Policies to finance energy efficiency: An applied welfare assessment

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Prepared by the Grantham Research Institute on Climate Change and the Environment, London School of Economics and Political Science

Partners: The Centre International de Recherche sur l'Environnement et le Développement (CIRED), and the German Institute for Economic Research (DIW)

Contact:

Dr. Daire McCoy

Research Officer

Grantham Research Institute on Climate Change and the Environment

London School of Economics and Political Science

Houghton Street

London WC2 2AE

Email: d.m.mccoy@lse.ac.uk

THE LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE

Grantham Research Institute on Climate Change and the Environment

Table of Contents

1. Executive Summary

This research is presented as three individual but linked work packages, initially setting up the problem and then examining the effectiveness of alternative solutions. The first work package (WP1) provides a thorough overview of the information asymmetries leading to under-investment in energy efficiency. The second work package (WP2) concentrates on loan facilities, specifically whether banks reward energy efficient projects by lending at lower rates to borrowers undertaking them. The third work package (WP3) provides an overview of the difficulty in assessing the cost-effectiveness of alternative programs, then examines just how effective energy efficiency measures actually are in delivering savings and whether they deliver cost-effective savings. A comparison of the relative cost-effectiveness of alternative initiatives is then provided.

Work-package 1

Energy efficiency, in particular home energy retrofits, can be seen as a credence good, the quality of which is never fully revealed to the buyer. This characteristic implies that energy efficiency measures often require expert services (e.g., energy audits) and are subject to a range of information asymmetries. An extensive review of these information asymmetries produced three main results. First, information asymmetries between landlords and tenants are significant, leading to both too little energy efficiency in rented dwellings and too much energy consumption when utilities are included in rental contracts. Second, dwelling energy efficiency, as measured by energy performance certificates (EPCs), tends to be capitalized into home prices, yet no counterfactual situation without EPC is available, so that it is unclear whether EPCs mitigate pre-existing information asymmetries between buyers and sellers. Third, the finance literature suggests that loan terms should be affected if the underlying asset is a credence good, yet this point has not been examined in the energy efficiency literature.

Work-package 2

The last point of WP1 has motivated an analysis of loan terms for home energy retrofit decisions. Financial intuition suggests that if retrofits effectively reduce energy expenditures, they should be appraised as a low-risk project and hence carry relatively low interest rates. The CIRED team has tested this prediction using posted interest rate data retrieved on the websites of French credit institutions on a weekly basis for two years (2015 and 2016). The analysis reveals that loan terms for home energy retrofits, contrary to intuition, carry relatively high interest rates. This is the result of an increasing spread between retrofits and other investments, only partially outweighed by an increasing discount for "green" projects. A possible explanation is that lenders use loan purposes to screen borrowers with a high willingness to pay and charge them high interest rates. From a policy perspective, these results call for more guarantees in the effectiveness of energy savings from home energy retrofits.

Work-package 3

The first part of WP3 is a review which illustrates the empirical challenges researchers face when evaluating energy efficiency programs while highlighting some recent academic studies. Particular attention is given to the difficulty in constructing a plausible control group when estimating the average impact of a program. Even when evaluations are conducted accurately, there remain questions of external validity that depend on market conditions and biases that arise from site selection. Finally, it is important to take into account the problem of free-ridership and rebound effects when assessing the cost-effectiveness of alternative programs.

Building on the previous work packages and part 1 of WP3, the final section provides an analysis of how well energy efficiency measures actually perform. Answering this seemingly straightforward question is crucial to understanding why households appear to under-invest in energy efficiency measures relative to what is socially or privately optimal, why lenders may or may not reward "green" investments and whether policies are actually cost-effective.

The analysis focuses on measures installed through the UK Supplier Obligations. Controlling for a range of empirical issues highlighted above by using state-of the art econometric techniques applied to an extremely large database of energy efficiency measures and metered energy consumption allows the research team to accurately quantify how well measures perform.

Results indicate that savings vary by household type, and over time, with households in more deprived areas experiencing much lower savings than those in more affluent areas. Not only this, but the savings erode more quickly over time - in some cases reducing by 50 percent within six years (for measures expected to last twice this amount of time). The measures are still generally net-present value (NPV) positive, but the returns are much lower than expected. This research also raises concerns over distributional issues given how the costs of policies are subsequently levied on households.

A comparison is then provided of the cost-effectives of the assessed interventions with a wide range of other initiatives, such as behavioural programmes, building code changes, subsidies and information provision. Results indicate that measures installed through the UK Supplier Obligations compare quite favourably with other policy initiatives.

2. Introduction

This report summarises research conducted at the Grantham Research Institute on Climate Change and the Environment, London School of Economics and Political Science (GRI-LSE), the Centre International de Recherche sur l'Environnement et le Développement (CIRED), and the German Institute for Economic Research (DIW), under the European Investment Bank (EIB) Institute Grant "Policies to Finance Energy Efficiency: an Applied Welfare Assessment" (EIBI/KnP/TT/ck (1-RGI-C311)). The report is produced in accordance with the provisions under the contract between the EIB and the LSE, which calls for a final technical report at the end of the third year of the project.

The three-year project initially was to run from April 2015 to March 2018. In Autumn 2017 an additional six month no-cost extension to the project was agreed. This extended the end-date of the project to October 2018. This is the final report and describes all work undertaken as part of this project.

The motivation for this project was to try to better understand the economics of energy efficiency. Energy efficiency is the most consensual option to meet energy saving targets. Many engineering studies (for instance McKinsey & Co. (2009)) have shown that it is the most cost-effective way to save energy and carbon dioxide emissions. Despite this, current investment levels appear suboptimal from an economic perspective. As a consequence, numerous policies are being implemented to meet energy saving targets. This work aims to provide some evidence regarding the economic efficiency of these policies.

The document is structured as follows: Section Three a review of activities; Section Four describes the dissemination activites undertaken by the research team; Section Five provides an overview of the project management; Section Six provides an overview of staffing at the three organisations over the course of this project. Following this, Sections Seven to Ten contain the detailed research projects undertaken. The results of which are now described in more detail.

3. Review of Activities

The aim of the project, as stated in the original proposal, is to evaluate the motivation for, and the effective performance of, energy efficiency subsidies and loan facilities, specifically looking at the housing sector in the UK, France and Germany. The research focuses on the identification of the market failures that hamper the financing of energy efficiency and the policies that can address these. The research then assesses how loans and other policies targeted at energy efficiency projects perform.

To achieve this, we have organised the research around three work packages:

- A first work package aiming to develop a conceptual framework and define policy evaluation criteria (WP1);
- A second work package concentrating on loan facilities (WP2);
- And a third work package looking at the performance of publicly funded energy efficiency measures (WP3).

Two other work packages complete the project, one dedicated to stakeholder engagement and dissemination activities (WP4) and another one putting in place a management structure between the three partner institutions for the good running of the project (WP5). An overview of work packages and partners responsible is provided in Table 1.

Table 1: Overview of work packages

Work Package	Title	Lead Partner
1	Energy efficiency as a credence good: A review of informational barriers to building energy savings	CIRED
2	How do lenders price energy efficiency? Evidence from posted interest rates for unsecured credit in France (lead partner: CIRED)	CIRED
3.1	Evaluating policy instruments for improving the efficiency of the building stock (lead partner: DIW)	DIW
3.2	An evaluation of the performance of energy efficiency measures and policies (lead partner: LSE)	LSE
4	Dissemination activities (lead partner: LSE)	LSE
5	Project management (lead partner: LSE)	LSE

All primary research activities have now been completed. However, over the coming year the research team will endeavour to publish all outputs from this project in top-tier scientific journals.

Over the course of the project, some issues emerged which caused a slight deviation from the original plan. (i) Due to issues regarding data availability there was a slight deviation in the final content of WP3 from what was originally proposed. This was explained to and agreed with the EIB over the course of the project (ii) Due to staffing issues at CIRED, the project team requested and were granted a no-cost extension of six months. Further information on the above is provided below in Section 5.

4. Dissemination activities: WP4 (lead partner: LSE)

Publications arising from this project

WP1

- A working paper of WP1 has been published in the French Association of Environmental and Resource Economists (FAERE) series, which ensures a wide visibility within the energy economics community. This can be accessed using the following link: Link to published working paper 1.
- This work was submitted to the Economics of Energy & Environmental Policy journal in February 2018. It is still under review with the journal. Edited by the International Association for Energy Economics, the journal is the most important publication in the field of energy at the intersection of economic research and policy.

WP2

- A working paper of WP2 has been published in the French repository HAL. This can be accessed using the following link: Link to published working paper 2.
- The manuscript will soon be submitted to a top-tier general-interest economics journal.
- The data collected in WP2, once cleaned and fully exploited, will be archived in an open-access data repository with a digital object identifier (DOI), so as to allow peer researchers to reuse them in other research projects.

WP3

- A working paper of WP3 has been published in the LSE, Grantham Research Institute Working Paper Series, sent to over 2500 stakeholders. This can be accessed using the following link: Link to published working paper 3.
- This work was submitted to the Journal of the Association of Environmental and Resource Economists in November 2018. This is the official research journal of the Association of Environmental and Resource Economists, and is the most important publication in the field of environmental and resource economics

Events organised by the project team

Workshop at DIW Berlin

In March 2017 a workshop was held at DIW in Berlin: "Policies to finance energy efficiency in the building stock" (March 1, 2017). Attendees included a range of policy makers, industry and academics. The event comprised 4 sessions:

a. The first of which focussed on the problems involving the estimation of causal effects of policies to finance energy efficiency. Foremost, these problems revolve around the design of an empirical identification strategy. The purpose of such a strategy is to allow robust judgements about what would have happened in the absence of support policies. The comparison of the outcome under government support and the counterfactual situation of its absence informs about the effect of the government support. The participants debated how the existing problems could be overcome and agreed that policy makers should involve researchers before the implementation of policy programs.
b. The second session focussed on grants and preferential loans. After giving an overview on the use of such policy measures across Europe, the presentation introducing the topic expounded the problem that the measured effects of efficiency improvements often fall short of the expected ones. A prominent reason why this might be the case is the rebound effect. In essence, efficiency improvements in the building sector reduce heating costs which triggers additional demand.

c. The third session comprised of two presentations. The first was given by Jonas Geisler, the deputy head of the German Federal Energy Efficiency Center (BfEE). It focussed on energy contracting as a means to finance energy efficiency. Essentially, contractors active in the building sector provide for new and more efficient heating equipment. The customers continue to pay for heat at the rate that prevailed before the efficiency improvement. The difference in payment caused by the efficiency increase refinances the contractor. The presentation addressed pros and cons of this technique. The second presentation of the third session contrasted financial measures with command and control instruments, in particular energy efficiency obligations.
d. The fourth sessions offered workshop participants the opportunity to raise questions and to discuss any of the topics presented in the course of the workshop.

End-of-project event at the Grantham Research Institute, London School of Economics

In May/June 2018 the research team organised an end-of-project workshop at the Grantham Institute. This consisted of a public lecture delivered by Professor Massimo Filippini of ETH Zurich on the evening of May 31st, followed by an event during the day of June 1st. Both events were attended by EIB staff. Other attendees number 50-60 in total at each day included, policy makers, members of the business community and LSE students

- **Prof. Massimo Filippini public lecture - "Energy related financial literacy, bounded rationality and the energy efficiency gap". May 31st.** Prof. Filippini delivered a lecture describing the energy efficiency gap, energy literacy, financial literacy, bounded rationality and how these concepts are related. Following this he will described two randomised-controlled trials which test the impact of decision support tools aiming to encourage the adoption of energy efficient appliances. Public policy implications of the results were then discussed. This event was chaired by Daire McCoy of the GRI. Further details are available here.

- **Half-day workshop – "Policies to finance energy efficiency", June 1st.** This event brought together key academics, policy makers and practitioners to present and discuss the results from this project and broader issues relating to financing energy efficiency. Further details on this event are available here. The workshop was structured as follows:
  - Presentation of results from the EIB University Research Sponsorship (EIBURS) project – Policies to finance energy efficiency. This was chaired by Francois Cohen of the University of Oxford and included the following presentations:
    1. "Energy efficiency as a credence good – A review of informational barriers to energy-efficiency investment." Louis-Gaëtan Giraudet (CIRED Paris)
    2. "Evaluating policy instruments for improving energy efficiency in the building stock." Puja Singhal (DIW Berlin)
    3. "How do lenders price energy-efficiency loans? Evidence from France." Anna Petronevich (CIRED Paris)

4. "How well do energy efficiency measures actually perform?" Daire McCoy (Grantham Research Institute, LSE)

- Following this we hosted a panel discussion on "Financing energy efficiency". This was chaired by Dr. Radhika Khosla (Research Director, Oxford India Centre for Sustainable Development, University of Oxford). Panellists included Peter Sweatman (CEO, Climate Strategies and Partners), Isidoro Tapia (Energy Economist, European Investment Bank), Ioannis Orfanos (Business Partner, Energy Transformation and Commercial Advisory, BEIS), Dr. Gesche Huebner (Senior Research Associate, Bartlett School Environment, Energy & Resources, UCL).

Conferences and seminars

In addition to the above organised events, the research team presented the analysis and results from this project at a wide range of international conferences and seminars.

Year 1: 2016

In 2016 the research team presented complementary energy efficiency activities closely related to this grant at several international energy efficiency conferences and events. These conferences provided an opportunity to discuss energy efficiency policies (mostly in Europe and the US) and also present the EIB project to other researchers:

François Cohen presented:

- "Consumer Myopia, Imperfect Competition and the Energy Efficiency Gap: Evidence from the UK Refrigerator Market" to the EIB Energy Efficiency team.
- "Adapting American Homes to Climate Change" at the 35th International Energy Workshop (Cork, 1-3 June 2016).
- "Adapting American Homes to Climate Change" at the Urban and Regional Economics Seminar of the Spatial Economics Research Centre of the London School of Economics (November 2016).

Daire McCoy presented:

- "The Impact of Home Energy Efficiency Upgrades on Social Housing Tenants" at the Grantham Institute Workshop, November 2016.

Louis-Gaëtan Giraudet presented:

- his work on information problems in Florence, EUI Annual Climate Conference, December 2016
- "Moral hazard and the energy efficiency gap: Theory and evidence" at the 2016 Energy Conference, Mannheim, Germany.

Year 2: 2017

The research team presented the work at a number of international academic and policy conferences:

Daire McCoy presented the work-in-progress and results from WP3 at:

- 2nd AIEE (Italian IAEE) Energy Symposium. "Current and Future Challenges to Energy Security". Rome November 2017
- Florence School of Regulation Annual Climate Conference 2017. Florence, November 2017

Louis-Gaëtan Giraudet presented his paper on "Moral hazard and the energy efficiency gap: Theory and evidence" at the European Association of Environmental and Resource Economics Conference, Athens, Greece.

Year 3: 2018

Daire McCoy presented the results from WP3 at the following events:

- VI International Academic Symposium: Facing the Energy Transition: Markets and Networks. IEB Barcelona, February 2018
- UK Network for Environmental Economists, envecon, March 2018
- World Congress of Environmental and Resource Economists, Gothenburg. June 2018
- LSE & Ofgem Energy and Environment Workshop, London. July 2018
- British Institute of Energy Economics, Oxford, September 2018

The results of WP2 were presented at the following events:

- LG Giraudet, March 16, 2018 : invited seminar, ETH Zurich (Switzerland)
- LG Giraudet, April 25, 2018: invited seminar, Mines ParisTech (France)
- A Petronevich, May 3, 2018: internal seminar, CIRED (France)
- A Petronevich, May 15, 2018: 7th Mannheim Energy Conference (Germany)
- LG Giraudet, May 16, 2018: Université of Montpellier workshop on energy efficiency (France)
- A Petronevich, May 16, 2018 Association française de sciences économiques, Paris (France)
- A Petronevich, May 25, 2018: ISEFI, Paris (France)
- LG Giraudet, June 21, 2018: 8th Atlantic Workshop on Energy and Environmental Economics, A Toxa (Spain)
- LG Giraudet, June 29, 2018: World Congress of Energy and Resource Economists, Gothenburg (Sweden)
- A Petronevich, August 30, 2018: European Economic Association, Cologne (Germany)

5. Project management: WP5 (lead partner: LSE)

As outlined in the proposal, the project is executed by research teams at three institutions, GRI-LSE, CIRED and DIW. Regular phone meetings between all partners have been held to ensure good advancement on all planned activities, keep track of the deliverables and address any emerging issues or risks.

Group meeting between all partners were held at:

- DIW Berlin (March 2, 2017). This consisted of presentations of each work package followed by detailed discussions and planning of next steps.
- LSE London (May 31, 2018). This consisted of project review and planning of final report

Revisions to WP3

For WP3 in particular, a difficulty arose due to the impossibility of getting the relevant administrative datasets merged in order to address all of the original deliverables. The analysis therefore focused on the National Energy Efficiency Framework Database (NEED). This allows an analysis of the effect of government and privately funded energy efficiency investments on energy consumption over time. It is not possible to identify specific schemes from this dataset, but we do know the proportion of various measures deployed within each scheme. This will allow us to determine the cost effectiveness of certain measures and to infer the cost-effectiveness of schemes based on these results.

The original aim of work package 3 was to examine the welfare impact of selected energy-efficiency policies. Specifically, this work package was to:

1. Examine the windfall effect of subsidies. How this varies by the type of investment.
2. Using statistical techniques, assess the energy savings imputable to the policies under study, then provide a cost-benefit analysis.
3. Discuss the statistical methodology developed with the objective of providing guidance to EU policymakers on data requirements and processing methods to evaluate future and on-going energy efficiency programmes.
4. Provide guidance on how to increase the cost-effectiveness of policies aiming to select and fund energy efficiency projects.

The empirical paper described in this report can address points 2, 3 and 4. However, we were unable to specifically address point 1 empirically. To account for this we have provided instead an extensive review of the principal problems affecting evaluations of energy efficiency policies and how best to overcome them. This issue was highlighted to the EIB in both annual reports and during meetings.

Six-month no-cost extension

In August 2017 it became apparent that due to staffing issues CIRED would have some difficulties exhausting their share of the budget for WP2 by the end of the project. Laurent Faucheux who had been working on the project at CIRED left to take up a position at the OECD. It took some time to recruit Laurent’s replacement.

As a result, the work on WP2 was delayed by several months. This personnel issue raised the prospect that CIRED would struggle to complete WP2 and exhaust their share of the funding by March 2018. The research team felt in prudent to ask the EIB for a no-cost extension of six months. This was agreed with Edward Calthrop, Fulceri Bruni Roccia and Henry von Blumenthal in October 2017.

6. Organisation and Staffing

GRI-LSE

Antoine Dechezleprêtre (Associate Professorial Research Fellow) contributed to WP3-5 and had the overall oversight of the project. In June 2017 Antoine took leave from the LSE to join the OECD for a 2-year position as Senior Economist jointly based in the Economics Department and the Environment Directorate. As a consequence, the daily management of the project was transferred to Daire McCoy.

Francois Cohen (Research Officer) contributed to WP3 and was part of the initial project team. In October 2016 Francois left LSE for the Graduate Institute in Geneva and was replaced by Daire McCoy at the end of his two-year post-doc.

Daire McCoy (Research Officer) replaced Francois and took responsibility for WP3 in October 2016. In June 2017, following Antoine's departure, Daire took over Principal Investigator duties for the entire project.

Raphaela Kotsch, a Master's student in Environmental Economics and Climate Change at LSE also worked on this project, under the supervision of Daire McCoy at the Grantham Research Institute and Sefi Roth, Assistant Professor of Environmental Economics, Department of Geography and Environment. Her thesis will focused on evaluating energy efficiency investments and contributed to WP3.

CIRED

Louis-Gaetan Giraudet (Senior Research Fellow) is responsible for the completion and is the main contact for WP1-2.

Laurent Faucheux completed his PhD in Economics at CIRED. He graduated with Master's Degrees in Economics of Sustainable Development, Environment and Energy from AgroParisTech in 2012. He was responsible for collecting the data and contributed to the empirical analysis in WP2. Laurent now works as an independent consultant

Anna Petronevich received her PhD from Université Paris 1 and Università Ca'Foscari. She was responsible for the core of the analysis of WP2. Anna is now with the Banque de France.

DIW

Karsten Neuhoff is head of the Department for Climate Policy at the DIW and professor for Energy and Climate Policy at TU Berlin. He is responsible for coordinating DIW's contribution to the project.

Anne Schopp completed her PhD in Economics at DIW Berlin and moved to work at the World Bank in Washington, D. C. She had led the initial project application.

Nolan Ritter took over the main duties of the project following Anne Schopp's departure. Ritter was a postdoctoral researcher at the Department for Climate Policy at the DIW and worked on the project from 1 April 2015 until 31st March 2018.

Puja Singhal, currently Research Associate at the Climate Policy department at DIW Berlin and a PhD Student, took responsibility for presenting the final output at the GRI Workshop at LSE in May 2018.

7. Work Package 1. Energy efficiency as a credence good: A review of informational barriers to building energy savings (lead partner: CIRED)

Author: Louis-Gaëtan Giraudet (Ecole des Ponts ParisTech, CIRED),

7.1 Abstract

Information problems have early been suspected to be the main barrier to energy-efficiency investment. I review the vast yet piecemeal research that has been carried out since. Focusing on energy efficiency in buildings, I organize the review around the concept of credence good: just like that of auto repairs or taxi rides, the quality of energy-efficiency measures is never fully revealed to the buyer; as a result, it is subject to multiple information asymmetries. My first contribution is to distinguish symmetric-information problems from information asymmetries. The former arise when information is either incomplete or imperfect, but equally shared by contracting parties; as non-market failures, these can be addressed by technological progress and insurance markets. My second contribution is to give structure to the information asymmetries associated with energy efficiency by disentangling screening, signalling, moral hazard and price discrimination within a variety of contractual relationships involving buyers and sellers, owners and renters, and borrowers and lenders. I find evidence of information asymmetries to be compelling in landlord-tenant relationships, unclear in real estate markets, and scarce in retrofit contracting and financing. I conclude by discussing the intricacies between informational and behavioural problems in energy-efficiency decisions.

7.2 Introduction

Energy-efficiency investments in residential and commercial buildings have uncertain returns. Long payback periods make them sensitive to an array of contingencies. Their net present value depends on stochastic factors such as future energy prices and weather conditions. It moreover depends on heterogeneous factors such as decision-makers' preferences (e.g., tolerance to cold, lighting habits) and constraint sets (e.g., physical properties of buildings, energy distribution infrastructure). To complicate matters further, many energy efficiency technologies require expert services, notably installation tasks, the quality of which can be difficult to verify. Lastly, energy efficiency measures involve a number of stakeholders, all of whom having vested but not necessarily aligned interests. This frequently includes, in addition to buyers and sellers, users of energy-consuming assets and, as purchase prices can be substantial, credit suppliers. In this context, who's to blame if an insulation investment doesn't deliver as promised? The tenant behaving in unexpected ways, a non-diligent installer, flawed engineering simulations, or simply bad luck with weather forecasts?

Such a bewildering array of possible answers illustrates the credence-good nature of energy efficiency (Sorrell, 2004). By definition, the value of credence goods is never fully revealed to the buyer, even long after purchase. Classical examples include medical treatments, taxi rides or auto repairs. As illustrated above, energy efficiency shares with these counterparts the following characteristics: sellers face heterogeneous buyers; the quality of the product is not easily verifiable; nor is it subject to complete liability rules. Altogether, these characteristics create a variety of information asymmetries, including adverse selection, moral hazard and price discrimination (Dulleck and Kerschbamer, 2006).

Albeit pervasive in the economy, information asymmetries have specific implications in the context of energy efficiency. As we shall see in this essay, they can explain low uptake of energy efficiency, a long-standing paradox known as the energy efficiency gap (Jaffe and Stavins, 1994). The problem has initially been identified through abnormally high implicit discount rates in decisions to purchase energy-efficient assets, suggesting that consumers discard supposedly profitable investment opportunities (Hausman, 1979; Train, 1985). More recent studies document another manifestation of the energy-efficiency gap, namely that energy savings measured after investment underperform those predicted by engineering simulations before investment (Metcalf and Hassett, 1999; Fowlie et al., 2015). Three categories of economic problems are usually put forward to explain the energy-efficiency gap: market failures that truly impair socially desirable energy-efficiency investments; non-market failures that restrict investment without affecting social welfare; and behavioural anomalies leading to individually irrational investment, with unclear implications for social welfare (Gillingham et al., 2009; Allcott and Greenstone, 2012; Gerarden et al., 2017).

Informational problems have been early pointed out as the main cause of the energy-efficiency gap (Howarth and Andersson, 1993; Huntington et al., 1994). As research has grown substantially since, the contention can now be examined. I hereby take stock and review the vast yet piecemeal research into information in energy-efficiency decisions. A preliminary finding is that information problems are ill-characterized within the usual three-fold categorization. I sort this out by stressing the dichotomy between market and non-market failures, which I restate as one between, respectively, asymmetric and symmetric information. I thereby complement existing research that has focused on behavioural anomalies as the main category of problems at the source of the energy-efficiency gap (Gillingham and Palmer, 2014; Allcott, 2016). My review is closest to Ramos et al. (2015), who also review information problems, with the important difference that I place less emphasis on behavioural problems and more emphasis on information asymmetries.

My contribution is two-fold. First, I find symmetric-information problems to be important. This includes incomplete information – e.g., infrequent billing of energy use, incomplete disclosure of product attributes, need for pre-retrofit audits – and imperfect information – e.g., uncertainty about energy prices and weather conditions. These problems are frequently mistaken for information asymmetries or behavioural anomalies, which is a source of overestimation of the energy-efficiency gap. While technological progress and insurance markets should suffice to overcome them, the effectiveness of these private solutions has not been examined.

