Fiscal Stimulus and Consumption Spending
Evidence from a €5 Billion Program*

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Abstract

We use vehicle registration and household-level expenditure data to study the consumption response to the German Cash-for-Clunkers program. Event-study estimates suggest that the program generated a large and sharply timed increase in private new-car registrations in 2009. The response returned toward baseline after the program, consistent with limited intertemporal substitution. Exploiting policy-induced eligibility criteria, we find little evidence that the program reduced spending on other durables, non-durables, or services, consistent with complementarity across consumption categories. A back-of-the-envelope calculation implies that each euro of program spending was associated with about 39–69 cents of additional non-car consumption.

Keywords: Fiscal stimulus; consumption; intertemporal elasticity of substitution; durable goods; Cash-for-Clunkers.

JEL classification: E21, E62, H24, H31, D12, D31, D91.

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1 Introduction

When facing economic downturns, governments frequently rely on fiscal stimulus programs—such as temporary tax reductions or price subsidies—to counteract declines in consumer demand (e.g., Shapiro and Slemrod, 2003; Johnson et al., 2006; Agarwal and Qian, 2014; Buettner and Madzharova, 2021; Orchard et al., 2025; Bachmann et al., 2026). While the goal of these policies is to boost immediate consumption, their effectiveness ultimately depends on whether they generate additional consumption or merely reallocate spending across time or across goods. In the aftermath of the Great Recession of 2009, 22 countries implemented so-called Cash-for-Clunkers (CfC) programs that subsidized new-car purchases when scrapping a used car. Among these countries, Germany implemented the largest such program worldwide, allocating €5 billion and subsidizing nearly 2 million purchases with a flat €2,500 rebate.[1] Scrappage schemes appear attractive to policymakers because they may boost economic activity during a recession and, in addition, improve fuel efficiency and reduce greenhouse gas emissions. However, there is little empirical evidence on the consumption response to such policies in European markets.

This paper exploits the German Cash-for-Clunkers program as a policy shock that temporarily lowered the price of new vehicles. We use administrative vehicle registration data and household-level expenditure data to analyze both intertemporal substitution (shifting purchases across time) and intratemporal substitution (shifting spending across goods). Our setting provides a rare opportunity to study a large fiscal stimulus targeted at a specific durable good. In effect, the rebate constitutes a temporary price cut that may generate income effects as well as substantial substitution effects. This allows us to quantify how much of the stimulus translated into additional consumption rather than reallocation of spending. Our setting helps address several threats to identification that often arise in policy evaluations. Announcement and implementation essentially coincided, and the size of the subsidy was unknown prior to the announcement, which rules out anticipation effects. Moreover,

[1]The German program was similar to the one introduced in the United States, but its budget exceeded that of the United States by €3 billion, amounting to 33% of the worldwide Cash-for-Clunkers budget of €15.3 billion (see Table A.1 in the appendix).

the precise eligibility criteria—such as the age of the clunker and the minimum ownership period—were also unknown ex ante, and their cutoffs were set administratively by the government. Hence, we exploit an exogenous shock to households' budget constraints that occurred unexpectedly and generated plausibly exogenous variation in incentives to purchase a car.

Building on a theoretical model in the spirit of Ogaki and Reinhart (1998) and Cashin and Unayama (2016), we develop three hypotheses that we test empirically. The first hypothesis is that the demand for new cars increased in 2009. Since stimulating car purchases was the intended objective of the policy, rejecting this hypothesis would imply that the fiscal stimulus had already failed at the program take-up stage. The second hypothesis is that intertemporal substitution did not crowd out the fiscal stimulus, that is, the increase in new-car purchases during 2009 was not fully offset by a reduction in new-car purchases over the post-program observation period. Evidence from the United States suggests the opposite: Mian and Sufi (2012), Hoekstra et al. (2017), and Green et al. (2020) show that the U.S. Cash-for-Clunkers program led to a strong increase in car purchases during the program period that was fully reversed afterward. Because the German program differed somewhat from the U.S. program, it is a priori unclear whether this strong intertemporal substitution was also present in Germany.

We test these first two hypotheses using administrative monthly new-car registrations from the German Federal Motor Transport Authority. Our main specification compares private and commercial registrations within the same two-digit postal region, manufacturer, and year-month using a Poisson pseudo-maximum likelihood (PPML) event-study design. Commercial registrations provide a within-market comparison group because they were not eligible for the subsidy, while region-manufacturer-year-month fixed effects absorb shocks common to both owner groups within the same local manufacturer market. We find a large and sharply timed increase in private registrations relative to commercial registrations beginning in February 2009. The response peaks in spring 2009, declines over the remainder of the year, and returns toward baseline after the program. In contrast to Mian and Sufi (2012) (for the United States), but in line with Helm et al. (2023) (for Germany, using a different design and different data), we do not find evidence of a full reversal in new-car purchases after the end of the program, although the cumulative estimates indicate a partial long-run offset by 2014. Counterfactual calculations imply about 1.45 million additional private registrations by December 2009 and about 1.52 million by the June 2010 registration deadline. These results support the first hypothesis and are consistent with the second: the program substantially increased new-car demand during the program period, and the 2009 increase was not fully crowded out by lower future registrations.

The third hypothesis implied by our theoretical model is that substitution between goods, that is, intratemporal substitution, did not crowd out the fiscal stimulus. In other words, cars were not purchased instead of other goods, but in addition to them, such that overall consumption increased. For example, excess purchases of new cars could have occurred at the expense of used cars or other transport modes such as public transport. Because the price of a new car typically exceeds monthly household income, the policy could also have shifted spending toward cars and away from other goods and services, such as groceries or dining out. Although one usually cannot observe precise eligibility status in Cash-for-Clunkers programs, we approximate eligibility using two highly predictive proxies available in the German Socio-Economic Panel (SOEP): car ownership in 2008 and whether the household purchased a car during the previous 12 months. These variables allow us to construct a proxy for exposure to the program and to identify a comparison group of households that were unlikely to be eligible. Having separated proxy-eligible and comparison households in this way, we test the third hypothesis using the rich set of information collected in the SOEP. Importantly, this includes detailed consumption expenditures from a special module of the 2009/2010 wave that coincided with a question on take-up of the Cash-for-Clunkers subsidy. In addition, we control for key determinants of consumption behavior such as patience and risk aversion.

We find that proxy-eligibility raised the probability of purchasing a car in 2009 by 6.5 percentage points in the full SOEP sample. Proxy-eligible households also spent 0.605 log points more on cars and 0.209 log points more in total, while no other consumption category declined significantly. Among households that purchased a car in 2009, the corresponding ITT estimates are 0.294 log points for car spending and 0.148 log points for total spending;

the effects on other durables, non-durables, and services are small and statistically insignificant. Lee bounds rule out a negative car-spending effect and place the lower bound for total consumption essentially at zero, although the buyer sample cannot rule out a null intensive-margin effect. Moreover, the buyer-sample estimates are smaller and less precise under PPML. Taken together, the evidence supports the third hypothesis and indicates that the program operated primarily through the extensive margin (i.e., inducing additional car purchases) rather than through large spending increases among households that would have purchased a car.

Further, we identify a 0.255-log-point difference between the buyer-sample effects on cars and non-durables, which corresponds to the numerator of the intratemporal elasticity. After rescaling by the take-up first stage and calibrating the program-induced change in user costs, the implied intratemporal elasticity ranges from approximately 0.446 to 1.030 under annual depreciation rates of 20–30% and interest rates of 0–3%. The absence of negative cross-category effects is consistent with durable-non-durable complementarity, although the precise elasticity remains sensitive to the user-cost calibration (Cashin and Unayama, 2016).

To further assess the program's effect on aggregate demand, we combine the registration-based counterfactual with the household-level expenditure estimates in a back-of-the-envelope calculation. Because receipt of the subsidy was conditional on financing a new-car purchase worth several times the rebate, the relevant outcome is expenditure mobilized per euro of public outlay rather than a conventional marginal propensity to consume (MPC) out of a windfall. The registration-based calculation implies €3.95 of additional vehicle expenditure per euro of program outlay by December 2009 and €4.15 by the June 2010 registration deadline. The household-based decomposition yields approximately €3.1–€4.4 of additional car expenditure and €3.5–€5.1 of additional total consumption per euro of outlay. These magnitudes largely reflect households' own funds mobilized by the subsidy. For comparison with the rebate-MPC literature, excluding the gross vehicle expenditure associated with participation leaves approximately €0.39–€0.69 of additional non-car consumption per euro of program outlay. This MPC-style statistic overlaps with the range reported for the 2008 Economic Stimulus Payments by Parker et al. (2013), although Misra and Surico (2014) show that estimates from those payments are sensitive to expenditure on new vehicles. Because participation in the German program required a large durable purchase, however, this statistic is not directly comparable to an MPC from an unconditional transfer and should not be interpreted as a direct rejection of the permanent income hypothesis (e.g., Fuchs-Schündeln and Hassan, 2016).[2]

This paper contributes to the literature in several ways. First, we evaluate the intratemporal and intertemporal consumption responses to the German Cash-for-Clunkers program within a life-cycle framework in the spirit of Ogaki and Reinhart (1998) and Cashin and Unayama (2016). This allows us to estimate the intratemporal elasticity of substitution between different types of goods and to bound the intertemporal elasticity of substitution for the targeted good, cars. Our calibration results suggest that the implied elasticity ranges from approximately 0.446 to 1.030. Further, the absence of a full post-program reversal is consistent with an intertemporal elasticity below one. This intertemporal pattern is in line with evidence of modest intertemporal substitution in Hall (1988), Ogaki and Reinhart (1998), Yogo (2004), Parker and Preston (2005), Cashin and Unayama (2016), and Baker et al. (2021), and contrasts with larger estimates obtained without household-level data, such as Buettner and Madzharova (2021). Whereas much of the existing literature identifies intertemporal substitution using anticipated tax changes, we exploit a large and unanticipated subsidy targeted at a specific durable good. In contrast to Cashin and Unayama (2016), who estimate intertemporal substitution and infer restrictions on cross-good complementarity, we observe expenditures across consumption categories and directly estimate their reduced-form responses for proxy-eligible and comparison households.

Our paper also contributes to the literature evaluating Cash-for-Clunkers programs as a specific type of fiscal stimulus policy. Studies of the United States show that the program successfully boosted short-term vehicle demand (e.g., Copeland and Kahn, 2013; Green et al., 2020). However, as Mian and Sufi (2012) and others show, this excess demand was fully reversed after the end of the program. This "pull-forward" behavior implies that the cu-

[2]The calculations measure gross expenditure associated with the program. They include households' own funds and abstract from general-equilibrium adjustments such as price and supply responses, displacement in the used-car market, and the financing of the program.

mulative effect of the program was zero (see also Li et al., 2013; Hoekstra et al., 2017).[3] While most empirical studies focus on the United States, we are the first to study consumption responses to the somewhat different German Cash-for-Clunkers program. In contrast to the U.S. evidence, we show using administrative registration data that the German rebate generated a large and sharply timed increase in private new-car registrations in 2009. The response returned toward baseline after the program, implying that the program was not fully offset by intertemporal substitution. Consistent with this finding, Helm et al. (2023), who study the impact of the German Cash-for-Clunkers program on local air quality, also document that the policy did not merely reallocate demand intertemporally. Furthermore, we are the first to use nationally representative household-level data with detailed information on consumption spending across goods. This builds on Hoekstra et al. (2017) and Green et al. (2020), who use household-level car purchase data but have limited information on other household expenditures. We are therefore able to provide a first estimate of the intratemporal substitution effects of the Cash-for-Clunkers program across different types of goods, which in turn allows us to bound the intertemporal elasticity of substitution.

Finally, we contribute to the literature on consumption responses to fiscal stimulus by studying a transfer conditional on a large durable purchase. Existing studies of unconditional rebates report MPCs ranging from below 20% to around 90% (Souleles, 2002; Johnson et al., 2006; Parker et al., 2013; Orchard et al., 2025), while Boehm et al. (2025) estimate responses of 23–61% to helicopter money. The German Cash-for-Clunkers program differs fundamentally from these interventions: households received the €2,500 rebate only after scrapping an old vehicle and financing the purchase of a new one. The relevant primary outcome is therefore expenditure mobilized per euro of subsidy rather than a conventional MPC out of a windfall. The registration-based counterfactual implies €3.95 of additional vehicle expenditure per euro of program outlay by December 2009 and €4.15 by the June 2010 registration deadline. The household-based decomposition implies approximately €3.1–€4.4 of additional car expenditure and €3.5–€5.1 of additional total consumption per euro of outlay, with most of the response arising from households induced to purchase a car. For

[3] Attanasio et al. (2022) confirm this result within a life-cycle model of household choices and consumption.

comparison with the rebate-MPC literature, excluding the gross vehicle expenditure tied to participation leaves approximately €0.39–€0.69 of additional non-car consumption per euro of program outlay. This is an MPC-style statistic, but it is not directly comparable to an MPC from an unconditional transfer.

The remainder of this paper unfolds as follows. In Section 2, we provide an overview of the Cash-for-Clunkers program and develop our hypotheses. In Section 3, we estimate the intertemporal effects of the Cash-for-Clunkers program, while we evaluate the intratemporal effects in Section 4. In Section 5, we provide a back-of-the-envelope calculation of the impact of the Cash-for-Clunkers program on aggregate demand. Finally, we conclude in Section 6.

