Synthesizing Cash for Clunkers: Stabilizing the Car Market, Hurting the Environment

Gregor Pfeifer, Saarland University

Stefan Klößner, Saarland University

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Abstract

We examine the impact of car scrappage programs on new vehicle registrations and respective CO2 emissions with a particular focus on Germany. To construct proper counterfactuals, we develop MSCM-T, the multivariate synthetic control method using time series of economic predictors. Applying MSCM-T to a rich data set covering 23 European countries, we find that the German program had an immensely positive effect of about 1.3 million program-induced new car registrations. Almost one million of those purchases were not pulled forward from future periods, worth more than three times the program's €5 billion budget. However, with a conservative net estimate of about 2.4 million tons of program-induced CO2 emissions, we find that the subsidy did severely hurt the environment. For other European countries with a comparable car retirement program we show further positive results regarding vehicle registrations. What is more, we also demonstrate that all of the observed non-scrapping countries could have considerably stabilized their respective vehicle market by adopting a scrappage subsidy.

JEL Codes: D04; D12; H23; H24; L62; Q50

Keywords: Cash for Clunkers; Synthetic Controls; Policy Evaluation; Car Market; CO2 Emissions

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1 Introduction

In response to the recent economic downturn, governments around the globe intervened with vehicle retirement schemes worth more than €15 billion in total. European countries contributed almost 60% of this amount with Germany affording the most expensive program utilizing €5 billion to subsidize two million car purchases. As for most countries, the primary purpose of such programs was twofold: stimulating the declining demand for new vehicles that threatened not only the local automotive market but the economy as a whole; and reducing road traffic related carbon dioxide (CO2) emissions.

In this paper, we seek to measure whether these goals were accomplished, examining the impact of scrappage schemes on purchases of new vehicles and on greenhouse gas emissions in Europe with a particular focus on Germany. To be more precise, we are evaluating effects on new passenger car registrations and how subsidized purchases can be divided into windfall gains, pull-forward effects, and on-top sales. In a next step, we estimate how corresponding CO2 emissions of new passenger cars behave in consequence of the policy intervention and relate those findings to our sales results. Finally, after conducting similar analyses for other scrapping countries, we test if countries that did not opt for a scrappage subsidy could have benefited if they had decided to do so.

The main challenge regarding an impact analysis of this kind arises from the difficulty to construct a reasonable counterfactual of new car registrations and CO2 emissions in absence of the subsidy—and vice versa. To identify our effects of interest, we use a rich data pool of 23 European countries and several covariates to which we apply a generalized and extended version of synthetic control methods (SCM).[1] SCM rests upon the comparison of outcome variables between a unit representing the case of interest, i.e. Germany as *affected* by the policy intervention, and otherwise similar but *unaffected* units reproducing an accurate counterfactual of Germany in absence of the scrappage program. An algorithm-derived combination of precisely weighted comparison units is supposed to better depict the characteristics

[1]SCM, introduced in Abadie and Gardeazabal (2003) and Abadie et al. (2010), were applied, i.a., by Coffman and Noy (2012) as well as Cavallo et al. (2013) (natural disasters), Hosny (2012) (free trade), Acemoglu et al. (2013) (political connections), Billmeier and Nannicini (2013) (economic growth), Jinjarak et al. (2013) (capital inflows), Kleven et al. (2013) (taxation of athletes), Bauhoff (2014) (school nutrition), and Belot and Vandenberghe (2014) (educational attainment).

of Germany than either any single comparison unit alone or an equally weighted combination of all or several available control units. Using an appropriate set of covariates, also known as economic predictors, SCM select the counterfactual unit as the optimally weighted average of such comparison units that best resemble the characteristics of Germany. The original SCM approach can address only one single variable of interest, additionally it is restricted to using only means of the economic predictors over time. We therefore introduce *MSCM-T*, the multivariate synthetic control method using time series. MSCM-T allows to use entire time series of the economic predictors, thereby exploiting their variation over time in order to find the best counterfactual for Germany. Furthermore, MSCM-T not only considers more than one variable of interest, i.e. multiple outcomes, but does so simultaneously, taking account of possible interdependencies between these.

Applying MSCM-T, we find that the German scrappage program had an immensely positive effect on new car registrations with more than one million program-induced vehicles during its peak time. In order to disentangle subsidized purchases, we build on a set of stylized timelines for different sub-groups and run a linear regression to estimate these groups' share in the overall sales effect. About $650,000$ sales were so-called windfall gains, i.e. subsidized purchases that would also have happened in absence of the policy intervention. The actually program-induced sales consist of about $440,000$ cars that have been pulled forward in time and about $850,000$ vehicles that would not have been purchased at all in absence of the German subsidy. The latter number implies that about almost one million newly registered cars happened on top of regular sales and were not simply pulled forward from future periods. In monetary terms, this is more than three times the €5 billion budget which would not have been realized without the scrappage scheme.

Regarding our second variable of interest, we find a positive effect of the program on overall greenhouse gas emissions over the new passenger cars' life time, i.e. higher CO2 pollution due to subsidized, newly purchased cars. Using our MSCM-T results to disentangle specific environmental effects, we show that, on the one hand, the groups of windfall gains and pull-forward purchases lead to *reduced* CO2 emissions in *2009*. On the other hand, however, because of *higher* CO2 emissions in *later years*, those groups increase emissions over the life time of the new vehicles by 2.4 million tons of CO2, causing notable environmental damage as a result of the stimulus program.

For every other European country with a comparable scrappage scheme we also find positive impacts on car registrations. The same applies to countries for which we ask what *would have* happened if they *would have* chosen to implement a scrappage subsidy but did not do so. Computing counterfactual new registration numbers, we show that all of these non-scrapping countries could have considerably backed their respective vehicle market adopting a stimulus program. Moreover, regarding CO2 emissions, mixed patterns emerge mostly similar to the German case.

By analyzing the consequences of scrappage schemes, we join a dynamic literature examining stimulus impacts of, i.a., tax rebates (e.g., Shapiro and Slemrod (2003a); Shapiro and Slemrod (2003b); Johnson et al. (2006); Agarwal et al. (2007); Shapiro and Slemrod (2009); Parker et al. (2013)), income tax changes (e.g., House and Shapiro (2006)), or government spending on infrastructure and education (e.g., Feyrer and Sacerdote (2011)). On a more profound level, our paper is closely related to several studies of scrappage programs as a reaction to the recent economic crisis. Amongst others, Mian and Sufi (2012), Li et al. (2013), Copeland and Kahn (2013), and Hoekstra et al. (2014) use alternative identification strategies to show that the positive effect of CARS, the 2009 U.S. scrappage program, on new vehicle sales during its two months duration came entirely at cost of a reversed effect the following months. Heimeshoff and Müller (2013) provide panel data estimates for new car registrations as a reaction to scrappage programs and find mixed effects across several OECD countries. Leheyda and Verboven (2013) evaluate scrappage schemes of 9 European countries using a difference-in-difference design and find that, overall, they had a positive effect on vehicle sales of about 16%.[2] They further find that scrapping schemes only had a small effect on average fuel consumption of new cars. Other studies, evaluating recent scrappage subsidies and their influences on the environment are, i.a., Knittel (2009) and, again, Li et al. (2013).[3] Both pieces look at the cost of reducing CO2 and find that, with about \$300-\$450 per ton, the U.S. Cash for Clunkers program was an expensive way to reduce greenhouse gases.

Our paper contributes to this literature in several ways. First, we provide clear counterfac-

[2] Moreover, Adda and Cooper (2000), Licandro and Sampayo (2006), and Schiraldi (2011), e.g., evaluate a French, a Spanish, and an Italian program from the late 90's respectively.

[3] More environmental analyses of past programs are available, e.g., those of Hahn (1995), Deysher and Pickrell (1997), Kavalec and Setiawan (1997), or Szwarcfiter et al. (2005).

tual evidence regarding new vehicle sales *and* greenhouse gas emissions caused by the German program. To the best of our knowledge, we are first to expand on such results by providing a specific model to precisely estimate the share and impact of different sub-groups in the overall sales and emission effect: windfall gains, pull-forward effects, and on-top sales. We use those findings to better understand whether the policy intervention was potentially effective, or even efficient. Second, since such an analysis demands a proper method of inference allowing for multiple, interdependent outcomes of interest, we develop MSCM-T. This generalized and extended version of SCM not only enables us to jointly synthesize with respect to several dependent variables, but also to utilize entire time series of economic predictors instead of only their means. Third, we enlarge the scope of such analyses by looking at eleven further scrapping countries and also make use of twelve more countries without a vehicle retirement program. We thereby do not only estimate the effects of *actual* scrapping schemes by using MSCM-T, but also evaluate *potential* effects that could have happened if a country would have chosen to implement such a policy intervention but did not do so. Such counterfactual analyses are of particular importance for policy makers.

The remainder of the paper is structured as follows: Section 2 describes car scrappage programs during the crisis with a particular focus on Germany and presents our data set including first descriptive evidence of the two outcomes of interest. We explain our implementation of MSCM-T in Section 3 while Appendix A outlines some auxiliary theory. Section 4 provides the analysis for the German case: after shortly illustrating some aspects of model specification, it presents and discusses our findings on how the program helped to stabilize the car market at the cost of hurting the environment in the long-run. Moreover, we discuss several policy implications based on our findings. Section 5 presents a number of sensitivity checks. We conduct further synthetic control analyses for other scrapping countries and also for non-scrapping countries across Europe in Section 6. Section 7 concludes.

2 Program and Data Description

2.1 Cash for Clunkers

During 2008–2010, there were similar scrappage programs in 22 different countries all over the world. The biggest overall budget was provided by Germany with €5 billion, followed by Japan with about €2.9 billion. The U.S. spent about €2 billion and ranks third.[4] In per capita figures, Germany invested about €61, almost three times as much as Japan, Italy, and Luxembourg respectively, who all spent a little more than €20. The U.S. ranks twelfth with about €6.50 per capita. Worldwide, about €15.3 billion were spent on vehicle retirement programs while the European Union contributed €8.8 billion or 58% of this sum. The €5 billion spent by Germany totaled 33% of the worldwide budget and 57% of what was spent by all European countries.

In Germany, the idea for a scrappage program was introduced by then vice-chancellor Steinmeier in an interview on December 27, 2008. Only two weeks later, the federal government passed the Economic Stimulus Package II including a scrappage scheme—called "environmental premium". The program officially started on January 14, 2009, financed by the Investment and Repayment Fund. First key points were published on January 16, 2009 by the responsible agency BAFA[5]. The subsidy of €2,500 could be requested by private individuals who scrapped an old passenger car which had to be at least nine years old and licensed to the applicant for at least 12 months. The new car had to be a passenger car fulfilling at least the emission standard Euro 4[6] and be licensed to the claimant. Applications were possible until the end of 2009 or the exhaustion of the budget. The latter happened on September 2, 2009, when 2 million new cars had been subsidized.[7]

[4] The U.S. government passed the Consumer Assistance to Recycle and Save Act on June 24, 2009. The offered premium was either $3,500 or $4,500 depending on the type of car purchased and the improvement in fuel economy from the old to the new vehicle. The $3 billion budget only lasted about two months subsidizing the purchase of roughly $700,000$ new vehicles.

[5] *Bundesamt für Wirtschaft und Ausfuhrkontrolle* (Federal Office of Economics and Export Control).

[6] European emission standards define the acceptable limits for exhaust emissions of new vehicles sold in EU member states. Actually, for the German case, this prerequisite was redundant since *all* new cars bought in 2009 were Euro 4 equipped anyway.

[7] More precisely, program administration had to be covered by the budget which implied that the actual number of subsidized purchases was slightly lower than 2 million: $1,933,090$.

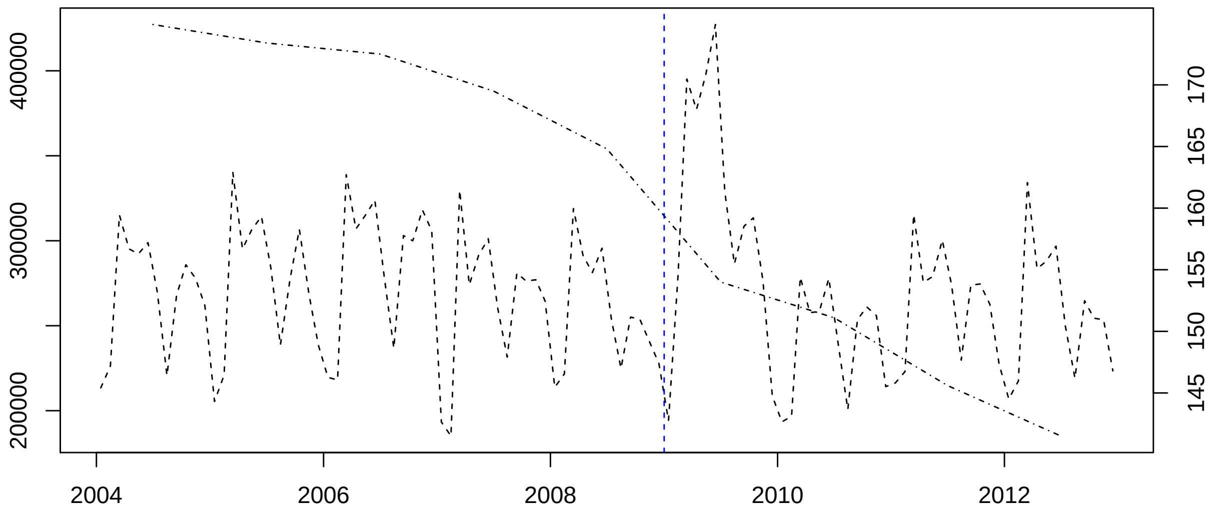
2.2 Data & Descriptive Evidence

For our upcoming analysis, we gathered rich data from EUROSTAT covering 12 and 11 European countries with and without a (recent) car scrappage program respectively. The former consist of Austria, France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Slovakia, Spain, and the UK. The latter are Belgium, Czech Republic, Denmark, Estonia, Finland, Hungary, Latvia, Lithuania, Poland, Slovenia, and Sweden. Our two outcomes of interest are the monthly figures of new passenger car registrations and the annual average CO2 emissions of newly registered passenger cars, with time series ranging from 2004 till 2012. We have chosen these variables for several reasons. First, we use figures for registrations of new cars instead of, e.g., sales, because the latter almost immediately translate into the former and data on new car registrations are collected by official agencies at monthly frequency. Only with data of this frequency, it becomes possible to disentangle the program's effects calculating how many of the subsidized vehicles would have been purchased anyway (windfall gains), how many had been shifted in time (pull-forward sales), and how many would not have happened in the absence of the intervention (on-top purchases). Second, concerning environmental pollution, we have decided to work with average CO2 emissions of new cars because these data allow to project overall CO2 emissions over the entire life time of the newly registered vehicles, separately for the different groups of buyers.[8] Finally, note that we are not trying to come up with a comprehensive environmental balance of the scrappage program. In that case, one would have to consider the environmental costs for retiring clunkers and the production of new cars, potential rebound-effects of newly purchased cars, etc. Instead, we concentrate on measuring the scrappage program's effects on the number of newly registered passenger cars and the exact CO2 pollution equivalent.

