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Quantifying free riders of purchase subsidies for electric cars in Germany – A forecast-based approach

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A B S T R A C T

Battery electric vehicles (BEV) are considered the central instrument in road transport decarbonization. To accelerate diffusion, many governments have introduced purchasing subsidies, yet the extent to which they induce additional adoption rather than subsidizing purchases that would have occurred regardless remains a key policy question. This study develops a forecast-based framework to quantify free ridership in Germany's Umweltbonus program, which distributed approximately 1.4 million BEV subsidies, totaling €6.9 billion between 2016 and 2023. The counterfactual is constructed by fitting Gompertz diffusion functions to BEV sales share data from two non-subsidy periods (2010 to mid-2016 and 2024 to 2025), which bracket the subsidy regime. Deviations between observed registrations and the forecast baseline during the subsidy period are interpreted as surplus sales, indicating that the implied free rider share amounts to approximately one-third of all subsidized purchases, which is lower than most prior estimates in the literature. An average subsidy of €4938 per vehicle is associated with a forecast-based surplus of about 119%. As fiscal cost-effectiveness metrics regarding the environmental protection idea pursued behind the incentive program, the cost per additional BEV was €7,485, and the cost per ton of CO_2 avoided over a ten-year vehicle lifetime was approximately €439. Higher subsidy amounts are associated with both larger surplus sales and a higher free rider share relative to the forecast baseline, highlighting a fundamental trade-off in incentive design.

1. Introduction

Since the first mass-market battery electric vehicles (BEVs) entered European markets in 2010, many countries have introduced policy packages to accelerate BEV uptake as part of their road transport decarbonization strategies. These incentives include usage privileges, expansion of fast-charging networks, and financial subsidies. Existing evidence suggests that these measures are generally associated with higher EV uptake, although estimates vary across contexts and study designs (Lieven, 2015; Rietmann and Lieven, 2019). In Germany, the Umweltbonus program supported BEV purchases from 2016 to 2023 and generated a large volume of granted subsidies, with fiscal costs in the order of billions of euros (Schlomann and Rao, 2024). Given the scale of spending, the policy debate has increasingly focused not only on whether subsidies coincide with higher adoption but also on how much adoption exceeds what would have happened independently.

As with all government spending financed by taxes, questions of effectiveness and efficiency arise. A central concern is additionality: what share of subsidized purchases would have occurred even without the subsidy (Boomhower and Davis, 2014). If, for example, a subsidy of $5000 per vehicle increases EV sales by 20% relative to a no-subsidy baseline, it is implied that 1 divided by 1.2, or 83.33%, of buyers are free riding and would have bought the vehicle anyway. Fig. 1 illustrates the situation.

In this illustrative calculation, most subsidy expenditure is interpreted as inframarginal transfers rather than as implied additional adoption. These transfers matter because they increase the cost per additional vehicle and, therefore, the implied cost of CO_2 abatement. The literature reports wide-ranging estimates of costs per induced EV, including values up to about $53,000 and averages of around $29,600 in one review (Sheldon, 2022). Assuming a new EV is driven for an average of 10 years, with an annual mileage of 13,000 km and tailpipe emissions of 130 g of CO_2 per km, it avoids 16.9 tons of CO_2. This implies a cost of $1750 per ton ($ 29,600 divided by 16.9 tons). Benchmarks for alternative abatement options can be substantially lower, for example, reported costs of capturing CO_2 from the air of $125–$335 per ton in IEA International Energy Agency (2022).

However, this type of calculation implicitly assumes a fully treated buyer population and a known counterfactual sales path in the absence of subsidies. In practice, the EV market is growing and evolving, so the key object is not a static baseline but a credible counterfactual sales trajectory without the policy. Without knowing the expected sales without subsidies, no reliable statement can be made about the proportion of free riders.

In this study, we operationalized free riding as forecast-based non-additionality, meaning subsidies paid for purchases not explained by estimated additional sales relative to a counterfactual forecast. The

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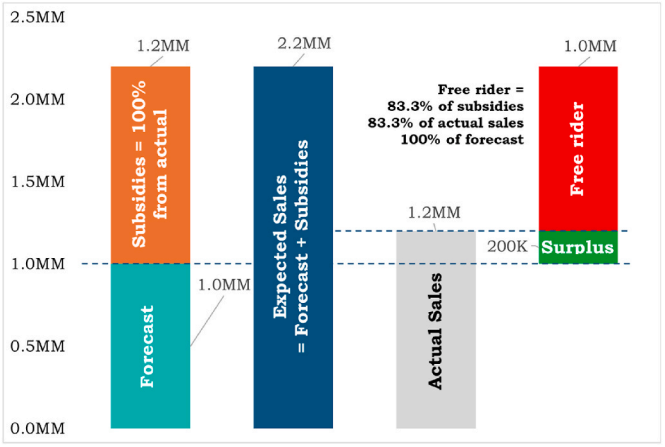


Fig. 1. Free riders when each EV buyer receives a subsidy with a 20% surplus.

contribution of this article is to answer the following questions:

Q1: How do EV sales during subsidy regimes compare to a diffusion-based, no-subsidy forecast, and how does the deviation vary by subsidy levels in terms of forecast-based surplus registrations under the subsidy regime in EV sales?
Q2: Do sales remain elevated in periods without subsidies relative to a pre-policy diffusion forecast consistent with post-program persistence (indirect or "spillover effect"; Girma et al., 2020).
Q3: What is the implied percentage of free riders depending on the level of subsidies (a) as part of all paid subsidies, (b) as part of total EV sales, and (c) as a share of the forecast baseline?
Q4: Under the scenario assumptions and as a fiscal cost-efficiency figure regarding the environmental protection idea, how do the implied cost per additional EV and the implied cost per ton of CO_2 avoided vary across subsidy amounts in the considered range?

These questions require a reference point: a counterfactual sales path without a subsidy. The empirically convenient feature of the German case is that between 2010 and 2025, there was one period of government-provided subsidies from 2016 to 2023 and two periods without subsidies, one at the beginning from 2010 to mid-2016 and one at the end from 2024 to 2025. We exploited these non-subsidy periods to estimate a diffusion-based forecast for BEV sales shares and interpret deviations during the subsidy period as forecast-based surplus registrations associated with the subsidy regime. Because the number of subsidies varied, we could also compare implied outcomes across different subsidy levels. Furthermore, comparing the post-program period with a pre-program forecast provides evidence of whether sales remain elevated after subsidies end, while recognizing that such persistence is not uniquely attributable to subsidies.

The paper's contribution is therefore a transparent accounting framework that links a diffusion-based counterfactual forecast, observed sales and subsidies, and implied additionality and free ridership metrics for Germany's BEV purchase incentives. Because the counterfactual is forecast-based, all results should be read as conditional on the maintained forecasting assumptions rather than as definitive causal effects.

2. Literature

2.1. Evidence of EV purchase incentives

To accelerate the diffusion of electric vehicles, governments use a variety of incentives. These include vehicle tax benefits, infrastructure benefits, traffic regulations (e.g., bus lane access or free parking), and, most directly, rebates for vehicle purchases (Jenn et al., 2018; Rietmann and Lieven, 2019).

The literature examining EV purchase subsidies is international and employs diverse methodological approaches, including panel regressions, difference-in-differences designs, and discrete choice models.

While the evidence generally confirms that financial incentives are associated with higher EV adoption (Table 1), the magnitude of effects has varied considerably across contexts. A consistent theme across studies is the heterogeneity of subsidy effectiveness between countries, regions, and demographic groups, with some research identifying distinct consumer segments that respond differently to incentives. Cost-effectiveness analyses revealed substantial variation in cost per induced vehicle purchase and in CO_2 abatement costs, suggesting that policy design and local market conditions significantly influenced program efficiency. In a literature review, Sheldon (2022) concluded that, while monetary subsidies are the most important incentives in EV uptake, programs are often overly expensive. However, dynamic effects and persistence mechanisms may partially offset short-run cost-effectiveness concerns.

Closely related to subsidy effectiveness is the concept of free riders. In the context of energy-efficiency programs, this term describes individuals who receive a subsidy for something they would have done anyway; that is, they are non-additional (Boomhower and Davis, 2014). Empirically, free ridership is not directly observed and must be inferred relative to a counterfactual baseline of adoption absent from the program.

Burra et al. (2024) studied free ridership for German EV subsidies and found a free-rider share of 56.5% for private cars and 87.1% for company cars from 2016 to 2021. The authors estimated a regression equation on sales figures during the subsidy period and set the subsidy coefficient to zero to estimate the counterfactual, leaving open the question of how far the interaction effects associated with the subsidies were eliminated. In the official evaluation report of the German EV subsidy (Rao et al., 2024), a survey-based approach estimated free-rider shares between 36.6% and 51.5% for private customers and 27%–41.2% for commercial customers. With a discrete choice model on US data, Xing et al. (2021) estimated a 70% free rider share, which also corresponds to a mean of 70% identified across seven studies (Sheldon, 2022).

Taken together, the evidence suggests that purchase incentives are often associated with higher BEV uptake but reported magnitudes and implied cost-effectiveness differ substantially across contexts and methods. Two issues recurred through the literature. First, the counterfactual trajectory without incentives was rarely observed and had to be constructed; second, the relationship between subsidized purchases and total registrations depends on subsidy take-up. These issues motivated an approach that treats additionality and free riding as accounting problems relative to an explicit forecast baseline derived from non-subsidy periods.

2.2. Counterfactuals and forecasting

To identify the effect of an intervention, valid counterfactuals are required; that is, "What would have happened without the intervention?" In the context of EV subsidies, researchers have used different empirical methods to construct a baseline in the absence of subsidies.

