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"Major Reforms in Electricity Pricing: Evidence from a Quasi-Experiment"

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Abstract

The global energy mix is being redefined, and with it the power industry's cost structure. In many countries, electricity-pricing systems are being revamped so as to guarantee fixed-cost recovery, often by raising the fixed charge of two-part tariff (TPT) schemes. However, consumer misperception of TPTs threatens to undermine the policy's outcome and puts the sector's much-needed transformation in jeopardy. We conduct a quasi-experiment with data from a major electricity price reform recently implemented in Spain and find robust evidence that consumers are failing to distinguish between fixed and marginal costs. As a result, the policy goal of cost recovery is not being achieved.

JEL classification: C99, D12, L11, L94, L98, Q41, Q48

Keywords: Fixed-cost recovery; Residential electricity demand; Renewables; quasi-experiment; two-part tariff.

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The electricity industry is undergoing an unprecedented transformation. The goal of addressing global climate change in line with the Paris Agreement has become heavily dependent on the electricity sector's capacity to achieve much higher levels of decarbonization. Indeed, renewable electricity generation grew by 6% in 2017 – the highest rate among all energy sources – accounting for 25% of global power output; however, in order to limit global warming to 1.5°C, renewable electricity will need to reach a global share of up to 97% by 2050 (IPCC 2018). Investment in clean energy rose from $88 billion USD in 2005 to around $300 billion per year since 2010 (International Energy Agency 2018), and to host this expanding fleet of renewable generators, a new investment cycle has been initiated to increase the capacity of the distribution network (European Commission 2015). Moreover, this supply of cleaner generation needs to operate in tandem with the thriving electrification of energy end use, which implies a greater interconnectedness and interdependence with other key sectors, including heating, transportation, and gas. And this without mentioning the impact of distributed energy resources, energy storage facilities, and digitalization for a smarter and price-responsive demand.

This major technology change has triggered a shift in the power sector's cost structure, with an intensification of its capital costs at a time when variable costs are declining because of an expanded renewable energy supply (Bushnell and Novan 2018; Würzburg et al. 2013; Sensfuß et al. 2008). As a result, current electricity pricing systems are proving inadequate to guarantee fixed-cost recovery. Typically, most countries operate a two-part tariff (TPT) which, in theory, should consist of two price components: a volumetric charge equal to the marginal cost, which ensures allocative efficiency, and a fixed fee equal to the consumer's share of fixed costs, which guarantees fixed-cost recovery (Coase 1946). In practice, however, fixed charges in electricity bills have tended to be low, while fixed costs have been largely covered by markups on the volumetric price component. Moreover, the rapid emergence of more energy-efficient appliances has further reduced the electricity industry's revenues. In many countries, utilities and policymakers have responded by attempting to allocate more fixed costs among the electricity bills' fixed charges.[1]

This is the case in at least 34 US states, where electric utilities have proposed shifting cost recovery from the volumetric component price to the fixed-part component (Wood et al. 2016); in 14 states, the proposed fixed charge increase was more than 100% (Whited et al. 2016). From a non-exhaustive list of 87 US electric utilities, the proposed increase in the customer charge (fixed part fee) was 61% (Baatz 2017). In a similar vein, the European Commission (2015) has called for more efficient electricity tariffs by increasing the share of fixed costs covered by fixed-charge price components. Only the Netherlands, Sweden, and Spain have substantial fixed-price components, financing more than 75% of the industry's fixed costs (Eurelectric 2016), and in the case of the Netherlands and Spain, this was due to reforms introduced in 2009 and 2013, respectively. Italy implemented similar reforms in 2016 (Chiaroni et al. 2017). Finally, Australia introduced new tariff structure rules in 2014 and, despite the flexibility shown by electricity utilities in adapting to their network and customer characteristics, the trend has been to increase fixed charges while lowering volumetric charges (Australian Energy Regulator 2016).

Recent studies have shown the extent to which households respond to marginal prices in non-linear price schedules, usually in the context of increasing block pricing (Ito 2014; Khan and Wolak 2013; Nataraj and Hanemann 2010; Borenstein 2009). In the case of electricity, because of the information cost of understanding non-linear pricing structures, there is evidence that consumers tend to respond to average prices rather than to marginal prices; importantly, such non-optimizing behaviors are affected by their degree of energy literacy (Blasch et al. 2017; Jessoe and Rapson 2014; Wolak 2011). It remains unclear, however, how households might respond to changes in the pricing system when the price change is in the fixed price component, which, in contrast to changes in pricing blocks, is

[1] By fixed charges, we refer to all non-volumetric price components of the electricity tariff, excluding taxes and any commercialization rates. However, fixed charges are defined differently across countries: in some, they are known as a standing charge, service charge, customer charge, connection charge, etc. and are usually charged as a fixed amount per day/month/year; e.g., Austria, Belgium, Germany, Ireland, Poland, Sweden, the UK, and the US. In other countries, the fixed charge corresponds to a fixed amount per contracted capacity load (€/kW), e.g. Finland, Greece, Portugal, Slovenia, Slovakia, and Spain. Finally, another group of countries operated both connection and capacity charges; e.g. Italy, France, and the Netherlands. There is a long tradition in economics devoted to the optimal tariff design in regulated sectors, and particularly in the electric utilities; see MIT Energy Initiative (2016).

totally independent of the kWh consumed and – more importantly – seeks cost recovery. A central assumption underpinning the TPT scheme is that consumers discriminate fixed costs from marginal costs. If they fail to do so, the price signals sent may well be distorted both in the short and the long run, affecting cost recovery, allocative efficiency, and investment decisions. In this paper, using a quasi-experimental design that exploits the major 2013 electricity price reform in Spain, we estimate a demand model to empirically evaluate household responses to a rise in the fixed charge of the residential electricity bill.

Ex-ante evidence shows that electricity consumption increases if the rise in the fixed charge is revenue-neutral, i.e. the fixed charge increase is combined with an offsetting reduction in the volumetric price component so that the average price suffers no change. Using a dataset from a US electric utility, Lazar (2013) finds that consumption would increase by 7% in this case. Baatz (2017) finds that a 100% revenue-neutral increase in the fixed charge results in a consumption increase of 3–9% in the short run (and 10–20% in the long run) in the US. Finally, Pearce and Harris (2007), also examining the US residential sector, provide further evidence for these results by showing how a revenue-neutral suppression of the fixed charge would result in a consumption reduction of 6.4% (as the marginal price increases). The findings of these ex-ante studies align with standard economic theory and conclude that consumers respond to marginal prices, which implies that they identify fixed charges as a separate cost.

The only relevant ex-post evidence is reported in a study conducted by Ito and Zhang (2018), who analyze the effect of a recent reform involving the introduction of a TPT in China's heating systems. Heating prices in China used to comprise just a fixed charge dependent solely on the dwelling size. The reform the authors analyze saw the introduction of a positive marginal price at the same time the existing fixed charge was reduced. In their quasi-experiment, they find that treated households reduced their heating usage by 20% in response to the reform. Therefore, they conclude that consumers distinguish between marginal and fixed costs, in line with the ex-ante evidence described above. However, for electricity demand in a developed country, our results point in the opposite direction.

The pricing reform in Spain saw the fixed charge raised 112% with a non-revenue neutral reduction in the regulated volumetric part of 35% (while wholesale prices remained relatively constant, as in Figure A1). Our identification strategy involves exploiting the data stratification method employed by the Spanish Household Budget Survey (HBS), which allows us to create a comparable control group that was unaffected by the reform. The principal result of a difference-in-differences model shows that, despite the marginal price reduction, households reduced their electricity consumption by 16% compared to those in the counterfactual scenario. This reduction in consumption is not consistent with a response to (declining) marginal prices, but rather to (increasing) average prices. Moreover, we note a 12% reduction in household electricity expenditure, meaning that revenue increase from the higher fixed charge was lower than the revenue loss from the resulting contraction in demand. By means of quantile regression, we also show that while the highest electricity consumers markedly reduced their electricity consumption, the lowest electricity consumers made much smaller reductions, even non-significantly different from zero at the bottom of the distribution; the reform was indeed regressive.

These findings have far-reaching yet timely policy implications. First, we show that rather than improving fixed-cost recovery, a rise in fixed charge tends to push electric utilities ever closer to a "death spiral" (Costello and Hemphill 2014) while doing little to guarantee their financial sustainability. This is critical, given the transformation the power sector is undergoing and the important challenges it faces globally. From an environmental perspective, price signals matter, and final outcomes hinge on consumers' (mis)perceptions of future pricing policies. Based on our robustness checks, we do not observe any significant differences between treated and control households in terms of their investments in new equipment/change of energy source, that is, the fall in demand is attributable to the reform. As discussed by Borenstein and Bushnell (2018), in the same way that low prices may encourage wasteful use (i.e. prices set below social marginal cost), excessively high prices can act as a barrier to a higher electrification of energy and transportation services. Indeed, our results seem to be consistent with this latter hypothesis.