Second, I find information asymmetries to be of a broader variety than previously thought. Assessment of their magnitude is however subject to methodological caveats. I disentangle screening, signalling, moral hazard and price discrimination within a variety of contractual relationships involving buyers and sellers, owners and renters, and borrowers and lenders. Information asymmetries appear to be important in building rental (signalling), in particular when rents include utility expenditures (moral hazard, screening). Evidence is more mixed in building sales. Buyers are found to respond to energy efficiency, yet only a handful analyses separate out the effect of energy-efficiency labels from that of other observable energy-efficiency characteristics. Lastly, important information asymmetries have been overlooked in the installation (moral hazard, signalling) and financing (screening, moral hazard, price discrimination) of energy-efficiency measures. This remains an important research gap, as these transactions are key to scaling up energy efficiency.

The review focuses primarily, though not exclusively, on evidence gathered from revealed-preference studies conducted in the residential sector. It does not address in detail behavioural anomalies, a problem highly relevant to energy-efficiency decisions yet fairly well covered elsewhere. Their relationships with information asymmetries, which are conceptually and empirically important, are nevertheless discussed briefly at the end of the paper. Another discussion follows on what can be expected from rapidly developing information technologies in overcoming barriers to energy efficiency.

The review proceeds as follows. Section 2 reviews symmetric-information problems. Section 3 introduces various types of information asymmetries with the example of home energy retrofits. Section 4 details adverse-selection problems. Section 5 details principal-agent problems. Section 6 puts the findings in perspective and Section 7 concludes.

7.3 Symmetric-information problems

I define symmetric-information problems as information imperfections or gaps identically faced by contracting parties. These are not market failures in that no party extracts an informational rent from the other. Market outcomes can be improved by information technologies, the development of which does not *a priori* require public support.[1] Though symmetric-information problems are normal components of well-functioning markets, the trouble in energy-efficiency research is that they are often ignored when predicting economically efficient levels of energy efficiency. This consistently leads to overestimation of the energy-efficiency gap.

7.3.1 Incomplete information

Incomplete information here denotes situations in which part of the information needed to make a decision is missing, or costly to obtain with current technology. The problem can be identified by observing changes in adoption patterns when people are provided with more complete information about energy efficiency. As detailed below, incomplete information affects several decision variables.

[1] Public support can be warranted if information technologies are subject to classical innovation market failures. The question of whether this is the case in the context of energy efficiency is outside of the scope of this paper.

7.3.1.1 Energy operating costs: Evidence from infrequent billing

The cost of operating energy-consuming devices is usually not known in real-time.[2] In the absence of consumption displays, which remain far from widespread, information is incomplete in at least two respects: it is only provided occasionally when fuel tanks are filled or infrequently when electricity and natural-gas meters are monitored; it concerns a bundle of usages. How does more complete information affect market outcomes?

A number of studies have examined experiments increasing the frequency of information through smart metering or in-home displays. These so-called feedback interventions, extensively reviewed in Abrahamse et al. (2005), Fischer (2008), Delmas et al. (2013), and Buchanan et al. (2015), are generally implemented by electricity utilities. They initially produced mixed results. Abrahamse et al. (2005) find little impact based on 38 studies. Scepticism is shared by Buchanan et al. (2013) who even document cases where more frequent information increased energy use. Delmas et al. (2013) draw slightly more positive conclusions, estimating an average reduction in energy use of 7.4% from 156 studies.

More recent studies tend to confirm the negative yet modest effect of information frequency on energy use. Matsukawa (2004) finds a significant effect of electricity monitoring devices in a Japanese experiment. Houde et al. (2013) ran an experiment with 1,500 employees from Google and found that participation in the feedback program yielded an average reduction in electricity use of 5.7%, persisting up to four weeks. In a similar experiment involving 1,500 Austrian households, Schleich et al. (2013) find an average 4.5% reduction of electricity use attributable to getting feedback, however concentrated around the median of the distribution. Delmas and Lessem (2014), in an experiment on UCLA campus find that real-time feedback was ineffective, while publicly visible conservation ratings reduced electricity use by 20%, with more effect for above median energy users. Jessoe and Rapson (2014) find that informed households are three standard deviations more responsive to price variations than uninformed households and that this cannot be attributed to price salience. Sexton (2015) studies the somewhat reverse experiment. The author finds that enrolment in automatic bill payment (which decreases the frequency with which consumers receive information) increases electricity use by 6% to 7%. Chen et al. (2015) find evidence that consumers inaccurately estimate energy use from appliances. Lastly, Tiefenbeck et al. (2016) finds a large effect of 22% on showering.

Overall, the effect of more frequent information on energy use seems to be specific to individual preferences, to the point that it its sign is ambiguous. It also seems to be more effective when targeting specific energy services. Lastly, it seems to vanish when information frequency is reverted to normal. One difficulty for evaluation is that most experiments include other treatments such as tips or comparison with peers, which

[2] I focus here on information about energy quantities, which is more often missing than information about energy prices. Still, it is important to note that peak versus off-peak electricity prices and energy tariff menus are not always displayed transparently (e.g., Sexton et al., 1989).

might confound identification of the purely informative effect. We will return to that point in Section 6.

7.3.1.2 Performance of standardized products: Evidence from energy labels

Next to information available while operating energy-consuming durables, information available at the time of purchase might also be incomplete. For standardized products such as electrical appliances, information about product performance is generally produced by normalized engineering calculations and displayed through labels. Assuming that labels are trustworthy,[3] how do consumers respond to the more complete information they convey? As we shall see, here too it is difficult to disentangle information and behavioural effects.

A few studies have examined the impact of the EnergyGuide label, a mandatory label implemented in 1979 in the United States reporting a cost figure based on average national usage and energy prices. Houde (2014) examines refrigerator purchases and finds that a fraction of consumers respond to this piece of information in a privately rational way. Meanwhile, others over-respond, an effect the author attributes to the coexisting Energy Star label, a voluntary label providing coarser information. A third fraction of consumers do not respond to either label. Newell and Siikamäki (2014) also find that Energy Star leads to cost-effective decisions, while over-reaction cannot be excluded. Davis and Metcalf (2016) find a heterogeneous response to EnergyGuide in an online stated-choice experiment, with more relevant information about local energy price leading to more rational decisions.

Mandatory labels in place in the European Union and China are framed within a discrete performance scale, thereby reconciling the accuracy of EnergyGuide and the conciseness of Energy Star. Zhou and Bukenya (2016) show in a discrete choice experiment that consumer's mean willingness-to-pay for efficient air-conditioning systems increased when the performance was framed in a more segmented way. The effect is more pronounced at the high-utilization end of the distribution. In the European Union, a similar experiment was conducted by Andor et al. (2016), who find results similar to Houde (2014), namely that some people respond to information only, while others respond to norms. In contrast, in an eye-tracking experiment, Waechter et al. (2015) find little impact of labels in decision-making.

In case labels are not sufficient, sales agents can offer an additional information channel. A few studies have examined this contention in field experiments. Anderson and Claxton (1982) found a positive impact of sales staff support on label awareness, but no apparent impact on refrigerator choice in 18 department stores in Western Canada. Likewise, Kallbekken et al. (2013) find no statistical effect of training of sales staff on the purchase of tumble driers and fridge-freezer in six megastores in Norway. In a randomized controlled trial involving 20,000 agents in call centres of a large US retailer, Allcott and Sweeney (2016) find that, unless combined with large rebates,

[3] At least two caveats apply here. First, the tests preceding label attribution could be subject to falsification, just like the widely publicized Volkswagen case revealed in the automobile sector (U.S. EPA, 2015). To my knowledge, the issue has not been investigated in appliances and other energy-consuming assets. Second, sellers can exploit labels to price-discriminate. We return to that point in Section 5.2.

information and sales incentives alone have zero statistical effect on the sales of water heaters.

7.3.1.3 Performance of tailored measures: Evidence from energy audits

In large-scale projects such as home energy retrofits, which combine several measures and products within an idiosyncratic architectural layout, ex ante assessment of energy savings cannot be standardized. Investment appraisal requires customized audits which typically come at a cost of a few hundred dollars (Alberini and Towe, 2015; Palmer et al., 2015).

Do audits produce accurate predictions? Evidence so far suggests a negative answer. The problem was first identified by Metcalf and Hassett (1999), who found that returns to insulation underperformed audit predictions. The result has recently been confirmed by other studies, such as Fowlie et al. (2015), Graff Zivin and Novan (2016) and Giraudet et al. (2018). Graff Zivin and Novan (2016) find that 79% of predicted savings are actually realized. Giraudet et al. (2018) find similar figures, on average, with ratios ranging from 31% to 352% depending on the measures considered. The discrepancies come from measurement errors and complexities inherent in thermal simulation algorithms (de Wilde, 2014; Hsu, 2014).[4] They can also be due to market failures such as moral hazard, as we will see in Section 5.

The next question of interest is: how (possibly inaccurate) audits modify investment decisions? This can be directly assessed by observing purchase behaviour. Early assessment of McDougall and Claxton (1983) found little or no effect of audits on homeowner's conservation activities. Frondel and Vance (2013), applying a mixed logit model in Germany find a mean positive effect, though with substantial heterogeneity, some people exhibiting negative responses to audits. Murphy (2014) finds even more counter-intuitive results in the Netherlands, with a treated group not reacting to audits while non-treated individuals make more energy-efficient investments. Palmer et al. (2013, 2015) find in a survey that the depth of an audit, as measured by the inclusion of such items as energy bill assessment, blower door test or infrared imaging, is an important determinant of follow-up on audit recommendations. Considine and Sapci (2016) estimate a significant but modest effect of audits on investment in a discrete-choice analysis of a program conducted in Wyoming. In the commercial sector, Anderson and Newell (2004), find that half of audits are followed up. Comparable effects have been observed in Germany (Schleich, 2004) and Sweden (Backlund and Thollander, 2015). In Italy, Barbetta et al., 2015 find no effect of audits on either the number of investments or the amounts invested in local public administrations.

The effect of audits can also be assessed indirectly by examining variation in energy use, under the assumption that it follows from unobserved investment. Using this technique, Hirst and Goeltz (1985) found that receiving a free audit induced significant but small energy savings. More recently, Alberini and Towe (2015) find that

[4] One source of error is the so-called "prebound effect" which arises when the baseline energy use against which savings are predicted is overestimated (Sunikka-Blank and Galvin, 2012).

participating in an audit program yields 5% energy savings on average, an effect commensurate with that estimated for rebates in the program.

Altogether, it is difficult to disentangle the quality of audits and their effect on investment. In addition, selection bias is an important concern in the small-scale studies reviewed here.

Results indicate that information relevant to energy-efficiency decisions is incomplete and that providing better information improves market outcomes. Yet the overall effect tends to be small and heterogeneous. The information gap is therefore probably modest.

7.3.2 Imperfect information

In addition to being incomplete, information about energy cost can be imperfect, in the sense that it bears some randomness. Energy prices are volatile in the short to medium term; energy needs, in turn, are determined by intrinsically random factors such as the weather. Combined with the irreversible nature of energy efficiency improvements, such randomness creates option values (Dixit and Pyndyck, 1994). These affect investment outcomes if decision-makers are risk-averse, which seems to be a valid assumption in the context of energy efficiency (Farsi, 2010). Using calibrated simulations, Hassett and Metcalf (1993) pointed out early that option values alone could entirely explain the high hurdle rates observed in energy-efficiency decisions. Sanstad et al. (1995) objected that this was only valid for a narrow range of decisions in which delay is not costly – unlike, say, window replacement, which is more expensive alone than if included in an earlier retrofit. Baker (2012) further restricts Hassett and Metcalf's result to binary decisions – for instance, whether or not to insulate – as opposed to discrete choices. In contrast, Ansar and Sparks (2009) follow Hassett and Metcalf's line and argue that incorporating technological change can produce high option values.

Whatever their size, option values, if unaccounted for, can be a source of overestimation of the energy efficiency gap. While insurance markets could provide a private solution to the problem, case studies are virtually inexistent.

7.4 Asymmetric information: A framework

Energy efficiency is subject to verifiability, liability and heterogeneity issues which together make the essence of credence goods and create information asymmetries – true market failures requiring public intervention (Dulleck and Kerschbamer, 2006). The problems are magnified by the high upfront costs and multiplicity of stakeholders involved in energy-efficiency investments. To illustrate, let us consider the measure which epitomizes these characteristics: home energy retrofit, e.g., insulation and improvements on weatherization systems. As summarized in Figure 1, the homeowner, who is central to the investment decision, may contract with four economic agents (some of whom might be herself): a tenant whose utility bill may or may not be included in the rent; a contractor selling and installing durable goods; a credit supplier; a subsequent buyer of the retrofitted home.[5]

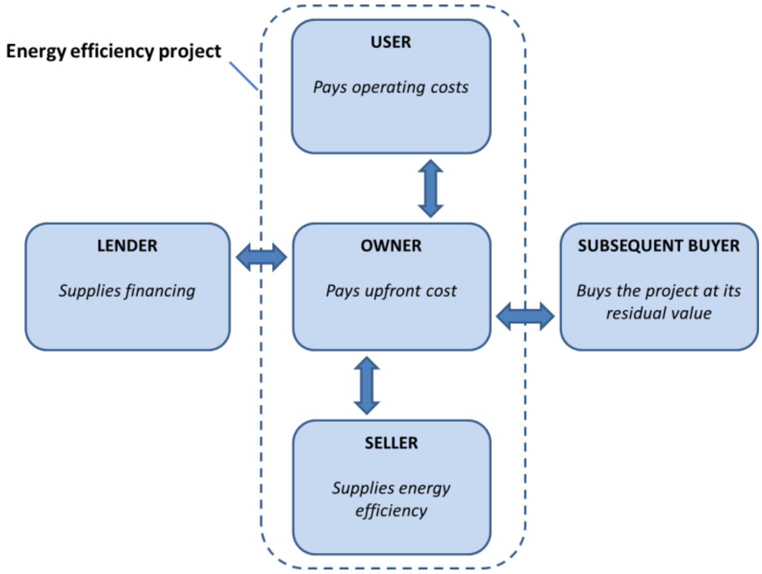

Figure 1: Main stakeholders and contractual relationships in home energy retrofits

Each of these relationships can be subject to a variety of information asymmetries, which I review below. I use the standard terminology of Mas-Colell et al. (1995), who classify information asymmetries in two broad categories – adverse selection and principal-agent problems – each encompassing subcategories – screening and signalling on the one hand, moral hazard and price discrimination on the other. The findings are summarized in Table 1.

7.5 Adverse selection

Adverse selection occurs when part of the relevant information is hidden to one party. Specifically, screening issues occur when the seller cannot observe buyers' types and signalling issues occur when the seller is unable to convey the quality of its products to prospective buyers. Either problem results in too little quality in the market – so-called lemons.

7.5.1 Signalling issues

7.5.1.1 Building sales

Perhaps the longest-studied information asymmetry associated with energy efficiency is signalling in building sales. The intuition is that hard-to-observe energy efficiency of a building unit is unlikely to be capitalized into sale prices. Research into the topic started in the early 1980s. At the time, energy efficiency was measured by past billing data or coarse labels describing the thermal integrity of the unit. Hedonic analyses found evidence of capitalization of energy efficiency into home sale prices (Johnson and Kaserman, 1983; Dinan and Miranowski, 1989; Gilmer, 1989; Nevin and Watson, 1998). This early literature was however criticized for failing to appropriately take into

[5] For lack of empirical evidence, I do not include other actors who too can engage in principal-agent relationships, such as energy suppliers, building certifiers and sales agents.

account the fragmented and local nature of housing markets and the difficulties associated with measuring costs and benefits of energy efficiency in housing (Laquatra et al., 2002).

The topic attracted renewed interest in the early 2010s with the advent of energy performance certificates such as that promoted by the European directive (hereafter EU-EPC) and the LEED and Energy Star labels in the United States. Larger datasets and more modern hedonic methods permitted more credible identification. Studying commercial buildings shortly after energy-efficiency labels became mandatory, Fuerst and MacAllister found that labelled buildings carried a price premium in the United States (Fuerst and MacAllister, 2011a) but not in the European Union (Fuerst and MacAllister, 2011b). Brounen and Kok (2011) identified a price premium associated with the EU-EPC in the Netherlands. Murphy (2014) nuances the finding by surveying purchasers, arguing that the EPC had little influence in sales negotiation. Kahn and Kok (2014) find a premium associated with LEED, Energy Star and other "green" labels in housing California. Hyland et al. (2013) and Stanley et al. (2016) find a similar premium in Ireland and Dublin, respectively. Harjunen and Liski (2014) find that more efficient heating technologies such as electric and district heating are capitalized in the Finnish housing market. Fuerst et al. (2015) find a significant effect of the EU-EPC in England. Myers (2016) finds evidence that changes in relative fuel prices cause changes in relative housing prices in Massachussets in a way that is consistent with full capitalization of energy savings. Lastly, Wahlström (2016) finds evidence of capitalization of the EU-EPC in Sweden. Like the responses discussed in Section 2.1.2, capitalization sometimes exceed the present value of energy savings. This is the case in US office buildings (Eichholtz et al., 2010, 2013) and homes from three US cities (Walls et al., 2017).

These studies together provide compelling evidence of full capitalization of energy-efficiency labels. They are less conclusive, however, as to whether labels fill an information gap. After all, the early studies reviewed above, despite their shortcomings, suggested that capitalization of energy efficiency was already effective prior to label implementation. Modern evaluations of labelling policies, in turn, do not compare situations with and without labels, which is the only way to determine whether labels operate by levelling the information shared by the buyer and the seller – thereby eliminating an information asymmetry – or simply by repeating information decision-makers can gather from observable features.

The most recent studies on the topic are beginning to fill this gap. Exploiting a dataset in which some dwellings were sold multiple times in Oslo, Norway, Olaussen et al. (2017) find that current EPCs explain sale prices in a way consistent with the studies discussed above, but also explain the prices of transactions that occurred before implementation of the EU-EPC policy. Furthermore, the authors find no evidence of a price premium after controlling for dwelling fixed effects. These results suggest that labels provide no additional information. Similar conclusions are reached by Fesselmeyer (2018) by exploiting price variation before and after certification in Singapore.[6] In ongoing work, Frondel et al. (2016) exploit a shift from voluntary to

[6] Similar results are obtained in the Korean market for televisions (Park, 2017).

mandatory disclosure of the EU-EPC in Germany and find that it causes a contraction of the energy efficiency premium for owners who would not voluntarily disclose. This can be interpreted as evidence that sellers of low-efficiency dwellings did not voluntarily engage in signalling.

7.5.1.2 Building rental

The question of capitalization similarly applies in rental markets: Do more energy-efficient buildings rent with a premium? Existing studies tend to offer positive answers, for instance Fuerst and McAllister (2011a) and Eichholtz et al. (2010, 2013) in US office buildings, Kok and Jennen (2012) in commercial buildings in the Netherlands, Heyland et al. (2013) in the Irish residential sector. Reichardt (2014) finds rent premia that exceed the value of savings on operating expenses in the United States. Like in building sales, these studies are limited in their ability to disentangle the purely informative effect of labels from other potential effects. Indeed, Bala et al. (2014) find that rents in Brussels in 2001 increased with observable features such double glazing and wall insulation, which suggests that energy efficiency was already capitalized without labels. In ongoing work, Dressler and Cornago (2017) address this methodological gap by exploiting a shift from voluntary to mandatory certification in Brussels similar to that exploited by Frondel et al. (2016) in housing sales. Their results provide suggestive evidence of strategic non-compliance with mandatory disclosure in those units, the EPC of which is below average.

Failure to signal energy efficiency in rental buildings can also be identified by comparing the efficiency of rented and owner-occupied units, all other things equal. Research along this line suggests that rented dwellings are less efficient than owner-occupied ones. Brechling and Smith (1994) find lower ownership of energy-efficient assets in rented properties than in owner-occupied ones in the United Kingdom. Scott (1997) finds similar results in Ireland. Davis (2012), using the U.S. Residential Energy Consumption Survey (RECS), documents that renters are significantly less likely to report having energy-efficient appliances such as refrigerators, clothes washers and dishwashers. Gillingham et al. (2012), using the same database, report that owner-occupied dwellings in California are 20% more likely to be insulated in the attic or ceiling than rented ones. Melvin (2018) extends the result to water heating, window thickness and weatherization. Myers (2015) finds that energy price movements cause shifts in rents of energy-efficient units when rents include utilities, but not otherwise, suggesting the market does not convey information about energy use. In Europe, Krishnamurthy and Kriström (2015) report that owners are more likely to have energy-efficient appliances, better insulation and heat thermostats than tenants.

The evidence here is clear: Signalling issues affect the rental of energy-efficient buildings. This may be due to rigid regulations that prevent landlords from passing investment costs onto rents. While labels seem to improve decisions, evidence is scarcer as to whether they encourage landlords to initiate energy-efficiency improvements.

7.5.2 Screening issues

7.5.2.1 Utility-included rent contracts

In many countries, rental contracts frequently include energy operating costs. In the United States, for instance, approximately 60 percent of housing rental contract include at least one energy or water utility (Choi and Kim, 2012). How does a landlord offering utility-included contracts adjust rents to the tenant's specific energy usage?

The question has been relatively little-studied. Levinson and Niemann (2004), using RECS and the American Housing Survey (AHS), find that rents in utility-included rental apartments are higher than for comparable metered apartments, but the difference is smaller than the difference in energy operating costs observed in the two types of apartments. This can be interpreted as a failure of the landlord to screen tenants with high-intensity energy usage. Myers (2015), similarly using the ASH and exploiting variation in energy prices finds that low-efficiency dwellings turnover faster than high-efficiency ones when tenants pay for energy, but not when utilities are included in the rent. Again, this suggests that tenants are less likely to self-select into the dwelling that best fits their preferences when they do not pay the marginal cost of energy.

These results together suggest that utility-included contracts lead to inefficient outcomes, favoring tenants with intensive energy usage and pricing others out of the market. One way to address this market failure could be to ban such contracts.

7.5.2.2 Energy-efficiency loans

In theory, adopting energy efficiency saves consumers money, thereby increasing their creditworthiness and reducing default risk. In a well-functioning credit market, the interest rate offered to consumers should therefore be lower for energy-efficiency investments than for otherwise similar investments. Investigating this hypothesis in commercial mortgages, An and Pivo (2018) find better loan terms for buildings that were certified green at loan origination than for other buildings which either are non-green or were certified green after loan origination. Though modest in magnitude, the effect is consistent with lenders efficiently using green labels as a screening device. Information asymmetries, if they affect energy-efficient projects, do thus not carry over to the financing process.

7.6 Principal-agent problems

Principal-agent problems are situations in which a principal hires an agent to perform a task. Moral hazard arises if the principal cannot observe the agent's ex post actions. Price discrimination arises if a multiproduct monopolist cannot observe the agents' types ex ante. Both categories produce undesirable behaviours and they are likely to affect the markets for energy efficiency.

7.6.1 Moral hazard

7.6.1.1 Utility-included rental contracts

Just like an insuree is expected to take little care of a product covered by an insurance contract, an energy user who does not face the marginal cost of energy is expected to over-use energy. Such moral hazard is substantiated in utility-included rental contracts. Levinson and Niemann (2004), using RECS data, find that US households use slightly more energy under such contracts. Maruejols and Young (2011) find similar effects in Canada. Gilliginham et al. (2012) similarly find in California that under such contracts, occupants are 16% more likely to change heating thermostat at night. Kahn et al. (2014) find evidence of a better environmental performance in those commercial buildings, the tenants of which face a positive marginal cost for electricity. Myers (2015) finds that landlords are more likely to make cost saving investments when they face the marginal cost of energy usage. The most credible evidence to date is provided by Elinder et al. (2017) who compare energy use before and after an intervention consisting in excluding utilities from rental contracts in Sweden. Compared to 1,000 tenants in the control group, the 800 treated tenants showed an immediate and permanent reduction in energy use by 25%.

Evidence here is compelling. All authors however underline that the effect is small in terms of excess energy use – which does not mean that welfare effects are unimportant. Here again, banning utility-included contracts could avoid over-use of energy, a problem even more critical in the presence of uninternalized energy-use externalities.[7]

7.6.1.2 Building retrofits

The quality of such retrofit works as attic insulation or duct sealing is hard to verify by non-experts, unless costly ex post audits involving thermo-photography or blower-door tests are commissioned. The informational context is conducive to moral hazard in the form of under-provision of installation quality by contractors.[8] Using data from a utility-sponsored retrofit program in Florida, Giraudet et al. (2018) find that energy-efficiency measures are subject to day-of-the-week effects if they are deemed hard-to-observe, but not otherwise. The day-of-the-week effect follows a specific pattern – energy savings are lower when the retrofit was completed on a Friday, as compared to other days of the week. The authors find that the problem can explain 65% of the discrepancy observed between predicted and realized savings.

Moral hazard can be addressed by professional certifications – a public solution – or energy-savings insurance – a private one. While the former incurs monitoring costs, the latter induces a moral hazard similar to that associated with utility-included contracts. Through calibrated simulations, Giraudet et al. (2016) suggest that certifications provide slightly more benefits than insurance. Note that reputation provides another private solution to moral hazard; yet to my knowledge, the issue has not been examined.

7.6.1.3 Energy-efficiency loans

As stated earlier, energy efficiency is supposed to reduce default risk, an important form of moral hazard in credit. Using US data from the Home Energy Rating System (HERS), Kaza et al. (2014) found that more energy efficiency, as measured by ENERGY

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[7] This prescription abstracts from benefits potentially associated with utility-included contracts, which can enable landlords to attract certain types of consumers and avoid them to install costly individual meters (Choi and Kim, 2012).

[8] In addition to moral hazard, it might be difficult ex ante to hire a diligent contractor. This screening problem is unexplored.