A first class of methods constructs these counterfactuals from untreated units, such as other regions. Difference-in-differences methods (e.g., Cerruti et al., 2024; Correia Sinézio Martins et al., 2024) are a common econometric tool, but they rely on a plausible control group with parallel trends. In EV markets, this is often difficult because most countries have some sort of incentive scheme that may also include non-monetary incentives (Sheldon, 2022). Thus, establishing a valid control group is challenging.

A second class of models studies customer demand. Here, counterfactual sales are generated by resimulating consumer choice after setting the subsidy component to zero. This approach is now standard in the EV subsidy literature. For instance, Sheldon and Dua (2020) estimated a

Table 1
Evidence of EV purchase incentives.

Study	Region and data	Subsidy type	Method	Main findings
Münzel et al. (2019)	32 European countries, 2010–2017 sales panel	National purchase incentives + fiscal benefits	Panel regression	5%–7% relative sales share increase per €1000 subsidies.
Correia Sinézio Martins et al., 2024	EV registration sales in 30 European countries from 2012 to 2021.	Purchase subsidies, tax incentives	Multi-period difference-in-differences	Purchase incentives increase BEV adoption by over 113% of adoption levels without such incentives.
Jenn et al. (2018)	Vehicle registrations across all 50 US states from 2010 to 2015	Rebates & tax credits; non-financial perks.	Panel models/quasi-experimental comparisons	Every $1000 in rebate/credit raises EV sales by about 2%–3%, on average.
Sheldon and Dua (2019)	~275k US new-vehicle purchases between 2014 and 2015	Financial subsidies	Revealed-preference vehicle choice model using	Federal credit induced ~17% of MY2015 PEV sales; cost per induced PEV ≈ $35.6k
Rietmann and Lieven (2019)	EV market shares in 20 countries	Monetary measures, traffic regulations and charging infrastructure	Structural equation model	Positive influence of monetary incentives and charging infrastructure on EV market shares. Strong interaction effects.
Sheldon and Dua (2020)	Survey of Chinese EV purchasers in 2017	Monetary subsidies	Revealed-preference vehicle choice model; counterfactual subsidy simulations	$1.90 per additional liter of gasoline saved, saving potential if limited subsidies to low-income customers.
Azarafshar and Vermeulen (2020)	EV sales in Canada (2012–2016)	Financial incentives	Discrete choice model leveraging different policies on province level	C$1000 higher incentive increases EV sales by 5%–8%, on average. About 35% of EV sales in rebating provinces are attributable to incentives. Implied CO_2 abatement cost ≈ C $480/ton.
Jenn et al. (2020)	Survey among EV buyers in California 2010–2017	Rebates + non-financial incentives	Latent class analysis of incentive importance	Importance of incentives increased over time (2010–2017); hetero-geneous clusters
Muehlegger and Rapson (2022)	California, targeted low/mid-income program	Income-targeted purchase subsidies	Quasi-experiment/policy discontinuities	Significant adoption increase (subsidy elasticity of EV demand of −2.1)
Cerruti et al. (2024)	BEV registration data from Switzerland (2014–2021)	BEV rebates + tax discounts	Difference-in-differences	Small but significant effect of rebates. Cost for CO_2 reduction: CHF 1223/ton.
Zhang et al. (2024)	EV registration data from 319 cities in China (2016–2019)	Range-based purchase subsidies	3-D panel/policy-rate variation	Cost of reducing CO2 emissions is about US$712 per ton
Sheldon and Dua (2024)	Purchase data from 23 countries (2010–2019)	Financial subsidies	Discrete choice model (fixed effects logit)	Short-run cost per induced PEV $14.9k–$62.4k; up to $1212/ton CO_2. Additional medium-run spillover effects.
Schub et al. (2025)	31 European countries 2011–2022	One-time incentives and recurring incentives	Panel regression	Increase of BEV registration shares by 8%–14% per €1000 one-time incentives and by 10%–20% per €1000 per year for recurring incentives.

discrete-choice model for China and then removed national subsidies from EV prices to predict sales under a no-subsidy scenario, allowing them to compute additionality and free ridership. Some demand models also include survey data in which buyers are directly asked about the effects of subsidies (e.g., Rao et al., 2024). While this direct approach mitigates potential confounders, it introduces new biases common in survey research, such as social desirability bias (Nederhof, 1985) or hypothetical bias (Murphy et al., 2005).

Finally, a third class of models treats the counterfactuals as a forecasting problem using the treated unit's own pre-intervention history. Returning to time-series econometrics (Box and Tiao, 1975), this approach is formally referred to as an interrupted time series (ITS, McDowall et al., 1980). The basic logic is to estimate a model on pre- and possibly post-policy data, then extrapolate it through the policy period. In this way, the forecast can be interpreted as the counterfactual path without the policy.

To forecast the diffusion of a new technology, such as electric vehicles, researchers have discussed different models to capture exponential growth. These include Logistic, Gompertz, Weibull, and Bass (Young, 1993). In a comparison of EV diffusion data from 20 countries, Kumar et al. (2022) found that the Gompertz model regularly outperformed more complex models (Lian et al., 2018).

To our knowledge, this is the first attempt to determine a sales counterfactual using a growth function. This approach could be problematic because functions like Gompertz's are purely descriptive time-series forecasting exercises that cannot describe causal relationships, as they have no theoretical foundation. In fact, relying on the time variable as a predictor is unsatisfactory. However, it is possible to transform the time-dependent Gompertz function $N(t) = K \cdot e^{-b \cdot e^{-\alpha \cdot t}}$ to a time-independent differential equation $\frac{dN}{dt} = \propto \cdot N \cdot ln\left(\frac{K}{N}\right)$ (with α as the decay rate of growth potential, K as the saturation limit, and N as the actual stock). This is the so-called autonomous Gompertz differential equation. Thus, the Gompertz model can be seen as a theory-driven model, like other growth models (with resource availability in the logistic function, with innovation and imitation in the Bass model),

Furthermore, its descriptive nature is advantageous here, as it subsumes all market activity under the Gompertz curve. The disadvantage attributed to growth functions—that they have only weak predictive value because only a few data points are projected ex-ante into the future—does not apply here, because the description of EV sales is based ex-post on actual sales figures in the subsidy-free periods. We consider the Gompertz approach a complementary contribution to the existing literature, not the best or only possible approach. It contributes to the need to increase confidence in scientific findings by testing their reproducibility (Open Science Collaboration, 2015).

3. Data, method, and procedure

3.1. Data assessment

Sales figures for pure BEVs were taken from the EV-Volumes dataset (EV-Volumes, 2026). Plug-in-hybrid vehicles (PHEVs) were not considered in this study because the actual carbon benefit is under debate. The subsidy figures come from the responsible federal office (Bundesamt für Wirtschaft und Ausfuhrkontrolle (BAFA) Bundesamt für Wirtschaft und Ausfuhrkontrolle Germany, 2023) and the evaluation by the Fraunhofer Institute on the "Umweltbonus" program (Rao et al., 2024). We distinguished between (i) the number of subsidized purchases (grant counts) and (ii) BEV registrations or sales. These two quantities may not align perfectly in time because registrations and the administrative grant process can be offset. To mitigate timing mismatches, we reported results primarily at the regime level and, in robustness checks, at aggregated frequencies such as quarters.

Restrictions apply to the manufacturer's suggested retail price (MSRP). If the MSRP exceeded €40,000, only a reduced subsidy was granted. Vehicles priced at over €65,000 were not subsidized at all. The list of eligible vehicles was taken from the BAFA documentation. We matched MSRPs from the eligibility lists with BEV sales figures from EV-Volumes (2026), resulting in three price groups. Between January 2010 and December 2025, 2,748,464 BEVs were sold in Germany, of which 1,684,338 (61.3%) had an MSRP of up to €40,000, 964,267 (35.1%) had an MSRP between €40,000 and €65,000, and 99,859 (3.6%) had an MSRP above €65,000. We excluded the last group from further analysis.

Appendix 1 traces the calculation of paid subsidies. We allocated the number of subsidies granted pro rata temporis by month to the sales figures. Given the potential timing mismatch between registrations and grant processing, this monthly allocation was treated as an approximation, while regime-level totals and averages were emphasized in the interpretation. In total, the German government granted about 1.4 million subsidies at an average of €4,938, which adds up to a total of about €6.9 billion. Table 2 presents a data summary.

3.2. Forecast of EV sales (counterfactual baseline)

A forecast function for monthly electric vehicle sales shares from 2010 to 2025 was needed to construct a counterfactual baseline representing expected diffusion in the absence of government purchase subsidies. In the automotive market, the Gompertz growth function is commonly used to generate forecasts from existing diffusion data (Dargay and Gately, 2018; Huo et al., 2007; Zachariadis et al., 1995). Equation (1) describes the Gompertz function.

$$f(t) = a \cdot e^{-b \cdot e^{-c \cdot t}} \quad (1)$$

where a is the upper saturation limit, b is a horizontal location parameter (the higher b is, the more the distribution curve moves to the right), and c is a parameter for the magnitude of the highest growth rate (slope in the inflection point). The slope becomes steeper as c increases. t is the time variable; in this study, it represents the month, starting with 1 for January 2010.

Equation (1) is the distribution function, that is, a cumulative adoption curve. However, we are looking for the density function, which represents the monthly increase. This is the first derivative:

$$f'(t) = a \cdot b \cdot c \cdot e^{-b \cdot e^{-c \cdot t} - c \cdot t} \quad (2)$$

The coefficients a, b, and c were chosen so that the predicted monthly sales shares matched the observed sales shares in periods without government purchase subsidies as closely as possible. To fit the Gompertz curves to the data, we set an upper limit of 45 million vehicles, so we only had to estimate the horizontal position parameter b and the slope at the inflection point c. Because the total vehicle fleet includes both BEVs and PHEVs, the saturation level for BEVs was set proportionally at 28 million (approximately 63%). For the period from 2010 to 2025, during which the main research question on the share of free riders was addressed, the saturation limit was almost irrelevant. The fitting process has little leeway due to the actual sales data from 2010 to June 2016 and January 2024 to December 2025. To analyze the effects of the predetermined saturation limit, we conducted a robustness check by setting the upper limit to 55 million vehicles.