This paper is, as far as we are aware, the first empirical evaluation of consumer response in an advanced economy to a non-revenue-neutral increase in the fixed charge of electricity bills, a policy currently under consideration in many countries. The following section describes the electricity pricing reform implemented in Spain. Section II describes our data and the natural experiment design. Section III presents the empirical analysis and results, and finally, Section IV concludes by discussing the main policy implications derived from the analysis.

I. Electricity pricing reform: the Spanish case

Prior to the reform, Spanish residential electricity demand had been shrinking for a number of years, the result of a global tendency driven by more affordable energy-efficient home appliances, including the rapid emergence of LEDs (Davis 2017). Moreover, Spain had been hit hard by the 2008 global financial crisis, leading to a widespread reduction in consumption. This decline in demand impacted the revenue of electricity companies to such an extent that threatened the financial sustainability of the entire system. In response, an electricity pricing reform was introduced with the aim of achieving a "better balance between the pricing system and the industry's cost structure" (Orden IET/1491/2013).

The Spanish electricity bill comprises a TPT pricing that takes the following form:

(1) $$p(q) = [FE_k + q(f + p))]taxes$$

The residential final price $p(q)$ is the sum of a fixed charge corresponding to the contracted capacity load E_k (in kW), multiplied by the regulated charge F, and the variable part that depends on electricity consumed q (in kWh). This is split in two parts: a regulated access rate f and a market-based part p (wholesale price) on which the electricity tax (5%) and VAT (21%) are then levied.

The reform we evaluate was implemented in August 2013 and led to a marked increase in the fixed charge part, F, the same time that f, a component of the variable part, was reduced. Thus, for electricity consumers with fewer than 10 kW of contracted capacity (that is, 94% of Spanish households), F increased by 112% while f decreased by 36%. For households with more than 10 kW of contracted capacity (just 6%), the reform was less dramatic. Figure 1 shows the evolution taken by the different price components described (see Figure A1 in the Appendix for further details on wholesale price evolution).

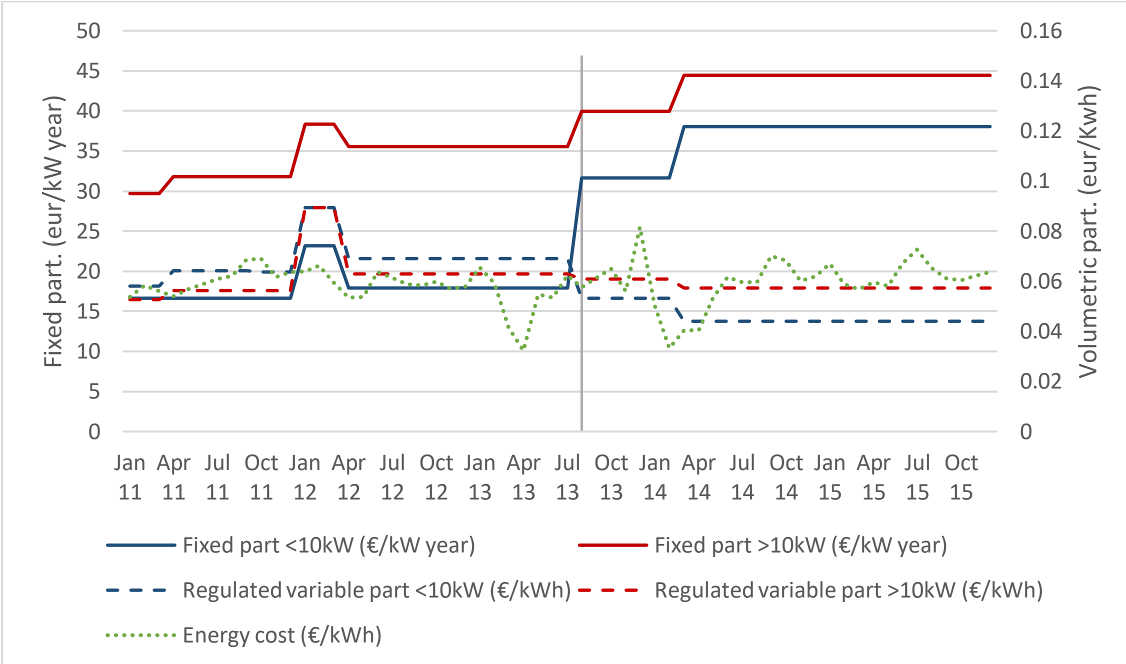

FIGURE 1. ELECTRICITY TARIFF COMPONENTS IN SPAIN

Notes: Solid lines: fixed charge evolution. Dashed lines: the regulated variable part. Dotted line: wholesale price evolution (source: data are from Red Electrica de España).

Figure 2 shows the evolution of both electricity consumption and expenditure in 26 two-week periods throughout 2013, with a decoupling between the indicators following reform, expenditure somehow remaining constant despite the fall in demand. As such, a (naive) interpretation might be that the reform prevented further revenue loss despite continuing fall in demand. However, Figure 2 obviously does not show how the two variables would have evolved had reform not been implemented; that is, it does not illustrate the counterfactual scenario. Our identification strategy, detailed in the section that follows, allows us to estimate this counterfactual scenario and therefore measure the reform's impact on household demand.

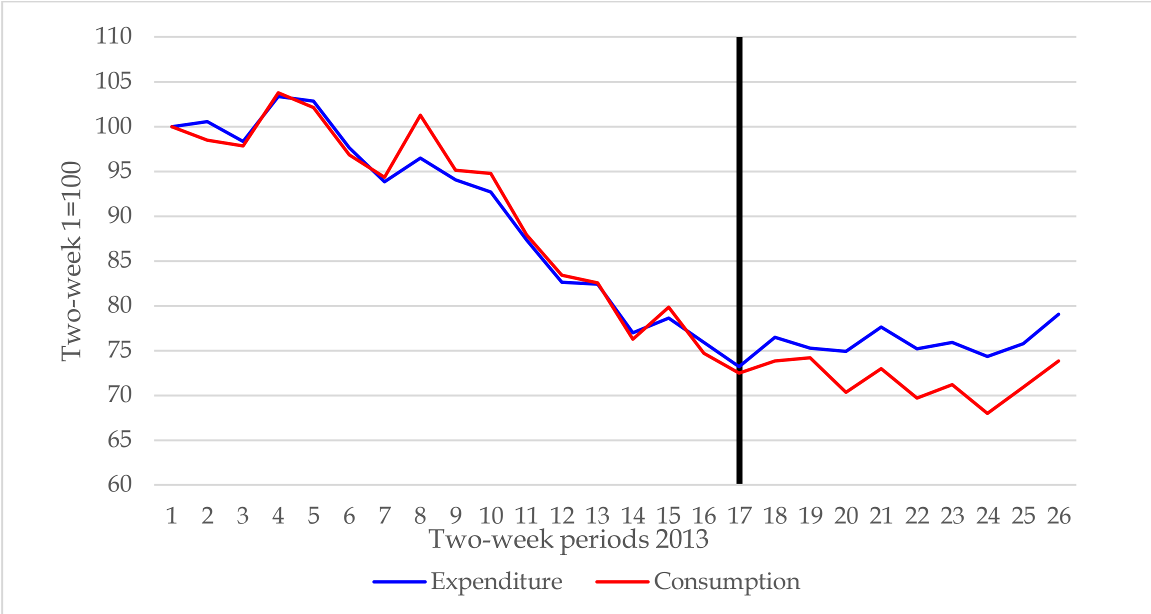

FIGURE 2. EVOLUTION IN ELECTRICITY CONSUMPTION AND EXPENDITURE IN 2013

II. Data and research design

A. Data

Data are taken, as indicated, from the Spanish HBS conducted by the Spanish statistics office, Instituto Nacional de Estadística (INE). This high-quality survey reports annual data at the household level for the period of 2006-2016 on the physical quantity of consumption of certain goods and their corresponding expenditure, including electricity billing data. Given that we seek to determine the impact of the 2013 reform, we limit our sample to the period of 2011-2014 to guarantee that no other significant events affect our data. Specifically, we use data from households interviewed between August 2011 and April 2014.[2] Participation in the survey is limited to two years, and only half of each year's cohort is (randomly) invited to participate the second year. Our sample comprises a panel

[2] In April 2014, there was a regulatory change in the electricity market, which resulted in a reduction in electricity prices.

of 25,775 observations, and we exploit both its panel data structure and cross-sectional dimensions.

B. Identification strategy

Natural experiments have become the gold standard for making causal inferences in the social sciences, above all in policy evaluation. Yet one of the main challenges that must be faced is that non-experimental data do not always provide a clean control group. Ideally, researchers would work on two randomly assigned samples, only one of which would be exposed to reform (treated group). Only under such conditions can the causal effects of the reform be unequivocally identified. Here, however, all Spanish households are exposed to the same electricity pricing and hence impacted by the same reform shock. As is usual, microdata are gathered on a yearly basis, which means the policy impact is easily confounded with other, often unobservable, factors, making it virtually impossible to separate the policy effect from these confounders. Our research design addresses this identification problem by exploiting existing discontinuities in fixed-part price variation at different interview dates. The sample design of the Spanish HBS is stratified in such a way that it provides identical groups of households across different rounds of interviewing. This means these groups are identical except for the interview date.

