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Evaluating the Causal Effects of Cash-for-Clunkers Programs in Selected Countries: Success or Failure?

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Abstract

This paper provides empirical evidence that Accelerated Vehicle Programs exhibit a positive influence on car registrations using unique aggregate monthly data for 23 OECD countries from 2000 to 2010. The effect is still traceable if dynamic panel data fixed effects methods are used to address the problem of unobserved heterogeneity and controlling for macroeconomic variables like industrial production, interest rate, unemployment rate and gasoline price. Furthermore our analysis reveals that passenger car sales varied considerably before the car scrappage scheme was put in place to fight the 2009 sales crisis. Compared to a simulated counterfactual situation we find a positive overall effect (until autumn 2010) of the recent Accelerated Vehicle Programs for chosen countries: the United States, South Korea, Germany and the United Kingdom. Simulation results further show that timing and duration of the policies seem much more important for its success than the budget allocated to the program.

JEL-classification: C33, C53, H25, L62

Keywords: Policy evaluation, Subsidy, Automotive industry, Simulation, Treatment effect

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1 Introduction

From September 2008 to January 2009, during the financial crisis, the automotive industry in OECD countries faced an average downturn of 20 percent in aggregate passenger car sales; the biggest worldwide cuts ever observed in this sector.¹ As the automotive industry is a key driver in major economies, with value added up to four percent of total output,² various OECD governments enacted indirect and direct market support measures to outbalance these unwanted effects. Triggered by Germany's Accelerated Vehicle Retirement program, similar schemes were enacted in over 25 countries worldwide between December 2008 and January 2011, as part of the corresponding national economic stimuli-programs.

Accelerated Vehicle Retirement (AVR) programs, Cash-for-Clunkers, car scrapping subsidies, or car scrappage schemes, all these wordings refer to the same phenomenon, namely vehicle owners receiving government subsidies for trading in an old vehicle for a new, more efficient car. This subsidy is not necessarily monetary, tax reductions on newly registered cars and public transport passes are also common features of these programs.³ **Table 1** gives an overview of AVR programs in selected countries, enacted between 2008 and 2011. The programs differ considerably in budget volume, individual bounty, timing and duration and car age preconditions.

The main governmental goal was shifting household expenditures from future periods to the present. The general argumentation in favor of car scrapping policies is the automotive industry being a key driver of the economy and therefore pushing car sales up is the same (from Governments' point of view) as promoting industry sales and thus increasing overall welfare. Antagonists argue that Cash-for-Clunker programs are not more than an expensive subsidy of the automobile sector without positive welfare effects in the long-run.

If the different car scrapping schemes worldwide were successful in the short and long run is an open research question and our paper tries to fill this gap. Empirical studies already conducted focus on single country evaluations of the car scrapping programs in various countries. The results are of different magnitude but in general no long-lasting effects of the AVR-programs can be found in terms of output and employment.⁴ The environmental impact is however positive, even if the cost-effectiveness of the car scrappage programs

¹See Haugh et al. (2010), Figure 5, p. 12.

²See Haugh et al. (2010), Figure 2, p. 9.

³See IHS Global Insight (2010) for a survey of the car scrapping programs in various countries.

⁴See Adda and Cooper (2000), Mian and Sufi (2012) and Li et al. (2012).

already examined is low.⁵ The reason being, that some car scrapping policies set the stage for pull-forward purchases of consumers, or backwards as a result of anticipatory effects, which is followed by a rapid decline after the end of the subsidy period.

This paper addresses two research questions:

(1) Did the car scrapping policy have an effect on total car sales during the subsidy period?

(2) What is the overall effect of the different policies considering even the periods following the subsidy?

In order to answer these questions we create a unique panel dataset with country level data for 23 OECD-countries. We first apply dynamic fixed effects estimation techniques to control for unobserved heterogeneity between countries. In line with previous studies we find a positive sales effect of the scrapping variable in our research setting, indicating an immediate boost in sales due to governmental car scrapping policies.

In a second part we calculate the effects of the car scrapping schemes on sales to approximate possible welfare effects for Germany, the USA, the United Kingdom and South Korea by simulating a counterfactual situation treating the introduction of the car scrapping scheme as a structural break. First of all, we estimate the model for the time period before the subsidy separately. Secondly, the counterfactual sales number is predicted using the fitted values of the before-subsidy period. Thirdly, we define the difference in sales between these two numbers as the effect of the Cash-for-Clunkers subsidy. Finally we conduct the same analysis for the period after the scrapping scheme and analyze whether the positive boost in sales during the subsidy period outweighs the decline thereafter. The results of this section indicate a positive sales effect over the entire period for the four countries of interest.

The remainder of our paper is organized as follows. The next section discusses the related literature, section three presents our dataset, the empirical strategy and regression results, section four depicts simulation results and section five concludes.

2 Literature Review

As stated in the introduction, the rationale for governments' decisions to implement car scrapping schemes changed considerably over time. This pattern can also be found in the literature.

In the 1990s economists were concerned with the optimal design of the

⁵See Knittel (2009), Hahn (1995) and Dill (2004).

Table 1: Overview of Characteristics of Scrappage Programs 2008-2011

Country	Bounty (in Euro)	Budget (in M Euro)	Environmental component	Scrapped Cars (absolute)	Timing	Duration (months)	Age Pre- condition
Australia	1,400	394	yes	200,000 *	Starting Jan 11	48	pre '95
Austria	1,500	45	no	30,000	Apr 09-Jul 09	4	pre '96
Canada	222	67	yes	101,000	Feb 09-Mar 11	26	pre '96
Cyprus	1,283-1,710	9	yes	10,039	Jan 09-Sept 09	9	15 years
France	700-1,000	600	yes	600,000	Dec 08-Dec 09	13	10 years
Germany	2,500	5,000	no	1,933,090	Jan 09-Sep 09	9	10 years
Greece	500-2,200	400	yes	137,920	Sept 09-Nov 09	2	unknown
Ireland	1,500	unknown	yes	unknown	Jan 10-Jun 11	18	10 years
Italy	1,500	2,000	yes	472,000	Feb 09-Dec 09	11	pre '00
Japan	2,222	3,289	yes	417,000 *	Apr 09-Sept 10	18	13 years
Luxembourg	1,500-1,750	10	yes	unknown	Jan 09-Jul 10	19	10 years
Netherlands	750-1,000	125	yes	51,700	May 09-Dec 10	21	unknown
Romania	900	30	no	32,327	Mar 09-Dec 09	10	20 years
Slovakia	1,500-2,000	55	no	44,200	Mar 09-Dec 09	10	pre '99
South Korea	1,551	309	no	890,000	May 09-Dec 09	8	pre 99
Spain	500-2,000	1,680	yes	190,000	Sept 08-Jul 10	23	10/12 years
UK	2,315	463	no	372,401	May 09-Mar 10	10	8/10 years
USA	2,557-3,288	2,084	yes	401,274	Jul 09-Aug 09	2	<25 years

Source: IHS Global Insight (2010); Haugh et al. (2010) and own research; Note: * estimated values

