Anticipation Effects in Vehicle Markets: Evidence from Sweden's Feebate Policy[*]

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Abstract

We study anticipatory behavior in response to Sweden's vehicle feebate policy, a reform announced seven months prior to implementation that subsidizes low-emission vehicles while imposing higher road taxes on high-emission vehicles. Using administrative data on vehicle registrations linked to individual and firm characteristics, we document that the early policy announcement caused anticipatory vehicle adoption, largely reflecting intertemporal substitution but also generating modest excess purchases. To avoid the forthcoming higher road taxes, households and firms systematically brought forward the adoption of more emissions-intensive vehicles, resulting in a dirtier composition of the vehicle fleet ahead of implementation. Anticipatory adoption is most pronounced among those for whom acting early is easiest or adapting to the policy is most costly. Dealer stock-management strategies, household and firm liquidity, and information salience amplify anticipatory purchases, while access to charging infrastructure mitigates them by lowering the cost of switching to cleaner vehicles. We develop an empirical framework to quantify the environmental implications of anticipatory behavior, accounting for excess adoption, the higher emissions intensity of pre-implementation purchases as well as the benefits from the accelerated fleet turnover, and estimate total environmental costs of approximately $28.39 million — around 21 percent of the policy's 2019 budget and 19 percent of its lifetime environmental benefits in 2019.

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I. Introduction

Climate policies are often announced far in advance of their implementation. This can be helpful in cases where early announcement gives firms time to invest in compliance technology (Dugoua 2023), but it can also allow polluters to shift their behavior forward in time, undermining the benefits of the policy, a phenomenon referred to as a "Green Paradox." In the vehicles setting, this concern is particularly acute: consumers may shift forward car purchases to avoid future bans, changes in technology, or increased costs, and because vehicles are durable goods with long lifespans, such anticipatory responses can lock in emissions trajectories that diminish the long-run environmental gains of the policy.[1]

Many countries and regions have proposed or enacted bans on the sale of new fossil-fuel vehicles by 2035, including the European Union (European Parliament and Council of the European Union 2023), the United Kingdom (HM Government 2021), and Canada (Government of Canada 2022), as well as several U.S. states following California's Advanced Clean Cars II standard (California Air Resources Board 2022). While such bans are central to long-run decarbonization strategies, they also raise the risk of substantial anticipatory behavior, including a surge in purchases of internal combustion engine vehicles before the bans take effect. Such pre-buying may undermine near-term emissions goals and prolong the presence of high-emitting vehicles on the road. Distributional concerns also arise if some demographic groups are better positioned to respond to upcoming restrictions.

The potential for anticipatory behavior is not limited to outright bans. Feebate systems provide a related but less stringent policy tool, combining subsidies for low-emission vehicles with higher annual registration fees for high-emission vehicles, and have already been implemented in a growing number of countries, including France, Norway, Sweden, Singapore, and New Zealand (International Council on Clean Transportation 2022). Although designed to shift adoption toward cleaner technologies, these policies may also induce anticipatory behavior if consumers accelerate purchases of high-emission vehicles before the fees take effect.[2] Whether such announcement effects undermine the environmental objectives of these policies ultimately depends on the magnitude of demand-side anticipatory responses — making precise empirical estimates of anticipatory behavior essential for evaluating and designing effective climate policy.

In this paper, we provide such estimates by exploiting the pre-announcement of Swe-

[1]Consumers adjusting their purchase timing can also have a pro-environment effect in some settings. In the months between the announcement of the end of federal electric vehicle incentives in the U.S. and the actual phaseout, electric vehicle sales spiked (https://www.npr.org/2025/09/30/nx-s1-5557153/ev-tax-credit-sales-spike).

[2]We use the term "adoption" to refer to the acquisition of new vehicles, whether through purchase or lease.

den's 2018 feebate (*bonus–malus*) reform — a system announced seven months prior to implementation — to study anticipatory behavior in vehicle markets. We combine administrative datasets covering all passenger vehicle registrations in Sweden from 2016 to 2019, linked to detailed vehicle attributes and sociodemographic characteristics of both individuals and firms, to address two core questions: (i) to what extent did consumers bring forward vehicle adoptions to avoid the upcoming registration fees, and how many of these adoptions represent additional purchases rather than mere timing shifts; and (ii) how did the emissions profile of newly registered vehicles change in anticipation of, and following, the reform.

First, we document pronounced anticipatory responses to the reform. In the seven months between announcement and implementation, individual registrations rose by approximately 36 per 100,000 residents (22 percent) while company registrations increased by around 30 per 100,000 residents (25 percent). Although the pre-implementation surge is largely offset by a similar post-implementation decline (19 percent for individuals and 24 percent for companies), which is consistent with substantial intertemporal substitution, a positive net effect remains, indicating that the reform induced additional vehicle adoption beyond simple timing shifts. The timing of anticipatory purchases differs across owner types: individuals concentrated their purchases in the months immediately preceding implementation, while companies appear to have shifted purchases over a longer window, consistent with companies having greater flexibility in replacement cycles. Decomposing by fuel type, the anticipatory surge is driven entirely by fossil-fuel vehicles, with petrol registrations accounting for the largest share of the pre-policy increase. In contrast, battery electric vehicle (BEV) registrations decline slightly ahead of implementation and increase markedly afterward, suggesting that prospective electric vehicle (EV) buyers delayed their adoption timing to be eligible for the upcoming rebate.

Second, vehicles adopted during the pre-policy period are more emissions-intensive, with average CO_2 emissions approximately 2.0 percent higher for individuals and 3.2 percent higher for companies, consistent with strategically bringing forward the adoption of dirtier models to avoid the higher road taxes introduced under the malus component of the reform. Following implementation, the composition of newly registered vehicles shifts toward cleaner alternatives, particularly among firms, which reduces their reliance on high-emission diesel vehicles. Together, these results suggest that anticipatory behavior amplified the environmental costs of the policy announcement by concentrating the adoption of high-emission vehicles in the pre-implementation period, while the post-policy incentive structure redirected adoption toward lower-emission cars.

In addition to quantifying the extent of anticipation, we document the mechanisms that shape anticipatory adoption. Anticipatory responses are largest where acting early is easiest

— as illustrated by dealer stock-management strategies that enabled consumers to avoid the fee with minimal behavioral change — or where adapting to the policy is most costly, as reflected in stronger responses among households facing high barriers to switching toward low-emission vehicles. Conversely, access to workplace charging infrastructure mitigated anticipatory purchases by lowering the cost of post-policy compliance.

First, we show that dealers play a central role in facilitating anticipatory adoption by expanding inventories and pre-registering high-emission vehicles ahead of implementation. These practices allowed dealers to lock in the lower pre-reform tax rate as road taxes were determined by the vehicle's initial registration date, enabling subsequent buyers to adopt high-emission vehicles without incurring the higher post-implementation fees. This circumvents the policy's intended incentive structure and represents a loophole in the reform.

Second, we document that financial flexibility is a key determinant of anticipatory behavior. High-income individuals and firms with greater net revenue exhibit the strongest anticipatory responses, advancing purchases and selecting higher-emission models, while low-income individuals make fewer anticipatory purchases and are less likely to adopt high-emission vehicles after implementation. These patterns reveal an uneven incidence of the feebate: financially stronger households and firms are best positioned to avoid the fee, while lower-income individuals are more likely to bear its costs.

Third, we provide two suggestive pieces of evidence that information salience shapes anticipatory responses. Aggregate Google search data show a marked increase in searches for "bonus–malus" prior to implementation, with search intensity peaking immediately afterward, suggesting broad awareness of the reform. The heterogeneous effects by education — where the most educated individuals exhibit the largest anticipatory responses — further indicate that access to and processing of policy-relevant information amplifies pre-policy purchasing behavior.

Finally, we examine factors related to the ease of policy compliance. Individuals and firms with higher prior vehicle usage and dirtier pre-policy fleets exhibit stronger anticipatory responses, consistent with greater incentives to avoid the forthcoming fee. Conversely, access to workplace charging infrastructure mitigated anticipatory purchases by lowering the cost of switching to a subsidized low-emission vehicle, and increased post-policy EV adoption.

Given the central role of vehicle policies in decarbonization strategies, we assess how anticipatory behavior affected the environmental effectiveness of the reform. We quantify the net environmental impact by decomposing three channels: (i) the environmental cost of advancing the purchase of higher-emission vehicles that, absent anticipation, would have been cleaner under the post-policy regime; (ii) the cost generated by the increase in total vehicle adoptions during the anticipation window; and (iii) the potential environmental benefit associated with the accelerated retirement of older, higher-emission vehicles. We estimate net environmental damages of approximately $27.8 million, equivalent to around 21 percent of the total budget allocated to the policy in 2019 and approximately 18 percent of the estimated lifetime environmental benefits of the reform in that year. These costs are disproportionately driven by firm anticipatory behavior, reflecting the greater emissions gap between anticipatory and post-policy purchases among firms, while for individuals the primary environmental cost stems from excess vehicle adoption rather than changes in vehicle composition.

This paper contributes to the "Green Paradox" literature on anticipatory behavior in advance of climate policies.[3] While much of this literature focuses on fossil fuel supply markets (Norman and Schlenker 2024; Hoel 2010; Sinn 2008; Ploeg and Withagen 2012, 2015), recent work documents similar dynamics in vehicle markets: Holland et al. (2021) show theoretically that bans on gasoline vehicles can induce pre-ban purchase spikes, and Rittenhouse and Zaragoza-Watkins (2018) empirically document anticipatory purchasing in the U.S. heavy truck market ahead of stricter emission standards. We extend this literature by exploiting rich Swedish administrative data to study anticipatory responses along multiple margins — adoption timing, vehicle composition, and emissions intensity — going beyond the question of whether anticipation occurs to characterize who responds and through which channels. We also study a feebate rather than an outright ban, documenting that even relatively moderate policy announcements can generate anticipatory responses and environmental costs, a finding with direct relevance for the design of climate policy more broadly.

Second, we contribute to a growing literature showing that vehicle policies can be undermined by strategic consumer responses, including emissions standards (Gruenspecht 1982), license plate–based driving bans (Davis 2008), incentivized vehicle retirement programs (Li et al. 2013), and fossil fuel vehicle bans (Holland et al. 2021). We build on this work by providing an empirically implementable framework that quantifies the environmental consequences of anticipatory behavior, decomposing announcement-induced damages into excess

[3] More broadly, our study relates to a literature documenting anticipatory behavior in response to expected policy changes that extends beyond the "Green Paradox" and "Blue Paradox" in environmental economics (Sinn 2012; Jensen et al. 2015; McDermott et al. 2019). In the context of firearm regulation and tobacco taxation, Bollman et al. (2025) document a "Steel Paradox," in which individuals increase gun purchases in anticipation of forthcoming bans, while Becker et al. (1994) show that smokers respond to expected tax hikes by stockpiling cigarettes to avoid future price increases. Similar patterns have been observed in labor economics, where anticipatory adjustments in earnings or employment prior to program eligibility are known as "Ashenfelter dips" (Ashenfelter and Card 1985). Likewise, in the context of birth timing, Gans and Leigh (2009) document how parents in Australia postponed childbirth to take advantage of a forthcoming subsidy. Macroeconomics research similarly emphasizes the intertemporal substitution of durable goods consumption in anticipation of future changes in prices or credit conditions (Ogaki and Reinhart 1998; Gowrisankaran and Rysman 2012; Gavazza and Lanteri 2021).

vehicle adoption, changes in vehicle composition, and accelerated vehicle turnover — objects that can be estimated across a wide range of vehicle policy settings. Relative to Holland et al. (2021), who develop a theoretical framework for how advance announcements induce pre-implementation purchasing, we provide direct empirical evidence on these responses and trace how they translate into realized emissions.[4]

Third, we add to research on existing feebate or bonus-malus systems. The evidence from the literature is mixed, with some papers finding that these systems can dramatically shift purchase behavior (Yan and Eskeland 2018), while others find limited effects on new cars (Alberini and Bareit 2019) and mixed effects on the retirement of older cars (Alberini et al. 2018).[5] Most closely related, D'Haultfœuille et al. (2014) note the existence of anticipatory purchases ahead of a French bonus–malus reform, though the anticipatory effects were more short-lived, likely reflecting the shorter announcement window relative to the Swedish reform we study. Relative to this existing evidence, we show that anticipatory responses can generate persistent excess adoption and systematically dirtier vehicle choices, substantially amplifying the environmental costs of policy announcements.

Finally, this paper contributes to the difference-in-differences literature by highlighting the importance of accounting for anticipation effects when policies are announced ahead of implementation. As Malani and Reif 2015 argue, failing to align the treatment timing with the onset of responses can lead to biased estimates, as anticipatory effects may manifest as pre-trends and be mistakenly interpreted as violations of the parallel trends assumption. Several studies address this concern by using the policy announcement date as the effective treatment timing (Di Maria et al. 2014; Alpert 2016), while others explicitly model both announcement and implementation periods to capture the full dynamic response to policy (Ladino et al. 2021; Dong and Klaiber 2019; Bilicka et al. 2025). Our setting provides an empirical illustration of this concern, documenting how anticipatory responses emerge precisely in the window between announcement and implementation.

The rest of the paper proceeds as follows. Section II presents the theoretical model of vehicle choice. Section III summarizes the policies and underlying data. Section IV provides the empirical methodology. Section V presents the empirical results. Section VI concludes with policy implications.

[4]Rittenhouse and Zaragoza-Watkins 2018 also measure the change in environmental damages using a similar approach to ours. However, due to the nature of the engine standards they consider, they focus on local pollutants while we focus on CO_2 emissions.

[5]Feebates can also lead to the Gruenspecht effect, whereby owners delay scrappage of existing high-emission vehicles; see Gruenspecht (1982). In our setting, we do not find significant evidence of reduced de-registration of vehicles.

II. Theoretical Model

When policies are announced in advance, forward-looking consumers may adjust the timing of durable goods purchases in anticipation of future taxes or subsidies. Such anticipation effects alter the effective policy incentives faced by consumers and can therefore change the optimal design of taxes and subsidies relative to a setting without anticipation. To make this point transparent, we develop a simple theoretical framework that shows how optimal vehicle taxes and subsidies depend on the presence and magnitude of anticipation effects.

We extend the theoretical discrete choice model from Holland et al. 2016, in which consumers choose between a gasoline vehicle and an EV, either of which can be purchased immediately (time $t = 0$) or one period in the future (time $t = 1$). As in their model, consumer utility comes from a composite good x and from miles driven over the vehicle lifetime – either gasoline miles g_t or electric miles e_t.

The government can implement a range of policy tools that apply to future (period 1) purchases, including a subsidy s for the purchase of an EV or a tax τ on the purchase of a gasoline vehicle, or a combination of these policies. Fuel prices and vehicle prices are fixed.

The indirect utility from purchasing a gasoline vehicle immediately is

$$V_{g0} = \max_{x,g_0} x + f(g_0) \text{ such that } x + p_g g_0 = I - p_\Psi$$

where p_g is the price of a gasoline mile, p_Ψ is the price of a gasoline vehicle, I is income, and $f()$ is concave.

Similarly, the indirect utility from purchasing a gasoline vehicle next period is

$$V_{g1} = \max_{x,g_1} x + \beta f(g_1) \text{ such that } x + \beta p_g g_1 = I - \beta(p_\Psi + \tau)$$

where utility from mileage and vehicle prices are accrued or paid next period, and are therefore discounted by the discount factor β.

We also define indirect utility from purchasing an EV, either now (V_{e0}) or in the future (V_{e1}), as follows:

$$V_{e0} = \max_{x,e_0} x + h(e_0) \text{ such that } x + p_e e_0 = I - p_\Omega$$
$$V_{e1} = \max_{x,e_1} x + \beta h(e_1) \text{ such that } x + \beta p_e e_1 = I - \beta(p_\Omega - s)$$

where p_e is the price of an electric mile, p_Ω is the price of an EV, and $h()$ is concave. The quasi-linear objective function means that there are no income effects.

The utility from each choice is the sum of indirect utility from fuel f and time t V_{ft} and an iid random variable ε_{ft} drawn from an extreme value distribution with mean 0 and standard deviation proportional to μ:

$$\mathcal{U}_{ft} = V_{ft} + \varepsilon_{ft}$$

Consumers choose the vehicle that maximizes utility U_{ft}. The probability that they choose a particular vehicle-time combination is

$$\pi_{ft} = \frac{\exp(V_{ft}/\mu)}{\sum_{h\in\{g,e\}}\sum_{r\in\{0,1\}}\exp(V_{hr}/\mu)}.$$

The expected utility of a new vehicle purchase is

$$\mathbf{E}[\max[\mathcal{U}_{g0}, \mathcal{U}_{g1}, \mathcal{U}_{e0}, \mathcal{U}_{e1}]] = \mu \ln(\exp(V_{g0}/\mu) + \exp(V_{g1}/\mu) + \exp(V_{e0}/\mu) + \exp(V_{e1}/\mu))$$

Driving both kinds of vehicles produces negative externalities, which are not considered by consumers. Gasoline vehicles produce δ_g of marginal damages in dollars/mi, while EVs produce δ_e of marginal damages in dollars/mi. We consider the welfare maximizing tax/subsidy policy, where welfare $\mathcal{W}$ is the sum of expected utility and expected revenue (R_t in each period t, which is a function of subsidy s or tax τ) minus expected pollution damages:

$$\mathcal{W} = \mu \ln(\exp(V_{g0}/\mu) + \exp(V_{g1}/\mu) + \exp(V_{e0}/\mu) + \exp(V_{e1}/\mu)) + R_0 + \beta R_1 - \delta_g(\underbrace{\pi_{g0}g_0}_{G_0} + \beta \underbrace{\pi_{g1}g_1}_{G_1}) - \delta_e(\underbrace{\pi_{e0}e_0}_{E_0} + \beta \underbrace{\pi_{e1}e_1}_{E_1})$$

Optimizing the welfare function gives the following result. The second-best tax in period 1 is given by τ^* where

$$\tau^* = \frac{\delta_g\left(\frac{\partial \pi_{g0}}{\partial \tau}g_0 + \beta\frac{\partial \pi_{g1}}{\partial \tau}g_1\right) + \delta_e\left(\frac{\partial \pi_{e0}}{\partial \tau}e_0 + \beta\frac{\partial \pi_{e1}}{\partial \tau}e_1\right)}{\beta\frac{\partial \pi_{g1}}{\partial \tau}}$$

When the policy in period 1 can be implemented as a surprise, such that period 0 purchases are already made, this expression simplifies to $\delta_g g_1 - \delta_e e_1$, as in Holland et al. 2016. The optimal tax in this case is a function of the difference in externalities associated with gasoline and EVs. A proof of these results is in Appendix A.

On the other hand, in the case when the policy is pre-announced, the optimal tax depends on the partial derivatives of the choice probabilities with respect to τ, i.e., it is necessary to understand how purchase probabilities will shift across fuel and time in response to future subsidies (or taxes). A period 1 gas tax induces two kinds of responses: an intertemporal shift to period 0 gas vehicles and a (policy-intended) shift to EVs. Only the latter generates the environmental benefit the tax is intended to produce. Thus, the optimal tax will depend on the magnitude of policy-intended fuel shifts relative to counterproductive intertemporal shifts. Specifically, we show in the appendix that as the share of gasoline purchases in period 1 that switch to gasoline purchases in period 0 (α) increases relative to the share that shifts to EVs, the optimal period 1 tax decreases ($\frac{\partial \tau^*}{\partial \alpha} < 0$) because the externality benefit of the policy declines. In the empirical results that follow, we examine the magnitude of these intertemporal shifts.

III. Background & Data

A. Swedish Feebate System

Since July 2018, Sweden has had a feebate system in place – referred to as the *bonus–malus* scheme – that rewards low-emission vehicles with rebates and penalizes high-emission vehicles with higher road taxes.[6] The policy aims to reduce transport-sector emissions by discouraging fossil fuel vehicles and encouraging adoption of cleaner alternatives.

The *bonus–malus* system, first announced in December 2017 (Ministry of the Environment 2017), applied to new vehicles registered after July 1, 2018. Eligibility for the bonus component was restricted to low-emission vehicles, defined as private cars, light lorries, and light buses complying with EU Euro 5 or 6 emission standards. Battery electric vehicles (BEVs) qualified for the maximum bonus, while plug-in hybrid electric vehicles (PHEVs) received a reduced bonus that declined with the vehicle's carbon dioxide emissions – that is, its standardized CO_2 emissions per kilometer as recorded at first registration. To discourage early resale abroad, the bonus was disbursed no sooner than six months following initial registration. The malus component imposes a higher annual vehicle tax on new fossil-fuel vehicles based on their carbon emissions during the first three years following registration. For diesel vehicles, an additional environmental and fuel surcharge is applied. In Appendix C, we present detailed information on the *bonus–malus* system and the changes introduced in each update.

Figure 1 illustrates the rebate schedule for vehicles with $CO_2 \leq 60$ g/km and the road taxes for petrol and diesel vehicles. As an example, a Volvo XC90 T8 (PHEV, 40 g/km,

[6] We refer to the Swedish bonus–malus system as a "feebate" policy, though it was not revenue-neutral. Bonus payments to low-emission vehicles consistently exceeded malus revenues, especially early on, reflecting its primary role as an environmental incentive rather than a fiscally balanced measure.

$76,000) received a bonus of $2,668, whereas a Volvo XC60 D5 (diesel, 148 g/km, $47,080) incurred an annual road tax of $716 for the first three years.

