Zero-Interest Green Loans and Home Energy Retrofits: Evidence from France

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Ilya Eryzhenskiy*, Louis-Gaëtan Giraudet†, Mariona Segú‡
Maryam Vahid Dastgerdi§

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Abstract

Zero-interest green loan programs (ZIGL) are gaining traction to address the tremendous financing needs implied by climate change mitigation. One such program, the French *Éco-Prêt à Taux Zéro*, significantly under-performed the expectations the government had for it at its inception in 2009. Using a difference-in-differences design applied to a panel survey of 10,000 households, we find that the program had a substantial, yet short-lived, impact on home energy retrofits. On the extensive margin, eligibility to the program significantly increased the rate of renovation by 3-4 percentage points (+25%) in the first two years, but not thereafter. The effect is strongest (7-8 p.p.) for low-income homeowners. On the intensive margin, the amount spent on renovation significantly increased only for the second year of the program, with at least an extra €1,100 (+9%). Our results are robust to a range of robustness checks, including placebo regressions and propensity score weighting. Our analysis points to unexplored barriers in both the demand and supply that can explain the short-lived effect of loans for home energy retrofits.

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

1 Introduction

Improving energy efficiency is celebrated as a key approach to mitigating climate change. This is especially the case in the building sector, which contributes 30% of global CO_2 emissions, two thirds of which stem from housing (IPCC, 2018). With a unit cost in the tens of thousands dollars, comprehensive home energy retrofits are challenging to deliver, calling for innovative financial instruments.

In this spirit, low-interest loan programs for home energy renovation are increasingly implemented around the world (Berry, 1984; Guertler et al., 2013). This is for instance the case in Germany with the KfW's Energy-efficient refurbishment program and in the United

*Corresponding author. Ecole des Ponts ParisTech, CIRED. ilya.eryzh@gmail.com
†Ecole des Ponts ParisTech, CIRED
‡CY Cergy Paris Université
§Ecole des Ponts ParisTech, CIRED

States with the Property Assessed Clean Energy (PACE) Financing program (Rose and Wei, 2020). In France, a zero-interest green loan (ZIGL) program called *Eco-Prêt à Taux Zéro* was implemented in 2009, granting households interest-free loans for investments in home energy upgrades that meet certain performance requirements. The loans are issued by banks, which in return get a compensation from the government. Upon launching the program, the government expected to subsidize 400,000 annual ZIGLs.[1] This target was deemed credible in retrospect in a recent technico-economic assessment (Giraudet et al., 2021a). Yet after a promising start, participation in the program fell in 2011 and remained within the 20,000-40,000 range thereafter – an order of magnitude below expectations.[2]

The question therefore arises as to whether ZIGL programs have been effective at all. The issue is all the more pressing in France, where, following recommendations from the Citizens' Convention for Climate, the current program is set to be enhanced and extended to target other green assets such as electric vehicles (CCC, 2020).

In this paper, we estimate the causal impact of the ZIGL program on investment in energy-efficient retrofits. To do so, we use a panel dataset of nearly 10,000 French households surveyed from 2000 to 2013 by ADEME, the French Energy Management Agency. We exploit a restriction in program eligibility to the buildings built before 1990 to estimate the impact of the program with a difference-in-differences strategy. We find the impact to be significant, yet short-lived, on both the intensive and extensive margins of investment. The estimated additional probability of renovation due to ZIGL eligibility is statistically significant in the first two years of the program only, equal to 4 percentage points in 2009 and 3 p.p. in 2010. This represents a substantial increase of about 25% in the renovation rate of the eligible group. The effect is most pronounced (+7.5 p.p.) for homeowners whose annual income falls below €19,000. On the intensive margin, ZIGL contributes an additional €1,100 spending on renovation (conditional on having renovated at least once in the observation period), statistically significant only in 2010 and equivalent to a 10 % increase in bottom line investment among the eligible group (€11,100).

The results are remarkably well reproduced when combining the difference-in-difference strategy with inverse propensity score weighting. Placebo regressions exploiting alternative construction periods to lure eligibility requirements further corroborate the results, while suggesting that the older pre-1949 dwelling group might play an important role in driving the effect. Re-running the baseline regression without that group confirms that its role is significant, but not crucial, to our results.

While our analysis only covers the first five years of the program, we note that participation continued to decrease after this period, which goes to suggest that the causes of the 2010-2011 drop had persistent effects. While our framework does not allow us to identify these causes, we discuss a few hypotheses, including high transaction costs and high opportunity costs on the supply side and financial literacy and debt aversion on the demand

[1] http://www.planbatimentdurable.fr/IMG/pdf/convention_ecoptz_26-02-09-2.pdf

[2] In the United Kingdom, the Green Deal program was launched in January 2013 and terminated in July 2015, with only 6,000 loans issued yearly over the period. The program was deemed a "failure" by the House of Commons. Like the French ZIGL program, it was launched with high expectations which would soon be dampened. Unlike its counterpart, however, the Green Deal did not go with subsidies and rather consisted of an inherently less appealing pay-as-you-go mechanism (Rosenow and Eyre, 2016).

side.

It was not until late 2019 that participation started rising again, to the point of reaching the level of 2011 in 2020. This trend reversal can plausibly be explained by a loosening of the performance requirements. Incidentally, the average amount borrowed decreased from about €16,000 to €12,000. In any case, the causes of the 2010-2011 drop need to be carefully investigated before the ZIGL program is enhanced and extended to other green assets, as recommended by the Citizens' Convention for Climate.

Our analysis contributes to the relatively scarce literature on subsidized loans, which have counterparts in other sectors of the economy, perhaps most notoriously in the markets for student loans (Cadena and Keys, 2012) and mortgages (Martins and Villanueva, 2006; Gruber et al., 2021). In France in particular, the ZIGL program is closely related to the *prêt à taux zéro*, a zero-interest loan program for first-time home buyers (Gobillon and le Blanc, 2005; Labonne and Welter-Nicol, 2017; Gobillon et al., 2022). Generally speaking, research on subsidized loans lies at the intersection between literature on subsidy programs – here, energy efficiency subsidies, perhaps the most widespread Pigovian instrument for internalizing energy-use externalities (Kerr and Winskel, 2020) – and literature on household finance (Zinman, 2015). Incidentally, our analysis contributes to both strands of the literature. Specifically, in the French context, it is closely related to empirical analyses of home renovation subsidies using the same dataset (Nauleau, 2014; Risch, 2020). This research agenda focuses in particular on the quantification of additional, or infra-marginal, participants in the program (Boomhower and Davis, 2014). Our analysis is also related to an empirical analysis of personal loans, focusing on interest-rate differentiation according to the type of asset purchased (Giraudet et al., 2021b). This emerging research agenda asks whether there are specific barriers in loan supply that may cause underinvestment in energy efficiency (Jaffe and Stavins, 1994; Gillingham et al., 2009; Giraudet, 2020). Finally, our finding that the effect is largest among low-income households echoes the analysis of Lindner et al. (2020), who find that lower interest rates induce a substitution of professional home retrofits for do-it-yourself interventions.

The remainder of this paper is organized as follows. Section 2 describes the zero-interest green loan program in greater detail. Section 3 presents the dataset. Section 4 presents the empirical strategy. Section 5 presents the results. In Section 6, we present some heterogeneity effects and in Section 7 we perfom robustness test on the results. Finally, Section 9 concludes.

2 Institutional Setting

The French *éco-prêt à taux zéro* program (Eco-PTZ, hereafter ZIGL) was implemented in April 2009 in the wake of the *Grenelle de l'environnement*, a public consultation held at the initiative of then-President Sarkozy on environmental policy. The program grants interest-free loans for investments in home energy upgrades that meet certain performance requirements, achieved by combining several measures on the building envelope and the heating system. The amount eligible for interest discharge is capped at €30,000 and the repayment period at 15 years. Homeowners can apply without income restriction. Eligibility to the program is restricted to housing units built before 1990.

