On the Back Burner: Experimental Evidence For Energy Transitions *

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Abstract

Many technology subsidies aim to shift household behavior, but adoption need not translate into sustained use. The challenge is acute when sustained use requires a complementary household investment, such as cookware for an induction stove or home charging for an electric vehicle. A randomized trial with 2,192 urban Nepali households varies subsidies (for induction stoves or compatible cookware) and information (cost or health benefits of electric cooking), and 29 months of administrative billing data trace adoption to long-run consumption. Subsidies raise adoption, but information alone does not. However, combined subsidy-and-information treatments shift the type of adoption: they push more households to acquire both the stove and the compatible cookware, and these complete adopters show the largest and most persistent electricity gains. A social-planner model translates these experimental responses into welfare comparisons. Restricting subsidies to the complete stove-and-cookware bundle delivers roughly 38% higher welfare than a universal subsidy and 29% higher welfare than targeting high-LPG users, showing that what households adopt matters as much as whom the policy targets.

JEL Codes: C93, O1, O33, Q48, Q56

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1 Introduction

Governments routinely subsidize technology adoption to achieve social objectives, from distributing bednets and water filters to promoting improved agricultural inputs and cleaner energy technologies.[1] A large literature has studied barriers to adoption and how subsidies, information, and credit can overcome them (see, e.g., Bailey et al., 2025a; Berry et al., 2020; Devoto et al., 2012; Duflo et al., 2011; Dupas et al., 2023; Guiteras et al., 2015; Lipscomb and Schechter, 2018; Muehlegger and Rapson, 2022). Yet the social returns that justify public investment typically depend on sustained technology use, which can require household-level complementary investments that are financial or behavioral: purchasing compatible inputs, acquiring and using new skills, or adapting established routines. For example, an electric vehicle may require home charging stations and adjustments to driving patterns, while electric cooking requires compatible cookware and new preparation techniques. Such complementary investments often take time and their returns may be uncertain to households.

Although the challenge to induce complementary investments is broadly applicable, the stakes are especially high when policy objectives are defined at the aggregate rather than individual level.[2] Energy transitions are such instances. Governments pursue electrification of end uses to achieve multiple objectives, including strengthening energy security by reducing dependence on imported fuels and boosting demand for domestically-produced electricity, while replacing dirty fuels with cleaner ones. Achieving these goals at scale requires not only upstream investments in generation capacity and infrastructure, but also overcoming the standard barriers to household technology adoption and co-investments while also ensuring decentralized adoption decisions sufficiently translate into sustained aggregate changes in demand. The complementary investment problem thus becomes a bottleneck for achieving the broader policy objective, not just household welfare.

We study this challenge through a ubiquitous household activity with significant po-

[1] Public investment is common in sectors where there are private and aggregate returns: Water, sanitation, and health technologies reduce individual exposure as well as community transmission of disease (Deutschmann et al., 2024; Dupas, 2014; Dupas et al., 2023; Geruso and Spears, 2018; Kremer et al., 2011). Agricultural training, fertilizer, and irrigation raise farm productivity and regional food security (Duflo and Pande, 2007; Duflo et al., 2008). Electric vehicles lower private operating costs while reducing local pollution and greenhouse gas emissions (Holland et al., 2016; Rapson and Muehlegger, 2023).

[2] Some examples: macro-level technology diffusion (mobile money, electronic payments), labor-market transitions (skill retraining during industrial shifts). These are all settings where the social return depends on scale.

tential for transformation: cooking. We focus on the transition from liquefied petroleum gas (LPG) to electric cooking in Nepal, where recent investments in domestic hydropower generation and improvements in reliability (Acharya et al., 2024; Mahadevan et al., 2026) eliminated the most commonly cited supply-side barriers and made electricity cleaner and cheaper than imported LPG.[3] Yet despite past government efforts to promote electric cooking, LPG remains the dominant urban cooking fuel (Acharya and Adhikari, 2021).[4]

We design a randomized experiment to evaluate policies available to a government planner in order to affect adoption and post-adoption usage. The randomized controlled trial was implemented in Kathmandu Valley with 3,521 LPG-using households, of whom 2,894 consented and were randomized. Our intervention combines two distinct but complementary policy tools that target different margins of behavior. Subsidies are designed to relax upfront liquidity constraints and shift adoption (i.e., whether households acquire an electric stove and compatible cookware). In contrast, information treatments are designed to address potential misperceptions regarding the operating costs of, and health benefits from, electric cooking, which may influence how households incorporate electric cooking into their daily routines. While both interventions could, in principle, affect adoption decisions, they are expected to operate through different primary channels. Subsidies directly reduce the cost of acquiring the technology, while information primarily affects the perceived returns to using it. If sustained use of the technology requires complementary investments and behavioral adjustment, then information interventions may be a particularly important tool for the planner.

Designed in partnership with the Nepal Electricity Authority (NEA), the national utility responsible for electricity generation, transmission, and distribution, the experiment randomly assigned households to one of nine groups. Households in those groups received low or high subsidy amounts (approximately USD 7.5 or 22.5, respectively), one of two information treatments (cost or health), the combination of both a subsidy and an information treatment, or nothing (the control group).[5] Subsidies were delivered as vouchers redeemable

[3]Prior evidence on improved fossil-fuel cookstoves suggests that their adoption is constrained by liquidity (Berkouwer and Dean, 2022), depends on targeting (Mobarak et al., 2012), and that usage may decline after adoption (Hanna et al., 2016a).

[4]For a broader discussion of electronic appliance adoption and use, see Meeks and Mahadevan (2025).

[5]The higher subsidy is an approximately 50% discount off the price of a single induction stove, and almost the full cost

at the country's largest, and most ubiquitous retail chain, embedding the experiment in the population and delivery channels readily available to a planner (List, 2022). We track outcomes using data from surveys, store purchase records, and 29 months of administrative billing records from NEA. After documenting the reduced-form adoption and usage responses, we develop a structural model that embeds household behavior within planner constraints, using the experimental variation to recover behavioral parameters and evaluate counterfactual subsidy designs.

The randomized experiment yields three main results. First, subsidies had large and economically meaningful effects on adoption, while information alone did not. Households offered the high subsidy were 54 percentage points more likely to adopt electric cooking (defined as acquiring either an induction stove or compatible cookware) relative to a control mean of 16 percent. The low subsidy increased adoption by 26 percentage points, though this effect primarily operated through cookware rather than stove purchases. Treatments that combined subsidy and information yielded similar adoption results to subsidies alone. In contrast, standalone cost and health information treatments generated negligible changes in adoption. These findings indicate that liquidity constraints were the primary barrier to adoption in this context, rather than information alone.

Second, using follow-up survey data, we examined short-run cooking behavior. Subsidy recipients shifted meaningfully toward electric cooking, reallocating 16 to 21 minutes of daily cooking time toward electricity. LPG use declined modestly but remained substantial, consistent with fuel-stacking behavior documented in prior work (Guta et al., 2022; Masera et al., 2000; Shankar et al., 2020). Information treatments alone did not significantly alter cooking behavior. Thus, while adoption translated into real changes in household fuel use, substitution was partial rather than complete.

Third, we studied how adoption translated into longer-run electricity consumption using detailed administrative billing data extending 15 months beyond the end of the intervention.

of compatible cookware. The cost information explained that, with the existing tariff, electric cooking is cheaper than using LPG, while the health information highlighted indoor air-pollution risks from LPG (Daouda et al., 2024; Lebel et al., 2022). If subsidies lower the barrier to acquiring the technology and information increases the perceived returns to using it, then combined treatments may be uniquely positioned to induce the complementary investments needed for sustained fuel switching. Correcting misperceptions about operating costs may boost initial adoption, but more importantly, may affect longer-term usage, especially among price-sensitive households (Allcott and Taubinsky, 2015; Bensch and Peters, 2015).

In contrast to the short-run adoption results, patterns of usage differed in meaningful ways based on whether households also received information treatments. The clearest and most durable consumption gains appeared in the High Subsidy & Cost Info, where billed electricity rose by roughly 9% by May 2025 (the end of our billing panel). Effects in the other arms were smaller and not always statistically significant. Event study estimates showed that these usage effects ramped up over time rather than appearing immediately after the intervention.[6] This dynamic pattern indicates that adoption does not mechanically translate into persistent increases in electricity demand. A useful feature of this setting is that the complementary investment (induction stoves and compatible cookware) is observable in household purchase records, letting us distinguish complete from partial adoption directly. Treatment arms that produced more complete-bundle adopters also produced the largest long-run usage gains, an association that motivates the model-based policy analysis below.

Taken together, the results highlight an important distinction between the drivers of adoption and the drivers of sustained usage. While subsidies successfully induced technology uptake by relaxing up-front costs, they did not on their own generate sustained increases in electricity demand. Information, while ineffective at shifting adoption, played a critical role in shaping post-adoption use behavior. The largest and most durable usage gains appeared in the cost-information-plus-high-subsidy arm. For policymakers concerned with energy transitions, the relevant outcome is not merely whether households acquire new technologies, but whether they incorporate them into sustained patterns of electricity use and which types of interventions can help them to achieve this goal.

These reduced-form results motivate a structural model of adoption and post-adoption electricity usage that evaluates counterfactual subsidy designs. The model formalizes the empirical distinction between inducing adoption and inducing sustained electricity use. It estimates learning curves by adopter type: households purchasing the complete stove-and-cookware bundle increase electricity use over time, while households purchasing only one component revert toward baseline usage. These dynamics imply that treatment design matters not only because it changes how many households adopt, but because it changes what

[6] In rural South Africa, increased electricity usage lagged electric stove adoption by 3 years (Dinkelman et al., 2026).

type of adoption occurs. Embedding these behavioral responses in a social planner's problem, we find that restricting the subsidy to the complete stove-and-cookware bundle generates roughly 73% more welfare than the experimental design. Sophisticated machine-learning-based targeting offers only modest gains beyond simpler observable rules, such as targeting based on LPG purchase records.

This paper makes three main contributions. First, we contribute to a thin but growing literature on energy transitions. Energy transitions are inherently macro processes: fuel generation, grid infrastructure, and policy planning require governments and social planners. Yet, transitions ultimately depend on decentralized decisions to adopt and use new technologies, highlighting the importance of micro-founded demand parameters to inform macro-level planning. This is especially true for end-use electrification, where replacing fossil fuels with electricity depends not only on household demand, but also on grid capacity and the broader generation mix.[7] A growing literature studies these issues in the context of electric vehicles, including the role of subsidies, charging infrastructure, and the strain that widespread electrification can place on electricity systems (Bailey et al., 2025a,b; Gallagher and Muehlegger, 2011; Holland et al., 2016; Li, 2023; Li et al., 2017, 2022; Springel, 2021; Xing et al., 2021). We bring this end-use electrification perspective to household cooking, where the planner's problem is closely related. Our contribution is to combine experimentally identified adoption decisions with administrative usage data, then use those estimates to evaluate counterfactual subsidy policies under planner constraints, bridging macro-oriented work on energy transitions with micro evidence. This complements recent work such as Fried et al. (2025), which models household equipment choice in a policy context.

Second, we contribute to the emerging literature on electric cooking within the transitions literature. Previous studies have examined shifts from biomass to improved stoves or to LPG (Abubakari et al., 2024b; Afridi et al., 2021, 2023; Kar et al., 2024; Meeks et al., 2019; Pattanayak et al., 2019), but evidence on the demand for electric stoves (Pattanayak et al., 2019) and on the transition from LPG to electric cooking remains limited (Gould et al., 2024, 2023). We contribute in particular to the literature on fuel shifting, which is

[7] Existing work examines the evolution of household adoption of electricity-using assets (Gertler et al., 2016).

underpinned by the well-established practice of fuel stacking, whereby the extent to which households shift from using an old fuel to a new one is critical for characterizing cooking transitions (Kar et al., 2024; Shankar et al., 2020). We extend this literature in two ways. First, by combining experimental variation with monthly electricity consumption data, we trace how usage evolves after adoption, documenting that increases in use emerge gradually and are not mechanically implied by take-up. Second, we observe complementary investments in stoves and cookware, moving beyond binary measures of adoption and capturing how households adopt cooking technology bundles in practice. Together, these results highlight the importance of understanding not only whether households adopt electric cooking, but also how they incorporate it into their cooking portfolio over time.

Finally, we contribute to the literature on the relationship between technology adoption, sustained use, and policy targeting. Prior work documents that technology adoption does not always translate into sustained use, due to post-adoption frictions such as maintenance failures, behavioral biases, or habit persistence (Ashraf et al., 2013; Cohen et al., 2015; Dupas, 2014; Dupas et al., 2024; Hanna et al., 2016b; Tarozzi et al., 2014). We complement this work by documenting a compositional dimension to the gap: treatment arms differ not only in how much adoption they generate but in what complementary investments households choose, and those who acquire both a stove and compatible cookware show the largest sustained usage gains in our data. The households most responsive to subsidies are not always those who translate adoption into long-run electricity use. The composition of adoption is endogenous to both the treatment and the household, so while we cannot interpret this association as a causal effect of complete adoption itself, the structural model in Sections 8 and 11 uses the association, under identification restrictions, to evaluate a policy that restricts subsidies to complete bundles and shows that such a design yields larger welfare gains than broader designs that maximize adoption rates.[8]

The paper proceeds as follows. Section 2 discusses the local context in Nepal. Section 3 describes the randomized experiment on electric cookstoves. Section 4 presents the data and

[8]A related literature studies the effectiveness of pricing and non-monetary costs in targeting households more likely to use a technology (Ashraf et al., 2013; Dupas et al., 2023). This also connects to a literature on social protection, including Abubakari et al. (2024a); Burlig and Sudarshan (2026); Hanna and Olken, eds (2026).

summary statistics. Section 5 outlines the empirical strategy, and Sections 6 and 7 report the reduced-form effects on adoption, cooking behavior, and billed electricity consumption. Section 8 develops the structural model of household adoption, post-adoption electricity usage, and the social planner's problem. Section 9 estimates the structural parameters, Section 10 decomposes the adoption-to-usage gap by adopter type, and Section 11 evaluates counterfactual subsidy designs. Section 12 concludes.

2 Background

2.1 Study setting: urban Nepal

The study was implemented in Bagmati Province, the country's economic center. Bagmati, which encompasses the Kathmandu Valley, is home to 6.1 million people and represents 21% of the country's total population (National Statistics Office, 2025). Although there are rural-urban differences in the cooking fuels used, LPG is the predominant fuel in Bagmati Province, where 62% of the population is urban (National Statistics Office, 2025); as of 2021, 70% cooked with LPG, 29% cooked with firewood, and less than 1% cooked with electricity (National Statistics Office, 2025).

Urban households' dependence on LPG resulted from the country's fuel subsidies and has caused concern in recent years. Petroleum imports account for 20% of Nepal's total imports and are greater than all of the country's exports (Koirala, 2023). Almost all LPG imports come from India; in 2019/20 alone, Nepal imported approximately 451,000 metric tons of LPG from India (Gautam, 2021). This reliance on imports, particularly those from India, induces both national security and financial stability concerns. A 2015 Indian blockade exposed the country's vulnerability to fuel supply disruptions and catalyzed a policy shift toward electrification (Acharya and Adhikari, 2021; Giri, 2015). As a result, in its recent planning exercise, the Government of Nepal indicated plans to promote electric cooking while gradually reducing the existing LPG subsidies (National Planning Commission, 2024).

2.2 Electricity sector in Nepal

Nepal's energy landscape makes it a natural laboratory for studying cooking transitions. The country's generation, transmission, and distribution of electricity are the responsibility of the Nepal Electricity Authority (NEA), which is a vertically-integrated government entity. Service delivery, billing, and collections are all tasks handled by NEA's Distribution Centers, which are smaller units that each cover electricity distribution for a portion of a province. There were 20 Distribution Centers in Bagmati Province.

Although Nepal made substantial gains over the past few decades, increasing electricity access rates from 14.1% in 1995/96 to 94% as of 2022/23 (National Statistics Office, 2025), poor electricity reliability and frequent load-shedding plagued electricity provision in Nepal until relatively recently (Timilsina and Steinbuks, 2021). In recent years, electricity reliability and quality of services have improved substantially (Sharma and Shrestha, 2023). Electricity generation has expanded, particularly since 2015/16 (National Statistics Office, 2025), greatly contributing to reliability improvements (Ghising, 2022; Kathmandu Post, 2023; Open Data Nepal, 2022). With hydropower supplying nearly 99% of electricity, resulting in minimal carbon emissions (Bhandari and Subedi, 2023), electric cooking is increasingly viable and environmentally friendly.

In an effort to encourage increased electricity consumption, including through electric cooking adoption, the government of Nepal had reduced electricity tariffs (Acharya et al., 2025) before this study. NEA's residential customers faced an increasing block price, which varied by both the amperage of the customers' connection and the kWh per month billed (NepalEnergyForum, 2023).[9] Our calculations show that, given the electricity tariff and the cost of an LPG cylinder in Nepal at the time of the study, electric cooking was cheaper for all residential consumers, irrespective of their baseline monthly electricity consumption (Appendix B). A non-inconsequential number of households in Kathmandu Valley owned electric stoves prior to our study, many of which were acquired during the earlier Indian LPG blockade. However, usage remained low, with one reported reason being a lack of compatible cookware.

[9]The full tariff schedule for households with single phase meters is in Appendix A.

3 Randomized Experiment on Electric Cookstoves

The following subsections detail key aspects of the randomized experiment: the sampling procedures employed, the experimental design used, and the intervention's components.

3.1 Sampling Process

In June and July 2023, the research team surveyed 3,521 households in the Kathmandu Valley. Our sample selection procedure comprises the two steps described below.

First, we start with 2,190 households that were previously surveyed by our research team in 2021. These households were all urban or peri-urban and situated within 1 km of the Bishnumati River, which runs north-south along the western side of Kathmandu, with approximately half the houses located to the western side of the river and the other half to the eastern side.

Second, in an effort to systematically extend the study area to include additional households that would be urban/peri-urban like the areas selected in the first step, but somewhat less densely concentrated, we used NEA data on transformers located within 2 km of the eastern side of the Bishnumati River.[10] We extended the study area north of the sample selected in the first step, up to the Tokha Municipality, as this remained within the Kathmandu Valley and was also proximate to locations of our collaborating partner store. There were 172 NEA transformers serving this area across 3 of NEA's Distribution Centers (Ratnapark, Maharajgunj, and Kuleshor). From the utility records of all households served by these 172 transformers, we randomly sampled 7 to 8 households per transformer, resulting in an additional 1,331 households selected in the second step. Maps of both the transformers and the partner store locations are in Appendix C.

From these two steps, we ended up with a 3,521 household sample. Of these households, 2,894 consented to participate, creating our study sample.

[10] We only had GPS location data for transformers on the eastern side of the river, as that was where smart meters were installed a few years prior to our study.

3.2 Experimental Design

The 2,894 households were randomly assigned by a computer to one of nine groups. The randomization forced balance on seven baseline household characteristics.[11]

Households could receive one of three electric cooking subsidy treatment levels: a high subsidy of 3,000 NPR (≈ USD 22.5), a low subsidy of 1,000 NPR (≈ USD 7.5), or no subsidy. Subsidies could be used to purchase an electric induction cookstove or the associated cookware (e.g., pots and pans) required to use an electric induction stove. The subsidies were intended to relax an adoption constraint, as the low and high subsidies offset a substantial share of the upfront costs required to purchase either an induction stove (which cost USD 30–45 for a one-burner stove and approximately double that for a two-burner stove) or the compatible cookware (which cost USD 10–22).

In addition to a subsidy component, households could be assigned to one of three information groups: those receiving either information on the operating costs of electric stoves (relative to LPG stoves), information on the health benefits from electric cooking, or no information. The information components were designed to influence how households perceive the costs and benefits of electric cooking.

The randomization assigned households to one of the following groups: a control group (those assigned to receive neither subsidy nor information) or one of eight treatment groups: High Subsidy, High Subsidy & Cost Info, High Subsidy & Health Info, Low Subsidy, Low Subsidy & Cost Info, Low Subsidy & Health Info, Health Info, and Cost Info. Household numbers assigned per group are depicted in Table 1, Panel A. The locations of households receiving the subsidy and the information treatments are mapped in Figures 1a and 1b, respectively.

[11] The baseline variables were collected via the 2023 household survey and include: household size, household head education (both if they have less than 1 year of schooling and if they have a bachelor's degree or more), if the house is connected via a smart meter, if the household already owns an induction cookstove, and an income proxy based on appliances (increasing in the number of appliances owned including microwave, AC and washing machine).

3.3 The Intervention

The intervention occurred in January and February 2024. The intervention proceeded as follows. All 2,894 households were visited by project staff during the two-month period. They were thanked for consenting to participate in the study and received a small mobile phone credit for their continued participation in the study.[12] The full script for this interaction is in Appendix D1. The interaction ended at that point for the control households. The households assigned to other groups then received their treatments.[13]

Households in the information treatment groups received a pamphlet with their respective messages regarding either the cost of or the health benefits from electric cooking. Pamphlet details are in Appendices D2 and D3.

Households receiving subsidy treatments received a paper explaining that they were randomly selected to receive a subsidy voucher for NPR 3000 (or NPR 1000) that could be used on either an induction stove, cookware compatible with an induction stove, or a combination of these eligible products. This document provided instructions for using the subsidy voucher at one of the partner's store locations.[14]

These households also received a physical voucher card with their household-specific subsidy amount listed. To use the subsidy, a household member had to bring the voucher to the customer service desk at one of the store locations within Kathmandu Valley. The subsidy could only be used towards induction cooking items approved by the research team and had to be redeemed by April 30, 2024. Additional details, including the pamphlets and the subsidy voucher, are in Appendix D4.

[12]The credit was for 100 Nepali Rupees, which was approximately 0.75 USD at the time.

[13]There were 78 households that were unreachable at this time, because they had moved or were not home during multiple visits by the research team. The breakdown of the unreachable households by treatment group is in Appendix Table D2.

[14]The partner was Nepal's largest chain store, with 27 stores throughout the country and 9 stores in Kathmandu Valley. The mega store is well known in the country and sells most household goods, from food to appliances, cookware, and furniture, as well as clothes.

4 Data

We use two main sources of data for this study: household survey data and administrative billing data, which are supplemented by store data. A timeline of the intervention relative to the data collection is in Figure 2. In the subsections below, we describe both data sources as well as our sample construction, and then perform baseline balance tests.

4.1 Individual Datasets

Survey Data. We collected two rounds of household survey data: a baseline and a follow-up. The surveys collected information on household characteristics, appliance and cookware ownership, and cooking practices, amongst other details. The baseline survey was conducted in June and July 2023, and the follow-up survey was conducted in June and July 2024. The follow-up survey was timed to be one year after the baseline survey and approximately 6 months after the onset of the intervention. Of the 2,894 households randomly assigned to treatment, 95% (2,740 households) were successfully re-surveyed at the follow-up.

Administrative Billing Data. The Nepal Electricity Authority (NEA) provided administrative billing data, which provide a measurement of households' monthly electricity consumption before and after the intervention. These data include information on the units consumed (kWh), amount due (Nepali Rupees, NPR), amount payable, and the date the bill was due. The billing data cover 29 months, from January 2023 to May 2025. Since the intervention took place in January and February 2024, the panel provides a full year of pre-intervention consumption and 15 months following the intervention's conclusion. Throughout the paper, we measure event time in months relative to December 2023, the last month before the intervention began: month 1 is January 2024 and month 17, the end of the panel, is May 2025. Event studies use December 2023 as the reference period. The difference-in-differences specifications use three calendar windows: a pre-period (January to December 2023), a short-run post-period (January to December 2024), and a long-run post-period (January to May 2025). This administrative panel provides a direct measure of electricity demand, enabling us to evaluate whether the experimental treatments led to durable shifts in household energy use.

4.2 Analytical Sample

Our analytical sample is constructed in several stages, which are summarized in Figure 3. We start with the 2,894 households that consented to participate in the study and were randomized into the 9 treatment groups. When limiting to households that completed the follow-up survey, the sample size falls to 2,740 households.

Over the course of the study, we learned that households with electric connections below 30 amperes were less compatible for induction stoves than those with 30 amperes or higher. Therefore, we limit our main analytical sample to households with electric connections rated at least 30 amperes; however, we also perform analyses on the full sample and report them in the appendices.[15] Among the 2,740 households re-surveyed at follow-up, 2,192 (80%) had a connection of at least 30 amperes. We limit our main analysis sample to these 2,192 households, but we also perform robustness checks for all results using the full sample of 2,740 households. Panel B in Table 1 presents the distribution of treatment assignments among the 2,192 households in our analytical sample.

Attrition. We assess sample attrition along two dimensions. Table E3 regresses an attrition indicator—equal to one for households that consented at baseline but were not interviewed at endline—on the full set of treatment-arm dummies, with the control group omitted; each coefficient therefore measures the difference in attrition relative to control. Table E4 reports a probit of the same indicator on baseline household characteristics (household size, education of the household head, smart-meter ownership, induction-stove ownership, and the number of electric cooking appliances). Attrition is not systematically related to treatment assignment: no arm differs from control at the 5% level, and the joint test of equal attrition across arms is only borderline ($p = 0.086$), with the two marginally significant arms moving in opposite directions. Attrition is, however, correlated with a few baseline characteristics—larger households are less likely to attrit, while households with a smart meter are somewhat more likely to do so—but because this selection is balanced across experimental arms, it does not threaten the internal validity of our treatment-effect estimates.

[15] At the time of the experiment, NEA guidance indicated that technically, a 15A connection was sufficient to operate an induction stove. In practice, however, households with 30A connections were more suited to reliable electric cooking when occurring alongside other appliances.

We also examine whether treatment assignment predicts exclusion from the final analysis sample after imposing the amperage restriction (Appendix Table E5). The only treatment arm that differs significantly from the control group is the Cost Information arm: households in this arm are about 5 percentage points less likely to be dropped from the final sample, significant at the 5% level.

Baseline Balance. Using baseline survey data, we check for balance across treatment groups in this 2,192 household sample using the variables on which balance was initially forced during randomization: household size, household head education (both if they have no formal education and if they have higher education), if the house has a smart meter installed, the number of electric cooking appliances owned, and whether the household owns an induction stove. Results from these balance tests show that none of the 8 treatment groups are statistically significantly different from the control group for our analytical sample (Appendix Table E6).

Baseline Descriptives. Baseline summary statistics provide a sense of electricity- and cooking-related behaviors prior to the intervention (Appendix Table E7). Approximately one-third of households pay for their electricity bills by cash/cheque, and the other two-thirds pay by credit. Respondents report, on average, that their prior month electricity bill was 2,197 Nepali rupees (∼USD 16 at the time). Although almost all households report being aware of and knowing their most recent electricity expenses (96%), only 54% report knowing what their monthly electricity consumption (in kWh) was. Approximately 85% of houses have a smart meter installed, which is indicative of the study's sampling strategy.

Reported electricity reliability was not perfect at baseline. There were reports of outages, although they varied by season. Respondents report just over one electricity outage per week, on average, in "normal months." In contrast, households report 5.4 outages per week, on average, in the "bad" months when electricity reliability substantially worsens.

Reports on appliances indicate that households had a substantial number of electric appliances at baseline. Most respondents have a refrigerator (89%), fan (83%), and kettle (64%). Approximately one-third of respondents report heating their houses in the winter, whereas almost no households (3%) report cooling with air conditioners.

Notably, 36% of the households report owning an induction stove at baseline, which is likely the result of both the government's prior efforts to encourage electric cooking, as well as household stove purchases during the 2015 Indian LPG blockade. Households spent a non-inconsequential amount of time on cooking at baseline: on average, approximately 2 hours per day (13.5 hours in the last week).

5 Empirical Strategy

In this section, we describe how we leverage the experimental variation in treatment assignment to estimate the effects of the intervention.

