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

Jinjing Chen Sebastian Tebbe Stephanie Weber†

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Abstract

We study anticipatory behavior in response to Sweden's vehicle feebate policy. Using administrative data on vehicle registrations linked to individual and firm characteristics, we document pronounced anticipation effects that are largely driven by intertemporal substitution but also lead to excess vehicle adoption. Vehicles adopted ahead of implementation are systematically dirtier and subsequently driven more intensively, amplifying the environmental damages of the policy announcement. Anticipatory adoption is most pronounced where anticipation is easiest or policy compliance is most challenging: in particular, dealer stock-management strategies, household and firm liquidity, and information salience all increase anticipatory purchases, while access to charging infrastructure shifts post-policy vehicle purchases. We quantify the environmental implications of anticipatory behavior, accounting for excess adoption, the higher emissions intensity of vehicles purchased in anticipation, and their more intensive subsequent usage. Ultimately, the environmental costs of the anticipatory behavior amount to approximately $62 million, or around half of the budget allocated to the policy in 2019.

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†Chen: University of Melbourne (jinjing.chen1@student.unimelb.edu.au); Tebbe: University of Melbourne (e-mail: sebastian.tebbe@unimelb.edu.au); Weber: University of Colorado Boulder (e-mail: stephanie.m.weber@colorado.edu). The authors did not receive external funding for this research, and we have no financial relationships or other potential conflicts of interest to disclose.

I. Introduction

Climate policies are often announced far in advance of their implementation. This can be helpful in cases where it 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, consumers may be able to shift forward car purchases in order to avoid future bans, changes in technology, or increased costs. Quantifying the extent of this behavior is important to understand the limitations of policies designed to reduce vehicle emissions.[1]

Many countries and regions have proposed bans on the sale of new fossil-fuel vehicles by 2035. 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, delaying environmental benefits. Distributional concerns also arise if some demographic groups are better able to circumvent upcoming restrictions than others.

Feebate systems provide a related policy tool, combining subsidies for low-emission vehicles with higher annual registration fees for high-emission ones. 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] In this paper, we study the extent of such anticipatory adoption in the context of Sweden's 2018 bonus–malus reform.[3] Anticipation effects will be larger in settings where either anticipation is easy (consumers can either recategorize vehicles without changing their behavior or slightly shift the timing of their behavior) or where complying with the policy is challenging (there are high costs to switching to subsidized vehicles). In addition to quantifying the extent of anticipation, we document how these channels influenced anticipatory purchases around Sweden's feebate policy: dealer behavior facilitated anticipatory purchases with limited behavior change by consumers, and while consumers with lower barriers to electric vehicle adoption made similar levels of anticipatory purchases, they were more likely to make electric vehicle purchases post-policy.

[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.

[3] 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.

To study anticipatory behavior in Sweden, we examine how individuals and firms responded to the reform across multiple margins—adoption timing, vehicle technology choice (fuel type and emissions profile), and driving behavior. To do so, we combine administrative datasets covering all passenger vehicles in Sweden from 2016 to 2020, including detailed vehicle attributes and sociodemographic characteristics of both firms and individuals. These data allow us to address three core questions: (i) to what extent do consumers bring forward vehicle adoptions to avoid the impending registration fees, and how many of these adoptions are additional; (ii) how do vehicle attributes change before and after the reform; and (iii) how does driving behavior differ for vehicles adopted before versus after policy implementation.

First, we document pronounced anticipatory responses to the reform. In the seven months between announcement and implementation, individual registrations rose by more than 35 per 100,000 residents (about 22 percent relative to baseline), and company registrations increased by more than 31 per 100,000 residents. Although the pre-implementation surge is largely offset by a post-implementation decline of similar magnitude—consistent with substantial intertemporal substitution—a positive residual remains, indicating that the reform induced additional adoption beyond simple timing shifts. Companies appear able to shift purchases over a longer window, consistent with fleet-management practices and greater flexibility in replacement cycles. Heterogeneity by fuel type reveals that the anticipatory surge is driven entirely by fossil-fuel vehicles. In contrast, battery-electric vehicle (BEV) registrations decline slightly before implementation and increase afterward, suggesting that BEV buyers respond directly to the incentive structure embedded in the feebate.

Second, vehicles purchased in the pre-policy period are systematically more polluting, with higher average CO_2 emissions and fuel intensity, consistent with individuals and firms bringing forward dirtier models to avoid higher future road taxes. Following implementation, newly registered vehicles are substantially cleaner, particularly among firms, which shift sharply away from high-emission diesel vehicles toward lower-emission alternatives. The results imply that anticipatory behavior amplified environmental damages by concentrating adoption of high-emission vehicles during the announcement period, while post-policy incentives shifted adoption toward cleaner technologies.

Third, vehicles adopted in anticipation of the bonus–malus reform were driven more intensively in later years than vehicles adopted after implementation. For individuals and companies alike, vehicles adopted during the announcement window exhibit higher subsequent usage than vehicles adopted immediately before the announcement, by roughly 146 and 288 kilometers, respectively. These increases are driven primarily by greater petrol vehicle usage and coincide with sharp reductions in diesel kilometers, consistent with a reallocation of driving across fuel types following the policy announcement. Electric vehicles

display a distinct pattern: EVs adopted by individuals prior to implementation are driven less than baseline EVs, while those adopted after implementation—when incentives were fully in place—are driven substantially more, suggesting that financially motivated adopters make more intensive use of electric cars. Taken together, the evidence shows that anticipation effects influenced not only the timing of vehicle purchases but also the composition of vehicles acquired and their subsequent usage intensity.

An important question is how anticipatory behavior affected the policy's environmental objectives. 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, together with the additional driving undertaken by vehicles purchased early; and (iii) the potential environmental benefit associated with the accelerated retirement of older, higher-emission vehicles. We find that the costs of anticipation are around $62 million USD, a cost that represents around half of the direct annual costs of the policy in 2019. These costs are disproportionately from firm anticipatory behavior, rather than individual anticipatory behavior, both because of differences in total kilometers traveled by firms and because firms who adopt vehicles under the policy tend to be more responsive to the policy.

Given the important role of vehicle policy in future decarbonization strategies, understanding the mechanisms underlying anticipatory adoption is essential. We examine several channels that may shape such responses. First, we show that dealers play a central role by expanding inventories before implementation and pre-registering high-emission vehicles. These actions allow dealers to meet the surge in pre-policy demand and enable subsequent buyers to avoid higher post-implementation registration fees. In particular, dealers expanded inventories and pre-registered high-emission vehicles before implementation, enabling subsequent buyers to avoid the higher registration fee. To the extent that these practices facilitated anticipatory purchases, they represent a potential loophole in the policy.

Second, an advantage of our administrative data is that it allows us to study heterogeneity in behavioral responses by various individual-level demographic characteristics, shedding light on the distributional consequences of the policy. Financial constraints appear to be a key determinant of anticipatory behavior: individuals in the top two disposable-income percentiles are substantially more likely to advance purchases and choose models with higher average emissions, whereas low-income individuals make fewer anticipatory purchases and are less likely to adopt high-emission vehicles after implementation. Gross income does not predict timing shifts. This pattern indicates that financial flexibility, rather than income alone, governs consumers' ability to respond to impending policy changes. Firms likewise

display heterogeneity, with those having higher net revenue, larger workforces, and greater value added exhibiting the largest anticipatory shifts. Together, these patterns indicate that the incidence of the feebate is uneven: financially stronger individuals and firms are more able to accelerate purchases to avoid the fee, while lower-income individuals—constrained in their ability to do so—are more likely to bear the policy's costs.

Third, we assess the information channel (Cerruti et al. 2023). 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 – provide further suggestive evidence that access to information shapes policy responsiveness. Finally, we also examine the role of existing vehicle fleets. Individuals and firms with higher prior vehicle usage and dirtier pre-policy fleets exhibit stronger anticipatory adoption of high-emission vehicles, consistent with stronger incentives to avoid the forthcoming fee. Finally, we evaluate access to workplace charging infrastructure and find that charging availability mitigated anticipatory responses and increased post-policy electric vehicle adoption.

This paper contributes to several strands of the literature. First, it extends the "Green Paradox" literature on anticipatory behavior in advance of climate policies. Many "Green Paradox" papers focus on oil and other fossil fuel markets, documenting that when the market expects climate policy, consumption (and emissions) are shifted to the present day (Norman & Schlenker 2024, Hoel 2010, Sinn 2008, van der Ploeg & Withagen 2012, 2015).[4] Holland et al. (2021) show that similar dynamics are at play in vehicle markets, where a ban on gasoline vehicles can lead to spikes in purchases before the ban, and Rittenhouse & Zaragoza-Watkins (2018) empirically document this anticipatory purchasing behavior in the US heavy truck market prior to the implementation of stricter engine emission standards.[5]

[4]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 & Card 1985). Likewise, in the context of birth timing, Gans & Leigh (2009) document how parents in Australia postponed childbirth to take advantage of a forthcoming subsidy. A long-standing literature in macroeconomics similarly emphasizes the intertemporal substitution of durable goods consumption in anticipation of future changes in prices or credit conditions (Ogaki & Reinhart 1998, Gowrisankaran & Rysman 2012, Gavazza & Lanteri 2021).

[5]Rittenhouse & 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. This makes comparison between our results more difficult.

Similarly, we document pre-buying behavior in advance of the implementation of a Swedish feebate system. Our contribution extends this literature by exploiting detailed Swedish administrative data that allow us to study anticipatory responses along multiple margins. In addition to documenting intertemporal substitution in vehicle purchases, we quantify the extent of excess adoption, characterize the emissions intensity of vehicles purchased in anticipation, and examine their subsequent usage. This approach enables us to assess the environmental consequences of policy announcements and to shed light on which consumers respond and through which channels.

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 bringing together estimates of anticipatory behavior with an empirical framework that quantifies its environmental consequences. Relative to Holland et al. (2021), who develop a theoretical framework showing how advance announcements of vehicle bans can induce pre-implementation purchasing, we provide direct empirical evidence on pre-implementation purchasing and trace how these responses translate into realized emissions. Our contribution is to bring these strands together by estimating the key behavioral responses using administrative vehicle data and implementing them in an empirical framework to quantify the environmental consequences of policy announcements. A significant contribution of our analysis is to deliver an empirically implementable decomposition of announcement-induced environmental damages into excess vehicle adoption, changes in vehicle characteristics, and driving responses—objects that can be estimated in a wide range of vehicle policy settings.

Additionally, 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 & Eskeland 2018), while others find limited effects on new cars (Alberini & Bareit 2019) and mixed effects on the retirement of older cars (Alberini et al. 2018). Notably, D'Haultfœuille et al. (2014) provides evidence of anticipatory purchases before a French bonus-malus policy was implemented, though the effects were shorter lived. Relative to existing evidence on bonus–malus systems, we show that anticipatory responses can generate persistent excess adoption, dirtier vehicle choices, and higher subsequent usage, substantially increasing the environmental costs of policy announcements.

Lastly, this paper also contributes to the difference-in-differences literature by emphasizing the importance of accounting for anticipation effects in contexts where policies are announced prior to their implementation. As Malani & Reif (2015) argue, failing to align the treatment timing with the onset of behavioral 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 & Klaiber 2019, Bilicka et al. 2025).

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.

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 explicitly 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 electric vehicle, 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 purchases, including a subsidy s for the purchase of an electric vehicle 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 electric vehicle, either now (V_{e0}) or in the future (V_{e1}) as follows:

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

where p_e is the price of an electric mile, p_Ω is the price of an electric vehicle, 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 electric vehicles 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_1^* where

$$\tau_1^* = \frac{\delta_g \left(\frac{\partial \pi_{g0}}{\partial t} g_0 + \beta \frac{\partial \pi_{g1}}{\partial t} g_1 \right) + \delta_e \left(\frac{\partial \pi_{e0}}{\partial t} e_0 + \beta \frac{\partial \pi_{e1}}{\partial t} e_1 \right)}{\beta \frac{\partial \pi_{g1}}{\partial t}}$$

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). A proof of these results is in Appendix A.

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), and thus we examine these responses in the empirical results that follow.[6]

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.[7] 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 between July 1, 2018 and March 31, 2024. 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. 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,

[6] Specifically, it can be shown that as the share of gasoline purchases in period 1 that switch to gasoline purchases in period 0 increases, the optimal period 1 tax decreases.

[7] 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.

its standardized CO_2 emissions per kilometer as recorded at first registration. The emission thresholds governing bonus eligibility became increasingly stringent over time. 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. 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 introduced in the 2018 phase. As an example, a Volvo XC90 T8 (PHEV, 40 g/km, \$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.

We also provide evidence that the public was aware of the implementation of the bonus-malus policy by analyzing Google Search Index data. Specifically, we observe a significant spike in Google search activity between the policy announcement and its implementation, as shown in Figure B6.

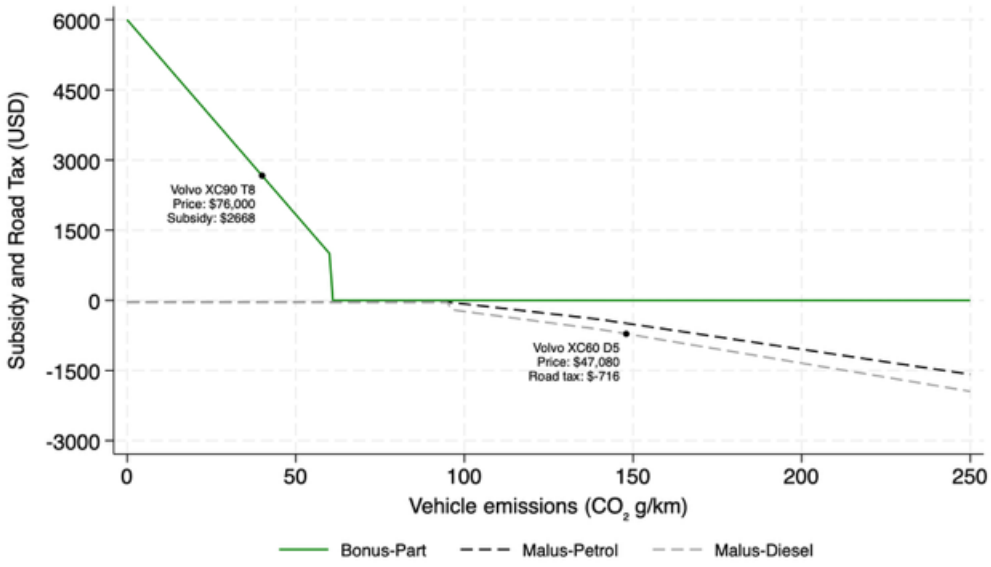


Figure 1: Bonus-malus system introduced in July 2018

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 $CO_2 \leq 60 g/km$. 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 the anticipatory effects of the Swedish feebate system, we combine information from several administrative sources provided by Statistics Sweden (*Statistiska centralbyrån*). The primary data sources are the Swedish vehicle register (*Fordonsregistret*), the longitudinal integrated database for health insurance and labor market studies (LISA), the Swedish occupational register (*Yrkesregistret*), the population and housing census (*Folk- och bostadsräkningar*), the Swedish business register (*Företagsregistret*), and the geographic database (*Geografidatabasen*) from 2016 to 2019. We supplement these data with information on the financial implications of vehicle reforms enacted by the Swedish government.

Vehicle data. We use data from the Swedish vehicle register on new passenger vehicles. The data includes information on the ownership type (private individual, company, dealer),[8] general status of the vehicle (registration date, whether it is leased, when the vehicle became the property of the current owner, in use or not, etc.), the specification of the vehicle (make, model, and trim), characteristics (service weight, odometer reading, fuel type, fuel intensity, particle filter, carbon emission intensity, etc.), the listing price and the annual vehicle kilometers traveled. Each registration includes a vehicle identification number and an anonymized individual- and firm-level identifier, uniquely linking each vehicle to a specific individual, company, and retailer in Sweden.

Demographic characteristics. To match individuals to their vehicles, we use individual identification numbers to link the Swedish vehicle registry to the LISA data, which contains sociodemographic information for Swedes over the age of 18 (gender, age, family status, disposable income, gross salary, education, and employment status). We link this data to the Swedish occupational register, which includes information on the gross salary, employment status, workplace industry code, and duration of employment on an annual basis. We also supplement the data with the geographic location of the residence of employees and the workplace, measured by 250m grid cells in all urban areas or 1000m cells in rural areas. We use data from the Swedish population registries to obtain population counts for each geographic unit. To examine whether pre-existing public charging infrastructure, particularly work chargers, influenced the magnitude of anticipation effects, we incorporate data from ChargeX (Uppladdning.nu), which provides detailed information on the number, location, and technical characteristics of Sweden's public charging stations.

