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FISCALLY REGRESSIVE SUBSIDIES AND THE SPATIAL DIFFUSION OF ELECTRIC VEHICLES IN FRENCH CITIES

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Fiscally Regressive Subsidies and the Spatial Diffusion of Electric Vehicles in French Cities*

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Abstract

This paper investigates the equity implications of the 2018 policy change regarding France's electric vehicle (EV) subsidies, focusing on its differential impact across French municipalities (i.e., communes) with varying income levels. Using a synthetic event study and a synthetic difference-in-differences approach, a significant and disproportionately lower rate of EV adoption is identified in communes with a higher share of non-taxable (i.e., lower-income) households. Although the nominal subsidy reduction for new EV purchases was the same (€1,500), the proportional financial burden was larger for lower-income households, thus inhibiting EV uptake compared with higher-income households. Robustness checks—including placebo tests, alternative treatment definitions, and the exclusion of outlier years and regions—support the validity of the causal inference. Additionally, the results show that the €1,000 additional benefit for non-taxable households on secondhand vehicles did not sufficiently offset the impact of the reduction in new EVs. These findings underscore the regressive effects of uniform subsidy cuts across income levels, which may hinder lower-income households from participating equally in the clean energy transition. This work contributes to the literature on environmental equity and offers policy-relevant insights for designing socially inclusive decarbonization strategies amid declining public subsidies across Europe.

Keywords: Electric Vehicle Adoption, France, Subsidy Change, Inequality, Synthetic Difference-in-Differences

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1. Introduction

The transition to low-carbon mobility is a cornerstone of climate policy across Europe, with electric vehicles (EVs) playing a central role. To accelerate adoption, many governments have introduced generous purchase subsidies. However, as fiscal pressures grow and EV costs decline, subsidy reductions are becoming more common. This raises a critical policy question: Do subsidy cuts affect all income groups equally, or do they risk exacerbating existing socioeconomic disparities in the adoption of clean technologies?

This paper investigates the equity implications of the French government's 2018 reform of EV subsidies, which introduced a reduction in financial support for new EV purchases. Specifically, the effect of the reform on EV adoption across French municipalities (i.e., communes) with varying shares of non-taxable households—a proxy for income levels—is examined. The nominal subsidy cut was the same for higher-income (taxable) and lower-income (non-taxable) households, but this study finds that lower-income households experienced a more pronounced decline in EV uptake. The additional €1,000 benefit on secondhand vehicles reserved for non-taxable households was insufficient to offset the effect of the general reduction, suggesting that the policy disproportionately affected lower-income communes.

To identify the causal effect of the subsidy change, an analysis is performed employing two complementary methodologies: a synthetic event study and a synthetic difference-in-differences (SDID) approach. Both methods provide evidence showing that the policy reform led to a greater decrease in EV fleet size in communes with higher shares of non-taxable households. These results are robust to a range of specifications, including alternative definitions of treatment, the exclusion of influential years (notably 2014), the removal of French overseas territories from the sample, and the inclusion of urbanization as a proxy for charging infrastructure and public transport access.

Beyond estimating the immediate policy effects, broader concerns about internal and external validity are investigated herein. Although the analysis is conducted at the communal level owing to data constraints, meaningful insights into the distributional consequences of national environmental policy are obtained. Moreover, the results remain stable across multiple robustness checks, including placebo tests, which confirm the credibility of the counterfactual groups. The potential unobserved confounders—such as political preferences and infrastructure availability—are briefly mentioned, but future research is necessary to directly address these factors.

The findings have significant policy implications, particularly in the context of France's ambitious climate targets, which include banning the sale of internal combustion engine vehicles by 2035 and restricting their use in cities such as Paris by 2030. As EV prices continue to decline because of technological progress, blanket subsidy reductions may appear increasingly justified. However, this paper demonstrates that such reductions can have regressive effects unless carefully designed. In the short term, more targeted support for lower-income households may be necessary to ensure a fair and inclusive transition to electric mobility.

2. Review of the Literature

The diffusion of EVs is considered essential for limiting greenhouse gas emissions in the transport sector; thus, policymakers have adopted a mix of actions combining regulatory measures and subsidies. Financial incentives are meant to alter price competition between types of vehicles by leveraging economic conditions and consumer characteristics.

Notably, Mukherjee and Ryan (2020) found that people with a university degree and long-distance commuters with higher cost savings over the car's lifetime are more prone to purchase an EV, which makes them more sensitive to financial incentives. Sovacool et al. (2019) analyzed respondents of a survey across Denmark, Finland, Iceland, Sweden, and Norway and, not surprisingly, found that people with higher incomes are more likely to own an EV. Similarly, Hennessy and Syal (2023) found that EV adoption and rebate use are concentrated in higher-income zip codes in California. Plötz et al. (2014) showed that middle-aged men living in multi-person households with technical professions and in rural/suburban areas have the highest willingness to buy EVs in Germany. Bas et al. (2021) employed machine learning—specifically, a preliminary random forest algorithm—to determine the most important variables for predicting the type of vehicle purchased, revealing that the county in which the user resides has the highest predictive power. This variable reflects a geographic income distribution, as some counties are wealthier than others, but household income is also classified as an important variable and may be more useful. Using a support vector machine with only socioeconomic variables, their model achieved approximately 0.799 accuracy, which is still relatively high. Thus, overall, income and socioeconomic factors have been shown to be important when determining EV purchase patterns.

However, the existing literature has primarily focused on the general regressivity of subsidies, neglecting the impact of such inequality on the diffusion of EVs. In this context, several equity issues arise even if the benefits of environmental policies, such as EV rebates, outweigh the costs. For multiple reasons, lower-income groups benefit less from environmental policies. The main reasons for this are differences in the characteristics of the individuals who buy EVs, price hurdles, and infrastructure differences for lower- and higher-income buyers. Ku and Graham (2022) showed that gains from environmental and energy policies, especially those that support EVs, are not equally shared by all income groups. Focusing on the California EV rebate program, they found a net regressive effect; specifically, the benefit distribution is highly regressive while the cost distribution is slightly progressive. Thus, wealthier neighborhoods benefit the most, whereas people of color and less-educated people benefit less (Ju et al., 2020). Similarly, Caulfield et al. (2021) studied the equity impacts of government subsidies for EVs and found that lower-income individuals have a financial barrier to shifting to EVs, revealing an income equity gap.

Hardman et al. (2017) also found that incentives should not be applied to high-end EVs because most purchasers would have bought the car regardless of the price, owing to environmental reasons, status, and other considerations. Additionally, they showed that EV-related education campaigns and incentives are important: individuals with lower knowledge of EV incentives did not become more interested in EVs, whereas those with higher knowledge showed increased interest.

The main reason for this trend is the price hurdle of purchasing a new car (Hardman et al., 2017). Noel et al. (2020) found that, according to experts, price is a common barrier because EV capital costs can be as much as twice that of a normal vehicle. For instance, a petrol VW Golf costs around 23,141€–30,000€ in France, and an electric VW Golf costs around 28,490€–33,950€. Likewise, upfront costs, such as the cost of installing charging infrastructure, are expected to be higher. Mukherjee and Ryan (2020) found that individuals living in apartments or renting homes are less likely to own an EV because they typically park on the street and do not have access to charging stations. Moreover, if they rent their home, they are less likely to invest in charging infrastructure.

3. Policy Change and Research Design

Introduced in France in 2008, the bonus-malus system penalizes the purchase of high-emission vehicles while giving incentives for buying low-emission alternatives. The conversion bonus instrument further encourages individuals to scrap their old, higher-polluting cars by offering support for the purchase of EVs or other lower-polluting vehicles, with the eligibility criteria becoming more stringent over time. In 2014, the ecological bonus provided €6500 for new EVs and other lower-polluting cars, such as hybrid and natural gas vehicles.

In 2015, the ecological bonus and conversion bonus provided a total subsidy of up to €10,000 (€6300 eco bonus, €3600 conversion bonus) as long as the subsidy did not exceed 27% of the vehicle's price. In 2018, the conversion bonus for new EVs was reduced by €1500 for all households, decreasing the total subsidy from €10,000 to €8500. Conversely, the conversion bonus for secondhand EVs increased from €7000 to €8000 for non-taxable households and from €6000 to €7000 for taxable households. It remains necessary to evaluate how the drop in the subsidy for new cars versus the increase in the subsidy for secondhand cars differentially impacted taxable and non-taxable households. This becomes especially important considering the recent decreases in subsidy levels across Europe. For example, Germany eliminated subsidies in 2023, and France began lowering them in 2021.

In response to the Gilet Jaune movement, the French government decided to address equity issues by substantially increasing the conversion bonus for new EVs in 2019 for people living in non-taxable households. These households could receive up to €5000 (capped at a maximum of 80% of the vehicle purchase price) when scrapping an older high-polluting car and purchasing a new or secondhand EV, reaching up to €11,000 in combined subsidies (€6000 eco bonus, €5000). Conversely, tax-paying households were eligible for a maximum combined subsidy of €8500. Tax-paying households received an additional €1000 conversion bonus when buying secondhand EVs, whereas non-tax-paying households received €5000. Consequently, a substantial decrease in EV purchase subsidies was observed in 2018, followed by an increase in 2019.

Herein, the impact of the 2018 reduction in the French conversion bonus from €10,000 to €8500 for new EVs on EV purchasing behavior for both non-taxable and taxable households in France is examined. Data are analyzed using a synthetic event study approach in a scenario where all groups are affected by the subsidy change but the magnitude of the subsidy reduction differs. Understanding these effects is relevant because similar subsidies are being reduced or eliminated across Europe.

