STOCKHOLM SCHOOL OF ECONOMICS

Department of Economics

5350 Master's Thesis in Economics

Academic Year 2024-2025

Unplugging Incentives: Evidence from Sweden's EV Policy Reversal

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Abstract: This study examines how mature electric vehicle markets respond to the removal of financial incentives, using Sweden as a case study. Between 2022 and 2024, Sweden implemented three major changes to policies affecting vehicle electrification: cutting fuel taxes, discontinuing the EV subsidy program, and reducing biofuel blend-in requirements. Using data from 15 European OECD countries, I construct a synthetic control for Sweden and estimate the effects of these policy changes in a Synthetic Difference-In-Differences framework. I find no significant reduction in battery electric vehicle registration rates following these policy changes. Furthermore, I show that at least one major EV supplier largely absorbed the subsidy removal through price adjustments. The results indicate that gasoline prices and urbanization levels remain significant determinants of EV adoption rates. These findings contribute to how mature EV markets might function without government support and suggest that the effectiveness of financial incentives may diminish as markets develop beyond early adoption phases.

Keywords: Electric Vehicle Adoption, Environmental Policy, Policy Incentives, Transport Electrification, Synthetic Difference-in-Difference

JEL: Q48, R48, H23

Supervisor: Julius Andersson

Date submitted: 9 December 2024

Date examined: 11 December 2024

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Examiner: Elena Paltseva

Acknowledgments

First and foremost, I would like to express my sincere gratitude to my supervisor, Julius Andersson, for providing invaluable guidance and patience throughout this project. I would also like to thank Saloni Drolia for continued encouragement and insightful comments throughout this process. Finally, I am deeply grateful to my partner, who has been an endless source of support along the way.

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1 Introduction

As governments worldwide have intensified financial incentives for electric vehicle (EV) adoption, Sweden has instead taken steps to dismantle key policies that helped make it a front-runner in vehicle electrification (International Energy Agency, 2024a). Since 2022, the Swedish government has made historical cuts in transport fuel taxation, abolished the EV subsidy program, and reduced the biofuel blend-in requirement for fuel producers (Pareliussen & Purwin, 2023). While extensive research has explored the effects of implementing financial incentives for EV adoption, less attention has been given to the impact of abolishing such policies. In this study, I estimate the impact of Sweden's new transport policy package on the registration rate of new battery electric vehicles (BEVs).

A significant transition toward electric vehicles presents an opportunity to greatly reduce greenhouse gas (GHG) emissions. According to the most recent IPCC assessment report, the transport sector accounted for 15 percent of global GHG emissions in 2019, of which road transport contributed 69 percent (IPCC, 2022). The transport sector currently accounts for one-third of Sweden's GHG emissions and remains one of its most challenging sectors to decarbonize. Despite a target of reducing transport emissions by 70 percent from 2010 levels by 2030, by 2023, emissions had decreased by only 34 percent — requiring future reductions to accelerate from the current 0.59 million yearly average to over 1 million tonnes of CO_2 equivalents annually to meet the goal (Naturvårdsverket, 2024).

The decarbonization of the transport sector has become a central focus of recent climate policies worldwide. For example, The U.S. Inflation Reduction Act notably targeted transport emissions through substantial EV purchase incentives (Allcott et al., 2024), yet the future of these policies now faces political uncertainty. In this context, Sweden's experience with dismantling EV incentives offers valuable insights into the effectiveness of financial incentives as tools for promoting transport electrification. As countries seek to develop and sustain climate policies, understanding the implications of removing such incentives becomes increasingly important. Given that vehicles entering the fleet today typically remain in use for at least a decade (Trafikanalys, 2024), the current rates of adoption will have a significant impact on emissions outcomes well into the future.

Between 2022 and 2024, the Swedish government implemented three major changes to its transport sector policy, which directly affected financial incentives for EV purchases. First, in May 2022, the Swedish government broke with three decades of increasing fuel taxation by introducing unprecedented cuts that temporarily reduced fuel taxes by over 20 percent (Prop. 2021/22:84; Skatteverket, 2023). Second, in November 2022, the Swedish government announced the immediate discontinuation of the EV subsidy, citing that the cost of owning and operating a low-emission vehicle was now comparable to that of conventional gasoline or diesel vehicles (Swedish Government, 2022b). The EV subsidy had been in place since 2018 and had until then entitled consumers up to SEK 70,000 off the EV purchase price (SFS 2107:1334). Third, the biofuel blend-in requirement, which requires fuel producers to blend in a certain share of biofuel into their gasoline and diesel products, was reduced to the EU-minimum levels of 6 percent in January 2024 (Prop. 2023/24:28). As such, this three-part policy package affected both the up-front price and the relative operating cost of EVs compared to conventional internal combustion engine vehicles (ICEVs).

In this study, I draw on recent advancements in empirical research to estimate the overall effect of the new transport policy package on BEV adoption using a synthetic difference-in-difference (SDID) framework. This methodology offers advantages over traditional difference-in-difference (DID) approaches by reducing reliance on parallel trend assumptions while increasing robustness in small samples (Arkhangelsky et al., 2021). I construct a credible counterfactual for Sweden using a donor pool of 15 European OECD countries and, controlling for key covariates — GDP per capita, energy prices, and degree of urbanization —, estimate both the average and dynamic effects of these policy changes from May 2022 through June 2024.

Despite the substantial changes to purchase and operating costs, I find that Sweden's dismantling of EV incentives had no significant average impact on BEV registration rates from May 2022 through December 2023. The dynamic analysis shows a statistically significant temporary increase in BEV registrations coinciding with the removal of the EV subsidy – likely reflecting a delivery delay between vehicle sales and registrations – before returning to baseline levels. Following the January 2024 reduction in the biofuel blend-in requirement, point estimates suggest a negative effect on BEV adoption, though this effect is statistically insignificant and characterized by substantial uncertainty. Moreover, higher gasoline prices and the degree of urbanization show a strong and positive association with increased BEV adoption rates.

To explain these findings, I propose two explanatory mechanisms. First, the increase in consumer purchase costs from removing financial incentives appears to have been partially absorbed by manufacturers rather than fully passed through to consumers, as evidenced by significant price reductions implemented by at least one major EV manufacturer following the policy changes. Second, the effectiveness of financial incentives likely varies substantially across geographical contexts. Through 2022, when Sweden began reducing its EV incentives, regions with higher shares of urban population showed systematically higher BEV adoption rates. This spatial heterogeneity suggests that uniform national financial incentives may have limited effectiveness in promoting widespread EV adoption, particularly in rural areas where range anxiety and charging infrastructure constraints likely remain significant barriers.

In this study, I make two main contributions to the current literature. First, I evaluate the impact of removing financial incentives for EV adoption, addressing an important gap in existing research, which has primarily focused on incentive implementation. Second, I assess this policy change in the context of a mature EV market characterized by already high EV penetration rates, broad model availability across price points, and diffusion beyond high-income early adopters — characteristics that suggest higher price elasticity of demand as middle-income households enter the market.

The remainder of this study is organized as follows. Section 2 establishes the broader context of transport sector emissions and reviews existing literature on EV adoption determinants. Section 3 details Sweden's institutional framework and recent policy changes in the transport sector. Section 4 describes the data and discusses the selection of control countries for the synthetic control donor pool. Section 5 presents the Synthetic Difference-in-Differences methodology used to estimate the effect of the policy changes. Section 6 reports the main results and examines their robustness across alternative specifications. Section 7 discusses potential mechanisms driving the results, particularly the role of manufacturer pricing strategies and regional adoption patterns. Section 8 concludes.

2 Background

Understanding the challenge of reducing transport sector emissions requires examining their position within Sweden's overall emissions landscape. The industrial sector generates the largest share of Sweden's GHG emissions, closely followed by the transport sector. While total national emissions have decreased from over 65 million tonnes of CO_2 equivalents in 1990 to shy of 45 million tonnes in 2023, Figure 1 shows that emissions in both the industrial and transport sectors have been relatively persistent over the last three decades. The persistence of transport emissions can be partly attributed to the long lifecycle of vehicles. Once a car enters the fleet, it typically remains in use for at least a decade (Trafikanalys, 2024). This longevity implies that vehicles entering the fleet today will continue to influence emissions for years, and emphasizes the importance of examining early trend shifts.

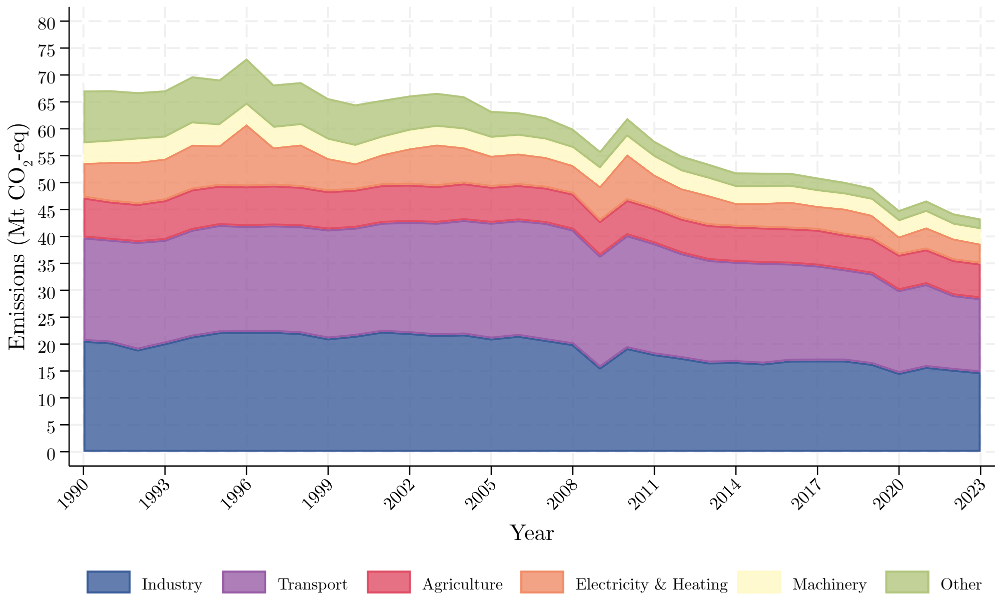


Figure 1: GHG emissions Sweden 1990 - 2023

Note: The figure plots Sweden's total yearly emission of greenhouse gases in million tonnes of CO_2-equivalents by industrial sector. Source: Naturvårdsverket (2024).

The domestic transport sector currently accounts for approximately one-third of total Swedish GHG emissions, dominated by road transport at 90 percent (Figure 2). The key driver of these emissions is passenger cars, which have consistently accounted for around two-thirds of domestic transport emissions (Naturvårdsverket, 2024). While electrification of the vehicle fleet offers a path to emissions reduction, its environmental effectiveness is conditional on the electricity used for charging being generated from renewable sources. This is largely the case in Sweden, where at least 80 percent of electricity production has been generated from renewable or nuclear sources since the 1980s (Energimyndigheten, 2023). Given both their dominant share of emissions and Sweden's clean electricity supply, reducing passenger car emissions appears to be both a relevant and feasible path for meeting Sweden's target of a 70 percent reduction in transport emissions from 2010 levels by 2030 (Naturvårdsverket, 2024; Prop. 2016/17:146).[1]

[1]Excluding domestic aviation, which is encompassed by the EU ETS.

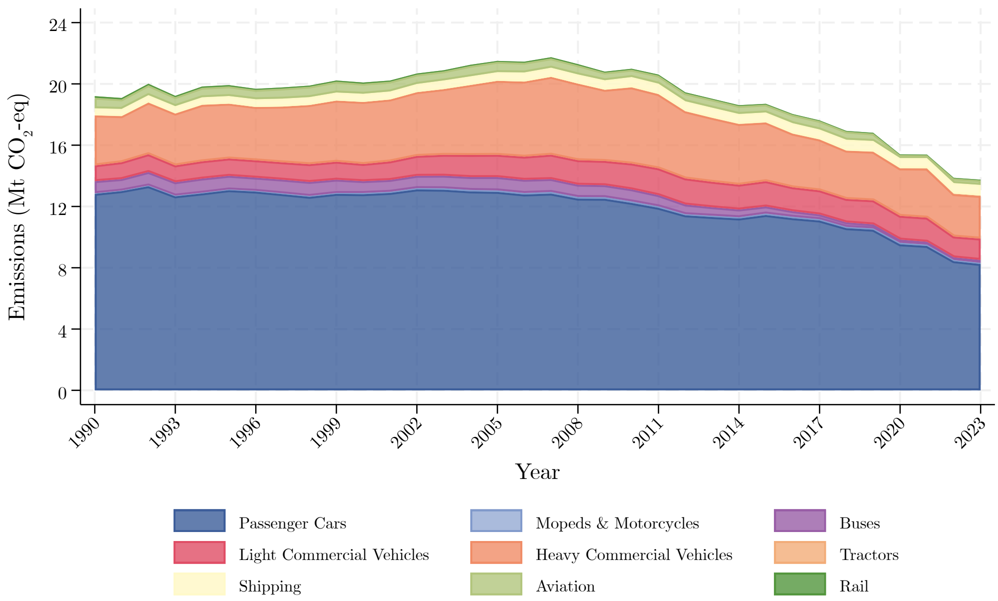


Figure 2: GHG emissions from domestic transport, Sweden 1990 - 2023

Note: The figure plots the Swedish transport sector's total yearly emission of greenhouse gases in million tonnes of CO_2 equivalents by vehicle segment. Note that tractors' share of emissions is below 0.2% throughout, and the share is therefore not visible in the graph. Source: Naturvårdsverket (2024).

While carbon pricing has traditionally been positioned as the first-best policy for emissions reduction, a growing body of literature suggests that successfully achieving broad emissions reduction requires a comprehensive policy framework that integrates carbon pricing with complementary policy instruments and considers both political feasibility and distributional effects (Axsen et al., 2020; Funke et al., 2024; Krogstrup & Oman, 2019; Stiglitz et al., 2024). Sweden is, therefore, an especially suitable case study to evaluate these effects, boasting some of the highest carbon tax rates on fuel in the world (International Energy Agency, 2024b). However, with recent policy changes towards the transport sector, Sweden may face challenges in reaching the 2030 emissions reduction target unless alternative measures are implemented to offset these changes. In particular, the extensive reduction in the biofuel blend-in requirement has raised concerns about potential adverse effects on emissions, with projections indicating an increase in emissions in 2024 (Pareliussen & Purwin, 2023; Persson et al., 2024). I outline and discuss the institutional setting of Swedish transport policy further in Section 3.

Understanding how households make vehicle purchase decisions is fundamental to analyzing the effects of policy changes on EV adoption, and the dynamics of these decisions have been widely studied in existing literature. Aggregate effects are often modeled in a discrete choice framework, in which households evaluate purchase options based on both immediate fixed costs, such as the purchase price, and factors affecting lifetime vehicle costs, such as fuel costs, insurance, maintenance, and perceived benefits of other vehicle features (Beresteanu & Li, 2011; Berry et al., 1995; Diamond, 2009; Egnér & Trosvik, 2018; Østli et al., 2017). This approach implicitly assumes that households are at least somewhat forward-looking — considering long-term ownership costs alongside the immediate purchase price. However, in a study of 86 million vehicle transactions, Allcott and Wozny (2014) find that consumers systematically undervalue future fuel savings when comparing vehicle options. Specifically, consumers are willing to pay only an additional \$0.76 in purchase price to reduce discounted future gasoline costs by \$1.00, indicating that up-front costs matter more in purchase decisions than long-term savings. Moreover, they find that vehicle markets respond to changes in gasoline prices with up to a six-month delay, suggesting the presence of sticky information in consumer decision-making. The tendency to undervalue future fuel savings relative to the up-front cost is particularly relevant when considering EVs, where purchase prices typically exceed those of conventional ICEVs, despite lower lifetime operating costs (Funke et al., 2024; International Energy Agency, 2024b). Consequently, the high up-front cost of purchasing an EV may be a considerable barrier for consumers, emphasizing the potential for EV policy incentives as mitigation tools to shift vehicle demand.

