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Power Play: Balancing Efficiency and Protection in Fixed vs. Variable Electricity Pricing

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Abstract

We study how households respond to being defaulted into an indexed-price electricity contract after a period under fixed prices, using high-frequency data from a major Italian utility during the 2021–2023 energy crisis. Quasi-random timing of the default generates large, unanticipated price shocks. Defaulting increases bills by up to 56 percent, while consumption declines slowly, by up to 9.8 percent after eighteen months, implying low expenditure elasticity. Welfare analysis shows that fixed-price contracts trade efficiency for protection: they distort consumption and generate deadweight losses, yet provide implicit subsidies that are regressive in absolute terms but progressive relative to income, highlighting central equity–efficiency trade-offs.

Keywords: Energy Economics, Electricity Markets, Price Elasticity of Demand, Consumer Behavior, Fixed-price Contracts, Welfare Analysis.

JEL Codes: Q52, Q58, Q41, D12, H23

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1 Introduction

Regulating commodity prices at times of high market volatility is a common policy recipe to protect consumers and firms, and to guarantee minimum levels of consumption. Fixed pricing schemes are also offered by firms to risk-averse consumers. Beside shielding consumers against market fluctuations, these contracts effectively redistribute welfare between households and retailers, acting as implicit subsidies to the former when market prices are high, and as implicit taxes when they are low. However, fixed-pricing schemes introduce distortions relative to retail prices that reflect the time-varying market demand and supply.

The energy market is a prime example of the trade-off between economic efficiency and consumer protection implied by the choice between fixed and indexed-price regimes. Variable price schemes are more efficient than fixed-price ones, as they align private and social marginal costs of energy (Borenstein 2005, Borenstein & Holland 2003). They also have positive environmental consequences, as they foster energy conservation when prices are high, as shown by studies on demand-response programs (Jessoe & Rapson 2014, Wolak 2011, Allcott 2011, Faruqui et al. 2010, Holland & Mansur 2008, Reguant et al. 2025). However, the energy crisis that hit Europe in 2022, leading to unprecedented increases in gas prices and in the electricity bills paid by households and firms, has made apparent the need for measures to protect consumers and firms from energy price shocks and to guarantee minimum levels of energy consumption (Fabra 2023). In Europe, a cornerstone of the EU’s 2022 proposed energy market reform was the enhancement of consumer protection and the fight against energy poverty through the broader availability of fixed-price retail contracts.¹ Energy markets’ increasing reliance on renewables contributes to exacerbating price volatility (International Energy Agency 2024). Fixed pricing is thus part of a long-term strategy to promote stability in a volatile energy system, and may be instrumental to guarantee public support for the energy transition, by reducing the adverse distributional impacts of decarbonization policies.

In this paper, we leverage rich administrative data from a major Italian energy utility to investigate households’ response to the transition from fixed- to variable-price electricity contracts across different margins and quantify the welfare implications for the different income groups. We focus on the period between January 2021 and March 2023, which captures all phases of the EU energy crisis—from the initial price increases in 2021, through Russia’s invasion of Ukraine in early 2022, to the subsequent record-high prices later that year. All households in our sample subscribed to a fixed-term, fixed-price contract, guaranteeing a fixed electricity price for up to 36 months since the contract start date. The duration of the fixed-price period depended on the specific contract terms. Upon its expiration, customers were automatically transitioned—henceforth (*defaulted*)—to a contract that tied the daily retail price of electricity to the wholesale price (National Single Price, PUN). The default to variable prices was clearly stated in the initial contract, but not made salient to consumers before it took place. The new contract conditions took effect on the first day of the month following the end of the fixed-price period. In our analysis, we exploit the quasi-random transition of households from fixed to variable prices at different times within our observation period. We therefore implement a difference-in-difference estimator with staggered treatment adoption, where the treatment is represented by the contract default. We account for cohort-time heterogeneity in treatment effect using the estimator proposed by Callaway & Sant’Anna (2021). Our identification strategy relies on the assumption that the timing of the energy market shock was unanticipated when the initial fixed-price contracts were

¹European Commission. (2023). *Proposal for a Regulation of the European Parliament and of the Council amending Regulations (EU) 2019/943 and (EU) 2019/942 as well as Directives (EU) 2018/2001 and (EU) 2019/944 to improve the Union’s electricity market design* (COM(2023) 148 final). Retrieved from https://ec.europa.eu/info/law/better-regulation/have-your-say/initiatives/13668-Electricity-market-reform-of-the-EUs-electricity-market-design_en

signed.

In this setting, we first quantify the impact of the contract default on households' electricity bills and consumption. In addition, our data allows us to examine other forms of adjustments to the price shock, such as requests for installment plans or unpaid bills and on contract switch. We then leverage the impact of the default on electricity bills to estimate the expenditure elasticity of consumption in an instrumental variable framework. Specifically, we exploit the exogenous price shocks caused by the default to overcome the endogeneity concerns affecting the estimation of elasticities, stemming from simultaneity, omitted variable bias and measurement error. Finally, using a welfare model and our estimates of the impact of the change in the pricing regime, we compute the implicit subsidies and deadweight losses entailed by fixed-price contracts across income groups. To this end, we employ the non-parametric estimator proposed by Reguant et al. (2025), which enables the inference of household income in the absence of direct economic data.

We find that households that defaulted into indexed-price contracts experience remarkable bill increases compared to those remaining under fixed-price contracts. Such an increase ranges from around 20% (~ 10 Euro/month) in the first bill to up to 56% (~ 30 Euro/month) from the third bill after the default. This effect on bills is consistent with the sharp rise in wholesale electricity prices that characterizes our study period. Households react to these bill shocks by curbing consumption, even though the adjustment in consumption is slower and more gradual than the increase in prices. Specifically, the consumption of treated households decreases by up to 5.5% in the first semester after the default, 7.6% in the second semester, and 9.8% in the third, compared to the control group. While consumption declines substantially, the response remains small relative to the large increase in energy spending, resulting in a relatively inelastic electricity demand, i.e., an estimated expenditure elasticity of -0.113. The delayed adjustment in consumption suggests that it may take time for households to attend to and adapt to the bill shocks, and that households' ability to adapt may be limited. We also detect evidence of financial distress following the default. Up to 1.8% of households resort to installment plans, while 0.6% miss bill payments. Our estimates are robust to potential attrition driven by households exiting the sample and anticipatory adjustments in energy consumption ahead of the default. These results align with previous findings. The short-term (up to one year) price elasticity of electricity demand computed by a meta-analysis of more than 400 studies is around -0.126 (Labandeira et al. 2017). Moreover, the continued decline in consumption up to one and a half years after the default supports the documented discrepancies between short-run and long-run consumption dynamics (Allcott & Rogers 2014, Ito 2015, Deryugina et al. 2020).

In terms of distributional impacts, the largest reductions in electricity consumption following the default are observed among households in the first, fourth, and fifth income quintiles. Households belonging to these quintiles reduce consumption by about 4 kWh (around 13%) per week relative to those remaining under fixed-price contracts. By contrast, middle-income households — particularly those in the third quintile — appear relatively unresponsive to changes in electricity prices. We find that the implicit subsidy received by households from retailers is regressive in absolute terms but progressive in relative ones. Households in the top and bottom quintiles receive around 7.1 and 5.4 Euro per week, respectively, accounting for about 1.1% and 1.8% of their income. Regarding the efficiency losses, results indicate that the distortion in consumption is a concave function of income. The deadweight loss amounts to approximately 6.3 % and 7% of the total subsidy for low- and high-income households, while it is negligible for the middle-income group. Consistent with these patterns, environmental externalities in terms of CO₂ emissions from electricity consumption are largest for households in the bottom and top income quintiles, amounting to societal losses of 20.39 and 22.87 Euro, respectively.

Our paper contributes to the literature that studies the efficiency and distributional effects of different pricing regimes and policies. While the implications of dynamic prices have been largely studied (Faruqui et al. 2010, Borenstein 2007, 2012, Wolak 2011, Ito et al. 2018, Jessoe & Rapson 2014), the effect of a shift from time-invariant to time-varying pricing is scant. Reguant et al. (2025) examines the distributional consequences of transitioning from monthly or yearly flat tariffs to time-varying electricity pricing. They find negligible distributional effects for Real-Time Pricing and progressive effects for Time-of-Use. Leslie et al. (2024) highlights how fixed-price tariffs entail regressive cross-subsidies between consumers, as renewables are increasingly adopted. More generally, the literature stresses the efficiency and distributional trade-offs of energy policies, such as taxes and subsidies (Drupp et al. 2025, Fetzner et al. 2024, Hahn & Metcalfe 2021, Pizer & Sexton 2019). Fixed-price retail regimes can protect consumers against price shocks and are therefore often discussed as consumer protection policies. Importantly, the degree of protection they provide depends on households’ vulnerability, which is closely related to income. We contribute to this literature by quantifying the implicit subsidy and deadweight losses generated by fixed-price regimes during periods of high price variability, relying on household-level income data, to accurately assess distributional impacts.

Second, our paper provides a causal estimate of the expenditure elasticity of electricity demand. Energy demand and elasticity have been largely researched (Krichene 2002, Alberini et al. 2011a). Several panel studies employ quasi-experimental or experimental approaches to cope with endogeneity issues, exploiting the exogenous variation induced by supply-shifting events and policy changes (Davis & Muehlegger 2010, Hausman & Kellogg 2015, Rubin & Auffhammer 2024, Hahn & Metcalfe 2021). Although estimates vary widely across contexts and countries (Labandeira et al. 2017), most studies find that electricity demand is inelastic in the short run and becomes more elastic over time (Allcott & Rogers 2014, Deryugina et al. 2020). We add to this literature by employing a novel instrument, the quasi-random default from fixed to indexed price during the European energy crisis, to study consumption responses to changes in price. A similar approach was employed by Ahlvik et al. (2025) to study the households’ response to the energy crisis in Finland. We differentiate with respect to this paper by providing a quantification of the welfare implications of fixed-price arrangements and exploiting hourly data to explore adaptation strategies and heterogeneity. Thanks to our unique administrative data, we are also able to evaluate the effect of the default and subsequent price increase on other behavioral domains beyond energy consumption, i.e., bill payment and contract switching. Finally, access to detailed billing information enables us to analyze actual electricity expenditures. This is an important outcome to monitor, as consumers have been found to respond to the average rather than the marginal price of electricity (Ito 2014).

The remainder of the paper is structured as follows. Section 2 describes the setting, data and methodology. Section 3 presents the empirical results. Section 4 discusses the distributional implications of the fixed price contracts. Section 5 concludes.

2 Data and Methodology

2.1 *Background: empirical setting and EU response*

Since the market liberalization in 2007, electricity consumers in Italy are free to choose their supplier, the conditions under which the service is provided, and to switch to a new supplier at any time. Available commercial offers in the free market include fixed-term, fixed-price contracts, typically offering a constant price for the raw electricity component for a given period of time, and variable-price contracts, which anchor

the electricity price to the National Single Price (PUN, *Prezzo Unico Nazionale*), the wholesale reference price of electricity purchased on the Italian Power Exchange market.² The PUN is computed daily from the quantities and prices formed in the different areas of the country and at various times of the day. The price of indexed contracts adjusts daily, based on the 30-day lagged moving average of the PUN. In the pre-crisis period (2016-2021), more than 80% of customers opted for fixed-price contracts (ARERA 2022, 2023).

The common commercial strategy of offering consumers a fixed price for electricity for a limited period, and then defaulting them into a variable-price contract, is designed to boost market share and enhance customer retention. Such commercial advantages come at the cost of assuming the risk of adverse price fluctuations on behalf of consumers. Retailers limit the negative consequences of such risks by restricting the duration of the fixed-price regime, typically between 12 and 36 months, and by charging consumers a risk premium over expected forward prices.

The duration of the fixed-price period and the pricing regime applied after its expiration are clearly stated in the contract. The date of the default is provided in the first bill after the initial conditions expire, not before. Moreover, this information is buried deep in the bill, and presented together with other details on each item making up the total bill, including the price of the energy component. Therefore, consumers need to carefully read the bill to acquire timely information on the timing of the default. Appendix Figure E1 clarifies the information available to the consumer, showing an anonymized example of how the updated contractual conditions are communicated in the bill.

Between late 2021 and 2023, the energy crisis that hit many EU countries, including Italy, led to a sizable increase in wholesale electricity prices. The upper-left panel of Figure 1 shows how the PUN rapidly climbed, peaking at around 0.66 Euro per kWh in September 2022, more than 600% higher than the pre-crisis levels in 2021. The shock was then reabsorbed around the second half of 2023, as the market adapted to the reduced gas supply from Russia by establishing new supply chains. During the high-price period, consumers who were anchored to an old fixed-price regime received a substantial implicit subsidy to their consumption. The upper-right panel of Figure 1 plots the average price per kWh faced by consumers defaulted into a variable-price contract against those who maintained their fixed-price arrangement. The price advantage of staying in the fixed regime is evident, as the variable price mirrors the fluctuations of the PUN.

The energy crisis attracted public attention, albeit with a slight delay relative to the timing of the first price increases. Using data from Google trends, the bottom panel of Figure 1 shows online searches on rising bills and electricity prices. Despite prices starting to climb in the second semester of 2021, public concern over energy prices changed dramatically only with the official start of the conflict in 2022.³

[FIGURE 1 ABOUT HERE]

The European Commission responded to the fluctuations with a reform proposal in March 2023, with the explicit purpose to protect consumers via *“the right to fixed-price contracts as well as dynamic-price contracts, the right to multiple contracts and to better and clearer contract information”*.⁴

²Other components of the bill, such as taxes and energy distribution fees, may vary unilaterally during the duration of the contract. These, if applicable, are expected to be homogeneous across contracts and providers.

³Data is obtained via Google Trends. Google. (2023). *Google Trends: Search term “caro bollette”, “prezzo energia elettrica”, “offerte energia elettrica”*. Retrieved January 28, 2025, from <https://trends.google.com/trends>

⁴European Commission (2023). *Proposal for a Regulation of the European Parliament and of the Council amending Regulations (EU) 2019/943 and (EU) 2019/942 as well as Directives (EU) 2018/2001 and (EU) 2019/944 to improve the Union’s electricity market design* (COM(2023) 148 final). Retrieved from https://ec.europa.eu/info/law/better-regulation/have-your-say/initiatives/13668-Electricity-market-reform-of-the-EUs-electricity-market-design_en.

2.2 Data and sample

We use data from an Italian multi-utility serving the entire country, with higher presence in the North-East (Appendix Figure A1 Panel (a)). Our dataset contains contractual information on Italian users equipped with second-generation smart meters (2GSM) recording hourly consumption data (in kWh). The sample includes contracts that were already active in January 2021, which we continue to monitor up to March 2023. The contract characteristics include the start and end dates, the contracted power, the different price components of the commercial offer associated with it, and the type of offer applied. In particular, we know if the contract defaults from its initial offer into a new one, typically from a fixed to an indexed-price regime, and the date when the defaults occurs. The data also includes billing information, such as the bill amount, issuance date, payment date, payment method, and any irregularities associated with the payment, including requests for installments or missed payments.⁵ We are able to geolocate users up to the postal codes level. Finally, for approximately 25% of contracts, the data include home size and number of occupants. As this information is only available for a subset of observations, we do not incorporate it into our analysis.

We focus our analysis on consumers under a fixed-price regime at the start of our sample period. We select fixed-price contracts with an initial duration between 24 and 36 months. Some customers in our sample experience the default before March 2023, while others remain under a fixed price for the entire duration of our sample period. To ensure the presence of a pre-treatment period during which all consumers are simultaneously under the fixed-price regime, we remove from the sample those customers who defaulted into new variable-price contracts between January and April 2021. The latest defaults in our sample occur in September 2023. We drop from the sample those rare cases in which multiple contracts are associated with one meter at the same point in time. We obtain a final sample of 89,931 2GSM contracts, 61,845 of which billing information is available.

All consumers are formally billed on a monthly or bi-monthly basis. In practice, our data on the exact day when each bill is issued reveal that several consumers are billed at irregular intervals, due to delays or corrections made by the provider.⁶ Thus, from the sample of consumers with billing data available, we further identify a subset of contracts that we classify as *regularly billed*. These consumers consistently receive their bills every 4–5 weeks (monthly billing, 11,660 contracts) or every 8–10 weeks (bi-monthly billing, 15,177 contracts) throughout the observation period. This additional sample restriction ensures comparability across observations when outcomes of interest are measured at the bill level, avoiding comparisons between bills that cover different time spans.⁷

Table 1 presents summary statistics. Daily consumption is on average 4.98 kWh, with considerable heterogeneity within the sample. This value is relatively similar to the national average of 5.8 kWh between 2021 and 2022 (ARERA 2022, 2023). However, it is way lower than consumption levels observed in studies using US-based samples. For instance, Jessoe et al. (2020) report daily household consumption 24.5 KWh in California. One reason lies in the fact that about 70% of Italian households use gas to heat space and water and electricity accounts for about 40.5% of households' total energy expenditure (ISTAT 2021). Around 65% households receive their bills every two months. This share is reduced to 57% when considering only regularly billed consumers. The rest of the households in their sample are instead billed monthly. The overall

⁵Bills can report a negative amount, typically occurring when a customer receives a reimbursement due to errors or omissions. We exclude these instances by replacing them with missing values.

⁶Occasionally, corrective bills are issued to adjust prior amounts, either upward or downward; however, these are recorded in our dataset as new bills. As a result, this may slightly overstate the number of distinct billing events.

⁷Appendix Table A1 compares summary statistics for the different subsamples. Households across samples appear largely comparable. Consumers who are regularly billed monthly exhibit slightly higher consumption levels and contracted power than those billed bimonthly. To address the possibility of selection, we replicate our main empirical results for both groups.

average monthly bill amount is around 48 Euro for bi-monthly billed consumers, and 66 Euro for monthly billed ones. The average electricity price in fixed-price periods is significantly lower than in variable-price periods. Over 96% of contracts apply a flat rate throughout the day, whereas the remaining 4% entail a dual-rate structure.⁸ Over our observation period, around 3.4% of bills are paid in installments, and 1.2% of bills are flagged as unpaid. Less than 1% of users switch to a different commercial offer by the same utility, while around 2.4% switch to a different provider.

[TABLE 1 ABOUT HERE]

In addition to consumer-level data, we collect data from the Italian National Statistics Institute (ISTAT) on average taxable income at the ZIP code level (Figure A1 Panel (b) in the Appendix).⁹

2.3 Methodology

Our identification strategy defines the expiration of the initial contract and the ensuing default from a fixed- to a variable-price regime as the treatment, exploiting variation in default timing across consumers to identify its effects. Similar to Ahlvik et al. (2025), we also rely for identification on the plausibly exogenous variation in the timing of contract default, and the price shock triggered by the Russian invasion. A household’s contract always defaults on the first day of the month.¹⁰ This generates a staggered treatment design, where a new cohort of contracts rolls into treatment status every month. By selecting households whose contract defaults between May 2021 and September 2023, we have a total of 22 *treated* cohorts (which are considered *not-yet-treated* before the contract change takes place) defaulting between May 2021 and March 2023, and 7 *never-treated* cohorts acting as control group (Appendix Figure A2). By selecting May 2021 as the starting point of our analysis, we observe all cohorts for at least four months before the treatment. This allows us to estimate at least two pre-treatment coefficients for all outcomes of interest and type of consumers, including those billed every two months.

We implement a difference-in-differences (DiD) strategy with staggered treatment adoption. A canonical DiD setup can be expressed by a two-way fixed effects (TWFE) linear regression specification:

$$Y_{igt} = \alpha_0 + \sum_{e \neq -1} \eta_e \cdot D_{it}^e + \phi_{g(i)} + \gamma_t + \epsilon_{igt} \quad (1)$$

where, depending on the specification, Y_{igt} is electricity consumption, bill amounts, probability of requesting an installment plan for a bill payment, probability that a bill is flagged as unpaid, and probability of switching contract or provider. The outcome of interest is defined for consumer under contract i , in cohort g , and at time t . The time dimension is represented by either year-week or bill cycles, depending on the level at which the outcome is measured. $D_{it}^e = \mathbf{1}\{t - T_i = e\}$, with T_i capturing the treatment time for unit i at event-time e , the time elapsed since the treatment’s start. The terms $\phi_{g(i)}$ and γ_t indicate respectively cohort (or consumer) and time fixed effects. When fixed effects are imposed at the coarser cohort level, we additionally control for the household average yearly, summer, and winter consumption, which are instead

⁸Dual-rate contracts differentiate prices between daytime, nighttime and weekends, but the average difference is small — approximately €0.014 per kWh.

⁹Income is reported in seven bins. Income bins are defined on yearly taxable income by the following bands (in Euro): 0-10,000; 10,000-15,000; 15,000-26,000; 26,000-55,000; 55,000-75,000; 75,000-120,000; 120,000+. For each ZIP code, the data reports the number of taxpayers in each income bin. Taxable income is computed taking the median of each income bin and weighting by the number of taxpayers in the bin.

¹⁰Although contracts may formally expire on any day, the new conditions take effect only from the first day of the month following expiration.

absorbed by finer contract level fixed effects. The estimation of η_e for all $e \geq 0$ identifies the treatment effect for each post-treatment period. We refer to this formulation as “dynamic”, as commonly used in the literature. To obtain the average treatment effect across the post-treatment periods, we estimate the following “static” model:

$$Y_{igt} = \alpha_0 + \beta D_{it}^g + \phi_{g(i)} + \gamma_t + \epsilon_{igt} \quad (2)$$

where D_{it}^g is an indicator equal to one when a household from cohort g has defaulted. In our setup, treatment can be considered an absorbing state, i.e., $D_{it}^g = 1$ for all $t \geq T_i$. While households may opt out of the indexed price and return to a fixed-rate contract, they do so under new terms, different from their original contractual conditions, implying the irreversibility of the initial treatment exposure in economic terms.

Recent research has shown that two-way fixed effects models can fail to estimate meaningful causal parameters when DiD designs are complex and treatment effects vary across individuals and over time, as in our context (e.g. De Chaisemartin & d’Haultfoeuille 2020, Sun & Abraham 2021, Wooldridge 2021, Callaway & Sant’Anna 2021, Borusyak et al. 2024). As such, in our preferred specifications, we apply the correction proposed by Callaway & Sant’Anna (2021) and estimate the Average Treatment Effect on the Treated (ATT) for all cohorts first treated at time g and for time $t \geq g$. Cohort-time ATTs ($\widehat{ATT}(g, t)$) are then aggregated using weights to be assigned across all positive lengths of exposure:

$$\sum_g \sum_t \mathbb{1}(t \geq g) w_{g,t} \widehat{ATT}(g, t) \quad (3)$$

The estimation relies on the existence of parallel trends between the treatment and control groups. This requires that differences in observables and unobservables between treatment and control groups remain constant over time, there is no change in the composition of groups according to characteristics correlated with the treatment, and there is no anticipatory behavior. To ensure that these conditions are satisfied, we test for the existence of differential pre-trends and exclude from our sample consumers who were not enrolled in a fixed-price offer in January 2021, as they may differ in their preferences for stable price schemes. Moreover, we select for our analysis only customers whose contracts set the fixed-price period between 24 and 36 months, implying that all households in our sample signed their contract in a period of relative market stability. Long-lasting fixed-price periods also plausibly prevent households from anticipating price spikes at the time the contract is stipulated. Indeed, the price shock that we consider originated from the conflict in Ukraine, which was unforeseeable before 2022. Nevertheless, consumers aware of market conditions may still anticipate the price shock as the end of fixed-rate period approaches. We further investigate this possibility in Section 3.3. Furthermore, we find no systematic differences in contract duration between never-treated cohorts that defaulted after March 2023 and the treated cohorts.¹¹ This further supports the notion that there are no systematic differences in prediction of future wholesale price or risk aversion between the two groups.

Drifting into variable-price contracts when prices are high provides a quasi-experimental setting to study the demand elasticity of consumers in our sample. Access to detailed billing information allows us to estimate elasticity with respect to expenditure –that is, the actual price paid per unit of energy consumption– rather than the marginal price of the raw energy component. This approach is particularly relevant, as consumers are shown to have an incomplete understanding of their tariff structure and perceive changes in their total

¹¹A standardized mean difference test comparing the contract duration of never-treated and treated cohorts shows only a minor discrepancy of 0.02 standard deviations, which is not statistically significant at the 5% level.

bill as more salient for decision-making (Ito 2014).

