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MEASURING THE WELFARE EFFECTS OF RESIDENTIAL ENERGY EFFICIENCY PROGRAMS

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ABSTRACT

We use a randomized experiment and a structural model to evaluate two home energy retrofit programs, which subsidized energy efficiency investments such as new insulation and heating systems. Two empirical findings drive the welfare analysis. First, the average energy savings were only 68 percent of the predictions provided to participants. Second, the programs' subsidies were not closely aligned with environmental externality reductions. In our model, the inflated savings predictions and misaligned subsidies mean that the programs reduced total surplus, but with correct savings predictions, a program that aligns subsidies with externality reductions would generate positive social returns. Energy efficiency programs deliver much smaller gains than Pigouvian energy taxes, both because few households participate in the programs and because program participants still consume too much energy when energy prices are below social marginal cost.

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I Introduction

At least since Pigou (1927), economists have argued that the optimal way to address externality problems such as climate change is corrective taxes (or similar policies such as cap-and-trade programs) that equate price and social marginal cost. One of the most important environmental policy challenges of the modern era is how to design policy when corrective taxes are infeasible for technical or political reasons. In theory, alternative policies that are not literally externality taxes can sometimes be designed to generate similar economic incentives. In practice, many energy and environmental policies—such as the U.S. fuel economy standards, renewable energy standards, and electric vehicle subsidies—have been shown to perform far worse than externality taxes, giving less environmental benefit per dollar of social cost.[1]

Energy efficiency is a classic example of this discussion. Engineering models have long suggested that the U.S. and other countries have vast potential to make low-cost energy efficiency investments that would reduce the environmental harms from energy use.[2] Motivated by this potential opportunity, policymakers have implemented a wide array of energy efficiency programs, with billions of dollars of benefits and costs each year in the U.S. alone (Gillingham, Newell, and Palmer 2009; Allcott and Greenstone 2012). But do energy efficiency investments actually deliver the savings predicted by engineering models? What are the welfare effects of energy efficiency programs? Are the designs close to optimal? If not, are there feasible design changes that could increase welfare?

In this paper, we study these questions in the context of two home energy efficiency retrofit programs in Wisconsin. In a home retrofit program, homeowners first have an energy audit and then make subsidized energy efficiency investments, such as new insulation or heating systems. Home retrofit programs are important examples of energy efficiency policy because they comprise a large share of nationwide residential energy efficiency program spending (CEE 2020) and will receive billions of dollars through the Inflation Reduction Act, and engineering models such as McKinsey & Co. (2013) suggest that they offer some of the lowest-cost carbon abatement. We evaluate the Wisconsin programs using administrative data and a 102,000-household randomized experiment that varied subsidies for energy audits.

We begin by laying out a model of program participation and social welfare. In the model, households derive quasilinear utility from consuming energy services (such as lighting, air conditioning, and space heating) and a numeraire good. A home production technology transforms three fuels (natural gas, electricity, and heating oil) into energy services. Energy efficiency investments improve that home production technology, reducing the energy inputs required to produce energy services. Households first decide whether to audit, knowing the distribution of possible energy efficiency investments but not which specific investments will be possible at their homes. Households that audit

[1] See, for example, Fullerton and West (2002), Muller and Mendelsohn (2009), Holland, Hughes, and Knittel (2009), Goulder, Jacobsen, and van Benthem (2012), Jacobsen (2013), Goulder, Hafstead, and Williams (2016), Holland et al. (2016), Fowlie, Greenstone, and Wolfram (2018), and Jacobsen et al. (2020).

[2] See, for example, Meier, Wright, and Rosenfeld (1983), Rosenfeld et al. (1993), and McKinsey & Co. (2009). On the basis of this and other research, the Alliance to Save Energy (2013) argues 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."

receive a choice set of proposed energy efficiency investments and choose which combination to make.

Because of both environmental externalities and retail price markups, retail energy prices differ from social marginal cost by an amount we call the "uninternalized externality." The first-best policy in our model would of course be to correct this distortion through energy taxes equal to the uninternalized externality. If energy taxes are unavailable, the second-best policy would be an energy efficiency program that subsidizes investments by the uninternalized externality reduction that they cause. The deadweight losses from having suboptimal energy taxes or suboptimal audit/investment subsidies depend on the magnitudes of the price distortions (each fuel's uninternalized externality or the difference between the actual versus second-best subsidies) and price responses (how much audit, investment, and energy demand respond to the distorted prices).

We begin our empirical analysis by documenting two additional distortions. First, the energy savings fell short of the predictions that the programs had provided to households that were deciding whether to make investments. Using energy bill data in a difference-in-differences design, we find that households realized only 68 percent of the engineering model predictions—even after adjusting predictions for how households might increase energy services use in response to improved energy efficiency. We do not have direct evidence on whether program participants believed the predictions, although subsequent survey evidence suggests that most people take such predictions at face value. This would generate deadweight loss by causing some people to make investments that they would not have made with unbiased predictions. This potential distortion is not unique to the Wisconsin programs: we are aware of seven academic studies comparing predicted versus actual savings for home retrofit programs, which report "realization rates" averaging 55 percent.

Second, the program subsidies differed substantially from our theoretical second best. Instead of subsidizing investments by the predicted uninternalized externality reduction, the programs subsidized investments by a step function of the predicted percent reduction in the household's total baseline energy use across all fuels (natural gas, electricity, and heating oil). This provided larger subsidies for investments at smaller homes, regardless of externality reductions, and did not account for the fact that natural gas, electricity, and heating oil have very different uninternalized externalities. At our baseline assumptions, including a \$190 social cost of carbon for 2020 (EPA 2023) and a three percent private discount rate for household energy savings, the average across households of the difference between uninternalized externality reductions from investments and investment subsidies received is \$1,632, which is 126 percent of the average investment subsidy. This "noise" in the subsidies relative to the second best generated deadweight loss, as some households were offered large subsidies for energy efficiency investments that did little for the environment, while other households were offered small subsidies for investments that would have done more. This "noise" is not unique to the Wisconsin programs: we show that the home retrofit subsidies offered by the top 10 energy efficiency programs nationwide (as rated by the American Council for an Energy-Efficient Economy (2020)) are even less closely aligned with uninternalized externality reductions than the Wisconsin subsidies.

To translate these distortions into deadweight losses, we must estimate how much audit and investment demand respond to price. Using the randomized audit subsidies from our experiment, we estimate that a \$100 audit subsidy increased audit takeup by 0.59 percentage points, a 49 percent increase over the control group's 1.2 percent takeup rate. We also find evidence of strong self-selection into the program: households that were induced to audit by the randomized audit subsidies were much less likely to invest than inframarginal households. Using non-experimental variation, we find that takeup increased steadily in predicted financial benefit (discounted retail energy savings net of subsidized cost): households took up 2.8 percent of investment combinations with more than positive \$3,000 predicted financial benefit, but only 0.8 percent of investment combinations with less than negative \$3,000 predicted financial benefit.

In the latter parts of the paper, we use this variation to estimate our structural model of program participation and energy use, and we use the model for counterfactuals and welfare analysis. Our initial counterfactuals assume that consumers believed the inflated savings predictions, as suggested by our survey evidence. Under that assumption, the programs *decreased* total surplus and had a marginal value of public funds (MVPF) of 0.93: they increased the sum of consumer surplus, producer surplus, and environmental externality reductions by only \$0.93 per dollar of subsidies.

We next consider the assumption that Wisconsin consumers somehow had correct beliefs corresponding to our estimates of actual energy savings. Under that assumption, the Wisconsin subsidies increased total surplus, with an MVPF of 1.01. However, subsidies set equal to uninternalized externality reductions (the second-best optimum in our model) would generate about 4 times larger total surplus gains. From a policy perspective, this underscores the potential for energy efficiency programs to increase welfare, if they provide unbiased information and are designed to subsidize the most environmentally beneficial investments.

In our model, Pigouvian energy taxes that align retail energy prices with social marginal cost (the first-best optimum in our model) would generate several orders of magnitude larger total surplus gains than even the second-best energy efficiency subsidies. This extreme difference arises for two reasons. First, relatively few households select into energy efficiency programs, while energy taxes affect all households. Second, even for households that do participate, energy efficiency subsidies do not address the energy use distortion when energy prices don't equal social marginal cost. From a policy perspective, this underscores that energy efficiency programs are not good substitutes for pollution taxes.

We also present an accounting-style benefit-cost analysis comparing the observed benefits of energy savings and air pollution reductions to the observed costs of audits and investments. At our baseline parameter assumptions, the benefit/cost ratio is 0.88. This low ratio is not specific to the Wisconsin programs: using separate administrative microdata, we find that comparable programs implemented nationwide had a slightly lower benefit/cost ratio.

There are several important caveats. First, our conclusions naturally depend on assumptions about the social cost of carbon and marginal damages from other air pollutants. We use standard assumptions from existing literature and show robustness to these assumptions. Second, our primary analyses assume that environmental externalities and retail markups are the only market failures. There is mixed evidence for other market failures related to asymmetric information, behavioral bias, and technological change (Allcott and Greenstone 2012), and we explore this in our counterfactuals.

Third, our structural model imposes assumptions including static decisionmaking, homogeneity of some parameters, a parametric distribution of unobserved heterogeneity, and a specific structure of expectations and information revelation. Fourth, while we use experimental variation to identify the audit takeup price response and a selection unobservable in our model, we use non-experimental variation to identify the investment takeup price response, and we import an outside estimate of the price elasticity of energy services demand. Fifth, we focus on economic efficiency, not distributional issues. Echoing Allcott, Knittel, and Taubinsky (2015) and Borenstein and Davis (2016), we find that wealthier homeowners are more likely to participate in the program. Sixth, the Better Buildings programs we study were funded with economic stimulus dollars, and job creation was an important motivation.[3] Our analysis is designed to ask whether these programs increase welfare in the absence of a macroeconomic stimulus benefit.

Our work contributes to several literatures. First, we extend efforts to formally model household energy efficiency investments and energy demand, such as Dubin and McFadden (1984), Hanemann (1984), and Davis (2008). Second, we contribute to a literature that uses field experiments to identify structural econometric models.[4] Third, we build on theoretical and empirical literatures examining self-selection into social programs and new technologies.[5] Fourth, we contribute to a rich literature evaluating the benefits and costs of energy efficiency policies.[6] Our results speak to a debate between optimists who point to large technical potential for energy efficiency and pessimists who argue that the engineering models understate costs and overstate benefits.[7] While we are aware of seven academic papers showing that energy savings can fall short of engineering predictions in home retrofit programs,[8] additional evidence is valuable because such results have not been fully accepted (e.g., Porter 2015). These findings relate to a long-standing Federal Trade Commission (1973) ruling that falsely representing energy savings from home improvement materials is "unfair or deceptive" and thus unlawful. Our structural model of program participation is new in the literature, and it allows us to compute the potential consumer harms from biased predictions and gains from counterfactual program designs.

Sections II–VI present the program overview, model, experimental design and data, parameter assumptions, and reduced-form results. Sections VII and VIII present the structural model estima-

[3]DOE (2015a) finds that the Better Buildings programs created or retained 10,191 full-time-equivalent jobs, or about one job for every $44,000 in federal outlays.

[4]This includes Todd and Wolpin (2006), DellaVigna, List, and Malmendier (2012), Adda, McConnell, and Rasul (2014), Duflo et al. (2018), Dellavigna et al. (2017), Allende, Gallego, and Neilson (2019), Attanasio et al. (2020), Oliva et al. (2020), Sadoff, Samek, and Sprenger (2020), Allcott, Gentzkow, and Song (2021), and others.

[5]This includes Willis and Rosen (1979), Bjorklund and Moffitt (1987), Heckman and Vytlacil (2005), Ashraf, Berry, and Shapiro (2010), Berry, Fischer, and Guiteras (2020), Cohen and Dupas (2010), Cohen, Dupas, and Schaner (2015), Alatas et al. (2016), Oliva et al. (2020), and others.

[6]The literature on residential energy efficiency investments and programs includes Hirst and Goeltz (1984, 1985), Hirst (1987), Metcalf and Hassett (1999), Davis (2008), Allcott (2011), Davis, Fuchs, and Gertler (2014), Levinson (2016), Fowlie, Greenstone, and Wolfram (2018), Boomhower and Davis (2020), Brandon et al. (2021), and others.

[7]See Joskow and Marron (1992), Jaffe and Stavins (1994), Joskow (1994), Lovins (1994), Gillingham, Newell, and Palmer (2009), Linares and Labandeira (2010), Allcott and Greenstone (2012), and others.

[8]The seven papers are Sebold and Fox (1985) Dubin, Miedema, and Chandran (1986), Hirst (1986), Zivin and Novan (2016), Fowlie, Greenstone, and Wolfram (2018), Giraudet, Houde, and Maher (2018), and Christensen et al. (2021).

tion and counterfactuals. Section IX concludes.

II Program Overview

The two Wisconsin programs we study were called Green Madison and Milwaukee Energy Efficiency. They were part of the national Better Buildings Neighborhood Program, which funded 41 similar programs through the 2009 economic stimulus bill (DOE 2021). The programs operated from mid-2010 through September 2013. While these stimulus programs were temporary, their design is comparable to ongoing residential retrofit programs run by utilities and state agencies around the country, and the Inflation Reduction Act authorizes $8.6 billion for home retrofit and electrification programs with very similar designs.

Like other residential retrofit programs, participation in the Wisconsin programs involved two steps. The first was the home energy audit, which involves a several-hour visit from a state-certified "energy consultant." During the audit, the energy consultant would offer to install compact fluorescent lightbulbs (CFLs) and faucet and shower aerators at no additional cost. After inspecting the house, the consultant would provide an audit report with a list of proposed energy efficiency investments, including the likely upfront cost, predicted energy cost savings, and resulting payback period of each. Appendix A presents an example audit report. The pre-subsidy price of an audit was $400.

The second step was to make energy efficiency investments, such as air sealing, new insulation, and new heating or cooling systems. Homeowners would work with program-certified contractors, who would provide formal cost estimates and then carry out the work. After the work was complete, the energy consultant would return to verify that the contractor had done the work properly.

The programs offered large subsidies for audits and investments. Madison and Milwaukee offered $200 and $300 audit subsidies, respectively, giving net-of-subsidy prices of $200 and $100. The programs also offered tiered investment subsidies of $500 in Madison ($750 in Milwaukee), $1000, $1500, and $2000, respectively, for making investments projected to save 10–14, 15–24, 25–34, or 35 or more percent of a home's baseline energy use. The investment subsidy amount was capped at the household's total investment cost. To cover remaining costs, participants were also eligible for loans of $2,500 to $20,000 from a local credit union at 4.5 to 5.25 percent interest rates.

To predict the energy savings from energy efficiency investments, the Wisconsin programs used the Targeted Retrofit Energy Analysis Tool (TREAT). TREAT is one of several engineering simulation models in widespread nationwide use; it was used for 28 percent of audits in the national Better Buildings Neighborhood Program data. 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 the Wisconsin programs.

TREAT and other engineering models are designed to predict energy savings from investments while holding constant energy services use. Our analyses below will account for how households might increase energy services consumption in response to improved energy efficiency.

III Model

III.A Overview

Our model captures the two-stage program participation process described in Section II. Figure 1 provides an overview, including notation introduced below. At the outset, households know their baseline energy intensity. They do not know the investments that would be proposed if they have an audit, but they do know the universe of possible choice sets from which their proposals would be drawn. First, households decide whether or not to have a home energy audit. If they audit, they receive their specific set of proposed investments. Second, households choose which combination of investments to make. Finally, households consume energy, given their home's (possibly improved) energy efficiency. In this static model, we think of energy consumption as the present discounted value over the life of a potential energy efficiency investment.

In this section, we first describe consumer preferences and choices in reverse order: energy consumption, investments, and audits. We then discuss government, producers, welfare, and optimal policy.

III.B Consumer Preferences and Choice

III.B.1 Consumption Utility

Households indexed by i choose consumption of energy services x (for example, lighting, heating, and air conditioning) and a numeraire n.[9] The home produces energy services from a vector of energy inputs (or "fuels") $\boldsymbol{e}$, which in our application are natural gas, electricity, and heating oil. An energy intensity vector $\boldsymbol{F}$ reflects the map from energy services to energy input use:

$$\boldsymbol{e}_i = x_i \boldsymbol{F}_i. \tag{1}$$

Energy inputs are sold at retail price vector $\boldsymbol{p}$, so the cost of energy services is $\boldsymbol{F}_i \cdot \boldsymbol{p}$. The household has exogenous income w_i. The household maximizes quasilinear utility subject to the budget constraint:

$$\max_{\{x_i, n_i\}} h_i(x_i) + n_i \quad s.t. \quad x_i \boldsymbol{F}_i \cdot \boldsymbol{p} + n_i \leq w_i \tag{2}$$

We define $\boldsymbol{e}_i^*(\boldsymbol{F}_i \cdot \boldsymbol{p})$ and $v_i(\boldsymbol{F}_i \cdot \boldsymbol{p})$, respectively, as the resulting energy input demand and indirect utility.

III.B.2 Energy Efficiency Investments

At baseline, each household is endowed with status quo energy intensity $\boldsymbol{F}_{i0}$. Energy efficiency investments change this at some cost.

[9] We model x as a scalar instead of a vector because we do not have data on use of individual energy services.

Households that audit receive a specific set of individual proposed investments. $\mathcal{J}_i$ is the household's choice set of all possible *combinations* of proposed investments, including the status quo of not investing. For example, for a household that received two individual proposed investments, attic insulation and a new heating system, $\mathcal{J}_i$ would have four elements: {status quo, insulation, new heating system, insulation & new heating system}. The investment stage is thus a multinomial discrete choice problem where the household chooses one element of the choice set $\mathcal{J}_i$.

Investment combination j has three characteristics: the new energy intensity $\boldsymbol{F}_{ij}$, the price p_{ij}, and an additional "non-energy benefit" ξ_{ij}, which might reflect warm glow utility from reducing energy use or the time and inconvenience of a home construction project. The status quo option is denoted $j = 0$.

Household i's indirect utility from choice j (in units of dollars) is

$$V_{ij}^I = v_i\left(\boldsymbol{F}_{ij} \cdot \boldsymbol{p}\right) - p_{ij} + \xi_{ij}. \tag{3}$$

Households choose the investment combination from choice set $\mathcal{J}_i$ that maximizes V_{ij}^I. $I_{ij} \in \{1, 0\}$ is an indicator for whether household i chooses investment combination j. We define $\Delta \boldsymbol{e}_{ij} \coloneqq \boldsymbol{e}_i^*\left(\boldsymbol{F}_{i0} \cdot \boldsymbol{p}\right) - \boldsymbol{e}_i^*\left(\boldsymbol{F}_{ij} \cdot \boldsymbol{p}\right)$ as the resulting energy use reduction, which will mostly be positive.

III.B.3 Home Energy Audits

Before the audit, consumers do not know what specific proposed investments they will receive. We assume that they have rational expectations: they know that their choice set $\mathcal{J}_i$ will be randomly drawn from universe of possible choice sets $\mathcal{U}$. $\mathbb{E}_{\mathcal{J}_i \in \mathcal{U}}\left[\max_{j \in \mathcal{J}_i}\left\{V_{ij}^I\right\}\right]$ is the expected investment value: the expectation over possible choice sets $\mathcal{J}_i \in \mathcal{U}$ of the indirect utility from the optimal choice in each choice set. Home energy audits have price p_i^A and additional non-energy benefit ξ_i^A, which might reflect the enjoyment of learning more about one's home or the time and inconvenience of the inspection.

Household i's expected utility from auditing (in units of dollars) is

$$V_{i1}^A = \mathbb{E}_{\mathcal{J} \in \mathcal{U}}\left[\max_{j \in \mathcal{J}}\left\{V_{ij}^I\right\}\right] - p_i^A + \xi_i^A \tag{4}$$

Households choose to audit if the expected utility exceeds the status quo. $A_i \in \{1, 0\}$ is an indicator for whether household i chooses to audit.

III.C Government, Producers, and Welfare

The government has three policy instruments: household-specific audit subsidies s_i^A, household- and investment-specific investment subsidies s_{ij}, and energy taxes $\boldsymbol{\tau}$. The marginal cost of public funds is λ.

Audits and investments have costs c^A and c_{ij} and are supplied in perfectly competitive markets with full pass-through of subsidies. Audit and investment prices are thus $p_i^A = c^A - s_i^A$ and $p_{ij} = c_{ij} - s_{ij}$, respectively. In Wisconsin and elsewhere in the U.S., natural gas and electricity are generally sold by regulated utilities, while heating oil is sold by competing providers. For a variety of reasons, utilities in the U.S. recover much of fixed costs through retail markups instead of fixed monthly charges (Davis and Muehlegger 2010; Borenstein and Davis 2012; Borenstein and Bushnell 2021). We thus model energy inputs as having constant marginal cost $\boldsymbol{c}$ and exogenous markup $\boldsymbol{\pi}$ with full pass-through of energy taxes. Energy prices are thus $\boldsymbol{p} = \boldsymbol{c} + \boldsymbol{\pi} + \boldsymbol{\tau}$.

There are two possible market failures. First, energy prices $\boldsymbol{p}$ may not equal social marginal costs, due to both the retail markup $\boldsymbol{\pi}$ and a constant marginal negative environmental externality denoted $\boldsymbol{\phi}$.[10] We refer to $\boldsymbol{\phi} - \boldsymbol{\pi}$ as the "uninternalized externality." Second, there is an "investment takeup distortion" γ_{ij}, which captures additional consumer benefits from investment j that households do not consider in their choices characterized above. We primarily use γ_{ij} to capture misperceptions of the private benefits from energy efficiency investments, as might be caused by engineering predictions that exceed actual savings. With misperceptions, the utilities defined in equations (2), (3), and (4) should be thought of as "perceived utility" (or "decision utility"), while true utility adjusts for the misperception as in equation (5) below. We can also use γ_{ij} to capture the fact that people who resell their houses may not capture the full benefit of energy efficiency investments if asymmetric information problems prevent the value of those investments from being capitalized into house prices. With that type of asymmetric information, homeowners choose the privately optimal investment combination j, and γ_{ij} is a benefit that accrues to the next owner.

Consumer surplus is the sum over households of indirect utility plus the investment takeup distortion, depending on audit and investment choices:

$$CS = \sum_i \left\{ (1 - A_i)\, v_i\left(\boldsymbol{F}_{i0} \cdot \boldsymbol{p}\right) + A_i \cdot \left[\sum_{j \in \mathcal{J}_i} I_{ij} \cdot \left(v_i\left(\boldsymbol{F}_{ij} \cdot \boldsymbol{p}\right) - p_{ij} + \xi_{ij} + \gamma_{ij} \right) - p_i^A + \xi_i^A \right] \right\}. \quad (5)$$

Total energy use is an analogous sum over households, depending on audit and investment choices:

$$\boldsymbol{e} = \sum_i \left\{ (1 - A_i)\, \boldsymbol{e}_i^*\left(\boldsymbol{F}_{i0} \cdot \boldsymbol{p}\right) + A_i \sum_{j \in \mathcal{J}_i} I_{ij} \boldsymbol{e}_i^*\left(\boldsymbol{F}_{ij} \cdot \boldsymbol{p}\right) \right\} \quad (6)$$

Total surplus is

$$W = CS + \underbrace{\boldsymbol{e} \cdot \boldsymbol{\pi}}_{\text{producer surplus}} - \underbrace{\boldsymbol{e} \cdot \boldsymbol{\phi}}_{\text{externality}} \underbrace{-\lambda \left(\sum_i A_i \cdot \left[s^A + \sum_{j \in \mathcal{J}_i} I_{ij} s_{ij} \right] - \boldsymbol{e} \cdot \boldsymbol{\tau} \right)}_{\text{government spending}}. \quad (7)$$

[10] Climate change and other energy use externalities might enter through the government budget (e.g., disaster recovery and health care costs paid by the government), consumer surplus (e.g., mortality and more or less comfortable temperatures), and producer surplus (e.g., lost worker productivity). Because of this ambiguity, we let externalities enter total surplus separately in equation (7).

III.D Optimal Policy

We now state the optimal tax and subsidy policies in our model, which provide useful benchmarks for policy evaluation later in the paper. To focus on the corrective benefits of taxes and subsidies, we assume $\lambda = 1$ for our primary analyses, as if the government can use lump-sum taxation and redistribution. (With $\lambda > 1$, it would be optimal to set energy taxes above the optimal corrective level to raise revenues.) In this section, we also focus on the case with $\gamma_{ij} = 0$, which might result from accurate information provision.

Lemma 1 states the standard result that the optimal policy is to align energy prices with social cost by setting the energy tax equal to the uninternalized externality. We call this the "first-best" policy.

Lemma 1. Assume $\lambda = 1$ and $\gamma_{ij} = 0$. The optimal policy is

$$\begin{aligned} \boldsymbol{\tau}^{FB} &= \boldsymbol{\phi} - \boldsymbol{\pi} \\ s_{ij}^{FB} &= 0 \\ s^{A,FB} &= 0. \end{aligned}$$

Lemma 2 is relevant for an energy efficiency program that can set audit and investment subsidies but cannot set energy taxes. The lemma states that if $\boldsymbol{\tau} = \mathbf{0}$ for exogenous reasons, the optimal policy is to set investment subsidies equal to the uninternalized externality reduction caused by the investment. We call this the "second-best" policy.

Lemma 2. Assume $\lambda = 1$, $\gamma_{ij} = 0$, and $\boldsymbol{\tau} = \mathbf{0}$. The optimal policy is

$$\begin{aligned} s_{ij}^{SB} &= \Delta \boldsymbol{e}_{ij} \cdot (\boldsymbol{\phi} - \boldsymbol{\pi}) \\ s^{A,SB} &= 0. \end{aligned}$$

Simple proofs of both lemmas are in Appendix B.

In Section VI, we will show that energy efficiency programs often set subsidies that are not closely related to uninternalized externalities. Figure 2 helps give intuition for the deadweight loss from suboptimal policies on the three margins of consumer choice in our model: energy use, investment takeup, and audit takeup.

The left panel displays the energy use equilibrium in a simplified case with only one fuel. The energy demand function is conditional on earlier audit and investment decisions. In the first-best optimum, the energy price equals social marginal cost ($\boldsymbol{p}^{FB} = \boldsymbol{c}+\boldsymbol{\phi}$), as indicated by the dashed line. Current energy prices are instead $\boldsymbol{p} = \boldsymbol{c}+\boldsymbol{\pi}$, and the resulting deadweight loss is the shaded triangle. With locally linear demand, its area is $\frac{1}{2}(\boldsymbol{\phi} - \boldsymbol{\pi})^2 \frac{d\boldsymbol{e}^*}{d\boldsymbol{p}}$, where $\frac{d\boldsymbol{e}^*}{d\boldsymbol{p}}$ is the energy price response.

The middle panel displays the investment takeup equilibrium in a simplified case with only one energy efficiency investment opportunity. The investment demand function is conditional on energy prices and audit takeup. In the second-best optimum, the investment price includes the optimal subsidy $s_{ij}^{SB} = \Delta \boldsymbol{e}_{ij} \cdot (\boldsymbol{\phi} - \boldsymbol{\pi})$, as indicated by the dashed line. If there were no investment subsidy, the investment price would be c_{ij}, and the resulting deadweight loss would be the shaded triangle at top left. If there were some above-optimal investment subsidy $s_{ij} > s_{ij}^{SB}$, the resulting deadweight loss would be the shaded triangle at bottom right. The area of each triangle is $\frac{1}{2}\left(s_{ij} - s_{ij}^{SB}\right)^2 \frac{d \Pr I_{ij}}{dp_{ij}}$, where $\frac{d \Pr I_{ij}}{dp_{ij}}$ is the investment price response. If subsidies vary across households, then the resulting deadweight loss depends on the mean squared error of the subsidies $\frac{1}{N}\sum_i \left(s_{ij} - s_{ij}^{SB}\right)^2$, reminscent of the results in Jacobsen et al. (2020).

The right panel displays the audit takeup equilibrium. The audit demand function is conditional on energy prices and the expected investment value. In the second-best optimum, there is zero audit subsidy, so the audit price would be c^A, as indicated by the dashed line. The shaded triangle is the deadweight loss from subsidizing audits. Its area is $\frac{1}{2}\left(s_i^A\right)^2 \frac{d \Pr A_i}{dp_i^A}$, where $\frac{d \Pr A_i}{dp_i^A}$ is the audit price response.

In essence, this paper is about quantifying the size of these deadweight loss triangles. We cannot directly implement this informal graphical analysis because there are multiple fuels and investment opportunities and because decisions overlap across the three margins: energy prices affect audit and investment demand, audit and investment demand affect the energy demand function, etc. Thus, Figure 2 is just for intuition, and we will need our full structural model for welfare evaluation. However, the figure is useful in that it illustrates the key economic forces that drive our results: deadweight losses depend on *distortions* (uninternalized externalities $\boldsymbol{\phi} - \boldsymbol{\pi}$, investment takeup distortions γ_{ij}, and the differences between actual and second-best subsidies) and *price responses* (for audits, investments, and energy use).

In Section IV, we describe a field experiment that helps to identify the audit price response $\frac{d \Pr A_i}{dp_i^A}$. In Sections V and VI, we present parameter assumptions and reduced-form evidence on distortions and price responses.

IV Experimental Design and Data

IV.A Experimental Design

Our experiment involved mailing promotional letters with randomly varying content to households eligible for the programs. The experimental population was all 101,881 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 June 2012. 79,994 households were randomly assigned to receive two identical letters between June 2012 and February 2013, with the remaining 21,887 assigned to a control group that did not receive letters. We used a re-randomization algorithm to ensure that treatment conditions were balanced on observables; Appendix Table A1 shows that this was successful.

The promotional letters described the program and next steps for participation, including the phone number to call to schedule a home energy audit. We randomly varied seven parts of the letter, including the audit subsidy amount and six non-price (informational or persuasive) factors. In the body of the paper, we focus on the subsidies, as they help identify our model. Appendix C presents example letters and information about the six non-price treatments, which had no detectable effects.

The randomly assigned subsidies reduced the home energy audit price quoted in the letters. The "next steps" box on the letter said, "Call to schedule a home energy audit. Usual cost: \$400. You pay only [p]!" There were four subsidy conditions. The *\$0 subsidy* group was quoted the standard program price, which was $p =$ \$200 in Madison and \$100 in Milwaukee. The *\$25 subsidy* and *\$100 subsidy* groups, respectively, were quoted $p =$ \$175 and \$100 in Madison and $p =$ \$75 and "nothing" in Milwaukee. The *\$25 gift card* group was quoted the same price as the \$0 subsidy group but was also informed that they could call a number to redeem a \$25 Visa cash card after completing the audit.

IV.B Data

We have three administrative datasets from the Wisconsin programs. Table 1 presents summary statistics. Panel A presents the first dataset: characteristics of each of the 101,881 households in the experimental population. For each household that audited, we observe the audit date, and for each household that invested, we observe the final investment completion date.

The second dataset includes the characteristics of all proposed investments (from the audit reports) and adopted investments for the 1,394 households in the experimental population that had an audit between June 2012 and September 2013. The characteristics include rated lifetime, pre-subsidy cost, and predicted savings of natural gas, electricity, and heating oil. There are 9,436 proposed investments in the full sample. To focus on energy efficiency, all analyses in this paper exclude health and safety projects (chimney liners and exhaust fans) and solar panels, leaving 9,068 proposed energy efficiency investments.

To construct choice sets for the investment takeup estimation in Section III, we also exclude zero-cost investments installed during the audit (because there is no plausibly exogenous price variation) and specific investments with data quality issues, leaving 6,100 proposed investments (4.4 per household that audited). Panel B of Table 1 presents summary statistics for that sample, and Appendix D.A has additional details. The most common types of proposed investments are insulation (64 percent), air sealing (22 percent), and new heating and cooling systems (13 percent); see Appendix Table A7. About 95 percent of proposed investments had a 20 year rated lifetime. The average proposed investment had a cost of \$1,486 and was predicted to save 8,794 thousand British thermal units (kBtu) per year of electricity, natural gas, and/or heating oil, which amounts to \$86 per year in retail energy costs using the energy price assumptions described below in Section V. About 51 percent of the proposed investments were adopted. At retail prices, 78, 6, and 17 percent of predicted savings from adopted investments were from natural gas, electricity, and heating oil, respectively.

