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The lifeline electricity tariff classification policy in Ghana: A hit or miss!

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Abstract

We evaluate Ghana's lifeline electricity tariff and the 2022 tightening of the subsidized block from 0–50 kWh to 0–30 kWh. We ask: (1) does the tariff reach poor households better than non-poor; (2) how do households perceive its welfare effects; and (3) what happens when consumption crosses the 30 kWh cutoff? Data come from a mixed-method pilot in Greater Accra and Central Regions: a survey of 1,098 households across 10 ECG districts, plus 10 focus-group discussions and 10 interviews. We combine selection models for beneficiary profiling, a doubly robust IPWRA, and a local regression discontinuity design around 30 kWh. Findings show low coverage and awareness; eligibility reduces bills but delivers muted non-energy welfare gains. The 30 kWh price cliff induces bunching and cuts in energy services, raising deprivation risks for near-threshold users. We recommend smoothing the cliff with micro-steps, rebates for near-miss households, and pairing pricing with efficiency support and information campaigns.

1. Introduction

Access to electricity is fundamental for meeting basic needs, supporting economic activity, and improving human development outcomes. Reliable energy services contribute to employment, health, education, and overall quality of life. As Oyuke et al. (2021, p. 1) note, inadequate electricity infrastructure—particularly electricity deficits—remains a major obstacle to socio-economic progress.

From an efficiency perspective, electricity prices should reflect the long-run marginal cost of supply so that utilities can recover costs and sustain service delivery. In practice, however, tariff setting in many low-income contexts is complicated by macroeconomic volatility (notably inflation and exchange-rate movements), which can drive up tariffs and make electricity less affordable for poor households. Lifeline tariffs address this tension by offering a subsidized block of electricity priced below cost, enabling low-income consumers to meet minimum consumption needs. Mpolo et al. (2020) argue that tariff policy in low-income countries must balance social stability, energy efficiency, and cost recovery, especially where poverty is widespread. Klug et al. (2022) similarly emphasize that lifeline tariffs can protect low-income households and support basic energy access.

The logic of lifeline tariffs rests on a key assumption: electricity demand increases with income, so a subsidized "lifeline" block disproportionately benefits low-income users while higher-income users effectively cross-subsidize through higher consumption and higher tariff blocks (Burgess & Paglin, 1981). In this sense, lifeline tariffs can function as an implicit income-transfer mechanism that supports minimum electricity consumption (Petersen, 1982; He & Reiner, 2016). More broadly, tariff design sits at the intersection of efficiency and equity objectives. Feldstein (1972) highlighted the need for a theory of public pricing that explicitly incorporates distributional goals, and recent work continues to frame electricity tariffs as inherently political and distributional instruments (Klug et al., 2022; Najeeb et al., 2023).

However, lifeline tariffs face well-documented challenges. Poor targeting can allow non-poor households to benefit, while structural constraints can prevent the poorest households from qualifying (Klug et al., 2022; Karim et al., 2024; Ikram, 2025). For example, shared meters in compound houses, reliance on inefficient appliances, limited access to separate connections, and weak consumer information can push poor households above eligibility thresholds or exclude them entirely (SEforALL, 2022). These concerns raise a central question for policy: are lifeline tariffs a hit—effectively protecting the poor—or a miss—delivering limited welfare gains due to weak targeting and behavioral distortions?

This study examines Ghana's lifeline electricity tariff, focusing on its incidence and welfare impacts. Ghana introduced lifeline tariffs before the power sector reforms of the 1990s, and the policy has been revised several times. A major reform in September 2022 reduced the lifeline band from 0–50 kWh to 0–30 kWh (PURC, 2024). The reform aimed to improve the financial sustainability of the power sector, encourage energy efficiency, and better target subsidies to households most in need (World Bank, 2023). Yet the reform also implies that households consuming 31–50 kWh—many of whom may be poor—now pay standard rates, potentially weakening the protective intent of the policy.

Despite the policy's importance, there is limited empirical evidence to guide lifeline tariff design in Ghana. Existing studies address related issues but do not directly evaluate the post-2022 lifeline threshold change. Karimu et al. (2024) assess welfare effects of reversing a residential cross-subsidy; Nkrumah et al. (2021) examine subsidy impacts during the COVID-19 period; and Keener and Banerjee (2005) analyze lifeline tariffs during the 2002–2003 tariff adjustments. A comprehensive assessment of Ghana's current lifeline tariff structure and its tightening remains missing.

Generating such evidence is especially relevant for regulators and development partners. The Public Utilities Regulatory Commission (PURC) requires robust analysis to inform tariff reviews. The World Bank (2023), for example, notes that the sector's financial stress is driven not only by technical and contractual factors but also by persistent shortfalls from below-cost-recovery tariffs. In this context, tariffs must be based on evidence rather than discretion (Perez-Arriaga, 2013; Mburamatare et al., 2022). Motivated by this gap, this study addresses three questions: (1) Does Ghana's lifeline tariff primarily benefit poor households relative to non-poor households? (2) What are the welfare implications for beneficiaries? (3) What has been the impact of tightening the lifeline band from 0–50 kWh to 0–30 kWh, particularly for poor and near-poor households?

This is a pilot study covering two regions—Greater Accra and Central—out of Ghana's 16 regions. Using a mixed-methods approach, we analyze data from a household survey of 1,098 households, 10 focus group discussions, and five expert interviews conducted across 10 districts. The study contributes to the lifeline tariff literature in four ways: it deepens understanding of distributional incidence across poor and non-poor households; sheds light on behavioral responses to threshold-based subsidies; clarifies trade-offs between utility sustainability and affordability; and provides evidence relevant to debates on balancing market efficiency with equity-oriented regulation (Ikram, 2025).

The remainder of the paper proceeds as follows. We first provide a background of the evolution of lifeline tariffs in Ghana, followed by the literature review and methodology. We then present and discuss results, and conclude with policy implications.

2. Evolution of Lifeline Electricity Tariffs in Ghana

The Public Utilities Regulatory Commission (PURC) sets electricity tariffs in Ghana and arbitrates disputes between utilities and consumers. Tariff reform predates wider power-sector reforms and reflects a long-standing effort to balance cost-reflective pricing with affordability (Keener & Banerjee, 2005). Before PURC was established in 1997, tariff setting was often ad hoc. Between 1994 and 1997, Ghana adopted regular adjustments to reflect cost drivers such as inflation and exchange-rate movements, consistent with a policy objective of moving tariffs toward long-run marginal cost. However, a tariff increase of over 300% in May 1997 triggered widespread protests, which contributed to the creation of PURC under Act 538 in October 1997 (Keener & Banerjee, 2005).

Under Act 538, PURC's mandate includes protecting consumer and provider interests, issuing tariff guidelines, and approving utility rates. The Act also emphasizes consumer protection, utility financial viability, uniform pricing across the country, efficient resource use, economic development, and differentiated tariffs across customer classes. In practice, PURC's task is to maintain affordability—especially for poor households—while ensuring revenue adequacy for utilities (Acheampong et al., 2021; World Bank, 2025). Tariffs are typically reviewed based on four key cost drivers: the generation mix (hydro versus thermal), the exchange rate, fuel costs, and inflation.

Ghana's lifeline tariff has been revised multiple times. The lifeline band was set at 50 kWh in 1992 and increased to 100 kWh in 1994. In 1998 it reverted to 50 kWh, with beneficiaries paying a fixed lifeline amount while households above the threshold paid a per-kWh rate. In 2006, the lifeline band was removed and replaced with a wider lowest band, but it was reintroduced in 2007. A major design change occurred in 2018, when the tariff structure shifted toward an increasing-block approach, with a lower rate applied to an initial block of consumption and differentiated service charges across customer groups. The most recent major change came in September 2022**,** when the lifeline threshold was tightened from 0–50 kWh **to** 0–30 kWh**.**

The stated rationale for tightening the threshold was to improve fiscal and sector sustainability and reduce subsidy leakage by better targeting low-income households. Critics, however, argue that the change excludes "borderline poor" households consuming 31–50 kWh**,**

Table 1: Timeline of Changes in Lifeline Tariff Rates

Year	Band	Features	Lifeline Tariff Rate	Service charge (GHp/month)
1992	0-50			
1994	0-100			
Feb-98	0-50	Block Charge, Cedis/Month	2000*	
Sep-98	0-50	Block Charge, Cedis/Month	4000*	
1-May-01	0-50	Block Charge, Cedis/Month	7800*	
1-Aug-02	0-50	Block Charge, Cedis/Month, Exclusive	14,000* (9000**)	
1-Mar-03	0-50	Block Charge, Cedis/Month, Exclusive	18,000* (13000**)	
1-May-05	0-50	Block Charge, Cedis/Month, Exclusive	19,080* (13000***)	
1-May-06	No Lifeline band	All customers consuming 0-300kWh paid GHp700 /kWh)	700*	5,000*
1-Nov-07	0-50	Exclusive/GHp/kWh	9.5	50
1-Jun-10	0-50	Exclusive/GHp/kWh	9.5	150
1-Mar-11	0-50	Exclusive/GHp/kWh	9.5	100
1-Oct-13	0-50	Exclusive/GHp/kWh	15.675	100
1-Jan-14	0-50	Exclusive/GHp/kWh	17.2002	324.5248
1-Jul-14	0-50	Exclusive/GHp/kWh	19.2797	363.7598
1-Oct-14	0-50	Exclusive/GHp/kWh	20.5393	387.5289
1-Apr-15	0-50	Exclusive/GHp/kWh	21.0795	397.7209
14-Dec-15	0-50	Exclusive/GHp/kWh	33.5586	633.1717
1-Apr-16	0-50	Exclusive/GHp/kWh	33.56	633.17
15-Mar-18	0-50	GHp/kWh	27.6858	213
1-Jul-19	0-50	GHp/kWh	30.778	213
1-Oct-19	0-50	GHp/kWh	32.606	213
1-Sep-22	0-30	Exclusive/GHp/kWh	41.9065	213
Feb-23	0-30	Exclusive/GHp/kWh	54.4627	213
1-Jun-23	0-30	Exclusive/GHp/kWh	64.462	213
1-Dec-23	0-30	Exclusive/GHp/kWh	63.4792	213
1-Jul-24	0-30	Exclusive/GHp/kWh	65.6664	213
1-Oct-24	0-30	Exclusive/GHp/kWh	67.6495	213
1-May-25	0-30	Exclusive/GHp/kWh	77.6274	213
1-Jul-25	0-30	Exclusive/GHp/kWh	79.5308	213
1-Oct-25	0-30	Exclusive/GHp/kWh	80.4389	213
1-Jan-26	0-30	Exclusive/GHp/kWh	88.3713	213

*Source: PURC Gazetted Tariffs; ESMAP (2006); * figures in old Ghana Cedis (1000 old Ghana Cedi = 1GHS; GHp100=1GHS); **Government absorbed 5000 out of the lifeline rate; *** Government Absorbed 6080 out of the lifeline rate.*

potentially undermining equity objectives. Common concerns include exclusion errors near the cutoff, leakage to better-off households (including through multiple meters), and limited integration with income data or social protection systems.

Overall, the evolution of Ghana's lifeline tariff underscores a persistent policy trade-off: maintaining affordability and equity while moving toward cost-reflective tariffs and improving utility finances. Recent reforms—especially the reduction to 30 kWh—have intensified debates about targeting accuracy and the welfare consequences for low-income and near-poor households.

3. Literature Review

3.1 Theoretical Review

The case for lifeline tariffs rests on the idea that public policy can shape resource distribution so that all social groups can meet basic needs. Accordingly, the theoretical foundations of lifeline tariffs draw on social protection theory, the energy justice framework, and welfare economics.

3.1.1 Welfare Economics

From a welfare economics perspective, lifeline tariffs are targeted consumption subsidies intended to raise the welfare of poorer households while limiting market distortions (Komives et al., 2005). They are often justified through the equity–efficiency logic of Ramsey pricing: lower prices for essential consumption support distributional objectives, while higher prices for larger users contribute to cost recovery (Ramsey, 1927; Crew & Kleindorfer, 1976). This approach typically assumes that low-consumption households are poorer and therefore more vulnerable to price increases. Feldstein (1972) further argues that public pricing frameworks should explicitly incorporate distributional equity.

In practice, however, quantity-based subsidies can target poorly and create exclusion risks. As Trémolet and Binder (2010) note, poor households are not always low consumers, and lifeline rates can become regressive when poor families exceed the subsidized block. This can occur in multi-tenant dwellings, where shared meters aggregate consumption, or where electricity is informally resold to unconnected neighbors. More broadly, as Schüring and Loewe (2021) emphasize, no single social protection instrument is sufficient in all contexts, underscoring the need for complementary targeting approaches.

3.1.2 Social Protection Theory

Social protection refers to policies and programs that protect people against poverty and risks to livelihoods and well-being (Trémolet & Binder, 2021, p. 1). Its benefits are often described through preventive and promotive functions—preventing households from falling into poverty and supporting pathways out of poverty—and through the ability to anticipate and manage future risks. Devereux and Sabates-Wheeler (2004) add a transformative function, emphasizing the role of social protection in reducing structural barriers to equality, inclusion, and social mobility, thereby supporting sustainable livelihoods. The World Bank (2001) similarly frames social protection as supporting risk prevention, mitigation, and coping.

In this context, lifeline electricity tariffs align closely with social protection objectives: they aim to reduce energy poverty and inequality by ensuring that vulnerable households can access a basic level of electricity at subsidized rates. As Komives et al. (2005) note, lifeline tariffs can be understood as a form of indirect social protection intended to shield low-income households from energy poverty.

3.1.3 Energy Justice Framework

The Energy Justice Framework (EJF) is an emerging approach that emphasizes fairness in energy access, distribution, and decision-making. It developed in response to concerns that energy systems can reproduce and intensify inequalities, often harming marginalized communities. Jenkins et al. (2016) describe energy justice as a cross-cutting research agenda that applies justice principles to energy policy, production, consumption, activism, security, and climate change. Sovacool and Dworkin (2015) define an energy-just system as one that equitably shares the benefits and burdens of energy services and treats people and communities fairly in energy decision-making. Trakic et al. (2024) conceptualize the framework through three dimensions: distributive justice (fair distribution of energy benefits and costs), procedural justice (inclusive participation in decisions), and recognition justice (respecting and addressing the needs of vulnerable groups).

The European Union Joint Research Centre (EUJRC) (2024) argues that energy justice adds a human dimension to energy systems that are often assessed mainly through technical and economic lenses. It asks how justice principles can guide decisions about the production, delivery, use, and impacts of energy. In policy practice, the EU's Green Deal similarly stresses that the transition must be fair and inclusive, with specific attention to reducing energy poverty and protecting those most vulnerable (EUJRC, 2024). As Macauley et al. (2019) note, an energy justice lens helps ensure that policies and programs promote equitable access to energy resources and technologies.

Together, welfare economics, social protection, and energy justice provide complementary lenses for analyzing lifeline tariffs. Welfare economics clarifies efficiency–equity trade-offs; social protection frames lifeline tariffs as a safety net against energy poverty; and energy justice provides criteria for assessing fairness in access, targeting, and decision-making. Collectively, these perspectives support the design and evaluation of lifeline policies that advance human well-being, equity, and inclusive development.

3.2 Empirical Review

Research on electricity pricing highlights persistent trade-offs between household welfare and utility financial sustainability. For Ghana, Kutorste (2022), using GLSS 7 and demand-system methods, shows that household energy demand responds to prices, income, and socio-demographic characteristics, with electricity and LPG appearing income-elastic. Quartey et al. (2022), using contingent valuation with over 3,000 households, find that mean willingness to pay for electricity is below the average tariff paid, suggesting affordability pressure. Evidence from other contexts similarly links higher income to higher electricity demand through asset acquisition (Fullerton et al., 2015).

A growing literature focuses specifically on lifeline tariffs and subsidy design. Keener and Banerjee (2014) find that lifeline tariffs can protect poor households, but awareness and coverage are weaker in rural areas. Klug et al. (2022), reviewing 82 studies, conclude that while lifeline protections can support basic consumption, quantity-based designs are often poorly targeted and can be regressive; they also highlight generally price-inelastic demand and the need to pair tariff increases with protections for low-income households. Evidence from Pakistan shows improved targeting over time but continued concentration of benefits among richer households, reinforcing the need to identify who the poor are and to set thresholds accordingly (Trimble et al., 2014). Related work also cautions that poor households may consume above common lifeline thresholds due to household size, shared meters, or informal resale, increasing exclusion risks (Trémolet & Binder, 2010; Prasad, 1983).

Empirical studies across Africa report mixed performance. For Ghana, Boakye (2020) and PURC (2023) note both leakage to non-poor low users and exclusion of unconnected poor households. Similar patterns appear elsewhere: in Kenya, lifeline subsidies improve affordability but miss many of the poorest (Beyene, 2021); in South Africa, free basic electricity reduces energy poverty but excludes off-grid poor households (Tewari & Shah,

2022); and in Zambia, subsidies are highly regressive, with most benefits accruing to richer households and measurable welfare losses under price increases (Maboshe et al., 2019).

Given targeting limitations, some studies examine alternatives or complements. Adofo and Koomson (2025) find that unconditional cash transfers can reduce energy poverty, with stronger effects for female-headed households. Evidence from COVID-19 relief in Ghana also shows that consumption- or meter-based designs can be regressive because higher users receive larger transfers, underscoring the importance of account and metering structures (Berkouwer et al., 2022; Nkrumah et al., 2021). Other proposals include energy stamps, weatherization, and time-of-day pricing (Kelly et al., 2016).

For Ghana's recent tightening of the lifeline band, Karimu et al. (2024) simulate tariff scenarios using GLSS data and show that residential tariff increases reduce welfare for lower-income households, with heterogeneous effects across income groups and trade-offs between equity and utility revenues. Ampofo et al. (2021) further suggest that householdsin the 30–50 kWh range are disproportionately poor, implying that reducing the threshold from 50 to 30 kWh may have unintended equity costs.

Overall, the literature suggests two consistent concerns: (i) lifeline tariffs are often sub optimal because consumption-based eligibility can produce both leakage and exclusion, and (ii) tightening thresholds may not deliver the intended efficiency gains when demand is price-inelastic among poor households, while potentially increasing welfare stress for households clustered near the cutoff.

4. Data and Method

4.1 Sampling design and framework

We employed a purposive and systematic sampling strategy to select survey areas and households. The study focused on Electricity Company of Ghana (ECG) districts in the Greater Accra and Central Regions. Given the high population concentration in Greater Accra, six ECG districts were selected to capture socio-economic variation and ensure adequate spatial dispersion. In the Central Region, four ECG districts were selected using the same criteria. Table 1 presents the selected ECG districts and their corresponding regional demarcations.

Sample size determination followed a multi-stage process. First, we set a total target sample of 1,098 households. This total was allocated across regions based on their shares of the total ECG customer population. District-level sample sizes were then determined using district population shares within each region. In practice, this allocation was implemented

flexibly to account for non-response during fieldwork. In areas with high non-response, the number of interviews was slightly increased in districts where households were more willing to participate. This adjustment explains differences between the expected and actual sample sizes.

Given the relatively small sample sizes compared with the underlying populations, and due to cost considerations, we did not strictly apply the conventional approach of dividing the population by the sample size to determine the sampling interval. Instead, we used a modified systematic sampling approach with a 100-factor rule. Specifically, the sampling interval was calculated as:

Sampling interval = Population ÷ (Expected sample size × 100).