Firm data. For companies, we add information on Swedish firms using the business register. This includes number of employees, net revenue, personnel cost, and social contri-

[8] 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.

bution cost. We also supplement the data with the geographic location of the workplace, again measured by 250m grid cells in all urban areas or 1000m cells in rural areas.

Policies and macroeconomic data. 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) and Monthly population data from 2016 to 2019 was obtained from Statistics Sweden. Monthly crude oil prices (in USD dollars) for the same period were collected from *Drivkraft Sverige*.

C. Sample construction

To measure the anticipation effect on new car registrations, we construct a monthly panel dataset of new passenger car adoption by end users. We distinguish two types of adopters who adopted at least one new vehicle: (1) private individuals and (2) non-retailer companies. We distinguish adopters by end-user type because private individuals and firms differ in their responses in terms of both new vehicle registrations and subsequent vehicle usage, which has important implications for driving behavior and environmental outcomes. To estimate the impact on subsequent driving behavior and vehicle characteristics, we restrict the sample to vehicles newly registered by private individuals and companies, resulting in a monthly panel of all new vehicle registrations by these end users.[9]

The sample period spans from January 2016 – coinciding with the introduction of the super green car premium (*supermiljöbilspremie*) – to July 2019. We end the sample at this point due to a noticeable increase in residual variance thereafter (see Figure 3), which likely reflects anticipatory behavior in response to the feebate policy changes set to take effect in 2020.[10]

Table 1 provides summary statistics on the vehicle fleet in 2017. Vehicles registered to individuals represent a slightly larger share of the new vehicle fleet than vehicles registered to companies. In our data, approximately 24% of vehicles used by individuals were leased, versus 75% of vehicles used by companies. Companies disproportionately use diesel vehicles, resulting in lower emissions intensity, fuel intensity, and particle value; corporate vehicles

[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.

[10]Since January 2020, the *bonus–malus* system has adopted the WLTP standard, which reports emissions for combustion vehicles approximately 20% higher than the previous test. This change further increased taxes on fossil-fuel cars and strengthened incentives for EV adoption.

are also heavier and more powerful than vehicles used by individuals. These differences may reflect a difference in preferences, usage patterns, and financial setup, which may in turn have implications for how the two groups respond to the feebate policy. In the results that follow, we separately estimate effects for individual and company cars.

Figure B8 shows that petrol and diesel vehicles account for the highest annual driving distances, with diesel vehicles consistently exceeding petrol among companies but not individuals. Electric, plug-in hybrid, and hybrid vehicles exhibit substantially lower and relatively stable travel distances over the 2016–2019 period.

Table 1: Vehicle summary statistics

	Individuals		Companies	
	Total	Std. Dev.	Total	Std. Dev.
Panel A: Vehicle Fleet				
New registrations	169,590	0.16	142,265	3.50
New lease	41,094	0.08	106,640	2.64
Petrol	102,320	0.13	25,835	1.43
Diesel	54,399	0.09	92,675	2.22
Hybrid	101,33	0.04	53,87	0.32
Plug-in hybrid	1,455	0.02	11,206	0.30
Battery electric	1,105	0.01	2,498	0.10
	Mean	Std. Dev.	Total	Std. Dev.
Panel B: Vehicle Characteristics				
Vehicle emissions (g/km)	125.05	36.23	122.49	41.86
Fuel intensity (L/100km)	5.41	1.49	5.25	1.37
Particle value	0.70	0.69	0.48	0.44
Vehicle weight (kg)	1984.01	375.42	2283.16	337.45
Engine power (kW)	106.38	47.30	131.03	49.73
N (Observations)	511,351		101,757	

Notes: This table presents 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).

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 electric vehicles (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 primarily by individuals and non-retailer firms (Figure B1), whereas dealers' purchasing behavior followed a more regular seasonal cycle, with registrations peaking toward the end of each calendar year.[12] Consistent with this compositional shift, the average 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 markedly, indicating that the policy both deterred high-emission registrations and accelerated the adoption of lower-emission vehicles once the malus was in place.

Figure B1 displays new vehicle registrations by owner type. Both individuals and firms exhibit a sharp pre-policy increase, concentrated in higher-emission vehicles. Figure B3 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.

IV. Empirical methodology

This section describes the empirical methodology used to identify anticipation effects and estimate dynamic policy responses, and provides validity tests of the underlying identification strategy.

[11] Although our primary focus is on pre-buying behavior of passenger cars, Figure B2 shows that similar anticipation effects are also evident among lorries.

[12] To ensure that these trends reflect actual adoption behavior, we match each new registration to its end user, as many vehicles are initially registered by dealers before being sold or leased to individuals or firms.

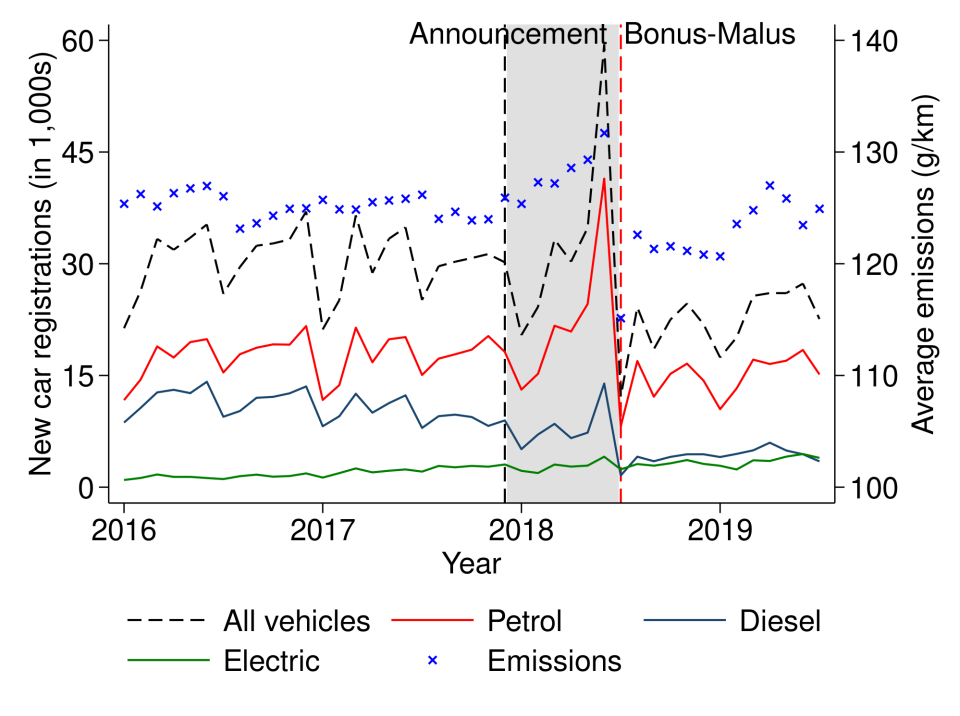


Figure 2: New vehicle registrations by fuel type

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 electric vehicles, 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.

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 previous studies (Lam & Bausell 2007, Rittenhouse & Zaragoza-Watkins 2018), we model monthly new vehicle adoptions as a function of key macroeconomic drivers as well as demographic characteristics for individuals and firm 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 empirical specification is estimated at the neighborhood-month level. We run the regression separately for all individuals in geographic unit i and for all firms in geographic unit f, as follows:

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

where the main outcome of interests, Q_{it} and Q_{ft}, refer to the total number of newly adopted passenger vehicles by individuals and companies per 100,000 residents in geographic unit i or f during month t.[13] 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 (including January 2019). We select this post-policy window for two main reasons: First, the residual variation in vehicle registrations largely dissipates by January 2019 (Figure 3), and the dynamic treatment effects become statistically insignificant for both individuals and companies around that time (Figure 4); and second, choosing this window ensures a symmetric pre- and post-policy period.[14] The coefficient of interest, γ, captures the anticipation effect prior to the implementation of the bonus-malus policy (i.e., the change in number of new vehicle registrations during the pre-policy period relative to the baseline months). The coefficient β measures the effect of the policy in the post-implementation period.

For the individual-level regressions, the covariate vector X_{it} comprises 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_{ft} includes average firm characteristics (number of employees, value added, net revenues) alongside the same macroeconomic indicators. Month fixed effects ρ_t control for seasonal trends, and μ_j captures geographic unit fixed effects. We estimate the regression on the neighborhood-level for total new car registrations, and on the individual-level for new car characteristics and vehicle kilometers traveled.

In addition to total vehicle registrations, we estimate equation (1) for new registrations of different fuel types–petrol, diesel, hybrid, PHEV, and BEV–to explore compositional changes in response to the policy. We also use this identification to examine intensive-margin adjustments among new vehicles in four vehicle characteristics where Q_{jt} is replaced by y_{jt}, representing average carbon emissions intensity (g/km), fuel intensity (liters/100 km), and particle emissions ($\mu g/m^3$).

[13] In addition, we test specifications using the logarithm of new registrations, and total registrations (Table E5)

[14] We also show that our results are robust to longer post-period definitions (Table E4).

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 in geographic unit i and companies in geographic unit f is given by:

$$Q_{jt} = \sum_{k \in \{-m,\ldots,0,\ldots,n\}} \gamma_k D_{m,t-k} + \rho_t + \mu_j + \delta X_{jt} + \varepsilon_{jt} \quad \text{for } j \in \{i, f\}, \tag{2}$$

where k indexes event time relative to the policy announcement (December 2017). 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 key identifying assumption underlying our empirical strategy is that, absent the announcement of reform, vehicle registrations would have followed similar trends across months before and after the policy announcement. While the parallel trends assumption is inherently untestable, the dynamic specification allows us to visualize the trajectory of vehicle registrations and environmental characteristics over time. We verify that there are no pre-trends in vehicle sales (Figure 4) or average vehicle characteristics before the announcement period, conditional on control variables and fixed effects.

C. Validity tests

To assess the validity of our identification strategy, we examine whether observable characteristics are balanced across buyers who adopted in the first and second half of the year. As a measure of imbalance across variables, we employ the normalized difference in means between the pre- and post group (Imbens & Rubin 2015, Austin 2009). The normalized difference is a scale-free measure of imbalance, with values of 0.2, 0.5, and 0.8 referred to represent small, medium, and large imbalances. Tables B1 and B2 report the means, and normalized difference in demographic and vehicle variables for individuals and companies in the year before the implementation (2017).

The normalized differences in 2017 indicate that individuals and companies who adopted new cars in the two periods were highly similar in both their demographic and vehicle characteristics, suggesting that seasonal composition in the car market is stable within a year. Likewise, the corresponding statistics for 2018 confirm that observable characteristics remain comparable across halves of the year. This evidence supports the validity of our design: we show that the key identifying assumption – that the composition of vehicle

buyers does not systematically differ between the first and second half of the year – holds for both individuals and companies.

V. Regression results

A. Anticipation effects

Before presenting the regression results, we first provide graphical evidence to illustrate the pre- and post-buy 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, firms, or dealers adjusted their purchasing behavior in anticipation of the policy, we would expect to observe systematically 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 clear pattern indicative of intertemporal shifts in vehicles sales among individuals and firms 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 the implementation, residuals decline sharply and turn negative, suggesting a drop in sales relative to model predictions. Notably, the pre-policy surge is sharper but shorter in duration—concentrated within the two months prior to implementation—whereas the post-policy decline unfolds more gradually over approximately six months, indicating an asymmetric response. A likely explanation for this asymmetry is consumers who shifted forward purchases that were planned for late 2018 did not want to acquire their new cars needlessly early. Additionally, the incentive structure of the bonus component may have affected the return to normal sales levels, as it resulted in a gradual increase in the adoption of BEVs, hybrids, and PHEVs (see Figure B3).

Table 2 presents the estimated pre- and post-policy coefficients from equation (1) for new vehicle registrations per 100,000 inhabitants, disaggregated by fuel type (petrol, diesel, and electric vehicles). For individuals (Panel A), column (1) indicates that monthly new registrations increased by roughly 36 vehicles per 100,000 people (corresponding to a 22 percent increase or a total of 24,593 anticipatory purchases during the pre-period) during the seven-month period between the policy announcement and implementation. Following the policy's implementation, the coefficients indicate an almost symmetric decline of approximately 31 vehicles per 100,000 inhabitants, implying that the pre-policy surge was primarily

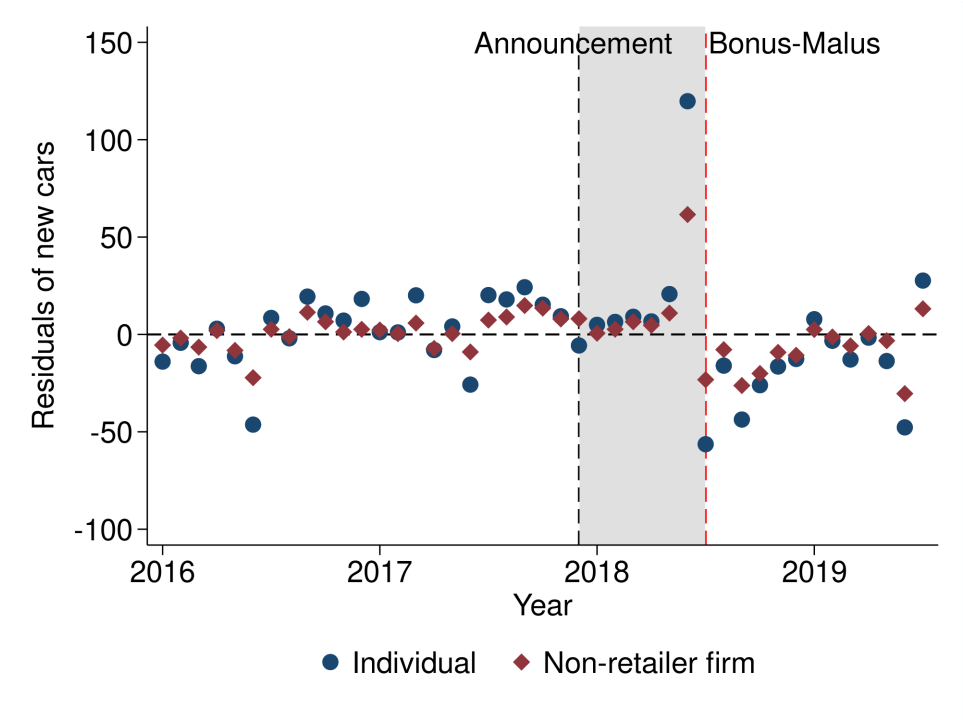


Figure 3: Monthly residuals of car registrations

Notes: This figure displays the residuals for individuals (blue), firms (brown), and dealers (purple) from the regression model in equation (1) at the municipality-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.

driven by intertemporal substitution.[15] In other words, many individuals advanced planned vehicle purchases to avoid the higher tax introduced under the malus component, rather than generating new demand, with around five vehicles per 100,000 representing additional cars.[16] For companies (Panel B), the pre-policy effects shows an increase of roughly 31 vehicles per 100,000 inhabitants (or a total of 21,430 anticipatory purchases), followed by a post-policy decline of about 27 vehicles. This pattern similarly points to substantial intertemporal substitution, with firms shifting purchases forward to avoid the higher fee. As with individuals, only a small residual—around four vehicles per 100,000—appears to reflect additional demand rather than timing shifts.

An important question is whether the observed anticipation effects also altered the com-

[15]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 reflect the combined responses of individuals and firms.

[16]Results remain robust when using alternative outcome measures, including the logarithm of new car registrations and total new car registrations (Table E5).

position 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 Part E, as it sheds light on whether the 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, diesel, plug-in hybrid electric, battery electric, and hybrid electric vehicles. Column (2) and (3) of Table 2 shows that most of the anticipation effects stem from additional petrol vehicles for individuals, with a smaller contribution from diesel vehicles. These pre-policy increases are followed by sharp declines in registrations for both petrol and diesel cars after implementation. In contrast, EV registrations show a reduction in the pre-period, followed by a post-policy increases, suggesting that the bonus component encouraged both a small degree of intertemporal substitution and addition of new cleaner vehicle technologies in the months following implementation. Because firms rely more heavily on diesel vehicles, they display stronger anticipation effects for diesel than for petrol cars (in percentage terms). In contrast to individuals, there are no anticipation effects for EVs among company vehicle purchases.

