The Impact of Dynamic Prices on Electric Vehicle Public Charging Demand: Evidence from a Nationwide Natural Field Experiment*

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Abstract

Understanding how to effectively influence electric vehicle (EV) charging behavior is critical for managing electricity grids powered by high levels of variable renewable generation. We present results from a large-scale natural field experiment conducted in the United Kingdom, involving approximately 110,000 EV customers and 60% of the country's public charging infrastructure. Within this network, we applied a price decrease to approximately one-fifth of chargers to bring their prices closer to the marginal cost of electricity in low-cost hours in Great Britain. Customers were randomly assigned to different price levels, allowing us to estimate the causal effect of price on charging demand and derive elasticities that capture short-run behavioral responses within a subset of the UK's public charging network. Customers were highly responsive to price: a 40% reduction in charging costs increased platform-wide charging activity by 117%, while a 15% price cut led to a 30% increase. Decomposing the increase in charging, we estimate that approximately half reflects substitution between charging apps. The balance could represent new demand or displacement from unobserved sources, which are difficult to disentangle in this setting. Our findings suggest that dynamic pricing for public EV charging generates large consumer welfare gains.

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1 Introduction

Road transport is responsible for approximately 15% of global greenhouse gas emissions (IPCC, 2022), a share that rises to around 25% in the United States and the United Kingdom, with most emissions stemming from cars and light-duty vehicles (EPA, 2023; Department for Transport, 2024). To reduce these emissions, policymakers have primarily focused on encouraging the replacement of internal combustion engine (ICE) vehicles with electric vehicles (EVs) (Rapson and Muehlegger, 2023*a*). According to the International Energy Agency (2024*a*), a medium-sized ICE vehicle currently emits roughly twice as much CO_2e as an EV, and this gap is expected to widen as electricity systems further decarbonize. The United Kingdom and European Union have set aggressive EV adoption targets for the 2030s.[1]

As EVs become a major driver of residential and public electricity demand, understanding the price elasticity of EV charging is essential for energy economists, regulators, and policymakers. This paper provides the first large-scale experimental estimate of that elasticity in public charging, a key public infrastructure challenge. From a general economics perspective, we study how consumers respond to short-run variation in marginal cost for a durable good's primary energy input, one of the few contexts where real-time marginal cost pricing is observable and manipulable at scale. This context offers rare insight into demand behavior in a high-frequency, high-salience, decentralized energy market.[2]

The anticipated growth in EV adoption will place new demands on electricity grids. Without intervention, unmanaged charging is likely to strain networks during peak periods, leading to grid instability, higher infrastructure costs, and ultimately increased costs for consumers (Richardson, 2013; Anwar et al., 2022; Bailey et al., 2024; Sarda et al., 2024; Li and Jenn, 2024; Turk et al., 2024). Dynamic pricing has been proposed as a way to enable flexibility in EV charging demand, shifting consumption away from peak periods and toward times when variable renewable energy is abundant - or even in excess - thereby reducing both the cost and carbon intensity of electricity supply (Muratori, 2018;

[1] The UK plans to end the sale of new cars powered solely by ICE by 2030, and hybrids and vans by 2035 (Department for Transport, 2025*b*). The European Union has legislated to effectively ban new ICE cars by 2035 as part of its "Fit for 55" climate package (European Commission, 2023). The US previously aimed for 50% of new vehicle sales to be electric by 2030, but this executive order has since been revoked by the Trump administration (The White House, 2025).

[2] While there is important general interest work on the price elasticity of gasoline demand (Parry and Small, 2005; Bento et al., 2009; Edelstein and Kilian, 2009; Davis and Kilian, 2011; Allcott and Wozny, 2014; Li et al., 2014; Coglianese et al., 2017; Levin et al., 2017; Knittel and Tanaka, 2021), no credible price elasticity of public EV charging demand exists.

Birk Jones et al., 2022). In essence, it can bring prices closer to the real-time marginal cost of electricity. However, the literature provides no causal evidence of this marginal pricing setup affecting EV charging demand.

Another potential benefit of dynamic pricing is to lower EV running costs for consumers, particularly for those more reliant on public charging infrastructure. Alongside sensitivity to upfront costs (Holland et al., 2021; Muehlegger and Rapson, 2022), price elasticity of EV demand reflects these recharging expenses (Zhang et al., 2018; Zhou and Li, 2018; Borlaug et al., 2020; Burlig et al., 2021; Bushnell et al., 2022; Gillingham et al., 2023; Barwick et al., 2024; Dorsey et al., 2025). Understanding how responsive consumers are to charging costs is therefore relevant to welfare analysis of various EV policies (Arkolakis et al., 2025). Many countries have implemented substantial incentives to promote EV adoption, which offer significant welfare benefits (Rapson and Muehlegger, 2023*b*; Gillingham et al., 2023; Allcott et al., 2024; Hahn et al., 2024; Knittel and Tanaka, 2024).

While observational studies have examined EV charging behavior,[3] there has been no experimental evidence to date on how price influence public charging demand among a representative population of EV users. Our paper presents the first field experiment to estimate the causal impact of public charging costs on charging demand and to assess the flexibility of charging behavior in response to periods of low marginal electricity costs, whether due to abundant renewable generation or unexpectedly low demand. We conducted the natural field experiment across the UK, covering approximately 60% of the country's public charging network. We reduced 18% of these chargers' prices during dynamic pricing events. Importantly, participants were unaware of the experiment, eliminating concerns about selection bias or experimenter demand effects. Our data allow us to observe a substantial share of EV charging behavior across the country, providing a uniquely comprehensive view of how public charging demand responds to dynamic pricing.

The nationwide field experiment was administered in collaboration with Octopus Electroverse, a public EV charging platform. The Electroverse app displays a map of public EV chargers across the United Kingdom, centered by default on the user's geolocation and showing an area of approximately 1 square kilometer. For each charger, the app provides real-time, accurate information on location, price per kilowatt-hour (kWh),

[3] For instance, Quirós-Tortós et al. (2015); Motoaki and Shirk (2017); Sheldon et al. (2019); Lee et al. (2020); Nehiba (2024); Weekx et al. (2025).

and station congestion (as price change was unrelated to congestion, our effects were not driven by price as a signal for charger abundance).[4] Access to full and accurate information is critical for realizing the benefits of the public EV charging network (Asensio et al., 2025).

The Electroverse platform enabled us to adjust public charging prices as part of the experimental design. We reduced prices during specific periods when the marginal cost of electricity was low, designed to incentivize EV charging at times of high low-carbon electricity supply and to support grid balancing. These lower prices were made possible by procuring wholesale electricity at lower costs and passing the savings on to consumers. The app notified users in advance - usually day-ahead - of these dynamic pricing events at participating chargers, which generally lasted for two hours.

We analyzed the charging behavior over an 11-week period of all UK EV drivers who had an Electroverse account and push notifications enabled, encompassing 109,711 eligible drivers, approximately 5% of the UK EV market.[5] Using an incomplete crossover experimental design, each customer was exposed to three out of four randomly assigned experimental conditions at different points in time: a 40% price reduction; a 15% price reduction; a carbon information message encouraging charging when "the grid is green" (with no price reduction); and a control condition (with no price change or informational message). By randomly lowering prices to better reflect marginal costs, and by independently varying carbon messaging, we were able to causally estimate the impacts of price incentives and non-price signals on public EV charging demand.

1.1 Primary findings

We found that demand for public EV charging through the Electroverse platform was highly responsive to price, with point elasticities ranging from –2 to –7 depending on the exact sample (though always reflecting short-run responses to temporary price events on the Electroverse network), suggesting substantial short-run flexibility in how and when consumers choose to refuel EVs. These estimates are among the highest elasticities observed in electricity markets and carry significant implications for welfare analysis,

[4]The app is free to any EV consumer and has many product features: (i) interactive map and filters to easily locate chargers by filtering based on connector types, charging speeds, availability (congestion), and network providers; (ii) plan trips with charging stops tailored to the EV's battery capacity and connector type; and (iii) the ability to initiate and monitor charging sessions directly from the consumer's phone, eliminating the need to interact with the charger physically.

[5]We assume 2.2 million EVs, including plug-in hybrids, in the UK (Society of Motor Manufacturers and Traders, 2025).

marginal cost pricing, and infrastructure investment. We begin by presenting several descriptive insights into charging behavior, followed by four main results from our natural field experiment.

Given our EV charger data, we estimated the first load duration curve for public EV charging across a country. We found that at peak, only 25% of the network was being used, and for more than 50% of the time, less than 10% of the EV charging network was being used. In addition, congestion at the location level was rare during our trial (see Figure 4). This demonstrates that during this experiment, we were not facing congestion due to supply-side constraints. We also documented the following baseline patterns: (i) most users are infrequent public charger users, likely because they rely primarily on home charging; (ii) among frequent users, charging typically occurs close to home, either at locations within 1 kilometer or at nearby fast chargers within a 5-kilometer radius; (iii) a substantial share of charging occurs far from home, likely reflecting usage during longer-distance travel; and (iv) weekend charging is disproportionately associated with chargers located farther away, suggesting more out-of-home travel on weekends. Since representative data on EV charging behavior is often scarce, these descriptive findings offer a valuable foundation for understanding the demand characteristics of our study sample, though we acknowledge that our findings are representative only of *today's* public EV charging customers in the UK.

Directly from the field experiment, first, we found very large demand responsiveness to the price. A 40% reduction in the charging price at the subset of participating chargers caused a 117% increase in demand across the network of charging points within the app, while a 15% price reduction led to a 30% increase. We estimated that equates to point price elasticities of demand – for charging through the Electroverse platform – of -2.9 and -2 respectively. These price elasticities of demand rise to -3.8 and -7.1 when we focus on consumption impacts at charging points that reduced prices during the events rather than the whole Electroverse network – noting that only approximately one fifth of chargers participated in the dynamic pricing events. The effects were consistent and persistent across the whole time period. Conversely, non-price messages to charge when the grid was "green" had no impact on charging demand.

Second, we found some evidence of substitution from other charging apps as well as possible temporal displacement of demand.[6] Approximately half of the observed treat-

[6]This distinction is possible due to the high-resolution data we collected, which includes public charging activity across both participating and non-participating chargers, as well as total electricity consumption at participating chargers, regardless of the app used. Additionally, for a subset of customers, we obtained half-hourly household electricity

ment effect was attributable to users shifting from other apps to using Electroverse (but accessing the same chargers). For grid operators, the relevant metric is the net increase in demand, regardless of app or platform, as this affects system stress and renewable integration. Accounting for our estimate that roughly half of the observed increase comes from app switching, we halve the headline price elasticities when assessing net demand changes during event hours: i.e., price elasticities from a 15% and 40% price reduction of -1 and -1.45, respectively. It is not clear to what extent the net new demand during event hours was demand creation or displacement. We find some evidence of temporal displacement within the public charging network or in-home charging behavior, but the confidence intervals around these estimates overlap 0.[7] Shifting of demand across time or between public and private charging is likely diffuse, making it challenging to measure precisely. Moreover, we acknowledge that there may be displacement from sources we did not observe, such as office chargers.

Third, we found variation in treatment effects consistent with economic theory. First, we observed heterogeneity by income: customers living in lower-income areas exhibited more elastic demand.[8] We also found substantial differences by vehicle type: large-battery EVs (over 77 kWh) show far greater increases in charging demand, as one would expect, which resulted in a price elasticity of -4.86 for these consumers, compared to -1.77 among those in the smallest battery quartile (14–51 kWh). In addition, we observed heterogeneity by situational context. Price elasticity was higher on weekends than weekdays, and varied by time of day: mornings showed the largest absolute increases in demand, while evenings showed the largest relative increases, perhaps consistent with when people's value of time is lower (Goldszmidt et al., 2020). Finally, we found that proximity to public chargers is also an important moderator of responsiveness – as distance to the nearest charger increases and charging presumably becomes more costly (in terms of time and battery energy), responsiveness declines.

Fourth, we found that the experimental price reduction encouraged consumers to try new public chargers. Overall, 25% of charging sessions took place at a charger from a new brand and 18% at a new charger from a brand previously used, implying that 59% of new chargers were also from new brands. However, this demand change did not persist;

consumption data.

[7]During the study period, home charging remained cheaper than public charging, although public charging is much faster than home charging (so opportunity cost of time should also be included here). Outside of price-decrease hours, public charging rates ranged from £0.50 to £0.90 per kWh. In contrast, customers on fixed-rate home tariffs paid £0.25–£0.27 per kWh, while those on a managed charging tariff paid just £0.07 per kWh during off-peak periods.

[8]For customers in local authorities with average disposable income below £19,000 per year, the price elasticity of demand in response to the 40% price increase is -4.32. All other quintiles show weaker responses.

drivers were no more likely to return to the new charger after the experiment ended than the control group. This pattern suggests that consumers were already near-optimized in their charging behavior, despite briefly responding to the temporary price incentive. This stands in contrast to some earlier findings that emphasize suboptimal travel or location choices (Larcom et al., 2017).

Fifth, we found that reducing EV public charging prices closer to the marginal cost has economic welfare benefits.[9] Given the large random price change, we were able to estimate the consumer surplus of EV public charging. At existing market prices, we found that for every £1 spent on EV charging, there was a consumer surplus transfer of £0.53. However, we also estimated the total consumer surplus change of moving to the wholesale marginal cost from the current market price for charging. At this point, we found that for every £1 spent on EV charging, there was a consumer surplus transfer of £5.63.

1.2 Contribution to the existing literature

Our natural field experiment contributes to the growing literature in energy economics on demand flexibility, price elasticity of demand, and EV charging behavior. In particular, we built on three recent important and innovative studies — Bailey et al. (2023), Garg et al. (2024), and La Nauze et al. (2024) — that examine how price variation affects EV charging. These studies focused on home charging (Bailey et al., 2023; La Nauze et al., 2024) and workplace charging (Garg et al., 2024), and consistently found that consumers respond to lower prices. Estimated elasticities ranged from –0.4 for Australian Tesla owners receiving 50–80% price reductions during peak hours (La Nauze et al., 2024), to –0.44 for UCSD employees offered a 50% discount on workplace charging (Garg et al., 2024), to –1.6 for Calgary customers responding to a 23% overnight price reduction (Bailey et al., 2023). Importantly, the price responsiveness we observed for public charging occurs in a different behavioral and socioeconomic context. Households less likely to have access to off-street parking for EV chargers, such as multiple-dwelling properties (Azarova et al., 2020) tend to correlate with lower income and property value (Chester et al., 2015; Zhang and Fan, 2025) - these public charging users, with more constrained access to charging infrastructure, may therefore respond more strongly to price incentives.

Our study differed from these studies in four key ways. First, we examined the price

[9]This result has been noted in other energy markets, see Joskow and Wolfram (2012); Hahn and Metcalfe (2021); Borenstein and Bushnell (2022); Hinchberger et al. (2024); Imelda et al. (2024).

elasticity of demand for many public chargers across the whole country, and merged in additional data that also allowed us to capture substitution between public and home charging home charging. Second, our experiment was a large-scale natural field experiment with no additional self-selection into the trial aside from having a notification enabled Electroverse account in the UK, providing excellent external validity for our estimates among today's users of public EV charging (Harrison and List, 2004), whereas Bailey et al. (2023), Garg et al. (2024), and La Nauze et al. (2024) are smaller framed field experiments with voluntary participation. Our partnership with a major public charging app enabled us to access granular charging and home consumption data without requiring selection (i.e., opt-in). Third, participants in our study were unaware they were part of an experiment, avoiding potential experimenter demand effects and Hawthorne effects that can arise in opt-in settings (Levitt and List, 2009). Fourth, our within-subject randomization across approximately 110,000 drivers allowed us to estimate heterogeneous treatment effects with greater statistical power, in contrast to the between-subject designs and smaller samples in prior work. Taken together, we believe our study meaningfully extends this important literature.[10,11]

We also related to the literature on new peak energy demand. The strong responsiveness to reductions in price has implications for a static time-of-use (ToU) tariff system: Bailey et al. (2024) showed that EV charging that was optimized by ToU could cause new peaks that might offset some of the benefits of such a system (see also Powell et al. (2022); Li and Jenn (2024)); this effect might not appear with a more dynamic pricing tool like the one used in our study.[12]

Furthermore, we contributed to the growing literature using large and novel datasets to better understand what influenced a consumer's driving and refueling behavior. For instance, Burlig et al. (2021) showed that EVs are driven less than the average internal combustion engine (ICE) vehicle, suggesting that EVs may serve as additional vehicles rather than direct substitutes.[13] Similarly, Dorsey et al. (2025) examined the value of EV charging demand over gasoline demand using multiple datasets; they estimated that

[10] Like Bailey et al. (2023), we found no effect of environmental messaging on charging behavior. Our null result was even stronger than that in Ito et al. (2018), who finds initial but diminishing effects, and contrasted with the positive effects reported in Garg et al. (2024). The absence of effects in our study may stem from the lack of self-selection and the broader population included.

[11] Our research is also related to studies on static time-of-use pricing, which have found significant shifts in home EV charging to off-peak hours (Burkhardt et al., 2023).

[12] Given that the EV dynamic charging prices are adjusting to the marginal cost of generation and where the marginal generator would likely be natural gas or renewables, there might be some attractive properties to such pricing, as opposed to coal (Gillingham et al., 2025).

[13] There is also a rich survey literature on the charging preferences of EV drivers (Carley et al., 2019; Forsythe et al., 2023).

people with EVs have annual refueling time costs of $7,763 (compared to $675 for ICE drivers), and ICE drivers make refueling decisions primarily driven by a station's *long-run* - rather than *current* - fuel price. Interestingly, our price elasticities of demand for charging on the Electroverse platform (-2 to -2.9) were larger than the gas price elasticities in the literature (e.g., -0.3 to -0.4 in Coglianese et al. (2017); Knittel and Tanaka (2021); Levin et al. (2017)). There could be many reasons for this (e.g., day ahead notice, short-term nature, etc.), but it does suggest that there are large social economic benefits of aligning charging prices with marginal cost. A further distinction is that most gasoline studies estimate elasticities using lower-frequency, more aggregated data, whereas our setting exploits daily, high-frequency data.[14]

Our findings are also relevant to literature arguing for the importance of supply-side factors in EV adoption rates and the overall EV policy debate (Li et al., 2017; Zhou and Li, 2018; Meunier and Ponssard, 2020; Springel, 2021; Remmy et al., 2022; Cole et al., 2023; Heid et al., 2024). These supply side factors depend in part on consumers' responsiveness to charging prices, which our study suggests is substantial. This relates, for example, to understanding the elasticity of investment in charging infrastructure, where demand and supply dynamics have created a "chicken-and-egg" problem for policymakers: should they prioritize subsidizing EV adoption or the deployment of EV charging networks (Cole et al., 2023)?[15] Our price elasticities can be used to help understand this question and are applicable to the related IO literature on induced new demand versus charger switching demand and charger competition.

Finally, our results have implications for policy responses to local charging costs.[16] Electricity in charging stations is generally more expensive than in residential settings because of additional costs to suppliers, such as those associated with running and maintaining charging infrastructure (Office for Zero Emission Vehicles, 2024).[17] Policymakers are exploring ways to reduce these costs, with a view to encouraging EV adoption at scale, but also to address equity concerns. It is notable that lower-income neighborhoods were

[14]The use of daily high-frequency data in our setting is fundamental to be able to measure such short-term price elasticities of fuel demand (Hughes et al., 2008; Levin et al., 2017).

[15]In the United States, subsidies have supported both EV purchases and charging infrastructure development, particularly under the Infrastructure Investment and Jobs Act (IIJA) and the Inflation Reduction Act (IRA), with a focus on benefiting low- and middle-income households. The United Kingdom has pursued the same goal through a combination of financial incentives, regulation (namely a mandate on new car sales), and charging infrastructure grants (the Local EV Infrastructure Fund).

[16]Our research is in similar spirit to Holland et al. (2016), whereas we are focusing on local charging costs and the local externalities it can create, rather than the direct health costs from local EV charging.

[17]We acknowledge however that the speed of the kWh going into the EV in public chargers is usually a lot quicker than for at-home chargers. Therefore, in terms of cost, once we include people's value of time, it is unclear whether public is more costly than at-home chargers.

the ones with the larger elasticities in our field experiment. While currently the vast majority of EV drivers in the UK (93%) have access to home charging (Britain Thinks, 2022), consumers may become more reliant on public charging as EV diffusion moves from "early adopter" to "early majority" (Rogers, 2003).[18] By 2040, it is expected that most people would be using public charging to some extent, including to supplement home or workplace charging (International Energy Agency, 2024*b*). Therefore, our estimated price elasticities of demand are important for understanding the benefits of such public infrastructure investment.

The remainder of the article is structured as follows. First, we describe the study design, including details of the experimental manipulations, the timing of the dynamic pricing events, the incomplete crossover randomization approach, and the data and analytical methods used. Next, we present key descriptive statistics. Third, we present the full set of results, covering treatment effects and tests for demand displacement. We then examine welfare impacts before concluding with a discussion of the results' potential implications for energy and transport policy.

2 Experimental design

In this section, we present the sample selection in the experiment (Section 2.1), the design of the treatments (Section 2.2), our use of crossover randomization (Section 2.3), the scheduling of the treatment (Section 2.4), and the availability of data before and during the field experiment (Section 2.5).

2.1 Eligibility into the field experiment

We implemented our field trial using eligible customers of the Electroverse app, a payment platform with Europe's largest consumer EV charging network. Electroverse functions as a payment platform that connects users to charging point operators (CPOs), rather than operating its own chargers.[19] In the UK, the app covers approximately 60%

[18]Early EV adopters are higher-income households (Borenstein and Davis, 2016; Chakraborty et al., 2019; Davis, 2019; Gillingham et al., 2023), and are thus more likely to have a garage and driveway for home charging than middle- and lower-income households, especially on multi-family dwelling.

[19]Most chargers listed on Electroverse can also be accessed directly through other apps or payment methods provided by each CPO. This means that a driver can charge at a participating Electroverse station without using the Electroverse app, although in our trial only sessions initiated via Electroverse were eligible for the experimental price reductions. Conversely, Electroverse users can also access non-participating CPOs through the app, although those chargers did not offer dynamic pricing during the experiment.

of the public charging network, linking users to more than 65,000 charging points[20] from approximately 100 charging point operators (CPOs). A subset of CPOs on the network is supplied by Octopus Energy Limited. Octopus Energy Limited and Electroverse, both part of Octopus Energy Group, make direct procurement savings at these charging points by encouraging customers to charge at times when wholesale electricity prices are lower. Seven of *these* CPOs participated in the dynamic pricing events, comprising 18% of Electroverse charging points (11% of the UK public network): Be.EV (770 charging points), Blink Charging (3,607), GeniePoint (1,675), IONITY (394), InstaVolt (1,372), Osprey (2,549), and Raw Charging (1,569). All remaining chargers did not offer the dynamic pricing intervention. The set of participating chargers remained the same across all events; there was no variation in which operators offered discounts from event to event. Among participating chargers, only 16% were slow chargers (under 7kW), 37% were fast chargers (under 50kW), 38% were rapid (under 150kW) and 9% were ultra fast chargers (from 150kW to 500kW) as shown in Table A1. We also show how much of the additional consumption is drawn from each kind of chargers in Figure A10.

Customers were deemed eligible for the field trial if, at the time of randomization on 16/12/2024, they met the following criteria: i) they had an active Electroverse account; ii) their device language was set to English; iii) they had enabled push notifications in the app; and iv) they had not opted out of receiving "Plunge Pricing" notifications (the marketing term for the price decreases). A total of 109,647 customers met these criteria at the beginning of the study; 64 new customers enrolled during the trial and were automatically assigned to a treatment group, with a final sample of 109,711 customers.[21] Our analysis was restricted to chargers within Great Britain, where the price decreases applied.[22]

2.2 Intervention design

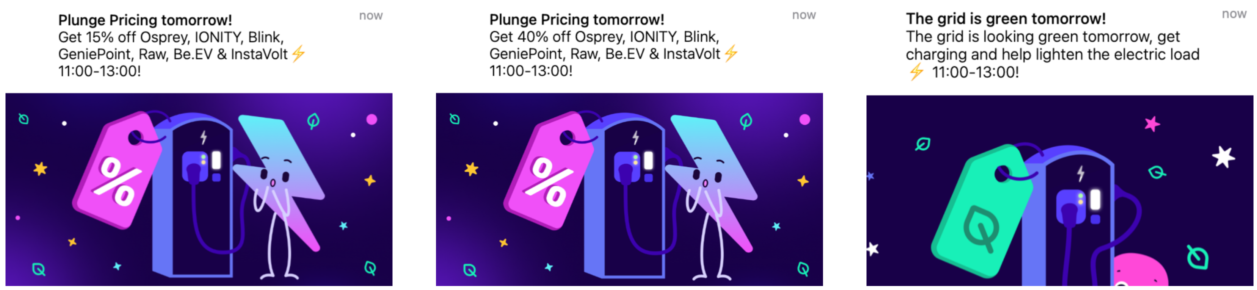
Before our study, Electroverse occasionally offered dynamic price reductions, which

[20]Note that a specific charging location can have multiple physical charging devices, and a device can have multiple pieces of Electric Vehicle Supply Equipment (EVSE). When we refer to a charging point or charger throughout the rest of the report, we are referring to an EVSE.

[21]Our pre-analysis plan had dictated excluding new customers, fleet users, and super users (a customer marker defined by Electroverse), but they were automatically randomized following the same process as existing customers (see Section 2.3), which we had not realized *in advance* would occur. For this reason, we have *included* these 330 customers in our analyses. We show that our results are almost unchanged in Table A38.

[22]Electroverse also operates in other countries, and British customers in our sample could use charging points outside the UK during the life of the trial, without receiving a price decrease. These transactions were excluded from our analysis.

Figure 1: Visual representation of the three notifications used in the experiment



(a) 15% price decrease

(b) 40% price decrease

(c) Green message

Notes: Customers in our sample were exposed to one of four trial arms: a control treatment or one of three push-notification-based treatments. These notifications were displayed on participants' phones. On the example event day shown, 11:00–13:00 was a period of relatively low wholesale energy cost and CO_2e intensity, though event timings varied across the study (Table A2 shows the dates, times, and durations of all events).

varied across events and CPOs. Importantly, these discounts were not randomized across customers within the same event, meaning our experiment is the first to measure causal effects at the individual level. Our experiment introduced this within-event randomization to identify causal effects. In the Electroverse UK network, only around 26% of stations apply time-of-use (ToU) pricing, and among participating CPOs, just 12% of charging stations do so, typically through a daytime price increase. This lower prevalence among participating charging points reflects the fact that about 88% of charge points with ToU tariffs are slow chargers, most of which are not part of the trial. The corresponding daily price profiles are shown in Figure A20. Because the randomization occurred at the user level, and most trial events took place at chargers without ToU pricing, we are confident that the estimated effects primarily capture the impact of our experimental intervention, rather than existing price variation across the day.

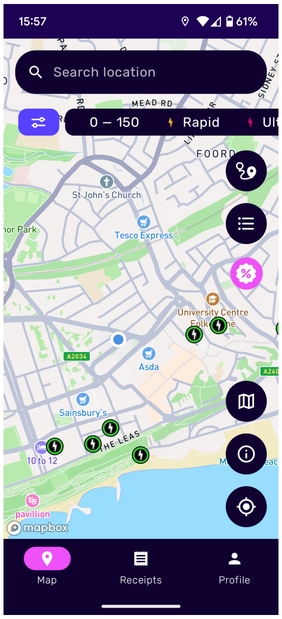
The customers in our sample were exposed to one of four trial arms: a control treatment or one of three push-notification-based treatments (as shown in Figure 1), with the in-app experience shown in Figure 2.[23,24]

Control group: Customers in the control group did not receive any push notifications about events' price decreases.