AVR programs. Studies in this line of research are Hahn (1995), Alberini et al. (1995) and Kavalec and Setiawan (1997). Alberini et al. (1995) derive a theoretical model of the reservation price (willingness to accept) as a function of various determinants. Their results for the 1992 Delaware scrappage program suggest that the selection problem for low scrappage incentives offered is quite substantial. For bounties below \$500 car owners scrap vehicles in the poorest condition with relatively short remaining life-time only and it is very difficult for the policymaker to fulfill the environmental targets of the car scrapping policy. Hahn (1995) constructs a car scrappage supply curve and conducts a cost benefit analysis of the first Cash-for-Clunkers program in California in 1992. The author's findings suggest that cost-effectiveness of an AVR program can most easily be met by accompanying the scrapping policy with an inspection and maintenance program and that the optimal scrapping incentive is \$1,500 for the examined policy. The work also shows that a car scrapping policy is only optimal for a transitional time period. Kavalec and Setiawan (1997) use simulation techniques to evaluate the scrappage programs in Los Angeles for 1999 until 2010. They found that a program design targeting 20 years and older vehicles is more cost effective in terms of emission reduction than a design in order to scrap 10 years and older cars. In addition the deteriorating effect on used car prices is less under the former design.

Alongside the literature on the optimal design of scrappage programs, authors were concerned with the effectiveness of the AVR programs in the 1990s in terms of reducing air-pollution (Alberini et al. (1996); Baltas and Xepapadeas (1999); Van Wee et al. (2000); Dill (2004)). All these studies find a positive effect of car scrapping policies on emission reduction. Alberini et al. (1996) additionally point out that car scrappage programs that explicitly target high pollutant cars are more cost-effective than those accepting any old vehicle. Baltas and Xepapadeas (1999) find evidence for Greece that highlight the effectiveness of the Greek scrappage program from 1991 to 1993. This program worked through tax reductions, in terms of reducing hydrocarbon and nitrogen oxides. In line with Hahn (1995), Van Wee et al. (2000) point out, that from an environmental point of view putting restrictions (like a catalytic converter) on operating vehicles is more cost-effective than introducing a scrappage incentive, using a dataset from the Netherlands. Dill (2004) uses survey data from two US AVR programs on the local level. She reports emission reduction numbers for different estimation techniques and finds less impact on emission reduction than the studies mentioned above. Miravete and Moral (2009) extend this literature with their work on the Spanish scrappage program in 1994. They find a long-run qualitative effect on the composition of the Spanish car-fleet from gasoline to more fuel-efficient diesel engines.

The first study conducting a policy evaluation by taking the counterfactual situation into account is the work of Adda and Cooper (2000). They use a dynamic microlevel discrete choice model to examine the short and long-run effects of two car scrappage subsidies in France 1992 and during 1994 to 1995. They calculate effects on output and public budget of these policies and find no positive output effect of car scrapping policies in the long-run. The reason for this is the side-effect, that car scrappage programs change the cross-sectional distribution of car ages that shows its effect some time after the end of the subsidy period.

A recent part of the literature is conducting evaluations of the 2009 CARS-/Cash-for Clunkers program in the United States. These studies are concerned with quantitative effects of the AVR policies in terms of output, employment and environmental aspects. Mian and Sufi (2012) use American cross-city variation in exposure to the Cash-for-Clunkers program. Their results suggest complete inter-temporal substitution and therefore the effect of the subsidy was completely reversed after seven months. Furthermore they were unable to detect a positive employment effect for cities more profiting from the Cash-for-Clunkers program, compared to less exposed cities. Li et al. (2012) examine the 2009 American CARS Program using a difference-in-difference approach. In their set-up Canada is serving as a control group for the estimations. Their results suggest a positive boost in sales during the subsidy period and creation of jobs in the automotive industry could also be found. However the environmental aspect as part of the target of the program came at high costs. One ton of CO_2 reduction costs \$91 of government revenue. This finding is far less than the \$450 per ton CO_2 found by Knittel (2009). However the latter is not concerned with the counterfactual situation.

The summary of the related literature shows, that to best of our knowledge there is no analysis conducting a car scrapping evaluation for various countries. The contributions so far focus on single-country case studies of one car scrapping policy. The aim of our paper is to close this gap in the literature using a multi-country panel analysis. The advantage of our research setting is that we are able to control for unobserved heterogeneity between the different countries of interest and therefore extracting the effects of car scrapping policies on aggregate car sales. Furthermore we add to the literature by simulating a counterfactual situation treating the introduction of the car scrapping scheme as a structural break, hence we are able to quantify effects of the policy for different countries.

The next section of the paper presents our empirical set-up and estimation results.

3 Empirical strategy

3.1 Data and summary statistics

Our panel-dataset consists of information of 23 OECD-countries collected on a monthly basis from January 2000 to December 2010. Information on variables and data sources are presented in **Table 2**. It comprises data for new passenger vehicle registrations in absolute values l_pc_reg and in index format l_pc_oecd respectively. As the data provided by the OECD is available for more countries in index format, we favor this data for our regressions. The Accelerated Vehicle Retirement participation of each country is modeled as the *clunker* variable, taking on one if such a scheme was at place during the observation period and zero otherwise.⁶

Furthermore we create variables for various determinants of the AVR programs. *Bounty* refers to the individual incentive paid for old vehicles if scrapped, *budget* summarizes information on the overall government’s budget dedicated to the car scrapping policy and the dummy variable *environment* refers to the fact that under some scrappage programs the new car purchase has to meet environmental requirements. For example under the American CARS-program the new car purchased had to be more fuel efficient than the retired vehicle.⁷ Furthermore we incorporate additional control variables, such as the industrial production index l_ip_OECD (as the monthly proxy for GDP), the harmonized unemployment rate UR and the three-months short term *interest_rate* controlling for the overall economic situation and financing requirements. These variables are available on a monthly basis from the OECD Main Economic Indicators database. Apart from that, we model the influence of gasoline prices as a three-months moving average $ma_p_gasoline$. We assume that the car purchasing decision on an aggregate level is taken in a rational manner and therefore depends on last month’s gasoline price, actual and next month’s fu-

⁶For some countries the beginning and end of the subsidy period was not clear cut, because of delivery delays in car production and the possibility of handing in application forms even after the budget was exhausted. In order to get comparable results we set the beginning and end of the subsidy period according to the budget and not to the actual delivery of the new cars purchased. For Germany the reservation for the subsidy was possible from January the 27th 2009 until September the 2nd 2009, as the budget of 5 billion Euros was exhausted. Until July the 31st 2010 it was still possible to hand in papers for example from the scrapping process (see BAFA (2010) for a detailed description of the administration process during the scrappage period). In our dataset we set *clunker* to one from January 2009 until September 2009.

⁷See Clowers (2010), Council of Economic Advisors (2009), Cooper et al. (2010) and Yacobucci and Canis (2010) for a detailed description of the CARS program.

