Electric Vehicle Sharing and Adoption: Evidence from BlueLA

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ABSTRACT. We estimate the impact of an Electric Vehicle (EV) sharing program—specifically BlueLA—on EV adoption. This program provides EVs in heavily trafficked areas in several low- to middle-income neighborhoods in Los Angeles. Using data on EV purchases from a subsidy program and a difference-in-differences strategy, we estimate that BlueLA increased new EV adoptions within the zip codes it entered. We document patterns consistent with reduced informational frictions increasing consumer willingness to adopt. Our findings suggest that policies increasing availability of EVs for public use may facilitate adoption, particularly in underserved areas.

KEYWORDS. Electric Vehicles, Car Sharing, Technology Adoption.
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1 Introduction

Although households with lower incomes are typically more price-sensitive, electric vehicle (EV) purchasing subsidies are often used by higher-income buyers who may have bought EVs without subsidies (Borenstein and Davis, 2016; Xing et al., 2021; Muehlegger and Rapson, 2022a; Linn, 2022). Additionally, vehicles driven by low-income households are typically less fuel efficient, implying relatively greater benefits to the reduction of pollution from the adoption of EVs (Ferrell et al., 2015; Jacobsen et al., 2023). This raises a key question of how to encourage EV adoption among low-income households. This article presents quasi-experimental evidence that a non-price intervention related to EVs spurred new EV purchases among low-income households. While perhaps unexpected that a car sharing program would spur households to adopt EVs, we document patterns consistent with (indirect) informational externalities as responsible for the finding.

The intervention we study is the rollout of the BlueLA EV car-sharing program. This program placed EVs for the general public in low- to middle-income neighborhoods in Los Angeles in busy areas, with the aim of increasing access to green technologies in communities that traditionally benefit less from them. The most relevant features for our work include: First, *anyone* could use an EV through the program, with added convenience for those immediately surrounding its locations. Second, the program's EVs were placed in highly central areas, obtainable due to involvement from local government. Third, as a public-private partnership, the costs of using the program are kept low, at least compared to competitive car-sharing programs.

We study the impact of the introduction of BlueLA on the takeup of an EV purchasing subsidy targeted at low- to middle-income households. We emphasize that the subsidies we consider are not directly related to BlueLA in any way, implying that any relationship between the two could only arise via some other mechanism. This observation distinguishes our exercise from previous work on how subsidies directly influence EV demand. Our interest is in how *other* channels, especially information, influence their effectiveness. The question of what determines the effectiveness of the subsidies, particularly among low- to middle-income households, is highly policy-relevant.

Our first contribution is to document that the BlueLA program has *spurred EV adoption* among the population that our data covers. Specifically, we estimate the change in EV purchases using the aforementioned subsidy within a zip code once BlueLA is introduced. We document economically meaningful increases attributable to the program, a finding that appears robust across different empirical methodologies. As this variation does not change access to the relevant subsidy, this finding suggests that BlueLA increased its effectiveness.

To explain this finding, we conjecture that by increasing the local prevalence of EVs, the program reduced the wedge between consumers' believed (subjective) surplus from EV adoption and the true (objective) surplus they would actually obtain. The core implications of this hypothesis are that (1) the informational environment is influenced by the local prevalence and availability of EVs, and (2) the latent utility from EV purchase is not. This hypothesis indeed explains our findings since reducing informational frictions can induce some otherwise non-purchasing consumers to buy. We use the term *familiarity* to refer to the degree to which perceived surplus aligns with true surplus, so that greater familiarity corresponds to a smaller gap between the two. We refer to this mechanism operating through this alignment as the *familiarity channel*. We articulate this formally in Section 5.1. The empirical implication is that BlueLA's impact on adoption stems from changes in information rather than shifts in preferences. While information and preferences are related—since better information may lead to choices that resemble preference shifts—information does not alter latent preferences but rather leads to consumer purchasing aligning more closely with them.

Toward isolating this channel, we separately consider the impact of BlueLA on hybrid cars (i.e., cars powered by an internal combustion engine and one or more electric motors that do not require chargers) from Battery Electric Vehicles (BEVs) and Plug-in Hybrids (PHEVs). A mechanism based purely on the program increasing demand for cars in general would amount to an amplification across all products, suggesting no variation across these categories. But as all BlueLA cars are plug-in vehicles, we hypothesize that the impact of familiarity should be concentrated primarily on cars sharing this defining distinctive feature.[1] To provide evidence on this mechanism, we analyze the impact of BlueLA on the adoption of conventional hybrids. Since such cars are fuel-efficient but otherwise similar to conventional gas-powered cars, an increase in their adoption caused by BlueLA would suggest that the program instead primarily raised demand for fuel efficiency rather than familiarity with plug-in technology. Overall, we document a negligible impact on conventional hybrids and larger, economically significant impacts on plug-in vehicles, as suggested by the familiarity channel. The fact that our findings are driven by the presence of *plug-in* capabilities rules out explanations where this distinction is irrelevant—such as the program simply increasing subsidy take-up directly.

Other analyses studying heterogeneity in our treatment effect isolate familiarity as a key mechanism. For instance, we connect program usage to the strength of the treatment effect, finding that our documented effect is more heavily concentrated among areas where program usage is higher. This prediction is consistent with the familiarity mechanism, to the extent that usage naturally induces consumers to learn more about EV characteristics, but contrasts with alternative channels under which entry alone proxies for unobserved area characteristics. Along similar lines, we also study the impact of distance by excluding treated zip codes and instead examining zip codes adjacent to treated areas. While most of our estimates lack statistical significance, the pattern suggests diminished but positive effectiveness in zip codes adjacent to those with BlueLA introduction, while also suggesting stronger effects when these adjacent zip codes are excluded. These findings suggest that the effects are concentrated in the immediate vicinity of BlueLA stations, which is consistent with a familiarity-based

[1] We are agnostic about whether the effect should be more concentrated on PHEVs or BEVs. While the program has used BEVs throughout its duration as we discuss in Section 2.1, PHEVs may form an intermediate option for consumers wary of moving away from gas cars entirely (e.g., due to "range anxiety"). Crucially, latent preferences would determine whether BEVs or PHEVs should experience more of an increase once consumers are more aware of EV characteristics. Hence, the strength of the familiarity channel would therefore depend on the significance of these factors.

mechanism operating through local exposure and visibility.[2]

We also study how the overall availability of chargers influences our findings. We mention that charger density was balanced between treatment and control zip codes prior to the entry of BlueLA. Moreover, the program could *not* have directly increased charger availability, since, as we discuss, usage of the chargers at BlueLA stations is restricted to BlueLA cars.[3] These points suggest that the increases in EV purchases we document are unlikely to be driven by charger availability that was itself correlated with the program's rollout. In fact, our causal estimates show a *negative* relationship between charger availability and the strength of our treatment effect. Interpreting pre-existing charger availability as a proxy for pre-existing familiarity within a zip code, this pattern provides evidence for our mechanism since it predicts a larger impact in areas with lower baseline familiarity (i.e., fewer chargers).

Our work joins a literature that has suggested a large role for informational channels to influence green technology adoption. Several studies find that many consumers do not know available subsidies or are mistaken about basic characteristics of EVs (Slowik and Jin, 2017), or appear to misoptimize when evaluating the costs and benefits of EV ownership (Bushnell et al., 2022). Using data on leasing contract renewals from Sweden, Tebbe (2023) finds significant peer effects for EV adoption and provides evidence that they are driven by information transmission. Although the contexts are different, we compare magnitudes explicitly below and find our results are roughly consistent. Heutel and Muehlegger (2015) shows that hybrid vehicle adoption rates were higher in states with initially high Toyota Prius penetration compared to those with high Honda Insight penetration, consistent with a model of consumer learning where exposure to the higher-quality Prius generates positive

[2]As shown in Appendix Figure A6, approximately 75% of members register for the program within the same ZIP code as a BlueLA station. Fewer than 20% reside in adjacent ZIP codes, and only 1% live farther away. Additionally, the number of registered members decreases as the distance from a BlueLA station increases. Finally, the number of users is positively correlated with the direct measure of program usage, namely, the number of trips taken.

[3]We note that past work has found that chargers can independently increase the demand for EVs (Li, 2019; Springel, 2021), and indeed, we find this relationship within our sample, as reported in Online Appendix Figure A2b. However, since BlueLA stations cannot be used to charge other cars, our channel is ultimately distinct.

information diffusion effects while the lower-quality Insight generated negative spillovers. Roberson and Helveston (2020) provides experimental evidence suggestive of our mechanism, showing that the use of EVs (in the form of test drives, arguably similar to what BlueLA provides to communities where present) increases their consideration. Graziano and Gillingham (2015) documents a similar phenomenon with solar panels, namely that their increased visibility in a neighborhood leads to more adoption.

Our broader message is that increasing familiarity with new socially efficient technologies (such as EVs) may be an effective way to spur their adoption. Insofar as EVs are less prevalent in low- to middle-income neighborhoods, these households may also correspondingly be less familiar with EVs. Addressing familiarity could thus be a crucial component of the push to spur EV adoption among these groups. Typically, settings involving new technologies feature greater familiarity with the status quo, potentially impeding adoption. It thus seems natural to conjecture that a familiarity gap is part of why EV adoption rates are lower among low- to middle-income households (despite substantial subsidies and comparable prices to gas counterparts after subsidies[4]). Our work suggests that this observation is particularly relevant to public discourse on EV adoption targets. These conclusions strike us as of paramount importance for policy since EV subsidies may be prohibitively expensive if they alone are used to meet these targets. Muehlegger and Rapson (2022a) surveys numerous findings and documents that even conservatively, the cost of inducing a new EV adoption may be higher than the face value of the subsidy, if not larger than the direct purchase of EVs. Our findings provide evidence that policies aimed at increasing familiarity with EVs (e.g., favoring their purchase in public programs) may reverse this dire conclusion.

The remainder of the paper is organized as follows. Section 2 provides background on the BlueLA program rollout and outlines the data sources. Section 3 details a series of empirical strategies. Section 4 presents the estimated average treatment effects on EV adoption. Section 5 reports tests on the mechanisms and robustness checks, including heterogeneous

[4]EV prices after purchasing subsidies are similar to those of gas counterparts, shown in Online Appendix Table A1.

effects, the impact of chargers, and spillover effects. Section 6 concludes.

2 Background

2.1 The BlueLA Program

BlueLA is a car-sharing program under a public-private partnership aimed particularly at increasing the accessibility of green technologies to income groups traditionally excluded from them—specifically, households in low- to middle-income neighborhoods. There have been three different plans since inception: (1) the standard membership at 5 dollars per month and 20 cents per minute of use; (2) community members (who qualify based on income) at 1 dollar per month and 15 cents per minute of use; and (3) one-month trial members at 40 cents per minute with no upfront fee.[5] Packages are also offered for extended rentals of 3 hours or 5 hours at lower rates. Community members have taken about 54% of all trips and utilized the BlueLA program more intensively, as shown in Online Appendix Figure A6. Additionally, in 2021, the average member took one trip per quarter, indicating that typical users utilize BlueLA occasionally rather than for daily commuting.

BlueLA cars are located at stations on public streets throughout LA; but, as per the program's goals, typically in areas where lower-income residents would use them more easily. Online Appendix Figure A1 shows a typical BlueLA station and the standard design of a BlueLA car. For our purposes, the notable feature is that these cars are highly visible and in easily accessible areas. The program itself is managed privately (by Blink Mobility since September 2020, and Bolloré before that). All cars are highly standardized, following the same designs and type. Bolloré's used its own vehicle (the Bolloré Bluecar) while operating the program, but since January 2021 the program has used exclusively Chevy Bolts. Both car models are BEVs.

[5]For reference, a current Zipcar membership costs 7 dollars per month and 11 dollars per hour, although often in private lots, in contrast to chargers at BlueLA stations, which take street parking spots.

Each BlueLA station usually involves enough chargers for a small number of cars, typically around five. BlueLA stations include designated spots and, importantly, are usable *exclusively* by BlueLA cars. Balancing the goals of finding locations for which car-sharing would be useful, visible, and accessible but also non-disruptive was conveyed to us as a dominant difficulty in our conversations with BlueLA. The process BlueLA follows to determine program location includes both formal criteria and informal factors, described in Ferguson and Holland (2019). Formal criteria include density, proximity to transit (with priority to locations near hubs), employment density (with priority to employment/retail centers), income levels (with priority to high-density areas with affordable/multi-family housing), transit modal shares, and suitability of EVs. However, location selection typically involves community input as well to determine where stations would be most useful. Online Appendix Figure A2a shows the location of zip codes with BlueLA stations at the end of 2021, the total number of accumulated adoptions in each zip code, as well as the locations of the EV Chargers as of December 2021.

The formal and informal criteria suggest that BlueLA does not target areas with observably higher pre-treatment EV adoption or EV-related infrastructure. This is supported by the balance test across zip codes with and without the BlueLA program, presented in Online Appendix Table E4. The results show that lower-income neighborhoods with higher populations tend to have the program, while other factors that could affect EV adoption—such as average EV subsidies received, charger density, and previous EV adoption—do not have a significant impact. Such expectations (insofar as they would be accurate) would suggest an apparent diminished potential need for the program, *inhibiting* entry. But as institutional details can only provide suggestive evidence, we provide more data-based evidence for exogeneity in Section 3.

BlueLA started operations in April 2018 and has grown steadily since. In October 2021, the program operated 60 electric vehicles, 185 charge points, and 37 charging stations, having served 2,671 members and 79,882 trips.[6] The program has continued to grow, with currently over 100 cars, 40 locations, and continuing expansion plans. BlueLA received significant funding from the California Air Resources Board (CARB). The LA Department of Transportation (LADOT) provides infrastructure in addition to some funding. However, Blink purchases and maintains cars and chargers. CARB provided a grant amount equal to $1.7 million, which later grew to approximately $4.6 million after an expansion. In addition, the program had around $24.2 million of private investment to install and operate the program; with some additional public funding, the program's total cost was approximately $31 million.

Advertising around BlueLA often highlights the benefits of driving Electric Cars. As an example, Blink Mobility's blog pays significant attention to the convenience of owning Electric Vehicles; in line with our mechanism, the blog even suggests users view the program as a low-cost step toward deciding whether to purchase an EV—despite also (naturally) suggesting users also consider the program as a primary option *instead* of car ownership (Blink Mobility Blog, 2022).[7] While just one anecdotal example, the advertisement of the advantages of EVs is a common part of the program's promotion, consistent with our proposed mechanism that the program influences familiarity with EVs.