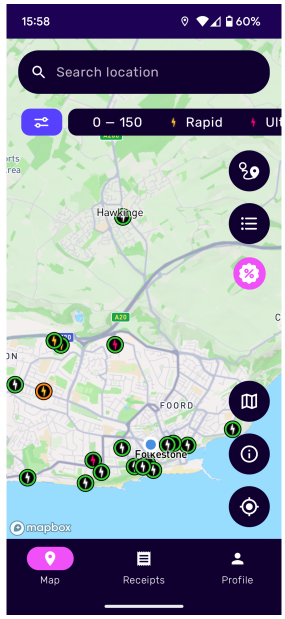
[23] In the framework of Athey (2025), this field experiment places us in the economist as market designer category.

[24] The default map setting is Zoom Level 16, roughly the level at which individual buildings are visible. (For more information on zoom levels, see https://docs.mapbox.com/help/glossary/zoom-level/.) However, the user can zoom in and out of the map and can search across the whole country. The users can also use the app to determine the compatibility of charging station to their vehicle, which is relatively homogeneous (Li (2019) shows the importance of such standards for welfare). Interestingly, the user can see location, price, speed and type of charger, and available chargers. This may mean more diversity in location of chargers compared to traditional gasoline markets, where stations tend to be concentrated on major commuting paths (Houde, 2012).

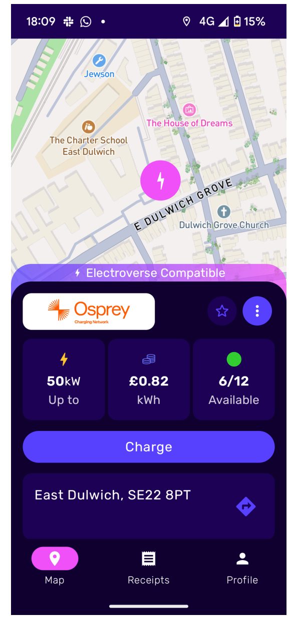
Figure 2: The Electroverse app under different pricing conditions



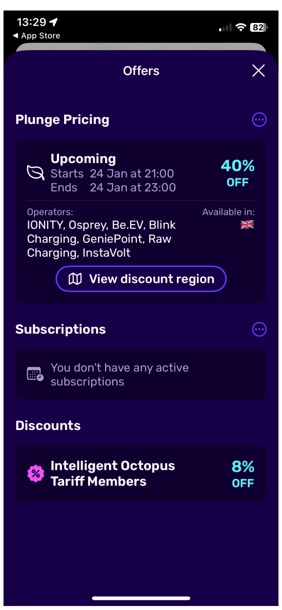
(a) No event – map view



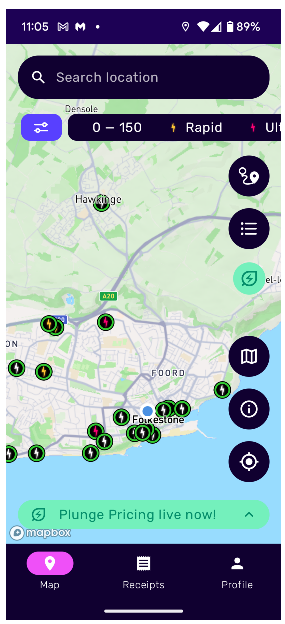
(b) No event – zoomed out map view



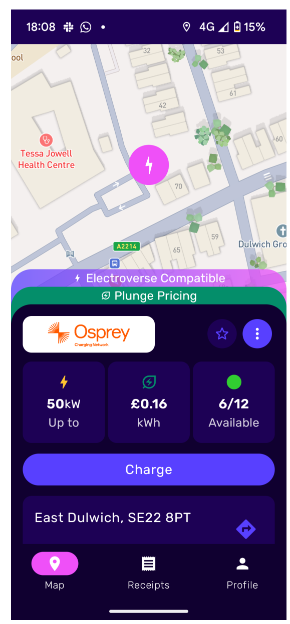
(c) No event – charger view



(d) Upcoming event – offers view



(e) Event – zoomed out map view



(f) Event – charger view

Notes: Panels (a), (b), and (c) show the Electroverse app under normal pricing conditions: (a) is the standard map view, (b) is a zoomed-out map view, and (c) is a charger-specific view (for an Osprey charger). Panel (d) shows the "Offers" tab when a dynamic pricing event is upcoming (note that the map is still the default screen, even in the lead-up to dynamic pricing events). Panels (e) and (f) show the map and charger views, respectively, during an active event. Discounted chargers are not flagged visually, but users can filter for participating CPOs such as Osprey.

15% price decrease group: Participants in this treatment received a push notification about a low price decrease, 15%, at selected charging stations. The message[25] read: *Plunge Pricing tomorrow! Get 15% off [operators names] [time slot]!* Participants could then locate eligible charging stations on the map in the app. Customers were entitled to the price decrease if they started charging during the specified time window, even if the session ended after the window closed. Since electricity unit rates varied across operators and stations, we used a fixed proportional price decrease to keep the message simple and clear.

40% price decrease group: This group received a push notification offering a high price decrease, 40%, at selected charging stations. The message was similar in structure: *Plunge Pricing tomorrow! Get 40% off [operators names] [time slot]!* The price decrease conditions were identical to those in the low price decrease group, with users able to locate eligible stations in the app and receive the price decrease for any charging session initiated during the event window.

Green message group: Customers in this treatment received a push notification highlighting the low-carbon intensity of electricity during dynamic pricing events – i.e., when the grid was "green" – but without any mention of price or price decreases. The message read: *The grid is green! The grid is looking green tomorrow, get charging and help lighten the electric load [time slot].* The message was designed to promote a pro-environmental goal by informing consumers when electricity was greenest. This treatment allows us to compare the effectiveness of pro-environmental messages and price incentives. It also allows us to understand the impact of incentives, holding fixed the receipt of a notification.

We have three main hypotheses in our field experiment. Our first null hypothesis is that reducing the charging price by 40% has no impact on charging demand; our second null hypothesis is that the 15% price decrease has no impact on demand; our third null hypothesis is that a green message will not affect charging demand. These three hypotheses belong to the same family so we will adjust for multiple hypothesis testing in our analysis.

[25] On the first event, the message read "today" as the event was later that day; this is true for the 40% and green message notifications for the first event, too.

2.3 Crossover randomization

Participants were randomized to one of four sequences. Each sequence consisted of three blocks of events, with varying numbers of treatment events in each block. Note that we did *not* randomize participants into new treatments after each event to reduce implementation complexity and avoid potential randomization errors. Sequence assignment was based on Electroverse's internal user IDs, which are unique, positive integers assigned sequentially according to the customer's join date.[26] This approach ensured balanced assignment across the user base, while preserving randomness with respect to the time when a customer joined the Electroverse platform. As shown in Table 1, we ran the trial for only three periods, leading to a four-treatment, three-block (4 x 3) incomplete crossover design (see e.g., Jemielita et al. (2019)), where blocks also had varying lengths (in terms of number of events). For example, a participant in Sequence 1 experienced eight 40% price decrease events in Block 1, six 15% decrease events in Block 2, and four control events in Block 3.

Table 1: Experimental design

	Sequence 1	Sequence 2	Sequence 3	Sequence 4	Events
Block 1	40% decrease	15% decrease	Control	Green Message	8
Block 2	15% decrease	Control	Green Message	40% decrease	6
Block 3	Control	Green Message	40% decrease	15% decrease	4
Participants	26,805	27,448	27,685	27,773	

Notes: Total number of participants at the beginning of our study: 109,647. During the trial, 64 new customers enrolled and were automatically assigned to a treatment group, with a final sample of 109,711 customers. We randomized customers into one of four sequences. Our experiment followed an incomplete Latin square design with one-week wash-out periods between three blocks of events.

The use of within-customer randomization (i.e., a crossover design) offered several advantages in our experimental setting. It increased statistical power by observing the same participant under multiple treatments and removing time-invariant differences across participants. It also allowed estimation of heterogeneous effects. And, it mirrored the dynamic pricing environments customers face in practice, enhancing external validity.

Two main concerns with within-customer randomization are carryover effects, where one treatment affects subsequent behavior, and order effects, where responses depend on treatment order. We mitigated both by including one-week washout periods (List, 2025).

[26] We randomized customers to one of four sequences using Python's modulo function, assigning each customer based on the remainder of their user ID divided by 4 (i.e., 0, 1, 2, or 3), implemented as `user_id % 4` in Electroverse's data environment.

Tests for carryover and order effects (see Section 4.2.1) revealed no evidence of either, suggesting the washout design was effective. A further potential concern is spillovers due to information sharing between participants, such as through social media, that could influence behavior in other groups. We monitored social media and Electroverse customer forums during the trial and did identify posts indicating some knowledge of differing price reductions between customers – ultimately, we found only approximately 10 such posts, which suggests that these spillovers were minimal and unlikely to bias our estimates.

2.3.1 Unbiasedness of the treatment effect due to randomization

Our randomization design ensures strong internal validity by satisfying both the assignment mechanism and exclusion restrictions necessary for identifying an unbiased treatment effect.

First, the three assignment mechanism conditions were met: (i) non-zero probability, as every Electroverse customer had a chance of assignment to any of the four experimental sequences; (ii) individualism—treatment assignment for one customer was independent of others' assignments and outcomes; and (iii) unconfoundedness, as treatment was assigned independently of potential outcomes.

Second, we satisfied four exclusion restrictions. (i) SUTVA holds: a customer's potential outcomes did not depend on the treatment assigned to others, and there was no variation in treatment form.[27] (ii) Observability was ensured: treatment was assigned only to observed customers, and while post-assignment attrition was possible, we tested this and found no evidence that treatment affects outcome observability. (iii) Complete compliance was guaranteed: all treated customers received the assigned message. With these conditions satisfied, (iv) we also had statistical independence, ensuring that treatment assignment was independent of potential outcomes. This validated our use of randomization to recover an internally unbiased estimate of the impact of charging prices on charging demand.

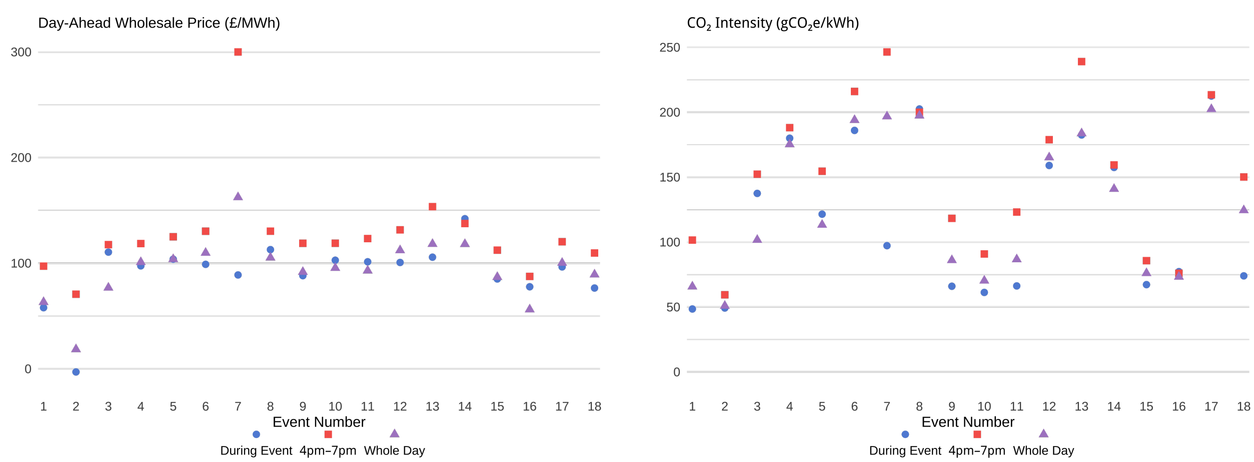
[27] The charging network was uncongested (see the load duration curve in Section 3.1), so treated users did not affect charger availability or outcomes for others. Were the network congested, treated users might have crowded out other users, biasing estimates upward. However, even if the charging network was crowded, someone in the treated group who charged in the incentive time period would have crowded out all other users equally, so the SUTVA violation effects would be small or negligible. See Goldszmidt et al. (2020) for a discussion of these issues in a two-sided platform randomization.

2.4 Scheduling of dynamic pricing events

As part of our field experiment, we held 18 dynamic pricing events between December 2024 and March 2025, which are reported in Table A2. Participants who were in one of the two price decrease groups were eligible if they *started* charging in the time window of the dynamic pricing events listed in Table A2, irrespective of the length of the charge. All events lasted two hours, except for events on December 28, 2024 (three hours) and January 02, 2025 (one hour). Dynamic pricing events were not scheduled to happen at a predetermined time interval; rather, Electroverse identified events within a given week to correspond to times when the price of wholesale electricity was predicted to be low with high probability. Figure 3 confirms that events tended to occur during hours with lower day-ahead prices (£/MWh) and CO_2e intensity (gCO_2e/kWh) compared to day as a whole, and especially to the 4pm-7pm period.

Averaged across all three treatment groups and all events, the notification was sent around 18 hours before the dynamic pricing event.

Figure 3: Day-Ahead prices (left) and CO_2e intensity (right)



Notes: These figures show average day-ahead prices (£/MWh) and CO_2e intensity (gCO_2e/kWh) during event hours (blue circle), 4pm-7pm (red square), and the across the whole day (purple triangle) for each day that had a dynamic pricing event. Events tended to occur during hours with lower day-ahead prices (£/MWh) and CO_2e intensity (gCO_2e/kWh) compared to day as a whole, and especially to the 4pm-7pm period. (4pm-7pm is traditionally considered the most supply-constrained "peak" hours in Great Britain.) We retrieved carbon intensity data from Great Britain's National Energy System Operator (NESO), using https://carbonintensity.org.uk/.

The wholesale cost of energy (and associated CO_2e intensity) varies widely across days and hours, with swings of £50 to £300 per MWh (£0.05 to £0.30 per kWh) being common. While the 15% and 40% price reductions (£0.10 and £0.28, respectively, from the typical £0.70/kWh marginal charging cost) do not exactly reflect the social marginal cost, they roughly correspond to the decreases in social marginal cost that occur during specific hours of the year.

2.5 Data

Our analysis used observed charging behavior via the Electroverse app. We focused on two main behavioral outcomes: i) whether a customer initiated a charge during the specified time window of the event; and ii) the volume of electricity consumed during charges, measured in kWh. As noted in Section 2.1, 18% of Electroverse charging points had price decreases during dynamic pricing events; the remaining points did not participate in the price changes. Importantly, we collected data on all charging activity through the app, regardless of whether a station received a price change. In other words, non-participating chargers are not a control group; they simply did not receive the experimental treatment, but we still observe customer behavior at those locations. This distinction arises naturally from the commercial arrangements between CPOs, Electroverse, and Octopus Energy Limited; it also enables an interesting heterogeneity analysis, discussed in Section 4.3.1, where we examine whether treated customers shifted from non-participating to participating chargers.

The dataset also included a range of individual-level variables. In particular, we observed pre-trial electricity consumption in September, October, and November 2024, and charging behavior (kWh charged, GBP spent, number of receipts, number of different charging stations visited) between January 1st and November 30th 2024. For the 89,202 participants (80%) who indicated the model of their EV, we included the usable battery capacity of the vehicle (we used the largest battery for those who owned more than one EV). We also recorded the customer's home postcode at the time they joined the Electroverse app (full information was available for 77,595 customers, 70% of the sample); we used this information to link contextual variables such as average income in the customer's local authority (Office for National Statistics, 2024), whether their home postcode was classified as rural[28] (Office for National Statistics, 2025, from the 2011 census), and the number of public EV chargers in their local authority as of January 2025 (Department for Transport, 2025*a*).

In addition to these charging data from Electroverse, we also integrated two other key data sources. First, we observed electricity consumption at the household level for a subset of customers with linked home smart meter data. This enabled analysis of potential

[28]This classification includes farming communities, rural tenants, and aging rural dwellers (retirees who move to rural areas).

spillover effects between home and public charging behavior. Second, we had access to high-frequency electricity consumption data from the public EV chargers participating in the dynamic pricing events, which allowed us to compare user-level responses with aggregate demand patterns at the station level.

3 Descriptive statistics

3.1 Load Duration Curve

In this section, we discuss the Load Duration Curve (LDC) for Electroverse chargers. A LDC provides a ranked view of power demand across time on the Electroverse Network, which accounts for around 60% of public charging infrastructure in the UK. It shows how frequently demand nears peak capacity, and how often infrastructure is operating below its potential. This LDC is important to plot out as we would like to know, ex ante, whether the UK was close to a congested network, which might temper any treatment effects from the field experiment.

More precisely, we present an analysis of usage patterns across approximately 49,000 electric vehicle supply equipment (EVSE) points in the UK, covering the period from December 19, 2024 to March 3, 2025.[29] Drawing on availability data provided by CPOs, we constructed a LDC to estimate the network's active demand profile over the course of the trial in panel (a) of Figure 4. The resulting LDC reveals a minimum estimated load of 72 megawatts and a peak approaching 460 megawatts — a wide range that reflects the high variability in user demand and chargers type. However, it is clear that at maximum demand, only 25% of the network is being used. And for about 50% of the time, at most 10% of the charging network is being used.[30] The red dots on the figure are the MW during each price plunge events, which shows that the events are not triggered during peak congested times for the grid.

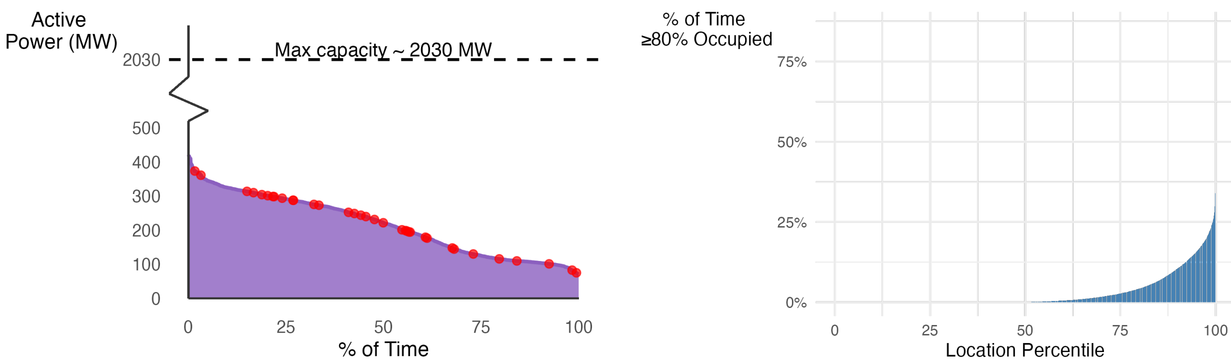
Panel (b) of Figure 4 shows the share of time that each charging location (a location can have several EVSEs) was occupied at or above 80% over the course of the trial. Chargers are ordered along the x-axis from least to most heavily used, and the y-axis shows the

[29]These 49,000 EVSEs represent the subset of the 65,000 EVSEs in our sample that were used at least once during the trial period. We exclude the others because they may have issues that prevented their use, which are not yet reflected in Electroverse's administrative data.

[30]In Figure A19, we show the same curve by charger speed. High speed chargers' LDC slopes are steeper, indicating more variability in peak demand and highlighting the potential for flexibility for high speed chargers.

proportion of time spent at high occupancy. The distribution is heavily right-skewed: approximately 99% of locations were occupied at 80% for less than 25% of the time, indicating that high congestion was rare at most sites. Only a small fraction of chargers saw sustained high utilization, while the vast majority are only briefly busy. We show the same share of time above 80% occupancy for non-participating and participating stations separately in Figure A1 and found an even lower share of congested stations for participating stations. Charging speed explains most of the differences are slow charging stations (mostly non-participating) are the most likely to be used overnight.

Figure 4: Utilization patterns across the Electroverse charging network



(a) Load duration curve (EV charging)

(b) Share of time each location was at or above 80% occupancy during the trial

Notes: Panel (a) presents the Load Duration Curve (LDC), showing the ranked estimated demand across all active chargers from December 19, 2024 to March 3, 2025. The red dots show MW during price plunge events. Panel (b) displays the share of time each charging location operated at or above 80% occupancy, sorted from least to most congested.

There are, however, important limitations to this analysis. In the absence of direct metering data for many devices, we relied on nameplate power ratings and observed availability to estimate load — a method that captures theoretical capacity rather than actual energy transferred. As a result, the estimates may overstate demand in cases where high-powered chargers are online but lightly used (e.g., a 150 kW charger delivering just 30 kW). This limitation is important to consider when interpreting the curve's upper and lower bounds. Nonetheless, the LDC provides meaningful insights into overall system usage, potential stress points, and opportunities for more intelligent demand-side management. We do not appear to be near any system-wide congestion at any point in time.

3.2 EV charging demand patterns and sample characteristics

We examined customers' charging behavior in the year prior to the trial's commencement and present here some stylized facts about public charging usage. Most EV owners appeared to have access to home chargers; public charging was often used occasionally, especially during weekends and holiday trips. The average distance between home locations (measured as the centroid of users' home postcodes) and charger locations increased sharply during public and school holidays (Figure A13), and to a lesser extent on weekends (Figure A14). However, public charging was also used locally by high-frequency users, either at slow charging stations located very close to home (under 600 m) or at fast chargers within 5 km of home.[31]

These use cases are reflected in the underlying composition of Electroverse users. Using a k-means clustering algorithm, we identified three distinct behavioral segments, which we grouped into three main user profiles.[32] The first consisted of users charging almost exclusively on slow chargers (84% of sessions on average). This group likely used public charging as a substitute for home charging. The second group included users with around 15 sessions annually, who charged at a median distance of 5 km and used fast chargers for 97% of their sessions. The proximity of the charger, type and frequency suggests that these users charged in "destination" chargers, typically found at places people visit, such as hotels, restaurants, or shopping centers. The third group comprised long-distance users — we included customers that only charged abroad and more occasional chargers in the UK (average 7 sessions per year), with median distances consistently exceeding 120 km from home. This group included weekend users (80% of sessions on weekends), holiday users (70% during holidays), and work-related users (80% on weekdays and less than 5% during holidays).

We present a density plot of users' median distance to chargers in Figure A15. Local slow-charging users were characterized by a distinct concentration of charging very close to home, with a sharp peak below 1 km. In contrast, long-distance users displayed a clearly separate distribution, with median distances typically well above 100 km. The overlap between local and long-distance users was minimal, indicating distinct usage

[31] Most on-street charging in residential areas offers slower charging speeds compared to fast charging stations, which are typically located near supermarkets, public parking, or other business hubs.

[32] We have data on pre-trial charging behavior for 54,547 out of 109,711 users in our sample. In order to unpack user differences, we classified users into three groups using a k-means clustering algorithm, based on four behavioral indicators: the median distance from home to charging points, the share of sessions at high-speed chargers, the share of sessions occurring on weekends, and the share of sessions during school or bank holidays. This method allowed us to reduce the dimensionality of the data and draw meaningful comparisons between groups.

patterns.

Our sample comprised a large user base and we saw no systematic differences in pre-trial charging behavior between the randomized treatment sequences.[33] Table A34 presents summary statistics for the full sample of 109,711 customers. In the 12 months prior to the trial (19 December 2023 to 18 December 2024), customers charged on average 121.9 kWh in five transactions and visited two different charging stations for a total cost of approximately £81. In the three months before the trial started, customers charged in public stations for around 13 kWh of electricity per month. On average, customers had been members of Electroverse for 16 months. A series of Kruskal–Wallis tests reveals no significant differences across these variables among the four sequences described in Table 1.

4 Results

This section presents the results of the experiment, examining how the different randomized interventions influenced consumer EV charging demand. The analysis follows an Intention-To-Treat (ITT) approach: participants are included for analysis in the sequence they were originally assigned to, regardless of whether they actively engaged in the trial. This method is particularly appropriate in our context: participants assigned to the control group never received notifications, making it impossible to determine a measure of engagement with the intervention, while participation in treatment groups was voluntary and therefore endogenous. More importantly, the charging behavior of consumers remains observable in the dataset even after they chose to drop out by disabling the notifications on their app; their exclusion would bias the results by omitting non-adherence to the intervention, when non-adherence itself can be influenced by the intervention.

4.1 Our empirical strategy

Our empirical strategy follows our pre-analysis plan (PAP) in our pre-registration of the field experiment (AEARCTR-0015061).[34] To formally estimate the effect of our interven-

[33]However, as discussed in Section 4.4, our treatment effect heterogeneity analyses found that local users were much more engaged than more occasional trip users.

[34]In the notes section for every figure and table, we specify whether the analysis was in the PAP or whether it is new analysis that deviates away from the PAP. We also list deviations in Appendix A3.

tions on consumer charging behavior, we employ regression analyses. In particular, our analyses focus on two primary outcomes: (1) the volume of electricity charged (in kWh), and (2) the likelihood that a customer charges at all during a dynamic pricing event. For both outcomes, we estimate an OLS regression of the form:

$$Y_{it} = \alpha + \beta T_{it} + \gamma' Z_i + \delta_t + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome for customer i during event t, T_{it} is a vector of treatment indicators for the Green message, 15% price decrease, and 40% price decrease (the Control group is the omitted category), Z_i is a vector of time-invariant consumer covariates (specifically: kWh charged in September 2024 (set to 0 if the customer was not yet a member), kWh charged in October 2024 (0 if not a member), kWh charged in November 2024 (0 if not a member); and a binary indicator for whether the customer charged at least once between January and November 2024). These variables are intended to capture persistent differences in baseline usage patterns across individuals, improving precision in our estimates by accounting for variation that is predictive of the outcome. While such controls may reduce residual variance and partially account for temporal dependence in behavior, they do not fully eliminate serial autocorrelation. Accordingly, we account for potential within-customer autocorrelation by clustering standard errors at the customer level; δ_t event fixed effects; and ε_{it} is the error term, clustered at the customer level. Note that t indexes the 18 discrete dynamic pricing events in our study; we do not use all hours in the data because including non-event hours as "controls" would conflate the treatment effect with potential temporal displacement, thereby biasing the estimates of the causal impact of the event-specific price changes.

The above specification captures average treatment effects, controlling for prior engagement with the platform and customer baseline consumption.

4.2 Main results

Our primary estimates assessed how electricity consumption responded to each treatment during dynamic pricing events. Figure 5 and Table A3 present our core pre-registered specification and results. Customers in the 40% price decrease group increased their consumption by an average of 0.0604 kWh during dynamic pricing events, a 117% increase over the control group. Those offered a 15% price decrease increased usage by

0.0154 kWh – a 30% increase over the control group, approximately a quarter of the effect of the larger price decrease. We estimated that these uplifts imply point price elasticities of demand of -2.9 and -2, respectively. However, it is important to emphasize that these uplifts and implied elasticities pertain only to charging through the Electroverse platform, as we do not observe customers' charging via other platforms. For this reason, the relative increases in public charging demand achieved by our treatments are uncertain, and so too are the implied price elasticities of demand for *any* public charging. The green message alone did not produce a statistically significant change in consumption.

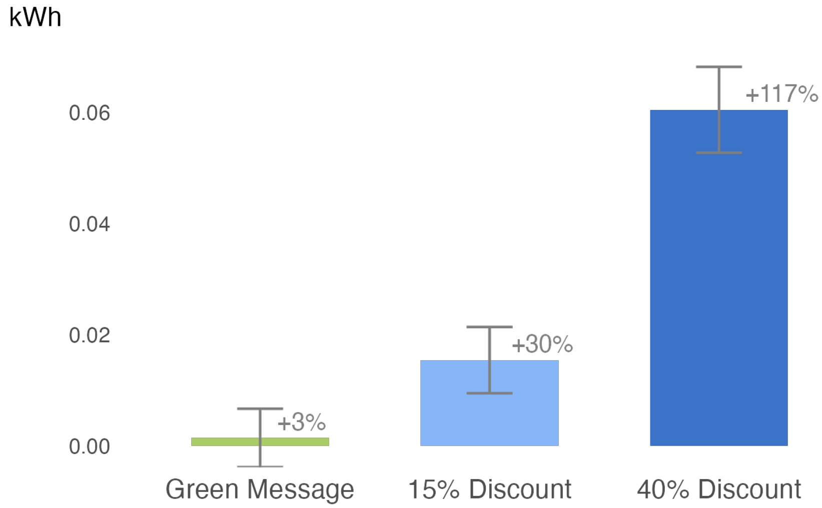
To account for the several outcomes and treatment arms, we applied two corrections to our p-values. First, we applied multiple hypothesis testing corrections to account for testing across two outcomes, kWh used and whether or not the user charged, and across the three treatment coefficients. Using the Romano-Wolf stepwise procedure to control the family-wise error rate (FWER), we found that the 15% and 40% discount treatments remained statistically significant at the 1% level ($p < 0.001$). We then applied the same approach — wild bootstrapping with stepwise multiple testing correction — to test the joint significance of the three treatment effects. Again, we found strong evidence of joint significance, with a corrected p-value less than 0.001.

