Measuring the Impact of Time-of-Use Pricing on Electricity Consumption: Evidence from Spain

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Abstract

We evaluate the effect of a time-of-use pricing program introduced in Spain on residential electricity consumption. Using a Difference-in-Difference approach, we find that households responded by reducing consumption during peak hours. We then use machine learning for variable selection and show that it is able to capture pre-trends unrelated to the policy, improving the credibility of our estimates. We find that the program could have reduced consumption by up to 9% during peak periods, with significant spillovers to weekends. Using a more conservative estimator, we find that it reduced consumption by at least 1-2% during peak periods. We find evidence of habit formation during periods of uniform pricing, accompanied by an adaptation process that ends with a permanent change in consumption behavior. The results suggest that a predetermined pricing program can enhance consumer awareness and increase household price elasticity, thus making it an effective policy tool to reduce peak electricity demand and improve market efficiency.

Keywords: demand response, dynamic pricing, electricity.
JEL: H23, L94, Q41, Q48

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1 Introduction

The costs of generating electricity vary significantly between sources and technologies. Simultaneously, electricity demand fluctuates considerably over time, yielding periods of peak demand. In those periods, the optimal mix includes generating capacity with high marginal costs to balance supply and demand. If prices faced by consumers do not reflect these cost variations, consumption will be distorted and generate market inefficiencies (Joskow and Wolfram, 2012). Thus, several governments have tried pushing regulatory reforms from time-invariant to dynamic or marginal cost pricing. These reforms are more and more urgent as the share of intermittent renewable generation increases.[1]

The efficiency gains from dynamic pricing will depend on the price mechanism and the presence of potential market failures and frictions. For instance, a day-ahead real-time pricing (RTP) scheme with constant hourly price changes may fail in the presence of imperfect information or adjustment costs. In that case, introducing automated load-shifting technologies might be necessary to induce a significant demand response (Bollinger and Hartmann, 2020; Fabra et al., 2021). Alternatively, policies with predetermined retail pricing could ease information constraints and short-term costs of shifting consumption. Time-of-use (TOU) pricing with predetermined tariffs varying between different periods within a day is an example of such a mechanism.

We study the introduction of a TOU pricing program in Spain on residential electricity consumption. In June 2021, the Spanish government introduced a new regulation where system and network charges (which traditionally amount to 50% of the total electricity bill) would be charged at three different marginal prices depending on the hour of the day and the day of the week. According to the new regulation, peak hours range from 10 am to 2 pm and from 6 pm to 10 pm, and off-peak hours cover hours from 12 am to 8 am during working days. The remaining hours in working days are considered mid-peak, while all hours on weekends and national holidays are off-peak.

To estimate the causal impact of the policy, we use demand data at the distribution area level for Spain and compare it with its nearby country, Portugal. Portugal is a natural control group for several reasons. First, geographically both countries have similar weather conditions, a determinant factor of electricity demand. Perhaps more importantly, Spain and Portugal trade in the same electricity wholesale market. This attenuates the possibility that our results are driven by differences in supply curves, consequently affecting equilibrium prices and quantities.

In the estimation, we focus on the responses of Spanish residential consumers who were under the regulated tariff during the period spanning from 2018 to September 2021.[2] The regulated tariff is offered by five main distribution groups, each of them being the only retailer entitled to offer the regulated tariff in a given distribution area. After the policy implementation, regulated prices increased by more than 60% on average, partly driven by rising prices in the wholesale market and partly by the policy change. Importantly, increases in energy costs impacted prices at all hours of the day, while the TOU policy has large price jumps within the day. This allows us to identify the effect of changes in TOU prices. Indeed, the reform provided large incentives

[1] According to Wolak (2019), setting day-ahead and real-time prices that reflect transmission network constraints will reduce the costs of integrating intermittent renewable generation capacity.

[2] We focus on this subset of consumers due to data availability and the fact that consumers on commercial tariffs do not necessarily have a time-of-use pricing scheme mimicking the regulated tariff, although many do. Households on the regulated tariff represent around 39% of residential consumers in our sample period.

to consume during cheap hours of the day: after the policy, prices at off-peak hours were 86% lower, while consuming at peak hours was 200% more expensive.

We estimate two empirical models. The first is a Differences-in-Differences (DID) fixed effects model, comparing hourly differences in electricity consumption before and after the policy in Spain's distribution areas and Portugal, controlling for a rich set of fixed effects. However, this richness of the data makes it difficult to choose the correct specification among many possible candidate controls. We thus turn to machine learning techniques (ML), particularly LASSO and forest estimators, for variable selection. In a nutshell, our empirical strategy consists of two steps. First, we use pre-treatment data to create a distribution area-specific electricity consumption model. We then use these estimates to create out-of-sample predictions for the post-treatment period following Burlig et al. (2020). While the difference between the actual outcome and the prediction in treated units already gives us an idea of the treatment effect, it does not control for time trends that would otherwise be accounted for in a DID setting. In a second step, we regress these prediction errors on a treatment dummy and the same set of fixed effects used in the DID strategy previously presented. The identifying assumption requires treated and control units to trend similarly in prediction errors, modifying the standard parallel trend assumption in DID settings.

Our results indicate an overall reduction in electricity consumption. In our preferred specification of the standard panel fixed effect regression, results are insignificant for off-peak hours, while we observe a 5.7% and 8.9% decrease during mid-peak and peak hours, respectively. We find evidence that using machine learning techniques helps to reduce the sensitivity of the estimates across different fixed-effect specifications. The algorithm helps capture pre-trends in consumption unrelated to the policy. In this model, consumption would have decreased by 6.4%, and 9.5% during mid- and peak hours, respectively.

We then split off-peak hours between weekdays and weekends. The goal of this analysis is threefold. First, it allows us to identify the within-day shift in consumption. Second, we define ”fake” mid- and peak hours during the weekend, thus identifying possible effects of the policy that could be related to habit formation. Third, methodologically, if unobserved confounders threatening the parallel trend assumption are similar between weekdays and weekends (for instance, due to aggregate shocks affecting one of the two countries), we may be able to reduce the bias of our estimates by computing the additional effect of the policy during weekdays when compared to weekends.

Notably, we observe a significant demand response during the weekends for all three periods. Indeed, the magnitude of the coefficients for off-peak and mid-peak hours during weekends is comparable to weekdays. In particular, looking at our ML model, weekend consumption decreased by 7.3% and 8.3% for mid- and peak hours and by 6.4% and 9.5% during weekdays. Taking this comparison, we define a lower bound of policy effects, this is, estimating the difference between weekdays and weekends. In that case, we still find a decrease in consumption during peak hours of 1.2%. However, this approach may be quite conservative, as the policy could have induced consumption pattern changes during weekends due to habit formation or some degree of misinformation on pricing during the weekend.

We complement our main analysis with two additional extensions. Using price data and the exogeneity of the policy, we can compute household price elasticities. We find significant price elasticities ranging between -0.08 and -0.135. These estimates are above what is usually found in the literature on electricity demand estimation, where consumers are usually found to be relatively insensitive to price variations. With that, we compute back-of-the-envelope demand responses given the price changes, and we find that our results match the estimated policy effects.

Our results align with the literature studying behavioral factors in explaining consumer responsiveness, suggesting that a predetermined pricing program, by enhancing consumer awareness, makes these prices more salient thus triggering stronger household responses. To reinforce this hypothesis, we present an exploratory analysis of the relationship between Google searches and the effect of the policy. Searching behavior is consistent with an adaption process, with households reducing their overall consumption during the first weeks, followed by a subsequent stabilization of policy effects and Google searches. The effect is particularly well timed around the time in which the policy is publicized, providing more credibility to the causal impact on consumption by the policy.

The paper is organized as follows: Section 2 reviews the literature on dynamic pricing in the electricity market. Section 3 describes the empirical setting and data. Section 4.1 presents the empirical strategy to identify the causal effect of the policy and discusses the results. Section 4.2 repeats the same analysis using the machine learning methodology and compares results with the standard panel fixed effect model. Section 5 provides a framework to compute price elasticities and compare them to the identified policy effects, as well as it relates these estimates with consumer searching behavior. Finally, Section 6 concludes.

2 Literature review

This paper contributes to the literature on time-varying pricing in the electricity market.[3] The rationalization of making electricity consumption more closely tied to the variations of the marginal cost of generation appeared already in the late 1950s (Steiner, 1957; Boiteux, 1960; Williamson, 1966). In the last two decades, there has been a push to reconsider dynamic pricing given the recent developments in the electricity market, such as the deployment of smart meters and the increasing share of intermittent renewable in the generation mix (Joskow and Wolfram, 2012).

Potential efficiency gains from dynamic pricing are studied, among others, by Borenstein (2005) and Borenstein and Holland (2005) in the context of real-time pricing (RTP). They show that, under ideal market conditions, adopting RTP would reduce peak electricity production capacity and would lead to significant welfare gains in the long run (up to 11% for the California electricity market). Holland and Mansur (2006) show that benefits are relatively modest in the short run, suggesting that a large portion of welfare benefits originates from a reduction in the construction of new generation capacity. Moreover, RTP proponents argue that real-time pricing could alleviate market power since a more elastic demand would reduce firms' incentives to curb their output to increase prices (Borenstein, 2002). [4]

In addition, Holland and Mansur (2008) show that dynamic pricing does not always bring down emissions, at least in the short run. By using exogenous changes in temperature and economic activity, they link variations

[3]Dynamic pricing methods are consistently used in other sectors where the capacity is limited in the short-term, such as airlines, hotels, and car rental firms (Elmaghraby and Keskinocak, 2003; McAfee and Te Velde, 2006; Gibbs et al., 2017).

[4]Poletti and Wright (2020) finds that efficiency gains from RTP are 41% larger in the presence of market power in the New Zealand market.

in load with variations in emissions, arguing that the conclusions could be helpful to link real-time pricing (affecting load variance) with environmental benefits. Unfortunately, the effect of a reduction in load variance is pollutant and location dependent. In particular, the shift of consumption towards off-peak hours drives SO_2, NO_X, and CO_2 emissions down in regions where peak demand is supplied with oil-fired power stations. Still, the effect becomes the opposite when hydroelectric power is more generally used to cover peak demand.

The benefits of dynamic pricing materialize only if households react to prices by adjusting their demand. Nevertheless, research has found that consumers generally exhibit inelastic demand. Allcott (2011), using data from an RTP pilot program, finds that enrolled households have an average elasticity of -0.1, with no load shifting from peak to low price hours. Similar results are obtained by Fabra et al. (2021) studying the first large-scale deployment of RTP in Spain in 2015. Both papers point out that low price variation is one of the possible reasons that explain the lack of demand response. They also highlight the importance of information and adaptation costs.

Jessoe and Rapson (2014) and Bollinger and Hartmann (2020) analyze more explicitly the impact of these costs and the role of assisting technology. Jessoe and Rapson (2014) analyze the effect of providing in-home devices that display households' electricity usage in real-time. They show that households provided with information were three standard deviations more responsive to prices than those without. Bollinger and Hartmann (2020) show that consumers incur adjustment costs and thus cannot shift consumption in response to short-term price changes unless they use automation technologies - such as programmable communicating thermostats.

Even if TOU prices adjust more accurately to high demand than flat rates, they fail to acknowledge all the differences in marginal costs within a day. As a result, efficiency gains were first estimated to be just 20% relative to RTP, according to Borenstein (2005). Woo et al. (2013) study the effect of TOU pricing and conclude that households respond by reducing electricity usage during peak hours (while off-peak either remains the same or increases only slightly) and lower overall electricity consumption. However, new estimates on the efficiency of TOU pricing relative to spot pricing suggest that well-designed pricing schemes perform relatively well in indicating relative price differences within days and provide relatively effective load-shifting incentives (Schittekatte et al., 2022). Importantly, Prest (2020) suggests that the success of TOU prices derives from getting consumers to pay attention, while getting the price right would be a second-order problem.

Faruqui et al. (2020) report that while nearly four hundred TOU rates have been tested in pilots around the globe, full-scale deployment of TOU rates is quite limited, with only 4% of residential consumers being on TOU rates. Moreover, the evidence drawn from these field experiments faces some important limitations. Sample sizes are usually small, and the design of the programs, in which signing up for dynamic rates is optional, increases the potential for selection bias. To overcome these concerns, Fowlie et al. (2021) partnered with a utility in California and implemented a large-scale randomized control trial to study the effects of opt-in vs. opt-out of dynamic pricing schemes. They find that households in the opt-in are more responsive than households in the opt-out group, although the aggregate effect can be substantially larger since participation is higher. Our case is a unique opportunity in that all households belonging to the regulated segment are put into the TOU tariff by default.

