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Louise Bernard, Andy Hackett, Robert D. Metcalfe, Luca A. Panzone, and
Andrew R. Schein

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

Louise Bernard

Centre for Net Zero

Andy Hackett

Centre for Net Zero

Robert D. Metcalfe

Columbia University

Centre for Net Zero & NBER

Luca A. Panzone

Newcastle University

Andrew R. Schein

Centre for Net Zero

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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 of short-run behavioral responses. Customers were responsive to price: a 40% price reduction was equivalent to an arc price elasticity of -0.90, while a 15% price cut was equivalent to an arc price elasticity of -0.85. Our findings suggest that dynamic pricing for public EV charging generated large consumer welfare gains.

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1 Introduction

To reduce greenhouse gas emissions from the transport sector, policymakers have focused on encouraging the replacement of internal combustion engine (ICE) vehicles with electric vehicles (EVs) (Rapson and Muehlegger, 2023a). According to the International Energy Agency (2024a), a medium-sized ICE vehicle currently emits roughly twice as much CO₂e 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 demand is essential for energy economists, regulators, and policymakers (Li and Jenn, 2024; Li et al., 2017). This paper provides the first large-scale experimental estimate of that elasticity in public charging, an emerging priority for public infrastructure investment. 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. Our findings build on prior work highlighting how energy consumers adjust behavior in response to price signals, both in terms of efficiency gains and potential rebound effects (Gillingham and Palmer, 2009; Gillingham et al., 2016).

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; 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.

¹The UK plans to end the sale of new cars powered solely by ICE by 2030, and hybrids and vans by 2035 (Department for Transport, 2025b). 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).

Another potential benefit of dynamic pricing is to lower EV running costs for consumers, particularly for those more reliant on public charging infrastructure. Using data from the English Housing Survey 2023–24, we estimated that only 67% of households in England have access to off-street parking, implying that roughly one-third would need to rely primarily on public charging if they owned an EV. Renters, lower-income households, and residents of London are substantially less likely to have off-street parking. As EV adoption diffuses beyond early adopters — who are disproportionately homeowners with driveways — public charging infrastructure may become increasingly important for EV owners. 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); however, we have not seen evidence that dynamic prices can help reduce overall running costs.²

While observational studies have examined EV charging behavior,³ there has been no experimental evidence to date on how price influences 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, of which 84% were fast chargers (>7 and 50 kW), rapid chargers (>50 and 150 kW), or ultra-fast chargers (>150 kW).⁴

The field experiment was administered in collaboration with Octopus Electroverse, a large UK public EV charging platform (hereafter, the partner platform). Electroverse is operated by Octopus Energy Group, which also owns a UK electricity utility (hereafter, the partner utility) that supplies electricity to the participating charge point operators (CPOs) in our trial. The app displays a map of public EV chargers across the United King-

²Many countries have implemented substantial incentives to promote EV adoption, which offer significant welfare benefits (Rapson and Muehlegger, 2023b; Gillingham et al., 2023; Allcott et al., 2024; Hahn et al., 2024; Knittel and Tanaka, 2024).

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

⁴Importantly, 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.

dom, 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 and connection type, price per kilowatt-hour (kWh), and station congestion (as price change was unrelated to congestion, our effects were not driven by price as a signal for charger abundance).⁵ Access to full and accurate information is critical for realizing the benefits of the public EV charging network (Asensio et al., 2025).

The partner 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. 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 account and push notifications enabled, encompassing 109,711 eligible drivers, approximately 5% of the total UK EV market.⁶ 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. The within design allowed us to increase power by 14% and allowed for a richer heterogeneity analysis.

We designed the levels of price reduction based on two scenarios that commonly occur in the British power market. First, when wholesale power prices fall to £0/MWh, the savings to procurement costs approach £100/MWh (about £0.10/kWh). This aligns with our 15% price reduction (approximately £0.10).⁷ Second, when specific distribution networks have excess electricity, the value of demand turn-up to absorb this excess is roughly £450/MWh of additional demand. After accounting for some of the price reduction being

⁵The 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.

⁶We assume 2.2 million EVs, including plug-in hybrids, in the UK (Society of Motor Manufacturers and Traders, 2025).

⁷Here, we simply mean that the difference between £100 and £0 per MWh is £0.10/kWh, in line with our 15% price decrease on the average charging cost of £0.70/kWh.

spent on inframarginal customers, this value is consistent with our 40% price reduction (approximately £0.30).

1.1 Primary findings

We found that demand for public EV charging through the partner platform was responsive to price. We estimated arc elasticities of -0.85 for the 15% price reduction and -0.90 for the 40% price reduction, in both cases reflecting short-run responses to temporary price events on the partner network. Our field experiment allowed us to identify how much of the total demand increase was net new demand in the price-reduction hours versus switching between charging apps. The aforementioned elasticities net out substantial app-switching rather than net new demand in the price-reduction hours. They carry significant implications for welfare analysis, marginal cost pricing, and infrastructure investment.

Before netting out app-switching, we found that 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. Given the large price changes, we looked at the impacts expressed as arc (midpoint) elasticities, finding -1.47 for the 40% reduction and -1.60 for the 15% reduction. By contrast, non-price “green” messages had no statistically significant effect on average. However, this aggregate null masks substantial heterogeneity: users located in close proximity to a charger did respond positively to these messages. These elasticities capture responsiveness of charging through the partner platform during event windows. They do not, on their own, identify the effect on total electricity demand or total public charging demand.

Second, to assess how these platform-level effects translated into net electricity demand, we leveraged several additional data sources that provided partial visibility into substitution across apps, chargers, and time. We had the unique ability to parse apart these substitution channels because of our partnership with the partner utility, which supplied electricity to many of the public chargers in our experiment. This allowed us to understand how much of the demand came from app shifting at the same charger. Moreover, due to the high-resolution data we collected, which included public charging activity across both participating and non-participating chargers, we could identify how much of the demand came from substitution from other CPOs.

Using these charger-level electricity data from participating chargers, we estimated that approximately half of the observed increase in app-mediated demand reflected users switching from other apps to access the same chargers, rather than an increase in kWh delivered at those chargers. For grid operators, the relevant metric is the net increase in electricity demand during event hours, regardless of platform. Adjusting for estimated app switching, we obtained the net-demand elasticities of -0.85 for the 15% reduction and -0.90 for the 40% reduction noted above.

We found suggestive but imprecisely estimated evidence of temporal displacement within the public charging network and in-home charging behavior. Confidence intervals around these estimates include zero, so we cannot rule out either meaningful displacement or pure demand creation.⁸ 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. Nonetheless, only 18% of the chargers on the partner network offered price reductions during dynamic pricing events, so our elasticity estimates might be a lower bound on what would happen if all CPOs had these wholesale market price reductions passed down to customers.

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.⁹ We also found substantial differences by vehicle type: large-battery EVs (over 77 kWh) showed greater increases in charging demand, as one would expect, which resulted in an arc price elasticity of -1.97 for these consumers, compared to -1.05 among those in the smallest battery quartile (14–51 kWh), for the 40% price decrease. In addition, we observed heterogeneity by situational context. Price responsiveness varied across the day. In absolute terms, the largest increases in charging occurred in the morning, when baseline demand is highest. However, relative increases - and implied elasticities - were strongest in the evening. One possible explanation is that drivers face lower opportunity costs of time in the evening compared to the morning commute period, making them more responsive to temporary price reductions during those hours (Goldszmidt et al., 2020). Finally, we found that proximity to public chargers is also

⁸During the study period, home charging remained cheaper than public charging. 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.

⁹For customers in local authorities with average disposable income below £40,000 per year, the arc price elasticity of demand in response to the 40% price decrease is -1.89. All other quintiles show weaker responses. Note that for all conditional average treatment effects, we include demand from app-switching, rather than deflating the demand increase to “net out” app-switching. Note also that we found that the proportion of demand from app switching was very similar between chargers in lower-than-median and higher-than-median-income areas.

an important moderator of responsiveness – as distance to the nearest charger increases and charging presumably becomes more costly (in terms of opportunity cost 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 in the treatment groups 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; 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. 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). 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 £0.25. We estimated the total consumer surplus change of moving to the wholesale marginal cost from the current market price for charging would be £5.00 for every £1 spent by customers.

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 re-

duction (Bailey et al., 2023). Importantly, the price responsiveness we observed for public charging occurs in a different behavioral and socioeconomic but yet somewhat more general 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 estimated the price elasticity of demand across a large network of public chargers and merged additional data that allow us to examine substitution between public and home charging. Second, our intervention was implemented as a large-scale natural field experiment with minimal additional self-selection beyond users having a notification-enabled account on the partner platform. This design enhanced external validity relative to smaller framed field experiments with voluntary enrollment (Harrison and List, 2004), such as Bailey et al. (2023), Garg et al. (2024), and La Nauze et al. (2024). While our sample represented a subset of UK EV drivers - specifically those who use the partner platform and enable notifications - it reflected a broad cross-section of active public charging users across the country. 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 relate 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

¹⁰Like 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 found 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.

¹¹Our 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; Metcalfe et al., 2025).

the one used in our study.¹²

Furthermore, we contribute 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 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. Our estimated price elasticities of demand for charging on the partner platform were larger than the gasoline price elasticities typically reported in the literature (e.g., 0.3 to 0.4 in Coglianesi et al. (2017); Knittel and Tanaka (2021); Levin et al. (2017)). Several features of our setting may explain this difference. The experiment provided day-ahead notice, captured short-run responses, and allowed for within-day interhour substitution to contribute to the estimated elasticities. Most gasoline studies estimate elasticities using lower-frequency, more aggregated data, whereas our setting exploits daily, high-frequency data where we controlled the assignment mechanism. Nevertheless, the relatively higher elasticities in our experiment suggest that there are large social economic benefits of aligning charging prices with marginal cost.

Finally, our results have implications for policy responses to local charging costs.¹⁴ 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).¹⁵ 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 customers from lower-income neighborhoods were 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

¹²Given 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).

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

¹⁴Our research is in similar spirit to Holland et al. (2016), whereas we are focusing on local charging costs and the local externalities they can create, rather than the direct health costs from local EV charging.

¹⁵We acknowledge, however, that the charging speed (in kW) 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 using public chargers is more costly than using at-home chargers.

EV diffusion moves from "early adopter" to "early majority" (Rogers, 2003).¹⁶ 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, 2024b). Therefore, our estimated price elasticities of demand are important for understanding the benefits of such public infrastructure investment.

The remainder of the paper is structured as follows. First, we describe the study design, including details of the experimental design, 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 natural field experiment

We implemented our natural field experiment using eligible customers of the partner app, a payment platform with Europe's largest consumer EV charging network. The app functions as a payment platform that connects users to charging point operators (CPOs), rather than operating its own chargers.¹⁷ In the UK, the app covers approximately 60% of the public charging network, linking users to more than 65,000 charging points¹⁸ from

¹⁶Early 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.

¹⁷Most chargers listed on the partner platform 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 station without using the partner app, although in our trial only sessions initiated via the app were eligible for the experimental price reductions. Conversely, users can also access non-participating CPOs through the app, although those chargers did not offer dynamic pricing during the experiment.

¹⁸Note 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.

approximately 100 charging point operators (CPOs). Chargers outside the partner network still appear on the app’s map, although customers cannot use the app to pay for sessions at them. These are most often residential or lamppost chargers, motorway chargers, and Tesla Superchargers — all of which customers can generally find without relying on app-based “discovery.” Very few chargers are missing from the map altogether; these are typically fully private chargers, such as those installed by hotels exclusively for their own guests.

A subset of CPOs on the network are supplied by the partner utility. This vertical integration enabled the intervention. Most CPOs tend to be on fixed-price tariffs, and thus do not capture the upside of customers changing their consumption to better align with wholesale power prices. The partner utility and the partner platform, both part of the same corporate group, can reduce procurement costs at chargers supplied by the partner utility by incentivizing customers to charge during periods of lower wholesale electricity prices. These CPOs participated in the dynamic pricing events, comprising 18% of the partner platform 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 charger in Figure A9.

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 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; an additional 64 new customers became eligible during the experimental period and were automatically assigned to one of our four experimental conditions, with a final sample of 109,711 customers.¹⁹ Our analysis was restricted to chargers within

¹⁹Our pre-analysis plan had dictated excluding new customers, fleet users, and super users (a customer marker defined by the partner platform), 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 (64 "new" and 266 from the other exclusion categories) in our analyses. We show that our results are unchanged when we exclude these customers (Table A19).

Great Britain, where the price decreases applied.²⁰

2.2 Intervention design

Before our study, the partner app occasionally offered dynamic price reductions, which varied across events and CPOs. Importantly, these discounts were not randomized across customers within the same event, meaning our field 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 partner platform UK network, only around 26% of stations apply static 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 A21. Because the randomization occurred at the user level, and most trial events took place at chargers without ToU pricing, the estimated effects captured the impact of our experimental intervention, rather than existing price variation across the day. We detected no concurrent dynamic pricing events by other providers during our trial's events.

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 2.²¹ Two of the treatments were price decreases: a 15% decrease and a 40% decrease. We used these price changes as they reflect two scenarios that commonly occur in the British power market. First, when wholesale power prices fall to £0/MWh, the savings to procurement costs approach £100/MWh (about £0.10/kWh), which aligns with our 15% price reduction (approximately £0.10). By this, we mean that the difference between £100 and £0 per MWh is £0.10/kWh, in line with our 15% price decrease on the average charging cost of £0.70/kWh. Second, when specific distribution networks have excess electricity, the value of demand turn-up to absorb this surplus can be substantial. In particular, Distribution Network Operators in Great Britain classify

²⁰The partner platform 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.

²¹The 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 stations 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 1: Visual representation of the three notifications used in the experiment



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 CO₂e intensity, though event timings varied across the study (Table A2 shows the dates, times, and durations of all events).

the value of demand turn-up at approximately £450/MWh, based on the logic that it can be a substitute for future network upgrade costs (UK Power Networks, 2025). After accounting for some of the price reduction being allocated to inframarginal customers, this corresponds to our 40% price reduction (approximately £0.30), though the precise relationship depends on the proportion of marginal versus inframarginal customers, which we did not know *ex ante*.

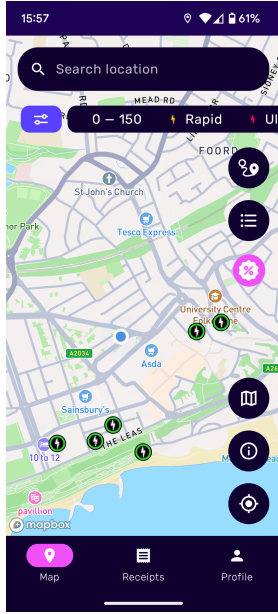
Control group: Customers in the control group did not receive any push notifications about events' price decreases.

15% price decrease group: Participants in this treatment received a push notification about a low price decrease, 15%, at selected charging stations. The message²² 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.

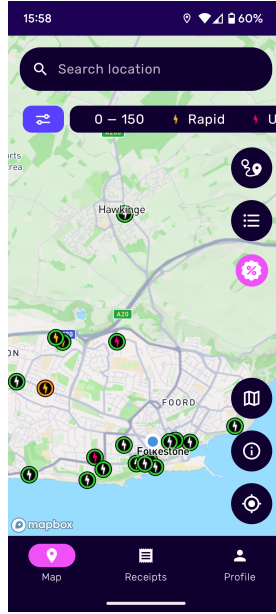
40% price decrease group: This group received a push notification offering a high price decrease, 40%, at selected charging stations. The message was similar in structure: *Plunge Pricing tomorrow! Get 40% off [operators names] [time slot]!* The price decrease conditions were identical to those in the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

²²On 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.

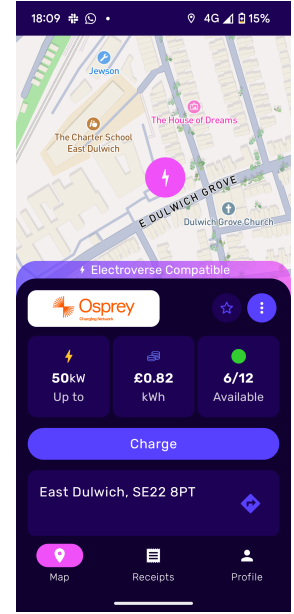
Figure 2: The partner app under different pricing conditions



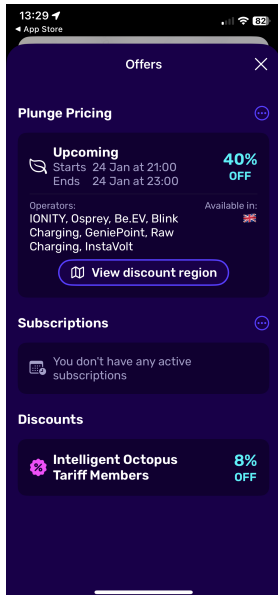
(a) No event – map view



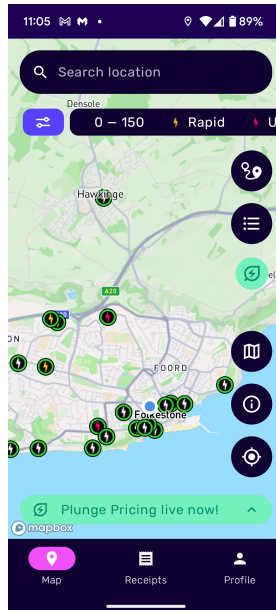
(b) No event – zoomed out map view



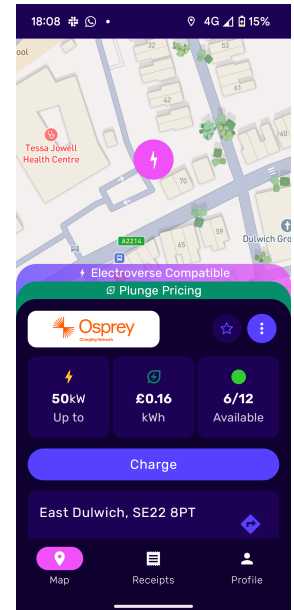
(c) No event – charger view



(d) Upcoming event – offers view



(e) Event – zoomed out map view



(f) Event – charger view

Notes: Panels (a), (b), and (c) show the 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 event. Discounted chargers are not flagged visually, but users can filter for participating CPOs such as Osprey.

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 allowed us to compare the effectiveness of pro-environmental messages and price incentives. It also allowed us to understand the impact of incentives, holding fixed the receipt of a notification.²³

We had three main hypotheses in our field experiment. Our first null hypothesis was that reducing the charging price by 40% has no impact on charging demand; our second null hypothesis was that the 15% price decrease has no impact on demand; our third null hypothesis was that a green message will not affect charging demand. These three hypotheses belong to the same family, so we adjusted for multiple hypothesis testing in our analysis (List et al., 2023).

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 the app’s internal user IDs, which are unique, positive integers assigned sequentially according to the customer’s join date.²⁴ This approach ensured balanced assignment across the user base, while preserving randomness with respect to the time when a customer joined the platform as shown in Table A15.

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

²³Also, this treatment was not CPO specific, so there are more opportunities for this group to change their behavior if motivated to do so.

²⁴We 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 the app’s data environment.

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. We front-loaded the first period to ensure we could capture the treatment effect before the first crossover, as a precaution against any possible spillover effects between crossovers. For the subsequent periods, we aimed for approximately six events, subject to market conditions.

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. The crossover design increased statistical power by observing the same participant under multiple treatments and removing time-invariant differences across participants.²⁵ It also allowed estimation of heterogeneous effects. And, it mirrored the dynamic pricing environments customers face in reality, enhancing external validity (List, 2020).

Two main concerns with within-customer randomization are carryover effects (i.e., causal transience), 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). 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 customer forums during the trial and did identify posts indicating some knowledge of differing price reductions between customers – ultimately, we found only

²⁵We compared a post-trial MDES calculation to a hypothetical scenario in which all customers kept their initial treatment assignment throughout the trial. For the post-trial calculation, we used the standard error of the estimated treatment effect from Table A3 (approximately 0.0032 kWh) together with the conventional 80% power threshold at the 5% significance level (i.e., 2.8 standard errors from the null). This implied an MDES of $2.8 \times 0.0032 = 0.00896$ kWh, or roughly 17% of average in-event consumption among Control customers, closely matching the pre-registered analysis plan estimate of 15%. This was 14% lower than the hypothetical MDES under fixed treatment allocation.

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

We randomized all eligible customers to one of the four experimental sequences independently and with non-zero probability prior to the trial’s start, ensuring unconfoundedness.

Second, the standard implementation and consistency conditions were met. (i) The Stable Unit Treatment Value Assumption held: a customer’s potential outcomes did not depend on the treatment assigned to other customers, and there was no variation in the treatment received. 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 in that case, any crowding-out effect would likely have been small because charging in the incentive window would have displaced other users symmetrically. See Goldszmidt et al. (2020) for a discussion of related issues in two-sided platform randomization. (ii) Outcome observability was ensured: treatment was assigned only to observed customers, and although post-assignment attrition was possible, we tested for differential attrition and found no evidence that treatment affected outcome measurement. (iii) Compliance was complete: all treated customers received the assigned message.

