Four Essays in Empirical Industrial Organization

Inaugural-Dissertation
zur Erlangung des akademischen Grades einer Doktorin
der Wirtschaftswissenschaften
(Dr. rer. pol.)

eingereicht an der Heinrich-Heine-Universität Düsseldorf

HEINRICH HEINE
UNIVERSITÄT DÜSSELDORF

vorgelegt von: Andrea-Louise Müller
geboren am 22.06.1985 in Bayreuth

Erstgutachter: Prof. Dr. Justus Haucap
Zweitgutachter: Dr. Ulrich Heimeshoff

Abgabedatum: 10. April 2015

Preface

This thesis was created while I was working as research assistant at the Duesseldorf Institute for Competition Economics. I would like to thank everybody there for contextual and personal support, including other PhD students, professors, office and student assistants.

First of all, I would like to personally thank my two supervisors and co-authors Justus Haucap and Ulrich Heimeshoff, who always gave valuable and important input, and to Ulrich Heimeshoff especially for helping with all the tricky empirical details. Furthermore, this thesis benefited from fruitful discussions with the other members of the Empirical Research Group: thanks to Lars Zeigermann and Johannes Muck for answering Stata programming questions, to Veit Böckers for being a co-author and becoming a friend, and to Anna Lu and Anne-Kathrin Barth, who were sharing an office with me, for living through all the ups and downs along the way.

Secondly, the different chapters were presented at national and international conferences, so that I am thankful to DICE and Konrad-Henkel-Stiftung for granting financial support to make this possible and to all participants and discussants for their valuable comments.

Most of all, I am in debt to Moritz Suppliet for his outstanding personal support and honest subject-related feed back in various places all around the world. I am not sure if I would have made it without him.

And last but not least, I have the best family in the world: My parents and my brother. They helped with all non-technical problems and supported me no matter what. Thank you for always being there.

But the most important people for the completion of this work are my neighbors and the best of grandparents, Irmgard and Karl Rheinstädtler, who never stopped asking when I would have this thesis finished, so that we could finally celebrate together. This thesis is for you.

Contents

1 Introduction **1**

2 Evaluating Cash-for-Clunkers in Selected Countries **5**

2.1 Introduction 6
2.2 Literature Review 9
2.3 Empirical analysis 11
2.3.1 Data and summary statistics 11
2.3.2 Model 14
2.3.3 Results 16
2.3.4 Sensitivity analysis 18
2.4 Simulations 22
2.4.1 Strategy 22
2.4.2 Simulation Results 23
2.5 Conclusion 26
Bibliography 29
Appendix 32

3 The Effects of the German Car Scrappage Scheme **37**

3.1 Introduction 38
3.2 Literature Review 39
3.3 The German Scrappage Program 41
3.4 Empirical Approach 45
3.4.1 Data 45
3.4.2 Methodological Considerations 46
3.4.3 Estimation Strategy and Calculation of the Counterfactual . . 47
3.4.4 Model Selection 50
3.5 Results 52
3.6 Conclusion 55
Bibliography 56

Appendix 59

4 Electricity Retail Prices and Photovoltaic Diffusion 65
4.1 Introduction 66
4.2 Literature Review 67
4.3 Description and Regulation of the German PV Market 69
4.4 Empirical Strategy 73
4.4.1 Data and Summary Statistics 73
4.4.2 Estimation Strategy 77
4.4.3 Results 80
4.5 Conclusion 82
Bibliography 84
Appendix 87

5 Why are Economists so Different? 92
5.1 Introduction 93
5.2 Experimental Design 94
5.3 Results 97
5.3.1 The Trust Decision (Player A) 97
5.3.2 The Trustworthiness Decision (Player B) 99
5.3.3 Regression Analysis 100
5.4 Conclusion 103
Bibliography 105
Appendix A 108
Appendix B 111

6 Conclusion 115

List of Figures

2.1 Simulation Results for Germany, USA, UK and South Korea 24
2.A1 Car Registrations for Representative Countries 36

3.1 Cars Bought and Scrapped under the Scrappage Program 43
3.2 Empirical Strategy and Timeline 48
3.3 Predictions and Realized Private Car Registrations 53
3.A1 Google Trends Search Volume and News Reference Index 60

4.1 Yearly New Installed Capacity and FiT PV Germany 2000-2013 . . . 72
4.A1 Maps of PV Installations, Electricity Price and Radiation 88
4.A2 Composition of a Household Electricity Price Bill 2011 89
4.A3 Feed-in and Auto-consumption Tariffs 2010-2012 90

5.1 Game Tree Binary Trust Game 95
5.2 Share of Trusting Students Within Subgroups 98
5.3 Share of Trustworthy Students Within Subgroups 99
5.A1 A Decisions by Field, Gender and Progress of Study 108
5.A2 B Decisions by Field, Gender and Progress of Study 109

List of Tables

2.1 Overview of Characteristics of Scrappage Programs 2008-2011 7
2.2 Variable Descriptions and Data Sources 12
2.3 Descriptive Statistics 13
2.4 Second Stage Fixed-Effects Regression Results 17
2.5 First Stage Regression Results 19
2.6 Sensitivity Analysis 21
2.7 Simulation Results for Germany, USA, UK and South Korea 27
2.A1 Panel Unit Root Tests 32
2.A2 Autocorrelation Tests and Prediction Quality 33
2.A3 Testing Anticipatory Effects 34
2.A4 Simulation Results (Other Countries) 35

3.1 The German Cash-for-Clunkers Program ”Umweltprämie” 42
3.2 Descriptive Statistics 45
3.3 Granger Causality between Segments, Model Selection 48
3.4 Prediction Error in Percent, Model Selection 51
3.5 Prediction Error in Percent, Out of Sample Prediction 52
3.6 Yearly Difference between Realized and Predicted Registrations . . . 54
3.A1 Mean and Variance of the Percentage of Private Car Holders 61
3.A2 Lag Length Selection and Information Criteria 61
3.A3 Augmented Dickey Fuller Unit Root Tests 61
3.A4 VAR Output, Model Selection 62
3.A5 VAR Residual Analysis, Model Selection 62
3.A6 VAR Output Robustness, Model Selection 63
3.A7 Monthly Difference between Realized and Predicted Registrations . . 64

4.1 Basic Assumptions of Chosen Customer Types 75
4.2 Summary Statistics 76
4.3 Second Stage Regression Results 81

4.A1 First Stage Regression Results 91

5.1 Bivariate Probit Regression of Trust and Trustworthiness Decision . . 102
5.A1 Overview Over All Sessions 109
5.A2 Summary Statistics 110

Chapter 1

Introduction

Empirical Industrial Organization has increasingly gained attention over recent years, in Germany and worldwide. In the wake of this, the "Research Group in Empirical Industrial Organization" at the DICE, University of Düsseldorf, was founded. The four essays that make up this dissertation were created while the author was part of that group. Basically, Empirical Industrial Organization is concerned with the analysis of imperfect markets and the organization of these markets. Einav and Levin (2010) give a nice overview of the recent trends in Empirical Industrial Organization. Since Schmalensee (1989), one important innovation is the renunciation of cross-industry studies, and a move towards more industry specific research. This idea was taken further by Bresnahan (1989) who coined the phrase "New Empirical Industrial Organization". Therefore, three of the four following chapters of this dissertation are pure industry case-studies. Chapters 2 and 3 are concerned with regulation and policy evaluation in the automotive industry, whereas Chapter 4 covers regulation in the electricity retail sector. The last chapter, Chapter 5, is somewhat different. It shows that for some experimental research questions it is necessary to employ sound empirical regression analysis to obtain the desired results. It analyzes the behavior of students in a trust game. The following paragraphs introduce the reader to the separate chapters of this dissertation.

Chapter 2, entitled **Evaluating the Causal Effects of Cash-for-Clunker Programs in Selected Countries: Success or Failure?** is joint work with Ulrich Heimeshoff and estimates the effects of Accelerated Vehicle Retirement Programs on car registrations, using a unique data set for 25 OECD countries from 2000 to 2010. Car scrappage programs and their success were a hot topic during the economic crisis in 2009. A worldwide overview of the success of these policies is nevertheless missing and this paper tries to close this gap. From a methodological point of view, we use a novel approach in simulating the counterfactual situation. We employ dynamic panel techniques to control for unobserved heterogeneity between the countries under consideration. Our analysis reveals that passenger car sales varied considerably before implementation of car scrappage schemes to fight the 2009 sales crisis. Compared to a simulated counterfactual, we find a positive overall effect of recent Accelerated Vehicle Retirement Programs for selected countries: the United States, South Korea, Germany and the United Kingdom. Simulation results show that timing and duration seem much more important for success than the budget allocated to the program.

The third chapter, entitled **The Effects of the German Car Scrappage Scheme: A Time Series Approach** is joint work with Ulrich Heimeshoff and Veit Böckers. It is closely related to the research presented in the second chapter. It also addresses the evaluation of car scrappage schemes. However, the chapter solely focuses on the German car scrappage program, also called "Umweltprämie" or "Abwrackprämie", that was implemented in January 2009 to stimulate automobile sales. For the German case-study we try to shed more light on the winners and losers of the scrappage scheme in terms of car market segments. Moreover, in order to obtain the counterfactual situation, the methodology is time-series econometrics. With the help of a vector autoregressive model we empirically assess the potential policy effect on certain segments, while controlling for possible inter-segment competition. In detail, we analyze potential pull-forward effects in new car registrations following the treatment period. Therefore, small and upper small car segments especially seem to have profited from the scrappage program as they make up 84% of the newly registered cars during the policy period. Results suggest that the policy has been successful in creating additional demand for both segments and the pull-forward effects are small in the first two years after the politically induced treatment.

The fourth chapter turns towards a completely different market - the energy retailing sector. The title of the chapter is **The Relationship Between Electricity Retail Prices and Photovoltaic Diffusion - Evidence from German Regional Markets**. This contribution tries to shed light on the influence factors of the diffusion of photovoltaic installations in Germany, using a fixed effects panel dataset from 2010 to 2012. The retail electricity sector is divided into a private household and a small business customer group. Results suggest that the diffusion of photovoltaic installations hinges to a large extent on the electricity retail price, and not only on the governmentally set feed-in tariffs. This can also be interpreted as the self-consumption influence of photovoltaic investment and has not been shown in the literature until now. The estimation is based on a unique panel data set with 671 zip code areas for the later level of photovoltaic adoption. For household customers, a 1% increase in the price variable is rewarded with a 10% increase in the stock of photovoltaic installations over all German zip code districts. For small business customers this effect goes up to 24%. This finding has important policy implications for the future policy set up in Germany and other major European countries that use feed-in tariffs as the main measure to promote photovoltaic growth.

Chapter 5 is entitled **Why are Economists so Different? Nature, Nurture and Gender Effects in a Simple Trust Game**, and is joint work with Justus Haucap. It shows another important field in the application of empirical methods: Large economic experiments. In this contribution the focus is on the analysis of individual behavior of economic agents in a class-room trust experiment. We are interested in the isolation of three personal characteristics on the experimental outcome: study progress, study major and gender. As we perform the analysis in class-room, it is essential to control for other important influence factors that could affect students' behavior, e.g. class-room size and risk preferences. These are the main reasons, why an empirical regression analysis is the only way to produce powerful results. To be exact, we analyze the behavior of 577 economics and law students in a simple binary trust experiment. While economists are both significantly less trusting and less trustworthy than law students, this difference is largely due to differences between female law and economics students. While female law students are already different in nature (during the first term of study) from female economists, the gap between them also widens more drastically over the course of their study compared to their male counterparts. This finding is rather critical as the detailed composition of students is typically neglected in most experiments.

Bibliography

Bresnahan, T. F. (1989). Empirical Studies of Industries with Market Power. *Handbook of Industrial Organization 2*, 1011–1057.

Einav, L. and J. D. Levin (2010). Empirical Industrial Organization: A Progress Report. *Journal of Economic Perspectives 24*(2), 145–162.

Schmalensee, R. (1989). Inter-Industry Studies of Structure and Performance. *Handbook of Industrial Organization 2*, 951–1009.

Chapter 2

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

Co-authored with Ulrich Heimeshoff

Textual and methodological contributions of Andrea-Louise Müller:

- Contribution to research idea and set-up
- All data collection
- Writing of the first draft of the paper
- Carrying out of the empirical analysis

____

Signature Co-author

2.1 Introduction

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

The terms AVR programs, Cash-for-Clunkers, car scrapping subsidies, or car scrappage schemes, all refer to the same phenomenon, namely vehicle owners receiving government subsidies for trading in an old vehicle for a new, in some cases more efficient car. The subsidies can be monetary, e.g. tax reductions on newly registered cars or non-monetary, e.g. public transport passes.[3] **Table 2.1** gives an overview of AVR programs in selected countries, enacted between 2008 and 2011. The programs differ considerably in budget volume, individual bounty, timing, duration and car age preconditions.

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

Whether the different car scrapping schemes worldwide were successful in the short and long run is an open research question and our paper tries to fill this gap. Empirical studies already conducted focus on single country evaluations of the car scrapping programs in various countries. The results are of different magnitude but in general no long-lasting effects of AVR-programs can be found in terms of output and employment.[4] The environmental impact is however positive, even if the cost-effectiveness of the car scrappage programs is taken into consideration,[5] because some car scrapping policies set the stage for pull-forward purchases of consumers, or

[1] See Haugh et al. (2010), Figure 5, p. 12.
[2] See Haugh et al. (2010), Figure 2, p. 9.
[3] See IHS Global Insight (2010) for a survey of the car scrapping programs in various countries.
[4] See Adda and Cooper (2000), Mian and Sufi (2012) and Li et al. (2013).
[5] See Knittel (2009), Hahn (1995) and Dill (2004).

Table 2.1: Overview of Characteristics of Scrappage Programs 2008-2011

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

Source: IHS Global Insight (2010); Haugh et al. (2010) and own research; Note: * estimated values; Portugal and Denmark not included as they have continuous AVR programs in place.

delay purchases as a result of anticipatory effects followed by a rapid decline after the end of the subsidy period.

This paper addresses two research questions:

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

(2) What is the overall effect of the different policies, including the periods following the subsidy?

In order to answer these questions we create a unique panel data set with country level data for 25 OECD countries. We first apply dynamic fixed effects estimation techniques to control for unobserved heterogeneity between countries. In line with previous studies we find a positive sales effect of the scrapping variable in our research setting, indicating an immediate boost in sales due to governmental car scrapping policies.

In a second part, we calculate the effects of the car scrapping schemes on sales to approximate possible welfare effects for Germany, the USA, the United Kingdom and South Korea by simulating a counterfactual situation treating the introduction of the car scrapping scheme as a structural break. Our simulation approach is related to the calculation of counterfactuals in macroeconomics. Macroeconomists often use estimated VAR models to create counterfactuals by forecasts based on pre-event data.[6] The main difference to our approach is that we use the estimated coefficients obtained from pre-scrappage time periods in order to create counterfactuals based on actual realizations of control variables during the treatment period. As a result, we do not run a pure forecast experiment as macroeconomists often do, but the similarity between our approach and theirs is using estimated coefficients from a baseline period for out-of sample predictions.

First of all, we estimate the model for the time period before the subsidy separately. Secondly, counterfactual sales are predicted using the estimated parameters of the before-subsidy period. Thirdly, we define the difference in sales between these two numbers as the effect of the Cash-for-Clunkers subsidy. Finally, we conduct the same analysis for the period after the scrappage scheme and analyze whether the positive boost in sales during the subsidy period outweighs the decline thereafter. The results of this section indicate a positive sales effect over the entire period for the four countries considered.

The remainder of our paper is organized as follows. The next section discusses the related literature, section 2.3 presents our data set, the empirical strategy and regression results, section 2.4 depicts simulation results and section 2.5 concludes.

[6] See Kilian (2008) for an analysis of counterfactual oil production in OPEC countries.

2.2 Literature Review

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

In the 1990s economists were concerned with the optimal design of the AVR programs. Studies in this line of research are Hahn (1995), Alberini et al. (1995) and Kavalec and Setiawan (1997). Alberini et al. (1995) derive a theoretical model of the reservation price (willingness to accept) as a function of various determinants. Their results for the 1992 Delaware scrappage program suggest that the selection problem for low scrappage incentives offered is quite substantial. For bounties below $500, car owners scrap vehicles in the poorest condition with a relatively short remaining life-time only, and it is very difficult for the policymaker to fulfill the environmental targets of the car scrapping policy. Hahn (1995) constructs a car scrappage supply curve and conducts a cost benefit analysis of the first Cash-for-Clunkers program in California in 1992. The author's findings suggest that cost-effectiveness of an AVR program can most easily be met by accompanying the scrapping policy with an inspection and maintenance program, and that the optimal scrapping incentive is $1,500 for the examined policy. The work also shows that a car scrapping policy is only optimal for a transitional time period. Kavalec and Setiawan (1997) use simulation techniques to evaluate the scrappage programs in Los Angeles from 1999 to 2010. They find that a program design targeting vehicles 20 years and older is more cost effective in terms of emission reduction than a design aimed at scrapping cars 10 years and older. In addition, the deteriorating effect on used car prices is less under the former design.

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

The first study conducting a policy evaluation by taking a counterfactual situation into account is the work of Adda and Cooper (2000). They use a dynamic micro level discrete choice model to examine the short- and long-run effects of two car scrappage subsidies in France, in 1992 and from 1994 to 1995. They calculate effects on output and public budget of these policies and find no positive output effect of car scrapping policies in the long run. The reason for this is the side-effect, that car scrappage programs change the distribution of car vintages.

A recent part of the literature is the conducting of evaluations of the 2009 CARS-/Cash-for Clunkers program in the United States. These studies are concerned with quantitative effects of the AVR policies in terms of output, employment and environmental aspects. Mian and Sufi (2012) use American cross-city variation in exposure to the Cash-for-Clunkers program. Their results suggest complete intertemporal substitution and therefore the effect of the subsidy was completely reversed after seven months.[7] Furthermore, they are unable to detect a positive employment effect for cities profiting more from the Cash-for-Clunkers program, compared to less exposed cities. Li et al. (2013) examine the 2009 American CARS Program using a Differences-in-Differences approach (DiD). In their set-up, Canada serves as a control group for the estimations. Their results suggest a positive boost in sales during the subsidy period and creation of jobs in the automotive industry. However, the environmental aspect as part of the target of the program comes at a high cost. One ton of $CO2$ reduction costs \$91 of government revenue. This finding is far less than the \$450 per ton $CO2$ found by Knittel (2009). However, the latter is not concerned with the counterfactual situation. To the best of our knowledge there is only the work of Leheyda and Verboven (2013) that analyzes car scrapping programs in a multi-country setting. They calculate the effects of scrappage subsidies using a data set on the level of car models for nine European countries. In order to compute the counterfactuals, the authors use a DiD approach with Belgium representing the control group. Their findings suggest that targeted schemes are more effective than non-targeted schemes in terms of sales effects, as well as the impact of these programs on the fuel economy being rather low.

The summary of the related literature shows that, to the best of our knowledge

[7]This result was recently reconfirmed by Hoekstra et al. (2014), who find a similar reversal effect of car sales regarding the CARS-program for a Texan sub-sample.

there is almost no analysis conducting a car scrapping evaluation for OECD countries. Except for that of Leheyda and Verboven (2013), the contributions so far focus on single-country case studies of one car scrapping policy. The aim of our paper is to close this gap in the literature using multi-country panel data. The advantage of our research setting is that we are able to control for unobserved heterogeneity between the different countries of interest, and therefore extract the effects of car scrapping policies on aggregate car sales. Furthermore, we add to the literature by simulating a counterfactual situation treating the introduction of the car scrapping scheme as a structural break; hence we are able to quantify effects of policies for different countries.

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

2.3 Empirical analysis

2.3.1 Data and summary statistics

Our panel data set consists of information of 25 OECD-countries collected on a monthly basis from January 2000 to December 2010. This choice of countries and time periods is due to data availability and the topic studied. Information on variables and data sources are presented in **Table 2.2**. It comprises data for new passenger cars' registrations in absolute values, *cars_absolute*, and in index format, *cars_index*, respectively. As the data in index format is available for more countries from the OECD, we favor this data for our regressions. The Accelerated Vehicle Retirement participation of each country is modeled as the *clunker* variable, taking the value one if such a scheme is in place during the observation period and zero otherwise.[8]

Furthermore, we incorporate additional control variables, such as the industrial production index, *industry_prod*, (as an important proxy for general economic conditions), the harmonized unemployment rate, *unemployment*, and the three-month short term interest rate, *interest_rate*, controlling for the overall economic situation

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

Table 2.2: Variable Descriptions and Data Sources

Variable	Description	Data Sources
cars_index	Index of new passenger car registrations, 2005 =100, s.a.	OECD (2011) Main Economic Indicators
cars_absolute	New Passenger car registrations/sales in absolute values, seasonally adjusted with dummies	ACEA (2011) for European countries (car registrations) and national statistical offices for non-European countries (car sales)
price_gas	Price of gasoline in Euro per liter, 3-months moving average process with equal weights, seasonally adjusted with dummies, historical monthly exchange rates used to convert currencies	Gasoline prices from European Commission (2011) and national energy agencies, Exchange rates available from Eurostat
clunker	Dummy variable, taking on the value of 1 if AVR- program is at place and zero otherwise	Haugh et al. (2010), IHS Global Insight (2010) and own research
industry_prod	Industry production index, 2005=100, s.a.	OECD (2011) Main Economic Indicators
unemployment	Harmonized unemployment rate in percent, s.a.	OECD (2011) Main Economic Indicators
interest_rate	Three months short term interest rates in percent, s.a.	OECD (2011) Main Economic Indicators

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

and financing requirements. These variables are available on a monthly basis from the OECD Main Economic Indicators database. Apart from that, we include gasoline prices as a three-month moving average, *price_gasoline*. We assume that the car purchasing decision on an aggregate level is taken in a rational manner and therefore depends on last month's actual gasoline price, and next month's future price, as a measure of gasoline price expectations.[9] The observed time trend is incorporated in a linear fashion. In a different specification we use monthly dummy variables as a robustness check.

With regard to the importance of time-series properties of the variables, as the underlying data set has quite a long time dimension (T= 132), we perform panel Unit-Root tests in order to avoid "spurious regressions". As the data set is unbalanced and Unit-Roots are probably of heterogeneous nature for the different countries, the Im-Pesaran-Shin-test tracing back to Im et al. (2003) is the natural choice. In the underlying data set the null hypotheses that all panels contain a Unit Root can be rejected for the dependent variable *pc_oecd* at the one percent significance level. Results of the Unit-Root tests for all the independent variables are summarized in **Table 2.A1** in the appendix.

Table 2.3: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
cars_index	3,119	99.06	19.47	18.1	237.2
cars_absolute	3,082	93,470	126,789	188	697860
clunker	3,300	0.09	0.29	0	1
price_gas	2,938	1.02	0.23	0.31	1.64
unemployment	3,300	7.38	3.62	1.8	20.6
interest_rate	3,217	3.57	2.40	0.07	19.82
industry_prod	3,296	98.77	11.73	59.3	156.7
time_trend	3,300	66.5	38.10	1	132
country_code	3,300	13	7.21	1	25

Table 2.3 gives an overview of summary statistics and section 3.2 specifies the empirical estimation strategy. The summary statistics show that the data set is of an unbalanced nature. This is mainly due to missing values for Eastern European countries (Czech Republic, Estonia, Hungary, Poland and Slovakia).[10] Furthermore,

[9] See Hicks (2009) for a similar modeling approach.

[10] The official car registrations on a monthly basis for these countries are only available from January 2003 on-wards. With respect to gasoline prices the same fact applies. Furthermore, data on gasoline consumer prices is also unavailable for Slovenia from 2004 on-wards.

it is evident that monthly car registrations over all countries fluctuate quite substantially, ranging from a minimum value of 18 percent of the January 2005 values to a maximum of 237 percent. The minimum numbers can be attributed to the distortions caused by the financial crisis, as all negative extreme values lie between late 2007 and the end of 2010. The same argument applies to the minimum values of the other control variables interest rate, industrial production and the maximum values of the unemployment rate variable. In contrast, the maximum numbers above 200 percent increase in car sales are evidence of the catching up process of automotive industries of Eastern European countries at the beginning of this century. The high fluctuation of gasoline prices is particularly explained through data-measurement, as we employ consumer end-prices including the extremely diverse tax component. The lowest gasoline prices, at only 31 (euro)-cents are found in the United States, whereas the peak value of 1.64 Euros per liter was charged in the Netherlands. For estimation, this variable is measured in natural logarithms further on.

