

Subsidies and Debt under Nonlinear Electricity Pricing ^{*}

Robin Burgess[†] Tim Dobermann[‡] Michael Greenstone[§] Faraz Hayat[¶]

Muhammad Usman Naeem^{||}

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Abstract

Electricity subsidies are widely used to improve affordability, but affordability can reflect distinct constraints: high marginal prices may discourage additional consumption, while households may struggle to finance electricity they have already consumed. We distinguish marginal-price incentives from expenditure relief in a randomised experiment with 1,731 households in Pakistan. Varying the subsidised tariff blocks and the rebate rate under an increasing-block tariff generates distinct reductions in average and marginal electricity prices. Rebate rates of up to 50% do not increase electricity consumption, including among households exposed to lower marginal prices. Instead, several subsidy arms increase repayment of outstanding utility debt, with effects most evident among households entering treatment with greater payment stress or existing arrears. The results show that under nonlinear pricing, subsidies can operate through household balance sheets rather than energy demand, highlighting the importance of distinguishing expenditure relief from marginal-price incentives.

JEL codes: D12, H23, H31, L94, O13, Q41.

Keywords: electricity subsidies; nonlinear pricing; household debt; arrears; energy demand; Pakistan; randomised experiment.

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[†]Department of Economics, London School of Economics and Political Science. Email: r.burgess@lse.ac.uk.

[‡]International Growth Centre. Email: t.dobermann@lse.ac.uk.

[§]Department of Economics, University of Chicago. Email: mgreenst@uchicago.edu.

[¶]International Growth Centre. Email: shayat1@uchicago.edu.

^{||}The Fletcher School and Department of Economics, Tufts University. Email: usman.naeem@tufts.edu.

1 Introduction

Electricity affordability is a major policy concern across countries. Energy is typically one of the largest items in household budgets, and global household spending on electricity alone exceeded US\$1 trillion in 2024 (International Energy Agency, 2026). Governments respond to these pressures through subsidies, regulated tariffs, and other measures that lower the prices households pay for energy (Clements et al., 2013). Yet the economic constraint that these policies relax is not always clear. High marginal prices may discourage households from consuming additional electricity, while households may also consume electricity and then struggle to finance the resulting bill, delaying payment or accumulating arrears.

These constraints imply different responses to the same subsidy. A reduction in the marginal price creates a direct incentive to increase electricity consumption. By contrast, a reduction in expenditure on electricity already being consumed can relieve pressure on household budgets and affect payment behaviour even if consumption does not change. Distinguishing these margins is therefore important for evaluating the incidence of electricity subsidies: a policy intended to improve affordability may operate through energy demand, household finances, or both. Existing work on electricity subsidies has primarily studied their effects on consumption, welfare, and incentives for service provision (Ito, 2015; McRae, 2015; Hahn and Metcalfe, 2021). Much less is known about whether subsidies alter repayment of pre-existing utility debt, creating a potentially important financial margin of subsidy incidence.

This paper asks whether electricity subsidies improve affordability primarily by changing electricity demand or by relaxing household financial constraints. The distinction turns on how a subsidy changes the price schedule. A lower marginal price reduces the cost of additional consumption and creates a direct incentive to use more electricity, whereas expenditure relief on units that would have been consumed anyway transfers resources to the household without directly changing the marginal incentive to consume. Under nonlinear tariffs, these two effects need not coincide, so the effect of a subsidy on electricity use alone need not reveal the constraint it relaxes. We therefore study how subsidy-induced changes in marginal incentives and expenditure relief translate into electricity consumption, household payments, and outstanding utility debt.

We answer this question using a randomised experiment with 1,731 households in Pakistan facing a nonlinear increasing-block electricity tariff. The experiment varies both the tariff blocks that receive a subsidy and the generosity of the rebate, generating different changes in effective electricity prices across households. Because households occupy different positions on the tariff schedule absent treatment, this variation creates economically distinct forms of price exposure: for some households the subsidy lowers expenditure on their existing consumption bundle without changing the marginal price of additional electricity, while for others it lowers both average and marginal prices. Combining random assignment with households' pre-treatment tariff positions therefore allows us to distinguish price exposure that lowers expenditure alone from exposure that also lowers the marginal price. We measure the resulting responses using monthly administrative records on electricity consumption, bills, payments, and outstanding arrears, complemented by

household surveys.

The four treatment arms vary which portion of the tariff schedule is subsidised and the subsidy rate. T1 subsidises the first 100 kWh, T2 subsidises units 101–200 kWh, T3 subsidises the full 0–200 kWh range, and T4 subsidises the same 0–200 kWh range at a higher rate. Because households enter treatment at different points on the nonlinear tariff, treatment assignment generates either expenditure relief alone or expenditure relief together with a lower marginal price, depending on the household’s tariff position. This variation allows us to compare demand responses across these distinct forms of price exposure while also examining how the subsidies affect payments and outstanding utility debt.

We first examine whether the subsidies increase electricity consumption. At the treatment-arm level, none of the four subsidies produces a detectable increase in billed energy over the subsidy year. More importantly, this null persists when we exploit the nonlinear tariff to isolate households whose marginal price falls. Among households with pre-treatment consumption of 0–100 kWh, the pooled comparison between households receiving both average- and marginal-price reductions and those receiving no effective price reduction yields an estimated change in consumption of -4.7 kWh per month. The 95% confidence interval rules out increases larger than approximately 4 kWh per month, or 5% of the control mean. We similarly find no increase in consumption among households in the 101–200 kWh range when comparing average- plus marginal-price reductions with average-price reductions alone. Taken together, these comparisons show that consumption does not increase when treatment lowers the marginal price, including relative to households receiving expenditure relief alone.

Instead, several subsidy arms generate adjustment in outstanding utility debt. Although we detect little change in the level of the arrears stock, which is large and persistent for many households, the treatments increase the probability of month-to-month debt reduction or clearance. T1 raises the monthly probability of reducing outstanding debt by 3.3 percentage points, or 25% relative to the control mean, and the probability of fully clearing debt by 1.9 percentage points, or 26%. T2 raises the probability of reducing debt by 2.3 percentage points, while T3 raises the probability of clearing it by 1.9 percentage points. These changes are economically meaningful in rupee terms: T1 increases the unconditional amount of monthly debt reduction by Rs 182, 42% of the control mean, and lowers net monthly debt accumulation by Rs 275, or 22% of the control mean. Pooling the four treatment arms, the increase in debt reduction amounts to approximately Rs 0.25 per rupee of realised subsidy expenditure (Appendix Table 5). Thus, expenditure relief that produces little detectable change in electricity use nevertheless changes how households service existing utility debt.

The repayment effects are most evident among households entering treatment under greater financial pressure. Among households reporting payment stress at baseline, T1 raises the probability of reducing debt by 4.1 percentage points and the probability of clearing it by 2.6 percentage points, increases of 31% and 38% relative to the corresponding control means. T2 and T3 also produce positive and statistically significant repayment effects in this group, whereas the corresponding estimates among households without baseline payment stress are small and statistically indistin-

guishable from zero. An administrative split yields a similar pattern: among households entering the subsidy year with positive arrears, T1 and T2 increase the probability of reducing debt by 4.2 and 3.3 percentage points, respectively. Formal differences across the financial-stress subgroups are generally imprecisely estimated, so we interpret this heterogeneity cautiously, but the pattern is consistent with expenditure relief being particularly consequential for households already struggling to meet payment obligations.

Several pieces of evidence help interpret the contrast between the consumption and debt responses. The absence of higher electricity use is unlikely to reflect a simple failure to understand or receive the intervention: in the treatment-monitoring survey, 98% of respondents reported clearly understanding the payment process and correctly calculated the subsidy associated with their consumption, 96% correctly identified consumption as the basis for the payment, and 83% reported receiving the payment associated with their January bill. Severe load shedding may constrain demand, but households facing relatively less load shedding do not exhibit a consistently larger consumption response when their marginal price falls. Survey responses also point towards financial adjustment: during treatment, households reported that the subsidy helped with both electricity payments and other household bills or items, and at endline, 58% of treated households report using the subsidy to pay their electricity bill, while substantial shares report using it for other household expenses. The survey cannot distinguish payments towards current charges from repayment of accumulated arrears, but taken together with the administrative records, the evidence is consistent with the subsidy relaxing household budget pressure broadly, with utility-debt repayment emerging as one important balance-sheet response rather than additional electricity consumption.

