E2e Working Paper 028

Measuring the Welfare Effects of Residential Energy Efficiency Programs

Hunt Allcott and Michael Greenstone

April 2017

Measuring the Welfare Effects of Residential Energy Efficiency Programs

Hunt Allcott and Michael Greenstone*

April 4, 2017

Abstract

This paper sets out a framework to evaluate the welfare impacts of residential energy efficiency programs in the presence of imperfect information, behavioral biases, and externalities, then estimates key parameters using a 100,000-household field experiment. Several results run counter to conventional wisdom: we find no evidence of informational or behavioral failures thought to reduce program participation, there are large unobserved benefits and costs that traditional evaluations miss, and realized energy savings are only 58 percent of predictions. In the context of the model, the two programs we study reduce social welfare by \$0.18 per subsidy dollar spent, both because subsidies are not well-calibrated to currently-estimated externality damages and because of self-selection induced by subsidies that attract households whose participation generates low social value. However, the model predicts that perfectly-calibrated subsidies would increase welfare by \$2.53 per subsidy dollar, revealing the potential of energy efficiency programs.

JEL Codes: D12, L94, Q41, Q48.

Keywords: Energy efficiency, program evaluation, randomized control trials, welfare analysis.

In many settings, market failures such as externalities, transaction costs, and imperfect information provide opportunities for socially-beneficial policy intervention. In practice, however, it

*Allcott: New York University, NBER, JPAL, and E2e. Address: New York University Department of Economics; 19 W. 4th St., 6th Floor; New York, NY 10012. Email: hunt.allcott@nyu.edu. Greenstone: University of Chicago, NBER, JPAL, and E2e. Address: University of Chicago Department of Economics; 1126 E. 59th Street; Chicago, IL 60637. Email: mgreenst@uchicago.edu. We are grateful to Lucas Davis, Amy Finkelstein, Stephen Holland, Pat Kline, Chris Knittel, Nick Kuminoff, Erin Mansur, Magne Mogstad, Nick Muller, Devin Pope, Jim Sallee, Andy Yates, and seminar participants at the 2016 AEA Annual Meeting, Berkeley, Chicago, Georgetown, Harvard, Harvard Business School, the 2015 NBER Summer Institute, Stanford, UCLA, University of Connecticut, the University of North Carolina, and Yale for helpful feedback, to K.V.S. Vinay for dedicated research management, to Harshil Sahai for superb research assistance, to Monica Curtis, Amy Wollangk, and others at the Wisconsin Energy Conservation Corporation for implementation and technical information, to Michael Blasnik, Michael Heaney, Dale Hoffmeyer, Jane Peters, Greg Thomas, and Ed Vine for technical information, and to the MacArthur Foundation for financial support. Code to replicate the analysis is available from https://sites.google.com/site/allcott/research.

may be difficult to identify all relevant market failures, or policymakers may be constrained in their ability to implement the first-best policy. In these "second-best" settings, well-intentioned policies may not achieve their full potential, and in some cases may even reduce welfare. This underscores the importance of carefully measuring market failures and estimating how corrective policies affect behavior.

Energy efficiency policy is a natural example of these issues. Energy efficiency decisions are widely believed to be affected by at least two types of market failures. First, most energy use causes negative environmental externalities, so if Pigouvian taxes are unavailable, subsidizing energy conservation could be beneficial. Second, imperfect information, credit constraints, and behavioral biases might cause people to forgo privately-beneficial energy efficiency investments.[1] Indeed, McKinsey (2009) argues that adoption of currently-available, cost-effective energy efficiency investments in the U.S. could generate $700 billion in net private cost savings. In this sense, energy efficiency closely resembles many other settings where consumers and firms appear reticent to make seemingly-beneficial investments, such as better business management, migration to places with higher wages, fertilizer and high-yielding variety seeds, and preventive health care.[2]

Motivated by these potential market failures, the U.S. and other countries have dramatically expanded energy efficiency policy in recent years.[3] Indeed, virtually all credible climate change mitigation plans assign a key role to energy efficiency in reducing greenhouse gas emissions.[4] However, many energy efficiency policies are explicitly second-best: for example, subsidizing energy efficiency instead of directly pricing externalities or providing information to directly address possible information imperfections. This opens the door to unintended distortions. Furthermore, the conventional program evaluation approaches used in practice may not be well-suited to identify these unintended distortions because they rely on accounting-style approaches instead of empirically-grounded economic models.

This paper formalizes an approach to modeling home energy efficiency investment decisions and uses this framework to evaluate two large energy efficiency programs in Wisconsin. Like many residential energy efficiency programs nationwide, these programs involved a two-step process. First,

[1] These privately-beneficial but unadopted investments are often referred to as an "Energy Efficiency Gap," and they have motivated significant interest in policy circles. For example, the Alliance to Save Energy (2013) writes that "energy efficiency is increasingly recognized as the lowest cost, most abundant and cleanest "source" of energy," offering "a win-win solution for economically and environmentally sustainable growth for America." This idea dates at least to the 1970s: Yergin (1979) writes that "If the United States were to make a serious commitment to conservation, it might well consume 30 to 40 percent less energy than it now does, and still enjoy the same or an even higher standard of living ... Overcoming [the barriers] requires a government policy that champions conservation ..."

[2] See Bloom *et al.* (2013), Bryan, Chowdhury, and Mobarak (2014), Duflo, Kremer, and Robinson (2011), Dupas (2014), Dupas and Robinson (2013), Foster and Rosenzweig (2010), Suri (2011), and many others.

[3] For example, the 2009 economic stimulus provided $17 billion for energy efficiency (Allcott and Greenstone 2012). Utility "demand-side management" expenditures grew 80 percent between 2008 and 2013 (CEE 2013, 2015). Twenty-six states now have Energy Efficiency Portfolio Standards, which require utilities to run energy efficiency programs.

[4] For example, the U.S. Energy Information Administration assumes that efficiency will account for 42 percent of CO2 reductions as part of one of its key mitigation plans (EIA 2015). The International Energy Agency (2015) projects that efficiency measures will reduce global demand growth to 2040 by 40 percent. The Clean Power Plan, the Obama Administration's flagship climate change policy, includes substantial opportunity for compliance through energy efficiency.

homeowners decide whether or not to have a home energy audit, during which they learn about a set of recommended energy efficiency investments, such as improved insulation or new heating systems. Second, they decide which of the recommended investments to undertake. The programs subsidized both audits and investments. Our model captures this two-step process, allowing both observed monetary returns and unobserved benefits and costs to affect takeup. The model accommodates the two key types of market failures introduced above: audit and investment takeup distortions such as imperfect information and behavioral biases, and uninternalized externalities that distort retail energy prices.

The model is estimated using the Wisconsin programs' administrative data, including costs and predicted energy savings for all investments recommended during the audit, paired with a 100,000-household randomized experiment. By varying the content of promotional letters across households, the experiment solves two key identification problems. First, randomly-assigned audit subsidies serve as an instrument to identify self-selection – that is, the correlation between unobservables in the audit and investment takeup decisions. Second, randomly-assigned informational and behavioral treatments identify the magnitude of six specific informational and behavioral distortions thought to reduce audit takeup.

Even before quantifying welfare effects, the program evaluation process generates several important empirical results. First, in the randomized experiment, there is no evidence of the hypothesized informational or behavioral failures. Within the letter variations, only price mattered: while a $100 audit subsidy increased takeup by 32 percent relative to control, all six informational and behavioral variations had statistically and economically insignificant effects.[5] This suggests that the informational and behavioral factors we tested are not barriers to takeup, although additional tests would be valuable. If this finding holds beyond the Wisconsin programs, this would undermine the basis for energy efficiency programs to subsidize home energy audits.

Second, in addition to observed monetary benefits and costs, energy efficiency investments entail large unobserved benefits and costs. Non-experimental investment takeup estimates imply that households that had audits were willing to pay an average of $330 for the unobserved attributes of a recommended investment, perhaps due to "warm glow" from contributing to externality reduction or from the improved comfort of a weatherized home. Furthermore, post-audit investment takeup was remarkably inelastic to monetary benefits and costs: consumers did not take up 40 percent of investments with private internal rates of return (IRRs) greater than 20 percent, and they did take up 36 percent of investments with negative private IRRs. This inelasticity implies that consumers perceive a wide dispersion in unobserved benefits and costs. These results highlight the importance of using revealed preference approaches to welfare analyses, instead of conventional accounting approaches that consider only observed monetary factors.

[5] Our results are especially important in light of Bertrand *et al.* (2010), Coffman, Featherstone, and Kessler (2013), Bhargava and Manoli (2015), and other studies showing that subtle variations of informational or advertising content can substantially affect behavior. Given our large samples, our zero statistical effects are not due to lack of precision, and we bound the effects of the non-price treatments at about one-quarter the size of the effects in Bertrand *et al.* (2010), after adjusting for income differences between South Africa and the U.S.

Third, we find strong experimental evidence of self-selection effects in program participation. Specifically, consumers that were marginal to our experimental audit subsidies were much less likely to invest than those that were inframarginal. This highlights one potential problem with policy proposals to expand residential energy efficiency programs by increasing audit subsidies: while additional households may be induced to audit, these marginal households could be much less interested in making subsequent investments, and thus would generate less externality reduction. This underscores the importance of more complete models of behavior when designing second-best policies. More broadly, this self-selection phenomenon arises in many settings. For example, trading off selection effects vs. the value of experimentation, should we subsidize college enrollment or college completion (Manski 1988, Altonji 1993)? When a job training program expands to enroll more people, are the marginal participants more or less likely to eventually find a job? Our design provides a rare opportunity to identify such selection effects using experimental variation.

Fourth, we estimate that realized energy savings fell well short of predictions. Specifically, the programs' simulation models predicted that the average household that had an audit made investments that would save \$153 per year at retail prices, or about 8.5 percent of baseline energy expenditures. In contrast, we estimate an average savings of \$89 per year, implying a "realization rate" of 58 percent. The shortfall cannot be explained by temporary weather patterns and is far too large to be caused by a "rebound effect" (i.e. increased utilization in response to the decreased cost of energy services). Identifying potential inaccuracies in simulation predictions is crucial because these predictions are given to consumers during audits (to help decide whether to make different investments) and to regulators and policymakers (to help decide whether to keep funding energy efficiency programs). This result, along with a similar result from Fowlie, Greenstone, and Wolfram (2015b), provides important new evidence on the urgency of this issue.[6]

With these results in hand, we turn to welfare evaluation and counterfactual policy analysis. In the model, the optimal investment subsidies would exactly equal the reduction in uninternalized externalities. For example, if an insulation improvement is projected to reduce climate damages and local air pollution by a present value of \$500 more than is internalized into retail energy prices, the optimal subsidy would be \$500. By contrast, the programs subsidized each investment by a round number multiplier on the share of the household's energy use that would be conserved. This generates three types of distortions. First, the program subsidies favored small households, where a given investment reduces a larger share of energy use. Second, the round number multiplier that the programs chose tended to be too generous relative to our estimate of the uninternalized externality reduction. Third, different investments reduce natural gas, electricity, and fuel oil in

[6] This paper's focus and setting differ from Fowlie, Greenstone, and Wolfram (2015b) in important ways. They study a different program, the Weatherization Assistance Program, which provides energy efficiency retrofits to low-income households at no cost. By contrast, the programs we study are open to households of all incomes and require participants to pay a meaningful share of costs. Furthermore, Fowlie, Greenstone, and Wolfram (2015b) focus on estimating energy use and "rebound" effects, while the core of our paper is the theoretical framework, demand estimation, and revealed preference welfare analysis. Finally, the field experiments differ: Fowlie, Greenstone, and Wolfram's (2015b) field experiment is designed to generate a first stage for estimating the effect of weatherization on energy use, whereas our field experiment is designed to identify self-selection as well as informational and behavioral market failures.

different proportions, and these different fuels have vastly different uninternalized externalities, so subsidizing energy savings favors investments that reduce energy use but don't necessarily reduce uninternalized externalities.

These distortions may seem subtle, but they turn out to make a very big difference. Using current standard estimates of uninternalized pollution damages based on a $39 social cost of carbon and local air pollution damages from Holland, Mansur, Muller, and Yates (2015), the model predicts that a program with perfectly-calibrated subsidies would increase welfare by $2.53 per subsidy dollar. By contrast, the model suggests that the program subsidies are so mis-targeted relative to our market failure estimates that they reduce welfare by $0.18 per subsidy dollar. The welfare estimates also highlight the consequences of the self-selection effects discussed above: counterfactual increases in audit subsidies generate less and less externality reduction per program dollar, because they induce audits by households that are increasingly unlikely to make energy efficiency investments. Taken together, these results highlights how potentially-beneficial public programs can leave large social welfare gains on the table when they rely on heuristic judgments about the form of market failures (e.g. that energy use *per se* is a problem, instead of measuring the uninternalized externalities that vary by fuel) and do not account for empirical factors that govern individual behavior (e.g. self-selection).

As a benchmark, the paper also presents welfare estimates using the conventional "accounting approach." These estimates suggest that the upfront audit and investment costs exceed the present value of reductions in energy, local air pollution, and greenhouse gases. Specifically, the social internal rate of return (including externality reductions) of investments made through the programs is negative 4.1 percent using the empirical estimates of energy savings. To help address the question of whether these results generalize outside the two Wisconsin programs, Appendix E presents a parallel analysis using data from 37 Better Buildings program sites nationwide. We find that the national programs had slightly worse IRRs than the Wisconsin programs.

This paper makes two primary contributions to the literature. First, it sets out a theoretical framework to evaluate the welfare consequences of residential energy efficiency programs in the presence of imperfect information, behavioral biases, and externalities, and shows how to estimate the key parameters using a field experiment. This approach is a departure from conventional approaches to evaluating energy efficiency programs that exclusively consider monetary net benefits, and it underscores that standard demand estimation, welfare analysis, and counterfactual simulation can be used in this setting. While researchers have long been interested to include non-monetary factors when evaluating home energy efficiency programs (see Skumatz (2008) for a list of 45 "non-energy benefits" studies), this paper demonstrates how they can be measured through standard revealed preference techniques.

Second, the bulk of the above empirical results run counter to the conventional wisdom among policymakers and practitioners about energy efficiency programs. While McKinsey (2009) and similar studies suggest that energy efficiency programs could generate large private and social benefits, the results imply that at least for the programs we study, this is not the case. This highlights the importance of additional peer-reviewed research and connects to literature in other domains suggesting that low adoption of apparently-beneficial technologies may be due to overestimated private benefits, not market failures.[7] At the same time, the results suggest that it is feasible to design socially desirable energy efficiency programs, but this may require more precise targeting of policies to market failures and more empirical knowledge of the parameters governing consumer behavior.

The paper proceeds as follows. Section I provides an overview of nationwide energy efficiency programs and our case study. Section II presents our theoretical framework. Sections III and IV detail the experimental design and data, while Sections V and VI present the empirical strategy and results for audit and investment takeup. Section VII estimates effects on energy use, Section VIII presents the welfare analyses, and Section IX concludes.

I Overview: A Case Study of Nationwide Programs

We focus on two energy efficiency programs in Wisconsin that were part of the national Better Buildings Neighborhood Program (BBNP). We begin by giving an overview of related programs, followed by more detail on the Wisconsin case study.

I.A Overview of Federal and State Energy Efficiency Programs

The programs we study facilitate and subsidize energy audits and energy efficiency investments, such as improved insulation and heating and cooling systems, at existing homes. Panel A of Table 1 gives an overview of related programs.[8] As shown in column 1, the Better Buildings Neighborhood Program (BBNP) ran from 2010-2013, facilitating approximately 119,000 energy efficiency retrofits, mostly at residential buildings. BBNP allocated $508 million through competitive grants to 41 state and local agencies, including the Wisconsin programs we study. Most of the $508 million funding came through the Energy Efficiency and Conservation Block Grant (EECBG) program shown in column 2. EECBG was established through the Energy Independence and Security Act of 2007, and it was allocated $3.2 billion through the 2009 American Recovery and Investment Act. Similar to BBNP, EECBG's stated goals were reducing energy costs, reducing carbon emissions, and job creation.

In addition to the stimulus-related programs, there are also longer-running "demand-side management" (DSM) programs, such as Wisconsin's statewide Focus on Energy program, to help residential, commercial, and industrial utility customers save energy. Column 3 shows that in 2013, 347 US and Canadian DSM programs spent $8.0 billion, and program participants saved an estimated $2.95 billion in that year. Finally, there are means-tested energy efficiency programs available only

[7] See Hanna, Duflo, and Greenstone (2016) on cookstoves, for example.

[8] The public expenditures and energy savings in this table are included only to give a sense of program magnitudes, not as a cost-benefit analysis. Public expenditures do not include any consumer investment costs, and value of predicted energy saved is based on simulation models with varying energy price assumptions.

to low-income consumers, the most important of which is the Weatherization Assistance Program shown in column 4. Fowlie, Greenstone, and Wolfram (2015a, 2015b) study that program.

Because these programs are either administered by the government or overseen by regulators, many program evaluation reports have been written: Billingsley *et al.* (2014) identify 4,200 evaluations of DSM programs alone. The standard evaluation uses a straightforward "accounting approach": compare the observed investment costs to the present discounted value of energy savings. Panel B of Table 1 presents some common assumptions that these evaluations make, based on a survey by the American Council for an Energy Efficient Economy (Kushler, Nowak, and Witte 2012). Only 30 percent of programs include benefits other than reduced energy use, and we are not aware of any that measure non-monetary investment costs. Nearly all programs use simulation predictions instead of empirical analysis to estimate energy savings, of which 70 percent use simulation predictions from states other than the state where the program was implemented. More than four out of five do not use empirical analysis to retroactively evaluate programs. Our paper shows how these assumptions can be relaxed and documents the importance of doing so.

These program evaluation assumptions are particularly relevant given the U.S. Environmental Protection Agency's proposed Clean Power Plan. The proposed plan allows states substantial leeway in how to comply, and it would allow compliance through energy efficiency programs and other mechanisms instead of cap-and-trade programs. Indeed, the National Association of State Energy Officials (2015) believes that "energy efficiency programs ... likely offer the most cost effective means for compliance under the pending EPA rule," and the American Council for an Energy Efficient Economy (2015) has found that "rapidly deployable energy efficiency policies can achieve nearly 70% of EPA's required greenhouse gas emissions by 2030." Compliance through energy efficiency could result in more (less) greenhouse gas abatement than expected under the Clean Power Plan if savings are evaluated through approaches that tend to understate (overstate) energy savings. Similarly, the Clean Power Plan's overall welfare effects would depend on the welfare effects of policies that the states choose to implement.

