Clean Cars, Dirty Margins: Portfolio Leakage and Electric Vehicle Subsidies*

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Abstract

Economists view Pigouvian pricing as central to efficient climate policy, yet transportation policy relies heavily on subsidies that misalign incentives across behavioral margins. We study the emissions consequences of Sweden's 2020 increase in plug-in hybrid vehicle (PHEV) subsidies. Using rich nation-wide administrative data and a fuzzy regression discontinuity design based on pre-determined lease-renewal timing, we estimate how exogenous subsidy exposure affects entire household vehicle portfolios and, importantly, driving behavior. Treated households choose lease-renewal vehicles that are 7.3 percent less fuel-intense, but retain vehicles that are 3.9 percent more fuel-intense, consistent with attribute substitution across cars. Driving responses offset as well: replacement vehicles are driven less, retained vehicles more. Consequently, the policy yielded a small and statistically insignificant decrease in total gasoline consumption. These portfolio effects are concentrated almost entirely among multi-vehicle households, who increase retained-car driving by 1,228 kilometers per year and retain dirtier vehicles — effects that are statistically indistinguishable from zero for single-vehicle households. Households with three or more cars expand their portfolio size. Implied carbon abatement costs of the policy average $1,061 per ton of CO_2 on a 100 percent clean electric grid, and we reject costs below $200 per ton at conventional levels. Accounting for within-household substitution substantially revises downward the direct environmental benefits of EV subsidies relative to standard assumptions.

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I. Introduction

Economists widely agree that Pigouvian prices are the linchpin of optimal climate policy, yet actual climate policy is typically comprised of subsidies, regulations and mandates. The main consequence of this distinction is that while Pigouvian prices align private and social incentives on most behavioral margins, implemented policies create an array of incentives that are misaligned with, and therefore likely to undermine, emissions reduction goals. Many climate policies operate through durable goods – for example, vehicles, appliances, industrial equipment – whose environmental consequences depend on both what is purchased and how intensively it is used. Also, in these settings, policy-induced changes in one part of a portfolio can reshape behavior elsewhere. A subsidy that encourages adoption of a cleaner technology may change what older capital is retained, how tasks are allocated across assets, and the intensity with which different assets are operated.

One critical sector that exhibits this distinction is transportation, where worldwide electric vehicle (EV) subsidies reached $41 billion in 2024 (International Energy Agency 2025). The incentives created by these subsidies differ from those of a carbon price since they do not raise the price of buying or driving conventional, higher emitting cars. Nonetheless, many models of decarbonization emphasize large emissions reductions from adoption of electric vehicles, typically through subsidies. These models often rely on undisciplined parameters of substitution in household vehicle portfolios and driving behavior (Knobloch et al. 2020; Keshavarzmohammadian et al. 2017).

Optimal second-best subsidy depends not only on the level of unpriced externalities but, importantly, on the substitution patterns between clean and dirty goods (e.g., Sallee 2025). In the EV context, relevant substitution margins arise from the fact that households typically own multiple cars. An accurate estimate of the environmental benefits generated by an EV subsidy must therefore account for several substitution margins. Does a subsidy-induced EV alter the composition of the vehicle portfolio? Is the EV driven as intensely as the car it displaced, and how does the choice affect the intensity of driving in other, potentially less energy efficient, cars in the household portfolio? Answers to these questions have eluded researchers for two main reasons. First, they require credibly exogenous variation in the level of EV subsidies; and, second, since such variation typically exists in household-level microdata, the researcher must observe household vehicle portfolio composition that includes time-varying usage for each car, which is rare.

To guide interpretation of the empirical results, we develop a simple model of household vehicle portfolio choice. A household entering the lease-renewal period with $N \geq 1$ vehicles chooses a new vehicle and a retention set simultaneously, then allocates driving

across the resulting portfolio. The model yields a decomposition of the total gasoline response into four margins: new vehicle fuel intensity, new vehicle driving, retained fleet composition, and retained fleet driving. Standard single-car analyses account only for the first two. Crucially, the model generates predictions that differ by pre-policy fleet size: for single-vehicle households the retained fleet margins are zero or theoretically ambiguous by construction, while for multi-vehicle households both are generically active. Because the subsidy lowers the cost of acquiring a cleaner vehicle without raising the cost of retaining existing ones, the model predicts that larger households will tend to expand their fleet rather than recompose it, the mirror image of what a Pigouvian carbon price would induce. These predictions are directly testable in our data.

While several papers estimate the effects of EV subsidies on vehicle choice (e.g., Xing et al. 2021; Muehlegger and Rapson 2022, 2023), and others examine EV usage patterns after adoption (e.g., Davis 2019; Tal et al. 2019; Burlig et al. 2021; Chakraborty et al. 2022), this paper is the first to estimate how EV subsidies affect the composition of the entire household vehicle portfolio and the allocation of driving across all vehicles in that portfolio. Following Tebbe (2023), we exploit the pre-determined timing of fixed-term leases in a regression discontinuity design that creates quasi-experimental exposure to different EV subsidy regimes across otherwise similar households. Swedish administrative data allows us to observe lease status and renewal timing alongside vehicle attributes and annual kilometers traveled for every vehicle owned by a household. This enables us to quantify the extensive margin of vehicle choice, the retention margin within the household fleet, and the intensive margin of driving reallocation. Using this approach, we estimate relevant margins of households' responses to an exogenous change in EV subsidies, and use the estimates to calculate the implied tons of carbon dioxide (CO_2) abated.

Sweden introduced a vehicle "feebate" system – which taxes high-polluting and subsidizes low-polluting cars – in July 2018 to accelerate the adoption low-emission vehicles. Our research design exploits a policy change in January 2020 that increased subsidies for plug-in hybrid electric vehicles (PHEVs). This policy change provides time-series variation in relative car prices that is, by itself, inadequate to support credible causal inference. However, we combine this variation with a key feature of the Swedish car market: a large share of people lease their vehicles and replace them on a fixed three-year cycle. Our identification strategy exploits the timing of, and constraints associated with, leasing contract renewals for households reaching the end of their three-year lease period just before or after the subsidy regime change.

To identify the causal effects of the policy, we employ a fuzzy regression discontinuity design that compares vehicle choice and driving behavior of households whose lease

renewal occurred just after the increase in the PHEV subsidy (treated group) to those whose renewal took place just before the policy change (control group). Assignment to treatment or control is therefore a function of decisions that the household made three years prior. A key advantage of this approach is that lessees could not have anticipated the policy change when initiating their lease, and deviations from the fixed lease schedule entail significant costs.

The paper presents several key results consistent with its main objective of obtaining a full accounting of the household vehicle emissions effects of adopting a subsidy-induced PHEV.[1] First, regarding vehicle choice, treated households select lease-replacement vehicles that are 7.3 percent *less* fuel-intense, on average, than those selected by control households. Vehicles that are retained by treated households are, on average, 3.9 percent *more* fuel-intense than those retained by control households. This result is consistent with "attribute substitution" across cars in a household vehicle portfolio, similar to the effect of fuel economy improvements documented in Archsmith et al. (2020). Overall, treatment causes a 3.0 percent decrease in fuel intensity of the household vehicle portfolio.

Second, a key advantage of our data is that we observe annual vehicle kilometers traveled for all cars in a household's portfolio. Using these unusually rich data, we find that lease-replacement cars in treated households are driven 2.7 percent *less* than lease-replacement cars in control households. Retained cars once again exhibit a countervailing effect. They are driven 4.1 percent *more* in treated households than control households. In total, household driving increases slightly, but not distinguishably from zero.

Third, these portfolio effects are concentrated almost entirely among multi-vehicle households, who increase retained-car driving by 1,228 kilometers per year and retain dirtier vehicles. For single-vehicle households both effects are statistically indistinguishable from zero. Moreover, households with three or more vehicles prior to the policy change expand their total fleet size, consistent with the model's prediction that subsidies induce fleet expansion rather than recomposition among households with the largest portfolios.

Next, we estimate the net effect on total household gasoline consumption. While it could be obtained by aggregating the extensive (vehicle choice) and intensive (driving) margin estimates just discussed, we estimate the effect directly after calculating gasoline consumption as the product of kilometers driven and fuel intensity (liters of gasoline per kilometer)[2]. The change in overall household gasoline consumption in treated households is indistinguishable from that in control households. This reflects a (significant) 8.1 per-

1. Note that this draft presents estimates of intent-to-treat effects, which we consider most relevant to policy. Future analysis may include estimates of local average treatment effects.

2. We use official fuel intensity ratings, although we acknowledge that true on-road fuel intensity may differ, as documented in Reynaert and Sallee (2021).

cent *reduction* in gasoline from lease-replacement cars and a (significant) 6.9 percent *increase* from retained cars.

Finally, we convert gasoline consumption savings into CO_2 emissions to evaluate the cost-effectiveness of the subsidy program. Our point estimates imply that the cost of carbon abatement of this policy is \$1,061 per ton of CO_2, and we can reject costs of CO_2 abated below \$200 per ton with 95 percent confidence. These elevated abatement costs are especially striking since our calculations assume a carbon-free electricity grid, as is roughly the case in Sweden, and that the cost of CO_2 abated from EV subsidies on grids with higher emissions would be even higher. Our estimates are substantially higher than other estimates in the literature. For example, Cole et al. (2023) estimate the cost per ton of abatement from EV subsidies to be \$95 per ton in the U.S.

These results highlight how households respond as a unit to changes in relative vehicle prices, with important substitution margins occurring beyond the choice and usage of a subsidized vehicle. These factors have important implications for effective policy design in transportation decarbonization as they re-emphasize why first best policies (e.g. Pigouvian pricing via a carbon tax or permit trading regime) are so aspirational.

While our findings highlight how omission of certain margins of adjustment within household portfolios undermine the cost-effectiveness of subsidy-based approaches to transportation decarbonization, it is worth noting that several benefits and costs of EVs, and even EV subsidies, fall outside the scope of this paper. We do not investigate questions relating to EV charging infrastructure (Li 2016; Springel 2021; Dorsey et al. 2025), electricity pricing (Holland and Mansur 2008; Ito et al. 2018; Borenstein and Bushnell 2022), or the marginal emissions arising from electricity generation (Graff Zivin et al. 2014; Archsmith et al. 2015; Holland et al. 2016). Furthermore, recent work has shown that EVs can contribute positively to grid operations as a distributed energy resource (Bailey et al. 2025; Metcalfe et al. 2026; La Nauze et al. 2024). On the other hand, EVs charged at home using high voltage chargers (e.g. Level 2) may impose external costs on distribution infrastructure, raising questions about the prospects for managed control (Rapson and Shaffer 2025; Burlig et al. 2026) or distribution-level pricing strategies (Turk et al. 2024). Finally, subsidies may be suitable to address market failures arising from research, development, and investment externalities, as suggested by Acemoglu et al. (2012) and Langer and Lemoine (2022). While important, these topics fall outside the scope of our inquiry.

Our findings are relevant to a broad set of jurisdictions that have adopted a subsidy-forward approach to transportation decarbonization. Sweden exhibits car-use patterns similar to other high-income Western European countries (Focas and Christidis 2017). In the U.S., research reveals substitution across vehicle attributes in household portfolios

in California, suggesting that our findings are likely relevant to multi-car households in general (Archsmith et al. 2020). The logic in this paper also extends beyond transportation policy. Subsidies for energy-efficient appliances may affect heating intensity or retention of older capital; education subsidies may reallocate effort or enrollment across institutions, including online learning; agricultural input subsidies may shift crop mix or land use. In each case, equilibrium adjustments within the portfolio determine policy effectiveness.

Even if transportation eventually reaches full electrification, understanding the transition path remains economically important. Because vehicles are durable goods and EVs still account for a relatively small share of the vehicle stock in most countries, complete fleet turnover will take many years. Climate damages depend on cumulative emissions rather than the eventual steady state, making emissions generated during the transition consequential for welfare. In addition, EV subsidies require substantial public expenditures, so the effectiveness with which they translate adoption into emissions reductions is a first-order policy concern. If PHEVs are imperfect substitutes for conventional vehicles, households may respond by retaining and continuing to use gasoline-powered cars rather than replacing them one-for-one. In that case, subsidy-induced EV adoption can coexist with relatively modest emissions reductions, substantially increasing the cost of carbon abatement. Understanding household substitution patterns is therefore essential both for evaluating the efficiency of current subsidy programs and for characterizing the path to a fully electrified vehicle fleet.

The remainder of the paper proceeds as follows. Section II discusses vehicle policies, the Swedish leasing market, and our data. Section III presents a model of household vehicle choice. Section IV presents our empirical strategy and potential threats to identification. Section V illustrates our main empirical results, and Section VI derives implications of the policy. The final Section VII offers concluding remarks.

II. Background & Data

Our empirical strategy exploits a change in vehicle rebates that differentially affected households reaching the end of a standardized three-year lease contract immediately before versus after the policy reform. In 2020, 43 percent of privately registered cars in Sweden were leased rather than purchased. In this section, we first describe the Swedish feebate system and the timing of relevant policy changes. We then introduce the administrative data used in the analysis, describe the construction of the lease-renewer sample, and present descriptive statistics for the households and vehicles in our study.

A. Swedish feebate system

Figure 1 illustrates the timeline of Sweden's vehicle feebate system. The policy was introduced in July 2018, combining purchase rebates for low-emission vehicles with higher annual road taxes for high-emission cars. In January 2020, the policy was amended to increase subsidies for plug-in hybrid electric vehicles (PHEVs). A second reform followed in April 2021, which further increased rebates for BEVs while reducing subsidies for PHEVs and raising road taxes, thereby shifting incentives toward fully electric vehicles. In this paper, we focus on the effects of the first policy change that increased subsidies for plug-in hybrid electric vehicles, and which offers the cleanest variation.

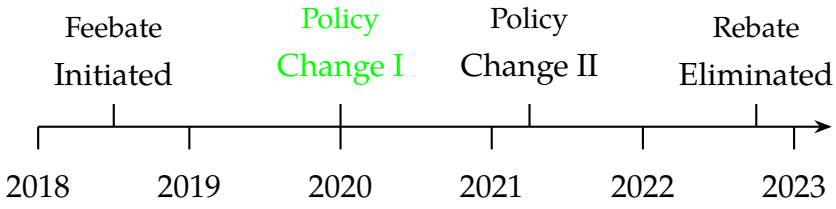


Figure 1: Timeline of vehicle feebate policy changes

Sweden's feebate system was designed to encourage the adoption of low-emission vehicles and deter the use of higher-emission ones. Under this system, financial incentives are provided for vehicles with lower emissions, while increased annual road taxes are imposed on those with higher emissions. The Swedish government introduced this policy through an ordinance issued in December 2017, which included the EV subsidies[3]. The amendment, effective from July 2018, applies exclusively to new vehicles registered in the Road Traffic Register from that date onward (Ministry of the Environment 2017).

Between July 2018 and December 2019, the vehicle rebate applied to vehicles that emit a maximum of 60 g/km of CO_2.[4] For battery electric cars (BEV) with zero tailpipe emissions, the highest possible subsidy amounts to 60,000 SEK ($\approx$ USD 6,312).[5] The bonus is reduced by a multiplication factor of 833 SEK ($\approx$ USD 88) for every gram of CO_2 emitted per kilometer. The vehicle rebate for these vehicles is calculated using the formula: *Rebate* = 60,000 - 833 $\times$ C, where C represents the grams of carbon emissions per kilometer emitted by the vehicle.[6] The minimum bonus of 10,000 SEK applied to vehicles emitting

3. We will refer to "subsidies" and "rebates" interchangeably throughout the paper.
4. The bonus is granted no earlier than six months after the purchase to prevent the resale of vehicles outside the country.
5. We convert SEK to US dollars using the exchange rate from January 1, 2020 ($0.1052/SEK).
6. The bonus is capped at 25 percent of the vehicle's new price when purchased by a household. For example, when adopting a PHEV, the price should be compared to that of the most similar internal combustion engine (ICE) model. CNG cars receive a bonus of 10,000 SEK ($\approx$ USD 1,052), independent of the CO_2

60 g/km of CO_2, with no bonus granted to vehicles exceeding this threshold. Figure 2 displays the vehicle rebate as a function of the vehicle emission level.

In January 2020, the Swedish government introduced modifications to its vehicle rebate system, which primarily impacted PHEVs, altering both the eligibility criteria for vehicle models and the corresponding bonus amounts. First, the CO_2 limit for new registrations to qualify for a climate bonus was increased to 70 g/km, thereby making more cars eligible for rebates. Second, the multiplication factor was reduced from 833 SEK to 714 SEK per gram of CO_2 emitted per kilometer, resulting in an increased vehicle rebate for all PHEVs emitting between 1 and 60 grams of CO_2 per kilometer. Importantly, the annual road taxes remained unchanged in 2020, ensuring that there was no differential impact on the adoption of fossil fuel vehicles. Our identification will exploit the timing of lease renewals along with the fact that PHEVs received different vehicle rebates after this policy change under the assumption that the policy change was orthogonal to lease-renewal timing.