Panel C of Table 1 presents the third dataset: household-level natural gas and electricity use, as reported on utility bills, for most households that audited. 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 (1,258 out of 1,394) agreed. We do not have energy use data for households that did not audit. To avoid bias when comparing predicted versus empirically estimated savings, we drop households that installed solar panels or were known to have participated in another energy efficiency program, leaving 1,192 (1,197) households with natural gas (electricity) data. We do not have consistent heating oil consumption data, but only 23 households made investments that were predicted to save heating oil. The household-level data generally begin at least a year before the audit and end in May 2015.

We deflate all dollar amounts to 2013 dollars. We also construct average base-65 heating and cooling degrees for the city and time period of each observation in the energy bill data, using data from NOAA (2015).

V Parameter Assumptions

In this section, we describe parameter assumptions based on evidence outside of our data.

V.A Discount Rate

Throughout the paper, we discount energy savings and household utility at a three percent annual rate over the lifetimes of energy efficiency investments. (Our social cost of carbon assumption appropriately uses a different discount rate for the social benefits of carbon abatement, which have a different time horizon and risk profile.) As benchmarks for the annual discount rate, the loans available to program participants had 4.5–5.25 percent nominal interest rates, the real post-World War II returns to the S&P 500 stock market index have been about five percent, and the real risk-free rate (as measured by yields on 20-year Treasury inflation-indexed securities) averaged one percent from 2010–2019 (Federal Reserve 2021).

V.B Energy Services Demand Elasticity

Because there is no exogenous variation in energy prices in our data, we take the price elasticity of energy services demand from the literature. Recent estimates of the price elasticity of residential natural gas demand are around -0.2 (Auffhammer and Rubin 2018), -0.1 (Davis and Kilian 2011), and -0.28 (Davis and Muehlegger 2010). Estimates of the elasticity of residential electricity demand include -0.09 (Ito 2014) and -0.20 to -0.31 (Dubin and McFadden 1984), and Borenstein and Bushnell (2021) assume -0.2. Given these estimates, we assume that energy services demand has a constant elasticity of $\eta = -0.2$.

V.C Differences Between Price and Social Marginal Cost by Fuel

Table 2 summarizes our energy cost and externality assumptions for electricity, natural gas, and heating oil; see Appendix D.C for additional details.

We calculate energy prices ($\boldsymbol{p}$) and acquisition costs ($\boldsymbol{c}$) to reflect averages over 2011–2014. 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). For natural gas acquisition costs, we use Wisconsin "city gate" prices from the EIA. For electricity acquisition costs, we use "all-in" prices for the MISO wholesale market, which includes Wisconsin, from Potomac Economics (2011–2014). These electricity prices include quantity-weighted average costs for energy and capacity, plus ancillary services and uplift charges. Since heating oil is a standardized commodity typically available from multiple competing providers, we assume that retail price equals marginal cost. Row 3 of Table 2 shows that marginal retail prices exceeded marginal acquisition costs by about 50 percent for natural gas and 300 percent for electricity.

To value the externalities ($\boldsymbol{\phi}$) from energy consumption in Wisconsin, we use standard emission rates and marginal damage assumptions from Holland et al. (2016) and other prior work. Our estimates include local air pollution (sulfur dioxide, nitrogen oxides, and particulates) and greenhouse gas emissions (both carbon dioxide and methane leakage from natural gas systems). For our primary estimates, we follow the U.S. EPA (2023) proposal of a \$190 per metric ton CO2 social cost of carbon for 2020, which is \$172 per ton after deflating to 2013 dollars. Row 4 of Table 2 shows that electricity in Wisconsin has much larger environmental externalities than natural gas and heating oil.

Row 5 of Table 2 shows that the resulting difference between social cost and retail marginal price varies materially across the three fuels, although electricity's higher retail markup partially offsets its higher environmental externality. In our model, the Wisconsin programs' approach of subsidizing energy efficiency investment based on energy savings (regardless of fuel) generates deadweight loss relative to the second-best subsidies, which depend on fuel-specific uninternalized externalities.

VI Reduced-Form Results: Distortions and Price Responses

We begin with evidence on two key distortions. We then present evidence on the audit and investment price responses.

VI.A Predicted versus Actual Energy Savings

VI.A.1 Empirical Strategy

In the administrative data, we observe predicted savings of each fuel for each proposed energy efficiency investment. The goal of this section is to estimate the realization rate: the ratio of actual to predicted savings. The realization rate matters for two reasons. First, we need actual savings, not just predicted savings, to calculate optimal investment subsidies and welfare effects. Second, a realization rate of less than one would imply that the programs provided biased predictions to program participants, which could cause misperceptions and thus distortions.

We estimate separate realization rates for natural gas and electricity. To do this, we estimate the average effect of audits on *measured* energy use using a difference-in-differences (staggered event study) design, and we divide that by the average effect of audits on *predicted* energy use estimated from an identical regression. We use two-way fixed effects models for our primary estimates, although we show that the estimator from Callaway and Sant'Anna (2021) yields similar results.

We define y_{fit} as a dependent variable (either measured energy use or the predicted post-audit energy use change) for fuel f at household i for the billing period ending on date t. To distinguish shorter-term from longer-term effects, $P_{it}^{<6}$ and $P_{it}^{\geq 6}$ are indicators for whether date t is less than six months or six or more months after household i's audit, respectively. For the billing period that includes the audit date, we pro-rate $P_{it}^{<6}$ on $[0, 1]$ to reflect the share of days after the audit. $\mathbf{W}_{it}$ denotes controls for heating and cooling degrees, $\nu_{ic(t)}$ denotes household-by-calendar month fixed effects for calendar months indexed c, and $\mu_{m(t)}$ denotes month-of-sample indicators for sample months indexed m.[11] The estimating equation is

$$y_{fit} = \tau_f^{\geq 6} P_{it}^{\geq 6} + \tau_f^{<6} P_{it}^{<6} + \beta \mathbf{W}_{it} + \nu_{ic(t)} + \mu_{m(t)} + \varepsilon_{fit}. \tag{8}$$

Standard errors are robust and clustered by household.

Measured energy use comes directly from the energy bill data described in Panel C of Table 1; recall that we only observe energy use for households that audited. To construct the predicted post-audit energy use change, we first construct Δe_{fit}^{pred}, the total predicted savings of fuel f from all investments made by household i as of the billing period ending in date t, from the predicted savings data described in Panel B of Table 1. $\Delta e_{fit}^{pred} = 0$ before the audit, then on the audit date adds the predicted savings from any investments installed during the audit (such as energy-efficient lightbulbs), then on the household's investment completion date adds the predicted savings from all other investments. If the audit or investment completion date occurs in the middle of a billing period, we pro-rate predicted savings over the billing period. We then adjust these predictions for weather and the energy services elasticity, as described below. Finally, to get predicted energy use *change* for the regressions instead of predicted energy *savings*, we multiply the predictions by -1.

The engineering predictions are for average weather conditions. If the empirically realized weather conditions differ, this could make actual versus predicted savings different even if predictions are unbiased in average weather. We thus construct weather-adjusted engineering predictions $\Delta e_{fit}^{pred,w}$ assuming that savings scale proportionally in degree days; see Appendix H.B. Robustness checks in Appendix Table A10 show that the weather adjustment makes little difference.

The engineering predictions hold energy services use constant. If households increased energy services use because their energy efficiency investments reduced the cost of energy services, i.e. if there

[11] 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, a second μ for all bills where the midpoint occurs in February 2012, etc. Then, there is one ν indicator variable for all bills from household i with midpoint in January of any year, a second ν for all bills from household i with midpoint in February of any year, etc.

is a so-called "rebound effect" (Borenstein 2015; Gillingham, Rapson, and Wagner 2016), this could make actual versus predicted savings different even if predictions are unbiased at constant energy services use.[12] We thus construct elasticity-adjusted engineering predictions $\Delta e_{fit}^{pred,w,\eta}$ assuming that the demand for energy services provided by fuel f has constant elasticity η; see Appendix H.C. The elasticity-adjusted predictions are approximately $\Delta e_{fit}^{pred,w,\eta} \approx (1+\eta) \cdot \Delta e_{fit}^{pred,w}$, so under our assumption that $\eta = -0.2$, the elasticity adjustment reduces predicted savings by about 20 percent.

We let τ_f^{meas} and τ_f^{pred} denote treatment effects from equation (8) for measured and predicted energy use. The realization rate is their ratio:

$$\chi_f = \frac{\tau_f^{meas}}{\tau_f^{pred}}. \tag{9}$$

In addition to the usual parallel trends assumptions required for difference-in-differences estimators to be unbiased, the realization rate calculation requires an additional assumption: that program participation involved only the observed investments reflected in Δe_{fit}^{pred}, and not any additional unmeasured changes. For example, imagine that home energy audits caused participants to be more careful about turning off the lights or to buy energy efficient appliances, neither of which would be reflected in Δe_{fit}^{pred}. These changes would properly be captured in $\hat{\tau}_f^{meas}$ as causal effects of the program, but they would bias upward the realization rate for observed investments. Any such unaccounted energy use reductions would thus bias against our finding that the realization rate is less than 100 percent.

In theory, the realization rate could be systematically different for households marginal to different levels of subsidies, but we do not see any mechanisms that would generate such systematic differences.

VI.A.2 Results

Figure 3 provides an event study illustration of energy use before and after home energy audits. To make the figure, we first estimate equation (8) replacing $P_{it}^{<6}$ and $P_{it}^{\geq 6}$ with indicators for each two-month period within an event window extending 18 months before and after the audit. We then add together the natural gas and electricity cofficients after transforming to units of monthly energy costs. The solid line with confidence intervals is measured use. The gray dashed lines are point estimates from the same regression with the engineering prediction and elasticity-adjusted prediction. As expected, the elasticity-adjusted predictions are about 15 to 20 percent smaller than the unadjusted predictions.

There is no pre-audit trend in measured energy use, which provides suggestive support for the required parallel trends assumption. Predicted and actual energy use decrease over the first six months after the audit as households gradually make energy efficiency investments. By about six months after the audit, predicted and actual use have stabilized. The point estimates suggest that predicted savings exceed measured savings by about 30 to 50 percent.

[12] For example, people might keep warmer indoor temperatures after installing a new heating system, or they might leave lights on more after installing energy-efficient lighting.

Figure 4 presents our estimates of the average longer-term post-audit effects: the coefficients on $P_{it}^{\geq 6}$ from equation (8). (See Appendix Table A9 for the formal regression estimates.) The first two groups of bars present the coefficients on $P_{it}^{\geq 6}$ for natural gas and electricity, transformed to units of annualized energy cost, while the third group of bars presents the sum of the two. For natural gas, the elasticity-adjusted predicted savings are \$122 per year, while the measured savings are only \$43, for a realization rate of 0.35. For electricity, the elasticity-adjusted predicted savings are \$16 per year, while the measured savings are \$52, for a realization rate of 3.17. Combining the two, the elasticity-adjusted predicted total savings are \$138, while the measured savings were \$94, so the overall realization rate is 0.68. The p-values of tests of equality between the elasticity-adjusted predictions and measured savings are < 0.001, 0.030, and 0.039, respectively, for natural gas, electricity, and their combination. The \$94 annual savings are about five percent of the sample average pre-audit spending of \$1,780 per year.

Even under a parallel trends assumption, two-way fixed effects estimators such as ours do not identify the average treatment effect on the treated (ATT).[13] Instead, they identify a variance-weighted ATT plus a term reflecting the change in treatment effects over time (Goodman-Bacon 2021). The variance weighting may be desirable in our setting because it improves precision, and our specification including $P_{it}^{<6}$ and $P_{it}^{\geq 6}$ roughly captures the changes in effects over time.[14] In Appendix E.C, we find that the Callaway and Sant'Anna (2021) estimator delivers similar estimates, with an overall realization rate of 0.62.

Because the realization rates are so different for natural gas versus electricity, the relative weight on 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 measured total savings are \$41 per year, and the overall realization rate is 0.49.

Other related work suggests that these results generalize outside of the Wisconsin programs. Seven peer-reviewed studies of home retrofit realization rates find an average realization rate of 55 percent; see Appendix Table A14. Included in those studies is Fowlie, Greenstone, and Wolfram (2018), who find a 30 percent realization rate for Weatherization Assistance Program retrofits for low-income households in Michigan. They similarly find that the engineering model used in Michigan overstated natural gas savings and understated electricity savings, but since gas comprises a larger share of savings in that program, the overall realization rate is far less than 100 percent. The TREAT software developers found realization rates of 60–70 percent in a recent New York study (PSD 2015b), also finding that the model overstated natural gas savings and understated electricity savings. The national Better Buildings program evaluation (DOE 2015a) found realization rates of

[13] For further discussion, see Abraham and Sun (2021), Goodman-Bacon (2021), Athey and Imbens (2022), Borusyak, Jaravel, and Spiess (2021), Callaway and Sant'Anna (2021), and de Chaisemartin and D'Haultfoeuille (2022).

[14] On variance weighting, Goodman-Bacon (2021) writes, "[the variance-weighted ATT] lies along the bias/variance tradeoff: the variance weights come from the fact that OLS combines 2x2 DDs efficiently but potentially moves the point estimate away from, say, the sample ATT. This tradeoff may be worthwhile." On changes in effects over time, Goodman-Bacon (2021) writes, "Note that this bias [from effects that vary over time] is specific to a single-coefficient specification. More flexible event-study specifications may not suffer from this problem."

0.47 and 0.59, respectively, for natural gas and electricity. Nadel and Keating (1991) summarize 11 non-peer reviewed studies with an average realization rate of 0.48.

Appendix E presents extensive robustness checks and additional analyses that help explain why measured savings fall short of predictions and why the results might differ for natural gas versus electricity. We do not have the data to estimate a realization rate for heating oil. Since most of the heating oil reductions are from oil-to-gas heating system conversions, we assume for consistency that heating oil has the same realization rate as natural gas.

This finding of overstated savings predictions has important implications: energy savings and environmental benefits are less than the engineering model predicted, and consumers who believed the biased predictions may have made investments that they would not have made with unbiased predictions. Our counterfactual simulations in Section VIII predict large benefits from providing consumers with unbiased information.

VI.B Investment Subsidies versus Uninternalized Externality Reduction

From Section III.D, the second-best optimal investment subsidies equal the uninternalized externality reduction caused by the investment: $s_{ij}^{SB} = \Delta \boldsymbol{e}_{ij} \cdot (\boldsymbol{\phi} - \boldsymbol{\pi})$. The Wisconsin programs' investment subsidies, which are step functions of the percent of baseline energy saved, are clearly different. Quantitatively, how different are they?

To answer this question, Figure 5 plots the total investment subsidy received against the second-best optimal subsidy s_{ij}^{SB}, for each household that had a home energy audit. The energy savings $\Delta \boldsymbol{e}$ are the sum of the weather- and elasticity-adjusted engineering predictions for all investments the household adopted, multiplied by the realization rates χ_f estimated in Section VI.A. The uninternalized externality $\boldsymbol{\phi} - \boldsymbol{\pi}$ is from row 5 of Table 2. Because of the tiered subsidy, the great majority of households received subsidies of \$0, \$500, \$750, \$1000, \$1500, or \$2000, although some households received different amounts because the investment subsidy was capped at the household's total investment cost. If the program subsidies were similar to the second-best optimum, the points on the graph would be close to the 45-degree line. In reality, they are quite different: the average absolute difference between uninternalized externality reduction and subsidy payment is \$1,632, which is 126 percent of the average subsidy. Appendix Table A15 documents even larger differences between the program subsidies and the second best if we assume 100 percent realization rates or ignore the retail markup.

On average, the program subsidies are less generous than the second-best subsidies given the marginal damage assumptions embedded in Table 2. In addition to this difference in average amount, the investment subsidies have four additional built-in distortions: (i) they scale in predicted energy savings as a percent of baseline energy use, which distorts subsidies toward households that use less at baseline; (ii) they involve step functions instead of linear functions of energy savings; (iii) they do not take account of the fact that natural gas, electricity, and heating oil have different uninternalized externalities; and (iv) the subsidies are capped at total cost. Heating oil has the largest uninternalized externality, and Figure 5 has separate markers for households with positive versus zero fuel oil savings, showing that this accounts for some of the high outliers.

Notably, Section 50121 of the Inflation Reduction Act (IRA) explicitly specifies that the retrofit programs it funds must have those same four features. Indeed, the deadweight losses from the IRA design might be larger than in the Wisconsin programs because the step functions have fewer steps and because the rebates are uniform nationwide even though the uninternalized externalities from electricity use vary widely in different regions (Borenstein and Bushnell 2021).

To understand whether these results might generalize outside the Wisconsin programs and the IRA, we collected data on subsidies offered by the top 10 energy efficiency programs nationwide, as rated by the American Council for an Energy-Efficient Economy's (2020) Utility Energy Efficiency Scorecard.[15] Most programs offer subsidies for insulation that scale in the cost or quantity of insulation installed and subsidies for new heating and cooling systems that depend on the system's energy efficiency rating or size; see Appendix Table A16. Only one program is similar to the Wisconsin programs in having subsidies that depend on predicted energy savings. Since an identical amount of insulation or an identical heating system could save very different amounts of energy in different houses with different consumption patterns, this suggests that the top 10 programs might be even further from the second-best than the Wisconsin programs.

To quantify the noise in subsidies offered by the top 10 programs, we apply the subsidies, retail markups, and marginal damages in each top-10 program's service area to the investments made by each Wisconsin household.[16] Figure 6 presents the resulting scatterplots for each area. The Chicago and Detroit subsidies are mostly smaller than uninternalized externality reductions, because the electric grids are dirty, retail markups are low, and the subsidies are not very generous. By contrast, the Baltimore, Connecticut, Massachusetts, and northern California subsidies are mostly larger than uninternalized externality reductions, because the electric grids are clean, retail markups are high, and the subsidies are generous. These aggregate results are reminiscent of the Borenstein and Bushnell (2021) finding that the utilities with the highest retail electricity price markups also spend more on energy efficiency. But the noise in each scatter plot also shows that *within* each utility, i.e. conditional on the overall home retrofit program spending, the subsidies do not incentivize households to make investments that generate the largest uninternalized externality reduction. Quantitatively, Appendix Table A15 shows that the subsidies available to consumers in the top 10 program areas have even more noise than the Wisconsin subsidies.

This "noise" in energy efficiency subsidies has an important implication: these programs generate less social benefit than they could with straightforward changes to their rebate structures. Our counterfactual simulations in Section VIII predict material benefits from aligning energy efficiency subsidies with uninternalized externality reductions.

[15]The top 10 programs are those run by Eversource (in Massachusetts and Connecticut), National Grid (in Massachusetts), San Diego Gas and Electric, Commonwealth Edison, Baltimore Gas and Electric, Los Angeles Department of Water and Power, DTE, and Portland General Electric.

[16]Specifically, we use the same natural gas and heating oil externalities from Table 2, we collect natural gas markups from utility websites, and we use electricity markups and externalities shared by Borenstein and Bushnell (2021), after adjusting to our updated social cost of carbon. We include all home retrofit subsidies available in each top-10 utility's service area—for example, we add the Peoples Gas rebates to the Commonwealth Edison electricity rebates in Chicago, and we add the Energy Upgrade California rebates to the Pacific Gas and Electric rebates in California.

VI.C Audit Price Response and Selection Effects

To identify the audit price response $\frac{d \Pr A}{dp^A}$, we exploit the randomly assigned audit subsidies. S_i^{25}, S_i^{100}, G_i, and L_i are indicators for the \$25 and \$100 subsidy, \$25 gift card, and letter treatment groups, respectively. $\boldsymbol{X}_i$ is the vector of household covariates from Panel A of Table 1 and a constant, and Y_i is a binary outcome variable. We estimate the following linear probability model:

$$Y_i = \tau^L L_i + \tau^{25} S_i^{25} + \tau^{100} S_i^{100} + \tau^G G_i + \boldsymbol{\beta} \boldsymbol{X}_i + \varepsilon_i, \tag{10}$$

with robust standard errors.[17]

Table 3 presents results. In column 1, the dependent variable is whether household i had a home energy audit. In column 2, the dependent variable is whether household i made any energy efficiency investment. We multiply the regression coefficients by 100 for readability, so they are in units of percentage points.

Takeup is associated with household characteristics $\boldsymbol{X}_i$ in intuitive ways: takeup is positively associated with house age (perhaps because older houses can benefit more from energy efficiency retrofits), wealth (as measured by property value and building footprint), and the share of vehicles in the census tract that are hybrids (a natural measure of interest in energy efficiency).

Being sent a letter with no additional subsidy increased audit takeup by a marginally insignificant 0.25 percentage points. The \$25 and \$100 audit subsidies increased audit takeup by 0.23 and 0.59 percentage points, respectively. This effect of the \$100 subsidy represents a 49 percent increase relative to the no-letter control group's 1.2 percent audit takeup rate.

In contrast, the letters and subsidies had statistically insignificant effects on investment. The point estimates suggest that a letter with no subsidy had almost exactly zero effect on investment, and the \$25 and \$100 audit subsidies increased investments by only 0.07 and 0.15 percentage points. For comparison, the bottom row of the table shows that $0.8/1.2 \approx 67\%$ of households in the control group that audited went on to invest. These results suggest a self-selection process where households whose audits were marginal to the treatments were less likely to invest than inframarginal households.

Figure 7 illustrates the selection effect of the audit subsidy. The figure shows takeup for households that were sent letters 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 in each subsidy group. The gray bars in the right panel show the investment probablity for the average household that audited in each subsidy group.

Using these two sets of bars, we can infer the red bars on the right panel: the investment probabilities for households that were marginal to each subsidy increase.[18] Intuitively, the marginal

[17] $\boldsymbol{X}_i$ includes the variables in the re-randomization algorithm. We control for these variables following the recommendation of Bruhn and McKenzie (2009). Using standard errors that do not account for re-randomization makes standard errors conservative (Morgan and Rubin 2012).

[18] Conditional on auditing at subsidy $S = s$,

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

where $M = 1$ is an indicator for being marginal to an audit subsidy increase to s from some $s' < s$. Re-arranging gives households induced to audit by higher subsidies must have had relatively low investment probabilities to generate the pattern of decreasing group-level average investment probabilities shown in the gray bars. Indeed, while the investment probability was 62 percent for households that audited at zero experimental subsidy, the red bars show that it was only 33 percent and 25 percent for households marginal to the \$25 and \$100 subsidies, respectively.

This selection effect and the resulting inelasticity of investments with respect to audit subsidies has an important implication: the environmental benefits of higher audit subsidies can be quite limited. Our structural model in Section VII includes a self-selection effect identified by this variation, and our counterfactual simulations in Section VIII predict large benefits from reducing audit subsidies below the Wisconsin program levels.

VI.D Investment Price Response

To identify the investment price response $\frac{d \Pr I_{ij}}{dp_{ij}}$, we exploit the variation in predicted financial benefits across investments. The predicted financial benefit of investment combination j at household i as $\Delta \boldsymbol{e}_{ij}^{pred} \cdot \boldsymbol{p} - p_{ij}$, where (unadjusted) predicted energy savings $\Delta \boldsymbol{e}_{ij}^{pred}$ is discounted at three percent over the investment lifetime, $\boldsymbol{p}$ is the vector of retail prices, and p_{ij} is the subsidized investment cost.

Figure 8 presents the distribution of predicted financial benefits and the takeup rate within each bin, across all investment combinations in the data.[19] The figure has two important implications. First, many of the proposed investments are net financial losers, even at marked-up retail energy prices, subsidized investment costs, and assuming that the predicted energy savings and lifetimes are correct. At the three percent discount rate, one quarter of proposed investment combinations would lose \$1829 or more, the median proposed investment combination would lose \$262, and 55 percent have negative net private benefits. Second, takeup rates increase steadily in predicted financial benefit: households took up 2.8 percent of investment combinations with more than positive \$3,000 predicted financial benefit, but only 0.8 percent of investment combinations with less than negative \$3,000 predicted financial benefit. Our structural model in Section VII uses this variation to identify the investment price response.

VII Structural Model Estimation and Results

VII.A Functional Forms and Empirical Implementation

We now impose additional functional form assumptions that allow us to estimate the model introduced in Section III. In this static model, energy consumption and utility are present discounted values, discounted over a 20-year horizon at our assumed three percent annual rate.

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 of Figure 7, and $\Pr(I = 1)$ and $\Pr(I = 1|M = 0)$ are from investment takeup rates in the gray bars in the right panel.

[19] Appendix Figure A12 presents the analogous figure for individual investments instead of investment combinations. Appendix Figure A13 presents the analogous figure after applying the empirical realization rates from Section VI.A.

We do not have direct evidence on program participants' beliefs about energy savings. However, we carried out a 200-person online survey where we showed people an audit report (including predicted savings) and elicited beliefs about actual savings; see Appendix G for details. Some participants reported concerns that predictions could be overstated, but the vast majority reported that they took the predictions at face value. The average respondent believed that the realization rate would be 0.99. Thus, for our initial analyses, we assume that consumers believed the predictions that the programs provided. With misperceptions, the utility functions that characterize consumer choice in this section should be thought of as "perceived" utility, and they differ from the "actual" consumer surplus used for welfare analysis due to the investment takeup distortion γ_{ij}.

VII.A.1 Consumption Utility

We assume that the sub-utility from energy services takes the constant relative risk aversion (CRRA) functional form:

$$h_i(x_i) = \omega_i \frac{\eta}{\eta+1} x_i^{\frac{\eta+1}{\eta}}, \tag{12}$$

where ω_i is the taste for energy services. Equation (12) delivers an energy services demand function with constant price elasticity η; see Appendix H.D.

Any change in energy intensity (from status quo $\boldsymbol{F}_{i0}$ to $\boldsymbol{F}_{ij}$) or price (from baseline $\boldsymbol{p}_0$ to $\boldsymbol{p}$) can be captured in an energy services cost scaling factor:

$$\rho_{ij} \coloneqq \frac{\boldsymbol{F}_{ij} \cdot \boldsymbol{p}}{\boldsymbol{F}_{i0} \cdot \boldsymbol{p}_0}. \tag{13}$$

For example, $\rho_{ij} = 0.8$ means that energy services cost 80 percent as much as they would in the status quo. Define $\Delta \boldsymbol{e}_{ij}^{pred}$ as the 3×1 vector of predicted fuel savings from investment combination j (without the elasticity or weather adjustments), and define $\boldsymbol{\chi}$ as the 3×1 vector of perceived fuel-specific realization rates given households' beliefs. In terms of observables, the perceived energy services cost scaling factor from investment j at price $\boldsymbol{p}$ is

$$\rho_{ij} = \frac{\left(\boldsymbol{e}_{i0} - \boldsymbol{\chi} \circ \Delta \boldsymbol{e}_{ij}^{pred}\right) \cdot \boldsymbol{p}}{\boldsymbol{e}_{i0} \cdot \boldsymbol{p}_0}, \tag{14}$$

where $\circ$ is the element-wise (Hadamard) product.

Define $v_{ij} \coloneqq v_i(\boldsymbol{F}_{ij} \cdot \boldsymbol{p}) - v_i(\boldsymbol{F}_{i0} \cdot \boldsymbol{p})$ as the perceived indirect utility gain (in units of dollars) from choice j at energy prices $\boldsymbol{p}$. We normalize each household's indirect utility to $v_i(\boldsymbol{F}_{i0} \cdot \boldsymbol{p}_0) \equiv 0$ at status quo energy intensity $\boldsymbol{F}_{i0}$ and baseline energy prices $\boldsymbol{p}_0$. Given quasilinear utility and the CRRA functional form from equation (12), we show in Appendix H.E that

$$v_{ij} = \frac{1}{\eta+1} \boldsymbol{e}_{i0}^{*} \cdot \boldsymbol{p}_0 \left(1 - \rho_{ij}{}^{\eta+1}\right). \tag{15}$$

Since $\rho_{ij} = 1$ at status quo energy intensity and baseline energy prices, this equation correctly returns $v_{i0} = 0$ at $\boldsymbol{F}_{i0}$ and $\boldsymbol{p}_0$. If energy services demand is fully inelastic ($\eta = 0$), then v_{ij} is simply the energy expenditure reduction from any change in energy services cost.

VII.A.2 Energy Efficiency Investments

We define $\boldsymbol{X}_j$ as a vector of six indicator variables for whether investment combination j includes investments in each of six types: air sealing, insulation, a heating/cooling system, windows, pipe wrap/duct sealing, and programmable thermostats. $\boldsymbol{X}_j$ also includes an indicator $1_{j>0}$ for any investment (excluding the status quo of $j = 0$). We define $1_{j>0}\sigma\iota_i$ as an idiosyncratic preference for making some investment ($j > 0$) instead of keeping the status quo. We assume that ι_i takes the standard normal distribution, so $\sigma\iota_i$ is normally distributed with mean zero and standard deviation σ. Since households are assumed to know their preferences (including $\sigma\iota_i$) before they audit, households with high ι_i are more likely to audit and invest, generating the self-selection seen in Figure 7. We define ϵ^I_{ij} as a type I extreme value idiosyncratic preference, and we define α^I as a scaling factor. Using those definitions, we parameterize the investment non-energy benefit as $\xi_{ij} = \left(\boldsymbol{\beta}^I \boldsymbol{X}_j + 1_{j>0}\sigma\iota_i + \epsilon^I_{ij}\right)/\alpha^I$.

The perceived indirect utility from choice j (in units of dollars) is thus

$$V^I_{ij} = v_{ij} - p_{ij} + \left(\boldsymbol{\beta}^I \boldsymbol{X}_j + 1_{j>0}\sigma\iota_i + \epsilon^I_{ij}\right)/\alpha^I. \tag{16}$$

Let $\tilde{V}^I_{ij} := \alpha^I V^I_{ij} - \epsilon^I_{ij} = \alpha^I\left(v_{ij} - p_{ij}\right) + \boldsymbol{\beta}^I \boldsymbol{X}_j + 1_{j>0}\sigma\iota_i$ be rescaled indirect utility net of the extreme value error; notice that $\tilde{V}^I_{i0} = 0$ for the status quo investment combination $j = 0$ at baseline energy prices $\boldsymbol{p}_0$. Given the extreme value error assumption, the probability of choosing investment combination j is the standard logit choice probability:

$$\Pr(I_{ij} = 1) = \frac{e^{\tilde{V}^I_{ij}}}{\sum_{k\in\mathcal{J}_i} e^{\tilde{V}^I_{ik}}}. \tag{17}$$

VII.A.3 Home Energy Audits

We assume that the universe of possible choice sets $\mathcal{U}$ is the sample distribution of choice sets for all households that audited in our data.[20] Using this assumption, the expected investment value $\mathbb{E}_{\mathcal{J}\in\mathcal{U}}\left[\max_{j\in\mathcal{J}}\left\{V^I_{ij}\right\}\right]$ can be written by applying the Small and Rosen (1981) logit consumer surplus formula to decision utility. Letting $\mathcal{A}$ and $N^A = 1{,}394$ denote the set and number of households that audited, respectively, and letting n index those households, we have

$$\mathbb{E}_{\mathcal{J}\in\mathcal{U}}\left[\max_{j\in\mathcal{J}}\left\{V^I_{ij}\right\}\right] = \frac{1}{\alpha^I}\frac{1}{N^A}\sum_{n\in\mathcal{A}} \ln\left(\sum_{j\in\mathcal{J}_n} e^{\tilde{V}^I_{nj}(\iota_i)}\right) + \kappa^I, \tag{18}$$

[20] This assumption that all households share the same universe of possible choice sets rules out selection through the choice set, i.e. that some households might know before the audit that they have particularly good energy efficiency opportunities. Appendix Table A17 provides suggestive regressions supporting this assumption: in the sample of households that audited, baseline energy use and the count, energy savings, and cost of proposed investments are all statistically uncorrelated with the randomly assigned audit subsidy.

where κ^I is a constant.