On the supply side, ZIGLs are issued by government-approved credit institutions, which in return receive compensation on each loan issued, the rate of which is equal to the rate on government bonds plus a spread of 0.35 percentage points. The loans are unsecured, implying that banks need to hold capital to cover them. The task of appraising the home energy upgrade project, initially allotted to the lender, was transferred in 2015 to the contractor in an attempt to simplify the program. Also since 2015, loan issuance is conditioned on energy upgrades be completed by a certified contractor holding the RGE label (*Reconnu Garant de l'Environnement*). The program is administered by the *Société de gestion des financements et de la garantie de l'accession sociale à la propriété* (SGFGAS).

The ZIGL program is part of a rich portfolio of incentive programs for home energy retrofits in France, also including the *crédit d'impôt pour la transition énergétique*, (CITE, formerly *crédit d'impôt pour le développement durable*, replaced in 2020 by *MaPrimeRénov'*), an income tax credit program implemented in 2005, and the *certificats d'économie d'énergie* (CEE), a utility-sponsored subsidy program implemented in 2006 (Giraudet et al., 2021a). Households are allowed to benefit from the three programs to finance the same investment, though the rules for combining ZIGL and CITE benefits varied over time: overlap was permitted for households whose annual income fell below €45,000 in 2009 and 2010; it was forbidden in 2011; permitted again in 2012 and 2013 with a €40,000 income ceiling; permitted with differentiated income ceilings (€25,000 for a single person and €25,000 for a couple, plus €7,500 per child) from 2014 to February 2016; and permitted without income restrictions since March 2016.

From a theoretical perspective, the rationale for ZIGLs is two-fold. On the one hand, as an energy efficiency subsidy, the instrument can be considered a Pigovian tool to internalize the negative externalities associated with energy use. In this regard, Giraudet et al. (2021a) find that, at least in a stylized form, the ZIGL program induces more leverage than its CITE and CEE counterparts, an effect they attribute to the tighter performance requirements associated with it. On the other hand, zero-interest rate programs can be seen as a policy remedy to information asymmetries in credit supply (Stiglitz and Weiss, 1981), a source of credit rationing among low-income households. Although the latter rationale was less emphasized than the former by public authorities when implementing the ZIGL program, both are important to consider, as our results will suggest.

In 2017, 24,284 ZIGLs were issued, totaling 436 million euros outstanding. The average project cost is €22,366, of which €17,976 were financed by a ZIGL over an average repayment period of 10.5 years (SGFGAS, 2017). So far, 537 credit institutions have been ZIGL-approved. They were reimbursed 120 million euros in 2016 (Hainaut et al., 2016). In 2017, as the program became part of the initiatives funded by the newly issued sovereign green bond, it raised a cost of 126 million euros (AFT, 2018). As of June 30th of 2018, 365,367 ZIGLs have been issued since 2009, hence an average annual number of 40,596. In a broader perspective, ZIGLs only account for 20% of household debt contracted for home energy renovations, which in turn finances only 20% of the total home energy renovations. Taking all types of loans together, home investors borrowed €10,267 for energy retrofits, at an

Table 1: ZIGL summary statistics.

Variable	2009	2010	2011	2012	2013
Number of loans	68225	79508	42324	33936	32448
Mean amount, EUR	16318	16798	17020	17119	17297
Mean duration, months	107	109	110	116	122
Share of problematic	0.013	0.017	0.020	0.018	0.013
Number of lenders	99	104	101	102	99

Notes: Data comes from SGFGAS administrative data.

average rate of 3% over 5.5 years (ADEME, 2016).

Table 1 presents selected summary statistics for the first 5 years of ZIGL implementation: 2009-2013. This is the post-implementation period we study in our empirical analysis presented below. The number of loans has plummeted from 79,508 to 42,324 in 2011 and has continued to decrease through 2013. The average loan size and duration of the loans have been, on the contrary rising slightly over the period. The share of problematic loans, marked by the "undue benefit" marker in the program data, has risen from 1.3% to 2% in 2009-2011, then reverted to 1.3% in 2013. Finally, the number of ZIGL-supplying credit institutions has risen from 99 to 104 in 2010, decreasing to 99 afterwards. While this paper mostly focuses on the impact of the loans issued over this 5-year period, Section 8 provides a preliminary discussion of the causes for the decline of ZIGL production and the role of problematic loans.

3 Data

We use the dataset *Maîtrise de l'Energie*, a representative survey carried out by TNS-SOFRES on behalf of the French Energy Management Agency, ADEME – henceforth the ADEME Survey. The dataset consists of a self-administered panel survey covering the 2000-2013 period. Participation was incentivized with a customer points system, offering a barbecue set as the largest possible reward. Respondents could then enter and exit the sample on a voluntary basis.

We focus on the 2005-2013 period so as to avoid capturing the effect of policies that were introduced in 2005 (see Section 2). We further restrict the sample by excluding renters and focusing on homeowners, who account for more than 90% of ZIGL applications.[3] In an effort to harness the panel dimension of the data, we ignore those households that were present in the sample for one period only. These restrictions, along with dropping observations with crucial variables missing (see below), leave us with a total of 9,657 respondents observed for at least 2 periods over a 9-year period, hence an unbalanced panel of 45,418 observations with 29% of respondents observed for 2 periods and 10% for all of the 9 periods.

The main variable of interest is the decision to renovate, a binary variable that takes the value of 1 if the household declares to have renovated their unit on a given year.[4] We drop

[3]See https://www2.sgfgas.fr/statistiques for summary statistics on ZIGL provided by SGFGAS.

[4]The definition of renovation in the questionnaire is "works aiming at reducing your energy consumption

254 observations for which this piece of information is missing. We also use a measure of the intensive margin of the renovation, which is the total amount spent on retrofit works. We then construct a measure of eligibility to the ZIGL program using information on the date of construction. This categorical variable is selected by survey respondents from among a menu of six possible options: "in 1948 or before", "between 1949 and 1974", "between 1975 and 1981", "between 1982 and 1988", "between 1989 and *the year before the survey year*" and "*survey year*". As discussed in Section 2, the cut-off year for eligibility is 1990, so all houses falling in the first four categories are considered eligible. We classify the fifth and the sixth category ("between 1989 and *the year before the survey year*" and "*survey year*") as our control group of non-eligible housing units. Note that our control is slightly inaccurate, for it contains a small subgroup of units that are actually eligible (those built in 1989).

In addition, we use a range of categorical variables as explanatory variables, including the age of the household head, their occupation (*professions et categories socioprofessionnelles*), income, the surface area of the house, the type of heating system and the fuel used, the type of settlement and the region of France. The names and categories of the variables are listed in Table A1. The income categories, an important control in our estimation, are not reported with stable cut-offs across years. To deal with this, we build a categorization system that accommodates these inconsistencies in the best possible way. This results in slight overlap between the [€19,000, €23,000] and [€22,800, €27,600] categories, a measurement error we think is innocuous.

Out of all the variables listed above, 1,792 observations are missing for income (4% of the sample) and 1,357 for surface area (3% of the sample). To make up for them, we implement an imputation procedure relying on ordered logit.[5] Finally, for the entire analysis, we use the sampling weights built by the data provider to ensure that our results are representative of the French population and not only of survey respondents.