2.2 Data

Our main analyses combine six data sources: (i) data from the Clean Cars 4 All Program (CC4A), (ii) data on EV purchases from the California Energy Commission, (iii) data on subsidy usage through the Clean Vehicle Rebate Project (CVRP), (iv) rollout dates for each BlueLA station, (v) charger availability data, and (vi) BlueLA usage data. Our primary

[6] The total light-duty EV sales in the zip codes with BlueLA stations was 4,953, which suggests that the size of the BlueLA program is significant relative to the EV market.

[7] While potentially suggestive that the program is a substitute, it is worth cautioning that the main subsidy program we study (CC4A) required vehicle trade-in; hence, it seems less likely that usage of BlueLA among these households was indeed substituting for their own ownership. While we are agnostic about whether the program is in fact a substitute, to the extent that it is, our findings would then provide a lower-bound on the impact of the informational externalities alone.

outcome is EV adoption using CC4A data, while we also consider the impact of BlueLA entry on CVRP takeup of low-/middle-income households, and EV adoption overall (not restricted to income).

For our CC4A data, rebate usage is publicly available at the transaction level. This particular subsidy is targeted at individuals from lower income brackets and is available in all neighborhoods targeted by BlueLA. This data includes the subsidy, the vehicle purchased, and the zip code in which the recipient of the subsidy lives. This subsidy required individuals to scrap an operational gas car, which was required to have been continuously registered in California for the prior two years. Thus, take-up of the subsidy reflects a decision to replace ownership rather than substitute toward car-sharing, mitigating concerns that BlueLA usage mechanically crowds out purchases. Individuals were able to buy new cars, or used cars no more than 8 years old with under 75,000 miles, from participating dealerships. Eligibility for the CC4A subsidy is based on income. During the study period, individuals with an income less than or equal to 400% of the Federal Poverty Level were eligible for subsidies[8], with the precise amount varying by income. Subsidy amounts can be larger for individuals in communities labeled as "disadvantaged" according to criteria used by the state of California; but, importantly, these criteria were fixed for almost our entire sample.[9] The average subsidy before the start of the BlueLA programs is \$8,051 for zip codes without BlueLA programs and \$8,243 for zip codes with BlueLA programs. The difference in subsidy amounts is not statistically significant, as shown in Table E4.

We mention that takeup of the subsidy need not capture all adoptions of EVs; aside from the aforementioned requirements, consumers needed pre-approval in order to use the subsidy. Accordingly, an implicit assumption in our analyses using these data is that BlueLA does not differentially affect subsidy use conditional on EV purchase. We view this as plausible

[8]For illustration, the 2023 Federal Poverty Level is \$14,580 for a one-person household and \$19,720 for a two-person household. Individuals can receive up to \$11,500 in subsidies for plug-in hybrid electric vehicles, plus up to \$2,000 for charging equipment or a pre-loaded charge card. For zero-emission vehicles, individuals can receive up to \$12,000, along with up to \$2,000 for charging equipment or a pre-loaded charge card.

[9]Specifically, the map used for this was the same in all quarters except Q4 2021, the last period of our sample.

because BlueLA was not institutionally linked to CC4A.

One important note is that this rebate data covers purchases of any EV, which includes Battery Electric Vehicles, Plug-in Hybrids, and gas hybrids,[10] although subsidy amount does vary depending on which category the car belongs to. In our initial analysis, our outcome variables will not distinguish between conventional hybrids and plug-in vehicles; however, we do so in Section 5.2 and discuss how the results vary across these different vehicle classes.

The CVRP program is a separate subsidy program, functioning as a rebate which could be applied for after car purchase. This program did not include a scrappage requirement, it *did* require the purchase to go toward a new car (so that used cars were ineligible, unlike CC4A). We note that the CVRP rebate did not allow for purchases to go toward a conventional hybrid, in contrast to CC4A. While the income caps for the CVRP program were well above what might be considered moderate income (e.g., \$150,000 for single filers and \$300,000 for joint filers), low-/moderate-income households were eligible for an extra subsidy.[11] While CC4A payments varied with income tiers, CVRP subsidies involved only these two levels (i.e., one standard and one for lower income households).[12] To make the analysis comparable, we restrict our analysis to those using the low-/moderate-income increased rebate.

We obtained the documented opening date for each location from the LA Department of Transportation (LADOT), using this to specify the entry date for each zip code. We aggregate entry location to be at the zip code level since subsidy usage location is at this level as well. This allows us to merge the two data sets using the number of purchases of EVs in a given zip code for each quarter as the unit of observation. We note that whenever BlueLA has "entered" a zip code, it has remained in that zip code throughout our sample. The usage data, provided by CARB, includes anonymized records of individual memberships (start and end dates) since the program's inception, as well as individual trip record for 2021.

[10]The eligibility of gas hybrids was removed as of July 2024, well outside of the window of our study period.

[11]The amount of this boost varied over the study period; while \$2000 from November 2016 through December 2 2019, after this the boost increased while the standard rebate was cut. In addition, the income eligibility for the boost increased from 300% to 400% of the Federal Poverty Level in January 2021.

[12]For new vehicle purchases, these subsides were stackable, so that usage of one did not prevent usage of another.

We supplement this with data on the number of charging stations and average income in each zip code. The former come from the Alternative Fuels Data Center (also used by, e.g., Li (2019)). This provides us with the number of public chargers present in any zip code for each quarter. Table 1 presents the summary statistics regarding the zip codes into which BlueLA has been introduced. Anticipating some of our methodologies to come, we group zip codes into "cohorts" depending on when the first BlueLA station was introduced. As we explain, part of our analysis will estimate treatment effects for each cohort. Aggregated income data in this table comes from the 2016-2020 American Community Survey and are 5-year estimates. Population at zipcode level is from the 2020 U.S. Census. Data on overall EV adoption is publicly available from the State of California.

3 Econometric Approach

We implement four empirical strategies to determine the impact of BlueLA on adoption. First, we estimate the following conventional difference-in-differences specification:

$$NewEVs_{it} = \beta Post_{it} + \gamma_i + \eta_t + \varepsilon_{it}, \tag{1}$$

where $NewEVs_{it}$ denotes the number of new EV adoptions in zip code i at time t, $Post_{it}$ is an indicator variable that denotes the event that a BlueLA station exists in zip code i at time t, and γ_i and η_t are fixed effects. We also estimate Equation (1) with charger covariates. Letting β_{it} denote the number of adoptions at time t in zip code i due to the introduction of BlueLA, then in the above equation,

$$\beta = \sum_{i,t} w_{it}\beta_{it}. \tag{2}$$

Under the assumption that the w_{it} are positive and sum to 1, β represents a weighted average of each β_{it} coefficient. However, some recent work cautions against the positivity assumption necessarily holding (see Roth et al. (2022) and De Chaisemartin and d'Haultfoeuille (2022) for insightful discussions of these issues). We address this issue in Section 4.2.

Our first identification assumption for β is that neighborhoods with BlueLA car-sharing stations would have had trends parallel to other neighborhoods if no BlueLA stations were built. To provide suggestive evidence for this assumption, we follow the standard approach of examining the following event study:

$$NewEVs_{it} = \sum_{k \geq \underline{k}, k \neq -1}^{k=\overline{k}} \delta_k D_{it}^k + \gamma_i + \eta_t + \epsilon_{it}, \tag{3}$$

where the dummy variables D_{it}^k indicate time compared to the event—that is, if BlueLA enters zip code i at time s_i, then $D_{it}^k = 1$ if $t - s_i = k$; we group all periods at or before period $\underline{k}$, as well as those at or after period $\overline{k}$. We omit the dummy for $k = -1$, normalizing it to be 0 (and correspondingly interpreting the other estimates in relative terms). The results from the Two-Way Fixed Effects estimator, with time at the quarter level and taking $\underline{k} = -6, \overline{k} = 9$, are presented in Figure 1c. Coefficients on δ_k do not appear to follow any particular pattern and overall appear relatively flat in this period as well.

We also verify that potentially confounding variables are uncorrelated with BlueLA entry in periods before rollout. Specifically, we check whether the number of public chargers in the previous quarter within a zip code and the number of EV adoption before 2018—the start of the first BlueLA program—predict BlueLA availability or memberships. Online Appendix Table E4 and Table E5 indicate that they do not. There, we also show that BlueLA is less likely to enter higher-income zip codes, consistent with the program's objective. While a neighborhood having lower income does not necessarily mean it has lower income *growth*, the subsidy program data we use restrict eligibility based on income using a uniform income threshold (i.e., a fixed percentage of the Federal Poverty Level) across the areas in our sample. As a result, the population observed in our adoption data is mechanically restricted to a similar income range across all zip codes. Consequently, cross-zip-code differences in average income levels do not mechanically map into differences in the income composition of observed adopters over time, mitigating concerns that BlueLA entry proxies for changes in the incomes of the treated population.[13] Consistent with this interpretation, we also show that there is no statistically significant correlation between pre-existing EV purchases and BlueLA entry. Overall, the findings in Table E4 suggest that the patterns outlined qualitatively in our discussion of institutional background in Section 2.1 also hold quantitatively in our sample.

We additionally require the Stable Unit Treatment Values Assumption (SUTVA), i.e., zip codes without BlueLA stations should not be affected by BlueLA entry. This assumption is more difficult, as usage of BlueLA is not restricted by location. Furthermore, an informational story would predict the impact of a BlueLA station to decay with distance without necessarily respecting zip code boundaries. We suspect this decaying impact would imply an *under*estimate (if anything) as control units benefiting from the intervention would imply a diminished differential impact; still, we seek to resolve doubts that contamination could play a role. We discuss spillovers in Section 5.5 and address the possible concerns with SUTVA there. While some of these results suggest the possibility of small spillovers to adjacent ZIP codes, removing these zip codes leads to the effect becoming (very) marginally strengthened. For now, we mention that very few control-group zip codes are close enough to treated zip codes for spillovers to plausibly cause significant bias. So, this finding, while reassuring, should not be particularly surprising.

One concern with conducting a pre-trend test using an event study framework with Two-Way Fixed Effects is that such tests can be underpowered, implying that passing a pre-trend test at best is uninformative and, at worst, introduces additional bias. Some issues around this are discussed in Roth (2022), which also provides a methodology we follow for testing whether a pre-trend test has sufficient power.

To assess the validity of the parallel trends assumption, we conduct two complementary robustness checks. First, following Roth (2022), we examine the power of the pre-trends test

[13] We emphasize that we do not interpret income targeting alone as per se ruling out differential trends; for this reason, we discuss extensively our analysis which aims to show an absence of pre-trends directly.

and find that a linear violation of slope 0.036 per period would go undetected approximately half the time given the precision of our estimates, suggesting that a visually clean pre-period alone provides limited reassurance. The pre-trends diagnostics further quantify this limitation: the Bayes Factor of 0.65 indicates that passing the pre-test provides negligible updating toward parallel trends given the low power of the test, while the Likelihood Ratio of 0.013 indicates that the observed pre-period coefficients are nevertheless substantially more consistent with parallel trends than with the hypothesized violation, lending some direct support to the identifying assumption beyond the pre-test alone.

Second, to directly address this concern, we implement the sensitivity analysis of Rambachan and Roth (2023), which asks whether the treatment effect remains identifiable under explicit assumptions about the magnitude of potential violations rather than relying on the pre-test to rule them out. Online Appendix E1 reports sensitivity bounds that allow for bounded deviations from parallel trends, both in terms of differences in levels and in the evolution of trends (i.e., changes in slopes over time). Under the relative magnitudes approach (see Figure E1a), the confidence intervals remain entirely above zero for values of M up to approximately 0.6, meaning the positive treatment effect is robust if the post-period violation of parallel trends is as large as the more than half of the worst pre-period deviation observed in the data. Under the smoothness restrictions approach (see Figure E1b), which bounds how much the trend is permitted to change between consecutive periods, the confidence intervals remain above zero across the full range of M values shown up to 0.12—more than three times the minimum detectable slope of 0.036—meaning the result is robust to trend non-linearities well beyond what the pre-test was powered to detect. Taken together, while we cannot rule out moderate violations of parallel trends on the basis of the pre-test alone, the Rambachan and Roth (2023) bounds demonstrate that such violations would need to be implausibly large — substantially exceeding anything observed in the pre-period — to explain away the estimated positive effect of BlueLA on EV adoption.

Our second empirical strategy, which complements the direct estimation of Equation (1), is to implement the Difference-in-Differences (DiD) estimator of Callaway and Sant'Anna (2021). This estimator avoids the issues of conventional two-way fixed effects regressions with heterogeneous treatment effects, in particular, issues related to negative weights. Instead of pooling time-varying treatments and calculating the average effect of BlueLA stations directly, Callaway and Sant'Anna (2021) estimates the treatment effect for each cohort at each time period. Treatment cohorts are defined by the groups of zip codes that share the same starting date as the first BlueLA station. Control units are those zip codes where no BlueLA station ever set up. Since the estimation of the disaggregated treatment effect for each cohort at each time period only involves treatments that occurred at the same time, this reduces the estimation to the canonical DiD setup with two groups and two periods.

To explore heterogeneity and dynamics in the treatment effects, we aggregate the estimated treatment effect for each cohort at each time period via a simple average. Similarly to equation (3), dynamic treatment effects can be estimated by averaging the disaggregated effects over time. Figure 1b presents the results from the event study analysis, taking time to be at the year level; the estimates using quarter-level observations are rather noisy (see Online Appendix Figure F4), making them more difficult to interpret. Still, neither plot reveals any pre-trend. Of note, positive treatment effects are detected roughly two years after the introduction of a BlueLA station. Such delays are consistent with the familiarity channel, as informational externalities should take time to have an effect—for instance, households open to purchasing an EV may not use the program immediately once introduced, as they may not realize the installation of a new station or opting to wait for some need to use the program to arise. These delays are consistent with other forms of information diffusion, although our work is agnostic on the precise details regarding the form this might take.

Lastly, we use two additional empirical strategies. First, we implement a synthetic difference-in-difference estimator using the methodology proposed in Arkhangelsky et al. (2021). Briefly, this method involves reweighting observations so that the trend in the average outcome (i.e., $NewEVs_{i,t}$) for the treated units is roughly parallel for treated and control units. Otherwise, the estimator essentially corresponds to a weighted least-square estimator of the treatment effect using these weights, as described above. As discussed in Arkhangelsky et al. (2021), this methodology is less susceptible to some of the criticism of staggered rollout DID designs, particularly those regarding bias introduced by pre-testing and reliance upon parallel trends. Second, we implement a propensity score matching (PSM) approach based on pre-treatment charger availability to address concerns that treatment assignment may be correlated with differential EV adoption trends. In particular, areas with more existing charging infrastructure may be both more likely to receive the BlueLA program and more likely to experience faster EV adoption. To mitigate this concern, we construct a matched control group of zip codes with a similar likelihood of receiving BlueLA, as predicted by the baseline number of chargers. Re-estimating the treatment effects on this matched sample serves as a robustness check to test if the baseline estimates are driven by pre-existing differences in EV infrastructure or not.