$\tilde{V}^I_{nj}(\iota_i)$ is the rescaled utility that household i perceives (given its ι_i) from investment combination j in the choice set $\mathcal{J}_n$ that was offered to household n in the data. To construct this, we combine the characteristics of investment combination j at household n (perceived indirect utility v_{nj}, cost c_{nj}, subsidy s_{nj}, and characteristics $\boldsymbol{X}_j$) with household i's idiosyncratic preference ι_i.

As defined earlier, L_i is the letter treatment group indicator, and $\boldsymbol{X}_i$ is the five house characteristics from Panel A of Table 1 (house age, property value, building footprint, a Madison indicator variable, and census tract hybrid vehicle share) and a constant.[21] We define ϵ^A_{i1} and ϵ^A_{i0} as type I extreme value idiosyncratic preferences, and we define α^A as a scaling factor. Using those definitions, we parameterize the audit non-energy benefit as $\xi^A_i = \left(\beta^L L_i + \boldsymbol{\beta}\boldsymbol{X}_i + \epsilon^A_{i1}\right)/\alpha^A$.

The perceived indirect utility from auditing (in units of dollars) is thus

$$V^A_{i1} = \mathbb{E}_{\mathcal{J}\in\mathcal{U}}\left[\max_{j\in\mathcal{J}}\left\{V^I_{ij}\right\}\right] - p^A_i + \left(\beta^L L_i + \boldsymbol{\beta}\boldsymbol{X}_i + \epsilon^A_{i1}\right)/\alpha^A, \tag{19}$$

where κ^I from equation (18) has been added to the constant in $\boldsymbol{\beta}$. The indirect utility from not auditing is

$$V^A_{i0} = v_i(\boldsymbol{F}_{i0}\cdot\boldsymbol{p}) + \epsilon^A_{i0}/\alpha^A. \tag{20}$$

Let $\tilde{V}^A_{ik} := \alpha^A V^A_{ik} - \epsilon^A_{ik}, k\in\{0,1\}$ be rescaled utility net of the extreme value error. Given the extreme value error assumption, the probability of auditing is:

$$\Pr(A_i = 1) = \frac{e^{\tilde{V}^A_{i1}}}{e^{\tilde{V}^A_{i0}} + e^{\tilde{V}^A_{i1}}}. \tag{21}$$

VII.A.4 Variable Construction Details

We set s_{ij} equal to the program investment subsidies described in Section IV.A, which are a step function of the household's predicted percent energy savings. Household i's audit subsidy s^A_i equals the experimental audit subsidy (\$0, \$25, or \$100) plus the program audit subsidy (\$200 in Madison, \$300 in Milwaukee). To avoid predictions that an investment combination would save an unrealistic share of household energy costs, we winsorize predicted savings at 75 percent of baseline use for natural gas and electricity, and at 100 percent of baseline use for heating oil. This affects 1.9 percent of investment combinations.

VII.B Estimation Procedure

We estimate the model by method of simulated moments. The parameters to be estimated are $\boldsymbol{\Theta} := \left[\alpha^A, \boldsymbol{\beta}^A, \alpha^I, \boldsymbol{\beta}^I, \sigma\right]$, where $\boldsymbol{\beta}^A := \left[\beta^L, \boldsymbol{\beta}\right]$. To simulate the audit and investment choice probabilities for household i, we take $M = 1000$ draws of $\iota \sim N(0,1)$, indexing draws by m. As a benchmark, we also estimate a fixed coefficient model with $\sigma = 0$.

[21] We exclude the gift card group indicator for simplicity, as Table 3 showed a point estimate of almost exactly zero effect.

There are three groups of moments. The first group sets the audit takeup residuals orthogonal to $\boldsymbol{x}_i^A := \left[-p_i^A, L_i, \boldsymbol{X}_i\right]$, the vector of observables in rescaled audit indirect utility $\tilde{V}_i^A$:

$$\boldsymbol{g}^A(\boldsymbol{\Theta}) = \frac{1}{N}\sum_i \boldsymbol{x}_i^A \left[A_i - \frac{1}{M}\sum_m \Pr\left(A_i = 1|\iota_m; \boldsymbol{\Theta}\right)\right], \tag{22}$$

where $\Pr\left(A_i = 1|\iota_m; \boldsymbol{\Theta}\right)$ is from equation (21). In the fixed coefficient model with $\sigma = 0$, household i's choice probability is simply $\Pr(A_i = 1|\boldsymbol{\Theta})$, so these moments are the same as in the standard logistic estimator. α^A and $\boldsymbol{\beta}^A$, the parameters multiplying $\boldsymbol{x}_i^A$, will be most sensitive to these moments.

The second group of moments analogously sets the investment takeup residuals orthogonal to $\boldsymbol{x}_{ij}^I := [v_{ij} - p_{ij}, \boldsymbol{X}_j]$, the vector of observables in rescaled investment indirect utility $\tilde{V}_{ij}^I$:

$$\boldsymbol{g}^I(\boldsymbol{\Theta}) = \frac{1}{N}\sum_i A_i \sum_{j\in\mathcal{J}_i} \boldsymbol{x}_{ij}^I \left[I_{ij} - \frac{\frac{1}{M}\sum_m \Pr\left(A_i = 1|\iota_m; \boldsymbol{\Theta}\right)\cdot \Pr\left(I_{ij} = 1|\iota_m; \boldsymbol{\Theta}\right)}{\frac{1}{M}\sum_m \Pr\left(A_i = 1|\iota_m; \boldsymbol{\Theta}\right)}\right], \tag{23}$$

where $\Pr\left(I_{ij} = 1|\iota_m; \boldsymbol{\Theta}\right)$ is from equation (17). The simulated choice probability accounts for how ι affects both the audit and investment probabilities. In the fixed coefficient model with $\sigma = 0$, the audit probabilities in the numerator and denominator cancel, so household i's choice probability is simply $\Pr\left(I_{ij} = 1|\boldsymbol{\Theta}\right)$, and these moments are the same as in the standard logit estimator. α^I and $\boldsymbol{\beta}^I$, the parameters multiplying $\boldsymbol{x}_{ij}^I$, will be most sensitive to these moments.

The final moment sets investment takeup residuals orthogonal to the experimental audit subsidy S_i, within the set of households that received a letter ($L_i = 1$). Letting $I_i \in \{1, 0\}$ (with no j subscript) be an indicator for making any energy efficiency investment ($j > 0$), this is:

$$g^S(\boldsymbol{\Theta}) = \frac{1}{N}\sum_i L_i A_i S_i \left[I_i - \frac{\frac{1}{M}\sum_m \Pr\left(A_i = 1|\iota_m; \boldsymbol{\Theta}\right)\cdot \Pr\left(I_i = 1|\iota_m; \boldsymbol{\Theta}\right)}{\frac{1}{M}\sum_m \Pr\left(A_i = 1|\iota_m; \boldsymbol{\Theta}\right)}\right], \tag{24}$$

where $\Pr\left(I_i = 1|\iota_m; \boldsymbol{\Theta}\right) = \sum_{j\in\mathcal{J}_i\setminus\{0\}} \Pr\left(I_{ij} = 1|\iota_m; \boldsymbol{\Theta}\right)$. This moment captures the selection effect—the relationship between audit subsidy and investment probability conditional on auditing—illustrated in Figure 7. In the fixed coefficient model with $\sigma = 0$, we drop this moment. σ will be most sensitive to this moment.

Define $\boldsymbol{g}(\boldsymbol{\Theta}) = \left[\boldsymbol{g}^A(\boldsymbol{\Theta}), \boldsymbol{g}^I(\boldsymbol{\Theta}), g^S(\boldsymbol{\Theta})\right]$ as the column vector of stacked moments. The method of simulated moments estimator is

$$\hat{\boldsymbol{\Theta}}^* = \arg\min_{\boldsymbol{\Theta}} \boldsymbol{g}(\boldsymbol{\Theta})'\boldsymbol{W}(\boldsymbol{\Theta})\boldsymbol{g}(\boldsymbol{\Theta}), \tag{25}$$

where $\boldsymbol{W}(\boldsymbol{\Theta})$ is a weighting matrix, which we estimate in a standard two-step procedure.

Consistency requires that the instruments $\boldsymbol{x}^A$, $\boldsymbol{x}^I$, and S are exogenous. A key strength of our analysis is that we exploit the randomized experimental audit subsidy S to identify the audit price response α^A and selection parameter σ. A key limitation is that the term $v_{ij} - p_{ij}$, which identifies the investment price response α^I, is not randomized and thus could be correlated with unobserved utility ϵ_{ij}.[22] For example, ϵ_{ij} might be positively correlated with indirect utility v_{ij} if more energy conservation brings more warm glow utility or in-home comfort. Alternatively, ϵ_{ij} might be negatively correlated with price p_{ij} if higher-cost investments also require more time and effort to carry out. In the welfare analysis, we will explore robustness to an alternative value of α^I.

Industry estimates suggest that direct mail open rates are about 90 percent (Robinson 2021). Households that did not open our promotional letters would have been unaware of the experimental audit subsidy, so the true audit price response may be slightly higher than we estimate.

VII.C Parameter Estimates

Table 4 presents parameter estimates. Columns 1 and 2 present the results of the fixed coefficient model with $\sigma = 0$, while columns 3 and 4 present the full random coefficient model. Columns 1 and 3 construct v_{ij} under the initial assumption that consumers believed the energy savings predictions ($\boldsymbol{\chi} = 1$), while Columns 2 and 4 construct v_{ij} assuming that consumers fortuitously knew our estimates of the actual $\boldsymbol{\chi}$ from Section VI.A.

Panel A presents the audit parameters α^A and $\boldsymbol{\beta}^A$. The audit dollarized benefit refers to $\mathbb{E}_{\mathcal{J}\in\mathcal{U}}\left[\max_{j\in\mathcal{J}}\left\{V_{ij}^I\right\}\right] - p_i^A$, which multiplies α^A in rescaled utility $\tilde{V}_i^A$. Since the expected investment value $\mathbb{E}_{\mathcal{J}\in\mathcal{U}}\left[\max_{j\in\mathcal{J}}\left\{V_{ij}^I\right\}\right]$ and audit cost c_i^A are constant across observations and the program subsidy varies by city (which we include in $\boldsymbol{X}_i$), the only variation that identifies α^A is the experimental audit subsidy. Consistent with the reduced-form results from Table 3, higher subsidies increase audit takeup. Also consistent with those reduced-form results, receiving a letter, house age, building footprint, the Madison indicator, and the census tract hybrid vehicle share are positively associated with audit takeup.

Panel B presents the investment parameters α^I and $\boldsymbol{\beta}^I$ as well as the selection parameter σ. The investment dollarized benefit refers to $v_{ij} - p_{ij}$. This is the same as the predicted financial benefit illustrated in Figure 8, except that it accounts for elastic energy services demand. Consistent with Figure 8, dollarized benefit is positively associated with investment takeup in columns 1 and 3. Air sealing and insulation have particularly high takeup (and thus higher β^I) conditional on dollarized benefit, while windows and pipe wrap/duct sealing have particularly low takeup (and thus more negative β^I). The investment constant is more negative in column 3 than in column 1, because the simulated households that invest with $\sigma \neq 0$ have high draws of ι that push down the constant.

Column 2 shows that when we assume that consumers somehow knew our estimates of actual realization rates, the estimated α^I becomes statistically insignificant and the point estimate is negative (i.e., wrong-signed). In other words, the empirically adjusted net financial benefits do not predict takeup, while the unadjusted benefits in column 1 do. This provides further suggestive evidence that consumers may have taken the unadjusted savings predictions at face value. Since α^I must theoretically be positive, column 4 presents random coefficient estimates assuming consumers knew actual savings, but fixing α^I at the estimate from column 3 (and correspondingly dropping

[22] In theory, the program's step function investment subsidy could deliver quasi-random variation in s_{ij}, but our sample is not large enough to deliver sufficient power.

$v_{ij} - p_{ij}$ from $\boldsymbol{x}^I$ in the $\boldsymbol{g}^I$ moment). The primary change from column 3 is that the investment constant becomes less negative to continue matching the average takeup rates despite the reduction in perceived benefits from investments.

VIII Welfare Analysis and Counterfactuals

In this section, we use the model and parameter estimates from Section VII to simulate the welfare effects of the Wisconsin subsidies and counterfactual policies.

VIII.A Total Surplus Calculation

We consider effects on total surplus in our sample of $N = 101{,}881$ Wisconsin households, given their observable characteristics and $M = 5$ draws per household of the investment takeup unobservable ι. We compute total surplus using equation (7), given the estimated utility function parameters from Table 4 and the audit and investment choice probabilities from equations (17) and (21).

In our initial estimates, we assume that households believed the predicted savings the programs had provided. Given those beliefs, the total perceived consumer surplus can be written by applying the Small and Rosen (1981) formula to perceived utility from audit takeup:

$$CS = \sum_i \frac{1}{M} \sum_m \frac{1}{\alpha^A} \ln\left(\exp\left(\tilde{V}_{i0}^A\right) + \exp\left(\tilde{V}_{i1}^A\left(\iota_m\right)\right)\right) + \kappa^A, \tag{26}$$

where κ^A is an unknown constant, and $\tilde{V}_{i1}^A$ depends on the expected investment value as described in equation (19).

In our initial estimates, we assume that our realization rates estimated in Section VI.A identify the actual energy savings. Define $\hat{\boldsymbol{\chi}}$ as the empirically estimated ("actual") realization rate vector, and define $\hat{\rho}_{ij}$ and $\hat{v}_{ij}$ as the actual energy services cost scaling factor and indirect utility calculated using $\hat{\boldsymbol{\chi}}$. The investment takeup distortion is the difference between actual and perceived indirect utility:

$$\gamma_{ij} = \hat{v}_{ij} - v_{ij}. \tag{27}$$

Except for a few investments that primarily saved electricity, predicted savings exceeds our estimate of actual savings, so $v_{ij} > \hat{v}_{ij}$. Thus, γ_{ij} is mostly negative, reflecting a utility loss relative to what consumers expected.

Given the CRRA functional form from equation (12), we show in Appendix H.F that household i's energy consumption after choosing investment combination j is

$$\boldsymbol{e}_{ij}^* = \rho_{ij}^{\eta} \boldsymbol{e}_{i0}^* \circ \left(\mathbf{1} - \boldsymbol{\chi} \circ \Delta \boldsymbol{e}_{ij}^{pred} \oslash \boldsymbol{e}_{i0}\right), \tag{28}$$

where $\oslash$ is the element-wide division operator. The term ρ_{ij}^{η} captures how energy services demand responds to the change in energy services cost, while the term $\left(\mathbf{1} - \boldsymbol{\chi} \circ \Delta \boldsymbol{e}_{ij}^{pred} \oslash \boldsymbol{e}_{i0}\right)$ captures the change in energy intensity from investment j.

To focus on the corrective benefits of taxes and subsidies, we set the marginal cost of public funds (MCPF) to $\lambda = 1$ for our primary estimates. Setting $\lambda = 1$ as a benchmark also allows us to abstract away from how the MCPF probably varies across policy instruments (Kleven and Kreiner 2006).[23]

VIII.B Welfare Evaluation

VIII.B.1 Assuming Households Believed Savings Predictions

Table 5 presents the simulated effects of the Wisconsin program subsidies and alternative policies, all compared to a baseline scenario with zero subsidies and zero energy taxes. Panel A lists the column from Table 4 from which we draw preference parameters $\mathbf{\Theta}$, as well as the policies: whether the programs provide unbiased savings predictions (i.e., predictions adjusted by our realization rates from Section VI.A) as well as the average audit and investment subsidy offers. The \$269 average audit subsidy reflects the \$200 and \$300 audit subsidies in Madison and Milwaukee and the fact that 69 percent of the sample is in Milwaukee. Panel B describes the effects on audit and investment probabilities, investment costs, and carbon emissions.

Panel C presents welfare effects, including the effects on perceived consumer surplus (using equation (26)), the investment takeup distortion (using the γ_{ij} from equation (27)), producer surplus, environmental externalities, and government spending, as well as total surplus (the sum of the first five rows of the panel, equivalent to equation (7)). These effects are discounted sums over 20-year investment lifetimes. The marginal value of public funds (MVPF) is the change in consumer surplus, producer surplus, and negative externality reduction per dollar of government spending (Hendren and Sprung-Keyser 2020). The social cost of carbon abatement is the change in total surplus (excluding carbon externality reductions) per ton of CO_2 reduction. We normalize effects by the number of households in the population, not the number of program participants, because program participation rates vary across counterfactuals. Since most households don't participate, the effects per household in the population are quite small.

Columns 1 and 2 use the preferences $\mathbf{\Theta}$ from column 3 of Table 4, which were estimated under the assumption that the Wisconsin households believed the programs' savings predictions. Column 1 is our primary evaluation of the Wisconsin programs as implemented. The model predicts that the Wisconsin program subsidies increased the audit probability by 0.15 percentage points but decreased the average investment probability among auditors by about 8 percentage points, due to the selection effects illustrated in Figure 7. The combination of these two effects was to increase investment probability by only 0.02 percentage points. Thus, audits responded more to the program subsidies than investments.

[23] In our setting, energy taxes would accrue to state or federal governments and could theoretically offset income or property taxes, while energy efficiency subsidies are generally funded by utilities or quasi-governmental agencies through energy price surcharges. The deadweight loss from these surcharges depends on the difference between energy price and social marginal cost, which varies by fuel and location per the discussion in Section V.C.

Panel C shows that the largest-magnitude surplus effects are increases in government spending and consumer surplus from the subsidies. Overall, the program subsidies *reduced* modeled total surplus by \$0.82 per household in the population. As we unpack further below, this negative effect arises in our model both because the savings predictions were inflated and because the subsidies were not aligned with uninternalized externality reductions.

In the model, the subsidies had an MVPF of 0.93, meaning that each dollar of subsidy outlays increased consumer surplus, producer surplus, and negative externality reductions by \$0.93. As a comparison, Hendren and Sprung-Keyser (2020) find MVPFs of 0.5 to 2 for social programs targeting adults, and much higher MVPFs for education and child health programs. Many of the policies evaluated in Hendren and Sprung-Keyser (2020) are for social programs that benefit lower-income people, so lower MVPFs might be acceptable. In contrast, energy efficiency programs are primarily corrective instead of redistributive, and Table 3 shows that people with lower property values were less likely to participate in the Wisconsin programs. This is consistent with prior evidence from Allcott, Knittel, and Taubinsky (2015) and Borenstein and Davis (2016) that energy efficiency rebates and tax incentives primarily go to higher-income Americans.

In the model, the program subsidies abated carbon dioxide emissions at an average cost of \$365 per ton, which is larger than our primary assumption for the social cost of carbon. This is less cost effective than the primary estimate of \$200 per ton from Fowlie, Greenstone, and Wolfram's (2018) evaluation of the Weatherization Assistance Program in Michigan, although our methodology differs substantially.

Column 2 evaluates a scenario where we use the same preference parameters $\boldsymbol{\Theta}$ but construct v_{ij} for the counterfactuals assuming that consumers correctly perceive the empirical realization rates $\hat{\boldsymbol{\chi}}$. This might correspond to a counterfactual program that provides unbiased energy savings predictions. Total surplus effects are now positive, and the MVPF rises to 1.01. This underscores the potential harms from giving consumers biased savings predictions, and the conversely the potential benefits from changing that practice.

VIII.B.2 Assuming Households Knew Our Estimates of Actual Savings

Columns 3–7 use the preference parameters $\boldsymbol{\Theta}$ from column 4 of Table 4, which were estimated under the assumption that the Wisconsin households somehow had correct beliefs corresponding to our estimates of actual energy savings. Column 3 shows that under that alternative assumption, the Wisconsin subsidies generate a slight increase in total surplus.

Columns 4–6 progressively modify the modeled subsidies to demonstrate the differences between the Wisconsin subsidies and the second-best subsidies from Lemma 2: zero audit subsidy and investment subsidies equal to uninternalized externality reductions. Column 4 eliminates the audit subsidy, column 5 eliminates subsidy "noise" (i.e., makes investment subsidies proportional to the uninternalized externality reduction) but maintains the same average subsidy offer of \$1,020 per portfolio, and column 6 is the second best. Most of the total surplus gains between columns 3 and 6 come from eliminating the audit subsidy in column 4. The total surplus gains from eliminating the investment subsidy "noise" in column 5 are smaller, and the incremental gains from getting the exact levels right in column 6 are smaller still.[24] Eliminating audit subsidies is relatively important in our model because audits don't directly generate any environmental benefits, and audits respond more to the program subsidies than investments due to the self-selection effects illustrated in Figure 7.

Column 7 evaluates the first-best policy from Lemma 1: eliminate energy efficiency subsidies and directly offset uninternalized externalities through corrective taxation. The results are striking: while audit and investment takeup don't change much, total surplus increases by \$2,027 per household in the population—three orders of magnitude larger than the gains from the second-best energy efficiency subsidies. There are two reasons why corrective taxation is so much better than energy efficiency subsidies in our model. First, very few households participate in energy efficiency programs, while energy taxes affect all households. Second, even for households that do participate, energy efficiency subsidies do not address the distortion to energy services use when energy prices don't equal social marginal cost.

Our model is not designed to cleanly evaluate this first mechanism: over the 20-year period of energy use we model, presumably more households would have audits than we see in our 16 months of takeup data. One benchmark assumption could be that takeup rates per unit time would have remained constant if the programs continued over 20 years instead of 16 months, in which case the audit takeup probability would be $(20 \times 12/16) \times 0.15\% \approx 2.3\%$. This suggests that the great majority of households would still not participate, and thus could only be affected by energy taxes.

We can isolate the second mechanism by computing the total surplus gains from applying energy taxes only to the subset of households induced to audit by the Wisconsin subsidies. Given the audit takeup effect of 0.15% from column 1 of Table 5, this is $\$2{,}027 \times 0.15\% \approx \$3.02$, which is still about 8 times larger than even the second-best subsidies. Thus, even if we equalize the number of households affected by optimal energy efficiency subsidies and optimal corrective taxation, corrective taxation eliminates much more deadweight loss.

VIII.B.3 Alternative Assumptions

Table 6 presents the effects of the Wisconsin subsidies under alternative assumptions. Column 1 replays the base case, identical to column 1 of Table 5. Column 2 presents an alternative analysis assuming we had estimated realization rates of $\boldsymbol{\chi} = \mathbf{1}$. This has two effects. First, it eliminates the investment takeup distortion. Second, it increases our accounting of the environmental benefits from energy efficiency investments. As a result, the total surplus effect would increase to \$1.42 per household and the MVPF would increase to 1.13.

Column 3 adjusts the social cost of carbon to \$51 in 2020 dollars, the Biden Administration's initial value (Interagency Working Group 2021), while column 4 increases our primary social cost of carbon assumption to \$250, and column 5 doubles all environmental externalities. Even in column

[24] Note that the MVPF and social cost of carbon abatement are both slightly worse for the second-best policy in column 6 compared to column 5. This is to be expected: the policy that maximizes total surplus may not maximize the MVPF or minimize the social cost per ton of carbon abated.

5, the programs still reduce total surplus.

Column 6 considers the possibility of additional market failures that could distort investment takeup. Myers (2019) finds that changes in energy costs from changes in oil and gas prices over time are fully capitalized into house prices. However, if changes in energy costs from energy efficiency investments are not fully capitalized and if potential program participants know this, this would reduce people's incentives to invest. To capture this, we add an investment takeup distortion γ_{ij} equal to half of the discounted lifetime retail energy cost savings. This does not affect investment takeup, but it does increase how our model values the investments that are taken up. This change increases the total surplus effect to $\$-0.06$ per household.

Finally, column 7 considers a higher marginal cost of public funds of $\lambda = 1.4$. Relative to column 1, this worsens the total surplus effect because it further penalizes the cost of energy efficiency subsidies. Mechanically, this does not affect the MVPF relative to column 1.

The total surplus effect, MVPF, and cost of carbon abatement do not change substantively when we fix α^I at three times its estimated value, assume inelastic energy demand ($\eta = 0$), or use a five percent discount rate; see Appendix Table A19. Assuming no selection ($\sigma = 0$) also changes the modeled audit demand slope and predicted effects on audit takeup, so the total surplus effect correspondingly becomes larger.

In Appendix I, we present an alternative accounting-style evaluation that dispenses with the structural model and simply compares observed benefits and costs. Specifically, we compare the observed costs of audits and investments to the discounted energy cost savings (valued at acquistion costs from row 2 of Table 2) and externality reduction benefits (valued using row 4 of Table 2). This parallels the approaches that energy efficiency program evaluators typically use, but it doesn't account for unobserved benefits and costs and cannot evaluate counterfactual policies. There are two key results. First, the accounting approach is consistent with our structural model in finding that the programs' costs outweighed the benefits: the benefit/cost ratio is only 0.88. Second, we show that this result generalizes outside of the Wisconsin programs: using separate administrative microdata, we find that comparable programs implemented nationwide had a slightly worse benefit/cost ratio.

VIII.C Additional Caveats and Discussion

We emphasize several additional caveats. First, like many structural models, we restrict the possible types of parameter heterogeneity: the idiosyncratic preferences ϵ^I, ϵ^A, and ι vary across people, but the other parameters are homogeneous.

Second, the modeled effects on audit and investment takeup relative to the zero-subsidy baseline scenario should be interpreted cautiously because a \$0 audit subsidy is far outside the support of our experimental subsidy variation. The logit and normal functional forms for ϵ^I, ϵ^A, and ι imply specific parametric demand functions that may or may not be realistic when extrapolated out of sample. The model's predicted effects of program subsidies on audit takeup in Table 5 are smaller than would be predicted under an alternative assumption of linear audit demand. With a larger audit takeup response, the magnitudes of total surplus effects would be larger, but the qualitative conclusions about the sources of distortions would not change.

Third, we use a static model of energy efficiency investment decisions. An alternative approach would be to model this as a dynamic discrete choice problem where consumers choose if and when to audit and invest, given their expectations of future energy prices and subsidies, analogous to prior work in other settings (Rust 1987; Hendel and Nevo 2006; Gowrisankaran and Rysman 2012). If consumers correctly predicted that the program subsidies were time-limited, the subsidies might have pulled forward some investments that would have been made over the next few years even in the absence of subsidies. In that case, the magnitudes of total surplus effects would be smaller, but the qualitative conclusions would again likely be unchanged.

Fourth, the rational expectations assumption introduced in Section III.B.3 is useful in pinning down pre-audit beliefs, and it seems intuitively realistic: people should be more likely to have an audit if they hear that that the subsidies are generous or the investments save a lot of money. However, we do not have direct evidence on how households decide whether to audit, and one could imagine alternative scenarios under which unbiased savings predictions don't matter as much as our model predicts in column 2 of Table 5.

Fifth, the total surplus calculations exclude overhead costs such as program design and administration, training, and marketing. The Wisconsin Better Buildings programs incurred \$0.46 in overhead costs for every \$1 of retrofit costs (DOE 2015b). We focus our benefit-cost analyses only on subsidy expenditures because the overhead costs may have had additional benefits that are difficult to quantify: program staff report that much of overhead costs represented investments intended to support an economically sustainable home retrofit market even after the stimulus funds were exhausted (Curtis 2017). Adding overhead costs to our calculations would of course reduce net benefits.

IX Conclusion

Using a randomized experiment and administrative data, we document two facts about home energy efficiency retrofit programs: energy savings fall short of the predictions from engineering models, and subsidies are not closely aligned with the environmental externalities that the programs were originally intended to address. Prior literature on realization rates and our survey work on the current top 10 energy efficiency programs suggest that both of these facts apply to energy efficiency programs across the country, not just the Wisconsin programs. In our model, the programs had a marginal value of public funds of 0.93, while a second-best optimal program would have an MVPF of 1.03, and corrective taxation would generate several orders of magnitude more benefits. These results speak to the challenges that arise when societies try to solve externality problems through approaches other than corrective taxation. Nevertheless, we think of our results as optimistic and constructive, as they point to concrete and feasible changes that could help energy efficiency programs achieve their full potential.

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Tables and Figures

Table 1: **Summary Statistics for Wisconsin Experimental Sample**

	Mean	Std. dev.	Min.	Max.
Panel A: House Characteristics				
House age (years)	67.18	23.03	0	182
Property value ($) / 1,000,000	0.16	0.09	0	2.82
Building footprint (sq. feet) / 1000	1.24	0.43	0	12.06
Madison	0.31	0.46	0	1
Census tract hybrid vehicle share	1.21	1.27	0	8.33
N=101,881 households				
Panel B: Proposed Investments				
Rated lifetime (years)	19.6	1.5	11	20
Cost ($)	1,486	1,370	17	20,000
Predicted total energy savings (kBtu/year)	8,794	10,339	−69	161,047
Predicted retail energy cost savings ($/year)	86	145	0	4,359
Invested	0.51	0.50	0	1
N=6,100 at 1,379 households				
Panel C: Natural Gas and Electricity Use				
Natural gas use (therms/day)	2.51	2.38	0	27.76
Electricity use (kWh/day)	20.70	12.30	0	217.97
N=61,846 observations at 1,192 households (natural gas)				
N=63,654 observations at 1,197 households (electricity)				

Notes: In Panel A, house age, property value, and building footprint are from county administrative data. Census tract hybrid vehicle share is the percent of registered vehicles in the Census tract that are hybrids, potentially ranging from 0 to 100. In Panel B, proposed investments include only those used for empirical estimates of investment takeup. Retail energy cost savings use average energy prices over 2011–2014. In Panel C, energy bills are observed only if a household had an audit and signed a release form.

Table 2: **Energy Price and Externality Assumptions ($/million Btu)**

Row		(1) Natural gas	(2) Electricity	(3) Heating oil
1	Marginal retail price	$8.19	$39.76	$25.17
2	Marginal acquisition cost	$5.44	$10.04	$25.17
3	Retail markup (row 1 – row 2)	$2.75	$29.73	$0
4	Environmental externality	$16.28	$51.13	$23.75
5	"Uninternalized externality" (row 4 – row 3)	$13.53	$21.41	$23.75

Notes: This table presents energy price and externality assumptions for the three main fuels in the data. All columns are reported in common units of dollars per million Btu. Energy prices are averages over 2011–2014, and externality savings are based on a $172 per ton social cost of carbon (in 2013 dollars). See Appendix D.C for details.