A potential concern of time-varying tariffs is their possible distributional impacts. With fixed tariff rates, households with a flatter consumption profile would cross-subsidize those with higher consumption at peak hours. The effect of transitioning to dynamic prices on low-income households is not clear ex-ante. Borenstein (2013) studies the effects of switching to critical peak pricing (CPP) and finds that, even assuming no demand response, low-income households face no significant changes in their electricity bills.[5] By combining substation data with demographics characteristics for a utility in Victoria (Australia), Leslie et al. (2021) find that areas with low housing prices, a high share of renters and elderly people are better off with RTP. Nevertheless, Cahana et al. (2022) find that while low-income households react to hourly variation in prices within a day, regressive impacts can arise from monthly variation depending on HVAC mode by income levels. In the end, efficient tariffs compatible with distributionally equitable rates are a matter of design. Burger et al. (2019) propose a two-part tariff with a fixed charge based on income (or other correlated measures) that would mitigate the adverse effects of dynamic pricing while maintaining most of the efficiency gains.

Our methodological approach is based on machine learning techniques. By permitting much more flexible nonlinear, high-dimensional models, these techniques have emerged as a powerful tool to build post-treatment counterfactuals, a key element in causal inference (Varian, 2016). In fact, Prest et al. (2023) find that prediction algorithms such as random forest or LASSO regressions are able to replicate experimental treatment effects, even in the absence of control groups. In this paper, we closely follow the method used in Burlig et al. (2020) in which first, they use pre-treatment data and LASSO regressions to estimate electricity consumption, and second, generate in- and out-of-sample predictions to construct prediction errors that are used to identify the policy treatment effects. Similar approaches are used in the context of energy efficiency in Christensen et al. (2023). Finally, in using machine learning to flexibly control for selection when we have a single control unit, this paper partly resembles Arkhangelsky et al. (2021), where the authors combine data-driven synthetic control and standard differences-in-differences techniques to compensate for the lack of parallel trends. While the re-weighting of units to match their pre-trends is different, we observe parallelisms between the conclusions.[6]

3 Context and Data

There are five main distribution groups in the Spanish electricity market.[7] These five groups compete in each other's territories as retailers alongside new entrants. In each distribution area, only the vertically integrated retailer can offer the regulated tariff. This tariff is prescribed by law based on market conditions, and it is unique throughout the country. In contrast, non-regulated or commercial tariffs can be offered by any retailer across distribution areas, and the contract terms are freely set.

Despite improvements in competition since deregulation, the Spanish retail market is still highly concentrated. There are five main retailing firms, which belong to the same business groups as the five largest distribution companies. In 2019, 39% of consumers were still on the default regulated tariff, and an additional 42% were served by the commercial brand of the distribution company. Thus, over 80% of residential households are still served by mainly the five business groups. In addition, Enrich et al. (2022) find a significant incumbent

[5] Critical Peak Pricing, or CPP, combines flat rates with significant price increases, typically in a limited number of hours annually in which the electricity grid is under high stress, such as extremely hot summer days.

[6] For instance, they find that when there is little systematic heterogeneity between units, the estimator might be less precise relative to the DID estimator.

[7] The largest distribution groups are Endesa, Iberdrola, Naturgy, EDP, and Repsol. There are also a few small distribution companies that serve much smaller areas.

advantage, with the probability of choosing the commercial brand of the distribution group higher on its own market, as well as strong consumer inertia.[8]

In recent years, the roll-out of smart meters has enabled the introduction of new and more flexible tariffs that can adjust better to the consumption profile of each household. The Spanish electricity bill has four components: energy costs, network charges, system charges, and taxes.[9] On average, system charges and energy costs represent the largest part of the bill (30% each), followed by network charges and taxes (each around 20%). In October 2015, households under the regulated tariff were put into an RTP scheme for the energy charge of their electricity bill. Despite this reform, Fabra et al. (2021) find that households did not respond to RTP, which may be attributed to the lack of consumer awareness, costly information acquisition, and small gains of demand response due to low price variation.

A significant change in the design of the tariffs was introduced on June 1, 2021. The policy change introduced mandatory TOU pricing in the regulated tariff, with system and network charges having three different tiers, depending on the hour of the day and the day of the week. According to the new regulation, peak hours ranged from 10 am to 2 pm and from 6 pm to 10 pm, and off-peak hours covered hours from 12 am to 8 am during working days. The remaining hours in the working days were considered mid-peak, while weekends and national holidays were fully off-peak.

In Figure 1, we represent on the left the different pricing schemes that consumers in the regulated tariff could choose from before the reform. Consumers were put into a flat rate and could opt-in to time-of-use charges in their electricity bill, facing 2 or 3 prices within a day. Before the policy, the share of consumers in the regulated tariff under TOU remained below 10%.[10] On the right-hand side, we show the new pricing scheme depending on the time of the day. Although the policy was compulsory for all residential contracts, retailers in the liberalized market could offer flat rates that compensate for the variation of the TOU tariff. Thus, we restrict our analysis to the regulated market.

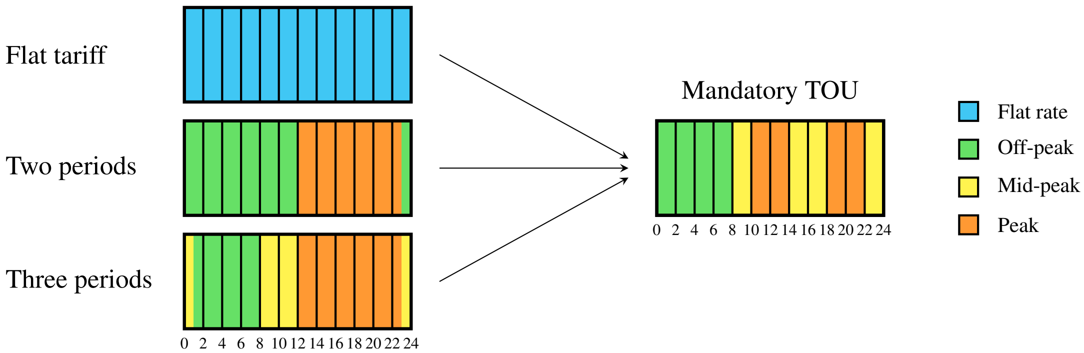
Table 1 and Figure 2a show how prices in the regulated segment changed after the policy for each TOU period. We take energy costs and system and network charges, which constitute the largest part of the electricity bill. In Table 1, we compare the months affected by the policy (June to September 2021) to the same period between 2018 and 2020. Figure 2a shows the evolution of these components throughout the whole period, including regulated prices for Portugal. Total regulated prices increased by more than 60%, partly driven by rising prices in the wholesale market translated into higher energy costs. Importantly, increases in energy costs are independent of the hour of the day. If energy costs would have affected total price differently between TOU periods, our policy estimates of within-day shifts would have captured price changes external to the policy. Next, the reform increased average charges by nearly 50%. Still, it provided incentives for load shifting: prices during off-peak hours were 86% lower while consuming at peak hours was 200% more expensive after the policy. In contrast, electricity prices in Portugal were decided for the year ahead. However, given the

[8] Byrne et al. (2022) provide experimental evidence of variations in consumer willingness and ability to search and bargain, contributing to the different consumer bases and price distribution between incumbents and entrants.

[9] System charges include subsidies to renewable energies, deficit payments, and compensation of the extra cost of generation in non-peninsular regions.

[10] In Portugal, nearly 90% of consumers in the regulated tariff pay flat rates, a share that has remained stable over our period of study, according to the Portugal Regulation Authority. Similar to Spain, consumers can choose between a flat rate, a two-period, and a three-period.

Figure 1: Tariffs Before and After the Policy



Notes: Available tariffs for the charges component of the electricity bill before and after the policy was implemented. Before June 2021, consumers were assigned a flat rate tariff by default, but could opt-in to different TOU programs, while the policy made compulsory a three-tier TOU pricing.

Table 1: Percentage Change of the Energy Components Before and After the Policy

	Average	Off-Peak	Mid-Peak	Peak
Total Price	61.4%	12.2%	39.6%	117.5%
Charges	49.1%	-86.3%	-5.1%	199.4%
Energy Costs	68.6%	71.2%	65.4%	70.2%

Notes: Average prices are weighted by consumption in the regulated segment. We restrict to the months of the policy: June to mid-September since system charges were reduced on 15 September 2021 to cope with rising energy prices. Taxes are not included.

recent increases in wholesale prices, regulated tariffs kept increasing during 2020 and 2021. Nonetheless, these prices were only updated monthly. In Section 5.1, we use the variation induced by the policy to estimate price elasticities.

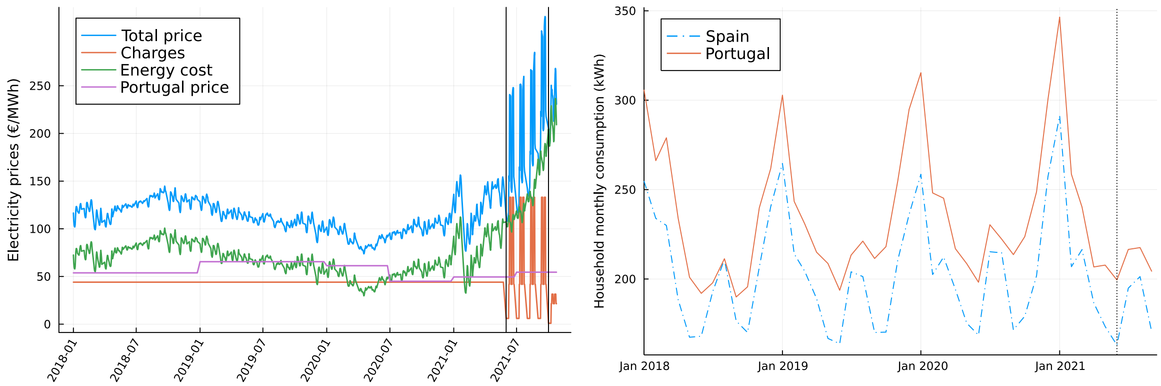
To estimate the causal effect of the policy, we compare the evolution of electricity consumption in Spain and its nearby country, Portugal, between January 2018 and September 14, 2021. In particular, we end our sample on 14 September since the Spanish authorities reduced system charges by 96% as a response to the rising energy prices. As a result, TOU tariffs were essentially canceled. Restricting the sample to September 2021 also avoids the most heated episodes of the energy crisis, which given how Spanish regulated prices are set, introduces substantial volatility.[11]

Consumption data for Spain come from the archives of the Spanish System Operator, *Red Eléctrica de España* (REE). These data measure the hourly demand of all the Programming Units that provide electricity into the national grid and are differentiated by regulated and non-regulated demand. The former only includes demand from residential households, whereas the latter includes domestic consumers, small- and medium-sized enterprises, and industrial consumers. This provides an additional reason for limiting our sample to the regulated segment, as we are only interested in households' demand response to the policy.

For the case of Portugal, electricity consumption data come from the Iberian electricity market operator, *Operador del Mercado Ibérico de Energía* (OMIE). We also restrict the sample to domestic consumers under

[11] As explained above, the energy component of the regulated price reflects day-ahead wholesale market prices.

Figure 2: Electricity prices and demand



(a) Decomposition of retail electricity prices

(b) Electricity Demand per Capita

Notes: Figure 2a plots the evolution of each price component of the Spanish regulated electricity price and the regulated prices for Portugal. Each observation corresponds to the monthly average price of a given hour weighted by consumption. The two vertical lines indicate the period with the new TOU schedule. Figure 2b plots the average household monthly consumption.

the regulated tariff. Unfortunately, there is only one leading distribution company in Portugal, and therefore the data are reported at the aggregate national level.[12]

Since aggregate consumption under the regulated tariff in Spain is roughly ten times larger than in Portugal, we take per capita consumption as our variable of interest. Figure 2b shows the evolution of demand per capita for Spain and Portugal. We observe that electricity consumption follows a clear seasonal pattern over the months of the year. Even if Portuguese households in our sample present a relatively greater consumption, which widens in the winter, an important surge in Spanish consumption occurs yearly in the months where the policy is analyzed, mainly in the summer. To control for the impact of weather, we obtained hourly temperature data from the Modern-Era Retrospective analysis for Research and Applications (MERRA-2) released by the NASA.[13] To match the temperature data at the 50x50km grid point-level with our consumption data, we weigh the temperature data by population in each distributing area.