Our first contribution is to connect electricity subsidy policy to household financial adjustment. A growing literature documents that difficulty paying utility bills, deferred-payment arrangements, and disconnections reveal economically important financial stress, and that utility-payment outcomes can be sensitive to household liquidity (Cicala, 2021; Barrage et al., 2019). In developing-country electricity systems, subsidies and non-payment are also closely tied to utility finances and the reliability of supply (Burgess et al., 2020). We show that expenditure relief from an electricity subsidy can itself alter this financial margin: households become more likely to reduce outstanding utility debt even when their electricity consumption does not increase. This broadens the usual accounting of subsidy incidence. When households carry pre-existing liabilities, part of the effect of a consumer subsidy may appear not in greater consumption of the subsidised good, but in the household balance sheet and in reductions in the provider’s outstanding receivables. Evaluating affordability policies through demand responses alone can therefore miss an economically consequential margin of adjustment.

Our second contribution is to the literature on household responses to nonlinear electricity prices and energy subsidies. Increasing-block tariffs are commonly used to pursue distributional objectives, but they create a wedge between average and marginal prices and therefore complicate both household choice and policy incidence (Borenstein, 2012; Ito, 2014). Existing work shows that consumers may respond to average rather than marginal prices under nonlinear tariffs (Ito,

2014), and evaluates how energy subsidies and electricity rebate programmes affect consumption, welfare, and incentives for infrastructure provision (Ito, 2015; McRae, 2015; Hahn and Metcalfe, 2021). We contribute by experimentally varying which portions of the same nonlinear price schedule are subsidised. This generates households for whom the subsidy primarily reduces expenditure on existing consumption and others for whom it also lowers the marginal price, allowing us to compare these economically distinct forms of price exposure within a common policy environment. The resulting demand null is informative because it helps isolate the financial channel: even when the experiment creates a direct marginal-price incentive to consume more electricity, we find little evidence that households do so.

The remainder of the paper proceeds as follows. Section 2 describes the electricity market, tariff structure, and payment environment in Pakistan. Section 3 presents the experimental design and data, and Section 4 develops the distinction between expenditure relief and marginal-price exposure under nonlinear pricing. Section 5 describes the empirical strategy. Section 6 presents the main results, Section 7 examines mechanisms and interpretation, and Section 8 discusses implications for electricity subsidy design. Section 9 concludes.

2 Setting

2.1 Electricity Distribution in Pakistan

At the time of the study, Pakistan’s electricity-distribution system comprised ten publicly owned distribution companies and one private utility (National Electric Power Regulatory Authority, 2023b). We partnered with the Peshawar Electric Supply Company (PESCO), the fourth-largest public distribution company in Pakistan by number of customers. PESCO supplies electricity to approximately 4.3 million customer accounts across Khyber Pakhtunkhwa, the north-western province bordering Afghanistan (Ministry of Energy, Power Division, 2024). Each electricity connection is associated with a customer account through which the utility records monthly consumption, charges, payments, and unpaid balances.

2.2 Nonlinear Tariffs and Billing

Residential customers in our study face a regulated increasing-block tariff. Monthly consumption, measured in kilowatt-hours (kWh), is divided into eight blocks: 0–100, 101–200, 201–300, 301–400, 401–500, 501–600, 601–700, and above 700 kWh. Each block has an associated per-unit price, and the marginal price generally rises across successive consumption blocks.

Residential customers are further classified as protected or non-protected. Protected customers face lower per-unit prices than non-protected customers at the same levels of consumption. In the first two blocks relevant to our experiment, protected customers face prices of Rs 7.74 and Rs 10.06 per kWh, while non-protected customers face prices of Rs 16.48 and Rs 22.95 per kWh, respectively (National Electric Power Regulatory Authority, 2023a). Protected status is determined on a rolling

basis: a customer is classified as protected if monthly consumption does not exceed 200 kWh in any of the preceding six months (K-Electric, 2023).

In Pakistan, the stated rationale for the increasing-block tariff and the protected–non-protected distinction is primarily distributional. Lower tariffs and targeted subsidies are directed towards low-consuming households, while households consuming more face higher prices and cross-subsidise them (Trimble et al., 2011; Ministry of Energy, Power Division, 2023).

Monthly electricity consumption is measured from successive meter readings and billed through a physical bill. Customers are organised into neighbourhood-level billing batches, with meters generally read on similar dates each month. The total amount due in each month consists of current-cycle charges plus any outstanding balance carried forward from previous months. Non-payment does not immediately trigger disconnection, allowing unpaid balances to persist across billing cycles.

Against this backdrop, we experimentally reduce electricity prices by up to 50% across different tariff blocks, with the subsidised block varying by treatment arm. Because the subsidy is delivered as a rebate rather than incorporated into the utility tariff, these interventions should be interpreted as changes in effective prices rather than changes in the regulated tariff itself.

2.3 Payments and Arrears

Households pay for electricity after consumption has been billed. Payments can be made online or at bank branches and post offices. Customers unable to pay the full amount due may request an extension or an instalment arrangement from the utility. As a result, the amount due in a given month may include unpaid balances accumulated over earlier billing cycles. The utility records the agreed instalment schedule and associated payment amounts (National Electric Power Regulatory Authority, 2021).

Payment difficulties are widespread and economically large. At baseline, 77% of households in the experimental sample report payment stress. Immediately before treatment, 50% of applications had positive arrears. Among these applications, the mean outstanding balance was Rs 96,036, approximately four times mean monthly household income, while the median was Rs 23,340. The corresponding exposure is also large on the provider’s balance sheet. In June 2023, PESCO reported Rs 181 billion in private-customer receivables, equivalent to approximately 58% of its Rs 312 billion in customer billings during FY2022–23 (National Electric Power Regulatory Authority, 2023b; Peshawar Electric Supply Company, 2023). Thus, unpaid electricity balances are economically important for both households and the utility, making debt repayment a potentially important margin through which electricity subsidies affect affordability and provider finances.

3 Experimental Design and Data

3.1 Sampling, Randomisation, and Survey Follow-up

The study was conducted in Charsadda District in Khyber Pakhtunkhwa, Pakistan (Figure 1). We selected 77 villages conditional on electricity access and the feasibility of making remote payments.

We then conducted a village census that yielded a sampling frame of 2,531 metered households. From this frame, we randomly sampled 1,731 households for the experiment, slightly above the powered target of 1,700 households to allow for attrition at baseline.

Figure 1: Study area

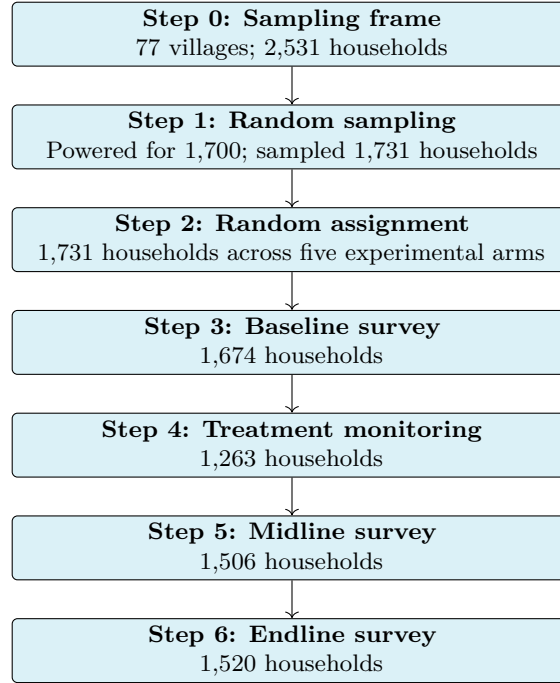


Notes: Panel A locates Khyber Pakhtunkhwa within Pakistan; Panel B locates Charsadda District, where the experiment was conducted, within Khyber Pakhtunkhwa. Boundaries are from Natural Earth and geoBoundaries.

The 1,731 households were randomised across five experimental arms, stratified by protected-customer status. Random assignment was conducted before baseline fieldwork, but households were not informed of their assignment until after completing the baseline survey. A customer is classified as protected if monthly consumption does not exceed 200 kWh in each of the preceding six months.