4 Results

4.1 Estimating and Interpreting the BlueLA Impact

We now present our main regression results on the purchases of new EVs via the introduction of the BlueLA program. We present results from the linear panel specification as in (1) in Table 2; since our dataset involves count data with a large number of 0 purchases, we also estimate a Poisson model and present the results in Table 2.[14] This table presents results for both purchases of plug-in vehicles (BEVs or PHEVs), as well as hybrids and overall EV adoptions; we compare the latter two in Section 5.2. This table also includes our findings using the synthetic DID estimator in Table 2. We mention that the estimates from this alternative are close to the estimates from the other estimation methods. In our discussion

[14] Alternative specifications that include alternative fixed effects and charger controls, and Negative Binomial regressions are shown in Online Appendix Table E3, which show very similar results.

of the impact of the BlueLA, we focus on the adoption of plug-in vehicles, which can be found in Panel B.

We find statistically significant increases in adoption once BlueLA enters a zip code. It is important to remember that the coefficients on each variable correspond to the number of new adoptions at the zip code/quarter level. Overall, EV adoption rates are somewhat low over the sample; for instance, the total number of EV adoptions in LA in 2020 using our subsidy is 304. Thus, even an economically meaningful increase in subsidy effectiveness need not correspond to a large absolute change in the counts.

To help benchmark these findings, we interpret the estimated coefficients as ZIP-code-level percentage changes in EV adoption following BlueLA entry.[15] Each coefficient in Table 2 is converted into an implied percentage increase relative to the average annual adoption rate per ZIP code in our sample over the years included in the empirical analysis. Using this common annual baseline, the implied increases are substantial: $26.6\%, 26.1\%, 55.6\%, 32.6\%$ and 28.7% for the linear, Poisson, CSDID, synthetic DID and PSM estimates, respectively. A natural interpretation of these findings is in terms of subsidy effectiveness. A purchase subsidy is more effective when households face fewer barriers to EV adoption. To the extent that BlueLA alleviates "familiarity" as a barrier, our estimates suggest an amplification of the adoption effects of the subsidy. However, we emphasize that we view this as a qualitative interpretation rather than a formal welfare calculation. Quantifying the resulting change in the marginal value of public funds would require additional assumptions about the share of adoptions induced by the interaction between BlueLA and the subsidy. We cannot identify this in our setting, since the subsidy is always available in areas where BlueLA is present.

To benchmark these magnitudes against those from related settings, we compare them to the impacts of other interventions. We emphasize that the goal of the BlueLA program is *not* to subsidize purchases in itself. Whether this program is a more efficient way to spur

[15] While it is mechanically possible to translate these estimates into implied counts of new adoptions, such calculations are sensitive to the time horizon considered, since BlueLA stations enter different ZIP codes at different times and remain in operation thereafter. We therefore focus on percentage changes, which are invariant to these choices.

EV adoption relative to subsidies depends on the value associated with the program as an amenity or the cost of using EVs instead of some alternative; articulating each is beyond the scope of our paper. With this caveat in mind, Muehlegger and Rapson (2022b) estimates that a 10% subsidy on EVs for low- and middle-income individuals corresponds to a 21% increase in new EVs.[16] It is worth noting that Muehlegger and Rapson (2022b) study the pilot phase of California's retire-and-replace subsidy program, which preceded the statewide Clean Cars 4 All rollout and was implemented only in two Air Quality Management Districts. While the institutional settings and scales differ—indeed, the BlueLA program operates at a limited scale within Los Angeles—the ZIP-code-level adoption responses we estimate in Table 2 are of a similar order of magnitude to the demand responses documented in Muehlegger and Rapson (2022b). We view this comparison as contextual benchmarking rather than a policy-equivalence statement, as the relevant interventions identify different parameters in different policy environments. Still, we interpret the similar magnitudes as highlighting the economic significance of BlueLA on subsidy takeup.

Continuing with this theme, our magnitudes appear comparable to those documented in Sweden by Tebbe (2023), which also argues for information transmission as an important mechanism. This paper finds an individual purchasing one new EV results in 0.111 new EV adoptions in their neighborhood; while smaller than our estimates for EV purchases (presented in Panel A of Table 2; 0.238 for the linear model and 0.242 for the synthetic DID estimator, 0.248 for the Poisson model[17]), the magnitudes are still broadly comparable in order, especially given that each BlueLA station can host multiple EVs.

[16] Another relevant comparison is with Springel (2021), which finds that a 10,000 Norwegian Kroner (KR) (US $1,239) per vehicle subsidy increase is associated with a 2.5% increase in EV sales, while a KR10,000 (US$1,239) per station increase in a charging station subsidy is associated with a 14.6% increase.

[17] The Poisson regression identifies the percent change in EV purchases; we convert it to an average increase in annual EV adoption by taking the product of the percentage increase and the average annual EV adoption.

4.2 Addressing Negative Weights

As mentioned in Section 3, recent research has noted that staggered rollout designs can suffer from interpretability issues and bias when the treatment effect is heterogeneous across groups and periods. While these concerns are partially addressed by the similarity of the results when using the synthetic DID estimator, we describe a few other steps we take to ensure the validity of our approach.

First, we follow De Chaisemartin and d'Haultfoeuille (2020) to test whether the negative weight issue occurs. Figure 1d shows the actual weights of each group-period pair in Two-Way Fixed Effect regression (2). Only 4 out of 252 pairs suffer from negative weights; even then, these are very small. This finding suggests the negative weight issue does not invalidate the conclusions reached so far.

Second, we implement the Callaway and Sant'Anna (2021) estimator, described above in Section 3, noting that this estimator is not subject to these particular concerns. This strategy produces treatment effect estimates by cohort, where (i) cohorts are defined by the groups of zip codes that share the same starting date of the first BlueLA station, and (ii) control units are those zip codes where no BlueLA station ever set up. Figure 1a shows the average treatment effect of the BlueLA program by cohort. Our study period covers 2015 to 2021 at the quarter and zip code level. The first BlueLA stations were up and running in April 2018, 13 quarters later. Aside from being less susceptible to bias related to treatment staggering, this figure illustrates more precisely which specific cohorts have had a more significant effect on takeup. Interestingly, we find positive average treatment effects for all but three cohorts (2018Q1, 2018Q2, and 2019Q1), for which point estimates cannot be statistically distinguished from 0.[18] The effect appears stable outside of these cohorts.

To translate these cohort-specific estimates into an aggregated treatment effect comparable to those from our other panel models, we take a weighted average over the treatment effects

[18]We note that, while BlueLA's public launch occurred in April 2018, our rollout data record three station installations in March 2018; these form the 2018Q1 cohort.

of different cohorts estimated via this approach, putting more weight on larger group sizes, as shown in Table 2. The average treatment effect we obtain corresponds to a coefficient of β equal to 0.360—an even *larger* impact, suggesting that the potential sources of bias related to the staggering of treatments lead to an understatement of the effect. This estimate corresponds to a total yearly increase of 125 new EV adoptions, which, under the same reasoning as before, would correspond to a 41.2% increase.

4.3 Adoption Outside of CC4A

We test the generalizability of the above findings by considering two additional datasets on adoption. First, we study takeup of subsidies via the Clean Vehicle Rebate Program. Unlike CC4A, the subsidies considered here do not include a trade-in requirement. The welfare implications absent the trade-in requirement are less clear, as we are not able to determine whether the adoption of an EV is *in place of* an existing car—so, it could very well be that several of these adoptions are in place of public transit. Nevertheless, to the extent that our main mechanisms do not seem to hinge on the nature of the subsidy program, it is not clear why this difference would be significant. Indeed, our findings largely replicate with this alternative dataset: Table 3 reports positive effects across all estimation methods. The estimates are statistically significant at the 5% level for the all specifications but the Poisson regression, while it is significant at the 10% level.

We also consider overall adoption, without restriction based on income. As discussed previously, BlueLA is particularly aimed at serving lower income households, so that one might expect less direct interaction compared to when restricting to the subsidy eligible population. This leads to a natural conjecture that we would find less of an effect overall, and indeed this is the case: Table 3 shows that the evidence for an effect on adoption among the population at large is substantially weaker, with estimated effects being of varying size and only one of the five estimation methods showing a positive impact significant at the 10% level. To the extent that lower-income households are much less represented in this dataset, this finding is favorable evidence that the program itself is responsible for these findings. For instance, one could have conjectured that BlueLA entry proxies for overall charger reliability in a zip code, suggesting increased adoption among the population overall. But this conjecture would have suggested no differential effects based on income, in contrast to our conjecture that the findings are driven by BlueLA itself.

4.4 Placebo Test

Lastly, we assess robustness through a placebo test in which treatment timing is randomly reassigned across ever-treated zip codes, holding the set of treated zip codes fixed. We repeat this procedure 500 times and re-estimate the specifications from Table 2 to characterize the distribution of placebo coefficients. As reported in Table 6, the placebo estimates are statistically indistinguishable from zero across all estimators except TWFE, consistent with the well-documented bias of TWFE under staggered adoption when not-yet-treated units serve as implicit controls (Goodman-Bacon, 2021). Importantly, the placebo coefficients are much smaller than the estimates obtained under the true rollout timing, suggesting that the timing of BlueLA entry is plausibly exogenous and that the estimates reported in Section 4.1 reflect the actual rollout rather than spurious patterns arising from random timing reassignment.

5 Mechanisms

We now provide evidence suggesting a role for an informational channel (i.e., familiarity). We are particularly focused on disentangling informational drivers from those which influence the latent utility of consumption. We view all of these as suggestive, as they are the predictions that would follow from our mechanism—acknowledging the natural limitations of our observational study in that the information level itself is not directly observable.

5.1 Formalizing Familiarity

We start by formalizing the familiarity channel in a simple framework to clarify how improved information can increase EV adoption. Consider the standard formulation of preferences over cars as being determined by their bundle of attributes, so that a consumer's *true* utility in terms of monetary value from a car of type j as a function of its characteristics $x_{j,1}, \ldots, x_{j,n}$ is equal to $\sum_{i=1}^{n} \beta_i x_{j,i}$ for $\beta_1, \ldots, \beta_n \geq 0$, plus some idiosyncratic preference ε_j. We posit that *preferences* (i.e., the β_is) are known by consumers, so that the expected utility from a car of type j is $\sum_{i=1}^{n} \beta_i \mathbb{E}[x_{j,i} \mid \mathcal{I}]$, where $\mathcal{I}$ represents all information available to the consumer. Suppose the consumer has an outside option (e.g., maintaining the current car, public transit, etc.) worth $\underline{u}_0$. In the face of uncertainty, the consumer would form an expectation over the utility, allowing us to say that the consumer purchases car j if:

$$j = \arg\max_{\tilde{j}} -p_{\tilde{j}} + \varepsilon_{\tilde{j}} + \sum_{i=1}^{n} \beta_i \mathbb{E}[x_{\tilde{j},i} \mid \mathcal{I}] \text{ and } -p_j + \varepsilon_j + \sum_{i=1}^{n} \beta_i \mathbb{E}[x_{j,i} \mid \mathcal{I}] \geq \underline{u}_0$$

The crux of the familiarity channel stems from the idea that consumers underappreciate certain characteristics associated with EVs—i.e., that typically, $\mathbb{E}[x_{\tilde{j},i} \mid \mathcal{I}] < x_{\tilde{j},i}$, consistent with survey evidence mentioned above. In particular, suppose the introduction BlueLA were to increase (i) $\underline{u}_0$ and (ii) $\mathbb{E}[x_{j,i} \mid \mathcal{I}]$. Whenever the latter increase is large enough for some car type j, the consumer would purchase this car, either over the outside option or over the alternatives. While such a change *could* crowd out adoption, it could also increase adoption for EVs if the change in information were significant enough relative to the change in $\underline{u}_0$.

Unfortunately, this discussion also illustrates difficulties with separately identifying *changes* in preferences with *changes* in information—indeed, an increase in $\mathbb{E}[x_{j,i} \mid \mathcal{I}]$ has the same impact as changing β_i. In simpler terms, the mere pattern that more EVs are purchased could either be due to a shift in preferences or a shift in information.[19] Along these lines, we cannot

[19] These difficulties are well-understood; see, for instance Lu (2019) on solutions possible in cases where analysts have access to interventions.

rule out that the observed changes reflect shifts in perceived rather than true surplus—for instance, through salience or persuasion—rather than purely improved information. A caveat behind our mechanism is that the program may have also increased adoption by altering the perceived surplus through non-informational channels, such as salience (Chetty et al., 2009) or mitigating behavioral biases (Handel and Schwartzstein, 2018; Allcott and Taubinsky, 2015). While our data do not allow us to disentangle these channels, both mechanisms suggest that exposure facilitates adoption.

Related to this point, what we refer to as familiarity overlaps conceptually with mechanisms related to social norm formation. For instance, exposure to the BlueLA program may lead individuals to update beliefs about the popularity or feasibility of EVs (e.g., upon observing frequent usage). Whether such effects fall within the familiarity channel depends on whether they primarily alter expectations about EV attributes—that is, whether they operate through $\mathbb{E}[x_{j,i} \mid \mathcal{I}]$, possibly via social observation or inference, or instead shift intrinsic preferences (e.g., through ε_i). The former would be consistent with our notion of familiarity, while the latter would not. Again, these mechanisms are difficult to disentangle empirically in practice, as both social exposure and direct experience can reduce perceived uncertainty about EV adoption.

Lastly, it is also worth mentioning that since the CC4A program required a trade-in, we can guarantee that the relevant population is those who had more prior experience with conventional gas cars. As one would expect more prior experience with one's own car, it is natural to conjecture a greater differential in beliefs for such households—particularly relative to other households who might have not purchased a car previously. Along these lines, the implicit assumption that preferences are themselves known seems more palatable for this population.

5.2 Hybrids vs. BEVs/PHEVs

We first consider differences in the treatment effect as we change the outcome variable to reflect different *kinds* of purchases. The rebates under CC4A apply to (1) Battery Electric Vehicles (BEVs), which do not contain a gas engine; (2) Plug-in Hybrid Electric Vehicles (PHEVs), which use chargers while also having a gas engine; and (3) Hybrid Electric Vehicles, which cannot be charged externally (and are gas-powered).

