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"Wild" Tariff Schemes: Evidence from the Republic of Georgia

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“WILD” TARIFF SCHEMES: EVIDENCE FROM THE REPUBLIC OF GEORGIA

Anna Alberini
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$$\frac{1!}{(m-1)!} p^{m-1} (1-p)^{n-m} = p \sum_{\ell=0}^{n-1} \frac{\ell+1}{n} \frac{(n-1)!}{(n-1-\ell)! \ell!} p^{\ell} (1-p)^{n-1-\ell} = p \frac{n-1}{n} \sum_{\ell=0}^{n-1} \left[\frac{\ell}{n-1} + \frac{1}{n-1} \right] \frac{(n-1)!}{(n-1-\ell)! \ell!} p^{\ell} (1-p)^{n-1-\ell} = p^2 \frac{n-1}{n} +$$

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“Wild” Tariff Schemes: Evidence from the Republic of Georgia

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Abstract:

Consumers often struggle to grasp complicated pricing plans, including increasing block rate (IBR) schemes, which have been used for decades by utilities in many parts of the world. The assumption that they encourage conservation has, however, recently been challenged (Ito, 2014). We take advantage of the unique IBR tariffs for electricity in the Republic of Georgia—where “overage” is penalized more heavily than in conventional IBRs—to ask whether consumers respond to price, and to which price specifically. Based on the data from several waves of the Georgia Household Budget Survey, we find evidence of “notches,” namely missing probability mass on the right of the lowest block cutoff and a spike in the frequency of monthly consumption to the left of it. This is in contrast with the “bunching” pattern predicted by Borenstein (2009) when demand is not completely inelastic, and with the empirical evidence in Borenstein (2009) and Ito (2014). During our study period (2012-2019), the tariffs were revised—both downwards and upwards—to a different extent in different blocks and at different times across the regions of the country. We devise difference-in-difference study designs that exploit such natural experiments, finding that consumption did increase when the tariffs were reduced and fell when they were raised. Ours is one of the few studies that exploits quasi experimental conditions to examine whether the response to price changes is symmetric. We find that it is, in that the implied price elasticity of electricity demand is in both cases -0.3 . Finally, we fit an electricity demand function, which results in an even stronger price elasticity (-0.5). Households seem to respond to the actual, average price (here equal to the marginal price) rather than to expected price. Our estimates of the price elasticity bode well for a carbon tax, an energy tax, or

simple tariff increases to help curb imports of gas-fired electricity from neighboring countries.

JEL: D12, Q41, Q48

Keywords: residential electricity demand, price elasticity, increasing block rates, tariff schemes, asymmetric response to price changes

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1. Introduction

Consumers often find it exceedingly difficult to grasp the full price of a product (Chetty et al., 2009), or changes thereof, especially when the latter are not transparently communicated to them (Finkelstein, 2009; Sexton, 2015), when pricing schemes are complicated, and when they require consumers to make predictions about their future demand for a good (Bar Gills and Stone 2012, DellaVigna and Malmendier, 2006).¹ Consumers' cognitive skills are for example stretched to the limit when it comes to estimating the fuel costs of a car, an essential part of the process of purchasing a vehicle. Consumers must forecast the future prices of motor fuels, combine such forecasts with technical information about vehicle's fuel economy, and predict how many miles they will drive the car (Allcott, 2011).

Utilities, such as electricity, gas or water, are another setting where pricing schemes can be complicated and result in multiple prices. This is the case with two-part tariffs, and/or increasing block rate (IBR) tariff schemes. While IBRs do not necessarily align the price charged to consumers with the cost of generating and delivering electricity to them, they have been used for decades in an effort to encourage conservation and/or help pay the bills of customers in the lower income brackets, who are to be subsidized by high-volume—and presumably wealthier—customers.

In the most common IBR schemes, consumers pay a higher price per unit of energy or water only on the consumption units that exceeds the previous block's cutoff. For example, in a two-block scheme with cutoff at, say, 300 kWh/billing period, a consumer would be billed $p_1 \times Q$

¹ DellaVigna and Malmendier (2006), for example, document how gym goers often choose membership plans that imply higher cost per visit than other options, and pay a premium for the option to cancel the membership every month--when in reality they stay enrolled longer. These findings suggest mistrust of certain types of pricing and membership commitment, overconfidence over future self-control and efficiency, and a number of cognitive difficulties when it comes to processing average and marginal prices and one's own demand for the good (gym visits).

if $Q \leq 300$, but p_2 (with $p_2 > p_1$) for each kWh above 300, for a total bill of $p_1 \times 300 + p_2 \times (Q - 300)$ if consumption Q exceeds 300. With an average price per kWh above the marginal block price, how is a consumer predicted to respond? Borenstein (2009) shows that consumers should be “bunching” around the block cutoff, with the “spike” at the block cutoff being more pronounced when the variance of the consumers’ error in predicting consumption is lower, or when the price elasticity of demand is stronger.

But it is typically difficult for consumers to observe in real time or predict with precision their consumption of electricity, natural gas, and water in any given billing period, and to grasp block rate schemes—especially when the blocks are numerous and combined with two-part pricing. Liebman and Zeckhauser (2004) argue that consumers facing complicated pricing schemes (or variable income tax rates) develop pricing heuristics and mental models of their own, and Rees-Jones and Taubinsky (2019) document evidence of such behaviors in incentivized experiments about the US federal income tax schedule.

Ito (2014) exploits quasi-experimental variation in electricity rates in a Southern California community, showing that the distribution of consumption is smooth and bears no evidence of “bunching” or “spikes” at the block cutoffs: Either the price elasticity of demand is zero, or consumers are responding to some price *other* than the marginal block price. Indeed demand depends on the *actual* average price (Foster and Beattie, 1981),² and neither marginal price nor *expected* average price are significantly associated with demand, once actual average price is controlled for. This defeats the incentive-to-conservation purpose of IBR pricing, as Ito shows theoretically that consumers that respond to average price use more electricity than if they were responding to marginal price. The price elasticity of demand is estimated to be -0.08.

² Foster and Beattie (1981) argued that consumers are more likely to respond to the average price because this only requires knowledge of the total bill and consumption.

Are these results unique to the Southern California context, or are they common to many other utilities' IBR schemes? If so, can IBR schemes be amended to maintain the incentive to conservation? In this paper we examine a unique IBR scheme from Georgia, a former Soviet republic in the Caucasus region.³ Since July 2006, residential electricity customers in the Republic of Georgia have faced a one-part, three-block IBR tariff scheme where the block cutoffs are 101 and 301 kWh/month. The tariffs are set by the regulator, are exogenous to individual households, and remain in place for three years.

Unlike most common IBRs, however, consumers whose electricity usage falls in the second block pay the price per kWh of the second block for their *entire* consumption during the billing period, not just for the consumption that exceeds 101 kWh. Likewise, consumers whose electricity usage falls in the third block pay the price per kWh of the third block for their entire consumption, not just for kWhs above 301. This unusual, almost “wild” tariff scheme—and the absence of a monthly fixed fee—imply that the marginal price and the average price per kWh are the same.

One would expect that such a stark difference in the total bill—compared to the bill calculated under traditional IBRs—would induce consumers to be careful and try to limit consumption to a stronger degree than they would under conventional IBRs. Does this scheme get consumers to pay attention to the usage blocks? Is it effective at encouraging conservation?

We use consumption data from January 2012 to November 2019 from the households that participate in Georgia's Household Budget Survey (HBS). The HBS collects data from households on a quarterly basis, but asks households to report expenditures for each of the three

³ As of January 2021, the population of the Republic of Georgia was 3.73 million. In 2019, its GDP per capita was \$14,863, which is roughly on par with that of its neighbor Azerbaijan (\$14,451) and slightly higher than that of Armenia (\$13,824). For comparison, Russia's GDP per capita in 2019 was \$28,213 and that of the EU \$44,491 (all figures in current PPP international dollars; source: The World Bank).

months in a quarter, allowing us to form a panel dataset that covers each participating household for 12 months. Participating household enter, and exit, the HBS in staggered quarters.

We ask three research questions: First, with such an unusual IBR system, does consumption “bunch” at the block cutoffs? Second, how pronounced is the responsiveness (if any) to the price of electricity? We take advantage of the fact that three electric utilities cover the residential market in Georgia. They all apply the same block cutoffs, but different block prices, and they changed the rates in different blocks at different times. This provides us with an opportunity to devise two natural experiments and apply difference-in-difference study designs to identify the responsiveness to price, which we summarize into a price elasticity. Third, what price elasticity can be inferred from fitting directly an electricity demand function? Is this price elasticity stable over time? Is there a role for *expected* price, or does consumption depend exclusively on actual price?

Our results are striking. First, histograms of the consumption data from the HBS suggest “notches,” rather than bunching, with missing probability mass to the right of the first block cutoff and excess probability mass to the left of that cutoff. We compare these histograms with those from a sample of residential accounts situated on the Tbilisi city border (a suburban location with single-family homes as well apartments in multi-family buildings, but of more modest nature than those found in Tbilisi City), which show moderate but clear, and statistically significant, evidence of bunching, at least for 2012-2015.

Second, our DID designs suggest that people did increase consumption when the prices were reduced, and did cut down on consumption when the tariffs were raised again. Ours is one of the few investigations that has sought to examine whether consumers respond symmetrically to price changes using micro-level data (as opposed to aggregate, economy-wide time series; see

Gately and Huntington, 2002, Adofo et al., 2013; Tajudeen, 2021), and, to our knowledge, the first to do so using a quasi-experimental approach.⁴

The implied price elasticity from our DID designs is -0.3. We interpret this as a short-run elasticity, noting that it is much stronger than the figures in recent quasi-experimental studies in the US (-0.09 in Deryugina et al., 2020, and in Ito, 2014). We also fit a demand function, relying on the changes in tariffs across places and over time, which points to a price elasticity that is even stronger (-0.49) and generally stable over our study period, except for the last three years.

Our “clean” setting (where the marginal block price is equal to the average price) provides us with an opportunity to test empirically whether *expected* price matter, in addition to or in lieu of the actual price. Expected price appears to be given heavier weight than actual price only towards the end of our study period, perhaps signaling weakened attention to the tariffs.

In sum, we find that consumers respond to the price of electricity, even though a telephone survey that we conducted nationwide in the summer of 2021 indicates that many of the survey participants do not know the price they paid per kWh in the most recent billing period. That consumers behave *as if* they respond to prices, or at least to the *direction* of the changes in prices, and that their responsiveness has such magnitude, is especially important, considering that electricity prices in Georgia are very low compared to those in neighboring or other European countries, and that Georgia has faced energy security issues in the last few years that make it important to limit residential electricity usage. Indeed, a modest price increase (or an energy tax, or a carbon tax of \$25/ton CO₂, as per the recommendations of international organizations) would reduce residential demand enough to do away with the need for imports.

⁴ Frondel and Vance (2013) find evidence of approximately symmetric responses to gasoline price changes using data from a panel of German drivers.

The remainder of this paper is organized as follows. Section 2 provides background information. Section 3 presents the data. Section 4 describes our methods, and section 5 the results. Section 6 concludes.

2. Background

A. The Residential Electricity Market in the Republic of Georgia

Electricity consumption has been growing in recent years in the Republic of Georgia. In 2019, Georgia consumed almost 13 million MWh—20% more than in 2015. This growth is attributed to improved economy activity, more extensive use of air conditioning in homes and in the hospitality sector,⁵ and cryptocurrency mining, which is currently thought to account for some 15% of the power load in Georgia (IEA, 2020). Almost 75-80% of all electricity generated in Georgia is hydroelectricity. Supply is abundant in the spring, but less so in the winter, which means that gas-fired plants and imports from Azerbaijan and Russia must be used to meet the demand.

As a gas and oil transit country, Georgia avails itself of inexpensive natural gas imports from these two neighboring countries to feed its thermal power plants. This results in gas-fired electricity priced below the market price. While the government does not actually use up budget to cover these implicit subsidies, it does miss out on a potentially important source of tax revenue (IEA, 2020).⁶

Energy security is an issue for Georgia. The country was at war with Russia, one of its two largest suppliers of oil and gas, in 2008. About a quarter of its hydropower capacity is run-

⁵ Levan Pavlenishvili of the International School of Economics at Tbilisi State University (ISET), personal communication, September 2021.

⁶ The implicit subsidies for natural gas supplied to the residential sector and for electricity were about 1.1% and 0.5% of GDP in 2017, or about 6.7% of budget spending for that year (IEA, 2020).

of-river, and the associated generation is subject to seasonal fluctuations. Its most important plant, the Enguri hydropower plant, which serves 33% of the electricity market and has storage (IEA, 2020), is located in, and shared with, Abkhazia, an autonomous region that the central government of Georgia has virtually no control over.

Some 60% of all electricity is used in the commercial and industrial sectors (IEA, 2020), and the residential sector accounts for 20% of all electricity. During our study period, the residential electricity market was controlled by three companies—Telasi (in Tbilisi), which is part-owned by the government (for about 25% of its assets), Kakheti Energy Distribution (in the Kakheti region), and Energo-Pro Georgia (EPG), which is privately owned, in the rest of the country. In 2017 EPG absorbed the Kakheti company. By the end of 2019, EPG accounted for 39% of the electricity market in Georgia, served over 1.2 million consumers, and owned and operated 15 medium and small hydropower plants.

B. Residential Electricity Bills

The billing period for residential customers is one month. Customers receive their bill by mail, SMS to the customer's mobile phone, email, or sometimes by a collector sent by the utility. Figure A.1 in the Appendix shows what is displayed when the customer opens the link sent by the utility via SMS. This is the same as a paper bill sent by mail. The bill displays the previous and the most recent meter readings, calculates consumption during the billing period as the difference between the two, and reports the tariffs, but omits the block cutoffs. The amount due is also displayed, along with the most recent payments to the utility, the due date, and the reminder that non-payment means that service will be cut off.

Residential customers pay the bill in person at the post office or at their bank, online, or at “pay boxes”—devices that resemble ATMs but are meant to take care of various types of payments rather than withdrawing or depositing cash from or into someone’s bank account. Occasionally payment is made to the collector sent by the utility.

C. Tariffs

In the Republic of Georgia residential customers are charged one-part, IBR tariffs. The tariffs are set by GNERC, the regulator, and are published and remain valid for three years (GNERC, 2021). The tariffs are therefore exogenous to an individual consumer, although, as always with IBRs, the price per kWh paid by the consumer is mechanically positively correlated, and endogenous, with consumption.