2.3.2 Model

A standard approach in the literature would suggest evaluating the AVR programs with the help of DiD techniques.[11] In our data set it is almost impossible to apply this approach for various reasons. First, a crucial point for applying DiD would be to find appropriate control groups. A perfect control group would require that in absence of scrappage policies sales' patterns in two countries with and without AVR would evolve in exactly the same way or show very comparable trends. This assumption is not fulfilled in our data set, as can be verified by visual inspection of **Figure 2.A1** in the appendix. Aggregate automotive sales seem to be very different, even before the scrappage programs were implemented. Second, our data set has a long time dimension of 132 periods and includes a comparably small number of countries; therefore DiD can lead to biased standard errors, as serial correlation plays a more important role. This issue has been explored in detail in the work of Bertrand et al. (2004) and is of importance here. The problem of potential autocorrelation is addressed in section 2.3.4. In order to evaluate the direct effects of AVR programs during subsidy periods we therefore estimate a panel data model for all countries for 132 different time periods. As we expect unobserved heterogeneity between the 25 countries to be an important concern in the model for car demand, we use fixed effects within estimation techniques. One can think of transportation infrastructure (number of highways, possibilities of substitution between different options, other means of transportation...) and time-invariant car market preconditions as exam-

[11] See for example Angrist and Pischke (2009), pp. 227-233 for a discussion of this approach.

ples of unobserved heterogeneity. Such factors are not explicitly included in the regression, but affect demand for passenger cars in a given country. Furthermore, monthly car sales are highly path dependent and thus a dynamic specification of the car demand model is called for. Examples of similar modelling approaches are Ryan et al. (2009) and Ramey and Vine (1996). We therefore specify the fixed-effects model with a lagged dependent variable among the explanatory variables. In order to avoid endogeneity problems, we estimate the following regression model with instrumental variables:

$$\begin{aligned} cars\_index_{i,t} = \beta_0 + c_i + \beta_1 cars\_index_{i,t-1} + \beta_2 clunker_{i,t} \\ + \Sigma\beta_l time_{t,l} + \Sigma\beta_k(X_{i,t,k}) + \varepsilon_{i,t} \end{aligned} \tag{2.1}$$

$cars\_index_{i,t}$ represents the number of new passenger cars registered, our measure of car demand in country i at time t. c_i incorporates time-invariant country fixed effects. $cars\_index_{i,t-1}$ characterizes the monthly new car registrations in a given country, lagged by one month. The AVR is represented through the binary variable $clunker_{it}$ set to one if a scrappage program was at place in country i at time t and zero otherwise. Furthermore, we include a time trend $time_{t,l}$, represented by 131 monthly time dummies in the model. The vector $X_{i,t,k}$ includes the k control variables as described in the previous section. These are the industrial production index, gross gasoline prices as a three-month moving average in natural logs, unemployment rates and short term interest rates, entering the equation without logarithms as the values for some countries are close to zero and would cause the dependent variable to reach infinity. $\beta's$ denote the parameters to be estimated and ε_{it} is the error term assuming standard properties.

Equation (2.1) is estimated using fixed effects within transformation in order to eliminate the unobserved heterogeneity component, c_i. The model crucially relies on the fact that strict exogeneity holds, i.e. no explanatory variable is correlated with the error time at any point in time. Apart from that, the lagged dependent variable is clearly endogenous; therefore we instrument it with its own further lags.[12] First-stage regression and over-identifying restriction tests are given in the results section as evidence for the validity of our instruments.

We expect positive signs for the scrapping incentive as boosting sales during the subsidy period is the main objective of the AVR programs and is shown in the literature (see Mian and Sufi, 2012; Miravete and Moral, 2009; Dill, 2004). As stated in Goodwin et al. (2004), fuel price elasticities are negative. However, we estimate the model on an aggregate demand level and therefore the influence is not

[12] See Arellano and Bond (1991).

that clear cut. An increase in gasoline prices over three months could also lead to more car purchases as the need for a more fuel-efficient new car is more striking. The industrial production variable is supposed to have a positive effect on vehicle sales, as car demand is a normal good and highly correlated with the business cycle (see Ramey and Vine, 1996). The unemployment rate is presumed to enter the regression model with negative coefficients, as the household income is crucially dependent on labor market participation. Short-term interest rates measure the financing conditions for car purchases on credit; therefore we expect a negative sign for the corresponding coefficient. The dynamic component of the car demand specification is expected to exhibit a quite large positive influence on contemporary car demand (see again Ryan et al., 2009; Ramey and Vine, 1996).

2.3.3 Results

Table 2.4 presents the results of the second stage of our instrumental variables regression, grouped by the instruments used for the lagged dependent variable. In the following discussion of the results we refer to column (1) of **Table 2.4** unless otherwise stated.

Most importantly, our results detect the expected positive coefficient for the explanatory variable *clunker*, which is significant at the one percent level. Thus in the overall data set of 25 OECD countries we find the scrapping policy has a statistically significant positive effect on car sales. The coefficient of 3.6 index points is far less than the ones obtained in case-studies in the literature, but it is an average effect over all programs with very different durations and success levels, as we will point out in the next section.

Furthermore, our results indicate that the persistence of car registrations is an important issue in automobile sales models and underlines the importance of the dynamic specification. The lagged dependent variable of automobile sales is significantly positive over all instrumental variable specifications used and with a magnitude of 0.87 in line with the one found in the univariate study of Ramey and Vine (1996).

The interest rate enters the regression with the expected negative, but statistically significant coefficient. It controls for the overall financing conditions in the economy, indicated by the short-term three-month interest rate. This finding suggests that a decrease in the interest rate increases individuals' car demand. The unemployment rate and the industrial production further exercise significant effects on automotive sales. The coefficients are in line with previous research and show the strong correlation of the business cycle and the purchase of new vehicles.

Table 2.4: Second Stage Fixed-Effects Regression Results (for different sets of instruments)

	(1) cars_index	(2) cars_index	(3) cars_index
L.cars_index	0.8828***	0.9200***	0.8850***
	(0.000)	(0.000)	(0.000)
clunker	3.5072***	3.2659***	3.5229***
	(0.001)	(0.004)	(0.001)
l_price_gas	-0.0869	0.7033	0.4034
	(0.977)	(0.821)	(0.893)
unemployment	-0.4776**	-0.3051*	-0.4619**
	(0.025)	(0.093)	(0.022)
interest_rate	-0.5911***	-0.6079***	-0.6123***
	(0.008)	(0.007)	(0.007)
industry_prod	0.1185***	0.1042**	0.1179***
	(0.002)	(0.017)	(0.002)
time dummies	yes	yes	yes
No. of obs.	2762	2745	2745
No of groups	25	25	25
Adj. R^2	0.7252	0.7162	0.7233
Wald χ^2	64.3***	64.8***	65.5***
Hansen J statistic	1.989	0.514	2.622
	(0.1585)	(0.4735)	(0.2696)
Stock and Yogo	159.81	159.94	132.02
10% max IV size	19.93	19.93	22.3

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

Gasoline prices do not show significant effects on car registrations. This is the only variable that seems sensitive to the different sets of instruments, but the statistical insignificance does not change over all specifications.

Regressions (1) and (2) use two lags, and (3) three lags as instrumental variables. First stage regression results, given in **Table 2.5**, indicate relevance of the instruments, as they exhibit a positive and significant influence on lagged car sales at the one percent significance level. Furthermore, the Stock and Yogo (2005) critical values suggest that the loss in efficiency of the instrumental variable regression compared to ordinary least squares is less than 10 percent. Hence a problem of weak instruments can be neglected.[13] Moreover, over-identifying restrictions can be tested through Hansens's J-test. Over all specifications (1) to (3), the null hypothesis of the instruments being valid cannot be rejected at the one percent significance level. Therefore we argue that the instruments used are exogenous, i.e. uncorrelated with the error term.

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

2.3.4 Sensitivity analysis

In order to show the robustness of our results one of the first concerns that might come to mind is autocorrelation of the standard errors. This could lead to wrong inference, especially because we are working with monthly data. Our empirical model is a dynamic fixed effects model with instruments; therefore the standard panel data autocorrelation tests, such as the Wooldridge test for serial autocorrelation in panel data, do not work in this context.[14] Hence we employ time series autocorrelation tests, proposed by Cumby and Huizinga (1992) separately for each country in the data set. The advantage of this test is that it is applicable even with endogenous regressors, thus after IV-regressions and in specifications with heteroscedasticity in the data. **Table 2.A2** in the appendix presents the autocorrelation test results for each country. In 22 out of 25 countries the null hypothesis of non-autocorrelated standard errors cannot be rejected; thus we argue that autocorrelation of the errors is not a serious concern in our data set.

[13] This finding can be derived from the regression **Table 2.4**, which gives the Stock and Yogo test statistic and the critical value for 10 percent bias due to IV implementation. As over all specifications (1) to (3), the test statistic clearly exceeds the critical value, one can conclude that the bias of instrumental variable estimation is less than 10 percent compared to an OLS regression set-up.

[14] See Drukker (2003); Wooldridge (2010) for a discussion of the Wooldridge test.

Table 2.5: First Stage Regression Results (for different sets of instruments)

	(1) L.cars_index	(2) L.cars_index	(3) L.cars_index
clunker	9.2110***	9.8362***	9.4127***
	(0.000)	(0.000)	(0.000)
l_price_gas	0.2763	0.4531	0.7508
	(0.941)	(0.908)	(0.843)
unemployment	-2.3633***	-2.6747***	-2.3385***
	(0.000)	(0.000)	(0.000)
interest_rate	-1.8478***	-1.8918***	-1.9042***
	(0.000)	(0.000)	(0.000)
industry_prod	0.2713***	0.2949***	0.2739***
	(0.000)	(0.000)	(0.000)
L7.cars_index	0.3902***		0.3659***
	(0.000)		(0.000)
L8.cars_index	0.1647***	0.3212***	0.1288***
	(0.000)	(0.000)	(0.001)
L9.cars_index		0.1748***	0.0694
		(0.000)	(0.104)
time_dummies	yes	yes	yes
No. of obs.	2,762	2,745	2,745
No. of groups	25	25	25
Shea Partial R^2	0.5964	0.5615	0.5967
Wald χ^2	22.0***	19.6***	22.6***

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

Secondly, we also show the mean absolute percentage errors (MAPE) of the model selection period for all countries, to stress the high prediction quality of our preferred model specification. On average the MAPE is 5.57% and lies between 3.5% in Sweden and 10.6% for the Hungarian time-series. The corresponding values are also provided in **Table 2.A2** in the appendix.

A third concern that needs to be addressed is the question of anticipatory effects or post-treatment effects of the accelerated vehicle retirement policy.[15] In case that leads of the policy variable affect current values of car registrations, the estimates could be misleading due to anticipatory effects. In most countries in the data set it can be economically argued that those effects are probably not of serious concern in the Cash-for-Clunkers case, as the programs were legislated shortly after the discussion period. For example in Germany the whole legislation process took only four weeks.[16] Nevertheless we additionally check these concerns with a separate regression including two leads and two lags to formally control for anticipatory and post-treatment effects. **Table 2.A3** in the appendix shows the regression results for the fixed effects estimation described in 2.3.3. Neither the leads nor the lags of the policy variable *clunker* show any statistically significant influence on car sales in the current period. Therefore, we argue that the causality clearly runs from the clunker today to car sales today. No anticipatory reform effects seem to be present.

A fourth issue, one might worry about is the integration of time effects in our estimation of the *clunker* effect. The underlying data set is characterized by a long time-structure of 132 periods, therefore the appropriateness of a linear time trend can be discussed. We re-estimate the regression model with a linear time trend instead of time dummies. Results of this regression can be found in **Table 2.6** (3). The incorporated linear time trend suggests a slight decrease in car sales over the eleven-year time-span of interest, which is in line with real world observations.The clunker variable, the lagged dependent variable and the unemployment rate show almost no change. Among the control variables, the coefficient of the industrial production index increases and the gasoline price coefficient switches signs; however the effect is still insignificant. None of the tests performed concerning the performance of the instruments and the goodness of fit vary significantly between the two specifications. The Adjusted R^2, however, decreases slightly, indicating that the specification with

[15]See Angrist and Pischke (2009), pp. 237-241.

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

Table 2.6: Sensitivity Analysis

	(1) basic cars_index	(2) logged l_cars_index	(3) time trend cars_index
L.cars_index	0.8828***		0.8716***
	(0.000)		(0.000)
L.l_cars_index		0.9180***	
		(0.000)	
clunker	3.5072***	0.0376***	3.6813***
	(0.001)	(0.000)	(0.001)
l_price_gas	-0.0869	-0.0014	0.9142
	(0.977)	(0.962)	(0.711)
unemployment	-0.4776**	-0.0041**	-0.4998**
	(0.025)	(0.025)	(0.042)
interest_rate	-0.5911***	-0.0094***	-0.6736***
	(0.008)	(0.001)	(0.000)
industry_prod	0.1185***	0.0010***	0.1519***
	(0.002)	(0.004)	(0.000)
time_trend			-0.0371***
			(0.003)
time_dummies	yes	yes	no
No. of obs.	2762	2762	2762
No of groups	25	25	25
Adj. R^2	0.7252	0.8157	0.6983
Wald χ^2	64.3***	62.6***	749.5***
Hansen J statistic	1.989	1.637	1.878
	(0.1585)	(0.2008)	(0.1706)
Stock and Yogo	159.81	295.373	145.693
10% max IV size	19.93	19.93	19.93

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

time dummies is superior to the linear time trend approach.[17]

In this section we show that the model is robust to different specifications. In order to draw further inference we simulate a counterfactual situation in section four and predict the overall sales effect in total numbers.

2.4 Simulations

2.4.1 Strategy

The perfect set-up in quantifying the sales effect of the governmental policy in our panel data set would be taking the difference between the total sales number realized (treatment effect) and the counterfactual situation in absence of the policy, usually referred to as the control group. For case study work on the AVR programs, this DiD approach has been used to evaluate the effectiveness of the Cash-for-Clunkers program in the USA (see Mian and Sufi, 2012; Li et al., 2013). For a panel analysis of various countries, the detection of a single country or a group of countries serving as a control group is almost impossible.[18] Thus, we simulate a counterfactual situation during and after the end of the policy period and compare the realized outcomes and the simulated sales numbers.

As mentioned, AVR programs have been implemented in 15 OECD countries out of the 25 countries that we use for our estimation.[19] For those countries that had various schemes in place during the examination period, we use the latest AVR program, because our aim is to show the effectiveness of recent policies between 2008 and 2010. The simulation relies on our basic specification of the fixed-effects-regression described in detail in the previous section and the regression results of **Table 2.4** (1). The time period is divided into three parts: T=0 is the period before the subsidy takes place; T=1 refers to the subsidy period; and T=2 specifies the months after the subsidy.[20]

[17]Apart from that, one might wonder whether the model is robust to adding the logarithms of the dependent variable and therefore the lagged dependent variable, on the right-hand-side as well. As the results in **Table 2.6** (2) show, the coefficients are not affected at all.

[18]For countries which had car scrappage schemes we would have to find a corresponding control group with similar car-market characteristics that did not have an AVR program in place.

[19]More explicitly the countries having had an AVR are in alphabetical order: Austria, Canada, France, Germany, Greece, Ireland, Italy, Japan, Korea, Luxembourg, the Netherlands, Slovakia, Spain, the USA and the United Kingdom.

[20]For some of the countries under consideration the AVR-program was still effective in 2011, see **Table 2.1**. Examples are Canada and Ireland.

The simulation procedure itself consists of four steps:

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

2. Simulation of the counterfactual car sale values $\hat{pc}_{counter_{i,m}}$for T=1 and T=2 with an iterative procedure, using $\hat{pc}_{i,m}$ values of T=0 for the lagged dependent variable on the right-hand-side of the equation, instrumented by lags 7 and 8. E.g. for the first month, m, of period T=1 the equation to be calculated is as follows:
$\hat{pc}_{counter_{i,m}} = \beta_0 + c_i + \beta_1 \hat{pc}_{i,m-1} + \Sigma\beta_l time_{m,l} + \Sigma\beta_k(X_{i,m,k}) + \varepsilon_{i,m}$
Using this stepwise prediction scheme, we avoid calculating counterfactual values including realizations of variables from period T=0. Alternatively, we could have estimated a static version of our model, which would clearly cause problems with the data generating process. As a result, we decided to use the dynamic specification and run a stepwise forecasting scheme.

3. The sales effect during and after the subsidy in comparison to the counterfactual is calculated as the difference of realized sales number, $pc_{oecd_{i,m}}$ and the simulated variable $\hat{pc}_{counter_{i,m}}$. The obtained variables are named $pc_{scrap_{i,m}}$ (for T=1) and $pc_{after_{i,m}}$(for T=2)

4. In terms of comparison we take the means of $pc_{scrap_{i,m}}$and $pc_{after_{i,m}}$in order to compare the overall monthly effects of the AVR programs by taking into account the different durations, m, of the AVR programs in different countries i. The difference of these mean values finally states the overall effect of the scrappage subsidy until the last month in the data set
$pc_{overall_i} = \bar{pc}_{scrap_i} - \bar{pc}_{after_i}$

The results are discussed in the following section.

2.4.2 Simulation Results

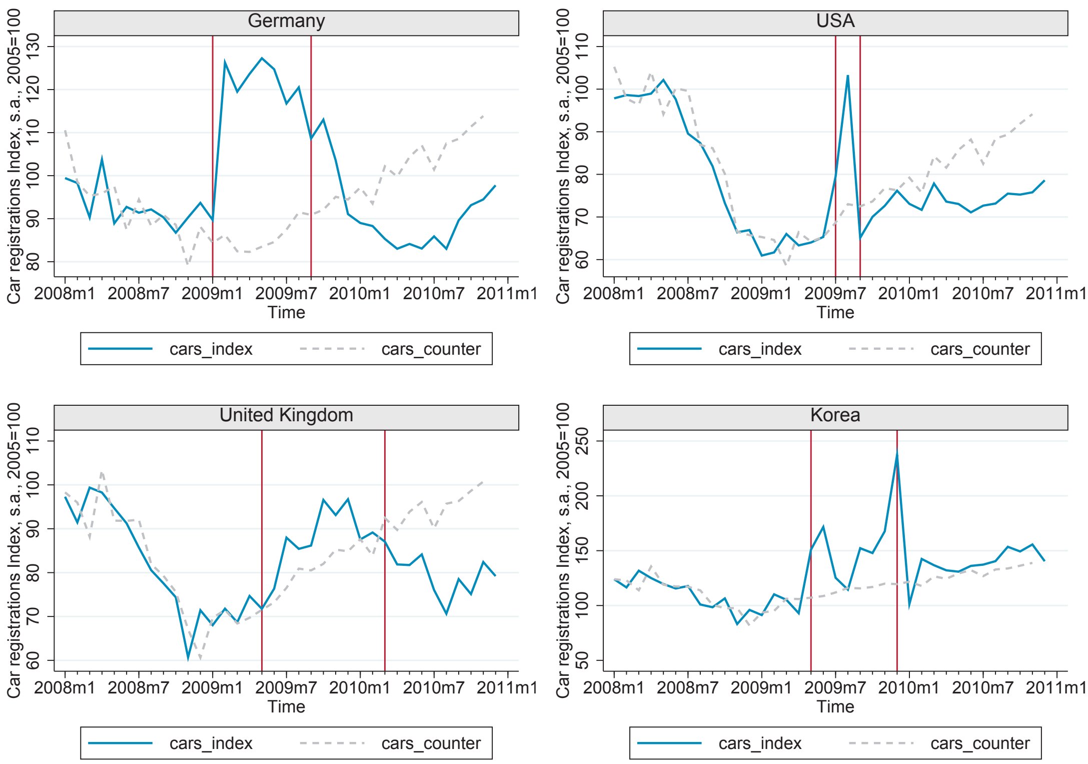
The presentation of the results is restricted to the most discussed AVR subsidies in South Korea, Germany, the USA and the UK. For each country we display the results graphically and in absolute numbers weighted by subsidy length and the overall budget of the scrappage program.[21]

[21] The calculated results for the remaining countries can be retraced in **Table 2.A4** in the appendix.

Graphical Presentation

Figure 2.1 illustrates the simulation results for each country separately. The solid line represents the observed number of car registration numbers per month, *pc_oecd*. The dashed line, however, describes the simulated counterfactual simulation results, *pc_counter*, during and after the subsidy period. For the pre-subsidy period *pc_counter* incorporates the fitted values of the fixed effects estimation. For graphical reasons the time span of the graphs is restricted and runs from January 2008 to December 2010, as the main period of interest is the one including the car scrappage policies implemented to fight declining vehicle sales as a result of the financial crisis. The beginning and end of the subsidy period is depicted through the two vertical lines. As shown in **Table 2.1** of the introduction the car scrappage subsidies in these four countries differ considerably in duration and budget.

Figure 2.1: Simulation Results for Germany, USA, UK and South Korea



On the one hand the simulation results illustrate similarities between the four countries. The effect of the car-scrappage scheme is visible as a big spike in the number of car registrations for all four countries and graphically support the fixed effects estimation results found in section 2.3.4. Furthermore, the drop after the end of the subsidy period can also be found for the country-quartet, although for Germany it is lagged by two months.

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

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

Calculation of the Size of the Policy Effects

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

In a first step, we calculate the overall effect of the car scrappage program for the four countries of interest. We therefore sum up the difference between the realized number of car-registrations and the predicted counterfactual ones. As these are all given in index format, we convert them to absolute values.[22] The estimated numbers are given in **Table 2.7**. Comparing these numbers to the number of cars that have been scrapped due to the corresponding governmental final reports,[23] we see that there are substantial differences, with the exception of the United Kingdom. The estimated number of scrapped cars is almost half the official number of cars reported for the USA and more than half for Germany and Korea. These first calculations indicate that a substantial number of cars would have been bought anyway because of normal replacement decisions or low interest rates, despite the automotive sales

[22]We do this with the help of the *pc_reg* variable. The data sources are incorporated in **Table 2.2**.

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

crisis.

We now turn to the question: What about the effect over the whole period, also taking the period after the subsidy into account? In order to answer this question **Table 2.7** depicts the simulation results in balance-sheet format for the United Kingdom, Germany, the USA and South Korea over the life-span of the AVR-program, *pc_scrap*, and the period thereafter, *pc_after*, as far as data is available. The first column represents the difference in passenger car registrations between the realized sales number and the simulated counterfactual situation during the subsidy period, and the second column indicates the identical difference, but for the months thereafter. As the subsidy period differs considerably between countries in terms of timing and duration, we weight the balance of each country with the number of months. The computed sales effect is displayed in index format with the mean value of the year 2005 representing 100. In order to give a more convincing picture, the difference in index formats is also recomputed in absolute car registration numbers per month. One can finally state that the positive effect of the car scrappage policy is way larger than the effect afterwards indicating some pull-forward effects from future periods. Even if the effect for some months after the subsidy is negative, compared to the counterfactual situation, we obtain a positive average car sales number for all four countries. For Germany and the United States the average sales effect over the whole period is almost the same number, roughly plus 38,000 cars per month. The Korean vehicle scrappage subsidy led to a slightly lower plus 37,630 cars per month, followed by the British program indicating 18,000 additional cars registered each month. One should additionally keep in mind that the overall budget in the United States was less than half of the German one,[24] but leading to the same number of new cars registered per month. Apart from that, South Korea's program seemed more successful than the British one, as it led to roughly 20,000 more cars sold, with a budget 150 million euros less. In **Table 2.A4** in the appendix an overview of the policy effects for the other countries can be found for the sake of completeness.

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

2.5 Conclusion

Using an OECD data set of 25 countries, we find a positive effect of car scrappage programs on overall car sales for as long as the subsidy is in place. This result is

[24]See **Table 2.1** for an overview of the countries budgets.

Table 2.7: Simulation Results for Germany, USA, UK and South Korea

	UK		Germany		USA		Korea	
	pc_scrap May 09- Mar 10	pc_after Apr 10 - Nov 10	pc_scrap Jan 09- Sep 09	pc_after Oct 09- Nov 10	pc_scrap Jul 09 - Aug 098	pc_after Sep 09- Nov 10	pc_scrap May 09- Dec 12	pc_after Jan 10- Nov 10
Difference in Car	482	-15,988	14,673	57,861	49,868	-33,965	43,934	-20,484
registrations	6,364	-24,677	110,640	23,441	139,712	-16,690	63,184	24,752
	23,295	-24,297	102,572	-9,378	**94,790**	-19,086	13,467	10,193
	9,156	-28,565	114,379	-22,802		-384	-1,578	7,760
	11,511	-50,981	121,111	-14,405		-28,326	36,777	1,594
	29,624	-36,194	111,002	-46,676		-18,489	31,073	3,892
	16,019	-47,842	81,068	-46,379		-29,593	47,991	10,541
	24,109	-37,251	80,241	-55,923		-36,769	118,202	7,579
monthly mean	-123	**-33,224**	49,062	-66,353		-58,572	**44,131**	19,829
values	10,686		**87,194**	-42,937		-78,786		12,999
	-10,944			-67,712		-45,283		16,879
	10,925			-52,410		-70,283		**8,685**
				-50,484		-63,861		
				-53,618		-76,729		
				-31,984		-84,564		
						-44,092		
Estimated sum of cars scrapped (total)	120,179	-265,795	784,748	-447,775	189,580	-661,380	353,050	95,534
Official no. of cars scrapped (total)	372,401	-	1,933,090	-	401,274	-	890,000	-
Balance in cars (monthly avg.)	-22,299		55,210		50,698		52,816	
Budget (in K Euros)	463,000		5,000,000		2,084,000		309,000	

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

Bibliography

ACEA (2011). *Monthly Provisional Vehicle Registration Figures.* Several Issues, Brussels.

Adda, J. and R. Cooper (2000). Balladurette and Juppette: A Discrete Analysis of Scrapping Subsidies. *Journal of Political Economy 108*(4), 778–806.

Alberini, A., W. Harrington, and V. McConnell (1995). Determinants of Participation in Accelerated Vehicle Retirement Programs. *RAND Journal of Economics 26*(1), 93–112.

Alberini, A., W. Harrington, and V. McConnell (1996). Estimating an Emissions Supply Function from Accelerated Vehicle Retirement Programs. *Review of Economics and Statistics 78*(2), 251–265.

Angrist, J. and J. Pischke (2009). *Mostly Harmless Econometrics: An Empiricist's Companion.* Princeton University Press, Princeton: NJ.

Arellano, M. and S. Bond (1991). Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations. *Review of Economic Studies 58*(2), 277–297.

BAFA (2010). *Abschlussbericht Umweltprämie.* Bundesamt für Wirtschaft und Ausfuhrkontrolle, Eschborn.

Baltas, N. and A. Xepapadeas (1999). Accelerating Vehicle Replacement and Environmental Protection: The Case of Passenger Cars in Greece. *Journal of Transport Economics and Policy 33*, 329–349.

Bertrand, M., E. Duflo, and S. Mullainathan (2004). How Much Should We Trust Differences-in-Differences Estimates? *Quarterly Journal of Economics 119*(1), 249–275.