Figure 1 displays the development of our variables of interest from 2004 to 2012.[9] Overall, new car registrations were quite stable over the years 2004 till 2008, with about $270,000$

[8]This would not be possible for alternative measures like average tons of CO2 emissions of *overall* passenger cars. Moreover, such alternatives mostly feature an undesirable signal-to-noise ratio (as we are solely interested in differences induced by the scrappage program which affected only a rather small subgroup of cars) as well as a shorter time horizon.

[9]In graphics, we use the following convention: when plotting monthly series, values are attributed to the 15th of the corresponding month and then connected by an interpolating straight line. Analogously, quarterly values are attributed right to the middle of the respective quarter (February 15th, May 15th, etc.) and annual values are attributed to July 1st of the corresponding year.



Note: The dashed line exhibits monthly new passenger car registrations (absolute level, left scale), the dashed-dotted line shows annual CO2 emissions of newly registered passenger cars (g/km, right scale). The blue vertical line indicates the beginning of the German scrappage program.

Figure 1: Monthly Registrations and Annual CO2 Emissions of New Passenger Cars in Germany

registrations per month and very consistent seasonalities.[10] Nevertheless, one can see a slightly positive development from the beginning of 2004 until the end of 2006 and, from then on, a crisis-induced decline until the end of 2008.[11] In 2009, eventually, there was a striking jump up to more than $400,000$ monthly new car registrations. One year later, this number dropped back to a little less than its regular 2004–2008 level ($240,000$ vs. $270,000$) featuring a notable dip which, however, seems to be much smaller than the spike in 2009. Afterwards, once again, there is a clear positive development of new passenger car registrations until the end of 2012. From this descriptive evidence, though, it is impossible to infer how many of the subsidized vehicles were pulled forward from 2010—or even later periods. Moreover, if we consider the economic crisis in 2007/2008, a comparison between 2009 and the overall 2004–2008 level (or even 2008 alone) might be misleading since 2007 and 2008 were already depressed by the economic downturn. Besides the previous discussion one has to take into account that there

[10]For our later analysis, we eliminate the seasonal pattern by accumulating the values of the respective last 12 months and relating to population sizes. More precisely, we construct our first dependent variable as a smoothed monthly time series of new passenger car registrations in per capita percentage points, i.e., covering 108 observation points per country.

[11]See also Table 2 in the appendix for annual numbers.

might be many factors that could impact the number of new passenger car registrations before and after the treatment, calling for a suitable strategy to calculate counterfactual values.

Our second dependent variable, average CO2 emissions of newly registered passenger cars, is also shown in Figure 1. In the first post-intervention period, CO2 emissions decline considerably—by almost 11 grams per kilometer.[12] This is by far the biggest drop throughout our observation period with a yearly emission decline of about 2.5 g/km on average over the first four periods. In the following post-treatment periods, till the end of our observation frame, emissions further fall, but rather slowly. On the one hand, this decline is distinctly smaller compared to 2008/2009, on the other hand, on average, larger than the corresponding decline over the pre-treatment periods.

At this point, we can just hypothesize that potential on top-sales should trigger CO2 emissions per kilometer to fall. Supposedly, this group predominantly purchased cars from the very bottom price-segment featuring low emission levels. Possible pull-forward sales are more complicated to interpret, however. Such purchases, compared to regular sales, presumably refer to small and ecofriendly vehicles. Moreover, due to the design of the premium, it is not likely that a respective old car was worth more than €2,500. Because of this, it is also not likely that people from that group upgraded from a low-value car to an expensive less eco-friendly one. Contrarily, in absence of the policy intervention, such purchases would have happened at a later point in time where, conceivably, manufacturers in general develop vehicles to be more ecofriendly. Hence, it is important to realize that the scrappage program may also have influenced average CO2 emissions of newly registered cars in 2010 or later years. Additionally, even though not upgrading to an expensive car, people buying in 2009 instead of a later period might still have used the subsidy to upgrade a bit compared to their default scenario. In sum, it seems reasonable to assume that the emissions of cars whose acquisition had been pulled forward were a little higher than those of on-top sales. Finally, potential buyers profiting from winfall gains have probably purchased vehicles with the largest CO2 emissions as they would have bought a new car even if no scrappage program had been implemented. These people benefited from an extra €2,500 check which might have been used as an "upgrade" to, e.g., add horsepower, an air conditioning system, or the like.

[12]Exact annual CO2 emissions numbers can be found in Table 2 in the appendix.

Since we cannot know a priori how the two million subsidized vehicles are composed of windfall gains, pull-forward effects, and on-top sales respectively, we must let the data speak on which of the emission effects is dominating (at what post-treatment period) and how this translates into specific pollution numbers caused by the policy intervention. Certainly, again, a descriptive check is not sufficient, necessitating an adequate method of inference. For this objective, we have gathered additional EUROSTAT data for ten covariates, all running from 2004 till 2008: seven variables broadly reflect the respective country's economy, the vehicle market, and the environmental periphery: annual GDP per capita (quarterly data); the unemployment rate (annual data); two harmonized consumer price indices (Cars and Energy, monthly data); the share of passenger cars on overall transportation (annual data); CO2 emissions per inhabitant (annual data); and environmental tax revenues from the passenger transportation sector (percentage of GDP, annual data). Three more covariates deliver individual and household specific indicators: net earnings (PPS; annual data); annual consumption expenditures per capita (quarterly data); and pensions per capita (annual data). For an overview and summary statistics of our entire data set, see Table 3 in the appendix.

3 Methodology

3.1 A First Approach

To answer our research question at hand, estimating the influence of the German scrappage program on new vehicle registrations and on corresponding CO2 emissions, we can think about a variety of different identification strategies. One of the most common designs used in similar settings is a Difference-in-Differences (Diff-in-Diff) approach, as in, e.g., Li et al. (2013), who use Canada as the control group when evaluating the U.S. program. In our case, though, France or the UK, the two European countries economically most similar to Germany, cannot be used as control groups, as these countries also implemented scrappage programs to fight the economic crisis. Alternatively, we have eleven European non-scrapping countries in our data pool which may serve as valid control groups. However, the luxury of being able to choose from many potential control units comes at the difficulty of finding a systematic way to pick the most appropriate one or even the most appropriate combination of controls. In Table 1, we show descriptive car registration and pollution Diff-in-Diff results for Germany against all possible control countries in our data set, using the years 2008 and 2009 as our pre-treatment and post-treatment periods respectively. We can see that registration Diff-in-Diff results vary from 0.89 per capita percentage points for Poland to 2.06 for Estonia which translate into absolute car levels of $726,170$ and $1,683,157$ respectively. Corresponding results for CO2 emissions vary even stronger, ranging from -1.4 g/km for Sweden to -11.9 for the Czech Republic. Even countries neighboring Germany, such as Belgium, Czech Republic, Denmark, and Poland, show no similar pattern, i.e. behave very heterogeneously, ranging from 0.89 to 1.58 and from -3.5 to -11.9 regarding vehicle registrations and CO2 emissions respectively. Taking an average over all potential counterfactual units, we see that Germany seems to benefit from the scrappage program by 1.50 per capita percentage points or about 1.2 million new car registrations within the year 2009. What is more, also the German environment seems to benefit from the policy intervention. Average CO2 emissions decline by 6.48 grams per kilometer which is an impressive number. These figures, though, are not nearly sufficient since we do not want to simply use the entire pool of available control units and naively attribute equal weights across units to measure the effects of interest. Moreover, by just comparing two years we cannot evaluate the exact duration of the program's impacts or potential pull-forward effects. Apart from that critique, this simple descriptive evidence makes it very clear that, depending on the counterfactual of choice, we can expect our results to vary considerably. Put differently, the results' dependence on the country we choose to compare Germany with indicates that the conditions for safely applying simple Diff-in-Diff methods are probably not met.[13] Findings based on this method of inference, hence, might be severely biased.

Subsequently, we therefore employ an alternative, but still very related identification strategy which, in contrast to Diff-in-Diff, still delivers reliable results when the parallel trend assumption is violated or when unobserved confounders vary with time. *Synthetic control methods* are based on the comparison of outcomes between units representing the case of interest, Germany as *affected* by the policy intervention, and otherwise similar but *unaffected* countries. In this design, several comparison units are intended to reproduce an accurate counterfactual of Germany in absence of the scrappage program. An algorithm-derived combination

[13] One identifying assumption in such a setting that may not be met is that treatment and control group follow the same trend in absence of the treatment, i.e. the scrapping program.

Table 1: Registration and Emission Difference-in-Difference Results for Germany against all Non-Scrapping Countries: 2008/2009

Country	Per Capita (%)	Absolute	CO2 (g/km)
Belgium	1.45	1,187,623	-5.10
Czech Republic	1.02	835,671	-11.90
Denmark	1.58	1,290,643	-3.50
Estonia	2.06	1,683,157	-3.70
Finland	1.79	1,466,028	-4.90
Hungary	1.81	1,483,821	-10.80
Latvia	1.60	1,306,920	-7.10
Lithuania	1.36	1,111,613	-6.70
Poland	0.89	726,170	-9.30
Slovenia	1.65	1,351,899	-6.90
Sweden	1.34	1,096,357	-1.40
Average	1.50	1,230,900	-6.48

Note: Difference-in-Differences results for new passenger car registrations in per capita percentage points and corresponding absolute levels, as well as for CO2 emissions in gram per kilometer.

of precisely weighted comparison countries is supposed to better depict the characteristics of Germany than either any single comparison country alone or an equally weighted combination of all or several available control countries. Using an appropriate set of economic predictors, SCM select the comparison unit as the optimially weighted average of such comparison countries that best resemble the characteristics of Germany.

3.2 Multivariate SCM using Time Series

For calculating synthetic controls we employ the multivariate synthetic control method using time series (MSCM-T). This method generalizes and extends the SCM approach of Abadie and Gardeazabal (2003) and Abadie et al. (2010) in two respects: first, it allows to use entire time series of the economic predictors, while the original approach is restricted to using only their means.[14] By exploiting the economic predictors' variation over time, we efficiently use all our data in order to find the best counterfactual for Germany. Second, MSCM-T not only considers more than one variable of interest, but it does so *simultaneously* to take account of possible interdependencies between these.[15] In the case at hand, MSCM-T provides a counterfactual

[14]In the context of SCM, these economic predictors are also called "pre-intervention" or "pre-treatment characteristics", "independent variables", or "covariates".

[15]The two generalizations introduced here are independent of each other: one might, depending on the application, synthesize with respect to only one variable of interest using time series data of the economic

Germany that mimics actual Germany with respect to both the number of new car registrations and the average CO2 emissions per km of newly registered cars exploiting variation over time of several economic predictors.

Regarding our notation, a first complication arises from the fact that we have *monthly* data for the number of vehicle registrations while we have *annual* data for CO2 emissions. We therefore use the following notation: we denote by $Y_{l,m,j}$ the value of the l-th variable of interest, with $l = 1$ denoting the number of new car registrations and $l = 2$ the amount of CO2 emissions, $m = 1, \ldots, M_l$ running over the M_l observations of variable l, and $j = 1, \ldots, J + 1$ (with $J = 11$) determining the country we are looking at: $j = 1$ denotes Germany, $j = 2$ is Belgium, and $j = 12$ stands for Sweden.[16]

In order to differentiate between pre- and post-intervention observations, we denote by M_l^{pre} the number of pre-intervention observations, such that $Y_{l,1,j}, \ldots, Y_{l,M_l^{\text{pre}},j}$ are observed prior to the program while $Y_{l,M_l^{\text{pre}}+1,j}, \ldots, Y_{l,M_l,j}$ come only after the intervention. As stated previously, the synthetic control method aims at producing an appropriate counterfactual of Germany which describes how our variables of interest would have developed if the German government had not introduced a scrappage program. In other words, the ultimate aim of MSCM-T is to come up with approximations $\widehat{Y}_{l,M_l^{\text{pre}}+1,j}, \ldots, \widehat{Y}_{l,M_l,j}$ of $\widetilde{Y}_{l,M_l^{\text{pre}}+1,j}, \ldots, \widetilde{Y}_{l,M_l,j}$, with the latter denoting the counterfactual values one would have observed if there had been no scrappage program. The true but unknown effect of the intervention on the m-th observation of variable l, given by

$$\text{Eff}_{l,m} := Y_{l,m,1} - \widetilde{Y}_{l,m,1} \tag{1}$$

for $m > M_l^{\text{pre}}$, is then approximated by $\widehat{\text{Eff}}_{l,m} := Y_{l,m,1} - \widehat{Y}_{l,m,1}$.

For determining the synthetic control, non-negative weights $w_2, \ldots, w_{J+1}$, summing up to unity, are used which are collected in a J-dimensional vector $W = (w_2, \ldots, w_{J+1})'$, such that w_2 denotes the weight for Belgium and w_{12} denotes the weight for Sweden. Given such weights W, the dependent variables for Germany, $Y_{l,m,1}$, are approximated by $\widehat{Y}_{l,m,1}(W) := \sum_{j=2}^{J+1} w_j Y_{l,m,j}$.

predictors, i.e. use an SCM-T approach; or one might consider synthesizing with respect to several variables of interest, relying only on means of the covariates, i.e. employ an MSCM approach.

[16] As is standard in the literature on SCM, we also use the terms “donors”, “donor units”, and ”control units” for the countries that are used to synthesize Germany. Taken together, all those control units make up the so called “donor pool”. For standard assumptions concerning the donor pool see Abadie et al. (2014). Analogously, we also call Germany the “treated unit”.

For $m \leq M_l^{\text{pre}}$, the approximation $\widehat{Y}_{l,m,1}(W)$ can be compared to $Y_{l,m,1}$, with

$$e_{l,m}(W) := Y_{l,m,1} - \widehat{Y}_{l,m,1}(W) \tag{2}$$

denoting the pre-intervention approximation error w.r.t. the m-th observation of the l-th variable of interest. For $m > M_l^{\text{pre}}$, the difference between actual and synthetic post-treatment values,

$$\widehat{\text{Eff}}_{l,m}(W) := Y_{l,m,1} - \widehat{Y}_{l,m,1}(W) \tag{3}$$

is an estimate of $\text{Eff}_{l,m}$, the treatment effect on the m-th value of variable l defined in (1).[17] It is easy to see that $\widehat{\text{Eff}}_{l,m}(W)$ can be decomposed as $\widehat{\text{Eff}}_{l,m}(W) = \text{Eff}_{l,m} + \left(\widetilde{Y}_{l,m,1} - \widehat{Y}_{l,m,1}(W)\right)$, i.e., as the sum of the true but unknown treatment effect plus an approximation error depending on the weights W. As stated above, the approximation aims at $\widehat{Y}_{l,m,1}(W)$ for $m > M_l^{\text{pre}}$ being close to the counterfactual values $\widetilde{Y}_{l,m,1}$. One therefore would like to choose the weights W such that the approximation error $\widetilde{Y}_{l,m,1} - \widehat{Y}_{l,m,1}(W)$ is minimized. Unfortunately, this is not operational, as the counterfactual values $\widetilde{Y}_{l,m,1}$ are, of course, unknown. As Abadie et al. (2010) show, it is possible to get unbiased estimates of the treatment effect if two other goals are pursued instead: the *first* goal is that the synthetic control should be a good approximation to Germany, at least *pre*-treatment, i.e., the pre-treatment approximation error $e_{l,m}(W)$ defined in (2) should be close to zero. This can be measured by the mean squared error w.r.t. variable l, $\text{MSE}_{Y,l}(W) := \frac{1}{M_l^{\text{pre}}} \sum_{m=1}^{M_l^{\text{pre}}} e_{l,m}(W)^2$, and condensed into the pre-treatment root mean squared error w.r.t. the variables of interest,

$$\Delta_Y(W) := \sqrt{\text{MSE}_{Y,1}(W) + \text{MSE}_{Y,2}(W)} = \sqrt{\sum_{l=1}^{2} \frac{1}{M_l^{\text{pre}}} \sum_{m=1}^{M_l^{\text{pre}}} \left(Y_{l,m,1} - \sum_{j=2}^{J+1} Y_{l,m,j} w_j\right)^2}. \tag{4}$$

Note that, in order to grant both dependent variables the same importance, the values of the dependent variables are rescaled to unit variance prior to any practical calculations.