Table 2: Variable Description and Data Sources

Variable	Description	Data Sources
l_pc_oecd	Index of passenger car registrations, 2005 =100, s.a., in natural logs	OECD (2011) Main Economic Indicators
pc_reg	Passenger car registrations in absolute values, seasonally adjusted with dummies	ACEA (2011) and national statistical offices
ma_p_gasoline	Price of gasoline in Euro per liter, 3-months moving average process with equal weights, seasonally adjusted with dummies	European Commission (2011) and national energy agencies
clunker	Dummy variable, taking on the value of 1 if AVR- program is at place or zero otherwise	Haugh et al. (2010), IHS Global Insight (2010) and own research
bonus	Individual incentive in Euro. If the bonus is a range the maximum available bounty is used	Haugh et al. (2010), IHS Global Insight (2010) and own research
environment	Dummy variable, taking on the value of 1 if AVR contains an environmental precondition of a new car or zero otherwise	Haugh et al. (2010), IHS Global Insight (2010) and own research
budget	Government budget dedicated to the national AVR program in million Euro per month under the assumption that the volume is constant between the subsidy months	Haugh et al. (2010), IHS Global Insight (2010) and own research
lip_OECD	Industry production index, 2005=100, s.a., in natural logs	OECD (2011) Main Economic Indicators
UR	Harmonized unemployment rate in percent, s.a.	OECD (2011) Main Economic Indicators
interest_rate	Three months short term interest rates in percent, s.a.	OECD (2011) Main Economic Indicators

Notes: s.a. = already seasonally adjusted by datasource

ture price, as a measure of gasoline price expectations.⁸ The observed time trend is incorporated in a linear fashion. In a different specification we use monthly dummy variables as a robustness check. All variables in absolute values are measured in Euros to current exchange rates.

With regard to the importance of time-series properties of the variables, as the underlying dataset has quite a long time dimension ($T=132$), we perform panel Unit-Root tests in order to avoid “spurious regressions”. As the dataset is unbalanced and the Unit-Root is probably of heterogenous nature for the different countries, the Im-Pesaran-Shin-test tracing back to Im et al. (2003) seems to be the natural choice. In the underlying dataset the null hypotheses that all panels contain a Unit Root can be rejected for the dependent variable *lpc_oecd* at the one percent significance level. This result is sufficient to show that the spurious regression concern is not an issue in this paper. For the sake of completeness test statistics of the Unit-Root tests for all the independent variables are summarized in **Table A.1** in the appendix.

Table 3 gives an overview of summary statistics and section 3.2 specifies the empirical estimation strategy.

Table 3: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
<i>lpc_reg</i>	2,892	10.6	1.45	5.24	13.46
<i>lpc_oecd</i>	2,963	4.6	0.20	3.31	5.47
<i>clunker</i>	3,036	0.1	0.36	0	1
<i>bounty</i>	395	985.8	739.01	222	3288
<i>environment</i>	395	0.6	0.48	0	1
<i>budget</i>	209	78.4	153.95	0.53	1042
<i>ma_p_gasoline</i>	2,857	1.2	0.39	0.31	3.12
<i>lip_OECD</i>	3,032	4.6	0.11	4.18	5.05
<i>UR</i>	3,036	7.2	3.67	1.8	20.6
<i>interest_rate</i>	2,973	3.4	2.21	0.07	19.82
<i>time_trend</i>	3,036	66.5	38.11	1	132
<i>country_code</i>	3,036	12	6.63	1	23

⁸See Hicks (2009) for a similar modeling approach. This work is not described in detail in section two, as it is a really short and rather incomplete essay.

3.2 Model

A standard approach in the literature would suggest to evaluate the AVR programs with the help of Differences-in-Differences estimation (DiD) techniques.⁹ In our dataset it is almost impossible to apply this approach for various reasons. First, a crucial point for applying DiD would be to find appropriate control groups. A perfect control group would require that in absence of the scrappage policy the sales pattern in two countries with and without AVR would exactly evolve in the same way. This assumption is not fulfilled in our dataset, as can be verified by visual inspection in **Figure A.1** in the appendix. Aggregate automotive sales seem to be very different even before the scrappage programs were implemented. Second, our dataset has a long time dimension of 132 periods and a comparably small number of countries, DiD can lead to biased standard errors, as serial correlation plays a more important role. This issue has been explored in detail in the work of Bertrand et al. (2004) and is of importance here. The problem of potential autocorrelation is addressed in section 3.4. In order to evaluate the direct effect of the AVR programs during the subsidy period we therefore estimate a panel data model for all countries for 132 different time periods. As we expect unobserved heterogeneity between the 23 countries being an important concern in the car market demand model, we use fixed-effects within estimation techniques. One can think of transportation infrastructure (number of highways, possibilities of substitution between different options, other means of transportation...) and time-invariant car market preconditions as examples. Such factors are not explicitly included in the regression, but affect demand for passenger cars in a given country. Furthermore, monthly car sales are highly path dependent and thus a dynamic specification of the car demand model is called for; examples of similar modelling approaches are Ryan et al. (2009) as well as Ramey and Vine (1996). We therefore specify the fixed-effects model with a lagged dependent variable among the explanatory variables. In order to avoid endogeneity problems, we estimate the following regression model with instrumental variables:

$$\begin{aligned} \log(pc_oecd_{i,t}) = & c_i + \beta_1 \log(pc_oecd_{i,t-1}) + \beta_2 clunker_{i,t} \\ & + \beta_3 time_t + \sum \beta_k (X_{i,t,k}) + \varepsilon_{i,t} \end{aligned} \tag{1}$$

$\log(pc_oecd_{i,t})$ represents the number of new passenger cars sold in natural logarithms, our measure of car demand in country i at time t . c_i incorporates time-invariant country fixed effects. $\log(pc_oecd_{i,t-1})$ characterises the monthly car sales in a given country, lagged by one month. The AVR is repre-

⁹See for example Angrist and Pischke (2009), 227-233 for a presentation of this approach.

sented through the binary variable $clunker_{it}$ set to one if a scrapping program was at place in country i at time t and zero otherwise. Furthermore, we include a linear time trend $time$ in the model. The vector $X_{it,k}$ includes the k control variables as described in the previous section. These are the industry production index in natural logs, the gross gasoline prices as three months moving average, the unemployment rate and short term interest rate, entering the equation without logarithms as the values for some countries are close to zero and would cause the dependent variable to reach infinity. β 's denote the parameters to be estimated and ε_{it} is the error term assuming standard properties.

Equation (1) is estimated using fixed effects within-transformation in order to get rid of the unobserved heterogeneity component c_i . It eliminates c_i by subtracting each variable's mean value. The model crucially relies on the fact that strict exogeneity holds, i.e. no explanatory variable is correlated with the error time at any point in time. Apart from that, the lagged dependent variable is clearly endogenous, therefore we instrument it with its own further lags. First-stage regression and overidentifying restriction tests are given in the results section in the next part as evidence for the validity of our instruments.

We expect positive signs for the scrapping incentive as boosting sales during the subsidy period is the main objective of the AVR-programs and is verified by the car scrapping evaluation literature, see Mian and Sufi (2012); Miravete and Moral (2009); Dill (2004). As stated in Goodwin et al. (2004) fuel price elasticities are negative. However we estimate the model on an aggregate demand level and therefore the influence is not that clear cut. An increase in the gasoline price over three months could also lead to more car purchases as the need for a more fuel-efficient new car is more striking. The industry production variable is supposed to have a positive vehicle sales effect, as car demand is a normal good and highly correlated with the business cycle, see Ramey and Vine (1996). The unemployment rate is presumed to enter the regression model with negative coefficients, as the household income is crucially dependent on the labor market participation. The short-term interest rate measures the financing conditions for a car purchase on credit, therefore we expect a negative sign for the corresponding coefficient. The dynamic component of the car demand specification is expected to exhibit a quite large positive influence on the car demand today (see again Ryan et al. (2009); Ramey and Vine (1996) as reference).