The baseline survey was fielded in June–July 2022 and covered household demographics and housing, income and livelihoods, assets and expenditure, electricity sources and use, electricity bills and payment history, and financial responses to electricity costs, including payment difficulty and borrowing. At the end of the baseline interview, treated households were informed of their assignment and received an arm-specific explanation of the subsidy; we describe the treatment arms and their implementation in Section 3.2. Once treatment rebates began in January 2023, we fielded a treatment-monitoring survey in March 2023 to verify that the intervention was being implemented as intended and to assess households’ understanding of the subsidy and tariff structure. A broader midline survey was conducted in September 2023 to measure changes in solar use, household income and electricity expenditure, electricity affordability and bill-payment frequency, and borrowing to pay electricity bills. For treated households, it also measured subsidy satisfaction, understanding, use, and perceived effects. After the treatments ended in December 2023, we fielded an endline survey to measure electricity-bill burden and payment coping, late payment and bill accuracy, and credit demand. For treated households, it also measured subsidy experience, use, satisfaction, and perceived effects on electricity consumption, expenditure, and livelihoods.

Figure 2: Experimental sample flow and survey follow-up



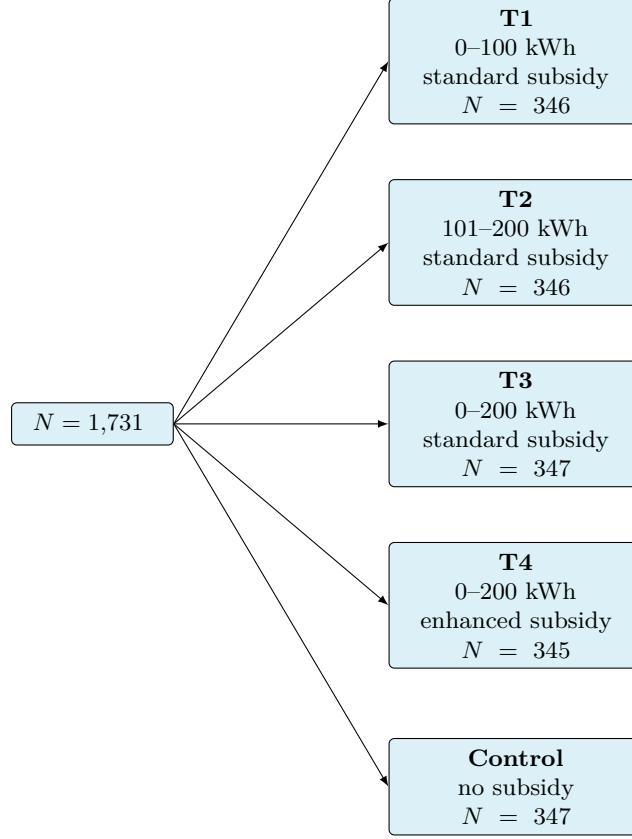
Notes: The figure summarises sample construction, random assignment, and completed interviews at baseline, treatment monitoring, midline, and endline. Random assignment covers all 1,731 sampled households; subsequent counts report completed interviews at each survey round.

3.2 Treatment Arms and Subsidy Implementation

The experiment includes four treatment arms and a control group. The treatment arms are defined by the tariff blocks subsidised and the subsidy rate used to construct the per-unit rebate. T1 subsidises consumption in the first tariff block, 0–100 kWh, at a rate equal to 30% of the applicable marginal price. For a protected customer, for example, the regulated price in this block is Rs 7.74 per kWh, implying an effective price of Rs 5.42 per kWh after the rebate. The utility bill itself is not reduced: households are billed under the prevailing regulated tariff and subsequently receive a research-funded rebate based on eligible consumption. T2 subsidises consumption in the second tariff block, 101–200 kWh, at the same 30% rate. T3 subsidises consumption in both of the first two tariff blocks, 0–200 kWh, also at the 30% rate. T4 covers the same 0–200 kWh range as T3, but at an enhanced subsidy rate of 50%.

We explicitly explained each treated household’s assigned subsidy and verified comprehension. To minimise field costs, households were randomised before baseline fieldwork, but treatment assignment was revealed only after the baseline questions had been completed. Treated households were then informed that they had been selected by lottery for a subsidy programme beginning in January 2023 and were told the rupee rebate per eligible kWh and the consumption block applicable to their assigned arm. Households were also informed that rebate payments would be made within approximately one month of receiving the relevant electricity bill. Enumerators explained the subsidy calculation using numerical examples, asked follow-up questions to assess understanding, repeated

Figure 3: Experimental arms and assignment counts



Notes: The figure reports random assignment across the four treatment arms and control group. Standard subsidies use a 30% subsidy rate; the enhanced subsidy uses a 50% rate. Control receives no experimental subsidy. Randomisation was stratified by protected-customer status.

the explanation when necessary, and then used additional examples to verify comprehension.

Payments were delivered in cash each month to the household head by our local field partner. Physical receipts were collected, independently verified by the research team, and supplemented with random back-checks. Households were initially informed that the subsidy programme would last for six months. During the fifth month, they were informed that the programme had been extended through the end of the year. When the programme was extended, we also increased the generosity of the rebate while preserving the original 30% and 50% subsidy rates used in its construction. From August through December, we increased the price base used to calculate the rebate by Rs 2 per kWh before applying the assigned subsidy rate. Households were informed directly of the resulting increase in the rupee rebate per eligible kWh. This adjustment affected only the research-funded rebate and did not alter the regulated tariff billed by the utility.

3.3 Administrative Billing and Payment Data

We obtained monthly billing and payment records from PESCO for the 1,731 households randomised into the experiment. The unit of observation is the application $\times$ month, where an application

corresponds to a unique utility customer account. Key administrative outcomes include billed energy (kWh), current-cycle billed amount (Rs), payment amount (Rs), arrears balance (Rs), and arrears age (months). These records allow us to construct monthly measures of debt reduction and debt clearance directly from changes in outstanding arrears. The total amount due equals current-cycle charges plus any arrears carried forward from previous billing cycles.

Our main analysis panel covers Jan 2022–Dec 2023 and contains 41,544 application-month observations (1,731 applications $\times$ 24 months). Billing outcomes are observed for every application-month in this balanced panel and are not imputed. Payment records are maintained separately by the utility; application-months with an observed bill but no posted payment are coded as zero payments, consistent with guidance from PESCO. Appendix Table 1 provides detailed definitions and construction of the administrative variables. Table 1 reports summary statistics for the experimental sample. Monthly billed energy averages 125 kWh, with a median of 100 kWh, exactly at the threshold between the first two tariff blocks targeted by the experiment. The 25th percentile of monthly payments is zero, consistent with substantial month-to-month variation in bill payment.

Table 1: Summary statistics for the experimental sample

	Mean	SD	Median	P25	P75	N
Panel A. Baseline household characteristics						
Household size (members)	7.71	3.68	7.00	6.00	9.00	1,630
Household-head age (years)	43.05	11.87	42.50	35.00	50.00	1,628
Household head with secondary education or above (=1)	0.33	0.47	0.00	0.00	1.00	1,630
Monthly household income (PKR)	24,242	14,187	24,000	17,000	30,000	1,607
Rooms with electricity	3.28	1.14	3.00	3.00	4.00	1,630
Any baseline payment stress (=1)	0.77	0.42	1.00	1.00	1.00	1,630
Panel B. Administrative billing and payment outcomes						
<i>B.1 Consumption & Billing</i>						
Billed energy (kWh)	125.20	98.85	100.00	58.00	169.00	41,544
Billed amount (PKR)	2,971	4,435	1,663	943	3,549	41,544
Total billed amount (PKR; includes arrears)	52,236	116,920	3,447	1,317	25,422	41,544
<i>B.2 Payments</i>						
Payment amount (PKR)	3,119	6,010	1,126	0	3,534	41,544
<i>B.3 Arrears & Debt</i>						
Arrears balance (PKR)	49,265	115,853	0	0	21,860	41,544
Arrears age (months)	16.11	34.76	0.00	0.00	8.00	41,544
<i>B.4 Adjustments & Surcharges</i>						
Total adj. amount (PKR)	87	766	0	0	0	41,544

Notes: Panel A reports baseline household characteristics; Panel B reports application $\times$ month administrative outcomes over Jan 2022–Dec 2023. Administrative outcomes are observed for the 1,731 randomly assigned applications. Payment is coded as zero when a bill is observed but no payment is posted. Arrears is the outstanding balance on the monthly bill. Variables marked = 1 are indicators.