Table A4 presents estimates of treatment effects under progressively richer specifications, allowing us to assess the stability of our results to the inclusion of potentially confounding covariates. Model (1) includes only the main treatment indicators. Model (2) implements the crossover estimator from Piantadosi (2005, p. 523), which accounts for block fixed effects and treatment-by-block interactions. Column (3) adds time-invariant consumer covariates from Table A3, as well as day fixed effects. Across models (1)–(3), the estimated effects of the three interventions remain robust.

4.2.1 Testing for carryover effects

Given our within-subject experimental design, we tested for potential carryover effects. To do this test, we measured daily consumption during the two washout periods - one between block 1 and block 2, and one between block 2 and block 3 - to determine whether consumers in different groups manifested different charging behavior. We assigned treatment on the basis of the experimental manipulations consumers were exposed to in the block that was just completed. Results in Table A33 show that during washout consumers charging behavior did not differ across the groups, with all posi-

Figure 5: Treatment effect (kWh charged per event)



Notes: Bars show treatment effects (kWh per dynamic pricing event) from Table A3, our primary analysis as specified in our pre-analysis plan. Error bars are 95% confidence intervals. Labels show the % change relative to the average consumption of 0.0517 kWh in the Control group. The 40% price decrease increased consumption by 117%, while the 15% price decrease led to a smaller but positive effect of 30%. The green message did not significantly affect usage.

tive but non-significant coefficients. Additionally, we compared consumption treatment group interactions with blocks in Tables A4 and A28; we found no significant interaction between treatment groups and blocks, an indication that the effects we observed and did not extend beyond the intervention period, providing complementary evidence against significant spillover effects. Finally, we examined how treatments affected whether customers unsubscribed from subsequent notifications, as well as whether notifications were delivered as intended; see Appendix A2.11. Overall opt-out rates were very low across all treatments.

4.3 Testing for substitution in energy consumption

This section explores the sources of increased electricity consumption during dynamic pricing events, as identified in Section 4.2. The results in Section 4.2 show a general increase in electricity consumption during event hours when a dynamic pricing event is in effect. This may originate from two distinct behavioral responses. First, the increase in kWh charged may reflect an overall increase in energy demand, as consumers drive more and require additional electricity due to lower charging costs. Second, the increase may stem from substitution of charging activity. This second mechanism includes substitution from non-discounted to discounted chargers; temporal substitution/displacement, where consumers delay or advance charging to align with the event, spatial substitution, such as users opting for public instead of home charging, or platform-based substitution (e.g., users charge via Electroverse rather than competing public charging apps). These forms of substitution involve a reallocation of when, where, or through which platform charging occurs rather than more total charging.[35]

Understanding the balance between net new charging and different sources of displacement is important from a system-level perspective. The objective of demand flexibility is to influence real electricity consumption to support system balancing. Temporal displacement of demand benefits the system, as does spatial displacement if demand is shifted to areas of high supply. That said, net new consumption can also contribute meaningfully to grid flexibility by absorbing surplus renewable generation or alleviating stress during periods of over-supply. However, if the observed increase in charging during events simply reflects shifts from alternative charge-point operators or payment platforms - rather than a genuine rise in electricity demand - then the intervention offers limited value for system balancing.

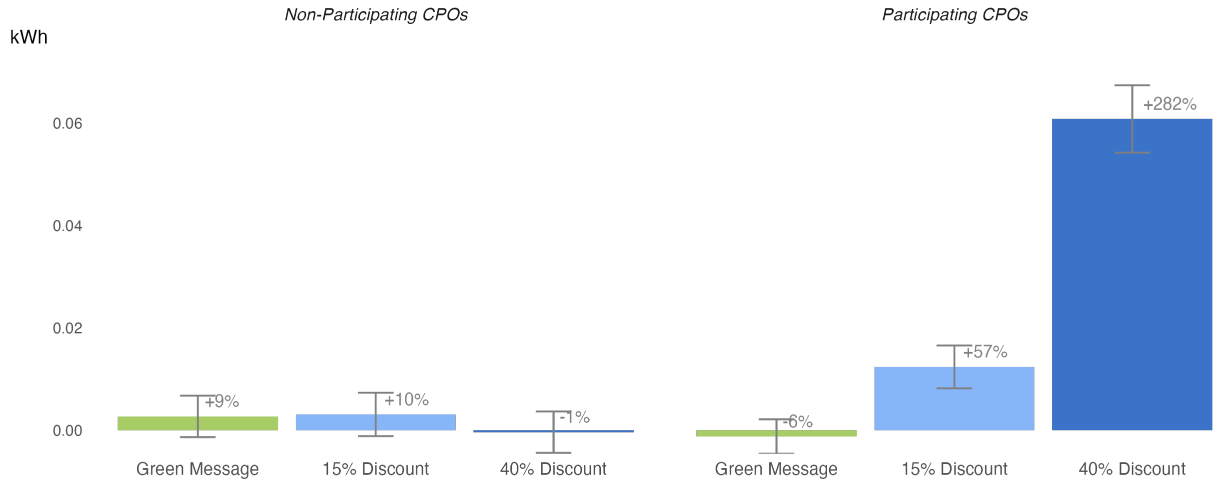
4.3.1 Substitution between charging point operators

First, we examined whether treatment effects differed depending on operator participation in the dynamic pricing events. The regression results presented in Section 4.2 referred to all chargers in the dataset. As noted in section Section 2.1, only seven operators were part of the dynamic pricing events, where these seven operators accounted for approximately 18% of the Electroverse charging network. As a result, many receipts that appeared in the previous regression were generated from charging points managed by CPOs that did not participate in dynamic pricing events. In this subsection, we analyzed treatment effects in participating vs non-participating charging points separately. Figure 6 and Table A28 break down the results from Table A3 by whether the operator participated in the dynamic pricing events.

We found no evidence of substitution from non-participating charging points to participating ones. The effect shown in Section 4.2 seemed to be concentrated at participating operators' stations – where a 40% price decrease increases consumption by 282%,

[35]In our setting, the good can be stored (as in battery charge), so there could be good reasons for intertemporal substitution Hendel and Nevo (2006, 2013). We differ from their approach since we randomize prices and allow brand preferences to be dynamic. And in our setting, intertemporal substitution can be welfare enhancing if the displacement demand comes from higher priced times with higher CO_2e intensity of the marginal generator.

Figure 6: Treatment effect (kWh), by whether the charging point operator (CPO) participated in dynamic pricing events.



Notes: Bars show treatment effects (kWh per dynamic pricing event) from Table A28, a spatial displacement check specified in our pre-registered analysis plan (as our second of three exploratory analyses). Error bars are 95% confidence intervals. Labels show the % change relative to Control group consumption. The effects were concentrated at charging points operated by CPOs that participated in the dynamic pricing events, where a 40% price decrease increased consumption by 282% and a 15% price decrease by 57% (from a control group average of 0.0215 kWh. No significant changes were observed at non-participating CPOs (control group average of 0.0302). The green message did not yield statistically significant effects in either case.

while a 15% price decrease increases the amount of kWh purchased by about 57% – whereas the price decreases had no effect on charging behavior in chargers operated by non-participating CPOs. The green message did not significantly influence behavior across either set of chargers.

In interpreting the treatment effect, the hypothetical "scaled-up" impact on demand – if all chargers in Electroverse's network participated in dynamic pricing events, rather than just the seven CPOs who did so during our trial – likely falls between two extremes: for the 40% price decrease, the 117% increase in demand across the entire network from Figure 5, versus the 282% increase when focusing only on the participating charging points in Figure 6. While this suggests a hypothetically far greater response than our headline increases, we caution against assuming this effect size would be replicated across the whole charging network because additional participating charges may, in part, cannibalize demand for each other. In other words, there may be a dilution effect where total customer response is limited by a finite amount of responsive users. [36]

This analysis focused on chargers *within* the Electroverse network (approximately

[36] In addition to these considerations, it is possible that current Electroverse customers may differ from future ones in their response to dynamice pricing.

60% of the public charging network). We could not assess switching from chargers outside the network, as we did not observe activity at those locations. However, we believe such switching was likely minimal, given that even within the network there was no evidence of displacement from non-participating to participating CPOs during events (as we have shown in this section).

4.3.2 App substitution

Although we did not find evidence of substitution from non-participating CPOs / chargers to participating ones (see Section 4.3.1 immediately above), customers may have changed how they paid for consumption at a given charger, switching from other payment methods to paying via Electroverse to take advantage of the price changes. Understanding this distinction is important for assessing the effectiveness of demand-side interventions from a grid operator's perspective, where only net new demand contributes to alleviating systems stress or supporting renewable integration.

Quantifying the extent of platform-switching is challenging, as our data only included charging sessions initiated via the Electroverse platform. However, all CPOs participating in the dynamic pricing intervention procured their electricity through Octopus Energy Limited, as described in Section 2, providing us with access to smart meter consumption data at the relevant charging points. This allowed for a valuable cross-validation: we could compare changes in electricity usage recorded through Electroverse receipts with changes in overall energy consumption at the same charging points. While smart meter data could not be disaggregated by treatment group, we applied a consistent pre-post estimation strategy to both data sources, allowing us to assess how much of the observed increase in Electroverse activity reflected actual system-level demand turn-up. To our knowledge, this kind of cross-platform validation using independent consumption data is rarely possible in public EV charging studies, and offers an important methodological contribution to the evaluation of dynamic pricing and other demand-side flexibility interventions.

To directly compare these measures, we first estimated the total increase in electricity usage based on our main treatment effect estimates from Table A3 (0.0154 and 0.0604). For this analysis, we ignored the green message treatment effect as it was not different from 0 at conventional levels of statistical significance. We applied this to the proportion of treated users in each arm ($\approx 25\%$), the total number of users in the sample (109,711), and the number of events (18). This produced an estimated increase of approximately 37,422 kWh.

We used this estimate to validate our pre-post comparison approach, which also measured changes in energy usage from receipts data. The pre-post method yielded a slightly higher estimate of 38,972 kWh compared to our experiment's estimate, but the two figures were closely aligned, confirming the pre-post method's validity . When we extended this approach to include all users at participating charging points,[37] the estimated increase rose to 57,517 kWh. In other words, users in the experimental sample accounted for approximately 68% of the kWh recorded in the receipts.

We repeated the same calculations using receipts from charging points for which we have smart meter data.[38] We found an increase of 27,215 kWh from users in the experimental sample and 41,135 kWh from all users. This again suggests that around 66% of electricity delivered through these chargers came from the experimental group, indicating similar usage patterns across participating stations.

Next, we applied the same pre-post estimation strategy to the smart meter data for the relevant charging points, which yielded an estimated increase in consumption of 19,726 kWh. To ensure comparability with the receipts-based estimates, we scaled this figure by 68%, corresponding to the share of receipts attributable to users in our experimental sample at these charging points. The adjusted smart meter-based estimate was 13,366 kWh, approximately half the size of the increase estimated using receipts.

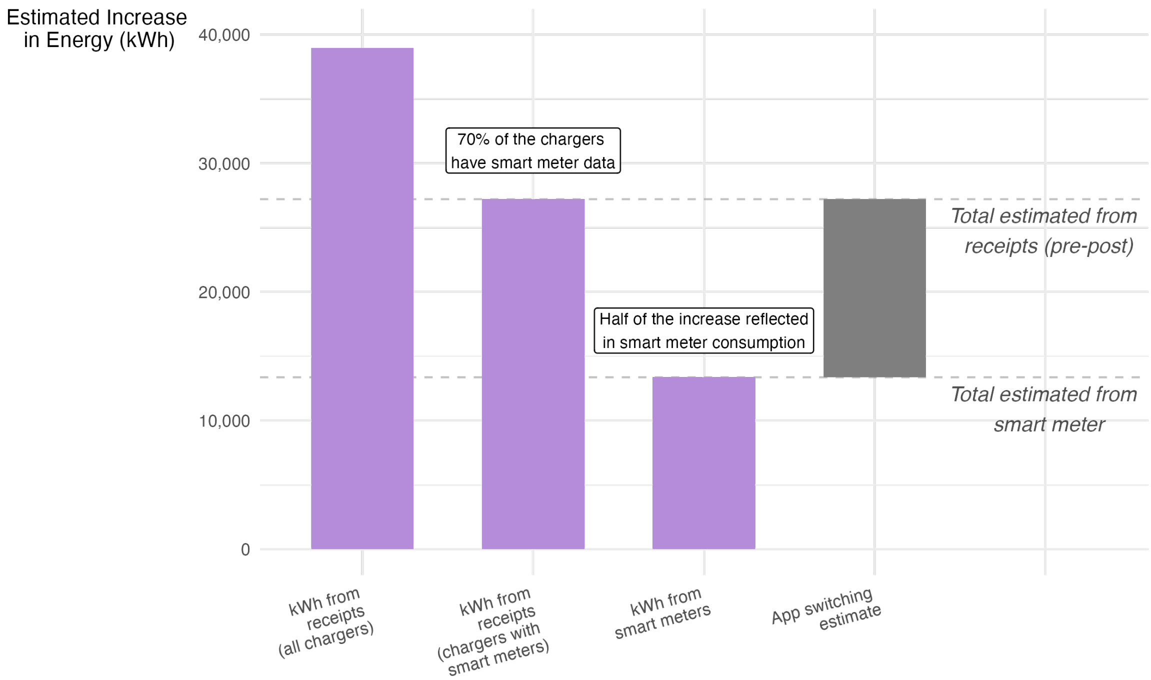
Thus, of the increase in electricity usage via the Electroverse app, a substantial portion seemed to be due to platform switching rather than net new demand. Specifically, approximately 51% of the increase in Electroverse receipts seems to come from users shifting their charging activity from other charging platforms.[39] This finding underscores the importance of distinguishing between changes in platform choice and changes in total electricity demand when evaluating demand-side interventions. Figure 9 provides a visual breakdown of the app substitution effect.

[37] This includes customers who were not randomized into treatment groups due to ineligibility, such as not having push notifications enabled or being registered in a different app country.

[38] We observe half-hourly consumption for only 68% of the charging stations in our dataset. Reasons for missing data include older meters without smart capabilities, smart meters without half-hourly readings enabled, malfunctioning smart meters, or poor address matching between the Octopus Energy Limited and Electroverse datasets.

[39] EV drivers may prefer using the native payment platform of the CPO. However, Electroverse offers convenience when traveling by allowing access to multiple networks through a single app, without the need to register for each CPO individually.

Figure 7: Comparison of receipts and smart meter-based estimates



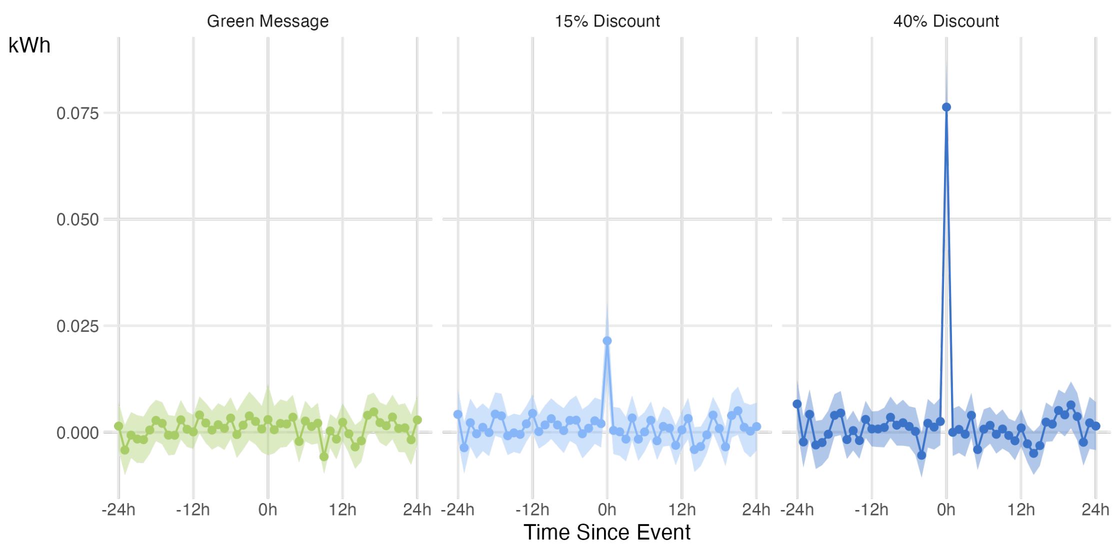
Notes: This figure compares pre-post estimates using all charging points and charging points with smart meter data available. Comparing the receipts-based and smart meter estimates (purple bars two and three), using the same chargers, reveals a smaller increase in smart meter usage. Overall, the comparison between bars two and three – visually decomposed in bar four – suggests that approximately half of the observed rise in receipts-based demand reflects substitution from other platforms, rather than it all being attributable to a net increase in demand. This exploratory analysis was not pre-specified in our analysis plan.

It is instructive to compare the large degree of substitution between payment platforms with the lack of substitution observed in Section 4.3.1. We attribute this difference to the nature of the substitution. CPOs and their chargers differ in charging speed, reliability, user experience, and, perhaps most importantly, location. Customers may be accustomed to particular CPOs or even specific chargers and therefore reluctant to switch away. By contrast, moving between payment platforms is comparatively easy, as it requires neither traveling to a different charger nor adapting to an unfamiliar CPO.

4.3.3 Temporal demand substitution

Event study: To examine more precisely whether EV charging was displaced to hours outside the dynamic pricing periods, we conducted an event study analysis centered on each event. Specifically, we estimated a series of separate OLS regressions, based on our main specification (Equation (1)), to measure treatment effects on electricity consumption (in kWh) for each hour within a 24-hour window surrounding the event. This resulted in 49 regressions: one for each of the 24 hours before the plunge, 24 hours after, and the event period itself. To ensure clean identification, we restricted this analysis to the nine events with non-overlapping 24-hour windows. The outcome variable was hourly kWh charged, and all users were included regardless of whether they charged during the event.

Figure 8: Event study estimates of treatment effects (kWh) before, during, and after dynamic pricing events, by treatment arm.



Notes: Point estimates and 95% confidence intervals from an event study regression specified in our pre-registered analysis plan (as our first of three exploratory analyses). This analysis differs slightly from our pre-analysis plan, where we planned to regress treatment interacted with the time since event. Due to computational limitations, we instead ran the main specification independently for each time period before and after events. Consumption rose sharply at the start of the dynamic pricing event for the 15% and 40% price decrease treatments, with little evidence of displacement before or after the event. Consumption remained unchanged for the green message treatment.

As shown in Figure 8, we found no notable reduction in consumption in the hours before or after the events, suggesting minimal evidence of displacement. Instead, consumption increased sharply at the start of the dynamic pricing event, consistent with the findings from our main regression. We also conducted an event study with 48 hours before and after the event (restricting the analysis to the three events that were spaced sufficient far from other events to enable this analysis). As shown in Figure A11, we find similar absence of temporal displacement when we go further back and forward in time.

Potential for diffuse temporal shifting: Despite the lack of evidence for temporal load-shifting outside event periods, we suspect there may be displacement that was too diffuse to detect. We note the power limitations of our analyses: only approximately 4% of customers charged at least once during events, and only approximately 0.25% of customer-event observations were non-zero. Our pre-analysis plan calculated a minimum detectable effect size (MDES) of approximately 15% of typical charging consumption (kWh) during events. An ex-post MDES calculation yielded a very similar figure.[40]

Examining pre- and post-event consumption in more depth: With this in mind, we conducted a set of exploratory analyses that aggregated charging activity into progressively wider post-event windows, ranging from immediately after the event to seven days later. This approach captures the cumulative impact of the treatment on charging, rather than examining changes hour by hour as in the event study. Because displacement effects may be more diffuse, our hope was that this aggregation would increase the likelihood of detecting any temporal shifts.

In practice, we regressed the total kWh charged within each window on treatment assignment, using the same set of controls as in the main specification. One important caveat is that as the aggregation window widens, the number of eligible events declines. This decline is because we only retained events whose post-event window did not overlap with the notification period of the subsequent event, and it means that as we progress further in time from the end of an event, our statistical power becomes lower (as the number of events in the regression reduces). It is also important to bear in mind that the composition of the events changes as the hours extend forward. Consequently, all events are included in the 6-hour window analysis, but the sample size declines with longer horizons, leaving only three events for the 168-hour (7 days) window.

There is no single analysis that is the obvious "main" specification for this set of exploratory analyses. However, for simplicity, we show here the analysis for the largest number of hours forward that still captures half (nine) of the events – this involves going out 83 hours. Across horizons, coefficients are not statistically significant. However, Figure Figure 9 suggests some evidence of displacement: (i) among treated participants, particularly those in the 40% discount group, there is an initial dip in charging relative to the control, consistent with short-term displacement; and (ii) across all groups, this effect

[40] We used the standard error of the estimated treatment effect from Table A3 (≈ 0.0030 kWh). Using the conventional threshold for 80% power at the 5% significance level (i.e., 2.8 standard errors from the null), the MDES is $2.8 \times 0.0030 = 0.0084$ kWh, which represents approximately 16% of average in-event consumption among Control customers.

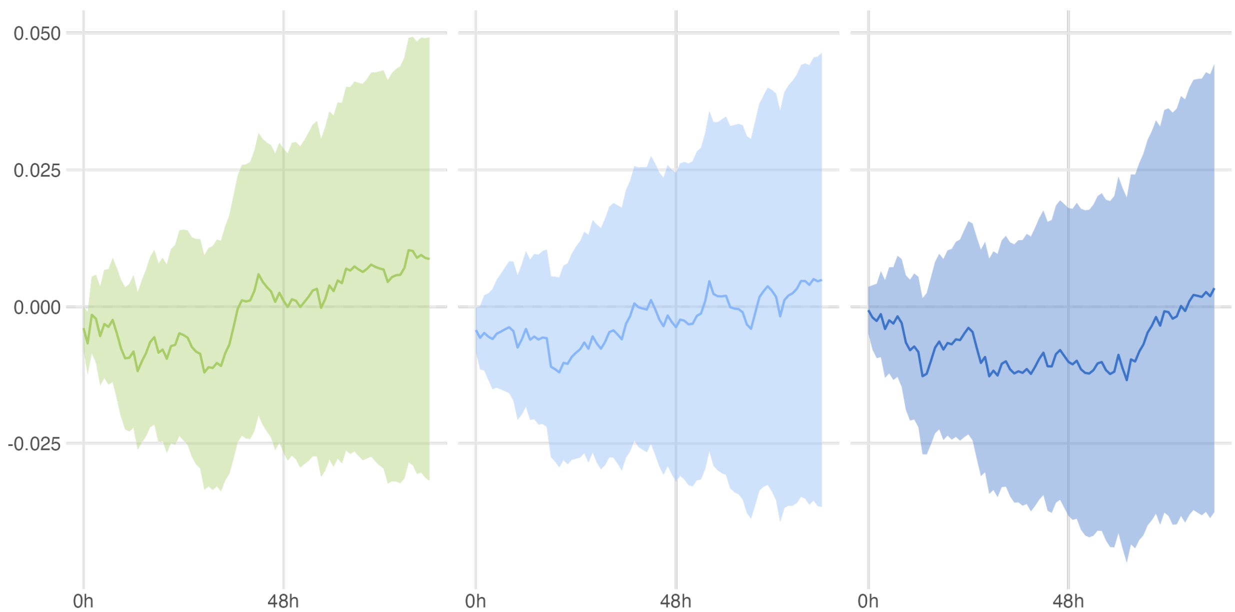
appears to dissipate within four days.

For completeness, we also plot the results for the full set of window lengths in Figure A12. Panel (A) shows the cumulative impact on charging for windows ranging from 1 hour to 7 days after the event. In this panel, the number of included events decreases as the window length increases. Panels (B)–(D) present results for progressively smaller subsets of events: all eligible events, which restricts the window to 6 hours because one event ended only six hours before the next day-ahead notification (B); half of the events, allowing extension to 83 hours, i.e., as we have shown in Figure 9 (C); and five events, allowing extension to 98 hours (D). The qualitative pattern is consistent across samples: a short-term dip for the price discount groups – most pronounced in the 40% discount arm, with some indication of a smaller dip for the 15% group – but with overall effects rarely being distinguishable from zero.

Despite the noisiness of the results, the negative point estimates in the discount groups *suggest* some post-event demand reduction, in the range of 0.01 kWh, for both discount groups. This reduction would be approximately 15 – 20% of the main effect in the 40% discount group (0.0604 kWh). Taking into account the result discussed in Section 4.3.2, this reduction could equate to 30−40% of the total *net* new demand. However, we caution that the confidence intervals are wide, stretching from −0.025 kWh to +0.0125 kWh.

There also appears to be suggestive evidence of slightly increased charging with Electroverse, in all three groups, over the day(s) following an event. The evidence for this is the slightly higher demand in the green message group in the post-event period, and the fact that the point estimate is no longer negative in the discount groups approximately two days after an event, suggesting a sort of "rebound" in those groups. We hypothesize that the event notification may have reminded customers to charge their vehicles—perhaps through the app specifically—and that this reminder effect is what we may be seeing. Again, though, we caution that the confidence intervals are wide and we certainly cannot reject the null of no effect.

Figure 9: Treatment effect (kWh) after the events

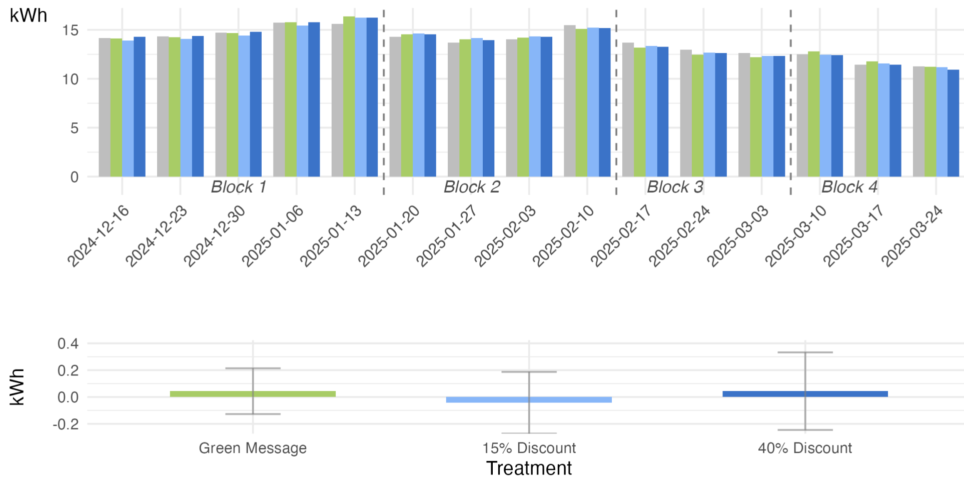


Notes: Point estimates and 95% confidence intervals from an exploratory event study regression analyzing post-event effects, with the outcome variable being consumption (kWh) relative to the control group. For simplicity, this figure presents the analysis for the largest number of hours forward (83 hours) that still captures half (nine) of the full event sample. The coefficients across all horizons are not statistically significant. However, the figure suggests some evidence of short-term displacement. Among the participants who saw 15% and 40% price reductions, there is an initial negative point estimate consistent with a short-term dip in charging relative to the control group immediately following the event (i.e., demand displacement). This effect appears to dissipate over time.

Finally, we note that the stabilization of these effects after approximately two days provides evidence that our one-week wash-out periods were sufficient to avoid carryover effects.