I.B The Madison and Milwaukee Programs

We study the Green Madison (GM) and Milwaukee Energy Efficiency (Me2) programs, which were operated jointly but branded separately in each city. The programs were managed by the Wisconsin Energy Conservation Corporation (WECC), a well-respected and highly professionalized program implementer, and they built on the existing design and infrastructure of the Wisconsin Focus on Energy home energy efficiency program. The two programs received part of a $20 million Wisconsin BBNP grant. They were wound down after stimulus funds were exhausted in late 2013, although similar programs continue in Wisconsin and around the country.

From a homeowner's perspective, program participation involved two steps. The first step had three sub-parts. First, a homeowner would schedule a free informational visit by an "Energy Advocate" to explain the program and discuss low-cost conservation opportunities. Second was a home energy audit by an "Energy Consultant," a state-certified independent contractor. During the audit, the Energy Consultant would often put in "direct install measures," primarily CFLs and faucet and shower aerators, at no cost to the homeowner. At the end of the audit, the Energy Consultant would provide an "audit report" with a list of recommended energy efficiency investments, including projected upfront costs, simulation predictions of annual energy cost savings, payback period, and lifetime energy savings for each investment. See Appendix A for an example audit report. Third, homeowners who were interested in making investments would schedule an initial visit by a program-certified contractor to provide a formal cost estimate. In the model below, we think of these three sub-parts collectively as the "audit" step.

The second step was for a contractor to actually perform the work in the consumer's home. In some cases multiple contractors were required for different type of work, for example one for insulation and one for HVAC. After the work was complete, the Energy Consultant would return for a "post-test" to verify that the contractor had done the work properly.

Many residential retrofit programs have a similar structure. While the programs try to make participation as easy as possible, it is clear that both audits and investments require consumers' time and effort as well as money. These time and effort costs represent part of the non-monetary attributes in our model.

To predict energy savings, the Wisconsin programs used a simulation model called the Targeted Retrofit Energy Analysis Tool (TREAT). TREAT is one of the important models used by energy efficiency programs nationwide, and 28 percent of audits in the national Better Buildings Neighborhood Program data used TREAT. TREAT has repeatedly satisfied Department of Energy validation protocols, "in which results from software programs are compared to results from other software programs" (PSD 2015a). This suggests that any differences between predicted and empirically estimated savings might not be limited to this software and to the Wisconsin programs.

The programs offered large investment subsidies. The bulk of payments were tiered subsidies of $1000, $1500, and $2000, for a homeowner making investments projected to save 15-24, 25-34, or more than 35 percent of energy use, respectively. There were also subsidies for correcting health and safety issues, doing air infiltration tests, "completion bonuses" for finishing projects before particular dates, and a means-tested subsidy for completing a large "Home Performance" retrofit. Appendix Table A.1 presents a breakdown of subsidies paid.

Program participants were also eligible for loans at 4.5 to 5.25 percent interest from a local credit union of $2,500 to $20,000 (up to 100 percent of installation costs), with terms from 3-10 years. While most people in Madison and Milwaukee probably did not know about this opportunity if they did not have an energy audit, the Energy Advocates and Energy Consultants would discuss financing opportunities during the audits, and the audit reports gave financing information in several places. Thus, for people who have had audits, credit constraints should not be a major barrier to takeup.

II Model

Paralleling the program structure detailed above, we model consumers in a two-step process of audit and investment decisions. We allow for three classes of market failures that motivate energy efficiency policy, as described in overview articles by Allcott and Greenstone (2012), Jaffe and Stavins (1994), and Gillingham, Newell, and Palmer (2009). First, imperfect information or behavioral barriers might distort consumers' decisions about whether to have an audit. Second, similar distortions could affect investment decisions. Third, environmental externalities and other distortions cause an investment's private benefits to differ from its social benefits.

II.A Setup

Heterogeneous consumers indexed by i engage in a two-step process. First, they decide whether to have a home energy audit; we represent this decision with $A_i = \{0, 1\}$. Second, consumers decide whether to make each of a set of potential investments $\mathcal{J}_i$, which are indexed by j; we represent each decision with $I_{ij} = \{0, 1\}$. Consumers cannot invest without having an audit. We assume that investment opportunities are independent in the sense that adopting one does not affect the benefits and costs of adopting another.

Audits and investments are provided in perfectly competitive markets at prices c_A and c_{ij}, respectively, where c_A is constant but c_{ij} varies across consumers and potential investments depending on the specifics of the consumer's house. Audits and investments have net non-monetary benefits ξ_{Ai} and ξ_{ij}, which are heterogeneous and could be positive or negative. Costs such as time and hassle during the audit and construction make ξ_{Ai} and ξ_{ij} more negative, while benefits such as a more comfortable home and warm glow from reducing externalities make ξ_{Ai} and ξ_{ij} more positive. In the empirical estimates, ξ_{Ai} and ξ_{ij} are interpreted as demand unobservables, capturing non-monetary benefits as well as all sources of econometric error.

Household energy use is determined by an additional optimization problem that we do not need to model explicitly; see Dubin and McFadden (1984) and Davis (2008). The present discounted value (PDV) of baseline household energy use without the investment is e_{0i}. The investment would reduce energy use per unit of energy services and, unless utilization is fully inelastic to the price of energy services, increase utilization, for a net PDV reduction of e_{ij}. Utilization elasticity (sometimes called the "rebound effect") enters the model as more positive non-monetary benefits ξ_{ij} and lower savings e_{ij}.

The policymaker can set an audit subsidy s_{Ai} and an investment subsidy s_{ij}.[9] We assume that subsidies are funded through a lump-sum tax T, so there is no additional cost of public funds due to a deadweight loss of taxation. A consumer with initial wealth y_i has utility function

[9] In the model, both subsidies can vary across consumers. In the actual Wisconsin programs, the audit subsidy varied across consumers by city and due to our experimentally-assigned subsidies, and the investment subsidy varied across consumers and investments depending on predicted energy savings.

$$U_i = y_i - e_{0i} - T + A_i \cdot \left\{ s_{Ai} - c_A + \xi_{Ai} + \sum_{j \in \mathcal{J}_i} I_{ij} \cdot (s_{ij} - c_{ij} + e_{ij} + \xi_{ij}) \right\}. \tag{1}$$

Define N_P as the number of consumers in the population. To maintain a balanced budget, the lump-sum tax must equal total subsidy disbursements:

$$T = \frac{1}{N_P} \sum_{i=1}^{N_P} A_i \cdot \left\{ s_{Ai} + \sum_{j \in \mathcal{J}_i} I_{ij} s_{ij} \right\}. \tag{2}$$

II.B Audit and Investment Decisions

In a model with no market failures, consumers' audit and investment decisions would maximize Equation (1). Our model nests this possibility but also flexibly allows for market failures that might justify audit and investment subsidies.

In the second step, consumers' investment decisions maximize utility in Equation (1), except that there is a reduced-form distortion γ_{ij} that can drive a wedge between utility and investment takeup. For example, γ_{ij} might represent imperfect information that remains even after the audit is complete, or γ_{ij} could be zero if the audit fully informs consumers and there are no other distortions. The investment decision is thus

$$I_{ij} = 1\left(s_{ij} - c_{ij} + e_{ij} + \xi_{ij} + \gamma_{ij} > 0\right). \tag{3}$$

Define $\lambda_i = \sum_{j \in \mathcal{J}_i} I_{ij} \cdot (s_{ij} - c_{ij} + e_{ij} + \xi_{ij} + \gamma_{ij})$ as the perceived private net benefit from investments that consumer i would make. Before the audit, consumers may be imperfectly informed about this private net benefit, receiving signal $\lambda_i + \gamma_{Ai}$. For example, in a simple rational information acquisition model in which consumers' prior is that they will receive $\mathbb{E}[\lambda]$ from investments, $\gamma_{Ai} = \mathbb{E}[\lambda] - \lambda_i$. The audit decision maximizes utility conditional on the signal of perceived private net investment benefits:

$$A_i = 1\left(s_{Ai} - c_A + \xi_{Ai} + \lambda_i + \gamma_{Ai} > 0\right). \tag{4}$$

More generally, γ_{Ai} could capture any informational or behavioral distortion affecting audit takeup. If γ_{Ai} and γ_{ij} tend to be positive (negative), this makes consumers more (less) likely to audit and invest.

II.C Social Welfare

We allow the retail energy price to differ from social marginal cost due to uninternalized externalities and other pricing distortions. Household i's baseline energy expenditures are below social cost by a PDV of ϕ_{0i}, and investment j reduces these uninternalized negative externalities by a PDV of ϕ_{ij}. Define $\mathbf{s}$ as the vector of audit and investment subsidies across all consumers, and notice that

U_i, A_i, I_i, and T are all implicitly functions of $\mathbf{s}$. Social welfare is just the sum over consumers of utility minus the uninternalized externality:

$$W(\mathbf{s}) = \sum_{i=1}^{N_P} \left[U_i - \phi_{0i} + \sum_{j \in \mathcal{J}_i} I_{ij} \cdot \phi_{ij} \right]. \tag{5}$$

The effect of subsidy vector $\mathbf{s}_1$ vs. $\mathbf{s}_0$ on social welfare is

$$\Delta W = W(\mathbf{s}_1) - W(\mathbf{s}_0). \tag{6}$$

The social welfare maximizing subsidies exactly offset the audit and investment takeup distortions: $s_{Ai} = -\gamma_{Ai}$ and $s_{ij} = \phi_{ij} - \gamma_{ij}$. If energy demand is fully inelastic, the equilibrium under those subsidies would be first-best.

II.D Empirical Approaches to Welfare Analysis

We compare two approaches to measuring the social welfare effect of an energy efficiency program. The "accounting approach" counts the monetary costs and benefits, plus uninternalized externality benefits, from the entire set of investments made at subsidy $\mathbf{s}_1$:

$$\Delta W_a = \sum_{i=1}^{N_P} A_i \cdot \left[(-c_A) + \sum_{j \in \mathcal{J}_i} I_{ij} \cdot (-c_{ij} + e_{ij} + \phi_{ij}) \right]. \tag{7}$$

$\Delta W_a = \Delta W$ under two assumptions: if no investments are made at $\mathbf{s}_0$ and if non-monetary net benefits are mean-zero, i.e. $\mathbb{E}[\xi_{Ai}|A_i = 1] = 0$ and $\mathbb{E}[\xi_{ij}|I_{ij} = 1] = 0$. As discussed above, most energy efficiency programs are evaluated using variants of the accounting approach. This approach is useful because it is not very informationally demanding: ΔW_a can be calculated using administrative data on the monetary costs and benefits of investments, which most programs already record. If empirical data on average energy savings e_{ij} are available, as in our setting, then empirical estimates can be substituted in place of simulation predictions. The two required assumptions may not hold, however. In particular, it would be quite a coincidence for non-monetary benefits to be mean-zero, and we will show that this is not the case in our data.[10] Furthermore, this approach does not allow evaluations of alternative counterfactual subsidy structures.

The "revealed preference approach" involves using observed audit and investment takeup decisions to estimate utility function parameters. It requires the same administrative data as the engineering approach, but introduces two additional identification problems. First, we need to identify the joint distribution of unobservables in the audit and investment takeup decisions, ξ_{Ai} and ξ_{ij}. Put differently, we need exogenous variation in prices or subsidies to identify the slopes

[10] Practitioners often call inframarginal consumers "free riders," and some evaluations attempt to measure a program's impact relative to counterfactual by scaling down ΔW_a by an estimate of the share of program adopters that are believed to be marginal.

of audit and investment demand, as well as the self-selection effects that connect the two demand functions. Second, we need to identify γ_{Ai} and γ_{ij}, the wedges between takeup and true utility. The randomized experiment described below helps to solve these two problems.

III Experimental Design

III.A Experimental Population and Randomization

We sent informational letters by direct mail to a subset of households eligible for the Green Madison and Me2 programs. The experimental population included all owner-occupied single-family homes in Madison and Milwaukee that were built in 1990 or before, had no lien on the property, and had not scheduled an audit prior to May 2012. The population includes 101,881 households, of which 31,213 are in Madison and 70,668 are in Milwaukee. 79,994 households were randomly assigned to receive two identical direct mail marketing letters between June 2012 and February 2013, with the remaining 21,887 assigned to control. We used a min-max t-stat re-randomization algorithm to ensure balance, and Appendix Table A.2 shows that this was successful.[11]

III.B Letter Variations

Appendix Figures A.5 and A.6 present example letters. They were printed on 8 1/2-by-11 paper and folded in half for mailing. When opened, the top half was a picture with a short headline. The bottom half includes simple text that describes the program, lays out next steps, and gives a phone number to call to schedule the home energy audit. We varied the letters along seven dimensions, including audit subsidies and six non-price treatments that were designed to address key market failures thought to reduce takeup of home energy audits. These can be roughly categorized into three "informational" market failures and three "behavioral" failures.

III.B.1 Informational Treatments

Appendix Table A.4 details the treatments designed to address informational market failures.

Benefit Information. The Benefit Info treatments provided hard information on the private and social benefits of typical investments that could be made through the program.[12] This was

[11] The balancing variables were house age, property value, building area, and the Madison indicator. To ensure unbiased standard errors, we control for the balancing variables when estimating treatment effects (Bruhn and McKenzie 2009).

[12] Based on the program's previous estimates, we assumed that a typical weatherization job would reduce energy use by 23 percent. We transformed this to private cost savings using average natural gas and electricity prices. We transformed this into reduced climate damages using emissions factors from the National Academy of Sciences and a \$21 social cost of carbon, which was the current official estimate at the time of the experiment (Greenstone, Kopits, and Wolverton 2013). We included no quantitative information about the benefits through local air pollution reduction. Most of the energy saved is natural gas, and since natural gas generates little local air pollution, we calculated relatively small damages. Program staff hypothesized that revealing this would reduce takeup and asked us to remove the quantitative information.

motivated by literature suggesting that imperfect information and biased beliefs could affect energy efficiency investment.[13]

Financing. The Financing treatments informed consumers that low-interest financing was available for investments made through the program. This was motivated by Berry (1984), Gillingham, Newell, and Palmer (2009), and others who propose that credit constraints could reduce investment in energy efficiency.

Comparison. The Comparison treatments put the Benefit Information in context by comparing the program's energy savings to other tangible energy use decisions. We compared program non-participation to wasteful actions such as leaving the lights on all day or leaving the door wide open in the winter, in order to make participation seem like the natural choice. These treatments were designed to address the biased beliefs documented by Attari *et al.* (2010), who show that consumers tend to underestimate the savings from large energy efficiency improvements like weatherization relative to small changes like turning off lights. While we have classified this as an "informational" treatment, one could equally classify it as "behavioral."

III.B.2 "Behavioral" Treatments

The top of Appendix Table A.5 details the treatments targeted at potential behavioral failures.

Graphical Prime. We varied the pictures and headlines at the top of the letters to emphasize four different benefits of weatherization: saving money, local and global environmental protection, and a more comfortable home. The psychology literature refers to such graphical variations as "primes": activating an idea, potentially without providing any information, in a way that affects subsequent related behavior (Meyer and Schvaneveldt 1971). Prior research suggests that even subtle graphical primes can be effective. For example, Bertrand *et al.* (2010) find that showing a female photo increases demand for loans by as much as a two percent reduction in the monthly interest rate, while Mandel and Johnson (2002) find that background images affect hypothetical choices in a simulated shopping environment.

Time Frame. The Time Frame treatments varied whether the Benefit Information was framed as a one-year or seven-year total. These treatments were motivated by Turrentine and Kurani (2007), who show that consumers have difficulty aggregating savings over time, and Camilleri and Larrick (2014), who find that aggregating savings over longer periods increases stated preference for energy efficiency.

Audit Cue. The Audit Cue treatments varied whether the letter used the phrase "home energy assessment" or "home energy audit" in five different places on the page. Many energy efficiency experts suggest that using the word "audit" can reduce takeup because it cues negative associations with taxes. Program staff asked us to randomize only 1/3 of households into the "audit" condition, because they hypothesized that the word "audit" would reduce takeup.

[13] See Allcott (2013), Allcott and Sweeney (2016), Allcott and Taubinsky (2015), Davis and Metcalf (2016), and Newell and Siikamaki (2013) for recent experimental analyses. See Gillingham, Newell, and Palmer (2009), Jaffe and Stavins (1994), Sanstad, Hanemann, and Auffhammer (2006) for overview articles discussing imperfect information.

III.B.3 Subsidy Treatments

The bottom of Appendix Table A.5 details the subsidy treatments.

Subsidy. The Subsidy treatments varied the price of the home energy audit. In the "next steps" box, the letter read: "Call to schedule a home energy [assessment/audit]. Usual cost: $400. You pay only X!" Control households paid the standard program price, which was X=$200 in Madison and $100 in Milwaukee. Two other groups were randomly assigned to $25 and $100 additional rebates, so their listed prices were $175 and $100 in Madison and $75 and "nothing" in Milwaukee. A fourth group was presented with the standard control group price, but was also informed that they would receive a $25 Visa cash card after completing the audit. For this group, a mock Visa cash card was included in the letter, in an effort to make the money salient.

The audit subsidy information was relatively subtle, appearing once in normal font near the bottom of the letter. By contrast, the Benefit Information was in bold in a larger font, the word "audit" or "assessment" appeared in five different places, and the Graphical Primes involved the entire top fold of the letter and a headline in very large font. Thus, when we find in Section VI that the subsidy has larger effects than the non-price treatments, it is not because the non-price treatments were more subtly implemented.

IV Data

Table 2 presents summary statistics. Panel A presents data for the 101,881 households in the Wisconsin experimental population. House age, property value, and building footprint are from county administrative data provided by the utilities. Hybrid share is the percent of registered vehicles in the Census tract that are hybrids, potentially ranging from 0 to 100. Of the households in the experimental population, 1.4 percent (1394) had a home energy audit and 0.8 percent (823) made an investment through the programs before they ended in September 2013.