Table 1: Bonus Subsidy and Conversion Subsidy by Year for Taxable and Non-Taxable Households

Year	Bonus subsidy for electric vehicles	Conversion subsidy		Total	
		Taxable households	**Non-taxable households**	**Taxable**	**Non-taxable**
2014	€6300 (after November 1, 2013, the overall amount of aid cannot exceed 27% of the purchase price of the vehicle); Décret n° 2007-1873 du 26 décembre 2007 instituant une aide à l'acquisition des véhicules propres, art. 3b (version in force until 30 Déc 2014)	€200 Décret n° 2014-1672 du 30 décembre 2014 instituant une aide à l'acquisition et à la location des véhicules peu polluants, art 4	€200 Décret n° 2014-1672 du 30 décembre 2014 instituant une aide à l'acquisition et à la location des véhicules peu polluants, art 4	€6500	€6500
2015	€6300 (cannot exceed 27% of the purchase price of the vehicle); Décret n° 2014-1672 du 30 décembre 2014 instituant une aide à l'acquisition et à la location des véhicules peu polluants, art. 3b	€3700 Décret n° 2015-361 du 30 mars 2015 modifiant le décret n° 2014-1672 du 30 décembre 2014 instituant une aide à l'acquisition et à la location des véhicules peu polluants	€3700 Décret n° 2015-361 du 30 mars 2015 modifiant le décret n° 2014-1672 du 30 décembre 2014 instituant une aide à l'acquisition et à la location des véhicules peu polluants	€10,000	€10,000
2016	€6300 (cannot exceed 27% of the purchase price of the vehicle); Article D251-7Version en	€3700 Décret n° 2015-361 du 30 mars 2015 modifiant le décret n° 2014-1672 du 30	€3700 Décret n° 2015-361 du 30 mars 2015 modifiant le décret n° 2014-1672 du 30 décembre 2014	€10,000	€10,000

	vigueur du 01 janvier 2016 au 04 janvier 2016 Création Décret n°2015-1823 du 30 décembre 2015 - art. & Article D251-7Version en vigueur du 04 janvier 2016 au 01 janvier 2017 Modifié par Décret n°2015-1928 du 31 décembre 2015 - art. 4	décembre 2014 instituant une aide à l'acquisition et à la location des véhicules peu polluants	instituant une aide à l'acquisition et à la location des véhicules peu polluants		
2017	€6000 (cannot exceed 27% of the purchase price of the vehicle); Article D251-7, Version en vigueur du 01 janvier 2017 au 19 février 2017 Modifié par Décret n°2016-1980 du 30 décembre 2016 - art. 1 & Article D251-7, Version en vigueur du 19 février 2017 au 01 janvier 2018 Modifié par Décret n°2017-196 du 16 février 2017 - art. 1	€4000 Article D251-8Version en vigueur du 19 février 2017 au 01 janvier 2018 Modifié par Décret n°2017-196 du 16 février 2017 - art. 1 & Article D251-8 Version en vigueur du 01 janvier 2017 au 19 février 2017 Modifié par Décret n°2016-1980 du 30 décembre 2016 - art. 1	€4000 €1000 for secondhand EVs Article D251-8Version en vigueur du 19 février 2017 au 01 janvier 2018 Modifié par Décret n°2017-196 du 16 février 2017 - art. 1 & Article D251-8 Version en vigueur du 01 janvier 2017 au 19 février 2017 Modifié par Décret n°2016-1980 du 30 décembre 2016 -	€10,000	€10,000
2018	€6000 (cannot exceed 27% of the purchase price of the vehicle); Article D251-7 Modifié par Décret n°2017-1851 du 29 décembre 2017 - art. 1	€2500 €1000 for secondhand EVs; Article D251-8, Version en vigueur du 01 janvier 2018 au 01 janvier 2019 Modifié par Décret n°2017-	€2500 €2000 for secondhand EVs; Article D251-8Version en vigueur du 01 janvier 2018 au 01 janvier 2019 Modifié par Décret n°2017-1851 du 29	€8500–€7000	€8500–€8000

		1851 du 29 décembre 2017 - art. 1	décembre 2017 - art. 1		
2019	€6000 (cannot exceed 27% of the purchase price of the vehicle); Article D251-7, Modifié par Décret n°2018-1318 du 28 décembre 2018 - art. 1	€2500 €1000 for secondhand EV; Article D251-8 Version en vigueur du 01 janvier 2019 au 01 août 2019 Modifié par Décret n°2018-1318 du 28 décembre 2018 - art. 1	€2500–€5000 (up to 80% of the purchase price, up to a maximum of €5000, if distance between home and place of work is greater than 30 km or who drives more than 12,000 km per year in his vehicle or if the income of the individual is lower than €6300) €2000–€4000 secondhand (up to 80% of the purchase price to a maximum of €4,000 if distance between home and place of work is greater than 30 km, or who drives more than 12,000 km per year in his vehicle or if the income of the individual is lower than €6300); Article D251-8 Version en vigueur du 01 janvier 2019 au 01 août 2019 Modifié par Décret n°2018-1318 du 28 décembre 2018 - art. 1	€8500	€11,000
2020	€7000 (< price of car is less than €45,000)	€2500	€5000 (up to 80% of the purchase price to a maximum of €3000 if distance between home and place of work is greater than 30 km or who drives more than 12,000 km	€9500	€12,000

per year in his vehicle, and if the income of the individual is lower than €13,489, or by anyone with an income < €6300.) €1500 secondhand (if fiscal income < €13,489); €3000 secondhand (up to 80% of the purchase price, up to a maximum of €3000, if distance between home and place of work is greater than 30 km or who drives more than 12,000 km per year in his vehicle, and if the income of the individual is lower than €13,489, or by anyone with an income < €6300). Article D251-8Version en vigueur du 01 janvier 2020 au 05 mars 2020 Modifié par Décret n°2019-1526 du 30 décembre 2019 - art. 1 & Article D251-8Version en vigueur du 05 mars 2020 au 01 juin 2020 Modifié par Décret n°2020-188 du 3 mars 2020 - art. 1 & €5000 (up to 80% of the purchase price to a maximum of €5000 if the income of the individual was lower than € 18000);

			Article D251-8Version en vigueur du 01 juin 2020 au 03 août 2020 Modifié par Décret n°2020-656 du 30 mai 2020 - art. 1	
	€3000 (€45,000–€60,000) Article D251-7 Version en vigueur du 01 janvier 2020 au 01 juin 2020, Modifié par Décret n°2019-1526 du 30 décembre 2019 - art. 1 & Article D251-7Version en vigueur du 01 juin 2020 au 21 janvier 2021 Modifié par Décret n°2020-656 du 30 mai 2020 - art. 1	€0 secondhand Article D251-8Version en vigueur du 01 janvier 2020 au 05 mars 2020 Modifié par Décret n°2019-1526 du 30 décembre 2019 - art. 1 & Article D251-8Version en vigueur du 05 mars 2020 au 01 juin 2020 Modifié par Décret n°2020-188 du 3 mars 2020 - art. 1 & Article D251-8Version en vigueur du 01 juin 2020 au 03 août 2020 Modifié par Décret n°2020-656 du 30 mai 2020 - art. 1 & Article D251-8Version en vigueur du 03 août 2020 au 01 juillet 2021 Modifié par Décret n°2020-955 du 31 juillet 2020 - art. 1	€5000 (up to 80% of the purchase price to a maximum of €5000 if distance between home and place of work is greater than 30 km or who drives more than 12,000 km per year in his vehicle and if the income of the individual is lower than €13,489 or by anyone with an income < €6300.) Article D251-8Version en vigueur du 03 août 2020 au 01 juillet 2021 Modifié par Décret n°2020-955 du 31 juillet 2020 - art. 1	

2021	€7000 (< price of car is less than €45,000) €3000 (€45,000–€60,000) Article D251-7Version en vigueur du 21 janvier 2021 au 01 juillet 2021 Modifié par Décret n°2021-37 du 19 janvier 2021 - art. 1 Modifié par Décret n°2021-37 du 19 janvier 2021 - art. 2 €6000 (< price of car is less than €45,000) €2000 (€45,000–€60,000) Article D251-7Version en vigueur du 01 juillet 2021 au 26 juillet 2021 Modifié par Décret n°2020-1526 du 7 décembre 2020 - art. 2 & Article D251-7Version en vigueur du 26 juillet 2021 au 01 juillet 2022 Modifié par Décret n°2021-977 du 23 juillet 2021 - art. 1	€2500 €0 secondhand Article D251-8Version en vigueur du 03 août 2020 au 01 juillet 2021 Modifié par Décret n°2020-955 du 31 juillet 2020 - art. 1 & Article D251-8Version en vigueur du 01 juillet 2021 au 26 juillet 2021 Modifié par Décret n°2020-1526 du 7 décembre 2020 - art. 2	€5000 (up to 80% of the purchase price to a maximum of €5000 if distance between home and place of work is greater than 30 km or who drives more than 12,000 km per year in his vehicle, and if the income of the individual was lower than €13,489, or by anyone with an income < €6300) Article D251-8Version en vigueur du 03 août 2020 au 01 juillet 2021 Modifié par Décret n°2020-955 du 31 juillet 2020 - art. 1 & Article D251-8Version en vigueur du 01 juillet 2021 au 26 juillet 2021 Modifié par Décret n°2020-1526 du 7 décembre 2020 - art. 2	€8500	€11,000

Households in France are classified as taxable or non-taxable depending on the income of the individuals in the household. In addition, the French taxation system works under the "quotient familial" mechanism, whereby the household's tax rate is adjusted according to the number of individuals living in a household. This allows for more equitable taxation according to specific family structures. Thus, a household is classified as non-taxable if its total annual income, divided by the number of taxable individuals, falls below the taxable threshold. This calculation also varies depending on the relationships between the individuals living in a household, such as marital status and number of children. The first two children count as half an individual, and the third child counts as one individual.