To verify that the public was aware of the reform, Figure B1 presents Google Search Index data for the term *bonus–malus*, which reveals an increase in search activity in the months preceding implementation, consistent with widespread awareness of the policy change.

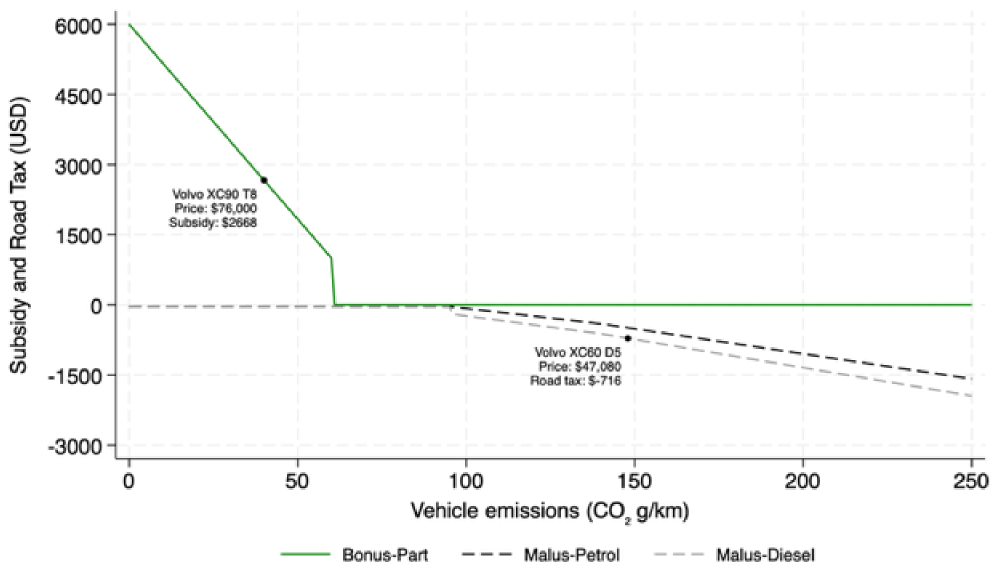


Figure 1: Bonus-malus system introduced in July 2018

Notes: This figure shows the bonus (one-time subsidy) for low-emission vehicles and the malus (annual road tax) for petrol and diesel vehicles as of July 2018. The green line represents the bonus for low-emission vehicles with $CO_2 \leq 60g/km$. The dashed gray line indicates the malus for petrol vehicles, while the lighter dashed gray line represents the malus for diesel vehicles, which are subject to a higher tax due to an additional environmental damage charge and extra fuel surcharge.

B. Data Sources

To study how individuals and firms respond in anticipation of Sweden's feebate system, we combine information from several administrative sources provided by Statistics Sweden (*Statistiska centralbyrån*). The primary data source is the Swedish vehicle register (*Fordonsregistret*), which covers all passenger vehicles in Sweden. For individuals, we supplement this with sociodemographic characteristics drawn from the longitudinal integrated database for health insurance and labor market studies (LISA), the Swedish occupational register (*Yrkesregistret*), and the geographic database (*Geografidatabasen*). For firms, we incorporate financial and employment characteristics from the Swedish business register (*Företagsregistret*). All data cover the period from 2016 to 2019. We supplement these data with information on the public charger infrastructure, the financial implications of vehicle reforms, and macroeconomic control variables.

Vehicle data. We use data from the Swedish vehicle register on new passenger vehicles. The data include information on the ownership type (private individual, company, or dealer),[7] the general status of the vehicle (registration date, whether it is leased, and when the vehicle became the property of the current owner), vehicle specifications and characteristics (make, model, trim, service weight, fuel type, fuel intensity, and carbon emission intensity), the manufacturer's suggested retail price, and annualized vehicle kilometers traveled. Each registration includes a vehicle identification number and an anonymized individual- and firm-level identifier, uniquely linking each vehicle to an individual, company, and retailer.

Demographic characteristics. To match individuals to their vehicles, we link the Swedish vehicle register to the LISA database using individual identification numbers. LISA contains sociodemographic information for the Swedish adult population (aged 16 and above) registered as of December 31 each year since 1990, including gender, age, family status, gross salary, education, and serves as the basis for our population counts at the geographic unit level. We further link these data to the Swedish occupational register, which provides annual information on employment status, workplace industry code, and duration of employment. We supplement both sources with geographic coordinates of individuals' place of residence and workplace, measured at the level of 250-meter grid cells in urban areas and 1,000-meter grid cells in rural areas.

Firm data. For companies, we draw on the Swedish business register, which provides firm-level information on the number of employees, net revenue, personnel costs, and social contribution costs. As with individuals, we supplement these data with the geographic location of each firm's workplace, measured at the level of 250-meter grid cells in urban areas and 1,000-meter grid cells in rural areas.

Additional data sources. We obtain information on the financial benefits of vehicle rebates and annual road taxes from government bills from the Ministry of the Environment 2017. Data on price and tax components for various fuel types are acquired from tax calculation conventions (*Beräkningskonventioner*) by the Swedish Ministry of Finance 2015. Monthly GDP data (in 2011 SEK) are obtained from Statistics Sweden, and monthly petrol as well as crude oil prices for the same period are drawn from *Drivkraft Sverige*.

To examine whether pre-existing public charging infrastructure influenced the magnitude of anticipation effects, we incorporate data from ChargeX (*Uppladdning.nu*), which

[7] We distinguish non-retailer companies from car dealers using the Swedish Standard Industrial Classification (SNI), which identifies the industry of the vehicle's legal owner.

provides detailed information on the number, location, and technical characteristics of Sweden's public charging stations. We merge the charger data with residential and workplace neighborhoods using geographic coordinates to spatially link charging infrastructure to individuals' places of residence and employment.

C. Sample construction

To measure anticipation effects on new car registrations, we construct two separate monthly panels of new passenger-car adoptions by end users — private individuals and firms — at the neighborhood level. We distinguish between these two end-user types because private individuals and firms differ systematically in both their vehicle registration responses and the emissions profile of adopted vehicles, with important implications for environmental outcomes and the mechanisms underlying anticipatory behavior.

We classify a vehicle as an end-user adoption if it is recorded as newly adopted in a given year (according to the Statistics Sweden indicator for "new" vehicles) and the registered owner at the end of that year is either a private individual or a firm. This definition accounts for cases in which a vehicle is initially registered to an intermediary — such as a leasing company, dealer, or manufacturer — and subsequently transferred to the final owner within the same year.[8] To ensure that these panels reflect actual adoption behavior, we match each new registration to its end user using the first vehicle registration date as the timing of adoption, accounting for the practice of dealers initially registering vehicles before transferring ownership to the end user.

To aggregate individual and firm-level data to the neighborhood level, we follow Statistics Sweden's definition of *Demografiska statistikområden* (DeSO), which delineates small and consistent geographic units nationwide. Specifically, we define neighborhoods as all individuals and firms residing within 250-meter grid cells in urban areas and 1,000-meter grid cells in rural areas. To estimate the impact on vehicle characteristics, we restrict the sample to vehicles newly registered by private individuals and companies.[9]

The sample period spans from January 2016 — coinciding with the introduction of the super green car premium (*supermiljöbilspremie*), a subsidy for low emission vehicles — to July 2019. We end the sample at this point to avoid contamination from anticipatory behavior in response to the January 2020 reform, which tightened the bonus–malus system

[8] We define dealer-mediated vehicles as those recorded as newly adopted in a given year for which either the immediate previous owner or the second previous owner is identified as a dealer. Although this definition may include a small number of vehicles transferred through staff arrangements, it captures the majority of dealer-mediated transactions.

[9] To exclude new vehicle adoptions by private individuals that were likely made for commercial purposes, we restrict the sample of private individuals to those who registered no more than three new cars between 2016 and 2019.

by adopting the WLTP emissions testing standard, reporting higher combustion vehicle emissions than the previous test and thereby increasing taxes on fossil-fuel vehicles. This is supported by a noticeable increase in residual variance in vehicle registrations after July 2019 (Figure 3), consistent with renewed anticipatory purchasing ahead of the 2020 reform.

Table 1 reports neighborhood-level summary statistics on vehicle fleet composition and average vehicle characteristics among newly registered cars for individuals (Column 1) and firms (Column 2) in the pre-implementation year (2017). Vehicles registered to individuals represent a slightly larger share of the new vehicle fleet than those registered to companies. Leasing is substantially more prevalent among firms, with approximately 76 percent of company vehicles leased compared to 24 percent for individuals. Firms disproportionately adopt diesel vehicles and register heavier, more powerful cars, though emissions intensity and particle values are slightly lower than for individually owned vehicles. These differences likely reflect variation in preferences, usage patterns, and financial arrangements between the two groups, and may in turn shape how each responds to the bonus–malus reform.[10] In the results that follow, we separately estimate effects for individual and company cars.

D. Descriptive evidence

Before turning to the regression analysis, we first present aggregate time trends in new passenger vehicle registrations for different fuel types. Figure 2 plots monthly new registrations between January 2016 and July 2019 for petrol (red), diesel (blue), and EVs (green), along with total new registrations (black dashed line). The figure also reports the average emissions intensity of newly registered vehicles. The vertical dashed line marks the policy announcement in December 2017, while the dashed red line denotes the implementation of the bonus–malus system in July 2018.

We observe a sharp spike in new registrations of petrol and diesel vehicles between the policy announcement and its implementation in July 2018, consistent with anticipation effects aimed at avoiding the higher taxes introduced under the malus component of the reform.[11] Following implementation, registrations dropped sharply and remained depressed for approximately six to eight months before gradually returning to pre-reform levels. This pattern is indicative of intertemporal substitution, whereby purchases were brought forward in anticipation of the policy. This pre-announcement surge was driven by both individuals and non-retailer firms (Figure B4). Consistent with this compositional shift, the average

[10] Figure B2 shows that petrol and diesel vehicles account for the highest annual driving distances across both groups, with diesel exceeding petrol among companies but not individuals. BEVs, PHEVs, and hybrid electric vehicles exhibit substantially lower and relatively stable travel distances over the 2016–2019 period.

[11] Although our primary focus is on pre-buying behavior of passenger cars, Figure B3 shows that similar anticipation effects are also evident among lorries.

Table 1: Vehicle summary statistics

	Individuals		Companies	
	Mean	Std. Dev.	Mean	Std. Dev.
A. Vehicle fleet				
New car registration	2.47	2.03	1.85	15.41
New lease	0.58	0.87	1.41	12.05
New petrol car	1.46	1.45	0.35	6.07
New diesel car	0.81	1.01	1.20	9.72
New hybrid car	0.16	0.41	0.07	1.30
New PHEV	0.03	0.17	0.15	1.37
New BEV	0.01	0.11	0.02	0.33
B. Vehicle characteristic				
Fuel intensity (liter/100km)	5.25	0.82	5.29	1.09
Vehicle emissions (gCO2/km)	124.95	21.21	124.44	34.23
Vehicle weight (kg)	1967.78	238.03	2261.51	314.37
Particle value	0.69	0.54	0.50	0.42
Engine Power (kW)	104.79	30.42	126.86	37.96
N(Observation)	71,832		71,844	

Notes: This table presents neighborhood-level descriptive statistics for the vehicle fleet (Panel A) and for the attributes of newly registered vehicles (Panel B) in 2017, separately for individuals (Column 1) and companies (Column 2).

emissions intensity of newly registered vehicles increased sharply in the months preceding implementation, reflecting the surge in purchases of high-emission petrol and diesel models. Immediately after the reform took effect, average emissions dropped, indicating that the policy both deterred high-emission registrations and accelerated the adoption of lower-emission vehicles once the malus was in place.

Figure B5 shows the time trends for electric cars. We observe a surge in hybrid and PHEV registrations prior to January 2020, coinciding with the introduction of a new emissions testing method into the bonus-malus system. This change implemented stricter emission testing standards, resulting in fewer PHEVs qualifying for the bonus. This surge likely reflects the urgency of informed pre-buyers who, anticipating the policy change, accelerated their purchasing decision.

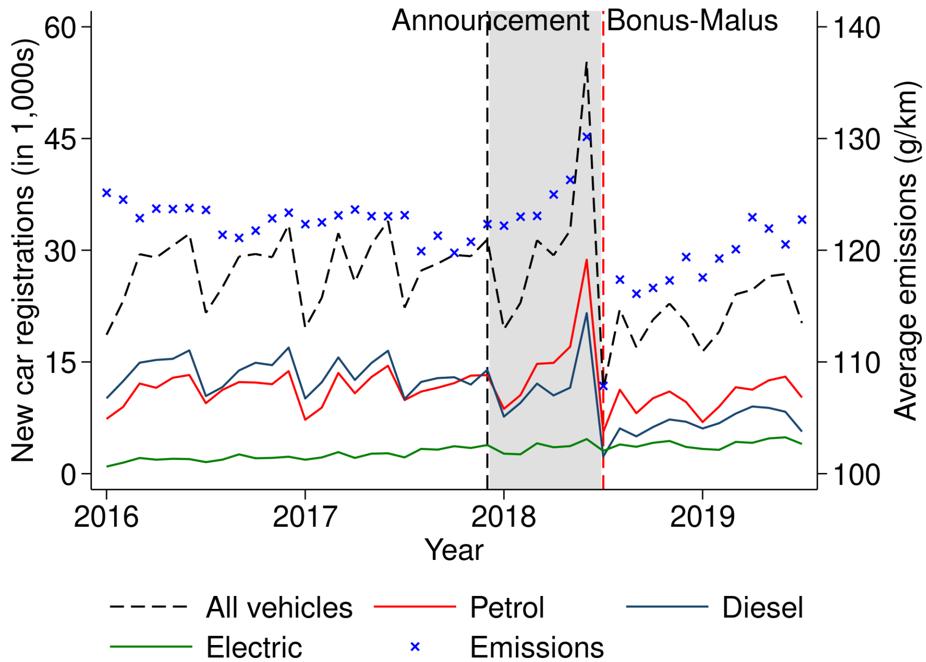


Figure 2: New vehicle registrations by fuel type

Notes: This figure shows monthly new passenger vehicle registrations (in 1,000s) in Sweden from January 2016 to July 2019 (left y-axis), disaggregated by fuel type together with the average emissions of newly registered vehicles (y-axis). The black dashed line indicates total new car registrations, while the red, blue, and green lines represent petrol, diesel, and EVs, respectively. The blue crosses represent average emissions of new cars. The vertical dashed line marks the policy announcement, and the dashed red line denotes the implementation of the bonus–malus system in July 2018.

IV. Empirical methodology

This section outlines the empirical strategy used to identify anticipation effects, estimate the dynamic response to the policy announcement, and provides validity tests of the underlying identification assumptions.

A. Main Specification

We estimate the impact of the policy using a panel regression model that compares monthly vehicle registrations before and after the policy announcement. Following Lam and Bausell (2007) and Rittenhouse and Zaragoza-Watkins (2018), we model monthly new vehicle adoptions as a function of key macroeconomic drivers, demographic characteristics for individuals, and firm-level characteristics for companies. In the absence of anticipation effects, these variables should account for most of the observed variation over time; systematic deviations during the announcement-to-implementation window are thus interpreted as anticipatory responses. The main specification is estimated at the neighborhood–month level. We run the regression separately for individuals i and firms f in neighborhood h and month t, as follows:

$$Q_{jht} = \gamma \text{Pre}_t + \beta \text{Post}_t + \delta X_{jht} + \rho_t + \mu_{jh} + \varepsilon_{jht} \quad \text{for } j \in \{i, f\}, \tag{1}$$

where the main outcomes of interest, Q_{iht} and Q_{fht}, refer to the total number of newly adopted passenger vehicles by individuals and companies per 100,000 residents in neighborhood h during month t. We also estimate equation (1) for new registrations of different fuel types–petrol, diesel, hybrid, EVs–to explore compositional changes in response to the policy. The indicator variable Pre_t equals 1 for the seven-month window between the policy announcement (December 2017) and implementation (July 2018), while Post_t equals 1 for the seven months following policy implementation (July 2018 through January 2019). We select this post-policy window for two main reasons. First, the dynamic treatment effects become statistically insignificant for both individuals and companies around January 2019 (Figure 4), and residual variation in vehicle registrations largely dissipates by this point (Figure 3). Second, this window ensures a symmetric pre- and post-policy period. The coefficient of interest, γ, captures the anticipation effect prior to implementation — that is, the change in new vehicle registrations during the pre-policy period relative to baseline. The coefficient β measures the effect of the policy in the post-implementation period.

For the individual-level regressions, the covariate vector X_{iht} comprises neighborhood-averaged demographic characteristics (age, gender, income, education, employment, marital, and parental status) and macroeconomic indicators (monthly Swedish GDP and real oil prices). For the company-level regressions, X_{fht} includes neighborhood-averaged firm characteristics (number of employees, value added, and net revenues) alongside the same macroeconomic indicators. Month-of-year fixed effects ρ_t control for seasonal trends, and μ_{jh} captures neighborhood-fixed effects. We estimate the regression on the neighborhood-level for total new car registrations, and on the individual-level for new car characteristics (conditional on adopting a new car).

In addition to total vehicle registrations, we examine intensive-margin adjustments by estimating equation (1) on a sample of newly registered vehicles. To do this, we replace the outcome variable Q_{jht} with y_{jt}, representing average carbon emissions intensity (g/km), fuel intensity (liters/100 km), and particle emissions (μg/m^3) and estimate this specification at the new vehicle×month level. This regression includes individual demographic and firm-level characteristics directly, rather than their neighborhood averages.

B. Dynamic estimates

To assess the timing and evolution of policy responses, we estimate a dynamic model that allows the effect of the policy to vary by month relative to the announcement and implementation dates. The event study specification for individuals i and companies f is given by:

$$Q_{jht} = \sum_{k \in \{-m,\ldots,0,\ldots,n\}} \gamma_k D_{m,t-k} + \rho_t + \mu_{jh} + \bar{X}_{jht} + \varepsilon_{jht} \quad \text{for } j \in \{i, f\}, \tag{2}$$

where k indexes event time relative to the policy announcement (December 2017), and D_t^k is an indicator variable equal to 1 in the month that is k periods relative to the announcement date, and 0 otherwise. The omitted category is the month immediately prior to the announcement (November 2017), so all coefficients γ_k are interpreted relative to that baseline. The remaining variables are defined as in equation (1).

C. Validity tests

The key identifying assumption underlying our empirical strategy is that, absent the policy announcement, vehicle registrations would have followed similar trends across months before and after the announcement, conditional on our control variables and fixed effects. We offer two pieces of evidence in support of this assumption. First, the dynamic specification in equation (2) allows us to visualize the trajectory of vehicle registrations and emissions characteristics over time. Figure 4 shows no evidence of pre-trends in either vehicle registrations or average vehicle characteristics prior to the announcement period.

Second, we assess covariate balance across buyers who adopted vehicles in the first and second half of 2017 — the year prior to implementation — to verify that the composition of vehicle buyers does not systematically vary within a year. Following Imbens and Rubin (2015) and Baker et al. (2025), we employ the normalized difference in means as a scale-free measure of imbalance, with values below 0.25 conventionally indicating negligible imbalance. Tables B1 and B2 report means, standard deviations, and normalized differences in demographic and vehicle characteristics for individuals and companies, respectively. The normalized differences are small across all variables, indicating that buyers in the two halves of 2017 were similar in both their demographic and vehicle characteristics. This suggests that the seasonal composition of the car market is stable within a year, supporting the validity of our identifying assumption that the composition of vehicle buyers does not differ between the first and second half of the year.

V. Regression results

A. Anticipation effects on car registrations and characteristics

Before presenting the regression results, we first provide graphical evidence to illustrate the pre- and post-purchase behavior by individuals and firms triggered by the policy change. To evaluate whether anticipatory responses affected vehicle sales, we analyze the residual variation — defined as the portion of variation unexplained by our regression model (equation 1, excluding the Pre and Post indicators) — around the policy implementation period. If individuals or firms adjusted their adoption behavior in anticipation of the policy, we would expect to observe positive residuals in the period preceding implementation, followed by negative residuals thereafter.

Figure 3 provides visual evidence supporting the anticipation hypothesis among individuals and non-retailer firms. The residuals exhibit a pattern indicative of intertemporal shifts in vehicle sales in response to the policy change. Specifically, in the months leading up to July 2018 — when the bonus-malus system was implemented — residuals are consistently positive, reaching a peak immediately prior to the policy's introduction. Following implementation, residuals turn negative, suggesting a drop in sales relative to model predictions. Notably, the pre-policy surge appears larger in magnitude than the post-policy decline, suggesting that anticipation led to net additional vehicle sales. The surge is also concentrated in the two months immediately preceding implementation, while the post-policy decline unfolds more gradually over approximately six months. A likely explanation for this asymmetric pattern is that individuals and firms who shifted forward purchases planned for later in 2018 did not want to acquire their new vehicles needlessly early, compressing anticipatory purchases into the period just before the reform took effect.