One common approach to model elasticity is to assume a linear demand function for electricity:

$$\log(\text{Consumption})_{igt} = \theta_0 + \theta_1 \log(\text{BillAmount})_{igt} + \phi_{g(i)} + \gamma_t + \epsilon_{igt} \quad (4)$$

where the log value of consumption defined for contract i , cohort g , and at time t is regressed on the corresponding bill amount. Other terms in the equation are defined as in Equation 2.

Estimating the elasticity parameter θ_1 poses endogeneity concerns. Here, we follow a similar approach to Hahn & Metcalfe (2021), who exploit the marginal reduction in energy prices induced by a randomized subsidy program for low-income consumers. In contrast, we use the exogenous variation in electricity prices generated by the default into a variable-price contract as an instrument. Our estimator is a local average treatment effect (LATE), which allows causal identification of the impact of changes in bill amounts on electricity consumption. The estimated effects refer to the compliers, i.e., consumers who would not have experienced a shock in their electricity bill, had their contract not defaulted. We argue that in our setting the estimated LATE is likely to approximate the average treatment effect (ATE) because the population consists predominantly of compliers, while we expect never-takers and always-takers to be rare. Never-takers are consumers whose price exposure does not change with the default, as they opt out by switching to a new fixed-price contract at conditions similar to their previous one. While such switching may have been possible in the very first months of our observation period, it became increasingly unfeasible as fixed-price offers became more expensive in response to higher wholesale prices. Always-takers may also exist, as consumers remain free to voluntarily switch to variable-price contracts regardless of the default. However, doing so would require subscribing to a new offer under less favorable commercial conditions than their existing contract, making this behavior relatively uncommon.

We define our instrumental variable specification such that treatment status, i.e., experiencing a contract default, will be the instrument used in the second stage to analyze the impact of the change in electricity expenditure on consumption. We estimate the model using a two-stage least squares (2SLS) estimator. The first stage is estimated through the following equation:

$$\log(\text{BillAmount})_{igt} = \pi_0 + \pi_1 D_{it}^g + \phi_{g(i)} + \gamma_t + \nu_{igt} \quad (5)$$

where all terms are defined as in the previous equations.

Our instrument needs to satisfy three main assumptions. First, the default must increase the marginal price of electricity (relevance condition). This is satisfied in our study period, given the rise in indexed prices following the Ukrainian war, and can be confirmed through the first stage. Second, the contract default must be orthogonal to any other factor influencing energy consumption (exogeneity assumption). This assumption holds implicitly if there is no endogenous selection into treatment. In other words, fixed-price customers should neither actively change their contract conditions before defaulting, nor have anticipated the market conditions they would be defaulted into when they adopted the fixed-price contract. In addition, the validity of our instrument requires that defaulting into an indexed price contract should influence consumption only through its effect on bill amounts (exclusion restriction assumption). A potential violation of this assumption could occur if consumers place intrinsic value on variable-price contracts that directly influences their consumption behavior. If such a preference is time-invariant, it would be absorbed by household fixed effects. However, if this intrinsic value varies over time—e.g., becoming more salient during high-price periods—it could bias the estimated elasticity.

3 Results

3.1 *Impact of defaulting on bills and behavior*

We analyze the dynamics of consumer adjustment to the new prices by estimating Equation 1. First, we examine how bill amounts changed in response to the default. In this specification, the time dimension t in Equation 1 represents billing periods. Hence, we consider the sample of regularly billed consumers. Since the n -th bill relative to the default date represents different time periods for monthly and bi-monthly billed customers, we estimate the model separately for these two groups of consumers. For conciseness, in this section we present the results for consumers who receive their bills bi-monthly, as this is the larger group in our sample. All results are replicated when we focus on consumers with monthly billing, and reported in Appendix B.1. Estimates are reported in Figure 2 (Panel A).

[FIGURE 2 ABOUT HERE]

The increase in bill amounts following the default appears both sharp and persistent. The rise in electricity expenditure is initially moderate —about 20 Euro in the first bill— which can be explained by the fact that the first bill after the default may include consumption that occurred before, i.e., priced at the original fixed rate. By the third bill received under new contract conditions, expenditure increases by nearly three times as much (approximately 60 Euro), with the effect remaining stable over subsequent billing cycles.

To understand to what extent consumption responds to the price signal received by consumers through their electricity bills, we estimate Equation 1 by imputing electricity usage to individual bills. We do so by summing the consumption recorded between two consecutive bills and attributing the total to the earlier bill in the pair (Panel B).

We observe a clearly estimated negative effect of losing fixed-price protection on electricity consumption. However, the consumption adjustment occurs more slowly and gradually than the expenditure increase. By the seventh bill received after the default, consumers still reduce their usage. By around the eighth or ninth bill, the effect appears to stabilize at approximately 27% lower consumption relative to fixed-price customers. Since consumption can be observed independently of the billing cycle, the same analysis can be conducted by including irregularly billed consumers, thereby extending the sample to all 2GSM households and effectively abstracting from the billing frequency. We therefore estimate Equation 1 using weekly consumption levels (Appendix Figure B3). In this specification, the time dimension t is year-week. The same dynamics in consumption adaptation emerge, as we find that, in the first semester after the default, the point estimates of weekly consumption oscillate around a slowly decreasing trend, with a contraction of up to 5.5%. In the second and third semesters, weekly consumption in the treatment cohorts falls by up to 7.6% and 15%, respectively. Thus, adaptation in consumption levels follows the price signal carried by electricity bills. However, this process is gradual and slow, hinting at the presence of frictions and potential sunk costs of adaptation.

In Table 2, we estimate a static model, as in Equation 2, and use two-way fixed effects and corrections for staggered implementation according to the strategies developed by De Chaisemartin & d’Haultfoeuille (2020) and Callaway & Sant’Anna (2021). We find that the overall ATT oscillates between 3.5% and 6%, depending on the specifications.

Similar patterns emerge when looking at the ATT by treatment cohort (Appendix Figure B4). The effect of the default on bill amounts remains constant until the last trimester of 2022, when the impact of the crisis

starts to fade ¹². Due to the data being censored at the end of February 2023, cohorts of consumers defaulting earlier are observed for a longer period of time and, in turn, show a relatively higher post-treatment effect on electricity consumption than those defaulting toward the censoring date (Appendix Figure B5).

[TABLE 2 ABOUT HERE]

The slow consumption response to the expenditure shock suggests that consumers may face adjustment barriers. In the absence of frictions, one would expect consumers to adapt to the new price and converge toward a new optimal consumption level promptly. A gradual adjustment instead points to potential constraints—such as limited information, habit persistence, or adjustment costs—that delay the response in electricity consumption. Behavioral barriers may obstacle adjustment even in the presence of perfect information about the contract change. Alternatively, consumers may be inattentive to, or unaware of, market prices and contract terms. In such cases, the increase in energy bills following the default may serve as a salient signal, prompting a reduction in electricity demand in the following months. The available data do not allow us to distinguish between all the different explanations. Yet, by checking for heterogeneity in the effect of defaulting across type of bill (paper mail vs. email) and the type of payment (manual vs. direct debit), and we do not find statistically significant differences between these groups. This, in turn, which may suggest that information barriers are not a primary factor behind adjustment frictions (Appendix Figure B6).

In addition to adjusting their consumption in response to changes in energy prices, households may engage in other forms of adaptation during periods of price shocks. For instance, consumers facing difficulties in paying their bills have the option to request installment payments at a positive interest rate after receiving a bill. Alternatively, they may choose to entirely skip a payment, in which case the bill will be marked as unpaid in our dataset.¹³ We refer to these instances collectively as “payment issues”, since both occurrences result in a utility loss for consumers, due to positive interest charges and/or penalties on the outstanding amount. Using our data, we can estimate the impact of the price shocks experienced on payment issues. Indeed, evidence of other forms of behavioral adjustments can substantially influence the discussion around the degree of protection that fixed-price contracts provide to consumers in volatile markets and the extent of market distortions that they generate.

We estimate our staggered DiD model (Equation 1) and use the probability of installment and of missing payments as dependent variables. Both outcomes indicate that the bill payment occurred with partial issues.

[FIGURE 3 ABOUT HERE]

The results in Figure 3 show a small but statistically significant increase in the probability of requesting an installment plan for a bill payment. Starting from the second bill after the default, consumers are between 1.29 and 1.84 percentage points more likely to reschedule their payments after receiving a bill. They also

¹²In June 2022, the Italian government introduced a set of measures to reduce energy costs for households and firms (*Decreto-Law No. 80/2022*). These measures, which took effect in the third quarter of 2022, included a reduction in electricity system charges and provisions for social energy bonuses. In our data, bill amounts are observed net of bonuses and deductions. Consequently, we cannot disentangle the extent to which the observed reductions in energy bills reflect the government measures versus the natural decline in wholesale prices, which, after peaking in September 2022, fell to slightly above pre-crisis levels by March 2023.

¹³We are unable to track the progression of electricity bills that are marked as unpaid by the utility. Consumers benefit from a temporary credit period—typically lasting between one and two months—granted by the provider. During this period, the energy provider usually issues payment reminders. If nonpayment persists, the provider may reduce the allotted power and eventually disconnect the household’s electricity supply. Ultimately, the utility may also pursue legal action to recover the overdue payment. In this case, additional penalties are often applied to the initial amount.

appear more likely to miss their payments, though this effect is less precisely identified, oscillating between 0.28 and 0.63 percentage points with reduced significance. These results highlight an additional purpose served by fixed price arrangements, i.e., preventing payment disruptions. Nevertheless, the magnitude of payment issues is limited relative to the consumption response, likely because rescheduling payments or skipping them offers only a temporary relief from high energy expenditures.

3.2 *Expenditure elasticity of electricity demand*

To obtain a measure of expenditure elasticity of electricity consumption, we estimate Equation 4 via 2SLS (Table 3).

[TABLE 3 ABOUT HERE]

Columns 1 and 2 report the estimate of elasticity using a naive linear model. The coefficients are positive and significant, even controlling for contract and year-by-week fixed effects. This is a strong signal of a simultaneity bias (e.g., rising wholesale prices during periods of high demand) or the presence of time-variant omitted variables (e.g., household characteristics) positively correlated with energy spending and consumption. We then turn to the IV model in Columns 3 and 4. Column 4 reports our preferred specification, which includes more conservative fixed effects at the contract level. We find a strong positive correlation between the contract default and bill amount, which is confirmed by the positive and statistically significant first-stage coefficient and the large F-statistics. Hence, we reject the weak instrument hypothesis. The expected increase in bill amounts ranges between 26.8% and 27.1%. The elasticity estimated in the second stage is now negative and significant: a 1% increase in the electricity bill is associated with a 0.094% to 0.113% decrease in electricity consumption, the latter constituting our preferred estimates. Low elasticity parameters suggest that the demand for electricity in our setting is quite unresponsive to changes in average price, in line with previous studies on EU countries and developed economies (Labandeira et al. 2017).

The high frequency of smart meter readings in our sample enables us to examine how consumption elasticity varies throughout the day, offering insights into consumers’ adaptation strategies in response to the energy price shock. To obtain hourly estimates of expenditure elasticity, we re-estimate our 2SLS model using, as the dependent variable, the average weekly consumption in each of the 24 hours in the day. The resulting coefficients, together with the average hourly consumption profile, are plotted in Figure 4. We find that elasticity remains relatively stable across hours, suggesting that load shifting across hours of the day is not households’ primary adaptation strategy to the higher prices. This pattern is consistent with the fact that most contracts in our dataset feature a flat hourly rate—under both fixed and indexed price regimes. Therefore, consumers face no direct monetary incentive to shift or redistribute their electricity usage across hours. Nonetheless, electricity consumption is somewhat more elastic during daytime hours and less so during peak demand periods, typically corresponding to evening hours between 6 p.m. and 11 p.m.. Overall, these results suggest that elasticity within the day reflects both consumer preferences and the extent to which they can modify consumption patterns at various times of the day.

3.3 *Anticipation of price shocks*

As the contract default approaches, forward-looking consumers may adjust their consumption habits in anticipation of less favorable future conditions. Systematic anticipatory behavior from households should be reflected in the presence of pre-trends, which we do not find in our main specification (Figure 2). However,

Ahlvik et al. (2025) find that knowledge about the energy crisis increases the awareness of consumers and stimulates an anticipatory contraction in electricity consumption. As a result, although it does not emerge clearly in aggregate, anticipatory behavior may still be correlated with the timing of the default and could bias our estimates. Our staggered design enables us to detect anticipation among consumers at various moments in time. For any treatment cohort g defaulting at time κ , we implement our staggered DiD model taking time $t = \kappa - n$ as a reference period, and examine the effect of treatment within the n -period window. We choose n to be three and six months before the default. As an example, Appendix Figure B7 illustrates two treatment cohorts –households defaulting in November 2021 and in May 2022– observed within a six-month anticipation window. The figure compares the anticipation effect of each cohort with the treatment effect of the cohort defaulting at the start of the anticipation period. The two examples represent cases of a modest anticipation effect (top panel) and no detectable anticipation effect (bottom panel).

To visualize anticipation across all treatment cohorts, we estimate the following refinement of Equation 2:

$$Y_{igt} = \alpha_0 + \xi A_{it}^g + \beta D_{it}^g + \phi_{g(i)} + \gamma_t + \epsilon_{ijt} \quad (6)$$

where A_{it}^g is an indicator equal to one when a household from cohort g has entered the anticipation period, n months before defaulting (D_{it}^g). Other terms are defined as before. Accordingly, ξ captures the average anticipation effect across the pre-treatment periods. Appendix Figure B8 displays the estimated values of ξ for three and six months of anticipation: the absence of anticipation can be rejected for some cohorts, particularly in the months between February 2021 and November 2021.¹⁴ However, we find no clear correlation between households’ anticipatory behavior and contemporaneous levels of energy prices or media coverage of the energy crisis (Figure 1). Consumption levels of not-yet-treated cohorts prior to the default display non-systematic fluctuations, consistent with the absence of significant pre-trends when considering the full sample of cohorts. Yet, as some cohorts do show statistically significant anticipatory behavior, we explore the robustness of our results to dropping these households from the sample. The ATT of defaulting to indexed-price contracts for the remaining cohorts is consistent with our previous estimates, only marginally smaller in magnitude and ranging between 2.8% and 6% (Appendix Table B1).

3.4 Attrition and Competitive Switching Behavior

When households default to new contract conditions, they may choose to switch to a different commercial offer and/or energy provider. While we can track consumers who switch to a different offer within the same utility, those who terminate their contract with the utility entirely disappear from our sample. This attrition may affect the validity of our empirical strategy. If consumers’ switching decisions are non-random and systematically related to either their treatment status or outcomes of interest, such correlations could introduce selection bias in our estimated ATT. To investigate whether attrition could be a concern in our design, we first look at the effect of defaulting on the probability of switching offer or leaving the utility (churn). The service plan active on a household’s contract is observed at the bill level. Moreover, we consider a household to have switched provider when the corresponding meter permanently disappears from our sample.¹⁵ Since the probability of switching likely increases over time (due to, for instance, initial

¹⁴Given that the data are left-censored in January 2021, the reference period in Equation 6 –defined as the four months preceding the start of the anticipation period– is necessarily shorter for cohorts that defaulted at the start of the observation period.

¹⁵This may include some limited error, e.g., contract disconnections due to the holder’s death.

effort in finding a new plan, gradual familiarity with market options, evolving service availability), we adjust our sample to focus on consumers in the treatment and control group with similar contract tenure. We thoroughly detail our strategy in Appendix Section B.6.

We find that switching is relatively rare: between 0.01 and 0.3% of customers change provider at any given period. The contract default has a statistically significant effect on the probability of switching, ranging from 0.6% to 1.6% (Appendix Table B2). This effect is particularly pronounced among households whose contracts defaulted during the initial spike in wholesale electricity prices. This suggests that the treatment may induce attrition based on unobservable household characteristics, such as attention to contract details or awareness of market conditions. To address concerns that attrition may bias our ATT estimates, we compute nonparametric bounds to the treatment effect in the spirit of Manski (1990) and Ichino et al. (2008). The treatment effect of the default on consumption remains within negative bounds (Appendix Figure B10), suggesting that selection due to attrition cannot explain the observed effect of defaulting.

4 Welfare analysis and distributional impacts

Fixed-price electricity contracts protect consumers from price volatility but distort consumption behavior by implicitly subsidizing (taxing) consumption when wholesale prices are high (low). In this section, we perform a welfare analysis of these contracts during the energy crisis, explicitly considering the trade-off between protection and efficiency. Specifically, we evaluate the transfer of welfare from providers to consumers and the resulting change in total surplus, accounting for deadweight losses and externalities from CO₂ emissions arising from the implicit price subsidies embedded in fixed-price contracts.

When evaluating the welfare implications of the fixed-price scheme, we also account for potential distributional effects by considering income as a key factor mediating consumers’ responses to price changes. Wealthier households, which typically consume more electricity, benefit disproportionately from fixed-price arrangements, making them regressive when market prices are high. At the same time, reactions to a transition toward variable pricing can be heterogeneous: higher-income consumers may be more informed about market conditions and better able to invest in energy efficiency, while lower-income households may be more constrained but also more sensitive to price fluctuations, as electricity spending represents a larger share of their budget. Hence, the overall distributional and efficiency impact of moving from fixed to variable pricing remains an empirical question.

4.1 *Estimating household income*

Our dataset does not include direct information on household income. To address this limitation, we estimate household-level income using the methodology developed by Reguant et al. (2025). They propose a non-parametric estimator that infers a household’s income by combining average income data at the zip-code level with observable household characteristics, including granular electricity consumption habits. Specifically, we classify households into six consumer types based on contracted power levels and consumption patterns derived from a *k-means* clustering analysis. These types are then linked to local income distributions, based on the distribution of residents across taxable income brackets from ISTAT at the zip-code level. A generalized method of moments (GMM) estimator is employed to infer the probability that each consumer type belongs to a given income category, based on the joint distribution of household types and taxable income within a province. Individual income is calculated as the weighted average of the median value of

each bracket and the corresponding probability that the household belongs to that bracket. Results indicate a positive association between income, contracted power, and average electricity consumption, consistent with previous evidence in the literature. We thoroughly detail these steps in Appendix C.

4.2 *Heterogeneous effect of defaulting across income groups*

After predicting household income, we examine heterogeneity in consumption responses to the default and the distortions induced by fixed prices across income groups. The sample is divided into income quintiles to capture variation across the distribution. Predicted taxable incomes range from 12.9–17.4 thousand euros in the bottom quintile to 26.2–40.5 thousand euros in the top quintile.¹⁶ We then estimate Equation 2 separately for each quintile, looking at the ATT of defaulting into variable-price contracts on bill expenditure and consumption levels (Figure 5).

[FIGURE 5 ABOUT HERE]

Interestingly, while the effect on bills is comparable across income quintiles (Panel a), the reaction of consumption to the new price is a concave function of income (Panel b). The most pronounced reductions are observed among households in the first, fourth, and fifth income quintiles where individuals consume around 4 kWh (roughly 13%) less per week compared to the control group, still under fixed prices. Households in the middle –particularly the third quintile– appear relatively unresponsive to the change in electricity prices.¹⁷

The inverted-U-shaped relationship between income and the ATT of contract default points to potential nuances in our context. Prior work has found a monotonic, increasing (Schulte & Heindl 2017), and occasionally even convex (Romero-Jordán et al. 2016) association between income and price elasticity of electricity demand. Our empirical setting has some peculiar features, which may contribute to explaining our different findings. First, average consumption levels differ across income quintiles, but the gap is relatively modest. Households in the bottom-quintile consume on average 5.02 kWh/day, which is 1.55 kWh/day less than those in the top-quintile. These figures indicate that consumers in our sample are unlikely to be operating at near-subsistence levels of electricity consumption, which may help explain why energy demand is more elastic than typically observed among low-income households. Second, the price increase analyzed in the paper took place amid significant economic turmoil in Italy, driven by the sharp decline in Russian natural gas supplies after the onset of the conflict, which resulted in higher import prices, rising inflation, and lower disposable income. Income pressures, as well as differences in the perception and salience of price shocks, may reflect distinct underlying motivations behind the similar adjustments in consumption behavior observed across consumers with different income levels. This, in turn, may have different implications for equity. Consumption reduction of low-income households, more often found to be less responsive to price changes, may reflect a form of forced austerity, rather than a voluntary correction of overuse or the investment in energy-saving technologies. For consumers in the top income quintiles, the increase in electricity prices is less likely to induce a newly binding budget constraint on energy consumption. However, richer households may have greater capacity to adapt their energy usage, for instance, through increased awareness of energy markets and prices, and investment in energy-saving technologies. Fixed-price arrangements during periods of high volatility may discourage investment in energy efficiency among both high –and low– income households.

¹⁶The timing of default of households in different income quintiles does not vary significantly (Appendix Figure C5).

¹⁷The result is similar when looking at log-transformed consumption levels (Appendix Figure B11). Consumers in the top and bottom quintiles consume around 12% to 13% less per week post default, while the ATT is not significant for households in the third quintile.

However, the resulting excess consumption may reflect different degrees of inefficiency across the wealth distribution. Our data do not allow us to fully characterize the nature of consumers’ behavioral responses, as they lack detailed information on appliances, energy efficiency, and household characteristics. Nevertheless, we find small but discernible differences in the consumption patterns of low- and high- income households following the default to indexed-price contracts. Compared with those in the bottom income quintile, households in the top quintile reduce their electricity use more substantially –by over 10 kWh/week– after one and a half years of treatment (Appendix Figure B12). Top-quintile households show higher responsiveness during daytime hours compared to the bottom quintile, which instead appears more responsive during the night; however, such differences are not statistically significant (Appendix Figure B13). A stronger reduction in daytime consumption relative to nighttime use may indicate more deliberate or efficiency-oriented adjustments, such as shifting flexible activities or investing in more efficient appliances, or adoption of other energy supply technologies (e.g. photovoltaic). In contrast, reductions concentrated during nighttime hours may reflect more passive curtailment, such as switching off appliances or reducing comfort (e.g., higher temperature with AC on in summer), rather than improvements in energy efficiency. The same differences in hourly patterns are not found across consumers in other quintiles (Appendix Figure B14). Middle-income groups’ modest to no response may therefore be a result of a combination of these opposing forces. A higher budget could make the increase in energy spending less salient compared to the bottom quintiles. However, compared to the top quintiles, these households may be rather unaware of the market and less efficiency driven, generating a stagnation in their consumption levels during price fluctuations.

We also check for heterogeneity in the effect of indexed prices on irregular payment instances (Figure 6). The probabilities of requesting installment payment on a bill and letting a bill go unpaid follow a more convex relationship with income.

[FIGURE 6 ABOUT HERE]

The effect is estimated less precisely across income quintiles. However, particularly for installment payments, the results again suggest greater salience of energy spending at the two tails of the income distribution. This translates into a statistically significant increase in the probability that a bill is paid in installments for households in the bottom and top income quintiles, while the effect remains statistically not significant for the middle quintiles. We find no statistical evidence that the probability of missed payments increases among lower-income quintiles after the default, possibly because, as previously argued, households in our sample do not exhibit near-subsistence consumption and face a relatively modest electricity expenditure burden.

4.3 *Welfare analysis and distributional implications of fixed prices*

When fixed-price contracts offer consumers more favorable conditions than those they would face under wholesale price indexing –the scenario analyzed in this paper– these contracts generate a redistribution of surplus between users and producers, with the latter effectively financing an implicit subsidy for the former. This transfer, however, is expected to generate inefficiency and reduce total surplus. First, by creating a wedge between production costs and the consumption price, it distorts consumption behavior, leading to consumption above a more efficient level and generating a deadweight loss. Second, because electricity consumption from non-renewable sources produces negative externalities in the form of greenhouse gas emissions –primarily CO₂, but also, to a lesser extent, methane and nitrous oxide– the additional consumption induced by the fixed-price subsidy increases these externalities. Moreover, heterogeneous responses in consumption

to variable prices across income groups imply differences in both the magnitude of the welfare redistribution and the overall surplus loss across household types. We apply a simple theoretical model to quantify the welfare implications associated with fixed prices in our setting, depicting the existing differences across income groups based on our results. We focus on estimating the transfer in surplus between consumers and producers, the associated deadweight loss, and the environmental externalities under the new equilibrium.