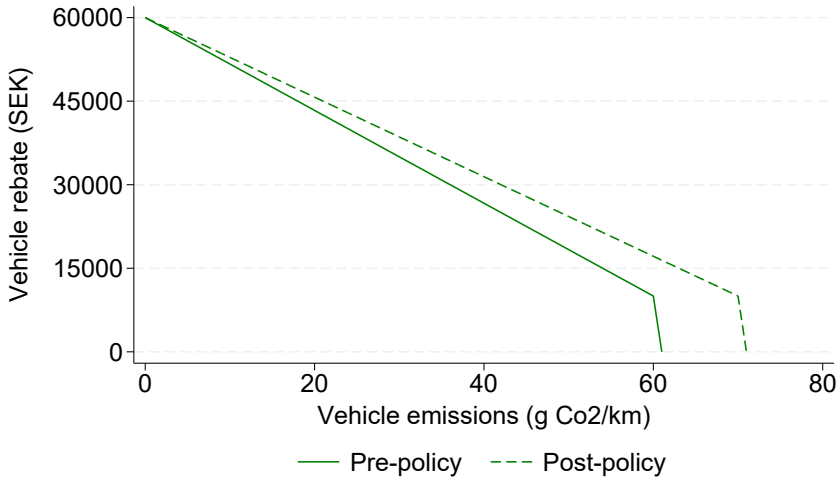


Figure 2: Rebate by vehicle emissions

Notes: This figure illustrates the vehicle rebate (SEK) as a function of the vehicle emissions for newly registered vehicles before the subsidy change (green solid line) and newly registered vehicles after the subsidy change (green dashed line).

B. Data sources

We combine data from several administrative sources provided by Statistics Sweden. These include the Swedish vehicle register (*Fordonsregistret*), the longitudinal integrated database

vehicle emissions.

for health insurance and labor market studies (*LISA*), and the geographic database (*Geografidatabasen*) from 2016 to 2022.

Vehicle data. The Swedish vehicle register contains information on all vehicle ownership and purchase records for the entire population of Sweden. The dataset includes details on the car's general status (registration date, owner type, whether it is leased, when the vehicle became the property of the current owner, and whether it is in use), the vehicle specifications (make, model, and trim), and various vehicle characteristics (service weight, fuel type, fuel efficiency, particle filter, carbon emissions).

Importantly, the register is a panel dataset that includes the annual vehicle kilometers traveled for each vehicle. Appendix B describes how the vehicle usage data is recorded in Sweden. For PHEVs, as we cannot observe whether the vehicle was driven on the electric or fossil drivetrain, we use the Worldwide Harmonized Light Vehicles Test Procedure (WLTP) standard for fuel consumption and adjust this value based on the share of kilometers driven using the fossil drivetrain from real-world usage patterns.[7] Appendix B.2 describes the adjustment we apply to WLTP-based utility factors to account for real-world PHEV electric driving behavior.[8] For BEVs, gasoline demand is zero.

Additionally, each registration records a unique vehicle identification number and a social security number equivalent, which uniquely identifies all individuals in Sweden. For our analysis, we restrict the dataset to privately owned passenger vehicles (*personbil klass 1*) and exclude those registered for commercial purposes.

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 socio-demographic information for Swedes over the age of 18, including gender, age, family status, income, gross salary, education, and employment status. To attribute commuting distances to each individual, we merge in data on employers and home locations via the geographic database.

C. Sample construction

Identifying the causal effects of EV subsidies on vehicle adoption and driving behavior requires changes in financial incentives that are orthogonal to unobserved preferences for

7. This approach relies on a fixed utility factor to approximate the share of electric driving and may not fully capture behavioral responses in drivetrain use. In particular, if the share of electric driving is endogenous to total mileage—for example, if marginal reductions in driving disproportionately reflect gasoline-powered trips—then assuming a constant average share may understate the gasoline savings associated with reduced PHEV driving. As a result, our estimates may be conservative with respect to net fuel savings.

8. We assume households with PHEVs drive on electricity only for a subset of total miles, with longer electric-only ranges corresponding to lower reliance on gasoline (Isenstadt et al. 2022).

vehicle choice and transportation demand. Comparing vehicle adoption and usage across households is challenging when replacement timing is endogenous and when households can freely adjust the timing of vehicle purchases in response to policy changes. To address these concerns, we focus on a sample in which vehicle replacement decisions occur at predetermined points in time and are therefore plausibly exogenous with respect to contemporaneous policy changes.

Our empirical analysis centers on households who initiated a vehicle lease between January 2016 and December 2017, and who subsequently reached the contractual renewal point approximately three years later. We exploit a key feature of the Swedish vehicle market that allow us to identify shifts in the adoption of new vehicles. Lease agreements in Sweden are typically written for exactly three years (1,095 days), generating a sharp concentration of vehicle replacement decisions at this horizon (Tebbe 2023). Figure 3 plots the probability of adopting a new vehicle as a function of days elapsed in the current lease contract. The figure reveals a pronounced spike in vehicle replacement at the three-year mark, indicating that leaseholders renew at or near the scheduled expiration date. We restrict the sample to leaseholders who renew their vehicle within a narrow window around the scheduled three-year expiration date, allowing for a ± 21 day deviation from the contractual renewal date, as indicated by the dashed lines in Figure 3. This restriction isolates leaseholders who are most directly exposed to the policy change and minimizes confounding variation arising from early terminations or lease extensions. To map individual renewal decisions to household-level outcomes, we further restrict the sample to families with exactly one individual at the three-year lease renewal, which excludes approximately 0.4 percent of the sample.

Treatment status is determined by the timing of lease expiration relative to the January 2020 subsidy reform. Households whose leases expired in 2019 serve as the control group, as they renewed prior to the increase in PHEV subsidies, while households whose leases expired in 2020 form the treated group and were eligible for the higher subsidy at renewal.

By combining predetermined variation in the timing of vehicle replacement decisions with a discrete change in financial incentives for plug-in hybrid electric vehicles, our empirical setting allows us to compare vehicle adoption and subsequent driving behavior across otherwise similar households. We exploit this variation to identify the causal effects of subsidy-induced PHEV adoption on household gasoline consumption, accounting for behavioral adjustments across the entire vehicle portfolio.

Figure A2 decomposes household gasoline consumption into its three determinants – fleet composition, kilometers per vehicle, and fuel intensity – disaggregated by fuel type. Consumption declines steadily over 2016–2023, with incremental reductions across all

three margins as the fleet shifts from petrol toward plug-in hybrid and battery electric vehicles, motivating the paper's central question of whether subsidy-induced adoption delivers these implied reductions or is attenuated by offsetting portfolio adjustments.

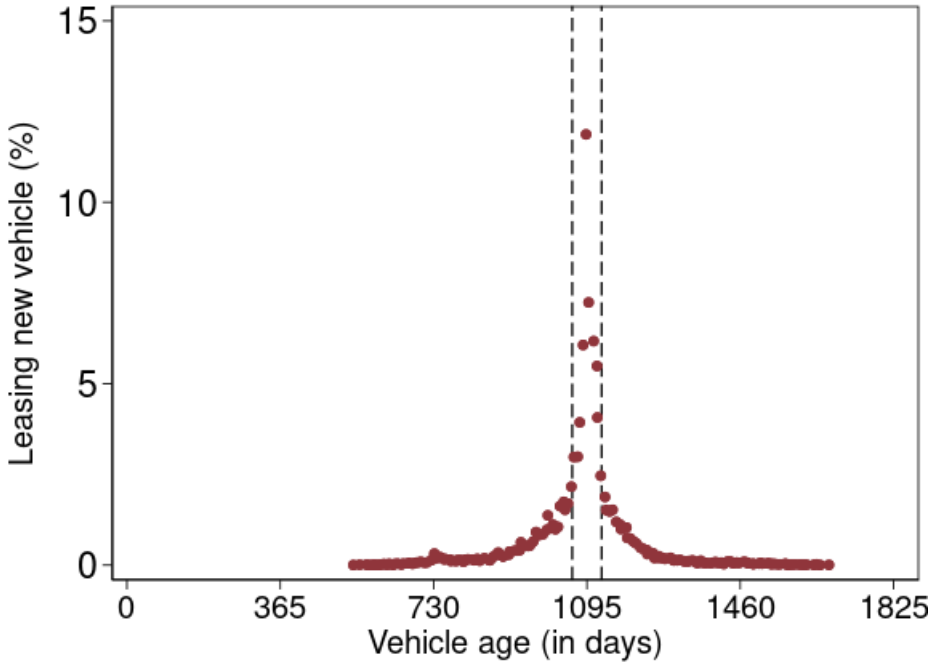


Figure 3: Probability of adopting a new vehicle by days in lease contract

Notes: The figure illustrates the contract renewal probability of leased cars with respect to the time in the current car contract (i.e., days since first registration). Dashed vertical lines reflect sample restrictions that we impose.

D. Descriptive statistics

The descriptive statistics demonstrate that our sample reflects the broader population of Sweden. Table A1 reports summary statistics for socio-demographic characteristics (Panel A) and vehicle attributes (Panel B) for households in our study sample, new car adopters, and the broader population of vehicle owners in Sweden. Households in our sample are similar in age to new car adopters and younger than the average Swedish household. They have higher incomes and slightly more years of education than both comparison groups. The share of women in our sample is somewhat higher than in either comparison group, while other demographic characteristics — self-employment and unemployment days — are broadly comparable across groups.

In terms of vehicle characteristics, cars in our sample are driven more intensively than those of both new car adopters and the average vehicle owner. Fuel intensity in our sample falls between the two comparison groups: higher than new car adopters' vehicles

but lower than the average Swedish vehicle. Consistent with the combination of higher mileage and fuel intensity, gasoline consumption in our sample is higher than in both comparison groups.

Table A2 reports average characteristics of newly registered vehicles for the treated group, the counterfactual group, and the full population of new vehicles. Vehicles adopted by treated households are, on average, more fuel-efficient than those adopted by the counterfactual group (4.95 vs. 5.16 liters/100km), which are in turn slightly more fuel-efficient than the average new vehicle registered in the same period (5.20 liters/100km). This suggests that households approaching lease renewal may have a tendency to select relatively fuel-efficient vehicles even in the absence of the subsidy change, implying that baseline vehicle choice among this group is already cleaner than for the average new car buyer in Sweden.

III. A Model of Household Vehicle Portfolio Choice

We develop a simple model of household vehicle portfolio choice that motivates our empirical decomposition and guides interpretation of the results. The model makes two contributions relative to standard single-car frameworks. First, it characterizes the household's joint decision over new vehicle adoption and retention of existing vehicles, showing how a subsidy that induces a cleaner new car may simultaneously change *which* older cars are retained and *how much* they are driven. Second, it yields comparative statics that differ by pre-policy household fleet size, providing directly testable predictions that we exploit in heterogeneity analysis.

A. Environment

A household enters the lease-renewal period owning $N \geq 1$ existing vehicles.[9] Each vehicle $k \in \{1, \ldots, N\}$ is characterized by three attributes: fuel intensity F_k (liters per kilometer, lower is cleaner), trip-type suitability $\sigma_k \equiv \phi_k^{\ell}/\phi_k^{d}$ (the ratio of local-trip to long-distance-trip productivity), and non-fuel attributes α_k capturing size, carrying capacity, and comfort.

At the lease-renewal date, the household makes two simultaneous decisions. First, it selects a new vehicle with attributes $(F_n, \sigma_n, \alpha_n)$ from a menu of available models $\mathcal{M}$. Second, it chooses a retention set $\mathcal{K} \subseteq \{1, \ldots, N\}$, trading in all vehicles not in $\mathcal{K}$ and retaining those that are. The post-renewal portfolio is therefore $\mathcal{E} \cup \mathcal{K}$, where $\mathcal{E} = \{n\}$

9. We do not consider households owning no cars as these would not fall within the purview of our research design that relies on the timing of lease renewal being exogenous to the timing of the subsidy policy.

denotes the single new vehicle. Portfolio size is endogenous: $|\mathcal{K}| \in \{0, 1, \ldots, N\}$. We define the three sets of vehicles as:

- **New Cars:** These are all new cars adopted after January 2019 by households assigned to either the treatment or control group
- **Retained Cars:** These are all other vehicles in the household other than the new vehicle.
- **All Cars:** These are all cars in the household around the policy change window.

The government operates a feebate: vehicle n receives a subsidy $\tau(F_n) \geq 0$ with $\tau'(F_n) \leq 0$, so that cleaner vehicles receive more generous support. The Swedish policy reform of January 2020 corresponds to a discrete upward shift in $\tau(\cdot)$ for plug-in hybrid electric vehicles (PHEVs), which combine low fuel intensity F_n with high local-trip suitability σ_n (their electric range is most effective for urban driving).

B. Stage 2: Driving Allocation

We solve the model by backward induction. Given a portfolio $\mathcal{E} \cup \mathcal{K}$, the household allocates annual kilometers $KM_j \geq 0$ to each vehicle j to maximize:

$$V(n, \mathcal{K}) = \max_{\{KM_j\}_{j \in \mathcal{E} \cup \mathcal{K}}} u\left(\sum_j \phi_j^{\ell} KM_j,\ \sum_j \phi_j^{d} KM_j,\ \sum_j \alpha_j KM_j\right) - p_g \sum_j F_j KM_j \qquad (1)$$

where $u(\cdot)$ is strictly concave in its three arguments (local trips L, long-distance trips D, and attribute services A), and p_g is the price of gasoline. The function $V(n, \mathcal{K})$ is the indirect utility from optimal driving given the portfolio.

The interior first-order conditions equate the marginal utility of a kilometer in vehicle j to its fuel cost:

$$\phi_j^{\ell}\, u_L + \phi_j^{d}\, u_D + \alpha_j\, u_A = p_g F_j, \quad \forall\, j \in \mathcal{E} \cup \mathcal{K}. \qquad (2)$$

Crucially, the equilibrium allocation of kilometers across vehicles depends on the full composition of the portfolio. A change in which vehicles are held, or in the attributes of the new vehicle, shifts the comparative advantage of each retained vehicle and thereby changes how much each is driven.

C. Stage 1: Portfolio Choice

The household chooses the new vehicle and the set of retained vehicles to maximize:

$$\max_{(F_n,\sigma_n,\alpha_n)\in\mathcal{M},\ \mathcal{K}\subseteq\{1,\ldots,N\}} V(n,\mathcal{K}) \ + \ \sum_{k\notin\mathcal{K}} R(F_k) \ - \ P(F_n) \ + \ \tau(F_n) \tag{3}$$

where $R(F_k)$ is the trade-in value received for vehicle k (increasing in fuel efficiency, i.e., decreasing in F_k), and $P(F_n)$ is the purchase price of the new vehicle. Vehicle k is retained if and only if its marginal contribution to portfolio value exceeds the trade-in opportunity cost:

$$\underbrace{V(n,\mathcal{K}) - V(n,\mathcal{K}\setminus\{k\})}_{\text{marginal portfolio value of vehicle } k} \ > \ \underbrace{R(F_k)}_{\text{trade-in value of vehicle } k} \tag{4}$$

The marginal portfolio value on the left-hand side captures how much indirect utility would be lost by removing vehicle k from the fleet. It depends on how well k complements the new vehicle n and the remaining retained vehicles in covering the household's preferences over the full vector of vehicle attributes.

Because the subsidy τ lowers the effective purchase price of a PHEV without raising the cost of retaining existing vehicles, it can induce PHEV adoption while simultaneously leaving the household free to expand the retained fleet, recompose it, or both. This asymmetry — changing the incentive to acquire clean vehicles but not the incentive to shed dirty ones — is the central difference between a subsidy and a Pigouvian carbon price.

D. Decomposition of Gasoline Demand

Our primary outcome of interest is household gasoline consumption $\text{Gas}_{i,t}$, which we construct by aggregating fuel use across all vehicles owned by household i in year t:

$$\text{Gas}_{i,t} = \sum_{j=1}^{N_{\text{cars},i,t}} F_{j,i,t} \times KM_{j,i,t}, \tag{5}$$

where fuel intensity ($F_{j,i,t}$) and annual kilometers traveled ($KM_{j,i,t}$) are measured at the vehicle level and summed over the household's entire vehicle portfolio. This construction captures not only changes in fuel intensity arising from the adoption of cleaner vehicles, but also behavioral responses through changes in driving. We decompose outcomes by vehicle type, distinguishing between newly leased vehicles (all cars adopted after 2019) and retained vehicles (all other cars remaining in the household fleet).

