Thin Markets, Thick Subsidies: Local Competition and the Incidence of Heat-Pump Grants

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Abstract

We estimate the impact of demand subsidies on the adoption of clean technologies and how this is mediated by competition in the installers market. The UK Boiler Upgrade Scheme raised the air-source heat-pump grant from £5,000 to £7,500 on 23 October 2023; Scotland did not change contemporaneously. Using 260,000 installations obtained from the Microgeneration Certification Scheme records, a border triple-difference implies that weekly air-source installations in English border areas rose by about 150%, concentrated in the first four weeks after increase. This impact was mediated by competition. Across heat-pump markets in England and Wales, a one-standard-deviation increase in competition is associated with a 3.7% larger post-implementation increase and with larger increases in active installers.

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1 Introduction

Demand-side subsidies are widely used to change the adoption of durable goods that generate external benefits or that may be underprovided due to private frictions, including information problems, principal-agent concerns, and inattention (Allcott & Greenstone, 2012; Gerarden et al., 2017; Gillingham & Palmer, 2014). A subsidy shifts households' willingness to pay, but the realized effects depend on how suppliers respond. For installed technologies, supply is a bundle of equipment and local services: site assessment, system design, installation, and commissioning. These tasks are performed by specialized contractors whose capacity and competitive environment vary across places. A demand-side subsidy can therefore affect installed quantity unevenly across space, and the difference can arise even when the subsidy is uniform at the national level.

A large body of empirical work on household energy programs has measured consumer responses to rebates and capital subsidies in markets for appliances, weatherization, rooftop solar, and electric vehicles (Davis et al., 2014; Fowlie et al., 2018; Gillingham & Tsvetanov, 2019; Muehlegger & Rapson, 2022; Springel, 2021). These studies often estimate average take-up responses, and in some settings they distinguish between adoption and utilization, while a separate literature studies the incidence of taxes and subsidies under imperfect competition, emphasizing that pass-through and quantity responses depend on market power, bargaining, and strategic interaction (Benzarti & Carloni, 2019; Gulati et al., 2017; Pless & van Benthem, 2019; Sallee, 2011).

In installed-technology markets, transaction prices are often negotiated and rarely observed in administrative datasets. Quantity responses provide a complementary lens on incidence, and they are directly relevant when policy objectives are stated in terms of deployed capital, for example installed low-carbon heating capacity. While recent work studies the economic determinants of heat pump adoption and the distributional consequences of electrification policies (S. Anderson & Kirkpatrick, 2024; Davis, 2024, 2025), the role of competition in the installers market remains under-explored.

Local installer markets are a natural setting in which market thickness mediates incidence. Entry and continued participation require training and certification, and local demand can be sparse in the short run (Bresnahan & Reiss, 1991). A large subsidy that shifts demand at a known date can therefore translate into installations in some places and into markups, waiting times, or rationing in others. Standard models of imperfect competition imply that, holding marginal costs fixed, a demand shifter produces a larger quantity response when the local market is more competitive and supply is more elastic (S. P. Anderson et al., 2001; Weyl & Fabinger, 2013). This paper studies these comparative statics in a setting where the same subsidy change applies to many local markets, and where administrative records allow market structure to be measured from realized installations.

We use a large, discrete increase in the United Kingdom's Boiler Upgrade Scheme (BUS), the main capital subsidy for low-carbon heating in existing buildings in England and Wales (Ofgem, 2023; UK Government, 2022). The BUS provides fixed-value vouchers for qualifying heat-pump

installations. Accredited installers apply for vouchers on behalf of households and deduct the grant from the invoice at installation (Ofgem, 2023). On Monday 23 October 2023, the BUS grant for air-source heat pumps increased from £5,000 to £7,500, and the grant for ground-source heat pumps increased from £6,000 to £7,500. The grant for biomass boilers remained £5,000.

Devolution within the UK provides a contemporaneous institutional contrast. The BUS only applies to England and Wales, while Scotland operates separate clean-heating programs with distinct rules and funding (Home Energy Scotland, 2025). The October 2023 BUS grant increase does not apply in Scotland. This separation yields a treated and untreated comparison at the England–Scotland border. It also defines a narrow estimand: the border design identifies the short-run effect of the BUS increase on certified installations in areas near the border, under assumptions about counterfactual trends in the England–Scotland gap. The design does not assume that the border estimand equals the national average effect, and it does not identify long-run adoption.

We leverage administrative certification records from the Microgeneration Certification Scheme (MCS) (Microgeneration Certification Scheme, 2025). Each record corresponds to a commissioned installation and reports a commissioning date, technology type, installer identifier, and installation postcode. We aggregate these microdata to weekly installation counts and map postcodes to Lower-layer Super Output Areas (LSOAs) and to Travel-to-Work Areas (TTWAs), which proxy for local markets. We link installer identifiers to the separate MCS installer registry that records certification status over time and headquarters locations. Several objects relevant for incidence are not observed in these administrative records. We do not observe voucher application dates, voucher withdrawals and re-issuance, the grant amount actually received, installation quotes, transaction prices net of the grant, waiting times, or household covariates. The paper therefore focuses on the quantity response in completed, certified installations. Since air-source heat pumps account for 93% of heat-pump installations in the estimation sample, and since the grant increase was larger for air-source systems, we focus on air-source installations as the primary outcome.

Our first empirical design estimates a causal short-run effect of the BUS grant increase on commissioned installations using a border triple difference (border design). Border comparisons exploit policy discontinuities when nearby places share many demand fundamentals (Dube et al., 2010; Holmes, 1998). We construct a weekly panel of LSOAs in postcode districts adjacent to the England–Scotland border. The identifying variation is the change in the England–Scotland installation gap after implementation in 2023, net of the same change in the same calendar weeks in 2020–2022. The year differencing addresses seasonality and border-specific shocks that recur at the same time of year, a salient concern because the policy change occurs in late October, shortly before the heating periods starts. Operationally, we align observations by week-of-year and include LSOA fixed effects and year-by-week fixed effects. The key identification assumption is a triple-difference analogue of parallel trends: absent the BUS increase, the change in the England–Scotland gap at the implementation week in 2023 would have matched the average change in that gap at the same week in prior years, conditional on fixed effects.

Our second design quantifies how the same subsidy change maps into installations across local markets with different pre-existing installer market design. We measure competition in the installers market at the TTWA level using the inverse Herfindahl–Hirschman index (HHI) of installer market shares, constructed from pre-policy installations. The inverse HHI can be interpreted as an effective number of active installers when shares are unequal. We compute this measure using installations over 2015–2021 and standardize it to have mean zero and unit variance. We then estimate a difference-in-differences specification that interacts our measure of competition with the treated-year and post-implementation indicators, while absorbing TTWA fixed effects and year-by-week fixed effects. The coefficient identifies differential policy exposure by pre-determined market structure under a stability condition: in the absence of the BUS increase, the 2023 change in installations around the implementation week would not have been systematically related to pre-policy competition, conditional on fixed effects. This design supports interpretation as effect heterogeneity of the subsidy shock. It does not by itself claim to identify the causal effect of changing market structure.

The estimates indicate large short-run responses on the quantity margin. In the border design, weekly air-source installations on the English side of the border increase after implementation relative to the Scottish side, relative to pre-implementation weeks, and relative to the same weeks in prior years. In the baseline specification with a single post indicator, the estimated log incidence-rate ratio is 0.915, which implies about a 150 percent increase in weekly installations in the post period within the estimation window (Table 1). Splitting the post period shows that the response is concentrated in the first four weeks after implementation. An event-time specification yields pre-implementation coefficients that are jointly indistinguishable from zero, supporting the identifying assumption within the short window used for estimation. Because the outcome is commissioning, these estimates capture short-run changes in completed installations and may combine new adoption with changes in timing. We do not observe contract dates or voucher application dates that would separate these channels.

Market structure predicts the extent to which the subsidy increase translates into installations. In the TTWA market heterogeneity design, a one-standard-deviation increase in pre-policy competition is associated with a 3.7 percent larger post-implementation increase in weekly air-source installations in 2023 relative to the same weeks in 2020–2022. Interestingly, the differential response emerges after the first month, in contrast with the quantity response in the border design. A discrete version of the design yields similar conclusions: TTWAs in the top quartile of our competition index have a larger post-implementation response than TTWAs in the bottom three quartiles, with the differential concentrated after week 4. These patterns are consistent with local competition scaling the supply response to a demand subsidy, even if they remain reduced-form evidence about heterogeneity rather than a causal estimate of market structure.

To relate the quantity response to supply-side adjustment mechanisms, we use installer identifiers and registry data to measure participation. The number of active installers rises after implemen-

tation in the treated border region, and the increase is larger in higher-competition TTWAs. The registry-based count of certified installers rises by less. It should be noted that these outcomes have measurement limits. Activity-based measures are mechanically linked to installations because an installer is observed as active when it completes installations. The data also do not include waiting times, and the evidence on spatial reallocation, proxied by installation distances and off-site installation shares, can be imprecise in this short window.

Our paper makes three contributions to the literatures that treat subsidies as a tool for changing durable-goods adoption but differ in their treatment of local supply. First, we provide a causal estimate of the short-run quantity incidence of a large, fixed-value subsidy increase in an installed-technology market, using a border triple-difference design that combines a contemporaneous untreated comparison with within-year week alignment. Second we document that the same subsidy change generates systematically different quantity responses across local markets with different pre-policy installer concentration. Third, we link this heterogeneity to changes in observed installer participation.

The rest of the paper is structured as follows. Section 2 describes the institutional setting and a benchmark framework linking subsidy incidence to local competition. Section 3 describes the administrative data and market constructions. Section 4 presents the border and heterogeneity designs. Section 5 reports the estimates and the supply-side diagnostics.

2 Institutional background and economic framework

2.1 Policy and market environment

The Boiler Upgrade Scheme (BUS) is the main UK government capital subsidy for low-carbon heating in existing buildings in England and Wales. The scheme is administered by Ofgem under statutory regulations, and delivers support through fixed-value vouchers that are redeemed after installation and commissioning (Ofgem, 2023; UK Government, 2022). The program is installer-led: an accredited installer applies for a voucher on behalf of a property owner, and the voucher value is deducted from the invoice once the installation is completed and commissioned.