Due to the aggregate nature of the demand data, we need information on the number of consumers per utility. The information on the number of consumers in Spain has been kindly provided by the Spanish National Markets and Competition Commission (CNMC, according to the Spanish acronym) based on the Reports on Oversight of the Retail Electricity Market.[14] The number of consumers is presented quarterly for all retailers across all distributing areas in Spain. We interpolate the data to monthly frequency using a monotonic algorithm proposed by Fritsch and Butland (1984). We obtain Portugal's consumer data from the Bulletins of the Liberalized Electricity Market, which provide the number of consumers under the regulated and the liberalized segment by month.[15]

As in many liberalized markets, the number of consumers under the regulated tariff in Spain and Portugal has decreased in recent years. In particular, Portugal has set the end date for the regulated tariff to 2025. As we

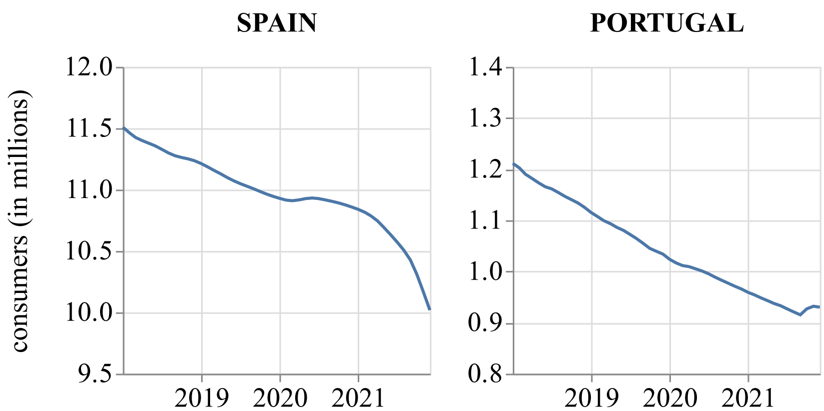
[12]In continental Portugal, 99% of low-voltage consumers have their energy distributed by EDP Distribuçao (see here). This large distribution area is comparable in terms of size and consumers to the largest distribution companies in Spain: Endesa and Iberdrola.

[13]See GES DISC.

[14]See IS Mercado Minorista de Gas y Electricidad.

[15]See Boletim do Mercado Liberalizado de Eletricidade.

Figure 3: Number of consumers



Notes: Evolution of consumers in the regulated market for Spain and Portugal. We interpolate quarterly data to monthly frequency.

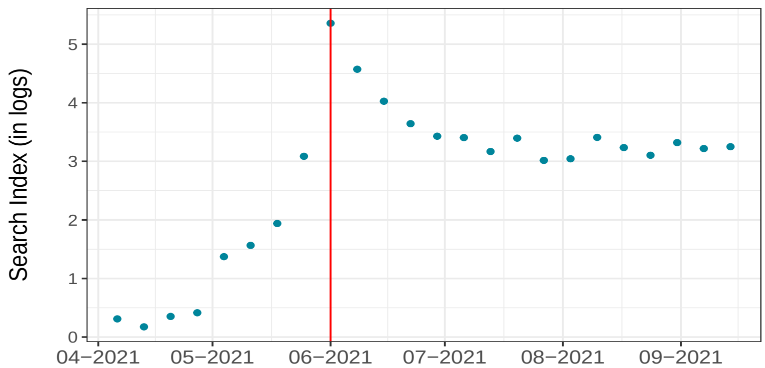
observe in Figure 3, in January 2018, the regulated tariff included over 11M consumers in Spain and over 1M in Portugal, representing respectively 41% and 20% of residential consumers. In the last year of our sample, the share of consumers in the regulated segment reached a record low at 34% in Spain and 15% in Portugal. However, during the months of the policy, the drop was steeper for Spain, suggesting that part of the consumers might have switched to the liberalized market in search of flat rates. By design, Spanish consumers with a regulated tariff are more affected by changes in wholesale prices, which may be a factor explaining the decline of consumers in the context of high price increases. In contrast, the regulated tariff in Portugal is determined ex-ante, and its prices stayed relatively lower than the commercial prices, thus mitigating the impact of price increases on end-users. This may explain the consumer surge under the regulated tariff in October 2021, which is another reason to limit the analysis to September 2021.[16]

If those consumers switching to a liberalized contract had different consumption patterns, our policy coefficients would be capturing these composition effects. On the one hand, opt-out households that are not willing to change their consumption patterns could be switching away. If these less price elastic consumers opted out of the regulated tariff, our estimates would be an upper bound of the average treatment effect. On the other hand, if switching patterns are correlated with the degree of awareness, our treated sample would over-represent unaware households, possibly leading to a lower bound of treatment effects. That said, it is important to keep in mind the decrease in consumers started months before the implementation of the policy, coinciding with increases in real-time prices. In fact, from January to September 2021, the regulated segment lost 4% of consumers, with half of those switching after June. This suggests that our estimates may not be primarily driven by these composition effects.

In addition, we use Google Trends data to capture consumer awareness of the new tariffs. This public tool uses a largely unfiltered sample of actual search requests occurring in a given location during a certain period of time.[17] To account for all searches related to the policy, we use different wordings and synonyms that are closely related to the topic in Spanish. A limitation of the data is the scale on which search interest

[16] Find more information on the Portuguese tariff on the regulator website.
[17] To read more, visit Google Trends' Help Page.

Figure 4: Google trends' weekly search index



Notes: This figure shows the logarithm of the search index based on keywords related to the policy in Spain. The search index is constructed on a scale from 0 to 100, where 100 represents the maximum number of topic searches for the period of interest.

is defined because it is not only contingent on the generated sample but also the combination of regions and periods selected by the researcher. In particular, search interest is given on a scale from 0 to 100, where 100 represents the maximum number of topic searches for given regions and period selection. For our analysis, we have collected data spanning from January 2018 to October 2021. Figure 4 shows the logarithm of the weekly search index of keywords related to the policy. While we observe a modest increase in searching during the weeks previous to the introduction of the policy (marked with a red line), most searches are concentrated during the first week after the implementation. This increase in consumer awareness is in line with the results derived from a Household Panel survey conducted by the Spanish regulatory body (CMNC),[18] where the share of consumers declaring not knowing its own electricity contract decreased from 35% in the first semester of 2021 to 17% in the second semester, after several years remaining relatively constant.

In sum, to analyze the effect of the policy, we use demand data for the domestic regulated segment at the distribution area level for Spain and the national level for Portugal. We drop the observations for 2020 due to the disruption of COVID-19 to electricity demand. In total, we end up with a panel of 142,000 hourly observations from the five Spanish distribution areas and Portugal, spanning from January 2018 to December 2019 and January 2021 to September 14, 2021. Summary statistics of the main variables are shown in Appendix Table 7.

4 Empirical strategy and Results

In this section, we describe the empirical strategy used to recover the effect of the policy on electricity consumption. Throughout the analysis, we will be comparing two different approaches: fixed effects vs machine learning. While machine learning techniques are not built to produce good parameter estimates, they can discover complex structures of the data that were not specified in advance (Mullainathan and Spiess, 2017), for example, by enabling us to select among a large set of covariates. In particular, we will closely follow Burlig et al. (2020), who use machine learning algorithms to estimate robust causal effects.

[18] See Estadísticas panel de hogares.

The main difference between the two approaches is that using machine learning tools and pre-treatment data, we create distribution area-specific models to predict electricity consumption in the post-treatment period. In the second step, we use these out-of-sample predictions to generate prediction errors that we incorporate as a dependent variable in the panel fixed effect model. On the other hand, the panel fixed effects model directly includes the observed electricity consumption as the dependent variable.

4.1 Panel fixed effects: Differences-in-Differences

To identify the potential demand response to the policy, we start by estimating the following Differences-in-Differences (DiD) regression:

$$y_{ith} = \beta_k D_{itk} + \delta_k P_{itk} + \gamma X_{ith} + \alpha_{ith} + \epsilon_{ith}, \tag{1}$$

where y_{ith} is the log of the demand per capita in the distribution area i at day t and hour h. Note that we treat Portugal as a control distribution area. D_{itk} is a dummy variable that equals one for all Spanish distribution areas after the policy was implemented for each TOU tariff $k = \{\text{Off-Peak, Mid-Peak, Peak}\}$. Therefore, β_k captures the effect of the policy at different tariffs. P_{itk} stands for Placebo, and it takes one for all Spanish distribution areas one month before introducing the policy, thus capturing possible pre-trends unrelated to the policy or anticipatory effects that could cast doubts on the validity of the parallel trend assumption. Ultimately, even if the policy was officially announced in early 2021, the media coverage broadly started a week in advance, as evidenced by the Google Trends searches. Control variables X_{ith} include hourly temperature and an interaction for whether the temperature is above or below 20ºC.[19] Finally, α_{ith} includes different combinations of fixed effects. We weigh observations by the number of consumers to make the sample representative and thus identify an average effect.

Table 2 reports the results of equation (1) under different fixed effects specifications. Specification (1) includes month-of-sample and area fixed effects, both interacted with weekend-hour fixed effects. Specification (2) allows for area hourly fixed effects to be different depending on each month of the year. That is, For each hour and type of day (weekdays and weekends), we control for differential patterns across distribution areas, months of the year, and common shocks or time trends at the monthly level. Specification (3) includes temperature controls. Finally, in Specification (4) we test the sensitivity of our estimates to a comparison focused on within-month-and-area variation across hours, with the inclusion of weekend-area-month of sample effects. In this latter case, the off-peak effect is not identified. We cluster standard errors at the area-month of sample level.[20]

First, we observe that the sign of the coefficients for off-peak hours change across specifications, but none

[19] Temperature can have heterogeneous effects on electricity consumption. While an increase in temperature for mild and cold weather might result in a decrease of electricity consumption (e.g. by reducing electric heating), an increase in temperature on hot days can induce higher usage of AC. Indeed, in our ML exercise, we show that daily maximum and minimum temperatures are better predictors than average hourly temperature. Thus, if controls are not correctly specified, one might suffer from linear misspecification bias (see Goff (2014) for a formal derivation).

[20] A good rule of thumb is to cluster at the level of treatment assignment. See Abadie et al. (2022) and Roth et al. (2022) for a discussion on the recent developments in the DiD literature. Here, the TOU intervention is defined at the area-month of sample level. That said, hours within a month potentially receive different levels of treatment. Because consumption within a day and week is very interrelated, we keep all hours of a month within an area in the same cluster.

Table 2: Panel fixed effects: Differences-in-Differences

	(1)	(2)	(3)	(4)
Policy				
Off-peak	0.028	-0.022	-0.023	
	(0.032)	(0.016)	(0.015)	
Mid-peak	-0.007	-0.057***	-0.058***	-0.068***
	(0.030)	(0.015)	(0.015)	(0.006)
Peak	-0.021	-0.089***	-0.092***	-0.100***
	(0.028)	(0.014)	(0.014)	(0.009)
Placebo				
Off-peak	-0.041	0.012	0.022	
	(0.021)	(0.016)	(0.017)	
Mid-peak	-0.060*	0.008	0.011	-0.013
	(0.024)	(0.027)	(0.028)	(0.014)
Peak	-0.020	0.015	0.019	-0.006
	(0.024)	(0.024)	(0.029)	(0.012)
R-sqr	0.001	0.003	0.004	0.002
Observations	142,000	142,000	142,000	142,000
Hour X Weekend X Month-of-Sample	Yes	Yes	Yes	Yes
Hour X Weekend X Area	Yes	Yes	Yes	Yes
Hour X Weekend X Area X Month		Yes	Yes	Yes
Hour X Weekend X Temp. controls			Yes	
Weekend X Area X Month-of-sample				Yes

Notes: This table presents estimates of Equation (1). The dependent variable is log consumption per capita. Observations are weighted by the number of consumers in each distribution area. Standard errors clustered at the area-month of sample level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

of them is significant. In contrast, some of the results for mid- and peak hours are significant and negative. In column (2), we observe that households reduce peak consumption by 5.7% and 8.9%. These results suggest that households reduce overall consumption in response to TOU prices, without any sign of load-shifting to off-peak hours. Placebo coefficients for mid- and peak hours do not exhibit any particular pre-trend in consumption patterns before the introduction of the policy. Overall, controlling for differences in calendar months across distribution areas seems to be the most important factor in correcting our estimates, including differences in temperature.