Summary statistics are presented in Table 2 for the years 2008 and 2013 – one year before and four years after program implementation. The share of renovating households is 17% in 2008 and 15% in 2013. A vast majority of the households (around 80% in both years) are eligible for ZIGL. Around a quarter live in apartments, with others living in houses. The most frequent settlement (around 30%) is rural areas; the second most frequent (around 27%) is towns of more than 100,000 inhabitants outside the Paris area. The most frequent age of household head category is 65 and more – 34% and 37% in the two periods –, while the share of the youngest household heads under 35 years old falls from 10% to 7%. The most frequent (around 47%) occupational status is non-employed, which includes retirees and students. The high share of household head above 65 suggests that the non-employed are mostly retirees. Household income is distributed rather evenly across the six categories, the most frequent being below €19,000 in 2008 and [€27,200-€36,600] in 2013 (both with 23%). The most frequent housing surface category is 100 to 149 square meters – 42% in 2008 and 37% in 2013. Finally, most respondents (around 40%) heat their dwelling with natural gas in individual heating systems.

or improve your comfort (heating, hot water, isolation, ventilation, etc.)".

[5]For more details, see description of the **mice** package of R.

Table 2: 2008 and 2013 summary statistics.

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

Notes: Survey weights are applied. Data comes from ADEME Survey.

4 Empirical strategy

We implement a difference-in-differences strategy to identify the causal impact of having access to a zero-interest green loan (ZIGL) on the probability of renovating a housing unit. We exploit the eligibility criteria associated with the age of the house to identify a treatment and a control group. In particular, we consider as treated those units that were built before 1990 and as control those built after 1990. We think the 1-year mismatch discussed above between this criterion and the housing age cut-off available in the dataset is unlikely to substantially bias our estimation. We then compare renovation outcomes for the two groups of households for different years before and after implementation of the program in 2009.

We estimate the following regression model:

$$R_{i,t} = \alpha + \delta \text{ Eligible}_{i,t} + \sum_t \beta_t \text{ Eligible}_{i,t} \times \tau_t + X_{i,t} + \tau_t + \epsilon_{i,t} \quad (1)$$

where $R_{i,t}$ is our outcome variable, $\text{Eligible}_{i,t}$ is equal to one if the housing was built before 1990, $\text{Eligible}_{i,t} \times \tau_t$ is the interaction of the treatment variable with time dummies, $X_{i,t}$ is a vector of time-varying controls and τ_t are time dummies. In some specifications, we also include respondent fixed effects μ_i. We use two different measures of $R_{i,t}$ to capture both the extensive and the intensive margins of investment. For the extensive margin, we use a binary indicator that is equal to 1 if a household i renovates in year t. For the intensive margin, $R_{i,t}$ is the euro amount spent on the renovation.

Our parameters of interest are β_t, representing the impact of being eligible to the program at every point in time. We therefore estimate the impact of the intention to treat rather than the direct impact of the program. One advantage of specification (1) is that it allows us to test the hypothesis of parallel trends between the two groups before the program implementation.

Figure 1: Evolution of renovation rates by treatment group, 2005-2013.

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

In Figure 1, we display the evolution of the renovation rate – the share of renovating households – for each group. While the rate of renovation remains constant at an average of 10% for the control group, it surges for the treatment group right after the implementation of the program. The trends are parallel before 2009 and only diverge thereafter, which suggests that our control group is adequate.

Table 3: Balancing test comparing ZIGL eligible and non-eligible (2008).

Variable	Category	Eligible (T) Mean	SD	Non-Eligible (C) Mean	SD	Diff	T-stat	p-value
Appartment	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	< 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
Heating main energy	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***
	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***
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 comes from 2008 ADEME Survey.

Next, we run a balancing test to compare the demographic and housing characteristics of the two groups. Table 3 reports the average values and standard deviation for the key control variables, as well as the t-stat and p-value of a means differences t-test. We observe that most variables statistically differ between the two groups. While this is not a challenge for our identification strategy, which only relies on the common-trend assumption, it suggests that these differences are important to control for in our main specification. In addition, we perform inverse probability weighting as a robustness exercise.

Finally, we also run the simple two-period DID strategy to evaluate to what extent the impact of eligibility is sustained over the post-implementation period. In this regression, we interact the variable of interest with Post_t, an indicator of the post-2008 period.

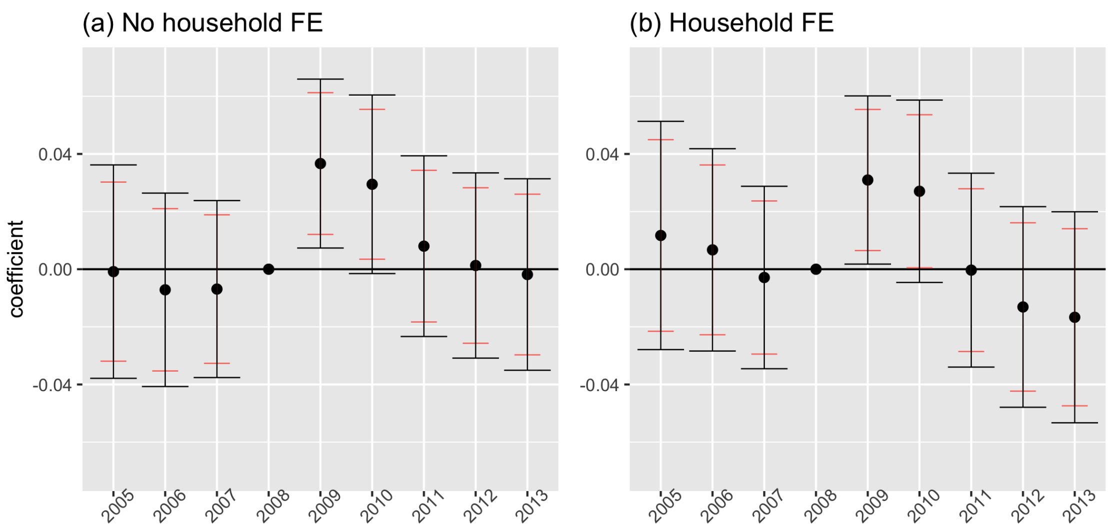
$$R_{i,t} = \alpha + \delta \ \text{Eligible}_{i,t} + \beta \ \text{Eligible}_{i,t} \times \text{Post}_t + X_{i,t} + \tau_t + \epsilon_{i,t} \tag{2}$$

5 Results

5.1 Effect of ZIGL eligibility on renovation — extensive margin.

Figure 2 presents the difference-in-differences coefficients of the main regression (equation 1), with and without household fixed effects, keeping 2008 as the baseline year. The results are broadly in line with the graphical evidence of Figure 1.

Figure 2: Effects of eligibility on renovation decision.



Note: Estimates of yearly difference-in-differences from equation (1), with the renovation dummy as the dependent variable. Confidence intervals: 95% in black, 90% in red. Left plot — specification with household controls (both constant and time-varying), but no household FE; right plot — specification with household FE and time-varying controls. Time FE used in both specifications. Standard errors clustered at the household level. See Table A1 for description of controls and Table A2 for regression results.

We first note that the two groups behave in a similar way before the implementation of the treatment, as none of the coefficients interacted with years 2005 to 2007 are significant. After the ZIGL introduction, there is a significant difference-in-differences coefficient, but only for the years 2009 and 2010. In the specification with controls but without fixed effects, the effect is 3.7 p.p. in 2009 (5% statistical significance) and 2.9 p.p. in 2010 (10 % significance). The estimates are similar, but smaller in the regressions with respondent fixed effects and controls: 3.1 p.p. in 2009 (5% significance) and 2.7 p.p. in 2010 (10% significance). Again, no significant difference in differences is found after 2010. Altogether, these effects are quite substantial, amounting to a 25% increase in the renovation rate of the eligible group.