Column (1) of Table 2 presents the estimated pre- and post-policy coefficients from equation (1) for new vehicle registrations per 100,000 (working age) inhabitants. For individuals (Panel A), monthly new registrations increased by roughly 37 vehicles per 100,000 people during the seven-month period between the policy announcement and implementation (corresponding to a 22 percent increase or a total of 21,611 anticipatory purchases)[12]. Following the policy's implementation, the coefficients indicate an almost symmetric decline of approximately 33 vehicles per 100,000 inhabitants, implying that the pre-policy surge was primarily driven by intertemporal substitution.[13] In other words, most individuals advanced

[12]Total anticipatory purchases are calculated by multiplying the monthly increase per 100,000 working age population by the number of months in the anticipatory period (7) and the Swedish working age population in hundreds of thousands (84.1)

[13]Table E1 shows that pooling individuals and firms yields registration effects that closely match the aggregate of the separate owner-type estimates, confirming that the overall pre- and post-policy effects

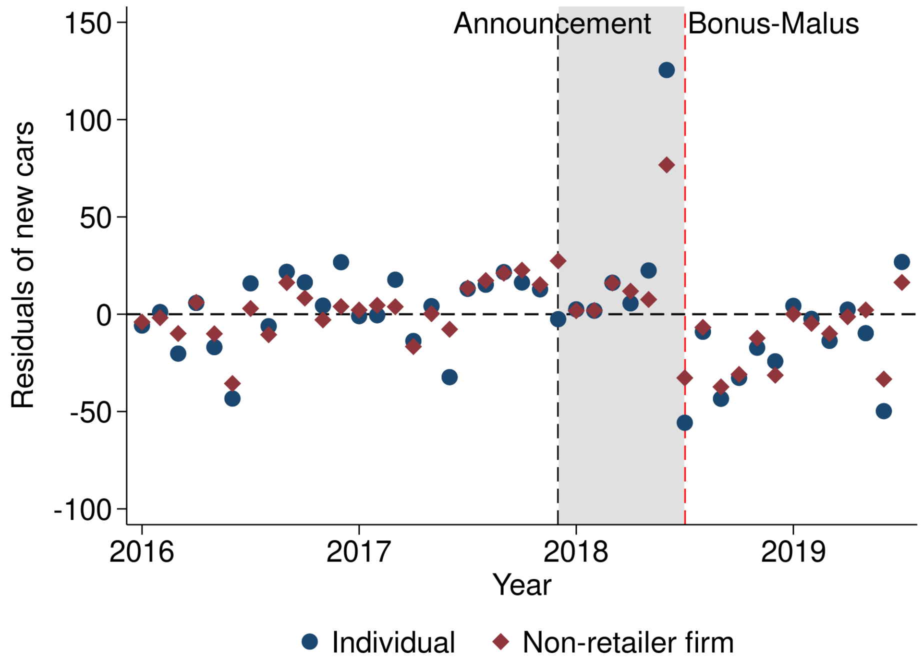


Figure 3: Monthly residuals of car registrations

Notes: This figure displays the residuals for individuals (blue circles), and firms (brown squares) from the regression model in equation (1) at the neighborhood-level, excluding the Pre and Post indicators. The y-axis represents the residuals for a given month—difference between actual and model-predicted registrations, while the x-axis indicates month. The vertical dashed line marks the policy announcement, and the vertical red line denotes the implementation of the bonus-malus system in July 2018. The shaded area represents the anticipation window between the announcement and implementation.

planned vehicle purchases to avoid the higher tax introduced under the malus component, rather than generating new demand, with around four vehicles per 100,000 representing additional cars.[14]

For companies (Panel B), the pre-policy effects show an increase of roughly 30 vehicles per 100,000 inhabitants (or a total of 17,613 anticipatory purchases), followed by a post-policy decline of about 28 vehicles. This pattern similarly points to substantial intertemporal substitution, with firms shifting purchases forward to avoid the higher road taxes. As with individuals, only a small residual – around two vehicles per 100,000 – appears to reflect additional demand rather than timing shifts. The anticipation response operated primarily

reflect the combined responses of individuals and firms.

[14]Table E2 confirms that extending the post-period does not restore symmetry between total anticipatory purchases and the subsequent decline in registrations, supporting the interpretation that a share of pre-policy adoptions reflect genuine excess demand rather than intertemporal substitution. Results also remain robust when using alternative outcome measures, including the logarithm of new car registrations and total new car registrations (Table E3).

through purchases for individuals and through leasing contracts for firms, consistent with differences in financing structures and fleet management practices between private individuals and companies (Table E4).[15] Finally, the anticipation response operates primarily through increased market participation for individuals, while for firms the response is more evenly split between new entrants and existing firms expanding their fleets (Table E5).[16] We also find evidence that anticipatory behavior extended to the second-hand vehicle market, with companies advancing used-vehicle purchases prior to implementation (Table E6).[17]

An important question is whether the observed anticipation effects also altered the composition of vehicles purchased by individuals and firms. Identifying which types of vehicles were brought forward is essential for assessing the environmental implications discussed in Section V.C, as it sheds light on whether pulled-forward purchases displaced the subsequent adoption of cleaner or more fuel-efficient models. To examine this, we estimate pre- and post-policy effects separately for five fuel categories: petrol (Column 2), diesel (Column 3), EVs[18](Column 4), and hybrid electric vehicles (Column 5).

Columns (2) and (3) of Table 2 show that for individuals, the anticipation effect is driven primarily by petrol vehicles, with a smaller effect from diesel. These pre-policy increases are followed by similar reductions in registrations for both fuel types after implementation. In contrast, EV registrations (column 4) decline in the pre-period and rise after implementation, suggesting that the bonus component encouraged both an intertemporal reallocation and net additional adoption of cleaner vehicles in the months following the reform. Because firms rely more heavily on diesel vehicles, they display stronger anticipation effects for diesel than for petrol (in percentage terms). In contrast to individuals, there are no anticipation effects for EVs among company vehicle adoptions.

[15]For individuals, the majority of the pre-effect came from vehicle purchases rather than leases. However, the post-policy effect on purchases is considerably more negative, and the share of new leases increased in the post-period, indicating that the policy also induced a substitution toward leasing among individuals. For companies, by contrast, a larger share of the adjustment occurred through new leasing contracts.

[16]For individuals, the anticipation effect is almost entirely driven by a higher participation rate — measured as the number of unique buyers relative to the population — with only a modest increase in vehicles per buyer. For firms, roughly half of the increase is driven by additional firms entering the market and the remainder by existing firms purchasing multiple vehicles.

[17]Announcing higher future costs for new fossil-fuel vehicles may increase demand for used cars and create incentives to advance second-hand purchases prior to implementation. At the same time, following implementation, prospective new-vehicle buyers may substitute toward used vehicles, generating offsetting post-announcement demand. For companies, the incentive to advance used-vehicle purchases dominates, with an increase of around 5 percent in pre-announcement registrations of second-hand vehicles, while for individuals the opposing forces of pre-reform demand and post-reform substitution toward used vehicles largely offset each other.

[18]We aggregate PHEVs and BEVs into one outcome variable and refer to these as "EVs" throughout the paper.

Table 2: Effect on car registrations by fuel type

	Vehicle fuel type				
	(1) New cars	(2) Petrol	(3) Diesel	(4) EVs	(5) Hybrid
A. Individuals					
Pre	36.71***	31.91***	4.62***	-0.89***	1.01***
	(0.98)	(0.77)	(0.51)	(0.13)	(0.24)
Post	-32.65***	-17.42***	-14.19***	1.99***	-2.98***
	(0.82)	(0.64)	(0.42)	(0.15)	(0.22)
%-Pre effect	21.69	30.78	9.07	-23.93	9.33
%-Post effect	-19.28	-16.81	-27.86	53.58	-27.48
B. Companies					
Pre	29.92***	13.52***	14.30***	.71	1.40
	(5.29)	(2.48)	(2.92)	(.69)	(.87)
Post	-28.11***	-3.28*	-22.84***	1.03	-.98**
	(3.99)	(1.79)	(2.28)	(.91)	(.41)
%-Pre effect	25.33	53.91	20.04	5.64	25.74
%-Post effect	-23.8	-13.07	-32	8.1	-17.97
Controls	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
Observations	257,312	257,312	257,312	257,312	257,312

Notes: This table presents pre- and post-policy regression estimates from equation (1) at the neighborhood-level for all new car registrations (column 1) and across four fuel types: petrol (Column 2), diesel (Column 3), EVs (Column 4), and hybrid electric vehicles (Column 5) for individuals (Panel A) and companies (Panel B). The outcome variables indicate the number of new car registrations for each fuel type per 100,000 residents in the neighborhood. All regressions include demographic characteristics for individuals and firm-level attributes for companies, along with macroeconomic controls, neighborhood fixed effects, and month-of-year fixed effects. All coefficients are reported in neighborhood×month. The percentage effects are reported beneath the coefficients. Robust standard errors are in parentheses. *, **, ***: statistically significant with 90%, 95%, and 99% confidence, respectively.

Beyond the timing and composition of vehicle purchases, the policy also affected the emissions intensity of vehicles acquired, which is an important margin given that the malus was explicitly designed to penalize high-emission vehicles. We now turn to examining how anticipatory behavior shaped the characteristics of vehicles adopted before and after implementation. We separate effects into three periods: the pre-implementation post-announcement period (pre), the post-implementation period (post), and a "steady state" period. We include the steady-state period because it serves as our counterfactual — capturing the emissions intensity of vehicles that would have been adopted in the absence of the policy announcement, once anticipatory dynamics have dissipated.[19]

Table 3 reports the estimated pre-, post-, and steady-state effects of the Feebate policy on CO_2 emissions (Column 1), fuel intensity (Column 2), and particle value (column 3) for individuals (Panel A) and companies (Panel B). During the pre-policy period, individuals registered vehicles with CO_2 emissions approximately 2.5 g/km higher than the baseline (2.0 percent), while companies registered vehicles with emissions 3.9 g/km above baseline (3.2 percent), consistent with bringing forward more polluting models to avoid the higher malus fee. Following implementation, newly registered vehicles were substantially cleaner, with individual registrations falling by 3.7 g/km and company registrations by 4.6 g/km. These patterns are mirrored across the other vehicle characteristics, with both fuel intensity (column 2) and particle values (column 3) rising during the pre-policy period and declining after implementation. The difference in car characteristics is more pronounced among companies, which registered especially high-emission vehicles during the anticipation period before switching toward substantially cleaner models after implementation.

The steady-state estimates reveal that vehicles purchased after anticipatory dynamics had dissipated were not as clean as those registered immediately after implementation for individuals, but were considerably cleaner for companies. For individuals, this likely reflects that once all of the anticipatory purchases were brought forward, the composition of new vehicle registrations reverted back toward its long-run equilibrium before the bonus–malus system.[20] For companies, the dynamic effects of the policy appear more persistent, suggesting that a larger fraction shifted long-term adoption toward rebate-eligible vehicles rather than merely reallocating purchases across time.[21]

[19]To the extent that vehicle emissions were falling over this time period across the set of product offerings due to technological change, this may underestimate the emissions of vehicles that would have been purchased under the policy if avoidance were not possible.

[20]If buyers of more polluting vehicles are more likely to make anticipatory purchases but are constrained in how far in advance they can act, we would expect emissions in the immediate post-policy period to fall by more than in the long run.

[21]This is consistent with Nordic Energy Research (2019), who find that the majority of vehicles receiving subsidies under the feebate policy were company cars, suggesting that firms were better positioned to permanently reallocate their fleets toward cleaner models.

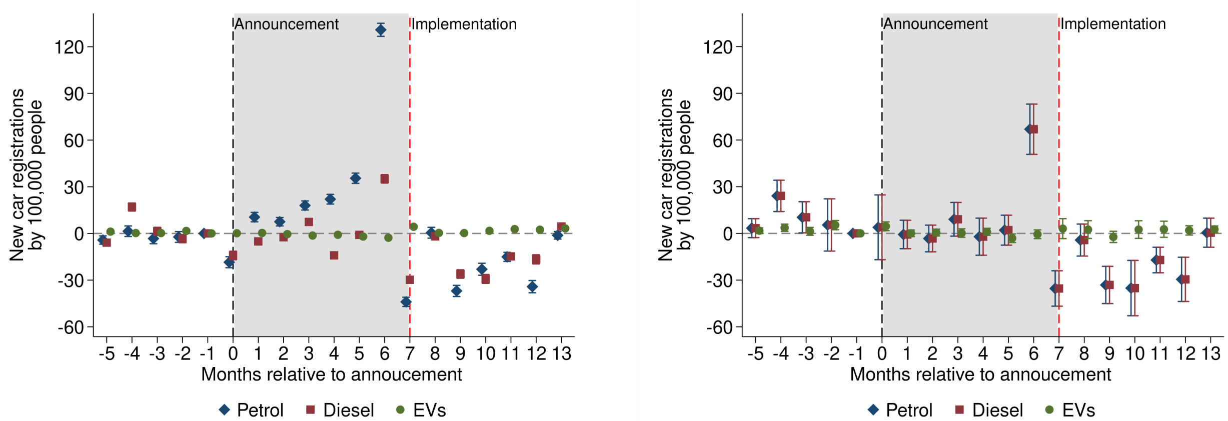
Table 3: Effect on car characteristics

	Car characteristics		
	(1) Carbon emission	(2) Fuel intensity	(3) Particle value
A. Individuals			
Pre	2.50***	0.14***	-0.02***
	(0.20)	(0.01)	(0.01)
Post	-3.70***	-0.07***	-0.24***
	(0.22)	(0.01)	(0.01)
Steady state	-1.05***	0.03***	-0.30***
	(0.25)	(0.01)	(0.01)
Mean characteristic	125.2	5.29	.65
B. Companies			
Pre	3.90***	.20***	.02***
	(.47)	(.01)	(.01)
Post	-4.57***	-.02	-.06***
	(.51)	(.02)	(.01)
Steady state	-3.92***	-.06***	-.10***
	(.59)	(.02)	(.01)
Mean characteristic	123.71	5.31	.47
Controls	Y	Y	Y
Month FE	Y	Y	Y

Notes: This table presents pre- and post-policy regression estimates from equation (1) on three car characteristics: vehicle emissions (Column 1), fuel intensity (Column 2), and particle value (Column 3) for individuals (Panel A) and companies (Panel B). We define the counterfactual period as the six months following post-period (February to July 2019). All regressions include demographic characteristics for individuals and firm-level attributes for companies, along with macroeconomic controls, geographic unit fixed effects, and month-of-year fixed effects. All coefficients are reported in individual×month. The average new car characteristic are reported beneath the coefficients. Robust standard errors are in parentheses. *, **, ***: statistically significant with 90%, 95%, and 99% confidence, respectively.

B. Dynamics

Figure 4 illustrates the dynamic effects of the policy on monthly new car registrations per 100,000 residents for individuals (Panel A) and companies (Panel B), measured relative to the month of policy announcement. The results are disaggregated by fuel type — petrol (blue), diesel (red), and EVs (green). For individuals, registrations increase just after the announcement and peak just before implementation, driven almost entirely by petrol vehicles with a smaller increase from diesel. This pre-policy spike followed by a post-implementation decline reflects strong anticipation effects, as individuals advanced fossil-fuel vehicle purchases to avoid the forthcoming higher road tax. EV registrations decline slightly during the anticipation period and rise after implementation solely for individuals, suggesting that prospective EV buyers delayed their adoption to take advantage of the upcoming bonus. For companies, the anticipatory surge of petrol and diesel vehicles is concentrated immediately before implementation. Following the policy's introduction, company registrations drop and remain below pre-announcement levels for around 6 months. This asymmetric adjustment characterized by a strong pre-policy increase and a sustained post-policy decline suggests that firms advanced vehicle purchases to compensate for long-term vehicle fleet adjustments.



(a) Individuals (b) Companies

Figure 4: Dynamic effects on car registrations by fuel type

Notes: This figure displays the dynamic effects for individuals (Panel A) and companies (Panel B) of the policy on car registrations per 100,000 people, measured monthly relative to the policy announcement. The results are disaggregated by fuel type, with petrol vehicles shown in blue, diesel vehicles in red and EVs in green. The vertical dashed line marks the policy announcement (month 0), and the vertical red line denotes the implementation of the bonus-malus system in July 2018 (month 7). The shaded area represents the anticipation window between the announcement and implementation.

Figure E1 presents the dynamic effects of the policy on vehicle carbon emissions (Panel A) and fuel intensity (Panel B). Both outcomes display similar temporal patterns. We find no significant changes during the pre-announcement period, consistent with the absence of anticipatory behavior before the policy was made public. Following the announcement, both carbon emissions and fuel intensity rise gradually, with the largest increases concentrated in the three months immediately preceding implementation, indicating that individuals and firms advanced purchases of more polluting and less fuel-efficient vehicles to avoid the forthcoming higher malus fee. After implementation, both outcomes decline sharply, reflecting a combination of policy-induced substitution toward cleaner vehicles and a temporary contraction in fossil-fuel vehicle demand as a result of intertemporal substitution — many purchases having already been brought forward into the pre-policy period.

C. Environmental impact

We have established that both individuals and firms increased vehicle adoption in anticipation of the policy, with pre-implementation registrations exceeding the subsequent post-policy decline — implying that a share of anticipatory purchases reflect excess adoption rather than pure intertemporal substitution — and that anticipatory vehicles were, on average, more emissions-intensive than those adopted after implementation. At the same time, early adoptions accelerated the retirement of older, higher-emission vehicles, generating an environmental benefit from faster fleet turnover.

The net environmental impact of anticipatory behavior therefore reflects a trade-off between two opposing forces: the environmental cost from adding additional vehicles to the fleet and substituting cleaner future purchases with dirtier early ones, against the environmental benefit from earlier replacement of older, more polluting vehicles. We now quantify this trade-off by decomposing the net emissions effect into its constituent parts.

Quantification of environmental costs and benefits. To set up the quantification of environmental costs, it is useful to clarify who is adopting vehicles in each period. The population consists of three groups: *anticipators*, who shift forward planned vehicle purchases to avoid the forthcoming higher fee; *excess anticipators*, who are induced by the policy announcement to adopt a vehicle in the pre-period but would not have purchased in the post-period absent the policy; and *non-anticipators*, who do not adjust their adoption timing. In each period, the population of vehicle buyers consists of different mixes of anticipators, excess anticipators, and non-anticipators, making purchases with characteristics that are unaffected (pre-implementation) or affected by the policy (post-implementation) (Figure 5).

Quantifying the environmental costs of anticipation requires two key objects: the emissions of vehicles purchased in anticipation of the policy, $\tilde{e}^{ant}$, and the counterfactual emissions of the vehicles that anticipators would have purchased had they been unable to anticipate the policy, $\tilde{e}^{no\ ant}$. Throughout, tildes denote derived parameters and hats denote estimated policy coefficients. Since neither object is directly observed in the data, we recover them by exploiting the fact that observed emissions in the pre-, post-, and steady-state periods represent weighted averages of emissions across the three groups — with weights determined by their share of car adoptions in each period. We describe this derivation in detail in Appendix F.1. We further assume that the emissions of excess anticipators, $\tilde{e}^{ant,ex}$, are equal to those of anticipators who shift purchases intertemporally, $\tilde{e}^{ant}$.

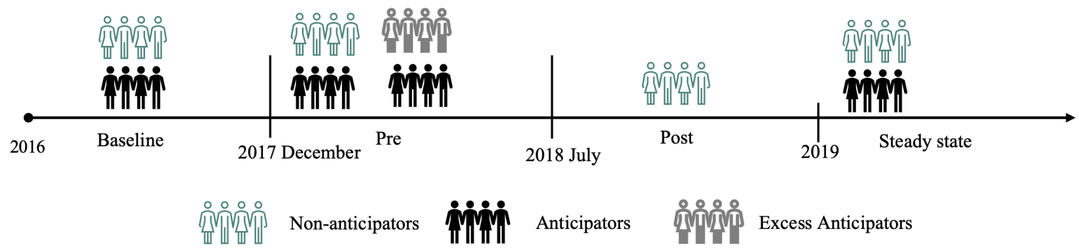


Figure 5: Anticipators, excess anticipators, and non-anticipators across policy phases

Notes: The figure illustrates three population groups—anticipators (black icons), excess anticipators (gray icons), and non-anticipators (green icons). Before December 2017, both anticipators and non-anticipators exhibit baseline adoption. Between the policy announcement and implementation, we observe baseline adoption for both groups plus additional purchases from anticipators who bring forward their demand and from excess anticipators who are induced to purchase a vehicle. In the post-implementation period, only non-anticipators contribute to new adoption, as anticipators have already shifted their purchases earlier. In the steady state, once anticipation effects dissipate, adoption reflects both groups' behavior under the policy.

First, we consider the damages from excess anticipators that increase the stock of vehicles in Sweden. These are calculated by multiplying the excess number of vehicle sales by the average emissions of these additional vehicles and their total vehicle kilometers over their lifetime:

$$\begin{aligned} \Delta E^Q = & \underbrace{\mathrm{Q}_i^{ant,ex} \times \tilde{e}_i^{ant,ex} \times \textstyle\sum_{h=1}^{H} \frac{1}{(1+r)^h} \times VKT_{i,h}^{pre}}_{\Delta \text{ Emissions from additional individual cars}} \\ & + \underbrace{\mathrm{Q}_f^{ant,ex} \times \tilde{e}_f^{ant,ex} \times \textstyle\sum_{h=1}^{H} \frac{1}{(1+r)^h} \times VKT_{f,h}^{pre}}_{\Delta \text{ Emissions from additional company cars}} \end{aligned} \quad (3)$$

where $Q_{i/f}^{ant,ex} = \widehat{\gamma_{i/f}^{Q}} + \widehat{\beta_{i/f}^{Q}}$ indicates the excess count of vehicles during the anticipation period;[22] $\tilde{e}_{i/f}^{ant,ex}$ refer to the emission damages of vehicles from excess adopters (measured in gram of CO_2/km). We multiply this by the vehicle kilometers traveled of these newly adopted cars $VKT_{i/f,h}^{pre}$ for each year h up until the end of the vehicle's lifespan H thereby

[22]This sum yields a net count of excess anticipators per month of anticipation period when the pre- and post-period are of equal duration. Otherwise, we can calculate total excess vehicles by multiplying $\widehat{\gamma^Q}$ by pre-period duration and $\widehat{\beta^Q}$ by post-period duration and taking the difference.

accounting for changes in driving behavior among anticipatory adopters.[23] We discount using a discount rate $r = 2\%$ (Office of Management and Budget 2023).