Policy tools to promote EV adoption among households can be broadly categorized into financial and non-financial incentives. Financial incentives include registration and ownership tax rebates, purchase subsidies, and VAT exemptions. As such, financial incentives can be structured to affect either the up-front purchase price or the lifetime variable costs of owning and operating an EV. Non-financial incentives encompass other benefits such as free parking and access to carpool lanes. In this study, I focus specifically on financial incentives, as the recent changes implemented by the Swedish government directly affect the costs of EV ownership and operation.

Financial incentives have been shown to increase EV adoption, although the evidence for the extent of their effectiveness has been mixed. Over the past decade, various countries have implemented a range of financial incentives to promote EV adoption.[2] Studies using U.S. data demonstrate notable positive effects: Wee et al. (2018) found that a \$1,000 increase in state-level incentives yielded a $5 - 11$ percent increase in EV registration rates, while Clinton and Steinberg (2019) estimated an 8 percent increase in BEV registrations for the same incentive increase. Overall, Clinton and Steinberg (2019) find that, between 2011 and 2015, these purchase incentives were associated with an 11 percent total increase in U.S. BEV registrations. However, cross-country evidence suggests more modest effects. In a study of 30 countries' EV adoption rates in 2012, Sierzchula et al. (2014) conclude that a \$1,000 increase in incentives only resulted in a 0.06 percent increase in the global EV market share. Instead, Sierzchula et al. (2014) emphasize the importance of charging infrastructure over financial incentives alone and found that an increase of one charging station per 100,000 residents is associated with an increase of 0.12 percent in the EV market share. Importantly, Langbroek et al. (2016) find that the effectiveness of financial incentives may diminish as EV adoption progresses, with consumers in later stages of adoption being less responsive to subsidies. This suggests that the role of financial incentives may need to adapt as markets mature. The implications are particularly relevant to Sweden's case, as its EV market has already evolved well beyond initial adoption phases.

However, the impact of removing these incentives, particularly at later adoption stages, remains understudied. The limited evidence available comes primarily from China, where both Kong et al. (2020) and Lu et al. (2022) examine the effects of subsidy removal. Kong et al. (2020). project a significant 40

[2]For an up-to-date summary of existing financial incentives for European countries, see ACEA (2024) and the European Alternative Fuels Observatory's individual country pages on road transport incentives and legislation.

percent decrease in EV market share following subsidy removal, while Lu et al. (2022) find that maintaining adoption rates would require substantial alternative incentives. I contribute to filling this gap by examining the impact of removing financial incentives in a mature EV market context.

Additionally, extensive evidence suggests that fuel price dynamics significantly impact the diffusion of EVs. For instance, Østli et al. (2017) find that Norway's fuel tax has helped shift demand towards low-emission vehicles. Beresteanu and Li (2011) and Diamond (2009) demonstrate a similar pattern in the U.S., indicating that rising gasoline prices shift consumer demand towards electric options. Furthermore, a study by Bushnell et al. (2022) show that fuel prices asymmetrically matter for consumers' vehicle choices in California. A change in gasoline prices has four to six times the effect on BEV sales per capita as a comparable change in electricity prices.

The literature also identifies several other factors affecting the EV adoption rate, among which range anxiety — the concern about depleting battery power before reaching a charging station — has been identified as a primary concern (Coffman et al., 2017). An expansion of charging infrastructure has been shown to relieve these concerns and have a positive association with the adoption rate of EVs in a variety of contexts (Clinton & Steinberg, 2019; Egnér & Trosvik, 2018; Sierzchula et al., 2014; Springel, 2021). However, the direction of causality remains ambiguous, where charging point density can potentially act as both a predictor and a result of an increased EV adoption rate (Egnér & Trosvik, 2018; Mersky et al., 2016; Stiglitz et al., 2024). Interestingly, Egnér and Trosvik (2018) also find that the impact of charging infrastructure was particularly pronounced in urban areas. This finding appears somewhat counterintuitive, given that range anxiety would reasonably be more pronounced for households in rural areas, and the marginal benefit of added charging infrastructure would then be higher. While Egnér and Trosvik (2018) provide valuable insights into Sweden's early EV market dynamics, I examine how financial incentives shape adoption patterns in today's mature market, where charging infrastructure and vehicle options have expanded significantly.

3 The Swedish Setting

To increase the adoption rate of EVs, policymakers have employed a multitude of instruments across different countries. Sweden has long been at the forefront of transport sector electrification, implementing a range of targeted policy instruments to promote the adoption of EVs while maintaining some of the highest carbon taxation rates globally (International Energy Agency, 2024b). However, since 2022, the Swedish government has taken steps to scale back key financial incentives that facilitated its transition toward electric mobility (Pareliussen & Purwin, 2023). In this study, I examine three significant changes to national transport policy: a historic reduction in fuel taxation, the abolishment of the EV purchase subsidy program, and a substantial decrease in the biofuel blend-in requirement for fuel producers.

These policy changes affect the incentive to purchase an EV through different channels. Taxes levied on gasoline do not directly affect the price of owning and operating an EV, but change the relative cost of fuel, making ICEVs more expensive to operate compared to BEVs, all else equal. The purchase subsidy directly reduced the immediate up-front cost for households purchasing an EV. Similar to the fuel tax, the blend-in requirement indirectly affects the relative cost of owning a BEV compared to an ICEV through its impact on fuel prices.

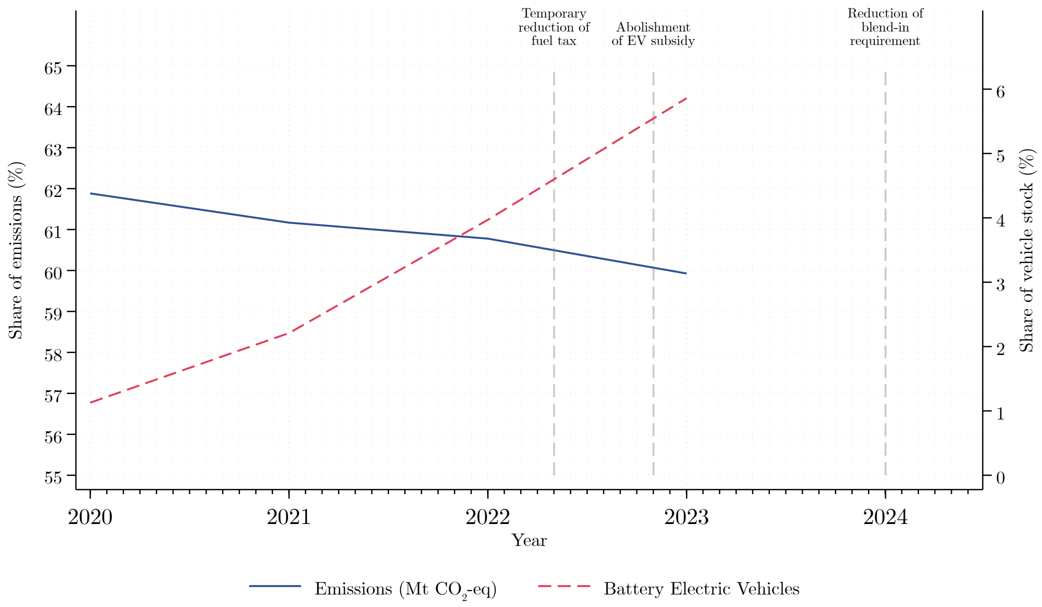


Figure 3: Policy Implementation Dates and EV Market Outcomes

Note: The figure plots the yearly share of emissions in the Swedish transport sector generated from passenger car tailpipe emissions (left axis), and the share of BEVs in the passenger car vehicle stock at the end of the year (right axis) from 2020 to 2023. Each of the policy changes and their respective timing are also shown.

Figure 3 illustrates both the timing of these policy changes and key trends in Sweden's transport sector. The figure plots two metrics: the share of emissions from passenger cars as a percentage of total transport emissions (left axis) and the BEV share of the total passenger car vehicle stock (right axis). From 2020 through 2023, the BEV share of the vehicle stock showed steady growth, though it did not surpass five percent until mid-2022. Over the same period, passenger cars' contribution to total transport emissions declined modestly from 62 to 60 percent, indicating the gradual impact of vehicle electrification on emissions reduction. The following sections detail the development and implementation of each policy instrument examined in this study.

3.1 Gasoline taxation

The price of gasoline at the pump can be decomposed into two main components: the raw product price and the taxes levied on top. The tax component of the pump price consists of three parts: the energy tax, the carbon tax, and the value-added tax (VAT). The VAT was first introduced to include gasoline in March 1990 and implies that a tax rate of 25 percent is imposed on the sum of the raw product price, the energy tax, and the carbon tax (Prop. 1989/90:111). The carbon tax was subsequently introduced in 1991 and has steadily increased since its introduction — today being among the highest in the world (Skatteverket, 2023; World Bank Group, 2024). As a result, taxes currently account for a substantial share of the gasoline price faced by consumers, making up around half of the total cost at the pump (Skatteverket, 2023).

However, in 2021, the Swedish government announced a historic cut off the energy tax applied to gasoline. The cut was two-fold: a five-month temporary cut of SEK 1.05 per liter and a permanent cut of SEK 0.40 per liter, amounting to a total tax decrease of approximately 21 percent (Skatteverket, 2023). Both were implemented starting May 1, 2022. Including VAT, this resulted in the effective price of gasoline decreasing by an unprecedented SEK 1.81 through September 2022 (Konjunkturinstitutet, 2022). While the five-month tax holiday was introduced as a measure to temporarily alleviate rising gasoline prices for consumers, the raw product price of gasoline simultaneously rose to almost completely offset the tax reduction and there is mixed evidence on the effect of the temporary tax reduction and its pass-through to consumers (Andersson & Tippmann, 2022; Drivkraft Sverige, 2024; Konjunkturinstitutet, 2022).

3.2 EV Subsidy

The Swedish EV subsidy was introduced as part of the "Bonus-Malus" policy in July 2018. It consisted of two legs: a purchase subsidy (Bonus) for low-emission vehicles and an increased tax rate (Malus) imposed on high-emission vehicles for a period of three years. Vehicles eligible for the subsidy included passenger cars, light commercial vehicles, and light buses that weighed up to 3.5 tonnes. The subsidy reimbursed up to 25 percent of the purchase price, up to a set cap. Initially, the subsidy cap was set to SEK 60,000 per vehicle, applicable to vehicles that emitted a maximum of 60 grams of CO_2 per kilometer. The emission threshold required to qualify for the subsidy was gradually lowered to 30 grams of CO_2 by January 2023. BEVs with zero emissions from usage qualified for the total subsidy amount, while eligible hybrid vehicles under the emission threshold received a reduced subsidy based on their CO_2 emissions per kilometer (SFS 2107:1334; Swedish Government, 2022a).

Additionally, subsidy eligibility was tied to the Swedish vehicle registry — only vehicles registered in Sweden qualified for the benefit. The program also included a six-month ownership requirement: the subsidy would only be paid out after the vehicle had remained with its original owner for six months following its initial registration (Transportstyrelsen, 2024).

Over the years, the subsidy amount has varied — ranging from a minimum of SEK 50,000 to a maximum of SEK 70,000 at its peak. While there was initially no set cap for the list price of vehicles eligible for the subsidy, a maximum list price requirement of SEK 700,000 was introduced in July 2022. Any EV models with list prices exceeding this threshold would effectively be excluded from subsidy eligibility. Moreover, an annual budget cap was imposed for the government's total subsidy expenses, which was generally sufficient to cover all qualifying vehicles each year. Payouts consistently remained under these caps, indicating consumers were not restricted by funding limitations (Transportstyrelsen, 2024). On November 7, 2022, the Swedish government announced the immediate discontinuation of the EV subsidy, effective the following day (Swedish Government, 2022b). Vehicles purchased by November 8, 2022, remained eligible for the subsidy provided they were registered by March 31, 2024.

3.3 Blend-in requirement

The blend-in requirement was introduced alongside the EV subsidy scheme in July 2018 and mandates fuel producers to blend in a certain share of biofuel into their gasoline and diesel products (Prop. 2017/12:01). Initially, blend rates were set at 2.6 percent for gasoline and 19.3 percent for diesel. Over the following years, these rates were gradually increased. By 2022, the biofuel blend-in requirement rate had reached 7.8 percent for gasoline and 30.5 percent for diesel. These levels were maintained through 2023. However, in January 2024, the government significantly reduced these requirements to 6 percent for both fuel types (Prop. 2023/24:28).

While the blend-in requirement does not directly reduce tailpipe emissions, the reduction mechanism is based on the idea that the crops used for the production of the biofuel will bind an equal amount of CO_2 equivalents from the atmosphere as would be released by the usage of the fuel, making the net effect on emissions zero. While the blend-in requirement does not directly affect fuel prices, it produces an upward pressure on prices faced by consumers through increased production costs.

The blend-in requirement had a clear impact on emissions reduction during its initial years of implementation. According to the Swedish Energy Agency, the policy achieved steadily increasing emissions reductions between 2018 and 2021, with the combined effect of biofuel blending in gasoline and diesel resulting in a reduction of over 15 million tonnes of CO_2 equivalents during this period. By 2021, the annual emissions reduction had reached more than 5 million tonnes of CO_2 equivalents, and the policy has been emphasized as crucial for Sweden's ability to meet its energy and climate policy goals (Energimyndigheten, 2022). While the biofuel blend-in requirement has been effective in reducing transport sector GHG emissions to date, Pareliussen and Purwin (2023) and Persson et al. (2024) raise concerns about the policy's future efficiency. They indicate that reducing the requirement to EU minimum levels of 6 percent may put Sweden off track in reaching its future emission targets.

4 Data

I collect country-level monthly data from The European Alternative Fuel Observatory (EAFO) on the share of newly registered BEV passenger cars in all European countries from January 2020 to June 2024.[3] Although data on the share of new BEV registrations is only available from January 2020, my analysis benefits from the granularity of monthly data, which provides considerable pre-intervention information. As noted by Abadie (2021), having sufficiently long pre- and post-treatment periods is a prerequisite for accurately fitting a synthetic control in the pre-intervention period, and effectively assessing the outcome in the post-treatment period. While the reduced biofuel blend-in requirement was only implemented in January 2024, any observable early effects are particularly relevant to any analysis of vehicle adoption rates. New vehicles typically remain in use for at least a decade, making early adoption trends significant indicators of long-term impacts.

The EAFO collects data across all European countries from local sources, including automotive associations, national statistical offices, and various government agencies. While the EAFO also collects data on plug-in hybrid vehicle (PHEV) registrations, this study's analysis is focused exclusively on BEVs. I make this restriction mainly due to the uniform nature of which these policy changes should reasonably affect the BEV adoption rate, but not necessarily that of PHEVs. For instance, PHEV owners may optimize their fuel use by switching between electricity and gasoline, depending on the two's relative price at a given time. Additionally, based on the level of emissions per kilometer, PHEVs would qualify for varying levels of rebates under the EV subsidy, making the impact of the subsidy differ between vehicle models and makes. As such, it is unclear how the policy changes would affect PHEV owners and, thereby, the incentive to purchase a PHEV. Due to the high volatility in the monthly BEV share, I create a smoothed series using a centered five-month rolling average, capturing variation from both the preceding and following quarters. This helps filter out any seasonal fluctuations and reduces the risk of over-fitting the model (Abadie, 2021).

An alternative approach to assess the impact of the policy changes is to use vehicle sales data as the outcome variable. However, granular vehicle sales data is not widely available as it relies on manufacturers' discretion to supply such data. Regardless, I argue that vehicle registrations are a better measure for evaluating policy effects in this context than vehicle sales. While sales data provides a direct measure of household purchase decisions, it may confound estimates by including vehicles sold outside of the country, exported vehicles, and vehicles that never enter the fleet (Clinton & Steinberg, 2019). Conversely, registrations reflect vehicles that actually enter a country's vehicle stock, making them subject to both registration-linked EV subsidies and changes in operating costs, such as fuel prices. Nonetheless, a drawback in using vehicle registrations is the lag between purchase and delivery, particularly for EVs, which often have longer delivery times than ICEVs. Depending on the make and model, this delay can range from one month up to a year (Stjerna, 2024).