STAR ratings, is associated with lower default and prepayment rates in residential mortgages. Applying a similar research design to commercial mortgages, An and Pivo (2018) confirm that greener buildings are associated with lower default rates. The effect is more important than that identified by the authors in relation to loan terms (cf. infra). Altogether, these results can be interpreted as efficient loan pricing, implying that information asymmetries in energy-efficiency loans are not economically important.

In home energy retrofits, an additional problem arises. Unlike other assets of comparable purchase price, say a car, an energy retrofit cannot be confiscated. Therefore, unless the retrofit is included in a mortgage, it cannot serve as credit collateral. This might lead lenders to raise interest rates in an effort to hedge against increased default risk (Palmer et al., 2012). The effect has not yet been empirically investigated.

7.6.2 Price discrimination

Price discrimination, also known as monopolistic screening, arises in the presence of two market failures: imperfect competition and adverse selection. A multiproduct seller having market power but no ability to screen consumer's types has an incentive to deteriorate the quality of low-end products so as to maintain high mark-ups on the sales of high-end products (Mussa and Rosen, 1978). If energy efficiency is the relevant dimension of quality, those distortions result in too little energy efficiency at the bottom of the product line (Fischer, 2005; Nauleau et al., 2015).[9] Houde (2014) exploits changes in the ENERGY STAR label in the US market for refrigerators and finds adjustments in the product line that are consistent with price discrimination. Spurlock (2013), exploiting simultaneous changes in minimum energy efficiency standards and ENERGY STAR, reaches the same conclusion for clothes washers. So do Cohen et al. (2017) using variation in energy prices in the UK market for refrigerators.

7.7 Discussion

7.7.1 Information problems and behavioural anomalies

Besides debate over the market-failure nature of barriers to energy efficiency, an important research effort has been dedicated to behavioural anomalies in energy-efficiency decisions in the past decade. Environmental topics, and energy efficiency in particular, offer interesting opportunities to test the predictions of the emerging field of behavioural economics (Shogren and Taylor, 2008; Gillingham and Palmer, 2014; Allcott, 2016). Consumers indeed seem to value energy savings in a way that is inconsistent with perfect rationality (Attari et al., 2010). Much research along this line

[9] Improving energy efficiency normally means minimizing energy use for a given level of energy service. Yet the term is frequently used in the broader sense of simply minimizing energy use, without necessarily holding energy service constant. This is typically the case in transportation, where a small car is regarded as more energy-efficient than a larger car. While a small car indeed allows one to cover more distance with the same amount of fuel, it also offers fewer services (e.g., limited capacity and comfort). If price discrimination operates along these other dimensions of energy services, it can lead to too small cars, which, if energy efficiency is used in the broader sense, can be interpreted as too much of it (Plourde and Bardis, 1999).

has focused on feedback experiments with peer comparison, in which consumers are provided with information about how their energy use compares to that of their neighbours (e.g., Allcott, 2011; Allcott and Rogers, 2014). Overall, such interventions are found to strengthen conservation behaviours, however with low persistence (Ayres et al., 2012; Delmas et al., 2013). This finding suggests that social norms influence individual's behaviour, a feature not captured by the standard microeconomic model.

As transpired throughout the review, however, behavioural anomalies are difficult to separate out from information problems. Most empirical settings simultaneously involve informational barriers – incomplete, imperfect or asymmetric information – and behavioural treatments. This is especially the case with energy-efficiency labels, which can serve either as a pure information provision addressing incomplete information, as a device levelling information between contracting parties, or as a social norm provoking departures from individual rationality. In randomized experiments in the lightbulb market, Allcott and Taubinsky (2015) provide information treatments and observe how they affect consumers' willingness-to-pay for compact fluorescent lightbulbs. The authors interpret the treatment as a "pure nudge" and assume that consumers' responses reveal the average marginal inattention bias. This study is an important first step that highlights the importance of heterogeneity in consumer responses. More research is however needed to disentangle purely informative and purely behavioural effects.[10]

7.7.2 What to expect from information technologies?

Given the central role information technologies have come to play in the economy, it seems natural to ponder on how they can support energy-efficiency improvements, which are subject to so many information problems. The works reviewed here suggests that smart metering and in-home displays of energy use can significantly improve market outcomes. So can emerging technologies such as thermo-photography and other tests which enable verification of building performance. Nevertheless, the algorithms used to predict energy savings still seem to lack accuracy. Another area for improvement is the development of platforms facilitating search for retrofit contractors.

The question examined here echoes a broader reflection about whether recent breakthroughs in information technology mean the end of information asymmetries (Cowen and Tabarrok, 2015). Preliminary research warrants healthy scepticism. For instance, internet markets do not seem to reduce price dispersion, with some platforms even engaging in obfuscation to compensate for increased competition (Ellison and Ellison, 2005; Levin, 2013). In addition, internet ratings, which are supposed to improve information, can be subject to manipulation (Luca and Zervas, 2016). Lastly, information technologies raise privacy concerns which go far beyond

[10] Preliminary work by Astier (2016) is worth mentioning here. The author proposes an interesting design to separate out information provision and social norms. Online participants are first randomly assigned to complete and incomplete information environments then randomly assigned to different treatments: comparative feedback, information only, and warning to outliers. While feedbacks produce additional energy savings, complete information is found to be a necessary condition for their effectiveness.

economic inquiry. Those issues are particularly sensitive in the context of energy use, which infuses nearly every aspect of everyday life.

7.8 Conclusion

Energy efficiency can be seen as a credence good, the performance of which is never fully revealed to the buyer. This characteristic is exacerbated by the high upfront costs and multiplicity of stakeholders involved in building investments. As a result, building energy efficiency is subject to an array of information asymmetries, arguably more so than other well-studied credence goods such as medical treatments, taxi rides or auto repair.

In this essay, I reviewed evidence of informational barriers to energy-efficiency investment, with particular attention to whether they qualify as market failures – in the context studied here, information asymmetries – or not – symmetric-information problems. I found that some information barriers are well documented, while others are either inaccurately characterized, not clearly established, or simply overlooked.

I first noted that information relevant to operating energy-consuming assets is incomplete and imperfect in many contexts, with unclear conclusions as to whether information provision improves market outcomes. I then moved to information asymmetries – that is, true market failures – and found them to be more important than previously thought. The longest-studied ones are associated with landlord-tenant relationships. While indistinctly referred to as "split incentives" in the literature, I classified them in three categories: signalling in rental buildings, moral hazard and screening in utility-included rent contracts. All of these are economically important, although they do not seem to induce dramatic over-use of energy. One implication is that banning utility-included contracts could improve social welfare. Another much studied information asymmetry is signalling in building sales, the analysis of which has been facilitated by implementation of energy performance certificates. Here, the conclusion is ambiguous. Prospective buyers seem to respond to information labels, but two counterfactuals are often missing to ascertain that labels operate by elimination of an information asymmetry: what occurs without labels (to identify information levelling), and what occurs with coarser labels (to identify a social-norm effect). Lastly, information asymmetries have been understudied in the context of labour-intensive supply (moral hazard and signalling) and financing (screening, moral hazard and price-quality discrimination) of energy efficiency.

To conclude, it is worth noting that retrofit commissioning occurs very upstream in the production of energy efficiency. Any information asymmetry associated with it might propagate in related transactions, such as building rental and sales. Downstream, on the other hand, financing is somehow the recipient of all other information asymmetries. In the United States alone, the market for energy-efficiency finance is estimated to amount to $100 billion annually (Freehling and Stickles, 2016). More research is therefore needed into these two crucial topics – retrofit commissioning and financing.

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Table 1: Information asymmetries in home energy retrofits

		BUYER/SELLER	OWNER/USER	BORROWER/LENDER
ADVERSE SELECTION	SCREENING		**Suggestive evidence** of incomplete pass-through of energy expenditures onto rents (*3 references*)	**Early evidence** of better loan terms for energy-efficient assets, suggesting no information asymmetry (*1 reference*).
	SIGNALLING	**Compelling evidence of capitalization** of energy savings into home prices, with little evidence of asymmetric information. *(22 references)*	**Compelling evidence of asymmetric information**, owing to several sources of variation: owner-occupied versus rented dwellings; utilities-included versus utilities-excluded rents. *(16 references)*	
PRINCIPAL-AGENT PROBLEMS	MORAL HAZARD	**Scarce evidence in home energy retrofits** (principal is the homeonwer, agent is the contractor). *(1 reference)*	**Compelling evidence of a moderate effect** (on energy savings) in utilities-included rents (principal is the landlord, agent is the tenant) *(6 references)*	**Early evidence** of lower default risk for energy-efficient assets, suggesting no information asymmetry (*3 references*).
	PRICE DISCRIMINATION	**Compelling evidence** of a "distortion at the bottom" in appliance markets (principal is the firm, agent is the consumer) *(3 references)*		Plausible. Not investigated.

Table 2: References associated to Table1

		BUYER/SELLER	OWNER/USER	BORROWER/LENDER
ADVERSE SELECTION	SCREENING		*25, 82, 94*	*9*
	SIGNALLING	*22,34, 38, 39, 43, 48, 50, 51, 52, 57, 60, 70, 73, 74, 80, 91, 93, 96, 98, 115, 121, 122*	*19, 21, 29, 35, 38, 39, 50, 51, 54, 70, 78, 79, 88, 94, 105, 109*	
PRINCIPAL-AGENT PROBLEMS	MORAL HAZARD	*58*	*40, 54, 75, 82, 84, 94*	*9, 77, 99*
	PRICE DISCRIMINATION	*26, 64, 114*		

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8. Work Package 2 How do lenders price energy efficiency? Evidence from posted interest rates for unsecured credit in France (lead partner: CIRED)

Authors: Louis-Gaëtan Giraudet (Ecole des Ponts ParisTech, CIRED), Anna Petronevich (Banque de France), Laurent Faucheux (CNRS, CIRED)

8.1 Abstract

Basic principles of loan pricing predict that the interest rate charged for energy efficiency investment is lower than for conventional investment. We test this hypothesis using a unique dataset of posted interest rates retrieved on a weekly basis from the websites of 15 lending institutions covering the near totality of the French market for unsecured credit. Crucially, our data are immune from sorting bias based on borrower characteristics. We find that the interest rate spread between conventional and energy efficiency investment was negative in 2015 and turned positive in 2016. A similar switch occurred to the spread between home renovation investment and vehicle investment. These results together imply that loans for home energy renovation were consistently charged relatively high interest rates. This can be interpreted as a new barrier to energy efficiency, with adverse consequences for scaling up home energy renovation. One possible explanation is that lenders use project characteristics as a screening device of unobservable borrower characteristics.

8.2 Introduction

Energy efficiency is recognized as the most cost-effective means of reducing carbon dioxide emissions at the source of anthropogenic global warming (IPCC, 2014). This is especially the case in the building and transport sectors, which together contribute 30% of global emissions, two thirds of which come from households. As an attribute of long-lived assets, energy efficiency necessarily raises financing issues. In France alone, 20 to 40% of home energy retrofits involve credit, mostly through unsecured loans (OPEN, 2016). Assuming a conservative upfront cost of 10,000€, meeting the 500,000 annual retrofit target set by the French Government thus creates annual borrowing needs of one to two billion euros. Scaling up energy efficiency therefore requires that sizable borrowing needs be satisfied in an economically efficient manner. Despite its importance, however, the issue has only received little attention so far.

According to basic principles of finance, interest rates should reflect the risks perceived by lenders. As a first approximation, the risk associated with energy efficiency investments can be considered low: by reducing energy expenditures, energy efficiency both increases the solvency of the investor and the resale value of the underlying asset – the latter phenomenon in particular being increasingly documented (Brounen and Kok, 2011; Giraudet, 2018). A well-functioning credit market should therefore offer lower interest rates for energy-efficient projects (hereafter "green projects") than for projects devoid of that attribute but otherwise similar (hereafter "conventional projects"). This simple prediction has recently been proved valid in the US market for commercial mortgages by An and Pivo (2018). Using ex post data from a loan programme, the authors find that those buildings that were certified green at loan origination obtained slightly but statistically significantly better loan terms than did their conventional counterparts.[11] To our knowledge, this is the only study that has investigated the matter. Its internal validity is however threatened by selection issues, as the authors could not control for borrowers' characteristics.

In this paper, we assess the validity of what we refer to as the "green discount" hypothesis in the French market for unsecured credit. We do so using a unique panel dataset of loan terms posted on credit institutions' websites. The data were retrieved every week, for two years, from loan simulators made available online by 15 institutions covering the near totality of the French market. Our approach differs from that of An and Pivo (2018) in several respects. First, beyond geographical focus, we consider a different market. While An and Pivo (2018) studied mortgage loans for new commercial buildings, we study unsecured loans for a variety of household investments; when it comes to buildings, we are concerned with the renovation of existing ones rather than new constructions.[12] This broader set of investments allows us to examine how the green attribute interacts with the designation of the project – in particular whether it is a vehicle or a renovation. Second, and perhaps most importantly, our data are immune from sorting bias, as the online simulators from which they originate do not query any information about borrower characteristics. We therefore avoid the selection issues faced by An and Pivo (2018). Third, these facilitating features come at the cost of handling ex ante, rather than ex post, data. This implies in particular that we cannot study default rates. Still, the fact that our posted data overestimate actual data by a mere 0.3 percentage point on average and that the two follow parallel trends lends external validity to our analysis.

We investigate two hypotheses – whether green projects are offered lower interest rates than their conventional counterparts on the one hand, whether renovation and vehicle projects are priced the same, regardless of any green attribute, on the other. We do so in a parsimonious econometric model that includes time and institution fixed effects and controls for loan characteristics. When considering the period as a whole, we fail to reject the first hypothesis and find higher interest rates for renovations than for retrofits, which leads us to reject the second hypothesis. Overall effects are small (except for green vehicles) but statistically significant and confirmed by statistical tests and robustness checks involving placebo tests. Looking at each year separately, we find that both results hold for 2016 but were reversed in 2015. In other words, the market seems to increasingly value the lower risk associated with green projects and offer increasingly higher interest rates for renovation projects than for vehicles. This has important consequences for green renovation projects, which, owing to the

[11] The authors additionally find that greener buildings entail lower default rates. They thus corroborate an earlier finding of Kaza et al. (2014) in the US market for residential mortgages. This robust result confirms one assumption of the "green discount" prediction, namely that green projects are less risky than conventional projects. According to An and Pivo (2018), however, the green attribute has a much smaller effect on loan terms than on default rates.
[12] Given the slow turnover of building stocks (typically 1% every year), the renovation of existing buildings is much more crucial for carbon dioxide emission reductions than are new constructions. This is especially true in the residential building stock, which is typically 50% larger than the commercial building stock.

interaction between these two trends, constantly carry relatively high interest rates. This is especially true for short-term loans (12 months).

Our contribution is two-fold. First, in documenting relatively high interest rates for home energy retrofits, we contribute to the literature on the factors causing slow adoption of energy-efficient technologies – a phenomenon known as the energy-efficiency gap (Jaffe and Stavins, 1994) which has recently gained renewed interest (Gillingham et al., 2009; Allcott and Greenstone, 2012; Gerarden et al., 2017). Specifically, we add to the scarce literature on energy efficiency loans (Palmer et al., 2012; Kaza et al., 2014; An and Pivo, 2018) by emphasizing the trade-offs between the green attribute and other dimensions of the underlying asset. Second, we document an anomaly, namely systematic differences in the interest rates offered for renovation- and vehicle-backed loans, whereas the risks associated with each project should not particularly differ. Considering that our data are immune from sorting bias, this suggests that loan designations might be used as a screening device of unobserved borrower characteristics. This finding, if confirmed in further research, could contribute to the literature on access to credit, which has already identified discrimination based on gender (Peterson, 1981) and ethnicity (Duca and Rosenthal, 1993) as important barriers.

The analysis proceeds as follows. Section 2 formulates testable hypotheses. Section 3 describes the data. Section 4 details the empirical approach. Section 5 discusses the results. Section 6 provides robustness checks. Section 7 concludes.

8.3 Testable hypotheses

Here we discuss in greater length the hypotheses that our dataset allows us to test. As stated in the introduction, basic principles of finance imply the following:

> **Hypothesis 1:** *Green projects carry lower interest rates than do projects devoid of that attribute but otherwise similar.*

Rejection of this hypothesis can be interpreted as evidence of an energy efficiency gap. An increasing number of studies point to energy retrofit projects that fail to deliver predicted energy savings (Metcalf and Hassett, 1999; Graff Zivin and Novan, 2016; Fowlie et al., 2018). While these studies attribute the missing savings to modeling flaws in engineering calculations, Giraudet et al. (2018) propose an alternative explanation rooted in information asymmetries. Evaluating a home weatherization program conducted in Florida, the authors provide evidence that retrofit contractors engage in moral hazard by under-providing quality in partly unobservable measures such as insulation installation or duct sealing. Thus confronted with a so-called lemons problem (Akerlof, 1970), the lender might internalize it and price energy-efficient assets the same as conventional, non-energy-efficient assets.

Now regardless of any energy efficiency consideration, a renovation and a vehicle are two household investments which, as a first approximation, carry comparable risk. In a well-functioning credit market, the following hypothesis should therefore hold:

> **Hypothesis 2:** *The interest rates for renovation and vehicle projects are identical.*

This hypothesis may however be rejected if the lender uses the loan designation as a screening device of unobserved borrower characteristics.[13] In this perspective, the most plausible conjecture formed by the lender is that households borrowing money to retrofit their home are wealthier than those borrowing money to purchase a vehicle. Indeed, vehicle purchases are largely disconnected from borrowers' home ownership status, while home energy retrofits are overwhelmingly conducted by homeowners, who tend to be wealthier. Such a conjecture can induce two countervailing effects. On the one hand, a wealthier borrower can be perceived as having a higher willingness to pay, which a price-discriminating lender may want to exploit by charging higher interest rates. This effect, which we refer to as the WTP channel, is common to the supply of any good. On the other hand, a wealthier borrower might be perceived as less likely to default, hence be charged a lower interest rate. Interestingly, this effect, which we refer to as the risk channel, is specific to loans. This leads us to consider an amended version of Hypothesis 2:

> **Hypothesis 2':** *Renovation projects carry lower interest rates than do vehicle projects.*

Rejection of Hypothesis 2' can be interpreted as dominance of the risk channel, while failure to reject it can be interpreted as dominance of the WTP channel.

8.4 Data

8.4.1 Collection

Our dataset consists of a panel of interest rates retrieved from online credit simulators. Most credit institutions in France make such simulators available to prospective borrowers. A simulator typically makes queries about the amount, duration and designation of the desired loan, from which it returns loan terms, characterized by the nominal interest rate, possibly some fees, and the annual percentage yield (*taux annuel effectif global*), which expresses the yearly cost of the loan. Importantly, simulators do not make queries about the applicant's characteristics. The resulting loan-term data are therefore plausibly immune from sorting bias based on applicants' characteristics observed to the lender.

We designed a web-scraping robot that ran such simulators on a weekly basis and assembled a panel dataset of simulated loan terms. We surveyed all credit institutions which, to our knowledge, offered online simulators for household unsecured credit in France during the observation period. This includes 15 institutions which are either the main retailer or some credit subsidiaries of the six main French banking groups, altogether covering 88% of issued household loans (Table 1). We maintained the robot for two years, from January 2015 to October 2016, which produced 93 weeks of data. Each week, for a given institution offering a given designation, the robot ran the simulator 108 times, combining 12 different amounts – ranging from 5,000€ to 32,500€,

[13] In practice, loans terms are negotiated between the lender and the borrower during the underwriting process, at which time the lender does observe many of the applicant's characteristics. Screening probably becomes irrelevant at that stage. It is more likely to occur earlier on when loan terms are posted, then generating differences in interest rates that subsequent negotiation might not completely clear. This early process is the one studied here.

with a step of 2,500€ – and 9 different maturities – ranging from 12 to 108 months, with a step of 12. The data thus produced are 4-tuples of institution, designation, amount and maturity.

Table 1: Characteristics of the institutions surveyed

Banking Group	Market share	ESCG member	Institution	Type of institution
BNP Paribas	11%	YES	**BNP Paribas**	Private bank
			Cetelem	Financial credit establishments
			Cofinoga	Financial credit establishments
			Domofinance	Financial credit establishments
BPCE	8%	NO	**Caisse d'épargne**	Cooperative bank
Crédit Agricole	10%	YES	**Crédit agricole**	Cooperative bank
			LCL	Private bank
			Sofinco	Financial credit establishments
Crédit Mutuel	48%	NO	**Cofidis**	Financial credit establishments
			Crédit Mutuel	Cooperative bank
			Financo	Financial credit establishments
			Prêt d'union	Financial credit establishments
La Banque Postale	6%	NO	**La Banque Postale**	Public bank
Socété Générale	4%	YES	**Franfinance**	Financial credit establishments
			Socété générale	Private bank

Note: Market shares computed by the authors using the Banque de France CEFIT data. The reported shares cover 88% of the market.

Several sampling issues made our panel dataset unbalanced. First, the menus of designations are specific to each institution, and the number of options each offers varies from 1 to 21 (median 4; mean 7.5). Overall, we recorded 90 different designations, which we grouped into categories, as we will see in the next section. Second, the available ranges of amount and maturity vary as well across institutions. Yet even though sampling was heterogeneous across institutions, this did not introduce a strong bias, as amounts and maturities are very close once averaged per loan category (Figure 1). The average loan size and maturity over the whole dataset are 16,782€ and 47 months, respectively.[14] Third, some data could not be retrieved for certain institutions on certain weeks. This is due to changes in websites that could not be detected early enough to adjust the design of the robot – a challenge common in web scraping (Cavallo and Rigobon, 2016). Overall, our panel dataset comprises 240,962 observations.

[14] To put these numbers in perspective, the average national averages are 11,449€ and 47 months, respectively.

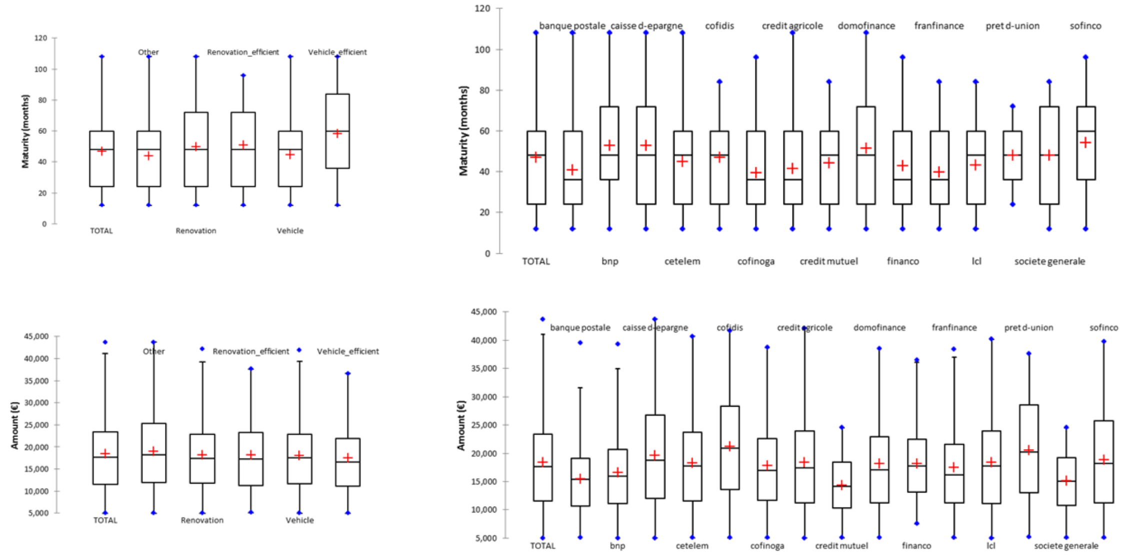

Figure 2: Summary statistics of simulated amounts and maturities

8.4.2 Loan categorization

The number and labelling of options offered by institutions in their menu of loan designations vary widely. After grouping redundant labels, we still handle 90 distinct designations, which are all variants of vehicle loans, home renovation loans, equipment loans, consumption loans, student loans, health loans and cash loans. These designations are representative of unsecured loans issued in France, 47% of which were dedicated to auto purchase in 2017, 19% to equipment purchase, 10% to home retrofits, 8% to consumption, 8% to liquidity, 4% to credit restructuring and 4% to tax payments (Mouillard, 2018).

To test the hypotheses stated in Section 2, we group the collected designations into broad categories. Combining the two hypotheses stated in Section 2, we are specifically interested in four categories: renovations; green renovations; conventional projects; and green projects. Given the large market share of vehicle projects, we sort this category out of conventional investments. Another motivation for doing so is that one institution makes a distinction between green and conventional vehicles. Our most granular categorization therefore has five items: renovations; green renovations; vehicles; green vehicles; and others. To test the two hypotheses separately, we also consider two more aggregate categorizations: one that groups all green categories on the one hand, all conventional categories on the other; another that groups all renovation categories on the one hand, all vehicle categories on the other. The three workable categorizations are detailed in Table 2. Overall, eleven institutions offer both vehicle and renovation loans; four institutions – Cetelem, Domofinance, Financo and Prêt d'Union – offer both green and conventional retrofits; and one – BNP Paribas – offers both green and conventional vehicles.