2 The €5 Billion Program

2.1 Institutional Background and Data

During the period 2008–2010, 22 countries around the world introduced Cash-for-Clunkers programs. Table A.1 in the appendix provides an overview. Altogether, €15.3 billion was spent on vehicle retirement schemes, roughly equivalent to the 2009 GDP of Estonia or Cyprus. The European Union contributed €8.83 billion, or 58% of this total. Germany alone spent €5 billion, accounting for 33% of the worldwide budget and 57% of the amount spent by all European countries combined. This made Germany the country with the largest overall budget, followed by Japan, which invested €2.9 billion, slightly more than half of the German amount. The United States spent €2 billion. Germany also spent by far the most per capita: €61, almost three times as much as Japan, Italy, and Luxembourg, each of which spent slightly more than €20 per inhabitant. The United States ranks twelfth, with €6.5 per capita. With almost 2 million cars subsidized by a flat rebate of €2,500,[4] Germany also led the worldwide ranking in this dimension. Japan ranked second with 1.8 million subsidized cars, while the United States subsidized about 680,000 automobiles.

[4]There were slightly fewer than 2 million subsidized purchases in Germany because the agency had to finance administrative costs out of the total budget.

The idea of a scrappage program in Germany entered the political agenda in late 2008. Vice Chancellor Steinmeier publicly announced it as part of a stimulus package on December 27, 2008. Only two weeks later, the government passed an economic stimulus package that included the scrappage scheme. The program officially started on January 14, 2009, and the first key details were published by the responsible agency, the Federal Office for Economic Affairs and Export Control, on January 16, 2009. Consumers, therefore, could not have responded to the policy before 2009. As a proxy for public interest in and awareness of the program, Figure A.1 in the appendix plots Google search trends from July 2008 to June 2009 for different names of the scheme. "Umweltprämie" (environmental premium) was the official name, while "Abwrackprämie" (scrapping premium) was the colloquial term most commonly used by consumers and the media. "Verschrottungsprämie" was a semantic alternative that was used much less frequently. Although interest in the policy increased slightly before the law took effect on January 14, 2009, there are no search hits in 2008. This supports the view that the German Cash-for-Clunkers program constituted an exogenous and unexpected shock: both the decision to participate and the actual purchase of a new car had to occur in 2009. Consumers could not anticipate this temporary price cut and adjust their behavior as early as 2008. Moreover, the second spike in search intensity clearly coincides with the budget increase and the switch from a paper-based to an online application procedure at the end of March and beginning of April.

In general, the subsidy of €2,500 could be claimed by private individuals who scrapped a used car that had to be at least nine years old and had been registered to the applicant for at least 12 months at the time of application. These eligibility criteria were again unforeseeable and effectively arbitrary, generating additional exogenous variation. The new car had to be a passenger car that fulfilled at least the Euro 4 emission standard and had to be registered to the applicant.[5] Applications were possible until the end of 2009 or until the budget was exhausted. The rebate was paid only after the purchase, although applicants could be confident of receiving it if the eligibility requirements were met and the budget had not yet

[5]European emission standards define acceptable limits for exhaust emissions of new vehicles sold in EU member states. In the German case, this requirement was redundant because *all* new cars purchased in 2009 already satisfied the Euro 4 standard.

been depleted. While the subsidy was granted to car buyers, dealers generally handled the scrappage process and communication with the federal agency. The program turned out to be extremely popular, and the original budget of €1.5 billion was at risk of being exhausted already in April. The government, therefore, quickly raised the budget to €5 billion, just a few days after switching from a paper-based to an online application procedure. By September 2, 2009, the budget had been exhausted after almost 2 million new-car purchases had been subsidized. By the end of 2009, the bulk of requests had been processed by the agency, implying that the effective program period ran from January to December 2009.[6]

2.2 Theoretical Considerations and Hypotheses

How does the temporary price cut for cars induced by the Cash-for-Clunkers program affect the consumption of cars and other goods? The standard life-cycle framework highlights two key channels through which behavior may respond. The most prominent one is captured by the intertemporal elasticity of substitution.[7] To the extent that the scrappage program lowers new-car prices relative to expected future prices, consumers have an incentive to shift new-car purchases to the period in which prices are temporarily low.[8] A second channel is intratemporal substitution: cars become cheaper relative to other consumption goods, and the extent of the resulting reallocation depends on the degree of substitutability between cars and other goods. In the framework of Ogaki and Reinhart (1998), a temporary change in the price of cars is reflected in the user cost of the service flow that cars provide. This framework forms the basis for the derivation and discussion of the hypotheses underlying our empirical analysis. Appendix B presents the theoretical framework, while Appendix C derives the underlying household life-cycle problem with durable consumption.

A temporary decline in the user cost of cars due to the price cut should increase the demand for cars during the program period. This implies an increase in new-car purchases

[6] Table A.2 in the appendix summarizes the most important dates and events over the program period.

[7] Other channels include adjustment costs, stockpiling, and price fluctuations.

[8] Economic incidence may differ from statutory incidence if dealers adjust transaction prices. Kaul et al. (2016) show for the German program that pass-through varied across price segments, but that consumers captured at least the statutory subsidy amount on aggregate.

while the subsidy is available. Both the intertemporal and intratemporal channels are consistent with our first hypothesis.

Hypothesis 1. *The demand for new cars increased in 2009.*

Strong intertemporal substitution would imply that households pulled forward new-car purchases that would otherwise have occurred after the program. This would lower the propensity to buy new cars after the end of the program. For example, Mian and Sufi (2012) find a complete reversal of the U.S. scrappage program CARS after only a few months. By contrast, if the program induces additional purchases rather than merely shifting the timing of purchases across periods, purchases should return to their pre-program level after the program but not fall below it. The cumulative effect on new-car purchases would then remain positive over the observation window (Helm et al., 2023).

Our second hypothesis is, therefore, that intertemporal substitution was not strong enough to reduce future car consumption. In other words, the new cars were purchased on average on top of, rather than instead of, planned future car expenditures. The main intuition is that once the program ends, relative prices revert and user costs return to their original level.[9] If the intertemporal elasticity of substitution is sufficiently low, households respond mainly by increasing the desired stock of cars rather than shifting purchases across periods. In that case, the program can generate additional purchases without a post-program reversal.

Hypothesis 2. *Intertemporal substitution does not fully crowd out the fiscal stimulus. The increase in new-car purchases during 2009 was not fully offset by a reduction in new-car purchases over the post-program observation window.*

A temporary decline in the user cost of cars may also affect other consumption categories if preferences are non-separable (Ogaki and Reinhart, 1998; Pakoš, 2011; Cashin and Unayama, 2016). If relative prices revert after the end of the program and new-car pur-

[9]Temporary income effects may offset substitution effects in similar ways. However, household incomes remained virtually unchanged in the data.

chases are sufficiently complementary to purchases of other goods and services, total non-car consumption may increase as well.

Hypothesis 3. *Intratemporal substitution does not fully crowd out the fiscal stimulus. Cars were not purchased instead of other goods, but in addition to them, such that overall consumption increased.*

3 Intertemporal Effects of the Scrappage Program

Our first two hypotheses concern the timing of new-car purchases and thus the intertemporal effects of the scrappage program. In Appendix B, we show that analyzing these effects allows us to bound the elasticity of intertemporal substitution. This requires that the policy first increases the demand for new cars (Hypothesis 1). The key idea is that the reduction in user costs is temporary. Any persistent effects—such as potential pull-forward dynamics—must therefore be driven by the elasticity of intertemporal substitution. If the demand for new cars returns to its pre-policy level after the program but does not fall below it, the intertemporal elasticity of substitution must be small and bounded by 1. In that case, there is no full reversal in new-car purchases and intertemporal substitution does not fully crowd out the fiscal stimulus (Hypothesis 2).[10]

The Cash-for-Clunkers policy was designed to stimulate household demand for new cars, a durable good that is typically purchased infrequently and at relatively high cost. Given the substantial price of a new vehicle, the flat subsidy of €2,500 might have been insufficient to induce additional purchases, particularly for liquidity-constrained households. Even for unconstrained households, participation might not have been attractive if the resale value of the old vehicle exceeded €2,500, in which case scrapping the car in order to obtain the subsidy would not be economically rational.

A first margin to consider is therefore the extensive margin, that is, whether a household purchased a new car at all, which provides evidence on the overall effectiveness of the program.

[10] See Appendix B for a formal discussion.

3.1 Data and Sample Construction

We use administrative monthly new-car registrations from the German Federal Motor Transport Authority (*Kraftfahrt-Bundesamt*) for 2005–2014. The underlying records distinguish private and commercial owners and report the five-digit postal code, manufacturer, model, fuel type, and vehicle characteristics. We aggregate postal codes to two-digit postal regions and construct monthly counts from the twelve monthly registration variables.

The main analysis compares private and commercial car registrations for the same manufacturer within the same two-digit postal region and year-month. We first aggregate the data to region-manufacturer-owner-year cells and retain region-manufacturer-year cells in which both owner groups are observed. Annual common support covers between 92% and 96% of observed region-manufacturer markets over 2005–2014. We begin the event window in April 2007 because the first quarter of 2007 was distorted by intertemporal substitution around the January 2007 VAT increase from 16% to 19%. After aggregating the data to region-manufacturer-owner-year cells, reshaping the registration counts into a monthly panel, and removing fixed-effect groups that are mechanically separated or singletons in Poisson estimation, the main sample contains 372,208 observations and 95 two-digit postal-region clusters. Table D.1 in the appendix presents descriptive statistics.

3.2 Empirical Strategy and Main Results

3.2.1 Manufacturer-Matched PPML Event Study

Let q_{rmot} denote the number of registrations in postal region r, for manufacturer m, owner group $o \in \{P, C\}$, and year-month t. The main specification models the conditional mean of q_{rmot} using Poisson pseudo-maximum likelihood (PPML). PPML retains observed zero-count cells, does not require the conditional variance to be Poisson, and permits coefficients to be interpreted as log incidence-rate ratios.

Let τ_t denote months relative to February 2009. December 2008, $\tau_t = -2$, is the omitted month. January 2009 is estimated explicitly as a transition month, and February 2009 is the first post-cutoff month. We estimate

$$\mathbb{E}[q_{rmot} \mid X] = \exp\left[\alpha_{rmo} + \lambda_{rmt} + \sum_{\substack{k\in\mathcal{K} \\ k\neq -2}} \beta_k \mathbb{1}\{o = P\}\mathbb{1}\{\tau_t = k\}\right]. \tag{1}$$

The fixed effects α_{rmo} absorb permanent differences between private and commercial registrations within each two-digit postal region and manufacturer cell. The fixed effects λ_{rmt} absorb arbitrary year-month-specific shocks common to both owner groups within the same region-manufacturer cell. Identification therefore comes from changes in the private-to-commercial registration ratio within the same matched manufacturer-market cell. Standard errors are clustered at the two-digit postal region.

Figure 1 reports the coefficients $\widehat{\beta}_k$ in their natural PPML scale. A coefficient is a log incidence-rate ratio: $\exp(\widehat{\beta}_k) - 1$ is the proportional change in the private-to-commercial registration ratio relative to December 2008. January 2009 is close to zero (0.018, standard error 0.037), supporting its interpretation as a transition month. The coefficient rises sharply to 0.756 in February, 1.060 in March, and peaks at 1.260 in May 2009. These estimates correspond to increases of 113%, 188%, and 252%, respectively, in the private-to-commercial registration ratio relative to December 2008.

The response declines during the second half of 2009. The coefficient is 0.615 in November and 0.366 in December, before falling to 0.061 in January 2010 and 0.009 in February 2010. Modest positive coefficients reappear around the June 2010 registration deadline, but they are small relative to the spring 2009 peak. The monthly path therefore shows a large and concentrated 2009 response followed by a return toward baseline rather than an immediate and pronounced post-program hole.

The pre-period does not display a sustained upward trend toward the cutoff. The final leads before the program are small: the October and November 2008 coefficients are -0.009 and -0.047, respectively, and January 2009 is close to zero. At the same time, the pre-period contains month-specific fluctuations, with a root-mean-square lead of 0.105. These fluctuations, together with the recurring movements visible in later post-program years, sug-

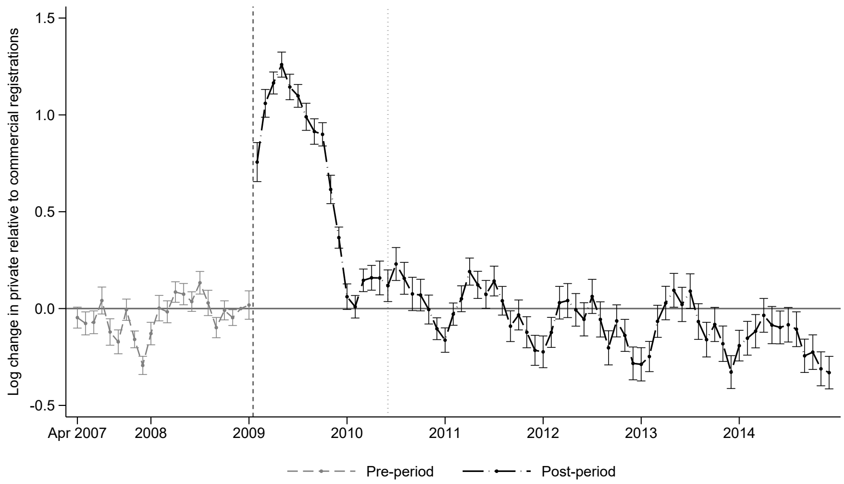

Figure 1: Private Relative to Commercial New-Car Registrations

Source: Own calculations based on data on registrations of new cars in Germany (2005–2014) provided by the German Federal Motor Transport Authority (*Kraftfahrt-Bundesamt*). Notes: This figure presents $\widehat{\beta}_k$ from Eq. (1). Coefficients are log incidence-rate ratios; $\exp(\widehat{\beta}_k) - 1$ gives the proportional change in the private-to-commercial registration ratio relative to December 2008. The dashed vertical line marks February 2009 and the dotted line marks the June 2010 registration deadline. The whiskers indicate 95% confidence intervals based on standard errors clustered at the two-digit postal region.

gest some remaining differential seasonality in the private-to-commercial registration ratio. The identifying variation in the baseline event study should therefore be interpreted as deviations from the matched manufacturer-market comparison, while robustness checks using lower-frequency aggregation help assess whether the main 2009 response is driven by within-year seasonality. The key event-study pattern is nevertheless consistent with the program interpretation: there is no pre-program drift toward treatment, the response turns sharply positive at the policy cutoff, and the large effect dissipates after the program period.