During fieldwork, enumerators were positioned approximately 100 meters apart within each community. For example, in Mampong, this approach required each enumerator to administer questionnaires to every 9th household (34,700 ÷ (37 × 100)). The same procedure was applied consistently across all districts.

Table 1: Sample selection breakdown

Administrative Region	ECG Regional Demarcation	District	Customer Pop.	Expected sample by rule	Expected Total Regional sample by rule	Actual sample Collected	Actual Total
Greater Accra Region	Accra East region	Mampong	34,700	37	248	47	253
		Dodowa	92,616	99		95	
		Adenta	102,998	110		111	
	Accra West region	Achimota	148,344	159	358	155	347
		Amasaman	77,488	83		78	
		Dansoman	107,718	116		114	
Central Region	Central region	Kasoa South	124,010	133	492	127	498
		Kasoa North	140,134	151		154	
		Winneba	74,287	80		94	
		Cape Coast	118,990	128		123	
		TOTAL	1,021,285		1,098	1098	1098

The sampling approach used in this study has the following limitations. First, focusing on selected ECG districts within the Greater Accra and Central Regions does not permit full

national representativeness. Therefore, the findings should be interpreted as regionally representative rather than nationally generalizable. Second, even though our sample allocation was initially defined as been proportional to customer population shares, the adjustments we made to address non-response introduce minor deviations from strict proportionality across districts. Third, modifying the systematic sampling approach using the 100-factor rule, though cost-effective and operationally feasible, may increase the risk of sampling bias relative to a fully randomized household listing. Finally, the relatively small sample size compared to the underlying population limits the precision of district-level estimates. Despite the sampling limitations, our sampling design remains appropriate for assessing distributional patterns and welfare implications of the lifeline electricity tariff within the selected regions.

4.2 Data and variables

We address three questions in this study: (1) Who benefits from the lifeline tariff? (2) What is the impact of the lifeline tariff policy on welfare? and (3) What are the welfare implications of tightening the lifeline tariff block from 0–50 kWh to 0–30 kWh? To answer these questions, we conducted a household survey in Ghana across 10 ECG districts between July 15 and August 15, 2025, covering 1,098 households. The survey collected information on electricity billing expenses; billing and connection characteristics (e.g., meter type and ownership); household demographics (e.g., gender and education of the household head, and household size); dwelling characteristics (e.g., dwelling type and number of households sharing a dwelling); appliance ownership; and location identifiers.

The survey also captured welfare outcomes, including annual household income and expenditure aggregates—education, health, food and water, and mobile data/airtime expenditures. We constructed logarithms of these outcome variables where appropriate. **Table 2** presents the descriptive statistics.

Table 2: descriptive statistics of variables

Var	N	Mean	Standard dev	Min	Max
Categorical					
Hh_educ	1098	NA	NA	NA	NA
Dwelling type	1098	NA	NA	NA	NA
Binary					
Meter_type (prepaid)	1098	0.734	0.442	0	1
Meter_own (shared)	1098	0.514	0.500	0	1

Gender of head (male)	1098	0.417	0.493	0	1
Continuous					
Electricity expenditure	1070	2,386.83	2,737.76	16	30,000
Household income	1051	37,837.72	39,869.2	0	480,000
Food & water expenditure	1098	21,924.04	31,355.95	0	730,000
Health expenditure	1098	2,350.72	4,377.393	0	60,000
Educational expenditure	1098	4,437.17	7,828.39	0	100,000
Mobile data & credit exp.	1098	1,592.837	2,498.77	0	36,000
No. Households sharing a dwelling	1096	4.158	3.575	0	30
Total stock of appliance	1098	10.176	6.608	0	88
Household size	1098	4.519	2.794	1	25
Electricity Consumption (kWh)	1070	340.692	1,359.702	2.24	31,187

Note: Except for total stock of appliance, household size, and number of households sharing a dwelling that depict information at the time of the survey, all the other continuous variables refer to annual figures. In the case of electricity, the 3-month average information was converted to an annual figure. Some respondents could not produce/were unwilling to disclose their billing information at the time of the survey. Similarly, some respondents decided not to disclose their income information at the time of the survey.

In addition to the household survey, we conducted focus group discussions and interviews with experts and key stakeholders to capture shared experiences of the lifeline tariff's welfare implications, households' coping strategies, and the policy's technical and operational feasibility. This qualitative component strengthens the external validity of the quantitative findings derived from the survey.

4.3 Identification of treatment and control groups

We define households whose actual electricity consumption falls within the current lifeline threshold (0–30 kWh) as the treated group, and those with consumption between 31–50 kWh as the control group. Before the lifeline block was tightened in September 2022, households consuming 0–50 kWh qualified for the lifeline tariff. We therefore do not expect households that previously qualified but lost eligibility under the new threshold to differ markedly in socio-economic characteristics from those that remain eligible. The tightening was

motivated by the view that increased use of energy-efficient appliances would reduce consumption among lifeline households.

Accordingly, we assume that if previously eligible lifeline households adopted more efficient appliances, their unconstrained consumption would fall below 50 kWh. This is a strong assumption, as not all households may have transitioned to energy-efficient appliances. However, if socio-economic characteristics are broadly similar between the 0–30 kWh and 31–50 kWh groups, then differences observed between them should largely reflect the policy change—primarily through its impact on electricity bills.

Table 4 reports descriptive statistics by consumption block. We focus on columns (2) and (3), which present statistics for current lifeline households and the "near-neighbor" control group, respectively. The final column reports mean-difference tests between these two groups. With the exception of mobile data/airtime expenditure—which likely reflects behavioral differences—there are no statistically significant differences in income or in expenditures on food and water, health, and education. In contrast, electricity bill expenditure differs significantly between the treated and control groups, suggesting that the observed differences are driven by the tightened eligibility threshold rather than underlying socio-economic differences.

Table 4: Descriptive statistics of outcome variables

Outcome var	(1) **<= 50 kWh** Mean	(2) **0 – 30 kWh (T)** Mean	(3) **31 – 50 kWh (C)** **Mean**	**Mean diff test** **(2 & 3)** **Ho: diff ! =0**
Annual Average Income	38,994.1 (111)	35398.71 (58)	42,928.68 (53)	t= -0.943 [0.3476]
Annual Food & water expense	22,507.12 (125)	23,821.74 (66)	21,036.53 (59)	Tstat=0.6522 [0.5155]
Annual Health expense	1,637.07 (125)	1,565.333 (66)	1,717.322 (59)	Tstat=-0.361[0.7188]
Annual Education expense	3,969.64 (125)	3,654.697 (66)	4,321.949 (59)	Tstat=-0.4605[0.6460]
Annual mobile data and credit expense	1,344.88 (125)	814.2424 (66)	1,938.475 (59)	Tstat=-1.9910[0.0487]
Annual average elec exp	258.08 (125)	162.7879 (66)	364.678 (59)	Tstat=-18.7139[0.000]

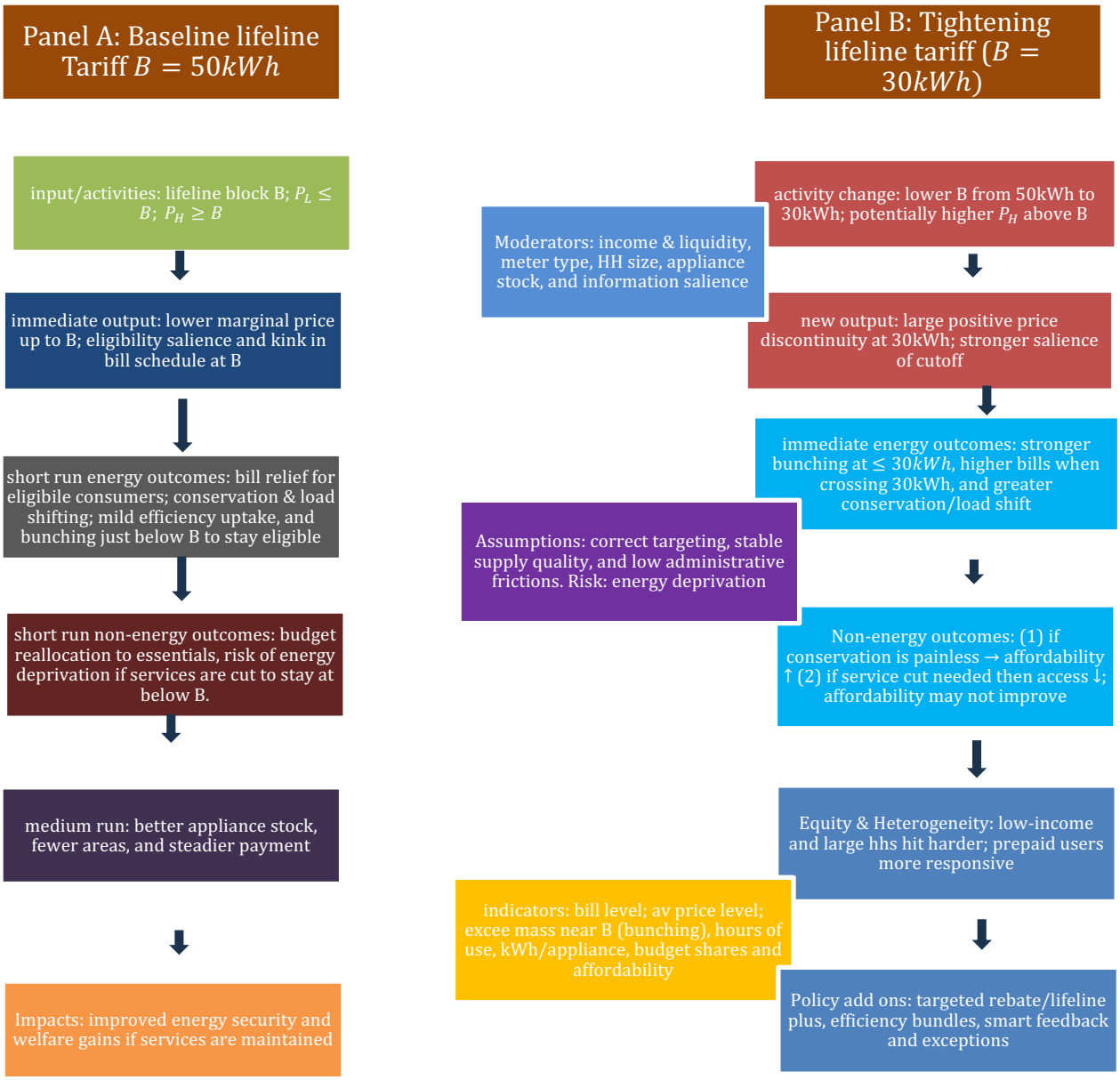
Figures in () are the number of observations

4.4 Theory of change of lifeline tariff policy and tightening

Lifeline tariff policy is a pro-poor intervention by the government to make electricity affordable and accessible to the less privileged in societies. It is implemented by providing tariff subsidy relief to households whose consumption fall within the policy threshold. Figure 1A shows the mechanism of impact of instituting a lifeline tariff policy on bills and other broad welfare indicators.

Intuitively, by instituting the lifeline pricing, the marginal price for households in the lifeline tariff block reduces, creating a salient kink in households electricity bill schedule. For eligible households, this creates bill relief and could induce energy conservation and load-shifting behaviours (see Panel A).

Technological innovation and penetration that improves energy efficiency is one of the reasons that may motivate why the government/regulator will find it useful to tighten the lifeline block. By tightening the lifeline tariff block, the marginal price for previously qualified households but who loses eligibility because of the tightening face a steeper bill schedule beyond the threshold, with the rate being much higher for those whose consumption exceeds the threshold by a marginal amount (see Panel B). Given this, the positive discontinuity in price becomes more consequential. Thus, households may either cross the threshold and face steep bill schedule or decide to bunch below the threshold by cutting down on energy needs/services. In the end, the net welfare effect of tightening the policy is driven by whether energy conservation could be achieved without compromising supply and demand for energy services. This may subsequently depend on the setting. Hosueholds whose consumption fall within 31-50 kWh and have limited liquidity, prepaid salience, and constrained appliance efficiency, for example, may be subjected to high bill pressure, which could increase energy deprivation especially for those who may decide to bunch to remain eligible. In such a situation, the broad general welfare gains of the policy may be limited. Thus, whether tightening the lifeline block will create general welfare gains beyond bill relief depends on the prevalence of bunching among eligible households who wants to stay eligible, and the bunching strategy adopted.



Note: RD at 30 kWh for local bill jump and bunching; DR-IPWRA for welfare effects, and heterogeneity by appliance stock, metre type, income and household size.

Figure 1A: Theory of change of lifeline tariff policy & Tightening (50 kWh to 30 kWh)

4.5 Empirical strategy

4.5.1 Objective 1: Identification and Profiling of beneficiaries of LLT

First, we determine beneficiaries through profiling and consumption pattern. To identify lifeline tariff customers, we use the previous three-month average consumption of the household at the time of the survey. This is because consumption behaviour influences the tariff blocks. So, it is possible for a household to transition between tariff blocks between months. We did not aggravate the problem of measurement errors by going further back into the past because of the difficulty of households to produce actual bills beyond three months, something we picked from the pre-testing of the questionnaire. Our preferred approach was to obtain a

five-year consumption data of households based on the meter numbers we picked from the field from the Electricity Company of Ghana, but this was not possible due to data challenges at the ECG. Even though we asked respondents to indicate whether they benefit from a lifeline tariff or not, we are careful to use this information for reasons of misrepresentation and misclassification.

Table 3 shows the cross-tabulation evidence of households by consumption block classification and by yes/no response to whether they benefit or not from a lifeline tariff. A total of 399 (representing 37.4%) households indicated they do not know whether they were benefiting from the current lifeline tariff threshold of <=30kWh. Out of this, 28 households (representing 7%) actual electricity consumption over a three-month period fell within the lifeline tariff, while the remaining households had actual consumption beyond this threshold.

For those who said yes to the question "are you currently benefiting from lifeline tariff below 30 kWh", they were 66 households in total. However, only 23 of these households had the actual consumption falling within the lifeline consumption block. For those who said no, there are 603 households in total, and 15 out of this number had their actual consumption within the lifeline consumption block. Clearly, relying on households own identification as beneficiaries could be problematic due to the misrepresentation. Due to these constraints, we based our identification of beneficiaries of the lifeline consumers by their average three-month consumption bill.

Table 3: cross-tabulation table

Are you currently benefiting from LLT under 30 kWh threshold?	**Consumption Categories**			**Total**
	0 – 30 kWh	31 – 50 kWh	> 50 kWh	
Do not Know	28	27	344	399
Yes	23	4	39	66
No	15	28	560	603
Total	66	59	943	1,068

Next, to determine beneficiaries through profiling, we use binomial and multinomial logit regressions to estimate selection into different treatment level blocks conditioned on pre-treatment covariates. For the binary logit regression, we use the treatment variable T_{mi} (1 if 0 – 30 kWh and 0 if >30kWh) to predict the probability of selecting into the current lifeline block relative to the local control group. Thus, we estimate Equation 1, where we control for pre-treatment covariates: gender, education level, household size, stock of electrical appliances, dwelling characteristics (i.e., type and number of households sharing a dwelling), and meter characteristics (type and ownership type) (see Jones et al., 2015 for a literature review on drivers of residential electricity consumption).

$$P_r(T_{3mi} = 1|X_i) = \Lambda(\alpha + X_i'\beta + \gamma_{de}) \quad (1)$$

Male and female-headed households may be differentiated by energy service needs and intensity of use, which could influence beneficiary status. Between male and female, household task, access to resources, and decision-making power may differ, and this can significantly affect energy use behaviour. Raty and Carlsson-Kanyama (2009) found that energy use differs significantly between male and female in specific energy services and for other services, they are indistinguishable. Education may correlate with income status and conservation behaviour. Highly educated households may demand for more energy services and hence affect their appliance portfolio on one hand but on another hand are better positioned to engage aggressively in energy conservation practices. While Gram-Hanssen et al. (2004) found evidence of education level reducing electricity consumption, Zhou and Teng (2013) and Leahy and Lyons (2010) found contrasting evidence. The total stock of appliance if in use correlate strongly with higher electricity consumption, limiting the potential to select into lower consumption blocks. Studies such as Wiesmann et al. (2011), Jones et al. (2015), and Nielsen (1993) found direct link between stock of appliance size and electricity consumption.

Dwelling characteristics may offer varying degree of consumption pooling, which could influence beneficiaries' status. For instance, households that share a particular dwelling with other households may experience consumption pooling, putting them into higher consumption block, albeit individually consumption levels may be lower. Also, consumption and bill pooling discourages the incentive to invest in energy conservation, as such benefits may never be realised in situations where consumption/bill is pooled. Others have also indicated that dwelling type affects electricity consumption. Studies such as Jones et al. (2015), Bedir et al. (2013), McLoughin et al. (2012), and Hamilton et al. (2013) indicate that electricity consumption increases with the degree of detachment of dwelling.

Lastly, meter characteristics can influence consumption behaviour and billing method. Metres that offer real time data can help households adjust behaviour to improve consumption. Such opportunities may not be available to postpaid meters that have ex post bill cycle. There are studies with evidence of electricity reducing effect of prepaid meters (Beyene et al., 2022; Jack and Smith, 2020 inter alia). However, electricity-saving benefits associated with adopting prepaid meters may trigger significant rebound effect (Alfawzan and Gasim, 2019), which could overturn the initial energy savings gains. There is also the risk of perceiving such gains

as an implicit decline in electricity price (Adanu et al., 2025), which could reverse the trend in electricity consumption.

Building on the profiling, we implement a multi-valued treatment specification within a multinomial logit framework, using the same pre-treatment covariates to estimate the probability of selection into each treatment level (0–30 kWh = 1; 31–50 kWh = 2; and >50 kWh = 3). Specifically, we estimate Equation (2), where T_i denotes the treatment category and X_i' is the vector of control variables.

$$P_r(T_i = k|X_i) = \frac{\exp(\alpha_k + X_i'\beta_k + \gamma_{kde})}{\sum_h \exp(\alpha_h + X_i'\beta_h + \gamma_{hde})} \quad (2)$$

4.5.2 Objective 2: Welfare impacts of lifeline tariff policy

Here, using the double-robust inverse probability weighting regression adjustment (DR-IPWRA) with trimming/common support, we estimate the causal effect of eligibility of lifeline tariff using the local control group (31 – 50 kWh) as the comparable group Let Y_i denote the vector of outcome indicators (i.e., electricity bill, income, and aggregate expenditures – health, education, food and water, and mobile data and credit) and $T_{30i} = 1[q_i \leq 30kWh]$ denote treatment variable and $c\ for\ q \in (31 - 50\ kWh)$ as the control group. Equation (3b) estimates the causal effect of the treatment on the outcome variables conditioned on the control variables.

$$T_{30i} = \begin{cases} 1, & q_i \leq 30 \quad Treated \\ 0, & q_i \in (31, 50) \quad local\ neighbour\ control \end{cases} \quad (3a)=$$

$$Y_i = \propto + \beta T_i + \gamma X_i + \varepsilon_i \quad (3b)$$

In an observational data, due to differences in base characteristics, average treatment effect cannot be obtained by observing differences in outcomes (Austin, 2011). Propensity score, which is a balancing score, can be used to estimate average treatment effects (Rosenbaum and Rubin, 1983). Conditioning on the propensity score, the distribution of the base covariates/characteristics become homogeneous between the treated and non-treated. Achieving common support is critical for obtaining unbiased weights and standard error especially for full sample analysis. Thus, our causal identification should rely on achieving common support, unconfoundedness, and SUTVA. In this study, we use the logit regression to estimate propensity score (i.e. Equation 4), controlling for gender, education level, dwelling characteristics, meter characteristics, flexible appliance stock and household size, and district-level fixed effects (i.e., X_i).

$$\hat{e}(X_i) = P_r(T_{30i} = 1|X_i) \quad (4)$$

From Equation 4, we do a distributional plot of the logit propensity score and examine for common support before and after percentile trimming. We then use the inverse probability weights derived from the propensity score (Rosenbaum, 1987) to construct a synthetic sample in which the distributional properties of the base covariates do not differ between the treated and control groups (Austin, 2011). Joffe et al. (2004) note that by weighting the regression by the inverse probability weights, one can estimate the causal effect from the treatment to the outcome indicator.