There is, however, a *second* important goal when constructing a synthetic control: in order for the counterfactual to be adequate *post*-treatment, too, the synthetic control should

[17] To estimate the treatment effect, one may also use $\widehat{\text{Eff}}_{l,m} - e_{l,M_l^{\text{pre}}}(W) = \left(Y_{l,m,1} - \widehat{Y}_{l,m,1}(W)\right) - \left(Y_{l,M_l^{\text{pre}},1} - \widehat{Y}_{l,M_l^{\text{pre}},1}(W)\right)$, in analogy to the Diff-in-Diff approach.

also provide a good approximation to Germany with respect to several economic variables which have predictive power for explaining the number of new vehicle registrations and CO2 emissions of newly registered cars in Germany. To this end, we consider $K = 10$ economic predictors: consumption expenditures (CE, $k = 1$), etc., and unemployment rate (UE, $k = 10$), see also Table 3. As these variables are also available for different observation frequencies, we denote their values by $X_{k,n,j}$, with $k = 1, \ldots, K$ running over all economic predictors, $n = 1, \ldots, N_k$ running over the *pre*[18]-treatment observation periods of economic predictor k, and $j = 1, \ldots, J+1$ running over all countries: e.g., $X_{1,2,2}$ denotes consumption expenditures in the second quarter of 2004 in Belgium, while $X_{4,13,1}$ stands for the harmonized index of consumer prices (cars) in January, 2006, in Germany.

In complete analogy to the approximation of the target variables, a vector W of weights for the control units induces an approximation for Germany's economic predictors: $X_{k,n,1}$ is approximated by $\widehat{X}_{k,n,1}(W) := \sum_{j=2}^{J+1} X_{k,n,j} w_j$, and we denote the difference between the treated unit's and the synthetic control's n-th value of the k-th economic predictor by

$$D_{k,n}(W) := X_{k,n,1} - \widehat{X}_{k,n,1}(W) = X_{k,n,1} - \sum_{j=2}^{J+1} X_{k,n,j} w_j.$$

For every $k = 1, \ldots, K$, these discrepancies can be transformed into a single number,

$$\mathrm{MSE}_{X,k}(W) := \frac{1}{N_k} \sum_{n=1}^{N_k} D_{k,n}(W)^2 = \frac{1}{N_k} \sum_{n=1}^{N_k} \left(X_{k,n,1} - \sum_{j=2}^{J+1} X_{k,n,j} w_j \right)^2,$$

the mean squared deviation between the treated and the synthetic control unit's values of covariate k. Finally, these discrepancies are weighted by some positive numbers $v_1, \ldots, v_k > 0$, to construct an overall measure of fit for the economic predictors:

$$\Delta_X(v_1, \ldots, v_K, W) := \sqrt{\sum_{k=1}^{K} v_k \, \mathrm{MSE}_{X,k}(W)} = \sqrt{\sum_{k=1}^{K} v_k \frac{1}{N_k} \sum_{n=1}^{N_k} \left(X_{k,n,1} - \sum_{j=2}^{J+1} X_{k,n,j} w_j \right)^2} \tag{5}$$

The weights $v_1, \ldots, v_K$, which are determined endogenously, allow to practically eliminate economic predictors that have no predictive power for the variables of interest: the correspond-

[18]Note that only *pre*-treatment values of the economic predictors are used. Thus, for MSCM-T to work, it is not necessary that the *post*-treatment values of the economic predictors are unaffected by the intervention.

ing v weights will be almost zero, annihilating these spurious explanatory variables.

In Abadie et al. (2010), it is shown that for W with $\Delta_X(v_1, \ldots, v_K, W) = 0 = \Delta_Y(W)$, the effect estimator defined in (3) is (asymptotically) unbiased in situations where a Diff-in-Diff approach would yield biased estimates. More generally, the smaller the errors $\Delta_X(v_1, \ldots, v_K, W)$ and $\Delta_Y(W)$, the smaller a potential bias of the effect estimator (3) will be. In Section A in the appendix, we formulate the corresponding results for our adopted setting: even with multiple variables of interest and a much more general data generating process than the one considered in Abadie et al. (2010), allowing for all variables (outcomes of interest *and* economic predictors) to depend on lags of each other in a vector autoregressive process, the effect estimator (3) is unbiased. If, additionally, unobserved confounders are present, even if not fixed over time, the estimator still is asymptotically unbiased.

As stated above, for constructing an appropriate synthetic control, it is important that the pre-treatment errors of both, the dependent variables, $\Delta_Y(W)$, and the economic predictors, $\Delta_X(v_1, \ldots, v_K, W)$, are small. To achieve this, we follow the literature and use the following procedure:

- first, we define a function W^* that maps predictor weights $v_1, \ldots, v_K$ onto those weights for the control units that minimize the approximation error of the economic predictors:

$$W^*(v_1, \ldots, v_K) := \operatorname*{argmin}_W \Delta_X(v_1, \ldots, v_K, W),$$

  with $\Delta_X(v_1, \ldots, v_K, W)$ as defined in (5),

- second, we use the function W^* to define a function $\Delta^*_Y(v_1, \ldots, v_K)$ by

$$\Delta^*_Y(v_1, \ldots, v_K) := \Delta_Y(W^*(v_1, \ldots, v_K)), \tag{6}$$

  with Δ_Y as defined in (4),

- for given predictor weights $v_1, \ldots, v_K$, the control unit weights $W^*(v_1, \ldots, v_K)$ are those that minimize the approximation error of the economic predictors, while $\Delta^*_Y(v_1, \ldots, v_K)$ is the corresponding approximation error w.r.t. the variables of interest: to determine $v_1, \ldots, v_K$ optimally, (6) is minimized w.r.t. $v_1, \ldots, v_K$, delivering optimal predictor weights $v_1^*, \ldots, v_K^*$ as well as corresponding weights for the control units, $W^*(v_1^*, \ldots, v_K^*)$.

Note that, regarding the minimization of (6), we can—without loss of generality—assume that $\sum_{k=1}^{K} v_k = 1$, as obviously $W^*(\alpha v_1, \ldots, \alpha v_K) = W^*(v_1, \ldots, v_K)$ for all $\alpha > 0$. Even with this restriction, however, there is not necessarily a unique optimizer of (6): it may be the case that different optimal weights $v_1^*, \ldots, v_K^*$ and $\widetilde{v}_1^*, \ldots, \widetilde{v}_K^*$ for the economic predictors lead to exactly the same weights for the donor units: $W^*(v_1^*, \ldots, v_K^*) = W^*(\widetilde{v}_1^*, \ldots, \widetilde{v}_K^*)$. Therefore, one must be very careful when interpreting $v_1^*, \ldots, v_K^*$. Furthermore, note that in our practical application, the values of all economic predictors are rescaled such that, for each predictor, the variance of all data belonging to that predictor equals unity. This is done in order to make comparisons between weights for different economic predictors meaningful.

For our calculations, we have implemented an algorithm to determine $v_1^*, \ldots, v_K^*$ and $W^*(v_1^*, \ldots, v_K^*)$ using the software R[19]. This algorithm works as follows: starting with randomly drawn $v_1^0, \ldots, v_K^0$, an optimizer supplied by R iteratively improves (6) by considering new choices for $v_1, \ldots, v_K$ until a local optimum is found. In order to avoid ending up with a local optimum different from the global one, we reiterate this process at least a few thousand times, choosing the result with the best pre-treatment fit.[20]

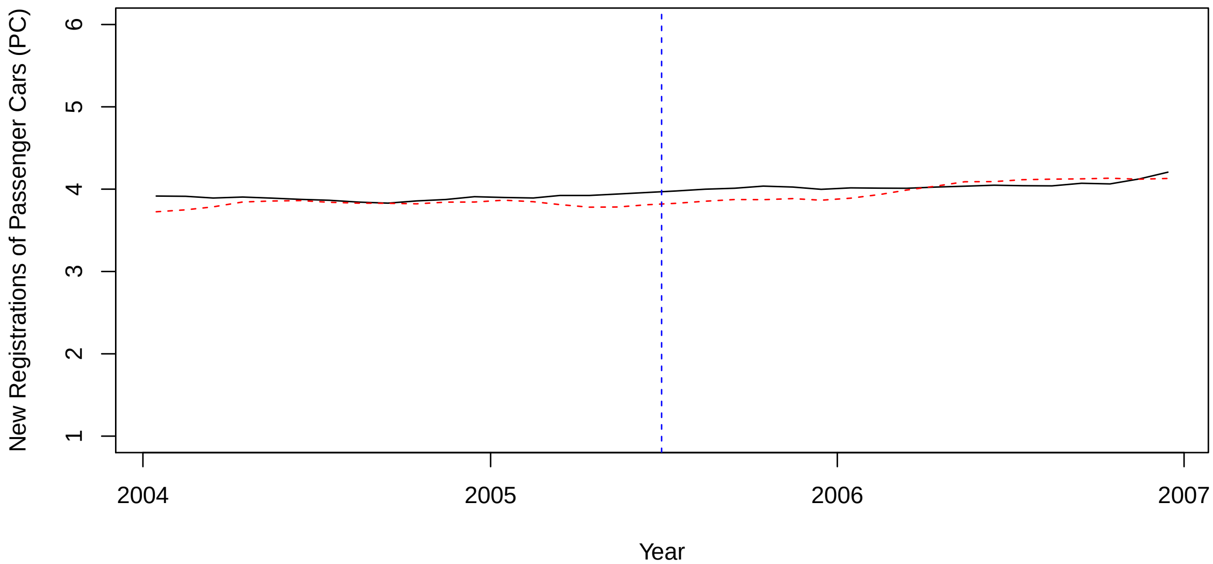
4 Evaluating the German Scrappage Program

4.1 Specification Check

Before running our model and evaluating potential treatment effects, we want to make sure to use MSCM-T with a specification allowing us to adequately build a synthetic Germany out of our donor pool consisting of 11 non-scrapping countries. Therefore, we start by picking a time frame as far as possible from our treatment cutoff of interest (January, 2009) but still long enough to be able to validate our synthetic counterfactual. We choose the first three years of our sample, i.e., the 36 months between January, 2004 and December, 2006, and divide this time span into two equal parts: a so called “training period” (first 18 months; January, 2004 till June, 2005) and a so called “cross-validation” period (next 18 months; July, 2005 till

[19] R Core Team (2014)

[20] For numerical reasons, we employed a lower bound of $1e-08$ for $v_1, \ldots, v_K$.



Note: Germany and Synthetic Germany are represented by a black solid and red dashed line respectively. The blue vertical line separates the 18 months training period from the 18 months cross-validation period.

Figure 2: Trends in Per Capita (%) New Passenger Car Registrations - Specification Check: Germany vs. Synthetic Germany over a Training Period and a Cross-Validation Period

December, 2006).[21]

Within the training period, we apply MSCM-T to produce the weights for the counterfactuals which then are compared against the actual dependent variables within the cross-validation period. This is done by checking the out-of-sample error, i.e., the relative root mean squared prediction error occurring within the validation period. Due to the cross-validation, we can check whether our combination of economic predictors (and donor pool countries) produces an adequately small error, i.e., a counterfactual that accurately mirrors the actual timeline within the validation period. If we are convinced they do, we can go on and estimate the treatment effect of the German scrappage program on new vehicle registrations and CO2 emissions.

Within the chosen cross-validation time frame—July, 2005 till December, 2006—MSCM-T produces an extremely good fit which becomes obvious when looking at Figure 2. Our specification yields a counterfactual for passenger car registrations that, with an relative out-of-sample root mean squared prediction error of about 2.57, nicely resembles the actual car

[21] Note that we use the terms "training period" and "cross-validation period" in a different way than Abadie et al. (2014).

registrations pattern during the 18 months of the validation period. Apparently, there is almost no difference at all between those two timelines. The corresponding out-of-sample error for the CO2 cross-validation comes to 1.72, implying an even better goodness of fit. Hence, based on this time frame's validation check, we are convinced utilizing a combination of economic predictors that will produce reliable counterfactuals in order to estimate unbiased treatment effects of the German scrappage scheme.[22]

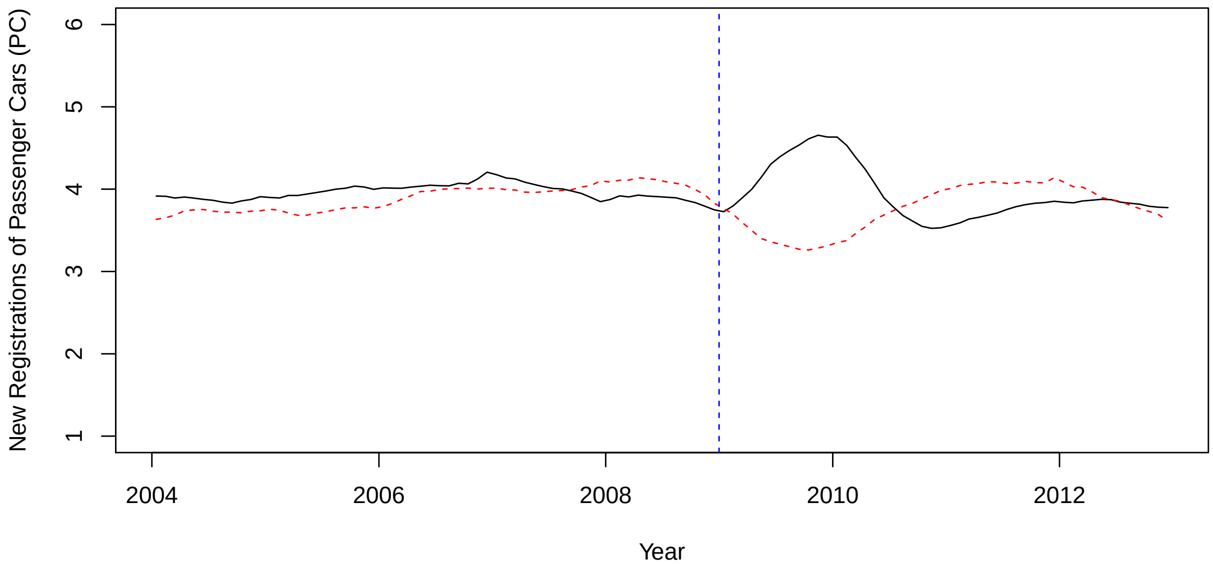
4.2 Effects on Sales

Figure 3 displays the percentage per capita car registrations timelines of Germany and its synthetic counterfactual for the 108 months from the beginning of 2004 till the end of 2012. The synthetic Germany adequately reproduces actual car registrations during the pre-program period demonstrating that there exists a combination of non-scrapping countries that replicates Germany's registration timeline before the policy intervention. To be more specific, Germany is synthesized by Belgium, Finland, and Sweden which are attributed a W-weight of about 48.84%, 1.85%, and 49.31% respectively. This MSCM-T optimization result is attained by utilizing a valid combination of v-weights which can be read as follows: consumption expenditures (ca. 4.65%); per Capita CO2 emissions (ca. 4.65%); per capita GDP (ca. 4.65%); net earnings (ca. 26.81%); consumer price index cars (ca. 4.65%); consumer price index energy (ca. 4.65%); pensions (ca. 4.65%); transportation tax revenues (ca. 5.11%); share of passenger cars (ca. 35.52%); and the unemployment rate (ca. 4.65%).[23]