To guide interpretation, we derive a structural decomposition of total gasoline consumption. Pre- and post-policy household gasoline consumption are:

$$G^{\text{pre}} = F_n^{\text{pre}}\, KM_n^{\text{pre}} + \sum_{k \in \mathcal{K}^{\text{pre}}} F_k\, KM_k^{\text{pre}} \tag{6}$$

$$G^{\text{post}} = F_n^{\text{post}}\, KM_n^{\text{post}} + \sum_{k \in \mathcal{K}^{\text{post}}} F_k\, KM_k^{\text{post}} \tag{7}$$

where $\mathcal{K}^{pre}$ and $\mathcal{K}^{post}$ are the retention sets chosen before and after the policy change, and F_k is the (fixed) fuel intensity of vehicle k. The change in total gasoline consumption is:

$$\Delta G = \underbrace{\left(F_n^{post} KM_n^{post} - F_n^{pre} KM_n^{pre}\right)}_{\text{new vehicle}} + \underbrace{\sum_{k \in \mathcal{K}^{post}} F_k\, KM_k^{post} - \sum_{k \in \mathcal{K}^{pre}} F_k\, KM_k^{pre}}_{\text{retained fleet}} \tag{8}$$

The new-vehicle term can be decomposed by adding and subtracting $F_n^{post} KM_n^{pre}$; the retained-fleet term can be decomposed by adding and subtracting $\sum_{k \in \mathcal{K}^{pre}} F_k\, KM_k^{post}$:

$$\Delta G = \underbrace{KM_n^{pre}\, \Delta F_n}_{\substack{(i)\text{ New vehicle} \\ \text{fuel intensity}}} + \underbrace{F_n^{post}\, \Delta KM_n}_{\substack{(ii)\text{ New vehicle} \\ \text{driving}}} + \underbrace{\sum_{k \in \mathcal{K}^{post}} F_k\, KM_k^{post} - \sum_{k \in \mathcal{K}^{pre}} F_k\, KM_k^{post}}_{\substack{(iii)\text{ Retained fleet} \\ \text{composition}}} + \underbrace{\sum_{k \in \mathcal{K}^{pre}} F_k\, \Delta KM_k}_{\substack{(iv)\text{ Retained fleet} \\ \text{driving}}} \tag{9}$$

where $\Delta F_n \equiv F_n^{post} - F_n^{pre}$, $\Delta KM_n \equiv KM_n^{post} - KM_n^{pre}$, and $\Delta KM_k \equiv KM_k^{post} - KM_k^{pre}$.

This decomposition makes the structure of each margin precise. Terms (i) and (ii) capture the direct effect of the subsidy on the new vehicle: households induced to adopt a PHEV select a vehicle with lower fuel intensity (margin i negative), while the effect on new vehicle driving (margin ii) is theoretically ambiguous — PHEVs may be driven less if their limited range crowds them into a subset of trips, or more if the lower per-kilometer cost of electric driving induces a rebound effect. Standard single-car analyses treat the policy effect as the sum of (i) and (ii), implicitly setting terms (iii) and (iv) to zero.

Margin (iii) is a difference of sums over two different retention sets — it captures the discrete recomposition of the fleet. It is non-zero whenever PHEV adoption changes which vehicles the household retains, and its sign depends on whether the vehicles that exit or enter $\mathcal{K}$ are cleaner or dirtier than those that remain. Margin (iv) is a sum over vehicles that were in the fleet both pre- and post-policy — it captures the continuous reallocation

of driving kilometers among retained vehicles in response to the changed portfolio composition.

Proposition 1 (Attribute Substitution — margin iii)**.** *If PHEV adoption causes the household to trade in vehicles that are, on average, cleaner than those retained, then margin* (iii) *is positive. If the traded-in vehicles are dirtier on average, the sign reverses.*

The direction is an empirical question. Our results are consistent with the first case: treated households retain vehicles that are, on average, more fuel-intensive than those retained by control households (Section V), implying margin (iii) > 0.

Proposition 2 (Driving Reallocation — margin iv)**.** *When PHEV adoption changes the attribute composition of the household portfolio, the equilibrium allocation of driving across vehicles adjusts. Households reallocate kilometers toward retained vehicles, so margin* (iv) *is positive.*

The mechanism follows from the first-order conditions in equation (2): adding a PHEV to the portfolio shifts the shadow prices of trip types and attributes, raising the marginal value of kilometers in retained vehicles relative to the PHEV for some uses. The specific channels through which this reallocation operates — whether trip-type specialization, non-fuel attribute preferences, or both — are not identified by the current empirical design and represent a natural direction for future work.

E. Implications for Cost-Effectiveness

The household's private optimization in equations (3)–(1) ignores the external cost of carbon emissions. To assess whether the feebate subsidy is a cost-effective instrument for reducing gasoline consumption, we compare the fiscal expenditure required to induce one additional PHEV adoption with the lifetime reduction in net carbon emissions that adoption generates.

Fiscal cost per marginal adopter. A subsidy τ is paid to all households whose new vehicle clears the emissions threshold, regardless of whether the subsidy *causally induced* their adoption decision. The population of subsidy recipients consists of two groups: *marginal adopters*, who switch to a PHEV because of the subsidy, and *inframarginal adopters*, who would have adopted a PHEV even in its absence and receive the transfer without generating additional emissions reductions. Let ε denote the share of subsidy-induced PHEV adoption, which we refer to as the marginal adopter share from now on. We measure this as the ratio of the post-reform increase in PHEV adoption share to the total PHEV adoption share observed immediately after the reform, $\Delta\varepsilon = \Delta\gamma^{\text{share}}/(\Delta\gamma^{\text{share}} + \gamma^{\text{pre}})$, where $\Delta\gamma^{\text{share}}$ is the discontinuous increase in PHEV adoption share at the policy cutoff (the marginal

adopters), and γ^{pre} is the pre-reform baseline PHEV adoption share (the inframarginal adopters who would have adopted without the subsidy). Appendix D.1 provides a derivation of how we compute the marginal cost of the subsidy. If $\Delta\gamma^{\text{subsidy}}$ denotes the change in the average subsidy paid per new vehicle following the policy reform, then the fiscal cost of inducing one additional PHEV adoption is:

$$\text{Cost per marginal adopter} \;=\; \frac{\Delta\gamma^{\text{subsidy}}}{\Delta\varepsilon} \tag{10}$$

The numerator captures both the expansion in subsidy eligibility and the increase in subsidy amounts under the new policy regime. The denominator scales by the share of recipients whose adoption decision is causally attributable to the subsidy.

Net carbon abatement per marginal adopter. Adopting a PHEV reduces gasoline consumption but raises electricity-related emissions. The decomposition in equation (9) characterizes the household's annual change in gasoline consumption ΔG, which combines all four margins — new vehicle fuel intensity, new vehicle driving, retained fleet composition, and retained fleet driving. Converting gasoline reductions to avoided CO_2 emissions requires a fuel-to-emissions factor α (kilograms of CO_2 per liter of gasoline; $\alpha \approx 2.3$).

The offsetting emissions from electric driving depend on three quantities: The energy efficiency of the electric drivetrain, denoted by e, the share of vehicle kilometers driven on electricity, ϑ, and the carbon intensity of electricity generation, CI^{grid}, which varies with the marginal generation technology (e.g., renewables, natural gas, or coal). The annual net change in CO_2 emissions per marginal adopter is therefore:

$$\Delta CO_2^{\text{annual}} \;=\; \underbrace{-\alpha \cdot \Delta G}_{\Delta CO_2 \text{ from gas demand}} \;-\; \underbrace{\Delta KM^e \cdot \vartheta \cdot e \cdot CI^{\text{grid}}}_{\Delta CO_2 \text{ from charging}} \tag{11}$$

where ΔKM^e is the additional kilometers driven on electricity by the marginal adopter.

Cost per ton of avoided emissions. To estimate how expensive the feebate subsidy for plug-in hybrid electric vehicles (PHEVs) is per ton of abated CO_2 emissions, we compare the additional fiscal cost of the policy to the discounted lifetime reduction in net carbon emissions induced by the subsidy. On the emissions side, net emissions reductions arise from the subsidy-induced change in lifetime gasoline consumption, offset by additional emissions from electricity used to charge electric vehicles. We measure gasoline displacement directly using household-level data and decompose it into changes from newly adopted vehicles (margins i and ii) and portfolio-level driving adjustments across retained vehicles (margins iii and iv), and we account for emissions from EV charging under alternative assumptions about the marginal electricity generation mix. This frame-

work allows us to translate our causal estimates of gasoline displacement into a measure of the marginal cost of carbon abatement implied by the policy.

The cost-effectiveness of the policy — the marginal cost of carbon abatement, CTA^{subsidy} — is the fiscal cost per marginal adopter divided by the present discounted value of net emissions reductions over the vehicle's lifetime. Let r denote the annual discount rate and H the vehicle's lifespan in years. Combining equations (10) and (11) gives the discounted subsidy-induced cost per unit of avoided emissions:

$$CTA^{\text{subsidy}} = \sum_{h=1}^{H} \frac{\overbrace{\frac{\Delta\gamma^{\text{subsidy}}}{\varepsilon}}^{\text{cost per marginal adopter}}}{\frac{1}{(1+r)^h}\left[\underbrace{-\alpha \cdot \Delta G}_{\Delta CO_2 \text{ from gas demand}} - \underbrace{\Delta KM^e \cdot \vartheta \cdot e \cdot CI^{\text{grid}}}_{\Delta CO_2 \text{ from charging}}\right]} \tag{12}$$

Equation (12) highlights four empirical quantities that must be estimated to evaluate the policy: (i) the change in average subsidy paid per new vehicle $\Delta\gamma^{\text{subsidy}}$, which we recover from the discontinuity in subsidy take-up at the policy threshold; (ii) the share of marginal adopters $\Delta\varepsilon$, measured as the ratio of the policy-induced increase in PHEV adoption to total post-reform PHEV adoption; (iii) the household-level change in gasoline consumption ΔG, which we construct using the four-margin decomposition in equation (9); and (iv) the additional kilometers driven on electricity ΔKM^e, which we measure using the electric drivetrain efficiency and charging patterns. We recover these four empirical objects using the discontinuity in lease renewals around the policy threshold. Section V provides the empirical magnitudes underlying each component, and Appendix D.1 provides a detailed derivation of $\Delta\gamma^{\text{subsidy}}$.

The decomposition in equation (9) characterizes household response along two behavioral margins — fuel intensity and driving behavior — separately for newly adopted vehicles and retained vehicles, which together determine total gasoline demand. Table 1 summarizes the predicted sign of each margin, separately for new and retained vehicles.

Table 1: Predicted signs of gasoline-demand margins

	Vehicle type	
	(1) New cars	**(2) Retained cars**
A. Driving behavior (km/year)	$\lessgtr 0$	$\lessgtr 0$
	Margin (ii)	*Margin (iv)*
B. Fuel intensity (liter/100km)	< 0	$\lessgtr 0$
	Margin (i)	*Margin (iii)*

Notes: The table reports the theoretically predicted sign of each margin in the gasoline-demand decomposition of equation (9), separately for newly adopted vehicles (column 1) and retained vehicles (column 2). Panel A reports the change in annual driving and Panel B the change in fuel intensity. Total household gasoline demand is the sum of the new-vehicle and retained-vehicle effects and can therefore be calculated from the two columns. New-vehicle fuel intensity (margin i) is negative by construction of the feebate ($\tau'(F_n) \leq 0$), which ensures the subsidy favors cleaner new vehicles. Predicted signs of retained-vehicle margins follow from Propositions 1 and 2; all other signs are theoretically ambiguous.

Each panel of Table 1 maps to a distinct margin of the gasoline decomposition. Panel C reports the fuel-intensity margins. Margin (i) — the change in new-vehicle fuel intensity — is unambiguously negative: the feebate steers households toward cleaner new vehicles by construction. Margin (iii) — the recomposition of the retained fleet — is theoretically ambiguous: it is positive if PHEV adoption causes households to retain vehicles that are, on average, more fuel-intensive than those they trade in (Proposition 1), and negative otherwise.

Panel B reports the driving margins. Margin (ii) — driving in the new vehicle — is theoretically ambiguous: PHEVs may be driven less if their limited electric range crowds them into a subset of trips, or more if the lower per-kilometer cost of electric driving induces a rebound effect. Margin (iv) — driving in retained vehicles — is positive when households reallocate kilometers toward retained vehicles to exploit their comparative advantage in trips for which the PHEV is poorly suited (Proposition 2).

Panel A aggregates these into total gasoline demand: the new-vehicle column combines margins (i) and (ii), while the retained-vehicle column combines margins (iii) and (iv). The asymmetry between new and retained vehicles is the central insight of the model. By lowering the cost of PHEV acquisition without raising the cost of retaining existing vehicles, the subsidy can simultaneously reduce gasoline use through the new vehicle (margins i and ii) while increasing gasoline use through the retained fleet (margins iii and iv).

This is in contrast to a Pigouvian carbon price, which would raise the marginal cost of all fossil fuel driving and induce households to shed dirty vehicles. We test these predictions directly in Section V by estimating each margin separately and comparing their signs and magnitudes against the predictions in Table 1.

IV. Empirical Strategy

A. Research Design

Estimating the causal effect of PHEV adoption on gasoline consumption faces several empirical challenges. First, subsidy availability and vehicle adoption are not randomly assigned across households. Households that choose to adopt subsidized PHEVs likely differ systematically from non-adopters in unobserved preferences for environmental goods, driving patterns, or willingness to adopt new technologies. These selection effects could bias cross-sectional comparisons if, for example, early PHEV adopters would have reduced their gasoline consumption even in the absence of the subsidy. Second, vehicle choice is inherently correlated with transportation demand: households anticipating higher future driving may select different vehicles than those expecting low vehicle usage, making it difficult to separate the effect of vehicle choice from underlying demand differences. Third, data limitations compound these challenges. Measuring gasoline displacement requires observing both individual vehicle kilometers traveled and the complete household vehicle portfolio over time, as substitution effects across vehicles within multi-car households can substantially offset reductions from newly adopted PHEVs. These data are rarely available in conventional datasets, which typically observe either aggregate household fuel purchases or individual vehicle attributes, but not both at the household-vehicle-year level necessary to identify behavioral responses.

We overcome these challenges by using comprehensive nation-wide Swedish administrative vehicle data on all vehicles in a household along with a fuzzy regression discontinuity design (RD) design to estimate the extent to which a newly acquired PHEV displaces gasoline demand. The discontinuity arises from changes in vehicle subsidies for PHEVs among households with expiring leasing contracts. Lessees whose contracts expired after January 2020 were eligible for extended vehicle subsidies, whereas those whose contracts expired before this cutoff were offered lower subsidies.

The RD design allows us to estimate the effect of the subsidy on both new vehicle choice as well and usage of new and retained cars. Specifically we estimate the following equation on outcome of interest y_{it}:[10]

10. See Imbens and Lemieux (2008) and Lee and Lee and Lemieux (2010) for details on the implementation

$$y_{it} = \alpha + 1[t \geq c](g_r(t-c) + \lambda) + 1[t < c]g_l(c-t) + \delta Z_i + e_{i,t}, \tag{13}$$

where c is the cut-off date for the vehicle rebate, t is the date of the leasing contract renewal, and g_r, and g_l are unknown functions. λ delivers the regression discontinuity estimate under the assumption that other policy changes and covariates are continuous at the threshold. We additionally control for covariates Z_i that may differ discontinuously across lease renewers on either side of the cutoff in the regression discontinuity design.

We exclude households with lease renewals three weeks before and after the policy change in January 2020, since households very close to the cutoff may have been able to strategically delay or accelerate renewal in response to the announced policy change. This "donut" restriction ensures that treatment assignment is driven by predetermined lease timing rather than contemporaneous vehicle choice.[11]

To credibly estimate the magnitude of the displacement effect, we assume that no other factors changed discontinuously at the cutoff date and that the sole channel through which fossil fuel kilometers are affected is the adoption of newly subsidized PHEVs. This formulation allows for downstream behavioral responses within the household vehicle portfolio—such as changes in the usage of retained vehicles—that are induced by PHEV adoption. Additionally, the validity of the 2SLS estimation relies on the monotonicity assumption, which requires that the vehicle rebate does not induce any household to forgo adopting a PHEV that it would have otherwise adopted in the absence of the subsidy.

Although our estimated treatment effects apply specifically to a population of leaseholders renewing their contracts after three years, we argue that this population is highly relevant for predicting future EV adoption and usage patterns. Leasing is becoming an increasingly dominant mode of vehicle acquisition in many countries, particularly for EVs, which account for a growing share of newly leased cars. Moreover, the structure of leasing markets implies that subsidies are directly passed through to leasing rates.

Intuition. To conceptualize how our identification strategy captures the displacement effect, consider two similar households illustrated in Figure 4. The control household (top) initiated a lease just before 2017 and renewed just before January 2020, before the policy change. The treatment household (bottom) initiated a lease just after 2017 and renewed just after January 2020, after the policy change, qualifying for the higher PHEV

and assessment of RD designs.