Table E6 presents the estimated pre- and post-policy coefficients for vehicle leases and purchases separately. The results indicate that for individuals, the majority of the pre-effect came from vehicle purchases rather than leases, consistent with individuals primarily advancing planned acquisitions of vehicles in anticipation of the reform. However, since the post-policy effect on purchases is considerably more negative and the share of new leases increased in the post-period (column 3), we also find evidence of a substitution toward new leases rather than purchases, indicating that the policy induced individuals to lease vehicles rather than buy them. In contrast, for companies, a larger share of the adjustment occurred through new leasing contracts, reflecting differences in financing structures and fleet management practices between firms and private individuals.

To assess whether excess vehicle registrations reflect additional buyers entering the market or a higher number of vehicles adopted per buyer, we decompose new registrations per 100,000 inhabitants into two margins: the participation rate, measured as the number of unique buyers relative to the population, and the number of vehicles purchased per buyer. Table E2 shows that, for individuals, the anticipation effect operates primarily through increased market participation, with many individuals advancing entry into vehicle adoption during the announcement period, and only a modest increase in vehicles per buyer. For firms, by contrast, the response is more evenly split across margins, with roughly half of the increase driven by additional firms entering the market and the remainder by existing firms purchasing multiple vehicles.

We also examine responses in the second-hand vehicle market. Announcing higher

future costs for new fossil-fuel vehicles may increase demand for used fossil-fuel cars and put upward pressure on used-vehicle prices, creating incentives for individuals and firms to advance purchases of second-hand vehicles prior to implementation. At the same time, following the implementation, prospective buyers in the new-vehicle market may substitute toward used vehicles, increasing post-announcement demand in the second-hand market. For individuals, these opposing forces appear to largely offset each other (Table E3). For companies, however, we find evidence of a modest increase of around 5% in pre-announcement registrations of second-hand vehicles, suggesting that the incentive to advance used-vehicle purchases dominates.

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	35.85***	31.29***	4.66***	-1.06***	0.89***
	(0.96)	(0.75)	(0.50)	(0.13)	(0.23)
Post	-31.41***	-17.37***	-12.90***	1.85***	-2.97***
	(0.80)	(0.62)	(0.41)	(0.15)	(0.21)
%-Pre effect	22.03	31.27	9.60	-30.13	8.52
%-Post effect	-19.31	-17.35	-26.6	52.5	-28.29
B. Companies					
Pre	31.24***	13.62***	15.52***	.67	1.40
	(5.33)	(2.49)	(2.94)	(.69)	(.86)
Post	-27.29***	-3.30*	-21.95***	.97	-.99**
	(4.01)	(1.81)	(2.30)	(.89)	(.40)
%-Pre effect	26.28	53.94	21.62	5.26	25.55
%-Post effect	-22.95	-13.06	-30.57	7.63	-18.16
Controls	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
Observations	255,397	255,397	255,397	255,397	255,397

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 three fuel types: petrol (Column 2), diesel (Column 3), plug-in hybrid and battery electric (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.

B. Car usage

Table 3 reports estimates of the effect of the bonus–malus policy announcement on subsequent vehicle usage, measured as average annual kilometers driven per vehicle.[17] Column (1) presents results for all new vehicles, while columns (2)–(4) report estimates separately for petrol, diesel, and electric vehicles. The analysis is restricted to vehicles newly registered

[17]This time period is chosen because vehicle kilometers traveled (VKT) is recorded starting at 36 months.

between January 2017 and July 2019. To assess how the vehicle adoption patterns would have evolved in the absence of anticipatory behavior, we assume that emissions following the post-implementation period reflect the "steady-state" emissions under the policy. Specifically, we designate a third time period (February-July 2019) the "steady-state" period, in which vehicle purchases are influenced by the policy but not by intertemporal substitution to avoid the policy, and include these months in the main regression model. The estimated coefficients therefore capture how post-2020 driving behavior differs for vehicles adopted in the pre-announcement, post-announcement, and steady-state periods relative to vehicles adopted in the baseline period (defined as the six months preceding the policy announcement

For individuals (Panel A), cars adopted during the announcement period are driven 145 kilometers (1.1%) more than baseline vehicles, with particularly large increases in petrol vehicle usage and sharp reductions in diesel kilometers. In contrast, vehicles adopted in the post-policy period show a reduction of 69 vehicle kilometers traveled (0.5%) in subsequent years, with a substantially larger effect for diesel cars. This is consistent with a reallocation away from diesel-intensive driving following the reform. EVs display a different pattern: EVs purchased by individuals in the pre-policy period are driven less than baseline EVs, whereas EVs adopted in the post-policy period are driven substantially more. This suggests that late EV adopters – who took advantage of the financial incentives – used these vehicles more.

For firms (Panel B), cars adopted in the pre-period show an increase of 288 total kilometers (1.1%), though with higher baseline usage levels. As with individuals, the results indicate a shift away from diesel toward petrol usage following the policy, consistent with changes in relative operating costs and tax treatment. For EVs, companies show sizable increases in post-policy usage, suggesting that EVs adopted after implementation were more actively deployed within corporate fleets. Taken together, the results indicate that anticipation effects influenced not only the timing of vehicle purchases but also the composition of vehicles acquired and their subsequent usage intensity. In particular, vehicles purchased ahead of the bonus–malus implementation in anticipation of higher future road taxes were driven substantially more in later years than vehicles purchased after the reform, highlighting that intertemporal substitution in adoption translated into persistent differences in driving behavior.

Table 3: Effect on vehicle usage by fuel type

	Vehicle kilometers				
	(1) New cars	(2) Petrol	(3) Diesel	(4) EVs	(5) Hybrid
A. Individuals					
Pre	145.6***	1409.5***	-1262.2***	-50.9***	52.3***
	(18.2)	(21.9)	(24.1)	(6.8)	(10.7)
Post	-69.4***	1134.6***	-2129.9***	487.8***	441.4***
	(19.3)	(24.0)	(25.6)	(10.2)	(13.0)
SteadyState	90.3***	1492.8***	-2223.3***	279.4***	541.1***
	(18.4)	(23.3)	(25.2)	(9.0)	(12.9)
%-Pre effect	1.13	18.62	-32.36	-11.58	5.13
%-Post effect	-.54	14.99	-54.61	110.98	43.27
Mean Driving	12940	7568.3	3900	439.5	1020
B. Companies					
Pre	288.4***	899.7***	-931.4***	129.3***	65.1**
	(72.4)	(35.2)	(84.6)	(33.9)	(27.0)
Post	-864.9***	597.9***	-3292.4***	1506.7***	348.2***
	(74.6)	(36.7)	(88.9)	(40.0)	(31.7)
Steady-state	-875.9***	660.3***	-3155.0***	1281.4***	407.2***
	(69.7)	(34.0)	(81.9)	(37.5)	(28.6)
%-Pre effect	1.35	33.37	-6.67	5.14	6.33
%-Post effect	-4.06	22.18	-23.58	59.84	33.89
Mean Driving	21299.3	2696.3	13963.4	2517.9	1027.5
Controls	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
Observations	292,980	292,980	292,980	292,980	292,980

Notes: This table presents pre- and post-policy regression estimates on vehicle kilometers traveled from equation (1) at the neighborhood-level for all new cars (column 1) and across three fuel types: petrol (Column 2), diesel (Column 3), plug-in hybrid and battery electric (Column 4), and hybrid electric vehicles (Column 5) for individuals (Panel A) and companies (Panel B). We restrict the sample to new vehicle registrations observed during the baseline, pre-policy, post-policy, and steady-state periods. The outcome variables measure differences in vehicle usage between 2020 and 2023 for vehicles registered in the pre-policy, post-policy, and steady-state periods, relative to vehicles registered in the baseline period. All estimates are reported separately by fuel type. All regressions include demographic characteristics for individuals and firm-level attributes for companies, along with macroeconomic controls, neighborhood fixed effects, and 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.

C. Car attributes

We previously showed (in Table 2) that the policy influenced intertemporal substitution and additional purchases differently by vehicle fuel type. Given that the malus was increasing in emissions even within fuel type, the policy may have also influenced behavior along the intensive margin, affecting car characteristics such as CO_2 emissions and fuel intensity. As in the vehicle adoption regressions, we separate effects in the pre-implementation post-announcement (pre), post-implementation (post), and "steady state" periods.[18]

Table 4 reports the estimated pre-, post-, and steady-state effects of the bonus–malus policy on vehicle characteristics for individuals (Panel A) and companies (Panel B). Both purchased vehicles with higher CO_2 emissions and fuel intensity in the pre-policy period, consistent with bringing forward more polluting models to avoid the higher fee. Following the implementation, newly registered vehicles were substantially cleaner, exhibiting lower average emissions, fuel intensity, and particle values. The response is stronger among companies, which purchased particularly high-emission vehicles in the anticipation period but switched to cleaner vehicles afterward.[19] In contrast, individuals' post-policy adjustments were more moderate. Finally, the steady-state estimates show that vehicles purchased several months after the policy stabilized were not as clean as the immediate post-policy cars for individuals, but considerably cleaner for companies. This pattern for individuals likely indicates that the intertemporal substitution was largely over by the steady-state period;[20] for companies, the dynamic effects of the policy may be more complex.

[18] 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.

[19] This finding is consistent with other policy work (https://www.nordicenergy.org/wp-content/uploads/2019/11/supergreenrebate_web.pdf), which finds that most of the cars that received the subsidy under the feebate policy were company cars. Firms, which tend to operate multiple vehicles, may have been in a better position to shift some of their vehicle adoptions to alternative fuel vehicles.

[20] As discussed in the environmental impact section below, if buyers of more polluting cars are more likely to make anticipatory purchases but anticipatory purchases can't be made more than 6 or 7 months in advance, we'd expect the emissions in the immediate post-policy period to fall by more than they will in the long-run.

Table 4: Effect on car characteristics

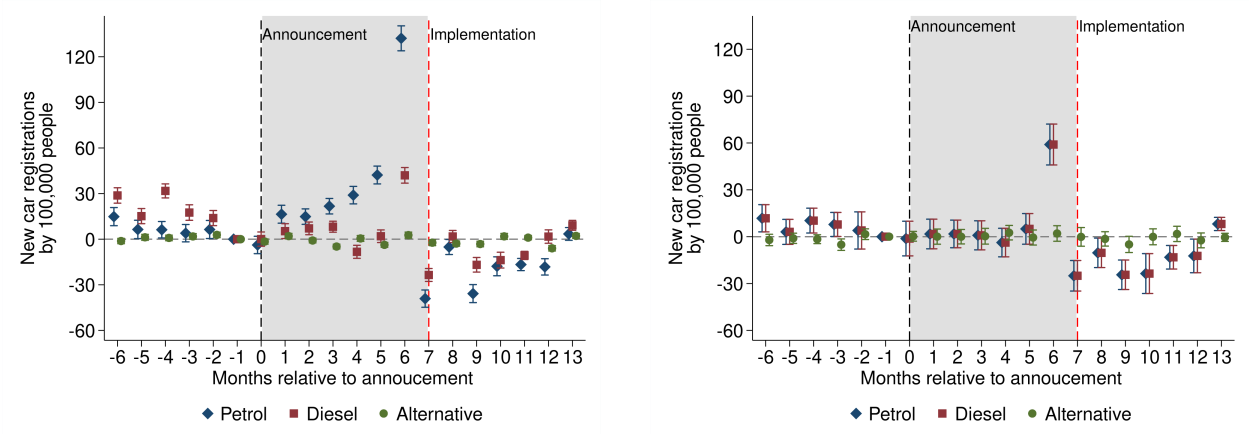
	Car characteristics		
	(1) Carbon emission	(2) Fuel intensity	(3) Particle value
A. Individuals			
Pre	2.49***	0.13***	-0.02***
	(0.20)	(0.01)	(0.01)
Post	-3.75***	-0.06***	-0.24***
	(0.22)	(0.01)	(0.01)
Steady state	-1.11***	0.03***	-0.30***
	(0.26)	(0.01)	(0.01)
Mean characteristic	125.16	5.31	.65
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.

D. 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 policy announcement. The results are disaggregated by fuel type—petrol (blue), diesel

(red), and alternative-fuel vehicles (green). For individuals, there is a pronounced increase in registrations beginning shortly after the announcement and peaking just before implementation, driven almost entirely by petrol vehicles, with a smaller increase by diesel cars. This sharp pre-policy spike followed by a steep decline after implementation reflects strong anticipation effects, as individuals advanced purchases of fossil-fuel vehicles to avoid the forthcoming higher fee. Registrations of alternative-fuel vehicles are slightly reduced during the announcement period. For companies, the anticipatory surge occurs immediately before implementation, with a pronounced rise in registrations of diesel vehicles. Following the policy's introduction, company registrations drop sharply and remain persistently below pre-announcement levels, suggesting a lasting contraction in fossil-fuel purchases. 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 alternative-fuel vehicles 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 and fuel intensity, respectively. Both outcomes display similar temporal patterns. We find no significant changes during the pre-announcement period, suggesting a lack of anticipatory behavior before the policy was made public. Following the announcement, both carbon emissions and fuel intensity begin to rise, with the largest increases occurring in the three months

immediately preceding the policy's implementation. This pattern indicates that consumers strategically advanced the purchase of more polluting and less fuel-efficient vehicles to avoid future penalties. After implementation, we observe a sharp decline in both emissions and fuel intensity. This drop reflects not only a shift toward cleaner vehicles incentivized by the policy but also a temporary dip in demand due to intertemporal substitution—many purchases had already been brought forward into the pre-policy period.

E. Environmental impact

We have shown that both individuals and firms (i) increased vehicle adoption in anticipation of the policy, and that the increase in registrations during the announcement period exceeded the subsequent post-implementation decline, implying that a share of these cars reflect excess adoption rather than intertemporal substitution;[21] (ii) vehicles adopted in anticipation were driven more intensively in subsequent years; and (iii) these anticipatory cars were, on average, more emissions-intensive than vehicles adopted after implementation. At the same time, these early adoptions likely accelerated the retirement of older, higher-emission vehicles, generating potential environmental benefits. The overall environmental impact of this anticipatory behavior therefore reflects a trade-off between two opposing effects: the environmental cost from adding additional vehicles to the fleet and substituting cleaner future purchases with dirtier early ones, and the environmental benefit arising from the earlier replacement of older, more polluting vehicles. In the following section, we quantify this net effect by comparing the additional emissions from early, high-emission excess vehicles to the reductions associated with accelerated fleet turnover.

Quantification of environmental costs and benefits. To set up our quantification of environmental costs, it is useful to understand who is adopting vehicles in which period. The population consists of three groups: anticipators, who shift forward their vehicle purchases to avoid the policy; excess anticipators who are induced by the policy to adopt a vehicle in the pre-period but do not appear to be intertemporally substituting for new vehicles that would have been adopted in the post-policy period; and non-anticipators, who do not shift forward their purchases. We consider the possibility that anticipators and excess anticipators buy vehicles that are systematically different from non-anticipators in the absence of policy and that these groups buy different vehicles when they make purchases while the feebate is in effect. To calculate the environmental effects of anticipation, we need to calculate

[21] The excess anticipators are perhaps surprising, but we demonstrate that these are unlikely to be intertemporal substitution from a longer post-period by comparing coefficients using longer post periods in table E4. Prolonging the post period does not lead to symmetry in the total anticipatory purchases vs. total reduction in purchases post-policy.

the difference between the vehicles that anticipators adopt pre-policy $\widehat{e^{ant}}$ and the vehicles they would adopt if they could not anticipate the policy $\widehat{e^{no\ ant}}$. We also need to know the emissions of the vehicles that excess anticipators adopt in the pre-period, $\widehat{e^{ant,ex}}$. 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 by the policy (prior to implementation) or affected by the policy (post implementation).