The close pre-treatment fit produced by this weighted mix of countries cannot only be seen in Figure 3, but also in Table 4 in the appendix. Therein, actual numbers of Germany's pre-program characteristics are compared to those of the synthetic Germany, and also to those of an overall donor pool average. It becomes clear that the synthetic counterfactual provides a much better comparison for Germany than the average of our sample of other non-scrapping countries. Exemplarily, Figure 9 in the appendix shows the fit for the energy consumer price index. This economic predictor, which has been attributed a weight to of about 4.65%, shows

[22]Moreover, when looking at a 24/36, a 30/30, and a 48/12 months design and re-running our cross-validation check, we find that in each and every of those attempts, our specification clearly works fine. All of those attempts outperform respective baseline models where we construct the counterfactual by simply using a single average per economic predictor instead of using its entire time series variation, i.e. an MSCM approach.

[23]Due to rounding imprecisions these figures sum up to slightly less than 100%.



Note: Germany and Synthetic Germany are represented by a black solid and a red dashed line respectively. The blue vertical line indicates the begin of the German scrappage program.

Figure 3: Trends in Per Capita (%) New Passenger Car Registrations: Germany vs. Synthetic Germany

that our synthesizing by MSCM-T works perfectly fine.

Before we continue converting this graphical movement into actual numbers, we want to test the significance of our results. Using a so called placebo study, we re-assign the treatment to a comparison unit. In other words, the actually treated unit, Germany, moves into the donor pool while one of the control units is synthesized instead. Applying this idea to each country from the original donor pool allows to compare the estimated effect of the German program on new vehicle registrations to the distribution of placebo effects obtained for other countries. One can consider the effect of the program to be significant if the estimated impact for Germany is greater relative to the outcome distribution of adequately synthesized placebo units. More precisely, by moving Germany into the donor pool we can test the null hypothesis of whether there is no difference in car registrations between Germany and the bulk of non-scrapping countries. In so doing, we are able to construct a corresponding p-value by estimating placebo effects for each control unit and then calculating the fraction of such effects greater or equal to the effect estimated for the unit of interest, Germany.[24] Figure 4 shows the results

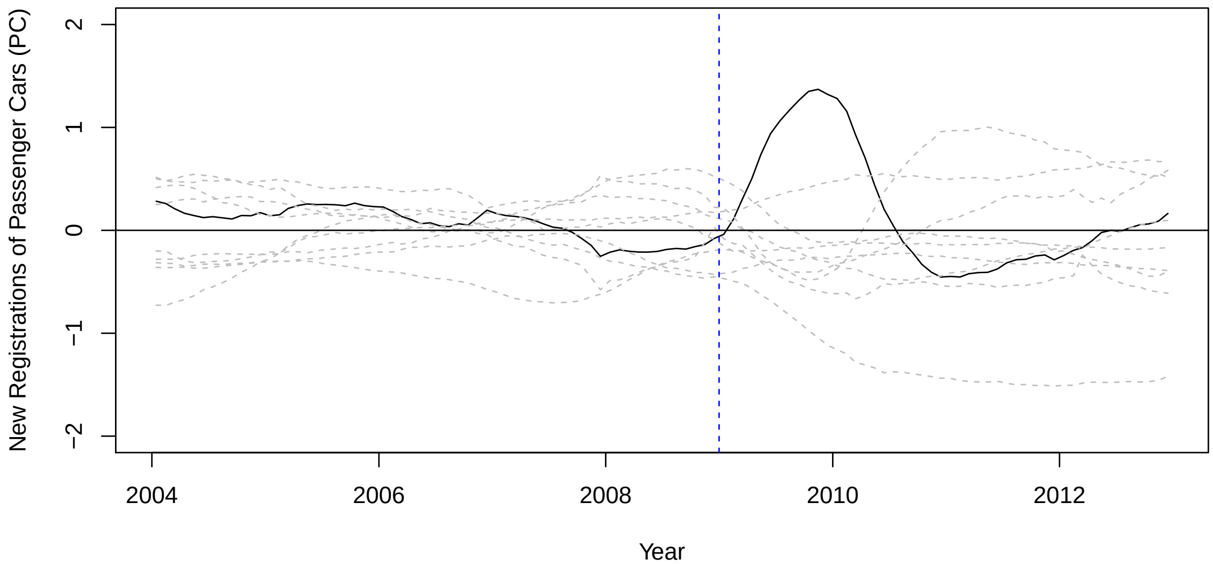
[24]See also Abadie et al. (2014).

of this placebo study where the effect of a respective country is given by the gap between its actual and synthetic timelines.[25] Germany's pre-treatment error is completely covered by a confidence band consisting of nine placebo countries' pre-program fits. After the treatment-cutoff, though, Germany's impact on new vehicle registrations clearly stands out of the bulk of control units, featuring an evidently significant treatment effect. Figure 3 reveals that this positive impact is amplified by the downward movement of the counterfactual trajectory during the treatment period. This effect is remarkable not only because it simply stands out of the mass of placebo outcomes but, above all, how it is shaped. There is a clear and steep rise of new vehicle registrations, notably different from some slowly increasing trends within the control pool. In fact, most of the control units just move slightly below the zero-line not featuring any vertical movement at all. Hence, our confidence that Germany's sizable synthetic control estimate actually reflects the effect of the scrappage stimulus is strengthened since no similar or larger estimates arose when the treatment was artificially re-assigned to units not directly exposed to the intervention. More specifically, under the null hypothesis of no differences in car registrations, we can expect (such) an effect to appear in only 1/10 of all cases.

Being positive about the significance of our new vehicle registrations results, we now can be more precise and translate those graphs into actual figures. Since our pre-treatment fit is not perfect, i.e., with an error very close to but not exactly zero, we use the last twelve months before the start of the program as our pre-treatment period and set its monthly differences against all corresponding post-treatment monthly differences. With this differential strategy, we are able to compute unbiased net-treatment effects over time which are displayed in Figure 5. From the beginning of the program on, cumulative new car registrations in Germany constantly rise, reaching more than one million already in August, 2009. Still, they grow even further arriving at a maximum of about 1.45 per capita percentage points respectively in late 2009.[26] After plateauing above the one million mark over the next couple of months, the effect slowly decelerates, at the turn of the year 2010/2011 reaching a level of about 900,000 around which it then oscillates.

[25] We excluded Belgium and Poland due to inadequately large pre-treatment errors (of more than three times the German error).

[26] Looking at December, 2009, we are able to compare our MSCM-T findings to the Diff-in-Diff results from Subsection 3.1. For the 2008-2009 period, we estimate an effect of about 1.40 per capita percentage points, while the simple Diff-in-Diff average from Table 1 was 1.50 and, hence, quite a bit larger.



Note: The difference between Germany and Synthetic Germany is represented by a black solid line. The dashed grey lines represent analogous differences for nine placebo countries without a scrappage program. Belgium and Poland have been excluded.

Figure 4: Per Capita (%) New Passenger Car Registrations Gaps for Germany and Placebo Countries

While those numbers appear to be impressive, we do not yet know how much of those program-induced new car sales actually happened on top of regular purchases or were simply pulled from future time periods respectively. Figure 3 suggest that both types of purchases might have been present. Quite plausibly, we expect that some portion of program-induced sales were indeed pulled forward from the future, in particular due to the decline of new car registrations somewhere in 2010. On the other hand, after this downturn, registrations seem to level off and do not remain below the counterfactual trajectory, so there is also quite some scope for program-induced on-top car sales. In a next step, we therefore formally disentangle those effects, building a simple model of pull-forward and on-top sales and using our MSCM-T results to estimate specific parameters.

Disentangling of Sales Effects

If a scrappage program induces additional car purchases, i.e. on-top sales, but no pull-forward effects, then these additional sales will happen during the life time of the program only. As it might take a little time for sales to translate into registrations, we allow for on-top sales leading to a cumulative registration effect first equaling zero until some time point t_0 which denotes the delay between the program start and registrations of subsidized cars. After t_0, cumulative effects on registrations will increase until the program is exhausted at time t_1. Thereafter, they will stay constant. The most simple model for the increase during the program's life time would be a linear increase, such that the cumulative effect of the program will evolve over time as a multiple of

$$y_t^{(o)} := \begin{cases} 0 & t_0 \leq t \\ t - t_0 & t_0 \leq t \leq t_1 \\ t_1 - t_0 & t_1 \leq t \end{cases},$$

resulting in a timeline as given by the blue dotted curve in Figure 5.

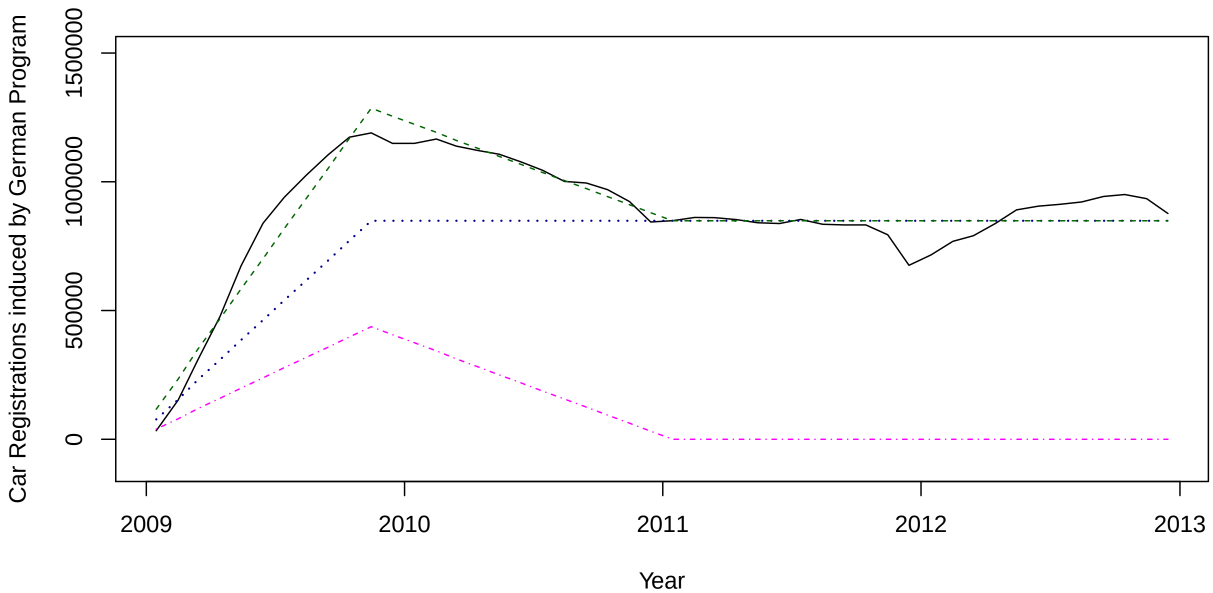
Now, let's assume that a scrappage program only induces pull-forward effects, but does not trigger on-top sales. In that case, sales that, in absence of the program, would have happened during some post-treatment period are shifted to the program's life time. Therefore, cumulative effects on registrations will be increasing from t_0 to t_1, but decreasing from t_1 to some time point t_2 denoting the end of the pull-forward horizon. At t_2, the pull-forward effect finishes and cumulative effects are zero from that point on. Again assuming linear curves, pull-forward effects would shift sales in time such that a multiple of

$$y_t^{(p)} := \begin{cases} 0 & t_0 \leq t \\ \frac{t_2 - t_1}{t_1 - t_0}(t - t_0) & t_0 \leq t \leq t_1 \\ t_2 - t & t_1 \leq t \leq t_2 \\ 0 & t_2 \leq t \end{cases}$$

prevails. The corresponding timeline is given by the magenta dashed-dotted curve in Figure 5.

If the two effects mix, i.e., in the presence of both additionally induced sales and pull-forward effects, a curve similar to the green dashed line in Figure 5 will emerge. In such a mixed case, one can read off the overall number of on-top sales by looking at the long-run value of the effect graph, while the total of pulled forward sales is given by the difference between the graph's maximal and long-run value. The cumulative effect y_t of the program at time t will be given by

$$y_t = \alpha\, y_t^{(o)} + \beta\, y_t^{(p)} + \varepsilon_t, \tag{7}$$



Note: The timeline of on-top sales is represented by a blue dotted line, the magenta dashed-dotted line shows pull-forward sales, while the green dashed line depicts the sum thereof and shows the fitted values for the regression model as in (7) ($t_0 = 3$, $t_1 = 11$, $t_2 = 30$). The cumulative difference between Germany and Synthetic Germany is represented by a black solid line.

Figure 5: Development of On-Top Sales and Pull-Forward Sales over Time

where $\alpha, \beta \geq 0$ are parameters and ε_t denotes noise. Given t_0, t_1, and t_2, (7) is a simple linear regression model that can be estimated using OLS.[27] Fixing t_1 at November 2009, since cumulative registrations then reach their maximum, we have run corresponding regressions for all sensible combinations of t_0, t_1, and t_2. Choosing the best result in terms of fit yields $t_0 = 0$, $t_1 = 11$, and $t_2 = 25$. Hence, the program shows an immediate effect in January, 2009, which remains in charge until November, 2009, while the pull-forward effect ends in January, 2011. The regression's coefficient of determination being 99.5%, the timeline estimated by the regression fits the "actual" one extremely well, see the green dashed and black solid lines in Figure 5. Additional sales are estimated to be approximately $77,100$ ($\widehat{\alpha} = 77,143.25$ with a standard error of $1,147.02$) for every month from January to November, resulting in ca. $850,000$ additionally registered cars, i.e. on-top sales, over these eleven months. For the pulled forward sales, we estimate that between December, 2009, and January, 2011, roughly $31,200$ registrations per month ($\widehat{\beta} = 31,222.82$ with a standard error of $2,000.21$) were shifted to the

[27] Estimating the model *with* an intercept, as is often standard when running OLS, does not alter our results. The intercept is estimated as $8,222.09$ with a standard error of $40,148.57$ being statistically insignificant.