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

Dependent variable:	(1) Audited	(2) Invested
Sent letter	0.252	-0.007
	(0.175)	(0.132)
\$25 audit subsidy	0.233	0.067
	(0.107)**	(0.082)
\$100 audit subsidy	0.591	0.153
	(0.206)***	(0.149)
\$25 gift card	0.017	-0.118
	(0.107)	(0.080)
House age (years)	0.016	0.010
	(0.002)***	(0.001)***
Property value (\$) / 1,000,000	1.670	0.513
	(0.715)**	(0.543)
Building footprint (sq. feet) / 1000	0.406	0.148
	(0.117)***	(0.088)*
Madison	0.066	-0.020
	(0.119)	(0.089)
Census tract hybrid vehicle share	0.256	0.152
	(0.055)***	(0.041)***
N	101,881	101,881
Control group mean (percent)	1.2	.8

Notes: This table presents estimates of equation (10), a linear probability model of audit takeup (in column 1) or energy efficiency investment takeup (in column 2). Coefficients are multiplied by 100 for readability. Robust standard errors are in parentheses. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

Table 4: **Takeup Parameter Estimates**

	(1) Believe predictions, $\sigma = 0$	(2) Know actual savings, $\sigma = 0$	(3) Believe predictions	(4) Know actual savings, column (3) $\hat{\alpha}^I$
Panel A: Audit Parameters				
Dollarized benefit / 10,000 (α^A)	39.38	39.38	139	139
	(10.62)	(10.62)	(75.65)	(76.25)
Received letter	0.12	0.12	1.06	1.06
	(0.07)	(0.07)	(1.23)	(1.23)
House age (years) / 100	1.04	1.04	5.45	5.47
	(0.11)	(0.11)	(3.36)	(3.39)
Property value ($) / 1,000,000	0.40	0.40	−1.41	−1.43
	(0.31)	(0.31)	(1.86)	(1.85)
Building footprint (sq. feet) / 1,000	0.22	0.22	0.91	0.92
	(0.05)	(0.05)	(0.56)	(0.56)
Madison	0.54	0.54	3.14	3.16
	(0.14)	(0.14)	(2.17)	(2.17)
Census tract hybrid vehicle share	0.15	0.15	0.57	0.57
	(0.03)	(0.03)	(0.35)	(0.36)
Constant	−66.93	138	−13.28	−13.33
	(23.11)	(109)	(6.55)	(6.61)
Panel B: Investment Parameters				
Dollarized benefit / 10,000 (α^I)	0.74	−0.32	1.63	1.63
	(0.21)	(0.22)	(0.57)	–
Air sealing	2.82	2.86	3.15	3.26
	(0.15)	(0.16)	(0.22)	(0.23)
Insulation	1.56	1.57	3.62	3.59
	(0.20)	(0.20)	(1.14)	(1.12)
Heating/cooling system	0.01	−0.15	0.41	0.52
	(0.11)	(0.11)	(0.19)	(0.14)
Windows	−0.89	−1.24	−0.52	−0.52
	(0.32)	(0.34)	(0.39)	(0.35)
Pipe wrap/duct sealing	−0.23	−0.19	−0.23	−0.21
	(0.65)	(0.65)	(0.68)	(0.68)
Programmable thermostat	0.66	0.69	1.09	1.08
	(0.27)	(0.28)	(0.44)	(0.45)
Constant	−6.14	−6.14	−77.19	−71.29
	(0.30)	(0.30)	(21.09)	(22.74)
Standard deviation of ι (σ)	0	0	28.05	25.71
	-	-	(8.51)	(9.16)
N	101,881	101,881	101,881	101,881

Notes: This table presents estimates of the audit and investment takeup parameters using the method of simulated moments estimator described in Section VII.B. Audit dollarized benefit refers to $\mathbb{E}_{\mathcal{J}\in\mathcal{U}}\left[\max_{j\in\mathcal{J}}\left\{V_{ij}^I\right\}\right] - p_i^A$; investment dollarized benefit refers to $v_{ij} - p_{ij}$.

Table 5: **Effects of Wisconsin Subsidies and Counterfactual Policies**

	(1) Wisconsin subsidies	(2) Wisconsin subsidies	(3) Wisconsin subsidies	(4) Zero audit subsidy	(5) Investment subsidies $\propto$ uninternalized externality	(6) Second best	(7) First best
Panel A: Preferences, Savings Predictions, and Subsidy Offers							
Preferences: column from Table 4	3	3	4	4	4	4	4
Unbiased savings predictions	No	Yes	Yes	Yes	Yes	Yes	Yes
Average audit subsidy ($)	269	269	269	0	0	0	0
Average investment subsidy ($/combination)	1,020	1,020	1,020	1,020	1,020	1,793	0
Panel B: Effects on Takeup, Spending, and Carbon Emissions							
Δ Audit probability (%)	0.15	0.14	0.15	0.02	0.02	0.04	0.04
Δ Investment probability | auditing (%)	−8.16	−8.08	−7.95	−0.20	−0.35	−0.69	−0.65
Δ Investment probability (%)	0.02	0.02	0.02	0.01	0.01	0.02	0.02
Δ Investment costs ($/household)	1.42	1.34	1.45	1.22	1.22	2.18	2.00
CO_2 emissions reduction (tons/household)	0.004	0.004	0.004	0.004	0.004	0.007	25.022
Panel C: Effects on Surplus							
ΔPerceived consumer surplus ($/household)	10.79	9.91	10.36	7.43	7.18	12.87	-25,115
ΔInvestment takeup distortion ($/household)	−0.91	0	0	0	0	0	0
ΔProducer surplus ($/household)	−0.21	−0.22	−0.23	−0.19	−0.27	−0.47	-1,934
ΔEnvironmental externalities ($/household)	0.87	0.84	0.90	0.77	0.92	1.62	5,491
ΔGovernment spending ($/household)	11.35	10.46	10.93	7.74	7.48	13.65	-23,585
ΔTotal surplus ($/household)	−0.82	0.08	0.10	0.26	0.35	0.37	2,027
Marginal value of public funds	0.93	1.01	1.01	1.03	1.05	1.03	0.91
Cost of carbon abatement ($/ton)	365	153	150	101	90	122	91

Notes: This table evaluates the Wisconsin programs and counterfactual policies. Column 5 sets investment subsidies proportional to uninternalized externalities while holding constant the Wisconsin programs' average investment subsidy offer. Column 6 is the second-best policy from Lemma 2: set investment subsidies equal to uninternalized externality reductions. Column 7 is the first-best policy from Lemma 1: set energy taxes equal to uninternalized externalities. Climate externality reductions are based on a $172 per ton social cost of carbon (in 2013 dollars). The marginal value of public funds equals (ΔPerceived consumer surplus + ΔInvestment takeup distortion + ΔProducer surplus + ΔEnvironmental externalities) / ΔGovernment spending. The cost of carbon abatement equals (ΔTotal surplus – social cost of carbon × ΔCO_2) / ΔCO_2.

Table 6: **Effects of Wisconsin Subsidies Under Alternative Assumptions**

	(1) Base case	(2) Unadjusted energy savings	(3) \$51 social cost of carbon	(4) \$250 social cost of carbon	(5) Double all environmental externalities	(6) Additional investment takeup distortion (half of energy savings)	(7) Higher marginal cost of public funds ($\lambda = 1.4$)
ΔTotal surplus (\$/household)	−0.82	1.42	−1.35	−0.59	−0.15	−0.06	−5.36
Marginal value of public funds	0.93	1.13	0.88	0.95	0.99	1.00	0.93
Cost of carbon abatement (\$/ton)	365	55	365	365	190	185	1,439

Notes: This table evaluates the Wisconsin programs under alternative assumptions. Column 1 presents the base case from column 1 of Table 5. Column 2 presents an alternative analysis assuming we had estimated realization rates of $\boldsymbol{\chi = 1}$. Columns 3 and 4 results with \$51 and \$250 social costs of carbon (in 2020 dollars). Column 5 doubles all environmental externalities. Column 6 adds an investment takeup distortion representing additional benefits from investments equal to half of retail energy cost savings. Column 7 presents an alternative analysis assuming a marginal cost of public funds of $\lambda = 1.4$. Climate externality reductions are based on a \$172 per ton social cost of carbon (in 2013 dollars).

Figure 1: **Program Participation Model**

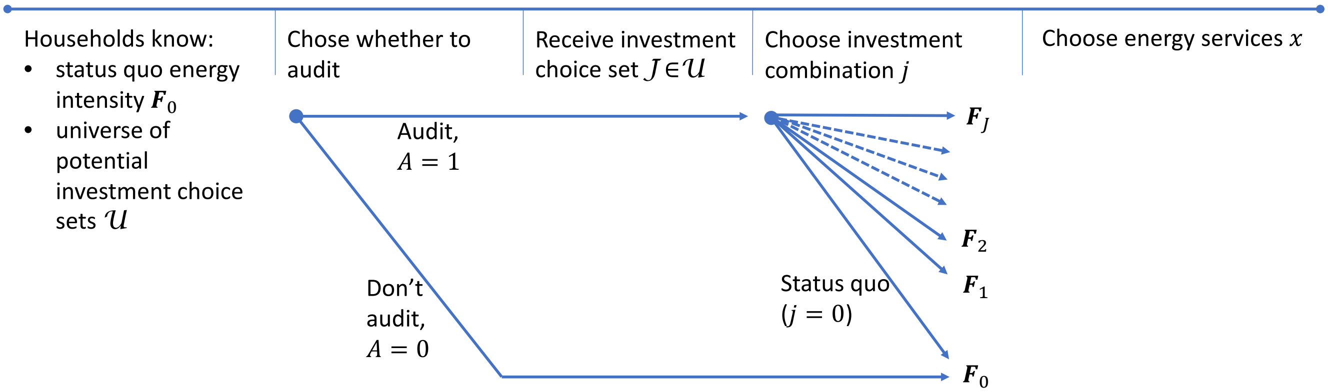
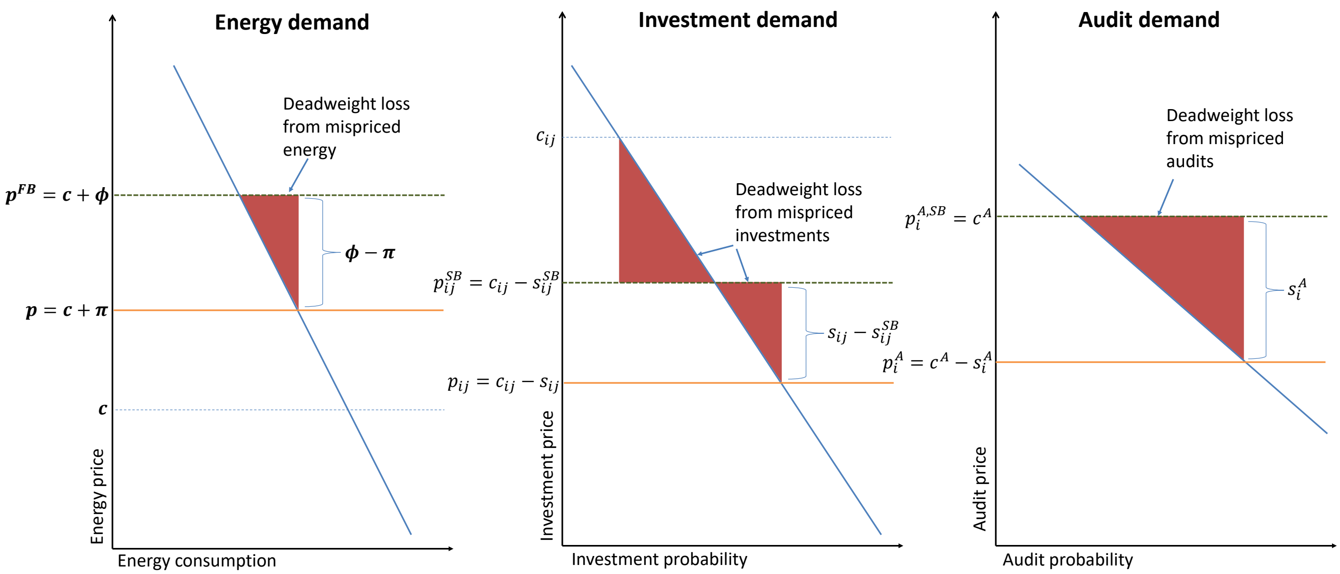
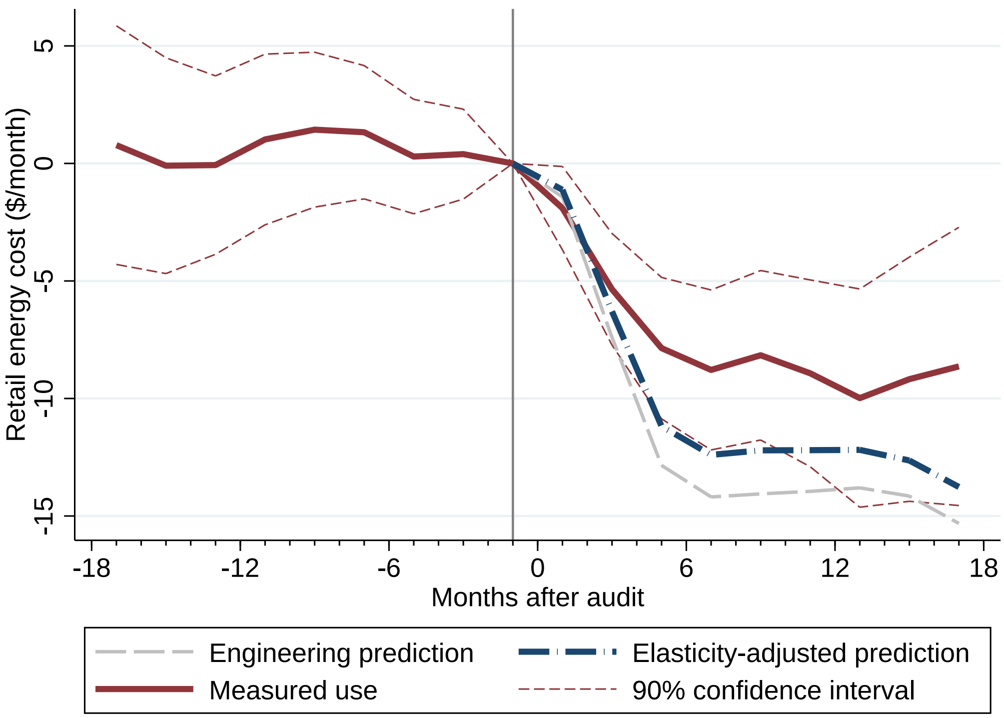


Figure 2: **Deadweight Losses from Suboptimal Policies**



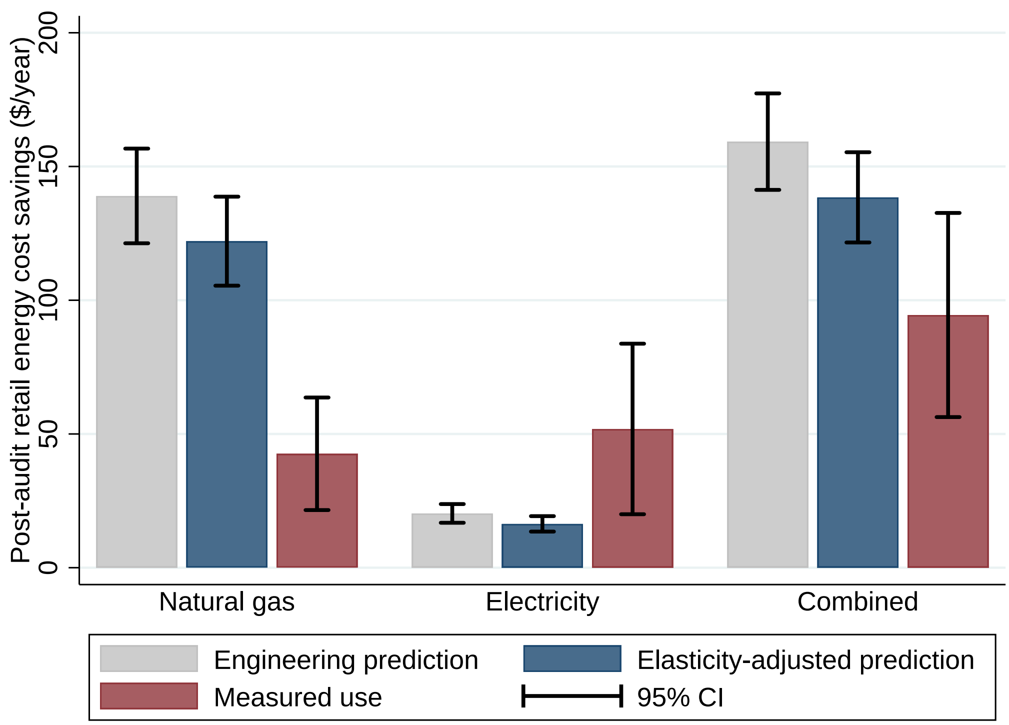
Notes: This figure illustrates the deadweight losses from suboptimal policies on the three margins of consumer choice in our model. The left panel shows the deadweight loss on the energy use margin from having zero energy tax (relative to the first-best optimum p^{FB}). The middle panel shows the deadweight loss on the investment takeup margin from zero investment subsidies or an above-optimal investment subsidy s_j (relative to the second-best optimum s_{ij}^{SB}). The right panel shows the deadweight loss on the audit takup margin from audit subsidy s^A (relative to the second-best optimum of zero audit subsidy).

Figure 3: **Energy Use in Event Time**



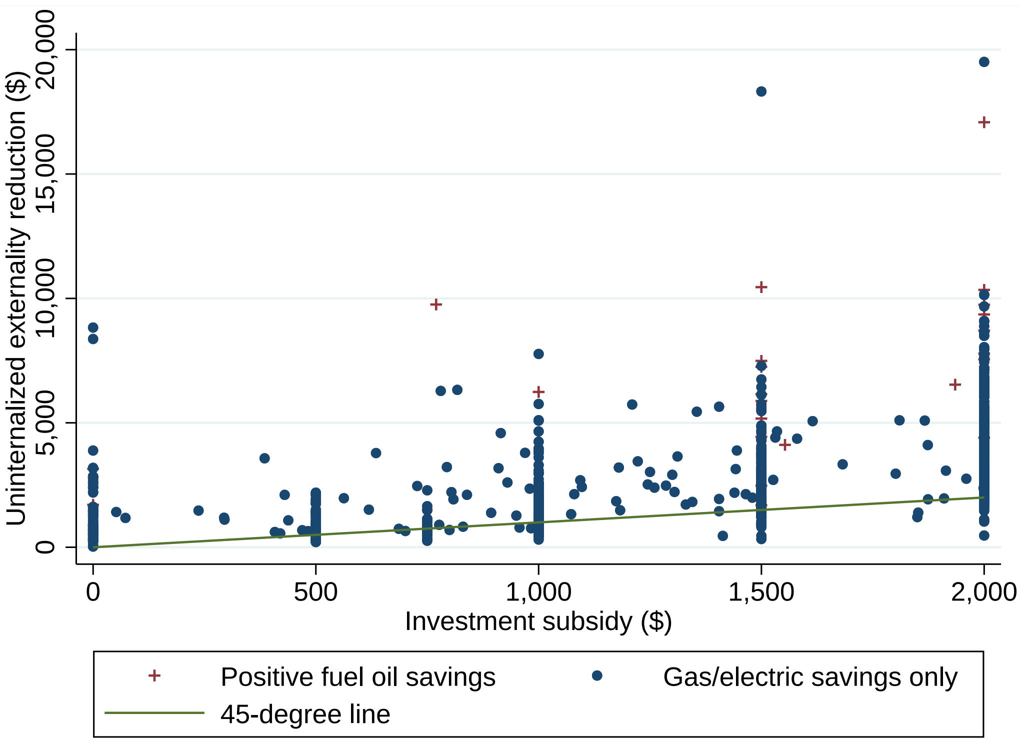
Notes: This figure presents energy use in event time relative to the household's audit. We construct the figure by estimating equation (8) separately for natural gas and electricity, replacing $\boldsymbol{P}_{it}$ with indicators for each two-month period within an event window extending 18 months before and after the audit. The excluded category is the month of the audit and the month before. We then multiply each coefficient by (365/12) $\times$ retail price to transform units to monthly retail cost and add the natural gas and electricity coefficients for each two-month period, computing standard errors using the delta method. Dashed lines are 90 percent confidence intervals. Average pre-audit retail energy costs are $1,780 per year.

Figure 4: **Engineering Predictions versus Empirical Estimates of Post-Audit Energy Savings**



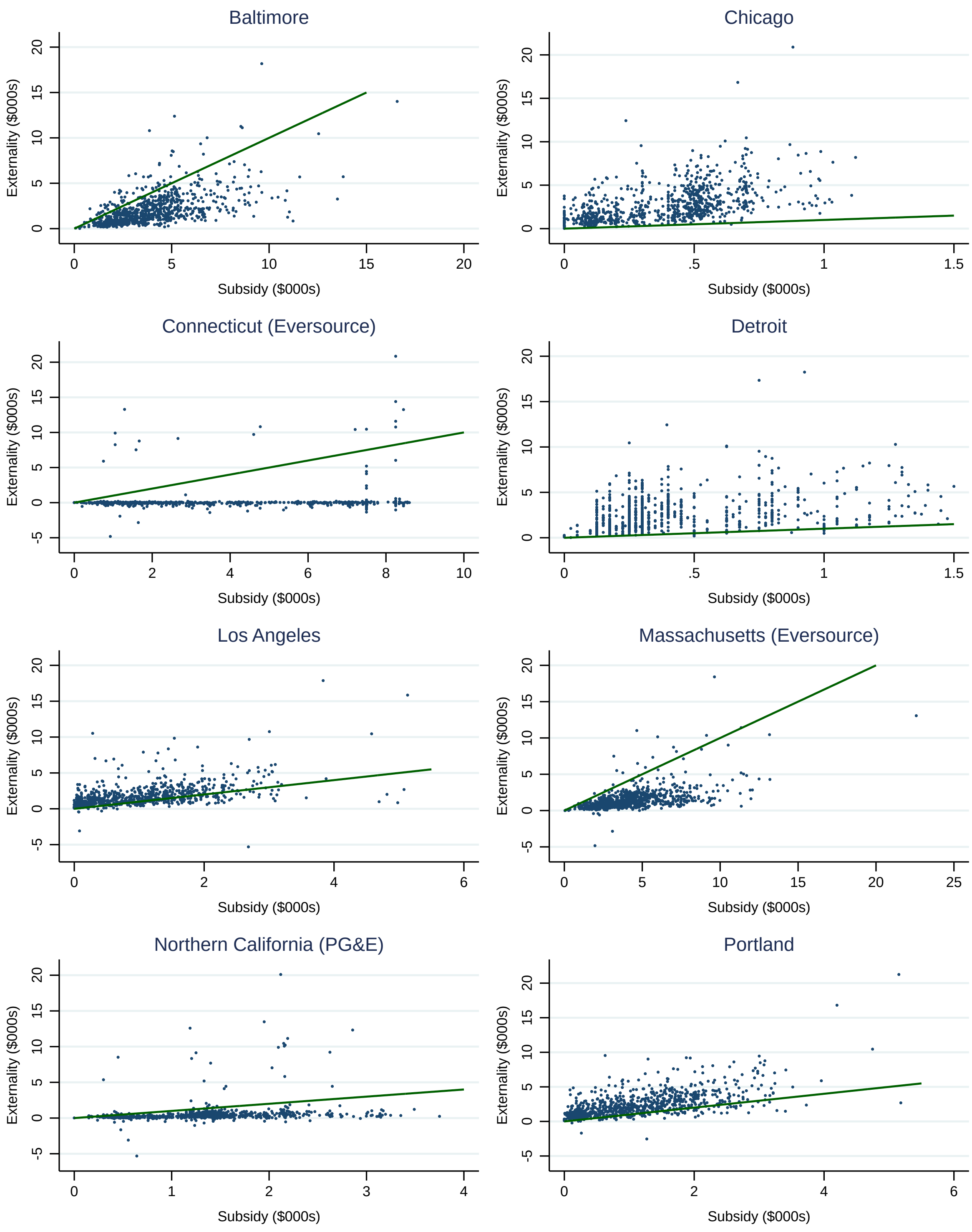
Notes: This figure presents simulated and actual post-audit energy cost savings. We construct the first two groups of bars by estimating equation (8) separately for natural gas and electricity and multiplying the ≥ 6 months post-audit coefficient by 365 $\times$ retail price to transform units to annualized retail cost. We construct the combined bars by adding the natural gas and electricity estimates, computing standard errors using the delta method. Average pre-audit retail energy costs are $1,780 per year.

Figure 5: **Subsidy Received versus Uninternalized Externality Reduction by Household**



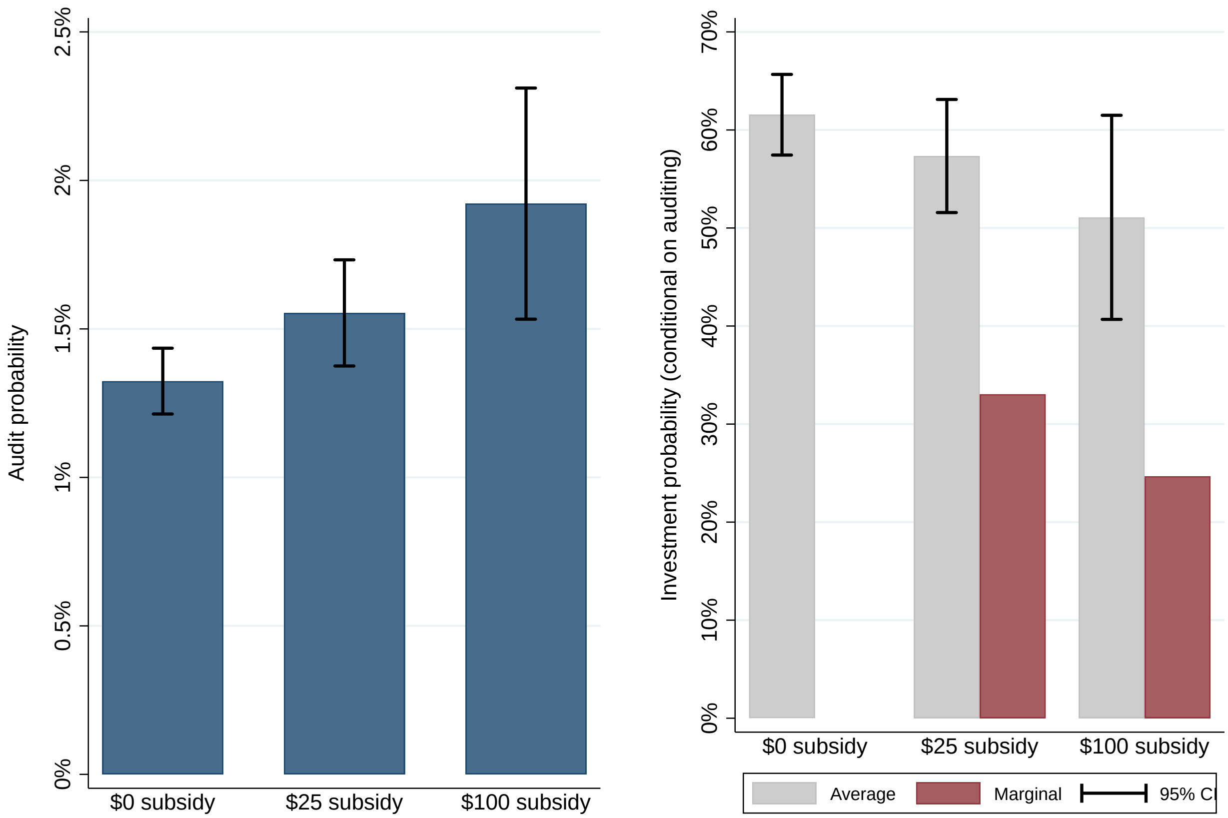
Notes: This figure presents the investment subsidy received versus the uninternalized externality reduction from all investments made, for each household that had an audit. Uninternalized externalities value energy savings at the difference between social cost and retail price, from row 5 of Table 2; this reflects both environmental externalities and retail markups.

Figure 6: **Subsidy versus Uninternalized Externality Reduction for Top 10 Programs**



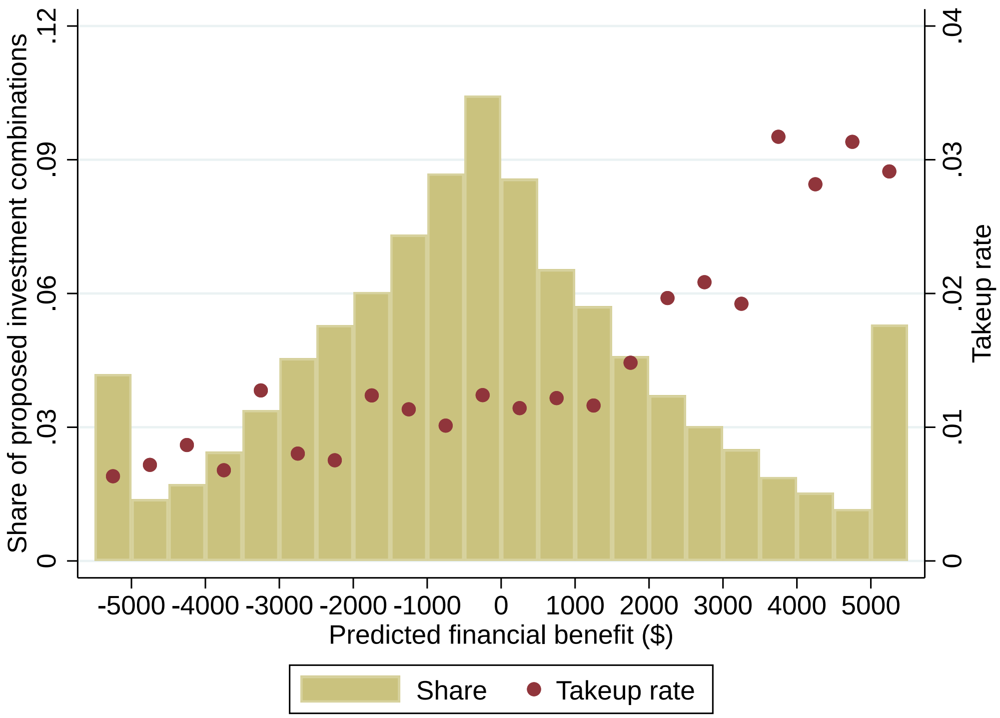
Notes: For each household that had an audit in the Wisconsin programs, this figure presents the resulting investment subsidy and uninternalized externality reduction in the service area of each of the top 10 energy efficiency programs as rated by the American Council for an Energy-Efficient Economy (2020). Solid (green) lines are 45-degree lines.

Figure 7: **Average and 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. The figure excludes households that were in the no-letter control group or the $25 gift card group.

Figure 8: **Predicted Financial Benefit and Investment Takeup**



Notes: The bars are a histogram of the predicted financial benefits $\Delta \boldsymbol{e}_{ij}\boldsymbol{p} - p_{ij}$ for all investment combinations in the data. The dots are the takeup rates within each bin. We winsorize at ±$5000 for readability.

Online Appendix

Measuring the Welfare Effects of Residential Energy Efficiency Programs

Hunt Allcott and Michael Greenstone

Table of Contents

A Example Audit Report

Me2
Milwaukee Energy Efficiency
Smart energy pays.

Email:
Me2@milwaukee.gov

Phone:
(877) 399-1203

May 21, 2013

Dear Aaron;

You're on your way to saving energy and money! Your home energy assessment results are in and Milwaukee Energy Efficiency (Me^2) is here to help.

The enclosed Home Performance with ENERGY STAR® report is your ticket to savings—not to mention a better, more comfortable home. The report tells you how your home performed**,** including**:**

- Facts about your home's insulation levels, ventilation, and air leakage.
- What's really going on—the source of your home's problems.
- Personalized and proven solutions to improve your home's performance.

Me^2 provides City of Milwaukee residents with access to more financial offers than ever before. Use available incentives and watch your savings add up:

- **Me^2 incentive**—Me^2 is giving you $750 to $2,000 when you complete all of the recommended measures.
- **Energy assessment reimbursement**—Complete recommended air sealing work or $1,000 in other energy efficiency improvements with a Participating Contractor and you'll get your $100 energy assessment fee back.
- **Health and Safety Grant**—You may qualify for a grant up to $1,000 to fix eligible health and safety issues, including electrical upgrades of knob and tube wiring and removal of asbestos, vermiculite, or an oil tank. To receive this grant, you must also complete the recommended energy-efficiency improvements.
- **Special financing**—Take advantage of optional energy-efficiency financing through Summit Credit Union. Flexible terms and fixed interest rates lower than average personal loan rates. Visit SummitCreditUnion.com/SummitMe2 or call 800.236.5560 to learn more or to get pre-approved.

Plus, you'll save on energy bills if you complete the improvements recommended in your report.

Next steps
You're so close to having an energy-efficient home. **KEEP GOING**. Decide which recommended projects you'd like to pursue. Pull out the Participating Contractor List. **Ask for estimates, schedule the work, and reap the benefits.**

Questions?
Your Me^2 team is here to help. Don't hesitate to contact me or your energy consultant. We can help answer questions, prioritize projects, and keep things moving forward.