There are a total of three blocks: up to and including 101 kWh/month, 102-301 kWh/month, and 302 and more kWh/month. Table 1 reports the tariffs for each block in Tbilisi, Kakheti, and the rest of Georgia from Jan 2012 to November 2019. On average the tariff per kWh in the second block is 29% more than that in the first block, and that in the third block 29% that in the second block.⁷

One important feature of the tariff scheme in Georgia is its difference with respect to conventional IBRs applied by electricity, gas and water utilities in many other parts of the world. A consumer whose electricity usage falls in the second block, for example, pays the second-block price per kWh on the full electricity consumption, not just on the portion that exceeds the first block. Likewise, a consumer whose usage falls in the third block pays the third-block price

⁷ These averages are weighted by the shares of each utility in our sample and the number of months that each new tariff stays in place.

for all kWhs used.⁸ One would expect this scheme to generate a large enough penalty on excessive consumption as to induce restraint. The incentive to conservation should be clear to the consumer, since with this tariff structure the average price per kWh is equal to the marginal price.⁹ We leave it to our empirical analyses below to check whether this is indeed the case, as the electricity tariffs in Georgia are among the lowest in the world, along with those of Russia, Azerbaijan and Belarus, for an approximate \$0.05/kWh (2017 PPP \$)(IEA, 2020).

Table 1 shows that in January 2013 the tariffs in the first and second block were reduced everywhere—by some 27% in the first block and 21% in the second—while those for the third block remained unchanged. This was done purely for political expediency as the newly elected government was rewarding the public for its support.¹⁰

EPG raised the tariffs again in August 2015; Telasi, the Tbilisi utility, in September 2015, while the Kakheti utility waited until February 2016. This time the tariff increases were largest in proportional terms in the first block (+37-45%) and smallest in the third (+21-22%).

These differentiated price drops/hikes provide us with two natural experiments (described below), which we exploit to see if households respond to price changes, and, if so, to see whether they respond symmetrically to decreasing and increasing prices. Further revisions in the tariffs in 2017 and 2018 provide additional variation, which, combined with the 2013-2016 changes, lets us estimate a demand function.

⁸ To illustrate, consider the tariffs in place in 2019 (which remained in force until the end of 2020). An Energo-Pro Georgia customer who used 101 kWh in a month would be billed $101 * 0.1424 = 14.38$ Georgia Lari (GEL), but $102 * 0.18207 = 18.57$ GEL if had used 102 kWh (a 28% increase with respect to the bill if the first-block rate had been applied to 102 kWh). A consumer who used 301 kWh would pay $301 * 0.18207 = 54.80$ GEL, but $302 * 0.22726 = 68.63$ GEL if he had used just one more kWh (a 25% increase over the bill he would have paid if the second-block rate had been applied to 302 kWh).

⁹ This does away with the disincentive that may exist with conventional IBRs if consumers respond to the average price instead of the marginal price (Ito, 2014).

¹⁰ In February 2012, ahead of the parliamentary elections, the opposition party Bidzina Ivanishvili (“Georgian Dream”) announced that the current energy tariffs were too high due to the corruption and/or incompetence of the current government and the regulator, and promised that if their party came to power, there would be a meaningful reduction in the tariffs (see <http://www.ivote.ge/images/doc/pdfs/ocnebis%20saarchevno%20programa.pdf>). Indeed, after the elections, the political coalition “Georgian Dream” kept its promise.

An important issue is whether consumers are aware of the tariffs and the tariff schemes. These are displayed on the web sites of GNERC (the regulator), the electricity utilities, and articles about impending tariff changes appear in the daily or periodical press. A Google Trends search for “electricity tariffs” suggests that the peak in search activity took place at the end of 2012 and beginning of 2013, and that search activity was more moderate, but regular, thereafter, especially around the time of tariff revisions (see figure A.2, panel A, in the Appendix). We also counted the number of articles about “electricity tariffs” published by the popular Georgian news agencies Interpressnews¹¹ and Ambebi¹² between January 2012 and December 2019. During this period, a total of 235 and 100 articles about “electricity tariffs” were posted on the Interpressnews and Ambebi websites, respectively, with many of them appearing when revisions were about to or had just taken place (see figure A.2, panels B and C).

3. The Data

We use household-level information about electricity consumption from a total of three sources: i) several waves of the Household Budget Survey (HBS), ii) a random sample of customers from around the Tbilisi city limits (covered by both Telasi and EPG) over the same period, and iii) our own nationwide telephone survey of some 1000 members of the general public. The residential electricity tariffs (available from the regulator as well as from the utilities) are matched to the data from i) and ii). Weather information was aggregated to daily average temperature and used to compute the total heating degree days (HDD) and cooling degree days

¹¹ <https://www.interpressnews.ge/>

¹² <https://www.ambebi.ge/>

(CDD) for each month in the sample at each HBS location and in Tbilisi, using 18° C as the base.¹³ We discuss sources of data i)-iii) below.

A. The HBS

Our main analyses rely on a total of 8 waves (from 2012 to 2019) of the HBS. The HBS is conducted every year by Georgia Stat, the national statistical service, with each participating household being contacted quarterly to collect data about the composition of the household, the dwelling, expenditures on several categories of goods and services, and income. Participating households join the HBS in staggered quarters and stay on it for a total of four quarters. Each wave of the HBS covers—depending on the year—between 5500 and 7500 households.

The Georgia HBS does not ask participants to report their domestic consumption of electricity and other fuels in physical units (e.g., kWh for electricity, cubic meters for natural gas, liters for liquid fuels): Each quarter, it simply asks respondents to report their *expenditures* on each fuel in each month of that quarter. This means that it is possible to form a monthly panel with electricity expenditures for T=12 months for each household.

We convert expenditures into kWhs consumed by matching the expenditure in a given month with the tariffs in place in that month at the respondent’s location. “Budget billing” does not exist in Georgia, and the utilities do not allow partial payment of the bills,¹⁴ so it is straightforward to convert the bill to kWh consumed.

For good measure we exclude from the sample persons who appear to be sharing housing (and hence splitting bills) with unrelated adults (26 individuals), as well as 73 households who report the same amount no matter what month, report implausible amounts or display

¹³ Weather data were obtained from the Global Land Assimilation System (GLDAS v2.1), which is a re-analyzed gridded climatic dataset, with 0.25° x 0.25° spatial and 3-hourly temporal resolution.

¹⁴ Customers can only request a 10-day extension to the due date.

implausible electricity expenditure patterns. Even so, many HBS respondents appear to have rounded their bills, which may create measurement error in the kWh consumed (and potentially in the price faced by respondent). For this reason, we deploy up to three alternate ways to compute consumption from the reported expenditure and check that our findings are consistent across the three approaches.¹⁵

By our preferred measure, the average consumption is 123 kWh/month. Figure 1 shows that consumption is, as expected, highly seasonal. Most urban households in Georgia use natural gas for space heating in the winter, while households in rural areas use primarily natural gas, firewood and propane,¹⁶ suggesting that the higher winter consumption of electricity is due primarily to lighting, appliance use, and use of electric space heaters as a supplement to the main heating system. Summers are warm in Georgia, but the HBS data reveal that no more than 7% of the households own proper air conditioning. Ownership of television sets and cell phones is almost universal. Surprisingly, only towards the end of our study period did the share of dwellings equipped with a refrigerator reach 90%. Overall, some 55% of the electricity consumption observations fall in the first block, 40% in the second block, and the remainder in the third.

¹⁵ We use the tariffs in place at the end of 2019 to illustrate our calculations. Under method 1, consumption in the first block must be billed up to 14.38 GEL, in block 2 between 18.57 and 54.80 GEL, and in block 3 68.63 GEL or more. We divide the reported expenditure by the appropriate block rate to obtain an estimate of the kWhs. When one consumer reports a figure that falls outside of the above listed intervals (for example, 17 GEL), we simply refrain from calculating the kWhs for this consumer. For the study period starting in January 2012, this method results in 228,270 valid observations and 36150 missing observations for kWh. Method 2 is similar, but handles the “uncertain cases” by placing in the second block (and hence applies the second block rate) anyone with bill greater than 14.38 GEL, and in the third block anyone with bill greater than 54.80 GEL. Method 3 is similar to method 2, but attributes the “uncertain cases” depending on the difference between the reported amount and the amounts at the cusp between blocks. For example, if the expenditure is 17 GEL, method 3 assumes that this customer fell in the second block because $\text{abs}(17.00-18.57) < (17.00-14.38)$. Method 2 and 3 result in no missing values for consumption. Table A.1 in the Appendix shows that the three alternate procedures yield similar consumption figures. All results reported in this paper are based on method 2.

¹⁶ The HBS indicates that in rural areas 54% of the heating expenditures are for natural gas, while 26.3% and 18% are spent on firewood and propane.

B. The Tbilisi City Limit Data

We obtained exact meter readings from January 2012 to December 2019 from a total of 10537 residential accounts located on the Tbilisi city limits. These data were provided by GNERC, the electricity regulator. We specifically requested usage and bills from accounts associated with homes on the city limits as we expect households and dwellings barely inside the city limits (Telasi customers) to be similar in all respects to households and dwellings barely outside the city limits (served by EPG). Since this is not a nationally representative sample, we use this panel dataset primarily for purposes of comparison with the HBS.

Some 39% of these homes are served by Telasi, and the remainder are served by EPG. Single-family homes account for almost 50% of the addresses, and units in multi-family buildings for 28.38%. We are unable to identify the exact type of dwelling in the remaining 21.78%.

The exact meter readings from the Tbilisi area average 165 kWh/month. They display the expected seasonal patterns (see figure 2) and the same slightly inverted-U shape over the study period as the data from the HBS. Direct comparison with the HBS is difficult, because the HBS does not describe the type of dwelling but does provide its size in square meters, whereas it is the opposite with the regulator-provided, exact meter readings. The latter are, however, free from measurement error, allowing us to check for “bunching” at the block cutoffs and examine the effects of some of the tariff changes.

C. Telephone Survey

In July 2021, we conducted a nationwide telephone survey of Georgia residents.¹⁷ The questionnaire elicited information about the electricity bills, consumption, tariffs, as well as conservation behaviors. The survey was meant to ascertain attentiveness to electricity consumption and awareness of the tariffs system, and as such was directed to the person in the household most familiar with the electricity bill.

We obtained a total of 1057 completed questionnaires. With the caveat that this survey was conducted some 18 months after the end of our HBS study period, and during the Covid-19 pandemic, the data reveal that almost all households by now receive their bill by SMS. Some 17.5% of them pay their bill in person (at bank or at the office of the utility), 40% using online or mobile banking, 60% using a pay box, and 2.5% directly on the utility website. When questioned about their most recent bill (from June or July 2021), 90% of the respondents were able to recall how much the bill was, and the remaining 10% were able to pinpoint ranges out of a pre-selected array that we read to them.

But fully 80% were unable to name the price per kWh they paid on the most recent bill,¹⁸ and 48% remained unable to say how many kWh they consumed during the most recent billing cycle, even after being prompted with possible consumption categories. Taken together with the fact that about 60% appeared to know about the very particular IBR scheme in Georgia, these statistics point to the possibility that the majority (but not all) of the consumers are aware of the blocks and seek to not to exceed them, because doing so results in higher prices per unit—even if they don't know exactly how much higher.

¹⁷ The survey was administered by Gamma, a market research company with headquarters in Tbilisi. A total of 8341 phone numbers were attempted. Of these, 4081 never answered. Of the 4260 that answered, 1057 agreed to participate in the survey, for a response rate of 24.8% out of the 4260 contacts made.

¹⁸ This cannot be attributed to lack of salience induced by the mode of payment, because the share of respondents who do not know the price per kWh is the same regardless of whether payment is made in person, at a pay box, or online.

4. Methods

A. Tariff Decrease Natural Experiment

In January 2013, the tariffs in the first and second block were abruptly reduced, while those in the third block remained unchanged. Tariffs were not changed again until August 2015 for EPG, September 2015 for Telasi, and February 2016 for the Kakheti utility.

We interpret this tariff revision, which occurred at the order of the government, as a natural experiment, and deploy a difference-in-difference study design to examine whether the tariff reduction engendered an increase in consumption, as predicted by economic theory as long as the demand is not completely inelastic. Our study period is January 2012 to December 2014, which brackets the tariff reduction but ends well ahead of the tariff hikes of summer 2015. The study design is summarized in table 2.

We regard the tariff reduction in the affected blocks as a binary treatment. The control group is comprised of the only unaffected households, namely those with consumption levels large enough to fall in the third block in every single month.¹⁹ The treatment group is comprised of everyone else. We fit the regression equation:

$$(1) \quad \ln E_{it} = \alpha_i + W_{it}\beta + \tau_t + D_{it}\delta + \varepsilon_{it}$$

where i denotes the household, t the month and year, E electricity consumption, and W a vector containing the logs of the heating degree days and cooling degree days. D is a dummy that takes

¹⁹ Implicit in this choice is the assumption that we correctly identified households that would have never moved to a lower consumption block for any reason, let alone the price of electricity. Simple t tests indicate that before the treatment our control group was no different from the treatment group in terms of dwelling size, but had higher income and household size, was much more likely to reside in Tbilisi, and virtually never applied for social assistance from the government. A probit regression results in coefficients of -0.0012 (t stat. -2.81) on the dwelling size, 0.4887 (t stat 12.85) on a Tbilisi dummy, 0.0583 (t stat. 19.70) on log income, 0.1676 (t stat. 2.43) on family size, 0.8725 (t stat 14.35) on not applying for social assistance, and -0.1348 (t stat. -1.69) for every adult-equivalent in the household. The likelihood ratio statistic of the null that all regressions do not matter is 1768.49 (p value less than 0.00001).

on a value of one in the months with the reduced tariffs only for the treatment group, and zero otherwise. Equation (1) contains household-specific fixed effects (α_i) and time dummies (τ_t), and is estimated using the “within” estimator. Parameter δ is the average treatment effect on the treated (ATT).

Equation (1) assumes that seasonal effects take the form of the same proportional increase in consumption from the baseline for both treated and control subjects, and that any trends are common between the two groups. The latter assumption is partially tested, and confirmed, in figure 3, which shows that the control group exhibited regular seasonal fluctuations in consumption around a stable level both before and after the tariff revisions. Before the tariff revisions, the treatment group households exhibited similar patterns, albeit at lower consumption levels; after the tariff revisions, their consumption grows.

We formally test the common trends assumption by focusing on the 2012 data, and fitting equation (1) without the treated dummy, but with region-by-month terms interacted with the treatment group dummy. An F test does not fail to reject the null that the coefficients on these interaction terms are jointly equal to zero at the conventional levels (F statistic 1.31, p value 0.1031).

B. Tariff Increase Natural Experiment

The next price revision occurred in the late summer of 2015 (in August for EPG and September for Telasi) or in early 2016 (February, for Kakheti) and affected all blocks—at different times in different parts of the country. We begin with selecting the span of time from January 2015 to January 2016 as our study period, covering 7 months before and up to 7 months after the tariff revisions in much of the country, but stopping short of the tariff revisions in

Kakheti. Again, we regard the tariff revisions as a binary treatment. The treatment group is comprised of customers in Tbilisi and the rest of the country; the control group is comprised of customers in Kakheti, who did not experience any tariff increase during this period.