The empirical setting in this paper is a discrete increase in BUS grant values that took effect on Monday 23 October 2023. Prior to that date, the guidance specifies grants of £5,000 for air-source heat pumps (ASHPs) and £6,000 for ground-source heat pumps (GSHPs). From 23 October 2023 onward, the grant for both ASHPs and GSHPs was increased to £7,500, while biomass boiler grants remain £5,000.

BUS eligibility ties installations to formal certification. Installers must be certified under the Microgeneration Certification Scheme (MCS) or an equivalent scheme, and installed heatpumps must also be chosen among MCS certified products (Microgeneration Certification Scheme, 2025). The guidance lists additional constraints that affect the mapping from the policy change to realized

installations, including a commissioning-date requirement (on or after 1 April 2022), technology-specific requirements, and an overall capacity limit of 45 kW thermal (Ofgem, 2023). This last requirement circumscribes BUS eligibility to the residential market. These features make MCS administrative commissioning records a policy-relevant measure of realized adoption. However, some margins remain unobserved in the MCS dataset, including project lead times, voucher application dates, and the negotiated invoice price net of the grant. These limitations inform our empirical design in Section 4.

Devolution in the UK creates a contemporaneous institutional contrast. BUS only applies to England and Wales. Scotland operates separate programs funded by the Scottish Government, including the Home Energy Scotland Grant and Loan scheme, which provides grant support for clean heating systems such as heat pumps alongside an optional interest-free loan (Home Energy Scotland, 2025). The Scottish scheme is administered separately from the BUS and has distinct eligibility and funding rules. However, it should be noted that the overall level of funding support offered to households remains comparable. For example, the Home Energy Scotland materials describe grant funding up to £7,500 for heating systems such as heat pumps, with an optional interest-free loan that can complement the grant.

Crucially for our empirical strategy, the October 2023 BUS grant increase was not mirrored by a comparable policy change in Scotland. The first design in Section 4 treat this institutional separation as a source of untreated variation for comparisons at the England–Scotland border.

Heat-pump adoption depends on a local supply chain because installation is a bundled service. A typical installation requires an initial assessment and system design, installation of the heat pump and associated hydronic components, and commissioning that satisfies certification requirements (Ofgem, 2023). These activities are labor-intensive and geographically constrained by travel time and the need for post-installation support. In such settings, a demand-side subsidy can be absorbed through several channels. It can raise completed installations, it can raise waiting times and shift commissioning dates, and it can increase installer markups. This paper focuses on the quantity margin, using high-frequency installation counts. While we do observe an "overall cost" field in a subset of the MCS records, installers do not report whether this cost is inclusive of government subsidies, or additional retrofit works that may have been required to fit a heat pump to a legacy heating system. As a result, we do not consider this data sufficient to support a reliable analysis of price pass-through at the transaction level, and we do not estimate price incidence results.

2.2 Economic framework: quantity responses and local competition

Our empirical object is the short-run quantity response of heat-pump installations to a discrete subsidy increase, and how that response varies with pre-existing local installer market structure. The following framework is a standard incidence environment with imperfect competition and local capacity, intended to discipline interpretation of the heterogeneity estimates contribution (Pless & van Benthem, 2019; Weyl & Fabinger, 2013).

Consider a local market m for heat-pump installations. Let Q_m denote the number of installations completed in a week, and let p_m denote the (gross) price paid to installers for an installation. A per-installation grant s reduces the consumer's out-of-pocket price, so that demand depends on $p_m - s$. In reduced form,

$$Q_m = D_m(p_m - s), \quad D'_m(\cdot) < 0, \tag{1}$$

where D_m captures local housing, technology suitability, and preferences. The October 2023 BUS change is a discrete increase in s for ASHPs and GSHPs in England and Wales.

On the supply side, installers are heterogeneous and compete in local markets. A convenient static benchmark is quantity competition with differentiated service and upward-sloping marginal cost due to capacity constraints. In a Cournot model with inverse demand $p_m = P_m(Q_m; s)$ and firm quantities q_{jm}, each installer j satisfies

$$P_m(Q_m, s) + q_{jm}\frac{\partial P_m(Q_m, s)}{\partial Q_m} = mc_{jm}(q_{jm}), \tag{2}$$

where $mc_{jm}(\cdot)$ allows for increasing marginal cost from scheduling, labor constraints, or travel. Aggregating (2) highlights the role of concentration. Multiply by market share $s_{jm} \equiv q_{jm}/Q_m$ and sum over installers:

$$P_m(Q_m, s) + Q_m\frac{\partial P_m(Q_m, s)}{\partial Q_m}\underbrace{\sum_j s_{jm}^2}_{HHI_m} = \sum_j s_{jm} mc_{jm}(q_{jm}). \tag{3}$$

The Herfindahl–Hirschman Index HHI_m enters as the sufficient statistic for how strongly oligopolistic behavior raises the wedge between price and a share-weighted marginal cost in this benchmark. A common summary of "effective" competition is the inverse HHI,

$$HHI_m^{-1} \equiv \left(\sum_j s_{jm}^2\right)^{-1}, \tag{4}$$

which equals the number of symmetric firms in a market with equal shares.

The comparative static of interest is the response of equilibrium quantity to a change in the subsidy, $\partial Q_m/\partial s$. In equation (3), a higher subsidy shifts demand outward (higher willingness to pay at each Q_m), and equilibrium adjusts through both the price level and the markup term proportional to HHI_m. Holding marginal-cost curvature fixed, higher concentration increases the markup component that must be offset for a given increase in quantity, implying a smaller $\partial Q_m/\partial s$ in more concentrated markets. The mechanism is standard: when competition is more intense, marginal revenue is closer to price, the industry behaves as if it has a more elastic supply curve, and a demand shifter translates more into quantity and less into markups (S. P. Anderson et al., 2001; Weyl & Fabinger, 2013). A simple linear-demand, constant-marginal-cost Cournot example

makes the mapping transparent. If $P_m(Q_m, s) = a_m + s - bQ_m$ and marginal cost is c_m, then with N_m symmetric installers,

$$Q_m(s) = \frac{N_m}{N_m + 1} \cdot \frac{a_m + s - c_m}{b}, \quad \frac{\partial Q_m}{\partial s} = \frac{N_m}{N_m + 1} \cdot \frac{1}{b}, \tag{5}$$

so the quantity response is increasing in the number of competitors. When market shares are unequal, HHI_m^{-1} provides an empirical proxy for the effective number of competitors that governs how close the industry is to the competitive limit. Equation (5) illustrates why an inverse-HHI exposure is a natural effect modifier for a common demand shifter.

The empirical measure of local competition follows directly from equation (4). For each local labor market m, the analysis constructs installer shares s_{jm} from pre-policy installations and computes HHI_m^{-1}, then standardizes it to a competition index A_m used as an exposure variable in the interacted difference-in-differences design. This use of A_m is descriptive about effect heterogeneity: under the benchmark logic above, higher A_m should predict a larger installation response to the subsidy increase. This framework does not, by itself, justify a causal interpretation of the effect of changing concentration, because A_m can be correlated with local demand fundamentals and supply conditions. The identifying content of our empirical strategy is therefore in the policy discontinuity and the fixed-effects designs, while the role of (4) is to provide a transparent mapping between a standard concentration statistic and the expected sign and timing of quantity responses under local imperfect competition.

3 Data and construction

3.1 Data sources and units

The primary dataset is an administrative dataset of heat-pump installations certified under the Microgeneration Certification Scheme (MCS). Each record corresponds to an individual commissioned installation and includes (i) a commissioning date, (ii) a technology type (air-source heat pump versus other heat-pump technologies recorded by MCS), (iii) an installer identifier (the installation company's MCS number) and installer name, and (iv) the installation postcode. The MCS dataset does not include household or building characteristics (for example, income, tenure, dwelling type, or the pre-existing heating system), nor does it include whether the household claimed a specific subsidy, the grant amount received, quotes, waiting times, or installer markups. While commissioning dates are observed at daily frequency, we use weekly aggregations in our analysis.

We link each installation to geography using a postcode lookup from the UK Office of National Statistics (ONS) that maps postcodes to (i) Lower-layer Super Output Areas (LSOA 2011) and (ii) Travel-to-Work Areas (TTWA), as well as to country and postcode-district identifiers. LSOA are a standard geographical unit in UK spatial datasets constructed from UK Census. They are designed to be relatively homogeneous in size and socioeconomic composition, and comprise on

average around 600 households. TTWAs are a commuting-based local labor-market definition also constructed by the ONS from Census data, which makes them well suited to define installers markets spatially. The same lookup provides latitude and longitude coordinates used for distance-based constructions in the Appendix. A small minority of installations (1.4%) do not report postcodes that could be mapped to these geographies, and we therefore exclude them from the constructed panels.

A separate MCS installer registry provides the exact headquarter location of each installer (mapped to postcodes and TTWA) and dates that delineate the period in which the installer is certified to install heat pumps. These registry fields are used to construct weekly measures of installer availability ("certified installers") and to assign installations to an installer's headquarter TTWA for off-site and distance measures.

3.2 Outcome construction

Our empirical designs use count outcomes constructed by aggregating MCS microdata to area-by-week cells. Let g index an area (LSOA or TTWA) and let w be a weekly index. Our main outcome of interest is the weekly number of air-source heat-pump installations in (g, w), denoted Y_{gw}^{AHP}.

We focus on air-source heat pumps (ASHPs)as the primary outcome for two reasons. First, they comprise the bulk of observed installations in the administrative records used here: 93 percent of heat-pump installations in the estimation sample are air-source, with the remainder split across ground-source and water-source technologies. As a result, the weekly count outcome for non-air-source technologies is very sparse at local levels, reducing precision and increasing sensitivity to idiosyncratic projects. Second, the October 2023 BUS change differentially shifts incentives across technologies. Scheme guidance indicates that the grant cap increased by a larger amount for air-source heat pumps than for ground-source heat pumps, concentrating the mechanical change in price net of subsidy on the air-source margin. We still report a pooled all-heat-pump outcome, Y_{gw}^{HP}, to document that the short-run increase is not an artifact of redefining the dependent variable.