This study estimates a double-robust IPWRA in which we weight the regression by the inverse probability weight and adjust with treatment and outcome controls. The treatment controls include gender, education, dwelling characteristics, meter characteristics, household size, appliance stock, and district-level fixed effects. The outcome controls include gender, education, dwelling characteristics, meter characteristics, flexible household size and appliance stock, and district-level fixed effects. Compared to other natural experiments techniques such as IV and difference-in-difference, the DR-IPWRA, by combining outcome regression with propensity score weighting, is robust to model misspecification of one (but not both) models (Funk et al., 2011; Robins et al., 2007; Bang and Rubins, 2005). The outcome equation is estimated by Equation (5):

$$O_t(X_i) = \mathrm{E}[Y_i|T_{30i} = t, X_i \qquad (5)$$

The treatment effect is derived as:

$$ATE = \frac{1}{n}\sum_i \left\{\frac{T_i(Y_{30i}-O_1(X_i))}{\hat{e}(X_i)} + O_1(X_i) - \frac{(1-T_{30i})(Y_i-O_0(X_i))}{\hat{e}(X_i)} - O_o(X_i)\right\} \quad (6)$$

To ensure that our DR-IPWRA estimates are internally consistent, we conducted some robustness checks on the results. The first is small sample inference using the wild-cluster bootstrap test. The second is heterogeneity analysis in which we re-estimate the DR-IPWRA by appliance terciles to determine where the gains, if any, associated with lifeline tariff eligibility concentrates. Third, we implement other strategies that improve the external triangulation of the DR-IPWRA results. The first approach estimates a mix multinomial logit with multiple treatment level effects (0 -30 kWh (1); 31 – 50 kWh (2), and > 50 kWh (3)). The other approach estimates the effect of perceived eligibility or not for the lifeline tariff using DR-IPWRA on trimmed common support with wild-cluster bootstrap test. In the survey, respondents were asked to indicate based on the socio-economic characteristics whether they qualify for a lifeline tariff block. Since these do not represent actual eligibility, we estimate intention-to-treat on the chosen outcomes.

Finally, respondents were asked to indicate their level of agreement or disagreement to nine statements on the perceived impacts of a lifeline tariff policy. First, we construct affordability and energy-consciousness index from the likert-scale questions and then implement DR-IPWRA to estimate average treatment effect on outcome indicators (affordability and energy consciousness and other non-energy items) for agree (4+5). Second, we estimate an ordered logit regression of the treatment on the outcomes for agree (4+5) between the treated and control group. Thus, we examine between the treatment and control groups, who is more likely to answer agree (4+5) to the likert-scale questions.

4.5.3 Objective 3: Welfare impact of tightening the lifeline tariff block

Here we define two lifeline block policy regimes: (1) historical (benchmark): lifeline tariff block $B = 50kWh$ and (2) tightened (recent): lifeline tariff block $B = 30kWh$. The prices are piecewise:

$$f(x) = \begin{cases} p_L, & q \leq B \\ p_H, & q \leq B \end{cases} \qquad (7)$$

The discrete bill jump due to the tightening of the benchmark lifeline tariff block is computed as: $EB = p_H B_H - p_L B_L + plus\ any\ fixed\ charges$, which we exploit for identification in the regression discontinuity design in this study. Thus, we backstop the story by estimating the impact of losing eligibility. Under the benchmark lifeline tariff block, households within consumption block 31 – 50 kWh qualified but now loses eligibility because of the tightening of the block to 0 – 30 kWh. This introduces discontinuity in the running variable, electricity consumption, at the threshold of 30 kWh.

We implement a local regression discontinuity design with local-polynomial inference to determine the billing and welfare implications of crossing the cutoff point for the lifeline tariff block. Regression discontinuity design is mild in assumptions (Hahn et al., 2001) and produces more credible causal inference than other natural experimental techniques like IV and difference-in-difference (Lee and Lemiuex, 2010). However, validity of regression discontinuity results relies strongly on (1) continuous potential outcomes and/ distribution of covariates at the cutoff, (2) no manipulation of the running variable at the cutoff, and local SUTVA. We implement formal manipulation test, and other robustness checks to support the RD identification.

We estimate equation (10), where ψ denotes the causal effect of crossing the cutoff, $f_{-,}$ & f_{+}denote the local polynomials estimated with the Calonico-Cattaneo-Titiunik bias

corrected RD using the MSE optimal bandwidth (h) selection, and X_i denotes the outcome controls which is the same as used in the DR-IPWRA regression.

$$Y_i = \alpha + \psi.1[q_i \geq c] + f_{-}(q_i - c).1[q_i < c] + f_{+}(q_i - c).1[q_i \geq c] + X_i'\beta + \gamma_{de} + \varepsilon_i \quad (8)$$

Several robustness checks were conducted to verify the internal consistency of the RD design results. First, to deal with potential problem of heaping, we estimate Donut local RD by dropping $\pm 2\ kWh$ bandwidth around the threshold/cutoff. Second, we use symmetric fix bandwidth. Third, we implement both placebo cutoff and covariate tests. The former tests fake thresholds to ensure there is no spurious jumps. The latter test whether the pre-treatment covariate evolve smoothly in the running variable. Both provide identification support to our local RD design: continuity in potential outcomes/covariates at the policy cutoff point. The last strategy ensures external triangulation of the local RD result. Respondents were asked to indicate their level of agreement to impact of tightening the lifeline tariff block to 30 kWh on five likert-scale question. Like the approach adopted in objective 2, we construct affordability and energy consciousness index and then implement the DR-IPWRA to estimate the treatment effect on these indexes and other non-energy outcomes. Next, we estimate an ordered logit to predict the probability difference in answering to the agree (4+5) to the questions between the treated and non-treated groups.

5. Results and Discussion

This section is divided into three main subsections. The first section addresses objective 1, second section addresses objective 2, and the last section addresses objective 3.

5.1 Knowledge level about lifeline tariff policy

Most respondents have not heard about the lifeline tariff policy; a trend observed across all districts. Figure 1B presents the distribution of responses—*Yes* (blue) and *No* (orange)—to the question of whether the household head was aware of the policy. The widespread lack of awareness suggests that the policy's effectiveness in reaching its intended beneficiaries could be significantly undermined. Poorer households are at risk of missing out on this opportunity, which may compel them to bear additional financial burdens through higher electricity bills

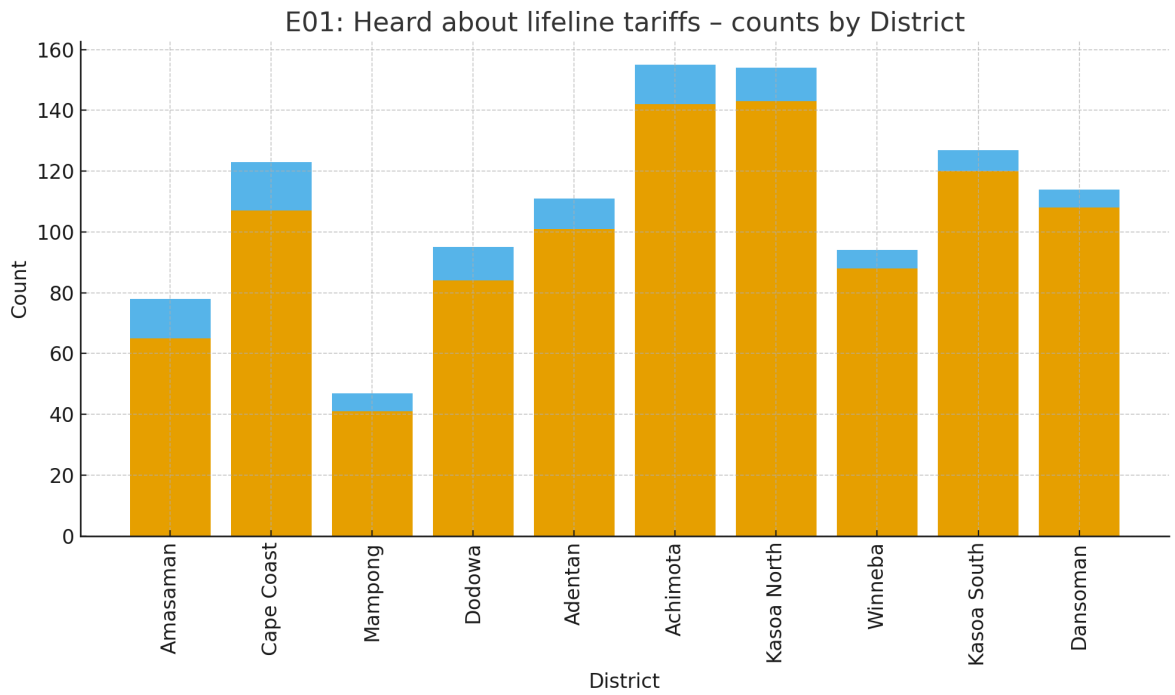


Figure 1B: Plot of whether households have heard of LLT Policy.

5.2 Objective I: Beneficiaries of Lifeline Tariff (Profiling)

Using the three-month average consumption measure, the survey indicates that only 66 households fall within the current lifeline tariff bracket, representing about 6.2% of the sample. Under the previous lifeline block of 0–50 kWh, 125 households would have qualified. Thus, tightening the threshold implies that 59 households lost eligibility. Table 5 reports the district distribution. Nearly half of the 66 current beneficiaries are in Winneba (31 households; 47%), with smaller numbers in Kasoa North (9; 13.6%) and Cape Coast (7; 10.6%).

The tightening from 0–50 kWh to 0–30 kWh resulted in the largest numbers of households losing eligibility in Cape Coast (15), Dodowa (10), and Winneba (10). These patterns reflect district economic characteristics: lower-income districts tend to have more lifeline consumers. As shown in Figure 2, Cape Coast, Kasoa North, and Winneba record comparatively lower incomes, consistent with the policy's intent to target poorer areas. However, under both the old and new blocks, the number of beneficiaries remains small, suggesting that the thresholds may be too restrictive relative to the consumption needs of poorer households. This raises concerns about the policy's targeting effectiveness.

Table 5: Consumption blocks by ECG district level

ECG Districts	0 – 30 kWh	31 – 50 kWh	>50 kWh	Total
Achimota	1 (.68)	1 (.68)	145 (98.64)	147
Adentan	5 (4.5)	4 (3.60)	102 (99.89)	111
Amasaman	1 (1.30)	1 (1.30)	75 (97.4)	77
Cape Coast	7 (5.69)	15 (12.2)	101 (82.11)	123
Dansoman	2 (1.75)	1 (.88)	111 (97.37)	114
Dodowa	4 (4.35)	10 (10.87)	78 (84.78)	92
Kasoa North	9 (6.08)	5 (3.38)	134 (90.54)	148
Kasoa South	4 (3.39)	6 (5.08)	108 (91.53)	118
Mampong	2 (4.26)	6 (12.77)	39 (82.98)	47
Winneba	31 (33.33)	10 (10.75)	52 (55.91)	93
Total	66 (6.17)	59 (5.51)	945 (88.32)	1070

Source: Authors' own construction from field data collected

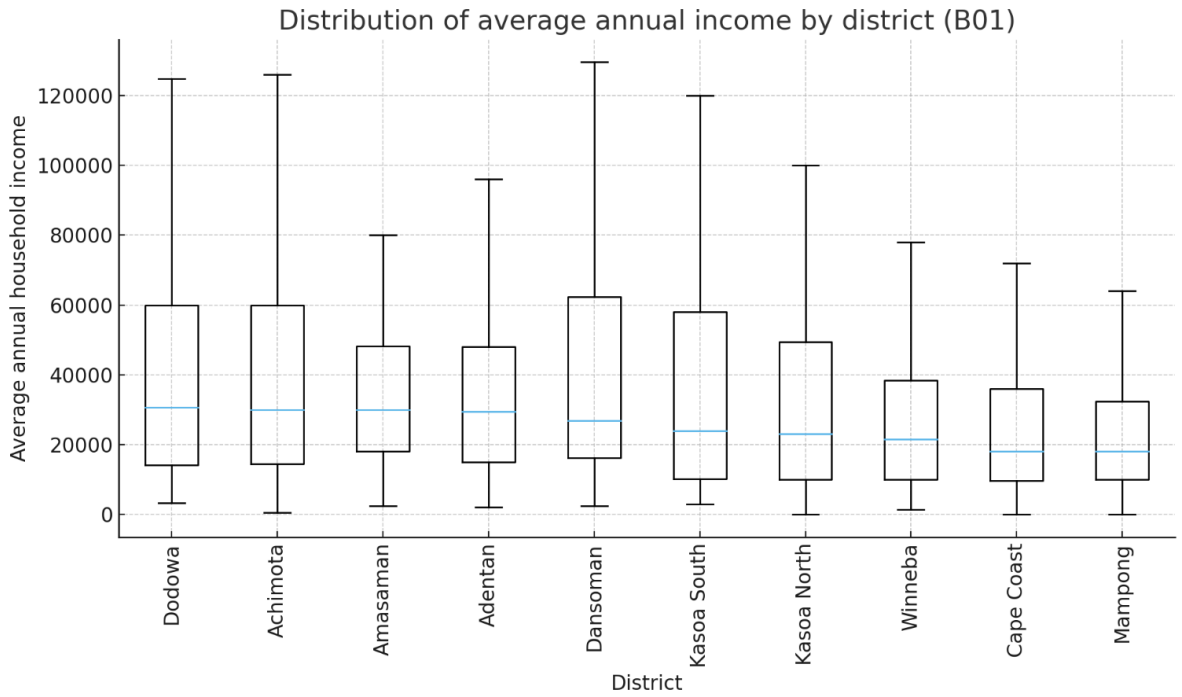


Figure 2: Plot of average income distribution by district level

Next, we profile beneficiaries by estimating a selection model to identify the characteristics associated with households sorting into different consumption blocks. The dependent variable is a three-level treatment outcome: 0–30 kWh = 1, 31–50 kWh = 2, and >50 kWh = 3. We control for district fixed effects, meter characteristics, gender, household size, appliance stock, and the education level of the household head. Table 6 reports the multinomial

logit estimates and marginal effects (with >50 kWh as the base category). For the binomial logit, we focus on two groups: 0–30 kWh and 31–50 kWh.

Regarding meter type, households with prepaid meters are less likely to fall into the lower consumption blocks and more likely to fall into the >50 kWh block, relative to households with postpaid meters; the binomial logit confirms this pattern. This indicates that meter type is an important factor shaping selection into consumption categories. Households with shared meters are also less likely to fall into the lower consumption blocks, although the estimates are weaker statistically. Larger households are less likely to fall within the lifeline block, and appliance stock strongly predicts selection out of the lifeline threshold. Dwelling type shows limited explanatory power: coefficients for compound houses and apartments are generally not statistically significant, though the negative sign for apartments suggests a lower likelihood of being in the lifeline block.

Male-headed households are more likely to fall within the lifeline block and less likely to be in the highest consumption block relative to female-headed households. A higher number of households sharing the same dwelling reduces the likelihood of falling within the lifeline block. Education effects are mixed: households with SHS or vocational education are less likely to be in the lower consumption blocks than households with no formal education, while households with technical education are less likely to be in the >50 kWh block.

Overall, the results suggest that postpaid meters, smaller household size, lower appliance ownership, and fewer households sharing a dwelling increase the likelihood of qualifying for the lifeline block. District effects indicate that households in Winneba, Dodowa, Kasoa North, Cape Coast, and Mampong are more likely to fall into the lower consumption blocks, consistent with greater eligibility in poorer areas. These quantitative patterns align with the qualitative findings.

Table 6: Profiling of lifeline beneficiaries

Dependent variable: Consumption blocks

Variables		**Dy/dx _ multinomial logit**	**Dy/dx _logit**
Meter_type (ref: postpaid)			
Prepaid	0-30 Kwh	-.0426** (.018601)	-.041** (.0178161)
	31-50 KWh	-.0355* (.0206827)	
	>50 KWh	.078*** (.0253755)	
Hsize	0-30KWh	-.006** (.0028498)	-.006** (.0027793)
	31-50 KWh	.002 (.0020894)	
	>50 KWh	.004 (.0030643)	

Meter Ownership (ref: not shared)			
Shared	0-30KWh	-.014 (.0146613)	-.014 (.0145922)
	31-50 KWh	-.002 (.0148426)	
	>50 KWh	.015 (.0183841)	
Total appliance stock	0-30KWh	-.016*** (.0030202)	-.015***(7.57e-08)
	31-50 KWh	-.007*** (.0021543)	
	>50 KWh	.022*** (6.37e-13)	
Dwelling type (ref: Single)			
Apartment	0-30KWh	-.012 (.0393879)	-.016 (.0356182)
	31-50 KWh	-.014 (.022493)	
	>50 KWh	.027(.0412278)	
Compound House	0-30KWh	.004 (.0162668)	.005 (.0162295)
	31-50 KWh	.016 (.0169724)	
	>50 KWh	-.020(.0214467)	
Gender (ref: female)			
Male	0-30KWh	.036** (.0143691)	.037*** (.0141376)
	31-50 KWh	-.006 (.01402)	
	>50 KWh	-.030 (.0186772)	
No. HHs sharing dwelling	0-30KWh	-.005*** (.0022349)	-.004** (.0021836)
	31-50 KWh	-.0002 (.0021433)	
	>50 KWh	.005* (.0028141)	
Education (ref: no formal education)			
Primary	0-30KWh	.022 (.0255154)	.019(.0253995)
	31-50 KWh	.006 (.0277005)	
	>50 KWh	-.028 (.0351483)	
JHS	0-30KWh	.001 (.0210928)	-.001(.0211121)
	31-50 KWh	.005 (.0212398)	
	>50 KWh	-.006 (.0267141)	
SHS	0-30KWh	-.038* (.0197962)	-.039* (.0200037)
	31-50 KWh	-.005 (.0207801)	
	>50 KWh	.042 (.0259512)	
Vocational	0-30KWh	-.070*** (.0169684)	
	31-50 KWh	-.053*** (.0173423)	
	>50 KWh	.123*** (1.18e-08)	
Technical	0-30KWh	.167 (.1052779)	.155 (.1057044)
	31-50 KWh	-.053*** (.0173423)	
	>50 KWh	-.114 (.1066368)	
Tertiary	0-30KWh	-.013 (.0298421)	-.018 (.5169563)
	31-50 KWh	.026 (.0317016)	
	>50 KWh	-.012 (.0392729)	

District (ref: Achimota)			
Adenta	0-30KWh	.033 (.0281799)	.038(.0265834)
	31-50 KWh	.029 (.0222484)	
	>50 KWh	-.062* (.0344284)	
Amasaman	0-30KWh	.022 (.0391092)	.014 (.0335713)
	31-50 KWh	.011 (.0242052)	
	>50 KWh	-.033 (.0437226)	
Cape Coast	0-30KWh	.066** (.0336017)	.075** (.0328601)
	31-50 KWh	.128*** (.0366675)	
	>50 KWh	-.194*** (.0407944)	
Dansoman	0-30KWh	.012 (.0264582)	.014 (.0243173)
	31-50 KWh	.0004 (.0163652)	
	>50 KWh	-.013 (.0300422)	
Dodowa	0-30KWh	.017 (.0269307)	.022 (.0248606)
	31-50 KWh	.088*** (.03095)	
	>50 KWh	-.105*** (.0391636)	
Kasoa North	0-30KWh	.016 (.0251462)	.019 (.0222254)
	31-50 KWh	.019 (.0170354)	
	>50 KWh	-.034 (.0295989)	
Kasoa South	0-30KWh	.008 (.0239005)	.009 (.0205015)
	31-50 KWh	.036 (.0235646)	
	>50 KWh	-.044 (.0326305)	
Mampong	0-30KWh	.070 (.0540782)	.079 (.0546238)
	31-50 KWh	.143** (.061158)	
	>50 KWh	-.212*** (.0683687)	
Winneba	0-30KWh	.108*** (.0329466)	.114*** (.0310871)
	31-50 KWh	.047** (.0224685)	
	>50 KWh	-.155*** (.0385736)	
Pseudo R2		0.2954	0.376
Log pseudo loglikelihood		-332.191	-155.2744
Wald Chi2(96)		1120.54	139.05
Prob> Chi2		0.000	0.000
Observation		1064	1087

Robust standard errors in (). ***, **, and * denotes 1%, 5%, and 10% statistical significance

The qualitative evidence indicates that the current lifeline tariff does not consistently reach the poorest households. Intended beneficiaries—low-income and vulnerable groups—are often excluded due to limited electricity access, shared-meter arrangements, and informal housing conditions. Instead, the policy tends to benefit low-to-middle-income and urban-poor households with existing connections, and in some cases non-poor households that maintain

low recorded consumption or exploit gaps in the metering system. Because eligibility is based solely on kWh consumption rather than socio-economic indicators, both poor and non-poor households can qualify; respondents cited cases where affluent households qualified because unoccupied homes recorded low usage.