The two-period regression (equation 2) yields the average effect of eligibility of the post-ZIGL introduction period. In line with the yearly estimates presented above, the effect is 1.8 p.p. for the pooled regression and 1 p.p. for that with household fixed effects, only the former being statistically significant (at 5%). These results suggest that the average effect of the program is not statistically significant over its running period. In other words, the program only had an effect on the first two years. Table A3 presents all coefficients of the two-period regression, including those associated with the control variables. It shows that renovation is more likely among younger households, non-employed (the omitted category), blue-collar or independent workers or managers as household heads, with income above €19,000, and living outside the Paris Area. As for housing characteristics, renovation is more frequent in large houses not heated with fuel oil.

5.2 Effect of ZIGL eligibility on renovation — intensive margin.

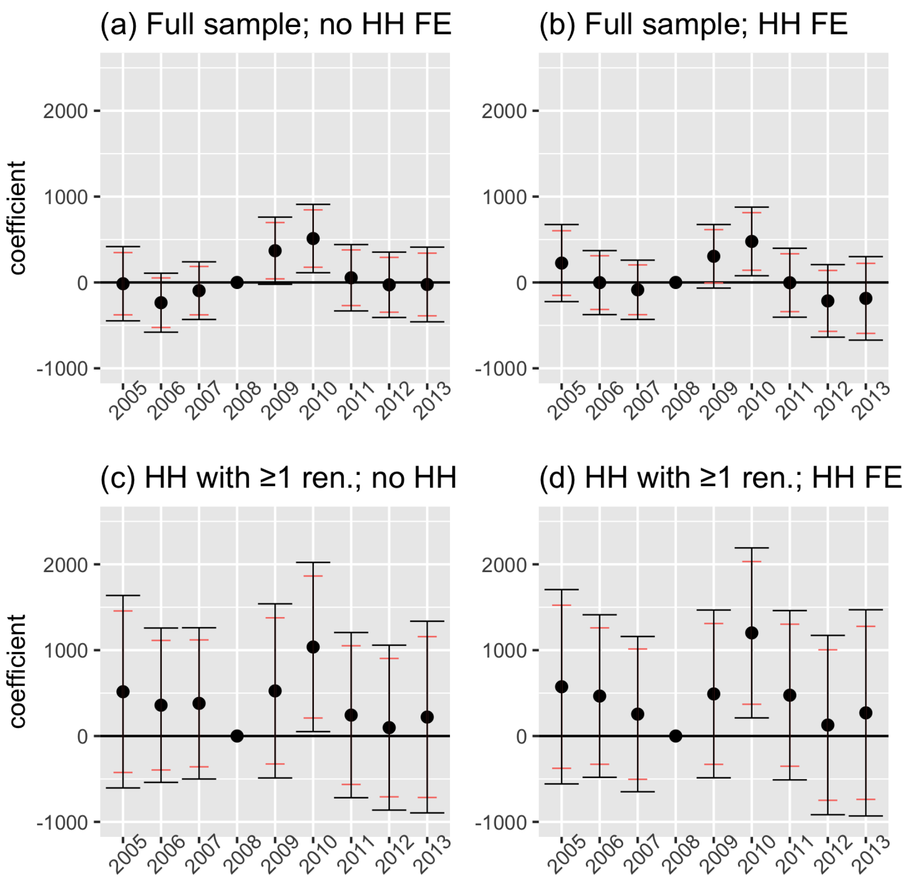
Assessing whether the program increased the amount spent on renovation is challenging, since the low share of renovating households generates a lot of zeros for the renovation amount variable. Since our interest lies in differences in differences, it is preferable to stick to a linear regression framework for tractability. We therefore focus on a subsample of households that have renovated at least once throughout 2005-2013. Note that we still have a significant portion of observations with a null renovation amount, as most households renovate only once or twice during the period. We illustrate the difference-in-difference coefficients for both the baseline sample and the subsample in Figure 3 and fully report them in Table A4 in the Appendix. The subsample of households that have renovated at least once has around twice fewer observations than the full one.

Here again, the pre-trends are parallel for all specifications, thus validating the difference-in-differences strategy for the intensive margin. Secondly, we find a positive and significant difference in differences for 2010, but not for 2009. The effect is around €500 of unconditional additional spending and €1,000 to €1,200 of additional spending conditional on having renovated at least once between 2005 and 2013. These coefficients are significant at 5% in all specifications. In terms of magnitude, these effects are moderate but not negligible: the average renovation before ZIGL introduction stood at roughly €11,100, meaning that the amount, conditional on having renovated, has increased by at least 9%. To put this figure into perspective, a loan taken in 2010 for €11,100 with fixed repayment over 120 months (i.e., the average repayment period of ZIGLs in 2010) would have cost €3,688 in interests at the 6% rate that then prevailed with personal loans, and €1,762 at 3%. This suggests that the benefit pass-through is incomplete.

The two-period regression with renovation amount as the dependent variable shows no significant effect for the post-ZIGL period, which can be explained by the fact that a significant effect was only observed on one year out of the five post-ZIGL years.

Overall, the effect we observe on the intensive margin provides yet another piece of evidence for the short-lived effect of the program.

Figure 3: Effects of eligibility on renovation amount.



Note: Estimates of yearly difference-in-differences from equation (1) for renovation amount, with 95% (black) and 90% (red) confidence intervals. (a), (b) — full sample, (c), (d) — sample of households with at least one renovation throughout 2005-2013. (a), (c) — specification with household controls (both constant and time-varying), but no household FE; (b), (d) — specification with household FE and time-varying controls. Time FE used in all specifications. Standard errors are clustered at household level. See Table A4 for regression results.

6 Heterogeneity of Effects

In order to better understand who benefits more from being eligible to the ZIGL program, we perform an heterogeneity test by household income. We take a triple difference-in-difference approach and interact all income categories with the eligibility variable and the post-2008 period variable in equation (2). We saturate the model by also including all bilateral interactions. Focusing on the period where the program was found to have an impact, we exclude the years 2011-2013. The results are presented in Table 4.

For the reference category whose income falls within €27,600-€36,600, the estimated effect is 2.5 p.p. in the specification without household fixed effects and 0.7 p.p. with fixed effects; neither effect is statistically significant. The effects do not statistically differ from the reference level for other income categories, except the lowest one with annual income below €19,000. For these households, participation increases by 7.1 p.p in the specification without household fixed effects (total effect 9.6 p.p.) and by 7.5 p.p. in the specification with household fixed effects (total effect 8.2 p.p.).

Table 4: Heterogeneity of effect by income group.

	Dependent variable:	
	Renovation this year	
	(1)	(2)
Eligible	0.101^{***}	-0.010
	(0.015)	(0.028)
Eligible × Post	0.025	0.007
	(0.022)	(0.023)
Eligible × Post× Income < 19k	0.071^{**}	0.075^{**}
	(0.032)	(0.035)
Eligible × Post× Income [19k, 22.8k)	-0.020	-0.001
	(0.038)	(0.039)
Eligible × Post× Income [22.8k, 27.6k)	0.010	-0.003
	(0.040)	(0.041)
Eligible × Post× Income [36.6k, 45.6k)	0.012	0.019
	(0.035)	(0.038)
Eligible ×Post × Income ≥ 45.6k	0.018	0.011
	(0.034)	(0.037)
Controls	Yes	Yes
HH FE		Yes
Year FE	Yes	Yes
Observations	28,770	28,770
R^2	0.030	0.436
Adjusted R^2	0.028	0.202
Residual Std. Error	0.397	0.360

Note: Estimates of triple differences in differences, obtained by interactions of the income variable with the Eligibility and Post variables in equation (2). The years 2011-2013 are excluded since the aggregate effect is not found for that period. Income category from €27,600 to €36,600 — the most frequent one — is the omitted category. Data comes from ADEME Survey.