We assume a linear downward-sloping electricity demand and a perfectly elastic supply. This simple market model is described in Appendix Section D. Let C_{iqt} be the consumption of household i belonging to income quintile q observed at time t , and let D_{iqt} take the value one when a household has defaulted as in Equation 2. The average monetary subsidy received by the households from the utility in each income quintile over our period of analysis (ΔM_q) can be calculated as:

$$\Delta M_q = \frac{1}{I_q} \sum_t \sum_i C_{iqt} (\tilde{p}_{iqt} - \bar{p}_{iq}) (1 - D_{iqt}) \quad (7)$$

where I_q is the number of households in each income quintile, $\tilde{p}$ is the indexed price determined in the wholesale market at time t , and $\bar{p}$ is the fixed price paid by household i . The transfer equals the overall subsidy received by the consumer during the time she remained under a fixed price arrangement. We can then analyze the transfer of surplus between retailers and consumers (ΔS), and a deadweight loss (DWL) in total surplus, which represents the efficiency loss.

To do so, we must assume a counterfactual efficient level of consumption that would have prevailed in the absence of low fixed prices. In other words, given the assumption of linearity, we need to specify the slope of the demand function. We use the treated consumers' cohorts as the counterfactual, representing the more efficient market equilibrium that would have emerged in the absence of the subsidy.

Let the counterfactual efficient consumption of consumer i in income quintile q and time t be equal to:

$$C_{iqt}^{CF} = C_{iqt} \left(1 + \widehat{ATT}_{q,t-T_i} \right) \quad (8)$$

where $\widehat{ATT}_{q,t-T_i}$ is the average treatment effect at event-time $t - T_i$ estimated for each income group. With a linear demand, it is easily shown (see Appendix D, Section D.2) that the surplus transfer (ΔS) and the deadweight loss (DWL) can be calculated as:

$$\begin{aligned} \Delta S_q &= \frac{1}{I_q} \sum_i \sum_t (\tilde{p}_{iqt} - \bar{p}_{iq}) \cdot C_{iqt} \left(1 - \frac{\widehat{ATT}_{q,t-T_i}}{2} \right) (1 - D_{iqt}) \\ \text{DWL} &= \frac{1}{2I_q} \sum_i \sum_t (\tilde{p}_{it} - \bar{p}_i) \cdot C_{it} \widehat{ATT}_{t-T_i} (1 - D_{it}) \end{aligned} \quad (9)$$

As consumption deviates from the counterfactual efficiency level, it implies an additional societal cost caused by increased emissions. To quantify it, we assume a social cost of carbon (SCC) of US\$185 per tonne of CO₂, according to recent estimates from Rennert et al. (2022).¹⁸ Moreover, we assume a CO₂ production of 260 gCO₂/kWh, as estimated in the 2022 National Inventory Report for Italy from ISPRA (ISPRA 2022).¹⁹ The social cost of CO₂ is obtained by multiplying the assumed cost per t/CO₂ with the average weekly deviation in consumption levels ($\widehat{ATT}$) for each income quintile.

¹⁸This value is converted to Euro using the average exchange rate during the analysis period (0.93 Euros/US dollar).

¹⁹Annual Inventory Submissions for Italy are performed by The Institute for Environmental Protection and Research (ISPRA) and submitted under the UN Framework Convention on Climate Change and the Kyoto Protocol.

Table 4 summarizes the welfare implications for a representative consumer across income quintiles on the average week (Panel A) and over the whole study period (Panel B).

[TABLE 4 ABOUT HERE]

In absolute terms, transfers implied by fixed prices appear heterogeneous and regressive, although not monotonically increasing with income (Column 1). Pairwise hypothesis testing shows that the detected differences are generally statistically significant, especially when considering total amounts (Appendix Table D2). Households in the bottom quintile receive an average subsidy of roughly 5.4 Euro per week, around 590 Euro in total, whereas consumers in the top quintile receive more than 7.1 Euro per week, 787.3 Euro in total. This pattern is reversed when looking at the transfer relative to the median household income in the quintile (Column 2). The implicit subsidy is around 1.8% for the bottom quintile, decreasing to 1.1% for the top quintile. As such, in relative terms, fixed prices generate a progressive redistribution mechanism. Conversely, the efficiency loss implied by the induced market frictions reflects the tendency illustrated in Figure 5 (Column 4). Households in the second and third quintiles generate a smaller DWL than those in the bottom and top quintiles. Around 4.12% (6.34%) of the average weekly (total) implicit subsidy received by consumers in the bottom quintile reflects a loss in overall surplus. This is even higher for households in the fourth and fifth quintiles, for which it reaches a maximum of 55.4 Euro per year and more than 7% of the surplus. Consumers in the bottom and top quintiles are also associated with higher externalities, producing a societal loss of respectively 20.39 and 22.87 Euro over the period considered (Column 7).

Finally, we consider the mean quantities across consumers in our sample to get a more general sense of the welfare implications of fixed prices. The average household under a fixed-price contract receives a subsidy of 5.79 Euro per week, implying a DWL in overall surplus of 0.30 Euro per week, and a social cost of CO₂ of 0.13 Euro. Over the entire period of our study, these quantities add up to 654.4 Euro, 33.29 Euro, and 14.43 Euro per consumer, respectively.

5 Discussion and conclusion

We leverage the transition from fixed to indexed-price electricity contracts for households in a period of uncertainty and high prices to assess both the distortion in consumption behavior induced by fixed prices and the nature of the transfer mechanism from retailers to consumers. Access to contract information, hourly electricity consumption, and billing data at the household level for a large sample of Italian consumers allows us to exploit the conditionally exogenous variation in the timing of the default into indexed-price contracts to characterize the behavior of consumers upon receiving the expenditure shock.

We document substantial adjustments in consumption levels, which, however, occur gradually. A delayed response suggests that consumers poorly anticipate the price shock and take time to adapt, even as the price signal becomes increasingly salient through repeated exposure to higher electricity bills. Adaptation through consumption may also be constrained, as households may be unable to reduce consumption below a certain subsistence/discomfort threshold. This last evidence is documented by a rather inelastic demand for electricity, with elasticity estimated to be around - 0.113. Consumers exhibit signs of financial distress in response to increased energy expenditures, occasionally requesting installment plans or missing bill payments. However, these behaviors remain relatively infrequent.

We find that the welfare transfer from retailers to consumers – arising from fixed prices remaining below wholesale prices – is regressive in absolute terms but progressive when measured as a share of income. We

also find that the efficiency losses associated with market frictions are more pronounced at the lower and upper ends of the income distribution. The fact that both low and high-income households reduce their electricity consumption in response to price changes with relatively different patterns in terms of timing of reduction and hourly distribution of saving efforts could be an indication of distinct underlying mechanisms. Lower-income households may cut consumption out of necessity, while higher-income households may do so because they can absorb the adjustment cost more easily, for instance, through investments in energy efficiency.

These findings are not a criticism of either fixed or indexed pricing. Instead, they convey the need for policymakers to pay attention to potential unintended consequences of universally applied fixed pricing, such as the delay of necessary energy transitions and the misallocation of public resources toward households that may not need support. Fixed-price arrangements available to consumers may offer an easily implementable policy solution to protect vulnerable consumers from the risk of energy poverty during the transition to renewable energy sources. However, they may do so at the expense of less efficient and equitable energy markets compared to more targeted approaches, such as usage-tiered subsidies. Improving the understanding and transparency of contracts can help minimize information frictions and increase consumer awareness, potentially enhancing efficiency in energy usage.

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6 Tables

Table 1: Descriptive Statistics

	All 2GSM	With billing	Bi-monthly (regular billing)	Monthly (regular billing)
N. contracts	89,931	61,845	15,177	11,660
	Mean	SD	Min	Max
Consumption (kWh)				
◦ Daily	4.98	3.49	0	83.87
◦ Weekly	34.85	31.20	0	6204.21
Contracted power (kW)	3.19	0.70	1.50	10.00
Building size (m ²)*	78.81	25.19	15	152
Household members*	1.96	1.04	1	6
Bill amount	81.63	80.82	0	13848.05
◦ Monthly	66.24	68.59	0	10313.71
◦ Bi-monthly	96.50	88.58	0	13848.05
P(monthly)				
◦ All	0.35	0.48	0	1
◦ Regularly billed	0.65	0.48	0	1
P(bi-monthly)				
◦ All	0.43	0.50	0	1
◦ Regularly billed	0.57	0.50	0	1
P(flat rate)	0.96	0.19	0	1
P(email bill delivery)	0.45	0.50	0	1
Fixed price (euros/kWh)	0.08	0.00	0.06	0.11
Variable price (euros/kWh)	0.21	0.12	0.07	0.58
P(Installment)	0.03	0.18	0	1
P(Default)	0.01	0.11	0	1
P(Switch offer)	0.01	0.08	0	1
P(Switch provider)	0.02	0.15	0	1
Contract duration (months)	29.20	2.04	24	36

Notes: The table reports descriptive statistics for our sample of 2GSM households. Statistics for time-varying variables are computed over the period from January 2021 to March 2023, or—from the household-specific default date to March 2023 in the case of variable contract prices. Consumers in the sample receive their bills either monthly or bi-monthly. Statistics on bill amounts and outcomes only observable through bills (payments and contract changes) only refer to customers with billing data available. Bill delivery methods include paper mail and email. Most contracts apply a flat rate throughout the day, while a minority apply a dual-rate structure (daytime vs. nighttime). Statistics for variable prices are computed using all household-day observations in which a contract has transitioned from a fixed- to an indexed-price regime.

* Information on dwelling size and household occupancy is available for approximately 25% of the sample.

Table 2: Effect of defaulting on energy consumption

$\log(\text{Consumption (Kwh)})$	(1) TWFE	(2) TWFE	(3) dC-DH DiD	(4) CS DiD
Treat $\times$ Post	-0.035*** (0.0066)	-0.038*** (0.0023)	-0.042 (0.031)	-0.062* (0.0335)
N	7,003,889	7,002,417	7,003,889	7,003,889
Contracts	89,931	88,459	89,931	89,931
Year-by-week FE	Y	Y	Y	Y
Contract FE		Y		
Cohort FE	Y		Y	Y
Controls	Y		Y	Y
Cluster level	cohort	contract	cohort	cohort

Notes: the table shows the estimates of a static staggered DiD model (Equation 2). Two-way fixed effects estimates (TWFE, Columns 1 and 2), and estimates corrected for staggered implementation according to the strategy developed by De Chaisemartin & d'Haultfoeuille (2020) (Column 3) and Callaway & Sant'Anna (2021) are compared. Controls include household average yearly, summer, and winter consumption. Standard errors (in parentheses) are clustered at the level indicated in the table. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Number of observations (N) and contracts across specifications are net of automatically dropped singletons under different fixed effects structures.

Table 3: Estimated average price elasticity of consumption

	OLS		IV	
	(1)	(2)	(3)	(4)
Elasticity ($\hat{\mu}$)	0.233*** (0.016)	0.083*** (0.002)	-0.094*** (0.015)	-0.113*** (0.009)
First stage				
Treat $\times$ Post			0.2714*** (0.034)	0.268*** (0.002)
N	5,319,884	5,319,842	5,319,884	5,319,842
Contracts	61,844	61,802	61,844	61,802
C-D F-stat			109,186.4	175,854.1
R-K F-stat			65.1	14,211.1
Year-by-week FE	Y	Y	Y	Y
Contract FE		Y		Y
Cohort FE	Y		Y	
Controls	Y	Y	Y	
Cluster level	cohort	contract	cohort	contract

Notes: the table shows the estimates for expenditure elasticity of demand of electricity consumption. OLS estimates (Columns 1 and 2) are obtained via the direct estimation of Equation 4. Consumption elasticity is computed via 2SLS in Columns 3 and 4, where first-stage coefficients are derived from Equation 5. The table reports the Cragg-Donald (C-D) and Kleibergen-Paap Wald (K-P) F statistic for weak instrument identification. Controls include household average yearly, summer, and winter consumption. Standard errors (in parentheses) are clustered at the contract level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Number of observations (N) and contracts across specifications are net of automatically dropped singletons under different fixed effects structures.

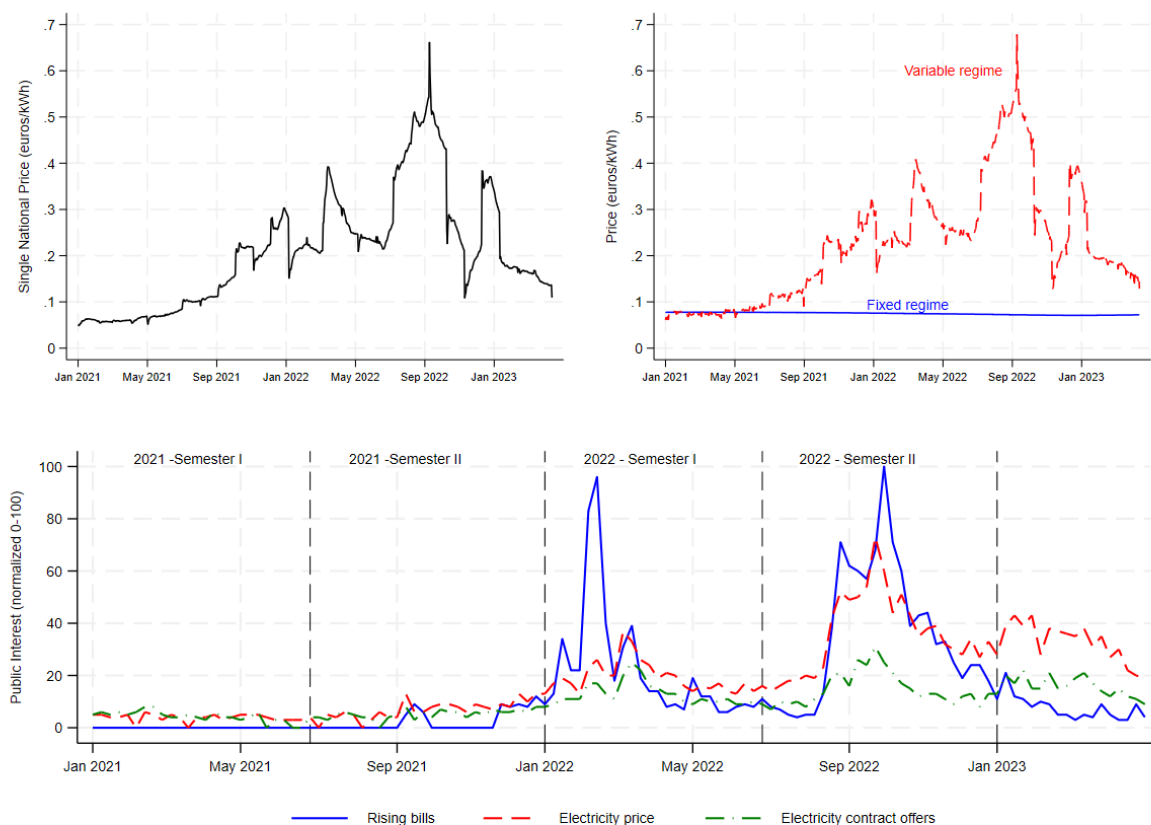
Table 4: Surplus transfers and efficiency losses across income groups

Income quintile	(1) ΔM (Euro)	(2) $\Delta M/\text{HH}$ income (%)	(3) ΔS (Euro)	(4) DWL (Euro)	(5) $\text{DWL}/\Delta M$ (%)	(6) ΔCO_2 (kg)	(7) Cost of CO_2 (Euro)
<i>Panel A: Weekly mean (SD)</i>							
1st	5.383 (4.33)	1.836 (1.48)	5.042 (3.96)	-0.341 (0.5)	4.12 (4.96)	1.049	-0.18
2nd	6.489 (5.31)	1.672 (1.37)	6.213 (5.05)	-0.275 (0.35)	3.04 (2.84)	0.478	-0.082
3rd	5.227 (4.28)	1.167 (0.96)	5.214 (4.26)	-0.014 (0.09)	0.13 (0.89)	0.156	-0.027
4th	4.755 (3.76)	0.988 (0.78)	4.402 (3.41)	-0.353 (0.38)	5.15 (4.18)	0.851	-0.146
5th	7.102 (5.7)	1.094 (0.88)	6.612 (5.22)	-0.49 (0.63)	4.64 (4.94)	1.176	-0.202
Overall mean	5.791 (4.66)	1.351 (1.09)	5.497 (4.37)	-0.295 (0.34)	4.117 (4.96)	0.742	-0.128
<i>Panel B: Total quantities over period of analysis</i>							
1st	608.27	1.83	569.70	-38.57	6.34	118.50	-20.39
2nd	733.22	1.64	702.12	-31.10	4.24	54.06	-9.30
3rd	590.68	1.15	589.14	-1.54	0.26	17.59	-3.03
4th	537.28	0.98	497.43	-39.84	7.42	96.19	-16.55
5th	802.56	1.07	747.16	-55.40	6.90	132.91	-22.87
Overall mean	654.40	1.97	621.11	-33.29	5.09	83.85	-14.43

The table reports the results of our welfare analysis per consumer in each income quintile, both per week (Panel A, standard deviations in parentheses) and over our entire study period (Panel B). Column 1 reports the average implicit monetary subsidy (ΔM) from retailers to consumers in absolute terms during our analysis period, by income quintile. Column 2 relates it to the average household income in the quintile. The implicit subsidy received by households is further decomposed between surplus being transferred from retailers to consumers (ΔS , Column 3) and deadweight loss (DWL, Column 4). Column 5 reports the ratio between the DWL and the monetary transfer. Panel A reports the weekly mean value, while the Panel B shows the total transfer over the sample period. Column 6 reports the change in CO_2 emissions associated with the distortion in consumption behavior induced by fixed prices, and Column 7 reports the social cost of CO_2 emissions in Euro.

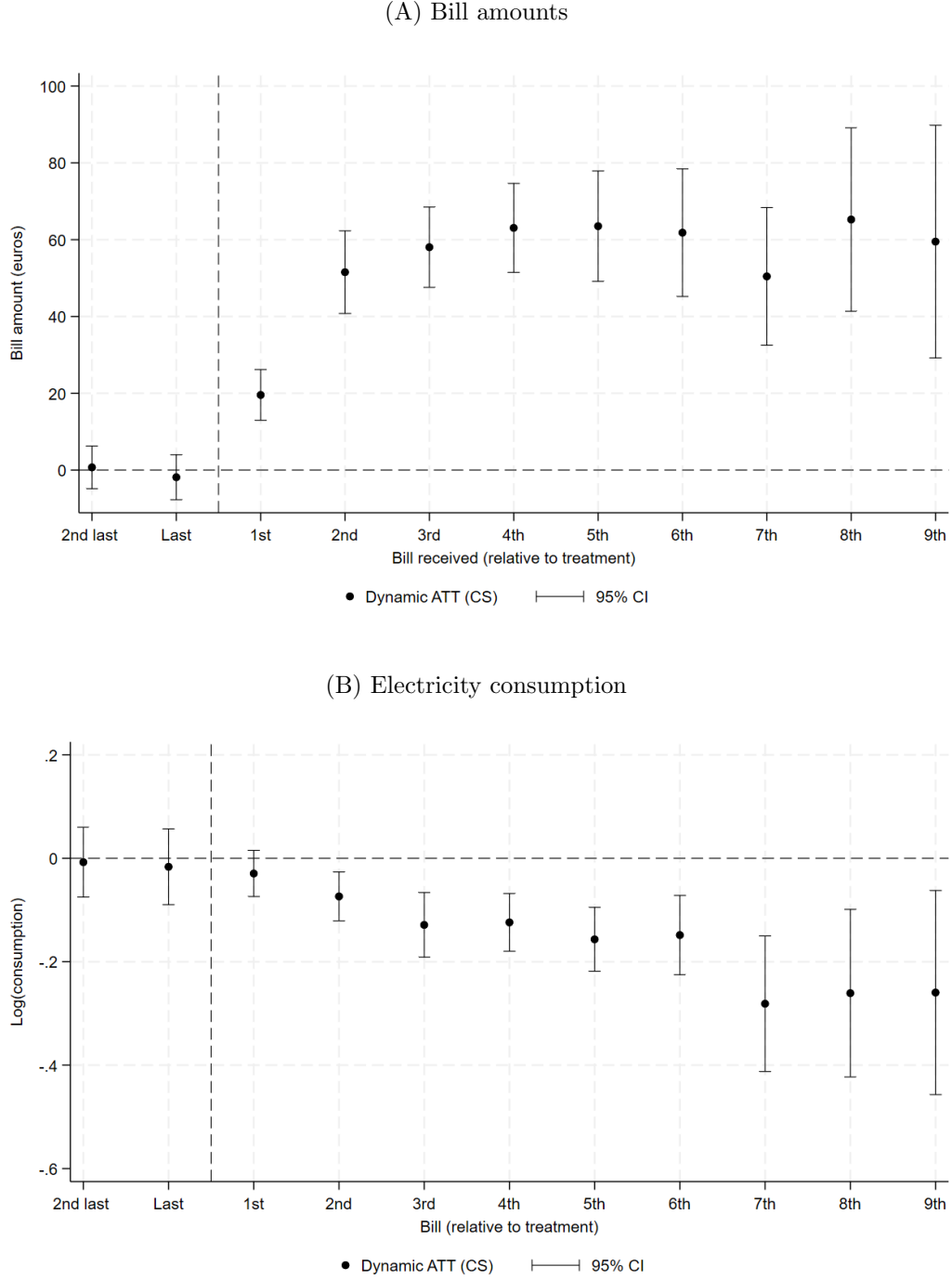
7 Figures

Figure 1: Time trend of PUN and price difference between fixed and variable schemes



Notes: the figure shows the strong fluctuations in the the Italian National Single Price (PUN) between 2021 and 2023 (upper-left panel) and the subsequent average gap between the price per kWh sustained by consumers in a fixed-price regime compared to consumers defaulted into PUN indexing on the day the default becomes effective (upper-right panel). Public interest for the situation in the energy market is measured using Google Trends data on three queries: rising bills (*caro bollette*), electricity price (*prezzo energia elettrica*), and commercial offers on electricity contracts (*offerte energia elettrica*). Google Trends normalizes data by dividing each data point by the total searches within its specific geographical area and time range, then scaling the results on a 0-100 scale. This allows for comparison of relative popularity between terms and locations, even if they have different total search volumes. A value of 100 represents the highest peak popularity for a given term within the chosen time frame, while 50 indicates half that peak, and 0 signifies insufficient data

Figure 2: Impact of default to indexed pricing on electricity consumption and bill amounts



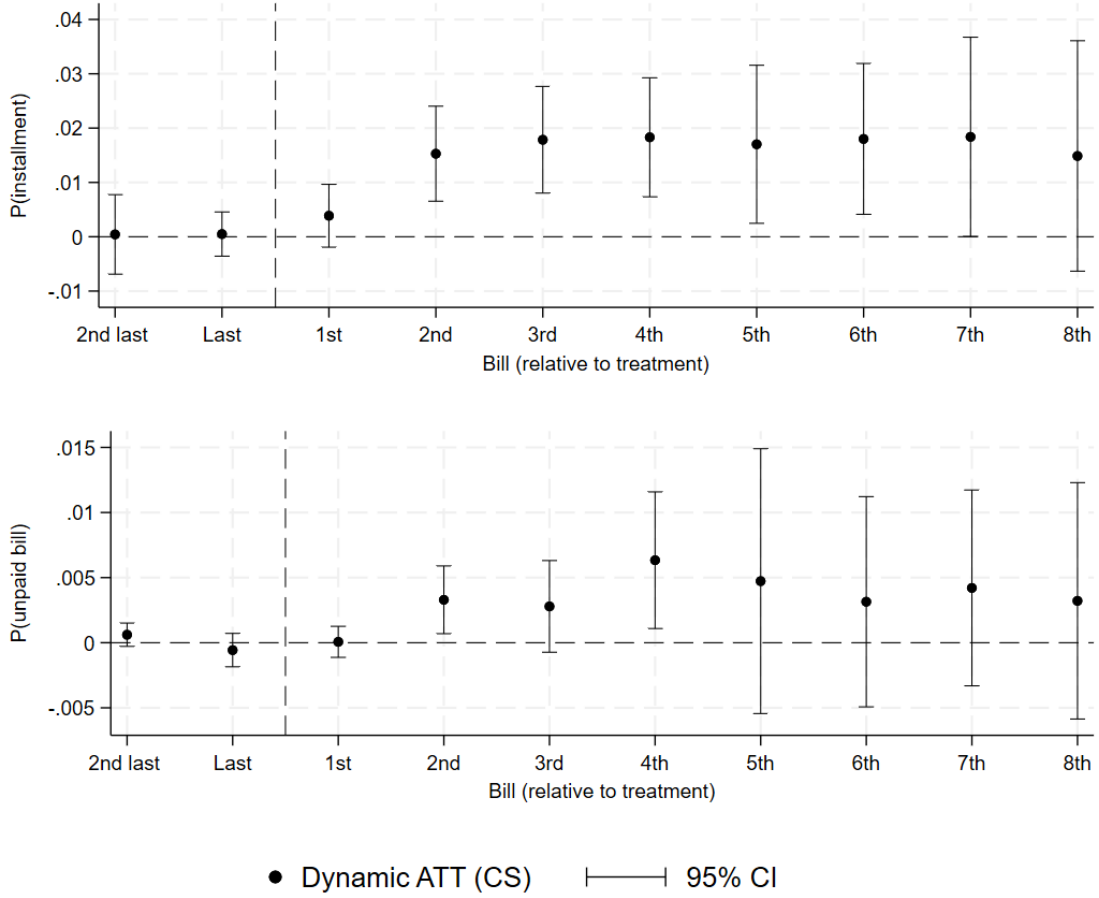
Notes: the figure plots the estimates for η_k coefficients in Equation 1 correcting for staggered treatment adoption.