3.3 Results

The main empirical results are shown in **Table 4**. The F-test for unobserved heterogeneity after the fixed effects estimation suggests existence of unobserved heterogeneity. The coefficients of the country dummy variables are not equal to zero at the one percent significance level, thus there is a highly significant influence of unobserved country characteristics on car registration figures. In order to control for this problem, we estimate the model with fixed effects. All estimations are conducted using robust standard errors, because heteroscedasticity is a serious concern in this dataset.¹⁰ The regressions in **Table 4** present the second stage Two Stage Least Squares (TSLS) results grouped by the instruments used for the lagged dependent variable. Regressions (1) to (3) uses two further lags and (4) to (5) three lags as instrumental variables. First stage regression results given in **Table 5** indicate relevance of the instruments as they exhibit a positive and significant influence on lagged car sales at the one percent significance level. Furthermore, the critical values Stock and Yogo (2005) derived in their seminal study suggest that the loss in efficiency of the instrumental variable regression compared to ordinary least squares is less than 10 percent. Hence a problem of weak instruments can be neglected.¹¹ Moreover, overidentifying restrictions can be tested through Hansens's J-test. Over all specifications (1) to (5) the null hypothesis of the instruments being valid cannot be rejected at the one percent significance level. Therefore we argue that the instruments used are exogenous, i.e. uncorrelated with the error term.

In the following discussion of the obtained results we refer to column (1) of **Table 4** apart from otherwise stated.

First of all, our results indicate that the dynamic set-up is an important issue in automobile sales models. The lagged dependent variable of automobile sales is significantly positive over all instrumental variable specifications used and with a magnitude of 0.88 in line with the one found in the univariate study of Ramey and Vine (1996). Our results clearly underline the persistence of car sales over time.

Second and most importantly our results detect the expected positive coefficient for the explanatory variable *clunker*, significant at the one percent level. Thus in the overall dataset of 23 OECD-countries we find a statistically sig-

¹⁰The Wald-test for groupwise heteroscedasticity in fixed effect regression model rejects the Null hypothesis of homoscedasticity at the 1 percent significance level.

¹¹This finding can be derived from the regression **Table 4**, which gives the Stock and Yogo test statistic and the critical value for 10 percent bias due to IV implementation. As over all specifications (1) to (5) the test stistic clearly exceeds the critical value, one can conclude that the bias of instrumental variable estimation is less than 10 percent compared to an OLS regression set-up.

nificant positive car sales effect of the scrapping policy. The coefficient of 0.03 is far less than the ones obtained in the case-study analysis in the literature, but it is an average effect over all programs with very different durations and success, as we will point out in the next section.

The interest rate enters the regression with the expected negative, but statistically significant coefficient. It controls for the overall financing condition for durable goods, indicated by the short term three months interest rate. This finding suggests that a decrease in the interest rate increases the car demand of individuals. The unemployment rate and the industry production further exercise significant effects on automotive sales. The coefficients are in line with previous research and show the strong correlation of the business cycle and the purchase of new vehicles. Furthermore the incorporated linear time trend suggests a slight decrease in car sales over the eleven year time span of interest which is in line with real world observations. The gasoline price variable exhibits a slightly positive, but statistically insignificant effect on car registrations. On an aggregate level we do not find evidence for a negative price-elasticity of gasoline prices.

Hence we conclude that we obtain a significant positive effect of the Cash-for-Clunkers subsidy in our panel dataset controlling for unobserved heterogeneity between countries.

3.4 Sensitivity analysis

In order to show the robustness of our results one of the first concerns that might come to mind is autocorrelation of the standard errors. This could lead to wrong inference, especially because we are working with monthly data. Our empirical model is a dynamic fixed effects model with instruments, therefore the standard panel data autocorrelation tests like the Wooldridge test for serial autocorrelation in panel data does not work in this context.¹² Hence we employ time series autocorrelation tests, proposed by Cumby and Huizinga (1992) for each country in the dataset separately. The advantage of this test is that it is applicable even with endogenous regressors, thus after IV-regressions and in specifications with heteroscedasticity in the data. **Table 6** presents the autocorrelation test results for each country. In 20 out of 23 countries the null hypothesis of non-autocorrelated standard errors cannot be rejected, thus we argue that autocorrelation of the errors is not a serious concern in our data set.

A second concern that needs to be addressed is the question of anticipatory

¹²See Drukker (2003); Wooldridge (2002) for a discussion of the Wooldridge test.

Table 4: Second Stage Fixed-Effects Regression Results (for different sets of instruments)

l_pc_oecd	(1)	(2)	(3)	(4)	(5)
L1. l_pc_oecd	0.8872*** (0.000)	0.9094*** (0.000)	0.8527*** (0.000)	0.8848*** (0.000)	0.8498*** (0.000)
clunker	0.0349*** (0.001)	0.0341*** (0.002)	0.0381*** (0.000)	0.0354*** (0.001)	0.0375*** (0.000)
ma_p_gasoline	0.0088 (0.572)	0.0101 (0.519)	0.0147 (0.359)	0.0109 (0.485)	0.0152 (0.351)
UR	-0.0045** (0.032)	-0.0034 (0.113)	-0.0059*** (0.009)	-0.0045** (0.030)	-0.0060*** (0.008)
interest_rate	-0.0059*** (0.001)	-0.0057*** (0.002)	-0.0057*** (0.009)	-0.0059*** (0.001)	-0.0061*** (0.001)
l_ip_OECD	0.1187*** (0.000)	0.1095*** (0.001)	0.1256*** (0.000)	0.1188*** (0.000)	0.1307*** (0.000)
time_trend	-0.0003*** (0.002)	-0.0003*** (0.003)	-0.0004*** (0.000)	-0.0003*** (0.001)	-0.0004*** (0.001)
No. of obs	2,669	2,651	2,615	2,651	2,597
No. of groups	23	23	23	23	23
Wald χ^2	437.8***	414.57***	342.14***	437.76***	339.22***
Adj R^2	0.7156	0.7102	0.7151	0.7141	0.7166
Hansen J statistic	1.432 (0.231)	0.000 (0.988)	0.852 (0.356)	1.609 (0.447)	3.692 (0.16)
Stock and Yogo	175.5	140.9	88.1	131.4	60.7
10% max IV size	19.93	19.93	19.93	22.3	22.3

Notes: P-values in paranthesis; heteroscedasticity robust standard errors reported;
The one period lagged dependent variable is instrumented by its own lags of different order:
(1) Lags 7 and 8; (2) Lags 8 and 9; (3) Lags 10 and 11; (4) Lags 7, 8 and 9; (5) Lags 10, 11 and 12
*** statistically significant at the 1% level; ** statistically significant at the 5% level; * statistically significant at the 10% level.