Challenges in identifying and reaching true beneficiaries are largely structural and data-related. In compound houses, shared meters aggregate multiple tenants' consumption, pushing usage above the threshold and disqualifying all households. Informal housing, weak customer identification systems, and limited meter availability compound these problems. Current metering practices—such as restrictions on providing separate meters to low-consuming tenants—further constrain equitable access.

5.3 Objective II: Welfare Effects of New Lifeline Tariff Block

This section estimates the welfare effects of lifeline tariff policy using the double-robust inverse probability weighting regression adjustment (DR-IPWRA hereafter).

5.3.1 Plot of common support

First, we assess common support by plotting the propensity score distributions for treated and control households. Figure 3 uses the full sample (no trimming). The two histograms are clearly separated, with substantial mass at the extremes: the control group concentrates near 0, while the treated group concentrates near 1. This indicates a positivity (overlap) violation and therefore limited common support. In such settings, weighting or matching methods may rely heavily on extrapolation, yielding unstable weights and unreliable standard errors.

To address this, we apply percentile trimming (2.5th–97.5th percentiles) to improve overlap. The post-trimming distributions are shown in Figure 4. Trimming substantially increases overlap: both groups now share a common propensity score range, with no large piles at the extremes. We therefore proceed with inverse probability weighting with regression adjustment (IPWRA) to estimate the treatment effect of the lifeline tariff policy on the overlap sample (rather than the full sample).

We also implement two additional approaches that are more robust to extreme propensity scores. First, we use overlap-weighted regression adjustment, which places greatest weight on observations in the region of strongest overlap. Second, we restrict attention to a

local kWh window around the policy cutoff, which further reduces sensitivity to tail observations. Both strategies help de-emphasize the extremes and improve identification.

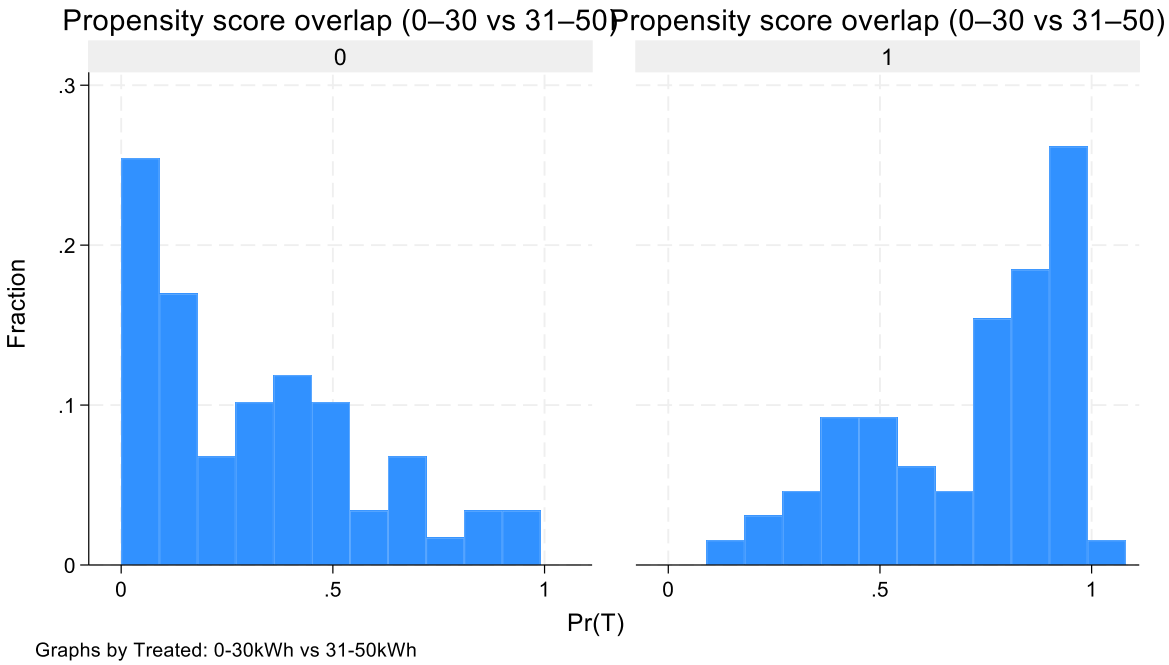


Figure 3: Histogram of Propensity score for treated and control (full sample)

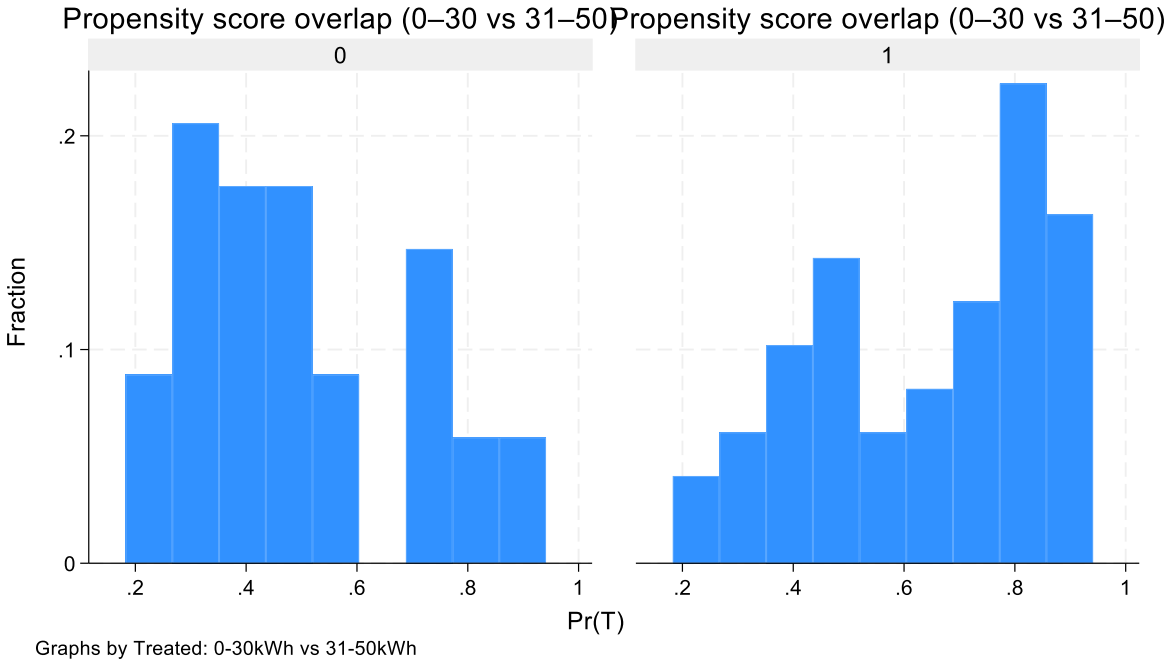


Figure 4: Histogram of Propensity score for treated and control (2.5 – 97.5th percentile trimming)

5.3.2 Using Double-Robust IPWRA

The evidence of common support justifies proceeding with IPWRA. Table 7 reports estimates for the outcome variables under alternative standard-error assumptions. The credibility of IPWRA depends on achieving covariate balance, and across specifications we find strong evidence of balance. The absolute standardized mean differences (SMDs) are zero for most covariates. SMDs for gender, total appliance stock, and secondary education are slightly above the 0.10 guideline but remain well below 0.20, which is generally considered acceptable. The variance ratios lead to the same conclusion: values are close to 1 for most covariates; although household size and appliance stock deviate somewhat, they remain within the conventional 0.5–2 range (see Appendix). Overall, treated and control households (31–50 kWh) are comparable on observed covariates, so differences in outcomes can plausibly be attributed to treatment rather than pre-existing differences.

The estimated average treatment effect on electricity bills is negative and statistically robust across all inference methods. Specifically, households in the 0–30 kWh block have electricity bills that are 56.2% lower than their nearest neighbours in the 31–50 kWh control group $[(\exp(0.826) - 1) \times 100]$. This highlights the strong cost-saving incentive created by the lifeline block and suggests that households may have incentives to bunch just below the threshold to remain eligible. Such bunching could enable budget reallocation—for example, households that successfully keep consumption below the cutoff may free resources for other uses.

We also find evidence of a positive income difference for lifeline households, statistically significant under robust, clustered, and (mostly) bootstrap standard errors. The point estimate implies that households within the lifeline block report incomes that are 54.7% higher than matched households above the threshold, conditional on covariates and within the overlap sample. While this could indicate improved economic welfare among beneficiaries, the strategies households use to bunch consumption could also be welfare-reducing. We examine these mechanisms later; for now, we interpret the income result cautiously and treat it as suggestive of potential gains depending on the bunching strategies adopted.

The potential income gains are consistent with the idea that reduced electricity bills may allow expenditure switching toward other priorities. We find weak evidence for education and health spending, but some evidence of higher spending on food and water and mobile data/airtime following bill reductions. The point estimates suggest increases of 135% and 147%, respectively, relative to the control group. However, these effects are sensitive to

inference—particularly under bootstrap standard errors—so we treat them as suggestive rather than conclusive.

Taken together, the doubly robust IPWRA estimates on the trimmed overlap sample—comparing 0–30 kWh households to matched 31–50 kWh households—show that tightening the lifeline threshold substantially reduces electricity bills and may free resources for other household needs. However, the estimated income and non-energy spending effects should be interpreted with caution given sensitivity to inference, the uncertain welfare implications of consumption-bunching strategies, potential residual selection, and the relatively small sample size.

Table 7: Treatment-effects Estimation of treatment on outcomes

Estimator: double-robust IPW Regression Adjustment

Treatment Model: Logit

Outcome	Effects	MI	MII	MIII	MIV
		Robust S.E	Cluster_Dist.	Cluster_Vill.	Boot. S.E.
Expenditure on Electricity					
	ATE				
	T	-0.8263***	-0.8263***	-0.8263***	-0.8263***
	(1 vs 0)	(0.08377)	(0.1239)	(0.1067)	(0.1733)
	PO (Mean)				
	T	5.8652***	5.8652***	5.8652***	5.8652***
	0	(0.0206)	(0.0345)	(0.0303)	(0.0357)
Obs		124	124	124	124
Income					
	ATE				
	T	0.4363**	0.4363**	0.4363**	0.4363
	(1 vs 0)	(0.1769)	(0.1933)	(0.1761)	(0.4537)
	PO (Mean)				
	T	9.8720***	9.8720***	9.8720***	9.8720***
	0	(0.1477)	(0.2603)	(0.1746)	(0.50082)
Observation		108	108	108	108
Health Expenditure					
	ATE				
	T	0.7814*	0.7814	0.7814	0.7814
	(1 vs 0)	(0.4521)	(0.6381)	(0.5390)	(0.9202)
	PO (Mean)				

		T	5.3212***	5.3212***	5.3212***	5.3212***
		0	(0.3697)	(0.6911)	(0.4655)	(0.5645)
Educational Expenditure						
	ATE					
		T	-0.2875	-0.2875	-0.2875	-0.2875
		(1 vs 0)	(0.6560)	(1.0513)	(0.8233)	(1.4036)
	PO (Mean)					
		T	4.5561***	4.5561***	4.5561***	4.5561***
		0	(0.4735)	(0.4468)	(0.5668)	(0.9898)
Food and Water Expenditure						
	ATE					
		T	0.8561***	0.8561*	0.8561**	0.8561
		(1 vs 0)	(0.3077)	(0.4743)	(0.3782)	(0.6591)
	PO (Mean)					
		T	8.6831***	8.6831***	8.6831***	8.6831***
		0	(0.2620)	(0.3369)	(0.3038)	(0.4435)
Mobile data & Credit						
	ATE					
		T	0.9053***	0.9053**	0.9053**	0.9053
		(1 vs 0)	(0.3122)	(0.4263)	(0.3982)	(0.5825)
	PO (Mean)					
		T	5.4188***	5.4188***	5.4188***	5.4188***
		0	(0.31073)	(0.5141)	(0.4296)	(0.4207)
Cov. outcome			YES	YES	YES	YES
Cov. Treatment			YES	YES	YES	YES
Obs			124	124	124	124

Next, the trimmed common-support sample is small (N = 108–124), which makes the estimates potentially sensitive to small-sample bias. To assess robustness, we conduct a small-sample sensitivity check using wild-cluster bootstrap inference by district for the average treatment effects. Table 8 reports the results. The wild-cluster bootstrap confirms that the estimated reduction in electricity bills for lifeline consumers is internally consistent and statistically significant. In contrast, the estimated income and non-energy spending effects (education, food and water, and mobile data/airtime) are weaker and less precisely estimated, and the evidence for switching toward non-energy expenses is particularly noisy.

Later in the paper, we apply an alternative approach that uses the full sample to mitigate the small-sample issue, but it comes with trade-offs. We therefore treat those results as supplementary (pseudo) robustness checks against the main estimates.

Table 8: Wild cluster bootstrap test for the treatment effect

	Electricity	**Income**	**Education**	**Health**	**Food and water**	**Data and credit**
Wild bootstrap t	-6.3607	1.6216	0.6896	2.8701	0.8507	1.5614
Prob>|t|	0.0058	0.2502	0.5202	0.0030	0.6797	0.4290

Because doubly robust IPWRA can be sensitive to extreme propensity scores—hence the trimming used to achieve common support—we also estimate a high-dimensional fixed-effects regression using overlap weights, which places emphasis on the overlap region. Table 9 presents the results. We again find strong and statistically significant evidence of a reduction in electricity bills following the tightening of the lifeline tariff block from 0–50 kWh to 0–30 kWh. In contrast, the estimated effects on income and non-energy expenditures (education, food and water, and mobile data/airtime) remain in the expected direction but are statistically weak. Evidence of expenditure switching is clearer for health spending.

Overall, these results reinforce the main finding of a substantial bill reduction after the policy tightening, while confirming that the income and non-energy spending effects are relatively weak and noisy.

Table 9: High-Dimensional Fixed-Effect Regression using overlap weights
Treated (1): 0 – 30 kWh
Control (0): 31 – 50 kWh

VARIABLES	(1) Household Income	(2) Electricity Expenditure	(3) Health Expenditure	(4) Educational Expenditure	(5) Food & Water Expenditure	(6) Mobile data & Water
Treatment	0.296	-0.896***	1.336**	-0.183	0.780	0.9412
	(0.226)	(0.139)	(0.493)	(0.954)	(0.594)	(0.5500)
Constant	9.386***	5.315***	5.619***	-4.265**	8.518***	2.7508**
	(0.530)	(0.182)	(1.723)	(1.545)	(0.922)	(0.8996)
Controls	YES	YES	YES	YES	YES	YES
District FE	YES	YES	YES	YES	YES	YES
Observations	108	124	124	124	124	124

R-squared	0.273	0.627	0.278	0.481	0.322	0.427

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

These findings are consistent with the qualitative evidence. The welfare benefits of the lifeline tariff are constrained by the policy's limited reach: where electricity access is restricted or metering inequities persist, the lifeline tariff contributes little to welfare improvement. Several interviewees argued that more meaningful welfare gains would come from community-level electrification—such as public lighting and shared solar infrastructure—rather than household-based subsidies.

5.3.3 Heterogeneity Analysis by Appliance holdings

The weak and noisy estimates for income and non-energy spending may reflect both observed and unobserved heterogeneity, implying that any gains could be concentrated among specific types of households. To explore this, we re-estimate average treatment effects by pre-policy appliance-stock terciles and plot where the effects concentrate.

Figure 5A shows the estimated bill reductions associated with tightening the lifeline block to 0–30 kWh. The largest reductions occur among households with low appliance stock (-0.9406), followed by those with medium (-0.926) and high appliance stock (-0.765). All three effects are statistically significant, reinforcing our earlier finding of a strong and robust reduction in electricity bills.

Figure 5B presents the income effects. The estimated income gain increases from the low appliance tercile, peaks among medium appliance-stock households, and then declines—becoming negative for high appliance-stock households. The effects are statistically significant for the medium and high terciles. In particular, tightening the lifeline block is associated with a positive income effect for medium appliance-stock households (0.774), but a negative effect for high appliance-stock households (-0.460). This suggests that any positive income gains from the tightened lifeline threshold are most likely concentrated among households with medium appliance holdings.