Next, the vehicle emission intensity changes resulting from the anticipatory vehicles adopted during the pre-policy period can be quantified as the additional emissions from anticipatory vehicles in comparison to the emissions of the counterfactual vehicles that would have been acquired had the policy (already) been implemented. We use the vehicle kilometers traveled of these newly adopted vehicles $VKT^{pre}_{i/f,h}$ during the pre-policy period to construct lifetime emissions. The net environmental cost of the intertemporal substitution anticipation effect is then given as the difference in emissions between the vehicle purchased during the pre-policy period and the corresponding vehicle that would have been purchased in the steady state, for individuals i and companies f:

$$\begin{aligned}\Delta E^A = &\underbrace{Q_i^{ant} \times \sum_{h=1}^{H} \frac{1}{(1+r)^h} \times \left(\tilde{e}_i^{ant} - \tilde{e}_i^{no\,ant}\right) \times VKT^{pre}_{i,h}}_{\Delta \text{ emissions from more polluting individual cars}} \\ &+ \underbrace{Q_f^{ant} \times \sum_{h=1}^{H} \frac{1}{(1+r)^h} \times \left(\tilde{e}_f^{ant} - \tilde{e}_f^{no\,ant}\right) \times VKT^{pre}_{f,h}}_{\Delta \text{ emissions from more polluting company cars}}\end{aligned} \tag{4}$$

where $Q^{ant}_{i/f}$ refers to the number of vehicles sales of anticipators and $\tilde{e}^{ant,ex}_{i/f}$ and $\tilde{e}^{no\ ant}_{i/f}$, respectively, refer to the emission damages of the vehicles adopted by individuals/firms in anticipation of the policy and the counterfactual vehicle that those individuals would buy under the policy in the steady state (i.e., the vehicle that would have been bought under the policy if anticipation was not possible). Emission differences are multiplied by the vehicle kilometers traveled for anticipatory vehicles, $VKT^{pre}_{i/f,h}$ for each year h up until the end of the vehicle's lifespan H. In subsequent discussion, we drop the i/f subscripts, as we take the same approach for individual vehicles and firms in separate regressions.

Note that both ΔE^A and ΔE^Q are expressed as increased emissions per month of pre-period and per 100,000 residents of Sweden. To calculate the total emissions effects, we multiply both by number of months in the pre-period and Swedish population in 100,000s.[24]

[23] In Sweden, vehicles must undergo mandatory inspection approximately three years after initial registration, at which point odometer readings are recorded and reported as annualized vehicle kilometers traveled; inspections are conducted annually thereafter. Since anticipatory purchases occurred primarily in the six months preceding July 2018, the first available VKT observations for these vehicles fall in the 2020–2022 period, which we use to proxy driving behavior over the vehicle's lifetime.

[24] Because the total adult population (aged 16 and above) in Sweden was 8.41 million in 2018, and our estimates are expressed in units of 100,000 individuals, we scale the results by a factor of 84.1.

We contrast this to the environmental benefits from early vehicle replacements. Individuals that made anticipatory purchases replaced the kilometers that they would have driven with an older vehicle with kilometers from the new vehicle over the m months between when they made their anticipatory purchases and when they would have bought a new vehicle otherwise. Since most anticipatory purchases are made in the month before the policy and vehicle purchases return to approximately baseline by January 2019, we assume that these retirements replaced vehicles 7 months early (i.e., $m = 7$) to be reasonably conservative. The benefits from retired vehicles are given by:

$$\Delta E^R = Q_i^{ant} \times \Big(\underbrace{\tilde{e}_i^{pre\ ret}}_{\text{Emissions of retired car}} - \underbrace{\tilde{e}_i^{ant}}_{\text{Emissions of new pre-period}} \Big) \times \frac{VKT_i^{pre}}{12} \times m +$$
$$Q_f^{ant} \times \Big(\underbrace{\tilde{e}_f^{pre\ ret}}_{\text{Emissions of retired car}} - \underbrace{\tilde{e}_f^{ant}}_{\text{Emissions of new pre-period}} \Big) \times \frac{VKT_f^{pre}}{12} \times m \qquad (5)$$

where $\tilde{e}^{pre\ ret}$ denotes the average emission intensity of vehicles retired due to anticipatory purchases, and $\tilde{e}^{ant}$ denotes the average emission intensity of anticipatory vehicles in the pre-policy period among individuals and firms that respond to the policy.

We take a slightly different approach to estimating the benefits of early retirement than to estimating the costs of anticipation. In practice, we do not observe a measurable spike in vehicle retirements around the policy. Table E7 reports estimates from analogous regressions used to detect anticipatory purchases and documents that the bonus-malus policy did not affect vehicle retirements or emission changes of retired cars for either individuals or companies.[25] Figure B6 provides additional evidence that neither vehicle deregistration patterns nor associated emissions display any apparent trend for individuals or firms.[26] Individuals and firms may not have actively deregistered vehicles, even as they stopped using them. Thus, we take a back-of-the-envelope approach to estimate equation (5): we assume that the maximum number of early retirements is the number of anticipatory purchases; for retired vehicle emissions, we use the average emissions of deregistered vehicles during this period; and we use the average vehicle kilometers traveled among anticipators.

The net environmental effect of the anticipatory adoption is given by the sum of the

[25] Statistics Sweden distinguishes between vehicles that are temporarily taken out of use (*avställda*) and those that are permanently deregistered (*avregistreringar*); because our interest lies in vehicles that permanently exit the fleet, we focus on deregistrations.

[26] There are two forces that might affect vehicle de-registration during this period. Anticipatory purchases would be expected to increase de-registrations, as consumers replace vehicles. On the other hand, the Gruenspecht effect might lead to delayed retirement of older vehicles by increasing used car values.

environmental costs, ΔE^Q, and ΔE^A, minus the environmental benefits from earlier retirement, ΔE_R.

Implementation. Table 4 reports the key population statistics (Panel A), all policy coefficients (Panel B), and the derived parameters (Panel C) that we use to recover the environmental costs and benefits. Table 5 reports the resulting estimates of total environmental damages attributable to anticipation effects and decomposes these impacts into environmental costs from excess vehicle adoption, emission intensity, as well as environmental benefits arising from the accelerated retirement of older vehicles. Results are presented separately for individuals and firms. Appendix F.2 provides the full set of formulas to recover the environmental costs and benefits. All environmental cost estimates are expressed in real 2020$, converted using the Consumer Price Index from Statistics Sweden.

Table 4: Parameter estimates used to calculate environmental effect

Coefficient	**Description**	**Period**	**Owner**	
			Individuals	Companies
Panel A: Registry Data				
Q_i^{pre}, Q_f^{pre}	Pre-period vehicle sales	Dec 2017–Jun 2018	196.7	149.2
VKT_i^{pre}, VKT_f^{pre}	Pre-period vehicle usage	2020-2020	13,259	21,333
e_i^{pre}, e_f^{pre}	Pre-period vehicle emissions	Dec 2017–Jun 2018	128.4	127.4
e_i^{post}, e_f^{post}	Post-period vehicle emissions	Jul 2018–Jan 2019	120.7	116.6
$e_i^{R,\ pre}$, $e_f^{R,\ pre}$	Pre-period retired vehicle emissions	Dec 2017–Jun 2018	187.1	185.7
H	Average vehicle lifespan		18	18
r	Annual discount rate		0.02	0.02
Panel B: Policy Coefficients				
$\widehat{\gamma_i^Q}$, $\widehat{\gamma_f^Q}$	Anticipatory vehicle sales effects (Table 2)	Dec 2017–Jun 2018	36.71	29.92
$\widehat{\beta_i^Q}$, $\widehat{\beta_f^Q}$	Post-period vehicle sales effects (Table 2)	Jul 2018–Jan 2019	-32.65	-28.11
$\widehat{\gamma_i^e}$, $\widehat{\gamma_f^e}$	Anticipatory vehicle emission effects (Table 3)	Dec 2017–Jun 2018	2.50	3.90
$\widehat{\beta_i^e}$, $\widehat{\beta_f^e}$	Post-period vehicle emission effects (Table 3)	Jul 2018–Jan 2019	-3.70	-4.57
$\widehat{\rho_i^e}$, $\widehat{\rho_f^e}$	Steady-state vehicle emission effects (Table 3)	Feb 2019–Jul 2019	-1.05	-3.92
$\widehat{\gamma_i^R}$, $\widehat{\gamma_f^R}$	Anticipatory vehicle retirement effects (Table E7)	Dec 2017–Jun 2018	0.26	-2.48
Panel C: Derived Parameters				
$\tilde{e}_i^{ant}$, $\tilde{e}_f^{ant}$	Vehicle emissions of anticipators (equation F.1)	Dec 2017–Jun 2018	139.3	142.95
$\tilde{e}_i^{no\ ant}$, $\tilde{e}_f^{no\ ant}$	Counterfactual vehicle emissions absent anticipation (equation F.2)	Jul 2018–Jan 2019	135.19	121.69

Applying a social cost of carbon approximated by Sweden's carbon dioxide tax of SEK 1,180 per ton (approximately $125) in 2019 (Sterner and Hammar 2021),[27] we estimate gross environmental damages of approximately $30.38 million, comprising $14.36 million from excess vehicle adoption and $16.02 million from the shift toward more emissions-intensive vehicles during the anticipation period. Netting out the estimated upper bound of $1.99 million in environmental benefits from accelerated fleet turnover, the policy announcement generates net environmental costs of approximately $28.39 million.[28]

[27]We convert SEK to US dollars using the exchange rate from January 1, 2020 ($0.1063/SEK).

[28]Our anticipation costs are about 28 percent the size of estimates from Rittenhouse and Zaragoza-

The environmental damages are disproportionately driven by firm anticipatory behavior, of which \$19.05 million is attributable to firms and \$9.34 million to individuals. For individuals, the primary cost stems from excess vehicle adoption (\$8.27 million), reflecting the larger excess adoption among individuals rather than pure intertemporal substitution (Table 2). For firms, the dominant cost comes from the shift toward more emissions-intensive cars during the anticipation period (\$14.06 million), consistent with firms bringing forward high-emission models ahead of implementation relative to their post-policy counterfactual (Table 3).

Table 5: Environmental cost decomposition

		Owner type (\$)	
	Total (\$)	Individual	Firm
Costs			
Excess adoption (ΔE^Q – equation 3)	**14.36**	8.27	6.09
Emission intensity (ΔE^A – equation 4)	**16.02**	1.96	14.06
Benefits			
Accelerated fleet turnover (ΔE^R – equation 5)	**1.99**	0.89	1.10
Net damage	**28.39**	**9.34**	**19.05**

Notes: This table reports the environmental costs and benefits from the anticipatory vehicle purchases separated by each component (column 1). We split the decomposition by owner type: individual (column 2) and firms (column 3). All costs are expressed in real 2020 USD, in millions.

To contextualize the magnitude of the environmental damages generated by anticipatory behavior, we benchmark them against two relevant comparisons. First, we compare them to the fiscal cost of the bonus–malus policy itself. According to the Ministry of Finance (2018), the allocated budget for the bonus–malus system in 2019 amounted to SEK 1.25 billion (\$133 million). Relative to this benchmark, the net environmental damages induced by anticipatory behavior — estimated at \$28.39 million — correspond to approximately 21 percent of the total budget allocated to the policy in 2019.

Second, we benchmark the environmental damages against the estimated lifetime environmental benefits of the reform from vehicles adopted in 2019. Using the Swedish National Audit Office (*Riksrevisionen*) (2020) estimate of 27 tons of CO_2 reduction per rebate-eligible car over its lifetime,[29] and a social cost of carbon in Sweden (\$125), the 44,778 cars that

Watkins (2018) in the trucking sector, though they focus on other pollutants. We estimate a total of 39,000 anticipatory car purchases vs. their 31,000 anticipatory truck purchases. Despite this difference, one might expect anticipation in the truck market to be more costly given both the higher pollution per kilometer and the greater kilometers traveled by these heavy-duty vehicles compared to passenger cars.

[29]This estimate assumes full additionality of subsidized vehicles and abstracts from substitution effects induced by the malus component of the policy. It should therefore be interpreted as an indicative benchmark

received rebates in 2019 generate estimated lifetime environmental benefits of approximately \$151 million. The net environmental damages induced by anticipatory behavior therefore correspond to approximately 19 percent of the total lifetime environmental benefits of the program in its first full year of operation, which suggests that anticipatory behavior impacted the environmental effectiveness of the reform.

D. Mechanisms

In this section, we investigate the mechanisms underlying anticipatory vehicle purchases. We consider five channels: (i) dealer behavior, (ii) financial constraints, (iii) prior fleet characteristics, (iv) information acquisition, and (v) local charging infrastructure.

We begin by examining the role of dealers. An important institutional feature shaping dealer responses to the bonus–malus reform is that the timing of a new vehicle's registration determines the applicable road tax. Dealers who registered vehicles with high emissions in their own name before the implementation date (July 2018) effectively locked in the lower pre-reform tax rate. These vehicles could then be leased or sold to customers without triggering the higher fee after July 2018. Because leasing companies typically retain legal ownership of vehicles, they were particularly well positioned to exploit this timing strategy.[30]

Figure 6 provides direct evidence of dealers' strategic behavior. Holding periods — defined as the number of days between a vehicle's initial registration by a dealer and its transfer to an end user — fall in the months before implementation. This is consistent with dealers rapidly liquidating pre-registered inventory that carried the lower pre-reform tax rate, making these vehicles comparatively attractive to end users. Following implementation, holding periods rise, reflecting the reduced demand for newly registered high-emission vehicles now subject to the higher malus fee, which took considerably longer to transfer to end users. Dealers used pre-registration as an arbitrage strategy, effectively circumventing the policy's intended incentive structure. This represents a loophole in the policy design, as dealers were able to intermediate a share of the anticipatory response by decoupling the timing of vehicle registration from the timing of end-user adoption.

Dealers may also respond through pricing: in anticipation of the stricter post-reform taxation of high-emission vehicles, dealers have incentives to discount or otherwise promote these models prior to implementation, potentially stimulating additional anticipatory pur-

rather than a precise estimate.

[30] Although we find no media or anecdotal evidence of an announcement effect among dealers in response to the July 2018 reform, a similar pattern emerged ahead of the 2025 tax changes targeting E85 (85% ethanol, 15% gasoline) vehicles. Subaru dealers in Sweden reportedly accelerated registrations and built up inventories of ethanol-compatible Outback models in anticipation of the policy change (see https://www.gerane.com/subaru/subaru-outback-surpasses-volvo-swedens-most-registered-car).

chases. However, we are not able to examine pricing strategies that dealers may have used to encourage buyers to shift their purchase patterns, though evidence suggests that this is possible (Busse et al. 2006). Our data contain only list prices (MSRP), so we cannot observe whether dealers offered discounts or negotiated price reductions in response to the policy.[31]

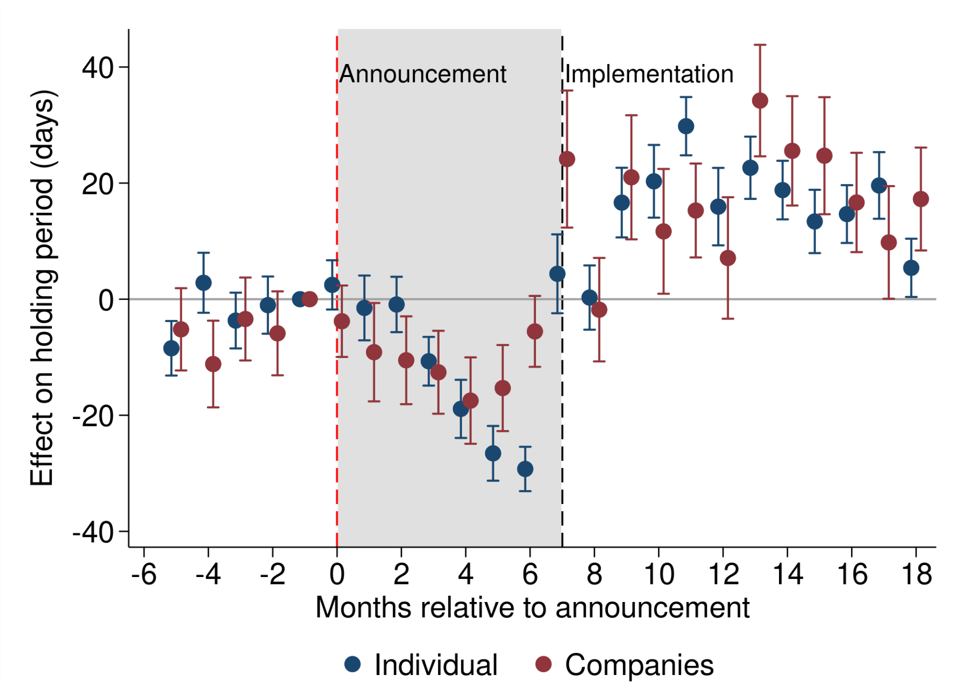


Figure 6: Holding period of dealers

Notes: This figure displays the dynamic effects for individuals (blue) and companies (red) of the policy on the holding period of dealers, measured monthly relative to the policy announcement. The outcome variable measures the number of days between a vehicle's first registration and the date when the dealer subsequently sold or leased it to an individual or company. Negative values indicate a reduction in the dealer's holding period, while positive values reflect an increase. We restrict the holding period to a minimum of one day and a maximum of 365 days. The vertical dashed line marks the policy announcement (month 0), and the vertical red line denotes the implementation of the bonus-malus system in July 2018 (month 7). The shaded area represents the anticipation window between the announcement and implementation.

Second, financial constraints may limit the ability of individuals and companies to shift the timing of vehicle adoption in response to the policy. To explore this channel, we separate individuals into quartiles based on disposable income and companies based on net revenue. Figure 7 shows the anticipatory (blue), post-policy (red), and net effects (gray) of the Feebate reform on new car registrations per 100,000 residents (Panel A) and average vehicle emissions

[31]Figure B7 shows the average list price of new vehicles over time, and there is no obvious trend break around the policy announcement or implementation period, suggesting no major compositional change in the vehicle mix in response to the policy.

(Panel B), disaggregated by income quartile. The point estimates reflect deviations from each group's underlying trend rather than absolute pre-policy levels.

The results reveal a distinct income gradient. High-income individuals (top quartile) register more vehicles with substantially higher average emissions during the anticipation period, consistent with greater financial capacity to advance purchases and avoid the forthcoming higher road tax.[32] In contrast, individuals in the lowest income quartile exhibit smaller anticipatory responses and are less likely to adopt high-emission vehicles ahead of implementation. This pattern suggests that financial flexibility is a key determinant of anticipatory behavior, with liquidity constraints preventing lower-income individuals from advancing vehicle purchases in response to the announcement.

Figure E3 documents analogous patterns for companies, with anticipatory responses increasing by net-revenue and value-added quartiles. High-revenue firms exhibit the largest pre-policy surge, consistent with greater financial capacity to advance fleet purchases and exploit the reform's timing to avoid the higher fee. Notably, the post-policy decline among top-quartile firms is considerably smaller than the pre-policy surge, suggesting that a substantial share of their anticipatory adoptions represent additional rather than intertemporal purchases. Lower-revenue firms, by contrast, show limited anticipatory responses but adjust more strongly after implementation by shifting toward cleaner vehicles, indicating that policy compliance rather than avoidance drives their behavior.

These heterogeneous responses have two implications. First, they speak to the distributional effects of feebate policies: financially stronger households and firms are better positioned to avoid the higher fee, while lower-income individuals are more likely to bear its costs. Second, the income gradient in anticipatory behavior provides empirical inputs for the design of group-specific feebate schemes, where differentiated subsidies or phase-in schedules could mitigate regressive distributional effects, an increasingly relevant consideration in jurisdictions where income-based vehicle incentives have been proposed or implemented.

Beyond income, we document heterogeneous anticipatory responses across a range of demographic characteristics (Figure E4). Among socio-economic dimensions — age, education, gender, and relationship status — a consistent pattern emerges: groups that exhibit stronger anticipatory responses also tend to purchase more emissions-intensive vehicles ahead of implementation. The education gradient is particularly informative: highly educated individuals display the largest anticipatory increases in registrations and select higher-emission models, consistent with greater awareness of and responsiveness to the forthcoming tax

[32]Figure E2 confirms this gradient dynamically: higher-income groups (Q3 and Q4) show a sharp increase in registrations during the anticipation window, while lower-income groups (Q1 and Q2) display a considerably more muted response.