Table 5: First Stage Regression (for different sets of instruments)

L1. l_pc_oecd	(1)	(2)	(3)	(4)	(5)
clunker	0.0979*** (0.000)	0.0982*** (0.000)	0.0916*** (0.000)	0.0992*** (0.000)	0.0931*** (0.000)
ma_p_gasoline	0.1087*** (0.000)	0.1154*** (0.000)	0.1237*** (0.000)	0.1125*** (0.000)	0.1272*** (0.000)
UR	-0.0217*** (0.000)	-0.0250*** (0.000)	-0.0302*** (0.000)	-0.0215*** (0.000)	-0.0303*** (0.000)
interest_rate	-0.0156*** (0.000)	-0.0158*** (0.000)	-0.0147*** (0.000)	-0.0157*** (0.000)	-0.0139*** (0.000)
l_ip_OECD	0.3336*** (0.000)	0.3625*** (0.000)	0.3861*** (0.000)	0.3386*** (0.000)	0.3808*** (0.000)
time_trend	-0.0013*** (0.000)	-0.0014*** (0.000)	-0.0014*** (0.000)	-0.0013*** (0.000)	-0.0155*** (0.000)
L7. l_pc_oecd	0.4232*** (0.000)			0.4109*** (0.000)	
L8. l_pc_oecd	0.1328*** (0.002)	0.1368*** (0.003)		0.1173** (0.023)	
L9. l_pc_oecd		0.3471*** (0.000)		0.0305 (0.535)	
L10. l_pc_oecd			0.3009*** (0.000)		0.2999*** (0.000)
L11. l_pc_oecd			0.0633 (0.2160)		0.0723 (0.2570)
L12. l_pc_oecd					-0.0155 (0.7870)
No. of obs.	2,669	2,651	2,615	2,651	2,597
No. of groups	23	23	23	23	23
Wald χ^2	175.5***	140.9***	88.1***	131.4***	60.7 ***
Shea Partial R^2	0.2666	0.1999	0.1187	0.2642	0.1164

Notes: p-values in paranthesis; heteroscedasticity robust standard errors reported;
The one period lagged dependent variable is instrumented by its own lags of different order:
(1) Lags 7 and 8; (2) Lags 8 and 9; (3) Lags 10 and 11; (4) Lags 7, 8 and 9; (5) Lags 10, 11
and 12
*** statistically significant at the 1% level; ** statistically significant at the 5% level; *
statistically significant at the 10% level

effects or posttreatment effects of the accelerated vehicle retirement policy.¹³ In case that the Granger causality of the policy variable and car sales is not solved, the estimates could be misleading if further leads of the clunker dummy variable affect the car sales today. In most countries in the dataset it can be economically argued that those effects are most likely not of serious concern in the Cash-for-clunkers case, as the programs were legislated really short after the discussion period. For example in Germany the whole legislation process took only four weeks.¹⁴ Nevertheless we additionally check these concerns with a separate regression including four leads and four lags to formally control for anticipatory and post-treatment effects. **Table A.2** in the appendix shows the regression results for the fixed effects estimation described in 3.3. Neither the four leads nor the lags of the policy variable *clunker* show any statistically significant influence on car sales in the current period. Therefore, we argue that the causality clearly runs from the clunker today to car sales today. No anticipatory reform effects seem to be present in a significant manner.

A third issue, one might worry about is the functional form used for the estimation of the *clunker* effect. The underlying dataset is characterised by a long time-structure of 132 periods, therefore the appropriateness of a linear time trend can be discussed. We reestimated the regression model with time-dummy variables for each month as well. Results of this regression can be found in **Table 7(1)**.

The clunker variable, the lagged dependent variable and the unemployment rate almost do not change. Among the control variables the coefficient of the industry production index decreases and the gasoline price coefficient switches signs, however the effect is still insignificant. All the tests performed concerning the performance of the instruments and the goodness of fit do not vary between the two specifications. The F-Test however decreases substantially, indicating that the specification with linear time trend is superior to the the time-dummy variable model.

Apart from that, one might wonder whether the model is robust to removing the logarithms of the dependent variable and therefore the lagged dependent variable on the right hand side as well. As the results in **Table 7 (2)** show the coefficients are not affected at all. We use the insights from this specification for our following simulation section. It is easier to quantify the effect in total numbers if the dependent variable follows a linear pattern.

In this section we showed that the model is robust to different functional

¹³See Angrist and Pischke (2009), 237-241.

¹⁴The German “Konjunkturpaket 2” was legislated in cabinet January, the 14th 2009 and passed “Bundesrat”, the final legislative entity of the German political system February, the 2nd 2009. The application process for the AVR was possible from January, the 27nd. See BAFA (2010) for a timeline of events.

Table 6: Cumby-Huizinga Autocorrelation Tests of Individual Country Time-Series

Country	Test statistic	p-value
Australia	0.55	0.4601
Austria	0.72	0.3971
Belgium	0.00	0.9935
Canada	0.27	0.6055
Denmark	2.45	0.1178
Finland	0.13	0.7191
France	0.56	0.4553
Germany	0.09	0.7581
Greece	1.40	0.2363
Ireland	0.02	0.8803
Italy	0.00	0.9663
Japan	1.28	0.2583
Korea	4.68	0.0305**
Luxemburg	0.04	0.8464
Netherlands	0.70	0.4016
Poland	1.13	0.2875
Slovakia	0.02	0.8942
Slovenia	0.02	0.8824
Spain	0.19	0.6664
Sweden	11.16	0.0008***
Switzerland	12.10	0.0005***
UK	0.52	0.4705
USA	0.67	0.4126

Notes: H_0 : non-autocorrelated at order 1 *** statistically significant at the 1% level; ** statistically significant at the 5% level; * statistically significant at the 10% level.

Table 7: Sensitivity Analysis

	(1)	(2)	(3)
l_pc_oecd	time_dummies	unlogged	basic
Lag1.l_pc_oecd	0.8872*** (0.000)	0.8700*** (0.000)	0.8892*** (0.000)
clunker	0.0349*** (0.001)	3.3418*** (0.003)	0.0345*** (0.001)
ma_p_gasoline	0.0088 (0.572)	0.6206 (0.717)	-0.0029 (0.859)
UR	-0.0045** (0.032)	-0.424* (0.070)	-0.0049** (0.015)
interest_rate	-0.0059*** (0.001)	-0.5209*** (0.002)	-0.0036 (0.140)
lip_OECD	0.1187*** (0.000)	12.291*** (0.000)	0.0831** (0.014)
time_trend	- -	-0.0258** (0.014)	-0.0003*** (0.002)
time dummies included	yes	no	no
No. of obs	2,669	2,669	2,669
No. of groups	23	23	23
Wald χ^2	35.5***	382.8***	437.8***
Adj R^2	0.72	0.62	0.71
Hansen J statistic	1.822 (0.177)	2.378 (0.123)	1.432 (0.231)
Stock and Yogo test	173.501	117.966	175.5
10% max IV size	19.93	19.93	19.93

Notes: P-values in paranthesis; heteroscedasticity robust standard errors reported; (1) to (3) uses Lags 7 and 8 as instruments for the lagged dependent variable. *** statistically significant at the 1% level; ** statistically significant at the 5% level; * statistically significant at the 10% level.

forms and specifications. In order to draw further inference we simulate a counterfactual situation in section four and predict the overall sales effect in total numbers.