One limitation of our analysis is that our policy estimates also capture increases in the energy cost component of the price during the months the policy was in place. However, given that those increases were constant between hours, we can still use the difference in policy effects to identify changes in TOU rates even after controlling for the month of sample at the area level, as shown in column (4). We find that the change in consumption between TOU periods using a within-month comparison is very similar and around 6.8% and 10.0% for mid- and peak hours, respectively. While energy costs started rising months before the implementation of the policy, our placebo coefficients do not suggest any particular pre-trend in electricity consumption. Therefore, the results align with a response to the policy implementation. Indeed, in the extensions below using Google Trends we show that the response to TOU prices appears to be sharp around the time of the implementation and media coverage.

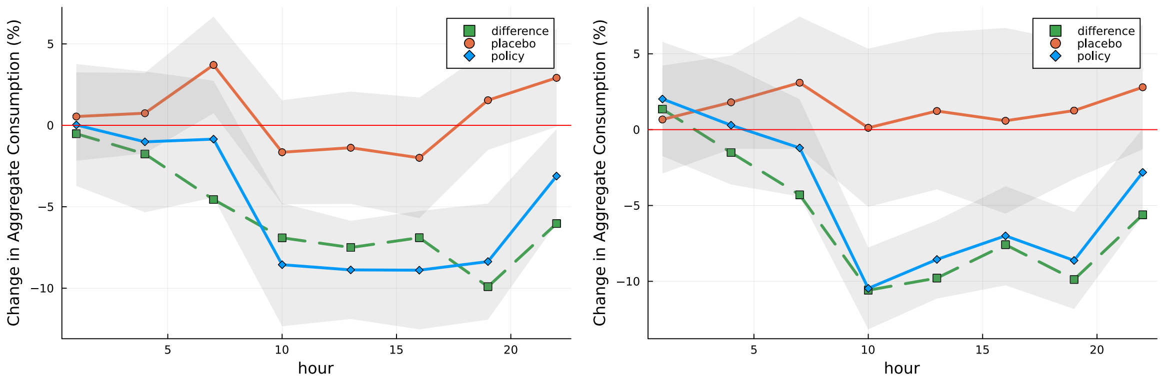
Week vs. weekend effects When comparing these different pricing regimes, and given that we control for weekdays vs. weekends, our estimate reflects variation in prices during weekdays, which are the days affected by the policy. However, there is a question on whether consumption by households was affected during weekends, even if hourly prices remained roughly the same. We next split the effect by differentiating between weekdays and weekends by estimating equation (2).

The goal of this analysis is threefold. First, as already mentioned, we want to identify changing consumption patterns within a day. Second, we define mid- and peak hours during the weekend, thus identifying possible effects of the policy that could be related to habit formation. Third, methodologically, if placebo tests exhibit the same patterns, we may be able to reduce the bias of our estimates due to a violation of the parallel trend assumption by computing the additional difference of the policy on weekdays with respect to weekends. For the sake of exposition, we define this model as the triple-differences (TD) model. In most cases, we estimate the effect for both groups (weekdays and weekends), instead of the traditional approach of identifying treatment effects as the difference in treatment between groups. Conceptually, this relates to whether we understand the treatment effects on weekends as true policy effects or as a control for unobserved confounders. The estimation equation becomes

$$y_{ith} = \sum_{w\in[0,1]} \beta_{kw} D_{itk} 1[\text{weekday} = w] + \sum_{w\in[0,1]} \delta_{kw} P_{itk} 1[\text{weekday} = w] + \gamma \text{temp}_{ith} + \alpha_{ithw} + \epsilon_{ith}. \quad (2)$$

Table 3 reports the results of the triple-differences model. Even though under the new TOU tariffs, weekend consumption is subject to off-peak prices, we observe a significant demand response for mid- and peak hours. These findings align with Fowlie et al. (2021), where consumers under CPP reduce their consumption on event

Figure 5: Panel fixed effects: Policy effects by hour of the day and type of day



(a) Weekends and Holidays

(b) Weekdays

Notes: These figures present estimates of Equation (2) - Specification (2) in blocks of 3 hours. The dependent variable is log consumption per capita. Observations are weighted by the number of consumers in each distribution area. Controls include area-month-hour-weekend and month-of-sample-hour-weekend fixed effects. Standard errors clustered at the area-month of sample level and confidence intervals reflect a 95% significance level.

and non-event days, pointing to habit formation. Indeed, the null effect on off-peak hours found in Table 2 was an average effect of first, increases during off-peak hours in weekdays (although not significant), and second, significant consumption cuts for mid- and peak hours during weekends. In fact, looking at column (2), during mid-peak hours, the reduction in consumption is higher during weekends. Finally, placebo coefficients, especially for off-peak hours, appear to be sensitive to the inclusion of fixed effects or controls. Looking at column (3), the inclusion of temperature controls might worsen the ability of the model to capture pre-trends unrelated to the policy. Recall that we weighted the temperature by population in each distribution area, although these areas are not perfectly defined. Finally, note that for each column, results for mid-peak and peak hours during weekdays are similar to the ones found in the DiD model (as they should be, given that the policy dummies are defined in the same way).

To explore the responses in a more flexible way during the day, we also estimate a treatment effect for blocks of three hours during the day. Figure 5 plots placebo and policy estimates of equation (2) - Specification (2) in blocks of 3 hours, along with their difference for each block. During weekdays, we observe two u-shaped curves corresponding to the hours with peak pricing. This is consistent to a reaction to mid and peak prices during TOU hours. There is also a decrease in consumption during weekends, although it is more diluted across hours and appears to be somewhat more confounded in the placebo effects. Overall, these patterns suggest some differential response between weekdays and weekends, although weekend consumption also appears to react to the policy.

4.2 Machine Learning

In this section, we repeat the same analysis but with a machine-learning estimator. This approach allows the researcher to include all possible interactions between controls, making the model selection process transparent and data-driven. To estimate the effect of the policy, we will proceed in two steps. First, we create a distribution

Table 3: Panel fixed effects: triple-differences

	(1)	(2)	(3)	(4)
Policy Weekday				
Off-peak	0.053	0.010	0.006	
	(0.037)	(0.018)	(0.017)	
Mid-Peak	-0.007	-0.057***	-0.058***	-0.068***
	(0.030)	(0.015)	(0.015)	(0.006)
Peak	-0.021	-0.089***	-0.092***	-0.100***
	(0.028)	(0.014)	(0.014)	(0.009)
Policy Weekend				
Off-peak	0.062	-0.002	0.001	
	(0.040)	(0.019)	(0.018)	
Mid-Peak	-0.016	-0.066***	-0.065***	-0.064***
	(0.026)	(0.017)	(0.016)	(0.011)
Peak	-0.026	-0.080***	-0.080***	-0.078***
	(0.023)	(0.016)	(0.016)	(0.014)
Placebo Weekday				
Off-peak	-0.042*	0.021	0.029	
	(0.019)	(0.017)	(0.018)	
Mid-Peak	-0.060*	0.008	0.011	-0.013
	(0.024)	(0.027)	(0.028)	(0.014)
Peak	-0.020	0.015	0.019	-0.006
	(0.024)	(0.024)	(0.029)	(0.012)
Placebo Weekend				
Off-peak	-0.046*	0.018	0.041*	
	(0.022)	(0.014)	(0.017)	
Mid-Peak	-0.060*	-0.007	0.004	-0.025***
	(0.026)	(0.016)	(0.018)	(0.007)
Peak	-0.013	0.006	0.003	-0.011
	(0.024)	(0.016)	(0.019)	(0.008)
R-sqr	0.002	0.004	0.005	0.002
Observations	142,000	142,000	142,000	142,000
Hour X Weekend X Month-of-Sample	Yes	Yes	Yes	Yes
Hour X Weekend X Area	Yes	Yes	Yes	Yes
Hour X Weekend X Area X Month		Yes	Yes	Yes
Hour X Weekend X Temp. controls			Yes	
Weekend X Area X Month-of-sample				Yes

Notes: This table presents hourly estimates of Equation (2). The dependent variable is log consumption per capita. Observations are weighted by the number of consumers in each distribution area. Standard errors clustered at the area-month of sample level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

area-specific model of electricity consumption. For that, consider the following standard regression:

$$Y_{iht} = \gamma_{ih} X_{iht} + \epsilon_{iht}, \tag{3}$$

where Y_{iht} is the electricity consumption per capita in the distribution area i at hour h in period t, and X_{iht} is a set of control variables. This vector of covariates includes month, weekend, and national holiday dummies, temperature, the minimum and maximum temperature of the day, and all possible interactions between these variables. In practice, we estimate a separate regression for each area-hour unit, so effectively, each variable is interacted with area and hour fixed-effects. Importantly, we only use pre-treatment observations to estimate Equation (3). We treat Portugal as another area unit (and the only non-treated unit).

We estimate equation (3) using the LASSO regularization technique to choose among all possible predictors with nonzero significant values. Note that these predictors may vary between hours of the day or areas.[21] We then use these models to generate in-sample and out-of-sample predictions of electricity consumption per capita in the post-treatment period. We compute prediction errors by comparing predictions with the actual outcome.

Figure 9 in Appendix B shows the fraction of models for which a variable is selected. We find that national holidays and weekends explain electricity consumption in both, Spain and Portugal. The maximum daily temperature appears to be even more important than the hourly temperature. As for months, Figure 9 suggests that Portugal has greater seasonal effects, with monthly dummies being an important factor for most hours of the day.

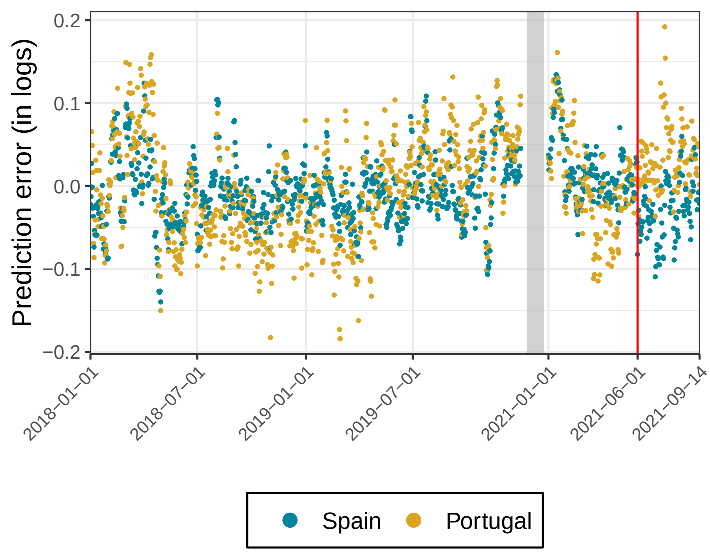
In the second step, we use the prediction errors as the dependent variable in the DiD and TD model presented in Section 4.1. Figure 6 plots the prediction errors for Spain and Portugal, with a break for the year 2020. The red line marks the date of the reform. Before its implementation, residuals are centered around zero, meaning that in-sample predictions generally approximate the observed data.[22] On the other hand, there is a decoupling in prediction errors between Spain and Portugal after June 2021, with prediction errors for Spanish distribution areas turning negative, meaning that the predicted consumption overstates the realized one. Intuitively, this should be related to the effect of the policy that we want to identify.[23] Note that Figure 6 pools hours with different TOU tariffs, and thus, the decrease in consumption is an average effect of the policy (or even related to the generalized increase in prices coinciding with the date of the policy). To gain intuition of the effects of the policy, Figure 11 in the Appendix plots prediction errors by TOU periods. One can see that while the ML estimation is able to predict consumption during off-peak periods, prediction errors become significantly

[21] We choose the level of regularization through cross-validation, where we partition the data into ten subsamples. We follow the commonly used "one standard error" rule to select the parameters with the best performance.

[22] For Spain, the plotted residuals in Figure 6 show the average among distribution areas, which lowers its variance with respect to Portugal.

[23] We also considered a random forest approach instead of the LASSO regression for the first step. Figure 10 plots the difference in prediction errors between the two methods for Spain and Portugal, and Figure 9c shows the most relevant variables. Even though the random forest approach seems to provide better in-sample predictions, especially in capturing seasonal patterns, regression results in step two are similar to LASSO, suggesting that these patterns were then captured in the panel fixed effect regression. Tables 9 and 10 show these results. Note that in the random forest approach, some placebo coefficients turn significant. This is not because the random forest approach is unable to control for pre-trends (the magnitude is close to the LASSO method), but because in general, we find that the random forest produces more precise estimates.