This analysis confirms the basic intuition that poorer homeowners are more liquidity constrained and hence benefit more from access to low-interest credit. We contribute to the literature by showing that this also results in higher rates of energy efficiency investments for such households. A recent study on homeowners in Luxembourg (Lindner et al., 2020) suggests an additional mechanism for the effect on low-income households. They document that low-income households, who tend to do home improvement works themselves, are more likely to turn to professional contractors when interest rates are lower.

7 Robustness tests

In this section, we use placebo regressions to check the validity of the treatment variable, split-sample regression to assess the role of influential observations, and regressions with propensity score weighting to better balance the control and treatment groups.

7.1 Placebo regressions & influential observations

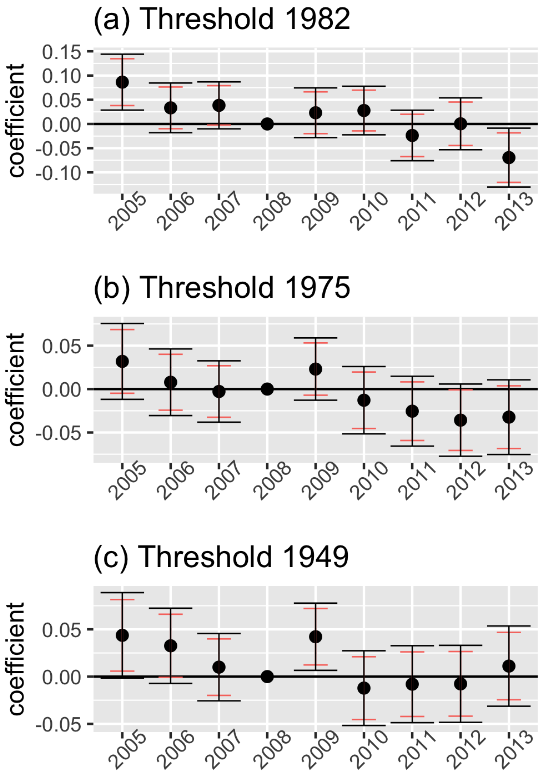
We test several fictional measures of eligibility to the program to make sure our variable captures the effect of the true criterion and not that of other relevant characteristics that differ between pre- and post-1989 houses. As seen in Section 3, the data are segmented into six periods of construction. The two most recent periods form the true control group. This leaves us with three fictional partitions between treatment and control for placebo regressions. We consider eligible the first three construction periods – i.e., built before 1982 – in the first placebo regression, the first two periods – before 1975 – in the second one and only the oldest houses – before 1949 – in the third one. To keep the true effects out of the placebo regressions, we exclude the true control group of houses built after 1988 from the fictional control groups.[6]

Figure 4 presents the difference-in-difference estimates of equation (1), for each of the three placebo eligibility measures, with household fixed effects (as the results without household FE are qualitatively equivalent). Considering 1982 as the eligibility cut-off results in significant difference in differences in 2005 (i.e., before the program was implemented) and in 2013. The coefficient in 2013 is negative, confirming that this measure is independent from the ZIGL program. Considering 1975 as the eligibility cut-off results in no significant difference in differences, except for 2012, when it is negative and statistically significant at the 10% level. Finally, considering 1949 as the eligibility cut-off results in somewhat similar results to the regression with the true eligibility measure. The coefficient for 2009 is significant at the 5% level and commensurate with the main result in Figure 2 – around 4 p.p. Note, however, that the pre-trends are not parallel for this placebo regression — the difference in differences is significant at 10% for 2005.

The positive and significant difference in differences found in 2009 for houses built before 1949 raises concerns that this category might drive our main result. To investigate this, we run our baseline regression on a sample that excludes the houses built before 1949 and includes all others. The results are presented in Figure A1 in the Appendix. The effect is mixed in 2009. It is statistically significant at the 10% level in the pooled regression, with a 3 p.p. effect, but not in the regression with household fixed effects. In contrast, the 2010 effect is statistically significant in both regressions and equal to roughly 3 p.p., in line with the whole-sample result. We therefore conclude that, while the oldest houses might significantly contribute to the high effect observed in 2009, this is less likely to be the case in 2010.

[6] Houses built in 1989 are not eligible for ZIGL, but we cannot distinguish them in the data — see Section 3

Figure 4: Placebo differences-in-differences.



Note: Placebo tests of yearly difference-in-differences from equation (1) for renovation decision (binary), with 95% (black) and 90% (red) confidence intervals. All regressions done after removing the true control group (year of construction after 1990). (a) — houses constructed before 1982 as placebo eligibility criterion; (b) — houses constructed before 1975 and (c) — houses constructed before 1949. All regressions include household FE.

7.2 Difference in Differences with Propensity Score Weighting

As discussed in Section 4, the eligible and non-eligible groups differ along several important dimensions, such as household age and income. The main results clearly show that controlling for these variables affects the results. To address the imbalance issue in a more comprehensive way, we use 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 results of the estimation are reported in Table A5 in the Appendix. Following Hirano and Imbens (2001), we then apply the inverse probability weighting to the data. These weights are combined with the survey weights used before.

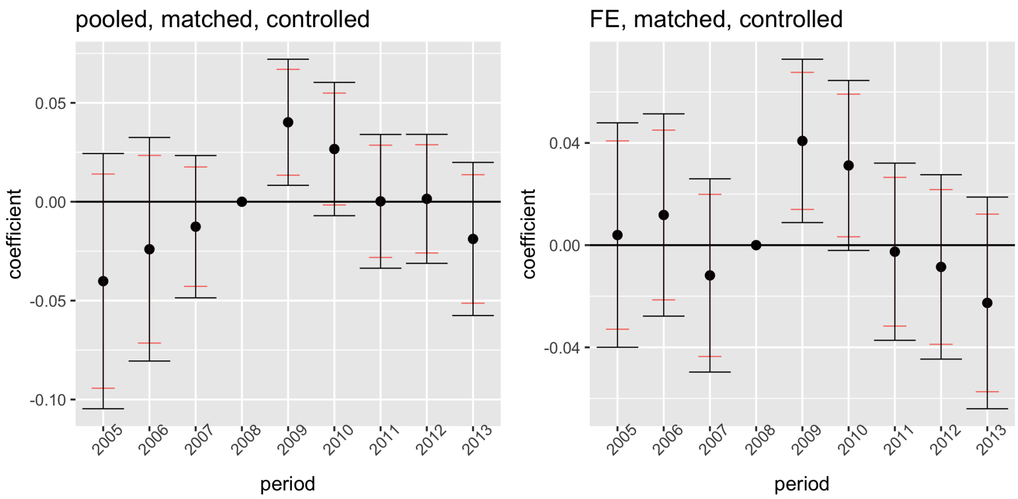
As depicted in Figure A2, all the observations in our sample fall within the common support area, implying they can all be used. Note that the very high frequencies of the eligible sub-sample in the high propensity score range reflect the fact that eligible households are much more frequent in the sample (around four times).

To check the effectiveness of the approach, we perform a balancing test with the new weights. The results reported in Table A6 of the Appendix show that half of the variables are 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 often used among the eligible).

Figure 5 presents the estimates of regression equation (1) with the inverse probability weighting based on propensity scores. The effect in 2009 increases to 4 p.p. in both pooled and household FE regressions, against 3.7 p.p. and 3.1 p.p. without weighting. These coefficients are significant at the 5% level. For 2010, the coefficients are 2.7 and 3.1 p.p. without and with FE, only the latter being significant at the 10% level. Table A7 in the Appendix presents detailed results of the estimation.

A better balance of covariates therefore generates remarkably similar results to the baseline estimation, with the effect somewhat stronger in 2009.

Figure 5: Effect of eligibility on renovation decision, with propensity score weighting.