We fit the regression equation:

$$(2) \quad \ln E_{it} = \alpha'_i + W_i \beta' + \tau'_t + d_{it} \delta' + e_{ijt}$$

which uses the same variables as equation (1) and includes household-specific and month fixed effects. This time d is a dummy that takes on a value of zero for Kakheti customers and customers in Tbilisi and the rest of the country before the tariff hikes, and a value of one for Tbilisi and rest of Georgia (excluding Kakheti) customers after the tariff hikes. We expect parameter δ' to be negative.

Figure 4, panel A, displays the average log monthly consumption in the treatment and control groups in the months preceding and following the tariff revisions. The figure shows that Kakheti consumers generally use less electricity than those in any other part of the country. Their consumption is roughly parallel to that of residents elsewhere before the tariff revisions, and is sustained thereafter, while that in Tbilisi and the rest of the country drops sharply right after the treatment and remains at a lower level than before.

We formally test for common trends using two possible models, both of which are fit to the data up to and including July 2015. The first model regresses log consumption on log HDD, log CDD, log income, and their interactions with a treatment group dummy, plus a quadratic time trend, and the interactions of the two time trend terms with the treatment group dummy. An F test indicates that the null that the latter two interactions enter insignificantly is not rejected at

the conventional levels. This is the case for both the full treatment group as well as when Tbilisi households are excluded from the treatment group.²⁰

In an alternate specification, the two time trends are replaced by month dummies. When Tbilisi is excluded from the sample, F statistics indicate that the coefficients on the month dummies are jointly significant, and that those on the interactions between the month dummies and the treatment group dummy are jointly insignificant, implying that the data are consistent with the common trend assumption, at least before the tariff reforms.²¹ This is not the case when Tbilisi is included.²²

Since the two alternate tests of the common trends assumption do not agree about the role of Tbilisi, we report below results with Tbilisi included and excluded, respectively, from the sample. In addition, we examine a longer study period—one that starts in January 2014 and still ends in January 2016 (see figure 4, panel B). This longer sample period allows us to abandon the difference-in-difference design and to include more flexible region-specific time trends.

C. Demand Function

We pool all consumers and all months from January 2012 to November 2019 to fit the residential electricity demand function:

$$(3) \quad \ln E_{it} = \alpha_i'' + W_{it}\beta'' + \gamma'' \cdot \ln \text{Income}_{it} + \lambda \cdot \ln \text{Price}_{it} + \tau_{jt}'' + \eta_{it}.$$

In addition to household fixed effects, equation (3) contains, at a minimum, region-by-month and region-by-year fixed effects (with region represented by the subscript j), which we hope will

²⁰ The F statistics for the coefficient on time trend and time trend square interacted with the treatment group dummy are 0.11 (p value 0.5435) when the full treatment group is used, and 2.38 (p value 0.0930) when Tbilisi is excluded from the treatment group.

²¹ The F statistic for the null that the coefficients on the month dummies interacted with the treatment group dummy is 1.70 (p value 0.1154).

²² With this sample, the F statistic for the null that the coefficients on the month dummies interacted with the treatment group dummy is 3.52 (p value 0.0018).

capture unobservable demand and supply shocks at the regional level as well as differential trends across different areas of the country. To further capture unobserved heterogeneity, we experiment with rural area-by-month and rural area-by-year fixed effects.

Price in equation (3) is, of course, endogenous, and for this reason we instrument log price with the log of the tariff in each block (Olmstead and Mansur, 2012). We therefore leverage variation in the tariffs within regions over time, and across regions, to identify the responsiveness to price.

Since each household makes its own decisions regarding consumption, budgets, etc., when fitting regressions (1), (2), and (3), we reason that the tariff change treatments apply to individual households, even though they may be in common with many other households (in the case of the tariff reduction natural experiment), with entire regions of the country (in the case of tariff increase natural experiment), or the entire country (when tariffs were changed simultaneously in the service territories of all utilities). For this reason our inference relies on standard errors clustered at the household level (Abadie et al., 2020; Cameron et al., 2015).

5. Results

A. Evidence of Bunching

Figure 5 displays histograms of consumption from the HBS data. For the sake of brevity we group the data into 2012-2015 and 2016-2019, and display the histogram for two alternate bin sizes.²³ The results are striking for two reasons. First, there is a clear spike at 101 kWh/mo, and another—albeit less pronounced—at 301 kWh. The latter tends to disappear over time. Second, the spikes—when they are present—do not conform with the predictions in Borenstein (2009), in that they are not symmetric around the block cutoffs. Instead, they are suggestive of “notches,”

²³ The results are similar for all bin widths between 10 and 20 kWh.

with missing probability mass to the right of each block cutoffs and inflated frequencies to the left of the 101 kWh block cutoff, suggesting that households were trying to limit consumption so that they could avail themselves of the lower rate per kWh.

One problem is that the distribution of monthly consumption inferred from the HBS is so lumpy, as is typical with self-reported data due to rounding (see Battistin et al., 2003, and Alberini et al., 2021, for other examples), that it is difficult to assess conclusively from visual inspection alone whether the notches are real or simply data lumpiness.²⁴ We formally tested for the presence of notches using the procedure devised by Mavrokonstantis (2019). The notch at 101 kWh is statistically significant at the conventional levels when we use the procedure that does not seek to smooth out the data lumpiness (see figures A.4 in the Appendix).^{25, 26}

For comparison, figure 6 displays histograms from the sample of over 10,000 residential customers living around the Tbilisi city limits, for whom we have exact meter readings and amounts billed for each month. The evidence from these data is different: They show a moderate amount of true bunching, at least until 2014-2015. The pattern subsequently vanishes.^{27, 28}

²⁴ The pattern from the HBS-calculated data is robust to amending the exact calculation of electricity consumption from the reported expenditure (available from the authors).

²⁵ Lumpiness or rounding appears to occur at assorted electricity consumption values (e.g., 146 or 197), without any particular pattern, and for this reason we are unable to apply the correction for rounding recommended by Mavrokonstantis (2019).

²⁶ For 2008-2019, the frequency of the observations just to left of 101 kWh is 41.8% higher (s.e. 13.7%) than predicted a 5-th order polynomial. For 2012-2019, the excess is 19% (s.e. 9.7%), for 2008-2015 47.9% (s.e. 16.9%), for 2011-2015 21.2% (12.7%) and for 2016-2019 28.6% (s.e. 9.9%). These terms however are very imprecisely estimated, and statistically insignificant, when we allow for the notch to be diffuse, i.e., frequency peaks for values below, in addition to, 101 kWh/month.

²⁷ We use the procedure and code from <http://www.rajchetty.com/utilities/>. In 2012-14, the frequency of the observations at 101 kWh is 29.58% higher than that predicted by fitting a 5-th order polynomial. The standard error around this estimate is 14.56%, for an asymptotic z equal to 2.03, which is marginally significant at the 5% level. In 2012-15, the effect is similar (27.56% with a standard error of 15.30%) but its statistical significance is weaker ($z=1.80$, which is only significant at the 10%). In 2016-19, the effect is 8.75% (s.e. 11.49%), and is statistically insignificant at the conventional levels.

²⁸ In this paper we use the dataset from the Tbilisi city limits solely for comparison purposes with our main dataset, the HBS. The Tbilisi city limit dataset is not representative of Georgia or even of the Tbilisi residential customers, and its participants experience only the Telasi and Energo-Pro Georgia variation in the tariffs over time. This dataset is a panel with the bulk of the participants contributing 100+ monthly observations.

Since the evidence from the consumption histograms is ambiguous, we turn to our DID study design and regression-based demand estimation to see whether consumers do appear to respond to the tariffs and the tariff scheme.

B. Responsiveness to Price: Tariff Reduction

We report the estimation results from our difference-in-difference study of the tariff reduction in table 3. The first column reports the results of the basic specification (i.e., the one in equation (1)), showing that coefficient δ is estimated to be 0.0797 (s.e. 0.0086).²⁹ Adding log income, or region-by-month fixed effects, or both, and/or narrowing the time window for the sample have minimal impact on this estimate. This confirms that customers do respond to prices—when prices are dropping.³⁰

One concern is that the size of the control group is very small compared to that of the treatment group.³¹ To make sure that results are not driven by the large imbalance in sample size across control and treatment groups, we selected at random customers from the treatment group to ensure that the sample split between treatment and control group was i) 95% v. 5%, ii) 92% v. 8%, and iii) 85% v. 15%, respectively.³² We ran 500 replications (i.e., randomly selected samples from the treatment group) for each of i), ii), and iii), and they all confirm that consumers *do* tend to increase consumption when the price drops. Specifically, the estimated δ from each of these exercises is on average 0.0746 (s.d. 0.0187) in the replications of type i), 0.0693 (s.d. 0.0264) in the replications of type ii), and 0.0546 (s.d. 0.0461) from the replications of type iii).

²⁹ We report standard errors clustered at the household level. The regular standard errors are equal to 0.0050 for each of the estimates reported in table 3, and hence are about 60% of the clustered standard errors.

³⁰ When we repeat the tariff reduction difference-in-difference study with the data from the Tbilisi city limits, we find qualitatively similar results, but the magnitude of the effect is even larger: Consumption increased by approximately 15%.

³¹ The control group contains 115 households (825 total observations) v. 11313 (91985) in the treatment group.

³² We stop at the 85%-15% split, where each replication only contains some 5500 observations. More balanced sample splits have even fewer observations.

If we consider that the tariff reductions implied a 25% reduction in price (based on the share of customers in each block), it follows that the price elasticity is $[\exp(0.0797)-1]/(-0.25)\approx -0.33$. If we use the lowest estimates from table 3 we obtain $[\exp(0.0734)-1]/(-0.25)\approx -0.3046$. It seems therefore reasonable to conclude that based on this quasi-experiment and associated DID, the price elasticity of electricity demand is about -0.3.

C. Responsiveness to Price: Tariff Increase

The results from our second difference-in-difference study—the one about tariff increases with the Kakheti region representing the control group—are displayed in table 4 for the 2015-2016 sample period. The basic specification of the DID regression suggests that consumption dropped by about 10% as a result of the tariff increases, an effect that falls only very slightly upon further entering log income in the regression or dropping households with extremely low or high consumption from the sample.³³ Excluding Tbilisi households from the sample suggests a stronger effect, namely a 12-13% drop in consumption.

This time we can take random draws from the treatment group to ensure a 50%-50% split between the control and the treatment groups. Based on 500 such replications, the mean δ' across the replications is -0.1304 (s.d. 0.0176).

In table 5 we present the results from estimation efforts where we expand the study period (now Jan 2014-Jan 2016), which allows us to relax the common trends assumption of a DID study and include a different set of time controls. The results confirm that when the tariffs were raised, consumption declined. The estimated drop in consumption ranges from 5% (in the

³³ We drop observations with less than 31 kWh/month as they suggest no or only partial occupancy or splitting electricity bills to the point that consumption cannot be inferred from the reported expenditure. We use 31 kWh/month as a basic cutoff because running a refrigerator alone uses 1-2 kWh/day. We drop consumers with a calculated consumption of 520 kWh/month because this is the top 1% of the distribution.

specification with month-year dummies) to about 12-13% (in the specifications with the region-specific time effects).

In sum, our estimates of the ATT, when faced with an overall 35.62% price increase, imply price elasticities ranging from -0.14 to -0.36, with the bulk of the estimates between -0.22 and -0.36. Comparison with the elasticities from section 5.B suggests that it is not unreasonable to conclude that—at least from 2012 to the end of 2015—customers were responding symmetrically to price increases and decreases.

D. Demand Function

To estimate the demand function in equation (3), we must pool the observations from all months and years, or at least enough month and years to ensure sufficient variation in the tariffs. Our basic regression, which is estimated by instrumenting for price, is based on a sample that excludes observations with consumption we judge to be too low (≤ 31 kWh/month) for a home with normal occupancy or higher than the 99th percentile of the distribution in the sample. Our base specification includes household-specific, region-by-month and region-by-year fixed effects. It also includes log HDD and log CDD, and log household income.

Results are displayed in table 6. In our base model, the price elasticity of demand is (in absolute value) just under 0.5.³⁴ Adding urban area-by-month and urban area-by-year fixed effects leaves this estimate virtually unchanged.^{35, 36}

³⁴ If we do not instrument for price, we obtain, as expected, a positive elasticity of 1.54 (clustered s.e. 0.0054).

³⁵ The price elasticity remains virtually when the sample is restricted to specific groups, such as the bottom or top 20% of the distribution of income in the same, or the households with at least one member with a bachelor's degree or post-grad education, or households without any such person. We also ran quantile regressions with fixed effects (Koenker, 2017) to see if the elasticity changed over the domain of the dependent variable, finding its point estimates to be very stable over quantiles from the 10th to the 90th (but statistically insignificant at the conventional levels for quantiles equal to or less than the 40th).

³⁶ In all of our specification the income elasticity is demand is positive, significant, and very stable around 0.05.

If we stop the sample at January 2016 the price elasticity is estimated to be -0.54. *Starting* the sample at January 2016, however, results in a much less pronounced elasticity (-0.25). We explore the issue of stability of the price elasticity over time in Table 7, where we present the results from fitting the demand function to “rolling windows” of three and four years, respectively.³⁷ Again, the elasticity appears to be stable over time, except for the very last three and four years of the study, when it drops by one-third or even by half.³⁸ The estimates of price elasticity from the first-difference IV estimator are similar to those from the within IV estimator.³⁹

This drop in elasticity may be an econometric artifact attributable to the more limited variation in tariffs in the last few years of our sample period. Some support for this explanation is provided by the fact that, when the sample is restricted only to three months before and three months after each tariff revisions, the price elasticity is estimated at -0.63 (clustered s.e. 0.0085).

Alternatively, less frequent tariff revisions may have reduced the attention that people paid to electricity prices. Some support for this explanation is provided by the fact that most of the online search activity appears to occur just around the time of the tariff revisions (see figures A.2 in the Appendix).

It is also possible that more prosperous financial circumstances may have resulted in less attention to the energy bills: After all, in 2019 real household income was approximately 23% higher than in 2012. But electricity expenditures were 3.7% of household income in 2012, 2.8% in 2013 and 2014 (due to the tariff reductions), and about 3.5% in the 2016-2019 period, suggesting that this is an unlikely explanation.

³⁷ This means that we create subsamples with the observations from 2012 to 2015, 2013 to 2016, etc. (four-year windows) or 2012 to 2014, 2013 to 2015, etc. (three-year windows).

³⁸ We are unable to estimate a meaningful elasticity for the 2012-15 period using our standard specification, as the region-by-time fixed effects absorb all of the variation in the tariffs.