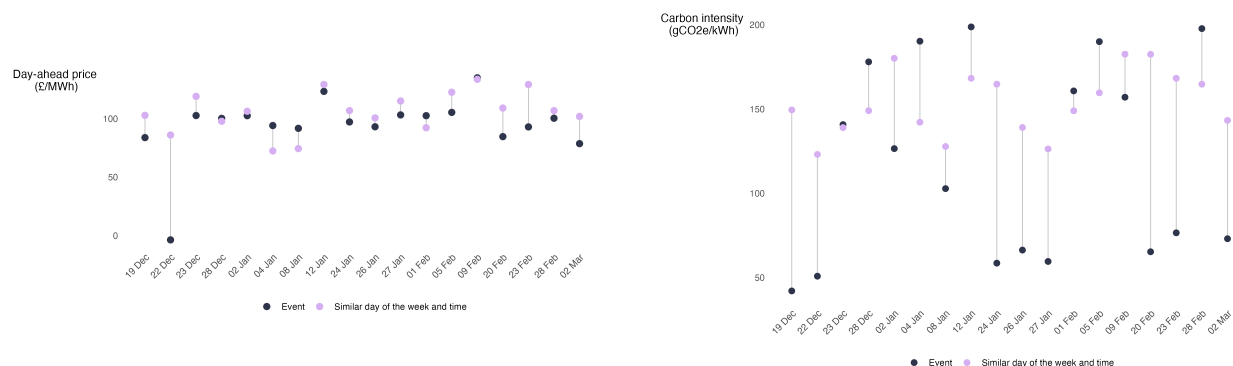
Under these conditions, randomization identifies the average treatment effect, allowing us to recover an internally unbiased estimate of the impact of charging prices on charging demand. Moreover, because customers neither selected into the experiment nor were aware of their participation, we classify this as a natural field experiment in the sense of Harrison and List (2004).

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, the partner team identified events within a given week to correspond to times when the price of wholesale electricity was predicted to be low with high probability. On average, households were notified approximately 18 hours before each event.

Figure 3: Day-Ahead prices (left) and CO₂e intensity (right)



Notes: These figures report average day-ahead wholesale prices (£/MWh) and grid carbon intensity (gCO₂e/kWh) during dynamic pricing event hours (dark points) and during matched baseline periods (purple points), defined as the same day of the week and settlement period within the trial window, excluding event days. Event hours were associated with lower wholesale prices and lower carbon intensity relative to comparable non-event periods. Day-ahead spot prices are retrieved from EPEX wholesale market data. Carbon intensity data are obtained from Great Britain's National Energy System Operator via the Carbon Intensity API (<https://carbonintensity.org.uk/>).

Figure 3 indicates that dynamic pricing events were generally scheduled during periods of relatively low wholesale prices and low carbon intensity, subject to the practical constraint of selecting 18 events over a three-month window. Relative to the matched baseline — defined as the same day of the week and settlement period during the trial, excluding event days — event hours were characterized by lower day-ahead prices (£/MWh) and lower CO₂e intensity (gCO₂e/kWh). Wholesale prices (and associated carbon intensity) varied substantially across days and hours, ranging from –£3.75 to £135 per MWh (0.4p to 13.5p per kWh). The mean wholesale price during events was £93.6 per MWh, compared to £106 for comparable non-event periods. Similarly, average carbon intensity during events was 120 gCO₂e/kWh, relative to 152 gCO₂e/kWh for matched baseline periods.

During the trial, event timing was determined using day-ahead wholesale prices and system operator carbon intensity forecasts. However, forecasts of marginal prices and marginal emissions were not available in real time. Since marginal signals are more closely aligned with the emissions and system cost consequences of demand changes,

access to such forecasts could potentially improve the targeting of future events.

2.5 Data

Our analysis used observed charging demand via the app. We pre-specified and 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 the app’s 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 demand at those locations. This distinction arises naturally from the commercial arrangements between CPOs, the partner app, and partner energy supplier; it also enables an interesting heterogeneity analysis, discussed in Section 4.3.3, 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 charging demand in September, October, and November 2024, as well as total kWh charged, GBP spent, number of receipts, and 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 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 rural²⁶ (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, 2025a).

In addition to these charging data from the app, we drew on two additional novel 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

²⁶This 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. Both of these are unique to our research team because of the vertical integration of the app and the energy retailer that supplied power to the participating CPOs.

3 Descriptive statistics

3.1 Load Duration Curve

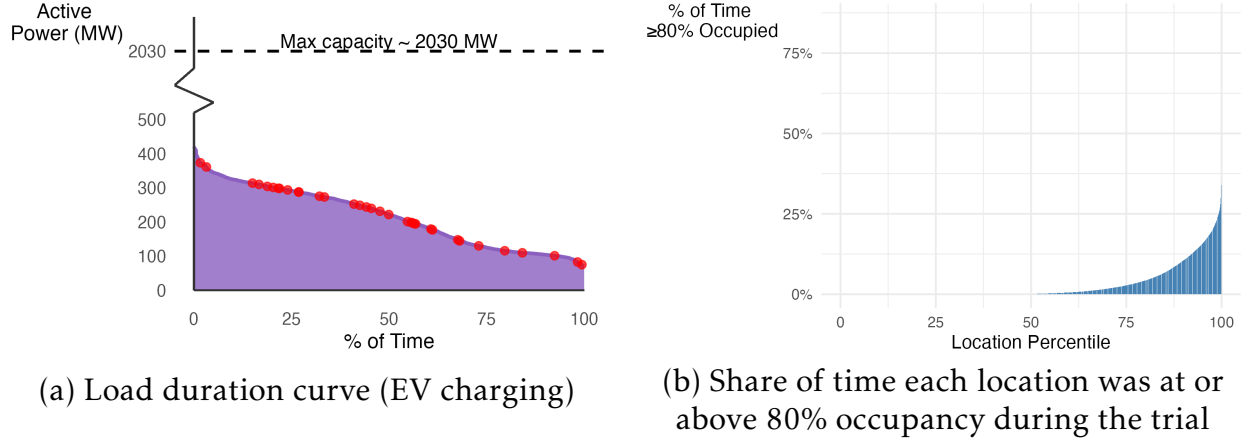
In this section, we discuss the Load Duration Curve (LDC) for the partner app’s chargers. An LDC provides a ranked view of power demand across time (in our case, all hours of several months) on the partner network, which accounts for around 60% of public charging infrastructure in the UK. It shows how 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.

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, 2025.²⁷ Drawing on availability data provided by CPOs, we constructed an 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.²⁸ The red dots on the figure are the MW during each price plunge event, which shows that the events were not triggered during peak congested times for the network. Compared to gas stations in the US, this level of utilization is low, but the levels are not substantially different – gas stations in the US are 24% utilized, on average (C-Store Dive, 2023).

²⁷These 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 the app’s administrative data.

²⁸In Figure A20, 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.

Figure 4: Utilization patterns across the partner charging network



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.

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 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 difference, as slow charging stations (mostly non-participating) are the most likely to be used overnight and thus be congested then.

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 man-

agement. 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 A14), and to a lesser extent on weekends (Figure A15). 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.²⁹ Our analysis also revealed that the majority of charging activity occurred in areas with a relatively similar income level to the user's residential area throughout the trial period. When comparing the average income of the user's home area (determined by Middle Layer Super Output Area (MSOA)) versus the average income of the area where they charge, we observed a strong pattern of income-matching. Specifically, approximately 40% of all charging transactions during the trial took place in an area categorized within the same income quintile as the user's home area, and the share of transactions systematically decreased as the income difference between the user's home location and the charging location increased (see Figure A2a). We observed a comparable pattern in the pre-trial period as well: Figure A2b shows that income-matching predominated even before the intervention, although users from lower-income areas tended to charge less frequently overall. These use cases are reflected in the underlying composition of users. Using a k-means clustering algorithm, we identified three distinct user types: a group of users charging almost exclusively on slow chargers, likely using public charging as a substitute for home charging; a second group that appeared to charge on occasional trips near their home; and a third group that charged mostly at chargers far from their home. For further details, see Appendix A1.9.

Our sample comprised a large user base and we saw no systematic differences in pre-trial charging behavior between the randomized treatment sequences.³⁰ Table A15 presents summary statistics for the full sample of 109,711 customers. In the 12 months

²⁹Most 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.

³⁰However, as discussed in Section 5, our treatment effect heterogeneity analyses found that local users were much more engaged than more occasional trip users.

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 the app for 16 months. A series of Kruskal–Wallis tests reveals no significant differences across these variables among the four sequences described in Table 1.

3.3 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 CPOs. Figures A17 and A18 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).

These geographic patterns motivated a closer look at whether dynamic pricing also influenced which charging locations customers chose to use. In particular, we also investigated habit formation in the form of customers changing which chargers they typically used (see Appendix A2.7). 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.

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 followed our pre-analysis plan (PAP) in our pre-registration of the field experiment (AEARCTR-0015061).³¹ To formally estimate the effect of our interventions on consumer charging behavior, we employed regression analyses focused 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 estimated an OLS regression of the form:

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

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 accounted 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 did not use all hours in the data because including

³¹In 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 [A4](#).

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. In summary, this specification captures average treatment effects, controlling for prior engagement with the platform and customer baseline consumption.

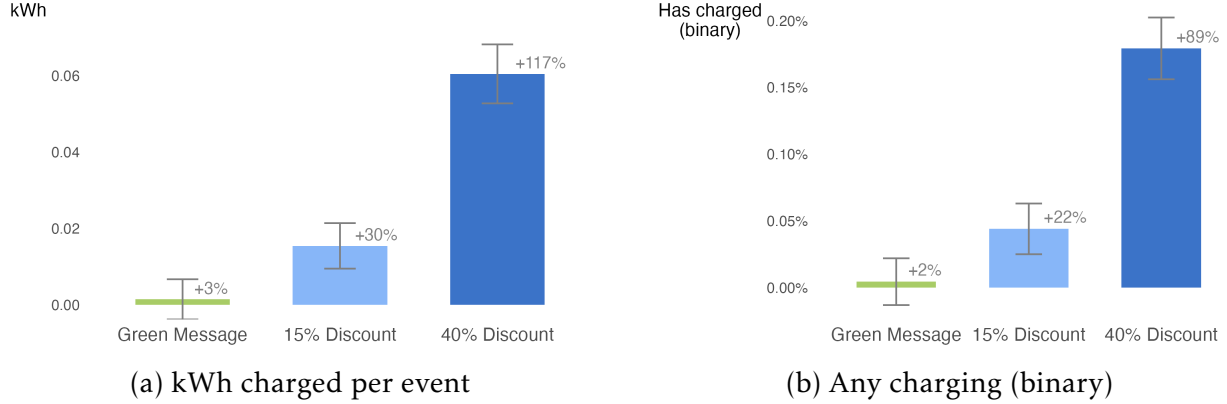
4.2 Main results

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. As shown in Figure 5a, 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 (whose average consumption was 0.0517 kWh). 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 arc price elasticities of demand of -1.47 for the 40% price decrease and -1.60 for the 15% price decrease.³² However, it is important to emphasize that these uplifts and implied elasticities pertain only to charging through the partner platform, as we did not observe customers’ charging via other platforms. For this reason, the relative increases in total 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.

Beyond total kWh charged, we also examined whether the treatments primarily shifted the extensive margin – whether customers charged at all during an event. As shown in Figure 5b, both price decreases increased the probability of any charging, with effects only slightly smaller than the corresponding kWh uplifts. This pattern indicates that most of the behavioral response operated through the extensive margin: customers became more likely to initiate a charging session on the app at discounted times, while con-

³²Arc elasticities were calculated using the midpoint formula: $\epsilon = \frac{\Delta Q/\bar{Q}}{\Delta P/\bar{P}}$. While the nominal price reductions were 15% and 40%, the arc elasticity formula uses the midpoint price as the denominator. To do so, we normalized the control price to $P_0 = 1$, which is without loss of generality since elasticities depend only on percentage price changes. The treated prices were therefore $P_1 = 0.85$ for the 15% discount and $P_1 = 0.60$ for the 40% discount. The arc elasticity uses the midpoint price $\bar{P} = (P_0 + P_1)/2$ as the denominator. For a 40% reduction, the percentage price change is then $\frac{\Delta P}{\bar{P}} = \frac{0.60-1}{0.80} = -0.50 = -50\%$, and for a 15% reduction, this is $\frac{\Delta P}{\bar{P}} = \frac{0.85-1}{0.925} = -0.162 = -16.2\%$. We divided the 15% coefficient ($\beta_{15} = 0.0154$) by the midpoint quantity: $(\frac{0.0154}{0.0517+0.0154/2})$ to obtain a 25.9% quantity increase. When divided by the -16.2% price change, this yielded an arc elasticity of -1.60. Similarly, the 40% coefficient ($\beta_{40} = 0.0604$) represented a 73.7% increase relative to the midpoint, resulting in an elasticity of -1.47 against the -50% price change.

Figure 5: Treatment effects on charging behavior



Notes: Bars show treatment effects from Table A3, our primary analyses as specified in our pre-analysis plan. Error bars are 95% confidence intervals. Panel (a) shows treatment effects on the app (kWh per dynamic pricing event). Labels show the percentage 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 produced a smaller positive effect of 30%. The green message did not significantly affect usage. Panel (b) shows treatment effects on the probability of initiating a charging session through the partner platform during an event. The effects are slightly smaller than those in Panel (a), indicating that most of the response reflects an increase in the likelihood of charging via the platform (an extensive-margin response at the platform level), with some additional adjustment in kWh conditional on charging.

ditional increases in session size were comparatively limited. Because kWh is both easier for readers to interpret and the parameter most relevant for assessing demand flexibility and system value, the results that follow focus on kWh consumption.³³

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

³³We present charging incidence summary statistics in a bar chart by event in Figure A19 and a summary table in Table A16.

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 (i.e., causal transience). 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 A14 show that during washout consumers charging behavior did not differ across the groups, with all positive but non-significant coefficients. Additionally, we compared consumption treatment group interactions with blocks in Tables A4 and A9; we found no significant interaction between treatment groups and blocks, an indication that the effects we observed did not extend beyond the intervention period, providing complementary evidence against significant spillover effects. Similarly, we interacted the treatment indicators with sequence in Table A5. We found that the effect of the 15% price decrease was slightly lower for sequence 2 – the groups of users who did not receive the 40% price decrease first. However, this difference was indistinguishable from a block effect, as all treatments showed slightly smaller effects in block 1, which included three nighttime events. Once these nighttime events were excluded, there was no remaining difference between sequence 1 and sequence 2. Finally, we examined how treatments affected whether customers unsubscribed from subsequent notifications, as well as whether notifications were delivered as intended; see Appendix A2.12. 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 the partner app 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.³⁴

4.3.1 App substitution

The first substitution we investigate is whether customers have changed how they paid for consumption at a given charger, switching from other payment methods to paying via the partner app to take advantage of the price changes. Alternative payment apps include Zap-Map, PlugShare, and easyCharging, as well as CPO-specific apps. 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 system stress or supporting renewable integration.

Quantifying the extent of platform-switching is challenging, as our data only included charging sessions initiated via the partner platform. However, all CPOs participating in the dynamic pricing intervention procured their electricity through the partner utility from the same corporate group as the charging platform, as described in Section 2, providing us with unique access to smart meter consumption data at the relevant charging points. Since these meters record total electricity delivered at the charger regardless of payment method, comparing them to app receipts directly reveals how much of the increase in app-mediated demand reflected real new charging versus users switching payment platforms to claim the discount.

This allowed for a valuable cross-validation: we could compare changes in electricity usage recorded through receipts with changes in overall energy consumption at the same charging points (note that this exploratory analysis was not pre-specified). While smart meter data could not be disaggregated by treatment group, this limitation is inconsequential for our purpose: since treatment was randomized and only notified (treated) users had any reason to change their payment behavior during events, the control group’s

³⁴In our setting, the good can be stored (as in battery charge), so there could be good reasons for intertemporal substitution as shown by Hendel and Nevo (2006, 2013). We differ from their approach since we randomized prices and allowed 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 CO₂e intensity of the marginal generator.

charging at these stations — via whatever app — should be identical during events and non-events. The smart meter comparison therefore cleanly isolates whether the increase in app receipts among treated users reflects real new electricity demand or merely a shift in payment platform. We applied a consistent pre-post estimation strategy to both data sources, allowing us to assess how much of the observed increase in 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,³⁵ 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.³⁶ 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.

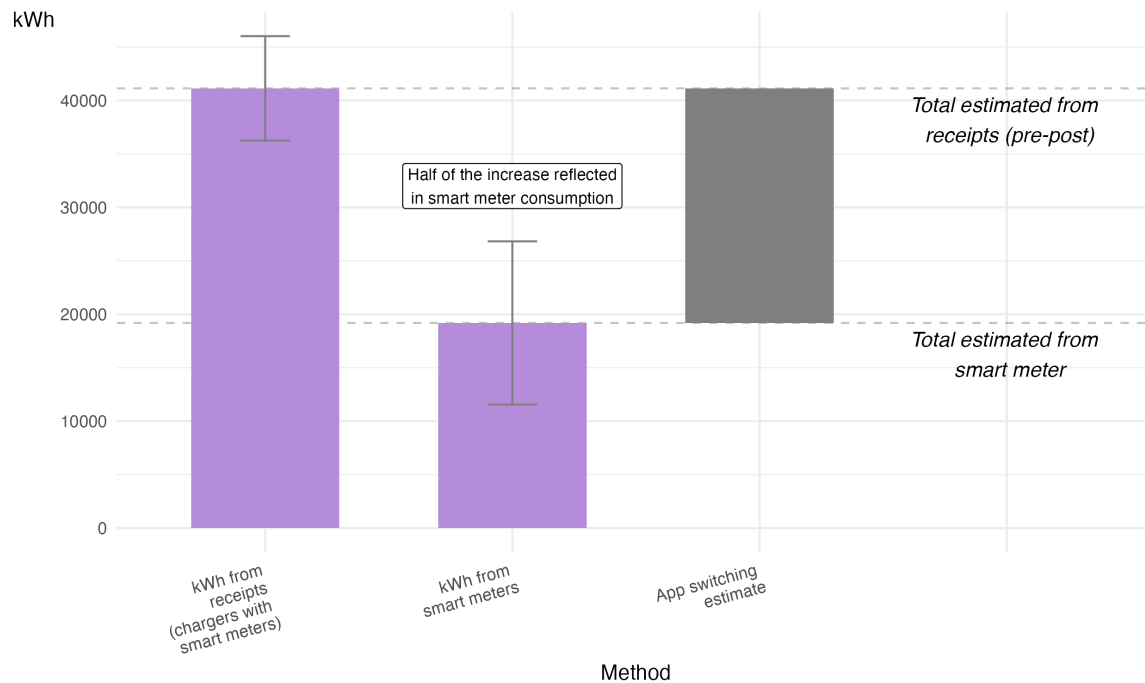
Next, we applied the same pre-post estimation strategy to the smart meter data for the relevant charging points, which yielded an estimated increase in consumption of 19,192 kWh. Thus, of the increase in electricity usage via the partner app, a substantial

³⁵This 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.

³⁶We observed 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 partner utility and partner app datasets.

portion seemed to be due to platform switching rather than net new demand. Specifically, approximately 53% of the increase in receipts seems to come from users shifting their charging activity from other charging platforms.³⁷ This finding underscores the importance of distinguishing between changes in platform choice and changes in total electricity demand when evaluating demand-side interventions. Figure 6 provides a visual breakdown of the app substitution effect.

Figure 6: Comparison of receipts and smart meter-based estimates



Notes: This figure compares pre-post changes in consumption measured from receipts data with the corresponding changes in smart meter readings, using the same set of charging stations. The receipts-based estimate (bar one) implies a much larger increase than the smart-meter-based estimate (bar two). The difference (bar three) suggests that roughly half of the rise seen in the receipts data reflected users switching from other charging apps or platforms, rather than a pure net increase in electricity consumption. This exploratory analysis was not pre-specified. Confidence intervals are computed by taking pre-post differences for each event at each charging point for which both receipts and smart meter data are available. To make these station-level estimates comparable to the RCT results, we rescaled the charging-point estimates and their standard errors by the number of events and stations.

We can estimate how this finding affects our headline price elasticities (-1.60 for the 15% price decrease, and -1.47 for the 40% price decrease). Assuming that only 50% of the observed demand increase represented new *net* demand, we again calculated the net

³⁷EV drivers may prefer using the native payment platform of the CPO. However, the partner app offers convenience when traveling by allowing access to multiple networks through a single app, without the need to register for each CPO individually.

arc elasticity using the midpoint formula, resulting in arc elasticities of -0.85 for the 15% price decrease and -0.90 for the 40% price decrease.³⁸

It is instructive to compare the large degree of substitution between payment platforms with the lack of substitution observed in Section 4.3.3. 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.

Next, we examined heterogeneity in app substitution by whether chargers were in lower-than-median and higher-than-median-income areas (again using Middle Layer Super Output Area for this analysis). We were not able to examine heterogeneity in app substitution by customer type given our reliance on charger-level smart meter data for the analyses. We found very similar overall app substitution patterns between these two types of chargers, as shown in Appendix A1.6. In both cases, our analyses suggested that app substitution accounted for approximately 50% of the overall treatment effect.