We estimated parameters by minimizing the sum of squared deviations between observed and predicted values using nonlinear regression. We assessed model fit using the coefficient of determination (R2), its statistical significance, the root mean squared error, and a paired-samples t-test as a descriptive check of whether differences between actual and predicted values were systematically different from zero.

We used the percentage share of total passenger car sales rather than absolute BEV sales figures. Absolute forecasts are susceptible to external shocks, such as changes in total market volume, that alter absolute figures, whereas relative changes can remain more stable. The forecasted sales shares in percentages were then converted back into absolute figures for further analysis by multiplying them by total sales. For the period after 2025, monthly sales of 250,000 passenger cars were assumed, corresponding to the average of the last 16 years.

Because the saturation limit of 28 million BEVs applies to absolute figures, a converted value a must be used for percentage-based estimation. With total monthly passenger car sales of about 250,000, the implied saturation level for the percentage scale is $a = 28{,}000{,}000/250{,}000 = 112$. This total is distributed across the three price groups according to their observed market shares: 68.637 for EVs ≤ €40,000, 39.294 for EVs between €40,000 and €65,000, and 4.069 for EVs > €65,000.

Table 2
EV sales and granted purchase subsidies for five periods.

Period	Time	Months	BEV Subsidy	Number of BEV Sales	Number of BEV Subsidies
1	January 2010–June 2016	78		39,979	
2	July 2016–June 2020	48	€2276	188,259	120,350
3	July 2020–December 2022	30	€5734	986,242	903,005
4	January 2023–December 2023	12	€3882	525,762	377,533
5	January 2024–December 2025	24		908,363	
	Total from Period 2 until 4	90	€4938	1,700,263	1,400,888
Normalized per month					
1	January 2010–June 2016			513	
2	July 2016–June 2020			3922	2507
3	July 2020–December 2022			32,875	30,100
4	January 2023–December 2023			43,814	31,461
5	January 2024–December 2025			37,848	
				18,892	15,565

3.2.1. Forecasted sales from July 2016 to June 2020

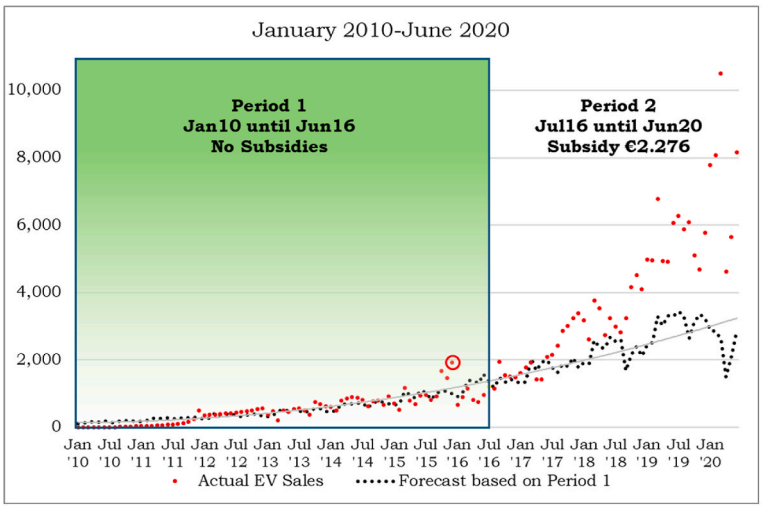
We calculated forecasts for the two lower price categories separately using different Gompertz functions. The forecasted numbers were then added for further analysis. Fig. 2 shows the actual sales shares (red dots, with month 1 being January 2010). Actual sales in Period 1 without subsidies, beginning in January 2010, were used to predict sales in Period 2 (black dots).

The forecast function fit the actual data reasonably well in Period 1 ($R^2 = .699$, $p < .001$, RMSE = .08351%). The assumption of equality of actual and predicted data cannot be rejected (t (77) = .614, $p = .541$, n. s.).

Using the interquartile range (IQR) method, we identified the value from December 2015 as a potential outlier (red circle in Fig. 2), with a *z-value* above 3. Because outliers can alter nonlinear regression results, a smoothed alternative was constructed for the sequence from November 2015 to January 2016 while maintaining the overall sum (instead of .54, .77, .31, the sequence was .62, .65, .35). Fitting the Gompertz function with this alternative series yielded only slightly different results. Given the negligible impact on coefficients and fit, the original data were retained for the main analysis.

3.2.2. Forecasted sales from July 2020 to December 2050

If the forecast based only on data from January 2010 to June 2016



The fluctuations in the dotted forecast line resulted from fluctuations in total sales figures, weighted by the monotonically increasing percentages of the Gompertz derivative.

Fig. 2. Forecast based on actual sales from January 10 to June 16.

were extrapolated to 2050, it would not accurately describe actual sales after Period 2.

An acceptable forecast function can be achieved by fitting the data from Periods 1 and 5 (solid green line in Fig. 2). For the actual and predicted data in these two periods, R^2 is .993 ($p < .001$) and RMSE = .59391%. Likewise, the assumption of data equality cannot be rejected (t (101) = .095, p = .964, n.s.). This alternative calibration, fitting Periods 1 and 5, is interpreted as a conservative ex post diffusion benchmark. If the subsidy program generated persistent market acceleration, sales in Period 5 would be elevated relative to the true no-subsidy scenario. Consequently, using Period 5 to anchor the baseline likely overestimates counterfactual sales and yields a conservative estimate of the subsidy-associated surplus.

This produced two forecast functions: one from July 2016 to June 2020, based on actual data from January 2010 to June 2016 (black dotted line in Figs. 2 and 3). The second function predicts sales from July 2020 to December 2050 (green solid line in Fig. 2), based on actual data from Period 1 before subsidies were granted (January 2010 to June 2016) and Period 5 after subsidies were granted (January 2024 to December 2025). The coefficients of the various Gompertz functions and fit indices are shown in Table 3.

After fitting the Gompertz functions, we conducted an outlier analysis (Appendix 2).

4. Results

4.1. Effectiveness of EV purchase subsidies

Fig. 2 compares the observed BEV registrations in Period 2 with the counterfactual path projected from Period 1 data. Relative to this forecast baseline, Period 2 registrations were higher. Predicted sales were 2214 per month (black dotted line in Fig. 2), while actual sales were 3922 per month, an increase of 77.14%. Under the maintained forecasting assumptions, this gap represents a forecast-based surplus in

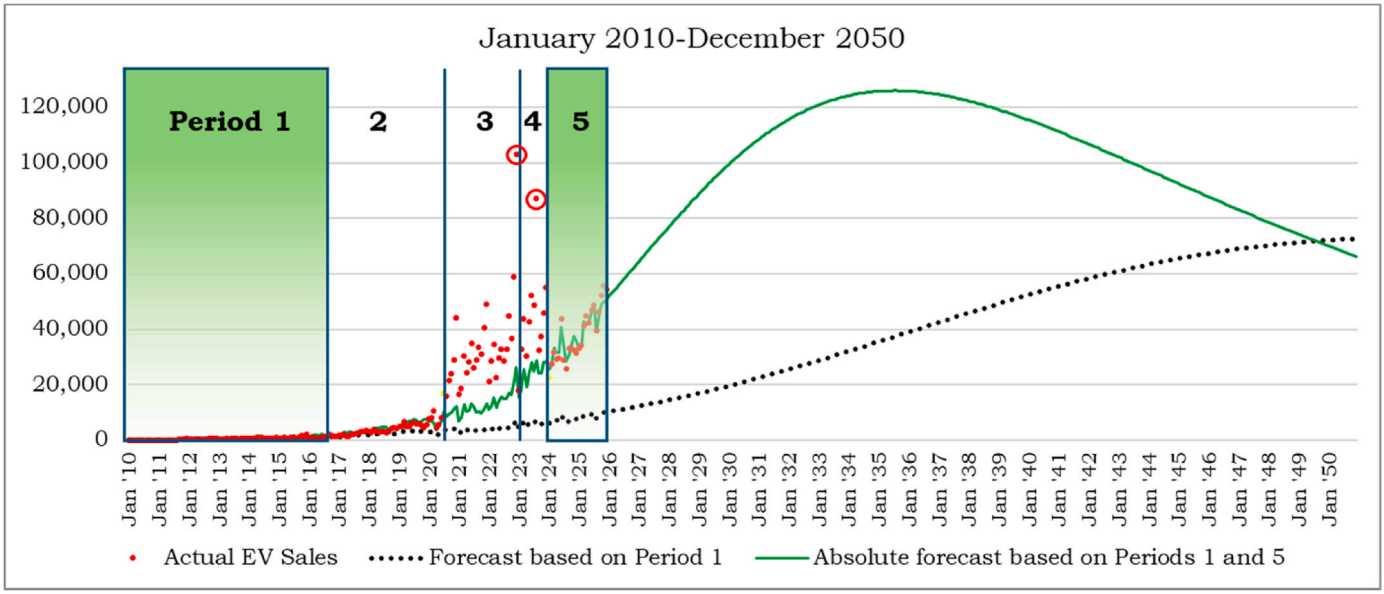


Fig. 3. Forecast based on actual sales from Periods 1 and 5.

Table 3
Gompertz coefficients and fit indices.[a].