Note: Estimates of yearly difference-in-differences from equation (1) for binary variable of renovation decision, using inverse probability weighting with propensity scores. 95% (black) and 90% (red) confidence intervals. Left plot — specification with household controls (both constant and time-varying), but no household FE; right plot — specification with household FE and time-varying controls. Time FE used in both specifications. Standard errors are clustered at household level. See Table A7 for regression results.

8 Discussion

Our results highlight two puzzles concerning both the level and the trend of home energy retrofits. What causes the overall participation level to be generally low, at least compared to initial expectations? What caused participation to consistently decline from 2011 onward? We discuss demand- and supply-side explanations for these two puzzles below.

8.1 Candidate explanations for low overall participation

On the borrower side, under-participation in interest-free loans has been documented in other sectors. Exploiting an eligibility-based difference-in-difference strategy similar to ours, Cadena and Keys (2012) find that one in six students in the United States turns down the interest-free loan offered by the Federal Student Aid program, even though it is offered as a default option. The authors interpret the outcome as self-control against over-spending. Both they and Wonder et al. (2008) invoke financial irrationality and debt aversion, which are consistent with mental accounting theories (Thaler, 1985) when faced with non-trivial computation such as debt (Herrmann and Wricke, 1998; Dynarski and Scott-Clayton, 2006). Debt aversion (Schleich et al., 2021), together with financial literacy (Blasch et al., 2019) are indeed increasingly discussed in relation to energy decisions. Still, results from choice experiments suggest that financial instruments can be crucial in helping households invest in energy efficiency (Schueftan et al., 2021). In the French case, imperfect information is another natural candidate. Yet preliminary evidence suggests borrowers are not dramatically less informed about the program than about other energy efficiency subsidy programs. Indeed, a survey conducted by ADEME (2016) indicates that retrofit contractors informed prospective homeowners about the reduced value-added tax 74% of the time, about the income tax credit 61% of the time and about ZIGLs 39% of the time. The same survey indicates that homeowner who conducted renovation works were aware of the same programs respectively 52%, 46% and 42% of the time.

On the lender side, the opportunity cost of providing ZIGLs might be so high that banks are reluctant to offer these loans. Giraudet et al. (2021b) indeed find that, all other things being equal, credit institutions offer higher interest rates for home retrofits than for assets of a comparable size, vehicles in particular. One reason might be that credit institutions perceive the former as a riskier investment. This is because home energy upgrades are increasingly recognized as a credence good subject to an array of information asymmetries (Giraudet, 2020). The riskiness is further exacerbated by the fact that, at the implementation of the ZIGL program, the burden of appraising the project fell into the banking institutions, which did not have expertise in this type of appraisals. Yet, transferring the burden of technically appraisal to retrofit contractors in 2015 did not result in any increase in the number of loans issued. Even more anecdotal evidence from online forums suggests that some credit institutions might discourage applications by charging prohibitive fees for ZIGL issuance that result in average percentage yields higher than those of the conventional loans they offer. Such strategies, which we refer to as 'obfuscation,' somehow exploit loopholes in the design of the program – the permission to charge fees of any amount. To the best of our knowledge, obfuscation of this kind is unstudied.

8.2 Candidate explanations for the decline in participation

To investigate the brutal drop that occurred at the turn of 2011, we conducted qualitative interviews with executives from the program administrator (SGFGAS) and the BPCE banking group. Importantly, we solicited interviewees who have been in office ever since the inception of the program. The interviews together suggest that, in the post-financial crisis context that prevailed in 2009, banks were eager to provide credit and therefore were very supportive of the program. The program however featured highly demanding administrative requirements which the administrator took time to learn to check. After an initial phase in which the banks relentlessly issued ZIGLs, the first control checks were completed in mid-

2010 and resulted in massive cases 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 made the banks realize how demanding the program truly was, urging them to pause issuance, and never to take over again.[7] Relatedly, the fact that many loan applications were ex-post rejected raises concerns that the associated retrofits were of a bad quality. One way to check this hypothesis would be to estimate the energy savings attributable to ZIGLs. This would indeed nicely complement our impact assessment with a cost-effectiveness assessment. Unfortunately, we were unable to do this. While energy expenditure data can be found in the dataset, they are roughly defined as a categorical variable. The categories are only a few and the energy spending was concentrated in the top category of the variable (€1,830 per year and more). Using them would thus necessarily lead us to under-estimate the difference in energy expenditure before and after retrofit.

Finally, the steadier and long decline that followed this event can plausibly be explained by the macroeconomic environment, in particular the general decline in market interest rates that resulted from quantitative easing by the European Central Bank. This trend has made the benefit of the program to households significantly smaller.

9 Conclusion

Using a rich survey panel dataset of about 45,000 observations from 10,000 households, we find that eligibility to the ZIGL program significantly increased home energy retrofits among eligible households, but only on the first two years of the program. The early effect is substantial on the extensive margin (+4 p.p., or +25%, to the rate of renovation) and, to a lesser extent, on the intensive one (+€1,100, or +9%). Importantly, it is most pronounced among the lowest income category, confirming the effectiveness of low-interest programs in easing access to credit. The results are robust to a range of alternative specifications.

Our results together suggest that ZIGL programs have a strong potential for encouraging home energy retrofits. Unfortunately, this potential remained largely untapped in the French case, due to a heavy administrative burden that took time to reveal itself and gave the program a bad reputation in the banking profession which several simplifications did not manage to dissipate.

Since late 2019, however, the program has taken a new turn. After new simplifications were added, not the least a loosening of the minimum performance requirements, participation is rising again. As one would expect from such a change, spending meanwhile decreased from an average €16,000 in 2019 to €12,000 in 2020. While it is good news that the program is gaining momentum again, the reduction in spending lessens one of the key merits of the previous version of the program – targeting comprehensive retrofits, which are likely to require additional leverage (Giraudet et al., 2021a). The program will be further simplified in 2022, in accordance with the one-stop-shop framework implemented at the EU level (Pardalis et al., 2021).

[7] Another contributor to the drop may be the suspension of the overlap between the program and its CITE counterpart in 2011 (see Section 2). Yet the reenactment of the overlap in 2012 did not reverse the trend, suggesting this mechanism played a modest role.

Much remains to be studied about the barriers to participation in ZIGL programs on both the demand and supply side. In subsequent research, we plan to investigate candidate barriers using data from *Banque de France*, coupled with administrative data for the universe of ZIGLs.

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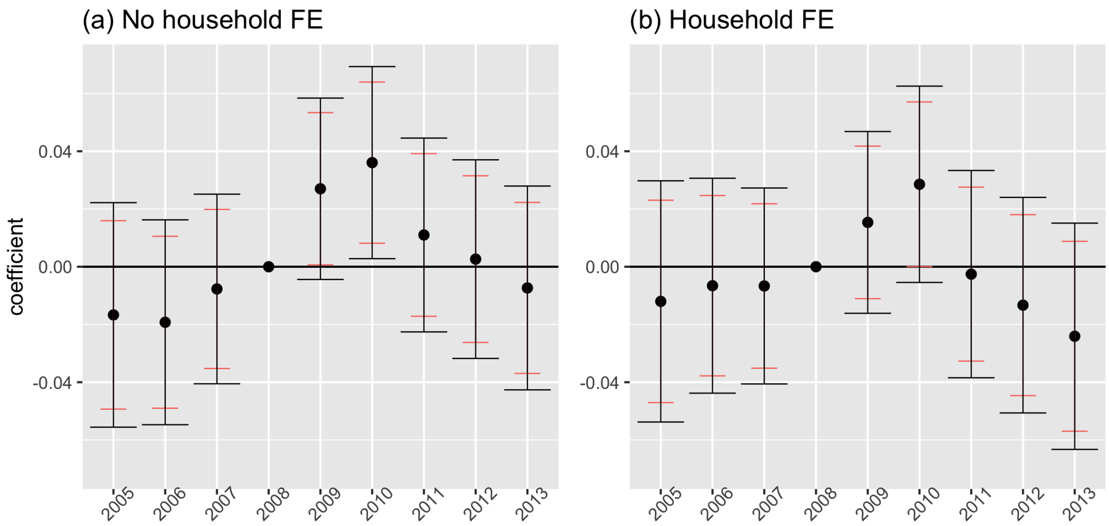
A Appendix