The Wisconsin programs' administrative data include the characteristics of each recommended and adopted investment at every household. Characteristics include investment type (e.g. insulation, air sealing, etc.), unsubsidized cost, and simulation predictions of annual energy savings in physical units of natural gas, electricity, and heating oil per year. Characteristics of adopted investments can differ from the audit report as contractors refine estimates, although on average they are very similar and in many cases identical. The most common types of recommended investments are various kinds of insulation (64 percent of recommendations), air sealing (22 percent) and new heating and cooling systems (11 percent).

Panel B details the two samples of investments that we construct. "Recommended investments" comprise households' choice sets for the investment takeup estimates in Section VI. This is the set of recommendations on the audit report, plus any investments that were adopted but did not appear on the audit report. "Adopted investments" are the investments considered in the "accounting approach" to welfare analysis in Section VIII. These include all subsidized investments.

The costs and predicted savings on the audit reports and in our data assume that investments are independent. For example, a recommended new heating system will have one row in the data with one cost and one predicted savings, with no information on how these might depend on whether the household also installs new insulation. Furthermore, the programs did not retain the data to exactly reconstruct the tiered investment subsidy that a household would receive with vs. without each investment.[14] Thus, since we do not have data on complementarities and substitutabilities, we assume in the model and empirical estimates that investments are independent.

Because our study is limited to evaluating energy efficiency investments, we exclude health and safety projects (improved ventilation and fire risk reduction) and solar photovoltaics from both the "recommended" and "adopted" investment samples. The recommended investments sample additionally excludes observations with zero or negative projected dollar savings (these appear to reflect model input errors), zero-cost and direct install measures (because they are free, there is no plausibly-exogenous price variation), appliances (takeup is imperfectly observed), and new hot water heaters (the program treated water heaters inconsistently across households). Our final samples of recommended and adopted investments include averages of 4.4 and 2.8 investments, respectively, per household audited.

We construct present discounted values using standard investment lifetimes provided by the program; 95 percent of investments in our final recommendations data have a 20 year assumed lifetime. We assume a five percent annual discount rate, approximately consistent with the real post-World War II returns to the S&P 500 stock market index and with the interest rates on loans available to program participants. In all PDV and IRR calculations, we assume that energy savings accrue in equal monthly installments.

We calculate energy prices to reflect averages over 2011-2014. In Madison and Milwaukee, natural gas and electricity are sold by regulated local monopolies, while heating oil is sold by multiple competing providers. We gathered retail marginal prices for natural gas and electricity from the Madison and Milwaukee utilities, and we use the Wisconsin average residential heating oil price from the Energy Information Administration (EIA). At retail prices, 76, 7.7, and 16 percent of savings from adopted investments are from natural gas, electricity, and heating oil, respectively. For natural gas acquisition costs, we use Wisconsin wholesale ("citygate") prices from EIA. For electricity acquisition costs, we use "all-in" wholesale market prices for the MISO market, which includes Wisconsin, from Potomac Economics (2011-2014). These "all-in" electricity prices include quantity-weighted average costs for energy and capacity, plus ancillary services and uplift charges. For heating oil, we assume that retail price equals marginal cost.

Panel C presents summary statistics for the electricity and gas usage microdata. Wisconsin law prohibits utilities from sharing energy use data with researchers unless the customer consents. Customers were asked to sign release forms during the audits, and 90 percent (1258 out of 1394) agreed, but we do not have energy use data for the larger group of unaudited households in the experimental population. We drop households that installed solar photovoltaics or are recorded as

[14]In particular, the program did not retain the household-specific baseline energy use estimates used to determine investment subsidy amounts.

having participated in another energy efficiency program for which we do not observe predicted savings; both of these factors would bias the comparison of empirically-estimated savings to predicted savings from investments made through these programs. The sample begins as early as January 2006 and ends in May 2015. The average household used 2.50 therms of gas and 20.6 kilowatt-hours of electricity per day during the sample period. We do not have consistent heating oil consumption data, but only 23 households made investments that were predicted to save heating oil.

Appendix B presents additional information on data preparation, categories of investments and subsidies, and energy price and externality assumptions.

V Empirical Strategy

We first specify the empirical analogue to Equation (4), the audit takeup equation. S_{Ei} is household i's experimental audit subsidy (either \$0, \$25, or \$100), G_i is an indicator for the \$25 gift card offer, $\mathbf{T}_i$ is the vector of indicators for informational and behavioral treatment groups, and $\mathbf{X}_i$ is a vector of the five household-level covariates from Panel A of Table 2: house age, property value, building footprint, a Madison indicator variable, and hybrid share. In the context of the model, the treatments $\mathbf{T}_i$ can affect γ_{Ai}, and household characteristics $\mathbf{X}_i$ can be associated with non-monetary preferences ξ_{Ai}, potential benefit from investing λ_i, and γ_{Ai}. Define $V_{Ai} = S_{Ei} + \varphi G_i + \tau \mathbf{T}_i + \beta_A \mathbf{X}_i + \kappa_A$ as the observed part of latent utility from auditing, where κ_A is a constant, and define ϵ_{Ai} as an econometric error. In the model, utility is money-metric, so the units on V_{Ai}, ϵ_{Ai}, etc., are dollars.

The empirical analogue to Equation (4) is

$$A_i = 1\left(V_{Ai} + \epsilon_{Ai} > 0\right). \tag{8}$$

We also specify an empirical analogue to Equation (3), the investment takeup equation. C_{ij} is the investment cost estimate, and E_{ij} is predicted retail energy cost savings over the assumed investment lifetime using a five percent annual discount rate. Recall that the bulk of investment subsidies were from tiered subsidies that increased by \$500 for every 10 percentage point increase in energy saved by adopted investments relative to the household's baseline. Because we do not have the data to exactly reconstruct the tiered program subsidies, we impute for each investment a linearized subsidy of $S_{ij} = \$5000\frac{E_{ij}}{E_{0i}}$, where E_{0i} is the PDV of household i's average pre-audit energy use, discounted over the investment lifetime. (To mirror the program subsidy structure, we cap this subsidy at \$3500, which winsorizes a handful of cases where $\frac{E_{ij}}{E_{0i}} > 0.7$.) Let $\xi_{ij} = \beta_I \mathbf{X}_i + \xi_j + \epsilon_{ij}$, where ξ_j is a constant and ϵ_{ij} is an econometric error, and define $V_{ij} = S_{ij} - C_{ij} + E_{ij} + \beta_I \mathbf{X}_i + \xi_j$ as the observed part of latent investment utility. The empirical analogue to Equation (3) is

$$I_{ij} = 1\left(V_{ij} + \epsilon_{ij} > 0\right). \tag{9}$$

.

In the theoretical model from Section II, ξ_{ij} represented non-monetary attributes such as comfort benefits and time costs. Empirically, these are interpreted as unobserved attributtes, capturing both non-monetary attributes and any other econometric errors, such as discount rates other than five percent and idiosyncratic monetary factors that are known to the consumer but unobserved on the audit reports. If consumers believe that realized energy savings will be lower (higher) than predictions presented on the audit report, this will show up as lower (higher) estimated ξ_{ij}.

When estimating Equation (9) and Equation (10) below, standard errors are clustered by household to allow for arbitrary within-household correlation in ϵ_{ij}.

We assume that $\eta_A \epsilon_{Ai}$ and $\eta_I \epsilon_{ij}$ are distributed standard normal, where η_A and η_I are scaling factors. If $\epsilon_{Ai} \perp \epsilon_{ij}$, then probit estimates of Equation (9) are relevant for the full 101,881-household population. Otherwise, probit estimates of Equation (9) are relevant only to the subset of households that had audits. Notice that because the *rescaled* errors are distributed standard normal, the estimated probit coefficients are the scaling factor times the coefficient written above. For example, the estimated coefficient on $\mathbf{T}_i$ in the audit takeup equation will be $\eta_A \tau$, and the estimated coefficient on S_{Ei} will be η_A. Dividing the estimated coefficient on $\mathbf{T}_i$ by the estimated coefficient on S_{Ei} will give $\eta_A \tau / \eta_A = \tau$. Analogously, the estimated constant term in the investment takeup equation is $\eta_I \xi_j$, so dividing by the price coefficient will give ξ_j in units of dollars.

We will begin by estimating separate probits for Equations (8) and (9). Our primary specification, however, allows for correlation between ϵ_{Ai} and ϵ_{ij} by jointly estimating the audit and investment takeup equations using the maximum likelihood approach of Van de Ven and Van Praag (1981). Defining $\rho = corr(\epsilon_{Ai}, \epsilon_{ij})$ and $\Phi_2(x, y, \rho)$ as the bivariate standard normal cumulative distribution of x and y with correlation ρ, the log-likelihood function is

$$\ln \mathcal{L} = \sum_i \left\{ \begin{array}{c} A_i \sum_{j \in \mathcal{J}_i} \left[I_{ij} \ln \Phi_2(V_{ij}\eta_I, V_{Ai}\eta_A, \rho) + (1 - I_{ij}) \ln \Phi_2(-V_{ij}\eta_I, V_{Ai}\eta_A, -\rho) \right] \\ +(1 - A_i) w \ln \left[1 - \Phi(V_{Ai}\eta_A) \right] \end{array} \right\}. \quad (10)$$

Inside the brackets, the first line sums over all investment opportunities for the households that did audit, while the second line sums over the households that did not audit. One feature of our data is that there are multiple investment decisions for each audit, which causes the households that audit to appear multiple times and thus receive more weight in estimating the audit takeup coefficients. To identify the audit takeup coefficients equally from households that did vs. did not audit, we weight the non-audited households with weight w equal to the average number of recommended investments per audit, while weighting each investment observation with weight 1.

Equation (10) delivers the parameters necessary for the revealed preference welfare analysis. We can now see how our RCT helps solve the two identification problems discussed earlier. First, this is a sample selection model in the spirit of Heckman (1979) and the literature that follows: we observe characteristics and takeup decisions for recommended investments only if a household audits. The randomly-assigned subsidies affect audit takeup but do not change investment incentives, which means that they act as an excluded instrument to identify the correlation between ϵ_{Ai} and ϵ_{ij}. In the literature that uses sample selection models, it is rare to have such an instrument. However, we do not have an instrument for price in the investment equation, so we need to assume that $(S_{ij} - C_{ij} + E_{ij}) \perp \epsilon_{ij} | \mathbf{X}_i$, i.e. that monetary characteristics are uncorrelated with unobservables affecting investment takeup. We discuss these issues further in the estimation results.

Second, the RCT helps to identify the audit takeup distortion γ_{Ai}. Intuitively, distortions can be measured in dollar terms by dividing the effect of removing the distortion by the effect of a price change. For example, if all consumers have $\gamma_{Ai} = -\$25$ due to imperfect information, providing full information will have the same effect on audit takeup as subsidizing audits by \$25. If instead $\gamma_{Ai} = -\$50$, providing full information will have twice the effect of the \$25 subsidy. Thus, if $\eta_A \tau$ is the effect of a treatment that fully removes an informational or behavioral distortion and η_A is the price effect, the dollar value of the audit takeup distortion is $\gamma_{Ai} = \eta_A \tau / \eta_A = \tau$.[15] Thus, our six informational and behavioral treatments test for particular sources of γ_{Ai}, although we cannot rule out the possibility of other audit takeup distortions.

VI Empirical Results

VI.A Audit Takeup

VI.A.1 Effects of Letter and Subsidy Treatments

Column 1 of Table 3 presents probit estimates of Equation (8), the effects of the experiment on audit takeup. We first present estimates replacing $\mathbf{T}_i$ with an indicator T_i for being mailed a letter. We present marginal effects, with coefficients multiplied by 100 for readability. The table's bottom row restates that 1.4 percent of households had an audit.

Receiving a letter with zero monetary incentives increased the probability of auditing by 0.158 percentage points, or about 13 percent of the control group mean. This relatively small effect strongly suggests that basic lack of awareness of the program was not a major barrier to takeup. The gift card had no effect, perhaps because of perceived transaction costs in activation. Money does matter, however: a \$100 subsidy increase affects audit probability by a point estimate of 0.525 percentage points, or 32 percent relative to control.[16] Even after heavy subsidies, demand for audits is remarkably low: households in the \$100 subsidy group in Milwaukee (Madison) needed to pay a net-of-subsidy price of only \$0 (\$100) for an audit, compared to a typical market price of \$400. Despite this, only 1.8 (2.2) percent of households in Milwaukee (Madison) in the \$100 subsidy group had audits.

Column 2 presents analogous probit estimates of whether the household made any investment.

[15] More precisely, Mullainathan, Schwartzstein, and Congdon (2012) show that if γ_{Ai} is homogeneous, a first-order approximation to γ_{Ai} is the ratio of the information effect to the price effect. Allcott and Taubinsky (2015), Chetty, Looney, and Kroft (2009), and Bronnenberg *et al.* (2015) use variants of this approach to identify informational and attentional distortions in other markets.

[16] Additional estimates of both audit and investment takeup show that the effect of the \$100 subsidy is not statistically distinguishable from four times the effect of the \$25 subsidy.

The point estimates suggest that a relatively small share of consumers marginal to the letters and experimental subsidies eventually invested. In the 21,887-household control group that did not receive informational letters, 64 percent of households that audited followed through with some investment. If consumers marginal to the treatments followed through at the same rate, then the ratio of estimates in column 2 to column 1 would also be 0.64. By contrast, the point estimates suggest that the average letter without experimental subsidy increased investments by about 26 percent of the increase in audits (0.041/0.158), and the experimental subsidies increased investments by 29 percent of the increase in audits (0.152/0.525). The fact that the marginal auditors are less likely to invest implies that ϵ_{Ai} is positively correlated with observable or unobservable investment attributes. We explore this issue more formally below.

The $\mathbf{X}$ covariates are associated with audit and investment takeup in intuitive ways. Takeup increases in house age: because building codes and construction techniques have improved, older houses can benefit more from weatherization retrofits. Takeup is positively correlated with hybrid vehicle share, perhaps because environmentalists benefit more due to warm glow. Takeup is also positively correlated with wealth, as measured by property value and building footprint.

VI.A.2 Effects of Informational and "Behavioral" Treatments

We also estimate a version of Equation (8) with the full set of $\mathbf{T}_i$ indicators. The full set of estimates is in Appendix Table A.7. In summary, only the experimental subsidies statistically significantly affect takeup relative to the omitted category. Furthermore, Wald tests in Appendix Table A.8 show that none of the six groups of informational or behavioral treatments jointly affected audit or investment takeup.

How precisely estimated are these zero effects? Figure 1 presents the point estimates and confidence intervals for each informational and "behavioral" treatment scaled by the effect of a $1 subsidy, i.e. $\eta_A\tau/\eta_A = \tau$. This translates the coefficient estimates into dollar terms, which is the same scale as utility and γ_{Ai}. All 90 percent confidence intervals include zero, and the average confidence interval bounds the effect at no more than the effect of a $30 to $40 price change.[17]

This normalization of treatment effects into dollar terms is useful for two reasons. First, it gives the dollar value of the informational distortion under the assumption that $\gamma_{Ai} = \tau$, as discussed above. In our experiment, one should not interpret any given treatment as removing all distortions, both because it is unlikely that our treatments fully addressed the hypothesized distortions and because there are other possible distortions that we could not test. Notwithstanding, the bounds on the information effects suggest that the magnitudes of the informational and behavioral audit takeup distortions that motivated our six treatments might be an order of magnitude smaller than

[17] For comparison, the three statistically significant advertising treatments in Bertrand *et al.* (2010) affected demand by the equivalent of a two percent change in monthly interest rate. At a median loan size of $150, a two-percent interest rate change is worth $3 per month, or $12 total for their four-month loans. This is about 2.5 percent of their population's $470 median gross monthly income. By contrast, our population's median gross monthly income is $4000, so 2.5 percent of median income is $100. Thus, after accounting for population income differences, we can bound the price-scaled effects of all our non-subsidy treatments at about 30-40 percent as large as the Bertrand *et al.* (2010) estimates.

the programs' \$200-\$300 audit subsidies.

Second, the normalization addresses the fact that many people don't read unsolicited mail. Imagine that share $r < 1$ of consumers who were mailed the letters actually read them, and the true treatment effect on letter readers is τ', so $\tau = r\tau'$. Then τ could be small either because the treatments had little effect on the letter readers (τ' is small) or because few people read the letters (r is small). Taking the ratio of the information effect to the subsidy effect divides out the r, giving the ratio of effects within the group of letter readers. This issue is crucial to interpreting our results, as these economically small coefficient *ratios* cannot be explained by people not reading the letters: if nobody read the letters, then the experimental subsidies would also have no effect. This discussion also clarifies that all coefficients and ratios are "local" to the subset of people who read the letters, and these people could in be systematically more or less informed or "behavioral" than the people who do not read the letters.

VI.B Investment Takeup

VI.B.1 Graphical Results

Figure 2 directly illustrates the identification of Equation (9). The vertical bars are a histogram of projected monetary benefit ($S_{ij} - C_{ij} + E_{ij}$) in the sample of recommended investments. We truncate the graph at $\pm$\$3000 for readability, plotting all smaller (larger) investments in the far left (right) bins. The dots illustrate the takeup rates within each bin.[18]

Figure 2 has three striking features. First, many of the recommended investments are disadvantageous from a purely financial perspective, even at subsidized investment costs and retail energy prices and assuming that the simulated savings and 20-year lifetimes are correct. One quarter of recommended investments lose \$638 or more, the median recommendation loses \$47, and 53 percent do not pay back at a five percent discount rate. Second, takeup rates are increasing in projected monetary net benefit, meaning that consumers clearly consider monetary incentives. The slope of takeup with respect to monetary benefit identifies η_I. Third, while takeup clearly increases in projected monetary benefit, the slope is quite gradual: a \$1000 increase in projected monetary benefit is associated with only about a five percentage point increase in takeup. Consumers did not take up 40 percent of investments with private internal rates of return (IRRs) greater than 20 percent, and they did take up 36 percent of investments with negative private IRRs. This implies that η_I is small: there must be wide dispersion in unobserved attributes to rationalize these takeup decisions. This highlights the importance of incorporating unobserved attributes into welfare analysis using the "revealed preference approach" instead of the "accounting approach."