Forecast	Gompertz-Coefficients	a	b	c	R^2	RMSE	*t*-test	
EVs ≤ €40,000								
Forecast based on Period 1		68.637	8.4639	0.00438	.681 $p < .001$	.0838%	$t = .560$ $p = .577$	(3)
Forecast based on Periods 1 and 5		68.637	10.9395	0.00727	.975 $p < .001$	.5223%	$t = .113$ $p = .910$	(4)
EVs>€40,000 and ≤€65,000								
Forecast based on Period 1		39.294	14.5828	0.00565	.611 $p < .001$	.0041%	$t = 1.048$ $p = .298$	(5)
Forecast based on Periods 1 and 5		39.294	42.0038	0.01452	.992 $p < .001$	.3139%	$t = .103$ $p = .918$	(6)
Aggregated EVs≤65.000								
Forecast based on Period 1		N/A	N/A	N/A	.699 $p < .001$	.0835%	$t = .614$ $p = .541$	
Forecast based on Periods 1 and 5		N/A	N/A	N/A	.993 $p < .001$	.5939%	$t = .045$ $p = .964$	

[a] Note that these functions calculate the percentage shares of the total monthly sales. The target figures for the period from January 2010 to October 2025 are calculated by multiplying the percentages by the actual total monthly sales. From 2026 on, percentage forecast is multiplied by 250,000.

registrations during the subsidy regime. This addresses Q1 by quantifying how registrations in a subsidy regime deviate from the diffusion-based, no-subsidy forecast.

4.2. Post-program persistence (possible indirect or spillover effect, Q2)

When the Period 1 forecast is extrapolated to the post-program period (Period 5), it underpredicts observed registrations. This is consistent with a level shift in BEV adoption relative to the early-period diffusion path. As shown in Fig. 2, an alternative diffusion calibration using Periods 1 and 5 can reproduce the post-program level. This level shift is consistent with persistence relative to the early-period forecast. Although both Period 1 and Period 5 are non-subsidy periods, they differ in more respects than just the subsidy program, for example, in market maturity and charging infrastructure. Because the Gompertz specification subsumes all market drivers within a single growth process, the following comparison is descriptive and is not intended to attribute the shift causally to the subsidy. A comparison of the Gompertz curves for the sales period with subsidies in Periods 3 and 4 with the forecast from Period 5 onward without subsidies yields an almost identical coefficient *b* for the slopes (.0080244 in Periods 3 and 4, .0081850 for the periods without subsidies 1 and 5). The curves differ mainly in the horizontal location parameter, corresponding to a rightward shift from Period 3 onward. Thirty-one months after May 2021, we obtained the same results from January 2024 onward ($t = .827$, $p = .412$, n.s.). Fig. 4 shows this parallel shift. Overall, the post-program pattern is consistent with Q2 in the sense that post-program registrations differ from the early-period diffusion forecast.

4.3. Implied free ridership

In this study, free ridership was operationalized as a forecast based on non-additionality, meaning the portion of subsidized purchases not accounted for by forecast-based surplus registrations under the subsidy regime. Simply put,

Free riders = Subsidized purchases − Surplus.

Since the surplus is defined as the actual sales minus the forecast, the formula can also be written as follows:

Free riders = Forecast + Subsidies – Actual.

Monthly sales of 2214 BEVs are predicted for Period 2 (July 2016 to June 2020), assuming no subsidies. During Period 2, 2507 subsidies of an average of €2.276 each were paid monthly (Table 2). If these subsidies were fully effective, expected monthly BEV sales would be forecast plus subsidies, totaling 4721 BEVs. However, 3922 BEVs were actually sold per month (Table 2). This represents a surplus of 1708 vehicles, or 77.14%. The difference between 4721 and 3,922, or 799, represents the implied free riders in Period 2, who received subsidies even though they would have purchased a BEV in the forecast baseline. The share of free riders is therefore 31.88% of total subsidies, 20.38% of total EV sales,

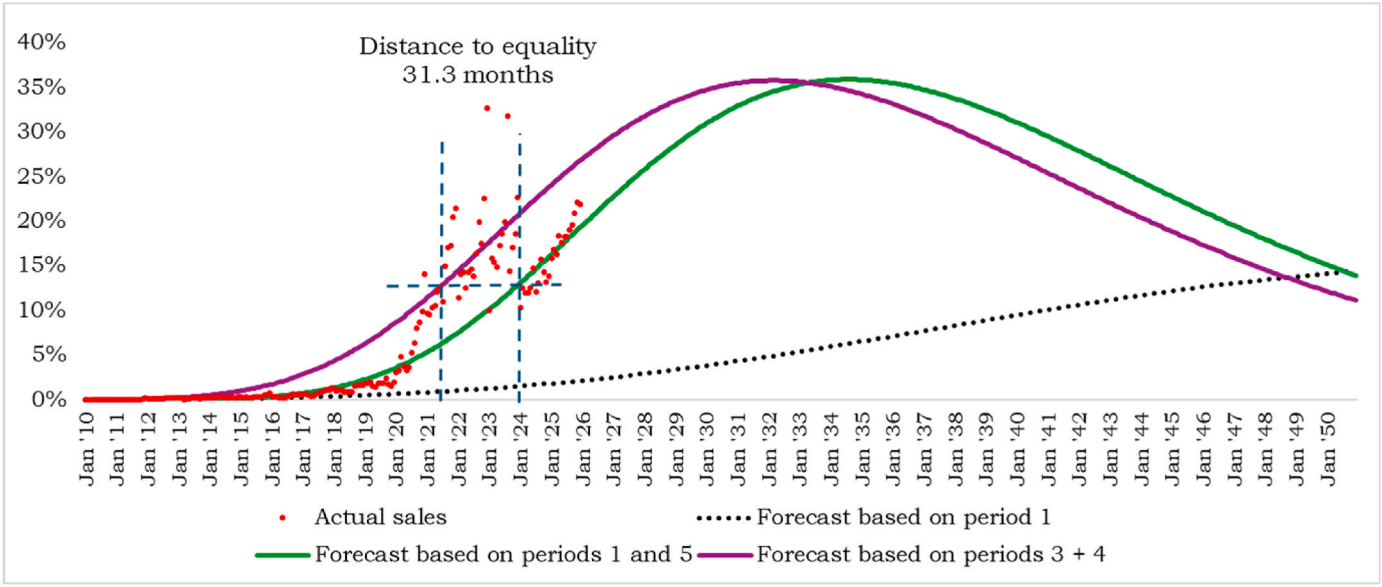


Fig. 4. Parallel shift of the Gompertz growth curves from May 2021 to January 2024.

Table 4
Forecasts, actual sales, and free riders.

Period	1	2	3	4	5	Average in Periods 2–4	Total in Periods 2–4
Subsidy amount		€2276	€5734	€3882		€4938	€4938
Months	78	48	30	12	24	90	
Forecast per month	535	2214	12,657	24,173	36,882	8623	776,074
Subsidies per month		2507	30,100	31,461		15,565	1,400,888
Expected Sales per month		4721	42,758	55,634		24,188	2,176,962
Actual sales per month	513	3922	32,875	43,814	37,848	18,892	1,700,263
Surplus per month		1708	20,217	19,640		10,269	924,189
Surplus % vs. forecast		77.14%	159.73%	81.25%		119.09%	119.09%
Subsidies % from actual		63.93%	91.56%	71.81%		82.39%	82.39%
Surplus % from subsidies		68.12%	67.17%	62.43%		65.97%	
Free rider per month		799	9883	11,821		5297	476,699
% of subsidies		31.88%	32.83%	37.57%		34.03%	34.03%
% of actual		20.38%	30.06%	26.98%		28.04%	28.04%
% of Forecast		36.10%	78.08%	48.90%		61.42%	61.42%
Cost per additional EV		€3341	€8537	€6218		€7485	
Cost per one 1-ton CO_2 avoided		€183	€498	€378		€439	

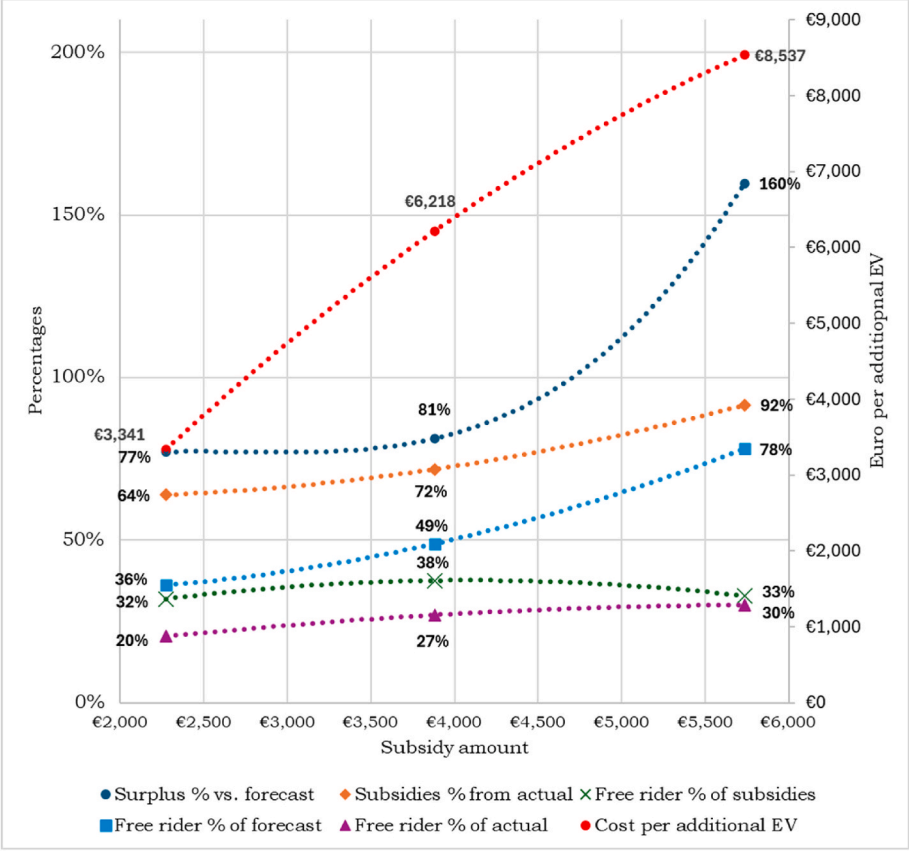


Fig. 5. Changes depending on subsidy amounts.

and 36.10% of forecasted sales volume. We calculated Periods 3 and 4 similarly. Table 4 summarizes all results, and Fig. 5 visualizes the main findings.