Figure 6: Machine Learning - Lasso: Prediction errors



Notes: This figure plots daily prediction errors, defined as the difference between the log of the demand per capita and the log of the prediction of our LASSO model. For Spain, the prediction error is the average over distribution groups.

negative during Mid- and Peak hours.[24]

The identifying assumption requires treated and control units to trend similarly in prediction errors, rather than electricity consumption, modifying the standard parallel trend assumption in DID settings. This is because the counterfactual prediction acts as a new pseudo-control observation, taking into account all controls flexibly (and individually) included in the first step. Alternatively, the prediction error can be interpreted as an additional difference in the DID (or Triple-Differences) models.

Table 4 presents the results for the DiD model of Equation (1) using prediction errors as the dependent variable. Focusing on column (2), mid-peak and peak coefficients are -6.4% and -9.5%, respectively, slightly larger than the ones found using the standard panel fixed-effect regression. Again, we observe an insignificant reduction in off-peak consumption. Recall that this coefficient is an average of changes in consumption during weekdays off-peak hours, and weekends, and thus this coefficient may be explained by household behavior during weekends.

In Table 5, we split the results between weekdays and weekends. Again, results for mid-peak and peak hours mirror those of the DiD model. Also, results suggest greater effects of the policy compared to the standard panel fixed-effect model. In particular, we find a 6.4% and 9.5% decrease during mid- and peak hours, respectively. However, we still do not find any evidence of load-shifting. Importantly, using a machine learning estimator seems to better capture hidden pre-trends unrelated to the policy, as evidenced by the robustness of placebo estimates across specifications. In particular, comparing estimates of columns (2) and (3), coefficients for off-

[24] Figure 12 shows the individual prediction for each distribution area and Portugal. The bigger differences between out-of-sample predictions and actual consumption are found for the EDP and Naturgy regions. In contrast, for Endesa and Repsol, predictions reasonably approximate the true evolution of electricity consumption. We can match these differences with the policy effects shown in Figures 13a and 13b, derived from estimating the Panel FE and Machine learning models for each distribution area. In all cases, the biggest effects are found in the EDP, Naturgy, and Iberdrola regions, while for Endesa and Repsol, the results are less conclusive.

Table 4: Machine learning - Lasso: Differences-in-Differences

	(1)	(2)	(3)	(4)
Policy				
Off-peak	-0.028*	-0.028	-0.029*	
	(0.014)	(0.015)	(0.015)	
Mid-peak	-0.065***	-0.064***	-0.064***	-0.068***
	(0.013)	(0.014)	(0.014)	(0.006)
Peak	-0.095***	-0.095***	-0.094***	-0.099***
	(0.012)	(0.013)	(0.013)	(0.007)
Placebo				
Off-peak	0.030	0.022	0.016	
	(0.017)	(0.018)	(0.017)	
Mid-peak	0.016	0.016	0.013	-0.013
	(0.017)	(0.026)	(0.024)	(0.009)
Peak	0.027	0.025	0.023	-0.004
	(0.016)	(0.025)	(0.023)	(0.008)
R-sqr	0.009	0.007	0.007	0.003
Observations	142,000	142,000	142,000	142,000
Hour X Weekend X Month-of-Sample	Yes	Yes	Yes	Yes
Hour X Weekend X Area	Yes	Yes	Yes	Yes
Hour X Weekend X Area X Month		Yes	Yes	Yes
Hour X Weekend X Temp. controls			Yes	
Weekend X Area X Month-of-sample				Yes

Notes: This table presents estimates of Equation (1). The dependent variable is the prediction error of log consumption from a LASSO area-specific model. Observations are weighted by the number of consumers in each distribution area. Standard errors clustered at the area-month of sample level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

peak hours are insensitive to the inclusion of possible bad controls. Recall that temperature is already taken into account in the first step, but it is the data-driven process that determines for which cases (i.e., for which area-hour units) the variable has explanatory power. Finally, conditional on choosing a plausibly correct specification (columns (4) for both models, where we can control for increases in energy costs common to different TOU tariffs), the better precision of the ML estimates turns some placebo coefficients slightly significant, but we do not think that this is sufficient to favor the standard panel fixed effects model over the ML estimates. We plot the hourly coefficients of the policy and placebo effects in Figure 7. Comparing it to the hourly coefficients of the standard fixed effects model in Figure 5, we find that the results are similar to our central specification.[25] However, there are still some patterns that could be of concern, with some positive consumption at night in our Placebo, particularly during the weekends.

We address these unobserved confounders by estimating the additional policy effects during weekdays with respect to weekends with a standard triple-differences model. Compared to Table 5, this effect can be estimated by the difference between the weekday and the weekend coefficient. Table 8 in the Appendix shows the results. Taking this scenario as a lower bound, we still find a decrease in consumption during peak hours of 1-2%. However, these effects are no longer significant for most specifications. That said, this approach may be too conservative for two reasons. First, in identifying only additional effects during weekdays with respect to weekends, we are implicitly shutting down other than the price effects of the policy, such as habit formation or some degree of misinformation on pricing during the weekend. Second, as shown in Table 5, on average, placebo tests do not show significant responses in most specifications. However, in column (4), which focuses on the within-month comparison, we find some mid-peak and peak reductions during placebo weekends (of about 1-3%). These are still substantially smaller than those estimated during weekends with the policy (6 -7%). It would also increase the triple difference effect if we corrected for this placebo effect. The hourly plots in Figure 7 also provide reassurance that these effects are much smaller than our main findings.

For the standard errors to be comparable, one should account for the noise generated in the first step of the estimation. To assess the relevance of these concerns, we implement a two-step bootstrap procedure. In the first step, we sample with replacement months of the sample during the pre-treatment period for each distribution area. We then use these data to estimate the area-specific LASSO model and obtain predictions for all months in the original data (this is, all pre- and post-treatment periods). We repeat this step twenty times to have twenty alternative predictions for each area. In the second step, we draw one prediction for each area, compute the prediction error and estimate the panel fixed-effect model. We repeat this process a thousand times.[26] Figure 15 in the Appendix shows the results of this exercise for each group of coefficients. The standard deviation of the bootstrapped distribution ranges between 0.002 and 0.005, suggesting that the noise generated in the first step is of smaller magnitude than the standard errors clustered at area-month of sample level. It is nevertheless reassuring that the mean of the distribution of the bootstrapped coefficients converges to the triple-differences coefficients estimated in the main analysis, suggesting that our results are not driven by a particular sample

[25] Hourly estimates for the fixed effects regressions are quite sensitive to the specification of choice, but they are very stable across specifications for the machine learning models.

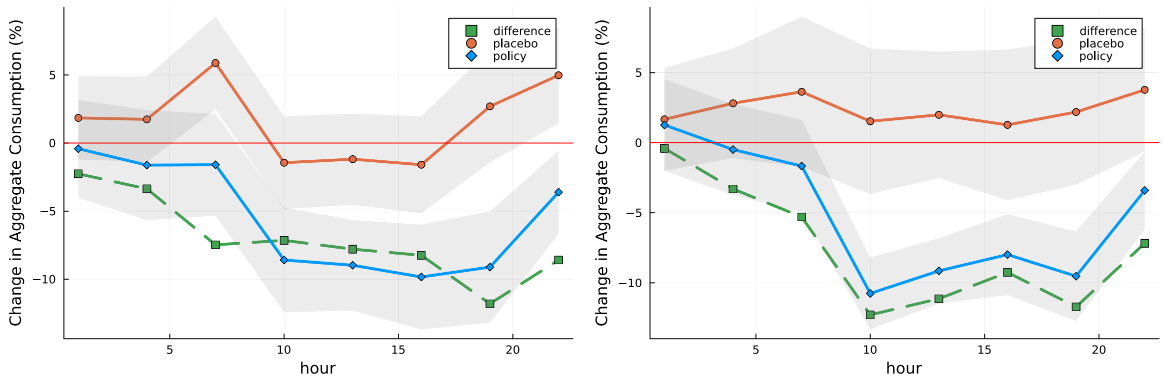
[26] Usually, at this stage, one would sample at the level of the events, estimate Equation (2), and get a distribution of policy coefficients. The standard deviation of this distribution would be the analogue to the clustered standard errors computed in the main analysis. This is the approach followed in Patnaik et al. (2013) based on Davison et al. (1986). Unfortunately, our treatment consists of only one event, which makes this event study bootstrapping strategy not applicable.

Table 5: Machine Learning: triple-differences

	(1)	(2)	(3)	(4)
Policy Weekday				
Off-peak	0.003	0.003	-0.000	
	(0.015)	(0.016)	(0.015)	
Mid-Peak	-0.065***	-0.064***	-0.064***	-0.068***
	(0.013)	(0.014)	(0.014)	(0.006)
Peak	-0.095***	-0.095***	-0.094***	-0.099***
	(0.012)	(0.013)	(0.013)	(0.007)
Policy Weekend				
Off-peak	-0.004	-0.008	-0.006	
	(0.017)	(0.018)	(0.017)	
Mid-Peak	-0.075***	-0.073***	-0.073***	-0.065***
	(0.018)	(0.017)	(0.016)	(0.011)
Peak	-0.084***	-0.083***	-0.084***	-0.076***
	(0.020)	(0.018)	(0.018)	(0.014)
Placebo Weekday				
Off-peak	0.040*	0.029	0.026	
	(0.019)	(0.020)	(0.019)	
Mid-Peak	0.016	0.016	0.013	-0.013
	(0.017)	(0.026)	(0.024)	(0.009)
Peak	0.027	0.025	0.023	-0.004
	(0.016)	(0.025)	(0.023)	(0.008)
Placebo Weekend				
Off-peak	0.045*	0.032*	0.026	
	(0.018)	(0.016)	(0.016)	
Mid-Peak	0.005	0.001	-0.006	-0.031***
	(0.015)	(0.017)	(0.014)	(0.004)
Peak	0.015	0.015	0.005	-0.017***
	(0.015)	(0.019)	(0.015)	(0.005)
R-sqr	0.010	0.008	0.008	0.003
Observations	142,000	142,000	142,000	142,000
Hour X Weekend X Month-of-Sample	Yes	Yes	Yes	Yes
Hour X Weekend X Area	Yes	Yes	Yes	Yes
Hour X Weekend X Area X Month		Yes	Yes	Yes
Hour X Weekend X Temp. controls			Yes	
Weekend X Area X Month-of-sample				Yes

Notes: This table presents estimates of Equation (2). The dependent variable is the prediction error of log consumption from a LASSO area-specific model. Observations are weighted by the number of consumers in each distribution area. Standard errors clustered at the area-month of sample level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

Figure 7: Machine Learning: Hourly Policy effects by type of day



(a) Weekends and Holidays

(b) Weekdays

Notes: These figures present estimates of Equation (2) - Specification (2) in blocks of 3 hours. The dependent variable is the prediction error of log consumption from a LASSO area-specific model. Observations are weighted by the number of consumers in each distribution area. Controls include area-month-hour-weekend and month-of-sample-hour-weekend fixed effects. Standard errors clustered at the area-month of sample level and confidence intervals reflect a 95% significance level.

during the estimation of the machine learning method.

5 Extensions

5.1 Price Elasticities

This section uses the exogeneity of the policy to estimate household price elasticities. Albeit far from a formal demand estimation analysis, we believe that it can improve the comparability of the results to previous studies, and, therefore, may be helpful for policy evaluation and guidance. We consider a modified version of the DiD model presented in Equation (1) substituting the policy indicator with actual electricity prices:

$$y_{ith} \;=\; \eta p_{ith} + \gamma X_{ith} + \alpha_{ith} + \epsilon_{ith}, \tag{4}$$

where y_{ith} is the consumption prediction error from our lasso model in the distribution area i at time t and hour h, and p_{iht} is the log of the electricity price. Thus, η can be interpreted as the price elasticity.

Estimating Equation (4) for only the treatment group would be problematic because the price coefficient is biased in a context of simultaneous equations where prices and quantities are jointly determined in equilibrium. However, by including Portugal data as our control group and using the policy as an instrument for electricity prices, we can break the simultaneity problem. We instrument the logarithm of the price with the logarithm of the sum of the charges component affected by the new TOU and the average energy cost prior to the policy. Given that the instrument is used to construct the total price, it satisfies the relevance condition by definition.[27]

The Spanish electricity prices come from the archives of the Spanish System Operator, *Red Eléctrica de España* (REE). See Table 1 and Figure 2a for summary statistics. Traditionally, regulated prices in Portugal

[27] Another problem in estimating Equation (4) is that these estimates do not take into account intra-day substitution patterns (cross-elasticities), where consumption in hour h could be affected by a price of a different hour of the day. Given the limited price variation, we only estimate the own elasticity.

