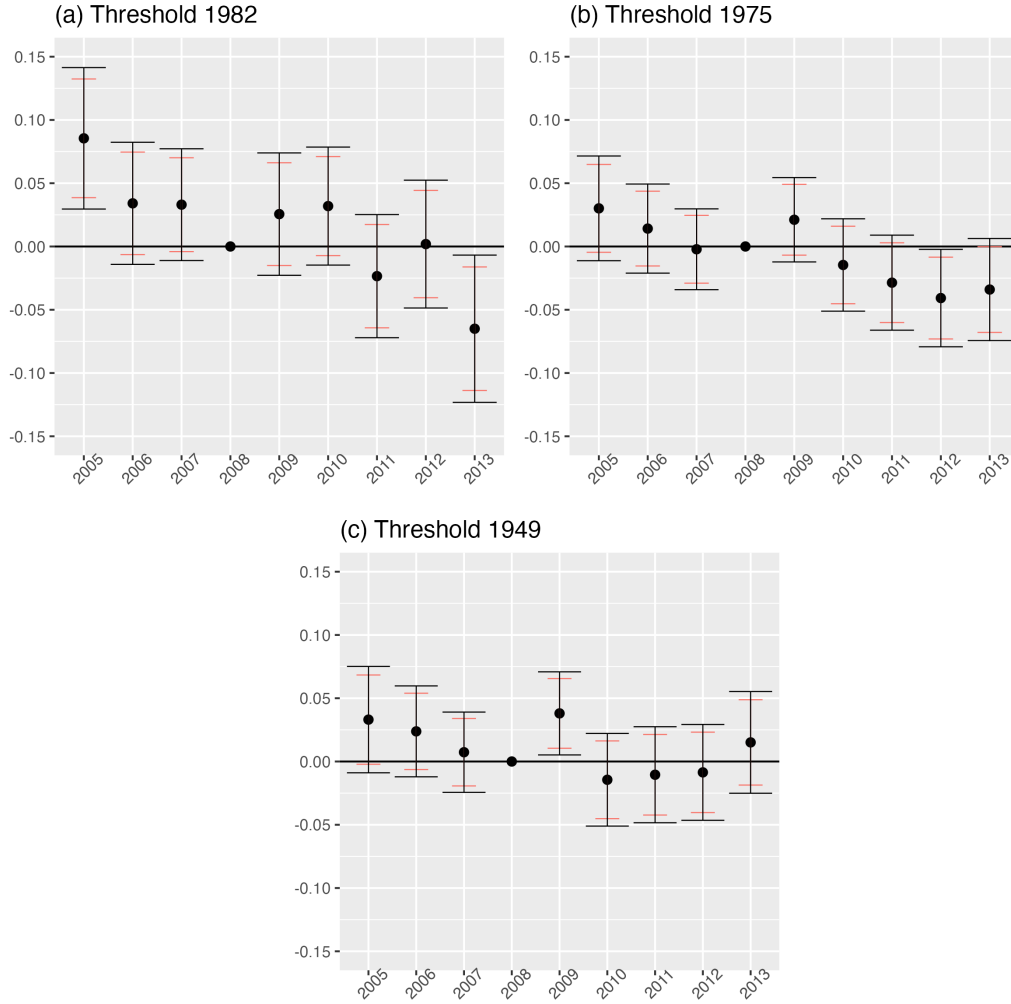
Marginal value of public funds Our estimated effects can be used to compute the marginal value of public funds (MVPF) associated with the program. As detailed in the Online Appendix (Section F), we start by computing leverage and find that one euro granted to banks for issuing a ZIGL induced €1.75 of additional household spending on renovation. Assuming a social cost of carbon of €115/tCO₂, these estimates lead to an MVPF of 0.98, in line with the 0.86-1.21 range exhibited by Hahn et al. (2024) for weatherization programs in the United States. When additionally accounting for the reduced exposure to cold-related illnesses permitted by retrofits, the MVPF rises to 1.03, and even 2.12 when the social cost of carbon is raised to €500/tCO₂.

5 Robustness checks

Placebo test To assess the validity of our treatment variable, we conduct a series of placebo regressions using fictional eligibility criteria. The six construction periods defined in our data (see Table A1) allow us to generate three alternative treatment-control partitions: pre-/post-1982, pre-/post-1975, and pre-/post-1949. We use each of them in placebo regressions, focusing on the extensive margin and excluding post-1989 units from the fictional control group to minimize the risk of capturing the true effect of eligibility. As shown in Figure 5, most coefficients around the program’s implementation date are not statistically different from zero, suggesting both groups followed parallel renovation trends. Exceptions include significant differences in 2005 and 2013 for the 1982 cut-off (5a), in 2012 for the 1975

cut-off (5b) and in 2009 for the 1949 cut-off (5c). The latter is of the same magnitude as the main estimate in Figure 3 (3.8 p.p.), suggesting our baseline 2009 effect may be partly driven by older dwellings.

Figure 5: Placebo differences-in-differences, extensive margin



Notes: Placebo event-study estimates of Equation 1 for the binary renovation decision, with 95% (black) and 90% (red) confidence intervals. All regressions exclude the true control group (buildings constructed after 1990). Placebo eligibility criteria: (a) houses built before 1982; (b) houses built before 1975; (c) houses built before 1949. All regressions include household fixed effects and the control variables listed in Table A1, as well as an indicator for past renovations before the ZIGL program. Standard errors are clustered at the household level. Source: ADEME survey.

Restricted treatment To test whether older houses drive our main effect, we re-estimate the baseline specification on a sample that excludes pre-1949 units. As shown in Figure OA8a, this leaves the 2010 effect essentially unchanged but attenuates the 2009 effect, confirming the oldest dwellings played a substantial role that year. In the Online Appendix (Section G), we further restrict the treated group by excluding all pre-1975 units, in order to better balance it with the control group. This further confirms the importance of older houses in 2009, as well as the robustness of our baseline results to their exclusion in 2010.

Flexible pre-trends analysis We test the robustness of our results to violations of exact parallel pre-trends using the approach proposed by [Rambachan and Roth \(2023\)](#). Applying their method to the average post-treatment effect, we consider two types of permissible deviations from an extrapolated pre-trend: (i) *smoothness restrictions* and (ii) *relative magnitude restrictions*. Under the smoothness restriction, our main coefficient remains statistically significant when allowing the difference in counterfactual trends to deviate by up to 0.6 p.p. from the linear extrapolation of pre-trends (Figure A5a). Under the relative magnitude restriction, the effect remains robust even when deviations reach 90% of the maximum pre-treatment difference in each post period (Figure A5b). When deviations equal the largest pre-treatment gap, the coefficient becomes only marginally insignificant at the 5% level.

Other robustness tests In the Online Appendix, we find that attrition is unlikely to bias our results – while eligible households in the 2008 cohort are slightly more likely to remain in the sample, applying inverse probability weighting yields estimates nearly identical to the baseline (Section H). We furthermore confirm our results using propensity score weighting to address covariate imbalance between the treatment and control groups (Section I).

6 Mechanisms

6.1 Demand side

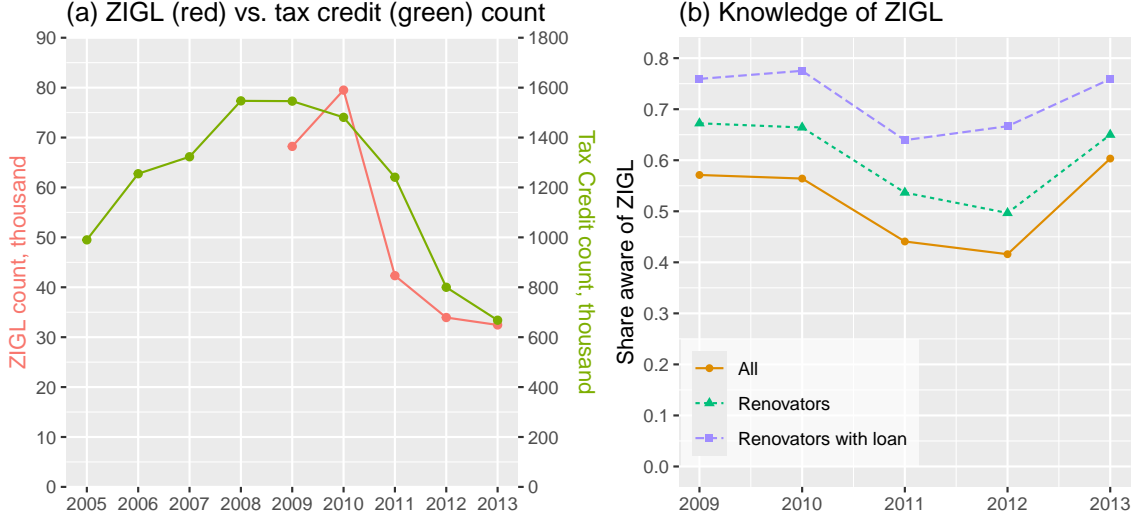
Exhaustion of the pool of marginal participants A natural hypothesis to explain the 2011 downturn is that the pool of eligible homeowners willing to participate was quickly exhausted. This seems unlikely, however, considering that the 128,000 ZIGLs issued in 2009 and 2010 represent only 1.3% of the 9.6 million homeowners eligible in 2011.⁸ One might counter-argue that these 128,000 beneficiaries were precisely the marginal households targeted by the program. Still, a simple comparison with the CITE program suggests that the pool of marginal participants is much broader. Considering a conventional loan of €17,000 incurring a 6% annual interest rate – the prevailing rate in 2011 – over a 110 month repayment period, a ZIGL corresponds to an implicit subsidy of about €5,000, hence a subsidy rate of 23% of the total loan cost.⁹ In contrast, with subsidy rates in the same 15%-30% range, the CITE program benefited 1.25 million homeowners in 2011 alone (see Figure 6a) – almost 10 times the number of ZIGL beneficiaries in the first two years.