³⁹ The price elasticity from the first-difference estimator is -0.5705 (s.e. 0.0226).

Finally, Telasi, the utility that serves Tbilisi, introduced SMS billing in 2015, followed by EPG in 2017. It is not impossible that salience and attentiveness dropped when the electricity bills became one of the myriad text messages that people tend to receive (and send) every day.

Based on informal conversations with local observers,⁴⁰ we wonder whether the sensitivity to the tariffs may have been numbed by government assistance. Unfortunately, our HBS datasets do not document government assistance broken down into the various types and categories (e.g., energy, housing, education, transportation, etc.). They do however tell us whether the household applied for government assistance and whether they obtained it. Using this information, we can form various subsamples, such as i) households that did not apply for assistance, ii) households that did not apply for assistance or applied but were not denied, and iii) households that applied for assistance and received it.

Table 8 repeats our analyses separately for each of these groups, finding that, controlling for income, those who relied on government assistance exhibited a stronger responsiveness to price changes and have a more elastic demand function, even in 2016-2019, when the other groups appear insensitive to price changes.

E. Expected v. Actual Price

Since residential consumers in Southern California do not exhibit any form of “bunching” around the block cutoffs, Ito (2014) hypothesizes that either they are completely price-inelastic, or they respond to some other price instead of the block marginal price. Two candidates for such alternative price are the expected and the actual average price.

In the Republic of Georgia, we find some evidence of “notches” and of responsiveness to price. Still, it is not impossible that households might be responding to both *expected* and actual

⁴⁰ Levan Pavlenishvili, ISET, personal communication, September 2021.

marginal price, marginal price of course being equal to average price per kWh given the unusual tariff structure in the Republic of Georgia. To explore this possibility, we need to compute a household's *expected* price per kWh for each month.

Since the panel component of our dataset is a relatively short one, with 12 monthly observations per household, we are unable to construct an expectation for consumption and price based on the same month in previous years. As an alternative, we construct expected consumption by assigning to each household the average consumption experienced by all other households with a similarly-sized home and the same number of household members in the same month and tariff period in the same region. This calculation is reasonable if, for example, the size of a home and the size of the family, along with climate and location, are the most important drivers of consumption.⁴¹ Once expected consumption is calculated, expected price follows directly from the tariffs in place at the time.

The log of expected price formed in this fashion is strongly correlated with log actual price (correlation coefficient 0.70). As shown in the first row of table 9, when log expected price replaces log actual price in the demand function, its coefficient is negative and significant, but—at about -0.06—smaller in absolute value than that on log actual price. This “expected price” elasticity is also very stable across the different time periods.

This expected price and the actual price faced by the household (and their logs) are too strongly correlated to be both included in the right-hand side of the demand function regression. However, in the spirit of Shin (1985), we can seek to discriminate between expected and actual price by assuming that i) the price that enters in the demand function is the consumer's “perceived” price, and ii) individuals form a “perceived” price, \hat{P} , as a function of both expected and actual price. Formally,

⁴¹ See, for example, Alberini et al. (2011).

$$(4) \quad \ln E_{it} = \alpha_i'' + W_{it}\beta'' + \gamma'' \cdot \ln \text{Income}_{it} + \lambda \cdot \ln \hat{P}_{it} + \tau_{jt}'' + \eta_{it},$$

and

$$(5) \quad \hat{P} = EP \cdot (P/EP)^\varphi,$$

where P denotes actual price and EP expected price. Coefficient φ is an “adjustment” parameter.

If φ is equal to 1, then the perceived price is the same as the actual price. If it is equal to zero, then the perceived price is equal to the expected price.

On plugging (5) into (4), we obtain

$$(6) \quad \ln E_{it} = \alpha_i'' + W_{it}\beta'' + \gamma'' \cdot \ln \text{Income}_{it} + \lambda \cdot \ln EP_{it} + (\varphi\lambda) \ln \left(\frac{P_{it}}{EP_{it}} \right) + \tau_{jt}'' + \eta_{it}.$$

If $\varphi=1$ the coefficients on log expected price and log θ , where $\theta=P/EP$, should be equal to each other (and to the price elasticity of demand). We can thus let the data tell us whether these two variables have the same coefficients, in which case consumers are responding to the actual price, or whether one of these two components of perceived price has a different (or even zero) weight.

The second row of table 9 indeed shows that both log expected price and log θ have negative and significant coefficients—for the entire study period, until 2016 and after January 2016. Technically speaking, with the full study period a Wald test rejects the null that the two coefficients are equal, but in practice they differ by only about 11%. This suggests to us that households were responding to the actual price, rather than the expected price, during the full study period.

The story however changes somewhat when attention is restricted to the earlier v. the later years. It would appear that in the earlier years, consumers placed a higher weight on the “unexpected” component of price, since the coefficient on log θ is some 30% stronger than that on the log of expected price. The story is reversed in the later years, when the overall price

elasticity is less (in absolute value) and the weight on the log of expected price is some 35% heavier than that on $\log \theta$.

6. Discussion and Conclusions

Whether consumers are able to process complex pricing schemes has been the subject of ongoing debate for decades. Contexts where complex pricing schemes are often observed—and consumers' ability to process them correctly seriously tested—include income taxation (Saez, 2010; Liebman and Zeckhauser, 2004), mobile phone plans (Grubb and Osborne, 2015; Bar-Gill and Stone, 2012), memberships in sports facilities (Della Vigna and Malmendier, 2006), and utilities. Water, electricity and natural gas are often supplied under two-part tariffs—a fixed fee and one that is proportional to consumption—and/or increasing block rate schemes, which are meant to penalize heavy consumption and imply that the marginal price is different from the average price per unit.

While standard economic theory posits that consumers should be responding to marginal block price, evidence that this may not be so dates back to Shin (1985) and has been recently examined in Ito (2014), who, based on data from Southern California, concludes that consumers respond (barely) to the average price.

We have examined data from the Republic of Georgia, where electricity tariffs are set by the regulator every three years, are exogenous to individual consumers, and follow a rather unusual IBR. Instead of paying the higher price for the kWhs that exceed the block cutoff, consumers pay the higher rate on their *entire* consumption volume. This should amplify the incentive to conserve, compared to a standard IBR scheme, as long as the demand is sufficiently price elastic, and should make the task of grasping price easier, since the marginal price and the average price are always the same (although they vary with the level of consumption).

We have used several waves of the Georgia HBS, from 2012 to 2019, plus a panel dataset from residential accounts on the Tbilisi city limits over the same period. In the summer of 2021, we conducted a telephone survey of consumers around the Republic of Georgia. With the caveat that this was 18 months later than the end of our HBS sample period, and during the Covid-19 pandemic, it is still shocking to learn that some 80% of the telephone survey respondents did not know their most recent bill's price per kWh.

Are the HBS and Tbilisi city limits data likewise suggestive of consumers that “do not know the price” and thus presumably “do not respond to it?” We examine the distribution of consumption from these two sources of data, finding that the HBS displays “notches” (with consumers trying to limit consumption below the 101 kWh cutoff). By contrast, the Tbilisi city limit data shows a modest degree of bunching around this cutoff, which vanishes in the second half of our study period. Both findings are in contrast with the behavior predicted in Borenstein (2009) under IBR schemes and with the complete lack of bunching or related patterns documented in Borenstein (2009) and Ito (2014) for Southern California residential customers.

We take advantage of differential tariff reductions in the different blocks, and tariff increases that occur at different times in different regions, which we interpret as natural experiments, and devise difference-in-difference designs based on binary treatments, which we apply to the HBS data. The results show clearly that consumption increases when the tariffs are reduced, increase when they are raised, and that the effect is symmetric, in that in both cases consumers behave *as if* the price elasticity is -0.3.

Our DID designs are agnostic about the functional form of the relationship between electricity consumption and price, and indeed these effects may occur even if consumers do not know exactly know the tariffs—as long as they know that they have changed. Indeed, online

search activity spikes around the time of the tariff changes, suggesting that at a minimum consumers respond to the downward or upward *direction* of the tariff revisions.⁴²

We also estimate a log-log electricity demand function, where we instrument for the price paid using are the tariffs in each block. This exercise indicates that the price elasticity is about -0.5. This price elasticity is generally stable across groups of households and over most subsets of the full study period, but is much less pronounced in the last three or four years. This may be an econometric artifact due to the limited tariff revisions in that period, the result of loss of salience to consumers due to the limited tariff revisions in that period (and to the adoption of SMS text messages to notify customers of the bills), or may follow from changes in the type of price consumers respond to.

Observers have suggested that government assistance may numb the attention that people pay to electricity prices, effectively reducing their salience. We find the opposite: Even controlling for income, household that receive government assistance (of any type, not necessarily just with electricity bills) have a stable, and strong, elasticity during this period, unlike other households.

Alternatively, consumers may be responding to a different price than the marginal/average price. We empirically test this conjecture by positing that it is “perceived” price that enters in the demand function, and that perceived price is function of both actual (marginal/average) price and “expected” price. We empirically test whether perceived priced is

⁴² Carter and Milon (2005) raise the issue of whether consumers actually acquire knowledge of the price they pay for utilities, on the ground that they should do so only if the benefits of acquiring this knowledge exceeds its costs. Using a sample from three Florida utilities where only 6% of the respondents state that they are aware of the marginal price of water, they fit residential water demand functions with endogenous switching between knowing and not knowing the price, finding that while both informed and uninformed household respond to a combination of average and marginal price, informed households have stronger elasticities with respect both marginal and average price. Informed households tended to consume less water than uniformed households anyway, and acquiring information actually appears to increase consumption, possibly because they had overestimated the price of service to begin with.

the same as actual price, finding that it is so for the full 2012-2019 study period, and that “expected” price gains some importance over “actual” price, but both lose ground in the demand function, in 2016-2019.

Whatever the explanation for this apparent drop in price elasticity (a short- or medium-run elasticity), it remains much higher than estimates from recent quasi-experimental studies in the US, which is -0.08 to -0.09 in the short or medium-long run (Ito, 2014; Deryugina et al., 2020) and -0.27 in the long run (Deryugina et al., 2020). It is however similar to that in Ukraine (Alberini et al., 2019).

Some simple calculations suggest that with a price elasticity of this magnitude (-0.5), energy taxes or simple tariff increases would curb the residential electricity demand enough to help improve Georgia’s energy security. In 2018 the Republic of Georgia’s electricity consumption was 12 TWh (IEA, 2020). Of these, 1.52 TWh had to be imported from Russia and/or Azerbaijan. If one assumes that the residential sector was responsible for imports in a manner proportional to its share of total electricity consumption (20.8%), imports to residential customers totaled $(12 \times 0.2081 \times 1.52 / 12) = 0.316$ TWh. A 25% increase in the price of electricity would be sufficient to reduce the demand from the residential sector by 12.5% or 0.312 TWh, enough to offset the imports.⁴³

In sum, we have found evidence that consumers in the Republic of Georgia *do* respond to tariff changes. Our data does not permit us to test whether the relatively strong responsiveness they exhibit is *due* to the unusual, almost “wild” IBR scheme, or whether the authorities chose this scheme *because* they were aware of the consumers’ strong responsiveness. The tendency of

⁴³ A 25% increase in the price of electricity is roughly comparable to the increase in the price of gas-fired electricity if a carbon tax of \$25/ton CO₂ were imposed, assuming about 549 grams/CO₂ per kWh. This would raise the price from \$0.05/kWh to \$0.06137/kWh (2017 PPP \$)—a 27.45% increase. Such an increase would reduce residential demand by 0.343 TWh, and thus more than offset the imports (assumed to be proportional to the residential sector’s share of all electricity consumption).

consumers in the Republic of Georgia to push consumption to below the first block cutoff suggests that many are aware that there is a “bill penalty” if they exceed that cutoff, whether or not they are aware of its exact magnitude. It also raises the possibility that they might be underconfident in their ability to predict consumption and the likelihood that they exceed that cutoff (Grubb and Osborne, 2015; Bar-Gill and Stone, 2012; DellaVigna and Marmandier, 2006), but—unlike in Borenstein (2009)—our relatively short panels do not allow us to formally test this conjecture. We leave that to future research.

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Figure 1. Monthly electricity consumption: HBS data, 2012-2019.

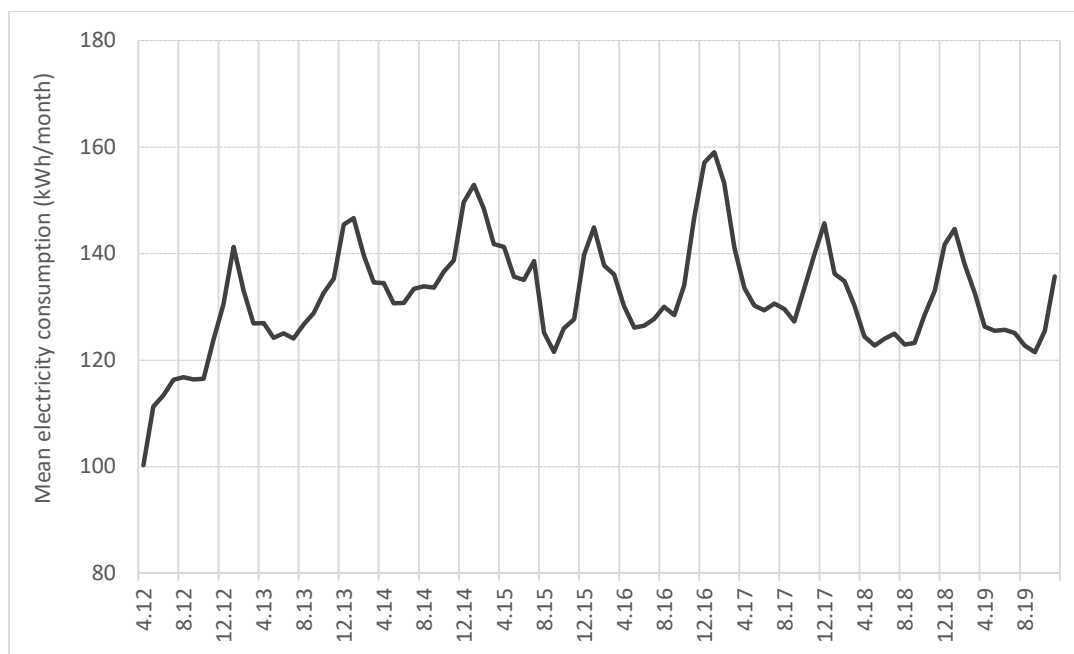


Figure 2. Monthly electricity consumption: Regulator-provided meter readings for dwellings on the Tbilisi city limits, 2012-2019.