The assumption that all EV buyers take advantage of the subsidies could not be confirmed (the subsidy % from actuals was only 63.93% in Period 2). As the subsidy amount increased, take-up rose. Take-up may remain below 100% due to ineligibility, administrative frictions, leasing, company car arrangements, or timing mismatches between grants and registrations.

Given that some buyers do not apply for the subsidy, the implied surplus is substantial across regimes, ranging from 77% (Period 2) to 160% (Period 3) above the forecast. The implied free rider share relative to all subsidies averages around one-third. The implied free rider share relative to actual sales is also low at approximately 28%. The implied free rider share relative to the forecast baseline increased with higher subsidy amounts, reaching 78.08% at the highest subsidy. These results correspond to Q3 and show how implied free ridership varies across subsidy levels under the maintained forecast baseline.

In the price range below €40k, the subsidy regimes show a larger forecast-based surplus relative to the forecast per €1000 subsidy (25% per €1000) than in the €40k–€65k range (21% per €1,000, and 24% across both price ranges). This difference is consistent with the smaller percentage discount resulting from the lower subsidy in the higher-priced class.

Government subsidies paid to free riders increase the implied cost per additional registration. In Period 2, the government subsidy was

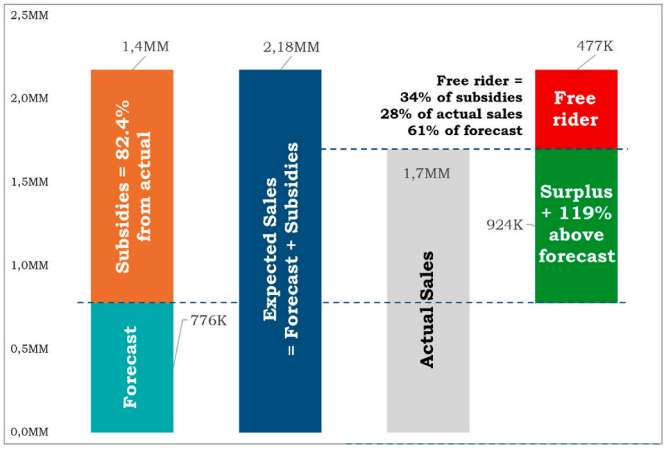


Fig. 6. Composition of actual sales from forecasts, subsidies, surplus, and free riders.

€2.276 and was paid out 120,336 times, which equals €273,884,736. Converted to an additional sale of 81,984 EVs, this amounted to €3341 per vehicle, or about 1.5 times the subsidy amount. This ratio mirrors the implied free rider share reported above. Fig. 6 shows the relationship between actual sales, forecasts, subsidies, surplus sales, and free riders.

Over the 6.5 years of subsidies, the German government supported 1.4 million EV purchases with a total of €6.9 billion, averaging a subsidy of €4938 per EV. However, only 924,189 more EVs were actually sold. The difference represents 476,699 free riders, or 34% of the subsidies. The cost of these free riders therefore amounts to €2.354 billion. This increases the price of each newly registered EV by €4.938/(1%–34%) = €7.485.

Two opposing patterns are visible across regimes. Higher subsidies are associated with a larger surplus relative to the forecast, which is close to linear at 24.67% per €1000 subsidy. At the same time, higher subsidies are also associated with a higher implied free rider share relative to the forecast baseline, at 12.14% per €1000 subsidy. This trade-off motivates examining how the implied cost per additional EV and the implied cost per ton of CO_2 avoided vary across subsidy amounts under the scenario assumptions, which is addressed in the cost section that follows and corresponds to Q4.

The robustness check, with a saturation limit of 55 million vehicles, shows only minimal differences; on average, the figures remain identical (free rider % of all subsidies: 34.19%; price for each additional EV sold: €7502). A plausibility check also supported the chosen saturation threshold of 28 million BEVs. Assuming growth without further subsidies (dotted line in Fig. 9), approximately 6.5 million BEVs are predicted for the end of 2030, assuming a scrapping rate of 3% per year. Together with PHEVs (factor 1.6), this will result in approximately 10.5 million EVs by the end of 2030. This is within the range of current estimations (ADAC, 2025; Statista, 2023).

5. Cost per additional BEV, avoided ton of CO_2, and forecast until 2050

One goal of the German environmental bonus program was to protect the environment by reducing CO_2 emissions through EVs. Therefore, two cost-effectiveness metrics are examined: the cost of each additional EV sold and the cost per ton of CO_2 saved.

5.1. Cost functions

To derive the implied cost for each additional BEV registration under the maintained forecast baseline, the total expenditure of the incentive program was divided by the number of additionally sold BEVs ("surplus" over the forecast). Let N_{sub} denote the number of subsidies paid in a period (or month), s the subsidy amount, and N_{sur} the forecast-based surplus in BEV registrations. The implied cost per additional BEV is then:

$$C_{EV} = \frac{Nsub * s}{Nsur} \tag{7}$$

EVs are considered zero-carbon vehicles in terms of tailpipe emissions. The average age of passenger cars in Germany is steadily increasing. It is currently around 10 years (Kraftfahrt-Bundesamt (KBA) Kraftfahrt-Bundesamt Germany, 2021). Therefore, the emission-reduction effect is calculated over a 10-year period, which corresponds to 120 months. Let km_t denote the average annual distance traveled per car and e^t the assumed tailpipe emission factor in grams CO_2 per kilometer. The implied cost per ton is:

$$C_{CO_2} = \frac{C_{EV}}{km_t \cdot e_t \cdot 10} \cdot 10^6 \tag{8}$$

where factor 10^6 converts grams to tons.

Table 4 provides data points for the three subsidy amounts for (i) the percentage of the subsidies relative to actual sales (take-up) and (ii) the percentage surplus over the forecast. Linear regressions, shown in Fig. 7a and b, interpolate the corresponding percentages over a range from €2000 to €6000.

The annual distance traveled per car decreases over time, partly because the number of cars increases. The historical figures (Odyssee-Mure, 2024) are projected for 2050 using linear regression (Fig. 7c).

Due to technological advancements, CO_2 emissions continue to decline (IEA International Energy Agency, 2017). A linear regression forecasts the development until 2050 (Fig. 7d), with the caveat that a reduction below 80 g per kilometer is not expected (roughly equivalent to a total fleet of 3-L cars).

The calculation is illustrated using January 2026 as an example, with an effective subsidy of €4000. Since January 2010 is $t = 1$, January 2026 corresponds to $t = 193$. Based on equation (2) and the coefficients for the forecast calculated from Periods 1 and 5 (Table 3), the forecast can be calculated for the two price groups and then aggregated. For reproducibility, note that evaluating Equation (2) using the coefficients from Table 3 yields the monthly BEV share in percentage points (e.g., 12.08 for January 2024), which is multiplied by the monthly total market reference figure and divided by 100 to obtain the absolute registration figures. Because the two price-group curves are not additive, they are calculated separately and summed only at the level of absolute registration figures.

For January 2026, the diffusion forecast implies a combined BEV sales share of 20.51% across the two eligible price groups (BEVs below €40,000 and BEVs between €40,000 and €65,000). This total share comprises 9.13% for BEVs below €40,000 and 11.38% for BEVs between €40,000 and €65,000. Multiplying the combined share by the monthly reference of 250,000 passenger cars yields a forecast of 51,271 BEVs for January 2026.

According to the surplus function (Fig. 7b), the additional registrations associated with a €4000 subsidy relative to the forecast amount to 82.81% of the forecast or 42,456 BEVs. The implied actual sales volume is, therefore, 93,727 BEVs. Of these, 72.72% are subsidized by the government under the take-up function (Fig. 7a), corresponding to 68,162 subsidies. Under the accounting identity used in this paper, the implied free riders are the difference between the expected sales volume under full effectiveness (forecast plus subsidies) and the observed actual sales volume. In this example, forecast plus subsidies equals 51,271 plus 68,162, which is 119,433. Actual sales are 93,727. The difference, 25,706, represents the implied free riders who receive the subsidy, even though they would purchase a BEV under the maintained forecast baseline. Total subsidy expenditure equals €272.6 million.