Panel A: The outcome variable is the bill amount in euros. The figure shows the price signal received by consumers following the default, with a steep increase in bill amounts.

Panel B: The outcome variable is defined as the log transformation of consumption attributed to the earlier bill received, measured in kWh. The figure shows the adaptation of consumption levels after defaulting, taking place more slowly compared to the almost sudden jump in bill amounts.

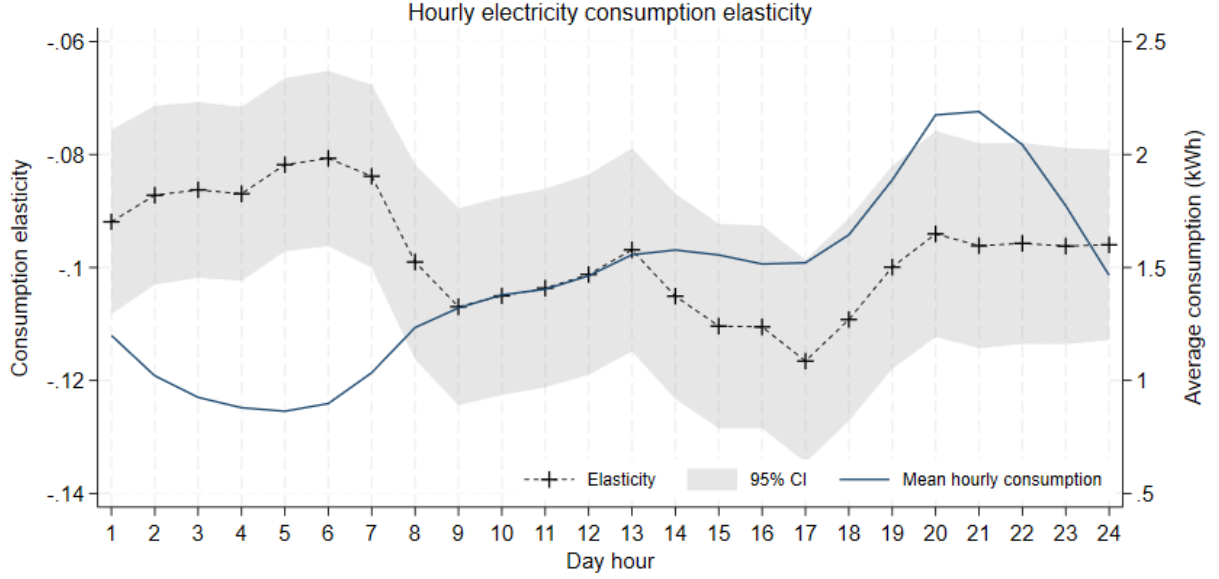
The estimates are obtained considering households regularly billed every two months. The specification includes contract fixed effects. To control for seasonality, it is added a fixed effect for each bill received after May 1st 2021. Standard errors are clustered at cohort level and bootstrapped.

Figure 3: Impact of default to indexed pricing on payment issues



Notes: the figure plots the estimates for η_k coefficients in Equation 1 correcting for staggered treatment adoption. The outcome variable in the panel above is defined as the probability of requesting an installment plan for a bill payment. In the panel below, the outcome variable is the probability that a bill is flagged as unpaid. The estimates are obtained considering households regularly billed every two months. The specification includes contract fixed effects. To control for seasonality, it is added a fixed effect for each bill received after May 1st 2021. Standard errors are clustered at cohort level and bootstrapped. The figure shows a small but significant increase in bills payment issues by consumers defaulted into indexed price contracts.

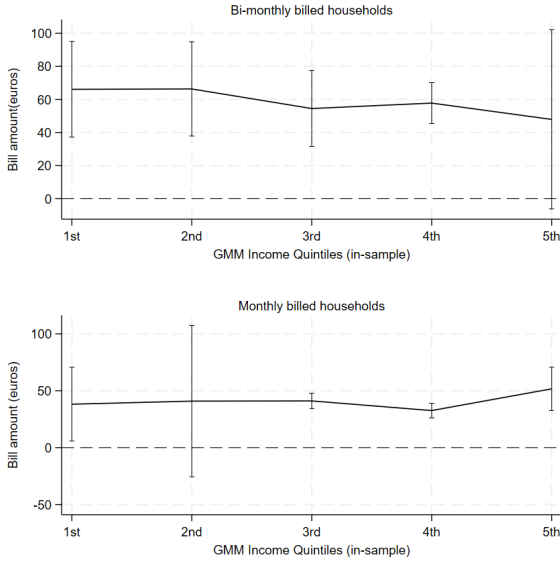
Figure 4: Impact of default to indexed pricing on weekly electricity consumption levels



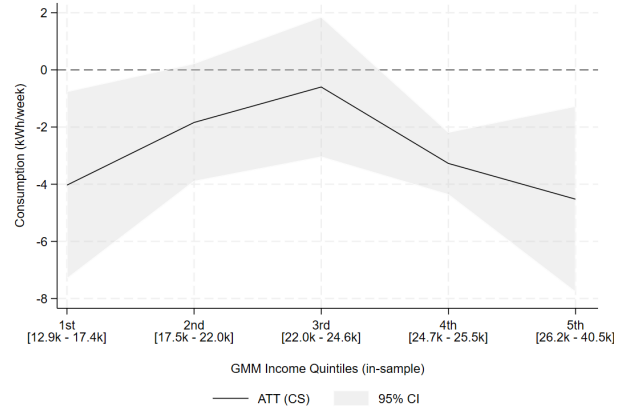
Notes: the figure plots consumption elasticity estimated at each hour of the day using our 2SLS model. The daily pattern of elasticities is plotted against mean hourly consumption, reported on the secondary axis. The specification includes contract and week-of-the-year fixed effects. Standard errors are clustered at cohort level. The figure shows slow adaptation to new electricity prices.

Figure 5: Heterogeneous treatment effect across income quintiles

(a) Bill amount

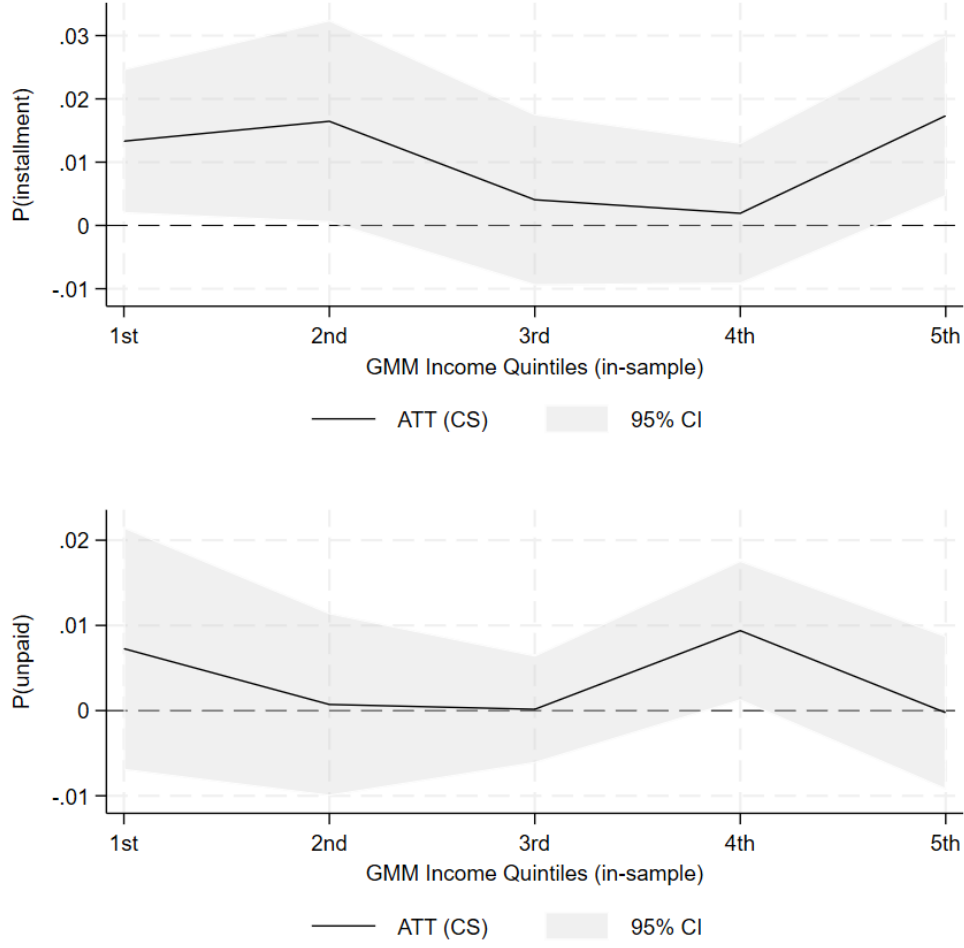


(b) Consumption (kWh)



Notes: the figure reports the ATT of defaulting into indexed pricing on bill amounts (a) and electricity consumption (b) for separate income quintiles in our sample. Income quintiles are computed using the estimated household-level income (GMM). Standard errors are clustered at the cohort level and bootstrapped. The regression includes household and year-week fixed effects.

Figure 6: Heterogeneous treatment effect on irregular payments across income quintiles



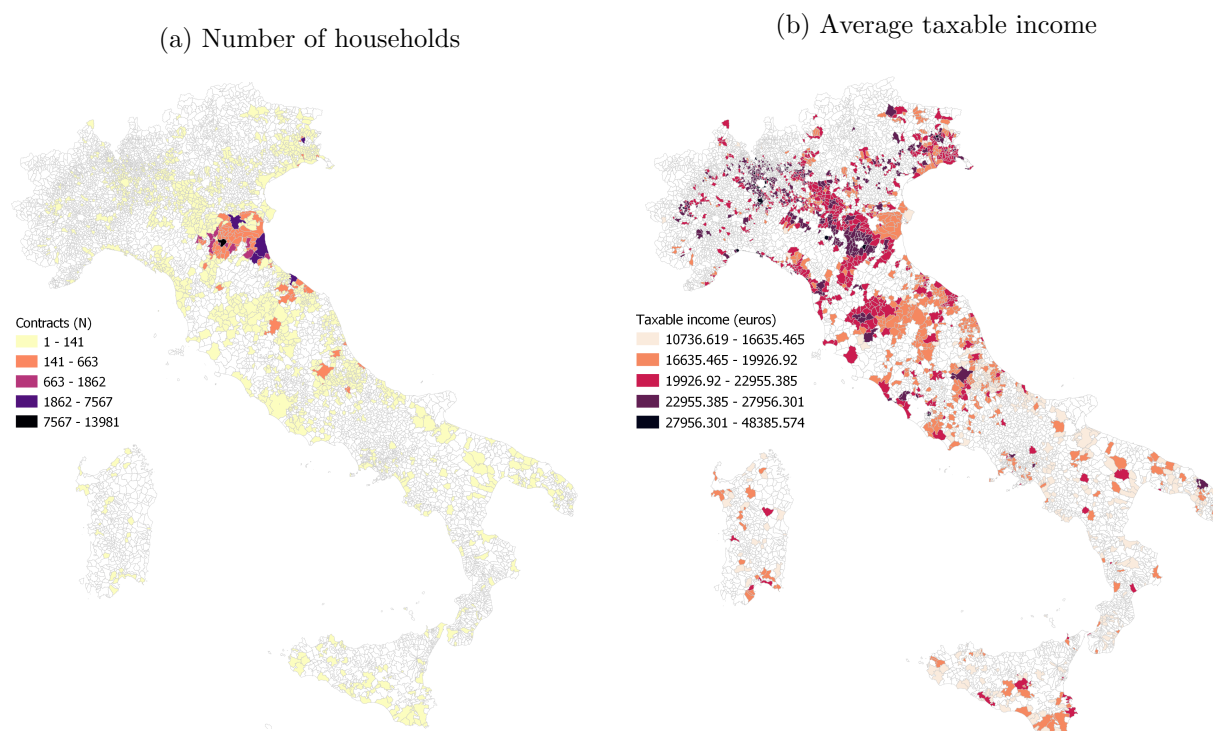
Notes: the figure reports the ATT of defaulting into indexed electricity pricing on the probability of requesting installment payment (top) and missing electricity payments (bottom) for separate income quintiles in our sample. Income quintiles are computed using the estimated household-level income (GMM). The estimates are obtained considering households regularly billed every two months. The specification includes contract fixed effects. To control for seasonality, it is added a fixed effect for each bill received after May 1st 2021. Standard errors are clustered at cohort level and bootstrapped.

Online Appendix

A Data sources

A.1 Geographical coverage

Figure A1: Geographical distribution of consumers in the sample



Notes: Figure (a) plots the geographical distribution of customers served by the provider in our sample at the municipality level. Figure (b) shows the geographical distribution of average taxable income across municipalities served by the provider. Taxable income is computed taking the median of each income bin and weighting by the number of taxpayers in the bin.

A.2 Comparing samples of 2GSM consumers

From our original sample of 2GSM consumers, we define several subsamples to study different outcomes of the default to indexed pricing. Table A1 compares the characteristics of households across these samples.

Table A1: Summary statistics across samples of 2GSM consumers

	All 2GSM	With billing	Bi-monthly (regular billing)	Monthly (regular billing)
N. contracts	89,931	61,845	15,177	11,660
Consumption (kWh)				
◦ Daily	4.98 (3.49)	5.23 (3.46)	4.91 (3.37)	5.76 (3.77)
◦ Weekly	34.85 (31.2)	36.17 (30.79)	33.52 (31.17)	39.90 (32.19)
Contracted power (kW)	3.20 (0.74)	3.20 (0.72)	3.17 (0.67)	3.31 (0.86)
Building size (m ²)*	79.23 (25.33)	79.20 (25.1)	79.36 (24.08)	80.21 (26.61)
Household members*	1.97 (1.05)	1.98 (1.04)	1.95 (1.04)	1.96 (1.04)
Bill amount	81.63 (80.82)	81.23 (74.34)	96.56 (75.1)	58.10 (48.12)
P(flat rate)	0.96 (0.19)	0.96 (0.19)	0.97 (0.18)	0.95 (0.21)
P(email bill delivery)	0.48 (0.5)	0.46 (0.5)	0.39 (0.49)	0.65 (0.48)
Fixed price (euros/kWh)	0.08 (0.004)	0.08 (0.004)	0.08 (0.003)	0.08 (0.003)
Variable price (euros/kWh)	0.21 (0.118)	0.21 (0.118)	0.21 (0.12)	0.21 (0.117)
Contract duration (months)	28.91 (3.95)	28.94 (3.85)	28.62 (4.46)	29.10 (3.48)

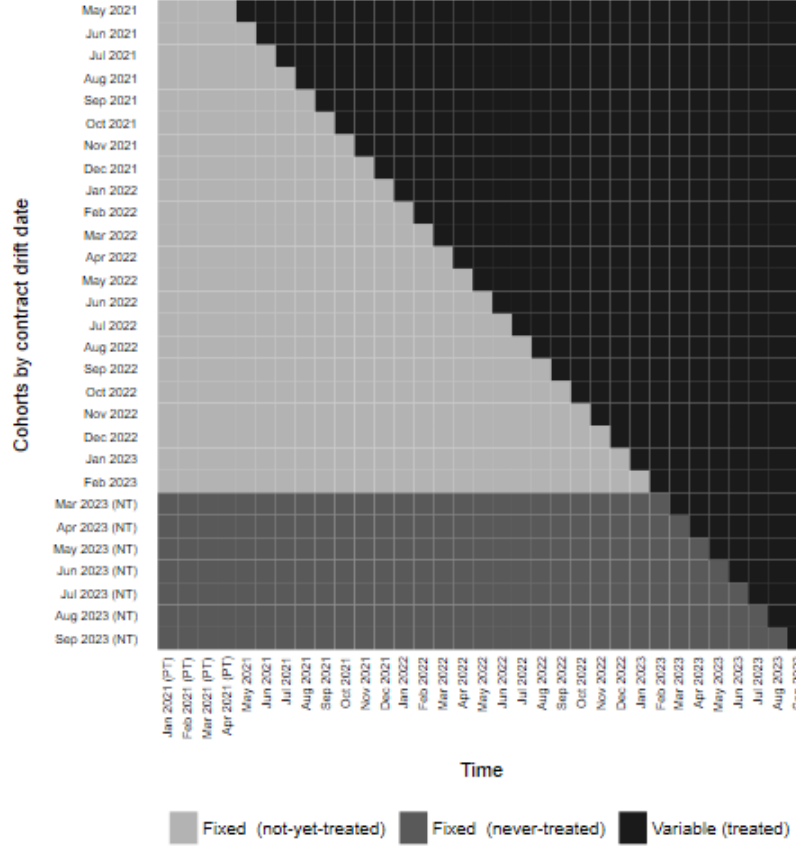
Notes: The table reports descriptive statistics across our samples of 2GSM households. The table reports mean values and standard deviations in parentheses. Statistics for time-varying variables are computed over the period from January 2021 to March 2023, or—from the household-specific default date to March 2023 in the case of variable contract prices. Statistics for variable prices are computed using all household-day observations in which a contract has transitioned from a fixed- to an indexed-price regime.

* Information on dwelling size and household occupancy is available for approximately 25% of the sample.

Overall, households across samples are quite comparable. The main differences concern contracted power and consumption, which are slightly higher among consumers who are regularly billed monthly compared to those billed bi-monthly. We produce our main results for the two groups of regularly billed households, showing rather similar results.

A.3 Treatment cohorts

Figure A2: Treatment status of households by month of contract default



Notes: the figure depicts the treatment status of households classified based on the month of the year in which electricity price indexing becomes effective and the contract defaults into a variable price regime. Never-treated cohorts (NT) maintain a fixed price per kWh throughout the entire period considered. Months between January and April 2021 are included to estimate pre-treatment effects (PT) also for the never-treated cohorts.

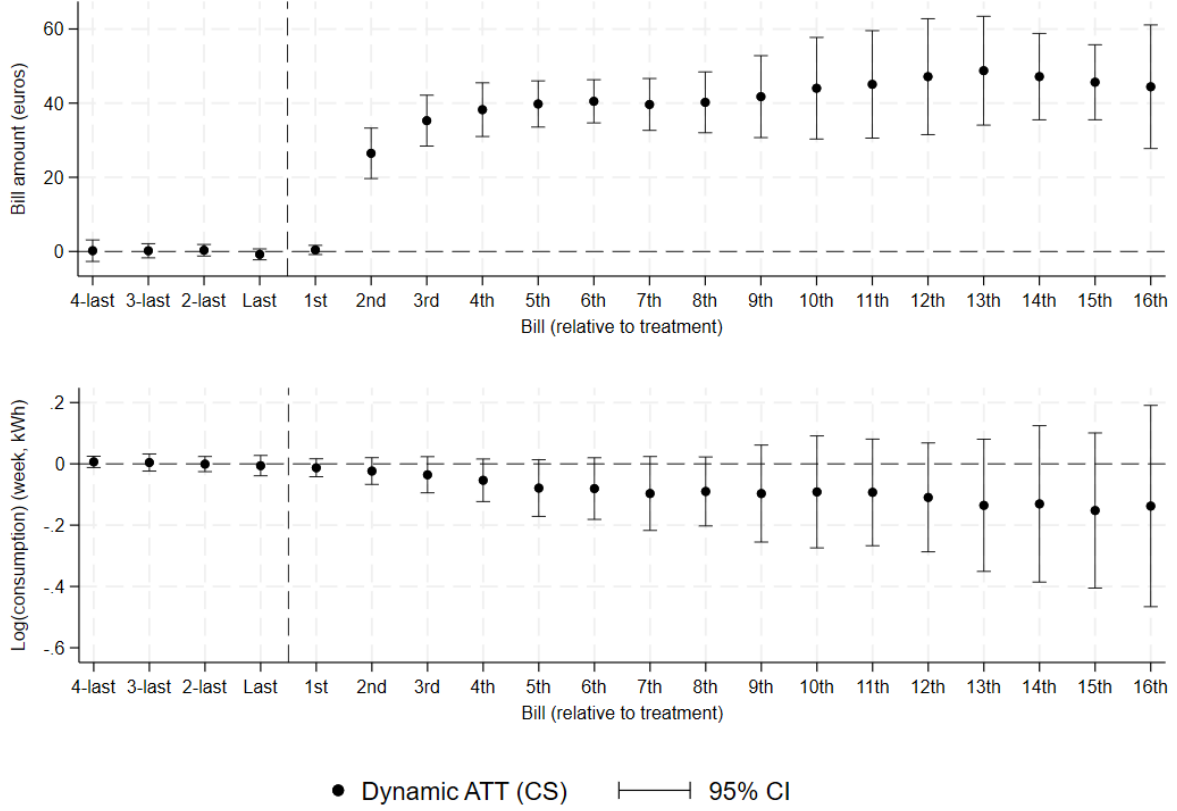
B Additional Results

B.1 Effects of defaulting: monthly consumers sample

In Section 3, we estimate the impact of defaulting into indexed electricity price on a series of outcomes defined at the bill level: consumption (Figure 2), electricity spending (Figure 2), late and missing payments (Figure 3). When the outcome is defined at the bill level, it is important to identify consumers with the same billing cycle, to avoid comparing moments too far away in time. For instance, consider the case where consumers with one-month and two-month billing cycles are compared without distinction. The outcome that refers to the n -th bill after being treated will be observed n months after treatment for monthly billed consumers, whereas for those billed every other month, it will be observed $2n$ months after treatment. Assuming the

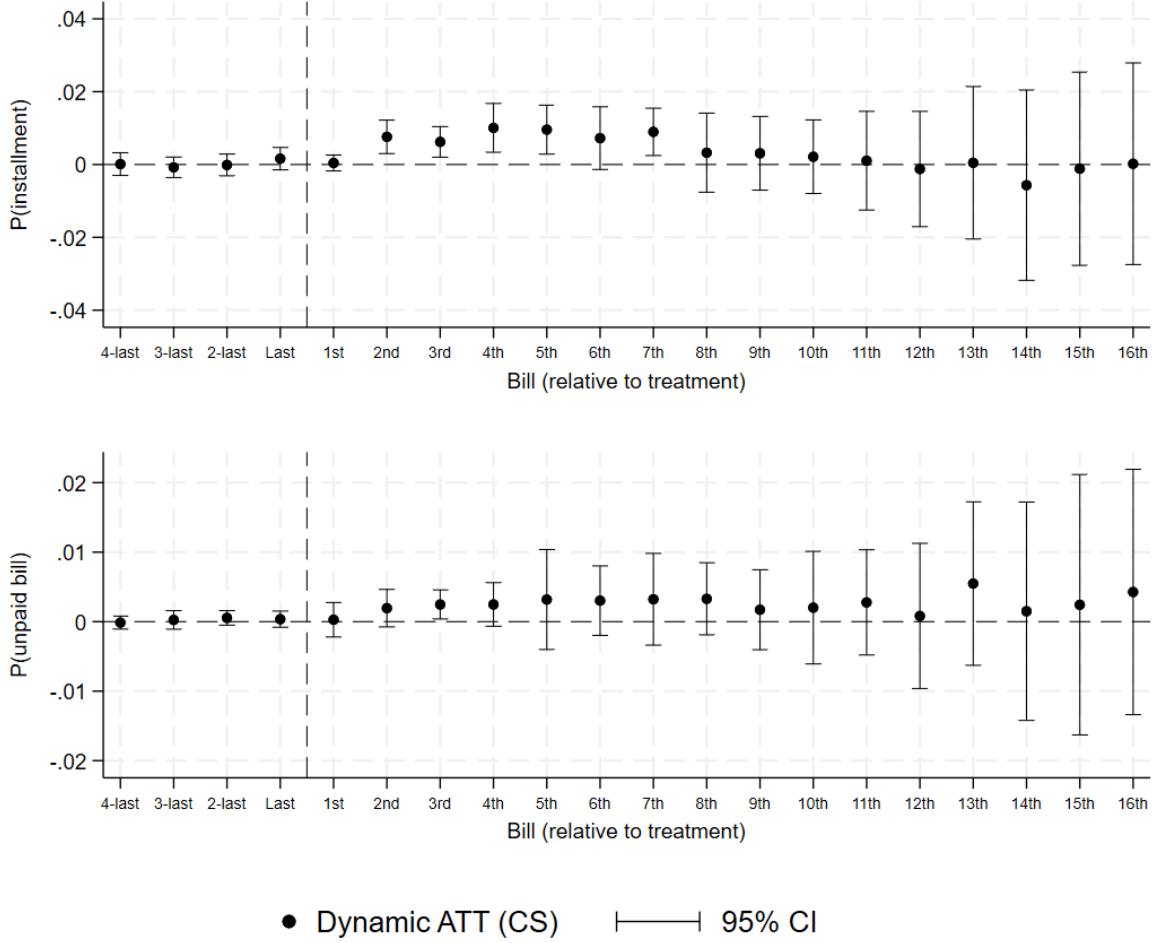
existence of a dynamic effect, with households gradually adapting to the shock, allowing this comparison may confound the results. In Section 3, we focused on consumers billed every two months, since this is the billing frequency most prevalent in the sample. Here, we report the same results estimated using the sample of households billed monthly.