4 Simulations

4.1 Strategy

The perfect set-up in quantifying the sale’s effect of the governmental policy in our panel dataset would be taking the difference between the total sales number realized (treatment effect) and the counterfactual situation in absence of the policy, usually referred to as the control group. For case study work on the AVR programs this difference-in-difference approach has been used to evaluate the effectiveness of the Cash-for-Clunkers program in the USA (see Mian and Sufi (2012); Li et al. (2012)). For a panel analysis of various countries the detection of a single country or a group of countries serving as a control group is almost impossible.¹⁵ Thus we simulate a counterfactual situation during and after the end of the policy period and compare the realized outcomes and the simulated sales numbers.

In order to generate the counterfactual situation, we further on restrict our sample to countries that conducted an AVR program. This reduces our dataset to 15 OECD countries.¹⁶ For those that had various schemes at place during the examination period, we use the latest AVR program, because our aim is to show the effectiveness of recent policies between 2008 and 2010. Moreover, the simulation relies on the unlogged version of the dynamic fixed effects model, reported in part 3.4 in **Table 7** (2), because we are interested in the absolute effect of the policy. The time period is divided into three parts: T=0 is the period before the subsidy takes place, T=1 refers to the subsidy period and T=2 specifies the months after the subsidy.¹⁷

The simulation procedure itself consists of four steps:

1. Estimation of the fixed-effects panel model for T=0 and obtain the fitted values $\hat{p}c_{i,m}$, where m indicates the corresponding month.

¹⁵For each country that had a car scrappage scheme we would have to find a corresponding country with similar car-market characteristics that did not have an AVR program at place.

¹⁶More explicitly the countries having had an AVR are in alphabetical order: Austria, Canada, France, Germany, Greece, Ireland, Italy, Japan, Korea, Luxembourg, the Netherlands, Slovakia, Spain, the USA and the United Kingdom. The regression results are not affected by this alteration of the data. The regression output table is available from the authors upon request.

¹⁷For some of the countries under consideration the AVR-program is still effective, see **Table 1**. Examples are Canada and Ireland.

- Simulation of the counterfactual car sale values $\hat{p}c_{counter_{i,m}}$ for T=1 and T=2 with an iterative procedure, using $\hat{p}c_{i,m}$ values of T=0 for the lagged dependent variable on the right hand side of the equation, instrumented by lags 7 and 8.

E.g. for the first month m of period T=1 the equation to be estimated looks as follows:

$$\hat{p}c_{counter_{i,m}} = c_i + \beta_1 \hat{p}c_{i,m-1} + \beta_2 time_m + \sum \beta_k (X_{i,m,k}) + \varepsilon_{i,m}$$

Using this stepwise prediction scheme, we avoid calculating counterfactual values including realizations of variables from period T=0. Alternatively, we had to estimate a static version of our model, which would clearly misspecify the data generating process. As a result, we decided to use the dynamic specification and run a stepwise forecasting scheme.

- The sales effect during and after the subsidy in comparison to the counterfactual is calculated as the difference of realized sales number $pc_{oced_{i,m}}$ and the simulated variable $\hat{p}c_{counter_{i,m}}$. The obtained variables are named $pc_{scrap_{i,m}}$ (for T=1) and $pc_{after_{i,m}}$ (for T=2)
- In terms of comparison we take the means of $pc_{scrap_{i,m}}$ and $pc_{after_{i,m}}$ in order to compare the overall monthly effects of the AVR programs by taking into account the different durations m of the AVR programs in different countries i . The difference of these mean values finally states the overall effect of the scrappage subsidy until the last month in the data set

$$pc_{overall_i} = \bar{p}c_{scrap_i} - \bar{p}c_{after_i}$$

Our approach is similar to the well known Event-Study-Approach frequently used in Financial Economics to measure stock price reactions to different shocks. Examples for such shocks are mergers or announcements of bank distress.¹⁸ Period T=1 in our analysis is what financial economists would call the Event Window. Compared to financial applications, we do not have the difficulty to find an appropriate size of the event window, because the event window is already defined by government as a result of the duration of the scrappage program.

The results are discussed in the following section.

4.2 Results

The presentation of the results is restricted to the most discussed AVR subsidies in South Korea, Germany, the USA and the UK. For each country we

¹⁸For a detailed discussion of the event study methodology see Degryse et al. (2009), 10-13.

display the results graphically and in absolute numbers weighted by subsidy length and the overall budget of the scrappage program.¹⁹

4.2.1 Graphical Presentation

Figure 1 illustrates the simulation results for each country separately. The solid line represents the observed car registration numbers per month *pc_oecd*. The dashed line, however, describes the simulated counterfactual simulation results *pc_counter* during and after the subsidy period. For the pre-subsidy period *pc_counter* incorporates the fitted values of the fixed effects estimation. For graphical reasons the time span of the graphs is restricted to January 2008 until December 2010, as the main period of interest is the one including the car scrappage policies implemented to fight the vehicle sales crisis as a result of the financial crisis. The beginning and end of the subsidy period is depicted through the two vertical lines. As shown in **Table 1** of the introduction the car scrappage subsidies in these four countries differ considerably in duration and budget.

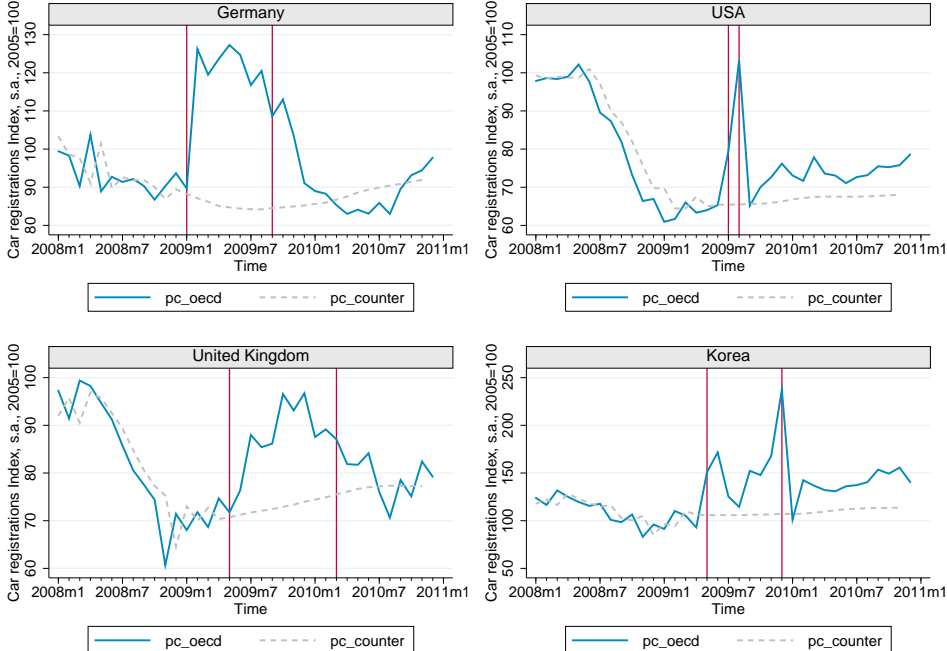
On the one hand the simulation results illustrate similarities between the four countries. The effect of the car-scrappage scheme is visible as a big spike in the car registration numbers for all four countries and graphically support the fixed effects estimation results found in paragraph 3.4. Furthermore the drop after the end of the subsidy period can also be found for the country-quartet, although for Germany it is lagged by two months.