The INE collects information by means of interviews conducted over two-week periods, during which all goods and services consumed are recorded, including any electricity bills. The INE then converts this fortnightly consumer data to yearly data. Sampling is stratified across 26 two-week periods of every year, which means we exploit 26 homogenous subsamples per year in defining our control and treated groups—that is, households whose electricity bills fall under the reform can be separated from those whose bills are unaffected by the reform. The key to our identification strategy is that a household's second interview is held in the same two-week period of the year as that of the interview conducted in its first year of participation; i.e., if household i was interviewed in the first two-week period of April in year t, it will be interviewed again in the same two-week period in $t+1$. Figure 3 presents our identification strategy in diagrammatic form.

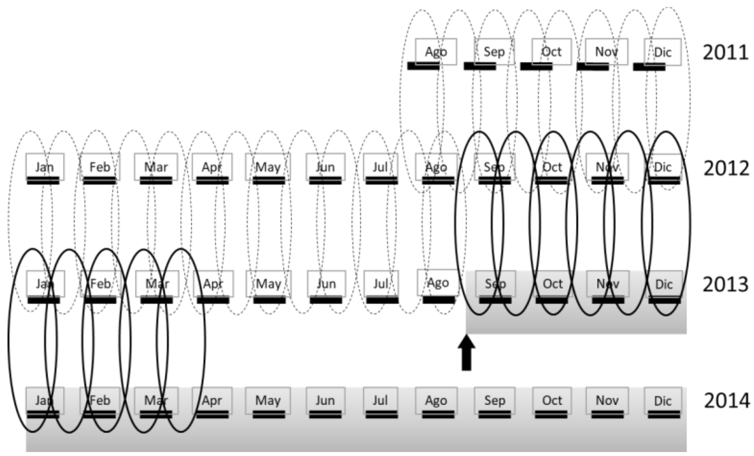

FIGURE 3. RESEARCH DESIGN

Notes: The figure shows the sample design of the Spanish HBS across the years sampled. Each circle represents a household interviewed twice, once each year, and both conducted in the same period. Continuous circles correspond to households in the treated group (households whose second interview was conducted later than August 2013), whereas dashed circles correspond to those in the control group (households for whom both interviews were conducted prior to the reform in August 2013).

In the first step, we distinguish the households whose two annual interviews occurred pre-reform (control group) from those whose second interview occurred post-reform (treated group). Let *D* be a dummy variable that is equal to 1 for the treated households such that:

(2)
$$D = \begin{cases} 0, & if\ i_1 < i_2 < R \\ 1, & if\ i_1 < R < i_2 \end{cases}$$

where i_1 is the first interview date, i_2 is the second interview date, and *R* is the date of the reform (i.e. August 1, 2013). Then *T*, a subset of *D*, is a post-reform dummy that identifies interviews conducted after the introduction of the reform. That is,

(3)
$$T = \begin{cases} 0, & if\ i_2 < R \\ 1, & if\ i_2 > R \end{cases}$$

Thus, while *D* allows us to identify the treated and control household groups, *T* is the main variable capturing the effect of the reform. If q_{i0} denotes the amount of electricity consumed by household *i* when $T = 0$, and q_{i1} denotes the amount consumed when $T = 1$, then we assume that $q_i = q_{0i} + T(q_{1i} - q_{0i})$. The key here lies in the assumption of a sampling balance between the treated and control groups prior to the reform, which, in our case, is guaranteed by the sample design. Table 1 shows the summary statistics by control and treatment groups. Figure 4 compares the empirical distribution of control ($D = 0$) and treated ($D = 1$) households before the reform in relation to a number of key variables. Given that households appear to be similar in both pre-reform subsamples, assignment to the treatment group would appear to be independent of their observable characteristics, including potential outcome. The only confounding differences are those that may arise from different weather conditions the households faced in the month of the interview. However, since weather is a function of geographical location and time, we use regional and quarterly dummies to control for its potential confounding effects. Hence, conditional on pre-reform observable characteristics, assignment to the treatment group appears to be as good as random assignment. In short, the conditional independence assumption seems plausible here.

TABLE 1. SUMMARY STATISTICS

	Full sample		**Control Group (D = 0)**		**Treated Group (D = 1)**	
Variable	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**
From August 2011 to April 2014						
T (treatment dummy)	0.19	0.39	0	0	0.5	0.5
Electricity consumption (kWh)	3,425	2,029	3,524	2,084	3,264	1,927
Electricity expenditure (€)	754	409	756	414	750	400
Individual price (€/kWh)	0.23	0.04	0.22	0.04	0.24	0.04
Total expenditure (€)	22,801	14,739	22,889	14,745	22,660	14,727
Total income (€)	23,793	14,791	23,919	14,753	23,588	14,850
Education level (household head)	2.56	1.09	2.56	1.08	2.56	1.09
Household economic situation	1.73	0.87	1.73	0.87	1.74	0.86
Household size	2.81	1.25	2.81	1.27	2.82	1.23
Elderly (dummy)	0.32	0.47	0.32	0.47	0.33	0.47
Retirement income (dummy)	0.41	0.49	0.41	0.49	0.42	0.49
Number of rooms	5.25	1.18	5.26	1.19	5.23	1.16
Surface (m2)	105	47	106	48	104	46
Capital of province	0.32	0.47	0.34	0.47	0.31	0.46
Autonomous community (region)	9.01	5.03	8.95	5.02	9.1	5.04
Municipality size	2.75	1.63	2.7	1.63	2.83	1.61
Population density	1.84	0.86	1.83	0.87	1.86	0.84
Renting (dummy)	0.1	0.3	0.09	0.29	0.1	0.3
Urban area (dummy)	0.81	0.4	0.8	0.4	0.81	0.39
Building age >25years (dummy)	0.63	0.48	0.63	0.48	0.62	0.49
Two-week period	14	8.35	13.43	7.63	15	9.35
Year	2012	1	2012	1	2013	1

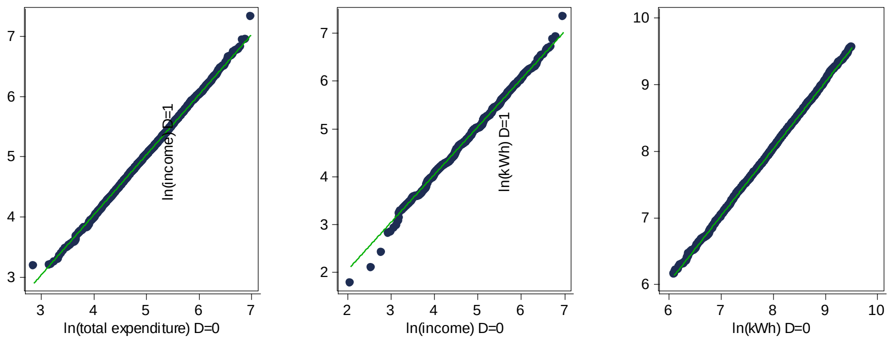

FIGURE 4. QQ-PLOT COMPARING COVARIATES PRE-REFORM

Notes: The panels show the empirical quantile-quantile plot for the quantiles of the control group; that is, households whose two interviews were conducted before the reform was implemented (D = 0) vs. the quantiles for the treated group corresponding to their first interview (D = 1).

C. Empirical model

Our baseline specification for estimating the average treatment effect (ATE) of the reform takes the form of a structural demand model:

(4) $$\ln(q_{it}) = \alpha + \beta \ln(p_{it}) + \beta \ln(y_{it}) + \gamma X_{it} + \delta T_{it} + \theta_i + \varphi_t + \varepsilon_{it}$$

where $\ln(q_{it})$ measures the natural log of kWh consumed by household i in year t, $\ln(p_{it})$ measures the natural log of total kWh price (including all taxes), and $\ln(y_{it})$ measures income. X is a set of relevant socioeconomic controls (age, household size, education level, region, etc.), including a quarterly dummy to control for any potential stationary effects. θ_i and φ_t are the household and the year-fixed effects, respectively, and ε_{it} is the error term. We further cluster standard errors at the household level. T is our post-reform treatment variable.

The coefficient δ measures the average effect of the reform. More formally, $\delta = (q_{0t} - q_{0t-1}) - (q_{1t} - q_{1t-1})$, a difference-in-differences estimator. Given the quasi-experimental setting, the policy change is systematically unrelated to other factors that may affect electricity consumption.

We complement the analysis with a quantile regression model (Koenker and Basset 1978) to assess the reform's distributional impact, an issue particularly relevant to electricity consumption. Given the φ-quantile linear distribution of q_i conditioned on X and T:

$$Q_{\varphi}(q|T,X) = \alpha_{\varphi}T + X'\beta_{\varphi} \tag{5}$$

The quantile regression estimator consists then of solving:

$$\left(\alpha_{\varphi}, \beta_{\varphi}\right) = \min_{\alpha,\beta}[\rho_{\varphi}(q - \alpha T - X'\beta)] \tag{6}$$

where $\rho_{\varphi} = (\varphi - 1)\lambda$ for $\lambda < 0$. Therefore, α_{φ} is the treatment effect in the φ-quantile. The model is estimated with bootstrap standard errors.