Table 2: Categorization of loan designations

Collected entires (90)	Categorization 1	Categorization 2	Categorization 3
Car, motorcycle	Conventional	Vehicle	Vehicle
Used car, used vehicle, used boat, used camping car, used trailer, used motorcycle	Conventional	Vehicle	Vehicle
Brand new vehicle, Brand new car, Brand new or less than 2-year-old car, brand new or less than 2-year-old camping car, brand new or less than 2-year-old trailer, brand new or less than 2-year-old motorcycle	Conventional	Vehicle	Vehicle
Brand new efficient car	Green	Vehicle	Vehicle green
Other works, decoration, construction, veranda, indoor/outdoor design	Conventional	Renovation	Renovation
Boiler, wood boiler, electrical heating, water heating, windows, insulation, heat pumps, heating, home improvement	Green	Renovation	Renovation green
Other project, consumption, relocation, wedding, birth, DIY supplies, holidays, event, leisure	Conventional	Other	Other
Health, Family problems	Conventional	Other	Other
Need for money, Need for cash, budget	Conventional	Other	Other
Student loan	Conventional	Other	Other
Electronic device, appliances, Hi-fi, furniture, computer accessories	Conventional	Other	Other

The categorization procedure is crucial. Most collected designation labels are unambiguous and their allocation to the appropriate category straightforward. This is not quite the case for green and conventional retrofits, which are nevertheless central to our analysis. Making a distinction between the two requires careful interpretation of the labels. Our chosen approach is to allocate to the green retrofit category those retrofit labels that plausibly affect the energy consumption of the household. This essentially includes measures on the envelope and space and water heating systems. As a robustness check, we subject this categorization to placebo tests and conclude that it is meaningful (see Section 6.2).

8.4.3 Descriptive statistics

We focus below on the average percentage yield (APY), which summarizes all characteristics of the loan, including the fees.

An obvious concern with our posted data is the extent to which they approximate actual data. Comparing the trend of the average interest rate in our dataset, weighted by the market share of the corresponding banking group, to that of issued loans, as provided by the Banque de France,[15] we find a positive spread on 73 weeks out of 93 (Figure 2). The mean percentage error over the whole period is 6.0% (mean absolute percentage error: 6.9%; standard error 4.7%), or a 0.3 percentage point. Such a relatively low error lends external validity to our data. Moreover, the fact that the rates on issued loans are almost systematically below posted rates can be interpreted as indirect evidence of the negotiation process lenders and borrowers may engage in.

[15] http://webstat.banque-france.fr/fr/browseChart.do?node=5385583&sortByView454=468&SERIES_KEY=MIR1.M.FR.B.A2B.A.R.A.2254U6.EUR.N&SERIES_KEY=MIR1.M.FR.B.A2B.A.R.A.2250U6.EUR.N

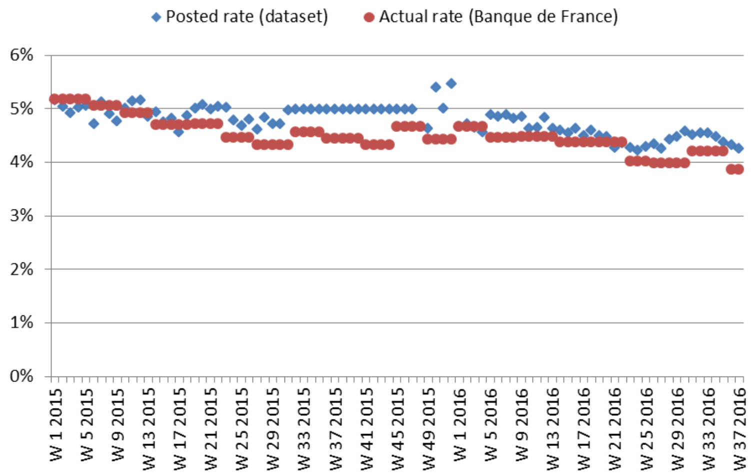

Figure 3: Comparison between posted and actual interest rates

The interest rates posted by credit institutions exhibit some dispersion across space and time. On average, the surveyed institutions update their interest rates every seven weeks and exhibit a coefficient of variation on interest rate of 33% (Figure 3, red square). As we will see later in regressions, dispersion is further substantiated by strong variations in average interest rates across banks. This indicates that despite operating in a highly competitive market (Europe Economics, 2009), institutions adopt heterogeneous pricing strategies, probably driven by differences in their borrower portfolio.

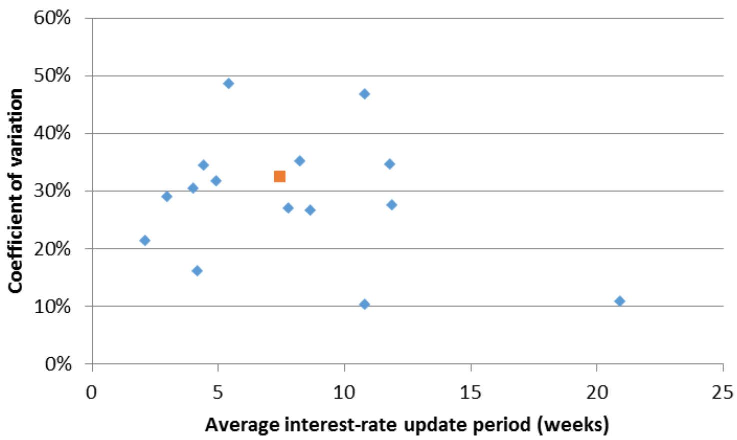

Figure 4: Dispersion of average interest rates across space and time, by institution

A glimpse into the time series of weighted averages of interest rate suggests that some clear, yet unstable, differences exist between categories (Figure 4). The two green categories tend to be associated with lower interest rates. In particular, the average interest rate on green vehicles – which we recall are offered by BNP Paribas only – drops significantly in early 2016. Another glimpse suggests that the interest rates averaged by maturity co-move to a large extent (Figure 5). Yet 12-month loans exhibit a peculiar pattern. In particular, their interest rate decreases more markedly than that of other maturities from early 2016 onwards. Figure 6 sheds light on the interaction between these phenomena by displaying the so-called yield curve (illustrating how interest rates vary with maturities) of the market, split by categories, at one point in

2015 and a year after. One can see that the yield curves of the two green categories have flattened and shifted downwards between 2015 and 2016.

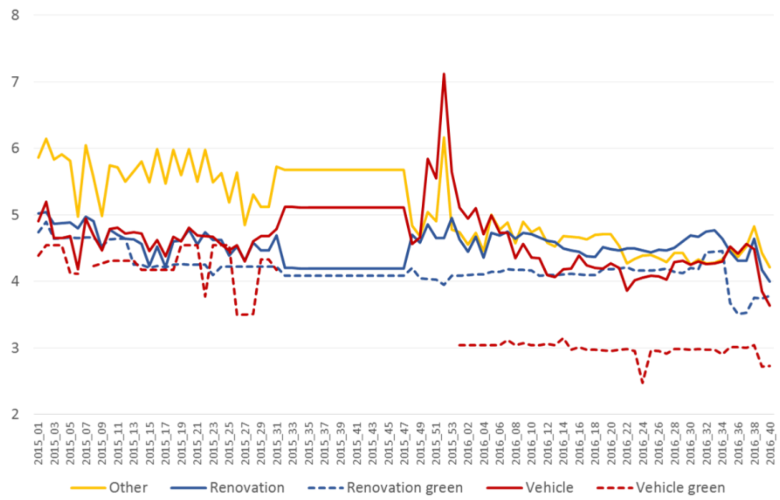

Figure 5: Time series of average spread (in percentage points), by category

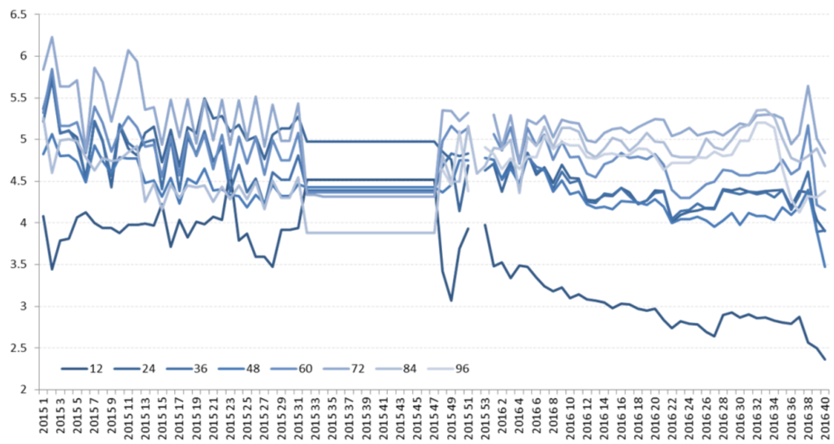

Figure 6: Time series of average spread (in percentage point), by maturity

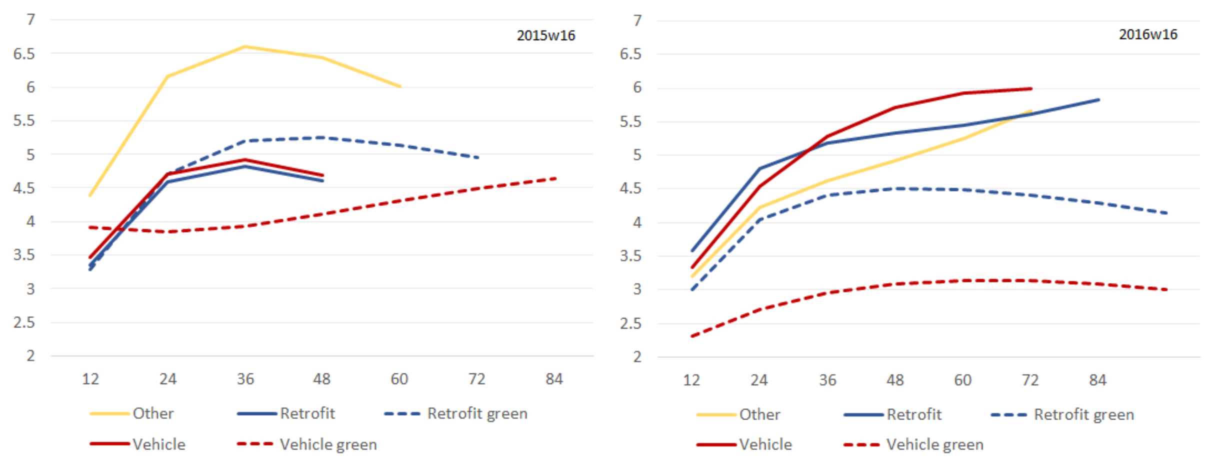

Figure 7: Empirical yield curves at two points in time, by category

8.5 Econometric model

Our goal is to make inference on how credit institutions perceive the risks associated with different loan designations. We consider the spread s between the posted interest rate i (measured as the APY) in our dataset and the spot yield of the government bond b of the same maturity:[16]

$$s_{kamtc} = i_{kamtc} - b_{mt},$$

where $k \in \{1, \dots, 15\}$ denotes the credit institution, $a \in \{5000, 7500, \dots, 32500\}$ the amount simulated in euros, $m \in \{12, 24, \dots, 108\}$ the maturity of the loan in months, c one category within one of the three retained categorization and t the week on which the loan was simulated. Focusing on the spread rather than the interest rate allows us to address potential endogeneity problems arising from the omission of factors simultaneously affecting loan terms and government bonds. Note that, as government bonds carried negative yields over the period, the spread is generally larger than the associated interest rate.

We consider a very parsimonious model that expresses the spread as a linear combination of the following determinants:

$$s_{kamct} = \alpha_0 + \alpha_1 L_{am} + \alpha_2 I_k + \alpha_3 T_t I_k + \beta_c D_c + \varepsilon_{kamct},$$

where L_{am} is a vector of loan characteristics, including the duration of the loan, its square, and the amount borrowed, I_k is a vector of institution fixed effects, T_t a vector of time fixed effects and D_c a vector of project categories. Through the institution fixed effect, we assume that different lenders adopt different pricing strategies, depending on their client portfolio, size or capitalization. The product $T_t I_k$ captures institutions' individual responses to changes in the macroeconomic and financial environment. The associated coefficient α_3 can be interpreted as the additional effect of a particular institution for a particular loan category with respect to the average effect of that institution α_2 and the average effect of that loan category β_c.[17]

The coefficients β_c associated with loan categories are our main estimates of interest. We subject them to t-tests in order to assess the hypotheses stated in Section 2, which we statistically reformulate as follows:

H1: $\beta_1^{green} \leq \beta_1^{conventional}$

H2: $\beta_1^{retrofit} \leq \beta_1^{vehicle}$

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[16] Source: ECB, Data Source in SDW: Government bond, nominal, all issuers whose rating is triple A - Svensson model - continuous compounding - yield error minimisation - Yield curve spot rate - Euro, provided by ECB

[17] The institution and institution*time fixed effects allow us to deal with the cross-institution correlation and the autocorrelation of the error terms. This increases the precision of our estimates. One would also like to cluster errors by designation or institution to account for intra-institution correlation. Yet that would be equivalent to assuming no correlation between the clusters, which, given the high degree of competition in the banking market, we consider a restrictive hypothesis. Moreover, a robust estimation would require many more clusters – typically 40 to 50 (Angrist and Pischke, 2009).

The sign in hypothesis H2 is motivated by the trends glimpsed in the time series (Figure 4). We test H1 with the two-item categorization, H2 with the three-item categorization and examine the interaction of the two hypotheses with the five-item categorization.

To improve external validity, we assign weights to our observations proportional to the market share of the corresponding banking group (Table 1). We further assign uniform weights to all subsidiaries within a banking group.

8.6 Estimation results

8.6.1 General effect of loan designation

We estimate three variants of the model with ordinary least squares (OLS): model 1 uses the two-item categorization; model 2 uses the three-item categorization; model 3 uses the five-item categorization (Table 3). As expected, the spread is positively related to the duration, though at a slightly decreasing rate. An additional year increases the spread by about 0.4 percentage point. In contrast, the amount has a very small, negative effect on the spread.

Table 3: OLS estimates of the baseline regression

Dependent variable: spread (percentage points)	Model 1 2 categories	Model 2 3 categories	Model 3 5 categories
Constant (Other)	4.50***	4.51***	4.51***
	(-39.66)	(-39.6)	(-39.58)
Duration (month)	0.03***	0.03***	0.03***
	(-41.17)	(-41.13)	(-41.01)
Duration^2	-0.00***	-0.00***	-0.00***
	(-20.92)	(-21.01)	(-20.86)
Amount (10,000€)	-0.02***	-0.02***	-0.02***
	(-45.83)	(-45.81)	(-45.76)
Green dummy	-0.02**		
	(-2.97)		
Renovation		0.03**	0.02*
		(-2.66)	(-2.07)
Vehicle		-0.04***	-0.03*
		(-3.35)	(-2.55)
Renovation green			0.04***
			(-3.76)
Vehicle green			-0.50***
			(-33.56)
Institution dummy	YES	YES	YES
Institution dummy*Time dummy	YES	YES	YES
N	240,962	240,962	240,962
R-sq	0.41	0.42	0.42
adj. R-sq	0.41	0.41	0.41

t-statistics in parentheses
* p<0.05, ** p<0.01, *** p<0.001

The comparison of projects dummies across models suggests that green projects are priced below conventional projects (model 1) and that vehicle projects are priced below renovation projects (model 2). These results are statistically significant at conventional levels and confirmed by t-tests (Table 4), but small in magnitude. Interacting the two dimensions in model 3, we see that the former result does not apply to renovations and is in fact driven by the strong discount observed on green vehicles, which we recall is attributable to one institution. Again, these results are statistically significant and confirmed by t-tests.

Table 4: Statistical tests on the baseline regression

	Model 1	Model 2	Model 3
	2 categories	3 categories	5 categories
H0: β_green=0	Rejected		
t-stat value	-2.97		
p-value	0.000		
H0: β_renovation<β_vehicle		Rejected	
t-stat value		8.05	
p-value		0.000	
H0: β_renovation_gr<β_renovation			Rejected
t-stat value			2.66
p-value			0.003
H0: β_vehicle_gr<β_vehicle			Not rejected
t-stat value			-35
p-value			0.000

These results together suggest that home energy efficiency is subject to a double energy efficiency gap: (i) renovation projects carry relatively high interest rates; (ii) within this category, the green attribute further increases the interest rate.

8.6.2 Effects by year of sample

Motivated by the changes observed in the time series by categories (Figure 4), we estimate the different models on year subsamples (Table 5). The coefficients associated with duration indicate a steeper yield curve in 2016. The green discount observed over the period is only effective in 2016; conversely, in 2015, green projects carry a higher interest rate (model 1). Likewise, the ranking observed over the period between renovation and vehicle projects only applies to 2016 and is reversed in 2015 (model 2). The change in the merit order of the five categories observed in 2016 is consistent with an interaction between these two shifts (model 3). Again, all results are statistically significant and confirmed by t-tests. This leads us to the conclusion that the double energy efficiency gap observed over the period is not consistent: in 2015, only its first dimension applies, whereas in 2016, only its second dimension applies. In other words, the market seems to increasingly recognize the lower risk associated with green projects, but charges increasingly higher interest rates for renovation projects than for vehicles.

Table 5: Evolution of the effects

Dependent variable:	2 categories		3 categories		5 categories	
spread (in percentage terms)	2015	2016	2015	2016	2015	2016
Constant (other)	4.857***	5.883***	5.134***	5.795***	5.133***	5.785***
	(44.7)	(28.59)	(46.35)	(27.67)	(46.29)	(27.6)
Duration (month)	0.0276***	0.0401***	0.0266***	0.0410***	0.0267***	0.0407***
	(20.34)	(43.16)	(19.49)	(43.77)	(19.59)	(43.49)
Duration^2	-0.0002***	-0.0002***	-0.0002***	-0.0002***	-0.0002***	-0.0002***
	(-16.60)	(-21.98)	(-15.43)	(-22.93)	(-15.51)	(-22.62)
Amount (10,000€)	-0.0241***	-0.0224***	-0.0238***	-0.0224***	-0.0238***	-0.0223***
	(-16.32)	(-42.85)	(-16.19)	(-42.94)	(-16.19)	(-42.86)
Green dummy	0.0642***	-0.0581***				
	(8.78)	(-8.55)				
Renovation			-0.449***	0.187***	-0.469***	0.197***
			(-21.12)	(15.95)	(-21.82)	(16.08)
Vehicle			-0.296***	0.0314*	-0.291***	0.0457***
			(-13.65)	(2.45)	(-13.32)	(3.54)
Renovation green					-0.328***	0.131***
					(-15.20)	(11.14)
Vehicle green					-0.269***	-0.781***
					(-12.10)	(-43.88)
Institution dummy	YES	YES	YES	YES	YES	YES
Institution dummy*Time dummy	YES	YES	YES	YES	YES	YES
N	69,695	171,267	69,695	171,267	69,695	171,267
R-sq	0.48	0.403	0.488	0.404	0.489	0.406
adj. R-sq	0.476	0.401	0.484	0.402	0.485	0.404

t statistics in parentheses
* p<0.05, ** p<0.01, *** p<0.001

8.6.3 Effects by loan maturity

Motivated by the changes observed in the time series by maturities (Figure 5), we estimate model 3 on duration subsamples, considering separately 12-month loans and loans with longer duration (Table 6). The ranking of categories for 12-month loans conforms that observed at the aggregate level. When considering loans with longer duration, this ranking changes in one important respect: green renovations are charged low interest rates only seconded by green vehicles. In other words, lenders seem to perceive green retrofits as riskier investments when financed by a short-term loan than when financed by a long-term loan. Further regressions on both year and maturity subsamples suggest that this phenomenon essentially occurred in 2016.

Table 6: Comparison of short-term and long-term effects

Dependent variable:	Duration		
spread (in percentage point)	12 months	>12 month	all
Constant (other)	2.847***	5.305***	4.51***
	(-51.02)	(-123.82)	(-39.58)
Duration (month)		-0.0181***	0.03***
		(-14.37)	(-41.01)
Duration^2		0.0002***	-0.00***
		(-18.83)	(-20.86)
Amount (10,000€)	-0.0192***	-0.0229***	-0.02***
	(-17.56)	(-43.78)	(-45.76)
Renovation	0.0826***	-0.0260*	0.02*
	(-3.83)	(-2.29)	(-2.07)
Renovation green	0.313***	-0.0585***	0.04***
	(-13.2)	(-5.60)	(-3.76)
Vehicle	0.0757**	-0.0517***	-0.03*
	(-3.12)	(-4.32)	(-2.55)
Vehicle green	0.049	-0.564***	-0.50***
	(-1.84)	(-37.02)	(-33.56)
Institution dummy	YES	YES	YES
Institution dummy*Time dummy	YES	YES	YES
N	34,135	206,827	240,962
R-sq	0.662	0.469	0.415
adj. R-sq	0.652	0.466	0.413

t statistics in parentheses
* p<0.05, ** p<0.01, *** p<0.001

8.6.4 Effects by lending institution

We run an alternative specification of model 3 with an additional interaction term $D_c I_k$ meant to capture the idiosyncratic way in which institutions price the risk associated with loan designations, as compared to the market. The results are displayed in Table 7. Generally speaking, Cofidis, Credit Mutuel, Société Générale et Cofinoga post the highest interest rates while LCL, BNP, Caisse d'Epargne and Cetelem post the lowest rates (column 1). The specific way in which an institution values a project category is given by the sum of the institution coefficient in the first column, the project category coefficient in the first row and the appropriate coefficient in the institution-category matrix. Thus estimated, the institutions' pricing strategies appear highly heterogeneous. In particular, among the institutions making a distinction between green and conventional renovations, Domofinance, Financo and Prêt d'union offer lower interest rates for the former, while Cetelem adopts the opposite strategy.

Table 7: Effects by loan type and lenders

	Institution FE	Loan category FE			
		Renovation	Renovation Green	Vehicle	Vehicle Green
		-0.324***	-0.000443	-0.413***	-0.774***
		Additional Category*Institution FE			
BNP	-0.813***	0.334***		-0.214**	
CAISSE D'EPARGNE	-1.087***	1.659***		2.134***	
CETELEM	-0.980***	0.578***	0.438***	0.0772	
COFIDIS	2.066***	0.244**		0.439***	
COFINOGA	0.449**	-0.296**		-0.141	
CREDIT AGRICOLE	-0.0628	0.390***		0.212*	
CREDIT MUTUEL	0.816***	-3.283***		-0.521***	
DOMOFINANCE	-0.456***	-0.340***	-0.586***		
FINANCO	-0.0472	-0.0922	-0.547***	-0.372***	
FRANFINANCE	-0.868***	0.463***			
LCL	-2.810***			1.300***	
PRET D'UNION	-0.353**			0.414***	
SOCIETE GENERALE	0.522**				
SOFINCO	-0.512**	1.482***			

We then exploit the fact that three banking groups – BNP Paribas, Crédit agricole, Société générale – are members the Environmental and Social Corporate Governance (ESCG) group to see if such a commitment has an impact on their pricing behaviour. We run model 3 on two subsamples respectively gathering ESCG members and non-members. The regressions are little informative as to whether the pricing of green projects varies between the two groups, as the former is the only one that makes a distinction between green and conventional vehicles, yet it makes no distinction between green and conventional renovations. Interestingly, however, the regressions suggest that the two groups adopt opposite pricing strategies with respect to Hypothesis 2 (Table 8). Specifically, ESCG institutions charge higher interest rates for renovations than for vehicles. Moreover, it is noteworthy that non-ESCG institutions charge lower interest rates for green renovations than for conventional ones.

Table 8: Effect of ESCG status

Dependent variable	ESCG status	
spread (in percentage point)	no ESCG	ESCG
constant (Other)	4.088***	4.444***
	(70.69)	(41.71)
Duration (month)	0.040***	0.031***
	(38.47)	(26.04)
Duration^2	-0.000***	-0.000***
	(-21.83)	(-18.81)
Amount (10,000€)	-0.020***	-0.014***
	(-33.25)	(-13.24)
Dummy Retrofit	-0.207***	0.082**
	(-15.73)	(3.28)
Dummy Retrofit Green	-0.626***	
	(-51.05)	
Dummy Vehicle	-0.165***	-0.347***
	(-12.80)	(-14.83)
Dummy Vehicle Green		-1.612***
		(-64.28)
Time fixed effects	YES	YES
N	215859	25103
R-sq	0.123	0.412
R-sq adj	0.122	0.409

t statistics in parentheses
* p<0.05, ** p<0.01, *** p<0.001

8.7 Robustness checks

8.7.1 Macroeconomic and financial controls

We substitute a set of macroeconomic and financial variables for time fixed effects and examine how it affects the values of the estimated coefficients of loan categories. We estimate the following model:

$$s_{kamct} = \alpha_0 + \alpha_1 L_{am} + \alpha_2 I_k + \alpha_3^M M_t + \alpha_3^F F_t + \beta_c D_c + \varepsilon_{kamct},$$

where M_t is a vector of macroeconomic variables, F_t a vector of financial variables, and all other variables are those defined in the previous model. Macroeconomic controls include: the inflation rate, as measured by the harmonized index of consumer prices; the unemployment rate, which approximates the phase of the business cycle; the interest rate on one-year government bonds in the Euro area, which captures the quantitative easing in which the European Central Bank (ECB) engaged during the period. Financial controls include: the spread between the return on the CAC40 index and the interest rate on one-year government bonds, which approximates the volatility of the stock market; the stress index provided by the ECB, which approximates the volatility in the bond market;[18] and investors' expectations, as measured by the slope of the yield difference between ten-year and one-year government bonds.

These substitutions do not qualitatively affect the results of the baseline model and preserve the ranking between the interest rates associated with different project categories (Table 9). Macroeconomic and financial factors explain a very modest part of the variation of the spread, which is consistent with previous findings (Gambacorta, 2008). Unemployment stands out at the only added control with a statistically significant effect. Its negative sign could be explained by a depressed demand, to which lenders respond with lower interest rates. Another explanation could be that unemployment insurance offered by lenders during the negotiation process can mitigate risks (Hsu et al., 2012). Despite being non-significant, estimates for the other variables have the expected polarity. Quantitative easing has a positive effect, suggesting that institutions benefited from a loosening of the monetary policy, possibly at the expense of consumers. Inflation too has a positive effect, suggesting that cost pass-through is affected by some market power. Higher risks in the equity market, as approximated by the two volatility indices, increase the spread, suggesting that lenders transfer part of the portfolio risks to their clients. The impact of the yield curve slope is positive, suggesting that optimistic expectations are associated with a higher demand for consumer loans.