On the other hand there are obviously important differences in car registration numbers, especially before implementation of the subsidy. For the United Kingdom and the USA we observe a huge drop before the AVR-programs. In the United Kingdom the decline started in March 2008 and the bottom was reached in November 2008, whereas for the USA the drop began in July 2008 and hit rock bottom in January 2009. Until the AVR program was launched (six months after the bottom in both countries) the car registration numbers slightly increased. For Germany and South Korea a similar pattern can not be found previous to the implementation of the subsidy.

In order to compare the positive effect of the car-scrappage policy with the decline thereafter we calculate the overall balance of the effect in the next section.

¹⁹The calculated results for the remaining countries can be retraced in **Table A.3** in the appendix.

Figure 1: Simulation Results for Germany, USA, UK and South Korea



4.2.2 Calculation of the Size of the Policy Effects

This section has two parts. First of all we analyze the effect of the car scrappage policy and then continue with the effect of the overall period by comparing the positive effect of the car-scrappage policy with the decline thereafter.

In a first step we calculate the overall effect of the car scrappage program for the four countries of interest. We therefore sum up the difference between the realized car-registrations and the predicted counterfactual ones. As these are all given in index format we convert them to absolute values.²⁰ The estimated numbers are given in the bottom of **Table 8**. If we compare these numbers to the number of cars that have been scrapped due to the corresponding governments final reports²¹, we see that there are substantial differences except for the United Kingdom. The estimated number of scrapped cars is almost half the official number of cars reported for the USA and more than half for Germany and Korea. These first calculations indicate that a substantial number of cars would have been bought anyway because of normal replacement decisions or low interest rates despite the automotive sales crisis.

We now turn to the question: what about the effect over the whole period, also taking account of the period after the subsidy. In order to answer this question **Table 8** depicts the simulation results in balance-sheet-format for the United Kingdom, Germany, the USA and South Korea over the life-span of the AVR-program *pc_scrap* and the period thereafter *pc_after*, as far as data is available. The first column represents the difference in passenger car registrations between the realized sales number and the simulated counterfactual situation during the subsidy period and the second column indicates the identical difference, but for the months thereafter. As the subsidy period differs considerably between countries in terms of timing and duration, we weight the balance of each country with the number of months. The computed sales effect is displayed in index-format with the mean value of the year 2005 representing 100. In order to give a more convincing picture the difference in index formats is also recomputed in absolute car registration numbers per month. One can finally state that the positive effect of the car scrappage policy is way larger than the effect afterwards indicating some pull-forward effects from future periods. Even if the effect for some months after the subsidy is negative, compared to the counterfactual situation, we obtain positive average car sales number for all four countries. For Germany and the United States the average

²⁰We do this with the help of the *pc_reg* variable. The datasources are incorporated in **Table 2**.

²¹The corresponding numbers for the four countries can be found in; BAFA (2010), Appendix for Germany, Cooke (2010), p. 24 for the United Kingdom, Clowers (2010), p.8 for the United States and Canis et al. (2010), p.12 for South Korea.

sales effect over the whole period is almost the same number of roughly plus 38,000 cars per month. The Korean vehicle scrappage subsidy led to a slight lower plus 37,630 cars per month, followed by the British program indicating 18,000 additional cars registered each month. One should additionally keep in mind that the overall budget in the United States was less than half of the German one²², but leading to the same number of new cars registered per month. Apart from that South Korea’s program seemed more successful than the British one, as it led to roughly 20,000 cars more sold, having 150 million Euros budget less. In **Table A.3** in the appendix an overview of the policy effects for the other countries can be found for the sake of completeness.

This leads to the conclusion that not only the budget volume, but also timing and implementation of the car scrapping subsidy is crucial for its effectiveness.

5 Conclusion

Using an OECD data set of 23 countries we find a positive effect of car scrappage programs on overall car sales, as long as the subsidy is in place. This result is obtained through a dynamic fixed effects model and is in line with findings of the related literature. The most striking advantage of our approach is that we are able to control for unobserved heterogeneity between countries. We find positive effects even if the countries’ AVR-programs differ considerably in the design of the subsidy. Apart from that we simulate a counterfactual situation and for the USA, the UK, Germany and South Korea and find a positive overall balance of registered cars, even if the months after the end of the scrappage subsidy are taken into account. Nevertheless timing and duration of the policy design seemed more important for its effectiveness than the overall budget. Our results suggest that almost the same number of additional cars were sold on monthly average in the USA and in Germany, the corresponding budgets are 2 billion Euros and 5 billion Euros respectively. However, the German scrappage scheme was implemented after a period of only slight decrease in automotive sales, whereas before the US scrappage subsidy was agreed upon a massive decline in sales is obvious. Furthermore our results do not suggest an immediate reversal of the scrappage policy after a few months. We only have two countries in our dataset, that exhibit a negative influence on car sales through the car scrappage subsidy. These two countries are the Netherlands and Greece, but this effect might be caused by other determinants as the general economic conditions.

²²See **Table 1** for an overview of the countries budgets.

Table 8: Simulation Results Calculating for the UK, Germany, the USA and Korea

	UK			Germany			USA			Korea		
	pc_scrap May 09- Mar 10	pc_after Apr 10 - Nov 10	pc_scrap Jan 09- Sep 09	pc_after Oct 09- Nov 10	pc_scrap Jul 09 - Aug 098	pc_after Sep 09- Nov 10	pc_scrap May 09- Dec 12	pc_after Jan 10- Nov 10				
Difference in Index points	1.02 5.12 16.27 13.34 13.72 23.68 19.76 22.74 13.11 14.27 11.50 14.05	5.79 5.06 7.13 -1.18 -6.70 1.20 -2.13 5.10 1.78	1.47 39.16 33.30 38.54 42.54 40.31 32.53 36.30 24.08 32.03	28.27 18.60 5.85 3.35 2.33 -1.43 -4.57 -4.49 -6.33 -3.99 -7.43 -1.27 1.68 2.53 2.36	14.05 37.82 25.94	-0.51 4.37 6.71 9.89 6.27 4.48 10.40 6.08 5.50 3.57 5.12 5.57 7.75 7.40 7.76 6.02	45.08 65.81 19.44 8.65 46.03 41.43 61.10 130.03 52.20	-5.70 35.07 28.34 22.58 19.93 24.13 24.66 27.32 40.23 35.95 41.90 26.76				
Estimated sum of cars scrapped (total)	314,182	29,032	797,275	91,572	239,181	416,669	419,324	295,648				
Official no. of cars scrapped (total)	372,401	-	1,933,090	-	401,274	-	890,000	-				
Balance in Index points (monthly avg.)	8.88		13.97			8.37		37.47				
Balance in cars (monthly avg.)	18,064		38,645		38,579			37,630				
Budget (in K Euros)	463,000		5,000,000		2,084,000			309,000				