To recap, this app-switching channel matters for how the remaining tests in this section should be read. Our receipts data only record activity once a customer transacts through the partner app, so a customer induced by the discount to switch from a competing app or payment method necessarily appears with no pre-event baseline on the partner platform regardless of whether the underlying charging session was itself displaced in time or location, or would have happened anyway at the same charger. Such a session shows up as a pure increase with no offsetting reduction elsewhere in our data: exactly the signature we would otherwise attribute to genuine demand creation. As a result, to the extent app-switching is present, both the charging-point-operator substitution test below (Section 4.3.3) and the event-study evidence in Section 4.3.4 are biased toward finding net new demand and against detecting true displacement.

³⁸We used key outputs from our pre-registered specification's results (Figure 5 and Table A3: control group average of 0.0517 kWh, an increase of 0.0154 kWh caused by the 15% price decrease treatment, and an increase of 0.0604 kWh caused by the 40% price decrease treatment. As discussed in Section 4.2, for the 40% reduction, the midpoint price decrease is $\frac{\Delta P}{P} = \frac{0.60-1}{0.80} = -0.50$, and for the 15% reduction, the price decrease is $\frac{\Delta P}{P} = \frac{0.85-1}{0.925} = -0.162$. For the 15% price decrease, we took the *net* uplift, removing the $\approx 50\%$ of uplift from app-switching, as $(0.5 \times 0.0154 = 0.0077)$ kWh. We divided this net uplift by the midpoint quantity $(0.0517 + \frac{0.0077}{2} = 0.0556)$ to obtain a 13.9% increase in net demand. When divided by the -16.2% price change, this yielded an arc elasticity of -0.85. Similarly, the net uplift for the 40% price decrease (0.0302 kWh) represents a 45.2% increase relative to the midpoint quantity $(0.0517 + \frac{0.0302}{2} = 0.0668)$, resulting in an arc elasticity of -0.90 against the -50% price change.

4.3.2 Mechanisms of demand increase: evidence from consumer survey

To better understand the mechanisms behind the increased charging observed in our field experiment, we then supplemented our quantitative analysis with a short consumer survey. The previous sections showed that consumers offered a 40% price decrease charged more, but we found only small, statistically insignificant signs of displacement. To explore users' charging habits, motivations, and engagement with the dynamic pricing events, we sent a survey to all customers who had charged during a dynamic pricing event after the trial ³⁹.

Reported behaviors among participants indicated a mix of substitution and new demand, which supports our analysis of Electroverse charging data. Of the 69 participants, 42% reported changing *when* they charged, 43% changed *where* they charged, and 26% charged more than usual. Overall, 55% indicated changing either the time or the location of their charge. While the sample was small, these responses suggested that the treatment effects observed in Tables A3 and A4 reflected a blend of time and location shifting, as well as some true demand creation.

That said, only 16% of participants reported switching apps, in contrast to the patterns observed in Section 4.3.1 in which we estimated 50% of increased Electroverse charging demand represented app-switching. The discrepancy is unsurprising: survey respondents are a self-selected group who completed the follow-up questionnaire and are therefore more likely to be loyal to the partner-platform than the average user; they might also be less inclined to admit switching due to social desirability bias.

4.3.3 Substitution between charging point operators

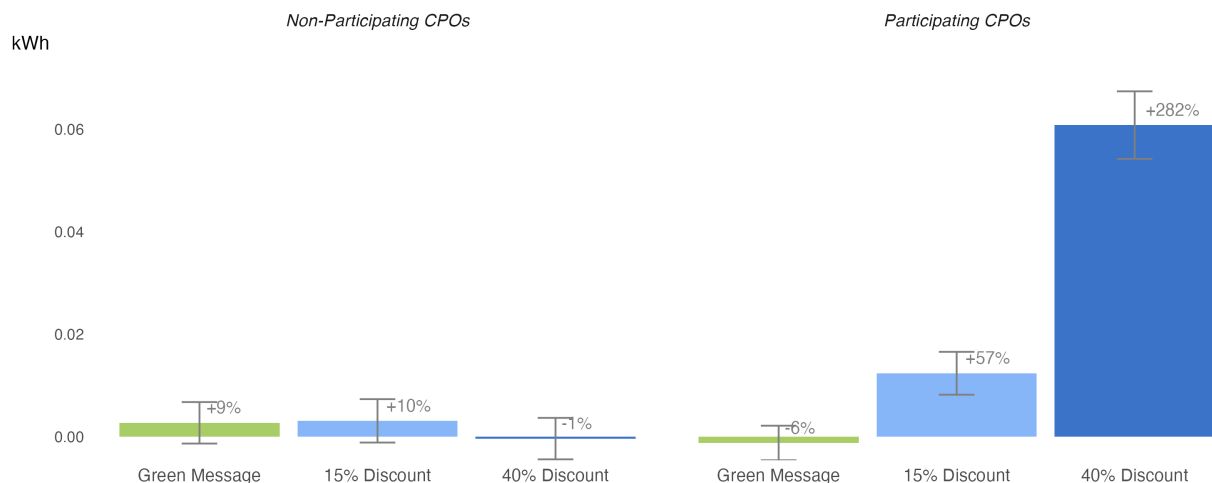
Taking into account that 43% of participants declared changing location (Section 4.3.2) and that half of the increase in partner-app receipts reflects platform switching rather than net demand (Section 4.3.1), we next look at displacement from non-participating to participating CPOs chargers ⁴⁰. The regression results presented in Section 4.2 referred to all chargers in the dataset. In this subsection, we analyzed treatment effects in participating vs non-participating charging points separately. Figure 7 and Table A9 break

³⁹This additional event took place on April 20th, 2026 from 12pm to 3pm. All eligible customers were offered a 15% discount. Out of 4,592 customers that charged during the day, 509 charged during the event at participating stations. 431 responded - among them 69 participants.

⁴⁰As 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 partner's charging network.

down the results from Table A3 by whether the operator participated in the dynamic pricing events.

Figure 7: 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 A9, 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.

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 increased consumption by 282%, equivalent to an arc elasticity of -2.34. The 15% price decrease increased the amount of kWh purchased by 57%, equivalent to an arc elasticity of -2.76. 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 app'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 7. 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 chargers 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.⁴¹

The above analysis focused on chargers *within* the partner network (approximately 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).

4.3.4 Temporal demand substitution

From the grid system operator and the retailer’s perspectives, the goal of price decreases during dynamic pricing events is to increase consumption in low-cost, low-constraint periods without generating new peaks, particularly in high-price, high-constraint periods. But the value of that demand depends on whether it is merely displacing consumption from nearby hours.

Two features of our analysis make this behavior hard to detect:

First, as noted in Section 4.3.1, any session that entered our receipts only because a customer switched payment platforms to claim the discount will, by construction, show no dip in the preceding or following periods, since that activity was never recorded on the partner platform to begin with.

Second, 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. Note that this 15% figure refers to the main event-period regression; the displacement regressions, which use noisier aggregated outcomes, have a correspondingly higher MDES — a point we return to when examining anticipatory behavior below.

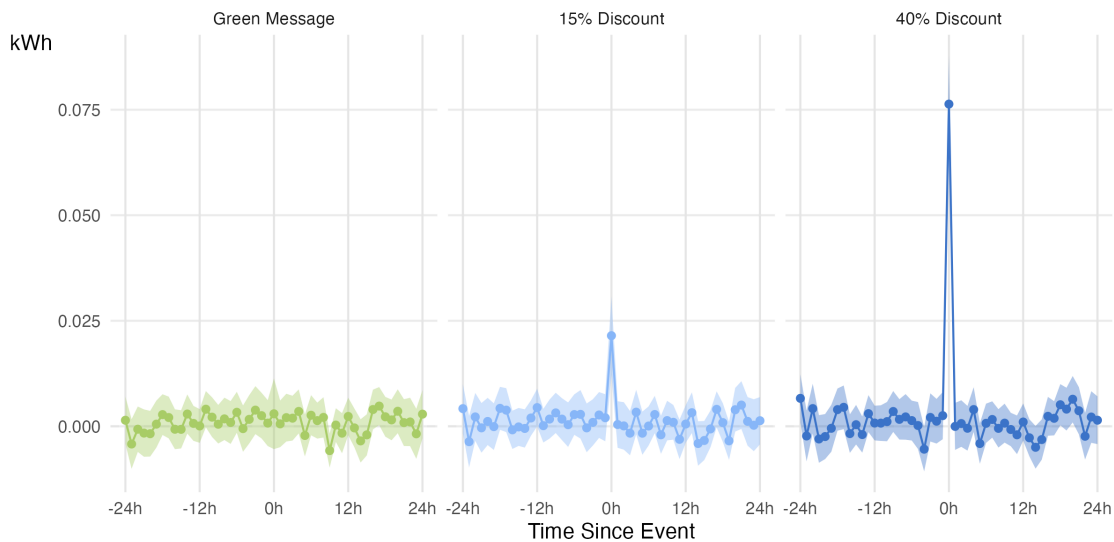
A null result in the tests below is therefore also consistent with real, but platform-invisible, temporal or spatial displacement - which is one reason we treat the 42% of

⁴¹In addition to these considerations, it is possible that current customers may differ from future ones in their response to dynamic pricing.

survey respondents reporting a change in when they charged (Section 4.3.2) as informative even where the event-study point estimates are not statistically distinguishable from zero.

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, con-

sumption 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 sufficiently far from other events to enable this analysis). As shown in Figure A12, we found similar absence of temporal displacement when we go further back and forward in time.

We also examined whether there was anticipatory displacement between customers receiving the notification about a dynamic pricing event and the event itself. Notifications were sent on average 18 hours before an event. This long notice period gave consumers time to adjust their charging plans. Our expectation was that users might curtail charging after learning about an upcoming price decrease, in order to charge during the event instead.

However, we found no evidence of this behavior. Table A12 shows results from a regression on treatment where the outcome was the amount of electricity charged at chargers between the time a notification was sent (we use the time the first notification was sent for each event). After netting out app switching, the treatment effect attributable to net demand is approximately 0.030 kWh for the 40% discount group (which is the largest treatment effect of the three treatments). If 42% of that - consistent with the share of respondents who reported changing when they charged (Section 4.3.2) - reflected anticipatory behavioral shifting, the implied pre-event dip would be roughly 0.013 kWh. This is well below our minimum detectable effect of approximately 0.022 kWh (standard error of $0.008 \text{ kWh} \times 2.8$). We therefore do not attach weight to the slightly positive point estimates.

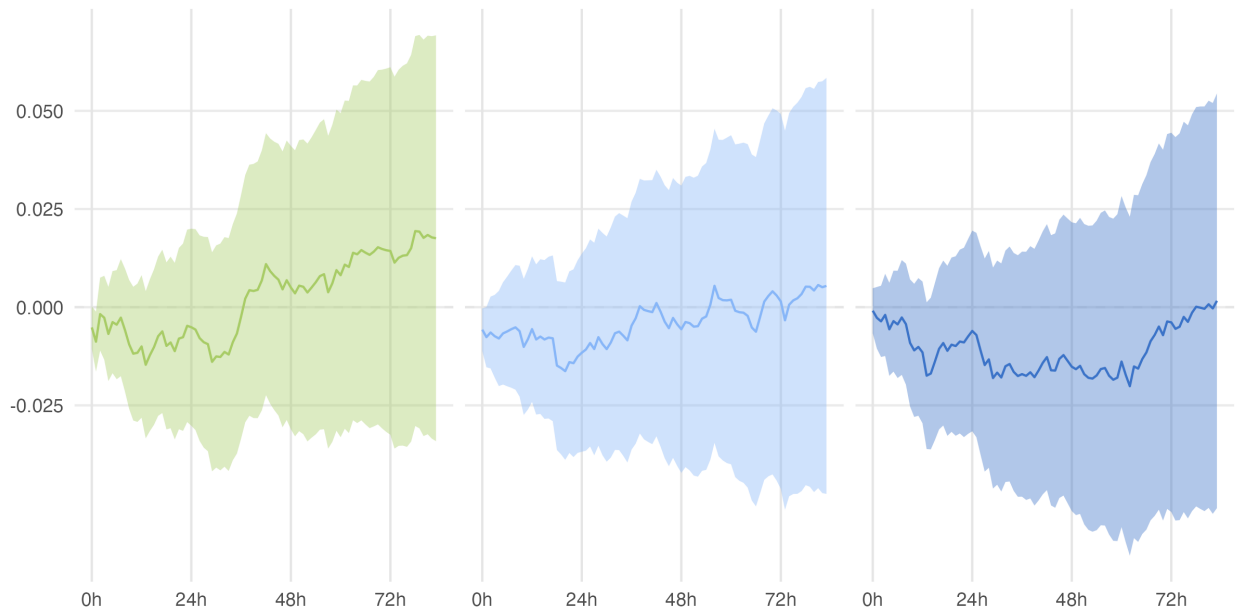
Examining post-event consumption in more depth: We conducted a set of exploratory analyses that aggregated charging activity into progressively wider post-event windows, ranging from immediately after the event to seven days later. This approach captures the cumulative impact of the treatment on charging, 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 – 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 was 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 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 appears to dissipate within four days.

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.

For completeness, we also plot the results for the full set of window lengths in Figure A13. 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.

The results are noisy. The negative point estimates in the discount groups suggested 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 4.3.1, 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 appeared to be suggestive evidence of slightly increased charging with the app, 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. 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, further supporting our findings in Section 4.2.1.

4.3.5 Substituting from home charging

Electricity system and network operators, as well as retailers, may be interested in whether consumers shifted charging from home to public stations. 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 customers also supplied by the partner utility. 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 A3 and Table A13 present the results from this analysis. 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.

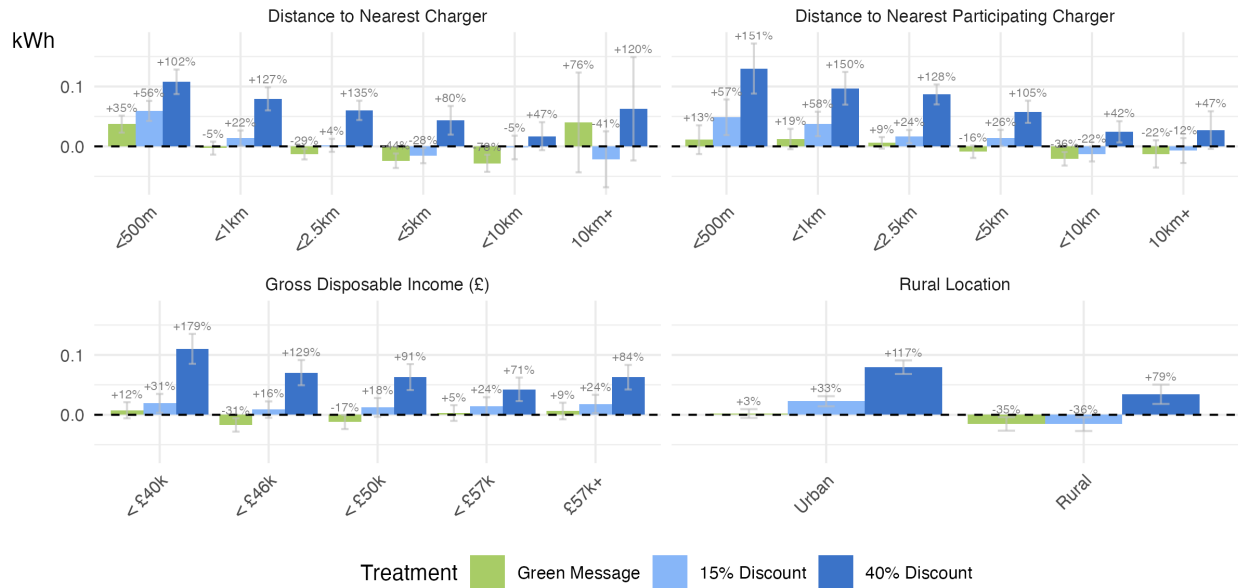
5 Conditional average treatment effects

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 A2.4 – Tables A6 and A7, which report every subgroup interaction for the kWh and has-charged outcomes, respectively). 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,⁴² 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.

Figure 10: Treatment effects by geography



Notes: This figure shows how treatment effects vary by geography for kWh charged. 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 MSA average total income. Bottom-right: urban users show higher baseline usage and stronger treatment effects.

Treatment effects varied by user profile, reflecting differences in charging demand and price sensitivity. Treatments were most effective among local users and almost null for more occasional users who rely on the partner app during weekend and holiday trips away from home (left-hand side of Figure A5).⁴³ 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

⁴²We excluded plug-in hybrids from this analysis

⁴³These users typically charged within 5 km of their listed home address, based on the clustering analysis described in more detail in Section 3.2.

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 A5).⁴⁴ 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.

We also found considerable variation in responses by location, particularly based on proximity to charging infrastructure (top left-hand side of Figure 10). 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 demand flexibility. As expected based on the analysis presented in Section 4.3.3, 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 10). The intervention was also more effective among customers in urban, rather than rural areas (bottom right-hand side of Figure 10), potentially due to greater distances to chargers or greater access to off-street home charging.

Treatment effects varied systematically with local socioeconomic context. As shown in Figure A5, responsiveness followed a U-shaped pattern across income quintiles: price effects were strongest in lower-income MSOAs and in the highest-income quintile, and weaker in middle-income areas.

The stronger responsiveness observed in lower-income areas may reflect greater price sensitivity or more limited access to home charging. Public charging remains substantially more expensive than home charging, and prior work suggests that lower-income households tend to exhibit higher price responsiveness in energy and fuel markets (West

⁴⁴The 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.

and Williams, 2017). Limited access to off-street parking - correlated with lower property values (Chester et al., 2015; Zhang and Fan, 2025) - may also increase reliance on public infrastructure.

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 A7). 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 A7).

Treatment effects also varied by the timing of events. The periods with the largest absolute (kWh) increases did not coincide with those showing the largest relative responses. Although the increase in kWh was greatest in the morning, reflecting higher base level, relative changes compared to the control group were largest in the evening (left-hand side of Figure A8), indicating higher price elasticity during evening hours.

Price reductions generated larger absolute increases on weekends, reflecting higher baseline charging levels, but relative effects were similar across weekdays and weekends (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.

6 Welfare impacts

Figure 11 shows the implied demand curve for public EV charging and the resulting changes in consumer surplus under the 15% and 40% price reductions. Given our field experiment, we are very confident about three points on the same demand curve (control,

15%, and 40% price reduction). At the control price of £0.70/kWh, consumers purchased 25,524 kWh, spending £17,867. A 15% discount to £0.60/kWh increased demand to 33,127 kWh, with total spending rising to £19,876, while a 40% discount to £0.42/kWh increased demand to 55,344 kWh, with total spending rising to £23,244. (These figures were calculated using the treatment coefficients from Table A3, multiplied by the average number of customers per trial arm (27,428) and the number of events (18).)

Consumer surplus gains were driven by three components: inframarginal consumers who paid lower prices on charging they would have undertaken anyway, customers who switched to the partner app from other payment platforms, and genuinely new charging demand. We reason that app-switchers and inframarginal consumers valued price reductions “pound for pound,” making their consumer surplus appear as rectangles rather than triangles. Assuming that non-app-switching demand reflected demand creation (rather than substitution), total consumer surplus rose from £4,499 at the control price to £7,777 under the 15% discount and to £17,908 under the 40% discount. These correspond to £0.39 and £0.77 of consumer surplus per £1 spent by customers, respectively, compared with £0.25 at baseline. The additional consumer surplus generated by the price reductions is £3,279 in the 15% arm and £13,409 in the 40% arm.⁴⁵

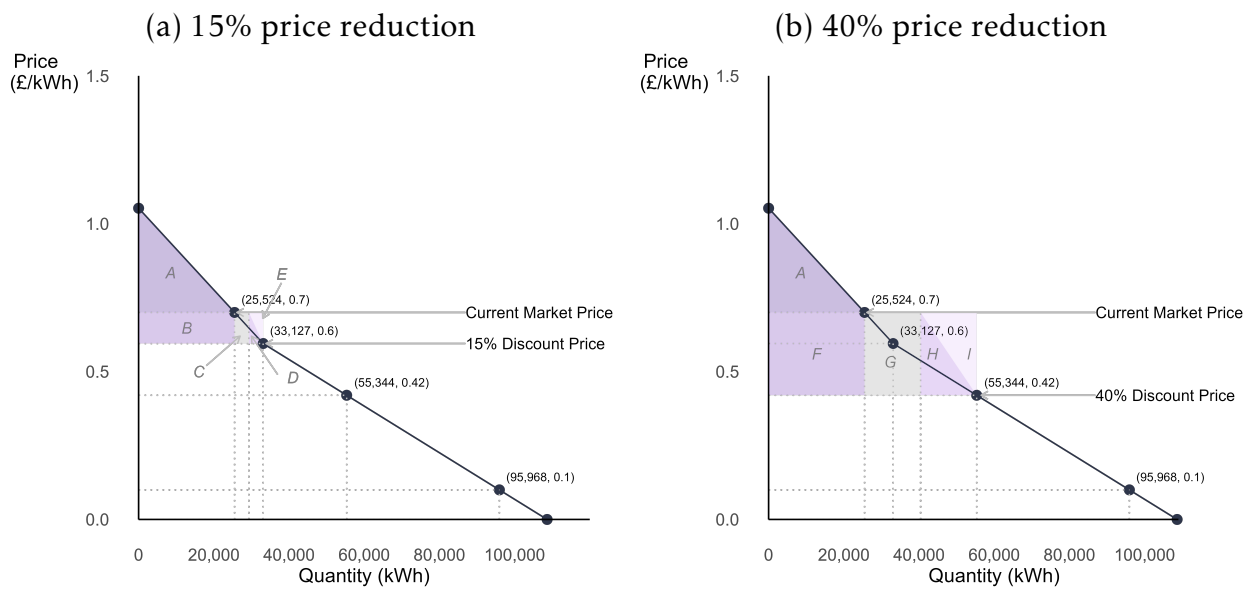
The results imply substantial consumer welfare gains from dynamic price reductions in public EV charging. Scaling similar events over the year could materially reduce the energy cost per mile for drivers without off-street parking, bringing public charging closer to parity with internal combustion engine vehicles and narrowing the gap with home charging (from 15–24p to 9–14p – where the cost per mile for internal combustion engine vehicles ranges from 13 to 17p, and home charging ranges from 2 to 7p per mile (Office for Zero Emission Vehicles, 2024; Zapmap, 2025)).