I collect data on several of the covariates identified in previous research as potential determinants of

[3]The vehicle classification is made in accordance with that established by the European Commission: passenger cars (M1) are vehicles used for the carriage of passengers with the number of seats not exceeding eight, excluding the driver.

the diffusion of EVs, but exclude any direct measure of charging infrastructure due to the endogeneity concerns raised by Egnér and Trosvik (2018), Mersky et al. (2016), and Stiglitz et al. (2024).

Detailed weekly average gasoline prices for the past week are available through the EU Weekly Oil Bulletin for EU member states. I aggregate the weekly price data by back-filling values for each date and then calculate a monthly average. For non-EU countries, I collect gasoline price data from local sources.[4] These are generally available as monthly averages. All prices are recalculated to EUR per liter using Eurostat's monthly average exchange rate spot price. In all cases, I collect gasoline prices inclusive of tax. This allows me to account for variations not just in the raw product price of gasoline but also in taxes levied across time and units. Since the after-tax price is what consumers face at the pump, it is reasonable to assume that this total price, rather than individual components, will most directly influence household behavior in response to price changes.

The adoption rate of EVs is likely to vary based on the share of the population living in urban areas. In areas with a higher share of rural households, it is reasonable to assume that range anxiety would be more pronounced due to, on average, longer driving distances. Additionally, the degree of urbanization may somewhat reflect the quantity of clustered infrastructure, suggesting it could also serve as a (not very precise) proxy for charging availability. For example, consider a country where a majority of people live in densely populated urban areas. Then, fewer charging stations per capita may be necessary to alleviate range anxiety. Quarterly data on the population living in cities, towns and suburbs, and rural areas is available through Eurostat for all European countries. I approximate the degree of urbanization by calculating the share of each country's total population residing in cities, towns, or suburbs. The data is converted to monthly frequency by linear interpolation.

From the Federal Reserve, I collect the Harmonized Index of Consumer Prices for Electricity, which is readily available at monthly frequency. While it is important to note that electricity prices are subject to extensive within-country variation — fluctuations that will not be reflected in a national monthly average — I include this to reflect any national level in variation, which seems particularly relevant given that the sample period overlaps with both the global pandemic and various geopolitical conflicts, which affected energy prices throughout Europe (Kuik et al., 2022).

I collect GDP per capita in PPP US dollars per person from the OECD databank as a measure of national income levels. Since EVs typically have a higher purchase price than conventional ICEVs, the level of income may influence adoption rates. GDP per capita is only available at quarterly intervals, and to convert it to monthly frequency, I distribute each quarterly value equally across its three constituent months. Any missing values are filled using linear interpolation.

The initial pool of 26 European OECD countries provides a set of control units that I propose share similar exposure to macroeconomic shocks, including the COVID-19 pandemic, semiconductor supply chain disruptions, recent geopolitical conflicts, and, most recently, rising inflation and hiking interest rates. From the donor pool, I construct the control sample following the criteria outlined in Abadie

[4]In the final donor pool sample, only prices for Norway and Iceland are collected from alternative sources. Gasoline prices from Iceland are collected from the Iceland Automobile Association and for Norway from Statistics Norway.

(2021) for the selection of appropriate counterfactual units in synthetic control methods. First, the units in the control sample are not allowed to have undergone similar interventions during the sample period. Any countries that have implemented major changes to financial incentives for EV adoption between January 2020 and June 2024 are therefore excluded from the control sample. This criteria disqualifies Estonia, Finland, Germany, Hungary, Latvia, Lithuania, Slovakia, Turkey and the United Kingdom. Additionally, I exclude Belgium and Switzerland, as these countries had region-specific EV policies rather than uniform national programs during the sample period. The final sample consists of a balanced panel with Sweden as the treated unit and 15 countries in the donor pool.

Table 1 presents descriptive statistics of covariates for Sweden and the control group average across two panels: the full sample period (Panel A) and the pre-treatment period only (Panel B). The comparison reveals a strong covariate balance, particularly in the pre-treatment period. Sweden and the control units show similar means across key variables, with GDP per capita being nearly identical ($4,223.15 versus $4,223.20) and the degree of urbanization showing minimal difference (69.27% versus 69.24%) in the pre-treatment period. While gasoline and electricity prices exhibit slightly higher means in Sweden compared to the control units, the differences are within one standard deviation. This strong balance of covariates in the pre-treatment period strengthens the validity of the selected control units as a basis for constructing the synthetic control.

Table 1: Descriptive Statistics of Covariates

	Sweden		Control Units	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Panel A: Full Sample				
Gasoline price	1.66	0.23	1.61	0.31
GDP per capita	4,268.22	112.43	4,328.30	1,940.97
Electricity price	145.07	31.67	133.03	47.36
Degree of urbanization	70.90	2.81	69.56	10.81
Panel B: Pre-treatment Period				
Gasoline price	1.54	0.22	1.43	0.27
GDP per capita	4,223.15	141.69	4,223.20	1,955.11
Electricity price	127.92	19.17	115.38	31.11
Degree of urbanization	69.27	3.11	69.24	10.83

Note: The table presents descriptive statistics of the covariates. Panel A presents statistics for the full sample period, January 2020 to June 2024. Panel B presents statistics for the pre-treatment period, January 2020 to April 2022. The control unit sample includes Austria, Czechia, Denmark, France, Greece, Iceland, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovenia, and Spain. Gasoline prices are measured in EUR per liter, GDP per capita is USD per person PPP Chain linked volume (2015 = 100), electricity prices are from the HCI (2015 = 100), degree of urbanization is the share (%) of the population residing in urban areas.

Figure 4 shows the evolution of monthly BEV registration shares for the countries in the final sample from January 2020 to June 2024. The figure reveals a clear hierarchy of BEV adoption rates. Two countries consistently display higher shares of BEV registrations than Sweden: Norway and Iceland. The top trend line in the graph is Norway — the only country maintaining BEV registration shares above 50 percent throughout the sample period, increasing to over 80 percent by mid-2023. While Iceland's registration rates occasionally surpass Sweden's, its pattern exhibits considerable monthly volatility. Sweden demonstrates steady growth in the share of new BEV registrations throughout the period, from approximately 10 percent in early 2020 to a peak of around 35 percent in 2023 before declining slightly to about 30 percent in 2024.

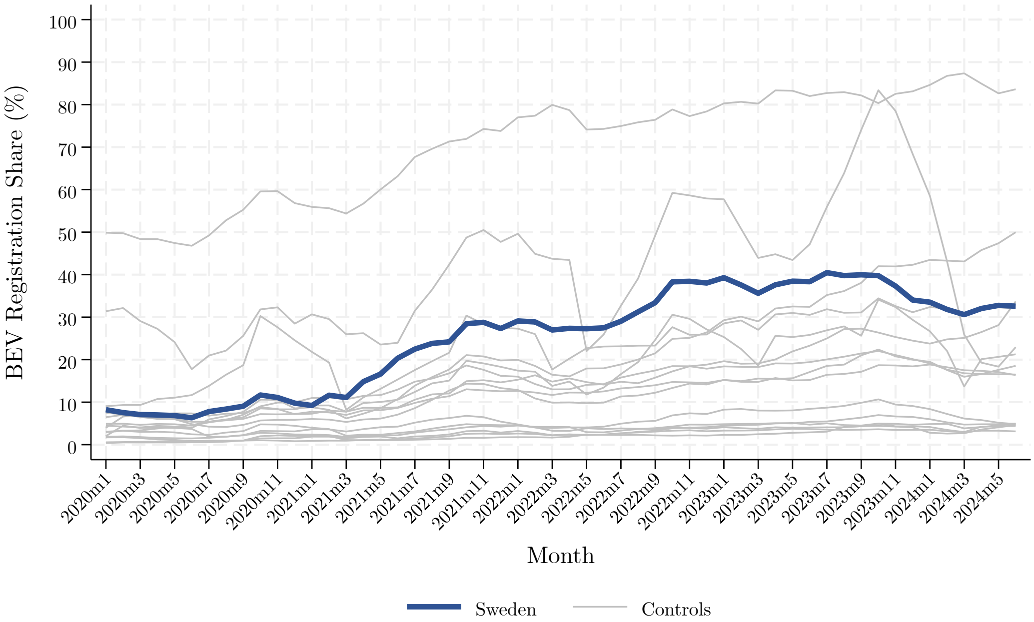


Figure 4: BEV Registration Shares

Note: The figure plots the monthly share of BEVs in total registrations for the 15 + 1 countries in the final sample from January 2020 to June 2024.

Most countries in the control sample show modest progress in BEV adoption. Starting from relatively low registration rates of less than 10 percent in early 2020, most countries exhibit a gradual upward trend but only reach BEV registration shares of 15 – 20 percent by the end of the sample period. This substantial gap between the leading Nordic countries — Norway, Iceland, and Sweden — and most other countries in the control group highlights significant variations in the pace of vehicle electrification across different countries. The pattern suggests that while EV adoption is increasing across OECD countries in Europe, the transition is occurring at markedly different rates from one country to another.

5 Empirical Methodology

5.1 The Synthetic-Difference-in-Difference Framework

To estimate the effect of the recent changes implemented in Sweden's transport policy on the market share of BEVs, I construct a credible counterfactual for Sweden, representing the outcome in the absence of these policy changes. Traditionally, comparative case studies in policy analysis have extensively employed Difference-in-Difference (DID) style models. These models typically rely on the assumption of parallel trends between the treated and control units to allow the recovery of the treatment effect, which suggests that unobserved unit- and time-fixed effects will be eliminated by taking double differences. While testing for pre-treatment parallel trends is possible, the assumption that these trends would have remained parallel in the absence of treatment is inherently untestable since we cannot observe the counterfactual. Beyond the limitations of the parallel trends assumption, DID models become particularly challenging to implement when the unit sample size is small. For example, when analyzing policy changes affecting an entire region or country, the limited number of available comparison units makes finding one credible counterfactual difficult (Abadie, 2021).[5]

One proposed alternative method to lessen the reliance on parallel trends in empirical case studies is the synthetic control method (SCM). The method operates on the premise that, in the absence of a single suitable control unit, a weighted average of multiple control units can serve as a more effective counterfactual. Introduced in a series of papers over the past two decades (Abadie & Gardeazabal, 2003; Abadie et al., 2010, 2015), this method seeks to mitigate the limitations of the DID by constructing a weighted average of control units to match the pre-intervention trend of the treated unit.[6] The SCM typically requires that the treated unit exists in the convex hull of the control units, such that unit weights are non-negative and add to one.

In this paper, I employ the Synthetic Difference-in-Difference (SDID) method, originally proposed by Arkhangelsky et al. (2021), which combines attractive features from both the DID and the SCM. Similar to DID methods, trends are allowed to vary at different levels in the pre-treatment period. Unlike the DID, which assigns equal time weights to all control units in the pre-intervention period, the SDID incorporates time weights that ensure greater weight is given to pre-treatment periods that more closely resemble the outcome in the post-treatment period, improving the precision of the estimation. Like SCMs, the SDID benefits from using a weighted average from the pool of control units to relax the dependence on parallel trend-type assumptions. By introducing an intercept parameter in the specification of the unit weights, the outcome of the treated unit is not restricted to be a convex combination of control unit outcomes, which adds flexibility compared to the SCM. As a result, more control group units generally receive non-negative weights compared to the SCM, which increases the robustness of the results by making them less dependent on a few units in the donor pool (Arkhangelsky et al., 2021; Clarke et al., 2023).

[5] Also see Athey and Imbens (2017) and Rambachan and Roth (2021) for a discussion on the limitations of DID models.
[6] For example applications see Billmeier and Nannicini (2013), Cavallo et al. (2013), Peri and Yasenov (2019), and Pinotti (2015).

The validity of implementing the SDID and constructing a credible synthetic control relies on several key identification assumptions outlined by Abadie (2021).

First, the approach assumes no anticipatory effects, as the estimator may be biased if households react to policy announcements before implementation. This is relevant for two of the three policy changes: the fuel tax cut and the reduction of the biofuel blend-in requirement were announced in advance, which may have caused households to adjust their behavior prior to the effective policy implementation. Conversely, the removal of the EV subsidy was implemented without prior notice, providing a clear cut-off point for identifying its effects.

Second, the method requires a suitable control group with similar underlying characteristics. I restrict the sample of control units to European OECD countries because they share comparable levels of economic development with Sweden, while their integration in the European market suggests they would be similarly affected by macroeconomic shocks.

Third, there should be no spillover effects between treated and control units. For the EV subsidy, this assumption is satisfied as the policy was strictly limited to vehicles registered in Sweden with a six-month ownership requirement (Transportstyrelsen, 2024). For fuel price policies, spillovers could theoretically occur in border regions, particularly along the Norway-Sweden border. If fuel prices decrease substantially in Sweden, a Norwegian household close to the border might choose to purchase fuel in Sweden rather than in Norway. This cross-border fueling behavior effectively lowers the cost of operating an ICEV relative to a BEV in Norway due to the Swedish policy changes. Such spillover effects could contaminate Norway's suitability as a control unit. I, therefore, address this concern in a robustness check by excluding Norway from the donor pool.

Fourth, while the sample contains sufficient post-treatment periods to evaluate the effects of the fuel tax changes and EV subsidy removal, the six-month window in the sample following the blend-in requirement reduction is relatively short. However, I retain this period in the analysis as early adoption patterns are informative given the decade-long average lifespan of vehicles entering the fleet.

Fifth, control units should not experience similar policy changes during the sample period, which I address through the sample selection discussed in Section 4.

5.2 Estimation Strategy and Inference

Following the specification of Arkhangelsky et al. (2021), the estimation of the average treatment effect on the treated (ATT) in the SDID framework requires a balanced panel of N units observed over T time periods. The control unit set is given by $N_{co} = N - N_{tr}$, where $N_{tr} = 1$ is the treated unit Sweden and $N_{co} = 16 - 1 = 15$ are the control countries in the donor pool.

The outcome of interest, the BEV registration share, for unit i in period t is denoted by Y_{it}, of which we will let $i = 1$ represent Sweden. Treatment is indicated by $W_{it} = 1$ if a unit i is treated in period t and otherwise $W_{it} = 0$, of which the control unit set N_{co} is assumed to be never treated, and Sweden is assumed to be always treated after period T_{pre}. The sample period is between January 2020 and June

2024, meaning the total number of monthly periods $T = 54$. Because the first major change to transport policy was implemented in May 2022, I will set this as the treatment timing and only fit the synthetic control on prior time periods. This gives $T_{pre} = 28$, $T_{post} = T - T_{pre} = 54 - 28 = 26$.