Sincerely,

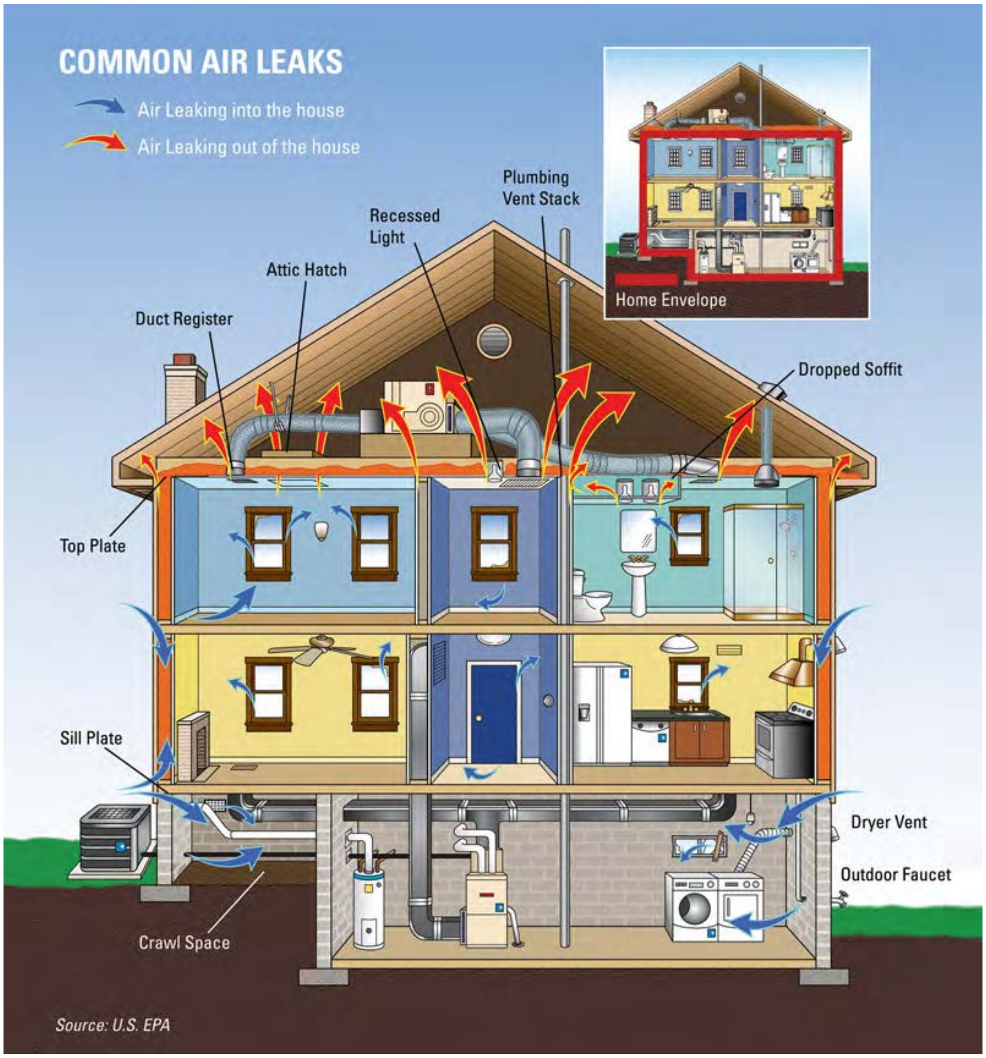
Margee Foulke-Evans
Me2 Energy Advocate
414-333-6245
mfoulke@weccusa.org

P.S. Enjoy the perks! Having an energy-efficient home is smart. You'll save energy and money.

Think Smart. Save Smart. Become an Energy Einstein! *www.smartenergypays.com*

Reference Home

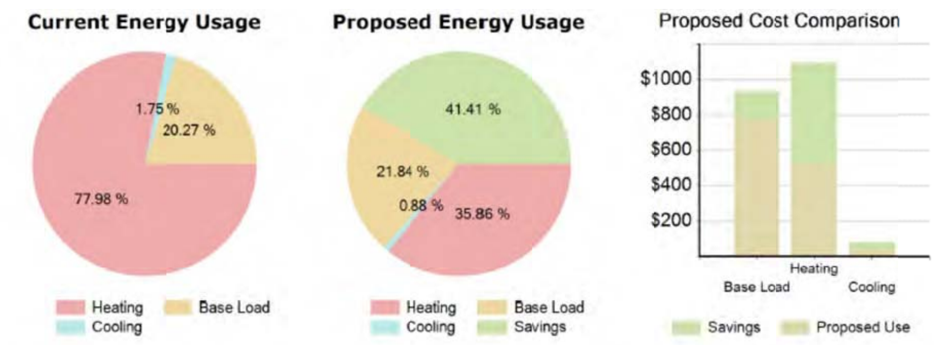
The below diagram is provided for reference as you read your personalized report. It illustrates common sources of home energy-efficiency issues and associated terminology.



Page **3** of **4**

Home Energy Snapshot - (Achieve 35+% Energy Savings)

How your home uses energy:



Heating usage includes all energy used to heat your home, similarly for cooling. They are both weather dependent. Base load is the energy use that is independent of the weather. This includes uses like appliances and lighting as well as hot water. This chart shows how your home is currently using energy among these different end uses.

Each improvement affects the energy profile of your house in different ways. Insulation will improve both heating and cooling while replacing a refrigerator will only improve the base-load. This chart indicates the proposed energy usage of your house with all the recommended improvements installed to indicate from where your savings will come.

Required energy-efficiency projects to achieve 35+% energy savings:

PROPOSED Improvement	Customer Cost	Annual Savings	Payback (years)	SIR	Savings Percent	Lifetime Savings
Attic Knee Wall Insulation	$225.00	$63.11	3.6	4.2	4.32%	$1,262.21
Vaulted Ceiling Insulation	$100.00	$12.23	8.2	1.8	0.79%	$244.53
Above Grade Wall Insulation	$3,400.00	$298.76	11.4	1.3	21.46%	$5,975.21
Air Sealing	$800.00	$70.05	11.4	1.3	5.21%	$1,400.91
Water Heater Improvement	$1,750.00	$160.95	10.9	1.0	-	$2,253.28
Rim Joist Insulation	$625.00	$31.18	20.0	0.8	2.25%	$623.55
Furnace Replacement*	$3,250.00	$68.05	47.8	0.3	5.02%	$1,361.00
Attic Insulation	$1,250.00	$21.74	57.5	0.3	1.43%	$434.83
Window Improvement	$11,500.00	$79.08	145.4	0.1	4.87%	$1,581.60

***Please note the software is only able to estimate gas savings for furnace replacements.**

Note: Costs and savings are estimates only. Payback and SIR do not factor in cash-back rewards. Work must be completed by Me2-participating contractors to count towards potential bonuses.

The savings percentage of the individual items will not necessarily add up to the overall proposed savings.

HOME PERFORMANCE WITH ENERGY STAR

Me2 Milwaukee Energy Efficiency *Smart energy pays.*

Energy project costs and financial incentives:

Depending on which improvements you choose to complete, you may be eligible for the following financial incentives:

Project Incentives	
Targeted Energy Savings	**Me² Incentives for Energy Savings**
35+% Energy Savings	$2,000*
25-34% Energy Savings	$1,500*
15-24% Energy Savings	$1,000*
10-14% Energy Savings	$750*
Additional Me² Incentives	
Complete recommended air sealing work or $1,000 in other energy-efficiency improvements with a Participating Contractor.	$100
Health & Safety Grant (Up to $1,000. Covers necessary electrical upgrades, removal of asbestos/vermiculite, or removal an oil tank. To receive this grant you must also complete the recommended energy-efficiency improvements.)	$1,000

***Incentive amount cannot exceed the energy efficiency improvement costs incurred to the customer. Total rewards from Me2 and Focus on Energy cannot exceed cost of installation.**

Plus, you may qualify for special energy-efficiency financing through Summit Credit Union. Energy-efficiency financing between $1,000 and $15,000 is available to Me² clients, and you can finance up to 100 percent of the costs of recommended improvements.

Visit SummitCreditUnion.com/SummitMe2 or call 800.236.5560 to explore your options or to get pre-approved.

Focus on Energy cash-back rewards may also be available. Visit www.FocusonEnergy.com or call 800-767-7077 to determine your eligibility for these rewards. The Me² program does not guarantee Focus on Energy incentives for any project. Homeowners are responsible for working with their contractor(s) to see if they qualify for any other incentive programs.

HOME PERFORMANCE WITH ENERGY STAR

Me²
Milwaukee Energy Efficiency
Smart energy pays.

B Proof of Lemmas 1 and 2

For this appendix, we model a continuum of consumers with types indexed by i, drawn from multidimensional distribution with density $H(i)$. This allows us to derive results by taking derivatives. We assume $\lambda = 1$. Thus, welfare can be written as

$$W = \int_i \left\{ \begin{array}{c} \left\{(1-A_i)\, v_i\left(\boldsymbol{F}_{i0}\cdot\boldsymbol{p}\right) + A_i \cdot \left[\sum_{j\in\mathcal{J}_i} I_{ij}\cdot\left(v_i\left(\boldsymbol{F}_{ij}\cdot\boldsymbol{p}\right) - p_{ij} + \xi_{ij} + \gamma_{ij}\right) - p^A + \xi_i^A\right]\right\} \\ -A_i \cdot \left[s^A + \sum_{j\in\mathcal{J}_i} I_{ij} s_{ij}\right] \end{array} \right\} dH(i) \tag{29}$$

$$+\boldsymbol{e}\cdot\left(\boldsymbol{\pi} - \boldsymbol{\phi} + \boldsymbol{\tau}\right). \tag{30}$$

The full first-order conditions are algebraically intensive because of the three-step consumer problem, but we can prove Lemma 1 by showing that the conjectured optimal policies jointly satisfy the first-order conditions. For the $\boldsymbol{\tau}$ FOC, we substitute s_{ij}^{FB} and $s^{A,FB}$ into equation (29) and take the derivative (using the Envelope Theorem), giving

$$\frac{dW}{d\boldsymbol{\tau}} = -\boldsymbol{e} + \boldsymbol{e} + \frac{d\boldsymbol{e}}{d\boldsymbol{\tau}}(\boldsymbol{\pi} - \boldsymbol{\phi} + \boldsymbol{\tau}) = \boldsymbol{0} \tag{31}$$

$$\boldsymbol{\tau}^{FB} = \boldsymbol{\pi} - \boldsymbol{\phi}. \tag{32}$$

For the s^A FOC, we analogously substitute s_{ij}^{FB} and $\boldsymbol{\tau}^{FB}$ into equation (29) and take the derivative, giving

$$\frac{dW}{ds^A} = \int_i A_i - A_i + \frac{d\Pr A_i}{dp^A} s^A \; dH(i) = 0 \tag{33}$$

$$s^{A,FB} = 0. \tag{34}$$

For the s_{ij} FOC, we substitute $s^{A,FB}$ and $\boldsymbol{\tau}^{FB}$ into equation (29) and take the derivative, giving

$$\frac{dW}{ds_{ij}} = \int_i A_i \sum_{j\in\mathcal{J}_i}\left(I_{ij} - I_{ij}\right) + A_i \sum_{k\in\mathcal{J}_i} \frac{d\Pr I_{ik}}{dp_{ij}}\left(\gamma_{ik} - s_{ik}\right) + \frac{d\Pr A_i}{dp_{ij}} \sum_{k\in\mathcal{J}_i} I_{ik}\left(\gamma_{ik} - s_{ik}\right) \; dH(i) = 0$$

$$s_{ij}^{FB} = \gamma_{ij}, \; \forall ij$$

Setting $\gamma_{ij} = 0$ gives Lemma 1 as stated in the text.

To prove Lemma 2, we can similarly showing that the conjectured optimal policies jointly satisfy the FOCs. For the s^A FOC, substitute $\boldsymbol{\tau} = \boldsymbol{0}$ and s_{ij}^{SB} into equation (29) and take the derivative, giving

$$\frac{dW}{ds^A} = \int_i A_i - A_i + \frac{d\Pr A_i}{dp^A} \cdot \left(-s^A + \sum_{j \in \mathcal{J}_i} I_{ij} \cdot (\gamma_{ij} - \Delta \boldsymbol{e}_{ij} \cdot (\boldsymbol{\phi} - \boldsymbol{\pi}) - \gamma_{ij} - \Delta \boldsymbol{e}_{ij} \cdot (\boldsymbol{\phi} - \boldsymbol{\pi})) \right) \, dH(i) = 0 \tag{35}$$

$$s^{A,SB} = 0. \tag{36}$$

For the s_{ij} FOC, we analogously substitute $\boldsymbol{\tau} = \mathbf{0}$ and $s^{A,SB} = 0$ into equation (29) and take the derivative, giving

$$\frac{dW}{ds_{ij}} = \int_i A_i \sum_{j \in \mathcal{J}_i} (I_{ij} - I_{ij}) + A_i \sum_{k \in \mathcal{J}_i} \frac{d\Pr I_{ik}}{dp_{ij}} (\gamma_{ik} - s_{ik} + \Delta e_{ik} \cdot (\boldsymbol{\pi} - \boldsymbol{\phi})) \tag{37}$$

$$+ \frac{d\Pr A_i}{dp_{ij}} \sum_{k \in \mathcal{J}_i} I_{ik} (\gamma_{ik} - s_{ik} + \Delta e_{ik} \cdot (\boldsymbol{\pi} - \boldsymbol{\phi})) \, dH(i) = 0 \tag{38}$$

$$s_{ij}^{SB} = \Delta e_{ij} \cdot (\boldsymbol{\pi} - \boldsymbol{\phi}) + \gamma_{ij} \tag{39}$$

Again setting $\gamma_{ij} = 0$ gives Lemma 2 as stated in the text.

C Experimental Design Appendix

The recipient households were randomly assigned to be mailed letters in one of eight initial waves in summer and fall 2012, on June 11, June 25, July 9, July 23, September 10, September 24, October 8, and November 22. Households were randomly assigned to be mailed a second letter in one of four final waves, mailed on November 12, 2012, and January 7, January 28, and February 4, 2013. The 1,051 households that scheduled an audit after May 2012 but before their initial or final wave's mail date were not sent additional letters. These households are still included in the analysis.

Appendix Figures A1 and A2 present example letters from the experiment.

We used a re-randomization algorithm to ensure balance. Specifically, we wrote an algorithm that carried out 500 randomizations for each city (Madison and Milwaukee) using 500 different seeds for the random number generator. For each seed, the algorithm regressed house age, property value, and building area on the set of all treatment group indicators and found the number of p-values less than 0.1 and the minimum p-value of each t-test. For each city, the algorithm then chose the randomization with the largest minimum p-value from the subset of randomizations with the fewest p-values less than 0.1.

Appendix Table A1 presents tests of balance on observables. Given the re-randomization approach, it is unsurprising that F-tests fail to reject balance, with high p-values. The census tract hybrid vehicle share, which was not part of the re-randomization algorithm, is also highly balanced across treatment groups; that F-test has a p-value of 0.993.

Figure A1: **Wisconsin Experiment Sample Letter 1**

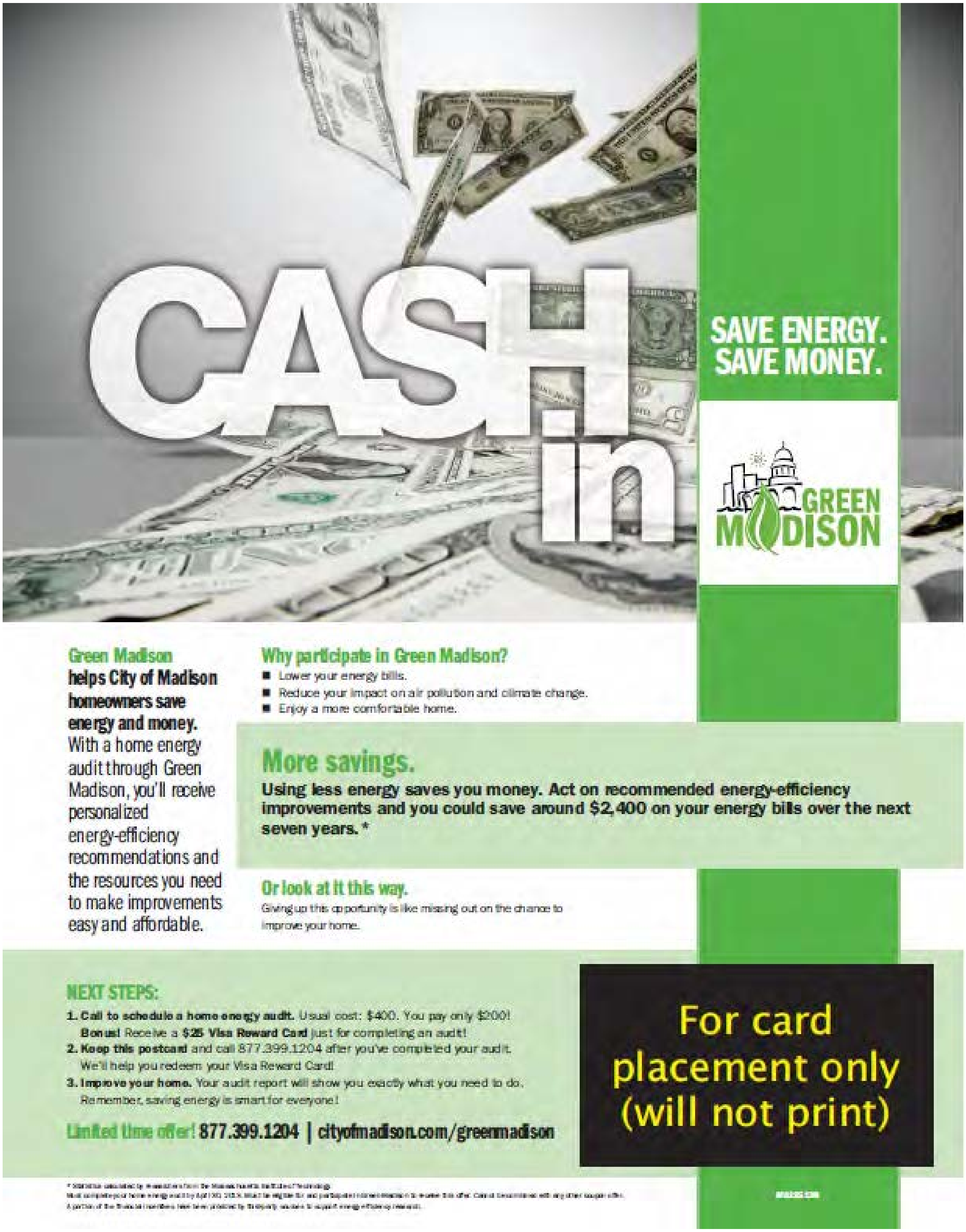


Figure A2: **Wisconsin Experiment Sample Letter 2**

Table A1: **Test of Balance on Observables in Wisconsin Experiment**

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	House age	Property value	Building area	Madison	Hybrid share
Sent letter	-0.238	-0.000	-0.001	0.002	-0.016
	(0.349)	(0.001)	(0.007)	(0.007)	(0.019)
Info: Financial	0.119	0.001	0.004	-0.002	0.003
	(0.296)	(0.001)	(0.006)	(0.006)	(0.016)
Info: Comfort	0.160	-0.000	0.001	0.001	0.005
	(0.295)	(0.001)	(0.006)	(0.006)	(0.016)
Info: Climate	-0.094	0.000	-0.006	0.001	0.018
	(0.293)	(0.001)	(0.006)	(0.006)	(0.016)
Info: Environment	0.399	-0.000	-0.001	0.001	0.013
	(0.296)	(0.001)	(0.006)	(0.006)	(0.016)
Info: Combined	0.299	-0.001	-0.008	-0.000	0.011
	(0.294)	(0.001)	(0.006)	(0.006)	(0.016)
Comparison: Door open	-0.032	-0.001	-0.000	-0.003	0.001
	(0.230)	(0.001)	(0.004)	(0.005)	(0.013)
Comparison: Lights on	-0.158	0.001	0.004	-0.002	-0.003
	(0.231)	(0.001)	(0.004)	(0.005)	(0.013)
Comparison: Car idling	-0.258	-0.000	-0.001	0.000	-0.000
	(0.228)	(0.001)	(0.004)	(0.005)	(0.013)
Financing: Credit	0.107	0.001	0.006	0.001	0.012
	(0.199)	(0.001)	(0.004)	(0.004)	(0.011)
Financing: Incentives	0.142	-0.000	-0.002	0.000	-0.001
	(0.199)	(0.001)	(0.004)	(0.004)	(0.011)
Prime: Financial	0.185	-0.001	-0.002	0.001	-0.014
	(0.230)	(0.001)	(0.004)	(0.005)	(0.013)
Prime: Climate	0.101	-0.000	0.004	0.002	-0.012
	(0.230)	(0.001)	(0.004)	(0.005)	(0.013)
Prime: Environment	0.187	-0.000	-0.001	-0.001	-0.002
	(0.230)	(0.001)	(0.004)	(0.005)	(0.013)
Time: Seven year	-0.066	0.001	0.003	-0.002	0.006
	(0.178)	(0.001)	(0.003)	(0.004)	(0.010)
Audit cue: Assessment	-0.065	0.000	0.002	-0.001	0.001
	(0.172)	(0.001)	(0.003)	(0.003)	(0.010)
$100 audit subsidy	0.111	0.001	0.002	0.002	0.005
	(0.355)	(0.001)	(0.007)	(0.007)	(0.019)
$25 audit subsidy	0.051	-0.001	-0.003	-0.001	-0.001
	(0.204)	(0.001)	(0.004)	(0.004)	(0.011)
$25 gift card	-0.035	0.001	-0.004	0.001	0.003
	(0.213)	(0.001)	(0.004)	(0.004)	(0.012)
N	101,881	101,881	101,881	101,881	101,881
F-test p-value	.992	.878	.582	1	.993

Notes: Robust standard errors are in parentheses. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

C.A Non-Price Letter Variations

The non-price letter variations were designed to reduce barriers thought to affect audit takeup. We loosely group these into "informational" and "behavioral" treatments.

C.A.1 Informational Treatments

Appendix Table A2 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.[25] This was motivated by literature suggesting that imperfect information and biased beliefs could affect energy efficiency investment.[26]

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 energy efficiency investment.

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."

C.A.2 "Behavioral" Treatments

Appendix Table A3 details the treatments inspired by results from the behavioral science literature.

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

[25] The numbers in the benefit information treatments in the promotional letters were based on our best *ex ante* estimates of the value of energy that the average participant would save. 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.

[26] See Allcott and Sweeney (2017), Allcott and Taubinsky (2015), Davis and Metcalf (2016), and Newell and Siikamaki (2014) for experimental studies. See Gillingham, Newell, and Palmer (2009), Jaffe and Stavins (1994), Sanstad, Hanemann, and Auffhammer (2006) for overview articles discussing imperfect information.

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.

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.

Table A2: **Details of "Informational" Treatments**

Treatment	Share	Text
Benefit Info		
Financial	1/6	**More savings.** Using less energy saves you money. Act on recommended energy-efficiency improvements and you could save around $[340 / 2,400] on your energy bills over the next [one year / seven years].
Comfort	1/6	**Feel better at home.** Using less energy can lead to greater comfort and a healthier home. When done right, energy-efficiency projects can improve indoor air quality while reducing humidity, drafts, and mold. Plus, you could save around $[340 / 2,400] on your energy bills over the next [one year / seven years].
Climate	1/6	**Reduce your carbon footprint.** Using less energy reduces greenhouse gas emissions that can contribute to climate change. Climate experts calculate that your reduced greenhouse gas emissions over the next [one year / seven years] would benefit people across the world by the equivalent of $[60 / 420]. Plus, you could save around $[340 / 2,400] on your energy bills over that same time.
Environment	1/6	**Help the planet.** Using less energy reduces local air pollution. Reductions in air pollution can lead to longer lives and reduced allergies and asthma, creating a healthier environment for you and your community. Plus, you could save around $[340 / 2,400] on your energy bills over the next seven years.
Combined	1/6	**Savings for you, your community, and our world.** Using less energy saves you money while reducing air pollution and greenhouse gases. Reductions in air pollution can lead to longer lives and reduced allergies and asthma, creating a healthier environment for you and your community. Climate experts calculate that your reduced greenhouse gas emissions over the next seven years would benefit people across the world by the equivalent of $[60 / 420]. Plus, you could save around $[340 / 2,400] on your energy bills over that same time.
Control	1/6	**Reduce energy use.** Reduce your energy use at home and enjoy the many benefits of energy efficiency. You'll soon see that conserving energy is great for you, your family, and the greater community.
Financing		
Credit	1/3	Low-interest, flexible term financing is available. Energy savings can help pay off your loan!
Incentives	1/3	Collect cash incentives!
Control	1/3	Remember, saving energy is smart for everyone!
Comparison		Giving up this opportunity . . .
Door	1/6	wastes as much energy as leaving your front door wide open for three hours each day in the winter.
Light	1/6	wastes more energy than leaving all the lights in your house on all day, every day.
Car	1/6	wastes about as much energy as leaving your car idling outside for an hour every day.
Control	1/2	is like missing out on the chance to improve your home.

Table A3: **Details of "Behavioral" Treatments**

Treatment	Share	Text
Graphical Prime		
Financial	1/4	**make BANK.** Shrink your energy bills. Get paid.
		CASH in. Save energy. Save money.
Environmental	1/4	**earth LOVER.** Save energy at home. Save the planet.
		breathe EASY. Energy-efficient home. Cleaner air.
Climate	1/4	**take CONTROL.** Save energy. Reduce your carbon footprint.
		PEACE of MIND. Energy-efficient home. Brighter future
Comfort	1/4	**COMFY.** Save energy. Live comfortably.
		COZY. Feel better in your home.
Time Frame		
Seven years	1/2	Example: "you could save around $2,400 on your energy bills over the next seven years."
One year	1/2	Example: "you could save around $340 on your energy bills over the next year."
Audit Cue		
Assessment	2/3	"home energy assessment"
Audit	1/3	"home energy audit"

C.A.3 Effects of Non-Price Letter Variations

Appendix Table A4 presents estimates of equation (10) including the full set of indicators for all "informational" and "behavioral" treatment variations. The only treatments that affect takeup are the subsidies. Furthermore, Wald tests in Appendix Table A5 show that none of the six groups of informational or behavioral treatments jointly affected audit or investment takeup.

Table A4: **Effects of All Treatment Variations on Audit and Investment Takeup**

Dependent variable:	(1) Audited	(2) Invested
Info: Financial	0.177	0.163
	(0.155)	(0.119)
Info: Comfort	0.088	0.108
	(0.151)	(0.114)
Info: Climate	-0.086	-0.075
	(0.148)	(0.111)
Info: Environment	0.047	0.165
	(0.152)	(0.118)
Info: Combined	-0.070	-0.033
	(0.149)	(0.112)
Comparison: Door open	-0.033	0.069
	(0.119)	(0.093)
Comparison: Lights on	-0.184	-0.076
	(0.114)	(0.088)
Comparison: Car idling	-0.023	-0.012
	(0.119)	(0.090)
Financing: Credit	0.013	0.048
	(0.102)	(0.078)
Financing: Incentives	0.056	0.074
	(0.102)	(0.078)
Prime: Financial	-0.119	-0.101
	(0.118)	(0.090)
Prime: Climate	0.013	0.061
	(0.120)	(0.094)
Prime: Environment	-0.095	-0.098
	(0.118)	(0.090)
Time: Seven year	-0.003	-0.009
	(0.092)	(0.071)
Audit cue: Assessment	-0.113	-0.016
	(0.090)	(0.068)
$100 audit subsidy	0.591	0.153
	(0.206)***	(0.149)
$25 audit subsidy	0.233	0.067
	(0.107)**	(0.082)
$25 gift card	0.017	-0.118
	(0.107)	(0.080)
N	101,881	101,881

Notes: This table presents estimates of equation (10), a linear probability model of audit or energy efficiency investment takeup on treatment group indicators and household characteristics. All regressions include controls for house age, property value, building footprint, city, and hybrid share. Robust standard errors are in parentheses. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

Table A5: **Tests of Whether Groups of Treatments Jointly Affect Takeup**

	(1)	(2)
Dependent variable:	Audited	Invested
Benefit info treatments	0.44	0.63
Comparison treatments	0.46	0.11
Graphical prime treatments	0.59	0.21
All "informational" treatments	0.60	0.25
All "behavioral" treatments	0.64	0.47
All non-subsidy treatments	0.65	0.28
Subsidy treatments	0.01	0.16

Notes: This table presents p-values of Wald tests of whether groups of coefficients in Appendix Table A4 jointly differ from zero.

D Data Appendix

D.A Wisconsin Experiment Data Preparation

We have separate files that include the characteristics of all proposed investments (from the audit reports) and adopted investments by household. We match the two by household ID and investment type. Characteristics of adopted investments can differ from the proposed investments listed on the audit report as contractors refine estimates, although on average they are very similar and in many cases identical. If the proposed investment differs from the adopted investment, we use the adopted investment characteristics.

Appendix Table A6 presents the sample sizes of proposed investments across all households and separately for the letter treatment and letter control groups. The "full sample" of proposed investments includes the initial list of proposed investments plus any investments that were adopted but did not appear on the audit report. "Energy efficiency investments" exclude health and safety projects (exhaust fans and chimney liners) and solar panels. "Choice set for model" additionally excludes zero-cost investments installed during the audit and other investments with data quality issues. Specifically, we exclude observations with zero or negative projected dollar savings (these appear to reflect model input errors or cases where one investment was grouped with others), appliances (takeup is imperfectly observed), and new hot water heaters (the program treated water heaters inconsistently across households).

Table A6: **Number of Proposed Investments in Initial and Final Samples**

	(1) All households	(2) Households in letter treatment group	(3) Households in letter control group
Full sample	9,436	7,650	1,786
Energy efficiency investments (used in Section VI)	9,068	7,343	1,725
Choice set for model (used in Section III)	6,100	4,939	1,161

D.B Better Buildings Neighborhood Program Data Preparation

The Better Buildings Neighborhood Program (BBNP) website provides household-level microdata for 75,110 retrofits of single-family homes at 37 different sites.[27] For each home, the data include total unsubsidized cost, engineering model predictions of total annual energy savings in dollars (top-coded at $2,500) and in physical quantities of natural gas, electricity, heating oil, propane, kerosene, and wood, and counts of investments made by category. We combine the physical quantities with state-specific average energy prices for 2011–2014 (described below) to construct annual cost savings, then construct present discounted values using five percent discount rates and investment life assumptions from Heaney and Polly (2015).

Column 1 of Appendix Table A7 presents counts of adopted investments. The distribution of investment categories in Wisconsin is similar to in the nationwide program: primarily insulation, air sealing, and new heating and cooling systems.

Because the data were reported by 37 different agencies, there are some inconsistencies and implausible observations. We drop observations with:

- missing or negative costs,
- missing or negative constructed cost savings,
- reported energy cost savings that are not top-coded and differ from our constructed cost savings by more than a factor of two, if either the reported or constructed annual energy cost savings exceed $1000,
- payback periods faster than one year and costs greater than $2500 (these are primarily air sealing, insulation, and windows, not CFL replacement, so a one-year payback strongly suggests misreported data), or
- constructed cost savings less than the 1st percentile or larger than the 99th percentile, similar to the Heaney and Polly (2015) approach.

This leaves 58,418 valid retrofits in our sample.

Appendix Table A7 summarizes costs and benefits by investment category for both the Wisconsin and national programs.

[27] The data are available from http://energy.gov/eere/better-buildings-neighborhood-program/downloads/better-buildings-neighborhood-program-data-1, with documentation at http://energy.gov/eere/better-buildings-neighborhood-program/downloads/better-buildings-neighborhood-program-data.

Table A7: **Summary of Adopted Investments in Wisconsin and National Programs**

	(1) National programs	(2)	(3)	(4) Wisconsin programs	(5)	(6)
Category	Number of adoptions	Number of audit reports	Number of adoptions	Mean cost ($)	Mean wholesale energy savings ($/year)	Mean wholesale energy savings ($/year)
Insulation	50,622	3,923	2,059	1,233	51	20.0
Air sealing	27,454	1,358	790	951	67	20.0
Exhaust fan	2,523	839	428	697	-	20.0
New heating/cooling system	25,567	707	279	3,584	156	18.5
New water heater	10,025	461	136	1,711	13	14.0
Replace lighting with CFLs	11,634	455	301	9	9	6.0
Aerators/showerheads	9,451	404	186	2	5	20.0
New windows	6,023	110	16	2,867	9	20.0
Programmable thermostat	5,104	49	61	305	21	11.0
Pipe/duct sealing/insulation	11,629	35	6	481	41	18.3
Appliance replacement	1,959	33	0	-	-	-
Chimney liner	1,139	21	0	-	-	-
Unspecified weatherization	11,550	-	-	-	-	-

Notes: Adoption counts for national programs reflect the number of households that made an investment in that category within the 58,418 households that have valid data. Count on audit reports and adoption counts for Wisconsin programs reflect the number of investments within the category from the 1,394 households that had audits, possibly including more than one per household. Cost is unsubsidized upfront cost, and energy savings are based on engineering model predictions and 2011–2014 wholesale energy prices.