Canis, B., J. Grimmett, M. Platzer, and B. Yacobucci (2010). Accelerated Vehicle Retirement Programs in Japan and South Korea: Background for Congress. *CRS Report for Congress 7-5700-R 41462.*

Clowers, A. (2010). Auto Industry: Lessons Learned from Cash for Clunkers Program. *United States Accountability Office GAO-10-486.*

Cooke, P. (2010). Recession, Scrappage and Sustainable Recovery. *Working Paper*, The University of Buckingham.

Cumby, R. and J. Huizinga (1992). Testing the Autocorrelation Structure of Disturbances in Ordinary Least Squares and Instrumental Variables Regressions. *Econometrica 60*(1), 185–195.

Dill, J. (2004). Estimating Emissions Reductions from Accelerated Vehicle Retirement Programs. *Transportation Research, Part D: Transport and Environment 9*(2), 87–106.

Drukker, D. (2003). Testing for Serial Correlation in Linear Panel-Data Models. *Stata Journal 3*(2), 168–177.

European Commission (2011). *Oil Bulletin*. Several Issues, Brussels.

Goodwin, P., J. Dargay, and M. Hanly (2004). Elasticities of Road Traffic and Fuel Consumption with Respect to Price and Income: A Review. *Transport Reviews 24*(3), 275–292.

Hahn, R. (1995). An Economic Analysis of Scrappage. *RAND Journal of Economics 26*(2), 222–242.

Haugh, D., A. Mourougane, and O. Chatal (2010). The Automobile Industry in and Beyond the Crisis. *OECD Economics Department Working Papers 745*, OECD Publishing, Paris.

Hicks, M. (2009). Some Preliminary Evaluation of the Cash for Clunkers Program. *CBER Policy Brief November*, Ball State University.

Hoekstra, M., S. L. Puller, and J. West (2014). Cash for Corollas: When Stimulus Reduces Spending. *Working Paper 20349*, National Bureau of Economic Research.

IHS Global Insight (2010). Assessment of the Effectiveness of Scrapping Schemes for Vehicles - Economic, Environmental and Safety Impacts. *Prepared for European Commission DG Enterprise and Industry*.

Im, K., M. Pesaran, and Y. Shin (2003). Testing for Unit Roots in Heterogeneous Panels. *Journal of Econometrics 115*(1), 53–74.

Kavalec, C. and W. Setiawan (1997). An Analysis of Accelerated Vehicle Retirement Programs Using a Discrete Choice Personal Vehicle Model. *Transport Policy 4*(2), 95–107.

Kilian, L. (2008). Exogenous Oil Supply Shocks: How Big are They and How Much Do They Matter for the US Economy? *Review of Economics and Statistics 90*(2), 216–240.

Knittel, C. (2009). The Implied Cost of Carbon Dioxide Under the Cash for Clunkers Program. *CSEM Working Paper 189*, Berkeley, CA.

Leheyda, N. and F. Verboven (2013). Scrapping Subsidies During the Financial Crisis: Evidence from Europe. *ZEW Discussion Paper 13-079*, Mannheim.

Li, S., J. Linn, and E. Spiller (2013). Evaluating Cash-for-Clunkers: Program Effects on Auto Sales and the Environment. *Journal of Environmental Economics and Management 65*(2), 175–193.

Mian, A. and A. Sufi (2012). The Effects of Fiscal Stimulus: Evidence from the 2009 Cash for Clunkers Program. *Quarterly Journal of Economics 127*(3), 1107–1142.

Miravete, E. and M. Moral (2009). Qualitative Effects of Cash-For-Clunkers Programs. *CEPR Discussion Papers*, DP 7517.

OECD (2011). *Main Economic Economic Indicators*. Several Issues, Paris.

Ramey, V. and D. Vine (1996). Declining Volatility in the U.S. Automobile Industry. *American Economic Review 96*(5), 1876–1889.

Ryan, L., S. Ferreira, and F. Convery (2009). The Impact of Fiscal and Other Measures on New Passenger Car Sales and CO2 Emissions Intensity: Evidence from Europe. *Energy Economics 31*(3), 365–374.

Stock, J. and M. Yogo (2005). Testing for Weak Instruments in Linear IV Regression. In D. Andrews and J. Stock (Eds.), *Identification and Inference of Econometric Models: Essays in Honor of Thomas Rothenberg*, pp. 80–108. Cambridge University Press.

Van Wee, B., H. Moll, and J. Dirks (2000). Environmental Impact of Scrapping Old Cars. *Transportation Research, Part D: Transport and Environment 5*(2), 137–143.

Wooldridge, J. (2010). *Econometric Analysis of Cross Section and Panel Data* (2 ed.). The MIT Press, Cambridge: MA.

Appendix

Table 2.A1: Panel Unit Root Tests

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

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

Table 2.A2: Autocorrelation Tests and Prediction Quality of Individual Country Time-Series

	Autocorrelation test		Prediction quality
Country	Test statistic	p-value	MAPE
Australia	0.029	0.862	3.9%
Austria	0.670	0.412	5.5%
Belgium	0.067	0.795	4.7%
Canada	0.018	0.891	3.6%
Czech Republic	0.088	0.766	5.0%
Estonia	0.896	0.343	7.1%
Finland	0.279	0.597	7.9%
France	0.095	0.757	4.4%
Germany	0.234	0.628	4.8%
Greece	1.193	0.274	7.0%
Hungary	0.011	0.913	10.6%
Ireland	0.105	0.745	9.1%
Italy	0.082	0.773	5.8%
Japan	0.488	0.484	4.7%
Korea	5.208	0.022**	7.7%
Luxembourg	0.195	0.658	5.3%
Netherlands	0.418	0.517	7.1%
Poland	1.790	0.18	5.3%
Slovakia	0.033	0.853	8.0%
Slovenia	0.100	0.75	3.9%
Spain	0.030	0.861	4.6%
Sweden	9.110	0.002***	3.5%
Switzerland	13.770	0.000***	4.8%
UK	0.249	0.617	4.2%
USA	0.159	0.689	4.3%

Notes: Cumby-Huizinga autocorrelation tests used with Ho: non-autocorrelated at order 1; *** statistically significant at the 1% level; ** statistically significant at the 5% level; * statistically significant at the 10% level. MAPE= Mean Absolute Percentage Error; Underlying regression: IV Regression unlogged with Lags 7 and 8 as instruments.

Table 2.A3: Testing Anticipatory Effects

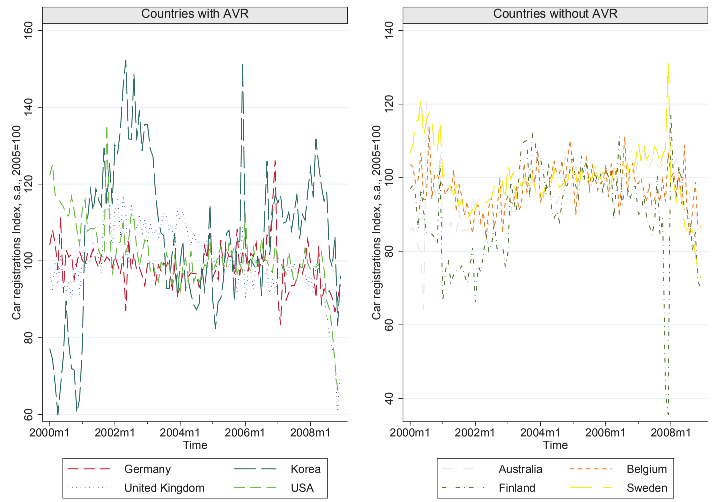
	cars_index
L.cars_index	0.8856***
	(0.000)
L2.clunker	-2.4428
	(0.379)
L.clunker	-7.3558
	(0.209)
clunker	14.1737**
	(0.021)
F.clunker	-1.1980
	(0.755)
F2.clunker	-1.5751
	(0.413)
l_price_gas	-0.2121
	(0.942)
unemployment	-0.4899**
	(0.017)
interest_rate	-0.6203***
	(0.006)
industry_prod	0.1054***
	(0.009)
No. of obs.	2,738
No of groups	25
Adj. R^2	0.7231
Wald χ^2	63.2***
Hansen J statistic	1.774
	(0.1829)
Stock and Yogo	172.53
10% max IV size	19.93

Notes: P-values in parentheses; Heteroscedasticity robust standard errors reported; One period lagged dependent variable is instrumented by its Lags 7 and 8; F-Test of F.clunker and F2.clunker shows a χ^2 of 1.33 with a corresponding p-value of 0.51.
*** statistically significant at the 1% level; ** statistically significant at the 5% level;
* statistically significant at the 10% level.

Table 2.A4: Simulation Results (Other Countries)

	Austria		Greece		Netherlands		Ireland	
	pc_scrap	pc_after	pc_scrap	pc_after	pc_scrap	pc_after	pc_scrap	pc_after
Sum of cars scrapped	18,698	-32,528	-20,962	-97,573	-90,066	-	1,733	-
Balance in cars	2,641		-15,119		-4,740		158	
	Spain		Luxembourg		Italy		Canada	
	pc_scrap	pc_after	pc_scrap	pc_after	pc_scrap	pc_after	pc_scrap	pc_after
Sum of cars scrapped	664,983	74,000	-414	-3258	486,586	-95,835	-14,338	-
Balance in cars	47,412		-836		4,804		-652	
	France		Japan		Slovakia			
	pc_scrap	pc_after	pc_scrap	pc_after	pc_scrap	pc_after		
Sum of cars scrapped	1,001,771	-	-188,204	-405,973	19,642	8,038		
Balance in cars	47,740		-213,442		2,695			

Figure 2.A1: Car Registrations for Representative Countries with and without AVR



Chapter 3

The Effects of the German Car Scrappage Scheme: A Time Series Approach

Co-authored with Veit Böckers and Ulrich Heimeshoff

Textual and methodological contributions of Andrea-Louise Müller:

- Contribution to research idea and set-up
- 50 percent of all data collection
- Writing of sections 3.1 to 3.3 of first draft
- Creation of all figures
- Complete revise of the empirical estimation December 2014

Signature Co-author 1

Signature Co-author 2

3.1 Introduction

In autumn 2008 the effects of the financial and economic crisis spilled over to Germany and led to a contraction in GDP of 2.0 percent in the fourth quarter of 2008.[1] Against this background, fiscal policy interventions were called for on a broad basis and through all political parties. This consensus finally culminated in the adoption of two large scale fiscal policy packages in late 2008 and in early 2009. The latter included the German Car Scrappage Program or "Cash for Clunkers". A subsidy of 2,500 € was granted to private consumers for scrapping a used car and buying a new one.

Certain aids or subsidies for car manufacturers have a long tradition in EU member countries. However, the aim of such programs has usually not been related to the effects of business cycles but aimed at aiding the automotive sector in general.[2] The German scrappage policy was extensively discussed in public and among economists. Waldermann (2009) summarizes the leading German economists' and lobbyists' opinion by stating that all opposed to this type of fiscal policy intervention. In more detail, the concerns refer to the favoritism of the automotive industry over other industry branches, the courting of specific voters in an election year[3] and that pull-forward effects will negate the positive contemporary outcome of the policy. Despite the growing debate about the German Cash for Clunkers program, it has not been empirically evaluated to the best of our knowledge. The aim of this paper is to fill this gap using a time-series approach simulating the counterfactuals, taking general economic conditions into account. Our study focuses on answering the following three questions:

1. Did the policy stimulate car sales?
2. Did consumers bring their car consumption forward from the future?
3. If there is an effect, does it vary between specific segments?

Our results suggest that the predicted car registration numbers for the time after the Accelerated Vehicle Retirement (AVR) program ended, are only slightly above the realized ones for the years 2010 and 2011, i.e. pull-forward effects have been only modest. Second, the policy seems to have had an overall positive effect as it led to

[1] See DESTATIS (2014): p.11.

[2] See Nicolini et al. (2013) for an empirical analysis of state aid for car manufacturers in EU member countries.

[3] See Goerres and Walter (2010) for an interesting answer to this question. They show that the AVR program had no substantial effect on voting behavior in Federal elections.

almost one million additional car registrations in comparison to the counterfactual situation. And third, results based on data from 2007, which is roughly one year before the financial crisis had an impact on Germany, suggest that the automobile industry may have been not as profoundly struck by the crisis as usually assumed. This is also in line with research on the nature of the financial crisis, which provides evidence, that the effects of the last financial crisis are not fundamentally different compared to former financial crises.[4] The remainder of the paper is as follows. Section 3.2 discusses the related literature on evaluations of car scrapping subsidies and part 3.3 describes the features and background of the German Cash-for-Clunkers program. Section 3.4 is dedicated to the empirical strategy, presentation of the data as well as the results. The last part concludes and gives an outlook on further research opportunities.

3.2 Literature Review

The literature on AVR programs started in the 1990s with the work on optimal policy design of car scrapping schemes.[5] This literature was supplemented by analyses concerned with environmental impacts of the policy, as the programs of the 1990s were more concerned with fighting pollution than boosting economic growth.[6]

More related to our approach is the more recent policy evaluation literature which analyzes the sales effects of different programs as case studies. This line of research was initially started by Adda and Cooper (2000), who measure and evaluate the long term effects of two French car scrapping programs of the 1990s using discrete choice methods based on a microlevel dataset. They find transitory sales effects shortly after the program and negative effects in the long run. Additionally, they point out that the policy effects were negative from a Governmental budget point of view, as the expenditures are not fully compensated through additional tax revenues. This approach is partly carried on by Schiraldi (2011). He extends the structural discrete choice model to a full equilibrium structural model, including examination of the used car market to analyze the effects of an Italian car scrapping policy of the 1990s. Results suggest a smaller sales effect than the one reported by Adda and Cooper (2000).

Apart from the discrete choice approach, there are recent studies analyzing the American CARS program of 2009 in terms of output and employment with aggregate data, which is similar to our data basis. Mian and Sufi (2012) and Cooper et al.

[4] See Reinhart and Rogoff (2009) for a discussion.
[5] See Hahn (1995); Alberini et al. (1995); Kavalec and Setiawan (1997).
[6] These papers are recently reviewed by Van Wee and De Jong (2011).

(2010) are studies in this line of research. Environmental effects of the CARS-program were additionally investigated by Li et al. (2013). Mian and Sufi (2012) and Li et al. (2013) are applying Difference-in-Differences (DiD) techniques, use car registration data and find a short term boost in sales for the two months policy period from August to September 2009 followed by a substantial decline via pull-forward effects after the program. The latter approach evaluates the policy using the Canadian car market as the control group, the former American cross-city variations in terms of participation rates in the program. Mian and Sufi (2012) show that seven months after the end of the policy the positive effect was completely reversed, so that the policy was even shorter lived than the findings in Li et al. (2013) suggest, where positive sales effects until December 2009 are reported. Furthermore, positive effects on employment are discovered in cities with higher exposure to the CARS-program in Mian and Sufi (2012) and are confirmed by Li et al. (2013).[7] Cooper et al. (2010) use a Two-Stage-Least-Squares (TSLS) time-series approach for simulating the counterfactual situation of no Cash-for-Clunkers program during the two months of the policy and two months afterwards. Their results suggest a boost in sales of 395,000 aditional cars and 40,200 new jobs and even net governmental revenues of 1.2 billion dollars.

Heimeshoff and Müller (2013) analyze the overall performance of the 2009-2010 programs in a multi-country panel by calculating a counterfactual situation based on estimates of dynamic demand functions of new car registrations. The results suggest different but overall positive sales effects with small pull-forward effects in most countries, concluding that the success of car scrapping policies relies heavily on timing, budget and durations of the AVR-programs. Leheyda and Verboven (2013) calculate the effects of scrappage subsidies using a dataset on the level of car models for nine European countries including monthly data from 2005 to 2011. They find that targeted schemes prevented a total car sales reduction of 17.4%, whereas non-targeted schemes only led to a sales drops of 14.8%. Compared to the effectiveness of the subsidies with regard to sales effects, the impact on average fuel consumption is rather low. They estimate that fuel economy would have been only 1.3% higher without the scheme in countries with targeted schemes and 0.5% higher in countries with non-targeted schemes.

For Germany, two reports present descriptive statistics on the car scrappage scheme. The governmental agency that was responsible for the implementation of the program, "Bundesamt für Wirtschaft und Ausfuhrkontrolle" (BAFA), describes the application process as well as numbers of cars scrapped and bought during

[7] Above all, they calculate a cost of $92 for each avoided ton of CO_2, a value that is quite high compared to other environmental policy programs.

the subsidy period in BAFA (2010). Additionally, Höpfner et al. (2009) report first effects of the car scrappage program in terms of environmental impacts using preliminary data from January 2009 until August 2009. These contributions do not take into account counterfactual situations, but solely depict sales patterns of all cars bought during the treatment period, without distinguishing between additional cars bought and vehicles purchased anyway. Besides, Kaul et al. (2012) examine the discounts granted to car purchasers during the German Cash-for Clunkers Program using a micro transaction dataset for six randomly chosen car dealers. Their work shows that in the larger car segments bigger discounts on list prices were given, as compared to the small market segments during the policy period.

Our contribution to the literature on car-scrapping evaluations is twofold. First of all, we focus on the German Cash-for-Clunkers program, for which, as to the best of our knowledge, there is no empirical study evaluating the effects of this subsidy in detail. Secondly, we use time-series econometric methods to construct the counterfactual sales pattern for each relevant segment in absence of the policy. This approach is chosen as a rather parsimonious way for counterfactual simulation. However, there are good reasons why to choose a time series approach instead of other econometric models for prediction. In empirical macroeconomics it has been shown, that quite simple time series models often outperform large macroeconometric models in terms of forecasting performance. This does not mean that structural models might not be superior in terms of estimating causal effects under certain circumstances, but for our purposes a time series approach is well suited.[8] Apart from that, automotive sales and registration patterns exhibit strong dynamic effects. Therefore, neglecting lagged dependent variables in the model misses an important aspect of analyzing car demand.[9]
The following section discusses the German Cash for Clunker Program in detail.

3.3 The German Scrappage Program

Table 3.1 shows the most important aspects of the German AVR program to get a better impression how the program was designed and what have been the Government's intentions implementing the program.

As a method to counterbalance the negative private consumption effects of the financial crisis, the German government agreed upon two large fiscal policy inter-

[8]See e.g. Diebold (1998) for a discussion of different paradigms of forecasting in macroeconomics.

[9]For a discussion of the path dependency of new car registrations see Ramey and Vine (1996) and Ryan et al. (2009).

Table 3.1: The German Cash-for-Clunkers Program "Umweltprämie"

Timing	January 27, 2009 (start of application) until September 2, 2009 (budget exhausted)
Budget	5 billion Euros
Incentive	2,500 Euros per car
Old car preconditions	1. Minimum age of nine years 2. Car had to be registered with the applicant for at least one year
New car preconditions	1. Fulfill emission standard Euro 4 2. New car or vehicle registered with another person or company for not more than 14 months (Jahreswagen)
Other features	1. Private consumers only 2. Short notice of policy
Aim	1. Reducing the age of the car fleet 2. Economic stimulus

Source: Own table, based on BMWi (2009).

vention packages called "Konjunkturpaket 1" on November, 5 2008 and two months later "Konjunkturpaket 2" on January, 14 2009. The German Cash-for Clunkers program was part of the second fiscal policy package and amounted to a budget of 1.5 billion of the 50 billion € package, so roughly 3 percent. As applications for the scrappage subsidy intensifies,[10] the German parliament decided to increase the overall budget of the policy to 5 billion € end of March 2009. This was after France, the second largest program that was implemented in Europe during the 2009/2010 automotive sales crisis.[11]

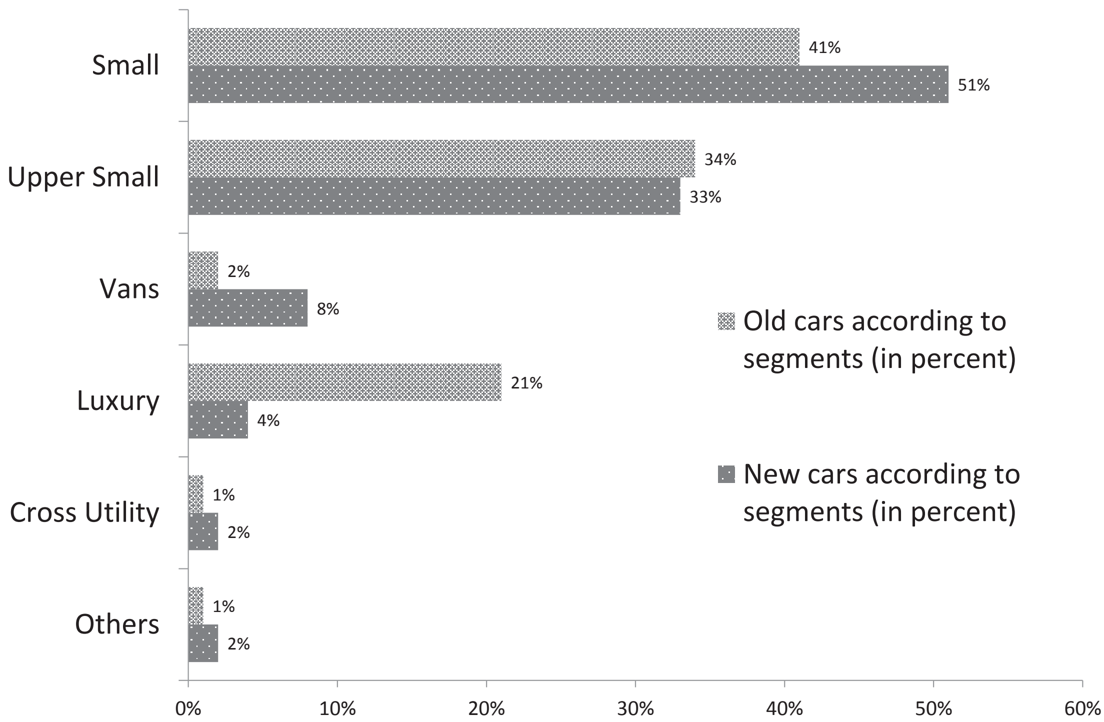
In contrast to other scrappage subsidies, like the American CARS scheme, and despite its official name, "environmental premium", the new car purchase was not tied to any environmental requirements. The demanded emission class *Euro 4*, that had to be fulfilled was mandatory for new car purchases in the EU from January 2006, onward. Moreover, under the German program the car did not have to be entirely new, but a car registered to another person for at most 14 months did also qualify for the governmental subsidy of 2,500 € per vehicle. Additionally, the new

[10]During the peak of consumer demand BAFA registered 270,000 incoming calls per day, (see BAFA, 2010, p.9) and received 7,000 applications per day on average, (see BAFA, 2010, p.7).

[11]See Heimeshoff and Müller (2013) for an overview of other policies conducted throughout this period.

car had to be continuously registered with the applicant for at least one year. Policy requirements stated a minimum age of nine years for the car scrapped, this led to an eligible pool of 17 million cars or 41 percent of all cars registered in Germany.[12] This incentive was only guaranteed to private car owners, commercial entities were excluded from the AVR program.

Figure 3.1: Cars Bought and Scrapped under the Scrappage Program



Source: Own graphic based on BAFA (2010);
Note: Upper luxury and sport utility segment are not included, as no cars of that segment were bought or scrapped under the scheme. Small segment is composed of so-called small and mini cars. Luxury is a combination of the lower luxury and middle luxury segment.

The final report BAFA (2010) stated two main effects of the German Cash-for-Clunkers Program. First a downsizing effect in car size could be noted, as especially the smallest car segment gained most in sales. Descriptive statistics of an intra-

[12]The numbers are taken from Höpfner et al. (2009): p.2.

segment comparison of scrapped cars and new car purchases show that small cars have gained ten percentage points under the policy. These effects are depicted in Figure 3.1. Luxury cars lost 17 percentage points and vans gained six. Car registration shares did not change considerably for sports utility, others and upper small market segments. Upper luxury cars and sport utility vehicle sales were not affected by the Accelerated Vehicle Scrappage program. As a result, there are no cars of this segment within the group of all cars bought and scrapped under the policy.

Before the empirical strategy is explained in the next section, the timing of the intervention has to be discussed in some detail. As stated before, the Cash-for-Clunkers program passed parliament January 14, 2009. The start of application was possible from January 27, 2009, so roughly two weeks afterwards. For the empirical implementation it is important, that the car scrappage subsidy was not extensively discussed or anticipated before January 2009, as this might lead to a bias caused by anticipatory effects [13] and the policy timing variable would have to be set to a different month in order to capture all policy effects. However, this is not an important issue here, because the period between the (public) discussion of the policy and the of implementation was rather short. To support our assumption, we use Google's trend search volume index as an indicator of public awareness. We conducted a search for the two German words relating to the policy "environmental premium" ("Umweltprämie") and "scrappage premium" ("Abwrackprämie").[14] Figure 3.A1 in the appendix shows that the time span for a potential Ashenfelter's dip was very short and no peak in search volume is visible for November and December 2008. As the timing between registering for the scrappage program with Bafa and the registration of the new car is more or less impossible to be completed within four working days,[15] we set the beginning of the scrappage program in our analysis to February 2009. The end of the German accelerated vehicle retirement program is not as clear cut. While the budget was exhausted on September 2, 2009, the period of new car registrations attributable to the scrappage program ends later. As the car industry faced substantial delivery delays at that time, due to the high demand for small cars, we set the end of the policy to December 2009, as the shortest delivery

[13]This anticipatory effect is also called Ashenfelter's dip. Ashenfelter (1978) analyzed the effect of training programs on earnings and found a potential bias caused by an individual's change in behavior just shortly before the treatment period. The change can be attributed for example to anticipation. This anticipation leads to an adaption in behaviour, e.g. lower effort or work load.