5.1 Intent-to-Treat

We estimate the effect of being randomly assigned to a particular treatment on outcomes related to the adoption and self-reported use of electric cooking. Because the groups are balanced at baseline, including balance on owning an induction stove, we estimate the impact of the intervention's treatment arms using the following simple specification:

$$\begin{aligned} Y_i = \alpha &+ \beta_1 \text{HighSubsidy}_i + \beta_2 \text{LowSubsidy}_i + \beta_3 \text{CostInfo}_i + \beta_4 \text{HealthInfo}_i \\ &+ \beta_5 (\text{HighSubsidy\&CostInfo}_i) + \beta_6 (\text{HighSubsidy\&HealthInfo}_i) \\ &+ \beta_7 (\text{LowSubsidy\&CostInfo}_i) + \beta_8 (\text{LowSubsidy\&HealthInfo}_i) + \varepsilon_i \end{aligned} \tag{1}$$

where, because households were randomly assigned into one of nine mutually exclusive treatment groups, HighSubsidy_i, LowSubsidy_i, CostInfo_i, HealthInfo_i, $\text{HighSubsidy\&CostInfo}_i$, $\text{HighSubsidy\&HealthInfo}_i$, $\text{LowSubsidy\&CostInfo}_i$, and $\text{LowSubsidy\&HealthInfo}_i$ are all indicator variables corresponding to the distinct experimental arms. The omitted category consists of households assigned to the control group, that is, those who received neither a subsidy nor an information treatment. The constant term α therefore represents the mean outcome for the control group.

Each coefficient β_k represents the difference in mean outcomes between households assigned to that specific treatment cell and households in the control group. For example, β_1

captures the effect of assignment to the high subsidy arm, β_3 captures the effect of assignment to the cost information arm, and β_5 captures the effect of assignment to the combined high subsidy and cost information arm. Because treatment cells are mutually exclusive, the coefficients do not represent incremental interaction effects in a factorial sense. Rather, they reflect the average treatment effect of each experimental arm relative to the pure control group. Finally, ε_i denotes the idiosyncratic error term.

5.2 Event Study Design

We estimate event study specifications with the electricity billing data, which allows us to both assess the validity of the parallel trends assumption and to examine the dynamic effects of the treatments over time. For each treatment arm $T \in \{1, \ldots, 8\}$, we estimate the following equation using the sample restricted to households in treatment group T and the control group:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k=-6, k\neq -1}^{17} \beta_k \times \mathbb{1}\{\text{Month}_t = k\} \times \mathbb{1}\{\text{Treatment}_i = T\} + \epsilon_{it} \tag{2}$$

where Y_{it} represents the log electricity consumption for household i in month t. The terms α_i and λ_t denote household and time fixed effects, respectively. The index k represents the month relative to the reference period, where $k = -1$ corresponds to December 2023 (the month prior to the start of the intervention). The summation creates a set of coefficients β_k for the pre-treatment period ($k \in \{-6, \ldots, -2\}$) and the post-treatment period ($k \in \{1, \ldots, 17\}$).

The coefficients β_k capture the differential change in consumption for the treated group relative to the control group in month k, compared to the base period of December 2023. We cluster standard errors at the household level.

The pre-treatment coefficients provide a direct test of the parallel trends assumption underlying the difference-in-differences framework. The estimates for $k < 0$ should be statistically indistinguishable from zero, indicating that treated and control households were on similar consumption trajectories prior to the intervention.

The post-treatment coefficients trace out the dynamic treatment effects over time. This allows us to examine whether electricity consumption responds immediately following treatment, whether effects grow gradually as households integrate electric cooking into daily routines, and whether any observed changes persist over the longer run. In this way, the event study complements the average post-treatment estimates by providing a flexible, non-parametric view of treatment dynamics.

5.3 Difference-in-differences Strategy

To estimate the effect of the treatments on monthly billed electricity consumption, we employ difference-in-differences specifications with a linear two-way fixed effects (TWFE) model. The estimation equation is defined as follows:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{T=1}^{8} \beta_T \left(Post_t \times \mathbb{1}\{\text{Treatment}_i = T\} \right) + \epsilon_{it} \tag{3}$$

where Y_{it} denotes the electricity consumption for household i at time t. As in the event study, α_i and λ_t represent household and time fixed effects, respectively. The household fixed effects absorb time-invariant heterogeneity across households, while the time fixed effects account for common shocks affecting all units simultaneously.

The interaction term captures the treatment effect. $Post_t$ is a binary variable equal to one from January 2024 onward covering the intervention and post-intervention months, and zero otherwise. The term $\mathbb{1}\{\text{Treatment}_i = T\}$ is an indicator function that equals one if household i was assigned to treatment arm T. The coefficients of interest are β_T, which estimate the differential impact of each treatment T relative to the control group during the post-treatment period. Finally, ϵ_{it} represents the error term. Standard errors are clustered at the household level to allow for serial correlation within households over time.

This approach provides several advantages in our setting. First, while random assignment ensures treated and control households are comparable in expectation, electricity consumption is noisy and highly seasonal. By differencing within household over time and netting out common time shocks, the two-way fixed effects model improves precision relative

to a purely cross-sectional post-treatment comparison.

Second, the administrative panel allows us to study shorter and longer-run impacts separately in addition to the dynamic effects estimated through the previously-described event studies. Adoption of a new cooking technology may not translate into immediate, stable changes in electricity demand. Usage may evolve gradually as households learn, adjust cooking practices, or spread use across household members. The panel structure lets us estimate whether treatment effects emerge slowly, persist, or fade out over an extended horizon, which is central to the policy question faced by the NEA.

6 Empirical Results: Adoption of Electric Cooking

Our empirical analyses distinguish clearly between adoption and usage outcomes. Accordingly, we first examine outcomes at the extensive margin, focusing on whether households adopt induction cookstoves and compatible cookware as a result of the intervention.

6.1 Main Adoption Results

Table 2 documents strong impacts of subsidies on the adoption of electric cooking. Adoption is defined based on whether the household reports purchasing an electric induction stove or the compatible cookware in the six months prior to the follow-up survey (regardless of whether they already owned an electric stove), as this covers the entire time period since the intervention.

Column 1 presents results for the estimated impact of the intervention on the adoption of any electric cooking device (either an induction stove or the compatible cookware). Households offered the high subsidy were 54 percentage points more likely to adopt, relative to the control group mean of 16 percent. The low subsidy also increased adoption by 26 percentage points. Both the information treatments had negligible effects on their own, and their combinations with subsidies produced results similar in magnitude to subsidies alone. These findings indicate that liquidity constraints, rather than misperceptions, are the primary barrier to adoption in this study context. We return in Section 10 to explore whether

information plays a role in shaping the *type* of adoption households undertake.

To understand how the different treatments impacted adoption, we assess adoption separately by the type of good acquired: compatible cookware (column 2) and induction stoves (column 3). We observe large effects in the adoption of induction-compatible cookware, with the high and low subsidies increasing cookware adoption by 44 and 24 percentage points (column 2), respectively, and again, the combinations of subsidies and information impact their adoption comparably to the subsidies alone. In contrast, the story differs for stoves. While the high subsidy raised stove ownership by 22 percentage points, neither the low subsidy nor its combinations with information had significant effects.

Column 4 examines the impacts of the intervention on complete adoption, defined as acquiring both an induction stove *and* compatible cookware during the study period.[16] The control mean for complete adoption is quite low, at about 2.8%. The effects of the treatments on complete adoption are smaller in magnitude but follow a similar logic. All high-subsidy arms (High Subsidy, High Subsidy & Cost Info, High Subsidy & Health Info) have statistically significant impacts on complete adoption. Interestingly, the coefficient on High Subsidy & Health Info is approximately double the magnitude of the coefficients on the other two (High Subsidy alone and High Subsidy & Cost Info). The low-subsidy-only arm does not have a significant effect; however, the low-subsidy-plus-information treatments (Low Subsidy & Cost Info and Low Subsidy & Health Info) do have small yet statistically significant effects.

6.2 Heterogeneity by Baseline Stove Ownership

We divide the study sample into two groups: those that already owned electric induction stoves at baseline versus those that did not, to understand if adoption patterns differed by these subsamples (Table E8). Among those households that did not previously own an induction stove (columns 1 - 4), the coefficients are similar to those in Table 2, although the magnitudes of the subsidy (and subsidy plus information) effects are slightly larger in the subsample that does not include those with baseline induction stove ownership.

[16]This definition of complete adoption refers only to new purchases of both a stove and cookware. It does not incorporate or account for whether the household owned an electric stove prior to the intervention. We address differences in baseline ownership within the heterogeneity results.

Ex ante, the effects on the subsample with baseline induction stove ownership are not obvious. We find the effects on overall adoption (column 5) are comparable to the main results. However, the subsidy (and subsidy plus information) treatments appear to predominantly induce adoption of induction-compatible cookware, rather than induction stoves, which is consistent with these households supplementing their existing induction arrangement with additional cookware. There are two notable departures for the two High subsidy & Info treatments. Both High Subsidy & Info treatments significantly increase the induction stove adoption (column 7), but only the High Subsidy & Health Info treatment significantly affects complete adoption (column 8), margins on which the High Subsidy alone had essentially no effect.

6.3 Robustness Checks

Full Sample Analyses. As a robustness check, these analyses are also performed on the full sample for both the main adoption regressions (Table E9) and the heterogeneity analyses (Table E10). The estimated effects are of very similar magnitudes to those in Tables 2 and E8 across all columns. The only difference of note is that the coefficient on Low Subsidy & Health Info becomes statistically significant in the full sample in both Table E9, as well as the subsample without baseline electric stoves in Table E8.

SUTVA. We present results supporting the claim that the stable unit treatment value assumption (SUTVA) holds. We test whether the different subsidy treatments (including the combination of subsidies and information treatments), which induced significant adoption among the treated households, had any effect on the control households' adoption of electric cooking. Table E11 presents regression results that show no evidence of statistically significant spillover effects.

7 Empirical Results: Use of Electric Cooking

From the perspective of the Government of Nepal, the ultimate policy question concerns whether electric cooking could help the country in making the energy transition. The key

question for this experiment, therefore, is whether the intervention translates into sustained changes in cooking practices and increases in electricity consumption.

In this section, we present intensive margin outcomes in two complementary ways. First, we examine short-run impacts on usage using self-reported measures collected in our endline survey. These measures allow us to assess substitution patterns between cooking fuels and the extent to which adoption translates into households' perceived behavioral change. Second, we perform multiple analyses utilizing the panel structure of the monthly billing dataset collected by NEA.

7.1 Time spent cooking

Table 3 presents reduced-form estimates of self-reported cooking behavior by treatment arm.[17] These are short-run impacts (approximately 3-4 months post-intervention), given that the intervention was completed in Spring 2024 and the endline survey data were collected that summer. Time-use measures tell a story similar to that of the effects on induction stove adoption. Electric cooking time (column 1) increases by approximately 21 minutes in the High Subsidy arm and 16 minutes in the Low Subsidy arm. The combination treatments providing the High Subsidy & Info led to comparable and statistically significant increases in electric cooking time. The impacts on time spent cooking with LPG are modest and imprecisely estimated (column 2).

Taken together, these results indicate that financial incentives translate into a meaningful but partial short-run transition toward electric cooking. Households increase electric cooking, yet LPG remains an important fuel, which is consistent with fuel-stacking behavior observed in many other settings.

7.2 Effects on billed electricity consumption

The cooking time results are supplemented with analyses utilizing the monthly billing data obtained from the Nepal Electricity Authority. Household electricity bills are an important

[17] Robustness checks using the full sample are presented in Table E12. The magnitudes of the coefficients remain stable across samples.

outcome measure for multiple reasons. They provide an objective measure that proxies for electric cooking and complements the subjective self-reported data on household cooking times, which can be subject to various biases. Additionally, given that the Government of Nepal's ultimate goal is to increase electricity consumption, these data provide a direct measure of the extent to which such an intervention helps achieve the policymakers' goals.

7.2.1 Event Studies

The event studies show the impacts of the intervention over time. Figure 4 presents event study estimates of the treatments' impacts on monthly log electricity consumption (December 2023 is the omitted month).[18] Vertical dotted lines designate the beginning and end of the intervention period, during which the households receiving any subsidy treatments could use their coupons at the mega-store's location towards purchasing induction stoves and compatible cookware. Several key findings emerge.

First, across all treatment arms, the pre-treatment coefficients are tightly centered around zero and statistically insignificant. There is no evidence of differential pre-trends or anticipatory effects prior to the intervention, lending support to the identifying assumptions underlying the difference-in-differences framework.

Second, several of the treatments lead to significant increases in electricity consumption during some months post-intervention. The High Subsidy & Cost Info treatment arm shows the largest and most durable increase in electricity consumption over time. Other treatments do lead to increased electricity consumption that is statistically significantly different in some months such as the High Subsidy & Health Info and the Low Subsidy & Cost Info treatment arms. Notably, households assigned to High Subsidy alone do not exhibit any increase in electricity consumption over the full post-treatment horizon.

Third, and importantly, treatment effects do not appear immediately following the intervention. Instead, consumption for all treatment groups remains level for the first few months after the intervention. Then, among the treatment arms that induce increases in

[18] Corresponding event studies with the full sample are presented in Figure E6. Also, event studies using the raw monthly data (i.e., not using moving averages) are in Figure E7.

electricity consumption, these increases occur gradually over time, indicating that households that do integrate electric cooking do so progressively over time, rather than instantaneously changing their cooking following the intervention.

Taken together, these results highlight a central contribution of the paper. The long-run consumption effects reveal that usage responses are not mechanically determined by adoption alone. For example, subsidy treatments generated very large increases in adoption, but we do not observe adoption translating into long-run usage. Our results show that adoption and sustained usage can diverge, and that long-run administrative consumption data provide a critical complement to short-run survey measures. In the context of energy transitions, what ultimately matters for utilities and policymakers is not merely technology uptake but persistent changes in electricity demand. The administrative panel allows us to observe precisely these longer-run adjustments.

7.2.2 Difference-in-differences results

We also use the panel data to conduct difference-in-differences analyses and estimate the aggregate treatment effects on log billed electricity consumption. Given that the event studies indicate a delay in the onset of effects on consumption, we analyze the short-run and long-run periods separately, with results presented in Table 4 columns 1 and 2, respectively.[19] Most effect sizes appear small and often statistically insignificant. The one exception is that the High Subsidy & Cost Info treatment arm induces a 9.3% increase in billed electricity consumption in the long-run (column 2), which is both statistically significant and meaningful in consumption terms.

We next turn to heterogeneity in usage patterns among households that adopted either an induction stove, compatible cookware, or both after the intervention. Table 5 reports the treatment effects on log electricity consumption in the short-run and long-run periods, disaggregated by adopter type.[20] While these results are not causal, they help us better

[19]Robustness checks using the full sample are in Table E13. Corresponding difference-in-differences results for the entire post-intervention time period together (i.e., not separating the post-period into short and long-run time periods) are in column 1 of both Tables E14 and E15, for the analytical and full samples, respectively.

[20]Robustness checks using the full sample are in Table E16. Corresponding difference-in-differences results for the entire post-intervention time period together are in columns 2-4 in Tables E14 and E15 for the analytical and full samples, respectively.

understand how adoption translates into a cooking change that begets an energy transition.

A few patterns emerge from these heterogeneity results. First, in the short-run, the increases in electricity consumption are primarily concentrated almost entirely among the "complete" adopters (column 3), with the Low Subsidy, Cost Info, Health Info, and High Subsidy & Cost Info treatment arms all showing statistically significant gains. Second, complete adopters are the only group that has significant increases in billed consumption in both time horizons. In the long-run, significant increases in billed consumption are observed across all three adopter types, although they are most consistent among the "complete" adopters (column 6). As in the short-run, the Cost Info and High Subsidy & Cost Info treatment arms continue to have both economically and statistically significant relationships. Third, the coefficients for several treatment arms strengthen or emerge over time. The complete adopters that had received the High Subsidy treatment have an insignificant short-run response to treatment, but in the longer run, this group displays a significant and not inconsequential increase in billed electricity consumption. Overall, the results suggest that full technology adoption is more strongly associated with persistent increases in electricity demand than partial adoption, suggesting that joint ownership of the stove and cookware may be important for translating adoption into sustained use.

These heterogeneity results provide a descriptive bridge between the reduced-form estimates and the model. They show that the relevant policy margin is not only whether households adopt, but whether they adopt the complete stove-and-cookware bundle that is more strongly associated with sustained electricity use.

7.3 Back-of-the-envelope Calculations

To assess whether the estimated increases in electricity bills are consistent with plausible changes in cooking behavior, we conduct back-of-the-envelope calculations to translate those estimates into implied minutes of electric cooking per day. Calculations are summarized here, with the full derivation in Appendix F.

Multiplying the long-run High Subsidy & Cost Info coefficient (0.093, for all households

in this arm) with the control mean of 206.79 kWh per month implies an additional 19.2 kWh per household per month, while the corresponding number for the subsample of complete adopters (coefficient 0.149) is 30.8 kWh per month. At a typical single-burner induction draw of 1.6 to 2.0 kW, these numbers translate to between 19 to 24 additional minutes of electric cooking per day for the full sample and 31 to 39 minutes for complete adopters.[21] These calculations assume households are using single-burner stoves; however, it is plausible that they are using double-burner stoves (they were sold on the market at the time of the experiment), which would halve the cooking times calculated here. The magnitudes of these back-of-the-envelope calculations are economically reasonable and consistent with the self-reported electric cooking-time results in Table 3.

8 Model

We develop a tractable structural model to discipline our policy analysis and connect experimental variation to behavioral parameters. The model captures the frictions households face in adopting and using electric cooking technology: upfront liquidity constraints, misperceptions about relative operating costs, and heterogeneity in preferences. Importantly, the model also allows adoption to generate broader changes in electricity use over time through learning and experience. In this setting, technology adoption is not merely a discrete purchase decision, but a path into sustained electrification.

At the same time, the framework incorporates the constraints faced by a social planner. While higher electricity consumption is socially desirable because it reduces dependence on imported LPG and improves health, additional load is not costless when generation or distribution capacity is tight. The planner, therefore, weighs household welfare gains against the marginal cost of serving additional electricity demand under capacity constraints, as well as the fiscal cost of subsidies.

The model generates empirically estimable equations that allow us to map structural parameters to reduced form treatment effects observed in the RCT. We preserve a clean

[21] Allowing for the stoves to cycle at half their rated draw approximately doubles these times.

adoption equation that directly corresponds to our experimental variation. We then extend the framework to connect adoption to long-run billed electricity consumption, which is the key outcome of interest for utilities and policymakers.

Although we calibrate the model to Nepal, the structure is intentionally simple and portable. The household adoption margin can be estimated wherever quasi-experimental variation in incentives is available, while the planner's shadow values for fiscal resources, capacity-constrained supply costs, and LPG dependence can be reparameterized for other contexts. In this way, the framework provides a general tool for identifying the optimal mix of subsidies and information policies in energy transitions.

8.1 Adoption

Households choose between cooking with LPG or electricity based on perceived costs, subsidies, and idiosyncratic preferences. Subsidies directly reduce the upfront cost of adopting electric cooking technologies, while information treatments primarily shift perceived operating costs and may affect both adoption decisions and post-adoption usage. The key economic forces are: (i) lower perceived electricity costs raise adoption and shape usage decisions conditional on adoption; (ii) subsidies enter concavely, so each additional dollar has a diminishing effect; and (iii) subsidies are more effective when perceived electricity costs are high. This third force, the interaction between subsidies and perceived cost, is quantitatively important.

Taking log-odds of the adoption probability and substituting for indirect utilities (see Appendix G for derivation) yields:

$$\log \frac{\pi_{Eg}}{1-\pi_{Eg}} = \underbrace{-\frac{\eta\theta}{1-\eta}\log P_{Eg}}_{\text{perceived cost}} + \underbrace{\theta\, s_{Eg}^{\rho}}_{\text{subsidy}} + \underbrace{\theta\gamma_P\left(s_{Eg}^{\rho} P_{Eg}\right)}_{\text{subsidy}\times\text{cost interaction}}, \tag{4}$$

where π_{Eg} is the probability of adopting electric cooking, P_{Eg} is perceived electricity cost (shifted downward by information treatments), s_{Eg} is the subsidy normalized by stove price, ρ governs subsidy concavity, η is the price sensitivity of cooking demand, and θ scales taste

heterogeneity. If $\gamma_P > 0$, subsidies work best for households who perceive electricity as expensive, precisely the group where a subsidy relaxes the binding constraint.

Four treatment arms from the RCT (control, info-only, high subsidy, high subsidy + info) exactly identify the four parameters $\{\eta, \theta, \rho, \gamma_P\}$, with the remaining two arms (low subsidy, low subsidy + info) serving as out-of-sample validation. The model reproduces observed adoption across the six arms to within five percentage points, with out-of-sample error on the two held-out arms below three percentage points (Figure H2; Table 6).

8.2 Electricity usage conditional on adoption

Adopting a stove does not guarantee a change in cooking behavior. The model decomposes post-adoption electricity usage into channels that each imply a different policy response. Among adopters, optimal cooking time is $T_{it}^* = (\eta/\tilde{P}_{it})^{1/(1-\eta)}$, where the effective perceived cost $\tilde{P}_{it}$ depends on:

$$\log \tilde{P}_{it} = \underbrace{\delta_r R_i}_{\text{reliability}} - \underbrace{\delta_L \, g(t)}_{\text{learning}} + X_i'\beta + \nu_i, \tag{5}$$

where R_i is baseline beliefs about grid reliability, $g(t) = 1 - e^{-\kappa t}$ is a learning curve that rises from zero toward one as households gain experience with electric cooking, X_i includes household characteristics, and ν_i is a residual preference term. Billed electricity captures total household consumption, not just cooking, so the learning channel may also reflect broader changes in electricity use patterns.

Each channel maps to a different policy lever. *Reliability* (δ_r): if outage beliefs raise the perceived cost of committing to electric cooking, grid investment helps. *Learning* (δ_L): the sign reveals whether households integrate electric cooking into their routines ($\delta_L > 0$, growing usage with experience) or revert after initial experimentation ($\delta_L < 0$); follow-up support, such as cooking demonstrations, could sustain households in the latter group. *Preference heterogeneity* (ν_i): a household-level residual capturing intrinsic cooking cost after accounting for the first two channels and observables. Households with high ν_i face high perceived cost and have low electric cooking demand; those with low ν_i have strong revealed preference for electric cooking. Identifying high-ν_i households helps avoid subsidizing stoves that will not

be used.

8.3 Targeting

A social planner chooses subsidies s_i and information assignments I_i to maximize aggregate household welfare net of grid congestion, the social cost of continued LPG reliance, and the fiscal burden of subsidies:

$$\max_{\{s_i, I_i\}} \sum_i U_i(s_i, I_i) - \delta \sum_i \pi_{Ei} \cdot \mathbb{E}[kWh_i \mid \text{adopt}] - \lambda \sum_i (1 - \pi_{Ei}) - \sum_i \left(s_i + \psi s_i^2\right), \quad (6)$$

$$\text{s.t.} \quad \sum_i s_i + c_{\text{info}} I_i \leq B,$$

where U_i is household i's indirect utility (derived in Appendix G), $\pi_{Ei}(s_i, I_i)$ is the adoption probability from equation (4), δ is the marginal social cost of additional cooking-related electricity demand under capacity constraints, λ is the social cost per non-adopter capturing foregone LPG displacement and energy-security gains, and ψ is a convexity penalty on subsidy rates that deters concentrating large per-household transfers. Calibration values for $\{\delta, \lambda, \psi\}$ and a sensitivity analysis showing that the policy ranking is robust to wide variation are in Appendix Tables G2 and G3; the closed-form expanded objective and the first-order condition with respect to s_i are derived in Appendix G.

The formulation highlights the planner's tradeoff: raising subsidies or shifting perceived prices boosts household welfare through higher adoption, but it also raises grid costs and fiscal burden. The RCT provides the variation needed to estimate the behavioral parameters in (4) and (5), which then discipline the counterfactual simulations in Section 11.

9 Estimation of Structural Parameters

We estimate the model using the reduced-form patterns documented in Sections 6 and 7: subsidies sharply increase adoption, information alone does not, and the High Subsidy & Cost Info arm produces the largest long-run electricity gains. The adoption equation is identified from variation across RCT treatment arms while the usage equation is estimated on

administrative billing data for adopters. Full identification arguments and estimation procedures are in Appendix H; here we summarize the approach and interpret the key estimates (Table 6).

The structural model pools the cost and health information treatments because this step is designed to recover the adoption and usage primitives used in the counterfactual policy simulations, rather than to separately estimate each informational mechanism. The reduced-form analysis continues to report treatment-arm-specific results, including the large long-run gains in the High Subsidy & Cost Info arm. In the structural model, separating every information-by-subsidy cell would leave small adopter-type cells once we condition on bundle choice and monthly billing histories. We therefore estimate a single information shift in the adoption equation and use the randomized subsidy variation to identify the adoption response that enters the counterfactuals.

9.1 Adoption

The adoption equation (4) has four parameters: ρ (subsidy concavity), θ (taste heterogeneity), η (price sensitivity of cooking demand), and γ_P (the subsidy–cost interaction). We identify them from four treatment arms through a system of moment conditions. The control group pins down baseline adoption. The Info arm, which shifts perceived electricity cost without offering a subsidy, identifies the price sensitivity η. The High Subsidy and High Subsidy & Info arms jointly identify θ and γ_P from the difference in adoption responses with and without information. In this step, the information arm pools the cost and health information treatments, and the High Subsidy & Info arm pools the corresponding cost-information and health-information high-subsidy arms. Finally, a grid search over ρ selects the concavity that minimizes out-of-sample prediction error on the two held-out arms (Low Subsidy and Low Subsidy & Info).

Three features of the estimates are worth highlighting. First, subsidies enter concavely ($\hat{\rho} = 0.55$): the first NPR 1,000 of discount matters substantially more than the next 2,000, so partial discounts fall on the flat portion of the response curve while full subsidies push adoption decisively above the control rate. Whether concentration or breadth maximizes

welfare depends on how the planner weights adoption against post-adoption usage, which we evaluate in Section 11. Second, the subsidy–cost interaction is bounded at zero ($\hat{\gamma}_P = 0$) in our amperage-restricted sample: adoption in the High Subsidy arm is statistically indistinguishable from adoption in the High Subsidy & Info arm, so the data do not support a structural complementarity between the subsidy lever and the information lever. In this setting, information shifts perceived cost, and subsidies relax the upfront-cost constraint, without a detectable interaction between them. Third, the model captures the broad pattern of adoption across treatment arms, with maximum out-of-sample prediction error of approximately three percentage points on the two held-out arms (Figure H2).

9.2 Post-adoption electricity usage

We estimate the usage equation (5) on 11,606 adopter-month observations from NEA billing records, covering 694 adopters across all nine treatment arms over event months 0 to 17 (December 2023 to May 2025). The estimation uses cluster fixed effects (169 clusters) and standard errors clustered at the household level. For the learning curve, we perform a grid search over $\kappa \in [0.01, 2.0]$ and select the rate that minimizes the sum of squared residuals.

The vector X_i includes nine baseline characteristics: household size, head age, education, home ownership, LPG consumption (kg/month), cooking time, number of electric appliances, meals cooked on LPG, and baseline stove ownership. These controls serve a specific purpose in interpreting the preference residual $\hat{\nu}_i$. By conditioning on household size and head age, the residual does not simply reflect larger families cooking more. Controlling for LPG consumption and meals on LPG rules out that heavy baseline cooks mechanically use more electricity. Number of appliances and home ownership absorb wealth differences in baseline electricity demand. Baseline stove ownership accounts for prior familiarity with induction cooking. Cooking time nets out general cooking intensity, and education controls for openness to new technology. What remains in $\hat{\nu}_i$ after these controls is genuine taste, or taste-adjacent unobservables, for electric cooking.

Two results stand out from the pooled estimation. First, baseline reliability beliefs

have no detectable effect on post-adoption electricity usage ($\hat{\delta}_r = +0.006$, not statistically significant). This is a substantively important null: it means that households that express greater concern about power outages do not use less electricity after adopting an electric stove. The gap between households that adopt and increase electricity usage versus those that do not is driven by preferences and learning, not by grid concerns. This suggests that grid reliability investments, while valuable for other reasons, would not by themselves close the adoption-to-usage gap.