III. Empirical analysis and results

A. Average effect

Table 2 reports the main electricity demand function estimates in which models 1 and 2 use household electricity consumption as the dependent variable, and models 3 and 4 use household electricity expenditure. The ATE corresponds to the coefficient δ of the treatment variable T that can be read as the percentage change in household electricity consumption/expenditure due to the reform. Since households receive their electricity bills at the end of the billing period (one month) and do not usually have access to daily information about their consumption or prices, the actual response to the reform should be registered one month after it was introduced. In this respect, the treated households correspond to those that receive, for a second time, a reformed electricity bill. The models are estimated by OLS (columns 1 and 3) and by LSDV (columns 2 and 4) so as to take into account the household fixed effects. All regressions are controlled for several relevant socioeconomic variables[3] and for quarterly dummies to control for potential stationarity issues.

The treatment coefficient is significant and negative in all specifications. According to the OLS specifications (column 1), treated households reduced their electricity consumption by about 18%. When unobserved heterogeneity is controlled for (column 2), the ATE is a 15% reduction. Therefore, despite the reduction in the volumetric rate, when the fixed charge is raised, households respond by reducing their electricity consumption. This first result is in line with studies conducted elsewhere that find that consumers react to average prices rather than to marginal prices (Ito 2014). Were consumers to respond to marginal prices, consumption would increase since, in contrast to the average price, the volumetric rate fell (because of the reform itself and because of wholesale prices; see Figure A1). This means consumers use electricity at a level lower than that expected from a TPT scheme, with lower allocative efficiency.

TABLE 2. EFFECT OF THE REFORM ON RESIDENTIAL ELECTRICITY CONSUMPTION AND EXPENDITURE (2011–2014).

VARIABLES	(1) ln(electricity consumption)	(2) ln(electricity consumption)	(3) ln(electricity expenditure)	(4) ln(electricity expenditure)
Reform (*T*)	−0.178***	−0.154***	−0.156***	−0.098***
	(0.027)	(0.028)	(0.027)	(0.028)
ln(price kWh)	−1.044***	−1.026***	−0.049*	−0.027
	(0.024)	(0.025)	(0.025)	(0.025)
ln(income)	0.396***	0.289*	0.393***	0.288*
	(0.075)	(0.120)	(0.075)	(0.120)
ln(income)^2	−0.028***	−0.028*	−0.028***	−0.028*
	(0.008)	(0.012)	(0.008)	(0.012)
Months since reform	0.020*	0.007	0.009	−0.016
	(0.008)	(0.009)	(0.008)	(0.009)
Constant	−0.305	0.393	4.568***	5.317***
	(0.245)	(0.358)	(0.245)	(0.358)
Household fixed effects	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	20,872	20,872	20,872	20,872
R-squared	0.295	0.177	0.189	0.024
R2 adj.	0.293	0.176	0.188	0.0231
Number of id		10,729		10,729

Robust standard errors in parentheses
*** p < 0.001, ** p < 0.01, * p < 0.05

[3] Specifications without controls have also been considered, but are not shown here, as the results are virtually identical. Models with different interactions of the treatment variable were also considered, but they too showed no relevant changes. See Table A2 in the Appendix.

Columns 3 and 4 provide the results for household electricity expenditure and thus illustrate whether the reform's main (revenue) objective is met. Here, the coefficients are also negative, albeit slightly smaller than those for consumption: When fixed effects are taken into account, households reduce expenditure by 10% (compared to the 15% reduction in consumption). This suggests that, despite the fixed charge increase and because consumers fail to respond to marginal prices, the reduction in consumption drags electricity expenditure down with respect to the pre-reform situation, as the increase in the fixed charge fails to compensate for the revenue loss associated with the contraction in demand.

An alternative explanation may lie with changes in contracted load capacities. Since fixed charges are linked to these contracted loads, households may have reacted to the reform by reducing their load capacities in order to cut their electricity bills. The available data do not allow us to test this hypothesis; however, while a reaction along these lines might be expected, according to Spain's energy regulator (CNMC, 2015), the average contracted load capacity did not change significantly in the sampled period (see Figure A3).[4] Yet, even if there had been a reduction in load capacities, this would fail to explain the significant reduction in electricity consumption unless the change in contracted load capacity was associated with increased investment in more efficient appliances (ruled out, as indicated, by our robustness checks) or with a radical change in habits.

These results are robust to different specifications and control variables; however, since the identification strategy depends on the interview date – as do the other controls, including distance from the reform and the quarterly dummies – Table 3 complements the previous results by limiting the sample to that of the year of reform itself, i.e. 2013. By so doing, we compare annual household consumption (expenditure) within the same year, and other regulatory or economic confounding events are ruled out. Different specifications are shown and, in this case, we cluster standard errors at the two-week period. Results remain consistent with the panel data estimation, albeit with a lower magnitude due to the smaller

[4] The CNMC periodically publishes the monthly average contracted load capacity. Figure A3 in the Appendix shows that the evolution of the average contracted capacity and the change in contracted capacities is not significant enough to explain the reduction observed.

sample size, as well as the lower number of treated households (those treated in 2014 are not considered). Yet, this cross-sectional evidence can be read in terms of the short-run effect of the reform, while higher panel data coefficients, insofar as they come from a within estimator, may be interpreted as the reform's long-run effect.

TABLE 3. EFFECT OF THE REFORM ON RESIDENTIAL ELECTRICITY CONSUMPTION (2013).

VARIABLES	(1) ln(electricity consumption)	(2) ln(electricity consumption)	(3) ln(electricity expenditure)	(4) ln(electricity expenditure)
Reform (*T*)	−0.124***	−0.096***	−0.082**	−0.078**
	(0.027)	(0.024)	(0.024)	(0.022)
ln(price kWh)		−1.093***		−0.102
		(0.054)		(0.051)
ln(income)		0.339**		0.337**
		(0.091)		(0.091)
ln(income)^2		−0.022*		−0.022*
		(0.009)		(0.009)
Constant	7.941***	−0.632	6.461***	4.321***
	(0.023)	(0.365)	(0.021)	(0.356)
Controls	No	Yes	No	Yes
Observations	18,770	14,894	18,770	14,894
R-squared	0.004	0.289	0.002	0.182
R2 adj.	0.00430	0.287	0.00215	0.180

Robust standard errors in parentheses
*** p < 0.001, ** p < 0.01, * p < 0.05

B. Distributional effects

The impact of an increase in the fixed-price component is, necessarily, heterogeneous across households and more specifically tends to be regressive. In the case of US residential natural gas, Borenstein and Davis (2012) show that a similar reform would be regressive, although financing fixed costs through markups of the volumetric rate was only mildly progressive because of the correlation between gas consumption and income. In their study of heating-price reform in China, Ito and Zhang (2018) also find that moving from a plain tariff to a TPT was regressive; however, this is in part explained by the particular ex-ante situation where the plain tariff was a function of household dwelling size (and therefore progressive). In this section, we analyze the distributional incidence of the Spanish reform across households.

Tables 4 and 5 show the results obtained when applying a quantile regression model (with bootstrapped standard errors) for consumption and expenditure, respectively. More specifically, they show the effects of the reform conditional on the deciles of electricity consumption. Since the coefficients in a quantile regression are not controlled by unobserved heterogeneity (as they are in the fixed effect model), the coefficients shown underestimate the actual impact, presumably in a similar way to the OLS estimations above.

TABLE 4. QUANTILE REGRESSION ON ELECTRICITY CONSUMPTION (2013)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	q10	q20	q30	q40	q50	q60	q70	q80	q90
Reform (*T*)	0.006	−0.056***	−0.054***	−0.082***	−0.095***	−0.119***	−0.135***	−0.155***	−0.169***
	(0.018)	(0.015)	(0.013)	(0.011)	(0.010)	(0.011)	(0.011)	(0.013)	(0.019)
ln(price kWh)	−0.946***	−1.082***	−1.136***	−1.168***	−1.168***	−1.159***	−1.168***	−1.192***	−1.184***
	(0.049)	(0.044)	(0.035)	(0.033)	(0.031)	(0.036)	(0.042)	(0.041)	(0.045)
ln(income)	0.560**	0.443***	0.456***	0.344**	0.203	0.204	0.213*	0.136	0.080
	(0.187)	(0.120)	(0.109)	(0.126)	(0.118)	(0.115)	(0.088)	(0.125)	(0.128)
ln(income)^2	−0.044*	−0.032**	−0.034**	−0.023	−0.007	−0.008	−0.009	−0.003	0.005
	(0.019)	(0.012)	(0.011)	(0.013)	(0.012)	(0.012)	(0.009)	(0.013)	(0.013)
Constant	−0.840	−1.231***	−1.462***	−1.258***	−0.846*	−0.653	−0.564	−0.330	0.002
	(0.535)	(0.366)	(0.303)	(0.366)	(0.364)	(0.419)	(0.356)	(0.399)	(0.433)
Observations	14,894	14,894	14,894	14,894	14,894	14,894	14,894	14,894	14,894
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
*** p < 0.001, ** p < 0.01, * p < 0.05