[14]The official designation of the premium was "environmental premium" but in the media the colloquial name became "scrappage premium".

[15]January, 27, the first day of possible registration with Bafa, was a Tuesday in 2009.

period was three months at that time.[16]

3.4 Empirical Approach

3.4.1 Data

In order to evaluate the German Cash for Clunkers Program empirically, we use data on new car registrations on the segment level. This data is available from the German Federal Transport Authority (KBA) on a monthly basis from March 2001 to December 2011. Additionally, we include the industrial production index (2005=100) from the German Federal Statistical Office (Destatis) into our dataset. For further specifications of the model we also use the monthly gasoline price including taxes (in € per liter) from the European Commission's oil bulletin and the three-months short term interest rate (in percent) from the OECD Main Economic Indicator database. All variables used are not seasonally adjusted, as this is done including seasonal effects into the regression to obtain comparable results for all estimates.

Table 3.2: Descriptive Statistics

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
small	130	38,981	18,483	22,042	141,686
upper small	130	32,626	11,640	17,835	90,981
industrial production	130	102.5	9.4	85.7	122.2
interest rate	130	2.66	1.29	0.64	5.11
gasoline price	130	1.22	0.17	0.95	1.66

Three alterations were made to the original data mentioned above. First, as stated in the previous section, commercial car holders did not qualify for the scrappage bounty and are excluded from total car registrations. The KBA introduced this differentiation on the segment level in January 2008, so there is no data available for previous months. To replace the missing data, the percentage of private car holders is assumed to be constant for March 2001 until December 2007. This share

[16]Official data on car delivery times for Germany is not available at the segment level and as monthly data. We therefore choose three months as the delivery time in 2009 was 16 to 17 weeks for top sellers of the small market segment: like Mazda 2, Ford Ka and VW Fox. These numbers are taken from Focus (2009). Furthermore Kaul et al. (2012) use the same specification and show that there were substantial sales attributed to the AVR program in their dataset in October, November and December 2009 (see Kaul et al., 2012, p.33).

is computed for the subsequent years 2008, 2010, 2011 and 2012. The year 2009 is left out, as this period was distorted by the AVR program. The corresponding share of private car holders in the small car segment is 51.4 percent with a variance of 1.4 percent from 2008 to 2011 (excluding 2009) and 42.5 percent and a variance of 5.2 percent for upper small cars.
Second, the absolute value of the industrial production cannot be used because it may suffer from endogeneity as 12.34 percent[17] of the overall value is due to production of automobiles and automotive parts. These numbers are deducted from the total industrial production aggregate, so that the altered industrial production index could serve as an exogenous control variable in the time-series regression. Third, our following analysis focuses on the the two car segments small and upper small instead of all eight, as they amount to 84 percent of all new cars bought under the car scrappage policy (see Figure 3.1).

3.4.2 Methodological Considerations

We are interested in the effects of the German AVR-program on car demand of German consumers. Therefore, it is essential to estimate the counterfactual situation without an AVR scheme to calculate demand effects. In Economics there is a large literature on the estimation of treatment effects, where the so called Difference-in-Differences-estimation (DiD) is a leading approach to estimate effects of policy measures (see Angrist and Pischke, 2009, p.227ff.). This approach is especially applied in fields as Labor and Population Economics. The crucial point in applying DiD is the construction of appropriate treatment and control groups. AVR schemes have been implemented in many OECD countries and even transition economies. As a result, it is rather difficult to find a sufficient number of countries for the control groups which are similar in structure to the countries in the treatment group, but have not implemented scrappage schemes. Note, that subjects in the control group should face similar trends as subjects in the treatment group. A simple graphical inspection of new car registrations in several OECD countries shows, that market trends in automobile sectors are rather different (see Heimeshoff and Müller, 2013, p.30). Furthermore, our analysis is based on a rather long time series of 130 periods, resulting from the monthly frequency of our data, which is difficult to handle in a DiD setting. Bertrand et al. (2004) show that the size of the standard errors in DiD-studies heavily depends on whether serial correlation exists in the data. Obviously, serial correlation is a more serious problem in long than in short panels, which is the case in our dataset. Finally, our analysis is a case study for the German

[17]The numbers are taken from DESTATIS (2011): p.12.

automobile sector. Therefore, it is not possible to create a panel dataset on which a DiD-study could rely. As a result, an international dataset is required to run such estimations, but even here DiD methodology is not always appropriate (see Heimeshoff and Müller, 2013). Additionally, a simple DiD would not be sufficient, because we would need to apply a difference-in-difference-in-difference estimator to obtain the results of the AVR scheme because of the short duration of the AVR program. Otherwise we would miss potential pull forward effects and our estimates could be positively biased.

We calculate counterfactuals using forecasts based on time series models estimated using the pre-program period in our dataset. This approach is well known in macroeconomics, where forecasts based on time series models as VARs are often used to estimate counterfactual situations.[18] This methodology is well suited for our case study of the effects of the German AVR program on new car registrations.

3.4.3 Estimation Strategy and Calculation of the Counterfactual

This section lays out our identification and estimation strategy in order to calculate the counterfactual car registrations in absence of the scrappage policy.

Figure 3.2 displays the time-series approach used to simulate the counterfactual situation. The dataset is divided into two parts: First the model selection period or pre-scrappage period (2001-2008) and second the out of sample prediction period (2009-2011) that encompasses the scrappage (2009) and post scrappage period (2010 and 2011), where we expect the potential influence of forwarded consumption to have an effect. We calculate additional demand in the scrappage period by comparing actual demand with forecasted car registrations. To identify potential pull-forward effects we compare actual car registrations after the end of the scrappage period with our counterfactual estimated via forecasts based on our time-series model.

The decision whether to use univariate or multivariate time series models for our two car segments is based on the results of Granger causality tests, whose results are given in Table 3.3 for the model selection period.

The most important property of our time series models to calculate accurate counterfactuals is their forecasting performance. We include the time series of other segments into the model for a certain segment if the additional variables help increasing the forecast performance of the model. If a variable Granger causes another variable, this implies that it helps forecasting the variable, e.g. new car registrations

[18] See Kilian (2008) for an application of these techniques calculating the size of oil supply shocks.

Figure 3.2: Empirical Strategy and Timeline

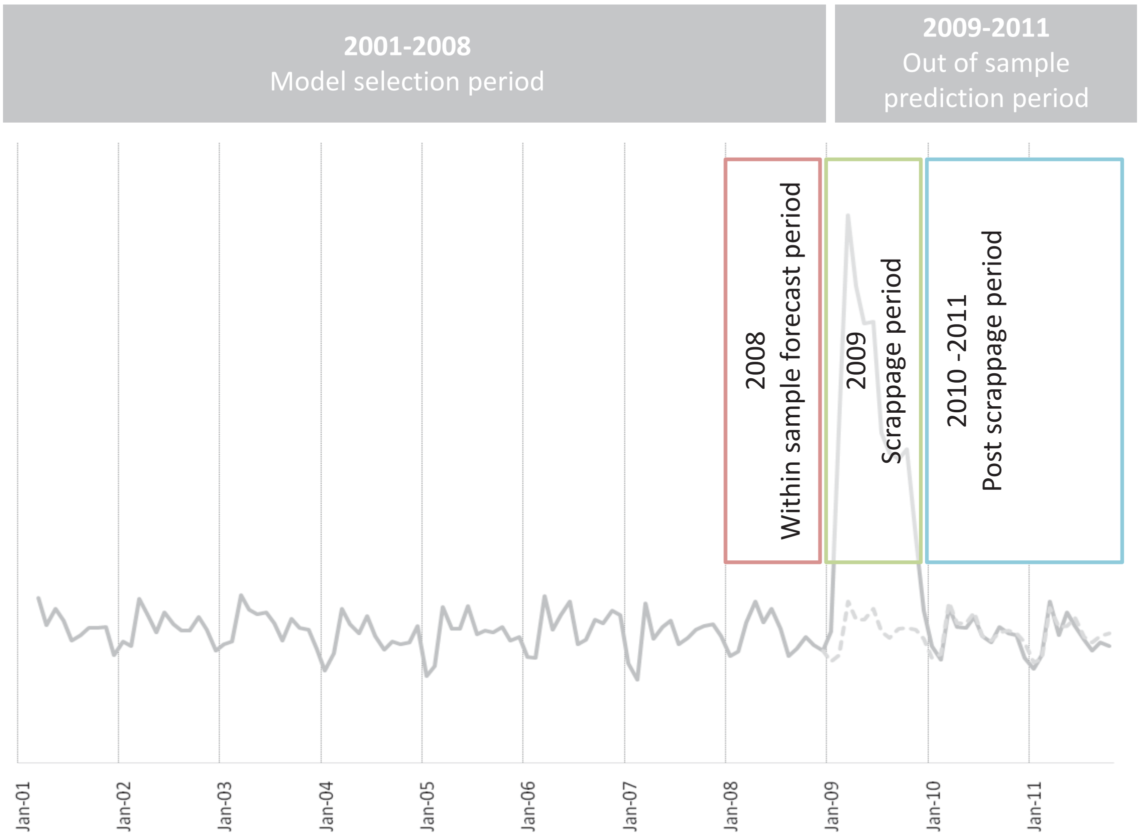


Table 3.3: Granger Causality between Segments, Model Selection

Excluded Variable	small	upper small
small	-	20.60***
upper Small	14.84***	-

Note: Stars indicate significance levels: *** 1%-level, ** 5%-level,* 10%-level.

in a given car segment.[19] The results of Granger causality tests show that new registrations in the upper small segment Granger cause new registrations in the small segment and vice versa. As a result, we use a VAR model including registrations for both segments to forecast registrations for the small and upper small segment. To be precise, the VAR model is a VARX model, because we include an additional exogenous control variable the industrial production index into our models. This selection is confirmed by checking the within-sample forecast performance for 2008 using the Mean Absolute Percentage Error (MAPE)[20]. In the next step, the appropriate time series model, now using data from 2001 up to January 2009, is chosen to predict the counterfactual car registrations for the years 2009 (the scrappage period), 2010 (the first ex post period) and 2011 (the second ex post period). The year 2010 is used to verify the forecast precision, as it is assumed that the subsidy effects will be worn out by then and the paths of the simulated and realized car registrations should be more or less equal again.

The VAR model, which is tested for autocorrelation and nonnormality of the residuals, contains a number of lagged endogenous and exogenous variables,[21] which are represented through the lag operator L and N, respectively. The number of lags is determined by l and n, hence $L^l(y) = y_{t-l}$ and $N^n(x) = x_{t-n}$. Let y describe endogenous variables, in our model new registrations in different car segments, x is a vector including economic indicators as the industrial production index and in the robustness section also gasoline price and interest rate and d a vector of deterministic variables controlling for trends and seasonalities. So the VAR model can be written in matrix form, where y_t is a vector for the VAR model:[22]

$$y_t = \beta(L)y_t + \delta(N)\ X_t + \gamma\ d_t + u_t \tag{3.1}$$

In the next step, we make dynamic predictions of the stable VAR process h steps ahead:

$$\hat{y}_{t+h} = c + \beta_1 y_{t+h-1} + ... + \beta_l y_{t+h-l} + \gamma_1 x_{t+h-1} + ... + \gamma_n x_{t+h-n} + \gamma\ d_{t+h}. \tag{3.2}$$

While the observed values of the exogenous variables are incorporated in these predictions, the endogenous lagged variables for the treatment period are based on predicted values. Such predictions, unlike the one-step-ahead forecasts, enable us

[19]See Hamilton (1994): pp. 302-307.

[20]See Clements and Hendry (2001): pp.25-27 and Hamilton (1994): pp.72-76

[21]See the first part of the appendix for the description of the variables.

[22]For our VAR models y_t, d_t, u_t are vector representations and X_t is a matrix consisting of exogenous variables.

to simulate the counterfactual situation. General economic shocks are incorporated into our predictions through the economic performance indicator industrial production. As robustness checks, we also include gasoline prices and interest rates into our models, but our results do not differ significantly using these specifications.

3.4.4 Model Selection

An adequate time series model has to be chosen in order to forecast the counterfactual situation. Forecasting can be done either by estimating univariate or multivariate time series models. While vector autoregressive models capture the competitive relationship between small and upper small segments to some extent, we also rely on the prediction error measure MAPE to determine the goodness of fit of the time-series model. Let y_i be the observed value at time point $i = 1...z$ and $\hat{y}_i$ the predicted value, then

$$MAPE = \frac{1}{z}\sum_{i=1}^{z}(y_i - \hat{y}_i)/y_i \tag{3.3}$$

The period used for model selection and estimation of our baseline model encompasses the time from January to November 2008 for two reasons. First, a sample reduction is attended by a loss of degrees of freedom, hence choosing an in-sample close to the later sample size is preferred. The second problem addresses the selection of a period without any severe structural changes, such as the financial crisis, which had its observable impact on German car production from December 2008 through the entire year 2009. Additionally, an increase in value-added tax in Germany from January 2007 on may have brought future consumption forward in 2006, but this can be observed in the data only through a drop in registrations, ranging from December 2006 until February 2007. Including an impulse dummy to capture this very short negative effect did not deliver any significant results and is henceforth not included in the models.

The lag order of our models has been determined using information criteria, e.g. Hannan and Quinn and Schwarz-Bayes.[23] The information criteria suggest a lag order of either one or two for the VAR model.[24] Estimating the VAR model with a lag order of one produces autocorrelation, however. We therefore use a VAR(2)

[23]See Lütkepohl (2005): pp.146-157.

[24]See Table 3.A2 in the appendix. Univariate models are incorporated for comparison of forecasting performance.

model as it yields no autocorrelation and produces a stable process with normally distributed errors.[25] Furthermore, the Augmented Dickey-Fuller test has also been applied to our data to check for potential unit roots. The corresponding test results show that all exogenous variables exhibit a unit root. Therefore, industrial production and gasoline price are included in the model using their first-differenced values. For interest rates even the second differences are used.[26]

As already discussed in section 3.4.3 Granger causality tests indicate that a VAR model is more appropriate than two separate AR models. Furthermore prediction quality is increased with the multivariate model and a VAR(2) is therefore the preferred regression set-up for the results. The corresponding MAPE values for the model selection period can be found in Table 3.4.4. The results clearly indicate that a vector autoregressive model has a higher forecast precision than an autoregressive model. The MAPE for upper small cars is 8.02 % and for the small car time-series only 6.15%.

Table 3.4: Prediction Error in Percent, Model Selection

Series	AR(1)	VAR(2)	VAR(2) Robust
Small	8.0%	6.15%	7.40%
Upper Small	10.4%	8.02%	10.1%

In order to check the robustness of our VAR(2) model, we re-estimate the model with two additional exogenous variables gasoline price and interest rate, controlling for the overall gasoline price level and financing conditions. The VAR model coefficients are presented in Table 3.A6 and the residual analysis tests in Table 3.A5 in the appendix. The additional variables do not change the coefficients of interest much and the residuals pass autocorrelation and Jarque-Bera tests. However, they lower insample forecast performance, our main measure of model selection. The last row of Table 3.4.4 shows the MAPEs. They are for small cars 7.4 percent compared to 6.1 percent from our main model, and 10.1 percent in contrast to 8.02 percent for upper small car registrations. We therefore rely on the VAR(2) model with one exogenous variable for the out of sample forecast.

The following section 3.5 summarizes the out of sample forecasting results.

[25]See Table 3.A5 in the appendix.
[26]See Table 3.A3 in the appendix for details.

3.5 Results

Based on the already described VAR(2) model we dynamically predict the two time-series for upper small and small car registrations 35 steps ahead from February 2009 up to December 2011.

Table 3.5: Prediction Error in Percent, Out of Sample Prediction

VAR (2)	2009	2010	2011
Small	51.7 %	7.1 %	11.5 %
Upper Small	48.6 %	7.9 %	15.8 %

The MAPEs in Table 3.5 show a large distortion for the year 2009, this is due to the German scrappage program. In 2010, however, the prediction errors are back to 7.1 percent for the small cars' time-series and 7.9 percent for upper small car registrations. We take the MAPEs for the year 2010 as an indicator of the forecast quality of our model. The corresponding errors for 2011 are higher than the 2010 numbers. An explanation is that there have been already 24 months forecasted and prediction quality tends to naturally decrease the longer the forecasting period. In Table 3.6 the effects of the car scrappage program are presented showing the yearly sums of differences between occurred car registrations and forecasted values.[27]

The scrappage program in 2009 led to an increase in new car registrations above the counterfactual situation in both segments (see Figure 3.3). Due to the program an additional 605,340 new cars in the small car segment were registered compared to the forecasted number. If the monthly registrations are considered, the effect of the policy can be traced from February 2009 to January 2010 with the biggest spike in March 2009, where 97,663 car registrations more than the counterfactual are predicted. From February 2010 onward there are, except for May 2011, always negative values for the differences in car registrations, i.e. the predicted sales numbers are above the real car registration numbers. This effect can be interpreted as a pull-forward effect of the car scrapping program. Nevertheless the positive effect of the program with 605,340 more cars sold clearly outweighs the negative pull-forward effect two years after the end of the program with 59,624 cars less sold compared to the counterfactual situation.

The effect of the program on upper small cars exhibited a slightly different picture. First of all, the car scrappage program affected upper small cars not as pro-

[27] See detailed monthly results in Table 3.A7 in the appendix.

Figure 3.3: Predictions and Realized Private Car Registrations for Segments Small and Upper Small (n.sa.)

foundly as the small car segment. Our calculations show that in 2009 385,768 cars of the upper small segment were sold due to the German Cash-for-Clunkers program compared to the counterfactual. This amount is only 64 percent of the small car registration due to the program, i.e. the program was less effective with upper small cars than with small cars, even if before the implementation of the program average monthly registration numbers for both categories were almost identical (small cars 34,626 cars and upper small 30,177 cars per month). Second, the monthly picture of car registrations in the upper car segment shows that the effect of the policy in terms of positive car registration differences ranges from February 2009 to July 2010. This is half a year longer than for the small cars. These numbers could also be attributed to the policy as car producers had severe delivery delays at that time that could lead to divergence of car sales and car registrations. As already discussed in Section 3.3, unfortunately, there is no official data on delivery times with monthly frequency and on the segment level, so that the interpretation can not be verified further. From August 2010 car registrations compared to the forecasted values are lower (with the exception of July 2011). In sum 2010 and 2011 41,484 cars less than under the counterfactual were registered. The overall effect for upper small cars is nevertheless positive. The total result of the policy, taking into account pull-forward effects of small and upper small cars, is therefore plus 889,999 cars.

Table 3.6: Yearly Difference between Realized and Predicted Registrations 2009-2011 (in absolute numbers)

Series	Treatment Period $\sum$ 2009 without Jan	Post Period $\sum 2010-11$
Small	605,340	-59,624
Upper Small	385,768	-41,484
Overall Effect	991,108	-101,109

Introducing the scrappage scheme seems to have been effective in terms of creating additional demand. Such an assessment, however, is purely focused on the automotive industry. The robustness of the forecasted time series can also be seen as an indicator for the actual impact of the financial crisis on car producers in Germany, meaning that all other policy measures, such as short-time work, have been successful in stabilizing the economy. Therefore, an additional and industry-specific measure like the scrappage scheme may have been unnecessary, if just keeping the level of new car registrations constant with regard to "normal" fluctuations might have been the goal of the policy. Additionally, the one-time impulse in additional new car sales may have come at the expense of substitution of other goods, so that other industries may have suffered from the car scrappage. It can certainly not be answered in our paper to what extent the incentivized new car purchases can be attributed either to a shift from households' savings to consumption or to the substitution of other consumable goods. At last, the true extent of the ex-post period of the potential pull-forward effect is unknown. It may well be, that some individuals would have bought a new car two, three or four years later if not for the scrappage programm. If so, a decline in new car registrations might be expected over the next few years.

3.6 Conclusion

In the wake of the financial crisis in 2008, the German government set up a large investment program in order to stabilize the German economy. The German automotive industry is one of the most prominent examples, because a scrappage programm was introduced in order to stabilize the industry and replace older cars with new and more ecological cars. In this paper, we focus on the effect of the car scrappage program on private new car registrations in the small and upper small car segments. Therefore, the analysis encompasses the extent to which additional new car sales have been induced in 2009 and the pull-forward effect. A vector autoregressive model is used to forecast potential during the outbreak of the financial and economic crisis and afterwards. While there seems to have been small pull-forward effects for small and upper small cars, the overall impact of the scrappage program is positive, i.e., the scrappage effect is larger than the pull-forward effect. In addition, a robustness check indicates that other policy programs seem to have counterbalanced the impact of the financial crisis. For future research, it would be interesting to see what effects the scrappage program had on competition in the automobile industry. Descriptive statistics suggest that German car producers have extensively profited from the policy.

Bibliography

Adda, J. and R. Cooper (2000). Balladurette and Juppette: A Discrete Analysis of Scrapping Subsidies. *Journal of Political Economy 108*(4), 778–806.

Alberini, A., W. Harrington, and V. McConnell (1995). Determinants of Participation in Accelerated Vehicle Retirement Programs. *RAND Journal of Economics 26*(1), 93–112.

Angrist, J. and J. Pischke (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press, Princeton: NJ.

Ashenfelter, O. (1978). Estimating the Effect of Training Programs on Earnings. *The Review of Economics and Statistics 60*(1), 47–57.

BAFA (2010). *Abschlussbericht Umweltprämie*. Bundesamt für Wirtschaft und Ausfuhrkontrolle, Eschborn.

Bertrand, M., E. Duflo, and S. Mullainathan (2004). How Much Should We Trust Differences-in-Differences Estimates? *Quarterly Journal of Economics 119*(1), 249–275.

BMWi (2009). *Richtlinie zur Förderung des Absatzes von Personenkraftwagen*. Bundesministerium für Wirtschaft und Technologie, Berlin.

Clements, M. P. and D. F. Hendry (2001). *Forecasting Non-Stationary Economic Time Series*. MIT Press, Cambridge: MA.

Cooper, A., Y. Chen, and S. McAlinden (2010). CAR Research Memorandum: The Economic and Fiscal Contributions of the Cash for Clunkers Program: National and State Effects. *Working Paper*, Center for Automotive Research, Ann Arbor, MI.

DESTATIS (2011). Indizes der Produktion und der Arbeitsproduktivität im Produzierenden Gewerbe. *Fachserie 4 Reihe 2.1*, Wiesbaden.

DESTATIS (2014). Volkswirtschaftliche Gesamtrechnungen. *Fachserie 18 Reihe 1.3*, Wiesbaden.

Diebold, F. (1998). The Past, Present, and Future of Macroeconomic Forecasting. *Journal of Economic Perspectives 12*(2), 175–192.

Focus (2009). Abwrackprämie - Wartezeiten für Kleinwagen auf Rekordniveau. *Focus online March 19*, available: http://www.focus.de/auto/neuheiten/abwrackpraemie/ abwrackpraemie–wartezeiten–fuer–kleinwagen–auf–rekordniveau_aid_ 382136.html [accessed 12 Apr 2012].

Goerres, A. and S. Walter (2010). Bread and Circuses? German Voters Reaction to the Governments Management of the Economic Crisis in 2008/9. *Working Paper*, University of Heidelberg.

Hahn, R. (1995). An Economic Analysis of Scrappage. *RAND Journal of Economics 26*(2), 222–242.

Hamilton, J. D. (1994). *Time Series Analysis*, Volume 2. Princeton University Press, Princeton: NJ.

Heimeshoff, U. and A. Müller (2013). Evaluating the Causal Effects of Cash-for-Clunkers Programs in Selected Countries: Success or Failure? *Working Paper*, Düsseldorf Institute for Competition Economics.

Höpfner, U., J. Hanusch, and U. Lambrecht (2009). Abwrackprämie und Umwelt. Eine erste Bilanz. *Institut für Energie-und Umweltforschung*, Heidelberg.

Kaul, A., G. Pfeifer, and S. Witte (2012). The Incidence of Cash for Clunkers - An Analysis of the 2009 Car Scrappage Scheme in Germany. *Working Paper 68*, University of Zurich.

Kavalec, C. and W. Setiawan (1997). An Analysis of Accelerated Vehicle Retirement Programs Using a Discrete Choice Personal Vehicle Model. *Transport Policy 4*(2), 95–107.

Kilian, L. (2008). Exogenous oil Supply Shocks: How Big are They and How Much do They Matter for the US Economy? *Review of Economics and Statistics 90*(2), 216–240.

Leheyda, N. and F. Verboven (2013). Scrapping Subsidies During the Financial Crisis: Evidence from Europe. *ZEW Discussion Paper 13-079*, Mannheim.

Li, S., J. Linn, and E. Spiller (2013). Evaluating cash-for-clunkers: Program effects on auto sales and the environment. *Journal of Environmental Economics and Management 65*(2), 175–193.

Lütkepohl, H. (2005). *New Introduction to Multiple Time Series Analysis*. Springer Verlag, Berlin: Germany.

Mian, A. and A. Sufi (2012). The Effects of Fiscal Stimulus: Evidence from the 2009 Cash for Clunkers Program. *Quarterly Journal of Economics 127*(3), 1107–1142.

Nicolini, M., C. Scarpa, and P. Valbonesi (2013). Aiding Car Producers in the EU: Money in Search of a Strategy. *Journal of Industry, Competition, and Trade 13*(1), 67–87.

Ramey, V. and D. Vine (1996). Declining Volatility in the U.S. Automobile Industry. *The American Economic Review 96*(5), 1876–1889.

Reinhart, C. M. and K. S. Rogoff (2009). *This time is Different: Eight Centuries of Financial Folly*. Princeton University Press, Princeton: NJ.

Ryan, L., S. Ferreira, and F. Convery (2009). The Impact of Fiscal and Other Measures on New Passenger Car Sales and CO2 Emissions Intensity: Evidence from Europe. *Energy Economics 31*(3), 365–374.