11. This approach is similar to (Dahl et al. 2014), who exclude births occurring immediately before and after a policy change to avoid potential strategic timing of births in response to announced reforms. They exclude a two-week window on each side of the policy cutoff to address concerns about strategic manipulation of birth timing.

subsidy (highlighted by the red dashed circles). Because lease timing was determined three years prior, neither household could have anticipated the subsidy change when initiating their contract. While the control household renews into a conventional fossil fuel vehicle, the treatment household adopts a subsidized PHEV (green car). Any subsequent differences in driving behavior between these otherwise similar households can therefore be attributed to the policy-induced difference in vehicle choice.

A key feature of this design is that we measure gasoline consumption in the same calendar years for both groups, as indicated by the red dashed vertical lines and shaded regions in Figure 4. Specifically, 2018 serves as the pre-period — roughly one year after the initial lease for both groups — and 2021 serves as the post-period — roughly one year after lease renewal for both groups. By holding calendar years fixed across groups, this comparison absorbs common time-varying shocks such as fuel price fluctuations, macroeconomic conditions, and nationwide vehicle policies that affect all households equally.

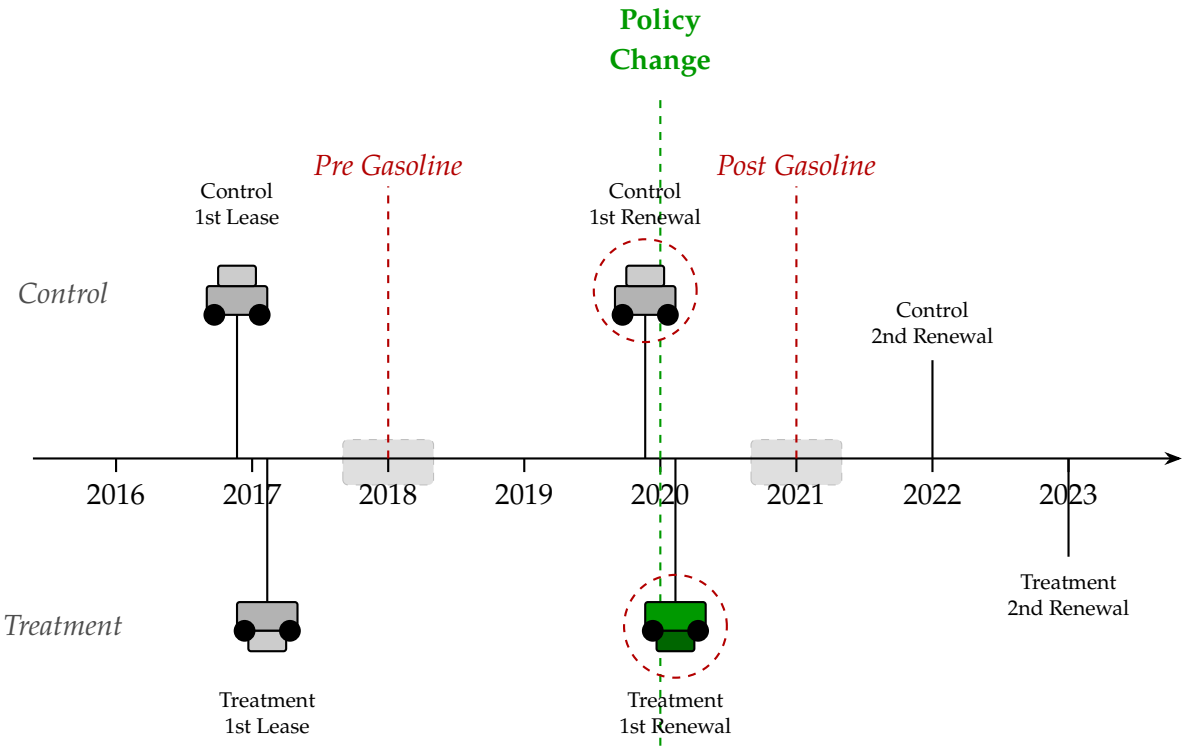


Figure 4: Identification intuition and calendar year comparison

Notes: Control households initiated leases just before 2017 and renewed just before 2020, *before* the January 2020 policy change. Treatment households initiated leases after 2017 and renewed after 2020, *after* the policy change, qualifying for the higher PHEV subsidy (green car). Red dashed vertical lines indicate the pre- (2018) and post- (2021) gasoline measurement years, held fixed across both groups. Shaded regions indicate measurement windows. Red dashed circles highlight the two new cars at the lease renewal.

In addition to the institutional features that lend credibility to our design, the timing of the subsidy change is unlikely to be confounded by COVID-19 mobility restrictions. First, unlike many other countries, Sweden did not impose legally enforced lockdowns or broad travel bans during the pandemic, instead relying on public health recommendations and limited social distancing measures; there were no nationwide stay-at-home orders or mandatory limits on travel comparable to the strict lockdowns seen elsewhere (Bohman et al. 2021). While mobility did decline during the early pandemic period, these changes appear to reflect voluntary behavioral responses rather than government-mandated travel restrictions (Born et al. 2021). Second, by comparing households with lease expiration immediately before and after the subsidy change, we hold constant household-specific driving patterns and seasonal effects and preserve the core variation in lease renewal timing relative to the January 2020 subsidy change. As a result, variation in lease timing relative to the subsidy remains plausibly exogenous and likely unaffected by pandemic-related shocks. Finally, while we exploit lease renewals around the 2020 event, driving behavior and gasoline consumption comparisons are between 2018 and 2021. We thus exclude 2020 as a comparison year for these outcomes.

B. Identification tests

The validity of our RD design requires that households cannot manipulate the assignment variable, the timing of the lease renewal, near the cutoff of the policy change. There is little opportunity to strategically time the starting of the lease contract 3-years before in anticipation of policy changes, as the implementation date for the reform was announced just before the respective policy. However, it is still possible that leaser deviate from the three-year lease renewal timing and either renew earlier or renew later to receive the bonus system. In Figure 5, we document, however, that the share of lease renewals around the policy change cutoff is smooth. In addition, as deviating from the lease contract is costly, we show that these deviations are typically more expensive than changes in vehicle policies. We also find that the average number of days between lease renewal and first lease initiation are similar for treated and control households (Figure C1).

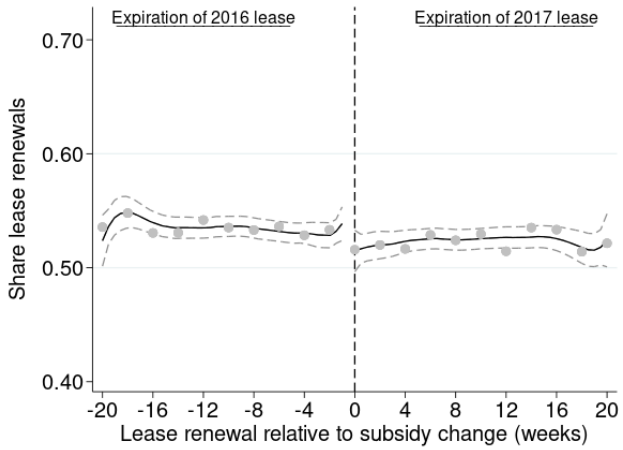


Figure 5: Lease renewal probability

Notes: The figure shows the share of households who adopt a new vehicle at the lease renewal around the 2020 policy change. Dashed vertical lines denote the subsidy change of January 2020 (normalized to 0).

Figure 6 presents the demographic characteristics of the leaseholders before and after the renewal of the contract, relative to the introduction of the subsidy. The plots indicate that key characteristics such as age, income, employment status, and education remain balanced across both periods, suggesting that leaseholders renewing their contracts before and after the policy change are comparable in terms of demographics. For gender, we find a slight imbalance; however, we show that including gender as a control in the RD specification (Table C7) does not meaningfully change the results. This supports the assumption that any observed differences in outcomes are likely attributable to the subsidy rather than changes in the underlying population composition.

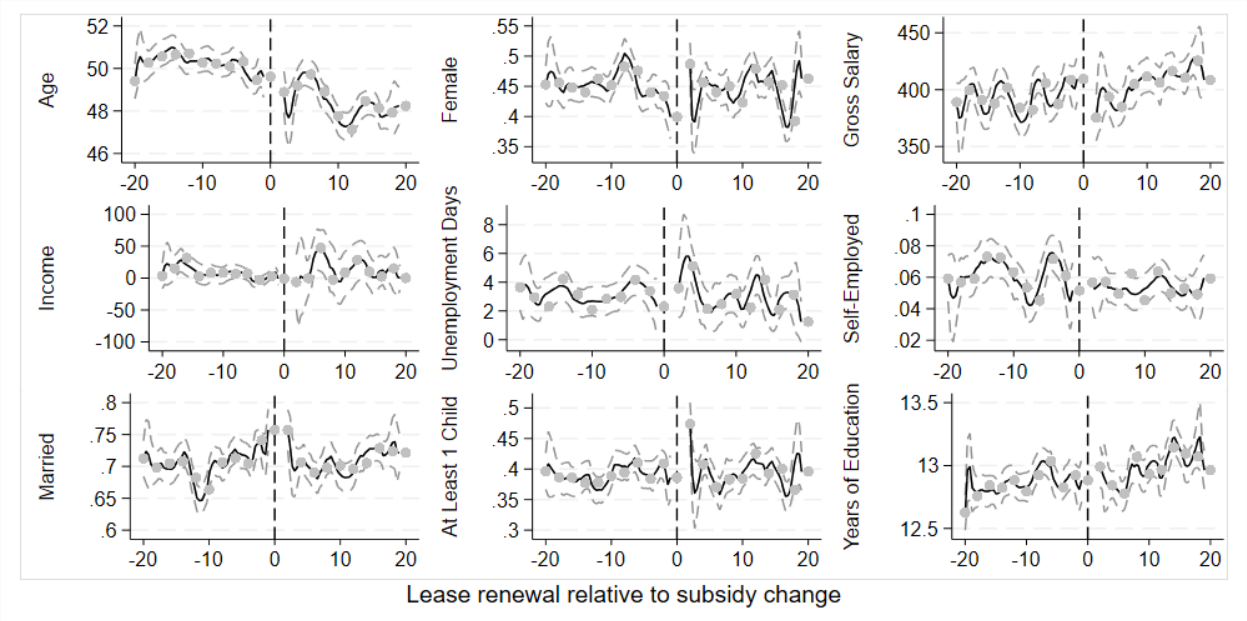


Figure 6: Demographic characteristics of leasers around policy

Notes: The figure shows nine demographic characteristics among individuals who adopt a new vehicle at the lease renewal around the 2020 policy change. Dashed vertical lines denote the subsidy change of January 2020 (normalized to 0).

Although our main analysis focuses on the 2018-2021 difference for main outcomes, we also verify that the pre-treatment (2018) distribution of vehicle ownership is comparable between control and treated households (Figure A1).

In addition to the rebate change, there were modifications to the vehicle emission standards that influenced vehicle rebates and annual road tax calculations. The tax system was updated to reflect the Worldwide Harmonized Light Vehicles Test Procedure (WLTP) for measuring CO_2 emissions. This method generally reports higher CO_2 values compared to the previously used New European Driving Cycle (NEDC) standard. Since WLTP-measured emissions tend to be higher than those reported under NEDC, this change effectively made a few PHEVs non-eligible for vehicle rebates.

Figure A3 presents both sides of the January 2020 reform for vehicle models registered in 2020: the left panel shows the change in annual road tax as a function of vehicle emissions, while the right panel shows the corresponding change in the vehicle rebate. The left panel therefore indicates how much more households would have had to pay in road tax under the revised schedule, while the right panel indicates how much higher the PHEV rebate became under the new subsidy formula. For vehicles with emissions between 115 and 125 g/km — a range representative of average petrol and diesel vehicles in our sample — the average increase in annual road tax was approximately 1,630 SEK

annually for the first three years. This is likely an overestimate of the road tax increase faced by treated households, since households can substitute toward vehicles that did not experience a substantial increase in road tax under the revised schedule. By comparison, the average PHEV rebate increased by approximately 5,387 SEK per average model, which may represent an underestimate given households likely adopt vehicles that faced a larger PHEV rebate increase.

What matters for vehicle choice is the total effective price difference between PHEVs and fossil-fuel vehicles, which combines both sides of the reform. Both changes push in the same direction: the higher rebate lowers the relative price of PHEVs, and the higher road tax raises the relative cost of high-emission vehicles. Our design, however, attributes the entire adoption response to the subsidy alone. This affects the cost-effectiveness calculation through two channels, both of which work in the same direction. First, part of the discontinuous increase in PHEV market share $\Delta\gamma^{\text{share}}$ is possibly also attributable to the road tax, so we overstate the share of marginal adopters ε and therefore understate the fiscal cost per marginal adopter in equation (10). Second, our design does not separate any additional emissions reduction generated by the road tax discouraging fossil-fuel vehicles, so we credit the subsidy with abatement it did not induce. Loading the full response of a two-sided reform onto one instrument is therefore a conservative approach: disregarding the road tax reduces the estimated cost of abatement unambiguously, and our estimate should be read as a lower bound.

V. Empirical Results

This section first quantifies the effect on household gasoline demand and its underlying components, then evaluates the cost of the feebate reform through changes in subsidy intensity and eligibility, and finally assesses the robustness of the results to alternative bandwidth choices.

A. Gasoline demand

Table 2 reports regression discontinuity estimates of the effect of the subsidy-induced lease renewal on gasoline demand using the calendar-year comparison, decomposed into total household gasoline use (Column 1), gasoline use from new vehicles (Column 2), and gasoline use from retained vehicles (Column 3). Panel A shows that the subsidy leads to a statistically significant reduction in gasoline consumption from newly leased vehicles of approximately 85 liters per year (8.1 percent). This reduction reflects both substitution toward cleaner vehicles at lease renewal and lower gasoline consumption by these

new vehicles. In contrast, gasoline consumption from retained vehicles increases by about 72 liters (6.9 percent), consistent with treated households reallocating driving toward retained fossil-fuel vehicles following the adoption of a subsidized PHEV. As a result, the net effect on total household gasoline consumption is a modest and statistically insignificant reduction of 13 liters per year.

Panels B and C provide insight into the underlying mechanisms. Panel B shows that treated households drive newly leased vehicles approximately 534 kilometers (2.7 percent) less per year, while driving of retained vehicles increases by 811 (4.1 percent) kilometers, contributing to the offsetting effects observed in total gasoline use. Table C4 documents the effect of the subsidy on vehicle usage by fuel type. We estimate that plug-in electric vehicles increase annual driving by 528 kilometers, while fossil fuel and hybrid vehicles show no statistically significant changes in usage. This represents a key parameter for our cost-abatement calculation, as the additional EV driving generates incremental electricity demand and associated emissions from charging.

Panel C indicates that fuel intensity declines significantly for newly leased vehicles by 0.39 liters per 100 kilometers (7.3 percent less fuel intense), reflecting the adoption of cleaner vehicles, whereas fuel intensity of retained vehicles increases by 0.21 liters per 100 kilometers (3.9 percent more fuel intense), which implies that treated household retain dirter vehicles. These results imply that while the subsidy substantially reduces gasoline demand from new vehicles, increased use of retained vehicles and retaining dirtier vehicles largely offsets these gains, limiting the overall reduction in household gasoline consumption.

Figure 7 presents visual evidence of the regression discontinuity design using the calendar-year comparison. Each panel plots binned means of the outcome variable against weeks from the January 2020 policy change, with separate linear fits on either side of the discontinuity threshold (indicated by the vertical dashed line at zero). The figure is organized in three rows corresponding to the three panels of Table 2: Panel A displays gasoline demand in liters, Panel B shows driving behavior in kilometers, and Panel C presents fuel intensity in liters per 100 kilometers. Within each row, the left column shows total household outcomes, the middle column shows outcomes for new vehicles, and the right column shows outcomes for retained vehicles.

The graphical evidence corroborates the regression results. For newly leased vehicles (middle column), there are clear negative discontinuities in both gasoline demand and kilometers driven, along with a reduction in fuel intensity. For retained vehicles (right column), positive discontinuities are visible in both gasoline demand and kilometers driven, accompanied by a modest increase in fuel intensity at the threshold.

Our main specification classifies all vehicles leased within the bandwidth period as "new vehicles." In Table C1, we provide a robustness check that separately examines vehicles leased within the bandwidth period versus those newly registered after the bandwidth period (post-July 2020). The results indicate no significant differences in vehicle adoption patterns outside the bandwidth period, confirming that all main treatment effects derive from vehicles leased within the bandwidth around the policy discontinuity.