TABLE 5. QUANTILE REGRESSION ON ELECTRICITY EXPENDITURE (2013)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	q10	q20	q30	q40	q50	q60	q70	q80	q90
Reform (*T*)	0.024	−0.037*	−0.039**	−0.061***	−0.080***	−0.098***	−0.115***	−0.143***	−0.148***
	(0.017)	(0.015)	(0.014)	(0.012)	(0.014)	(0.012)	(0.010)	(0.014)	(0.022)
ln(price kWh)	0.053	−0.090*	−0.148***	−0.171***	−0.167***	−0.173***	−0.181***	−0.208***	−0.215***
	(0.039)	(0.035)	(0.035)	(0.030)	(0.026)	(0.028)	(0.026)	(0.038)	(0.046)
ln(income)	0.611***	0.432***	0.463***	0.360***	0.224**	0.220**	0.208**	0.152	0.172
	(0.172)	(0.120)	(0.109)	(0.093)	(0.085)	(0.080)	(0.077)	(0.107)	(0.136)
ln(income)^2	−0.049**	−0.031*	−0.035**	−0.024*	−0.010	−0.010	−0.009	−0.004	−0.006
	(0.018)	(0.012)	(0.011)	(0.010)	(0.009)	(0.008)	(0.008)	(0.011)	(0.014)
Constant	4.027***	3.750***	3.446***	3.690***	4.119***	4.234***	4.387***	4.547***	4.604***
	(0.490)	(0.404)	(0.349)	(0.299)	(0.263)	(0.302)	(0.265)	(0.369)	(0.452)
Observations	14,894	14,894	14,894	14,894	14,894	14,894	14,894	14,894	14,894
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
*** p < 0.001, ** p < 0.01, * p < 0.05

The conditional median estimate coincides very closely with the conditional mean estimates reported in Table 3. The ATE estimate therefore provides a good measure of the central location of the distribution (a 9.6% reduction for consumption and an 8% reduction for expenditure). However, the remaining conditional quantiles are statistically different from the average effect. Figure 5 further shows that the effect of the reform differs markedly across deciles: While the top deciles respond much more markedly than average, the bottom deciles respond very little or even fail to react at all (the latter being the case for the decile at the very bottom, 0.10). The top decile (0.90) reacted to the reform by reducing electricity consumption by as much as 17%.

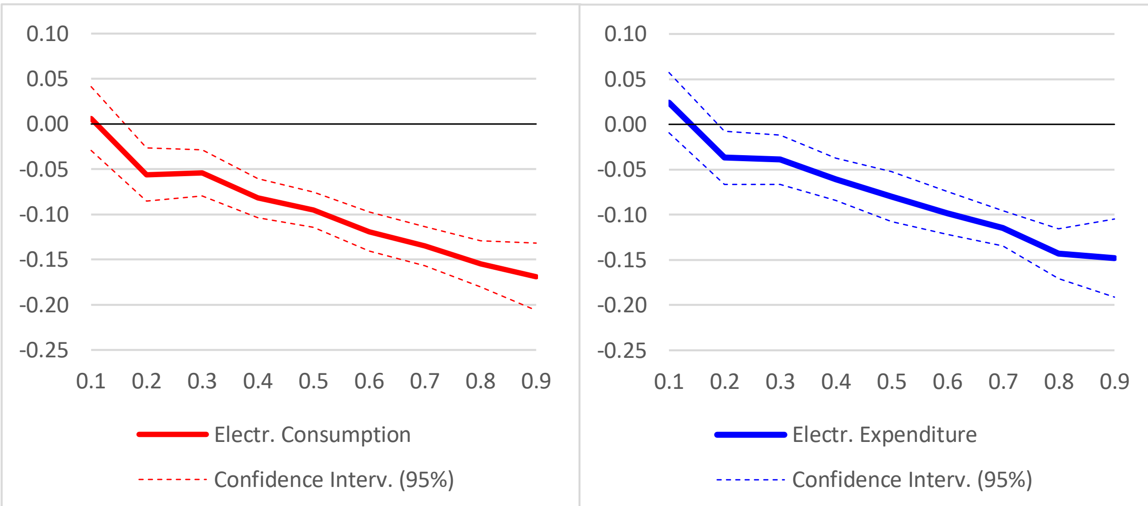

FIGURE 5. REFORM EFFECT BY QUANTILES OF ELECTRICITY CONSUMPTION AND EXPENDITURE.

Two caveats should, however, be noted when interpreting the results for the extreme deciles. First, for households with more than 10 kW of contracted capacity (5.9%), ostensibly the highest electricity consumers, the fixed-part increase was 25%, which was much lower than for the rest of the households. This discontinuity in the treatment could have affected the estimated impact in the top deciles. Second, at the time of the reform, about 10% of Spanish households were entitled to a social bonus program providing for a 25% reduction in their electricity bills when they met at least one of the program's conditions. Of the households entitled to the social bonus, 81% meet the conditions on the grounds that they contract the low capacity charge (less than 3 kW). Other conditions include unemployment, different categories of disability, and large families. These households are among the lowest electricity consumers, which means that our estimation for this bottom decile may be confounded by this policy. Since we observe neither participation on the social bonus program nor the capacity contracted, we cannot control for these potential confounders. Fortunately, however, these characteristics are limited to the extreme deciles and do not prevent us from observing the heterogeneity of the reform's impact on different electricity consumption profiles.

Interestingly, however, and as reported by Borenstein and Davis (2012), household electricity consumption and household income are only weakly correlated (Figure A4), so impact on consumption levels does not necessarily correspond to impact on income levels.[5] To measure the reform's distributional incidence, Figure 6 shows the relative burden of electricity expenditure (as measured by the median) across income levels for the time period covered by our analysis. The steep decline across income deciles suggests that electricity price increases, such as those introduced by the 2013 Spanish reform, have regressive effects.

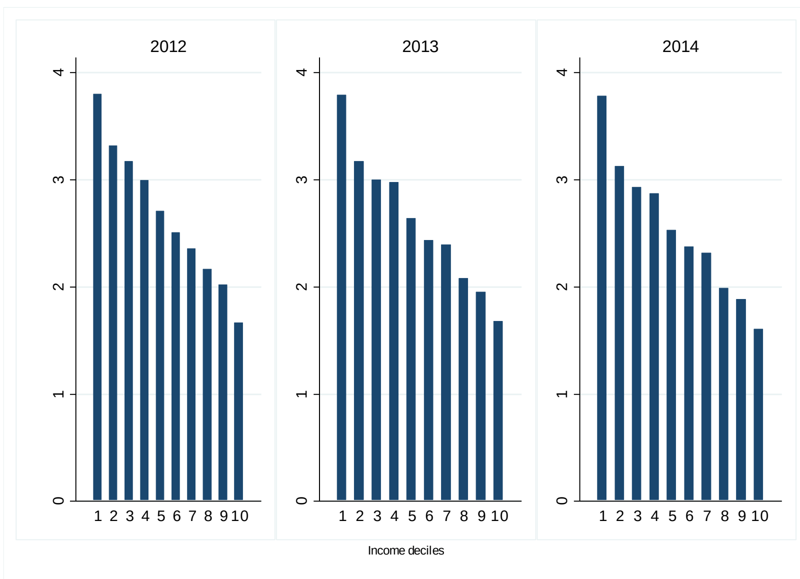

FIGURE 6. ELECTRICITY EXPENDITURE AS A SHARE OF TOTAL EXPENDITURE.

To unambiguously quantify the reform's degree of implicit regressivity, we compute the two progressivity indices proposed by Suits (1977) and Kakwani (1977) for both treated and control households. We do so by computing Gini indices and concentration curves of

[5] Table A2 in the Appendix shows a specification in which the reform interacts with income and this results in not being statistically significant. Table A3 shows that when fixed effects are taken into account, the reform impact is not statistically different for different income groups.

electricity expenditure and total household expenditure (Sterner 2012; Sahn and Younger 2003). Both indices show negative signs, suggesting the reform was regressive (Table 6).

TABLE 6. KAWKANI AND SUITS INDICES FOR ELECTRICITY EXPENDITURE BEFORE AND AFTER THE REFORM

	T = 0	**T = 1**
Kawkani progressivity index	−0.2247	−0.2305
Suits progressivity index	−0.2268	−0.2332

C. Robustness checks

In order to examine the robustness of the above estimates, we conduct two additional analyses: First, we test that the results are indeed driven by the reform and not by potential confounders, such as changes in home appliances or switching energy sources. By controlling for these factors, we can see the extent to which our core estimates are driven by exogenous technology changes or by the reform itself. Second, we perform a falsification test by estimating placebo reforms. These are also estimated by means of a non-parametric method (matching estimator) so that we test our results using a different methodology.