[18] Appendix Figure A.7 re-creates this figure with internal rate of return instead of net benefit on the x-axis. Because IRR and net benefit are closely connected, the figure looks very similar.

VI.B.2 Estimation Results

Table 4 presents marginal effects from probit estimates of Equation (9). Except for in column 4, we limit the sample to observations with $\|(S_{ij} - C_{ij} + E_{ij})\| \leq \$5000$ to reduce the influence of outliers. Column 1 presents estimates excluding household covariates $\mathbf{X}_i$. Consistent with Figure 2, takeup increases by 5.9 percentage points for every \$1000 increase in projected monetary benefit.

Column 2 adds the $\mathbf{X}_i$ covariates to column 1; the marginal effect of $\hat{\eta}_I$ does not change. The probit coefficient (i.e., not the marginal effect) on $(S_{ij} - C_{ij} + E_{ij})$ in column 2 is $\hat{\eta}_I \approx 0.148$. Because $\sigma_{\eta_I \epsilon_{ij}} = 1$ by the normalization of the probit model, we have $\hat{\sigma}_{\epsilon_{ij}} = \frac{1}{\hat{\eta}_I} \approx \frac{1}{0.148} \approx 6.8$. Given that $(S_{ij} - C_{ij} + E_{ij})$ is in units of \$000s, this implies that the standard deviation of ϵ_{ij} is about \$6,800. This quantifies the observation from Figure 2 that there is wide dispersion in unobserved attributes. Given such a wide dispersion, it would be highly coincidental for unobserved attributes to be mean-zero, as the accounting welfare analysis approach assumes.

Because $(S_{ij} - C_{ij} + E_{ij})$ is not randomly assigned, we can only cautiously interpret $\hat{\eta}$ as a demand slope. The unobservable ϵ_{ij} would be positively correlated with E_{ij} if energy savings bring warm glow utility or are associated with more in-home comfort. Furthermore, ϵ_{ij} would be negatively correlated with C_{ij} if higher-cost projects also require more non-monetary effort to implement, e.g. if larger home construction jobs are both more costly and more of a hassle for the homeowner. Both of these possible correlations would bias $\hat{\eta}_I$ upward, meaning that investment takeup might be even more inelastic than we estimate.

To explore this issue, Column 3 allows separate $\hat{\eta}_I$ coefficients on future savings E_{ij} vs. net upfront costs $C_{ij} - S_{ij}$. The coefficients are economically and statistically similar to each other and to the $\hat{\eta}_I$ coefficients in columns 1 and 2. Thus, either the η_I coefficient is reasonably unbiased, or ϵ_{ij} happens to have very similar correlations with E_{ij} vs. $C_{ij} - S_{ij}$.[19]

Column 4 includes the additional 110 recommended investments with $\|(S_{ij} - C_{ij} + E_{ij})\| > \$5000$. These estimates could be more heavily driven by outliers – for example, there are four recommendations with $\|(S_{ij} - C_{ij} + E_{ij})\| \geq \$25,000$. The $\hat{\eta}_I$ coefficient is somewhat smaller, further reinforcing the finding of inelastic demand.

The theoretical framework and welfare analysis allow for various market failures that might affect demand for audit and investments. One potential example is asymmetric information problems that could prevent investments' full benefits from being capitalized into home resale prices. If people have some foresight into their possible future moves, such a market failure would cause households that move sooner to be less likely to invest. However, column 5 shows that the 37 audited households that close their utility accounts more than six months post-audit but during our sample are no less likely to invest. This provides no evidence that this potential market failure is relevant for investment decisions, although it could in principle affect whether people have audits.

Column 6 includes an additional control for the experimental audit subsidy offered to household i, in units of \$100s. This subsidy does not affect investment incentives conditional on auditing, but it does cause selection into auditing. Households that audited under a \$100 larger audit subsidy

[19] Additional estimates with the Chamberlain (1980) fixed effects logit estimator give similar results.

are a remarkable 12.9 percentage points less likely to invest, conditional on $\mathbf{X}_i$ and $S_{ij} - C_{ij} + E_{ij}$. This selection effect implies that there is a positive correlation between ϵ_{Ai} and ϵ_{ij}.

Figure 3 illustrates this selection effect. The figure presents takeup decisions for households that received the marketing letter and were offered a \$0, \$25, or \$100 experimental audit subsidy; the \$25 gift card group is excluded. The left panel shows the audit probability as a function of the audit subsidy. The \$100 experimental subsidy increases audit takeup from 1.3 to 1.9 percent. The light (gray) bars on the right panel show the average investment probability by subsidy group, conditional on auditing. As in column 5 of Table 4, the \$100 subsidy group is about 13 percentage points less likely to invest.

Using the average investment probabilities and the share of marginal vs. inframarginal consumers implied by audit takeup in the left panel, we back out the investment probability for households marginal to each subsidy increase.[20] Even before calculating the exact numbers, we know that the marginal auditors must have markedly lower investment probabilities for higher subsidies to cause such a large decrease in average investment probability. Indeed, the dark (red) bars in the right panel show that marginal investment probabilities drop sharply as the subsidy increases. While 54 percent of households that audit at zero experimental subsidy make some investment, point estimates imply that only 25 percent of households that are marginal to the \$25 subsidy do so. Remarkably, the point estimates imply that among the households that audit at a \$100 subsidy but do not audit at a \$25 subsidy, only three percent make an investment.

One policy implication is that programs interested in maximizing investments per subsidy dollar should think carefully about what to subsidize. Subsidizing audits will draw in proverbial "tire kickers": households that are interested in audits, perhaps because they are less busy or want free CFLs, but may not be interested in larger investments. This strong selection plays a key role in Section VIII's counterfactual analysis of higher subsidies.

VI.C Joint Audit and Investment Takeup

The counterfactual policy analyses that we conduct below require demand parameters relevant for the full population. The above finding that the marginal auditors are much less likely to invest makes clear that a joint model of audit and investment takeup is necessary to estimate parameters for the full population.

Table 5 compares joint and independent estimates of the audit and investment takeup equations. Columns 1 and 2 present the independent probit estimates of Equations (8) and (9), while columns 3 and 4 present estimates of Equation (10). Columns 1 and 3 present stripped-down estimates,

[20] Specifically, conditional on auditing at subsidy $S_A = s$,

$$\Pr(I=1) = \Pr(I=1|M=1) \cdot \Pr(M=1) + \Pr(I=1|M=0) \cdot (1 - \Pr(M=1)), \tag{11}$$

where $M = 1$ is an indicator for being marginal to an audit subsidy increase from s_0 to s: $M = 1\left(-s < \tau \mathbf{T}_i + \beta_A \mathbf{X}_i + \kappa + \epsilon_{Ai} < -s_0\right)$. Re-arranging gives an equation for $\Pr(I=1|M=1)$, the investment probability for marginal consumers. $\Pr(M=1)$ is from audit takeup rates illustrated in the left panel, and $\Pr(I=1)$ and $\Pr(I=1|M=0)$ are from investment takeup rates in the light bars in the right panel.

excluding household covariates $\mathbf{X}_i$, while columns 2 and 4 include $\mathbf{X}_i$ and replace the constant ξ_j with separate indicators for all six investment categories: air sealing, insulation, heating/cooling systems, windows, pipe and duct sealing and insulation, and programmable thermostats. The top panel of column 2 is the same as column 1 of Table 3, while the bottom panel of column 1 is the same as column 1 of Table 4, except that we now present coefficient estimates, not marginal effects.

The experimental audit subsidy S_{Ei}, gift card indicator G_i, and letter treatment indicator T_i are included in the audit takeup equation but excluded from the investment takeup equation, thus identifying the correlation between ϵ_{Ai} and ϵ_{ij}.[21] The estimated $\hat{\rho} \approx 0.94$ is remarkably high, driven by the sharp decrease in investment probability at higher subsidy levels illustrated in Figure 3.

Comparing the independent and joint estimates in columns 1 vs. 3 and 2 vs. 4, we see that the audit takeup parameters are all statistically indistinguishable. The investment takeup parameters, however, are all statistically different. The starkest difference is that the "constant" terms $\widehat{\eta_I \xi_j}$ are much more negative in the joint estimates in column 3 compared to the independent estimates in column 1. This reflects the positive correlation between ϵ_{Ai} and ϵ_{ij}. In column 1, the independent probit estimate of investment takeup identifies the constant $\widehat{\eta_I \xi_j}$ relevant for the subset of households that audited. By contrast, the joint estimate in column 3 returns the constant relevant for the full population. The subsample of audited households have high draws of ϵ_{Ai}, and because ρ is large, they also have high draws of ϵ_{ij}. Thus, the constant $\widehat{\eta_I \xi_j}$ for the audited sample is much larger (i.e. less negative) than for the full sample of 101,881 households. This underscores that the households that audited are a self-selected group that is significantly more interested in making investments.

Dividing the estimated constant term $\widehat{\eta_I \xi_j}$ by $\hat{\eta}_I$ gives $\hat{\xi}_j$, the mean unobserved attribute, in units of dollars. In column 1, we find that across all recommended investments in the sample of audited households, the mean unobserved attribute is slightly positive: $\hat{\xi}_j \approx 0.0485/0.147 \approx \$329$. This indicates that on average, audited households positively value recommended energy efficiency investments above their financial net present value, perhaps due to warm glow or the expectation of a more comfortable weatherized home. This approach is important because it captures and respects consumers' preferences, allowing for the possibility that consumers perceive costs and benefits that the analyst does not observe. These results contrast with the assumption in the "accounting approach" that the mean unobserved attribute is zero.

Figure 4 presents the estimated $\hat{\xi}_j$ from a regression that is identical to the investment takeup probit in column 1, except that it allows the constant term to vary by investment category. We calculate standard errors using the Delta method. The close correspondence between takeup rates (against the right axis) and $\hat{\xi}_j$ (against the left axis) emphasizes that takeup rates identify $\hat{\xi}_j$: if many consumers adopt an investment after being informed about its costs and benefits, we infer that they must derive more utility from it. Air sealing, insulation, and programmable thermostats have the most positive $\hat{\xi}_j$, while windows and pipe and duct sealing/insulation have the most

[21] We could also use the full set of non-price treatment indicators $\mathbf{T}_i$ as excluded variables, but they reduce precision because they do not affect takeup.

negative.

VII Effects on Energy Use

VII.A Empirical Strategy

The private and social benefits of energy saved are a crucial part of the welfare evaluation. What are the empirical estimates of energy savings, and how do they compare to the simulation predictions?

Define Y_{it} as natural gas or electricity use (in therms/day or kWh/day) for household i for the billing period ending in date t. $\mathbf{P}_{it}$ is a pair of post-audit indicators: to distinguish shorter-term from longer-term effects, we allow different coefficients on $\mathbf{P}_{it}$ for the first six months vs. later. For the billing period that includes the audit date, we pro-rate $\mathbf{P}_{it}$ on $[0,1]$ to reflect the share of days after the audit. $\mathbf{W}_{it}$ is a vector of two weather controls: average heating degrees and average cooling degrees in the household's city (Madison or Milwaukee) over the billing period ending in t, from NOAA (2015). ν_i is a household-by-calendar month fixed effect, and μ_m is the set of month-of-sample indicators for all months m in the sample.[22] The estimating equation is

$$Y_{it} = \alpha \mathbf{P}_{it} + \omega \mathbf{W}_{it} + \nu_i + \mu_m + \varepsilon_{it}. \tag{12}$$

Standard errors are robust and clustered by household to allow for arbitrary serial correlation in ε_{it}.

To compare empirical estimates to the simulation predictions, we also fit Equation (12) with predicted savings as the dependent variable. Denote E_{it} as the total predicted daily savings (in therms/day or kWh/day) for all observed investments made by household i as of the billing period ending in date t. ($E_{it} = 0$ before the first investment is made, and thus $E_{it} = 0$ before the audit.) If the investment install date occurs in the middle of the billing period ending in date t, we pro-rate predicted savings over the billing period.

The simulated savings are for average weather conditions. If the empirically realized weather conditions differ, this could make the empirical results differ from predictions even if predictions are unbiased in average weather. We thus weather-adjust the predictions assuming that savings scale proportionally in degree days.[23] The "realization rate" for natural gas or electricity will be the ratio of the $\hat{\alpha}$ for actual energy use to the $\hat{\alpha}$ for predicted savings.

[22]For example, there is one μ indicator variable that takes value 1 for all bills t where the midpoint of the billing period occurs in January 2012, then another μ for all bills where the midpoint occurs in February 2012, etc. Then, there is one fixed effect ν for all bills of household i with midpoint in January of any year, a second fixed effect for all bills of household i with midpoint in February of any year, etc.

[23]Specifically, we classify investments into four seasonality categories: constant (hot water and lighting), cooling (cooling system improvements), heating (heating system improvements), and cooling or heating (all others, such as insulation, air sealing, etc.). For cooling (heating) categories, $E^w_{it} = E_{it} \cdot \frac{W_{it}}{\bar{W}_i}$, where W_{it} is the mean base-65 cooling (heating) degree days for billing period t and $\bar{W}_i$ is the average cooling (heating) degrees for household i's city (Madison or Milwaukee) between 2000 and 2011. The equation for E^w_{it} for the "cooling or heating" seasonality category is identical except that W_{it} and $\bar{W}_{it}$ are the sum of heating plus cooling degrees. Appendix Table A.9 presents robustness checks without the weather adjustment.

If $\mathbf{P}_{it} \perp \varepsilon_{it} | (\mathbf{W}_{it}, \nu_i, \mu_m)$, then α is the average causal effect of the audit and ensuing investments. Even under this assumption, however, there are two reasons to interpret α carefully. First, households may simultaneously change utilization behaviors, so α would differ from causal effect of investments under constant utilization. This is important for interpreting the realization rate because TREAT and other simulation models predict savings under constant utilization. A realization rate less than 100 percent could thus reflect either a utilization increase, which the simulation models are explicitly not trying to capture, or systematic modeling bias, which the simulation models should be trying to avoid. This is also important for interpreting the accounting welfare analysis, because a utilization increase generates an increase in consumer welfare that the accounting approach does not capture.

Second, households may make unobserved investments, such as purchasing CFLs or appliances through retailers not affiliated with the programs. This is clearly important for the realization rate, because the simulation models only predict savings from observed investments, and for the accounting welfare analysis, because we do not include the costs of investments we do not observe. We discuss these issues more below.[24]

VII.B Empirical Results

VII.B.1 Graphical Results

Figure 5 is a standard event time graph, which illustrates effects and allows a visual test for pre-trends. To make the figure, we estimate Equation (12) replacing $\mathbf{P}_{it}$ with indicators for each two-month period within an event window extending 18 months before and after the audit. The figure combines separate estimates for natural gas and electricity, weighting each two-month period's coefficients by sample average retail prices and multiplying by 365 to transform units to annualized retail energy cost savings.

Figure 5 shows that there are no pre-audit trends in energy use. Immediately after the audit, energy use decreases. By about six months post-audit, energy use has stabilized at almost $100 less than the pre-audit annual average. Relative to a pre-audit average of about $1800 per year, this represents a five percent reduction.

The light gray lines are the simulation predictions, from using E_{it} instead of energy use as the dependent variable in the same regression. Predicted energy use decreases immediately after the audit as "direct install" measures (primarily CFLs and low-flow showerheads) are put in, then decreases gradually over the first six months post-audit as consumers make larger investments. By about six months post-audit, predicted energy use has stabilized at approximately $150 less than baseline. Appendix D presents separate figures for natural gas and electricity, as well as a series of other graphical robustness checks.

[24]Neither of these two problems is solved by a randomized encouragement research design. Such a design credibly identifies the causal impact of the encouragement, but households could also change utilization behaviors or make unobserved investments.

VII.B.2 Estimation Results

Table 6 presents formal estimates of Equation (12). Columns 1 and 2 are for natural gas, while columns 3 and 4 are for electricity. Columns 1 and 3 use the weather-adjusted simulation predictions E_{it} as the dependent variable, while columns 2 and 4 use energy use Y_{it}. Since the typical investment is predicted to last 20 years and the graph shows savings stabilizing only after about six months after the audit, we focus on the coefficients for ≥ 6 months.

After six months, the simulations predict 0.444 therms/day natural gas savings and 0.408 kWh/day electricity savings. By contrast, the actual savings were 0.128 therms/day and 1.013 kWh/day. Figure 6 summarizes these results. Natural gas savings amount to only 29 percent of predictions, while electricity savings are 248 percent of predictions. In total, the average household that had an audit saves \$89 per year in retail gas and electricity costs. The total predicted savings are \$153 per year, so the realization rate is 58 percent at retail prices. We have done extensive additional analysis and robustness checks; interested readers should see Appendix D.

Because the realization rates are so different between natural gas and electricity (29 vs. 248 percent), the relative weight given to the two fuels matters for the overall realization rate. For example, because electricity has a higher retail markup over acquisition cost than natural gas, combining the two fuels at acquisition costs increases the weight on natural gas. Valuing energy at acquisition cost, the average audited household reduces gas and electricity use by \$38 per year, and the realization rate is 41 percent. Adding externalities and valuing energy at social marginal cost, the average audited household reduces gas and electricity use by \$94 per year, and the realization rate is 52 percent.

What could explain the difference between simulated and empirically-estimated savings? For natural gas, additional analyses in Appendix Table A.11 show smaller or statistically zero shortfalls for large and common investments (insulation and new heating/cooling systems) but particularly large empirical shortfalls for smaller and less common investments. For electricity, the additional analyses suggest that the excess savings could result from unobserved actions such as appliance replacement that would not be recorded in the programs' administrative data. When adjusting the accounting welfare analysis in Section VIII for empirically-realized savings, we assume that the excess electricity savings accrue because of the *observed* investments, which may cause us to understate total investment costs for the empirically-estimated energy savings. This will bias against our finding that program costs outweighed benefits.