Schiraldi, P. (2011). Automobile Replacement: A Dynamic Structural Approach. *RAND Journal of Economics 42*(2), 266–291.

Van Wee, B. and G. De Jong (2011). Accelerating Car Scrappage: A Review of Research into the Environmental Impacts. *Transport Reviews 31*(5), 549–569.

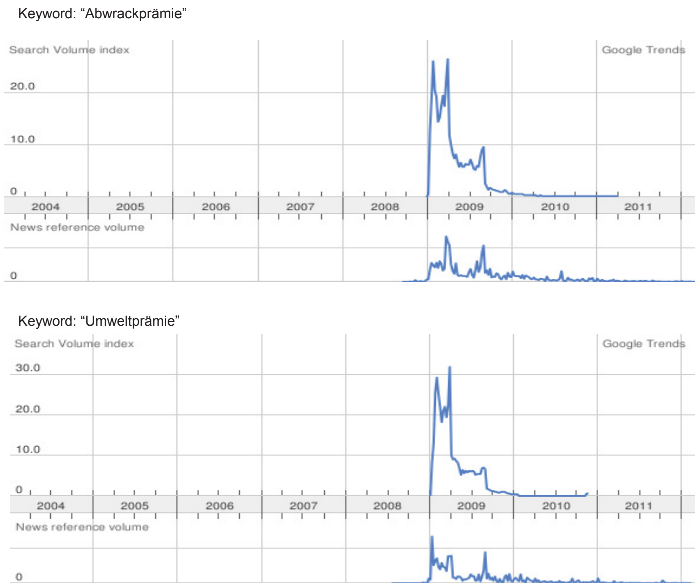
Waldermann, A. (2009). Konjunktur-Strohfeuer - Ökonomen Wettern gegen Abwrackprämie. *Spiegelonline April 20*, available: http://www.spiegel.de/wirtschaft/konjunktur–strohfeuer–oekonomen–wettern–gegen–abwrackpraemie–a–618197.html [accessed 23 Feb 2012].

Appendix

Let y denote the variable of interest, x an exogenous variable, c a constant factor, d a monthly deterministic effect and u an i.i.d. error term. Therefore, the setup of our vector autoregressive model is as follows:

$$
\begin{aligned}
i &= small, upper\ small \\
j &= industry\ production, unemployment\ rate \\
t &= time\ period \\
l &= lag\ length\ of\ endogenous\ variable \\
n &= lag\ length\ of\ exogenous\ variable \\
y_t &= (y_{small,t}, y_{upper\ small,t}) \\
x_t &= (x_{industry\ production,t}, x_{unemployment\ rate,t}) \\
d_t &= (m_{1,t}, m_{2,t}, m_{3,t}, ...m_{11,t}, trend) \\
u_t &= (u_{small,t} u_{upper\ small,t}) \\
\beta, \gamma, \delta &= Matrix\ of\ coefficients
\end{aligned}
$$

Figure 3.A1: Google Trends Search Volume and News Reference Index



Source: Google Trends, available: http://www.google.de/trends [accessed 29 Feb 2012].

Table 3.A1: Mean and Variance 2008 to 2011 (without 2009) of the Percentage of Private Car Holders of all Car Holders per Segment

	Small	Upper small
Mean	51.4	42.5
Variance	1.4	5.2

Table 3.A2: Lag Length Selection and Information Criteria

Information Criteria	small	upper small	VAR
SBIC Value	19.06	18.77	37.55
SBIC Lag Length	1	1	1
HQIC	18.79	18.48	36.90
HQIC Lag Length	1	1	2

Table 3.A3: Augmented Dickey Fuller Unit Root Tests

ADF Test Lag(2)	small	upper small	industrial production		
			x_t	$\triangle\ x_t$	
Test statistic	-5.10	-5.01	-2.07	-8.74	
ADF Test Lag(2)	gasoline price		interest rate		
	x_t	$\triangle\ x_t$	x_t	$\triangle\ x_t$	$\triangle^2\ x_t$
Test statistic	-0.43	-6.09	-0.54	-2.24	-6.92
1% Critical Value			-3.54		
5% Critical Value			-2.91		
10% Critical Value			-2.59		

Table 3.A4: VAR Output, Model Selection

	Small	Upper Small
Small	0.172	-0.376***
	(0.110)	(0.0917)
L2.Small	0.358***	-0.0266
	(0.123)	(0.103)
L.Upper Small	0.525***	0.685***
	(0.140)	(0.117)
L2.Upper Small	-0.462***	0.104
	(0.137)	(0.114)
D.Industrial Production	428.4***	234.2***
	(93.90)	(78.29)
LD.Industrial Production	214.2	11.64
	(135.5)	(113.0)
L2D.Industrial Production	105.8	-73.58
	(96.62)	(80.56)
Constant	16048.4***	20156.1***
	(4937.2)	(4116.5)
Monthly Dummies included	Yes	Yes
Observations	79	79
RMSE	2201.37	1835.43
R^2	0.8275	0.8849

Note: standard errors in parenthesis; stars indicate significance levels:
*** 1%-level; ** 5%-level; * 10%-level

Table 3.A5: VAR Residual Analysis, Model Selection

Test	VAR (1)	VAR(2)	VAR(2) Robust
Autocorrelation			
LM Value Lag 1	11.45	2.67	4.79
Prob >chi2	0.02	0.61	0.31
LM Value Lag 2	-	4.53	3.84
Prob >chi2	-	0.33	0.42
Nonnormality			
Jarque-Bera Test Value	1.59	1.24	0.66
Jarque-Bera Prob > chi2	0.81	0.87	0.96

Table 3.A6: VAR Output Robustness, Model Selection

	Small	Upper Small
L.Small	0.210*	-0.334***
	(0.112)	(0.0888)
L2.Small	0.357***	-0.0647
	(0.127)	(0.101)
L.Upper Small	0.564***	0.680***
	(0.153)	(0.121)
L2.Upper Small	-0.460***	0.146
	(0.149)	(0.119)
D.Industrial Production	414.7***	231.1***
	(98.75)	(78.48)
LD.Industrial Production	195.9	13.94
	(136.1)	(108.2)
L2D.Industrial Production	79.25	-85.37
	(101.1)	(80.34)
D.Gasoline Price	-5577.4	-13357.5**
	(7460.7)	(5929.2)
LD.Gasoline Price	-1965.3	-10862.6*
	(7981.4)	(6343.0)
L2D.Gasoline Price	9660.3	8440.0
	(7952.9)	(6320.3)
D2.Interest Rate	611.1	766.8
	(3209.7)	(2550.9)
LD2.Interest Rate	764.8	-2272.8
	(2885.3)	(2293.0)
L2D2.Interest Rate	2932.8	789.7
	(3258.9)	(2589.9)
Constant	13478.7***	18753.5***
	(4978.7)	(3956.7)
Monthly Dummies included	Yes	Yes
Observations	78	78
RMSE	2270.76	1804.62
R^2	0.83	0.90

Note: standard errors in parenthesis; stars indicate significance levels:
*** 1%-level; ** 5%-level; * 10%-level

Table 3.A7: Monthly Difference between Realized and Predicted Registrations 2009-2011 (in absolute numbers)

Date	Small	Upper Small
2009m2	57,387	22,404
2009m3	97,663	29,298
2009m4	85,615	42,963
2009m5	77,076	56,364
2009m6	73,046	58,456
2009m7	49,652	44,034
2009m8	47,826	35,347
2009m9	42,226	33,112
2009m10	44,942	34,685
2009m11	23,733	22,087
2009m12	6,173	7,020
2010m1	2,959	1,234
2010m2	-1,564	598
2010m3	-5,190	2,575
2010m4	-2,276	4,370
2010m5	-272	918
2010m6	-2,880	1,111
2010m7	-1,044	832
2010m8	-1,152	-1,344
2010m9	-405	-164
2010m10	-1,469	-1,231
2010m11	-2,312	-5,177
2010m12	-6,207	-6,822
2011m1	-2,720	-4,203
2011m2	-616	-2,368
2011m3	-2,843	-5,986
2011m4	-2,181	-2,960
2011m5	1,117	-6,397
2011m6	-2,507	-2,608
2011m7	-361	4,510
2011m8	-5,504	-1,078
2011m9	-4,118	-1,461
2011m10	-2,323	-634
2011m11	-6,259	-6,047
2011m12	-9,498	-9,153

Chapter 4

The Relationship Between Electricity Retail Prices and Photovoltaic Diffusion - Evidence from German Regional Markets

4.1 Introduction

With the help of the 2010 Energy Concept, the German Government, newly elected in 2009, set goals for the reduction of greenhouse gas emissions, and the future diffusion path of renewable energy sources (RES). Overall, the greenhouse gas emissions should be reduced by 80% compared to the 1990 value and this is supposed to be achieved mainly by the promotion of RES. Until 2020, 35% of Germany's electricity production is supposed to come from these sources. This percentage is even enhanced up to 80% in 2050.[1] These and other similar targets by the European Union and G8 countries set the stage for extensive subsidization of Photovoltaic (PV) and other RES technologies in Germany, culminating in new record additions of renewable energy production numbers every year. This process was further amplified by the Fukushima nuclear disaster in March 2011, which led to Germany's complete phase-out of all nuclear power plants and, therefore, piled the pressure on RES promotion to reach the ambitious climate protection goals already mentioned. The German Government from then on called the challenging RES promotion plan the energy transition ("Energiewende"). As a result, the electricity production from RES reached 151.7 billion kilo-watt hours (kWh) in 2013, and thereof 20% based on the PV technology.[2]

The high levels of new capacity building of PV and other RES is often only attributed to the introduction and alterations of feed-in tariffs (FiTs), the price paid to PV producers for every kwh fed into the electricity grid. This FiT is regulated and guaranteed for a period of 20 years. Subsidization began with the passing of the Renewable Energy Act in 2000 and has been multiply amended, most recently in 2009, 2012 and 2014. In fact, those FiTs for PV were substantially reduced over the period from 2009 to 2012 from 48 ct/kWh in 2009 to 35.5 ct/kWh in 2012,[3] which is a decrease of 26%, so that this evolution explains only part of the picture. Other aspects of this are that the above mentioned period was a time of steadily decreasing PV installation costs, because we are talking about a period of financial stagnation and decline in most European countries and a time of increased competition between PV panel producers. This resulted in a five to six times reduction of PV panel prices from 2002 to 2012.[4] At the same time, electricity retail prices were rising during this period, mostly because the FiTs are financed through pass on to electricity

[1] See Bundesregierung (2010).

[2] The numbers are taken from BDEW (2014), p. 16.

[3] These FITs are average yearly photovoltaic FiTs over all types of customers and come from BDEW (2014), p. 40-41.

[4] See Maubach (2013), pp. 117-140 for an interesting discussion of the German energy transition from 2009 to 2012.

consumers via the so called Renewable Energy Surcharge (”EEG Umlage”). These two contradicting effects led to the PV technology reaching grid parity somewhere between late 2012 and early 2013, i.e. the cost of production of 1 kWh of electricity undercut the buying costs of electricity from the retailer.[5] This effect fostered the use of PV installations for self-consumption of the used electricity.

It is the aim of this paper to disentangle the FiT and the electricity retail price effect on PV diffusion in Germany, against the background of falling PV system prices from 2010 to 2012. The electricity retail price is interpreted as the opportunity cost of PV investment and its influence is neglected in most empirical case-studies on this subject. Our paper tries to close this gap. The results suggest that the electricity retail price plus the auto consumption premium (net of the Renewable Energy Surcharge) is even more influential for PV investment than the pure FiT for household investors and small business customers. For household customers, a 1% increase in price leads to a 10% higher stock of PV installations over all German zip code districts. For small business customers this effect goes up to 24%. We find the corresponding FiT influence to be 7% for households and 3.8% for small business customers. This result leads to the following policy implications: If the German regulator wants to promote investment in PV panels, the electricity retail price is the more promising part to draw on. The electricity retail price leaves a lot of room to do so, as there are various governmental taxes and levies to cut. This is an insight often neglected, especially in public debate on this topic.

The remainder of the paper is structured as follows: The related literature will be described in Section 4.2, followed by an in-depths presentation of the German PV market and its regulation. Section 4.3 comprises the empirical methodology, data and presentation of the results. Section 4.4 gives a summary and concludes.

4.2 Literature Review

A substantial part of the empirical literature on diffusion of innovation is based on the seminal paper by Griliches (1957) on the diffusion of hybrid seed corn in the United States, proposing that the diffusion process of a new technology follows an S-shaped relationship. This idea was seized by Rogers (1962), who developed a five stage diffusion model and led to implementation of the Bass model, traced back to Bass (1969). The author demonstrates that the purchase decision of a durable good hinges crucially on the number of previous buyers of the product. This finding is

[5]The exact date of grid parity is difficult to measure, but see Prez et al. (2013) and Bhandari and Stadler (2009) for examples. Both studies detect a point at the end of 2012 or beginning of 2013.

taken into account in our model, as we use the stock of PV installations to model the later stage of the diffusion process. This model and enhanced versions, remain a major research setting, used especially in marketing sciences.

The empirical diffusion literature with respect to adoption of the PV technology builds on the marketing literature as well as dynamic panel and time-series techniques. It can be grouped into multi-country studies and single country case studies.

Multi-country studies started with Beise (2004), who uses a simple aggregate panel data set of 13 countries at the early stage of diffusion from 1992 to 2002 and estimates the influence of subsidies and sunshine radiation on the adoption of PV panels. The results suggest that FiTs (and system price subsidies) are the most important drivers of PV diffusion. Radiation is found to matter as well, but it is less important. Furthermore, the analysis defines Japan and Germany as lead market countries in the observed study period. Almost the same time period (1992-2006) is examined by Guidolin and Mortarino (2010). The authors forecast the evolution of PV panels until 2020 for eleven countries, using a VAR model approach, and confirm the results that governmental policies to promote solar PV foster PV diffusion significantly. Comparably, De La Tour and Glachant (2013) use a multi-country setting and a VAR model approach in order to answer the slightly different question if FiT influence panel prices or vice versa. Their results suggest that PV panel prices are temporarily increased before FiT changes and that solar module prices are an important driver of PV diffusion. From this branch of literature we take the insight, that FiT and PV module prices matter significantly for any empirical model of PV adoption.

The already mentioned multi-country studies are amended by empirical case studies of PV diffusion in various countries. We are aware of European PV diffusion studies on Italy (Palmer et al., 2013), the United Kingdom (Richter, 2014) and three studies on Germany (Rode and Weber, 2012; Leepa and Unfried, 2013; Müller and Rode, 2013). Furthermore, there is work on the US market (Bollinger and Gillingham, 2012; Graziano and Gillingham, 2015) and Japan (Zhang et al., 2011). The majority of these studies are interested in social spill-over effects on the diffusion of PV installations. It is their aim to answer the question of how much the already installed base in a specific area affects the newly installed PV modules in a given geographical market. All of the studies find strong peer effects of PV diffusion patterns for the above mentioned countries (Richter, 2014; Bollinger and Gillingham, 2012; Graziano and Gillingham, 2015; Rode and Weber, 2012; Müller and Rode, 2013). Therefore, our econometric model uses the stock of PV installations as the dependent variable in order to control for the installed base and market saturation.

Among the country case studies, only two papers look at the influence of subsidy policy on PV diffusion: Zhang et al. (2011) and Leepa and Unfried (2013). The former shows that higher regional governmental policies and smaller installation costs are the main drivers of PV diffusion in 47 Japanese prefectures, using a yearly panel data set from 1996 to 2006. Leepa and Unfried (2013) however, explore the effects of changes in the German FiT on the diffusion of German PV installations. They analyze different types of FiT systems and predict PV investments in Germany using a weekly time-series data set from 2009 to 2011. Their main result is that linearly decreasing FiTs or those linked to the evolution of PV panel prices would lead to PV investment close to the governmental targets in Germany.

Our most important contribution to the literature is that we explicitly model the own consumption possibility of a PV installation represented by the electricity retail price and own consumption tariffs. These opportunity costs of electricity generation on the household level are not studied in the existing literature. In order to do that, we create a unique monthly panel data-set at the later stage of PV diffusion from 2010 to 2012 and conduct a detailed analysis of the determinants at the zip code level in Germany. A second aspect that we contribute to the literature is our in-depth analysis of two different customer groups. A similar dis-aggregation is, to the best of our knowledge, not carried out in existing empirical work. We draw from the existing literature the insight that policy subsidy changes (like FiT and self-consumption surcharges) are important drivers, as well as the influence of sunshine radiation and the direct PV module panel costs. The detailed estimation set-up is presented in section 4.4. The next section gives an overview of important regulations of the German PV market and describes the development of PV adoption over the last 20 years.

4.3 Description and Regulation of the German PV Market

Basically, the regulation of the PV market in Germany can be subdivided into a) direct subsidies and loans granted to PV investors and b) subsidization of this specific technology via FiTs and own consumption tariffs. In the following paragraphs both types of governmental interventions are described in detail starting with the direct subsidy packages. The start of governmental promotion was marked in 1991 by the 1,000-Roofs-Program of the German Federal Ministry of Education, Research and Technology. The aim of this program was to foster the introduction of PV installations for detached and two-family homes. Around 70% of the overall installation costs was financed by the government and about 2,400 decentralized roof-top systems were installed due to this first subsidization program.[6] In 1999, the 100,000-roofs-program was introduced and targeted to create 300 MW of additional capacity for different customer groups. It comprised a funding scheme based on loans by the state owned bank KfW and led to 261 MW of additional capacity or 55,000 installations. Both programs are considered to be less influential than the introduction of the FiT system.

In 1991, the Electricity Feed-in Act (Stromeinspeisegesetz/StrEG) was introduced. This act guaranteed feed-in tariffs and unlimited priority feed-in for electricity from all renewable energy sources. Public budget funds were not involved in the financing of the FiT, as the burden imposed by the law is solely borne by electricity suppliers. This act led especially to the construction of wind power plants and not so much to the installation of PV panels on private customers' roofs, as the FiT were not cost-effective for this technology at the beginning of the 1990s.[7]

In 1999, the Renewable Energy Sources Act (EEG) was agreed upon. This Act serves as the beginning of large scale public support for decentralized installations from renewable sources. The basic contents of the EEG are fixed remuneration for operators of renewable energy plants guaranteed for twenty years. The tariffs are technology specific. For PV the FiT in 2000 granted from network operator to PV producer was 50.62 ct/kWh[8]. From 2002 onwards a yearly degression of the PV FiT of 5% was additionally agreed upon. The costs of the FIT granted from network operators to PV electricity producers is passed on to electricity consumers via the Renewable Energy Act Surcharge (EEG Umlage), which added up to 0.2 ct/kWh in 2000.[9]

The Renewable Energy Act was substantially amended in the years 2004, 2009, 2012, and recently in 2014. Subsequently, the most important changes and novelties are mentioned, but with respect to PV promotion only.

The first amendment, in 2004, mainly comprised a further increase of FiT for PV installations and those raised FiTs were also differentiated between customer groups. Smaller producers of electricity are granted with higher FiT than large scale producers. Furthermore, the annual degression of remuneration for new plants was increased to 6.5% as of 2006. The amendment of 2009 introduced an additional premium tariff for self-consumed electricity in order to additionally promote the

[6] See Hoffmann (2008) for further details of the program.

[7] See IEA (2014) and BMWI (2014).

[8] An overview over all FiT with respect to PV can be found on http://www.sfv.de/lokal/mails/sj/verguetu.htm

[9] An English translation of the original Renewable Energy sources act can be found in IWR (2014). The value of EEG Umlage is taken from BDEW (2014), p.45.

self-consumption of electricity. The tariff was differentiated between levels of self-consumption. Additionally, the German Government introduced an obligation to register every new PV installation with the German Federal Network Agency. In 2010, another amendment of the amendment 2009 was introduced that basically reduced auto tariffs and FiTs for 2010 and 2011 out of band.[10] The creation of the PV registration data enabled the regulators to introduce further degression rates that depended on the number of new PV installations, which were in effect from January 2012 onwards. That means the degression rate in 2012 depends on the sum of the installed capacity in 2011.

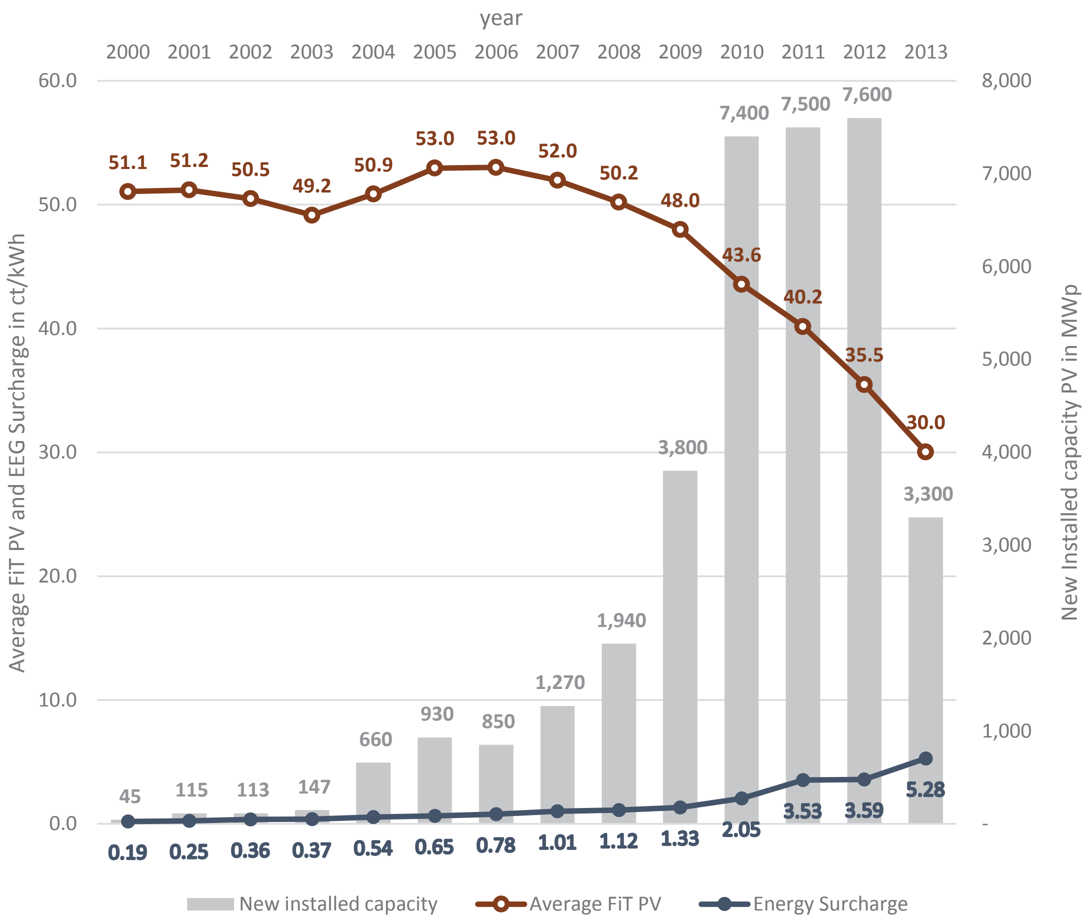
The Amendment of the Renewable Energy Act 2012 with respect to PV also contained important changes. First, the premium of self-consumption was abolished from April 2012. Second, the classification according to which the FiTs are differentiated was changed again, with the result that PV installations >10MW are not granted any FiT at all from April 2012. Third, the FiT are decreased one time by 15% from January 2012 and obtain a further automatic monthly degression of 1% starting from April 2012. Fourth, a market premium is provided if generated electricity is sold directly on the electricity market.[11]

An overview of the evolution of the yearly new installed capacity, the energy surcharge paid by electricity customers and the degression of FiTs can be retraced in Figure 4.1. From 2004 until 2012 the new installed capacity increased every year, with high values around 7,000 MWp each year 2010 to 2012. The most recent value in 2013 suggests that the building of PV installations halved compared to the 2012 value. This was the first drop in the construction of photovoltaics in Germany since 2006. To a large extend this growth of PV panels explains the increase of the energy surcharge, for which of course all types of renewable energies is responsible, not only PV. As stated in the paragraphs above, the average FiTs across all customer groups were steadily decreased by the German regulator in order to keep the high rates of new installations at bay. Nevertheless, FiTs do not seem to be the only influence factor that matters for new PV installations. High rates of PV installations were visible from 2010 to 2012, even if the FiTs were substantially cut by the regulator e.g. with the help of amendment 2010 of the Renewable Energy Act. The explanation of the other determinants that drive PV diffusion is the main part of the next section, which outlines the empirical strategy of the paper.

[10]See Clearingstelle EEG (2014).

[11]See BMWI (2012) for a summary of the Amendment of the Renewable Energy Act 2012.

Figure 4.1: Yearly New Installed Capacity and FiT PV Germany 2000-2013



Source: Own graphic based on BSW-Solar (2014), p.5; BDEW (2014), pp.40-41 and Mayer and Burger (2014), p.2.

4.4 Empirical Strategy

The following part describes the empirical set-up of the paper starting with data issues and summary statistics, followed by an in-depth analysis of the regression model and ending with the presentation of the results.

4.4.1 Data and Summary Statistics

The information on PV installations in Germany on the zip code level is derived from two sources. First, since January 2009, every new PV installation unit or the expansion of an already existing system needs to be registered with the German Federal Network Agency (BNetzA). This data is publicly available and includes the exact registration date. Each data point specifies an installation and its corresponding nominal capacity. Second, the network operators are obliged to publish the number and details of each new PV installation on their web pages in order to facilitate market transparency.[12] This data is collected, combined and managed by the German Photovoltaic Association (DGS) and can be downloaded from their homepage. This dataset is available from the beginning of the PV diffusion in the 1960s until the present and comprises information on the commissioning date, the exact address, the nominal capacity and, since 2012, the annual production of electricity of each plant. In order to undertake the following analysis the two datasets are merged into a single one.[13] Also, the stock of PV installations is calculated using the German Photovoltaic Association's data by summing up the new PV installations per zip code from January 1999 until December 2008. From January 2009 the new installations from the matched data set are added each month. Depreciation rates are not taken into account, as the typical life span of a PV installation is considered to be at least 20 years.[14]

The explanatory variables basically include electricity prices and governmental subsidies, namely feed-in tariffs and own consumption tariffs. The electricity retail price data comes from the consumer comparison platform Verivox. This data is an average of the 50 cheapest electricity retail prices (without prepayment and deposit contracts)[15] in a zip code area and is available for different consumption levels.