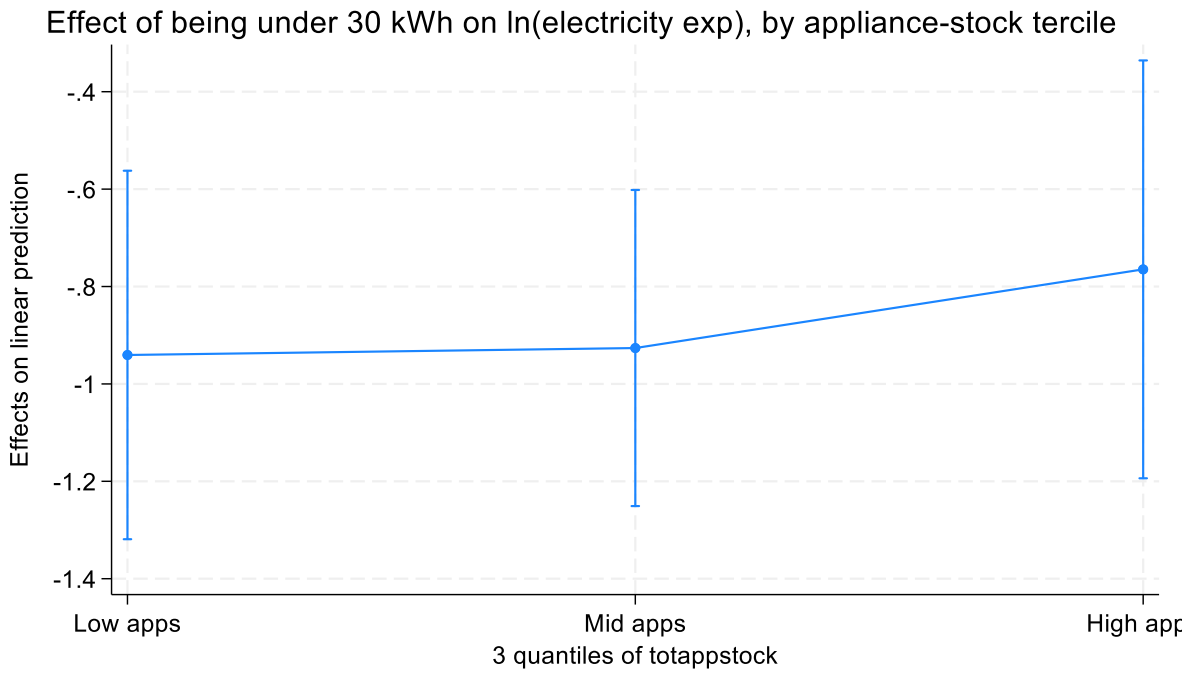


Figure 5A: Treatment effect on electricity bill by appliance terciles

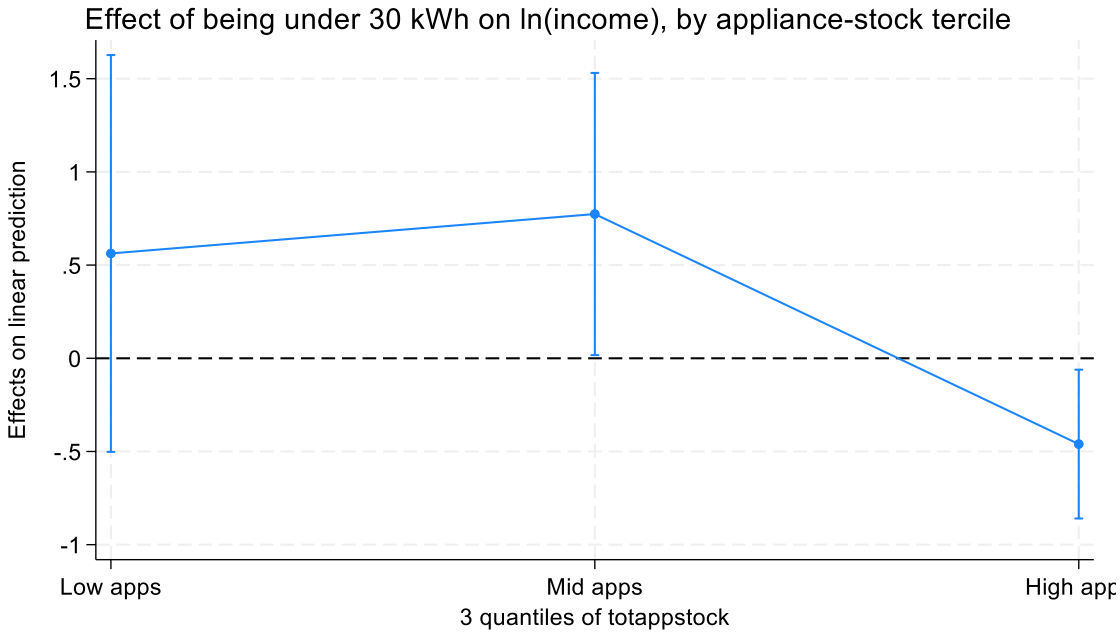


Figure 5B: Treatment effect on household income by appliance terciles

For education, Figure 5C shows heterogeneous effects, but none are statistically significant. This suggests that heterogeneity does not explain the education spending results. Figure 5D shows a similar pattern for health expenditures.

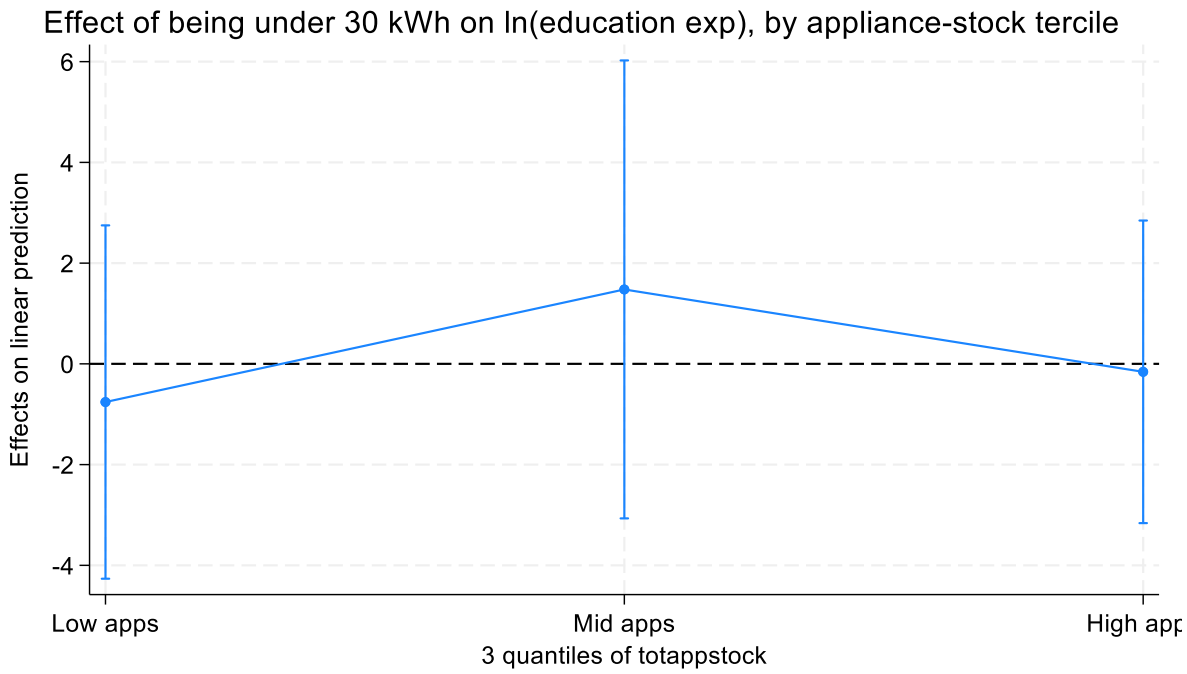


Figure 5C: Treatment effect on household educational expense by appliance terciles

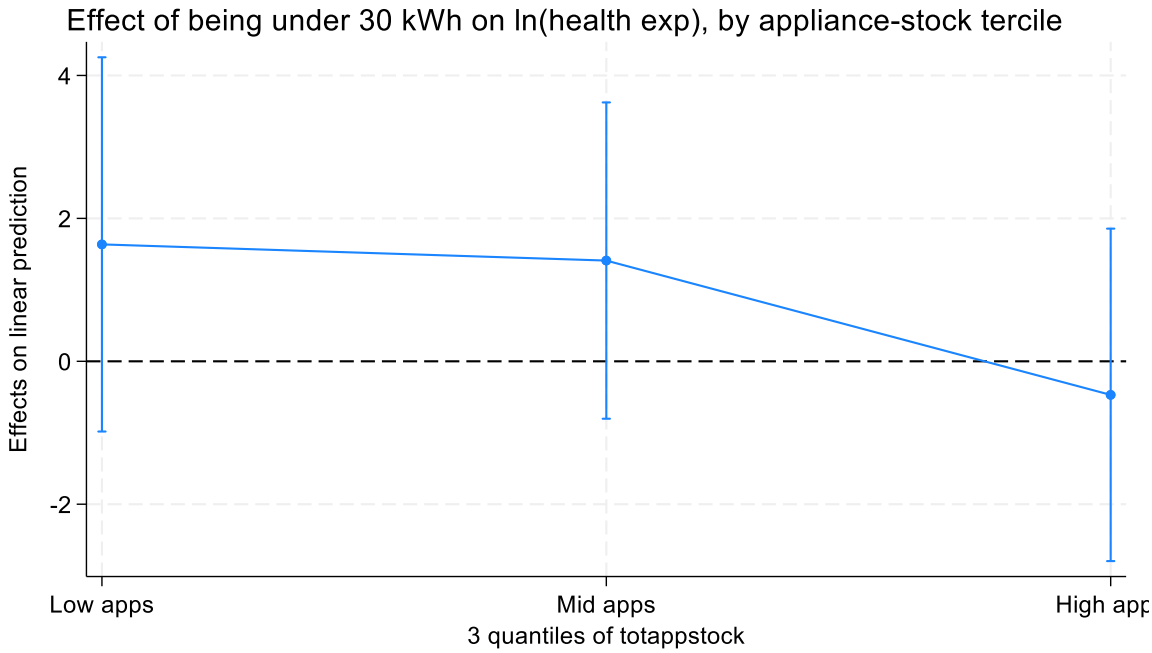


Figure 5D: Treatment effect on household health expense by appliance terciles

For food and water, Figure 5E shows that tightening the threshold significantly increases spending among households with low appliance stock. This indicates that expenditure switching toward food and water—following the reduction in electricity bills—is concentrated in this group, with an estimated increase of 1.6145. A similar pattern appears for mobile data/airtime (Figure 5F): the effect is statistically significant and largest for

households with low appliance stock. Overall, the results suggest that the tightened policy, by substantially reducing electricity bills, induces meaningful expenditure switching toward food and water and mobile data/airtime primarily among households with lower appliance holdings.

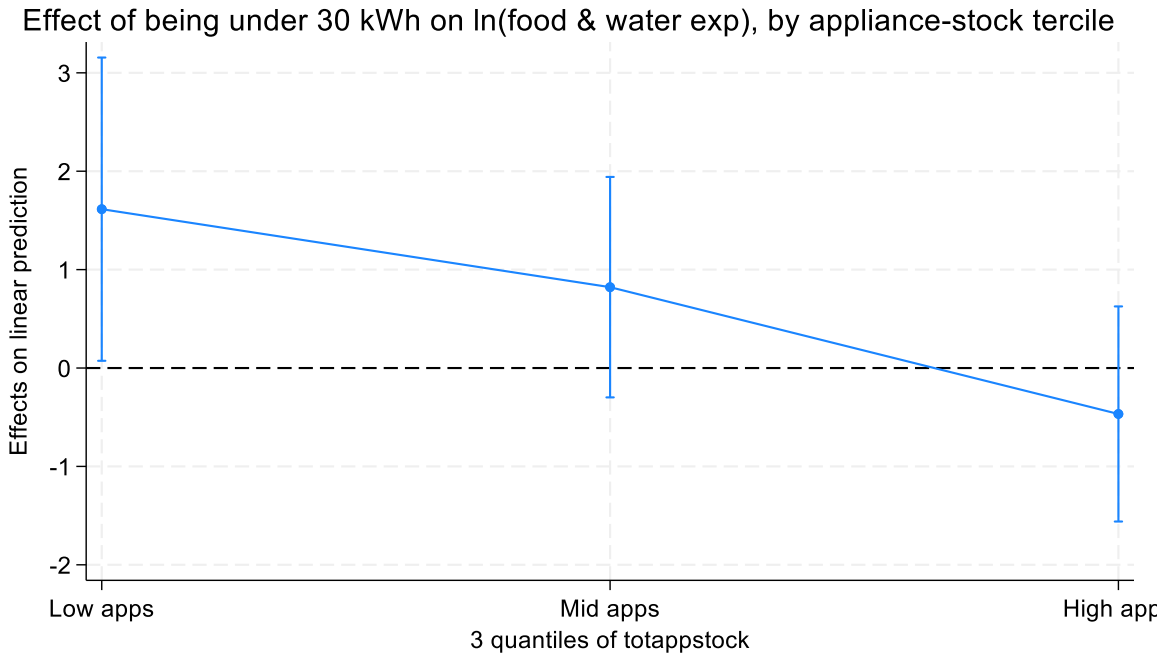


Figure 5E: Treatment effect on households' expense on food and water by appliance terciles

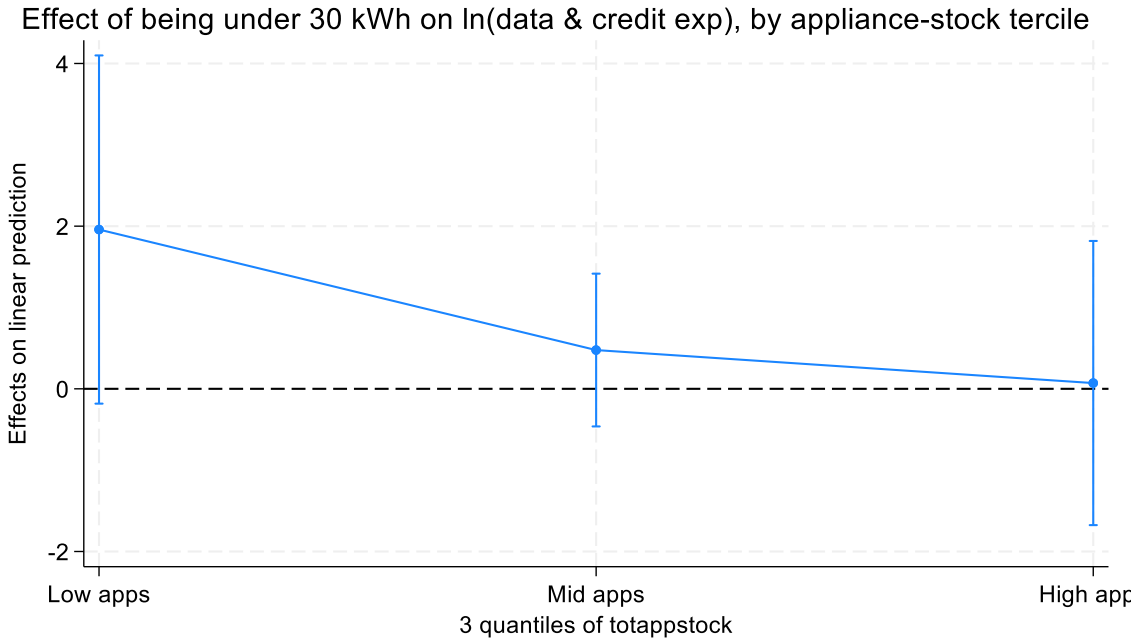


Figure 5F: Treatment effect on household expense on mobile data and credit by appliance terciles

Taken together, tightening the lifeline tariff block significantly reduces electricity bills for lifeline-eligible households relative to non-eligible households. This effect is heterogeneous: it is strongest for households with low appliance stock, moderate for those with medium appliance stock, and weakest for those with high appliance stock.

We also observe positive income effects associated with the tightening and the resulting bill reductions, but these gains are most evident among households with medium appliance stock and are absent for households with high appliance stock. Although the point estimates for low appliance-stock households are positive, they are not statistically significant.

The non-energy spending effects are noisier. We find no meaningful heterogeneity in spending shifts for education and health, but clear heterogeneity for food and water and mobile data/airtime, with gains concentrated among households with low appliance stock. Overall, tightening the lifeline policy appears more likely to induce spending shifts toward basic necessities for households with lower appliance holdings.

5.3.4 Other robustness Checks

Mix Multivariate logit treatment-Effect Regression

To address the small-sample issue, we estimate a multi-valued treatment-effects model with three consumption categories: 0–30 kWh, 31–50 kWh, and >50 kWh. Table 10 reports both the selection and outcome (treatment-effect) equations.

For the selection model, household size has a negative association with selection into the 0–30 kWh category but a positive association with selection into 31–50 kWh, implying that larger households are less likely to fall within the current lifeline block. Similarly, households sharing a dwelling are less likely to be in the lifeline block, conditional on other factors. Appliance stock has a strong, negative, and statistically significant effect for both 0–30 kWh (-0.521) and 31–50 kWh (-0.239): households with more appliances are less likely to be in the lower-consumption categories. These patterns are consistent with earlier results showing that appliance holdings strongly sort households out of lifeline eligibility.

The outcome equation shows negative and statistically significant effects on electricity bills for both the 0–30 kWh and 31–50 kWh groups relative to the >50 kWh reference group. The estimates imply that households in the 0–30 kWh category pay about 33–34% lower bills $[(\exp(-0.4106) - 1) \times 100]$than those consuming >50 kWh, while households in the 31–50 kWh category pay about 21% lower bills. The corresponding marginal effects are -2.51 and -1.55 (in bill units). In short, lower-use households spend less on electricity than higher-use households, with the reduction largest for the current lifeline block. This pattern is consistent

with the idea that households that lose eligibility when the threshold tightens face higher bills relative to those who remain eligible (we return to this mechanism later). The selection terms also suggest that some reductions in bills may reflect reductions in energy services (i.e., energy deprivation) rather than efficiency gains.

Overall, these results reinforce the earlier finding that tightening the lifeline block generates meaningful bill differences around the threshold, while also raising concerns about behavioural responses to remain within the block. The signs on household size, shared dwelling, and appliance stock in the outcome equation are consistent with intuition.

The selection correction terms are also informative. The estimated lambda is positive for both the 0–30 kWh and 31–50 kWh categories and statistically significant for the lifeline group, indicating that unobserved factors that increase the likelihood of being in the lifeline block are also associated with higher electricity bills. This is consistent with bunching behaviour just below the threshold and underscores the importance of correcting for selection among lifeline customers. Finally, lnalpha indicates substantial dispersion, supporting the use of the Gamma-log specification.