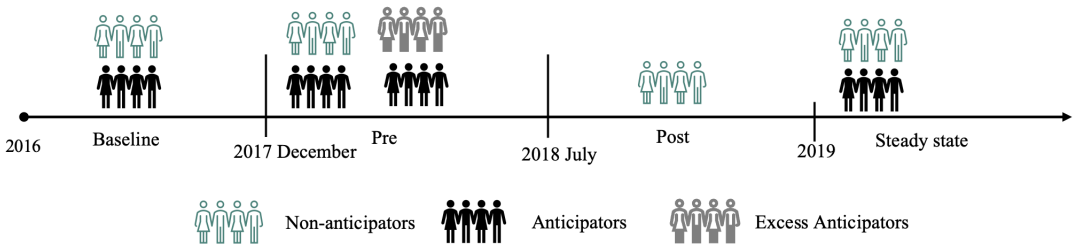


Figure 5: Illustration of anticipators, excess anticipators, and non-anticipators across policy phases

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 derived from the pre- and post-period coefficients in Table (2) by the average emissions of these additional vehicles and their total vehicle kilometers over their lifetime:

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

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] $e_{i/f}^{ant,ex}$ 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 $VKT_{i/f,h}^{ant,ex}$ for

[22]This sum yields a net count of excess anticipators per month of anticipation period in specifications

each year h up until the end of the vehicle's lifespan H thereby accounting for changes in driving behavior among anticipatory adopters. We discount using discount rate $r = 2\%$.[23] As shown in Table 3, vehicles purchased during the announcement period—prior to policy implementation—are driven more intensively in subsequent years than vehicles purchased after implementation. We further assume that the emissions of excess anticipators, $e^{ant,ex}$, and their vehicle kilometers traveled, $VKT^{ant,ex}$ are equal to the corresponding emissions and vehicle kilometers traveled of anticipators who shift their purchases intertemporally, e^{ant} and VKT^{ant}, respectively.

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. Because anticipatory vehicles are also driven more intensively than their counterfactual counterparts in the absence of anticipation, we weight emissions by total vehicle kilometers traveled over the vehicle's lifespan for both anticipatory and counterfactual vehicles. 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:

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

where $Q_{i/f}^{ant}$ refers number of vehicles sales of anticipators and $e_{i/f}^{ant,ex}$ and $e_{i/f}^{no\ ant}$, 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). Emissions are weighted by vehicle kilometers traveled for anticipatory vehicles, $VKT_{i/f,h}^{ant}$, and for the counterfactual vehicles in the absence of anticipation, $VKT_{i/f,h}^{no\ ant}$ for each year h up until the end of the vehicle's lifespan H. In subsequent

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.

[23] https://www.aeaweb.org/forum/4118/guidance-regulatory-analysis-circular-analysis-circular

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.

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:

$$\Delta E^R = Q_i^{ant} \times \Big(\underbrace{e_i^{ant\ ret}}_{\text{Emissions of retired car}} - \underbrace{e_i^{ant}}_{\text{Emissions of new pre-period}} \Big) \times VKT_i^{month} \times m + Q_f^{ant} \times \Big(\underbrace{e_f^{ant\ ret}}_{\text{Emissions of retired car}} - \underbrace{e_f^{ant}}_{\text{Emissions of new pre-period}} \Big) \times VKT_f^{month} \times m \quad (5)$$

where $\widehat{e^{ant\ ret}}$ and $\widehat{e^{ant}}$ refer to the emissions intensity of vehicles retired as a result of anticipatory purchases and newly registered in the pre-period by households responding to the policy.

The net environmental effect of the anticipatory adoption is given by the sum of the environmental costs, ΔE^Q, and ΔE^A, minus the environmental benefits from earlier retirement, ΔE_R.

Implementation. Table 5 reports the key population statistics (Panel A), the derived parameters (Panel B), and all policy coefficients (Panel C) that we use to derive the environmental costs and benefits. Table 6 reports the resulting estimates of total environmental damages attributable to anticipation effects and decomposes these impacts into environmental costs from excess vehicle adoption, emissions, and changes in driving behavior, as well as environmental benefits arising from the accelerated retirement of older vehicles. Results are presented separately for individuals and firms. Appendix F.3 provides the full set of formulas and assumptions underlying these calculations. All environmental cost estimates are expressed in real 2021$, converted using the Consumer Price Index from Statistics Sweden.

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.[24] Figure B5 provides additional evidence that neither vehicle deregistration patterns nor associated emissions display any apparent trend for individuals or firms.[25] There is some possibility that consumers may not have actively deregistered vehicles, even as they stopped using them. Thus, we take a more back-of-the-envelope approach to estimating eq. (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/month among anticipators.

Table 5: 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	189.9	150.0
VKT_i^{pre}, VKT_f^{pre}	Pre-period vehicle usage	2020-2023	13,004	22,111
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	35.85	31.24
$\widehat{\beta_i^Q}$, $\widehat{\beta_f^Q}$	Post-period vehicle sales effects (Table 2)	Jul 2018–Jan 2019	-31.41	-27.29
$\widehat{\gamma_i^{VKT}}$, $\widehat{\gamma_f^{VKT}}$	Anticipatory driving effects (Table 3)	2020-2023	145.6	288.4
$\widehat{\beta_i^{VKT}}$, $\widehat{\beta_f^{VKT}}$	Post-period driving effects (Table 3)	2020-2023	-69.4	-864.9
$\widehat{\rho_i^{VKT}}$, $\widehat{\rho_f^{VKT}}$	Steady-state driving effects (Table 3)	2020-2023	90.3	-875.9
$\widehat{\gamma_i^e}$, $\widehat{\gamma_f^e}$	Anticipatory vehicle emission effects (Table 4)	Dec 2017–Jun 2018	2.49	3.92
$\widehat{\beta_i^e}$, $\widehat{\beta_f^e}$	Post-period vehicle emission effects (Table 4)	Jul 2018–Jan 2019	-3.75	-4.57
$\widehat{\rho_i^e}$, $\widehat{\rho_f^e}$	Anticipatory vehicle emission effects (Table 4)	Feb 2019–Jul 2019	-1.11	-3.90
$\widehat{\gamma_i^R}$, $\widehat{\gamma_f^R}$	Anticipatory vehicle retirement effects (Table E7)	Dec 2017–Jun 2018	0.26	-2.48
Panel C: Derived Parameters				
$\widehat{e_i^{ant}}$, $\widehat{e_f^{ant}}$	Vehicle emissions of anticipators (equation F.1)	Dec 2017–Jun 2018	139.10	142.30
$\widehat{e_i^{no\ ant}}$, $\widehat{e_f^{no\ ant}}$	Counterfactual vehicle emissions absent anticipation (equation F.2)	Jul 2018–Jan 2019	135.11	121.83
$\widehat{VKT_i^{ant}}$, $\widehat{VKT_f^{ant}}$	Vehicle traveled of anticipators (equationF.3)	Dec 2017–Jun 2018	13,630	23,207
$\widehat{VKT_i^{no\ ant}}$, $\widehat{VKT_f^{no\ ant}}$	Counterfactual vehicle traveled absent anticipation (equation F.4)	Jul 2018–Jan 2019	13,572	20,910

Thus, the total environmental damages from the additional company vehicles amount

[24]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.

[25]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.

to an estimated $61.57 million when applying a social cost of carbon of $128 per tonne of CO_2.[26] Notably, in terms of both costs of excess adoption and emissions, we see that the damages from firm anticipatory behavior are much more substantial than damages from individual anticipatory behavior. This is because, while we see similar degrees of anticipatory purchases on the extensive margin (see Table 2), the firm anticipators drive more in general, and exhibit larger gaps in driving and vehicle emissions per km between vehicles purchased before the policy vs. after (see tables 3 and 4).

Table 6: Environmental cost decomposition

		Owner type ($)	
	Total ($)	Individual	Firm
Costs			
Excess adoption (ΔE^Q – equation 3)	**28.25**	11.08	17.17
Emissions (ΔE^A – equation 4)	**33.32**	2.88	30.44
Benefits			
Retirement of older cars (ΔE^R – equation 5)	**2.81**	1.2	1.61
Total damage	**58.76**	**12.76**	**46**

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 2021 USD, in millions.

To contextualize the magnitude of the environmental damages generated by announcement effects, we benchmark them against two relevant comparisons: (i) the fiscal cost of the Bonus–Malus policy itself, and (ii) the environmental benefits from inducing adoption of cleaner vehicles. According to the (Ministry of Finance 2018), the allocated budget for the Bonus–Malus system in 2019 amounted to SEK 1.25 billion (approximately $133 million).[27] Relative to this benchmark, the environmental damages induced by anticipatory behavior – estimated at $61.57 million USD – correspond to roughly 46 percent of the total budget allocated to the Bonus–Malus system in 2019.

F. Mechanisms

In this section, we investigate the mechanisms underlying anticipatory vehicle purchases. We consider five channels: (i) dealer behavior, (ii) financial constraints, (iii) information

[26] Our anticipation costs are about half the size of estimates from Rittenhouse & Zaragoza-Watkins (2018) in the trucking sector, though they focus on other pollutants. We estimate a total of 46,000 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.

[27] We convert SEK to US dollars using the exchange rate from January 1, 2020 ($.1063/SEK).

acquisition (Cerruti et al. 2023), (iv) prior fleet characteristics, 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 internal combustion engine vehicles or PHEVs with high emissions in their own name before the July 1st implementation date effectively locked in the lower pre-reform tax rate. These vehicles could then be leased or sold to customers after July 2018 without triggering the higher fee. Because leasing companies typically retain legal ownership of vehicles, they were particularly well positioned to exploit this timing strategy.[28] 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 purchases.

Figure 6 provides direct evidence of dealers' strategic timing. Holding periods—defined as the number of days between a vehicle's initial registration by a dealer or leasing company and its subsequent transfer to an end user—fall sharply in the months before implementation for both individuals and firms. This pattern is consistent with dealers accelerating the handover of already-registered vehicles in order to clear inventory ahead of the policy. After implementation, holding periods rise markedly, reflecting the large stock of vehicles that dealers and leasing companies had pre-registered before July 1 and then sold or leased gradually without incurring the higher tax. Together, these dynamics indicate that dealers actively used pre-registration and delayed transfer to consumers as an arbitrage strategy. While this behavior does not alter the conclusion that substantial anticipatory purchasing occurred, it shows that an important share of the response was intermediated through dealers who shifted registrations forward while maintaining flexibility over the timing of final delivery to end users.

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 only contains list prices (MSRP), so we cannot observe whether dealers offered discounts or negotiated price reductions in response to the policy.[29]

[28] 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).

[29] Figure B4 shows the average list price of new vehicles over time, and there is no obvious trend break around the policy announcement or implementation period. This tells us that there was not a major compositional change in vehicles in response to the policy.

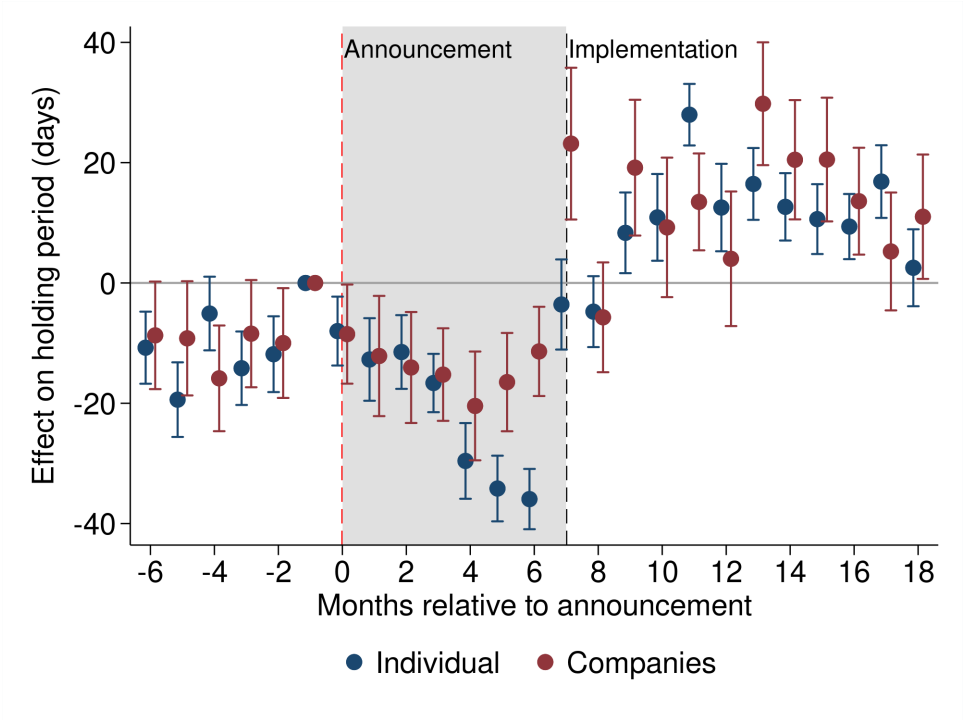


Figure 6: Holding period of dealers

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, another potential mechanism is financial constraints, which may limit the ability of individuals and companies to shift the timing of vehicle adoption in response to the policy change. To explore this channel, we separate individuals into quartiles based on their disposable income levels and companies based on their net revenue. Figure 7 shows the anticipatory (blue), post-policy (red), and net effects (gray) of the Swedish feebate system on two outcomes: new car registrations per 100,000 residents (Panel A) and average vehicle emissions (Panel B) at the municipal level, disaggregated by income quartile. These estimates capture deviations from existing trends rather than absolute pre-policy levels: the point estimates reflect changes relative to each group's underlying trajectory of vehicle sales and emissions. The results reveal a distinct income gradient in response to the policy. High-income individuals (top quartile) exhibit strong anticipatory reactions, registering more vehicles with substantially higher average emissions before the policy came into effect–suggesting greater

capacity to respond to the higher road taxes by adopting new polluting vehicles.[30]

In contrast, individuals in the lowest income quartile display smaller anticipatory changes and are less likely to adopt polluting vehicles in the pre-implementation period. This pattern implies a redistribution of polluting vehicle use from lower- to higher-income groups, potentially undermining the equity and environmental goals of the policy. This divergence may reflect underlying financial constraints. While higher-income groups accelerated adoption of lower-emission vehicles, lower-income individuals showed limited change in vehicle adoption, pointing to challenges in responding flexibly to the policy announcement. This suggests that liquidity or access to flexible financial resources plays a more important role in enabling individuals to respond to the policy by advancing vehicle purchases. Figure E6 shows similar patterns across net-revenue and value-added quartiles for companies, with the largest anticipatory response concentrated in the top quartile. This pattern suggests that firms with greater financial capacity are better able to advance purchases and strategically exploit the timing of the reform to avoid the higher fee. Moreover, these anticipatory adoptions appear to be largely additional rather than purely intertemporal, as the post-policy decline is considerably smaller than the pre-policy surge. By contrast, smaller firms respond primarily after implementation by adopting cleaner vehicles, indicating a stronger adjustment toward low-emission technologies once the policy is in place.

The heterogeneous income and net revenue responses to Sweden's feebate policy matters for two reasons. First, it contributes to the broader discourse on the incidence and distributional effects of feebate policies. Second, it provides the necessary inputs to design group-specific feebate schemes. Such tailored approaches may be particularly relevant for policy discussions, where income-based subsidies have been considered or implemented.

In addition to describing heterogeneous responses by income, we present a series of results that describe how different demographic groups and previous vehicle attributes responded to Sweden's feebate system. Figure E4 presents heterogeneous treatment effects across four additional socio-economic dimensions: age, relationship and parental status, education, and gender. Figure E7 presents heterogeneous responses based on individuals' previous vehicle fleet characteristics—emissions, age, usage, and number of vehicles.

We observe a substantial anticipatory increase in car registrations among individuals with different education levels. Those with higher education tend to purchase more polluting vehicles, whereas less educated individuals are more likely to purchase less polluting cars, suggesting that highly educated individuals were more responsive to the anticipated increase

[30]The dynamic pattern in Figure E2 confirms this income gradient in vehicle adoption: 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 less pronounced response.

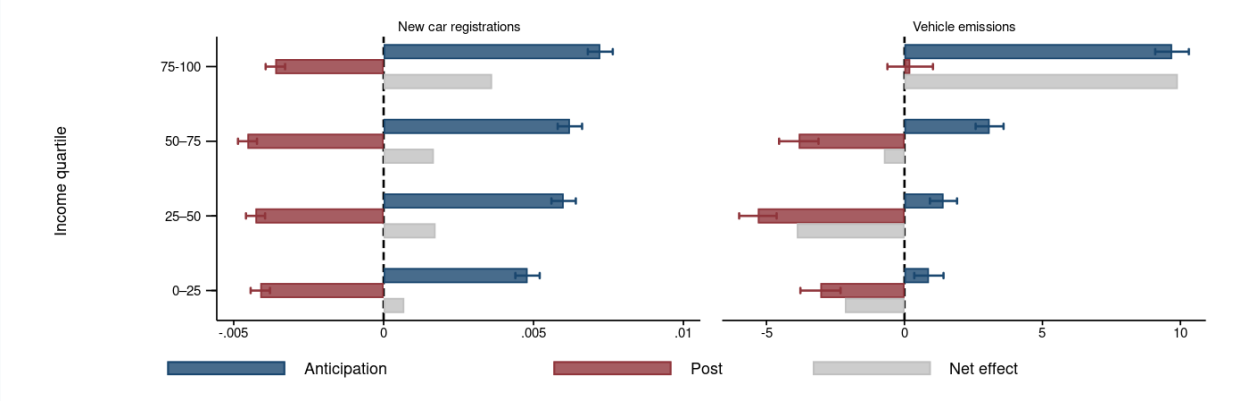


Figure 7: Anticipatory effects by income

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.

in annual road taxes on high-emission vehicles. Similarly, older individuals and males also exhibit clear anticipatory behavior in vehicle registrations, with their choices being associated with higher emission levels. A comparable pattern is observed among individuals with different relationship statuses: those in couples tend to purchase more polluting vehicles than singles, despite both groups displaying a pronounced anticipatory increase in car purchases. Figure E8 illustrates the spatial heterogeneity of anticipatory and post-policy effects across Swedish counties, showing stronger pre-policy increases and post-policy declines in fossil-fuel vehicle registrations in the more urbanized south relative to the less populated north. Overall, these patterns indicate that anticipatory behavior was widespread but not uniformly green—certain demographic groups actively responded to avoid future costs, but did so by accelerating the purchase of higher-emission vehicles.