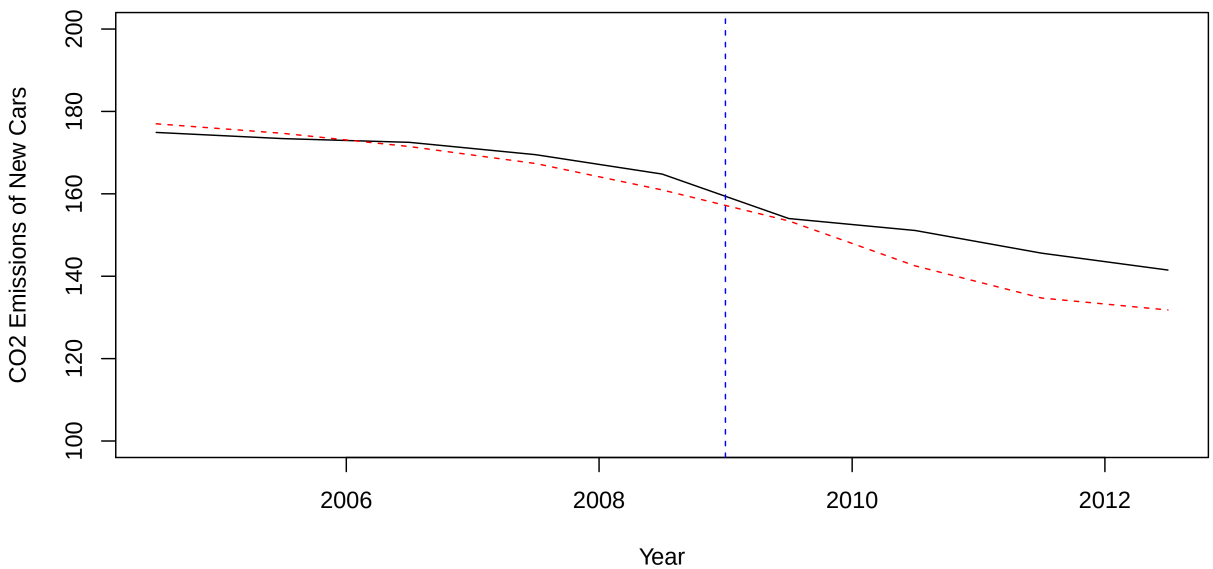
program period in order to benefit from the €2,500 subsidy, totaling approximately 440,000 sales that increased registrations during the program period. All in all, we estimate that the 1,285,695 registrations induced by the German scrappage program consist of 848,576 sales that would not have happened in absence of the subsidy, i.e. on-top purchases, and 437,120 sales that have been pulled forward in time. Conversely, knowing that there existed 1,933,090 subsidized cars in total, we infer that 647,395 subsidized purchases were simply windfall gains, i.e. would also have happened without the policy intervention.

While empirical evidence suggests that the U.S. scrappage program did not lead to long-lasting impacts on new vehicle sales, our results suggest that for the German case things are quite different. Mian and Sufi (2012), e.g., state that the U.S. program increased the number of vehicles purchased during its two months period but that this effect was completely reversed over the following ten months. This means that the U.S. program could not generate on-top sales at all but that 100% of program-induced purchases happened on cost of future years. Our findings, however, lead to the conclusion that in Germany less than 35% were pulled forward leaving almost one million newly registered cars as program-induced on-top sales. Extrapolating those program induced on-top purchases by using the median price for German cars implies a notable multiplier effect with respect to the original €5 billion budget—it more than triples.

4.3 Effects on the Environment

We start our discussion of the German scrappage program's impact on the environment by drawing on our disentangled results from Subsection 4.2. Because of quite some buyers pulling forward their purchase to benefit from the subsidy in 2009 (about 440,000), we expect the intervention to cause average CO2 emissions of newly registered cars *after 2009* being larger than they would have been without a scrappage program. Assuming the obvious, namely that the majority of such cars were small and ecofriendly,[28] emissions are supposed to grow since those program-induced vehicles are now "missing" due to the fact they were pulled into the program period. The timelines of new passenger cars' average CO2 emissions for Germany and

[28] This makes even more sense when thinking about the design of the scrappage program requiring to scrap off a "clunker", most likely being worth less than €2,500. Hence, subsidized buyers were typically not customers buying pricey cars and would usually not upgrade from a clunker to a new expensive car.

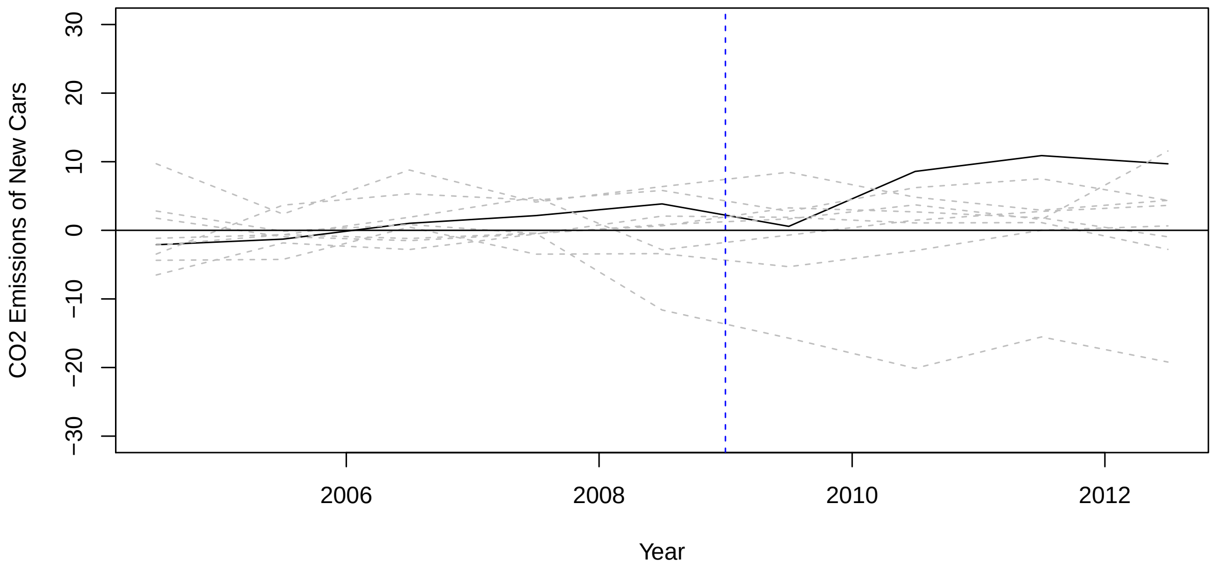


Note: Germany and Synthetic Germany are represented by a black solid and a red dashed line respectively. The blue vertical line indicates the begin of the German scrappage program.

Figure 6: Trends in CO2 Emissions (g/km) of New Passenger Cars: Germany vs. Synthetic Germany

its synthetic counterfactual, given in Figure 6, confirm this expectation: actual emissions after 2009 are considerably above their counterfactual equivalent. Furthermore, the corresponding difference is much larger than the pre-treatment approximation error which is indeed quite small, showing that the synthetic Germany adequately reproduces actual vehicle greenhouse gas emissions during the pre-program period. This can also be seen from Figure 7 which shows the gap between actual and counterfactual average CO2 emissions for Germany (solid line) as well as for placebo countries (dashed lines). The program's effect on newly registered cars' emissions *after 2009* is significantly positive, located well above the confidence band.[29] With respect to *2009 itself*, there are conflicting effects in progress: on the one hand, on-top sales (about $850,000$) as well as pull-forward sales (about $440,000$) of typically rather small and ecofriendly cars, happening in 2009 only because of the subsidy, lead to *smaller* emission levels as compared to a counterfactual scenario. On the other hand, buyers who even in case of no policy intervention would have bought a new car (about $650,000$ windfall gains) might have been incentivized by the subsidy to buy larger cars, thereby *increasing* actual emissions as

[29] In Figure 7, we have excluded all placebo countries with a pre-treatment approximation error larger than three times that of Germany (Belgium, Lithuania, and Sweden).



Note: The difference between Germany and Synthetic Germany is represented by a black solid line. The dashed grey lines represent analogous differences for eight placebo countries without a scrappage program. Belgium, Lithuania, and Sweden have been excluded.

Figure 7: CO2 Emissions (g/km) of New Passenger Cars Gaps for Germany and Placebo Countries

compared to their counterfactual equivalent. From Figures 6 and 7, we find that actual and counterfactual CO2 emissions in 2009 are almost identical, their difference lying at the very center of the band determined by the placebo countries' gaps.[30] We therefore conclude that the effects described above essentially offset each other, implying that the subsidy indeed lured buyers who profited from windfall gains into purchasing less ecofriendly cars.

In the following, we combine the disentangled sales findings from Subsection 4.2, relevant figures from the German Census Bureau, and, finally, our environmental MSCM-T estimates to formally disentangle environmental effects. In a last step, we compose such effects to a net sum of pollution caused by the German scrappage program.

Disentangling of Environmental Effects

To disentangle environmental effects of the German scrappage program, we first introduce some notation: c denotes average CO2 emissions of certain buyer groups; these are p for

[30]Comparing our MSCM-T findings for 2009 with the simple Diff-in-Diff average from Subsection 3.1, we observe a notable difference. While MSCM-T delivers an effect of around -3.27 g/km, the Diff-in-Diff average from Table 1 is about twice as large.

“pulled forward”, w for ”windfall gain”, o for ”on-top”, n for “non-subsidized”, and s for ”subsidized”. $\widehat{c}$ denotes corresponding CO2 quantities in absence of a scrappage program, i.e. counterfactual emissions delivered by MSCM-T. To evaluate the environmental effects of the intervention, we not only draw on actual figures being part of our data set and counterfactual numbers provided by MSCM-T, but also on the following quantities taken from IFEU (2009): average CO2 emissions of subsidized cars (142 g/km), average CO2 emissions of scrapped cars (200 g/km), average distance driven per year per passenger car ($10,000$ km), and the average life span of a passenger car (15.4 years).

From our data, we know that the average CO2 emission of the $3,794,418$ actual new car registrations in Germany in 2009 was 154 g/km, a weighted average of $1,933,090$ subsidized cars emitting 142 g/km and $1,861,328$ non-subsidized sales emitting significantly more CO2:

$$154 = \frac{1,861,328}{3,794,418}\, c_{\mathrm{n},2009} + \frac{1,933,090}{3,794,418}\, 142. \tag{8}$$

From this equation, we can back out $c_{\mathrm{n},2009} = 166.46$. Hence, subsidized and non-subsidized cars contributed 142 g/km and 166.46 g/km respectively, revealing that the scrappage program indeed shifted demand towards small and ecofriendly cars.

Trying to disentangle the subisdized cars’ emissions into the emissions of $437,120$ pulled forward sales, $848,576$ on-top sales, and $647,395$ windfall gains, we therefore have:

$$142 = \frac{647,395}{1,933,090}\, c_{\mathrm{w},2009} + \frac{437,120}{1,933,090}\, c_{\mathrm{p},2009} + \frac{848,576}{1,933,090}\, c_{\mathrm{o},2009}, \tag{9}$$

i.e., 142 is a weighted average of the unknown quantities $c_{\mathrm{w},2009}$, $c_{\mathrm{p},2009}$, and $c_{\mathrm{o},2009}$, for which it is plausible to assume that $c_{\mathrm{w},2009} \geq c_{\mathrm{p},2009} \geq c_{\mathrm{o},2009}$ as already outlined in Section 2.

In absence of a scrappage program, only the $1,861,328$ non-subsidized and the $647,395$ windfall gain sales would have happened in 2009, such that the counterfactual average CO2 emissions in 2009 of 157.27 g/km (estimated by MSCM-T) can be written as

$$157.27 = \frac{1,861,328}{1,861,328 + 647,395}\, c_{\mathrm{n},2009} + \frac{647,395}{1,861,328 + 647,395}\, \widehat{c}_{\mathrm{w},2009}, \tag{10}$$

from which, using $c_{\mathrm{n},2009} = 166.46$ from (8), we find $\widehat{c}_{\mathrm{w},2009} = 130.85$. Since we assumed

$c_{\mathrm{w},2009} \geq 142$, we conclude $\widehat{c}_{\mathrm{w},2009} < c_{\mathrm{w},2009}$, meaning that windfall gainers bought less ecofriendly cars than they would have in absence of a scrappage program. This makes sense considering the fact that such purchases would have been made anyways and, hence, people directly benefited from a €2,500 rebate usable as an upgrade.

With these figures at hand, we can estimate by how much CO2 emissions changed in 2009 due to the scrappage program affecting the behavior of the groups of windfall gains and pull-forward sales. For instance, if we assume that $c_{\mathrm{p},2009} = 142$, we find a *reduction* of CO2 emissions w.r.t. the group of pull-forward sales from $437,120 \cdot 10,000 \cdot 200 \cdot 10^{-6} = 874,239$ tons of CO2 to $437,120 \cdot 10,000 \cdot 142 \cdot 10^{-6} = 620,710$ tons of CO2, i.e. a reduction by $253,529$ tons of CO2. For the windfall gains, however, we find, analogously assuming $c_{\mathrm{w},2009} = 142$,[31] that the scrappage program *increased* CO2 emissions from $647,395 \cdot 10,000 \cdot 130.85 \cdot 10^{-6} = 847,095$ to $647,395 \cdot 10,000 \cdot 142 \cdot 10^{-6} = 919,301$, i.e. it induced a toxic windfall of $72,206$ tons of CO2. As a net effect over both groups, we then find that the German program *reduced* CO2 emissions in *2009* by $181,323$ tons. However, this reduction comes at the price of *increased* CO2 emissions in *later years*: for the group of windfall gains, the toxic windfall of $72,206$ tons of CO2 will happen every year over the life span of the new car, i.e. this annual toxic windfall will be realized over 15.4 years, resulting in an overall increase of CO2 emissions due to the group of windfall gains of $72,206 \cdot 15.4 = 1,111,970$ tons of CO2. This program-induced toxic windfall more than offsets the reduction from the group of pull-forward sales which totaled only $253,529$ tons of CO2. Moreover, if those buyers that pulled forward their purchase to 2009 also bought less ecofriendly cars than they would have done in absence of a scrappage program, then the policy invervention hurt the environment even more. We therefore now estimate the counterfactual average CO2 emissions of newly registered cars that those buyers would have purchased.