3.4 Survey Data and Measures

We conducted baseline and follow-up household surveys to complement the administrative data. Baseline household characteristics are used for descriptive statistics, randomisation-balance checks, and heterogeneity analyses. In particular, we define baseline payment stress as an indicator for

whether the household reports difficulty paying its electricity bill, non-payment or late payment, or borrowing or reducing other expenditure to finance electricity bills. We use this measure to test whether the financial effects of the subsidies are larger among households experiencing greater payment stress before treatment.

3.5 Balance and Pre-Treatment Trends

Appendix Table 2 shows balance across treatment arms in baseline household characteristics and pre-treatment administrative outcomes. Figure 4 plots monthly trends in billed energy, billed amount, payment amount, and arrears by experimental arm. The treatment-arm series evolve similarly over Jan–Dec 2022, before the subsidy begins in Jan 2023. Persistent level differences in outcomes such as billed amount and arrears are absorbed by the application fixed effects in our empirical specification (Section 5). Appendix Table 3 provides formal tests for differential pre-treatment trends across experimental arms.

4 Conceptual Framework

4.1 Experimental Variation in Price Schedules

The experiment superimposes arm-specific, research-funded rebates on the existing nonlinear increasing-block tariff. Households continued to be billed under the prevailing utility tariff and subsequently received payments based on eligible electricity use. The control group received no experimental rebate.

The four treatment arms vary which tariff blocks are subsidised and the subsidy rate. T1 subsidises units in the 0–100 kWh block, while T2 subsidises units in the 101–200 kWh block. T3 subsidises both blocks at the standard rate, covering consumption from 0 to 200 kWh, and T4 covers the same range at an enhanced rate. The standard arms use a 30% subsidy rate, while the enhanced arm uses a 50% rate.

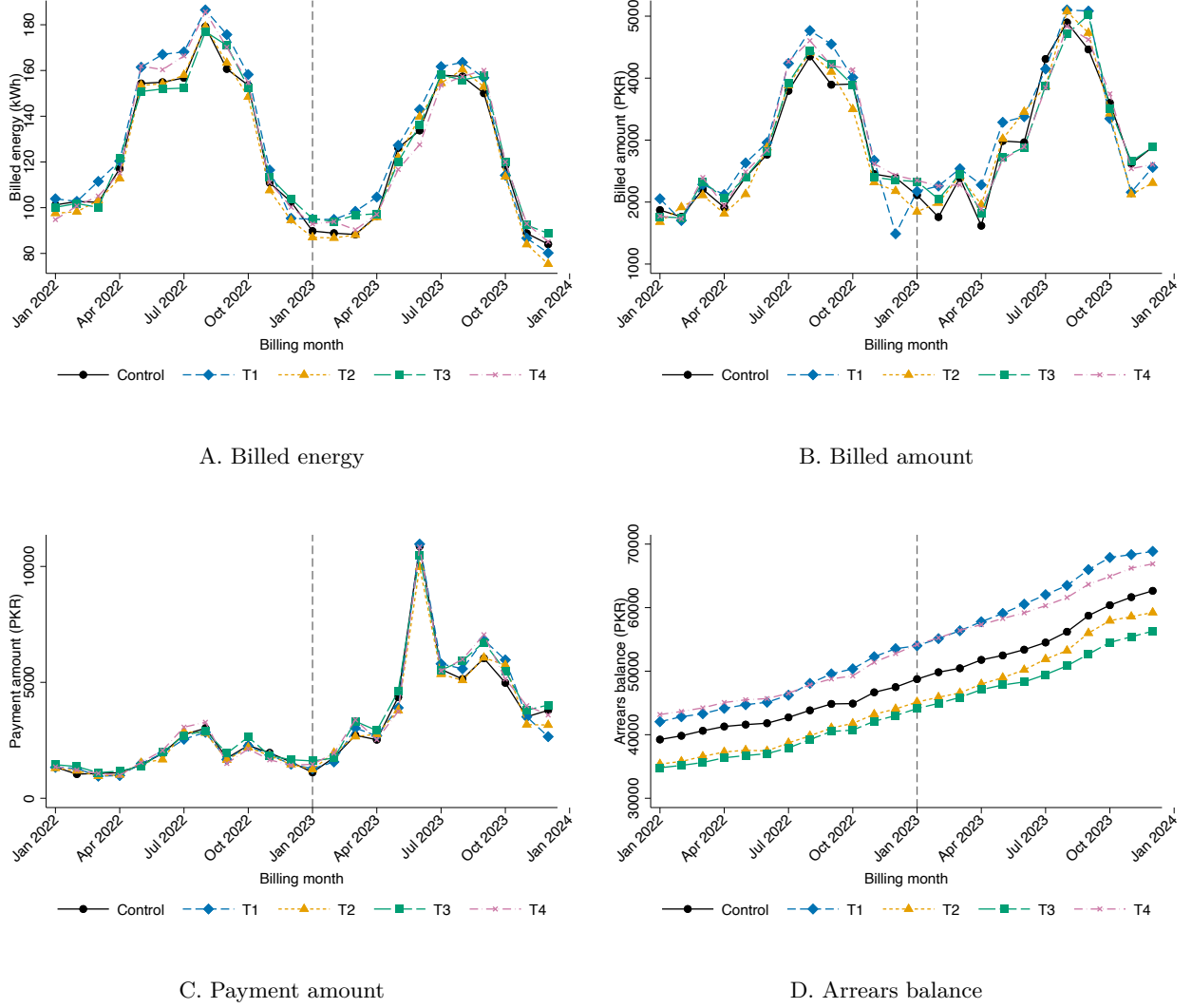
The regulated residential tariff differs between protected and non-protected customers, so a given treatment arm generates different rupee price reductions across the two groups. Figure 5 plots the resulting effective marginal-price schedules for the control group and each treatment arm, separately for protected and non-protected customers.

4.2 From Assignment to Price Exposure

Rebate receipt. Under a nonlinear increasing-block tariff, assignment to a treatment arm does not by itself determine whether a household receives an experimental rebate. The subsidised block must also overlap with the household’s realised consumption. Let G_i denote household i ’s assigned treatment arm and q_{it} its realised electricity consumption in month t . Rebate receipt can therefore be written as

$$R_{it} = R(G_i, q_{it}), \tag{1}$$

Figure 4: Raw trends in administrative outcomes by experimental arm



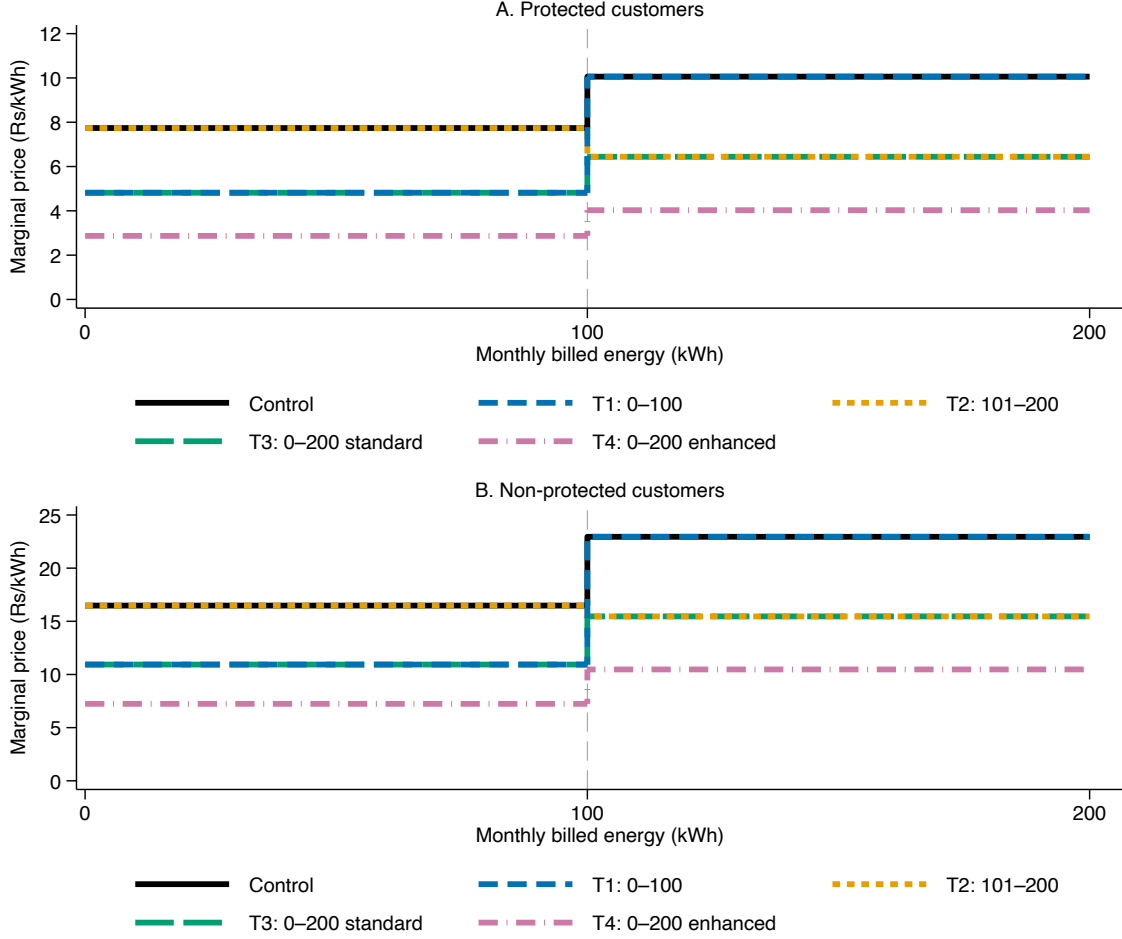
Notes: The figure reports monthly treatment-arm means over Jan 2022–Dec 2023. The vertical dashed line marks the start of the subsidy year in Jan 2023. Outcomes are measured at the application $\times$ month level. Bill excludes carried-forward arrears; arrears is the outstanding balance on the monthly bill.