Third, we examine the information channel. Whether individuals or firms react to a policy announcement depends in part on their awareness of the reform. That high education individuals had larger anticipatory responses is suggestive evidence that the information channel may be relevant. Although we lack micro-level data on information exposure, Google search activity provides suggestive evidence on aggregate attention. As shown in Figure B6, search intensity for bonus–malus rises steadily in the months before implementation, spikes immediately prior to the policy taking effect, and peaks shortly afterward. This pattern indicates that public awareness of the reform increased substantially around the implementation date, suggesting that many potential buyers were actively seeking information about the policy during the anticipation window. We also empirically examine whether interest in

the policy — as proxied by Google search trends for the bonus-malus scheme — is associated with the observed effects.[31] We divide counties into three groups based on the intensity of search activity: low, medium, and high (Figure B7). Table E8 shows that counties with higher search activity tend to exhibit larger increases in total vehicle purchases. However, when comparing anticipatory responses relative to pre-policy purchase levels, the differences across search-intensity groups are modest. This suggests that online search behavior may not be a key driver of anticipatory effects.

A key mechanism driving the anticipation effect is that forward-looking individuals internalize the future tax increase and strategically adjust the timing of their vehicle adoption. The heterogeneity analysis based on prior vehicle fleet characteristics (Figure E7) supports this interpretation. Individuals with high vehicle usage and more emission-intensive fleets—those facing the largest increase in tax burden—display pronounced anticipatory behavior, advancing adoption ahead of the policy's implementation. In contrast, low-usage or cleaner-vehicle owners show limited responsiveness, consistent with weaker incentives to adjust the time of adoption. The pronounced negative post-policy effect among low-usage individuals, or those owning relatively new and cleaner vehicles, further indicates adjustments along the participation margin, as these individuals appear to extend the lifespan of their existing cars rather than adopting new ones.

Another potential mechanism underlying the observed anticipation effects may be the availability of public charging infrastructure. Individuals with greater access to chargers may be less inclined to adopt cars before the policy change and instead adopt plug-in hybrid and battery electric vehicles once the policy takes effect. To test this mechanism, we compare individuals residing in neighborhoods with similar levels of public charging infrastructure during the last quarter of 2017, while stratifying workplace public charger availability into tertiles (Table E9). We find limited differences in the extent of pre-purchasing behavior in new car registrations, particularly in percentage terms, but adoption of new EVs after the policy implementation is increasing in workplace charger availability. This suggests that existing public charging infrastructure may play a limited role in shaping the magnitude of the observed anticipation effects, especially relative to policy compliance.

[31] Google Trends reports a normalized index of search intensity rather than raw search counts. The index measures the share of searches for a given term relative to all Google searches in a region-month and is scaled from 0 to 100 within the selected download window. Because we downloaded all counties jointly in a single query over a fixed time window, the normalization is common across regions, making the index directly comparable across counties.

VI. Conclusion

This paper examines how individuals and firms can circumvent environmental policy when it is announced in advance. In vehicle markets, consumers may bring forward adoption to avoid future costs, bans, or technological constraints. Exploiting the pre-announcement of Sweden's feebate system, we document pronounced anticipatory responses in the timing and composition of new vehicle purchases. These responses are highly heterogeneous: high-income individuals and high–net revenue firms exhibit the strongest shifts, highlighting the central role of financial flexibility in advancing adoption. Anticipatory purchases are mainly petrol and diesel vehicles, which are more emission-intensive. Moreover, the surge in pre-policy adoptions is not fully offset after implementation, implying that anticipation generated additional vehicles rather than merely shifting the timing of adoption. We also show that vehicles adopted in the anticipation window are driven substantially more than those purchased after implementation, further magnifying their environmental impact. Ultimately, this translates into environmental damages on the order of $62 million, equivalent to about half of the government's budget for the program in 2019.

A key mechanism enabling these responses is dealer behavior. We show that dealers substantially increased inventories and strategically pre-registered high-emission vehicles prior to implementation. This practice allowed them to lock in the lower pre-policy tax and subsequently sell or lease these vehicles after implementation without incurring the higher fee. These timing strategies amplified the volume of pre-policy adoptions and effectively expanded consumers' ability to avoid the policy. Taken together, we document intertemporal substitution, a shift toward dirtier vehicles before the policy took effect, an increase in total vehicle adoption, additional driving, and dealer-facilitated pre-registration—all of which contribute to long-run environmental damages and weaken the intended environmental benefits of the feebate policy. These findings underscore that the timing of policy announcements can materially influence behavioral responses and, if not carefully managed, can undermine both the environmental effectiveness and distributional fairness of climate policy. Designing future vehicle regulations requires accounting for anticipatory behavior and the incentives of intermediaries, particularly in markets where purchases are infrequent, durable, and sensitive to policy-induced cost changes.

More broadly, these findings highlight the importance of accounting for anticipation effects in both policy design and evaluation. Methodologically, they underscore that such dynamics can bias causal inference strategies (e.g., difference-in-differences) that assume treatment effects begin only at implementation.

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 – on the one hand, it allows vehicle manufacturers to invest in product R&D so that they can introduce new alternative fuel vehicles and begin to shift their production capacity toward these vehicles, but on the other hand, it also allows consumers to strategically evade the policy by purchasing internal combustion engine vehicles before the bans take effect. While eventual bans on gasoline and diesel vehicles may be a key part of long-term decarbonization efforts, understanding the magnitude and nature of this anticipatory behavior is crucial for designing effective emissions reduction policies. As the world moves toward 2035 and beyond, designing climate policies that are robust to anticipatory behavior will be essential to achieving meaningful emissions reductions.

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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,

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

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,

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

consistent with single-time period intuition.

B. Additional descriptive statistics

B.1. Time trends of vehicle registration

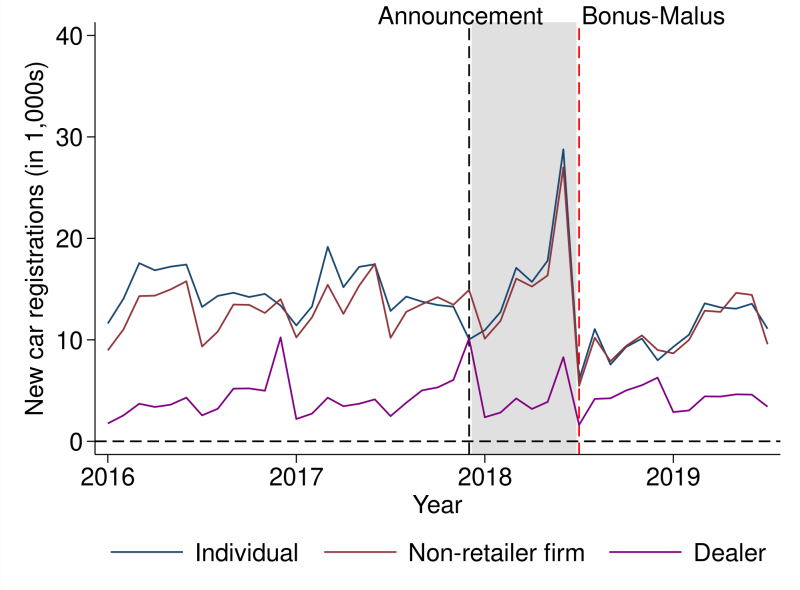


Figure B1: New vehicle registrations by user type

Notes: This figure shows monthly new passenger vehicle registrations (in 1,000s) in Sweden from January 2016 to July 2019, disaggregated by owner type. The blue, brown, and purple lines represent individual owners, non-retailer firms, and car dealers, 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.

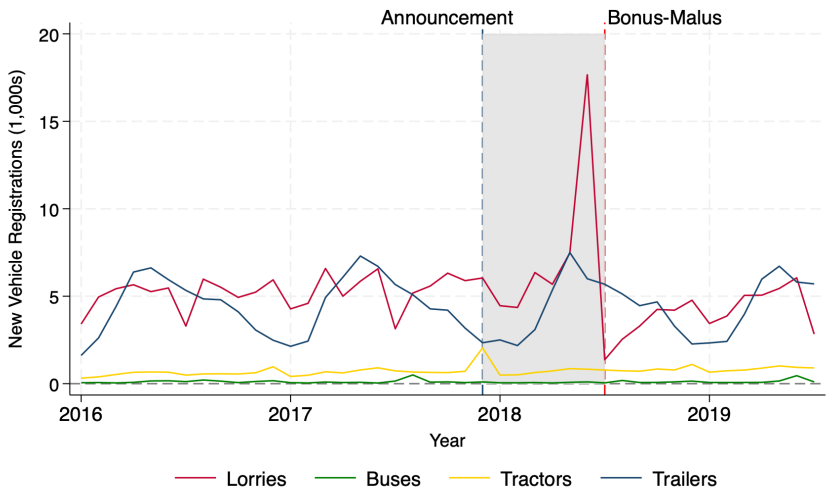


Figure B2: New vehicle registrations by vehicle type

Notes: This figure shows monthly new vehicle registrations (in 1,000s) in Sweden from January 2016 to July 2019, disaggregated by vehicle type. The cranbery, green, gold, and navy lines represent lorries, busses, tractors, and trailors, 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.

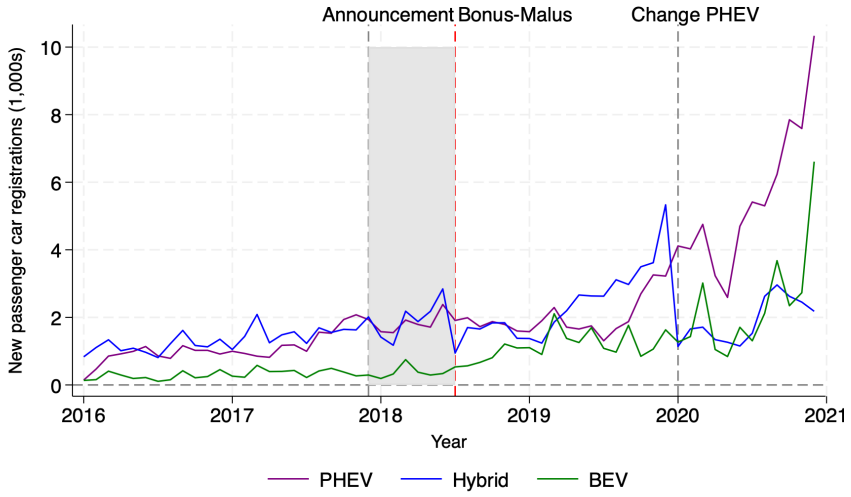


Figure B3: New electric vehicle registrations by powertrain

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.

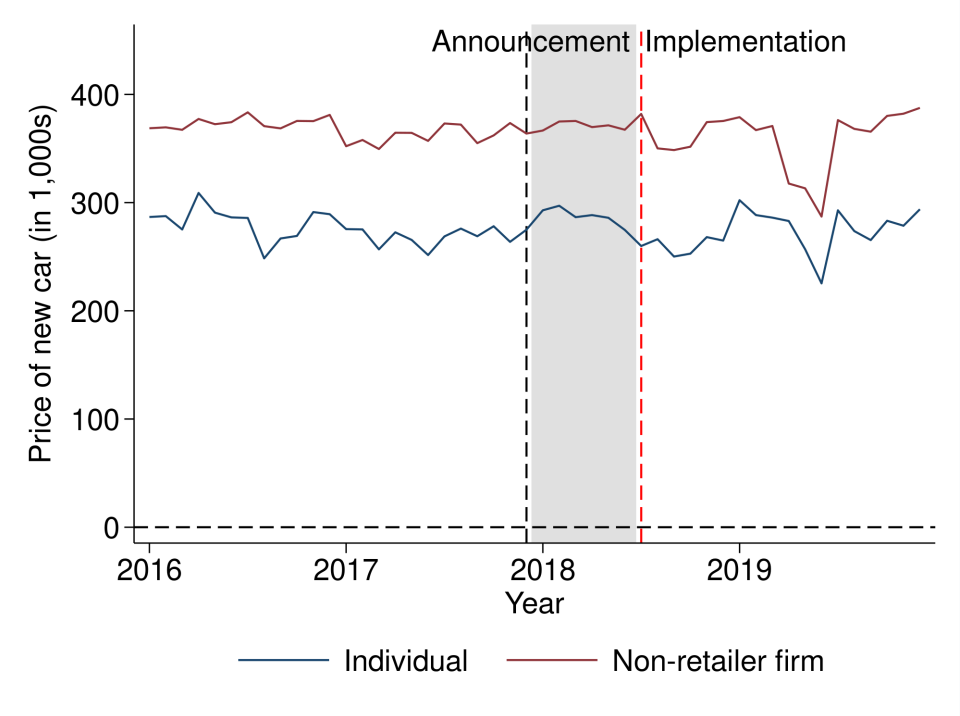


Figure B4: Listing Price of new vehicles

Notes: This figure displays the new listing price of cars adopted by individuals (blue), firms (blue). 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.

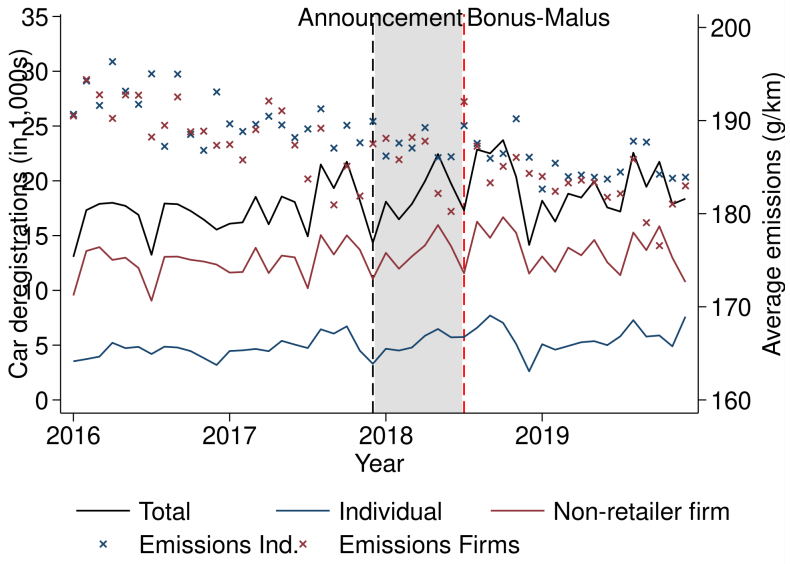


Figure B5: Car de-registrations by owner type

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.