Policy interference The conditions for jointly benefiting from the ZIGL and CITE programs have varied over time, which might have created interference. Joint claims were allowed in 2009 and 2010 for households below the €45,000 income ceiling, disallowed in 2011 and reallocated in 2012 and 2013 with a €40,000 income ceiling. While we do observe a drop in ZIGL-eligible renovations in 2011, no rebound occurs when the two programs could be claimed together again (Figure 6a). Still, the ZIGL decline seems to follow that of CITE subsidies. We therefore cannot rule out that policy interference was significant.

⁸Even restricting our attention to households below the €19,000 income ceiling only yields 4.6%. Data come from the Filocom administrative dataset, only individual houses built before 1990 are included.

⁹Table OA2 provides yearly values for the subsidy rate.

Figure 6: Demand-side mechanisms



Notes: ZIGL data come from the program administrator (SGFGAS), CITE data from [Waysand et al. \(2017\)](#). Respondents in the 2011 knowledge sample: 4,646 overall, 792 renovators, 122 renovators with loans.

Imperfect information Limited information about the program is another possible explanation for low uptake. As shown in Figure 6b, 57% of households were aware of the program in 2009 and 2010. This rate was even higher among renovators, and especially the borrowers among them. It nevertheless declined to 42% in 2011-2013. Given the central role played by commercial banks in the program, this fall might be due to their reducing their advertising efforts. In the following section, we explore why and how this might have been the case.

6.2 Supply side

The opportunity cost of banks As shown in Figure 1, the gap between the rate on government bonds and that on consumer loans has consistently been in the 3.5 p.p. range, hence much larger than the 1.35 p.p. used by the government to compensate banks. Issuing a ZIGL instead of one of their own products therefore exposed banks to a roughly 2 p.p. opportunity cost. To assess whether this impacted ZIGL production, we examine the association between the production of ZIGL by a given bank in a given local market and its opportunity cost, measured as the weighted average interest rate on its loans to local households. In a quasi-Poisson regression with bank, local market and quarter fixed effects as well as time-varying controls for local markets, we find that a 1 p.p. increase in opportunity cost is associated with a 5% decline in ZIGL issuance by a given bank in a given area (Table 1). The effect remains virtually unchanged when controlling for bank-time fixed effects. We interpret this result as suggestive evidence that loan officers nudge ZIGL applicants towards traditional, interest-bearing credit products to finance renovations. For this mechanism to operate, however, consumers must be imperfectly informed about ZIGLs; otherwise, they would never choose a costly loan when eligible for a zero-interest alternative. The data and

model are further detailed in the Online Appendix (Section J).

Table 1: Effect of banks' opportunity cost on ZIGL activity

Dependent Variable: Model:	Nb of ZIGL		
	(1)	(2)	(3)
<i>Variables</i>			
Opportunity cost	-0.2177*** (0.0461)	-0.0463* (0.0251)	-0.0458* (0.0268)
Controls	Yes	Yes	Yes
<i>Fixed-effects</i>			
Time	Yes	Yes	
Bank		Yes	
Local Market		Yes	Yes
Bank \times time			Yes
Observations	14,726	14,726	14,726
Pseudo R ²	0.206	0.475	0.496

Notes: Estimates of the Poisson regression [OAJ7](#) with different sets of fixed effects. Opportunity cost is measured as a weighted average of a bank's consumer loan and housing loan interest rates in a local market, in a given quarter (see Equation [OAJ6](#)). Significance codes: ***0.01, **0.05, *0.1. Bank \times Market standard-errors in parentheses. Data sources: Banque de France (M_CONTRAN and FECA datasets), INSEE (census results), ZIGL administrator.

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. 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 number of ZIGLs.

7 Conclusion

Launched in 2009, France's Zero-Interest Green Loan (ZIGL) program aimed to stimulate home retrofit investment by expanding access to credit. Using a rich panel dataset and an event-study design, we find that these objectives were reached in the early years. Eligibility

for the program boosted retrofit investment on the extensive margin (+22%) and, to a lesser extent, on the intensive margin (+4–5%), leading 1.5% of eligible households to switch from fossil fuels to cleaner heating systems. These effects imply a marginal value of public funds of roughly 1, suggesting the €200 million annually spent on the program efficiently reduced environmental externalities. The program primarily benefited low-income homeowners, despite not particularly targeting them, which testifies to its effectiveness at alleviating credit constraints. In this regard, it aptly complements direct subsidies, which tend to preferentially stimulate the intensive margin.

In 2011, however, ZIGL production collapsed, only to recover in 2019 – a period outside the scope of our analysis. This decline cannot be fully explained by falling market interest rates. Instead, we find suggestive evidence that banks reduced their advertising of the program in an effort to steer borrowers into their conventional, interest-bearing products. This interpretation is supported by a concomitant decline in public awareness of the program. 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.

These results raise important questions about program design. Are commercial banks the best vehicle for low-interest green loans? Germany and the United States -unlike France- rely on public lenders. Taking the same approach could help overcome the incentive problem while harnessing economies of scale from pooling applications. Another issue is whether increasing the banks' compensation would increase take-up, which additionally raises acceptability issues. In any case, improving information provision may be a fairer and more cost-effective way to boost participation, since fully informed households would never choose a costly loan over a zero-interest option. These lessons are crucial as policymakers are considering extending ZIGL programs to other assets, such as electric vehicles.

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 and the restriction on eligibility have been considerably eased, the maximum amount and repayment period have been increased and so has the banks' compensation. Meanwhile, energy prices and interest rates have increased, thus increasing the implicit benefit of ZIGLs. As a result, participation gradually rose from about 36,000 loans in 2019 to 113,000 in 2024, while the average amount borrowed is about 10% lower. Further analysis is required to assess how these new conditions have affected the program's performance.

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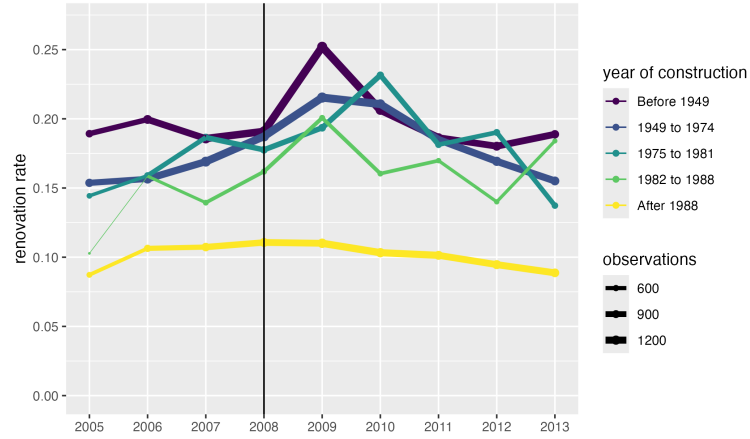
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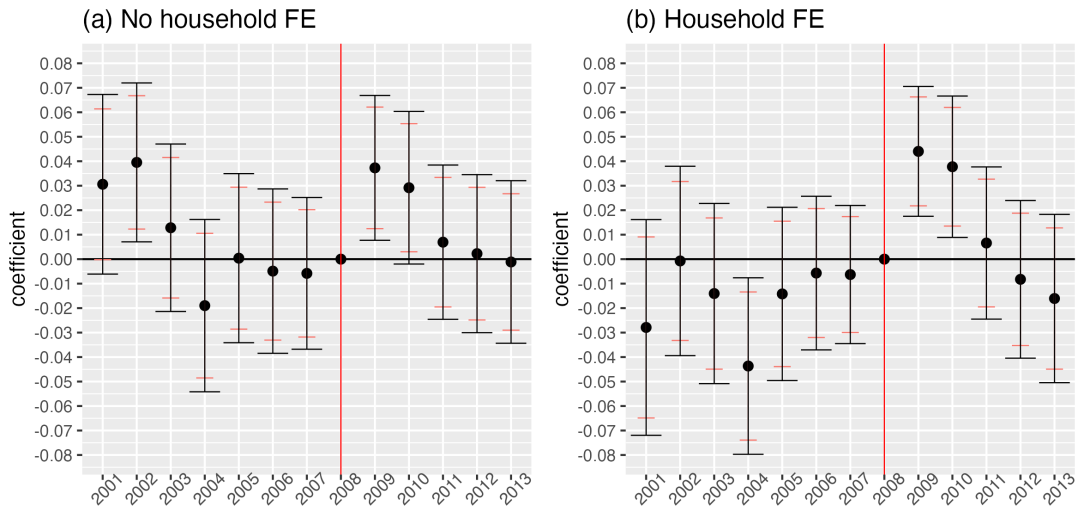
In-print Appendix

Figure A1: Renovation rates by house age interval



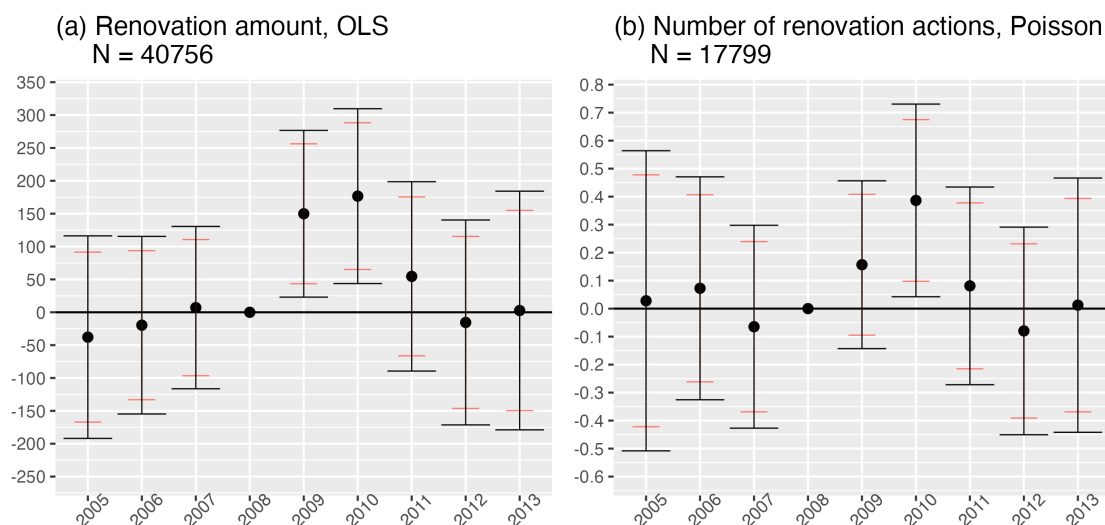
Survey weights are applied to the rates calculation. Data source: ADEME survey.