The non-taxable threshold is updated each year to implement fiscal policy and account for inflation. Table 2 shows the evolution of the non-taxable income bracket since 2014. Households that fall below this income share do not have to pay any income taxes and often qualify for other social programs that aid poorer households and support social equity.

Table 2: French Income Tax Brackets for Non-Taxable Individuals (2014–2021)

Year	Income Bracket 1 (€)
2014	Up to 6011
2015	Up to 9690
2016	Up to 9700
2017	Up to 9710
2018	Up to 9807
2019	Up to 9964
2020	Up to 10,064
2021	Up to 10,084

The income brackets and tax rates are sourced from the national tax code (Code Général des Impot)

The subsidy is disbursed through two main channels: either directly at the time of purchase, when an individual buys a vehicle at a dealership, or through reimbursement via the official government website. Vehicles are most commonly purchased at a dealership, where the bonus is already calculated into the purchase price. The dealership usually requests the bonus from the government and then applies it directly, advertising the original price and showing the subsidy as a separate deduction.

In the literature, there are significant gaps in the analysis of equity in EV promotion policies. This is mainly because most studies on the demographic preferences for EVs were conducted before the implementation of subsidy programs and equitable subsidy structures. Hence, research on the effects of real-world subsidies remains scarce, especially in European countries. Notably, Caulfield et al. (2022) evaluated how income, car ownership, and economic status affect EV take-up using household chargers as a proxy for EV ownership in Europe. More attention has been focused on regional US policies; for example, Graham (2022) and Ju (2020) evaluated the regressivity of California's EV rebate program.

This gap is addressed herein. A national policy has recently been designed and implemented to reduce the subsidy for new EVs by the same amount for both taxable and non-taxable households while differentially increasing the subsidy for secondhand EVs for taxable and non-taxable households. As previously mentioned, the total subsidy for new EVs decreased from €10,000 to €8500. Moreover, the total subsidy for secondhand EVs increased from €6000 to €7000 for taxable households, whereas it increased from €7000 to €8000 for non-taxable households. In the following year, the subsidy level increased to €11,000 for non-taxable households, likely in response to a substantial decrease in subsidy uptake by poor households in the prior year. Owing to the recent reduction in subsidy levels in Europe, particularly in Germany and France since 2021, research is needed to evaluate how decreasing subsidies affects the uptake of EVs in lower-income (non-taxable) and higher-income (taxable) households.

Using a synthetic event study and an SDID approach, this research paper evaluates the impact of the decrease in France's 2018 conversion bonus program for tax-paying and non-tax-paying households on the EV fleet. By focusing on this reform, this study helps elucidate how equity-oriented changes to subsidy structures influence EV adoption.

4. Data Used in This Study

A dataset was constructed by merging variables from multiple sources to support the analysis. The dataset includes information on the EV fleet and the socioeconomic characteristics of households at the communal level.

Paris is excluded from the main analysis owing to its high levels of infrastructure, population density, and economic activity compared with other regions. The Ile de France region (comprising the Paris metropolitan area) accounts for one-sixth of the French population, with more than 11 million inhabitants. Moreover, several EV-related policies only apply to the Ile de France region. Thus, the Paris region is omitted from the analysis to avoid the model being heavily influenced by estimations from the Paris commune, which may impact the model's general validity for other regions.

The dependent variable in this study is the annual EV fleet in French communes between 2014 and 2021, sourced from the 2023 municipal data for passenger cars, referred to as the SDES dataset. This dataset provides comprehensive information on the French car fleet, categorized by vehicle type, energy usage, and Crit'Air sticker. Specifically, it details the number of registered electric and hydrogen vehicles, as well as petrol and other gas-run cars, in each commune as of the 1st of January of each year. Note that the dataset gives only a combined measure of hydrogen and EVs; however, the total number of hydrogen cars on French roads is negligible: In 2021, the total French fleet of hydrogen cars was around 400 vehicles, whereas the total number of registered EV passenger cars was more than 400,000, according to the SDES dataset. As of 2021, EVs made up 2.32% of the total fleet of passenger cars, and this number is expected to grow in the coming years.

Figure 1: French Fleet by Fuel Type

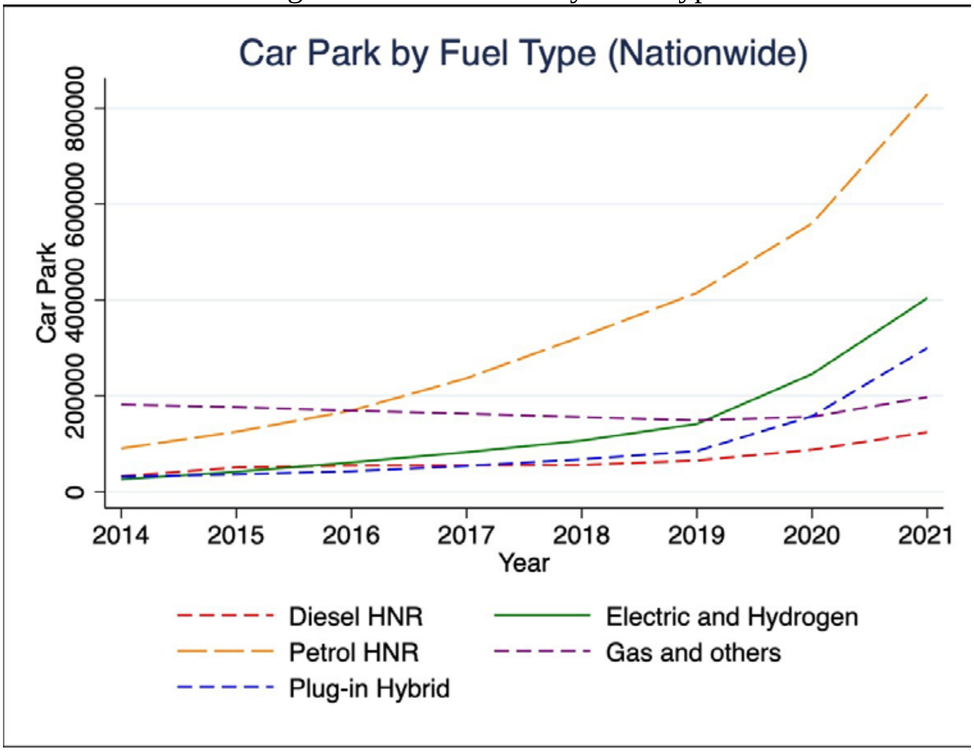


Figure 1 illustrates a nationwide (excluding Paris) increase in the EV fleet over the years. In 2014, the number of EVs was approximately 25,958, and by the end of 2021, more than 403,000 EVs had been registered. The diagram does not show that diesel and petrol cars reach around 21 and 15.5 million vehicles, respectively.

The EV fleet data from the French government is available only at the national, regional, and communal levels and is not accessible at the individual level because of data protection measures. Therefore, communal-level data is the highest resolution available for this study.

The treatment and control variables are constructed using four separate datasets: the Filosofi dataset (income), the National Institute of Statistics and Economic Studies (INSEE; Institut National de la Statistique et des Études Économiques) dataset (population), and additional datasets on education and urbanization level. The Filosofi dataset provides communal-level indicators of taxed households, which are households made up of a group of taxable individuals that live in the same dwelling. The data is based on tax data obtained from personal income tax returns and data on social benefits, which are evaluated by the INSEE. Considering data privacy issues, the INSEE reconstitutes declared and disposable income at the communal level. Based on these constraints, very small communes (fewer than 50 households or 100 inhabitants) are excluded entirely from the original dataset, and communes with fewer than 1000 households or 2000 inhabitants are only reported with limited indicators. Consequently, these small communes are excluded from this study's analysis.

Population data is used to determine the commune population size, thus controlling for the effects of many people living in an area, which varies with time and may impact vehicle purchases. This variable is added to all the model specifications. The education dataset is used to determine the number of people in a commune with a high school diploma. Education level has been shown to influence EV purchasing behavior, and education levels can differentially change over time for the treatment and control groups. Additionally, the non-EV fleet is accounted for to control for broader vehicle purchasing trends. Finally, an urbanization categorical variable is constructed to generate a dummy variable indicating the level of urbanization. Following the classification set by the European Union, a commune is considered dense if more than half its population lives in a concentrated area.

The EV conversion subsidy that this paper focuses on distinguishes between non-taxable and taxable households to determine the level of the subsidy. The Filosofi dataset provides a variable indicating the share of taxable households in each commune for 2014–2021. From this, an indicator for the share of non-taxable households is constructed. A commune is assigned to the treatment group if more than 60% of households in the commune are non-taxable households. Conversely, a commune belongs to the control group when the share of non-taxable households is below 60%. As mentioned above, the dataset does not include this variable for communes with fewer than 1000 households or 2000 inhabitants. Figure 2 presents a map of the prevalence of non-taxable households for communes with available data.