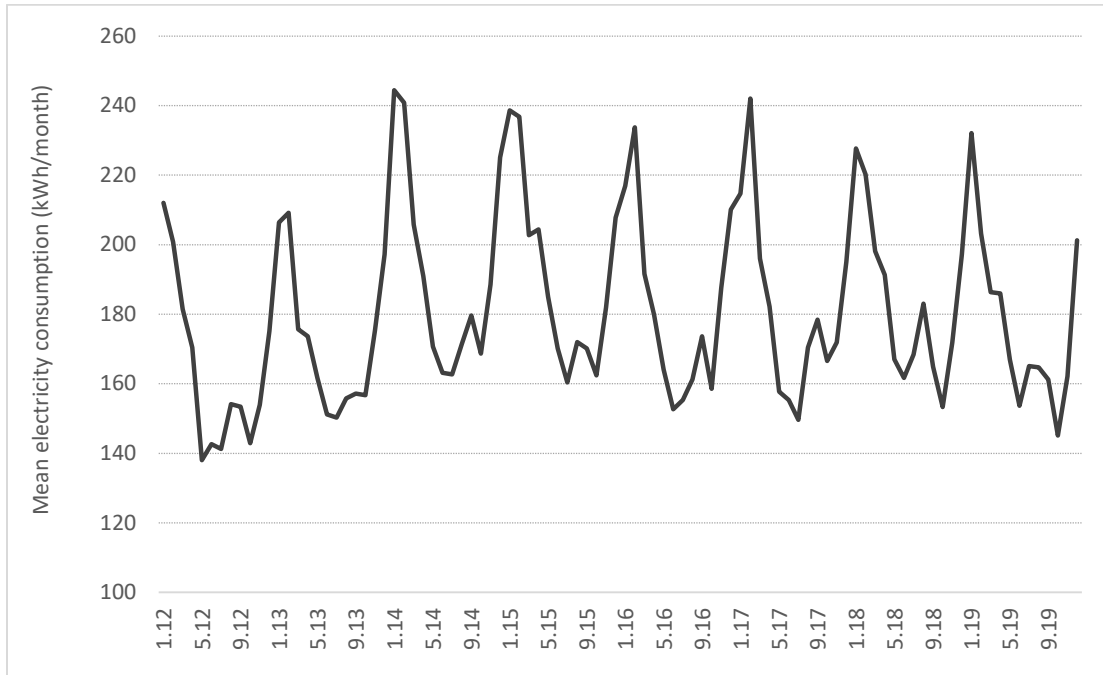


Figure 3. Average of log monthly electricity consumption for the control and treatment groups in the months preceding and following the nationwide reduction of the tariffs in blocks 1 and 2.

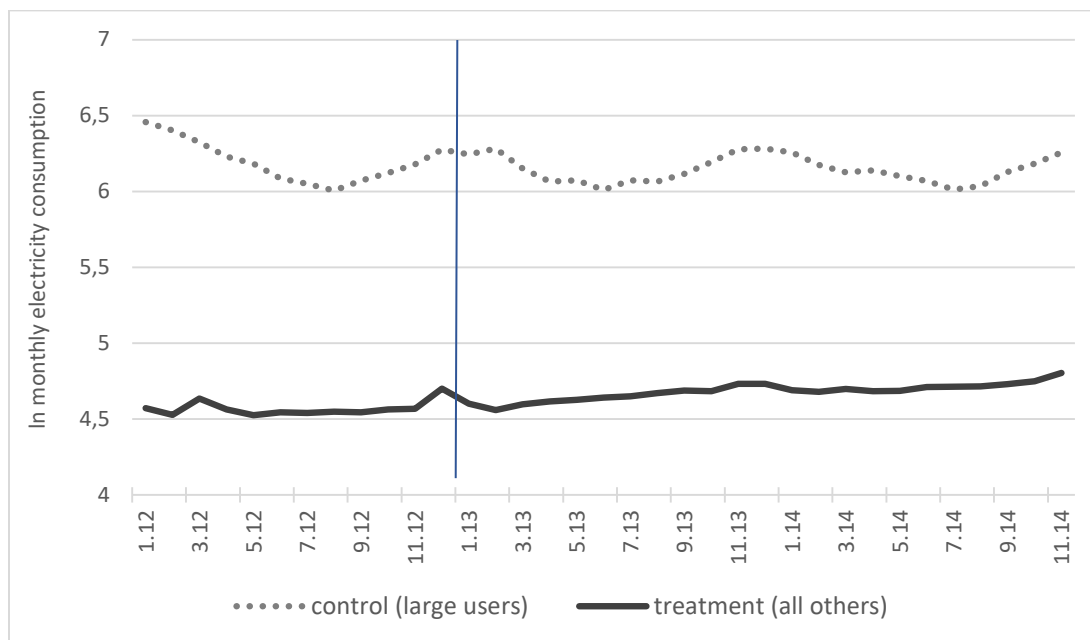
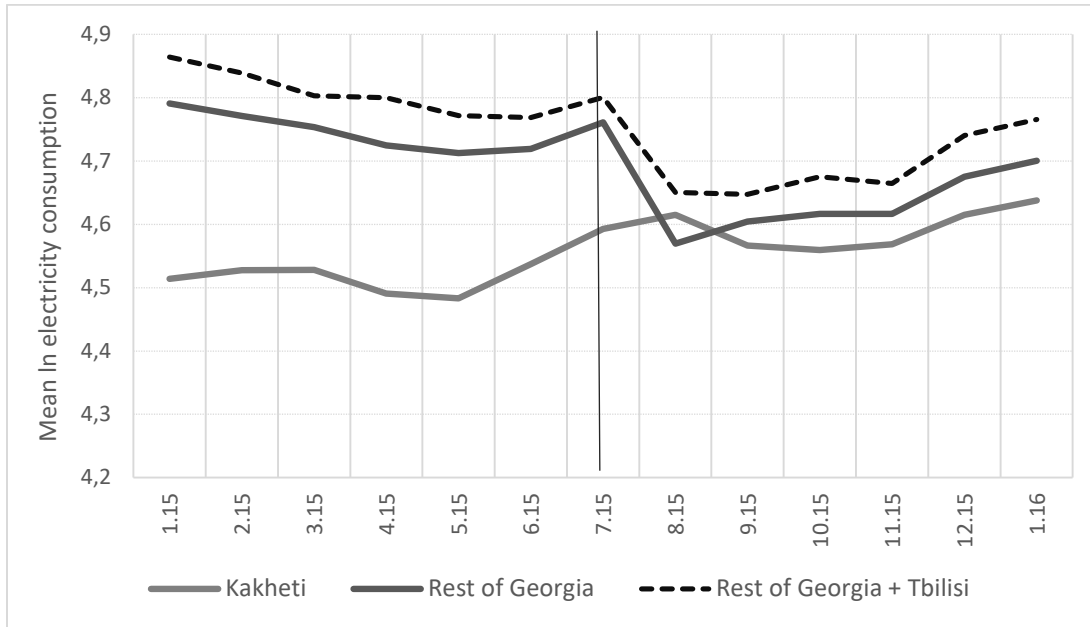


Figure 4. Average of log monthly electricity consumption for the control and treatment groups in the months preceding and following the tariff hikes.

A. January 2015-January 2016



B. January 2014-January 2016

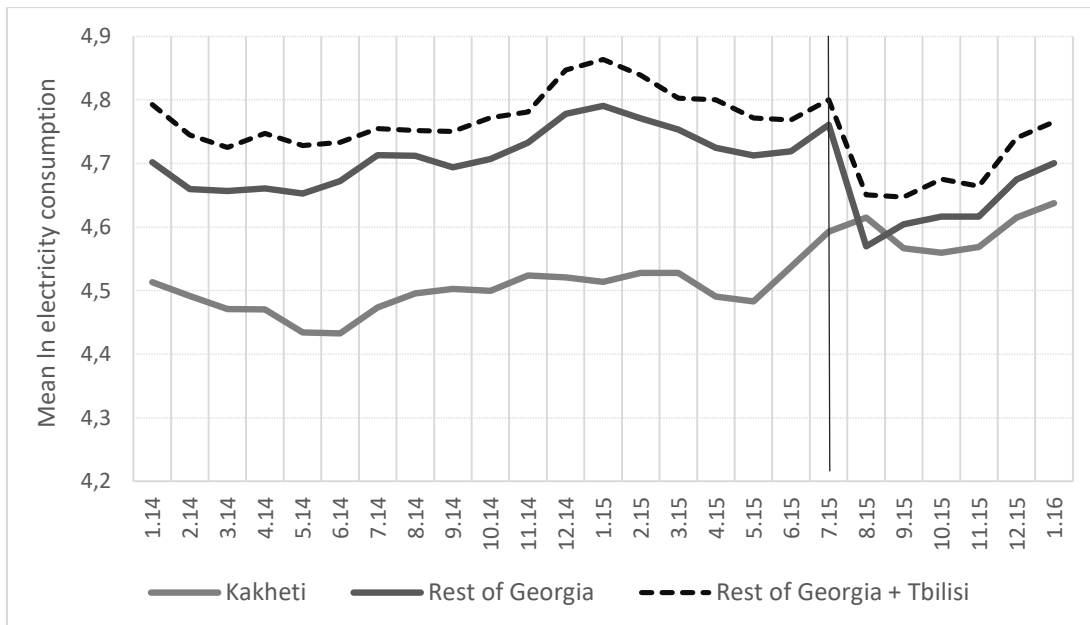
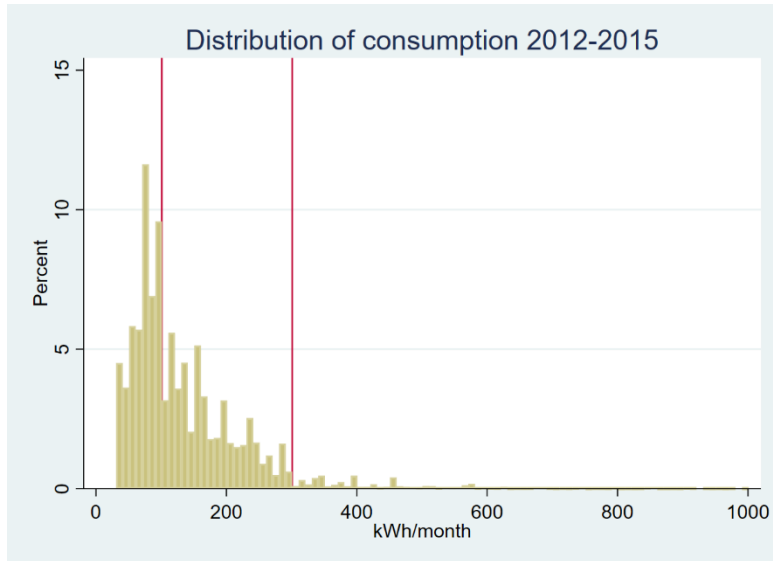
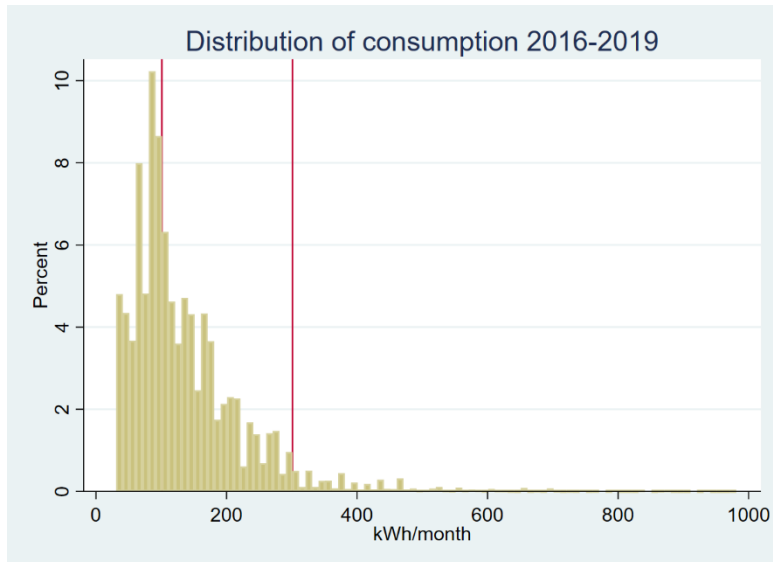


Figure 5. Histograms of monthly electricity consumption from the HBS.

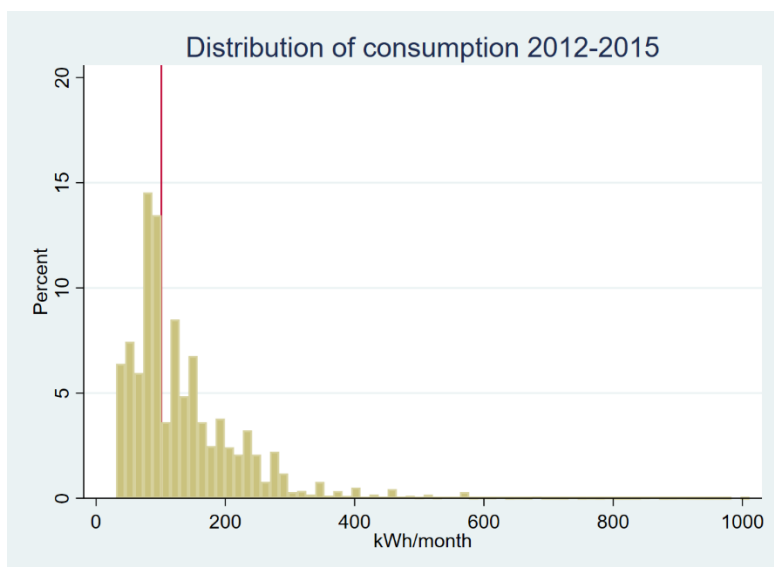
A. 2012-2015, bin width = 10 kWh



B. 2016-2019, bin width = 10 kWh



C. 2012-2015, bin width = 14 kWh



D. 2016-2019, bin width = 14 kWh

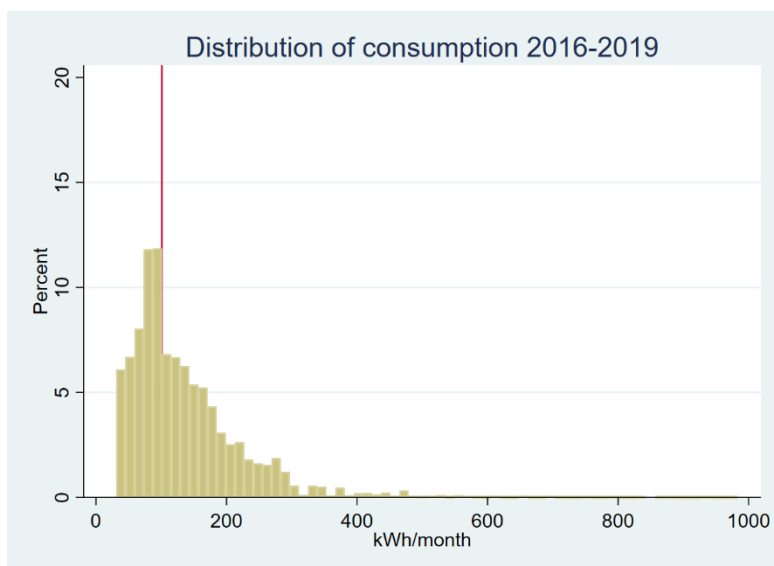
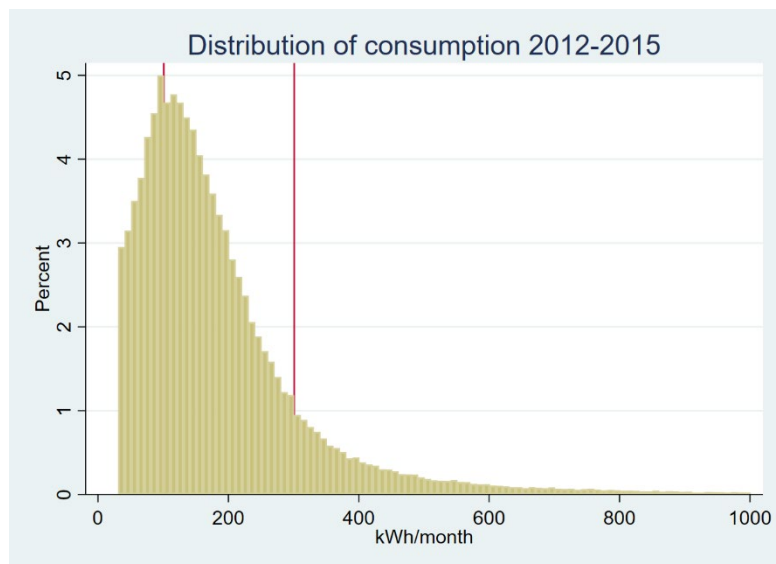


Figure 6. Histograms of monthly electricity consumption from a panel of residential accounts on the Tbilisi city limits. Consumption is exactly measured as per meter reading.