3.2.2 Robustness Checks

The manufacturer-matched event study in Eq. (1) compares private and commercial registrations within the same two-digit postal region, manufacturer, and year-month. This design removes shocks that are common to both owner groups within narrowly defined manufacturer markets. We consider three remaining concerns. First, the estimated response may partly reflect changes in product composition within manufacturers. Second, the result may depend on the weighting of disaggregated manufacturer-region cells. Third, the monthly estimates may contain differential within-year seasonality in the private-to-commercial registration ratio. We address these concerns using alternative PPML specifications, reported in Figure E.1 in the appendix.

First, we estimate the following model allowing for a finer region-manufacturer-model-fuel design, effectively replacing the index m for a manufacturer in Eq. (1) with j for manufacturer-model-fuel cells.

$$\mathbb{E}[q_{rjot} \mid X] = \exp\left[\alpha_{rjo} + \lambda_{rjt} + \sum_{\substack{k\in\mathcal{K} \\ k\neq -2}} \beta_k \mathbb{1}\{o = P\}\mathbb{1}\{\tau_t = k\}\right]. \tag{2}$$

The fixed effects α_{rjo} absorb permanent differences between private and commercial registrations within each region-product cell, while λ_{rjt} absorb arbitrary year-month-specific shocks common to both owner groups within the same region-product cell.

Figure E.1 compares this finer product-matched specification with the baseline manufacturer-matched PPML event study. The finer specification preserves the central pattern: the private-to-commercial registration ratio rises sharply after the introduction of the program, remains elevated through much of 2009, and returns toward baseline thereafter. The magnitudes are slightly smaller than in the manufacturer-level specification, indicating that part of the baseline response reflects shifts toward models and fuel types disproportionately purchased by private households. However, the timing of the break and the subsequent normalization remain visible when product composition is held fixed. The main result is therefore not solely an artifact of within-manufacturer changes in the model or fuel mix.

In the next step, we ask whether the main pattern survives aggregation over manufacturers. The check is relevant because the baseline estimand is constructed from disaggregated manufacturer-region markets. If the results were driven by a small number of manufacturers, by sparse manufacturer cells, or by the implicit weighting induced by the high-dimensional PPML design, the aggregate specification could produce a different event-study path. At the region-owner-year-month level, we estimate

$$\mathbb{E}[q_{rot} \mid X] = \exp\left[\alpha_{ro} + \lambda_{rt} + \sum_{\substack{k \in \mathcal{K} \\ k \neq -2}} \beta_k \mathbb{1}\{o = P\}\mathbb{1}\{\tau_t = k\}\right]. \tag{3}$$

The fixed effects α_{ro} absorb permanent differences between private and commercial registrations within each two-digit postal region, while λ_{rt} absorb arbitrary year-month-specific shocks common to both owner groups within the same region and year-month.

The region-aggregated monthly estimates in Figure E.1 are very close to the manufacturer-matched baseline. The timing, peak, and post-program normalization are essentially unchanged. This agreement is not mechanical: aggregation changes the observation cells, fixed effects, cell weights, and the estimand. The similarity therefore indicates that the main pattern is not driven by idiosyncratic manufacturer-market cells or by the weighting of disaggregated manufacturer observations.

Finally, we estimate yearly specifications that avoid the seasonality observed in the other specifications by summing over months within a calendar year.

$$\mathbb{E}[q_{roy} \mid X] = \exp\left[\alpha_{ro} + \lambda_{ry} + \sum_{\substack{k \in \mathcal{K} \\ k \neq 2008}} \beta_k \mathbb{1}\{o = P\}\mathbb{1}\{\tau_y = k\}\right]. \tag{4}$$

The fixed effects α_{ro} absorb permanent differences between private and commercial registrations within each two-digit postal region, while λ_{ry} absorb arbitrary year-specific shocks common to both owner groups within the same region.

The annual estimates confirm the main pattern at a lower frequency. The private-to-commercial registration ratio rises strongly in 2009 and then falls sharply in 2010. The estimates are close to zero in 2011 and become moderately negative in later years. Because the annual specification cannot identify the monthly timing of the response, it is not a substitute for the baseline event study. Rather, it shows that the large 2009 effect is not generated by recurring month-of-year fluctuations. Taken together, the robustness checks support the main interpretation: the scrappage program generated a large, concentrated increase in private registrations relative to commercial registrations in 2009, followed by a return toward baseline once the program period ended.

3.2.3 Local Difference-in-Discontinuities

To further address the month-specific fluctuations in the long pre-period, we estimate local piecewise-trend models around February 2009. Let $R_t = \tau_t$ and $Post_t = \mathbb{1}\{R_t \geq 0\}$. For a bandwidth h, the main local specification is

$$\begin{aligned}\mathbb{E}[q_{rmot} \mid X] = \exp\Big[&\alpha_{rmo} + \lambda_{rmt} + \theta_h \mathbb{1}\{o = P\} Post_t \\ &+ \pi_{\text{pre},h} \mathbb{1}\{o = P\} R_t + \pi_{\Delta,h} \mathbb{1}\{o = P\} R_t Post_t\Big], \quad -h \leq R_t \leq h-1.\end{aligned} \tag{5}$$

The coefficient $\pi_{\text{pre},h}$ captures the pre-program slope of the private-commercial registration gap, while $\pi_{\Delta,h}$ captures the change in that slope after the cutoff. The post-program slope is therefore $\pi_{\text{pre},h} + \pi_{\Delta,h}$. The parameter θ_h is the immediate change in the private-to-commercial registration ratio at the cutoff, conditional on separate pre- and post-cutoff slopes. We also estimate a two-period January–February difference-in-differences without slope terms. For $h \in \{12, 15, 18\}$, we replace α_{rmo} with region-manufacturer-owner-by-calendar-month fixed effects, allowing each local unit to have its own recurring seasonal pattern.

Table 1 shows positive discontinuities across all bandwidths. The January–February estimate is 0.733, corresponding to an incidence-rate ratio of 2.08. The piecewise-trend estimate rises from 0.752 in the ±3-month window to 1.261 in the ±12-month window and remains 1.193 in the ±18-month window. Allowing manufacturer-specific recurring seasonality yields estimates between 1.205 and 1.261. The recurring-seasonality estimates are close to the corresponding baseline estimates, indicating that the discontinuity is not explained by stable owner-specific seasonal patterns.

The level of the local estimate depends on the bandwidth because the pre-treatment path is not globally linear. We therefore emphasize the sign, timing, and consistency across specifications rather than a single preferred point estimate.

Table 1: Local Difference-in-Discontinuities

Specification	Window	Coefficient (SE)	IRR	Observations
Two-period DiD	Jan.–Feb.	0.733 (0.030)	2.082	7,048
Unit and market-month FE	±3	0.752 (0.034)	2.120	23,034
Unit and market-month FE	±6	0.944 (0.034)	2.570	47,178
Unit and market-month FE	±9	1.124 (0.027)	3.077	71,618
Unit and market-month FE	±12	1.261 (0.030)	3.529	95,632
Unit and market-month FE	±15	1.235 (0.027)	3.437	119,618
Unit and market-month FE	±18	1.193 (0.028)	3.297	143,156
Recurring seasonality	±12	1.205 (0.024)	3.338	87,196
Recurring seasonality	±15	1.261 (0.022)	3.530	109,714
Recurring seasonality	±18	1.215 (0.024)	3.372	131,638

Source: Own calculations based on data on registrations of new cars in Germany (2005–2014) provided by the German Federal Motor Transport Authority (*Kraftfahrt-Bundesamt*). Notes: This table presents $\hat{\theta}_h$ from Eq. (5). The two-period DiD and piecewise-trend specifications absorb region-manufacturer-owner fixed effects and region-manufacturer-year-month fixed effects. The recurring-seasonality specifications replace the region-manufacturer-owner fixed effects with region-manufacturer-owner-calendar-month fixed effects. All specifications cluster standard errors at the two-digit postal region. IRR denotes incidence-rate ratio.

3.2.4 Heterogeneity by Vehicle Characteristics

In the next step, we examine whether the local response differs across predetermined vehicle characteristics. Weight, horsepower, and displacement groups are defined using registration-weighted medians in 2007–2008. For characteristic group g, we estimate the 12-month local model

$$\begin{aligned}\mathbb{E}\Big[q^{(g)}_{rmot} \mid X\Big] = \exp\Big[&\alpha^{(g)}_{rmo,s(t)} + \lambda^{(g)}_{rmt} + \theta_g \mathbb{1}\{o=P\}Post_t \\ &+ \pi_{pre,g}\mathbb{1}\{o=P\}R_t + \pi_{\Delta,g}\mathbb{1}\{o=P\}R_tPost_t\Big],\end{aligned} \tag{6}$$

where $s(t)$ denotes calendar month. The specification allows manufacturer-owner units to have group-specific recurring seasonality.

Table 2 shows that the response is concentrated among smaller vehicles. The coefficient is 1.497 below the median weight and 0.563 above it; 1.636 for low-horsepower vehicles and 0.565 for high-horsepower vehicles; and 1.597 for low-displacement vehicles and 0.419 for high-displacement vehicles. The fuel estimates are 1.472 for petrol, 0.462 for diesel, and 0.374 for alternative-fuel vehicles. All estimates are positive and statistically precise, although the alternative-fuel estimate is less precise than the petrol and diesel estimates.

Table 2: Heterogeneity in the February 2009 Discontinuity

Characteristic	Group	Coefficient (SE)	95% CI	IRR
Weight	Low	1.497 (0.030)	[1.438; 1.557]	4.470
Weight	High	0.563 (0.017)	[0.529; 0.596]	1.755
Horsepower	Low	1.636 (0.025)	[1.587; 1.685]	5.134
Horsepower	High	0.565 (0.020)	[0.526; 0.603]	1.759
Displacement	Low	1.597 (0.027)	[1.544; 1.649]	4.936
Displacement	High	0.419 (0.019)	[0.381; 0.456]	1.520
Fuel	Petrol	1.472 (0.032)	[1.410; 1.534]	4.358
Fuel	Diesel	0.462 (0.022)	[0.420; 0.505]	1.588
Fuel	Alternative	0.374 (0.064)	[0.248; 0.500]	1.454

Source: Own calculations based on data on registrations of new cars in Germany (2005–2014) provided by the German Federal Motor Transport Authority (*Kraftfahrt-Bundesamt*). Notes: This table reports the immediate discontinuity from 12-month local PPML specifications with manufacturer-specific recurring seasonality. Weight, horsepower, and displacement splits use registration-weighted 2007–2008 medians. The groups describe heterogeneity and do not define untreated controls. IRR denotes incidence-rate ratio.

Similar to findings for the United States, these results are consistent with the fixed subsidy producing a larger proportional price reduction for smaller, less powerful cars and thus a shift toward cheaper models.

3.3 Additional Registrations and Fiscal Magnitudes

We use the main PPML estimates to construct an event-study counterfactual for private registrations. Let $\widehat{\mu}_{it}^1$ denote the fitted conditional mean for private region-manufacturer cell i in month t, including the estimated event-month coefficient. The counterfactual removes that coefficient while retaining the estimated fixed effects:

$$\widehat{\mu}_{it}^0 = \widehat{\mu}_{it}^1 \exp(-\widehat{\beta}_t), \tag{7}$$

$$\widehat{\Delta Q}_t = \sum_i \left(\widehat{\mu}_{it}^1 - \widehat{\mu}_{it}^0\right) = \sum_i \widehat{\mu}_{it}^1 \left[1 - \exp(-\widehat{\beta}_t)\right]. \tag{8}$$

We cumulate Eq. (8) from January 2009 through horizon H and present results in Table 3. Later negative coefficients reduce the cumulative total and therefore measure any potential post-program offset. The matched sample covers approximately 99.97% of national private registrations, so scaling to national totals has almost no effect.

By December 2009, the model implies 1.445 million additional private registrations (95% confidence interval 1.307–1.582 million), equal to 74.7% of the 1.933 million subsidized purchases. The estimate rises to 1.515 million by the June 2010 registration deadline and 1.557 million by December 2010. It then declines to 1.170 million by December 2014 (95% confidence interval 0.623–1.716 million). This is a partial long-run cumulative offset, not an immediate post-program reversal.

To translate registrations into targeted vehicle expenditure, we value each additional car at the average subsidized-car price, $\bar{p}$ = €13,682, and divide by the €5 billion program outlay:

$$\widehat{M}_Q(H) = \frac{\bar{p}\,\widehat{\Delta Q}(H)}{G}. \tag{9}$$

This calculation follows the logic of targeted vehicle-expenditure measures in the Cash-for-Clunkers literature, including Hoekstra et al. (2017). It is not a GDP multiplier or a total-consumption multiplier because it assumes a constant vehicle price and does not subtract spending displaced from other goods or other periods.