Second, the pooled learning coefficient is moderately negative ($\hat{\delta}_L = -0.109$), suggesting that, on average, adopters' electricity usage drifts downward over time. But this pooled estimate masks sharply different dynamics by adopter type, which turn out to be central to understanding why some treatment arms produce persistent electricity gains while others do not. We use the by-type estimates below to connect adopter composition to sustained electricity demand.

9.3 Treatment effect heterogeneity

The pooled estimates recover average parameters, but the γ_X term in the adoption equation allows household characteristics to moderate subsidy effectiveness. A planner needs to know which households respond most on the adoption margin and, separately, which translate adoption into sustained electricity usage. These need not be the same households. We estimate both dimensions using machine learning methods, with details in Appendix I.

For adoption, we estimate a causal forest (Athey et al., 2019; Wager and Athey, 2018) that leverages RCT randomization to recover each household's conditional average treatment effect $\hat{\tau}_i$.[22] A best linear projection of the forest onto standardized covariates yields the γ_X coefficients used in the counterfactual targeting exercise (Section 11). For usage, a separate regression forest predicts each household's kWh growth $\hat{y}_i$ from baseline characteristics. This second estimate is predictive, not causal, since there is no experimental variation in usage

[22] We use the `grf` package in R (Athey et al., 2019), estimating the causal forest with 4,000 trees on $N = 1{,}299$ households in subsidy and control arms. The CATE exercise uses subsidy and control arms because it estimates which households respond most to the subsidy. Info-only arms are excluded from this step because they shift perceived cost but do not provide a subsidy. The regression forest for electricity usage uses the same package and settings, trained on 461 subsidy-arm adopters with billing data in months 0–2 and 12–14. See Appendix I for details.

intensity conditional on adoption. Splitting at the median of each dimension yields four household types.

The central finding is that these two margins, predicted adoption response ($\hat{\tau}_i$) and predicted kWh growth ($\hat{y}_i$), are only weakly correlated. The correlation between $\hat{\tau}_i$ and $\hat{y}_i$ is only 0.2, statistically distinguishable from zero but explaining only about 4% of the variation: household characteristics predicting strong adoption responses are largely distinct from those predicting sustained electricity gains (Appendix Table I7). A program that looks successful by take-up may look quite different in terms of long-run electricity demand.

Figures 6 and 7 characterize the four cells. "Fuel switchers" (High Adopt, High Use) respond to subsidies and increase consumption (+0.31 log points from Table I6). They have the lowest prior stove ownership of the four cells (28.7%) and the fewest baseline reliability concerns (Figure 7). Further, 67% of their adopters cook with electricity for the first time, the highest share across cells (Appendix Table I6). At the other extreme, Low Adopt, Low Use households have high prior stove ownership (44.4%) and negative kWh growth, consistent with equipment replacement. The off-diagonal cells underscore the independence of the two margins: "equipment replacers" (High Adopt, Low Use) are subsidy-responsive but do not change cooking behavior, while Low Adopt, High Use households would increase usage if induced to adopt.

Note that "High/Low Adopt" refers to predicted *marginal response* to a subsidy ($\hat{\tau}_i$), not adoption level. A household with high baseline adoption propensity may respond little on the margin, and vice versa. Realized adoption rates in Table I6 therefore do not monotonically follow the labels (see Appendix I).

These results carry a clear lesson: fuel switchers offer the highest return per subsidy dollar for genuine fuel switching, because they both respond to incentives and change behavior. Equipment replacers respond but do not increase usage, so targeting on take-up alone overstates program impact. The weak correlation between the predictors of adoption vs usage means that evaluating subsidy programs solely on adoption can be misleading. We return to these tradeoffs in Sections 10 and 11, where the γ_X coefficients estimated here become central inputs.

10 Decomposition and Mechanisms

The reduced-form results show that treatment arms differ not only in how many households adopt, but in what households adopt. Households purchasing the complete stove-and-cookware bundle follow a fundamentally different trajectory than those purchasing only one component, and treatment arms differ sharply in the composition of types they produce.[23]

10.1 Treatment arms induce different adopter types

Treatment arms differ not only in how many households adopt but in the *kind* of adoption they generate (Table 7). The High Subsidy & Info arm produces the highest rate of complete adoption: 19.6% of households purchase both stove and cookware, versus 11.8% for subsidy-only and 3.2% for info-only. Among all adopters, nearly a third (31%) chose the complete bundle in the High Subsidy & Info arm versus roughly a fifth (21.1%) for subsidy-only. Information combined with a generous subsidy does not merely push more households over the adoption threshold; it shifts the margin toward a qualitatively different form of adoption.

This matters because usage responses diverge by type. Most complete adopters increase electricity usage after adoption, while most cookware-only adopters decrease it (Figure 5), consistent with experimentation that does not persist. Arms producing more complete adopters generate larger gains not because they produce more adopters, but because those adopters behave differently.

10.2 Learning curves differ by adopter type

The pooled estimates in Section 9 show a moderate downward drift in usage ($\hat{\delta}_L = -0.109$). Disaggregating by type reveals sharply different trajectories (Table 6, Panel C; Figure 9). Complete adopters show the largest positive learning trajectory ($\hat{\delta}_L = +0.085$), gradually integrating electric cooking over a slow adjustment ($\hat{\kappa} = 0.11$) still unfolding at month

[23]The analyses in this section use all 694 adopters across all nine treatment arms, corresponding to 11,606 adopter-months of billing data. The CATE analysis in Section 9.3 uses a restricted sample of 1,299 households in subsidy and control arms. Appendix Figure G1 summarizes how the model samples relate to one another.

14, the end of the decomposition's long-run window. Cookware-only adopters show rapid reversion ($\hat{\delta}_L = -0.165$, $\hat{\kappa} = 1.19$), consistent with trying new cookware and falling back on LPG. Stove-only adopters show no detectable trend. Appendix Table H5 reports these estimates with household-level cluster bootstrap standard errors that account for uncertainty in $\hat{\kappa}$: the cookware-only reversion remains significant at the 1% level, while the complete-adopter trajectory retains its sign and magnitude with wider confidence bands in the smaller sample.

10.3 Connecting household types to learning trajectories

The CATE analysis identified fuel switchers as the highest-return type. The composition results explain why: 39% purchase the full bundle versus 18% among equipment replacers (Figure 8). Fuel switchers also have the lowest prior stove ownership (29% vs. 44%) and 67% of their adopters cook with electricity for the first time (Appendix Table I6). Because complete adopters are the only type with a positive estimated learning trajectory, this composition difference accounts for their sustained electricity gains.

Decomposing predicted usage confirms that composition is the dominant channel. Reliability beliefs contribute nothing ($\hat{\delta}_r \approx 0$, not significant). The preference residual $\hat{\nu}_i$ varies modestly across types. What separates fuel switchers from equipment replacers is not unobserved preferences but the type of adoption and the learning trajectory that follows.

The evidence supports a chain from treatment design to long-run impact that runs through composition. Information combined with high subsidies shifts adopter composition toward the complete bundle, and complete adopters show the largest (structurally estimated) positive trajectory among adopter types. The composition step is RCT-identified at the arm level, while the composition-to-usage step is associational at the household level, since households choose what to buy. The counterfactual simulations in the next section use both steps, under the model's identification restrictions, to evaluate policies that work directly on the composition margin.

11 Counterfactual Policy Simulations

The decomposition documents a chain from treatment design to long-run usage that runs through adopter composition. We now ask whether a planner who acts on this chain can allocate the same budget more effectively. We explore this through counterfactual simulations varying the subsidy level, targeting rule, and intervention form, holding spending fixed at the RCT budget ($B = 2{,}180{,}000$ NPR). All counterfactuals bundle cost information with a subsidy. The planner chooses a target set $N_t \subseteq N$ and subsidy level $s \in [0, 1]$, optimizing the welfare function in equation (6). Adoption heterogeneity enters through the γ_X coefficients from the CATE best linear projection (Section 9.3).[24] The expected usage conditional on adoption, $\mathbb{E}[kWh_i \mid \text{adopt}]$, is evaluated at a 24-month horizon using the type-specific learning curves from Panel C of Table 6, weighted by the adopter-type composition each policy induces. This means the planner values a complete adopter (who integrates electric cooking) more than a cookware-only adopter (who reverts toward baseline). Figure 10 reports welfare gains from eight policies, normalized to the RCT.

The counterfactuals combine three inputs from the preceding analysis: adoption parameters estimated from randomized treatment-arm variation, type-specific usage trajectories estimated from adopters' billing histories, and household rankings from the CATE exercise.

Concentration dominates partial discounts: The budget funds 311 free bundles or a 14% discount for all 2,192 households. The blanket discount achieves 1.25× the RCT welfare. Giving free stove-plus-cookware bundles to 311 randomly chosen households achieves 1.73×. Concavity ($\hat{\rho} = 0.55$) means partial discounts fall on the flat part of the response curve while full subsidies push adoption above 85%. The RCT performed well because it already concentrated high subsidies on a subset of arms.

Observable targeting rules: We consider four rules a government could implement without extensive baseline surveys or machine learning: targeting households without a prior electric stove (1.18×), those reporting high grid reliability (1.10×), households with a bach-

[24] The adoption equation implies a θ multiplier on γ_X, but since θ is a scalar it does not affect household rankings; we absorb it into γ_X.

elor's degree or higher (1.24×), and top LPG users from administrative records (1.34×). All four rules outperform the RCT. Top LPG users perform best, since the welfare function rewards adoption that displaces existing LPG, and these households have the most LPG to displace.

CATE targeting: CATE-ranked selection of the 1,000 most responsive households achieves 1.41×. Targeting top LPG users performs nearly as well (1.34×). But CATE targeting requires a baseline survey and machine learning infrastructure, while LPG targeting uses existing administrative data. The incremental gain may not justify the cost.

Bundle restriction: Restricting the subsidy to the full stove-plus-cookware bundle generates the largest welfare gain among the policies we consider. At a unit cost of 7,000 NPR, the budget funds free bundles for 311 randomly chosen households. Every bundle adopter is, by construction, a complete adopter, the adopter type with the largest positive learning trajectory (Panel C of Table 6). The free bundle lottery achieves 1.73× the RCT welfare. The lesson is that the form of the subsidy matters as much as its size or targeting: requiring complete adoption channels the budget toward adopters with the largest realized usage growth.

12 Conclusion

Electrification of household end uses is increasingly central to strategies for reducing carbon dioxide emissions, improving indoor air quality, and enhancing energy security. In contexts where households rely on imported gas for cooking, shifting to electricity offers the dual benefits of cutting CO_2 emissions and mitigating health risks, while also reducing vulnerability to supply shocks. Nepal provides a particularly compelling case: nearly all of its electricity is generated from renewable hydropower, and recent investments have alleviated the chronic load-shedding that once constrained adoption. As the government seeks both to expand electricity consumption and to reduce dependence on imported LPG, the key policy question becomes how best to spur adoption and sustained use of electric cooking.

Using a large-scale randomized controlled trial, we show that the binding constraint

for adoption is upfront cost, consistent with a broad literature on technology subsidies. A 50% subsidy increases adoption of electric cooking equipment by 54 percentage points, while information treatments alone have no effect. Second, adoption and sustained use are not automatically linked. The households most responsive to subsidies are not the same ones that translate adoption into lasting increases in electricity demand. Among the households we observe, those that purchase the complete stove-and-cookware bundle show the largest positive usage trajectory. Treatment arms that combine information with subsidies produce disproportionately more of these complete adopters, which generates larger sustained electricity gains. Third, restricting subsidies to the full stove-and-cookware bundle, rather than the stove alone, dominates spreading thin discounts widely, raising welfare by 73% over the experimental design.

These findings carry a concrete policy implication. Nepal's government aims for 25% of households to adopt electric cooking by 2030. Our estimates suggest this goal is achievable with concentrated subsidies at modest fiscal cost, but the largest gains come from programs that favor complete adoption rather than piecemeal equipment purchases. A blanket subsidy program generates only modest welfare gains compared to one that requires the full stove-and-cookware bundle.

There are several avenues for future work to explore. Our billing panel ends 15 months after the intervention concluded and cannot determine whether the positive learning trajectory of complete adopters eventually levels off or continues to grow. The welfare simulations hold macro parameters fixed, while in practice, large-scale adoption could strain grid capacity. Finally, our sample is urban and peri-urban, and bounds the extent to which we can generalize these findings to rural areas.

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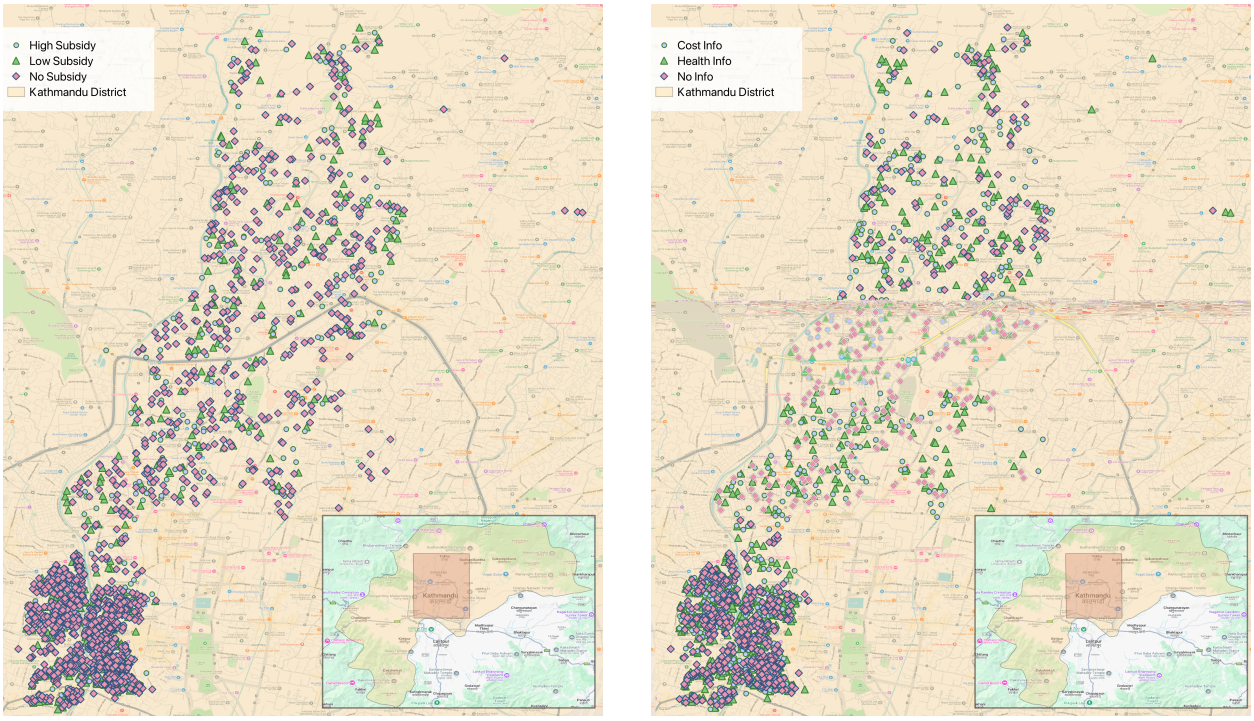
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Figures and Tables

Figures

Figure 1: Spatial Spread of Households by Treatment Group



(a) Subsidy treatment groups **(b)** Information treatment groups

Notes: The Kathmandu district boundary data are from 2015 and were obtained from Code for Nepal.

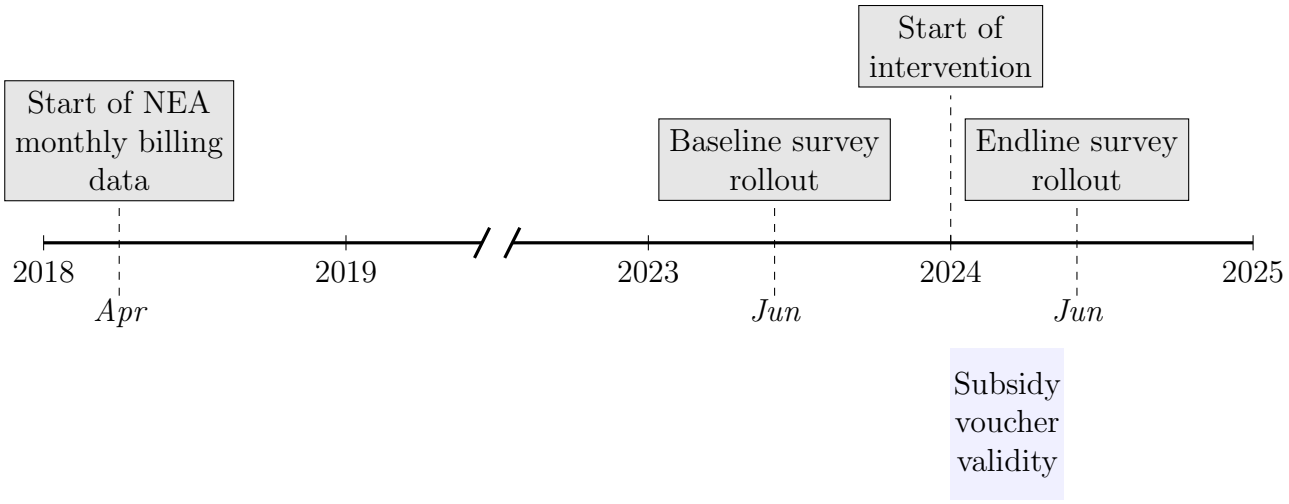


Figure 2: Timeline of surveys and key activities

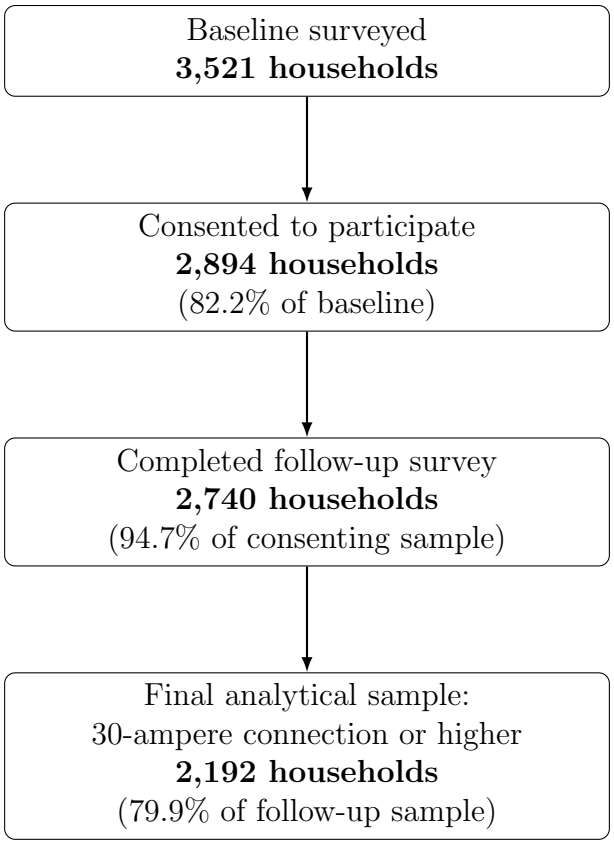
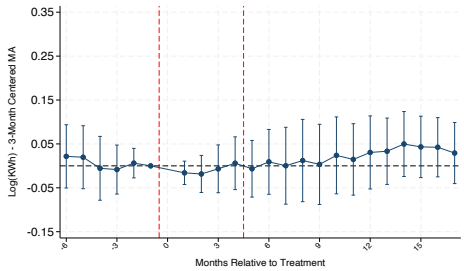


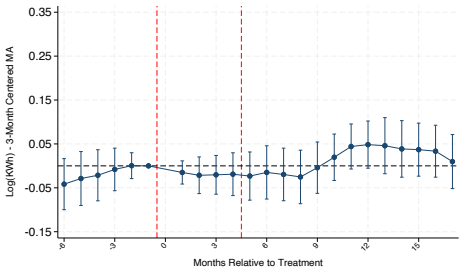
Figure 3: Sample Construction

Notes: Households were surveyed in summer 2023 and asked whether they consented to participate in a study on electric cooking; 2,894 (82.2%) agreed. Of these, 2,740 (94.7%) completed the follow-up survey. The final analysis sample is restricted to households with a 30-ampere or higher electrical connection, as lower-amperage connections do not provide sufficient electrical capacity to run the electric stoves promoted in this study, making the treatment effectively infeasible for those households.

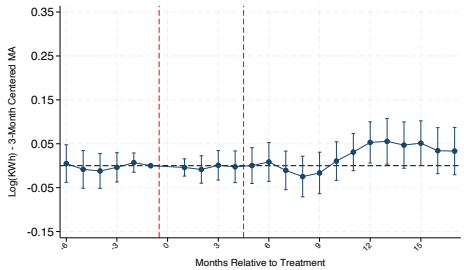
Figure 4: Event Study Estimates of Treatment Effects on Log Electricity Consumption



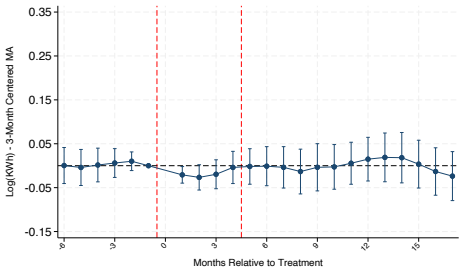
(a) High Subsidy Only



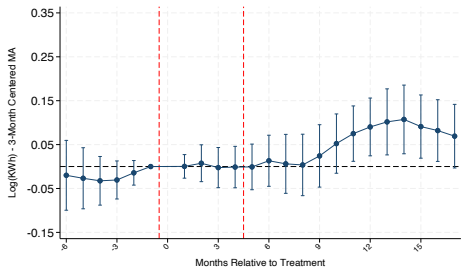
(b) Low Subsidy Only



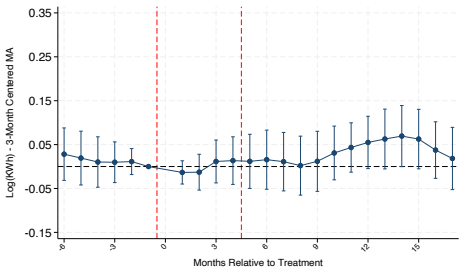
(c) Cost Information Only



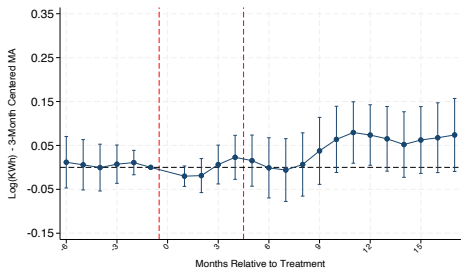
(d) Health Information Only



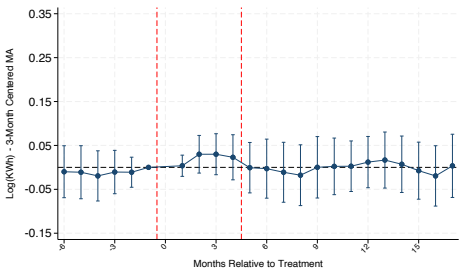
(e) High Subsidy & Cost Information



(f) Low Subsidy & Cost Information



(g) High Subsidy & Health Info.



(h) Low Subsidy & Health Info.

Notes: This figure plots event study estimates of the effect of treatment arms on log electricity consumption (three-month centered moving average), based on Equation (2). Each panel compares a specific treatment arm relative to the control group. The reference period is December 2023. Vertical red dotted lines indicate the beginning and end of the intervention window, during which time households receiving subsidy treatments may use their subsidy coupon. Standard errors are clustered at the household level. Whiskers represent 95% confidence intervals. Figure E6 in the Appendix shows the event studies with the full sample that completed the follow-up survey. Figure E7 in the Appendix shows the same event studies with the raw kWh consumption (i.e., without the moving average).

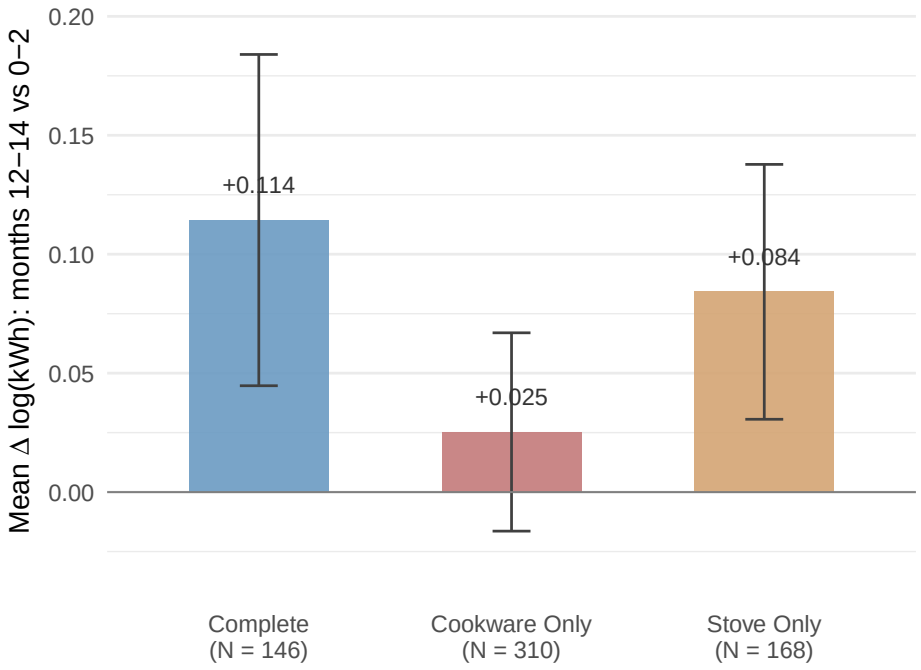


Figure 5: Change in Post-Adoption Electricity Usage by Adopter Type

Notes: Bars show mean changes in total billed electricity usage by adopter type, measured as Δ log(kWh) between months 0–2 and months 12–14 after the intervention. Electricity data are from Nepal Electricity Authority administrative billing records and capture total household consumption, not cooking alone. Complete = purchased both stove and induction-compatible cookware; Cookware Only = cookware without stove; Stove Only = stove without cookware. The sample includes $N = 624$ non-control adopters with billing data in both the early window (months 0–2) and long-run window (months 12–14). Figure 9 uses the larger sample of $N = 694$ adopters across all nine arms because the trajectory model uses all available post-adoption billing months rather than requiring every household to appear in both endpoint windows.

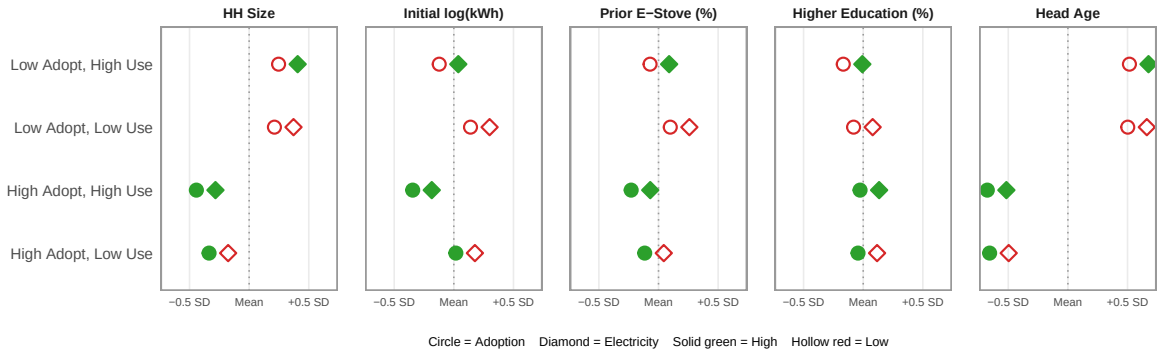


Figure 6: Baseline Household Characteristics by Predicted Household Type

Notes: Each point shows the cell-level mean for the indicated baseline characteristic, normalized to the sample mean and standard deviation. Household types are defined over $N = 1{,}299$ households in subsidy and control arms, equal to 863 subsidy-arm households and 436 control households. Cells use median splits on the causally estimated adoption treatment effect $\hat{\tau}_i$ (causal forest) and realized kWh growth from months 0–2 to months 12–14. Circles denote the adoption response dimension; diamonds denote the electricity usage growth dimension. Green indicates above-median and red indicates below-median response. The vertical dotted line marks the sample mean. All covariates are from the baseline household survey.