where R_{it} denotes the research-funded rebate received by household i in month t . For example, a household assigned to T2 receives no rebate if it consumes 50 kWh, but receives a rebate on units 101–150 if it consumes 150 kWh.

Realised price changes. Realised consumption also determines which effective price changes materialise for a given treatment assignment. Let ΔAP_{it} and ΔMP_{it} denote the treatment-induced changes in household i 's average and marginal effective electricity prices in month t . These realised price changes can be written as

$$\Delta AP_{it} = \Delta AP(G_i, q_{it}), \quad \Delta MP_{it} = \Delta MP(G_i, q_{it}). \quad (2)$$

Figure 5: Experimental variation in effective marginal-price schedules



Notes: The figure plots effective marginal-price schedules under the Aug–Dec 2023 implementation, separately for protected and non-protected customers. Standard and enhanced arms apply rebate rates of 30% and 50%, respectively. The Aug–Dec rebate base includes the additional research-funded Rs 2 per kWh described in Section 3.2; the regulated tariff is unchanged.

For example, a T2 household consuming 150 kWh receives a rebate on units 101–150, while its next unit also lies within the subsidised block. Treatment therefore lowers both its average effective price and the marginal price of additional consumption. By contrast, a T1 household consuming the same 150 kWh receives a rebate on its first 100 units, but its next unit lies in the unsubsidised 101–200 kWh block. Its average effective price falls, while its marginal price remains unchanged.

Ex ante price exposure. The preceding relationships are ex post: treatment assignment and realised consumption jointly determine the rebate received and the price changes that ultimately materialise. For behavioural responses, however, the relevant object is the price change to which the household is exposed before choosing its realised consumption.

Let q_{it}^0 denote household i 's counterfactual electricity consumption in month t , defined as the amount it would consume in the absence of the experimental rebate. Counterfactual consumption

locates the household on the tariff schedule before treatment can affect its consumption choice. Let E_{it}^{AP} and E_{it}^{MP} denote household i 's ex ante average- and marginal-price exposure, respectively. We define these objects as

$$E_{it}^{AP} = \Delta AP(G_i, q_{it}^0), \quad E_{it}^{MP} = \Delta MP(G_i, q_{it}^0), \quad (3)$$

so that ex ante exposure corresponds to the treatment-induced average- and marginal-price changes evaluated at the household's counterfactual consumption. Treatment assignment and counterfactual tariff position therefore jointly determine price exposure before realised consumption q_{it} is chosen.

For example, consider a household assigned to T1 whose counterfactual consumption is 50 kWh. Because its counterfactual consumption lies within the subsidised 0–100 kWh block, T1 exposes the household to both an average-price and a marginal-price reduction. If its counterfactual consumption were instead 150 kWh, the rebate would still lower the average effective price of its counterfactual consumption bundle by subsidising the first 100 units, but would leave the marginal price of the next unit unchanged. Table 2 summarises the resulting exposure categories across treatment arms and counterfactual tariff positions.

Table 2: Ex ante price exposure by treatment arm and counterfactual tariff position

Treatment arm	Counterfactual consumption		
	0–100 kWh	101–200 kWh	Above 200 kWh
T1: 0–100 standard	AP + MP	AP only	AP only
T2: 101–200 standard	No exposure	AP + MP	AP only
T3: 0–200 standard	AP + MP	AP + MP	AP only
T4: 0–200 enhanced	AP + MP	AP + MP	AP only

Notes: Ex ante exposure is determined by treatment assignment and the household's counterfactual tariff position. “No exposure” indicates that counterfactual consumption does not reach the subsidised block; “AP only” indicates that the counterfactual bundle is subsidised but the next unit is not; “AP + MP” indicates that the next unit is also subsidised. T4 generates the same exposure categories as T3 but larger price reductions. Exact price changes are omitted.

4.3 Predictions

Marginal-price exposure. A marginal-price reduction lowers the effective price of the next unit and therefore creates a direct incentive to increase electricity consumption. Consumption responses should therefore be strongest where treatment lowers the marginal price.

Average-price exposure. An average-price reduction lowers effective expenditure on the household's counterfactual consumption bundle. If the marginal price is unchanged, there is no direct incentive to change consumption at the margin, although demand may still respond through income or liquidity effects. Consumption responses should therefore be weaker under AP-only exposure than under AP + MP exposure.

Payments and arrears. Average-price relief can affect financial outcomes even when consumption does not change. By lowering effective electricity expenditure, the rebate may free resources for

payments to the utility and thereby reduce outstanding arrears. If this response reflects liquidity relief, the financial effects should be strongest among households entering treatment with greater payment stress or positive arrears.

These predictions organise the empirical analysis. We first estimate reduced-form treatment effects on consumption, billed amounts, payments, and arrears. We then examine consumption responses by ex ante average- and marginal-price exposure, before testing whether financial effects are concentrated among households with greater pre-treatment payment stress or positive arrears.

5 Empirical Strategy

Our main specification estimates intent-to-treat effects of the four experimental subsidy arms during the subsidy year, Jan–Dec 2023, using monthly administrative billing and payment records for Jan 2022–Dec 2023. Jan–Dec 2022 serves as the pre-treatment period.

$$Y_{it} = \alpha_i + \lambda_t + \sum_{g=2}^5 \beta_g (\mathbb{1}\{G_i = g\} \times \mathbb{1}\{t \in 2023\}) + \varepsilon_{it}, \quad (4)$$

where Y_{it} denotes an administrative outcome for household i , observed through its utility customer account, in billing month t , and G_i denotes randomly assigned experimental arm. The control group is indexed by $G_i = 1$, while $G_i \in \{2, 3, 4, 5\}$ correspond to T1–T4. The terms α_i and λ_t denote application and billing-month fixed effects, respectively. Application fixed effects absorb time-invariant differences across customer accounts, such as persistent differences in household electricity demand or payment behaviour, while billing-month fixed effects absorb shocks common to all applications in a given month, such as seasonal variation in electricity demand or changes in the regulated tariff. Each coefficient β_g measures the intent-to-treat effect of treatment arm g : the change in the outcome from the pre-treatment year to the subsidy year for arm g , relative to the corresponding change in the control group.

We estimate equation (4) by ordinary least squares, absorbing application and billing-month fixed effects. Standard errors are clustered at the application level. Baseline protected-customer status, which was used as a randomisation stratum, is time-invariant and absorbed by the application fixed effects. Tables report the control-group mean during the subsidy year.

This arm-level specification provides the reduced-form benchmark for the empirical analysis. To examine responses by ex ante price exposure, we proxy each household’s counterfactual tariff position using its mean monthly billed energy over Jan–Dec 2022. We divide households into the tariff-position groups defined in Section 4.2, estimate equation (4) within each group, and form linear contrasts across randomly assigned treatment arms corresponding to the “No exposure”, AP-only, and AP + MP comparisons. Because tariff position is defined entirely using pre-treatment consumption, these comparisons are not selected on treatment-induced changes in electricity use.