To this end, we utilize that, according to our data set, the total number of newly registered cars in 2010 in Germany was $2,887,275$, with average CO2 emissions of 151.1 g/km. In absence of the scrappage program, there would have been $12 \cdot 31,222.82 = 374,674$ additional sales that actually were pulled forward in time due to the scrappage premium. The counterfactual

[31] We will see later that these assumptions are indeed conservative.

CO2 emissions of 146.37 in 2010 as estimated by MSCM-T can therefore be written as

$$146.37 = \frac{2,887,275}{2,887,275 + 374,674} 151.1 + \frac{374,674}{2,887,275 + 374,674} \widehat{c}_{\mathrm{p},2010}, \tag{11}$$

from which we find $\widehat{c}_{\mathrm{p},2010} = 109.88$. Again, this can be related to the unknown $c_{\mathrm{p},2009}$ which is probably larger (recall that the three unknown quantities $c_{\mathrm{w},2009} \geq c_{\mathrm{p},2009} \geq c_{\mathrm{o},2009}$ averaged to 142 g/km, see (9)). This would mean that $\widehat{c}_{\mathrm{p},2010} < c_{\mathrm{p},2009}$, thus people who pulled their purchase from 2010 into 2009 presumably bought higher polluting vehicles in 2009 than they would have in 2010 without the policy intervention taking place. This is plausible since, at a later point in time, manufacturers probably develop vehicles to be more ecofriendly. Moreover, people buying in 2009 instead of a later period might have used the rebate to upgrade a little compared to a respective purchase without a subsidy check some time later.

We now have all the ingredients to estimate the scrappage program's effect on emissions of the groups of windfall gains and pull-forward sales over the new cars' whole life cycle. For the first group, we find actual emissions over 15.4 years of $647,395 \cdot 10,000 \cdot 15.4 \cdot c_{\mathrm{w},2009}$. Maintaining the above assumption of $c_{\mathrm{w},2009} = 142$ g/km, we find these to be equal to $14,157,229$ tons of CO2. Using $\widehat{c}_{\mathrm{w},2009} = 130.85$ from (10), we find the counterfactual emissions to be $647,395 \cdot 10,000 \cdot 15.4 \cdot 130.85$ or $13,045,259$ tons of CO2. Therefore, the policy intervention is responsible for increased CO2 emissions by toxic windfall amounting to $\mathrm{E_w} := 14,157,229 - 13,045,259 = 1,111,970$ tons of CO2—as already calculated above.

For the sales that have been pulled forward in time, we find that over the life span of the new car, this car's emissions of CO2 are equal to $437,120 \cdot 10,000 \cdot 15.4 \cdot c_{\mathrm{p},2009}$, where $c_{\mathrm{p},2009}$ is unknown, but somewhere around 142 since $c_{\mathrm{p},2009}$ is the median of $c_{\mathrm{w},2009}$, $c_{\mathrm{w},2009}$, and $c_{\mathrm{o},2009}$ which average to 142 g/km (see (9)). Without a Cash for Clunkers program, the old car would have still been in use for an average time span of one year[32] and only then be replaced by a new car emitting $\widehat{c}_{\mathrm{p},2010}$ g/km per year. Therefore, using $\widehat{c}_{\mathrm{p},2010} = 109.88$ g/km from (11), emissions of CO2 would, over the life span of the counterfactual new car, have totaled $437,120 \cdot 10,000 \cdot 1 \cdot 200 + 437,120 \cdot 10,000 \cdot 15.4 \cdot 109.88$ or $9,558,930$ tons of CO2. Again maintaining the above assumption of $c_{\mathrm{p},2009} = 142$ g/km, we arrive at an estimate of

[32] Bearing in mind that pull-forward purchases are observable between December, 2009 and January, 2011, this assumption appears quite conservative.

$8,271,017$ tons of CO2 for the actual emissions, entailing that the pulled forward sales lead to additional CO2 emissions of $\mathrm{E_p} := 9,558,930 - 8,271,017 = 1,287,912$ tons of CO2.[33] Summing up the results for the two groups of windfall gains and pull-forward sales, we find that the German program reduced CO2 emissions only in 2009, but over the life time of the new cars increased CO2 emissions by $\mathrm{E_w} + \mathrm{E_p} = 2,399,882$ tons of CO2.[34]

While we cannot come up with a definite pollution figure for the group of on-top sales,[35] it is plausible to assume that this group will definitely not suffice to offset the joint effect of the groups of windfall gains and pull-forward purchases. Taken together, due to sales upgrades in comparison to a situation without a scrappage program, both categories comprise a Cash for Clunkers induced effect of about 2.4 million tons of CO2.

4.4 Policy Implications

Our results point out that the German scrappage program triggered economic and ecological effects to work in very opposite directions. While policy makers could back the domestic vehicle market by generating more than one million subsidy-induced car purchases, they provoked CO2 emissions to notably rise—at least over the long run. This is due to the groups of windfall gains and pull-forward sales which were incentivized to use the €2,500 for a vehicle upgrade.

The calculations presented above were done assuming average CO2 emissions of these two groups vehicles' of 142 g/km each, making sure not to overestimate the environmental damage attributed to the German program. However, it appears plausible that people profiting from the windfall gain bought less ecofriendly cars than those pulling forward their purchase, simply because the former's cars typically were older than the latter's, which would not have been scrapped in absence of the intervention. Therefore, the *net subsidy*, i.e. the difference between

[33]The calculation presented here is in fact conservative as we used the life span of the new car for calculating total emissions, leading to a total time span of 16.4 years for the counterfactual while the time span used for estimating actual emissions is only 15.4 years. If calculations were done over a fixed time span of 15.4 years, the counterfactual value would become smaller and additional CO2 emissions induced by the Cash for Clunkers program would turn out to be larger.

[34]The estimate of this net effect is, again, conservative, i.e., if we do not assume that $c_{\mathrm{w},2009} = 142 = c_{\mathrm{p},2009}$, but for instance $c_{\mathrm{w},2009} = 145 > 141 = c_{\mathrm{p},2009}$, then a larger effect results for $\mathrm{E_w} + \mathrm{E_p}$, e.g. $2,631,662$. This holds true for all combinations of $(c_{\mathrm{w},2009}, c_{\mathrm{p},2009}, c_{\mathrm{o},2009})$ fulfilling (9), $c_{\mathrm{w},2009} \leq 200$, and $c_{\mathrm{o},2009} \geq 100$.

[35]We cannot be positive about their true alternative. Would it be riding the bus, then on-top sales would rather increase CO2 emissions. Would it be buying a used car, on the other hand, this group would rather decrease CO2 emissions. A modest guess is that such effects might offset each other leaving on-top sales as rather neutral.

the €2,500 subsidy amount and the old car's worth, by tendency was larger for the group of windfall gains, resulting in a stronger incentive for corresponding buyers to upgrade the vehicle. We, thus, present results for one other, somewhat less modest possibility of these groups' average CO2 emissions: assuming that windfall gains and pull-forward sales bought cars emitting 150 g/km and 139 g/km respectively, we find a toxic windfall of $1,909,560$ tons of CO2, while the contribution to environmental damage by pull-forward purchases amounts to "only" $1,085,963$ tons of CO2.[36]

All in all, we are therefore quite convinced that particularly the category of windfall gains led to undesirable consequences which are twofold. First, it considerably hurt the environment due to unwanted vehicle upgrades. Second, it wasted quite a share of the subsidy funds with around $650,000$ sales worth about one third of the budget amount. Phrased differently, the German program was effective in stabilizing the car market, but far from being efficient.

So, when we ask what to learn from these findings for future scrappage programs to come, there are several implications worth discussing. To begin with, it is hard to think about designs that dis-incentivize the group of windfall gains. *Reducing* the subsidy amount is not working: even with a net scrappage rebate of only €1, such people would still skim off the single Euro. Since they want to purchase a car anyways, they will simply do so and, additionally, take along any subsidy amount whatsoever. What is more, quite logically, the more policy makers reduce the subsidy value, the more share of pull-forward and on top-sales they lose. *Increasing* the subsidy amount would lead to the reversed, i.e., favored, effect concerning the latter groups. Furthermore, the larger the subsidy, the more it makes sense to scrap an old, middle or upper class vehicle worth more than €2,500 and replace it by a new similar one. Therefore, a larger subsidy might lead consumer demand being shifted away from small and mini cars, a probably desired outcome considering the domestic vehcile market rather focusing on upscale car brands. However, a subsidy increase would still leave us with the problem of windfall gains wasting a huge, in this case an even bigger, amount of the overall budget. Moreover, as shown by our calculations, this again would lead to increasing CO2 emissions over the life time of the new passenger cars.

Another idea would be to introduce progressive subsidy amounts, i.e. rebates which depend

[36] In this case, overall program-induced CO2 emissions total $2,995,5232$ tons, significantly more than our conservative estimate of $2,399,882$ tons from the preceding Subsection.

on, e.g., the improvement in fuel economy (MPG). We do not think, though, that such a design would necessarily be beneficial either. One has to bear in mind that windfall gains, over all three groups of buyers, most likely had the oldest car at their disposal. Such people would have purchased a car during times of crisis also in absence of a subsidy which leads to the conjecture that they simply "had to" purchase. Probably, they could not wait any longer to replace their old car, meaning that they had a clunker with very bad mileage at their disposal. In turn, this would yield the biggest (mileage) improvement from old to new car over the three sub-groups of subsidized buyers implying that windfall gains, again, would receive quite some share of the overall budget. As a consequence, one might think about an eligibility criterion which is only attached to the new car, e.g., MPG once again. This would lead to a "neutralization" of the trade-in vehicle's status so that windfall gains would not unnecessarily be treated as privileged in terms of the specific subsidy amount they receive. In light of this discussion, we have calculated what the average CO2 emission level of all subsidized cars would have had to be for the German program to imply a neutral, long-run effect on the environment: 133.94 g/km. As this amount is substantially lower than the actual value of 142 g/km, we can safely state that by only enforcing environmental standards regarding new (subsidized) cars, it would have been quite a task to design the scrappage program such that it acts ecologically neutral. Moreover, the German car manufacturing lobby might not favor such a solution since domestic brands are not exactly kown for their economical mileage.

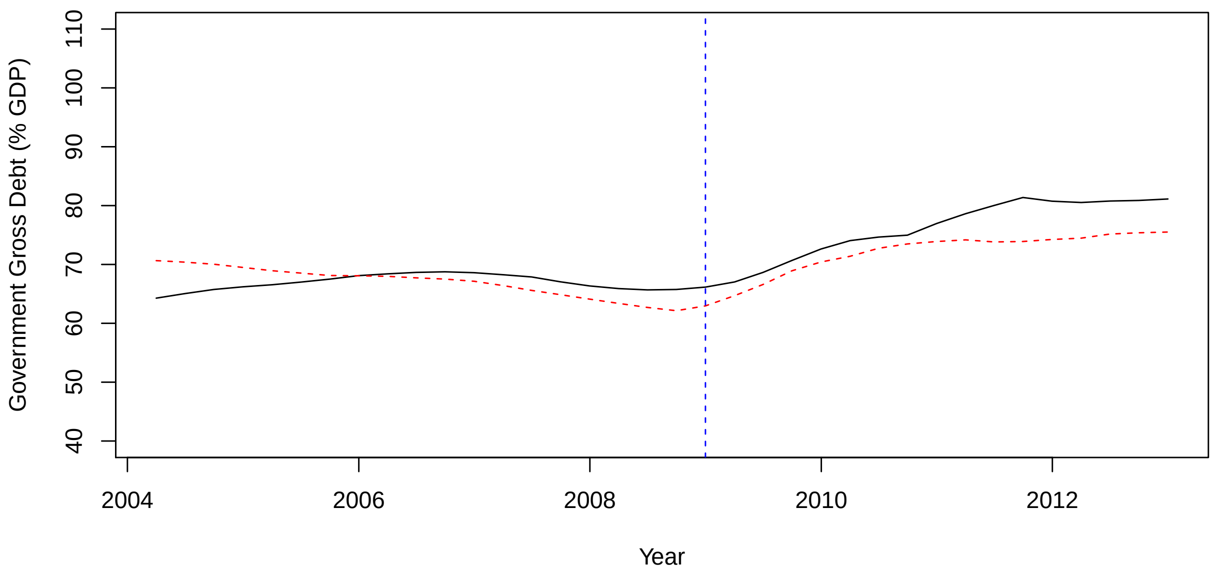
Leaving aside the *design* of the subsidy, we eventually address the *timing* of the German Cash for Clunkers program. During 2007/2008, many car owners have delayed their purchases of new vehicles due to the economic crisis, sticking to their rather eco-unfriendly, old cars. Therefore, these vehicles' replacement was long overdue when the German program was finally launched in 2009, resulting in a large group of cars that would have been replaced anyway, simply because the vehicle could no longer be used.[37] Therefore, incentivizing the corresponding car holders was more or less unnecessary, and they took the chance to invest the €2,500 subsidy into larger, more polluting cars. Naturally, then, the question arises whether it would have been better if an analogously designed program had been launched earlier, e.g. in 2008.

[37]In Germany, after an initial period of three years, passenger cars must be presented for inspection at officially approved facilities every two years. Without passing such an inspection, cars must not be used on public roads.

Although we cannot answer this question definitely without making debatable assumptions, we can shed some light on this issue. First, with respect to the *economic* stimulus intended with the program, by preventing the delay of car renewals, intervening earlier would most likely have helped to stabilize the car market during the crisis, averting or at least mitigating the notable downturn it actually suffered from. Moreover, this could have been achieved without having to sacrifice too much of the subsidy budget due to the group of windfall gains. Second, as far as *environmental* aspects are concerned, there would have been the following advantages of an earlier intervention: cars which were way past their best would not have been driven in 2008, and the rather large emission amount produced by these old, very eco-unfriendly vehicles would have been avoided. Furthermore, because these cars' value in 2008 was higher than in 2009, the net subsidy offered to corresponding car holders would have been smaller, less incentivizing to upgrade towards larger vehicles. Therefore, presumably, intervening earlier would have been better with respect to both economic *and* environmental aspects.

5 Sensitivity Analysis

We examine the robustness of our results by conducting several sensitivity checks. First, note that the cross-validation of our preferred specification, assigning a placebo cutoff far off the actual treatment as shown in Subsection 4.1, Figure 2, already is a legitimate sensitivity study. Our confidence that a particular synthetic control estimate reflects the actual impact of interest would vastly diminish if we obtained estimated effects of similar or even greater magnitudes at times where the intervention did not take place. In that case, we would have to assume that our synthetic controls do not provide good predictors of the trajectories of new passenger car registrations or CO2 emissions in Germany in periods when the scrappage program did not occur. Recall that we did not see any reaction at the placebo cutoff after the first 18 months of our data sample in Figure 2, indicating validity of our results. Using the same idea, we check an alternative placebo cutoff, July, 2006, dividing the *whole* pre-treatment period into two equal halves—30 months respectively. Figure 10 in the appendix clearly shows that there is no jump at or even around this cutoff whatsoever. Consequently, we can state that we find a very large effect for the 2009 German scrappage scheme, but no effect at all



Note: Germany and Synthetic Germany are represented by a black solid and red dasehd line respectively. The blue vertical line indicates the begin of the German scrappage program.