The BlueLA program, by contrast, uses only BEVs. In principle, familiarity could lead to any of these increasing (e.g., if the program were to make consumers understand the size of the environmental benefits of all EVs), but we expect the impact on familiarity to matter more for BEVs/PHEVs. Indeed, hybrids are more analogous to conventional cars and less immediately similar to the cars used by BlueLA. At the same time, if the demand-based variables that determined BlueLA entry (or undetected pretrends) were also correlated with purchases of more fuel-efficient cars, then the effect we document should apply to both of these categories uniformly—if not on hybrids primarily, given the greater similarity to conventional gas cars.[20]

In this sense, distinguishing between the impact of BlueLA on conventional hybrids versus plug-in vehicles (BEVs and PHEVs) provides a "placebo test" to see the extent to which factors unrelated to the program itself were responsible for the findings, as we would expect either no difference or for the effect to be concentrated among conventional hybrids. In the terminology of our model, changes in preferences β_i (e.g., due to shifting neighborhood characteristics) holding information fixed would suggest an increase in adoption for all otherwise-similar cars. To the extent that the information changes *only* impact plug-in vehicles, we would then expect our main results to not replicate for conventional hybrids.

Table 2 presents comparisons of the effect on BEVs and PHEVs versus hybrids.[21] Consistent

[20] We note that charger availability is one factor that would apply to BEVs/PHEVs more than hybrids; we address this in the following section.

[21] For completeness, we also present analysis decomposing our findings *between* BEVs and PHEVs in Online Appendix F.

with the conjecture that familiarity is responsible for the results, we find that the effect is dominated among BEVs and PHEVs and either minor or non-existent among hybrids; point estimates place the estimated effect at roughly half for hybrids compared to EVs, with none of the coefficients being significant, a stark contrast to the regression results for BEVs/PHEVs. We also estimate separate cohort-specific treatment effects using the CSDID estimator with PHEV/BEVs and hybrids as an outcome variable in Online Appendix Figures F1a and F1c, respectively. These figures also reveal that the treatment effects are sizable for plug-in vehicles and significantly smaller for conventional hybrids for most cohorts.[22]

5.3 Program Usage

To further isolate the familiarity mechanism, we examine how the strength of our treatment effect varies with program usage. The natural conjecture is that the more the program is used, the stronger the shifts in local perceptions of EVs.

Using our data on program usage, we divide zip codes into subgroups based on whether the number of users in a given zip code with BlueLA is above or below the median number in our sample. We then re-estimate our main specifications within each subgroup. Across all estimation methods, the treatment effect is consistently larger for the high-usage group—precisely the finding predicted by the familiarity mechanism, as shown in Panel A in Table 4. While this analysis is not immune to concerns about unobserved heterogeneity, these results are nevertheless suggestive, as our mechanism predicts variation in the treatment effect that aligns closely with the empirical results, under the assumption that usage is not systematically correlated with unobserved variables.

[22] The comparison at the quarter level is also provided in Online Appendix Figure F4, although the added noise in these estimates, as with overall EV purchases, makes them more difficult to interpret.

5.4 The Impact of Chargers on the Treatment Effect

We now turn to an analysis of the heterogeneity in our estimates as the number of available chargers varies. This analysis both provides evidence for an informational channel driving the result and also rules out the findings as somehow being driven by program entry correlating with charger entry. Specifically, we take the number of public charging stations as a proxy for familiarity, as chargers may themselves impact the extent to which EVs are prevalent or visible in a given area.[23] In particular, if EVs are already more prevalent, then there should be less scope for familiarity to influence purchases. Insofar as charging stations correlate with EV ownership and EV visibility within a neighborhood, they also should correlate with the local prevalence of EVs and, thus, familiarity.

To see whether these patterns are borne out, we follow a similar procedure as in Section 5.3, dividing zip codes into subgroups based on charger availability. We then repeat our main analysis within each subgroup to see how the magnitude depends on which group is under consideration.[24] Zip codes are labeled as high charger availability if the number of public charging stations is higher than the median. Panel B in Table 4 presents the finding that zip codes with lower charger availability, and thus less information about EVs, are much more responsive to BlueLA (see Online Appendix Table F2 for the analogous table with the purchase of either a BEV or a PHEV as an outcome variable; the same message emerges from this as well). Our point estimates are lower across every estimate in the high-charger group compared to the low-charger group, consistent with the hypothesis that familiarity has a larger impact if pre-existing information is more limited.

[23] We resort to proxies since we lack direct measurements of familiarity. Online Appendix Figure A2b documents that overall the number of public chargers itself does appear to be correlated with an increase in adoption, at least up to a point. This observation suggests that familiarity should be greater in areas with more chargers. Note that our estimates of the average number of EV purchases in a zip code with a given number of public chargers become noisy when the latter becomes large.

[24] We view these patterns as suggestive, rather than a decomposition, as dividing into subgroups also changes the variation in entry timing.

5.5 Spillovers Across Zip Codes

Since BlueLA usage or consumer activity is not restricted by zip code, in principle, the impact of BlueLA entry in one zip code could spill over into others. An informational story would still predict an effect that decays with distance. Still, being in the immediate vicinity may matter more if everyday usage is important but less if occasional usage (e.g., a "test drive") suffices. Here, we study whether there is decay and how much.

We partition the control group into *first-adjacent neighborhoods* (untreated zip codes bordering treated zip codes), *second-adjacent neighborhoods* (untreated, non-first-adjacent zip codes bordering first-adjacent zip codes), and all others. After doing so, we estimate the effect of BlueLA stations on EV adoption in first-adjacent and then second-adjacent neighborhoods.[25]

Table 5 presents the findings from this analysis (the analogous findings for BEVs and PHEVs are presented in Online Appendix Table F3). We repeat our estimation of the TWFE estimator, Poisson model, synthetic DID estimator, and Callaway and Sant'Anna estimator (see Section 3). The left panel of Table 5 shows small but positive spillover estimates in first-adjacent neighborhoods, although these estimates are statistically insignificant. The right panel of Table 5 indicates that the effect of BlueLA stations is weakly negative in second-adjacent neighborhoods, although the estimates are all statistically insignificant.

As discussed in Section 3, control-group contamination is a potential concern despite the small number of observations from first-adjacent neighborhoods. Our findings suggest spillovers are very weak and, if anything, limited to first-adjacent zip codes. Nevertheless, we re-estimate the effect of BlueLA stations in the same zip codes by excluding this potentially contaminated group. Online Appendix Table B1 Panel A shows that, broadly speaking, the estimated effect tends to be slightly larger when first-adjacent neighborhoods are excluded, as predicted by our identification assumptions and the informational mechanism.[26] Thus,

[25]We exclude the treated group in this analysis.

[26]For completeness, Table B1 also reports results separately for BEV/PHEV and Hybrid purchases in Panels B and C, respectively, removing adjacent zip codes as well.

these tests provide weakly suggestive evidence that the program's impact is concentrated closer to BlueLA stations.

6 Conclusion

This paper has documented that zip codes where BlueLA stations were introduced experienced an increase in EV adoption compared to those without them. We documented patterns consistent with informational externalities being responsible for this finding. While we have documented effects across two different subsidy programs targeted at low-to-middle-income households, we acknowledge that these findings concern a particular program in a particular area. The extent to which these results would scale or replicate—particularly in settings with different urban density, baseline EV familiarity, or program visibility—is an interesting area for future work, but one we are unable to answer in the present exercise.

Our findings concern effects of BlueLA unrelated to its primary mandate. Whether a similar program could be viable elsewhere likely would depend on numerous considerations. Some factors that facilitated BlueLA's expansion, like support from state government, might be difficult to replicate. Still, we do expect addressing the information gap facing EVs to have similar impacts more broadly, particularly in light of the related work described above.

This claim has enormous significance for policy. Given the low rates of EV adoption among lower-income households (despite large subsidies), addressing familiarity could distribute the gains from green technology innovations to a group that often misses them—and form an important component of the goal of universal adoption advanced by policymakers. Increasing familiarity may be crucial for subsidies to be effective in the first place.

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Appendix

Cohort	# Zip Codes	Avg. # EV/PHEV	Avg. # EV/PHEV per 1,000	Avg. # Hybrid	Avg. # Hybrid per 1,000	Avg. Income ($)	Avg. # Chargers	Avg. # Chargers per 1,000
2018 Q1	3	462	8.66	1,507	30.04	48,322	7	0.141
2018 Q2	2	291	8.78	814	18.93	40,481	28	1.014
2018 Q3	2	532	10.60	1,505	30.21	44,083	11	0.276
2018 Q4	1	387	10.00	1,339	34.60	49,068	4	0.103
2019 Q1	2	549	15.91	1,653	47.35	37,216	58	1.537
2019 Q4	1	780	12.58	2,243	36.18	56,389	13	0.210
2020 Q2	2	591	13.74	1,528	36.54	58,195	8	0.192
2020 Q3	1	431	13.33	1,108	34.27	45,499	55	1.701
Control	97	605	19.51	1,253	36.36	73106	12	0.413

Table 1: The Rollout of BlueLA: Summary Statistics by Groups of Zip Codes in Jan 2021

Note: A cohort is defined as a group of zip codes that share the same starting date as the first BlueLA stations. We note that, while BlueLA's public launch occurred in April 2018, three station installations in March 2018 for the 2018Q1 cohort. The cohort name indicates the quarter when the first BlueLA stations were set up. Income data is from the 2016-2020 American Community Survey 5-year estimates. Public charger information is from the Department of Energy's Alternative Fuels Data Center and we calculate the number of total public chargers available for each zip code by Jan 2021. The total number of EVs is from the State of California, which provides the universal EV counts by zip codes by Jan 2021. Population denominators for the per 1,000 columns are from the 2020 Decennial Census.

	Panel A: All EVs				
	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)
BlueLA × Entry	0.238	0.248	0.360	0.242	0.251
	(0.108)	(0.138)	(0.163)	(0.112)	(0.117)
Percentage increase	27.2%	28.1%	41.2%	27.7%	28.7%

	Panel B: BEVs+PHEVs				
	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)
BlueLA × Entry	0.155	0.232	0.324	0.190	0.167
	(0.077)	(0.158)	(0.141)	(0.095)	(0.079)
Percentage increase	26.6%	26.1%	55.6%	32.6%	28.7%

	Panel C: Hybrids				
	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)
BlueLA × Entry	0.082	0.306	0.036	0.065	0.079
	(0.052)	(0.216)	(0.115)	(0.061)	(0.052)
Percentage increase	14.1%	35.8%	6.2%	11.2%	13.6%
Observations	3,108	3,108	3,108	3,108	3,108
Year-Quarter FEs	Yes	Yes	Yes	Yes	Yes
zip code FEs	Yes	Yes	Yes	Yes	Yes

Table 2: Main Results: the Influence of BlueLA Stations on Adoption of EVs Using CC4A

Note: This table reports the average effect of BlueLA Stations on the number of incremental EV purchases for all kinds of EVs (including BEVs, PHEVs, and hybrids) in each zip code each quarter, using linear, Poisson panel regressions, and Callaway and Sant'Anna's estimator in Figure 1a, synthetic DiD, and propensity score matching on pre-treatment chargers respectively. Panel B reports the same average effect for the adoption BEVs and PHEVs. Panel C reports the average effect of BlueLA for the adoption of hybrid vehicles. The unit of observation is at year-quarter and zip code levels. *BlueLA* indicates whether there are or there will be BlueLA stations in a zip code. *Entry* indicates whether there are already BlueLA stations in a zip code. Standard errors are two-way cluster standard errors at year-quarter and zip code levels. Estimates excluding adjacent ZIP codes are presented in the Appendix B1.

	Panel A: Broader Subsidy Programs				
	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)
BlueLA × Entry	2.274	0.223	2.673	1.777	2.968
	(1.114)	(0.124)	(0.896)	(0.858)	(1.146)
Percentage increase	24.3%	29.0%	28.5%	19.0%	31.7%

	Panel B: Sales Outside Broader Subsidy Programs				
	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)
BlueLA × Entry	−3.077	−0.016	−2.964	−1.435	−4.108
	(3.462)	(0.075)	(2.223)	(2.014)	(3.629)
Percentage increase	−11.5%	−1.6%	−11.1%	−5.4%	−15.4%

	Panel C: Overall Sales				
	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)
BlueLA × Entry	−0.796	0.113	−0.126	1.047	−1.121
	(2.962)	(0.056)	(2.199)	(2.150)	(3.049)
Percentage increase	−2.2%	12.0%	−0.3%	2.9%	-3.1%
Observations	3,108	3,108	3,108	3,108	3,108
Year-Quarter FEs	Yes	Yes	Yes	Yes	Yes
zip code FEs	Yes	Yes	Yes	Yes	Yes

Table 3: The Influence of BlueLA Stations on BEV and PHEV Adoption Through Broader Subsidy Programs and Overall Sales

Note: Panel A includes all EV sales under the broader subsidy programs that target disadvantaged communities funded by the Greenhouse Gas Reduction Fund. Data source: https://cleanvehiclerebate.org/en/rebate-map. Panel B includes all purchases that are outside the broader subsidy programs. Panel C includes the overall sales that encompass both subsidy-assisted purchases and those made without program support. Estimates excluding adjacent ZIP codes are presented in the Appendix Table D1 for sales using broader subsidy programs, outside the broader subsidy programs, and the overall sales respectively.

	Panel A: By Membership Enrollment									
	< Median					≥ Median				
	Linear	Poisson	CSDID	SDID	PSM	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BlueLA × Entry	0.148	0.234	0.230	0.123	0.141	0.222	0.461	0.406	0.287	0.202
	(0.111)	(0.218)	(0.087)	(0.195)	(0.110)	(0.113)	(0.285)	(0.139)	(0.118)	(0.115)
Observations	2,296	2,296	2,296	2,296	2,296	2,296	2,296	2,296	2,296	2,296

	Panel B: By Charger Availability									
	< Median					≥ Median				
	Linear	Poisson	CSDID	SDID	PSM	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BlueLA × Entry	0.300	0.458	0.340	0.352	0.291	0.055	0.159	0.285	−0.051	0.045
	(0.103)	(0.258)	(0.240)	(0.133)	(0.098)	(0.121)	(0.250)	(0.085)	(0.139)	(0.119)
Observations	1,288	1,288	1,288	1,288	1,288	1,204	1,204	1,204	1,204	1,204
Year-Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
zip code FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4: The Influence of BlueLA Stations on Adoption of EVs (BEVs and PHEVs): Heterogeneity

Note: This table reports the effect of BlueLA stations on the EV adoption in the same zip code by membership enrollment and charger availability for BEVs and PHEVs. Note that zip codes adjacent to the treated zip codes are excluded to deal with the potential spillover effect. Zip codes are categorized as below or above the median of the membership enrollment based on the total number of memberships accumulated in 2021. Control zip codes remain those that have never participated in the BlueLA program. Low/high charger availability is defined as below/above the median of the available chargers in LA. *BlueLA* indicates whether there are or will be BlueLA stations in the zip code. *Entry* indicates whether there are already BlueLA stations in the zip code. Standard errors are two-way cluster standard errors at year-quarter and zip code levels. We replicate this table using sales in broader subsidy programs in the Appendix Table D2.