[18] Euro area (changing composition), Stress subindice - Bond Market - realised volatility of the German 10-year benchmark government bond index, yield spread between A-rated non-financial corporations and government bonds (7-year maturity bracket), and 10-year interest rate swap spread, Contribution.

Table 9: Effect of macroeconomic and financial controls

Dependent variable	Baseline model with controls for			
spread (in percentage point)	Baseline model	Macro factors	Financial factors	Macro and financial factors
Constant (Other)	4.509***	6.789***	-5.224	-5.129
	(-39.58)	(-6.94)	(-0.00)	(-0.00)
Duration (month)	0.033***	0.033***	0.033***	0.033***
	(-41.01)	(-41.02)	(-41.01)	(-41.02)
Duration^2	-0.000***	-0.000***	-0.000***	-0.000***
	(-20.86)	(-20.86)	(-20.86)	(-20.86)
Amount (10,000€)	-0.023***	-0.023***	-0.023***	-0.023***
	(-45.76)	(-45.75)	(-45.76)	(-45.75)
Dummy Retrofit	0.022*	0.020*	0.022*	0.020*
	(-2.07)	(-1.88)	(-2.07)	(-1.88)
Dummy Retrofit Green	0.039***	0.038***	0.039***	0.038***
	(-3.76)	(-3.63)	(-3.76)	(-3.63)
Dummy Vehicle	-0.028*	-0.032**	-0.028*	-0.032**
	(-2.55)	(-2.83)	(-2.55)	(-2.83)
Dummy Vehicle Green	-0.495***	-0.499***	-0.495***	-0.499***
	(-33.56)	(-33.78)	(-33.56)	(-33.78)
One-year bonds		11.33		-1.269
		(0.34)		(-1.23)
Price index		0.2		-0.025
		(0.97)		(-0.68)
Unemployment		-0.112***		-0.112***
		(-6.29)		(-6.29)
CAC40			1.867	2.166
			(-0.65)	(0.65)
Stress index			15.84	17.02
			(1.03)	(-0.65)
Yield curve slope			0.685	-0.069
			(0.49)	(-0.39)
N	240,962	240,962	240,962	240,962
R-sq	0.415	0.416	0.415	0.416
adj. R-sq	0.413	0.413	0.413	0.413

t statistics in parentheses
* p<0.05, ** p<0.01, *** p<0.001

8.7.2 Placebo tests

As stated in Section 3.2, we build our own categorization of the 90 distinct designations recorded by the robot. While most designations labels are clear enough to be categorized in a straightforward manner, green-renovation labels are subject to interpretation. We conduct two placebo tests to examine the relevance of our categorization in general, and that of the green-renovation category in particular.

In the first placebo test, we randomly assign each of the 90 designations to one out of five arbitrary categories, following a uniform distribution. We then produce OLS estimates of model 3 with these categories, simply labelled 1 to 5. We repeat this procedure 1,000 times. Figure 7 displays the distribution of estimated coefficients for all categories. Table 10 displays the mean of obtained coefficients and p-values. The table confirms that the coefficients estimated for arbitrary categories are centered around zero. The mean of the p-value is 0.5 and it is uniformly distributed, as it should be under the null hypothesis that the value of each of the coefficients is zero. The results lead us to the conclusion that our five-item categorization is meaningful.

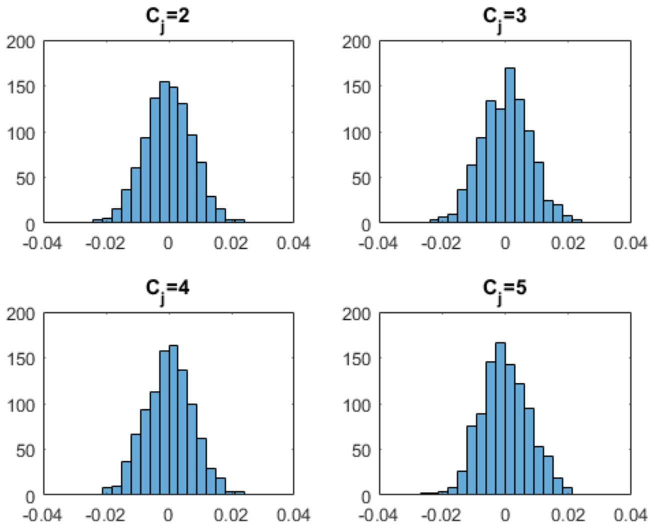

Figure 8: Placebo test on all categories

Table 10: Placebo test on all categories

	$C_j = 2$	$C_j = 3$	$C_j = 4$	$C_j = 5$
$\hat{\beta}_1$	0.00	0.00	0.00	0.00
$\hat{\sigma}_{\beta_1}$	0.01	0.01	0.01	0.01
p-value	0.50	0.50	0.51	0.50

In the second placebo test, we restrict the procedure to those designations which initially fell in either renovation or green renovation categories. We randomly assign those designations to two arbitrary categories while maintaining other designations in their initial category (vehicle, green vehicle and other). We then estimate model 3 and repeat the procedure 1,000 times. The distributions of estimated coefficients appear much narrower for the two vehicle categories than for the two arbitrary renovation categories (Figure 8). The latter are moreover centered around the same value. The mean p-value of 0 indicates that, on average, the null hypothesis on the insignificance of the coefficients is rejected (Table 11). Moreover, the probability distribution of the p-value is not uniform but has a bell shape skewed towards zero, as it should when the null is rejected. This indicates that, irrespective of the green attribute, the retrofit category has a significant impact on the spread. A statistical test fails to reject the null hypothesis that estimated coefficients for the two arbitrary categories are equal (F(1,239939)=0.16; Prob>F=0.6901), as the two placebo categories are now indistinguishable. However, they are different from our baseline estimates obtained with our categorization (F(1,239939)=9.03;Prob>F=0.0001), thus implying that our categorization of conventional and green renovations is also meaningful.

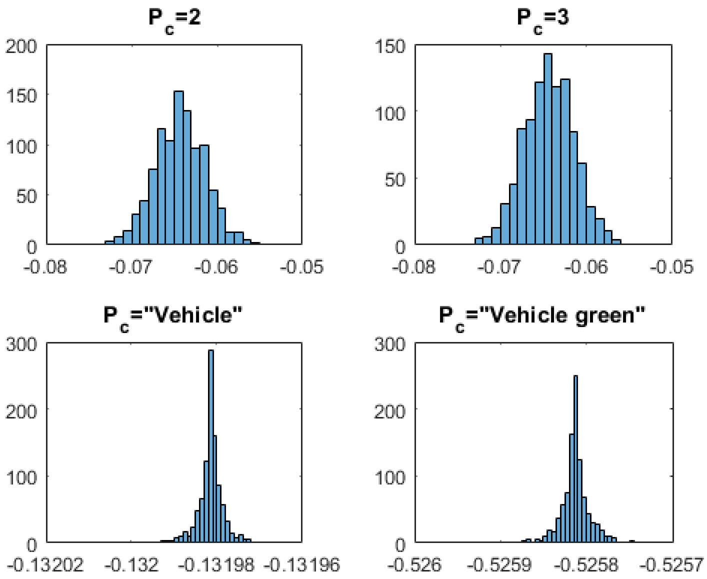

Figure 9: Placebo test on renovation categories

Table 11: Placebo test on renovation categories

	Retrofit 1	Retrofit 2	Vehicle	Vehicle green
$\hat{\beta}_1$	-0,06	-0,06	-0,13	-0,53
$\hat{\sigma}_{\beta_1}$	0,01	0,01	0,01	0,03
p-value	0,00	0,00	0,00	0,00

8.8 Conclusion

We have assembled a unique panel dataset of simulated-loan data to investigate how the interest rate for green projects compares to that of conventional projects on the one hand, how the interest rate for renovations compares to that of vehicles on the other. Regarding the first hypothesis, we found a green discount in 2016, but not in 2015. This result is consistent with the notion that financial agents increasingly value environmental aspects, as recently substantiated by An and Pivo (2018) in the US market for commercial mortgages and Karpf and Mandel (2018) in the US market for municipal bonds. Regarding the second hypothesis, the differences we observe in the interest rates offered for different types of loans is consistent with lenders using loan designation as a screening device. Specifically, our findings suggest dominance of the risk channel in 2015 and dominance of the WTP channel in 2016 in lenders' pricing strategies. Generally speaking, our results are small in magnitude but robust to a variety of specifications. They together suggest that different types of information asymmetries might affect the market for unsecured credit in France, at different points in time. This is particularly true for home energy retrofits, which can be interpreted as a new form of energy efficiency gap.

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9. Work Package 3.1. Evaluating policy instruments for improving the efficiency of the building stock (lead partner: DIW)

Authors: Nolan Ritter (DIW, MCC), Puja Singhal (DIW)

9.1 Abstract

Increasing energy efficiency is one of the routes pursued by the European Commission to reduce energy consumption and carbon emissions. In this regard, the building stock may offer considerable potential for reductions. To realize this expected potential, a battery of political instruments is applied. While the cost of financial support measures is easily measured, this is often not the case for command and control mechanisms. Moreover, there exist serious problems regarding the evaluation of causal treatment effects on energy consumption. These are laid out here. Particular regard is given to identification strategies and the existence of rebound effects and free-ridership.

9.2 Introduction

Since the first oil crisis of 1973, many policy measures have been implemented to curb energy consumption. Initially, the main focus was to decrease the level of dependence on energy imports. Now, the focus is on reducing energy consumption to preserve the environment and to protect the climate. To reduce the overall carbon intensity and to curb the impact of climate change, all sectors have to make contributions. With approximately 27% of global energy consumption and about 17% of global CO2 emissions (Nejat et al., 2015), the residential sector is a significant contributor. Because most of the residential energy consumption is for heating, increases in the efficiency of the building stock may be a viable option to reduce emissions.

Other means of reducing energy consumption focus on price instruments such as taxes. However, there is evidence that household are often unaware of the energy prices they pay (Martiskainen, 2007; Wilson and Dowlatabadi, 2007; Frederiks et al., 2015; Ramos et al., 2015). Consequently, if households are ignorant of the prices they pay, price based instruments will be ineffective. This problem is exacerbated because households are not interested in the energy per se. Rather, households want to consume an energy service, for example in the form of an ambient in-door temperature.

Another kind of policy instrument focuses on regulation. Amongst the best published analyses is the one by Levinson (2016), who focusses on residential energy consumption comparing buildings in California before and after the implementation of mandatory building codes in 1978. The intuition behind the analysis is simple. If building codes are an effective way to curb energy consumption, similar households should consume less energy if the house is of a newer vintage. While it is an excellent analysis, the under- lying data from the Residential Appliance Saturation Study and from the Residential Energy Consumption Survey lacks important information. For example, no information is available on whether energy efficiency improvements have been made during the life time of the house. There is also the problem that buildings of low energy efficiency from early vintages might have already be decommissioned which would lead to biased estimates because of sample selection. Bias resulting from the indicated problems may lead Levinson (2016) to conclude that building codes are ineffective. A study by Jacobsen and Kotchen (2013) that finds that building codes are effective supports this notion.

Levinson (2016) and Jacobsen and Kotchen (2013) are just two examples of analyses that focus on building efficiency measures that arrive at conclusions diametrically opposed to one another. This may be a consequence of the many problems that impede the estimation of causal effects and the robust evaluation of programs aimed at improving energy efficiency in the building stock, such as missing or inadequate control groups, sample selection, or the rebound effect.

The analysis continues with an overview of identification strategies before turning to empirical problems such as free-ridership and the rebound effect. Following this we examine the existing literature on the evaluation of efficiency measures in buildings. The final section summarizes and concludes.

9.3 Identification strategies

The primary goal of support mechanisms in the building sector is to increase energy efficiency. Because the support is expressly targeted at efficiency improvements, an appropriate evaluation must focus on improvements in energy efficiency. For example, in case that old windows were substituted for newer ones, we would expect an increase in the airtightness of the house. However, most often the effectiveness of a treatment aiming at increasing efficiency is measured in terms of its impact on energy consumption. This is not in line with the intended primary aim of the treatment and, thus, may not reveal whether the intended consequences of the treatment were achieved.

Nevertheless, let Y_{it} indicate energy consumption for household *i* at time *t*, then

$$Y_{it} = \beta . X_{it} + \delta . T + \epsilon_{it}$$

β describes the effect of the control variables in *X*, while δ indicates the effect of the binary treatment *T* . *T* takes on a value of 1 in case that a household participates in the treatment. It is 0 otherwise. $\varepsilon_{it} = v_{it} + \eta_i$ is an error term that comprises of a random error term *v* and a household fixed effect η_i with $E(v_{it}) = 0$. For simplicity, it is assumed that there exist only 2 time periods. $t = 0$ is the pre-treatment period, while $t = 1$ is the post-treatment period.

If all confounding factors *X* were controlled for, the *treatment effect on the treated* compares the treated *i* at time *t* = 1 in case they are treated (*T* = 1) to the unobservable counterfactual if the treated had not been treated (*T* = 0):

$$\delta = E\left[Y_{i=1,t=1} \middle| X = x, T = 1\right] - E\left[Y_{i=1,t=1} \middle| X = x, T = 0\right]$$

However, the treated can only ever be observed having been treated. Thus, the expression $E(Y_{i=1,t=1} \mid X = x, T = 0)$ has to be replaced with observations from another group that shares similar characteristics, but is untreated, and, thus approximates the outcome in the treated had they not been treated. Frondel and Schmidt (2005) provide for a more comprehensive overview than presented here.

One potential way to approximate $E(Y_{i=1,t=1} \mid X = x, T = 0)$ is to substitute the expression with $E(Y_{i=1,t=0} \mid X = x, T = 0)$, that is the outcome in the group of the treated in the period before the treatment was administered. Figure 1 displays apparent increases and decreased in the treated across time when comparing $t = 0$ and $t = 1$.

If there are any unaccounted-for influences on the outcome, then the before and after comparison leads to biased assessments. In Figure 2, the difference in the outcome between $t = 0$ and $t = 1$ is the same as in Figure 1a. In addition, the gray line indicates the outcome in the absence of unaccounted for influences. The shock displayed in Figure 2a introduces a positive bias $+\Delta E$. Thus, the treatment effect is smaller than apparent. Figure 2b displays the effect of a negative shock that leads to an underestimation of the real effect.

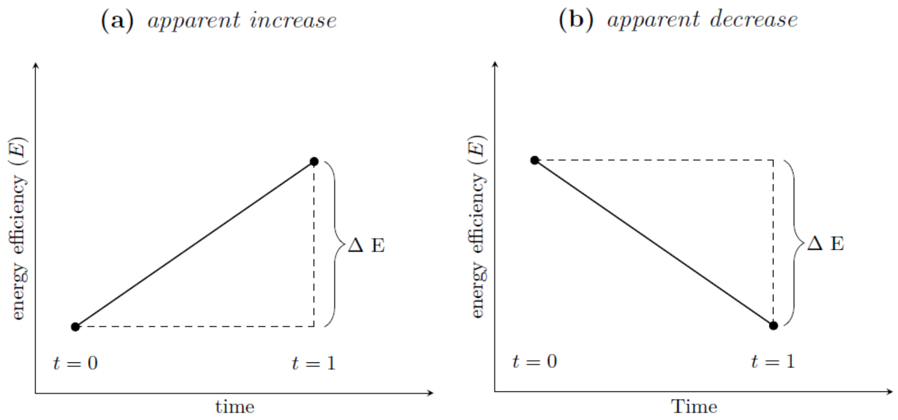

Figure 10: Apparent changes

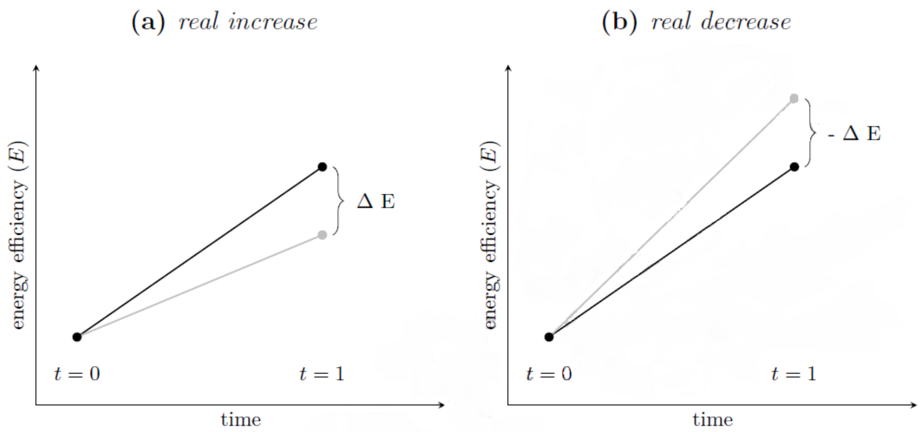

Figure 11: Real changes

If there exist some differences, these need to be controlled for in order not to bias the assessment. Figure 3 indicates a difference in the level of efficiency at time *t* = 0. Between *t* = 0 and *t* = 1, efficiency declines in the treatment group (black line) and the control group (gray line). However, the decline is larger in magnitude in the control compared to the treatment group. Yet, not the total difference in efficiency at *t* = 1 but the total difference that exceeds the difference that existed at the outset indicates the impact of the treatment.

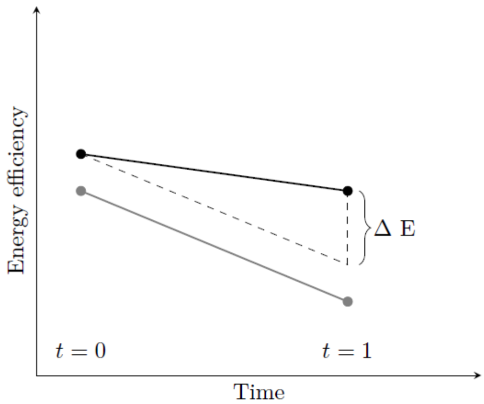

Figure 12: Counterfactual example 2

A cross-section analysis is a potential alternative to before-after comparisons. This approach approximates the effect of the treatment according to

$$\delta = E\left[Y_{i=1,t=1} \middle| X = x, T = 1\right] - E[Y_{i=0\ t=0} | X = x, T = 0]$$

Thus, the counterfactual is approximated by observations from untreated households. Here, it is implicitly assumed that there are no unobservable differences between the treated and the untreated. Because the observations are from the same time period, any general shock may be assumed to influence the treated and the non-treated in a similar manner. However, the main problem with this evaluation approach is that the treated may have self-selected into the treatment. If the government offers support for efficiency improvements, these are most likely taken up by households who are environmentally conscious, or better informed. Thus, it is likely that those who self-select are systematically different from those who did not. Obviously, the assumption of the absence of any kind of self-selection in voluntary treatments is untenable. In case of command-and-control measures, it is not possible to employ a cross-sectional approach when the treatment leaves no one untreated.

The difference-in-difference estimator

$$\delta = E\left[Y_{i=1,t=1} - Y_{i=1,t=0} \middle| X = x, T = 1\right] - E\left[Y_{i=0,t=0} - Y_{i=0,t=0} \middle| X = x, T = 0\right]$$

compares the difference in the outcome of the treated across time with the difference in the outcome of the non-treated, also across time. The difference-in-differences estimator exploits the cross-sectional dimension to account for any changes in the observable characteristics. The longitudinal dimension is used to remove any unobserved fixed effects. Frondel and Schmidt (2005) indicate, however, that the difference-in-difference estimator can be quite restrictive, especially in the face of heterogeneous effects.

The gold standard in evaluating the efficiency of support measures is the time tried method of experimentation. The effects of voluntary treatments with self-selection are identified by gathering those who would self-select into treatment and randomly select some for treatment (S = 1) and others to form a control group (S = 0). Thus, the identification would be according to

$$\delta = E\left[Y_{i=1,t=1} \mid X = x, T = 1, S = 1\right] - E\left[Y_{i=1,t=1} \mid X = x, T = 1, S = 0\right]$$

Because the control group consists of those who would have self-selected into treatment had they not been barred, the effect of self-selection is ac- counted for. The random assignment implies that treatment is independent of any observable and unobservable characteristics. Whether the randomization was successful can be tested by trying to explain treatment status using observable characteristics. In the limit, we can be confident that unobservable characteristics are similarly distributed in the treatment and the control group. While assumptions have to be made in any analysis, experiments require only minimal assumptions, such as unit homogeneity (Holland, 1986). This assumption states that similar observational units behave similarly given the same treatment. This assumption also has to be met for all the other identification approaches presented here.

While experiments are undoubtedly the most reliable evaluation method, they are not always feasible. When the government implements command- and-control measures, everyone gets treated. Consequently, it is impossible to randomly allocate observation units into a control group.

9.4 Empirical problems

Choosing an appropriate identification strategy is the necessary condition for identifying causal effects. But such a strategy is not sufficient on its own. Additional empirical problems have to be overcome. These include free-ridership, sample selection, or the market situation in general.

9.4.1 Free ridership

Any assessment that pursues the question of whether a support scheme for energy efficiency improvements is effective, must demonstrate a causal relationship between the treatment and the outcome. Demonstrating correlation is insufficient because it leaves room for the possibility that efficiency improvements would have taken place in the absence of the treatment any- way. In the literature, this possibility is discussed under the name of free ridership.

Free-ridership indicates that households would have invested in energy efficiency improvements irrespective of the availability of policy tools such as preferential loans or grants. In this case, the preferential loans have no impact on the efficiency gains at all. Failing to identify this kind of free- ridership, researchers may attribute positive effects to the policy measure when in fact there are none. This would ultimately place an upward bias on the effect of policy programs on energy efficiency improvements.

The second possible case is that households may have increased the scale of the energy efficiency improvements. Households may have used a share of the support to increase the scale of the investment and used the remainder to lessen their investment. Again, failing to account for this kind of free- ridership, places an upwards bias on the estimated efficacy of the policy instrument.

The third possibility is that households have increased the scale of the investment by an amount that was equal to the support that they were given. In this case, no bias is introduced. In the fourth case, the support given triggered investments greater than the amount that was given as support. Fifth, and finally, it is also possible that the investment would not have been made in the absence of the support scheme. This raises the question as to whether the entirety of the efficiency gains should be attributed to the policy measure without which the gains would not have been made. Although free-ridership is a well-known issue in economics, especially in the provision of public goods, there exist only a small number of studies focusing on free-ridership in the realm of energy efficiency improvements. The analysis by Grosche and Vance (2009) draws on data of about 2, 500 households owning and residing in single-family homes. They indicate that between 1995 and 2004 about 64% implemented at least one efficiency measure. In particular, household were asked to indicate whether they improved 1) the roof insulation, 2) facade insulation, 3) replaced windows, or 4) re- placed heating equipment. Their survey also elicited information about the costs of the improvement. In addition, Grosche and Vance (2009) asked households to indicate whether they would have carried out the improvements in the absence of support measures. Altogether, they found that up to 50% of household would have carried out the measures irrespective of support mechanisms which leads the authors to conclude that free-ridership is a serious issue.

Combining a regression discontinuity approach to evaluate a large-scale residential energy-efficiency program, Boomhower and Davis (2014) arrive at a similar conclusion. About 50% of the households in the study would have adopted the energy efficiency improvement without receiving any subsidy. The evidence that the analysis reveals let the authors conclude that policy instruments targeted at improving energy efficiency in the building sector were "almost certainly not cost-effective."

Nauleau (2014) provides evidence for free-ridership in home energy efficiency improvements from France. In 2005, France created a tax credit for home insulation. Combining information on about 24, 000 households observed between 2002 and 2011 with a logit model with random individual effects, Nauleau (2014) finds that the rate of free-ridership is between 40% and 85%.

Malm (1996) is also highly skeptical about the efficiency of programs that seek to improve energy efficiency. While most of the existing literature on free-ridership is based on asking households the hypothetical question whether they would have done the improvement irrespective of support (Joskow and Marron, 1992; Eto et al., 2000), Malm (1996) uses the Energy Information Administration's Residential Energy Consumption Survey (RECS) to create a reference for households that were involved in a demand side management program. This quasi experimental approach indicates that about 89% of household who received support for the improvement of their heating system would have done so in any case.

Altogether, these findings suggest that free-ridership is a widespread phenomenon. Most observed improvements would have taken place in the absence of financial support. Hence, financial support seems to do little in the way of enabling efficiency improvements.

9.4.2 Sample selection

As laid out by Heckman (1979), self-selection of households into a voluntary treatment leads to non-random samples and bias. The households observed participating in financial support schemes for efficiency measures have little in common with a random sample drawn from the population. Hence, any insights gathered cannot be extended to the general population. Moreover, since the selection mechanism is often times unclear, one cannot control for its effect on the effectiveness of the treatment. In some cases, the self- selection bias can be dealt with employing a Heckman estimation model when strong correlates exist that explain the chance of observation but not the outcome.

It seems prudent to assume that the basis for self-selection into efficiency measures is the need for replacement, for example in the case of heating equipment. Replacing new equipment would imply small efficiency gains at high cost. This assumption is corroborated by the fact that refurbishment hardly ever occurs in new buildings, except when deficiencies have been detected.

Hartman (1988) is among the first to have addressed the issue of self- selection into voluntary energy conservation programs. His work was motivated by the fact that the existing analyses had compared households who had self-selected into conservation programs with households who had not participated. He was concerned that choosing the wrong counterfactual would bias the results and lead to biased conclusions on how to support efficiency measures.