Figure B1: Impact of default to indexed pricing on consumption and spending (monthly cycle)



Notes: the figure plots the estimates for η_k coefficients in Equation 1 correcting for staggered treatment adoption. The outcome variable in the above panel is defined as the log transformation of consumption attributed to the earlier bill received, measured in kWh. In the panel below, the outcome variable is the bill amount in Euro. The estimates are obtained considering households regularly billed every month. The specification includes contract fixed effects. To control for seasonality, it is added a fixed effect for each bill received after May 1st 2021. Standard errors are clustered at cohort level and bootstrapped.

Figure B2: Impact of default to indexed pricing on inefficient payment instances (monthly cycle)



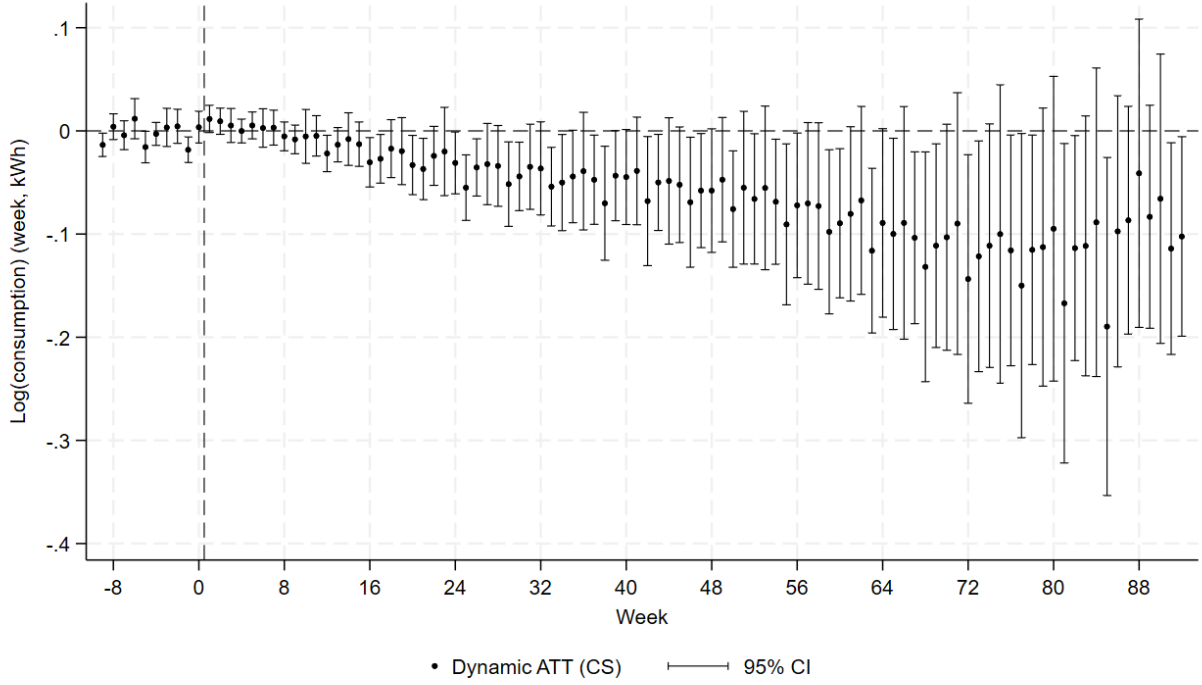
Notes: the figure plots the estimates for η_k coefficients in Equation 1 correcting for staggered treatment adoption. The outcome variable in the panel above is defined as the probability of requesting an installment plan for a bill payment. In the panel below, the outcome variable is the probability that a bill is flagged as unpaid. The estimates are obtained considering households regularly billed every month. The specification includes contract fixed effects. To control for seasonality, it is added a fixed effect for each bill received after May 1st 2021. Standard errors are clustered at cohort level and bootstrapped.

Despite reduced significance, possibly due to the reduced number of households considered, Figure B1 and Figure B2 show very similar trends to the ones described in Section 3 for consumers billed every two months. After defaulting into indexed prices gradually reduce their consumption. The adaptation takes place with consistent lag with respect to the signal received through energy bills, which almost instantly increase. A similarly small increase in the probability of paying via installment and missing on bill payments is also detected, with the effect fading over time. The results support the robustness of our findings.

B.2 Effect of indexed price defaulting: weekly consumption

The availability of high-frequency consumption data enables an analysis of the effects of defaulting to indexed-price contracts at a finer temporal scale than the billing cycle. Equation 1 is estimated using weekly consumption as the unit of analysis (Figure B3).

Figure B3: Impact of default to indexed pricing on weekly electricity consumption levels

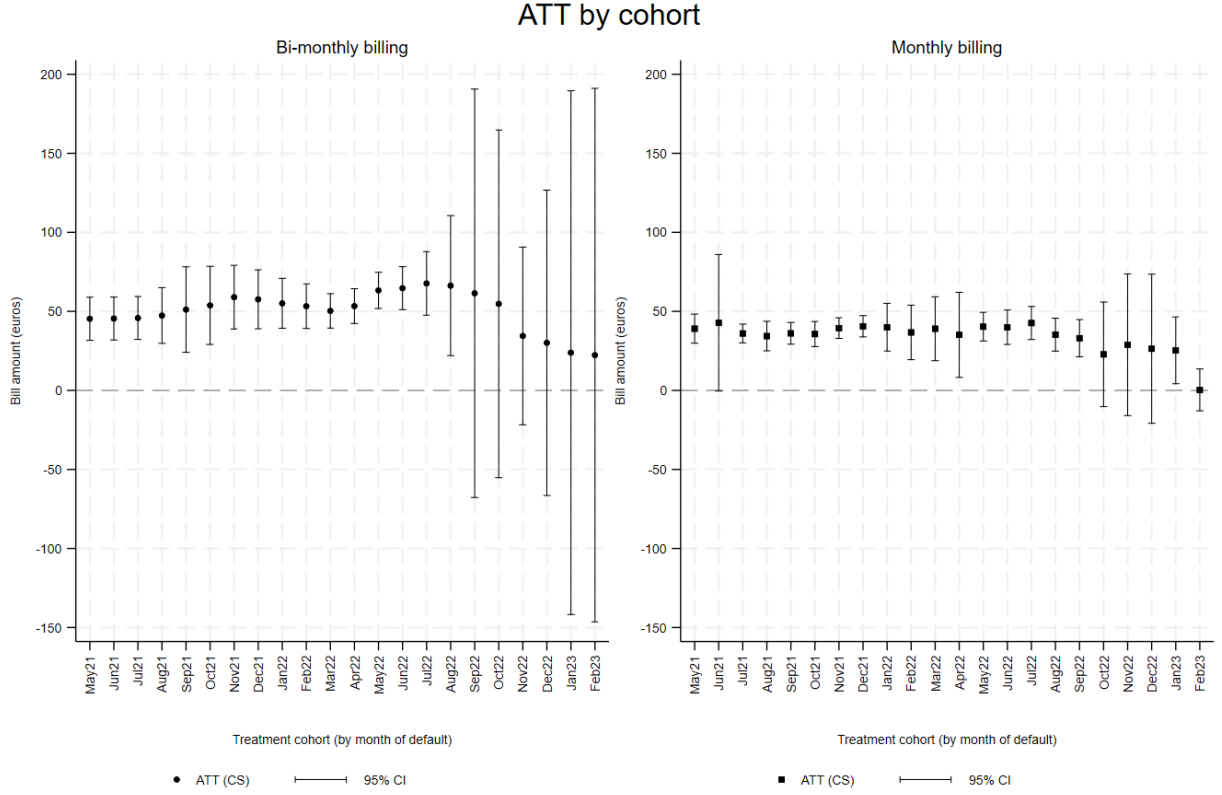


Notes: the figure plots the estimates for η_k coefficients in Equation 1 correcting for staggered treatment adoption. Each coefficient represents a specific week relative to the onset of the contractual default into indexed electricity pricing (Week 0). The outcome variable is defined by the log transformation of weekly consumption in kWh. The specification includes contract and week-of-the-year fixed effects. Standard errors are clustered at cohort level and bootstrapped. The figure shows slow adaptation to new electricity prices.

B.3 Treatment effect by cohort

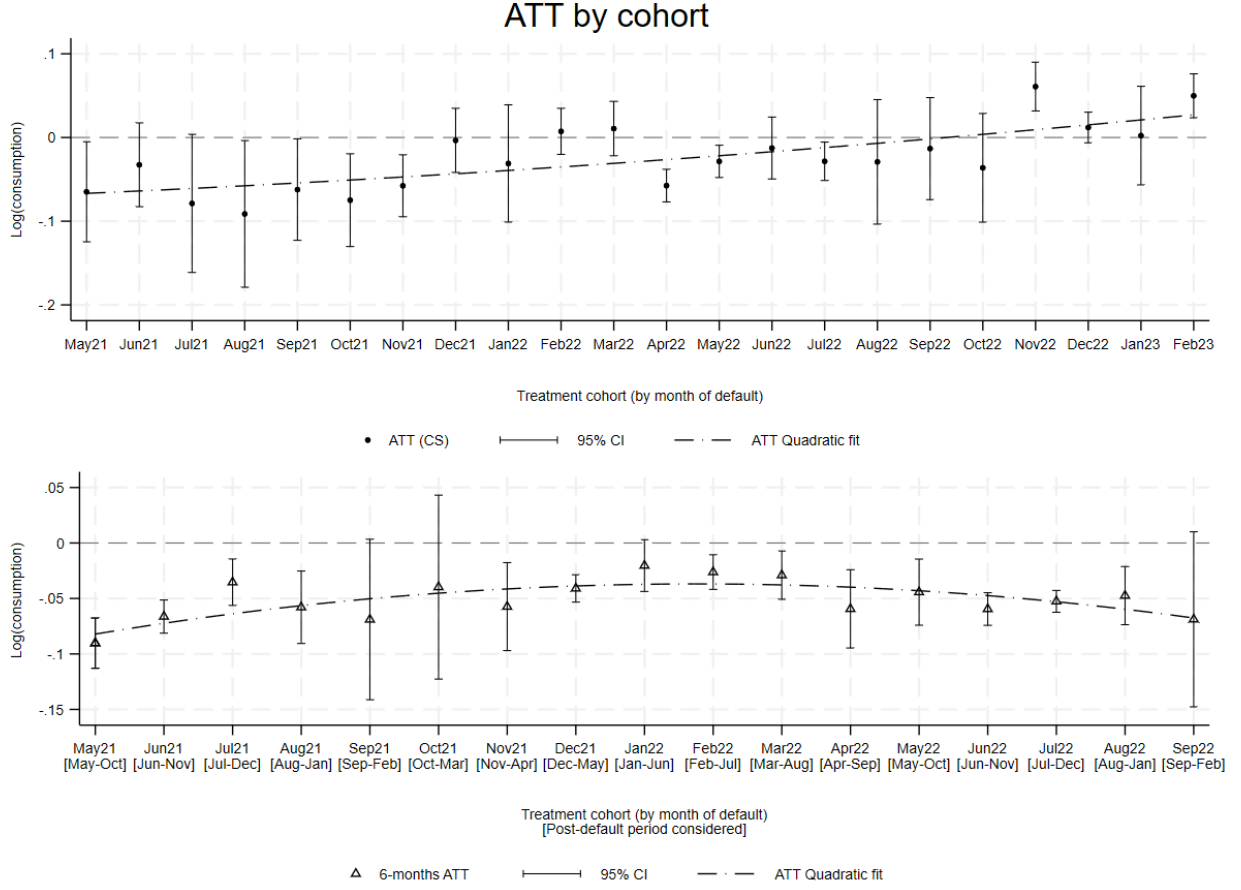
The estimates of the ATT from Equation 2 reported in Table 2 consider the average effect across all treatment cohorts. We decompose the effect to investigate the existing differences across cohorts. The results are reported in Figures B4 and B5.

Figure B4: Effect of defaulting (ATT) on bill expenditure by cohort



Notes: the figure reports the ATT of defaulting to indexed-price contracts on bill expenditure by treatment cohort, identified by the year-month of defaulting into an indexed price regime. The ATT is computed using the entire post-treatment period and applying the correction proposed by Callaway & Sant'Anna (2021). The results are presented separately for households billed every two months (left panel) and every month (right panel). As the surge in wholesale prices is progressively absorbed by the market, the effect on electricity spending declines. For later cohorts, the censoring in our sample period increases uncertainty around point estimates. Standard errors are clustered at cohort level and bootstrapped.

Figure B5: Effect of defaulting (ATT) on electricity consumption by cohort

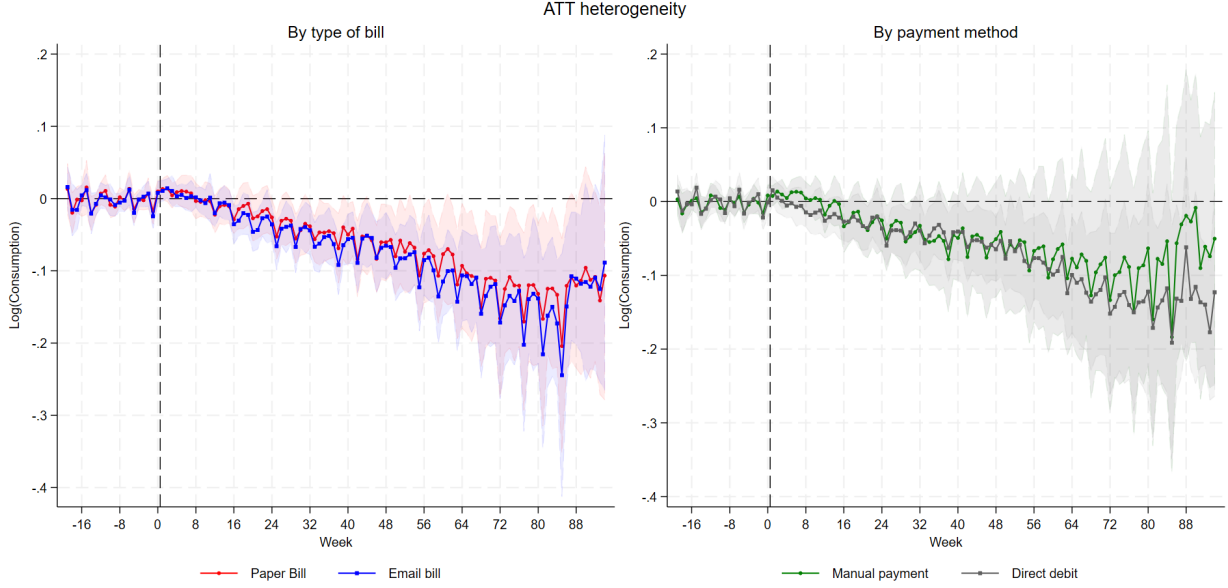


Notes: the figure reports the ATT of defaulting to indexed-price contracts on log transformed electricity consumption by treatment cohort, identified by the year-month of defaulting into an indexed price regime. In the above panel, the ATT is computed using the entire post-treatment period and applying the correction proposed by Callaway & Sant'Anna (2021). For early-treated cohorts, observed for a longer period of time, the contraction in consumption is generally higher than for later cohorts. In the bottom panel, the treated cohort is included alone together with the control group, and the time period is restricted to six months before and after households in the cohort default. No correction is required in this case, hence the coefficient is obtained via two-way fixed effects. When the same post-treatment period is observed for all cohorts, the ATT a flatter trend, moderately decreasing for cohorts defaulting in winter 2022. Standard errors are clustered at cohort level and bootstrapped.

B.4 Heterogeneity by type of bill and payment method

We assess heterogeneity in the estimated ATT by dividing the sample into consumers who receive their bills by paper mail or email, and those who pay manually or via direct debit, as both the billing format and the payment method may influence the salience of the price signal following the default.

Figure B6: Effect of defaulting (ATT) by type of bill and payment method

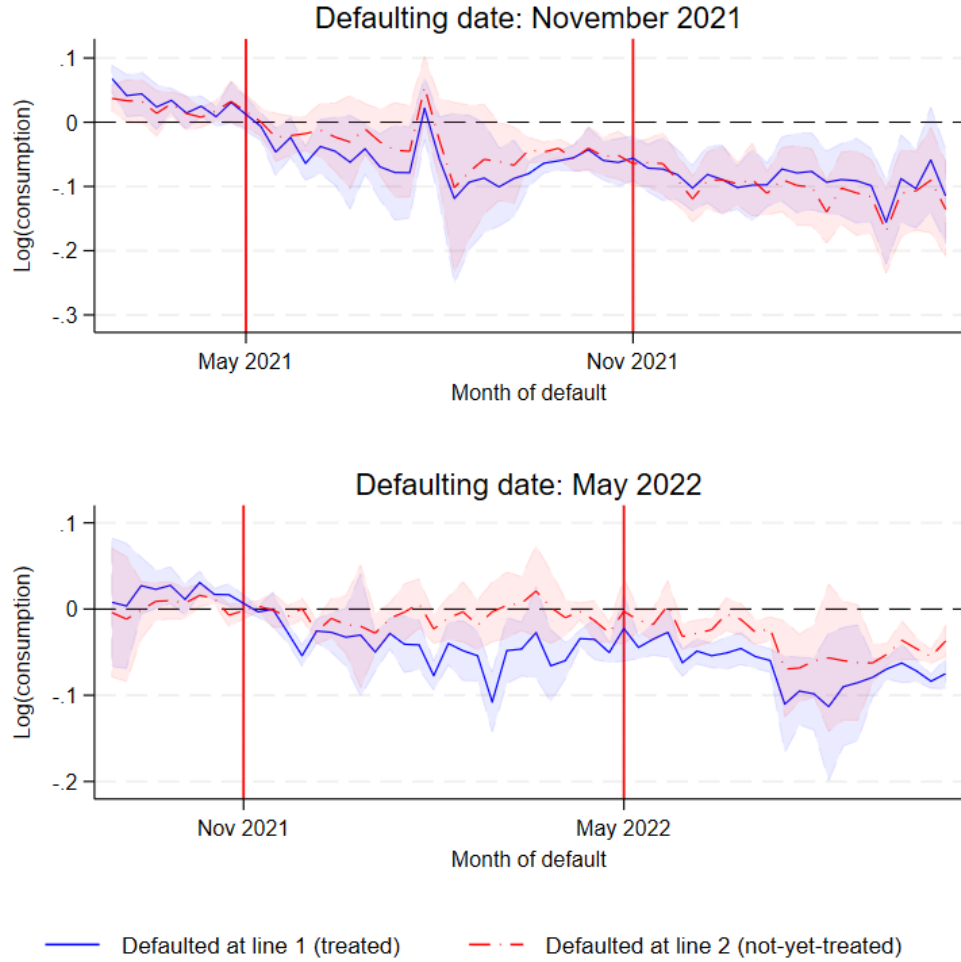


Notes: the figure plots the estimates for η_k coefficients in Equation 1 correcting for staggered treatment adoption. The sample is split by type of bill (paper mail vs. email, left panel) and payment method (manual vs. direct debit, right panel). Each coefficient represents a specific week relative to the onset of the contractual default into indexed electricity pricing (Week 0). The outcome variable is defined by the log transformation of weekly consumption in kWh. The specification includes contract and week-of-the-year fixed effects. Standard errors are clustered at cohort level and bootstrapped. The figure shows no statistically significant difference across these household characteristics.

B.5 Anticipation effect

Anticipatory behavior approaching the end of fixed term arrangements can be quantified by comparing not-yet-treated cohorts with the control group of never-treated households as the time of defaulting approaches. In Figure B7, we compare six months prior to treatment for two cohorts, contracts defaulting in November 2021 and contracts defaulting in May 2022. The post-treatment effect for cohorts treated in May 2021 and November 2021 respectively is also included for comparison.