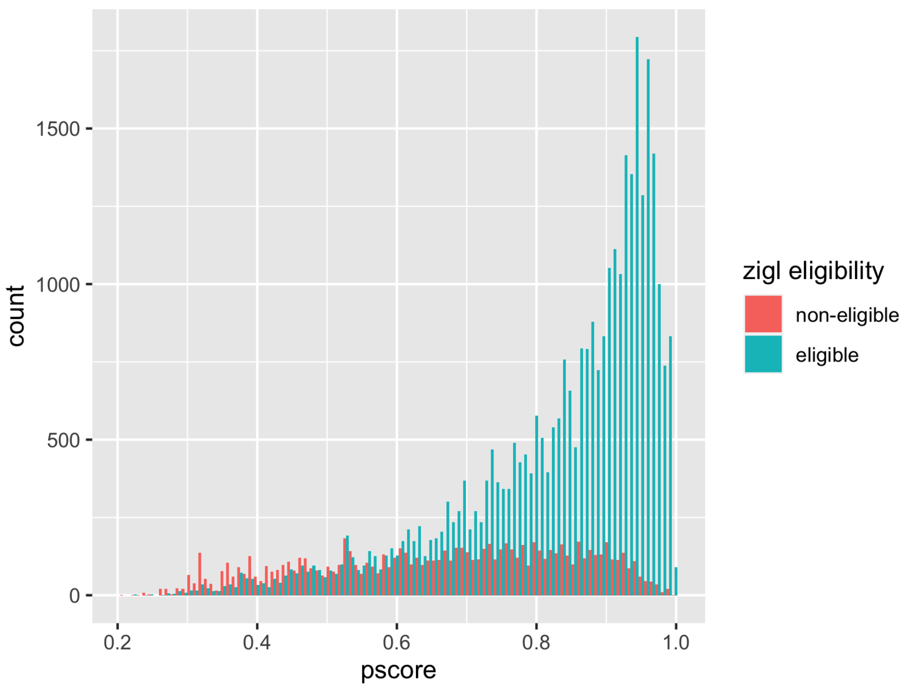
A.1 Figures

Figure A1: Effects of eligibility on renovation decision, excluding houses built before 1949.



Notes: Estimates of yearly difference-in-differences from equation (1) for renovation decision (binary), excluding houses built before 1949. Data comes from ADEME Survey. Back to Section 7.

Figure A2: Common support of treatment and control.



Notes: Propensity scores obtained from a logit regression of ZIGL eligibility on the covariates of Table A1, except region. Data comes from ADEME Survey. Back to Section 7.2.

A.2 Tables

Table A1: Description of control variables.

Variable	Values
Age of HH 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 HH head (*PCS*)	Agricultural; Trade/entrepreneur; Independent/management; Intermediary; Employee; Worker; Non-employed*
Income	Less than 19k; 19 to 23k; 22.8 to 27.6k; 27.2 to 36.6k*; 36.6 to 45.6k; 45.6k € and more
Agglomeration type	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	Less than 50 m^2; 50 to 74; 75 to 99 ; 100 to 149* ; 150 m^2 and more
Main energy for heating	Gas*, Electricity, Fuel Oil, Other
Heating system type	Individual non-electric*, Individual electric, Central
Dwelling type	House*, Apartment

Notes: * signals the omitted category in all regressions. Data comes from ADEME Survey. Back to Section 3.

Table A2: Effect of eligibility on renovation decision — extensive margin.

	Dependent variable:			
	Renovation this year			
	(1)	(2)	(3)	(4)
Eligible	0.073***	0.109***	0.022	0.027
	(0.012)	(0.012)	(0.021)	(0.021)
Eligible × 2005	−0.001	−0.002	0.008	0.011
	(0.019)	(0.019)	(0.020)	(0.020)
Eligible × 2006	−0.007	−0.007	0.004	0.007
	(0.017)	(0.017)	(0.018)	(0.018)
Eligible × 2007	−0.006	−0.006	−0.003	−0.003
	(0.016)	(0.016)	(0.016)	(0.016)
Eligible × 2009	0.040***	0.037**	0.033**	0.031**
	(0.015)	(0.015)	(0.015)	(0.015)
Eligible × 2010	0.030*	0.030*	0.027*	0.027*
	(0.016)	(0.016)	(0.016)	(0.016)
Eligible × 2011	0.009	0.008	0.001	−0.001
	(0.016)	(0.016)	(0.017)	(0.017)
Eligible × 2012	0.005	0.001	−0.011	−0.013
	(0.016)	(0.016)	(0.018)	(0.018)
Eligible × 2013	0.005	−0.002	−0.014	−0.017
	(0.017)	(0.017)	(0.018)	(0.019)
Controls		Yes		Yes
HH FE			Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	42,418	42,418	42,418	42,418
R^2	0.010	0.035	0.371	0.374
Adjusted R^2	0.010	0.033	0.186	0.188
Residual Std. Error	0.403	0.399	0.366	0.365

Notes: Column 1 to 4 are estimates of equation (1). Survey weights are applied. Figure 2 plots estimates of columns (2) and (4). See controls description in Table A1. Standard errors are clustered at household level. Data comes from 2008 ADEME Survey. Back to Figure 2.

Table A3: Effect of eligibility on renovation decision – two-period, with controls.

	Dependent variable:			
	Renovation this year			
	(1)	(2)	(3)	(4)
Eligible	0.070*** (0.007)	0.105*** (0.008)	0.019 (0.020)	0.025 (0.020)
Eligible×Post	0.021** (0.009)	0.018** (0.009)	0.011 (0.010)	0.009 (0.010)
Age < 25		0.060 (0.050)		−0.085 (0.071)
Age 25 to 34		0.151*** (0.014)		0.041 (0.034)
Age 35 to 44		0.100*** (0.012)		−0.006 (0.027)
Age 45 to 54		0.054*** (0.012)		−0.018 (0.021)
Age 55 to 64		0.049*** (0.008)		−0.010 (0.013)
Occup. Agriculture		−0.070*** (0.022)		−0.164** (0.081)
Occup. Trade.Entrep.		−0.044** (0.018)		0.001 (0.039)
Occup. Indep.Mngmnt		−0.010 (0.012)		−0.036 (0.028)
Occup. Intermediary		−0.022* (0.011)		−0.010 (0.024)
Occup. White-collar worker		−0.026** (0.012)		−0.010 (0.027)
Occup. Blue-collar worker		−0.015 (0.012)		−0.011 (0.027)
Agglom. Paris Area		−0.048** (0.020)		−0.134 (0.094)
Agglomeration > 100k inhab.		−0.001 (0.008)		−0.054 (0.034)
Agglom. 20 to 100k inhab.		0.005 (0.008)		0.027 (0.031)
Agglom. < 2k inhab.		0.002 (0.008)		0.009 (0.021)
Appartment		−0.050*** (0.007)		−0.054** (0.027)
Surface < 50 sq.m		−0.039*** (0.014)		−0.070*** (0.027)
Surface 50 to 74 sq.m		−0.028*** (0.008)		−0.034** (0.014)
Surface 75 to 99 sq.m		−0.016*** (0.006)		−0.017* (0.009)
Surface > 150 sq.m		0.013* (0.008)		−0.008 (0.011)
Income < 19k €		−0.027*** (0.007)		0.002 (0.011)
Income 19 to 23k €		−0.004 (0.008)		−0.004 (0.010)
Income 22.8 to 27.6k €		−0.002 (0.007)		0.002 (0.009)
Income 36.6 to 45.6k €		0.002 (0.007)		0.001 (0.009)
Income > 46.6k €		0.004 (0.008)		0.018 (0.011)
Heat. energy Elect.		0.060** (0.024)		−0.040 (0.032)
Heat. energy Fuel Oil		0.004 (0.007)		−0.077*** (0.023)
Heat. energy Other		0.045* (0.025)		−0.009 (0.034)
Heat. type Central		0.014 (0.010)		0.034 (0.026)
Heat. type Indiv. Elect.		−0.063** (0.026)		0.028 (0.039)
Heat. type Other		−0.035 (0.025)		0.031 (0.037)
HH FE			Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	42,418	42,418	42,418	42,418
R^2	0.010	0.034	0.371	0.374
Adjusted R^2	0.010	0.033	0.186	0.188
Residual Std. Error	0.403	0.399	0.366	0.365