	Number of New EV Adoption									
	In First-Adjacent Zip Codes					In Second-Adjacent Zip Codes				
	Linear	Poisson	CSDID	SDID	PSM	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Adjacent BlueLA $\times$ Entry	0.080	0.231	0.019	0.046	0.081	-0.018	-0.151	-0.300	-0.075	-0.064
	(0.067)	(0.175)	(0.094)	(0.087)	(0.076)	(0.070)	(0.238)	(0.156)	(0.102)	(0.099)
Observations	2,716	2,716	2,716	2,716	2,716	2,100	2,100	2,100	2,100	2,100
Year-Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
zip code FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

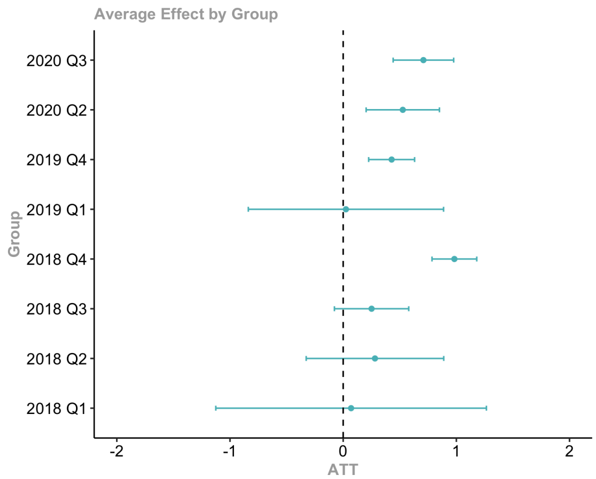
Table 5: The Influence of BlueLA Stations on Adoption of EVs (BEVs and PHEVs): Spillover

Note: This table reports the effect of BlueLA stations in an adjacent zip code (and the adjacent zip code's neighbor) on the number of incremental EV purchases for BEVs and PHEVs in zip codes where there is no any BlueLA station. Zip codes (14 out of 111) with at least one BlueLA station during our study period are excluded from the sample. Of the remaining zip codes, 24 of them are adjacent to zip codes with at least one BlueLA station. *Adjacent BlueLA* indicates whether there are or there will be BlueLA stations in an adjacent zip code. *Entry* indicates whether there are already BlueLA stations in an adjacent zip code. Standard errors are two-way cluster standard errors at year-quarter and zip code levels.

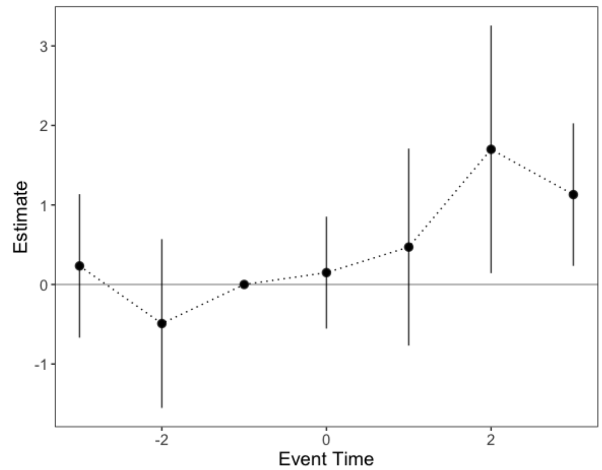
	Number of New EV Adoption				
	Panel A: BEV+PHEV				
	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)
bluela × post	0.102	0.185	0.081	0.090	0.086
	(0.045)	(0.109)	(0.178)	(0.070)	(0.083)
	Panel B: Hybrid				
	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)
bluela × post	0.043	0.179	0.103	0.021	0.076
	(0.028)	(0.142)	(0.107)	(0.031)	(0.055)
Year-Quarter	Yes	Yes	Yes	Yes	Yes
Zipcode	Yes	Yes	Yes	Yes	Yes
Observations	3,108	3,108	3,108	3,108	3,108

Table 6: Placebo: Scrambled Treatment Effects of BlueLA

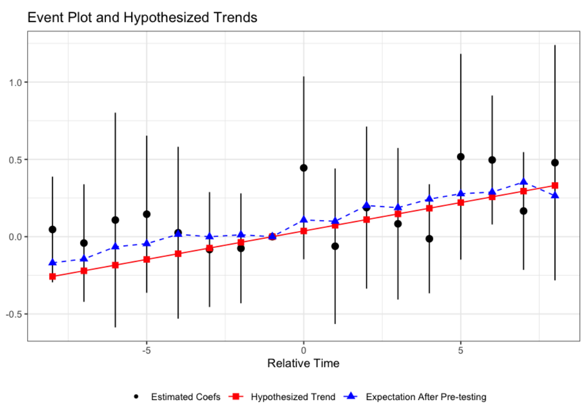
Note: This table reports the average treatment effect of the scrambled treatment and the 95% confidence intervals based on the 500 iterations using the specifications in Table 2. Treatment timing is scrambled for 500 times for Zipcodes where BlueLA stations entered during the study period. Observations only include purchases within the subsidy program at the zipcode-quarter level.



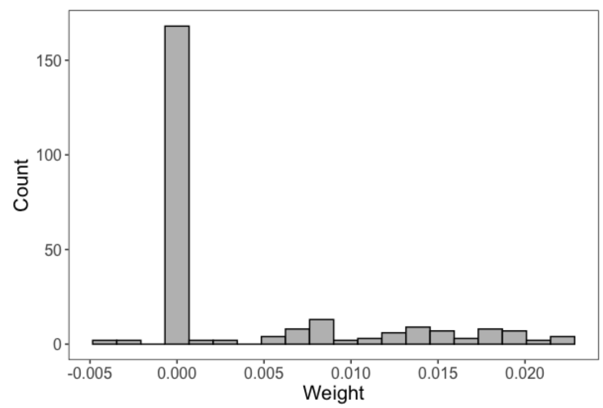
(a) Treatment Effect by Cohort



(b) Treatment Effect by Year



(c) Parallel Trend Test Using Roth (2022)



(d) Histogram of Two-Way Fixed Effect Weights

Figure 1: Treatment Effects of BlueLA on EV Adoption for EVs

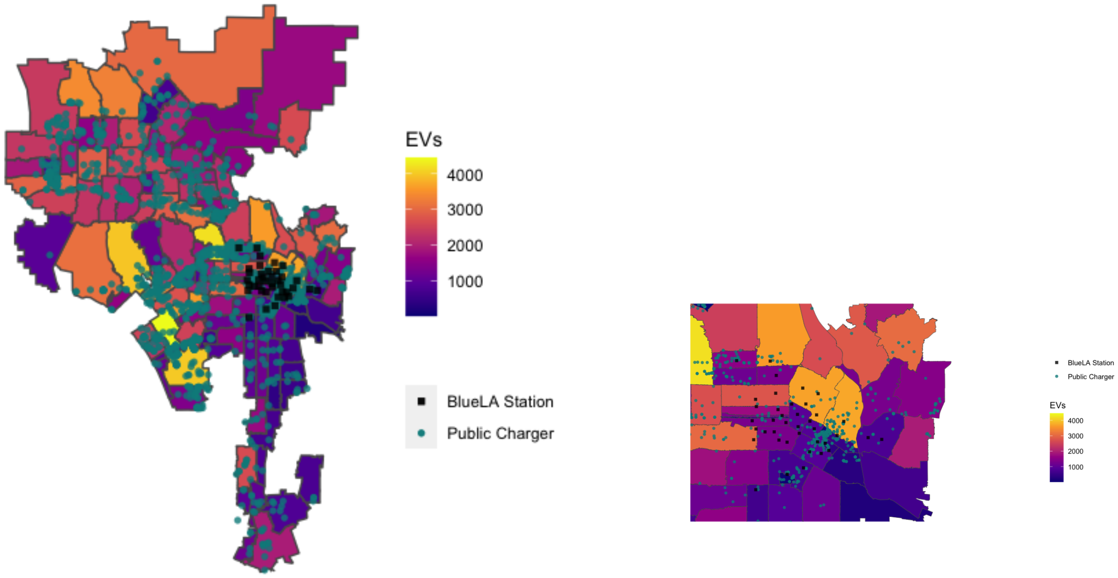
Note: Figure (a) and (b) show the treatment effect of BlueLA on EV adoption for all types of EVs (BEVs/PHEVs) by cohort and by year respectively, using dynamic effects from Callaway and Sant'Anna (2021). Note that the year before the treatment year is set as the baseline, as suggested in Roth (2024). Figure (c) shows the parallel trend test proposed in Roth (2022) under the power of 0.5. Black dots are estimated coefficients of conventional TWFE. Red squares are the minimum linear trend detectable with a power of 0.5. The minimum slope of a detectable pre-trend is 0.0367. Blue triangles are coefficient expectations conditional on passing. The reported Bayes Factor and Likelihood Ratio are 0.648 and 0.013 respectively. Figure (d) displays the histogram of TWFE weights proposed in Goodman-Bacon (2021).

Online Appendix for “Electric Vehicle Sharing: Crowding Adoption Out or In?”

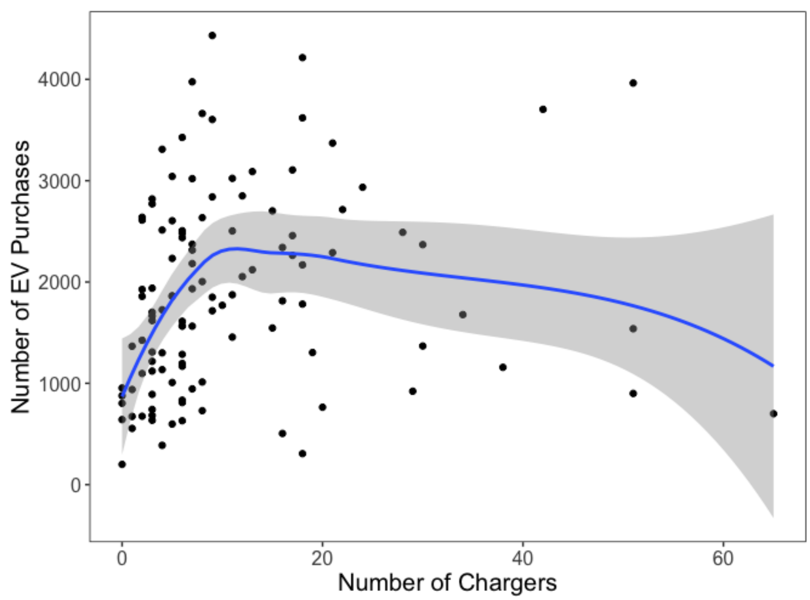
Jonathan Libgober and Ruozi Song

A BlueLA Car Sharing Program

Figure A1: A Representative BlueLA Charging Station



(a) EV Chargers, BlueLA Stations and EV Takeups up to Dec. 2021



(b) Relationship between Public EV Chargers and Takeups in Los Angeles by Zip Code as of Jan. 2021

Figure A2: EV Adoptions, BlueLA Stations, and Chargers in LA

Note: Panel (a) reports the EV takeups, locations of BlueLA stations and public chargers in LA in 2021. EV takeups is from California Department of Motor Vehicles. The locations of BlueLA stations are from BlueLA's Report to City of Los Angeles. Charger information is from the Department of Energy's Alternative Fuels Data Center. Panel (b) shows the relationship between the availability of public chargers and EV takeups.

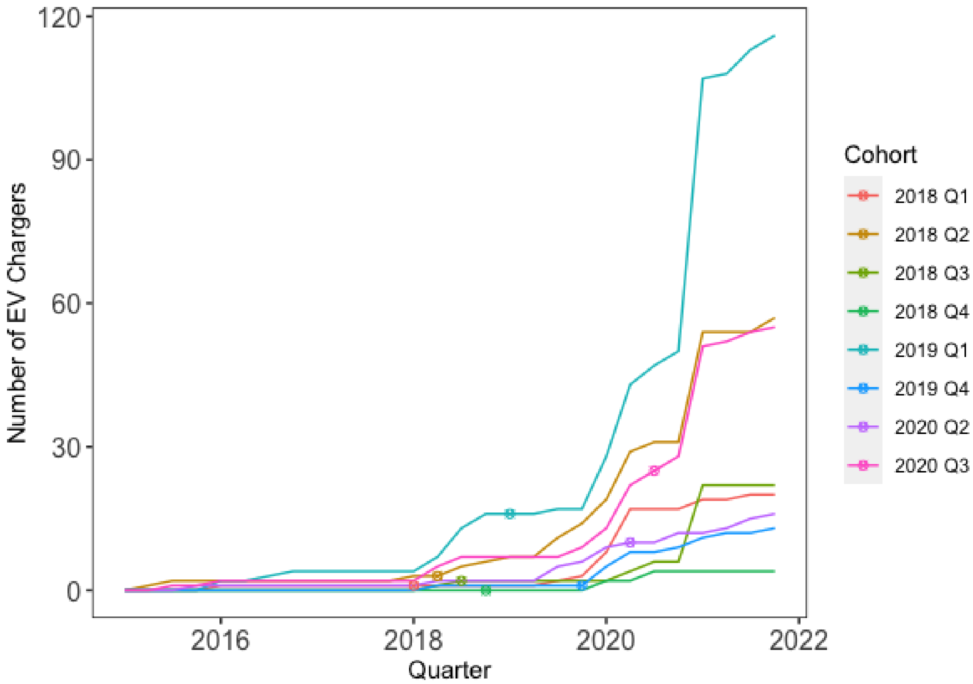
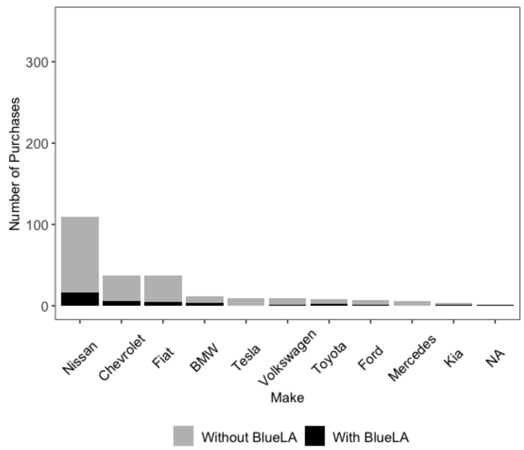
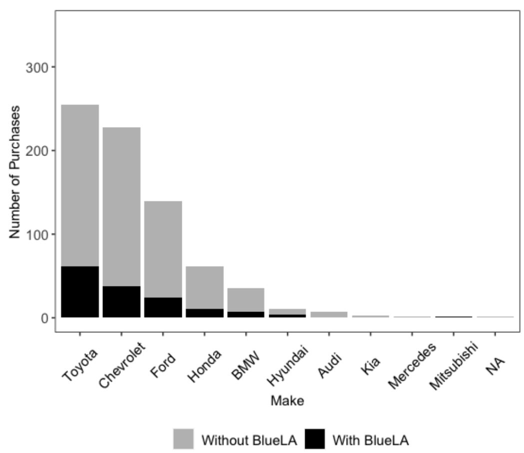


Figure A3: Number of EV Chargers by Cohort over Time

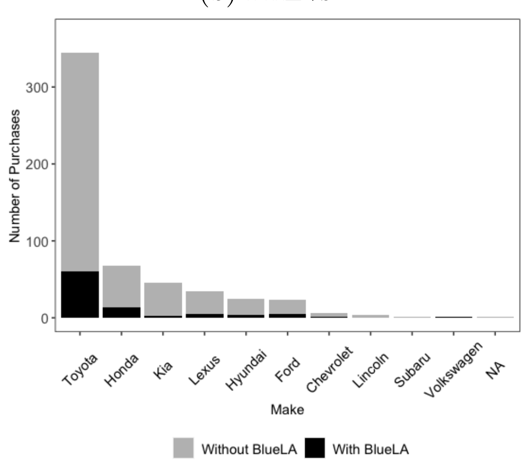
Note: This figure shows the number of public chargers available over time for each cohort (i.e., zip codes with the same initial entry date). Squares indicate when the first BlueLA station was established. There was no outstanding increases in the number of chargers around the entry of BlueLA stations.