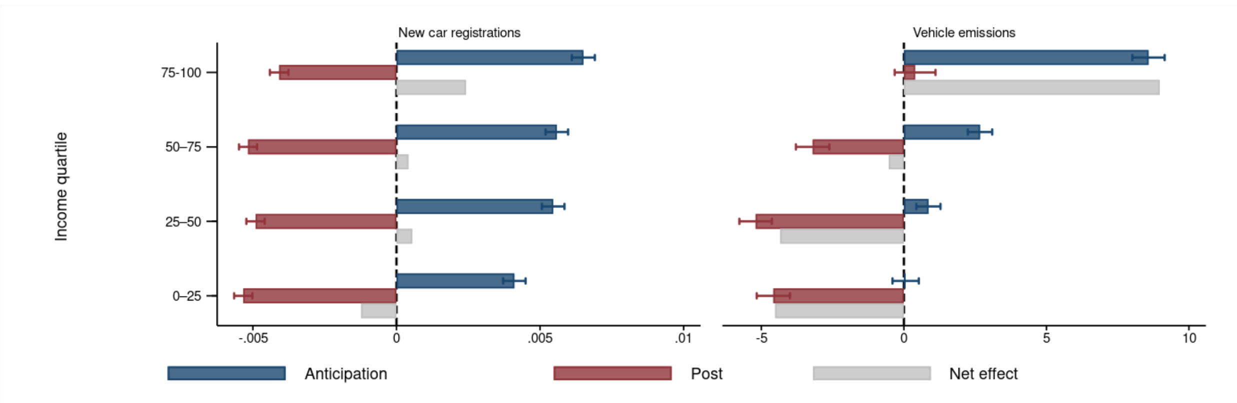


Figure 7: Anticipatory effects by income

Notes: This figure plots the anticipation- (blue), post-(red), and net effect (gray) of the Swedish Feebate system across four income quartiles on new car registrations (Panel A) and vehicle emissions (Panel B) at the individual-level. Income groups are based on 2017 data. The underlying regression specification is illustrated in equation (D.1). 95%-confidence intervals are indicated through whiskers and reflect robust standard errors clustered.

increase. Older individuals, males, and those in couples exhibit similar patterns. Taken together, these patterns indicate that anticipatory behavior was widespread but systematically skewed toward demographic groups with greater capacity and incentive to respond — and that this response resulted in the adoption of higher-emission vehicles.

A third mechanism through which anticipatory behavior operates is the incentive to avoid costs imposed on existing high-emission fleets. Forward-looking individuals with high prior vehicle usage and more emissions-intensive fleets — those facing the largest increase in tax burden under the malus — have the strongest incentive to advance adoption ahead of implementation. Consistent with this, Figure E5 shows that these individuals display the most pronounced anticipatory responses, registering more vehicles and selecting higher-emission models before the policy took effect. In contrast, low-usage individuals and those already owning relatively clean vehicles exhibit less anticipatory behavior, consistent with weaker incentives to adjust adoption timing. The post-policy decline among these groups further suggests adjustment along the participation margin — rather than advancing purchases, these individuals extend the lifespan of their existing vehicles and delay new adoption.

Fourth, whether individuals and firms respond to a policy announcement depends in part on their awareness of the reform, and access to policy-relevant information may amplify anticipatory responses among better-informed individuals or firms. The finding that highly educated individuals exhibit the largest anticipatory responses is consistent with this interpretation. Although we lack micro-level data on information exposure, aggregate Google search activity provides suggestive evidence on public attention. Figure B1 shows that search intensity for *bonus–malus* rises in the months preceding implementation, spikes immediately before the policy takes effect, and peaks shortly afterward. This pattern indicates that awareness of the reform increased substantially during the anticipation window, consistent with many potential buyers actively seeking information about the forthcoming tax change ahead of the implementation date.

Fifth, another potential mechanism underlying the observed anticipation effects may be the availability of public charging infrastructure. Individuals with greater access to workplace chargers face lower costs of switching to EVs, and may therefore be less inclined to advance fossil-fuel vehicle purchases ahead of the policy and more likely to adopt EVs once the bonus takes effect.[33] To test this, we stratify individuals by workplace public charger availability into tertiles during the last quarter of 2017, holding residential charging infrastructure fixed (Table E8). We find limited heterogeneity in anticipatory purchasing behavior across charging tertiles, suggesting that infrastructure availability does not meaningfully shape the magnitude of pre-policy responses. However, post-policy EV adoption increases with workplace charger availability, indicating that charging infrastructure plays an important role in facilitating the transition toward cleaner vehicles following implementation.

VI. Conclusion

This paper examines how individuals and firms circumvent environmental policy when it is announced in advance. Exploiting the pre-announcement of Sweden's 2018 bonus–malus reform, we document pronounced anticipatory responses in the timing and composition of new vehicle purchases. The surge in pre-policy adoptions is not fully offset after implementation, implying that anticipation generated excess vehicles rather than merely shifting the timing of adoption, and anticipatory purchases are systematically more emissions-intensive, amplifying the environmental costs of the policy announcement.

A key mechanism enabling these responses is dealer behavior. Dealers substantially increased inventories and strategically pre-registered high-emission vehicles prior to implementation, locking in the lower pre-policy tax and subsequently selling or leasing these vehicles after implementation without incurring the higher fee. These timing strategies effectively expanded consumers' ability to avoid the policy and represent a potential loophole that future feebate designs should seek to close. Beyond dealer behavior, anticipatory responses are highly heterogeneous: high-income individuals and high-net-revenue firms exhibit the strongest anticipatory shifts, highlighting the central role of financial flexibility in advancing adoption. These patterns reveal an uneven incidence of the policy — financially stronger

[33]The importance of charging availability for EV adoption has been well documented (Li et al. 2017; Springel 2021; Li 2023).

households and firms were able to avoid the fee, while lower-income individuals were more likely to bear its costs.

Taken together, we document intertemporal substitution, a compositional shift toward dirtier vehicles ahead of implementation, excess vehicle adoption, and dealer-facilitated pre-registration — all of which contribute to long-run environmental damages. We quantify these damages using an empirical decomposition that accounts for excess adoption, changes in vehicle composition, and the accelerated retirement of older vehicles, estimating net environmental costs of approximately \$28.39 million, equivalent to around 21 percent of the government's budget for the program in 2019. These costs are disproportionately driven by firm anticipatory behavior: firms shifted toward more emissions-intensive vehicles ahead of implementation, whereas for individuals the primary environmental cost stems from excess vehicle adoption.

These findings underscore that the timing of policy announcements can influence behavioral responses and, if not carefully managed, can undermine both the environmental effectiveness and distributional fairness of climate policy. Many countries and regions are now moving toward bans on the sale of new fossil-fuel vehicles by 2035. Announcing these bans far in advance entails a tradeoff: while advance notice allows manufacturers to invest in product R&D and shift production capacity toward cleaner alternatives, it also allows consumers and dealers to strategically evade the policy. Designing future vehicle regulations that are robust to anticipatory behavior will require careful attention to announcement timing, dealer registration rules, and targeted support for lower-income households to ease compliance.

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Appendix

Anticipation Effects in Vehicle Markets:
Evidence from Sweden's Feebate Policy

Jinjing Chen *Sebastian Tebbe* *Stephanie Weber*

A. Proofs

Recall that $G_t = \pi_{gt} g_t$ and $E_t = \pi_{et} e_t$. For a policy ρ,

$$\frac{\partial \mathcal{W}}{\partial \rho} = \mu \left(\frac{1}{\sum_{f,t} \exp(V_{ft}/\mu)} \right) \left(\frac{1}{\mu} \exp(V_{g0}/\mu) \frac{\partial V_{g0}}{\partial \rho} + \frac{1}{\mu} \exp(V_{g1}/\mu) \frac{\partial V_{g1}}{\partial \rho} + \frac{1}{\mu} \exp(V_{e0}/\mu) \frac{\partial V_{e0}}{\partial \rho} + \frac{1}{\mu} \exp(V_{e1}/\mu) \frac{\partial V_{e1}}{\partial \rho} \right) - \delta_g \left(\frac{\partial G_0}{\partial \rho} + \beta \frac{\partial G_1}{\partial \rho} \right) - \delta_e \left(\frac{\partial E_0}{\partial \rho} + \beta \frac{\partial E_1}{\partial \rho} \right) + \frac{\partial R_0}{\partial \rho} + \beta \frac{\partial R_1}{\partial \rho}$$

which we simplify to

$$\frac{\partial \mathcal{W}}{\partial \rho} = \pi_{g0} \frac{\partial V_{g0}}{\partial \rho} + \pi_{g1} \frac{\partial V_{g1}}{\partial \rho} + \pi_{e0} \frac{\partial V_{e0}}{\partial \rho} + \pi_{e1} \frac{\partial V_{e1}}{\partial \rho} - \delta_g \left(\frac{\partial G_0}{\partial \rho} + \beta \frac{\partial G_1}{\partial \rho} \right) - \delta_e \left(\frac{\partial E_0}{\partial \rho} + \beta \frac{\partial E_1}{\partial \rho} \right) + \frac{\partial R_0}{\partial \rho} + \beta \frac{\partial R_1}{\partial \rho} \tag{A.1}$$

We also have that

$$\frac{\partial G_0}{\partial \rho} = g_0 \frac{\partial \pi_{g0}}{\partial \rho} + \pi_{g0} \frac{\partial g_0}{\partial \rho} \tag{A.2}$$

and similar for G_1, E_0, and E_1.

A.1. Subsidy

Now we consider a subsidy s for electric vehicles in period 1. From the envelope theorem, $\frac{\partial V_{g0}}{\partial s} = \frac{\partial V_{g1}}{\partial s} = \frac{\partial V_{e0}}{\partial s} = 0$ and $\frac{\partial V_{e1}}{\partial s} = \beta$. Substituting these into the first-order condition A.1, with $\rho = s$, and setting the expression equal to zero, we get

$$\pi_{e1} \beta - \delta_g \left(\frac{\partial G_0}{\partial s} + \beta \frac{\partial G_1}{\partial s} \right) - \delta_e \left(\frac{\partial E_0}{\partial s} + \beta \frac{\partial E_1}{\partial s} \right) + \frac{\partial R_0}{\partial s} + \beta \frac{\partial R_1}{\partial s} = 0$$

The expected tax revenue $R_0 = 0$ and $R_1 = -s\pi_{e1}$, so

$$\frac{\partial R_1}{\partial s} = -\pi_{e1} - s \frac{\partial \pi_{e1}}{\partial s}, \;\; \frac{\partial R_0}{\partial s} = 0$$

Substituting this into the FOC, we get

$$-\beta s\frac{\partial \pi_{e1}}{\partial s} - \delta_g\left(\frac{\partial G_0}{\partial s} + \beta\frac{\partial G_1}{\partial s}\right) - \delta_e\left(\frac{\partial E_0}{\partial s} + \beta\frac{\partial E_1}{\partial s}\right) = 0$$

so the optimal subsidy s is given by

$$s = \frac{-\left[\delta_g\left(\frac{\partial G_0}{\partial s} + \beta\frac{\partial G_1}{\partial s}\right) + \delta_e\left(\frac{\partial E_0}{\partial s} + \beta\frac{\partial E_1}{\partial s}\right)\right]}{\beta\frac{\partial \pi_{e1}}{\partial s}}$$

From A.2, we know

$$\frac{\partial G_0}{\partial \rho} = g_0\frac{\partial \pi_{g0}}{\partial s} + \pi_{g0}\frac{\partial g_0}{\partial s} = g_0\frac{\partial \pi_{g0}}{\partial s}$$

because of the lack of income effects, so $\frac{\partial g0}{\partial s} = 0$. We can write similar relationships for G_1, E_0, and E_1. Substituting these into the expression for s,

$$s = \frac{-\left[\delta_g\left(\frac{\partial \pi_{g0}}{\partial s}g_0 + \beta\frac{\partial \pi_{g1}}{\partial s}g_1\right) + \delta_e\left(\frac{\partial \pi_{e0}}{\partial s}e_0 + \beta\frac{\partial \pi_{e1}}{\partial s}e_1\right)\right]}{\beta\frac{\partial \pi_{e1}}{\partial s}} \tag{A.3}$$

Note that if the policy is announced after period 0 purchases have been made, $\frac{\partial \pi_{g0}}{\partial s} = \frac{\partial \pi_{e0}}{\partial s} = 0$. Since by definition, $\pi_{g1} = 1 - (\pi_{g0} + \pi_{e0}) - \pi_{e1}$, we have that $\frac{\partial \pi_{g1}}{\partial s} = -\frac{\partial \pi_{e1}}{\partial s}$. Substituting these into A.3,

$$s = \frac{-\delta_g\beta\frac{\partial \pi_{g1}}{\partial s}g_1 + \delta_e\beta\frac{\partial \pi_{g1}}{\partial s}e_1}{-\beta\frac{\partial \pi_{g1}}{\partial s}} = \delta_g g_1 - \delta_e e_1$$

which is the same as the result from Holland et al. 2016.

A.2. Tax

The results for a tax τ on gasoline vehicles in period 1 are essentially identical.
From the envelope theorem, we have:

$$\frac{\partial V_{g0}}{\partial \tau} = \frac{\partial V_{e0}}{\partial \tau} = \frac{\partial V_{e1}}{\partial \tau} = 0, \quad \frac{\partial V_{g1}}{\partial \tau} = -\beta$$

Again substituting these into the first-order condition A.1 and setting it equal to 0, we get

$$-\pi_{g1}\beta - \delta_g\left(\frac{\partial G_0}{\partial \tau} + \beta\frac{\partial G_1}{\partial \tau}\right) - \delta_e\left(\frac{\partial E_0}{\partial \tau} + \beta\frac{\partial E_1}{\partial \tau}\right) + \frac{\partial R_0}{\partial \tau} + \beta\frac{\partial R_1}{\partial \tau} = 0$$

We also have that expected revenue in period 1 is $R_1 = \tau\pi_{g1}$, so:

$$\frac{\partial R_1}{\partial \tau} = \pi_{g1} + \tau\frac{\partial \pi_{g1}}{\partial \tau}.$$

while again, $R_0 = 0$ and $\frac{\partial R_0}{\partial \tau} = 0$. Thus, combined with the definitions of $\frac{\partial G_0}{\partial \tau}$, $\frac{\partial G_1}{\partial \tau}$, $\frac{\partial E_0}{\partial \tau}$, and $\frac{\partial E_1}{\partial \tau}$, the optimal tax τ is given by

$$\tau^* = \frac{\delta_g\left(\frac{\partial \pi_{g0}}{\partial \tau}g_0 + \beta\frac{\partial \pi_{g1}}{\partial \tau}g_1\right) + \delta_e\left(\frac{\partial \pi_{e0}}{\partial \tau}e_0 + \beta\frac{\partial \pi_{e1}}{\partial \tau}e_1\right)}{\beta\frac{\partial \pi_{g1}}{\partial \tau}}$$

indicating that the elasticities of demand for the different vehicle categories in different time periods are important for determining the optimal tax.

And, again, if the policy is announced after period 0 purchases are made,

$$\tau = \frac{\delta_g\beta\frac{\partial \pi_{g1}}{\partial \tau}g_1 - \delta_e\beta\frac{\partial \pi_{g1}}{\partial \tau}e_1}{\beta\frac{\partial \pi_{g1}}{\partial \tau}} = \delta_g g_1 - \delta_e e_1$$

consistent with single-time period intuition.

1. *Comparative statics.* Let $\frac{\partial \pi_{g1}}{\partial \tau} = \varepsilon_g^1$, where $\varepsilon_g^1 < 0$, because consumers will reduce purchases of gasoline vehicles in period 1 in response to the tax. Let α represent the share of period 1 gas buyers who switch to buying a period 0 gas vehicle, so $1 - \alpha$ is the share who switch to an EV. Let γ represent the share of new EV buyers who switch to period 0 EV purchases and $1 - \gamma$ is therefore the share who switch to period 1 EV purchases. For simplicity, assume that vehicles are driven the same in period 0 and period 1, so we drop the time subscripts from g and e.

We can rewrite the optimal tax as:

$$\begin{aligned}\tau^* &= \frac{\delta_g\left(-\alpha\varepsilon_g^1 g + \beta\varepsilon_g^1 g\right) - \delta_e\left((1-\alpha)\gamma\varepsilon_g^1 e + \beta(1-\alpha)(1-\gamma)\varepsilon_g^1 e\right)}{\beta\varepsilon_g^1} \\ &= \frac{\delta_g g(\beta - \alpha) - \delta_e e(1-\alpha)[\gamma + \beta(1-\gamma)]}{\beta}\end{aligned}$$

As α, the share of gas buyers that intertemporally substitute to non-taxed gas vehicles, increases,

$$\frac{\partial \tau}{\partial \alpha} = -\frac{\delta_g g}{\beta} + \frac{\delta_e e(\gamma + \beta(1-\gamma))}{\beta}$$

which will be negative under the assumption that the total externalities from gas vehicles exceed the total externalities from EVs.

That is, the optimal tax decreases as consumers are increasingly likely to intertemporally shift their gas vehicle purchases rather than responding to the policy as policymakers might prefer and switching to untaxed less polluting vehicles.

B. Additional descriptive statistics

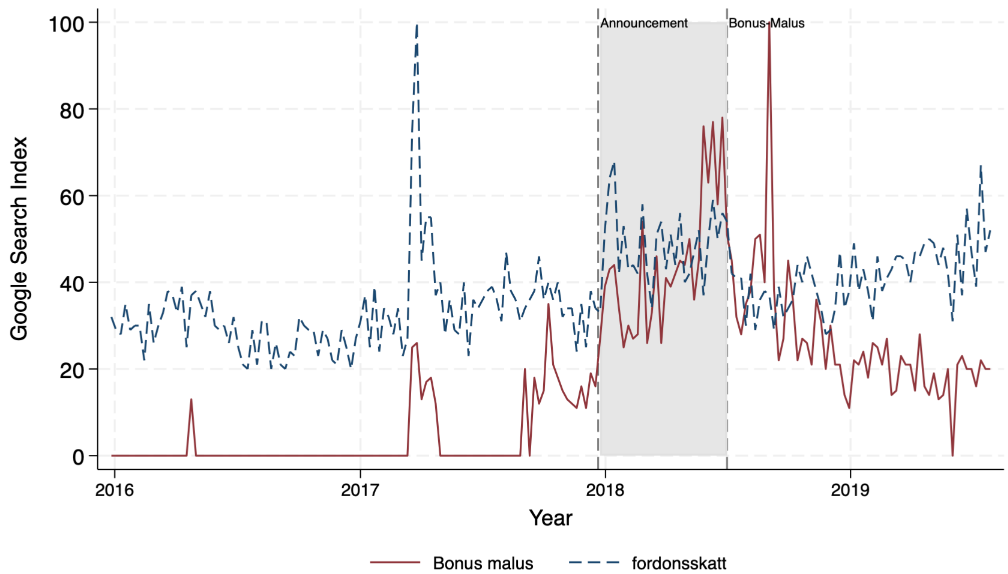
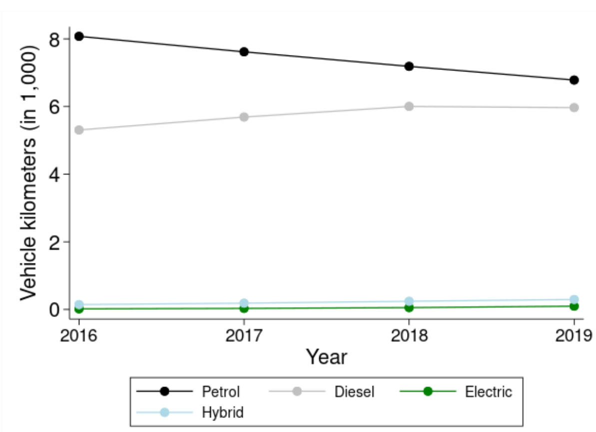
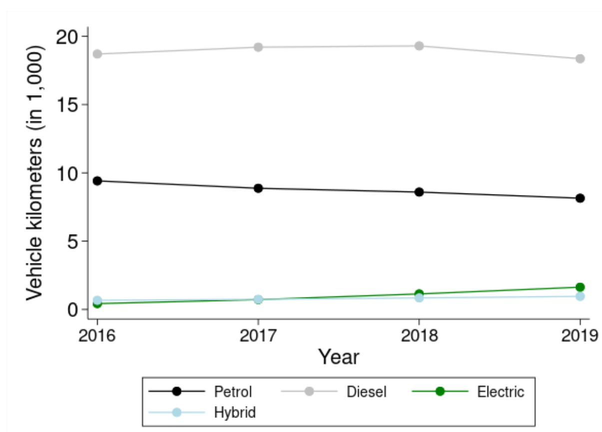


Figure B1: Google trends for bonus malus-related keywords

Notes: This figure plots Google Search Index data for the terms *bonus–malus* (red) and *fordonsskatt* (blue) between 2016 and 2020. The vertical dashed lines mark the policy announcement (December 2017) and implementation (July 2018), and the shaded area denotes the anticipation window between these two events.



(a) Individuals



(b) Companies

Figure B2: Driving behavior by fuel type

Notes: This figure presents the average annual vehicle kilometers traveled for four fuel types: petrol (black), diesel (gray), battery and plug-in hybrid electric (green), and hybrid (blue) for individual owners (Panel A) and companies (Panel B) over the period 2016 until 2019.