3.3 Time indexing and week alignment

Time is indexed using ISO weeks to align calendar weeks consistently across years. The ISO 8601 standard defines a week as starting on Monday, with the first week of the year being the one containing the first Thursday. This structure eliminates partial weeks at year boundaries, ensuring consistent seven-day periods that facilitate precise alignment and comparison across different years – a necessary feature to implement our empirical designs. For each installation with commissioning date d, we compute its ISO year $t(d)$ and ISO week-of-year $w_y(d) \in 1, \ldots, 52/53$, and then aggregate all installations at the area by ISO week level.

The policy change of interest is the Boiler Upgrade Scheme (BUS) grant increase that took effect on 23 October 2023, which was the Monday of ISO week 43 of 2023. The event-time index we use in all of our specifications thus aligns weeks within each year by ISO week-of-year, and the relative

timing to event rel $\equiv w_y(d) - 43$ is consistently compared to ISO week 43. This allows us to support comparisons across multiple years that net out recurring seasonality around late October.

For estimation and for constructing balanced panels, we also assign each ISO week a running index that counts weeks from a fixed origin Monday (ISO week 1 of 2009). This index is used to order observations and, in the empirical analysis, to support clustering by common calendar weeks.

3.4 Analysis samples

Two samples are used throughout. First, the border design sample is built from LSOAs located in postcode districts adjacent to the England–Scotland border. England-side districts are Cumbria (CA) and Northumberland (NE); Scotland-side districts are Dumfries and Galloway (DG) and Scottish Borders (TD). We assign each installation to a postcode district using the postcode lookup and retain LSOAs within these districts. The estimation window is symmetric around the implementation week using the event-time index rel, with a baseline window including the seven weeks before and after rel = 0 for each year. In our main specifications, the constructed LSOA-week panel includes years 2020–2023 for the main comparisons, with 2023 as the treated year and 2020–2022 as comparison years.

The short-run focus is imposed by both institutional timing and identification. The policy is implemented at ISO week 43. A symmetric event window that extends too far into the post period mechanically reaches the late-December holiday weeks. In calendar terms, week 43 plus eight is typically Christmas week, while week 43 plus nine includes New Year's Eve. The empirical design aligns weeks within a given year, so extending the post window beyond week 50 necessarily changes the composition of weeks in ways that are difficult to model with high-frequency fixed effects. We therefore adopt a baseline window of ± 7 weeks around implementation, which covers approximately two months on each side while staying within the pre-holiday portion of the calendar year. The sensitivity of estimates to shorter and wider windows is reported in the Appendix as robustness.

Second, the competition sample is a TTWA-week panel for TTWAs in England and Wales. This sample uses the same event-time construction and the same baseline ± 7-week window around rel = 0, again focusing on years 2020–2023. Scotland is excluded from this sample because the competition design interprets the policy change as applying to England and Wales; the outcome is constructed directly from installations located in those TTWAs.

3.5 Pre-policy competition in the installers market

Local competition is measured at the TTWA level using pre-policy installation shares across installers. For each TTWA m, we compute installer-level counts N_{jm} in a pre-policy period, where j indexes installers and N_{jm} is the number of heat-pump installations in TTWA m performed by installer j over the pre-period. We form installer shares $s_{jm} = N_{jm} / \sum_k N_{km}$ and compute the inverse Herfindahl index as described in equation (4).

This quantity equals the "effective number" of equal-sized installers that would generate the same HHI as the observed share distribution. The main measure used in the paper is a standardized version,

$$A_m \equiv \frac{HHI_m^{-1} - \overline{HHI^{-1}}}{\mathrm{sd}(HHI^{-1})}, \tag{6}$$

where the mean and standard deviation are computed across TTWAs in the competition sample. The baseline pre-policy period used to compute shares is 2015–2021. A robustness construction uses 2015–2019. The Appendix describes both windows and the motivation for using pre-periods that end before the policy year.

The installer registry supports two additional measures used in Appendix analyses. First, we compute the number of certified heat-pump installers in each TTWA-week by counting installers headquartered in that given TTWA who are certified to install any type of heat-pump in that given week. Second, we compute the number of installers headquartered in each TTWA as of the pre-policy period using a registry snapshot and restricting to installers classified as heat-pump certified. The registry does not report capacity, staffing, or workload, so these measures capture extensive margins (certification and presence) rather than intensity.

3.6 Descriptive timing around the BUS increase

Figure 1 plots weekly heat-pump commissioning counts in England and Wales around the implementation week. The figure uses the weekly aggregation described above, defines week 0 as the ISO week starting 23 October 2023, and shows a ±26-week window. The plot is descriptive and does not adjust for seasonality, geography, or contemporaneous shocks.

4 Empirical strategy

The October 2023 increase in the Boiler Upgrade Scheme (BUS) grant creates a discrete, nationwide demand shift for certified heat-pump installations in England and Wales. The empirical challenge is to separate the effect of this demand shift on installation quantities from seasonality, national or UK-wide shocks, and local factors that differ across places and time. We use two complementary sources of variation. In a first desgin, we use a geographically local triple-difference around the England–Scotland border, exploiting that the policy changes sharply in England but not in Scotland at the same time. In our second design, within England and Wales, we use pre-2023 installer market structure as a predetermined exposure that shifts the local capacity to translate the demand shock into realized installations.

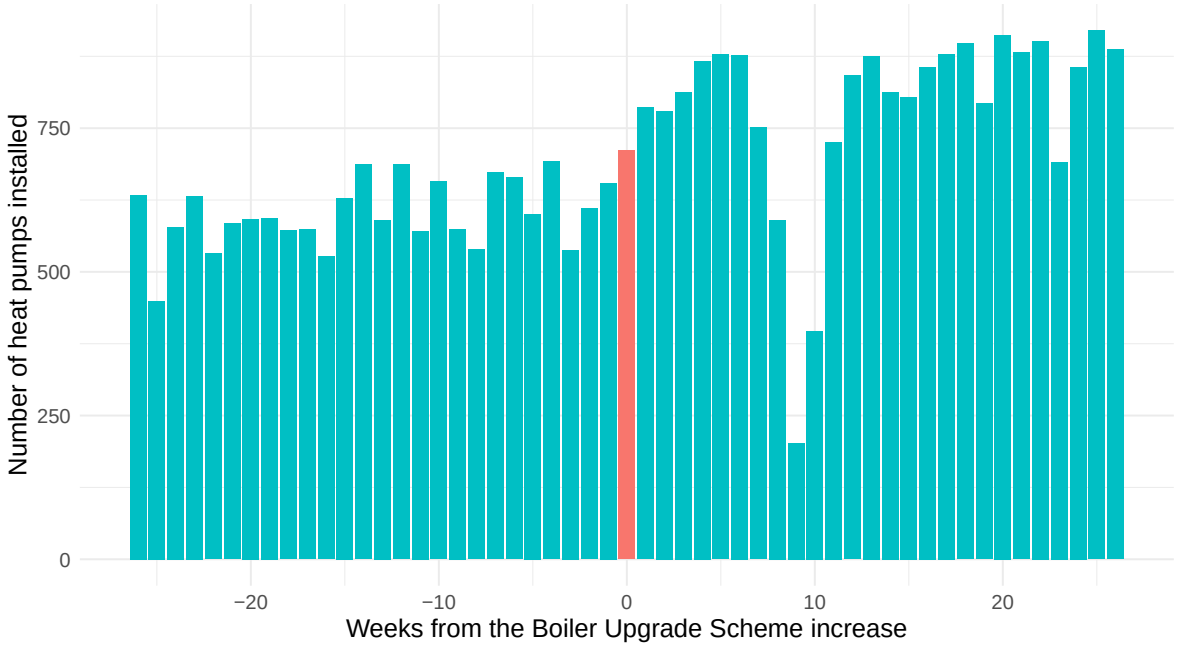


Figure 1: Weekly heat-pump installations in England and Wales around the October 2023 BUS grant increase

Notes: The figure plots the weekly number of heat-pump installations recorded in MCS commissioning data for England and Wales. The unit of time is the ISO week, aggregated by the Monday start date of the ISO week containing the commissioning date. The horizontal axis reports weeks relative to the BUS grant increase, with week 0 defined as ISO week 43 of 2023 (starting Monday, 23 October 2023); the bar for week 0 is highlighted. The sample is restricted to ±26 weeks around week 0 and includes all heat-pump technology types observed in the MCS extract. The figure is descriptive and does not incorporate the geographic comparisons, within-year week alignment across years, or fixed effects used in the empirical designs.

4.1 Event timing

We describe the construction of our time indexing scheme in section 3.3. Two features of this construction matter for identification. First, the year-by-week-of-year fixed effects defined below absorb common shocks at the weekly frequency, including seasonality and national changes in interest rates, energy prices, or awareness that vary at the same time for all locations in the estimation sample. Second, the alignment by week-of-year avoids attributing typical late-October patterns to the policy change.

Of course, this means that "event week" in placebo years is not the exact calendar date of 23 October, but the same ISO week number (43). Yet this is the relevant counterfactual if the main confound is within-year seasonality rather than a calendar-date-specific shock. The identifying variation is therefore local in time: the estimands describe short-run changes in commissioning volumes in a window around implementation, not medium-run adoption over subsequent months.

4.2 Border triple-difference around the England–Scotland boundary

Units, outcomes, and sample. The border design is estimated on a panel of Lower-layer Super Output Areas (LSOAs) indexed by i, at the weekly frequency defined above. The primary outcome is a weekly count of commissioned air-source heat-pump installations, Y_{i,t,w_y}, constructed by aggregating Microgeneration Certification Scheme (MCS) installation records to an LSOA-week cell. We treat commissioning as the economically relevant timing because subsidy eligibility requires a certified installation.

The border sample restricts geography to postcode-district prefixes CA and NE on the England side and DG and TD on the Scotland side. These areas contain the LSOAs adjacent to the border region in which cross-country differences in housing stock, climate, and installer travel distances are plausibly smaller than in a national England–Scotland comparison. The border design identifies a local effect for this region. It does not, by itself, deliver a national average effect for England and Wales, and it does not impose that external validity.

We construct a balanced LSOA-week panel for the estimation window. For each LSOA in the border geography, we include every ISO week in the window for each year in the analysis period and assign $Y_{i,t,w_y} = 0$ in LSOA-week cells with no recorded installation. This step matters because many LSOA-week cells have zero installations, and dropping zero cells would induce composition changes in the set of observed LSOA-week cells that are mechanically correlated with demand shifts.