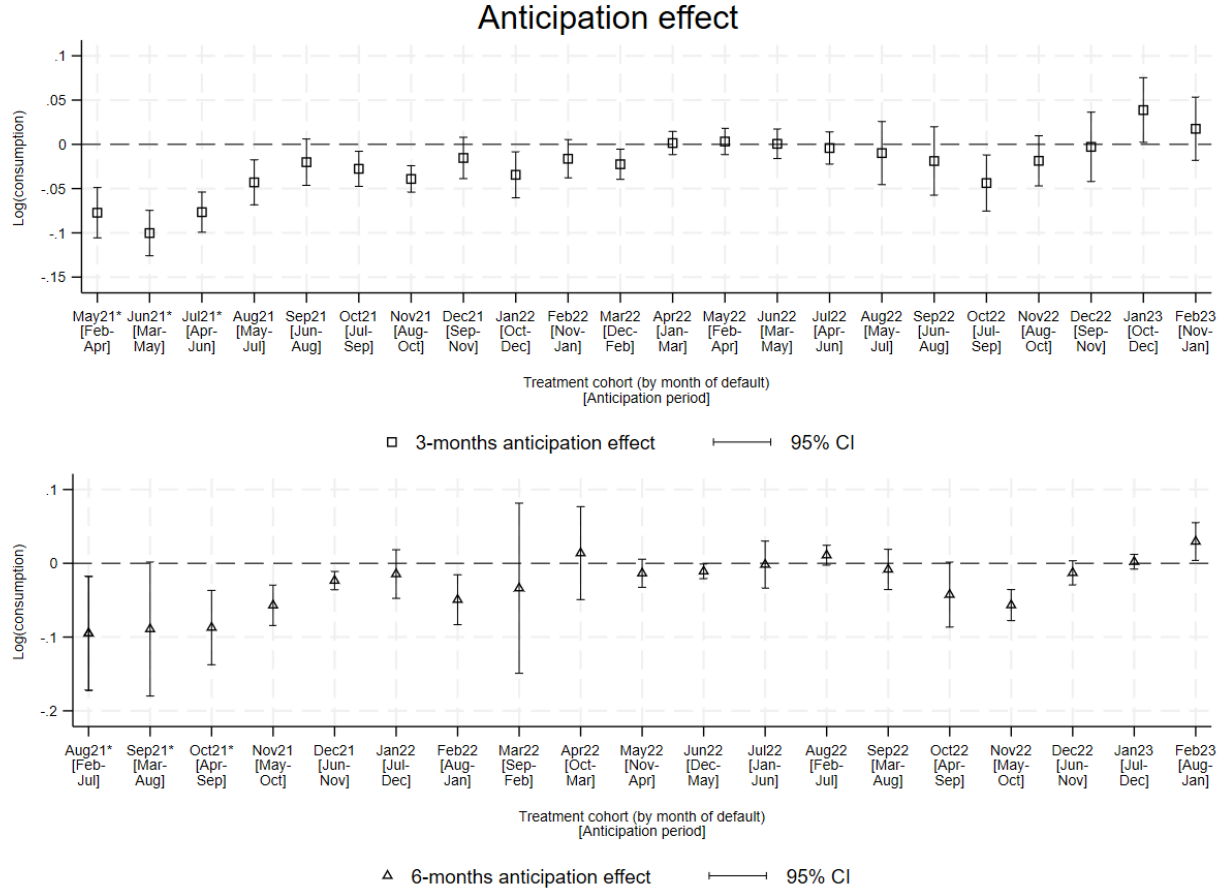
Figure B7: Contract default anticipation — example



Notes: The figure plots estimates from Equation 1 using weekly data. The reference period is the first week of May 2021 (top panel) and the first week of November 2021 (bottom panel), and the reference group are never-treated cohorts. The blue line captures the effect of defaulting on the cohort of consumers whose contract changes at the first red line (top: May 2021; bottom: Nov 2021). The red line captures the trend in consumption during the same days of cohorts defaulting six months later (top: Nov 2021; bottom: May 2022). As such, the red line captures anticipatory behavior from households ahead of their contract default. Standard errors are clustered at cohort level and bootstrapped. 95% confidence interval are reported.

The average anticipation effect across pre-treatment periods can be obtained from estimating Equation 6 for each treated cohort. Figure B8 plots the estimates for ξ by cohort, identified by the month of default to indexed prices.

Figure B8: Effect of defaulting (ATT) by cohort



Notes: the figure reports estimates of ξ from Equation 6, obtained separately from each treatment cohort. A window of 3 months (top panel) and 6 months (bottom panel) before defaulting is considered for anticipation. The reference group are never-treated households, and the reference period is the four months before the anticipation window starts.

*For these cohorts, the reference period is shorter than four months, as our observation period starts in January 2021. Standard errors are clustered at cohort level and bootstrapped. 95% confidence interval are reported.

Anticipation of default appears strong in the initial cohorts, but quickly fades in later cohorts, oscillating without a distinct pattern.

The estimates of the ATT of defaulting to indexed price contracts are then repeated removing those cohorts for which the anticipation effect is statistically significant.

Table B1: Effect of defaulting on energy consumption - non-anticipating cohorts

$\log(\text{Consumption (Kwh)})$	3-month anticipation				6-month anticipation			
	(1) TWFE	(2) TWFE	(3) dC-DH DiD	(4) CS DiD	(5) TWFE	(6) TWFE	(7) dC-DH DiD	(8) CS DiD
Treat $\times$ Post	-0.045*** (0.0132)	-0.047*** (0.0032)	-0.028 (0.0423)	-0.047** (0.0227)	-0.038*** (0.0104)	-0.041*** (0.0028)	-0.039 (0.0399)	-0.06** (0.0281)
N	4,077,992	4,076,929	3,050,333	4,077,992	4,948,771	4,947,615	4,319,198	4,948,771
Year-by-week FE	Y	Y	Y	Y	Y	Y	Y	Y
Contract FE		Y				Y		
Cohort FE	Y		Y	Y	Y		Y	Y
Controls	Y		Y	Y	Y		Y	Y
Cluster level	cohort	contract	cohort	cohort	cohort	contract	cohort	cohort

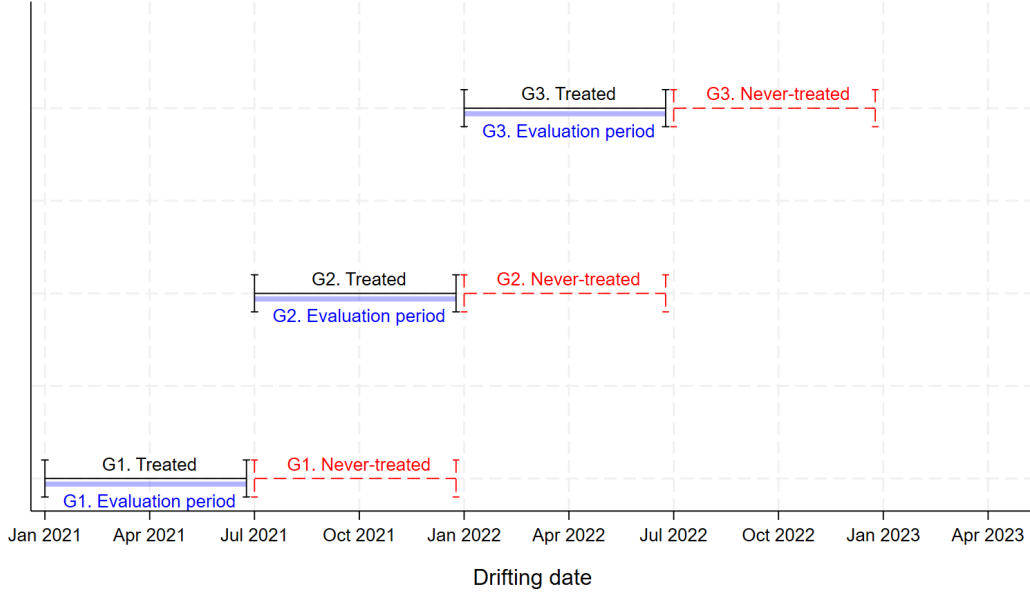
Notes: the table shows the estimates of a static staggered DiD model (Equation 2) on a restricted sample of households. Specifically, all cohorts showing statistically significant anticipatory behavior considering a pre-treatment period of three (Columns 1 to 4) and six months (Columns 5 to 8), as shown in Figure B8, have been removed. Two-way fixed effects estimates (TWFE), and estimates corrected for staggered implementation according to the strategy developed by De Chaisemartin & d’Haultfoeuille (2020) and Callaway & Sant’Anna (2021) are compared. Controls include household average yearly, summer, and winter consumption. Standard errors (in parentheses) are clustered at the level indicated in the table. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.6 Attrition and competitive switching: detailed strategy and results

Attrition in our treatment cohorts—specifically, households that switch to a different provider—may raise concerns, particularly if competitive switching is correlated with unobserved consumer characteristics that also influence consumption and payment behavior. We explore this potential source of bias by first estimating the impact of contract drift on the probability that a consumer switches either to another provider or to a different contract offer.

The probability of competitive switching likely increases over time for several reasons. The initial effort involved in selecting a new plan (which may reduce switching in the short run), the limited initial familiarity with market options and the provider’s service, and the increasing availability of alternative offers over time may contribute to increasing the likelihood of switching as the time in the variable price contract increases. Moreover, in competitive markets, providers often introduce promotional offers to attract customers and gain market share, further influencing switching behavior. However, this poses a concern about the empirical strategy used to study consumption and payment outcomes. Our never-treated group is composed of households who entered their fixed-term contracts later than the treatment cohorts. This, in turn, may violate the parallel trend assumption, as the counterfactual probability of switching provider and/or offer in the absence of default may not be the same between the two groups. To overcome this problem, we focus on treated cohorts and use not-yet-treated households as an appropriate counterfactual. We proceed as follows. We split the sample into six-month windows (W). For each window, we look at households who default into indexed price contracts during the six months. These are the treated households. The control group is instead now defined as not-yet-treated consumers who will receive treatment in the following six-month window. Given the time span of our analysis, we can define three windows. Figure B9 visually clarifies the strategy.

Figure B9: Sample construction: competitive switching following default



Notes: the figure shows how treatment and control groups are defined within each window (W). We estimate the ATT of defaulting into indexed price on competitive switching probabilities according to Equation 2. Households whose long-term fixed price arrangements (24 months or more) expire within six months of each other are more likely to share similar baseline incentives for competitive switching.

Households in each window default to a variable price at most 12 months apart, which helps preserve the parallel trend assumption. The strategy relies on the hypothesis that treatment and control cohorts within each window W share similar incentives to switch provider and offer in the absence of treatment. Since treatment adoption remains staggered within each window, we correct for cohort-time potential heterogeneity in treatment effects. The results in Appendix Table B2 show that consumers are unlikely to switch provider or commercial offer. At any given bill, the average probability of a switch oscillated between .01% and .03% for the first window, increasing to 0.1% to 0.3% in the following windows. However, when fixed price arrangements cease, households become more active. The increase in the probability of switching provider (commercial plan) after the drift is positive and statistically significant in most combinations of window and billing frequency, oscillating between 0.6 (0.6) percentage points and 1.6 (1.5) percentage points. The treatment effect is stronger in the second half of 2021, when the first jump in the PUN took place. This may be explained by rising electricity prices prompting a faster reaction in more active and attentive households, whereas more passive consumers tend to remain in their newly indexed plan even as prices continue to rise.

Table B2: Impact of indexed price defaulting on contract and provider choice

	W1. Jan21 - Dec21		W2. Jul21 - Jun22		W3. Jan22 - Dec22	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT - Switch provider	0.006*** (0.002)	0.009 (0.007)	0.016*** (0.002)	0.012*** (0.001)	-0.002 (0.011)	0.009** (0.004)
Billing frequency	bimonthly	monthly	bimonthly	monthly	bimonthly	monthly
Mean Prob	0.0003	0.0001	0.002	0.001	0.003	0.001
SD Prob	0.017	0.01	0.04	0.03	0.052	0.037
N	55,964	65,720	105,181	164,213	73,667	132,472
ATT - Switch offer	0.002 (0.004)	0.01*** (0.004)	0.015*** (0.005)	0.008*** (0.003)	0.007 (0.007)	0.006** (0.003)
Billing frequency	bimonthly	monthly	bimonthly	monthly	bimonthly	monthly
Mean Prob	0.0001	0.0001	0.002	0.001	0.003	0.001
SD Prob	0.012	0.012	0.042	0.026	0.052	0.033
N	55,964	65,720	105,181	164,213	73,667	132,472

Notes: the table reports the ATT on competitive switching behaviour of households in the six months after defaulting into indexed price regimes, correcting for staggered treatment adoption. The estimates are obtained considering households regularly billed. The specification includes contract fixed effects. To control for seasonality, it is added a fixed effect for each bill received after January 1st 2021. Standard errors are clustered at the cohort level and bootstrapped. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

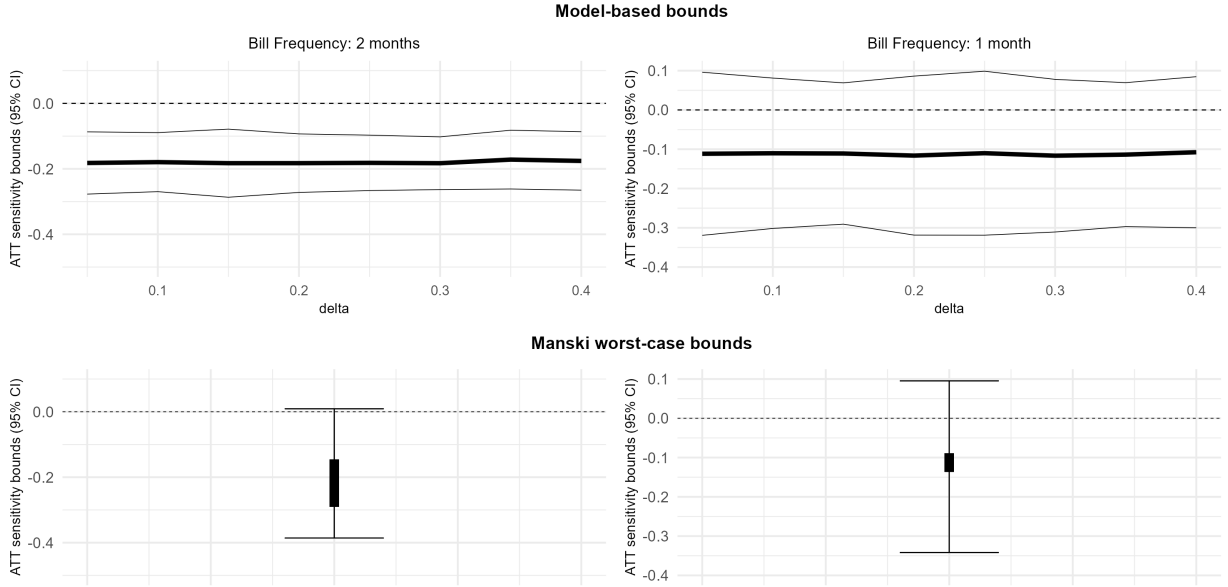
The small absolute increase in competitive switching behavior suggests that retention remains high following the contract drift. However, the observed pattern may indicate that treatment may induce selective attrition in our sample based on unobservable household characteristics—such as market knowledge or risk aversion—that could also influence consumption choices. To this aim, we test the robustness to attrition of our estimated ATT of indexed price defaulting on consumption levels using a bounding and sensitivity strategy. We calculate two sets of nonparametric bounds to our treatment effect. The first is a set of worst-case bounds in the spirit of Manski (1990). Bounds are calculated by trimming the treated and control groups. The lower bound is set by assuming that all treated dropouts had the worst possible outcome, whereas the control dropouts had the best, and vice versa for the upper bound. This is a standard assumption-free methodology, easily applicable to our staggered treatment adoption design. We also calculate a second set of model-based bounds using a methodology that builds on the contribution of Ichino et al. (2008). We refer to a latent/unobserved variable U that affects both attrition and outcome. We can define $p_1 = P(U = 1|T = 1)$ as the probability of such an unobserved factor being present in the treated group ($T = 1$), and $p_0 = P(U = 1|T = 0)$ the same probability in the control group. We can then assume U to have a linear additive effect on the outcome Y , such that:

$$Y^{\text{true}} = Y - \delta \cdot U$$

Based on p_1 , p_0 , and δ , we can compute ATT bounds that account for simulated attrition bias. We assume the incidence of U in both groups is $U \sim \text{Bernoulli}(p)$, where $p = p_1$ for the treated and $p = p_0$ for the control households. We then estimate the new ATT on the simulated Y^{true} instead of Y . The strategy is sensitive to the choice of the parameters. We thus repeat the simulation with multiple combinations, specifically $p_1 \in \{0.6, 0.7, 0.8, 0.9\}$, $p_0 \in \{0.2, 0.3, 0.4\}$, and $\delta \in \{0.05, 0.1, 0.15, 0.2, 0.25, 0.3, 0.35, 0.4\}$. We report the

results in Appendix Figure B10.

Figure B10: Non-random attrition bounds to the estimated ATT

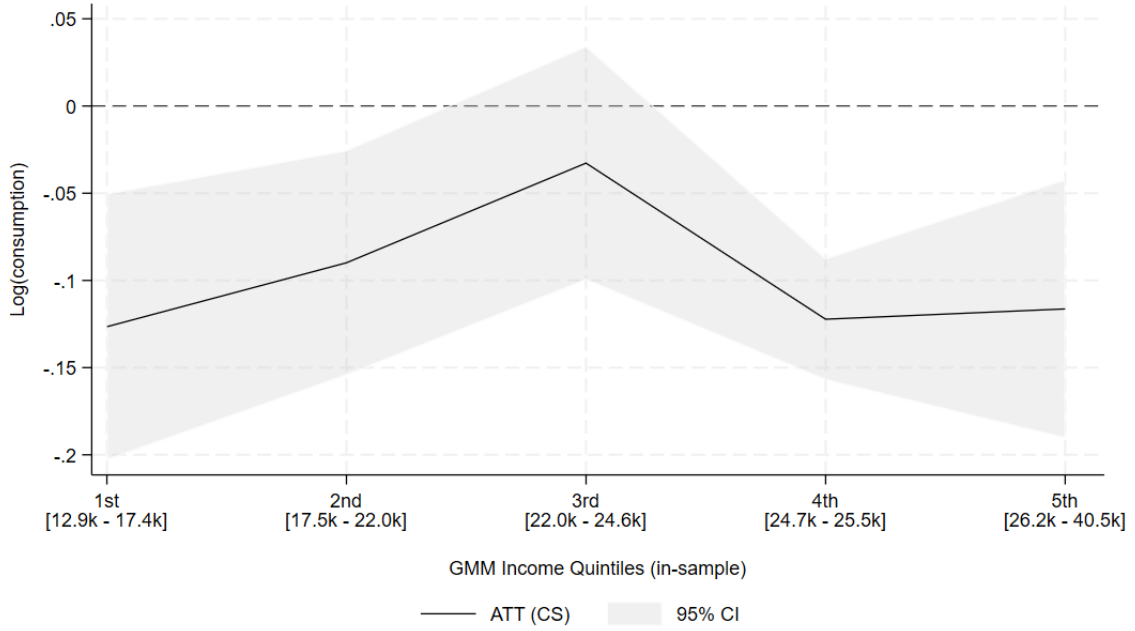


Notes: the figure shows nonparametric bounds to the ATT under model-based and worst-case selection bias. Model-based bounds are reported for the combination of parameters p_0 , p_1 , and δ that yields the lowest ATT in absolute value. Thin lines represent 95% confidence bands. Standard errors clustered at the cohort level and bootstrapped.

B.7 *Effect heterogeneity by income quintile*

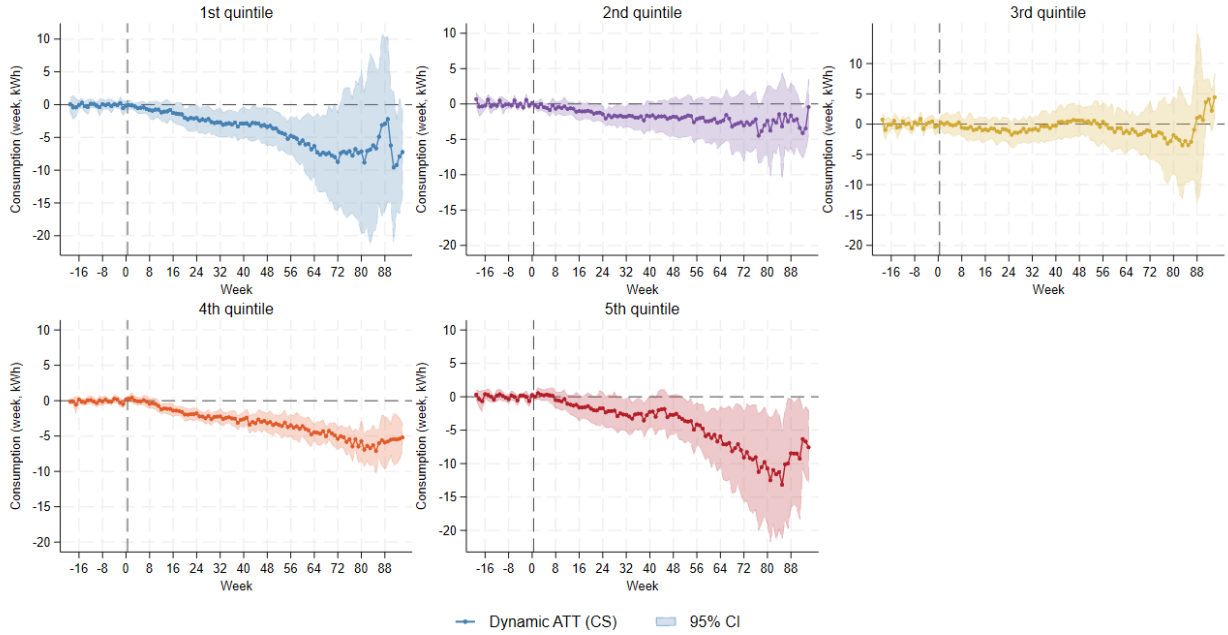
We test differences in the treatment effect of indexed-price contracts across our estimated income quintiles. We report here the dynamic ATT effect on consumption obtained by estimating Equation 1 (Figure B12) separately for each group, and the static ATT from Equation 2 on log-transformed consumption (Figure B11). We also show hourly patterns of consumption reduction across quintiles (Figure B13 and B14).

Figure B11: Heterogeneous treatment effect across income quintiles (static)



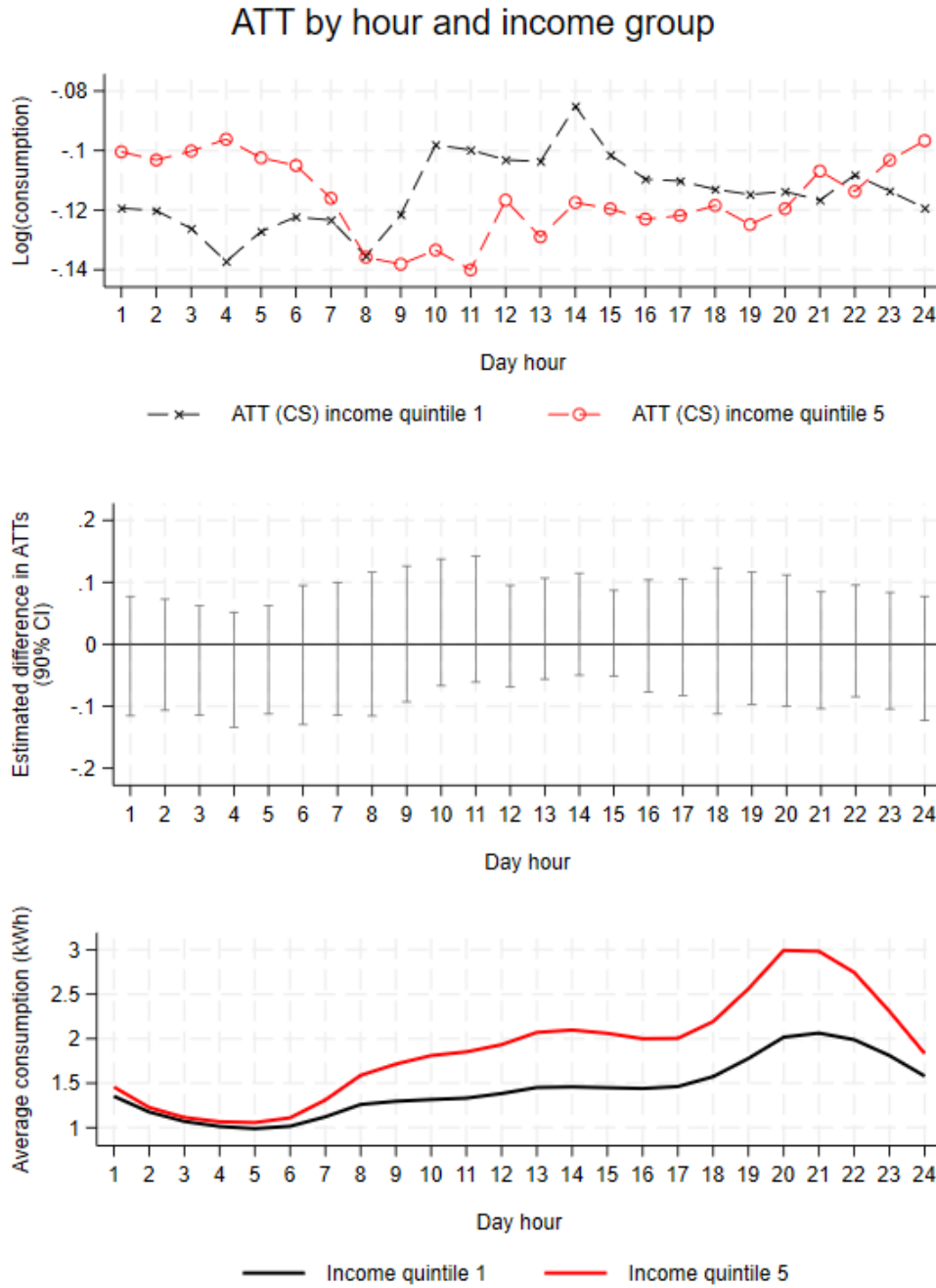
Notes: the figure reports the ATT of defaulting into indexed pricing on electricity consumption (in log) for separate income quintiles in our sample. Income quintiles are computed using the estimated household-level income (GMM). Standard errors are clustered at the cohort level and bootstrapped. The regression includes household and year-week fixed effects.