The SDID unit weights $\hat{\omega}_i^{\text{sdid}}$ are constructed such that the average outcome in the treated units is approximately parallel to the weighted average of the control units in the pre-treatment period $t = 1, ..., T_{pre}$ by minimizing the absolute distance between treatment and control units in the pre-treatment period:

$$\left(\widehat{\omega}_0, \widehat{\omega}^{\text{sdid}}\right) = \underset{\omega_0 \in \mathbb{R}, \omega \in \mathbb{R}^+}{\arg\min} \sum_{t=1}^{T_{pre}} \left(\underbrace{\omega_0}_{intercept} + \underbrace{\sum_{i=N_{tr}+1}^{N} \omega_i Y_{it} - \frac{1}{N_{tr}} \sum_{i=1}^{N_{tr}} Y_{it}}_{\substack{outcome \\ distance}} \right)^2 + \underbrace{\zeta^2 T_{pre} \|\omega\|_2^2}_{\substack{regularization \\ parameter}}, \quad (1)$$

In simple terms, the solution to the minimization problem is the weights $\hat{\omega}_i^{\text{sdid}}$ that across all pre-treatment periods minimize the sum of the distance between the BEV registration share in Sweden and that in a weighted average of outcomes in the control group $\sum_{i=1}^{N_{co}} \omega_i Y_{it}$, plus a constant level shift $\widehat{\omega}_0$. Because we only have one treated unit, $i = 1$, equation (1) reduces to:

$$\left(\widehat{\omega}_0, \widehat{\omega}^{\text{sdid}}\right) = \underset{\omega_0 \in \mathbb{R}, \omega \in \mathbb{R}^+}{\arg\min} \sum_{t=1}^{T_{pre}} \left(\underbrace{\omega_0}_{intercept} + \underbrace{\sum_{i=N_{tr}+1}^{N} \omega_i Y_{it} - Y_{1,t}}_{\substack{outcome \\ distance}} \right)^2 + \underbrace{\zeta^2 T_{pre} \|\omega\|_2^2}_{\substack{regularization \\ parameter}}. \quad (2)$$

The intercept term ω_0 allows for the level of pre-treatment trends to differ between the treatment and control units, and ζ is a regularization parameter that ensures the uniqueness of unit weights. In addition, we impose some restrictions on the weights: all weights have to be non-negative and sum to one. Note, however, that because of the intercept term $\widehat{\omega}_0$, this restriction does not require the actual outcome of the Swedish BEV registration share to exist within the convex hull of the control unit outcomes.

Similarly, the time weights $\hat{\lambda}_t^{\text{sdid}}$ are constructed from the control unit sample such that the average post-treatment outcome for each control unit differs by a constant $\widehat{\lambda}_0$ from the weighted average of the pre-treatment outcome of that same control unit:

$$\left(\widehat{\lambda}_0, \widehat{\lambda}^{\text{sdid}}\right) = \underset{\lambda_0 \in \mathbb{R}, \lambda \in \mathbb{R}^+}{\arg\min} \sum_{i=N_{tr}+1}^{N} \left(\underbrace{\lambda_0}_{constant} + \underbrace{\sum_{t=1}^{T_{pre}} \lambda_t Y_{it}}_{\substack{pre-treatment \\ weighted\ average}} - \underbrace{\frac{1}{T_{post}} \sum_{t=T_{pre}+1}^{T} Y_{it}}_{\substack{post-treatment \\ average}} \right)^2 + \underbrace{\zeta^2 N_{co} \|\lambda\|_2^2}_{\substack{regularization \\ parameter}}. \quad (3)$$

in which a very small regularization parameter is also included to ensure the uniqueness of time weights.[7]

The ATT of treatment W_{it} is then given by the two-way fixed effects regression in equation (4), which is the main specification in this study:

$$\left(\hat{\tau}^{\text{sdid}}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it}^{res} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_i^{\text{sdid}} \, \hat{\lambda}_t^{\text{sdid}} \right\} \tag{4}$$

where $\hat{\tau}^{\text{sdid}}$ is the estimated ATT, α_i are the unit-fixed effects and β_t are the time-fixed effects. The unit-fixed effects absorb any time-invariant differences between countries, such as geographic characteristics or persistent institutional features. The time-fixed effects capture temporal variations that affect all countries similarly, such as global macroeconomic shocks or common technological advances in the automotive industry. Y_{it}^{res} is the residuals from regressing the outcome variable, the BEV registration share, on a vector of control variables:

$$Y_{it}^{res} = Y_{it} - X_{it}\hat{\beta} \tag{5}$$

Contrary to the SCM, the implementation of the SDID treats adjustments for covariates as a pre-processing task in which the variation of the outcome Y_{it} from covariates is removed prior to calculating the SDID. $\hat{\beta}$ is found through an optimization procedure allowing for the efficient calculation of time and unit weights, λ and ω. In accordance with the concerns raised by Clarke et al. (2023) regarding the potential sensitivity of equation (4) from the scaling of covariates, I standardize all controls to their Z-scores, such that the mean of all covariates is 0 and the standard deviation is 1.

Arkhangelsky et al. (2021) propose three approaches for variance estimation in the SDID framework: bootstrap, jackknife, and placebo-based estimators. While bootstrap and jackknife methods are designed for large panels with many treated units, they become unreliable when the number of treated units is small. Given that this study has a single treated unit, Sweden, I employ the placebo-based inference procedure. This approach involves restricting the sample to the control units only, randomly assigning treatment, and then re-estimating $\hat{\tau}^{\text{sdid}}$ for each placebo assignment. By repeating this procedure multiple times, we obtain a vector of placebo estimates that allows the estimation of the placebo variance and standard errors. The detailed algorithm for the placebo-based variance estimation is documented in Table A1 of Appendix A1.

5.3 Event-study Design

Building on the general SDID framework, I implement an event-study design to examine the dynamic effects of Sweden's three major transport policy changes. While the ATT provides an average effect over the entire post-treatment period, it could mask temporal heterogeneity from these distinct policy interventions. As noted by Arkhangelsky et al. (2021), we can rewrite the estimator $\hat{\tau}^{\text{sdid}}$ as:

[7] See Appendix A1 for the specification of the functional form of the regularization parameter in both the calculation of the unit and time weights.

$$\hat{\tau}^{\text{sdid}} = \underbrace{\left(\frac{1}{T_{post}} \sum_{t=T_{pre+1}}^{T} Y_{1,t} - \sum_{t=1}^{T_{pre}} \hat{\lambda}_t^{\text{sdid}} \, Y_{1,t} \right)}_{Pre-Post\ difference\ Sweden} - \underbrace{\sum_{i=N_{tr}+1}^{N} \hat{\omega}_i \left(\frac{1}{T_{post}} \sum_{t=T_{pre+1}}^{T} Y_{it} - \sum_{t=1}^{T_{pre}} \hat{\lambda}_t^{\text{sdid}} \, Y_{it} \right)}_{Pre-Post\ difference\ Synthetic\ Sweden} \quad (6)$$

which can be rearranged to:

$$\hat{\tau}^{\text{sdid}} = \underbrace{\frac{1}{T_{post}} \sum_{t=T_{pre+1}}^{T} \left(Y_{1,t} - \sum_{i=N_{tr}+1}^{N} \hat{\omega}_i Y_{it} \right)}_{Post-treatment\ difference} - \underbrace{\sum_{t=1}^{T_{pre}} \left(\hat{\lambda}_t^{\text{sdid}} \, Y_{1,t} - \sum_{i=N_{tr}+1}^{N} \hat{\omega}_i \hat{\lambda}_t^{\text{sdid}} \, Y_{it} \right)}_{Pre-treatment\ difference} \quad (7)$$

Building on Ciccia (2024), it is then possible to disaggregate $\hat{\tau}^{\text{sdid}}$ into distinct event-study estimators that measure the treatment effect ℓ periods after the adoption of treatment, where $\ell \in T_{pre} + 1, ..., T$. Let $\hat{\tau}_{\ell}^{\text{sdid}}$ denote the treatment effect ℓ periods after the initial treatment timing:

$$\hat{\tau}_{\ell}^{\text{sdid}} = \underbrace{\left(Y_{1,\ell} - \sum_{i=N_{tr}+1}^{N} \hat{\omega}_i Y_{i,\ell} \right)}_{\substack{Post-treatment \\ difference\ in\ period\ \ell}} - \sum_{t=1}^{\ell-1} \left(\hat{\lambda}_t^{\text{sdid}} Y_{1,t-} \sum_{i=N_{tr}+1}^{N} \hat{\omega}_i \hat{\lambda}_t^{\text{sdid}} \, Y_{it} \right) \quad (8)$$

The event-study approach allows me to trace out period-specific treatment effects relative to the synthetic control, potentially disentangling the immediate impact of the fuel tax cuts (May through September 2022), the separate effect of the EV subsidy removal (November 2022 onwards), and the additional impact of reducing the biofuel blend-in requirement (January 2024 onwards). This is particularly relevant as the policies likely affected BEV adoption through different channels — either through changes in upfront costs or operating costs relative to ICEVs. Moreover, the period between November 2022 and December 2023 provides a window where observed effects can be primarily attributed to the EV subsidy removal, before the introduction of the biofuel blend-in requirement changes. The event-study can also help validate the identification strategy by examining pre-trends and detecting potential anticipatory effects, as some of these policy changes were announced before their implementation.

6 Results

6.1 Covariate Analysis

I begin by running regressions of the full sample to assess the relative importance of the included covariates and to test for perfect multicollinearity among them. As emphasized by Clarke et al. (2023), confirming the absence of perfect multicollinearity is fundamental for the validity of the subsequent SDID estimation, which may be sensitive to the inclusion of redundant covariates. I calculate variance inflation factors, which indicate no concerning level of multicollinearity, with all values well below conventional thresholds and a mean VIF of 1.27. Table 2 presents the resulting point estimates from running baseline regressions.[8]

Table 2: Baseline Regressions

	Battery Electric Vehicle Share		
GDP per capita	5.275*	1.385	2.960
	(2.61)	(6.46)	(5.77)
Gasoline price	5.850***	4.269**	5.125***
	(1.68)	(1.58)	(1.33)
Electricity price	5.330	-0.725	-0.555
	(5.44)	(0.81)	(0.79)
Degree of urbanization	1.419	8.938**	7.894**
	(2.61)	(3.21)	(3.07)
Treatment			10.754***
			(1.99)
Year-Month FE	No	Yes	Yes
Country FE	No	Yes	Yes
Observations	864	864	864
Adjusted R^2	0.361	0.919	0.924

Note: The table presents the point estimate for covariates and standard errors in parentheses. The dependent variable is the monthly share of BEV registrations in all specifications. Standard errors are clustered at the country level. All independent variables have been standardized to have a mean of 0 and a standard deviation of 1. Stars indicate significance at levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The sample includes $N_{tr} = 1$ and $N_{co} = 15$.

Table 2 presents three regression specifications. Column 1 reports the point estimates of a naïve OLS regression with only the covariates included. Column 2 adds time- and unit-fixed effects to control for time-invariant country characteristics and time-variant trends commonly affecting all countries, independent of treatment status. The substantial increase in adjusted R^2 from 0.361 to 0.919 suggests that controlling for time- and unit-fixed effects accounts for a significant portion of the variation in BEV registration share. Column 3 includes the treatment dummy in a standard two-way fixed effects (TWFE) regression from the DID framework. The directions of the point estimates in the TWFE specification are generally in line with findings from previous literature. An increase in GDP per capita, gasoline prices, or the degree of urbanization is associated with a higher share of BEV registrations, while an increase in electricity prices has a negative association, though not all coefficients are statistically significant.

[8]The VIF factors are presented in Table A2 in Appendix A2.

Across all specifications, gasoline prices have a statistically significant positive and large association with the share of new BEV registrations. A one standard deviation increase in gasoline price is associated with a $4.3 - 5.8$ percentage point average increase in BEV registration share. The robustness of this relationship across specifications suggests that gasoline prices may be particularly salient in consumers' vehicle choice decisions. The degree of urbanization also shows a statistically significant relationship with the BEV registration share once the fixed effects are included, with a one standard deviation increase corresponding to a $7.9 - 8.9$ percentage point average increase in the share of BEV registrations. This implies that countries with a larger share of the population residing in urban areas see higher BEV adoption rates on average, possibly correlating with greater access to charging infrastructure, shorter average trip distances, and consequentially, less range anxiety.

GDP per capita and electricity prices generally show no statistically significant relationship with the BEV registration share. For GDP per capita, this lack of significance may be driven by the interpolation method, where quarterly values are simply distributed equally across the three months of each quarter, creating limited month-to-month variation. The insignificant coefficient on electricity prices suggests that actual operating costs may be less salient to consumers than alternative fuel prices in vehicle choice decisions, supporting previous findings by Bushnell et al. (2022).

While the BEV registration share shows a large and statistically significant positive association with the treatment (10.75 percentage points), this estimate relies on the validity of the parallel trends assumption discussed in Section 3. A comparison of pre-treatment trends in BEV registration shares reveals substantial differences between Sweden's path and that of the control group average, as documented in Figure A1 in Appendix A2. In the following section, I instead employ the SDID framework to develop a more suitable counterfactual for estimating the policy effects.

6.2 Main results

In the main analysis, I examine how Sweden's transport policy changes affected BEV registration rates using the SDID specification outlined in equation (4). The treatment dummy $W_{it} = 1$ for Sweden from May 2022 onward, when Sweden implemented its first major policy change by heavily reducing fuel taxes. The sample period includes all monthly observations from January 2020 through June 2024. I estimate the model both with and without covariates, with the covariate specification using residualized outcomes of the BEV registration share derived from the fixed effects regression presented in Column 2 of Table 2.

Table 3: Main SDID Results

	BEV Share	BEV Share
Treatment	0.651	-0.055
	(4.09)	(5.02)
Controls	No	Yes
Observations	864	864

Note: The table presents the point estimate for the ATT and standard errors in parentheses. The dependent variable is the monthly share of BEV registrations. Standard errors are clustered at the country level. Time- and unit-fixed effects are included across specifications. Column two includes GDP per capita, gasoline prices, electricity prices and degree of urbanization as control. Stars indicate significance at levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The sample includes $N_{tr} = 1$ and $N_{co} = 15$.

Table 3 presents the average treatment effects on BEV registration shares, estimated both with (Column 1) and without covariates (Column 2). The unconditional model shows a slight positive effect of 0.651 percentage points, while the model controlling for GDP per capita, degree of urbanization, electricity prices, and gasoline prices shows a minimal negative effect of -0.055 percentage points. The sign of the latter coefficient aligns more closely with findings in previous literature, which suggest that removing financial incentives should decrease BEV adoption rates. However, unlike the findings of Kong et al. (2020), which project significant decreases in market share following a subsidy removal, the treatment effects in Sweden are statistically insignificant and small in magnitude. Instead, the statistically insignificant treatment effect provides initial evidence in support of Langbroek et al. (2016) finding that financial incentives become less important as EV markets mature.

To better understand these results, Figure 5 visualizes the residualized outcome series for the SDID specification with covariates (Column 2 of Table 3). The figure plots Sweden's BEV registration share series against its synthetic counterpart and indicates the optimal time weights derived from solving equation (3) in the triangles along the horizontal axis. These time weights are optimally chosen to place greater emphasis on pre-treatment periods that more closely resemble post-treatment periods in the control group. In this case, the majority of the time weight is assigned to November 2021.

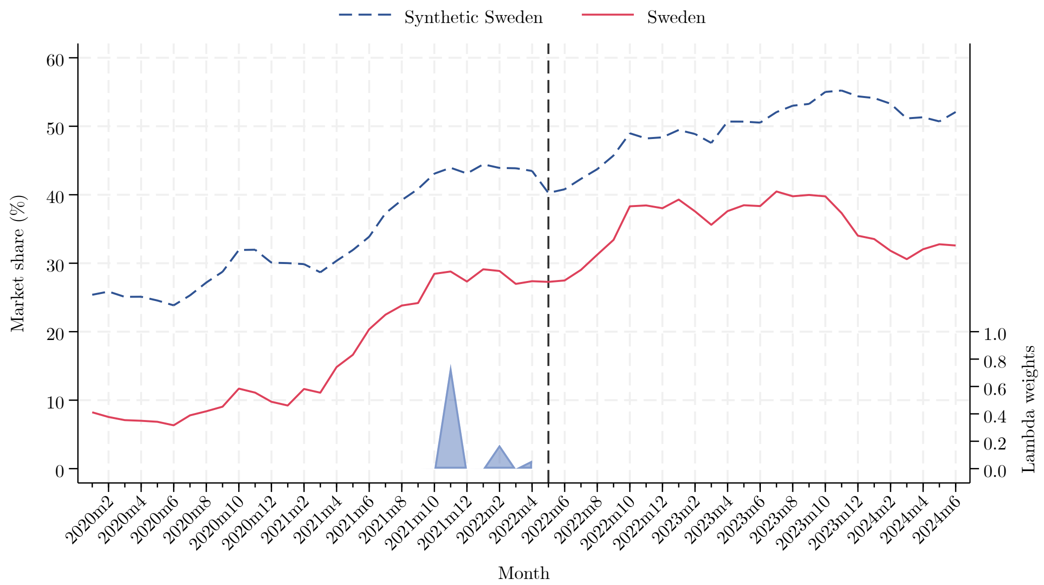


Figure 5: BEV Registration Shares and Time Weights - Full Sample Period

Note: The figure plots the BEV registration share for Sweden and the synthetic control ("Synthetic Sweden") across the full sample period, January 2020 to June 2024. Treatment timing is indicated by the vertical line in May 2022. The time weights are indicated by the triangles along the x-axis, and determined by solving the optimization problem in equation (3).