Table 10: Mix Multivariate Logit Treatment-Effect Regression

	All three treatment levels_robust S.E.	**All three treatment levels_cluster district S.E**
Ref: _T (> 50 kWh)		
_T (0 – 30 kWh		
Hhsize	-0.1076	-0.1076**
	(0.0714)	(0.0532)
no_hhs_sharing_dwelling	-0.1014*	-0.1014**
	(0.0521)	(0.0391)
total appliance stock	-0.5210***	-0.5210***
	(0.0675)	(0.0673)
constant	1.2680***	1.2680*
	(0.4664)	(0.7337)
_T (31 – 50 kWh		
Hhsize	0.0701	0.0701
	(0.0501)	(0.0492)
no_hhs_sharing_dwelling	-0.0146	-0.0146
	(0.0380)	(0.0282)
total appliance stock	-0.2389***	-0.2389***
	(0.0494)	(0.0616)

constant	1.4552***	-1.4552**
	(0.3990)	(0.6096)
Electricity expenditure		
_T (0 – 30 kWh)	-0.4106***	-0.4106***
	(0.0192)	(0.0188)
dy/dx	-2.5094***	-2.5094***
	(0.06203)	(0.06203)
_T (31 – 50 kWh**)**	-0.2357***	-0.2357***
	(0.0084)	(0.0081)
dy/dx	-1.5455***	-1.5455***
	(0.05545)	(0.05545)
HHsize	0.00012	0.00012
	(0.0012)	(0.0012)
no_hhs_sharing_dwelling	0.0014*	0.0014*
	(0.0008)	(0.0008)
total appliance stock	0.0053***	0.0053***
	(0.0006)	(0.0005)
constant	-2.7893***	-2.7893***
	(0.0900)	(0.0971)
/lnalpha	4.7505***	4.7505***
	(0.0922)	(0.1004)
/ lambda_T (0 – 30 kWh)	0.0199*	0.0199*
	(0.0146)	(0.0114)
/ lambda_T (31 – 50 kWh)	0.0113	0.0113
	(0.0080)	(0.0101)
Alpha	115.6389***	115.6389***
	(10.6648)	(11.6156)
Observation	1064	1064
Log likelihood	-1506.997	-1506.997

Figures in [] are robust standard errors. *, **, and *** denote 10%, 5%, and 1% statistical significance

Estimating Intent-to-treat (ITT)

Finally, we estimate the effects of perceived lifeline eligibility (Yes = 1, No = 0) on the outcome variables. Although this measure may involve misclassification, it provides a useful robustness (pseudo) check for small-sample sensitivity. We implement doubly robust IPWRA on the trimmed overlap sample and apply wild-cluster bootstrap inference by district. Table 11 reports the results. Because the treatment is perceived rather than actual eligibility, we interpret these estimates as an intent-to-treat (ITT)-style effect capturing salience and knowledge of the policy.

Covariate-balance tests indicate good overall balance after weighting across all specifications. Perceived eligibility is associated with a meaningful reduction in electricity bills, statistically significant under both robust and wild-cluster inference. The point estimate implies about a 20% lower electricity bill relative to households that do not perceive themselves as eligible. For income and most non-energy spending outcomes, effects are largely null under wild-cluster bootstrap. One exception is education expenditure, which shows a statistically significant negative effect even under wild-cluster inference; while the bill reduction is plausible, this education result should be interpreted cautiously.

Overall, the ITT estimates provide external triangulation for the main IPWRA results. The direction of effects is consistent, but magnitudes are smaller under ITT, as expected, because perceived eligibility mixes compliers and non-compliers—attenuating estimates toward zero relative to treatment-on-the-treated effects. Therefore, a smaller ITT is compatible with the larger IPWRA effect estimated for actual eligibility (≤30 kWh). The null income and non-energy spending results are also not inconsistent: perceived eligibility may be weakly correlated with actual eligibility (a weak "first stage"), and the effective number of compliers may be small. Taken together, this triangulation strengthens our conclusion that tightening the lifeline threshold substantially reduces electricity bills, while the income and non-energy spending effects remain less clear.

Table 11: Perception-based DR (pseudo-ITT on trimmed support)
Treatment-effects Estimation
Estimator: double-robust IPW Regression Adjustment
Treatment Model: Logit

Effects		Electricity exp	Income	Education	Health	Food & Water	Data & Credit
ATE							
	Z yes	-0.227***	0.0178	-0.4713***	-0.037	-0.138	0.073
	(1 vs 0)	(0.0707)	(0.0954)	(0.1707)	(0.2579)	(0.1341)	(0.1161)
Percent		-20.324***	1.7914	-37.581***	-3.663	-12.886	7.615
		(5.6312)	(9.7152)	(10.6528)	(24.8439)	(11.6830)	(12.4903)
PO (Mean)							
	Z yes	7.4149***	10.272***	5.9888***	6.478***	9.355***	6.560***
	0	(0.1065)	(0.0734)	(0.1567)	(0.1548)	(0.0874)	(0.1082)
Cov balance test							
Chi2(10)		9.9298	10.6443	10.2653	10.2653	10.2653	102653
Prob>chi2		0.4467	0.3859	0.4175	0.4175	0.4175	0.4175
Wild boot test Z_yes		-3.1816	0.1738	-2.3292	-0.1424	-0.9852	0.6673
Prob>|t|		0.0079	0.8753	0.0416	0.888	0.344	0.5536
Obs		1,066	1,035	1086	1086	1086	1086

Perceived Impacts of Lifeline Tariff

Survey respondents were asked to rate the perceived impacts of the lifeline tariff policy using Likert-scale statements covering energy-related indicators and selected welfare outcomes. The scale shows high internal consistency (Cronbach's alpha = 0.936), exceeding the 0.70 benchmark and indicating strong reliability. Figure 6 presents the distribution of responses across the nine impact statements, while Table 12 reports the corresponding descriptive statistics.

Both sources indicate strong perceived impacts, especially on energy-related outcomes—such as improved access to electricity, lower bills, greater energy consciousness, and affordability. For example, 72.1% of respondents strongly agreed that the lifeline tariff reduces monthly electricity expenses, and 76.8% strongly agreed that it makes electricity more affordable. In addition, 78.3% and 76.8% agreed that the policy promotes energy consciousness and the adoption of energy-efficient appliances, respectively — consistent with incentives to keep consumption below the eligibility threshold.

These perceptions align with our doubly robust IPWRA results showing substantial reductions in electricity bills following the tightened lifeline block. The Likert evidence therefore provides face validity for the measured bill effects. The strong agreement that the policy encourages conservation and efficiency also aligns with our evidence of behavioral adjustment consistent with bunching just below the threshold.

Perceived welfare spillovers beyond energy are more mixed and weaker, though still largely positive: 68.4% agreed the policy improves spending on food and water, 64.8% on education, 64.4% on healthcare, and 69.9% on other essentials. The higher shares of disagreement on these items suggest heterogeneity in perceived spillovers. This pattern is consistent with our quantitative findings: clear and robust bill reductions, but weaker and less precisely estimated effects on non-energy expenditures. Such spillovers may be gradual, heterogeneous, and difficult to identify causally. Overall, the Likert-scale evidence does not contradict the IPWRA results; rather, it helps contextualize them—strong confidence on affordability and bill reductions, but more tentative evidence on broader welfare gains.

Table 12

: Impact of Lifeline Tariff – 9 Likert Items: Descriptives with 95% CI

Item	Question	N	Mean (1–5)	95% CI Low	95% CI High	SD	Disagree (%)(1+2)	Neutral (%)(3)	Agree (%)(4+5)
G01	G01. The lifeline tariff improves electricity access	1084	4.023	3.96	4.086	1.055	9.3	17.2	73.5
G02	G02. Lifeline tariff reduces monthly electricity expenses	1082	3.945	3.876	4.014	1.158	13.8	14.1	72.1
GO3	GO3. The lifeline tariff improves ability to pay for other essential expenses	1087	3.853	3.784	3.921	1.149	15	15.1	69.9
G04	G04. The lifeline tariff improves ability to afford food and water	1088	3.834	3.764	3.903	1.173	16.2	15.3	68.4
G05	G05. The lifeline tariff contributes to better access to healthcare	1074	3.73	3.658	3.802	1.206	17.8	17.8	64.4
G06	G06.The lifeline tariffs contribute to better access to education	1073	3.768	3.696	3.839	1.196	17.9	17.2	64.8
G07	G07. The lifeline tariff influences adoption of energy efficient appliances	1070	4.065	4.006	4.124	0.984	7.8	15.3	76.8
G08	G08. The lifeline tariff improves energy conscious behaviour	1069	4.114	4.056	4.173	0.977	7.1	14.6	78.3
G09	G09. The lifeline tariff makes electricity more affordable	1053	4.077	4.012	4.142	1.07	9.1	14.1	76.9

Note: %(1+2) is share of disagree; 3% is share of neutral; %(4+5) share of agree

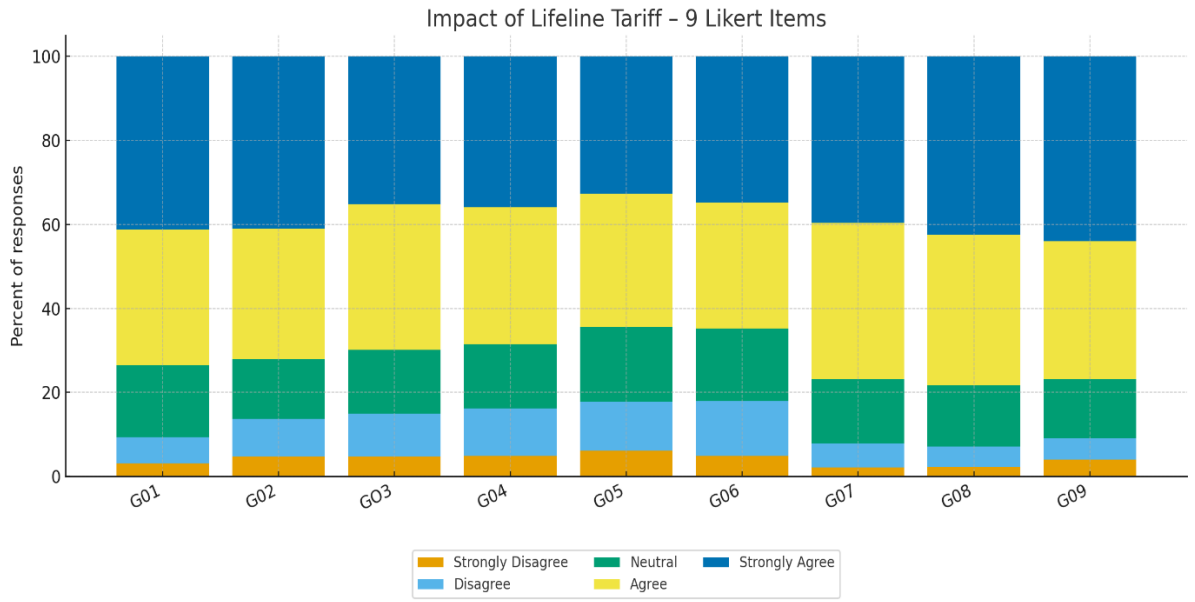


Figure 6: Response distribution of 5 Likert scale questions on LLT impact

Next, we examine how treated and control households differ in the likelihood of agreeing (Likert 4 or 5) with statements about the lifeline tariff's perceived energy and non-energy impacts. We first construct two indices from the energy-related items: an affordability index (G01, G02, and G09) and an energy-consciousness index (G07 and G08). We then estimate doubly robust IPWRA on these indices using the trimmed common-support sample. Table 13 reports the results.

The estimated average treatment effect on the affordability index is -0.622, statistically significant at the 10% level. The negative ATE implies that treated households score lower on the composite affordability index than the control group. In other words, although treated households are more likely to report that the lifeline tariff reduces their electricity bills, they report weaker overall affordability. This pattern is consistent with our argument that energy deprivation may be prevalent among households just below the threshold: to remain eligible under the tighter block, households may bunch consumption and limit appliance use to keep bills low, potentially reducing access to energy services and comfort.

In contrast, the average treatment effect on the energy-consciousness index is positive and statistically significant, indicating that tightening the threshold induces current beneficiaries to adjust consumption behavior to remain within the lifeline block. While this may reflect improved conservation, it may also come with welfare costs in the form of energy deprivation, particularly for households near the cutoff.

Table 13: Test treatment differences on idx_aff/idx_cons

Estimator: double-robust IPW Regression Adjustment

Treatment Model: Logit

Effects		Idx_affordability	Idx_ Energy consciousness
ATE			
	T30	-0.622*	0.419*
	(≤30 vs >30)	(0.3181)	(0.2173)
PO (Mean)			
	T30	-0.0307	-0.0005
	(≤30 vs >30)	(0.0512)	(0.0342)
Cov balance test			
Chi2(10)		3.199	NA
Prob>chi2		0.9763	
Obs		1,041	1,023

Second, we estimate—using an ordered logit model—the probability that treated and control households agree/strongly agree (Likert 4 or 5) with statements about the lifeline tariff's perceived impacts on energy-related and non-energy outcomes. The goal is to determine which group is more likely to endorse each statement, conditional on observables and within the common-support sample.

Table 14 reports results for the energy-related statements. Relative to households consuming >30 kWh, households within the current lifeline band (≤30 kWh) have a higher probability of agreeing that the lifeline tariff "reduces monthly electricity bills." The difference test indicates about a 5.2 percentage-point higher likelihood of agreement among ≤30 kWh households, suggesting that treated households perceive meaningful bill relief.

For the other energy-related statements (improved access, affordability, energy consciousness, and adoption of energy-efficient appliances), ≤30 kWh households show slightly higher probabilities of agreement than >30 kWh households, but the differences are small and statistically insignificant. Taken together, these patterns are consistent with higher energy consciousness among treated households and with behavior consistent with consumption bunching just below the threshold. They also align with the interpretation that households keep bills down primarily through tighter consumption management rather than technology switching.

For non-energy outcomes, we find no statistically distinguishable treatment effects after conditioning on observables and restricting to common support (Table 15).

Overall, these findings align with the doubly robust IPWRA results showing clear bill reductions but limited evidence of broader welfare gains. Thus, *if indeed the lifeline tariff policy does free budget, it fails to show up as strong perceived gains, a sign to suggest that households may be trading off energy services rather than reallocating budget to other spending items.* The negative ATE on affordability aligns with this result: staying within the lifeline tariff block reduces electricity expense but this does not automatically translate into higher perceived overall affordability, which supports the foregone energy services argument.

Table 14: Estimator: Ordered logit on the 1–5 scale with *P*(4)+*P*(5) on common support sample for energy-related effects

Dy/dx	LLT improves access	LLT reduces electricity bill	LLT makes electricity affordable	LLT induces energy consciousness	LLT encourages energy efficient adoption
T30= 0	0.821***	0.852***	0.856***	0.844***	0.850***
	(0.0187)	(0.0218)	(0.0172)	(0.0157)	(0.01897)
T30= 1	0.883***	0.905***	0.872***	0.861***	0.853***
	(0.0313)	(0.0283)	(0.0301)	(0.0399)	(0.0248)
Difference test	0.062	0.0523***	0.0164	0.01695	0.0032
	(0.0386)	(0.0245)	(0.0267)	(0.0370)	(0.0177)
Cov control	YES	YES	YES	YES	YES
Psuedo R^2	0.0148	0.0148	0.0088	0.0062	0.0883
Obs	1,048	1,044	1,015	1,032	1,031

Standard errors computed using the Delta method. The controls: meter type, metre ownership type, household size, flexible appliance stock (level and square), dwelling type, gender, education of household head, and number of households sharing dwelling, and district fixed effects.

Table 15: Ordered logit on the 1–5 scale with *P*(4)+*P*(5) on common support sample for outcomes

Dy/dx	LLT improves education access	LLT improves health access	LLT ability to pay food and water	LLT ability to pay other essentials
T30= 0	0.827***	0.822***	0.848***	0.847***
	(0.0212)	(0.0215)	(0.0247)	(0.0269)
T30= 1	0.823***	0.817***	0.823***	0.883***
	(0.0371)	(0.0350)	(0.0423)	(0.0382)
Difference test	-0.0039	-0.0048	-0.0245	0.0362
	(0.0369)	(0.0302)	(0.0328)	(0.0296)
Cov control	YES	YES	YES	YES

Psuedo R^2	0.0118	0.010	0.0137	0.0109
Obs	1,023	1,025	1,038	1,037

Standard errors computed using the Delta method. The controls: meter type, metre ownership type, household size, flexible appliance stock (level and square), dwelling type, gender, education of household head, and number of households sharing dwelling, and district fixed effects.

5.4 Objective III: Impact of Change in lifeline tariff threshold from 0 – 50 kWh to 0 – 30 kWh

In this section, we examine the impact of tightening the lifeline tariff threshold from 0–50 kWh to 0–30 kWh. Our earlier estimates of the average treatment effect for households consuming ≤30 kWh relative to near neighbors (31–50 kWh) already speak to this question. Here, however, we take a complementary approach by estimating the cost of losing eligibility.

Under the previous policy, households consuming 0–50 kWh qualified for the lifeline tariff. After the reform, households with consumption above 30 kWh but ≤50 kWh lost eligibility. To estimate the effect of this loss, we implement a regression discontinuity design (RDD) at the 30 kWh cutoff. Specifically, we compare households just below the threshold (still eligible, ≤30 kWh) with those just above it (no longer eligible, 31–50 kWh) in a narrow bandwidth around 30 kWh, while flexibly controlling for the running variable (electricity consumption in kWh).

5.4.1 Manipulation Test

Figure 7 and Table 16 below show the manipulation test for the running variable: electricity consumption. The Global McCrary test (i.e., local-polynomial density) indicates failure to reject the null hypothesis of continuity in the running variable at the cut-off (i.e., 30 KWh). Thus, on global specification, there is no manipulation in the running variable at the cutoff. However, the local binomial checks do flag asymmetry just around the cut-off, particularly for small half-windows: 3.96, 6.34. 11.10, and 13.48. Asymmetry disappears for wider windows (>= 18-26) as p-values become large. This pattern depicts a clear case of excess mass or bunching just below the cut-off point in a tight neighbourhood.

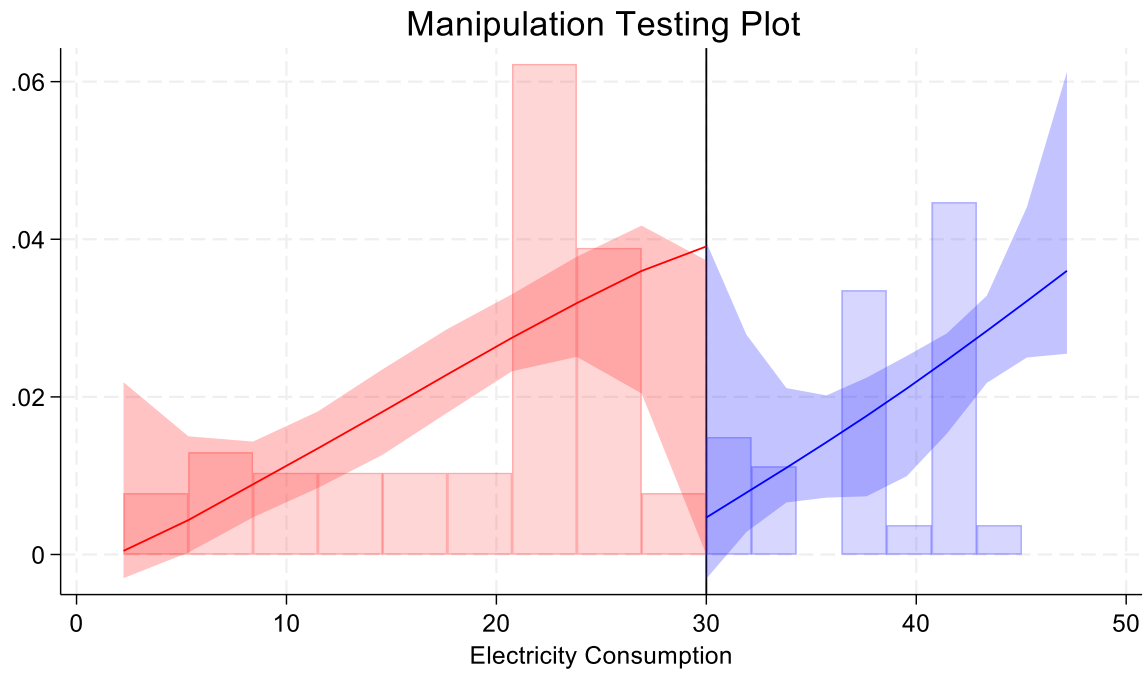


Figure 7: Regression discontinuity plot for manipulation test

Table 16: McCrary RD Manipulation Test
Model: unrestricted
Observation: 125
BW Method: Comb
Kernel: Triangular
VCE method: Jackknife

C=30	**Left of C**	**Right of C**
No. of obs	66	59
Eff. No. of obs	64	59
Order est. (p)	2	2
Order bias (q)	3	3
BW est. (h)	25.370	25.370
Running variable: electricity consumption		
Method	T	P>|T|
Robust	-0.0233	0.9814

	P-values of binomial tests (Ho: prob =0.5		
Window length/2	**<C**	**>=C**	**P>|T|**
3.960	17	5	0.0169
6.339	18	7	0.0433
8.718	23	16	0.33368
11.097	43	18	0.0019

13.476	47	29	0.0505
15.854	51	30	0.0257
18.233	54	59	0.7069
20.612	58	59	1.0000
22.991	61	59	0.9273
25.370	63	59	0.7861

This suggests that after the lifeline block was tightened from 0–50 kWh to 0–30 kWh, some households deliberately reduced electricity use to remain eligible. Consistent with this interpretation, the survey indicates that 36.4% of current lifeline customers reported taking steps to reduce consumption following the tightening. Common strategies include intermittent or infrequent appliance use, scheduling appliance use, and complete avoidance of certain appliances. The appliances most frequently avoided were electric irons and, to a lesser extent, televisions, while fans and light bulbs were more often managed intermittently or scheduled.