[12]See Renewable Energy Act (EEG) 2009 and 2012 §§45 to 52.

[13]Further information on this merge and other additional information on data issues is given in the Appendix.

[14]German PV companies usually give a warranty on the functioning of rooftop installations of between 20 and 25 years. See http://www.photovoltaik.org/wissen/garantie as a reference.

[15]Verivox provided us with data on comparable price levels for the 50 cheapest contracts for all zip codes. The use of this data is automatically deducting basic contracts ("Grundversorgungsver-

As this data was provided to us only from January 2010 to June 2012, the whole empirical analysis is restricted to this time period. For the empirical analysis the Renewable Energy Surcharge is deducted from the price. This is crucial, because the Renewable Energy Surcharge reflects last year's sum of installed PV modules and is therefore endogenous.[16] As described in section 4.3, the level of feed-in tariffs (*feed in*) and the auto consumption tariff (*auto tariff*) have been frequently changed by the regulator throughout the time span of investigation. In Figure 4.A3 in the Appendix the evolution of the two parameters can be retraced. Both governmental subsidies were substantially reduced over the period of interest. The auto consumption tariff was even completely removed, from April 2012, for both customer groups.

The focus of the empirical set-up is to further disentangle the analysis in two customer groups: household customers (HH) and small business customers (BC). The basic assumptions are summarized in Table 4.1. As the typical customer of a PV installation in the household sector is a house owners with a four-person household, the yearly consumption level of 5,000 kWh is chosen to determine the price level. The corresponding nominal capacity band for this customer group is 1-10 kilo-watt peak (kWp). For small business customers, and also multi-family houses, a consumption level of 20,000 kWh is chosen, and the corresponding PV capacity is 11-50 kWp.[17]

Additionally the estimation uses three control variables. First, *interest_rate* is used as a proxy for the overall financial costs of the long term investment decision in PV installations. The corresponding interest rate represents the long-term interest rate for Germany taken from the OECD's monthly monetary and financial statistics

trag"), that are subject to a different regulatory regime and are consequently more expensive. In Germany 36.7% of the households in 2012 were supplied through these contracts (see Bundesnetzagentur and Bundeskartellamt, 2014, p. 131) and are not included in this study. The contracts without prepayment and deposits are chosen as they were almost exclusively offered by two single energy companies called TelDaFax and FlexStrom (see Verivox, 2010, p. 92). However, in June 2011, in the middle of our period under review, TelDaFax filed for insolvency. This affected the price level in a special way that is not relevant to our research, and so, these contract types are not incorporated into our work.

[16] A stylized electricity bill for a German household customer of 2011 can be retraced in Figure 4.A2 in the Appendix.

[17] The average consumption level of a four person household is taken from Energieagentur-NRW (2011), p.5 and the average PV size for this segment is based on usual classification in the photovoltaic industry. Furthermore, the feed-in tariffs are also classified from 1-10 kWp for private customers. The consumption level of the small business customer segment is assumed to be four times as large as the household level and the capacity of the PV installation is correspondingly chosen.

Table 4.1: Basic Assumptions of Chosen Customer Types

Customer group	Electricity consumption (per year)	Nominal capacity
Household (HH)	5,000 kWh	1-10 kWp
Small business customer (BC)	20,000 kWh	11-50 kWp

Source: Own assumptions based on usual classification in the photovoltaic industry.

(MEI) database. Unfortunately, regional interest rates within Germany are unavailable to researchers. Second, weather, more precisely the global radiation in various parts of Germany, is an important determinant of the PV investment decision. This data is available on the basis of geometrical territorial units in the form of 1 km grids ("Raster") based on the Gauss-Krueger coordinate system from Germany's National Meteorological Service (Deutscher Wetter Dienst "DWD"). This latitude and longitude information is translated into zip code data by geocoding using OpenStreetMap (OSM) data. After that, the radiation number is modified, so that every data point represents the average daily amount of global radiation over the last twelve months in the given region and the unit of measurement is Watt-hour per square meter (Wh/m^2). Third, the PV investment decision is driven by the price level of the PV panels, which is included via the monthly price index for a crystalline module of 1 Watt peak (Wp) of German/European origin. The corresponding variable in the model is named *price_panel* and comes from Solarserver. Another important factor that needs to be considered is that the period 2010 to 2012 included the Fukushima nuclear disaster in Japan on March 11, 2011, which led to the announcement of a gradual phase-out of all nuclear power plants in Germany. In an economic experiment considering German subjects, Gallier et al. (2014) show that the earthquake and tsunami in Japan had a positive influence on the willingness to contribute to climate protection. We therefore argue that investment decisions in PV panels by German households and small business customers could be influenced also by this important event and control for that with the help of a dummy variable, set to one after the nuclear disaster and named *fukushima*. The summary statistics of the described dataset are presented in Table 4.2.

All variables described in this section are aggregated on zip code (three digit) level. The overall panel dataset comprises 20,130 observations: 671 zip code areas over a period of 30 months (January 2010 until June 2012). The stock data for both customer groups shows a diverse picture. On average the stock of PV installations of the HH category per zip code is 688 installations and for BC a little bit less

Table 4.2: Summary Statistics

Variable	Unit	Mean	Std. Dev.	Min	Max	Obs	Data-source	Available online from
StockPV_HH	absolute	688.26	647.14	1	4,510	20,130	BNetzA	http://www.bundesnetzagentur.de and
StockPV_BC	absolute	473.55	703.65	0	5,756	20,130	and DGS	http://www.energymap.info
Pricewauto_HH	ct/kWh	37.18	5.52	22.47	46.9	20,130	Verivox	Prices upon request, auto tariffs from
Pricewauto_BC	ct/kWh	36.42	5.87	21.71	46.17	20,130		http://www.photovoltaik-web.de
FiT_HH	ct/kWh	30.41	5.85	19.11	39.14	20,130	SFV	http://www.sfv.de/lokal/mails/sj/
FiT_BC	ct/kWh	30.31	6.05	18.13	39.14	20,130		verguetu.html
Radiation12	Wh/m^2	2,925	328	844	3,524	20,130	DWD	Upon request
Interestrate	%	2.47	0.6	1.3	3.34	20,130	OECD	http://www.stats.oecd.org/MEI
Pricepanel	EUR/W	1,538	355	930	2,030	20,130	Solarserver	http://www.solarserver.com/service
Trend	-	15.5	8.66	1	30	20,130	-	
Trendsq	-	315.17	276.54	1	900	20,130	-	
Fukushima	-	0.53	0.5	0	1	20,130	-	
Population	absolute	115,126	56,876	3,598	510,604	20,130	Destatis	www.regionalstatistik.de
Expanse	km^2	408.97	354.35	4	1970	20,130	Destatis	www.regionalstatistik.de
House quota	%	0.81	0.13	0.47	0.97	20,130	Destatis	www.regionalstatistik.de

Note: Variable affix *HH* represents household customers and *BC* small business customers.

(473). It is important to note that the standard deviation is very large in both cases. The stock of PV installations varies substantially over German zip code areas. This is on the one hand not surprising, because radiation levels vary a lot over Germany as well (see the Radiation12 variable), but on the other hand FiT and auto consumption tariffs are set at the federal level and are the same for all German zip codes. The average retail electricity price (including all taxes, duties and the auto consumption tariff) is 37 ct/kWh for the HH customer group and 36 ct/kWh for BC. Both price variables show a standard deviation of around 6 ct/kWh. The regional variation in the prices is due to differences in regulated network costs. Traditionally, network costs in Eastern Germany are higher, basically because the networks are newer. Nevertheless, the retail price of electricity also includes the different competitive pressures between regions. In Table 4.A1 in the Appendix maps of Germany and the regional variation of radiation, electricity retail price and photovoltaic installations are presented. The interest rate of long term investment over the period of observation was 2.47 %. The average price of a 1 W crystalline PV panel was 1,538 EUR. This price almost linearly decreased from 2,030 EUR in January 2010 to 930 EUR in June 2012. The next section explains the empirical estimation strategy.

4.4.2 Estimation Strategy

We use panel data on the zip code three digits level over a monthly time period of two and a half years. As the cross sectional dimension comprises 670 zip code areas, unobserved heterogeneity needs to be controlled for. Therefore, the estimation strategy is a fixed effects model using the with-in transformation to get rid of the unobserved heterogeneity problem. The with-in transformation deletes time constant variables by subtracting their mean values. Examples for those determinants are population by square kilometer, the number of available roof-tops to build a PV installation on and the household income in a given zip code district. The examples mentioned are assumed to be time invariant, as they are long-term influence factors and our available dataset covers the time span of 30 months only. The development of the dependent variable was explained in detail in the last section. We employ the stock of PV installation in a given zip code area, because it implicitly includes the already installed PV panels on the roof tops in this area and one does not have to choose a dynamic set-up to account for this important aspect.

The following model is estimated separately for the HH and BC group using the fixed effects approach, described above:

$$\begin{aligned} StockPV_{it} = \beta_i + \beta_1\ Pricewauto_{it-3}\ +\ \beta_2\ FiT_{t-1}\ +\ \beta_3\ Interestrate_{t-3} \\ +\ \beta_4\ Pricepanel_{t-3}\ +\ \beta_5\ Radiation12_{it-3}\ +\ \beta_6\ Fukushima_{t-3} \\ +\ \beta_7\ Trend_t\ +\ \beta_8\ Trendsq_t + \epsilon_{it} \end{aligned}$$

Subscript i refers to the zip code area and t represents the corresponding month under review. Most of the variables are lagged by three months. This is done in order to account for the delay between the investment decision and the actual construction on the rooftop itself. Three months seems to be realistic, as it has been already used in the literature, e.g. in Richter (2014).[18] The FiT is lagged one month only, because it is announced way in advance and is therefore easily anticipated by the investor.

Basically, the PV installation decision is assumed to depend on three groups of determinants.

First, the electricity generated via a PV panel can fulfill two purposes. On the one hand, it can be used to serve self-consumption needs, i.e. a household can use the generated electricity directly for any electricity related purpose. In that case, the PV investor gets a subsidy for every kwh self-consumed from the government and at the same time she avoids buying electricity from the electricity provider for those hours. Therefore, the opportunity cost, i.e. the electricity retail price, enters the investment decision. In the regression equation this relationship is combined in the *Pricewauto* variable. It comprises the electricity retail price and the auto-tariff for a given month, t, in a zip code, i. Furthermore the renewable energy surcharge has to be deleted from the variable in order to avoid direct endogeneity, because the renewable energy surcharge calculation for the present year/month directly depends on the last year/month PV installation amount. On the other hand, the produced electricity can be fed into the electricity grid. As a result, the PV investor is granted the corresponding feed-in tariff for its PV size (FiT). This variable only varies over time t, but not over cross-sectional units.

Second, there is the group of financial determinants that need to be controlled for. First of all, the PV module prices need to be considered and are directly included in the regression (via *Pricepanel*). This is the monthly crystalline price per panel from European origin. The variable is endogenous, but the problem is mitigated through the use of lagged dependent variables and the coefficient of the panel price variable

[18] Moreover, the PV industry specifies waiting times between 5 and 10 weeks for residential customers and 5 to 15 weeks for commercial customers. See http://www.pvgrid.eu/database/pvgrid/germany/ for verification.

is not used to infer causal interpretation. Furthermore, most PV constructors use external financing for their investment. This fact is represented by inclusion of the German monthly long-term interest rate (*Interestrate*).

Third, the weather conditions in a given zip code area change over time, therefore the irradiation level of the last 12 months is included (via *Radiation12*).

Fourth, the time structure of the period of consideration needs to be addressed. We include a linear time trend (*Trend*) to control for everything unobserved that changes over time, but is the same over all zip codes. Apart from that, a squared trend is also used (*Trendsq*) to control for the non-linearity in diffusion models and to represent the fact, that we look at the PV investment at a later level of diffusion, where a higher market saturation is expected. As already mentioned, we also assume a structural break in the data. We have data before and after the Fukushima catastrophe and there is evidence that people after the accident were more open towards energy production via renewable energy sources, like PV. Therefore the *Fukushima* Dummy is included.

In order to solve the simultaneity concern between electricity retail prices and the PV installations in a zip code area, the electricity retail prices are instrumented using Hausman instruments first presented in Hausman et al. (1994). They propose the use of prices in other cities as instruments for the price in a given city, the underlying assumption being that the underlying demand shocks are independent across regions. We follow this approach in order to get regional variation in the used instruments. Detailed cost shifting information (like the oil price) is unavailable at the zip code level in Germany and therefore cannot be applied. The Hausman instruments used are the prices of nearest neighbors of a given zip code in terms of population, expanse and house quota.[19] Each year the closest neighbor and the second closest neighbor prices are used as instruments for the price. The price instruments vary over zip code and months, and the corresponding tests of exogeneity and relevance are presented in the following results section.

[19]The house quota variable represents the share of houses containing one or two flats in one house. It is supposed to proxy the share of houses suitable for PV installations in the HH segment. All information is available on the municipality level on a yearly basis and is transformed in zip code level information using a key provided by Axciom. In the frequent case that we obtain more than one zip code per municipality key (AGS), the data is dis-aggregated with the number of households per zip code. This information is included in the underlying Verivox price dataset. The descriptive statistics and datasources are included in Table 4.2.

4.4.3 Results

Before the main results of the regression are presented we analyze the first stage of our two stage least squares regression in order to show that the instruments are relevant and exogenous. The first stage regression in Table 4.A1 in the Appendix demonstrates that the Hausman instruments are jointly relevant, as the F-Test (Test of excluded instruments) shows a high value of 106.28 and 107.87, so that the null hypothesis of the two instruments not being relevant can be rejected. This argument is fortified by the Stock and Yogo test. As the test statistic clearly exceeds the critical values derived in Stock and Yogo (2005), it can be argued that the loss in efficiency using TSLS regression compared to OLS is less than 10 %. The second property of exogeneity can be tested using the Hansen J statistic. The results are incorporated in Table 4.3 and suggest that the Null hypothesis is not rejected. Consequently, we can argue that our instruments are exogenous.

The results of the second stage of the instrumental regression are presented in Table 4.3.[20] In order to grant comparable results for all variables, the independent and dependent variables are calculated in natural logarithms, so that the coefficients can be interpreted as elasticities. The only exceptions are the trend variables and the Fukushima Dummy variable.

Basically, the results show that all coefficients incorporate the expected sign, but the magnitude of the coefficients varies between household customers and small business customers. The two variables of interest electricity retail price and feed-in tariff both enter the regression with a highly significant and positive coefficient. Regarding household customers, a 1% higher electricity price at the time of planning of the PV installation leads to a 9.3% higher stock of PV panels three months later in a given zip code area. This coefficient is statistically significant at the 1% level. The FiT coefficient for this customer group is slightly smaller and indicates that a 1% increase in FiT triggers a 7% increase in PV stock in a given zip code. They are similar in magnitude, so that the influence of own consumption and feed-in for this segment seems to be almost equally important for PV diffusion. From a governmental point of view the results suggest that a 1% decrease in the own consumption tariff or feed-in tariff are equally successful in slowing down the progress of PV investment for household customers. The picture changes, however, if business customers are considered. Our results for the period 2010 to 2012 show that a rise in the FiT of 1% leads to 3.8% more PV installations, almost half of the corresponding value for household customers. Surprisingly, the coefficient for the electricity retail price is 24%, so more than two times as large as for household customers. These results sug-

[20]The results change only slightly if the panel model is estimated in first differences. A bias due to Unit Roots is therefore not a problem in this regression.

Table 4.3: Second Stage Regression Results

	l_StockPV_HH	**l_StockPV_BC**
L3.l_Pricewauto_HH	0.093***	
	(0.008)	
L.l_FiT_HH	0.071***	
	(0.003)	
L3.l_Interestrate	-0.056***	-0.086***
	(0.002)	(0.003)
L3.l_Pricepanel	-0.250***	-0.680***
	(0.008)	(0.016)
L3.l_Radiation12	0.000***	0.000***
	(0.000)	(0.000)
L3.Fukushima	0.023***	0.023***
	(0.001)	(0.002)
Trend	0.018***	0.038***
	(0.000)	(0.001)
Trendsq	-0.000***	-0.001***
	(0.000)	(0.000)
L3.l_Pricewauto_BC		0.249***
		(0.014)
L.l_FiT_BC		0.038***
		(0.005)
Observations	18,117	18,117
R-squared	0.898	0.871
Number of zip codes	671	671
Hansen J statistic	0.007	0.100
p-value	0.934	0.751
Stock and Yogo value	106.3	107.9
10 % max IV size	19.93	19.93

Note: Fixed effects panel data model, instruments chosen nearest neighbors, cluster robust standard errors in parentheses; *** significant at the 1 % level; ** significant at the 5 % level; * significant at the 10 % level.

gest that the own-consumption tariff and the electricity retail bill for this customer group is way more influential in promoting or depressing PV installation growth. It also hints that the own consumption possibility seems to be more influential for the larger customer group than for owners of smaller PV plants, and might be one of the reasons why the German legislator abolished the auto consumption tariff from April 2012.

The financial control variables also exhibit the expected negative signs for both types of PV investors. A 1% increase in the interest rate leads, for HH, to 5.6% and for BC to 8.6% less PV installations. This effect stresses the point that the interest rate, as the price of loans, is also determining the PV investment choice. The second financial variable is the PV panel price itself, which we expect to enter the regression strongly negative. This variable shows negative values of 25% (HH) and 68% (BC). As already mentioned in subsection 4.4.2, this variable is, however, not the main result of interest and should be interpreted with care. Our results further show that the radiation level plays only a minor role for the investment decision in PV panels. Last year's sum of radiation is positively significant, but the coefficient approaches zero, so that no influence of this variable can be found. This is not very surprising, as the governmental subsidies during this period were substantial and granted to every PV investor no matter the climatic preconditions.

Moreover, the trend variable represents the expected positive value controlling for the use of the stock variable and the squared trend enters the regressions significantly negative. This is in line with Griliches (1957), as the period of 2010-2012 marks a time of slowing down growth rates of PV dispersion, because the diffusion process enters the declining part of the predicted S-shaped diffusion path.[21] The last interesting observation is the Fukushima dummy variable. It shows that the fact that the investment decision is made after the Fukushima nuclear disaster affects both customer types in a further positive way. The period after March 2011 shows an additional 2.3% of PV installations, controlling for all the other factors affecting PV investment already described. This is in line with e.g. Gallier et al. (2014). In other words the exogenous shock on nuclear energy in Japan influenced the German investment decision in PV panels, as a renewable energy source.

4.5 Conclusion

This work unfolded the determinants of PV diffusion in Germany for a unique panel datatset of 671 zip code areas for a 30 month time-span. We focus especially on

[21] See Table 4.1 for visual inspection on the aggregate level.

the interplay of electricity retail price (including an auto consumption tariff) and governmental feed-in tariffs on PV diffusion for two different types of PV investors. Our results show that both variables seem to substantially matter for the adoption of PV technologies. Nevertheless, the electricity retail price effect seems to dominate the feed-in tariff effect. This difference is way larger for small business investors than for household investors. Hence, the main policy implication is that if it is the government's aim to slow down the path of installation growth, the abolition of the auto consumption tariff in April 2012 is supposed to be more effective than the pure cutting of FiTs. Moreover, the influence of the electricity retail price should be taken into account for every PV diffusion research paper, which has not been done in most empirical research on PV adoption. Leaving it out neglects an important factor explaining PV diffusion.

Bibliography

Bass, F. M. (1969). A New Product Growth for Model Consumer Durables. *Management Science 15*(5), 215–227.

BDEW (2014). Erneuerbare Energien und das EEG: Zahlen, Fakten, Grafiken. *German Association of Energy and Water Industries*, Berlin, 1–95.

Beise, M. (2004). The International Adoption of Photovoltaic Energy Conversion. *International Journal of Energy Technology and Policy 2*(3), 272–283.

Bhandari, R. and I. Stadler (2009). Grid Parity Analysis of Solar Photovoltaic Systems in Germany using Experience Curves. *Solar Energy 83*(9), 1634–1644.

BMWI (2012). Die wichtigsten Änderungen der EEG-Novelle zur Photovoltaik 2012. *German Federal Ministry for Economic Affairs and Energy*, Berlin, 1–3.

BMWI (2014). Stromeinspeisegesetz vom 7.12.1990. `http://www.erneuerbare-energien.de/EE/Redaktion/DE/Dossier/eeg.html?cms_docId=72462`. [Accessed: 2014-12-01].

Bollinger, B. and K. Gillingham (2012). Peer Effects in the Diffusion of Solar Photovoltaic Panels. *Marketing Science 31*(6), 900–912.

BSW-Solar (2014). Statistische Zahlen der deutschen Solarstrombranche April 2014. *German Solar Association*, Berlin, 1–4.

Bundesnetzagentur and Bundeskartellamt (2014). Monitoringbericht 2013. *German Federal Network Agency and German Federal Cartel Office*, Bonn, 1–319.

Bundesregierung (2010). Energiekonzept - für eine umweltschonende, zuverlässige und bezahlbare Energieversorgung. *German Federal Government*, Berlin, 1–40.

Clearingstelle EEG (2014). EEG 2009 - (6) Fünftes Änderungsgesetz (sog. PV-Novelle). `http://www.clearingstelle-eeg.de/eeg2009_aenderung5`. [Accessed: 2014-12-12].

De La Tour, A. and M. Glachant (2013). How do Solar Photovoltaic Feed in Tariffs Interact with Solar Panel and Silicon Prices? An Empirical Study. *Working Paper*, MINES Paris Tech, Paris.

Energieagentur-NRW (2011). Erhebung Wo im Haushalt bleibt der Strom? Anteile, Verbrauchswerte und Kosten von 12 Verbrauchsbereichen in Ein- bis Sechs-Personen-Haushalten. http://1-stromvergleich.de/wp-content/uploads/erhebung_wo_bleibt_der_strom.pdf. [Accessed: 2015-02-01].

Gallier, C., A. Löschel, and B. Sturm (2014). Did Fukushima Matter? Empirical Evidence of the Demand for Climate Protection in Germany. *Applied Economics Letters 118*(12), 846–851.

Graziano, M. and K. Gillingham (2015). Spatial Patterns of Solar Photovoltaic System Adoption: The Influence of Neighbors and the Built Environment. *Journal of Economic Geography*, Forthcoming.

Griliches, Z. (1957). Hybrid Corn: An Exploration in the Economics of Technological Change. *Econometrica 25*(4), 501–522.

Guidolin, M. and C. Mortarino (2010). Cross-Country Diffusion of Photovoltaic Systems: Modelling Choices and Forecasts for National Adoption Patterns. *Technological Forecasting and Social Change 77*(2), 279–296.

Hausman, J., G. Leonard, and J. D. Zona (1994). Competitive Analysis with Differenciated Products. *Annales d'Economie et de Statistique 34*, 159–180.

Hoffmann, V. U. (2008). Damals war's - Rückblick auf die Entwicklung der Photovoltaik in Deutschland. *Sonnenenergie 6*, 38–39.

IEA (2014). IEA/IRENA Joint Policies and Measures Database - Global Renewable Energy. *International Energy Agency*, Paris.

IWR (2014). Act on Granting Priority to Renewable Energy Sources (Germany) (Renewable Energy Sources Act). http://www.iwr.de/re/iwr/info0005e.html. [Accessed: 2014-12-11].

Leepa, C. and M. Unfried (2013). Effects of a Cut-off in Feed-in Tariffs on Photovoltaic Capacity: Evidence from Germany. *Energy Policy 56*, 536–542.

Maubach, K.-D. (2013). *Energiewende - Wege zu einer bezahlbaren Energieversorgung.* Springer VS, Wiesbaden.

Mayer, J. N. and B. Burger (2014). Kurzstudie zur historischen Entwickung der EEG-Umlage. *Frauenhofer Institut für Solare Energiesysteme*, Freiburg, 1–17.

Müller, S. and J. Rode (2013). The Adoption of Photovoltaic Systems in Wiesbaden, Germany. *Economics of Innovation and New Technology 22*(5), 519–535.

Palmer, J., G. Sorda, and R. Madlener (2013). Modeling the Diffusion of Residential Photovoltaic Systems in Italy: An Agent-based Simulation. *Working Paper 9*, Institute for Future Energy Consumer Needs and Behavior (FCN), Aachen.

Prez, D., V. Cervantes, M. J. Bez, and F. T. Domnguez (2013). PV Grid Monitor Residential Sector. *ECLAREON*, Berlin, 1–94.

Richter, L.-L. (2014). Social Effects in the Diffusion of Solar Photovoltaic Technology in the UK. *EPRG Working Paper 1332*, University of Cambridge, UK.

Rode, J. and A. Weber (2012). Does Localized Imitation Drive Technology Adoption? A Case Study on Solar Cells in Germany. *Working Paper*, TU Darmstadt.

Rogers, E. M. (1962). *The Diffusion of Innovations* (1 ed.). Free Press, New York: NY.

Stock, J. and M. Yogo (2005). Testing for Weak Instruments in Linear IV Regression. In D. Andrews and J. Stock (Eds.), *Identification and Inference of Econometric Models: Essays in Honor of Thomas Rothenberg*, pp. 80–108. Cambridge University Press, Cambridge: UK.