Treatment definition. Let E_i indicate whether LSOA i is in England ($E_i = 1$) or Scotland ($E_i = 0$). The policy change occurs in England and Wales; in the border design, Scottish LSOAs serve as the contemporaneous untreated comparison. Let $\mathbb{1}t = 2023$ denote the treated year and let Post_{t,w_y} denote the post-implementation weeks in year t. We use pre-policy years $t \in 2020, 2021, 2022$ as controls for seasonality and border-region patterns around the same week-of-year.

Baseline specification. We estimate a Poisson pseudo-maximum-likelihood (PPML) model for weekly counts. The baseline conditional mean specification is

$$\mathbb{E}\left[Y_{i,t,w_y} \mid \alpha_i, \delta_{t,w_y}\right] \exp\left(\alpha_i + \delta_{t,w_y} + \theta_1 E_i \times \mathbb{1}_{t=2023} + \theta_2 E_i \times \text{Post}_{t,w_y} + \beta E_i \times \mathbb{1}_{t=2023} \times \text{Post}_{t,w_y}\right) \quad (7)$$

Here α_i are LSOA fixed effects and δ_{t,w_y} are year-by-week-of-year fixed effects. The year-by-week effects absorb any component that is common across England and Scotland in a given ISO week-of-year and year, including national shocks and seasonality. The LSOA effects absorb time-invariant cross-sectional differences within the border region, such as housing composition and baseline installer market structure. The included England-by-year and England-by-post interactions allow the England side of the border to have a different level in 2023 and a different within-year pre/post pattern that is stable across years, netting out these components from the triple interaction.

The coefficient of interest β is a triple-difference estimand: it measures whether the within-

year change from pre to post in 2023 differs between England and Scotland, net of the analogous within-year England–Scotland difference in earlier years.

Identifying assumptions. The border triple-difference requires a parallel-trends condition in the DDD sense. The identifying restriction is that, absent the BUS increase, the change in the England–Scotland gap in expected outcomes from pre to post around the event week in 2023 would have followed the same pattern as in the control years. The year-by-week fixed effects make this a conditional statement: it is a restriction on differential England–Scotland movements net of shocks common to both sides of the border in the same year-week.

The border design also requires that no other policy or measurement change occurs at the same time in Scotland that differentially affects the Scottish border region relative to the English border region. The institutional background provides the basis for treating Scotland as untreated with respect to the October 2023 BUS increase. The design does not require England and Scotland to be identical; it requires that any differences that shift the England–Scotland gap at the same aligned time in 2023 would also have shifted the gap in the same way in earlier years, after conditioning on fixed effects.

Dynamic specification for diagnostics. To assess whether the DDD parallel-trends restriction is plausible in the weeks leading up to implementation, we estimate an event-time saturated version of (7):

$$\mathbb{E}\left[Y_{i,t,w_y} \mid \alpha_i, \delta_{t,w_y}\right] = \exp\left(\alpha_i + \delta_{t,w_y} + \theta_1 E_i \times \mathbb{1}_{t=2023} + \sum_{k \in \mathcal{K} \setminus -1} \beta_k E_i \times \mathbb{1}_{t=2023} \times \mathbb{1}_{\mathrm{rel}_{t,w_y}=k}\right) \quad (8)$$

with $\mathcal{K} = -7, \ldots, 7$. We omit rel $= -1$, so each β_k is interpreted relative to the week immediately preceding implementation. The pre-implementation coefficients $\beta_k : k \leq -2$ provide a within-window diagnostic for differential movements in the England–Scotland gap in 2023 that are not present in earlier years.

4.3 Competition as predetermined exposure within England and Wales

Motivation and unit of analysis. The second design studies heterogeneity in the short-run installation response across local markets with different pre-policy installer market structure. The policy change is national within England and Wales, so there is no untreated within-country region that is a natural control. Identification comes from comparing more competitive versus more concentrated local installer markets within the treated geography, in a specification that uses the same within-year week alignment as the border design and the same pre-2023 years to net out seasonality.

The unit of observation is a Travel-to-Work Area (TTWA) indexed by m, aggregated to TTWA-week counts Y_{m,t,w_y}. TTWAs are a standard delineation of local labor markets and provide a natural

scale for installer operations and consumer search. The analysis sample includes TTWAs in England and Wales.

Interacted difference-in-differences specification. The competition variable captures pre-existing local market structure and installer presence and is not randomly assigned. We therefore treat it as an effect modifier: our object of interest is how the policy-induced demand shift maps into installations in markets with different A_m, conditional on fixed effects, rather than a strictly causal effect of changing market structure holding policy fixed. The baseline interacted PPML specification is

$$\mathbb{E}\left[Y_{m,t,w_y} \mid \alpha_m, \delta_{t,w_y}\right] \exp\left(\alpha_m + \delta_{t,w_y} + \theta_1 A_m \times \mathbb{1}_{t=2023} + \theta_2 A_m \times \text{Post}_{t,w_y} + \beta A_m \times \mathbb{1}_{t=2023} \times \text{Post}_{t,w_y}\right), \tag{9}$$

where α_m are TTWA fixed effects and δ_{t,w_y} are year-by-week-of-year fixed effects. The main effect of A_m is absorbed by α_m; the included lower-order interactions allow markets with different A_m to have different levels in 2023 and different average pre/post patterns within year. The coefficient β is the parameter of interest. It is identified from differential changes around implementation in 2023 across TTWAs with different levels of competition in the installers market, net of differential changes in the same aligned weeks in 2020–2022.

For interpretability and to allow nonlinearity, we also estimate a version that replaces A_m with an indicator for the top quartile of HHI^{-1} within the England and Wales sample. We also use the same post-period split as in the first design, allowing the association between competition and the policy response to differ in the first four weeks and later weeks.

Identifying condition for effect heterogeneity. The identifying restriction for (9) is a conditional stability condition. Absent the grant increase, the 2023 deviation in TTWA-level installations around ISO week 43 would not be systematically related to pre-policy competition after conditioning on TTWA fixed effects, year-by-week fixed effects, and the included lower-order interactions in (9). This restriction rules out, for example, that high-competition markets experience a distinct local shock in late October 2023 that is unrelated to the subsidy increase but affects heat-pump installations at the same time. Because A_m is predetermined, this stability condition is plausible when the main confounds are time-invariant market characteristics and common national shocks, but it is not strictly guaranteed. We therefore treat the interacted estimates as evidence on heterogeneity of the policy response under this stability condition and provide placebo and robustness exercises that probe whether similar A_m-related breaks occur in other years or under alternative constructions of A_m.

4.4 Estimation, inference, and scope

PPML with high-dimensional fixed effects. Weekly installations are nonnegative counts with many zeros, especially at the LSOA level. We use PPML with a log link, following Santos Silva and Tenreyro (2006). PPML is consistent under correct specification of the conditional mean even when the Poisson distribution is misspecified and provides a natural way to incorporate multiplicative fixed effects. The log link also yields coefficients that can be interpreted as log changes in expected counts for interaction terms, with $\exp(\cdot)$ giving incidence-rate ratios.

Balanced panels and zero outcomes. For both the LSOA and TTWA designs, we construct balanced panels over the event-time window by taking the Cartesian product of geographic units and weeks present in the time spine and then merging in aggregated installation counts. Cells with no recorded installations are retained with outcome set to zero. This construction ensures that estimated effects reflect changes in installation intensity rather than changes in the set of unit-week observations with positive counts.

Standard errors. Inference uses multiway clustered standard errors, allowing for correlated shocks within geography over time and common shocks across geographies within week. In the border design, standard errors are clustered by LSOA and by a global week index that orders ISO weeks over the full sample period. In the competition design, standard errors are clustered by TTWA and by the same global week index. This follows the multiway clustering framework in Cameron et al. (2011). The global week cluster captures correlation induced by aggregate shocks not fully absorbed by year-by-week fixed effects within the restricted window. The LSOA or TTWA cluster captures serial correlation within local areas.

What is identified. The border DDD identifies a local causal effect of the BUS increase on weekly commissioned installations in the border region under the parallel-trends restriction described above. Because the policy change is nationwide within England and Wales, the border design is the only component that uses an untreated contemporaneous comparison group. The effect is local in geography and time, and it is an effect on commissioning counts. The data do not include consumer prices paid net of subsidies, quoted prices, waiting times, or capacity utilization measures, so our empirical strategy does not seek to identify pass-through or welfare.

The competition design identifies heterogeneity in the policy response across TTWAs under the conditional stability restriction described above. It does not identify the causal effect of changing market structure absent the policy shock. The interpretation is therefore comparative: how the same policy-induced demand shift is reflected in installation quantities in markets with different pre-policy installer concentration.

4.5 Threats to identification and diagnostic exercises

Anticipation and completion timing. Commissioning dates capture completion and certification, not the date of consumer decision or grant application. If consumers or installers anticipated the policy change and adjusted scheduling, some effects may appear before or immediately after implementation even without an underlying change in desired installations. The event-time specification (8) is designed to detect discontinuities in the pre-period within the restricted window. The post-period split between the first four weeks an the rest of the post-treatment allows the timing of the response to differ between the first month and later weeks, which is consistent with completion lags and capacity adjustment. The design does not separate installations that were initiated pre-policy from those initiated post-policy.

Cross-border spillovers and displacement. In the border region, installers can serve households on both sides of the England–Scotland boundary. If the BUS increase induces installers to reallocate effort from Scotland to England, the border DDD could capture both an increase in England and a potential reduction in Scotland. In this case, the estimand is a net effect on installations in the border region that includes reallocation. This is relevant for short-run incidence in a geographically integrated service market, but it complicates interpretation as a purely additive increase in installations. In section 5.3, we examine outcomes that proxy for installer reallocation and capacity, such as the distance between installation sites and installer headquarters, the share of installations performed outside an installer's home market, and counts of certified and recently active installers. These measures do not, by themselves, identify a structural mechanism, but they can indicate whether the short-run response is consistent with capacity expansion versus geographic displacement.