Figure A2: Effects of eligibility on renovation decision, extended



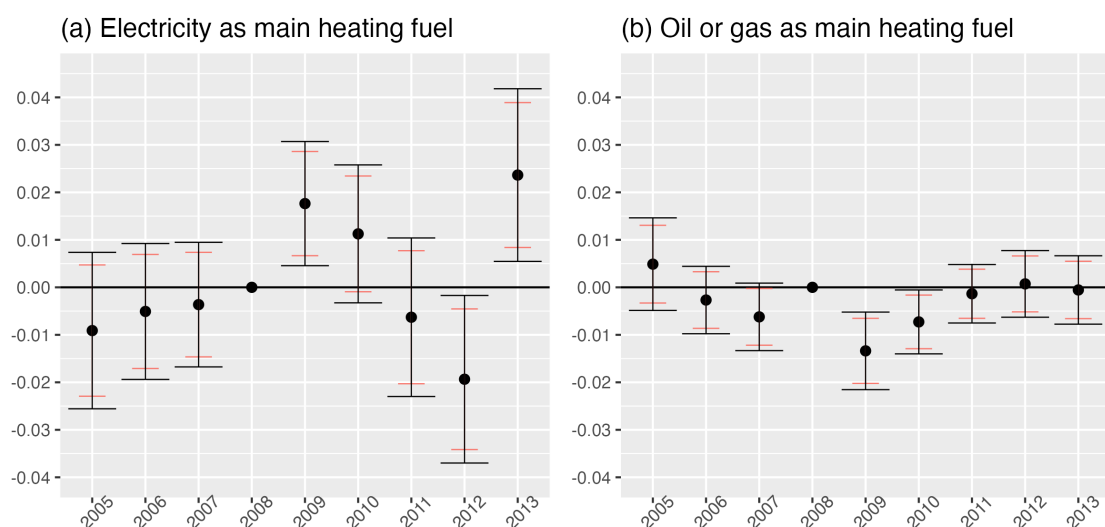
Notes: Estimates for the event study of Equation 1 on sample 2001-2013, with the renovation dummy as the dependent variable. Confidence intervals: 95% in black, 90% in red. Specification: (a) with household controls (both constant and time-varying), but no household FE; (b) with household FE and time-varying controls. Time FE used in both specifications. Standard errors clustered at the household level. Controls are all variables of Table A1 and an indicator for past renovations pre-ZIGL. Data source: ADEME survey.

Figure A3: Effect of eligibility on the intensive margins of renovation.



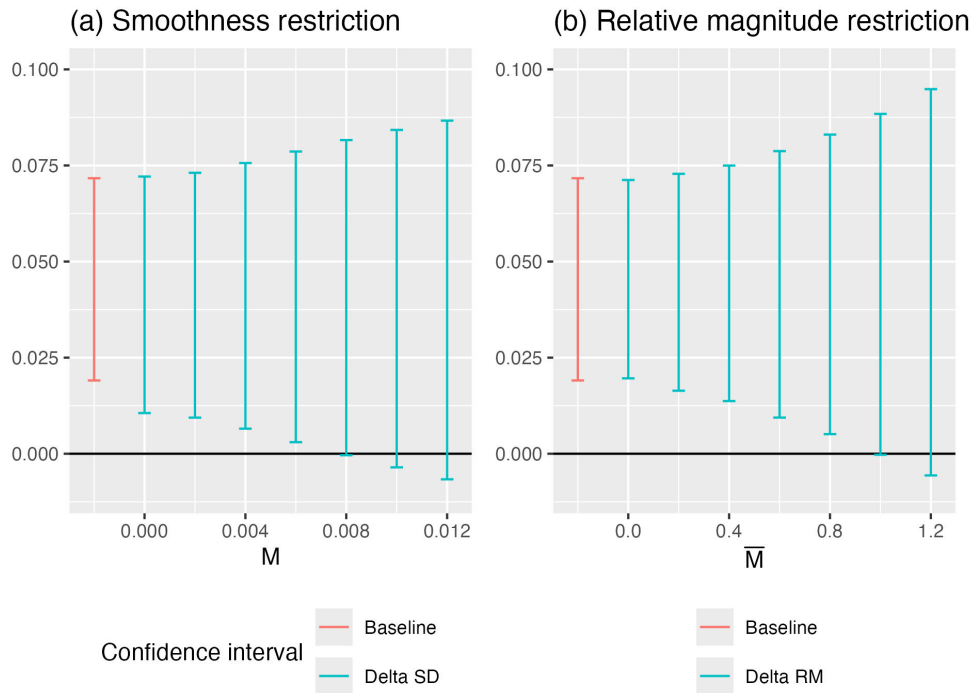
Notes: Event-study estimates of renovation amounts and the number of renovation actions. Renovation amount: OLS regression with household fixed effects and time-varying controls, N=40,755. Number of actions: quasi-Poisson regression with the same explanatory variables, N=19,586. Survey weights are applied. Controls are variables of Table A1 and an indicator for past renovations pre-ZIGL. Standard errors clustered at the household level. Data source: ADEME Survey.

Figure A4: Effects of eligibility on heating fuel switching.



Notes: Estimates for the event study of Equation 1, with heating by electricity/oil or gas as the dependent variable. Confidence intervals: 95% in black, 90% in red. Household and time fixed effects used in both regressions. Standard errors clustered at the household level. Data source: ADEME survey.

Figure A5: Robust confidence intervals ([Rambachan and Roth, 2023](#))



Notes: Estimates for the average of the $\beta_{2009} \dots \beta_{2013}$ effects of Equation 1, allowing for small violations of the parallel trends hypothesis, following ([Rambachan and Roth, 2023](#)). Panel (a) allows any linear pre-trend consistent with the data to continue in the post period, with additional uncertainty of $\pm M$ times the value of the trend. Panel (b) allows the largest deviation from parallel trends found between any two consequent pre-treatment dates, multiplied by \bar{M} , to occur between any post-treatment dates. Data source: ADEME Survey.

Table A1: Description of categorical variables.

Variable	Values
<i>ZIGL eligibility</i>	
Dwelling construction period	Before 1949; 1949 to 1974; 1975 to 1981; 1982 to 1988; 1989 to survey year−1; survey year
<i>Control variables</i>	
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 (<i>PCS</i>)	Agricultural; Trade/entrepreneur; Independent/management; Intermediary; Employee; Worker; Non-employed*
Income of the household	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
<i>Descriptive statistics</i>											
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
Total implied subsidy, M€	190.06	199.08	115.206	83.26	68.95	61.71	34.18	26.31	31.04	25.28	21.71
Implied subsidy rate	0.17	0.15	0.16	0.14	0.12	0.12	0.08	0.07	0.07	0.07	0.05
<i>Years studied in the different analyses</i>											
Baseline analysis (Sections 4 and 5)	X	X	X	X	X						
Demand-side analysis (Section 6)	X	X	X	X	X	X	X	X	X	X	X
Supply-side analysis (Section 6.2)				X	X	X	X	X	X	X	X

Notes: Data source: program administrator (SGFGAS).

Table A3: Summary statistics for 2008 and 2013

Variable	Category	2008		2013	
		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
Multi-family unit	Yes/No	0.25	0.43	0.26	0.44
Municipality category	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
Age of head	Rural	0.30	0.46	0.29	0.45
	< 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
Occupation of household head	> 65 y.o.	0.34	0.47	0.37	0.48
	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
Income	White-collar worker	0.07	0.26	0.08	0.27
	< 19k €	0.22	0.42	0.20	0.40
	19k to 22.8k €	0.14	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 €	0.15	0.36	0.13	0.34
Surface	> 45.6k €	0.14	0.35	0.18	0.38
	< 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.	0.39	0.49	0.37	0.48
Heating main energy	> 150 sq.m.	0.17	0.38	0.14	0.34
	Electricity	0.31	0.46	0.32	0.47
	Fuel Oil	0.20	0.40	0.17	0.38
Heating type	Gas	0.42	0.49	0.39	0.49
	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
N		5406		4295	

Notes: Survey weights are applied. Data source: ADEME Survey.

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

Variable	Category	Eligible (T)		Non-Eligible (C)		Diff	T-stat	p-value
		Mean	SD	Mean	SD			
Multi-family unit	Yes/No	0.27	0.44	0.19	0.39	0.07	4.99	0***
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: Extensive margin: robustness to past renovations

Dependent Variable:	Renovation this year		
Model:	(1)	(2)	(3)
<i>Variables</i>			
Eligible \times 2005	-0.0157 (0.0189)	-0.0111 (0.0193)	0.0107 (0.0202)
Eligible \times 2006	-0.0090 (0.0160)	-0.0048 (0.0166)	0.0067 (0.0179)
Eligible \times 2007	-0.0083 (0.0143)	-0.0062 (0.0146)	-0.0033 (0.0161)
Eligible \times 2009	0.0454*** (0.0134)	0.0367*** (0.0135)	0.0308** (0.0149)
Eligible \times 2010	0.0402*** (0.0146)	0.0393*** (0.0152)	0.0265 (0.0161)
Eligible \times 2011	0.0092 (0.0158)	0.0167 (0.0164)	-0.0018 (0.0172)
Eligible \times 2012	-0.0040 (0.0164)	0.0100 (0.0172)	-0.0137 (0.0178)
Eligible \times 2013	-0.0114 (0.0177)	0.0131 (0.0186)	-0.0166 (0.0187)
Renovation last 9 years pre-2009	-0.4122*** (0.0112)		
Renovation last 9 years		-0.3643*** (0.0085)	
<i>Fixed-effects</i>			
Year	Yes	Yes	Yes
Household	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	42,416	42,416	42,416
R ²	0.42015	0.43738	0.37410
Within R ²	0.07791	0.10530	0.00467

Notes: Estimates from the event study of Equation 1, with the renovation dummy as the dependent variable. Controls include all variables in Table A1 and: an indicator for renovations over last 9 years, considering only year before the ZIGL program (model 1) or an indicator for renovations over last 9 years (model 2) no control for past renovations (model 3). Standard errors are clustered at the household level. Source: ADEME survey.