The simulation models assume constant utilization before vs. after the investment, and one natural explanation for the natural gas shortfall could be the "rebound effect," i.e. that consumers increase utilization in response to a decrease in the cost of energy services. Appendix D presents two calculations showing that the rebound effect is highly unlikely to explain the full gas shortfall. First, a utilization elasticity of -0.98 would be required to explain the full shortfall. By contrast, the most closely-related estimates of energy utilization elasticity are much smaller: -0.06, -0.3, and -0.22 for washing machines (Davis 2008), home electricity (Dubin and McFadden 1984), and autos (Gillingham 2014), respectively. Second, a large indoor temperature change of 7.5 degrees

Fahrenheit would be required to explain the full shortfall. By contrast, the Fowlie, Greenstone, and Wolfram (2015b) find no statistically significant post-weatherization temperature change in a low-income (and thus likely more price elastic) population in Michigan, and they can reject a post-weatherization temperature change of more than 1.4 degrees Fahrenheit with 90 percent confidence in a two-sided test. To the extent that utilization is not fully inelastic, the associated increase in consumer welfare is excluded from the "accounting approach," biasing those benefit estimates downward. In theory, the revealed preference analysis does capture this welfare increase as part of ξ_{ij}.

A realization rate of less than 100 percent is consistent with previous findings. Fowlie, Greenstone, and Wolfram (2015b) find a 40 percent realization rate in their Michigan RCT. They similarly find that the simulation model overestimates natural gas savings and underestimates electricity savings, but since gas comprises a larger share of savings, the overall realization rate is far less than 100 percent. The TREAT software developers found median household-level realization rates around 60-70 percent in recent New York study (PSD 2015b), also finding that their model understated electricity savings and overstated gas savings. DOE's analysis of realization rates at other Better Buildings Neighborhood Program sites were 47 percent and 59 percent, respectively, for natural gas and electricity (DOE 2015c).[25]

VIII Welfare Analysis

In this section, we present welfare analyses using the "accounting" and "revealed preference" approaches. First, we detail assumptions for environmental externalities and market failure parameters ϕ_{ij}, γ_{Ai}, and γ_{ij}.

Both the accounting and revealed preference approaches require estimates of environmental externalities per unit of energy use, for each of the three fuels in our data: natural gas, electricity, and fuel oil. We calculate externality reductions in real 2013 dollars using natural gas and heating oil emission factors (i.e. pollution emissions per unit of energy use) from the AP-42 database (EPA 1995) and electricity emission factors from Holland, Mansur, Muller, and Yates (2015). For each fuel, we then multiply those emission factors by pollutant-specific marginal damages, also from Holland, Mansur, Muller, and Yates (2015). Key assumptions for marginal damages are a \$39 social cost of carbon, based on estimates from the Interagency Working Group on the Social Cost of Carbon (2013), a three percent life-cycle leakage rate from natural gas systems from Howarth *et al.* (2012) and Abrahams *et al.* (2015), a \$6 million value of a statistical life, and a fine particulate dose response function from Pope *et al.* (2002).

The revealed preference approach requires estimates of present discounted value of the *uninternalized* externality, denoted ϕ_{ij} in our model.[26] (If environmental externalities are already priced

[25] Additionally, an early study by the American Council for an Energy Efficient Economy (Nadel and Keating 1991) reviewed 11 prior studies with a mean realization rate of 48 percent. Blasnik (2010) discusses studies from the 1990s with realization rates between 50 and 70 percent.

[26] Formally, ϕ_{ij} is the sum over fuels $f \in \{natural\ gas, electricity, fuel\ oil\}$ of the PDV of an investment's energy

into retail marginal energy costs, then they do not distort investment decisions.) As Davis and Muehlegger (2010) point out, most utilities mark up retail marginal prices above marginal acquisition cost in order to cover fixed costs such as meter reading and overhead, and this markup partially offsets the environmental externalities.[27] Section IV presented assumptions for energy acquisition costs, as well as discount rates and investment lifetimes. In our sample, retail marginal prices exceed acquisition costs by 50 percent and 297 percent for natural gas and electricity, respectively. Thus, while the mean recommended investment in our sample reduces environmental externalities by a present discounted value of \$598, the mean ϕ_{ij} is only \$261.

The revealed preference approach also requires estimates of γ_{Ai} and γ_{ij}. Our RCT found statistically and economically zero treatment effects of variations designed to identify six informational and behavioral barriers to audit takeup. In our base case estimates, we therefore assume $\gamma_{Ai} = 0$.

In the context of the Wisconsin programs, most of the investment takeup market failures γ_{ij} that have been discussed in the energy efficiency literature are not relevant. The home energy audits and written audit reports provide clear information about the costs and benefits of possible investments. There is no credit constraint, as consumers have full financing available. Credit availability also reduces the potential role of present bias: allowing consumers to borrow means that investment does not require a reduction in current consumption. Our sample includes only owner-occupied homes, so there is no landlord-tenant distortion discussed by Davis (2012), Gillingham, Harding, and Rapson (2012), and Myers (2015). In Table 4, we found no evidence that investment takeup is affected by asymmetric information problems that might prevent investments' full benefits from being capitalized into home re-sale prices. In our base case estimates, we therefore assume that $\gamma_{ij} = 0$: other than uninternalized externalities, no market failures affect investment takeup. Due to the inherent uncertainty in these parameters, we will also present welfare estimates under alternative assumptions for γ_{Ai} and γ_{ij}.

VIII.A Welfare Effects Using the "Accounting Approach"

Table 7 presents welfare effects using the "accounting approach." Costs comprise audit and investment costs, while benefits comprise energy savings (at acquisition cost) plus environmental externality reduction.[28] We assume that the unsubsidized audit cost is $c_A = \$400$, based on typical market prices. Column 1 uses the simulation predictions to calculate energy and externality reduc-

savings times the retail price distortion:

$$\phi_{ij} = \sum_f \sum_{t=1}^{T} \delta^t \cdot Energy\ Saved_{ij,f} \cdot (Social\ Marginal\ Cost_f - Retail\ Marginal\ Price_f),$$

where δ is the discount factor, t indexes time, T is the investment lifetime, and Social Marginal Cost is the per-unit acquisition cost plus the per-unit environmental externality.

[27] A more economically efficient pricing structure would be to pass through fixed costs as fixed monthly charges. Regardless of whether this rate structure is desirable on equity grounds (see Borenstein and Davis (2012)), it still generates a price distortion.

[28] To be clear, this is the full environmental externality reduction, not the *uninternalized* externality reduction ϕ. Environmental externality reduction plus energy savings (at acquisition cost) also equals *uninternalized* externality reduction plus energy savings (at retail price).

tions. Column 2 multiplies the simulation predictions by realization rates of 2.48 for electricity and 0.29 for natural gas and heating oil, on the basis of Table 6 and Figure 6.[29]

Under the assumptions of the accounting approach, the programs reduces welfare. Using the simulation predictions, the Wisconsin programs have a benefit/cost ratio of 0.92 at five percent discount rates, with an internal rates of return of 4.0 percent. After adjusting for the empirical shortfall, the benefit/cost ratio is 0.43 and the IRR is negative 4.1 percent.

Is it possible that the two programs that we evaluated happened to have particularly low returns? In Appendix E, we analyze data from all 37 Better Buildings Neighborhood Programs nationwide that reported data to the U.S. Department of Energy (DOE). While we were unable to run experiments or gather recommended investment data from these other sites to carry out the full revealed preference analysis, we can replicate this accounting evaluation. The national programs performed worse than the Wisconsin programs. Using the simulation predictions, the national programs have a benefit/cost ratio of 0.75 at five percent discount rates, with an IRR of 1.5 percent. After adjusting with the DOE's estimate of the empirical shortfall, the benefit/cost ratio is 0.38 and the IRR is negative 5.2 percent. By these metrics, society would have been much better off without these programs.

The results of the accounting approach depend heavily on energy price and externality assumptions. Instead of using recent energy prices, one alternative would be to use forecasts over the investment lifetimes. The U.S. Energy Information Administration (2015) Annual Energy Outlook predicts that the benchmark Henry Hub natural gas price will average $1.01 higher in real terms over 2013-2032 than it was over 2011-2014. When passed through to citygate prices, this would increase acquisition costs (and thus the value of natural gas savings) by 19 percent, slightly increasing net benefits. We could also proxy for the social marginal cost of electricity using the cost of a new combined cycle natural gas plant. Using the Annual Energy Outlook levelized cost estimates from EIA (2015) and our same per-unit externality damage assumptions, we calculate an electricity social marginal cost of $0.106 per kilowatt-hour, which is 16 percent lower than our main assumptions. This is lower because while the levelized production cost is higher than 2011-2014 market prices, this is more than offset by the natural gas plant's relatively low local air pollution emissions. Using this lower electricity cost would slightly reduce net benefits. Very large changes to the energy price or externality assumptions would be required to make the programs welfare enhancing in Table 7: for empirically-adjusted benefits to exceed costs, energy acquisition costs would need to be 4.2 times larger, all externality damages would need to be 3.3 times larger, or the social cost of carbon would need to be $239 per ton.

[29]The vast majority of heating oil savings are from oil-to-gas heating system conversions. Although we do not have empirical estimates of the realization rates for heating oil, we need to adjust heating oil savings by the natural gas adjustment factor to avoid predicting artificial decreases in total energy use.

VIII.B Welfare Effects Using the "Revealed Preference Approach"

VIII.B.1 Simulation Procedure

We now evaluate the welfare impacts of a subsidy change from $\mathbf{s}_0$ to $\mathbf{s}_1$ using the "revealed preference approach." We first show how to determine the welfare effect conditional on one draw of each ϵ_{Ai} and ϵ_{ij}, and we then simulate draws over the joint distribution of ϵ_{Ai} and ϵ_{ij}. In our base case estimates, we use the simulation predictions of energy savings, which gives an upper bound on welfare effects both because the empirically-estimated externality reductions are lower and because consumers may have made investment decisions with the apparently incorrect belief that the simulation predictions were unbiased.

We must first define several terms. We denote household i's vector of ϵ_{Ai} and ϵ_{ij} draws as ϵ_i, and we denote the vector of all households' ϵ_{Ai} and ϵ_{ij} draws as ϵ. We denote the vector of program subsidies as $\mathbf{s}_p$. We use "hats" to signify that a variable depends on empirical estimates, which we take from the joint estimates in column 4 of Table 5. For example, $\hat{\lambda}_i(\mathbf{s};\epsilon_i) = \sum_{j\in\mathcal{J}_i} \hat{I}_{ij}(\mathbf{s};\epsilon_{ij}) \cdot (\hat{V}_{ij}(\mathbf{s}) + \epsilon_{ij})$.[30]

The empirical analogue to the utility function in Equation (1) is

$$\hat{U}_i(\mathbf{s};\epsilon) = y_i - E_{0i} - \hat{T}(\mathbf{s};\epsilon) + \hat{A}_i(\mathbf{s};\epsilon_i) \cdot \left\{ \begin{array}{c} \left(\hat{V}_{Ai} + \epsilon_{Ai}\right) + \left(\hat{\lambda}_i(\mathbf{s};\epsilon_i) - \hat{\lambda}_i(\mathbf{s}_p;\epsilon_i)\right) \\ -\gamma_{Ai} - \sum_{j\in\mathcal{J}_i} \hat{I}_{ij}(\mathbf{s};\epsilon_{ij}) \cdot \gamma_{ij} \end{array} \right\}. \tag{13}$$

To understand the terms inside the brackets in Equation (13), recall that $\hat{V}_{Ai} + \epsilon_{Ai}$ is the empirical analogue to $s_{Ai} - c_A + \xi_{Ai} + \lambda_i + \gamma_{Ai}$, the perceived utility from the audit and potential investments. The λ_i in our empirical estimate of $\hat{V}_{Ai}$ is $\hat{\lambda}_i(\mathbf{s}_p;\epsilon_i)$, corresponding to existing program subsidies $\mathbf{s}_p$. Therefore, to simulate $\hat{U}_i$ for a counterfactual subsidy $\mathbf{s}$, we subtract $\hat{\lambda}_i(\mathbf{s}_p;\epsilon_i)$ and add $\hat{\lambda}_i(\mathbf{s};\epsilon_i)$. If we did not do this, utility would be unaffected by a change in investment subsidies. The final two terms account for possible differences between observed takeup and true utility from Equation (1). We subtract γ_{Ai} if consumer i audited, to account for misperceived audit benefits, and we subtract γ_{ij} if consumer i made investment j, to account for misperceived investment benefits. In our base case, where we assume $\gamma_{Ai} = \gamma_{ij}$, Equation (13) is a standard estimate of consumer surplus based on observed market decisions.

Define $\hat{W}(\mathbf{s};\epsilon)$ as the empirical analogue to social welfare from Equation (5) conditional on a draw of ϵ. The empirical analogue to the welfare change in Equation (6) is

$$\Delta\hat{W} = \int \hat{W}(\mathbf{s}_1;\epsilon) - \hat{W}(\mathbf{s}_0;\epsilon)\; d\Phi_2(\epsilon_{Ai}, \epsilon_{ij}, \hat{\rho}). \tag{14}$$

[30]When fitting $\hat{A}_i$ and $\hat{V}_{Ai}$, we make two modifications. First, the latent utility from auditing did not include a term for the (non-experimental) program audit subsidy offer, as this would have been collinear in the estimation. To simulate counterfactuals at different subsidy levels, we re-write the audit takeup intercept as a function of the program audit subsidy s_{Ai} offered to household i: $\kappa_A = \tilde{\kappa}_{Ai} + s_{Ai}$. Second, to reflect conditions in the absence of our RCT, we set $S_{Ei} = G_i = T_i = 0$.

We simulate the integral over 250 draws of ϵ and present the mean of results across draws. Because the sample is so large, results are very similar even with many fewer draws.[31]

We present two tables of results. First, we evaluate the program subsidies and alternative policies under our base case assumptions for ϕ_{ij}, γ_{Ai}, and γ_{ij}. Second, we evaluate the program subsidies under alternative assumptions. The broad array of counterfactuals and assumptions illustrates the applicability of our approach to a broad class of settings.

VIII.B.2 Results: Program Subsidies and Alternative Policies Under Base Case Assumptions

Table 8 reports the simulated effects of alternative residential energy efficiency programs with different audit and investment subsidies. Panel A details the specific program subsidy assumptions, Panel B reports the impacts on households and uninternalized externalities, and Panel C presents welfare impacts.

Column 1 reports the baseline case with audit and investment subsidies set to zero. This counterfactual indicates that the market for home energy audits and retrofits would almost entirely disappear in the absence of government intervention. For example, the results suggest that over the sample period, only 0.008 percent of households would have an audit, and only 0.005 percent of possible energy efficiency investments would be adopted. Across all 101,881 households in the sample, the mean investment expenditures are \$0.21, and the mean value of uninternalized externality benefits ϕ_{ij} from adopted investments is \$0.02. This finding, like the others in the table, requires extrapolation along the audit and investment demand curves, which assume a particular functional form for the joint distribution of ϵ_{Ai} and ϵ_{ij}. Notwithstanding, the qualitative implication seems clear.

Column 2a presents the revealed preference welfare evaluation of the Wisconsin programs at the subsidy levels employed in these programs. Panel A restates that the audit subsidies were \$200 and \$300 in Madison and Milwaukee, respectively, and that we use the linearized investment subsidy of $\$5000\frac{E_{ij}}{E_{0i}}$, i.e. 5,000 times the predicted share of savings in household energy use. The mean investment subsidy offer is 24 percent of investment cost. The estimates in Panel B suggest that the program subsidies effectively create this market, increasing audit and investment takeup rates to 1.17 and 0.65 percent, respectively. The costs of creating the market are also evident: \$14.44 per sample household to fund audit and investment subsidies, or about \$1.5 million in total across all 101,881 sample households. Under these subsidies, the average sample household spends \$40.35 on energy efficiency investments, which sums to \$4.1 million in the full sample. The investments are projected to produce around \$9.62 of uninternalized externality reductions over their lifetimes

[31] When simulating $\Delta\hat{W}$, any household in the sample could be predicted to audit when it receives high simulation draws of ϵ_i, but we do not have characteristics of recommended investments for households that did not audit. For these households, we simulate characteristics of recommended investments with a random draw of the $\mathcal{J}_i$ for a household that did audit. We find no evidence against this approach: as shown in Appendix Table A.14, the costs, energy savings, and count of a household's recommended investments are highly insignificantly correlated with the household's experimental audit subsidy.

per sample household, or about \$0.67 per dollar of subsidies.

Panel C presents the simulated social welfare effects of subsidies compared to the zero-subsidy counterfactual in column 1. The first two rows of Panel C consider the two components of social welfare. The first row reports the change in consumer utility. Per Equation (13), this depends on subsidy amounts, the monetary benefits and costs of investments, draws of ϵ_{Ai} and ϵ_{ij}, and the resulting takeup decisions and lump-sum taxes to fund the subsidies. In this scenario, the existing Wisconsin programs reduce consumer utility (ignoring externalities) by \$12.22 per sample household. All scenarios, including the social optimum, involve a decrease in consumer utility because households are being taxed to induce themselves to make decisions that they would not otherwise make, thus distorting choices from the perceived private optimum. Of course, these losses can be offset with social gains from uninternalized externality reduction. The second row of Panel C shows that relative to the no subsidy counterfactual, the program subsidies induce investments that reduce uninternalized externalities by an average of \$9.61 per sample household.

The social welfare change is the consumer utility change plus the uninternalized externality reduction. In total, the program subsidies generate a social welfare loss of \$2.61 per household, or about \$254,000 in aggregate in our sample. This amounts to a social loss of \$0.18 for every \$1 in subsidy. Thus, from the perspective of our model, the program subsidies are worse than having no subsidies at all.

Great increases in residential energy efficiency investments play an important role in virtually all climate mitigation plans. Column 2b probes the feasibility of achieving this goal by tripling both audit and investment subsidies. To be clear, this scenario is relatively extreme. It involves \$600 and \$900 audit subsidies in Madison and Milwaukee, respectively, so given a market price of \$400, households would *receive* \$200 and \$500 to have an audit. The average investment subsidy offer would be 69 percent of cost. Thus, it is not surprising that this causes 9.94 percent of households to audit over the sample period, or about nine times more than the program subsidies in column 2a.