As shown in the previous section, our results are robust to different specifications and time spans. However, in recent years, not only have home appliances become more energy-efficient, but new labeling regulations have made this information more readily available to consumers who are better placed to optimize energy-saving investments. To ensure that our estimates are not being confounded by this technology trend, Table 7 includes additional controls covariates related to household investment in new home appliances, particularly investments in (i) all kinds of home appliances, (ii) small electric appliances such as bulbs, and (iii) changes in the heating or water boiler. Compared to our core estimations, the reform coefficient remains virtually unchanged; the panel data model in column 1 shows that the reform coefficient remains at −16%, although this estimate is only significant at the 0.1 level. For cross-sectional models, results remain at −9%, as for the previous specifications. Investment in home appliances does, however, show a significant impact on electricity consumption (columns 2 and 3), albeit with a positive impact on electricity demand.

TABLE 7. EFFECT OF REFORM WHEN CONTROLLING FOR HOUSEHOLD INVESTMENT IN DIFFERENT HOME APPLIANCES

VARIABLES	(1) ln(electricity consumption)	(2) ln(electricity consumption)	(3) ln(electricity consumption)
Reform (T)	−0.164	−0.0899**	−0.0985**
	(0.0899)	(0.0310)	(0.0314)
ln(price kWh)	−0.999***	−1.238***	−1.166***
	(0.0731)	(0.0654)	(0.0975)
ln(income)	0.437	0.123	0.161
	(0.338)	(0.109)	(0.274)
ln(income)^2	−0.0368	0.00331	0.00286
	(0.0349)	(0.0110)	(0.0274)
ln(investment home appliances)	0.00716	0.0148***	0.0162**
	(0.00503)	(0.00384)	(0.00514)
ln(investment bulbs & other)	0.00301	−0.00149	−0.00182
	(0.00361)	(0.00244)	(0.00330)
Change energy source water boiler (= 1)			0.0589
			(0.0656)
Change energy source heating (= 1)			0.0695
			(0.0477)
Constant	0.296	−0.754	−0.479
	(0.948)	(0.530)	(0.826)
Year FE	Yes	No	No
Household FE	Yes	No	No
Observations	5,595	3,888	1,653
R-squared	0.159	0.181	0.199
Number of id	4,415		
Controls	NO	NO	NO
R2 adj.	0.158	0.180	0.195

Robust standard errors in parentheses
*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Second, our falsification test involves the introduction of a placebo reform in the same period, but one year earlier. This means that treated and control households are once again allocated in relation to the week they were interviewed, but this time as if the reform had occurred one year earlier, in August 2012 as opposed to August 2013. No other reform measures were adopted at that time that might have confounded our placebo estimates. Table 8 shows the coefficients of the placebo reform for consumption and expenditure in columns 1 and 2, respectively (columns 3 and 4 show previous results for the sake of comparison). The placebo coefficient is non-significant in the case of consumption, confirming the estimated effect of the reform. In contrast, in the case of electricity expenditure, the coefficient shows a significant positive sign that can only be driven by the increase in electricity prices on the wholesale market at that time (see Figure A1).

TABLE 8. PLACEBO REFORM REGRESSION ESTIMATES COMPARED WITH THOSE OF ACTUAL REFORM

VARIABLES	(1) ln(electricity consumption)	(2) ln(electricity expenditure)	(3) ln(electricity consumption)	(4) ln(electricity expenditure)
Placebo reform	0.031	0.059*		
	(0.026)	(0.026)		
Reform (T)			−0.096***	−0.078**
			(0.024)	(0.022)
ln(price kWh)	−1.124***	−0.115*	−1.093***	−0.102
	(0.040)	(0.042)	(0.054)	(0.051)
ln(income)	0.399**	0.398**	0.339**	0.337**
	(0.117)	(0.118)	(0.091)	(0.091)
ln(income) x ln(income)	−0.028*	−0.028*	−0.022*	−0.022*
	(0.012)	(0.012)	(0.009)	(0.009)
Constant	−0.846	4.182***	−0.632	4.321***
	(0.415)	(0.427)	(0.365)	(0.356)
Controls	Yes	Yes	Yes	Yes
Observations	14,705	14,705	14,894	14,894
R-squared	0.293	0.186	0.289	0.182

Robust standard errors in parentheses
*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

We performed further falsification tests using a matching estimator based on the Mahalanobis distances (calculated with relevant covariates) between treated and control groups (Table 9). After ensuring both the real treatment and placebo samples are well balanced (Table A4, Appendix) and therefore comparable, the non-significance of the placebo reform further confirms our results: The reform caused a contraction in demand.[6]

TABLE 9. MATCHING ESTIMATOR (2013 SAMPLE)

	Electricity Consumption	Electricity Expenditure	Electricity Consumption	Electricity Expenditure
Reform T (1 vs 0)	−0.142***	−0.113***		
	(0.0154)	(0.0151)		
Placebo reform			−0.011	0.0784***
			(0.0168)	(0.0165)
Estimator	Nearest N.	Nearest N.	Nearest N.	Nearest N.
Matches requested	10	10	10	10
Distance metric	Mahalanobis	Mahalanobis	Mahalanobis	Mahalanobis
Sample	2013	2013	2012	2012
Observations	8,185	8,185	7,208	7,208

Robust standard errors in parentheses
*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

[6] Chetty et al. (2009) show how tax salience has an impact on consumer demand: The more salient the tax, the more consumer demand overreacts. In Spain, electricity prices have been historically salient for various political and economic reasons. A simple Google Trends search using the words "factura de la luz" (electricity bill), "luz" (electricity), "ahorro factura de la luz" (savings on electricity bill), and any other suggestions provided by the Google algorithm shows how these search queries increased significantly, peaking just after the August 2013 reform. Indeed, if we include households that paid the first "reformed" electricity bill in the treated group (recall our treated group in the analysis includes those households that received a second reformed bill), the OLS coefficient is 0.119 (vs. 0.178 as reported in Table 2). This difference in coefficients can be attributed to the different salience acquired by electricity prices immediately after the reform. Further research is needed to disentangle the particular drivers of potential behavioral responses.

Coefficients remain consistent across the different model specifications and methods. Hence, based on the quasi-experimental research design applied to a structural demand model in a highly representative population sample, our robustness checks confirm that the reform led to a reduction in electricity demand that ended up reducing electricity expenditure (and thus negatively affecting the industry's cost recovery efforts).

IV. Conclusion and discussion

Full cost recovery is critical to ensure reliable and sustainable electricity supplies, and moreover, it is a principle that underpins electricity tariff setting. As the power sector has been subject to major transformations in recent years, increasing the share of fixed costs recovered through the fixed charge to consumers via their electricity bill – as opposed to markups at the volumetric rate – has become commonplace on regulatory agendas in many countries. In this paper, we have shown that, owing to household misperceptions of true marginal costs, electricity industry revenues do not rise as expected following an increase in the fixed charge. Using the data collection method employed by the HBS to analyze the 2013 major Spanish electricity reform, we find quasi-experimental evidence that, despite a decrease in the marginal price, households reduce their electricity consumption when faced with an increase in the fixed charge on their electricity bill. This strongly suggests that consumers fail to discriminate between marginal and fixed costs and thus optimize consumption at the average price rather than the marginal price. This result has timely and far-reaching policy implications for upcoming reforms to electricity price settings.

First, suboptimal behavior of this type results in lower electricity expenditure, which in turn significantly undermines fixed-cost recovery. To the extent that consumers react to a (higher) average price, as opposed to a (lower) marginal price, the reduction in consumption cuts household expenditure to the point that the revenues raised from the higher fixed charge may fail to offset the revenue loss from the contraction in electricity demand. The Spanish electricity industry actually ended up in a worse financial situation than if the reform had not been introduced.

An alternative hypothesis that we unfortunately cannot test directly is that households responded to the reform by adapting their contracted load capacity, and therefore reduced their electricity expenditure. However, according to the statistics published by the Spanish energy regulator, the average load capacity did not change significantly following the reform. In any case, although in theory electricity expenditure could have fallen because of changing capacities, this would not explain the sizeable reduction observed in electricity consumption unless the capacity reduction was linked to radical changes in consumption habits or to a reduction in household energy needs—that is, to an investment in more efficient appliances. Our robustness checks discarded this possibility.

Second, the impact of a reform of this kind is heterogeneous across households and electricity price increases brought about by a rise in the fixed-change component are regressive, in line with the general findings of the literature. Moreover, a quantile regression shows that the highest electricity consumers reduce their electricity consumption and expenditure the most, while the lowest electricity consumers fail to respond at all.

Third, allocative efficiency, the other ultimate aim of a TPT scheme, is not achieved as consumer misperceptions of the fixed price component result in their reacting to a higher average price rather than to the true marginal price. As a result, electricity consumption is lower than it would have been had consumers reacted to the marginal price. From a climate policy perspective, this lower level of consumption can be considered a positive side effect, as it reduces energy-related emissions. From a welfare perspective, however, this is positive only if the pre-existing electricity price was below its social marginal cost and thus the reform implicitly solves this issue (thanks to consumer misperceptions). If the resulting price signal is higher than the social marginal cost, this would entail a lower-than-optimal electricity use.