Figure 2: Distribution of Taxable Households in Mainland France

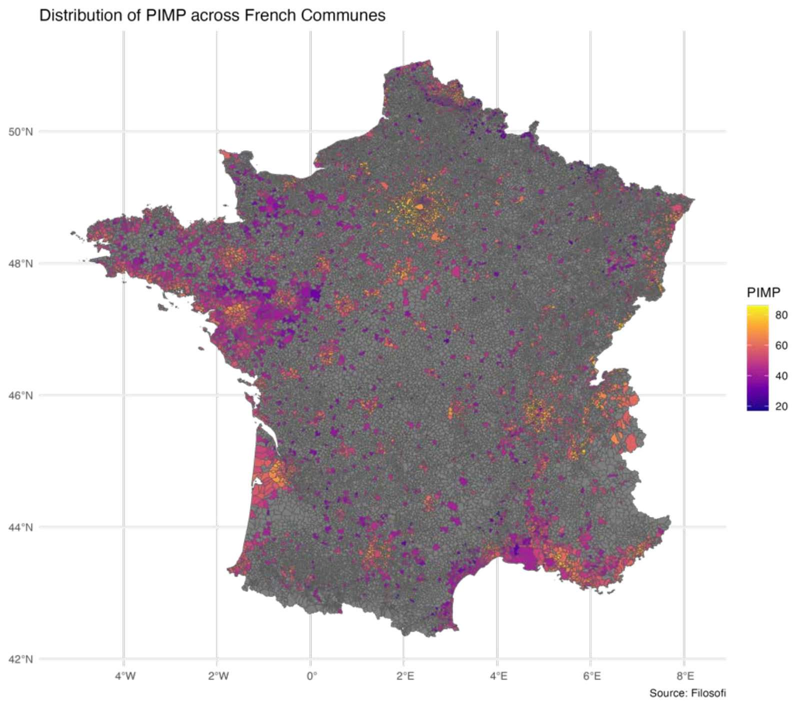


Table 1 shows the evolution of the maximum total subsidy level. In 2018, the maximum subsidy for new EVs was cut by €1500, from €10,000 to €8500, for both taxable and non-taxable households. At the same time, the total subsidy for secondhand EVs rose by €1000, reaching €7000 for taxable households and €8000 for non-taxable households. This paper focuses on the effect of these asymmetric changes in the subsidy level in 2018. Treatment status is defined at the communal level by focusing on the distribution of non-taxable households across French communes.

5. Methodology

This paper aims to assess the subsidy's differential impact on non-taxable versus taxable households. Given that the data are only available at the communal level, it is not possible to directly compare taxable versus non-taxable households. Instead, communes that have a low share of non-taxable households are compared with communes that have a high share of non-taxable households to understand the subsidy's effect. However, in higher-income communes with a higher share of taxable households, key characteristics such as population density and education may differ from those of lower-income communes. These pre-existing differences can influence the adoption of EVs, inhibiting the direct comparison between lower- and higher-income communes. To address this, a synthetic event study approach is employed, as previously described (Ciccia, 2024). A weighted combination of communes with a high share of taxable households is constructed, forming a synthetic control group that resembles the communes with a high share of non-taxable individuals. This synthetic group of communes with a higher share of taxable households is now comparable to those with a high share of non-taxable households, enabling an estimation of the subsidy's differential effect.

The event study helps clarify the dynamic effects of the policy treatment, namely the subsidy decrease in 2018, on the EV fleet in France (Miller, 2023). By examining the temporal aspect, the policy's immediate and long-term impacts can be elucidated.

Another reason to conduct the event study is to determine whether the synthetic control and the treatment group are similar before the policy introduction, as well as to observe any pre-intervention patterns that can help support the identification strategy. Typically, the synthetic framework should lead to two comparable groups. However, it is useful to test this further rather than relying on the SDID diagram. If the differences between the two groups are low before the policy introduction, the credibility of the findings may increase.

Moreover, the event study clarifies whether anticipatory effects differentially impacted either group. Table 3 compares communes with a high and a low share of non-taxable households, showing that several characteristics are significantly different between the two groups.

Table 3: Comparing Variables Between Communes with High and Low Shares of Non-taxable Households

Variable	(1) All samples	(2) Communes with a high share	(3) Communes with a low share	(4) Difference
Population size	10,401.354	9,515.888	10,591.942	−1,076.06***
	(20,394.090)	(13,432.157)	(21,596.545)	(0.000)
Median standard of living	22,051.037	18,255.443	22,868.006	−4,612.56***
	(3,622.301)	(1,820.147)	(3,385.842)	(0.000)

Income-tax-paying households share	53.813	38.287	57.154	−18.867***
	(11.581)	(5.349)	(9.684)	(0.000)
Poverty rate	13.548	21.919	11.549	10.370***
	(6.637)	(6.724)	(4.799)	(0.000)
1st decile of the standard of living	12,490.081	10,288.627	12,963.926	−2,675.30***
	(2,028.982)	(1,190.405)	(1,852.082)	(0.000)
9th decile of the standard of living	37,305.031	30,769.885	38,711.668	−7,941.782***
	(8,258.044)	(4,033.257)	(8,258.437)	(0.000)
Population with a baccalaureate	1,072.911	935.159	1,102.561	−167.402***
	(2,050.931)	(1,422.667)	(2,161.291)	(0.000)
Number of other cars registered	5,764.320	4,878.615	5,954.961	−1,076.35***
	(9,469.889)	(6,085.555)	(10,040.254)	(0.000)
Observations	33,424	5,920	27,504	33,424

Values are the means with standard deviations in parentheses, and p-values are determined using t-tests. *** $p < 0.01$.

First, the differential effects of the treatment on the EV fleet levels for taxable versus non-taxable households are evaluated. Each outcome—the number of EVs at the communal level Y_{it}—is observed for a given unit (i) and time (t). Treatment is given by D_{it}. The treatment variable is a binary variable taking the value of either 0 or 1 depending on the treatment status. Because the policy was uniformly implemented in 2018, block treatment assignment is employed following Arkhangelsky et al. (2021).

The treatment variable is based on the share of taxable/non-taxable households within a commune. A commune is defined as treated if the share of non-taxable households in any time period is above or equal to 60%, and households with less than 60% non-taxable households are classified as control communes. This method is verified by examining whether other treatment assignments have similar effects.

The policy change took effect in 2018, and thus the post-treatment variable is defined to indicate communes with a high share of non-taxable households from 2018 onward, marking the post-policy period for the treatment group. The model specification is expressed as follows:

$$Y_{it} = \mu + \alpha_i + \beta_t + \sum_{k=-K}^{K} \tau_k D_{it}^k + X_{it}\gamma + \varepsilon_{it}$$

where Y_{it} is the dependent variable, representing the number of EVs at the communal level, μ represents the global intercept of the equation, and D_{it}^k is a dummy variable that indicates how many periods away from the treatment time the unit is for a given commune. The main coefficient of interest is τ, aiming to estimate the average treatment effect on the treated group $\hat{\tau}^{SE}$, which is the coefficient on $D_{it}^{(k)}$. Furthermore, α_i represents the unit fixed effects for each commune, which is necessary to control for factors in communes that remain constant over time. Similarly, β_t characterizes time-fixed effects for each year to control for time-specific factors across the communes.

The D_{it}^k term indicates how the treatment effect evolves and the impact in each year after the policy introduction. Notably, France introduced an increase in the subsidy in 2019, the year following the subsidy change. Therefore, it is crucial to understand how the subsidy change in 2018 affected that year's EV purchases. The overall result may be underestimated or obstructed because of the increase in the subsidy in the following years.

Covariates are also included, namely population size, education level, and non-EV fleet, as described in Section 4. These are represented by a vector X_{it}, and γ is the vector of their coefficients. These are characteristics that affect treatment and outcomes. Standard errors are computed using different measures to allow for unit and time weights. The model also contains placebo and bootstrap errors. The average treatment effect on the treated group (ATT; the SDID estimator) is determined by solving the following weighted least squares problem.

$$\left(\{\hat{\tau}_k^{SE}\}_k, \hat{\mu}, \hat{\alpha}, \hat{\beta}, \hat{\gamma}\right) = \arg\min_{\{\tau_k\}_k, \mu, \alpha, \beta, \gamma} \sum_{i=1}^{N} \sum_{t=1}^{T} \left(EVreg_{it} - \mu - \alpha_i - \beta_t - X_{it}\gamma - \sum_k D_{it}^{(k)} \tau_k \right)^2 \hat{w}_i^{SE} \, \hat{\lambda}_t^{SE}$$

The $\hat{w}_i^{SE}$ terms are the estimated synthetic control weights for the control communes, and the $\hat{\lambda}_i^{SE}$ terms are the synthetic time weights that are assigned by year.

Let M be the number of control communes and N be the number of treatment communes. The weight vector is defined as $\mathbf{W} = (w_1, w_2, \ldots, w_m)$, where the weights are such that the constraint $w_m \geq 0$ is satisfied and $\sum_{m=1}^{J} w_m = 1$. The same logic is applied to the time weights.

The goal is to match the pre-treatment trends of control communes (communes with a high share of taxable households) with those of the treated communes (communes with a high share of non-taxable households). Therefore, unit weights are chosen so that $\sum_{i=1}^{M} \hat{w}_i^{sdid}$, $Y_{it} \approx N^{-1} \sum_{i=M+1}^{N_{tot}} Y_{it}$ for all $t = 1, \ldots, T_{pre}$ and $i = 1, \ldots, M$.