Sample selection is also discussed by Allcott (2015) who in his rigorous evaluations regarding energy conservation and efficiency measures (All- cott, 2011; Allcott and Rogers, 2014) has often relied on data provided by OPOWER. When conducting field experiments, the implicit assumption is that the sample sites are a good approximation of the target cites. Sample cites, however, are often chosen for their ability to successfully implement the experiment. It is also the case that successful programs are more likely to agree to a robust evaluation intending to use the evaluation results to further their own cause. In contrast, programs that expect unfavorable results from robust evaluations will not participate (Pritchett, 2002). Using the OPOWER data set, Allcott (2015) presents evidence that the first 10 out of 111 randomized controlled trials on energy conservation comprising information of more than 8.6 million households across the United States arrive at considerably higher estimates compared to the following 101 trials. This form of sample selection places an upwards bias on the estimate of the causal effect of efficiency improvements.

Levinson (2016) analyses the effect of building codes on energy consumption. California introduced building codes for residential buildings in 1978, so that buildings constructed after 1978 should be more energy efficient compared to houses built before. To find out whether building codes reduce energy consumption, he compares the present energy consumption of households residing in house from different construction areas controlling for a wide range of confounding factors such as income, house type, or family size. One problem with this approach is that buildings from before 1978 have a higher probability of being deconstructed already is higher compared to newer buildings. It seems also reasonable to assume that the most inefficient buildings are deconstructed first. Thus, the houses from earlier construction eras that still exist are most likely to most efficient ones built, while the houses from newer construction eras still comprise a good portion of the inefficient houses. This type of sample selection introduces a downward bias on whether building codes lead to reductions in energy consumption.

9.4.3 The rebound effect

Energy efficiency improvements in the building stock decrease the cost of heating service. All else equal, including household behavior, we would expect to find that the same thermal comfort level can now be achieved using less energy. However, all is not equal. The increase in energy efficiency goes hand in hand with decreases in the cost of heating service which will trigger additional demand if demand is not yet fully saturated. A good description of the rebound effect is provided by Binswanger (2001).

Khazzoom (1980) provides the first economic analysis of the rebound effect. He suggests that energy consumers are interested in the service that the energy provides rather than the energy itself. Energy efficiency μ is the link between energy service s and energy consumption e:

$$\mu = \frac{s}{e}$$

Consumption is optimal in case that the marginal cost equals the marginal price *P*. Consequently, the marginal cost or the price of the energy service is

$$P = \frac{p_e}{u}$$

where p_e indicates the price of energy. From equation (7) it is possible to derive the elasticity of the service demand $\eta_p(s)$:

$$\eta_p(s) = \frac{ds}{dP} * \frac{P}{s} = \frac{\delta(\mu e)}{\delta\left(\frac{p_e}{\mu}\right)} \frac{\frac{p_e}{\mu}}{\mu e} = \frac{\delta e}{\delta p_e} \frac{p_e}{e} = \eta_p(e)$$

The elasticity of energy demand with respect to efficiency $\eta_\mu(e)$ is related to the elasticity of the demand for service *s*

$$\eta_\mu(e) = \frac{\delta e}{\delta \mu} \frac{\mu}{e} = \frac{\delta \frac{s}{\mu}}{\delta \mu} = \left[\frac{\delta s}{\delta \mu} \frac{1}{\mu} - \frac{s}{\mu^2}\right] \frac{\mu}{e}$$

$$= \frac{\delta s}{\delta \mu}\frac{\mu}{s} - 1 = \frac{\delta s}{\delta \frac{p_e}{P}}\frac{\frac{p_p}{P}}{s} - 1 = \frac{\delta s}{\delta \frac{1}{p}}\frac{\frac{1}{p}}{s} - 1$$

$=- \eta_p(s) - 1$

Thus, a 1% increase in energy efficiency leads to a reduction in energy demand by $(1 - |\eta_p(s)|)$%.

Figure 4 presents how households optimize their utility in the case of two goods (x_1, x_2) of which x_2 is energy. In combination with the prices p_1 and p_2 and the available budget, the optimal combination between budget line B_1 and the $U_1$is found in point *A*. When an efficiency improvement is made, the price for the energy service declines. As a consequence, the slope of the budget line decreases. The new budget line B_2 allows the household to achieve a higher utility level. The shift from point *A* to point C indicates the ultimate effect.

The total of the rebound effect is a combination of a substitution and an income effect. In the hypothetical situation that the household had achieved utility level U_1 with a budget line that had had the same slope as the budget line after the efficiency improvement B_2, the optimal combination would have been found in point *B*. The shift from *A* to *B* is the substitution effect, while the shift from *B* to *C* is the income effect.

The rebound effect is a well-studied phenomenon in the economic literature. Comprehensive overview material is provided by Sorrell et al. (2009) and Thomas and Azevedo (2013). It was first discussed by Jevons (1866) who found it paradoxical that increases in efficacy would lead to increases in consumption. He ultimately feared that technological development to- wards higher efficiency would ultimately speed up the process that would ultimately lead to the exhaustion of England's coal reserves. In particular, Jevons (1866) found that technological development in steam engines had increased their efficiency, decreased their operating costs, and, hence, increased the demand for steam engines and coal

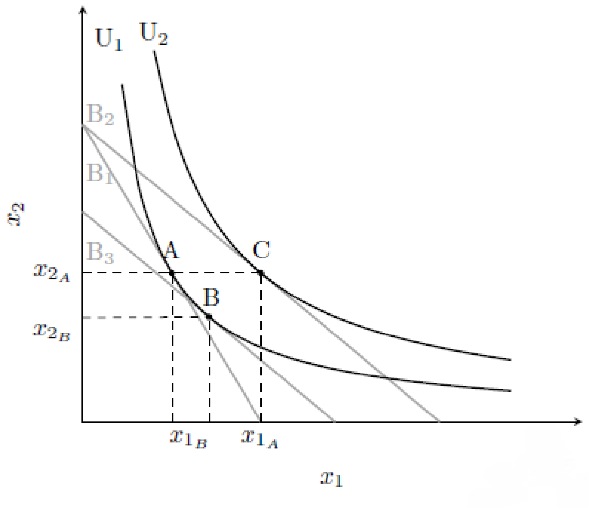

Figure 13: Substitution and income effect

The estimates regarding the magnitude of the rebound effect in residential energy consumption is characterized by a considerable range. Combining time-series and cross-sectional data from Austria, Haas and Biermayr (2000) estimate that about 20% to 30% of the gains from energy efficiency are used for additional energy service consumption. An analysis focusing on Catalonia finds that the rebound effect is about 35% in the short and around 49% in the long run.

Available estimates on the size of the rebound effect not only differ across countries, there is also considerable variation in the estimates for households of the same country for about the same time period. For example, using cross-sectional data on about 1,200 households from the United States, Schwarz and Taylor (1995) estimates that the rebound for heating is between 1% and 3%. Somewhat higher are the findings by Davis (2008), who concludes that the rebound is around 6%. In comparison, Hsueh and Gerner (1993) estimate a short-run rebound effect between 35% and 58%, depending on whether the electricity or natural gas is used for heating. For Canada, Dolhitt (1986) relies on cross sectional data and reports estimates of the rebound effect on the order of 10% to 17% in the short run and between 35% and 60%. Also, for Canada, Guertin et al. (2003), find that in the short run the effect is between 29% and 47%.

The wide range of estimates between 0.6% and 60% is reflected in the wide range of estimation approaches used to identify rebound effects in general (Sorrell et al., 2009). Some studies focus on individual households where others use aggregated data, for example at the level of the county or the region. There exist static models that provide a single estimate and dynamic models that differentiate between the short and the long run. Some analyses are carried out using time-series data, while other either rely on cross sectional data, pooled cross sections, or panels. Researchers use single or multi equation models in which the dependent variable may enter in linear, log-linear, double log, or translog form.

Altogether, the wide range of potential estimates for the rebound effect causes uncertainty in the evaluation of efficiency programs. Moreover, the rebound effect lays open a considerable flaw in focusing on reductions in energy consumption to measure whether treatments have increased energy efficiency. In the example outlined above, considerable efficiency improvements were achieved that cut the cost of energy service in half. However, there are no reductions in the level of energy consumption. Before and after the implementation of the efficiency measure, the consumption is x_{2A} . In order to measure the effect of treatments aiming to improve energy efficiency, it would be necessary to measure the efficiency increases directly instead of inferring them via reductions in energy consumption. Moreover, measures to increase efficiency work in incremental steps. While the first steps to increase efficiency improvements may not have statistically significant effects, mostly because of the standard errors, effects should become measurable eventually.

9.4.4 The market situation

Whether financial support for the adoption of efficiency improvements has an impact on the energy efficiency in the building stock also depends on the market conditions. Figure 5 indicates the equilibrium market outcome for energy efficiency measurements for a given supply function S_1. Assuming that the demand for energy efficiency improvements is D_1, the equilibrium occurs at point *A* characterized by a price level p_A and a quantity of q_A. The newly introduced preferential loans shift Demand from D_1 to D_2. As a consequence, the new equilibrium occurs at a higher price p_B and a higher quantity q_B. In this case, the induced additional demand leads to the desired outcome of additional energy efficiency measures.

But a shift in the demand curve because of financial support does not necessarily lead to the desired outcome. For example, had the initial demand been described by D_2, the policy measure would have shifted demand to D_3. But because the supply of energy efficiency measures turns totally inelastic once the quantity exceeds q_B, any additional support would only have increased the price while the quantity would have remained fixed. In this market situation, the same policy measure would exhaust public resources without contributing to the desired outcome.

The example above implicitly assumed that the supply function for efficiency improvements was fixed. This may not be realistic. For example, the suppliers who offer efficiency improvements might also be active on other markets, for example in the construction of new homes. In case that related markets become more attractive, the supply function for energy improvements would adapt according to changes in opportunity costs. In case that the efficiency supply coincides with impacts on the supply function, the estimated effect of the efficiency improvements would be biased. In case that price increases in related markets increase the opportunity costs, there would be a downward bias. In case that opportunity costs decrease, for example when support measures run out, there would be an upward bias.

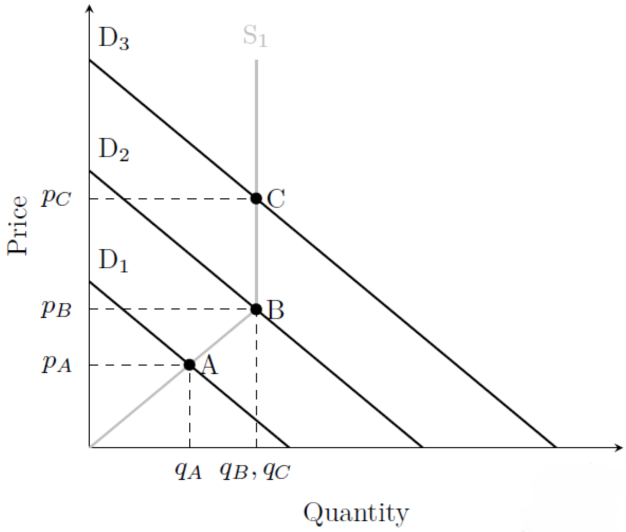

Figure 14: Constraints in the supply of efficiency increases

9.5 Program evaluations

The literature covers a wide range of evaluations of specific programs efficiency measures. A sizeable portion of these analyses follow an engineering approach which makes assumptions about capital costs, discount rates, energy prices, and investment horizons (Allcott and Greenstone, 2012). Examples of the engineering approach can be found in Meier et al. (1983); Goldstein et al. (1990); Koomey et al. (1991); Brown et al. (1998); Rosenfeld et al. (1993); Stoft (1995); Blumstein and Stoft (1995) or Granade et al. (2009). A common, implicit assumption of the engineering approach is that the level of heat demand is independent of energy service costs. Hence, the models do not account for demand increases triggered by the cost lowering effect of efficiency increases. As a consequence, the engineering approach often overestimates the effectiveness of the treatment.

Incidentally, this overstatement may be at the heart of the energy paradox which states that households forgo beneficial investments in energy efficiency because they apply excessive discount rates (Allcott and Wozny, 2014). Metcalf and Hassett (1999) use data from the Residential Energy Consumption Survey conducted by the United States Department of Energy to compare actual savings from efficiency improvements to those predicted by engineering studies. As expected, the actual savings fall short of the predictions. Analyzing actual, metered energy consumption reveals that improvements to attic insulation reduce consumption by about 10%, compared to a 50% reduction indicated in an engineering study by Blasnik (1990). In light of the actual savings, households may not apply excessive discount rates when forgoing risky investments that reduce energy costs by 10%. The existing studies on efficiency improvements can also be classified in terms of whether the evaluation focusses on voluntary or mandatory measures. Most of the existing data focusses on mandatory measures, and thus, avoids the problem of self-selection. Other forms of sample selection, however, still apply. Mandatory programs may also receive more attention because of data availability. For example, most countries survey households and elicit information about energy consumption, efficiency improvements and socio-economic information. Mandatory programs affect all households so that representative surveys can be used to analyze their impact.

Voluntary programs with self-selection are most often too specific to be analyzed using available, regular surveys and have to be addressed by collecting other data. To collect data is not only costly, it has to be done in a manner that overcomes the self-selection issue.

One prominent example of mandatory regulation can be found in Levinson (2016). He analyzes the extent to which California's building codes reduce residential energy consumption. Mandatory building codes for new buildings were implemented in 1978. Houses built under the building regulation should be more efficient compared to those built before. Under otherwise similar circumstances, energy consumption should be lower in more efficient buildings. An analysis by engineers expected reductions in energy consumption on the order of 80%, while the regression approach in Levinson (2016) finds reductions of considerably smaller magnitude, about 30%. Because the observed savings fall short of the engineering study, Levinson deems building codes as inefficient. However, in his analysis, Levinson indicates that there is potential sample selection bias because old and inefficient houses have already been deconstructed while the treatment group comprises new and old dwellings. Because building codes are mandatory, there is no issue with free-ridership. Levinson's study results are independent of the general market situation because the control and the treatment group is exposed to the market situation in a similar manner.

In contrast to Levinson, Jacobsen and Kotchen (2013) find that building codes work quite well. Jacobsen and Kotchen (2013) focus on the tightening of building codes in Florida in 2002. Comparing energy consumption in buildings constructed before and after the change in the regulation while controlling for a battery of socio-economic and building characteristics, they conclude that the regulation lowered electricity consumption by about 4% and natural gas consumption by 6%. In addition to the impact on energy consumption, the analysis also addresses the question over which time horizons the increased investment costs imposed by the new regulation can be recovered: between 3.4 and 6.5 years. As in Levinson's study, the command-and-control measure analyzed by Jacobsen and Kotchen (2013) may suffer from sample selection, but not from free-ridership or the market situation.

Analyses that focus on financial support measures are few and far between. There are, for example, some analyses of the CO2-Building Rehabilitation Programme from Germany that offers grants of up to 30,000 Euro per dwelling to owners who want to increase energy efficiency. Kuckshinrichs et al. (2010) employ an input-output model to approximate the effects of the program, while Gabriel and Balmert (2007); Clausnitzer (2009); Clausnitzer et al. (2010) pursue an engineering approach to approximate reductions in energy consumption. These are laudable endeavors, however, they are prone to overestimating the true effects. Rosenow and Galvin (2013) prepare a meta-analysis of existing evaluations and conclude that none take account of free-ridership.

Sample selection is also not addressed. Moreover, the applied identification strategies also do not allow to claim causal effects. While much attention is devoted to efficiency improvements on either a voluntary or a mandatory basis, there are also studies that focus on influencing consumption by interventions targeting behavior. A prominent empirical investigation is available from Allcott (2011). Other prominent examples are Allcott and Rogers (2014); Newell and Siikamaki (2014) and Allcott and Taubinsky.

9.6 Summary and conclusion

The aim of this piece is to illustrate problems in evaluating the causal effects of treatments intending to increase energy efficiency in the building sector. The necessary condition to identify causal effects is the choice of an appropriate identification strategy. It was shown that the often applied before-and-after comparison is prone to errors. In addition, it was discussed how to approximate the counterfactual situation which would have occurred had there been no intervention.

While an appropriate identification strategy is the necessary condition to unearth causal effects, the sufficient condition is that any empirical problem that introduces bias is adequately accounted for. These problems include free-ridership, sample selection, and the market situation. Free-ridership occurs when households benefit from financial support measures but would have engaged in efficiency measures in the absence of support mechanisms. The studies presented here indicate that free-ridership rates tend to be high. This implies that support for voluntary energy efficiency measures have little to no impact on energy efficiency improvements.

There are also different types of sample selection that introduce bias to the estimates of the effect of programs designed to increase energy efficiency in the building stock. For one, intrinsically motivated households are more likely to self-select into the voluntary programs. These households are systematically different from households randomly drawn from the general population. Thus, households who did not select into the treatment are a poor control group. This introduces bias into the estimates on the efficacy of measures to improve energy efficiency. In addition to the self-selection by households, there is also potential self-selection of sites where to evaluate efficiency programs. Programs who agree to a rigorous evaluation process are most likely more successful compared to programs that do not agree to robust evaluation.

Our literature review also indicates that evaluations of financial sup- port mechanisms for energy efficiency improvements are few and far be- tween compared to evaluations of command-and-control measures. Robust evaluations applying appropriate identification strategies seem unavailable. Consequently, the causal effects of these programs remain unclear.

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10. Work Package 3.2. An evaluation of the performance of energy efficiency measures and policies (lead partner: LSE)

Authors: Daire McCoy (LSE), Raphaela Kotsch (LSE)

10.1 Introduction

How much savings do energy efficiency measures actually deliver? Answering this seemingly straightforward question is crucial to understanding why households appear to under-invest in energy efficiency measures relative to what is socially or even privately optimal, the so-called "energy-efficiency gap".[19] However, there has been much debate on the size of this gap and the relative contribution of market failures, behavioural anomalies or model and measurement error. A recent paper by Gerarden et al. (2015) argues that the energy efficiency gap may not be as large as expected and that unobserved costs, overstated savings from adoption, consumer heterogeneity, inappropriate discount rates and uncertainty may all contribute to the low adoption rate not being as "paradoxical as it first appears". This is a problem that affects both individual incentives to invest and evaluations of policies aimed at encouraging adoption of energy efficiency measures, as overstated returns make private investments seem more attractive, and makes policies appear more cost-effective than they actually are. This issue is exacerbated by the fact that many policy evaluations rely on ex-ante engineering estimates of savings, and rarely consider behavioural responses.

A November 2017 Wall Street Journal op-ed by Sam Ori explored "Why Government energy-efficiency pro- grams sound great-but often don't work" (Ori, 2017). This piece focused on research which demonstrated that the upfront investment costs can be up to twice the actual savings, and the engineering estimates of energy savings can be more than three times what is actually realised (Fowlie et al., 2015). Other research by Allcott and Greenstone (2017) adds further evidence of overstated savings from ex-ante engineering estimates and also highlights large unobserved benefits and costs which evaluations tend to miss.

Evaluations of energy efficiency improvements tend to take a short time-scale, usually a window of 1-2 years on either side of the intervention in order to assess the magnitude of savings. This is despite the fact that time-scale has proven an important factor when examining the impact of building energy codes on energy con- sumption (Kotchen, 2017), and on the effect of behavioural interventions to reduce energy consumption (Allcott and Rogers, 2014). Treatment effects can vary over time due to a range of factors. This could also be the case for energy efficiency measures as

[19] The reluctance of some consumers to make energy saving investments that offer them seemingly positive net-present value (NPV) returns has been widely studied. For example see Hausman (1979); Blumstein et al. (1980); Jaffe and Stavins (1994); Golove and Eto (1996); Allcott and Greenstone (2012).

specific factors related to usage patterns in any particular period may bias results both before and after, while poor installation quality or degradation in the installed equipment may affect the results post-installation. Variation over time could affect the accuracy of measurement, the attractiveness of the investment, or the cost-effectiveness of a government scheme. Further, variations in energy prices both before and after the installation may affect both expectations and realisations of the investment's net-present value.

This research contributes by providing information on the persistence and heterogeneity of savings associated with installing energy efficiency measures. Uniquely, we demonstrate how the savings from measures change over time for different household types. Not only do households in more deprived areas experience lower energy savings, the savings erode more quickly over time - in some cases reducing by 50 percent within six years (for measures expected to last twice this amount of time). This result has important implications for improving our understanding of the investment incentives households face and also for improving our evaluations of energy efficiency policies.

In order to conduct this analysis we exploit an extremely large database of home energy efficiency upgrades and metered energy consumption2, covering over four million households and a period of eight years. By combining statistical matching and a range of panel econometric estimators we control for unobserved heterogeneity and selection into various government schemes which funded the upgrades. Another novel feature of this analysis is that our database covers the universe of households entering energy efficiency schemes administered by energy suppliers in the UK, thus reducing the potential for "site-selection bias" as identified by Allcott (2015).

The data allows us to examine the variation in performance depending on when measures were installed, how they perform over time; how this varies by dwelling and socioeconomic characteristics, and ultimately how this affects the cost-effectiveness of measures for different household types. Results indicate significant cross-sectional and temporal variation in energy savings, that the persistence of savings varies by the type of measure installed and the socioeconomic characteristics of the household. The measures are generally still NPV positive, and compare favourably with the cost-effectiveness of other initiatives, but the returns are much lower than expected. This research also raises concerns over distributional factors given how the costs of policies are subsequently levied on households.

The rest of the paper is organised as follows; Section 2 provides the context in which this analysis takes place; Section 3 the data; Section 4 describes the methodological approach employed and considerations under- taken; Section 5 outlines the results; Section 6 provides the results of robustness checks and sensitivity analysis; Section 7 provides a concluding discussion.

10.2 Background

The Supplier Obligation (SO), first introduced to the UK in 1994, has become the principal policy instrument for implementing energy efficiency improvements in the domestic sector in the UK (Rosenow, 2012). The Supplier Obligations are an example of a "Tradable White-Certificate" (TWC) scheme. These are regulatory mechanisms, employing a market-based approach to deliver energy savings. Theoretically they can be considered a hybrid subsidy-tax instrument, in which suppliers provide subsidies for energy efficiency upgrades that are then recovered through increased energy prices (Giraudet et al., 2012), having parallels with traditional demand-side management (DSM) programmes in that companies are required to invest in projects that ultimately reduce demand for their product (Sorrell et al., 2009b).

As outlined in Bertoldi and Rezessy (2008) and Giraudet et al. (2012), SOs have three main features: an obligation is placed on energy companies to achieve a quantified target of energy savings; savings are based on standardised ex-ante calculations; the obligations can be traded with other obligated parties. This flexibility ideally allows suppliers to choose the most cost-effective way to reach their target. Suppliers bear the cost of installations in the first instance, costs are then passed through to their entire population of customers through increases in energy prices (Chawla et al., 2013). Clearly, this may have distributional consequences if certain segments of the population are less likely to avail of the schemes. To alleviate this concern, targets were imposed regarding the proportion of savings to be achieved from lower income groups.

The former Department of Energy and Climate Change (DECC)[20], sets the savings targets which are then enforced by the energy regulator, the Office of Gas and Electricity Markets (Ofgem). Ofgem sets and administers individual savings targets for each energy supplier. Energy suppliers have various options to achieve their targets such as contracting installers, subsidising energy efficiency products, cooperating with local authorities, delivery agents or supermarkets, or directly working with their customers (Rosenow, 2012).

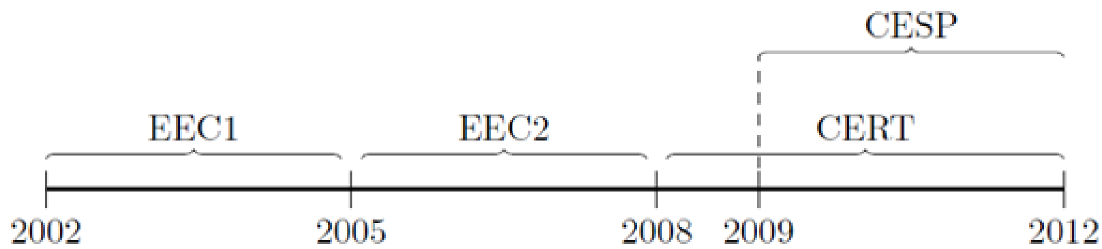

Figure 15: UK Energy Efficiency Programmes 2005-2012

Figure 1 gives an overview of SOs from 2002-2012. The first Energy Efficiency Commitment (EEC1) ran from 2002 to 2005, followed by EEC2 in 2005. In 2008, EEC2 was replaced by the Carbon Emissions Reduction Target (CERT) which ran until 2012. In 2009, the Community Energy Saving Programme (CESP) was introduced in parallel with CERT. While the main architecture of SOs did not change, the savings targets and the costs of the delivering the programmes increased over time. Rosenow (2012) provides a comprehensive overview of the main changes in each scheme from 1994

[20] now Department for Business, Energy and Industrial Strategy (BEIS)

- 2012 with regards to the target, the costs, social equity implications and other changes in design. The main change concerned the target size, increasing substantially in lifetime savings from 2.7 to 494 terawatt hours (TWh) between 1994 and 2012 (Rosenow, 2012).

From 2002, all programmes included a target for disadvantaged households and fuel poverty increasingly came to the fore. Eventually, CESP only allowed projects to be carried out in specific low-income areas of Britain, the lowest 10-15% of areas ranked in Income Domain of the Indices of Multiple Deprivation (Hough and Page, 2015). Thus, CESP was only available in certain geographical regions. Furthermore, CESP introduced a new bonus structure that incentivised the installation of multiple measures in a single dwelling and the treatment of as many dwellings as possible in the same area (Duffy, 2013). Table 1 summarises the key features of the schemes under consideration.