We also examined *pre*-event impacts of our treatments on demand by regressing the treatment on consumption between event notification and event start, using the same set of controls as in our main specification. Results were again not statistically significant for any of the three treatments. That said, to our surprise, the direction of the effect was, if anything, toward a pre-event consumption increase – ranging from 0.0087 kWh (green message) to 0.0157 kWh (40% discount). The latter effect was over 25% of the main in-event demand increase impact of the 40% discount. We hypothesize that this result was driven by the reminder effect of the notification prompting some customers to charge their vehicles immediately, prior to the discount window, but we again caution that the confidence intervals are sufficiently wide that we cannot reject the null of no effect.

Figure 10: Average daily consumption (kWh) recorded at home (top), impact of treatment (kWh) (bottom).



Notes: The top panel of the figure shows average daily home energy consumption by treatment group across all trial weeks. Bars show estimated daily electricity consumption at home (kWh). The bottom panel shows treatment effects from Table A32, a robustness check outlined in our pre-registered analysis plan. Error bars are 95% confidence intervals. Estimates are based on a subset of our sample – 17,188 Octopus Energy customers – observed over the 74 trial days. No statistically significant differences in home energy use are detected across treatment groups.

4.3.4 Substituting from home charging

To assess whether the increase in EV charging during dynamic pricing events reflected a substitution away from home charging, we analyzed home electricity consumption for a subset of 17,188 Octopus Energy customers. For these users, we observed daily home electricity usage (in kWh) across the 74 trial days, spanning 11 weeks from December to March. To do so, we estimated the main regression Equation (1) but changed the main outcome to daily home consumption.

Figure 10 and Table A32 present the results. The top panel of the figure shows average daily home energy consumption by treatment group across all trial weeks. Consumption was generally higher at the start of the trial (December) and tapered off by March, reflecting expected seasonal variation. There were no discernible differences between groups over time. The bottom panel reports average treatment effects, again showing no statistically significant differences between treatments and control. These findings suggest little evidence of displacement to home charging. Participants exposed to price decrease did not appear to reduce their home energy usage relative to Control customers.

4.3.5 Summarizing what we found regarding substitution

In this section, we summarize what we found regarding potential *sources* of increased electricity consumption during dynamic pricing events, as identified in Section 4.2 – i.e., the extent to which we detected demand substitution. In order to consider all possible sources together, we have summarized our substitution analyses in Table 2. We show the effects both in absolute terms and as a proportion of the average treatment effect of 0.0604 kWh for the 40% price decrease from Table A3. Positive values in columns 2-5 reflect *crowding in* – an unexpected result. Negative values reflect the expected reduction in demand (i.e., substitution). In summary:

- Substitution between charging point operators (Section 4.3.1) – from those not participating in dynamic pricing events to those participating – appears to have accounted for little of the overall increase in consumption. The point estimate is close to zero, with a 95% confidence interval of $[-10\%, +10\%]$.

- In contrast, substitution across apps (Section 4.3.2) accounted for approximately half of the observed increase in consumption.

- Temporal substitution prior to the event (Section 4.3.3) yielded a positive point estimate, indicating higher consumption before event hours (approximately +14.5%). This may reflect noise in the estimates or a behavioral response such as increased engagement with the Electroverse app following event notifications. The 95% confidence interval spans $[-2.5\%, +42.3\%]$.

- Temporal substitution following the event (Section 4.3.3) was more consistent with expectations, with an estimated reduction in consumption of 16%, but again note the large amount of statistical noise (95% CI: $[-40\%, +20\%]$).

- Substitution in home charging (Section 4.3.4) was estimated with substantial imprecision. The point estimate was near zero, with the confidence interval encompassing both large reductions and increases in consumption, suggesting the data are insufficient to draw strong conclusions.

- Charging at workplaces or at public chargers not included in the Electroverse network was unobserved in our data, and therefore not included in this decomposition.

Overall, we view the uncertainty in some of these results as partly reflecting the inherent difficulty of measuring substitution behavior in real-world electricity consump-

Table 2: Summary of demand substitution, relative to main effect

Substitution Channel	**Point Estimate (kWh)**	**95% CI**	**As % of Main Effect**	**95% CI, % Effect**
Observed/Estimated Channels				
Between Electroverse CPOs[a]	0.000	[−0.004, 0.004]	1%	[−7.5%, +6.2%]
App Substitution[b]	N/A	N/A	−51%	N/A
Temporal: Before Event	0.0087	[−0.008, 0.025]	14.5%	[−2.5%, +42.3%]
Temporal: After Event	−0.0100	[−0.025, 0.013]	−16.0%	[−40%, +20%]
Home Charging (Weekly)[c]	0.0436	[−0.245, 0.333]	70%	[−403%, +555%]
Unobserved/Unestimated Channels				
Office Charging[d]	—	—	—	—
Non-Electroverse Network[d]	—	—	—	—

Notes: The main effect is defined as the average treatment effect of the 40% discount, 0.0604 kWh. Negative values in columns 2-5 reflect reduction in demand (i.e., substitution). Positive values reflect extra demand. [a] This channel measures substitution between Electroverse CPOs participating in the dynamic pricing events and those that were static. The result is statistically insignificant. [b] No confidence interval is available for App Substitution as this is a comparison of two summary statistics (receipts vs smart meter data) and not an estimate derived from a single regression model. [c] The home charging estimate is calculated over a one-week period, which may include multiple events. The 70% estimate and corresponding wide CI reflect this multi-day measurement period. [d] Office Charging and Non-Electroverse Network charging were not observable in the data.

tion. Obtaining precise estimates of these mechanisms would require substantially larger samples than are feasible in most field settings, given the diffuse and noisy nature of consumption data outside event hours. A fully comprehensive design would also require pairing high-frequency meter and receipt data, such as those used here, with vehicle telemetry data from all customers involved in the trial. Doing so would then enable researchers to capture all charging activity across locations and over time. Implementing such a design would be logistically complex and resource-intensive, bordering on infeasible in a commercial context. In this sense, our field trial pushes the limits of what can realistically be measured in situ, while still offering rare experimental evidence on how dynamic pricing shapes electric vehicle charging behavior.

4.4 Conditional average treatment effects

We explored how treatment effects varied across different consumer subgroups by interacting the interventions with key demographic and contextual variables. We interacted treatment dummies with respondents' demographic characteristics to help explain the heterogeneity observed in the coefficients (for full tabular results, see Appendix A2.3 – Tables A5 to A15). These analyses were not pre-specified, with the exception of the distance analysis, which was pre-specified in the exploratory section of the pre-analysis plan. To investigate this variation, we used the following covariates: (i) user profile based on past charging history; (ii) whether the customer was "active," defined as having charged at least once between January and November 2024; (iii) the customer's MSOA average total household income, divided into quintiles; (iv) and (v) distance to the nearest charger and participating charger, each divided in five distance bands; (vi) whether the customer lived in a rural area, where EV adoption is generally lower (McKinney et al., 2023); (vii) EV battery size, divided into quintiles based on the usable capacity within our sample,[41] measured as the usable capacity of the EV battery (for users with multiple EVs, the largest battery was used); and (viii) the new and used vehicle price, each divided into quintiles based on our sample distribution. To assess whether treatment effectiveness varied by time of day, we also interacted treatments with a set of dummies for time of day (based on the following intervals: Morning-baseline, >6am–12 noon; Afternoon, >12–6pm; Evening, >6pm–midnight; Night, >midnight–6am) and with whether the event occurred on a weekday or weekend.

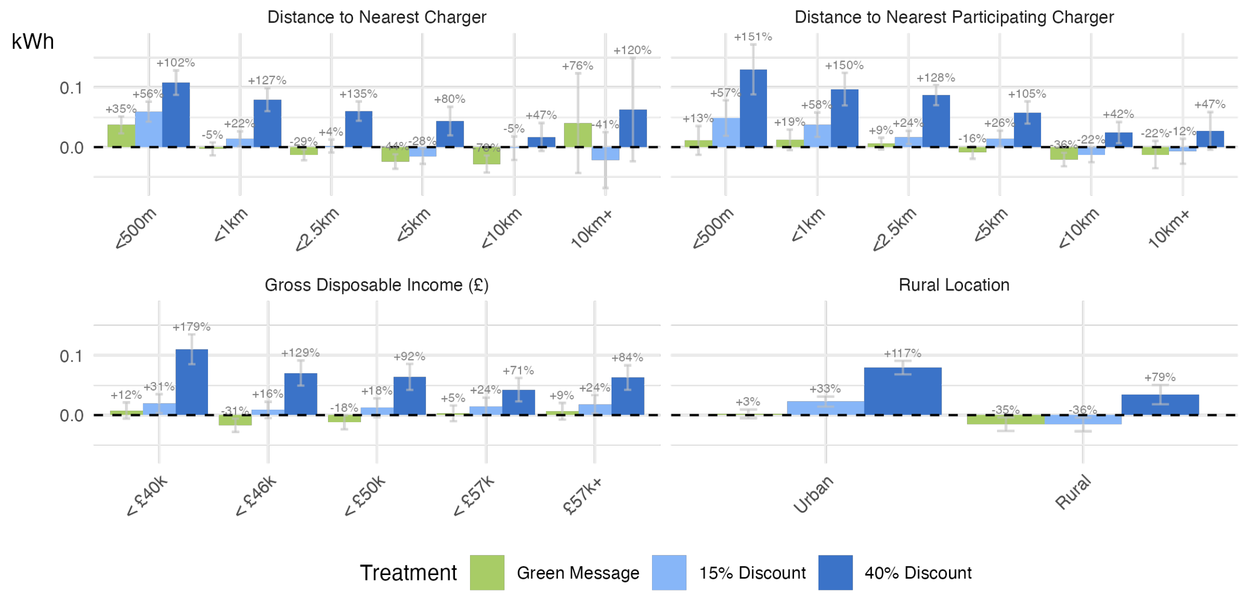
Treatment effects varied by user profile, reflecting differences in charging behavior and price sensitivity. Treatments were most effective among local users and almost null for more occasional users who rely on Electroverse during weekend and holiday trips away from home (left-hand side of Figure A3).[42] Among local users, the treatments were also more effective for those who primarily used fast-charging chargers, compared to those who mostly used slow chargers near their home; we speculate that the latter customers may not have a home charger. For those using local slow chargers, which are generally cheaper, it may be that the price decreases at fast chargers were not sufficient for them to travel to these instead. As local users tend to be more frequent users, treatments were also more effective among active users, defined as those who charged at least once between January and November 2024 (right-hand side of Figure A3).[43] Interestingly, the green message was effective at eliciting a response from both local and more active users, with an impact smaller than the price drops for fast chargers but comparable to the 15% price reduction among local slow-charging users.

[41]We excluded plug-in hybrids from this analysis

[42]These users typically charged within 5 km of their listed home address, based on the clustering analysis described in more detail in Section 3.2.

[43]The decision not to focus our sample on "active" customers by excluding "non-active" users was based on the rationale that dynamic pricing could encourage non-active users to become active, and we do observe some of this shift on the "extensive" margin.

Figure 11: Treatment effects (kWh) by geography



Notes: This figure shows how treatment effects on kWh charged vary by geography. Top-left: users farther from chargers are less responsive to dynamic pricing. Top-right: this effect is stronger when measuring distance to participating chargers. Bottom-left: we observe a U-shaped relationship by MSOA average total income. Bottom-right: urban users show higher baseline usage and stronger treatment effects.

We also found considerable variation in responses by location, particularly based on proximity to charging infrastructure (top left-hand side of Figure 11). The largest effects were observed among users whose postcodes are within 500 meters of any charger, with effects declining sharply, and becoming negligible, beyond 10 km (this latter group is small and has large standard errors, so coefficients should be interpreted with caution). Notably, the green message elicited an increasing treatment effect among users located within 500 meters of a charger, contrary to the overall result. However, beyond 2.5 km, the green message may have even backfired, with users in this group less likely to charge than those in the control group. While these coefficients should not be interpreted causally, as they may reflect user sorting rather than a direct effect of the treatment, they nonetheless highlight the importance of charger convenience and proximity in enabling behavioral flexibility. As expected based on the analysis presented in Section 4.3.1, effects were strongest for the 15% and 40% price drop treatments. There was essentially no impact of the price drops for users located more than 10 km from a participating charger (top right-hand side of Figure 11). The intervention was also more effective among customers in urban, rather than rural areas (bottom right-hand side of Figure 11), potentially due to greater distances to chargers or greater access to off-street home charging.

Treatment effects varied by socioeconomic context. They were less effective among customers in higher-income MSOAs (bottom left-hand side of Figure A3). The lower price sensitivity observed in wealthier areas may reflect greater access to home charging, given previous studies have found a correlation between off-street parking and property value (Chester et al., 2015; Zhang and Fan, 2025). It is also possible that households with less disposable income are more price-responsive - particularly given the comparatively high public charging rates - which previous studies have suggested may be the case for gasoline prices (West and Williams, 2017). That said, our data suggested a potential U-shaped relationship: dynamic pricing events elicited stronger responses in lower-income areas *and* in the highest income quintile. This may partly reflect spatial patterns in infrastructure density, as higher-income areas tend to host denser and faster-charging networks, making discounts more salient and accessible—particularly given that proximity to chargers is an important moderator of responsiveness.

Treatment effects also varied according to *some*, but not all, hypothetically relevant EV characteristics. As expected, we found larger effects for owners of vehicles with larger battery capacities (left-hand side of Figure A6). However, we did not observe any consistent patterns in response to treatments based on the new or used price of the vehicle (middle and right-hand side of Figure A6).

Treatment was also heterogeneous with respect to when events happened – though the times of day featuring the largest absolute (kWh) effects diverged from the times featuring the largest relative changes in demand. While the effect in kWh was the largest in the morning, the relative change compared to the control group average for that period of the day was the highest in the evening (left-hand side of Figure A8), suggesting relatively higher elasticity during those hours. The price decreases had a greater absolute effect on weekends, though the relative effects were similar regardless of weekday versus weekend (right-hand side of Figure A8).

We focused our interpretation of heterogeneity analysis on customers responsiveness to the 40% discount. However, we note that the 15% price decrease followed a similar pattern to the 40% price decrease, though with a smaller magnitude and a faster decline in its effect over distance. Finally, the green message treatment's effects were concentrated among the most active local users.

4.5 Spatial EV charging demand patterns during the trial

Charging activity during dynamic pricing events was geographically concentrated, with notable differences in location patterns between participating and non-participating charging point operators. Figures A16 and A17 show the density of public EV chargers which customers used to charge when a plunge event was on (including the Control and green message treatments). As discussed previously, because transactions were run through CPOs' chargers, we can separate whether the CPO participated in dynamic pricing events or not. As the map illustrates, the majority of charging stations were not participating. Both participating and not participating chargers are mostly located around urban areas. However, there is still a noticeable difference in spatial distribution. Figure A17 shows the relative participation of electric vehicle chargers across UK postcode areas, calculated as the share of participating chargers in each area divided by the national share. Areas shaded in darker green have a higher-than-average level of participation, while lighter areas indicate under-representation. Notably, charger participation is disproportionately high in regions such as Northern Ireland, the Midlands, parts of the North West, and South West England. In contrast, areas with below-average participation include much of Scotland, northern and central Wales, and several southern urban zones including parts of London.

Transaction-level data from the field experiment reveal how charging behavior varied across treatment groups during dynamic pricing events. Table A35 reports descriptive statistics for transactions observed during the dynamic pricing events in our field experiment, classed by treatment the consumer faced when the transaction occurred. Receipts originated from both those charging stations that participated in dynamic pricing events, and those that did not. Since the data reflect only sessions where charging occurred, group differences capture patterns among those who opted to charge, rather than isolating the causal impact of treatment. In the table, we observe slightly higher kWh charged among the 15% and 40% price decrease groups, suggesting a somewhat increased consumption among those who chose to charge (i.e., an extensive margin effect). The average price paid (£/kWh) is significantly lower when a price decrease is in place; however, note that the reduction in price is not exactly 15% or 40% relative to the control and green message groups, because not all receipts happened in CPOs that participated in dynamic pricing events, and consumers did not always select 15% and 40% price decrease chargers even when eligible for a price decrease.

We can also look at how event-level outcomes varied across dynamic pricing events and treatment groups. Figure A18 provides a descriptive overview of charging activity across dynamic pricing events, measured by the number of receipts (i.e., individual charges) recorded during a plunge event. This figure does not reflect energy consumed, but it hints at underlying treatment effects. Specifically, we observe that the 15% price decrease encouraged additional charging on the extensive margin, and the 40% price decrease generated even more uptake. Cross-referencing dates with the timing observed in Table A2, this figure also reveals substantial variation across event types: daytime and evening events attracted more activity compared to overnight or early-morning events, as did events held over the weekend.

We also investigated habit formation in the form of customers changing which chargers they typically used (see Appendix A2.6). The 15% and 40% price reductions increased both overall charging activity and the likelihood of visiting a new charging location, suggesting that discounts encouraged exploration of stations. However, first-time visits triggered by dynamic pricing events were less likely to lead to returns within 30 days compared to visits outside the trial, indicating limited evidence of habit formation. While repeated discounting temporarily boosted return visits, the price reductions did not appear to generate sustained changes in user behavior.

5 Welfare impacts

The price reductions in our experiment generated substantial consumer welfare gains, as customers benefited both from lower prices and increased usage. At the control price of £0.70/kWh, consumers in our study purchased 25,524 kWh, spending £17,867. A 40% discount to £0.42/kWh increased demand to 40,434 kWh, while total spending fell to £16,982 (Figure 12). These figures were calculated using the treatment coefficients from Table A3, multiplied by the average number of customers per trial arm (26,979) and the number of events (18), then halved to account for app substitution, which accounted for roughly 50% of the demand increase.

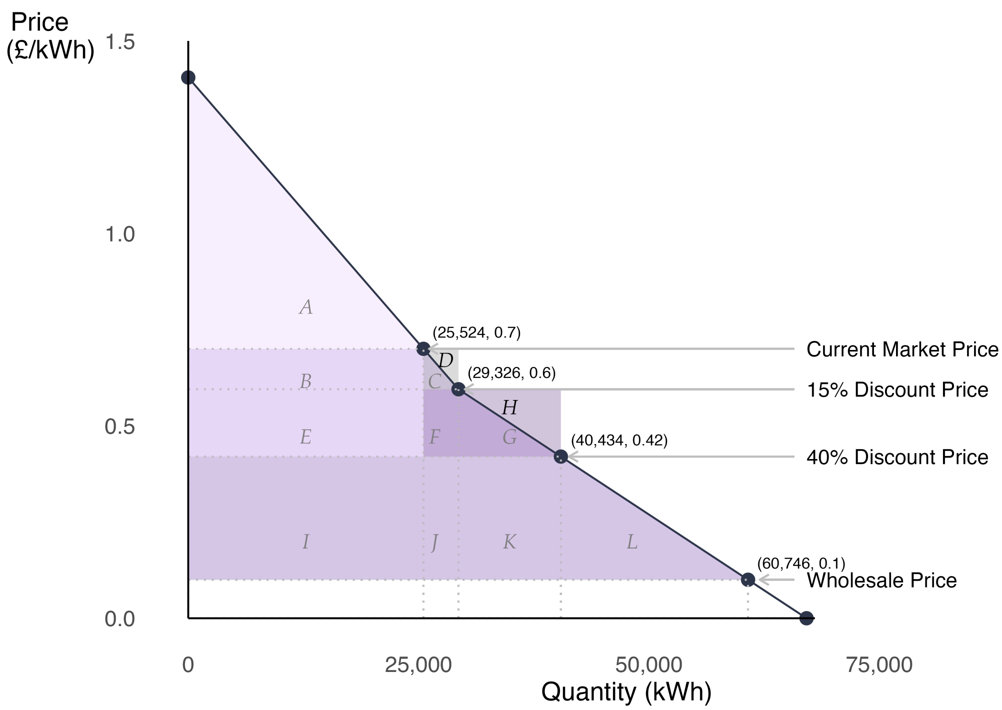
This approach provides a reasonable approximation of new demand, but it may underestimate total consumer surplus for two reasons. First, app-switching customers moving from other platforms also gained from lower prices, which we do not fully capture. Second, any substitution – temporally, from other public chargers, or home chargers – implies additional uncounted gains, since consumers still value the lower price even if the increase in observed kWh represents shifted rather than net new demand. In both cases, the welfare impact is larger than our baseline estimate, because consumers value the price decrease "pound for pound" when their consumption is reallocated rather than new. For example, in the 15% price decrease scenario, assuming the alternative price was again £0.70, the consumer surplus gain would correspond to rectangle C + D (rather than just triangle C). This applies to both app switchers and non-switchers due to the coincidence that app switching accounted for approximately 50% of the total demand increase. In both of these cases, consumers value the price decrease "pound for pound", meaning the welfare impact is double our baseline estimate (i.e., the rectangle C + D rather than the triangle C).

Assuming, for simplicity, pure demand creation, and ignoring the consumer gain of the app switchers, we calculated that total consumer surplus rose significantly, reaching £17,981 (area A + B + C + E + F + G) for the 40% price decrease — equivalent to £1.06 of surplus for every £1 spent by customers. The surplus was calculated as the ratio of total consumer surplus over total consumer spending. The gain in surplus from the price reduction alone, captured by area B + C + E + F + G under the demand curve, was £8,984, or about £0.53 of incremental surplus per pound spent by customers. The incremental surplus was calculated as the change in consumer surplus from original price to 40% divided by the consumers' spending at 40% discount.

Zooming into the margin, we see that the additional unit of electricity consumed at the lower price yielded a marginal consumer surplus of approximately £0.64 per kWh. This translates to a marginal benefit of £1.52 per £1 spent by customers, demonstrating the effectiveness of price reductions at unlocking latent demand and delivering high value to consumers, particularly when electricity is inexpensive to generate and supply.

If prices were reduced further to reflect the marginal cost of supply — assumed here to be approximately £0.10/kWh (i.e., £100/MWh – see Figure 3) — the welfare gains would become even larger (assuming a constant price elasticity of demand). At this marginal price, consumption would expand to 60,746 kWh, and consumer spending would fall to just £6,075. Total consumer surplus would climb to £34,170 (all triangles and rectangles except triangles D and H), or more than £5.63 in surplus per pound spent. The increase in surplus compared to the original price would be £25,172, a nearly threefold return per unit of spending by customers. This highlights the considerable deadweight loss associated with pricing electricity well above marginal cost, and the efficiency gains that can be achieved by aligning retail prices more closely with marginal cost, especially in contexts where low-carbon or surplus generation is abundant.

Figure 12: Consumer surplus change from price reductions



Notes: Demand curve and consumer surplus graph. This graph shows the demand curve for electric charging accounting for app substitution. We have assumed a kinked linear demand curve for simplicity, where the kink is at the 15% discount level, and we have extrapolated the rest of the graph linearly. Region A shows the consumer surplus (£8,997) at the original non-discounted price of £0.70/kWh where demand is 25,524 kWh. Area B + C + E + F + G shows the change in consumer surplus (£8,984) from the original price to when it is discounted by 40%, where demand becomes 40,434 kWh. Region I + J + K + L further shows the change in consumer surplus (£16,189) if charging prices are equivalent to wholesale prices of approximately 10p/kWh, in which case we estimate demand would become 60,746 kWh. Regions D and H show the additional consumer surplus if the demand increase came from demand substitution rather than creation.

Building on this, we estimate that dynamic public EV pricing could substantially lower the energy cost per mile for EV drivers without off-street parking. Our survey results suggest one third of our sample relied on public charging, but this proportion is likely to grow in future. Based on the highest price reduction tested in our trial, these users could reduce their energy cost per mile from 15–24p to 9–14p. This brings them close to parity with the energy cost per mile for ICE vehicles, which ranges from 13 to 17p, and narrows the gap with home charging, typically between 2 and 7p per mile (Office for Zero Emission Vehicles, 2024; Zapmap, 2025).

From a grid balancing perspective, we calculate that our treatments resulted in a total increase in charging consumption through Electroverse of 37.4 MWh. However, we note that approximately half of this treatment effect appears to be due to app substitution (see Section 4.3.2). From the perspective of grid system operators, only net new demand contributes to alleviating system stress or integrating renewable generation. Consequently, we estimate that the demand increase, net of app substitution, amounted to 18.7 MWh over our 11-week trial.

Distribution Network Operators (DNOs) in Great Britain differ in how much they value demand turn-up to manage local network constraints. Publicly available data from UK Power Networks (UKPN), the DNO for London, the South East, and the East of England, shows that it pays an average of £446/MWh for demand turn-up services (UK Power Networks, 2025). While this figure is not directly comparable to wholesale electricity prices, as it reflects payment for increased demand rather than total consumption, it can be used to approximate the value of the 18.7 MWh of demand turn-up observed in our field experiment. If procured by a DNO offering similar compensation to UKPN, this demand response would be worth approximately £8,415.

This estimate provides a rough indication of the potential grid-balancing value of the turn-up we identified in our 11-week trial, based on a subset of participating chargers, at the distribution level. The net demand turn-up would be larger over the course of the year and with greater coverage of the charging network. It may also contribute to absorbing excess renewable generation to address constraints on the transmission network, potentially contributing to a reduction in curtailment if targeted in the right locations (for example, Scotland, where growing curtailment of renewable generation due to transmission constraints is increasing balancing costs for the system operator (National Electricity System Operator, 2024). We also note that grid constraints will change over time and are highly sensitive to changes in grid infrastructure, electricity markets, and balancing services.[44]

Producer surplus impacts are uncertain and complicated to determine. We are not able to share data on the mark-up of our delivery partner, the public EV charging plat-

[44]There are also potential CO_2e abatement benefits from demand turn-up, assuming that it displaced consumption during higher-CO_2e-intensity times of day, although they are small because of the relatively clean UK grid. Although we did not find direct evidence of such temporal displacement, we can make a hypothetical assumption that the turn-up displaced consumption during the evening peak (4pm-7pm). Under this assumption, we estimate that 914 kg of CO_2e would be abated. To arrive at this figure, we summed the combined treatment effects' impact during each event and multiplied the total by the difference in carbon intensity between the event hours and the 4pm-7pm peak hours, based on NESO's published grid carbon intensity for each half-hour of each day. However, we must adjust this estimate to account for the app substitution effect, which leads to a revised estimate of approximately 450 kg of CO_2e abatement. According to the UK government's Department for Energy Security and Net Zero (2023) valuation of carbon, CO_2e abatement in 2025 is valued at £260 in £$_{2020}$ per metric ton (approximately £325 in £$_{2025}$). Therefore, the hypothetical CO_2e abatement resulting from our experiment is worth approximately £146. Applying these effects across this year (52 weeks, rather than just 11) would imply approximately 2100kg of CO_2 abatement, or £686.

form, due to commercial sensitivities, and we lack data on the mark-up of CPOs. Assuming a competitive market, there would then be no producer surplus being reduced by the intervention. It is also worth noting that the costs of this sort of intervention are near zero, as the price decreases stem from public EV charging platforms passing on lower procurement costs during low-cost periods. (That said, the price decreases in our experiment were not the result of an optimization algorithm, but were determined ex ante in consultation with our delivery partner, based on their expertise in power procurement and assessment of levels that would be feasible during periods of low wholesale prices.) We therefore conclude that the intervention should generate economic welfare gains, stemming from benefits for consumers, and potentially the grid and CO_2e abatement. Benefits would increase with increased adoption of EVs, which comprise only 4% of cars on UK roads today, and greater charging infrastructure. Higher penetration of renewable generation, particularly if this outstrips transmission network expansion, may also increase the value of smart public charging in future.

6 Conclusion

We found that public EV charging demand through the Electroverse platform was highly price elastic — much more so than residential electricity or gasoline demand. This likely reflects the flexibility drivers have over when and where they charge. By contrast, a non-price environmental encouragement had no effect, consistent with earlier findings. Lower prices at public chargers may thus help reduce reliance on home charging and support broader EV adoption. However, these estimates capture short-run behavioral responses to temporary price signals within the Electroverse network, representing about 60% of the UK public charging infrastructure, rather than general parameters for the entire EV charging market or for long-term investment analysis.