Overall, we find robust evidence that the reform led to a reduction in residential electricity demand. Given our quasi-experimental research design, applied here in a structural model, and the representativeness of our sample, the policy implications identified are clearly not limited to Spain and should be of interest to those about to undertake similar reforms. Yet, a number of key research questions remain unanswered by our analysis: Most specifically, why it is that consumers fail to identify marginal costs. A potential hypothesis – and one confirmed by the literature – is that of imperfect information: First, in relation to prices, as non-linear tariffs are complex and require considerable cognitive effort to be understood; and second, in relation to quantities, as most consumers are unable to track their cumulative electricity consumption during the billing period.

A further potential driver that may help explain our results is the salience of the reform itself. It is well established in the literature that, in the case of gasoline, the salience of new taxes can have its own impact on demand, regardless of actual prices (Rivers and Schaufele 2015; Li, Linn, and Muehlegger 2014; Davis and Killian 2011). Likewise, tax persistence compared to market prices also has an impact on demand (Tiezzi and Verde 2018; Davis and Killian 2011). Both salience and persistence result in a significantly greater impact on demand than is otherwise expected from market price elasticities; however, little research has been conducted to date on these issues in the case of the electricity market, with the sole exception of Gilbert and Zivin (2014). It could be that both salience and persistence reinforced the demand response identified here. In all circumstances, correcting imperfect information at the household level would undoubtedly improve market efficiency in both the short and long run. Indeed, climate stability could well depend on it.

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APPENDIX

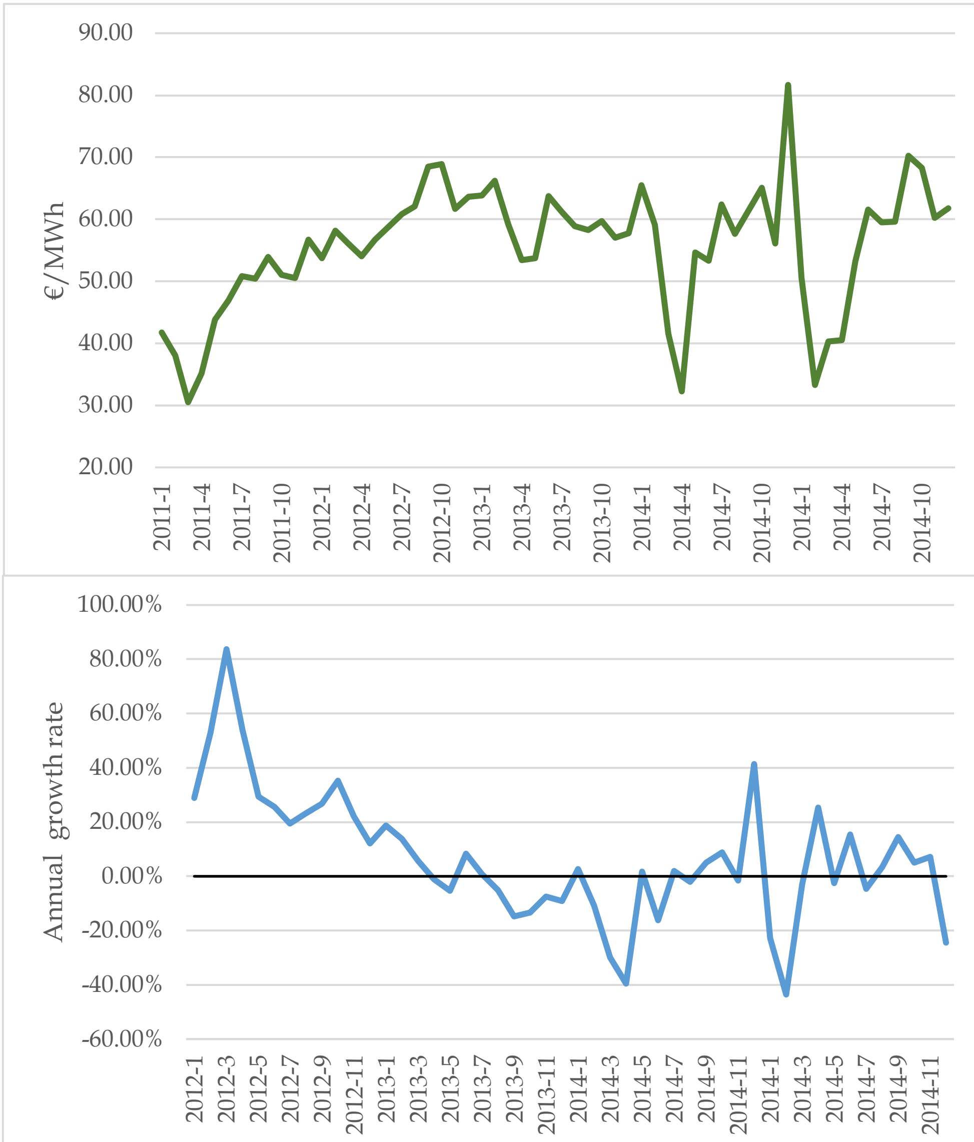

FIGURE A1. WHOLE SALE PRICE EVOLUTION IN SPAIN

Notes: The data are from Red Eléctrica de España (REE).

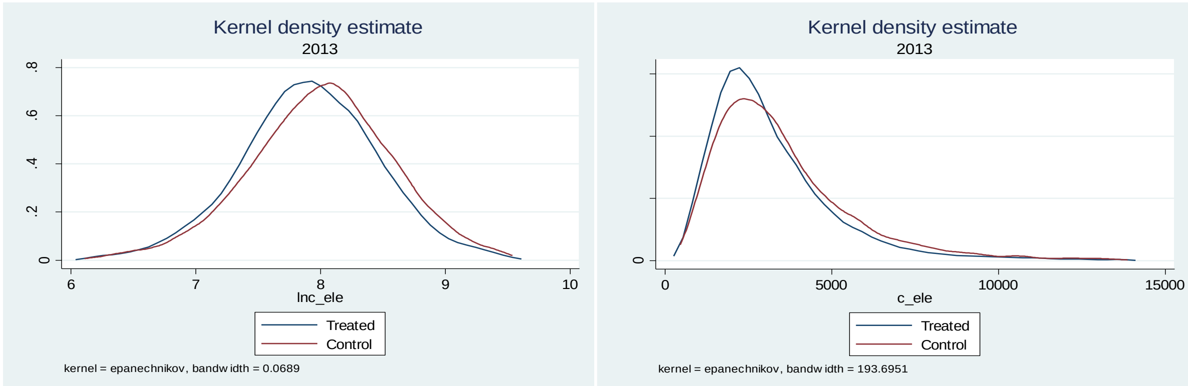

FIGURE A2. KERNEL DENSITIES OF KWH CONSUMED PER HOUSEHOLD IN TREATED AND CONTROL GROUPS

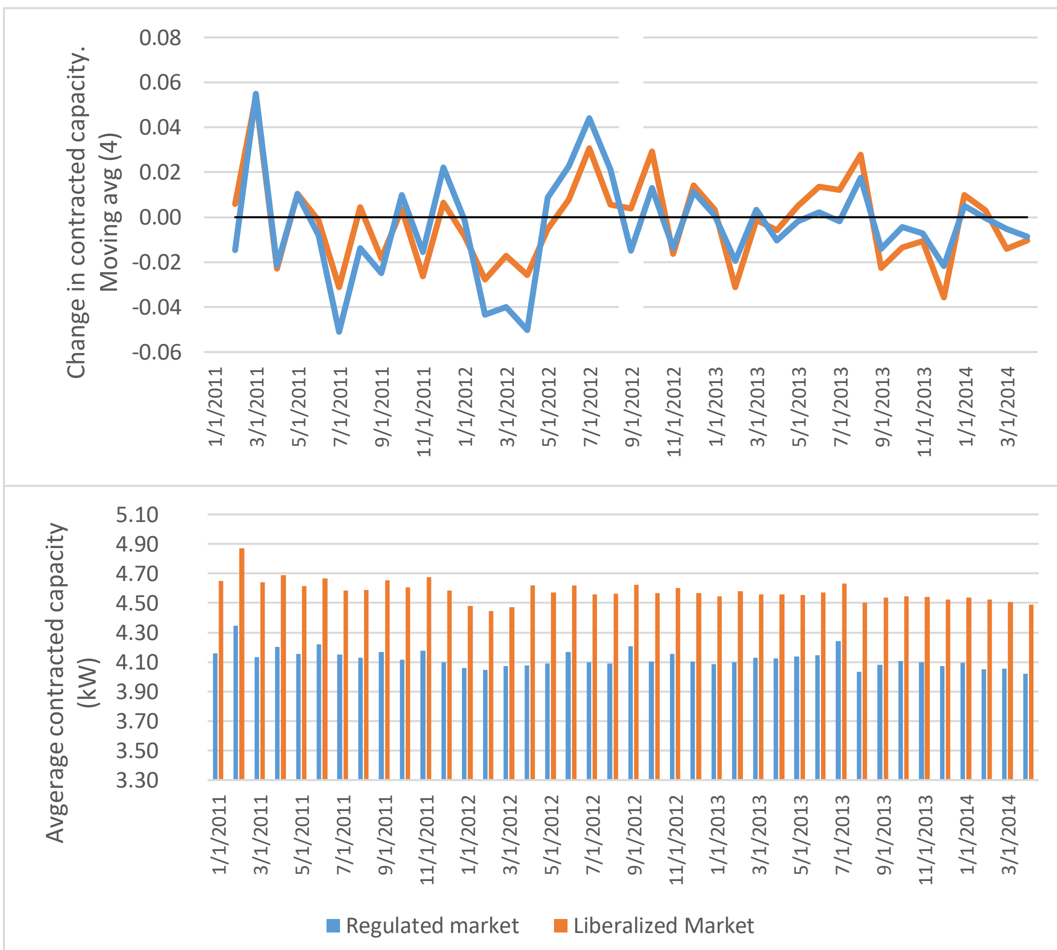

FIGURE A3. EVOLUTION OF THE AVERAGE CONTRACTED CAPACITY LOAD

Notes: These average contracted capacity loads correspond to total average capacity of regulated and liberalized markets. This is virtually equivalent to contracted capacities in tariffs 2.0A that make above 90% of consumers. (Data taken from Boletín de Indicadores Eléctricos, CNMC (2011–2015)).