Figure 8: Trends in Governmental Gross Debt (as % of GDP): Germany vs. Synthetic Germany

when artificially re-assigning the treatment period in our data to a date before 2009, i.e. we are confident that the effect estimated for the German policy intervention is indeed attributable to the policy intervention itself.

Furthermore, we want to see whether some arbitrary placebo *outcome* would show a noticeable reaction to the real cutoff of our main model, thereby testing for its reliability or susceptibility. We choose two outcomes that move with the economy but supposedly are unaffected by the scrappage subsidy: per capita oil consumption (annual data) and Governmental debt (quarterly data). Again, we use our MSCM-T approach to jointly optimize those two placebo outcomes using the known set of economic predictors and donor countries. Figure 8 shows the effect of the German scrappage program on Governmental debt which, obviously, is non-existent. Figure 11 in the appendix shows the equivalent graph for per capita oil consumption. For this economic outcome, the German scrappage program did not have a measurable effect either, quite as one would have expected ex ante.

In a last attempt to fundamentally challenge our main findings, we slightly lifted the lower bar for the v-weights of our economic predictors to an economically meaningful bound of 1%, still finding our results to be confirmed. In summary, all of the above-mentioned sensitivity checks strongly support the credibility of our findings, leading to the conclusion that the German scrappage program indeed had a significantly positive and long-standing effect on new passenger car sales, at the cost of increased CO2 emissions over these cars' life time.

6 Evaluations across Europe

Since our data does not only consist of Germany and several countries which *did not* implement any scrappage scheme around 2009 (our donor pool), but also of 11 further countries that *did* so, our analysis is not limited to the German case. Applying the same method, just replacing Germany by, e.g., France, is a straightforward extension which we implement in Subsection 6.1 by looking at our two dependent variables. Quite logically, our data also allows us to ask the reversed question, namely whether there exist countries not having implemented any scrappage scheme that would have benefited from positive car registrations or a reduction in vehicle CO2 emissions if they had decided to do so. In Subsection 6.2, we therefore want to shed light on what *would have* happened to our outcomes of interest if a non-scrapping country *would have* introduced a car retirement subsidy.

6.1 Evaluation of other Scrapping Countries

It is interesting to see how other European countries' vehicle sales and corresponding pollution levels did react as an answer to their respective program and to compare size and duration of these effects across countries. One has to bear in mind, though, that governments implemented their programs at different points in time over different intervals, using various budget volumes.[38] Considering these facts, we carry out eleven further studies analogous to the German one. Before turning to the results, we have a closer look at the weights for the economic predictors, the weights distributed to control countries when synthesizing a respective treatment country, and the significance of respective effects using placebo studies.

Interestingly, over all twelve analyses, each and every economic predictor has a minimum

[38] There might exist more differences, e.g., the funding of the program (by the government vs. by the government *and* car manufacturers), age and other requirements concerning the old car, environmental and other criteria regarding the new car, or further policies that were implemented during the respective program period.

close to or exactly at our lower bound and a maximum at least greater than 12%—ranging till more than 90%. This means that all of the ten controls show their raison d'être within our model. For some country, a specific predictor may not be that important to achieve a proper fit, for another country, though, the very same predictor may play a significant role. Since we cannot anticipate every such case in advance, it makes sense to use all ten economic predictors throughout every analysis. Eventually, Table 5 in the appendix provides the results of the MSCM-T's optimization procedure. It sheds light on whether a weight—and if so, of what magnitude—is distributed to specific control countries to help producing a counterfactual for a given scrapping country. Most countries are represented by three or more control units with almost every control unit (besides Estonia) used at least once to help reproducing a specific scrapping country. Thereby, analogously to our economic predictors, we make good use of the available countries within our donor pool.

Furthermore, when inspecting the MSCM-T results of all twelve scrapping countries (including Germany), we have to exclude Ireland, Luxembourg, and the UK due to poor pre-treatment fits as well as Greece, Italy, the Netherlands, and Portugal because of insignificant results. This leaves us with five countries revealing significant impacts on new passenger car registrations: Austria, France, and Slovakia feature consistently positive effects and show trajectories similar to Germany. Spain, on the contrary, shows a negative effect. When talking about Spain, we also want to consider respective negative outcomes of Portugal, Italy, and Greece, even though we excluded those countries due to insignificant results. On closer inspection, we can see that the severity of negative registrations impacts across those countries did not start before 2011, two years after the policy intervention. One year earlier already, there are essentially no negative effects. Moreover, *every nation* seems to have benefited—even though not necessarily significantly—from the respective policy intervention in the short run. This also becomes evident given the shape of the effect trajectories all virtually looking like an inverted U-curve: increasing car registrations after the program start and declining figures during the following periods. Hence, we come to the conclusion that the long-term negative impact on new vehicle registrations for *P*ortugal, *I*taly, *G*reece, and *S*pain is rather caused by the sovereign-debt crisis than by a 2009 vehicle stimulus program. This does not necessarily mean that there is no long-run stimulus impact at all, but this effect might be simply superimposed by other economic influences which we cannot easily distinguish. On the contrary, at least in the short- and medium-run, all countries seem to have clearly benefited from the respective scrappage scheme with respect to new passenger car registrations. This finding suggests that, for all of the evaluated nations, this policy intervention did help to stabilize or even boost domestic vehicle markets throughout and after the recent financial crisis. At the very least, our counterfactual analysis suggests evidence for the efficacy of scrappage subsidies.

Analogous results for CO2 emissions look as follows: we exclude France, Italy, Luxembourg, Portugal, and Slovakia due to inappropriate pre-treatment fits. Austria, Spain, and the UK deliver insignificant results. This leaves us with three countries revealing significant effects: Greece shows negative figures, meaning reduced CO2 emissions across the post-treatment period; the Netherlands and Ireland also feature negative effects on vehicle related pollution, albeit to a lesser extent. For countries with positive registration outcomes, corresponding pollution results—even though not significant—essentially look the same: during the first post-treatment period there is a reduction of average CO2 emissions which is then (more than) offset by an increase in emissions throughout the rest of the observation frame. This is completely consistent with our observations for the German case in Subsection 4.3.

6.2 Evaluation of Non-Scrapping Countries

In a next step, we switch twelve countries *with* a scrappage program into the donor pool and look at the effects of eleven previous control units *without* such a program. One caveat in so doing is that the newly generated donor pool is somewhat heterogenous, i.e., there exist different intervention cutoffs and program durations across scrapping countries. Hence, we have to arbitrarily assume a treatment start for our units of interest and thus can only conduct this analysis very roughly.[39] We decide to set this cutoff to January, 2009, since the majority of programs within our donor pool started in early 2009. Because of this heterogeneity we defer each donor country's time series so that all scrapping programs start at the same point in time. This way, we homogenize our donor pool with respect to the cutoff, i.e., the program start, and are able to run our established model. Furthermore, as it is not suitable to consider donor units with outcomes that were subject to idiosyncratic shocks during the study period,

[39] Moreover, the caveats mentioned in Subsection 6.1 also apply.

we restrict the post-treatment frame to 2009/2010 since we have seen that Portugal, Italy, Greece, and Spain were hit by the sovereign-debt crisis starting early or mid 2011.

Table 6 in the appendix displays the results of the MSCM-T optimization procedure, analogous to the previous subsection. Again, we are making good use of our donor pool with most countries being represented by three or more control units. Spain is the only unit left unused to help reproducing a specific non-scrapping country. Moreover, featuring similar figures as before, all of the ten economic predictors are useful within our model. Before looking at potential effects regarding new passenger car registrations, we have to exclude Estonia, Finland, Latvia, Lithuania, and Sweden due to inappropriate pre-intervention fits. Moreover, Belgium, Czech Republic, Denmark, and Poland deliver insignificant results when running our standard placebo study. This leaves us with just two countries, Hungary and Slovenia, showing significantly positive effects of a hypothetic scrappage program on new passenger car registrations. Particularly, Hungary exhibits impressive figures ending up with a potential plus of about 2.3 per capita percentage points, i.e., more than $200,000$ new vehicle registrations at the end of 2010. On the other hand, leaving aside the significance of results, over about the first 15 months after the beginning of a potential program, *every single nation* would have benefited regarding new car sales. Once again, the shape of respective effect trajectories more or less appears like an inverted U-curve with increasing vehicle registrations after the potential program start and declining numbers throughout the following periods. Those findings suggest that for all of the evaluated eleven countries without a scrappage program during the recent crisis, such a policy intervention could have helped to at least stabilize domestic vehicle markets.

When conducting analogous analyses for passenger cars' CO2 emissions, the following patterns emerge: we have to exclude Estonia, Finland, Latvia, Lithuania, Poland, and Sweden due to poor pre-treatment fits. Moreover, Belgium, Denmark, and Slovenia feature insignificant results. This means that there are two countries revealing significant effects, Czech Republic and Hungary. Both show the known effect-trajectory: during the first post-treatment period we observe a reduction of average CO2 emissions which is then reversed throughout the following year. Actually, even if not necessarily significant, almost all of the pollution results show this development, similar to our findings for Germany.

7 Conclusion

Car scrappage subsidies are supposed to smooth negative impacts on one of the most important markets within our economy. Since vehicle retirement plans typically involve goals beyond hard sales figures—above all, environmental aspects—and demand a substantial budget, a sound decision on a particular policy intervention becomes not only more difficult but also potentially more expensive. In order to be able to examine the impact of scrapping schemes, in our case on car purchases and respective CO2 emissions, it is key to compute reliable counterfactuals. Developing and applying MSCM-T, the multivariate synthetic control method using time series of economic predictors, we are making use of a rich pool of various European countries, with and without respective scrappage programs, and several covariates to reproduce an accurate counterfactual of the country of interest.

Our findings reveal that it is non-trivial for governments to decide in favor of or against scrappage subsidies during times of crisis. In the case of Germany, we find that the policy intervention indeed provided substantial short-run and medium-run stimulus to the new car market. We show an enormous effect on new passenger car registrations still being evident four years after the treatment. Being the number one European country in terms of employees within the automotive market and regarding new passenger cars production, the German scrappage program distinctly stabilized one of its most important markets and its overall economy. It did so by not only borrowing car purchases from the future but also by generating a large amount of additional sales on top of regular ones within the program period. Our estimates reveal that out of the 2 million subsidized vehicles about $650,000$ would have been purchased anyways (windfall gains); without the intervention, roughly $440,000$ cars would have been bought at a later point in time (pull-forward effects); and slightly more than $850,000$ vehicle sales would not have been realized at all without the introduction of the scrappage subsidy (on-top sales), amounting to a value of roughly € 18 billion.

With respect to greenhouse gas emissions, however, analyzing the effects of the scrappage program is much more complicated. Because of the pulling forward of about half a million purchases, the actual average CO2 emissions in *2010* were larger than they would have been without the intervention, as purchases of these small, ecofriendly cars now took place during the program in 2009, but not in 2010. This reasoning also implies that in *2009*, actual average

CO2 emissions of new cars should have been considerably lower than their counterfactual equivalent. However, actual and counterfactual values for 2009 are almost identical. This is because windfall gains and pull-forward purchases, by the €2,500 subsidy, were lured into buying less ecofriendly cars. Therefore, the German scrappage program will lead, over the life span of new cars of approximately fifteen years, to an overall *increase* of CO2 emissions of more than $2,000,000$ tons.

We also run analogous analyses for other European countries with a comparable scrappage program. Our results suggest that, at least over the first 24 months after the policy intervention, scrappage subsidies did stabilize or even boost each of the respective domestic vehicle markets. For instance, the number two passenger car producing nation within Europe, France, features a substantial cumulative impact on vehicle registrations of 2.24 per capita percentage points or more than one million additional cars. We also show that for all countries which *did not* implement a vehicle retirement program, it would have been considerably better to do so with respect to new car registrations. In Hungary, e.g., a scrappage subsidy could have prevented annual vehicle sales to decline from their long-term level of ca. 2 per 100 inhabitants to about only 0.43, backing the purchase of more than half a million new cars. Corresponding CO2 analyses are mostly in line with our estimates for Germany.

All in all, our findings suggest that, while scrappage programs considerably support domestic economies by backing or even boosting their respective vehicle market's sales, they tend to hurt the environment by increasing greenhouse gas emissions. Because of these economic and ecological effects working in very opposite directions, it is difficult to give clear advice for car retirement schemes to come. We saw that it is particularly important to dis-incentivize the group of windfall gains which is bad in two ways: first, it wastes quite a share of the subsidy funds since those cars would have been purchased anyways—also in absence of a scrappage program. Second, it hurts the environment by producing a substantial amount of additional CO2 emissions due to an unnecessary subsidy-induced vehicle upgrade. When caring about environmental *and* economic impacts, it might help to faster implement stimulus interventions in times of crisis, also in order to prevent the buildup of old cars not getting replaced. In practice, however, the main focus might still remain on the outcome policy makers are able to reliably influence: car sales.

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A MSCM-T Theory

For every country ($j = 1, \ldots, J+1$), we denote by

$$V_{j,t} := (Y_{1,t,j}, Y_{2,t,j}, X_{1,t,j}, \ldots, X_{K,t,j})'$$

the $(2+K)$-dimensional stacked vector consisting of the number of new car registrations in that country within month t, the average CO2 emissions of these cars, and the corresponding values of the economic predictors. We further denote by $A_{0,t}, \ldots, A_{p,t}$ possibly time-varying

$(2+K)\times(2+K)$-dimensional regressor matrices and by $\varepsilon_{j,t}$ a $(2+K)$-dimensional error term. We assume that, for every country j, $V_{j,t}$ is a vector autoregressive process of order p:

$$V_{j,t} = \sum_{h=0}^{p} A_{h,t} V_{j,t-h} + \varepsilon_{j,t} = A_{0,t} V_{j,t} + \ldots + A_{p,t} V_{j,t-p} + \varepsilon_{j,t},$$

for which $I - A_{0,t}$ is invertible for all t, with I denoting the $(2+K)$-dimensional identity matrix. The above equation can then be rewritten as

$$V_{j,t} = (I - A_{0,t})^{-1} \left(\sum_{h=1}^{p} A_{h,t} V_{j,t-h} + \varepsilon_{j,t} \right) = \sum_{h=1}^{p} \widetilde{A}_{h,t} V_{j,t-h} + \widetilde{\varepsilon}_{j,t},$$

with $\widetilde{A}_{h,t} := (I - A_{0,t})^{-1} A_{h,t}$ and $\widetilde{\varepsilon}_{j,t} := (I - A_{0,t})^{-1} \varepsilon_{j,t}$. Denoting by T_0 the time of the intervention, the following proposition holds:

Proposition 1. *1. if $T_0 \geq p$ and $W = (w_2, \ldots, w_{J+1})$ fulfils $V_{1,t} = \sum_{j=2}^{J+1} w_j V_{j,t}$ for all $t \leq T_0$, then the effect estimator defined in Section 3 is unbiased.*

2. assume now that we have possibly time-varying unobserved confounders

$$u_{j,t} := \left(\lambda_{1,t} \mu_{1,j}, \ldots, \lambda_{2+K,t} \mu_{2+K,j} \right)',$$

with $\lambda_{i,t} \in \mathbb{R}^{1 \times F_i}$ denoting the values of the F_i unobserved confounders affecting the i-th component of the stacked vector V and $\mu_{i,j} \in \mathbb{R}^{F_i \times 1}$ describing the corresponding factor loadings of unit j. If

$$V_{j,t} = \sum_{h=0}^{p} A_{h,t} V_{j,t-h} + u_{j,t} + \varepsilon_{j,t} = A_{0,t} V_{j,t} + \ldots + A_{p,t} V_{j,t-p} + u_{j,t} + \varepsilon_{j,t},$$

then the effect estimator is asymptotically unbiased under standard assumptions.