(a) BEVs



(b) PHEVs



(c) Hybrids

Figure A4: Top Makes of EV Adoptions in CC4A by EV Type

Note: This figure reports the top 10 popular makes of EVs by EV type, which were purchased using the subsidies provided by the CC4A program from 2015 to 2021.

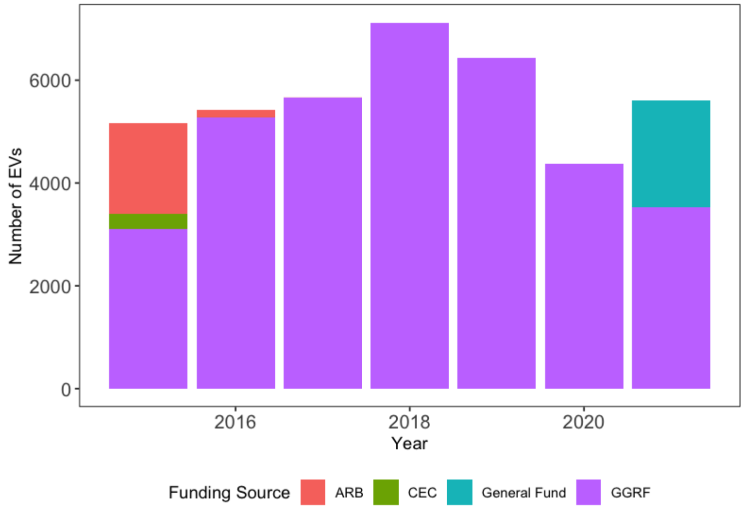
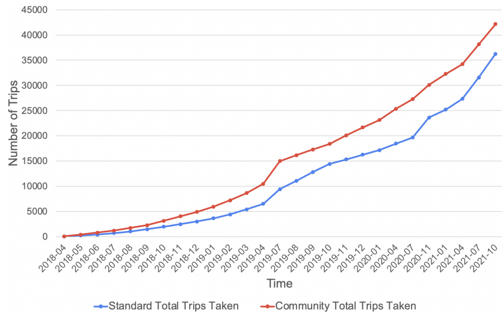
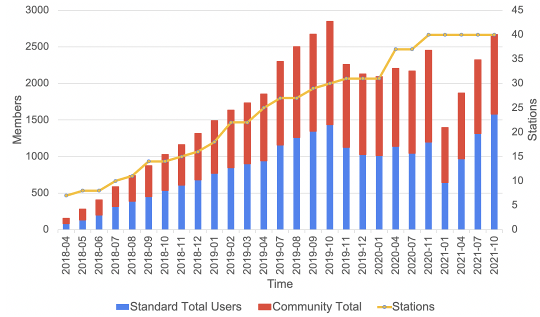


Figure A5: EV Adoptions by Funding Source

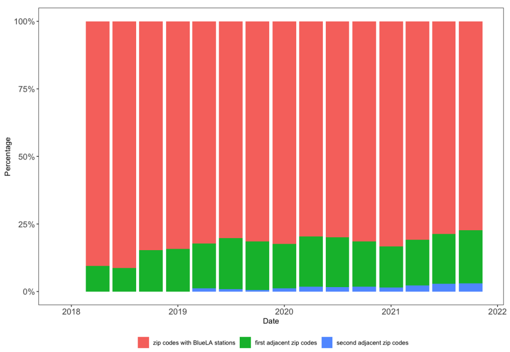
Note: The figure reports the EV adoption trend by funding source.



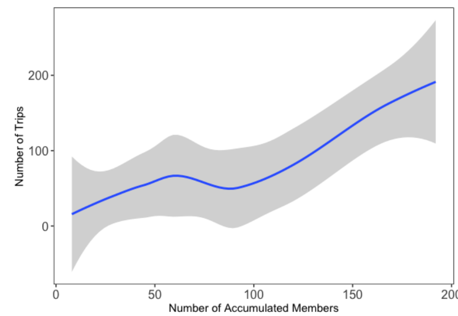
(a) Number of Trips



(b) Number of Users and Stations



(c) Number of Users by Zones



(d) Number of Users and Trips

Figure A6: Utilization of BlueLA by Membership Type

Note: Data source is Los Angeles Department of Transportation, which managed the BlueLA program. Community members are qualified based on income and have cheaper access to the BlueLA program (more details in Section 2.1).

Make	Model	Tech	Avg. Price	Incentive	Price after Incentive	Gas Counterpart Price
Toyota	RAV4	Hybrid	\$28,500	\$7,000	\$21,500	\$26,150
Toyota	Camry	Hybrid	\$27,270	\$7,000	\$20,270	\$24,970
Toyota	Corolla	Hybrid	\$23,600	\$7,000	\$16,600	\$20,025
Toyota	RAV4 Prime	PHEV	\$38,100	\$9,152	\$28,948	\$26,150
Ford*	Fusion	Hybrid	\$22,265	\$6,250	\$16,015	\$20,517
Ford	Escape	PHEV	\$34,320	\$9,152	\$25,168	\$26,130
Honda	CR-V	Hybrid	\$30,560	\$7,000	\$23,560	\$25,350
Kia	Sportage	Hybrid	\$27,490	\$7,000	\$20,490	\$23,990
Hyundai*	Tucson	PHEV	\$30,235	\$9,500	\$20,735	\$26,135

Table A1: Popular EV and Gas Counterpart Price Comparison

Note: To get a sense of how expensive EVs are after incentives compared to their gas counterparts, this table compares the MSRPs of popular EV models with the non-EV versions of the same models in 2021. The EV subsidies used to calculate the EV prices after incentives are the average incentives in zip codes with BlueLA stations in 2021 by technology.

* The latest Ford Fusion is the 2020 version, so the MSRP and the incentive in the table reflects the 2020 version. The earliest PHEV Hyundai Tucson is the 2022 version, so the MSRP and the incentive in the table reflects the 2022 version.

B Results Excluding Adjacent Zip Codes

	Panel A: All EVs				
	Excluding Adjacent Zip Codes				
	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)
BlueLA × Entry	0.250	0.287	0.371	0.243	0.242
	(0.109)	(0.128)	(0.166)	(0.129)	(0.112)
Percentage increase	28.6%	33.2%	42.5%	27.8%	27.7%

	Panel B: BEVs+PHEVs				
	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)
BlueLA × Entry	0.181	0.315	0.318	0.224	0.164
	(0.080)	(0.168)	(0.150)	(0.098)	(0.081)
Percentage increase	31.1%	37.0%	54.6%	38.5%	28.2%

	Panel C: Hybrids				
	Linear	Poisson	CSDID	SDID	PSM
	(1)	(2)	(3)	(4)	(5)
BlueLA × Entry	0.069	0.236	0.053	0.041	0.102
	(0.051)	(0.216)	(0.117)	(0.060)	(0.057)
Percentage increase	11.9%	26.6%	9.1%	7.0%	17.5%
Observations	2,492	2,492	2,492	2,492	2,492
Year-Quarter FEs	Yes	Yes	Yes	Yes	Yes
zip code FEs	Yes	Yes	Yes	Yes	Yes

Table B1: Main Results: the Influence of BlueLA Stations on Adoption of EVs Using CC4A (Excluding Adjacent Zip Codes)

Note: The top panel reports the average effect of BlueLA Stations on the number of incremental EV purchases for all kinds of EVs (including BEVs, PHEVs, and hybrids) in each zip code each quarter, using linear, Poisson panel regressions, and Callaway and Sant'Anna's estimator in Figure 1a, synthetic DiD, propensity score matching on pre-treatment chargers respectively. Panel B reports the same average effect for the adoption BEVs and PHEVs. Panel C reports the average effect of BlueLA for the adoption of hybrid vehicles. The unit of observation is at year-quarter and zip code levels. *BlueLA* indicates whether there are or there will be BlueLA stations in a zip code. *Entry* indicates whether there are already BlueLA stations in a zip code. Standard errors are two-way cluster standard errors at year-quarter and zip code levels.

C Alternative Treatment Definition

	New EV Adoption			
	Panel A: BEVs+PHEVs			
	Linear	Poisson	CSDID (<=2 stations)	CSDID (>2 stations)
	(1)	(2)	(3)	(4)
# of Stations	0.062	0.136	0.287	0.323
	(0.034)	(0.079)	(0.062)	(0.142)
	Panel B: Hybrids			
	Linear	Poisson	CSDID (<=2 stations)	CSDID (>2 stations)
	(1)	(2)	(3)	(4)
# of Stations	0.051	0.246	0.199	-0.179
	(0.020)	(0.060)	(0.078)	(0.229)
Observations	3,108	3,108	3,108	3,108
Year-Quarter FEs	Yes	Yes	Yes	Yes
zip code FEs	Yes	Yes	Yes	Yes

Table C1: The Influence of Accumulated BlueLA Stations on Adoption of EVs Using CC4A

Note: This table reports the average effect of the increasing number of BlueLA stations on the number of incremental EV purchases for EVs (including BEVs and PHEVs) in each zip code each quarter, using linear, Poisson panel regressions, and DID estimator with a continuous treatment in Callaway et al. (2024) respectively. The unit of observation is at year-quarter and zip code levels. Standard errors are two-way cluster standard errors at year-quarter and zip code levels.

D Results Using Alternative Sales Data

	Linear	Poisson	CSDID
	Panel A: Broader Subsidy Programs		
	(1)	(2)	(3)
BlueLA × Entry	2.364	0.222	2.946
	(1.177)	(0.126)	(0.965)
	Panel B: Sales Outside Broader Subsidy Programs		
	Linear	Poisson	CSDID
	(1)	(2)	(3)
BlueLA × Entry	-4.280	-0.016	-3.705
	(3.690)	(0.076)	(2.392)
	Panel C: Overall Sales		
	Linear	Poisson	CSDID
	(1)	(2)	(3)
BlueLA × Entry	-1.956	0.117	-0.568
	(3.165)	(0.056)	(2.281)
Year-Quarter FEs	Yes	Yes	Yes
Zip code FEs	Yes	Yes	Yes
Observations	2,492	2,492	2,492

Table D1: The Influence of BlueLA Stations on Adoption of EVs Through Broader Subsidy Programs and Overall Sales (excluding adjacent zipcodes)

Note: EV sales includes all BEV and PHEV purchases under the broader subsidy programs that target disadvantaged communities funded by the Greenhouse Gas Reduction Fund (Data source: https://cleanvehiclerebate.org/en/rebate-map), outside the broader subsidy programs, and the overall sales respectively.

	Panel A: By Membership Enrollment					
	< Median			>= Median		
	Linear	Poisson	CSDID	Linear	Poisson	CSDID
	(1)	(2)	(3)	(4)	(5)	(6)
BlueLA × Entry	-4.591	-0.082	-3.470	-4.288	0.083	-3.940
	(5.451)	(0.103)	(3.931)	(3.883)	(0.041)	(1.925)
Observations	2,296	2,296	2,296	2,296	2,296	2,296
	Panel B: By Charger Availability					
	< Median			>= Median		
	Linear	Poisson	CSDID	Linear	Poisson	CSDID
	(1)	(2)	(3)	(4)	(5)	(6)
BlueLA × Entry	-0.775	0.135	-0.725	-8.407	-0.076	-6.202
	(4.378)	(0.063)	(2.140)	(5.305)	(0.096)	(4.324)
Observations	1,288	1,288	1,288	1,204	1,204	1,204
Year-Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
Zip code FEs	Yes	Yes	Yes	Yes	Yes	Yes

Table D2: The Influence of BlueLA Stations on Adoption of EVs Outside the Subsidy Program (BEVs+PHEVs): Heterogeneity

Note: EV sales only includes purchases outside the subsidy program. First-adjacent zipcodes are excluded.

E Additional Robustness Checks

	EV Adoption					
	OLS					
	Without income trend			With income trend		
	BEV	PHEV	hybrid	BEV	PHEV	hybrid
	(1)	(2)	(3)	(4)	(5)	(6)
BlueLA × Entry	0.071	0.085	0.082	0.079	0.037	0.071
	(0.016)	(0.085)	(0.052)	(0.017)	(0.084)	(0.050)
income × t				0.002	−0.013	−0.003
				(0.002)	(0.006)	(0.004)
	Poisson					
	Without income trend			With income trend		
	BEV	PHEV	hybrid	BEV	PHEV	hybrid
	(1)	(2)	(3)	(4)	(5)	(6)
BlueLA × Entry	0.925	0.003	0.303	1.002	-0.056	0.334
	(0.341)	(0.181)	(0.215)	(0.352)	(0.209)	(0.216)
income × t				0.061	−0.039	0.017
				(0.056)	(0.033)	(0.031)
	CSDID Aggregated Over Cohort					
	Without income trend			With income trend		
	BEV	PHEV	hybrid	BEV	PHEV	hybrid
	(1)	(2)	(3)	(4)	(5)	(6)
BlueLA × Entry	0.129	0.173	0.036	0.144	0.219	0.100
	(0.023)	(0.135)	(0.114)	(0.030)	(0.134)	(0.148)
	CSDID Aggregated Over Year					
	Without income trend			With income trend		
	BEV	PHEV	hybrid	BEV	PHEV	hybrid
	(1)	(2)	(3)	(4)	(5)	(6)
BlueLA × Entry	0.120	0.139	-0.011	0.135	0.163	0.022
	(0.033)	(0.174)	(0.167)	(0.043)	(0.141)	(0.176)
Year-Quarter	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,108	3,108	3,108	3,108	3,108	3,108

Table E1: The Influence of BlueLA Stations on EV Adoption (with income trends over time)

Note: EV sales only includes purchases within the subsidy program. Income is measured in units of $100,000 USD.