Table 2: Effect on gasoline demand changes

	Vehicle type		
	(1) All cars	(2) New cars	(3) Retained cars
A. Gasoline Demand (liter)			
Post-subsidy lease renewal	-12.70	-84.50***	71.80***
	(27.93)	(15.66)	(27.70)
Mean (t-1)	1041.56	0	1041.56
B. Driving behavior (km/year)			
Post-subsidy lease renewal	277	-534**	811*
	(443)	(251)	(457)
Mean (t-1)	19822	0	19822
C. Fuel intensity (liter/100km)			
Post-subsidy lease renewal	-.16**	-.39***	.21**
	(.07)	(.06)	(.08)
Mean (t-1)	5.33	5.1	1.04

Notes: This table reports the RD estimates using calendar year comparisons for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) new cars, and (3) retained cars. The mean dependent variables are reported below the coefficients. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We exclude households with lease renewals three weeks before and after the policy change in January 2020. We use triangular kernel weights. The unit of observation is household×year.

Table C2 disaggregates the baseline results by household vehicle portfolio size, sep-

arating households that owned exactly one vehicle in 2021 from those owning more than one. This split is motivated by the intuition that our effects on retained vehicles can only arise in households that have a second car to reallocate driving toward.

Turning first to new vehicles, both single- and multi-vehicle households reduce driving of their new cars by a similar magnitude (507 and 452 kilometers, respectively), consistent with PHEVs being driven less than the conventional vehicles they replace. However, multi-vehicle households adopt somewhat cleaner PHEVs, reducing fuel intensity by 0.42 liters per 100 kilometers compared to 0.33 for single-vehicle households, resulting in a larger reduction in gasoline demand from new vehicles (87 versus 70 liters). Turning to retained vehicles, the gasoline demand effect is concentrated almost entirely among multi-vehicle households: these households increase driving of retained cars by 1,228 kilometers per year and retain dirtier vehicles (0.21 liters per 100 kilometers more fuel intense), together producing a 92-liter increase in gasoline consumption from retained cars. In contrast, this effect is statistically insignificant and economically negligible for single-vehicle households.

The broader implication is that the extent to which EV subsidies translate into meaningful reductions in gasoline consumption depends on the vehicle portfolio structure of the population: in countries with a high prevalence of multi-vehicle households, the substitution of driving toward retained fossil-fuel vehicles may be even be higher, further eroding the emissions benefits of subsidy programs.

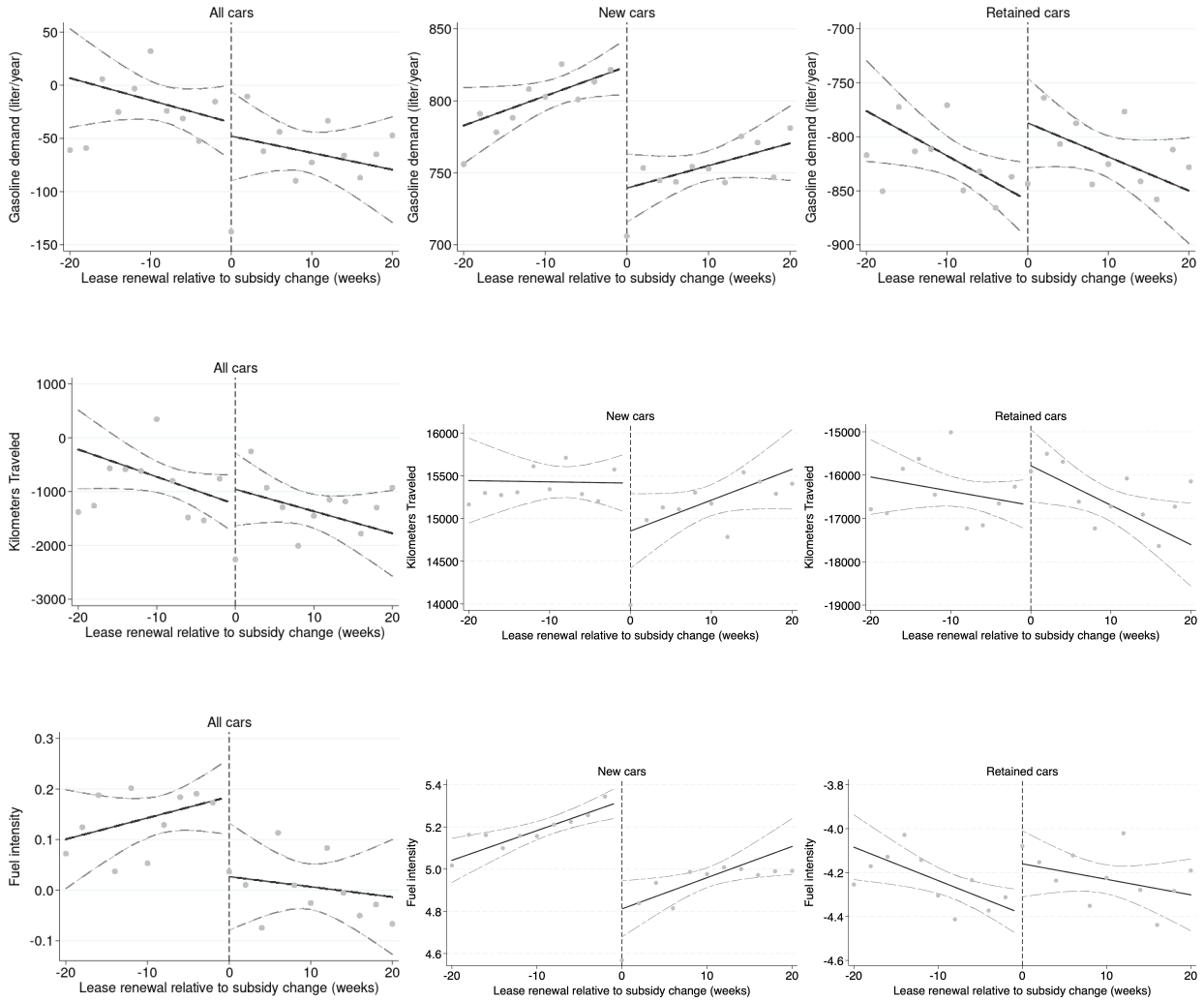


Figure 7: RD estimates on gasoline demand, kilometers traveled, and fuel intensity

Notes: This figure plots the RD estimates using calendar year comparisons for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) new cars, and (3) retained cars. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We use triangular kernel weights. The unit of observation is household×year.

A potential concern is that the total emission changes from the vehicle subsidy could be affected if vehicles displaced from treated households are subsequently driven differently once they pass to a new owner. This is a second-order substitution margin our household-level accounting does not directly capture. While we cannot estimate the full downstream emission effects of the household that acquires an exiting car, we can test whether the exiting vehicles themselves change usage differently depending on whether

they came from a treated or control household. To do this, we take all vehicles that exited treated and control households at the three-year lease renewal in 2019 and 2020 and use our RD comparing the change in each vehicle's annual kilometers traveled from 2018 (under the original owner) to 2021 (under its new owner) across the January 2020 policy threshold. We find no significant difference in this change between vehicles exiting treated versus control households (Figure C3): cars sold by treated households are not driven any more or less intensively by their next owners than cars sold by control households. Figure A4 confirms this descriptively: average annual kilometers traveled for exiting vehicles, plotted separately by lease-renewal cohort (2019 control vs. 2020 treated) follows a similar trajectory for both groups. While this test cannot rule out more diffuse effects further down the used-vehicle ownership chain, it suggests that exiting vehicles are not absorbed into meaningfully different driving patterns once they leave the first household. This alleviates concerns that our main estimates are offset by unobserved second-order substitution among subsequent owners.

Figure 8 provides a graphical decomposition of the estimated change in gasoline consumption from driving behavior and fuel intensity, separately for all vehicles (top panel), newly leased vehicles (middle panel), and retained vehicles (bottom panel). Following the decomposition of gasoline demand in equation (D3), each component represents the product of the estimated regression discontinuity coefficient and the corresponding pre-treatment mean. The total effect on gasoline consumption (dark gray bars) is decomposed into changes attributable to driving behavior (red bars) and fuel intensity (light gray bars).

For newly leased vehicles, the reduction in gasoline consumption of approximately 83 liters per year is driven roughly equally by two mechanisms: lower fuel intensity—reflecting substitution toward cleaner vehicles such as PHEVs and BEVs—and reduced vehicle kilometers traveled. The fuel intensity effect captures the adoption of more fuel-efficient vehicles, while the driving behavior effect reflects reduced usage of these newly leased vehicles.

In contrast, retained vehicles exhibit an increase in gasoline consumption of approximately 86 liters per year, reflecting both increased driving and a rise in fuel intensity. This pattern is consistent with households reallocating kilometers from newly adopted PHEVs to their existing fossil-fuel vehicles, while also shifting usage toward relatively less fuel-efficient vehicles within the retained fleet. Because these are existing vehicles, the change in fuel intensity reflects compositional or usage shifts rather than changes in underlying vehicle technology.

At the household level (top panel), these opposing forces—reduced gasoline con-

sumption from new vehicles and increased consumption from retained vehicles—largely offset each other, yielding only a modest net increase in total gasoline consumption. The decomposition illustrates that while the subsidy successfully induces adoption of cleaner vehicles and reduces their fuel intensity, substitution in driving patterns across the household vehicle portfolio substantially erodes the potential gasoline savings. This finding highlights the importance of accounting for multi-vehicle household responses when evaluating the effectiveness of vehicle subsidy policies.

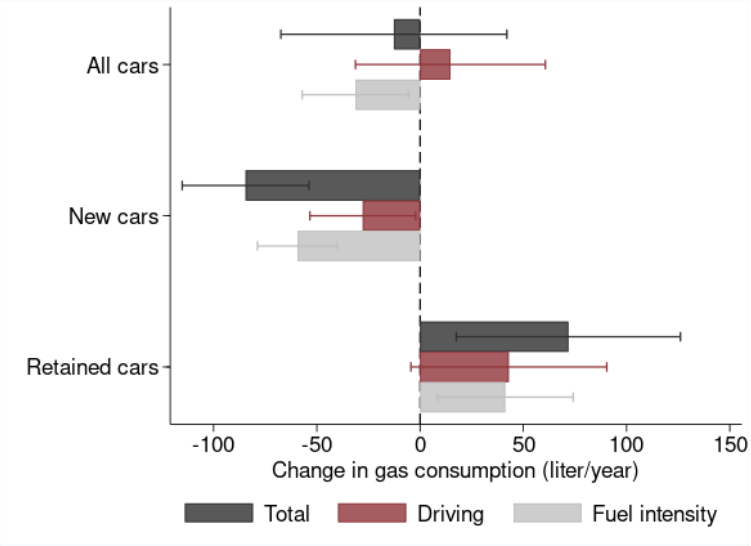


Figure 8: Decomposition of gasoline demand changes

Notes: This figure decomposes the estimated change in household gasoline consumption following the January 2020 subsidy increase into components attributable to driving behavior (red bars) and fuel intensity (light gray bars), with total effects shown in dark gray bars. Each component is calculated as the product of the estimated RD coefficient and the pre-treatment mean, following the decomposition in equation (D3). The top panel shows total household effects across all vehicles, the middle panel shows effects for newly leased vehicles only, and the bottom panel shows effects for retained vehicles only. Horizontal lines represent 95 percent confidence intervals constructed using robust standard errors. All outcomes are measured in liters per year using the calendar-year comparison.

B. Financial cost of feebate system

Evaluating the fiscal cost per marginal adopter under the January 2020 reform requires estimating two key policy effects: the change in average subsidy received per new vehicle ($\Delta\gamma^{\text{subsidy}}$), and the share of subsidy-induced PHEV adoption (ε).

Average subsidy. Figure 9 illustrates the average subsidy per new car among households at lease renewal around the January 2020 reform, and Table 3 column (2) reports the underlying regression coefficient capturing the discontinuity in subsidy payments at

the policy threshold. The policy shows a clear discontinuity in the average subsidy at the policy cutoff and we estimate that the reform increased the average subsidy per new vehicle by approximately 2,066 SEK (≈ $217.3). Relative to a pre-reform baseline of roughly 1,161 SEK (≈ $122) in November 2019, This represents an 78 percent increase in the average subsidy payment. This effect captures the total fiscal impact of the reform on new vehicle subsidies, reflecting both the expansion of subsidy eligibility to a broader set of vehicles (the extensive margin) and higher subsidy amounts for eligible vehicles under the new policy regime (the intensive margin). We assume full pass-through of the subsidy to consumers — that is, the increase in the subsidy translates one-for-one into a reduction in the effective purchase price faced by households.[12]

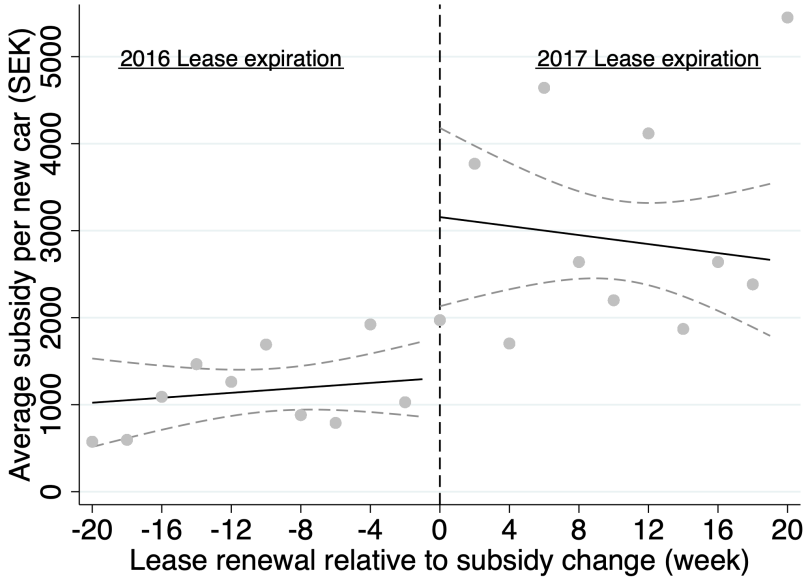


Figure 9: Average subsidy per new car

Notes: The figure illustrates the average subsidy per new car among households at the three year lease renewal. The data is grouped into biweekly intervals surrounding the 2020 policy change. The dashed vertical lines indicate the subsidy adjustment implemented in January 2020, with the time of the policy change normalized to zero.

Marginal adopter. Table 3 column (1) reports the first-stage estimate of the policy's effect on the market share of subsidized vehicles, which increased by 4.3 percentage points following the reform. Relative to the pre-reform baseline of 2.3 percent in November 2019, this implies that the total post-reform PHEV market share rose to 6.6 percent. Using

12. To assess the validity of the full pass-through assumption, we estimate equation (B3) using a balanced panel of car models in 2019 and 2020 (Swedish Tax Agency 2019, 2020). We find that listing prices of subsidy-eligible vehicles declined slightly relative to non-eligible vehicles following the reform. This is consistent with full — or even more than full — pass-through of the subsidy to consumers.

the formula $\varepsilon = \Delta\gamma^{\text{share}}/(\Delta\gamma^{\text{share}} + \gamma^{\text{pre}})$, we compute the share of marginal adopters as $\varepsilon = 4.3/(4.3 + 2.3) = 0.65$. This means that 65 percent of post-reform PHEV adopters switched to a PHEV because of the subsidy (marginal adopters), while the remaining 35 percent would have adopted a PHEV even in the absence of the policy (inframarginal adopters). The marginal adopter share scales the fiscal cost in equation (10): because only 65 percent of subsidy recipients generate additional emissions reductions, the cost per marginal adopter is $1/0.65 \approx 1.54$ times the average subsidy payment. This implies that the cost per marginal adopter is $\Delta\gamma^{\text{subsidy}}/\varepsilon = 2{,}066/0.65 = 3{,}178$ SEK ($\approx$ \$334) per new vehicle purchased. Note that this is the fiscal cost per marginal adopter among all new vehicle purchases at lease renewal, not per PHEV specifically, as the subsidy applies to all households renewing their lease regardless of which vehicle type they ultimately adopt. This estimate represents the key fiscal cost parameter we use to evaluate the cost-effectiveness of the PHEV subsidy per ton of abated CO_2 emissions.

In contrast, the cost per new PHEV car is substantially higher. Column (3) of Table 3 reports the 2SLS estimate, computed as the ratio of the reduced-form effect on average subsidy (column 2) to the first-stage effect on PHEV market share (column 1). The implied fiscal cost to the government per additionally subsidized PHEV is 48,433 SEK (approximately \$5,095). This figure is somewhat above the estimate of Nilsson et al. (2024), who find a fiscal cost of approximately €2,331 per additionally subsidized alternative-fuel vehicle (in 2006 prices), equivalent to roughly \$3,089 in 2020 dollars.[13] The higher cost per PHEV in our setting likely reflects the more generous subsidy levels under the 2020 policy.