We observed clear demand responses to dynamic pricing events but no significant evidence of load-shifting outside them. The lack of observed substitution from home or other chargers may reflect limited statistical power or unobserved behaviors, such as choosing an EV over an ICE vehicle. We do find substantial customer switching from competing platforms to Electroverse in response to price changes.

Our results suggest that brief price reductions did not generate long-term habitual changes in charging behavior. This lack of persistence may be useful from a welfare perspective. Because wholesale prices and social marginal costs fluctuate unpredictably, it is beneficial for customers to remain responsive to specific signals rather than develop fixed charging patterns. In this sense, the transitory nature of the response may increase confidence that demand can flexibly adjust to periods of low-cost, low-carbon electricity, which could be preferable to inducing habitual behavior that might be misaligned with system conditions on specific days and hours.

Our results highlight the potential for demand flexibility in public charging, especially when prices align with real-time grid conditions. While static pricing currently dominates, dynamic pricing could improve grid efficiency. Such pricing does not require vertically integrated firms, as was the case here. Operators can still pass through low wholesale prices under varied business models, provided they are themselves exposed to those price signals through the design of wholesale markets. One example is dynamic time-of-use tariffs available to non-domestic electricity customers, including charge point operators. Open data schemes for charge point operators could also ensure drivers are exposed to dynamic prices across the entire charging network, support a transparent and competitive retail market.

Flexible public charging supports both customer affordability and grid integration of renewables. Realizing this potential will depend on market designs that reflect the true time- and location-specific value of electricity, including network constraints. With the right incentives, public charging can become a valuable tool for managing growing EV demand.

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Appendix

Table of Contents

A1 Appendix figures

A1.1 Congestion

We compared the proportion of time each station was highly utilized (80% occupied) over the entire trial, distinguishing between participating and non-participating sites. For each station, we calculated the share of recorded hours above this threshold and ranked stations by percentile.

As shown in **??**, non-participating stations were substantially more congested. The top quartile of non-participating sites exceeded 50% of hours above the 80% threshold, with some reaching up to 75%, whereas even the busiest participating stations rarely exceeded 15%. This divergence reflects underlying differences in charger type and usage context: non-participating stations are more often slow, on-street or kerbside chargers used for overnight residential charging, where occupancy naturally remains high for extended periods. In contrast, participating stations are typically faster chargers located in public or semi-public settings, with shorter session durations and more variable utilization.

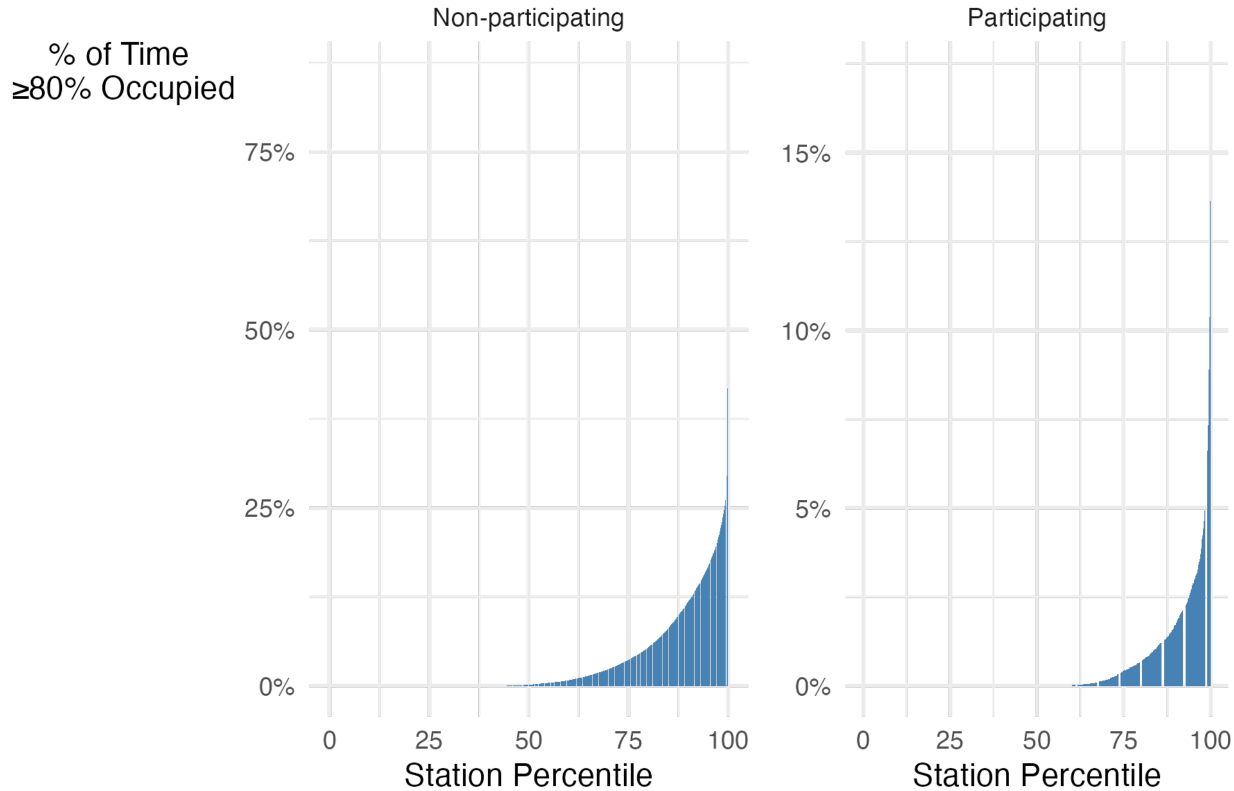


Figure A1: Share of time each location was at or above 80% occupancy during the trial by participation

A1.2 Conditional average treatment effects (CATEs) analysis figures

To contextualize the heterogeneity analysis presented in the CATE figures below, Figure A2 summarizes the number of users in each subgroup used to compute conditional treatment effects. We report group sizes for the binary activity split (active vs. inactive), the user segmentation used in our profiles clustering, as well as categories based on distance to chargers, battery capacity, and EV price brackets.

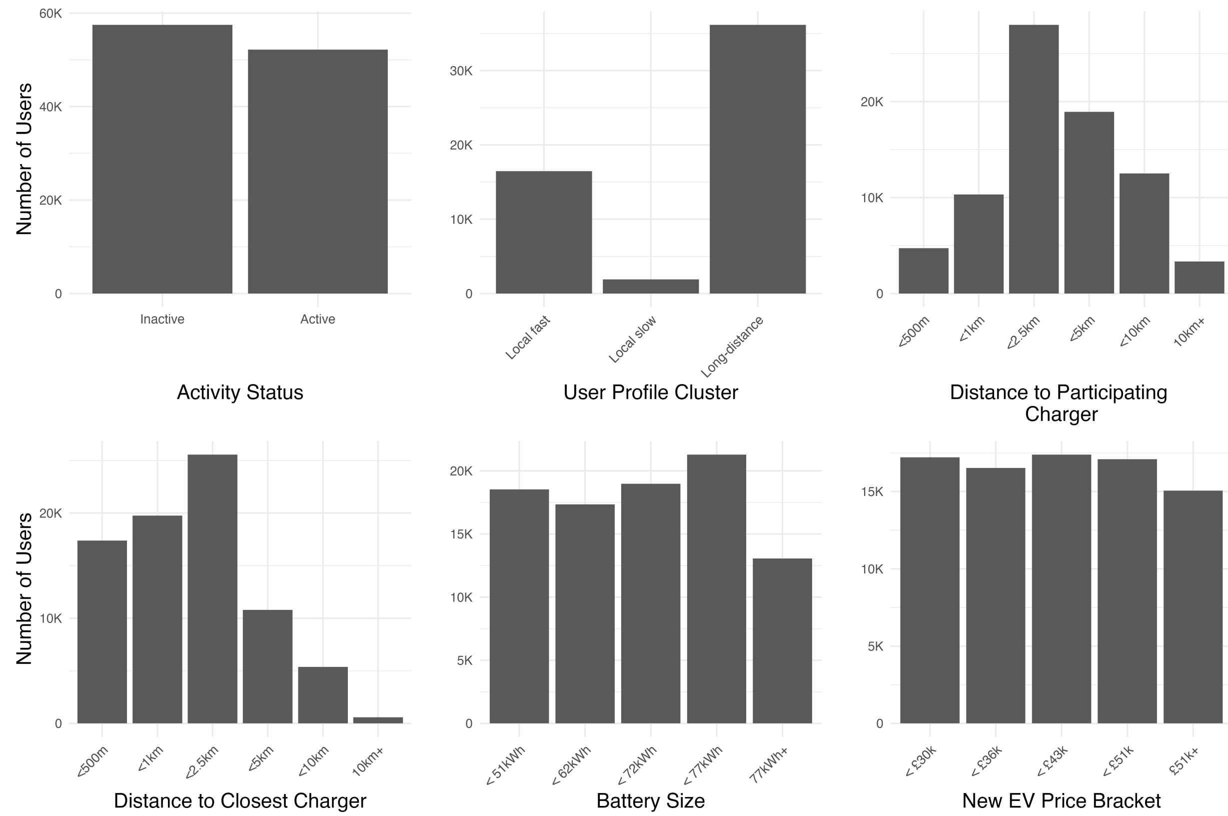


Figure A2: User characteristics frequency

A1.2.1 CATEs by user type

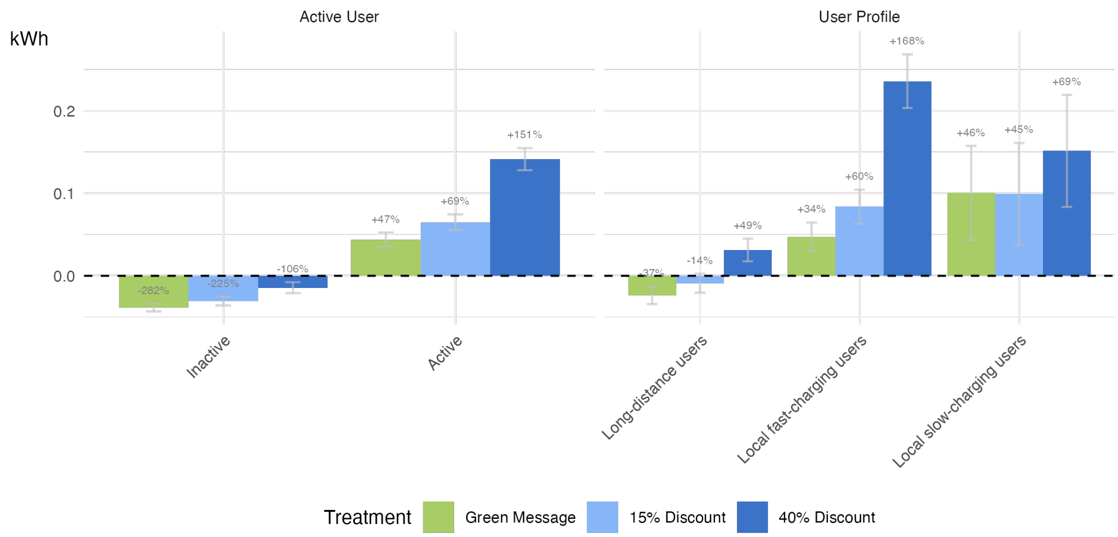
We explored how treatment effects vary by different aspects of customers/users of the Electroverse app.

The left-hand side of Figure A3 shows the interaction between treatment groups and whether a user was "active", measured as having charged at least once in the period Jan-Nov 2024. (See Table A6 for tabular results.)

The right-hand side of Figure A3 shows how treatment effects vary by user profile, based on pre-trial charging behavior. Occasional users who charge only during trips (long-distance users) showed very little engagement with dynamic pricing events. In contrast, local users accounted for most of the treatment response, particularly those who rely on fast charging stations. Interestingly, the green message appears effective in increasing charging rates among these users. (See Table A5 for tabular results.)

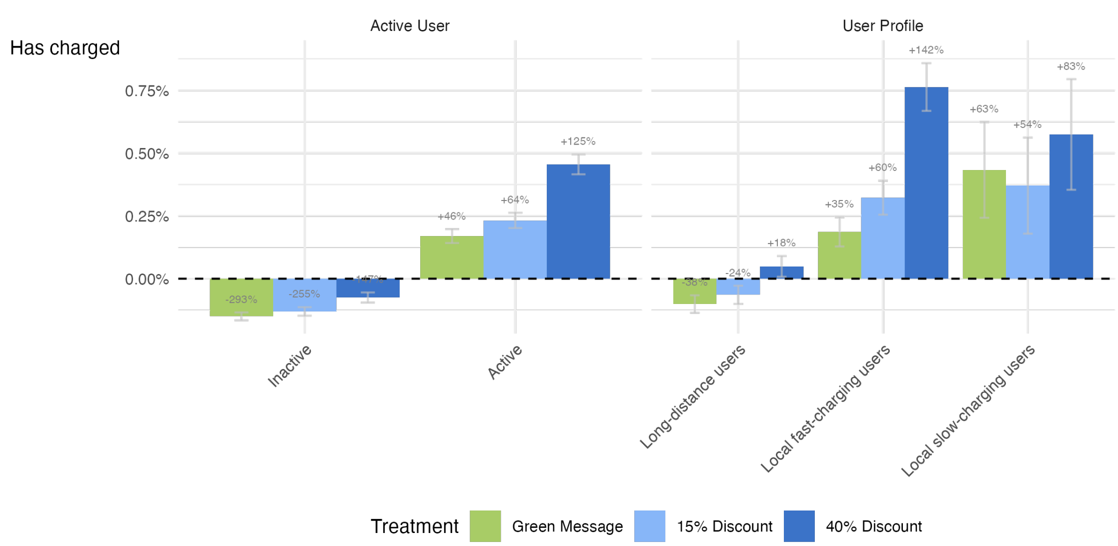
In Figure A4, we show the same analysis for whether or not a customer has charged during the events. We find the same patterns.

Figure A3: Treatment effects (kWh) by user characteristics



Notes: This figure shows how treatment effects on kWh charged vary across user types. The left panel splits users by prior app activity (i.e., whether they had charged at least once before the trial), while the right panel breaks users into clusters based on pre-trial charging behavior. Results suggest stronger treatment effects among more engaged users, particularly local fast-charging users.

Figure A4: Treatment effects on charging incidence (binary) by user characteristics



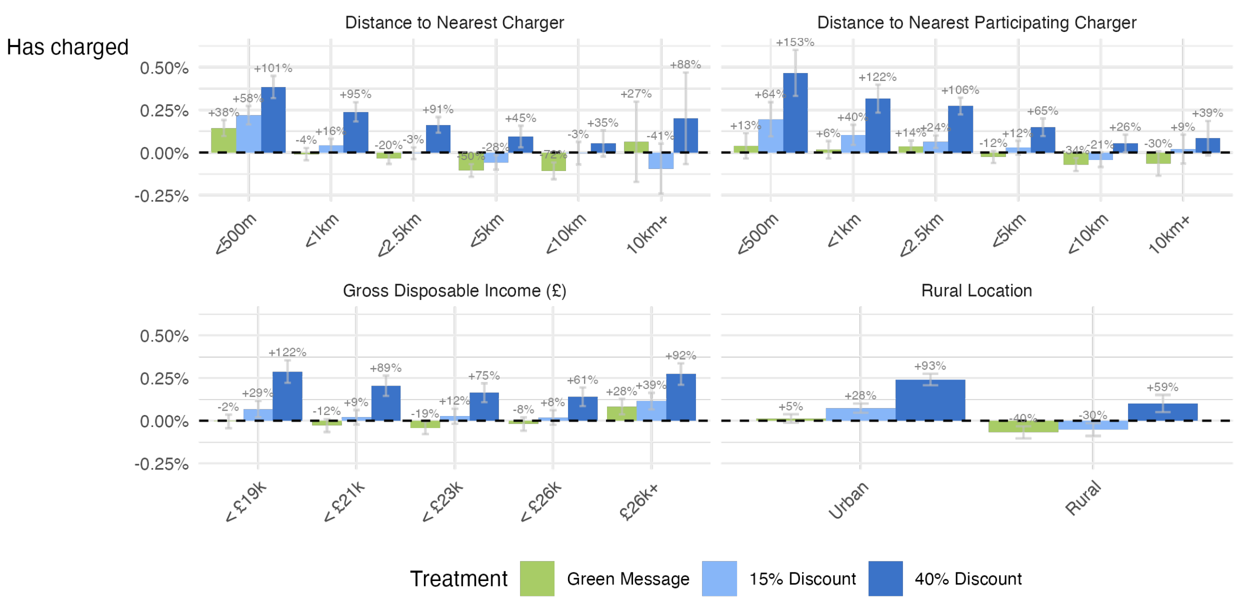
Notes: This figure replicates the analysis in Figure A3, but for the binary outcome of whether a user charged during events. Patterns are consistent: treatment effects are concentrated among previously active and fast-charging local users.

A1.2.2 CATEs by geography

We next analyzed how treatment effects vary by distance to the nearest charger and other geographical parameters.

The top left-hand side of Figure 11 shows the interaction between treatment groups and distance to the nearest charger. There was a clear negative relationship between distance and engagement with dynamic pricing events. Interestingly, we see an increase in treatment effect for the green message as long as the distance to the nearest charger is under 500 m. However, past 2.5 km, the green message may be backfiring, with users in this group even less likely to charge than the control group. (See Table A7 for tabular results.)

Figure A5: Treatment effects on charging incidence (binary) by geography



Notes: This figure replicates the analysis in Figure 11 using the binary outcome of whether a user charged during events. The same patterns hold across all geographic dimensions.

The top right-hand side of Figure 11 shows the same interaction but for distance to the nearest *participating* charger. As expected from the participating stations analysis, the effects are stronger for the treatment arms with 15% and 40% decrease in price. There is effectively no impact of the price decrease past 10 km. (See Table A8 for tabular results.)

The bottom left-hand side of Figure 11 displays the treatment effects in relation to users' Middle layer Super Output Areas (MSOA) Annual Total Income from the 2020 Financial year. The data suggests a U-shaped relationship: dynamic pricing events elicited stronger responses in lower-income areas, while the highest income quintile showed a greater reaction across all treatments, excluding the green message. (See Table A10 for tabular results.)

Finally, the bottom right-hand side of Figure 11 illustrates the treatment effects by user location (rural vs. urban). Urban users exhibited higher kWh charging than rural users overall and also demonstrated a greater increase in charging compared to the urban control group. (See Table A9 for tabular results.)

In Figure A5, we show the same graphs for whether or not participants have charged during the events. We find again the same pattern.

A1.2.3 CATEs by EV characteristics

We next analyzed how treatment effects varied by characteristics of the customer's vehicle.

The left-hand side of Figure A6 shows the treatment effects across EV battery size quintiles. A clear positive correlation existed between battery size and charging increase, both in absolute kWh and relative to the control group within each quintile. Hybrid vehicles were excluded from this analysis. (See Table A15 for tabular results.)

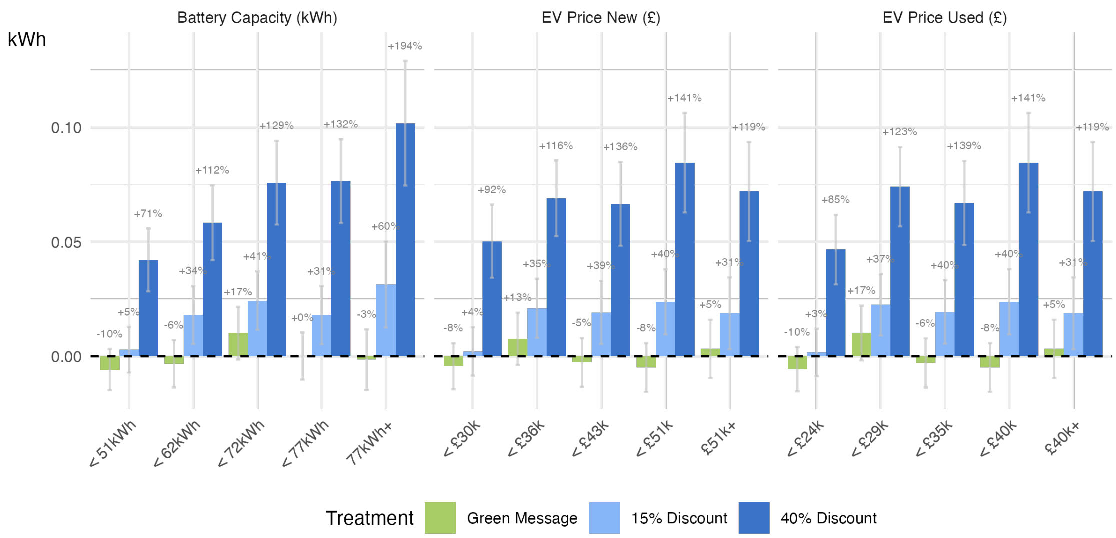
The middle panel of Figure A6 shows the interaction between treatment groups and the price of EV as if it were new, based on pricing from Autotrader, CarGurus, specific EV brand websites, and other popular UK car purchasing sites. There was no clear interaction between treatment effects and value of the EV. (See Table A11 for tabular results.)

Finally, the right-hand side of Figure A6 shows the interaction between treatment groups and the price of EV (used), based on pricing from Autotrader, CarGurus, specific EV brand websites, and other popular UK car purchasing sites. There was no clear interaction between treatment effects and value of the EV. (See Table A12 for tabular results.)

In Figure A7, we show the same graphs for whether or not participants have charged during the events. We find a slightly different patterns: participants with smaller batteries are more likely to charge during the events than participants with larger batteries (however, the relative increase compared to the control groups are similar). It shows that part of the larger consumption by battery size is mostly a mechanical results of the larger batteries.

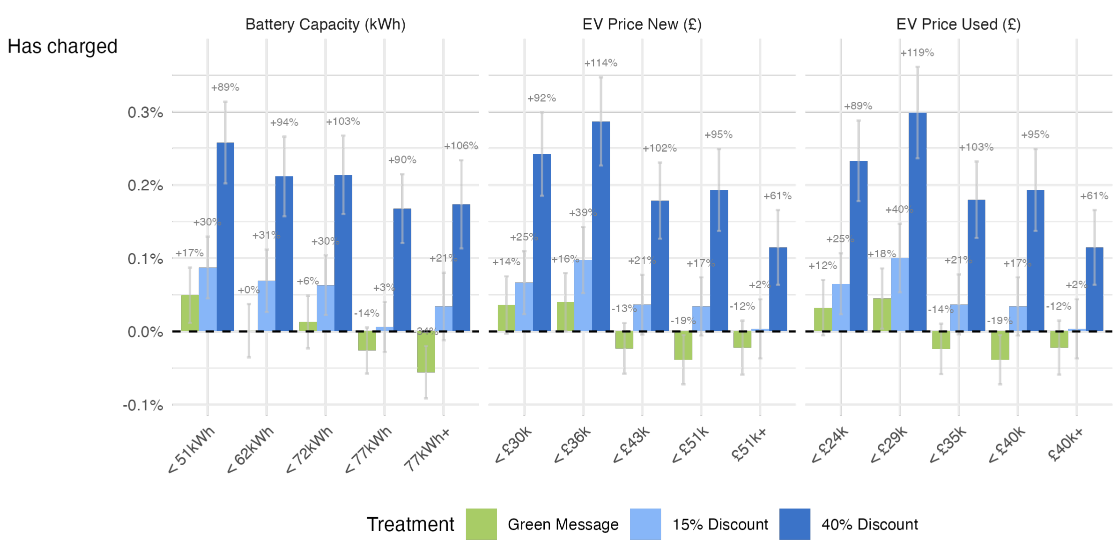
When looking at EV prices, we find EV owners in the highest price quintiles are less likely to charge than other drivers both in absolute and relative terms.

Figure A6: Treatment effects (kWh) by battery capacity and EV price



Notes: This figure shows how treatment effects vary by vehicle characteristics. Left: charging increases with battery size, likely reflecting both capacity and usage patterns. Middle and right: no clear relationship is observed between treatment effects and vehicle price (new or used). Hybrid vehicles excluded.

Figure A7: Treatment effects on charging incidence (binary) by battery capacity and EV price



Notes: This figure replicates the analysis in Figure A6 using the binary outcome of whether a user charged during events. Users with smaller batteries are slightly more likely to charge, suggesting that differences in total kWh partly reflect battery capacity. High-value EVs are associated with lower event participation.

A1.2.4 CATEs by time-related event characteristics

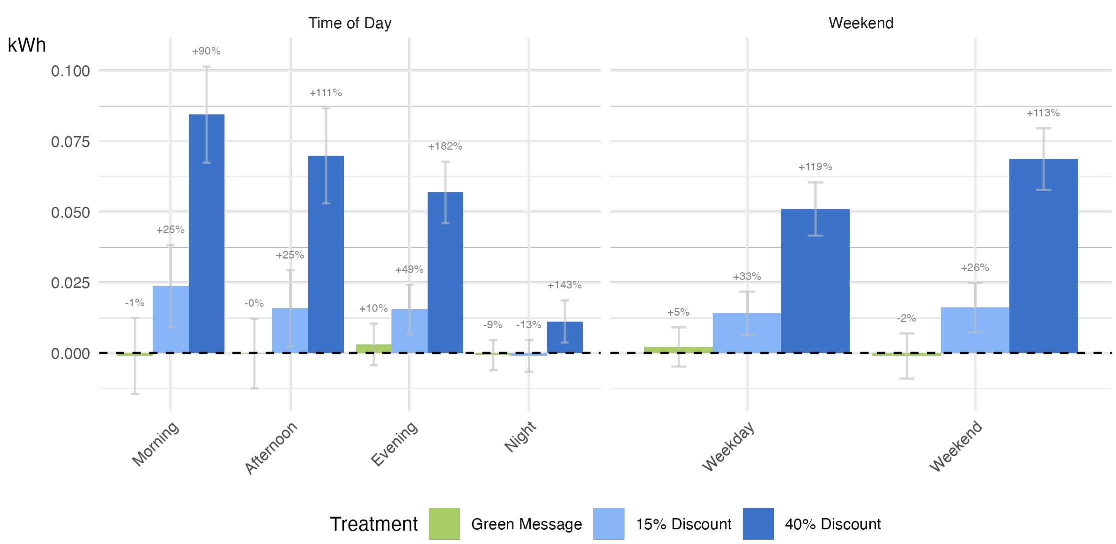
Finally, we analyzed how treatment effects varied depending on when the event occurred.

The left-hand side of Figure A8 shows the interaction between treatment groups and time of the day. While the effect in kWh was the largest in the morning, the *relative* change compared to the control group average for that period of the day was the highest in the evening. (See Table A13 for tabular results.

Finally, the right-hand side of Figure A8 shows the interaction between treatment groups and weekday vs weekend. While the absolute kWh charged was higher during weekends, the *relative* increases compared to the control group were of similar magnitude for all treatments. (See Table A14 for tabular results.)

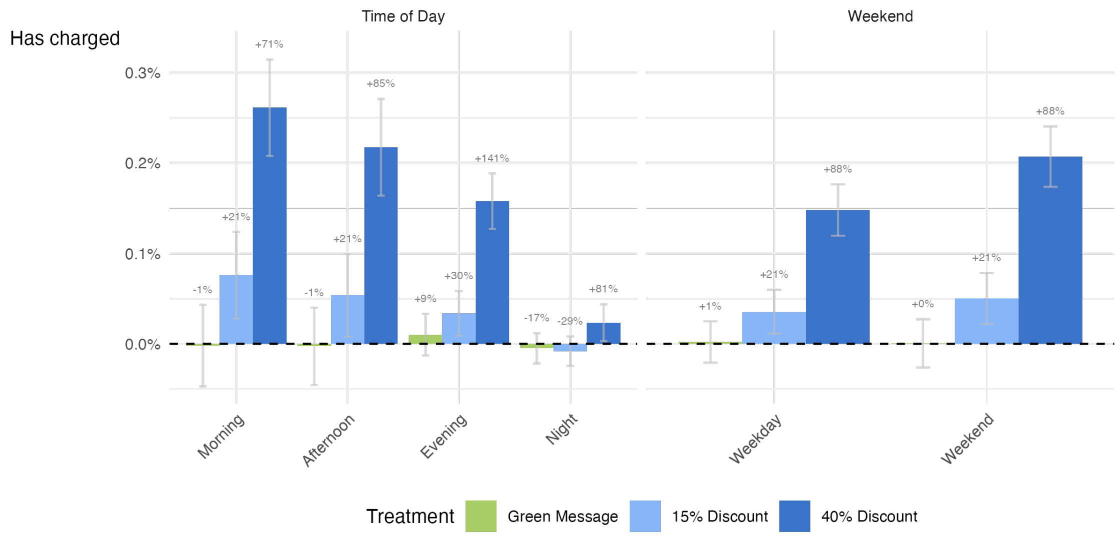
As before, we show very similar results when looking at whether or not participants have been charging during events in Figure A9.