We also examine whether financial responses vary with households’ pre-treatment financial position. We estimate treatment effects separately by baseline payment stress, measured in the

pre-treatment household survey, and by whether the application had positive arrears in Dec 2022, immediately before the subsidy year. We report the corresponding differences in treatment effects across subgroups. Both dimensions of heterogeneity are therefore determined before treatment. Survey-based mechanism outcomes in Section 7 use experimental assignment at the relevant survey round, with outcome-specific specifications described there.

6 Results

6.1 Average Reduced-Form Effects of the Subsidy Arms

The average effect of each treatment arm over the full subsidy year is statistically indistinguishable from zero for the main administrative outcomes: billed energy, billed amount, payment amount, and arrears balance. The reduced-form estimates therefore provide little evidence that any treatment arm changed average consumption, billed amounts, payments, or the arrears stock over the subsidy year.

Table 3: Average reduced-form effects during the subsidy year

	Energy (kWh) (1)	Bill (PKR) (2)	Payment (PKR) (3)	Arrears (PKR) (4)
T1 $\times$ Subsidy year	-2.1 (4.1)	-5 (160)	166 (236)	2,611 (2,177)
T2 $\times$ Subsidy year	0.5 (4.2)	34 (180)	-55 (233)	583 (2,157)
T3 $\times$ Subsidy year	2.7 (3.8)	-28 (154)	239 (235)	-651 (1,926)
T4 $\times$ Subsidy year	-1.9 (3.9)	-148 (157)	227 (243)	1,193 (2,006)
Control mean, subsidy year	115.0	3,051	4,364	55,069
Application fixed effects	Yes	Yes	Yes	Yes
Billing-month fixed effects	Yes	Yes	Yes	Yes
Observations	41,544	41,544	41,544	41,544
Applications	1,731	1,731	1,731	1,731

Notes: The unit of observation is application $\times$ month. Estimates compare each treatment arm with control over Jan 2022–Dec 2023, with 2023 defined as the subsidy year. All specifications include application and billing-month fixed effects; standard errors are clustered by application. Bill excludes carried-forward arrears; arrears is the outstanding balance on the monthly bill. Control means are for the subsidy year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We first examine the consumption null more closely because the nonlinear tariff generates sharp predictions for demand depending on whether treatment lowers the marginal price. The annual estimates do not appear to mask meaningful within-year responses: Appendix Figure 1 shows no sustained increase in billed energy in any treatment arm over the subsidy year, while Appendix Table 4 shows that the absence of a demand response persists during Aug–Dec 2023, when the experimental rebates became more generous. The arm-level averages nevertheless combine households facing economically distinct price changes under the nonlinear tariff. Section 6.3 therefore tests the demand prediction directly by comparing consumption responses according to whether

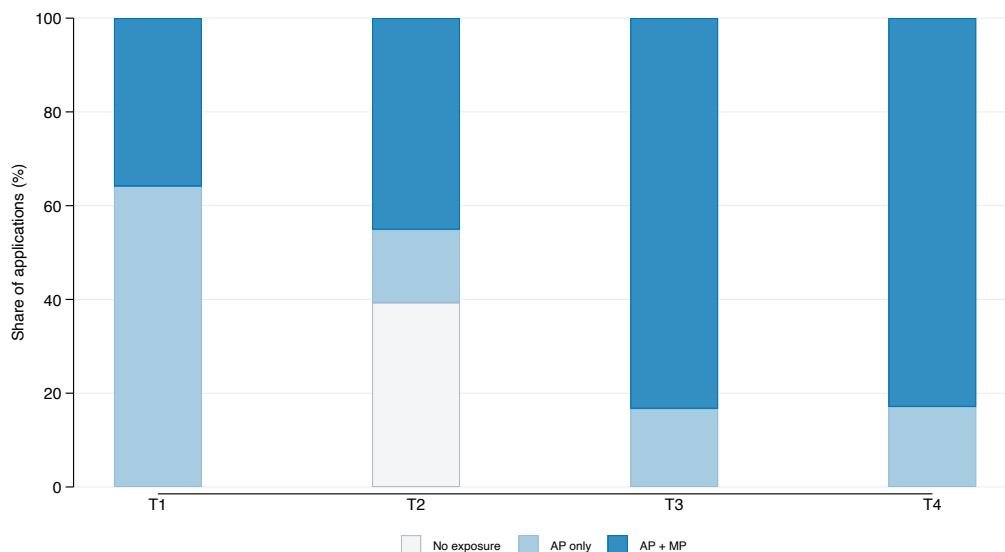
treatment lowered the average price only or both the average and marginal price.

We return to financial adjustment in Section 6.4. The arrears outcome in Table 3 measures the monthly stock of outstanding debt; later analyses examine whether the subsidies changed transitions in that stock, including debt reduction and clearance.

6.2 Ex Ante Average- and Marginal-Price Exposure

We proxy each household’s counterfactual tariff position using its mean monthly billed energy over Jan–Dec 2022 and combine this pre-treatment consumption measure with random treatment assignment to classify households as facing “No exposure”, “AP only”, or “AP + MP” exposure. Because the tariff-position proxy is constructed entirely from the pre-treatment year, it is unaffected by treatment-induced consumption changes. It is nevertheless an approximation to the unobserved month-specific counterfactual: a household classified using its 2022 mean may have occupied a different tariff block in a particular month absent treatment. Figure 6 reports the resulting exposure shares within each treatment arm. Most T1 households receive AP-only exposure, whereas the large majority of households assigned to T3 and T4 receive both average- and marginal-price reductions.

Figure 6: Ex ante price exposure generated by treatment assignment and baseline consumption



Notes: The figure reports the share of applications in each ex ante price-exposure category by treatment arm. Exposure combines treatment assignment with mean monthly billed energy over Jan–Dec 2022. “No exposure” indicates that baseline consumption does not reach the subsidised block; “AP only” that the baseline bundle is subsidised but the next unit is not; “AP + MP” that the next unit is also subsidised. T4 generates the same exposure categories as T3 but larger price reductions.

This variation in exposure provides the basis for the price-exposure contrasts in the next subsection, which test whether consumption responds differently under AP-only exposure and AP + MP exposure.

6.3 Consumption Responses by Price Exposure

Table 4 compares randomly assigned treatment arms within fixed baseline-consumption groups. Because ex ante exposure depends jointly on treatment assignment and baseline tariff position, comparisons across exposure categories in the full sample would also reflect pre-treatment differences in consumption. Conditioning on baseline-consumption group holds the relevant pre-treatment tariff region fixed while using random assignment across arms to generate economically distinct price-exposure contrasts. Among households consuming 0–100 kWh at baseline, T1 and T3 generate standard-rate AP + MP exposure while T2 generates no exposure; the pooled comparison additionally includes T4, which generates AP + MP exposure at the enhanced rate. Among households consuming 101–200 kWh, T2 and T3 generate AP + MP exposure while T1 generates AP-only exposure. Above 200 kWh, T1–T3 generate AP-only exposure and are compared with the control arm.

The estimates provide no evidence that electricity consumption increases when treatment lowers the marginal price. Among households consuming 0–100 kWh at baseline, the estimated effect of standard-rate AP + MP exposure relative to no exposure is -2.6 kWh per month, while the broader comparison pooling standard- and enhanced-rate AP + MP exposure yields an estimate of -4.7 kWh. Among households consuming 101–200 kWh, AP + MP exposure relative to AP-only exposure yields an estimate of -0.9 kWh, while above 200 kWh the estimated effect of AP-only exposure relative to control is 1.3 kWh. None of these estimates is statistically distinguishable from zero. The pooled AP + MP comparison in the 0–100 kWh group is particularly informative: its 95% confidence interval rules out consumption increases larger than approximately 4 kWh per month, or 5% of the control mean. The comparison above 200 kWh is considerably less precise.

Table 4: Consumption responses by ex ante price exposure

	Monthly billed energy (kWh)		
	(1)	(2)	(3)
Baseline consumption	0–100 kWh	101–200 kWh	Above 200 kWh
AP + MP versus no exposure	-2.61 (4.56)		
Pooled AP + MP versus no exposure	-4.70 (4.38)		
AP + MP versus AP only		-0.90 (3.99)	
AP only versus control			1.27 (11.20)
Control mean, subsidy year	75.3	125.2	178.7
Application fixed effects	Yes	Yes	Yes
Billing-month fixed effects	Yes	Yes	Yes
Observations	15,168	19,512	6,864
Applications	632	813	286

Notes: Baseline-consumption groups use mean monthly billed energy over Jan–Dec 2022 as a proxy for counterfactual tariff position. Column (1) compares T1 and T3 with T2; the pooled contrast additionally includes T4. Column (2) compares T2 and T3 with T1; column (3) compares T1–T3 with control. Estimates use application and billing-month fixed effects over Jan 2022–Dec 2023. Standard errors are clustered by application. Control means are subgroup-specific subsidy-year means. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

6.4 Financial Adjustment and Debt Repayment

With little evidence of a demand response, the subsidies may instead affect households’ outstanding utility debt. Table 3 examines treatment effects on the monthly stock of arrears. Because this stock can be large and persistent, economically meaningful repayment may translate into only a modest change in its level and therefore be difficult to detect in the stock outcome.