Table 3: Effect on market share and average subsidy

	First stage	Reduced form	
	(1) Share of subsidy cars	(2) Average subsidy	(3) 2SLS estimate
Post-subsidy lease renewal	0.043***	2,060***	48,433***
	(0.011)	(565)	(3,724)
Mean (t-1)	.023	1059.88	1059.88

Notes: This table reports the first-stage effect of the policy change on the market share of subsidized vehicles (Column 1), the reduced-form effect on the average subsidy received per household (in SEK) (Column 2), and the implied cost per additionally subsidized vehicle (in SEK), computed as the ratio of the reduced-form to the first-stage estimate (Column 3). The mean dependent variables are reported below the coefficients. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We exclude households with lease renewals three weeks before and after the policy change in January 2020. We use triangular kernel weights. The unit of observation is household×year.

13. We inflate the original estimate from 2006 to 2020 prices by dividing by 0.846 using the Swedish Consumer Price Index, and convert to US dollars using the EUR/USD exchange rate of 1.1216 on January 1, 2020.

C. Mechanisms

The retained-fleet response documented above could operate through two distinct margins, which our reduced-form design cannot fully separate. On the *intensive margin*, treated households may retain a vehicle of similar type but shift its usage or composition toward a dirtier car within an unchanged portfolio size — for instance, the treated household may keep a less efficient SUV relative to the control households who retain a more fuel-efficient sedan. On the *extensive margin*, treated households may simply retain *more* vehicles than the control households, mechanically adding a car's worth of fuel intensity and driving to the household total. Because both margins move fuel intensity and driving of retained cars in the same direction, our design cannot cleanly attribute the retained-fleet effect to one margin versus the other.

We can, however, provide direct evidence on the extensive margin. Figure 10 reports the effect of the subsidy reform on the number of vehicles retained by the household. Treated households retain 0.06 more vehicles than control households, indicating that at least part of the retained-fleet response operates through an expansion of portfolio size rather than through recomposition of a fixed-size fleet alone.

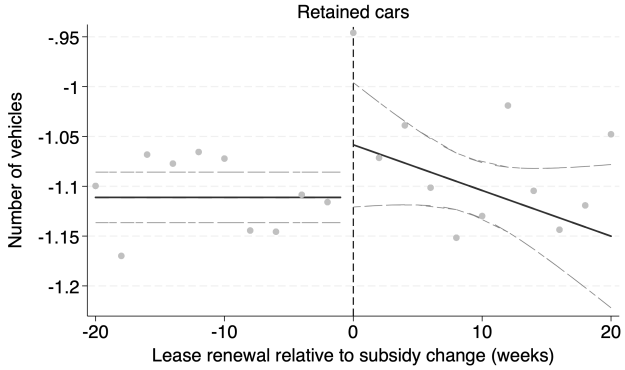


Figure 10: Effect on number of retained cars

Notes: The figure illustrates the change in the number of retained vehicles between 2018 and 2021 among households at the three year lease renewal. The data is grouped into biweekly intervals surrounding the 2020 policy change. The dashed vertical lines indicate the subsidy adjustment implemented in January 2020, with the time of the policy change normalized to zero.

D. Robustness

We assess the robustness of our main results through several alternative specifications that address potential threats to identification and alternative methods of controlling for confounding factors.

No "donut" specification. As a robustness check, we test whether our results are sensitive to the exclusion of observations near the cutoff. While our main specification excludes households with lease renewals near the policy cutoff in January 2020 to address potential manipulation concerns, we find that including these near-cutoff observations yields qualitatively similar results. Table C5 presents estimates without the donut restriction. Although the sample of compliers increases slightly and point estimates are somewhat smaller in magnitude, the overall pattern of results remains unchanged: subsidized PHEV adoption significantly increased gasoline consumption from retained vehicles.

Lease-timing controls. Our baseline specification compares gasoline consumption in the same calendar years (2018 and 2021) across treated and control households. This approach addresses the concern that treated and control households may differ slightly in the duration they retain their vehicles, as lease renewal dates vary across groups. While controlling for lease-year-month fixed effects may ensure that any differences in vehicle age or replacement timing do not confound our estimates (Table C6), we find no meaningful difference in our main results using the RD design with covariates (Calonico et al. 2019).

Bandwidth choice. In addition to assessing the robustness of our results to different identification strategies, we further examine the sensitivity of our key findings to the length of the time window (i.e., the number of weeks included before and after the reform). We first examine the robustness of our main results to the choice of window length. A window that is too short may fail to capture meaningful shifts in vehicle choices following the policy change and limits the number of comparable lease renewal periods. Conversely, a window that is too long may smooth over changes in vehicle supply, which can introduce shifts in market shares. Figure C2 reports the regression discontinuity estimates for gasoline demand (Panel A), vehicle kilometers traveled (Panel B), and fuel intensity (Panel C) as the window length varies from 8 to 40 weeks. Each panel displays the point estimates for newly leased vehicles and retained vehicles, along with their corresponding 95 percent confidence intervals. With the exception of the narrowest windows, the estimated effects are remarkably stable across window lengths. In particular, the estimates converge quickly and remain nearly identical for window lengths of approximately 20 weeks and above, with larger windows yielding more precise estimates. This stability indicates that our results are not sensitive to the specific choice of bandwidth and supports the robustness of our main findings.

VI. Direct Cost-Effectiveness of EV Subsidies

To quantify the social cost of carbon emissions associated with the policy, as formalized in equation (12), we combine our estimated policy elasticities for subsidy costs and gasoline consumption with parameter values drawn from the Swedish vehicle registry. Using data on retired vehicles, we estimate the average vehicle lifespan in Sweden to be approximately 18 years. Future gasoline savings are discounted to present value using an annual discount rate of r = 5 percent. Table 4 summarizes the parameter values employed in the abatement cost calculations.

Table 4: Parameter estimates used to calculate abatement costs

Coefficient	Description	Period	Value	Source
Panel A: Registry Data				
h	Vehicle lifespan index (years)	2005-2023	18	Vehicle registry
e	Energy efficiency of electric engine (kwh/km)	2019	0.25	Vehicle registry
ϑ	Share of driving on electric drivetrain	2019	0.3	Vehicle registry
Panel B: Policy Coefficients				
ΔG	Change in annual gasoline demand (liters)	2018/2021	-12.7	Table 2
ΔKM^e	Change in electric KMs	2018/2021	528	Table C4
$\Delta\gamma^{\text{subsidy}}$	Change in the average subsidy per new car	2020	217.3	Table 3
$\Delta\varepsilon$	Share of marginal adopter	2020	0.65	Table 3
Panel C: Parameter assumptions				
r	Annual discount rate		0.05	
CI^{grid}	Carbon intensity of grid (renewables, gas, or coal)		(0, 0.25, 0.45)	

Under the assumption that electric vehicle charging does not generate additional carbon emissions – corresponding to a scenario in which renewable generation is on the margin – the implied cost per ton of carbon abated (CTA) under the renewables scenario can be calculated as follows:[14]

$$CTA^{renewable} = \sum_{h=1}^{18} \frac{\frac{\$217.3}{0.65}}{\frac{1}{(1+0.05)^h} \cdot \left(2.3 \frac{\text{kg CO}_2}{1} \cdot 12.7 \frac{1}{\text{year}} \cdot \frac{1\,\text{t}}{1000\,\text{kg}}\right)}$$

14. For our baseline CO_2 calculations, we assume zero emissions from the electric portion of PHEV use, focusing only on end-of-pipe emissions and excluding upstream emissions from electricity generation. Given Sweden's predominantly renewable electricity mix, marginal emissions from vehicle charging are effectively zero (Morfeldt et al. 2021).

$$= \frac{\$334}{0.3415\,\text{t CO}_2} \approx \$978 \text{ per ton CO}_2$$

The estimated cost of carbon abatement implied by the policy is approximately $1,061 per ton of CO_2. Relative to a benchmark social cost of carbon of $126 per ton, this implies that the policy would require a vehicle lifespan of 151 years to break even on climate benefits alone—more than four times the average vehicle lifespan of 18 years.

Even when evaluated using the upper bound of the 95 percent confidence interval for gasoline reduction (67 liters per year), which translates to 1.09 metric tons of CO_2 abated over the discounted vehicle lifespan, the policy cost per ton remains $199.4. This estimate marginally exceeds the social cost of carbon benchmark of $126 per ton in Sweden and is also above the most recent Environmental Protection Agency estimate of the social cost of carbon of $190 per ton, suggesting the policy is not even cost-effective even under the most optimistic scenario for emissions reductions. Under dirtier electricity mixes—where the marginal emissions from charging PHEVs are larger than zero—the climate benefits would be further diminished, making the policy even more cost-ineffective. Thus, even the most favorable interpretation of our results indicates that PHEV subsidies fail to deliver cost-effective climate benefits. We further back out the confidence with which we can reject a cost of CO_2 abatement below $200 per ton and find that this threshold is rejected at exactly the 5 percent significance level.

We can also evaluate the policy using only the gasoline reduction from new vehicles, excluding the offsetting effect of retained fossil fuel vehicles. As documented in Table 2, the gasoline demand reduction from new cars alone equals 84.5 liters per year, which translates to 1.36 metric tons of CO_2 abated over an 18-year discounted vehicle lifespan. This yields a cost per ton of $159—nearly cost-effective relative to the social cost of carbon of $126 per ton. This is close to Cole et al. (2023), who estimate costs of approximately $95 per ton of CO_2 abated from the Inflation Reduction Act of 2022 (IRA). However, when we account for this retention effect, the net emissions reduction falls substantially, pushing the cost per ton to $1,061—more than four times the social cost of carbon. This comparison underscores an important methodological point: evaluating vehicle subsidies without accounting for their effect on the existing vehicle fleet can yield systematically biased and overly optimistic estimates of cost-effectiveness.

We further assess the sensitivity of these estimates to assumptions about the marginal electricity generation mix. Using a carbon intensity of 0.45 tCO_2/MWh for natural gas–fired generation (U.S. Environmental Protection Agency 2023) and 0.91 tCO_2/MWh for coal-fired generation (International Energy Agency 2023) and subtracting the additional emissions from EV charging, the implied cost of abatement rises to $2,719 per ton under

a natural gas scenario and becomes negative under a coal scenario. These values substantially exceed standard estimates of the social cost of carbon, indicating that, once charging-related emissions are taken into account, the subsidy is unlikely to be cost-effective on emissions grounds alone.

A. Bootstrap validation of abatement cost estimates

To validate the robustness of our findings with respect to our sample, we complement our main estimates with a bootstrapped analysis of the three policy coefficients required to compute the CTA: the change in gasoline demand, the average subsidy, and the share of marginal adopters. Figure 11 plots the empirical sampling distributions of these coefficients, obtained from 2,000 bootstrap draws with replacement.

The bootstrapped distributions are reassuring: the means and standard deviations of all three coefficients closely mirror their point estimates from the main sample, confirming that our results are not driven by any particular set of observations. We then use each set of bootstrapped coefficients to compute the implied CTA following equation (12).[15] Across the 2,000 draws, only 99 yield a CTA below \$200 per ton of CO_2 (4.95 percent), allowing us to reject a cost-effectiveness threshold of \$200 per ton at the 5 percent significance level. This result reinforces our main conclusion: once the retention effect on the existing vehicle fleet is accounted for in the gasoline demand estimates, the implied abatement cost substantially exceeds benchmarks reported in the literature, and this finding is robust to sampling variation in the underlying coefficients.

15. A standard error for the CTA cannot be directly computed from the bootstrap draws. Approximately one third of the bootstrapped gasoline demand estimates are positive, implying a negative CTA, and a substantial share of the remaining estimates are very small in magnitude, yielding extremely large CTA values as the denominator approaches zero. Together, these features cause the standard errors of the bootstrapped CTA distribution to diverge.

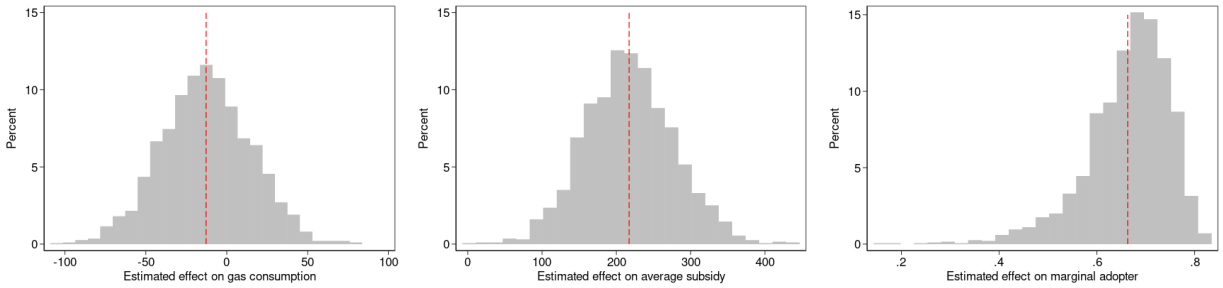


Figure 11: Bootstrapped distribution of RD estimates

Notes: This figure displays the bootstrapped sampling distributions of the RD estimates, obtained using calendar year comparisons, for all policy coefficients required to compute the CTA: for gasoline demand changes (Panel A), average subsidy (Panel B), and the share of marginal adopters (Panel C). Estimates are derived from 2,000 bootstrap draws with replacement. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We use triangular kernel weights. The unit of observation is household×year.

VII. Concluding Remarks

This paper provides causal evidence on how households adjust across an entire portfolio of durable goods in response to subsidy-based climate policy. Exploiting a policy-induced increase in Swedish PHEV subsidies and predetermined lease renewal timing, we show that while subsidies shift new vehicle choice toward cleaner models, households simultaneously reallocate driving toward retained gasoline vehicles. The net effect on gasoline consumption is small and statistically indistinguishable from zero.

For regulators, the central implication is that technology subsidies do not replicate the incentive structure of Pigouvian pricing. Because EV subsidies lower the cost of acquiring clean vehicles without raising the marginal cost of driving dirty ones, households can substitute across assets in ways that attenuate emissions reductions. Adoption rates of clean technology therefore provide an incomplete and potentially misleading measure of policy performance.

For researchers and modelers of the energy transition, the results underscore the importance of embedding empirically disciplined substitution patterns into quantitative frameworks. Policies that target discrete technologies operate in sub-optimal environments where unpriced margins remain active. More generally, in any setting where agents allocate effort or utilization across multiple assets – e.g. housing, agricultural inputs, education, or health investments –interventions that fail to price the underlying externality may reshuffle costs rather than reducing them. The contrast between subsidy-based policy

and Pigouvian pricing is not merely conceptual; it has first-order implications for projected emissions and welfare.

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Appendix

Clean Cars, Dirty Margins: Portfolio Responses to Electric Vehicle Subsidies

Teevrat Garg *David Rapson* *Sebastian Tebbe*

A. Additional Summary Statistics

Table A1: Sample characteristics

	Our sample		Household with new car		Swedish household	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
A. Demographic characteristic						
Age	51.89	13.47	51.77	14.84	54.16	16.59
Female	0.43	0.44	0.35	0.41	0.36	0.44
Total income (1,000 SEK)	406.82	290.55	325.54	340.42	220.31	243.13
Self-employed	0.07	0.23	0.08	0.22	0.07	0.21
Unemployment days	3.17	23.23	4.44	20.87	7.99	33.79
Married	0.62	0.48	0.58	0.44	0.36	0.45
Years of education	12.90	2.23	12.76	2.13	12.15	2.43
B. Vehicle characteristic						
Number of cars	1.48	0.73	1.90	22.77	1.12	4.77
Vehicle kilometers traveled	18,753	8,558	15,176	72,095	10,857	18,996
Fuel intensity (l/100km)	5.28	1.13	4.72	2.08	6.20	1.76
Gasoline demand (l)	989.65	526.11	771.52	4581.45	630.29	1189.45
N(Observation)	6,028		145,994		4,831,807	

Notes: The table presents 2021 summary statistics for socio-demographic characteristics (Panel A) and vehicle attributes (Panel B) across three groups: households in our study sample, households that adopted a new vehicle, and the average Swedish household.

Table A2: Car characteristics by treatment group

	Treated car		Counterfactual car		New car	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
A. Car characteristic						
Gasoline demand (l)	695.79	215.48	722.40	232.29	705.49	395.27
Vehicle kilometers traveled	13,996	2,851	13,884	3,218	13,572	6,492
Fuel intensity (l/100km)	4.95	1.19	5.16	1.23	5.20	1.76
N(Observation)	2,665		4,023		388,057	

Notes: This table reports the average characteristics of newly registered vehicles in 2021 for three groups: households whose leases expired in 2020 (treated group), households with lease expiration in 2019 (counterfactual group), and the full population of newly registered vehicles during the corresponding period (new car).