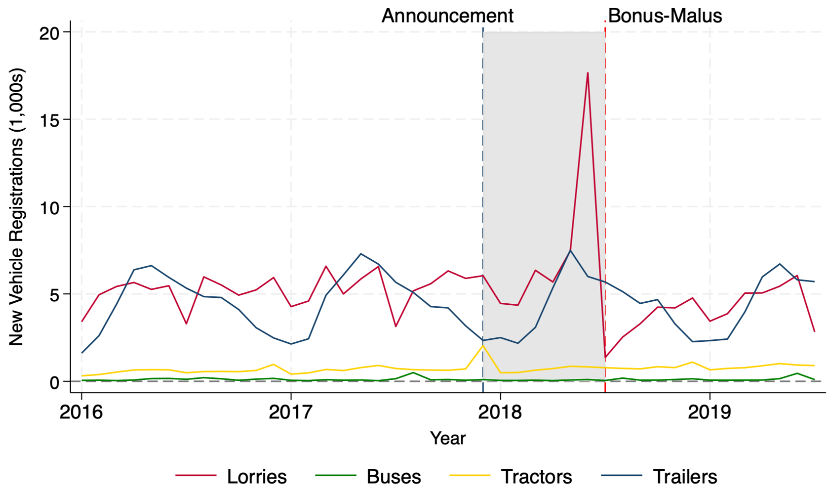


Figure B3: New vehicle registrations by vehicle type

Notes: This figure shows monthly new vehicle registrations (in 1,000s) in Sweden from January 2016 to July 2019, disaggregated by vehicle type. The cranberry, green, gold, and navy lines represent lorries, busses, tractors, and trailers, respectively. The vertical dashed line marks the policy announcement, and the dashed red line denotes the implementation of the bonus–malus system in July 2018.

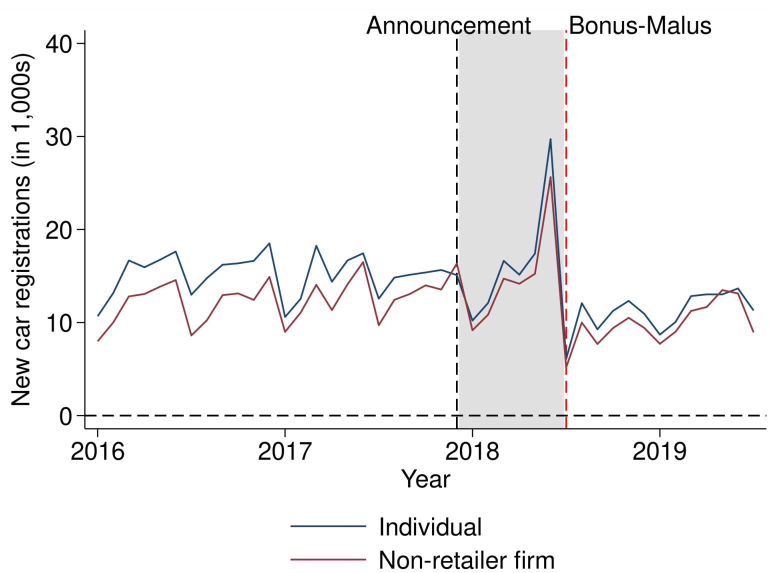


Figure B4: New vehicle registrations by user type

Notes: This figure shows monthly new passenger vehicle registrations (in 1,000s) in Sweden from January 2016 to July 2019, disaggregated by owner type: individuals (blue), and non-retailer firms (brown), respectively. The vertical dashed line marks the policy announcement, and the dashed red line denotes the implementation of the bonus–malus system in July 2018.

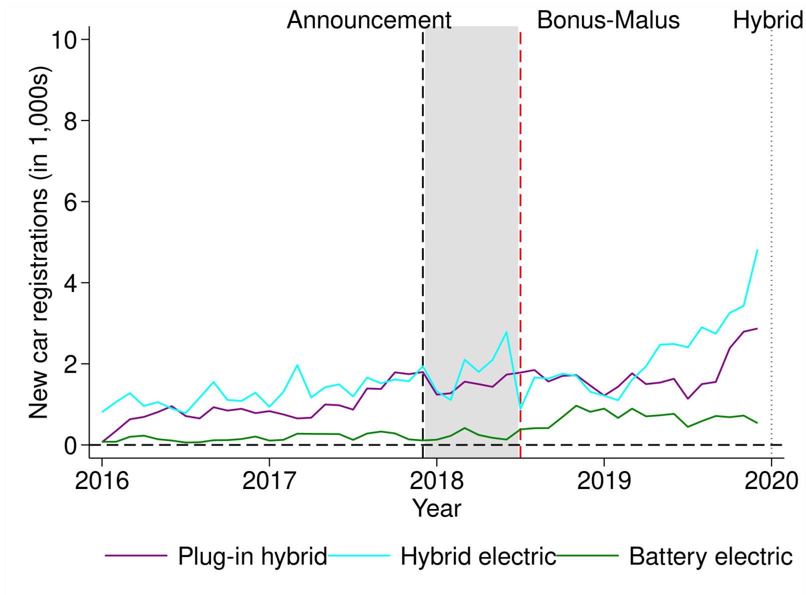


Figure B5: New EV registrations by powertrain

Notes: This figure shows monthly new electric vehicle registrations (in 1,000s) in Sweden from January 2016 to December 2020. The purple, cyan, and green lines represent plug-in hybrid electric, hybrid electric, and battery electric vehicles, respectively. The vertical dashed line marks the policy announcement, the dashed red line denotes the implementation of the bonus–malus system in July 2018, and the dotted line refers to the bonus-changes in January 2020.

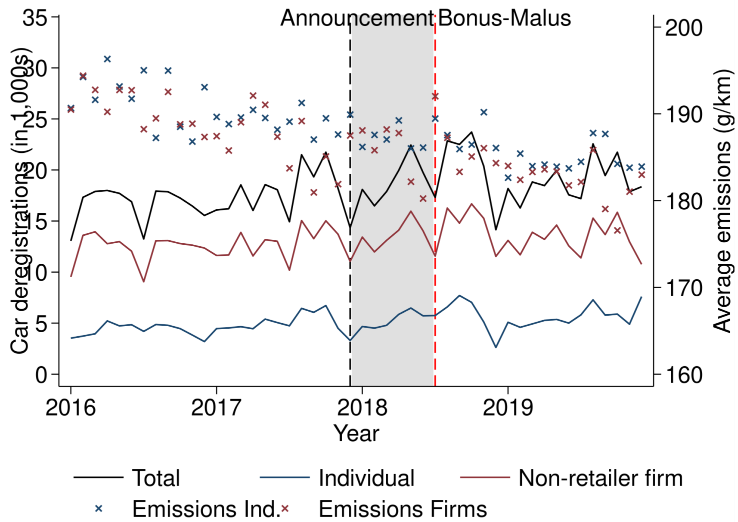


Figure B6: Car de-registrations by owner type

Notes: This figure shows monthly car de-registrations (in 1,000s) in Sweden from January 2016 to July 2019 (left y-axis), disaggregated by owner type together with the average emissions of de-registered vehicles (y-axis). The black, blue, and red, lines represent de-registrations by all owners, individual owners, and non-retailer firms, respectively. The vertical dashed line marks the policy announcement, and the dashed red line denotes the implementation of the bonus–malus system in July 2018.

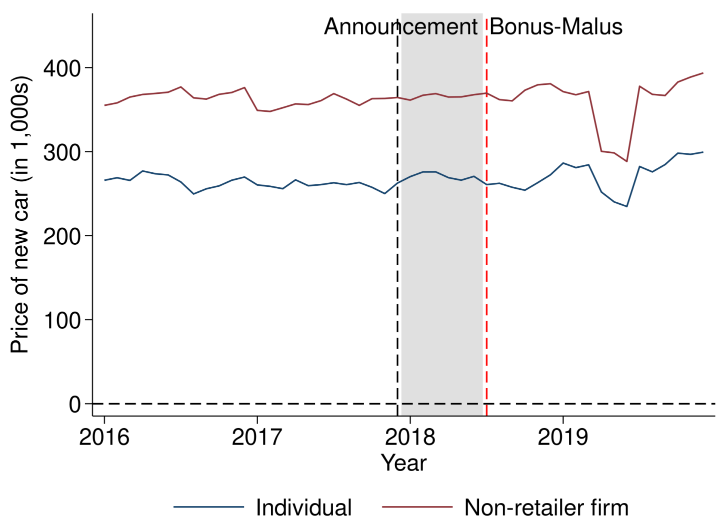


Figure B7: Listing price of new vehicles

Notes: This figure displays the new listing price of cars adopted by individuals (blue), and firms (red). The vertical dashed line marks the policy announcement, and the vertical red line denotes the implementation of the bonus-malus system in July 2018. The shaded area represents the anticipation window between the announcement and implementation.

Table B1: Balance table for individuals

	First Half (2017)		Second Half (2017)		Norm. Diff. (2017)
	Mean	Std. Dev.	Mean	Std. Dev.	
A.Demographic characteristics					
Female	0.35	0.48	0.35	0.48	-0.00
Age	52.44	15.18	52.05	15.15	0.03
Gross Salary (in tho.)	347.70	368.46	352.06	350.92	-0.01
Disposable Income (in tho.)	317.96	653.07	318.39	960.94	-0.00
Unemployment Days	2.74	22.69	2.73	22.19	0.00
Married	0.64	0.48	0.65	0.48	-0.00
At Least 1 Child	0.33	0.47	0.33	0.47	-0.01
Years of Education	12.41	2.52	12.46	2.52	-0.02
B.Car characteristics					
Fuel intensity (liter/100km)	5.23	1.19	5.28	1.13	-0.04
Vehicle emissions (gCO2/km)	125.27	29.83	124.58	29.45	0.02
Vehicle weight (kg)	1963.52	336.01	1971.04	323.29	-0.02
Particle value	0.68	0.66	0.70	0.70	-0.04
Engine Power (kW)	105.15	44.79	104.84	41.62	0.01
N(Observation)	86,524		83,200		

Notes: This table reports the covariate balance of demographic characteristics (Panel A) and vehicle attributes (Panel B) for individuals who adopted a new vehicle in the first half of 2017 (Column 1) and second half of 2017 (Column 2). The Norm. Difference (2017) column captures the normalized difference in means between the first and second half of 2017, where the normalized difference expresses the raw difference in means between groups scaled by the square root of the average within-group variance (Imbens and Rubin 2015; Austin 2009; Baker et al. 2025).

Table B2: Balance table for companies

	First Half (2017)		Second Half (2017)		Norm. Diff. (2017)
	Mean	Std. Dev.	Mean	Std. Dev.	
A.Demographic characteristics					
Value Added (in tho.)	86.72	546.10	90.75	570.36	-0.01
New Revenue (in tho.)	353.66	2826.43	377.69	3004.98	-0.01
Number of Employees	101.61	640.29	107.09	665.81	-0.01
B.Car characteristics					
Fuel intensity (liter/100km)	5.25	1.17	5.31	1.20	-0.05
Vehicle emissions (gCO2/km)	125.34	37.07	122.02	40.67	0.09
Vehicle weight (kg)	2272.71	334.97	2273.66	334.97	-0.00
Particle value	0.48	0.44	0.49	0.45	-0.01
Engine Power (kW)	128.59	41.71	128.14	42.81	0.01
N(Observation)	31,622		30,682		

Notes: This table reports the covariate balance of demographic characteristics (Panel A) and vehicle attributes (Panel B) for companies who adopted a new vehicle in the first half of 2017 (Column 1) and second half of 2017 (Column 2). The Norm. Difference (2017) column captures the normalized difference in means between the first and second half of 2017, where the normalized difference expresses the raw difference in means between groups scaled by the square root of the average within-group variance (Imbens and Rubin 2015; Austin 2009; Baker et al. 2025).

C. Swedish Feebate system

Table C3 summarizes the evolution of the climate bonus policy since the introduction of the first round of the bonus–malus system in July 2018. During the first phase (July 2018 to December 2020), only BEVs with zero emissions qualified for the maximum rebate of 60,000 SEK. The bonus decreased by 833 SEK for each gram of CO_2 emitted per kilometer, up to 60 g/km, with a minimum bonus of 10,000 SEK. In the second phase, from January 2020 to March 2021, the CO_2 threshold for bonus eligibility was raised to 70 g/km, and the reduction per additional gram of CO_2 was fixed at 714 SEK. The minimum bonus for PHEVs also increased from 10,000 to 17,000 SEK, resulting in higher subsidies for PHEVs and hybrid vehicles. However, the simultaneous adoption of the Worldwide Harmonized Light Vehicle Test Procedure (WLTP) in January 2020 reduced bonus eligibility for a broader range of models, as many recorded higher CO_2 emissions under the new test and thereby exceeded the threshold. Some models consequently became ineligible. From 2018 through 2022, eligibility for the climate bonus was determined by the vehicle's registration date. However, for the policy update on 8 November 2022, the relevant cutoff shifted to the order date—low-emission vehicles ordered after this date became ineligible for the bonus, even if they were registered before the policy was officially phased out. Vehicles ordered before 8 November 2022 may still qualify, but their eligibility is governed by the updated bonus rules at the time of registration.[34] As of 31 March 2024, the climate bonus was fully abolished. Consequently, no newly registered low-emission vehicles are eligible for bonus support, regardless of order date.

Table C3: Bonus System by Policy Period

	July 2018–December 2019	January 2020–March 2021	April 2021–June 2022	July 2022–December 2022	January 2023–February 2024
Price Cap	None	None	None	700,000	700,000
BEV Bonus	60,000	60,000	70,000	70,000	50,000
PHEV Bonus Range (SEK)	10,000–60,000	17,000–60,000	10,000–45,000	5,000–20,000	5,000–10,000
Threshold for Bonus (CO_2 g/km)	60	70	60	50	30
PHEV Bonus Formula (SEK)	$60{,}000 - CO_2 \times 833$	$60{,}000 - CO_2 \times 714$	$45{,}000 - CO_2 \times 583$	$20{,}000 - CO_2 \times 300$	$10{,}000 - CO_2 \times 167$
Differences of Bonus (companies vs private)	Bonuses to companies could not exceed 35% of the price difference between the bonus car and the "closest comparable fossil car".			None	None

Notes: This table illustrates the evolution of the bonus policy over time, encompassing four distinct phases of policy changes. In each phase, both the maximum and minimum bonuses have varied, as well as the threshold for CO_2 (g/km) emissions applicable to all eligible low-emission vehicles. Low emission vehicles ordered after 8 November 2022 are no longer eligible for bonus.

[34] Based on bonus information from *Transportstyrelsen*, low emission vehicles ordered after 8 November are no longer eligible for bonus.

Table C4 presents details of the malus component of the policy, including several rounds of adjustments. The malus system implies that new fossil fuel cars registered from July 2018 will have a higher annual road tax (malus) for the first three years and from the fourth year the tax is reduced back to the "normal" level that was valid before the bonus-malus system. Under the CO_2-based system, the annual road tax for petrol vehicles includes a basic charge (360 SEK) along with a CO_2 tax supplement for emissions exceeding a specified threshold. The basic charge is 360 SEK ($38) per year and applies to all vehicles covered by malus. The CO_2 charge for petrol-fueled cars is 82 SEK ($9) per gram of CO_2 if the vehicle emits more than 95 grams and up to 140 grams of CO_2 per kilometer, and 107 SEK ($11) per gram if the vehicle emits more than 140 grams per kilometer. For diesel vehicles, an additional environmental charge (250 SEK) and a fuel charge are also applied. The same structure applies to hybrid electric vehicles. In the case of alternative-fueled vehicles, the tax comprises the annual basic charge along with half of the CO_2 tax supplement that would otherwise apply to petrol vehicles.

Table C4: Malus System by Policy Period

Policy Period	Lower Limit (g/km)	Upper Limit (g/km)	Penalty (Lower)	Penalty (Upper)	Penalty (Diesel)
July 2018 – March 2021	95	140	82	107	250+
April 2021 – May 2022	90	130	107	132	13.52×
July 2022 – Present	75	125	107	132	CO_2

Notes: This table presents the changes in the malus policy over time. The lower limit indicates the threshold at which the CO_2 emission penalty begins, while the upper limit denotes the level beyond which emissions incur a higher penalty. The Penalty (Lower) refers to the marginal tax rate applied to emissions that exceed the lower limit but remain below the upper limit, whereas the Penalty (Upper) applies to emissions that surpass the upper limit. For all diesel vehicles, an additional annual environmental charge of 250 SEK and a fuel charge are applied.

D. Additional regression models

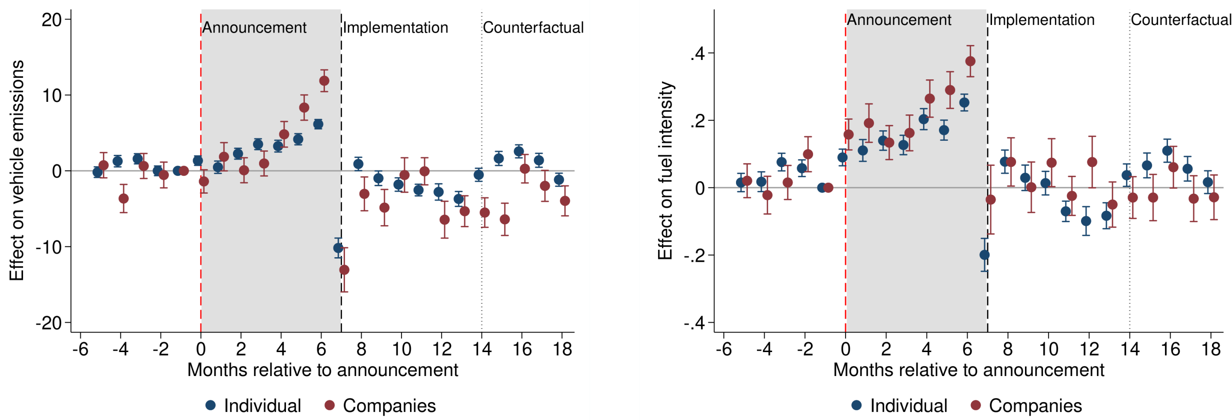
D.1. Heterogeneity models

To examine heterogeneity in responses to Sweden's feebate policy, we assess variations in new car registrations and average vehicle emissions at individual level. The analysis is stratified along several socioeconomic dimensions—including income, age, number of children, educational attainment, and gender—as well as previous vehicle fleet characteristics, such as average vehicle age, emissions, fuel intensity. We use the following specification to conduct this analysis:

$$\begin{aligned} Y_{jt} = \sum_{q=1}^{4} \beta_q \cdot \text{Pre}_t \times \text{Group}_{jq} + \sum_{q=1}^{4} \delta_q \cdot \text{Post}_t \times \text{Group}_{jq} + X_{jt}\gamma + \delta_j + \\ \sum_{q=1}^{4} \sum_{m=1}^{12} \alpha_{mq} Month_m \times \text{Group}_{jq} + \varepsilon_{jt} \quad \text{for } j \in \{i, f\} \end{aligned} \tag{D.1}$$

where Y_{it} refers to either a binary variable equal to 1 if individual i purchases a new car in month t, and 0 otherwise, or to the average emissions of the newly purchased cars by individual i in month t. Group_{iq} denotes a set of categorical group indicators based on income quartiles, gender, education quartiles, number of children quartiles, and quartiles of previous vehicle attributes. In the case of gender, Group_{iq} is a binary variable equal to 1 if individual i is female, and 0 otherwise. X_{it} represents a vector of control variables including individual demographics and the macroeconomic controls included in our main regressions, and $Month_m \times \text{Group}_{iq}$ captures month-group fixed effects, allowing each group has different seasonal pattern.

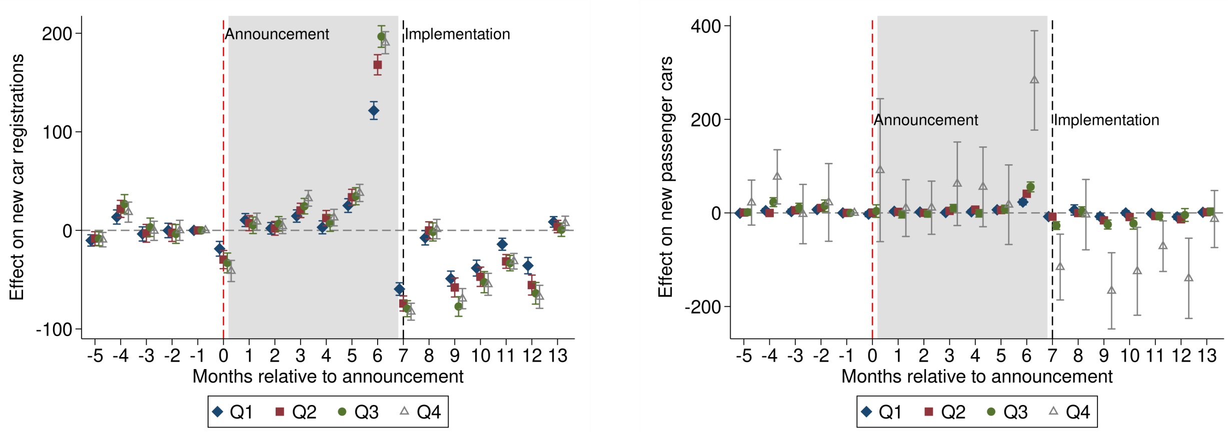
E. Additional results



(a) Vehicle Emissions CO_2 (g/km) (b) Fuel intensity (liters/100 km)

Figure E1: Dynamic treatment effects on emissions and fuel intensity

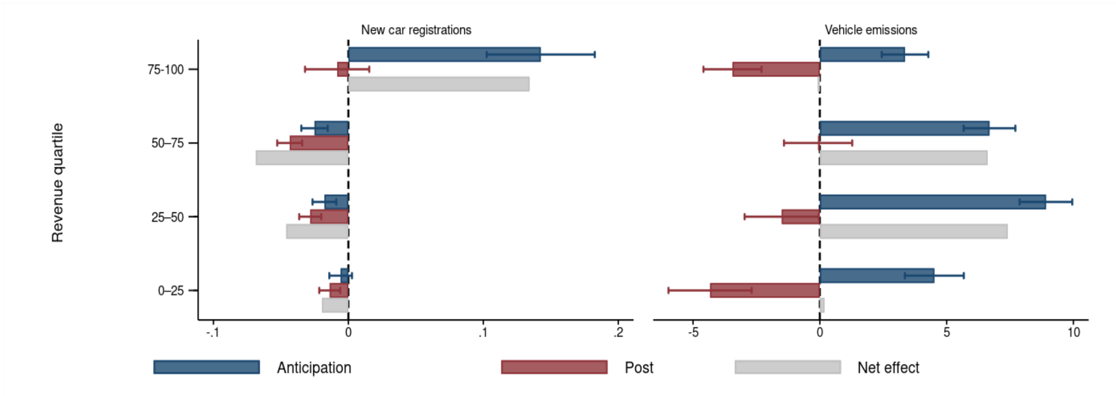
Notes: This figure displays the dynamic effects of the policy average vehicle emissions (Panel A) and average fuel intensity (Panel B) for individuals and companies of newly registered cars. The vertical dashed red lines indicate the timing of the policy announcement (month 0) and implementation (month 7), with the shaded gray area denoting the anticipation window between these two events.