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A Appendix

Figure A.1: Car registrations for representative countries with and without AVR

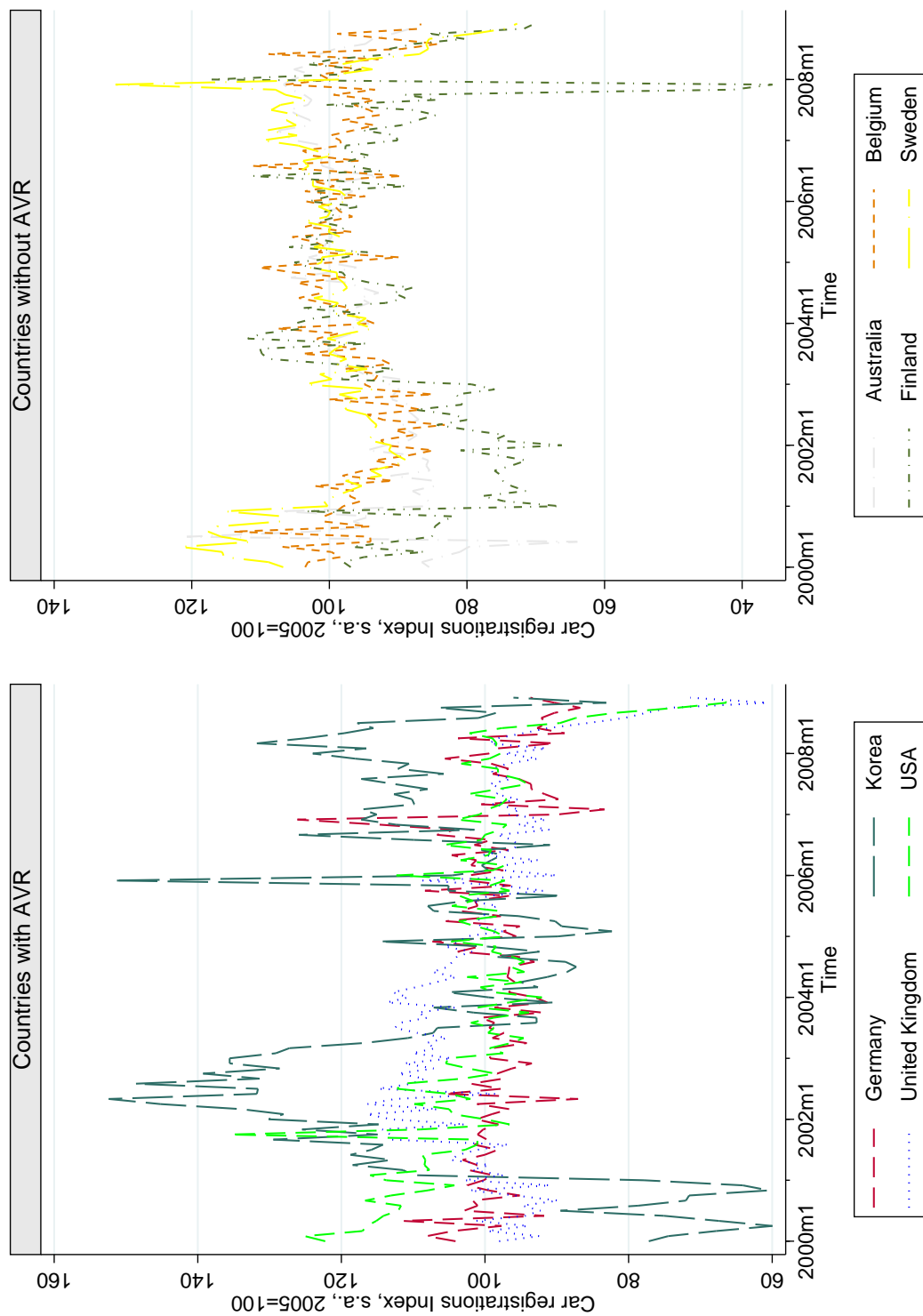


Table A.1: Panel Unit Root Tests

Variable	Test	Lag	Test statistic	p-value	trend
<i>Dependent variable</i>					
l_pc_oecd	Im-Pesaran-Shin	0.8	-6.9***	0.000	yes
<i>Independent variables</i>					
interest_rate	Im-Pesaran-Shin	1	-1.6*	0.052	no
lip_OECD	Im-Pesaran-Shin	0.7	0.8	0.784	yes
ma_p_gasoline	Im-Pesaran-Shin	1	-11.0***	0.000	yes
UR	Im-Pesaran-Shin	0.8	2.4	0.992	no

Notes: Ho: all panels contain a unit root; The Im-Pesaran-Shin test conducted uses the Akaike information-criterion to define the optimal lag-structure of the underlying test-statistic; *** statistically significant at the 1% level; ** statistically significant at the 5% level; * statistically significant at the 10% level.

Table A.2: Testing Anticipatory Effects

L_pc_oecd	
L1.l_pc_oecd	0.8896*** (0.000)
L1.clunker	-0.0483 (0.364)
L2.clunker	-0.0318 (0.389)
L3.clunker	0.0128 (0.684)
L4.clunker	-0.0067 (0.691)
clunker	0.1325** (0.017)
F1.clunker	-0.0157 (0.656)
F2.clunker	-0.0622 (0.119)
F3.clunker	0.0564 (0.191)
F4.clunker	-0.0147 (0.572)
ma_p_gasoline	0.0114 (0.464)
UR	-0.005** (0.021)
interest_rate	-0.0058*** (0.002)
lip_OECD	0.0976*** (0.003)
time_trend	-0.0003 (0.003)
No. of obs	2,602
No. of groups	23
Adj R^2	0.7019
Wald χ^2	210.2***
Hansen J statistic	1.891 (0.169)
Stock and Yogo test	173.9 19.93

Notes: P-values in paranthesis; heteroscedasticity robust standard errors reported;The one period lagged dependent variable is instrumented by its Lags 7 and 8;*** statistically significant at the 1% level; ** statistically significant at the 5% level; * statistically significant at the 10% level.

Table A.3: Simulation Results (other countries)

	Austria		Greece		Netherlands		Ireland	
	pc_scrap	pc_after	pc_scrap	pc_after	pc_scrap	pc_after	pc_scrap	pc_after
Sum of cars scrapped	18,166	34,076	-13,892	-46,564	-16,285	-	4,551	-
Balance in Index points	10.18		-17.93		-2.21		2.89	
Balance in cars	2,612		-4,030		-857		414	
	Spain		Luxembourg		Italy		Canada	
Sum of cars scrapped	pc_scrap	pc_after	pc_scrap	pc_after	pc_scrap	pc_after	pc_scrap	pc_after
Balance in Index points	667,907	154,598	11,379	1,511	690,273	257,640	129,205	-
Balance in cars	23.91		13.86		10.82		8.34	
	30,463		560		20,168		5,873	
	France		Japan		Slovakia			
Sum of cars scrapped	pc_scrap	pc_after	pc_scrap	pc_after	pc_scrap	pc_after		
Balance in Index points	1,329,381	-	745,972	-190,299	22,786	15,813		
Balance in cars	32.14		7.02		38.61			
	55,391		27,784		1,838			