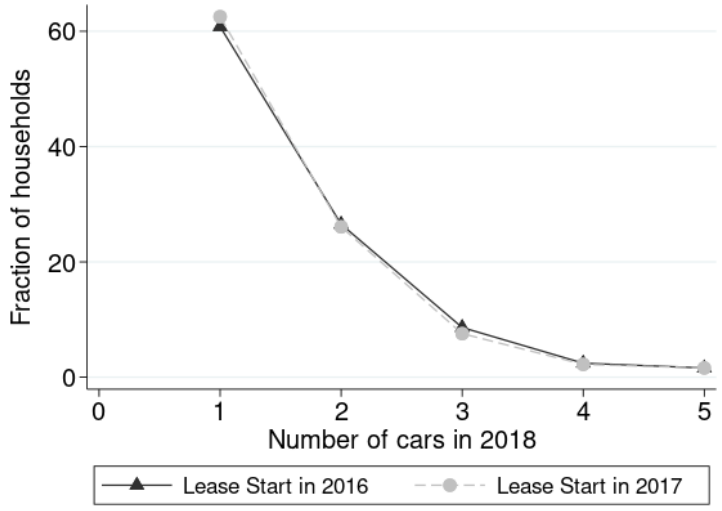
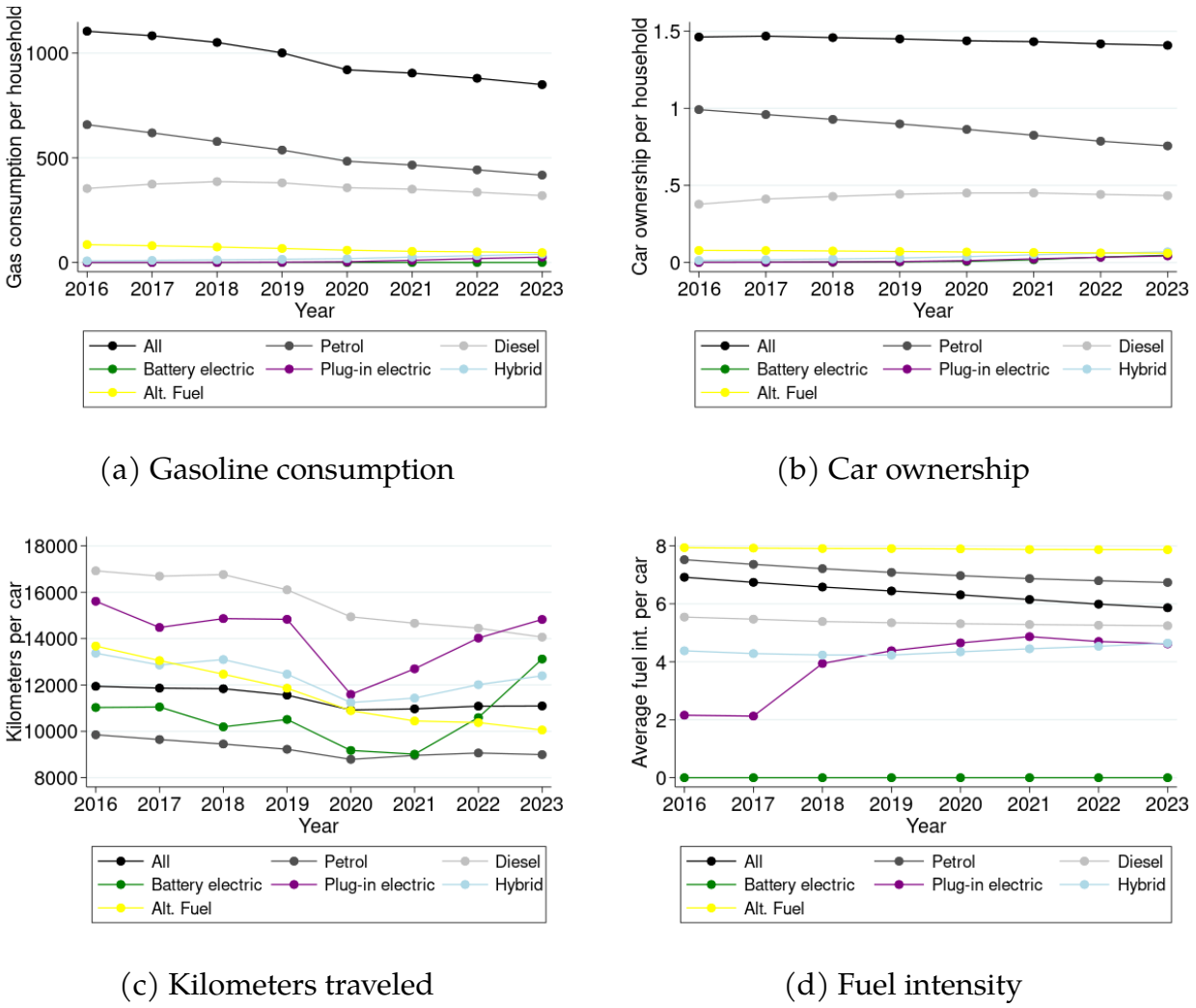


Figure A1: Distribution of number of vehicles

Notes: This figure presents the pre-treatment distribution of the number of vehicles owned by control (dark gray) and treated (light gray) households.



(a) Gasoline consumption

(b) Car ownership

(c) Kilometers traveled

(d) Fuel intensity

Figure A2: Gasoline demand trends

Notes: This figure plots household gasoline consumption and its three multiplicative components between 2016 and 2023, separately by vehicle fuel type. Panel (a) shows average gasoline consumption per household (liters per year). Panel (b) shows the average number of cars owned per household. Panel (c) shows average annual kilometers traveled per vehicle. Panel (d) shows average fuel intensity per vehicle (liters per 100 km). Within each panel, series are shown for all vehicles (black), petrol (gray), diesel (light gray), battery electric (green), plug-in hybrid (purple), hybrid (light blue), and alternative fuel (yellow). The sample comprises all private passenger vehicle owners in Sweden. Gasoline consumption (Panel a) is the product of vehicle count (Panel b), kilometers traveled (Panel c), and fuel intensity (Panel d), aggregated across the household fleet.

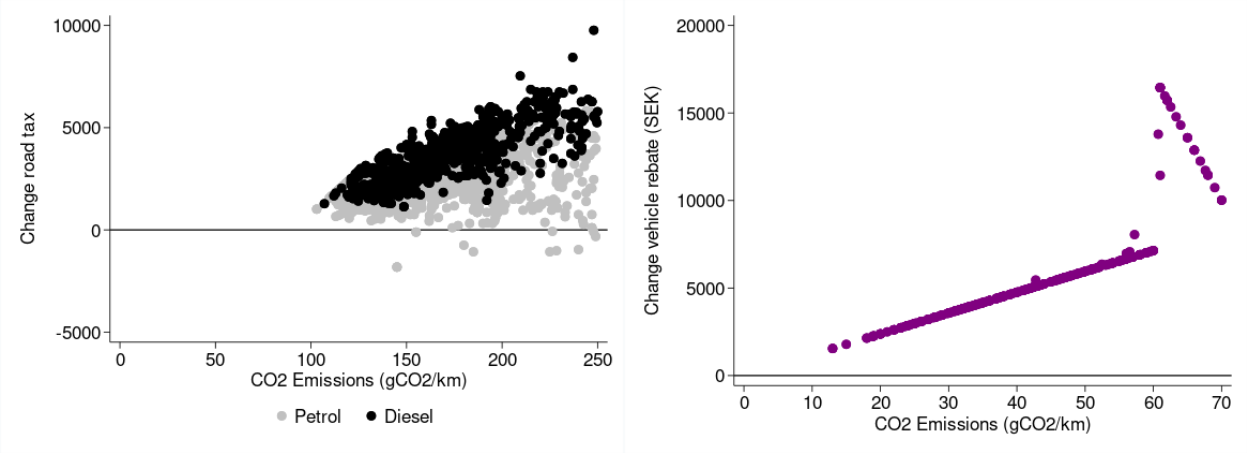


Figure A3: Change in road tax and vehicle rebate by CO_2 emissions

Notes: This figure plots the change in annual road tax (left panel) and the change in vehicle rebate for each available model in Sweden (right panel) as a function of vehicle CO_2 emissions (g/km) following the January 2020 policy reform. The left panel distinguishes petrol (gray) and diesel (black) vehicles. The right panel plots the change in vehicle rebate (SEK) for PHEVs eligible under the revised bonus-malus schedule. Values are calculated using the pre- and post-reform road tax and rebate formulas described in Section A.

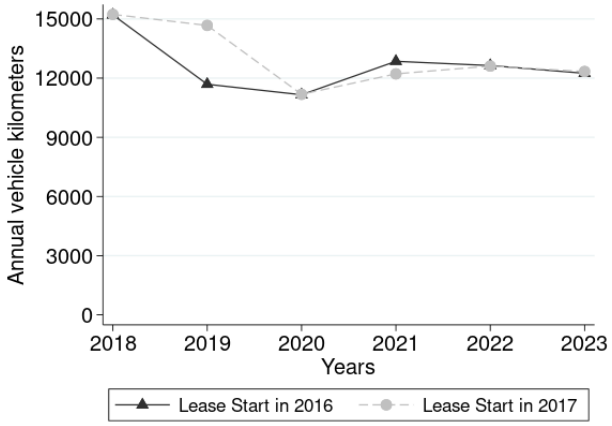


Figure A4: Kilometers traveled of exiting cars

Notes: The figure shows annual vehicle kilometers traveled between 2018 and 2023 for vehicles that exited the household at the three-year lease renewal between 2019 and 2020. Treated households are those whose lease renewed after the January 2020 subsidy reform; control households are those whose lease renewed before the reform.

B. Additional estimations

B.1. Swedish odometer readings

The annual vehicle kilometers traveled data in the register is derived from odometer readings collected by the Swedish Transport Agency (*Transportstyrelsen*) during mandatory vehicle inspections and periods of deregistration. To calculate annual vehicle usage, at least one inspection must have taken place between January 1st of the reference year and January 31st of the following year. Inspection data is matched to the Road Traffic Register (*Vägtrafikregistret*) using registration numbers. The odometer readings from inspections are then paired, forming *odometer pairs*, which consist of two consecutive readings, with at least one recorded after January 1st of the reference year. An odometer reading is typically recorded during a regular inspection but can also originate from the registration date, where vehicle kilometers are set to zero.

A vehicle may have multiple odometer pairs in a given year if it has been inspected at least twice during that period. If no prior inspection exists, an odometer pair is created using the first registration date as the initial inspection date and an odometer reading of zero. For a detailed description, see *Mileage Database.*

Using odometer pairs, the annual vehicle kilometers traveled (KM) are estimated as:

$$\text{KM} = \underbrace{\left(\frac{\text{Odometer}_{t+1} - \text{Odometer}_{t}}{\text{Operational Days}}\right)}_{\text{Daily Usage}} \times \text{Days}. \tag{B1}$$

where $Odometer_{t+1}$ and $Odometer_t$ refer to the odometer reading at the second and first inspection, $Days$ are the number of days the vehicle was in operation between inspections. The number of operational days are calculated as the total number of days between inspections minus any periods when the vehicle was deregistered. To determine the total usage for a given year, the daily mileage is multiplied by the number of days within that year.

For vehicles without an inspection during the reference year, and thus lacking a valid odometer pair, vehicle usage is estimated using similar vehicle models with vehicle usage data and that have been inspected. Between 25 percent of reported mileage data for a given reference year is based on estimates. Newly registered vehicles typically do not require inspection until three years after registration for passenger cars.

B.2. PHEV utility factor adjustment

For PHEVs, the share of kilometers driven on the electric drivetrain is not directly observed in administrative data. Instead, manufacturers report an official *Utility Factor* (UF)

under the Worldwide Harmonized Light Vehicles Test Procedure (WLTP), which maps a vehicle's certified charge-depleting range to the fraction of driving assumed to occur in electric mode. These WLTP utility factors are used for regulatory compliance and emissions certification and are based on standardized test-cycle assumptions.

Formally, the WLTP utility factor is defined as

$$UF(R_{\text{CDC}}, d_n) = 1 - \exp\left[-\sum_{i=1}^{10} c_i \left(\frac{R_{\text{CDC}}}{d_n}\right)^i\right], \tag{B2}$$

where R_{CDC} denotes the WLTP-certified charge-depleting range in kilometers, $d_n = 800$ km is the normalization parameter specified in European regulation, and $\{c_i\}_{i=1}^{10}$ are fixed coefficients defined by the regulatory framework.

While these WLTP-based utility factors represent official manufacturer-reported values, a growing body of evidence shows that they overstate real-world electric driving for PHEVs. In practice, PHEV users tend to drive a smaller share of kilometers in electric mode than implied by WLTP test cycles, due to charging constraints, trip patterns, and user behavior.

To account for this discrepancy, we adjust the WLTP utility factors following the approach proposed by the International Council on Clean Transportation (ICCT). Specifically, we preserve the regulatory functional form of the utility factor but replace the WLTP normalization parameter with an empirically calibrated value, $d_n^{\text{adj}} = 4{,}260$ km, which reflects observed PHEV driving behavior in real-world data.

Figure B1 illustrates how this ICCT adjustment reduces the implied share of electric driving relative to the WLTP-based utility factor, particularly for PHEVs with longer certified electric ranges. Throughout the analysis, we use these ICCT-adjusted utility factors to allocate PHEV vehicle kilometers traveled between electric and gasoline driving. This approach ensures that our fuel-use and emissions calculations are grounded in observed usage patterns rather than regulatory test-cycle assumptions.

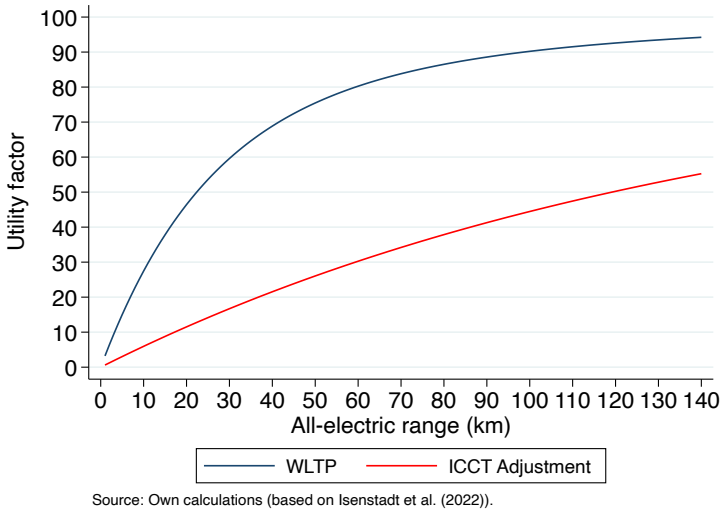


Figure B1: PHEV usage by range

Notes: WLTP refers to the Worldwide Harmonized Light Vehicles Test Procedure, providing the official driving range and utility factors. The ICCT adjustment reflects observed PHEV driving patterns, so we adjust the WLTP-based PHEV utility factors to align with actual usage.

B.3. Listing price pass-through analysis

To examine to what extent the January 2020 bonus-malus reform was passed through into listing prices, we construct a balanced panel of car models observed in both 2019 and 2020 using the Swedish Tax Agency (*Skatteverket*) new car price lists (Swedish Tax Agency 2019, 2020). Listing prices are deflated to 2020 SEK using the Swedish CPI. We retain only models present in both years, yielding a balanced two-period panel. We estimate the following difference-in-differences specification:

$$P_{jt} = \alpha + \beta \left(\text{Subsidy}_j \times \text{Post}_t \right) + \gamma \, \text{Subsidy}_j + \delta \, \text{Post}_t + \varepsilon_{jt} \tag{B3}$$

where P_{jt} is the CPI-deflated listing price of car model j in year $t \in \{2019, 2020\}$. Subsidy_j is a time-invariant indicator equal to one if model j is bonus-eligible under the bonus-malus system, and zero otherwise. Post_t is an indicator equal to one for the post-reform year 2020 and zero for 2019. The coefficient of interest, β, captures the change in listing prices for bonus-eligible vehicles relative to non-eligible vehicles following the reform. A positive β would indicate that manufacturers partially capture the subsidy through higher pre-subsidy prices (incomplete pass-through to consumers), while $\beta = 0$ would be consistent with full pass-through to consumers. Standard errors are clustered at the car model level.