Figure A8: Treatment effects (kWh) by time of the day and weekday vs weekend



Notes: This figure shows how treatment effects vary by time of day (left) and weekday vs. weekend (right). The absolute effect is highest in the morning, but relative increases are strongest in the evening. Weekend charging is higher overall, but relative treatment effects are similar to weekdays.

Figure A9: Treatment effects on charging incidence (binary) by time of the day and weekday vs weekend

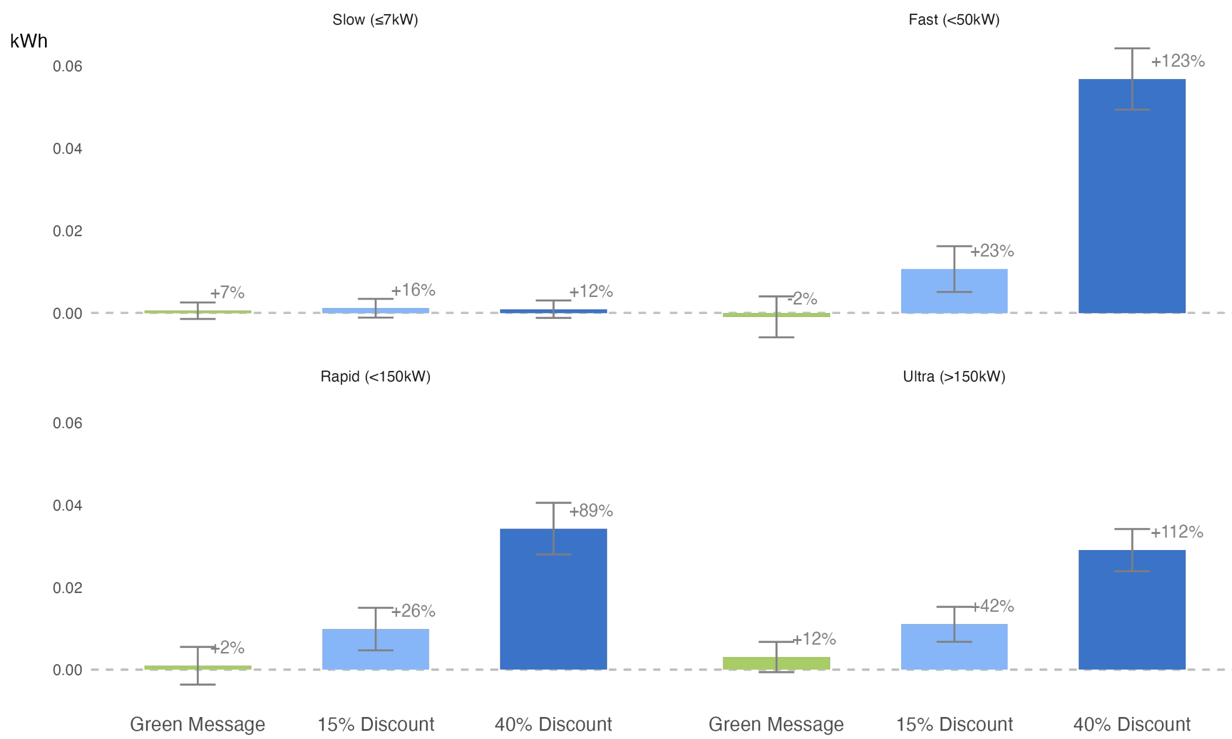


Notes: This figure shows treatment effects on the probability of charging during an event by time of day (left) and weekday vs. weekend (right). The pattern mirrors the kWh analysis: higher absolute engagement in the morning and on weekends, but stronger relative shifts in the evening.

A1.3 Consumption during events by charger speed

In this section, we decomposed the consumption during events by the type of chargers used during the transaction. The consumption during the events comes from fast chargers (> 7kW), which is likely driven by the composition of chargers among participating CPOs (see Table A1.

Figure A10: Treatment effects (kWh) by speed of the charger



Notes: This figure breaks down consumption during events by the speed of the charger used. Most of the observed increase comes from fast chargers (>7kW), reflecting the charger mix among participating operators. See Table A1 for details.

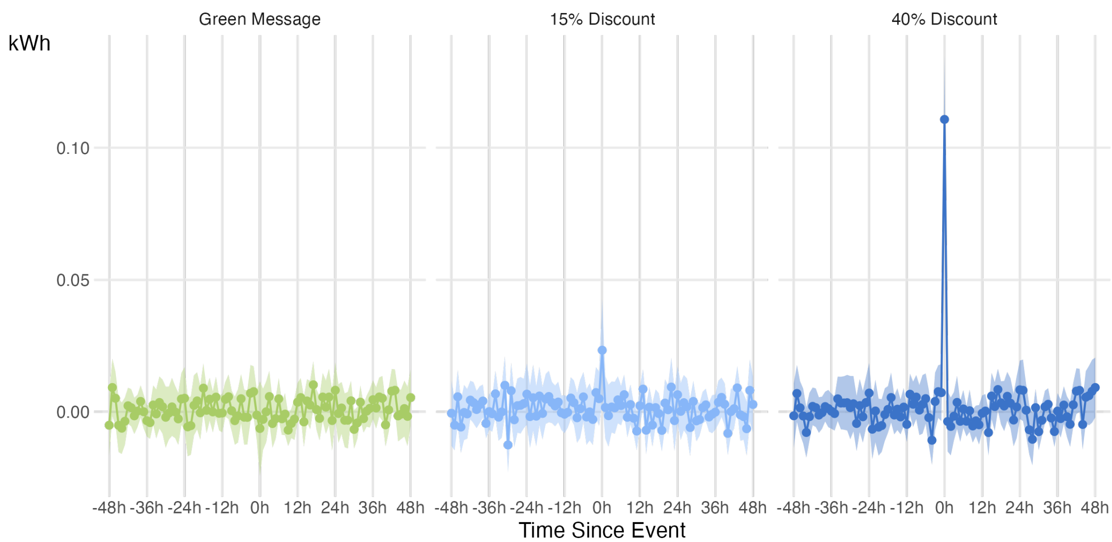
Most of the participating EV charging stations are fast chargers (above 50 kW), typically located at destination sites such as supermarkets or leisure centers. This contrasts with slow chargers, which are more often associated with off-street residential settings. In Table A1, we report the breakdown of Electric Vehicle Supply Equipment (EVSE) — that is, the physical charging units installed at charging stations — separately for participating and non-participating CPOs, by charger speed. Participating chargers tend to be faster.

Table A1: EVSE Speed Distribution by Participation Status

speed	Non-participating	Participating
Slow (7kW)	23468 (47.5%)	1906 (16.0%)
Fast (<50kW)	16685 (33.8%)	4363 (36.6%)
Rapid (<150kW)	5266 (10.7%)	4550 (38.1%)
Ultra (>150kW)	4002 (8.1%)	1117 (9.4%)

A1.4 Event study - 48 hours before and after event

Figure A11: Event study for 48h window around the events



Notes: This graph shows the main regression for 48h before and after events – only for the three events that had 48 hours "before" and "after" not overlapping with other events' "before" and "after" periods.

A1.5 Displacement using increasing time windows

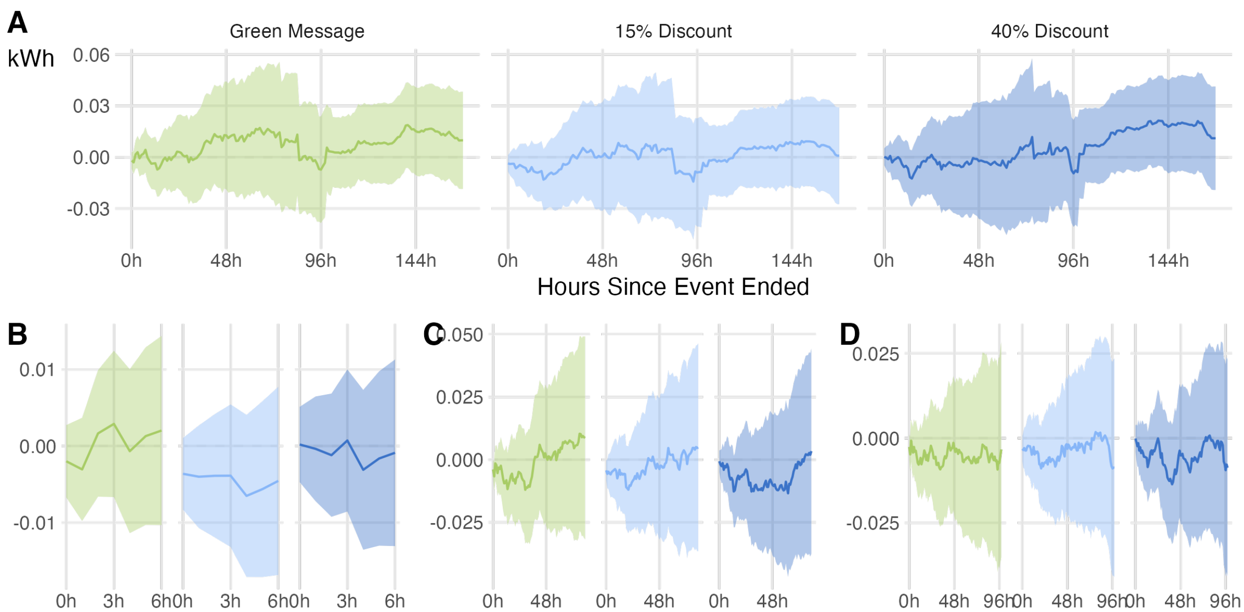
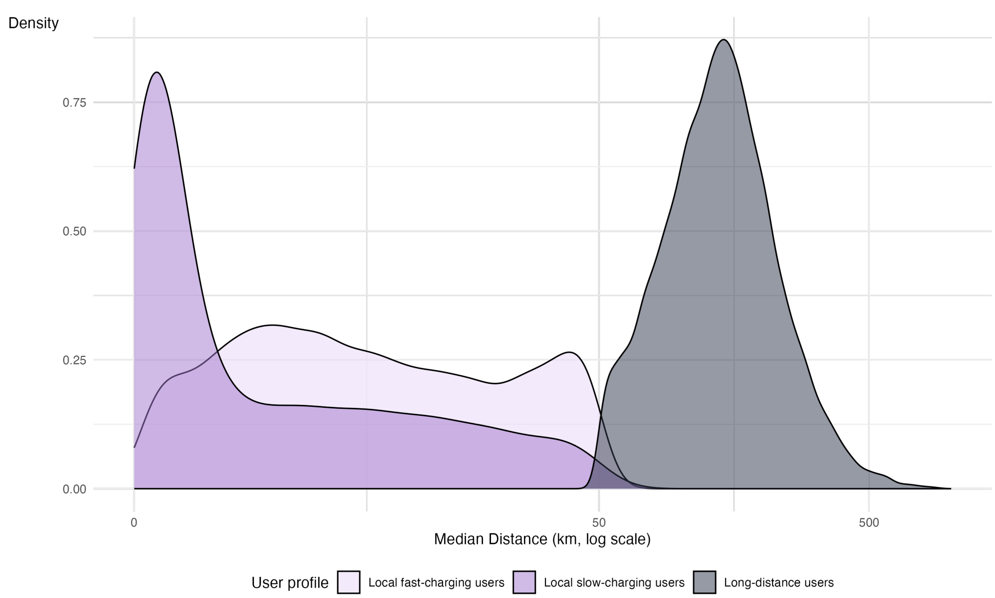


Figure A12: Treatment effect (kWh) after the events

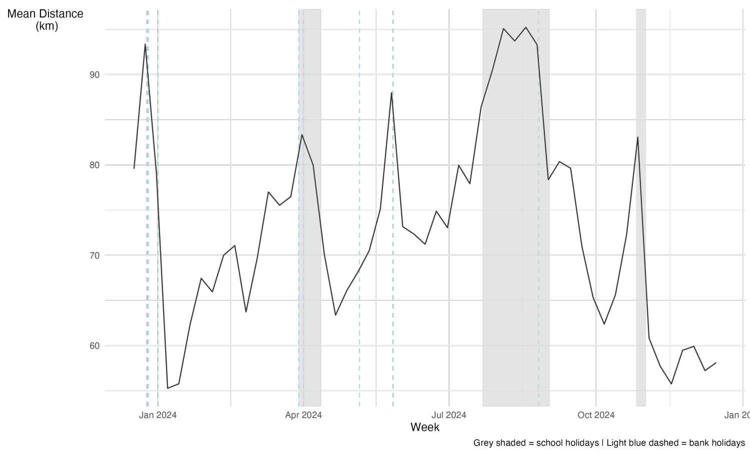
A1.6 Descriptive figures

Figure A15: Median distance to charger by user profile



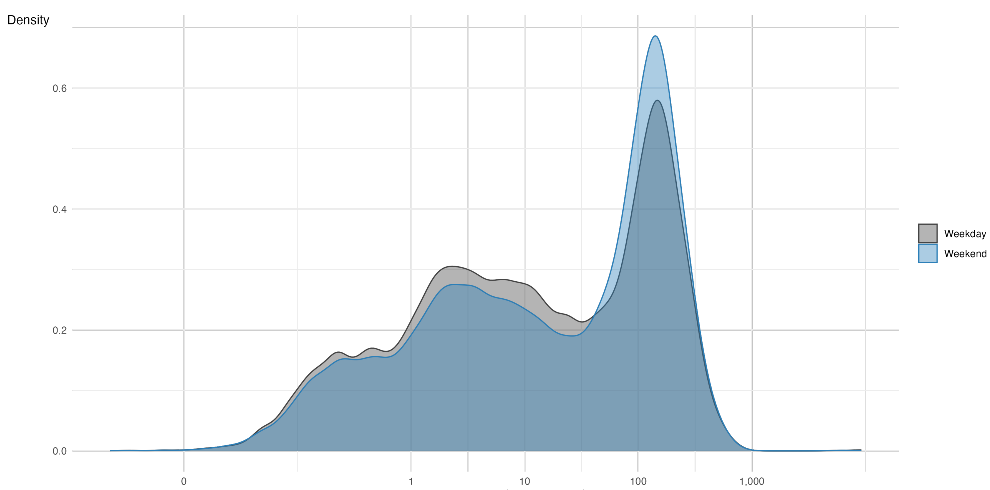
Notes: Distribution of users' median distance to charger, by user profile. Local users predominantly charged within 1 km of home, while long-distance users typically charged over 100 km away. The x-axis is shown on a logarithmic scale to better represent the skewed distribution and to highlight both local and long-range behavior.

Figure A13: Weekly mean distance to chargers



Notes: Mean distance between users' home postcodes (centroids) and charger locations for all charging sessions in 2024. Distances increase notably during school and bank holidays, suggesting greater use of public charging during longer trips.

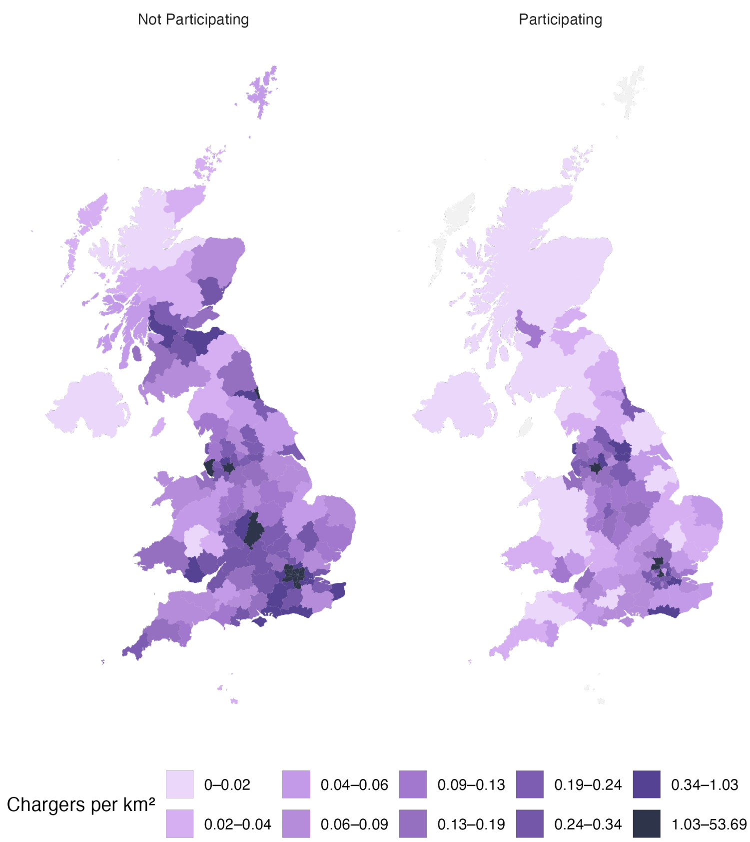
Figure A14: Distance to chargers for weekdays and weekends



Notes: Mean distance between users' home postcodes (centroids) and charger locations, averaged by day of the week. Weekend charging is associated with longer travel distances, indicating more frequent long-distance charging behavior.

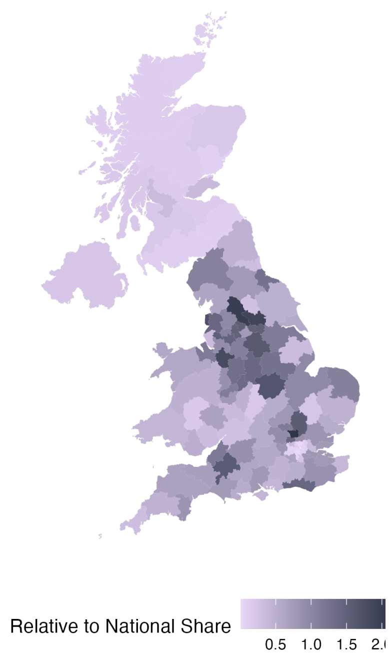
A1.7 Maps

Figure A16: Density of chargers who offered dynamic pricing in our trial, by postcode area of Great Britain



Notes: This map shows the density of public EV charging stations used at least once during the trial period, aggregated at the UK postcode area level and separated by whether the charging point operator participated in dynamic pricing events. While charging activity was geographically concentrated in urban areas for both groups, there are notable differences in spatial distribution, with higher participation rates in regions such as the Midlands, parts of the North West, and South West England.

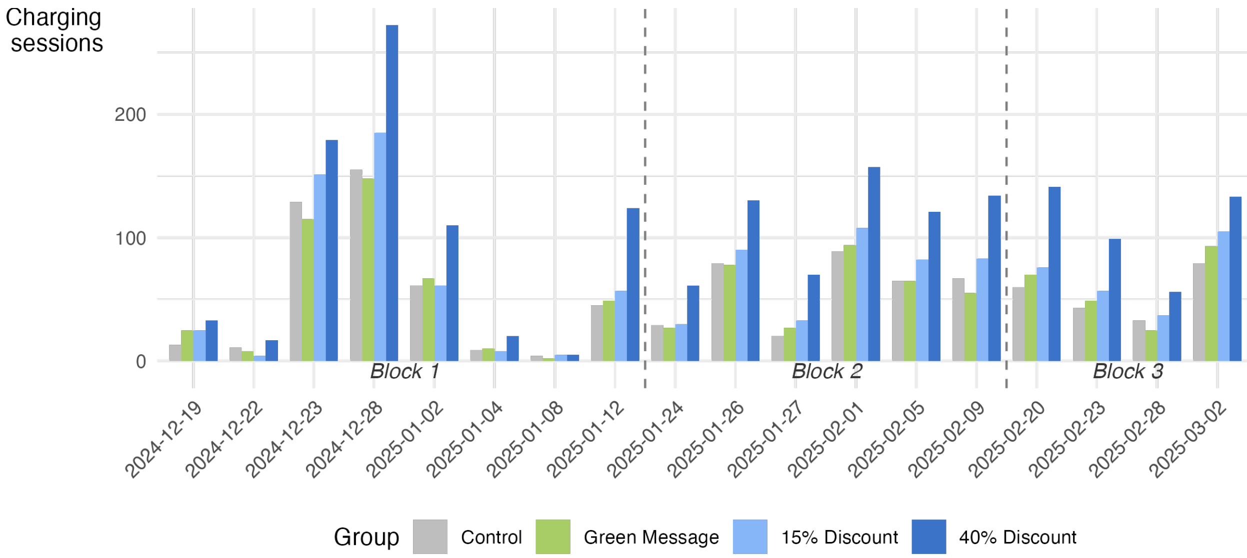
Figure A17: Share of participating charging stations relative to the national average by postcode area



Notes: This map shows the relative participation of public EV chargers across UK postcode areas, calculated as the share of participating chargers in each area divided by the national average. Darker areas indicate above-average participation, while lighter areas indicate below-average representation.

A1.8 Receipts per event

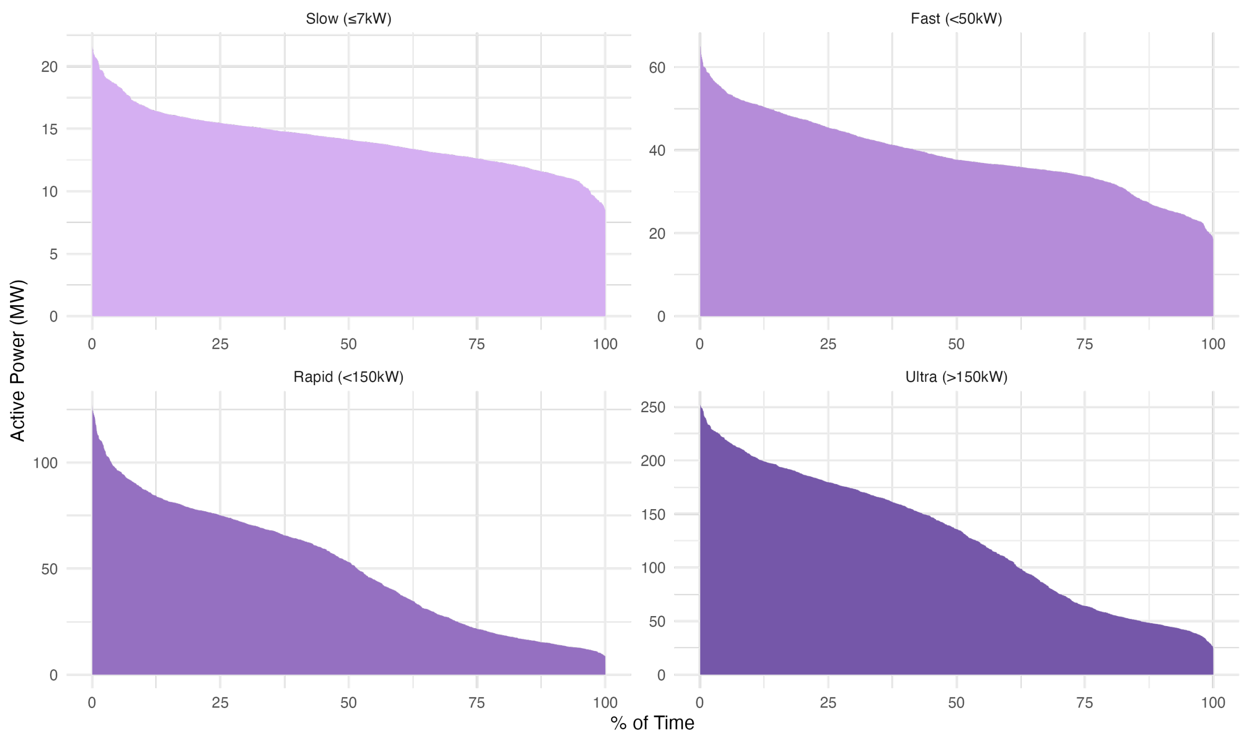
Figure A18: Number of charging sessions during dynamic pricing events, by treatment



Notes: Bars show the total number of receipts (i.e., charging sessions) recorded during each dynamic pricing event. This figure captures activity on the extensive margin rather than energy consumed. The 15% and 40% price decreases are associated with increased charging frequency. Event-level variation reflects differences in timing (e.g., daytime vs. overnight) and day of week (e.g., weekend vs. weekday), as detailed in Table A2.

A1.9 Load Duration Curve by charger speed

Figure A19: Load duration curve by charger speed



Notes: This figure presents the Load Duration Curve (LDC) for Electroverse chargers by charger speed, using data from December 19, 2024 to March 3, 2025. The LDC ranks demand levels over time and helps assess how often the charging network approaches capacity. High-speed chargers (particularly rapid and ultra-rapid) show steeper LDC slopes, indicating more variable peak demand and a greater potential for flexibility. The maximum theoretical capacity of each charger type is as follows: 196 MW for slow, 465 MW for fast, 673 MW for rapid, and 1,067 MW for ultra-rapid chargers.

A1.10 Time of Use versus flat rate chargers

Using data from Electroverse post-trial, we examined the prevalence and timing of ToU electricity pricing across public charge points in the United Kingdom. The analysis identifies connectors that apply more than one energy price within a day and reconstructs their 15-minute price profiles to capture hourly variation in tariffs. Overall, about 26% of EVSEs in the dataset apply a ToU tariff, while the remaining 74% operate under a flat rate. Among ToU chargers, the vast majority are slow units (around 88%), with a smaller share of fast chargers (about 12%), and almost no rapid or ultra-rapid devices using vari-

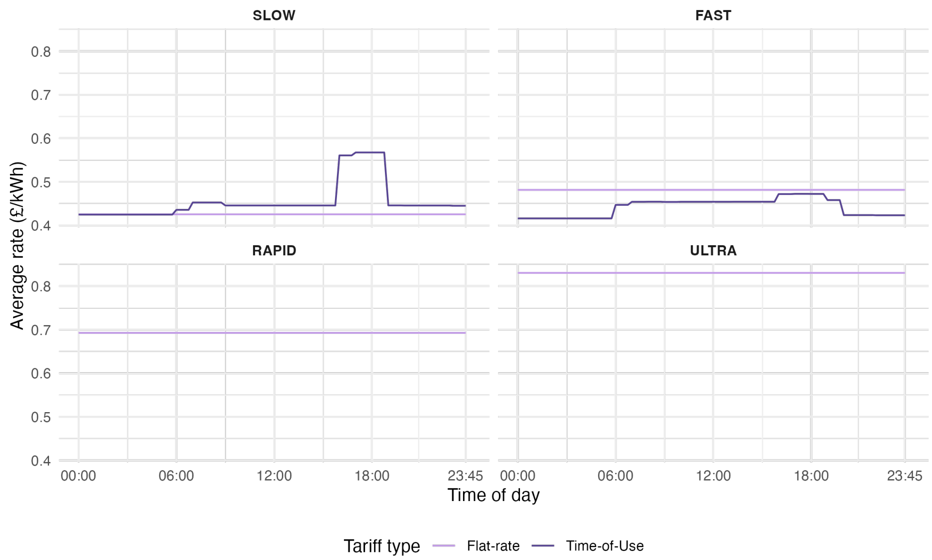


Figure A20: Daily price profiles by speed

able pricing. When focusing on operators that later participated in the flexibility trial, only 13% of their connectors exhibit ToU pricing (almost all fast), compared with 27% among non-participating operators. This lower share of ToU among participating EVSEs is consistent with the higher proportion of fast charging stations.