	EV Adoption per 1,000 Households			
	OLS			
	BEV	PHEV	Hybrid	Conventional
	(1)	(2)	(3)	(4)
BlueLA × Entry	0.002	0.002	0.002	0.0005
	(0.0003)	(0.002)	(0.001)	(0.0004)
	Poisson			
	BEV	PHEV	Hybrid	Conventional
	(5)	(6)	(7)	(8)
BlueLA × Entry	0.996	0.045	0.274	-0.066
	(0.322)	(0.159)	(0.230)	(1.17)
	CSDID			
	BEV	PHEV	Hybrid	Conventional
	(9)	(10)	(11)	(12)
BlueLA × Entry	0.0029	0.0038	0.0003	0.0001
	(0.0006)	(0.0032)	(0.0032)	(0.0002)
	PSM on Number of Chargers			
	BEV	PHEV	Hybrid	Conventional
	(9)	(10)	(11)	(12)
BlueLA × Entry	0.003	0.001	0.001	−0.001
	(0.0006)	(0.0032)	(0.0032)	(0.0002)
Year-Quarter	Yes	Yes	Yes	Yes
Zipcode	Yes	Yes	Yes	Yes
Observations	3,108	3,108	3,108	3,108

Table E2: The Influence of BlueLA Stations on Adoption per 1,000 Households

Note: EV sales only includes purchases within the subsidy program.

	Panel A: Effect on New EV Adoption: All EVs											
	Linear				Negative Binomial				Poisson			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
BlueLA × Entry	0.407	0.403	0.238	0.239	0.628	0.612	0.265	0.259	0.610	0.589	0.264	0.258
	(0.121)	(0.119)	(0.105)	(0.107)	(0.151)	(0.147)	(0.125)	(0.126)	(0.146)	(0.142)	(0.125)	(0.126)
# Chargers	−0.001	0.010	0.003	0.004	−0.002	0.016	0.006	−0.0002	−0.001	0.017	0.006	−0.0004
	(0.007)	(0.014)	(0.002)	(0.006)	(0.016)	(0.027)	(0.006)	(0.016)	(0.015)	(0.025)	(0.006)	(0.016)
# Chargers^2		−0.0003		−0.00002		−0.0004		0.0001		−0.001		0.0001
		(0.0002)		(0.0001)		(0.0003)		(0.0002)		(0.0003)		(0.0002)

	Panel B: Effect on New EV Adoption: BEVs+PHEVs											
	Linear				Negative Binomial				Poisson			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
BlueLA × Entry	0.290	0.287	0.178	0.181	0.695	0.674	0.308	0.306	0.671	0.644	0.306	0.303
	(0.089)	(0.089)	(0.080)	(0.080)	(0.173)	(0.171)	(0.166)	(0.165)	(0.171)	(0.168)	(0.167)	(0.165)
# Chargers	−0.001	0.007	0.001	0.003	−0.004	0.019	0.003	0.0002	−0.003	0.020	0.003	−0.001
	(0.004)	(0.010)	(0.002)	(0.006)	(0.014)	(0.029)	(0.007)	(0.023)	(0.014)	(0.027)	(0.007)	(0.023)
# Chargers^2		−0.0002		−0.0001		−0.0006		0.0001		−0.0006		0.0001
		(0.0002)		(0.0001)		(0.0004)		(0.0004)		(0.0004)		(0.0004)

	Panel C: Effect on New EV Adoption: Hybrids											
	Linear				Negative Binomial				Poisson			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
BlueLA × Entry	0.117	0.116	0.060	0.058	0.507	0.497	0.194	0.208	0.502	0.492	0.194	0.180
	(0.056)	(0.055)	(0.050)	(0.051)	(0.239)	(0.236)	(0.215)	(0.221)	(0.237)	(0.234)	(0.214)	(0.219)
# Chargers	0.0005	0.003	0.002	0.0005	0.0030	0.0120	0.012	−0.006	0.003	0.012	0.012	−0.001
	(0.003)	(0.006)	(0.002)	(0.001)	(0.019)	(0.029)	(0.010)	(0.021)	(0.019)	(0.028)	(0.010)	(0.017)
# Chargers^2		−0.0001		0.00003		−0.0002		0.0004		−0.0002		0.0002
		(0.0001)		(0.0001)		(0.0002)		(0.0001)		(0.0002)		(0.0001)
Year-Quarter FEs			Yes	Yes			Yes	Yes			Yes	Yes
Zip code FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,108	3,108	3,108	3,108	3,108	3,108	3,108	3,108	3,108	3,108	3,108	3,108

Table E3: Robustness Check: Alternative Specifications of the Effect of BlueLA on EV Adoption

Note: This table reports the average effect of BlueLA Stations on the number of incremental EV purchases in each zip code each quarter, using alternative linear, Negative Binomial regressions, and Poisson panel regressions respectively. Note that zip codes adjacent to the treated zip codes are excluded to deal with the potential spillover effect. The unit of observation is at year-quarter and zip code levels. BlueLA indicates whether there are or there will be BlueLA stations in a zip code. Entry indicates whether there are already BlueLA stations in a zip code. The number of public chargers available is included as a control. Standard errors are clustered at year-quarter and zip code levels.

	Mean (without BlueLA)	Mean (with BlueLA)	Difference	P-value
Subsidies ($1,000)	8.051	8.243	0.192	0.374
Number of chargers 000 people	0.013	0.015	0.002	0.331
Number of EV adoption	0.327	0.390	0.063	0.255
Household income ($1,000)	73.794	46.848	-26.946	0.000
Population (1,000)	38.848	43.605	4.758	0.002

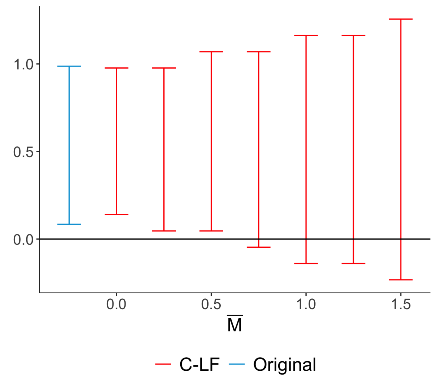
Table E4: Robustness Check: Testing Selection of BlueLA Stations

Note: This table tests whether zip code characteristics differ significantly between zip codes with and without BlueLA programs before the introduction of the first BlueLA program in 2018. P-values test the hypothesis that the mean values between zip codes with and without BlueLA programs are the same, using a two-sample t-test. Income refers to the 5-year estimated income from the 2016-2020 American Community Survey for each zip code. Population data is from the 2020 U.S. Census. The number of EV adoptions represents the quarterly new EV adoption at the zip code level.

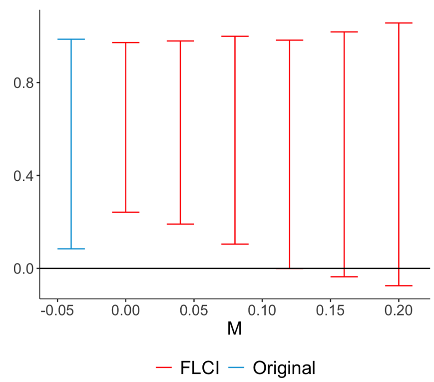
	Dependent variable: Accumulated Number of Members					
	OLS			Poisson		
	(1)	(2)	(3)	(4)	(5)	(6)
$\#\text{Chargers}_{-1}$	-0.071	0.020	0.019	0.012	0.048	0.048
	(0.088)	(0.088)	(0.088)	(0.027)	(0.024)	(0.025)
$\#\text{Chargers}^2_{-1}$	0.009	0.007	0.007	0.000	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.000)	(0.000)	(0.000)
Income($1k)		-0.055	-0.057		-0.044	-0.044
		(0.007)	(0.007)		(0.008)	(0.007)
# EVs before 2018			-0.213			0.006
			(0.159)			(0.211)
Year-Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,997	2,997	2,997	1,665	1,665	1,665

Table E5: Robustness Check: Testing Selection of BlueLA Membership

Note: This table checks whether the number of accumulated members were selected in each zip code each quarter, using OLS and Poisson regressions respectively. The unit of observation is at year-quarter and zip code levels. # Chargers_{-1} is the number of public chargers available in the previous quarter. Income is the 5-year estimated income from the 2016-2020 American Community Survey in the zip code at the unit of $1000. There are less observations under Poisson regressions as year-quarters with only 0 outcomes are removed automatically.



(a) Relative Magnitudes Approach



(b) Smoothness Restrictions Approach

Figure E1: Treatment Effects of BlueLA on EV Adoption Using Rambachan and Roth (2023) Bounds

Note: Observations only include purchases within the subsidy program and are aggregated at zipcode-year level. Income trend is included.

F Further Decompositions of Car Types

For completeness, we also replicate our empirical strategy using hybrids and charge-capable cars as the outcome variable. For hybrids, we report (a) the event study for the impact of BlueLA on hybrids (as discussed in Section 3) in Online Appendix Figure F1d and (b) the Roth (2022) test in Online Appendix Figure **??**. The histogram of fixed-effect weights for hybrids is the same as the one for BEVs/PHEVs, as in Figure 1d, since the number of observations and the timing of BlueLA entries remain the same. We present the event study for charge-capable cars in Online Appendix Figure F1b. This event study qualitatively resembles the corresponding event study for overall EV purchases, much more so than the event study for hybrids. The Roth (2022) test for BEVs/PHEVs is presented in Online Appendix Figure F2; here, we detect no unique pre-trend for BEVs/PHEVs. The theme that emerges from these tests is that the impact of hybrids is minor compared to the impact on charge-capable cars. Note that our story does not require per se that there are *no* spillovers onto hybrids at all; indeed, consumers may perceive some overlap in characteristics between BlueLA cars and plug-in vehicles. Rather, our story is about the *relative* magnitude of the effects, and on this point, the comparison of our findings across these different outcome variables tells a clear story—the effect we document is indeed predominantly concentrated on charge-capable cars.

While we do not take a stand among the predictions of our hypothesis regarding the breakdown in purchases *between* BEVs and PHEVs—as this would depend more sensitively on consumer willingness to abandon gas capability altogether—we present results for each category separately in Online Appendix Table F1. We find statistically significant increases for both kinds of cars, although our point estimates point to slightly more adoptions of PHEVs relative to BEVs. We also present estimates of within-cohort treatment effects, as well as event-study estimates at the year level (again, as discussed in Section 3), separately for each category in Online Appendix Figure F3. We present the parallel trend test of Roth (2022) (discussed in Section 3) for each outcome variable in Online Appendix Figure F2. We do not discern any notable patterns between PHEVs and BEVs; further analysis of the differences in adoption behavior across these different classes of cars is beyond the scope of our exercise.

	Including Adjacent Zip Codes						Excluding Adjacent Zip Codes					
	Linear		Poisson		CSDID	SDID	Linear		Poisson		CSDID	SDID
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Effect on New EV Adoption: BEVs												
BlueLA	−0.009		−0.128				−0.010		−0.132			
	(0.030)		(0.433)				(0.030)		(0.433)			
BlueLA × Entry	0.060	0.071	0.663	0.925	0.129	0.068	0.064	0.076	0.725	0.980	0.130	0.073
	(0.012)	(0.016)	(0.258)	(0.341)	(0.023)	(0.027)	(0.011)	(0.016)	(0.247)	(0.331)	(0.023)	(0.023)
Panel B: Effect on New EV Adoption: PHEVs												
BlueLA	0.085		0.405				0.079		0.373			
	(0.050)		(0.211)				(0.052)		(0.222)			
BlueLA × Entry	0.086	0.085	0.061	0.003	0.173	0.104	0.105	0.106	0.140	0.099	0.191	0.137
	(0.098)	(0.085)	(0.232)	(0.181)	(0.135)	(0.119)	(0.100)	(0.087)	(0.239)	(0.188)	(0.137)	(0.104)
Observations	3,108	3,108	3,108	3,108	3,108	3,108	2,492	2,492	2,492	2,492	2,492	2,492
Year-Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
zip code FEs		Yes		Yes	Yes	Yes		Yes		Yes	Yes	Yes

Table F1: Main Results: the Influence of BlueLA Stations on Adoption of BEVs and PHEVs

Note: The top panel reports the average effect of BlueLA Stations on the number of incremental BEV purchases in each zip code each quarter, using linear, Poisson panel regressions, and Callaway and Sant'Anna's estimator in Figure 1a respectively. Panel B reports the same average effect for PHEVs. The unit of observation is at year-quarter and zip code levels. *BlueLA* indicates whether there are or there will be BlueLA stations in a zip code. *Entry* indicates whether there are already BlueLA stations in a zip code. Standard errors are two-way cluster standard errors at year-quarter and zip code levels.

	Panel A: By Membership Enrollment											
	< Median						≥ Median					
	Linear		Poisson		CSDID	SDID	Linear		Poisson		CSDID	SDID
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
BlueLA	0.103		0.318				0.015		0.065			
	(0.082)		(0.229)				(0.082)		(0.345)			
BlueLA × Entry	0.164	0.149	0.296	0.238	0.229	0.128	0.213	0.223	0.431	0.461	0.414	0.285
	(0.113)	(0.111)	(0.226)	(0.218)	(0.084)	(0.163)	(0.101)	(0.113)	(0.218)	(0.284)	(0.134)	(0.127)
Observations	2,296	2,296	2,296	2,296	2,296	2,296	2,296	2,296	2,296	2,296	2,296	2,296
	Panel B: By Charger Availability											
	< Median						≥ Median					
	Linear		Poisson		CSDID	SDID	Linear		Poisson		CSDID	SDID
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
BlueLA	0.032		0.184				0.104		0.238			
	(0.081)		(0.327)				(0.073)		(0.239)			
BlueLA × Entry	0.315	0.283	0.610	0.448	0.342	0.348	−0.039	0.026	−0.068	0.169	0.285	−0.031
	(0.097)	(0.100)	(0.242)	(0.260)	(0.258)	(0.131)	(0.134)	(0.115)	(0.305)	(0.252)	(0.091)	(0.127)
Observations	1,288	1,288	1,288	1,288	1,288	1,288	1,204	1,204	1,204	1,204	1,204	1,204
Year-Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
zip code FEs		Yes		Yes	Yes	Yes		Yes		Yes	Yes	Yes

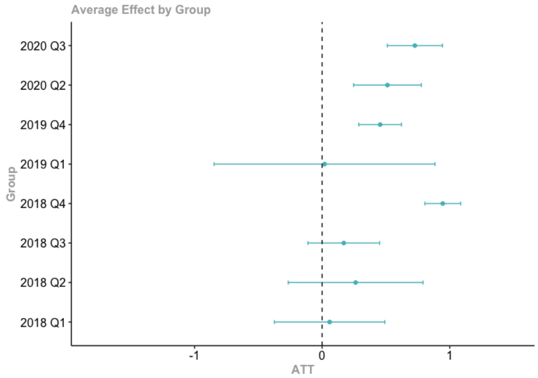
Table F2: The Influence of BlueLA Stations on Adoption of BEVs/PHEVs: Heterogeneity

Note: This table reports the effect of BlueLA stations on the EV adoption in the same zip code by BlueLA membership and charger availability. Note that zip codes adjacent to the treated zip codes are excluded to deal with the potential spillover effect. Low/high charger availability is defined as below/above the median of the available chargers in LA. *BlueLA* indicates whether there are or will be BlueLA stations in the zip code. *Entry* indicates whether there are already BlueLA stations in the zip code. Standard errors are two-way cluster standard errors at year-quarter and zip code levels.