Other contemporaneous shocks. The year-by-week-of-year fixed effects control for common shocks at the national weekly level, but they do not control for country-specific shocks in late October 2023 that differentially affect England and Scotland. The border design therefore depends on the institutional premise that there is no Scotland-specific contemporaneous policy shift at the same week that differentially affects the Scottish border region. The competition design has an analogous concern: any 2023-specific local shocks correlated with pre-policy market structure could bias the interaction coefficient. The diagnostic strategy uses placebo implementation years, alternative constructions of A_m from earlier years, and controls for baseline adoption intensity to probe whether the estimated heterogeneity is specific to the 2023 policy change.

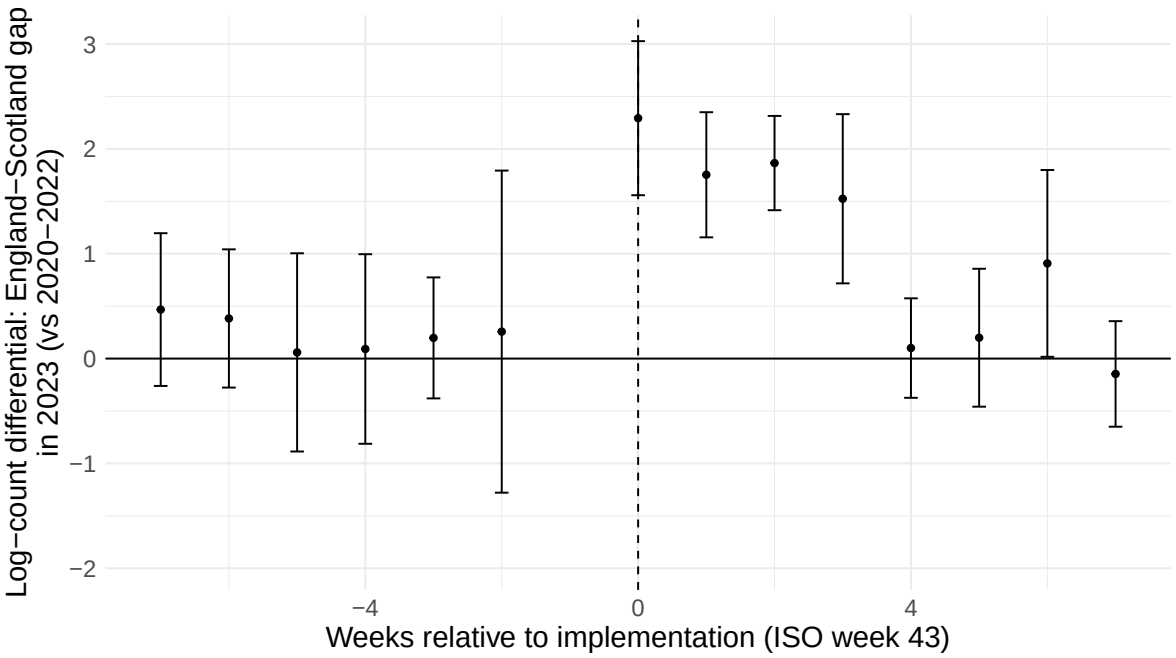


Figure 2: Border triple-difference event study for ASHP installations

Notes: The figure reports event-study coefficients from a Poisson fixed-effects regression of weekly ASHP installation counts at the LSOA level in the England–Scotland border sample. The plotted points correspond to the interaction between an England indicator, a treated-year indicator for 2023, and event-time indicators for weeks relative to ISO week 43 (the BUS implementation week). The omitted reference week is event time −1. Error bars are 95 percent confidence intervals based on two-way clustered standard errors (LSOA and global week index). The specification includes LSOA fixed effects and year-by-week-of-year fixed effects, thus identification comes from within-year week alignment across years. The sample consists of LSOAs in postcode districts adjacent to the border, observed in 2020–2023 and restricted to the ±7 week window around the implementation week. Coefficients are in log-count units (log incidence-rate ratios) and can be exponentiated to obtain multiplicative effects on the conditional mean.

5 Results

5.1 Short-run effect near the England–Scotland border

The border triple-difference estimates quantify the change in weekly ASHP installations in English border areas after the BUS increase in 2023, relative to Scottish border areas and relative to the same within-year weeks in 2020–2022. Figure 2 plots the event-study coefficients for the triple interaction. The pre-implementation coefficients (relative to the omitted week −1) are centered near zero. A Wald test of joint nullity for the six pre-period coefficients from event weeks −7 through −2 yields a p-value of 0.915, consistent with no detectable pre-trend in the England–Scotland gap within the ±7 week window. Post implementation, the coefficients jump upward and then attenuate within the short horizon shown.

Table 1 reports the corresponding average post-implementation effects in the ±7 week window. Column (1) imposes a single post indicator and estimates a precisely estimated triple-interaction coefficient of 0.915. In a log-link count model, this coefficient implies that the conditional mean of

Table 1: Border triple-difference estimates of the BUS effect on installations

	Air-source HPs			All HPs
	(1)	(2)	(3)	(4)
Triple-interaction	0.915***			
	(0.297)			
Triple-interaction, weeks 0-3		1.697***	1.800***	1.624***
		(0.277)	(0.296)	(0.267)
Triple-interaction, weeks 4+		0.149	0.070	0.060
		(0.300)	(0.310)	(0.299)
LSOA FE	Yes	Yes	Yes	Yes
Time FE (weekly)	Yes	Yes	Yes	Yes
Rural-Urban × Time FE	No	No	Yes	No
Observations	30,420	30,420	30,167	30,660
AIC	12,723	12,691	12,824	13,121

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Each column reports coefficients from a Poisson fixed-effects regression of weekly installation counts in the England–Scotland border sample, estimated by PPML with high-dimensional fixed effects. The dependent variable is the weekly count of air-source heat pumps (columns 1–3) or all heat pumps (column 4), aggregated to LSOA-week. The key regressor is the triple interaction between an England indicator, a treated-year indicator for 2023, and post-implementation indicators defined relative to ISO week 43 within each year. The saturated set of interaction terms are included in all specifications, but are omitted here for brevity. The full results table is included in Table C.1. All specifications include LSOA fixed effects and year-by-week-of-year fixed effects. Column (3) additionally includes rural–urban-by-week fixed effects to allow differential seasonal patterns by rural status within the short window. Standard errors (in parentheses) are two-way clustered by LSOA and global week index. The estimation window is ±7 weeks around implementation in all columns.

weekly ASHP installations in the treated English border LSOAs increases by a factor of about 2.5 relative to the control comparison. This is a local short-run effect near the border: the identifying variation is within a narrow set of postcode districts and a two-month window, and the estimand is the change in the England–Scotland gap in 2023 relative to the same gap around ISO week 43 in 2020–2022.

Columns (2) and (3) relax the single-step post effect by allowing distinct coefficients for the first four post weeks and for weeks 4 and later. This split is motivated by two features of the data and institutional setting. Commissioning dates reflect completion, so the earliest post weeks can include installations that were scheduled before the grant change but completed after it. At the same time, adjustments on the supply side, such as scheduling additional crews or activating installers, may take more than a few weeks. Further, Figure 2indicates that the response is concentrated within the first 4 weeks of implementation.

We therefore permit a separate first-month response (weeks 0–3) and a later response (weeks 4+), without fitting a fully flexible time path that would be underpowered in the short window.

Column (2) indicates that the short-run increase is concentrated in the first month. The later coefficient is small and statistically indistinguishable from zero.

To ensure that differential rural seasonality within the narrow window does not explain this first-month increase, we include adds rural–urban-by-week fixed effects in Column (3) and obtain a similar pattern: a large and statistically significant coefficient of 1.8 for weeks 0–3 and a near-zero coefficient for weeks 4+. Column (4) repeats the timing-split specification for all heat-pump installations (air-source plus ground- and water-source)and again produces a similar temporal pattern, with very similar magnitude.

Figure 3 translates the event-study estimates into an absolute magnitude for the treated English border area. The figure plots the cumulative implied number of additional ASHP installations in treated English border LSOAs relative to a model-implied counterfactual that shuts off the triple-interaction terms, centered so that cumulative effects equal zero at event week -1. The cumulative series rises sharply at implementation and then flattens within the short window. The underlying calculations imply roughly 30 additional installations in event week 0 and about 90 cumulative additional installations by event week 3. By event week 7, there are around 100 cumulative additional installations in the treated border area. This calculation is local by construction: it aggregates predicted mean differences within the treated English border LSOAs in 2023, given the fitted event-study model and the fixed-effects structure, and it does not extrapolate beyond the window.

5.2 Effect heterogeneity by pre-policy competition

The competition heterogeneity design quantifies how the post-implementation change in installations varies with pre-policy installer concentration at the local labor-market level. Table 2 reports estimates for England and Wales TTWAs within the same ± 7 event window. Competition is measured as a standardized inverse Herfindahl–Hirschman index (inverse HHI) constructed from installer shares in a pre-policy period, with the baseline measure computed using installations from 2015–2021. The interaction structure estimates differential effects of the BUS increase with increased market competition, conditional on TTWA fixed effects and year-by-week-of-year fixed effects.

Column (1) estimates a coefficient of 0.036, which implies that a one-standard-deviation increase in pre-policy competition is associated with a 3.7 percent larger post-implementation increase in the conditional mean of weekly ASHP installations. Interestingly, Column (2) indicates that this differential effect is concentrated *after* the first month: the weeks 4+ coefficient implies a 5.1 percent larger post change per standard deviation of competition.

Columns (3) and (4) assess nonlinearity by comparing TTWAs in the top quartile of inverse HHI to the bottom three quartiles and reveal a similar temporal pattern. Column (4) again locates the differential response in weeks 4+, with a full 14.2% larger post-BUS upgrade response per s.d. of competition for TTWAs in the top quartile of inverse HHI.