Online Appendix

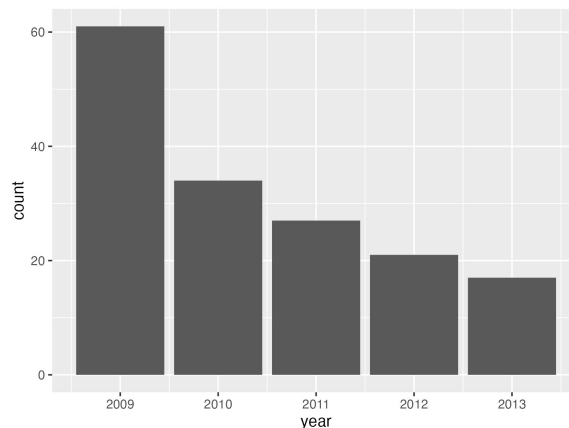
A ZIGL users in the ADEME survey

The ADEME survey includes a question on ZIGL utilization, which is asked only to respondents who report both undertaking a renovation and using a loan to finance it. As a result, this question effectively targets around 5% of renovators, whereas national statistics for the same period indicate that 35–42% of renovators relied on borrowing (ADEME (2011), p. 20). This discrepancy, combined with the fact that nonresponses cannot be distinguished from negative answers, suggests that ZIGL participation is likely under-reported in our data. With these limitations in mind, the reported number of participants is 160. This yields an F-statistic of 4.34 in the first-stage regression, preventing us from conducting an IV estimation.

Despite the small sample, these figures remain consistent with the orders of magnitude and patterns identified in our analysis. First, the 160 ZIGL beneficiaries represent 2.0% of the 8,027 renovators in the sample, which is close to the 1.7% of owner-occupiers implied by Figure 1 for the 2009–2013 period. Second, given potential under-reporting, this 2.0% share can be interpreted as a lower bound of the true participation rate, which our estimates place closer to 4%. For instance, the 61 beneficiaries reported in 2009 would correspond to an increase in retrofit rates of about one percentage point under perfect reporting. Our estimate for that year has a lower bound of two percentage points, consistent with under-reporting in the survey data. Third, the temporal pattern of reported participation, shown in Figure OA1, closely mirrors the yearly counts presented in Figure 1, and aligns with our finding of short-lived effects on the extensive margin.

Finally, to assess whether ZIGL participants are indeed more likely to be low-income households—as suggested by our heterogeneity results—we draw on administrative data from the program and compare the income distribution of beneficiaries to national statistics for French homeowners (INSEE). ZIGL users have significantly lower average incomes (€28,640 versus €37,815), and a larger share fall below an annual income of €19,000 (34.5% versus 27.0%). These differences support our conclusion that the program was particularly effective among low-income households.

Figure OA1: Number of ZIGL beneficiaries in sample



B Heterogeneity: By income levels

In a triple-difference specification applied to the 2005–2010 subsample, where the four income categories are interacted with the eligibility variable and a $Post_t$ dummy equal to one for 2009–2010, the lowest-income group is 6.2 p.p. more likely to renovate than the next group, which itself is 4.4 p.p. more likely to renovate after 2009. Because low-income households are also more likely to live in older dwellings, we verify that housing age is not driving these heterogeneous effects. As shown in columns 3 and 4, the positive and significant coefficient for low-income homeowners is reinforced when the oldest dwellings are excluded from the sample.

Table OA1: Heterogeneity of extensive margin effects: triple differences

Dependent Variable:	Renovation this year			
	All houses		No pre-1945 houses	
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Eligible	0.1083*** (0.0142)	0.0095 (0.0240)	0.0924*** (0.0149)	0.0378 (0.0258)
Eligible \times Post	0.0191 (0.0226)	0.0435* (0.0238)	0.0255 (0.0242)	0.0368 (0.0255)
Eligible \times Post \times Income < 19k	0.0704** (0.0330)	0.0616* (0.0348)	0.0750** (0.0355)	0.0773** (0.0381)
Eligible \times Post \times Income [27.2k, 36.6k)	0.0042 (0.0318)	-0.0184 (0.0316)	-0.0117 (0.0341)	-0.0181 (0.0346)
Eligible \times Post \times Income \geq 36.6k	0.0114 (0.0294)	0.0063 (0.0317)	0.0127 (0.0316)	0.0222 (0.0339)
Controls	Yes	Yes	Yes	Yes
<i>Fixed-effects</i>				
Year	Yes	Yes	Yes	Yes
Household		Yes		Yes
<i>Fit statistics</i>				
Observations	28,767	28,767	21,374	21,374
R ²	0.04440	0.50804	0.04187	0.50074
Within R ²	0.04249	0.13159	0.03986	0.11830

Notes: Estimates of triple differences in differences, obtained by interactions of the income variable with the Eligibility_{*i,t*} and Post_{*t*} variables. The years 2011-2013 are excluded since the aggregate effect is not found for that period. Income category from €19,000 to €27,600 — 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.

C Heterogeneity: By action type

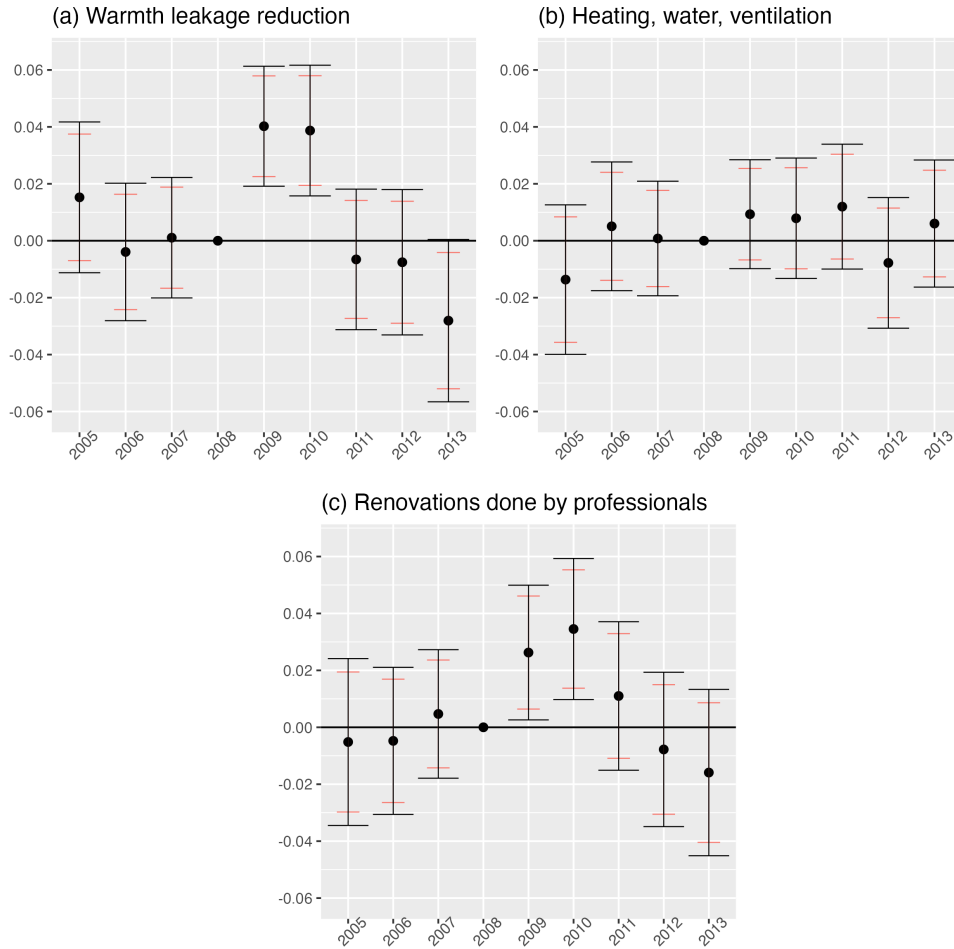
We now examine the heterogeneous effects of eligibility across different types of renovation actions. We begin with the technical components of renovation projects, which we group into two broad categories (from the 32 detailed in the dataset): (i) reduction in warmth leakage,¹⁰ and (ii) upgrades to heating, water heating, or ventilation systems. For each category, we construct a binary variable equal to one if

¹⁰This category includes: internal wall insulation; external wall insulation; roof, attic, floor, or ceiling insulation; duct sealing; window insulation; double glazing; and window shutter installation or replacement.

at least one action from that category was undertaken, and use it as the outcome variable in the event-study regression (1). As shown in Figure OA2, we find a positive and statistically significant effect for warmth leakage reduction—around four percentage points in both 2009 and 2010 (panel a)—while the effects for heating, water heating, or ventilation systems are not statistically significant (panel b). This suggests that the baseline effect on aggregate renovation activity is mainly driven by an increase in insulation and other warmth leakage-reducing actions.