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. However, we have not attempted to estimate producer surplus effects, as we lack sufficient data on the underlying cost structures of key participants (both our implementation partner, and CPOs). With that noted, we have considered whether our results might indicate that CPOs should strategically lower prices to increase revenue and profits. The headline price elasticities were high. In practice, however, we do not believe such

⁴⁵ Assuming demand creation is *conservative* because substitution would imply a similar “pound-for-pound” valuation of the extra consumption, similar to the inframarginals and app-switchers. In all cases, these “pound-for-pound” valuations assume no switching costs / disutility from choosing an alternative source of charging. Full details of these calculations, including alternative assumptions under demand substitution and extensions to marginal-cost pricing, are provided in Appendix A3.

price reductions would increase margins. Crucially, we found no evidence of meaningful switching *between* CPOs (Section 4.3.3), implying that discounting would not pull demand away from rival CPOs in a way that offsets the lost margin on discounted sessions. Indeed, after netting out app switching, arc price elasticities were below 1 in absolute value, indicating inelastic demand; for example, -0.85 for the 15% price reduction and -0.90 for the 40% price reduction. In the absence of lower wholesale prices or turn-up payments, therefore, we would expect dynamic discounts to largely cannibalize existing revenue rather than generate profitable incremental volume.

Figure 11: Consumer surplus change from 15% and 40% price reductions



Notes: This graph shows the demand curve for electric charging accounting for app substitution. We have assumed a kinked linear demand curve for simplicity, with the kink at the 15% discount level, and we have extrapolated the rest of the curve linearly. The 15% price reduction increased demand to 33,127 kWh, and the 40% price reduction increased demand to 55,344 kWh. In each case, we believe approximately half of the increase arose from app switching—that is, charging through the partner app by customers who would have charged anyway via a different payment platform (Section 4.3.1). In both cases, Area A shows the consumer surplus (£4,499) at the original non-discounted price of £0.70/kWh, where demand is 25,524 kWh. Area B shows the change in consumer surplus for inframarginal customers who were consuming before the subsidy but now pay less. Assuming half of the extra demand from 25,524 kWh to 33,127 kWh was app substitution, the consumer surplus for these customers is valued “pound for pound,” as these customers were previously paying £0.70/kWh but were instead able to pay £0.60/kWh. This is shown by Area C for the 15% reduction. As discussed in Section 4.3.4, we are uncertain whether the extra non-app-switching demand reflects genuine demand creation or demand substitution. Area D is the consumer surplus for this increase in demand if it represents demand creation. Area E shows the additional consumer surplus if the demand increase from these non-app-switchers instead reflects substitution; in that case, as with app-switchers, these customers would value the price reduction pound for pound, assuming no disutility from shifting charging activity. For the 40% reduction, we apply similar logic. Area G represents the consumer-surplus gain from app-switchers, assuming they were previously paying £0.70/kWh but were instead able to pay £0.42/kWh. The remaining half of the increase (from 25,524 to 55,344 kWh) comes from *non*-app-switchers. Area H shows the consumer surplus if this increase represents demand creation. Area I shows the additional consumer surplus if the increase instead reflects substitution.

From a system perspective, our treatments generated 18.7 MWh of net new charging

after accounting for app switching over our 11-week trial, suggesting a potential role for targeted price reductions as a grid-balancing tool. Distribution Network Operators (DNOs) in Great Britain pay for upward demand response to manage local constraints. For example, UK Power Networks reports an average cost of £446/MWh for demand turn-up services (UK Power Networks, 2025). Using this benchmark, we conducted an illustrative welfare exercise in which a DNO subsidizes public EV charging to deliver turn-up in constrained areas. Applying our estimated demand creation elasticity and valuing avoided turn-up costs alongside consumer surplus and carbon externalities, we found that such a subsidy could be welfare-enhancing, with a marginal value of public funds of approximately 1.4 under baseline assumptions.⁴⁶

7 Conclusion

We found that public EV charging demand through the partner platform was responsive to price decreases. 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, we acknowledge that these estimates capture short-run behavioral responses to temporary price signals within the partner 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. We do find substantial customer switching from competing platforms to the partner app 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

⁴⁶Full details of this calculation, including assumptions and sensitivity to spatial targeting, are provided in Appendix A3.

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, supporting 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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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 Figure A1, 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.

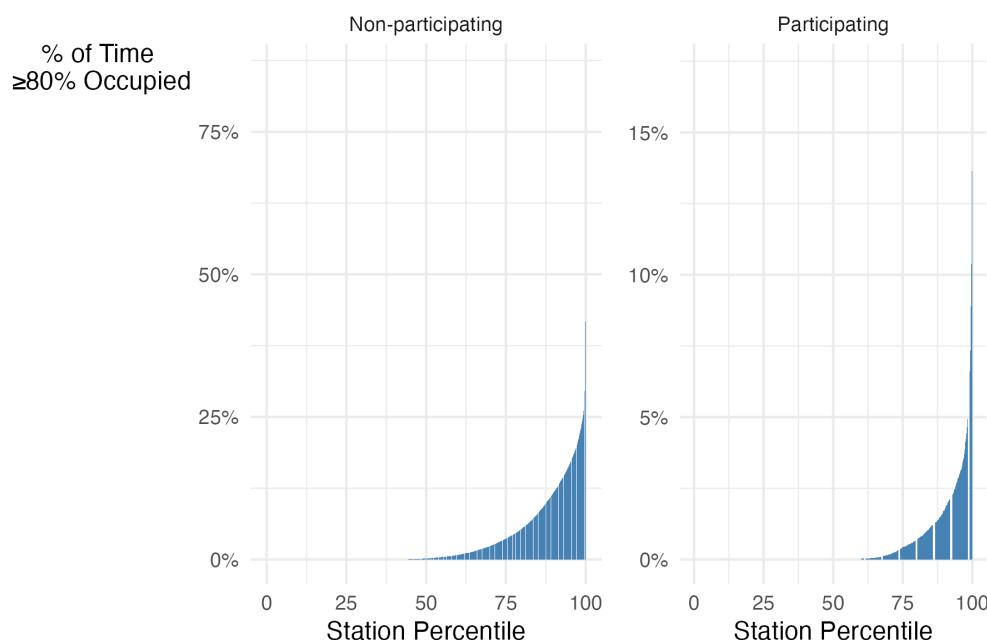


Figure A1: Share of time each location was at or above 80% occupancy during the trial by participation

A1.2 User Income Area vs Charger Income Area

Figure A2a illustrates the charging destination choices of EV users based on the average income of their home MSOA relative to the average income of the MSOA where the charging event occurred during event trials. For construction, the MSOA median annual income was first discretized into five equal quintile groups. The Sankey diagram visually represents the charging pathways, where the thickness of each flow stream is directly proportional to the total number of charges that occurred between the user's home income area and the charging location's income area. The analysis reveals a strong pattern of income-matching: on average, for each user income quintile, approximately 40% of transactions occur in an area of a similar income group, with this share gradually declining as the income difference between the user's home and the charger's location increases.

Figure A2b shows similar patterns for the year preceding the trial. The overall structure of flows is largely unchanged, except that lower-income users charge less frequently in the pre-trial period. This aligns with the evidence in Figure 10, which indicates that users living in lower-income MSOAs exhibit higher price elasticities during the trial.

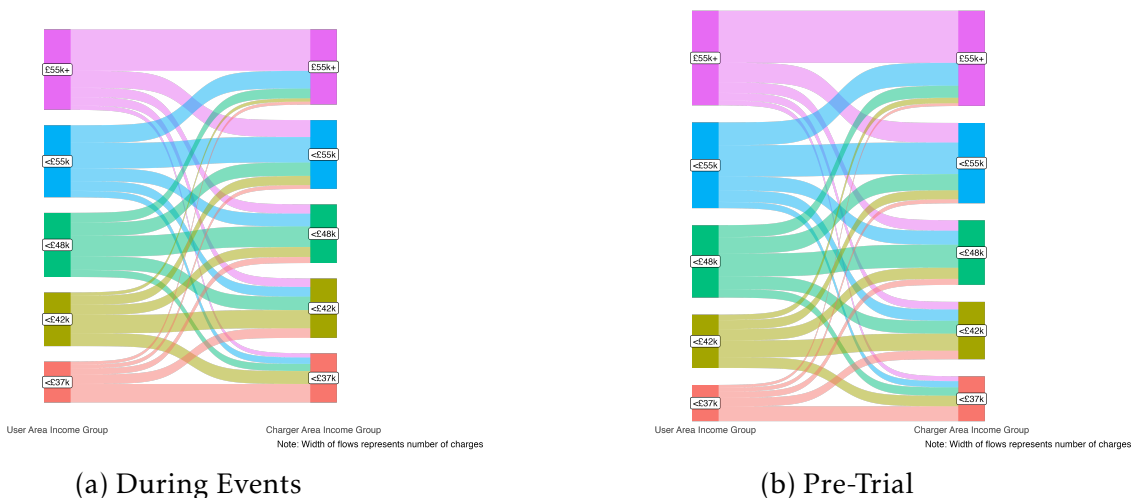
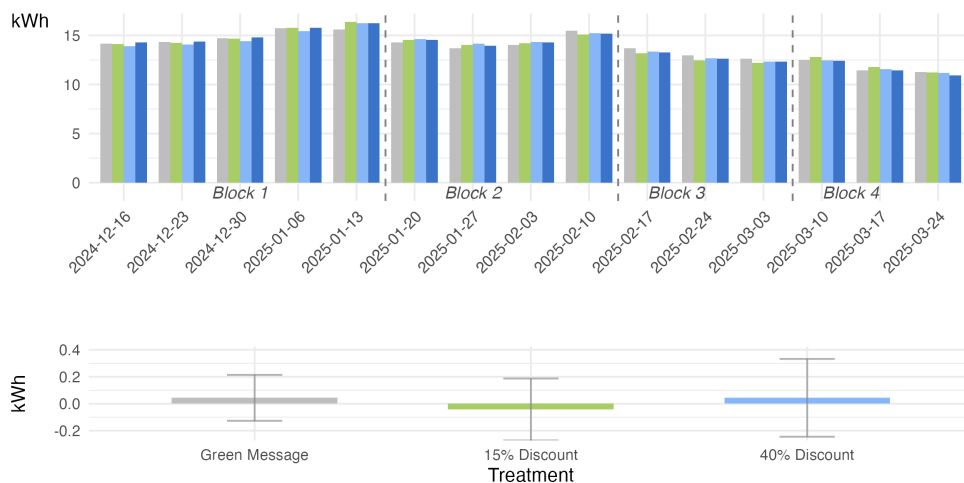


Figure A2: Flow of Charges: User Area Income vs. Charger Area Income

A1.3 Home Charging

Figure A3: 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 A13, 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 customers of the partner utility – observed over the 74 trial days. No statistically significant differences in home energy use are detected across treatment groups.

A1.4 Conditional average treatment effects (CATEs) analysis figures

To contextualize the heterogeneity analysis presented in the CATE figures below, Figure A4 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.

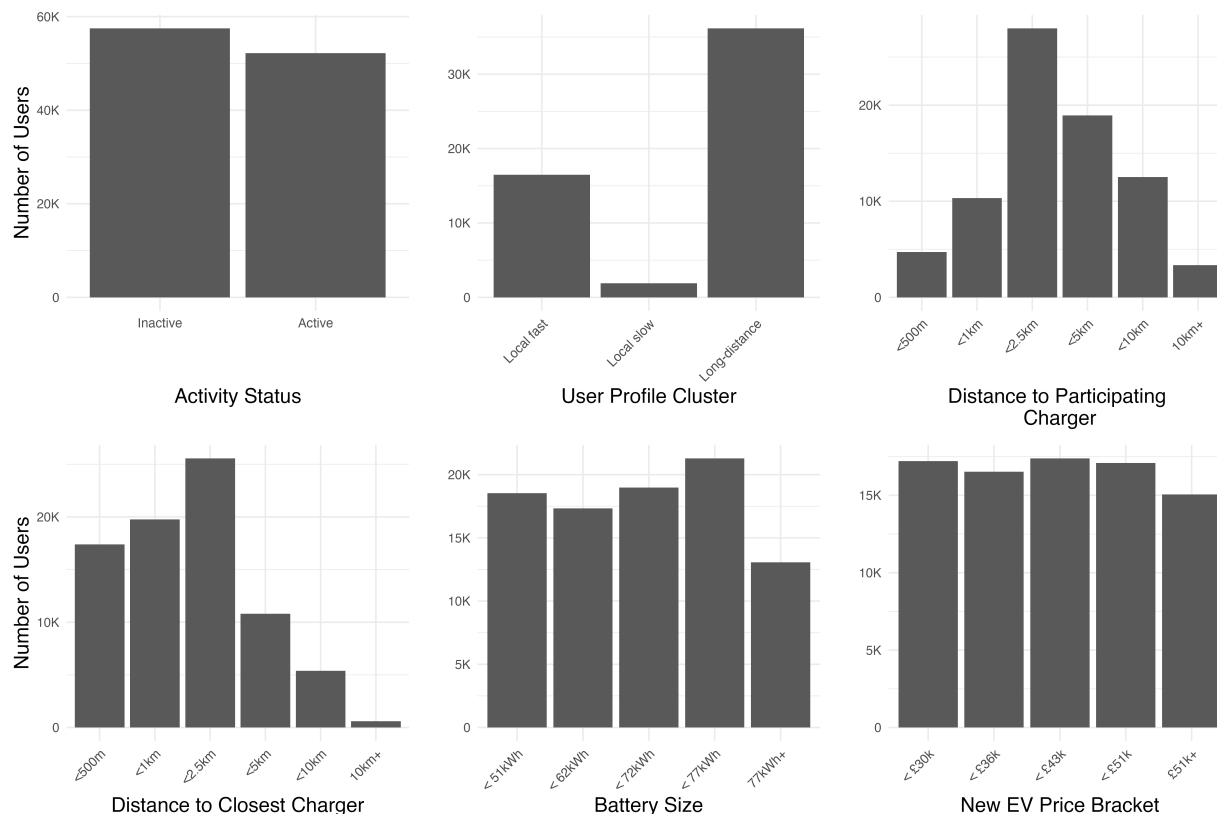


Figure A4: User characteristics frequency

A1.4.1 CATEs by user type

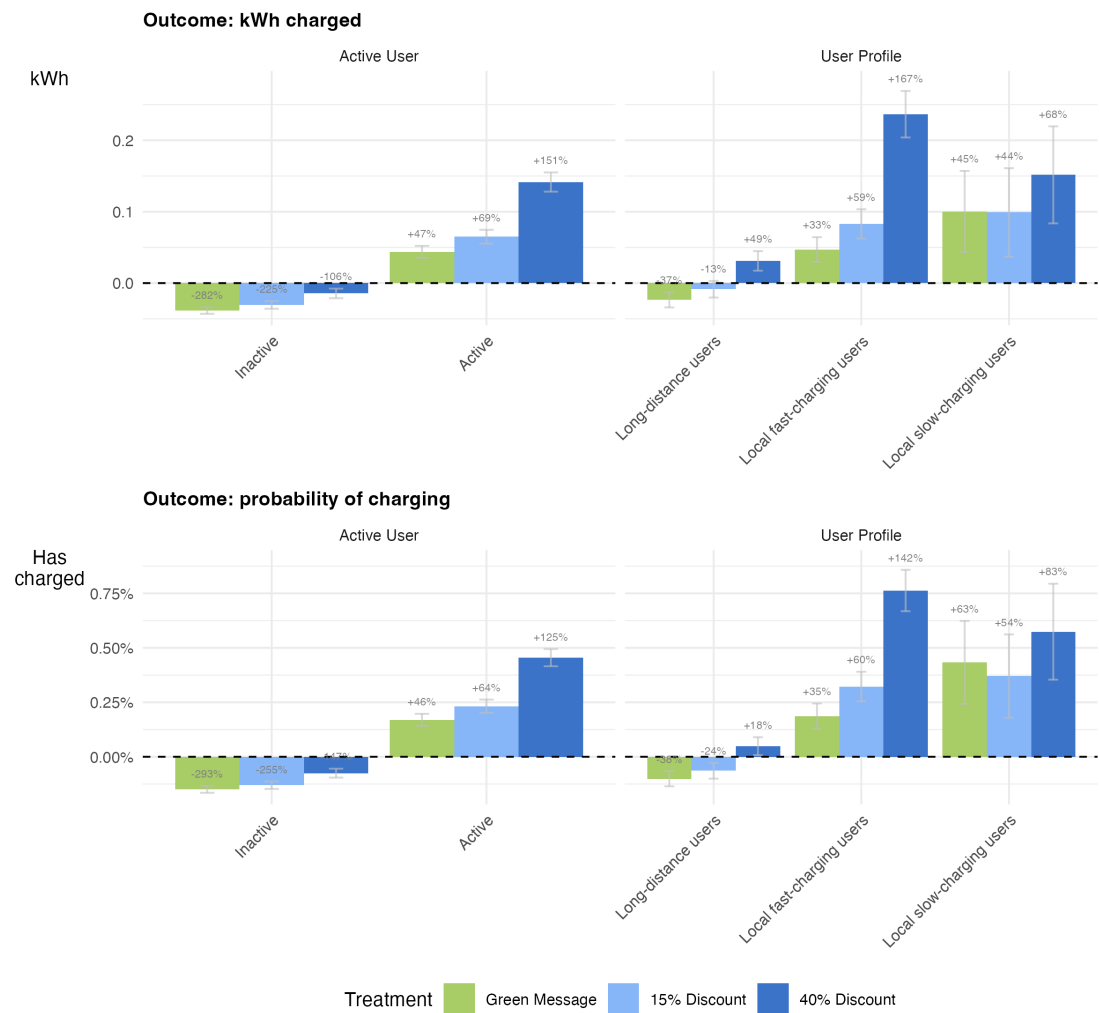
We explored how treatment effects vary by different aspects of customers/users. The left-hand side of Figure A5 shows the interaction between treatment groups and whether a user was "active", measured as having charged at least once in the period Jan-Nov 2024.

The right-hand side of Figure A5 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.

The bottom row of Figure A5 repeats this analysis for the binary outcome of whether a user charged during events; the same patterns hold. (Full tabular results for both outcomes and all subgroups are in Tables A6 and A7.)

Figure A5: Treatment effects by user characteristics

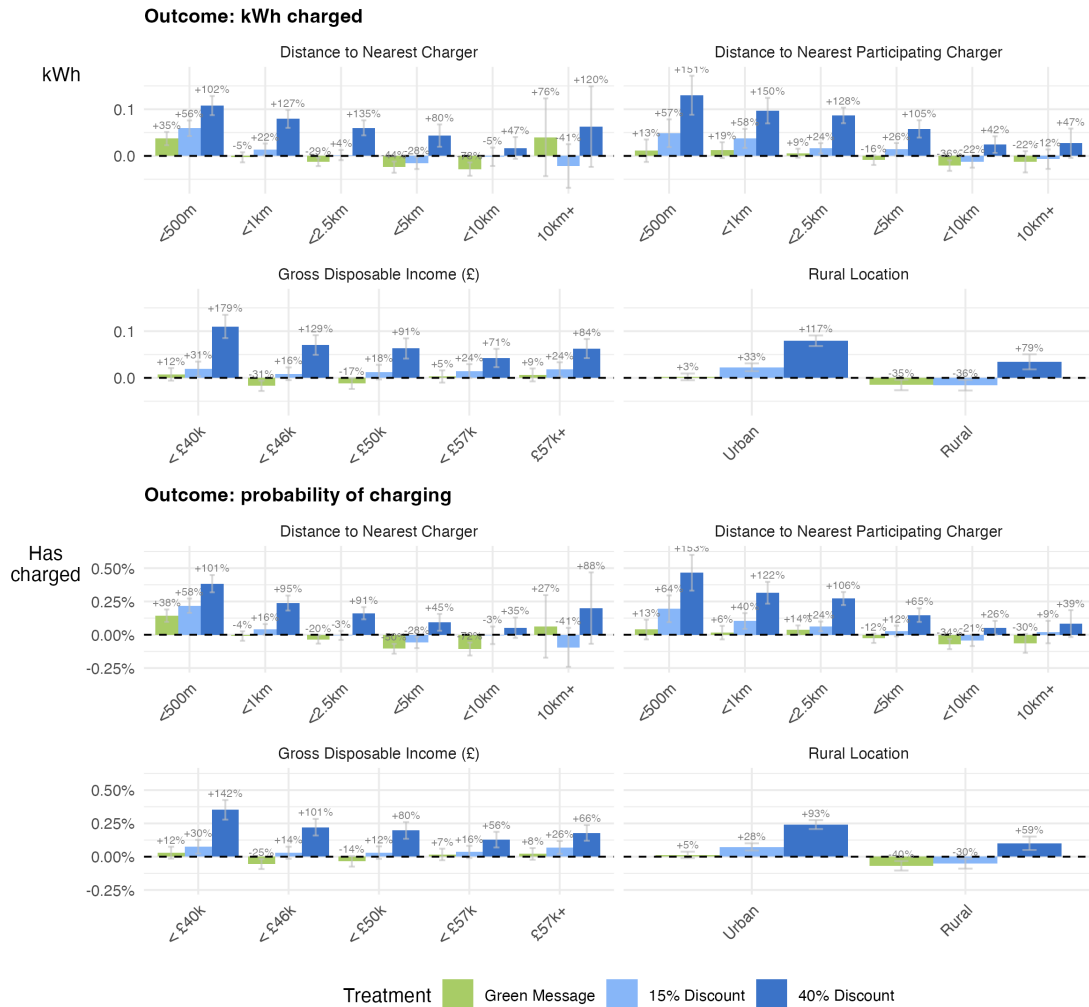


Notes: This figure shows how treatment effects vary across user types. The left panel splits users by prior app activity (i.e., whether they had charged at least once before the trial), while the right panel breaks users into clusters based on pre-trial charging behavior. The top row shows the effect on kWh charged; the bottom row shows the effect on the binary outcome of whether a user charged during events. Results suggest stronger treatment effects among more engaged users, particularly local fast-charging users, and are consistent across both outcomes.