D.C Energy Price and Externality Assumptions

All fuel prices are means for 2011–2014. We observe the state for each home retrofitted in the national Better Buildings Neighborhood Program data, so we match in state-specific average fuel prices for 2011–2014.[28] We use natural gas "city gate" prices as wholesale prices.

For electricity and retail natural gas in the Wisconsin programs, we gathered marginal retail prices from staff at the Madison and Milwaukee utilities. Both utilities use time-invariant residential electricity prices, except that Madison charges higher prices during the June–September summer peak. We construct a consumption-weighted average price using 2011–2014 monthly average electricity consumption data for Madison Gas and Electric reported on the EIA Form 826. For natural gas, both utilities have prices that vary by month. We construct consumption-weighted average retail prices using Wisconsin 2011–2014 monthly average retail natural gas consumption from EIA.[29] For city gate natural gas, wholesale electricity, and residential heating oil prices, we use the Wisconsin data from the same EIA sources as for the national program.

Externality damage assumptions build primarily on Holland et al. (2016), who graciously shared their data with us. Their key assumptions are a $6 million value of a statistical life and a fine particulate dose response function from Pope et al. (2002). For electricity, Holland et al. (2016) provide estimates of average marginal damages by pollutant, NERC region, and hour. We weight the hours by residential load shapes from the Integral Analytics DSMore model and sum across pollutants to get average marginal damages by state per kilowatt-hour conserved. Holland et al. (2016) also provided county-by-pollutant marginal damages for non-point sources, which are relevant for natural gas and heating oil combustion from sources such as homes that do not have smokestacks. We inflate these to 2013 dollars, collapse these to the state level, weighting by 2011 population, and multiply the state average marginal damages by pollutant-specific emission factors for natural gas, heating oil, propane, kerosene, and wood from the AP-42 database (EPA 1995).

Drawing on Howarth et al. (2012) and Abrahams et al. (2015), we assume that three percent of natural gas leaks as methane during drilling and transportation before arriving in homes. We translate this to carbon dioxide equivalents using a methane global warming potential of 34 from the Intergovernmental Panel on Climate Change (Myhre et al., 2013). This increases the natural gas climate change externality by 38 percent.[30]

[28] For retail electricity, these are from the EIA "Electricity Data Browser," available from http://www.eia.gov/electricity/data.cfm. Quantity-weighted "all-in" wholesale electricity prices for 2011-2014 in the ERCOT, New York, New England, PJM, Midcontinent, and California power markets are from data reported by market operators to Potomac Economics and presented in Figure 4 of Potomac Economics (2015). These prices include energy prices, ancillary services and uplift charges, and capacity costs where applicable, so they reflect the long-run marginal cost of electricity. We map each US state to one of these markets, and we impute the national average price for states that are not in one of the markets. Citygate natural gas prices are from http://www.eia.gov/dnav/ng/ng_pri_sum_a_epg0_pg1_dmcf_a.htm. Residential retail natural gas prices are from http://www.eia.gov/dnav/ng/ng_pri_sum_a_epg0_prs_dmcf_a.htm. Residential heating oil prices are from http://www.eia.gov/dnav/pet/pet_pri_wfr_a_epd2f_prs_dpgal_w.htm, and residential propane prices are from http://www.eia.gov/dnav/pet/pet_pri_wfr_a_epllpa_prs_dpgal_w.htm.

[29] This is available from http://tonto.eia.gov/dnav/ng/hist/n3010wi2m.htm.

[30] Although this leakage also affects climate damages from electricity consumption, we do not include this because our data from Holland et al. (2016) do not include whether natural gas was on the margin.

For heating oil sulfur content, we assume 2,500 parts per million, except in Connecticut, Delaware, Maine, Massachusetts, New Hampshire, New Jersey, New York, and Pennsylvania, which are phasing in maximum fuel oil sulfur content regulations by 2018. In these states, we construct the present value of average sulfur content over 2013–2032, using a five percent discount rate and maximum allowable fuel sulfur content reported by the New England Fuel Institute (2014). We assume that maximum fuel oil sulfur content restrictions larger than 2,500 ppm are non-binding.

Appendix Table A8 summarizes the energy price and externality assumptions. Climate externality refers to carbon dioxide emissions, plus methane leakage for residential natural gas. Column 1 presents averages for the Wisconsin experiment population. Retail gas and electricity prices differ only slightly between Madison and Milwaukee, and the table presents the average weighted by the share of the audited population in each city. Column 2 presents averages across all households in the national sample.

Table A8: **Energy Price and Externality Assumptions**

	(1) Wisconsin programs	(2) National programs
Natural gas ($/therm)		
Retail price	0.82	1.10
City gate price	0.54	0.53
Climate externality	1.53	1.53
SO2/NOx/PM externality	0.10	0.09
Retail price-social cost	−1.35	−1.04
Retail price-social cost ($/mmBtu)	−13.53	−10.43
Electricity ($/kWh)		
Retail price	0.14	0.14
Wholesale price	0.03	0.05
Climate externality	0.11	0.10
SO2/NOx/PM externality	0.07	0.05
Retail price – social cost	−0.07	−0.06
Retail price – social cost ($/mmBtu)	−21.41	−16.83
Heating oil ($/gallon)		
Price	3.50	3.82
Climate externality	2.11	2.11
SO2/NOx/PM externality	1.19	1.13
Retail price – social cost	−3.30	−3.24
Retail price – social cost ($/mmBtu)	−23.75	−23.34

Notes: This table presents household-weighted averages of prices and externality damages per unit of energy. Energy prices are averages over 2011–2014, and externality savings are based on a $172 per ton social cost of carbon (in 2013 dollars). The bottom row of each panel uses the fact that there are 293 kWh, 10 therms, or 7.19 gallons of electricity, natural gas, or heating oil per million British thermal units (mmBtu), respectively.

E Appendix to Estimates of Effects on Energy Use

E.A Additional Event Study Figures

Figure 3 combines event time estimates for natural gas and electricity. In this appendix, we present separate estimates for each fuel and also consider using the first investment date as the event, instead of the audit. We define "first investment date" as the audit date for households that installed investments during the audit, and the final investment completion date for all other households. In some figures, the ATTs become less negative over time. This is due to seasonality: many audits happened in the summer and winter, when energy consumption is high. Both the weather-adjusted predictions and the actual energy savings may decrease in the spring and fall when the weather is more mild.

Appendix Figures A3 and A4 present the natural gas and electricity estimates underlying Figure 3. In each figure, Panel (a) presents the estimates relative to the audit date, while Panel (b) presents the estimates relative to the first investment date. There are no significant pre-audit or pre-investment trends in either gas or electricity use. Immediately after the audit or investment, energy use decreases. By the end of the first year post-audit, natural gas use averages 0.09 therms/day lower, and electricity use averages about 1.6 kWh/day lower. Relative to pre-audit averages of 2.4 therms/day and 21.4 kWh/day, these represent approximately four and seven percent reductions.

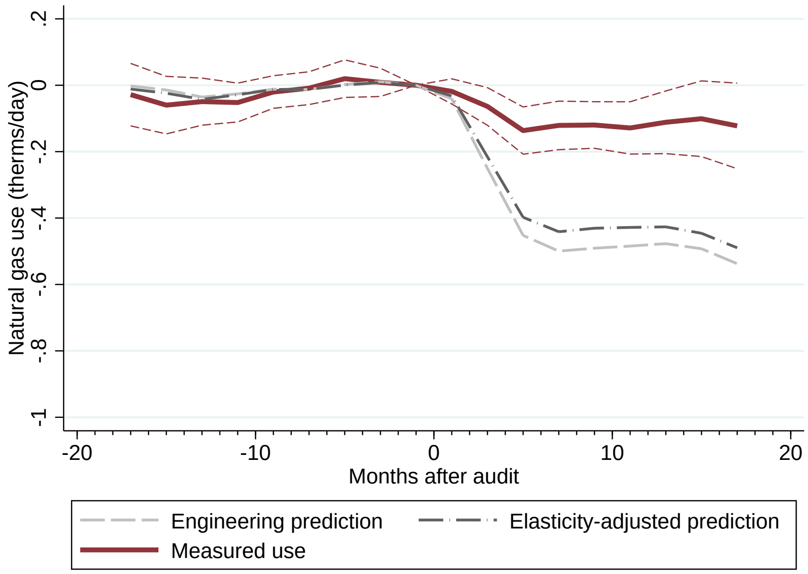
Relative to the empirical estimates, the engineering models predict much larger natural gas savings, both post-audit and post-investment. By contrast, the empirical estimates for post-audit electricity savings are much larger than predicted, and the post-investment estimates are noisy and indistinguishable from the prediction. Note that although $\Delta e_{it}^{pred} = 0$ for all t before the audit, the coefficient estimates differ slightly from zero before the audit and investment due to the inclusion of month-of-sample effects $\mu_{m(t)}$ in the two-way fixed effects model.

Appendix Figures A5 and A6 present analogues to Appendix Figures A3 and A4, except limiting the sample to a "balanced" set of households whose energy bills are observed over the entire event window of 18 months before to 18 months after. (For the investment figures, households that made no investments are also included as controls.) This eliminates the possibility of compositional effects, i.e. that systematically different sets of households would identify treatment effects for different months. The "balanced" group of households turns out to have made larger investments: on average, they were projected to save 17 percent more gas and 37 percent more electricity than the non-balanced group by the end of the first year post-audit. Thus, the energy savings in Figures A5 and A6 are somewhat larger than for the full sample. Otherwise, the figures look very similar.

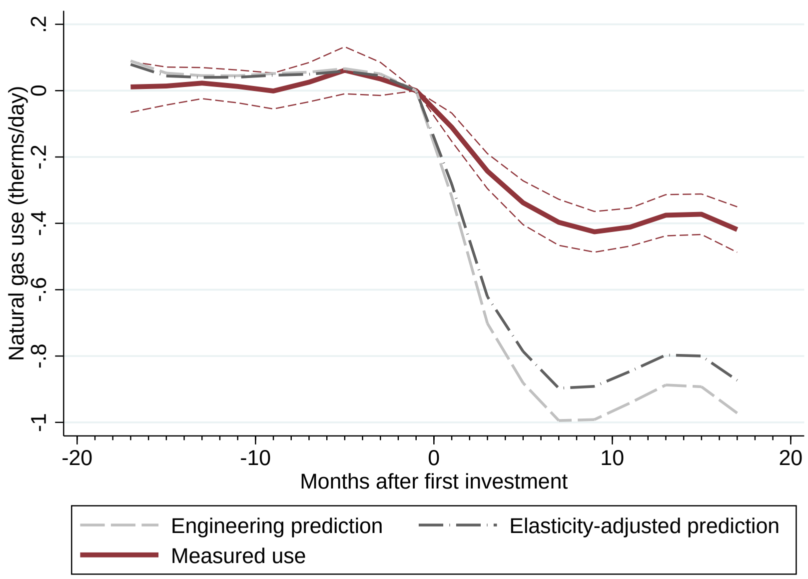
Appendix Figures A7 and A8 present analogues to Figures A5 and A6, except over a shorter time window. Because these figures limit to the "balanced" set of households observed over the entire window in order to eliminate compositional effects, limiting to a smaller window allows us to include additional households that are observed only over a shorter period. This and other time windows all give essentially the same pictures as Figures A5 and A6.

Appendix Figure A9 presents the audit event studies for households with no recorded investments. Again, there are no discernible pre-audit trends. The point estimates suggest a slight but imprecisely estimated post-audit increase in natural gas use.

Figure A3: **Natural Gas Use in Event Time**



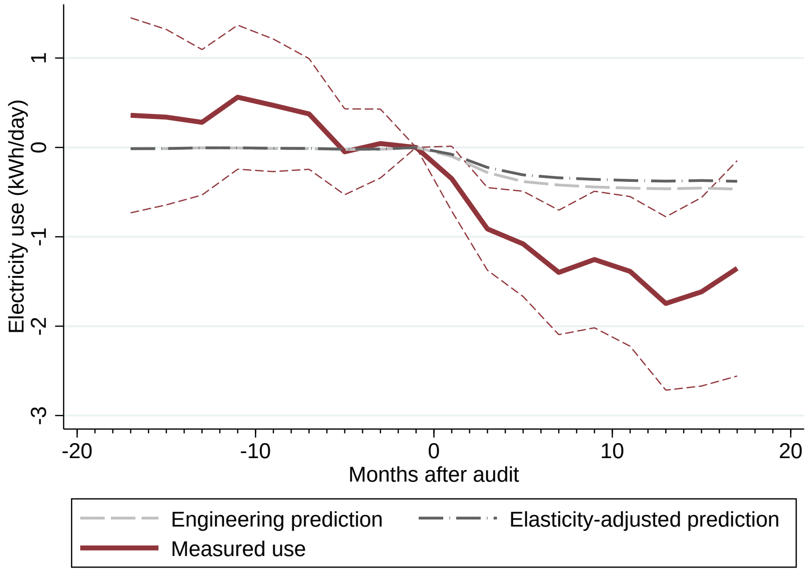
(a) **Post-Audit**



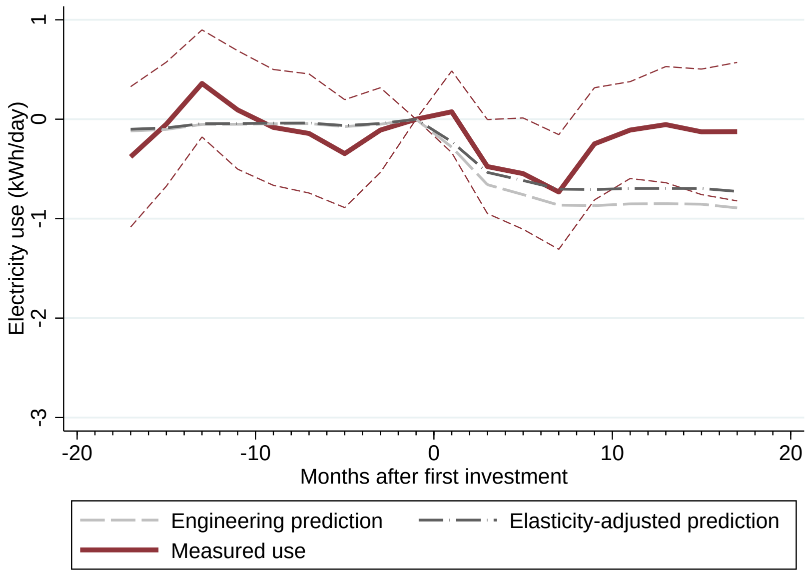
(b) **Post-Investment**

Notes: This figure presents energy use in event time relative to the household's audit or investment. Dashed lines are 90 percent confidence intervals. Average pre-audit natural gas use is 2.4 therms/day.

Figure A4: **Electricity Use in Event Time**



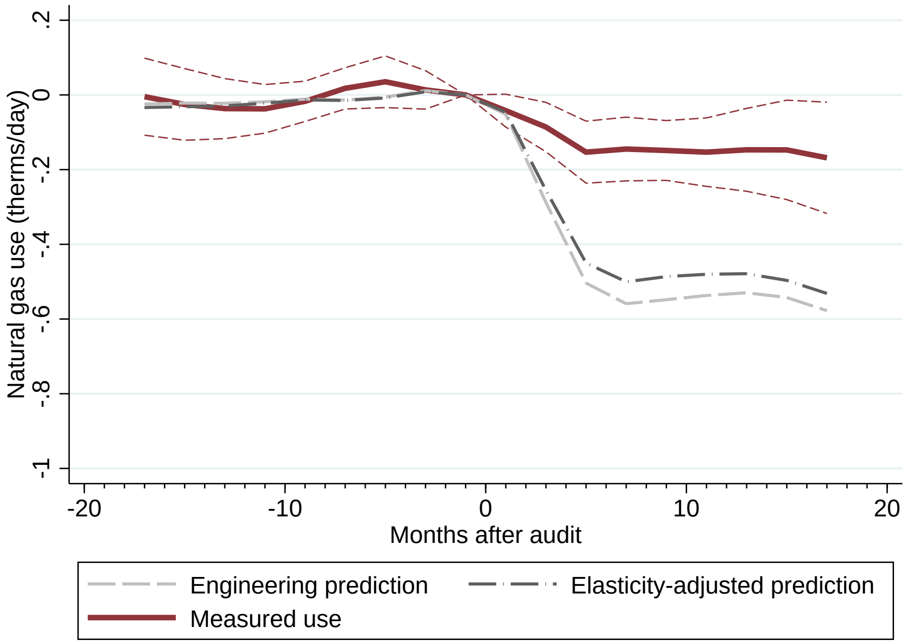
(a) **Post-Audit**



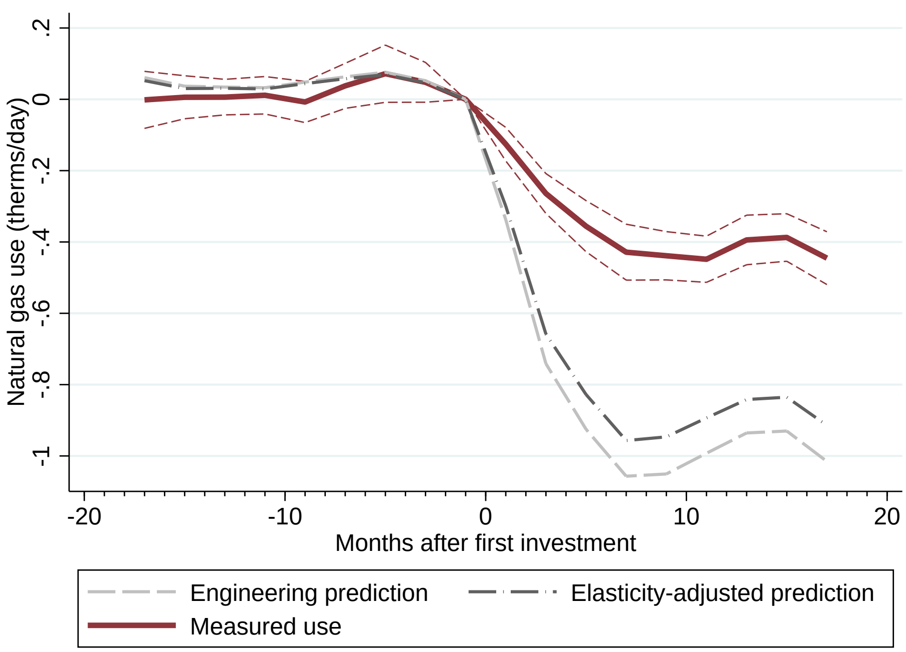
(b) **Post-Investment**

Notes: This figure presents energy use in event time relative to the household's audit or investment. Dashed lines are 90 percent confidence intervals. Average pre-audit electricity use is 21.4 kWh/day.

Figure A5: **Natural Gas Use in Event Time in Balanced Sample**



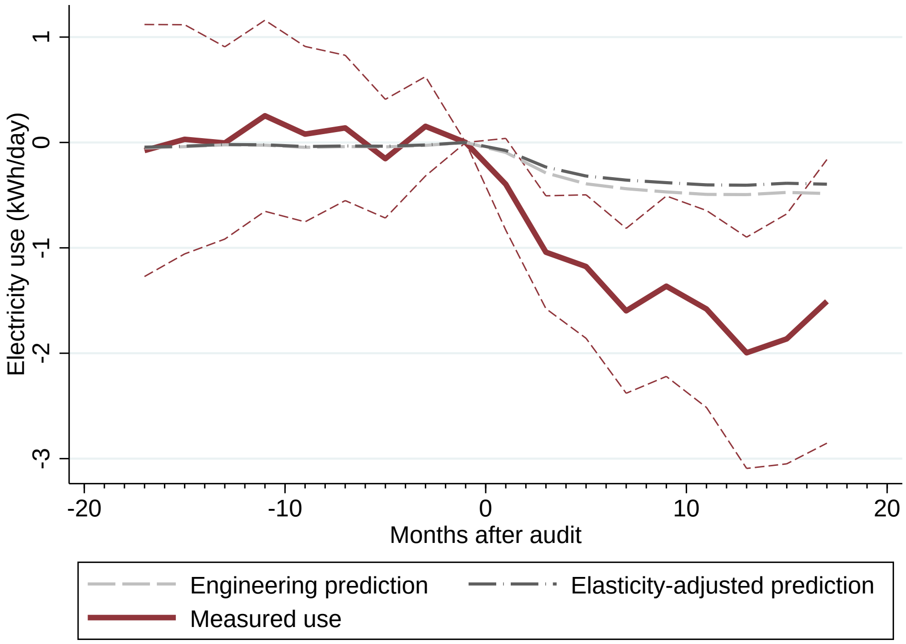
(a) **Post-Audit**



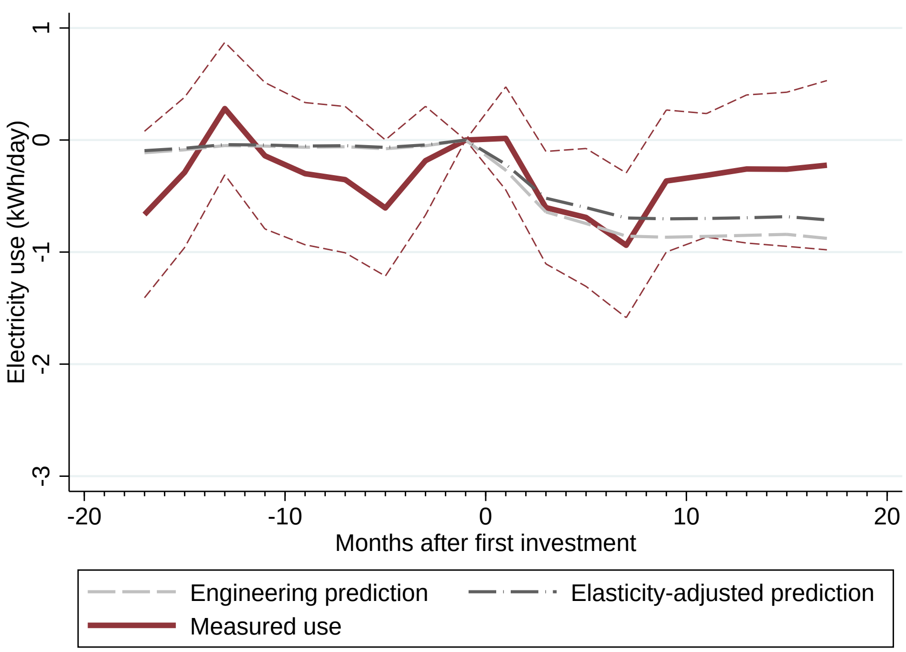
(b) **Post-Investment**

Notes: This figure presents energy use in event time relative to the household's audit or investment. Dashed lines are 90 percent confidence intervals. Average pre-audit natural gas use is 2.4 therms/day. This parallels Figure A3 but includes on the "balanced" sample of households.

Figure A6: **Electricity Use in Event Time in Balanced Sample**



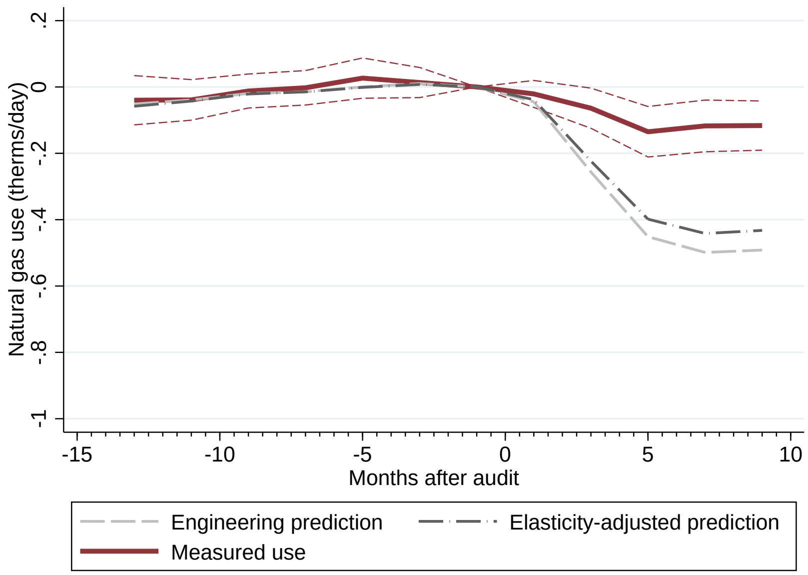
(a) **Post-Audit**



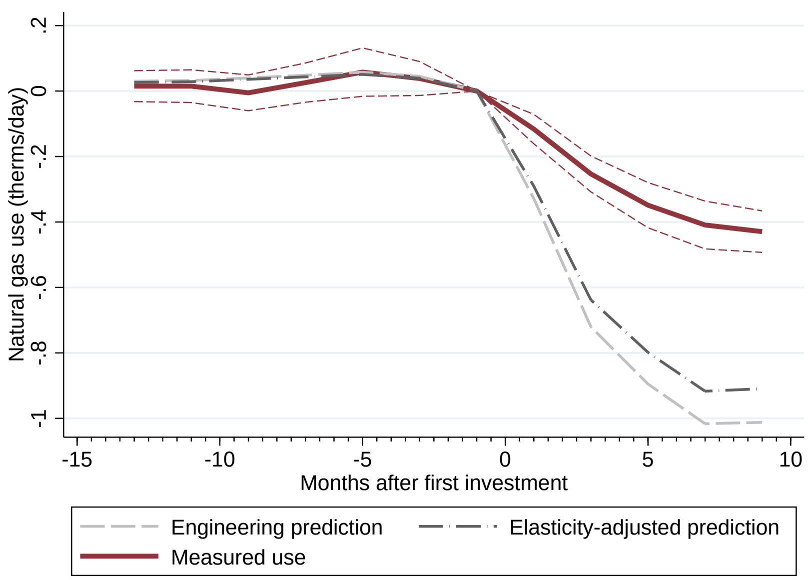
(b) **Post-Investment**

Notes: This figure presents energy use in event time relative to the household's audit or investment. Dashed lines are 90 percent confidence intervals. Average pre-audit electricity use is 21.4 kWh/day. This parallels Figure A4 but includes on the "balanced" sample of households.

Figure A7: **Natural Gas Use in Event Time in Balanced Sample over Shorter Window**



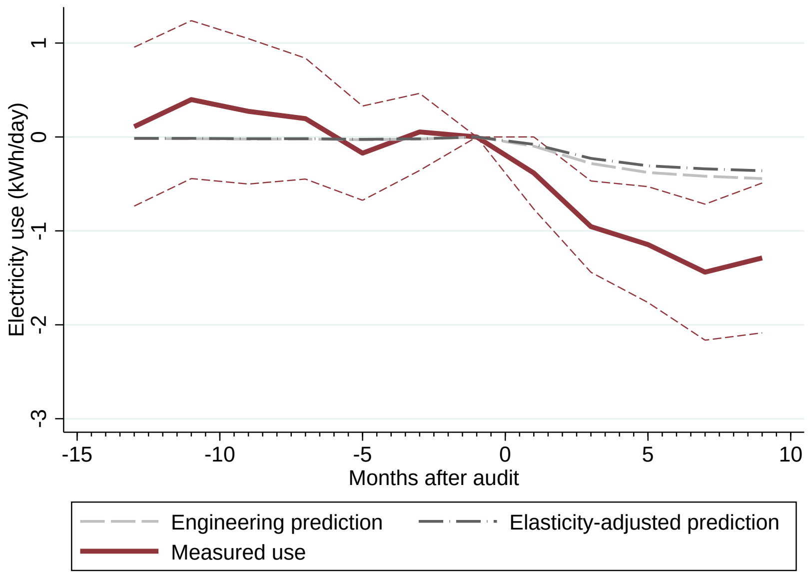
(a) **Post-Audit**



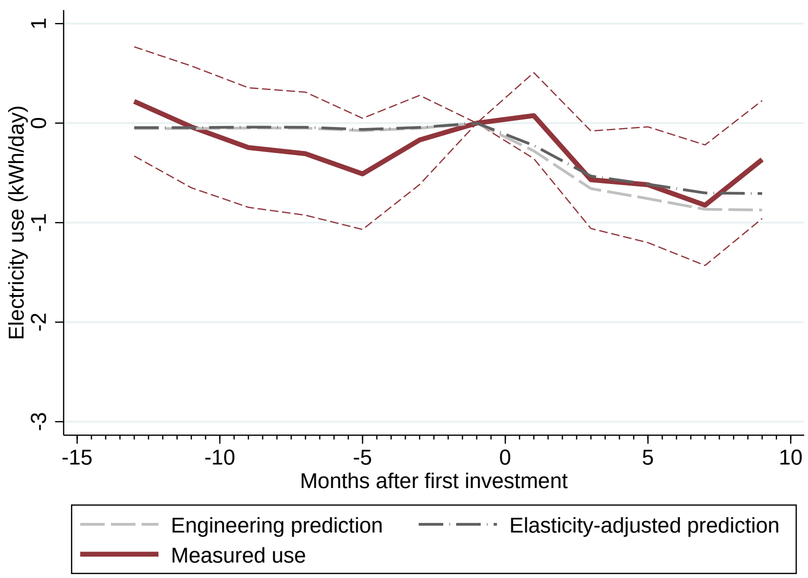
(b) **Post-Investment**

Notes: This figure presents energy use in event time relative to the household's audit or investment. Dashed lines are 90 percent confidence intervals. Average pre-audit natural gas use is 2.4 therms/day. This parallels Figure A5 but covers a shorter time window.

Figure A8: **Electricity Use in Event Time in Balanced Sample over Shorter Window**



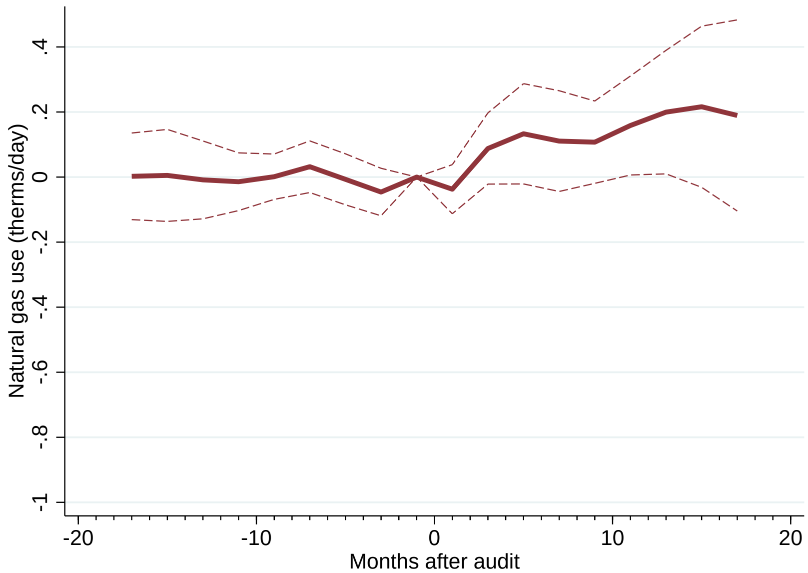
(a) **Post-Audit**



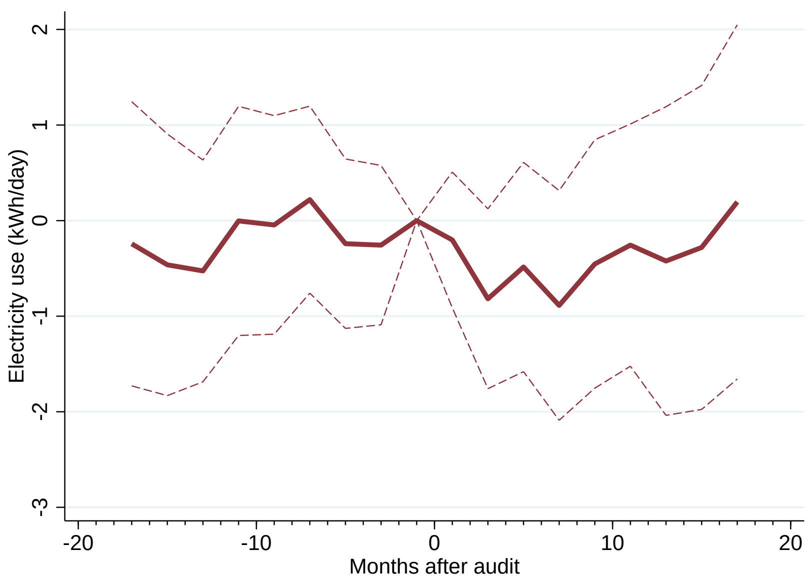
(b) **Post-Investment**

Notes: This figure presents energy use in event time relative to the household's audit or investment. Dashed lines are 90 percent confidence intervals. Average pre-audit electricity use is 21.4 kWh/day. This parallels Figure A6 but covers a shorter time window.