Table 6: Elasticity estimates

	(1)	(2)	(3)	(4)	(5)
Log(price)	-0.092***	-0.087***	-0.082***	-0.135***	-0.108***
	(0.015)	(0.016)	(0.015)	(0.011)	(0.014)
Δ 25 - 75th					0.010
					(0.013)
Δ 75 - 90th					0.010
					(0.013)
Δ >90th					0.011
					(0.012)
Observations	142,000	142,000	142,000	142,000	142,000
Hour X WE X Month-of-Sample	Yes	Yes	Yes	Yes	Yes
Hour X Weekend X Area	Yes	Yes	Yes	Yes	Yes
Hour X Weekend X Area X Month		Yes	Yes	Yes	Yes
Hour X Weekend X Temp. controls			Yes		
Weekend X Area X Month-of-sample				Yes	

Notes: This table presents estimates of Equation (4). All variables are in log-form. Prediction errors are the difference between the observed demand per capita (in logs) and the predicted one (in logs). Column (5) shows the results of the IV specification when we split the sample by percentiles of the energy cost component. Observations are weighted by the number of consumers in each distribution area. Standard errors clustered at the area-month of sample level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

were decided for the year ahead. However, given the recent increases in wholesale prices, regulated tariffs kept increasing during 2020 and 2021.[28] Nonetheless, these prices were only updated monthly. Thus, they add limited variation given that our regressions include month-of-sample fixed effects.

Table 6 presents the demand parameters estimated using Equation (4). Looking at columns (1) to (4), we observe significant price elasticities ranging between -0.082 and -0.135. This suggests consumers substantially responded to TOU prices. To further understand demand response to changes in prices, column (5) of Table 6 shows results when we estimate equation 4 splitting the sample based on the percentile of the energy cost after the policy was implemented.[29] In the context of the European crisis, this amounts to roughly separating days over time. The estimates do not display any additional effect at different price levels.[30] This is reassuring in our context, as it implies that our results of changes in hourly consumption patterns are not explained by growing average electricity prices.

We can use these estimates to compute back-of-the-envelope demand responses given price changes and, thus, connect these results with our policy estimates. For that, we focus on column (4), which provides the cleanest comparison. This amounts to testing the implied elasticity that arises from the differential response to mid-peak and peak prices. In what follows, we take the elasticity of -0.135 found in column (4) of Table 6, and

[28] See the Portuguese regulator website.

[29] For each regression, we kept all the observations for Portugal and for Spain prior to the policy, while we divided the days after the policy was implemented based on average daily real-time prices in Spain.

[30] Prest (2020) finds that awareness is the major factor in predicting higher treatment effects in an experiment with different TOU tariffs. The paper finds a declining price elasticity, suggesting that different price levels did not trigger different demand responses. While the test is not equivalent, our results also point out awareness as a major factor explaining consumer response.

the policy effects during weekdays estimated using prediction errors (see column (4) in Table 5).

Recall from Figure 2a that the newly implemented TOU rates were 6, 42, and 133€/MWh for off-, mid-, and peak hours, respectively, while they were constant before for a majority of households. According to these price changes and the estimated elasticity, electricity consumption should have decreased by 4.8% and 16.2% in mid- and peak hours, respectively.[31] These results suggest that consumers overly responded in mid-peak hours (6.8%) and under responded to peak hours (9.9%), for the effects to be consistent with a constant and independent elasticity. While imperfect, we believe consumers responded (at least on average) consistently to the observed price changes. In the end, we should take into account that not all consumption can be easily shifted. Thus, these differences can reflect the impossibility of households to perfectly time their consumption. Indeed, spillovers to nearby hours are common in the critical peak pricing literature (e.g., Jessoe and Rapson (2014)).

5.2 Google Trends

In this section, we present an exploratory analysis of the relationship between Google searches and the effect of the policy. As shown in Figure 4, there was only a modest increase in searching during the weeks previous to the introduction of the policy, while most searches were concentrated during the first week, returning to initial levels afterward. It is this one-time shock nature and the subsequent lack of variation that did not allow us to proceed with a more formal analysis to establish some sort of causality between searching behavior and consumer response.

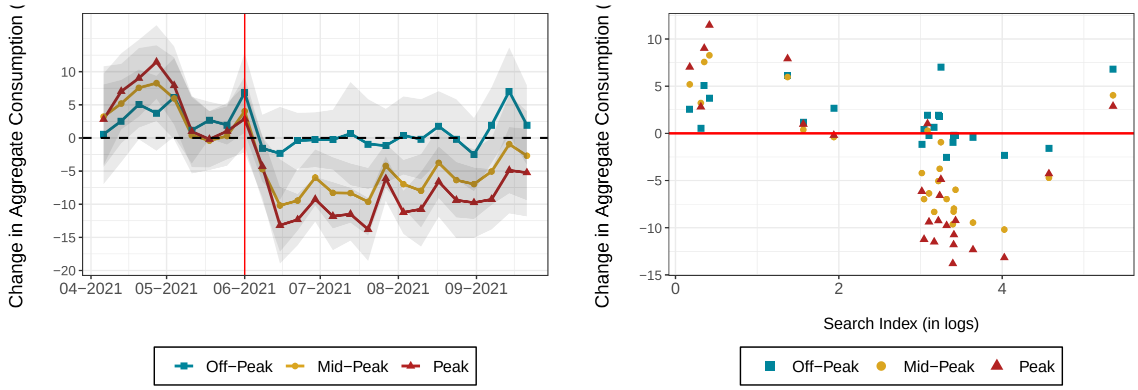
To study the relationship between search and consumption behavior, Figure 8a shows the weekly change in consumption by TOU tariff starting two months prior to the policy, thus capturing both policy and placebo effects. We estimate these effects using Equation (1) on weekdays consumption data with prediction errors as the outcome variable. Figure 8b combines both graphs by plotting the relation between searches and policy effects. We find the following results. First, in concordance with the estimated placebo effects, if any we see an increase in demand prior to its implementation. Importantly, we do not observe any differential patterns between hours of the day. These weeks correspond to the left-most points in Figure 8b. Second, starting one week after the introduction of the policy (the first week that is completely treated), it starts a process of divergence in consumption between TOU tariffs. This trend is maintained during the following weeks, although there is a significant reduction in search. Even though it is not possible to draw causal conclusions from this relationship, these estimates are consistent with an adaptation process that took around three weeks and ended with a permanent change in household behavior.

6 Conclusions

We study the effects of a TOU pricing program for residential electricity demand introduced in Spain in June 2021. Under the new regulation, system and network charges (accounting for 50% of the overall electricity bill) had three tiers during weekdays, while weekends were fully off-peak. To identify the causal impact of

[31] Apart from the change in TOU tariffs, to compare price differences with our policy estimates we need the price for the energy cost. We take 100€/MWh, which approximates the price of electricity during the first month after the introduction of the policy.

Figure 8: Google Trends and Policy Effects



(a) Policy effects by week

(b) Google Searches and Policy effects

Notes: Figure 8a presents estimates of Equation (1) on weekdays consumption data with prediction errors as the outcome variable. We aggregate the treatment dummies at the TOU tariff level and interact them with week-of-sample fixed effects. Observations are weighted by the number of consumers in each distribution area. We include area-month-weekend-hour, and week-of-sample-hour fixed effects. Standard errors clustered at the area-month of sample level and confidence intervals reflect a 95% significance level.

the policy, we estimate a Differences-in-Differences model using Portugal to control for cross-sectional and temporal confounders. We compare two empirical models, the first being the standard fixed effects panel model. Using this method, we find a significant reduction in consumption of 5.7% and 8.9% for mid and peak hours on average, while we do not find any load-shifting to hours with lower prices. We then split off-peak hours between weekdays and weekends. We observe a significant demand response during weekends for all three periods, pointing to some sort of habit formation.

We then turn to machine learning techniques for variable selection, given the richness of fixed effects in this setting. We do not find evidence that using machine learning techniques helps to reduce the sensitivity of the policy estimates across different specifications. A possible explanation for the sensitivity of this approach could come from the lack of heterogeneity of treated units and treatment dates. Nonetheless, the algorithm is able to capture pre-trends unrelated to the policy, improving the credibility of our estimates and reducing the risk of including bad controls. In particular, during weekdays, consumption decreased by 6.4%, and 9.5% during mid- and peak hours, respectively. We also find significant decreases during weekends, suggesting spillover effects to untreated hours due to misinformation or habit formation.

This paper contributes to the discussion on whether dynamic pricing schemes are an effective tool to change consumer behavior and help to reduce overall energy consumption. We find that salience can be a crucial factor in driving consumer response and that, while a system with frequent price changes may need to be accompanied by a process of automation, we find that a TOU pricing scheme can have significant effects given its foreseeable nature and thus, forming new consumption habits. All in all, economic incentives can be an effective tool, especially when the long-run effects such as habit formation are taken into account.

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7 Appendix

A Tables

Table 7: Summary statistics

			Spain			
variable	*units*	*mean*	*st. dev.*	*minimum*	*median*	*maximum*
demand	MWh	3064.91	909.76	1466.20	2983.40	7029.90
consumer	Million	11.07	0.28	10.43	11.10	11.51
demand per capita	Wh/cons.	276.65	81.13	137.13	270.40	648.56
temperature	Celsius	15.84	7.18	-1.30	15.16	35.21
high temperature	Binary	0.30	0.46	0.00	0.00	1.00
			Portugal			
variable	*units*	*mean*	*st. dev.*	*minimum*	*median*	*maximum*
demand	MWh	344.50	97.94	160.60	316.10	816.30
consumer	Million	1.07	0.09	0.92	1.09	1.21
demand per capita	Wh/cons.	320.95	85.62	164.02	297.45	820.30
temperature	Celsius	16.95	8.48	-3.72	15.89	44.63
high temperature	Binary	0.33	0.47	0.00	0.00	1.00

Notes: Sample between January 2018 and September 14, 2021, excluding 2020. The unit of observation is an hour-distribution area. There are five distribution areas in Spain and one distribution area in Portugal. $N = 142,000$.

Table 8: Lasso: triple-differences additional effect

	(1)	(2)	(3)	(4)
Policy				
Off-peak	-0.004	-0.008	-0.006	
	(0.017)	(0.018)	(0.017)	
Mid-Peak	-0.075***	-0.073***	-0.073***	-0.065***
	(0.018)	(0.017)	(0.016)	(0.011)
Peak	-0.084***	-0.083***	-0.084***	-0.076***
	(0.020)	(0.018)	(0.018)	(0.014)
∆ Policy Weekday				
Off-peak	0.007	0.011	0.006	
	(0.010)	(0.009)	(0.005)	
Mid-Peak	0.010	0.009	0.009	-0.002
	(0.013)	(0.011)	(0.009)	(0.009)
Peak	-0.012	-0.012	-0.009	-0.023*
	(0.015)	(0.013)	(0.012)	(0.010)
Placebo				
Off-peak	0.045*	0.032*	0.026	
	(0.018)	(0.016)	(0.016)	
Mid-Peak	0.005	0.001	-0.006	-0.031***
	(0.015)	(0.017)	(0.014)	(0.004)
Peak	0.015	0.015	0.005	-0.017***
	(0.015)	(0.019)	(0.015)	(0.005)
∆ Placebo Weekday				
Off-peak	-0.005	-0.003	-0.001	
	(0.005)	(0.005)	(0.005)	
Mid-Peak	0.011*	0.015	0.019	0.018**
	(0.005)	(0.010)	(0.011)	(0.006)
Peak	0.012**	0.010	0.018*	0.013**
	(0.004)	(0.008)	(0.009)	(0.005)
R-sqr	0.010	0.008	0.008	0.003
Observations	142,000	142,000	142,000	142,000
Hour X Weekend X Month-of-Sample	Yes	Yes	Yes	Yes
Hour X Weekend X Area	Yes	Yes	Yes	Yes
Hour X Weekend X Area X Month		Yes	Yes	Yes
Hour X Weekend X Temp. controls			Yes	
Weekend X Area X Month-of-sample				Yes

Notes: This table presents estimates related to Equation (2) but computing the additional effect of the policy during weekdays with respect to weekends. Observations are weighted by the number of consumers in each distribution area. Standard errors clustered at the area-month of sample level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