The timing pattern in Table 2 differs from the border design, where the average effect is concentrated in weeks 0–3. This is consistent with the heterogeneity design capturing differential adjust-

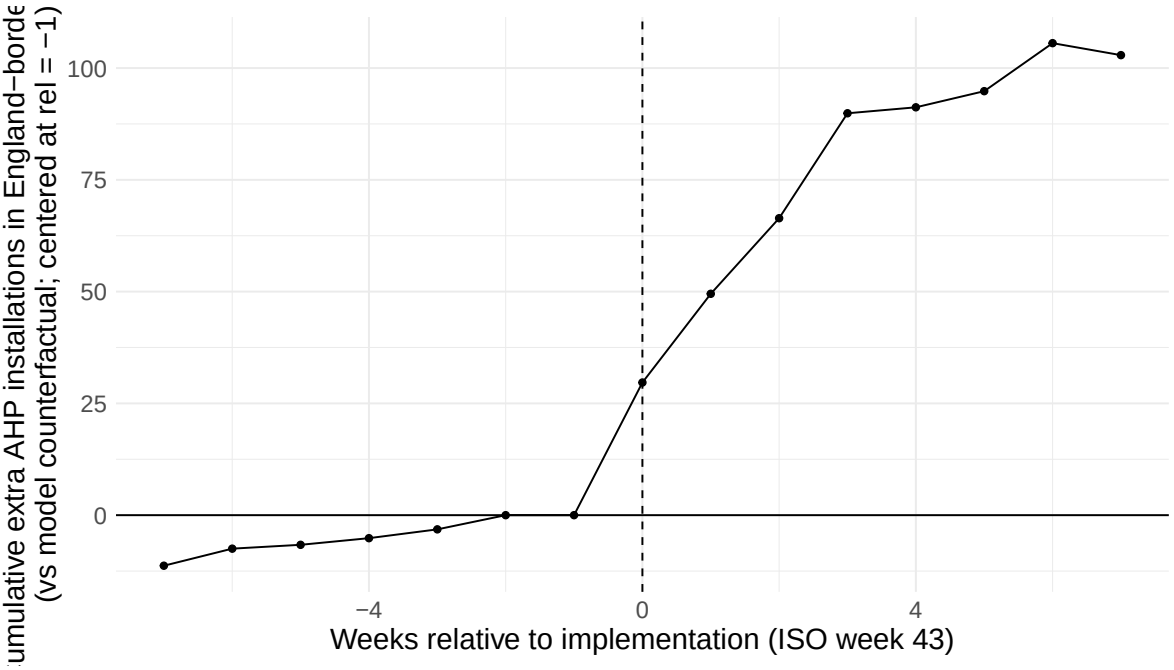


Figure 3: Cumulative implied additional ASHP installations in treated English border areas

Notes: The figure reports the cumulative number of additional ASHP installations implied by the border event-study model in Figure 2. For treated observations (English border LSOAs in 2023), we compute the model-predicted conditional mean with treatment and a counterfactual mean that divides out the estimated event-time triple-interaction term for each week. The difference between the predicted mean and the counterfactual mean is aggregated over treated LSOAs within each event week and cumulated over event time. The series is centered so that the cumulative effect at event week −1 is zero. The vertical line at event time 0 marks implementation week. The calculation uses the same ±7 week estimation window as the event-study model.

ment across markets that may manifest with delay, while the border design captures an immediate average response in a local area.

Several design choices embedded in Table 2 discipline interpretation. The competition metric in each market is constructed from a long pre-policy window ending in 2021, and it is time invariant by construction. This reduces the risk that the competition proxy mechanically reflects the policy period. The TTWA is used as the market unit because it is a commuting-based local labor-market definition and aligns with the location of installer headquarters in the installer registry. The identifying assumption is that, conditional on TTWA fixed effects and year-by-week-of-year fixed effects, there are no competition-correlated shocks in 2023 that differentially affect installations at ISO week 43. The results therefore speak to heterogeneity in the installation response to the subsidy change across pre-existing market structures, even if it does not strictly identify the causal effect of changing market structure itself.

5.3 Adjustment evidence from installer activity and spatial allocation

We now turn to potential adjustment mechanisms. Tables 3 and 4 report outcomes that proxy supply-side adjustment and reallocation. These outcomes are measured using the installer registry

Table 2: Heterogeneity in the installation response by pre-policy competition (TTWA level)

	Continuous market access		Top quartile of market access	
	(1)	(2)	(3)	(4)
Triple-interaction	0.036***		0.105*	
	(0.014)		(0.062)	
Triple-interaction, weeks 0-3		0.022		0.078
		(0.019)		(0.065)
Triple-interaction, weeks 4+		0.050***		0.133**
		(0.010)		(0.067)
TTWA FE	Yes	Yes	Yes	Yes
Time FE (weekly)	Yes	Yes	Yes	Yes
Observations	10,380	10,380	10,380	10,380
AIC	40,838	40,841	40,821	40,822

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Each column reports coefficients from a Poisson fixed-effects regression of weekly ASHP installation counts at the TTWA-week level for England and Wales, estimated by PPML with high-dimensional fixed effects. The key regressor is the interaction between a treated-year indicator for 2023, post-implementation indicators defined relative to ISO week 43 within each year, and a pre-policy competition measure. In columns (1) and (2), competition is the standardized inverse HHI computed using installer shares in TTWA-level installations over 2015–2021 (mean zero, standard deviation one). In columns (3) and (4), our continuous competition metric is discretized as an indicator for the top quartile of inverse HHI relative to the bottom three quartiles. The saturated set of coefficients from the triple-difference are included but omitted for brevity. All specifications include TTWA fixed effects and year-by-week-of-year fixed effects. Standard errors (in parentheses) are two-way clustered by TTWA and global week index.

and installer identifiers in the MCS installation data. They do not claim to establish a structural mechanism, but they provide diagnostics relevant to two concerns for interpreting the main quantity results: whether the short-run increase is accommodated by changes in installer participation and whether it reflects spatial reallocation of installations across installer territories.

Table 3 uses two measures derived from installer headquarters locations: the mean distance between an installation and the installer headquarters and the share of installations completed outside an installer's headquarters TTWA ("off-site" installations). Columns (1) and (3) report border triple-difference estimates; columns (2) and (4) report competition interactions within England and Wales. The distance outcome is estimated in logs, and the off-site share is estimated using a probit specification for the share outcome. The border estimates are imprecise for distance in both post subperiods. For the off-site share, the weeks 4+ indicates a decline in the share of installations conducted outside an installer's headquarters TTWA in treated English border areas relative to the comparison. This pattern is consistent with a greater reliance on locally headquartered installers later in the post period within the short window. The competition interactions for distance are negative and statistically significant in weeks 0–3, implying that higher-competition markets exhibit

Table 3: Distance to installer headquarters and off-site installation shares

	Distance to installer		Share of off-site installations	
	Scotland TDiD	Market access	Scotland TDiD	Market access
	(1)	(2)	(3)	(4)
Triple-interaction, weeks 0-3	0.344	-0.095**	0.244	-0.020
	(0.345)	(0.047)	(0.529)	(0.048)
Triple-interaction, weeks 4+	-0.375	-0.049	-1.128**	-0.007
	(0.442)	(0.055)	(0.462)	(0.058)
TTWA FE	Yes	Yes	Yes	Yes
Time FE (weekly)	Yes	Yes	Yes	Yes
Observations	59,352	7,324	55,499	6,935
AIC	133,346	19,495	34,830	6,531

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Columns (1) and (2) use log(mean distance) as the dependent variable, where mean distance is the TTWA-week average of the distance (km) between the installation postcode centroid and the installer headquarters coordinate, computed for ASHP installations with non-missing installer identifiers and valid coordinates. Columns (3) and (4) use the TTWA-week share of "off-site" installations as the dependent variable, defined as the fraction of ASHP installations in a TTWA-week whose installer headquarters TTWA differs from the installation TTWA; these columns are estimated using a probit link at the TTWA-week level. Columns (1) and (3) report border triple-difference specifications comparing English versus Scottish border areas in 2023 relative to 2020–2022; columns (2) and (4) report the competition interaction specifications in England and Wales. All columns include TTWA fixed effects and year-by-week-of-year fixed effects, within the ±7 window. Standard errors (in parentheses) are two-way clustered by TTWA and global week index.

a smaller increase in installation distance immediately after implementation. The corresponding interactions for the off-site share are small and statistically indistinguishable from zero.

Table 4 reports two measures of installer participation: the number of certified heat-pump installers active in a TTWA-week based on the installer registry ("certified installers") and the number of installers that installed at least one heat pump in the previous four weeks within a TTWA ("active installers"). The certified-installer count is based on the MCS installers registry and does not directly depend on contemporaneous installations, and is measured at weekly frequency by overlapping the registry start and end dates with the ISO week. The active-installer count is constructed from the MCS installation data and is therefore mechanically related to installations by definition; for that outcome, coefficients should be interpreted as describing changes in the concentration of realized activity rather than pure entry.

Two patterns in Table 4 align with the heterogeneity in Table 2. First, the competition interactions for active installers are positive and statistically significant in both subperiods. Second, in the border triple-difference columns, the active-installer coefficient in weeks 0–3 implies a 55 percent increase in active installer identifiers in treated English border areas relative to the comparison within the estimation window. The corresponding certified-installer coefficient in weeks 0–3

Table 4: Installer participation: certified and active installer counts

	Certified installers count		Active installers count	
	Scotland TDiD	Market access	Scotland TDiD	Market access
	(1)	(2)	(3)	(4)
Triple-interaction, weeks 0-3	0.057*	-0.003	0.435**	0.015*
	(0.032)	(0.005)	(0.172)	(0.009)
Triple-interaction, weeks 4+	0.029	-0.007	0.273	0.014**
	(0.058)	(0.005)	(0.204)	(0.007)
LSOA FE	Yes	No	No	No
TTWA FE	No	Yes	Yes	Yes
Time FE (weekly)	Yes	Yes	Yes	Yes
Rural-Urban × Time FE	No	No	No	No
Observations	76,680	9,840	1,140	10,380
AIC	261,220	32,965	4,889	47,088

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports Poisson fixed-effects estimates for two installer participation outcomes measured at weekly frequency. 'Certified installers" counts the number of installers certified to install heat pumps whose certification window overlaps the relevant ISO week in the installer registry, aggregated to TTWA-week; the border specification assigns certification counts to LSOA-weeks via the LSOA's TTWA. 'Active installers" counts, for each TTWA-week, the number of distinct installer identifiers that installed at least one heat pump in the previous four weeks in that TTWA. Columns (1) and (3) report border triple-difference specifications comparing English versus Scottish border areas in 2023 relative to 2020–2022. Columns (2) and (4) report competition interaction specifications for England and Wales. All specifications estimated within the ±7 window, with TTWA and year-by-week-of-year fixed effects. Standard errors (in parentheses) are two-way clustered by TTWA and global week index.

weakly significant and consistent with a 5.9% increase, while the weeks 4+ coefficient is smaller and statistically indistinguishable from zero.