To account for CO2, the 42,456 additional BEVs are treated as zero-tailpipe-emission vehicles. With an average annual mileage of

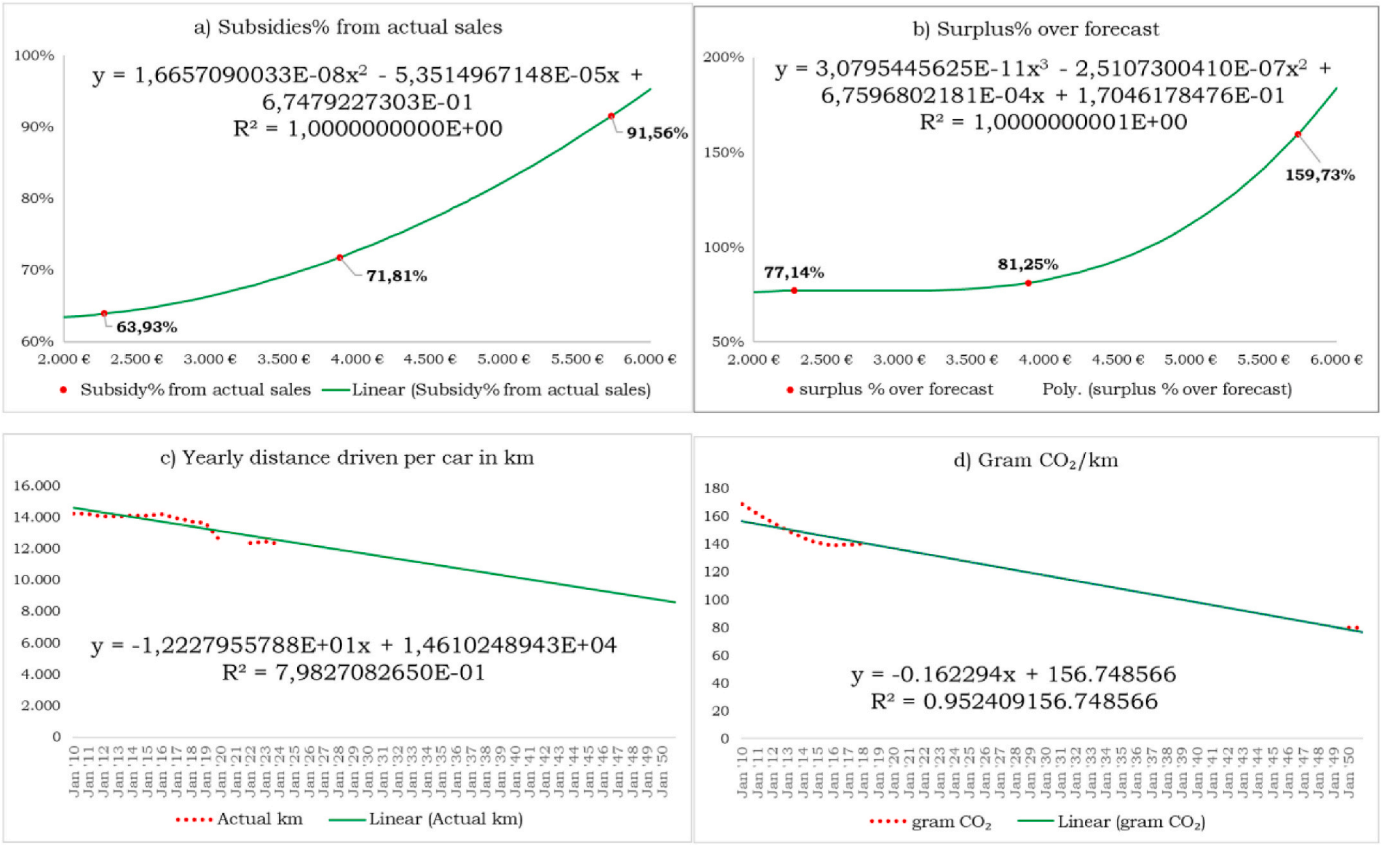


Fig. 7. Regressions for subsidy%, surplus%, yearly distance, and gram CO_2/km.

12,250 km (1021 km per month) and assumed tailpipe emissions of 125 g per km (Fig. 7c and d), each additional BEV corresponds to avoided tailpipe emissions of 127,604 g per month. Across all additional BEVs, this amounted to about 5576 tons per month. Over 10 years, the implied total savings are about 650,000 tons of CO_2. Dividing the total expenditure yields an implied cost of about €418 per ton of CO_2.

Dividing the total cost of the incentive program (€272.6 million) by the additionally sold BEVs (a surplus of 42,456) yields an implied cost of €6422 per additional BEV.

5.2. Subsidy optimum

Under the interpolations used in Fig. 7 and the resulting cost curves in Fig. 8, there is no interior subsidy amount between €2000 and €6000 at which the implied cost per additional BEV or the implied cost per avoided ton is minimized. The shape of the subsidy take-up and surplus functions implies that subsidies increase from the outset, while the increase in forecast-based surplus remains comparatively flat at lower subsidy amounts and only rises more strongly at higher subsidy levels. As a result, implied costs increase sharply from low subsidy amounts and do not exhibit a minimum within the considered interval.

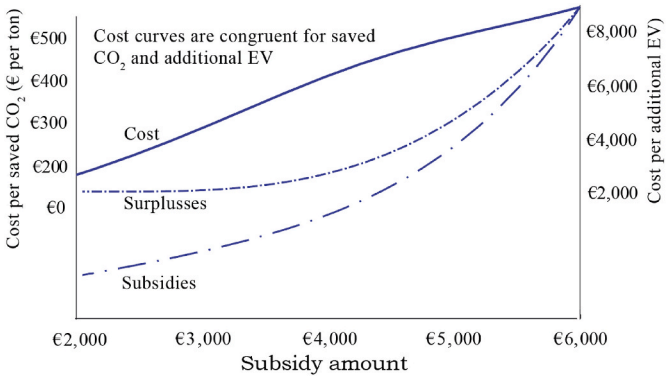


Fig. 8. Number of subsidies, surpluses, and cost curves depending on the subsidy amount.

Given this pattern, the question is how a government strategy could still help meet a long-run road transport decarbonization goal by 2050 under budget constraints. Under the diffusion forecast baseline, the implied BEV fleet would reach about 7.2 million in 2030 (the original target was 15 million) and about 23 million BEVs in 2050, including 37 million PHEVs. However, vehicles are also scrapped. Assuming a scrapping rate of 3% of the total fleet annually, the implied fleet size would be about 6.5 million BEVs in 2030 and 23.7 million EVs in 2050, substantially below full transition. At the same time, paying a constant subsidy such as €4000 until 2050 is not feasible. Even if such a subsidy were maintained, the fleet would approach 40 million BEVs only at a fiscal cost of about €6 billion per year, which is likely unsustainable.

A different interpretation of the empirical patterns is that temporary subsidy cycles may be associated with a lasting upward shift in the sales path, as described in Fig. 4. We emphasize that the following analysis is illustrative and conditional in nature and does not constitute a causal prediction. It is based on the assumption that the level shift observed in Period 5 is, at least in part, persistent and replicable—an assumption that, while consistent with our data, has not been causally identified (see Section 6.2). The results should therefore be understood as tentative "what-if" scenarios that illustrate the magnitude of possible dynamic effects if the shift persists, rather than as point forecasts or direct policy recommendations. If such a shift could be replicated roughly 2.5 years after subsidy payments end, episodic rather than permanent support might achieve a larger long-run increase in sales.

To illustrate this logic, we ran a simulation based on the relationships

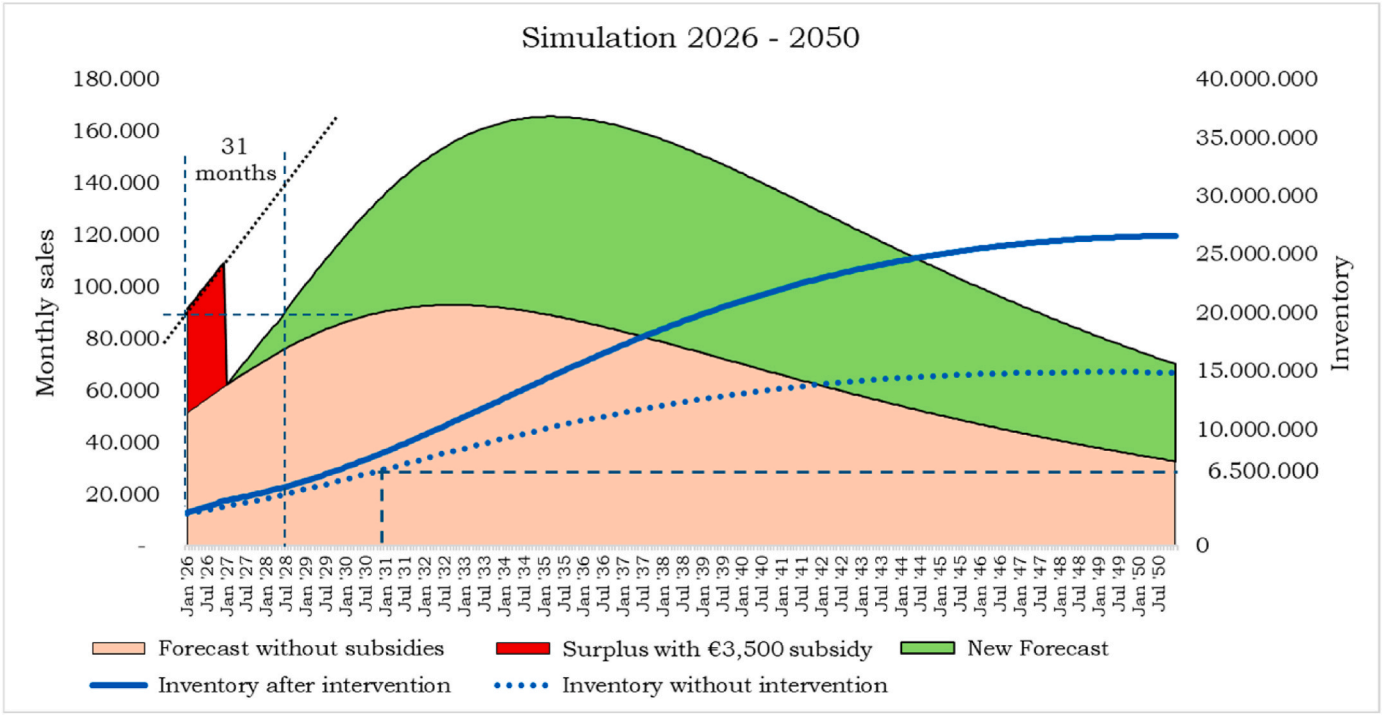


Fig. 9. Simulated BEV sales and inventories with a twelve-month subsidy of €3500 in 2026.

estimated thus far. In the simulation, a one-year subsidy of €3500 per BEV in 2026 increases sales by 78.11% relative to the forecast for January 2026 to December 2026, from 675,360 to 1,202,856 BEVs. The simulation would pay 831,822 subsidies, amounting to almost €2.9 billion in 2026. After subsidy payments end, the simulation assumes that after about 31 months from January 2027, the sales curve repeats with a similar slope, implying sustained higher registrations relative to the forecast baseline. Under this assumption, and using an annual scrapping rate of 3%, the total number of BEVs could grow to approximately 27 million by 2050, including PHEVs, to about 43 million EVs. Fig. 9 illustrates the simulated situation.