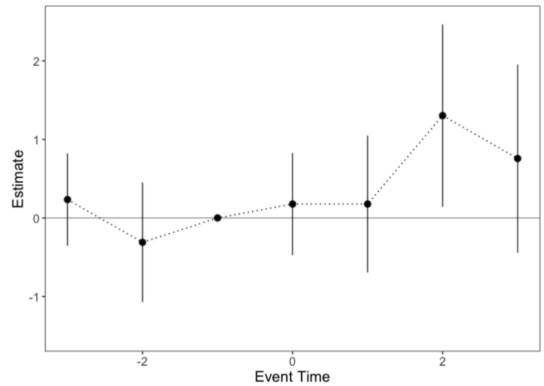
	Number of New EV Adoption in First-Adjacent Zip Codes						Number of New EV Adoption in Second-Adjacent Zip Codes					
	Linear		Poisson		CSDID	SDID	Linear		Poisson		CSDID	SDID
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Adjacent BlueLA	−0.010		−0.037				−0.084		−0.339			
	(0.048)		(0.186)				(0.059)		(0.254)			
Adjacent BlueLA × Entry	0.071	0.079	0.199	0.229	0.010	0.048	0.048	−0.018	0.218	−0.155	−0.175	−0.076
	(0.072)	(0.068)	(0.185)	(0.176)	(0.089)	(0.089)	(0.084)	(0.069)	(0.258)	(0.236)	(0.147)	(0.097)
Observations	2,716	2,716	2,716	2,716	2,716	2,716	2,100	2,100	2,100	2,100	2,100	2,100
P-value												
(H_0: treatment effect = spillover effect)	0.399	0.327	0.816	0.805	0.075		0.301	0.071	0.718	0.045	0.018	
Year-Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
zip code FEs		Yes		Yes	Yes	Yes		Yes		Yes	Yes	Yes

Table F3: The Influence of BlueLA Stations on Adoption of BEVs/PHEVs: Spillover

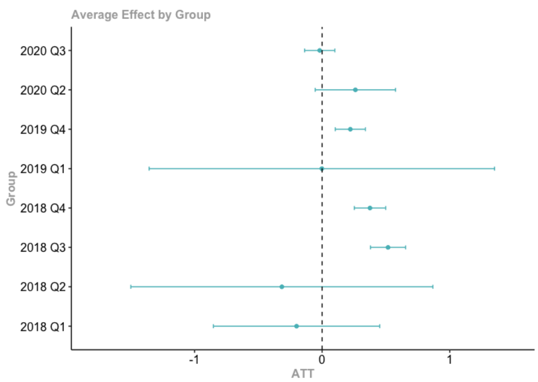
Note: This table reports the effect of BlueLA stations in an adjacent zip code (and the adjacent zip code's neighbor) on the number of incremental EV purchases for BEVs/PHEVs in zip codes where there is no any BlueLA station. Zip codes (14 out of 111) with at least one BlueLA station during our study period are excluded from the sample. Of the remaining zip codes, 24 of them are adjacent to zip codes with at least one BlueLA station. The unit of observation is at year-quarter and zip code levels. *Adjacent BlueLA* indicates whether there are or there will be BlueLA stations in an adjacent zip code. *Entry* indicates whether there are already BlueLA stations in an adjacent zip code. Whether the treatment effects excluding the first-adjacent zip codes, as shown in Panel B in Table 2, are significantly different from the spillover effects are tested through Chi-squared tests. For CSDID, the difference is tested using t-test. Standard errors are two-way cluster standard errors at year-quarter and zip code levels.



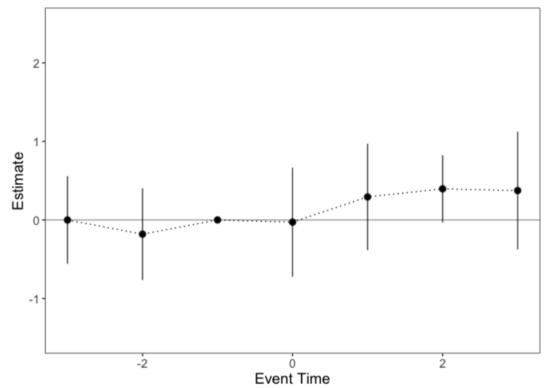
(a) By Cohort: BEV+PHEV



(b) By Year: BEV+PHEV



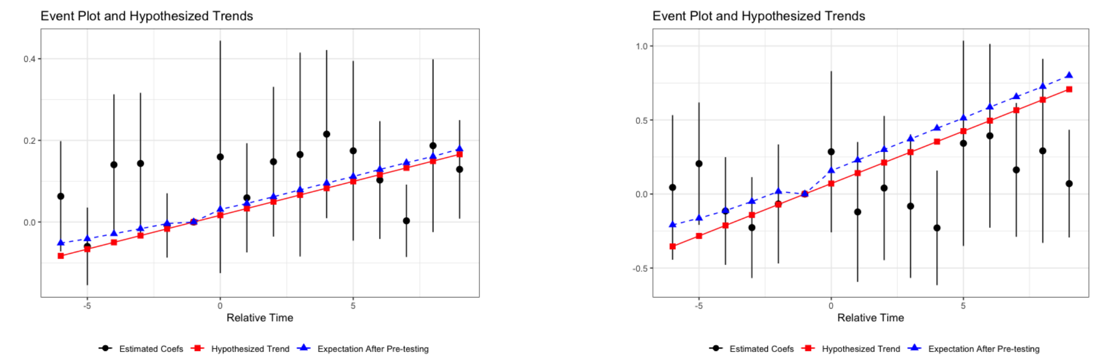
(c) By Cohort: Hybrid



(d) By Year: Hybrid

Figure F1: Treatment Effects of BlueLA on EV Adoption

Note: Figure (a) and (c) show the treatment effect of BlueLA on EV adoption by cohort for EVs that include BEVs and PHEVs, and hybrids respectively. Figure (b) and (d) show the treatment effect of BlueLA on EV adoption by year, using dynamic effects from Callaway and Sant'Anna (2021). Note that the year before the treatment year is set as the baseline, as suggested in Roth (2024).

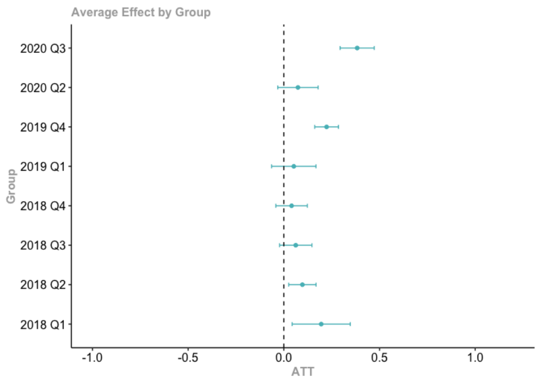


(a) Parallel Trend Test: BEV

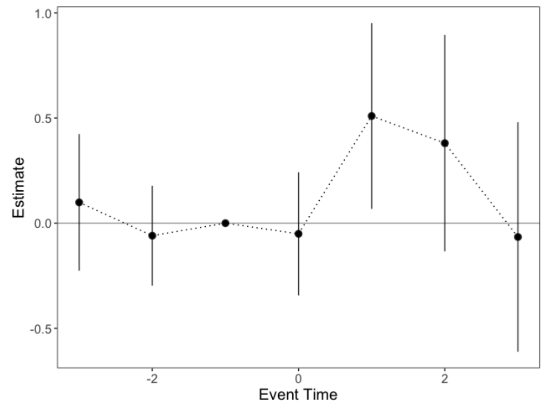
(b) Parallel Trend Test: PHEV

Figure F2: Parallel Trend Tests: BEVs and PHEVs

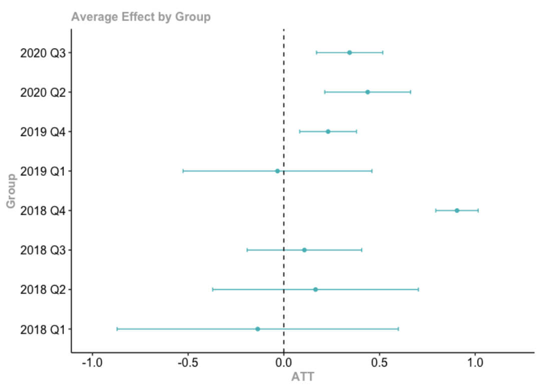
Note: Figure (a) and (b) show the parallel trend test proposed in Roth (2022) under the power of 0.5. Black dots are estimated coefficients of conventional TWFE. Red squares are the minimum linear trend detectable with a power of 0.5. The minimum slope of a detectable pre-trend is 0.017 for BEVs and 0.071 for PHEVs. Blue triangles are coefficient expectations conditional on passing. The reported Bayes Factor and Likelihood Ratio are 0.637 and 0.264 respectively for BEVs, and 0.603 and 0.031 for PHEVs. Under an alternative power of 0.8, the minimum slope of a detectable pre-trend is 0.028 for BEVs and 0.112 for PHEVs. The reported Bayes Factor and Likelihood Ratio are 0.254 and 0.028 respectively for BEVs, and 0.241 and 0.001 for PHEVs.



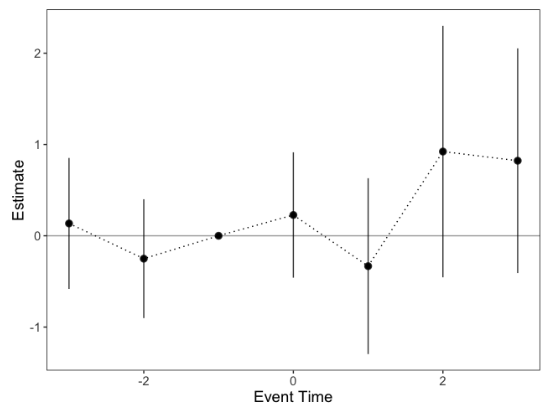
(a) By Cohort: BEV



(b) By Year: BEV



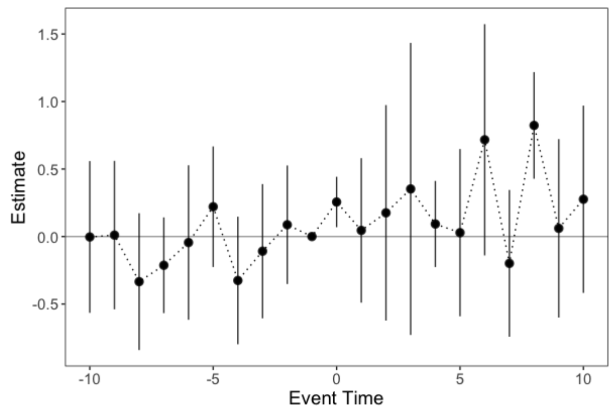
(c) By Cohort: PHEV



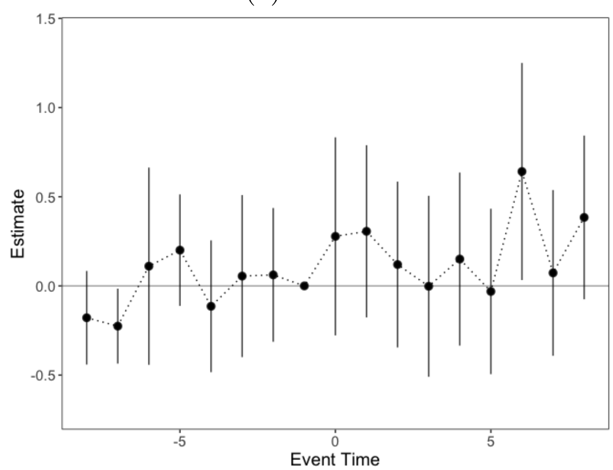
(d) By Year: PHEV

Figure F3: Treatment Effects of BlueLA on EV Adoption

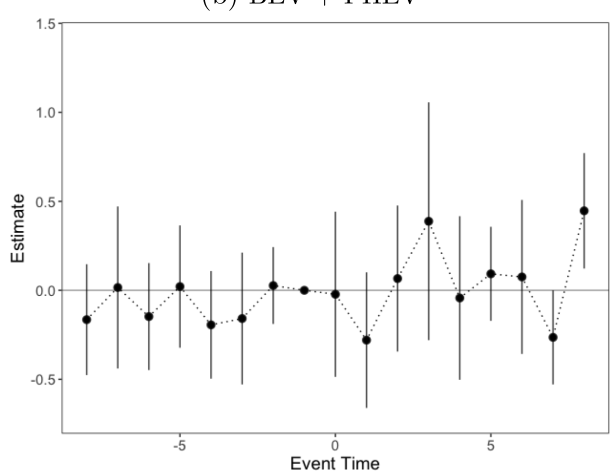
Note: Figure (a) and (c) show the treatment effect of BlueLA on EV adoption by cohort for BEVs and PHEVs respectively. Figure (b) and (d) show the treatment effect of BlueLA on EV adoption by year, using dynamic effects from Callaway and Sant'Anna (2021). Note that the year before the treatment year is set as the baseline, as suggested in Roth (2024).



(a) Overall



(b) BEV + PHEV



(c) Hybrid

Figure F4: Dynamic Effects of BlueLA Station on EV Adoption by Quarter

Note: This plot displays the dynamic effects of BlueLA stations on EV adoption by quarter, using Callaway and Sant'Anna (2021) estimator. Dynamic effects are obtained by simple averages across groups at each period. The confidence intervals are based on a 5% significance level.