A. 2012-2015



B. 2016-2019

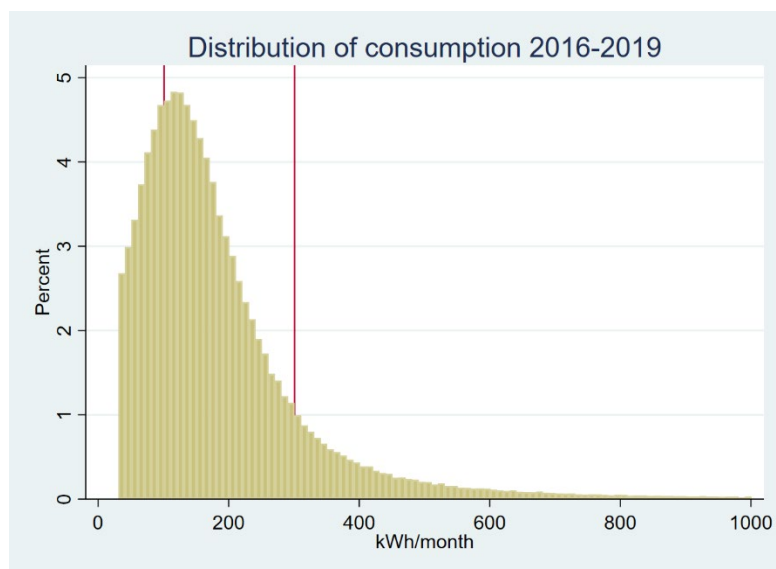


Table 1. Electricity tariffs, in Tetri/kWh

Period	Kakheti			Tbilisi			EnergoPro		
	less than 101	<101, 301>	more than 301	less than 101	<101, 301>	more than 301	less than 101	<101, 301>	more than 301
Oct 2010-Dec 2012	12.980	16.520	17.499	13.476	16.001	17.700	12.980	16.520	17.499
Jan 2013-Mar 2013	9.440	12.980	17.499	9.936	12.461	17.700	9.440	12.980	17.499
Apr 2013-Jul 2015	8.944	12.980	17.499	9.475	12.461	17.700	9.003	12.980	17.499
August 2015	8.944	12.980	17.499	9.475	12.461	17.700	12.956	16.933	21.452
Sep 2015 - Jan 2016	8.944	12.980	17.499	12.980	16.992	21.476	12.956	16.933	21.452
Feb 2016-Aug 2017	12.980	16.520	17.499	12.980	16.992	21.476	12.956	16.933	21.452
Sep 2017-Dec 2017	12.956	16.933	21.452	12.980	16.992	21.476	12.956	16.933	21.452
Jan 2018-Nov 2019	14.243	18.207	22.727	14.549	18.561	23.045	14.243	18.207	22.727

Note: All amounts in nominal Georgia Tetri/kWh and inclusive of 18% VAT. 100 Tetri = 1 Lari \approx 0.34-0.66 USD (2012-2019 with a downward trend)

Table 2. Summary of study designs

	Natural experiment: tariff decrease	Natural experiment: tariff increase	Demand function
Period	Jan 2012-Dec 2014	Jan 2015-Jan 2016	Jan 2012-Nov 2019
Control group	Large consumers (monthly consumption always > 301 kWh)	Kakheti region	n/a
Treatment group	Everyone else	Tbilisi, Rest of Georgia	n/a
Nobs	92,187	34,379	240,900

Table 3. Results from the price decrease natural experiment, DID design.

	(1) Base	(2)	(3)	(4)
ATT	0.0797*** (0.0086)	0.0734*** (0.0086)	0.0738*** (0.0086)	0.0775*** (0.0099)
Log income	No	Yes	Yes	Yes
Household FEs	Yes	Yes	Yes	Yes
Region-by-month FEs	No	No	Yes	Yes
Period	Jan 2012 - Dec 2014	Jan 2012 - Dec 2014	Jan 2012 - Dec 2014	Jan 2012 - Jan 2013
Obs.	92,187	92,187	92,187	29,839

Note: Control group: Large consumers (monthly consumption always > 301 kWh). Treatment group: everyone else. The base specification includes log HDD, log CDD, and month dummies, in addition to household-specific fixed effects. Standard errors (in parentheses) are clustered at the household level. * = significant at the 10% level. ** = significant at the 5% level. *** = significant at the 1% level.

Table 4. Results from the price increase natural experiment DID. Sample period: Jan. 2015-Jan. 2016.

	ATT (parameter δ')	Sample	Specification	N
(1)	-0.0999*** (0.0106)	All	ln(HDD), ln(CDD)	34,379
(2)	-0.0996*** (0.0105)	All	ln(HDD), ln(CDD), ln(Income)	34,356
(3)	-0.0814*** (0.0096)	All regions, only 31<kWh<520	ln(HDD), ln(CDD)	32,925
(4)	-0.0813*** (0.0095)	All regions, only 31<kWh<520	ln(HDD), ln(CDD), ln(Income)	32,905
(5)	-0.1282*** (0.0127)	No Tbilisi	ln(HDD), ln(CDD)	28,616
(6)	-0.1278*** (0.0127)	No Tbilisi	ln(HDD), ln(CDD), ln(Income)	28,593
(7)	-0.1135*** (0.0017)	No Tbilisi, only 31<kWh<520	ln(HDD), ln(CDD)	27,384
(8)	-0.1133*** (0.0117)	No Tbilisi, only 31<kWh<520	ln(HDD), ln(CDD), ln(Income)	27,364

Note: Household fixed effects and month fixed effects always included. Standard error (in parentheses) are clustered at the household level.

* = significant at the 10% level. ** = significant at the 5% level. *** = significant at the 1% level.

Table 5. Results from the price increase natural experiment. Sample period: Jan. 2014-Jan. 2016.

Type of time fixed effects		ATT	ATT if 31<kWh<520
(1)	Month dummies	-0.1153*** (0.0081)	-0.1007*** (0.0073)
(2)	Month dummies, year dummies	-0.1029*** (0.0089)	-0.0837*** (0.0080)
(3)	Month-by-year dummies	-0.0574*** (0.0144)	-0.0042*** (0.0135)
(4)	Month-by-year dummies, Includes log income	-0.0556*** (0.0144)	-0.0426*** (0.0134)
(5)	Month dummies, linear time trend	-0.1033*** (0.0089)	-0.0872*** (0.0080)
(6)	Region-by-month, year dummies	-0.1195*** (0.0097)	-0.1032*** (0.0086)
(7)	Region-by-month, region-by-year	-0.1222*** (0.0098)	-0.1055*** (0.0086)

Note: Standard errors (in parentheses) clustered at the household level. Household fixed effects always included.

* = significant at the 10% level. ** = significant at the 5% level. *** = significant at the 1% level.

Table 6. Demand function estimation results.

	(1) Base specification	(2)	(3)	(4)
Price elasticity of demand	-0.4998*** (0.0265)	-0.5057*** (0.0265)	-0.5422*** (0.0310)	-0.2526*** (0.0758)
Fixed effects				
Household	yes	yes	yes	yes
Region-by-mo.	yes	yes	yes	yes
Region-by-year	yes	yes	yes	yes
Urban-by-month		yes		
Urban-by-year		yes		
Period	Jan 2012 - Nov 2019	Jan 2012 - Nov 2019	Jan 2012 - Jan 2016	Jan 2016 - Nov 2019
N	240,900	240,900	122,000	122,000

Note: Logarithm of HDD and logarithm of CDD always included in the model. The sample excludes observations with ≤ 31 kWh/month and ≥ 520 kWh/month. Standard errors (in parentheses) clustered at the household level.

* = significant at the 10% level. ** = significant at the 5% level. *** = significant at the 1% level.

Table 7. Demand function estimation results: Separate periods.

<u>A. Four-year rolling window</u>	
	Price elasticity
2012-2015	-0.5510*** (0.0312)
2013-2016	-0.5232*** (0.0295)
2014-2017	-0.5003*** (0.0289)
2015-2018	-0.5351*** (0.0290)
2016-2019	-0.2293*** (0.0751)
<u>B. Three-year rolling window</u>	
	Price elasticity
2012-2014	n/a
2013-2015	-0.5244*** (0.0327)
2014-2016	-0.5174*** (0.0313)
2015-2017	-0.5199*** (0.0306)
2016-2018	-0.3302*** (0.0819)
2017-2019	n/a

Note: The sample excludes observations with ≤ 31 kWh/month and ≥ 520 kWh/month. The model includes household-specific, region-by-month and region-by-year fixed effects. Standard errors (in parentheses) are clustered at the household level.

* = significant at the 10% level. ** = significant at the 5% level. *** = significant at the 1% level.

Table 8. Effect of tariff decrease/increase and demand function: Additional Robustness Checks.

A. ATT (coefficients δ and δ')

What	Did not apply for assistance	Did not apply for assistance or did not get it	Applied for and received assistance
Price increase DID (Jan 2015-Jan 2016)	-0.1093*** (0.0134) N=18,569	-0.0951*** (0.0111) N=29,244	-0.1337*** (0.0312) N=5,135
Price decrease DID	0.0459*** (0.0121) N=46,543	0.0649*** (0.0095) N=74,540	0.1113*** (0.0120) N=45,644

B. Demand function (price elasticity $\hat{\lambda}$)

What	Did not apply for assistance	Did not apply for assistance or did not get it	Applied for and received assistance
Entire study period	-0.5443*** (0.0378) N=133,108	-0.4973*** (0.0291) N=206,315	-0.4546*** (0.0368) N=107,792
On and after January 2016	-0.1045 (0.1162) N=71,122	-0.1882** (0.0847) N=107,802	-0.4084*** (0.0993) N=50,789

Note: Base specification of each model, standard errors (in parentheses) clustered at the household level.
 * = significant at the 10% level. ** = significant at the 5% level. *** = significant at the 1% level.

Table 9. Demand function – expected vs. actual price

Demand function specification	All periods	Until Jan 2016 (included)	From Jan 2016 (included)
log expected price (instead of log price, within estimator)	-0.0687*** (0.0130)	-0.0622*** (0.0168)	-0.0634*** (0.0211)
log expected price	-0.5002*** (0.0292)	-0.5186*** (0.0407)	-0.2395*** (0.0818)
log θ (where $\theta=(1+\Delta/\text{expected}$ price, within IV estimator, as θ is endogenous)	-0.5692*** (0.0323)	-0.6783*** (0.0347)	-0.1765*** (0.0808)
Wald test that the two coefficients are equal	17.58 ($p<0.0001$)	47.24 ($p<0.0001$)	8.23 ($p=0.0041$)

Note: The sample excludes observations with ≤ 31 kWh/month and ≥ 520 kWh/month. The model includes household-specific, region-by-month and region-by-year fixed effects, log HDD, Log CDD and log household income. Standard errors (in parentheses) clustered at the household level. The Wald test uses the clustered variance-covariance matrix of the estimates.

* = significant at the 10% level. ** = significant at the 5% level. *** = significant at the 1% level.

Appendix

Table A.1 Comparison between alternate calculations of electricity consumption in kWh/month, Jan 2012 – Nov 2019


	Valid obs.	Mean	p10	p25	Median	p75	p90	p95
Method 1	228,270	124.29	34	59	100	157	235	300
Method 2	264,420	123.24	39	63	93	154	236	286
Method 3	264,420	125.35	39	63	100	154	241	308

Note: We use the tariffs in place at the end of 2019 to illustrate our calculations. Under method 1, consumption in the first block must be billed up to 14.38 GEL, in block 2 between 18.57 and 54.80 GEL, and in block 3 68.63 GEL or more. We divide the reported expenditure by the appropriate block rate to obtain an estimate of the kWh. When one consumer reports a figure that falls outside of the above listed intervals (for example, 17 GEL), we simply refrain from calculating the kWh for this consumer. For the study period starting in January 2012, this method results in 228,270 valid observations and 36,150 missing observations for kWh. Method 2 is similar, but handles the “uncertain cases” by placing in the second block (and hence applies the second block rate) anyone with bill greater than 14.38 GEL, and in the third block anyone with bill greater than 54.80 GEL. Method 3 is similar to method 2, but attributes the “uncertain cases” depending on the difference between the reported amount and the amounts at the cusp between blocks. For example, if the expenditure is 17 GEL, method 3 assumes that this customer fell in the second block because $\text{abs}(17.00-18.57) < (17.00-14.38)$. Method 2 and 3 result in no missing values for consumption.