We therefore examine month-to-month transitions in the same arrears stock. *Debt reduced* equals one when an application has strictly positive arrears in the previous consecutive billing month and that balance falls in the current month. *Debt cleared* is a stricter measure that equals one when a strictly positive previous arrears balance falls to zero or below.

Table 5: Effects on debt repayment during the subsidy year

	Debt	
	(1)	(2)
	Reduced (=1)	Cleared (=1)
T1 × Subsidy year	0.033*** (0.011)	0.019*** (0.007)
T2 × Subsidy year	0.023** (0.011)	0.011 (0.008)
T3 × Subsidy year	0.018 (0.012)	0.019** (0.008)
T4 × Subsidy year	0.012 (0.011)	0.011 (0.008)
Control mean, subsidy year	0.134	0.074
Application fixed effects	Yes	Yes
Billing-month fixed effects	Yes	Yes
Observations	41,544	41,544
Applications	1,731	1,731

Notes: The unit of observation is application × month. Debt reduced equals one when strictly positive arrears in the previous consecutive billing month fall in the current month; debt cleared equals one when they fall to zero or below. Other transitions equal zero. Estimates compare each treatment arm with control over Jan 2022–Dec 2023 and include application and billing-month fixed effects; standard errors are clustered by application. Control means are for the subsidy year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 shows that T1 increases the monthly probability of a debt-reduction event by 3.3 percentage points, or approximately 25% of the control mean during the subsidy year, with the estimate statistically significant at the 1% level. T1 also increases the probability of fully clearing outstanding debt by 1.9 percentage points, or approximately 26% of the control mean, again significant at the 1% level. T2 increases the monthly probability of a debt-reduction event by 2.3 percentage points, or approximately 17% of the control mean, with the estimate significant at the 5% level. T3 produces a similar increase in the probability of fully clearing outstanding debt as T1—1.9 percentage points, or approximately 26% of the control mean—with the estimate significant at the 5% level.

If the debt-repayment response reflects subsidy-induced relief reducing outstanding utility debt, the pattern should also be visible among households that enter the subsidy year with debt already

outstanding. This is the case for debt reduction: among households with positive arrears in Dec 2022, T1 and T2 increase the probability of reducing outstanding debt by 4.2 and 3.3 percentage points, respectively (Appendix Table 10). Full clearance is naturally less common among households entering treatment with larger and more persistent arrears, so debt reduction captures adjustment even when outstanding balances remain positive.

Table 6 asks whether the higher debt-repayment probabilities in Table 5 translate into economically meaningful changes in the arrears stock and lower net debt accumulation. Column (2) shows that T1 increases the unconditional monthly amount of debt reduction by Rs 182, or approximately 42% of the control mean during the subsidy year, with the estimate statistically significant at the 1% level. T4 increases the corresponding debt-reduction amount by Rs 130, or approximately 30% of the control mean, with the estimate statistically significant at the 10% level. Pooling the four treatment arms on the common sample linked to subsidy-payment records, monthly debt reduction increases by Rs 105 relative to Rs 415 of realised subsidy expenditure, or approximately Rs 0.25 of additional debt reduction per rupee of subsidy (Appendix Table 5). Columns (3) and (4) show little evidence of offsetting effects on debt increases. Consistent with the reduction-side effects, column (5) shows that T1 lowers net monthly debt accumulation by Rs 275, or approximately 22% of the control mean during the subsidy year, with the estimate statistically significant at the 10% level.

Table 6: Decomposition of debt adjustment during the subsidy year

	Debt reductions		Debt increases		Net change
	(1)	(2)	(3)	(4)	(5)
	Any (=1)	Amount (PKR)	Any (=1)	Amount (PKR)	Amount (PKR)
T1 × Subsidy year	0.033*** (0.011)	182.3*** (70.0)	-0.005 (0.014)	-92.4 (146.9)	-274.8* (152.4)
T2 × Subsidy year	0.023** (0.011)	58.0 (73.8)	-0.006 (0.015)	11.8 (171.3)	-46.2 (175.3)
T3 × Subsidy year	0.018 (0.012)	79.7 (61.1)	-0.012 (0.015)	-85.7 (137.5)	-165.4 (141.0)
T4 × Subsidy year	0.012 (0.011)	129.5* (67.1)	-0.002 (0.014)	-91.8 (137.7)	-221.3 (146.0)
Control mean, subsidy year	0.134	438.9	0.399	1,698.6	1,259.6
Application fixed effects	Yes	Yes	Yes	Yes	Yes
Billing-month fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	41,544	41,544	41,544	41,544	41,544
Applications	1,731	1,731	1,731	1,731	1,731

Notes: The unit of observation is application × month. Positive debt is arrears capped below at zero. Debt reductions and increases measure whether, and by how much, positive debt falls or rises relative to the previous consecutive billing month. Net change is current minus previous positive debt; negative estimates indicate lower net debt accumulation. Estimates compare each treatment arm with control over Jan 2022–Dec 2023 and include application and billing-month fixed effects; standard errors are clustered by application. Control means are for the subsidy year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Taken together, these results show meaningful adjustment on debt-repayment margins in several subsidy arms even though effects on the level of the arrears stock are difficult to detect. Treatment increases the frequency and amount of debt reductions, the reduction effects remain visible among

households entering the subsidy year with positive arrears, and the resulting changes translate into lower net debt accumulation.

6.5 Heterogeneity by Pre-Treatment Financial Stress

Debt responses should be more pronounced among households exhibiting signs of payment stress before treatment. We construct a composite indicator of baseline payment stress that equals one if the household reports difficulty paying its electricity bill, non-payment of the most recent bill, any late or missed payment, or borrowing or reducing other expenditure to finance electricity payments.

Table 7 shows that among households with baseline payment stress, T1–T3 generate positive and statistically significant effects on debt reduction and clearance. For T1, the probability of reducing outstanding debt increases by 4.1 percentage points, approximately 31% of the corresponding control mean, with the estimate statistically significant at the 1% level. The probability of fully clearing outstanding debt increases by 2.6 percentage points, approximately 38% of the corresponding control mean, also significant at the 1% level. By contrast, the corresponding estimates among households without baseline payment stress are small and statistically indistinguishable from zero. Formal differences between the two subgroups are generally imprecisely estimated, although the difference in T3’s effect on debt clearance is statistically significant.

Appendix Table 11 provides a complementary administrative split based on whether the application entered the subsidy year with positive arrears. The pattern for debt reduction is again stronger among households entering treatment with outstanding debt: T1 and T2 increase the probability of reducing debt by 4.2 and 3.3 percentage points, respectively, while the corresponding effects among applications without positive arrears are smaller and statistically insignificant. The formal differences across the two debt-status groups are, however, imprecisely estimated.

Taken together, the heterogeneity patterns are consistent with debt-repayment responses being most visible among households entering treatment under greater financial pressure, although the evidence for differential treatment effects across subgroups is not uniformly precise. More broadly, the results in this section show no increase in electricity consumption, including where treatment lowers the marginal price, while adjustment instead appears on utility-debt repayment margins. Section 7 examines the mechanisms that may account for this pattern.