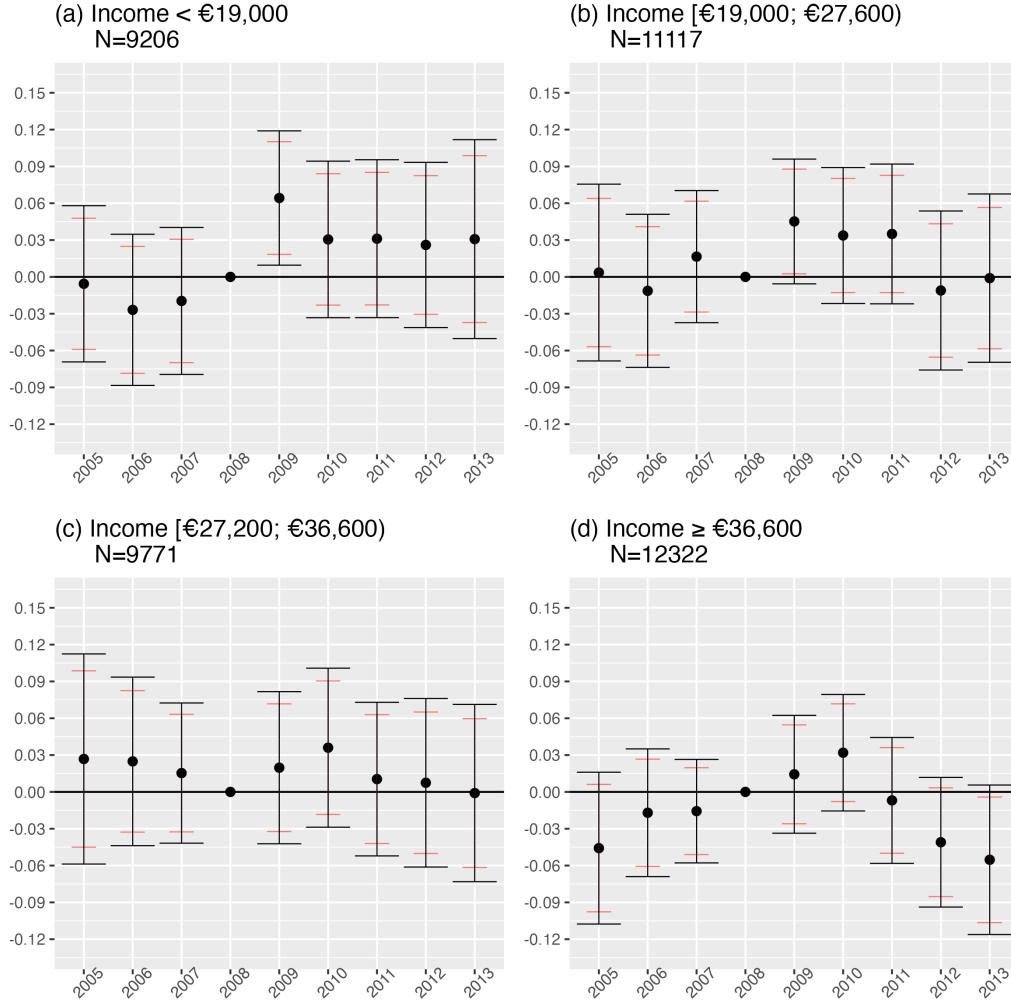
We then turn to the distinction between professional and DIY or undeclared work, recorded in the dataset as a binary variable. We find a relative increase in professional renovations, particularly in 2010 (panel c), indicating that ZIGL eligibility encouraged households to substitute informal or self-performed work with professional services. In a split-sample analysis, this effect is most pronounced among low-income households (Figure OA3), supporting the hypothesis that the substitution from DIY to professional work constituted an important margin of adjustment for this group.

Figure OA2: Effects of eligibility on various types of renovation actions



Notes: Estimates of 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. Controls are variables in Table A1 and an indicator for past renovations pre-ZIGL. Data source: ADEME survey.

Figure OA3: 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. Controls are variables in Table A1 and an indicator for past renovations pre-ZIGL. Data source: ADEME survey.

D Heterogeneity: By action cost

Actions with a high upfront cost provide yet another proxy for the intensive margin. More strongly impeded by credit constraints, they are indeed more likely to be stimulated by credit facilities. We therefore identify ‘costly renovations’ and re-run our baseline regressions using it as the dependent variable. Considering the distribution of amounts spent on 32 different types of actions, we take an action to be ‘costly’ if its value is above €763 or higher in at least 80% of cases.¹¹ This provides us with 8 costly actions out of 32, with an average cost of €4,693.¹² Then, we define ‘costly renovations’ as those containing at least one costly action. This definition involves roughly half of the renovations.

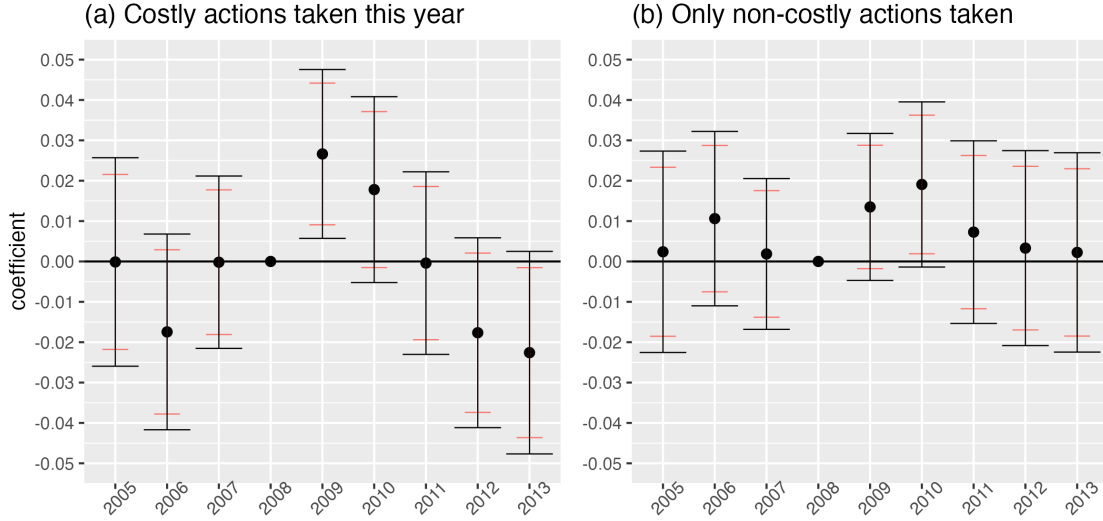
We re-estimate Equation 1 with two alternative outcome variables: (a) a dummy equal to 1 if a

¹¹We take €763 as a threshold because spending is reported in intervals: it corresponds to actions in the interval [€763-€1,523] or higher ones.

¹²Installation of a boiler or water heater, installation of a heat pump, installation of a solar water heater, window replacement with double glazing, installation of a closed fireplace, installation of a wood stove, replacement of boiler (with and without change of fuel type).

costly renovation was taken and 0 otherwise (i.e., no renovation occurred or it was a non-costly one); and (b) a dummy equal to 1 if a non-costly renovation was taken and 0 otherwise (i.e., no renovation occurred or it was a costly one). Since renovations of the two types are roughly equal in number, the two variables by design have similar average values around 0.08. The results of the two regressions with household fixed effects suggest that the 2009 effect is mainly driven by costly actions and the 2010 one by non-costly actions (Figure OA4).

Figure OA4: Effects on costly and non-costly renovations.



Notes: Event-study estimates of regression 1 with costly and non-costly renovations as dependent variables. Both outcome variables equal to 0 in the absence of renovation. Both regressions include household and time fixed effects. Survey weights are applied. Standard errors clustered at the household level. Controls are variables in Table A1 and an indicator for past renovations pre-ZIGL. Data source: ADEME Survey.

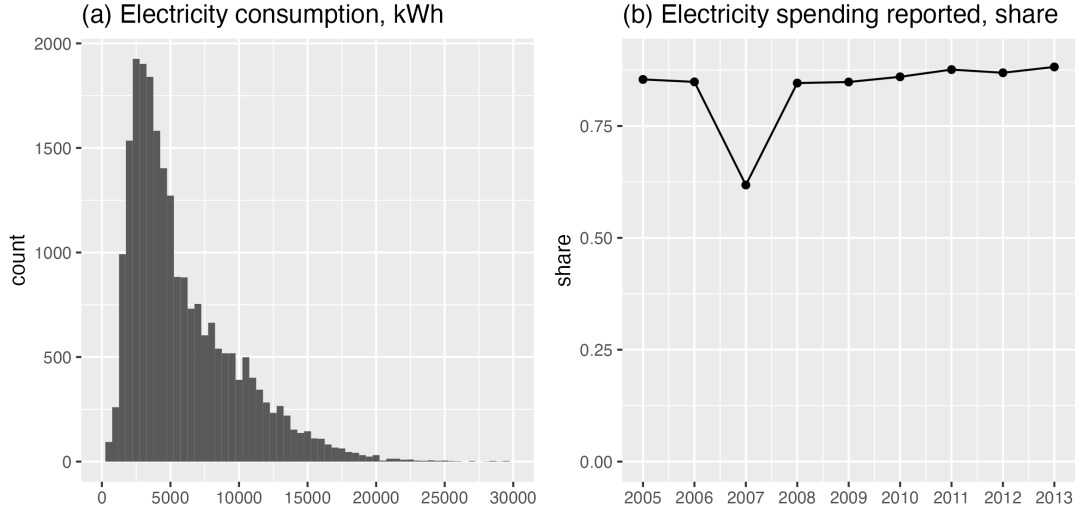
Taking these various proxies together, the effect of ZIGL eligibility was therefore weaker, and more limited in time, on the intensive margin of investment than it was on the extensive one.

E Electricity consumption

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 2006-2013.¹³ 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 panel b of Figure OA5 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 OA5.

¹³We thus ignore variation in household energy prices across France, which is very limited anyway.

Figure OA5: Distribution of electricity consumption, electricity response rate



Notes: (a) Distribution of electricity consumption from reduced sample described in Section 3.1. (b) Share of households that report (nonzero) electricity spending, by year. Data source: ADEME Survey for electricity spending, Enerdata and Pegase for electricity prices.