Table A.2 Descriptive statistics for household-specific variables, HBS 2012-2019

Variable	Mean	Std. Dev.	Min	Max
Income, constant 2010				
Lari per month	735.107	768.105	0.753	27133.240
Missing income info.	0.003	0.055	0	1
Household size	3.449	1.879	1	15
No. workers	2.072	1.438	0	10
No. retired	0.700	0.751	0	4
Size of the home (m ²)	88.126	46.688	8	400
Kakheti	0.103	0.304	0	1
Tbilisi	0.155	0.362	0	1
Rest of Georgia	0.742	0.438	0	1
At least one person with university degree	0.429	0.495	0	1

Figure A.1. Energo Pro bill displayed on line when the customer opens the link sent by SMS.



კეთილმოგობის ცენტრი
[1] 302-260 111 მისამართი: ქ. ვაკე, მ. შ.ა. 250 ვაკე

სერვის ცენტრი: წყალტუბო
1310208090 დურმიმხანნი ფულურიბე
პირდაპირი მისამართი მუხიანი

მობილური: 6889335
ტარიფი: 17.731/21.707/26.227

თარიღი	ჩვენების ტიპი	ჩვენება	კოეფ.	კვტხი	თანხა
29.12.20	ჩვენება	22,752.00			
28.01.21	ჩვენება	22,890.00	1	138.00	29.96

ტარიფის ცვლილება: -0.48
200კვტ სუბსიდი: -29.48

მიმდინარე დავალიანება: 0.00
წინა კვირის დავალიანება: -0.05
გადახდილი თანხა: 0.00
სულ გადასახადი: -0.05

დავალიანების თანხის გადახდის ბოლო ვადა: 13.02.2021

დავალიანების გადაუხდელობის შემთხვევაში შეიქმნება ელ-ენერჯის მიწოდება.

მომხმარებელთა საყურადღებოდ!

საქართველოს ენერჯეტისი და წყალმომარაგების მარეგულირებელი ეროვნული კომისია (შემდგომში – "მარეგულირებელი კომისია") რეკომენდაციას გაძლევთ მიაწოდოთ მომსახურე საწარმოს მობილური ტელეფონის ნომერი ან/და ელექტრონული ფოსტის მისამართი, რათა ისარგებლოთ დარიცხული თანხის, გადახდის ბოლო ვადის, გათიშვის ზუსტი მიზეზისა და მომარაგების აღდგენის სავარაუდო დროის, ასევე განაცხადის რეგისტრაციის ნომრისა და თარიღის, საპასუხო წერილის გაგზავნის შესახებ ტექსტური შეტყობინების სერვისით.

მომსახურე საწარმოს ან მისი თანამშრომლის მიერ მომხმარებლისთვის რაიმე სახის მოთხოვნის წყვეტების შემთხვევაში მომხმარებელს უფლება აქვს მოითხოვოს ამ მოთხოვნის სამართლებრივი საფუძველი (შესაბამისი აქტის, მუხლის ან/და პუნქტის მითითება). მომსახურე საწარმოს თანამშრომლები ვალდებული არიან მომხმარებელს მიაწოდონ სრული ინფორმაცია ელექტროენერჯით მომარაგების მომსახურებასთან დაკავშირებით, განუმარტონ მომხმარებლისა და საკუთარი უფლება-მოვალეობანი.

იმ შემთხვევაში, თუ მომსახურე საწარმოს თანამშრომლები არღვევენ თავიანთ მოვალეობებს და არ რეაგირებენ თქვენს საჩივარზე, შეგიძლიათ მიმართოთ მომსახურე საწარმოს საჩივრებზე რეაგირების სამსახურს, მარეგულირებელ კომისიას მისამართზე:

ქ.თბილისი, ა.მიცკევიჩის ქუჩა #19
ვებ-გვერდი: <http://www.gnrc.org>
ელ-ფოსტა: mail@gnrc.org
ტელ: 16216

ან მარეგულირებელ კომისიასთან არსებულ მომხმარებელთა ინტერესების საზოგადოებრივ დამცველს მისამართზე:

ქ.თბილისი, ა. მიცკევიჩის ქუჩა #19
ტელ: (032) 2420190
ვებ-გვერდი: <http://www.pdci.ge>
ელ-ფოსტა: mail@pdci.ge
სს "ენერჯო-პრო ჯორჯიას"
ცხელი ხაზი: 032 2471707
ვებ-გვერდი: www.energo-pro.ge
ელ-ფოსტა: customers@energo-pro.ge

დარიცხული თანხის გადახდა დამატებითი საფასურის გადახდის გარეშე შესაძლებელია:

სს "ენერჯო-პრო ჯორჯიას" მომსახურების ცენტრებში განთავსებულ სს "ნოვა ტექნოლოჯის" სწრაფი გადახდის აპარატებში (PAY BOX).

შენიშვნა: ეტაპობრივად მოხდება სწრაფი გადახდის აპარატების დამატება

კომპანია გაფრთხილებთ!

ელექტროენერჯის დატაცება ან მისი მოხმარება აღრიცხვის გარეშე ან აღრიცხვის წესების დარღვევით ისევე ადმინისტრაციული დაუცველი ოჯახების მონაცემთა ერთიან ბაზაში და მონიჭებული სარეკორდო ქულა კოლა ან ნაკლები 70 ათასზე. ელექტროენერჯის ტარიფის ზრდის შედეგად დატარდება 100 ლარით, იურიდიული პირი და დაწესებულება 15000 ლარით).

ელექტროენერჯის საფასურის გადაუხდელობის, ელექტროენერჯის უკანონოდ მოხმარების ან/და უსაფრთხოების წესების დარღვევის გამო ქსელიდან გათიშული მომხმარებლის ელექტროენერჯის მომარაგების აღდგენის საფასური შეადგენს 2 ლ.

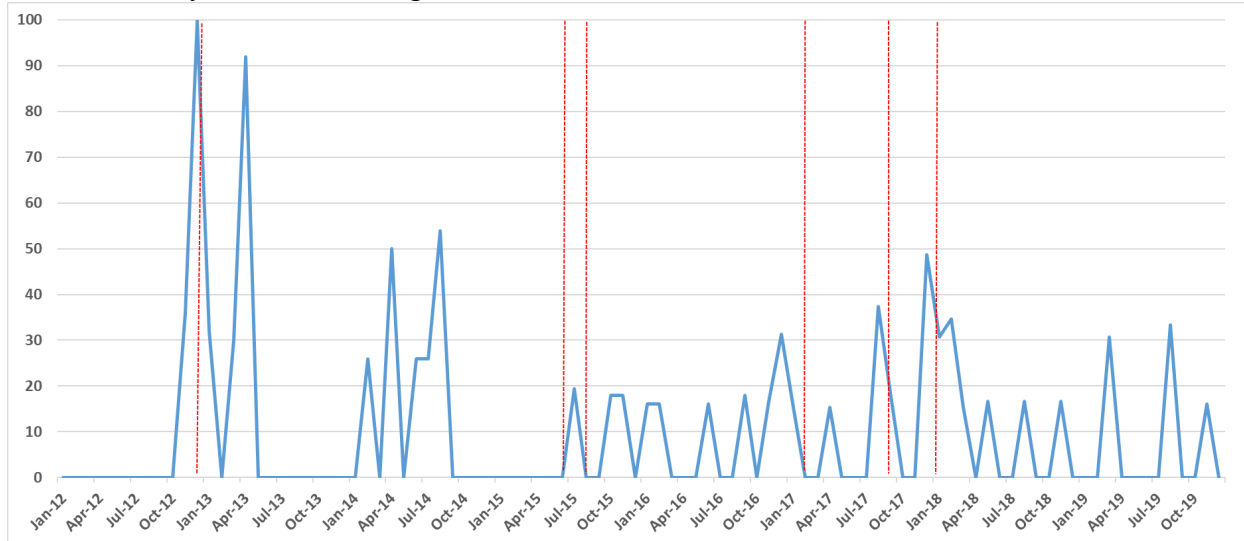
სს "ენერჯო-პრო ჯორჯიას" თანამშრომლებს არ აქვთ უფლება მოახდინონ ელექტროენერჯის საფასურის, ან სხვა სახდლის მიღება.

სამანკო რეკვიზიტები

1. თიბისი ბანკი: GE12თB115336030100001
2. საქართველოს ბანკი: GE40BG000000152516100

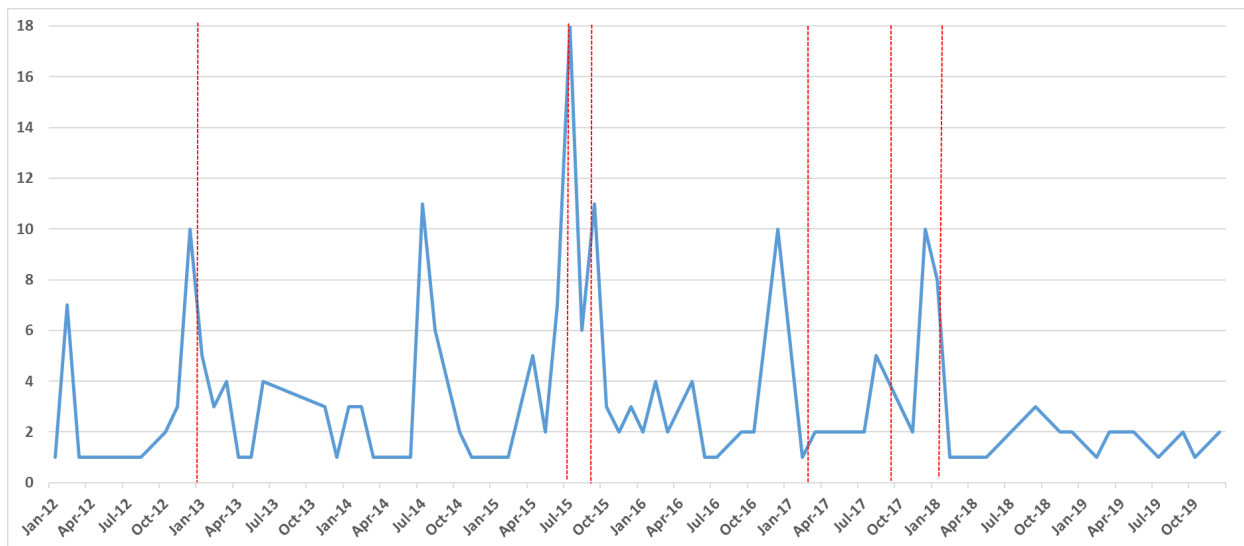
Figure A.2. Evidence of online searches about electricity tariffs in Georgia.

A. Google Trends Index of Relative Search Interest for 4 different alternate spellings of “electricity tariffs” in Georgian.



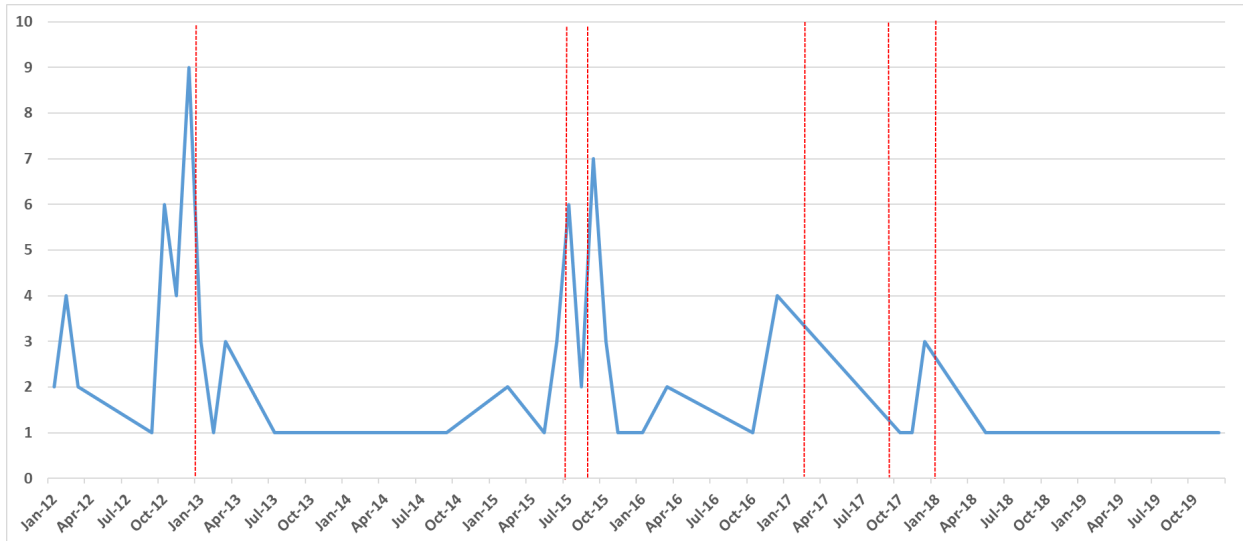
Note: The red dashed vertical lines represent the tariff change periods.

B. Number of articles published in Interpressnews, an online news service, about “electricity tariffs.”



Note: The red dashed vertical lines represent the tariff change periods.

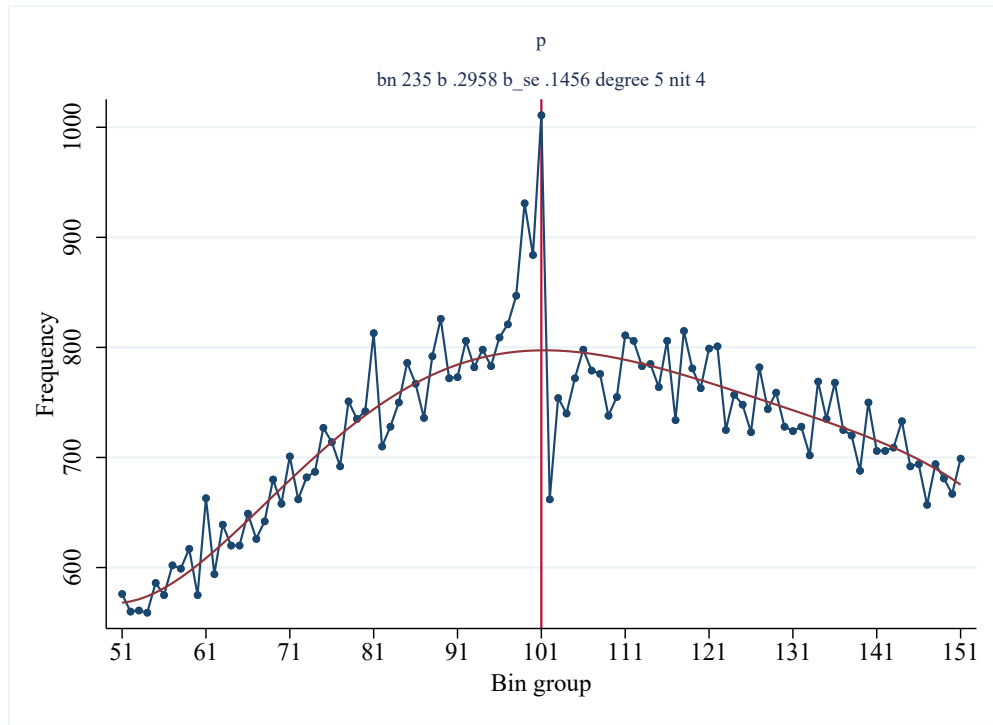
C. Number of articles published in Ambebi, an online news service, about “electricity tariffs.”