However, this increase in audits is not matched by an increase in investments: only about one percent of possible investments in the sample are adopted, and the average investment across all households is just shy of \$70. This finding of a much smaller investment response is largely explained by the earlier finding that audit subsidy increases draw in households that are increasingly negatively selected in their interest in making investments. This alternative policy would cost more than \$140 per household in subsidies and would produce only 30 cents of externality reduction per subsidy dollar, significantly lower than the program's actual subsidy. Overall, our model implies that such a subsidy would reduce welfare by about \$39 per sample household, meaning that this sort of program expansion is not justified by this metric.

Columns 3a and 3b explore a different approach to structuring the investment subsidies. Specifically, column 3a considers investment subsidies set to exactly offset the uninternalized externality ϕ_{ij}, while column 3b considers investment subsidies of $2\phi_{ij}$. (To parallel the program subsidies, we also cap these subsidies at \$3500 per investment.) In both columns, there is no audit subsidy.

Except for the \$3500 cap, the column 3a subsidies are socially optimal in our model if $\gamma_{Ai} = \gamma_{ij} = 0$ and energy use is fully price inelastic.

There are three differences between the programs' investment subsidies and the "socially optimal" subsidies in column 3a. First, the program subsidies scale as a share of household energy use instead of the amount of energy conserved. This causes a distortion by encouraging low-usage households to make investments that generate less uninternalized externality reduction. Second, the program subsidies are relatively generous: they cover an average of 24 percent of cost, compared to 16 percent in column 3a. This induces more energy efficiency investment than would be optimal under our uninternalized externality assumptions.

Third, the program subsidies scale in energy savings (in physical energy units such as million British thermal units (mmBtu)) instead of uninternalized externality reductions. For any given fuel, uninternalized externality reductions scale linearly in energy saved. However, our estimates of the retail price distortions per physical unit of energy very different across the three fuels: as shown in Appendix Table A.6, retail price is below social marginal cost by \$1.75 and \$12.01, respectively, per mmBtu for natural gas and heating oil, while retail price exceeds social marginal cost by \$3.18 per mmBtu of electricity. Different recommended investments at different homes save different proportions of the three fuels. Thus, the program subsidies induce investments that save more electricity and less heating oil compared to subsidies that are precisely targeted to the uninternalized externality. Put more simply, the subsidies induce investments with high energy reductions that do not necessarily have high uninternalized externality reductions.

These three factors matter a lot. The subsidies in column 3a generate a social welfare gain of \$21.76 per sample household, or \$2.22 million across the full Wisconsin sample. This welfare gain is accomplished with only \$8.59 per household in subsidies, and there is \$3.06 in uninternalized externality benefit per dollar of subsidy. The primary driver of this improved performance is that the uninternalized social costs of heating oil (primarily from carbon and sulfur emissions) are very large (\$12.01 per mmBtu) compared to those from natural gas and electricity (\$1.75 and -\$3.18 per mmBtu). When we shift from the program investment subsidies in column 2a to investment subsidies equal to ϕ_{ij} in column 3a, the marginal investments tend to be those that conserve significant amounts of heating oil. Even in column 3b, with subsidies that are double the "social optimum" in our model, the welfare effect is positive, although this is naturally less positive than in 3a because subsidies exceeding ϕ_{ij} induce some investments with social costs larger than social benefits.

Columns 4a and 4b focus on the effects of audit subsidies in isolation. Specifically, they present results for counterfactual programs with zero investment subsidies and audit subsidies equal to the actual program amounts (column 4a) and twice the program amounts (column 4b). At both amounts, audit subsidies reduce social welfare. Subsidizing audits is an inefficient way to reduce uninternalized externalities compared to simply subsidizing uninternalized externality reduction.

Comparing column 4b to column 4a sharply illustrates the impact of self-selection – that is, the correlation of ϵ_{Ai} and ϵ_{ij}. The higher subsidy increases audit takeup by a factor of 13, from

0.17 percent to 2.26 percent. However, the investment takeup rate and investment expenditures increase by only a factor of five. The bottom row of Panel B shows that as a result, this subsidy increase generates only four cents of externality benefit per subsidy dollar, compared to 15 cents in column 4a. This underscores a core problem that affects efforts to expand energy efficiency programs by drawing in households with higher audit subsidies: the households that are marginal to these subsidies are more likely to be "tire-kickers" who are not very interested in making efficiency investments.

These simulations have limitations. For example, the joint normal functional form assumption determines how demand responds to counterfactual subsidies, so we must be especially cautious in interpreting these out-of-sample predictions. Additionally, the investment takeup elasticity is estimated from non-experimental data and is lower than our *ex ante* expectations. Furthermore, there is considerable uncertainty in the market failure parameters γ_{Ai}, γ_{ij}, and ϕ_{ij}, which we explore in the next section.

Even noting these limitations, two main qualitative conclusions are likely to be robust. First, it is important to calibrate energy efficiency program subsidies to the market failures that might justify them, and failing to do so can leave surprising amounts of money on the table. This message may be even more important outside of the Wisconsin programs: many other programs across the country offer investment subsidies that scale as a percentage of investment *costs*, thus encouraging high-cost investments with no regard for either energy savings or environmental benefits. Second, subsidizing audits can be an ineffective way to reduce externalities, both because audits don't directly generate externality reduction and because audit subsidy increases draw in consumers who are less and less interested to invest.

VIII.B.3 Results: Program Subsidies Under Alternative Assumptions

Table 9 presents welfare evaluations of the existing program subsidies (i.e. those in column 2a of Table 8) under alternative assumptions for γ_{Ai}, γ_{ij}, and ϕ_{ij}. Panel A describes the alternative assumptions for each scenario, including the assumed γ_{Ai} and the average assumed γ_{ij} and ϕ_{ij} across all recommended investments. Panel B presents the welfare results, paralleling the bottom panel of Table 8.

Column 1 re-prints the base case assumptions, giving the same welfare results as column 2a of Table 8. Column 2 considers a scenario in which energy efficiency improvements are not capitalized into home resale prices due to asymmetric information problems between home sellers and buyers. While we do not formally model the details of this asymmetric information problem, we can capture its implications: existing homeowners that improve energy efficiency effectively impose positive externalities on future home buyers who receive a more energy efficient home without paying a higher price. As a benchmark, we add a positive externality of 40 percent of the present discounted value of empirically-adjusted retail energy cost savings.[32] Under this assumption, the program

[32]This is roughly consistent with a homeowner who sells after 10 years and capitalizes none of the investment in the sale price, giving the buyer another ten years of a 20-year investment lifetime at a five percent annual discount

subsidies generate 41 cents of net social welfare gains for each subsidy dollar spent.

Column 3 presents results after doubling the social cost of carbon, using \$78 instead of \$39. The average environmental externality increases by less than a factor of two, because externalities include local air pollutant emissions as well as carbon, but the average *uninternalized* externality (i.e. environmental externality net of the retail energy price markup) more than doubles. The program subsidies now significantly increase welfare, generating 67 cents in net social welfare gains per subsidy dollar.

Columns 4-6 take into account the results in Section VII that the simulation predictions overstate natural gas savings and understate electricity savings. In column 4, we empirically adjust only the uninternalized externality ϕ_{ij}. This is not the same as simply reducing all ϕ_{ij} by the 58 percent realization rate calculated earlier. Instead, the empirically-adjusted average ϕ_{ij} of recommended investments happens to be almost exactly zero, meaning that on average, the energy conserved by recommended investments is priced very close to social marginal cost. This happens because we calculate that the retail marginal price is *above* social marginal cost for electricity, while it is below social marginal cost for natural gas. The empirical adjustment weights electricity substantially more, and gas substantially less, so this re-weighting reduces the average uninternalized externality. Naturally, the substantial reduction in uninternalized externality benefits substantially worsens the program's estimated welfare effects.

Column 5 takes the next step and assumes that consumers took the audit report's energy savings predictions at face value, whereas only the empirically-adjusted savings were realized. This misperception implies deadweight loss, because consumers made investments that they would not have made if they knew that the true savings were lower. We accommodate this in the model by calculating the "actual savings" for investment ij (by multiplying the simulation predictions of savings on the audit report by the fuel-specific realization rates from Table 6), then setting γ_{ij} equal to the simulation predictions minus the "actual savings." In this scenario, the program subsidies generate a large social welfare loss of \$1.92 per subsidy dollar, or \$2.8 million for the full sample. This starkly highlights how important it is for energy efficiency programs to accurately inform consumers about how much energy they can expect to save.

Column 6 repeats column 5, except after doubling the social cost of carbon. This somewhat improves the welfare results, although the improvement is attenuated by the fact that the carbon savings are attenuated by the empirically estimated realization rates.

In column 7, we leave aside our own assumptions for γ_{Ai}, γ_{ij}, and ϕ_{ij} and assume that the program subsidies were optimally calibrated: that is, that γ_{Ai} is \$200 (\$300) in Madison (Milwaukee), and $\phi_{ij} = \$5000\frac{E_{ij}}{E_{i0}}$, truncated at \$3500. Under these assumptions, the existing program subsidies would be socially optimal, and they would achieve the first best in our model if home energy use were fully price inelastic. The welfare gains amount to \$2.17 per sample household, or \$220,000 for the full sample.

rate.

VIII.C Discussion

Both the "accounting" and "revealed preference" welfare approaches ignore important additional issues. First, the "revealed preference" model abstracts away from dynamic considerations. At least in theory, consumers might delay such investments due to uncertainty about future energy prices (Metcalf and Rosenthal 1995), although we have seen no empirical or anecdotal evidence that this actually happens. Furthermore, consumers' ξ_{Ai} and ξ_{ij} could vary over time, for example as they get a raise, move into a new house, or because their interest in home improvements varies with the age of their house or their personal circumstances. We think of our takeup estimates as reflecting the takeup that would be expected over a period like our sample period, and we could expect additional takeup over a longer period as households receive new draws of ξ_{Ai} and ξ_{ij}.

Second, the calculations exclude administrative and marketing costs. In total, the Wisconsin Better Buildings programs facilitated \$25.4 million in retrofit costs and also incurred \$528,000 in marketing and outreach and \$11.2 million in other program expenses (DOE 2015b). Applying this 0.46 ratio of overhead/retrofit costs to the investment costs in the experimental sample gives overhead costs of \$2.1 million. This reduces the benefit/cost ratios in Table 7 to 0.65 and 0.30 with the simulation predictions and empirical adjustment, respectively. \$2.1 million is just slightly less than the welfare gains from the "socially-optimal" subsidies in Column 3a of Table 8.

Third, part of the goal of the Better Buildings programs was to make investments that would support an economically sustainable home retrofit market. On the basis of their budget records, WECC staff tell us that more than \$7.9 million of WECC's \$20 million EECBG grant was spent in ways that continued to support local retrofit markets after the grant ended, including a loan loss reserve, program design efforts, technical training and mentoring for contractors, and funding for cities to continue parts of the programs (Curtis 2017). This does not affect the interpretation of Tables 7 and 8, as those include only direct audit and investment costs, but it means that the overhead costs discussed in the above paragraph have additional benefits that we have not quantified.

Fourth, the Better Buildings programs were funded with economic stimulus dollars, and job creation was an important motivation.[33] Our analysis is designed to ask whether these programs would be welfare enhancing in the absence of a macroeconomic stimulus benefit.

IX Conclusion

This paper lays out a framework to evaluate the welfare impacts of residential energy efficiency programs in the presence of imperfect information, behavioral biases, and externalities, and implements the framework with the help of a 100,000-household randomized field experiment. This exercise demonstrates that standard revealed preference analysis can be used to evaluate residential energy efficiency programs and that the results can be used to assess a wide range of policies.

[33] DOE (2015c) finds that BBNP created or retained net 10,191 full-time-equivalent jobs, or about one job for every \$44,000 in federal outlays.

The empirical results are remarkable in that they run counter to many aspects of the conventional wisdom about energy efficiency programs. First, the data provide no statistically or economically significant evidence of any of the six potential informational and behavioral failures that were tested. Second, consumer decisions imply large unobserved benefits and costs that conventional accounting-style evaluations do not measure. Third, there is evidence of strong self-selection in these programs, such that it is less socially beneficial to increase audit subsidies because the marginal participants are less likely to make externality-reducing investments. Fourth, the estimated energy savings from energy efficiency investments are only 58 percent of predicted savings. Fifth, the programs reduced welfare. In the accounting-style evaluation, the programs have an internal rate of return of negative 4.1 percent. In our revealed preference model, the programs reduce welfare by $0.18 per subsidy dollar. The welfare results demonstrate the significant implications of even subtle-seeming policy design issues.

While these results may not be encouraging for the program structure we evaluated, the counterfactual simulations suggest that if Pigouvian taxes or other more direct approaches are not feasible, there are significant opportunities for energy efficiency programs to increase welfare. Specifically, we find that a program with perfectly-calibrated subsidies could increase welfare by $2.53 per subsidy dollar. It is apparent that residential energy efficiency programs have significant potential, but reaching this potential requires restructuring policies to better target the market failures that motivate them.

References

[1] Abrahams, Leslie S., Constantine Samaras, W. Michael Griffin, and H. Scott Matthews. 2015. "Life Cycle Greenhouse Gas Emissions From U.S. Liquefied Natural Gas Exports: Implications for End Uses." *Environmental Science and Technology* 49: 3237-3245.

[2] Allcott, Hunt (2013). "The Welfare Effects of Misperceived Product Costs: Data and Calibrations from the Automobile Market." *American Economic Journal: Economic Policy*, Vol. 5, No. 3 (August), pages 30-66.

[3] Allcott, Hunt, and Michael Greenstone (2012). "Is There an Energy Efficiency Gap?" *Journal of Economic Perspectives*, Vol. 26, No. 1 (Winter), pages 3-28.

[4] Allcott, Hunt, and Richard Sweeney (2016). "The Role of Sales Agents in Information Disclosure: Evidence from a Field Experiment." *Management Science*, forthcoming.

[5] Allcott, Hunt, and Dmitry Taubinsky (2015). "Evaluating Behaviorally-Motivated Policy: Experimental Evidence from the Lightbulb Market." *American Economic Review*, Vol. 105, No. 8 (August), pages 2501-2538.

[6] Alliance to Save Energy (2013). "Energy Efficiency and the President's Climate Action Plan." Available from http://www.ase.org/resources/energy-efficiency-and-presidents-climate-action-plan. Accessed September 13, 2016.

[7] Altonji, Joseph (1993). "The Demand for and Return to Education When Education Outcomes Are Uncertain." *Journal of Labor Economics*, Vol. 11, No. 1 (August), pages 48-83.

[8] American Council for an Energy Efficient Economy (2015). "Section 111(d) of the Clean Air Act." Available from http://aceee.org/topics/section-111d-clean-air-act. Accessed June 8, 2015.

[9] Attari, Shahzeen, Michael DeKay, Cliff Davidson, and Wandi Bruine de Bruin (2010). "Public Perceptions of Energy Consumption and Savings." *Proceedings of the National Academy of Sciences*, Vol. 107, pages 16054-16059.

[10] Berry, Linda (1984). "The Role of Financial Incentives in Utility-Sponsored Residential Conservation Programs: A Review of Customer Surveys." *Evaluation and Program Planning*, Vol. 7, No. 2, pages 131-141.

[11] Bertrand, Marianne, Dean Karlan, Sendhil Mullainathan, Eldar Shafir, and Jonathan Zinman (2010). "What's Advertising Content Worth? Evidence from a Consumer Credit Marketing Field Experiment." *Quarterly Journal of Economics*, Vol. 125, No. 1 (February), pages 263-306.

[12] Bhargava, Saurabh, and Dayanand Manoli (2015). "Psychological Frictions and the Incomplete Take-Up of Social Benefits: Evidence from an IRS Field Experiment." *American Economic Review*, Vol. 105, No. 11 (November), pages 3489-3529.

[13] Billingsley, Megan, Ian Hoffman, Elizabeth Stuart, Steven Schiller, Charles Goldman, and Kristina LaCommare (2014). "The Program Administrator Cost of Saved Energy for Utility Customer-Funded Energy Efficiency Programs." Lawrence Berkeley National Laboratory Report No. 6595E. https://emp.lbl.gov/sites/all/files/lbnl-6595e.pdf

[14] Blasnik, Michael (2010). "Energy Models vs. Reality." Presentation, Southface Lecture Series (January).

[15] Bloom, Nicholas, Benn Eifert, Aprajit Mahajan, David McKenzie, and John Roberts (2013). "Does Management Matter? Evidence from India." *Quarterly Journal of Economics*, Vol. 128, No. 1 (February), pages 1-51.

[16] Borenstein, Severin, and Lucas Davis (2012). "The Equity and Efficiency of Two-Part Tariffs in U.S. Natural Gas Markets." *Journal of Law and Economics*, Vol. 55, No. 1, pages 75-128.

[17] Bronnenberg, Bart, Jean-Pierre Dubé, Matthew Gentzkow, and Jesse Shapiro (2015). "Do Pharmacists Buy Bayer? Sophisticated Shoppers and the Brand Premium." *Quarterly Journal of Economics*, Vol. 130, No. 4 (November), pages 1669-1726.

[18] Bruhn, Miriam, and David McKenzie (2009). "In Pursuit of Balance: Randomization in Practice in Development Field Experiments." *American Economic Journal: Applied Economics*, Vol. 1, No. 4 (October), pages 200-232.

[19] Bryan, Gharad, Shyamal Chowdhury, and Ahmed Mushfiq Mobarak (2014). "Underinvestment in a Profitable Technology: The Case of Seasonal Migration in Bangladesh." *Econometrica*, Vol. 82, No. 5 (September), pages 1671-1748.

[20] Camilleri, Adrian R., and Richard P. Larrick (2014). "Metric and Scale Design as Choice Architecture Tools." *Journal of Public Policy & Marketing*, Vol. 33, No. 1 (Spring), pages 108-125.

[21] CEE (Consortium for Energy Efficiency) (2013, 2015). "State of the Efficiency Program Industry." Boston MA: Consortium for Energy Efficiency. Available from http://www.cee1.org/annual-industry-reports. Accessed May 20, 2015.

[22] Chamberlain, Gary (1980). "Analysis of Covariance with Qualitative Data." *Review of Economic Studies*, Vol. 47, pages 225-238.

[23] Chetty, Raj, Adam Looney, and Kory Kroft (2009). "Salience and Taxation: Theory and Evidence." *American Economic Review*, Vol. 99, No. 4 (September), pages 1145-1177.