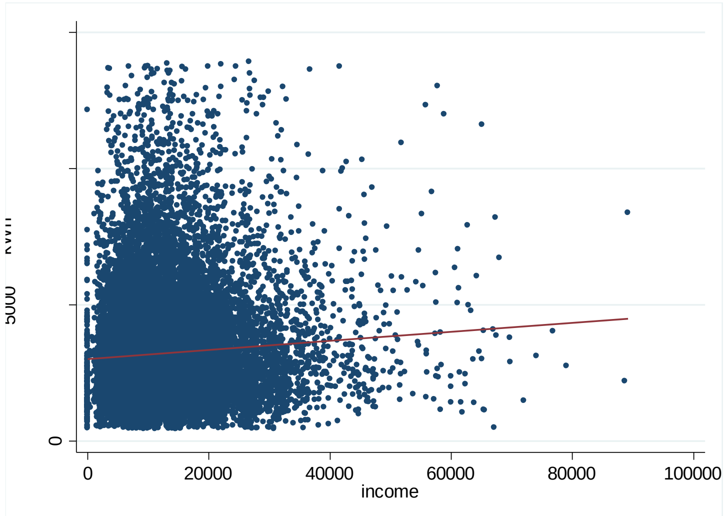

FIGURE A4. ELECTRICITY CONSUMPTION AND HOUSEHOLD INCOME

Notes: Graph shows correlation between household electricity consumption and equivalent adult income.

TABLE A2. EFFECTS ON ELECTRICITY CONSUMPTION AND EXPENDITURE. SPECIFICATIONS WITH INTERACTIONS (NO CONTROLS). 2011–2014

VARIABLES	(1) ln(electricity consumption)	(2) ln(electricity consumption)	(3) ln(electricity consumption)	(4) ln(electricity consumption)	(5) ln(electricity expenditure)	(6) ln(electricity expenditure)	(7) ln(electricity expenditure)	(8) ln(electricity expenditure)
Reform (T)	−0.199***	−0.703	−0.165***	−0.212	−0.177***	−0.680	−0.109***	−0.180
	(0.026)	(0.467)	(0.025)	(0.454)	(0.026)	(0.467)	(0.025)	(0.454)
T x ln(price kWh)		−0.015		−0.036		−0.015		−0.041
		(0.050)		(0.049)		(0.051)		(0.049)
T x ln(income)		0.175		−0.084		0.177		−0.088
		(0.140)		(0.140)		(0.140)		(0.139)
ln(income)	0.194**	0.166*	0.212*	0.225*	0.190**	0.162*	0.213*	0.225*
	(0.068)	(0.075)	(0.095)	(0.099)	(0.068)	(0.075)	(0.095)	(0.099)
ln(price kWh)	−1.186***	−1.184***	−1.026***	−1.021***	−0.191***	−0.189***	−0.027	−0.021
	(0.021)	(0.023)	(0.023)	(0.024)	(0.021)	(0.023)	(0.023)	(0.024)
ln(income)^2	0.003	0.006	−0.018		0.003	0.006	−0.018	
	(0.007)	(0.008)	(0.010)		(0.007)	(0.008)	(0.010)	
Distance to T	0.021**	0.021**	0.008	0.009	0.010	0.009	−0.016*	−0.015
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Constant	−0.673**	−0.595*	0.724**	0.734**	4.208***	4.286***	5.643***	5.656***
	(0.215)	(0.234)	(0.269)	(0.280)	(0.215)	(0.234)	(0.269)	(0.280)
Observations	25,613	25,613	25,613	25,613	25,613	25,613	25,613	25,613
R-squared	0.203	0.203	0.175	0.175	0.080	0.080	0.025	0.025
Controls	NO	NO	NO	NO	NO	NO	NO	NO
R2 adj.	0.203	0.202	0.175	0.175	0.0794	0.0793	0.0243	0.0243
Number of id			12,868	12,868			12,868	12,868

Robust standard errors in parentheses
*** p < 0.001, ** p < 0.01, * p < 0.05

TABLE A3. SPECIFICATIONS WITH INCOME QUINTILE INTERACTIONS

	(1)	(2)	(3)	(4)
VARIABLES	lnc_ele	lnc_ele	lng_ele	lng_ele
Reform (*T*)	0.0664*	−0.1589***	0.0855**	−0.1029**
	(0.0274)	(0.0383)	(0.0260)	(0.0383)
(T = 0) x quintile 1	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
(T = 0) x quintile 2	0.0472**	0.0182	0.0477**	0.0186
	(0.0142)	(0.0160)	(0.0143)	(0.0160)
(T = 0) x quintile 3	0.1068***	0.0001	0.1072***	0.0001
	(0.0141)	(0.0177)	(0.0142)	(0.0177)
(T = 0) x quintile 4	0.1257***	−0.0088	0.1260***	−0.0081
	(0.0112)	(0.0196)	(0.0112)	(0.0196)
(T = 0) x quintile 5	0.1467***	−0.0098	0.1466***	−0.0092
	(0.0137)	(0.0227)	(0.0136)	(0.0227)
(T = 1) x quintile 1	−0.2009***	−0.0021	−0.2013***	−0.0027
	(0.0365)	(0.0320)	(0.0375)	(0.0320)
(T = 1) x quintile 2	−0.1196***	0.0072	−0.1208***	0.0065
	(0.0267)	(0.0290)	(0.0273)	(0.0290)
(T = 1) x quintile 3	−0.0390*	−0.0086	−0.0402*	−0.0089
	(0.0162)	(0.0277)	(0.0157)	(0.0277)
(T = 1) x quintile 4	−0.0303	0.0101	−0.0329	0.0095
	(0.0190)	(0.0276)	(0.0197)	(0.0275)
ln(price kWh)	−1.0935***	−1.0293***	−0.1027	−0.0300
	(0.0543)	(0.0250)	(0.0519)	(0.0250)
Constant	0.3203	1.1152***	5.2659***	6.0341***
	(0.3549)	(0.2081)	(0.3382)	(0.2080)
Year fixed effects	No	Yes	No	Yes
Household fixed effects	No	Yes	No	Yes
controls	Yes	Yes	Yes	Yes
Observations	14,963	20,959	14,963	20,959
R-squared	0.2848	0.1780	0.1768	0.0238
R2 adj.	0.283	0.177	0.174	0.0226
Number of id		10,745		10,745

Robust standard errors in parentheses
*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

TABLE A4. PLACEBO TEST BALANCE

	Reform Sample 2013			Placebo Sample 2012		
Variable	n	Mean	S.D.	n	Mean	S.D.
Electricity consumption	18770	3,272.9	2,005.1	18394	3,598.1	2,207.9
Electricity expenditure	18770	730.5	408.4	18394	761.7	430.6
Income	18624	23,358.0	15,063.5	18291	23,669.2	14,561.2
Total expenditure	18770	28,746.0	16,735.0	18394	29,690.7	17,189.9
Education level	18770	2.6	1.1	18394	2.6	1.1
Household economic situation	15270	1.8	0.9	15031	1.8	0.9
Household social situation	18763	3.2	1.7	18391	3.2	1.7
Number household members	18770	2.8	1.3	18394	2.8	1.3
Elderly	18770	0.3	0.5	18394	0.3	0.5
Retired pension	18705	0.4	0.5	18367	0.4	0.5
Number of rooms	18769	5.2	1.2	18384	5.2	1.2
House surface	18385	104.0	47.2	18032	104.1	47.2
Capital of province	18770	0.3	0.5	18394	0.3	0.5
Autonomous region	18770	9.0	5.0	18394	9.0	5.0
Town size	18770	2.7	1.6	18394	2.7	1.6
Population density	18770	1.8	0.9	18394	1.8	0.9
Tenure	18770	0.1	0.3	18394	0.1	0.3
Urban-rural	18770	0.8	0.4	18394	0.8	0.4
Old building (25+ years)	18760	0.6	0.5	18384	0.6	0.5
Interview date	18770	12.4	7.1	18394	12.4	7.1

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