Table 1: Overview of Supplier Obligations

	ECC1	ECC2	CERT	CESP
Target	62 TWh	130 TWh	494 TWh	19.25 Mt CO2
Annual costs (millions)	167	400	1,158	unknown
% savings in priority group	50%	50%	40%	10-15% most deprived areas
Measures installed				
Cavity wall insulation	791,524	1,760,828	2,568,870	3,000
Loft insulations	754,741	1,780,302	3,897,324	23,503
Replacement heating system	366,488	2,018,812	31,986	42,898

Source: Based on information from Lees (2006, 2008); Rosenow (2012); Duffy (2013)

A key feature of all previous evaluations of the above policies is that the energy savings achieved were based on model ex-ante estimates and not actual ex-post data. Engineering model estimates tend to overstate actual savings significantly, as they are derived from lab-based estimates and factors such as occupancy and behaviour are typically not considered. This would lead to concern over the accuracy of measurement regarding both the energy savings achieved and the cost-effectiveness of various policies in delivering savings.

This work leverages an extremely large dataset of energy efficiency measures to shed new light on the savings delivered by the principal policy initiatives in the UK during the period 2002-2012.

10.3 Data

The National Energy Efficiency Database (NEED) contains dwelling-level data on four million households, over an eight-year period. Information comes from a range of sources including meter point electricity and gas consumption data, Valuation Office Agency (VOA) property attribute data, the Homes Energy Efficiency Database (HEED) containing data on energy efficiency measures installed, and modelled data provided by Experian on household characteristics. An overview of data types and sources is provided in Table 2.

Table 2: Data sources combined in NEED

Variable type	Source
Energy efficiency measures	HEED/Ofgem/DECC
Energy consumption	Energy Suppliers
Property attributes	V.O.A.
Household characteristics	Experian

Source: The Department of Business Energy and Industrial Strategy

The remainder of this section will discuss the measures installed, energy consumption and the socioeconomic characteristics of households. Further detail on all variables contained within the dataset is provided in Table A1 and descriptive statistics in Table A2. This includes detailed dwelling information.

10.3.1 Measures installed

The NEED database includes measures installed through EEC2, CERT and CESP schemes. These schemes were by far the most prevalent mechanism for delivering energy savings in residential dwellings in the UK over this period. The database does not include an exhaustive list of measures installed as part of the various schemes, appliances and lighting also featured but are not included. However, as Table 3 demonstrates, insulation and heating comprised the vast majority of estimated energy savings across various schemes over this period. In total over two million measures were installed over the period within our sample, this is graphically represented in Figure 2.

Table 3: Energy savings by scheme and measure

	EEC1 2002-2005	EEC2 2005-2008	CERT 2008-2012
Insulation	56%	75%	66.20%
Heating	9%	8%	8.20%
Lighting	24%	12%	17.30%
Appliances	11%	5%	5.90%
Other	-	-	2.40%

Source: Lees (2006, 2008); Ofgem (2013)

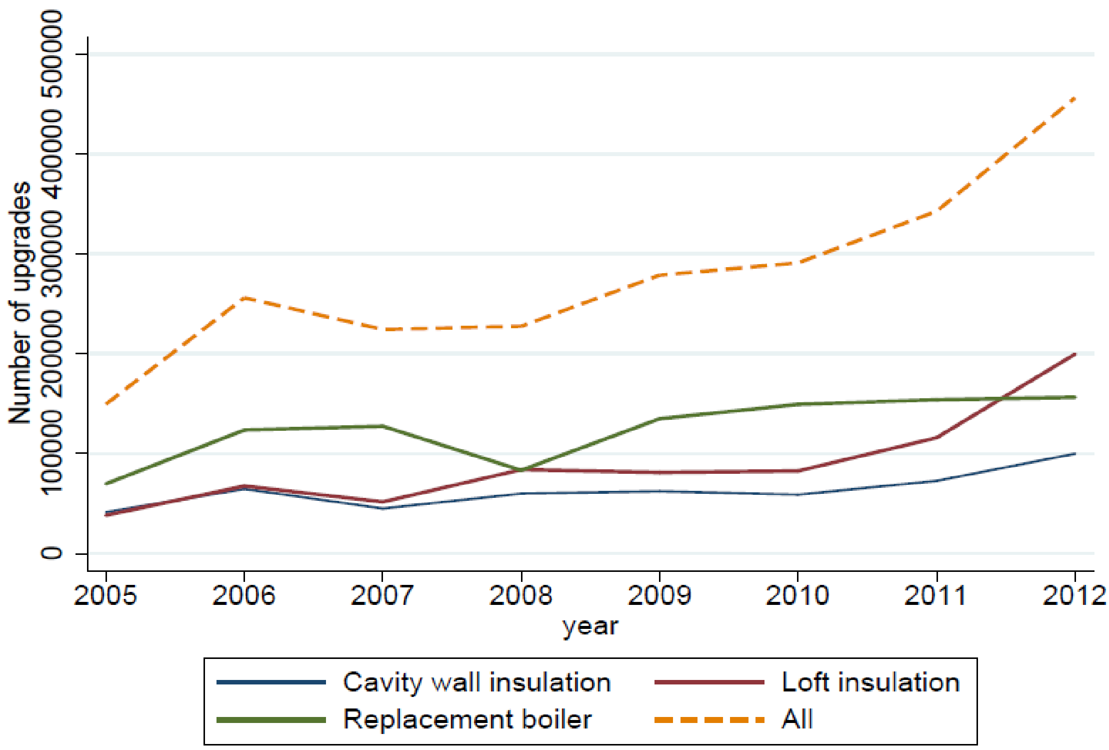

Figure 16: Energy efficiency measures installed, 2005-2012. Source: author's calculation based on NEED data

All insulation installations in our dataset were funded through government schemes. Heating upgrades were funded through both public and private means. In the early part of the sample (pre-2007) boiler installations were likely to have been funded through government schemes, however government support for replacement boilers was withdrawn during EEC2, as a combination of previous support schemes and new building regulations in 2005 had already delivered a significant penetration of new condensing boilers. Therefore, the boiler data we report on is a combination of publicly and privately funded investments. As specified in the 2005 Building Regulations, all replacement boilers were required to be condensing gas or oil and have a minimum efficiency rating of 86 percent.

10.3.2 Energy consumption

Figure 3 illustrates that on average, gas consumption reduced by 27% between 2005 and 2012 and electricity consumption reduced by 14%. Both of these trends are encouraging signs that the various polices in place over this period were having an effect.

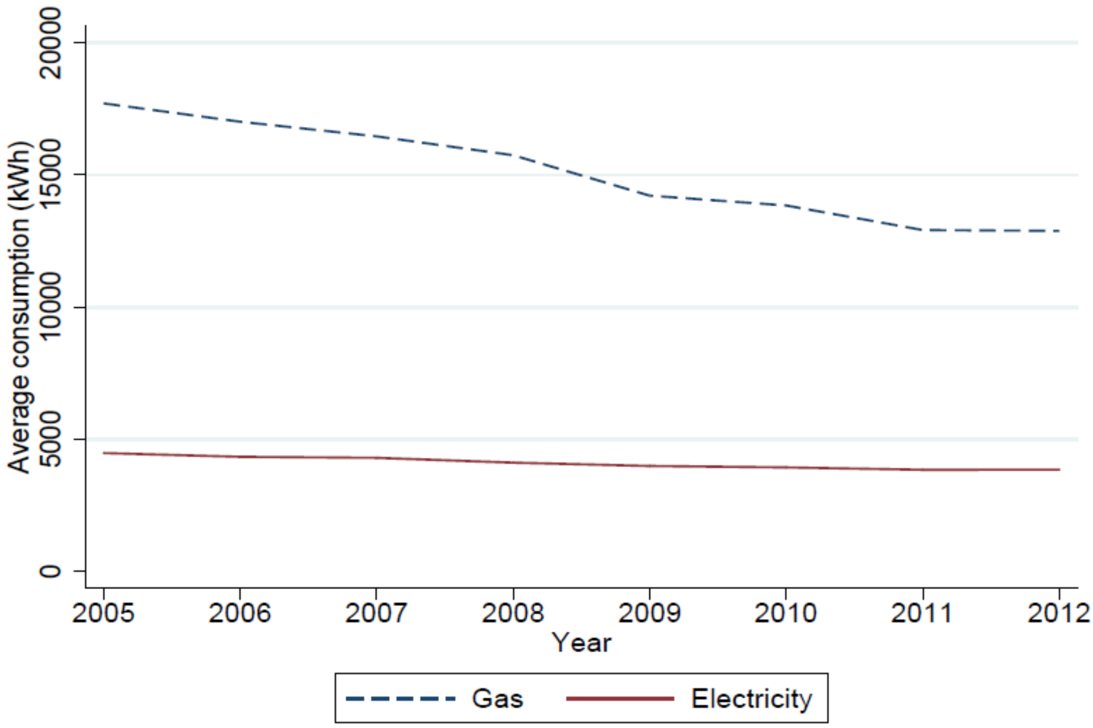

Figure 17: Average domestic energy consumption UK, 2005-2012. Source: author's calculation based on NEED data

10.3.3 Socioeconomic characteristics

The NEED dataset comprises information on household characteristics modelled by Experian and matched with indicators based on the geographic location of the property (DECC, 2016). For reasons of data protection, the dataset was anonymised and household-level information on variables such as income and tenure-type are not available. However, the dataset does include two composite indicators of the socio-economic background of the households.

1. **Index of multiple deprivation (IMD)**: NEED contains two variables describing IMDs: IMD 2010 for England and IMD 2011 for Wales. Both indicators classify Lower Layer Super Output Areas (LSOAs) according to a quintile ranking that is based on eight different domains that are incorporated using a weighting scheme. The first quintile (IMD=1) indicates the most deprived areas. Table 4 shows the composition of domains that are incorporated in the indicators and their weight in percent.
2. **Fuel poverty indicator (FP):** Combining data from the English Housing Survey and Census data, the fuel poverty indicator indicates if households are fuel poor based on the households' income and energy requirements, as well as on fuel prices (BEIS, 2013).

Table 4: Composition of IMD in %

	England 2010	Wales 2011
Income	22.5	23.5
Employment	22.5	23.5
Health	13.5	14
Education	13.5	14
Access/barriers to services	9.3	10
Living environment/ housing	9.3	5
Physical environment	0	5
Crime [Wales: Community Safety]	9.3	5

Source: Payne and Abel (2012); ONS (2011)

10.4 Econometric approach

10.4.1 The model

Energy consumption is determined by a range of factors such as temperature, characteristics of the dwelling and its inhabitants, and energy prices. We estimate the following baseline panel specification:

$$\ln(y_{it}) = \alpha_i + \gamma_t + \rho_{rt} + \delta \sum_{j=1}^{3} D_{ijt} + \propto \varepsilon_{it}$$

Where y_{it} denotes consumption of either electricity or natural gas (both in kWhs) by household *i* in year *t*, α_i is a household fixed-effect, γ_t is a year fixed-effect which controls for unobserved factors which vary at an annual level such as broader macroeconomic conditions and weather patterns, ρ_{rt} is a year-by-region fixed effect to control for factors which vary at a sub-national level, such as more localised economic shocks and weather patterns, D_{it} is the treatment dummy. The key parameter of interest is δ the average treatment effect on the treated (ATT). The model is estimated as a first-differenced fixed effects panel specification controlling for unobserved time-invariant household characteristics which might affect energy consumption. Over the course of the analysis, a variety of extensions to the above are estimated, to account for interactions between upgrades and household socioeconomic characteristics, and to examine the performance of upgrades over time. All models are estimated for both gas and electricity consumption. Standard errors are clustered at the household level in all specifications. As will be described in the following sections, the data allow us to create multiple treatment and control groups. Treatment groups are created for the entire sample period and for each individual year of upgrade. This allows us to examine how treatment effects vary over time.

10.4.2 Identification

10.4.2.1 The problem of unobserved heterogeneity

The fixed effects estimators described above are based on the assumption of conditional mean independence or unconfoundedness, selection on observables or ignorability (Caliendo and Kopeinig, 2005; Angrist and Pischke, 2009; Wooldridge, 2010), which requires that both of the following equations hold:

$$E\left[Y_{it}^{0} \middle| A_i, t, X_{it}, D_{it}\right] = E\left[Y_{it}^{0} \middle| A_i, t, X_{it}\right]$$

And

$$E\left[Y_{it}^{1} \middle| A_i, t, X_{it}, D_{it}\right] = E\left[Y_{it}^{1} \middle| A_i, t, X_{it}\right]$$

Thus, it assumes that Dit is strictly exogenous and as good as randomly assigned conditional on Ai (Angrist and Pischke, 2009). As we are primarily interested in the effect on the households who availed of the schemes - the average treatment effect on the treated (ATT), and not necessarily the effect on the whole population - the average treatment effect (ATE), the condition of unconfoundedness can be relaxed and equation (3) can be ignored. The parameter of interest is, the ATT is defined as:

$$ATT = E\left[Y_{it}^{1} - Y_{it}^{0} \middle| D_{it} = 1\right]$$

There is strong evidence that the presence of unobserved heterogeneity leads to inaccurate estimates of the ATE and ATT in a fixed-effects OLS setting (Ferraro and Miranda, 2017; Gibbons et al., 2014). Self-selection bias occurs as households voluntarily decide to apply upgrades in their homes or take part in government funded schemes, potentially causing the treatment and control group to differ systematically in aspects that both affect their likelihood of taking part in energy efficiency programs, and their energy consumption, causing the fail- ure of the conditional mean independence assumption (Wooldridge, 2010). Unobserved heterogeneity between households means that households respond differently to common shocks. For instance, increasing energy prices might lead to different behaviour of low- and high-income households. Second, the crucial assumption of a linear model with additive and homogeneous effects implies that the fixed effect estimates give a weighted average based on the frequency of groups as well as the sample variances within groups (Gibbons et al., 2014). This is problematic as the fixed effects estimator overweights groups that have larger variance of treatment conditional upon other covariates and underweights groups with smaller conditional variance if heterogenous treatment is prevalent (Ferraro and Miranda, 2017). One strategy to overcome this threat and to obtain consistent and unbiased estimators is to pre-process the data through statistical matching (Wooldridge, 2010). The following section outlines this approach.

10.4.3 Matching

Policy evaluations of secondary data typically employ statistical matching, along with differences-in-differences estimation, or exploit the longitudinal nature of the data with a panel fixed-effects specification. However, both of these measures may suffer from bias through either unobserved temporal effects or unobserved heterogeneity. Recent research has shown that by combining these methodologies, the accuracy of evaluations can approach that achieved by a randomised-controlled trial (RCT) (Ferraro and Miranda, 2017).

Coarsened-exact matching (CEM) is a non-parametric statistical procedure which improves the estimation of causal effects by reducing imbalance in observed variables between treatment and control groups (Iacus et al., 2008; Blackwell et al., 2009). Iacus et al. (2012) compare CEM with a range of other matching methods using Monte Carlo simulations and conclude that CEM has superior performance in terms of the bias and variance of the ATT. Alberini and Towe (2015) use a similar approach in an analysis of analysis of home energy audits in the state of Maryland.

In this case we are concerned with balancing the group that received the energy efficiency upgrades with the group that did not. By balancing all observed variables we can isolate the effect of the upgrade on energy consumption. Covariates on which the matching is performed should be predictors of household energy consumption and simultaneously impact the uptake of energy efficiency upgrades. The IMD of the area in which the household resides, provides information on the household's socioeconomic environment, an important predictor of energy consumption and energy efficiency uptakes. Hamilton et al. (2014) finds a strong relationship between the uptake rate of energy efficiency upgrades and neighbourhood income levels.

While more specific information on household socioeconomic characteristics, such as employment status, income and health are significant predictors of energy expenditures, they are found to have a smaller impact than that of dwelling characteristics and household size (Longhi, 2015). The period in which the dwelling was built has an important impact on residential energy consumption (Brounen et al., 2012; Harold et al., 2015). In order to account for regional differences in weather patterns, we include a variable reflecting the region in which the dwelling is located. Alberini and Towe (2015) provide evidence that matching solely based on dwelling or household characteristics is not sufficient and can be optimised if past energy usage is also included. By performing matching on energy consumption in prior years, we can account for unobservable household and property characteristics that might vary over time, such as the household size, composition and appliance usage. Taking into consideration all these factors, matching is performed on the following variables: property age, fuel-type, energy consumption in prior years, region and the IMD of the area in which the household resides.

10.4.3.1 Quality of matching

The quality of the matching process depends on the similarity in the distribution of covariates between treated and matched control group. This is commonly assessed by comparing the standardised difference and variance ratio of the variables in both groups, before and after matching (Caliendo and Kopeinig, 2005). The standardised difference is the difference in sample means in the treated and control group, divided by the corresponding sample variances. Formally:

$$d = \frac{\bar{x}_{treatment} - \bar{x}_{control}}{\sqrt{\frac{s^2{}_{treatment} + s^2{}_{control}}{2}}}$$

It allows for a comparison of balance which is independent of the sample size and measurement unit (Austin, 2009). The smaller the difference, the better, and it is recommended that this ratio should not exceed 10 percent (Austin, 2009).

The variance ratio measures the ratio of the mean variance in the treated and control group for each covariate.

Formally:

$$F = \frac{s^2{}_{treatment}}{s^2{}_{control}}$$

This should be close to unity (Austin, 2009; Ferraro and Miranda, 2017). A significant divergence from this indicates that the matching model is miss-specified. Further methods of balance diagnostic include assessing the magnitude of the difference between treatment and matched control group covariates using tests for statistical significance. However, the use of the t-test for balance testing is criticised for several reasons under which the most problematic is the dependence on the sample size. For instance, randomly discarding control units will always increase the balance, falsely indicating a better balance (Imai et al., 2008).

As can be demonstrated by Figure 4 and Tables B1 and B2 our extremely large sample size allows a high level of precision in matching. A high degree of balance is achieved on both variables used in matching and variables not used in matching as can be seen from the standardised differences, variance ratios and the distributions of matched electricity and gas consumption.

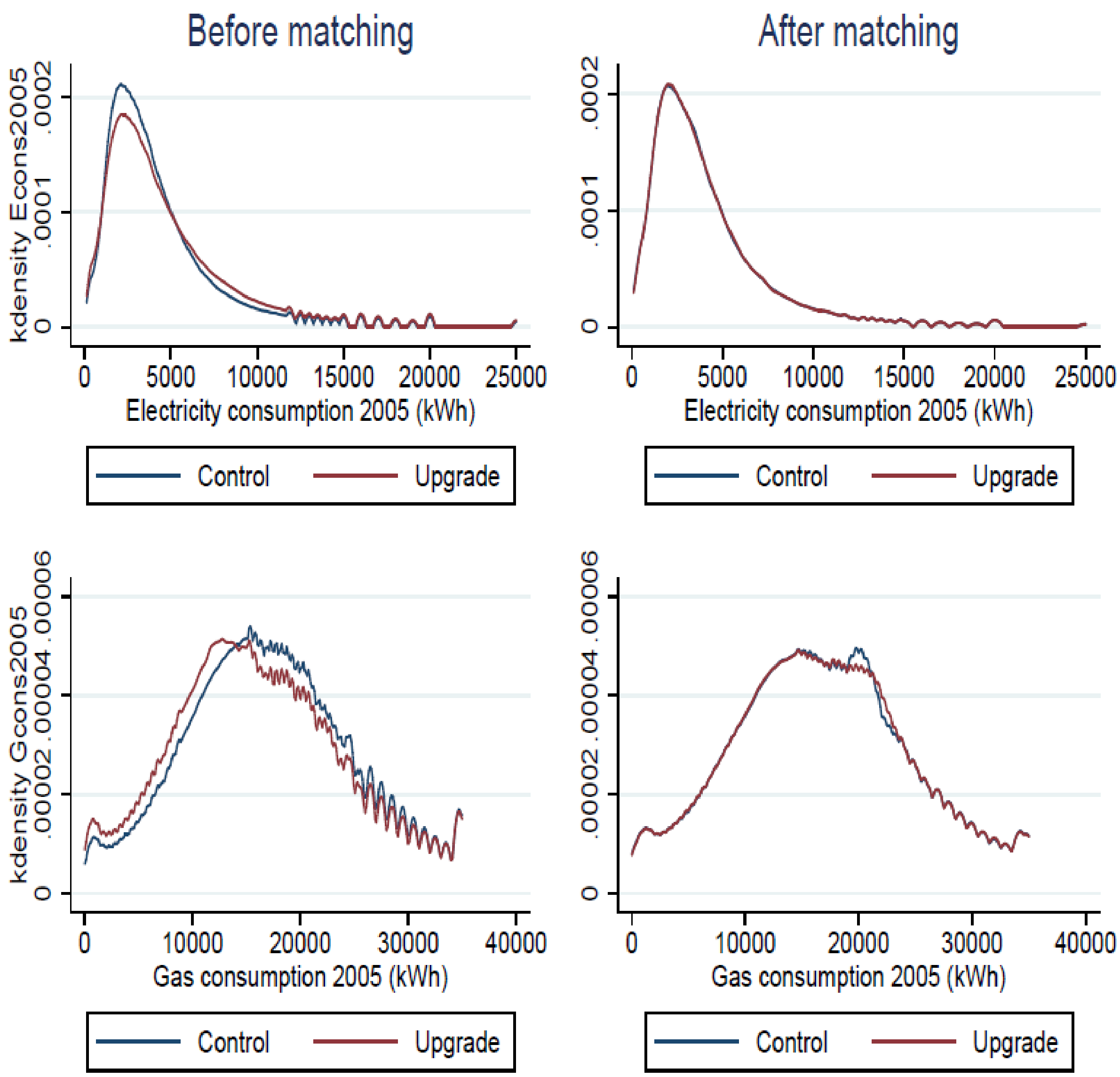

Figure 18: Energy consumption before and after matching. Source: author's calculation based on NEED data

Another important element in assessing the quality of matching is that the parallel paths assumption is not violated. This assumption states that without treatment, the average change for the treated group would have been equal to the observed average change in the control group. Figure 5 demonstrates that this assumption holds for all treatment and control groups.

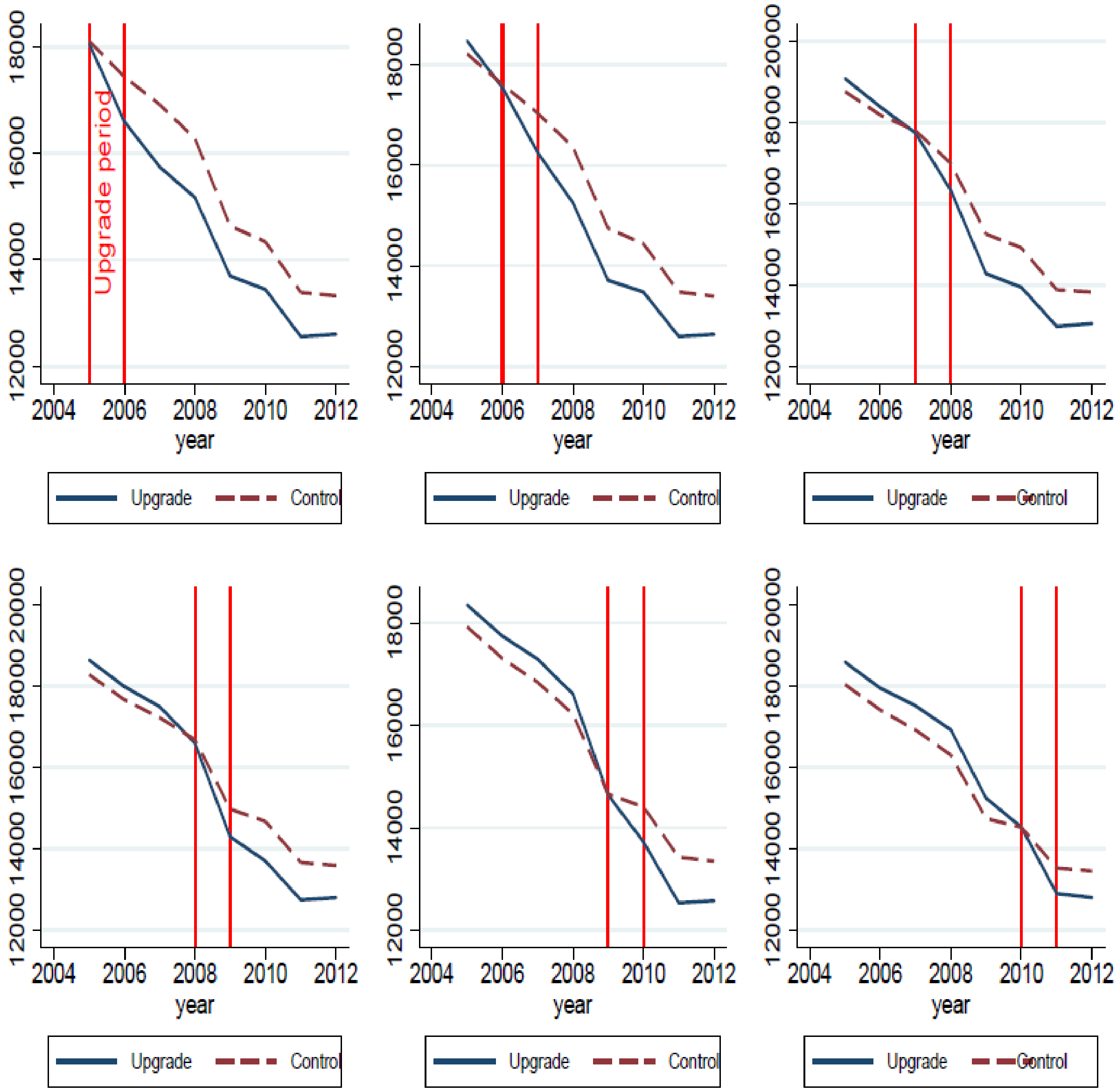

Figure 19: Energy consumption trend in upgrade and control group. Note: For each year in which an upgrade occurs we create separate upgrade and control groups. Source: author's calculation based on NEED data

10.5 Results

All reported results are estimates of the average treatment effect on the treated (ATT) and can be interpreted as percentage energy savings. Multiple upgrade and control groups are created for the entire period of analysis and for each individual year. This allows us to calculate the average effect and to examine trends over time. Analysis is restricted to households with electricity consumption between 100 and 25,000 kWh, and gas consumption between 3000 and 50,000 kWh. Outliers are excluded to minimise risk of inclusion of invalid consumption readings or non-domestic properties. Following this we create dummy variables to indicate if household energy (either electricity or gas) changed by more than 50, 60 or 70 percent in any given year. These dummy variables are then used in sensitivity analysis to control for any large changes which might have been the result of unobserved changes in occupancy. For comparison purposes we also calculate the energy savings in kWh for electricity and total energy (Table C1). As these measures primarily impact heating and natural gas is by far the main fuel used for heating the focus of our analysis will be on a subset of dwellings using natural gas for heating.