Note: Columns (1) to (4) are estimates of Equation (2). Survey weights are applied. See Table A1 for the baseline (omitted) category of each categorical variable. Data comes from ADEME Survey. Back to Section 5.

Table A4: Effect of eligibility on renovation amount.

	Dependent variable:			
	Renovation amount, €			
	Full sample		HH with ≥1 renovation	
Eligible	1,244.0***	513.5*	938.6**	729.4
	(144.5)	(284.0)	(364.6)	(637.6)
Eligible × 2005	−14.7	225.6	516.2	573.9
	(220.5)	(229.0)	(571.9)	(577.2)
Eligible × 2006	−235.9	−2.0	359.1	465.6
	(175.3)	(190.2)	(458.7)	(482.8)
Eligible × 2007	−95.5	−85.4	380.4	255.0
	(171.3)	(176.2)	(449.4)	(461.3)
Eligible × 2009	369.8*	304.7	525.7	490.3
	(199.5)	(188.8)	(517.5)	(498.4)
Eligible × 2010	511.4**	477.8**	1,036.9**	1,201.3**
	(203.2)	(203.8)	(502.8)	(505.1)
Eligible × 2011	54.8	−3.2	243.5	475.3
	(197.1)	(204.8)	(490.9)	(502.8)
Eligible × 2012	−27.3	−214.8	97.7	127.9
	(194.2)	(215.6)	(490.0)	(532.7)
Eligible × 2013	−23.6	−185.6	221.0	269.8
	(221.9)	(248.0)	(569.5)	(612.5)
Controls	Yes	Yes	Yes	Yes
HH FE		Yes		Yes
Year FE	Yes	Yes	Yes	Yes
Observations	42,418	42,418	21,695	21,695
R^2	0.03	0.3	0.03	0.3
Adjusted R^2	0.03	0.2	0.03	0.1
Residual Std. Error	5,079.1	4,740.0	6,717.0	6,496.5

Notes: Estimates of equation (1) for amounts spent on renovation for full sample and subsample of households who renovated at least once over 2005-2013. Survey weights are applied. Standard errors are clustered at household level. Data comes from ADEME Survey. Back to Section 5.2.

Table A5: Propensity scores — regression.

	Dependent variable:
	Eligible
Age < 25	0.498*** (0.164)
Age 25 to 34	1.072*** (0.152)
Age 35 to 44	−0.863*** (0.103)
Age 45 to 54	0.032 (0.058)
Age 55 to 64	0.036 (0.031)
Occup. Agriculture	0.520*** (0.094)
Occup. Trade.Entrep.	0.560*** (0.087)
Occup. Indep.Mngmnt	0.451*** (0.084)
Occup. Intermediary	0.451*** (0.088)
Occup. White-collar worker	0.194** (0.082)
Occup. Blue-collar worker	0.621*** (0.091)
Agglom. Paris Area	−0.258*** (0.050)
Agglom. > 100k inhab.	−0.350*** (0.058)
Agglom. 20 to 100k inhab.	−0.709*** (0.055)
Agglom. < 2k inhab.	−0.732*** (0.054)
Surface < 50 sq.m	−0.075 (0.079)
Surface 50 to 74 sq.m	−0.393*** (0.078)
Surface 75 to 99 sq.m	−0.669*** (0.080)
Surface > 150 sq.m	−0.342*** (0.086)
Income < 19k €	−0.320*** (0.048)
Income 19 to 23k €	−0.474*** (0.047)
Income 22.8 to 27.6k €	−0.692*** (0.041)
Income 36.6 to 45.6k €	−0.870*** (0.046)
Income > 46.6k €	−1.012*** (0.048)
Heat. energy Elect.	−0.142 (0.137)
Heat. energy Fuel Oil	0.604*** (0.044)
Heat. energy Other	0.091 (0.145)
Heat. type Central	−1.546*** (0.085)
Heat. type Indiv. Electric	−2.293*** (0.159)
Heat. type Other	−1.952*** (0.161)
Appartment	−0.458*** (0.042)
Constant	4.302*** (0.151)
Observations	42,418
Log Likelihood	−21,336.330
Akaike Inf. Crit.	42,736.660

Notes: Estimates of logit regression for propensity scores. Survey weights are applied. Data comes from ADEME Survey. Figure A2 plots the predicted propensity scores for the eligible and non-eligible. Data comes from ADEME Survey. Back to Section 7.2.

Table A6: Balancing test with propensity score weighting (2008).

Variable	Category	Eligible (T) Mean	SD	Non-Eligible (C) Mean	SD	Diff	T-stat	p-value
Appartment	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	< 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
Heating main energy	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***
	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 the statistics calculated with inverse probability weighting using propensity scores, as well as the survey weights. Data comes from 2008 ADEME Survey. Back to Section 7.2.

Table A7: Effect of eligibility on renovation decision, with propensity score weighting.

	Dependent variable:			
	Renovation this year			
	(1)	(2)	(3)	(4)
Eligible	0.102***	0.101***	0.009	0.011
	(0.013)	(0.012)	(0.025)	(0.025)
Eligible × 2005	−0.049	−0.040	0.003	0.004
	(0.037)	(0.033)	(0.022)	(0.022)
Eligible × 2006	−0.027	−0.024	0.010	0.012
	(0.028)	(0.029)	(0.020)	(0.020)
Eligible × 2007	−0.007	−0.013	−0.011	−0.012
	(0.019)	(0.018)	(0.019)	(0.019)
Eligible × 2009	0.039**	0.040**	0.041**	0.041**
	(0.017)	(0.016)	(0.016)	(0.016)
Eligible × 2010	0.026	0.027	0.030*	0.031*
	(0.017)	(0.017)	(0.017)	(0.017)
Eligible × 2011	0.001	0.0002	−0.003	−0.003
	(0.018)	(0.017)	(0.018)	(0.018)
Eligible × 2012	0.006	0.001	−0.008	−0.009
	(0.017)	(0.017)	(0.018)	(0.018)
Eligible × 2013	−0.017	−0.019	−0.022	−0.023
	(0.020)	(0.020)	(0.021)	(0.021)
Controls		Yes		Yes
HH FE			Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	42,418	42,418	42,418	42,418
R^2	0.022	0.045	0.401	0.403
Adjusted R^2	0.022	0.044	0.224	0.226
Residual Std. Error	0.526	0.520	0.469	0.468

Notes: Estimates of equation (1), with inverse probability weighting using logit-estimated propensity scores, along with survey weights. Standard errors are clustered at household level. Figure 5 plots results of columns (2) and (4). Data comes from ADEME Survey. Back to Section 7.2.