B.2. Google search trends

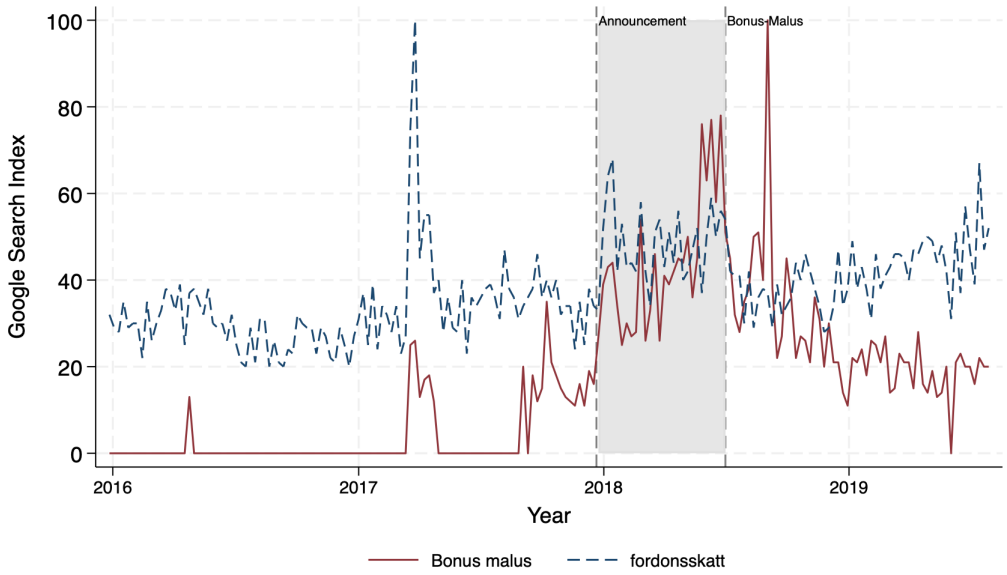


Figure B6: Google trends for bonus malus-related keywords

Notes: This figure shows the google trends of word “bonus-malus" (red) and fordonsskatt (blue) between 2016 and 2020. Vertical dashed red lines mark the timing of the policy announcement (December 2017) and implementation (July 2018), while the grey shaded area denotes the anticipation window between these two events.

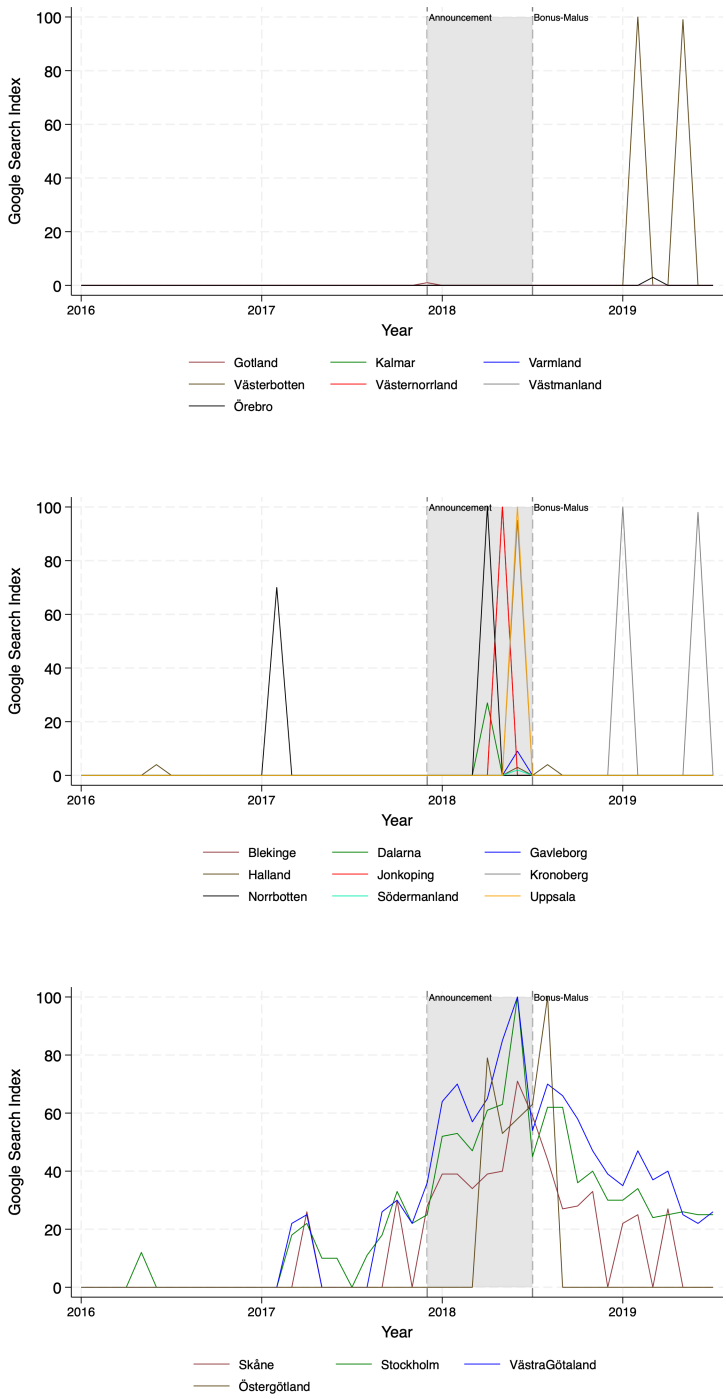
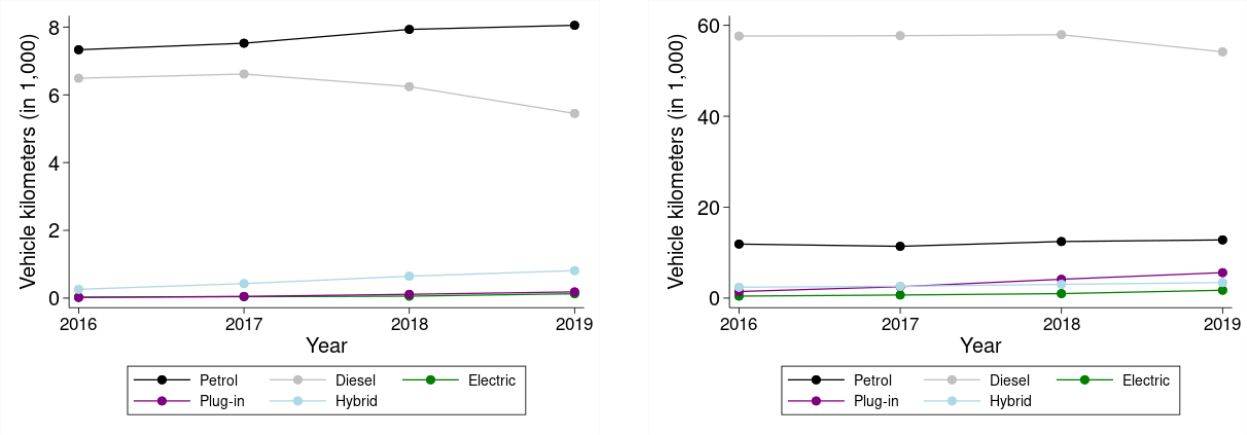


Figure B7: Google search trends by tercile

Notes: This figure shows the Google trends for the phrase "bonus-malus" for counties with low, medium, and high Google search intensity between announcement and implementation period. Vertical dashed red lines mark the timing of the policy announcement (December 2017) and implementation (July 2018), while the grey shaded area denotes the anticipation window between these two events.



(a) Individuals (b) Companies

Figure B8: Driving behavior by fuel type

Notes: This figure presents the average annual vehicle kilometres travelled for five fuel types: petrol (black), diesel (gray), battery electric (green), plug-in hybrid electric (purple), and hybrid (blue) for individual owners (Panel A) and companies (Panel B) over the period 2016 until 2020.

B.3. Balance tests

Table B1: Balance table for individuals

Variable	First Half (2017)	Second Half (2017)	Norm. Diff. (2017)	Norm. Diff. (2018)	Diff (18–17)
Panel A: Demographic characteristics					
At Least 1 Child	0.32	0.33	0.01	0.04	0.02
Disposable Income	318491.30	318692.56	0.00	-0.02	-0.02
Age	52.46	51.98	-0.03	-0.07	-0.03
Gross Salary	344363.59	345193.96	0.00	0.00	0.00
Disposable Income	318491.30	318692.56	0.00	-0.02	-0.02
Unemployment Days	2.93	2.95	0.00	0.01	0.01
Married	0.64	0.64	-0.00	-0.05	-0.05
Years of Education	12.40	12.46	0.03	0.05	0.02
Panel B: Car characteristics					
Total Weight	1987.76	1979.48	-0.02	-0.19	-0.17
Fuel Intensity	5.40	5.43	0.02	-0.12	-0.14
Emission Intensity	125.42	124.61	-0.02	-0.22	-0.20
Particle Value	0.69	0.73	0.06	-0.38	-0.44
Engine Power	107.14	105.47	-0.04	-0.16	-0.12
Fossil Fuel Cars	0.93	0.91	-0.08	-0.21	-0.13
Alternative Fuel Cars	0.07	0.09	0.08	0.21	0.13

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. The Norm. Difference (2018) column reports the differential between the second and first half of 2018, estimated relative to the average first-half versus second-half differences in 2018.

Table B2: Balance table for companies

Variable	First Half (2017)	Second Half (2017)	Norm. Diff. (2017)	Norm. Diff. (2018)	Diff (18–17)
Panel A: Company characteristics					
Value Added	85558.28	94802.36	0.02	0.06	0.04
Net Revenue	349183.73	392845.40	0.01	0.05	0.04
Number of Employees	99.33	111.24	0.02	0.07	0.05
Panel B: Car characteristics					
Total Weight	2283.75	2282.48	-0.00	-0.10	-0.10
Fuel Intensity	5.23	5.27	0.03	-0.19	-0.22
Emission Intensity	123.99	120.78	-0.08	-0.29	-0.21
Particle Value	0.48	0.48	0.01	-0.26	-0.27
Engine Power	131.70	130.27	-0.03	-0.09	-0.06
Fossil Fuel Cars	0.75	0.70	-0.10	-0.21	-0.10
Alternative Fuel Cars	0.12	0.17	0.14	0.26	0.12

Notes: This table reports the covariate balance of demographic characteristics (Panel A) and vehicle attributes (Panel B) for comapnies 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. The Norm. Difference (2018) column reports the differential between the second and first half of 2018, estimated relative to the average first-half versus second-half differences in 2018.

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.[32] 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.

[32] Based on bonus information from *Transportstyrelsen*, low emission vehicles ordered after 8 Noverember is 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

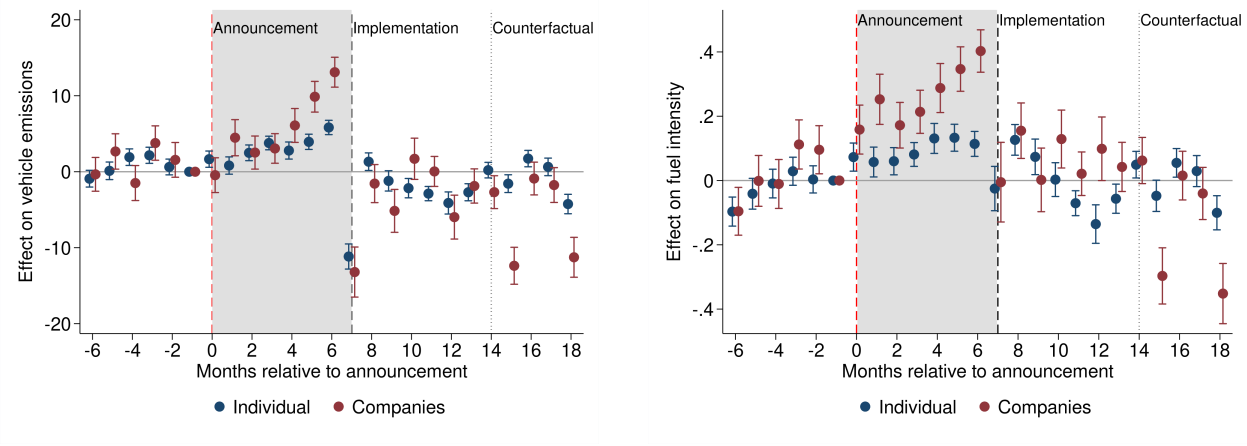
To examine heterogeneity in responses to Sweden's feebate policy, we assess variations in new car registrations and average vehicle emissions at individual level. The analysis is stratified along several socioeconomic dimensions—including income, age, number of children, educational attainment, and gender—as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

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

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

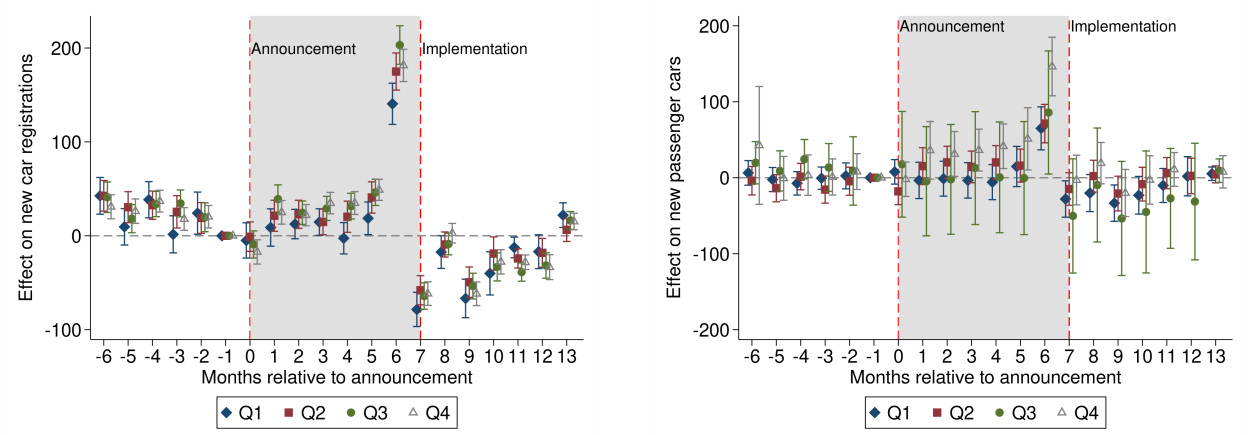
E.1. Dynamic Effect



(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.

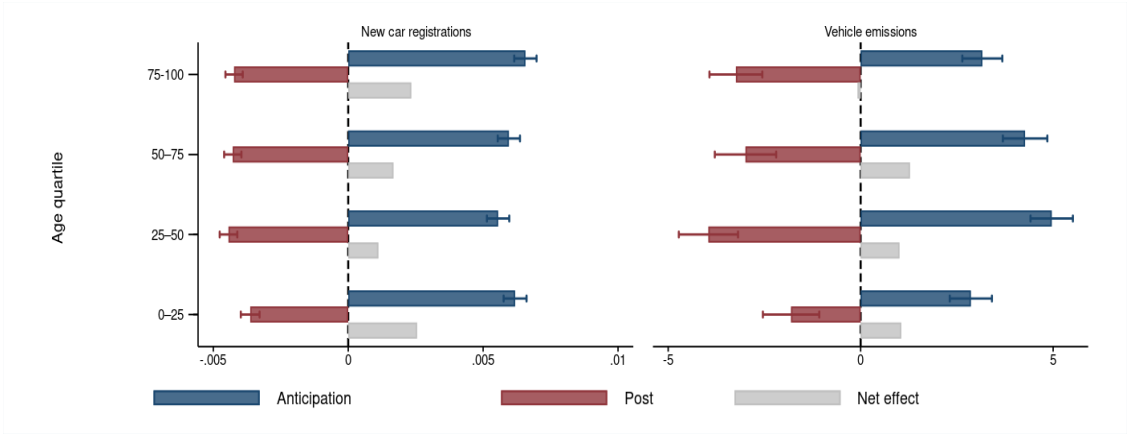


(a) Income by individuals

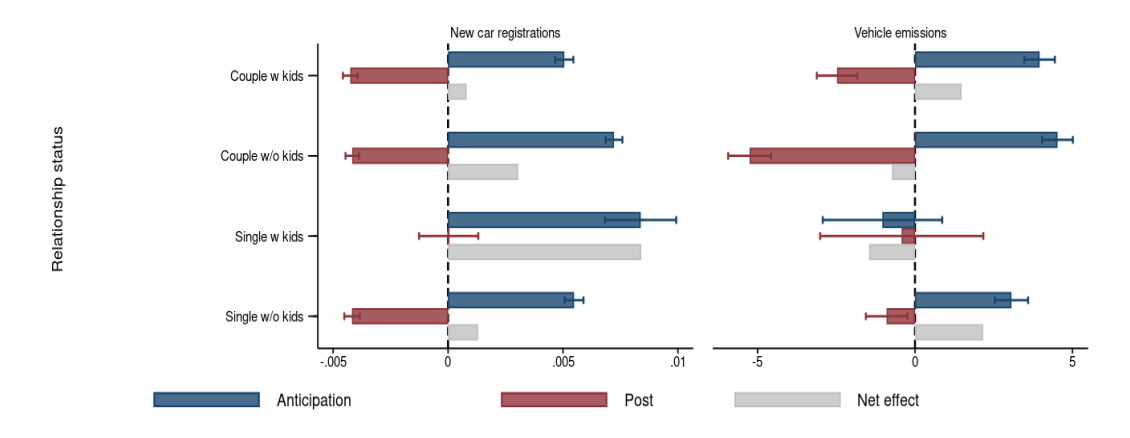
(b) Revenue by companies

Figure E2: Dynamic effects by income and revenue group

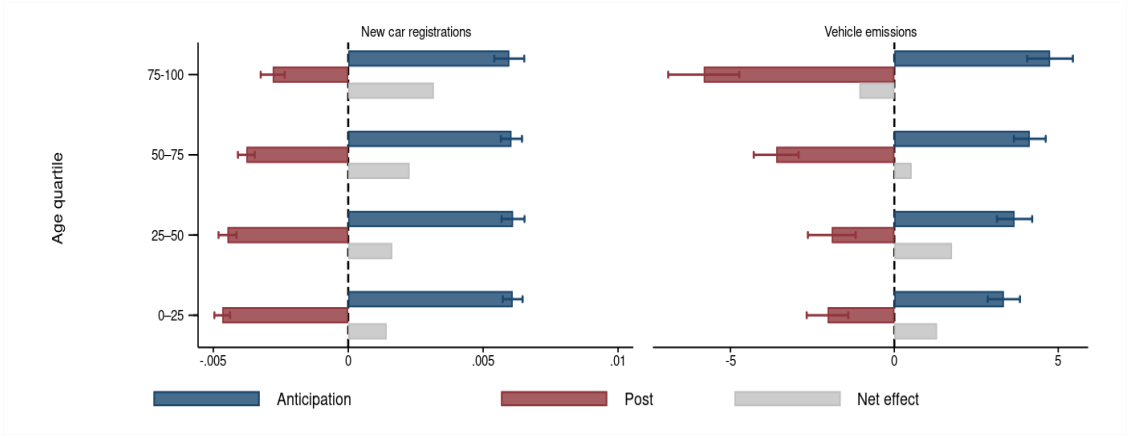
Notes: This figure displays the dynamic effects of the policy on car registrations per 100,000 people disaggregated by four income quartiles for individuals and four net revenue quartiles for companies, measured monthly relative to the policy announcement. Vertical dashed red lines mark the timing of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