A1.4.2 CATEs by geography

We next analyzed how treatment effects vary by distance to the nearest charger and other geographical parameters, shown in Figure A6 in the main text (which reports both the kWh and has-charged outcomes; see Tables A6 and A7 for tabular results). There was a clear negative relationship between distance and engagement with dynamic pricing events, and an increase in treatment effect for the green message as long as the distance to the nearest charger was 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. Effects were stronger for the 15% and 40% price-drop treatments when measured against distance to the nearest *participating* charger, with effectively no impact past 10 km. By MSOA income, the data suggest a U-shaped relationship: dynamic pricing events elicited stronger responses in lower-income areas, while the highest income quintile also showed a greater reaction across all treatments except the green message. Urban users exhibited higher kWh charging than rural users overall and a greater increase in charging relative to their own control group. These patterns hold for both outcomes shown in Figure A6.

Figure A6: Treatment effects by battery capacity and EV price



Notes: This figure shows how treatment effects vary by geography, for kWh charged (top row) and the binary outcome of whether a user charged during events (bottom row). 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. Patterns are consistent across both outcomes.

A1.4.3 CATEs by EV characteristics

We next analyzed how treatment effects varied by characteristics of the customer's vehicle. The left-hand side of Figure A7 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.

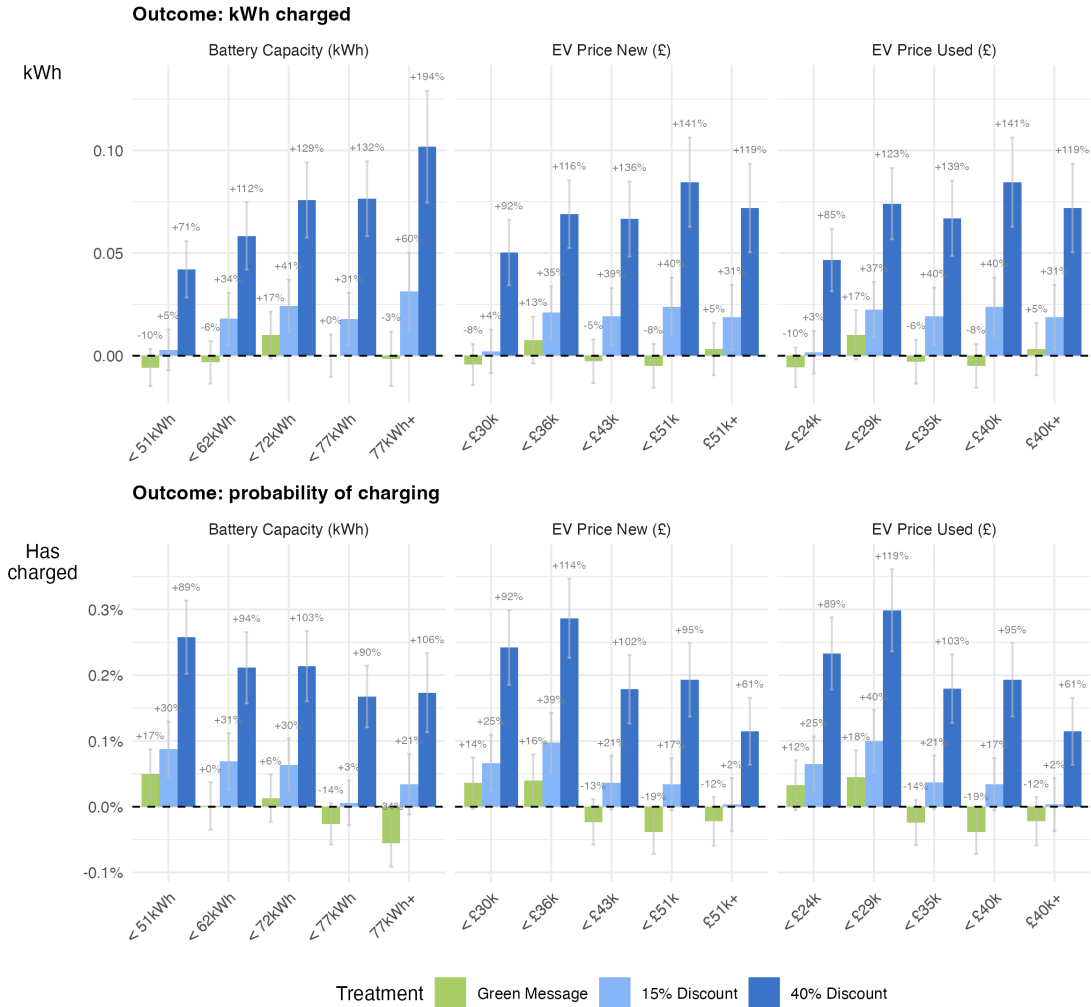
The middle panel of Figure A7 shows the interaction between treatment groups and

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.

Finally, the right-hand side of Figure A7 shows the interaction between treatment groups and the price of EV (used), based on the same pricing sources. There was no clear interaction between treatment effects and value of the EV.

The bottom row of Figure A7 repeats this analysis for whether or not participants charged during the events. Here we found 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 group is similar). This suggests that part of the larger consumption by battery size is mostly a mechanical result of the larger batteries. When looking at EV prices, we found EV owners in the highest price quintiles are less likely to charge than other drivers, both in absolute and relative terms. (See ??? for tabular results.)

Figure A7: Treatment effects by battery capacity and EV price



Notes: This figure shows how treatment effects vary by vehicle characteristics, for kWh charged (top row) and the binary outcome of whether a user charged during events (bottom row). 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. Smaller-battery owners are slightly more likely to charge on the binary outcome, suggesting the kWh gradient partly reflects battery capacity mechanically; high-value EVs are associated with lower event participation on both outcomes.

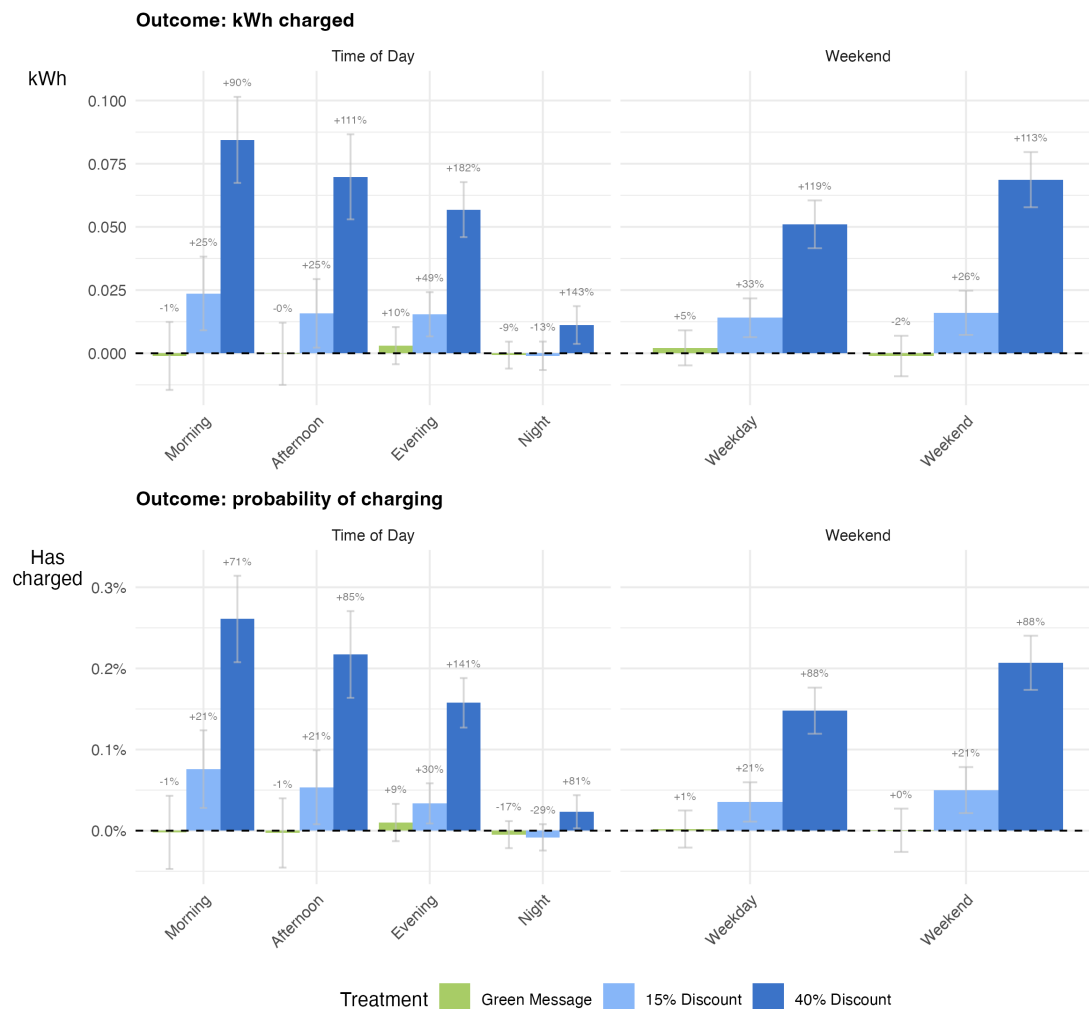
A1.4.4 CATEs by time-related event characteristics

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.

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.

As before, we find very similar results when looking at whether or not participants charged during events (bottom row of Figure A8; see ??? for tabular results).

Figure A8: Treatment effects by time of day and weekday vs weekend

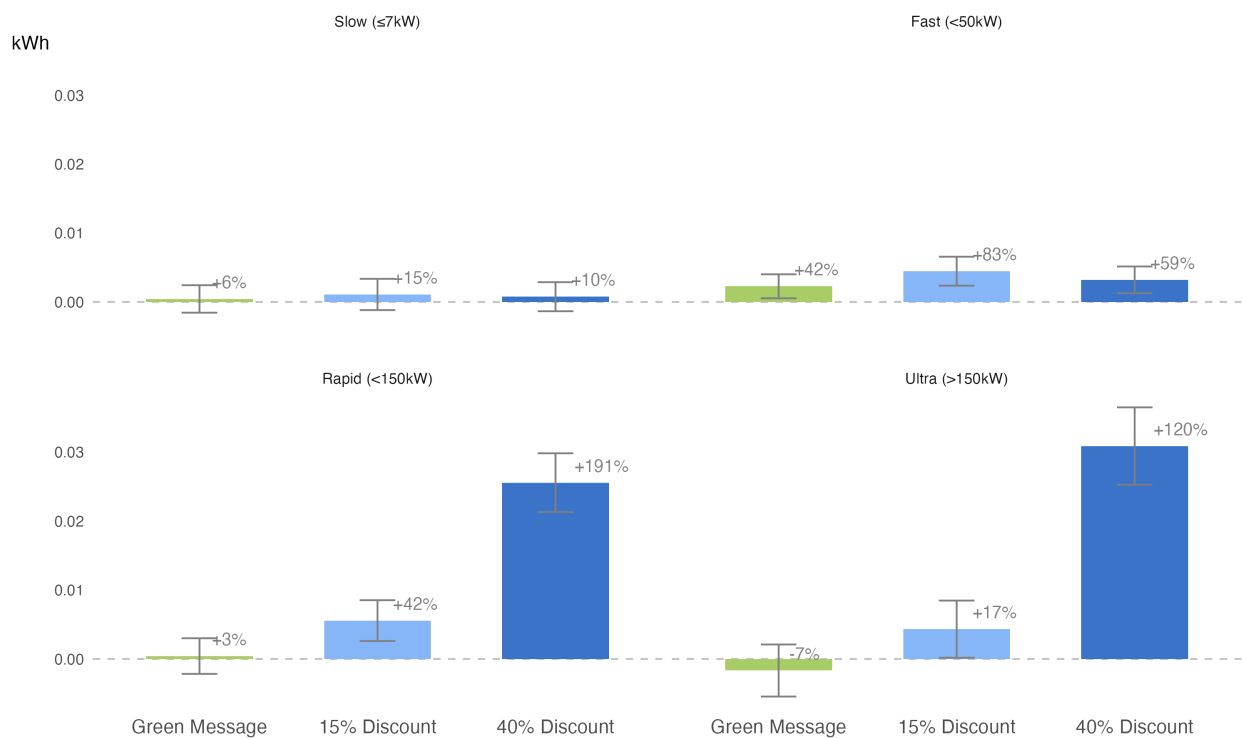


Notes: This figure shows how treatment effects vary by time of day (left) and weekday vs. weekend (right), for kWh charged (top row) and the probability of charging during an event (bottom row). The absolute effect is highest in the morning, but relative increases are strongest in the evening. Weekend charging is higher overall, but relative treatment effects are similar to weekdays. The pattern is consistent across both outcomes.

A1.5 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 rapid and ultra chargers (>50kW), which is likely driven by the composition of chargers among participating CPOs and the short span of the events (see Table A1).

Figure A9: 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 rapid and ultra chargers (>50kW), reflecting the charger mix among participating operators. See Table A1 for details.

A1.6 App substitution by income areas

For each income group, we compare the total increase in kWh implied by receipts to the increase observed directly in smart-meter data for the same charging stations. The difference represents the portion of receipts growth attributable to “app switching”—users moving transactions from other platforms to the partner app because of the discount.

- Below-median income: The receipts-based increase is 17,848 kWh, while the smart-meter increase is 9,123 kWh, implying that around 51% of the apparent rise in receipts reflects new charging and 49% substitution.
- Above-median income: The receipts-based increase is 17,606 kWh versus 8,185 kWh in smart-meter usage, implying approximately 54% substitution.

Across both income groups, these results align with the overall estimate of about 49% app substitution reported in the main analysis.

Figure A10: App substitution estimates for below median income MSOAs

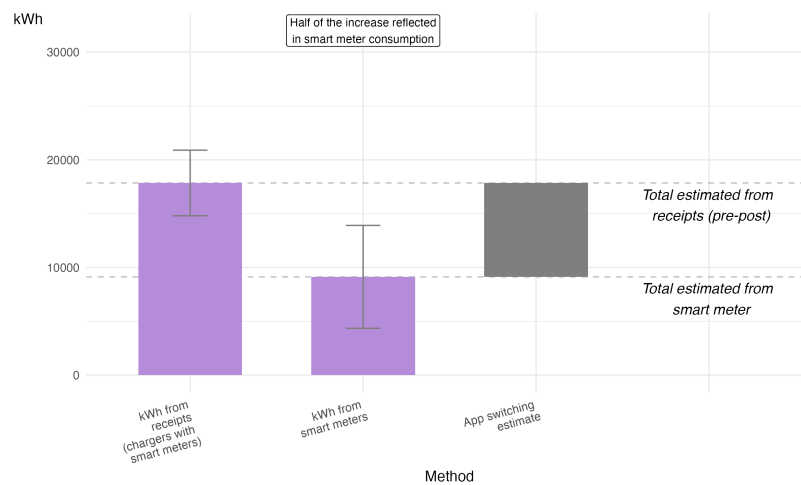
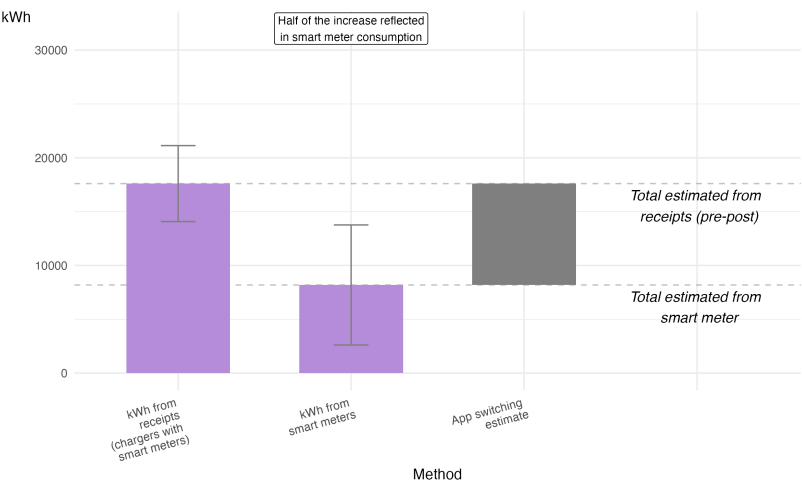
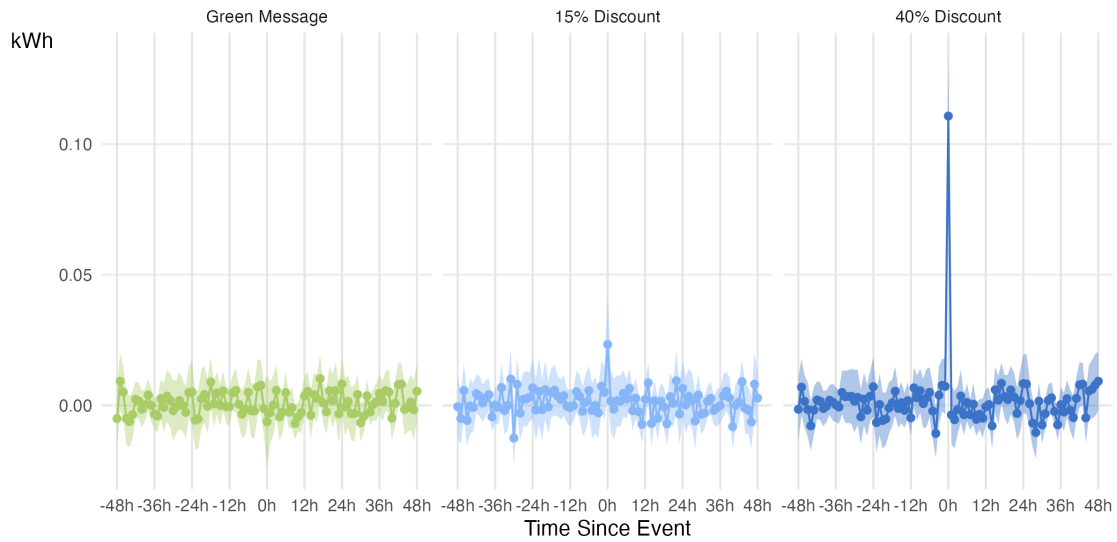


Figure A11: App substitution estimates for above median income MSOAs



A1.7 Event study - 48 hours before and after event

Figure A12: 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.