The implied quantity-based vehicle-expenditure ratio is 3.95 by December 2009, 4.15 by June 2010, and 3.20 by December 2014. The corresponding fiscal cost per additional car is €3,460, €3,299, and €4,275. These values should be interpreted as gross expenditure on vehicles associated with the estimated additional registrations per euro of public outlay.

3.4 Interpretation

The administrative registration data show a large and sharply timed increase in private registrations relative to commercial registrations beginning in February 2009. The response peaks in spring 2009, declines through the remainder of the year, and is close to zero at the beginning of 2010. This pattern is robust to finer model-and-fuel matching, aggregation to region-year-month cells, and aggregation to yearly registrations. Local difference-in-discontinuities estimates remain positive across bandwidths and after allowing manufacturer-owner units to have their own recurring seasonal patterns.

Table 3: Implied Additional Registrations and Fiscal Magnitudes

Horizon	Additional cars (95% CI)	Share subsidized	Cost per car	Vehicle-expenditure ratio
December 2009	1,444,882 [1,307,385; 1,582,379]	0.747	€3,460	3.954
June 2010	1,515,475 [1,343,297; 1,687,652]	0.784	€3,299	4.147
December 2010	1,557,047 [1,348,718; 1,765,376]	0.805	€3,211	4.261
December 2011	1,562,216 [1,279,360; 1,845,073]	0.808	€3,201	4.275
December 2014	1,169,577 [622,758; 1,716,396]	0.605	€4,275	3.200

Source: Own calculations based on data on registrations of new cars in Germany (2005–2014) provided by the German Federal Motor Transport Authority (*Kraftfahrt-Bundesamt*). Notes: This table shows additional registrations calculated from Eq. (8) and scaled to national private registrations. Confidence intervals use the delta method conditional on the fitted fixed effects and treat market coverage, the average vehicle price, and program outlay as fixed. The ratio in Eq. (9) uses an average vehicle price of €13,682 and program outlay of €5 billion.

Taken together, these results indicate that the empirical strategy isolates a strong direct effect of the scrappage scheme on new-car purchases. The timing of the response is difficult to reconcile with a smooth pre-existing trend: the increase begins at the policy cutoff, is concentrated during the program year, and dissipates after the program period. We therefore conclude that Hypothesis 1 is supported: the demand for new cars increased significantly in 2009.

The evidence does not, by itself, imply that the full gross increase in vehicle expenditure represents additional aggregate demand. A subsidy for durable goods can raise contemporaneous purchases either by inducing additional purchases or by shifting purchases forward from future periods. The monthly estimates return toward baseline without a pronounced short-run hole, while later negative coefficients imply at most a partial cumulative offset through 2014. Because the long pre-period is broadly stable but not perfectly flat, this interpretation also relies on the local specifications and on commercial registrations as a credible within-market comparison group.

In contrast to Mian and Sufi (2012), but consistent with Helm et al. (2023), we find no sharp reversal through 2014, which is consistent with limited pull-forward over the observed horizon. Instead, the estimates suggest that the 2009 increase was followed by normalization rather than a large post-program collapse. This pattern is consistent with limited intertemporal substitution: the subsidy appears to have induced a substantial contemporaneous purchase response without fully crowding out future purchases. The result is in line with evidence that the intertemporal elasticity of substitution is often modest (Hall, 1988; Ogaki and Reinhart, 1998; Yogo, 2004; Parker and Preston, 2005; Cashin and Unayama, 2016; Baker et al., 2021), and contrasts with larger estimates obtained without household-level data (e.g., Buettner and Madzharova, 2021). We therefore also find support for Hypothesis 2: intertemporal substitution did not fully crowd out the fiscal stimulus. New cars purchased in 2009 were, on average, not merely one-for-one substitutes for future purchases, implying an increase in overall car consumption over the observation window. However, household expenditure data remain necessary to quantify substitution across goods and the total-consumption multiplier.

4 Intratemporal Effects of the Scrappage Program

Hypothesis 3 concerns how the policy-induced price reduction affects expenditures across different consumption goods within the same period. In Appendix B, we show that these responses allow us to identify the degree of complementarity between cars and other goods and to bound the intertemporal elasticity of substitution using a theoretical framework based on Ogaki and Reinhart (1998) and Cashin and Unayama (2016).

4.1 Proxy-Eligible Group and Comparison Group

The scrappage program effectively created two groups of households: those eligible for the subsidy and those who were not. As discussed above, anticipation effects are unlikely because the program was announced only one month before its implementation. In this sense, the reform can be interpreted as an exogenous shock to household budgets. In addition, eligibility depended on criteria that were predetermined and therefore difficult to manipulate. One requirement was that the old vehicle had to be registered to the household for at least 12 months prior to the application. The second requirement was that this car had to be at least nine years old. These groups are also representative of the average car owner, since the average vehicle age in Germany at the time was about 8.5 years according to the German

Federal Motor Transport Authority. Consequently, there is substantial mass on both sides of the eligibility threshold.[11]

For the empirical analysis, we rely on data from the German Socio-Economic Panel (SOEP, described in Appendix F). Although actual eligibility status is not directly observable, two survey questions allow us to construct a proxy-eligible group and a comparison group based on two indicators. The first indicates whether the household owned a car in 2008; households without a car were not eligible for the program. The second records whether a household purchased a car during the previous 12 months; if so, the household would again be ineligible. We define the eligibility proxy EP_i as

$$EP_i = \begin{cases} 0 & \text{if no car owned in 2008 } \textit{or} \text{ car purch. in last 12 months} \\ 1 & \text{if car owned in 2008 } \textit{and} \text{ car not purch. in last 12 months.} \end{cases} \tag{10}$$

This proxy leaves room for misclassification in one direction: some households classified as proxy-eligible own a vehicle younger than the nine-year threshold and are in fact ineligible. Our estimates should therefore be interpreted as lower-bound intention-to-treat (ITT) effects. As a check on this source of attenuation, we also construct a stricter proxy that additionally exploits the SOEP car-purchase histories back to 2000: a household is classified as proxy-eligible under the strict proxy only if its vehicle was verifiably at least nine years old in 2009. The strict proxy is available for a subset of households and, as expected, predicts higher CfC take-up (46.5% versus 40.5% under the baseline proxy).

If the eligibility rules generate quasi-random assignment, eligibility should be the main determinant of program participation. To assess this assumption, we estimate probit models of program participation including our eligibility proxies as well as a rich set of controls such as income, wealth, savings, patience and risk aversion, and household composition.[12] Table H.1 in the appendix reports the resulting marginal effects. Both proxy variables are highly statistically significant and economically strong predictors of program participa-

[11] Take-up of the subsidy might nevertheless be incomplete. Information frictions, individual valuations, or the market value of the old vehicle could reduce participation rates.

[12] See Table F.1 in the appendix for a description of the control variables used.

tion. Their coefficients are virtually unchanged across specifications in Columns (2) and (3). Consistent with this interpretation, Figure H.1 in the appendix shows that the predicted participation probabilities are similarly distributed among program participants.

In the next subsections, we present ITT effects throughout, since proxy-eligibility does not necessarily imply actual program participation. This parameter captures the impact of being proxy-eligible for the Cash-for-Clunkers subsidy on household consumption behavior and, therefore, reflects the policy effect of the program as implemented.[13]

4.2 Program Effects in the Full Sample

An important feature of our design is that proxy-eligibility is defined for all households, not only for car buyers. Our first household-level evidence therefore comes from the full SOEP sample (N = 12,790; N = 7,322 with the complete set of control variables). In a first step, we estimate the effect of proxy-eligibility on the probability of purchasing a car in 2009 using a linear probability model—the household-level analogue of the registration evidence in Section 3. Then, we estimate the consumption response for category c on the full sample by

$$Expenditures_{c,i} = \beta_{c,0} + \beta_{c,1} EP_i + \beta_{c,2} \boldsymbol{X}_i + u_{c,i}, \tag{11}$$

where $Expenditures_{c,i}$ denotes the log of expenditures (plus €1) of household i, EP_i is defined in Eq. (10), and $u_{c,i}$ is the error term.

Table 4 reports the results. Proxy-eligibility raises the probability of purchasing a car in 2009 by 6.5 percentage points, relative to a baseline purchase probability of 11.3% (see Table F.1 in the appendix). This is a large extensive-margin response and mirrors, in the

[13] While we observe program take-up and could in principle instrument participation using our eligibility proxies, such estimates would identify a local average treatment effect for the subset of households whose participation decision was induced by proxy-eligibility. Because car purchases are large and infrequent, IV estimates scale the ITT effect by the first-stage difference in take-up rates and yield very large coefficients that primarily reflect the lumpy nature of car expenditures. We therefore focus on the ITT estimates and use the first stage (0.045 with a robust standard error of 0.004 in the full sample and 0.253 with a robust standard error of 0.041 in the car-buyer sample) only to interpret magnitudes in Section 4.4.

Table 4: Effect of Cash-for-Clunkers on Consumption: Full Sample

	Car purchase	Log expenditures				
		CAR	DCG	NDCG	SER	Total
CfC eligibility proxy	0.065***	0.605***	−0.017	0.090***	0.471***	0.209***
	(0.008)	(0.070)	(0.089)	(0.015)	(0.054)	(0.017)
Controls	✓	✓	✓	✓	✓	✓
# Households	7,322	7,322	7,322	7,322	7,322	7,322

Source: Own calculations based on the SOEP (2007–2010). Notes: This table shows the effect of Cash-for-Clunkers proxy-eligibility, Eq. (10), in the full SOEP sample, i.e., without conditioning on a 2009 car purchase. Column (1) reports estimates from a linear probability model for purchasing a car in 2009. Columns (2) to (6) report ITT effects on log expenditures (plus €1). Robust standard errors in parentheses. Romano-Wolf step-down p-values (Romano and Wolf, 2005) using 4,999 bootstrap replications, adjusted for multiple testing across the five categories are 0.000 (CAR), 0.843 (DCG), 0.000 (NDCG), 0.000 (SER), and 0.000 (Total). *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

household data, the registration evidence of Section 3.[14]

Turning to expenditures, proxy-eligible households spend 0.605 log points more on cars than comparison households, and total consumption is 0.209 log points higher. Importantly, no expenditure category declines significantly: the ITT effects on durables, non-durables, and services are −0.017, 0.090, and 0.471, respectively.[15] The full-sample evidence is therefore directly informative about Hypothesis 3: the program raised car spending and overall consumption without crowding out other categories.[16]

4.3 Spending Responses among Car Buyers

The full-sample estimates combine two margins: proxy-eligible households are more likely to purchase a car (extensive margin), and, conditional on purchasing, they may spend different

[14]In the Online Appendix, we show that the SOEP data yield the same qualitative conclusion with regard to Hypotheses 1 and 2 as the KBA registration data across alternative estimation designs.

[15]Because car-owning and non-car-owning households may still differ along other dimensions, we mitigate this in two ways. First, all specifications control for income in 2008, wealth, savings, patience and risk aversion, and household composition. Second, the same qualitative pattern—a large positive car effect and no negative cross-category effect—holds in the buyer sample of Section 4.3, where treatment and control households all purchase a car. We also verify that proxy-eligibility does not predict the change in household income between 2008 and 2009, so differential income shocks do not drive the full-sample estimates.

[16]The full-sample estimates are also robust to the functional form of the outcome. Because expenditures contain zeros, $\log(x+1)$ treatment effects are not invariant to the unit of measurement (Chen and Roth, 2024). Re-estimating Table 4 by Poisson pseudo-maximum likelihood (PPML) yields similar effects on mean expenditures of 0.823 (0.159) for cars and 0.194 (0.024) for total consumption, with positive point estimates for all other categories (Table I.1 in the appendix).

Table 5: Effect of Cash-for-Clunkers on Consumption: Car Buyers

	Log expenditures				
	CAR	DCG	NDCG	SER	Total
CfC eligibility proxy	0.294**	−0.170	0.039	0.034	0.148**
	(0.116)	(0.376)	(0.045)	(0.125)	(0.062)
Controls	✓	✓	✓	✓	✓
# Households	892	892	892	892	892

Source: Own calculations based on the SOEP (2007–2010). Notes: This table shows the effect of Cash-for-Clunkers proxy-eligibility on the expenditures of households that purchased a car in 2009. Columns (1) to (5) report ITT effects on log expenditures (plus €1). Robust standard errors in parentheses. Romano-Wolf step-down p-values (Romano and Wolf, 2005) using 4,999 bootstrap replications, adjusted for multiple testing across the five categories are 0.029 (CAR), 0.865 (DCG), 0.763 (NDCG), 0.865 (SER), and 0.029 (Total). *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

amounts (intensive margin). To isolate the intensive margin and to compare treated and control households that all undertake the same discrete purchase decision, we next restrict the sample to households that purchased a car in 2009 and re-estimate Eq. (11) (N = 1,624; N = 892 with the complete set of control variables).

Conditioning on a 2009 car purchase requires care, because the purchase decision is itself an outcome of the program: proxy-eligible buyers include households drawn into the market by the subsidy, while comparison buyers are a self-selected group. The buyer-sample estimates should therefore be read as descriptive of the intensive margin. We address the selection margin formally below using the bounding approach of Lee (2009).