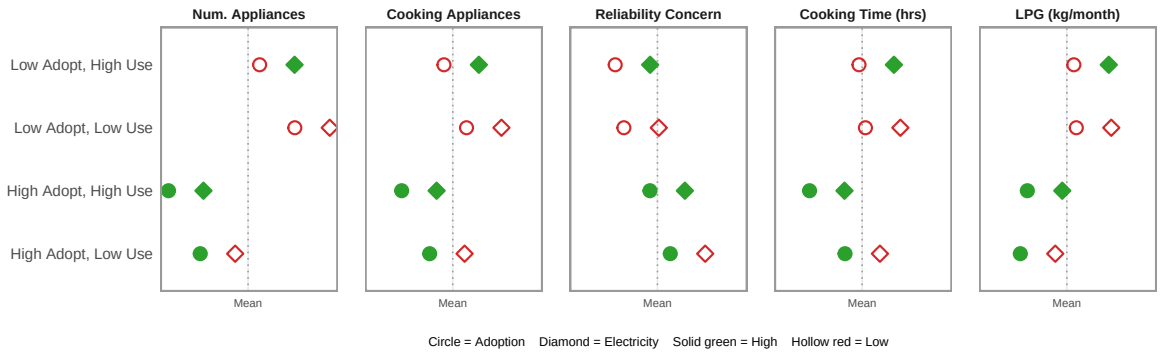


Figure 7: Energy and Targeting Covariates by Predicted Household Type

Notes: Each point shows the cell-level mean for the indicated energy or targeting variable, normalized to the sample mean and standard deviation. Household types are defined over $N = 1{,}299$ households in subsidy and control arms, equal to 863 subsidy-arm households and 436 control households. Cells use median splits on the causally estimated adoption treatment effect $\hat{\tau}_i$ (causal forest) and realized kWh growth from months 0–2 to months 12–14. Circles denote the adoption response dimension; diamonds denote the electricity usage growth dimension. Green indicates above-median and red indicates below-median response. The vertical dotted line marks the sample mean. All variables are from the baseline household survey.

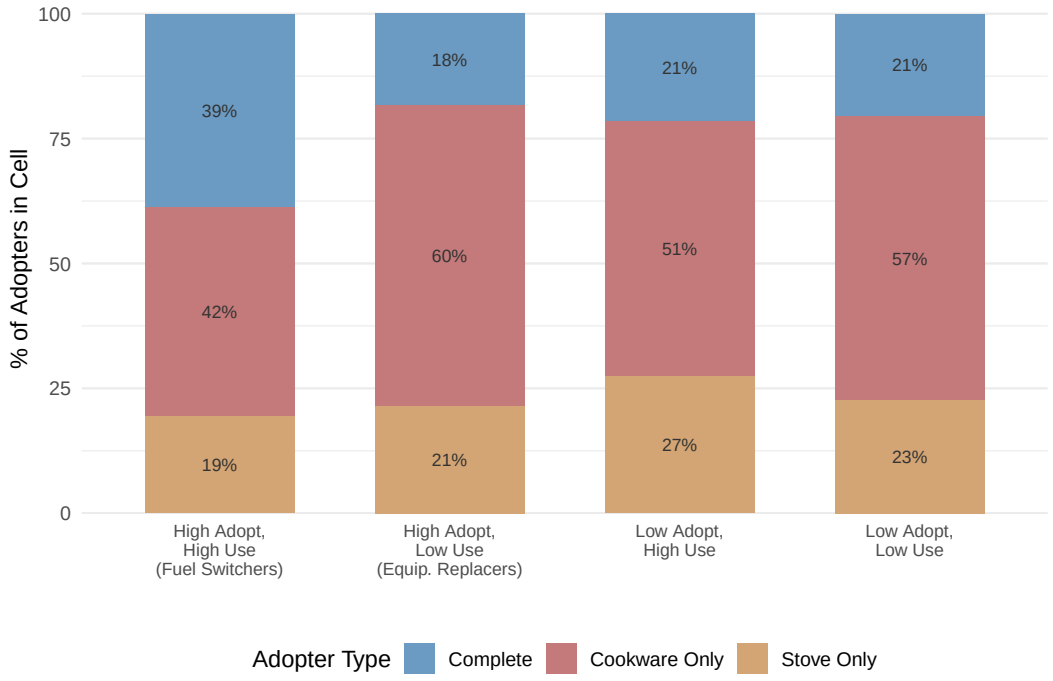


Figure 8: Adopter Composition by Predicted Household Type

Notes: Stacked bars show the share of adopters in each household type who purchased the complete bundle (stove + cookware), cookware only, or stove only. Household types are defined within the CATE sample of $N = 1{,}299$ households in subsidy and control arms. The plotted adopter sample is the $N = 531$ adopters in those arms with billing data in both months 0–2 and months 12–14, comprising 461 subsidy-arm adopters and 70 control adopters. Adoption type is from the endline survey.

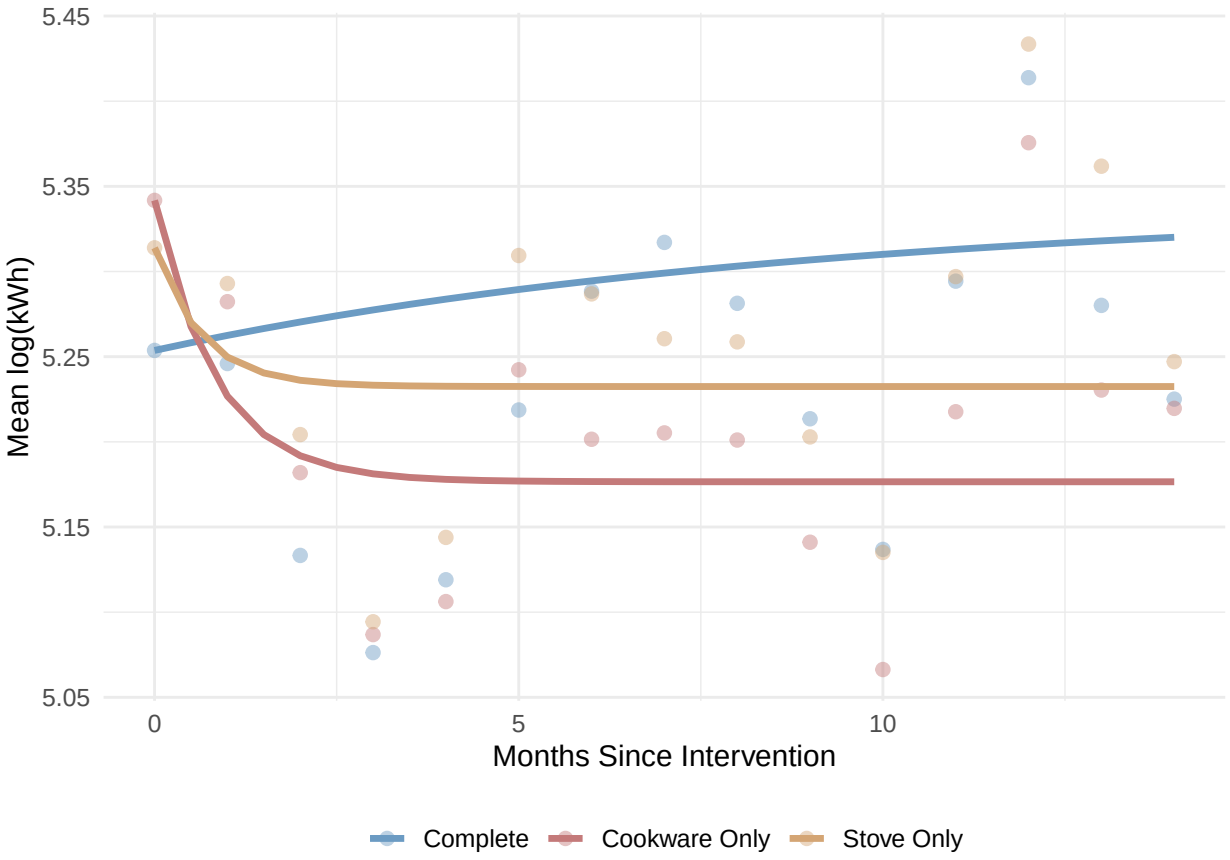


Figure 9: Post-Adoption Electricity Trajectories by Adopter Type

Notes: Points show monthly mean log(kWh) by months since intervention, separately for each adopter type. Solid lines show fitted values from the usage equation (5), estimated separately by type via OLS with cluster fixed effects (169 clusters) and standard errors clustered at the household level. The learning curve $g(t) = 1 - e^{-\hat{\kappa}t}$ is estimated by grid search over $\kappa \in [0.01, 2.0]$. Electricity data are from Nepal Electricity Authority billing records and capture total household consumption, not cooking alone. Complete = purchased stove and induction-compatible cookware ($N = 160$); Cookware Only = cookware without stove ($N = 337$); Stove Only = stove without cookware ($N = 197$). Total: 694 adopters and 11,606 adopter-months across all nine arms. Figure 5 uses $N = 624$ non-control adopters because it requires billing data in both the months 0–2 and months 12–14 endpoint windows.

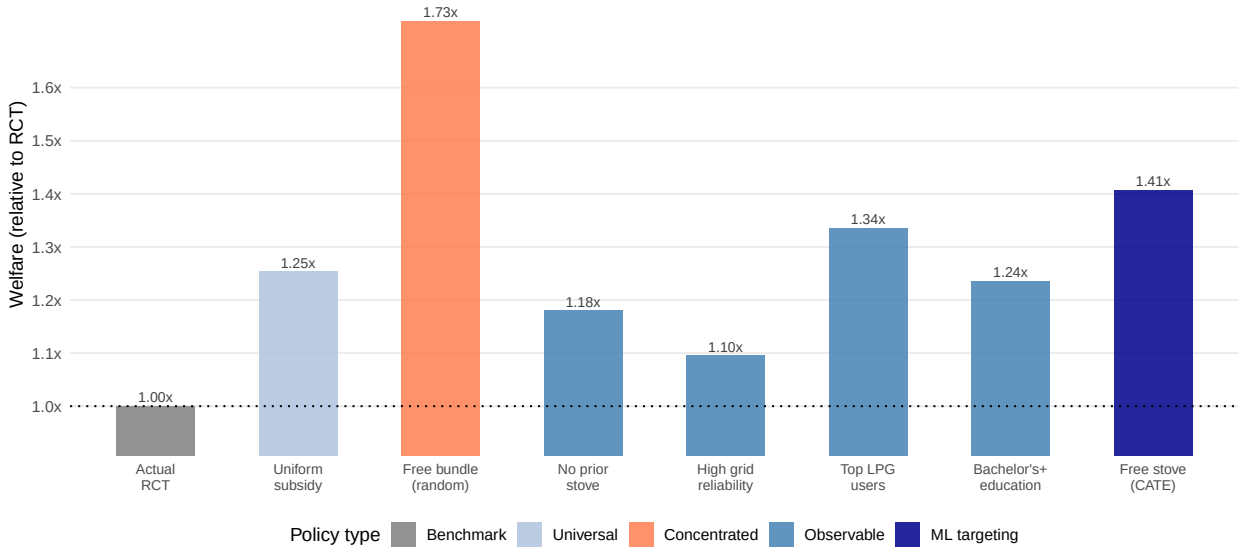


Figure 10: Counterfactual Welfare Gains under Alternative Subsidy Designs

Notes: Bars show planner welfare under eight subsidy designs, expressed as the welfare gain over a no-subsidy baseline and normalized so that the actual RCT design equals 1.00 (dotted line). Welfare is the planner objective in equation (6): aggregate household indirect utility, net of the grid-congestion cost of additional cooking-related electricity demand (δ), the energy-security cost of continued LPG dependence among non-adopters (λ), and the fiscal cost of subsidies. Every policy spends the same budget of NPR 2.18 million, equal to the RCT's actual subsidy disbursement, and bundles cost information with the subsidy. Expected electricity usage per adopter is projected over a 24-month horizon using the type-specific learning curves in Panel C of Table 6, weighted by the adopter-type composition each policy induces. Policies, from left to right: the actual RCT allocation (benchmark); a uniform subsidy spreading the budget as a 14% discount to all households; a free-bundle lottery giving the complete stove-and-cookware bundle to 311 randomly selected households; four observable targeting rules (households without a prior electric stove, households reporting high grid reliability, top LPG users from administrative purchase records, and households with a bachelor's degree or higher); and CATE targeting, which gives free stoves to the 1,000 households with the largest causal-forest adoption treatment effects (Appendix I). Simulations use the analysis sample of households with 30-ampere or higher connections (N = 2,192). Calibration values for $\{\delta, \lambda, \psi\}$ and sensitivity of the policy ranking are in Table G3. The vertical axis begins at 0.95 to make differences across policies visible.

Tables

Table 1: Household Random Assignment by Treatment

	Subsidy Treatments			
Info Treatments	High (3000 NPR)	Low (1000 NPR)	No subsidy	Row totals
Panel A. Full Sample				
Induction Cooking Costs	195	181	576	952
Induction Cooking Health	191	201	585	977
No Info	184	196	585	965
Column totals	570	578	1746	2894
Panel B. Households with 30-Ampere Connection or Higher				
Induction Cooking Costs	151	137	444	732
Induction Cooking Health	140	147	449	736
No Info	142	146	436	724
Column totals	433	430	1329	2192

Notes: The information and subsidy treatments were assigned through a multi-arm randomization, where the combined information-plus-subsidy condition was treated as a separate treatment arm. Each cell depicts the number of households randomly assigned to that combination of treatments. Panel A includes the full randomized sample of 2,894 households, which is the complete list of households surveyed in 2023 that consented to participate in the study. Based on the project budget, approximately 600 households were intended to be assigned to the high and low subsidy groups, and 1800 households assigned to a no subsidy group. Numbers were a bit lower across all three groups, as we randomized across 2,894 households (instead of 3,000 in total). Panel B restricts the sample to the 2,192 households with an electricity connection of 30-amperes or more that completed the follow-up survey.

Table 2: Treatment Effect on Extensive Margin

	(1) Adoption	(2) Compatible Cookware	(3) Induction Stove	(4) Complete Adoption
High Subsidy	0.537***	0.441***	0.216***	0.120***
	(0.0425)	(0.0442)	(0.0413)	(0.0309)
Low Subsidy	0.264***	0.242***	0.0361	0.0136
	(0.0446)	(0.0416)	(0.0312)	(0.0182)
Cost Info	0.0241	0.00506	0.0276	0.00851
	(0.0255)	(0.0200)	(0.0209)	(0.0118)
Health Info	0.0199	0.00173	0.0195	0.00143
	(0.0253)	(0.0197)	(0.0205)	(0.0112)
High Subsidy & Cost Info	0.449***	0.376***	0.177***	0.105***
	(0.0435)	(0.0430)	(0.0389)	(0.0287)
Low Subsidy & Cost Info	0.226***	0.264***	0.0301	0.0674**
	(0.0453)	(0.0434)	(0.0315)	(0.0263)
High Subsidy & Health Info	0.497***	0.399***	0.313***	0.215***
	(0.0439)	(0.0446)	(0.0439)	(0.0372)
Low Subsidy & Health Info	0.268***	0.280***	0.0420	0.0541**
	(0.0445)	(0.0424)	(0.0316)	(0.0240)
Constant	0.161***	0.0940***	0.0940***	0.0275***
	(0.0176)	(0.0140)	(0.0140)	(0.00785)
Observations	2192	2192	2192	2192

Notes: All dependent variables are indicator variables. Compatible cookware indicates if the household reported in the follow-up survey that they bought induction-compatible cookware in the 6 months before the survey was conducted. Induction stove indicates if the household reported buying an induction stove. The variable adoption combines both of the previous variables (i.e., indicates adoption of either cookware or induction stove). Complete adoption indicates that the household bought both an induction stove and induction-compatible cookware. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively. Table E9 in the Appendix shows the same estimations with the full sample that completed the follow-up survey.

Table 3: Treatment Effect on Cooking Behavior

	(1) Elec. Time Cooking	(2) LPG Time Cooking
High Subsidy	20.62***	-5.963
	(6.373)	(5.462)
Low Subsidy	15.61**	-10.21*
	(6.383)	(5.307)
Cost Info	3.942	-0.338
	(3.850)	(3.741)
Health Info	0.346	-2.115
	(3.679)	(3.785)
High Subsidy & Cost Info	13.12**	-6.953
	(5.734)	(4.917)
Low Subsidy & Cost Info	7.086	-9.072*
	(6.351)	(5.079)
High Subsidy & Health Info	13.90**	-4.124
	(6.601)	(5.685)
Low Subsidy & Health Info	9.034	-5.002
	(5.629)	(5.574)
Constant	50.37***	119.7***
	(2.672)	(2.679)
Observations	2192	2184

Notes: The table presents estimates of treatment effects on cooking behavior outcomes. The outcome variables are (1) time spent cooking in minutes on an average day using electric appliances, and (2) time spent cooking in minutes on an average day using an LPG stove. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively. Table E12 in the Appendix shows the estimates with the full sample that completed the follow-up survey.

Table 4: Treatment Effects on log(kWh) by Time Horizon

	(1) Short-run	(2) Long-run
High Subsidy	-0.0141	0.0294
	(0.0279)	(0.0326)
Low Subsidy	0.0105	0.0509*
	(0.0214)	(0.0299)
Cost Info	-0.00900	0.0346
	(0.0152)	(0.0233)
Health Info	-0.0150	-0.00563
	(0.0161)	(0.0255)
High Subsidy & Cost Info	0.0221	0.0931**
	(0.0307)	(0.0413)
Low Subsidy & Cost Info	-0.0110	0.0224
	(0.0220)	(0.0323)
High Subsidy & Health Info	0.0154	0.0581
	(0.0230)	(0.0354)
Low Subsidy & Health Info	0.0216	0.0196
	(0.0200)	(0.0293)
Observations	51885	36669
Period	Short-run	Long-run
Num. Households	2192	2192
Household FE	Yes	Yes
Month-Year FE	Yes	Yes
Control Mean (kWh)	206.79	206.79

Notes: Pre-period: January–December 2023. Short-run post-period: January–December 2024. Long-run post-period: January–May 2025. Outcome is winsorized log(kWh). All specifications include household and month-year fixed effects; standard errors are clustered at the household level. Control mean is the mean of winsorized kWh for the control group over the pre-period. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively. Table E13 shows the estimates with the full sample.

Table 5: Treatment Effects on log(kWh) by Time Horizon and Adopter Type

	Panel A: Short-run			Panel B: Long-run		
	(1)	(2)	(3)	(4)	(5)	(6)
High Subsidy	-0.0267	-0.0546	0.0916	0.0378	-0.0204	0.170**
	(0.0370)	(0.0581)	(0.0673)	(0.0813)	(0.0509)	(0.0685)
Low Subsidy	0.0367	-0.0330	0.145**	0.0865	-0.0191	0.113
	(0.0681)	(0.0387)	(0.0679)	(0.0635)	(0.0485)	(0.112)
Cost Info	0.00769	-0.0372	0.112**	0.0523	0.0226	0.181***
	(0.0258)	(0.0432)	(0.0479)	(0.0465)	(0.0513)	(0.0615)
Health Info	0.0259	0.0208	0.148*	0.0913**	0.0502	0.113
	(0.0337)	(0.0511)	(0.0887)	(0.0404)	(0.0671)	(0.162)
High Subsidy & Cost Info	-0.0560	0.0258	0.0641**	-0.00143	0.116*	0.149**
	(0.126)	(0.0439)	(0.0307)	(0.132)	(0.0624)	(0.0660)
Low Subsidy & Cost Info	0.00263	-0.0148	0.0583	0.136**	-0.0116	0.0377
	(0.0442)	(0.0335)	(0.0841)	(0.0606)	(0.0503)	(0.0976)
High Subsidy & Health Info	0.0110	0.0337	0.0372	0.0312	0.113**	0.0833
	(0.0546)	(0.0429)	(0.0419)	(0.0823)	(0.0532)	(0.0693)
Low Subsidy & Health Info	0.121**	0.0170	0.0566	0.156*	0.00707	0.134**
	(0.0560)	(0.0374)	(0.0471)	(0.0840)	(0.0497)	(0.0603)
Observations	14931	17642	13780	10557	12470	9743
Adopter Type	Stove	Cookware	Complete	Stove	Cookware	Complete
Num. Households	631	746	582	631	746	582
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean (kWh)	206.79	206.79	206.79	206.79	206.79	206.79

Notes: Panel A covers the short-run post-period (January–December 2024). Panel B covers the long-run post-period (January–May 2025). Pre-period: January–December 2023. Each column restricts the treated group to the indicated adopter type (Stove, Cookware, or Complete) while keeping the control. Outcome is winsorized log(kWh). All specifications include household and month-year fixed effects; standard errors are clustered at the household level. Control mean is the mean of winsorized kWh for the control group over the pre-period. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively. Table E16 shows the estimates with the full sample.

Table 6: Structural Parameter Estimates: Adoption, Pooled Usage, and Usage by Adopter Bundle

Panel A: Adoption equation (4)*, identified from RCT*

Parameter	Description	Estimate	Identification
ρ	Subsidy concavity	0.55	Grid search (min. OOS error)
θ	Taste heterogeneity	3.609	Sub. × info system
η	Price sensitivity	0.209	Info-only arm
γ_P	Sub. × cost interaction	0.000	Sub. × info system
Model fit (adoption rates by arm)		Observed	Predicted
	Control	0.161	0.161
	Low subsidy†	0.425	0.397
	High subsidy	0.697	0.648
	Info only	0.182	0.182
	Info + high subsidy	0.632	0.682
	Info + low subsidy†	0.408	0.435

Panel B: Usage equation (5)*, pooled across adopters*

Parameter	Description	Estimate (boot SE)	Identification
$\hat{\delta}_r$	Reliability belief	+0.006 (0.010)	Cross-sectional
$\hat{\kappa}$	Learning rate (pooled)	1.47 (0.14)	Grid search
$\hat{\delta}_L$	Learning coeff. (pooled)	−0.109 (0.016)	Within-HH time path

Panel C: Parametric usage equation by adopter bundle (3-way)

$$\log \tilde{P}_{it} = \delta_r R_i + \delta_L\, g(t;\kappa) + X_i'\beta + \mu_c + \varepsilon_{it}, \quad g(t;\kappa) = 1 - e^{-\kappa t}$$

Adopter type	$\hat{\kappa}$ (boot SE)	$\hat{\delta}_L$ (analytic / boot SE)
Complete (stove + cookware), $N = 160$	0.11 (0.66)	+0.085 (0.044 / 0.080)
Cookware only, $N = 337$	1.19 (0.18)	−0.165 (0.024 / 0.020)***
Stove only, $N = 197$	1.55 (0.61)	−0.081 (0.032 / 0.084)

Panel D: Realized log(kWh) change, months 12–14 vs months 0–2 (non-parametric)

Adopter type	Mean Δ log(kWh) (boot SE)
Complete (stove + cookware), $N = 160$	+0.107 (0.033)***
Cookware only, $N = 337$	+0.020 (0.020)
Stove only, $N = 197$	+0.070 (0.027)***

Notes: The structural adoption sample is restricted to households with electrical service of 30 amps or above ($N = 2{,}192$). **Panel A** reports adoption parameters from equation (4). Subsidy is normalized by $K =$ NPR 7,000; information shifts perceived cost from $P = 1$ to $P = 0.85$. The † marks the two held-out arms used for out-of-sample fit checks. **Panel B** reports the pooled usage equation on 11,606 adopter-month observations from 694 adopters across all nine arms and 169 household clusters. **Panel C** re-estimates the usage equation by adopter bundle using the same 694 adopters. Complete = stove and induction-compatible cookware; Cookware only = cookware without stove; Stove only = stove without cookware. The $\hat{\delta}_L$ column reports analytic and bootstrap standard errors. **Panel D** reports the within-household change in mean log(kWh) from months 0–2 to months 12–14 for the same adopter sample, by bundle type. Billing data measure total household electricity, not cooking-specific electricity. Standard errors are household-level cluster bootstrap with 100 replicates. Significance: $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 7: Adopter Composition by Treatment Arm

	Info only	Subsidy only	Info + low subsidy	**Info + high subsidy**
Total HHs	893	288	284	**291**
Adopters	163	161	116	**184**
Adoption rates (% of HHs)				
Adoption (stove or cookware or both)	18.3	55.9	40.8	**63.2**
Complete adoption (stove + cookware)	3.2	11.8	9.2	**19.6**
Among adopters (%)				
Complete adoption (stove + cookware)	17.8	21.1	22.4	**31.0**
Cookware only	35.6	56.5	67.2	**45.1**
Stove only	46.6	22.4	10.3	**23.9**

Notes: Adoption is defined as purchasing an electric induction stove, cookware, or both by the endline survey. Complete = purchased both stove and induction-compatible cookware. Cookware only = cookware without stove; Stove only = stove without cookware. Adopter-type shares (among adopters) sum to 100% within each arm. Each column pools across cost- and health-information variants of the corresponding treatment (for example, Info + high subsidy pools treatments 1 and 4). Sample restricted to households with electrical service of 30 A or above; 5A and 15A connections are excluded throughout. The table uses all non-control treatment arms, N = 1,756 households. The pure control arm is excluded from this cross-arm composition comparison but included in the usage model and Appendix Table H4.

Online Appendix

A Nepal's Electricity Tariff Schedule

Table A1: Tariff for Residential Consumers, Single Phase

	5 Ampere		15 Ampere		30 Ampere		60 Ampere	
Units (kWh) per month	Minimum charge	Per kWh	Minimum charge	Per kWh	Minimum charge	Per kWh	Minimum charge	Per kWh
Panel A: Tariff in effect FY 2022/23 – FY 2023/24								
0–20	30	0.00	50	4.00	75	5.00	125	6.00
21–30	50	6.50	75	6.50	100	6.50	125	6.50
31–50	50	8.00	75	8.00	100	8.00	125	8.00
51–100	75	9.50	100	9.50	125	9.50	150	9.50
101–250	100	9.50	125	9.50	150	9.50	200	9.50
Above 251	150	11.00	175	11.00	200	11.00	250	11.00
Panel B: Tariff in effect FY 2024/25								
0–20	30	0.00	50	4.00	75	5.00	125	6.00
21–30	50	6.50	75	6.50	100	6.50	125	6.50
31–50	30	8.00	75	8.00	100	8.00	125	8.00
51–100	30	9.50	100	9.50	125	9.50	150	9.50
101–250	100	9.50	125	9.50	150	9.50	200	9.50
Above 251	100	11.00	110	11.00	200	11.00	250	11.00

Notes: Monetary values are expressed in Nepalese rupees (NPR). Panel A corresponds to the tariff schedule in effect during FY 2022/23–FY 2023/24. Panel B corresponds to the revised tariff schedule in effect during FY 2024/25. Beginning in FY 2021/22, domestic consumers with a 5 Ampere connection receive the first 20 kWh at zero marginal charge, subject to the condition that if monthly consumption exceeds 20 kWh, the first 20 units are charged at NRs. 3.00 per kWh.

B Cost Comparison: LPG versus Electric Cooking

Given that an LPG cylinder cost NPR 1,895 in Nepal at the time of the study, it was cheaper for all consumer types, irrespective of their current monthly electricity consumption, to switch to electric cooking.