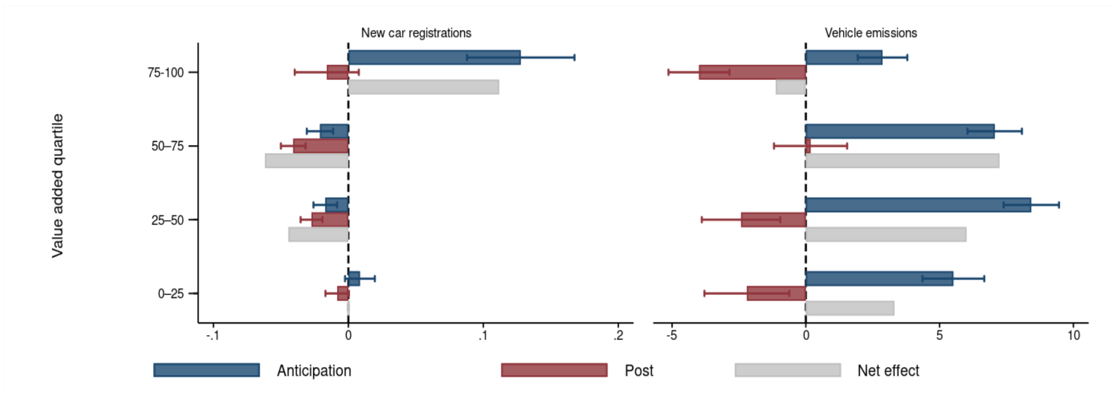
(a) Income by individuals (b) Revenue by companies

Figure E2: Dynamic effects by income and revenue group

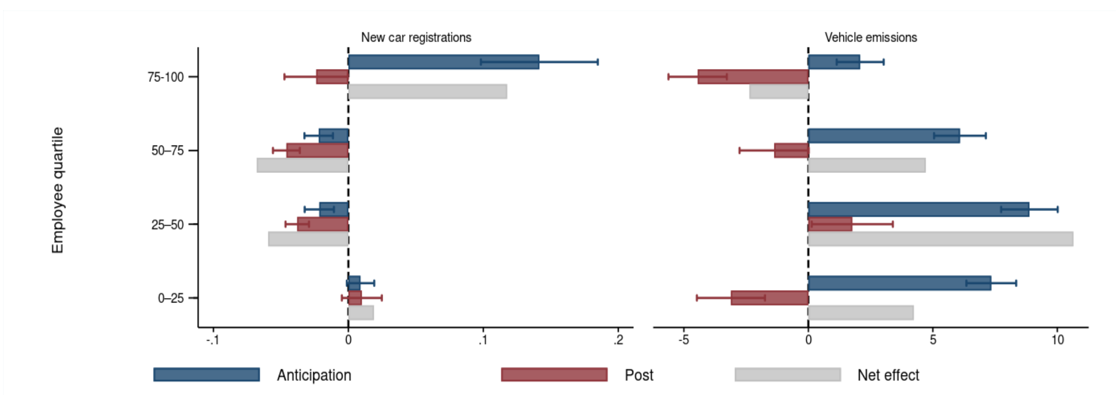
Notes: This figure displays the dynamic effects of the policy on car registrations per 100,000 people disaggregated by four income quartiles for individuals and four net revenue quartiles for companies, measured monthly relative to the policy announcement. Vertical dashed red lines mark the timing of the policy announcement (month 0) and implementation (month 7), while the gray shaded area denotes the anticipation window between these two events.



(a) Net revenue



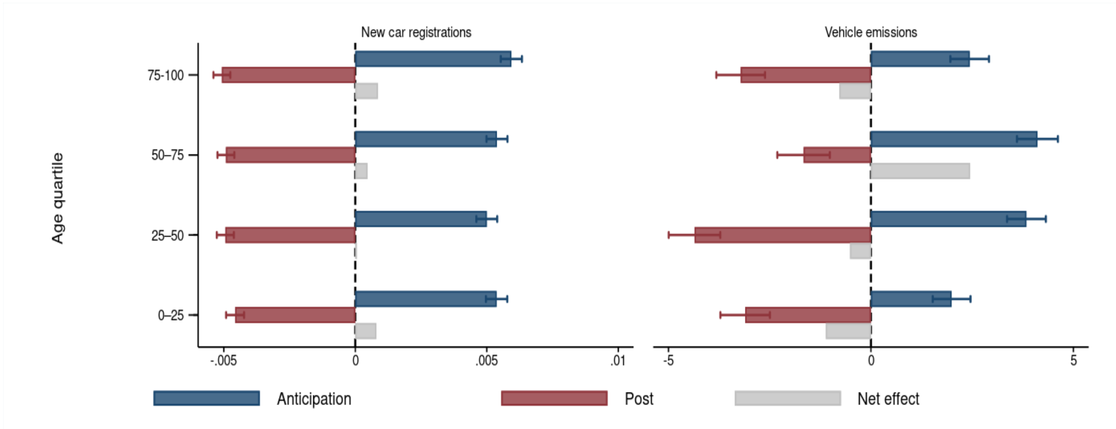
(b) Added value



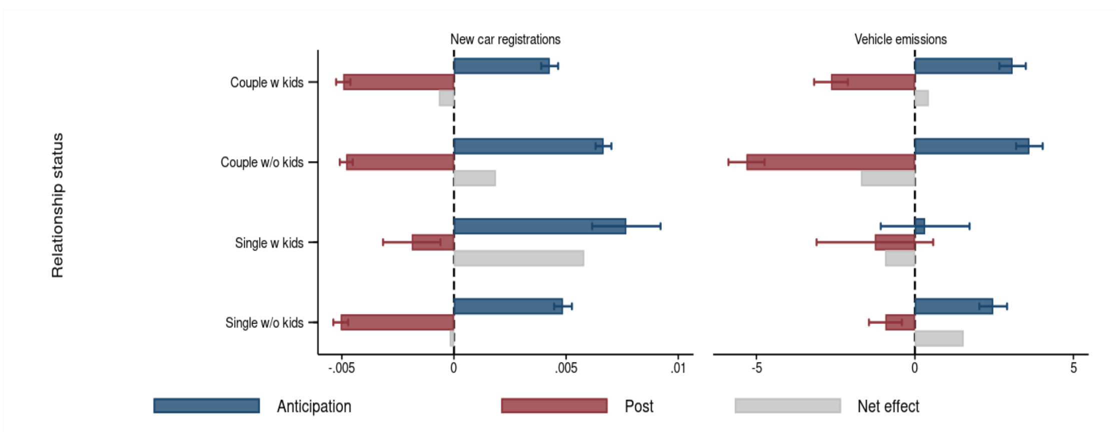
(c) Number of employee

Figure E3: Anticipatory effects by firm characteristics

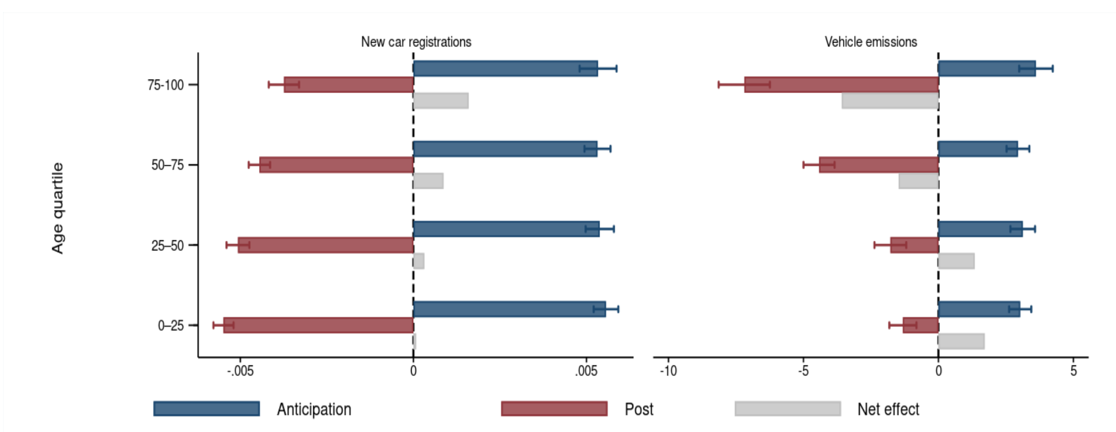
Notes: This figure plots the anticipation (blue), post (red), and net effect (gray) of the Swedish feebate system on new car registrations and vehicle emissions by firm characteristics. Panel (a) reports quartiles based on firms' net revenues in 2017; Panel (b) compares firms' value added; and Panel (c) presents the number of employees. The underlying regression specification is illustrated in equation (D.1). 95% confidence intervals are shown as whiskers and reflect robust standard errors clustered at the municipality level.



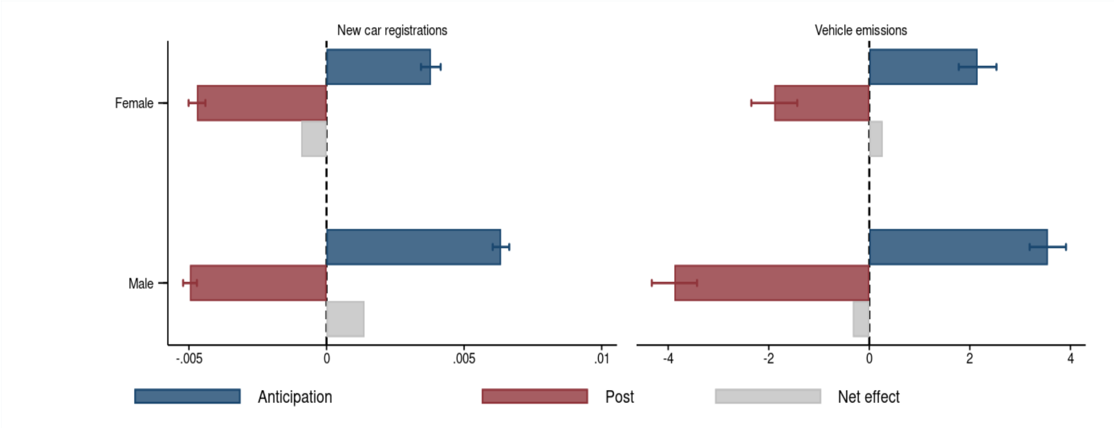
(a)Age



(b) Relationship status and children



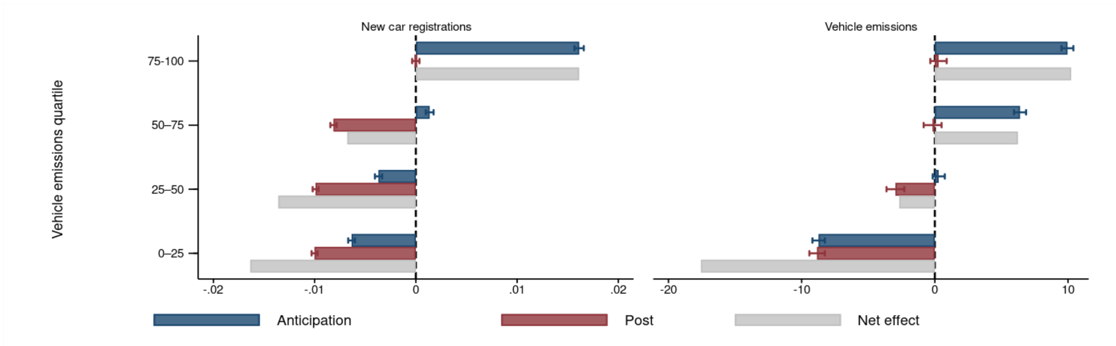
(c) Years of education



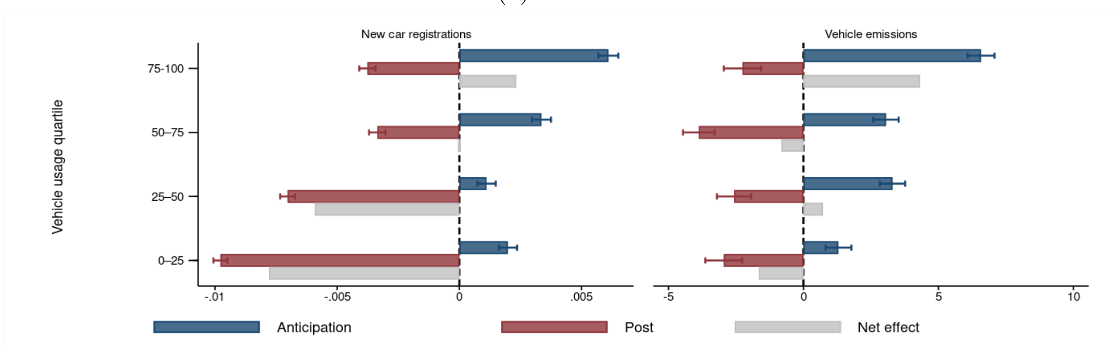
(d) Female

Figure E4: Anticipatory effects by demographics

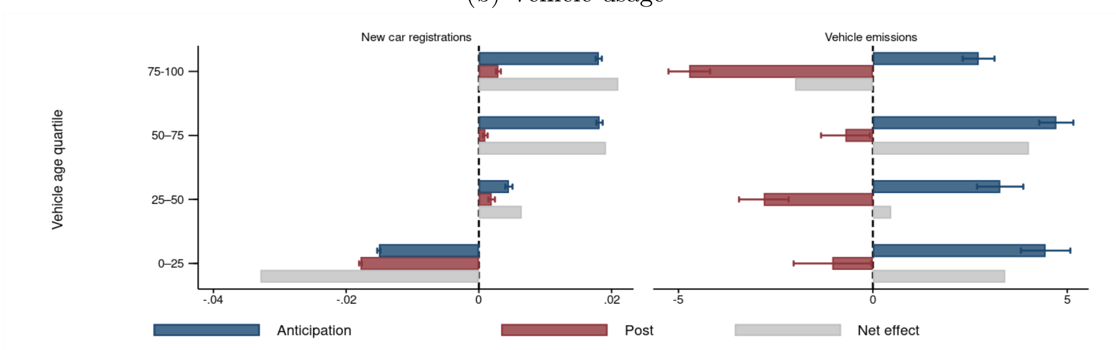
Notes: This figure plots the anticipation (blue), post (red), and net effect (gray) of the Swedish feebate system on new car registrations and vehicle emissions by demographic groups. Panel (a) shows four age quartiles based on 2017 demographics; panel (b) compares singles and couples with and without children; panel (c) compares education levels; and panel (d) compares female and males. The underlying regression specification is illustrated in equation (D.1). 95% confidence intervals are shown as whiskers and reflect robust standard errors clustered at the municipality level.



(a)Emission



(b) Vehicle usage



(c) Vehicle age

Figure E5: Anticipatory effects by previous vehicle fleets

Notes: This figure plots the anticipation (blue), post-policy (red), and net (gray) effects of the Swedish feebate system on new car registrations and vehicle emissions by firm characteristics. Panel (a) reports quartiles based on individuals' previous vehicle emissions in 2017; Panel (b) groups individuals by previous vehicle usage; and Panel (c) presents results by previous vehicle age. The underlying regression specification follows equation (D.1). Whiskers indicate 95% confidence intervals based on robust standard errors clustered at the municipality level.

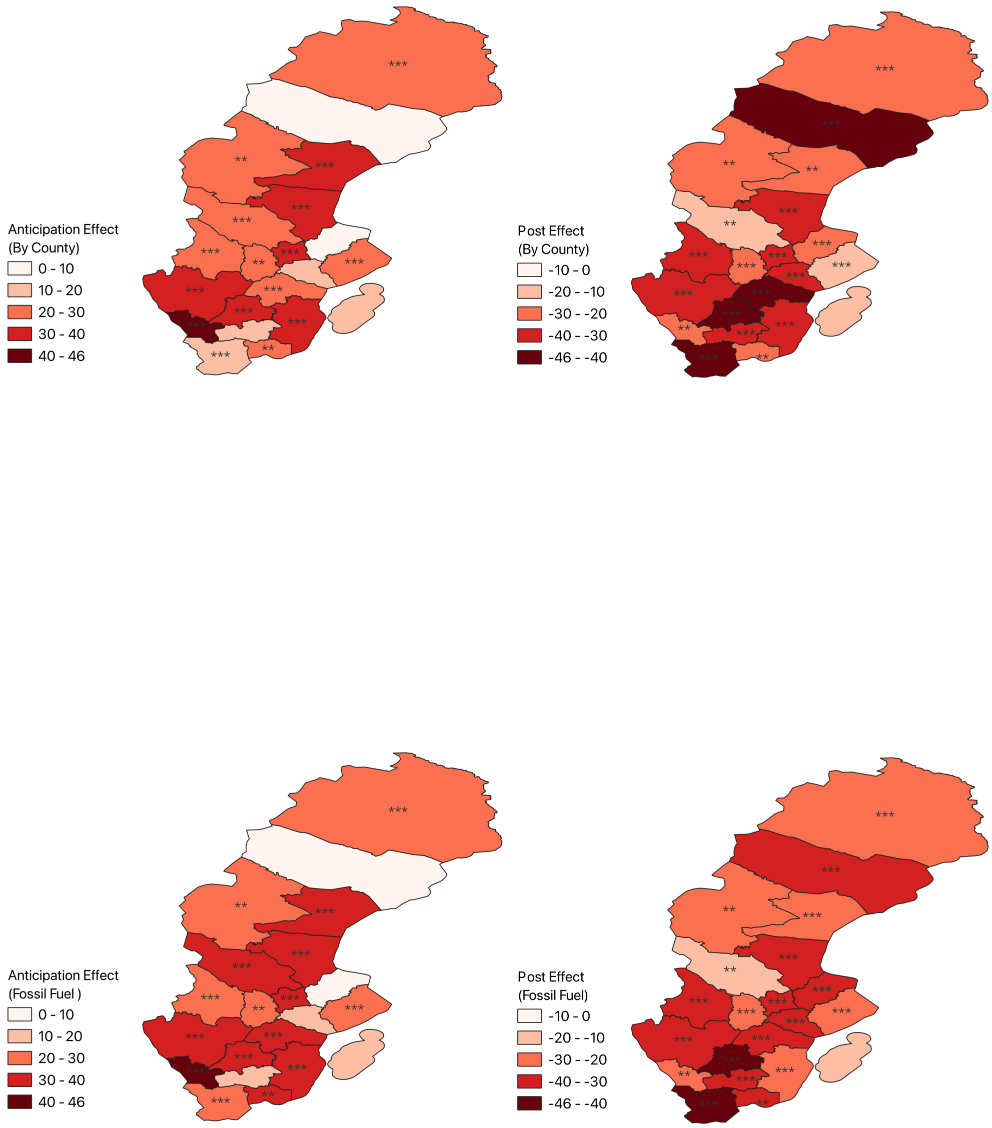


Figure E6: Anticipation and post-policy effects by county

Notes: This figure shows the anticipation (left) and post-policy (right) effects of the Swedish Feebate system on new car registrations per 100,000 residents. The top row presents the effects for all new vehicle registrations, while the bottom row displays the corresponding effects for fossil fuel vehicle registrations only. Darker shades indicate larger effect magnitudes. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table E1: Combined effect on new car registrations

	Vehicle fuel type				
	(1) New cars	(2) Petrol	(3) Diesel	(4) EVs	(5) Hybrid
A. New car registrations					
Pre	67.05***	45.30***	19.49***	-0.25	2.42**
	(10.29)	(5.20)	(4.66)	(1.07)	(1.02)
Post	-59.89***	-20.32***	-36.48***	3.02**	-4.04***
	(6.91)	(3.12)	(3.73)	(1.42)	(0.59)
%-Pre effect	23.33	35.18	15.94	-1.51	14.86
%-Post effect	-20.84	-15.78	-29.83	18.43	-24.84
Controls	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
Observations	257,312	257,312	257,312	257,312	257,312

Notes: This table presents pre- and post-policy regression estimates from equation (1) at the neighborhood-level for all new car registrations combined for all individuals and companies (column 1) and across four fuel types: petrol (Column 2), diesel (Column 3), plug-in hybrid and battery electric (Column 4), and hybrid electric vehicles (Column 5). The outcome variables indicate the number of new car registrations for each fuel type per 100,000 residents in the neighborhood. All regressions include demographic characteristics, firm-level attributes, macroeconomic controls, neighborhood fixed effects, and month-of-year fixed effects. All coefficients are reported in neighborhood×month. The percentage effects are reported beneath the coefficients. Robust standard errors are in parentheses. *, **, ***: statistically significant with 90%, 95%, and 99% confidence, respectively.