C. Additional Figures and Tables

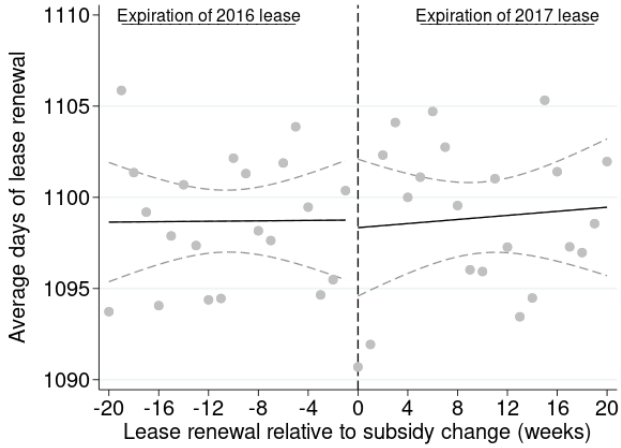


Figure C1: Number of days between second and first lease

Notes: The figure shows the average number of days between the lease renewal date and the date of the lease initiation for households who adopt a new vehicle at the lease renewal around the 2020 policy change. Dashed vertical lines denote the subsidy change of January 2020 (normalized to 0).

Table C1: Gasoline demand from renewal and non-renewal cars

	Vehicle type			
	(1) All cars	(2) Renewal	(3) Non-renewal	(4) Retained cars
A. Gasoline Demand (liter)				
Post-subsidy lease renewal	-12.70	-82.49***	-2.01	71.80**
	(27.93)	(14.43)	(7.09)	(27.70)
Control Pre-Mean (2018)	1042	0	0	1042
B. Driving behavior (km)				
Post-subsidy lease renewal	277.4	-476.5**	-30.2	811.1*
	(442.6)	(196.9)	(146.6)	(456.8)
Control Pre-Mean (2018)	19822	0	0	19822
C. Fuel intensity (liter/100km)				
Post-subsidy lease renewal	-0.16**	-0.38***	-0.07	0.21**
	(0.07)	(0.06)	(0.52)	(0.08)
Control Pre-Mean (2018)	5.3	.	.	5.3

Notes: This table reports the RD estimates using calendar year comparisons for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) newly leased cars during the bandwidth period (3) new cars after the bandwidth period (July 2020), and (4) retained cars. Covariate-adjusted 95 percent confidence interval, robust p-values and the mean dependent variables are reported below the coefficients. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We exclude households with lease renewals three weeks before and after the policy change in January 2020. We use triangular kernel weights. The unit of observation is household×year.

Table C2: Gasoline demand changes by vehicle portfolio size

	Vehicle type		
	(1) All cars	(2) New cars	(3) Retained cars
A. Gasoline Demand (liter)			
Single-vehicle household	-54.11**	-77.63***	23.51
(N=4,950)	(22.17)	(16.50)	(18.68)
Two-vehicle household	-11.09	-82.59**	71.50
(N=2,118)	(62.23)	(37.83)	(62.59)
Three-vehicle household	77.33	-119.09**	196.42
(N=824)	(148.78)	(52.14)	(152.36)
B. Driving behavior (km/year)			
Single-vehicle household	-739**	-589**	-149
	(371)	(244)	(320)
Two-vehicle household	900	111	790
	(1038)	(623)	(1109)
Three-vehicle household	2147	-1257	3404
	(2257)	(975)	(2391)
C. Fuel intensity (liter/100km)			
Single-vehicle household	-.10	-.34***	.30***
	(.08)	(.07)	(.09)
Two-vehicle household	-.45***	-.56***	.20**
	(.14)	(.14)	(.08)
Three-vehicle household	.08	-.33	.20**
	(.18)	(.20)	(.08)

Notes: This table reports the RD estimates using calendar year comparisons, estimated separately for households with exactly one vehicle in 2021 and households with more than one vehicle in 2021, for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) new cars, and (3) retained cars. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We exclude households with lease renewals three weeks before and after the policy change in January 2020. We use triangular kernel weights. The unit of observation is household$\times$year.

Table C3: Number of car changes by initial vehicle portfolio size

	Vehicle type		
	(1) All cars	(2) New cars	(3) Retained cars
A. Number of cars			
Single-vehicle household	0.01	-0.02	0.04
	(0.03)	(0.01)	(0.03)
Two-vehicle household	.02	.01	.01
	(.07)	(.04)	(.07)
Three-vehicle household	.29*	-.07	.36**
	(.15)	(.08)	(.15)

Notes: This table reports the RD estimates using calendar year comparisons, estimated separately for households with exactly one vehicle in 2021 and households with more than one vehicle in 2021, for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) new cars, and (3) retained cars. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We exclude households with lease renewals three weeks before and after the policy change in January 2020. We use triangular kernel weights. The unit of observation is household$\times$year.

Table C4: Effect on vehicle usage by fuel type

	Vehicle type		
	(1) Plug-in	(2) Fossil	(3) Hybrid
A. Vehicle Ownership			
Post-subsidy lease renewal	0.037***	0.029	-0.074
	(0.014)	(0.037)	(0.523)
Control Pre-Mean (2018)	0.004	1.447	–
B. Vehicle Kilometers			
Post-subsidy lease renewal	528.1***	-269.1	18.4
	(180.3)	(501.3)	(253.3)
Control Pre-Mean (2018)	50.9	18833.8	937.4

Notes: This table reports the RD estimates using calendar year comparisons for changes in annual vehicle kilometers traveled, separately by fuel types: (1) PHEVs and BEVs (2) petrol and diesel cars, and (3) hybrid and alternative-fuel cars. The mean dependent variables are reported below the coefficients. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We exclude households with lease renewals three weeks before and after the policy change in January 2020. We use triangular kernel weights. The unit of observation is household×year.

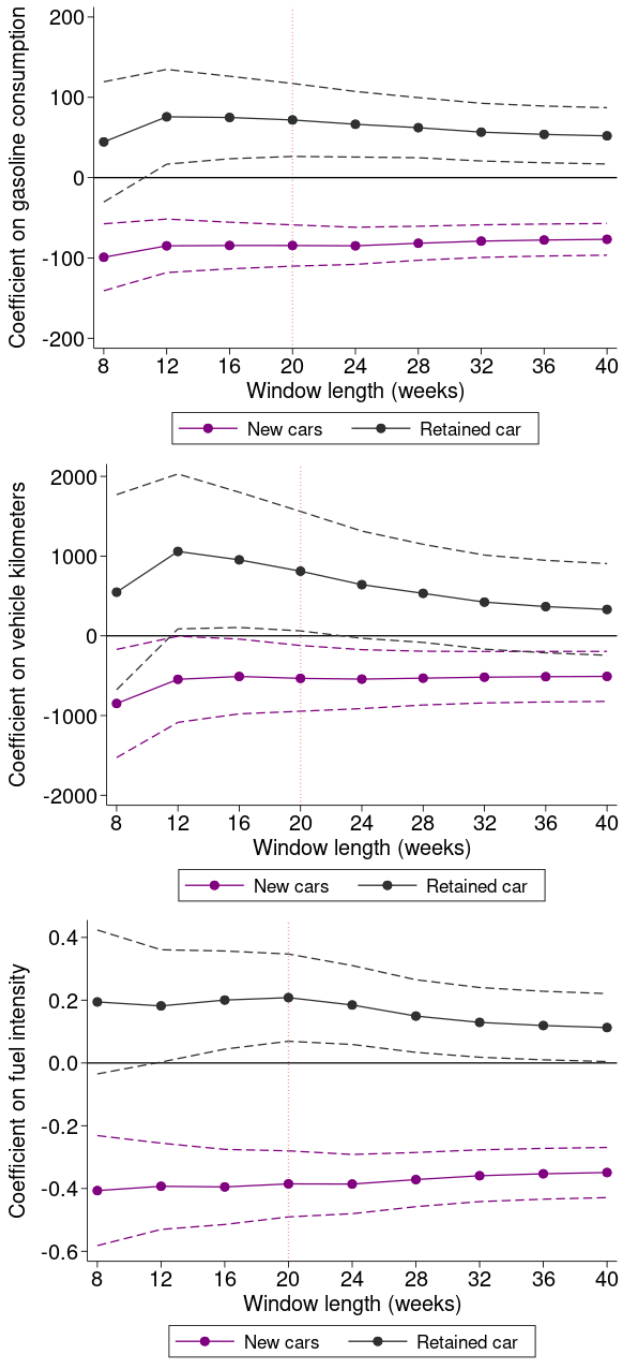


Figure C2: Coefficient estimates by window length

Notes: The figure show the regression discontinuity estimate on the gasoline demand (Panel A), kilometers travelled (Panel B), and fuel intensity (Panel C) for new (purple) and retained cars (black) bu window length. Red dashed vertical lines denote the current window.

Table C5: Gasoline demand using no donut restriction

	Vehicle type		
	(1) All cars	(2) New cars	(3) Retained cars
A. Gasoline Demand (liter)			
Post-subsidy lease renewal	12.67	-76.12***	88.79
	(54.88)	(29.55)	(54.18)
Mean (t-1)	1041.56	0	1041.56
B. Driving behavior (km/year)			
Post-subsidy lease renewal	589	-705	1294
	(865)	(478)	(874)
Mean (t-1)	19822	0	19822
C. Fuel intensity (liter/100km)			
Post-subsidy lease renewal	-.10	-.33***	.40***
	(.11)	(.11)	(.15)
Mean (t-1)	5.35	5.11	1.05

Notes: This table reports the RD estimates using calendar year comparisons for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) new cars, and (3) retained cars. Covariate-adjusted 95 percent confidence interval, robust p-values and the mean dependent variables are reported below the coefficients. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We use triangular kernel weights. The unit of observation is household$\times$year.

Table C6: Gasoline demand controlling for lease-timing controls

	Vehicle type		
	(1) All cars	(2) New cars	(3) Retained cars
A. Gasoline Demand (liter)			
Post-subsidy lease renewal	15.41	-67.31	82.72
Robust 95% CI	[-71 ; 95]	[-118 ; -25]	[1 ; 166]
Robust p-value	0.769	0.003	0.047
Control Pre-Mean (2018)	1042	0	1042
B. Driving behavior (km/year)			
Post-subsidy lease renewal	821.20	-305.69	811.07
Robust 95% CI	[-284 ; 2322]	[-1104 ; 402]	[-262 ; 2443]
Robust p-value	0.125	0.361	0.114
Control Pre-Mean (2018)	19822	0	19822
C. Fuel intensity (liter/100km)			
Post-subsidy lease renewal	-0.14	-0.33	0.18
Robust 95% CI	[-.397 ; -.006]	[-.551 ; -.171]	[-.093 ; .406]
Robust p-value	0.043	0.000	0.219
Control Pre-Mean (2018)	5.3	.	5.3

Notes: This table reports the RD estimates using calendar year comparisons for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) new cars, and (3) retained cars. Covariate-adjusted 95 percent confidence interval, robust p-values and the mean dependent variables are reported below the coefficients. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We exclude households with lease renewals two weeks before and after the policy change in January 2020. We control for Lease-Year Month fixed effects. We use triangular kernel weights. The unit of observation is household$\times$year.

Table C7: Gasoline demand controlling for gender

	Vehicle type		
	(1) All cars	(2) New cars	(3) Retained cars
A. Gasoline Demand (liter)			
Post-subsidy lease renewal	-25.92	-102.70	76.78
Robust 95% CI	[-162 ; 33]	[-178 ; -70]	[-36 ; 155]
Robust p-value	0.195	0.000	0.224
Control Pre-Mean (2018)	1039	0	1039
B. Driving behavior (km/year)			
Post-subsidy lease renewal	293.09	-598.10	891.19
Robust 95% CI	[-1442 ; 1651]	[-1545 ; 168]	[-785 ; 2372]
Robust p-value	0.894	0.115	0.325
Control Pre-Mean (2018)	19854	0	19854
C. Fuel intensity (liter/100km)			
Post-subsidy lease renewal	-0.27	-0.50	0.25
Robust 95% CI	[-.699 ; -.197]	[-.889 ; -.405]	[-.104 ; .464]
Robust p-value	0.000	0.000	0.214
Control Pre-Mean (2018)	5.22	.	5.22

Notes: This table reports the RD estimates controlling for gender using calendar year comparisons for gasoline demand changes (Panel A), annual vehicle kilometers traveled (Panel B) and fuel intensity (Panel C), separately for (1) all cars, (2) new cars, and (3) retained cars. Covariate-adjusted 95 percent confidence interval, robust p-values and the mean dependent variables are reported below the coefficients. We restrict the sample to a bandwidth of 20 weeks on either side of the January 2020 policy change. We exclude households with lease renewals two weeks before and after the policy change in January 2020. We control for Lease-Year Month fixed effects. We use triangular kernel weights. The unit of observation is household×year.

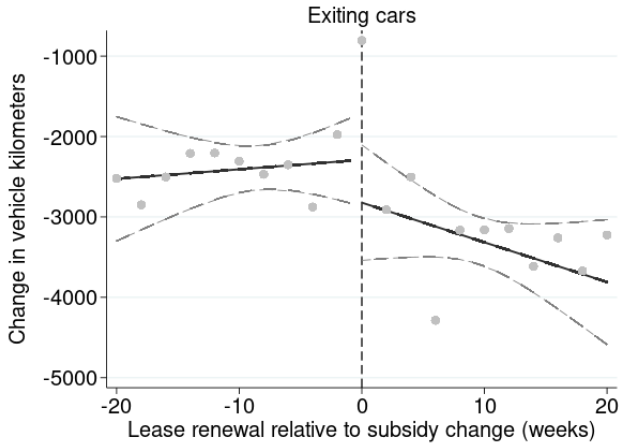


Figure C3: Effect on kilometers of exiting cars

Notes: The figure shows the change in annual vehicle kilometers traveled between 2018 and 2021 for vehicles that exited a household between 2019 and 2020, tracking their usage under the new owner. The data is grouped into biweekly intervals surrounding the 2020 policy change. The dashed vertical lines indicate the subsidy adjustment implemented in January 2020, with the time of the policy change normalized to zero.

D. Derivation of Policy Implications

D.1. Cost of policy

Let the total cost of the subsidy policy be:

$$C = J \cdot \bar{S} = M \cdot \gamma \cdot \bar{S} \tag{D1}$$

where J is the number of subsidized vehicles, M is the number of newly registered vehicles, $\gamma = \frac{J}{M}$ is the market share of subsidized vehicles, $\bar{S}$ is the average subsidy per subsidized vehicle. Both γ and $\bar{S}$ are functions of the subsidy S_j. In particular, γ is endogenous to S_j as more generous subsidies increase the adoption rate.

To analyze how the subsidy affects total cost per newly registered vehicle (divided by M), we compute the total derivative of cost per vehicle with respect to S_j:

D.2. Decomposition of gasoline consumption

A household's h total car-related gasoline demand G is equal to the fuel intensity (measured in l/100km) (F_i) multiplied by the annual vehicle kilometers traveled (KM_i), summed over all cars i in a given year t. We partition this expression into two vehicle categories: (1) retained cars ($\mathcal{K}$), and (2) new cars ($\mathcal{E}$)

$$Gas_h = \sum_{i=1}^{N} I_i \cdot F_i \cdot KM_i = \underbrace{\sum_{k \in \mathcal{K}} I_k \cdot F_k \cdot KM_k}_{\text{(1) Retained cars}} + \underbrace{\sum_{e \in \mathcal{E}} I_e \cdot F_e \cdot KM_e}_{\text{(2) New entrant cars}} \tag{D2}$$

where $I_i \in \{0, 1\}$ indicates whether car i is in the vehicle portfolio.

To measure how the subsidy change τ affect a household's gasoline demand, we differentiate the gasoline demand with respect to the subsidy change τ as follows:

$$\frac{dGas}{d\tau} = \underbrace{\sum_{k \in \mathcal{K}} \left(F_k \frac{d\, KM_k}{d\tau} + KM_k \frac{d\, F_k}{d\tau} \right)}_{\text{(1) Driving in retained cars and fuel in exiting cars}} + \underbrace{\sum_{e \in \mathcal{E}} \left(F_e \frac{d\, KM_e}{d\tau} + KM_e \frac{d\, F_e}{d\tau} \right)}_{\text{(2) Driving and fuel of new cars}} \tag{D3}$$

This formulation decomposes the total change in household gasoline demand, $\frac{dG}{d\tau}$, into two behavioral margins—*vehicle intensity* and *driving behavior*—for each of the two vehicle categories: entrants, and retained vehicles.

- **(1) Retained cars** ($\mathcal{K}$): describe adjustments in *fuel efficiency* and *driving intensity*

among continuing vehicles, corresponding to within-fleet behavioral or technological responses.

- **(2) New entrant cars** ($\mathcal{E}$): capture the effect of changes in *fuel intensity* ($dF_e/d\tau$) and *vehicle usage* ($dKM_e/d\tau$) among newly adopted vehicles entering the household fleet.

Hence, the total derivative $\frac{dG}{d\tau}$ captures four distinct margins—two (fuel intensity and driving behavior) for each of the two fleet categories—providing a decomposition of how household fuel demand responds to the feebate change. We estimate each policy coefficient empirically using our RD design.

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