Table 5 reports the results. Car spending is 0.294 log points higher while total consumption is 0.148 log points higher. Cross-category effects are small and statistically indistinguishable from zero, and—importantly—no category declines. Adjusting inference for multiple testing across the five categories, the car and total-consumption effects remain significant at the 5% level (Romano-Wolf p = 0.029), while the cross-category effects are not statistically significant. The buyer-sample evidence is thus consistent with the full-sample evidence: proxy-eligible households increased car expenditures without reducing spending elsewhere, supporting Hypothesis 3.

Selection into purchasing. Because proxy-eligibility raises the purchase probability, the composition of proxy-eligible and comparison buyers may differ. Following Lee (2009), we bound the proxy-eligibility effect among buyers by trimming the proxy-eligible-buyer outcome distribution by the differential selection rate (a trimming proportion of 0.215). The bounds for the car-spending effect are [0.181, 0.886], excluding zero even in the worst case. For total consumption the bounds are [−0.004, 0.400]: the lower bound just includes zero, so while the buyer-sample data cannot rule out a null intensive-margin effect on total consumption under extreme selection, they do rule out crowding out. The bounds for non-durables, [−0.185, 0.218], are symmetric around zero, consistent with no cross-category response. Table I.2 in the appendix reports the full set of bounds.[17]

Functional form. Re-estimating Eq. (11) by PPML yields buyer-sample effects of 0.100 (0.107) for cars and 0.055 (0.068) for total consumption—smaller than the log estimates and not statistically significant (Table I.1 in the appendix). The intensive-margin estimates are therefore sensitive to the weighting of large expenditures implicit in the levels specification, whereas the full-sample estimates of Section 4.2 are robust across functional forms. This pattern indicates that the program's effect operates predominantly through the extensive margin—additional and earlier purchases—rather than through large proportional increases in spending among households that would have purchased in any case. We return to this decomposition in Section 5.

Proxy quality and control-group composition. Table I.3 in the appendix re-estimates the main specification after excluding recent-buyer controls, after excluding no-car controls, and after replacing the baseline proxy with a stricter age-history proxy. The car-spending effect remains positive in all specifications. The strict proxy uses observed purchase histories to identify a subset of households whose vehicle is more likely to satisfy the nine-year age requirement; in this smaller sample, the estimated effects are slightly larger than in the baseline. This is consistent with attenuation from classification error in the baseline proxy, but it

[17] The bounds in Table I.2 are calculated without covariate adjustment using the full sample of 12,790 households, of whom 1,624 purchased a car.

should not be read as evidence based on perfectly observed legal eligibility. The strict proxy remains imperfect (exact vehicle age, registration history, and the identity of the scrapped vehicle remain unobserved) and is available only for a selected subset of households. The total-consumption effect is weaker when identification relies on recent-buyer controls alone, which we view as the least informative configuration. This concern applies primarily to the buyer-sample intensive-margin estimates. The full-sample ITT estimates use all households and therefore do not rely on the small set of proxy-ineligible households that purchased a car in 2009.

4.4 Implied Substitution Elasticities

In the framework of Ogaki and Reinhart (1998), the eligibility treatment corresponds to a temporary reduction in the user cost of cars, as shown in Appendix B, and the difference between the treatment effects for cars and non-durables identifies the intratemporal elasticity of substitution $\varepsilon = -(\Delta \log k_t - \Delta \log x_t)/\Delta \log Q_t$. The numerator is directly estimated: $0.294 - 0.039 = 0.255$ log points in the buyer sample (see Table 5).

Converting this reduced-form difference into an elasticity requires taking a stand on the denominator, the change in log user costs, and on the wedge between the ITT and the response of actual participants. Because only participants receive the price cut, the proxy-eligibility indicator shifts the expected user cost by the first-stage take-up differential times the participant-level user-cost change, so the appropriate numerator for the participant-level elasticity is the ITT difference scaled by the first-stage estimate (0.253), i.e., 0.255/0.253. The participant-level user-cost change, in turn, depends on the rebate's share of the purchase price ($\nu \approx 0.2$ for the average subsidized vehicle, gross of incidence effects; Kaul et al., 2016) and on the depreciation and interest rates, since $Q_t = p_t(1 - \nu_t) - \frac{1-\delta}{1+r} p_{t+1}$. For plausible depreciation rates the user cost is a small fraction of the price, so a given rebate implies a large proportional user-cost reduction; for low depreciation rates the user cost approaches zero and the elasticity is not identified from this variation.

Table J.1 in the appendix reports the implied ε over a grid of calibrations. Under annual depreciation rates between 20 and 30% and $r \in \{0, 0.03\}$, the implied elasticity ranges from roughly 0.446 to 1.030; higher depreciation rates push the estimate above one, and the limiting calibration $\delta = 1$, $r = 0$ (a one-period durable) delivers $\varepsilon \approx 4.516$. Non-durable and service expenditures did not fall in response to a large induced increase in car expenditures, in the full sample or the buyer sample, which rules out strong intratemporal substitution and, combined with the increase in overall consumption, implies $\varepsilon < \sigma$ within the model of Appendix B. Together with the absence of a full post-program reversal in Section 3, which bounds σ below one, the evidence is consistent with low intertemporal substitution and durable-non-durable complementarity documented by Cashin and Unayama (2016), while the precise magnitude of ε remains sensitive to calibration.

5 The Impact of Cash-for-Clunkers on Aggregate Demand

This paper shows that the Cash-for-Clunkers program significantly increased demand for new cars and that, on average, proxy-eligible households did not reduce spending on other consumption categories. In this section, we combine both pieces of evidence in a back-of-the-envelope assessment of the program's effect on aggregate demand. This calculation measures the gross additional consumption expenditure associated with the program per euro of public outlay. Because participation required households to finance a purchase several times the size of the rebate, most of this expenditure consists of private funds mobilized by the subsidy rather than direct spending out of the transfer; the calculation is therefore not a marginal propensity to consume (MPC) out of a windfall and is not directly comparable to MPC estimates from unconditional rebate programs.

Registration-based evidence. In Section 3, the event-study counterfactual implies 1.445 million additional private registrations by December 2009, corresponding to 74.7% of all subsidized purchases (i.e., 1,933,090). The estimate rises to 1.515 million by the June 2010 registration deadline (78.4%) and remains positive, at 1.170 million, by December 2014 (60.5%). Valuing these additional registrations at the average price of subsidized vehicles, €13,682, implies gross additional vehicle spending of approximately €19.77 billion by December 2009 and €20.73 billion by the June 2010 registration deadline. Relative to the €5 billion program budget, this corresponds to a targeted vehicle-expenditure ratio of 3.95 by December 2009 and 4.15 by June 2010 (see Table 3), and a fiscal cost per additional car of approximately €3,460 and €3,299, respectively.[18]

Household-based evidence. The registration data measure additional vehicle purchases but are silent on other spending. The SOEP estimates of Section 4 allow an independent calculation that covers total consumption and that decomposes the response into an extensive and an intensive margin. Let N^E denote the weighted number of eligible households, ΔP the eligibility effect on the purchase probability, P^0 the counterfactual purchase rate (the observed weighted rate among eligible households, 13.9%, minus ΔP), $\bar{k}^1$ the mean car spending of eligible buyers (€13,296), and $\bar{k}^0 = \bar{k}^1 / \exp(\hat{\beta}_{CAR})$ its counterfactual under the intensive-margin ITT of Table 5. Additional aggregate car spending is then

$$\Delta K \; = \; N^E \Big[\Delta P \cdot \bar{k}^1 \; + \; P^0 \big(\bar{k}^1 - \bar{k}^0 \big) \Big], \tag{12}$$

where the first term in brackets is the extensive margin (spending by induced buyers) and the second the intensive margin (higher spending by households that would have purchased in any case). The calculation for total consumption is analogous, with the extensive-margin term using the spending difference between eligible buyers and eligible non-buyers (€26,140 versus €10,404).

Using the number of proxy-eligible households ($N^{\tilde{E}} = 19.9$m) and the extensive-margin estimate ($\Delta P = 6.5$ percentage points) yields 19.9m $\times$ 0.065 $\times$ €13,296 $=$ €17.2bn and 19.9m $\times$ 0.074 $\times$ (€13,296$-$€9,909) $=$ €5.0bn. Thus, the €22.2 billion car-expenditure estimate consists of an extensive-margin component of about €17.2 billion from additional car buyers and an intensive-margin component of about €5.0 billion. For total consumption

[18] Klößner and Pfeifer (2018) estimate that about 44% of car purchases under the Cash-for-Clunkers program correspond to "on-top" purchases. Our estimated share of 61 to 78% is larger. Using their estimate would imply around 1,933,090 $\times$ 0.44 $=$ 850,560 additional purchases. Valued at the average subsidized vehicle price of €13,682, this corresponds to €11.64 billion in additional vehicle demand, or €2.3 per euro of the €5 billion program outlay. The corresponding fiscal cost is about €5,900 per additional vehicle.

Table 6: Program-Associated Additional Consumption per Euro of Outlay

	Additional spending (bn €)		Per euro of outlay	
	Cars	Total cons.	Cars	Total cons.
Registration-based (Table 3)				
Counterfactual, Dec. 2009	19.77	–	3.95	–
Counterfactual, June 2010	20.73	–	4.15	–
Household-based decomposition, Eq. (12)				
$\Delta P = 3.1$ pp (no controls)	15.5	17.4	3.10	3.49
$\Delta P = 6.5$ pp (with controls)	22.2	25.7	4.44	5.13
Full-attribution scenario				
Full-sample PPML, proxy-eligible pop.	20.7	44.6	4.14	8.92

Source: Own calculations based on the SOEP (2007–2010) and the German Federal Motor Transport Authority (*Kraftfahrt-Bundesamt*) (2005–2014). Notes: This table reports the additional aggregate expenditure associated with the Cash-for-Clunkers program implied by the household-level decomposition in Eq. (12), alongside the registration-based estimates of Table 3 and the full-attribution benchmark that attributes the entire weighted spending gap between proxy-eligible and comparison households (PPML, full sample) to the program. All figures are gross expenditure associated with the program; they include households' own funds mobilized by the subsidy and do not net out general-equilibrium adjustments.

this sum is €25.7 billion. Table 6 summarizes. Per euro of the €5 billion outlay, the program was thus associated with roughly €3.1–4.4 of additional car expenditure and €3.5–5.1 of additional total consumption in the short run.

Two features of these figures deserve emphasis. First, the household-based and registration-based estimates are of a similar order of magnitude: the decomposition implies additional car spending of €15.5–22.2 billion, bracketing the €19.8 billion implied by the administrative registration counterfactual, even though the two calculations use entirely different data, identification, and aggregation. The bulk of the effect operates through the extensive margin, consistent with the functional-form evidence of Section 4.3. Second, additional total consumption exceeds additional car spending: the program did not merely relabel spending as car spending, in line with the absence of negative cross-category effects in Tables 4 and 5.

Several caveats apply. The decomposition attributes the proxy-eligible-comparison differences of Section 4 causally to the program; to the extent that proxy-eligible households (car owners with older vehicles) would have spent differently for unrelated reasons, the decomposition may overstate the program's causal effect. As a mechanical benchmark, Table 6 therefore also presents a full-attribution scenario that applies the entire full-sample PPML expenditure difference to the weighted proxy-eligible population. This scenario yields €20.7 billion in car expenditure and €44.6 billion in total consumption. The calculation also abstracts from general-equilibrium effects such as price adjustments, supply responses, displacement in the used-car market, and the financing of the program.

How should these magnitudes be interpreted? Participants received rebates totaling about €4.8 billion, but receipt was tied to the purchase of a new car. The relevant object is therefore not a conventional MPC, but the amount of expenditure mobilized per euro of subsidy. By this metric, the German program induced an additional vehicle purchase at a fiscal cost of roughly €3,300–3,500. This exceeds the statutory €2,500 rebate because some subsidized purchases would have occurred even without the program, but it remains well below the average subsidized vehicle price of €13,682.

For comparability with the rebate-MPC literature, we also compute an MPC-style statistic that excludes the gross vehicle expenditure mechanically tied to participation. The household decomposition implies additional total consumption of €17.4–25.7 billion and additional car expenditure of €15.5–22.2 billion, leaving €2.0–3.5 billion in additional non-car consumption. Dividing by the €5 billion program outlay yields 0.39–0.69; using actual rebate payments of €4.83 billion yields 0.40–0.72. This statistic is close to conventional rebate MPCs but is driven by proxy-eligibility that was conditional on a durable purchase.

6 Conclusion

Facing the Great Recession in 2008, many countries introduced Cash-for-Clunkers programs that subsidized new-car purchases conditional on scrapping an old vehicle. These policies aimed not only to improve fuel efficiency but also, and perhaps more importantly, to stimulate economic activity. Germany implemented by far the largest program, allocating €5 billion and subsidizing nearly 2 million new-car purchases. This paper uses vehicle registration data and household-level expenditure data to estimate the consumption response to this large and unanticipated fiscal stimulus.

Using administrative vehicle registration data, our results show a sharp increase in private new-car registrations relative to commercial registrations in 2009. In contrast to findings for the United States (see Mian and Sufi, 2012), we do not observe a full reversal of new-car purchases after the program ended. Instead, private registrations return toward baseline afterward and do not exhibit a sharp immediate post-program reversal, although the cumulative estimates indicate a partial long-run offset by 2014. The counterfactual calculations imply approximately 1.45 million additional private registrations by December 2009 and 1.52 million by the June 2010 registration deadline. This suggests that many cars were purchased on top of, rather than instead of, future purchases. Thus, the evidence supports the hypothesis that intertemporal substitution did not fully crowd out the stimulus over the observed horizon and is consistent with an intertemporal elasticity below one.