In the same way, the time weights are chosen to balance trends in the communes with high and low shares of non-taxable households: $\sum_{t=1}^{T_{pre}} \hat{\lambda}_t^{SE}$, $EVcarpark_{it} \approx T^{-1} \sum_{t=1}^{T_{pre}} Y_{it}$ for all $i = 1, \ldots, M$.

6. Empirical Evidence

6.1 Synthetic Event Study

The event study results are reported in both a graphical and a table format. Table 4 demonstrates that the overall impact of the treatment is a decrease of approximately 9 EVs that are registered in French communes with a high share of non-taxable households (i.e., the treated group, ATT). The confidence interval is such that 0 is not included, indicating that the treatment effect is significant ($p < 0.05$). This overall result suggests that the policy introduction had a negative and significant impact on communes with a high share of non-taxable households. Moreover, the estimate for 2018 is approximately −1.65 ($p < 0.05$). This estimate reflects the effect of the subsidy reduction for new EVs/increase for secondhand EVs in that year, before the subsidy was increased again in 2019. Considering that subsequent years may be influenced by other policies, the 2018 estimate offers a particularly clear identification of the impact of the subsidy reduction.

Table 4: Synthetic Event Study Results

	(1)	(2)	(3)	(4)
ATT estimate	−9.0448**	−9.0448**	−3.5585**	−3.5585
	(0.9665)	(2.136)	(1.1323)	(3.1906)
2018 estimate	−1.6454**	−1.6454**	−0.9830	−0.9830
	(0.3733)	(0.7346)	(0.7869)	(0.5721)
Number of switchers	740	740	740	740
Controls	✓	✓	-	-
Bootstrap	✓	-	-	✓
Placebo	-	✓	✓	-

Control variables account for the population size of the commune, the number of registered non-EVs, and the number of individuals with a high school diploma. The standard error is given in parentheses below the estimate. ** $p < 0.05$.

Table 5 shows the treatment effects for the regression analysis of model 2 in the pre- and post-treatment periods. The effects in the pre-treatment years are close to 0, demonstrating that the two groups are comparable. Moreover, the confidence intervals indicate that 2017 may have borderline significant effects, whereas 2014, 2015, and 2016 have distinctly nonsignificant effects. Overall, these values are relatively close to 0, which supports the assumption of no pre-treatment differences between the two groups. The complete tables with all the estimates for models 2, 3, and 4 can be found in Appendix A.

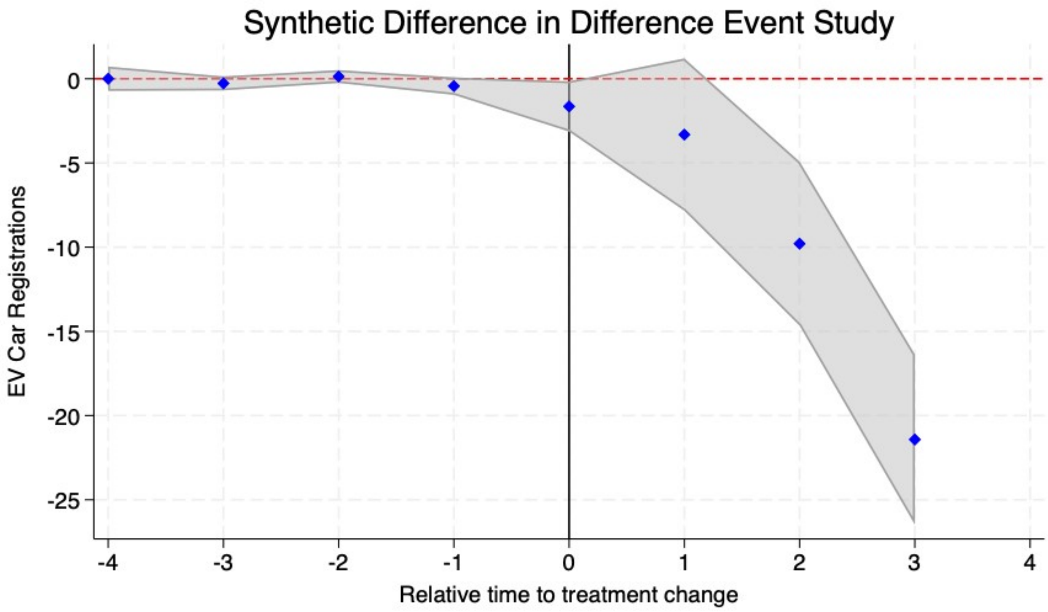
The post-treatment effects are also shown in Table 5, demonstrating how the effects of the policy on the treatment and control communes evolve. Here, the primary year of interest is 2018 (effect 1). The effect becomes larger over the years, reaching −21.4 in 2021 (effect 4), and is significantly different after 2020 (effect 3). The results are plotted in Figure 3, and the gray area represents the confidence intervals.

Table 5: Estimates and Confidence Intervals with All Controls and Placebo Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

Estimate	Coef.	SE	LB CI	UB CI	Switchers
ATT	−9.0448	2.1362	−13.2318	−4.8579	740
Effect 1	−1.6454	0.7346	−3.0852	−0.2056	740
Effect 2	−3.3178	2.4537	−8.127	1.4914	740
Effect 3	−9.794	2.8219	−15.325	−4.263	740
Effect 4	−21.4222	3.0446	−27.3896	−15.4549	740

Placebo 1	−0.4367	0.2536	−0.9338	0.0604	740
Placebo 2	0.1324	0.2448	−0.3473	0.6121	740
Placebo 3	−0.2748	0.3249	−0.9116	0.3619	740
Placebo 4	0.0	0.2577	−0.5051	0.5051	740

Figure 3: Synthetic Event Study Graphical Results



To validate the estimated ATT values, a series of robustness checks and alternative specifications are implemented. These include adjustments to methodological choices, such as alternative treatment assignments and excluding specific years and communes from the analysis. Additionally, a placebo test is conducted by randomly assigning treatment status to control units, thereby assessing whether observed effects may be attributable to unobserved factors. The main specification is also re-estimated using the number of non-EVs in the fleet as the dependent variable to further evaluate outcome specificity. Moreover, a dummy variable is implemented to account for different urbanization levels, and an SDID design is employed to estimate the ATT. This method produces similar estimates and reinforces the main findings of the synthetic event study.

As a first robustness check, the definition of treatment status for communes is varied. The baseline results presented in the previous section rely on a definition in which treatment communes are those that exhibited a share of non-taxable households exceeding 60%. In this specification, treatment is instead defined based on the share of non-taxable households in 2018, the year of policy implementation. In other words, communes are treated if their share of non-taxable households surpasses the 60% threshold in 2018. This alternative definition addresses potential concerns that the baseline definition may be confounded by unobserved factors occurring pre-treatment.

The same synthetic event study methodology is applied using this revised treatment assignment. As shown in Table 7, the estimated ATT remains negative (−9.414) and significant ($p < 0.05$) under specifications employing both the placebo (model 1) and bootstrap-based inference (model 2). Under this stricter pre-policy definition, the results are similar to those of the baseline treatment definition, supporting the validity of the main findings.

Next, the French overseas territories are excluded from the analysis to assess their potential influence on the estimated treatment effect. The exclusion accounts for potential cultural, economic, and demographic differences between mainland France and overseas territories. The latter are poorer on average and exhibit a higher incidence of non-taxable households. The excluded territories include Guadeloupe, Martinique, French Guiana, Réunion, Saint Pierre and Miquelon, Mayotte, Wallis and Futuna, French Polynesia, and New Caledonia. Table 6 compares the key characteristics of mainland and overseas communes in France.

Table 6: Balance Table Comparing Commune Characteristics in French Overseas Territories Versus Mainland France

	(1)	(2)	(3)	(4)
Variable	All samples	French overseas territories	Mainland France	Difference
Population size	10,401.354	13,406.481	10,381.807	3,024.675**
	(20,394.090)	(15,111.218)	(20,422.701)	(0.030)
Median standard of living	22,051.037	17,708.725	22,079.281	−4,370.56***
	(3,622.301)	(2,651.044)	(3,610.743)	(0.000)
Share of tax households paying income tax	53.813	33.574	53.944	−20.370***
	(11.581)	(9.003)	(11.480)	(0.000)
Poverty rate	13.548	30.222	13.429	16.793***
	(6.637)	(6.975)	(6.483)	(0.000)
1st decile of the standard of living	12,490.081	8,618.080	12,515.267	−3,897.19***

	(2,028.982)	(938.281)	(2,009.898)	(0.000)
9th decile of the standard of living	37,305.031	35,542.242	37,316.500	−1,774.26***
	(8,258.044)	(5,931.145)	(8,269.871)	(0.002)
Population with a baccalaureate	1,072.911	1,402.753	1,070.766	331.99**
	(2,050.931)	(1,898.930)	(2,051.736)	(0.018)
Number of other cars registered	5,764.320	7,238.421	5,754.732	1,483.69**
	(9,469.889)	(8,348.706)	(9,476.107)	(0.022)
Observations	33,424	216	33,208	33,424

Values are the means with standard deviations in parentheses, and p-values were determined using t-tests. ** $p < 0.05$, *** $p < 0.01$.