A1.8 Displacement using increasing time windows

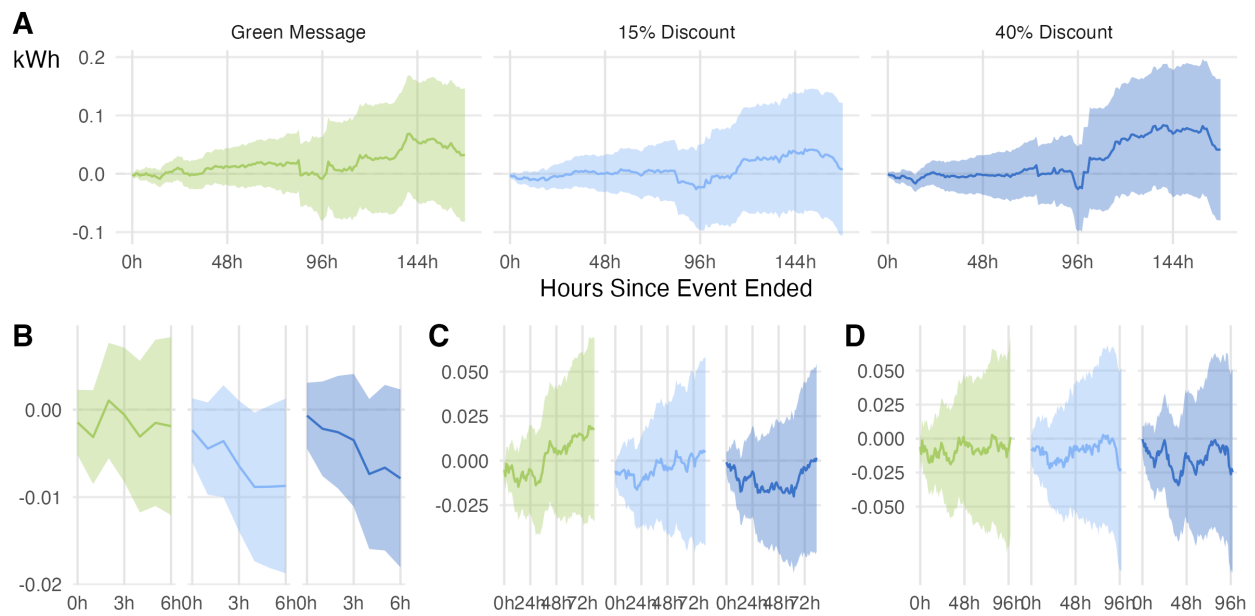


Figure A13: Treatment effect (kWh) after the events

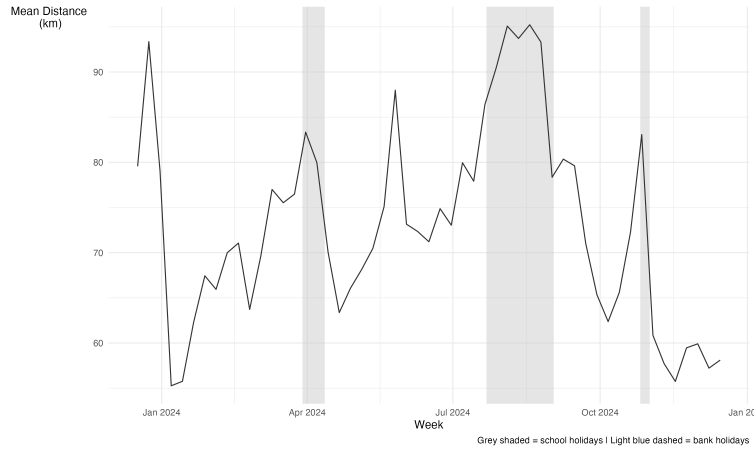
A1.9 Descriptive figures

Using a k-means clustering algorithm, we identified three distinct behavioral segments, which we grouped into three main user profiles.⁴⁷ 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 suggest 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 users 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).

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, where >96% of these charging sessions happening at fast, rapid, or ultra-fast chargers. The overlap between local and long-distance users was minimal, indicating distinct usage patterns. The density plot Figure A16 show the distribution of users' median distance to chargers.

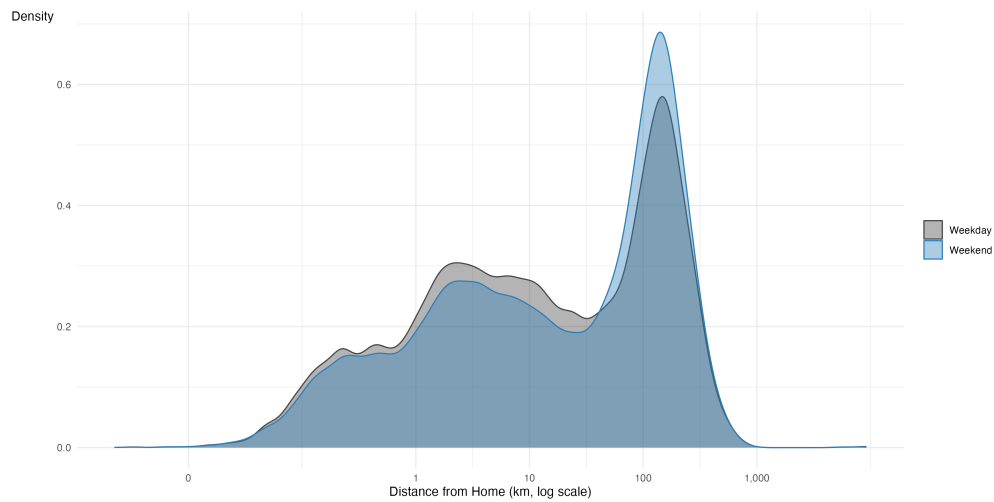
⁴⁷We have data on pre-trial charging behavior for 54,547 out of 109,711 users in our sample. In order to unpack user differences, we classified users into three groups using a k-means clustering algorithm, based on four behavioral indicators: the median distance from home to charging points, the share 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.

Figure A14: Weekly mean distance to chargers



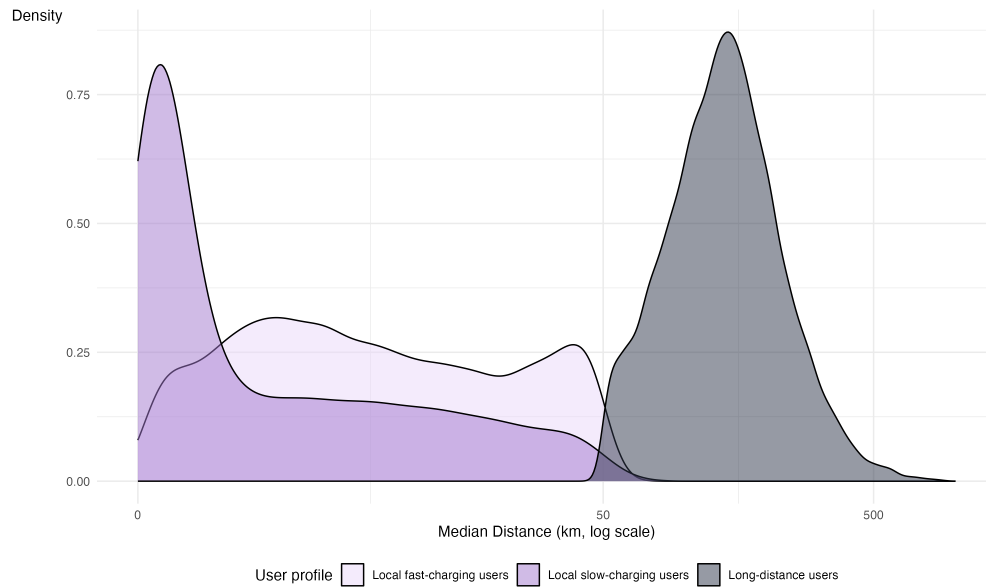
Notes: Mean distance between users' home postcodes (centroids) and charger locations for all charging sessions in 2024. Distances increase notably during school and bank holidays, suggesting greater use of public charging during longer trips.

Figure A15: 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.

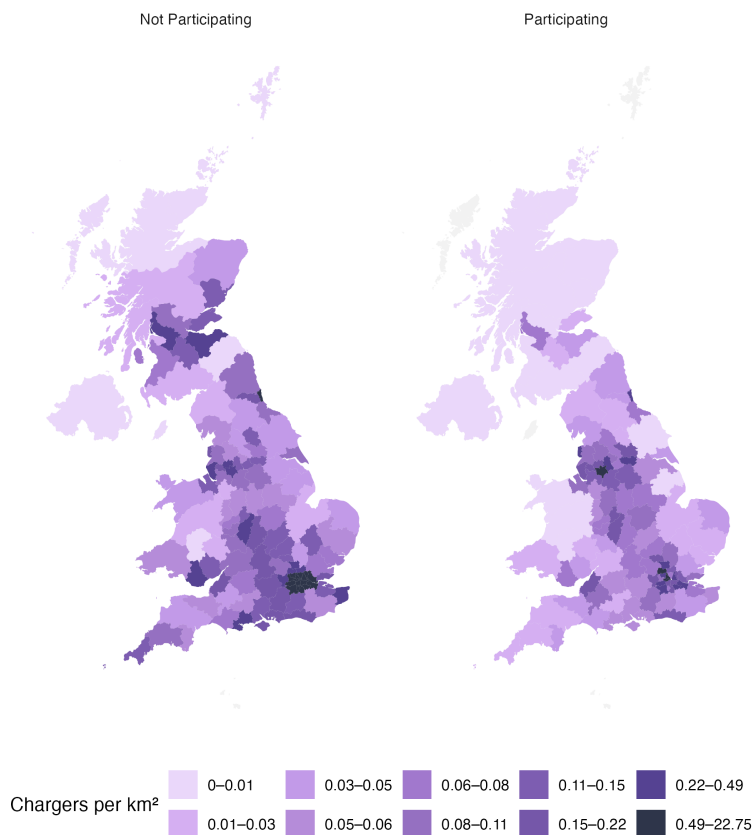
Figure A16: Median distance to charger by user profile



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.

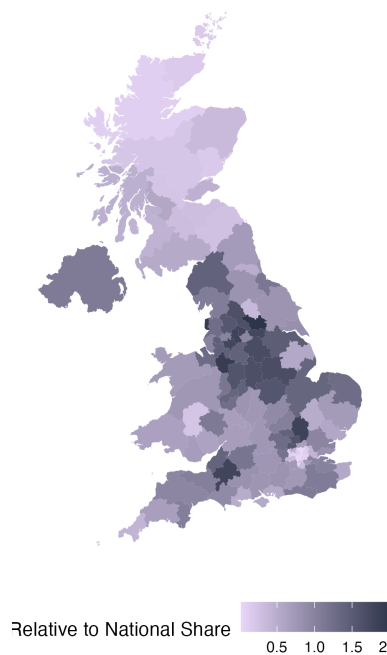
A1.10 Maps

Figure A17: 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.

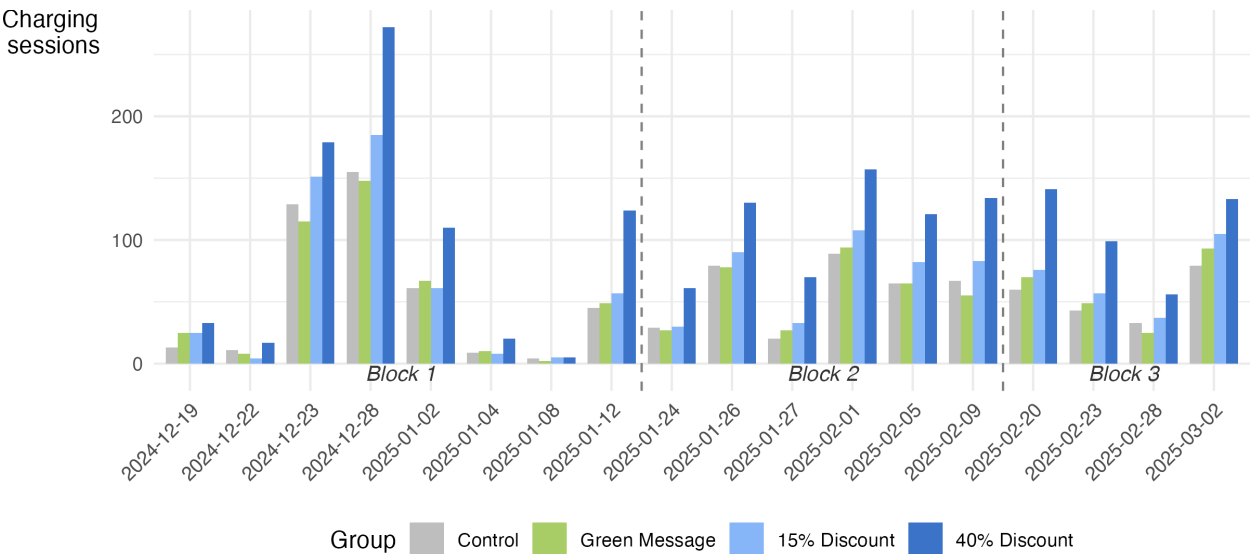
Figure A18: Share of participating charging stations relative to the national average by postcode area



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.

A1.11 Receipts per event

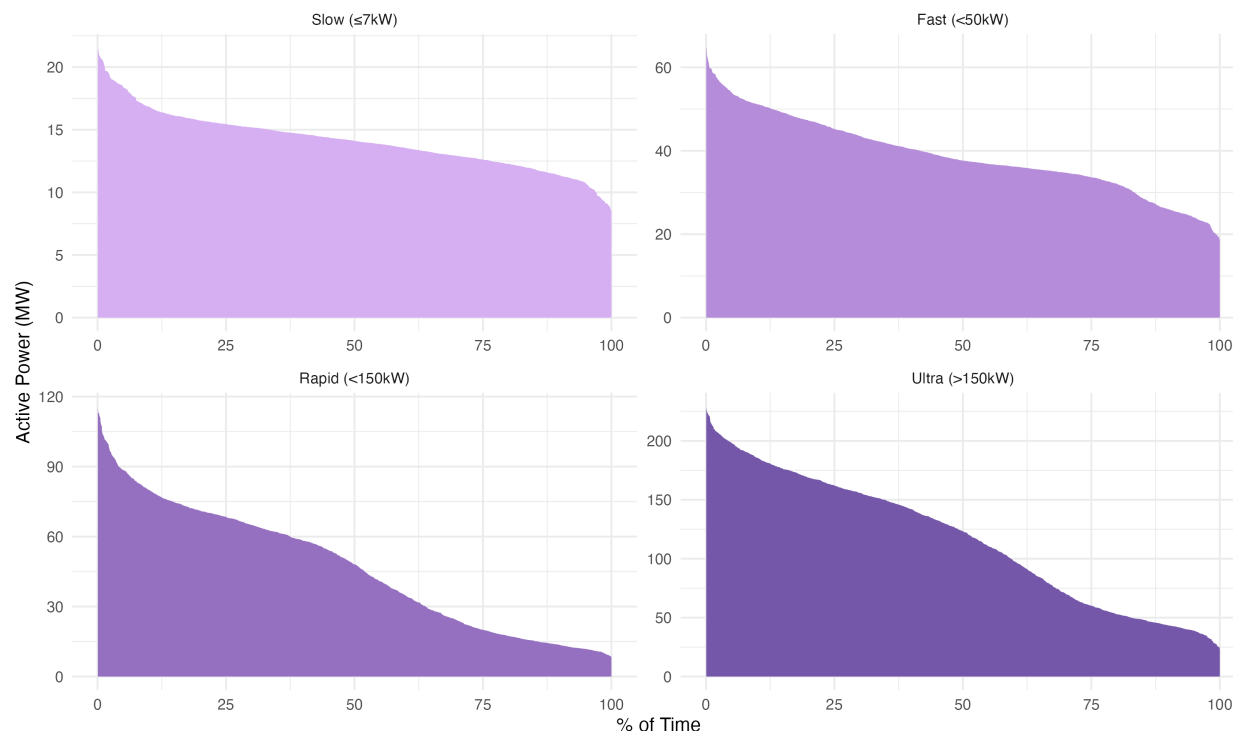
Figure A19: Number of charging sessions during dynamic pricing events, by treatment



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.

A1.12 Load Duration Curve by charger speed

Figure A20: Load duration curve by charger speed



Notes: This figure presents the Load Duration Curve (LDC) for the chargers in the partner network 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.

A1.13 Time of Use versus flat rate chargers

Using data 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 variable pricing. When focusing on operators that later participated in the flexibility trial, only 13% of

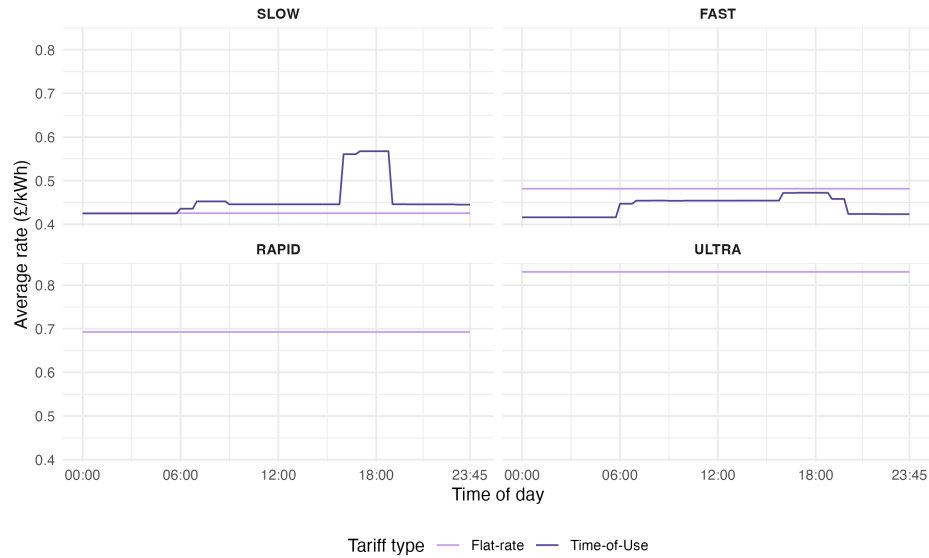


Figure A21: Daily price profiles by speed

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 shown in Figure A21 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 Participating chargers distribution

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%)

A2.2 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	9am–noon	Morning	18	Day-ahead
	02/01/2025	noon–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
<i>Washout</i>					
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	noon–2pm	Afternoon	19	Day-ahead
	05/02/2025	noon–2pm	Afternoon	19	Day-ahead
	09/02/2025	5pm–7pm	Afternoon	24	Day-ahead
<i>Washout</i>					
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

A2.3 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
<i>Variables</i>				
Green Message	0.0015 (0.0027)	4.4×10^{-5} (8.94×10^{-5})	0.0065 (0.0153)	3.78×10^{-5} (8.85×10^{-5})
15% Discount	0.0154*** (0.0030)	0.0004*** (9.7×10^{-5})	0.0682*** (0.0152)	0.0004*** (8.85×10^{-5})
40% Discount	0.0604*** (0.0039)	0.0018*** (0.0001)	0.2327*** (0.0146)	0.0018*** (0.0001)
kWh - September	0.0006*** (0.0001)	1.63×10^{-5} *** (3.1×10^{-6})	0.0004*** (8.07×10^{-5})	
kWh - October	1.66×10^{-5} (1.43×10^{-5})	4.97×10^{-7} (4.37×10^{-7})	9.29×10^{-6} *** (3.24×10^{-6})	
kWh - November	0.0018*** (0.0001)	5.81×10^{-5} *** (3.04×10^{-6})	0.0016*** (6.02×10^{-5})	
Active User	0.0344*** (0.0032)	0.0016*** (9.3×10^{-5})	0.4781*** (0.0140)	
<i>Fixed-effects</i>				
Date	Yes	Yes	Yes	
<i>Avg. Outcome Control</i>				
	0.0517	0.0020	0.0020	
<i>Fit statistics</i>				
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

Notes: 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

Table A4: Impact of the experimental stimuli on kWh charged

Dependent Variable: Model:	Consumption (kWh)		
	(1)	(2)	(3)
<i>Variables</i>			
Green Message	0.0005 (0.0027)	-0.0007 (0.0041)	0.0013 (0.0040)
15% Discount	0.0150*** (0.0031)	0.0105** (0.0045)	0.0106** (0.0044)
40% Discount	0.0599*** (0.0040)	0.0553*** (0.0059)	0.0549*** (0.0059)
Green Message × Block 2		-0.0013 (0.0065)	-0.0033 (0.0063)
15% Discount × Block 2		0.0057 (0.0072)	0.0053 (0.0070)
40% Discount × Block 2		0.0077 (0.0094)	0.0098 (0.0092)
Green Message × Block 3		0.0075 (0.0073)	0.0059 (0.0072)
15% Discount × Block 3		0.0119 (0.0081)	0.0141* (0.0079)
40% Discount × Block 3		0.0092 (0.0104)	0.0098 (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)
<i>Fixed-effects</i>			
Date	Yes	Yes	Yes
<i>Avg. Outcome</i>			
Control	0.0517	0.0517	0.0517
<i>Fit statistics</i>			
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*

Notes: Estimates in this table correspond to robustness checks outlined in our pre-registered analysis plan

Table A5: Impact of the experimental stimuli on kWh charged (interacted with sequence)

Dependent Variable:	Consumption (kWh)	
Model:	Full sample (1)	Excluding nighttime events (2)
<i>Variables</i>		
Treatment = Green Message × sequence = 2	0.0072 (0.0059)	0.0072 (0.0059)
Treatment = Green Message × sequence = 3	-0.0020 (0.0047)	-0.0020 (0.0048)
Treatment = Green Message × sequence = 4	0.0013 (0.0040)	0.0016 (0.0063)
Treatment = 15% Discount × sequence = 1	0.0158*** (0.0054)	0.0158*** (0.0054)
Treatment = 15% Discount × sequence = 2	0.0106** (0.0044)	0.0175** (0.0069)
Treatment = 15% Discount × sequence = 4	0.0247*** (0.0066)	0.0250*** (0.0066)
Treatment = 40% Discount × sequence = 1	0.0549*** (0.0059)	0.0813*** (0.0089)
Treatment = 40% Discount × sequence = 3	0.0648*** (0.0081)	0.0649*** (0.0081)
Treatment = 40% Discount × sequence = 4	0.0648*** (0.0071)	0.0651*** (0.0071)
kWh - September	0.0006*** (0.0001)	0.0006*** (0.0001)
kWh - October	1.66×10^{-5} (1.43×10^{-5})	1.99×10^{-5} (1.72×10^{-5})
kWh - November	0.0018*** (0.0001)	0.0020*** (0.0001)
Active User	0.0344*** (0.0032)	0.0425*** (0.0038)
<i>Fixed-effects</i>		
Date	Yes	Yes
<i>Fit statistics</i>		
Observations	1,974,798	1,645,665
Number of Accounts	109,711	109,711
R ²	0.00717	0.00754
Pseudo R ²	0.00189	0.00191
F-test	26.395	30.870
Control	0.0517	0.0606

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

A2.4 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 A6 reports results for kWh charged and Table A7 for the binary outcome of whether a customer charged during the event; in both tables, rows are grouped by moderator (with the regression's sample size noted alongside each group), and columns report the three treatment arms.