While these behaviors reflect energy conservation, they also raise concerns about energy deprivation and energy poverty. In particular, complete avoidance and intermittent use can reduce access to important energy services (e.g., ironing and household comfort/entertainment), implying welfare costs alongside bill savings.

Figure 8 illustrates this mechanism using a budget-constraint diagram with a lifeline "kink" at 30 kWh versus 50 kWh. It shows that tightening shifts the optimal choice toward the new kink (bunching at 30 kWh), thereby reducing attainable utility. The x-axis measures energy services (kWh/month) and the y-axis represents consumption of a composite good. Under the old tariff (kink at 50 kWh), the household's optimum is around 40 kWh of energy services and 60 units of the composite good (gold indifference curve). After tightening (kink at 30 kWh), the new optimum shifts to roughly 30 kWh and 70 units of the composite good, moving the household to a lower indifference curve (unobserved), indicating a utility loss. This stylised result is consistent with the empirical evidence of bunching at 30 kWh following the reform.

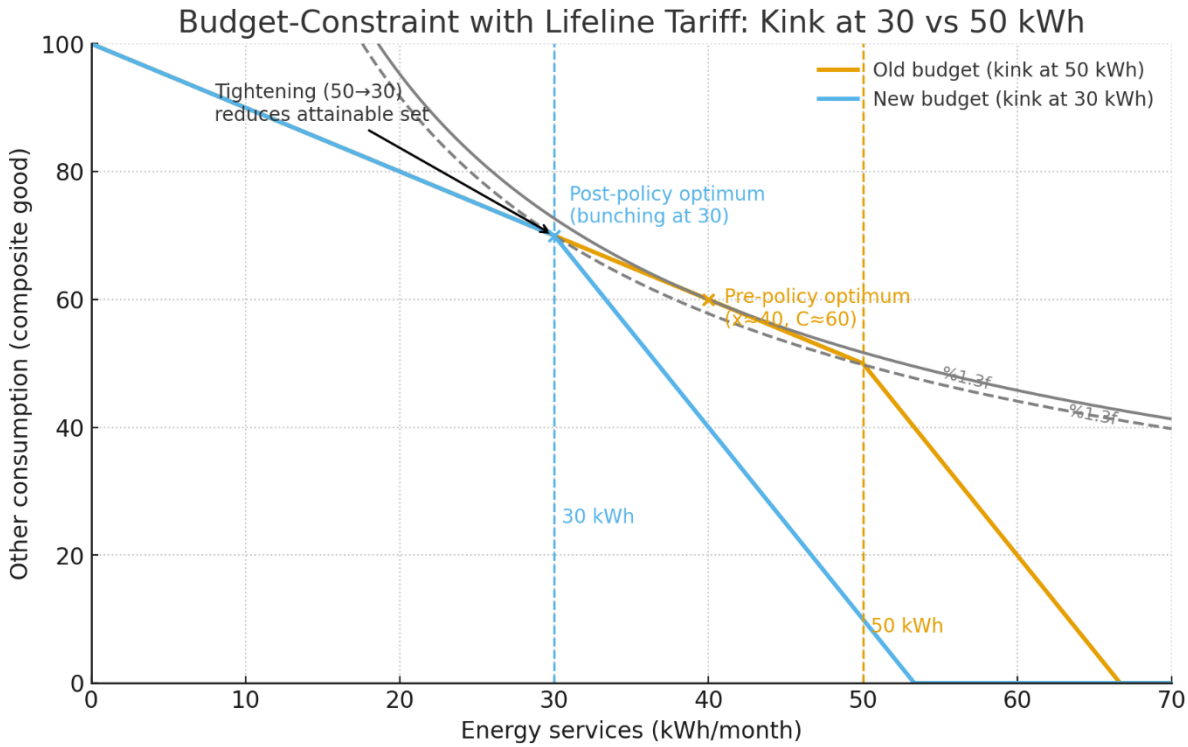


Figure 8: Budget-Constraint with Lifeline Tariff Kink at 30 kWh and 50 kWh

Source: Authors' Construction

5.4.2 Estimating The Regression Discontinuity Effects for Electricity bills

Figure 9 presents the regression discontinuity plot using automatic bandwidth selection. To the left of the cutoff, the fitted relationship is concave: within the lifeline block, electricity bills increase with kWh but flatten as consumption approaches the threshold. To the right of the cutoff, bills rise more nearly linearly. This pattern is consistent with the tariff structure: the marginal price increases once consumption exceeds the threshold, generating a discrete jump in bills at 30 kWh and a steeper schedule thereafter.

The positive discontinuity at the cutoff is interpreted causally as evidence that households just above the threshold pay higher electricity bills than comparable households just below it, over and above the smooth relationship between bills and kWh. We replicate the plot using multiple bandwidths and obtain consistent results: households that cross the new lifeline threshold face higher—and more steeply increasing—electricity bills (see Figure 10). In other words, tightening the block means that households whose consumption barely exceeds 30 kWh move onto a costlier tariff schedule, raising their bills even for small differences in consumption.

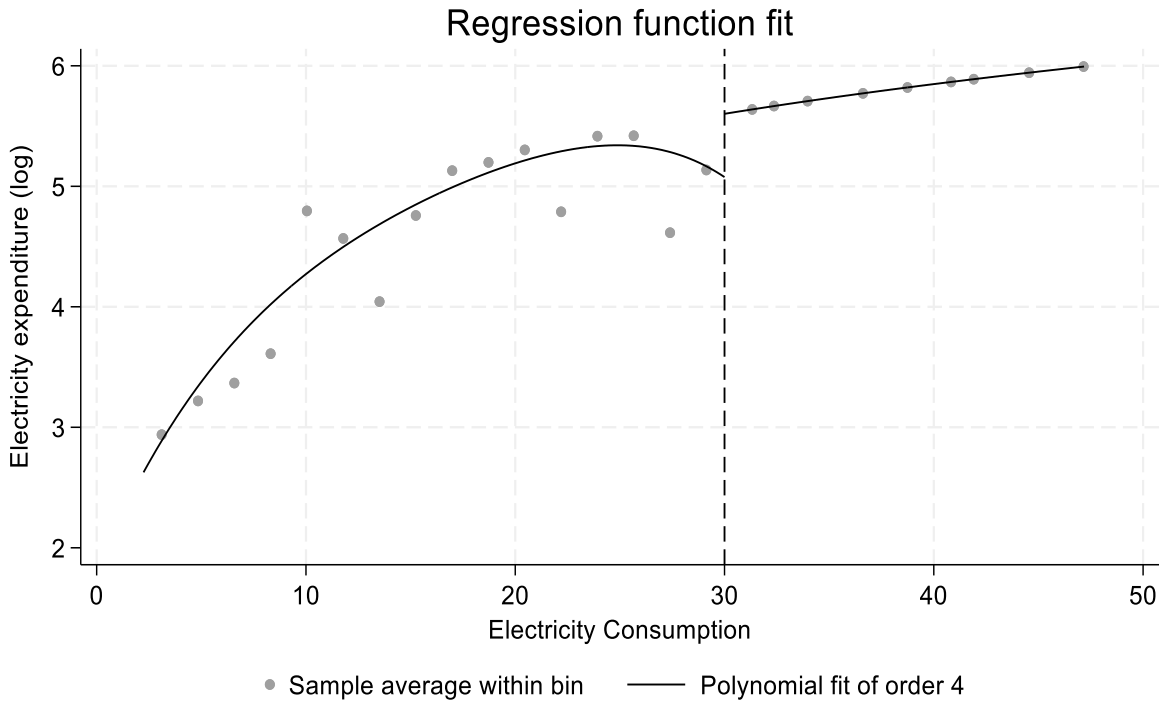


Figure 9: RD Plot of Electricity Expense using automatic bandwidth selection

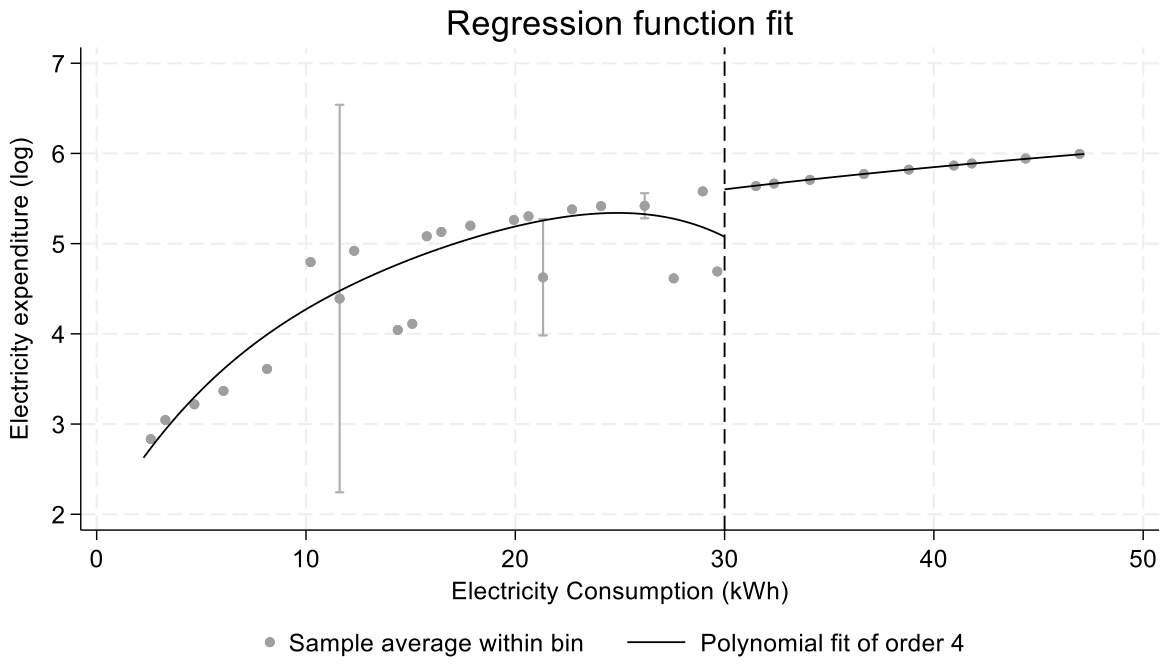


Figure 10: RD Plot of Electricity Expense with many bins

The figures provide descriptive evidence of a discontinuity at the threshold. We formally estimate this jump using a regression discontinuity (RD) design. Because global

higher-order polynomials can be sensitive, we report a robust bias-corrected local quadratic RD with MSE-optimal bandwidth selection, which targets the local effect at 30 kWh. Table 17 presents the estimates. Column (1), which uses data-driven bandwidth selection, shows a statistically significant positive discontinuity in electricity expenditure at the cutoff: households whose consumption exceeds 30 kWh (and thus lose eligibility) spend 0.641 more on electricity.

Earlier results (McCrary and local binomial checks) indicated bunching just below 30 kWh. If households that "shave" consumption differ systematically from others, a sharp RD could be biased. As a robustness check, we estimate a donut RD, dropping observations within ±2 kWh of the threshold to reduce sensitivity to local manipulation. We also test fixed symmetric bandwidths, add covariate adjustments, and use alternative windows and placebo cutoffs near the policy threshold. The estimated discontinuity remains robust across these specifications (columns 2–5 for fixed bandwidths; columns 5 and 8 for covariate adjustment; and donut exclusions around the cutoff).

Overall, the positive jump in electricity bills at the threshold is consistent with the ordered-logit and doubly robust IPWRA results. The RD estimates capture the price/bill discontinuity faced by households that cross the cutoff, while the ordered-logit results show that treated households perceive bill relief. At the same time, the IPWRA findings caution that this relief may partly reflect constrained consumption (i.e., bunching/energy deprivation), with limited evidence of broader welfare gains.

Table 17: RD Effects of lifeline policy tightening on electricity expenditure

Outcome: log of electricity expenditure/bill

	(1) BD (Auto[259])	**(2) BD (10)**	**(4) BD (12)**	**(5) BD (15)**	**(6) BD(15)**	**(7) Donut RD**	**(8) Donut RD**
RD Effect	0.64057**	0.51362***	0.64664**	0.53315**	0.54794**	0.41724**	0.41324**
	[2.0042]	[18.6064]	[2.3675]	[2.0773]	[2.3412]	[2.1252]	[2.1187]
Cov. Control	NO	NO	NO	NO	YES	NO	YES
No. obs							
Left 30	66	66	66	66	66	64	64
Right 30	59	59	59	59	59	55	55
Eff. Obs							
Left 30	46	17	46	50	50	48	48
Right 30	29	17	29	30	30	26	26

Order est. (p)							
Left 30	2	2	2	2	2	2	2
Right 30	2	2	2	2	2	2	2
Order est. (q)							
Left 30	3	3	3	3	3	3	3
Right 30	3	3	3	3	3	3	3
Obs	124	124	124	124	124	124	124

Figures in [] are robust z-statistics. *, **, and *** denote 10%, 5%, and 1% statistical significance

As a further robustness check—given evidence of consumption bunching—we estimate RD specifications using alternative treatment and control windows around the 30 kWh cutoff, while flexibly controlling for the running variable (monthly kWh). The aim is to assess whether similar "jumps" appear at arbitrary thresholds that could indicate spurious discontinuities. Table 18 reports the results.

Across a range of asymmetric windows, households just below the threshold consistently face significantly lower electricity bills than comparable households just above it. The estimated discontinuity ranges from -0.25 to -0.74 log points (approximately 22%–52%). The largest effects occur in the tightest windows closest to the cutoff (e.g., 25–30 vs. 31–45/50), indicating that the tightening generates the largest bill differences for households nearest the threshold. This pattern is consistent with an increasing-block tariff with a discrete jump at 30 kWh: a downward discontinuity at the cutoff and a steeper slope above it. Overall, the stability of estimates across plausible window choices supports a local causal effect of the lifeline threshold on electricity bills.

Table 18: Flexible control for running variable (local polynomial) Outcome: electricity expenditure (log)

Treated	15-30kWh	20-30kWh	20-30kWh	20-30kWh	25-30kWh	25-30kWh
Control	31-50 kWh	31-50kWh	31-40kWh	31-45kWh	31-45kWh	31-50kWh
Treatment	-0.2500**	-0.433**	-0.531**	-0.436**	-0.737***	-0.539***
	(0.1025)	(0.1803)	(0.2221)	(0.1403)	(0.0747)	(0.1555)
Constant	4.930***	5.955***	5.931***	6.667***	11.244***	7.229***
	(0.2231)	(0.9484)	(1.1043)	(0.9446)	(1.2492)	(0.7003)
Controls	YES	YES	YES	YES	YES	YES
District FE	YES	YES	YES	YES	YES	YES
Observations	109	101	59	72	47	76
R-squared	0.764	0.776	0.636	0.703	0.782	0.793

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Next, we calibrate the monthly bill schedule for households in the 0–30 kWh and 31–50 kWh ranges, allowing for a discrete jump at the new lifeline cutoff. The calibration uses the average monthly bills at 30 kWh, 31 kWh, and 50 kWh, obtained from the Public Utilities Regulatory Commission (PURC) Tariff Reckoner: B(30)=GHS 27.47, B(31)=GHS 70.05, and B(50)=GHS 106.41. Using these points, we estimate the size of the bill jump at the cutoff and the implied marginal rate above the threshold.

Figure 11 plots the calibrated schedule and overlays it on the local RD estimates. The bill increases sharply when consumption crosses the threshold: moving from 30 to 31 kWh raises the monthly bill by about GHS 40.7 (≈ US$3.31). Above the cutoff, the implied marginal rate is GHS 1.914 per kWh (≈ US$0.16 per kWh), so bills then rise approximately linearly with additional kWh.

This calibration reinforces our earlier conclusion that the incentive to bunch just below the threshold is strongest for households near the cutoff. While this can induce conservation, households whose unconstrained demand is slightly above 30 kWh may resort to welfare-reducing strategies—such as intermittent electricity use or avoiding certain appliances—to remain eligible. The risk of energy deprivation is therefore greatest among households closest to the threshold, where the average price jump is largest. As consumption rises further above 31 kWh, the effective burden of the discrete jump becomes less pronounced. Thus, the policy burden associated with the tightening of the lifeline policy is highest among households whose consumption is close to the cut-off.

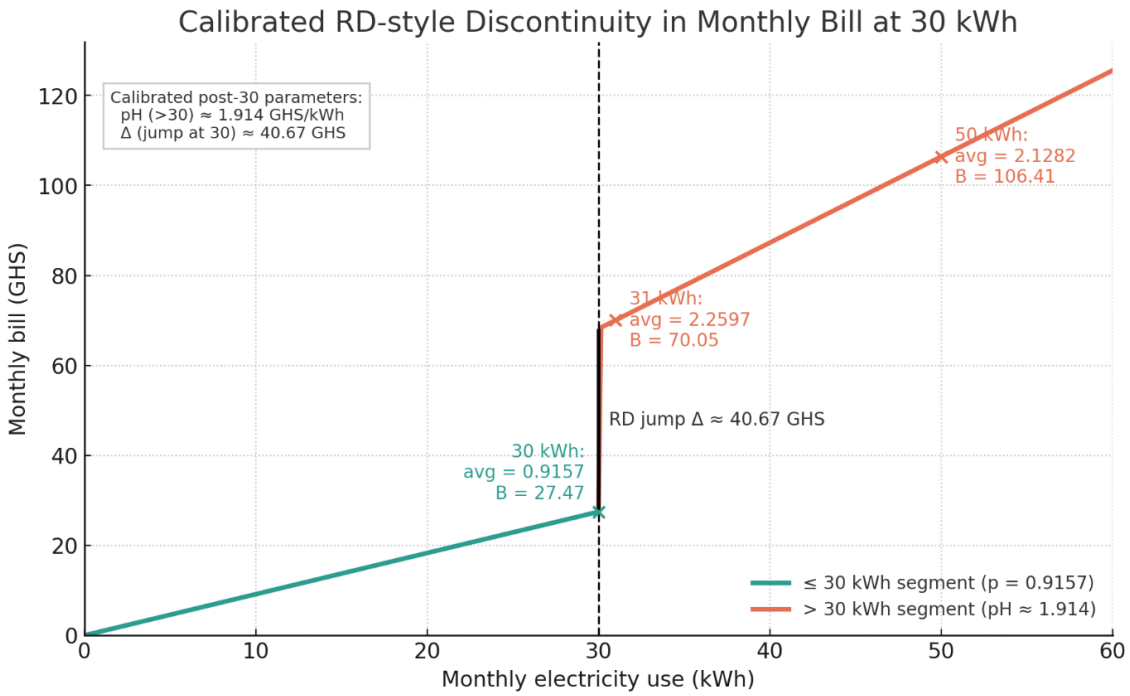


Figure 11: Calibrated RD-style discontinuity in bill schedule at 30 kWh

5.5 Income effects of tightening lifeline policy

Table 19 reports donut RD estimates for income and non-energy (basic-needs) expenditures, where we exclude observations close to the cutoff to reduce sensitivity to potential local manipulation. The estimated RD effect on income is negative and statistically significant, indicating that after the lifeline block was tightened, households whose consumption exceeds the threshold have lower income than comparable households just below the cutoff (who remain eligible). This pattern is consistent with our IPWRA results, which suggested potential income gains for households that remain within the lifeline block.