Then, we proceed by using the log transformation of energy spending to account for the skewness 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 OA6. 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.¹⁴

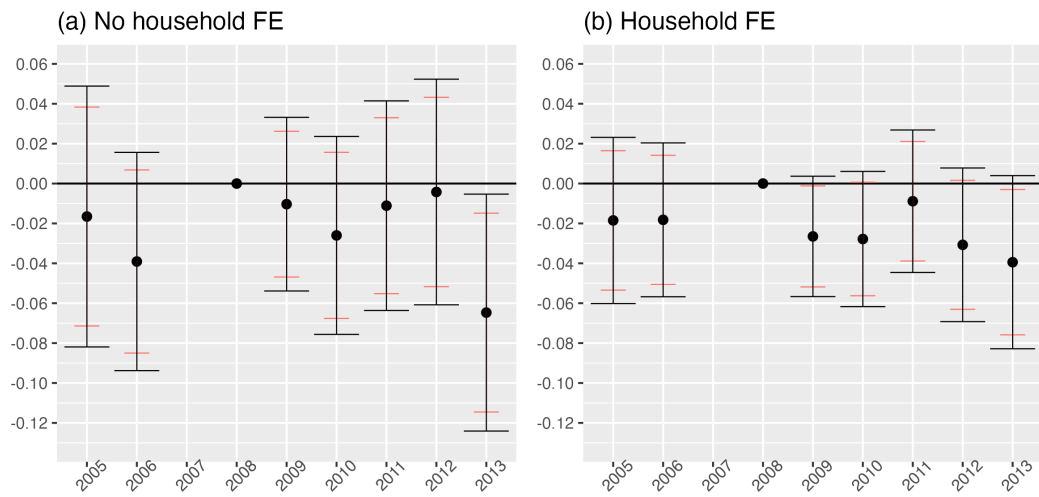
The weak significance of these effects and their absence in 2010-2012 is likely due to low statistical power, the sample being half the size of that used to study investment. Moreover, their magnitude is likely smaller than the one we would find for energy consumption as a whole, since only 31% of households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

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 OA7, 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.

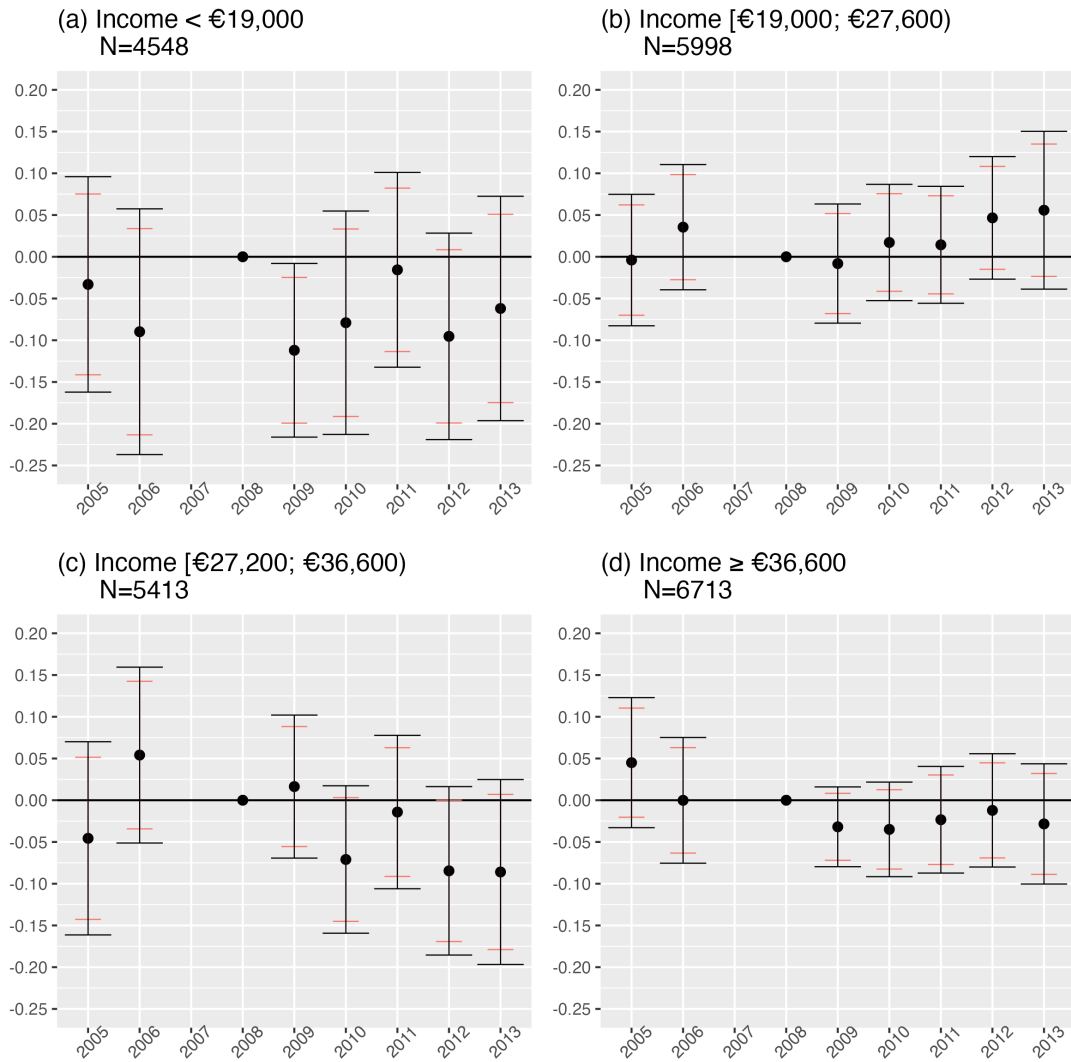
¹⁴Using the same dataset, Glachant and Blaise (2019) find a 0.64% reduction in energy use for each €1,000 of retrofit investment. Their analysis focuses on energy savings from all types of investment, regardless of any policy stimulus. Incidentally, their sample is broader – in particular, it is not restricted to homeowners nor electricity use. In turn, their identification does not rely on such an exogenous variation as our eligibility criterion.

Figure OA6: 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. Data sources: ADEME survey for electricity spending and controls, Enerdata and Pegase for electricity prices.

Figure OA7: 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 A1 for a description of controls. Data sources: ADEME survey for electricity spending and controls, Enerdata and Pegase for electricity prices.

F Marginal value of public funds

Leverage To compute leverage, 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 investment for the eligible group.¹⁵ We then divide it by the subsidy rate, that is, the euro amount expressed in percentage of the underlying investment cost. The approach is summarized in Equation OAF1.

$$\text{Leverage}_t = \frac{\% \text{ extensive margin effect}_t + \% \text{ intensive margin effect}_t}{\% \text{ subsidy rate}_t} \quad (\text{OAF1})$$

One important aspect of a ZIGL program is the disconnect between the subsidy pocketed out by the government to compensate banks (based on the rate on government bonds plus a fixed spread) and the price signal perceived by households, which is equal to the interest they would have paid on a regular loan backing the same investment. The two are related through the market interest rate (see Figure 1), but the gap between them cannot be interpreted as subsidy pass-through, for the banks have no room to adjust their price – they charge no interests. This calls for computing two different leverage estimates – a public one based on the subsidy effectively given to banks and a private one based on the implicit subsidy perceived by homeowners. We think public leverage is a more relevant indicator, for it connects the true policy effect to the true policy cost.

Applying the formula to our yearly event-study coefficients, as displayed in Table OA2, we find a public leverage of 1.75 and a private leverage of 1.15 in the 2009-2010 period, when the program was found to be effective. In other words, every euro granted by the government to banks for issuing ZIGLs induced a 1.75 increase in retrofit investment in that period. The lower private leverage reflects the government subsidy not fully covering banks’ opportunity cost. Both leverage values fall well below 1 in subsequent years.¹⁶

To put these numbers in perspective, the 2009-2010 estimates fall within the same range as those estimated for other loan and energy efficiency subsidy programs in micro-simulation works in France. Gobillon and le Blanc (2005) found a 1.1-1.3 public 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. Giraudet et al. (2021a) exhibit a private 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.

Marginal value of public funds We now use our leverage estimates to calculate the marginal value of public funds (MVPF), an indicator recently proposed by Hahn et al. (2024) to summarize the welfare effects of climate policies based on empirical estimates of their effects. The MVPF is intended to be more comprehensive than the widely used cost per tCO₂ abated, which ignores how public money is transferred to participants and the co-benefits they might enjoy.

The formula proposed by Hahn et al. (2024) measures the benefits induced by a policy (tax or subsidy) changing the consumption of a good x that generates an externality V . The externality is valued in-context (in the case of CO₂ emissions, at the locally-estimated social cost of carbon) and

¹⁵This 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.

¹⁶We report the post-2010 leverage based on the point estimates of the effects, but it can be regarded as null since the effects are not distinguishable from 0.

Table OA2: Leverage calculation

Year	Extensive margin	Intensive margin	Subsidy bank	Public leverage	Subsidy household	Private leverage
2009	24.7%***	3.9%**	17.1%	1.67	25.1%	1.14
2010	21.9%***	4.6%**	14.9%	1.78	22.5%	1.18
2011	5.0%	1.4%	16.0%	0.40	23.3%	0.27
2012	-2.2%	-0.4%	14.3%	-0.18	24.7%	-0.11
2013	-6.2%	0.1%	12.3%	-0.50	24.1%	-0.25

Notes: The extensive and margin effects are calculated as the coefficients of the event-study regressions (Figure 3b), divided by the average value of the associated outcome for eligible households in 2008: 0.18 for the extensive and €3843 for the intensive margin. Significance codes: ***0.01, **0.05, * 0.1. The subsidy to banks is derived from program data. The subsidy to households is derived from interest rate data and program data. Leverage indicators are calculated using Equation OAF1, with different subsidy specifications. Our preferred leverage indicator is highlighted in bold font.

the good x is assumed to be supplied competitively. The formula reads:

$$MVPF = \frac{1 + \frac{V}{p}(-\epsilon)}{1 + \frac{\tau}{p}(-\epsilon)} \quad (\text{OAF2})$$

where p is the net cost of the good, τ is the policy and $-\epsilon = \frac{dx}{-d\tau} \frac{p}{x}$ is the price elasticity of the demand for good x , derived from estimates of the policy effect. A natural benchmark to grasp the essence of this formula is what happens when the policy perfectly internalizes the externality, such that $\tau = V$. Then, $MVPF = 1$, which means that €1 of public money (either raised or spent) generates €1 of social welfare. If the policy is a subsidy, and its value is below that of the externality, the implied MVPF is expected to be above 1. The formula can accommodate multiple externalities, as is often needed with climate policies – think of local pollution from burning fossil fuels.