Detailed calculations are provided in Table B1 use heat values of 51 MJ/kg for LPG[25] and 3.6 MJ/kWh for electricity. According to Nepal Gas, a big cylinder contains 14.2 kg of LPG[26], which means a cylinder holds 724.2 MJ of heat energy in it. Using an efficiency of 90%[27] for electric induction stoves and a conservative estimate of 55%[28] for gas stoves, it was found that one cylinder of LPG is equivalent to 122.9 kWh of electricity.

Initial Monthly Consumption (kWh)	**Min Charge (NPR)**	**Energy Charge (NPR)**	**Initial Bill (NPR)**	**One Cylinder Equivalent Additional Consumption (kWh)**	**Min Charge (NPR)**	**Energy Charge (NPR)**	**Revised Bill (NPR)**	**Marginal Cost of One Cylinder Equivalent (NPR)**
20	75	100	175	142.9	150	1207.9	1357.9	1182.9
30	100	165	265	152.9	150	1302.9	1452.9	1187.9
50	100	325	425	172.9	150	1492.9	1642.9	1217.9
100	125	800	925	222.9	150	1967.9	2117.9	1192.9
250	150	2225	2375	372.9	200	3577.3	3777.3	1402.3
400	200	3875	4075	522.9	200	5227.3	5427.3	1352.3
650	200	6625	6825	772.9	200	7977.3	8177.3	1352.3

Table B1: Cost calculations of shifting one cylinder equivalent cooking from LPG stove to electric induction stove with a 30-ampere electricity connection

[25] A heat value of LPG represents the energy released when one kilogram of LPG is combusted. A heat value of 51 MJ/kg is a conservative estimate taken from the World Nuclear Association and corroborated from the International Energy Agency.

[26] The cylinder capacity is reported on the Nepal Gas website.

[27] This statistic on efficiency was taken from Sweeney et al. (2014).

[28] This is a conservative estimate taken from: https://tcdorji.wordpress.com/2022/01/19/electric-stove-vs-lpg-stove-which-cooking-technology-is-more-cost-effective/.

C Maps of Study Area

Certain landmarks such as the Bishnumati River, which runs north-south, and the Ring Road, which circles the city of Kathmandu, are easily identifiable across the maps in this section.

1 Sampling Area and Transformer Locations

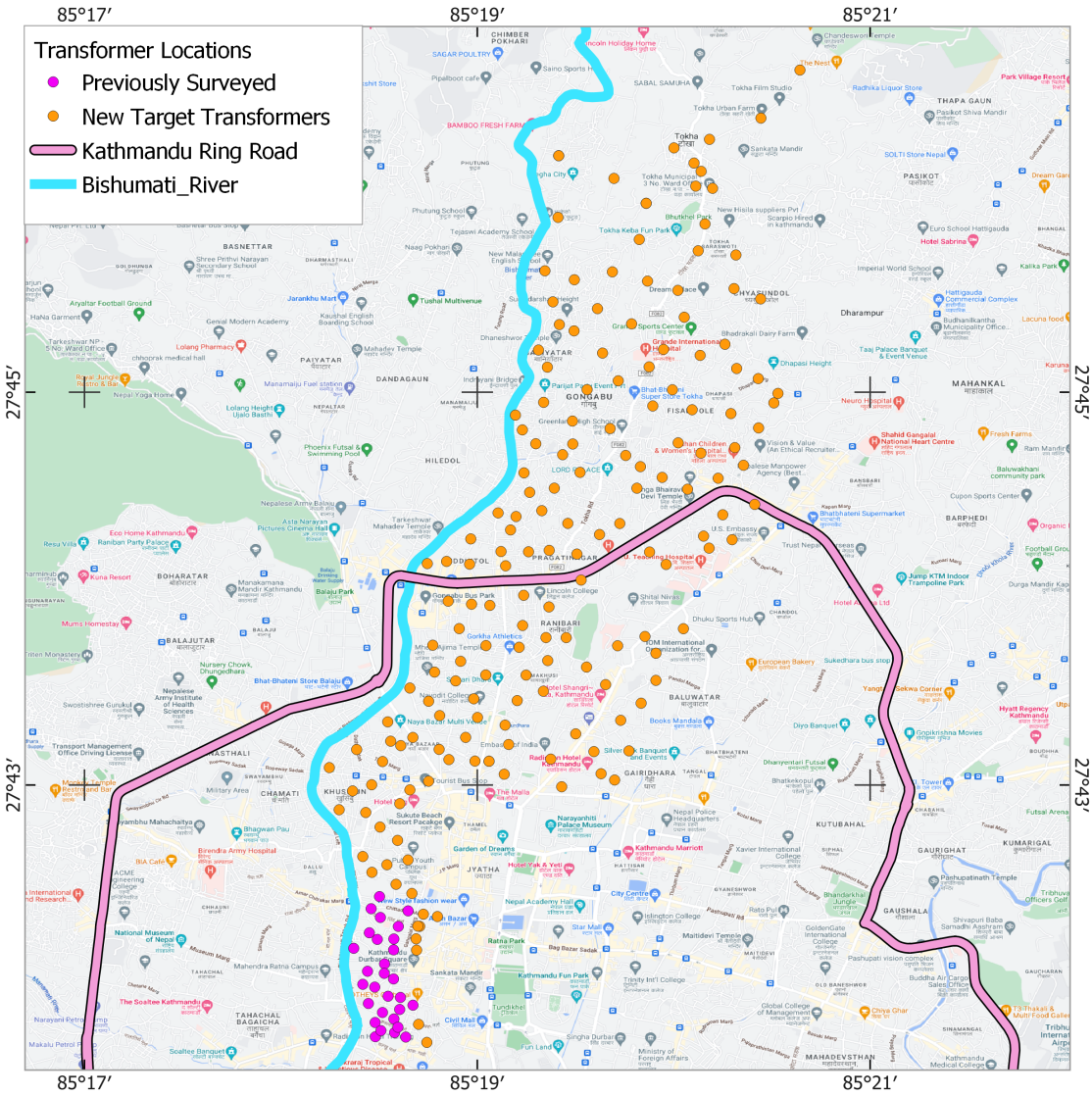


Figure C1: Map of Transformers Used in the Sampling Strategy

Notes: This map shows the locations of transformers located within 2 km to the east of the Bishnumati River. The map does not show transformers on the western side of the river, as the distribution system on the western side was not connected to smart meters, and therefore we did not have GPS location data for these transformers.

2 Superstore Locations in Kathmandu Valley

Our partner store locations were spread out across the region under study (see Figure C2), providing easy access to participating households. Subsidy vouchers, provided through some of the treatments, could be used at any of these stores to purchase an induction stove or induction-related cookware.

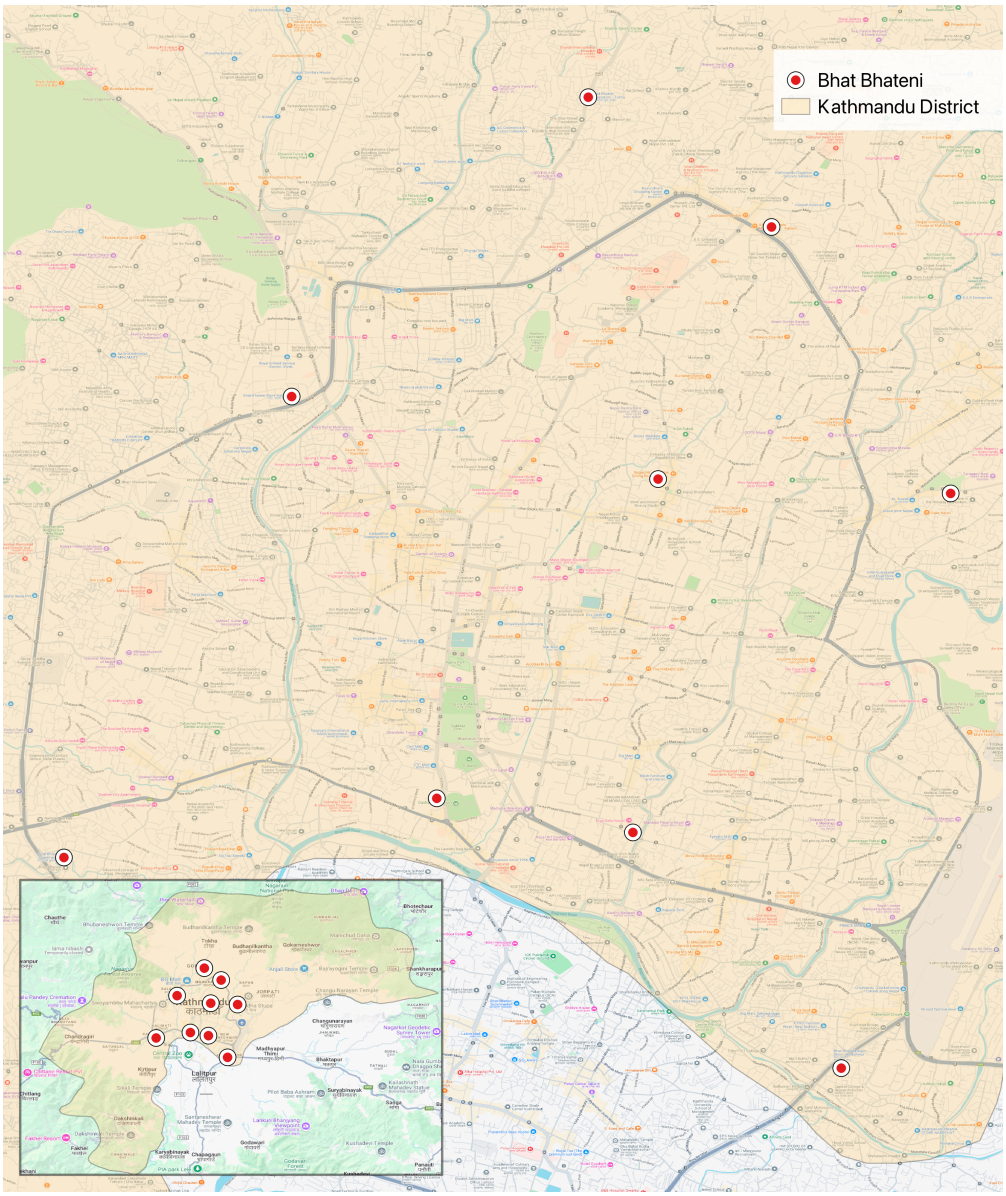


Figure C2: Mega store locations in Kathmandu

This map was created using the names of store locations from the store's website, which were then used to search their locations on Google Maps.

D Details of the Intervention

1 Text Regarding Phone Credit for Continued Study Participation

The following message was read to all study participants (including those assigned to the control group) approached in January through February 2024 during the intervention:

"Namaste, my name is XXXXXX and I am from New ERA, which is a Kathmandu based research organization. I am here to thank you again for your continued support in the study conducted by Asian Development Bank in collaboration with Nepal Electricity Authority and New ERA. The study is being conducted to understand electricity use including electric cooking by the households and to understand electricity related needs and concerns of the households, as well as to support Nepal in improving its electricity transmission and distribution system. Also, we would like to provide you with a token of appreciation in the form of a recharge card worth Rs 100 on behalf of Asian Development Bank for giving us your valuable time in the previous survey and for your support in the next round of survey to be conducted around June/July, 2024."

2 Regarding Cost of Electric Cooking

The cost message, which was printed on the pamphlets given to study participants randomly assigned to receive them, states: "CAN YOUR COOKING NEEDS BE MET FOR LESS MONEY? Did you know that induction and electric cooking could result in savings for your household? Find out if electric cooking is suited to your household needs and you could save hundreds of rupees ! With a one-time investment into an electric or induction cookstove, you can easily transition your cooking needs to electric. If you have an existing connection with the NEA (15 amp connection or above), and purchase an electric cookstove with over 66% efficiency, shifting your cooking medium could result in hundreds of rupees of savings per month!"

An image of the message provided through the intervention regarding the cost of electric cooking is on the left side of Figure D3.

3 Regarding Pollution from Electric Cooking

The health message, which was printed on the pamphlets given to study participants randomly assigned to receive them, states: "DID YOU KNOW THAT ELECTRIC COOKING IS THE "CLEANEST" WAY TO COOK IN NEPAL? Studies in Nepal have shown that electric cooking creates less household air pollution (fine particulate matter, PM2.5) than cooking with the other currently available fuels, including kerosene, biomass (dung, firewood, etc), and even LPG. This is important as studies show that burning fuels is harmful to health and associated with increased risk of disease."

An image of the message provided through the intervention regarding the pollution and

health effects of cooking is on the right side of Figure D3.



Figure D3: Messages Regarding Cost (left) and Health (right) of E-Cooking

Notes: These graphics depict information pamphlets provided to study participants randomly assigned to receive them. The cost message is on the left and the health message is on the right.

4 Subsidy Treatments

Households that were randomly assigned to the subsidy treatment received a coupon card, as shown in Figure D4.

The subsidy vouchers could be used in Bhat Bhateni mega locations throughout Kathmandu Valley. There are 9 superstore locations throughout Kathmandu Valley (mapped in Figure C2). To use the subsidy, a household member had to bring the voucher to the customer service desk in the Bhat Bhateni store. Households had to be a Bhat Bhateni Club Card member or register to become one if they were not already. It costs nothing to become a member and is essentially a system through which shoppers earn points and rewards based on their prior purchases, like one would find in many major retailers in the United States. Given the coverage of the superstore, many people in the study were already Club Card members. Because of their own club card system and their store's system of giving annual "rewards" to their customers, Bhat Bhateni already had a voucher system set up with employees trained to receive vouchers.

The households were then directed towards the induction stoves and induction-related cookware within the store. Prior to the intervention, Bhat Bhateni had provided the research

team with a list of all induction stoves and related cookware sold in their stores. The voucher card could only be used towards induction-related cooking items that were previously approved by the research team. The subsidy coupons were eligible to be redeemed from the date that the households received them through April 30, 2024.

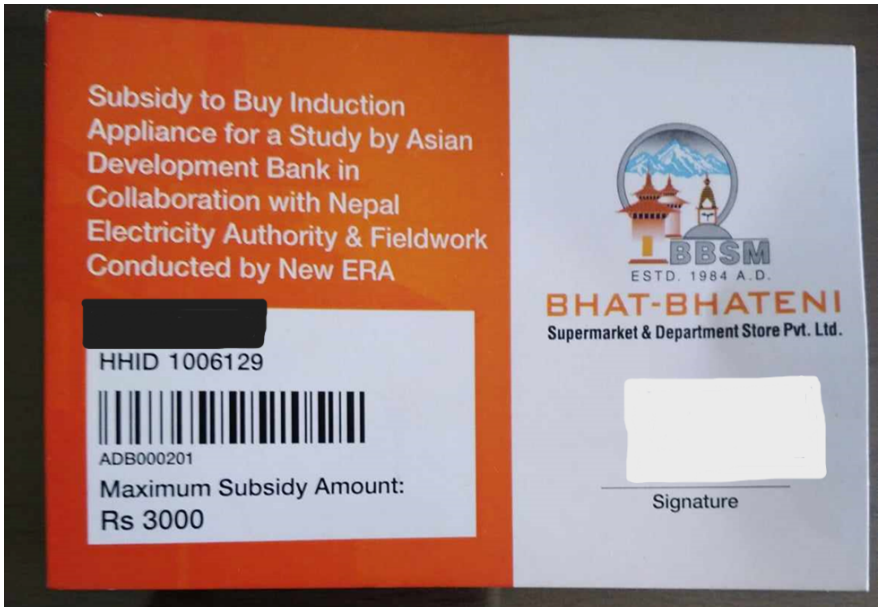


Figure D4: Example of Subsidy Card Provided

Notes: This is an example of the actual subsidy card received by those households randomly assigned to receive one. The name of the household member who was the survey respondent is printed in the rectangle marked in black. The signature of that person is in the rectangle marked in white.

4.1 Regarding Lower Subsidy

The lower subsidy message, which was printed on the pamphlets given to study participants randomly assigned to receive them, states: "Your household has been randomly selected to receive a 1000 NPR subsidy to purchase an induction stove!!!!! If you already have an induction stove, you can use this voucher to purchase a second stove or new pots and pans that are appropriate for and compatible with an induction stove. These vouchers can be used at your local Bhat Bhateni store in Kathmandu Valley. To use the subsidy, you must bring this voucher to the customer service desk in the Bhat Bhateni store. This voucher is for your house and should not be given to others. To use this voucher, the customer from your household will need to be a member of BBSM Clubcard. If your household is not already a Clubcard member, that is ok! You can sign up at the Customer Service Desk of any BBSM branch. This voucher subsidy is valid until April 30th, 2024. All details are printed on the voucher."

An image of the voucher for this lower subsidy is on the left side of Figure D5.

4.2 Regarding Higher Subsidy

The higher subsidy message, which was printed on the pamphlets given to study participants randomly assigned to receive them, states: "Your household has been randomly selected to receive a 3000 NPR subsidy to purchase an induction stove!!!!! If you already have an induction stove, you can use this voucher to purchase a second stove or new pots and pans that are appropriate for and compatible with an induction stove. These vouchers can be used at your local Bhat Bhateni store in Kathmandu Valley. To use the subsidy, you must bring this voucher to the customer service desk in the Bhat Bhateni store. This voucher is for your house and should not be given to others. To use this voucher, the customer from your household will need to be a member of BBSM Clubcard. If your household is not already a Clubcard member, that is ok! You can sign up at the Customer Service Desk of any BBSM branch. This voucher subsidy is valid until April 30th, 2024. All details are printed on the voucher."

An image of the voucher for this higher subsidy is on the right side of Figure D5.

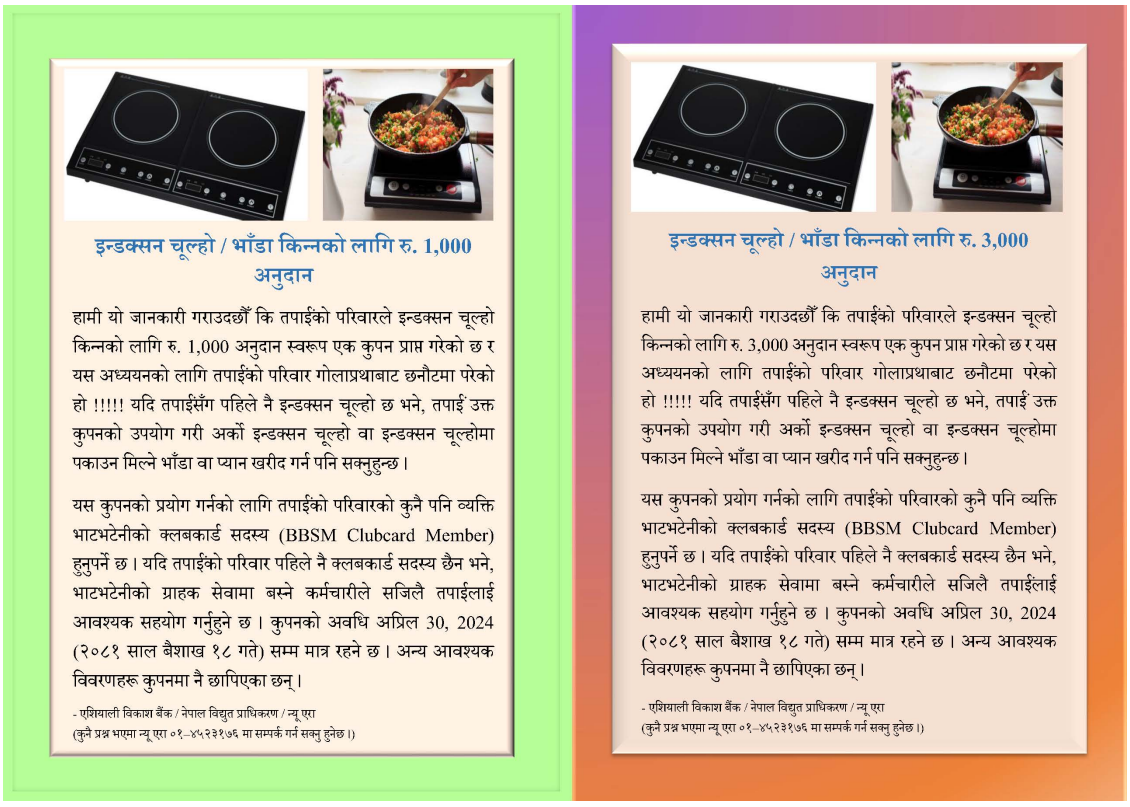


Figure D5: Messages Regarding Subsidies

Notes: These graphics depict pamphlets on the subsidies for electric cooking provided to study participants randomly assigned to receive them. The lower subsidy (1000 NPR) message is on the left and the higher subsidy (3000 NPR) message is on the right.

5 Treatments Not Delivered

Despite consenting to future participation, 78 households were not reachable at the time of the intervention. We show the breakdown of these by treatment group in the table that follows.

Table D2: Undelivered Interventions, by Group

Randomized Assignment Group	Frequency	Percent
Low Subsidy	8	10
Cost Info	20	26
Health Info	15	19
High Subsidy & Cost Info	3	4
Low subsidy & Cost Info	5	6
High Subsidy & Health Info	2	3
Low Subsidy & Health Info	4	5
Control	21	27
Totals	78	100.00

Notes: A total of 2,894 households were randomized across groups. The households counted here are those that were unable to be visited at the time of intervention. The control households were meant to receive a phone credit at that time, and the treatment households were intended to receive a phone credit plus the treatment to which they were assigned.

E Additional Tables, Figures and Event Studies

Table E3: Test for Differential Attrition by Treatment Arm

	(1)
High Subsidy	-0.021
	(0.015)
Low Subsidy	0.024
	(0.020)
Cost Info	0.023*
	(0.014)
Health Info	-0.005
	(0.012)
High Subsidy & Cost Info	-0.002
	(0.017)
Low Subsidy & Cost Info	0.002
	(0.018)
High Subsidy & Health Info	0.004
	(0.018)
Low Subsidy & Health Info	0.017
	(0.019)
Constant	0.048***
	(0.009)
Joint test p-value	0.234
Observations	2894

Notes: The dependent variable equals one if the household consented at baseline but was not interviewed at endline (attrited), and zero otherwise. All regressors are indicators for treatment assignment, with the control group omitted, so each coefficient is the difference in the attrition rate relative to control. Robust standard errors are in parentheses. "Joint F-test" and "Joint test p-value" report the test that all treatment-arm coefficients are jointly equal to zero. No arm differs from control at the 5% level; two arms are marginally significant at the 10% level in opposite directions (Cost Information, higher attrition; High Subsidy, lower attrition), and the joint test is borderline ($p = 0.086$). Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively.

Table E4: Probit of Attrition on Baseline Household Characteristics

	(1)
Household size	-0.110***
	(0.027)
Household head with no education	-0.154
	(0.114)
Household head with higher education	0.104
	(0.096)
Household has smart meter	0.200**
	(0.091)
Household has an electric induction stove	0.033
	(0.135)
# of electric cooking appliances	0.001
	(0.082)
Constant	-1.332***
	(0.130)
Joint test p-value	0.000
Pseudo R^2	0.031
Observations	2894.000

Notes: Probit regression of the attrition indicator (equal to one if the household consented at baseline but was not interviewed at endline) on baseline household characteristics. Entries are probit coefficients with robust standard errors in parentheses. "Joint χ^2 test" and "Joint test p-value" report the Wald test that all coefficients are jointly equal to zero. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively.

Table E5: Probability of Dropping from the Final Analysis Sample by Treatment Arm

	(1)
High Subsidy	-0.011
	(0.035)
Low Subsidy	-0.019
	(0.034)
Cost Info	-0.047**
	(0.024)
Health Info	-0.019
	(0.024)
High Subsidy & Cost Info	-0.029
	(0.034)
Low Subsidy & Cost Info	-0.014
	(0.035)
High Subsidy & Health Info	0.009
	(0.036)
Low Subsidy & Health Info	0.001
	(0.035)
Constant	0.217***
	(0.018)
Observations	2740

Notes: The dependent variable is an indicator equal to one if a household is excluded from the final analysis sample and zero otherwise. The final analysis sample is defined as households with electricity connections of 30-ampere or higher. All regressors are indicators for treatment assignment, with the control group omitted. Robust standard errors are reported in parentheses. A positive coefficient indicates a higher probability of being excluded from the final sample relative to the control group. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively.

Table E6: Balance Matrix – Households with 30-Ampere Connections or Higher

	High Sub. + Cost Info	Low Sub. + Cost Info	Cost Info Only	High Sub. + Health Info	Low Sub. + Health Info	Health Info Only	High Subsidy	Low Subsidy	Control Group
HH Size	4.50	4.32	4.34	4.32	4.33	4.30	4.35	4.45	4.39
	(.158)	(.1505)	(.0969)	(.1667)	(.1639)	(.097)	(.1602)	(.1588)	(.0965)
	[0.56]	[0.68]	[0.68]	[0.70]	[0.72]	[0.48]	[0.79]	[0.76]	
HH Head with no education	0.21	0.15	0.19	0.19	0.18	0.19	0.18	0.21	0.18
	(.0334)	(.0303)	(.0187)	(.0335)	(.0316)	(.0185)	(.0321)	(.0336)	(.0186)
	[0.46]	[0.29]	[0.76]	[0.81]	[0.86]	[0.82]	[0.84]	[0.57]	
HH Head with Higher Education	0.17	0.23	0.21	0.16	0.18	0.20	0.20	0.21	0.21
	(.0308)	(.0359)	(.0194)	(.0314)	(.032)	(.0189)	(.034)	(.034)	(.0194)
	[0.35]	[0.63]	[0.85]	[0.26]	[0.54]	[0.83]	[0.96]	[0.88]	
HH has smart meter	0.87	0.85	0.84	0.89	0.85	0.85	0.85	0.85	0.86
	(.0277)	(.0303)	(.0174)	(.0262)	(.0295)	(.017)	(.0299)	(.0297)	(.0166)
	[0.82]	[0.86]	[0.41]	[0.29]	[0.77]	[0.56]	[0.82]	[0.75]	
HH has induction cook	0.26	0.28	0.28	0.28	0.28	0.29	0.25	0.25	0.30
	(.0357)	(.0384)	(.0213)	(.038)	(.0371)	(.0213)	(.0366)	(.0358)	(.0219)
	[0.37]	[0.68]	[0.54]	[0.69]	[0.69]	[0.72]	[0.32]	[0.24]	
# of Electric Cooking Appliances	0.51	0.51	0.54	0.51	0.56	0.51	0.53	0.51	0.56
	(.0601)	(.0656)	(.0349)	(.0645)	(.0634)	(.0337)	(.0653)	(.0593)	(.0376)
	[0.48]	[0.52]	[0.74]	[0.54]	[0.98]	[0.30]	[0.68]	[0.51]	
Mean monthly kWh 2023 (billing data)	211.68	201.65	209.79	270.55	220.55	229.21	203.78	211.47	210.42
	(11.909)	(12.4755)	(7.7611)	(64.328)	(14.7564)	(10.7012)	(12.7696)	(12.0438)	(7.4966)
	[0.93]	[0.55]	[0.95]	[0.35]	[0.54]	[0.15]	[0.65]	[0.94]	
N	151	137	444	140	147	449	142	146	436

Notes: Sample restricted to households with a 30-ampere connection or higher. Standard errors in parentheses; p-values of the difference between treatment and control in square brackets. The N row indicates the number of households per group. The total number of households that completed the follow-up survey and have a 30-amp or higher connection is 2,192.