[24] Coffman, Lucas, Clayton Featherstone, and Judd Kessler (2016). "Can Social Information Affect What Job You Choose and Keep?" *American Economic Journal: Applied Economics*, forthcoming.

[25] Curtis, Monica (2017). Personal communication. Janary 13th.

[26] Davis, Lucas (2012). "Evaluating the Slow Adoption of Energy Efficient Investments: Are Renters Less Likely to Have Energy-Efficient Appliances?" in The Design and Implementation of U.S. Climate Policy, Don Fullerton and Catherine Wolfram, Eds. Chicago, IL: University of Chicago Press.

[27] Davis, Lucas, and Erich Muehlegger (2010). "Do Americans Consume Too Little Natural Gas? An Empirical Test of Marginal Cost Pricing." *RAND Journal of Economics*, Vol. 41, No. 4, pages 791-810.

[28] Davis, Lucas, and Gilbert Metcalf (2016). "Does Better Information Lead to Better Choices? Evidence from Energy-Efficiency Labels." *Journal of the Association of Environmental and Resource Economists*, Vol. 3, No. 3 (September), pages 589-625.

[29] DOE (U.S. Department of Energy) (2014). http://energy.gov/eere/wipo/energy-efficiency-and-conservation-block-grant-program. Accessed May 14, 2014.

[30] DOE (U.S. Department of Energy) (2015a). "Better Buildings Neighborhood Program: History." http://energy.gov/eere/better-buildings-neighborhood-program/history. Accessed June 8, 2015.

[31] DOE (U.S. Department of Energy) (2015b). "Wisconsin Data Dashboard." http://energy.gov/eere/better-buildings-neighborhood-program/downloads/wisconsin-data-dashboard. Accessed June 13, 2015.

[32] DOE (U.S. Department of Energy) (2015c). "Evaluation of the Better Buildings Neighborhood Program." http://www1.eere.energy.gov/analysis/pdfs/bbnp_volume_1_final_evaluation_072215.pdf. Accessed August 18, 2015.

[33] Dubin, Jeffrey, and Daniel McFadden (1984). "An Econometric Analysis of Residential Electric Appliance Holdings and Consumption." *Econometrica*, Vol. 52, No. 2 (March), pages 345-362.

[34] Duflo, Esther, Michael Kremer, and Jonathan Robinson (2011). "Nudging Farmers to Use Fertilizer: Theory and Experimental Evidence from Kenya." *American Economic Review*, Vol. 101, No. 6, pages 2350-2390.

[35] Dupas, Pascaline (2013). "Short-Run Subsidies and Long-Run Adoption of New Health Products: Evidence from a Field Experiment." *Econometrica*, Vol. 82, No. 1 (January), pages 197-228.

[36] Dupas, Pascaline, and Jonathan Robinson (2013). "Why Don't the Poor Save More? Evidence from Health Savings Experiments." *American Economic Review*, Vol. 103, No. 4 (June), pages 1138-1171.

[37] EIA (U.S. Energy Information Administration) (2015). "Annual Energy Outlook." Available from http://www.eia.gov/forecasts/aeo/. Accessed September 3, 2015.

[38] EPA (U.S. Environmental Protection Agency) (1995). "AP 42, Ffth Edition: Compilation of Air Pollutant Emission Factors, Volume 1: Stationary Point and Area Sources. Chapter 1: External Combustion Sources." Available from http://www.epa.gov/ttn/chief/ap42/ch01/index.html. Accessed June 12, 2015.

[39] Foster, Andrew, and Mark Rosenzweig (2010). "Microeconomics of Technology Adoption." *Annual Review of Economics*, Vol. 2, pages 395-424.

[40] Fowlie, Meredith, Michael Greenstone, and Catherine Wolfram (2015a). "Are the Non-Monetary Costs of Energy Efficiency Investments Large? Understanding Low Take-up of a Free Energy Efficiency Program." *American Economic Review, Papers and Proceedings*, forthcoming.

[41] Fowlie, Meredith, Michael Greenstone, and Catherine Wolfram (2015b). "Do Energy Efficiency Investments Deliver? Evidence from the Weatherization Assistance Program." Working Paper, University of Chicago (June).

[42] Gillingham, Kenneth (2014). "Identifying the Elasticity of Driving: Evidence from a Gasoline Price Shock in California." *Regional Science and Urban Economics*, Vol. 47 (July), pages 13-24.

[43] Gillingham, Kenneth, Matthew Harding, and David Rapson (2012). "Split Incentives and Household Energy Consumption." *The Energy Journal*, Vol. 33, No. 2, pages 37-62.

[44] Gillingham, Kenneth, Richard Newell, and Karen Palmer (2009). "Energy Efficiency Economics and Policy." *Annual Review of Resource Economics*, Vol. 1, pages 597-619.

[45] Greenstone, Michael, Elizabeth Kopits, and Ann Wolverton (2013). "Developing a Social Cost of Carbon for US Regulatory Analysis: A Methodology and Interpretation." *Review of Environmental Economics and Policy*, Vol. 7, No. 1, pages 23-46.

[46] Hanna, Rema, Esther Duflo, and Michael Greenstone (2016). "Up in Smoke: The Influence of Household Behavior on the Long-Run Impact of Improved Cooking Stoves." *American Economic Journal: Economic Policy*, Vol. 8, No. 1 (February), pages 80-114.

[47] Heckman, James (1979). "Sample Selection Bias as a Specification Error." *Econometrica*, Vol. 47, No. 1 (January), pages 153-161.

[48] Holland, Stephen, Erin Mansur, Nicholas Muehller, and Andrew Yates (2015). "Measuring the Spatial Heterogeneity in Environmental Externalities from Driving: A Comparison of Gasoline and Electric Vehicles." NBER Working Paper No. 21291 (June).

[49] Howarth, Robert, Drew Shindell, Renee Santoro, Anthony Ingraffea, Nathan Phillips, and Amy Townsend-Small. (2012). "Methane Emissions from Natural Gas Systems." Available from http://www.eeb.cornell.edu/howarth/publications/Howarth_et_al_2012_National_Climate_Assessment.pdf. Accessed August 15, 2015.

[50] Interagency Working Group on the Social Cost of Carbon (2013). "Technical Support Document - Technical Update of the Social Cost of Carbon for Regulatory Impact Analysis - Under Executive Order 12866." Available from http://www.whitehouse.gov/sites/default/files/omb/inforeg/social_cost_of_carbon_for_ria_2013_update.pdf

[51] International Energy Agency (2015). "World Energy Outlook 2015." Available from http://www.worldenergyoutlook.org/weo2015/. Accessed August 18, 2016.

[52] Jaffe, Adam, and Robert Stavins (1994). "The Energy-Efficiency Gap: What Does it Mean?" *Energy Policy*, Vol. 22, No. 10, pages 804-810.

[53] Kushler, Martin, Seth Nowak, and Patti Witte (2012). "A National Survey of State Policies and Practices for the Evaluation of Ratepayer-Funded Energy Efficiency Programs." American Council for an Energy-Efficient Economy Report Number U122 (February).

[54] Mandel, Naomi, and Eric Johnson (2002). "When Web Pages Influence Choice: Effects of Visual Primes on Experts and Novices." *Journal of Consumer Research*, Vol. 29, pages 235–245.

[55] Manski, Charles (1988). "Should We Subsdize Enrollment in or Completion of Postsecondary Schooling?" Available from http://files.eric.ed.gov/fulltext/ED315523.pdf.

[56] McKinsey & Co. (2009). "Unlocking Energy Efficiency in the U.S. Economy." Available from http://www.mckinsey.com/clientservice/electricpowernaturalgas/downloads/US_energy_efficiency_full_report.pdf. Accessed June 25, 2014.

[57] Metcalf, Gilbert, and Donald Rosenthal (1995). "The "New" View of Investment Decisions and Public Policy Analysis: An Application to Green Lights and Cold Refrigerators." Journal of Policy Analysis and Management, Vol. 14, No. 4 (Autumn), pages 517-531.

[58] Meyer, David, and Roger Schvaneveldt (1971). "Facilitation in Recognizing Pairs of Words: Evidence of a Dependence Between Retrieval Operations." *Journal of Experimental Psychology*, Vol. 90, pages 227-234.

[59] Mullainathan, Sendhil, Joshua Schwartzstein, and William Congdon (2012). "A Reduced-Form Approach to Behavioral Public Finance." *Annual Review of Economics*, Vol. 4, pages 17.1-17.30.

[60] Myers, Erica (2015). "Asymmetric Information in Residential Rental Markets: Implications for the Energy Efficiency Gap." Working Paper, UC Berkeley (September). Available from https://ei.haas.berkeley.edu/research/papers/WP246.pdf

[61] Nadel, Steven, and Kenneth Keating (1991). "Engineering Estimates vs. Impact Evaluation Results: How Do They Compare and Why?" Research Report U915, American Council for an Energy-Efficient Economy. Available from http://www.aceee.org/research-report/u915

[62] National Association of State Energy Officials (2015). "State 111(d) Resource Hub." Available from http://111d.naseo.org/. Accessed June 8, 2015.

[63] Newell, Richard, and Juha Siikamaki (2013). "Nudging Energy Efficiency Behavior: The Role of Information Labels." NBER Working Paper No. 19224 (July).

[64] NOAA (National Oceanic and Atmospheric Administration) (2015). "Climate Data Online." Available from http://www.ncdc.noaa.gov/cdo-web/search. Accessed June 14, 2015.

[65] Pope, C. Arden, Richard T. Burnett, Michael J. Thun, Eugenia, E. Calle, Daniel Krewski, Kazuhiko Ito, and George D. Thurston (2002). "Lung Cancer, Cardiopulmonary Mortality, and Long-term Exposure to Fine Particulate Air Pollution." *Journal of the American Medical Association*, Vol. 287, No. 9 (March 6), pages 1132-1141.

[66] Potomac Economics (2012-2015). "State of the Market Report for the MISO Electricity Markets." Available from https://www.misoenergy.org/MarketsOperations/IndependentMarketMonitor/Pages/IndependentMarketMon
Accessed August 18, 2015.

[67] PSD (Performance Systems Development) (2015a). "HERS BESTEST & DOE APPROVED." Available from http://psdconsulting.com/software/treat/hers-bestest-doe-approved/. Accessed June 8, 2015.

[68] PSD (Performance Systems Development) (2015b). "NYSERDA Home Performance with Energy Star Realization Rate Attribution Study." Prepared for New York State Energy Research and Development Authority. Ithaca, NY: Performance Systems Development.

[69] Sanstad, Alan, Michael Hanemann, and Maximilian Auffhammer (2006). "End-use Energy Efficiency in a "Post-Carbon" California Economy: Policy Issues and Research Frontiers." In Michael Hanemann and Alexander Farrell, Eds., Managing Greenhouse Gas Emissions in California. Berkeley, CA: The California Climate Change Center at UC Berkeley.

[70] Skumatz, Lisa (2008). "Non-Energy Benefits Publications / Bibliography for Skumatz / SERA." Available from http://www.serainc.com/NEBPublicationsforWebSite2008.pdf. Accessed July 9, 2015.

[71] Suri, Tavneet (2011). "Selection and Comparative Advantage in Technology Adoption." *Econometrica*, Vol. 79, No. 1, pages 159–209.

[72] Turrentine, Thomas, and Kenneth Kurani (2007). "Car Buyers and Fuel Economy?" *Energy Policy*, Vol. 35, pages 1213-1223.

[73] Van de Ven, Wynand, and Bernard Van Praag (1981). "The Demand for Deductibles in Private Health Insurance." *Journal of Econometrics*, Vol. 17, pages 229-252.

[74] Yergin, Daniel (1979). Energy Future: The Report of the Energy Project at the Harvard Business School. New York: Random House.

Tables and Figures

Table 1: **Overview of Related Energy Efficiency Programs**

Panel A: Policy Overview

	(1)	(2)	(3)	(4)
	Better Buildings Neighborhood Program (2010-2013)	Energy Efficiency and Conservation Block Grants (total)	U.S. and Canada Ratepayer-Funded Efficiency Programs (year 2013)	Weatherization Assistance Program (per year, pre-stimulus)
Number of programs	41	∼2000	347	∼400
Public expenditures	\$508 million	\$3.2 billion	\$8.0 billion	\$250 million
Buildings retrofitted	119,404	86,000		100,000
Value of predicted energy saved	\$669 million		\$2.95 billion	\$453 million

Panel B: Program Evaluation Assumptions

	Share of programs
Include non-monetary benefits	30%
Use simulation predictions of savings	97%
of which: Use simulation predictions from other states	70%
Do not evaluate programs retrospectively	81%

Notes: "Public expenditures" includes costs of program operation, including administrative costs and subsidies, but not any investment costs paid by consumers. Better Buildings Neighborhood Program information in column 1 is from DOE (2015b,c). Energy Efficiency and Conservation Block Grant information in column 2 is from DOE (2014). U.S. and Canada ratepayer-funded program information in column 3 is from CEE (2015), and energy savings are those that accrue in 2013 only. Weatherization Assistance Program information in column 4 is from DOE (2011,2015) and Eisenberg (2010). Program evaluation assumptions are from Kushler, Nowak, and Witte (2012).

Table 2: **Summary Statistics**

Variable	Mean	Std. Dev.	Min.	Max.
Panel A: Wisconsin Household Data *(N=101,881 households in experimental population)*				
House age (years)	67.2	23.0	0	182
Property value ($000s)	156	90.7	0	2816
Building footprint (sq. feet/1000)	1.24	0.43	0	12.1
Madison	0.31	0.46	0	1
Census tract hybrid vehicle share	1.20	1.26	0	8.33
Audited	0.014	0.12	0	1
Invested	0.008	0.09	0	1
Panel B: Wisconsin Simulation Estimates of Monetary Costs and Benefits *Recommended investments (N=6100 at the 1394 households that had audits)*				
Cost ($)	1493	1472	17.1	43,600
Retail energy cost savings ($/year)	86.1	145	0.01	4359
Invested	0.51	0.50	0	1
Adopted investments (N=3834 at the 1394 households that had audits)				
Cost ($)	1206	1208	0.5	14,475
Retail energy cost savings ($/year)	79.9	134	-526	2501
Panel C: Wisconsin Electricity and Natural Gas Usage *N=1212 households (gas); N=1217 households (electricity)*				
Number of gas bills observed	51.9	14.2	9	88
Average gas use (therms/day)	2.50	0.98	0.20	9.59
Number of electricity bills observed	53.2	14.2	8	88
Average electricity use (kWh/day)	20.6	9.27	4.16	80.6

Notes: Recommended investments in Panel B include only those used for empirical estimates of investment takeup, and adopted investments in Panel B include only those used for the "accounting approach" to welfare analysis. Energy bills are observed only if a household had an audit. Energy prices are averages over 2011-2014. See Appendix B for more details.

Table 3: **Effects of Letter and Subsidy Treatments on Audit and Investment Takeup**

	(1)	(2)
Dependent Variable:	Audited	Invested
Received letter	0.158	0.041
	(0.093)*	(0.070)
Experiment audit subsidy ($00s)	0.525	0.152
	(0.146)***	(0.119)
Subsidy: $25 gift card	-0.006	-0.125
	(0.101)	(0.081)
House age (years)	0.014	0.009
	(0.001)***	(0.001)***
Property value ($millions)	0.750	0.204
	(0.439)*	(0.376)
Building footprint (sq. feet/1000)	0.326	0.130
	(0.076)***	(0.062)**
Madison	0.171	0.018
	(0.113)	(0.089)
Census tract hybrid vehicle share	0.198	0.122
	(0.039)***	(0.031)***
N	101,881	101,881
Dependent variable mean (percent)	1.4	.8

Notes: This table presents estimates of Equation (8), a probit model using the sample of all households in the Wisconsin experiment. We present marginal effects, with coefficients multiplied by 100 for readability. Audited is an indicator for whether the household had a home energy audit, and Invested is an indicator for whether the household made any energy efficiency investment. Robust standard errors in parentheses. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

Table 4: **Post-Audit Investment Takeup Estimates**

	(1)	(2)	(3)	(4)	(5)	(6)
Subsidy - Cost + Savings (\$000s)	0.059	0.059		0.028	0.059	0.059
	(0.006)***	(0.006)***		(0.007)***	(0.006)***	(0.006)***
Constant	0.019	0.132	0.134	0.111	0.133	0.151
	(0.012)	(0.056)**	(0.056)**	(0.056)**	(0.056)**	(0.056)***
Energy Savings PDV (\$000s)			0.056			
			(0.007)***			
Subsidy - Cost (\$000s)			0.062			
			(0.007)***			
Closed account					0.026	
					(0.083)	
Experiment audit subsidy (\$00s)						-0.129
						(0.046)***
N	5,990	5,990	5,990	6,100	5,990	5,990
Household covariates	No	Yes	Yes	Yes	Yes	Yes
Exclude outliers	Yes	Yes	Yes	No	Yes	Yes

Notes: This table presents marginal effects estimates of Equation (9), a probit model using the sample of recommended investments in the Wisconsin experiment. All columns except for column 4 exclude outliers with net private monetary benefits larger than \$5,000 in absolute value. The additional variable in column 5, Closed account, is an indicator for the 124 investments at 37 households that closed their gas and electric accounts more than six months after the audit but before our data end. Robust standard errors in parentheses, clustered by household. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

Table 5: **Joint Estimates of Audit and Investment Takeup**

	(1)	(2)	(3)	(4)
	Independent Probits		Joint Estimates	
Audit Parameters				
Received letter	0.0470	0.0482	0.0295	0.0318
	(0.0279)*	(0.0282)*	(0.0253)	(0.0254)
Experiment audit subsidy ($00s)	0.156	0.160	0.178	0.182
	(0.0438)***	(0.0443)***	(0.0436)***	(0.0447)***
Subsidy: $25 gift card	-0.00131	-0.00169	0.0376	0.0372
	(0.0304)	(0.0307)	(0.0280)	(0.0281)
Constant	-2.260	-2.813	-2.265	-2.833
	(0.0236)***	(0.0462)***	(0.0224)***	(0.0459)***
Household covariates	No	Yes	No	Yes
Investment Parameters				
Subsidy - Cost + Savings ($000s)	0.147	0.119	0.0619	0.0497
	(0.0150)***	(0.0173)***	(0.0213)***	(0.0182)***
Constant	0.0485		-2.363	
	(0.0308)		(0.141)***	
Investment category indicators	No	Yes	No	Yes
Household covariates	No	Yes	No	Yes
$\hat{\rho}$			0.941	0.944
			(0.0599)***	(0.0608)***