Using household-level expenditure data, our results provide little evidence that higher car spending displaced other consumption. In the full sample, proxy-eligibility raised the probability of purchasing a car by 6.5 percentage points and was associated with increases of 0.605 log points in car spending and 0.209 log points in total consumption, without a significant decline in any other category. Among car buyers, the corresponding estimates are 0.294 and 0.148 log points. Selection bounds and alternative functional forms indicate that the program's effect operated predominantly through the extensive margin, while the precise intensive-margin effect is less certain. The 0.255-log-point difference between the buyer-sample effects on cars and non-durables is the numerator of the intratemporal elasticity rather than the elasticity itself. After rescaling for take-up and calibrating the user-cost change, the implied elasticity ranges from approximately 0.446 to 1.030 under commonly used depreciation and interest-rate assumptions, with the precise magnitude remaining sensitive to calibration. Qualitatively, the absence of negative cross-category effects rules out strong contemporaneous substitution away from other consumption.

Finally, the registration-based and household-based calculations produce similar estimates of the expenditure mobilized by the program. The household decomposition implies approximately €3.1–4.4 of additional car expenditure and €3.5–5.1 of additional total consumption per euro of public outlay in the short run. These figures include the private funds that households contributed to their vehicle purchases and should therefore not be interpreted as conventional MPCs. After excluding the gross vehicle expenditure tied to participation, the implied increase in non-car consumption is approximately €0.39–0.69 per euro of program outlay. This MPC-style statistic facilitates comparison with the rebate literature, but the conditional nature of the subsidy limits a direct comparison with unconditional transfers or with the predictions of the permanent-income hypothesis.

Overall, our findings suggest that the German Cash-for-Clunkers program was effective in boosting short-term demand during the Great Recession without a full offset from lower future registrations. These results are relevant for policymakers who consider similar policies during economic downturns. The estimates presented in this paper provide a benchmark for evaluating future scrappage programs and for comparing their effectiveness with alternative fiscal stimulus policies. For example, during the COVID-19 pandemic, a new scrappage scheme was debated in Germany.[19] Instead, the German government implemented a temporary reduction in value-added tax to stimulate consumption (Bachmann et al., 2026). Comparing the effectiveness of such broad-based fiscal policies with targeted programs like Cash-for-Clunkers remains an important topic for future research.

[19] Eventually, a program similar to the Cash-for-Clunkers scheme—the so-called "Innovationsprämie" (innovation premium)—was introduced in June 2020. Households purchasing an electric or hydrogen-powered vehicle received an additional subsidy of €3,000 without being required to scrap a used car.

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Appendix

A Institutional Background

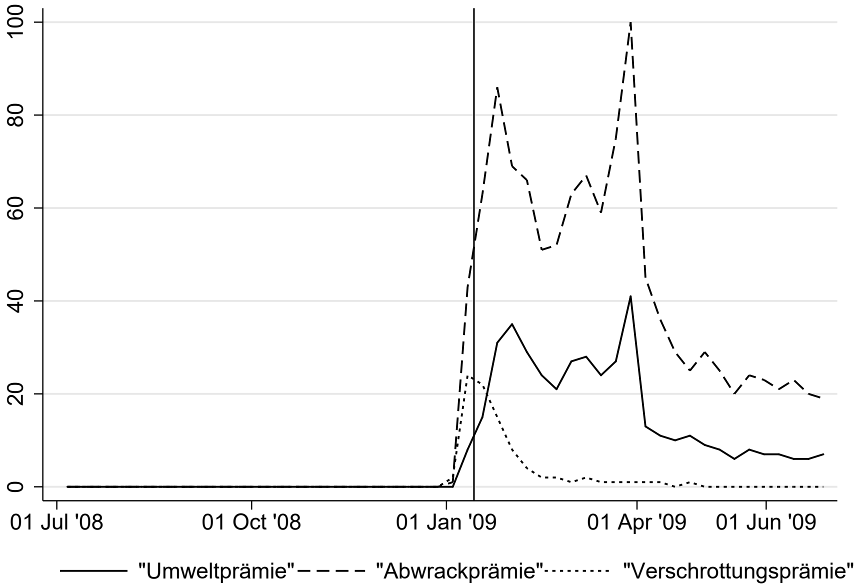

Figure A.1: Google Searches for Cash-for-Clunkers Policy in Germany

Source: Own calculations based on Google Trends. Notes: Data from `https://trends.google.de/`. The vertical solid line marks January 14, 2009, the day the Cash-for-Clunkers program came into effect. "Umweltprämie:" official term of the policy; "Abwrackprämie:" colloquial term of the policy; "Verschrottungsprämie:" term used in earlier, non-specific discussions about Cash-for-Clunkers policies.

Table A.1: Budgets for Scrapping Schemes Worldwide 2008–2011

Country	Period	Budget in m €	Budget per capita in €	Subsidized vehicles
Austria	09/01–09/07	45.00	5.39	30,000
Canada	09/01–11/07	35.62	1.06	70,000
China (1)	09/06–10/01	125.94	0.60	233,222
China (2)	10/01–10/06	679.70		508,146
Cyprus (1)	09/01–09/09	8.50	10.67	2,300
Cyprus (2)	09/10–10/12	2.00		1,000
France	08/12–10/12	600.00	9.32	900,000
Germany	09/01–09/09	5,000.00	60.97	1,933,090
Greece	09/09–09/11	70.00	6.22	77,234
Ireland	10/01–11/06	46.38	10.38	33,580
Italy	09/02–09/12	1,284.00	21.38	700,000
Japan	09/06–10/09	2,900.00	22.82	1,800,000
Luxembourg	09/01–10/07	10.00	20.26	6,000
Malaysia	09/01–09/12	33.10	1.29	33,100
Netherlands	09/05–10/03	130.00	7.89	64,625
Portugal	09/01–09/12	45.00	4.23	41,735
Romania (1)	09/03–09/12	30.00	1.40	32,327
Romania (2)	10/01–10/11	25.00	1.16	9,000
Russia (1)	10/03–10/11	255.80	2.72	400,000
Russia (2)	10/11–11/09	125.00		100,000
Serbia	09/04–09/12	5.00	0.68	5,000
Slovak Republic	09/03–09/12	55.30	10.22	34,981
South Korea	09/05–09/12	307.40	6.34	307,400
Spain	09/05–10/09	560.00	12.22	480,000
UK	09/05–10/02	920.00	14.94	314,190
USA	09/07–09/11	2,000.00	6.51	677,842
World		15,298.74		8,794,772
EU		8,831.18		4,660,062

Source: Own calculations based on ACEA (Association des Constructeurs Européens d'Automobiles) (2010a;b), Cooper et al. (2010), Haugh et al. (2010), IHS Global Insight (2010a;b). Population data for 2009 are taken from the World Bank (2020). Notes: Foreign currencies are converted into euros using average 2009 exchange rates.

Table A.2: Timeline of the German Cash-for-Clunkers Program

Date	Event
	Discussion and preparation
October 14, 2008	The president of the German Association of the Automotive Industry (VDA) proposes a scrappage program
December 27, 2008	Vice chancellor Steinmeier announces a scrappage scheme as part of a stimulus package
	Launching and start
January 14, 2009	Government introduces the scheme through a bill, start of the application period
January 16, 2009	Federal agency publishes details on the procedure
January 27, 2009	Paper-based application possible
February 20, 2009	Applicants have to hand in the original of the car license instead of just a copy
	Online application and budget increase
March 30, 2009	Change to online applications and reservations
April 7, 2009	Budget increase from €1.5 to €5 billion announced
May 28, 2009	The German parliament approves the budget increase
June 1, 2009	Law to finance the budget increase passes
July 2, 2009	Possibility to receive the subsidy for nearly new vehicles (up to 14 months old)
	End of the program
September 2, 2009	Budget of €5 billion is exhausted
June 30, 2010	Deadline for registering a new car and submitting the remaining documents

Source: Own presentation. Notes: This table shows the timeline of main events related to the introduction and implementation of the Cash-for-Clunkers program.

B Theoretical Background

We derive our Hypotheses 1 to 3 from a life-cycle model for the demand for durable and non-durable goods. Appendix C provides the derivations. In this framework, we assume for simplicity that cars are the only durable good and that the Cash-for-Clunkers program unexpectedly reduced car prices, so households could not save in anticipation. Data on consumer prices for cars from the Federal Statistical Office (CC13-0711) show that car prices were virtually constant in the years around the policy change. Month-to-previous-year's-month growth rates average 0.2% between January 2008 and December 2012.[20] Therefore, we assume constant car prices $p_{t-1} = p_t = p_{t+1} = p$ for non-participants. The Cash-for-Clunkers scrappage scheme introduced a price cut in period t to $p_t(1-\nu_t)$ for program participants, so $p_{t-1} = p_{t+1} = p > p_t(1-\nu_t)$, where ν_t denotes the temporary price cut.[21] This price cut reduced the user cost of cars in period t, so that $Q_t < Q_{t+1} = Q_{t-1}$; the user cost reverted to its unsubsidized level in period $t+1$. Equivalently, without requiring prices to be constant, we can compare the user costs of participants and non-participants within each period: $Q_t^{\text{CfC}} < Q_t^{\text{NoCfC}}$, while $Q_{t+1}^{\text{CfC}} = Q_{t+1}^{\text{NoCfC}}$ and $Q_{t-1}^{\text{CfC}} = Q_{t-1}^{\text{NoCfC}}$. In the framework of Ogaki and Reinhart (1998), the user costs are defined as

$$Q_t = p_t(1-\nu_t) - \frac{1-\delta}{1+r} p_{t+1}, \tag{13}$$

where r is the interest rate, δ is the depreciation rate, and $\nu_t = 0$ for non-participants. The absolute effect of the price cut on user costs does not depend on depreciation or the interest rate: the user-cost difference between program participants and non-participants is $-\nu_t p_t$ in period t. The corresponding proportional or log change in user costs, which we use in the elasticity calibration (see Table J.1), does depend on δ and r, because these parameters determine the level of user costs. Because the price cut was temporary and we assume no other price changes, $Q_{t-1} = Q_{t+1}$ for program participants. The optimal allocation of

[20] The average price change before the policy change in 2008 was 0.6%, and the average price change after the policy change in 2010–2012 was 0.2%.

[21] Although the actual price cut corresponded to a lump sum of €2,500, ν_t is a proportional price reduction that varied with the price of the car. For an average new car that costs around €12,000 (see Table F.1), the Cash-for-Clunkers program led to a relative price cut of around $\nu_t = 2{,}500/12{,}000 \approx 0.2$.

consumption between the durable car stock k_t and non-durable consumption x_t is

$$\frac{k_t}{x_t} = \frac{b}{1-b} Q_t^{-\varepsilon}, \tag{14}$$

where $0 < b < 1$ weights cars relative to non-durable goods according to household tastes.[22] If cars and non-durables are perfect complements, that is, $\varepsilon = 0$, a change in user costs does not change the proportions in which car services and non-durables are consumed. With $\varepsilon = 1$, preferences are Cobb–Douglas: a reduction in user costs increases the car-service stock relative to non-durable consumption, while leaving relative expenditure shares unchanged. If $\varepsilon > 1$, car services and non-durables are substitutes, and a reduction in user costs increases expenditure on cars relative to non-durables. We expect cars to be complementary to non-durable goods, that is, $0 < \varepsilon < 1$.

Eq. (14) defines the elasticity of intratemporal substitution in terms of changes in the durable car stock and non-durable consumption among program participants relative to non-participants. In the empirical application, the SOEP records car-purchase expenditure rather than the durable stock directly. We therefore use the difference between the estimated car-expenditure and non-durable-expenditure effects as a proxy for the numerator $\Delta \log k_t - \Delta \log x_t$. Table 5 gives a reduced-form difference of $0.294 - 0.039 = 0.255$ log points. This difference is not itself an elasticity. Because the empirical treatment is proxy-eligibility whereas only participants receive the price reduction, the participant-level numerator additionally requires rescaling by the first-stage take-up differential. Combining this rescaled numerator with calibrated values of $\Delta \log Q_t$ produces the elasticity estimates reported in Table J.1.

The empirical car-expenditure effect indicates that the program increased gross investment in the durable car stock. In the model, a reduction in user costs raises the desired car stock k_t. Through the stock-accumulation equation $k_t - (1-\delta)k_{t-1}$, this increase in the desired stock generates additional vehicle purchases during the program period.

[22] In the empirical application, we interpret observed car expenditure as gross investment in the durable stock.

The Euler equation for the durable car stock shows that the growth rate of the car stock is determined by the cut and reversal of user costs in periods t and $t+1$, the degree of complementarity or substitutability captured by the elasticity of intratemporal substitution ε, and the desire to "pull forward" consumption from period $t+1$ to t, captured by the elasticity of intertemporal substitution σ:

$$\frac{k_{t+1}}{k_t} = [\beta(1+r)]^{\sigma} \left(\frac{1+\frac{b}{1-b} Q_{t+1}^{1-\varepsilon}}{1+\frac{b}{1-b} Q_t^{1-\varepsilon}} \right)^{\frac{\sigma-\varepsilon}{\varepsilon-1}} \left(\frac{Q_{t+1}}{Q_t} \right)^{-\varepsilon}, \tag{15}$$

where β is a discount factor that captures household impatience.