7 Mechanisms and Interpretation

7.1 Price Exposure and the Absence of a Demand Response

One possible explanation for the absence of a consumption response is that households did not understand the nonlinear tariff or the experimental subsidy. The survey evidence does not support a simple version of this explanation. At baseline, approximately 72% of respondents correctly reported that the per-unit electricity price varies with consumption. More directly, the treatment-monitoring survey indicates that the experimental subsidy was generally well understood. Ninety-eight per cent of respondents reported clearly understanding the payment process and correctly calculated

Table 7: Debt-repayment effects by baseline payment stress

	Debt	
	(1) Reduced (=1)	(2) Cleared (=1)
<i>Panel A. No baseline payment stress</i>		
T1 × Subsidy year	0.005 (0.022)	0.004 (0.017)
T2 × Subsidy year	-0.010 (0.025)	-0.010 (0.018)
T3 × Subsidy year	-0.009 (0.025)	-0.014 (0.017)
T4 × Subsidy year	0.007 (0.022)	0.003 (0.017)
<i>Panel B. Baseline payment stress</i>		
T1 × Subsidy year	0.041*** (0.013)	0.026*** (0.008)
T2 × Subsidy year	0.034*** (0.013)	0.021** (0.009)
T3 × Subsidy year	0.025* (0.013)	0.029*** (0.009)
T4 × Subsidy year	0.014 (0.013)	0.014 (0.009)
<i>Panel C. Payment stress minus no payment stress</i>		
T1 × Subsidy year	0.036 (0.025)	0.022 (0.019)
T2 × Subsidy year	0.044 (0.028)	0.031 (0.020)
T3 × Subsidy year	0.034 (0.029)	0.043** (0.019)
T4 × Subsidy year	0.007 (0.026)	0.011 (0.019)
Control mean, no payment stress	0.140	0.090
Control mean, payment stress	0.134	0.069
Application fixed effects	Yes	Yes
Billing-month fixed effects	Yes	Yes
Observations	39,120	39,120
Applications	1,630	1,630

Notes: The unit of observation is application × month. Baseline payment stress equals one if the household reports difficulty paying its electricity bill, non-payment or late payment, or borrowing or reducing other expenditure to finance electricity payments. Panels A and B report treatment effects without and with baseline payment stress; Panel C reports the difference in treatment effects between these groups. Debt outcomes are defined as in Table 5. Estimates use application and billing-month fixed effects over Jan 2022–Dec 2023; standard errors are clustered by application. Control means are subgroup-specific subsidy-year means. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the subsidy associated with their February consumption, while 96% correctly identified electricity consumption as the basis for the payment calculation. Many respondents required some assistance with the arithmetic, but fewer than 1% were unable to calculate the subsidy. Eighty-three per cent also reported having received the payment associated with their January bill. These patterns make it unlikely that the absence of a demand response primarily reflects a failure to understand or receive the intervention.

A second possibility is that severe electricity-supply constraints limited households' ability to

increase consumption even when the marginal price fell. Baseline supply conditions were indeed restrictive: households reported a median of 15 hours of load shedding per day on days with outages. Appendix Table 7 therefore repeats the main price-exposure contrasts separately for households reporting at most 12 hours of daily load shedding at baseline and those reporting more than 12 hours. The estimates provide some suggestive evidence that supply conditions may matter for particular comparisons, but no consistent pattern in which marginal-price reductions increase consumption among households facing relatively lower load shedding. In particular, the pooled AP + MP contrast remains statistically indistinguishable from zero in the lower-load-shedding group, and the corresponding difference across supply groups is imprecisely estimated. Supply constraints may therefore contribute to the weak demand response in this setting, but the heterogeneity evidence does not indicate that they alone account for the overall consumption null.

Endline reports provide complementary evidence on households' own responses to the subsidy. Among treated households, 72% report that the subsidy left their electricity consumption unchanged, compared with 13% reporting an increase and 15% reporting a decrease. These shares are similar across the four treatment arms. At the same time, 37% of treated households report that the subsidy caused them to pay more attention to the number of electricity units they consumed. Taken together with the administrative estimates, these results suggest that the absence of a demand response is not readily explained by households failing to notice or understand the price incentive. Instead, even where the experiment reduced the marginal price, the predominant response was little change in electricity consumption. The next subsection examines why the resulting expenditure relief instead appears on household debt-repayment margins.

7.2 Liquidity Relief and Debt Repayment

The debt-repayment effects suggest that subsidy-induced expenditure relief affected household financial adjustment rather than increasing electricity consumption. This interpretation is consistent with the heterogeneity in Section 6.5: the repayment effects are most visible among households entering treatment with greater payment stress or outstanding arrears, precisely where additional resources should have the greatest value for meeting unpaid electricity obligations.

The survey evidence indicates that the subsidy relaxed the broader household budget rather than being narrowly earmarked to electricity. At the September midline, while subsidies were still being delivered, 65% of treated households reported that the subsidy had affected their living situation. Among these households, 51% reported that it helped pay electricity bills and 55% reported that it helped pay other bills or household items. Endline responses show a similar pattern: 58% of treated households report using the subsidy to pay the electricity bill, while 54% report using it for other household expenses and 53% report that the subsidy reduced household expenses (Appendix Table 9).

These responses therefore do not suggest that households narrowly earmarked the subsidy to electricity. Instead, they are consistent with broad expenditure relief. The administrative results identify reduced utility debt as one important balance-sheet response to that relief: households

reduced outstanding electricity obligations even though electricity consumption did not increase.

The survey does not distinguish payments towards current electricity charges from payments towards accumulated arrears, so it does not directly identify the source of the administrative debt-repayment effect. Taken together, however, the evidence supports a consistent interpretation: the subsidy relaxed household budget pressure broadly, while one measurable consequence of that relief was greater repayment of outstanding utility debt rather than increased electricity consumption.

8 Implications for Electricity Subsidy Design

The results highlight that electricity affordability can reflect economically distinct constraints. Households may consume too little because electricity is expensive at the margin, or they may struggle to finance electricity they have already consumed. These constraints imply different behavioural responses to subsidy policy. A reduction in the marginal price creates a direct incentive to increase consumption, while a reduction in effective expenditure on the existing consumption bundle can instead relieve payment pressure without changing electricity use.

Our experiment distinguishes these margins under nonlinear pricing by generating price exposure that lowers expenditure alone and exposure that also lowers the marginal price. Despite substantial reductions in effective marginal prices, we find little evidence of increased electricity consumption. Instead, several subsidy arms increase debt repayment, with effects most evident among households entering treatment with greater payment stress or outstanding arrears. In this setting, subsidy-induced adjustment therefore appears to operate through household balance sheets rather than through additional electricity demand. Evaluations based only on consumption responses would miss this margin of adjustment.

This distinction matters for subsidy design. If the policy objective is to expand electricity use, the relevant question is whether the subsidy lowers the marginal price faced by households whose consumption is constrained by price. If the objective is instead to improve affordability among households struggling to meet existing payment obligations, expenditure relief on inframarginal consumption may be consequential even when it generates little demand response. More generally, where electricity arrears are widespread, consumer subsidies may partly function as liquidity or debt relief, with benefits appearing through reduced household indebtedness rather than increased consumption.

These results also have implications for utilities and governments evaluating the incidence of subsidy programmes. In our experiment, the rebates were research-funded and did not reduce the amount billed by PESCO. At the same time, the subsidies increased reductions in outstanding customer debt: pooling the four treatment arms, the increase amounts to approximately Rs 0.25 per rupee of realised subsidy expenditure (Appendix Table 5). This highlights a provider-side receivables channel that is typically absent from evaluations based only on consumption responses. For subsidies financed directly through lower utility tariffs, the provider-side financial effect would depend on the mechanical loss in current revenue relative to the induced reduction in outstanding

customer debt.

9 Conclusion

Electricity subsidies are commonly used to improve affordability, but the constraint they relax need not operate through electricity demand. In a randomised experiment with 1,731 households in Pakistan, we vary the tariff blocks receiving a subsidy and the generosity of the rebate. Under the nonlinear tariff, this variation generates economically distinct reductions in average and marginal effective electricity prices, allowing us to compare expenditure relief on existing consumption with price exposure that also creates a direct incentive to consume additional electricity.

We find little evidence that electricity consumption increases, including among households exposed to lower marginal prices and during the period when rebates became more generous. Instead, the subsidies generate adjustment on household financial margins. Treatment increases the frequency and amount of reductions in outstanding utility debt and lowers net debt accumulation, with effects most evident among households entering the experiment with greater payment stress or existing arrears. Survey evidence indicates that the subsidy relaxed the broader household budget, while the administrative data show that one important consequence was greater repayment of outstanding utility debt.

These results show that electricity affordability can reflect more than the price of additional consumption. In many developing-country electricity systems, where subsidies are widespread and unpaid balances are substantial, subsidies can affect household and utility finances through household balance sheets even when electricity demand does not respond. More broadly, evaluating subsidy policy under nonlinear pricing requires distinguishing marginal-price incentives from expenditure relief and tracking the financial margins through which households adjust.

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