Table A6: Impact of the experimental stimuli on kWh charged, by subgroup

	Green Message	15% Discount	40% Discount
User Profile (N = 981,846)			
Long-distance users	-0.0235*** (0.0054)	-0.0086 (0.0059)	0.0311*** (0.0070)
Local fast-charging users	0.0469*** (0.0089)	0.0830*** (0.0105)	0.2365*** (0.0166)
Local slow-charging users	0.1002*** (0.0291)	0.0990*** (0.0316)	0.1516*** (0.0347)
Active User (N = 1,974,798)			
Inactive	-0.0383*** (0.0024)	-0.0306*** (0.0027)	-0.0145*** (0.0034)
Active	0.0438*** (0.0043)	0.0650*** (0.0049)	0.1416*** (0.0069)
Distance to Nearest Charger (N = 1,430,352)			
<500m	0.0371*** (0.0072)	0.0592*** (0.0085)	0.1079*** (0.0105)
<1km	-0.0030 (0.0055)	0.0138** (0.0065)	0.0794*** (0.0098)
<2.5km	-0.0129*** (0.0046)	0.0018 (0.0056)	0.0601*** (0.0082)
<5km	-0.0240*** (0.0062)	-0.0154** (0.0065)	0.0435*** (0.0122)
<10km	-0.0283*** (0.0073)	-0.0019 (0.0101)	0.0170 (0.0119)
10km+	0.0400 (0.0425)	-0.0216 (0.0238)	0.0628 (0.0441)
Distance to Nearest Participating Charger (N = 1,401,156)			
<500m	0.0111 (0.0123)	0.0486*** (0.0152)	0.1300*** (0.0213)
<1km	0.0123 (0.0087)	0.0375*** (0.0104)	0.0970*** (0.0139)
<2.5km	0.0059 (0.0049)	0.0162*** (0.0057)	0.0867*** (0.0085)
<5km	-0.0090* (0.0053)	0.0143** (0.0068)	0.0578*** (0.0095)
<10km	-0.0208*** (0.0058)	-0.0128** (0.0063)	0.0243*** (0.0091)
10km+	-0.0126 (0.0116)	-0.0070 (0.0106)	0.0271* (0.0161)
Rural Location (N = 1,427,940)			
Urban	0.0021 (0.0037)	0.0226*** (0.0043)	0.0795*** (0.0058)
Rural	-0.0153*** (0.0057)	-0.0157*** (0.0058)	0.0343*** (0.0082)
Gross Disposable Income (£) (N = 1,277,280)			
< £40k	0.0076 (0.0069)	0.0191** (0.0080)	0.1102*** (0.0127)
< £46k	-0.0170*** (0.0057)	0.0087 (0.0070)	0.0704*** (0.0107)
< £50k	-0.0122** (0.0059)	0.0123 (0.0080)	0.0630*** (0.0111)
< £57k	0.0030 (0.0067)	0.0147** (0.0075)	0.0425*** (0.0100)
£57k+	0.0063 (0.0071)	0.0180** (0.0078)	0.0628*** (0.0105)
Battery Capacity (kWh) (N = 1,605,636)			
< 51kWh	-0.0058 (0.0046)	0.0029 (0.0051)	0.0421*** (0.0070)

Table A6: Impact of the experimental stimuli on kWh charged, by subgroup (*continued*)

	Green Message	15% Discount	40% Discount
< 62kWh	-0.0032 (0.0053)	0.0180*** (0.0065)	0.0583*** (0.0083)
< 72kWh	0.0101* (0.0059)	0.0243*** (0.0065)	0.0758*** (0.0093)
< 77kWh	0.0001 (0.0053)	0.0180*** (0.0065)	0.0765*** (0.0093)
77kWh+	-0.0015 (0.0068)	0.0313*** (0.0096)	0.1018*** (0.0139)
EV Price New (£) (N = 1,498,788)			
< £30k	-0.0043 (0.0051)	0.0021 (0.0054)	0.0502*** (0.0081)
< £36k	0.0076 (0.0058)	0.0210*** (0.0066)	0.0690*** (0.0084)
< £43k	-0.0027 (0.0054)	0.0192*** (0.0070)	0.0666*** (0.0093)
< £51k	-0.0049 (0.0054)	0.0238*** (0.0073)	0.0845*** (0.0111)
£51k+	0.0032 (0.0065)	0.0188** (0.0080)	0.0720*** (0.0110)
EV Price Used (£) (N = 1,498,788)			
< £24k	-0.0056 (0.0049)	0.0017 (0.0053)	0.0466*** (0.0078)
< £29k	0.0102* (0.0061)	0.0225*** (0.0068)	0.0741*** (0.0089)
< £35k	-0.0029 (0.0054)	0.0193*** (0.0071)	0.0670*** (0.0094)
< £40k	-0.0049 (0.0054)	0.0238*** (0.0073)	0.0845*** (0.0111)
£40k+	0.0032 (0.0065)	0.0188** (0.0080)	0.0720*** (0.0110)
Time of Day (N = 1,974,798)			
Morning	-0.0011 (0.0069)	0.0237*** (0.0074)	0.0844*** (0.0087)
Afternoon	-0.0002 (0.0063)	0.0158** (0.0069)	0.0698*** (0.0086)
Evening	0.0030 (0.0038)	0.0154*** (0.0045)	0.0568*** (0.0056)
Night	-0.0007 (0.0027)	-0.0010 (0.0029)	0.0112*** (0.0038)
Weekend (N = 1,974,798)			
Weekday	0.0021 (0.0035)	0.0140*** (0.0039)	0.0510*** (0.0048)
Weekend	-0.0011 (0.0041)	0.0160*** (0.0045)	0.0687*** (0.0056)

Notes: Each row block reports coefficients from a separate regression interacting treatment with the row's moderator, with date fixed effects and standard errors clustered by account (in parentheses). Significance: * p<0.10, ** p<0.05, *** p<0.01.

Table A7: Impact of the experimental stimuli on charging status (0-1), by subgroup

	Green Message	15% Discount	40% Discount
User Profile (N = 981,846)			
Long-distance users	-0.0010*** (0.0002)	-0.0006*** (0.0002)	0.0005** (0.0002)
Local fast-charging users	0.0019*** (0.0003)	0.0032*** (0.0003)	0.0076*** (0.0005)
Local slow-charging users	0.0043*** (0.0010)	0.0037*** (0.0010)	0.0057*** (0.0011)
Active User (N = 1,974,798)			
Inactive	-0.0015*** (0.0001)	-0.0013*** (0.0001)	-0.0008*** (0.0001)
Active	0.0017*** (0.0001)	0.0023*** (0.0002)	0.0046*** (0.0002)
Distance to Nearest Charger (N = 1,430,352)			
<500m	0.0014*** (0.0002)	0.0022*** (0.0003)	0.0038*** (0.0003)
<1km	-0.0001 (0.0002)	0.0004** (0.0002)	0.0024*** (0.0003)
<2.5km	-0.0004** (0.0002)	-0.0000 (0.0002)	0.0016*** (0.0002)
<5km	-0.0011*** (0.0002)	-0.0006*** (0.0002)	0.0009*** (0.0003)

Table A7: Impact of the experimental stimuli on charging status (0-1), by subgroup
(continued)

	Green Message	15% Discount	40% Discount
<10km	-0.0011*** (0.0002)	-0.0000 (0.0003)	0.0005 (0.0004)
10km+	0.0006 (0.0012)	-0.0009 (0.0007)	0.0020 (0.0014)
Distance to Nearest Participating Charger (N = 1,401,156)			
<500m	0.0004 (0.0004)	0.0019*** (0.0005)	0.0047*** (0.0007)
<1km	0.0002 (0.0003)	0.0010*** (0.0003)	0.0032*** (0.0004)
<2.5km	0.0004** (0.0002)	0.0006*** (0.0002)	0.0027*** (0.0003)
<5km	-0.0003 (0.0002)	0.0003 (0.0002)	0.0015*** (0.0003)
<10km	-0.0007*** (0.0002)	-0.0004** (0.0002)	0.0005** (0.0003)
10km+	-0.0006* (0.0004)	0.0002 (0.0004)	0.0008 (0.0005)
Rural Location (N = 1,427,940)			
Urban	0.0001 (0.0001)	0.0007*** (0.0001)	0.0024*** (0.0002)
Rural	-0.0007*** (0.0002)	-0.0005*** (0.0002)	0.0010*** (0.0003)
Gross Disposable Income (£) (N = 1,277,280)			
< £40k	0.0003 (0.0002)	0.0007*** (0.0003)	0.0035*** (0.0004)
< £46k	-0.0006*** (0.0002)	0.0003 (0.0002)	0.0022*** (0.0003)
< £50k	-0.0003* (0.0002)	0.0003 (0.0002)	0.0020*** (0.0003)
< £57k	0.0002 (0.0002)	0.0004 (0.0002)	0.0013*** (0.0003)
£57k+	0.0002 (0.0002)	0.0007*** (0.0002)	0.0018*** (0.0003)
Battery Capacity (kWh) (N = 1,605,636)			
< 51kWh	0.0005** (0.0002)	0.0009*** (0.0002)	0.0026*** (0.0003)
< 62kWh	0.0000 (0.0002)	0.0007*** (0.0002)	0.0021*** (0.0003)
< 72kWh	0.0001 (0.0002)	0.0006*** (0.0002)	0.0021*** (0.0003)
< 77kWh	-0.0003 (0.0002)	0.0001 (0.0002)	0.0017*** (0.0002)
77kWh+	-0.0006*** (0.0002)	0.0003 (0.0002)	0.0017*** (0.0003)
EV Price New (£) (N = 1,498,788)			
< £30k	0.0004* (0.0002)	0.0007*** (0.0002)	0.0024*** (0.0003)
< £36k	0.0004** (0.0002)	0.0010*** (0.0002)	0.0029*** (0.0003)
< £43k	-0.0002 (0.0002)	0.0004* (0.0002)	0.0018*** (0.0003)
< £51k	-0.0004** (0.0002)	0.0003* (0.0002)	0.0019*** (0.0003)
£51k+	-0.0002 (0.0002)	0.0000 (0.0002)	0.0011*** (0.0003)
EV Price Used (£) (N = 1,498,788)			
< £24k	0.0003* (0.0002)	0.0007*** (0.0002)	0.0023*** (0.0003)
< £29k	0.0004** (0.0002)	0.0010*** (0.0002)	0.0030*** (0.0003)
< £35k	-0.0002 (0.0002)	0.0004* (0.0002)	0.0018*** (0.0003)
< £40k	-0.0004** (0.0002)	0.0003* (0.0002)	0.0019*** (0.0003)
£40k+	-0.0002 (0.0002)	0.0000 (0.0002)	0.0011*** (0.0003)
Time of Day (N = 1,974,798)			
Morning	-0.0000 (0.0002)	0.0008*** (0.0002)	0.0026*** (0.0003)
Afternoon	-0.0000 (0.0002)	0.0005** (0.0002)	0.0022*** (0.0003)
Evening	0.0001 (0.0001)	0.0003*** (0.0001)	0.0016*** (0.0002)
Night	-0.0000 (0.0001)	-0.0001 (0.0001)	0.0002** (0.0001)
Weekend (N = 1,974,798)			
Weekday	0.0000 (0.0001)	0.0004*** (0.0001)	0.0015*** (0.0001)
Weekend	0.0000 (0.0001)	0.0005*** (0.0001)	0.0021*** (0.0002)

Notes: Each row block reports coefficients from a separate regression interacting treatment with the row's moderator, with date fixed effects and standard errors clustered by account (in parentheses). Significance: * p<0.10, ** p<0.05, *** p<0.01.

A2.5 Consumption during events by charger speed

Table A8: Impact of the experimental stimuli on kWh charged by EVSE speed

Dependent Variables: Model:	Slow (kWh) (1)	Fast (kWh) (2)	Rapid (kWh) (3)	Ultra (kWh) (4)
<i>Variables</i>				
Green Message	0.0004 (0.0010)	0.0023** (0.0009)	0.0004 (0.0013)	-0.0017 (0.0019)
15% Discount	0.0011 (0.0012)	0.0045*** (0.0011)	0.0056*** (0.0015)	0.0043** (0.0021)
40% Discount	0.0008 (0.0011)	0.0032*** (0.0010)	0.0256*** (0.0022)	0.0309*** (0.0029)
kWh - September	3.72×10^{-5} (4.52×10^{-5})	0.0002*** (6.35×10^{-5})	0.0002*** (5.6×10^{-5})	0.0002*** (5.39×10^{-5})
kWh - October	4.21×10^{-6} (5.04×10^{-6})	3.25×10^{-6} (3.24×10^{-6})	9.83×10^{-6} (1.03×10^{-5})	-7.01×10^{-7} (7.45×10^{-7})
kWh - November	0.0005*** (7.73×10^{-5})	0.0003*** (4.1×10^{-5})	0.0004*** (6.24×10^{-5})	0.0006*** (5.88×10^{-5})
Active User	-0.0020 (0.0017)	-0.0012 (0.0014)	0.0150*** (0.0016)	0.0226*** (0.0020)
<i>Fixed-effects</i>				
Date	Yes	Yes	Yes	Yes
<i>Avg. Outcome</i>				
Control	0.0073	0.0054	0.0134	0.0257
<i>Fit statistics</i>				
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	0.00194	0.00198	0.00205

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

Notes: This table show consumption in kWh by speed categories as follows: *Slow* ($\leq 7\text{kW}$), *Fast* ($< 50\text{kW}$), *Rapid* ($< 150\text{kW}$), and *Ultra* ($> 150\text{kW}$).

A2.6 Spatial displacement

Table A9: Impact of the experimental stimuli on kWh charged, by participating CPOs

Dependent Variable:	Consumption (kWh)	
Model:	Participating CPOs (1)	Non-Participating CPOs (2)
<i>Variables</i>		
Green Message	-0.0012 (0.0017)	0.0027 (0.0021)
15% Discount	0.0124*** (0.0021)	0.0031 (0.0022)
40% Discount	0.0608*** (0.0034)	-0.0004 (0.0021)
kWh - September	0.0002*** (6.19×10^{-5})	0.0003*** (9×10^{-5})
kWh - October	1.26×10^{-5} (1.35×10^{-5})	3.94×10^{-6} (5.24×10^{-6})
kWh - November	0.0007*** (8.31×10^{-5})	0.0011*** (8.37×10^{-5})
Active User	0.0283*** (0.0025)	0.0061*** (0.0022)
<i>Fixed-effects</i>		
Date	Yes	Yes
<i>Avg. Outcome</i>		
Control	0.0215	0.0302
<i>Fit statistics</i>		
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*

Notes: The estimates of this table refer to the charging spatial displacement checks indicated in our pre-registration plan, exploratory analysis 3.

A2.7 Investigating habit formation: new location & return visits

Table A10 reports regression results estimating the likelihood that a charging session occurs at a new location, using treatment group assignment as the explanatory variable (using a simple linear probability model). The analysis mirrors the main regressions on overall charging activity: we found 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 were consistent when estimated separately by trial block, suggesting no evidence of carry-over effects across blocks. The results indicate that financial incentives not only increased charging activity,

but also encouraged users to explore new locations, possibly reflecting willingness to travel or trying new stations. To be noted, 59% of first visits to a location were also the first time using a charger of this CPO.

Table A10: Impact on First Location Visit During the Trial

Dependent Variable: block Model:	Full sample (1)	First Visit		
		Block 1 (2)	Block 2 (3)	Block 3 (4)
<i>Variables</i>				
Green Message	-1.51×10^{-5} (5.9×10^{-5})	4.21×10^{-5} (8.98×10^{-5})	-0.0001 (0.0001)	4.19×10^{-6} (0.0001)
15% Discount	0.0002** (6.22×10^{-5})	0.0002** (9.51×10^{-5})	7.77×10^{-5} (0.0001)	0.0001 (0.0001)
40% Discount	0.0008*** (7.3×10^{-5})	0.0009*** (0.0001)	0.0007*** (0.0001)	0.0009*** (0.0002)
kWh - September	1.43×10^{-6} (9.26×10^{-7})	2.14×10^{-6} (1.44×10^{-6})	-6.72×10^{-7} (1.47×10^{-6})	3.18×10^{-6} (1.89×10^{-6})
kWh - October	1.36×10^{-8} (4.2×10^{-8})	-1.99×10^{-8} * (1.08×10^{-8})	-3.3×10^{-8} * (1.85×10^{-8})	1.51×10^{-7} (1.97×10^{-7})
kWh - November	7.62×10^{-6} *** (9.83×10^{-7})	5.86×10^{-6} *** (1.36×10^{-6})	1.03×10^{-5} *** (1.9×10^{-6})	7.15×10^{-6} *** (2.12×10^{-6})
Active User	0.0010*** (5.16×10^{-5})	0.0011*** (7.81×10^{-5})	0.0010*** (8.74×10^{-5})	0.0008*** (0.0001)
<i>Fixed-effects</i>				
Date	Yes	Yes	Yes	Yes
<i>Avg. Outcome</i>				
Control	0.0009	0.0009	0.0009	0.0008
<i>Fit statistics</i>				
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 fostered sustained changes in user behavior, we analyzed the likelihood of returning to the same location within 30 days following a customer’s first visit. The unit of observation was the first visit by an account to a given location, and the analysis excluded 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 were 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 were followed by a return within 30 days, compared to 22% when the initial visit occurred outside the trial. This difference is reflected in the regression results presented in Table A11, where the coefficient on “First Visit Dur-

ing 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 have attracted first-time visits, they did not appear to translate into habitual usage.

Table A11: Impact on Return Visits Within 30 Days

Dependent Variable: Model:	Returned Within 30 Days (1)
<i>Variables</i>	
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)
<i>Fixed-effects</i>	
Date	Yes
<i>Fit statistics</i>	
Observations	417,114
Number of Accounts	67,340
R ²	0.02402
<i>Clustered (Account) standard-errors in parentheses</i>	
<i>Signif. Codes: ***: 0.01, **: 0.05, *: 0.1</i>	

A2.8 Temporal displacement

Table A12: kWh charged between notification and event start

Dependent Variable: Model:	Consumption (kWh) (1)
<i>Variables</i>	
Green Message	0.0103 (0.0079)
15% Discount	0.0157* (0.0081)
40% Discount	0.0087 (0.0085)
kWh - September	0.0043*** (0.0006)
kWh - October	5.33×10^{-5} (3.9×10^{-5})
kWh - November	0.0135*** (0.0005)
Active User	0.1062 (0.1016)
<i>Fixed-effects</i>	
Date	Yes
<i>Avg. Outcome</i>	
Control	0.3670
<i>Fit statistics</i>	
Observations	1,974,798
Number of Accounts	109,711
R ²	0.05833
<i>Clustered (Account) standard-errors in parentheses</i>	
<i>Signif. Codes: ***: 0.01, **: 0.05, *: 0.1</i>	

Notes: 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.

A2.9 Home electricity consumption

Table A13: Impact on Home Consumption (kWh)

Dependent Variable: Model:	Consumption (kWh) (1)
<i>Variables</i>	
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	7.17×10^{-5} (4.73×10^{-5})
kWh - November	0.0006 (0.0027)
Active User	1.057 (2.832)
<i>Fixed-effects</i>	
Date	Yes
<i>Avg. Outcome</i>	
Control	13.9844
<i>Fit statistics</i>	
Observations	29,141,254
Number of Accounts	17,188
R ²	0.00332
<i>Clustered (Account) standard-errors in parentheses</i>	
<i>Signif. Codes: ***: 0.01, **: 0.05, *: 0.1</i>	

Notes: The estimates in this table refer to robustness checks outlined in our pre-registered analysis plan.

A2.10 Electricity consumption during wash-out periods

Table A14: 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
<i>Variables</i>				
Green Message	0.0174 (0.0120)	0.0004 (0.0003)	0.0161 (0.0099)	0.0005 (0.0003)
15% Discount	0.0078 (0.0119)	0.0003 (0.0003)	0.0067 (0.0100)	0.0002 (0.0003)
40% Discount	0.0147 (0.0130)	0.0003 (0.0004)	0.0099 (0.0109)	0.0003 (0.0004)
kWh - September	0.0050*** (0.0007)	0.0001*** (1.96×10^{-5})	0.0008*** (0.0002)	
kWh - October	5.65×10^{-5} (3.72×10^{-5})	1.58×10^{-6} (1.09×10^{-6})	8.75×10^{-6} *** (3.22×10^{-6})	
kWh - November	0.0164*** (0.0006)	0.0005*** (1.53×10^{-5})	0.0028*** (0.0001)	
Active User	0.0863*** (0.0162)	0.0071*** (0.0004)	0.5423*** (0.0115)	
<i>Fixed-effects</i>				
Date	Yes	Yes	Yes	
<i>Avg. Outcome</i>				
Control	0.4579	0.0158	0.0158	
<i>Fit statistics</i>				
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

Notes: 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.