Table E2: Estimates for different post-treatment definitions

	Post-coefficient			
	(1) Jan 2019	(2) Feb 2019	(3) Mar 2019	(4) Apr 2019
A. Individuals				
Pre	36.71***	36.73***	34.07***	33.83***
	(0.98)	(1.01)	(1.05)	(1.11)
Post	-32.65***	-27.96***	-29.41***	-26.14***
	(0.82)	(0.81)	(0.83)	(0.87)
B. Companies				
Pre	29.92***	29.51***	27.49***	26.36***
	(5.29)	(5.56)	(5.74)	(6.00)
Post	-28.11***	-24.86***	-25.65***	-24.17***
	(3.99)	(4.08)	(4.33)	(4.60)
Controls	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Observations	257,312	257,312	257,312	257,312

Notes: This table presents neighborhood-level pre- and post-policy regression estimates from equation (1) for individuals (Panel A) and companies (Panel B), using four alternative post-policy treatment windows spanning from July 2018 to (1) January 2019 (baseline), (2) February 2019, (3) March 2019, and (4) April 2019. The outcome variables measures the total number of new car registrations in each geographic area. All regressions include average demographic characteristics for individuals and firm-level attributes for companies, along with macroeconomic controls, DeSO-fixed effects, and month-of-year fixed effects. All coefficients are reported in neighborhood×month. Robust standard errors are in parentheses. *, **, ***: statistically significant with 90%, 95%, and 99% confidence, respectively.

Table E3: Effect on log and total new car registrations

	Outcome definition		
	(1) Registrations per 100k	(2) Log registrations	(3) Total registrations
A. Individuals			
Pre	36.71***	0.15***	0.52***
	(0.98)	(0.00)	(0.01)
Post	-32.65***	-0.15***	-0.45***
	(0.82)	(0.00)	(0.01)
B. Companies			
Pre	29.92***	.10***	.45***
	(5.29)	(.01)	(.08)
Post	-28.11***	-.10***	-.42***
	(3.99)	(.01)	(.06)
Controls	Y	Y	Y
Month FE	Y	Y	Y
Observations	257,312	105,979	257,312

Notes: This table reports the pre- and post-policy regression estimates for different outcome variable definitions from equation (1) for individuals (Panel A) and companies (Panel B). The outcome variables indicate the the number of new car registrations per 100,000 residents (column 1), logarithm of new car registrations (column 2) and the total number of new car registrations (column 3). All regressions include average demographic characteristics for individuals and firm-level attributes for companies, along with macroeconomic controls, neighborhood-fixed effects, and month-of-year fixed effects. All coefficients are reported in neighborhood×month. The percentage effects are reported beneath the coefficients. Robust standard errors are in parentheses. *, **, ***: statistically significant with 90%, 95%, and 99% confidence, respectively.

Table E4: Effect by adoption type

	Registrations by adoption type		
	(1) Purchase	(2) Lease	(3) Share of lease
A. Individuals			
Pre	31.43***	5.28***	-0.02***
	(0.84)	(0.48)	(0.00)
Post	-24.71***	-7.94***	0.02***
	(0.69)	(0.43)	(0.00)
B. Companies			
Pre	8.35***	21.58***	-.00
	(3.17)	(4.06)	(.00)
Post	-7.78***	-20.33***	.00
	(2.52)	(3.10)	(.00)
Controls	Y	Y	Y
Month FE	Y	Y	Y

Notes: This table reports the pre- and post-policy regression estimates by adoption type from equation (1) for individuals (Panel A) and companies (Panel B). The outcome variables indicate the number of new purchases (column 1) and new leases per 100,000 residents (column 2) as well as the share of newly leased vehicles as a function of all new vehicles. All regressions include average demographic characteristics for individuals and firm-level attributes for companies, along with macroeconomic controls, neighborhood fixed effects, and month-of-year fixed effects. All coefficients are reported in neighborhood×month. Robust standard errors are in parentheses. *, **, ***: statistically significant with 90%, 95%, and 99% confidence, respectively.

Table E5: Effect by participation margin

	Outcome		
	(1) Regs. per 100k	(2) Participation Rate	(3) Cars per Buyer
A. Individuals			
Pre	36.71***	35.89***	0.05***
	(0.98)	(0.96)	(0.00)
Post	-32.65***	-31.34***	-0.08***
	(0.82)	(0.80)	(0.00)
%-Pre effect	21.69	22.09	6.34
%-Post effect	-19.28	-19.29	-8.94
B. Companies			
Pre	29.92***	12.91***	.08***
	(5.29)	(.54)	(.02)
Post	-28.11***	-11.20***	-.12***
	(3.99)	(.47)	(.02)
%-Pre effect	25.33	10.93	10.48
%-Post effect	-23.8	-9.49	-17.22
Controls	Y	Y	Y
Month FE	Y	Y	Y
Observations	257,312	257,312	257,343

Notes: This table reports pre- and post-policy regression estimates from equation (1) at the neighborhood level. Column (1) presents effects on total new car registrations per 100,000 residents, column (2) reports effects on the participation rate, measured as the number of unique buyers per 100,000 residents, and column (3) reports effects on the intensive margin, measured as the number of new car registrations per unique buyer. All regressions include demographic characteristics for individuals and firm-level attributes for companies, along with macroeconomic controls, neighborhood fixed effects, and month-of-year fixed effects. All coefficients are reported in neighborhood×month. The percentage effects are reported beneath the coefficients. Robust standard errors are in parentheses. *, **, ***: statistically significant with 90%, 95%, and 99% confidence, respectively.

Table E6: Effect on second-hand car registrations

	Type of used vehicle				
	(1) Any car	(2) Petrol	(3) Diesel	(4) EVs	(5) Hybrid
A. Individuals					
Pre	0.35	0.72	-0.08	-0.14***	-0.41***
	(2.02)	(1.71)	(0.89)	(0.04)	(0.15)
Post	-2.27	-4.37**	0.80	-0.10**	-0.30**
	(2.01)	(1.70)	(0.87)	(0.04)	(0.14)
%-Pre effect	.05	.15	-.05	-41.29	-9.29
%-Post effect	-.34	-.91	.53	-30.7	-6.68
B. Companies					
Pre	3.94**	2.02*	1.66**	-.05	.21
	(1.72)	(1.16)	(.70)	(.12)	(.13)
Post	-.44	-.20	-.03	.12	-.01
	(1.36)	(.84)	(.69)	(.13)	(.10)
%-Pre effect	5.07	5.54	4.74	-5.22	22.3
%-Post effect	-.56	-.56	-.08	12.16	-.56
Controls	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
Observations	257,312	257,312	257,312	257,312	257,312

Notes: This table presents pre- and post-policy regression estimates from equation (1) at the neighborhood-level for all second-hand car registrations (column 1) and across four fuel types: petrol (Column 2), diesel (Column 3), plug-in hybrid and battery electric (Column 4), hybrid electric vehicles (Column 5) for individuals (Panel A) and companies (Panel B). The outcome variables indicate the number of second-hand car registrations for each fuel type per 100,000 residents in the neighborhood. All regressions include demographic characteristics for individuals and firm-level attributes for companies, along with macroeconomic controls, neighborhood fixed effects, and month-of-year fixed effects. All coefficients are reported in neighborhood×month. The percentage effects are reported beneath the coefficients. Robust standard errors are in parentheses. *, **, ***: statistically significant with 90%, 95%, and 99% confidence, respectively.

Table E7: Effect on car de-registrations by fuel type

	Vehicle fuel type				Emissions
	(1) New cars	(2) Petrol	(3) Diesel	(4) EVs	(5) Retired Cars
A. Individuals					
Pre	0.26	0.43	0.16	-0.01	1.08
	(0.75)	(0.67)	(0.23)	(0.02)	(0.79)
Post	0.67	-0.29	0.99***	0.02	0.62
	(1.04)	(0.96)	(0.23)	(0.02)	(0.82)
%-Pre effect	.48	.98	1.74	-10.77	.57
%-Post effect	1.23	-.68	10.73	16.44	.33
B. Companies					
Pre	-2.48	-1.66	-.27	-.36**	.77
	(5.69)	(4.76)	(1.06)	(.17)	(1.24)
Post	3.58	4.47	-.25	-.28*	1.15
	(5.68)	(4.94)	(.92)	(.17)	(1.18)
%-Pre effect	-1.63	-1.32	-1.32	-36.8	.41
%-Post effect	2.36	3.54	-1.23	-28	.62
Controls	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y

Notes: This table presents pre- and post-policy regression estimates from equation (1) at the neighborhood-level for all car de-registrations (column 1) and across four fuel types: petrol (Column 2), diesel (Column 3), plug-in hybrid, battery, and hybrid electric (Column 4), in addition to the average emissions of retired cars (Column 5) for individuals (Panel A) and companies (Panel B). The outcome variables in column (1) - (4) indicate the number of car de-registrations for each fuel type per 100,000 residents in the neighborhood. The outcome variables in column (5) indicate the average emissions of retired cars. All regressions include demographic characteristics for individuals and firm-level attributes for companies, along with macroeconomic controls, neighborhood fixed effects, and month-of-year fixed effects. All coefficients are reported in neighborhood×month. The percentage effects are reported beneath the coefficients. Robust standard errors are in parentheses. *, **, ***: statistically significant with 90%, 95%, and 99% confidence, respectively.

Table E8: Effect on new car registrations public charger infrastructure

	No Charger		Few Charger		Many Charger	
	(1) New Cars	(2) EVs	(3) New Cars	(4) EVs	(5) New Cars	(6) EVs
Pre	34.65***	-0.86***	38.11***	-0.79***	37.15***	-1.00***
	(1.43)	(0.18)	(1.40)	(0.18)	(1.36)	(0.18)
Post	-32.30***	1.35***	-31.83***	2.14***	-33.70***	2.40***
	(1.15)	(0.23)	(1.13)	(0.23)	(1.12)	(0.23)
%-Pre effect	20.11	-30.04	20.77	-24.84	21.56	-34.67
%-Post effect	-18.75	47.06	-17.34	67.5	-19.56	83.08
Controls	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
Observations	257336	257336	257336	257336	257336	257336

Notes: This table reports the pre- and post-policy regression estimates from equation (1), where pre and post are interacted with an indicator for individuals with few, medium, and many workplace charger by tertile. The outcome is either number of all new car registrations and all new electric cars per 100,000 residents in the respective geographic area. Both regressions include average demographic characteristics, the number of public chargers within the neighborhood, macroeconomic controls, neighborhood fixed effects, and month-of-year fixed effects. All coefficients are reported in neighborhood×month. The percentage effects are reported beneath the coefficients. Robust standard errors are in parentheses. *, **, ***: statistically significant with 90%, 95%, and 99% confidence, respectively.

F. Environmental effects

F.1. Deriving unobserved emissions

We derive vehicle emissions for two unobserved sub-groups before and after the feebate policy (Table 4, Panel C):

1. Emissions from vehicles purchased in anticipation of the policy, $\tilde{e}^{ant}$
2. Emissions from vehicles that would have been purchased by avoider if they had been surprised by the policy and could not make anticipatory purchases, $\tilde{e}^{no\ ant}$

First, the observed emissions during the pre-period are the weighted average of the emissions of vehicles that would have been purchased during that period regardless of the policy and the anticipatory (and excess anticipatory) purchases. We can calculate the predicted baseline vehicle sales (Q^{base}) as the observed quantity during the pre-period, Q^{pre}, less the effect of the policy in the anticipation period, $\widehat{\gamma^{Q}}$ from equation (1). Likewise, predicted baseline emissions e^{base} equal the observed emissions during the pre-period e^{pre}, less the change in emissions in the anticipation period $\widehat{\gamma^{e}}$ from equation (1). The change in vehicle sales during the pre-period is $Q^{ant\ tot} = \widehat{\gamma^{Q}}$, which is a combination of anticipators Q^{ant} and excess anticipators $Q^{ant,ex}$. We can thus calculate e^{pre} (observed in the data) as a weighted average of e^{base} (estimated, as above), e^{ant} and $e^{ant,ex}$:

$$e^{pre} = \frac{e^{ant} \times Q^{ant} + e^{ant,ex} \times Q^{ant,ex} + Q^{base} \times e^{base}}{Q^{ant} + Q^{ant,ex} + Q^{base}}$$

Assuming that the average emissions of intertemporal anticipators and excess anticipators are the same, this can be rearranged to yield an expression for $\tilde{e}^{ant}$:

$$e^{\tilde{ant}} = \frac{e^{pre} \times Q^{pre} - Q^{base} \times e^{base}}{Q^{ant\,tot}} = \frac{e^{pre} \times Q^{pre} - (Q^{pre} - \widehat{\gamma_Q}) \times (e^{pre} - \widehat{\gamma}_e)}{\widehat{\gamma_Q}} \qquad \text{(F.1)}$$

Second, to estimate the counterfactual emissions of the anticipatory purchasers had they not adjusted their purchase timing in response to the policy (i.e., had they been surprised by it), we assume that these emissions can also be recovered using a weighted average. Specifically, we assume that anticipatory purchases can only be made for up to 7 months, and thus observed steady state emissions represent an average of non-anticipators' post-policy emissions (e^{post}) and anticipator' post-policy emissions when they cannot make anticipatory purchases $\tilde{e}^{no\ ant}$:

$$e^{SS} = \frac{e^{no\ ant} \times Q^{no\ ant} + e^{post} \times Q^{post}}{Q^{SS}}$$

Solving for $e^{no\ ant}$, and defining everything relative to the pre-period emissions and quantities, we apply the identities $Q^{post} = (Q^{pre} - \widehat{\gamma}^Q) + \widehat{\beta_Q}$, $Q^{no\ ant} = -\widehat{\beta_Q}$, $Q^{SS} = Q^{post} + Q^{no\ ant}$, $e^{SS} = (e^{pre} - \widehat{\gamma}^e) + \widehat{\rho}^e$, and $e^{post} = (e^{pre} - \widehat{\gamma}^e) + \widehat{\beta}^e$ to obtain:

$$\tilde{e}^{no\ ant} = \frac{[(e^{pre} - \widehat{\gamma}^e) + \widehat{\rho}^e] \times (Q^{pre} - \widehat{\gamma}^Q) - [(e^{pre} - \widehat{\gamma}^e) + \widehat{\beta}^e] \times [(Q^{pre} - \widehat{\gamma}^Q) + \widehat{\beta_Q}]}{-\widehat{\beta_Q}} \tag{F.2}$$

F.2. Implementation of environmental estimates

This section outlines how we implement our unobserved emissions using parameters from Table 4 and recover the environmental cost and benefit estimates for the decomposition in Table 5.

Recovering unobserved emissions. Following equations (F.1) and (F.2), we use the parameter estimates reported in Table 4 to recover the implied emissions of anticipatory and counterfactual (no-anticipation) vehicles for both individuals and firms.

$$\begin{aligned}
\tilde{e}_i^{ant} &= \frac{e_i^{pre} \times Q_i^{pre} - (Q_i^{pre} - \widehat{\gamma_i^Q}) \times (e_i^{pre} - \widehat{\gamma_i^e})}{\widehat{\gamma_i^Q}} \\
&= \frac{128.4\ \frac{gCO_2}{km} \times 196.7 - (196.7 - 36.71) \times (128.4 - 2.50)\ \frac{gCO_2}{km}}{36.71} = 139.3\ \frac{gCO_2}{km} \\
\tilde{e}_f^{ant} &= \frac{e_f^{pre} \times Q_f^{pre} - (Q_f^{pre} - \widehat{\gamma_f^Q}) \times (e_i^{pre} - \widehat{\gamma_f^e})}{\widehat{\gamma_f^Q}} \\
&= \frac{127.4\ \frac{gCO_2}{km} \times 149.2 - (149.2 - 29.92) \times (127.4 - 3.90)\ \frac{gCO_2}{km}}{29.92} = 142.95\ \frac{gCO_2}{km}
\end{aligned}$$

$$\begin{aligned}
\tilde{e}_i^{no\ ant} &= \frac{[(e_i^{pre} - \widehat{\gamma_i^e}) + \widehat{\rho_i^e}] \times (Q_i^{pre} - \widehat{\gamma_i^Q}) - [(e_i^{pre} - \widehat{\gamma_i^e}) + \widehat{\beta_i^e}] \times [(Q_i^{pre} - \widehat{\gamma_i}^Q) + \widehat{\beta_i^Q}]}{-\widehat{\beta_i^Q}} \\
&= \frac{[(128.4 - 2.5) - 1.05] \times (196.7 - 36.71) - [(128.4 - 2.5) - 3.70)] \times [(196.7 - 36.71) - 32.65]}{32.65} \\
&= 135.19\ \frac{gCO_2}{km} \\
\tilde{e}_f^{no\ ant} &= \frac{[(e_f^{pre} - \widehat{\gamma_f^e}) + \widehat{\rho_f^e}] \times (Q_f^{pre} - \widehat{\gamma_f^Q}) - [(e_f^{pre} - \widehat{\gamma_f^e}) + \widehat{\beta_f^e}] \times [(Q_f^{pre} - \widehat{\gamma_f^Q}) + \widehat{\beta_f^Q}]}{-\widehat{\beta_f^Q}} \\
&= \frac{[(127.4 - 3.90) - 3.92] \times (149.2 - 29.92) - [(127.4 - 3.90) - 4.57] \times [(149.2 - 29.92) - 28.11]}{28.11} \\
&= 121.69\ \frac{gCO_2}{km}
\end{aligned}$$

Excess adoption. We calculate the total emissions generated by the additional vehicles acquired by individuals (i) and companies (f) as follows:

$$\begin{aligned}
\Delta E^Q &= 84.1 \times 7 \times Q^{ant,ex} \times \sum_{h=1}^{H} \frac{1}{(1+r)^h} e^{ant,ex} \times VKT_h^{pre} \\
\Delta E_i^Q &= 84.1 \times 7 \times (36.71 - 32.65) \times \sum_{h=1}^{18} \frac{1}{(1+0.02)^h} \\
&\quad \times 139.3\ \frac{gCO_2}{km} \times 13{,}259\ km \times \frac{1 \text{ ton}}{10^6 \text{ g}} \approx 66{,}182 \text{ tCO}_2, \\
\Delta E_f^Q &= 84.1 \times 7 \times (29.92 - 28.11) \times \sum_{h=1}^{18} \frac{1}{(1+0.02)^h} \\
&\quad \times 142.95\ \frac{gCO_2}{km} \times 21{,}333\ km \times \frac{1 \text{ ton}}{10^6 \text{ g}} \approx 48{,}716 \text{ tCO}_2, \\
\Delta E^Q &= \Delta E_i^Q + \Delta E_f^Q = 114{,}898 \text{ tCO}_2
\end{aligned}$$

Emissions intensity. To quantify emissions from vehicles purchased by anticipators between the policy announcement and implementation, we focus on the fact that anticipatory vehicles are more emissions-intensive than the vehicles that would have been purchased in the absence of anticipation. We then scale emissions using the average vehicle kilometers traveled of vehicles adopted during the pre-policy period to proxy for anticipators' driving behavior. Using the parameters from Table 4, we calculate emissions for individuals and firms as follows:

$$\Delta E^A = 84.1 \times 7 \times Q^{ant} \times \sum_{h=1}^{H} \frac{1}{(1+r)^h} \left(\widehat{e^{ant}} - \widehat{e^{no\,ant}}\right) \times \widehat{VKT_h^{pre}}$$

$$\Delta E_i^A = 84.1 \times 7 \times 32.65 \times \sum_{h=1}^{18} \frac{1}{(1+r)^h} \times (139.3 - 135.19)\ \frac{gCO_2}{km}$$

$$\times\ 13{,}259\, km \times \frac{1 \text{ ton}}{10^6 \text{ g}} \approx 15{,}703\, \text{tCO}_2$$

$$\Delta E_f^A = 84.1 \times 7 \times 28.11 \times \sum_{h=1}^{18} \frac{1}{(1+r)^h} \times (142.95 - 121.69)\ \frac{gCO_2}{km}$$

$$\times\ 21,333\ km\ \times \frac{1 \text{ ton}}{10^6 \text{ g}} \approx 112,520\ \text{tCO}_2,$$

$$\Delta E^A = \Delta E_i^A + \Delta E_f^A = 128{,}223\ \text{tCO}_2.$$

Benefits from early retirement. To calculate the environmental benefits of the policy, we compute the difference between the average emissions intensity of vehicles retired in the pre-policy period due to anticipatory purchases and the emissions intensity of anticipatory vehicles newly registered in the pre-policy period, and substitute this difference into the equation (5).

$$\Delta E_i^R = 84.1 \times 7 \times 32.65 \times (187.1\ \frac{gCO_2}{km} - 139.3\ \frac{gCO_2}{km})$$

$$\times \frac{13,259\ km}{12} \times 7 \times \frac{1 \text{ ton}}{10^6 \text{ g}} \approx 7,106\ \text{tCO}_2,$$

$$\Delta E_f^R = 84.1 \times 7 \times 28.11 \times (185.7\ \frac{gCO_2}{km} - 142.95\ \frac{gCO_2}{km})$$

$$\times \frac{21,333\ km}{12} \times 7 \times \frac{1 \text{ ton}}{10^6 \text{ g}} \approx 8,804\ \text{tCO}_2,$$

$$\Delta E^R = \Delta E_i^R + \Delta E_f^R = 15{,}910\ \text{tCO}_2.$$

To express emissions in monetary terms, we multiply vehicle lifetime emissions by the Swedish carbon tax of \$125 per ton of CO_2, which we use as a proxy for the social cost of carbon.

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