Taken together, the activity measures are consistent with an increase in the set of installers performing installations immediately after the subsidy change, particularly in the high-competition markets. The certified-installer results suggest some contemporaneous change in the pool of certified installers near the border, but the effect is modest in magnitude and imprecisely estimated.

The distance and off-site outcomes in Table 3 provide limited evidence for large-scale spatial reallocation. In particular, the market-competition interactions for the off-site share are close to zero, and the border design does not show an increase in off-site activity in the later weeks. Within the confines of the measured outcomes and the short window, the patterns are more consistent with additional local activity rather than with a large shift toward longer-distance installations.

5.4 Robustness checks

The ±7 week window is a design choice motivated by the calendar position of ISO week 43. Because the empirical design aligns weeks within year, extending the post window mechanically brings holiday weeks into the analysis. These weeks plausibly differ in installation activity for reasons unrelated to subsidy generosity, including seasonal shutdowns and reporting lags. We therefore interpret sensitivity to wider windows as evidence on contamination rather than as a search over horizons.

Appendix Table C.2 reports the border triple-difference estimate from the single-post specification (analogous to Table 1 column 1) as the window width expands from ±4 to ±9 weeks. The estimated coefficient declines monotonically with window width: from 1.417 at ±4 weeks to 0.642 at ±9 weeks. This pattern is consistent with additional late-year weeks diluting the immediate post-implementation change documented in Table 1 and Figure 2. Given that the policy change occurs late in the calendar year, and given the within-year alignment, the window sensitivity reinforces the rationale for keeping the baseline window short.

Appendix Tables C.3 and C.4 report two placebo exercises for the border design. The placebo-year table assigns "treated year" status to earlier years while keeping the implementation week fixed at ISO week 43 and uses the same border sample. The estimated triple interaction is small and statistically indistinguishable from zero in the pseudo-treated years shown. The alternative-border placebo compares a set of Welsh and English districts of similar scale to the Scottish border sample and estimates the same triple-difference specification. The placebo-border estimates are close to zero and statistically indistinguishable from the main effect, suggesting that the main border estimates are not mechanically generated by the estimation procedure applied to an arbitrary border. These placebo tests are consistent with the identifying assumption that, absent the BUS change, the change in the England–Scotland gap around ISO week 43 in 2023 would have resembled that gap change in earlier years.

Appendix Tables C.5 and C.7 provide analogous diagnostics for the competition heterogeneity design. Recomputing the competition index using 2015–2019 data yields interaction estimates similar to the baseline measure (Appendix Table C.5). Appendix Table C.7 assigns placebo treated years and yields near-zero interaction effects.

Our competition design results could simply indicate that high competition areas are places where heat pumps were already more feasible or desired, which would make them respond more to any subsidy. To test against that possibility, Appendix Table C.6 adds an interaction based on baseline adoption intensity (mean pre-period installations) and yields an interaction with the competition index that remains positive and statistically significant, while the interaction with baseline adoption is small and statistically indistinguishable from zero.

Collectively, these results support interpreting Table 2 as evidence that the post-implementation installation response varies systematically with pre-policy market structure, conditional on the fixed effects and within-year alignment.

6 Conclusion

We study the short-run incidence of a large demand subsidy in an installed-technology market. The October 2023 increase in the Boiler Upgrade Scheme grant for heat pumps provides a discrete, nationwide demand shift in England and Wales, with Scotland serving as a contemporaneous untreated comparison. Using Microgeneration Certification Scheme commissioning records and a border triple-difference design around the England–Scotland boundary, we estimate a large increase in completed air-source installations on the English side. In our baseline specification, the estimated post coefficient implies a 150% increase in weekly air-source installations in treated English border areas (Table 1), concentrated in the first four weeks after implementation (Figure 2). Aggregating the event-study predictions implies about 100 additional air-source installations within two months in the treated English border area (Figure 3).

The same subsidy change maps into installations differently across local markets. In a TTWA-level interacted design, pre-policy installer competition, measured by a standardized inverse HHI computed from pre-2023 installations, predicts a larger post-implementation increase in weekly heat pump installations. A one-standard-deviation increase in competition is associated with a 3.7% larger post-implementation change in 2023 relative to the same weeks in 2020–2022, with the differential concentrated *after* week 4 (Table 2). Installer participation patterns are consistent with supply-side adjustment along the extensive margin. Active installer counts rise after implementation in treated border areas and increase more in higher-competition TTWAs (Table 4), while distance and off-site installation measures provide limited evidence of large short-run spatial reallocation (Table 3).

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A Additional data construction details

A.1 Cleaning of technical and cost fields

The MCS extract contains an "overall cost" field and a "total installed capacity" field that are not used as outcomes in the main analysis but are used to pre-process the dataset. Two cleaning steps are applied before aggregation. First, reported zeros in cost and capacity are treated as missing. Second, we trim the joint distribution by dropping installations with cost or capacity outside the 1^{st} and 99^{th} percentiles of the respective non-missing distributions. Cost is converted to real 2015 GBP using the ONS annual CPI series: for installation year y, real cost equals nominal cost divided by $(\text{CPI}_y/100)$. The data do not include itemized cost components, financing terms, or the timing of payments, so the cost variable should be interpreted as a reported total at commissioning.

A.2 Balanced-panel grid construction

For each geography (LSOA and TTWA), we build a complete area-by-week grid using the set of areas observed in the postcode lookup and the set of ISO weeks spanned by the microdata over the relevant period. The time spine includes the ISO year, ISO week-of-year, week start date, and the fixed-origin week index. Aggregated outcomes are merged into this grid. For count outcomes, unmerged cells are assigned zero. For means, unmerged cells correspond to area-weeks with no installations and therefore have undefined means; in the constructed dataset these cells are set to zero for convenience, but any analysis that uses means conditions on having at least one installation in the area-week.

A.3 Installer headquarter merges and recycled identifiers

The installer identifier in the MCS installation extract is the installation company's MCS number. The installer registry also reports installer identifiers and names. A small number of installer identifiers (19) are reused across time in the registry. For these cases, headquarter and certification information are merged using installer name rather than identifier, and the corresponding identifiers are excluded from identifier-based merges. The MCS registry does not include a unique, time-invariant firm identifier that resolves all such cases without using names.

A.4 Distance and off-site measures

For each installation with a successfully merged installer headquarter location and with valid installation coordinates, we compute the great-circle (Haversine) distance between the geolocated installation and headquarter coordinates, in kilometers. We define an installation as off-site if the installation TTWA differs from the installer's headquarter TTWA. We aggregate these measures to TTWA-week cells: mean distance, counts of in-TTWA and out-of-TTWA installations, and the

share of off-site installations. These measures are only defined for installations with valid coordinates and a successfully assigned headquarter location.

A.5 Certified and active installer counts

Certified installers in TTWA m during week w are counted from the installer registry as those whose valid certification window includes the week start date. The registry does not report the intensity of activity within a certification spell. Active installers in TTWA m during week w are defined as the number of distinct installers that completed at least one installation in TTWA m in the prior four weeks (inclusive of week w). Because this measure is constructed from installations, it mechanically co-moves with installation volumes – albeit wih a lag. It is therefore interpreted as a descriptive measure of short-run participation rather than as an independent measure of capacity.

B Empirical strategy details

B.1 Week construction and global week index

The ISO-week construction produces two time indices used in estimation. The within-year index is (t, w_y), where t is the ISO year and w_y is the ISO week-of-year. The event-time index rel_{t,w_y} is defined with respect to the ISO week of the BUS increase and is integer-valued by construction.

The global week index w is a running count of ISO weeks since a fixed origin week. Let origin denote the Monday starting ISO week 1 of 2009. For any observation week start date $\text{weekstart}_{t,w_y}$, define

$$w \equiv \left\lfloor \frac{\text{weekstart}_{t,w_y} - \text{origin}}{7 \text{ days}} \right\rfloor \tag{10}$$

This index is used for clustering and for specifications that include time fixed effects interacted with a coarse geography attribute (for example, rural versus urban) at the weekly level.

B.2 Competition measurement

Competition is based on installer concentration within TTWAs in a pre-policy period. For each TTWA m and installer j, let N_{jm} be the number of recorded installations in the pre-policy period. Installer shares are $s_{jm} = N_{jm} / \sum_{j'} N_{j'm}$. The inverse HHI is computed as in equation (4).

Two features of this construction are relevant for interpretation. First, HHI_m^{-1} is computed using installation shares, not counts of registered installers, so it captures the distribution of realized activity rather than the size of the installer registry. Second, because HHI_m^{-1} is computed from a pre-policy window, it is mechanically independent of post-policy outcomes, but it can still correlate with local demand fundamentals and with unobserved factors that shape the 2023 response. The main specifications absorb time-invariant TTWA characteristics via fixed effects and probe remaining concerns using alternative pre-period constructions of HHI_m^{-1} and controls for baseline adoption intensity.

The discretized competition index specification replaces A_m with an indicator for being in the top quartile of HHI_m^{-1} within the analysis sample. This specification does not impose linearity in the relationship between concentration and response.

B.3 Placebo and robustness designs

The diagnostic exercises use the same aligned-week framework as the main specifications.

For the border design, placebo treated years are implemented by re-defining the treated year indicator as $\mathbb{1}_{t=y}$ for a placebo year y and defining control years as the three years preceding y. The event week remains ISO week 43 in each year, so the placebo tests evaluate whether there are spurious discontinuities at the same week-of-year in years without the policy change (Tables C.3 and C.7).

An additional geographic placebo uses a Wales–England pseudo-border of comparable size, constructed from a small set of postcode-district prefixes on each side, to assess whether border geography alone generates spurious breaks in the outcome (Table C.4).

Window-width robustness is implemented by re-estimating the border DDD with symmetric windows $|\text{rel}| \leq h$ for alternative values of h (Table C.2). Expanding the window increases exposure to seasonal changes and end-of-year timing shifts, so the identifying content comes primarily from the pre-specified short window.