Figure A9: **Energy Use in Event Time for Households with No Recorded Investments**



(a) **Natural Gas**



(b) **Electricity**

Notes: This figure presents energy use in event time relative to the household's audit, for the subsample of households that made no recorded investments. Dashed lines are 90 percent confidence intervals. Average pre-audit natural gas use is 2.4 therms/day, and average pre-audit electricity use is 21.4 kWh/day.

E.B Formal Estimates of Equation (8)

Appendix Table A9 presents estimates of equation (8). Columns 1–3 are for natural gas, while columns 4–6 are for electricity. Columns 1 and 4 use weather-adjusted (but not elasticity-adjusted) engineering predictions as the dependent variable, columns 2 and 5 use the elasticity- and weather-adjusted engineering predictions, and columns 3 and 6 use measured energy use.

After six months, the elasticity-unadjusted engineering predictions imply 0.465 therms/day natural gas savings and 0.410 kWh/day electricity savings, or about 18 and 1.9 percent reductions relative to pre-audit average gas and electricity use, respectively. The elasticity-adjusted predictions are about 15 to 20 percent smaller. In contrast, the measured savings are 0.142 therms/day and 1.048 kWh/day, each of which represents about a five percent reduction.

Columns 3 and 6 show that more extreme weather (as measured by more heating or cooling degrees) is associated with more energy use. Comparing the cofficients in columns 3 versus 6 shows that natural gas use is more strongly associated with heating degrees, while electricity use is more strongly associated with cooling degrees. This is what would be expected given that cooling is often delivered by electricity-powered air conditioners, while heating is often delivered by gas-fueled heating systems. Since the engineering predictions are weather-adjusted, we see some association with heating and cooling degrees in columns 1, 2, 4, and 5.

Appendix Table A10 presents robustness checks and extensions to Appendix Table A9. Columns 1 and 2 present natural gas estimates, while columns 3 and 4 present electricity estimates. Columns 1 and 3 limit the sample to households that made no observed investments, paralleling Appendix Figure A9. Three of the four coefficients on the post audit indicators are statistically insignificant, which would suggest that these households do not make significant unobserved investments. For natural gas, however, the coefficient on *Post audit ($\geq$6 months)* is positive and significant with 90 percent confidence.[31]

Columns 2 and 4 repeat columns 1 and 4 of Table A9, except with weather-unadjusted and elasticity-unadjusted engineering predictions as the dependent variable. The estimates are similar to but slightly smaller than the estimates using weather-adjusted predictions. This implies that the post-audit and post-investment samples include slightly more extreme temperatures, in which more savings would be expected relative to prediction. The ≥ 6 month gas realization rate using the weather-unadjusted and elasticity-unadjusted engineering predictions is $-0.142/-0.429 \approx 33$ percent. For comparison, the weather-adjusted and elasticity-unadjusted natural gas realization rate is 0.31.

In the body of the paper, we defined $\mathbf{P}_{it}$ as a pair of post-audit indicators, estimating the effects of audits plus subsequent investments. Many households audited but did not invest, so the post-audit indicators from Table A9 represent the analogue of an intent-to-treat effect. Appendix Table A11

[31] Additional regressions show that this apparent post-audit gas use increase is unlikely to be driven by unobserved energy efficiency investments: the increase is no larger at households that received proposals of new heating and cooling systems or water heaters. The increase is unlikely to be driven by substitution from electricity to gas: post-audit gas use changes are not associated with electricity use changes. We do find that the apparent usage increase is limited to only above-median natural gas users. Our best guess is that the apparent increase is an idiosyncratic result for a subset of heavy users where the difference-in-differences controls are imperfect.

parallels Table A9, except adding post-investment indicators to $\mathbf{P}_{it}$. The predicted post-investment savings now load more heavily onto the post-investment indicators.

Using the post-investment coefficients, the natural gas and electricity realization rates are 0.49 and 0.28, respectively. When combined at retail prices, this gives an overall realization rate of 0.40. The electricity realization rate is considerably lower than in Table A9, but there are additional measured electricity savings in column 6 that load onto the post-audit indicators. There are two potential explanations. First, for households that did not install any investments during the audit, some investments made through the program may have been completed before the household's final investment completion date. Second, households may have made additional investments outside the program, such as appliance replacement. We use the post-audit event study in the body of the paper because of possible measurement error in investment completion dates and to focus on a conservatively high realization rate.

E.B.1 Explaining the Empirical Shortfall

To better understand the differences between the simulation and empirical estimates, we construct a "shortfall" variable $e_{it} - \Delta\tilde{e}_{it}^{adj}$, measuring the difference between actual use and elasticity- and weather-adjusted predicted savings. Columns 1 and 3 of Appendix Table A12 regress this shortfall on the interaction of post-audit indicators with indicators for whether the household made made one of the three most common investments: insulation, heating/cooling, and "other," which includes air sealing, new water heaters, new windows, pipe and duct sealing and insulation, and programmable thermostats. Positive coefficients mean that the investment is associated with a savings shortfall, while negative coefficients mean that the investment is associated with excess savings. Column 1 shows that insulation and heating/cooling are not statistically significantly associated with the shortfall variable, although the t-statistic on insulation is around 1.0. By contrast, natural gas shortfalls are statistically significantly associated with investments in the "other" category. (We do not have sufficient power to further disaggregate the "other" category.) Consistent with our results, a recent report by the TREAT model developers (PSD 2015b) also finds that air sealing and insulation predictions are important contributors to low realization rates. The TREAT model is "in close alignment with the predictions from best-in-class modeling tools" (PSD 2015b), so this finding may be more general than just TREAT.

Columns 2 and 4 add an indicator for whether a new appliance appears in the household's list of recommended measures. The program's takeup data do not include appliances because they were not subsidized, but the fact that this last interaction is significant suggests that these households may have reduced both natural gas and electricity use through unobserved appliance purchases. However, only 25 households in the energy use data were recommended appliances, so this is not enough to fully explain the excess post-audit electricity savings relative to the simulation predictions.

Program staff also speculated that a reason why the engineering model understated electricity savings could be that the model did not properly account for how insulation, air sealing, and heating/cooling system improvements also reduce loads on electric-powered furnace fans.

See Blasnik (2010), Nadel and Keating (1991), and (PSD 2015b) for more in-depth discussions of why simulations can systematically overestimate empirically realized savings. One broad class of explanations has to do with the assumptions and parameters used by the program and its auditors. For example, a home's baseline energy use could be overstated, giving excess simulated savings. This explains most of the empirical shortfall in the (PSD 2015b) New York study. In the Wisconsin programs, however, only 12 percent of pre-audit observations were calibrated with estimates instead of the audited household's true baseline energy use, so this seems unlikely to explain much of the our estimated shortfall.[32] Another factor that could cause excess simulated savings in Wisconsin and elsewhere is that simulations often assume best-case installation scenarios that could not be realized except through great effort by the most expert contractors.

[32] The default calibrations were 23 percent lower and 12 percent higher than the average audited household's pre-audit use of gas and electricity, respectively.

Table A9: **Post-Audit Energy Use Changes**

	(1)	(2)	(3)	(4)	(5)	(6)
	Natural gas (therms/day)			Electricity (kWh/day)		
Dependent variable:	Engineering prediction	Elasticity-adjusted prediction	Measured use	Engineering prediction	Elasticity-adjusted prediction	Measured use
Post audit (<6 months)	-0.297	-0.257	-0.102	-0.297	-0.237	-0.912
	(0.025)***	(0.024)***	(0.029)***	(0.026)***	(0.021)***	(0.241)***
Post audit (≥6 months)	-0.465	-0.408	-0.142	-0.410	-0.331	-1.048
	(0.030)***	(0.028)***	(0.036)***	(0.036)***	(0.030)***	(0.328)***
Average cooling degrees	-0.012	-0.012	0.035	-0.010	-0.009	0.718
	(0.002)***	(0.001)***	(0.003)***	(0.004)***	(0.003)***	(0.033)***
Average heating degrees	-0.012	-0.012	0.111	-0.006	-0.005	0.144
	(0.002)***	(0.002)***	(0.003)***	(0.002)***	(0.002)***	(0.016)***
N	61,845	61,845	61,845	63,654	63,654	63,654

Notes: This table presents estimates of equation (8) with daily usage of natural gas and electricity, respectively, as the dependent variables. Columns 1 and 4 use weather-adjusted (but not elasticity-adjusted) engineering predictions as the dependent variable, columns 2 and 5 use the elasticity- and weather-adjusted engineering predictions, and columns 3 and 6 use measured energy use. Average pre-audit natural gas usage is 2.4 therms/day, and average pre-audit electricity use 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 include household-by-calendar month fixed effects and month-of-sample fixed effects. Robust standard errors are in parentheses, clustered by household. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

Table A10: **Post-Audit Energy Use Changes: Alternative Estimates**

	(1)	(2)	(3)	(4)
	Natural gas (therms/day)		Electricity (kWh/day)	
Dependent variable:	Non-investors' measured use	Weather-unadjusted engineering prediction	Non-investors' Energy use	Weather unadjusted engineering prediction
Post audit (<6 months)	0.068	-0.237	-0.416	-0.293
	(0.043)	(0.019)***	(0.395)	(0.025)***
Post audit ($\geq$6 months)	0.112	-0.429	-0.145	-0.403
	(0.057)*	(0.026)***	(0.524)	(0.034)***
Average cooling degrees	0.043	0.001	0.803	-0.002
	(0.004)***	(0.001)	(0.059)***	(0.003)
Average heating degrees	0.120	0.000	0.171	-0.001
	(0.005)***	(0.001)	(0.026)***	(0.001)
N	22,455	61,845	23,098	63,654

Notes: This table presents alternative estimates of equation (8). Columns 1 and 3 present estimates for households with no recorded investments. Columns 2 and 4 present estimates of Equation (8) with weather-unadjusted and elasticity-unadjusted engineering predictions as the dependent variable. Average pre-audit natural gas use is 2.4 therms/day, and average pre-audit electricity use 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 include household-by-calendar month fixed effects and month-of-sample fixed effects. Robust standard errors are in parentheses, clustered by household. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

Table A11: **Post-Investment Energy Use Changes**

	(1)	(2)	(3)	(4)	(5)	(6)
	Natural gas (therms/day)			Electricity (kWh/day)		
Dependent variable:	Engineering prediction	Elasticity-adjusted prediction	Measured use	Engineering prediction	Elasticity-adjusted prediction	Measured use
Post audit (<6 months)	0.011	0.019	0.013	-0.071	-0.054	-0.827
	(0.021)	(0.020)	(0.030)	(0.019)***	(0.016)***	(0.264)***
Post audit (≥6 months)	0.018	0.026	0.064	-0.027	-0.019	-0.949
	(0.021)	(0.020)	(0.037)*	(0.021)	(0.018)	(0.353)***
Post investment (<6 months)	-0.850	-0.760	-0.323	-0.628	-0.509	-0.224
	(0.030)***	(0.029)***	(0.030)***	(0.037)***	(0.030)***	(0.295)
Post investment (≥6 months)	-1.038	-0.935	-0.460	-0.838	-0.681	-0.193
	(0.031)***	(0.030)***	(0.037)***	(0.052)***	(0.043)***	(0.347)
Average cooling degrees	-0.012	-0.012	0.035	-0.010	-0.008	0.719
	(0.001)***	(0.001)***	(0.003)***	(0.003)***	(0.003)***	(0.033)***
Average heating degrees	-0.014	-0.013	0.111	-0.007	-0.006	0.144
	(0.001)***	(0.001)***	(0.003)***	(0.002)***	(0.002)***	(0.016)***
N	61,845	61,845	61,845	63,654	63,654	63,654

Notes: This table presents estimates of equation (8) with daily use of natural gas and electricity, respectively, as the dependent variables, using data from the Wisconsin experiment. This parallels Table A9, except adds post-investment indicators. Columns 1 and 4 use weather-adjusted (but not elasticity-adjusted) engineering predictions as the dependent variable, columns 2 and 5 use the elasticity- and weather-adjusted engineering predictions, and columns 3 and 6 use measured energy use. Average pre-audit natural gas use is 2.4 therms/day, and average pre-audit electricity use 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 include household-by-calendar month fixed effects and month-of-sample fixed effects. Robust standard errors are in parentheses, clustered by household. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

Table A12: **Associations of Energy Savings Shortfalls with Specific Investments**

	(1)	(2)	(3)	(4)
	Natural gas (therms/day)		Electricity (kWh/day)	
Post audit × Insulation	0.135	0.114	-1.027	-1.295
	(0.136)	(0.134)	(1.453)	(1.356)
Post audit × Heating/cooling	-0.016	-0.010	-0.548	-0.461
	(0.064)	(0.063)	(0.539)	(0.534)
Post audit × Other	0.301	0.319	1.262	1.496
	(0.138)**	(0.136)**	(1.488)	(1.393)
Average cooling degrees	0.048	0.048	0.728	0.729
	(0.003)***	(0.003)***	(0.033)***	(0.033)***
Average heating degrees	0.124	0.124	0.150	0.151
	(0.003)***	(0.003)***	(0.017)***	(0.017)***
Post audit × Recommended appliance		-0.218		-3.268
		(0.067)***		(0.887)***
N	61,845	61,845	63,654	63,654

Notes: This table presents alternative estimates of equation (8), interacting $\boldsymbol{P}_{it}$ with indicators for installed and recommended investment categories. The dependent variable is $e_{it} - \Delta\tilde{e}_{it}^{pred}$, the difference between actual use and elasticity- and weather-adjusted predicted savings. Average pre-audit natural gas use is 2.4 therms/day, and average pre-audit electricity use 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 include household-by-calendar month fixed effects and month-of-sample fixed effects. Robust standard errors are in parentheses, clustered by household. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

E.C Callaway and Sant'Anna (2021) Estimator

In this appendix, we present estimates of the effects of audits on actual and predicted energy use using the Callaway and Sant'Anna (2021, henceforth "CS") estimator instead of the two-way fixed effects (TWFE) estimator from equation (8).

There are several differences between the structure of the CS estimator and our primary TWFE estimator. First, CS does not accommodate time-varying controls (such as our weather controls $\boldsymbol{W}_{it}$) or household-by-calendar month fixed effects. Second, to avoid having too many groups and time periods in the CS estimator, we reshape the data from household-by-billing date to household-by-month of sample. Third, since almost all households had audits over a 15-month period and the CS estimator uses only pre-treatment households as controls, the CS ATT estimates get very noisy for periods more than 12 months after the audit, so we present separate estimates for 6–12 months versus ≥ 13 months post-audit.

Panel A of Appendix Table A13 presents TWFE estimates that parallel the required CS structure: no weather controls, household fixed effects (but not household-by-calendar month fixed effects), reshaped household-by-month data, and an additional indicator for ≥ 13 months post-audit. Columns 1 and 3 use elasticity- and weather-adjusted engineering predictions as the dependent variables, while columns 2 and 4 use measured energy use. Compared to the primary estimates in columns 2, 3, 5, and 6 of Appendix Table A9, these estimates are moderately smaller but broadly comparable and often not statistically distinguishable. The largest differences are in the electricity use estimates, which we find are mostly explained by switching from household-by-calendar month fixed effects to household fixed effects.

Panel B presents the CS estimates using the same sample and control variables as Panel A. The reported CS sample sizes are smaller because the CS estimator drops all observations after the last audit date (August 2013) because there are no remaining unaudited households to serve as controls. We ignore the generally noisy estimates for ≥ 12 months post-audit. The natural gas estimates are very similar, except that the 6–12 month effects on predictions in column 1 are larger for CS than TWFE. The electricity estimates are generally larger for CS than TWFE, although the effects on measured use in column 4 are not statistically distinguishable.

The main parameter of interest is the realization rate: the ratio of the estimates in columns 2 versus 1 and 4 versus 3. The 6–12 month realization rates for TWFE from Appendix Table A13 for natural gas, electricity, and their combination at retail prices are 0.41, 2.46, and 0.65, respectively. The corresponding realization rates for CS are 0.32, 2.55, and 0.62. The corresponding elasticity-adjusted realization rates from the primary estimates in Appendix Table A9 (0.35, 3.17, and 0.68) are comparable and conservative for our argument, in that the primary combined realization rate is slightly closer to one.

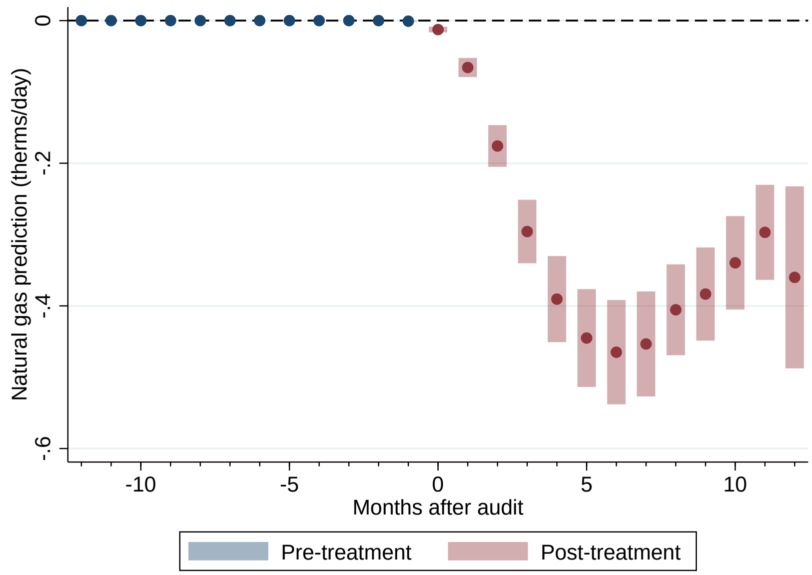
Appendix Figures A10 and A11 present the Callaway and Sant'Anna (2021) event study plots. ATTs more than 12 months post-audit become very noisy, so we cut off the graph at that point. The non-monotonicity in the ATTs over time, which is driven by seasonality, is more visible in these figures, perhaps partly driven by the lack of weather controls and household-by-calendar month fixed effects.

Table A13: **Two-Way Fixed Effects versus Callaway-Sant'Anna (2021) Estimator**

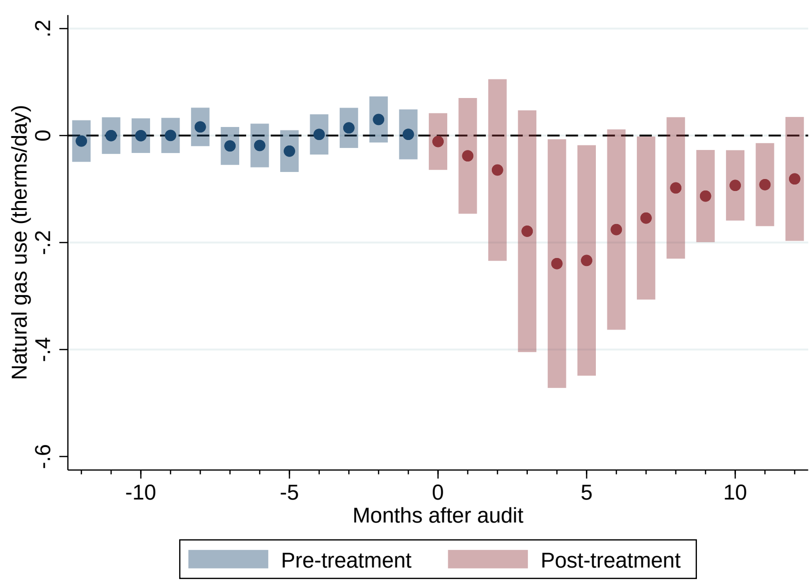
	Natural Gas (therms/day)		Electricity (kWh/day)	
	(1) Elasticity-adjusted prediction	(2) Measured use	(3) Elasticity-adjusted prediction	(4) Measured use
	Panel A: Two-Way Fixed Effects Estimates			
Post audit (<6 months)	−0.216***	−0.098*	−0.187***	−0.559*
	(0.017)	(0.039)	(0.017)	(0.232)
Post audit (6-12 months)	−0.339***	−0.137***	−0.276***	−0.677*
	(0.026)	(0.036)	(0.026)	(0.329)
Post audit (≥13 months)	−0.366***	−0.138*	−0.267***	−0.866
	(0.048)	(0.067)	(0.041)	(0.493)
N	63,794	63,794	65,133	65,133
	Panel B: Callaway-Sant'Anna (2021) Estimates			
Post audit (<6 months)	−0.192***	−0.106	−0.198***	−0.870**
	(0.014)	(0.071)	(0.015)	(0.416)
Post audit (6-12 months)	−0.416***	−0.133**	−0.389***	−0.993
	(0.032)	(0.059)	(0.040)	(0.670)
Post audit (≥13 months)	−0.390***	−0.372*	−0.403	2.395
	(0.074)	(0.193)	(0.267)	(2.033)
N	39,144	39,144	40,246	40,246

Notes: Panel A presents two-way fixed effects estimates with household fixed effects and month-of-sample fixed effects on data reshaped to household-by-month. Panel B presents parallel estimates using the Callaway and Sant'Anna (2021) estimator. Columns 1 and 3 use the elasticity- and weather-adjusted engineering predictions as the dependent variable, and columns 2 and 4 use measured energy use. Average pre-audit natural gas usage is 2.4 therms/day, and average pre-audit electricity use is 21.4 kWh/day. Standard errors are in parentheses, clustered by household. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

Figure A10: **Natural Gas Use in Event Time**



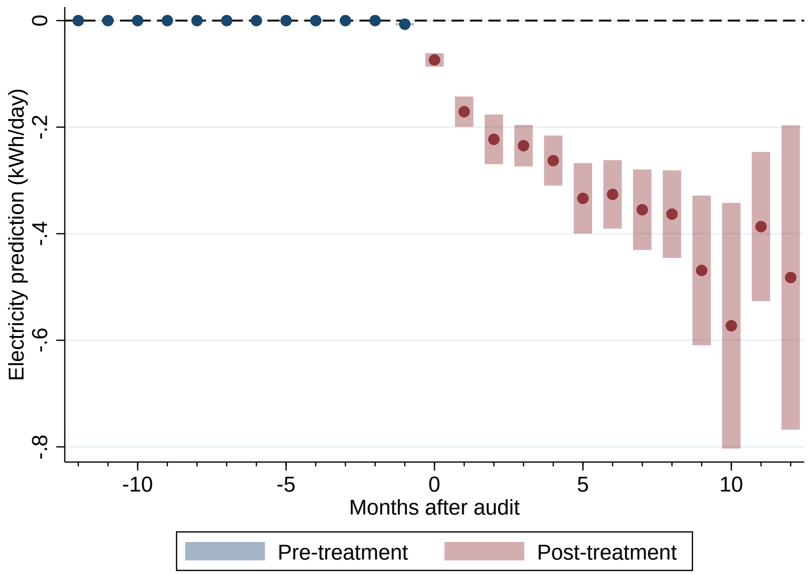
(a) **Elasticity-Adjusted Predictions**



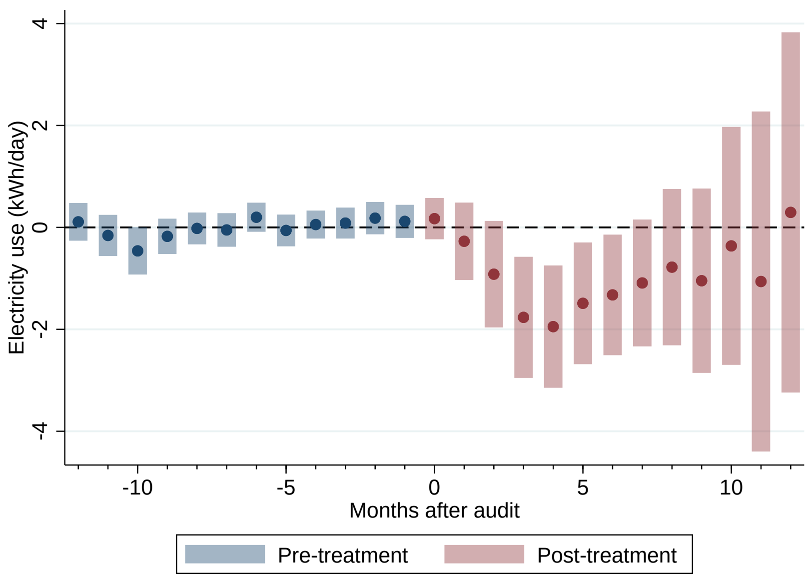
(b) **Measured Use**

Notes: This figure presents natural gas use in event time using the Callaway and Sant'Anna (2021) estimator. Average pre-audit natural gas use is 2.4 therms/day. Bars are 95 percent confidence intervals.

Figure A11: **Electricity Use in Event Time**



(a) **Elasticity-Adjusted Predictions**



(b) **Measured Use**

Notes: This figure presents natural gas use in event time using the Callaway and Sant'Anna (2021) estimator. Average pre-audit electricity use is 21.4 kWh/day. Bars are 95 percent confidence intervals.

F Model-Free Results Appendix

Table A14: **Peer-Reviewed Realization Rate Estimates in Home Retrofit Programs**

Paper	Location	Realization rate
Sebold and Fox (1985, Table 3)	San Diego	0.53
Dubin, Miedema, and Chandran (1986, page 323)	Florida	˜0.9
Hirst (1986, Table 2)	Pacific Northwest	0.63
Zivin and Novan (2016, Table 5)	San Diego	0.29
Fowlie, Greenstone, and Wolfram (2018, page 1600)	Michigan	0.30
Giraudet, Houde, and Maher (2018, Table 5)	Florida	0.68
Christensen et al. (2021, page 2)	Illinois	0.51

Notes: This table reports the average realization rate for papers that report multiple values.

Table A15: **Root Mean Squared Difference Between Subsidy and Uninternalized Externality Reduction**

Row	Scenario	RMSE ($)
1	Program subsidies versus adjusted uninternalized externalities	2,490
2	Program subsidies versus unadjusted uninternalized externalities	8,710
3	Program subsidies versus adjusted environmental externalities	3,663
4	Program subsidies versus unadjusted environmental externalities	8,397
5	Top 10 program subsidies versus adjusted uninternalized externalities	2,831

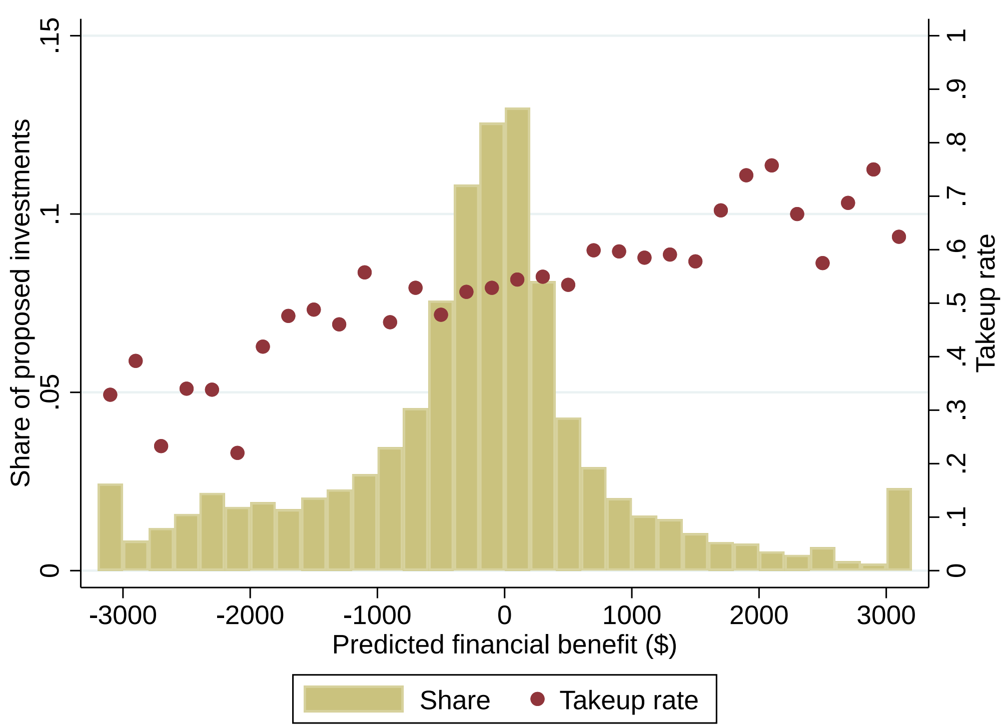
Notes: This table presents the root mean squared error (RMSE) $\sqrt{\frac{1}{N}\sum_i \left(s_{ij} - s_{ij}^{SB}\right)^2}$ of the difference between program subsidies and the second-best optimum. (In the simplified graphical discussion from Section III.D and in the formal analysis of Jacobsen et al. (2020), the magnitude of the distortion from subsidies that differ from the second best optimum is proportional to the RMSE.) Row 1 presents the RMSE based on the data displayed in Figure 5. Rows 2 and 4 do not consider the empirical adjustments from Section VI.A. Rows 3 and 4 use only the environmental externality (row 4 from Table 2) instead of the uninternalized externality (row 5 from Table 2). Row 5 is the RMSE across the service areas of the top 10 energy efficiency programs as rated by the American Council for an Energy-Efficient Economy (2020).