Proof. The proof of the first assertion is completely analogous to the proof of (5) in Abadie et al. (2010, p. 504f) while the second assertion can be proven in the same manner as (1) in Abadie et al. (2010, p. 503ff). □

B Tables and Figures

Table 2: Annual Registrations and Average CO2 Emissions per km of New Passenger Cars in Germany

Year	Registrations	CO2 (g/km)
2004	3,225,177	174.9
2005	3,297,147	173.4
2006	3,465,031	172.5
2007	3,165,967	169.5
2008	3,078,652	164.8
2009	3,794,418	154.0
2010	2,887,275	151.1
2011	3,151,570	145.6
2012	3,091,931	141.5

Table 3: Summary Statistics

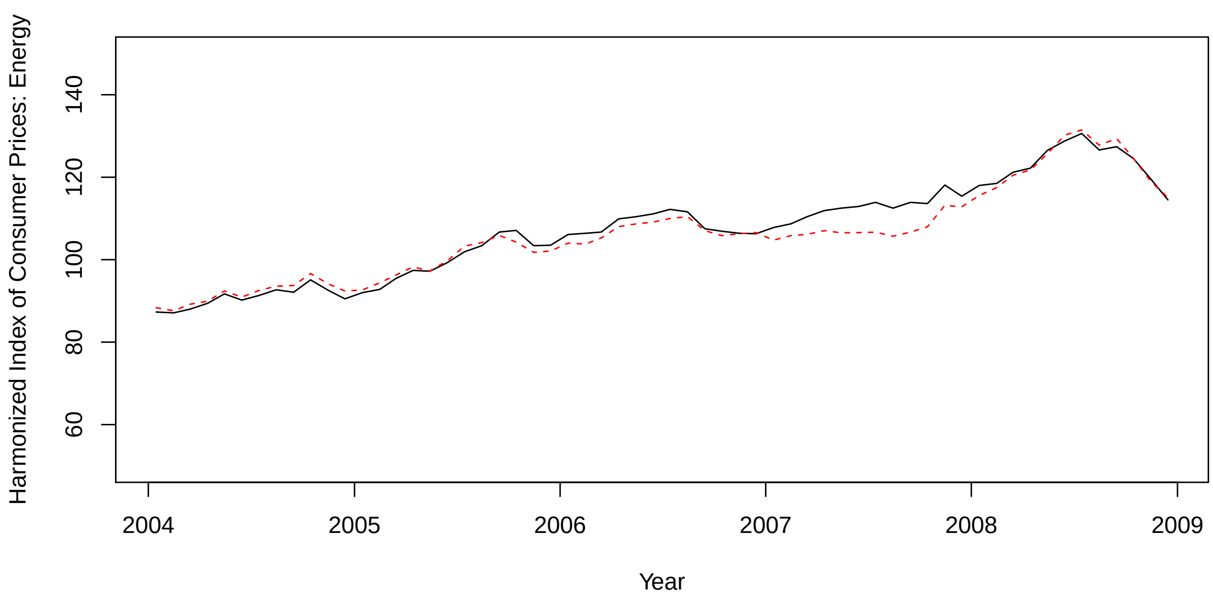
Variable	Mean	Median	Minimum	Maximum	Frequency
NRPC	2.79	2.72	0.15	10.99	monthly
CO2	153.43	153.15	117.00	197.20	annual
CE	17,282.17	19,850.00	3,700.00	37,000.00	quarterly
CO2_pc	9.43	8.90	3.40	26.00	annual
GDP	23,900.22	25,100.00	4,400.00	78,500.00	quarterly
HICPC	99.25	99.94	74.95	112.30	monthly
HICPE	107.59	106.57	80.36	171.28	monthly
NE	14,356.72	15,042.05	4,206.67	23,897.88	annual
PNS	2,525.39	2,295.82	351.69	6,231.47	annual
PTTR	0.61	0.48	0.04	2.26	annual
SPC	81.52	83.30	59.80	91.00	annual
UE	7.45	7.20	2.70	19.40	annual

Note: NRPC stands for New Registrations of Passenger Cars; *CO2* stands for CO2 Emissions of New Passenger Cars; *CE* stands for Consumption Expenditures; *CO2_pc* stands for CO2 Emissions Per Capita; *GDP* stands for Gross Domestic Product; *HICPC* stands for Harmonized Index of Consumer Prices: Cars; *HICPE* stands for Harmonized Index of Consumer Prices: Energy; *NE* stands for Net Earnings; *PNS* stands for Pensions; *PTTR* stands for Passenger Transportation Tax Revenues; *SPC* stands for Share of Passenger Cars; and *UE* stands for Unemployment Rate.

Table 4: Economic Predictor Means Before Germany's Scrappage Program

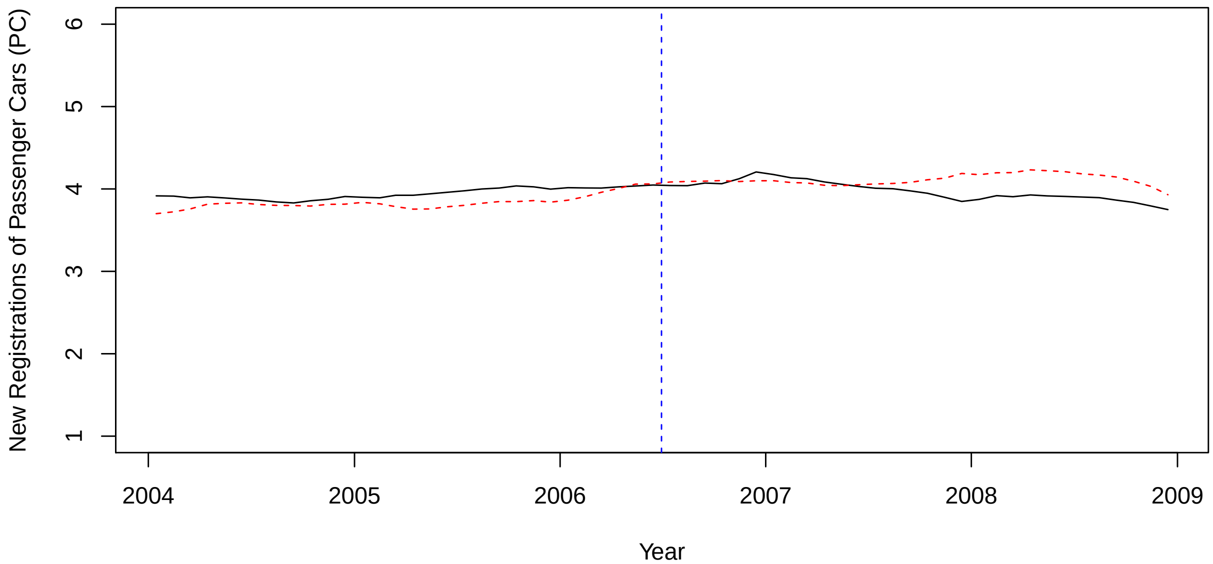
Control	**Real**	**Synthetic**	**Average**
CE	21,275.00	23,737.55	13,633.64
CO2_pc	10.48	8.79	8.64
GDP	28,025.00	32,117.68	18,302.73
HICPC	102.26	100.33	98.50
HICPE	106.99	105.91	108.32
NE	23,047.24	20,736.53	11,713.41
PNS	3,608.43	3,661.11	1,894.98
PTTR	0.37	0.61	0.60
SPC	85.70	82.21	80.08
UE	9.78	7.31	7.47

Note: Means for all economics predictors over the pre-program period January, 2004 till December, 2008. *CE* stands for Consumption Expenditures; *CO2_pc* stands for CO2 Emissions Per Capita; *GDP* stands for Gross Domestic Product; *HICPC* stands for Harmonized Index of Consumer Prices: Cars; *HICPE* stands for Harmonized Index of Consumer Prices: Energy; *NE* stands for Net Earnings; *PNS* stands for Pensions; *PTTR* stands for Passenger Transportation Tax Revenues; *SPC* stands for Share of Passenger Cars; and *UE* stands for Unemployment Rate.



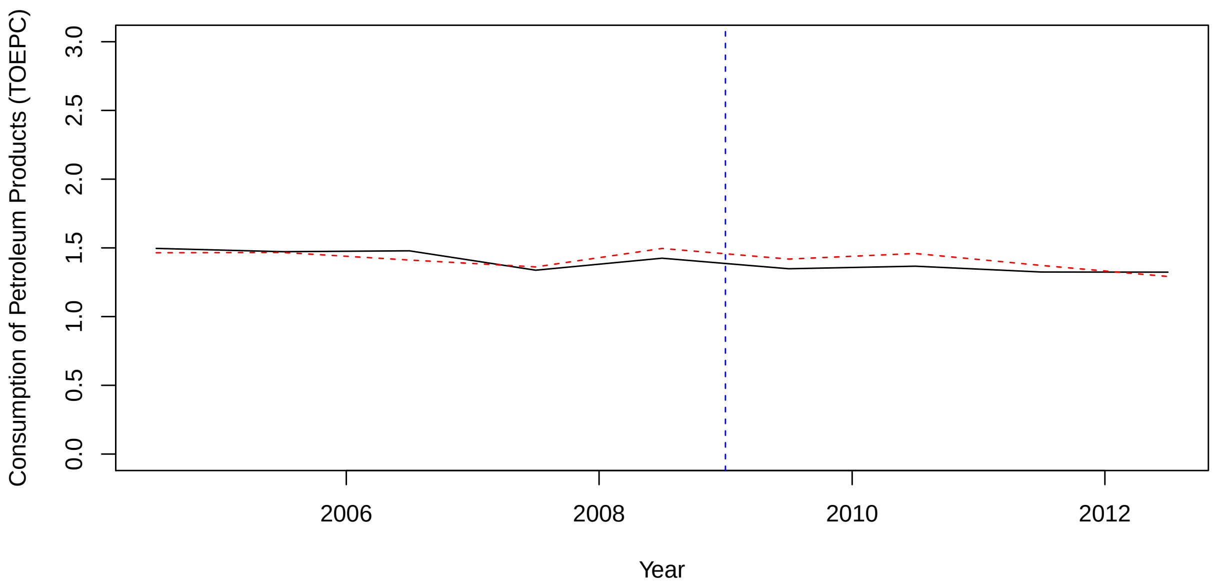
Note: Germany and Synthetic Germany are represented by a black solid and a red dashed line respectively.

Figure 9: Harmonized Index of Consumption Energy: Germany vs. Synthetic Germany



Note: Germany and Synthetic Germany are represented by a black solid and red dasehd line respectively. The blue vertical line separates the 60 months pre-treatment period into two equal halves.

Figure 10: Trends in Per Capita (%) New Passenger Car Registrations - Placebo Cutoff: Germany vs. Synthetic Germany over the Pre-Treatment Period



Note: Germany and Synthetic Germany are represented by a black solid and red dasehd line respectively. The blue vertical line indicates the beginning of the German scrappage program.

Figure 11: Trends in Per Capita Petroleum Consumption (Tonnes Oil): Germany vs. Synthetic Germany

Table 5: W Weights over all Scrapping Countries

Country	AUT	FRA	GER	GRE	IRL	ITA	LUX	NED	POR	SVK	ESP	UK
Belgium	50.55	58.18	48.84	15.10	47.36	80.21	86.39	22.71	29.08	0.00	60.17	55.58
Czech Republic	5.77	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	7.23	0.00	0.00
Denmark	0.49	0.00	0.00	0.00	9.01	0.00	0.00	15.93	4.48	0.00	0.00	0.00
Estonia	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Finland	0.00	0.00	1.85	34.78	43.63	0.00	0.00	40.87	0.00	0.00	0.00	19.66
Hungary	16.71	24.87	0.00	10.10	0.00	0.00	0.00	0.00	0.00	30.61	25.31	0.00
Latvia	0.00	0.00	0.00	12.09	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Lithuania	0.00	2.95	0.00	0.00	0.00	0.00	0.00	9.70	0.00	7.23	0.00	0.00
Poland	0.00	14.01	0.00	16.03	0.00	19.79	0.00	0.00	66.44	54.92	14.53	0.00
Slovenia	4.28	0.00	0.00	11.91	0.00	0.00	0.00	10.80	0.00	0.00	0.00	0.00
Sweden	22.20	0.00	49.31	0.00	0.00	0.00	13.61	0.00	0.00	0.00	0.00	24.76

Note: W weights attributed to control units (rows/non-scrapping countries) for every synthesized unit (columns/scrapping countries). Abbreviations stand for: *AUT* (Austria); *FRA* (France); *GER* (Germany); *GRE* (Greece); *IRL* (Ireland); *ITA* (Italy); *LUX* (Luxembourg); *NED* (Netherlands); *POR* (Portugal); *SVK* (Slovakia); *ESP* (Espania); *UK* (United Kingdom).

Table 6: W Weights over all Non-Scrapping Countries

Country	BEL	CZE	DEN	EST	FIN	HUN	LAT	LTU	POL	SLO	SWE
Austria	0.00	21.78	0.00	0.00	0.00	29.96	0.00	0.00	0.00	61.27	0.00
France	7.40	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Germany	0.00	0.00	0.00	0.00	64.71	0.00	0.00	69.40	0.00	0.00	40.31
Greece	0.00	0.00	0.00	100.00	0.00	0.00	100.00	0.00	3.68	0.00	0.00
Ireland	0.00	0.00	1.27	0.00	0.70	0.00	0.00	0.00	0.00	0.00	0.00
Italy	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	12.72	0.00
Luxembourg	26.59	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Netherlands	0.00	0.00	75.53	0.00	34.59	0.00	0.00	0.00	0.00	0.00	17.14
Portugal	49.88	15.09	23.21	0.00	0.00	9.04	0.00	0.00	4.33	24.30	0.00
Slovakia	0.00	63.13	0.00	0.00	0.00	60.99	0.00	30.60	91.99	1.71	0.00
Spain	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
United Kingdom	16.13	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	42.55

Note: W weights attributed to control units (rows/scrapping countries) for every synthesized unit (columns/non-scrapping countries). Abbreviations stand for: *BEL* (Belgium); *CZE* (Czech Republic); *DEN* (Denmark); *EST* (Estonia); *FIN* (Finland); *HUN* (Hungary); *LAT* (Latvia); *LTU* (Lithuania); *POL* (Poland); *SLO* (Slovenia); *SWE* (Sweden).