For the competition design, placebo treated years are implemented analogously within England and Wales. In addition, the analysis uses an alternative competition index construction based on an earlier pre-policy period (Table C.5) and a specification that interacts the treated-year-by-post indicator with a TTWA-specific baseline adoption intensity, measured as a pre-period mean of weekly air-source installations (Table C.6). This control allows markets with higher baseline adoption to have a different 2023 discontinuity at the implementation week, independent of the level of competition.

B.4 Supply-side diagnostics and measurement limits

To probe spillovers and supply adjustment, we compute outcomes derived from linking installations to installer identifiers and to installer headquarters locations.

The distance outcome is constructed at the installation level as the haversine distance between the installation postcode centroid and the installer headquarters coordinates, then aggregated to TTWA-week means. This variable is only observed for installations whose installer identifier can be matched to a headquarters location in the installer registry. TTWA-week cells with no matched installations have undefined means and therefore do not contribute to distance regressions after log transformation.

The off-site share is defined as the share of installations in a TTWA-week performed by installers whose headquarters TTWA differs from the installation TTWA. This outcome is undefined in TTWA-week cells with zero installations and, like the distance measure, is estimated on the subset of TTWA-week cells with at least one matched installation.

Installer availability is proxied using two measures. The "certified installers" count is based on installer registry start and end dates for heat-pump certification and is defined for all TTWA-week cells. The "active installers" count is defined using installation activity, counting installers with at least one installation in the previous four weeks within a TTWA. Because this measure is mechanically related to installation volumes, it is treated as a descriptive indicator of short-run market participation rather than an independent outcome.

C Additional robustness and results

Table C.1: Border triple-difference estimates: full coefficient set

	Air-source HPs			All HPs
	(1)	(2)	(3)	(4)
Triple-interaction	0.915***			
	(0.297)			
Triple-interaction, weeks 0-3		1.697***	1.800***	1.624***
		(0.277)	(0.296)	(0.267)
Triple-interaction, weeks 4+		0.149	0.070	0.060
		(0.300)	(0.310)	(0.299)
England $\times$ $y = 2023$	-1.368***	-1.368***	-1.187***	-1.300***
	(0.247)	(0.247)	(0.270)	(0.240)
England $\times$ $w_y \geqslant 43$	-0.324			
	(0.247)			
England $\times$ $w_y \in [43, 46]$		-0.786***	-0.797***	-0.699***
		(0.248)	(0.264)	(0.237)
England $\times$ $w_y \geqslant 47$		0.087	0.084	0.196
		(0.259)	(0.270)	(0.258)
LSOA FE	Yes	Yes	Yes	Yes
Time FE (weekly)	Yes	Yes	Yes	Yes
Rural-Urban $\times$ Time FE	No	No	Yes	No
Observations	30,420	30,420	30,167	30,660
AIC	12,723	12,691	12,824	13,121

* p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01

Notes: This table reports the same specifications as Table 1, but includes the additional interaction terms that are not absorbed by the fixed effects. The dependent variables, samples, fixed effects, clustering, and ± 7 event window are identical to Table 1. The additional coefficients clarify which components of the England–year and England–post interactions are netted out by the fixed effects structure and which remain identified. Coefficients are log incidence-rate ratios from PPML.

Table C.2: Sensitivity of the border estimate to the event-window width

	4 weeks	5 weeks	6 weeks	7 weeks	8 weeks	9 weeks
Triple-interaction	1.417***	1.226***	1.112***	0.915***	0.755***	0.642**
	(0.355)	(0.336)	(0.316)	(0.297)	(0.292)	(0.286)
England $\times$ $y = 2023$	-1.435***	-1.453***	-1.412***	-1.368***	-1.285***	-1.200***
	(0.306)	(0.284)	(0.264)	(0.247)	(0.241)	(0.225)
England $\times$ $w_y \geqslant 43$	-0.705**	-0.526*	-0.415	-0.324	-0.288	-0.255
	(0.302)	(0.286)	(0.266)	(0.247)	(0.230)	(0.216)
LSOA FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE (weekly)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,968	18,920	24,284	30,420	36,040	41,496
AIC	7,398	9,193	11,030	12,723	14,095	15,101

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Each column reports the triple-interaction coefficient from the border triple-difference specification analogous to Table 1 column (1), estimated on progressively wider symmetric windows around ISO week 43. The dependent variable is the weekly count of ASHP installations at the LSOA-week level. All specifications include LSOA fixed effects and year-by-week-of-year fixed effects and use two-way clustered standard errors (LSOA and global week index). The table is a diagnostic for sensitivity to calendar-time composition. Windows wider than ± 7 weeks extend into the late-December holiday weeks in the post period because the policy occurs in ISO week 43.

Table C.3: Border triple-difference placebo treated years (ISO week 43)

	(2018)	(2020)	(2022)
Triple-interaction	-0.579	0.543	-0.264
	(0.622)	(0.567)	(0.439)
England $\times$ $y = 2023$	0.658	-0.467	-0.179
	(0.464)	(0.518)	(0.349)
England $\times$ $w_y \geqslant 43$	0.153	-0.473	-0.250
	(0.395)	(0.311)	(0.297)
LSOA FE	Yes	Yes	Yes
Time FE (weekly)	Yes	Yes	Yes
Observations	12,480	15,300	25,620
AIC	4,187	5,440	9,482

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table estimates the border triple-difference specification on placebo treatment years, keeping the event week fixed at ISO week 43 and defining the treated-year indicator as the placebo year shown in the column header. The dependent variable is the weekly count of ASHP installations at the LSOA-week level. The sample consists of the same border districts used in the main design, and each placebo regression uses the three preceding years as controls. All specifications include LSOA fixed effects and year-by-week-of-year fixed effects and use two-way clustered standard errors (LSOA and global week index). A triple-interaction estimate near zero is consistent with the absence of systematic discontinuities at ISO week 43 in years without the 2023 BUS policy change.

Table C.4: Alternative-border placebo: Wales–England districts

	(1)	(2)
Triple-interaction	0.280	0.268
	(0.303)	(0.301)
England $\times$ $y = 2023$	-1.232***	-1.236***
	(0.272)	(0.263)
England $\times$ $w_y \geqslant 43$	-0.050	-0.021
	(0.207)	(0.210)
LSOA FE	Yes	Yes
Time FE (weekly)	Yes	Yes
Observations	20,940	20,806
AIC	9,425	9,612

* p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01

Notes: The table estimates a placebo border specification comparing selected Welsh and English postcode districts of similar scale to the Scotland–England border sample. The dependent variable is the weekly count of ASHP installations at the LSOA-week level, and the treated-year indicator is 2023. The "post" indicator is defined relative to ISO week 43 within year, and the estimation window is ± 7 weeks. Column (2) adds rural–urban-by-week fixed effects. Standard errors are two-way clustered by LSOA and global week index. This test successfully that applying the same estimation procedure to an arbitrary nearby border does not generate a spurious discontinuity around ISO week 43 in 2023.

Table C.5: Competition index measured using 2015–2019 installations

	Continuous market access		Top quartile of market access	
	(1)	(2)	(3)	(4)
Triple-interaction	0.045***		0.105*	
	(0.013)		(0.062)	
Triple-interaction, weeks 0-3		0.021		0.078
		(0.016)		(0.065)
Triple-interaction, weeks 4+		0.067***		0.133**
		(0.012)		(0.067)
TTWA FE	Yes	Yes	Yes	Yes
Time FE (weekly)	Yes	Yes	Yes	Yes
Observations	10,380	10,380	10,380	10,380
AIC	40,832	40,832	40,821	40,822

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table replicates Table 2 using a competition measure computed from TTWA-level installer shares over 2015–2019 rather than 2015–2021. The dependent variable remains the TTWA-week count of ASHP installations in England and Wales, estimated by PPML with TTWA fixed effects and year-by-week-of-year fixed effects. Standard errors are two-way clustered by TTWA and global week index, and the estimation window is ±7 weeks around ISO week 43. The comparison assesses sensitivity to the pre-period used to construct the competition index.

Table C.6: Competition interactions controlling for baseline adoption intensity

	Mean installations (2015-2021)	Mean installations (2020-2022 outside treatment window)
	(1)	(2)
Triple-interaction with market access	0.039***	0.039**
	(0.013)	(0.017)
Triple-interaction with mean installations in pre-period	-0.014	-0.003
	(0.029)	(0.013)
TTWA FE	Yes	Yes
Time FE (weekly)	Yes	Yes
Observations	10,380	10,380
AIC	40,528	40,668

* p < 0.1, ** p < 0.05, *** p < 0.01

Notes: This table augments the competition heterogeneity specification by adding an interaction between the post-implementation indicator and a TTWA-level measure of baseline adoption intensity, defined as the mean weekly ASHP installations in a pre-period. Column (1) uses 2015–2021 to compute the baseline mean; column (2) uses 2020–2022 observations outside the ±7 treatment window. The dependent variable is the TTWA-week count of ASHP installations in England and Wales, and the specification includes TTWA fixed effects and year-by-week-of-year fixed effects. Standard errors are two-way clustered by TTWA and global week index. The goal is to assess whether the competition interaction is capturing differential trends correlated with baseline adoption levels rather than heterogeneity in the policy response.

Table C.7: Competition design: placebo treated years (ISO week 43)

	(2018)	(2020)	(2022)
Triple-interaction	-0.029	-0.052	0.016
	(0.080)	(0.068)	(0.024)
TTWA FE	Yes	Yes	Yes
Time FE (weekly)	Yes	Yes	Yes
Observations	10,380	10,380	10,380
AIC	24,380	28,609	36,118

* p < 0.1, ** p < 0.05, *** p < 0.01

Notes: This table estimates the competition interaction specification on placebo treated years, defining the treated-year indicator as the placebo year shown in the column header while keeping the event week fixed at ISO week 43. The dependent variable is the TTWA-week count of ASHP installations in England and Wales. Competition index is the standardized inverse HHI computed using the baseline pre-policy window. The specification includes TTWA fixed effects and year-by-week-of-year fixed effects and uses two-way clustered standard errors (TTWA and global week index). Near-zero placebo interactions support the assumption that competition heterogeneity does not predict differential discontinuities around ISO week 43 absent the 2023 policy change.