Table E7: Summary Statistics – Households with 30-Ampere Connections or Higher

	Mean	S.D.	Min	Max	N
HH has smart meter	0.85	0.35	0	1	2192
Cash/Cheque for Elec. Bill	0.35	0.48	0	1	2192
Credit for Elec. Bill	0.65	0.48	0	1	2192
Know Most Recent Elec. Expense	0.96	0.18	0	1	2192
Recent Elec. Expense (NPR)	2196.70	2851.25	14	60000	2115
Know Most Recent Elec. Consumption	0.54	0.50	0	1	2192
Recent Elec. Consumption (KWh)	209.67	214.02	0	4676	1174
Avg. Elec. Outages (per week, Normal Months)	1.24	2.24	0	21	2142
Avg. Elec. Outages (per week, Bad Months)	5.36	4.39	0	30	1834
Voltage Fluctuation in 12 Months	0.34	0.48	0	1	2192
Elec. Fluctuation $\geq$ Once/Week	0.07	0.26	0	1	696
Appliance for Voltage Fluctuation	0.21	0.40	0	1	2192
Prop. that heat house	0.34	0.47	0	1	2192
# of light bulbs per HH	22.46	16.00	2	223	2192
# of EE bulbs per HH	21.50	15.81	0	223	2192
# of Elec. appliances per HH	17.03	7.26	1	54	2192
Prop. with fan	0.83	0.37	0	1	2192
Prop. with fridge	0.89	0.32	0	1	2192
Prop. with rice cooker	0.44	0.50	0	1	2192
Prop. with pressure cooker	0.01	0.08	0	1	2192
Prop. with induction stove	0.36	0.48	0	1	2192
Prop. with microwave	0.32	0.47	0	1	2192
Prop. with washing machine	0.49	0.50	0	1	2192
Prop. with AC	0.03	0.18	0	1	2192
Prop. with kettle/pot	0.64	0.48	0	1	2192
Prop. with an EV (endline)	0.03	0.17	0	1	2192
Hours spent on cooking (last week)	13.50	5.44	0	44	2173

Notes: Sample restricted to households with a 30-ampere connection or higher. The variables used to generate this table are from the baseline survey.

Table E8: Treatment Effect on Extensive Margin by Baseline E-Stove Ownership

	(1) Adoption	(2) Compatible Cookware	(3) Induction Stove	(4) Complete Adoption	(5) Adoption	(6) Bought Ind. Compatible Cookware	(7) Induction Stove	(8) Complete Adoption
High Subsidy	0.553***	0.405***	0.325***	0.178***	0.506***	0.512***	0.00113	0.00732
	(0.0520)	(0.0532)	(0.0550)	(0.0437)	(0.0739)	(0.0743)	(0.0333)	(0.0228)
Low Subsidy	0.238***	0.207***	0.0549	0.0241	0.289***	0.267***	0.0250	0.00288
	(0.0565)	(0.0494)	(0.0458)	(0.0278)	(0.0724)	(0.0713)	(0.0358)	(0.0189)
Cost Info	0.0446	0.0271	0.0403	0.0228	-0.0160	-0.0431	0.0137	-0.0135
	(0.0310)	(0.0209)	(0.0294)	(0.0177)	(0.0451)	(0.0411)	(0.0240)	(0.00954)
Health Info	0.0225	0.0146	0.0188	0.0109	0.0107	-0.0293	0.0265	-0.0135
	(0.0299)	(0.0198)	(0.0283)	(0.0165)	(0.0463)	(0.0420)	(0.0255)	(0.00954)
High Subsidy & Cost Info	0.463***	0.391***	0.231***	0.158***	0.416***	0.332***	0.118**	0.0341
	(0.0562)	(0.0547)	(0.0546)	(0.0436)	(0.0699)	(0.0707)	(0.0491)	(0.0286)
Low Subsidy & Cost Info	0.217***	0.258***	0.0509	0.0917**	0.237***	0.264***	-0.000541	0.0265
	(0.0554)	(0.0515)	(0.0450)	(0.0373)	(0.0780)	(0.0773)	(0.0323)	(0.0295)
High Subsidy & Health Info	0.521***	0.412***	0.373***	0.264***	0.448***	0.382***	0.169***	0.103**
	(0.0524)	(0.0525)	(0.0545)	(0.0479)	(0.0803)	(0.0824)	(0.0645)	(0.0501)
Low Subsidy & Health Info	0.212***	0.225***	0.0593	0.0717**	0.363***	0.371***	0.0161	0.0242
	(0.0535)	(0.0481)	(0.0443)	(0.0337)	(0.0761)	(0.0756)	(0.0358)	(0.0280)
Constant	0.139***	0.0521***	0.122***	0.0347***	0.203***	0.176***	0.0405**	0.0135
	(0.0204)	(0.0131)	(0.0193)	(0.0108)	(0.0332)	(0.0315)	(0.0163)	(0.00954)
Observations	1396	1396	1396	1396	796	796	796	796
Owned estove at baseline	No	No	No	No	Yes	Yes	Yes	Yes

Notes: All dependent variables are indicator variables. Compatible cookware indicates if the household reported in the follow-up survey that they bought induction-compatible cookware in the 6 months before the survey was conducted. Induction stove indicates if the household reported buying an induction stove. The variable adoption combines both of the previous variables (i.e. indicates adoption of either cookware or induction stove). Complete adoption indicates that the household bought both an induction stove and induction-compatible cookware. Columns (1)–(4) restrict the sample to households that did not own an electric stove at baseline, while columns (5)–(8) restrict it to households that already owned an electric stove at baseline. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively. Table E10 in the Appendix shows the same estimations with the full sample that completed the follow-up survey.

Table E9: Treatment Effect on Extensive Margin — Full Sample

	(1) Adoption	(2) Compatible Cookware	(3) Induction Stove	(4) Complete Adoption
High Subsidy	0.529***	0.413***	0.230***	0.115***
	(0.0381)	(0.0392)	(0.0369)	(0.0268)
Low Subsidy	0.270***	0.245***	0.0439	0.0188
	(0.0396)	(0.0368)	(0.0278)	(0.0166)
Cost Info	0.0210	0.00347	0.0279	0.0104
	(0.0221)	(0.0170)	(0.0183)	(0.0104)
Health Info	0.0153	-0.000452	0.0174	0.00165
	(0.0217)	(0.0166)	(0.0177)	(0.00953)
High Subsidy & Cost Info	0.471***	0.394***	0.192***	0.115***
	(0.0387)	(0.0385)	(0.0351)	(0.0263)
Low Subsidy & Cost Info	0.237***	0.259***	0.0399	0.0621***
	(0.0401)	(0.0381)	(0.0282)	(0.0225)
High Subsidy & Health Info	0.532***	0.413***	0.321***	0.201***
	(0.0379)	(0.0390)	(0.0385)	(0.0319)
Low Subsidy & Health Info	0.273***	0.277***	0.0610**	0.0653***
	(0.0391)	(0.0370)	(0.0287)	(0.0220)
Constant	0.147***	0.0844***	0.0880***	0.0251***
	(0.0150)	(0.0118)	(0.0120)	(0.00664)
Observations	2740	2740	2740	2740

Notes: All dependent variables are dummies. Compatible cookware indicates if the household reported buying induction-compatible cookware in the last 6 months before the endline interview. Induction stove indicates if the household reported buying an induction stove. The variable adoption combines both of the previous variables (i.e. indicates adoption of either cookware or induction stove). Complete adoption indicates that the household bought both an induction stove and induction-compatible cookware. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively.

Table E10: Treatment Effects on Adoption by Baseline E-Stove Ownership — Full Sample

	(1) Adoption	(2) Compatible Cookware	(3) Induction Stove	(4) Complete Adoption	(5) Adoption	(6) Compatible Cookware	(7) Induction Stove	(8) Complete Adoption
High Subsidy	0.534***	0.372***	0.326***	0.164***	0.520***	0.506***	0.0205	0.00685
	(0.0463)	(0.0461)	(0.0481)	(0.0372)	(0.0671)	(0.0683)	(0.0324)	(0.0190)
Low Subsidy	0.241***	0.204***	0.0690*	0.0324	0.305***	0.286***	0.0220	0.00282
	(0.0498)	(0.0432)	(0.0410)	(0.0255)	(0.0649)	(0.0640)	(0.0294)	(0.0155)
Cost Info	0.0429	0.0255	0.0385	0.0211	-0.0210	-0.0448	0.0182	-0.00567
	(0.0271)	(0.0178)	(0.0259)	(0.0154)	(0.0386)	(0.0353)	(0.0202)	(0.00908)
Health Info	0.0199	0.0150	0.0140	0.00912	0.00411	-0.0348	0.0282	-0.0107
	(0.0257)	(0.0167)	(0.0244)	(0.0140)	(0.0396)	(0.0358)	(0.0213)	(0.00756)
High Subsidy & Cost Info	0.474***	0.395***	0.253***	0.175***	0.458***	0.373***	0.115***	0.0293
	(0.0497)	(0.0485)	(0.0489)	(0.0397)	(0.0625)	(0.0638)	(0.0430)	(0.0240)
Low Subsidy & Cost Info	0.227***	0.258***	0.0534	0.0836***	0.252***	0.256***	0.0179	0.0226
	(0.0487)	(0.0449)	(0.0393)	(0.0317)	(0.0702)	(0.0694)	(0.0311)	(0.0245)
High Subsidy & Health Info	0.550***	0.407***	0.392***	0.249***	0.497***	0.443***	0.138**	0.0836**
	(0.0449)	(0.0454)	(0.0473)	(0.0409)	(0.0704)	(0.0727)	(0.0534)	(0.0410)
Low Subsidy & Health Info	0.232***	0.240***	0.0771*	0.0852***	0.340***	0.332***	0.0404	0.0328
	(0.0475)	(0.0429)	(0.0399)	(0.0310)	(0.0667)	(0.0662)	(0.0339)	(0.0258)
Constant	0.130***	0.0459***	0.116***	0.0324***	0.182***	0.160***	0.0321**	0.0107
	(0.0175)	(0.0109)	(0.0167)	(0.00923)	(0.0283)	(0.0270)	(0.0129)	(0.00756)
Observations	1767	1767	1767	1767	973	973	973	973
Owned estove at baseline	No	No	No	No	Yes	Yes	Yes	Yes

Notes: All dependent variables are indicator variables. Compatible cookware indicates if the household reported in the follow-up survey that they bought induction-compatible cookware in the 6 months before the survey was conducted. Induction stove indicates if the household reported buying an induction stove. The variable adoption combines both of the previous variables (i.e. indicates adoption of either cookware or induction stove). Complete adoption indicates that the household bought both an induction stove and induction-compatible cookware. Columns (1)–(4) restrict the sample to households that did not own an electric stove at baseline, while columns (5)–(8) restrict it to households that already owned an electric stove at baseline. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively.

Table E11: Spillovers: Effects of transformer treatment intensity on control households

	(1) Adoption	(2) Bought Ind. Compatible Cookware	(3) Induction Stove
Control in PropTG_mid	-0.0801	-0.0739	-0.0493
	(0.141)	(0.133)	(0.284)
Control in PropTG_high	-0.105	-0.0633	-0.0472
	(0.051)	(0.191)	(0.297)
Observations (Households)	1878	1878	1878

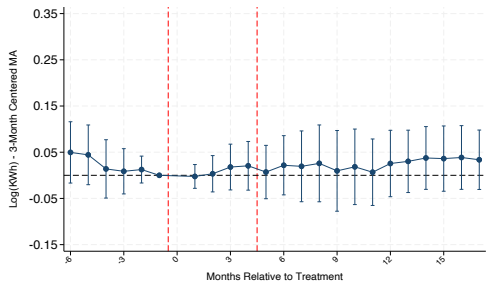
Notes: This is the spillovers extension to Table 2 in the manuscript, except these analyses are performed for the households on the eastern side of the Bishnumati River, as that is the region of Kathmandu Valley for which we have data on the transformers to which households are served. Transformer (abbreviated as "TG") treatment intensity is found by dividing the total number of households randomly assigned to any of the subsidy treatments within a transformer by the total number of NEA customers served by that transformer. We create 3 binary indicators to represent bins of treatment intensity: `PropTG_low`, which equals 1 if the transformer treatment intensity is less than 0.01 and zero otherwise; `PropTG_mid` which equals 1 if the intensity is between 0.01 and 0.05 and zero otherwise; and `PropTG_high`, which equals 1 if the treatment intensity is greater than 0.05 and zero otherwise. We then create binary indicators for the interactions each of the 9 groups of households within the three transformer treatment intensities. Each regression controls for the treated households in the three intensities of transformer treatment. The two coefficients of interest are "Control in PropTG_mid" and "Control in PropTG_high" which are control households in transformers with mid- and high-treatment intensities. These can be interpreted as relative to the omitted group, which is comprised of control households in `PropTG_low`, which are the households assigned to control status located within the transformers with the lowest proportion treated.

Table E12: Treatment Effect on Cooking Behavior — Full Sample

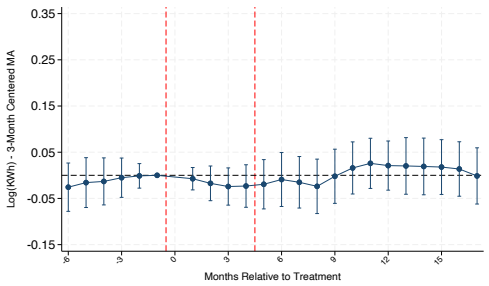
	(1) Elec. Time Cooking	(2) LPG Time Cooking
High Subsidy	17.39***	-7.340
	(5.364)	(4.722)
Low Subsidy	14.27***	-9.978**
	(5.494)	(4.663)
Cost Info	3.852	-0.758
	(3.351)	(3.322)
Health Info	0.207	-3.152
	(3.186)	(3.288)
High Subsidy & Cost Info	13.72***	-6.416
	(5.080)	(4.417)
Low Subsidy & Cost Info	7.393	-7.595
	(5.418)	(4.754)
High Subsidy & Health Info	16.06***	-8.226*
	(5.749)	(4.906)
Low Subsidy & Health Info	10.24**	-6.339
	(4.894)	(4.698)
Constant	47.72***	120.0***
	(2.306)	(2.338)
Observations	2740	2731

Notes: The table presents OLS estimates of treatment effects on cooking behavior outcomes. The outcome variables are (1) time spent cooking (minutes) using electric appliances, (2) time spent cooking (minutes) using an LPG stove. The sample includes all households that completed the follow-up survey independently of their electricity connection capacity. Standard errors in parentheses. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively.

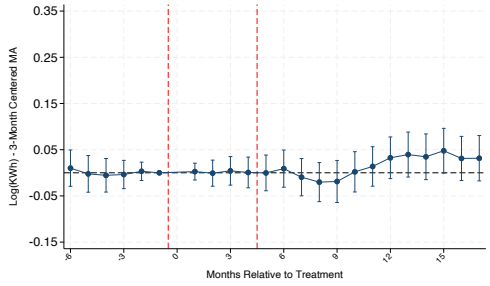
Figure E6: Event Study Estimates of Treatment Effects on Log Electricity Consumption — Full Sample



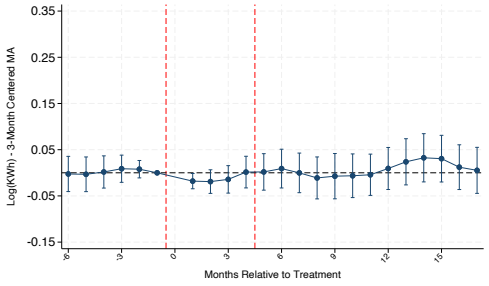
(a) High Subsidy Only



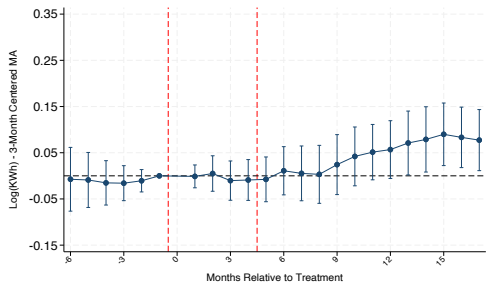
(b) Low Subsidy Only



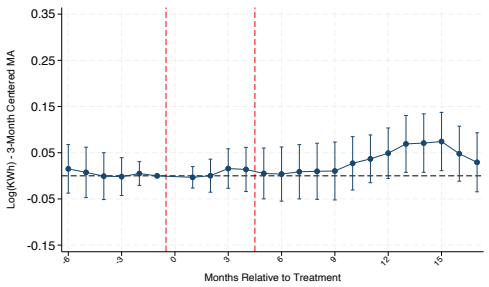
(c) Cost Information Only



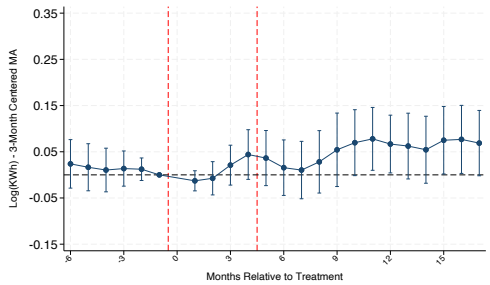
(d) Health Information Only



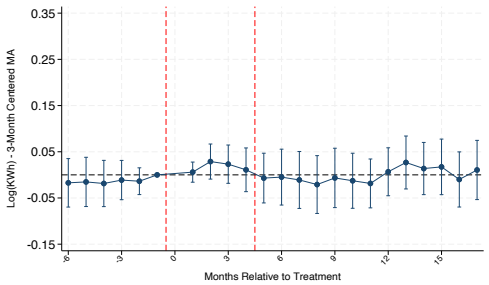
(e) Cost Information & High Subsidy



(f) Cost Information & Low Subsidy



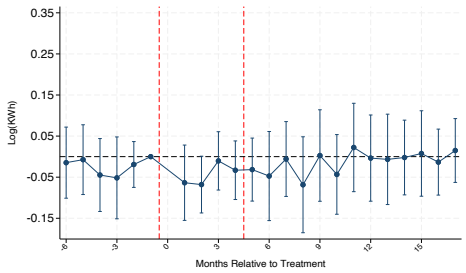
(g) Health Information & High Subsidy



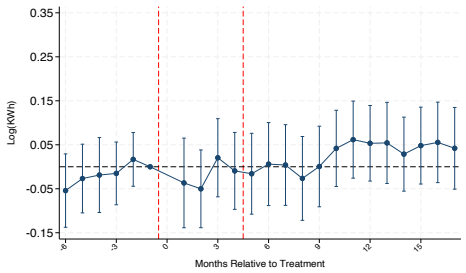
(h) Health Information & Low Subsidy

Notes: This figure plots event study estimates of the effect of each treatment arm on log electricity consumption (three-month centered moving average), based on Equation (2). Each panel compares a specific treatment arm relative to the control group. The reference period (omitted category) is December 2023. Vertical red dotted lines indicate the intervention window. Standard errors are clustered at the household level. Whiskers represent 95% confidence intervals. The sample includes all households that completed the follow-up survey independently of their electricity connection capacity.

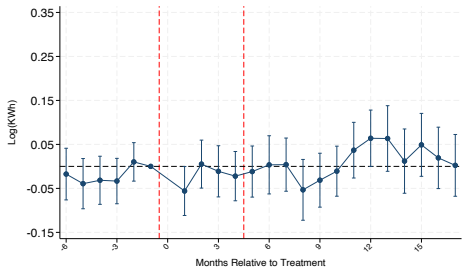
Figure E7: Robustness: Event Study Estimates of Treatment Effects on Log Electricity Consumption (Raw, No MA)



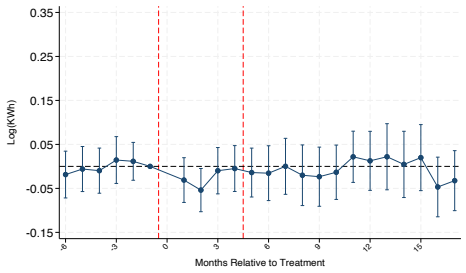
(a) High Subsidy Only



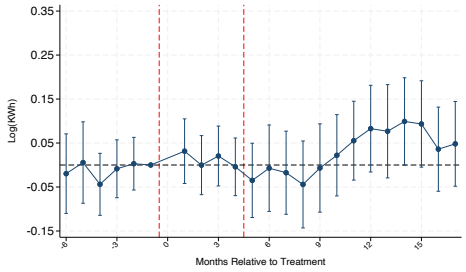
(b) Low Subsidy Only



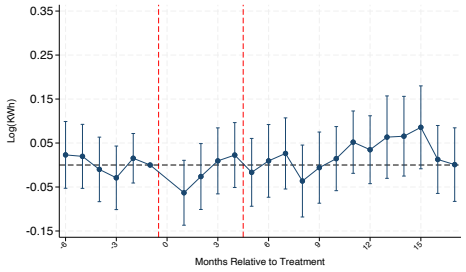
(c) Cost Information Only



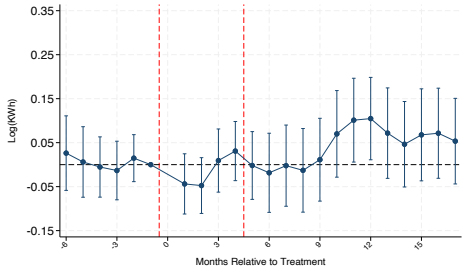
(d) Health Information Only



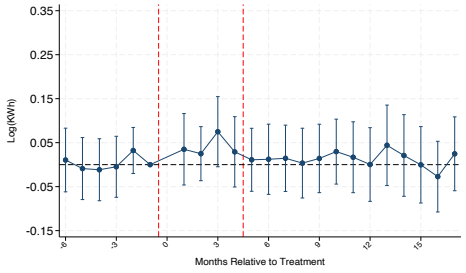
(e) Cost Information & High Subsidy



(f) Cost Information & Low Subsidy



(g) Health Information & High Subsidy



(h) Health Information & Low Subsidy

Notes: This figure plots monthly event study estimates of the effect of treatment on log electricity consumption, based on Equation (2). Each panel compares a specific treatment arm relative to the control group. The reference period is December 2023. Unlike the main paper figure, the outcome variable is raw winsorized log kWh rather than a three-month centered moving average. Vertical red dotted lines demarcate the intervention deployment period. Standard errors are clustered at the household level. Whiskers represent 95% confidence intervals.

Table E13: DiD Estimates — Outcome: log(kWh), Full Sample

	(1) Short-run	(2) Long-run
High Subsidy	-0.0286	0.00247
	(0.0246)	(0.0305)
Low Subsidy	0.000281	0.0272
	(0.0203)	(0.0279)
Cost Info	-0.0177	0.0211
	(0.0140)	(0.0213)
Health Info	-0.0189	0.0121
	(0.0148)	(0.0230)
High Subsidy & Cost Info	0.00370	0.0732**
	(0.0266)	(0.0360)
Low Subsidy & Cost Info	-0.00982	0.0326
	(0.0190)	(0.0288)
High Subsidy & Health Info	0.0143	0.0477
	(0.0220)	(0.0325)
Low Subsidy & Health Info	0.00282	0.0246
	(0.0184)	(0.0260)
Observations	64081	45294
Period	Short-run	Long-run
Num. Households	2706	2706
Household FE	Yes	Yes
Month-Year FE	Yes	Yes
Control Mean (kWh)	199.37	199.37

Notes: Pre-period: January–December 2023. Short-run post-period: January–December 2024. Long-run post-period: January–May 2025. Outcome is winsorized log(kWh). All specifications include household and month-year fixed effects; standard errors are clustered at the household level. Control mean is the mean of winsorized kWh for the pure control group (treatment arm 9) over the pre-period. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively.

Table E14: Treatment Effects on Log(kWh) by Adopter Type

	(1) Log(kWh)	(2) Log(kWh)	(3) Log(kWh)	(4) Log(kWh)
High Subsidy	-0.00132	-0.00746	-0.0443	0.114*
	(0.0271)	(0.0448)	(0.0526)	(0.0647)
Low Subsidy	0.0224	0.0512	-0.0288	0.136*
	(0.0222)	(0.0650)	(0.0384)	(0.0711)
Cost Info	0.00392	0.0213	-0.0197	0.131***
	(0.0159)	(0.0296)	(0.0419)	(0.0497)
Health Info	-0.0122	0.0448	0.0294	0.140
	(0.0176)	(0.0333)	(0.0513)	(0.103)
High Subsidy & Cost Info	0.0423	-0.0400	0.0508	0.0891**
	(0.0325)	(0.127)	(0.0465)	(0.0369)
Low Subsidy & Cost Info	-0.000462	0.0418	-0.0136	0.0523
	(0.0232)	(0.0402)	(0.0367)	(0.0868)
High Subsidy & Health Info	0.0278	0.0163	0.0570	0.0508
	(0.0253)	(0.0605)	(0.0426)	(0.0489)
Low Subsidy & Health Info	0.0211	0.131**	0.0139	0.0794*
	(0.0209)	(0.0541)	(0.0384)	(0.0462)
Observations	62607	18022	21287	16629
Adopter Type	Full Sample	Stove	Cookware	Complete
Num. Households	2192	631	746	582
Household FE	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes
Control Mean (kWh)	206.79	206.79	206.79	206.79

Notes: The pre-treatment period spans January–December 2023, and the post-treatment period covers January 2024–May 2025. The outcome variable is winsorized log electricity consumption (kWh). Columns 1–4 present estimates for the full sample, stove-only adopters, cookware-only adopters, and complete adopters (stove and cookware), respectively. A common pure control group is included across all specifications. All regressions include household and month-year fixed effects. Standard errors are clustered at the household level. Control mean is the mean of winsorized kWh for the control group over the pre-period. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively. Table E15 shows the estimates with the full sample that completed the follow-up survey.

Table E15: Treatment Effects on Log(kWh) by Adopter Type — Full Sample

	(1) Log(kWh)	(2) Log(kWh)	(3) Log(kWh)	(4) Log(kWh)
High Subsidy	-0.0196	-0.0350	-0.0619	0.0851
	(0.0244)	(0.0383)	(0.0482)	(0.0594)
Low Subsidy	0.00811	-0.0310	-0.0401	0.130**
	(0.0208)	(0.0805)	(0.0334)	(0.0562)
Cost Info	-0.00631	0.0191	-0.0232	0.130***
	(0.0146)	(0.0293)	(0.0415)	(0.0482)
Health Info	-0.00996	0.0577	0.0214	0.140
	(0.0160)	(0.0424)	(0.0484)	(0.0919)
High Subsidy & Cost Info	0.0235	-0.0276	0.0404	0.0780**
	(0.0280)	(0.104)	(0.0389)	(0.0354)
Low Subsidy & Cost Info	0.00321	0.0173	-0.0139	0.0810
	(0.0201)	(0.0339)	(0.0321)	(0.0755)
High Subsidy & Health Info	0.0239	0.00120	0.0649	0.0493
	(0.0238)	(0.0455)	(0.0523)	(0.0430)
Low Subsidy & Health Info	0.00917	0.106**	0.0127	0.0578
	(0.0186)	(0.0443)	(0.0362)	(0.0407)
Observations	77323	22635	26191	20748
Adopter Type	Full Sample	Stove	Cookware	Complete
Num. Households	2706	792	917	726
Household FE	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes
Control Mean (kWh)	199.37	199.37	199.37	199.37

Notes: The pre-treatment period spans January–December 2023, and the post-treatment period covers January 2024–May 2025. The outcome variable is winsorized log electricity consumption (kWh). Columns 1–4 present estimates for the full sample, stove-only adopters, cookware-only adopters, and complete adopters (stove and cookware), respectively. All regressions include household and month-year fixed effects. Standard errors are clustered at the household level. The sample includes all households that completed the follow-up survey independently of their electricity connection capacity. Control mean is the mean of winsorized kWh for the control group over the pre-period. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively.