Table 7 (models 3 and 4) shows that the estimated ATT remains negative (−8.954) and significant ($p < 0.05$) after excluding the French overseas territories. The results are similar to the original model with all the communes, suggesting that the original treatment effect is not disproportionately impacted by the communes from the overseas territories.

According to Figure 3, the lambda weight used in constructing the synthetic control is particularly high for 2014, indicating that 2014 strongly influences the estimated time trend. By excluding all observations from 2014, a robustness check is conducted to assess whether the main results are disproportionately influenced by this single year. As shown in Table 7 (models 5 and 6), the estimated effects remain negative (−4.884) and significant ($p < 0.05$) after removing 2014 from the dataset. However, the effect for 2018 is not significant anymore. Overall, this supports the conclusion that the observed treatment effect is not only driven by effects specific to that year (i.e., 2018).

Table 7: Methodological Robustness Checks for the Synthetic Event Study

	Treatment Choice		Excluding Overseas Territories		Excluding Observations from 2014	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT estimate	−9.414**	−9.414**	−8.954**	−8.954**	−4.884**	−4.884**
	(2.062)	(0.921)	(2.128)	(0.966)	(2.347)	(1.747)
2018 estimate	−1.738**	−1.738**	−1.696**	−1.696**	−0.178	−0.178
	(0.718)	(0.404)	(0.733)	(0.377)	(0.772)	(0.474)
Number of switchers	753	753	717	717	740	740

Controls	✓	✓	✓	✓	✓	✓
Bootstrap	-	✓	-	✓	-	✓
Placebo	✓	-	✓	-	✓	-

Control variables account for the population size of the commune, the number of registered non-EVs, and the number of individuals with a high school diploma. ** $p < 0.05$.

A variable capturing the level of urbanization is included in the model to account for potential heterogeneity related to EV charging infrastructure or public transportation availability. Controlling for urbanization mitigates the risk of including systematic differences between communes, which may bias the estimates.

The urbanization measure is derived from the "grille de densité" classification provided by INSEE and is constructed using data from the Fideli 2018 dataset. The categorical variable classifies communes into seven categories: large urban centers, intermediate urban centers, small towns, urban peripheries, rural market towns, rural areas with dispersed housing, and rural areas with very dispersed housing.

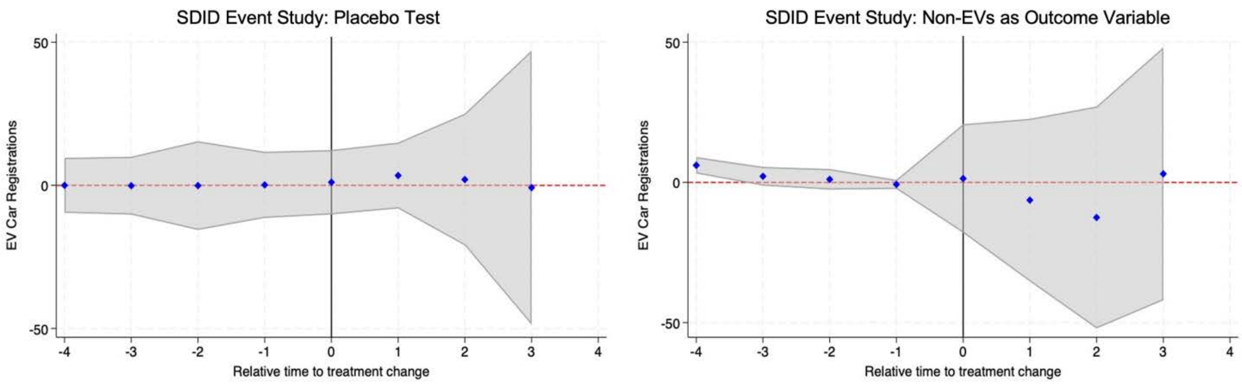
The variable is formulated as a set of six dummy variables, with large urban centers serving as the reference category. The model specification remains the same as before except for the inclusion of the $\sum_{d=1}^{6} \delta_d W_{it}^d$ term, which accounts for the effects of different urbanization levels.

$$Y_{it} = \mu + \alpha_i + \beta_t + \sum_{k=-K}^{K} \tau_k D_{it}^k + X_{it}\gamma + \sum_{d=1}^{6} \delta_d W_{it}^d + \varepsilon_{it}$$

Table 8 (models 3 and 4) presents the results of the estimation using placebo and bootstrap estimation methods. Both estimates are negative (−9.023) and significant ($p < 0.05$). This suggests that incorporating urbanization into the estimation does not greatly influence the estimated effect, validating the results of the baseline model.

Placebo inference is conducted following the approach reported by Clarke et al. (2024). In this procedure, placebo treatment is randomly assigned to control units while initially treated units are excluded from the analysis. Herein, treatment is randomly assigned to communes with a low share of non-taxable households, excluding those with a high share. This approach helps clarify whether the observed treatment effect is attributable to the actual policy intervention or driven by unobserved confounding factors. The results of the placebo estimation are presented in Table 8 (models 3 and 4), indicating that the ATT estimate (1.43) is neither negative nor significant. These results show that randomly assigning treatment does not generate a discernible effect.

Figure 4: Graphical Results for the Synthetic Event Study



Finally, the outcome variable is changed to the fleet of all non-EVs registered in a commune. The policy effects should be specific to EV adoption; thus, there should be no effect on the fleet of other vehicles. Table 8 (models 5 and 6) and Figure 4 demonstrate that the ATT estimate (−3.602) is not significant and has a very large standard error. Thus, the original findings are not driven by broad shifts in the general vehicle fleet. This reinforces the main conclusions.

Table 8: Robustness Checks for the Synthetic Event Study

	Urbanization extension		Placebo test		Non-EVs as outcome variable	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT Estimate	−9.023**	−9.023**	1.436	1.436	−3.602	−3.602
	(2.06)	(0.967)	(12.060)	(1.878)	(16.360)	(8.135)
Effect 2018 Estimate	−1.608**	−1.608**	1.088	1.088	1.376	1.376
	(0.713)	(0.362)	(4.907)	(0.650)	(10.493)	(4.761)
Number of switchers	740	740	1711	1711	740	740
Controls	✓	✓	✓	✓	✓	✓
Bootstrap	-	✓	-	✓	-	✓
Placebo	✓	-	✓	-	✓	-

Control variables account for the population size of the commune, the number of registered non-EVs, and the number of individuals with a high school diploma. ** $p < 0.05$.

6.2 Synthetic Difference-in-Differences Estimator

Table 9 shows the results of the SDID estimation. The model is run using two different variance-covariance estimation methods to estimate the standard errors using placebo testing and bootstrapping.

Using both methods, negative ATT estimates of around −9.175 ($p < 0.01$) are obtained, indicating that the effect of the reduction in subsidies on new EVs/increase in subsidies on secondhand EVs in 2018 led to a relative decline in the EV fleet in communes with a higher share of non-taxable households compared with that in communes with a lower share of non-taxable households. The ATT captures the causal impact of the 2018 policy on treated communes—those with a high proportion of non-taxable households. The negative value demonstrates that their EV fleet was lower than that of their synthetic counterparts—communes with a low proportion of non-taxable households. Ultimately, the equity-based change in subsidy disproportionately affected regions with a higher concentration of non-taxable households.

Even though the subsidy on new EVs was decreased by the same amount for non-taxable and taxable households, and the subsidy for secondhand EVs increased by €1000 more for non-taxable households, the bonus for non-taxable households on secondhand EVs was insufficient to maintain the same level of EV adoption between communes with lower and higher shares of non-taxable households. Therefore, even though the policy attempted to address equity by reducing the subsidy for poorer households, it did not manage to provide more equity or sufficiently motivate EV uptake in communes with poorer households.

Table 9: Synthetic Difference-in-Difference Estimates

	Model (Placebo)	Model (Bootstrap)
ATT	−9.175***	−9.175***
	(2.866)	(1.068)
N	33,424	33,424

The unit of observation is a commune. The standard error is shown in parentheses below the estimate. *** $p < 0.01$.

Figure 5: Synthetic Difference-in-Differences Results for the Estimated Mean EV Fleet

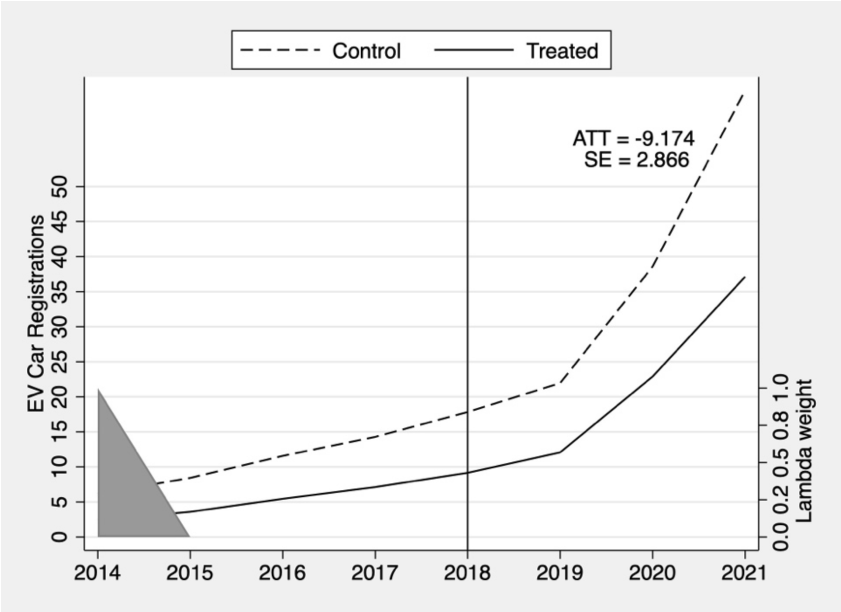


Figure 5 plots the estimated EV fleet for the synthetic control group (dotted line) and the treatment group (solid line) for 2014–2021, showing the mean values. The black vertical line signifies the year of interest, when the equity-based change in the subsidy policy was introduced.