Table 9: Random Forests: Differences-in-Differences

	(1)	(2)	(3)	(4)
Policy				
Off-peak	-0.017*	-0.018*	-0.018*	
	(0.009)	(0.008)	(0.008)	
Mid-peak	-0.053***	-0.054***	-0.054***	-0.065***
	(0.010)	(0.009)	(0.009)	(0.005)
Peak	-0.081***	-0.082***	-0.082***	-0.093***
	(0.009)	(0.008)	(0.008)	(0.007)
Placebo				
Off-peak	0.027	0.027*	0.025	
	(0.018)	(0.013)	(0.013)	
Mid-peak	0.023	0.022	0.021	-0.009**
	(0.014)	(0.013)	(0.013)	(0.003)
Peak	0.029*	0.027*	0.027*	-0.004
	(0.012)	(0.011)	(0.011)	(0.004)
R-sqr	0.017	0.014	0.014	0.006
Observations	142,000	142,000	142,000	142,000
Hour X Weekend X Month-of-Sample	Yes	Yes	Yes	Yes
Hour X Weekend X Area	Yes	Yes	Yes	Yes
Hour X Weekend X Area X Month		Yes	Yes	Yes
Hour X Weekend X Temp. controls			Yes	
Weekend X Area X Month-of-sample				Yes

Notes: This table presents estimates of Equation (1). Observations are weighted by the number of consumers in each distribution area. Standard errors clustered at the area-month of sample level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

Table 10: Random Forests: triple-differences

	(1)	(2)	(3)	(4)
Policy Weekday				
Off-peak	0.009	0.010	0.011	
	(0.011)	(0.010)	(0.010)	
Mid-Peak	-0.054***	-0.055***	-0.055***	-0.065***
	(0.010)	(0.009)	(0.009)	(0.005)
Peak	-0.082***	-0.083***	-0.082***	-0.093***
	(0.009)	(0.008)	(0.008)	(0.007)
Policy Weekend				
Off-peak	0.004	0.003	0.003	
	(0.013)	(0.011)	(0.011)	
Mid-Peak	-0.059***	-0.057***	-0.058***	-0.061***
	(0.012)	(0.011)	(0.011)	(0.011)
Peak	-0.069***	-0.070***	-0.070***	-0.073***
	(0.012)	(0.010)	(0.010)	(0.012)
Placebo Weekday				
Off-peak	0.032	0.031*	0.030*	
	(0.017)	(0.012)	(0.013)	
Mid-Peak	0.023	0.021	0.021	-0.010**
	(0.014)	(0.013)	(0.013)	(0.004)
Peak	0.030*	0.027*	0.027*	-0.004
	(0.012)	(0.012)	(0.012)	(0.005)
Placebo Weekend				
Off-peak	0.027	0.028*	0.025	
	(0.020)	(0.014)	(0.014)	
Mid-Peak	0.014	0.014	0.012	-0.014***
	(0.017)	(0.013)	(0.013)	(0.002)
Peak	0.026	0.027*	0.028*	-0.001
	(0.017)	(0.014)	(0.013)	(0.003)
R-sqr	0.021	0.017	0.017	0.008
Observations	142,000	142,000	142,000	142,000
Hour X Weekend X Month-of-Sample	Yes	Yes	Yes	Yes
Hour X Weekend X Area	Yes	Yes	Yes	Yes
Hour X Weekend X Area X Month		Yes	Yes	Yes
Hour X Weekend X Temp. controls			Yes	
Weekend X Area X Month-of-sample				Yes

Notes: This table presents estimates related to Equation (2). Observations are weighted by the number of consumers in each distribution area. Standard errors clustered at the area-month of sample level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

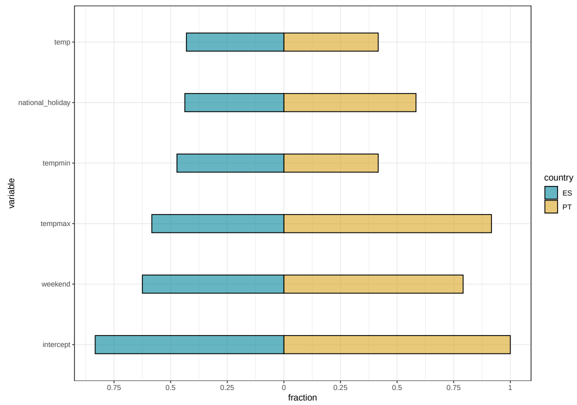
Table 11: Random Forests: triple-differences additional effect

	(1)	(2)	(3)	(4)
Policy				
Off-peak	0.004	0.003	0.003	
	(0.013)	(0.011)	(0.011)	
Mid-Peak	-0.059***	-0.057***	-0.058***	-0.061***
	(0.012)	(0.011)	(0.011)	(0.011)
Peak	-0.069***	-0.070***	-0.070***	-0.073***
	(0.012)	(0.010)	(0.010)	(0.012)
Δ Policy Weekday				
Off-peak	0.005	0.007	0.008	
	(0.010)	(0.008)	(0.008)	
Mid-Peak	0.004	0.003	0.003	-0.004
	(0.013)	(0.010)	(0.010)	(0.008)
Peak	-0.013	-0.013	-0.012	-0.020*
	(0.013)	(0.011)	(0.010)	(0.008)
Placebo				
Off-peak	0.027	0.028*	0.025	
	(0.020)	(0.014)	(0.014)	
Mid-Peak	0.014	0.014	0.012	-0.014***
	(0.017)	(0.013)	(0.013)	(0.002)
Peak	0.026	0.027*	0.028*	-0.001
	(0.017)	(0.014)	(0.013)	(0.003)
Δ Placebo Weekday				
Off-peak	0.004	0.003	0.004	
	(0.004)	(0.002)	(0.003)	
Mid-Peak	0.009*	0.007	0.009	0.004
	(0.004)	(0.004)	(0.004)	(0.004)
Peak	0.004	-0.000	-0.001	-0.003
	(0.006)	(0.005)	(0.005)	(0.004)
R-sqr	0.021	0.017	0.017	0.008
Observations	142,000	142,000	142,000	142,000
Hour X Weekend X Month-of-Sample	Yes	Yes	Yes	Yes
Hour X Weekend X Area	Yes	Yes	Yes	Yes
Hour X Weekend X Area X Month		Yes	Yes	Yes
Hour X Weekend X Temp. controls			Yes	
Weekend X Area X Month-of-sample				Yes

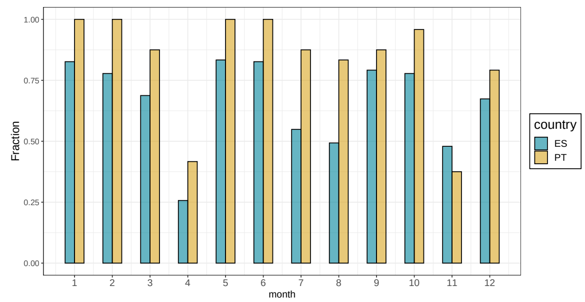
Notes:This table presents estimates related to Equation (2) but computing the additional effect of the policy during weekdays with respect to weekends. Observations are weighted by the number of consumers in each distribution area. Standard errors clustered at the area-month level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

B Figures

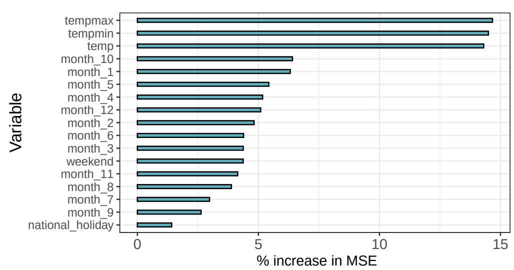
Figure 9: Machine learning: predictive capacity of control variables



(a) LASSO: fraction of models selecting controls

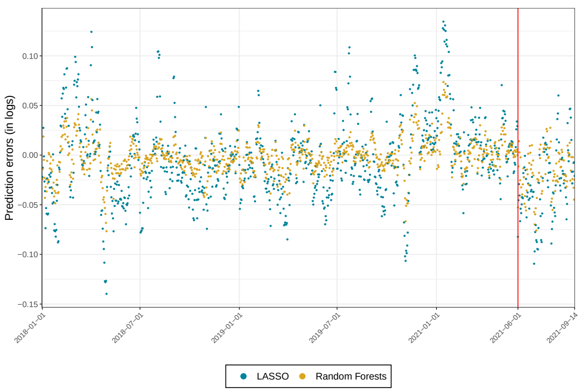


(b) LASSO: fraction of models selecting months

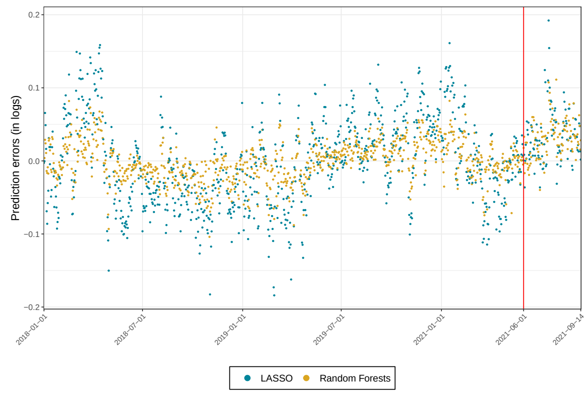


(c) Random Forest: variable importance

Figure 10: Prediction errors by method



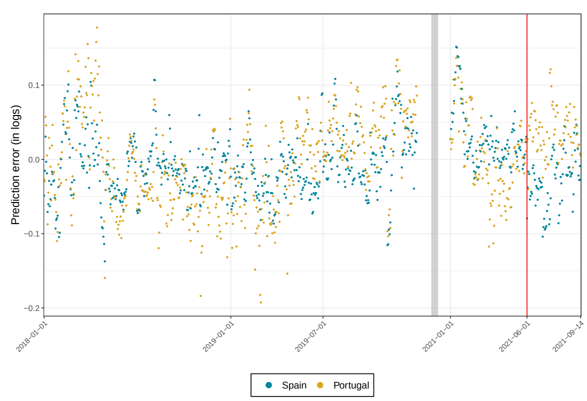
(a) Spain



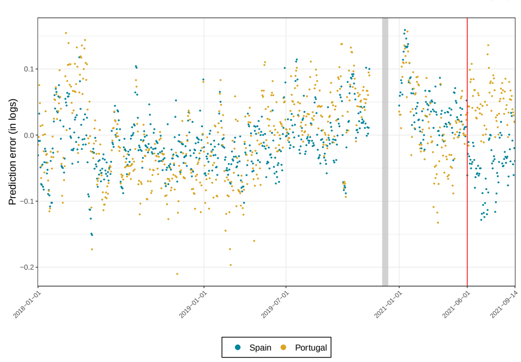
(b) Portugal

Figure 11: Lasso: Prediction errors by TOU period

(a) Off-Peak

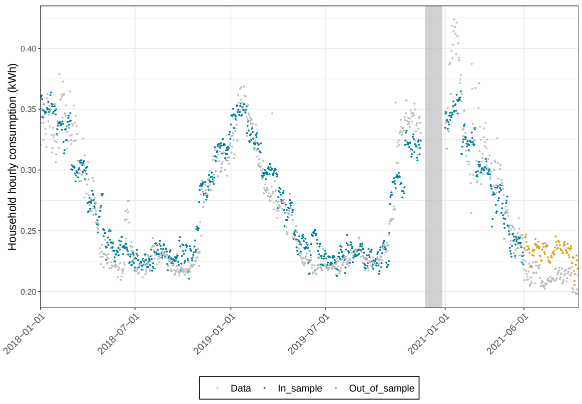


(b) Mid-Peak

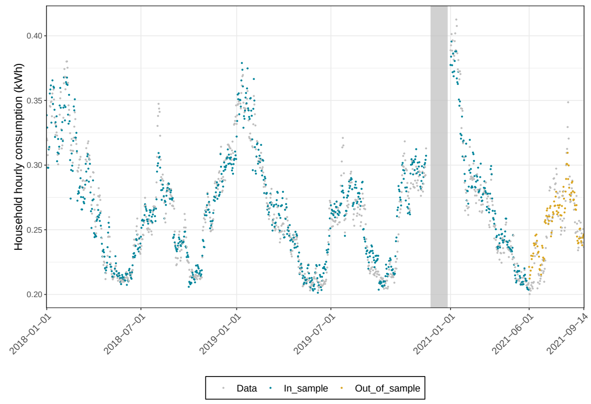


(c) Peak

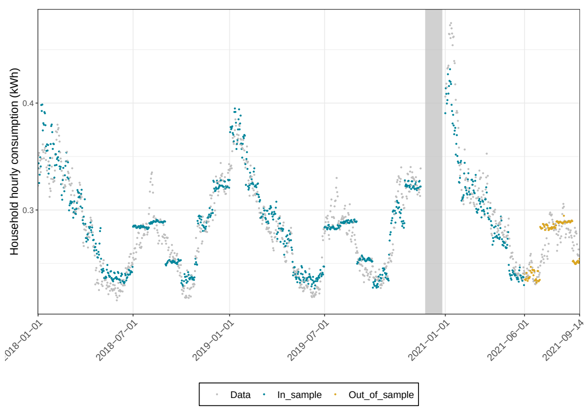
Figure 12: Lasso: in- and out-of-sample predictions by distribution area