An initial comparison of pre-treatment trends between Sweden and the synthetic control in Figure 5 shows that the synthetic control closely tracks the actual Swedish BEV registration pattern, supporting the validity of the SDID approach. However, the average treatment effect may mask temporal heterogeneity in policy responses, and indeed, Figure 5 reveals distinct patterns in the post-treatment period that support this possibility. First, the gap between the control trend line and Sweden's trend line appears to narrow as Swedish BEV registration rates increase relative to the control. Notably, this increase slightly proceeds the removal of the EV subsidy, even though the abolishment was only announced one day ahead in November 2022. This is indicative of an anticipatory effect that may stem from earlier policy signals about a possible paradigm shift in transport policy, potentially stemming from the fuel tax reduction implemented in May of that year. Subsequently, this gap widens as Sweden's BEV share declines compared to the control unit, suggesting a potentially adverse effect on BEV registration rates following the reduction of the biofuel blend-in requirement in January 2024.

Table 4 documents the unit weights used to construct the synthetic control in both specifications and Figure 6 provides a visualization showing both how these weights are distributed across donor pool countries and the outcome difference of each country relative to Sweden.

Table 4: Distribution of Synthetic Control Weights

Panel A: Without Covariates					
Austria	0.11	Ireland	0.15	Norway	0.42
Czechia	0.00	Italy	0.00	Poland	0.00
Denmark	0.13	Luxembourg	0.05	Portugal	0.03
France	0.00	Netherlands	0.00	Slovenia	0.00
Greece	0.00	Iceland	0.11	Spain	0.00
Panel B: With Covariates					
Austria	0.09	Ireland	0.17	Norway	0.43
Czechia	0.00	Italy	0.02	Poland	0.00
Denmark	0.15	Luxembourg	0.04	Portugal	0.03
France	0.01	Netherlands	0.00	Slovenia	0.00
Greece	0.00	Iceland	0.05	Spain	0.00

Note: The table presents the unit weight assigned to each country in the donor pool. Unit weights are calculated by solving the optimization problem specified in equation (2). The adoption time is May 2022 in both specifications.

As shown in Table 4, Norway receives the largest weight (0.42 – 0.43) in constructing the synthetic control, followed by Ireland (0.15 – 0.17) and Denmark (0.13 – 0.15) in both specifications. Norway's dominant weight is consistent with its position as a global BEV adoption leader and its similarities to Sweden in terms of socioeconomic characteristics. The dispersion of remaining weights across multiple countries suggests that the synthetic control draws on a broad base of comparison units, potentially increasing the robustness of the estimates.

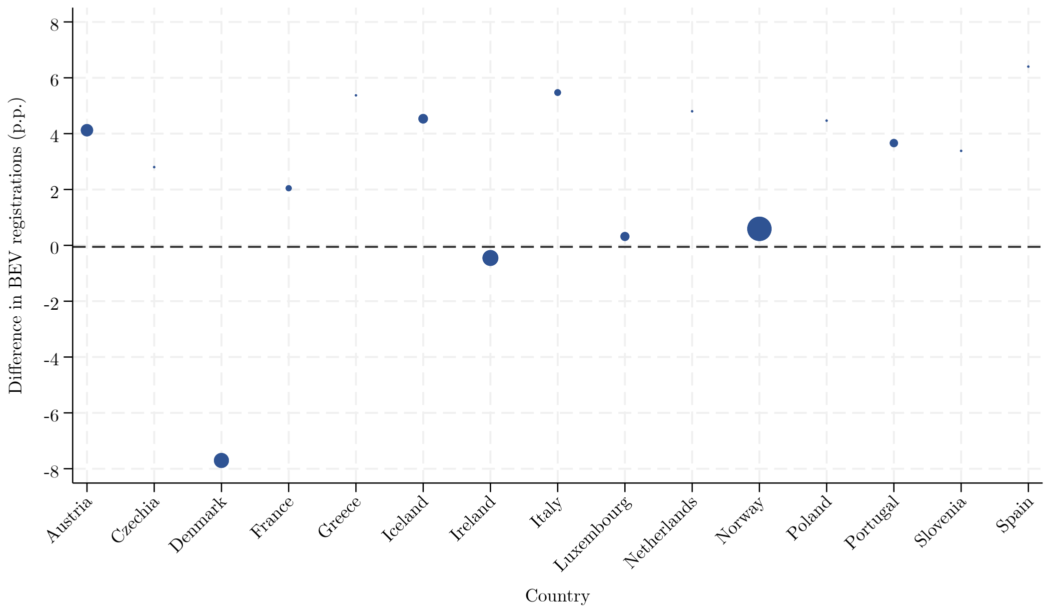


Figure 6: Country-by-Country Outcome Difference

Note: The figure plots the relative importance of unit weights country-by-country, and their relative outcome difference to Sweden. A larger dot size indicates a larger weight in the synthetic control. The horizontal line indicates the weighted average of these differences – the estimated effect.

Although the ATT estimates in Table 3 show no significant overall effect, it is important to analyze dynamic treatment effects over time for two key reasons. First, assessing any treatment effects before the treatment begins helps confirm that conditional parallel trends are valid during the pre-treatment period. Second, examining effects over time is crucial because multiple policy changes have taken place during the post-treatment period. Initially, a spike in BEV registrations might occur right after the subsidy cutoff was announced, as households were given only one day's notice and might have rushed to purchase before the abolishment. Any adverse effect from removing the subsidy would then be delayed since EVs typically have longer delivery times than conventional ICEVs. Finally, the removal of the blend-in requirement in January 2024 created additional disincentives for BEV adoption by putting upward pressure on the relative price of operating an EV by making gasoline cheaper.

To assess any temporal heterogeneity in the treatment effect, I estimate a dynamic version of the SDID specification, as outlined in equation (8). The dynamic SDID allows for the estimation of treatment effects month-by-month relative to the initial treatment date and thereby shows any differential effect over time. The point estimates and confidence intervals are plotted in Figure 7.

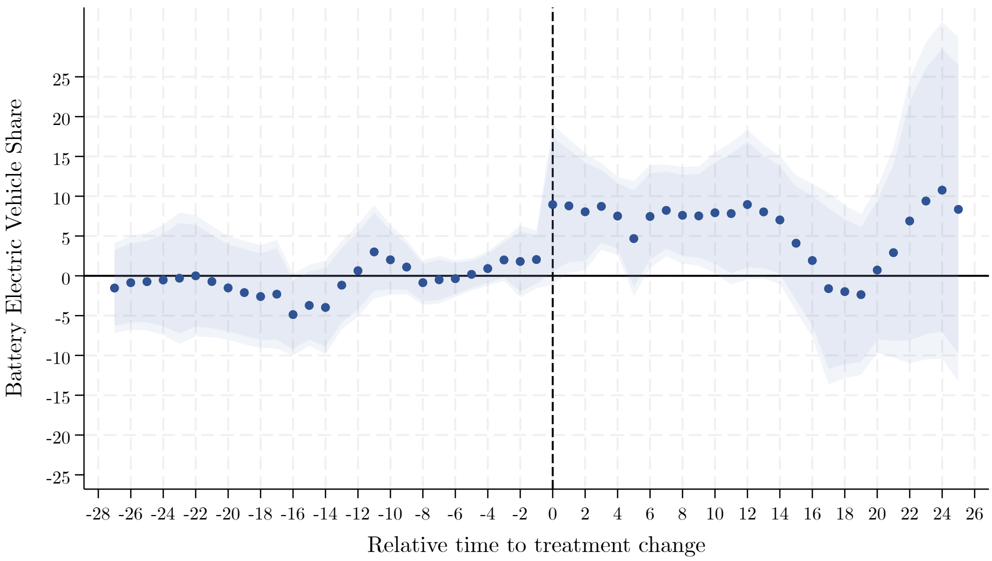


Figure 7: Dynamic Treatment Effects

Note: The figure plots dynamic treatment effects for all periods. The vertical line indicates the date of treatment onset, May 2022. The x-axis values represent each month relative to the onset of treatment. The inner (darker) shaded area is the 95% confidence interval around each point estimate, while the outer (lighter) shaded area is the 90% confidence interval.

From Figure 7, we can determine that the assumption of conditional parallel trends generally holds and that there is no identifiable pre-treatment effect. If anything, there is a slight downward deviation 16 months before the onset of treatment (January 2021), which then disappears at least nine months before the onset of treatment. After the treatment, there is a sustained and statistically significant increase in the registration rate of BEVs, consistent with a lag in the delivery time of vehicles purchased before the subsidy abolishment. This initial surge gradually diminishes, returning to baseline levels halfway through 2023, which suggests that the policy changes had a limited medium-term impact on adoption patterns.

The precision of the estimates decreases clearly from January 2024, when the biofuel blend-in requirement was reduced. While Figure 5 points to a negative effect during this period, the dynamic estimates tell a less straightforward story. The widening confidence intervals suggest that, expectedly, six months of data is not sufficient to detect any early impact of the policy change. We should, therefore, be cautious in drawing conclusions about the effects of this latest policy change based on the estimates in the main specification.

Overall, the results suggest that removing financial incentives had little to no effect on EV adoption in Sweden, at least leading up to the removal of the biofuel blend-in requirement in January 2024. While most point estimates are statistically indistinguishable from zero, we observe some variation over time. To verify the robustness of these findings, I next conduct several robustness checks focusing on alternative treatment timings and the sample composition.

6.3 Robustness Checks

The results of the main estimation raise three important concerns about the validity of the main specification. First, because we are concerned about potential spillovers from Swedish policy changes on Norway, and as evidenced in the main results, Norway is assigned the majority of the weight in constructing the synthetic control, I estimate an alternative specification, in which Norway is removed from the donor pool. As an additional benefit, this also serves as a general robustness test to verify that results are not heavily driven by one specific unit in the donor pool.

Second, there is uncertainty around the treatment timing given the structure of the fuel tax reduction. Recall that the reduced fuel tax included both a permanent component and a larger temporary component that expired after five months. This could be interpreted either as a partial removal of treatment after five months or as an enduring political signal to consumers that Sweden's era of increasing fuel taxation had ended. Conversely, it is also possible that forward-looking consumers, realizing the transitory nature of the fuel tax reduction, did not implement this into their vehicle purchase decision. Regardless, the interpretation affects how we view the entire post-treatment period. Given that a requirement for synthetic control estimations is that once a unit is treated, it is treated in all subsequent periods, I address this by reassigning the treatment timing to the abolishment of the EV subsidy.

Finally, I separate my analysis into distinct periods as they relate to January 2024, when an additional policy change was introduced. As indicated by the main results, there is significant uncertainty about the estimates following the reduction of the biofuel blend-in requirement. In an effort to improve on these, I split the sample and examine the abolishment of the EV subsidy and the reduced biofuel blend-in requirement separately.

All point estimates are presented in Table 5, and unit weights for each estimation in Table 6. The four covariates are included across all specifications. The following sections briefly discuss each result separately.[9]

[9]Country-by-Country Outcome Difference plots for each specification is presented in Figures A2 – A5 in Appendix A2. Point estimates for each month in the dynamic analyses are presented in Tables A3 – A4 in Appendix A2.

Table 5: Robustness Check Results

	Drop Out Norway	Treatment = 2022m11	EV Subsidy Abolishment	Blend-in Requirement Reduction
Treatment	0.763	-2.724	2.545	-3.695
	(5.38)	(5.08)	(3.33)	(9.96)
Controls	Yes	Yes	Yes	Yes
Observations	810	864	768	416

Note: The table presents the point estimate for the ATT and standard errors in parentheses. The dependent variable is the monthly share of BEV registrations, and the controls are GDP per Capita, gasoline prices, electricity prices, and degree of urbanization in all columns. Standard errors are clustered at the country level. Time- and unit-fixed effects are included across specifications. Column 1 drops out Norway from the donor pool. Column 2 moves the treatment timing to November 2022. Column 3 drops out all periods after December 2023. Column 4 drops out all periods before May 2022 and moves the treatment timing to January 2024. Stars indicate significance at levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The sample includes $N_{tr} = 1$ and $N_{co} = 15$ for columns 2-4, and $N_{co} = 14$ in column 1.

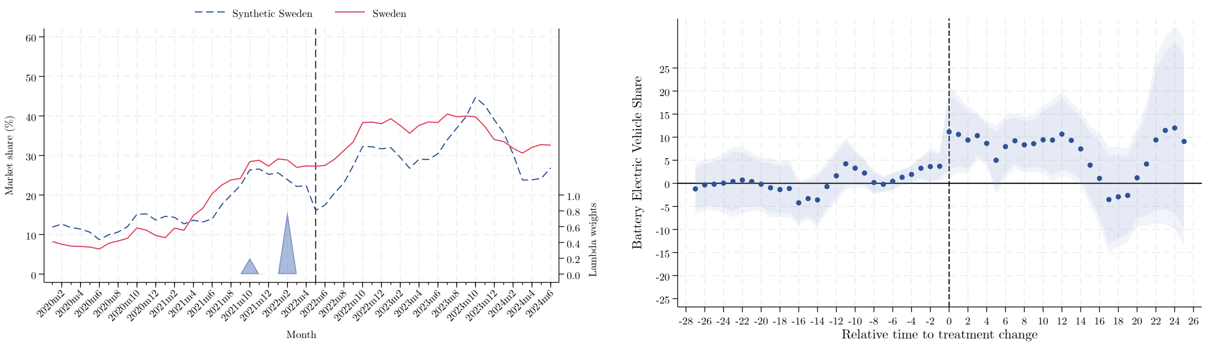
Table 6: Distribution of Synthetic Control Weights – Robustness Checks

Panel A: Drop out Norway					
Austria	0.11	Ireland	0.19	Poland	0.00
Czechia	0.00	Italy	0.00	Portugal	0.06
Denmark	0.14	Luxembourg	0.16	Slovenia	0.00
France	0.05	Netherlands	0.00	Spain	0.00
Greece	0.00	Iceland	0.28		
Panel B: Treatment = 2022m11					
Austria	0.08	Ireland	0.18	Norway	0.46
Czechia	0.00	Italy	0.03	Poland	0.00
Denmark	0.18	Luxembourg	0.02	Portugal	0.02
France	0.01	Netherlands	0.00	Slovenia	0.00
Greece	0.00	Iceland	0.02	Spain	0.00
Panel C: Drop 2024					
Austria	0.09	Ireland	0.17	Norway	0.45
Czechia	0.00	Italy	0.02	Poland	0.00
Denmark	0.15	Luxembourg	0.03	Portugal	0.04
France	0.02	Netherlands	0.00	Slovenia	0.00
Greece	0.00	Iceland	0.05	Spain	0.00
Panel D: Drop before 2022m05, Treatment = 2024m04					
Austria	0.08	Ireland	0.17	Norway	0.17
Czechia	0.00	Italy	0.00	Poland	0.02
Denmark	0.00	Luxembourg	0.10	Portugal	0.05
France	0.00	Netherlands	0.14	Slovenia	0.08
Greece	0.08	Iceland	0.05	Spain	0.04

Note: The table shows unit weights assigned to each country in the donor pool to construct the synthetic control in each of the robustness checks specifications. Unit weights are calculated by solving the optimization problem for each specification. The adoption times are May 2022, November 2022, May 2022, and January 2024 respectively.