Figure B12: Heterogeneous treatment effect across income quintiles (dynamic)



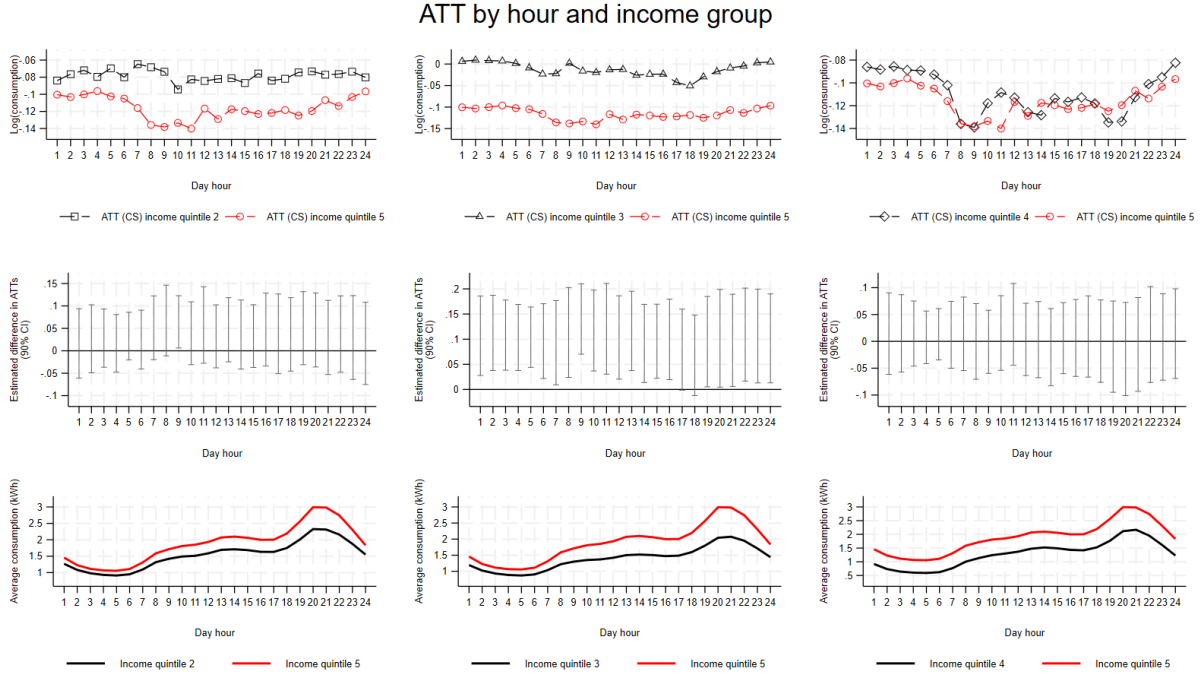
Notes: the figure plots the estimates for η_k coefficients in Equation 1 separately for each income quintile, correcting for staggered treatment adoption. Each coefficient represents a specific week relative to the onset of the contractual default into indexed electricity pricing (Week 0). The outcome variable is defined by weekly consumption in kWh. The specification includes contract and week-of-the-year fixed effects. Standard errors are clustered at cohort level and bootstrapped.

Figure B13: Heterogeneous hourly treatment effect across income quintiles



Notes: the figure reports the ATT of defaulting to indexed-price contracts on log-transformed consumption by hour of the day and income quintile (top panel). Estimated 90% confidence intervals based on t-tests for differences in the coefficients across the two income groups are shown (middle panel). Average consumption patterns are also displayed (bottom panel). Despite similar average daily consumption patterns, consumers in the top quintile reduce electricity usage more strongly during daytime, compared to those in the bottom quintile.

Figure B14: Heterogeneous hourly treatment effect across income quintiles



Notes: the figure reports the ATT of defaulting to indexed-price contracts on log-transformed consumption by hour of the day and income quintile (top panel). Estimated 90% confidence intervals based on t-tests for differences in the coefficients across the two income groups are shown (middle panel). Average consumption patterns are also displayed (bottom panel). Despite differences in the magnitude of consumption adaptation, daily reduction patterns look similar across consumers in the top four income quintiles.

C Non-parametric estimation of household income

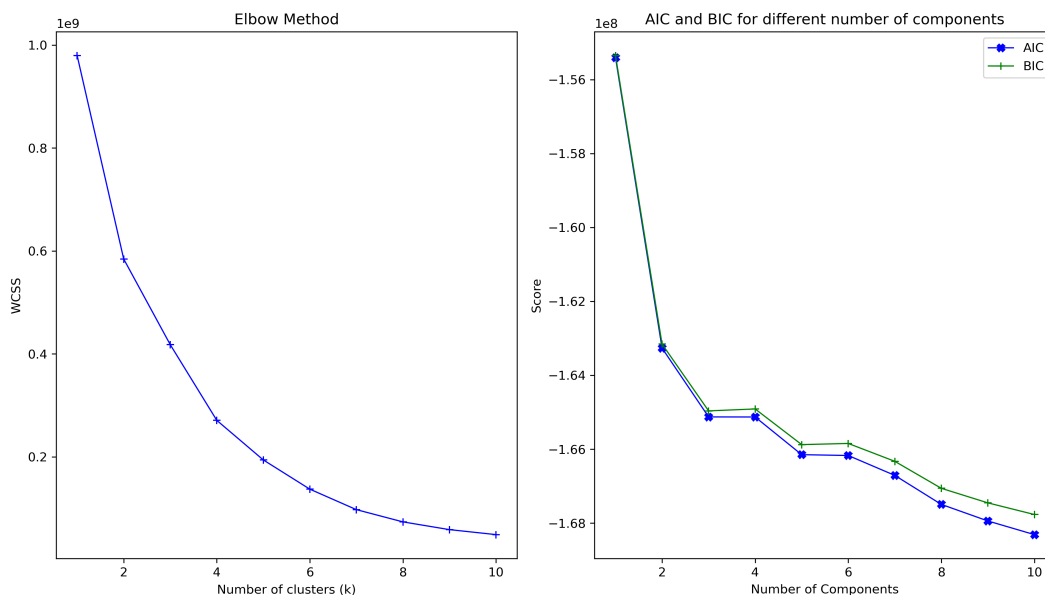
C.1 Classification of electricity users: *kmeans clustering*

Following the methodology developed by Reguant et al. (2025), we use observable characteristics and detailed information on consumption habits to segment consumers for our non-parametric estimator of household income. We employ a *k-means* clustering algorithm, an iterative, unsupervised machine learning algorithm that partitions observations into clusters by minimizing within-cluster variance. By clustering consumers based on their consumption habits and the household characteristics available in the data, we aim to produce a stratification of individuals, and use the resulting variation to estimate income at the household level. We use a total of 100 variables to capture consumption patterns and household characteristics. We conceptually divide these into three categories. We include average weekday and weekend daily consumption in kWh (*levels*) and calculate the ratio of winter and summer consumption to annual consumption (*seasonality*). We also calculate the mean and standard deviation of hourly consumption share for each of the 24 hours by weekday and weekend (*patterns*). All variables are calculated using only the period in which the initial fixed price applies to the household's contract. To select the optimal number of clusters we use as main evaluation metrics in tandem the BIC (Bayesian Information Criterion) and AIC (Akaike Information Criterion), and

the Elbow Method.²⁰ These criteria are well suited to our setting because our data display a complex structure with potentially many interactions (i.e. clusters not necessarily spherical), making model-based evaluation more informative. In contrast, distance-based metrics such as the Silhouette score may perform poorly in high-dimensional, heterogeneous consumer data, since they rely on geometric separation between clusters and can fail to identify meaningful groupings when cluster boundaries are irregular or overlapping.

Our selection methods suggest the presence of three clearly identified clusters (Appendix Figure C1). The optimal number of clusters is obtained at the point where the within-cluster-sum-of-square (WCSS) and AIC/BIC score curves exhibit a sharp change or "bend". Knee points, where the curve appears to slow down or flatten, are identified using first and second derivative algorithms as well as visual inspection. The selected number of clusters is three.

Figure C1: Selection methods employed to determine the number of clusters through *kmeans*



Notes: the figure plots the Elbow Method Within-Cluster Sum of Squares (WCSS), AIC (Akaike Information Criterion), and BIC (Bayesian Information Criterion) criteria used to classify contracts into clusters. AIC/BIC select the number of components — i.e., sub-distributions in a likelihood-based mixture model — which we then interpret as clusters.

Figure C2 presents the results of a *k-means* clustering algorithm that classifies consumers into three groups based on their electricity usage. We display average daily consumption patterns for weekdays and weekends, along with average consumption levels and the number of households in each cluster. The algorithm identifies two main patterns.

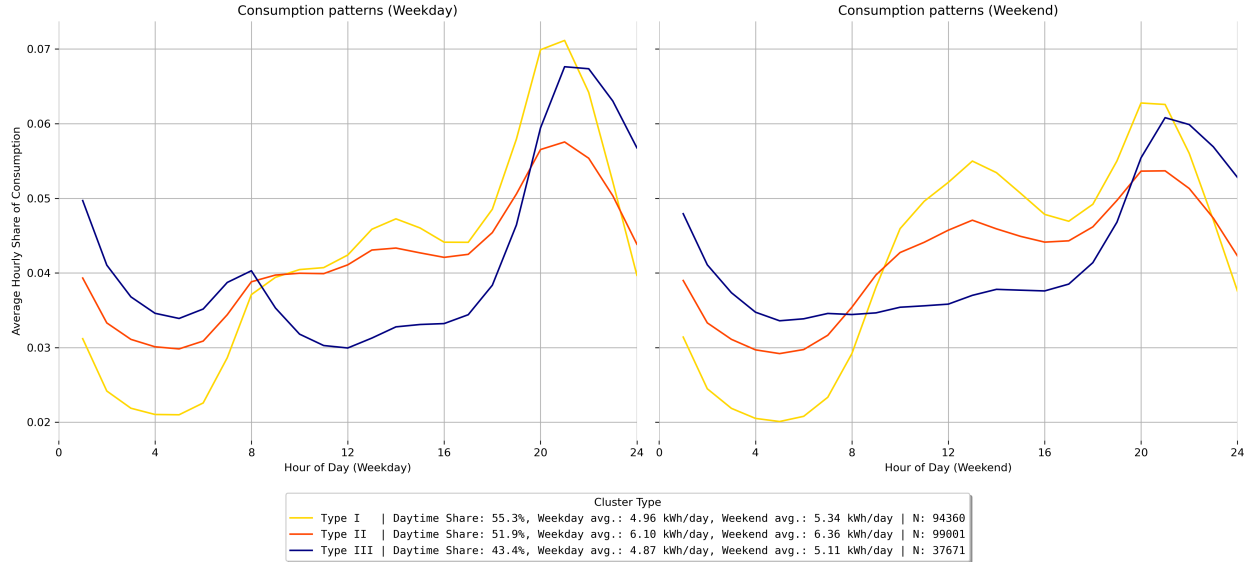
The first (*W-shaped*) features a gradual increase in consumption over the day, with two peaks—one around noon (especially on weekends) and a more pronounced one in the evening. Within this group, the algorithm distinguishes two types based on average daily consumption. Type I households are less energy-intensive, with average consumption per day of 4.96 kWh (5.34 kWh) during weekdays (weekends). Type II consumes more than one additional kWh per day on average.

The second pattern (*U-shaped* - Type III) shows relatively lower daytime consumption and higher usage

²⁰While the Elbow method directly selects the number of clusters by minimizing the within-cluster sum of squared distances (WCSS), AIC/BIC select the number of components—i.e., sub-distributions in a likelihood-based mixture model. We interpret components as clusters in our design.

during evening and nighttime hours. The reduction in daytime consumption of Type III households could be explained by a secondary source of electricity, such as photovoltaic (solar) systems.

Figure C2: Consumption distribution and level of *kmeans* types of users



Notes: the figure shows the results of the *kmeans* classification of contracts. The three clusters group households according to their electricity consumption profiles over the day, dwelling size, number of household occupants. Average hourly share of consumption over the 24 hours are reported for weekdays (left) and weekends (right) for the different Types of consumers classified through the *kmeans* clustering algorithm.

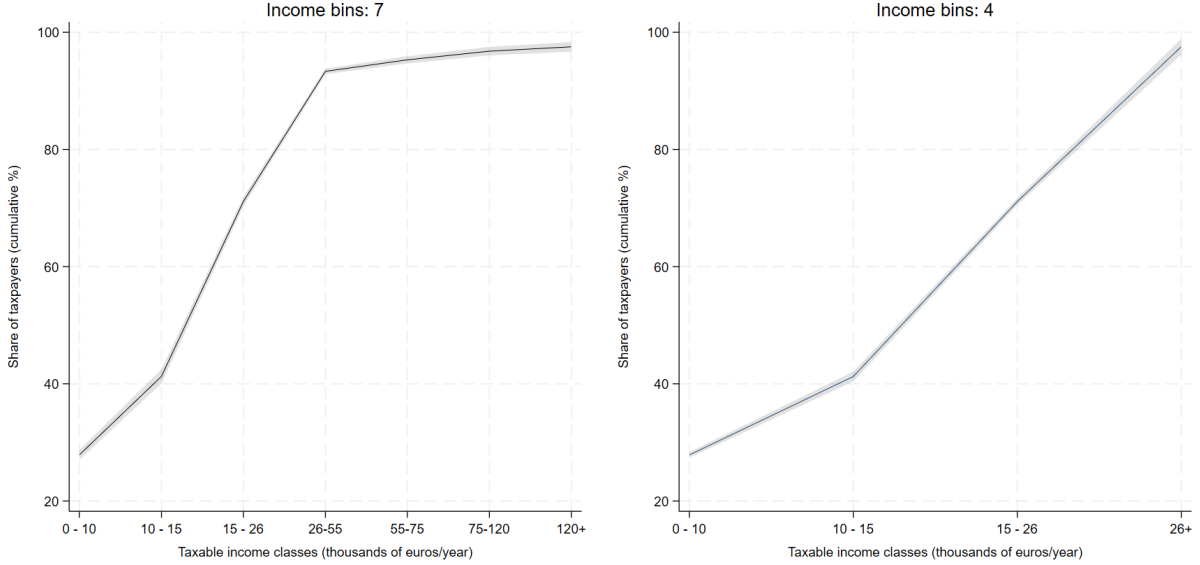
C.2 Income bins

To compute our non-parametric household income estimator via GMM, we use moments originating from the income distribution at the zip-code level. ISTAT reports the number of taxpayers in seven bins based on yearly taxable income:

- From 0 to 10,000 Euro
- From 10,000 to 15,000 Euro
- From 15,000 to 26,000 Euro
- From 26,000 to 55,000 Euro
- From 55,000 to 75,000 Euro
- From 75,000 to 120,000 Euro
- Over 120,000 Euro

The last four bins typically contain a disproportionately small number of taxpayers (less than 10%). To improve the precision of our estimates of household income and avoid moments generated from extremely small predicted probabilities, we collapse them into a single bin. This gives us a more balanced distribution of taxpayers across the four remaining bins (Appendix Figure C3).

Figure C3: Cumulative distribution of taxpayers into income bins



Notes: the figure plots the cumulative distribution of taxpayers across 7 (left) and 4 (right) income bins. Confidence intervals at the 95% level are reported.

C.3 Estimation algorithm

To estimate household income, we associate each household with a set of probabilities assigning it to a given income bin, depending on its type and location. First, we separate between household with high (4.5 kW or more, H) and low (3 kW or less, L) contracted power. Second, we apply the classification of consumers based on the *kmeans* clustering algorithm. We then combine this classifications with taxable income distributions computed by ISTAT, computing the number of taxpayers in income bin $b \in B$ for zip code $z \in Z$. The estimator will then compare the distribution of consumers of each type with the income distribution in a given zip code. The rationale behind the estimator is the following: if a given zip code has relatively high income and a large share of households with a certain type and contracted power, the algorithm will assign a higher likelihood of high income for households of that type in that area. Let ω_k^p be a household with contracted power $p \in \{L, H\}$ and of *kmeans* type k . For simplicity, let each combination of p and k define a type of household $n \in N$. The estimator will be using the following moment conditions:

$$\begin{aligned} \min_{\theta} \sum_z \rho_z \sum_b^B \left(P_b^z - \sum_{\omega_n} \theta_b^n \mathbb{P}^z(\omega_n) \right) \\ \text{s.t. } \sum_b^B \theta_b^n = 1 \quad \forall \omega_n \in \Omega \end{aligned} \quad (10)$$

where ρ_z is the weight for the number of taxpayers in zip code z . P_b^z is the share of taxpayers in income bin b and zip code z . $\mathbb{P}^z(\omega_n)$ is instead the probability of observing a household of type n in the zip-code, calculated using our consumer-level dataset according to the following equation:

$$\mathbb{P}^z(\omega_n) = \frac{1}{HH_z} \sum_i \mathbb{1}(\theta^i = \theta_n) \quad (11)$$

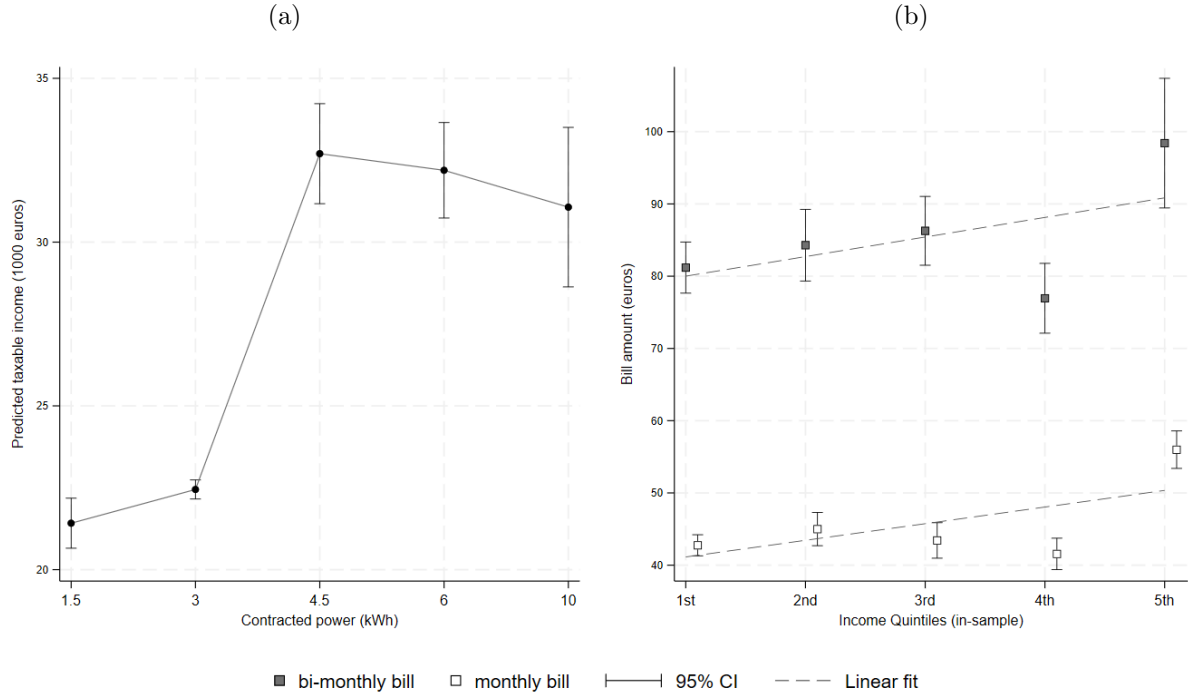
The parameters θ_b^n can be estimated via generalized method of moments (GMM), and allow to assign a set of probabilities to a household belonging to a given income bin. The objective function implies a set of $(B - 1) \times N$ unknown probabilities. As such, at least N zip codes are needed to identify θ for all b . We therefore aggregate zip codes at the province level. This implies assuming that the income distribution of a given type will be the same across zip codes within the province.²¹ The estimator imposes no minimum household count per zip code; however, for all income bins to be potentially assigned a non-zero probability, a minimum of four households in the zip code is required. Our sample of consumers covers a large share of the country (Figure A1). However, the market share of the utility in some areas is very low. To reduce the noise generated by our estimator, we require a province to contain at least 18 zip codes. Each zip code is also required to have information on at least 12 households. We show that our estimator is robust to relaxing these thresholds in Section C.5.

C.4 Validation

The non-parametric estimator described in Section 4 allows us to predict income at the household level. The resulting values can be used to check some empirical relationship between income and other observable consumer characteristics. Appendix Figure C4 shows that predicted income is positively correlated with contracted power, with a clear discontinuity between low (≤ 3 kWh) and high (≥ 4.5 kWh) power brackets. Splitting the households into predicted income quintiles, we also notice that consumers in the top quintiles tend to spend more on electricity. This intuitive result is in line with the findings of Reguant et al. (2025), and in accordance with the consensus that electricity and appliances can be considered normal goods (Holtedahl & Joutz 2004, Alberini et al. 2011b). We also notice that households across income quintiles do not differ significantly in the timing of default (Figure C5).

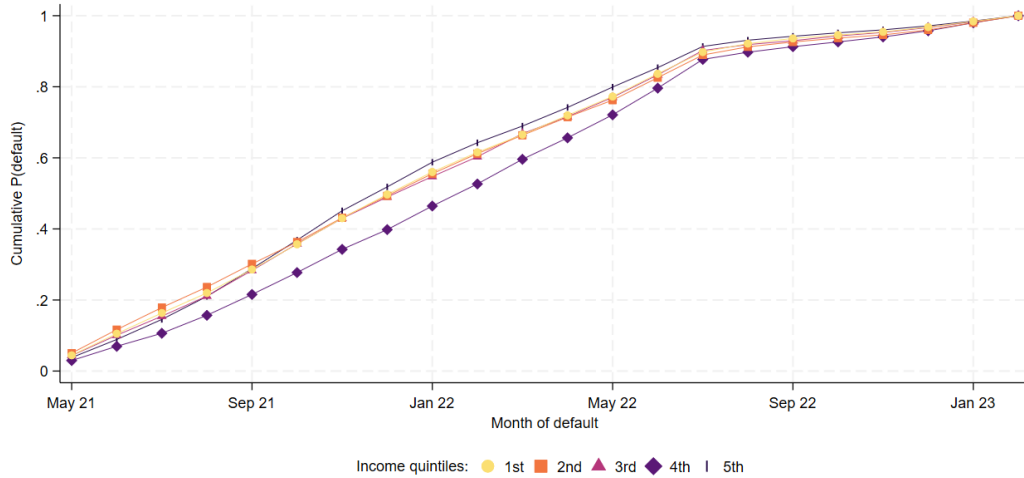
²¹We make one exception for the city of Bologna, which accounts for a large share of households in our sample. In this case, we aggregate zip codes separately for the metropolitan area of Bologna and the rest of the province.

Figure C4: Estimated household income, contracted power, and electricity spending



Notes: the figure shows the correlation between predicted household income and other observable household characteristics. Panel (a) shows the estimated average household income as a function of contracted power. Panel (b) displays the estimated electricity bill by income quintile.

Figure C5: Timing of default by income quintile



Notes: the figure shows the cumulative probability of having defaulted into an indexed-price contract by households in our sample, plotted by year-month and income quintile.