The synthetic treatment and control groups followed similar trends until 2018. At this time, the model predicts a slight increase in EVs for the synthetic control group (taxable households) and a lower increase for the treatment group (non-taxable households). The widening of the gap between the synthetic control and the treated groups highlights the differential effect of the subsidy.

In 2020, the graph shows an increase in EVs for the control and treatment groups but a lower rate of increase for the treatment group. This aligns with another policy change introduced by the French government in 2019. The maximum combined subsidies were increased for non-taxable households up to €11,000, whereas taxable households did not receive a subsidy increase for new or secondhand EVs.

7. Conclusion

This study analyzes how France's 2018 policy (i.e., decreasing the purchase subsidy for new EVs while slightly raising the subsidy for secondhand EVs) differentially affected the purchase behavior of taxable and non-taxable households. Overall, the results from both estimation methods, namely the synthetic event study and the SDID approach, support the hypothesis that the 2018 decrease in subsidy level for new vehicles had a negative impact on the EV fleet for non-taxable households compared with that for taxable households.

From the synthetic event study, the causal effect of the policy change on the treated group is –9.045 EVs in the fleet compared with the control group. This result is supported by the SDID study, showing an ATT of −9.175. Both methods, therefore, highlight that the subsidy change had a more negative impact on EV purchases among the non-taxable households than the taxable households. A decrease of €1500 is a smaller proportion of a taxable household's income than a non-taxable household's income. Thus, a lower-income (non-taxable) household is less likely to purchase an EV following this policy change, even though they could potentially receive €1000 more than a higher-income (taxable) household in secondhand EV purchases. This is consistent with the general difference observed in EV purchasing behavior in previous studies, showing that lower-income individuals are less likely to buy an EV.

Moreover, French communes with a higher share of non-taxable households have a lower prevalence of EVs, as visualized in Figure 5. This effect may also be related to differences in educational backgrounds between lower- and higher-income communes. In this analysis, differences in educational background and population size are controlled.

The internal validity of the study is supported by deliberate methodological choices, careful selection of covariates, and the incorporation of an event study design. Robustness checks include alternative definitions of the treatment variable, exclusion of overseas communes that may bias the results, removal of observations from 2014, and the implementation of a placebo test.

The main estimates originate from a regression analysis in which communes are classified as treated if, at any point during the study period, more than 60% of households were non-taxable. As a robustness check, treatment is instead assigned based on whether the share of non-taxable households exceeded 60% in 2018, the year that the policy of interest was introduced. The results show that the treatment effect remains similarly negative and significant. Varying the treatment definition demonstrates that the results are not endogenous to trends in the post-treatment period.

As another check, French overseas territories are excluded from the sample. The overseas territories differ demographically and economically from mainland France, generally exhibiting a lower standard of living, a higher poverty rate, and higher population density. Thus, the paper removes them from the sample to ensure that the results are not driven by these communes.

Observations from 2014 are excluded to assess their influence on the results. The lambda weight of 2014 was especially high, indicating a strong impact on the synthetic control group's time trend. Excluding 2014, the estimated ATT is –4.884, which is less negative than the original estimate of –9.045. However, the effect remains both strongly negative and significant.

Finally, a placebo test demonstrates that randomly assigning treatment to communes does not lead to significant or negative ATT values. Thus, the pre-treatment and post-treatment effects are not significant and are close to 0. Moreover, the estimated ATT is also close to 0 and insignificant, thereby supporting the conclusion. Overall, these robustness checks support the main results showing that the policy introduction led to a differential EV uptake rate, which was lower for communes with a high share of non-taxable households.

The main limitation of this study is the limited data availability, constraining the resolution to the communal level, including data on the education level, population size, and EV fleet. The policy was implemented at the household level; however, owing to data privacy issues, it is impossible to identify the number of EVs at the household level. Nonetheless, the differences in the share of non-taxable households among the communes can be leveraged to identify wealthier and poorer communes, thereby elucidating how the EV fleet is influenced by differences in socioeconomic composition at the communal level. In the future, this study can be improved by using individual-level data sourced from the French SDES and Filosofi datasets after an application process.

Another limitation relates to the potential issue of unobservable factors influencing EV adoption. For instance, differences in political stances and charging infrastructure across communes may introduce bias into the results. Meanwhile, additional covariates, such as communal political alignment, the number of charging stations, public transport accessibility, and road density, could be included to improve the balance between the treated and control groups. However, the potential issue of unobservable factors is addressed by testing for pre-treatment differences between the treated and synthetic control groups using a placebo test and an event study. These checks provide evidence that the synthetic control is a good counterfactual, making it unlikely that unobserved factors are driving the changes in EV uptake.

As a proxy for charging infrastructure and public transport, which are more accessible in larger cities, a measure of urbanization is included in the robustness checks. Similar results are obtained with and without this variable.

The present study is generalizable to the extent that the French context is similar to other European contexts, such as Germany, where similar EV subsidy policies have been introduced with similar "progressive" aspects. Moreover, the level of subsidies for EVs has generally decreased throughout Europe. Thus, the results of this study, which confirm that a decrease in the subsidy level impacts lower-income households more than higher-income households, may be valuable for policymakers who want to make subsidy reductions more equitable in the long run.

Furthermore, data limitations affect the external validity of the findings. The French dataset on communal characteristics, such as size, population, income, and share of non-taxable households, does not include data for very small communes with less than 100 individuals; hence, this study cannot cover the effects of policy on very small communes and cannot be generalized to them.

Another limitation regarding the external validity of the results originates from the fact that the EV market is quickly evolving, and the prices of EVs are decreasing. As such, the increased affordability may impact the necessity and effect of EV subsidies and diminish the differential impact of a subsidy reduction. Similarly, charging infrastructure is gradually being built across Europe, thereby changing infrastructure availability.

Finally, the study's external validity is supported by the post-treatment trend observed in the synthetic event study. After 2019, the onset of COVID-19 and the fuel price shocks caused by the Ukraine–Russia war disproportionately affected the lower-income communes, exacerbating structural economic problems. Despite these disruptions, the increasing trend of the EV fleet remained stable. Notably, the event study results demonstrate the direct effect of the 2018 subsidy change during the pre-COVID period in 2019 and 2020 (results for 2020 are from January). Thus, these estimates are not confounded by pandemic or war-related shocks, strengthening the credibility of the findings and indicating that the observed effects are strongly attributed to the policy change rather than other external factors.

A Appendix A: Complete Tables for Main Model Specifications

Table A1: Estimates and Confidence Intervals with Controls and Bootstrap Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

Estimate	Coef.	SE	LB CI	UB CI	Switchers
ATT	−9.0448	0.9665	−10.9393	−7.1504	740
Effect 1	−1.6454	0.3733	−2.3771	−0.9137	740
Effect 2	−3.3178	0.9841	−5.2467	−1.3888	740
Effect 3	−9.7940	1.3518	−12.4436	−7.1443	740
Effect 4	−21.4222	1.6600	−24.6759	−18.1686	740
Placebo 1	−0.4367	0.2824	−0.9902	0.1167	740
Placebo 2	0.1324	0.2622	−0.3815	0.6463	740
Placebo 3	−0.2748	0.3396	−0.9404	0.3907	740
Placebo 4	0.0000	0.1839	−0.3605	0.3605	740

Table A2: Estimates and Confidence Intervals without Controls and Bootstrap Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

Estimate	Coef.	SE	LB CI	UB CI	Switchers
ATT	−3.5585	1.1323	−5.7778	−1.3392	740
Effect 1	−0.983	0.5721	−2.1043	0.1383	740
Effect 2	−2.115	0.6471	−3.3834	−0.8467	740
Effect 3	−3.5987	1.3952	−6.3333	−0.8641	740
Effect 4	−7.5372	2.2291	−11.9062	−3.1683	740
Placebo 1	−0.2272	0.4921	−1.1917	0.7373	740
Placebo 2	0.198	0.4272	−0.6394	1.0354	740
Placebo 3	0.3477	0.367	−0.3721	1.0675	740
Placebo 4	0.6514	03154	0.0332	1.2697	740

Table A3: Estimates and Confidence Intervals without Controls, Placebo Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

Estimate	Coef.	SE	LB CI	UB CI	Switchers
ATT	−3.5585	3.1906	9.8121	2.6951	740
Effect 1	−0.983	0.7869	−2.5253	0.5593	740
Effect 2	−2.115	2.5872	−7.1859	2.9558	740
Effect 3	−3.5987	4.0543	−11.5452	4.3478	740
Effect 4	−7.5372	5.6349	−18.5817	3.5073	740
Placebo 1	−0.2272	0.0741	−0.3723	−0.0821	740
Placebo 2	0.198	0.2067	−0.2072	0.6032	740
Placebo 3	0.3477	0.1965	−0.0375	0.7328	740
Placebo 4	0.6514	0.0554	0.5428	0.7601	740