A2.11 Descriptive statistics

Table A15: 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.

Transaction-level data from the field experiment reveal how charging demand varied across treatment groups during dynamic pricing events. Table A16 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.

Table A16: 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 transaction-level records in which users chose to charge during dynamic pricing events. Multiple transactions per account–event may occur in this table; in the main regression dataset, transactions are aggregated to the account–event level. Comparisons therefore describe behavioral patterns among charging transactions 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.

A2.12 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 A22 and Figure A23). We then estimated the effect of each treatment on unsubscription using linear and probit regressions with date fixed effects and user-level clustering. Table A18 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.

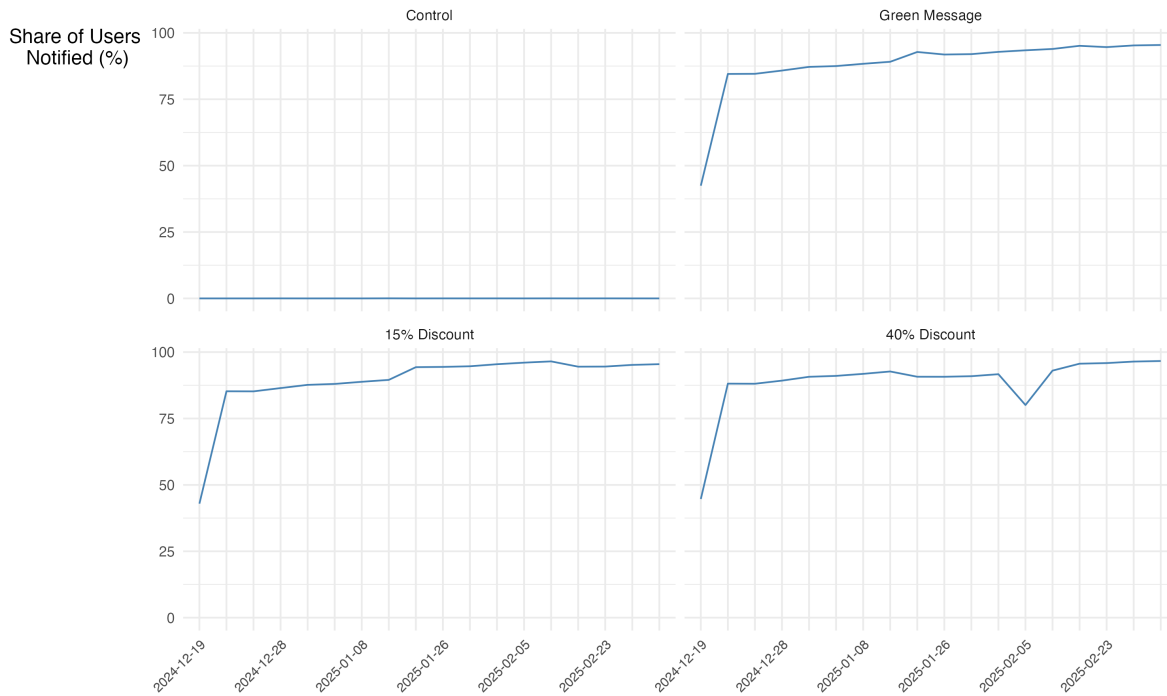
Figure A22 shows the share of users who were successfully notified on each event date, by treatment group. During the first notification event, a technical error prevented approximately half of the users in the treated groups from being contacted. We repeated our main analysis in Table A17, where we 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 A23 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.

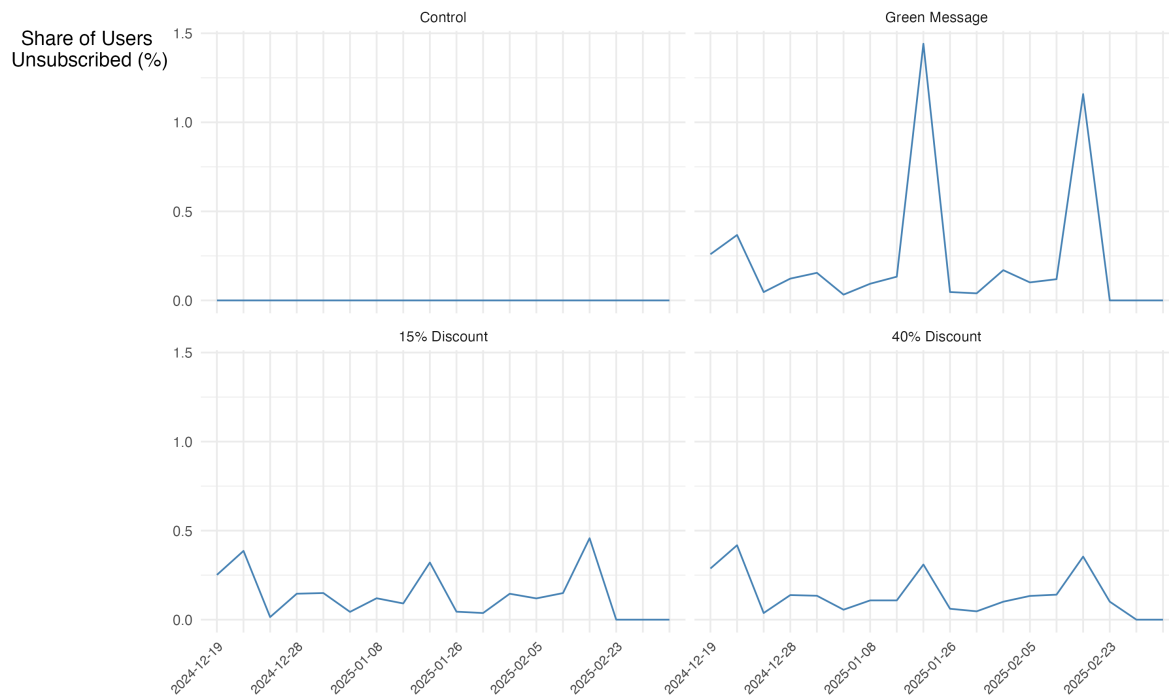
Table A18 presents estimates of the impact of different notification treatments on users' likelihood of unsubscribing. Column (1) reports results from a linear probability model with date fixed effects and standard errors clustered at the user level. Columns (2) and (3) report results from a probit model, 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 0.14 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 A22: 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 A17 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 A23: 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 A17: Robustness check: Impact of the experimental stimuli on kWh charged omitting Event 1 participants not in the notification list

Dependent Variable:	Consumption (kWh)	
Sample Model:	Full sample (1)	Excluding non contacted customers (2)
<i>Variables</i>		
Green Message	0.0015 (0.0027)	0.0012 (0.0027)
15% Discount	0.0154*** (0.0030)	0.0155*** (0.0031)
40% Discount	0.0604*** (0.0039)	0.0619*** (0.0040)
kWh - September	0.0006*** (0.0001)	0.0006*** (0.0001)
kWh - October	1.66×10^{-5} (1.43×10^{-5})	1.66×10^{-5} (1.43×10^{-5})
kWh - November	0.0018*** (0.0001)	0.0018*** (0.0001)
Active User	0.0344*** (0.0032)	0.0355*** (0.0033)
<i>Fixed-effects</i>		
Date	Yes	Yes
<i>Avg. Outcome</i>		
Control	0.0517	0.0517
<i>Fit statistics</i>		
Observations	1,974,798	1,928,319
Number of Accounts	109,711	109,711
R ²	0.00716	0.00717
<i>Clustered (Account) standard-errors in parentheses</i>		
<i>Signif. Codes: ***: 0.01, **: 0.05, *: 0.1</i>		

Notes: 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 A18: Unsubscription rate by treatment

Dependent Variable: Model:	(1) OLS	Unsubscribed (2) Probit	(3) Marginal effects
<i>Variables</i>			
Green Message	0.0024*** (6.89×10^{-5})	3.425*** (0.0094)	0.0024*** (6.9×10^{-5})
15% Discount	0.0014*** (5.28×10^{-5})	3.253*** (0.0118)	0.0014*** (6.9×10^{-5})
40% Discount	0.0014*** (5.34×10^{-5})	3.260*** (0.0118)	0.0014*** (6.9×10^{-5})
kWh - September	-3.7×10^{-7} (5.52×10^{-7})	-5.48×10^{-5} (0.0002)	
kWh - October	-1.25×10^{-8} *** (3.87×10^{-9})	-0.0001 (0.0002)	
kWh - November	-1.57×10^{-6} *** (4.48×10^{-7})	-0.0005** (0.0002)	
Active User	-0.0029*** (0.0008)	-0.4015*** (0.0714)	
Constant		16.26*** (5.053)	
Date		-0.0011*** (0.0003)	
<i>Fixed-effects</i>			
Date	Yes		
<i>Fit statistics</i>			
Observations	1,974,798	1,974,798	
Number of Accounts	109,711	109,711	
R ²	0.00234		
BIC	-7,537,653.7	37,471.5	

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

A2.13 Main analysis excluding customers pre-specified in the PAP

Table A19: 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
<i>Variables</i>				
Green Message	0.0013 (0.0027)	4.27×10^{-5} (8.9×10^{-5})	0.0064 (0.0154)	3.65×10^{-5} (8.8×10^{-5})
15% Discount	0.0155*** (0.0030)	0.0004*** (9.67×10^{-5})	0.0695*** (0.0152)	0.0004*** (8.8×10^{-5})
40% Discount	0.0602*** (0.0039)	0.0018*** (0.0001)	0.2343*** (0.0146)	0.0018*** (0.0001)
kWh - September	0.0006*** (0.0001)	1.63×10^{-5} *** (3.12×10^{-6})	0.0004*** (8.09×10^{-5})	
kWh - October	1.65×10^{-5} (1.43×10^{-5})	4.95×10^{-7} (4.36×10^{-7})	9.25×10^{-6} *** (3.24×10^{-6})	
kWh - November	0.0018*** (0.0001)	5.89×10^{-5} *** (3.09×10^{-6})	0.0016*** (6.11×10^{-5})	
Active User	0.0351*** (0.0032)	0.0017*** (9.31×10^{-5})	0.4933*** (0.0143)	
<i>Fixed-effects</i>				
Date	Yes	Yes	Yes	
<i>Avg. Outcome</i>				
Control	0.0512	0.0020	0.0020	
<i>Fit statistics</i>				
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

Notes: Estimates in this table follow the pre-registered analysis plan and exclude customers who joined after the start of the trial, fleet users, and super users.

A2.14 Main analysis using individual fixed effects

Table A20: Impact of the experimental stimuli on kWh charged

Dependent Variables: Model:	Consumption (kWh) (1)	Has Charged (binary) (2)
<i>Variables</i>		
Green Message	-0.0011 (0.0031)	-2.02×10^{-5} (0.0001)
15% Discount	0.0149*** (0.0033)	0.0004*** (0.0001)
40% Discount	0.0592*** (0.0041)	0.0018*** (0.0001)
<i>Fixed-effects</i>		
Date	Yes	Yes
Account	Yes	Yes
<i>Avg. Outcome</i>		
Control	0.0517	0.0020
<i>Fit statistics</i>		
Observations	1,974,798	1,974,798
Number of Accounts	109,711	109,711
R ²	0.07822	0.07816
Pseudo R ²	0.02141	-0.02598
F-test	465.49	465.11

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

A3 Additional welfare calculations and grid impacts

This appendix provides the full welfare decomposition underlying Figure 11, together with extensions to marginal-cost pricing and an illustrative calculation of grid-balancing value.

In our context, consumer surplus is not always simply the area under the demand curve. First, we considered app-switching customers – where in Section 4.3.1 we estimated that almost exactly 50% of the increase in demand for charging through the partner platform came from customers who would have charged anyway but switched from another payment platform to the partner app to do so. These customers gained from lower prices. We modeled their valuation as 100% of the price decrease, assuming they were previously paying £0.70/kWh but were instead able to pay £0.42/kWh. For this reason, their consumer surplus forms a rectangle rather than a triangle.

A similar case concerns demand substitution – such as temporally, i.e., from public chargers at different times of the day or week, or from other chargers entirely (such as a customer’s home charger). Where the new demand comes from such substitution, rather than pure demand creation, the consumer surplus is again a rectangle rather than a triangle, by similar logic concerning a “pound for pound” valuation. In both cases, this implies costless switching, i.e., no disutility from substitution.

We therefore distinguish three groups. First, purely inframarginal customers – those who would have charged through the partner app during the event and benefit from lower prices – generate consumer surplus of Area F (£7,147). Second, app-switchers generate consumer surplus of Area G (£4,175), assuming no disutility from switching. Finally, genuinely additional demand generates consumer surplus of Area H (£2,087) if it reflects demand creation, or H + I if it reflects substitution.

At baseline prices, we calculated that for every £1 spent on EV charging there is £0.25 of consumer surplus, obtained by dividing Area A (£4,499) by control-group expenditure (£17,867).

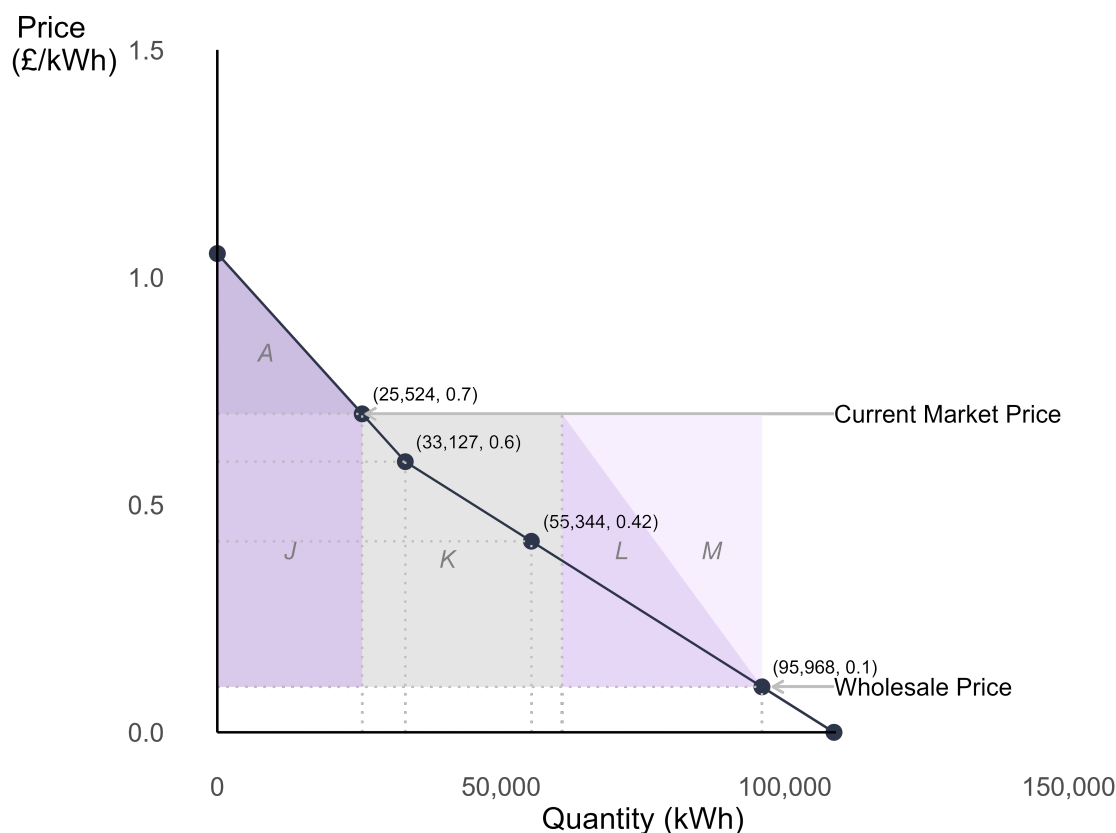
For the 15% price reduction, total consumer surplus is £7,777 (Area A + B + C + D), assuming demand creation for non-app-switchers. This corresponds to £0.39 of surplus per £1 spent. Additional consumer surplus is £3,279 (Area B + C + D), or £0.17 per £1 spent.

For the 40% price reduction, total consumer surplus is £17,908 (Area A + F + G + H),

again assuming demand creation. This corresponds to £0.77 of surplus per £1 spent. Additional consumer surplus is £13,409 (Area F + G + H), or £0.58 per £1 spent.

If prices were reduced further to reflect marginal cost of supply, assumed to be approximately £0.10/kWh, consumption without substitution would expand to 95,968 kWh and consumer spending would fall to £9,597. Total consumer surplus would rise to £51,513 (Area A + J + K + L), or more than £5.00 per pound spent. The increase in surplus relative to the original price would be £47,014.

Figure A24: Consumer surplus from price being subsidized to the wholesale price



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, with the kink at the 15% discount level, and we have extrapolated the rest of the curve linearly. The graph also shows the implied consumer surplus if charging prices were equal to wholesale prices of approximately 10p/kWh, in which case we estimated demand would increase to 95,968 kWh. Area A shows the consumer surplus (£4,499) at the original non-discounted price of £0.70/kWh, where demand is 25,524 kWh. Area J shows the change in consumer surplus for inframarginal customers who were consuming before the subsidy but now pay less. Assuming half of the extra demand from 25,524 kWh to 95,968 kWh reflects app substitution, the consumer surplus for these customers is valued pound for pound, as these customers were previously paying £0.70/kWh but are instead able to pay £0.10/kWh; this is shown by Area K. Area L is the consumer surplus for the remaining half of the increase in demand if it represents demand creation. Area M shows the additional consumer surplus if this increase instead reflects demand substitution rather than creation.

We turn next to our illustrative marginal value of public funds (MVPF) of demand turn-up to solve a distribution issue. As noted in Section 6, 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). In the long run, these turn-up programs could enable DNOs to accommodate greater variable electricity supply in their network at a lower cost than network upgrades (on the margin).

Using this value of upward demand response, we can estimate the welfare effects of a price reduction to solve a hypothetical DNO constraint. We found that although the cost per kWh of turn-up was higher than alternatives available to UKPN, examining the program through the framework of marginal value of public funds (MVPF) suggested it could be welfare-enhancing. Specifically, we assumed that a DNO such as UKPN might subsidize public EV charging to deliver turn-up in areas where it required increased demand and, for the purpose of this exercise, treated UKPN as part of the government.⁴⁸

To make the MVPF calculation transparent, we normalize to £1 of mechanical government expenditure on the subsidy. Using our estimated demand creation elasticity of 1.45 and an original electricity price of £0.70/kWh, £1 of mechanical subsidy expenditure implies an increase in demand of $1.45/0.70 = 2.07$ kWh. This figure should therefore be interpreted as kWh induced per £1 of government spending, rather than as the effect of a £1/kWh subsidy rate.

Using these numbers, we calculated the hypothetical MVPF. In the numerator, the willingness to pay for the subsidy for the inframarginal consumers would be £1. Assuming demand creation, to be conservative, we model that the subsidy would cause an increase in demand of electricity, which has a negative carbon externality. We use a social cost of carbon of £331 per tonne of CO_2e ,⁴⁹ multiplied by the carbon intensity of CO_2 per

⁴⁸MVPF for a change in τ :

$$MVPF = \frac{xd\tau + Vdx}{xd\tau + \tau dx} \quad (2)$$

$$= \frac{1 + \frac{V}{p}(-\epsilon)}{1 + \frac{\tau}{p}(-\epsilon)} \quad (3)$$

where $\epsilon = \frac{dx}{dp} \frac{p}{x}$ is the price elasticity of demand, x is original demand, p is the original price, and V is the marginal social value (positive or negative) of the induced electricity consumption, e.g., the carbon externality per kWh (Hahn et al., 2024).

⁴⁹£260 per tonne of CO_2e in 2020 (Department for Energy Security and Net Zero, 2023)

kWh, which was approximately 0.000125 tonnes of CO_2 per kWh during events. We have to remove the proportion of the SCC which affects the UK government (1% of £331).⁵⁰ The marginal demand increase from the subsidy is 2.07 kWh and therefore the numerator of the MVPF = $1 - 2.07 (0.0409) = 0.915$. The denominator of the equation would be the government cost. The mechanical cost is £1, and the additional demand of 2.07 kWh will lead to the government spending £0.28/kWh on subsidies. However, the government saves £0.45/kWh due to UKPN avoiding spending on alternative turn-up programs (i.e., the £450/MWh discussed immediately above). There is also a CO_2 e cost for the UK government which is 1% of £331 per tonne of CO_2 released (the UK governments budget as a % of global GDP). We have ignored producer surplus and costs given our uncertainty about their cost structure and mark-up, as noted above. Therefore the denominator of the MVPF = $1 + 2.07 (0.28 - 0.45 + 0.0004) = 0.649$. This gives us an MVPF of $0.915/0.649 = 1.409$ for a Government subsidy to reduce EV public charging prices to solve distribution constraints.⁵¹

⁵⁰See <https://ifs.org.uk/taxlab/taxlab-key-questions/how-do-uk-tax-revenues-compare-internationally>

⁵¹There are uncertainties regarding how our intervention would scale "down" to specific local areas facing export constraints. If only half of the additional charging actually occurred in the desired locations and times, the MVPF would drop to approximately 0.82.

A4 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 A19 (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 the partner app), 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 un-

observed 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 [A20](#); results are very similar to our main results in Table [A3](#).