We also find evidence that policy tightening is associated with lower spending on basic needs—notably food and water and mobile data/airtime—for households just above the threshold relative to those just below it. These results broadly support the possibility of income and non-energy spending effects linked to the tightened lifeline block. However, we treat them as suggestive rather than conclusive. In contrast, the electricity-bill reduction effect is robust and clearly attributable to the policy threshold.

We interpret the weaker and less stable income and non-energy spending effects as reflecting behavioral responses—particularly consumption bunching—which may involve energy deprivation among households whose unconstrained needs lie just above the threshold. Such constraints can limit broader welfare gains even when bills fall.

These findings are consistent with the qualitative evidence. The reduction of the lifeline threshold from 50 to 30 kWh has had substantial welfare implications, particularly for near-poor households and residents of multi-tenant compound houses. Households that previously consumed 31–50 kWh now face higher bills. As one respondent put it, "once you go outside the lifeline, your bill doubles," highlighting the steep cost increase faced by vulnerable consumers. This prompts many to ration electricity use to remain below the new cutoff. Such rationing can result in under-consumption with adverse consequences for health, safety, and comfort—for example, reduced lighting and limited ironing.

Table 19: Welfare Effects of Policy tightening

	Income		**Food & Water Exp.**		**Mobile Data & Credit**	
	Donut RD (1)	Donut RD (2)	Donut RD (1)	Donut RD (2)	Donut RD (1)	Donut RD (2)
RD Effect	-3.2106**	-2.8159**	-2.6562**	-2.482**	-3.344	-1.702
	[-2.4820]	[-2.2051]	[-2.1282]	[-2.0472]	[-0.5436]	[-0.2676]
Covariate	NO	YES	NO	YES	NO	YES

No. Obs						
Left 30	57	57	64	64	64	64
Right 30	50	50	55	55	55	55
Eff. Obs						
Left 30	44	44	48	48	48	48
Right 30	26	26	26	26	26	26
Order est. (p)						
Left 30	2	2	2	2	2	2
Right 30	2	2	2	2	2	2
Order est. (q)						
Left 30	3	3	3	3	3	3
Right 30	3	3	3	3	3	3
Observation	107	107	119	119	119	119

Figures in [] are robust z-statistics. *, **, and *** denote 10%, 5%, and 1% statistical significance

5.6 Placebo test

To support our RD identification assumption, we performed a placebo covariate test on pre-treatment characteristics (i.e., household size and total appliance stock). Table 20 shows the results. The RD effects for both covariates are positive but statistically insignificant at the 30-kWh cut-off point, which is what is required for our RD identification. In the regression discontinuity design, the placebo covariates are pre-treatment characteristics that are expected to evolve smoothly in the running variable: electricity consumption. The statistically insignificant positive discontinuity effect suggests the absence of detectable jump at the cut-off. Thus, there is no evidence that households just below or above the 30-kWh threshold differ systematically in appliance stock or household size beyond the smooth trends in electricity consumption. The positive signs for the RD effects indicate that the local slope extrapolations would imply a slightly higher value above the threshold. However, the confidence intervals include zero, therefore we treat these as noise and not effects.

In all, the placebo covariate test provides strong support for the RD identification assumption: continuity of potential outcomes or covariates at the cut-off. Combining this with our McCrary checks (which showed evidence of local bunching only), we argue that our main result regarding the positive discrete jump in electricity expenditure following the tightening of the policy is unlikely to be driven by compositional differences in appliance holdings or household size at the policy cut-off point.

Table 20: Placebo covariate test

		Household size	Total appliance stock
RD Effect		0.30081	2.5144
		[1.0935]	[0.9703]
No. Obs			
	Left 30	66	66
	Right 30	59	59
Eff. Obs			
	Left 30	50	50
	Right 30	30	30
Order est. (p)			
	Left 30		
	Right 30	2	2
		2	2
Order est. (q)			
	Left 30		
	Right 30	3	3
		3	3
Observation		125	125

Figures in [] are robust z-statistics. *, **, and *** denote 10%, 5%, and 1% statistical significance

5.7 Perceived Effects of losing eligibility

To qualitatively triangulate the RD findings, respondents were asked to rate the perceived impact of reducing the lifeline threshold from 0–50 kWh to 0–30 kWh across nine indicators covering both energy and non-energy outcomes. The scale is highly reliable (Cronbach's alpha = 0.919), exceeding the 0.70 benchmark. Figure 13 shows the response distribution, and Table 21 reports the descriptive statistics.

Overall, respondents perceive that the tighter threshold increases energy-saving behavior. About 56% and 53% agree that the change makes households more energy conscious and encourages adoption of energy-efficient appliances, respectively. This supports the interpretation that post-tightening, behavioral adjustment becomes common—consistent with the RD evidence of bunching just below 30 kWh. In practice, this adjustment appears driven primarily by efforts to avoid the bill jump rather than by improvements in welfare.

On affordability and electricity expenses, perceptions point to increased strain. A plurality report higher electricity expenses after tightening (about 41% agree, 36% are neutral, and 23% disagree), and a majority perceive lower affordability (52% agree, 33% neutral, 15%

disagree; mean = 3.38, above the neutral midpoint of 3). These responses provide qualitative validation of the RD result: crossing the tighter block is associated with a discrete increase in bills.

For access and potential crowding-out of other expenditures, responses cluster just above neutral (means ≈ 3.02–3.08), with comparable shares agreeing (31–37%) and disagreeing (27–30%). This suggests some pressure but less decisive evidence of broader welfare spillovers. Rather, it implies that households cope largely by reducing energy services, consistent with our earlier findings of limited improvements in perceived affordability even among ≤30 kWh households despite lower bills.

Taken together, respondents perceive bill shocks that induce behavioral adjustment to remain within the lifeline block. While tightening may promote conservation, it is also associated with affordability stress and no clear evidence of broad welfare gains—reinforcing our conclusion that the current design risks increasing energy deprivation unless complemented by more targeted support.

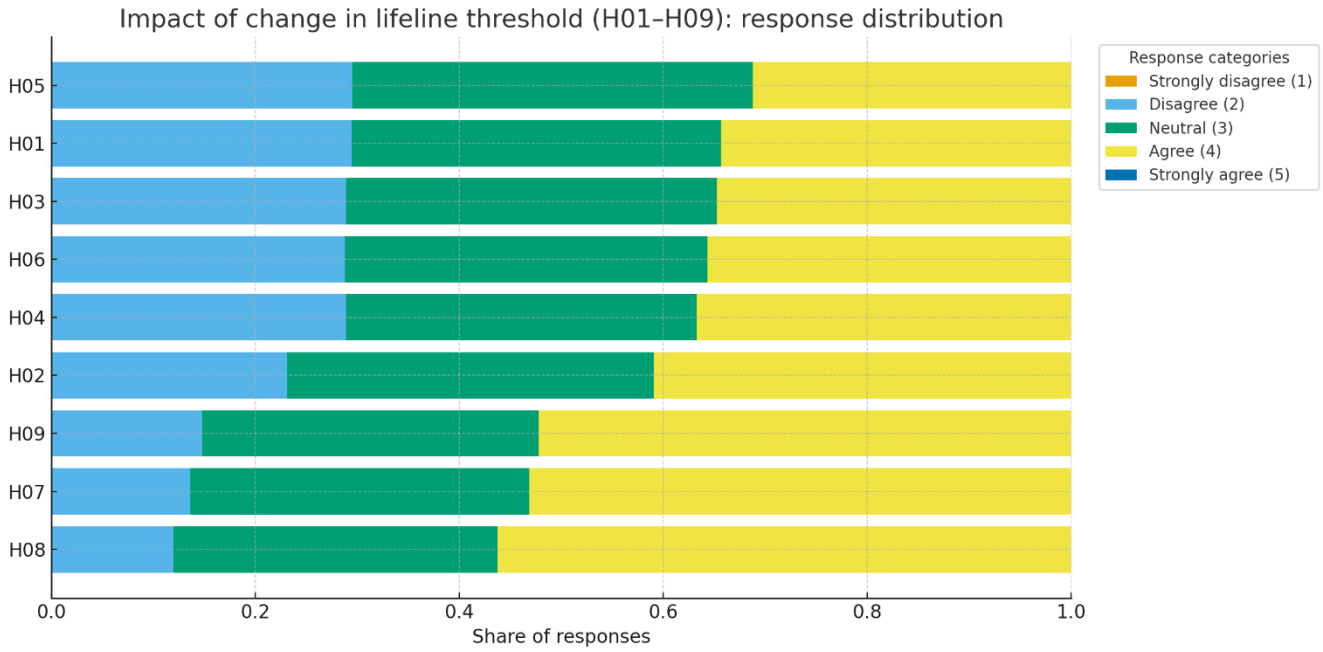


Figure 13: Figure 3: Response distribution of 5 Likert scale questions on LLT impact

Table 21: Descriptive Statistics for Impact of change in LLT threshold (0-50KWh to 0 -30KWh)

Impact Statements	N	Mean	SD	%(1+2)	3%	%(4+5)
H01. The new lifeline tariff has reduced my electricity access	872	3.048	0.798	0.295	0.362	0.343
H02. The new lifeline tariff has increased my monthly utility expenses	942	3.178	0.781	0.231	0.359	0.410
H03.The new lifeline Tariff has reduced my ability to pay for other essential expenses	938	3.059	0.796	0.289	0.364	0.348
H04. The new lifeline tariff has reduced ability to afford basic needs like food and water	932	3.078	0.806	0.289	0.344	0.367
H05. The New lifeline tariff has reduced my ability to access to healthcare	933	3.017	0.779	0.295	0.393	0.312
H06. The new lifeline tariff has reduced my ability to pay for educational expenses	911	3.069	0.800	0.288	0.356	0.357
H07. The new lifeline tariff has influenced my adoption of energy efficient appliances	977	3.396	0.716	0.136	0.332	0.532
H08. The new lifeline tariff has made more energy conscious	977	3.44	0.700	0.120	0.317	0.563
H09. The lifeline tariff has made electricity less affordable	875	3.375	0.728	0.147	0.330	0.522

Note: %(1+2) is share of disagree; 3% is share of neutral; %(4+5) share of agree

Next, we estimate differences in the probability of agreeing (Likert 4 or 5) that tightening the lifeline block affected energy and non-energy indicators. We first construct affordability and energy-consciousness indices and then use doubly robust IPWRA on the trimmed common-support sample to compare treated households (≤30 kWh) with controls (>30 kWh).

Table 22 reports the results. For the affordability index (higher = worse), households within the lifeline block show slightly better affordability than the control group, though the effect is small. The estimated average treatment effect on the energy-consciousness index is positive, suggesting that lifeline households are more likely to adjust consumption behavior to remain eligible after tightening, but the estimate is not statistically significant. Covariate-balance tests are satisfied, supporting the ignorability assumption within the trimmed overlap sample.

Overall, after adjustment, treated households are not worse off on affordability and appear somewhat more energy-conscious, although the effects are statistically weak. The

direction of the estimates is consistent with the RD evidence of a discrete upward jump in electricity expenditure at the 30 kWh cutoff.

Table 22: Ttest treatment differences on idx_aff/idx_cons
Estimator: double-robust IPW Regression Adjustment
Treatment Model: Logit

Effects		Idx_affordability	Idx_ Energy consciousness
ATE			
	T30	-0.352	0.789
	(≤30 vs >30)	(0.2254)	(0.5901)
PO (Mean)			
	T30	0.032	-0.0002
	(≤30 vs >30)	(0.0719)	(0.0907)
Cov balance test			
Chi2(10)		2.061	1.7084
Prob>chi2		0.996	0.998
Obs		962	937

Second, we estimate—using an ordered logit model—the probability of agreeing/strongly agreeing (Likert 4 or 5) with each individual energy and non-energy statement, comparing treated households (≤30 kWh) with non-treated households (>30 kWh). Table 23 reports results for the energy indicators. Consistently, households within the lifeline block are more likely to agree that the tighter block increases bill pressure, constrains access, and makes them more energy conscious. This aligns with the consumption-bunching narrative: after the reform, households tighten usage and pursue conservation to avoid crossing the threshold and triggering the bill jump.

For the non-energy indicators, households with ≤30 kWh are also more likely to agree that the change reduces their ability to afford education and health expenditures, and these differences are statistically significant (Table 24). A similar pattern appears for perceived ability to afford food/water and other essentials, although these estimates are less statistically robust.

Overall, these results are consistent with the IPWRA and RD evidence: the tightened threshold creates a local price/bill discontinuity, households perceive increased bill pressure, and remaining eligible often requires constraining consumption—potentially implying foregone energy services with no clear improvements in broader welfare.

Table 23: Estimator: Ordered logit on the 1–5 scale with $P(4)+P(5)$ on common support sample for perceived energy-related effects due to change in block from 0 - 50 kWh to 0 – 30kWh

Dy/dx	ΔLLT reduce access	ΔLLT increases electricity bill	ΔLLT makes electricity less affordable	ΔLLT induces energy consciousness	ΔLLT encourages energy efficient adoption
T30= 0	0.625***	0.638***	0.669***	0.689***	0.676***
	(0.0357)	(0.0378)	(0.0388)	(0.0465)	(0.039)
T30= 1	0.805***	0.733***	0.681***	0.709***	0.699***
	(0.0371)	(0.0509)	(0.0686)	(0.0744)	(0.0690)
Difference test	0.180***	0.095*	0.0117	0.0198	0.023
	(0.0311)	(0.0502)	(0.0835)	(0.0598)	(0.0553)
Cov control	YES	YES	YES	YES	YES
Psuedo R^2	0.031	0.0156	0.0378	0.0155	0.0244
Obs	821	875	830	917	918

Standard errors computed using the Delta method. The controls: meter type, metre ownership type, household size, flexible appliance stock (level and square), dwelling type, gender, education of household head, and number of households sharing dwelling, and district fixed effects.

Table 24: Ordered logit on the 1–5 scale with $P(4)+P(5)$ on common support sample for outcomes as a result of changing tariff block from 0 – 50 kWh to 0 – 30kWh

Dy/dx	ΔLLT reduce education access	ΔLLT reduces health access	ΔLLT reduces ability to pay food and water	ΔLLT reduces ability to pay other essentials
T30= 0	0.640***	0.610***	0.656***	0.640***
	(0.0434)	(0.0474)	(0.0475)	(0.0438)
T30= 1	0.735***	0.694***	0.676***	0.661***
	(0.0629)	(0.0642)	(0.0735)	(0.0723)
Difference test	0.095*	0.083*	0.020	0.020
	(0.0519)	(0.0504)	(0.0328)	(0.0454)
Cov control	YES	YES	YES	YES
Psuedo R^2	0.0309	0.0326	0.0298	0.0232
Obs	851	866	864	870

Standard errors computed using the Delta method. The controls: meter type, metre ownership type, household size, flexible appliance stock (level and square), dwelling type, gender, education of household head, and number of households sharing dwelling, and district fixed effects.

6. Conclusion and Policy Recommendations

This study set out to (i) identify who benefits from Ghana's lifeline tariff, (ii) estimate its impact on energy and non-energy welfare outcomes, and (iii) assess how tightening the lifeline block from 0–50 kWh to 0–30 kWh affects households. Using household survey data from 1,098 households across ten ECG districts, we combined multinomial/binary selection models

for beneficiary profiling, doubly-robust IPWRA on a trimmed common-support sample, and a local RD design around the 30 kWh cutoff, with extensive robustness checks (small-sample inference, ITT, overlap-weighted FE models, perception-based proxies, placebo cutoffs, symmetric and donut bandwidths). The following results emerged from the study:

Who benefits from lifeline tariff Policy? Only a small share of households benefits from the lifeline tariff, and beneficiaries are disproportionately located across ECG districts with heavy concentration in low-income areas. Household size, appliance stock, meter type/ownership, education, and dwelling characteristics systematically sort households into low- and higher-consumption blocks. Smaller households with limited appliance stock, lower education, and fewer households sharing a dwelling are more likely to qualify.

Welfare effects at 30 kWh threshold. Lifeline eligibility delivers bill relief, but its broader welfare gains are muted. Near the threshold, many households reduce or delay energy services to remain below 30 kWh, implying conservation via deprivation rather than via efficiency. This crowds out potential positive spillovers to education, health, food/water, and digital expenditures.

Effect of tightening policy from 50kWh to 30kWh. The discrete price jump at 30 kWh induces bunching just below the cutoff and raises bills sharply for households whose unconstrained demand lies just above 30 kWh. Given similar socio-economic profiles on either side of the cutoff, the tightening risks distributional inequities—penalizing near-poor or larger households that narrowly exceed 30 kWh.

Awareness gaps. Awareness of the lifeline policy is low—even among potential beneficiaries—limiting take-up and blunting intended protections.

The big story here is that though lifeline policy protects some low-consumption households via bill relieve, tightening the policy to 30 kWh has increased consumption bunching, forcing energy deprivation rather than energy efficiency. A redesigned schedule with micro-steps, targeted cushioning for near-threshold users, strong awareness, and efficiency support can retain conservation benefits while safeguarding welfare—especially in low-income districts—thereby aligning the policy with affordability, equity, and sustainable use.

Specifically, the regulator should soften the cliff at 30 kWh by introducing multiple micro-steps (e.g., 0–30, 30–35, 35–40 kWh) instead of the single steep jump at the current threshold or a smoother increasing block tariff that would preserve household level conservation incentives while at the same time reducing bill shocks for near-threshold users.

Again, the regulator can implement a targeted cushioning for “near-miss” households. This could be done for example, via the introduction of automatic, time-limited rebates or

credits for those customers repeatedly billed in the 30–40 kWh range, where bunching and deprivation are most likely to occur. The regulator can use metering data to flag eligible accounts.

To minimize deprivation and ensure that the policy promotes efficiency, the regulation can pair pricing with efficiency, not deprivation. This could be achieved for example by bundling the lifeline with low-cost efficiency kits (LEDs, fridge seals, smart plugs) and usage feedback (SMS/app prompts on nearing 30 kWh) so reductions come from efficiency and load-shifting rather than cutting essential services. Finally, the regulator needs to design district-tailored education campaigns to promote the awareness level among households.

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