Given ongoing policy discussions about renewed support measures in Germany, this scenario would provide an opportunity to assess whether patterns consistent with a similar level shift recur in subsequent periods.

6. Discussion, limitations, and conclusions

6.1. Discussion

This study developed a forecast-based accounting framework to assess free ridership in Germany's BEV purchase subsidy program. The analysis yielded several findings that contribute to the ongoing policy debate on the effectiveness of EV incentives. Table 5 lists the differences and similarities with the existing literature to clarify this study's findings.

The estimated free-rider share of approximately one-third of subsidized purchases (34.19% across all regime periods) is lower than the estimates reported in prior research. Burra et al. (2024) identified free-rider shares of 56.5% for private cars and 87.1% for company cars in Germany, while Xing et al. (2021) estimated 70% for US markets, a figure consistent with the 70% average identified across seven studies in Sheldon's (2022) review. The official evaluation of the German Umweltbonus program reported that the survey-based approach yields a comparable estimate of 36.6% to 51.5% for private customers (Rao et al., 2024).

Within the accounting framework applied here, roughly two-thirds of subsidized payments correspond to purchases that exceed the forecast baseline. This proportion remains almost constant across all three subsidy amounts, suggesting that the relationship between subsidies and forecast-based surplus registrations is stable across regimes. The comparatively low value of €7504 per additional EV sold, relative to existing literature, is consistent with this pattern. The near constancy of free rider shares reflects the similarly stable ratio between forecast-based surplus and forecast-based free rider components, which remains close to two across regimes (Table 4). Consequently, the graphical decomposition shown in Fig. 5 is similar across subsidy levels. While higher subsidy amounts are associated with larger free rider volumes, they are also associated with larger forecast-based surpluses, indicating that higher incentives coincide with registrations beyond the baseline trajectory.

The divergence between our estimates and prior findings reflects methodological differences in counterfactual construction. Studies employing discrete choice models or survey-based approaches implicitly assumed full subsidy take-up among purchasers and model individual-level substitution patterns. Our forecast-based approach instead treated aggregate sales trajectories as the unit of analysis, with free ridership defined residually as subsidized purchases not explained by forecast-based surpluses. Our data showed subsidy take-up rates ranging from 64% to 92% across regime periods, suggesting that assuming universal subsidy application may overstate free-rider behavior in certain contexts.

The positive relationship between subsidy magnitude and forecast-based surplus registrations under the subsidy regime aligns with the broader literature. The estimated 24.67% increase in surplus sales per €1000 of effective subsidy falls within the range reported by Münzel et al. (2019), who found 5%–7% relative sales share increases per €1,000, and Schub et al. (2025), who identified 8%–14% increases in BEV registration shares per €1000 of one-time incentives. Cross-study comparisons should be interpreted cautiously, given differences in outcome measures (absolute surplus vs. market shares), time periods, and geographic contexts.

The observation that BEV registrations in the post-subsidy period (2024–2025) returned to levels comparable to the subsidized period (2021–2023) within about 2.5 years provides descriptive evidence

Table 5
Differences and similarities with existing literature.

Study and findings	This study's findings	Study and findings	This study's findings
Münzel et al. (2019) €1000 higher subsidy increases the PEV sales share by about 5.4%.	€1000 higher subsidy increases the PEV sales share by about 24%.[a]	Correia Sinézio Martins et al. (2024) Purchase incentives possibly increase BEV adoption by over 113% compared to adoption levels without such incentives.	Purchase incentives possibly increase BEV adoption by over 118.4% compared to adoption levels without such incentives.
Jenn et al. (2018) Every $1000 in rebate/credit raises EV sales by about 2%–3% on average.	Every $1000 in rebate/credit raises EV sales by about 20% on average.[b]	Sheldon and Dua (2019) Federal credit induced ~17% of MY2015 PEV sales; cost per induced PEV ≈ $35.6k.	Government's subsidies induced ~48.3% PEV sales, cost per induced PEV ≈ $7.5k.
Sheldon and Dua (2020) $1.90 per additional liter of gasoline saved; saving potential if limited subsidies to low-income customers.[c]	$1.26 per additional liter of gasoline saved.	Azarafshar and Vermeulen (2020) C$1000 higher incentive increases EV sales by 5%–8% on average. About 35% of EV sales in rebating provinces are attributable to incentives. Implied CO_2 abatement cost ≈ C$480/ton.[d]	C$1000 higher incentive increases EV sales by 15.5% on average. About 48.3% of EV sales are attributable to incentives. Implied CO_2 abatement cost ≈ C$682/ton.
Cerruti et al. (2024) Small but significant effect of rebates. Cost for CO_2 reduction of CHF 1223/ton.	Cost for CO_2 reduction of CHF 418/ton.	Zhang et al. (2024) The cost of reducing CO_2 emissions is about US$712 per ton.	Cost of reducing CO_2 emissions is about US $475 per ton.
Sheldon and Dua (2024) Short-run cost per induced PEV $14.9k–$62.4k; up to $1212/ton CO_2. Additional medium-run spillover effects.	Cost per induced PEV $3.7k–$9.2k; $475 per saved ton of CO_2. Similar spillover effects.	Schub et al. (2025) Increase of BEV registration shares by 8%–14% per €1000 one-time incentives and by 10%–20% per €1000 per year for recurring incentives.	Increase of BEV registration shares by 24% per €1000 per year for recurring incentives.
Burra et al. (2024) Free rider share of 56.5% for private cars and 87.1% for company cars from 2016 to 2021. Cost of the subsidy per induced BEV among private consumers is €9718 while it is €30,780 among companies.	From 2016 to 2023, free rider share of 34,19% of all subsidies, 28.2% of actual sales, and 61.5% of forecast. On average, the subsidy per induced BEV is €7504.	Rao et al. (2024) Free rider share between 36.6% and 51.5% for private consumers and 27% and 41.2% for commercial consumers.	Free rider share of all subsidies is an average of 34.19%. Free rider share of actual sales is 28.2% on average. Free rider share of forecast from ~40% for low and 80% for high-subsidy amounts.
Xing et al. (2021) 70% free rider share.	See Rao et al. above.		

[a] In Germany, the average subsidy of €4938 increases EV sales by 119%. Thus 119%/4.938 = 24%.
[b] €1000 = $1200. €4938 ≈ $5926. 119%/5.926 = 20%.
[c] In Germany, the subsidies cost €439 per ton CO_2 saved. One liter gasoline produces 2.3 kg of CO_2. Thus, 434.78 L of gasoline produce 1 ton of CO_2. €439/434.78 L result in €1.01 per liter, or $1.26, respectively (2020 exchange rate).
[d] 1 CAD = €1.55 (2020 exchange rate). In Germany, when the average subsidy of €4938 = C$7653 increases sales by 119%, then C$1000 increase sales by 15.5%. Surplus of 921,878/Total sales 1,700,263 = 54.2%.

consistent with persistence or spillover effects. This pattern aligns with theoretical arguments that subsidies may accelerate market development through learning-by-doing, infrastructure investment, and network effects that persist after incentives are withdrawn (Sheldon and Dua, 2024). However, as noted in Section 4.2, the post-program period differs from the pre-program period in multiple dimensions beyond subsidy availability, including vehicle model diversity, charging infrastructure density, and consumer familiarity, precluding definitive causal attribution to subsidy-induced spillovers. The next subsidy round, planned for 2026 and allocating €3 billion to approximately 850,000 EVs, will provide an additional observation window to assess whether similar patterns emerge.

The finding that higher subsidies are associated with both increased surplus sales and increased free ridership (as a share of forecasted sales) highlights the fundamental trade-off in subsidy design. Policymakers seeking to maximize induced adoption may prefer higher subsidy levels, while those prioritizing fiscal efficiency may favor lower amounts that minimize inframarginal transfers. The framework developed here provides a transparent basis for evaluating these trade-offs under explicitly stated assumptions.

6.2. Limitations

Several limitations should be considered when interpreting this study's findings. The central limitation concerns constructing the counterfactual sales trajectory. Unlike quasi-experimental designs that exploit policy discontinuities or variation across comparable jurisdictions, our approach relies on forecasting using diffusion models fitted to non-subsidy periods. This raises several concerns.

First, the Gompertz function imposes a particular parametric structure on the diffusion process. While this functional form has fit well in prior EV adoption studies (Kumar et al., 2022), alternative specifications (e.g., logistic, Bass, or flexible nonparametric approaches) could yield different counterfactual trajectories and, consequently, different free-rider estimates. In the German case, non-subsidy observations at both the beginning and end of the observation window constrain variation across monotonic growth models. A logistic specification yielded approximately 9% lower forecasts, resulting in slightly reduced free-rider shares of roughly 30% of subsidies paid.

Growth functions are not time series forecasting exercises. In the logistic function, the underlying principle is limited resource availability, which produces the typical sigmoid curve. In the Bass model, it is innovation and imitation; in the Gompertz model, in the case of technology/market diffusion, it is a process in which resistance to further growth increases proportionally to the logarithm of the saturation already reached. For researchers seeking a causal relationship, relying on the time variable may be unsatisfactory. However, the time-dependent Gompertz function can be transformed into a time-independent differential equation: the autonomous Gompertz differential equation.