Table E16: DiD Estimates, Heterogeneity by Adopter Type — Outcome: log(kWh)

	Panel A: Short-run			Panel B: Long-run		
	(1)	(2)	(3)	(4)	(5)	(6)
High Subsidy	-0.0540	-0.0697	0.0667	0.0105	-0.0433	0.131**
	(0.0339)	(0.0528)	(0.0609)	(0.0652)	(0.0470)	(0.0653)
Low Subsidy	-0.0569	-0.0346	0.133**	0.0319	-0.0532	0.122
	(0.0858)	(0.0338)	(0.0540)	(0.0729)	(0.0449)	(0.0876)
Cost Info	0.00253	-0.0426	0.111**	0.0578	0.0241	0.180***
	(0.0267)	(0.0428)	(0.0462)	(0.0426)	(0.0507)	(0.0592)
Health Info	0.0353	0.00885	0.123	0.113**	0.0520	0.177
	(0.0422)	(0.0482)	(0.0843)	(0.0479)	(0.0633)	(0.146)
High Subsidy & Cost Info	-0.0485	0.0175	0.0534*	0.0233	0.0999*	0.138**
	(0.103)	(0.0369)	(0.0300)	(0.110)	(0.0525)	(0.0639)
Low Subsidy & Cost Info	-0.0196	-0.0150	0.0805	0.106**	-0.0117	0.0822
	(0.0340)	(0.0294)	(0.0720)	(0.0518)	(0.0443)	(0.0877)
High Subsidy & Health Info	0.00911	0.0411	0.0355	-0.0163	0.123**	0.0825
	(0.0416)	(0.0510)	(0.0373)	(0.0669)	(0.0620)	(0.0615)
Low Subsidy & Health Info	0.0915**	0.0154	0.0338	0.141**	0.00705	0.115**
	(0.0453)	(0.0380)	(0.0413)	(0.0692)	(0.0430)	(0.0520)
Observations	18753	21706	17194	13261	15345	12158
Adopter Type	Stove	Cookware	Complete	Stove	Cookware	Complete
Num. Households	792	917	726	792	917	726
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean (kWh)	199.37	199.37	199.37	199.37	199.37	199.37

Notes: Panel A covers the short-run post-period (January–December 2024). Panel B covers the long-run post-period (January–May 2025). Pre-period: January–December 2023. Each column restricts the treated group to the indicated adopter type (Stove, Cookware, or Complete) while keeping the pure control (treatment arm 9). Outcome is winsorized log(kWh). All specifications include household and month-year fixed effects; standard errors are clustered at the household level. Control mean is the mean of winsorized kWh for the pure control group over the pre-period. Significance at 1%, 5% and 10% are indicated by ***, ** and *, respectively.

F Back-of-the-envelope Calculations: Electricity Consumption and Time Cooking with Electric Induction Stoves

To put our results in context and check whether the impacts that we are estimating are of reasonable magnitudes, we calculate back-of-the-envelope estimates of the number of hours a household would need to use an electric induction stove to increase their electricity bill by certain quantities (kWh) per month.

1 Increases in Billed Consumption

To fix ideas, we take the coefficient of interest from Table 4 to understand the estimated longer-run increases in billed consumption that result from a treatment. "High Subsidy & Cost Info" is of particular interest in the longer-run as it has both the largest impact on billed electricity consumption in the causal analyses using the full sample (Column 2) and continues to be of substantial magnitude and statistically significant in the heterogeneity analyses with the subsample of complete adopters in Table 5, Column 6.

First, we take the coefficient on "High Subsidy & Cost Info" in the full sample (0.093). Multiplying this coefficient by the control group mean implies an increase of $\approx$ 19.23 kWh per month (206.79 kWh $\times$ 0.093 = 19.23 kWh) in billed electricity consumption resulting from a household receiving the cost information along with the higher subsidy.

Next, we focus on the subsample of complete adopters. Multiplying this coefficient (0.149) for the same group ("High Subsidy & Cost Info") by the control group mean implies an increase of $\approx$ 30.81 kWh per month (206.79 kWh $\times$ 0.149 = 30.81 kWh) in billed electricity consumption resulting from a household receiving the cost information along with the higher subsidy.

2 Translating Consumption Increases into Cooking Time

We want to use the billed electricity consumption increases from the prior subsection to understand the implied increase in electric cooking time required in order to achieve these consumption increases. For the purposes of these calculations, we assume that:

- households that adopt an electric stove during the study period are using a single burner electric induction cooktop stove;[29]
- these induction stoves have a rated input power between 1600W (1.6kW) and 2000W (2 kW);[30]
- all of the increase in billed electricity is due to electric cooking.

[29] These were the models most commonly sold in the markets, including at our retail partner's stores, during the experiment.

[30] At the time of the experiment, the models of single-burner induction stoves available for purchase at the partner store had a rated input power between 1600W (1.6kW) and 2000W (2 kW).

Given that induction stoves do not always run at full rated power for an entire cooking session (they cycle on and off to hold a temperature and may have certain settings with lower power levels that draw considerably less), it is feasible that the average draw during use of these stoves is half of the rated power (in other words, between 0.8–1.0 kW). For this reason, we calculate the number of additional minutes of electric cooking that would translate into 19.1 and 30.6 kWh increases in monthly electricity bills for two scenarios:

1. Using a stove of a given rated power, when running at full rated power continuously;
2. Using a stove of a given rated power, when running, on average, at half rated power.

Results from the calculations for these two scenarios are in Table F1. In scenario 1, when the stove is running at full rated power, a 19.2 kWh increase in a household's monthly electricity bill corresponds with an additional 19 to 24 minutes of electric cooking per day, depending on the stove's rated power. A 30.8 kWh increase in a household's monthly electricity bill corresponds with an additional 31 to 39 minutes of electric cooking per day, depending on the stove's rated power. Scenario 2 essentially doubles those cooking times.

Notably, if households were using double-burner stoves, rather than single-burner stoves as assumed for these calculations, the increases in electricity bills would be achieved with less time spent (essentially half) on electric cooking. It is plausible that households are using double-burner stoves, as they were readily available in the stores at the time of the experiment.

Table F1: Increase in monthly electricity consumption by stove operating assumption

	(1)	(2)	(3)	(4)
			Electric cooking time in	
	Electricity bill increase (kWh per month)	Rated stove power (kW) running at	hours per month	minutes per day
Scenario 1: Assume running at full rated power continuously	19.2	1.6	12	24
	19.2	2	9.6	19.2
	30.8	1.6	19.25	38.5
	30.8	2	15.4	30.8
Scenario 2: Assume running at half power, on average	19.2	0.8	24	48
	19.2	1	28.5	38.4
	30.8	0.8	38.5	77
	30.8	1	30.8	61.6

G Additional Details on Structural Model

This appendix provides additional derivations and assumptions underlying the structural model presented in Section 8 of the main text. We expand on the household problem, the adoption equation, and the social planner's problem to make clear how each functional form arises.

1 Household Problem

Each household i in group g chooses between two cooking modes, LPG ($m = L$) and electricity ($m = E$), and an intensity of use T_{mg}. Utility has three components: (i) consumption utility net of perceived costs, (ii) transfers from subsidies if applicable, and (iii) idiosyncratic taste shocks:

$$\log U_{img} = \log C_{mg} + \log S_{mg} + \log \varepsilon_i. \tag{1}$$

Consumption utility. We assume consumption utility is

$$C_{mg} = T_{mg}^{\eta} - P_{mg}\, T_{mg}, \tag{2}$$

where P_{mg} is the perceived unit cost of cooking (monetary cost plus any perceived health costs) and $\eta \in (0, 1)$ governs the curvature.

The household chooses T_{mg} to maximize C_{mg}. Taking the FOC:

$$\frac{\partial C_{mg}}{\partial T_{mg}} = \eta T_{mg}^{\eta-1} - P_{mg} = 0. \tag{3}$$

Solving gives the optimal intensity of use:

$$T_{mg}^{*} = \left(\frac{\eta}{P_{mg}}\right)^{\frac{1}{1-\eta}}. \tag{4}$$

Indirect consumption utility. Plugging T_{mg}^{*} back into C_{mg} yields indirect consumption utility:

$$C_{mg}^{*} = \eta \left(\frac{\eta}{P_{mg}}\right)^{\frac{\eta}{1-\eta}} - P_{mg} \left(\frac{\eta}{P_{mg}}\right)^{\frac{1}{1-\eta}}. \tag{5}$$

This simplifies to

$$C_{mg}^{*} = \bar{\eta}\, P_{mg}^{\frac{-\eta}{1-\eta}}, \tag{6}$$

where $\bar{\eta} \equiv \eta^{\frac{\eta}{1-\eta}} - \eta^{\frac{1}{1-\eta}}$ is a positive constant that normalizes consumption utility.

Transfers Transfers from subsidies are denoted S_{mg}. For LPG, $S_{Lg} = 1$. For electricity, subsidies enter concavely and may interact with household and group characteristics:

$$\log S_{Eg} = s_{Eg}^{\rho}(1 + \gamma_P\, P_{Eg} + \gamma_X\, X_g + \gamma_A\, A_{-i,g}), \tag{7}$$

with $\rho \in (0,1)$ capturing concavity of subsidy value.

Indirect utility Indirect utility for household i from choosing mode m is then

$$V_{mg} = \bar{\eta} P_{mg}^{\frac{-\eta}{1-\eta}} S_{mg}. \tag{8}$$

The full utility is $U_{img} = V_{mg}\varepsilon_i$.

2 Adoption Equation

Distributional assumption We assume the idiosyncratic shocks ε_i are drawn from a Fréchet distribution with shape parameter $\theta > 0$. This is a standard assumption in random utility models because it yields tractable choice probabilities. In particular, the "logit trick" implies:

$$\Pr(m = E) = \frac{V_{Eg}^{\theta}}{V_{Eg}^{\theta} + V_{Lg}^{\theta}}, \qquad \Pr(m = L) = \frac{V_{Lg}^{\theta}}{V_{Eg}^{\theta} + V_{Lg}^{\theta}}. \tag{9}$$

Adoption share Thus the adoption share of electricity is

$$\pi_{Eg} = \frac{V_{Eg}^{\theta}}{V_{Eg}^{\theta} + V_{Lg}^{\theta}}. \tag{10}$$

Taking the odds ratio:

$$\frac{\pi_{Eg}}{1 - \pi_{Eg}} = \left(\frac{V_{Eg}}{V_{Lg}}\right)^{\theta}. \tag{11}$$

Since $V_{Lg} = \bar{\eta}$ and $V_{Eg} = \bar{\eta} P_{Eg}^{\frac{-\eta}{1-\eta}} S_{Eg}$, the ratio simplifies to

$$\frac{\pi_{Eg}}{1 - \pi_{Eg}} = \left(P_{Eg}^{\frac{-\eta}{1-\eta}} S_{Eg}\right)^{\theta}. \tag{12}$$

Log-odds form Taking logs, we obtain the estimating equation for adoption:

$$\log \frac{\pi_{Eg}}{1 - \pi_{Eg}} = -\frac{\eta\theta}{1-\eta} \log P_{Eg} + \theta s_{Eg}^{\rho} + \theta\gamma_P(s_{Eg}^{\rho} P_{Eg}) + \theta\gamma_X(s_{Eg}^{\rho} X_g) + \theta\gamma_A(s_{Eg}^{\rho} A_{-i,g}). \tag{13}$$

Equation (13) highlights the roles of perceived costs, subsidies, and heterogeneity in determining adoption probabilities.

3 Electricity Usage Decomposition

The adoption equation determines which households take up the stove. Among adopters, what matters for policy is whether and how much electricity usage changes. This subsection

details the decomposition of post-adoption usage introduced in equation (5).

From adoption to usage. Among adopters, electricity usage for cooking is governed by optimal cooking time $T_{it}^* = (\eta/\tilde{P}_{it})^{1/(1-\eta)}$, where $\tilde{P}_{it}$ is the effective perceived cost of electric cooking. Total billed electricity for cooking is $kWh_{it}^{\text{cook}} = \alpha\, T_{it}^*$, with α converting cooking hours to kilowatt-hours.

Estimating equation. The reduced-form estimating equation for electricity usage among adopters is:

$$\log kWh_{it} = \alpha + \delta_r R_i + \delta_L\, g(t) + X_i'\gamma + \mu_c + \epsilon_{it}, \tag{14}$$

where R_i is baseline reliability belief (Likert 1–10), $g(t) = 1 - e^{-\kappa t}$ is the learning curve with t months since intervention, X_i includes nine baseline household characteristics (household size, head age, higher education, homeownership, LPG consumption, cooking time, number of electric appliances, meals cooked on LPG, and baseline stove ownership), and μ_c is a cluster fixed effect (169 clusters). Standard errors are clustered at the household level. Billing data captures *total* household electricity, not just cooking kWh.

Estimation proceeds in two steps. First, a grid search over $\kappa \in [0.01, 2.0]$ (step 0.01) computes $g_\kappa(t) = 1 - e^{-\kappa t}$ for each candidate and estimates equation (14) by OLS with cluster fixed effects; the κ minimizing SSR is selected. Second, we report the coefficients at the optimal κ.

Identification. Each parameter draws on a distinct source of variation. The reliability coefficient δ_r comes from cross-sectional variation in baseline beliefs among adopters: comparing two households observed at the same month since adoption, one with high outage concern and one with low, the difference in electricity usage after controlling for observables identifies δ_r. Because the RCT randomizes subsidy receipt independently of baseline beliefs, adoption timing does not confound this comparison.

The learning parameters κ and δ_L are identified from the within-household time path of billed electricity. With monthly observations from month 0 through 14, the trajectory shape pins down κ (convergence speed) and δ_L (direction and magnitude). The sign of δ_L is unrestricted: $\delta_L > 0$ indicates usage grows with experience, $\delta_L < 0$ indicates reversion after initial experimentation.

Preference residual. The residual $\hat{\nu}_i^{\text{reg}} = T_i^{-1} \sum_t \hat{\epsilon}_{it}$ is the time-averaged residual from equation (14), capturing household-level electricity demand after accounting for reliability, learning, and observables. Because X_i controls for income, appliance ownership, and household size, a large positive $\hat{\nu}_i^{\text{reg}}$ reflects genuinely high intrinsic demand rather than economic constraints. Note the sign convention: in the cost equation (5), high ν_i means high perceived cost (low demand), while in the kWh regression a positive residual means high usage. We follow the cost-equation convention in the main text, where low ν_i indicates strong demand for electric cooking.

4 Social Planner's Problem

The social planner chooses subsidy levels s_{Eg} and information treatments I^c_{Eg}, I^h_{Eg} to maximize aggregate welfare. Higher electricity use reduces reliance on LPG and improves health, but additional electricity demand is not costless when generation or distribution capacity is constrained. The planner therefore internalizes the marginal social cost of supplying electricity as well as the fiscal cost of subsidies.

The planner's objective is:

$$\max_{s_{Eg}, I^c_{Eg}, I^h_{Eg}} \quad \sum_i U_{ig} - \delta kWh_{Eg} - \lambda(1 - \pi_{Eg}) - (s_{Eg} + \psi s^2_{Eg}), \tag{15}$$

where δ is the marginal social cost of supplying electricity when capacity is constrained, λ captures the social cost of continued LPG reliance, and ψ is a convexity parameter that penalizes high subsidy rates, deterring the planner from concentrating large per-household transfers on a small group.

Billed electricity consumption. Total long-run billed electricity consumption is:

$$kWh_{Eg} = K^0_g + \pi_{Eg}\alpha T^*_{Eg} + \pi_{Eg}(H_g - 1)K^0_g, \tag{16}$$

where K^0_g is baseline electricity demand and H_g is the post-adoption electrification factor defined in the main text.

Expanded formulation. Substituting the expressions for indirect utility and billed electricity, the planner's problem can be written as:

$$\max_{s_{Eg}, I^c_{Eg}, I^h_{Eg}} \quad \underbrace{\bar{\eta}\left[\left(P_{Eg}^{\frac{-\eta}{1-\eta}} S_{Eg}\right)^{\theta} + 1\right]^{1/\theta}}_{\text{(i) Household option value}} \tag{17}$$

$$- \underbrace{\delta\, kWh_{Eg}}_{\text{(ii) Capacity-constrained supply cost}} \tag{18}$$

$$- \underbrace{\lambda(1 - \pi_{Eg})\bar{\eta}}_{\text{(iii) LPG dependence}} \tag{19}$$

$$- \underbrace{(s_{Eg} + \psi s^2_{Eg})}_{\text{(iv) Subsidy cost}}. \tag{20}$$

First-order condition with respect to s_{Eg}. Differentiating with respect to s_{Eg}:

$$\frac{\partial W}{\partial s_{Eg}} = \frac{\partial \pi_{Eg}}{\partial s_{Eg}}\left[\bar{\eta} P_{Eg}^{\frac{-\eta}{1-\eta}} - \delta\left(\alpha T^*_{Eg} + (H_g - 1)K^0_g\right) + \lambda\bar{\eta}\right] - (1 + 2\psi s_{Eg}). \tag{21}$$

The optimal subsidy balances three forces: the welfare gain from increasing adoption, the marginal social cost of additional electricity demand under capacity constraints, and the fiscal cost of subsidies.

Calibration. Four social-planner parameters are not identified by the RCT: the grid congestion cost δ, the social cost of LPG dependence λ, the subsidy-convexity penalty ψ, and the planning horizon T. We calibrate each from external sources (Table G2); B and K_{bundle} are taken from RCT records and retail prices. The welfare ranking depends on relative comparisons across policies, not absolute levels, so the conclusions are robust to wide variation in these choices (Appendix Table G3).

Table G2: Calibration of Social Planner Parameters

Parameter	Value	Units	Source
B (subsidy budget)	2,180,000	NPR	RCT disbursement
K_{bundle} (bundle cost)	7,000	NPR	Store retail
T (planning horizon)	24	months	NPC (2024)
λ (LPG dependence)	10,000	NPR / HH	LPG savings
δ (grid congestion)	5,000	NPR / HH-share^2	NEA (2025)
ψ (subsidy convexity)	1.0	scalar	Penalty on s^2

B: 546 HHs × NPR 3,000 (high subsidy) + 542 HHs × NPR 1,000 (low subsidy).
K_{bundle}: induction stove NPR 5,000–6,000 + cookware NPR 1,000–2,500.
T: half of Government of Nepal's 2030 target of 25% household electric-cooking adoption (National Planning Commission, 2024).
λ: 12 NPR/kWh cooking heat × 40 kWh/HH/month × 24 months. The 12 NPR/kWh = NPR 150/kg LPG (Nepal Oil Corporation, 2024) ÷ 14 kWh/kg (IEA Advanced Motor Fuels Technology Collaboration Programme, 2024) ÷ 55% burner efficiency (Bhatia and Angelou, 2015).
δ: implies ∼4 NPR/kWh marginal cost at full adoption; conservative relative to NEA's 6.08 NPR/kWh average power purchase and 7.69 NPR/kWh India import (Nepal Electricity Authority, 2025); consistent with the 5.77 NPR/kWh evening-peak premium in Kandel et al. (2025).
ψ: convexity parameter on the fiscal cost of subsidies. Enters as $\psi \cdot N_t \cdot s^2$, deterring concentration of large per-household transfers. Set to 1.0, the welfare ranking is invariant to $\psi \in [0.5, 2.0]$ because optimal subsidy fractions stay well below the convex region (Appendix Table G3).
Sensitivity ranges and welfare-ranking robustness are reported in Appendix Table G3.

Table G3: Welfare Sensitivity to Calibration Parameters

Parameter	Setting	RCT (=1.00)	Uniform subsidy	CATE top-N	Bundle (random)
Grid congestion cost (δ), NPR / HH-share^2					
	0	1.00	1.31	1.41	1.65
	2,500	1.00	1.28	1.41	1.69
	5,000*	1.00	1.25	1.41	1.73
	10,000	1.00	1.19	1.41	1.81
LPG dependence (λ), NPR / HH					
	5,000	1.00	1.17	1.46	1.93
	10,000*	1.00	1.25	1.41	1.73
	15,000	1.00	1.31	1.39	1.58
	20,000	1.00	1.35	1.37	1.48
Subsidy convexity (ψ), scalar					
	0.5	1.00	1.25	1.41	1.73
	1.0*	1.00	1.25	1.41	1.73
	2.0	1.00	1.25	1.41	1.73
Planning horizon (T), months					
	12	1.00	1.25	1.40	1.69
	18	1.00	1.25	1.40	1.71
	24*	1.00	1.25	1.41	1.73
	36	1.00	1.25	1.41	1.74
	60	1.00	1.25	1.41	1.74
Bundle cost (K_{bundle}), NPR					
	5,000	1.00	1.25	1.41	2.51
	7,000*	1.00	1.25	1.41	1.73
	10,000	1.00	1.25	1.41	1.14

Cells report welfare relative to the actual RCT under one perturbation of each calibration parameter, holding all others at baseline. Relative welfare is the gain over the no-policy benchmark, normalized to the RCT's gain. Baseline values (*): $\delta = 5{,}000$, $\lambda = 10{,}000$, $\psi = 1.0$, $T = 24$, $K_{\text{bundle}} = 7{,}000$.

Policies: *Uniform subsidy* optimizes a population-wide subsidy rate. *CATE top-N* targets households by predicted adoption response. *Bundle (random)* gives a free stove+cookware bundle to a budget-constrained random subset of households (all of whom become Complete adopters).

Varying the LPG-displacement assumption (kWh/HH/month) is subsumed by the λ sweep, since λ is calibrated as 12 NPR/kWh $\times$ kWh/HH/month $\times$ horizon.

The Bundle policy maximizes welfare in 18 of 19 cells (95%). The exception is $K_{\text{bundle}} = 10{,}000$, where the budget buys too few bundles ($N_B < N_{\text{CATE-targeted}}$) and CATE top-$N$ targeting overtakes.

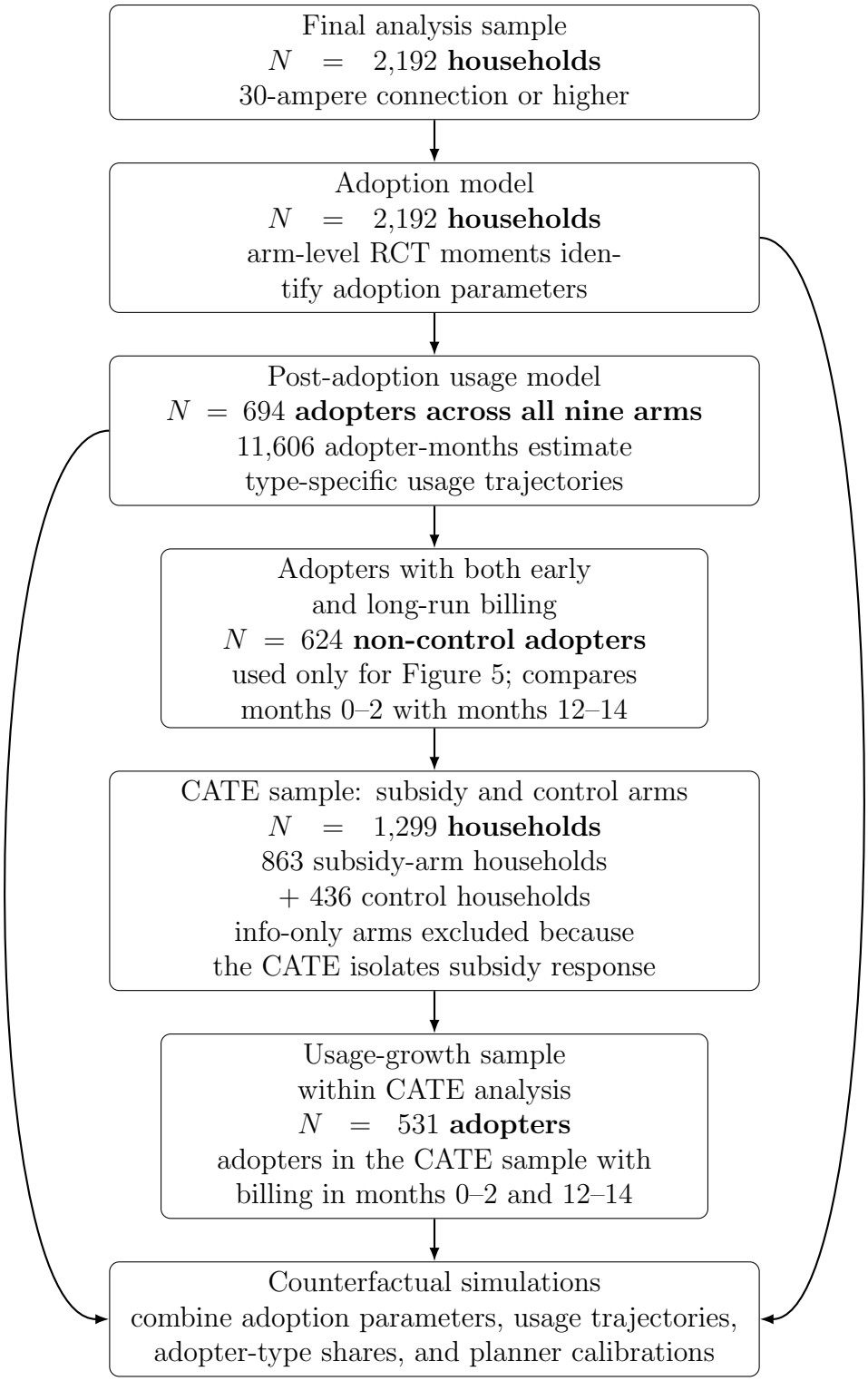


Figure G1: Structural Model Samples and Inputs

Notes: The adoption model uses the full 30-ampere analysis sample. Cost and health information treatments are pooled in the structural adoption step to estimate a single information shift; the reduced-form tables report treatment-arm-specific effects. The usage model uses all adopters across all nine randomized arms and estimates trajectories from the monthly billing panel. The $N = 624$ sample is not a separate model input; it is the non-control adopter sample used for Figure 5, which compares realized changes between months 0–2 and 12–14. The CATE analysis uses the subsidy and control arms to estimate which households respond most to the subsidy. Its $N = 1{,}299$ sample equals 863 subsidy-arm households plus 436 control households. Within that sample, 531 adopters have billing data in both the early and long-run windows; the prediction of kWh growth is trained on the 461 subsidy-arm adopters in this group.

H Estimation Procedure and Parameter Estimates

This appendix details how we identify and estimate the structural parameters in Section 8.

1 Adoption: Identification and Estimation

The adoption equation (4) has four parameters $\{\rho, \theta, \eta, \gamma_P\}$, identified from four RCT treatment arms through a simultaneous system.

Step 1: Intercept and price sensitivity. The control group ($s = 0$, $P = 1$) pins down $\beta_0 = \text{logit}(\pi_{\text{control}})$. The information-only arm ($s = 0$, $P = 0.85$) shifts only perceived cost, identifying η through $\beta_P = (\text{logit}(\pi_{\text{info}}) - \beta_0)/\log(0.85)$, where $\beta_P = -\eta\theta/(1-\eta)$.

Step 2: Subsidy and interaction. The high-subsidy arm ($s = 3000/7000$, $P = 1$) and high-subsidy-plus-info arm ($s = 3000/7000$, $P = 0.85$) jointly identify θ and γ_P from:

$$\gamma_P = \frac{d_{HI} - d_H}{d_H \cdot P_{\text{info}} - d_{HI}}, \qquad \theta = \frac{d_H}{s_H^{\rho}(1+\gamma_P)},$$

where $d_H = \text{logit}(\pi_H) - \beta_0$ and $d_{HI} = \text{logit}(\pi_{HI}) - \beta_0 - \beta_P \log(P_{\text{info}})$.

Step 3: Grid search over ρ. For each $\rho \in [0.10, 1.00]$ (step 0.01), we solve the system above and compute out-of-sample prediction error on the two arms not used in identification: low subsidy and low subsidy + info. The ρ minimizing this error is selected ($\hat{\rho} = 0.55$).