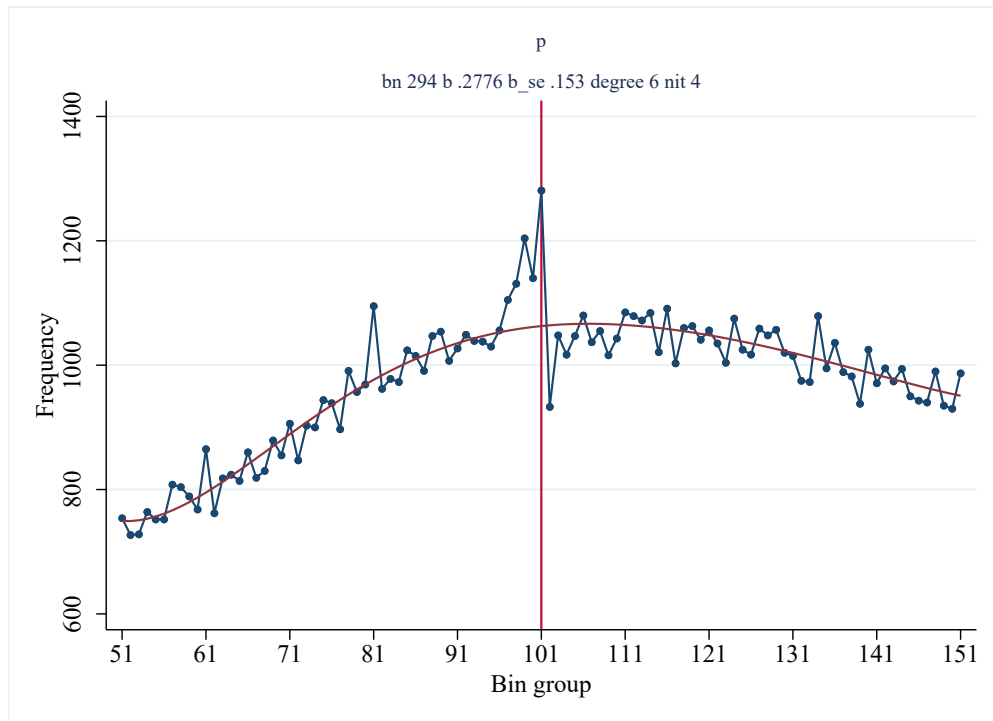
Note: The red dashed vertical lines represent the tariff change periods.

Figure A.3. Statistical test of bunching in the Tbilisi city limits data.

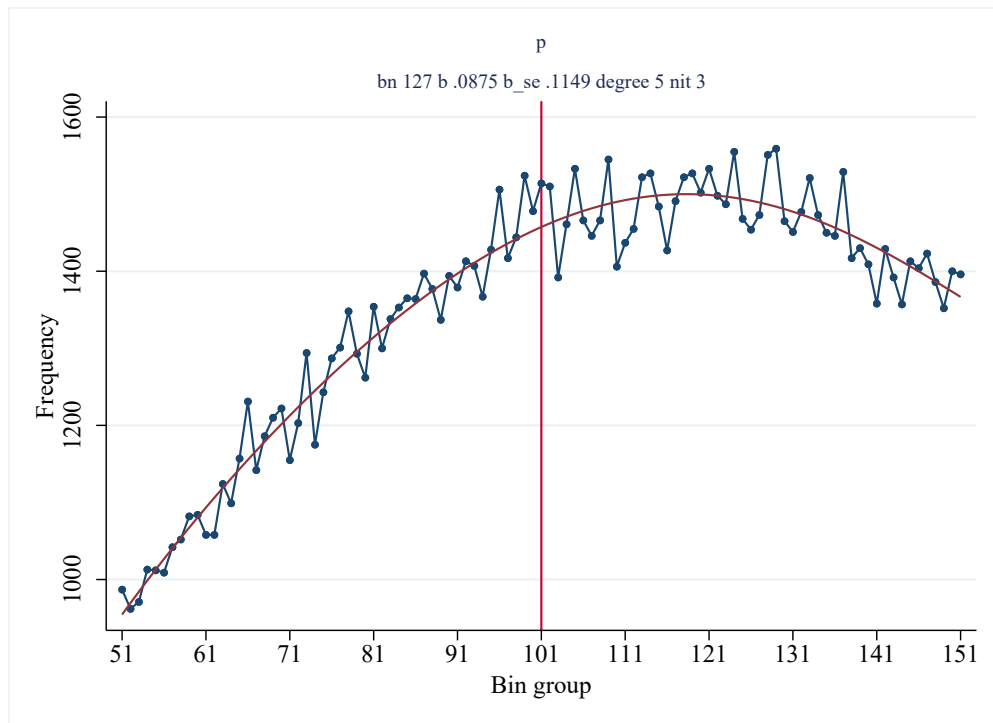
A. 2012-2014.



B. 2012-2015.



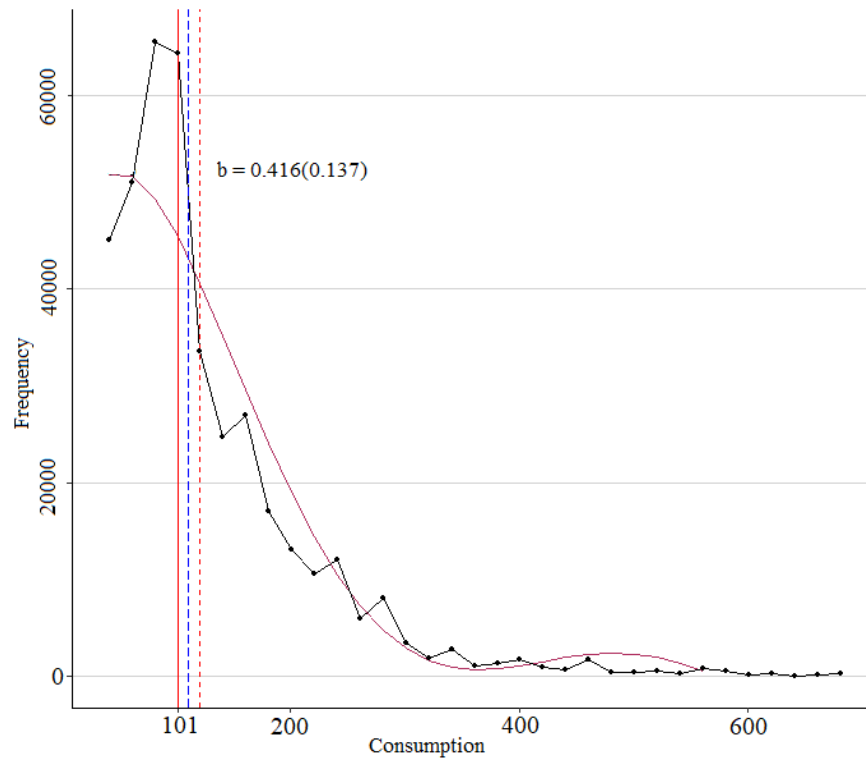
C. 2016-2019.



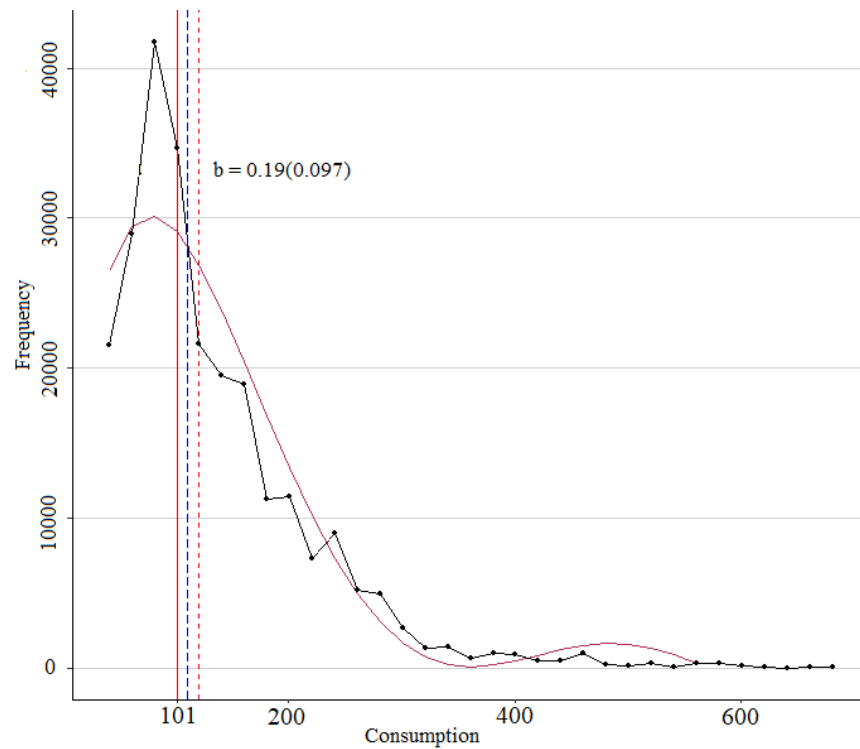
Note: These three graphs present the results from a formal test of bunching around the 101 kWh/month cutoff using the Tbilisi city limits electricity consumption data. “b” is the increase in the actual frequency of the observations with respect to the frequency predicted by a smooth polynomial of order n (5 or 6 in these graphs), so 0.2776 means at 27.75% increase. The standard error of “b” is also displayed in each graph. See <http://www.rajchetty.com/utilities/>.

Figure A.4. Statistical tests of the presence of “notches” at 101 kWh/month.

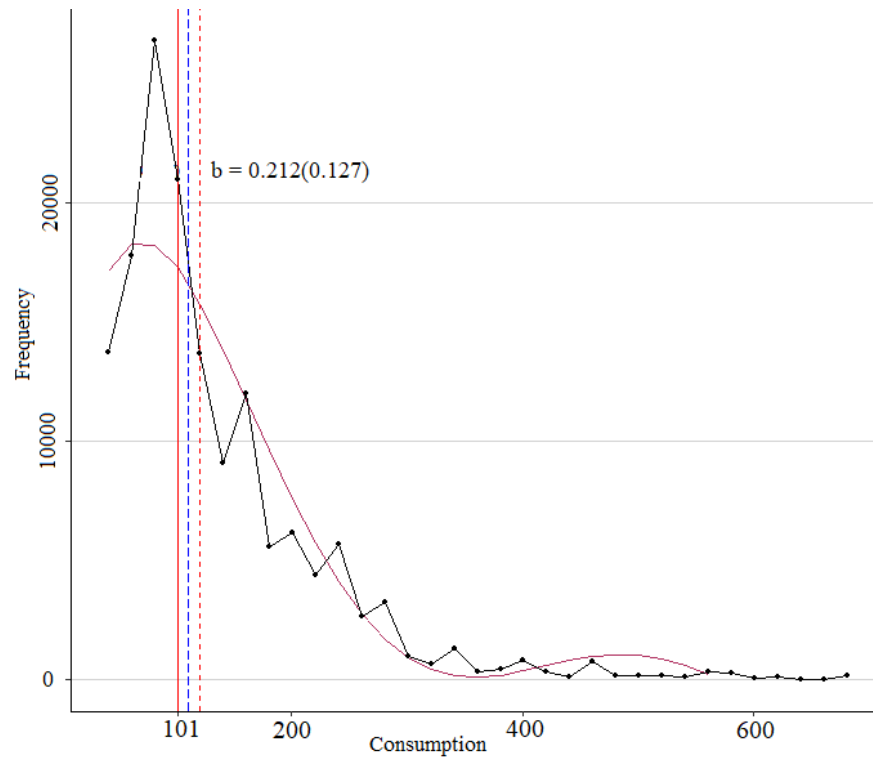
A. 2008-2019.



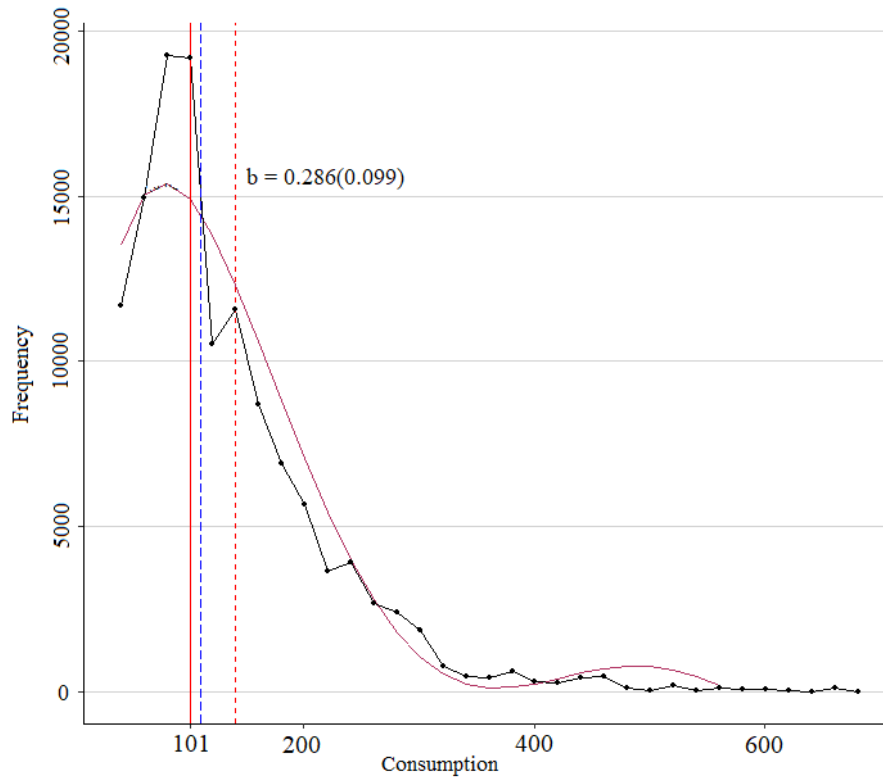
B. 2012-2019.



C. 2011-2015.



D. 2016-2019.



Note: these graphs display the results of formal tests of notches at 101 kWh/month using the Mavrokonstantis (2019) R code. “b” is the increase in the actual frequency of the observations with respect to the frequency predicted by a smooth polynomial of order 5, so 0.286 means at 28.6% increase. The standard error of “b” is also displayed in each graph.

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