The reconstructed daily profiles showed in Figure A20 show that ToU tariffs mainly affect slower charge points, typically introducing higher evening rates. It is important to note that these patterns describe current tariff structures as of November 2025, not the period of the trial itself, which pre-dated the creation of this dataset.

A2 Appendix tables

A2.1 Event characteristics

Table A2: Characteristics of dynamic pricing events

	Plunge Date	Time	Type	Notice (hrs)	Notice Type
Block 1					
	19/12/2024	10pm–midnight	Evening	12	Same-day
	22/12/2024	5am–7am	Night	17	Day-ahead
	23/12/2024	11am–1pm	Morning	18	Day-ahead
	28/12/2024	9pm-12am	Morning	18	Day-ahead
	02/01/2025	12pm-1pm	Afternoon	19	Day-ahead
	04/01/2025	5am–7am	Night	14	Day-ahead
	08/01/2025	3am–5am	Night	9	Day-ahead
	12/01/2025	7pm–9pm	Evening	26	Day-ahead
Washout					
Block 2					
	24/01/2025	9pm–11pm	Evening	28	Day-ahead
	26/01/2025	9am–11am	Morning	20	Day-ahead
	27/01/2025	9pm–11pm	Evening	28	Day-ahead
	01/02/2025	12pm–2pm	Afternoon	19	Day-ahead
	05/02/2025	12pm–2pm	Afternoon	19	Day-ahead
	09/02/2025	5pm–7pm	Afternoon	24	Day-ahead
Washout					
Block 3					
	20/02/2025	11am–1pm	Morning	18	Day-ahead
	23/02/2025	7pm–9pm	Evening	26	Day-ahead
	28/02/2025	9pm–11pm	Evening	25	Day-ahead
	02/03/2025	11am–1pm	Morning	18	Day-ahead

A2.2 Main results

Table A3: Impact of the experimental stimuli on kWh charged

Dependent Variables:	Consumption (kWh)	Has Charged (binary)		
Model:	(1)	(2)	(3)	(4)
	OLS	OLS	Probit	Marginal effects
Variables				
Green Message	0.0015	4.4×10^{-5}	0.0065	3.78×10^{-5}
	(0.0027)	(8.94×10^{-5})	(0.0153)	(8.92×10^{-5})
15% Discount	0.0154***	0.0004***	0.0682***	0.0004***
	(0.0030)	(9.7×10^{-5})	(0.0152)	(0.0001)
40% Discount	0.0604***	0.0018***	0.2327***	0.0018***
	(0.0039)	(0.0001)	(0.0146)	(0.0002)
kWh - September	0.0006***	$1.63 \times 10^{-5***}$	0.0004***	
	(0.0001)	(3.1×10^{-6})	(8.07×10^{-5})	
kWh - October	1.66×10^{-5}	4.97×10^{-7}	$9.29 \times 10^{-6***}$	
	(1.43×10^{-5})	(4.37×10^{-7})	(3.24×10^{-6})	
kWh - November	0.0018***	$5.81 \times 10^{-5***}$	0.0016***	
	(0.0001)	(3.04×10^{-6})	(6.02×10^{-5})	
Active User	0.0344***	0.0016***	0.4781***	
	(0.0032)	(9.3×10^{-5})	(0.0140)	
Fixed-effects				
Date	Yes	Yes	Yes	
Avg. Outcome				
Control	0.0517	0.0020	0.0020	
Fit statistics				
Observations	1,974,798	1,974,798	1,974,798	
Number of Accounts	109,711	109,711	109,711	
R^2	0.00716	0.00814		
Pseudo R^2	0.00189	-0.00261	0.11995	
F-test	32.980	37.497		

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Estimates in this table refer to the primary specification outlined in Section 1 of our pre-analysis plan. Romano-Wolf p-values (100 bootstrap replications) for the two main OLS outcomes are 0.77, 0.01 and 0.01 for Green Message, 15% Discount, and 40% Price Decrease, respectively.

Table A4: Impact of the experimental stimuli on kWh charged

Dependent Variable:	Consumption (kWh)		
Model:	(1)	(2)	(3)
Variables			
Green Message	0.0005	-0.0007	0.0013
	(0.0027)	(0.0041)	(0.0040)
15% Discount	0.0150***	0.0105**	0.0106**
	(0.0031)	(0.0045)	(0.0044)
40% Discount	0.0599***	0.0553***	0.0549***
	(0.0040)	(0.0059)	(0.0059)
Green Message × Block 2		-0.0013	-0.0033
		(0.0065)	(0.0063)
15% Discount × Block 2		0.0057	0.0053
		(0.0072)	(0.0070)
40% Discount × Block 2		0.0077	0.0098
		(0.0094)	(0.0092)
Green Message × Block 3		0.0075	0.0059
		(0.0073)	(0.0072)
15% Discount × Block 3		0.0119	0.0141*
		(0.0081)	(0.0079)
40% Discount × Block 3		0.0092	0.0098
		(0.0104)	(0.0101)
kWh - September			0.0006***
			(0.0001)
kWh - October			1.66×10^{-5}
			(1.43×10^{-5})
kWh - November			0.0018***
			(0.0001)
Active User			0.0344***
			(0.0032)
Fixed-effects			
Date	Yes	Yes	Yes
Avg. Outcome			
Control	0.0517	0.0517	0.0517
Fit statistics			
Observations	1,974,798	1,974,798	1,974,798
Number of Accounts	109,711	109,711	109,711
R^2	0.00101	0.00101	0.00717

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Estimates in this table correspond to robustness checks out-lined in our pre-registered analysis plan.

A2.3 CATEs analysis tables

The estimates in these tables are based on a variation of the primary analysis outlined in our pre-registered analysis plan, where the treatment variables (Green message, 15% Discount, and 40% Discount) are interacted with selected covariates.

Table A5: Impact of the experimental stimuli on kWh charged interacted with User Profile

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message×Local fast-charging	0.0469***
	(0.0089)
Green Message×Local slow-charging	0.1002***
	(0.0291)
Green Message×Long-distance	-0.0235***
	(0.0054)
15% Discount×Local fast-charging	0.0830***
	(0.0105)
15% Discount×Local slow-charging	0.0990***
	(0.0316)
15% Discount×Long-distance	-0.0086
	(0.0059)
40% Discount× ocal fast-charging	0.2365***
	(0.0166)
40% Discount×Local slow-charging	0.1516***
	(0.0347)
40% Discount×Long-distance	0.0311***
	(0.0070)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	981,846
Number of Accounts	54,547
R^2	0.00235

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A6: Impact of the experimental stimuli on kWh charged interacted with Active User

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message × Inactive	-0.0384***
	(0.0024)
Green Message × Active	0.0438***
	(0.0043)
15% Discount × Inactive	-0.0306***
	(0.0027)
15% Discount × Active	0.0650***
	(0.0049)
40% Discount × Inactive	-0.0145***
	(0.0034)
40% Discount × Active	0.1416***
	(0.0069)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,974,798
Number of Accounts	109,711
R^2	0.00196

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A7: Impact of the experimental stimuli on kWh charged interacted with Distance to Nearest Charger

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message × <500m	0.0371***
	(0.0072)
Green Message × <1km	-0.0030
	(0.0055)
Green Message × <2.5km	-0.0129***
	(0.0046)
Green Message × <5km	-0.0240***
	(0.0062)
Green Message × <10km	-0.0283***
	(0.0073)
Green Message × 10km+	0.0400
	(0.0425)
15% Discount × <500m	0.0592***
	(0.0085)
15% Discount × <1km	0.0138**
	(0.0065)
15% Discount × <2.5km	0.0018
	(0.0056)
15% Discount × <5km	-0.0154**
	(0.0065)
15% Discount × <10km	-0.0019
	(0.0101)
15% Discount × 10km+	-0.0216
	(0.0238)
40% Discount × <500m	0.1079***
	(0.0105)
40% Discount × <1km	0.0794***
	(0.0098)
40% Discount × <2.5km	0.0602***
	(0.0082)
40% Discount × <5km	0.0435***
	(0.0122)
40% Discount × <10km	0.0170
	(0.0119)
40% Discount × 10km+	0.0628
	(0.0441)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,430,352
Number of Accounts	79,464
R^2	0.00136

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A8: Impact of the experimental stimuli on kWh charged interacted with Distance to Nearest Participating Charger

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message × <500m	0.0112
	(0.0123)
Green Message × <1km	0.0123
	(0.0087)
Green Message × <2.5km	0.0059
	(0.0049)
Green Message × <5km	-0.0090*
	(0.0053)
Green Message × <10km	-0.0208***
	(0.0058)
Green Message × 10km+	-0.0126
	(0.0116)
15% Discount × <500m	0.0486***
	(0.0152)
15% Discount × <1km	0.0375***
	(0.0104)
15% Discount × <2.5km	0.0162***
	(0.0057)
15% Discount × <5km	0.0143**
	(0.0068)
15% Discount × <10km	-0.0128**
	(0.0063)
15% Discount × 10km+	-0.0070
	(0.0106)
40% Discount × <500m	0.1300***
	(0.0213)
40% Discount × <1km	0.0970***
	(0.0140)
40% Discount × <2.5km	0.0867***
	(0.0085)
40% Discount × <5km	0.0578***
	(0.0095)
40% Discount × <10km	0.0243***
	(0.0091)
40% Discount × 10km+	0.0271*
	(0.0161)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,401,156
Number of Accounts	77,842
R^2	0.00131

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A9: Impact of the experimental stimuli on kWh charged interacted with

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message × Urban	0.0021
	(0.0037)
Green Message × Rural	-0.0153***
	(0.0057)
15% Discount × Urban	0.0226***
	(0.0043)
15% Discount × Rural	-0.0157***
	(0.0058)
40% Discount × Urban	0.0795***
	(0.0058)
40% Discount × Rural	0.0343***
	(0.0082)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,427,940
Number of Accounts	79,330
R^2	0.00126

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A10: Impact of the experimental stimuli on kWh charged interacted with Gross Disposable Income (£)

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message × < £19k	-0.0043
	(0.0058)
Green Message × < £21k	-0.0075
	(0.0058)
Green Message × < £23k	-0.0119**
	(0.0056)
Green Message × < £26k	-0.0037
	(0.0063)
Green Message × £26k+	0.0212***
	(0.0069)
15% Discount × < £19k	0.0209***
	(0.0076)
15% Discount × < £21k	0.0072
	(0.0067)
15% Discount × < £23k	0.0112
	(0.0071)
15% Discount × < £26k	0.0053
	(0.0068)
15% Discount × £26k+	0.0317***
	(0.0077)
40% Discount × < £19k	0.0934***
	(0.0113)
40% Discount × < £21k	0.0715***
	(0.0106)
40% Discount × < £23k	0.0510***
	(0.0091)
40% Discount × < £26k	0.0468***
	(0.0097)
40% Discount × £26k+	0.0909***
	(0.0111)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,427,940
Number of Accounts	79,330
R^2	0.00126

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A11: Impact of the experimental stimuli on kWh charged interacted with EV Price New (£)

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message × < £30k	-0.0043
	(0.0051)
Green Message × < £36k	0.0076
	(0.0058)
Green Message × < £43k	-0.0027
	(0.0054)
Green Message × < £51k	-0.0049
	(0.0054)
Green Message × £51k+	0.0032
	(0.0065)
15% Discount × < £30k	0.0021
	(0.0054)
15% Discount × < £36k	0.0209***
	(0.0066)
15% Discount × < £43k	0.0192***
	(0.0070)
15% Discount × < £51k	0.0238***
	(0.0073)
15% Discount × £51k+	0.0188**
	(0.0080)
40% Discount × < £30k	0.0503***
	(0.0081)
40% Discount × < £36k	0.0690***
	(0.0084)
40% Discount × < £43k	0.0666***
	(0.0093)
40% Discount × < £51k	0.0845***
	(0.0111)
40% Discount × £51k+	0.0720***
	(0.0110)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,498,788
Number of Accounts	83,266
R^2	0.00123

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A12: Impact of the experimental stimuli on kWh charged interacted with EV Price Used (£)

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message × < £24k	-0.0056
	(0.0049)
Green Message × < £29k	0.0102*
	(0.0061)
Green Message × < £35k	-0.0029
	(0.0054)
Green Message × < £40k	-0.0049
	(0.0054)
Green Message × £40k+	0.0032
	(0.0065)
15% Discount × < £24k	0.0017
	(0.0053)
15% Discount × < £29k	0.0225***
	(0.0068)
15% Discount × < £35k	0.0193***
	(0.0071)
15% Discount × < £40k	0.0238***
	(0.0073)
15% Discount × £40k+	0.0188**
	(0.0080)
40% Discount × < £24k	0.0466***
	(0.0078)
40% Discount × < £29k	0.0741***
	(0.0089)
40% Discount × < £35k	0.0669***
	(0.0094)
40% Discount × < £40k	0.0845***
	(0.0111)
40% Discount × £40k+	0.0720***
	(0.0110)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,498,788
Number of Accounts	83,266
R^2	0.00124

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A13: Impact of the experimental stimuli on kWh charged interacted with Time of Day

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message × Morning	-0.0010
	(0.0069)
Green Message × Afternoon	-0.0002
	(0.0063)
Green Message × Evening	0.0030
	(0.0038)
Green Message × Night	-0.0007
	(0.0027)
15% Discount × Morning	0.0237***
	(0.0074)
15% Discount × Afternoon	0.0158**
	(0.0069)
15% Discount × Evening	0.0154***
	(0.0045)
15% Discount × Night	-0.0010
	(0.0029)
40% Discount × Morning	0.0844***
	(0.0087)
40% Discount × Afternoon	0.0698***
	(0.0086)
40% Discount × Evening	0.0568***
	(0.0056)
40% Discount × Night	0.0112***
	(0.0038)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,974,798
Number of Accounts	109,711
R^2	0.00105

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A14: Impact of the experimental stimuli on kWh charged interacted with Weekend

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message × Weekday	0.0021
	(0.0035)
Green Message × Weekend	-0.0011
	(0.0041)
15% Discount × Weekday	0.0140***
	(0.0039)
15% Discount × Weekend	0.0160***
	(0.0045)
40% Discount × Weekday	0.0510***
	(0.0048)
40% Discount × Weekend	0.0687***
	(0.0056)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,974,798
Number of Accounts	109,711
R^2	0.00102

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A15: Impact of the experimental stimuli on kWh charged interacted with Battery Capacity (kWh)

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message × < 51kWh	-0.0058
	(0.0046)
Green Message × < 62kWh	-0.0032
	(0.0053)
Green Message × < 72kWh	0.0101*
	(0.0059)
Green Message × < 77kWh	7.96×10^{-5}
	(0.0053)
Green Message × 77kWh+	-0.0015
	(0.0068)
15% Discount × < 51kWh	0.0029
	(0.0051)
15% Discount × < 62kWh	0.0180***
	(0.0065)
15% Discount × < 72kWh	0.0243***
	(0.0065)
15% Discount × < 77kWh	0.0180***
	(0.0065)
15% Discount × 77kWh+	0.0313***
	(0.0096)
40% Discount × < 51kWh	0.0421***
	(0.0070)
40% Discount × < 62kWh	0.0583***
	(0.0083)
40% Discount × < 72kWh	0.0758***
	(0.0093)
40% Discount × < 77kWh	0.0765***
	(0.0093)
40% Discount × 77kWh+	0.1018***
	(0.0139)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,605,636
Number of Accounts	89,202
R^2	0.00126

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A16: Impact of the experimental stimuli on kWh charged interacted with Battery Capacity (kWh)

Dependent Variable:	Has Charged (binary)
Model:	(1)
Variables	
Green Message × < 51kWh	0.0005**
	(0.0002)
Green Message × < 62kWh	1.11×10^{-5}
	(0.0002)
Green Message × < 72kWh	0.0001
	(0.0002)
Green Message × < 77kWh	-0.0003
	(0.0002)
Green Message × 77kWh+	-0.0006***
	(0.0002)
15% Discount × < 51kWh	0.0009***
	(0.0002)
15% Discount × < 62kWh	0.0007***
	(0.0002)
15% Discount × < 72kWh	0.0006***
	(0.0002)
15% Discount × < 77kWh	6.06×10^{-5}
	(0.0002)
15% Discount × 77kWh+	0.0003
	(0.0002)
40% Discount × < 51kWh	0.0026***
	(0.0003)
40% Discount × < 62kWh	0.0021***
	(0.0003)
40% Discount × < 72kWh	0.0021***
	(0.0003)
40% Discount × < 77kWh	0.0017***
	(0.0002)
40% Discount × 77kWh+	0.0017***
	(0.0003)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,605,636
Number of Accounts	89,202
R^2	0.00167

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A17: Impact of the experimental stimuli on kWh charged interacted with User Profile

Dependent Variable:	Has Charged (binary)
Model:	(1)
Variables	
Green Message × Local fast-charging users	0.0019***
	(0.0003)
Green Message × Local slow-charging users	0.0043***
	(0.0010)
Green Message × Long-distance users	-0.0010***
	(0.0002)
15% Discount × Local fast-charging users	0.0032***
	(0.0003)
15% Discount × Local slow-charging users	0.0037***
	(0.0010)
15% Discount × Long-distance users	-0.0006***
	(0.0002)
40% Discount × Local fast-charging users	0.0076***
	(0.0005)
40% Discount × Local slow-charging users	0.0057***
	(0.0011)
40% Discount × Long-distance users	0.0005**
	(0.0002)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	981,846
Number of Accounts	54,547
R^2	0.00325

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A18: Impact of the experimental stimuli on kWh charged interacted with Active User

Dependent Variable:	Has Charged (binary)
Model:	(1)
Variables	
Green Message × Inactive	-0.0015***
	(7.94×10^{-5})
Green Message × Active	0.0017***
	(0.0001)
15% Discount × Inactive	-0.0013***
	(8.69×10^{-5})
15% Discount × Active	0.0023***
	(0.0002)
40% Discount × Inactive	-0.0008***
	(0.0001)
40% Discount × Active	0.0046***
	(0.0002)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,974,798
Number of Accounts	109,711
R^2	0.00263

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A19: Impact of the experimental stimuli on kWh charged interacted with Distance to Nearest Charger

Dependent Variable:	Has Charged (binary)
Model:	(1)
Variables	
Green Message × <500m	0.0014***
	(0.0002)
Green Message × <1km	-9.47×10^{-5}
	(0.0002)
Green Message × <2.5km	-0.0004**
	(0.0002)
Green Message × <5km	-0.0010***
	(0.0002)
Green Message × <10km	-0.0011***
	(0.0002)
Green Message × 10km+	0.0006
	(0.0012)
15% Discount × <500m	0.0022***
	(0.0003)
15% Discount × <1km	0.0004**
	(0.0002)
15% Discount × <2.5km	-4.53×10^{-5}
	(0.0002)
15% Discount × <5km	-0.0006***
	(0.0002)
15% Discount × <10km	-4.02×10^{-5}
	(0.0003)
15% Discount × 10km+	-0.0009
	(0.0007)
40% Discount × <500m	0.0038***
	(0.0003)
40% Discount × <1km	0.0024***
	(0.0003)
40% Discount × <2.5km	0.0016***
	(0.0002)
40% Discount × <5km	0.0009***
	(0.0003)
40% Discount × <10km	0.0005
	(0.0004)
40% Discount × 10km+	0.0020
	(0.0014)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,430,352
Number of Accounts	79,464
R^2	0.00188

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A20: Impact of the experimental stimuli on kWh charged interacted with Distance to Nearest Participating Charger

Dependent Variable:	Has Charged (binary)
Model:	(1)
Variables	
Green Message × <500m	0.0004
	(0.0004)
Green Message × <1km	0.0002
	(0.0003)
Green Message × <2.5km	0.0004**
	(0.0002)
Green Message × <5km	-0.0003
	(0.0002)
Green Message × <10km	-0.0007***
	(0.0002)
Green Message × 10km+	-0.0006*
	(0.0004)
15% Discount × <500m	0.0019***
	(0.0005)
15% Discount × <1km	0.0010***
	(0.0003)
15% Discount × <2.5km	0.0006***
	(0.0002)
15% Discount × <5km	0.0003
	(0.0002)
15% Discount × <10km	-0.0004**
	(0.0002)
15% Discount × 10km+	0.0002
	(0.0004)
40% Discount × <500m	0.0047***
	(0.0007)
40% Discount × <1km	0.0032***
	(0.0004)
40% Discount × <2.5km	0.0027***
	(0.0003)
40% Discount × <5km	0.0015***
	(0.0003)
40% Discount × <10km	0.0005**
	(0.0003)
40% Discount × 10km+	0.0008
	(0.0005)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,401,156
Number of Accounts	77,842
R^2	0.00180

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A21: Impact of the experimental stimuli on kWh charged interacted with

Dependent Variable:	Has Charged (binary)
Model:	(1)
Variables	
Green Message × Urban	0.0001
	(0.0001)
Green Message × Rural	-0.0007***
	(0.0002)
15% Discount × Urban	0.0007***
	(0.0001)
15% Discount × Rural	-0.0005***
	(0.0002)
40% Discount × Urban	0.0024***
	(0.0002)
40% Discount × Rural	0.0010***
	(0.0003)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,427,940
Number of Accounts	79,330
R^2	0.00172

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A22: Impact of the experimental stimuli on kWh charged interacted with Gross Disposable Income (£)

Dependent Variable:	Has Charged (binary)
Model:	(1)
Variables	
Green Message × < £19k	-4.31×10^{-5}
	(0.0002)
Green Message × < £21k	-0.0003
	(0.0002)
Green Message × < £23k	-0.0004**
	(0.0002)
Green Message × < £26k	-0.0002
	(0.0002)
Green Message × £26k+	0.0008***
	(0.0002)
15% Discount × < £19k	0.0007***
	(0.0002)
15% Discount × < £21k	0.0002
	(0.0002)
15% Discount × < £23k	0.0003
	(0.0002)
15% Discount × < £26k	0.0002
	(0.0002)
15% Discount × £26k+	0.0011***
	(0.0002)
40% Discount × < £19k	0.0029***
	(0.0003)
40% Discount × < £21k	0.0020***
	(0.0003)
40% Discount × < £23k	0.0016***
	(0.0003)
40% Discount × < £26k	0.0014***
	(0.0003)
40% Discount × £26k+	0.0027***
	(0.0003)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,427,940
Number of Accounts	79,330
R^2	0.00172

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A23: Impact of the experimental stimuli on kWh charged interacted with EV Price New (£)

Dependent Variable:	Has Charged (binary)
Model:	(1)
Variables	
Green Message × < £30k	0.0004*
	(0.0002)
Green Message × < £36k	0.0004**
	(0.0002)
Green Message × < £43k	-0.0002
	(0.0002)
Green Message × < £51k	-0.0004**
	(0.0002)
Green Message × £51k+	-0.0002
	(0.0002)
15% Discount × < £30k	0.0007***
	(0.0002)
15% Discount × < £36k	0.0010***
	(0.0002)
15% Discount × < £43k	0.0004*
	(0.0002)
15% Discount × < £51k	0.0003*
	(0.0002)
15% Discount × £51k+	3.51×10^{-5}
	(0.0002)
40% Discount × < £30k	0.0024***
	(0.0003)
40% Discount × < £36k	0.0029***
	(0.0003)
40% Discount × < £43k	0.0018***
	(0.0003)
40% Discount × < £51k	0.0019***
	(0.0003)
40% Discount × £51k+	0.0011***
	(0.0003)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,498,788
Number of Accounts	83,266
R^2	0.00169

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A24: Impact of the experimental stimuli on kWh charged interacted with EV Price Used (£)

Dependent Variable:	Has Charged (binary)
Model:	(1)
Variables	
Green Message × < £24k	0.0003*
	(0.0002)
Green Message × < £29k	0.0004**
	(0.0002)
Green Message × < £35k	-0.0002
	(0.0002)
Green Message × < £40k	-0.0004**
	(0.0002)
Green Message × £40k+	-0.0002
	(0.0002)
15% Discount × < £24k	0.0007***
	(0.0002)
15% Discount × < £29k	0.0010***
	(0.0002)
15% Discount × < £35k	0.0004*
	(0.0002)
15% Discount × < £40k	0.0003*
	(0.0002)
15% Discount × £40k+	3.51×10^{-5}
	(0.0002)
40% Discount × < £24k	0.0023***
	(0.0003)
40% Discount × < £29k	0.0030***
	(0.0003)
40% Discount × < £35k	0.0018***
	(0.0003)
40% Discount × < £40k	0.0019***
	(0.0003)
40% Discount × £40k+	0.0011***
	(0.0003)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,498,788
Number of Accounts	83,266
R^2	0.00169

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A25: Impact of the experimental stimuli on kWh charged interacted with Time of Day

Dependent Variable:	Has Charged (binary)
Model:	(1)
Variables	
Green Message × Morning	-2.09×10^{-5}
	(0.0002)
Green Message × Afternoon	-2.82×10^{-5}
	(0.0002)
Green Message × Evening	0.0001
	(0.0001)
Green Message × Night	-4.89×10^{-5}
	(8.5×10^{-5})
15% Discount × Morning	0.0008***
	(0.0002)
15% Discount × Afternoon	0.0005**
	(0.0002)
15% Discount × Evening	0.0003***
	(0.0001)
15% Discount × Night	-8.25×10^{-5}
	(8.28×10^{-5})
40% Discount × Morning	0.0026***
	(0.0003)
40% Discount × Afternoon	0.0022***
	(0.0003)
40% Discount × Evening	0.0016***
	(0.0002)
40% Discount × Night	0.0002**
	(0.0001)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,974,798
Number of Accounts	109,711
R^2	0.00142

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A26: Impact of the experimental stimuli on kWh charged interacted with Weekend

Dependent Variable:	Has Charged (binary)
Model:	(1)
Variables	
Green Message × Weekday	2.02×10^{-5}
	(0.0001)
Green Message × Weekend	5.01×10^{-6}
	(0.0001)
15% Discount × Weekday	0.0004***
	(0.0001)
15% Discount × Weekend	0.0005***
	(0.0001)
40% Discount × Weekday	0.0015***
	(0.0001)
40% Discount × Weekend	0.0021***
	(0.0002)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	1,974,798
Number of Accounts	109,711
R^2	0.00139

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

A2.4 Consumption during events by charger speed

Table A27: Impact of the experimental stimuli on kWh charged by EVSE speed

Dependent Variables:	Slow (kWh)	Fast (kWh)	Rapid (kWh)	Ultra (kWh)
Model:	(1)	(2)	(3)	(4)
Variables				
Green Message	0.0005	-0.0009	0.0010	0.0031
	(0.0010)	(0.0025)	(0.0023)	(0.0019)
15% Discount	0.0011	0.0106***	0.0099***	0.0110***
	(0.0012)	(0.0028)	(0.0026)	(0.0022)
40% Discount	0.0009	0.0568***	0.0343***	0.0290***
	(0.0011)	(0.0038)	(0.0032)	(0.0026)
kWh - September	3.55×10^{-5}	0.0004***	0.0004***	0.0004***
	(4.53×10^{-5})	(9.19×10^{-5})	(9.4×10^{-5})	(9.63×10^{-5})
kWh - October	4.2×10^{-6}	1.34×10^{-5}	6.78×10^{-6}	1.73×10^{-5}
	(5.04×10^{-6})	(1.13×10^{-5})	(5.99×10^{-6})	(1.49×10^{-5})
kWh - November	0.0005***	0.0015***	0.0014***	0.0012***
	(7.86×10^{-5})	(0.0001)	(0.0001)	(9.38×10^{-5})
Active User	-0.0020	0.0354***	0.0192***	0.0118***
	(0.0017)	(0.0030)	(0.0028)	(0.0025)
Fixed-effects				
Date	Yes	Yes	Yes	Yes
Avg. Outcome				
Control	0.0073	0.0463	0.0383	0.0259
Fit statistics				
Observations	1,974,798	1,974,798	1,974,798	1,974,798
Number of Accounts	109,711	109,711	109,711	109,711
R^2	0.00312	0.00568	0.00527	0.00579

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
This table show consumption in kWh by speed categories as follows:
Slow (≤7kW), Fast (<50kW), Rapid (<150kW), and Ultra (>150kW).