Notes: Columns 1 and 2 present the independent probit estimates of Equations (8) and (9), while columns 3 and 4 present estimates of Equation (10) using the maximum likelihood estimator of Van de Ven and Van Praag (1981). These are coefficients, not marginal effects. All estimates exclude outlier investments with net private monetary benefits larger than $5,000 in absolute value. $\hat{\rho}$ is the estimated correlation between the errors in the audit and investment takeup equations. Robust standard errors in parentheses, clustered by household. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

Table 6: **Post-Audit Energy Use Changes**

	(1)	(2)	(3)	(4)
	Natural Gas (therms/day)		Electricity (kWh/day)	
Dependent Variable:	Simulation Prediction	Energy Use	Simulation Prediction	Energy Use
Post audit (<6 months)	-0.284	-0.093	-0.296	-0.874
	(0.026)***	(0.029)***	(0.026)***	(0.238)***
Post audit ($\geq$6 months)	-0.444	-0.128	-0.408	-1.013
	(0.032)***	(0.036)***	(0.036)***	(0.319)***
N	61,845	61,845	63,655	63,654

Notes: This table presents estimates of Equation (12) with daily usage of natural gas and electricity, respectively, as the dependent variables. Columns 1 and 3 have the simulation predictions as the dependent variable, while columns 2 and 4 have energy use as the dependent variable. Mean pre-audit natural gas usage is 2.40 therms/day, and mean pre-audit electricity usage is 21.4 kWh/day. Average marginal natural gas price is $0.82 per therm, and average marginal electricity price is $0.136 per kWh. All columns control for heating and cooling degrees, household-by-calendar month fixed effects, and month-of-sample fixed effects. Robust standard errors in parentheses, clustered by household. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

Table 7: **Welfare Effects: Accounting Approach**

	(1)	(2)
Source of energy savings estimates:	Simulation Predictions	Empirically Adjusted
Cost ($millions)		
Audit costs (at $400 per audit)	0.56	
Investment costs	4.52	
Total cost	5.08	
Energy Savings ($millions present value at 5% discount rate)		
Natural gas	1.92	0.56
Electricity	0.07	0.18
Heating oil	0.61	0.18
Total	2.61	0.92
Externality Reduction ($millions present value at 5% discount rate)		
Climate (at $39 per ton CO2)	1.37	0.57
SO2/NOx/PM	0.72	0.68
Total	2.09	1.26
Summary		
Benefits - Costs ($millions)	-0.39	-2.91
Benefit/Cost ratio	0.92	0.43
Internal rate of return (percent)	4.0	-4.1

Notes: Column 1 uses energy savings projected by simulation models, while column 2 adjusts for empirically-observed savings. Column 2 multiplies electricity and gas/heating oil savings from Column 1 by 2.48 and 0.29, respectively, based on the estimates in Table 6. Energy savings are calculated at average wholesale prices over 2011-2014, and externality reductions are based on a $39 social cost of carbon and a $6 million value of a statistical life; see Appendix B for details.

Table 8: **Welfare Effects: Revealed Preference Approach**

	(1)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Scenario:	No Subsidy	Program Subsidy		Externality Subsidy Only		Audit Subsidy Only	
Panel A: Program Subsidy Assumptions							
Audit subsidy in Madison/Milwaukee	$0	$200/300	$600/900	$0	$0	$200/300	$400/600
Investment subsidy multiplier on share energy saved	–	5,000	15,000	–	–	–	–
Investment subsidy multiplier on externality reduction	–	–	–	1	2	–	–
Panel B: Program Impacts on Households and Externalities							
Audit probability	0.008%	1.17%	9.94%	0.27%	0.70%	0.17%	2.26%
Investment probability	0.005%	0.65%	1.03%	0.11%	0.40%	0.03%	0.16%
Subsidy paid per sample household	$0	$14.44	$141.95	$8.59	$17.66	$0.59	$13.29
Investment expenditures per sample household	$0.21	$40.35	$68.92	$11.69	$31.43	$1.39	$6.42
Uninternalized externality benefit from investments per sample household	$0.02	$9.62	$42.94	$26.28	$30.15	$0.09	$0.54
Uninternalized externality benefit per dollar subsidy	--	0.67	0.30	3.06	1.71	0.15	0.04
Incremental uninternalized externality benefit per incremental dollar subsidy (relative to previous column)		0.67	0.26		0.43		0.04
Panel C: Program Impacts on Welfare (Relative to No Subsidy)							
ΔConsumer utility (net of tax) per sample household		-$12.22	-$82.18	-$4.51	-$10.45	-$0.40	-$10.31
ΔUninternalized externality per sample household		$9.61	$42.93	$26.27	$30.14	$0.08	$0.52
ΔWelfare per sample household		-$2.61	-$39.25	$21.76	$19.69	-$0.33	-$9.79
ΔWelfare per dollar subsidy		-0.18	-0.28	2.53	1.11	-0.55	-0.74
Incremental Δwelfare per incremental dollar subsidy (relative to previous column)		-0.18	-0.29		-0.23		-0.75

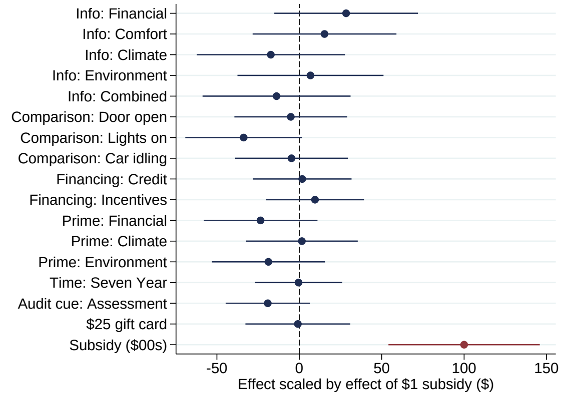
Notes: This table reports averages across 250 simulated draws of ϵ. "Program Subsidy" refers to an investment subsidy that scales in the share of household energy saved. "Externality Subsidy" refers to an investment subsidy that scales in uninternalized externality reduction. Both subsidies are restricted to be between $0 and $3,500 per investment. Investment probability divides the number of adopted investments by the total number of possible investments that could be made by the 101,881-household sample. "Per sample household" means per household in the 101,881-household Wisconsin sample. Uninternalized externality reductions are discounted at a five percent discount rate using the programs' assumed investment lifetimes and simulation predictions of savings, based on a $39 social cost of carbon and a $6 million value of a statistical life; see Appendix B for details. In Panel C, consumer utility is from Equation (13), and ΔWelfare is from Equation (14).

Table 9: **Welfare Effects of Program Subsidies Under Alternative Market Failure Assumptions**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Scenario:	Base Case	40% of Retail Energy Savings Not Capitalized	Double Social Cost of Carbon	Empirically Adjust Externality	Empirically Adjust Savings and Externality	Empirically Adjust and Double the SCC	Distortions Equal Program Subsidies
Panel A: Market Failure Assumptions							
Audit distortion γ_{Ai} in Madison/Milwaukee	\$0	\$0	\$0	\$0	\$0	\$0	\$200/300
Mean investment distortion γ_{ij}	\$0	\$0	\$0	\$0	\$489	\$489	\$0
Mean investment uninternalized externality ϕ_{ij}	\$272	\$516	\$668	-\$3	-\$3	\$159	\$365
Panel B: Program Impacts on Welfare (Relative to No Subsidy)							
ΔConsumer utility (net of tax) per sample household	-\$12.22	-\$12.22	-\$12.22	-\$12.22	-\$27.03	-\$27.03	-\$9.15
ΔUninternalized externality per sample household	\$9.61	\$18.11	\$21.94	-\$0.76	-\$0.76	\$4.49	\$11.31
ΔWelfare per sample household	-\$2.61	\$5.90	\$9.73	-\$12.98	-\$27.79	-\$22.54	\$2.17
ΔWelfare per dollar subsidy	-0.18	0.41	0.67	-0.90	-1.92	-1.56	0.15

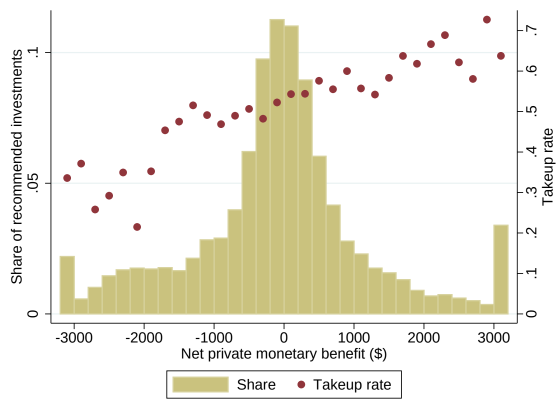
Notes: This table reports averages across 250 simulated draws of ϵ. Column 2 assumes that energy efficiency investments impose an additional positive externality on future home buyers equal to 40 percent of present discounted retail energy cost savings. Column 3 uses a \$78 social cost of carbon instead of \$39. Column 4 adjusts uninternalized externalities ϕ_{ij} by multiplying electricity and gas/heating oil savings by 2.48 and 0.29, respectively, based on the estimates in Table 6. Column 5 repeats column 4, additionally assuming that consumers misperceive energy savings because they do not anticipate the empirical shortfall. Column 6 repeats column 5, except using a \$78 social cost of carbon. Column 7 assumes that market failures γ_{Ai} and ϕ_{ij} exactly equal the existing program subsides. "Per sample household" means per household in the 101,881-household Wisconsin sample. In Panel B, consumer utility is from Equation (13), and ΔWelfare is from Equation (14).

Figure 1: **Audit Takeup Treatment Effects Scaled by Effect of a $1 Subsidy**



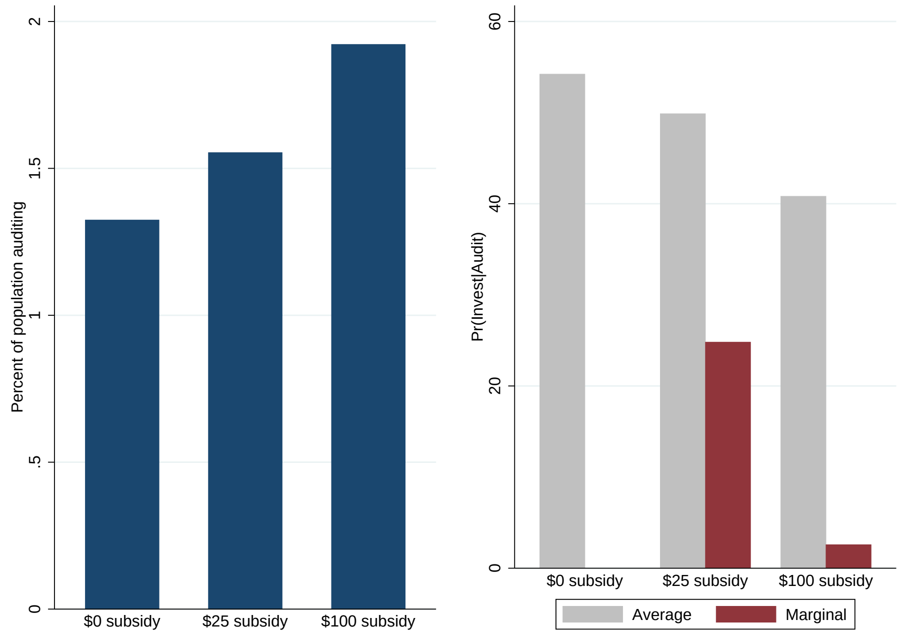
Notes: This figure presents the point estimates and 90 percent confidence intervals for the treatment effect estimates from the Wisconsin audit takeup experiment, scaled by the effect of a $1 subsidy. The estimating equation is Equation (8), and formal estimates are in Appendix Table A.7.

Figure 2: **Net Private Monetary Benefit and Investment Takeup**



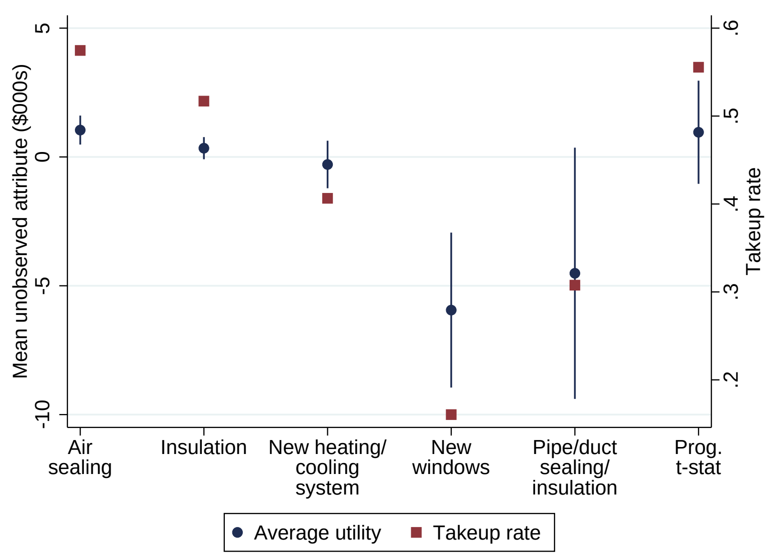
Notes: Net private monetary benefit is the present discounted value of retail energy cost savings net of subsidized cost, or $(S_{ij} - C_{ij} + E_{ij})$ in the notation of the model. Energy cost savings are computed at the 2011-2014 average retail natural gas and electricity prices in the household's city (Madison or Milwaukee). Subsidized costs are the upfront cost net of an imputed subsidy of $500 per 10 percentage point projected decrease in the household's pre-audit energy use.

Figure 3: **Marginal Investment Probabilities by Subsidy Level**



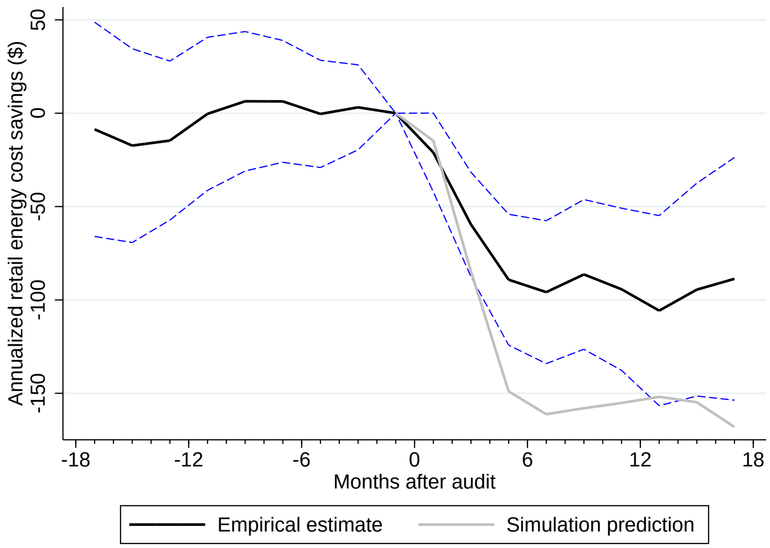
Notes: The left panel presents audit probability for each experimental audit subsidy group. The light (gray) bars on the right panel show the average investment probability by subsidy group, conditional on auditing. The dark (red) bars on the right panel show the investment probability for households marginal to each audit subsidy increase. Households that were in the letter control group or the $25 gift card group are excluded from this figure.

Figure 4: **Mean Unobserved Attribute of Recommended Investments**



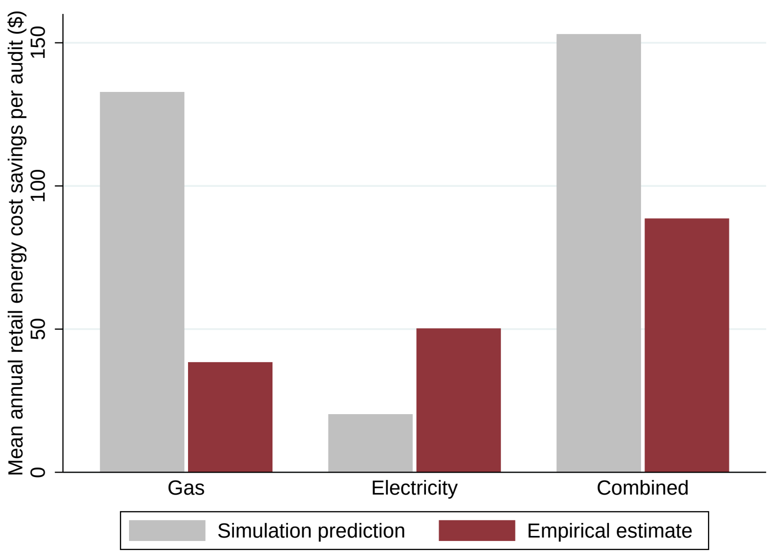
Notes: Point estimates and 90 percent confidence intervals for the dollar value of the mean unobserved attribute by investment category, or ξ_j in the notation of the model. This uses estimates in column 1 of Table 4. Standard errors are calculated using the Delta method.

Figure 5: **Energy Use in Event Time**



Notes: This figure presents energy use in event time relative to the household's audit. The figure is constructed by estimating Equation (12) separately for natural gas and electricity, replacing P_{it} with indicators for each two-month period within an event window extending 18 months before and after the audit. The excluded category is the month of the audit and the month before. After estimating separate equations for natural gas and electricity, we combine coefficients and standard errors for each two-month period, weighting fuels by their retail prices and multiplying by 365 to transform units to annualized retail cost savings. Dashed lines are 90 percent confidence intervals. Mean pre-audit retail energy costs are $1804 per year.

Figure 6: **Simulation Predictions vs. Empirical Estimates of Post-Audit Energy Savings**



Notes: This figure takes the simulated and actual effects on energy use for households ≥6 months post-audit from Table 6. The regression coefficients are in therms per day (for gas) and kilowatt-hours per day (for electricity). They are multiplied by 365 × sample average retail energy prices to give mean annual retail energy cost savings per audit.