6.3.1 Drop out Norway

Given the possibility of cross-border spillover effects between Norway and Sweden, I estimate an additional specification in which I exclude Norway from the sample. This gives $N_{co} = 14$, while all else remains the same as in the main specification in Section 6.3.2. Using the original treatment timing, I estimate the main specification from equation (4), and present these results in Column 1 of Table 5, with the corresponding residualized series (Panel A) and dynamic effects (Panel B) shown in Figure 8.



(a) BEV Registration Shares

(b) Dynamic Treatment Effects

Figure 8: Result Plots – Dropping Out Norway

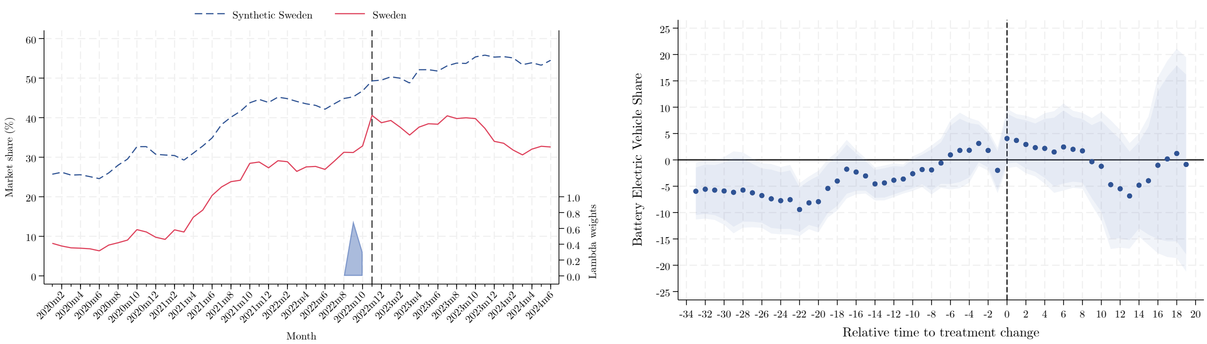
Note: Panel A plots the BEV registration share for Sweden and the synthetic control ("Synthetic Sweden") across the full sample period, January 2020 to June 2024. Treatment timing is indicated by the vertical line in May 2022. The time weights are indicated by the triangles along the x-axis, and determined by solving the optimization problem in equation (3). Panel B plots dynamic treatment effects for all periods. The x-axis values represent each month relative to the onset of treatment. The inner (darker) shaded area is the 95% confidence interval around each point estimate, while the outer (lighter) shaded area is the 90% confidence interval.

As shown in Panel A, the pre-treatment fit between the Swedish outcome series and the synthetic control weakens moderately with the exclusion of Norway compared to the main specification. Even so, the event-study plot in Panel B indicates that there is no discernable pre-treatment effect. Notably, the bulk of the unit weight is now assigned to Iceland (0.28), which in other specifications receives a weight no larger than 0.11. The ATT is slightly positive (0.76) but insignificant. Overall, the results from dropping Norway from the sample are in line with the main findings; we see an initial significant increase in the BEV registration rate, which then reverts to the baseline.

6.3.2 Treatment Timing

In an alternative interpretation, I consider the May through September 2022 fuel tax reduction as transitory, assuming that forward-looking consumers rationally examine the temporary nature of the policy change and don't incorporate it into their vehicle purchase decision. This then suggests that the November 2022 removal of the EV subsidy is the true treatment onset. Although the subsidy ended specifically on November 8th, I classify the entire month of November 2022 as treated, due to the BEV registration data only being available at the monthly level, and the fact that the majority of November was after the removal of the EV subsidy. Under this interpretation, I re-estimate the SDID specification from equation (4), defining treatment as $W_{it} = 1$ for Sweden from November 2022 onward. This gives $T_{pre} = 34$, $T_{post} = T - T_{pre} = 54 - 34 = 20$, while all else remains the same as in the main specification in Section 6.3.2. Note that because the post-tax gasoline price is included as a covariate, any variation from actual changes in gasoline prices resulting from the fuel tax reduction from May through September 2022 is accounted for. Table 5, Column 2 presents these results, with the corresponding visualizations in Figure 9.

The average treatment effect remains statistically insignificant under this specification, though the ATT coefficient turns negative (–2.72). Table 6, Panel B provides the unit weights for this estimation, which only differ slightly from those in the main results: Norway receives the majority of the weight (0.46), followed by Denmark (0.18) and Ireland (0.18).



(a) BEV Registration Shares

(b) Dynamic Treatment Effects

Figure 9: Result Plots – Adjusted Treatment Timing

Note: Panel A plots the BEV registration share for Sweden and the synthetic control ("Synthetic Sweden") across the full sample period, January 2020 to June 2024. Treatment timing is indicated by the vertical line in November 2022. The time weights are indicated by the triangles along the x-axis, and determined by solving the optimization problem in equation (3). Panel B plots dynamic treatment effects for all periods. The x-axis values represent each month relative to the onset of treatment. The inner (darker) shaded area is the 95% confidence interval around each point estimate, while the outer (lighter) shaded area is the 90% confidence interval.

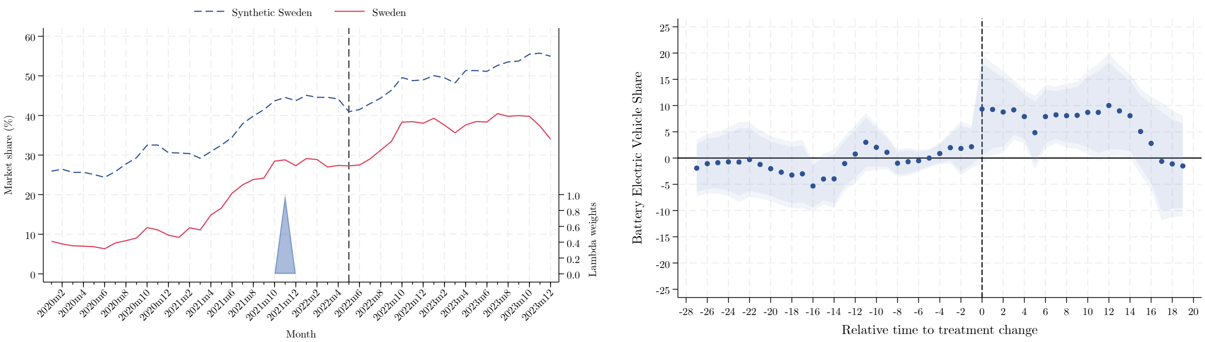
Examining Figure 9, the evidence suggests that this interpretation of the treatment timing does not necessarily hold merit. Panel A indicates that because November is only partially treated, the initial large increase in BEV registrations following the subsidy removal puts upward pressure on the trend already in November 2022, making this an inappropriate baseline point to assign the treatment shift. This validates the strategy of the main specification, in which I backdate the treatment to May 2022, and only fit the synthetic control on the periods prior to this date. Moreover, Panel B reveals violations of the conditional parallel trends assumption during parts of the pre-treatment period, though the model assigns time weights primarily to periods near the cut-off, where there are no identifiable pre-treatment effects. Despite these validity concerns, the treatment effect remains statistically insignificant across all post-treatment periods and does thereby not differ from prior results.

6.3.3 Reduced Sample Timeframe

Because of the additional onset of treatment in January 2024 through the introduction of the biofuel blend-in requirement, I divide the sample period in an attempt to isolate the effect of the two latter policy changes. Regardless of whether we interpret the fuel tax reduction as a transitory price shock or a signal of a new fuel tax policy paradigm, the findings from moving the treatment date to November

2022 in the previous section indicate that the robustness of the model relies on the assignment of the treatment date in May 2022, and I, therefore, revert to consider the first onset of treatment to be May 2022. I then split the sample into two periods.

First, I analyze the period leading up to the change in the biofuel blend-in requirement by excluding the last six months of the sample, focusing on effects between May 2022 and December 2023. While this reduces the post-treatment periods available for analysis, it still provides sufficient time periods to identify short-term trend shifts. This gives $T_{pre} = 28$, $T_{post} = T - T_{pre} = 48 - 28 = 20$, while all else remains the same as in the main specification in Section 6.3.2. The results are presented in Table 5 (Column 3) and Figure 10.



(a) BEV Registration Shares

(b) Dynamic Treatment Effects

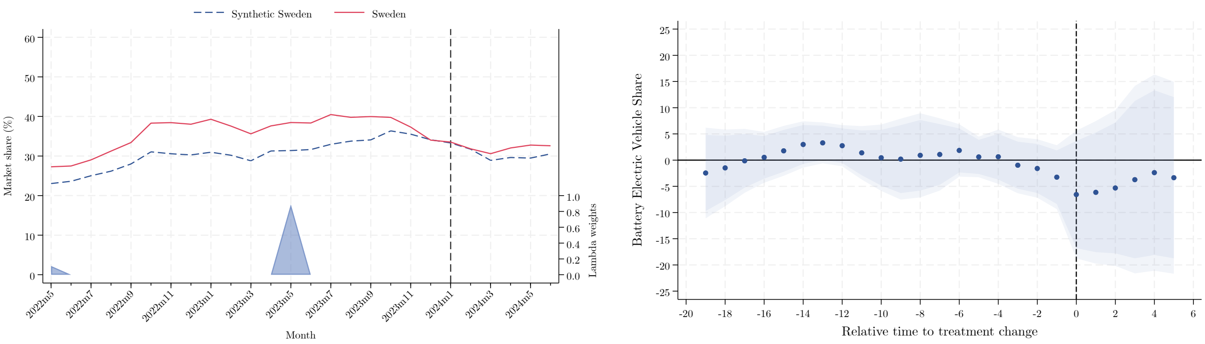
Figure 10: Result Plots – EV Subsidy

Note: Panel A plots the BEV registration share for Sweden and the synthetic control ("Synthetic Sweden") a reduced sample period, January 2020 to December 2023. Treatment timing is indicated by the horizontal line in May 2022. The time weights are indicated by the triangles along the x-axis, and determined by solving the optimization problem in equation (3). Panel B plots dynamic treatment effects for all periods. The x-axis values represent each month relative to the onset of treatment. The inner (darker) shaded area is the 95% confidence interval around each point estimate, while the outer (lighter) shaded area is the 90% confidence interval.

The pre-treatment fit is almost identical to the main specification, which is unsurprising as we fit the synthetic control on the same pre-treatment periods and donor pool units as in the main specification. However, the entire time weight is now assigned to November 2021 while the bulk of the unit weight is assigned to Norway (0.45), followed by Ireland (0.17) and Denmark (0.17).

In Panel B of Figure 6, we observe an immediate positive and statistically significant effect on BEV registration rates, consistent with the results of the main specification in Section 6.3.2. The average treatment effect for the entire period remains statistically insignificant, indicating that even after accounting for the potential downward pressure from the January 2024 policy change and the increased uncertainty in estimates following it, we find no evidence of a sustained impact on BEV registrations.

Second, I examine the isolated effect of the blend-in requirement reduction by treating the period from May 2022 to December 2023 as the new baseline pre-treatment period and analyzing the subsequent six months through June 2024. I define treatment as $W_{it} = 1$ for Sweden from January 2024 onward. This gives $T_{pre} = 20$ and $T_{post} = T - T_{pre} = 26 - 20 = 6$, while all else remains the same as in the main specification in Section 6.3.2. The results are presented in Table 5 (Column 4) and Figure 11.



(a) BEV Registration Shares

(b) Dynamic Treatment Effects

Figure 11: Result Plots – Biofuel Blend-in Requirement

Note: Panel A plots the BEV registration share for Sweden and the synthetic control ("Synthetic Sweden") across a reduced sample period, May 2022 to June 2024. Treatment timing is indicated by the vertical line in January 2024. The time weights are indicated by the triangles along the x-axis, and determined by solving the optimization problem in equation (3). Panel B plots dynamic treatment effects for all periods. The x-axis values represent each month relative to the onset of treatment. The inner (darker) shaded area is the 95% confidence interval around each point estimate, while the outer (lighter) shaded area is the 90% confidence interval.

In this specification, the unit weights are distributed with a higher degree of dispersion than in the previous specifications, meaning that more donor pool countries receive a larger unit weight. Norway and Ireland are each assigned 0.17 in weight, followed by the Netherlands at 0.14. The majority of the time weight is assigned to May 2023 (Panel A of Figure 11). Panel B of Figure 11 shows that there is no indication of pre-treatment effects present under this specification. While Figure 11 and the negative ATT point estimate (–3.67) suggest a slight decrease in registration rates following the reduction of the biofuel blend-in requirement, this change is not statistically significant. The limited post-treatment window of six months substantially reduces statistical power, resulting in maintained wide confidence intervals from January 2024 onward. Consequently, while I find no significant treatment effect, the large standard errors suggest that more post-treatment time periods are needed to draw definitive conclusions about the reduction of the biofuel blend-in requirement's impact.

7 Discussion

The empirical analysis shows that policy changes had little to no overall effect on the BEV adoption rate in Sweden, with the average treatment effect remaining statistically insignificant across all specifications. While the dynamic estimations show an initial increase in BEV registrations, which I attribute to delivery delays, this effect subsequently reverted to pre-treatment levels. Generally, these results need to be understood within the broader economic context of the sample period, which may have significantly influenced vehicle purchasing patterns. For example, the automotive industry faced substantial production constraints due to global semiconductor shortages, while many economies, including Sweden, experienced rising inflation and subsequent interest rate increases. Given Swedish households' relatively high debt-to-income ratio (Eurostat, 2024), it is feasible that the changes in BEV adoption rates observed during the sample period are explained partly by responses to common macroeconomic conditions, which are accounted for by the time-fixed effects, rather than solely by policy changes. Following the removal of the blend-in requirement in January 2024, my estimates show notably wider confidence intervals, making it difficult to draw conclusions about this specific policy change's effects.

My baseline TWFE regression analysis identifies two key factors driving BEV adoption patterns. The degree of urbanization exhibits a strong and statistically significant positive relationship with BEV registration shares, where a one standard deviation increase corresponds to a 7.9 - 8.9 percentage point rise in new BEV registrations. Additionally, gasoline prices show a consistently significant positive relationship with BEV registrations across all specifications, while electricity prices demonstrate no significant effect. This asymmetric response aligns with findings from Bushnell et al. (2022), who show that gasoline prices have a substantially larger impact on BEV sales than comparable changes in electricity prices.

Beyond these broader economic conditions, I propose two specific mechanisms related to the structure of the Swedish transport policies that may explain the limited impact of removing financial incentives. First, the strong relationship between the degree of urbanization and BEV adoption suggests that financial incentives may have heterogeneous effects across geographical regions, potentially limiting the impact of uniform national policies. Second, as with any point subsidy removal, the resulting increase in up-front purchase cost may be asymmetrically distributed between consumers and producers, with no guarantee that the cost increase is fully passed through to final consumer prices. I elaborate on each of these mechanisms in the following sections.

7.1 EV Subsidy Incidence

An important consideration in interpreting the absence of an effect of Swedish transport policy changes beyond the macroeconomic context is the role of manufacturer pricing responses. The removal of a point subsidy is economically equivalent to the introduction of a point tax, which should, all else equal, reduce demand at a given price point. Depending on the relative price elasticity, the real price increase may then asymmetrically affect consumers and producers, and the more inelastic of the two will generally take on a larger burden of the price change.

In the Swedish EV market, three manufacturers dominate sales: Volvo, Volkswagen, and Tesla (Mobility Sweden, 2024). While detailed historical price data for EVs is generally scarce, the available price points for Tesla models provide some insight into how at least one major manufacturer responded to this policy shift. The price evolution for the most sold passenger car models shows that Tesla implemented significant price reductions in the Swedish market following the removal of the EV subsidy. Tesla reduced prices by SEK 139,000 – 150,000 for the Model Y and SEK 110,000 for the Model 3 (Skatteverket, 2024), substantially exceeding the original SEK 70,000 subsidy amount. The magnitude of these reductions provides indicative evidence that suggests at least one of the major EV manufacturers had considerable pricing flexibility, which raises questions about the EV subsidy's effectiveness in lowering actual consumer prices rather than supporting manufacturer margins.