10.5.1 The effect of energy efficiency upgrades by year of upgrade

Table 5 shows that the energy savings over time are quite consistent for each measure, regardless of when the installation took place. Annual gas savings for cavity wall insulation range from 8-11 percent, loft insulation 2-3 percent, and replacement heating systems 8-10 percent. These results are consistent with Adan and Fuerst (2015) and Hamilton et al. (2016) who perform similar analysis on this dataset and other related data. This gives us confidence in the accuracy of our central estimates and we proceed on that basis.

Table 5: The effect of energy efficiency upgrades on energy consumption

	(1) Full sample	(2) 2006 upgrades	(3) 2007 upgrades	(4) 2008 upgrades	(5) 2009 upgrades	(6) 2010 upgrades	(7) 2011 upgrades
Cavity wall insulation	-0.094***	-0.097***	-0.111***	-0.099***	-0.098***	-0.097***	-0.101***
	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Loft insulation	-0.030***	-0.026***	-0.031***	-0.028***	-0.027***	-0.039***	-0.035***
	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Replacement boiler	-0.092***	-0.080***	-0.093***	-0.087***	-0.102***	-0.109***	-0.099***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Control variables	Y	Y	Y	Y	Y	Y	Y
Household fixed effects	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y
Year*region fixed effects	Y	Y	Y	Y	Y	Y	Y
Observations	5502936	617022	545627	564756	730447	746573	871379
Number of households	687,925	77128	68203	70595	91306	93322	108922
R squared	0.349	0.327	0.353	0.370	0.369	0.386	0.367

Notes: This table reports coefficient estimates and standard errors from eight separate regressions. The dependent variable in all regressions is the logarithm of annual gas consumption in kilowatt hours. Column(1) "All" denotes efficiency upgrades occuring at any time during the sample period. Columns (2-8) relate to upgrades occuring only in the relevant year. Each individual year denotes upgrades occuring solely in that year. For each upgrade group a matched control group is created using coarsened-exact matching. The sample includes billing records from 2005 to 2012. Standard errors are clustered at the household level. Triple asterisks denote statistical significance at the 1% level; Double asterisks at the 1% level; single asterisks at the 10% level.

10.5.2 Heterogeneity and persistence in returns to energy efficiency upgrades

10.5.2.1 By measure and IMD group

The next set of results, presented in Figure 6, show the interaction of the treatment variable with the variable indicating the socioeconomic characteristics (deprivation level) of the area in which the household resides. Energy savings are much greater for those households living in more affluent areas (IMD = 5), compared to those in less affluent areas (IMD = 1). This is true for all upgrade types. Combining all measures, the annual savings range from approximately 15 percent for those in the lowest IMD category to approximately 25 percent for those in the highest. This result raises concerns over distributional issues as the costs of these policies were likely applied as a flat-rate tariff on energy bills (Chawla et al., 2013). If savings are concentrated in the higher income groups, this suggest a further loading of policy costs onto those least able to afford it. Particularly as a flat-rate charge is already regressive, disproportionately affecting those on lower incomes.

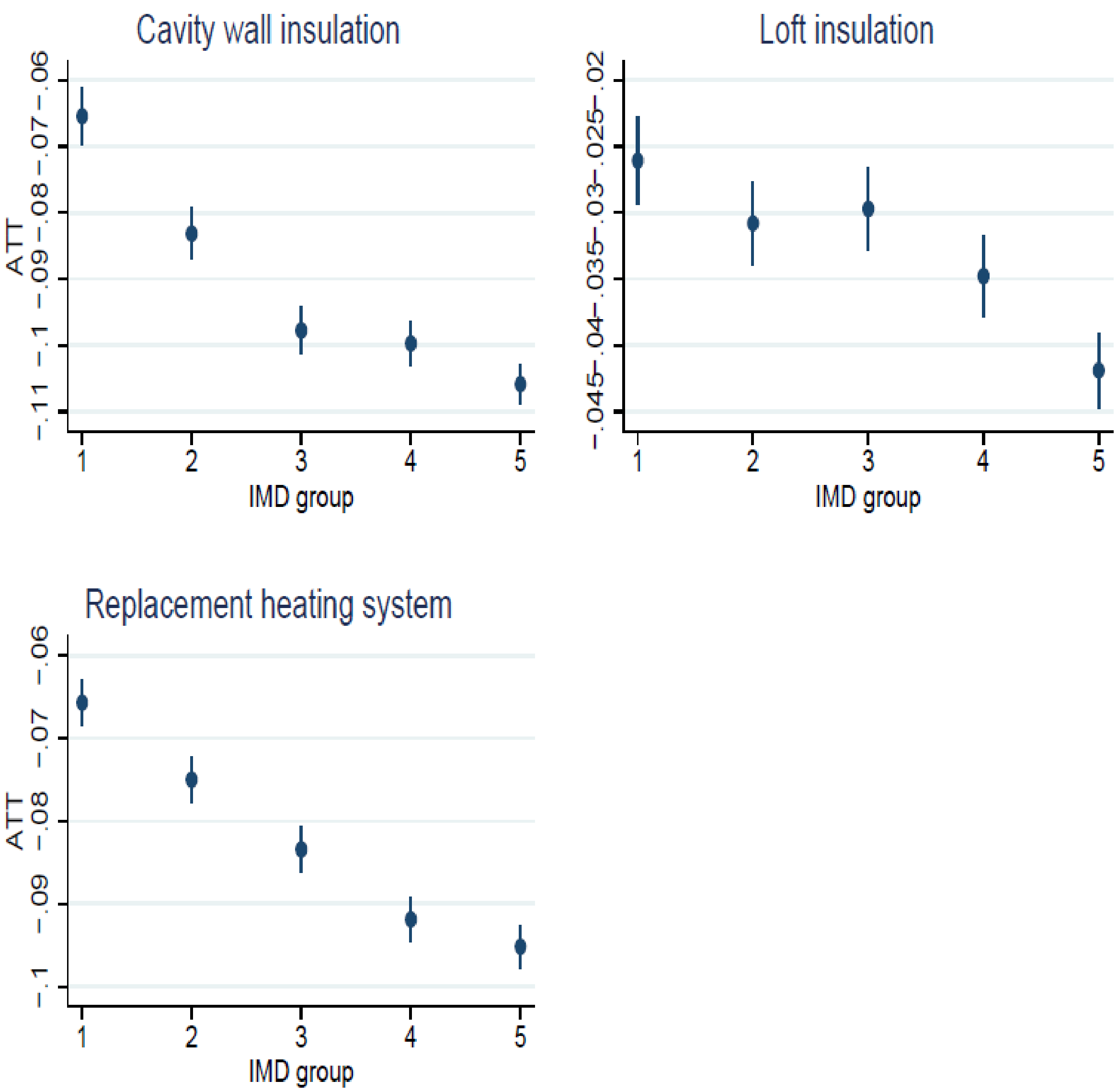

Figure 20: ATT for different IMD groups. Source: author's calculation based on NEED data

10.5.2.2 By measure and over time

A key novelty of this research is the ability to examine how measures perform over longer periods of time. Figure 7 presents results of estimations in which the treatment variable is interacted with the year variable to examine the persistence of savings over time. The reported results are for upgrades occurring between 2006 and 2007. This period is chosen as it allows a matched control group to be created using 2005 consumption level, and allows the analyst to observe the longest possible post-upgrade time series. Cavity wall and loft insulation show no clear time trend or degradation. This is not surprising as these measures are expected to last for 30-40 years (Dowson et al., 2012). However, for replacement heating systems the ATT shows a clear decreasing time path. This indicates that energy savings are greatest in the years immediately following installation and decreases thereafter. Given that the estimated lifespan for condensing gas boilers of this vintage was 12 years (Dowson et al., 2012), our results have implications for assessments of both household investment decisions and policy evaluations and require deeper analysis.

Figure 21: Persistence of ATT over time by measure. Source: author's calculation based on NEED data

10.5.2.3 Heating system replacements by IMD group over time

The reduction in savings could be due to degradation of equipment or a behavioural response from households. Changes to building regulations in 2005 mandated all replacement boilers to have a minimum of 86% efficiency. Other than this we do not have any detailed product characteristics. However, by decomposing this trend by socioeconomic group it is possible to examine whether this effect varies for different household types. Not only are energy savings less for those in lower income areas, the trend of decreasing savings over time is much more pronounced for these households. The savings for those is the lowest IMD group have halved within five years.

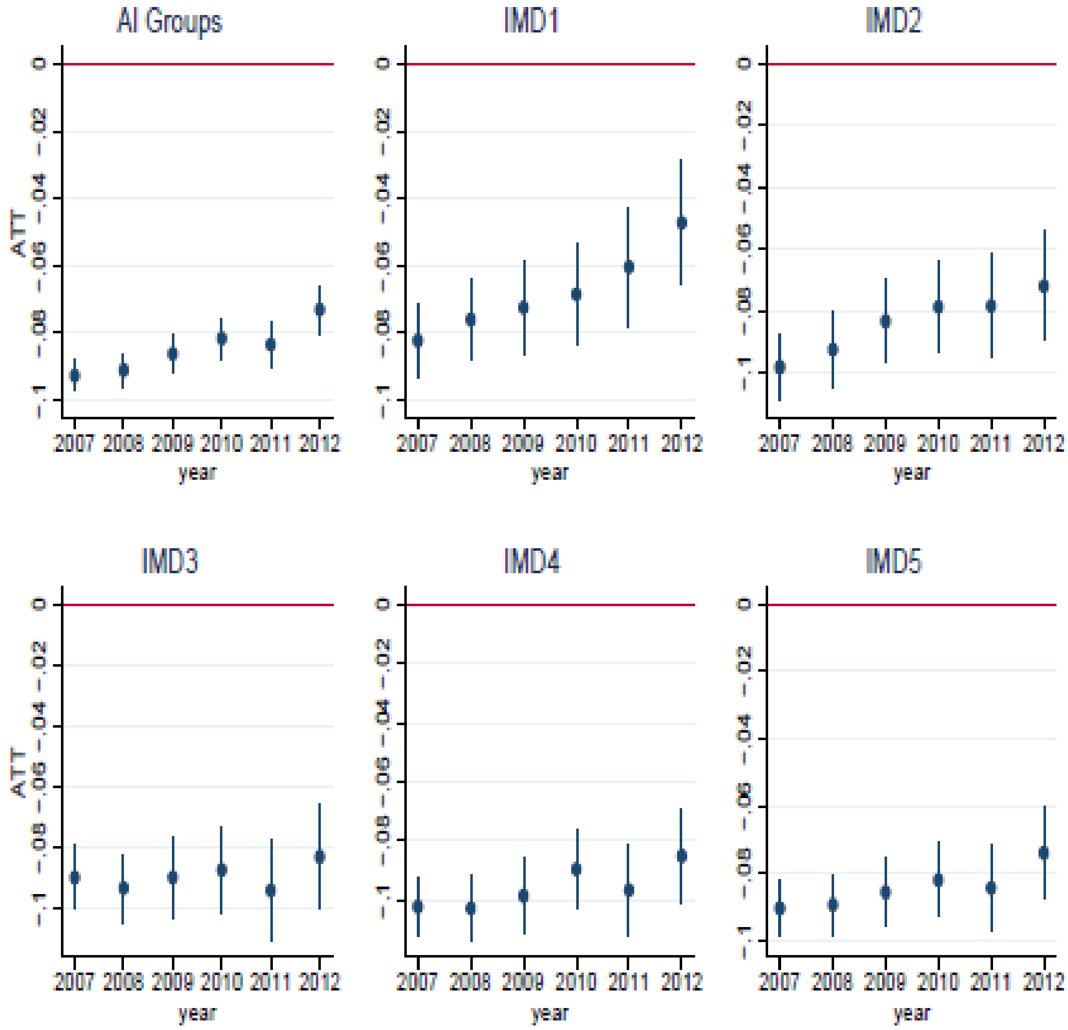

Figure 22: ATT for replacement heating system. Persistence over time and by IMD group. Source: author's calculation based on NEED data

It is not clear what exactly is causing this erosion of savings, particularly whether it is caused by a behavioural response or due to technical factors. The Home Energy Efficiency Database (HEED) sheds some light, however a number of data quality issues exist[21], making firm inference problematic. This database allows us to break down

[21] Not least the fact that a considerable proportion of heating control installations are in dwellings of "Unknown" tenure

heating system replacements funded through the supplier obligations by tenure type. Information is also provided on whether households had heating control systems installed along with their heating replacement. From Table 6 it is clear that a very low proportion of condensing boiler replacements were accompanied by installation of heating controls during this period. The proportion of households with heating system controls is much higher for "Owner Occupier" than for other categories, apart from "Unknown". Given that many more deprived households live in categories such as "Privately rented", "Rented from a housing association", "Rented from Local Authority" and "Social housing" it is quite possible that this is a contributing factor to the smaller and less persistent savings that these households experience.

Table 6: Heating system replacement and heating control installation 2002-2012

Heating system measure	Condensing Boiler	Condensing Boiler (Intelligent Controls)	Percentage with heating controls
Other tenure	1,001	8	0.80%
Owner Occupier	117,737	4,167	3.54%
Privately rented	33,020	102	0.31%
Rented from a housing association	11,272	22	0.20%
Rented from Local Authority	21,455	27	0.13%
Social housing	15,402	25	0.16%
Unknown	14,307	2,941	20.56%
Grand Total	214,194	7,292	3.40%

Source: Home Energy Efficiency Database (HEED). Energy Savings Trust

10.5.3 Cost effectiveness of measures

10.5.3.1 Estimated costs to suppliers and private costs of measures

Taking our estimates of both the ATT and the impact over time for different households we can develop more realistic assessments of the cost-effectiveness of measures than have been previously calculated. In order to do this, we need information on the costs. Cost estimates can be difficult to obtain and exhibit a wide degree of variation exists. To that end some assumptions must be made in order to make some back-of-the-envelope calculations. Usefully, a number of published academic and policy papers provide cost-estimates. The estimates we present in Table7 are based on a range of previous studies, outlined in more detail in TablesE1, E2 andE3. The costs we present are the costs incurred by the energy companies in installing each measure. These may understate the actual costs in some cases and can thus be considered a lower-bound. For example, the cost for replacement heating system used (£200) for past policy evaluations is the assumed additional cost of installing a high efficiency system, over and above a typical lower-efficiency system. We consider this estimate to be a lower bound. The Energy Savings Trust estimate the costs of boiler replacements to range from £700 to £6,000 (EST, 2013). On average the installation of condensing boiler costs around £2,400 per dwelling. In a 2015 review Frontier Economics assumed that the fixed cost of for gas-fired condensing boilers lie between £2,200-3,000 (Economics, 2015). For the purposes of comparison, we consider the upper bound for a replacement heating system installation to have been £2000 in 2006.

Table 7: Cost assumptions for each measure

Measure	Cost assumptions (£)
Cavity wall insulation	350
Loft insulation	285
Replacement boiler (policy cost)	200
Replacement boiler (private cost)	2000

Source: Based on Lees (2005, 2008), Shorrock (2005)

10.5.3.2 Internal rate of return (IRR)

The internal rate of return (IRR) on a project is the discount rate (r) that yields a net-present value of zero, or the discount rate at which the average value of avoided discounted future energy costs equals the upfront investment cost. Formally, this can be calculated using the below formula:

$$NPV = \sum_{t=1}^{T} \frac{C_t}{(1+r)^t} - C_0$$

Where T is the estimated lifespan of the measure, Ct are the avoided energy costs in year t, C0 is the upfront investment cost and r is the IRR which we solve for. The IRR is calculated based on the econometric estimates we observe, for varying estimated lifespans of measures and assuming constant future energy prices. While these measures were largely funded by the energy companies, it is useful to estimate the private returns as this would be a critical factor in determining uptake in the absence of such schemes.

Table 8 presents estimated IRRs for each measure, calculating the IRR for 10, 20 and 30 years. Our preferred estimated lifespan is 10 years for replacement heating system and 30 years for both types of insulation. Assuming a lifespan of 30 years or more, cavity wall insulation is an attractive investment yielding a return of 16 percent. Loft insulation is less attractive yielding 6 percent. Whether to invest in a heating system depends greatly on whether one uses the estimated policy cost or private cost. The low and highly negative returns in some cases suggest that there may not be much of an energy efficiency gap to explain regarding replacement heating systems.

Table 8: IRR for each measure

	Cavity wall	Loft	BoilerL	BoilerU
IRR 10	7%	-8%	17%	-24%
IRR 20	15%	4%	23%	-7%
IRR 30	16%	6%	23%	-2%

The next set of results, presented in Table 9 presents IRR estimates for each measure and each IMD group, along with the sample average for comparison purposes. A considerable degree of variation exists around the sample average, with households living in more deprived areas experiencing much lower returns than those in more affluent areas.

Table 9: IRR for each measure and IMD group

	Sample Average	IMD1	IMD2	IMD3	IMD4	IMD5
Cavity wall	16%	11%	14%	16%	17%	18%
Loft	6%	5%	5%	5%	7%	9%
BoilerL	17%	12%	14%	17%	20%	22%
BoilerU	-24%	-26%	-25%	-24%	-23%	-22%

The final set of IRR estimates we present, adjusts the future energy savings from a heating system replacement to correspond with the observed estimates in Figure 8. In this case, savings erode more quickly over time for households living in more deprived areas. Taking this in account results in a further reduction in the IRR for lower income households.

Table 10: IRR for each measure and IMD group adjusting for time-path of energy savings

	IMD1	IMD2	IMD3	IMD4	IMD5
BoilerL	6%	12%	16%	20%	19%
BoilerU	-29%	-26%	-23%	-21%	-22%

10.5.3.3 Other measures of cost-effectiveness

To broaden the perspective somewhat we also consider two other measures of cost-effectiveness: the cost per tonne of CO2 removed and the cost per kWh of energy saved. These are calculated at the sample average and allow a comparison of the overall cost of these policies with other similar initiatives. The estimated cost per kWh of energy saved is calculated by summing up the annual estimated savings over the expected lifetime of the measure. To calculate the cost per tonne of CO2 removed, we convert our kWh estimates based on the estimated CO2 produced in consuming one kWh of gas and electricity based on DEFRA/DECC greenhouse gas conversion factors. These are reported in Table 11.

Cavity wall insulation is the most cost-effective measure, followed by loft insulation and replacement heating systems. Relative to the estimated social cost of carbon and natural gas prices, insulation and the lower estimate for replacement heating systems seem relatively cost effective. However, at the upper bound of replacement heating system cost it does not represent an attractive investment.

Table 11: Cost-effectiveness of each measure

	GBP per tonne of CO2	GBP per kWh
Cavity wall insulation	36	0.0072
Loft insulation	90	0.0171
BoilerL	60	0.0141
BoilerU	600	0.1412

Converting the cost per kWh saved in 2000 GBP to 2015 USD it is possible to compare the cost of these interventions with a wide range of other initiatives, such as behavioural programmes, building code changes, subsidies and information provision. These are based on a recent review paper by Gillingham et al. (2018).

As demonstrated in Table 12, the measures we evaluate appear to have been quite cost-effective compared to a range of interventions funded by a range of policies such as behavioural interventions, building code changes, subsidies and information provision.

Table 12: International comparison of cost-effectiveness of energy efficiency interventions

Intervention type	Reference	Evaluation type	Relevant subset	Percent reduction in energy usage	Engineering estimates of percent reduction in energy usage	Cost effectiveness (cents per kWh saved, 2015 USD)
Behavioral programs	Allcott (2011)	RCT	NA	2		3.6
	Allcott & Rogers (2014)	RCT	One-shot intervention			4.4
			Two-year intervention			1.1 to 1.8
			Four-year intervention			1.2 to 1.8
	Ayres et al. (2012)	RCT	Sacramento, California	2		5.5
			Puget Sound, Washington	1.2		2
Building codes	Novan et al. (2017)c	RD analysis	NA	1.3	20	24.4
Efficient equipment or energy savings subsidy	Alberini & Towe (2015)	Matching	NA	5.3		3.9
	Alberini et al. (2016)	DID	Rebate of $1,000 or more	0		
			Rebate of $450	5.5		47.9
			Rebate of $300	6.2		28.2
	Burlig et al. (2017)	Machine learning	NA	2.9 to 4.5	11.6 to 18	
	Davis et al. (2014)	DID regression	Refrigerators	8		27.2
			Air conditioners	plus 1.7		4.5
Information provision	Alberini & Towe (2015)	Matching		5.5		
Supplier Obligation (TWC)	McCoy & Kotsch (2018)	Matching, FE regression	Cavity wall insulation	9.4	20.0	1.54 to 2.31
			Loft insulation	3	5.2	3.65 to 5.47
			Replacement heating system	9.2	24.9	3.02 to 30.19
Previous estimate of UK Supplier obligation Lees, 2008)						1.92

Adapted from Gillingham et al (2018)

10.6 Discussion and conclusions

The aim of this research was to estimate the extent to which heterogeneity and persistence effect the returns to commonly installed energy efficiency measures, and how this ultimately impacts household incentives and the cost-effectiveness of policies. By combining statistical matching and a range of panel econometric estimators we control for unobserved heterogeneity and selection into various government schemes which funded the upgrades. Leveraging an extremely large database of energy efficiency measures and metered consumption allows us to systematically explore heterogeneity. The database includes the universe of households entering into energy efficiency schemes administered by energy suppliers in the UK, mitigating site-selection bias.

Our results indicate that cavity wall insulation and heating system replacement (installation of a condensing gas boiler) result in an energy saving of about 10 percent of annual consumption, while loft insulation results in approximately a three percent reduction. These savings are consistent regardless of when the measures were installed over the sample period. Households living in more deprived areas observe less savings (both in absolute and percentage terms) than those in more affluent areas. This result is true for all measures examined. In addition to this, savings from heating system replacements erode quickly over time for the most deprived households but remain stable for more affluent households. As far as we are aware this result has not been shown before. It is not entirely clear what is causing this result. Due to changes in the UK building regulations in 2005, all boiler replacements we observe are required to be of 86% or higher efficiency. Therefore, this finding would not appear to be as a result of differences in quality of the system. However, heating system controls appear to have been installed less frequently for lower income households, and this could be a contributing factor. Condensing boilers require regular servicing. If servicing rates differed systematically across the population this might also have an impact on the results. Along with potential technical explanations, behavioural factors might also be driving some of this effect. We could be observing a delayed rebound effect as households become accustomed to warmer internal temperatures over time, and the reduced cost of heating services allow those previously income constrained households greater thermal comfort.

The results of this analysis provide both academic and policy insights. Our academic contribution is to provide new evidence on the performance of energy efficiency measures over time allowing us to better quantify the size of the energy-efficiency gap. As far as we are aware this is the first paper to examine longer run effects and how they vary by levels of household deprivation. This adds to a growing literature examining longer run effects of building energy codes (Kotchen, 2017) and information stimuli (Allcott and Rogers, 2014).

While we cannot identify the precise source of the savings erosion, we can quantify how it how it affects the financial return on investment. This provides important insights for both policy design and policy evaluation. Pay-as-you-save financing mechanisms are becoming increasingly popular for energy efficiency. For example, the Green Deal was a recent policy initiative in the UK (2011-2015) which provided households with loans in order to finance energy efficiency measures at interest rates of approximately eight percent. This was widely considered to have been a spectacular failure. The National Audit Office conducted an independent audit of the Green Deal scheme, finding that during it's lifespan the scheme only funded one percent of energy efficient measures installed nationally. It also found that the scheme saved negligible amounts of CO_2 and that households did not see the loans as an attractive proposition. Concerns were raised prior to the Green Deal policy that it would not have sufficient appeal for householders. These relate to a range of factors, including uncertainty regarding energy savings, limited financial appeal, and limited awareness of the scheme (Dowson et al., 2012). A key factor in limiting its appeal were the high rates of interest charged on loans (Rosenow and Eyre, 2016). Given the results we observe, it is clear that this rate is not sufficiently low to provide incentives for many households to partake in this scheme. In particular, low income households would actually lose money by making these improvements unless energy prices rise significantly. Market-based interventions will only work for certain segments of the population and policy needs to take this into account.

A question one might ask is why lower income households experience lower rates of return? A body of literature on the rebound effect identifies changes in energy service consumption that might reduce the expected savings (Sorrell et al., 2009a). This could be welfare enhancing if lower income households trade-off increased internal temperatures, resulting in improved well-being and even health outcomes, with lower energy savings. A focus of research should be to better quantify the health and well-being impacts of upgrades and their heterogeneity across social groups.[22]

Ultimately, this research helps to improve our understanding of the incentives faced by households when making energy efficiency investments, provides a methodology for better evaluating the cost-effectiveness of public policies and raises new concerns over the how the costs and benefits of policies are distributed. It also suggests that the energy-efficiency gap requires less explanation than some would suggest. At an individual household level, the private benefits of energy efficiency investments need to be re-considered with a greater focus on the non-financial benefits. While at a societal level a greater focus on carbon emissions reduction, as opposed to cost-savings is required.

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[22] For examples see Wilkinson et al. (2009); Hamilton et al. (2015) on health and Hills (2012); Watson and Maitre (2015) on fuel poverty

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