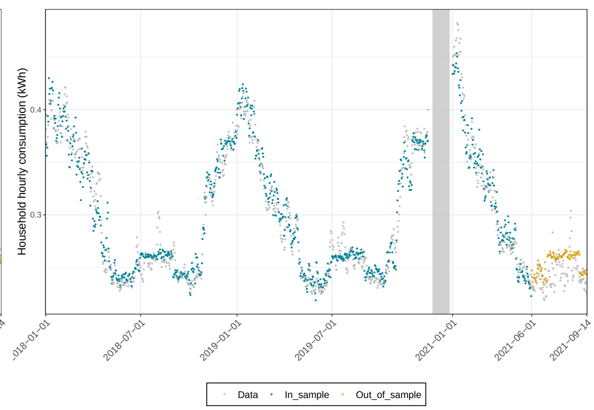
(a) EDP



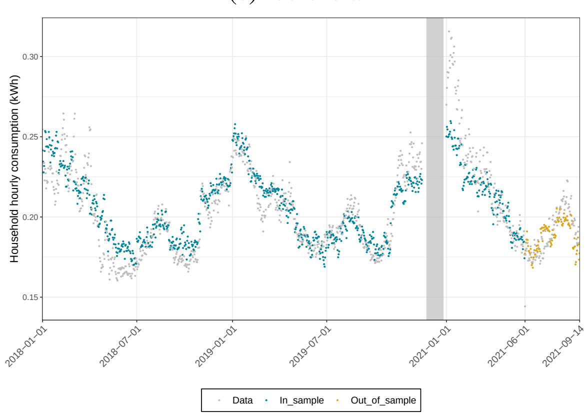
(b) Endesa



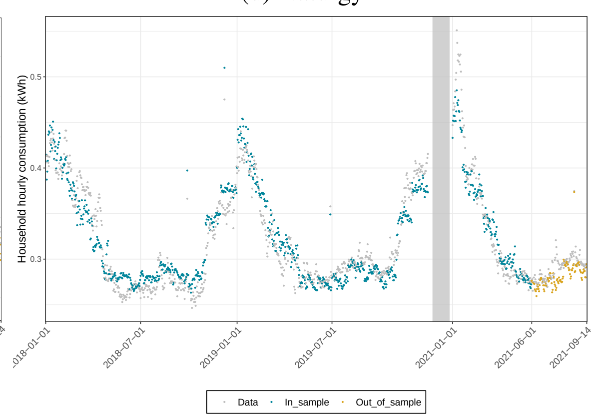
(c) Iberdrola



(d) Naturgy

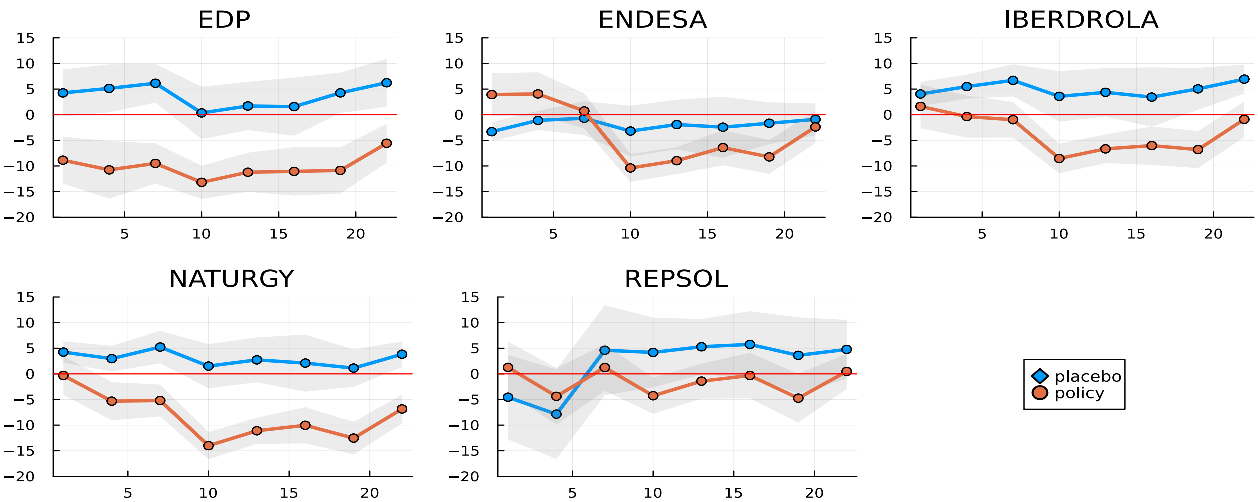


(e) Repsol

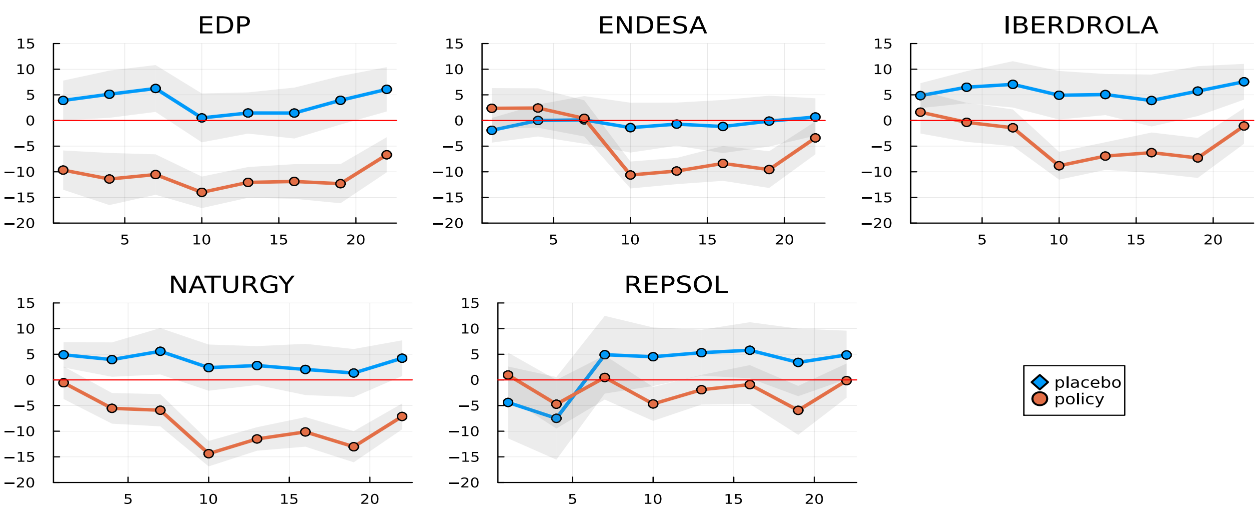


(f) Portugal

Figure 13: Triple-differences coefficients by distribution area and for weekdays

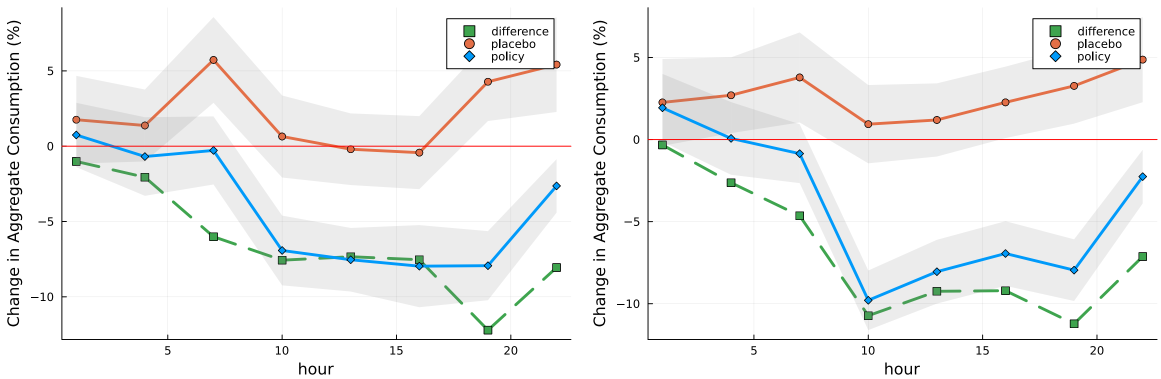


(a) Panel Fixed Effects



(b) Machine Learning: Lasso

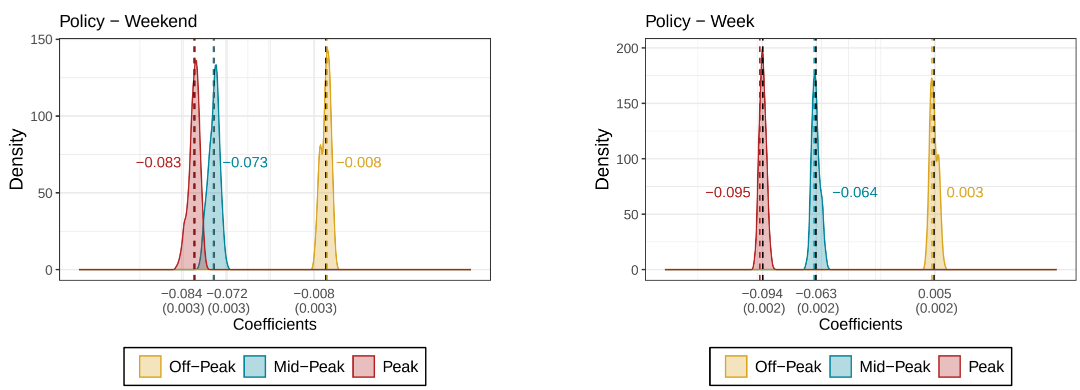
Figure 14: Random Forests: Policy effects by hour of the day and type of day



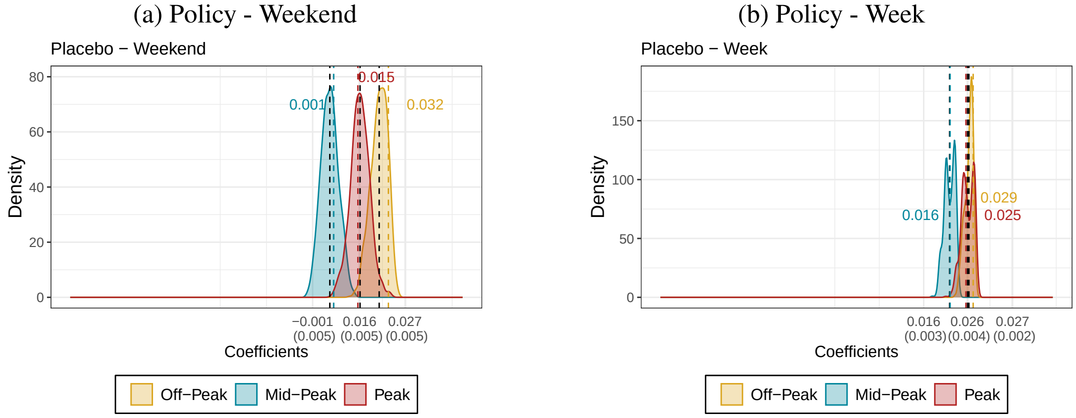
(a) Weekends and Holidays (b) Weekdays

Notes: These figures present estimates of Equation (2) - Specification (2) in blocks of 3 hours. Observations are weighted by the number of consumers in each distribution area. Controls include area-month-hour-weekend and month-of-sample-hour-weekend fixed effects. Standard errors clustered at the area-month of sample level and confidence intervals reflect a 95% significance level.

Figure 15: Bootstrapped Distribution of Triple-Difference LASSO coefficients



(a) Policy - Weekend (b) Policy - Week



(c) Placebo - Weekend (d) Placebo - Week