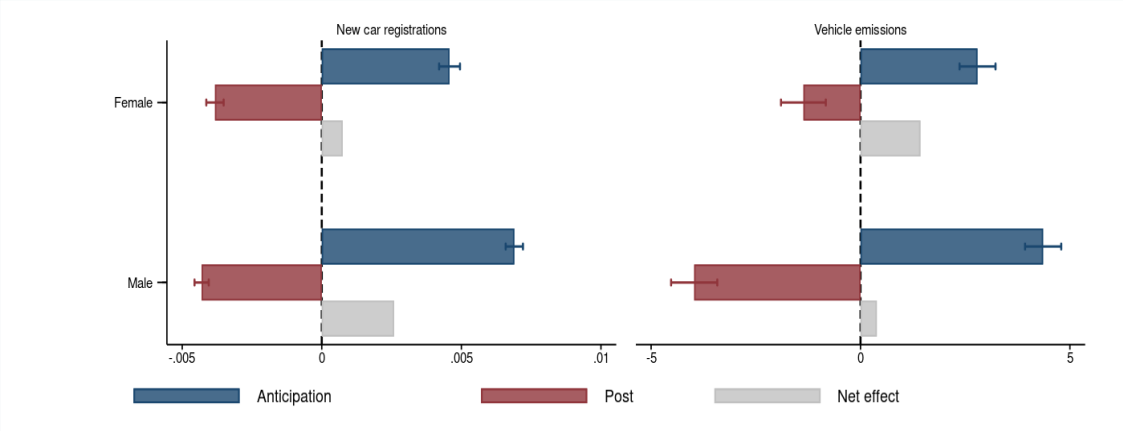
(a)Age



(b) Relationship status and children



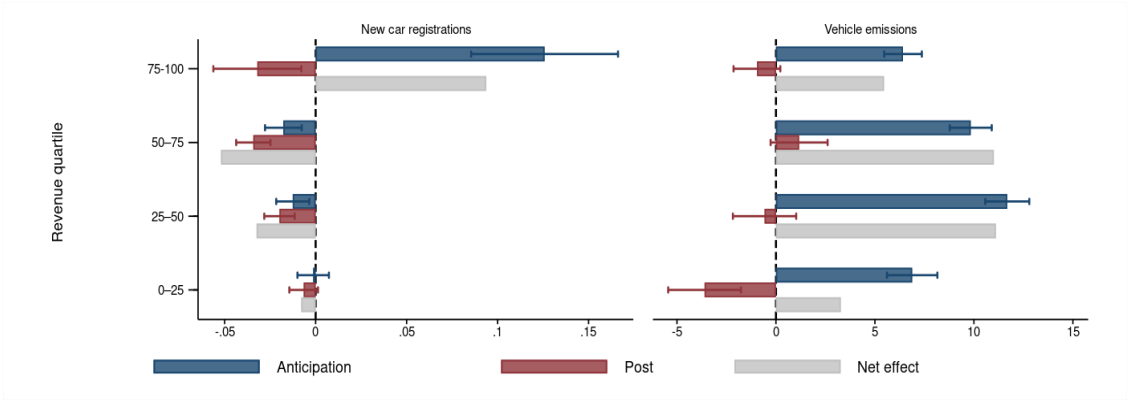
(c) Years of education



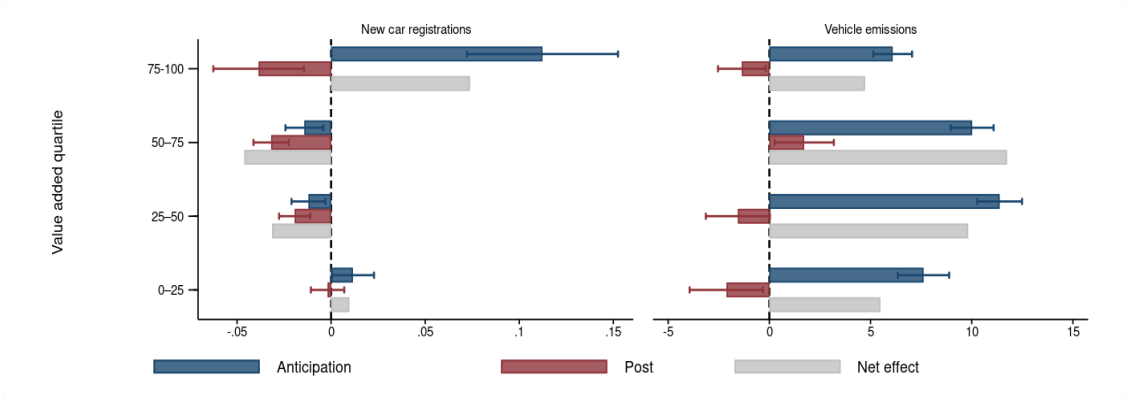
(d) Female

Figure E4: Anticipatory effects by demographics

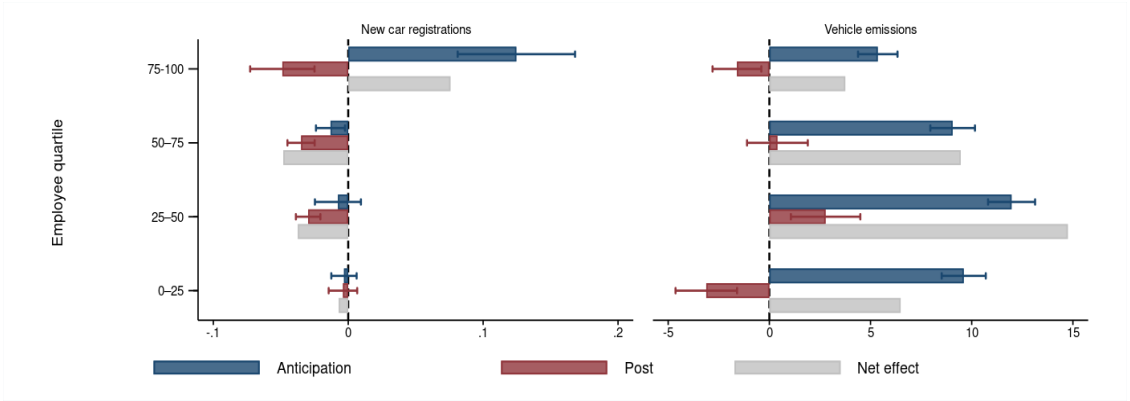
Notes: This figure plots the anticipation (blue), post (red), and net effect (gray) of the Swedish feebate system on new car registrations and vehicle emissions by demographic groups. Panel (a) shows four age quartiles based on 2017 demographics; panel (b) compares singles and couples with and without 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) Net revenue



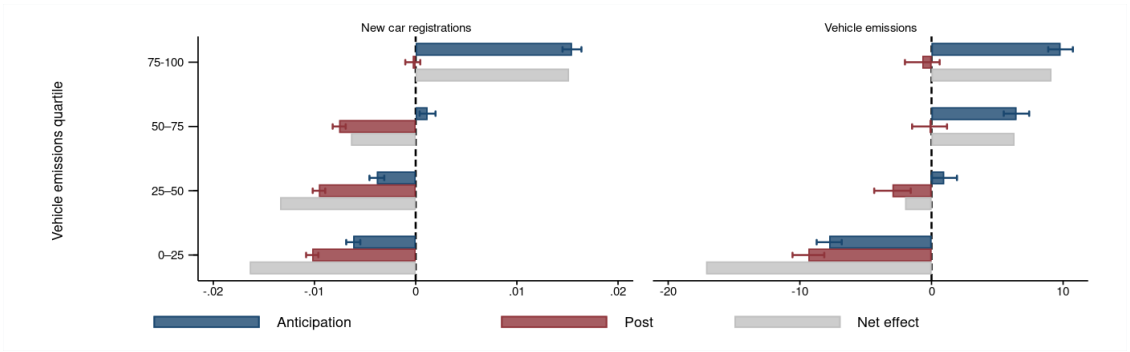
(b) Added value



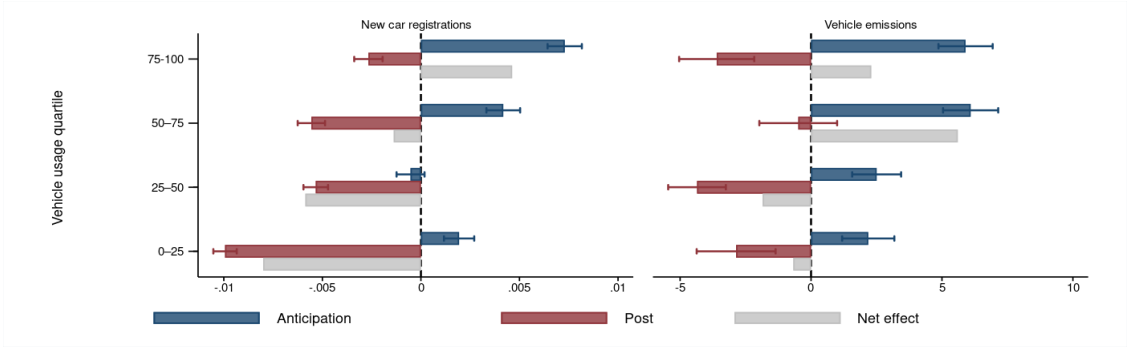
(c) Number of employee

Figure E6: Anticipatory effects by firm characteristics

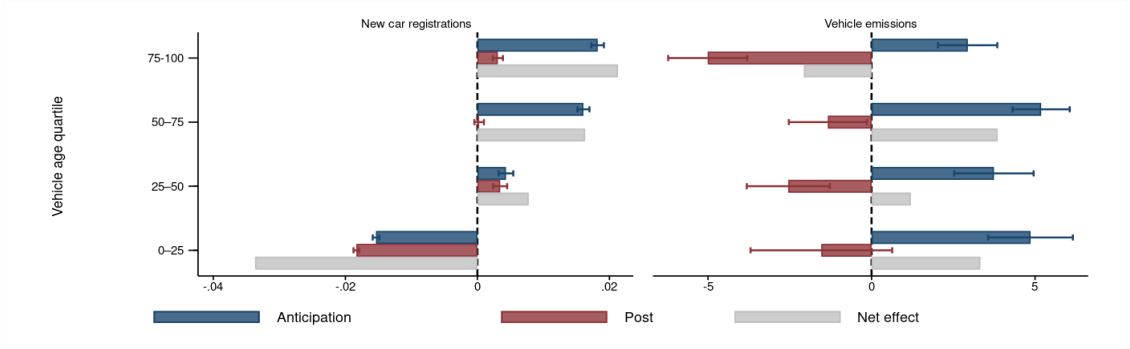
Notes: This figure plots the anticipation (blue), post (red), and net effect (gray) of the Swedish feebate system on new car registrations and vehicle emissions by firm characteristics. Panel (a) reports quartiles based on firms' net revenues in 2017; Panel (b) compares firms' value added; and Panel (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.



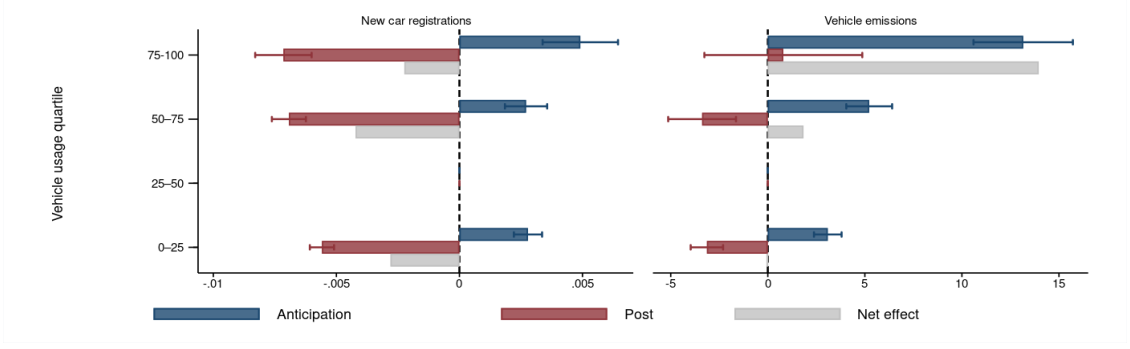
(a)Emission



(b) Vehicle usuage



(c) Vehicle age



(d) Number of Vehicles

Figure E7: 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; Panel (c) presents results by previous vehicle age; and Panel (d) by previous number of vehicles. The underlying regression specification follows equation (D.1). Whiskers indicate 95% confidence intervals based on robust standard errors clustered at the municipality level.

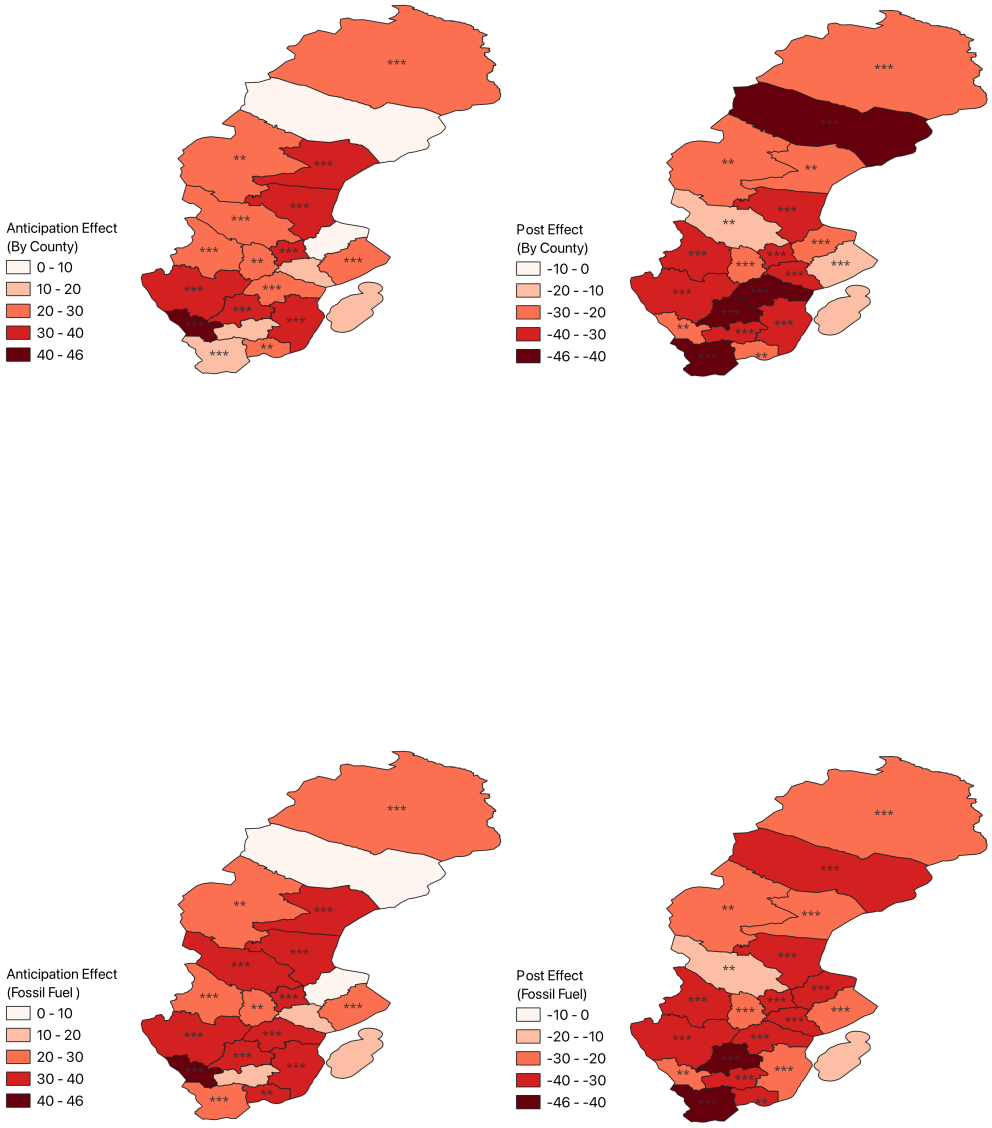


Figure E8: Anticipation and post-policy effects by county

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.