Verivox (2010). Energiemarktreport 2009. *Verivox and Kreutzer Consulting*, Heidelberg, 1–121.

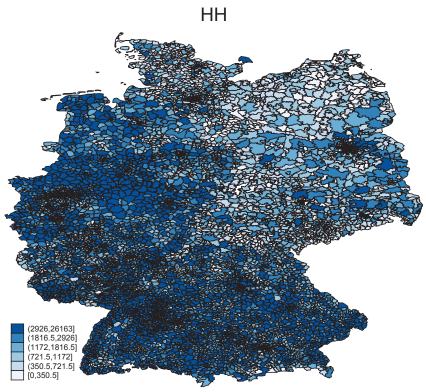
Zhang, Y., J. Song, and S. Hamori (2011). Impact of Subsidy Policies on Diffusion of Photovoltaic Power Generation. *Energy Policy 39*(4), 1958–1964.

Appendix

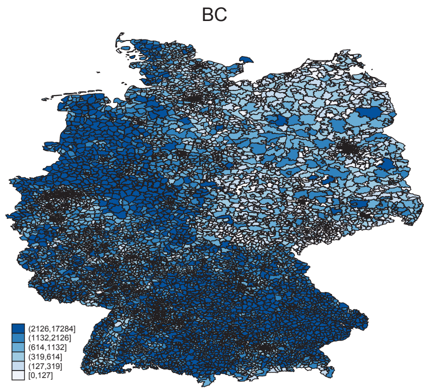
Data issues

Additional information PV installation data
The data alterations of the two PV panel sources already described are carried out in order to solve two problems, especially with the Federal Network agency's data. First, the registration date does not necessarily correspond with the commissioning date that is the relevant date for this analysis. In order to solve this problem a stepwise matching process is carried out: the registration date is varied by plus/minus two months. A second data issue that needs attention is the problem of solar parks. As this analysis only takes into account PV installations of households and small business customers, the solar parks have to be deleted from the original dataset. Installations are considered solar park modules if there exist more than three similar installations in terms of zip code, date, and nominal capacity, and if the nominal capacity exceeds 5 kWp. Some still dubious results are deleted via manual investigation of solar parks in Germany. After the data alterations, the merged dataset consists of 576,784 installations on a daily basis, which is 78% of the original Federal Network Agency data and 77% of the data points of the German Photovoltaic Association.

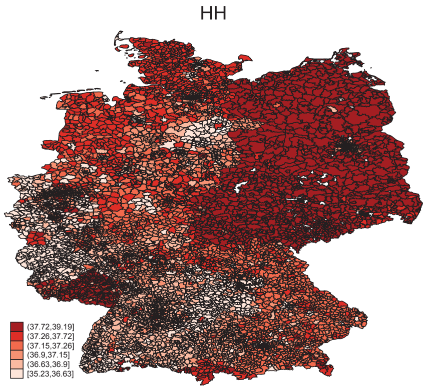
Figure 4.A1: Maps of PV Installation Stock until June 2012 and Average Electricity Retail price (incl. auto tariff), Radiation Level from January 2010 until June 2012



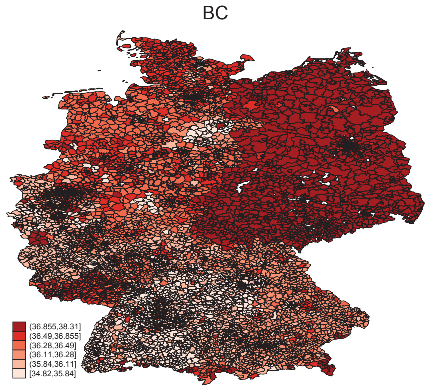
(a) PV installations (absolute)



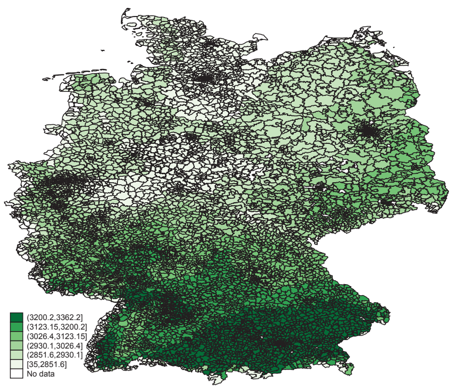
(b) PV installations (absolute)



(c) Average Price with auto tariff in ct/kWh

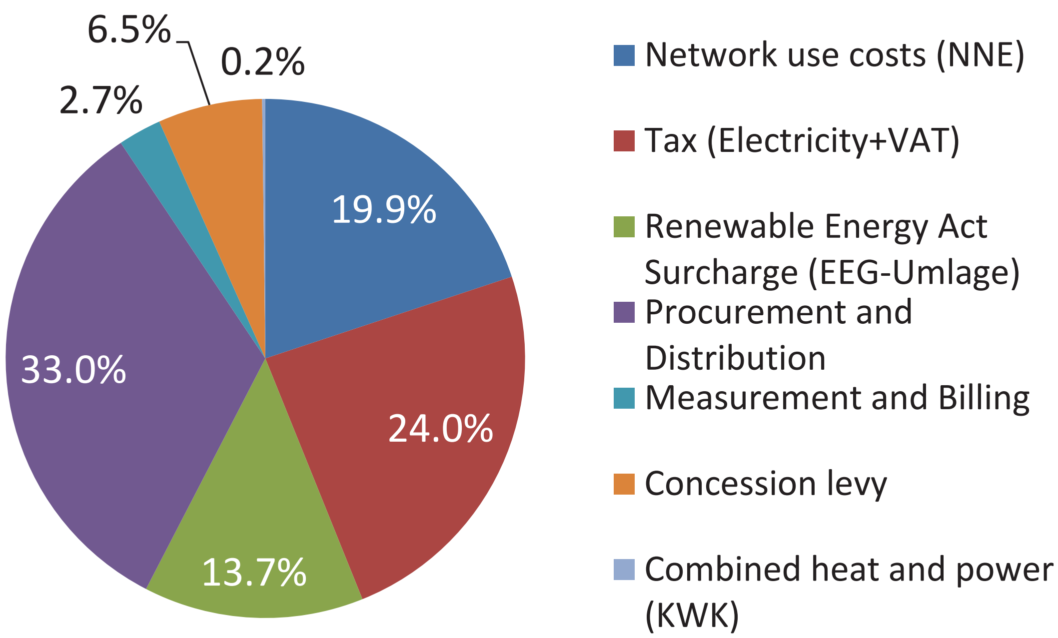


(d) Average Price with auto tariff in ct/kWh



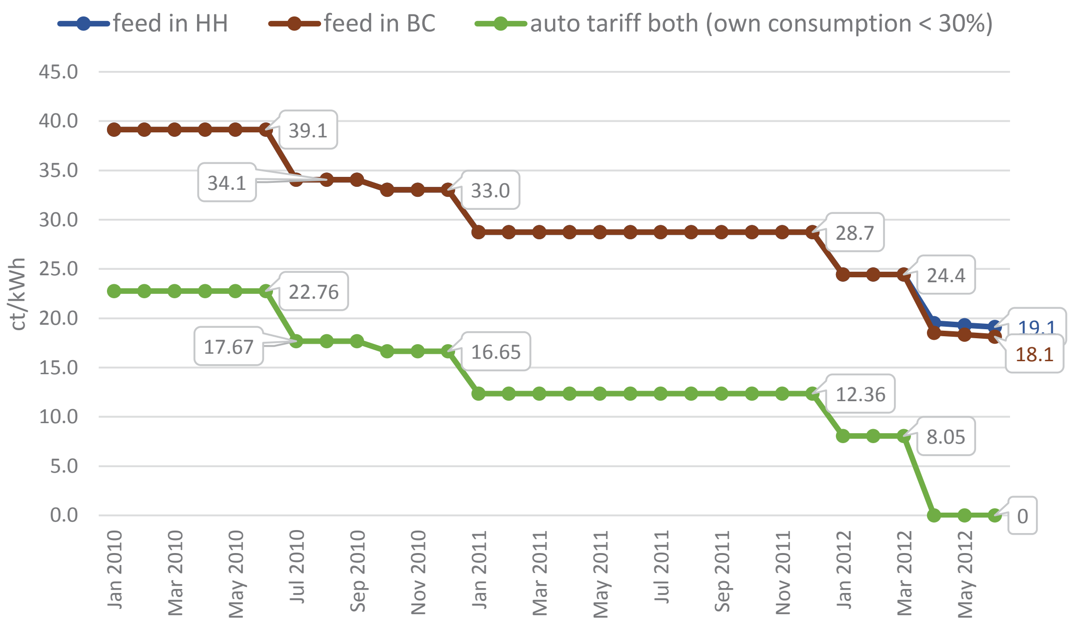
(e) Average daily Radiation last 12 months in Wh/m^2

Figure 4.A2: Composition of Household Electricity Price Bill April 2011 (3,500 kWh Consumption Level)



Source: Data taken from Bundesnetzagentur (2012), p. 145, design: Own graphic

Figure 4.A3: Feed-in and Auto-consumption Tariffs 2010-2012



Source: http://www.sfv.de/lokal/mails/sj/verguetu.html and http://www.photovoltaik-web.de/eigenverbrauch-pv/verguetungssaetze-eigenverbrauch.html

Table 4.A1: First Stage Regression Results

	L3.l_Pricewauto_HH	**L3.l_Pricewauto_BC**
L.l_FiT_HH	0.009***	
	(0.001)	
L3.l_Interestrate	-0.006***	-0.005***
	(0.001)	(0.001)
L3.l_Pricepanel	0.014***	0.011***
	(0.002)	(0.002)
L3.l_Radiation12	0.000*	0.000***
	(0.000)	(0.000)
L3.Fukushima	0.002***	0.002***
	(0.000)	(0.000)
Trend	-0.001***	-0.001***
	(0.000)	(0.000)
Trendsq	0.000***	0.000***
	(0.000)	(0.000)
L3.l_Pricewauto_NN1_HH	0.483***	
	(0.015)	
L3.l_Pricewauto_NN2_HH	0.462***	
	(0.015)	
L.l_FiT_BC		0.007***
		(0.001)
L3.l_Pricewauto_NN1_BC		0.490***
		(0.015)
L3.l_Pricewauto_NN2_BC		0.458***
		(0.015)
Number of zip codes	671	671
Observations	18,117	18,117
Shea Partial R squared	0.920	0.929
Test of excl. instruments	106.82	107.87
	(0.000)	(0.000)

Note: Fixed effects panel data model, instruments chosen nearest neighbors strategy, cluster robust standard errors in parentheses; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Chapter 5

Why are Economists so Different? Nature, Nurture and Gender Effects in a Simple Trust Game

Co-authored with Justus Haucap

Textual and methodological contributions of Andrea-Louise Müller:

- Contribution to research idea in experimental context
- Construction and execution of the experimental sessions
- Carrying out of the empirical analysis
- Writing of the first draft of the paper

____
Signature Co-author

5.1 Introduction

Economists are different from most other people. This is not so much a hypothesis anymore, but can safely be considered a received wisdom by now. Ever since Marwell and Ames (1981) conducted their famous experiment on the free-riding of economists, there has been a rather extensive body of literature on the forms, as well as on the sources of differences between economists and other individuals. The overwhelming majority of papers finds that economists do not only hold different values and views of the world (see, e.g., Gandal et al., 2005; Haferkamp et al., 2009; Haucap and Just, 2010; Jacob et al., 2011), but also report that economists are more selfish and less trustworthy than others (see, e.g., Carter and Irons, 1991; Frank et al., 1993, 1996; Frank and Schulze, 2000; Lundquist et al., 2009). A small minority of papers has found the opposite though (see, e.g., Yezer et al., 1996). With respect to trust games, economists are typically found to be both, less trusting and less trustworthy than other people.

Major parts of the literature on the behavior of economists focus on the question whether economists are different by nature even before they begin their studies, the argument being that economics students self-select into the study of economics (see, e.g., Carter and Irons, 1991; Frey and Meier, 2005; Cipriani et al., 2009), or whether students that study economics adopt different values or patterns of behavior over the course of their studies - the so-called nurture hypothesis (see, e.g., Stigler, 1959; Scott and Rothman, 1975; Haucap and Just, 2010). Haucap and Just (2010) provide evidence for the presence of nature effects which are strengthened through nurture. For a survey of much of the literature on the differences between economists and other people also see Kirchgässner (2005).

In another and almost completely unrelated stream of economic literature, a probably even less controversial finding has been reported and analyzed, namely that women are different and behave differently from men. The study of gender effects has been especially popular in the experimental and behavioral economics literature. As the excellent survey by Croson and Gneezy (2009) reports, an almost received wisdom is now that, if gender effects are found at all, women tend to be more careful (or risk-averse) and, therefore, less trusting than their male counterparts. At the same time, females tend to be more trustworthy (once they are trusted by others) if gender effects can be identified (see, e.g., Croson and Buchan, 1999; Schwieren and Sutter, 2008; Chaudhuri and Gangadharan, 2007). More recent surveys by Rau (2012) on trust games and by Ergun et al. (2012) on both trust and deception games basically support this view, even though some studies do not find any gender effects (see, e.g., Clark and Sefton, 2001).

Surprisingly enough, there has been, to the best of our knowledge, hardly any literature which combines these two strands of research even though some questions appear to be obvious such as: Are female economists predominantly female or predominantly economists or, put differently, do female economists behave more like typical economists (i.e., less trusting and also less trustworthy) or do they rather exhibit the behavior found to be typical for females in trust games (i.e., less trusting, but more trustworthy)? Given the literature above, a second question is obviously whether and how this behavior may be affected by studying economics. Interestingly, May et al. (2014) have recently found that male and female economists in the American Economic Association appear to differ rather substantially in their views on economic policy issues, such as health insurance, education, and labor standards. These survey-based results already provide some evidence that male and female economists may differ.

This paper aims at shedding some light on the questions just mentioned. For this purpose we have conducted a simple classroom experiment with (i) law students and economics students (ii) in both introductory and more advanced classes and found the following: Firstly, female economists are less trusting than both male economists and female (and male) law students, which may suggest that being female and an economist at the same time fortifies distrust in others. In addition, for female economics students the lack of trust appears to be further nurtured through the study of economics in an even stronger fashion than for male economics students. In sharp contrast, female law students become more trusting over the course of their studies. Secondly, and somewhat surprisingly, female economists are the least trustworthy group in our experiment both at the beginning of their studies and even more so when they are more advanced. We also find evidence for similar nurture effects among male economists and male law students who both become less trustworthy as their studies proceed, while we do not find these nurture effects for female law students who remain a highly trustworthy group.

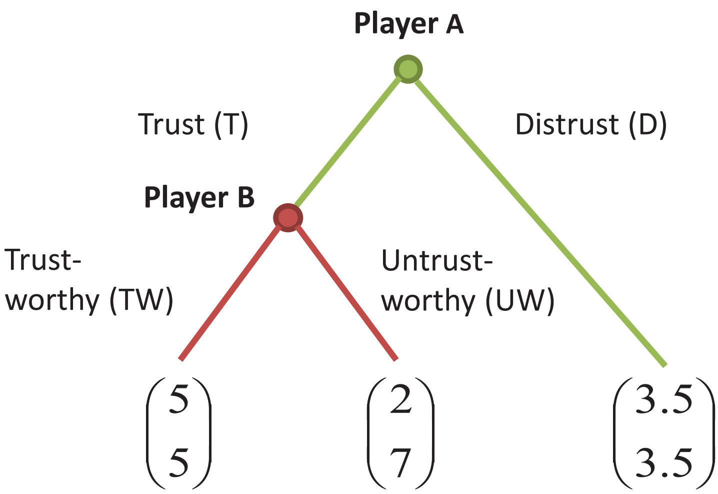
The rest of this paper is organized as follows: The experimental design will be described in detail in Section 5.2 before the results are reported in Section 5.3. Sections 5.4 offers a summary and concludes.

5.2 Experimental Design

The experiment is based on a sequential prisoner's dilemma game or binary trust game following Blanco et al. (2010). The game tree is given in Figure 5.1.

Two players, A and B, sequentially decide between two options. Player A can decide either to trust (T) or distrust (D) player B, before player B can decide to behave either trustworthy (TW) or untrustworthy (UW). If player A chooses to

Figure 5.1: Game Tree Binary Trust Game



distrust (D), the game ends and both players receive 3.50 EUR each. Player B's decision is irrelevant for the payoffs in this case. If player A decides to trust (T), player B's action is decisive for the payoffs of both A and B. If player B is trustworthy (TW), both players receive 5 EUR each, while player A is paid 2 EUR and player B 7 EUR if player B is untrustworthy (UW). Clearly, the only subgame perfect equilibrium of the game is (D, UW) so that a payoff of 3.50 EUR is predicted for each player. Note though that if, for some reason, player A does not expect player B to be a perfectly rational and selfish profit maximizer with certainty, player A's beliefs about player B's trustworthiness matter in our sequential trust game. In fact, trusting player B is optimal for player A if she believes that the probability of player B being trustworthy is at least 50 percent.[1]

The experiment was conducted in paper-based fashion during six different economics and law lectures in their usual class rooms at the University of Düsseldorf in 2012. Class room experiments were used in order to recruit typical economics and law students (without selection effects) in a natural environment where students usually also interact. The specific lectures were chosen so as to recruit economics

[1] Player A is indifferent between trust (T) and distrust (D) if

$$5p + 2(1-p) = 3.50 \Leftrightarrow 3p = 1.5 \Leftrightarrow p^* = 0.5$$

and law students in their respective introductory classes, as well as students with more advanced training. An overview of the respective lectures is provided in Table 5.A1 in Appendix A. Law students were chosen as a comparison group to economists since the absolute number of students is very similar and both economics and law have an almost equal percentage of male and female students. In contrast, most natural sciences have a male-female student ratio of about 4:1 while many other social sciences and humanities show almost the opposite ratio of male to female students.[2] This distinct study pool composition is crucial for our work, as we analyze the behavior of students in eight subcategories (gender, study major and progress of study). Hence, we need to make sure that there are enough observations in each subgroup to draw inference.

The experiment was conducted using the so-called strategy method, where subjects have to make a decision in both roles, as player A as well as player B. The final role (A or B) was later randomly assigned to individuals after they had marked their decision. Hence, only one of the players' own two choices was in the end decisive for individuals' payoffs.[3] Players were randomly matched after all choices had been made. The experiments were conducted in five steps: First, every student was given instructions with control questions to ensure that participants understood the game. Second, the experimenter distributed and collected the decision sheets where individuals marked their player A decision (T or D) and their player B decision (TW or UW) as well as a questionnaire on individual characteristics like gender, age, study information and questions on risk attitude and beliefs.[4] Third, the students attended the lecture. Fourth, the experimenter randomly matched student pairs and then analyzed the data outside the class room while the students attended the lecture. Fifth, after the end of the lecture, students were paid according to their own and their assigned partner's choice. Only one randomly assigned role was payoff-relevant.

[2]Psychology, for example, has a female student percentage of 86 percent in Düsseldorf, while mathematics only has 30 percent female students. The composition of the student pool at the University of Düsseldorf is summarized online at http://www.hhu.de/home/universitaet/weiterfuehrend/die-universitaet-in-zahlen-und-fakten/die-universitaet-in-zahlen/studierendenstatistik.html.

[3]Brandts and Charness (2011) compare outcomes of games using the strategy and the direct response method and find that in 25 out of 29 studies surveyed there was no significant difference between the two methods. We, therefore, use the strategy method in order to obtain more observations and also to enhance the understanding of the game as a whole, as students are forced to think through both players' decisions.

[4]The instructions, control questions, and the questionnaire are provided in Appendix B. Every decision sheet additionally held a participation number, so that the students were able to claim their corresponding payment after the end of the lecture.

The six experimental sessions resulted in an overall sample size of 577 students. All of them made their decisions in the role of player A and player B. 51 percent of the students are female, and 52 percent are economists. Hence, we have an almost equal split between the various groups. About two thirds of the participants were first-year students without previous training in economics or law. The share of students that have a minor in economics or have already changed their field of study is small, one and nine percent, repectively.[5]

5.3 Results

5.3.1 The Trust Decision (Player A)

Descriptive results (including test statistics) of the trust decision for economics vs. law students, male vs. female students, and first-year vs. advanced students are presented in Figure 5.A1 in Appendix A. Fewer economists and fewer female students tend to trust in their partner's trustworthiness than law students and male students (both significant at least at the 5 percent level).[6] These findings are pretty much in line with the literature on trust games in combination with gender issues[7] and almost replicate the trust results in Dasgupta and Menon (2011), who find in their study that 43 percent of the economists trust.

The results become more interesting once we further split the sample. The bars in Figure 5.2 represent the percentage of trusting individuals in the eight possible subgroups. The first four bars (from left to right) show the trusting decision of economists, grouped by gender and the second four bars the decision of the law students, also divided between females and males. The left bar in each group represents first semester choices, whereas the second bar indicates decisions of the more advanced students. Advanced female economists are the least trusting group with only 23 percent trusting while the fraction of trusting students is highest among advanced female law students (80 percent). The difference between these two groups is much smaller during the first year of study when 47 percent of first-year female law students trust and 39 percent of first-year female economics students.

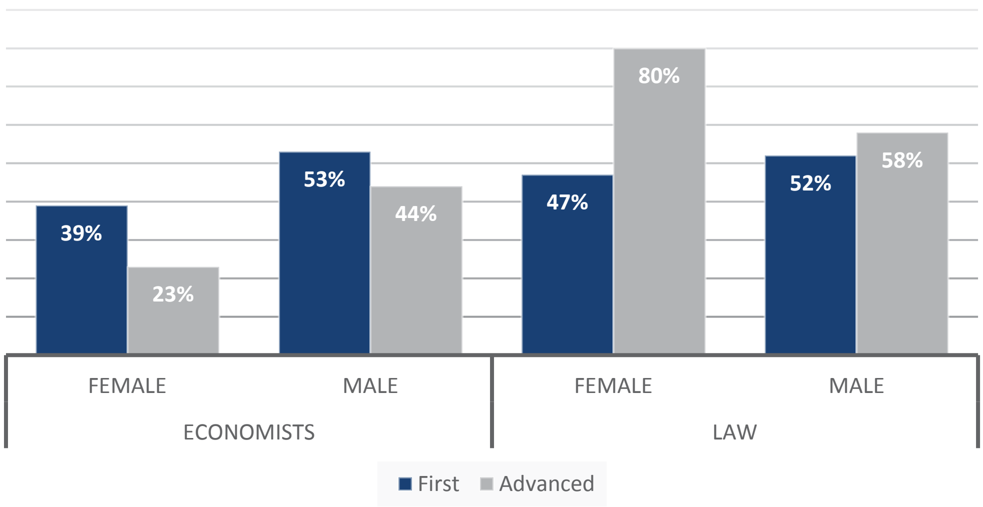
Note that the fraction of trusting subjects among female economics students varies by progress of study. Among the older female economists 39 percent trust,

[5]Further details are given in Table 5.A2 in Appendix A.

[6]We use the Chi Square test to test the difference between the categorical variables gender, major and study level.

[7]See Croson and Gneezy (2009), Table 3 for an extensive overview of experiments in trust games.

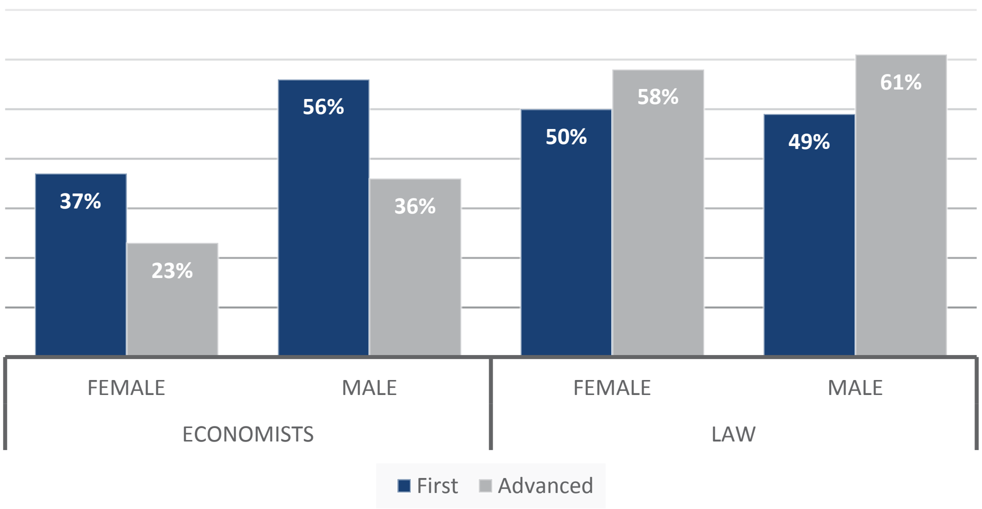
Figure 5.2: Share of Trusting Students Within Subgroups



whereas among their first semester counterparts only 23 percent, a difference of 16 percentage points. The opposite effect is found among female law students, where 47 percent of the beginners trust, compared to 80 percent among the more advanced. This may suggest that learning effects are rather strong among female students. For their male counterparts, the direction of movement is similar but on a much smaller scale. The trust level among male law students is higher for the more advanced with 58 percent trusting choices, compared to 52 percent among the freshman law students. The effect for male economists goes in the opposite direction, with a share of 53 percent trusting choices at the beginning of study and a lower level of 44 percent for the corresponding first semester subgroup.

Also note that the trust levels are very similar between male law and economics students in the first year (52 and 53 percent - the difference is statistically not significant), and the two fractions of trusting students are higher than among both female law students in their first year (47 percent) and female economics students in their first year (39 percent). Hence, at the beginning of their studies gender effects appear to dominate any nature effects with respect to the field of study, i.e., females are primarily females and, secondly, economists (or lawyers) when they enter university.