Table A16: **Insulation and HVAC Rebate Structures at ACEEE's Top 10 Energy Efficiency Programs**

Utility	Insulation rebate structure (cost, quantity, predicted energy savings)	HVAC rebate structure (cost, efficiency rating, size, predicted energy savings)	Differentiate by primary fuel? (Yes/No)
Eversource of Massachusetts	Cost	Size	No
National Grid of Massachusetts	Cost	Size	No
San Diego Gas and Electric	N/A	N/A	N/A
Commonwealth Edison	N/A	Efficiency rating	No
Baltimore Gas and Electric	Predicted energy savings	Predicted energy savings	No
Pacific Gas and Electric	N/A	Efficiency rating	No
L.A. Dep't of Water and Power	N/A	Efficiency rating	No
Detroit Edison	Quantity	Efficiency rating	No
Portland Gas and Electric	Quantity	Efficiency rating	Yes
Eversource of Connecticut	Quantity	Size & efficiency rating	No

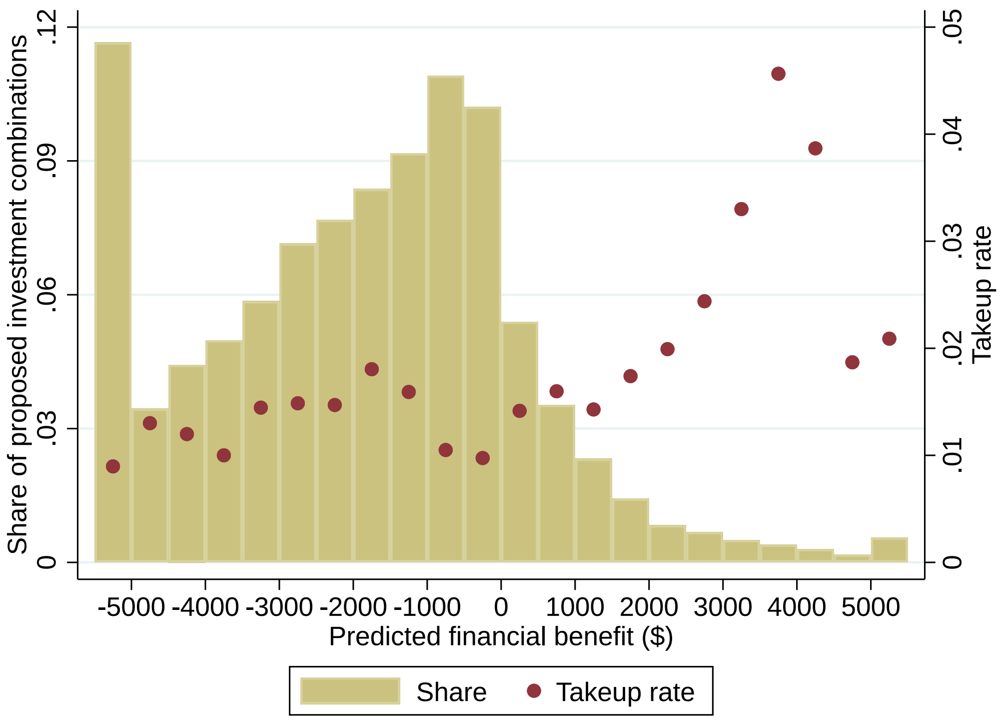
Notes: This table presents the structure of the subsidies offered for insulation as well as heating, ventilation, and air conditioning (HVAC) systems by the top 10 energy efficiency programs in the U.S. as rated by the American Council for an Energy Efficient Economy (ACEEE 2020).

Figure A12: **Predicted Financial Benefit and Investment Takeup, for Individual Investments**



Notes: The bars are a histogram of the predicted unsubsidized financial benefits $\Delta \boldsymbol{e}_{ij}\boldsymbol{p} - c_{ij}$ for all individual investments in the data. This figure parallels Figure 8, except that it considers all *individual* investments (instead of all investment *combinations*) and does not account for the investment subsidy (because the step function can be computed only for investment combinations). The dots are the takeup rates within each bin. We winsorize at ±$3000 for readability.

Figure A13: **Empirically Adjusted Financial Benefit and Investment Takeup**



Notes: The bars are a histogram of the empirically adjusted financial benefits $\hat{\boldsymbol{\chi}} \circ \Delta \boldsymbol{e}_{ij} \boldsymbol{p} - p_{ij}$ for all investment combinations in the data, where $\hat{\boldsymbol{\chi}}$ is the 3×1 vector of realization rates estimated in Section VI.A. The dots are the takeup rates within each bin. We winsorize at $\pm\$5000$ for readability.

G Belief Survey

To provide evidence on the extent to which people believe energy savings predictions in the context of home retrofit programs, we carried out a 200-person survey in May 2022. We recruited participants on MTurk, limiting to adults 25 and older within the U.S. with approval rates greater than 98 percent and more than 50 previous tasks approved. Relative to other online survey platforms, MTurk participants may be less representative of the U.S. population, but they often provide higher-quality survey responses, and response quality was particularly important for this survey.

The survey began by describing home energy efficiency retrofit programs: "In these programs, homeowners have a home energy audit and can receive rebates for energy-saving home improvements such as new insulation or heating systems. The programs are often a partnership between your local government and electric utility."

As shown in Appendix Figure A14, the survey then asked people to take the perspective of someone who had just had a home energy audit. It showed people a simplified version of the savings predictions on the audit report from page 3 of Appendix A, including only two of those proposed investments: attic knee wall insulation (with predicted annual savings of $63.11) and air sealing (with predicted annual savings of $70.05). The survey told participants that "the annual energy savings predictions are from a model calibrated to your house and energy use patterns." The survey then asked, "how much money do you think you would save per year from [attic knee wall insulation / air sealing]?" and asked people to explain their answer in a text box. The survey concluded with an attention check. All participants passed the attention check, but we dropped about 15 percent of responses where participants wrote non-sensical explanations.

Appendix Figure A15 presents the distribution of responses. A minority of people report savings beliefs less than the reported amounts, with explanations such as, "I looked at the annual savings listed in the chart and lowered my expectations a bit because that is a best-case-scenario number, most likely." About 92 percent of people report savings beliefs close or exactly equal to the amount listed on the audit report. A typical explanation was simply, "It's listed as the annual savings on the chart." The average realization rate belief was 0.99.

Figure A14: **Belief Survey Text**

Imagine you are a homeowner, and you've just had a home energy audit.
You receive the following information about possible improvements at your home. The annual energy savings predictions are from a model calibrated to your house and energy use patterns.

Proposed Investment	***Customer Cost***	***Annual Savings***	***Payback (years)***
Attic Knee Wall Insulation	**$225**	**$63.11**	**3.6**
Air Sealing	$800	$70.05	11.4

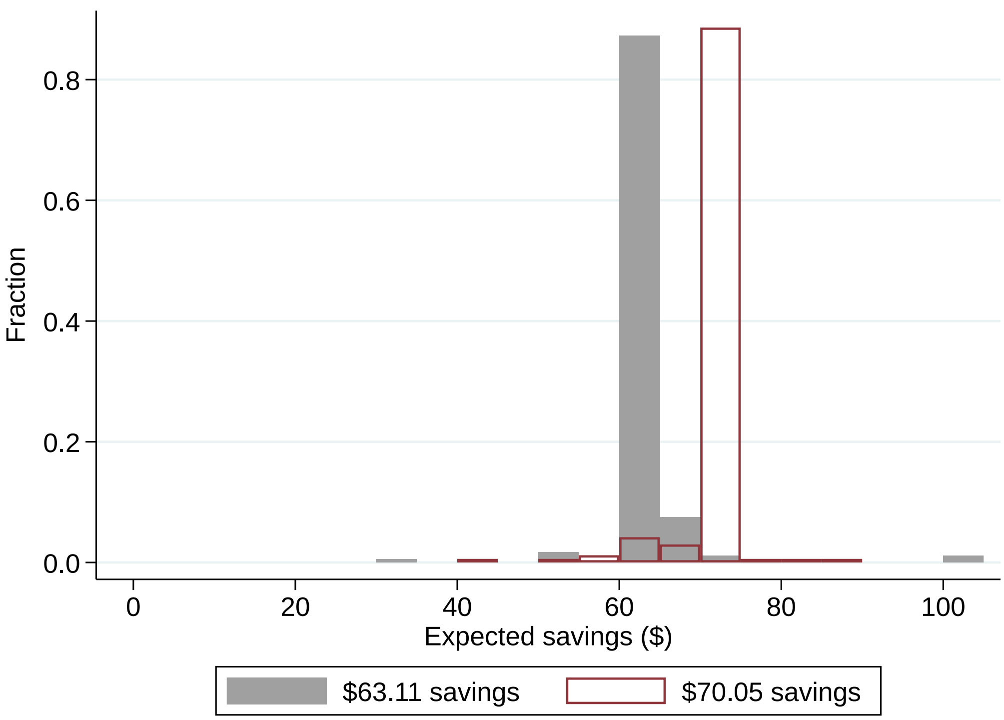
We want to understand how people evaluate the proposed improvements.

In your opinion, is **attic knee wall insulation** a good investment?

○ Yes ○ No

How much money do you think you would save per year from **attic knee wall insulation** ($)?

Figure A15: **Belief Survey: Distribution of Expected Savings**



Notes: This figure presents the distribution of responses to the question, "How much money do you think you would save per year from [attic knee wall insulation / air sealing]?" Participants were given a chart showing that attic knee wall insulation had a predicted annual savings of $63.11 and air sealing had a predicted annual savings of $70.05.

H Model Appendix

We drop many i subscripts throughout the appendix to be concise.

H.A Energy Services and Energy Demand

Substituting in the budget constraint into the CRRA utility function from equation (12) gives

$$u(x) = \omega \frac{\eta}{\eta + 1} x^{\frac{\eta+1}{\eta}} + w - x\boldsymbol{F} \cdot \boldsymbol{p}. \tag{40}$$

The first-order condition for the household's choice of energy services is

$$\frac{du}{dx} = \omega x^{1/\eta} - \boldsymbol{F} \cdot \boldsymbol{p} = 0. \tag{41}$$

Thus, the household's choice is

$$x^* = x^*(\boldsymbol{F} \cdot \boldsymbol{p}) = \left(\frac{\boldsymbol{F} \cdot \boldsymbol{p}}{\omega} \right)^{\eta}. \tag{42}$$

The resulting energy input demand is

$$e = x\boldsymbol{F} = \left(\frac{\boldsymbol{F} \cdot \boldsymbol{p}}{\omega} \right)^{\eta} \boldsymbol{F}. \tag{43}$$

H.B Weather-Adjusted Engineering Predictions

To construct the weather-adjusted engineering predictions, we classify individual investments into four seasonality categories indexed by s: 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 the cooling (heating) category, W_t^s is the daily average cooling (heating) degree days for billing period t, and $\bar{W}^s$ is the daily average cooling (heating) degrees from 2000–2011. For the "cooling or heating" seasonality category, W_t^s and $\bar{W}^s$ are the sum of heating plus cooling degrees. We define Δe_{fijt}^{pred} as the predicted savings for individual investment j. The weather-adjusted engineering prediction is

$$\Delta e_{fit}^{pred,w} = \Delta e_{fit}^{pred} \cdot \frac{\sum_s \sum_{j \in s} \frac{W_t^s}{\bar{W}^s} \Delta e_{fijt}^{pred}}{\sum_s \sum_{j \in s} \Delta e_{fijt}^{pred}}. \tag{44}$$

H.C Elasticity-Adjusted Engineering Predictions

We now derive the elasticity-adjusted predicted savings $\Delta \boldsymbol{e}_j^{pred,\eta}$ as a function of the elasticity-unadjusted predictions $\Delta \boldsymbol{e}_j^{pred}$. Holding energy services use constant, the predicted consumption reductions are

$$\Delta \boldsymbol{e}_j^{pred} = \left(\frac{\boldsymbol{F}_0 \cdot \boldsymbol{p}}{\omega}\right)^{\eta} \boldsymbol{F}_0 - \left(\frac{\boldsymbol{F}_0 \cdot \boldsymbol{p}}{\omega}\right)^{\eta} \boldsymbol{F}_j \tag{45}$$

$$= \left(\frac{\boldsymbol{F}_0 \cdot \boldsymbol{p}}{\omega}\right)^{\eta} (\boldsymbol{F}_0 - \boldsymbol{F}_j). \tag{46}$$

Adjusted for the change in energy services demand, the actual consumption reductions are

$$\Delta \boldsymbol{e}_j^{pred,\eta} = \left(\frac{\boldsymbol{F}_0 \cdot \boldsymbol{p}}{\omega}\right)^{\eta} \boldsymbol{F}_0 - \left(\frac{\boldsymbol{F}_j \cdot \boldsymbol{p}}{\omega}\right)^{\eta} \boldsymbol{F}_j \tag{47}$$

$$= \left(\frac{\boldsymbol{F}_0 \cdot \boldsymbol{p}}{\omega}\right)^{\eta} \boldsymbol{F}_0 - \left(\frac{\rho_j \boldsymbol{F}_0 \cdot \boldsymbol{p}}{\omega}\right)^{\eta} \boldsymbol{F}_j \tag{48}$$

$$= \left(\frac{\boldsymbol{F}_0 \cdot \boldsymbol{p}}{\omega}\right)^{\eta} \left(\boldsymbol{F}_0 - \rho_j^{\eta} \boldsymbol{F}_j\right) \tag{49}$$

$$= \Delta \boldsymbol{e}_j^{pred} \circ \left[\left(\boldsymbol{F}_0 - \rho_j^{\eta} \boldsymbol{F}_j\right) \oslash (\boldsymbol{F}_0 - \boldsymbol{F}_j)\right], \tag{50}$$

where $\circ$ is the element-wise (Hadamard) multiplication operator, and $\oslash$ is the element-wide division operator. Define $x_0 = x^*(\boldsymbol{F}_0 \cdot \boldsymbol{p}_0)$ as baseline energy services demand.

If there is only one fuel, then $\rho = F_j/F_0 = 1 - \Delta e_j^{pred}/e_0$, and equation (50) simplifies to

$$\Delta e_j^{pred,\eta} = \Delta e_j^{pred} \frac{F_0 - \rho_j^{\eta} F_j}{F_0 - F_j} \tag{51}$$

$$= \Delta e_j^{pred} \frac{F_0/F_0 - \rho_j^{\eta} F_j/F_0}{F_0/F_0 - F_j/F_0} \tag{52}$$

$$= \Delta e_j^{pred} \frac{1 - \rho_j^{\eta+1}}{1 - \rho_j} \tag{53}$$

$$= \Delta e^{pred} \frac{1 - \left(1 - \Delta e_j^{pred}/e_0\right)^{\eta+1}}{\Delta e_j^{pred}/e_0}. \tag{54}$$

Adding back all fuel and household subscripts, the elasticity-adjusted engineering prediction for Section VI.A is

$$\Delta e_{fit}^{pred,adj} = \Delta e_{fit}^{pred} \cdot \frac{1 - \left(1 - \Delta e_{fit}^{pred}/e_{fi0}\right)^{\eta+1}}{\Delta e_{fit}^{pred}/e_{fi0}}. \tag{55}$$

H.D Energy Services Demand Elasticity

The demand slope with respect to the price of energy services is

$$\frac{dx^*}{d(\boldsymbol{F} \cdot \boldsymbol{p})} = \eta \left(\frac{1}{\omega}\right)^{\eta} (\boldsymbol{F} \cdot \boldsymbol{p})^{\eta-1}, \tag{56}$$

so the price elasticity of demand for energy services is

$$\frac{dx^*}{d(\boldsymbol{F}\cdot\boldsymbol{p})}\frac{\boldsymbol{F}\cdot\boldsymbol{p}}{x^*}=\eta\left(\frac{1}{\omega}\right)^{\eta}(\boldsymbol{F}\cdot\boldsymbol{p})^{\eta-1}\cdot(\boldsymbol{F}\cdot\boldsymbol{p})\left(\frac{\boldsymbol{F}\cdot\boldsymbol{p}}{\omega}\right)^{-\eta}=\eta. \tag{57}$$

H.E Indirect Utility Formula

Substituting $x^*(\boldsymbol{F}\cdot\boldsymbol{p})$ back into the utility function gives indirect utility:

$$\begin{aligned}
v(\boldsymbol{F}\cdot\boldsymbol{p}) &= \omega\frac{\eta}{\eta+1}x^*(\boldsymbol{F}\cdot\boldsymbol{p})^{\frac{\eta+1}{\eta}}+w-x^*(\boldsymbol{F}\cdot\boldsymbol{p})\boldsymbol{F}\cdot\boldsymbol{p} && (58)\\
&= w+\frac{\eta}{\eta+1}\omega\left(\frac{\boldsymbol{F}\cdot\boldsymbol{p}}{\omega}\right)^{\eta+1}-\left(\frac{\boldsymbol{F}\cdot\boldsymbol{p}}{\omega}\right)^{\eta}\boldsymbol{F}\cdot\boldsymbol{p} && (59)\\
&= w+\frac{\eta}{\eta+1}\left(\frac{\boldsymbol{F}\cdot\boldsymbol{p}}{\omega}\right)^{\eta}\boldsymbol{F}\cdot\boldsymbol{p}-\left(\frac{\boldsymbol{F}\cdot\boldsymbol{p}}{\omega}\right)^{\eta}\boldsymbol{F}\cdot\boldsymbol{p} && (60)\\
&= w+\frac{-1}{\eta+1}\left(\frac{\boldsymbol{F}\cdot\boldsymbol{p}}{\omega}\right)^{\eta}\boldsymbol{F}\cdot\boldsymbol{p}. && (61)
\end{aligned}$$

We normalize $v(\boldsymbol{F}_0\cdot\boldsymbol{p}_0)\equiv 0$. The change in indirect utility from a change in energy services cost to $\boldsymbol{F}_j\cdot\boldsymbol{p}'$ is

$$\begin{aligned}
v_j &= \frac{-1}{\eta+1}\left[\left(\frac{\boldsymbol{F}_j\cdot\boldsymbol{p}'}{\omega}\right)^{\eta}\boldsymbol{F}_j\cdot\boldsymbol{p}'-\left(\frac{\boldsymbol{F}_0\cdot\boldsymbol{p_0}}{\omega}\right)^{\eta}\boldsymbol{F}_0\cdot\boldsymbol{p}_0\right] && (62)\\
&= \frac{-1}{\eta+1}\left[\left(\frac{\rho_j\boldsymbol{F}_0\cdot\boldsymbol{p}_0}{\omega}\right)^{\eta}\rho_j\boldsymbol{F}_0\cdot\boldsymbol{p}_0-\left(\frac{\boldsymbol{F}_0\cdot\boldsymbol{p_0}}{\omega}\right)^{\eta}\boldsymbol{F}_0\cdot\boldsymbol{p}_0\right] && (63)\\
&= \frac{-1}{\eta+1}\left(\frac{\boldsymbol{F}_0\cdot\boldsymbol{p}_0}{\omega}\right)^{\eta}\boldsymbol{F}_0\cdot\boldsymbol{p}_0\left[\rho_j^{\eta+1}-1\right] && (64)\\
&= \frac{1}{\eta+1}\boldsymbol{e}_0^*\cdot\boldsymbol{p}_0\left(1-\rho_j^{\eta+1}\right), && (65)
\end{aligned}$$

where $\boldsymbol{e}_0^*\cdot\boldsymbol{p}_0$ is baseline energy expenditures.

H.F Counterfactual Energy Consumption Formula

In this appendix, we write energy input demand at price $\boldsymbol{p}$ after choosing investment combination j as a function of observables.

Adjusting equation (46) for the realization rate $\boldsymbol{\chi}$, we have

$$\boldsymbol{\chi}\circ\Delta\boldsymbol{e}^{pred}=\left(\frac{\boldsymbol{F}_0\cdot\boldsymbol{p}}{\omega}\right)^{\eta}\boldsymbol{F}_0\circ(\boldsymbol{F}_0-\boldsymbol{F}_j)\oslash\boldsymbol{F}_0=\boldsymbol{e}_{i0}\circ(\boldsymbol{F}_0-\boldsymbol{F}_j)\oslash\boldsymbol{F}_0=\boldsymbol{e}_{i0}\circ(\boldsymbol{1}-\boldsymbol{F}_j\oslash\boldsymbol{F}_0). \tag{66}$$

Rearranging gives

$$\boldsymbol{\chi} \circ \Delta \boldsymbol{e}^{pred} \oslash \boldsymbol{e}_{i0} = \mathbf{1} - \boldsymbol{F}_j \oslash \boldsymbol{F}_0 \tag{67}$$

$$\boldsymbol{F}_j \oslash \boldsymbol{F}_0 = \mathbf{1} - \boldsymbol{\chi} \circ \Delta \boldsymbol{e}^{pred} \oslash \boldsymbol{e}_{i0}. \tag{68}$$

Energy input demand is

$$\begin{aligned} \boldsymbol{e}_j^* &= \left(\frac{\boldsymbol{F}_j \cdot \boldsymbol{p}}{\omega}\right)^{\eta} \boldsymbol{F}_j && (69)\\ &= \left(\frac{\rho_j \boldsymbol{F}_0 \cdot \boldsymbol{p}_0}{\omega}\right)^{\eta} \boldsymbol{F}_j && (70)\\ &= \rho_j^{\eta} \left(\frac{\boldsymbol{F}_0 \cdot \boldsymbol{p}_0}{\omega}\right)^{\eta} \boldsymbol{F}_0 \circ \boldsymbol{F}_j \oslash \boldsymbol{F}_0 && (71)\\ &= \rho_j^{\eta} \boldsymbol{e}_{i0}^* \circ \left(\mathbf{1} - \boldsymbol{\chi} \circ \Delta \boldsymbol{e}^{pred} \oslash \boldsymbol{e}_{i0}\right). && (72) \end{aligned}$$

H.G Additional Tables

Table A17: **Association of Baseline Energy Use or Proposed Investment Characteristics with the Experimental Audit Subsidy**

	(1) Baseline energy use ($/year)	(2) Proposed investments: number	(3) Proposed investments: savings ($/year)	(4) Proposed investments: cost ($/investment)
Audit subsidy ($)	0.656	0.002	-0.117	-0.193
	(0.694)	(0.003)	(0.414)	(0.638)
N	1,394	1,394	1,394	1,394

Notes: This table presents regressions of baseline energy use (column 1) and characteristics of proposed investments (columns 2–4) on the experimental audit subsidy, in the sample of all households that audited. Robust standard errors are in parentheses. *, **, ***: statistically different from zero with 90, 95, and 99 percent probability, respectively.

Table A18: **Takeup Parameters: Alternative Estimates**

	(1) Primary estimates	(2) Fix $3 \times \alpha^I$	(3) Inelastic energy demand ($\eta = 0$)	(4) 5% discount rate
Panel A: Audit Parameters				
Dollarized benefit / 1,000 (α^A)	139	147	139	139
	(75.65)	(90.55)	(75.73)	(76.34)
Received letter	1.06	1.02	1.06	1.08
	(1.23)	(1.15)	(1.23)	(1.27)
House age (years) / 100	5.45	5.64	5.45	5.49
	(3.36)	(3.87)	(3.37)	(3.42)
Property value ($) / 1,000,000	−1.41	−1.33	−1.42	−1.45
	(1.86)	(1.91)	(1.86)	(1.88)
Building footprint (sq. feet) / 1,000	0.91	0.92	0.91	0.92
	(0.56)	(0.60)	(0.56)	(0.57)
Madison	3.14	3.17	3.15	3.18
	(2.17)	(2.33)	(2.17)	(2.21)
Census tract hybrid vehicle share	0.57	0.62	0.57	0.57
	(0.35)	(0.43)	(0.35)	(0.36)
Constant	−13.28	−13.77	−13.29	−13.36
	(6.55)	(7.54)	(6.57)	(6.69)
Panel B: Investment Parameters				
Dollarized benefit / 10,000 (α^I)	1.63	4.89	1.56	1.19
	(0.57)	–	(0.53)	(0.41)
Air sealing	3.15	3.01	3.16	3.20
	(0.22)	(0.22)	(0.22)	(0.22)
Insulation	3.62	3.22	3.62	3.79
	(1.14)	(0.94)	(1.14)	(1.25)
Heating/cooling system	0.41	0.94	0.41	0.38
	(0.19)	(0.16)	(0.18)	(0.17)
Windows	−0.52	0.12	−0.54	−0.63
	(0.39)	(0.40)	(0.39)	(0.37)
Pipe wrap/duct sealing	−0.23	−0.40	−0.22	−0.19
	(0.68)	(0.67)	(0.68)	(0.69)
Programmable thermostat	1.09	0.96	1.09	1.13
	(0.44)	(0.43)	(0.45)	(0.45)
Constant	−77.19	−67.89	−76.88	−80.50
	(21.09)	(20.12)	(20.98)	(22.41)
Standard deviation of ι (σ)	28.05	24.29	27.93	29.37
	(8.51)	(8.11)	(8.46)	(9.03)
N	101,881	101,881	101,881	101,881

Notes: This table presents estimates of the audit and investment takeup parameters, using method of simulated moments. Audit dollarized benefit refers to $\mathbb{E}_{\mathcal{J}\in\mathcal{U}}\left[\max_{j\in\mathcal{J}}\left\{V_{ij}^I\right\}\right] - p_i^A$; investment dollarized benefit refers to $v_{ij} - p_{ij}$. Column 1 repeats the primary estimates from column (2) of Table 4. Column 2 fixes α^I at $3\times$ the value from column (1) and drops $v_{ij} - p_{ij}$ from $\boldsymbol{x}_{ij}^I$ in the moment described in equation (23). Column 3 constructs v_{ij} assuming $\eta = 0$. Column 4 uses a five percent discount rate (instead of three percent).

Table A19: **Effects of Wisconsin Subsidies Under Alternative Assumptions**

	(1) Base case	(2) No selection ($\sigma = 0$)	(3) Fix $3 \times \alpha^I$	(4) Inelastic energy demand ($\eta = 0$)	(5) 5% discount rate
ΔTotal surplus ($/household)	-0.82	-12.08	-1.84	-0.81	-0.64
Marginal value of public funds	0.93	-0.14	0.85	0.93	0.94
Cost of carbon abatement ($/ton)	365	326	310	369	421

Notes: Ths table evaluates the Wisconsin programs under alternative assumptions. Column 1 presents the base case from column 1 of Table 5. Column 2 presents estimates with no self-selection, using the preference parameter estimates from column 1 of Table 4. Column 3 presents estimates with more elastic investment demand, using the preference parameter estimates from column 2 of Appendix Table A18. Column 4 presents estimates with inelastic energy demand, using the preference parameter estimates from column 3 of Appendix Table A18. Column 5 presents estimates that hold marginal damages constant but use a five percent discount rate (instead of three percent). Climate externality reductions are based on a $172 per ton social cost of carbon (in 2013 dollars).

I Observed Benefits and Costs of Wisconsin and National Better Buildings Programs

In this appendix, we present an accounting-style evaluation of the observed benefits and costs of the Wisconsin programs. To test whether our results from Wisconsin generalize more broadly, we augment the analysis with microdata from all 37 Better Buildings programs nationwide that reported data to the Department of Energy. We prepared the nationwide data according to the process described in Appendix D.B.

Table A20 describes the nationwide data. These data are more limited than for the Wisconsin programs: for each household retrofitted through the program, we observe the categories of investments made (as in Wisconsin, typically insulation, air sealing, and heating and cooling), total (unsubsidized) cost, and simulation estimates of total annual energy savings in physical units. The primary two fuels saved are electricity and natural gas, but the data also include predicted savings of heating oil, propane, kerosene, and wood. We group these latter four fuels as "other fuels" for Appendix Table A21. We translate physical units to dollars and externality reductions using the same process described in Section IV, with data from the state and electricity market where the site is located. Of the 75,110 households in the original data, 58,418 survive the data cleaning process described in Appendix D.

Appendix Table A21 presents our estimates of the observed benefits and costs of investments made through the programs. Columns 1 and 2 present results for the Wisconsin programs, while columns 3 and 4 present the nationwide BBNP results.[33] The observed costs are audit and investment costs; the observed benefits are the social value of energy savings (valued at acquisition costs and externalities reported in rows 2 and 4 of Table 2) discounted at three percent. Columns 1 and 3 use the engineering predictions to calculate energy and externality reductions. Column 2 multiplies the Wisconsin simulation estimates by realization rates of 3.17 for electricity and 0.35 for natural gas and other fuels, on the basis of the empirical estimates presented in Figure 4.[34] Column 4 multiplies the national program simulation estimates by realization rates of 0.59 for electricity and 0.47 for natural gas and other fuels, on the basis of the DOE's (2015a) national Better Buildings program evaluation.

The programs' observed costs outweighed their benefits. Using the engineering predictions, the Wisconsin and national programs have benefit/cost ratios of 2.14 and 1.61 at three percent discount rates, respectively, with internal rates of return of 13.31 and 9.62 percent. After adjusting for the empirical realization rates, the benefit/cost ratios are 0.88 and 0.81, and the IRRs are 1.08 and 0.63 percent. Thus, the national programs performed slightly worse than the Wisconsin programs, giving

[33]We assume that the unsubsidized audit cost is $c_A = \$400$, based on typical market prices. The national Better Buildings program dashboard reports a grand total of 138,323 single-family home audits and 74,493 retrofits. Applying that 54 percent follow-through rate implies that 108,474 audits were required to generate the 58,418 valid projects in our data.

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

further evidence that our qualitative results are not specific to Wisconsin.

Very different assumptions about energy prices or environmental externalities would be required for empirically adjusted benefits to exceed costs in the Wisconsin programs: energy acquisition costs would need to be 1.5 times larger, or all environmental externalities would need to be 1.2 times larger, or the social cost of carbon would need to be $206 per ton. Furthermore, some natural alternative assumptions worsen the benefit/cost ratios. For example, natural gas and heating oil prices were higher over 2011–2014 (the period we use to construct acquisition costs) than they were from 2015–2021, so using more recent prices would decrease net benefits. As another example, the levelized cost of electricity from new onshore wind farms is less than our assumed social marginal cost of electricity (EIA 2021), so assuming that new wind farms are the long-run marginal electricity source would also decrease net benefits.

The Department of Energy released an official evaluation of the nationwide Better Buildings Neighborhood Program DOE (2015a). The DOE report evaluates BBNP as an economic stimulus program, which is relevant given that it was funded through the 2009 American Recovery and Reinvestment Act, but does not present a benefit-cost analysis that one would use under normal macroeconomic conditions. By contrast, our analysis does not consider economic stimulus benefits but is more appropriate under normal conditions. The DOE's headline benefit/cost ratio of 3.0 is the ratio of economic activity created (i.e. private sector expenditures plus tax revenues) divided by federal government outlays. This calculation is not comparable to ours. Indeed, it is mechanically opposite: energy efficiency investment costs count as a cost in our framework, while they count as a benefit in the DOE framework because they represent economic stimulus.

The observed benefits and costs of investments made through the program provide a transparent benchmark for program evaluation. However, there are two important limitations. First, this approach does not account for unobserved benefits and costs experienced by program participants. As shown in Section VI.D, inelastic investment takeup decisions suggest a wide dispersion in unobservables. Second, this model-free approach does not allow us to evaluate counterfactual policies. We would need to simulate a world without the program subsidies to evaluate their causal effects, because some investments might have been made even without subsidies. Furthermore, it would be useful to consider alternative subsidy structures that might be more socially beneficial. Our model and counterfactuals in Sections VII and VIII address these limitations.

Table A20: **Summary Statistics for National Better Buildings Program Data**

	Mean	Std. dev.	Min.	Max.
Total cost ($)	6,705	5,711	100	30,000
Retail energy cost savings ($/year)	472	430	13	2,734

Notes: The sample includes the 58,418 households that made investments and survive the data cleaning process described in Appendix D.B. Energy prices are averages over 2011–2014; see Appendix D.C for details.

Table A21: **Observed Benefits and Costs, Including National Programs**

	(1)	(2)	(3)	(4)
	Wisconsin programs		*National programs*	
Source of energy savings estimates:	Engineering predictions	Empirically adjusted	Engineering predictions	Empirically adjusted
Cost ($millions)				
Audit costs (at $400 per audit)	0.33		43.39	
Investment costs	4.82		391.72	
Total cost	5.15		435.11	
Energy savings ($millions at 3% discount rate)				
Natural gas	2.28	0.70	89.91	42.26
Electricity	0.09	0.22	49.68	29.31
Other fuels	0.72	0.22	79.57	37.40
Total	3.09	1.14	219.16	108.97
Environmental externality reductions ($millions at 3% discount rate)				
Climate (at $172 per ton CO_2)	7.09	3.02	414.21	207.51
SO2/NOx/PM	0.84	0.39	69.21	37.60
Total	7.94	3.41	483.42	245.11
Summary				
Benefits – costs ($millions)	5.87	−0.60	267.48	−81.03
Benefits – costs ($/household that invested)	7,143.79	−731.05	4,578.72	−1386.99
Benefit/cost ratio	2.14	0.88	1.61	0.81
Internal rate of return (percent)	13.31	1.08	9.62	0.63

Notes: Columns 1 and 2 present estimates for the Wisconsin sample. Columns 3 and 4 present estimates for the 58,418 households that invested under the national Better Buildings program and have valid data. Columns 1 and 3 use the engineering predictions. Column 2 multiplies predicted electricity and gas/other fuels savings from column 1 by realization rates of 3.17 and 0.35, respectively, based on the estimates in Table A9. Column 4 multiplies predicted electricity and gas/other fuels savings from column 3 by realization rates of 0.59 and 0.47, respectively, based on estimates from DOE (2015a). Energy savings are calculated at average wholesale prices over 2011–2014. Climate externality reductions are based on a $172 per ton social cost of carbon (in 2013 dollars).

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