However, these price changes were not unique to the Swedish market (Wedberg, 2024). To account for this, I perform a simple back-of-the-envelope calculation of the subsidy incidence using Norway as a counterfactual. By observing the relative change in prices between Norway and Sweden around the November 2022 EV subsidy removal, we get an approximate estimate of the Swedish subsidy incidence. Figure 12 plots the average vehicle price of the two most sold Tesla models in Sweden and Norway between May 2022 and December 2023.

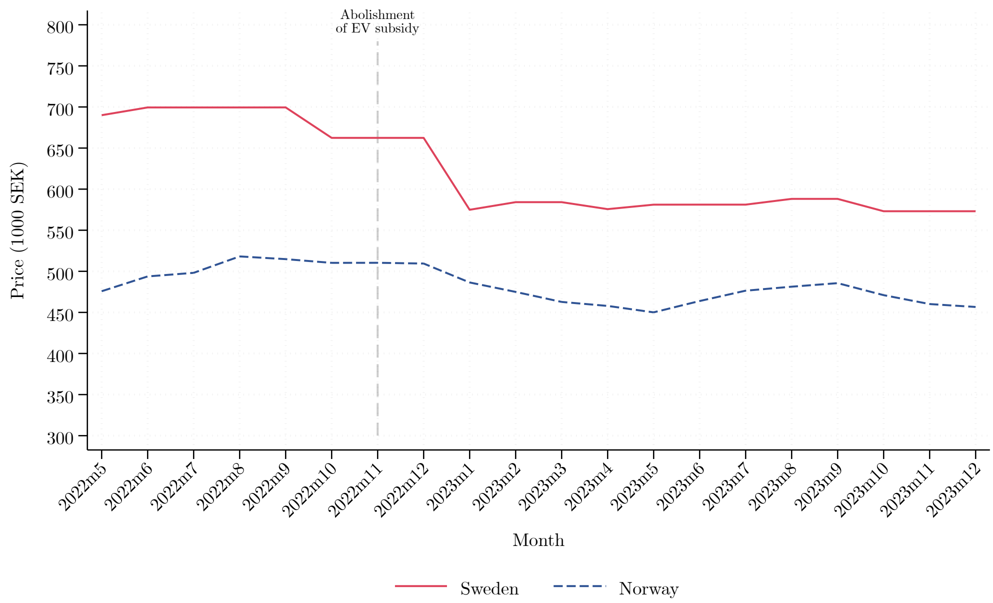


Figure 12: Average Tesla Price Development

Note: The figure plots the price development of the average price of Tesla Model 3 and Tesla Model Y LR in Norway and Sweden between May 2022 and December 2023. Norwegian prices have been converted to SEK using the monthly average exchange rates from Sveriges Riksbank (2024). Price data sources: Bie (2023), Rask (2021, 2022, 2023a, 2023b, 2023c, 2024a, 2024b, 2024c, 2024d), Skatteverket (2024), and Tesla (2024a, 2024b).

Figure 12 shows that the average price decreased more dramatically in Sweden than in Norway following the January 2023 price cut. A simple TWFE regression with time- and unit-fixed effects suggests that Tesla reduced prices in Sweden by approximately SEK 73,000 more than in Norway following the policy change.[10] This price reduction closely matches the original subsidy amount of SEK 70,000, indicating

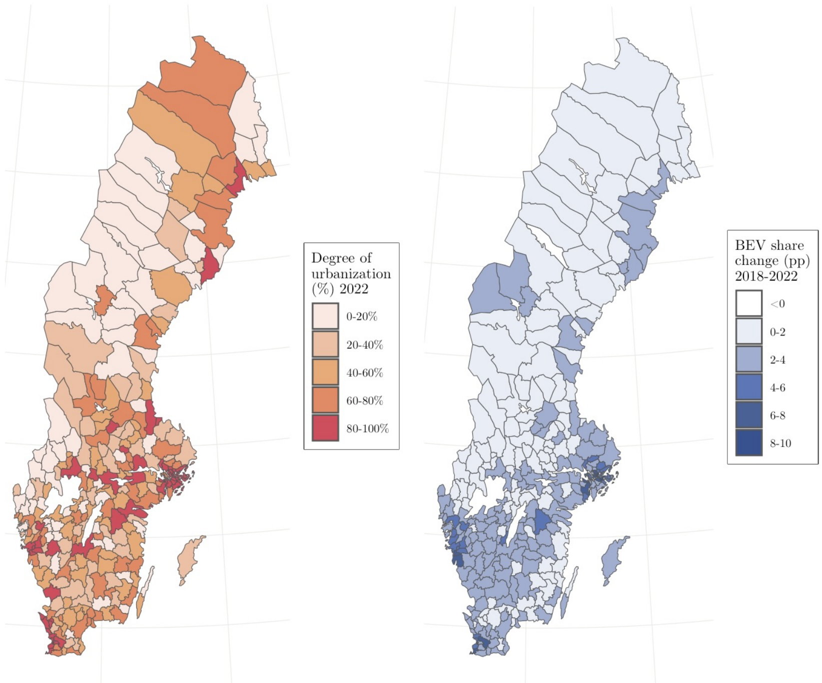
[10]The full regression results are presented in Table A5 in Appendix A2. The point estimate is significant at the 0.1% level.

that Tesla largely absorbed the subsidy's removal through price adjustments. While this does not provide evidence of the complete EV supplier response on the Swedish market, it indicates that the increase in up-front purchase price following the subsidy removal was at least partially absorbed by EV manufacturers.

Beyond the size of the price reduction, the shift from government subsidy to manufacturer price cuts may have actually improved accessibility for some consumers. As noted in Section 3.2, the EV subsidy program required a six-month ownership period before the effective subsidy eligibility, creating both a delay in financial benefit and a potential barrier for liquidity-constrained households. Especially during a period of high inflation and increasing interest rates, this delay likely reduced the subsidy's real value to consumers. In contrast, manufacturer price reductions provided immediate savings in the up-front purchase price.

7.2 Spatial Heterogeneity in Treatment Effects

The robust positive relationship between the degree of urbanization and BEV adoption suggests another potential explanation for the limited impact of Swedish transport policy changes: heterogeneity in the effectiveness of financial incentives across different geographical contexts. Financial incentives may have been more effective in urban areas, indicating that national policies did not uniformly affect household vehicle choices across regions. While the results from the covariate analysis provide an initial indication of spatial heterogeneity between degrees of urbanization at the country level, it does not account for within-country variation. To examine these spatial patterns within Sweden, I present municipality-level data on urbanization and the BEV share of the vehicle stock in Figure 13.



(a) Degree of Urbanization **(b)** BEV Share Change

Figure 13: Patterns of Spatial Heterogeneity

Note: Panel A plots the degree of urbanization as the share (%) of the municipal population living in urban areas in 2022. Panel B plots the change in BEV's share (percentage points) in the total municipal vehicle stock from 2018 to 2022. Sources: Statistics Sweden (2024) and Trafikanalys (2019, 2020, 2021, 2022, 2023).

Panel A shows the degree of urbanization in 2022, measured as the share of municipal residents living in urban areas.[11] The map shows a clear north-south divide: in most southern regions, at least $40 - 60$ percent of municipal populations reside in urban areas, while in northern Sweden, significant degrees of urbanization appear primarily along the coast, with inland regions showing urbanization rates below 20 percent.

Panel B depicts the change in BEV vehicle stock share from 2018 to 2022, corresponding to the period when the EV subsidy was in effect. The map demonstrates that BEV adoption is concentrated heavily in southern Sweden, particularly around the three major metropolitan areas: Stockholm, Göteborg, and Malmö. These cities and their surrounding suburb municipalities saw increases in BEV vehicle stock exceeding eight percentage points. In contrast, northern regions showed limited growth, with modest increases primarily along the coast and minimal adoption in the rural inland areas. This geographical pattern suggests substantial spatial heterogeneity in policy effectiveness, indicating that financial incentives may have had a limited impact on EV adoption in rural areas.

[11] Statistics Sweden uses the broad definition of urban areas as "a continuous settlement with at least 200 inhabitants".

8 Concluding remarks

In this study, I examine the impact of removing financial incentives for BEV adoption in a mature EV market, addressing an important gap in the literature, which has primarily focused on the implementation of EV incentives. When controlling for reasonable covariates, I find no statistically significant reduction in the Swedish registration share of new BEVs from May 2022 through December 2023, following reductions in fuel taxes and the abolishment of the EV subsidy program. The result is robust across multiple specifications, showing an initial increase in BEV registration rates — likely reflecting vehicle delivery delays — before reverting to baseline levels. Following the January 2024 reduction in the biofuel blend-in requirement, point estimates suggest a negative effect on BEV adoption, though this effect is statistically insignificant and characterized by substantial uncertainty.

Furthermore, my analysis shows that both the degree of urbanization and gasoline prices are important determinants of BEV adoption. Both show a significant and large positive average association with the BEV registration rate, while electricity prices have a non-significant relationship with BEV registrations. These results align with previous literature indicating that gasoline prices are more salient in consumers' vehicle purchase decisions than electricity prices.

Building on the finding that urbanization significantly affects BEV adoption rates, I propose that the absence of an average treatment effect may partially result from treatment uptake heterogeneity. Financial incentives for EV adoption appear to have had a more considerable impact on the growth of the BEV share of the vehicle stock in urban areas, while rural areas have substantially less growth. Together, these findings suggest that urban households that, on the margin, could be induced to purchase a BEV by financial incentives were successfully targeted by the policies, but households in rural areas may face barriers beyond the costs of purchasing and owning a BEV. While I do not directly observe the impact of access to charging infrastructure, this supports previous research indicating that range anxiety remains a considerable barrier to BEV adoption.

Regarding the abolishment of the EV subsidy, I additionally suggest that the lack of an average treatment effect may have resulted from incomplete price pass-through to consumers. At least one leading supplier of EVs in the Swedish market introduced significant price reductions shortly after the subsidy's removal. This indicates that at least one major manufacturer absorbed most of the subsidy removal into its margins, suggesting that the EV subsidy may not have effectively reduced consumer prices but instead padded manufacturer margins. While removing the EV subsidy did not significantly reduce BEV registrations in the period following its removal, this outcome likely reflects both market adaptations and heterogeneity in incentive responses across urban and rural areas.

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Appendices

A1 Definitions

The regularization parameters ensure the uniqueness of both unit and time weights. The regularization parameter for unit weights is given by:

$$\zeta = \left(N_{\mathrm{tr}} T_{\mathrm{post}}\right)^{1/4} \hat{\sigma} \text{ with } \hat{\sigma}^2 = \frac{1}{N_{\mathrm{co}}\left(T_{\mathrm{pre}}-1\right)} \sum_{i=1}^{N_{\mathrm{co}}} \sum_{t=1}^{T_{\mathrm{pre}}-1} \left(\Delta_{it} - \bar{\Delta}\right)^2,$$
$$\text{where } \Delta_{it} = Y_{i(t+1)} - Y_{it}, \quad \text{and} \quad \bar{\Delta} = \frac{1}{N_{\mathrm{co}}\left(T_{\mathrm{pre}}-1\right)} \sum_{i=1}^{N_{\mathrm{co}}} \sum_{t=1}^{T_{\mathrm{pre}}-1} \Delta_{it}\,. \tag{A1}$$

The regularization parameter for unit weights is constructed to match the size of a typical period-to-period outcome change Δ_{it} for control units in the pre-treatment period, scaled by $(N_{tr}T_{Post})^{(1/4)}$. The regularization parameter for time weights is defined as:

$$\zeta = 1 \times 10^{-6} \widehat{\sigma} \tag{A2}$$

In which $\widehat{\sigma}$ is defined as in equation A1. For a more detailed discussion on the construction of the regularization parameters, please refer to Arkhangelsky et al. (2021).

Table A1 presents Algorithm 4 from Arkhangelsky et al. (2021), which is employed to calculate the standard errors in all specifications.

Table A1: Algorithm for Placebo Variance estimator

Data:	$\mathbf{Y}_{co,\cdot}, N_{tr}, B$
Result:	Variance estimator $\hat{V}_{\tau}^{placebo}$
1.	for $b \leftarrow 1$ to B do
2.	Sample N_{tr} out of the N_{co} control units without replacement to ‘receive the placebo’;
3.	Construct a placebo treatment matrix $\mathbf{W}_{co,\cdot}^{(b)}$ for the controls;
4.	Compute the SDID estimator $\hat{\tau}^{(b)}$ based on $(\mathbf{Y}_{co,\cdot}, \mathbf{W}_{co,\cdot}^{(b)})$;
5.	end
6.	Define $\hat{V}_{\tau}^{placebo} = \frac{1}{B}\sum_{b=1}^{B}(\hat{\tau}^{(b)} - \frac{1}{B}\sum_{b=1}^{B}\hat{\tau}^{(b)})^2$;

Note: This table shows the algorithm to calculate the placebo variance estimator for the standard errors of all SDID specifications. This is a direct reproduction of Algorithm 4 in Arkhangelsky et al. (2021), included here for completion.

A2 Additional Result Plots and Tables

Table A2 presents the variance inflation factors calculated to determine the presence of perfect multicollinearity in the covariates included in the empirical analysis. $VIF = 1$ indicates no correlation in variables, $1 < VIF \leq 5$ indicates moderate correlation, while $VIF > 5$ indicates severe collinearity.

Table A2: Variance Inflation Factors

	VIF	1/VIF
Gasoline price	1.39	0.717
Degree of urbanization	1.30	0.767
Electricity price	1.22	0.821
GDP per capita	1.16	0.863
Mean VIF	1.27	

Note: The table presents VIF for all covariates included in the empirical analysis. VIF values below 5 indicate absence of severe multicollinearity. All independent variables have been standardized to have mean of 0 and standard deviation of 1.

Gasoline prices have the highest VIF factor at 1.39, which indicates some moderate collinearity but far from perfect multicollinearity. The mean VIF is 1.27, and we can conclude that perfect multicollinearity is not a concern for including these four covariates in the main analysis.

Figure A1 presents parallel trend evaluation plots for the TWFE regression in Table 2 Column 3. The left panel shows the raw BEV registration series while the right panel shows the de-trended series after adjusting for a linear time trend.

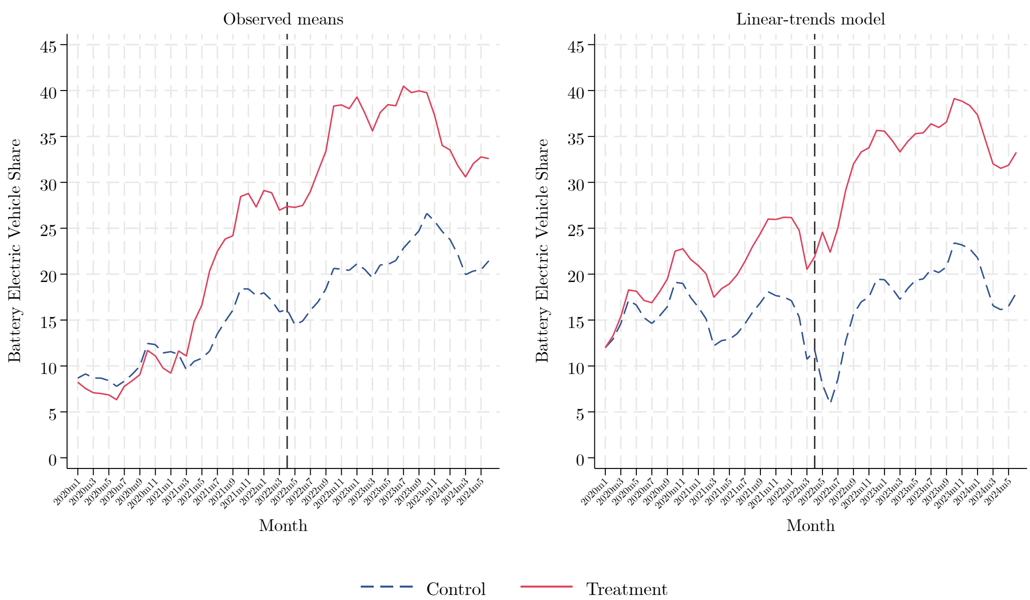

Figure A1: TWFE Parallel Trends

Note: The left panel plots the BEV registration share in Sweden ("Treatment") and the average of the Control units from January 2020 to June 2024. The right panel show the de-trended series.