Figure 5.3: Share of Trustworthy Students Within Subgroups



5.3.2 The Trustworthiness Decision (Player B)

Figure 5.A2 in Appendix A summarizes player B's decisions (whether or not to be trustworthy) for economics vs. law students, male vs. female students, and first-year vs. advanced students. Not very surprisingly, economists are less trustworthy than law students (significant at the 1 percent level). This result is comparable to Dasgupta and Menon (2011). More surprisingly, 49 percent of the male students are trustworthy, but only 41 percent of the female students in our game (significant at the 5 percent level). This finding contrasts with results from other trust games, summarized in Croson and Gneezy (2009), which typically find women to be more trustworthy than men. Finally, first-year students are more trustworthy than advanced students (significant at the 5 percent level).

As before, the detailed analysis of our eight subgroups (female/male - economics/law students - first-year/advanced) provides some deeper insights. The results are summarized in Figure 5.3 and can be interpreted similar to Figure 5.2. As can be easily seen, advanced female economists are not only the least trusting group (when acting as player A), but also the least trustworthy one. Only 23 percent of the advanced female economics students decide to be trustworthy while among first-year female economists 37 percent still act trustworthily. Similarly, the level of trustworthiness declines among male economics students from 56 percent among first-year male economics students to 36 percent among advanced male economics students. Note that while the decline is stronger in absolute terms among male economists, when compared to their female economist companions (-20 percentage points for males, -14 percentage points for females), the relative decline is almost similar (36 percent for males, 38 percent for females). In contrast, trustworthiness increases among law students. First-year female law students decide to be trustworthy in 50 percent of all cases (compared to 49 percent among their male colleagues) while the respective figures for advanced law students are 58 (female) and 61 (male) percent. Hence, with respect to the trustworthiness decision, there do not appear to be differences in learning between male and female students once we control for their field of study.

5.3.3 Regression Analysis

In order to isolate the effects of gender, subject and progress of study that affect trust and trustworthiness among students, we estimate a seemingly unrelated bivariate probit model with standard errors clustered at the class level, where i represents the corresponding student. This identification is derived directly from the experimental set-up. For player A's decision the beliefs about the reciprocity level of the group are decisive, whereas for player B they are irrelevant. Furthermore, the two decisions are made by the same student, so that we need to avoid correlation of the error terms.[8]

The regression equation looks as follows:

$$\begin{aligned} Decision_i = {} & \beta_1\, FemaleFirstEcon_i \; + \; \beta_2\, FemaleAdvEcon_i \; + \; \beta_3\, MaleFirstEcon_i \\ & + \beta_4\, MaleAdvEcon_i \; + \; \beta_5\, FemaleFirstLaw_i \; + \; \beta_6\, FemaleAdvLaw_i \\ & + \beta_7\, MaleAdvLaw_i \; + \; \sum_{k=8}^{K} \beta_k\, Controls_i \; + u_i \end{aligned}$$

The two decisions are estimated in two separate regressions (*A decision* and *B decision*). Male first-year law students (*MaleFirstLaw*) serve as the reference category. The coefficients displayed are average marginal effects. Hence, they can be interpreted as the percentage change associated with each respective subgroup compared to male first-year law students. Furthermore, we include control variables

[8]The test for a bivariate model being necessary is given in the last row of Table 5.1. As can be seen, the null hypothesis ($\rho = 0$) can be rejected at the 1 percent significance level.

such as the student's age (*Age*), their risk attitude (*Risk*), whether they actually study or have in the past studied economics as a minor (*Minor_Econ*)[9], whether they had a course with economics content in high school (*Econ_School*), whether they have changed their field of study in the past (*Study_Change*), whether the number of students in the class exceeds 50 (*Sizemore50*) and the student's belief about the fraction of untrustworthy students in their particular class (*Beliefs*). The results are summarized in Table 5.1.

The regression analysis confirms what we have seen in our descriptive analysis. Advanced female economists are the least trusting subgroup, followed by first-year female economics students. While the trusting behavior of male first-year economics students is statistically not different from first year male law students (as inspection of Figure 5.2 already suggests), the male economists' trust vanishes as their studies progress. Hence, for both male and female economics students we find a nurture effect regarding their trusting decision, while we only find a nature (or self-selection) effect for female economics students who are already significantly less trusting when they take up their studies. Among law students we find that female law students trust less than their male counterparts when they enter university, but more when they have advanced in their study. For law students, we can only identify learning effects for female students. With respect to our control variables it is not surprising that beliefs about an increasing fraction of untrustworthy students in the class and a larger class size decrease the likelihood to trust. Furthermore, older students have a stronger tendency to trust. All other control variables exhibit statistically insignificant coefficients.

Regarding the trustworthiness decision, the descriptive impressions of Figure 5.3 are basically also supported by our regression analysis. Among economists, female students are the main driving force behind the lower trustworthiness levels compared to law students. Advanced female economists are 25 percent less likely to be trustworthy than a fellow advanced female law student, and even first-year female economics students are 15 percent less likely to be trustworthy than their fellow first-year female students of law. For male students, the comparable figures show that the probability of an advanced male economics student being trustworthy is about 22 percent lower than for an advanced male law student. Somewhat surprisingly, among male first-year students the likelihood of an economist being trustworthy is about 8 percent higher than for a law student. Note, however, that only male law students become more trustworthy as their studies proceed, while we do not find a similar learning effect for female law students. Among the control variables a minor

[9]Note that this applies to about two percent of the law students (one percent of all students, but obviously only applicable to law students, and not to economists).

Table 5.1: Bivariate Probit Regression of Trust and Trustworthiness Decision

	A decision	B decision
Female-First-Econ	- 0.110***	- 0.149***
	(0.03)	(0.024)
Female-Adv-Econ	- 0.267***	- 0.250***
	(0.03)	(0.043)
Male-First-Econ	- 0.014	0.083***
	(0.018)	(0.008)
Male-Adv-Econ	- 0.103**	- 0.112***
	(0.041)	(0.013)
Female-First-Law	- 0.047***	0.000
	(0.004)	(0.005)
Female-Adv-Law	0.153**	0.013
	(0.066)	(0.067)
Male-Adv-Law	- 0.015	0.116***
	(0.07)	(0.035)
Age	0.015***	0.005
	(0.004)	(0.003)
Minor_Econ	0.069	- 0.298**
	(0.043)	(0.133)
Risk	0.001	0.000
	(0.001)	(0.001)
Econ_School	- 0.033	- 0.063
	(0.029)	(0.042)
Study_Change	- 0.068	0.043
	(0.071)	(0.057)
Sizemore50	- 0.030**	- 0.014
	(0.013)	(0.03)
Beliefs	- 0.005***	-
	(0.001)	-
No of obs	549	549
Wald test of $\rho = 0$	$\chi^2 = 8.54$	Prob> $\chi^2 = 0.0035$

Note: Seemingly unrelated bivariate probit regression with clustered standard errors; average marginal effects displayed; reference category for interactions: Male-First-Law;
A-decision=1 is the trusting possibility and B-decision=1 is the trustworthy choice; Standard errors in parentheses; *** significant at 1 percent level ** significant at 5 percent level * significant at 10 percent level.

in economics is associated with a reduction of the likelihood to be trustworthy of about 30 percent (statistically significant at the 5 percent level).

A further pairwise comparison of the regression coefficient shows that differences between female law and economics students are much larger than those between male students. This finding already applies to first-year students, but the gap widens as the students progress in their respective studies. Regarding the trust level (player A), the difference in the coefficients for first-year female economics and law students is -0.110-(-0.047) =-0.063 while there is statistically no difference between the trust levels of first-year male law and economics students. The gap widens between advanced students, where the difference is -0.267-0.153=-0.420 for female economics and law students and -0.103 for their fellow male students. A similar pattern can be observed regarding trustworthiness levels: Among first-year students, female economists are about 15 percent less likely to be trustworthy than female law students, and this number increases to 25 percent among advanced female students. Again, the comparable differences between male economics and law students are 0.083 among first-year students and -0.112-0.116=-0.228 among advanced students. Hence, we find that differences in the behavior of female law and economics students tend to be larger than those between male students. For both male and female students these differences increase as students progress with their respective studies.

Quite generally, our results suggest that both nature and nurture effects are at work when explaining levels of trust and trustworthiness among economists, but that nurture or learning effects appear to be more pronounced among female economists.

5.4 Conclusion

This paper has analyzed the behavior of 577 economics and law students in a simple binary class-room trust experiment. While economists are both significantly less trusting and trustworthy than law students, this difference is initially largely due to differences between female law and economics students. While female law and economics students are already different in nature (during the first term of their respective studies), the gap between them also widens more drastically over the course of their study compared to their male counterparts with respect to their trust level. Regarding trustworthiness we find nurture effects for both male and female economists which made them less trustworthy and more selfish, while we find an opposite nurture effect for male law students. In our view these findings are rather critical as the detailed composition of students, apart from gender, is typically neglected in most experiments reported in the economics literature. Our results show, however, that study majors and progress of study also affect trust and trusting levels in experimental games. In order to provide the possibility of replication, this information should be displayed in every trust game study using students as experimental subjects.

Bibliography

Blanco, M., D. Engelmann, A. Koch, and H. Normann (2010). Belief Elicitation in Experiments: Is There a Hedging Problem? *Experimental Economics 13*(4), 412–438.

Brandts, J. and G. Charness (2011). The Strategy- versus the Direct-Response Method: A First Survey of Experimental Comparisons. *Experimental Economics 14*(3), 375–398.

Carter, J. and M. Irons (1991). Are Economists Different, And If So, Why? *Journal of Economic Perspectives 5*(2), 171–177.

Chaudhuri, A. and L. Gangadharan (2007). An Experimental Analysis of Trust and Trustworthiness. *Southern Economic Journal 73*(4), 959–985.

Cipriani, G. P., D. Lubian, and A. Zago (2009). Natural Born Economists? *Journal of Economic Psychology 30*(3), 455–468.

Clark, K. and M. Sefton (2001). The Sequential Prisoner's Dilemma: Evidence on Reciprocation. *Economic Journal 111*(468), 51–68.

Croson, R. and N. Buchan (1999). Gender and Culture: International Experimental Evidence from Trust Games. *American Economic Review 89*(2), 386–391.

Croson, R. and U. Gneezy (2009). Gender Differences in Preferences. *Journal of Economic Literature 47*(2), 448–474.

Dasgupta, U. and A. Menon (2011). Trust and Trustworthiness Among Economics Majors. *Economics Bulletin 31*(4), 2799–2815.

Ergun, S., T. García-Muñoz, and M. F. Rivas (2012). Gender Differences in Economic Experiments. *Revista Internacional de Sociología 70*, 99–111.

Frank, B. and G. Schulze (2000). Does Economics Make Citizens Corrupt? *Journal of Economic Behavior & Organization 43*(1), 101–113.

Frank, R., T. Gilovich, and D. Regan (1993). Does Studying Economics Inhibit Cooperation? *Journal of Economic Perspectives 7*(2), 159–171.

Frank, R., T. Gilovich, and D. Regan (1996). Do Economists Make Bad Citizens? *Journal of Economic Perspectives 10*(1), 187–192.

Frey, B. S. and S. Meier (2005). Selfish and Indoctrinated Economists? *European Journal of Law and Economics 19*(2), 165–171.

Gandal, N., S. Roccas, L. Sagiv, and A. Wrzesniewski (2005). Personal Value Priorities of Economists. *Human Relations 58*(10), 1227–1252.

Haferkamp, A., D. Fetchenhauer, F. Belschak, and D. Enste (2009). Efficiency Versus Fairness: The Evaluation of Labor Market Policies by Economists and Laypeople. *Journal of Economic Psychology 30*(4), 527–539.

Haucap, J. and T. Just (2010). Not Guilty? Another Look at the Nature and Nurture of Economics Students. *European Journal of Law and Economics 29*(2), 239–254.

Jacob, R., F. Christandl, and D. Fetchenhauer (2011). Economic Experts or Laypeople? How Teachers and Journalists Judge Trade and Immigration Policies. *Journal of Economic Psychology 32*(5), 662–671.

Kirchgässner, G. (2005). (Why) Are Economists Different? *European Journal of Political Economy 21*(3), 543–562.

Lundquist, T., T. Ellingsen, E. Gribbe, and M. Johannesson (2009). The Aversion to Lying. *Journal of Economic Behavior & Organization 70*(1), 81–92.

Marwell, G. and R. Ames (1981). Economists Free Ride, Does Anyone Else? Experiments on the Provision of Public Goods. *Journal of Public Economics 15*(3), 295–310.

May, A., M. G. Mcgarvey, and R. Whaples (2014). Are Disagreements Among Male and Female Economists Marginal at Best? A Survey of AEA Members and Their Views on Economics and Economic Policy. *Contemporary Economic Policy 32*(1), 111–132.

Rau, H. (2012). Trust and Trustworthiness: A Survey of Gender Differences, in S. McGeown (Ed.). *Psychology of Gender Differences* , 205–224.

Schwieren, C. and M. Sutter (2008). Trust in Cooperation or Ability? An Experimental Study on Gender Differences. *Economics Letters 99*(3), 494–497.

Scott, J. H. and M. P. Rothman (1975). The Effect of an Introductory Economics Course on Student Political Attitudes. *Journal of Economic Education 6*(2), 107–112.

Stigler, G. (1959). The Politics of Political Economists. *Quarterly Journal of Economics 73*(4), 522–532.

Yezer, A. M., R. S. Goldfarb, and P. J. Poppen (1996). Does Studying Economics Discourage Cooperation? Watch What we Do, Not What we Say or How we Play. *Journal of Economic Perspectives 10*(1), 177–186.

Appendix A

Figure 5.A1: A Decisions by Field, Gender and Progress of Study

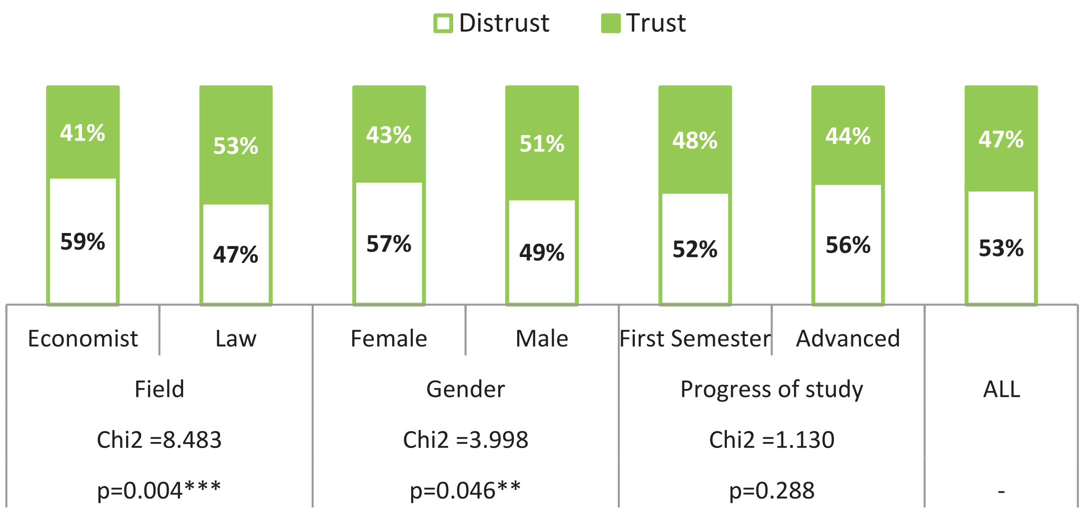


Figure 5.A2: B Decisions by Field, Gender and Progress of Study

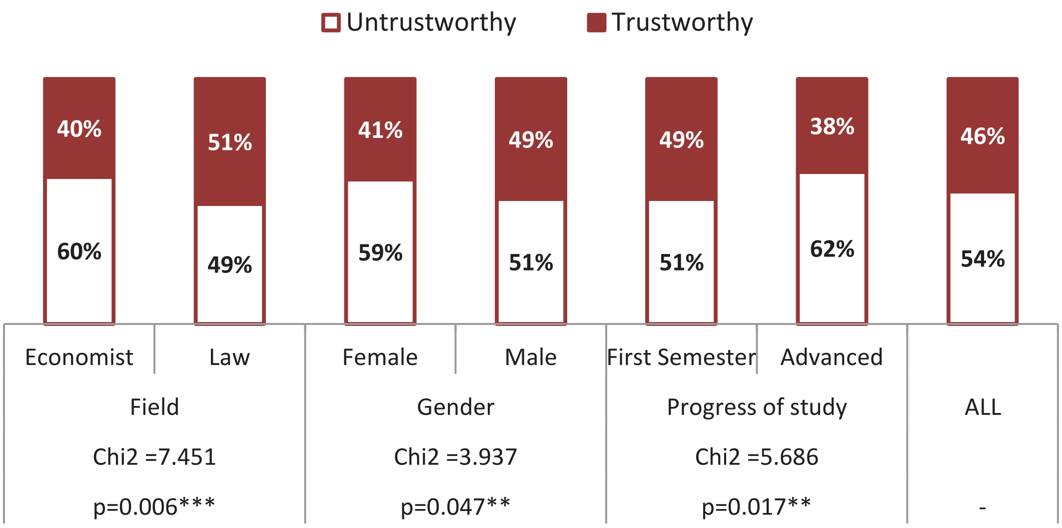


Table 5.A1: Overview Over All Sessions

	Course	Major	Students	Term
Session 1	Economic Policy	Economics	85	Summer 2012
Session 2	Economic Policy	Economics	36	Summer 2012
Session 3	Municipal Law	Law	48	Summer 2012
Session 4	German Civil Code	Law	231	Winter 12/13
Session 5	Microeconomics	Economics	99	Winter 12/13
Session 6	Microeconomics	Economics	79	Winter 12/13

Table 5.A2: Summary Statistics

Variable	Description	Obs	Mean	Std. Dev.	Min	Max
A decision	Dummy (1=T)	577	0.47	0.50	0	1
B decision	Dummy (1=TW)	577	0.45	0.50	0	1
Economist	Dummy	577	0.52	0.50	0	1
Female	Dummy	577	0.51	0.50	0	1
#Semesters	Absolute	575	2.11	1.80	1	9
First semester	Dummy	577	0.67	0.47	0	1
Age	Absolute	573	21.30	2.84	16	44
Minor_Econ	Dummy	556	0.01	0.10	0	1
Risk	Absolute amount	575	36.49	31.80	0	100
Econ_School	Dummy	577	0.33	0.47	0	1
Study_Change	Dummy	575	0.09	0.29	0	1
Sizemore50	Dummy	577	0.71	0.46	0	1
Beliefs	Percentage	574	68.91	24.66	5	100
Payoff	EUR	577	4.05	1.44	2	7

Appendix B

B.1 Instructions

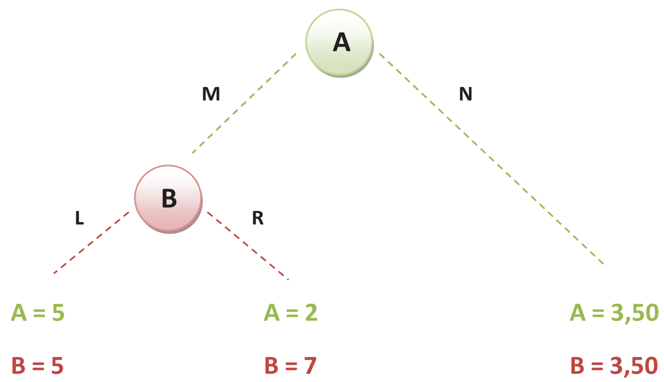
Welcome to the decision experiment! Please read the instructions carefully.

Introduction

During the experiment you are making decisions that allow you to earn money. All amounts indicated are in Euros. The sum of money you earn depends on your decision and on the decision of other participants. The experiment takes place anonymously so that you will not know the other participant with whom you interact. Except from the experimenter, only you will know the result and the amount of money you are going to earn. Please note that from now on and during the whole experiment you are not allowed to communicate with other participants. If this is the case, we have to stop the experiment. If you have any questions, please raise your hand and the experimenter will come to you. At the end of these instructions you will find some control questions. These control questions give you and the experimenter the last chance to check whether you understood the instructions for this experiment. Your performance in answering the control questions have no effect on your earnings from this experiment.
In a second step we will distribute the decision sheets. Decisions you state on this sheet are the foundation of your earnings.
The third and final stage of the experiment consists of completing the personal questionnaire truthfully.
After the course the realized earnings will be paid by the experimenter.

Experimental proceedings

The foundation of the experiment is the following game:



Two players A and B sequentially decide between two alternatives. The numbers indicate how many € each player can earn with her decision. The top, green number show the earnings for player A, the lower, red number the earnings for Player B.
Player A can choose between strategy "M" and strategy "N". If he opts for strategy "N", the decision of the other player becomes irrelevant, the game is therefore over, and both players receives 3.50 €. If player A chooses strategy "M," the decision of player B determines the payoffs of both players. Player B can choose between strategy "L" and strategy "R". If he chooses "L", player A and player B earn 5 € each. If he opts for "R", player A earns 2 € and player B 7 €.
You and all other participants of the experiment will make one decision in the role of Player A and one decision in the role of Player B. Beforehand you do not know what choice the other player makes, and you are unaware what role is actually used to determine your earnings. After your decision, it is randomly determined with equal probability whether you are player A and the other player B, or the other player A and you are player B. Please answer the following control questions.

Control questions

Question 1:
You are player A.
Assume that player B chooses strategy R. What is your payoff if you...

a) ...choose strategy M? _____€ What will B earn? _____€

b) ...choose strategy N? _____€ What will B earn? _____€

Question 2:
You are player B.
Assume that player A chooses strategy M. What is the amount of money you earn if you...?

a) ...choose R? _____€ What will A earn? _____€

b) ...choose L? _____€ What will A earn? _____€

Question 3:
You are player B.
What payoff do you earn for each of your corresponding decision possibilities, if player A chooses strategy N?

a) For strategy L? _____€ What will A earn? _____€

b) For strategy R? _____€ What will A earn? _____€

B.2 Questionnaire

1. What percentage of your fellow students do you think did choose decision ”R” as player B?
   ___%

2. You are . . . ? ☐ female ☐ male

3. How old are you?
   ____

4. What is the level you currently take courses in?
   ☐ Bachelor ☐ Master ☐ Diploma (German equivalent to Master)
   ☐ State examination (German equivalent to LL.M) ☐ other

5. How many semesters have you been studying? ____________________

6. What is your study major? ______________________________

7. Do you take an economics-related class as a minor subject? ☐ Yes ☐ No
   If yes, which one? ______________________________

8. Did you change subjects during your study? ☐ Yes ☐ No
   If yes, from which subject? ______________________________

9. Did you take a course with an economic focus in your last two years of secondary school education? (e.g. Law and economics, Politics,. . .)
   ☐ Yes ☐ No

10. Imagine that you win 100 € in a lottery. You can deposit the whole amount, just a share or nothing in your bank account. The sum that you deposit will double with a probability of 50% or it will bisect with probability of 50%. Which sum will you pay into your account?
    ☐ Everything, I deposit 100 €
    ☐ 80 €
    ☐ 60 €
    ☐ 40 €
    ☐ 20 €
    ☐ Nothing, I keep 100 €

Thank you for your participation!

Chapter 6

Conclusion

This thesis showed the use of modern empirical methods like time-series, panel and seemingly unrelated probit regressions in four fields of application: the automotive market, the RES market and behavioral economics.

In Chapter 2, scrappage premiums worldwide were analyzed with the help of simulations based on dynamic panel methods. The results suggest, that the policies were, in general, successful in the promotion of automotive sales in times of economic recessions. However, the policies were not equally beneficial. The simulation results point out that for some countries more cars were sold compared to a counterfactual situation, even if the assigned budget was far smaller.

Those more general results were complemented by the study presented in Chapter 3: the time-series analysis of the German car scrappage scheme, which was the program with the second largest budget. A comparison of the realized number of cars sold and the counterfactual for a disaggregated data set on the car segment level reveals no substantial pull-forward effect. This has been shown for small and upper small cars, the ones that were mostly sold under the program, so that the German policy was mostly able to achieve its goals. A further interesting area of research would be to further disentangle the competitive effects on the single car producers under the program. Who gained and who lost disproportionally? Furthermore, it would be an improvement to carry out a separate regression and include the used car market, because this part of the market was also substantially affected by the government intervention.

In a fourth Chapter, PV diffusion in Germany has been carefully assessed, and the most important result is that the price elasticity of the electricity retail price is way bigger than the elasticity of the FiT. Consequently, the policy implications are that a cut of taxes or subsidies by the government would be far more effective than a further increase of the FiTs in order to gain a higher diffusion of PV installations. This effect might have changed over the last couple of years, as the own-consumption possibility that drives this effect became more technically feasible in the period under consideration (2009 to 2012). For further research it would be interesting to see if the obtained results still hold if a more recent time period is inspected. The data was unfortunately not available for a longer time period. This section also stressed, that leaving out the electricity retail price, as in most PV diffusion research papers, misses a crucial, not only a minor determinant of PV diffusion.

The last chapter, Chapter 5, demonstrates that the gender and economist vs. non-economist differences already discovered in experimental research are mainly driven by female economists. They are found to trust and reciprocate substantially less than their male counterparts and also less than other law students. The main goal of this study was to stress that the subject pool is of crucial importance in economic experiments. Not controlling for gender and study major can lead to biases.

Eidesstattliche Versicherung

Ich, Andrea-Louise Müller, versichere an Eides statt, dass die vorliegende Dissertation von mir selbstständig, und ohne unzulässige fremde Hilfe, unter Beachtung der "Grundsätze zur Sicherung guter wissenschaftlicher Praxis an der Heinrich-Heine-Universität Düsseldorf" erstellt worden ist.

Düsseldorf, der 8. April 2015

Unterschrift