B Appendix B: Robustness Checks Complete Tables

B.1 Treatment Definition Change: Regression Results

Table B4: Estimates and Confidence Intervals with Controls and Bootstrap Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

	Estimate	SE	LB CI	UB CI	Switchers
ATT	−9.414	0.921	−11.219	−7.609	753
Effect 1	−1.738	0.404	−2.531	−0.946	753
Effect 2	−3.495	0.914	−5.287	−1.702	753
Effect 3	−10.308	1.278	−12.813	−7.803	753
Effect 4	−22.115	1.616	−25.283	−18.947	753
Placebo 1	−0.487	0.332	−1.137	0.164	753
Placebo 2	0.110	0.321	−0.519	0.739	753
Placebo 3	−0.309	0.419	−1.129	0.512	753
Placebo 4	0.000	0.240	−0.471	0.471	753

Table B5: Estimates and Confidence Intervals with Controls, Placebo Standard Errors and Confidence Intervals, with 2000 Bootstrap Replications

	Estimate	SE	LB CI	UB CI	Switchers
ATT	−9.414	2.062	−13.456	−5.372	753
Effect 1	−1.738	0.718	−3.146	−0.331	753
Effect 2	−3.495	2.349	−8.099	1.109	753
Effect 3	−10.308	2.702	−15.603	−5.012	753
Effect 4	−22.115	2.972	−27.941	−16.289	753
Placebo 1	−0.487	0.250	−0.977	0.004	753
Placebo 2	0.110	0.242	−0.363	0.584	753
Placebo 3	−0.309	0.312	−0.921	0.304	753
Placebo 4	0.000	0.270	−0.528	0.528	753

B.2 Excluding French Overseas Territories: Regression Results

Table B6: Estimates and Confidence Intervals with Controls and Urbanization Dummy and Bootstrap Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

	Estimate	SE	LB CI	UB CI	Switchers
ATT	−8.9543	0.9662	−10.8481	−7.0605	717
Effect 1	−1.6955	0.3765	−2.4334	−0.9576	717
Effect 2	−3.3454	0.9805	−5.2672	−1.4237	717
Effect 3	−9.6194	1.3662	−12.2971	−6.9417	717
Effect 4	−21.1569	1.6526	−24.396	−17.9178	717
Placebo 1	−0.4538	0.2881	−1.0184	0.1109	717
Placebo 2	0.1383	0.276	−0.4026	0.6792	717

Placebo 3	−0.2907	0.35	−0.9767	0.3952	717
Placebo 4	0	0.1997	−0.3913	0.3913	717

Table B7: Estimates and Confidence Intervals with Controls, Placebo Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

	Estimate	SE	LB CI	UB CI	Switchers
ATT	−8.9543	2.1284	−13.126	−4.7826	717
Effect 1	−1.6955	0.7327	−3.1315	−0.2595	717
Effect 2	−3.3454	2.4859	−8.2178	1.5269	717
Effect 3	−9.6194	2.7911	−15.0899	−4.1489	717
Effect 4	−21.1569	3.0163	−27.0689	−15.245	717
Placebo 1	−0.4538	0.2542	−0.9521	0.0446	717
Placebo 2	0.1383	0.2505	−0.3528	0.6293	717
Placebo 3	−0.2907	0.3257	−0.9291	0.3476	717
Placebo 4	0	0.2712	−0.5316	0.5316	717

B.3 Excluding Observations from 2014: Regression Results

Table B8: Estimates and Confidence Intervals with Controls and Bootstrap Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

	Estimate	SE	LB CI	UB CI	Switchers
ATT	−4.884	1.747	−8.309	−1.459	740
Effect 1	−0.178	0.474	−1.108	0.751	740
Effect 2	0.627	2.047	−3.386	4.64	740
Effect 3	−4.617	2.392	−9.305	0.071	740
Effect 4	−15.368	2.415	−20.101	−10.635	740
Placebo 1	−0.004	0.067	−0.136	0.127	740
Placebo 2	0.085	0.118	−0.146	0.316	740
Placebo 3	0.001	0.036	−0.069	0.07	740

Table B9: Estimates and Confidence Intervals with Controls, Placebo Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

	Estimate	SE	LB CI	UB CI	Switchers
ATT	−4.8841	2.3469	−9.4841	−0.2841	740
Effect 1	−0.1783	0.7723	−1.692	1.3355	740
Effect 2	0.627	2.729	−4.7217	5.9758	740
Effect 3	−4.6172	3.1147	−10.7221	1.4877	740
Effect 4	−15.3681	3.2681	−21.7735	−8.9627	740
Placebo 1	−0.0043	0.1314	−0.2619	0.2533	740
Placebo 2	0.0846	0.1818	−0.2717	0.4409	740
Placebo 3	0.0007	0.0733	−0.143	0.1445	740

B.4 Urbanization Extension: Regression Results

Table B10: Estimates and Confidence Intervals with Controls and Urbanization Dummy, Bootstrap Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

	Estimate	SE	LB CI	UB CI	Switchers
ATT	−9.0231	0.9699	−10.9242	−7.1220	740
Effect 1	−1.6077	0.3618	−2.3169	−0.8985	740
Effect 2	−3.3302	1.0319	−5.3528	−1.3076	740
Effect 3	−9.7818	1.3464	−12.4207	−7.1430	740
Effect 4	−21.3726	1.6245	−24.5566	−18.1887	740
Placebo 1	−0.3956	0.2438	−0.8735	0.0823	740
Placebo 2	0.1583	0.2230	−0.2789	0.5955	740
Placebo 3	−0.2173	0.2895	−0.7846	0.3501	740
Placebo 4	0.0000	0.1423	−0.2789	0.2789	740

Table B11: Estimates and Confidence Intervals with Controls and Urbanization Dummy, Placebo Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

	Estimate	SE	LB CI	UB CI	Switchers
ATT	−9.0231	2.0599	−13.0605	−4.9856	740
Effect 1	−1.6077	0.7129	−3.0049	−0.2105	740
Effect 2	−3.3302	2.3695	−7.9743	1.3139	740
Effect 3	−9.7818	2.7170	−15.1072	−4.4565	740
Effect 4	−21.3726	2.9664	−27.1867	−15.5586	740
Placebo 1	−0.3956	0.2496	−0.8848	0.0937	740
Placebo 2	0.1583	0.2356	−0.3034	0.6200	740
Placebo 3	−0.2173	0.3210	−0.8465	0.4120	740
Placebo 4	0.0000	0.2509	−0.4918	0.4918	740

B.5 Placebo Test: Regression Results

Table B12: Estimates and Confidence Intervals with Controls and Bootstrap Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

	Estimate	SE	LB CI	UB CI	Switchers
ATT	1.436	1.878	−2.246	5.118	1711
Effect 1	1.088	0.65	−0.186	2.363	1711
Effect 2	3.431	2.208	−0.897	7.759	1711
Effect 3	2.025	2.471	−2.817	6.868	1711
Effect 4	−0.801	2.717	−6.125	4.524	1711
Placebo 1	0.162	0.31	−0.447	0.77	1711
Placebo 2	−0.089	0.312	−0.701	0.523	1711
Placebo 3	−0128	0.374	−0.862	0.606	1711

Placebo 4	0	0.185	−0.364	0.364	1711

Table B13: Estimates and Confidence Intervals with Controls, Placebo Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

	Estimate	SE	LB CI	UB CI	Switchers
ATT	1.436	12.06	−22.202	25.074	1711
Effect 1	1.088	4.907	−8.529	10.705	1711
Effect 2	3.431	11.021	−18.171	25.033	1711
Effect 3	2.025	16.842	−30.985	35.036	1711
Effect 4	−0.801	25.483	−50.746	49.145	1711
Placebo 1	0.162	6.971	−13.501	13.825	1711
Placebo 2	−0.089	8.173	−16.107	15.929	1711
Placebo 3	−0.128	5.06	−10.045	9.79	1711
Placebo 4	0	3.97	−7.781	7.781	1711

B.6 Non-EVs as the Outcome Variable: Regression Results

Table B14: Estimates and Confidence Intervals with Controls and Bootstrap Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

	Estimate	SE	LB CI	UB CI	Switchers
ATT	−3.602	8.135	−19.547	12.342	740
Effect 1	1.376	4.761	−7.954	10.707	740
Effect 2	−6.325	7.612	−21.245	8.595	740
Effect 3	−12.514	10.072	−32.255	7.226	740
Effect 4	3.053	12.86	−22.152	28.258	740
Placebo 1	−0.707	1.105	−2.873	1.46	740
Placebo 2	1.087	1.531	−1.914	4.087	740
Placebo 3	2.181	1.982	−1.704	6.066	740
Placebo 4	6.114	2.235	1.733	10.494	740

Table B14: Estimates and Confidence Intervals with Controls, Placebo Standard Errors and Confidence Intervals with 2000 Bootstrap Replications

	Estimate	SE	LB CI	UB CI	Switchers
ATT	−3.602	16.36	−35.668	28.463	740
Effect 1	1.376	10.493	−19.191	21.943	740
Effect 2	−6.325	15.555	−36.813	24.164	740
Effect 3	−12.514	24.559	−60.65	35.622	740
Effect 4	3.053	24.659	−45.278	51.385	740
Placebo 1	−0.707	0.864	−2.4	0.986	740
Placebo 2	1.087	1.712	−2.269	4.442	740
Placebo 3	2.181	1.959	−1.659	6.02	740
Placebo 4	6.114	1.309	3.548	8.679	740

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