If $\sigma \to 0$, households do not purchase cars that they planned to buy later due to the scrappage program (and do not react to changes in the interest rate). With $\sigma \to 1$, substitution effects become large enough to cancel income effects, and with $\sigma \to \infty$, the desired car stock becomes extremely responsive to changes in relative prices. If cars are perfect complements to non-durables, an increase in the weighted user costs in period $t+1$ by one percentage point should decrease car consumption growth by σ%.[23]

Taking the logarithm of both sides of Eq. (15) gives:

$$\log(k_{t+1}/k_t) = \kappa + \frac{\sigma-\varepsilon}{\varepsilon-1} \log(F_{t+1}/F_t) - \varepsilon \log(Q_{t+1}/Q_t), \tag{16}$$

where $F_{t+1}/F_t = \left(\frac{1+\frac{b}{1-b} Q_{t+1}^{1-\varepsilon}}{1+\frac{b}{1-b} Q_t^{1-\varepsilon}} \right)$ and κ is the constant $\sigma \log[\beta(1+r)]$. The substitution effect of the reduced user costs leads to a higher desired car stock and greater gross investment in t and lower gross investment in $t+1$. The income effect may increase car expenditures in t as well but cannot be used for car expenditures in $t+1$ because the program required participants to purchase one by December 31, 2009.

Cashin and Unayama (2016) show that the growth rates of car expenditures depend on the relative magnitude of the elasticity of intratemporal substitution compared to the elasticity of intertemporal substitution as in Eq. (16). If the utilities derived from using the car and consuming the non-durables are additively separable, that is, if $\sigma = \varepsilon$, the response of car

[23]That is, $d\log(k_{t+1}/k_t) = -\sigma dlog(1+b/(1-b)Q_{t+1})$.

expenditures reflects the full impact of the temporary reduction of user costs. Households choose a higher level of car expenditures in period t compared to $t+1$. If $\sigma, \varepsilon > 0$, the partial derivatives k_{t+1}, $\frac{\partial k_t}{\partial Q_t} < 0, \frac{\partial k_{t+1}}{\partial Q_t} > 0$ show the familiar signs, regardless of whether $\sigma < \varepsilon$ or $\sigma > \varepsilon$. This implies our first hypothesis:

Hypothesis 1. *Demand for new cars increased in 2009.*

The second hypothesis relies on the Euler equation for the durable car stock as well. Changes in the user costs are predicted to be temporary, whereas the elasticity of intertemporal substitution alone shapes any permanent effects (e.g., "pull-forward" effects). If the demand for cars reverts completely after the end of the program, the intertemporal elasticity of substitution must be small and is bounded by $\sigma < 1$. Our results are consistent with Hall (1988), Ogaki and Reinhart (1998), Yogo (2004), Parker and Preston (2005), Cashin and Unayama (2016), and Baker et al. (2021), but smaller than estimates that do not rely on household data (e.g., Buettner and Madzharova, 2021).

Hypothesis 2. *Intertemporal substitution does not fully crowd out the fiscal stimulus. The increase in new-car purchases during 2009 was not fully offset by a reduction in new-car purchases over the post-program observation window.*

The cross-effects of the scrappage scheme on other goods are given by the Euler equation for non-durables.

$$\frac{x_{t+1}}{x_t} = [\beta(1+r)]^{\sigma} \left(\frac{1 + \frac{b}{1-b} Q_{t+1}^{1-\varepsilon}}{1 + \frac{b}{1-b} Q_t^{1-\varepsilon}} \right)^{\frac{\sigma-\varepsilon}{\varepsilon-1}}. \tag{17}$$

The growth rate of non-durables is determined very similarly to that of cars. With additively separable utility, $\varepsilon = \sigma$, non-durable consumption is unaffected by the scrappage scheme. The positive full-sample estimate rejects this prediction, although the corresponding buyer-sample estimate is small and statistically insignificant. Generally, the direction of the cross-effects depends on the magnitude of ε (Cashin and Unayama, 2016). If $\varepsilon < \sigma$, the reduction in user costs increases non-durable consumption and reinforces "pull-forward" effects. Non-durable expenditures will increase in t and decrease in $t + 1$. If $\varepsilon > \sigma$, a reduction of the user costs *reduces* non-durable expenditures in t and increases them in $t + 1$ as intratemporal substitution works against the "pull-forward" effects. Therefore, overall consumption would increase only if $\varepsilon < \sigma$, which we find in our main results. If $\varepsilon < \sigma$, the model predicts that the temporary reduction in user costs increases non-durable consumption during the program period. The positive full-sample estimate and the absence of a statistically significant negative effect in the buyer sample are consistent with this prediction. However, the precise value of ε, and therefore any numerical lower bound on σ, depends on the calibration of the user-cost change.

Cars may be viewed as luxury goods, whereas non-durables are necessities. The CES specification assumes that preferences are homothetic. Pakoš (2011) finds instead that preferences are nonhomothetic and that income effects are present. Such (transitory) income effects are consistent with our findings and thus our empirical specification attempts to identify the combined income and intertemporal substitution effects. In summary, they imply that overall consumption increases and is not crowded out by intratemporal substitution.

Hypothesis 3. *Intratemporal substitution does not fully crowd out the fiscal stimulus. Cars were not purchased instead of other goods, but in addition to them, such that overall consumption increased.*

C Household Optimization Problem

The present value of instantaneous utility over all periods is

$$\sum_{t=s}^{T} \beta^{t-s} \frac{\sigma}{\sigma - 1} u_t^{1-\frac{1}{\sigma}},$$

where β is the household discount factor capturing time preferences and σ is the intertemporal elasticity of substitution.

The Lagrangian for the intertemporal optimization problem is

$$L(x_t, k_t, a_t, \lambda_t) = \sum_{t=s}^{T} \left\{ \beta^{t-s} \frac{\sigma}{\sigma - 1} u_t^{1-\frac{1}{\sigma}} + \lambda_t \beta^{t-s} \right.$$
$$\left. \left[(1+r)a_{t-1} - a_t + w_t - x_t - p_t(k_t - (1-\delta)k_{t-1}) \right] \right\},$$

where λ_t is the Lagrange multiplier in current value terms.

The first-order condition for the consumption of non-durables is

$$\frac{\partial u_t}{\partial x_t} u_t^{-1/\sigma} = \lambda_t,$$

where λ_t represents the marginal utility of wealth in period t. The condition for the optimal stock of consumer durables is

$$\frac{\partial u_t}{\partial k_t} u_t^{-1/\sigma} = \lambda_t p_t - \lambda_{t+1} \beta (1-\delta) p_{t+1}.$$

The condition for optimal wealth is

$$\frac{\lambda_t}{\lambda_{t+1}} = \beta(1+r).$$

At the optimum, the budget constraint

$$a_{t-1}(1+r) + w_t - a_t = x_t + p_t(k_t - (1-\delta)k_{t-1})$$

is binding. It includes the stock of wealth at the end of period t, a_t, and saving or borrowing $a_t - a_{t-1}$. Total income includes interest income ra_{t-1} and labor income w_t. Labor and interest income, together with any reduction in net financial assets, finance current purchases of non-durable consumption goods x_t, and gross investment $k_t - (1-\delta)k_{t-1}$ in the durable stock. The effective consumer price of durables is denoted by p_t, including any applicable subsidy, while the price of non-durables is normalized to one. Thus, for a program participant in the policy period, p_t in this appendix corresponds to the gross price multiplied by $1 - \nu_t$ in Appendix B. Investments depreciate at rate δ.

Define the user costs Q_t as

$$Q_t \equiv p_t - \frac{1-\delta}{1+r} p_{t+1} = \left(\frac{\partial u_t}{\partial k_t} \bigg/ \frac{\partial u_t}{\partial x_t} \right).$$

Finally, under the assumption of CES instantaneous utility of the form

$$u_t = \left(b^{1/\varepsilon} k_t^{\frac{\varepsilon-1}{\varepsilon}} + (1-b)^{1/\varepsilon} x_t^{\frac{\varepsilon-1}{\varepsilon}} \right)^{\frac{\varepsilon}{\varepsilon-1}},$$

where $0 < b < 1$ weights cars relative to non-durable goods according to household tastes, we can solve the model for the equations in Appendix B.

D KBA Data

Table D.1: Descriptive Statistics of KBA Data

	All	Private	Commercial
Mean registrations per cell	70	58	83
Standard deviation	249	111	333
Median	21	24	19
90th percentile	149	127	172
Share of zero-count cells (%)	6	6	6
Total registrations	26,106,890	10,725,740	15,381,150
Number of observations	372,208	186,104	186,104

Source: Own calculations based on data on registrations of new cars in Germany (2005–2014) provided by the German Federal Motor Transport Authority (*Kraftfahrt-Bundesamt*). Notes: This table shows summary statistics of our sample from the KBA data. The data are aggregated to monthly region–manufacturer–owner cells.

E Robustness Check on Intertemporal Substitution

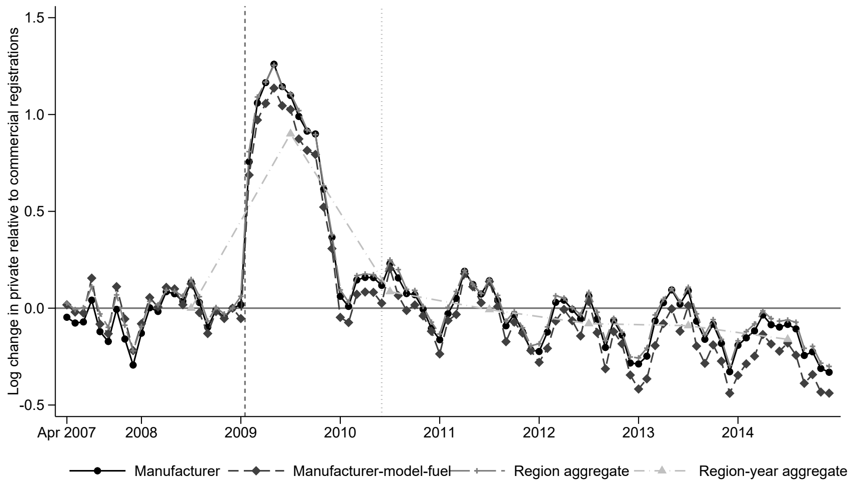

Figure E.1: Robustness to Product Matching, Geographic Aggregation, and Yearly Aggregation

Source: Own calculations based on data on registrations of new cars in Germany (2005–2014) provided by the German Federal Motor Transport Authority (*Kraftfahrt-Bundesamt*). Notes: This figure presents $\hat{\beta}_k$ based on Eqs. (1), (2), (3), and (4). The solid series matches by manufacturer. The dashed series matches by manufacturer, model, and fuel. The long-dashed series aggregates by region-year-month. The dash-dotted series aggregates by region-year. The first three series use two-digit postal regions and December 2008 as the omitted month. The region-year aggregated series uses 2008 as omitted year. The dashed vertical line marks February 2009 and the dotted line marks the June 2010 registration deadline. Confidence intervals are omitted for readability.

F SOEP Data

As our main data source to test Hypothesis 3, we rely on the German Socio-Economic Panel (SOEP, version v31), a representative survey of German households conducted annually (Wagner et al., 2007). Our sample consists of 12,790 households. The relevant wave provides detailed information on consumption expenditures for the year 2009, the year in which the Cash-for-Clunkers policy was implemented in Germany. It covers all major components of a typical household consumption basket. In addition, a unique feature of this wave is that it reports whether a household purchased a car in 2009 and—if so—whether the purchase was subsidized by the scrappage premium.

The data are aggregated at the household level because both income and expenditure information are reported for households rather than individuals. An important advantage of the SOEP is that it provides a rich set of additional household and individual characteristics, including household disposable income, wealth, savings, measures of risk and time preferences, and family status. For variables collected at the individual level (such as age or preferences), we use the characteristics of the economic household head, defined as the person with the highest individual income in the household.

Table F.1 provides an overview of the SOEP data. Panel A shows that about two thirds of households owned a car in 2008. One year later, in 2009, 11.3% purchased a car (new or used), and roughly one third of these purchases were subsidized through the Cash-for-Clunkers program. This corresponds to about 4% of all households in the SOEP sample. A comparison with official statistics from the Federal Office for Economic Affairs and Export Control and the German Federal Motor Transport Authority highlights the representativeness of the SOEP: 1,933,090 households, corresponding to 4.8% of all German households, received the subsidy.