A2.5 Spatial displacement

Table A28: Impact of the experimental stimuli on kWh charged, by participating CPOs

Dependent Variable:	Consumption (kWh)	
	Participating CPOs	Non-Participating CPOs
Model:	(1)	(2)
Variables		
Green Message	-0.0012	0.0027
	(0.0017)	(0.0021)
15% Discount	0.0124***	0.0031
	(0.0021)	(0.0022)
40% Discount	0.0608***	-0.0004
	(0.0034)	(0.0021)
kWh - September	0.0002***	0.0003***
	(6.19×10^{-5})	(9×10^{-5})
kWh - October	1.26×10^{-5}	3.94×10^{-6}
	(1.35×10^{-5})	(5.24×10^{-6})
kWh - November	0.0007***	0.0011***
	(8.31×10^{-5})	(8.37×10^{-5})
Active User	0.0283***	0.0061***
	(0.0025)	(0.0022)
Fixed-effects		
Date	Yes	Yes
Avg. Outcome		
Control	0.0215	0.0302
Fit statistics		
Observations	1,974,798	1,974,798
Number of Accounts	109,711	109,711
R^2	0.00267	0.00560
Pseudo R^2	0.00082	0.00191
F-test	12.226	25.727

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
The estimates of this table refer to the charging spatial displacement checks indicated in our pre-registration plan, exploratory analysis 3.

A2.6 Investigating habit formation: new location & return visits

Table A29 reports regression results estimating the likelihood that a charging session occurs at a new location, using treatment group assignment as the explanatory variable (using simple linear probability model). The analysis mirrors the main regressions on overall charging activity: we find no significant effect for the Green Message group, while both the 15% and 40% price reduction treatments lead to statistically significant increases in the probability of visiting a new location. These effects are consistent when estimated separately by trial block, suggesting no evidence of carry-over effects across blocks. The results indicate that financial incentives not only increase charging activity, but also encourage users to explore new locations, possibly reflecting willing to travel or trying new stations. To be noted, 59% of first visit to a location are also the first time using a charger of this CPO.

Table A29: Impact on First Location Visit During the Trial

Dependent Variable:		First Visit		
block	Full sample	Block 1	Block 2	Block 3
Model:	(1)	(2)	(3)	(4)
Variables				
Green Message	-1.51×10^{-5}	4.21×10^{-5}	-0.0001	4.19×10^{-6}
	(5.9×10^{-5})	(8.98×10^{-5})	(0.0001)	(0.0001)
15% Discount	0.0002**	0.0002**	7.77×10^{-5}	0.0001
	(6.22×10^{-5})	(9.51×10^{-5})	(0.0001)	(0.0001)
40% Discount	0.0008***	0.0009***	0.0007***	0.0009***
	(7.3×10^{-5})	(0.0001)	(0.0001)	(0.0002)
kWh - September	1.43×10^{-6}	2.14×10^{-6}	-6.72×10^{-7}	3.18×10^{-6}*
	(9.26×10^{-7})	(1.44×10^{-6})	(1.47×10^{-6})	(1.89×10^{-6})
kWh - October	1.36×10^{-8}	-1.99×10^{-8}*	-3.3×10^{-8}*	1.51×10^{-7}
	(4.2×10^{-8})	(1.08×10^{-8})	(1.85×10^{-8})	(1.97×10^{-7})
kWh - November	7.62×10^{-6}***	5.86×10^{-6}***	1.03×10^{-5}***	7.15×10^{-6}***
	(9.83×10^{-7})	(1.36×10^{-6})	(1.9×10^{-6})	(2.12×10^{-6})
Active User	0.0010***	0.0011***	0.0010***	0.0008***
	(5.16×10^{-5})	(7.81×10^{-5})	(8.74×10^{-5})	(0.0001)
Fixed-effects				
Date	Yes	Yes	Yes	Yes
Avg. Outcome				
Control	0.0009	0.0009	0.0009	0.0008
Fit statistics				
Observations	1,974,798	877,688	658,266	438,844
Number of Accounts	109,711	109,711	109,711	109,711
R^2	0.00151	0.00217	0.00101	0.00090

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

To further examine whether dynamic pricing events foster sustained changes in user behavior, we analyze the likelihood of returning to the same location within 30 days following a customer's first visit. The unit of observation is the first visit by an account to a given location, and the analysis excludes any return visits that occurred during subsequent dynamic pricing events, to isolate the effect of habit formation from short-term price responsiveness. The results indicate that customers whose first visit occurred during a dynamic pricing event are significantly less likely to return within 30 days than those whose initial visit happened outside the trial period. Specifically, only 14% of first visits during a dynamic pricing event are followed by a return within 30 days, compared to 22% when the initial visit occurs outside the trial. This difference is reflected in the regression results presented in Table A30, where the coefficient on "First Visit During Trial" is negative and statistically significant. When return visits during subsequent price events are included, the coefficient becomes positive, suggesting that repeated discounting can temporarily increase returns, but without these follow-up events, there is little evidence that the initial discount fosters long-term behavioral change. Overall, the findings suggest that while dynamic pricing events may attract first-time visits, they do not appear to translate into habitual usage.

Table A30: Impact on Return Visits Within 30 Days

Dependent Variable:	Returned Within 30 Days
Model:	(1)
Variables	
First Visit During Trial	-0.0168**
	(0.0084)
kWh - September	6.68×10^{-5}*
	(3.58×10^{-5})
kWh - October	2.01×10^{-6}
	(1.55×10^{-6})
kWh - November	0.0002***
	(1.33×10^{-5})
Active User	-0.0309***
	(0.0031)
Fixed-effects	
Date	Yes
Fit statistics	
Observations	417,184
Number of Accounts	67,341
R^2	0.02422

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

A2.7 Temporal displacement

Table A31: kWh charged between notification and event start

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message	0.0103
	(0.0079)
15% Discount	0.0157*
	(0.0081)
40% Discount	0.0087
	(0.0085)
User Characteristics	Yes
Fixed-effects	
Date	Yes
Avg. Outcome	
Control	0.3670
Fit statistics	
Observations	1,974,798
Number of Accounts	109,711
R^2	0.05833

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
The estimates in this table refer to robustness checks outlined in our pre-registered analysis plan,related to the temporal displacement of charging behavior in Exploratory Analysis 2.

A2.8 Home electricity consumption

Table A32: Impact on Home Consumption (kWh)

Dependent Variable:	Consumption (kWh)
Model:	(1)
Variables	
Green Message	0.0435
	(0.0870)
15% Discount	-0.0428
	(0.1173)
40% Discount	0.0436
	(0.1474)
kWh - September	-0.0013
	(0.0030)
kWh - October	0.00007
	(0.00005)
kWh - November	0.0006
	(0.0027)
Active User	1.057
	(2.832)
Fixed-effects	
Date	Yes
Avg. Outcome	
Control	13.9844
Fit statistics	
Observations	29,141,254
Number of Accounts	17,188
R^2	0.00332

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
The estimates in this table refer to robustness checks outlined in our pre-registered analysis plan.

A2.9 Electricity consumption during wash-out periods

Table A33: Impact of the experimental stimuli on kWh charged

Dependent Variables:	Consumption (kWh)	Has Charged (binary)		
Model:	(1)	(2)	(3)	(4)
	OLS	OLS	Probit	Marginal effects
Variables				
Green Message	0.0174	0.0004	0.0161	0.0005
	(0.0120)	(0.0003)	(0.0099)	(0.0003)
15% Discount	0.0078	0.0003	0.0067	0.0002
	(0.0119)	(0.0003)	(0.0100)	(0.0003)
40% Discount	0.0147	0.0003	0.0099	0.0003
	(0.0130)	(0.0004)	(0.0109)	(0.0004)
kWh - September	0.0050^{***}	0.0001^{***}	0.0008^{***}	
	(0.0007)	(1.96×10^{-5})	(0.0002)	
kWh - October	5.65×10^{-5}	1.58×10^{-6}	$8.75\times10^{-6***}$	
	(3.72×10^{-5})	(1.09×10^{-6})	(3.22×10^{-6})	
kWh - November	0.0164^{***}	0.0005^{***}	0.0028^{***}	
	(0.0006)	(1.53×10^{-5})	(0.0001)	
Active User	0.0863^{***}	0.0071^{***}	0.5423^{***}	
	(0.0162)	(0.0004)	(0.0115)	
Fixed-effects				
Date	Yes	Yes	Yes	
Avg. Outcome				
Control	0.4579	0.0158	0.0158	
Fit statistics				
Observations	1,316,532	1,316,532	1,316,532	
Number of Accounts	109,711	109,711	109,711	
R^2	0.06346	0.06594		
Pseudo R^2	0.01129	-0.05174	0.14657	
F-test	412.97	430.27		

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
The estimates in this table refer to robustness checks not included in our pre-analysis plan, conducted to test for the presence of carryover effects in the crossover trial.

A2.10 Descriptive statistics

Table A34: Descriptive characteristics of the sample

	Sequence				Total	Kruskal–Wallis	
	1	2	3	4		χ^2	p
Consumption (kWh) – Sep	12.91 (48.78)	13.12 (59.14)	12.91 (49.07)	12.75 (49.73)	12.92 (51.86)	4.144	0.246
Consumption (kWh) – Oct	17.07 (262.91)	17.82 (494.83)	24.65 (1010.6)	14.45 (61.77)	18.51 (580.38)	1.616	0.656
Consumption (kWh) – Nov	14.4 (57.76)	14.41 (58.67)	14.51 (56.85)	13.9 (58.56)	14.3 (57.96)	3.968	0.265
Consumption (kWh) – Last Year	120.9 (502.06)	121.38 (671.74)	129.59 (1092.75)	115.7 (387.34)	121.9 (716.8)	4.821	0.185
Expenditures (£) – Last Year	80.38 (316.02)	80.94 (417.58)	84.32 (584.02)	77.8 (255.78)	80.86 (413.11)	4.996	0.172
Days since registration	488.69 (283.19)	493.55 (285.25)	491.48 (283.34)	493.85 (285.86)	491.91 (284.43)	5.946	0.114
Transactions – Last Year	5.01 (15.05)	5.11 (17.8)	5.11 (15.71)	4.95 (15.44)	5.05 (16.04)	4.834	0.184
Locations visited – Last Year	2.06 (4.15)	2.03 (4.13)	2.07 (4.07)	2.02 (4.09)	2.04 (4.11)	5.271	0.153
Consumers	26,805	27,448	27,685	27,773	109,711		

Notes: Cells report means with standard deviations in parentheses. Treatment sequence assignments are detailed in Table 1. Kruskal–Wallis tests assess equality of distributions across sequences.

Table A35: Descriptive characteristics of transactions

	Control	Green message	15% price decrease	40% price decrease	Total	χ^2
kWh charged	25.74 (15.52)	25.86 (15.53)	27.4 (16.74)	29.49 (17.01)	27.54 (16.45)	42.729***
Expenditures (£)	17.98 (11.87)	17.64 (11.77)	17.77 (11.41)	15.31 (9.81)	16.88 (11.08)	45.524***
Average price paid (£/kWh)	0.7 (0.3)	0.68 (0.21)	0.65 (0.16)	0.54 (0.57)	0.63 (0.39)	1123.121***
Number of observations	1,198	1,863	992	1,007	5,060	

Notes: Cells report means with standard deviations in parentheses. Treatment assignments follow the Latin Square design shown in Table 1. χ^2 values are from Kruskal–Wallis tests for equality of distributions across groups. Observations reflect only transactions in which users chose to charge during dynamic pricing events, so comparisons capture behavioral patterns among chargers rather than causal treatment effects. Price variation is influenced by CPO participation and consumer charger choice; as a result, realized price decreases differ from the nominal 15% and 40% values associated with treatment assignments.

A2.11 Notification and unsubscription analysis

To evaluate notification outcomes and user unsubscription behavior, we constructed an event-level user panel combining attempted notifications, unsubscribe events, treatment assignment, and baseline consumption covariates. Using this panel, we calculated and plotted the share of users who received a notification and those who unsubscribed, separately by treatment arm (Figure A21 and Figure A22). We then estimated the effect of each treatment on unsubscription using linear and probit regressions with date fixed effects and user-level clustering. Table A37 presents the results from the OLS specification

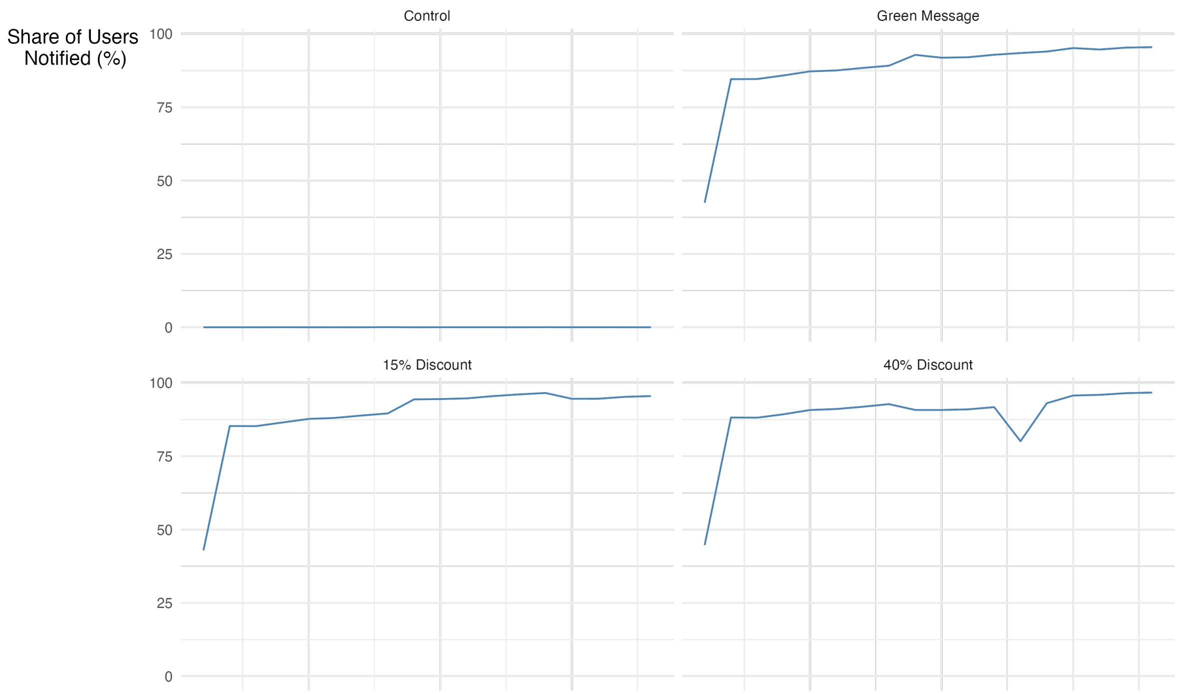
(Column 1) and the probit model (Column 2), as well as average marginal effects from the probit model (Column 3), showing how each message type affected opt-out rates.

Figure A21 shows the share of users who were successfully notified on each event date, by treatment group. During the first notification event, a technical error prevented approximately half of the users in the treated groups from being contacted. We repeated our main analysis in Table A36, where we excluded from our sample for Event 1 those participants who were not in the trial notification list in the first event of the trial (irrespective of their group membership). Results were extremely similar to our main analysis (Table A3).

This issue was identified and fully resolved before the second event, after which notification rates rose sharply and remained above 90% for all treatment groups. The control group was not intended to receive notifications and is shown here for reference. A small number of users — fewer than 10 per event — received notifications intended for other treatment arms. These errors were typically associated with multiple accounts linked to the same device or phone number. In the analysis, we kept the original assignment as treatment. Figure A22 plots the share of users unsubscribing from future messages following each notification. Unsubscription rates were near zero for the control group but notably higher for all treated groups. The green message treatment consistently triggered the highest opt-out rates, followed by the 15% and 40% Discount groups. These patterns suggest that opt-out behavior was responsive to the type of message received, with non-financial appeals (like the green framing) generating more resistance than discount-based incentives. However, overall opt-out rates were very low across all treatments.

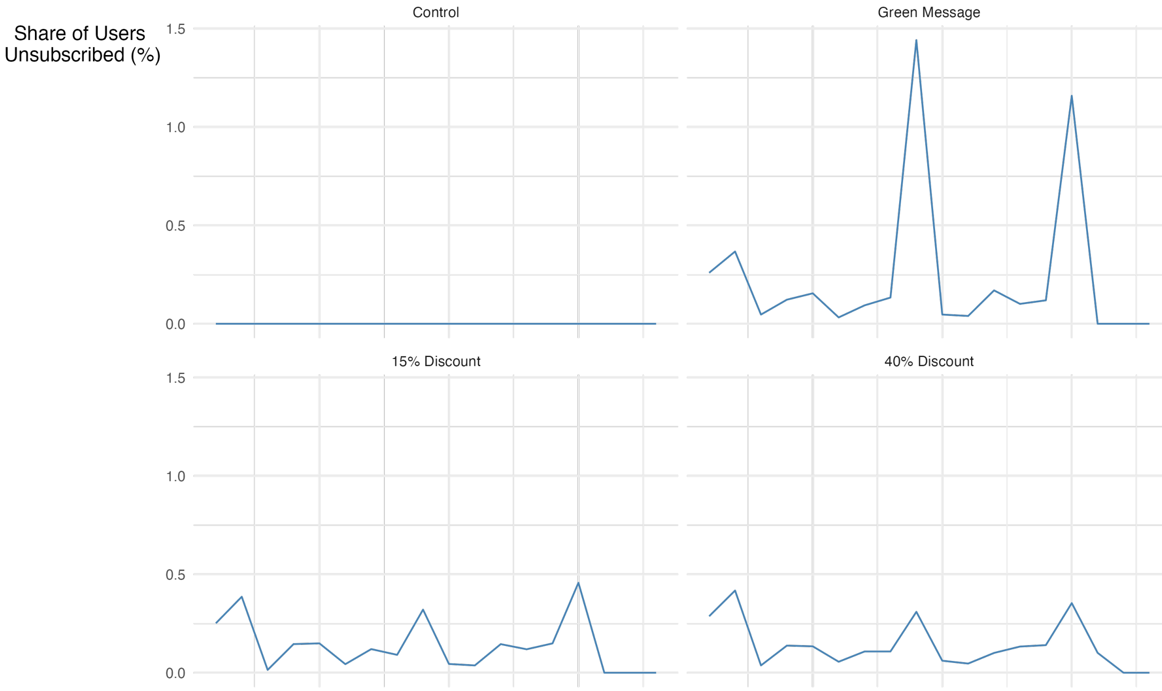
Table A37 presents estimates of the impact of different notification treatments on users' likelihood of unsubscribing. Column (1) reports results from a linear probability model with date fixed effects and standard errors clustered at the user level. Columns (2) and (3) report results from a probit model, with Column (3) showing average marginal effects derived from the specification in Column (2). In the probit model with date fixed effects, two date groups with no variation in unsubscription outcomes were automatically dropped. This excluded 219,422 observations, which do not contribute to estimation in nonlinear fixed effects models. Across specifications, all treatments significantly increased the likelihood of unsubscription relative to the control group. The green message treatment had the largest effect, increasing the probability of unsubscribing by approximately 0.26 percentage points. The 15% and 40% Discount treatments each increased unsubscription by about 0.16 percentage points. These marginal effects were small in absolute terms. All models included date fixed effects to account for time-specific factors influencing opt-out behavior. We kept unsubscribed users in all of our analyses following their original assignments.

Figure A21: Share of users notified (or attempted)



Notes: This figure shows the share of users in each treatment arm for whom a notification was attempted or successfully delivered on each event date. Notification rates were below 50% during the first event due to a technical issue, which was resolved before the second event. See Table A36 for a regression analysis showing that our results are robust to the removing non-contacted customers (including those who *would* not have been contacted, in the Control group). Control users were not meant to receive notifications and are shown for comparison only. A small number of users were sent incorrect messages due to account linkage errors; we maintain these users in their original treatment allocation in our analysis.

Figure A22: Unsubscription rate



Notes: This figure shows the share of users in each treatment arm who unsubscribed from notifications following each event. Unsubscription was nearly zero in the control group and highest in the green message group, followed by the 15% and 40% Discount groups. Despite variation across treatments, absolute unsubscription rates remained low overall.

Table A36: Robustness check: Impact of the experimental stimuli on kWh charged omitting Event 1 participants not in the notification list

Dependent Variable:	Consumption (kWh)	
Sample	Full sample	Excluding non contacted customers
Model:	(1)	(2)
Variables		
Treatment = GreenMessage	0.0015	0.0012
	(0.0027)	(0.0027)
Treatment = 15%Discount	0.0154***	0.0155***
	(0.0030)	(0.0031)
Treatment = 40%Discount	0.0604***	0.0619***
	(0.0039)	(0.0040)
kWh - September	0.0006***	0.0006***
	(0.0001)	(0.0001)
kWh - October	1.66×10^{-5}	1.66×10^{-5}
	(1.43×10^{-5})	(1.43×10^{-5})
kWh - November	0.0018***	0.0018***
	(0.0001)	(0.0001)
Active User	0.0344***	0.0355***
	(0.0032)	(0.0033)
Fixed-effects		
Date	Yes	Yes
Avg. Outcome		
Control	0.0517	0.0517
Fit statistics		
Observations	1,974,798	1,928,319
Number of Accounts	109,711	109,711
R^2	0.00716	0.00717

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
The analysis excludes from Event 1 only those participants who were not in the trial notification list in the first event of the trial if they were allocated in one of the three treatment arms.

Table A37: Unsubscription rate by treatment

Dependent Variable:	Unsubscribed	
Model:	(1)	(2)
	OLS	Probit
Variables		
Green Message	0.0024***	3.719***
	(6.89×10^{-5})	(0.0123)
15% Discount	0.0014***	3.552***
	(5.28×10^{-5})	(0.0129)
40% Discount	0.0014***	3.562***
	(5.34×10^{-5})	(0.0125)
kWh - September	-3.7×10^{-7}	-3.05×10^{-5}
	(5.52×10^{-7})	(0.0002)
kWh - October	$-1.25 \times 10^{-8***}$	-0.0001
	(3.87×10^{-9})	(0.0002)
kWh - November	$-1.57 \times 10^{-6***}$	-0.0005**
	(4.48×10^{-7})	(0.0002)
Activity	-0.0029***	-0.4248***
	(0.0008)	(0.0756)
Fixed-effects		
Date	Yes	Yes
Fit statistics		
Observations	1,974,798	1,755,376
Number of Accounts	109,711	109,711
R^2	0.00234	
BIC	-7,537,653.7	34,871.9

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

A2.12 Main analysis excluding customers pre-specified in the PAP

Table A38: Impact of the experimental stimuli on kWh charged

Dependent Variables:	Consumption (kWh)	Has Charged (binary)		
Model:	(1)	(2)	(3)	(4)
	OLS	OLS	Probit	Marginal effects
Variables				
Green Message	0.0013	4.27×10^{-5}	0.0064	3.65×10^{-5}
	(0.0027)	(8.9×10^{-5})	(0.0154)	(8.88×10^{-5})
15% Discount	0.0155***	0.0004***	0.0695***	0.0004***
	(0.0030)	(9.67×10^{-5})	(0.0152)	(0.0001)
40% Discount	0.0602***	0.0018***	0.2343***	0.0018***
	(0.0039)	(0.0001)	(0.0146)	(0.0002)
kWh - September	0.0006***	1.63×10^{-5}***	0.0004***	
	(0.0001)	(3.12×10^{-6})	(8.09×10^{-5})	
kWh - October	1.65×10^{-5}	4.95×10^{-7}	9.25×10^{-6}***	
	(1.43×10^{-5})	(4.36×10^{-7})	(3.24×10^{-6})	
kWh - November	0.0018***	5.89×10^{-5}***	0.0016***	
	(0.0001)	(3.09×10^{-6})	(6.11×10^{-5})	
Active User	0.0351***	0.0017***	0.4933***	
	(0.0032)	(9.31×10^{-5})	(0.0143)	
Fixed-effects				
Date	Yes	Yes	Yes	
Avg. Outcome				
Control	0.0512	0.0020	0.0020	
Fit statistics				
Observations	1,968,858	1,968,858	1,968,858	
Number of Accounts	109,381	109,381	109,381	
R^2	0.00729	0.00826		
Pseudo R^2	0.00193	-0.00264	0.12193	
F-test	33.474	37.967		

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Estimates in this table refer to a non specified robustness checks excluding customers that joined after the start of the trial, are fleet or super users.

A2.13 Main analysis using individual fixed effects

Table A39: Impact of the experimental stimuli on kWh charged

Dependent Variables:	Consumption (kWh)	Has Charged (binary)		
Model:	(1)	(2)	(3)	(4)
	OLS	OLS	Probit	Marginal effects
Variables				
Green Message	-0.0011	-2.02×10^{-5}	0.0061	0.0006
	(0.0031)	(0.0001)	(0.0250)	(0.0025)
15% Discount	0.0149***	0.0004***	0.0991***	0.0104***
	(0.0033)	(0.0001)	(0.0247)	(0.0028)
40% Discount	0.0592***	0.0018***	0.3553***	0.0446***
	(0.0041)	(0.0001)	(0.0248)	(0.0039)
Fixed-effects				
Date	Yes	Yes	Yes	
Account	Yes	Yes	Yes	
Avg. Outcome				
Control	0.0517	0.0020	0.0533	
Fit statistics				
Observations	1,974,798	1,974,798	76,086	
Number of Accounts	109,711	109,711	4,227	
R^2	0.07822	0.07816		
Pseudo R^2	0.02141	-0.02598	0.10397	
F-test	465.49	465.11		

Clustered (Account) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Estimates in this table refer to the main specification with individual (account) fixed effects.

A3 Deviations from our pre-registered analysis plan

This experiment was pre-registered with the American Economic Association (AEA) under Trial No. 0015061. While we aimed to follow the pre-registered analysis plan as closely as possible, a number of deviations became necessary in the course of implementation and analysis. Below, we detail the key departures from this plan. Where applicable, we have noted corresponding changes to specifications via footnotes in the main text.

Sample: Our pre-registered analysis plan had dictated excluding new customers, fleet users, and super users, but they were automatically randomized following the same process as existing customers (see Section 2.3), which we had not realized *in advance* would occur. For this reason, we have *included* these 330 customers in our analyses. We show that our results are almost unchanged in Table A38 (compare with main results in Table A3).

Outcome measure: Our pre-registered analysis plan defined two primary outcome measures: (1) a binary indicator for whether a consumer charged at any point during the event, and (2) total kWh charged during the event. For clarity and conciseness, our main analysis focuses on outcome (2). However, we have included regressions including the binary indicator as well in Table A3.

Event study: Our event study analysis differed slightly from our pre-analysis plan, where we planned to regress treatment interacted with the time since event. Due to computational limitations, we instead ran the main specification independently for each time period before and after events.

Regression specifications: In our pre-registered regression specification, we outlined three sets of controls: (1) time fixed effects, referring to dummies for time of day (e.g., morning, afternoon, evening, night), day of the week, and month of the transaction; (2) an individual-specific intercept; and (3) a vector of time-varying customer characteristics, such as the duration of the charge. In our final specification, we included event-level fixed effects and four time-invariant covariates: September, October, and November consumption (each set to zero for customers who had not yet joined Electroverse), and a binary indicator for whether the customer had at least one charge between January and November 2024. We did not include time-varying customer characteristics or individual fixed effects. This choice reflected a balance between model interpretability, statistical power, and computational feasibility. While individual fixed effects offer strong control for unobserved customer-level differences, they reduce estimation power, and in probit models drop observations for customers with no variation in the outcome. Random effects were computationally intensive at the scale of the data. Controlling for pre-treatment charging behavior offered a tractable alternative that captures some of the same individual-level variation in a transparent and interpretable way. Note we show results using individual fixed effects in Table A39; results are very similar to our main results in Table A3.