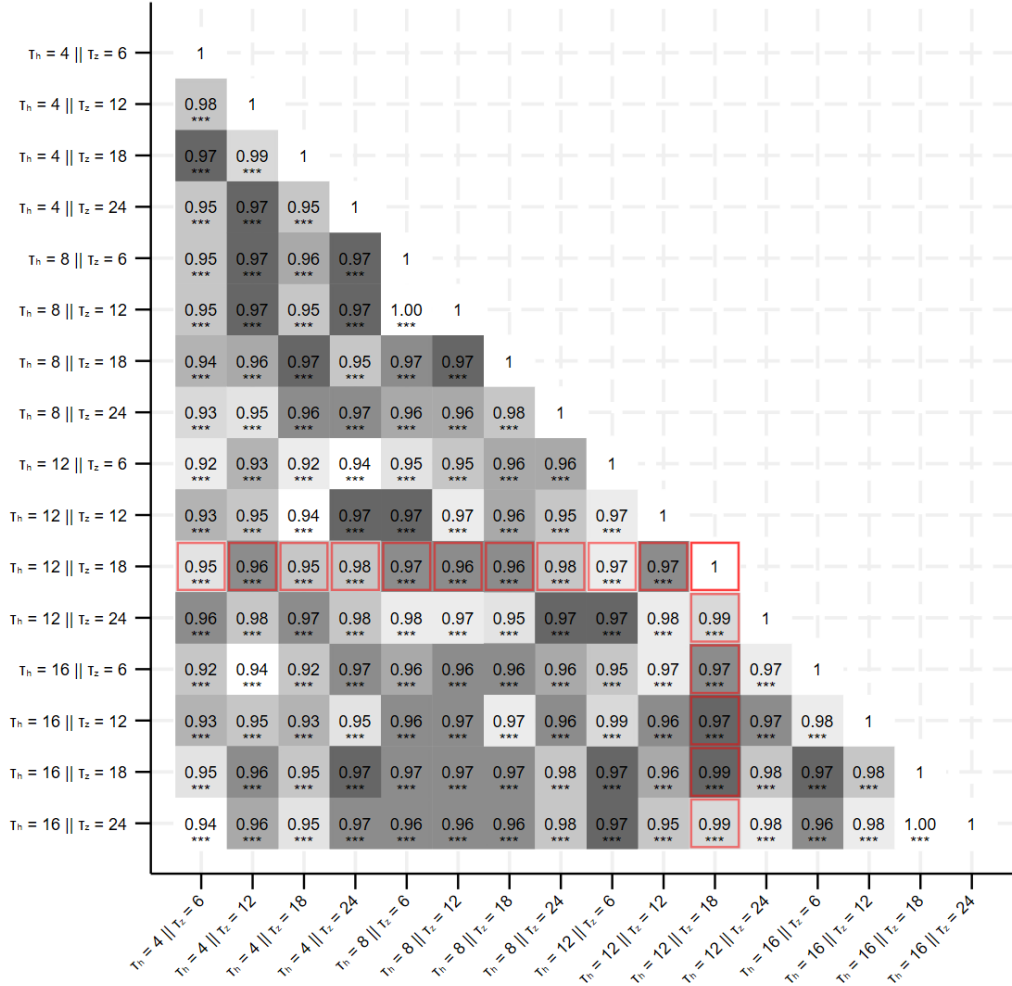
These results provide empirical support to the validity of our strategy.

C.5 *Robustness to small sample thresholds*

To compute the non-parametric estimator described in Section 4, we account for small sample problems that could increase noise by imposing a threshold on the minimum number of zip codes per province and households per zip code. We apply a simple rule of thumb based on two requirements. One is an objective requirement of our algorithm, which is used to estimate $(B - 1) \times N$ probabilities. Each zip code provides $(B - 1)$ moments, implying that a minimum number of $N = 6$ zip codes per province is necessary to estimate all θ probabilities. The second requirement relates to the possibility of artificially null income shares. In fact, despite the estimator could be computed without any restrictions on the number of households per province and zip code, for all income bins to potentially have a non-zero share within a zip code, a minimum of four households is required.

In our preferred specification, we adopt both requirements and simply triple the corresponding thresholds. While this choice is arbitrary, it reflects the expectation that moments derived from larger samples yield more precise estimates of the probability parameters. Importantly, there is no objective rule for determining sample size. As such, we consider the impact of these thresholds on the estimator. We propose a simple sensitivity analysis. We consider four threshold values on the number of households per zip code ($\tau_h \in \{4, 8, 12, 16\}$) and the number of zip codes per province ($\tau_z \in \{6, 12, 18, 24\}$). We then estimate θ parameters via GMM for all combinations of these thresholds. We then predict income at the household level in each iteration and classify households into income quintiles. To assess the sensitivity of the estimator to sample thresholds, we compute the pairwise probability that a household is classified into the same income quintile across two iterations. We infer the probability using Pearson correlation coefficients. We show the results with the help of a heat plot (Figure C6).

Figure C6: Heterogeneous treatment effect on irregular payments across income quintiles



Notes: the figure reports the Pearson correlation coefficient (r) of the predicted income quintile of a household estimated under different combinations of τ_h and τ_z . Significance level refer to the null hypothesis of $r = 0$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Households are much likely to be classified in the same income quintile regardless of the imposed thresholds.

The likelihood of a household falling within the same income quintile under the different thresholds imposed on the sample is consistently higher than 90%. Hence, the estimator appears robust to the restrictions in sample size, which supports the validity of our results.

D Welfare analysis: additional material

D.1 Indexed prices for consumers

The actual price per kWh paid by households whose contract becomes indexed to the wholesale electricity price varies with the National Single Price (PUN) and some time-invariant components that vary with each

contract. Contracts in our sample can be flat-rate or dual-rate. Considering a flat-rate contract, the indexed price per kWh will be calculated as follows:

$$p_{it}^{\text{indexed}} = p_i^0 + (\overline{\text{PUN}}_t - \text{pun}_i^0) + \Delta i go_i \quad (12)$$

where p_i^0 and pun_i^0 are predetermined and contract-specific time-invariant values, $\overline{\text{PUN}}_t$ is the the monthly moving average of the PUN price lagged 1 month (the previous calendar month relative to the month of t), and $\Delta i go_i$ is a contract and period-specific adjustment for the costs of the guarantees of origin. For dual-rate contracts, which have different rates applying to two consumption brackets (f_1 and f_2), contract-specific components are defined for both brackets and weighted with a flat rule:

$$p_{it}^{\text{indexed}} = \left(\frac{3}{4} p_i^{f1} + \frac{1}{4} p_i^{f2} \right) + \left(\overline{\text{PUN}}_t - \left(\frac{3}{4} \text{pun}_i^{f1} + \frac{1}{4} \text{pun}_i^{f2} \right) \right) + \Delta i go_i \quad (13)$$

where all components are defined as above and simply indexed for the corresponding consumption bracket. We refer to this formulation of indexed prices to calculate the difference between fixed and variable prices in Section 4.3.

Contract-specific cost components induce only marginal variation across across households with indexed price contracts, whose price is almost entirely determined by the fluctuations in the PUN. Simple dominance analysis (DA) shows how the PUN accounts for more than 98% of the variation in the actual price paid by indexed price consumers (Table D1).

Table D1: Dominance analysis of indexed price components

	Dominance stat.	Standardized dominance stat.	Ranking
$\overline{\text{PUN}}_t$	0.9808	0.9964	1
pun_i^0	0.0019	0.0019	2
p_i^0	0.0009	0.0009	3
$\Delta i go_i$	0.0008	0.0008	4

Notes: the table shows conditional dominance statistics of the various components of household indexed price. Conditional dominance statistics are an extension of Shapley values to determine the relative importance of independent variables. Statistics and rankings are obtained using the command *domin* in Stata 18.

D.2 Modeling surplus components

During periods of rising wholesale electricity prices, fixed price contracts induce a transfer of surplus from the retailer to the consumer. We model surplus redistribution by assuming that the inverse demand of final consumers is a linear function of consumption quantity, $p = dC + a$, where $d = \frac{\partial p}{\partial C}$, for any time t and household i . The model is illustrated in the left panel of Figure D1. Let $\tilde{p}$ and $\bar{p}$ be the current wholesale indexed and fixed electricity prices. Without loss of generality, it can be assumed a perfectly elastic linear electricity supply from providers, such that the supply curve under fixed and indexes price is respectively given by $p = \bar{p}$ and $p = \tilde{p}$. In the absence of frictions in the price signal, the equilibrium consumption level would be determined at the intersection between demand and the supply curve at level $\tilde{p}$. Let C^* represent the market-clearing level in this case. A fixed-term arrangement anchoring the actual price to a lower $\bar{p}$ implies that the household receives a subsidy of $(\tilde{p} - \bar{p})$ for each unit of consumption. This, in turn, implies

overconsumption and a deadweight loss (DWL) in overall surplus. Let $\bar{C}$ be the consumption associated with a price level anchored at $\bar{p}$. It is easily shown that under these assumptions, the DWL can be calculated as:

$$DWL = \frac{1}{2} (\bar{C} - C^*) (\tilde{p} - \bar{p}) \quad (14)$$

whereas the redistribution in surplus from providers to the final consumer will be equal to:

$$\Delta S = \frac{1}{2} (3C^* + \bar{C}) (\tilde{p} - \bar{p}) \quad (15)$$

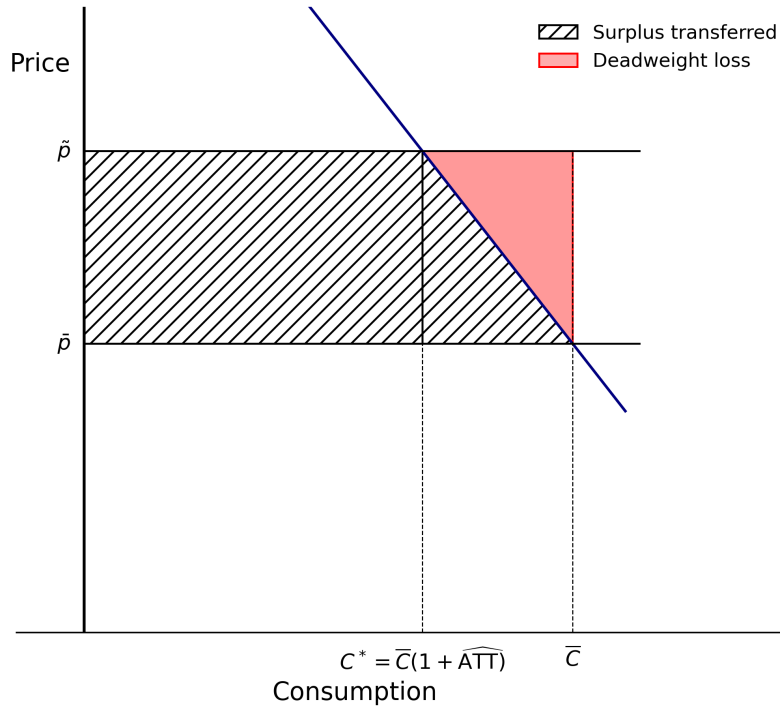
We use our empirical model of consumption behavior to characterize the counterfactual consumption in the absence of frictions. In our empirical setting, C^* is observed in our cohorts of treated consumers in the long run, when the contract has become indexed to the PUN and adjustment has taken place. We can therefore use the estimated ATT effect ($\widehat{ATT}$) of defaulting to indexed price of consumption to calculate what C^* would have been for those consumers who still remained anchored to their initial fixed arrangement. We can write C^* as:

$$C^* = \bar{C} (1 + \widehat{ATT}) \quad (16)$$

As a result, we can equivalently express DWL and ΔS as:

$$\begin{aligned} DWL &= \frac{1}{2} (\bar{C} \cdot \widehat{ATT}) (\tilde{p} - \bar{p}) \\ \Delta S &= \bar{C} \left(1 - \frac{1}{2} \widehat{ATT} \right) (\tilde{p} - \bar{p}) \end{aligned} \quad (17)$$

Figure D1: Surplus transferred between retailers and consumers and deadweight loss



Notes: the figure shows the surplus transferred from retailers to households (diagonally hatched area) and the DWL (red area) under the assumption of linear demand function from final consumers and perfectly elastic supply of providers.

This simple model formalizes how the redistribution of surplus from retailers to households comes at the expense of a partial loss in overall surplus and, as such, market efficiency. However, our findings highlight discrepancies in long-run adaptation in consumption to indexed price contracts across consumers in different income quintiles which, in turn, imply different counterfactual consumption levels in the absence of price frictions. We aim to use our model and our empirical findings to capture these differences across consumers and quantify them during the period of our analysis, under the maintained assumption of linearity of the inverse demand function in the domain $[C^*, \bar{C}]$ for each income group and for all periods. Let $\widehat{ATT}_q$ be the estimated effect of variable price indexing for consumers in income quintile q . Calculating the total transfer and net surplus loss over the analysis window requires accounting for the time-varying nature of both indexed prices and treatment effects. As such, let C_{iqt} be the consumption of household i in income quintile q observed at time t and let D_{iqt} define a period in which a household defaults into the indexed price regime as in Equation 2. Let I_q be the number of households for any given income group. Whenever anchored prices undercut wholesale indexed prices, for each unit of consumption, the consumer will be received a positive monetary transfer from the retailer for an amount equal to the difference between the indexed price and the fixed price. In Figure D1, this is equivalent to the area included between the two supply curves at the consumption level observed at every period. The average across households in quintile q and time can be computed as:

$$\Delta M_q = \frac{1}{I_q} \sum_t \sum_i C_{iqt} (\tilde{p}_{iqt} - \bar{p}_{iq}) (1 - D_{iqt}) \quad (18)$$

By similar logic to the one explained with our model, the above quantity can be thus decomposed into the surplus that is indeed transferred to the consumer and the DWL:

$$\begin{aligned} \Delta S_q &= \frac{1}{I_q} \sum_i \sum_t (\tilde{p}_{iqt} - \bar{p}_{iq}) \cdot C_{iqt} \left(1 - \frac{\widehat{ATT}_{q,t-T_i}}{2} \right) (1 - D_{iqt}) \\ \text{DWL} &= \frac{1}{2I_q} \sum_i \sum_t (\tilde{p}_{it} - \bar{p}_i) \cdot C_{it} \widehat{ATT}_{t-T_i} (1 - D_{it}) \end{aligned} \quad (19)$$

D.3 Comparing differences across income quintiles

Table 4 reports the differences in surplus transferred from the retailer to households across income quintiles. To assess whether these average values differ significantly between groups, we conduct pairwise hypothesis tests by regressing the outcome variable on income quintile indicators. The resulting p-values quantify the statistical significance of the differences between coefficients (Table D2).

Table D2: Pairwise hypothesis testing of differences in transfer across income quintiles

Quintiles	ΔM			ΔS			DWL			ΔCO_2			Cost of CO_2		
	contrast	SE	p-value	contrast	SE	p-value	contrast	SE	p-value	contrast	SE	p-value	contrast	SE	p-value
	Weekly														
2nd vs 1st	1.11	0.64	0.09	1.17	0.6	0.05	0.07	0.06	0.25	-0.57	0	0.00	0.1	1.2E-17	0.00
3rd vs 1st	-0.16	0.57	0.79	0.17	0.55	0.75	0.33	0.05	0.00	-0.89	0	0.00	0.15	1.2E-17	0.00
4th vs 1st	-0.63	0.54	0.24	-0.64	0.49	0.19	-0.01	0.06	0.85	-0.2	0	0.00	0.03	5.2E-18	0.00
5th vs 1st	1.72	0.67	0.01	1.57	0.62	0.01	-0.15	0.08	0.05	0.13	0	0.00	-0.02	9.5E-18	0.00
3rd vs 2nd	-1.26	0.64	0.05	-1	0.62	0.11	0.26	0.03	0.00	-0.32	0	0.00	0.06	1.5E-17	0.00
4th vs 2nd	-1.73	0.61	0.00	-1.81	0.57	0.00	-0.08	0.05	0.11	0.37	0	0.00	-0.06	1.0E-17	0.00
5th vs 2nd	0.61	0.73	0.40	0.4	0.68	0.56	-0.22	0.07	0.00	0.7	0	0.00	-0.12	1.3E-17	0.00
4th vs 3rd	-0.47	0.54	0.38	-0.81	0.51	0.11	-0.34	0.04	0.00	0.7	0	0.00	-0.12	1.0E-17	0.00
5th vs 3rd	1.88	0.67	0.01	1.4	0.63	0.03	-0.48	0.06	0.00	1.02	0	0.00	-0.18	1.3E-17	0.00
5th vs 4th	2.35	0.64	0.00	2.21	0.59	0.00	-0.14	0.07	0.05	0.33	0	0.00	-0.06	7.9E-18	0.00
	Total over period of analysis														
2nd vs 1st	-1.62	6.49	0.80	10.86	6.18	0.08	12.49	0.32	0.00	-71.03	0.91	0.00	0.16	0.16	0.00
3rd vs 1st	-189.59	4.55	0.00	-153.62	4.39	0.00	35.97	0.2	0.00	-97.44	0.83	0.00	0.14	0.14	0.00
4th vs 1st	-49.04	3.91	0.00	-51.38	3.7	0.00	-2.34	0.22	0.00	-16.61	0.95	0.00	0.16	0.16	0.00
5th vs 1st	190.51	5.45	0.00	173.69	5.17	0.00	-16.82	0.3	0.00	13.65	1.18	0.00	0.2	0.20	0.00
3rd vs 2nd	-187.96	6.06	0.00	-164.48	5.84	0.00	23.48	0.24	0.00	-26.4	0.4	0.00	0.07	0.07	0.00
4th vs 2nd	-47.42	5.59	0.00	-62.24	5.34	0.00	-14.82	0.25	0.00	54.42	0.61	0.00	0.11	0.11	0.00
5th vs 2nd	192.13	6.76	0.00	162.82	6.44	0.00	-29.31	0.32	0.00	84.68	0.93	0.00	0.16	0.16	0.00
4th vs 3rd	140.54	3.14	0.00	102.24	3.11	0.00	-38.3	0.07	0.00	80.82	0.48	0.00	0.08	0.08	0.00
5th vs 3rd	380.09	4.93	0.00	327.3	4.76	0.00	-52.79	0.22	0.00	111.09	0.85	0.00	0.15	0.15	0.00
5th vs 4th	239.55	4.35	0.00	225.06	4.13	0.00	-14.49	0.23	0.00	30.26	0.97	0.00	0.17	0.17	0.00

Notes: the table reports pairwise hypothesis testing of coefficients obtained from regressing the outcome reported in the top row on a set of indicators for each income bin. The estimation is repeated for weekly average transfers and total transfers over the sample period. Contrasts report simple differences between income bin coefficients. Delta-method heteroskedasticity robust standard errors and unadjusted p-values are reported.

E Additional Material

E.1 Billing specifications

Consumers can learn about their contract conditions through the information provided in their energy bills, which are typically delivered either by postal mail or e-mail. A standard energy bill consists of approximately five pages, with the total amount due and the payment deadline prominently displayed on the first page. Figure E1 presents an anonymized example of such a bill; all identifying information about the provider and the customer has been removed, and the monetary amounts shown are purely illustrative. Panel A displays a regular bill received by a consumer, while Panel B shows the first bill issued after the consumer was defaulted into the new contract conditions.

The first page contains a summary of the amount due, along with the payment deadline. Page 2 provides contact information and additional details about the provider, while page 4 reports a breakdown of the household's consumption profile. The information on contract conditions, including the notification of the new terms and the revised prices for energy and related components, can instead be found on pages 3 and 5. Although this information is technically available to consumers, accessing it requires active monitoring and careful reading, as the notifications are not prominently displayed and are unlikely to be noticed during a superficial inspection of the bill.

Figure E1: Example of electricity bill structure

i. Page 1

A - Regular bill

Riepilogo		1 / 5
Servizi intestati a Codice cliente		
Energia Elettrica 66,63 €		
Totale da pagare 66,63 €		
Scadenza 16 giugno 2022		

B - First bill after default

Riepilogo		1 / 5
Servizi intestati a Codice cliente		
Energia Elettrica 101,31 €		
Totale da pagare 101,31 €		
Scadenza 18 agosto 2022		

Figure E1: (continued)

ii. Page 2

A - Regular bill

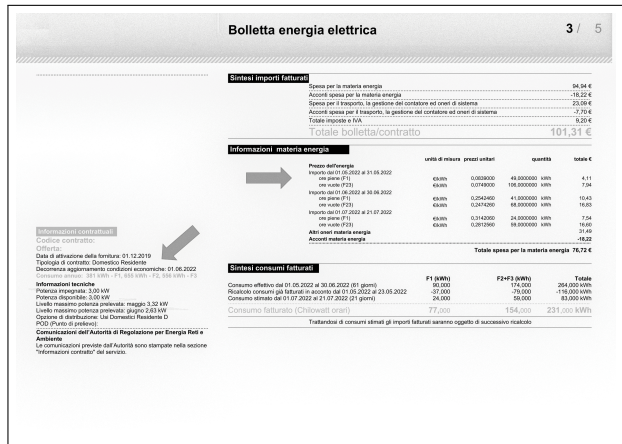
Informazioni utili			2 / 5
Contatti	Modalità di pagamento	Comunicazioni e reclami scritti	
Consumo stimato e Autodibattito			
Altre informazioni			
Situazione contabile riferita al codice raggruppamento contratto			

B - First bill after default

Informazioni utili			2 / 5
Contatti	Modalità di pagamento	Comunicazioni e reclami scritti	
Consumo stimato e Autodibattito			
Altre informazioni			
Situazione contabile riferita al codice raggruppamento contratto			

iii. Page 3

B - First bill after default



iv. Page 4

B - First bill after default

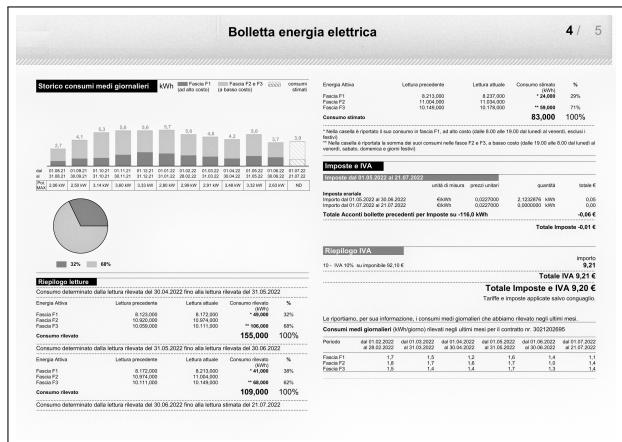
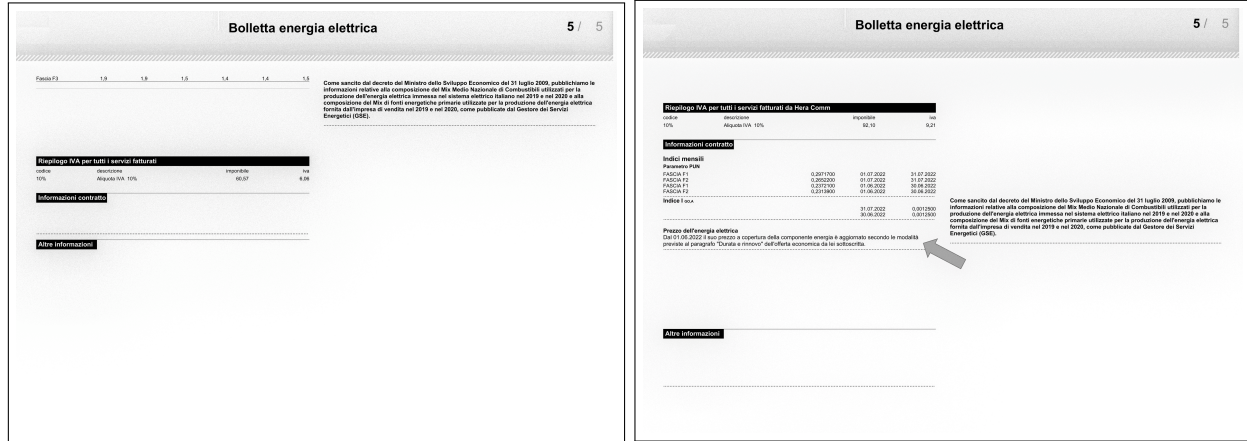


Figure E1: (continued)

v. Page 5

A - Regular bill

B - First bill after default



Notes: the figure reports an example of electronic bill received by the consumer. Panel A shows a regular bill; Panel B shows the first bill after the consumer was defaulted into the new contract conditions. Arrows are added to highlight where the user can retrieve salient information about new price conditions. Identifying information about the provider and the customer has been removed. Monetary amounts are purely illustrative