Importantly, in the case of a home energy retrofit (referred to as weatherization in Hahn et al. (2024)), the net cost p is the investment cost i net of the reduction in energy expenditure. The latter can be either directly measured, or derived from engineering estimates. The two approaches are known to differ widely (Fowle et al., 2018; Christensen et al., 2021), for a variety of reasons, including the rebound effect – a more intensive use of the good following an energy efficiency upgrade. It is therefore more reliable to use direct estimates of energy savings if they are available.

The formula can be applied to the ZIGL program, with some adjustments. One difficulty is that, as discussed in Section E, our estimates of induced energy savings are unreliable. It is therefore difficult for us to assess the net cost of the retrofits. To overcome this difficulty, we propose a series of approximations based on theoretical insights from the literature. First, we ignore the rebound effect, following an argument based on the envelope theorem that the gains from re-optimizing utility after investment are second-order compared to those from reducing the externality (Giraudet et al. (2018), Section 3.2.3). This insight is actually consistent with Hahn et al. (2024)’s observation that including rebound effect estimates in MVPF calculation hardly affects the result. Second, we abstract from energy savings altogether, using a result from the literature in industrial organization that, ignoring the rebound effect and further assuming perfect competition – as in Hahn et al. (2024) – optimal energy efficiency subsidies have an *ad valorem* rate equal to the fraction the externality weighs in the (full) energy cost (Nauleau (2014), Section 3.2.3). This condition can be written as:

$$\frac{C}{e + C} = \frac{\tau}{i}, \quad (\text{OAF3})$$

with τ the subsidy amount, i the investment cost, C the CO₂ externality and e the price of energy. Instead of making assumption about energy savings, this formula only requires assumptions about the energy price, the carbon intensity of energy and the social cost of carbon. With these approximations, we propose the following formula to assess the MVPF of the ZIGL program:

$$MVPF_{ZIGL}^C = \frac{1 + \frac{C}{e+C}\lambda}{1 + \frac{\tau}{i}\lambda} \quad (\text{OAF4})$$

where λ is our estimated leverage. This formula verifies that $MVPF = 1$ when condition [OAF3](#) is met.

Another difficulty specific to ZIGL programs is the disconnect between the public cost and the subsidy perceived by households. While in [Hahn et al. \(2024\)](#)'s formula the elasticity ϵ is based on the subsidy τ so there is an alignment between public expenditure and price signals, this cannot be assumed with ZIGL (see our discussion on leverage above).¹⁷ To address this problem, we consider three specifications of the MVPF. In Specification 1, we consider the effective subsidy given to banks and the public leverage that derives from it, so $\tau = 16\%$ and $\lambda = 1.75$ in 2009-2010. In Specification 2, we alternatively consider the signal received by homeowners and the private leverage derived from it, so $\tau = 24\%$ and $\lambda = 1.15$. In our preferred specification, numbered 3, we combine public leverage ($\lambda = 1.75$), which we deem the most relevant one, to the true signal perceived by households ($\tau = 24\%$).

We start with considering CO₂ emissions reduction as the sole benefit, as in Equation [OAF4](#). Assuming an average carbon content of energy of 150gCO₂/kWh in France and a social cost of carbon of €115/tCO₂ in 2009-2010, based on the most recent estimate ([France Strategie, 2019](#)), each kWh of energy entails a social cost $C = €0.02$. Further assuming an average energy price of $e = €0.08/\text{kWh}$ in that period, $\frac{C}{e+C}$ is equal to 22%. This fraction goes up to 94% if we assume a significantly higher social cost of carbon of €500/tCO₂.¹⁸

Next to climate aspects, a recent French study points to significant co-benefits to home energy retrofits in terms of reduced exposure to cold-related illness ([Dervaux et al., 2023](#)). A household who (i) belongs to the bottom 30% of the income distribution and (ii) occupies a dwelling whose annual energy consumption exceeds 380 kWh/m² per year (representing 2% of the dwelling stock in 2024, according to [Baba-Moussa and Ribon \(2024\)](#)) is exposed to respiratory and cardio-vascular diseases, generating an annual social cost of €5,700 in terms of induced health expenditure, morbidity and mortality. Averaging these costs over the whole dwelling stock yields €45 per dwelling, and $H = €675$ once accumulated over a 20 year lifetime and discounted at the official 3.2% rate. Note that this estimate is probably biased downward, as we ignore the fact that (i) low-income households may be overrepresented in least performing housing and (ii) they benefit disproportionately from the ZIGL program, according to our study. Notwithstanding, further assuming an average investment $i = €19,000$, as is the case in our dataset, yields $H/i = 3.6\%$. We can now extend the MVPF formula to account for these additional benefits:

$$MVPF_{ZIGL}^{C+H} = \frac{1 + \left(\frac{C}{e+C} + \frac{H}{i} \right) \lambda}{1 + \frac{\tau}{i}\lambda}. \quad (\text{OAF5})$$

The results are summarized in Table [OA3](#). In our preferred specification, we find an MVPF of

¹⁷[Hahn et al. \(2024\)](#)'s formula can be adjusted to accommodate pass-through – see their ‘Imperfect competition’ section. But we cannot use them, since again, the gap we are discussing here cannot be interpreted as pass-through.

¹⁸This alternative assumption is motivated by the social cost of carbon being constantly revised upwards ([France Strategie, 2025](#)) and a recent study pointing to a global value of \$1,300/tCO₂ ([Bilal and Känzig, 2024](#)).

0.98, in line with the 0.86-1.21 range elicited by [Hahn et al. \(2024\)](#) for weatherization programs in the U.S.. The fact that it is slightly below 1 is due to the implicit subsidy rate (24%) slightly exceeding the externality rate (22%). In other words, the externality is slightly over-internalized. Adding health benefits turns the value above 1. The alternative specifications entail slightly higher MVPF values. All values significantly increase under the €500/tCO₂ assumption.

Table OA3: MVPF calculation

Social cost of carbon Specification	€115/tCO ₂		€500/tCO ₂	
	w/o health	w/ health	w/o health	w/ health
1. Public leverage, effective subsidy	1.08	1.14	2.07	2.12
2. Private leverage, implicit subsidy	0.98	1.02	1.63	1.67
3. Public leverage, implicit subsidy	0.98	1.03	1.87	1.92

Note: Our preferred specification is highlighted in bold font.

G Robustness: Restricted treatment

We also assess the robustness of our main results to a further restriction of the treated group. Our objective is to compare housing units that are as similar as possible, differing only in their eligibility for the ZIGL program. To that end, we limit the treated group to houses built between 1975 and 1990 and compare them with units built after 1990, yielding a comparable number of observations (9,330 vs. 10,957). The advantage of this restriction is a better balance between treated and control groups. In t -tests for 2008, 12 of the 32 differences in covariates become statistically insignificant.¹⁹ Panel (b) of Figure OA8 reports the results for this restricted sample. Consistent with the previous exercise, we find no significant effect in 2009, while the 2010 impact remains stable at 4.2 percentage points (p.p.). Overall, excluding older houses yields results consistent with the vintage-specific renovation trends documented in Figure A1.

Figure OA8: Effects of eligibility on renovation decision, excluding oldest houses



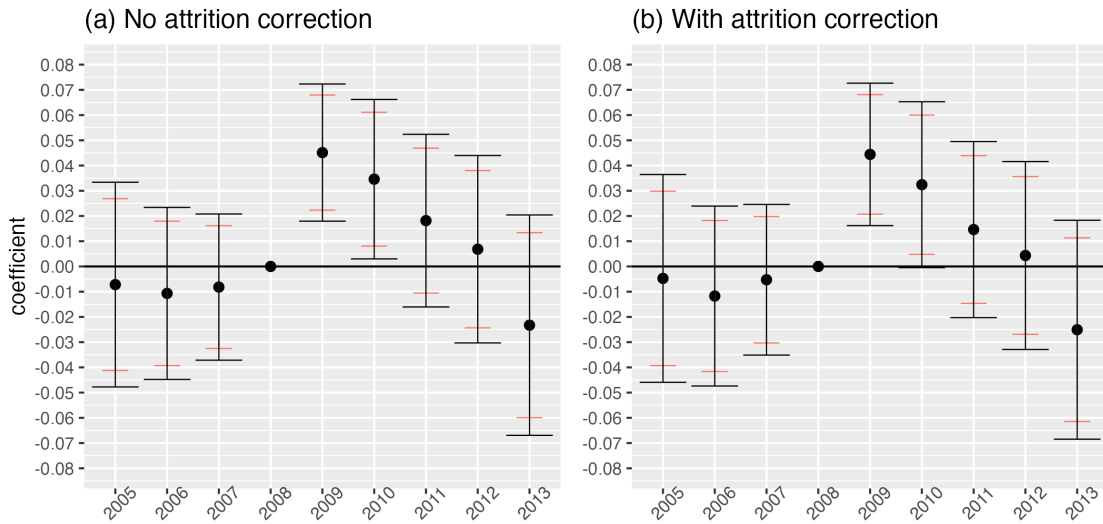
Notes: Estimates for the event study of Equation 1 for renovation decision (binary), excluding houses built before 1949 (Panel a) or built before 1975 (Panel b). Confidence intervals: 95% in black, 90% in red. Both regressions include household FE, time FE and time-varying controls. Standard errors clustered at the household level. Controls are variables in Table A1 and an indicator for past renovations pre-ZIGL. Data source: ADEME Survey.

¹⁹If we were to further restrict the treated group to houses built between 1982 and 1988—excluding those from 1975–1981—the sample size would fall by two-thirds, while only two additional variables would become balanced.