Panel B of Table F.1 presents data on consumption expenditures. Although the reliability of survey-based consumption data is sometimes questioned, this does not appear to be a concern for the SOEP. Marcus et al. (2013) and Noll and Weick (2015) compare SOEP consumption data with the Income and Consumption Survey (“Einkommens- und

Table F.1: Descriptive Statistics of SOEP Data

	Unit	# Households	Mean	Median	SD
Panel A: Car variables					
Car purch. '09	D[0/1]	12,790	0.113	0.00	0.32
CfC	D[0/1]	12,790	0.040	0.00	0.20
HH has car in '08	D[0/1]	12,790	0.675	1.00	0.47
Car purch. last 12 months	D[0/1]	12,790	0.069	0.00	0.25
Panel B: Main consumption categories					
Total	Euro	12,790	11,114	8,400	10,575
Non-durables (NDCG)	Euro	12,790	5,437	4,800	3,680
Services (SER)	Euro	12,790	3,648	2,400	5,590
Durables w/o Cars (DCG)	Euro	12,790	631	0	2,291
Durables w/o Cars > 0 (DCG)	Euro	6,091	1,578	800	3,411
Cars (CAR)	Euro	12,790	1,398	0	5,084
Cars > 0 (CAR)	Euro	1,624	12,351	11,000	9,645
Panel C: Monetary control variables					
HH income '08 ($\times 10^3$)	Euro	7,707	30.363	25.14	24.76
No savings '08	D[0/1]	7,704	0.394	0.00	0.49
Net wealth '07 ($\times 10^5$)	Euro	7,533	1.639	0.51	4.50
Panel D: Household characteristics					
Patience low	D[0/1]	7,899	0.219	0.00	0.41
Patience medium	D[0/1]	7,899	0.281	0.00	0.45
Patience high	D[0/1]	7,899	0.500	0.00	0.50
Risk aversion low	D[0/1]	8,801	0.397	0.00	0.49
Risk aversion medium	D[0/1]	8,801	0.278	0.00	0.45
Risk aversion high	D[0/1]	8,801	0.325	0.00	0.47
Age HH-head	Years	12,790	53.413	53.00	17.28
Couple, no children	D[0/1]	12,790	0.294	0.00	0.46
Couple, with children	D[0/1]	12,790	0.226	0.00	0.42
Single, no children	D[0/1]	12,790	0.407	0.00	0.49
Single, with children	D[0/1]	12,790	0.063	0.00	0.24

Source: Own calculations based on the SOEP (2007–2010). Notes: This table shows summary statistics of our sample from the SOEP. All statistics are calculated using SOEP sampling weights. Panel A: variables measuring car purchase behavior and car endowment at the household level. Panel B: aggregate household expenditure variables. Panel C: monetary household information. Panel D: additional household characteristics.

Verbrauchsstichprobe," EVS) and find close agreement between the two sources.[24] Panel B shows that households purchasing a car in 2009 spent on average about €12,000, roughly the price of a new subcompact or city car in Germany. Car expenditures therefore represent a particularly important component of household consumption, as total annual consumption averages around €11,000. By comparison, expenditures on other durable goods average about €630, while non-durable goods and services amount to about €5,400 and €3,600, respectively. We classify goods according to the "Personal Consumption Expenditures" framework of the U.S. Department of Commerce. Table G.1 in the appendix shows how SOEP consumption categories are grouped into non-durable consumption goods (NDCG), durable consumption goods (DCG), and services (SER). Because our main interest lies in the effect of the policy on car purchases, we treat car expenditures separately and follow the standard convention of defining durable goods excluding cars. The standard deviation of car expenditures is much larger than that for other consumption categories, reflecting the wide range of vehicle prices. For example, a new compact car costs about €20,000.

Panel C summarizes information on disposable household income in 2008. Average annual net income amounts to roughly €30,000. In contrast, the distribution of net equivalence wealth[25]—available for 2007—is highly skewed: the median is about €50,000, while the mean reaches €160,000. One reason for this pronounced skewness is that almost 40% of households report no monthly savings.

Finally, Panel D of Table F.1 reports household characteristics. A key advantage of the SOEP is the availability of experimentally validated measures of risk aversion and patience. Both preferences are measured on an eleven-point Likert scale. We group observations into terciles of the respective distributions to obtain indicators for low, medium, and high levels of patience and risk aversion. The average age of household heads in the sample is 53 years. We distinguish four household types: couples with children (23%), couples without children (29%), single parents (6%), and singles without children (41%).

[24]The EVS is conducted every five years, and participating households record consumption in detailed household accounts.

[25]Net equivalence wealth is calculated using the OECD equivalence scale.

G Classification of Consumption Goods and Services

Table G.1: Consumption Categories

Goods	Category
Groceries Clothes/Shoes Personal Hygiene	Non-Durable Consumption Goods (NDCG)
Motorcycle Microwave, Dishwasher, Freezer Washing Machine, Tumble Dryer Stereo TV, DVD Player, DVD Recorder PC/Laptop Furniture	Durable Consumption Goods (DCG)
Dining/Drinking Outside Culture Leisure Holiday Public Transportation	Services (SER)
Used Cars New Cars	Cars (CAR)

Source: Own presentation. Notes: Categorization based on "Personal Consumption Expenditures" (PCE): Bureau of Economic Analysis (U.S. Department of Commerce).

H Predicted Probability of Cash-for-Clunkers Take-Up

Table H.1: Probit Estimation Results of Cash-for-Clunkers Take-Up

	No controls	+ Monetary control	+ Household characteristics
HH has car in '08	0.143***	0.383***	0.399***
	(0.024)	(0.110)	(0.122)
Car purch. last 12 months	−0.283***	−0.287***	−0.275***
	(0.064)	(0.070)	(0.070)
HH income '08 ($\times 10^3$)		−0.002***	−0.003***
		(0.001)	(0.001)
No savings '08		−0.032	−0.040
		(0.036)	(0.037)
Net wealth '07 ($\times 10^5$)		0.001	0.000
		(0.004)	(0.004)
Patience low			−0.005
			(0.046)
Patience high			−0.001
			(0.040)
Risk aversion low			−0.025
			(0.040)
Risk aversion high			−0.025
			(0.045)
Age HH-head			0.003*
			(0.001)
Couple, with children			0.056
			(0.042)
Single, no children			−0.043
			(0.054)
Single, with children			−0.050
			(0.080)
# Households	1,624	907	892

Source: Own calculations based on the SOEP (2007–2010). Notes: This table shows marginal effects at the means of the following probit estimation with varying sets of control variables $P(CfC_i = 1|X_i) = \Phi\left(\gamma_0 + \gamma_1 Car08_i + \gamma_2 Car08\_12m_i + \gamma_3 \boldsymbol{X}_i\right)$, where Φ denotes the cumulative distribution function of the standard normal distribution, $Car08_i$ is a dummy equal to 1 if the household owned a car in 2008, and $Car08\_12m_i$ is a dummy equal to 1 if the household stated in 2008 to have bought a car within the last 12 months. These two variables capture proxy-eligibility of the Cash-for-Clunkers program. Sample: households that bought a car in 2009. Households that serve as the base category are couples without children. Standard errors in parentheses. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

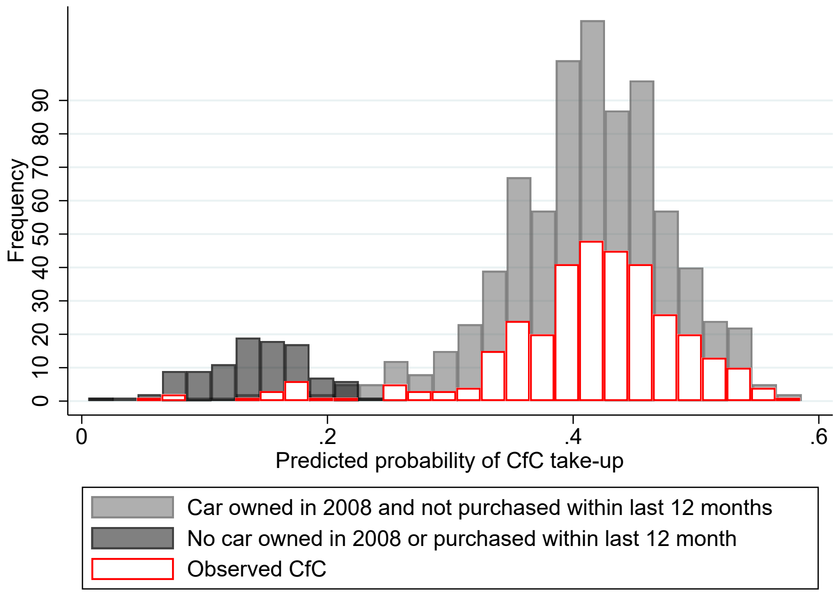

Figure H.1: Distributions of Predicted Take-up Probabilities

Source: Own calculations based on the SOEP (2009). Notes: This figure shows the distribution of the propensity to participate in the Cash-for-Clunkers policy (see Table H.1 for the estimation details). Histograms are shown for three subgroups: (i) households that fulfill the eligibility criteria based on our proxy variables (light-gray), (ii) households that do not fulfill the eligibility criteria based on our proxy variables (dark-gray), and (iii) households that participate in the Cash-for-Clunkers policy (red).

I Selection Bounds, Functional Form, and Proxy Robustness

Table I.1: Effect of Cash-for-Clunkers on Consumption: PPML Estimates

	CAR	DCG	NDCG	SER	Total
Panel A: Full sample					
CfC eligibility proxy	0.823***	0.082	0.084***	0.191***	0.194***
	(0.159)	(0.142)	(0.015)	(0.036)	(0.024)
Controls	✓	✓	✓	✓	✓
# Households	7,322	7,322	7,322	7,322	7,322
Panel B: Car buyers					
CfC eligibility proxy	0.100	−0.256	0.038	0.025	0.055
	(0.107)	(0.239)	(0.046)	(0.125)	(0.068)
Controls	✓	✓	✓	✓	✓
# Households	892	892	892	892	892

Source: Own calculations based on the SOEP (2007–2010). Notes: This table re-estimates the effect of Cash-for-Clunkers proxy-eligibility on expenditures by Poisson pseudo-maximum likelihood (PPML). Panel A uses the full sample; Panel B uses the sample of households purchasing a car in 2009. Robust standard errors in parentheses. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table I.2: Lee Bounds on the Proxy-Eligibility Effect among Car Buyers

	CAR	Total	NDCG
Lower bound	0.181**	−0.004	−0.185***
	(0.075)	(0.042)	(0.040)
Upper bound	0.886***	0.400***	0.218***
	(0.065)	(0.039)	(0.039)
95% CI for effect	[0.057; 0.992]	[−0.073; 0.464]	[−0.250; 0.282]
Trimming proportion	0.215	0.215	0.215
# Households	12,790	12,790	12,790
# Selected (buyers)	1,624	1,624	1,624

Source: Own calculations based on the SOEP (2007–2010). Notes: This table reports Lee (2009) bounds on the effect of Cash-for-Clunkers proxy-eligibility on log expenditures (plus €1) among households purchasing a car in 2009, treating the 2009 car purchase as the selection margin. The proxy-eligible-buyer outcome distribution is trimmed by the differential selection rate. The 95% confidence intervals for the treatment effect account for the partial identification of the bounds. Standard errors in parentheses. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table I.3: Proxy Quality and Control-Group Composition

	Excl. recent buyers	Excl. no-car controls	Control-subtype dummy	Strict age proxy
CAR	0.484**	0.227*	0.471**	0.356***
	(0.197)	(0.133)	(0.197)	(0.133)
Total	0.301***	0.092	0.296***	0.184***
	(0.105)	(0.072)	(0.105)	(0.067)
NDCG	0.141	0.002	0.137	0.057
	(0.098)	(0.045)	(0.098)	(0.052)
# Households	819	864	892	422

Source: Own calculations based on the SOEP (2007–2010). Notes: This table re-estimates Table 5 on the buyer sample under alternative treatment and control definitions. Column (1) excludes control households that purchased a car within the 12 months before the reform; Column (2) excludes control households without a car in 2008; Column (3) includes both control subtypes and adds a recent-buyer indicator (its coefficient in the CAR regression is 0.243, standard error 0.228); Column (4) replaces the baseline proxy with the strict proxy that additionally requires the vehicle to be verifiably at least nine years old in 2009. All models use the full set of control variables. Robust standard errors in parentheses. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

J Calibrated Intratemporal Elasticity of Substitution

Table J.1: Calibrated Intratemporal Elasticity of Substitution

ν	δ	r	$\Delta \log Q$	ε (unscaled)	ε (rescaled)
0.15	0.15	0.03	−1.954	0.130	0.516
0.15	0.20	0.00	−1.386	0.184	0.727
0.15	0.20	0.03	−1.114	0.229	0.905
0.15	0.30	0.00	−0.693	0.368	1.454
0.15	0.30	0.03	−0.631	0.404	1.596
0.15	0.50	0.00	−0.357	0.715	2.825
0.15	0.50	0.03	−0.345	0.740	2.924
0.15	1.00	0.00	−0.163	1.569	6.200
0.20	0.20	0.03	−2.260	0.113	0.446
0.20	0.30	0.00	−1.099	0.232	0.917
0.20	0.30	0.03	−0.979	0.260	1.030
0.20	0.50	0.00	−0.511	0.499	1.973
0.20	0.50	0.03	−0.492	0.518	2.048
0.20	1.00	0.00	−0.223	1.142	4.516
0.25	0.30	0.00	−1.792	0.142	0.562
0.25	0.30	0.03	−1.516	0.168	0.665
0.25	0.50	0.00	−0.693	0.368	1.454
0.25	0.50	0.03	−0.665	0.383	1.515
0.25	1.00	0.00	−0.288	0.886	3.503

Source: Own calculations based on the SOEP (2007–2010). Notes: This table reports the intratemporal elasticity of substitution $\varepsilon = -(\Delta \log k - \Delta \log x)/\Delta \log Q$ implied by the buyer-sample ITT difference of 0.255 log points (Table 5) over a grid of calibrations of the user-cost change, $\Delta \log Q = \log\big[(1-\nu) - \frac{1-\delta}{1+r}\big] - \log\big[1 - \frac{1-\delta}{1+r}\big]$, where ν is the rebate's share of the purchase price, δ the annual depreciation rate, and r the interest rate. The final column rescales the numerator by the first-stage take-up differential conditional on controls (0.253). Combinations with lower depreciation rates than shown imply non-positive post-rebate user costs, for which the elasticity is not identified from this variation and which are therefore omitted. At $\delta = 1$ the entries for $r = 0$ and $r = 0.03$ coincide.