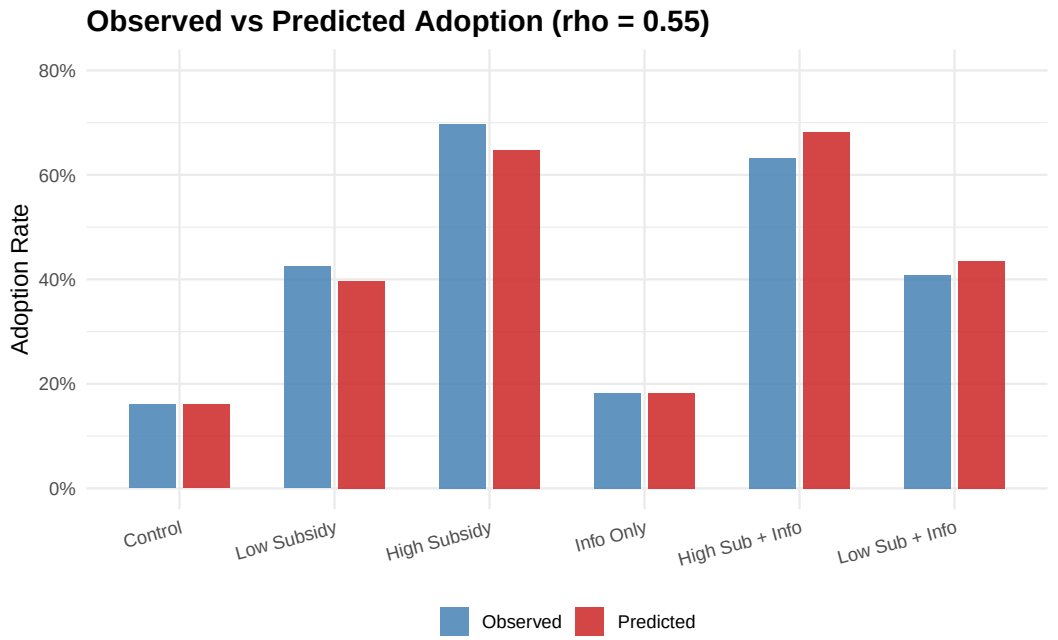


Figure H2: Model Fit: Observed and Predicted Adoption Rates by Treatment Arm

Notes: Bars show observed and model-predicted adoption rates for each of the six RCT treatment arms (N = 2,192 households). Observed rates are from the endline survey. Predicted rates are from the structural adoption equation (4) with parameters $\{\rho, \theta, \eta, \gamma_P\}$ identified from four arms (control, info-only, high subsidy, high subsidy + info). Low subsidy and low subsidy + info are out-of-sample validation arms. Maximum out-of-sample prediction error is approximately three percentage points at $\hat{\rho} = 0.55$.

Table H4: Adopter Composition by Treatment Arm

	Control	Info Only	Subsidy Only	Info + Low Sub	Info + High Sub
Adoption					
Total HHs	436	893	288	284	291
Any adoption (%)	16.1	18.3	55.9	40.8	63.2
Complete adoption (%)	3.2	3.2	11.8	9.2	19.6
Among adopters					
% complete (stove + cookware)	20	17.8	21.1	22.4	31
% first-time (among complete)	71.4	100	79.4	84.6	84.2
Revealed demand ($\hat{\nu}_i$)					
All adopters	0.065	0.062	-0.076	0.007	-0.019
Complete adopters	0.169	0.115	-0.061	0.124	0.04

Notes: Adoption is defined as purchasing an electric induction stove, cookware, or both by the endline survey. Complete = purchased both stove and induction-compatible cookware. First-time = did not own an electric stove at baseline. $\hat{\nu}_i$ is the household-level time-averaged residual from the usage equation (5), estimated on Nepal Electricity Authority billing data with HCLUSTER_2 fixed effects and standard errors from HH-level cluster bootstrap; higher values indicate stronger revealed demand for electric cooking conditional on baseline reliability beliefs and observable household characteristics. Info Only pools cost-information and health-information arms; Info + Low Sub and Info + High Sub similarly pool across information type. Sample restricted to households with electrical service of 30 A or above (i.e., capable of running an induction stove); 5 A and 15 A connections are excluded throughout. Sample size: N = 2,192 households across arms 1–9. Companion to Table 7 (which collapses Control into the cross-arm comparison) and to Panel C of Table 6.

Table H5: Parametric Learning-Curve Estimates by Adopter Bundle (Appendix Companion to Table 6, Panel C)

Adopter Type	N HHs	$\hat{\kappa}$ (boot SE)	$\hat{\delta}_L$ (analytic SE)	$\hat{\delta}_L$ (boot SE)
Pooled	694	1.47 (0.14)	−0.109 (0.016)	−0.109 (0.016)***
Complete	160	0.11 (0.66)	+0.085 (0.044)	+0.085 (0.080)
Stove Only	197	1.55 (0.61)	−0.081 (0.032)	−0.081 (0.084)
Cookware Only	337	1.19 (0.18)	−0.165 (0.024)	−0.165 (0.020)***

Notes: Companion to Panel C of Table 6. Estimating equation: $\log \tilde{P}_{it} = \alpha + \delta_r R_i + \delta_L\, g(t;\kappa) + X_i'\beta + \mu_c + \varepsilon_{it}$ with $g(t;\kappa) = 1 - e^{-\kappa t}$ and t measured in months since intervention. R_i is the household's baseline grid-reliability belief (1–10 scale, survey item M10); X_i are baseline household covariates (size, head age, education, homeownership, baseline LPG kg, cooking time, number of electric appliances, number of meals on LPG, prior electric stove ownership); μ_c are HCLUSTER_2 fixed effects. $\hat{\kappa}$ is selected by grid search over $[0.01, 2.0]$ at step 0.01 to minimize within-HH residual SSR. Bootstrap standard errors are HH-level cluster bootstrap (100 replicates), marginalizing over $\hat{\kappa}$ uncertainty. Analytic standard errors are OLS at the grid optimum and condition on $\hat{\kappa}$. The ratio of bootstrap to analytic SE diagnoses where $\hat{\kappa}$ is well-identified versus where the trajectory is poorly fit by the saturating-exponential form (small N or non-monotone path). Complete = purchased both stove and induction-compatible cookware; Stove Only = stove without cookware; Cookware Only = cookware without stove. Outcome is winsorized log of total household monthly billed kWh from Nepal Electricity Authority data. Sample restricted to households with electrical service of 30 A or above (i.e., capable of running an induction stove). Significance from bootstrap p-values: $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

2 Post-Adoption Electricity Usage: Identification and Estimation

We estimate the usage equation (14) on 11,606 adopter-month observations from Nepal Electricity Authority billing records, covering 694 adopters across all nine treatment arms over event months 0 to 17 (December 2023 to May 2025). The estimation uses cluster fixed effects (169 clusters) to absorb group-level heterogeneity, with standard errors clustered at the household level. Because billing data captures total billed household electricity consumption rather than cooking alone, the learning coefficient δ_L reflects changes in overall electricity use patterns following stove adoption, not cooking kWh specifically.

The learning curve introduces a nonlinearity that prevents standard OLS from estimating all parameters simultaneously. We therefore proceed in two steps. First, for each candidate $\kappa \in [0.01, 2.0]$ (step size 0.01), we compute the learning curve $g_\kappa(t) = 1 - e^{-\kappa t}$ and estimate equation (14) by OLS, selecting the κ that minimizes the sum of squared residuals. Second, we report the coefficients from the regression at this optimal κ. The grid search is necessary because κ enters nonlinearly through the exponential; conditional on κ, all remaining parameters are linear and estimated by OLS in a single pass.

We then split the sample by adopter type (complete, cookware only, stove only) and re-estimate, allowing separate κ and δ_L for each type. This is the decomposition that reveals the sharply divergent learning trajectories discussed in Section 10. The identification of each parameter and the construction of the preference residual $\hat{\nu}_i$ are detailed in Appendix G.

I Heterogeneous Treatment Effects: Estimation Details

Section 9.3 summarizes our approach to estimating household-level heterogeneity in both the adoption response to subsidies and post-adoption electricity usage growth. This appendix provides the full estimation details, sample construction, and additional results.

1 Data and Definitions

Adoption: We define adoption as a binary indicator equal to one if the household purchased or received an electric induction stove by the endline survey, corresponding to the `true_adoption` variable used throughout the paper.

Electricity usage: We measure long-run electricity usage from administrative billing data provided by the Nepal Electricity Authority. For each household we compute the change in mean log(kWh) between the first three months after the intervention (months 0–2) and a long-run window 12 to 14 months post-intervention. We refer to this difference as *kWh growth*. This measure captures which households increased their billed electricity consumption over time, the behavioral margin that distinguishes genuine fuel switching from a one-time equipment purchase.

Sample: The analysis sample includes all households assigned to any subsidy arm (treatment groups 1, 2, 4, 5, 7, 8) or the pure control group (group 9), conditional on having matched billing data, yielding $N = 1{,}299$ households (531 adopters: 461 in subsidy arms, 70 in the control group). We exclude info-only groups (3 and 6) because the causal forest estimates a binary treatment effect and these households fit neither side of the subsidy contrast: they received an intervention that shifted perceived cost ($P = 0.85$ vs. $P = 1$) but no subsidy. Including them as controls would pool two different counterfactuals; including them as treated would conflate the subsidy and information margins. The structural model identifies the information effect separately through η (Section H). Since all counterfactual policies bundle information at negligible cost, the planner needs to know which households respond most to the subsidy, which is what the CATE isolates.

Covariates: Both models use the same 15 baseline covariates: household size, head age, higher education (bachelor's degree or above), homeownership, monthly LPG consumption (kg), reliability beliefs (Likert 1–10 response to "I am worried about the reliability of electricity supply"), whether unplanned outages are reported as the main electricity service problem, voltage fluctuation reported as a problem, whether the household reduced non-essential spending due to electricity costs, weekly cooking time (hours), number of electricity outages in a normal month, number of electric appliances, access to credit from a commercial or government bank, number of meals cooked on an LPG stove (out of 12 meal-slots over three days), and whether the household owned an electric stove at baseline. All covariates are from the baseline survey to avoid contamination by treatment effects. Missing values are median-imputed; all variables are standardized to mean zero and unit variance.

2 Estimation

Adoption CATE (causal): We estimate the conditional average treatment effect of subsidies on adoption using a causal forest (Athey et al., 2019; Wager and Athey, 2018), which leverages experimental randomization to estimate individual-level treatment effects $\hat{\tau}_i = \mathbb{E}[Y_i(1) - Y_i(0) \mid X_i]$, where Y_i is adoption and W_i indicates assignment to any subsidy arm. The forest uses all 1,299 households (both treated and control), since the control group provides the counterfactual adoption rate at each point in covariate space. We use 4,000 trees and the default sample-splitting procedure in `grf`, which separates the observations used to choose tree splits from those used to estimate treatment effects within leaves. This estimate is **causal**: the RCT randomization ensures that, within any region of covariate space, subsidy assignment is independent of potential outcomes.

Predicted electricity usage (predictive): Because there is no experimental variation in electricity usage intensity conditional on adoption, we cannot estimate a causal effect of treatment on usage among adopters. Instead, a regression forest trained on 461 treated adopters predicts kWh growth $\hat{y}_i$ from baseline covariates and we generate predictions for all 1,299 households in the subsidy-control CATE sample. This estimate is **predictive, not causal**: it identifies which household profiles are associated with larger increases in billed electricity, but selection into adoption is endogenous and the forest conditions on this selection.

Joint classification: Splitting the sample at the median of each dimension yields a 2×2 matrix:

- **High Adopt, High Use** ("fuel switchers"): above-median on both margins.
- **High Adopt, Low Use** ("equipment replacers"): subsidy-responsive but below-median kWh growth.
- **Low Adopt, High Use**: below-median adoption response but above-median kWh growth.
- **Low Adopt, Low Use**: below-median on both dimensions.

Table I6 reports realized outcomes, adopter composition, and demographics for the four cells. Several patterns merit discussion.

The "High/Low Adopt" labels refer to predicted *marginal response* to a subsidy ($\hat{\tau}_i$), not to adoption level. This distinction matters: realized adoption rates do not follow the labels monotonically. Low Adopt, High Use households have a realized adoption rate of 54.2%, higher than the 50.9% for High Adopt, High Use, because they tend to have higher baseline propensity (36% already own a stove) and would adopt at elevated rates regardless. High Adopt households have lower baseline stove ownership (29% for fuel switchers) but respond strongly to the subsidy on the margin.

The composition of adoption varies sharply across cells. Among fuel switchers, 39% purchased the complete bundle versus only 18% among equipment replacers, who are predominantly cookware-only adopters (60%), consistent with households that already own a stove adding cookware without changing cooking behavior. Fuel switchers also have the highest share of adopters cooking with electricity for the first time (67% vs. 53%), reinforcing

that these households are genuinely transitioning rather than replacing equipment.

The Δ log(kWh) column validates the usage dimension: fuel switchers show +0.31 log points between months 0–2 and 12–14, while equipment replacers show −0.213, consistent with experimentation followed by reversion. Table I7 reports energy behavior and targeting variables for the same cells.

Table I6: Household Types: Realized Outcomes and Household Demographics

	High Adopt, High Use	High Adopt, Low Use	Low Adopt, High Use	Low Adopt, Low Use
Cell definition				
$\hat{\tau}_i$ (adoption)	Above med.	Above med.	Below med.	Below med.
$\hat{y}_i$ (usage)	Above med.	Below med.	Above med.	Below med.
Realized outcomes (subsidy arms)				
N	226	197	225	214
Adoption rate	0.509	0.513	0.542	0.575
Δ log(kWh)	+0.310	−0.213	+0.276	−0.176
Adopter composition (%, all adopters)				
Complete	38.8	18.3	21.5	20.6
Cookware only	41.9	60.3	51.1	56.7
Stove only	19.4	21.4	27.4	22.7
First-time elec. cooker	66.7	57.9	63.0	52.5
Household demographics				
HH size	3.7	3.9	5.0	5.0
Head age	49.8	50.0	64.5	64.3
Higher education (%)	21.9	21.2	16.3	19.8
Owns stove at BL (%)	28.7	34.2	36.3	44.4
Initial log(kWh)	5.00	5.46	5.12	5.40

Notes: Households are partitioned into a 2×2 grid by median splits on (i) the adoption conditional average treatment effect $\hat{\tau}_i$, estimated by a causal forest (4,000 trees, honesty splitting) on the $N = 1{,}299$ households in subsidy and control arms; and (ii) realized post-adoption electricity-use growth, measured as the within-household change in mean log monthly billed kWh from months 0–2 to months 12–14. The $N = 1{,}299$ CATE sample equals 863 subsidy-arm households and 436 control households; info-only arms are excluded because this exercise isolates subsidy response. "High" = above median; "Low" = below median. Realized adoption rates and Δ log(kWh) are from the 862 subsidy-arm households assigned to one of the four cells. Adopter composition and first-time rates are computed over the 531 adopters in subsidy and control arms with billing data in both endpoint windows, including 461 subsidy-arm adopters and 70 control adopters. Demographics are cell means over all 1,299 households. First-time = household did not own an electric stove at baseline. Complete = purchased both stove and induction-compatible cookware. Sample restricted to households with electrical service of 30 A or above. Cell-assignment file (`cate_cell_assignments.csv`) is archived in the replication package.

Table I7: Household Types: Energy and Targeting Variables

	High Adopt, High Use	High Adopt, Low Use	Low Adopt, High Use	Low Adopt, Low Use
Energy behavior				
LPG consumption (kg/month)	11.0	10.8	12.5	12.5
Meals on LPG stove (of 12)	10.3	10.2	10.6	9.9
Cooking time (hrs/week)	12.7	13.6	14.0	14.1
Electricity perceptions				
Reliability concern (1–10)	7.01	7.23	6.63	6.73
Num. electric appliances	14.9	16.0	18.0	19.2

Notes: Cell assignments are identical to Table I6: median splits on the adoption CATE $\hat{\tau}_i$ (causal forest) and realized kWh growth from months 0–2 to months 12–14 over $N = 1{,}299$ households in subsidy and control arms. The sample equals 863 subsidy-arm households and 436 control households; info-only arms are excluded because this exercise isolates subsidy response. All values are cell-level means from the baseline survey. Reliability concern is the household's response to survey item M10 on a Likert scale (1 = not worried, 10 = very worried about electricity supply reliability), elicited at baseline. Meals on LPG stove counts how many of the 12 meal slots in the baseline cooking-diary were prepared on an LPG stove. Cooking time is total household cooking hours per week. Number of electric appliances counts all plug-in devices in working condition at baseline (excludes induction stove, which is the treatment outcome). Sample restricted to households with electrical service of 30 A or above. Companion to Table I6 (which reports realized adoption, usage, demographics, and adopter composition by cell).

3 Additional Results

Adoption heterogeneity: The mean adoption CATE is 0.368 (sd = 0.023), confirming a large average subsidy effect (Figure I4). A best linear projection (BLP) of the CATE onto covariates (Figure I5) identifies one significant predictor: households reporting unplanned outages as their main electricity problem have a 4.4 percentage point larger adoption response. Household size (−0.034), head age (−0.034), and reduced non-essential spending (−0.033) are suggestive negative predictors but not individually significant.

Electricity usage heterogeneity: The regression forest identifies a distinct set of predictive drivers (Figure I6). The number of electric appliances is the strongest predictor of kWh growth (normalized importance = 0.28), followed by cooking time (0.14), head age (0.11), and reliability beliefs (0.10). Prior stove ownership and education rank near the bottom despite being correlated with adoption, confirming that the two margins are driven by different household characteristics. This divergence in predictive drivers underlies the near-orthogonality reported in the main text.

Joint scatter: Figure I3 plots each household's adoption CATE against predicted kWh growth. The low correlation ($r = 0.20$) is visible in the near-circular cloud.

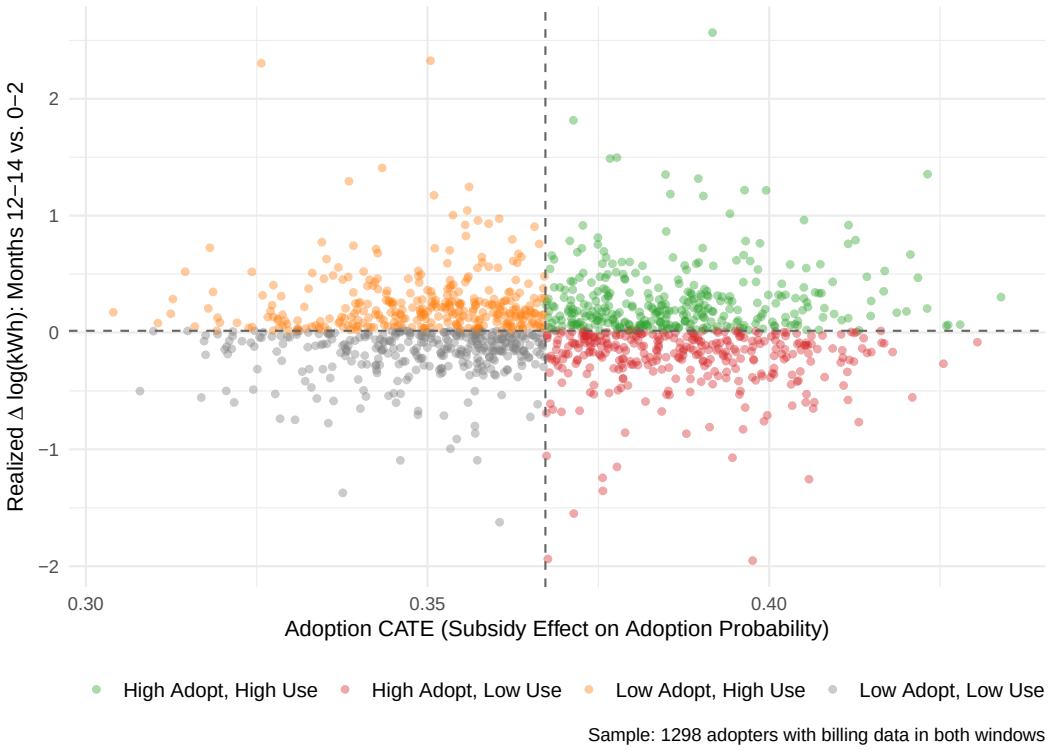


Figure I3: Joint Distribution of Predicted Adoption Response and Electricity Usage Growth

Notes: Each point represents one household in the CATE sample (N = 1,299: 863 subsidy-arm households and 436 control households). Horizontal axis: household-level conditional average treatment effect of subsidy receipt on adoption, causally estimated by a causal forest (4,000 trees, honesty splitting). Vertical axis: predicted change in log(kWh) between months 0–2 and 12–14 after the intervention, from a regression forest trained on the 461 subsidy-arm adopters with billing data in both windows. Dashed lines indicate median splits defining the four predicted household types. Correlation between the two dimensions: $r = 0.20$.

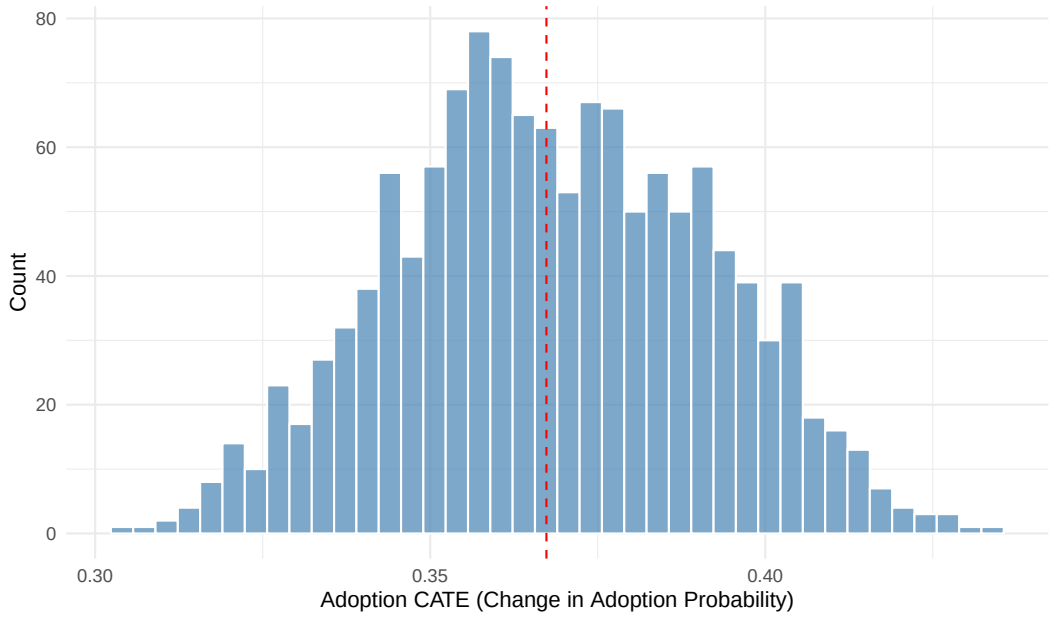


Figure I4: Distribution of Household-Level Adoption Treatment Effects

Notes: Histogram of household-level conditional average treatment effects of subsidy receipt on electric stove adoption ($\hat{\tau}_i$), estimated by a causal forest (4,000 trees, honesty splitting) on the CATE sample of $N = 1{,}299$ households: 863 subsidy-arm households and 436 control households. Treatment is an indicator for assignment to any subsidy arm. Adoption is defined as purchasing an electric induction stove by the endline survey. The dashed red line indicates the sample median. Mean $\hat{\tau} = 0.368$ (sd $= 0.023$).

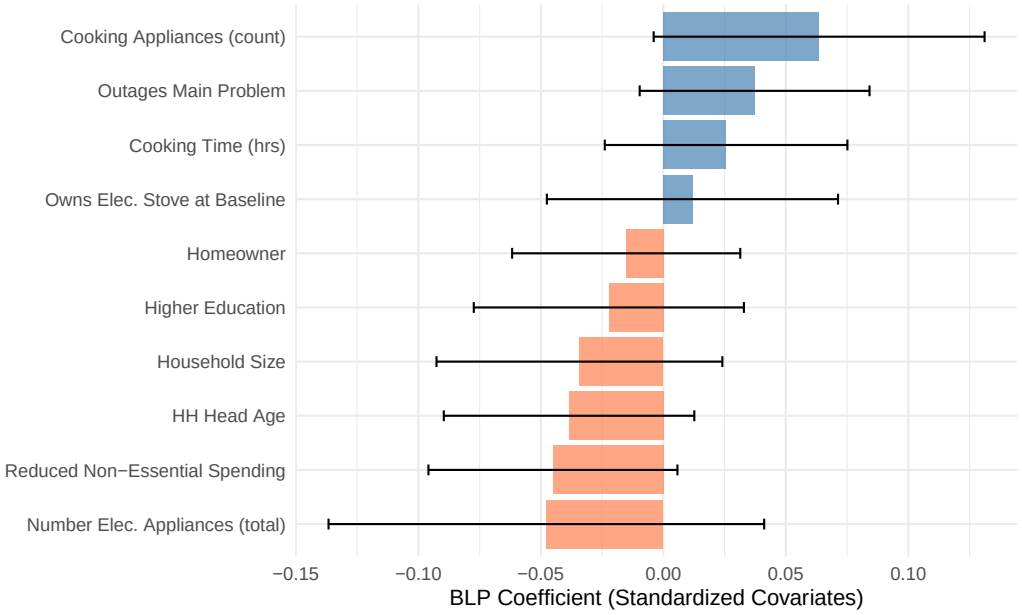


Figure I5: Best Linear Projection of Adoption Treatment Effects onto Baseline Covariates

Notes: Coefficients from a best linear projection of the household-level adoption treatment effect $\hat{\tau}_i$ onto 15 standardized baseline covariates using the CATE sample (N = 1,299: 863 subsidy-arm households and 436 control households). The projection identifies which observable characteristics are associated with larger or smaller adoption responses to subsidies. Whiskers show 95% confidence intervals (cluster- and heteroskedasticity-robust standard errors). The ten covariates with the largest absolute coefficients are shown.

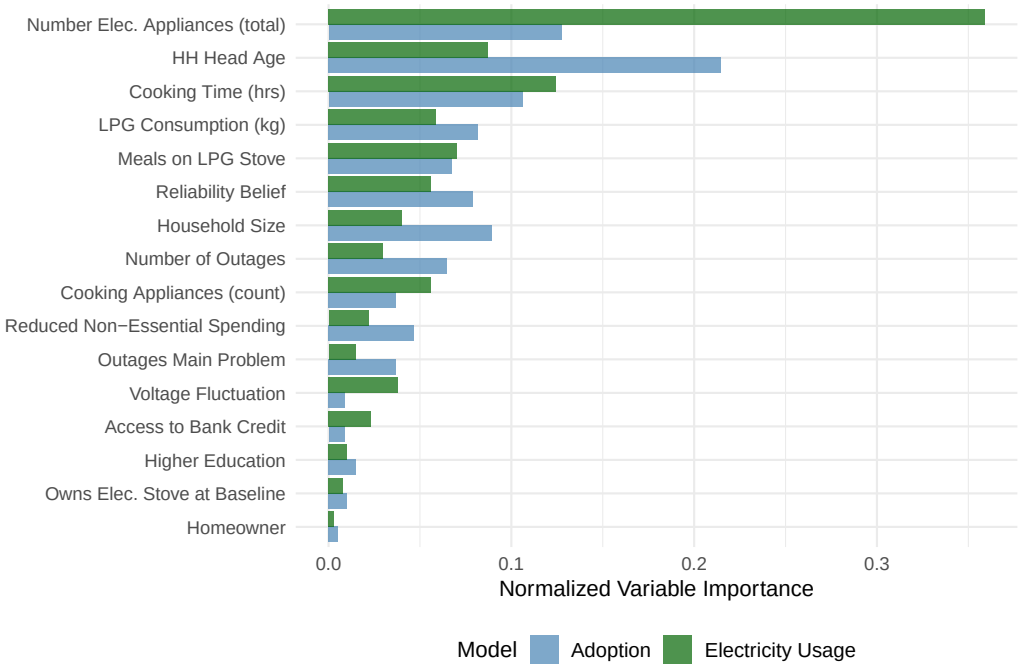


Figure I6: Variable Importance: Adoption Response versus Electricity Usage Prediction

Notes: Normalized variable importance from the causal forest (adoption treatment effect, $N = 1{,}299$ households in subsidy and control arms) and regression forest (predicted electricity usage growth, trained on the 461 subsidy-arm adopters with billing data in months 0–2 and 12–14). Variable importance measures each covariate's contribution to the forest's splits, capturing nonlinear and interactive effects. Values are normalized so that the most important variable equals 1 within each model. All 15 baseline covariates are included in both models.