The left panel of Figure A1 clearly shows that the pre-treatment trends differ substantially between Sweden and the control group average. Sweden's BEV adoption rate increased more steeply than the control group average in the pre-treatment period, suggesting that the parallel trends assumption is violated. Even after detrending the data (right panel), the pre-treatment patterns remain quite different between Sweden and the control group average, particularly in the periods closest to the policy changes.

Figures A2 through A5 present Country-by-Country Outcome Difference plots for each of the robustness check specifications.

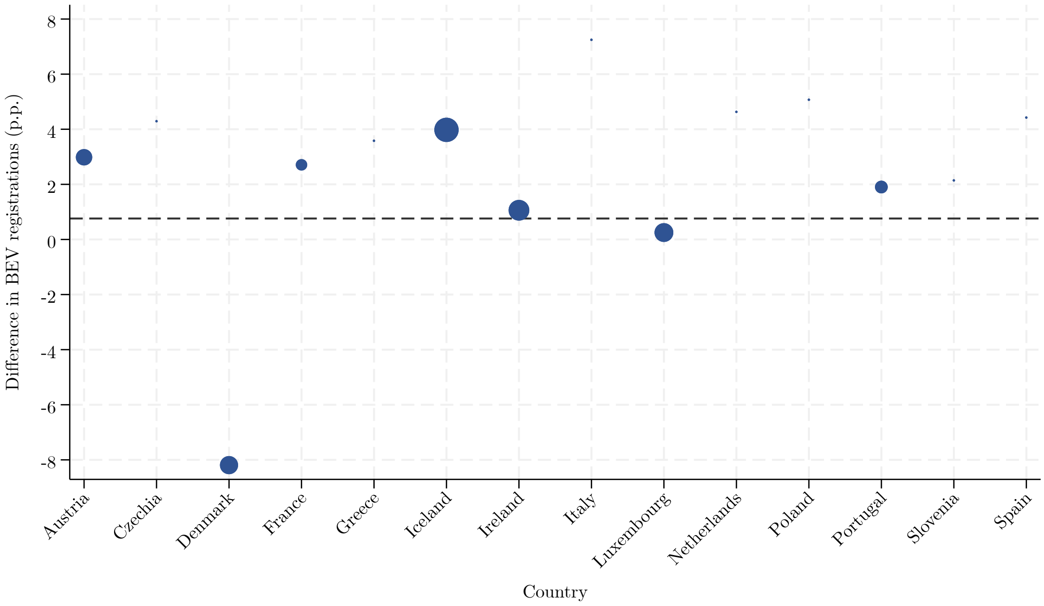

Figure A2: Country-by-Country Outcome Difference — Drop out Norway

Note: The figure plots the relative importance of unit weights country-by-country, and their relative outcome difference to Sweden. A larger dot size indicates a larger weight in the synthetic control. The horizontal line indicates the weighted average of these differences – the estimated effect.

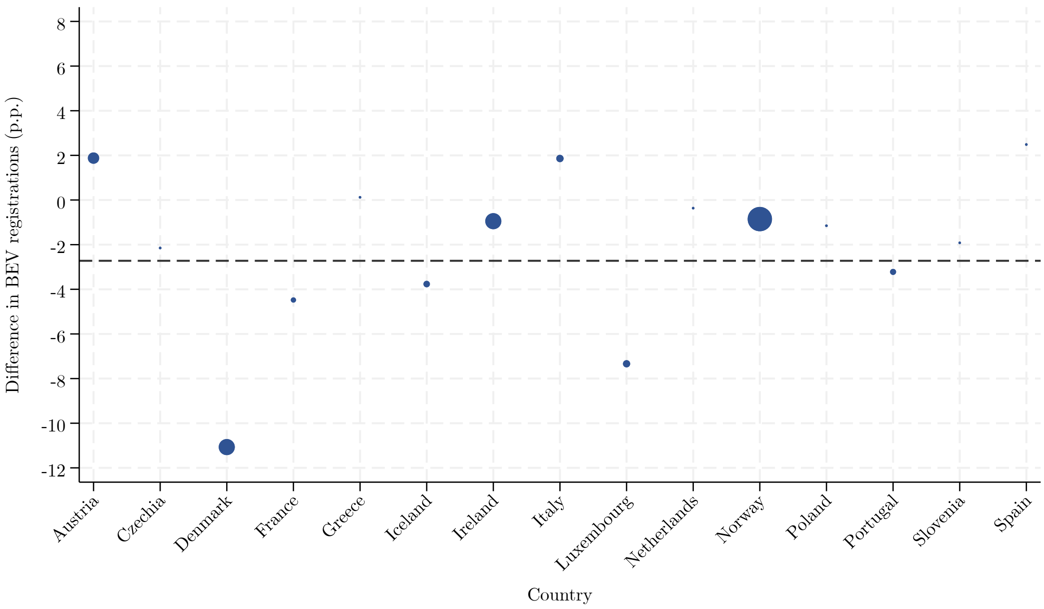

Figure A3: Country-by-Country Outcome Difference — Adjusted Treatment Timing

Note: The figure plots the relative importance of unit weights country-by-country, and their relative outcome difference to Sweden. A larger dot size indicates a larger weight in the synthetic control. The horizontal line indicates the weighted average of these differences – the estimated effect.

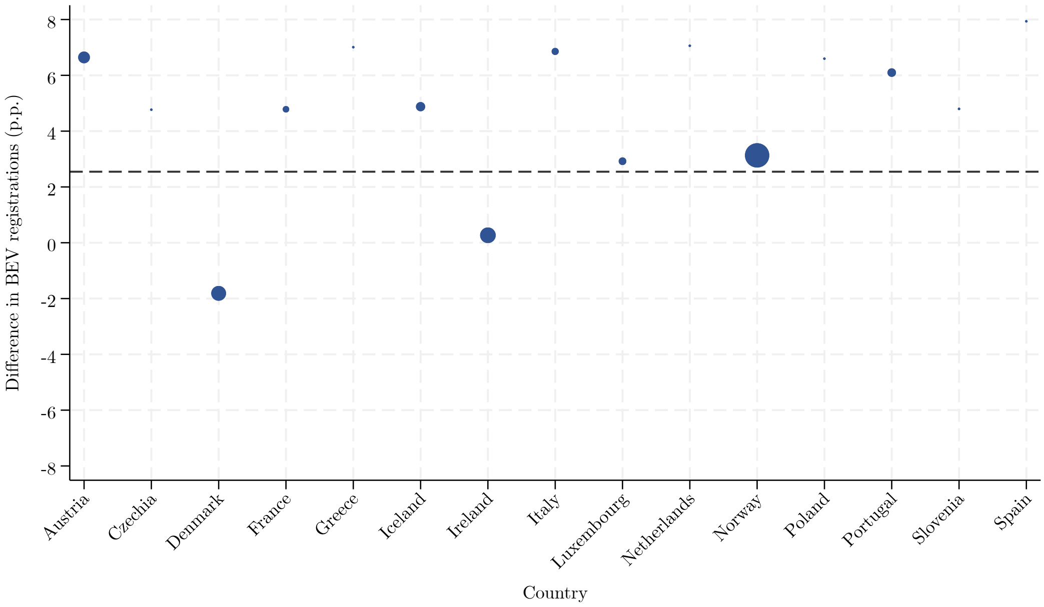

Figure A4: Country-by-Country Outcome Difference — EV Subsidy

Note: The figure plots the relative importance of unit weights country-by-country, and their relative outcome difference to Sweden. A larger dot size indicates a larger weight in the synthetic control. The horizontal line indicates the weighted average of these differences – the estimated effect.

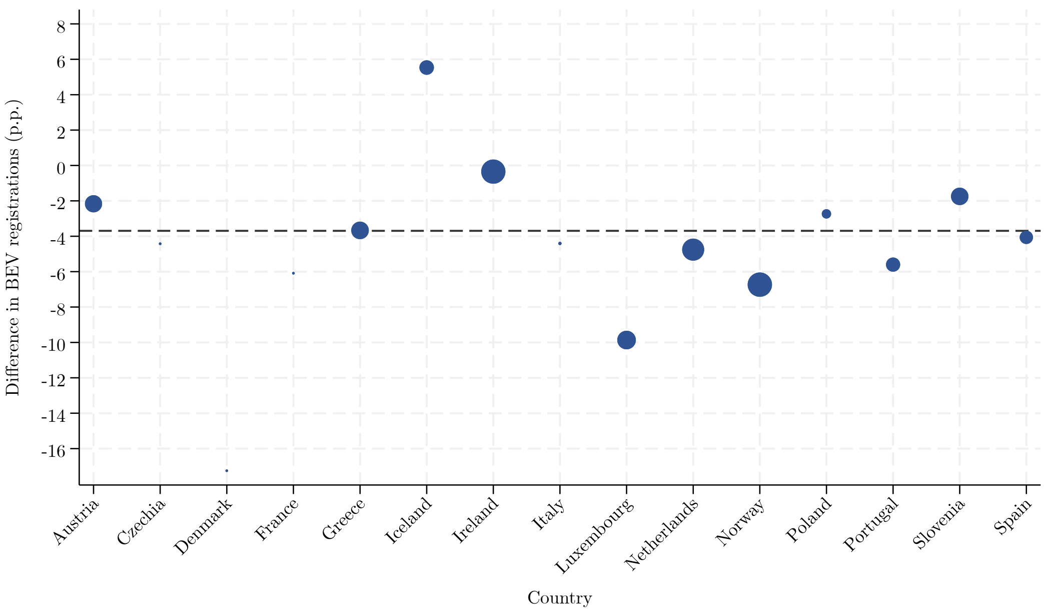

Figure A5: Country-by-Country Outcome Difference — Biofuel Blend-in Requirement

Note: The figure plots the relative importance of unit weights country-by-country, and their relative outcome difference to Sweden. A larger dot size indicates a larger weight in the synthetic control. The horizontal line indicates the weighted average of these differences – the estimated effect.

Overall, the synthetic control specification draws from a large range of control units, which suggests that the results are not particularly driven by any one unit in the donor pool, even though Norway generally receives the largest unit weight.

Table A3 presents the point estimates for each month's dynamic effect from the main specification in Section 6.2. The first column presents the point estimate, and the second column presents standard errors. The estimates in Column 1 of Table A3 indicate that there is a statistically significant initial increase in the BEV registration rate, which then becomes insignificant in August 2023.

Table A3: Dynamic Treatment Effects — Main specification

	BEV Share	
	Estimate	Standard Error
May 2022	8.950*	5.112
Jun 2022	8.789**	4.292
Jul 2022	8.046**	3.752
Aug 2022	8.728***	2.775
Sep 2022	7.512***	2.505
Oct 2022	4.680	3.678
Nov 2022	7.458**	3.304
Dec 2022	8.227***	2.940
Jan 2023	7.606**	3.103
Feb 2023	7.527**	3.184
Mar 2023	7.924**	3.870
Apr 2023	7.821*	4.553
May 2023	8.957*	4.796
Jun 2023	8.036*	4.296
Jul 2023	7.024*	4.102
Aug 2023	4.095	4.336
Sep 2023	1.924	4.942
Oct 2023	-1.611	6.133
Nov 2023	-1.983	5.545
Dec 2023	-2.362	5.153
Jan 2024	0.726	5.292
Feb 2024	2.909	6.725
Mar 2024	6.892	9.118
Apr 2024	9.394	10.157
May 2024	10.763	10.795
June 2024	8.348	11.032

Note: The tables shows the dynamic SDID point estimates for all post-treatment months. The dependent variable is the BEV registration share. The left column presents the point estimates and the right column standard errors. All standard errors are clustered at the country level. Significance is indicated by stars at levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4 show the point estimates for each month's dynamic effect from the robustness check specifications in Section 6.3. Column 1 corresponds to the dynamic specification in Section 6.3.1; Column 2 corresponds to the dynamic specification in Section 6.3.2; Column 3 corresponds to the first dynamic specification in Section 6.3.3; and Column 4 corresponds to the dynamic specification in Section 6.3.3. Table A5 presents the point estimate of the TWFE regression to calculate the subsidy incidence in Section 7.1. The point estimate is significant at the 0.1 percent level.

Table A4: Dynamic Treatment Effects - Robustness Checks

	Drop out Norway		Treatment = 2022m11		EV subsidy		Blend-in requirement	
	Estimate	SE	Estimate	SE	Estimate	SE	Estimate	SE
May 2022	11.159**	(5.039)	4.059	(2.870)	9.331*	(5.334)		
Jun 2022	10.606**	(4.249)	3.697	(2.549)	9.258**	(4.483)		
Jul 2022	9.371**	(3.698)	2.931	(2.783)	8.772**	(3.996)		
Aug 2022	10.335***	(2.632)	2.324	(2.788)	9.182***	(2.918)		
Sep 2022	8.668***	(2.401)	2.178	(3.411)	7.899***	(2.571)		
Oct 2022	5.005	(3.793)	1.495	(3.961)	4.835	(3.572)		
Nov 2022	7.947**	(3.445)	2.450	(4.213)	7.897**	(3.096)		
Dec 2022	9.221***	(3.037)	2.008	(3.732)	8.231***	(2.758)		
Jan 2023	8.334***	(3.117)	1.718	(3.676)	8.071***	(3.122)		
Feb 2023	8.588***	(3.132)	-0.344	(4.227)	8.139**	(3.266)		
Mar 2023	9.431**	(3.767)	-1.214	(5.261)	8.698**	(4.066)		
Apr 2023	9.366**	(4.530)	-4.700	(6.237)	8.700*	(4.713)		
May 2023	10.661**	(4.679)	-5.481	(5.708)	10.005**	(5.065)		
Jun 2023	9.317**	(4.232)	-6.846	(5.113)	8.985**	(4.460)		
Jul 2023	7.456*	(4.115)	-4.809	(4.987)	8.060**	(4.086)		
Aug 2023	3.941	(4.394)	-3.958	(6.221)	5.051	(4.179)		
Sep 2023	1.074	(4.949)	-1.024	(8.521)	2.799	(4.545)		
Oct 2023	-3.524	(5.788)	0.176	(9.502)	-0.629	(5.733)		
Nov 2023	-2.922	(5.495)	1.215	(10.134)	-1.121	(5.173)		
Dec 2023	-2.623	(5.175)	-0.866	(10.377)	-1.512	(4.897)		
Jan 2024	1.193	(5.354)					-6.567	(6.217)
Feb 2024	4.177	(6.819)					-6.148	(6.939)
Mar 2024	9.401	(9.177)					-5.312	(7.588)
Apr 2024	11.467	(10.421)					-3.726	(9.126)
May 2024	11.996	(11.189)					-2.386	(9.555)
Jun 2024	9.081	(11.455)					-3.352	(9.351)

Note: The tables shows the dynamic SDID point estimates for all post-treatment months. The dependent variable is the BEV registration share. The left column of each specification presents the point estimates and the right column standard errors. All standard errors are clustered at the country level. Significance is indicated by stars at levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Subsidy Incidence Result

	Price
Treatment	-73.019***
	(0.000)
Year-Month FE	Yes
Country FE	Yes
Observations	40
R-squared	0.987

Note: The table presents the point estimates and standard errors in parentheses. The dependent variable is price in SEK 1,000. Standard errors are clustered at the country level. Stars indicate significance at levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The sample includes Norway and Sweden observed between May 2022 and December 2023, of which Sweden is treated from November 2011.