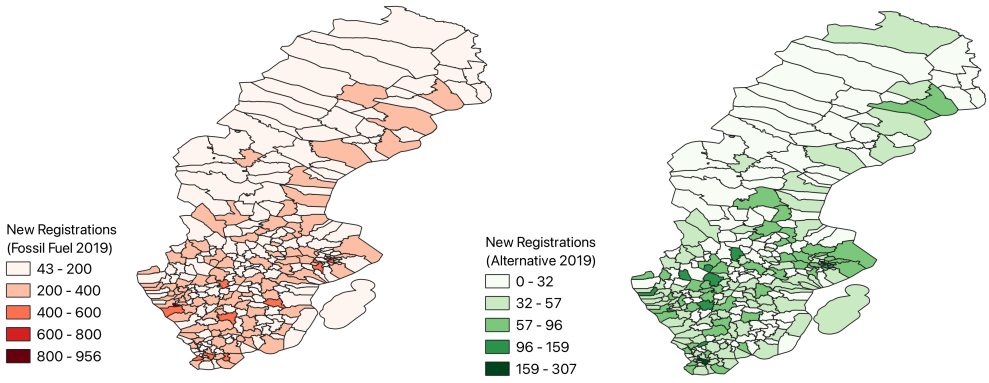


Figure E9: New Vehicle Registration per 100,000 by fuel type

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	65.86***	44.47***	19.53***	-0.53	2.32**
	(9.91)	(4.92)	(4.58)	(1.06)	(1.04)
Post	-59.43***	-20.83***	-35.30***	2.75*	-3.98***
	(6.94)	(3.12)	(3.75)	(1.41)	(0.57)
%-Pre effect	23.45	35.53	16.3	-3.3	14.59
%-Post effect	-21.17	-16.64	-29.46	16.97	-24.99
Controls	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
Observations	255,385	255,385	255,385	255,385	255,385

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: 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	30.37***	13.31***	.08***
	(5.29)	(.54)	(.02)
Post	-27.60***	-10.81***	-.12***
	(3.97)	(.46)	(.02)
%-Pre effect	25.71	11.26	10.45
%-Post effect	-23.37	-9.15	-17.19
Controls	Y	Y	Y
Month FE	Y	Y	Y
Observations	257312	257312	257343

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 E3: Effect on second-hand car registrations

	Type of used vehicle				
	(1) Used cars	(2) Petrol	(3) Diesel	(4) EVs	(5) Hybrid
A. Individuals					
Pre	2.17	1.39	0.95	-0.15***	-0.41***
	(2.01)	(1.71)	(0.89)	(0.04)	(0.15)
Post	-1.81	-4.35**	1.16	-0.10**	-0.29**
	(2.01)	(1.70)	(0.87)	(0.04)	(0.14)
%-Pre effect	.32	.29	.63	-45.11	-9.21
%-Post effect	-.27	-.9	.76	-30.24	-6.43
B. Companies					
Pre	3.98**	2.05*	1.66**	-.05	.22
	(1.74)	(1.17)	(.71)	(.12)	(.13)
Post	-.47	-.22	-.03	.12	-.01
	(1.37)	(.85)	(.70)	(.13)	(.10)
%-Pre effect	5.10	5.59	4.73	-4.97	22.48
%-Post effect	-.6	-.6	-.1	12.36	-.92
Controls	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
Observations	255,397	255,397	255,397	255,397	255,397

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 E4: Estimates for different post-treatment definitions

	Post-coefficient			
	(1) Jan 2019	(2) Feb 2019	(3) Mar 2019	(4) Apr 2019
A. Individuals				
Pre	38.06***	37.94***	35.53***	34.75***
	(0.95)	(0.99)	(1.02)	(1.08)
Post	-26.47***	-22.90***	-24.49***	-22.60***
	(0.78)	(0.78)	(0.80)	(0.85)
B. Companies				
Pre	34.92***	34.58***	32.88***	31.85***
	(4.32)	(4.53)	(4.67)	(4.95)
Post	-23.93***	-21.25***	-21.89***	-20.76***
	(3.55)	(3.59)	(3.75)	(4.01)
Controls	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Observations	255,571	255,571	255,571	255,571

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 E5: Effect on log and total new car registrations

	Outcome definition		
	(2) Registrations per 100k	(2) Log registrations	(3) Total registrations
A. Individuals			
Pre	35.85***	0.15***	0.51***
	(0.96)	(0.00)	(0.01)
Post	-31.41***	-0.15***	-0.44***
	(0.80)	(0.00)	(0.01)
B. Companies			
Pre	34.13***	.10***	.51***
	(4.29)	(.01)	(.07)
Post	-23.57***	-.10***	-.35***
	(3.50)	(.01)	(.06)
Controls	Y	Y	Y
Month FE	Y	Y	Y
Observations	255,397	105,932	255,397

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 E6: Effect by adoption type

	Registrations by adoption type		
	(1) Purchase	(2) Lease	(3) Share of lease
A. Individuals			
Pre	31.11***	4.73***	-0.03***
	(0.82)	(0.47)	(0.00)
Post	-23.49***	-7.92***	0.02***
	(0.68)	(0.42)	(0.00)
B. Companies			
Pre	9.57***	24.56***	-.00
	(2.86)	(3.10)	(.00)
Post	-6.62***	-16.95***	.00
	(2.19)	(2.68)	(.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 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: Regression by google search trends

	Google Search Index		
	(1) Low	(2) Medium	(3) High
Pre	22.256***	24.755***	28.295***
	(4.230)	(3.342)	(3.055)
Post	-33.973***	-31.983***	-34.394***
	(3.644)	(2.892)	(2.675)
%-Pre effect	17.64	18.37	17.48
%-Post effect	-26.92	-23.73	-21.25
Mean	126.19	134.79	161.84
Controls	Y	Y	Y
Month FE	Y	Y	Y
Observations	3139	3784	5203

Notes: This table reports the main regression results for counties grouped by low, medium, and high Google search intensity for the term bonus-malus during the policy announcement period. Counties are classified into these groups based on their cumulative Google Search Intensity, measured as the sum of the normalized Google Search Index values for bonus-malus over the announcement window. The outcome variables indicate the number of new car registrations per 100,000 residents. All regressions include individual demographic, macroeconomic control variables, as well as municipality-fixed effects and month-fixed effects. All coefficients are reported in municipality×month. The percentage effects and the monthly number of new car registrations is reported beneath the coefficients. Robust standard errors are in parentheses. *, **, ***: statistically significant with 90%, 95%, and 99% confidence, respectively.

Table E9: 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	33.11***	-1.04***	37.68***	-0.81***	38.75***	-0.68***
	(1.77)	(0.24)	(1.80)	(0.26)	(1.75)	(0.24)
Post	-33.03***	1.22***	-31.28***	2.03***	-32.94***	2.70***
	(1.47)	(0.27)	(1.54)	(0.29)	(1.51)	(0.28)
%-Pre effect	19.22	-36.24	20.53	-25.65	22.49	-23.72
%-Post effect	-19.17	42.45	-17.04	63.93	-19.12	93.76
Controls	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
Observations	85784	85784	85792	85792	85760	85760

Notes: This table reports the pre- and post-policy regression estimates from equation (1) for individuals with few, medium, and many workplace charger by tertile on number of all new car registrations and all new electric cars per 100,000 residents in the respective geographic area. All regressions include average demographic characteristics, the number public charger 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 three unobserved sub-groups before or after the feebate policy:

1. Emissions from vehicles purchased in anticipation of the policy, e_t^{ant}
2. Emissions from vehicles that would have been purchased by avoiders if they had been surprised by the policy and could not make anticipatory purchases, $e_t^{no\ ant}$
3. Emissions from vehicles that were retired early in the pre-period due to anticipatory purchases

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 policy and the anticipatory (and excess anticipatory) purchases. We can calculate the predicted baseline vehicle sales ($\widehat{Q^{base}}$) as the observed quantity during the pre-period, Q^{pre}, less the effect of the policy in the pre-period, $\widehat{\gamma_Q}$ from eq. (1). Likewise, predicted baseline emissions $\widehat{e^{base}}$ equal the observed emissions during the pre-period $e^{pre} - \widehat{\gamma_e}$ from eq. (1) when emissions

are the outcome variable. 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), $\widehat{e^{ant}}$ and $e^{ant,ex}$:

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

If we assume that the average emissions of intertemporal anticipators and excess anticipators are the same, this can be rearranged to yield an expression for e^{ant}:

$$e^{ant} = \frac{e^{pre} \times Q^{pre} - Q^{base} \times e^{base}}{Q^{ant\,tot}} = \frac{e^{pre} \times Q^{pre} - (Q^{pre} - \widehat{\gamma_Q}) \times (e^{pre} - \widehat{\gamma_e})}{\widehat{\gamma_Q}} \tag{F.1}$$

This expression isolates the average emissions of vehicles purchased in anticipation of the policy.

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 ancitipators' post-policy emissions when they cannot make anticipatory purchases $e^{no\ ant}$:

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

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:

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

F.2. Deriving unobserved vehicle kilometers traveled

We calculate the kilometers traveled by calculating VKT^{ant}, the vehicle kilometers traveled by anticipators, and $VKT^{no\ ant}$, the vehicle kilometers traveled by anticipators when they cannot anticipate (i.e., those who purchased vehicles during the steady state period), using equations analogous to the emissions calculations. We also estimate a version of equation (1) where outcome variable is annual vehicle kilometers traveled for new vehicles, and calculate empirical VKT^{pre}, VKT^{post}, γ^{VKT} and β^{VKT}.

$$VKT^{ant} = \frac{VKT^{pre} \times Q^{pre} - (Q^{pre} - \widehat{\gamma_Q}) \times (VKT^{pre} - \widehat{\gamma^{VKT}})}{\widehat{\gamma_Q}} \tag{F.3}$$

$$\begin{aligned} VKT^{no\ ant} = & \frac{\left[(VKT^{pre} - \widehat{\gamma}^v) + \widehat{\rho}^v\right] \times (Q^{pre} - \widehat{\gamma}^Q)}{-\widehat{\beta}_Q} \\ & - \frac{\left[(VKT^{pre} - \widehat{\gamma}^v) + \widehat{\beta}^v\right] \times \left[(Q^{pre} - \widehat{\gamma}^Q) + \widehat{\beta}_Q\right]}{-\widehat{\beta}_Q} \end{aligned} \tag{F.4}$$

We use the parameters reported in Table 5 to recover the above unobserved variables and the value of these variables are reported in the same Table 5.

F.3. Implementation of environmental estimates

This section outlines how environmental cost and benefit estimates for individuals and firms are constructed for the decomposition in Table 6, using parameters from Table 5.

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

$$\Delta E^Q = 98 \times 7 \times Q^{ant,ex} \times \sum_{h=1}^{H} \frac{1}{1+r}^{h} e^{ant,ex} \times VKT_h^{ant,ex}$$

$$\Delta E_i^Q = 98 \times 7 \times (35.85 - 31.41) \times \sum_{h=1}^{18} \frac{1}{1+0.02}^{h}$$

$$\times 139.10 \ \frac{gCO_2}{km} \times 13{,}630 \ km \times \frac{1 \text{ ton}}{10^6 \text{ g}} \approx 86{,}572 \text{ tCO}_2,$$

$$\Delta E_f^Q = 98 \times 7 \times (31.24 - 27.29) \times \sum_{h=1}^{H} \frac{1}{1+r}^{h}$$

$$\times 142.30 \ \frac{gCO_2}{km} \times 23{,}207 \ km \times \frac{1 \text{ ton}}{10^6 \text{ g}} \approx 134{,}159 \text{ tCO}_2,$$

$$\Delta E^Q = \Delta E_i^Q + \Delta E_f^Q = 220{,}731 \text{ tCO}_2$$

Emissions from anticipators. To quantify emissions from vehicles purchased by anticipators between the policy announcement and implementation, we consider two counterfactual channels. First, anticipatory vehicles are more emissions-intensive than the vehicles that would have been purchased in the absence of anticipation. Second, anticipatory vehicles are driven more intensively than they would have been under the steady-state policy regime absent anticipation. We therefore recover vehicle kilometers traveled for anticipatory vehicles and construct the corresponding no-anticipation counterfactual in which anticipators are unable to anticipate the policy. Using the parameters from Table 5, we calculate the emission for individuals and firms as follow:

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

$$\Delta E_i^A = 98 \times 7 \times 35.85 \times \sum_{h=1}^{18} \frac{1}{1+0.02}^{h} \times (139.10 \ \frac{gCO_2}{km} \times 13,630 \ km$$

$$- \ 135.11 \ \frac{gCO_2}{km} \times 13,572 \ km) \times \frac{1 \text{ ton}}{10^6 \text{ g}} \approx 22,470 \text{ tCO}_2$$

$$\Delta E_f^A = 98 \times 7 \times 31.24 \times \sum_{h=1}^{18} \frac{1}{1+0.02}^{h} \times (142.30 \ \frac{gCO_2}{km} \times 23,207 \ km$$
$$- \ 121.83 \ \frac{gCO_2}{km} \times 20,910 \ km) \times \frac{1 \text{ ton}}{10^6 \text{ g}} \approx 237,848 \text{ tCO}_2,$$

$$\Delta E^A = \Delta E_i^A + \Delta E_f^A = 260{,}318 \text{ tCO}_2.$$

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).

$$\Delta E_i^R = 98 \times 7 \times 35.85 \times (187.1 \ \frac{gCO_2}{km} - 139.1 \ \frac{gCO_2}{km})$$
$$\times \frac{13,630 \ km}{12} \times 7 \times \frac{1 \text{ ton}}{10^6 \text{ g}} \approx 9,386 \text{ tCO}_2,$$

$$\Delta E_f^R = 98 \times 7 \times 31.24 \times (185.7 \ \frac{gCO_2}{km} - 142.30 \ \frac{gCO_2}{km})$$
$$\times \frac{23,207 \ km}{12} \times 7 \times \frac{1 \text{ ton}}{10^6 \text{ g}} \approx 12,591 \text{ tCO}_2,$$

$$\Delta E^R = \Delta E_i^R + \Delta E_f^R = 21{,}977 \text{ tCO}_2.$$

To express emissions in monetary terms, we multiply vehicle-level lifetime emissions by the Swedish carbon tax of \$128 per ton of CO_2, which we use as a proxy for the social cost of carbon. This calculation yields estimated environmental damages of \$28.25 million from excess anticipators and \$33.32 million from anticipators. In total, policy anticipation generates approximately \$61.57 million in environmental damages. Using a back-of-the-envelope approach, we estimate that the upper bound of the environmental benefits from early vehicle retirement induced by anticipatory purchases is \$2.81 million. Netting out these benefits, the policy announcement results in a net environmental cost of approximately \$58.76 million.