H Robustness: Attrition

As discussed in Section 3.1, with 29% of respondents observed for two periods and 10% for the whole nine periods, attrition might be a concern. In order to check whether entry and exit from the sample is correlated with ZIGL eligibility, we focus on those households present in the sample in 2008 and construct an indicator of their presence in subsequent years. After controlling for the variables of Table A1, we find the correlation to be significant, with eligible households having a 6 p.p. additional probability of staying in the sample. To check that non-random attrition is not biasing our results, we run a test suggested by Wooldridge (2010) that weighs observations according to their probability to stay in the sample. To do so, we use the fitted values of regressing the eligibility dummy on all the controls as a propensity score and rerun our baseline regression with inverse probability weighting. As displayed in Figure OA9, the results of the attrition-weighted regression (panel b) are very close to those obtained in the baseline setting (panel a), save for a less precisely estimated coefficient in 2010.

Figure OA9: Extensive-margin regression, 2008 cohort, with and without attrition correction



Notes: Estimates for the event study of Equation 1, estimated for the subsample of households present in 2008. Panel (a) regression uses survey weights from ADEME only, panel (b) regression interacts the survey weights with the inverse of fitted probability of being present in the sample in a given year. Confidence intervals: 95% in black, 90% in red. Household and time fixed effects used in both regressions. Standard errors clustered at the household level. Sample size: 29,994 in both regressions. Data source: ADEME survey.

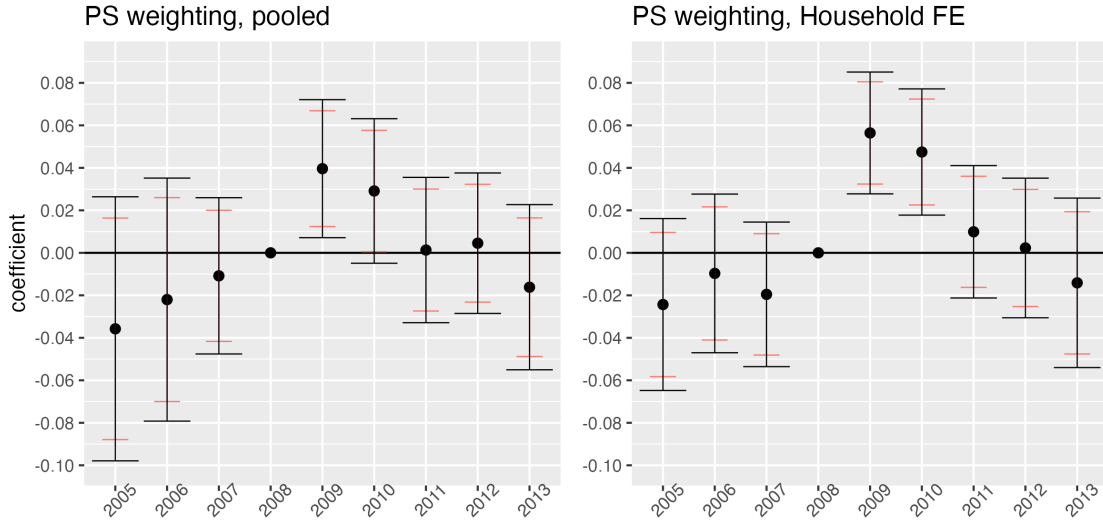
I Robustness: Imbalance between treatment and control

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 OA4. 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 OA11, 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 OA5 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 OA10: 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. Data source: ADEME survey

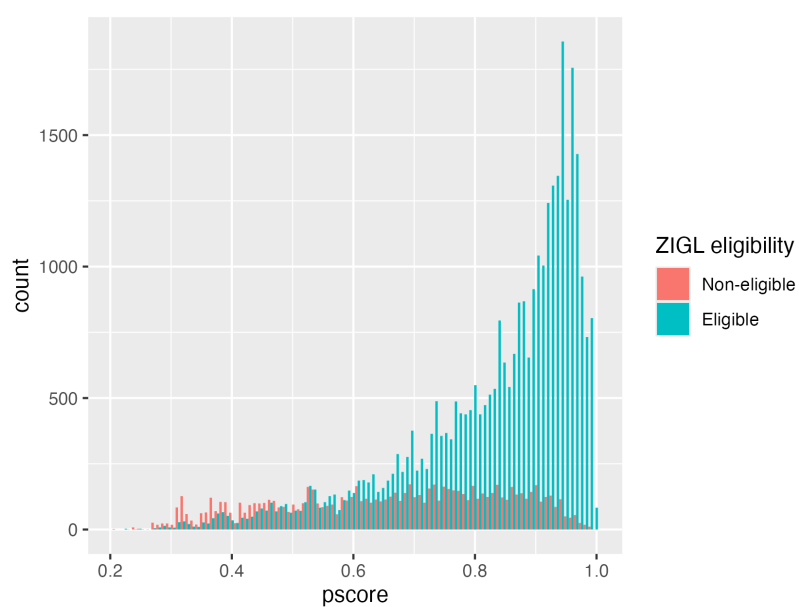
Figure OA10 presents the estimates of regression 1 with the inverse probability weighting based on propensity scores. The effects remain robust, and may have even become stronger as the point estimates move to around 5 p.p. range in both 2009 and 2010 in the specification for household fixed effects. This 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.

Table OA4: Propensity scores — Logit regression

	<i>Dependent variable:</i>
	Eligible
Age < 25	−1.366*** (0.270)
Age 25 to 34	−1.409*** (0.269)
Age 35 to 44	−0.729*** (0.269)
Age 45 to 54	−0.129 (0.269)
Age 55 to 64	−0.042 (0.270)
Occup. Agriculture	0.512*** (0.094)
Occup. Trade.Entrep.	0.564*** (0.087)
Occup. Indep.Mngmnt	0.447*** (0.084)
Occup. Intermediary	0.451*** (0.088)
Occup. White-collar worker	0.191** (0.082)
Occup. Blue-collar worker	0.617*** (0.091)
Agglom. Paris Area	−0.260*** (0.050)
Agglom. > 100k inhab.	−0.353*** (0.058)
Agglom. 20 to 100k inhab.	−0.709*** (0.055)
Agglom. < 2k inhab.	−0.731*** (0.054)
Surface < 50 sq.m	−0.071 (0.079)
Surface 50 to 74 sq.m	−0.387*** (0.078)
Surface 75 to 99 sq.m	−0.662*** (0.080)
Surface > 150 sq.m	−0.344*** (0.086)
Income < 19k €	−0.321*** (0.048)
Income 19 to 23k €	−0.462*** (0.047)
Income 22.8 to 27.6k €	−0.666*** (0.041)
Income 36.6 to 45.6k €	−0.861*** (0.046)
Income > 46.6k €	−1.013*** (0.048)
Heat. energy Elect.	−0.154 (0.137)
Heat. energy Fuel Oil	0.605*** (0.044)
Heat. energy Other	0.081 (0.145)
Heat. type Central	−1.541*** (0.085)
Heat. type Indiv. Electric	−2.277*** (0.159)
Heat. type Other	−1.941*** (0.161)
Appartment	−0.452*** (0.042)
Constant	4.896*** (0.305)
Observations	42,419
Log Likelihood	−21,343.380
Akaike Inf. Crit.	42,750.750

Notes: Estimates of logit regression for propensity scores. Survey weights are applied. Figure [OA11](#) plots the predicted propensity scores for the eligible and non-eligible. Significance codes: ***0.01, **0.05, *0.1. Data source: ADEME Survey.

Figure OA11: 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.

Table OA5: Balancing test with propensity score weighting (2008)

Variable	Category	Eligible (T)		Non-Eligible (C)		Diff	T-stat	p-value
		Mean	SD	Mean	SD			
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.

J Mechanisms: Supply side

Whether own-loans and ZIGLs act as complements (through cross-selling and commercial relationship strengthening) or substitutes is unclear. We address this question by estimating banks' opportunity cost at the local market level. We use interest rate data on consumption and housing loans from Banque de France, which records all loan originations from sampled branches of most French banks at a quarterly frequency since 2012. From these data, we compute the average interest rate and total loan amount for each bank within a given local banking market²⁰. We restrict the analysis to 2012–2018 to avoid distortions from administrative changes to the ZIGL program after 2018 (see Section 2). The final sample consists of 14,726 observations from 49 banks across 704 local markets, each observed for between two and 26 quarters. Then, for each bank–market–quarter observation, we measure the opportunity cost as the average interest rate on all loans, weighted by loan amounts:

$$\text{Opportunity cost}_{b,a,t} = \frac{\sum_i (\text{Interest rate}_{i,b,a,t} \cdot \text{Loan amount}_{i,b,a,t})}{\sum_i \text{Loan amount}_{i,b,a,t}} \quad (\text{OAJ6})$$

where b is the bank identifier, a the local market, t the quarter, and i the loan. This measure reflects both banks' ability to charge higher interest rates in less competitive local markets and their heterogeneity in product mix. Since consumption loans typically carry higher rates, local specialization in these products increases the opportunity cost of issuing ZIGLs.

We relate our measure of opportunity cost to the number of ZIGLs issued by each bank in a given area and period. Specifically, we estimate a Poisson regression of the ZIGL count variable, including time-varying controls and multiple fixed effects:

$$\ln(\mathbb{E}[\# \text{ZIGL}_{b,a,t}]) = \beta \cdot \text{Opportunity cost}_{b,a,t} + X_{a,t} + \varphi_b + \gamma_a + \tau_t \quad (\text{OAJ7})$$

where τ_t denote time fixed effects, and φ_b and γ_a represent bank and local market fixed effects, respectively. The vector $X_{a,t}$ includes controls for population size, share of the elderly, unemployment rate among the prime-aged, share of wage workers among the active, share of long-term contracts among wage workers, and housing stock structure: share of housing units built before 1990, share of main residences, share of rented among main residences. To further isolate the impact of opportunity cost, we also estimate a specification with bank–time fixed effects ($\varphi_{b,t}$). The coefficient of interest, β , is interpreted as the semi-elasticity of ZIGL issuance with respect to opportunity cost. The results are reported in Table 1.

²⁰A local banking market corresponds to the INSEE-defined *bassin de vie*, the smallest area within which residents can access essential services and amenities. There are 1,666 such areas in France.