Furthermore, the descriptive nature of growth functions is not a disadvantage in this study. The function was fitted to the actual sales figures in subsidy-free periods 1 and 5, ensuring that this ex-post analysis yields a valid function curve for these periods. For these two periods, the actual sales are correctly predicted. The counterfactual generated during the subsidy payment periods (periods 2, 3, and 4) simply results from connecting the endpoint in June 2016 to the starting point in January 2024. Assuming strictly monotonic growth, the counterfactual is unlikely to follow a substantially different monotonic path, and plausible shocks such as COVID-19 would lower rather than raise it.

Second, calibrating the long-run forecast using both Period 1 (pre-subsidy) and Period 5 (post-subsidy) data raises circularity concerns.

Table 6
Sensitivity of the implied free-rider share to assumed subsidy-induced spillover in the Period-5 anchor of the counterfactual.

Assumed Period-5 spillover (φ)	Counterfactual (Periods 2–4)	Free riders, % of subsidies	% of actual sales	% of forecast
0% (reported baseline)	776,074	34.0%	28.0%	61.4%
10%	698,467	28.5%	23.5%	57.1%
20%	620,859	22.9%	18.9%	51.8%
30%	543,252	17.4%	14.3%	44.9%

Note: φ = 0% corresponds to the reported calibration for periods 1 + 5. Any positive spillover assumption reduces the counterfactual and thus the implied free-rider share, so the specified value represents an upper bound. The counterfactual is scaled in proportion to the period 5 anchor value; see the text.

Because Period 5 registrations may reflect the persistent effects of the subsidy program (through infrastructure development, model availability, or changed consumer preferences), incorporating post-program data into the counterfactual construction could bias estimates of program effects. Instead of merely noting this problem, we quantify its direction and magnitude. Let φ denote the proportion of the observed level in Period 5 that is attributable to subsidy-induced spillover effects rather than to the actual trend without subsidies. Since Period 1 is close to zero, the counterfactual curve during the subsidy period is determined almost exclusively by the level in Period 5. Lowering this anchor by φ therefore reduces the counterfactual level by approximately the same proportion. According to the balance equation used here (free riders = forecast + subsidies − actual sales), the implied number of free riders depends positively on the counterfactual level. Any correction of the spillover effects, therefore, reduces the estimated proportion of free riders rather than increasing it (Table 6). The stated value of about one-third thus represents an upper bound: The more actual registrations in Period 5 are increased by persistent subsidy effects—precisely the scenario that gives rise to concerns of circularity—the higher the resulting counterfactual value, and the more a corresponding correction reduces the implied free-rider share. Taking to its quantitative conclusion, this concern shifts the estimate toward fewer, not more, free riders. This conservative approach is consistent with the explicitly illustrative nature of the simulation in Section 5.2: There, persistent effects serve as a policy tool under specified assumptions, while the main estimate of the free-rider share is constructed so that any persistence would only correct it downward. We note that the table approximates the recalibration based on the dominant anchor in period 5, rather than re-estimating the Gompertz curve for each value of φ; however, the direction of the effect follows directly from the balance identity and is independent of the curve's exact shape.

The forecasting approach does not explicitly control for time-varying factors that may have influenced EV adoption independently of subsidies. These include: (i) declining battery costs and improved vehicle range over the study period, (ii) expansion of public and private charging infrastructure, (iii) introduction of new vehicle models, including entry of new manufacturers, (iv) changing fuel prices and electricity costs, (v) regulatory developments, such as emissions standards and urban access restrictions, and (vi) shifting consumer attitudes toward sustainability and electric mobility.

To the extent that these factors evolved favorably during the subsidy period and contributed to adoption growth, the forecast-based surplus may overstate the subsidy's causal contribution. Conversely, if adverse shocks (such as supply chain disruptions during the COVID-19 pandemic) depressed sales in certain periods, surplus estimates may understate effectiveness during those periods.

The interpolations used to relate subsidy levels to surplus rates and take-up rates (Fig. 7a and b) were estimated from only three data points corresponding to the three subsidy regimes. Extrapolation beyond the observed range (€2276–€5734) and interpolation within it both carry substantial uncertainty. True relationships may exhibit threshold effects or diminishing returns.

Our study did not consider the industrial policy aspects of the environmental bonus program. The government certainly intended to provide positive incentives for the German automotive industry. However, the program was not limited to locally produced vehicles. Only 37.2% of the subsidized vehicles were German-made. The program primarily focused on improving environmental conditions.

6.3. Conclusion

This study contributes a transparent framework that links diffusion-based counterfactual forecasting with observed sales and subsidy data to derive implied additionality and free ridership metrics. Applied to Germany's Umweltbonus program, the framework yields a free-rider estimate of approximately one-third of subsidized purchases, lower than most prior estimates in the literature. The analysis also provides descriptive evidence consistent with post-program persistence effects.

The methodological approach developed here offers certain advantages for policy evaluation, particularly in contexts where quasi-experimental variation is unavailable or where researchers seek to complement demand-side estimates with aggregate forecasting approaches. At the same time, the reliance on parametric diffusion assumptions and the inability to control for time-varying confounders represent important constraints on causal interpretation.

This analysis suggests several avenues for future research. First, the forecast-based approach could be extended to other national contexts in which subsidy regimes have been introduced and subsequently withdrawn, enabling cross-country validation of the framework and examining how market characteristics moderate subsidy effectiveness. Second, integrating the aggregate forecasting approach with individual-level discrete choice models could help reconcile the divergent free-rider estimates in the literature and clarify the role of subsidy take-up behavior in driving these differences. Third, the post-program persistence patterns observed in this study warrant more rigorous investigation using methods better suited to causal identification, such as synthetic control approaches or regression discontinuity designs exploiting the abrupt termination of the German program in December 2023. Fourth, future work could extend the welfare analysis beyond CO_2 abatement costs to incorporate consumer surplus effects, distributional considerations, and interactions with other policy instruments, such as charging infrastructure investments and vehicle emissions standards. Finally, as EV markets mature and approach higher penetration levels, research examining whether the relationships between subsidies and adoption change across different phases of the diffusion curve would inform the design of phase-out strategies for purchase incentives.

CRediT authorship contribution statement

Theo Lieven: Writing – original draft, Project administration, Methodology, Formal analysis, Data curation, Conceptualization. **Matthias Brüning:** Writing – review & editing, Writing – original draft, Validation, Methodology, Conceptualization.

Declaration of the usage of generative AI

During the preparation of this work, the authors used the AI tools "ChatGPT" and "Claude" to compile formulas and improve language and readability. The authors reviewed and edited the output as needed and take full responsibility for the content of the published article.

Appendix 1. Determination of the average subsidy amount actually paid, considering the price categories of the cars (≤€40T/> €40T)

Period 2	*1a) July 2016–October 2019*	*1b) November 2019–June 2020*	
	all vehicles	BEVs ≤ €40,000	
Subsidy amount	€2000	€3000	
		BEVs > €40,000	
Subsidy amount		€2500	
	July 2016 to October	November 2019 to June	
Number of **sales**	2019	2020	Total
EVs ≤ €40,000	125,067	48,819	173,886
EVs > €40,000	7940	6.433	14,373
Total	133,007	55,252	188,259
	71%	29%	
	July 2016 to October	November 2019 to June	
Number of **subsidies**	2019	2020	Total
EVs ≤ €40,000	79,953	31.209	111,162
EVs > €40,000	5076	4112	9188
Total	85,029	35,321	120,350
Total Subsidy Amounts	€159,905,380	€93,626,865	€253,532,244
	€10,151,748	€10,281,202	€20,432,950
	€170,057,128	€103,908,067	€273,965,195
∅ subsidy amount			**€2276**

Period 3	*July 2020–December 2022*	Period 4	*January 2023–December 2023*
	BEVs ≤ €40,000		BEVs ≤ €40,000
Subsidy amount	€6000	Government	€4500
	BEVs > €40,000		BEVs > €40,000
Subsidy amount	€5000		€3000
Number of sales		Number of sales	
EVs ≤ €40,000	723,812	EVs ≤ €40,000	309,182
EVs > €40,000	262,430	EVs > €40,000	216,580
Total	986,242	Total	525,762
Number of subsidies		Number of subsidies	
EVs ≤ €40,000	662,724	EVs ≤ €40,000	222,014
EVs > €40,000	240,281	EVs < €40,000	155,519
Total	903,005	Total	377,533
Total Subsidy Amounts		Total Subsidy Amounts	
EVs ≤ €40,000	€3,976,341,639	EVs ≤ €40,000	€999,062,001
EVs > €40,000	€1,201,406,968	EVs > €40,000	€466,557,666
Total	€5,177,748,606	Total	€1,465,619,667
∅ subsidy amount	**€5734**	∅ subsidy amount	**€3882**

Appendix 2. Outlier analysis

We compared the forecast values with actual sales across Periods 2, 3, and 4. As shown in Fig. 3, the interquartile range method identified two outliers: December 2022 (Period 3) and August 2023 (Period 4). Upon closer examination, we retained the August 2023 data point, as it was only a mild outlier ($z\text{-}score = 2.52$). In contrast, December 2022 represents an extreme outlier ($z\text{-}score = 4.25$). This spike is likely attributable to anticipatory purchasing, or pull-forward effects, ahead of the 25% subsidy reduction effective January 1, 2023. Although the government announced this reduction on July 26, sales did not immediately surge in August. While actual sales exceeded the forecast by a factor of 3.2 between August and November 2021, they were only 2.5 times higher during the same interval in 2022. This delay suggests that supply chain disruptions and 6–9-month delivery times postponed the announcement's visible impact. When comparing the surplus calculated with the December outlier against a baseline, excluding it, we found no statistically significant difference ($t(52) = 1.790$, $p = .079$). Consequently, we retained this outlier in the final analysis.

Data availability

Research Link Provided.
Free rider Supplement final (Original data) (Figshare)

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