EMPIRICAL ESSAYS ON WAGE DYNAMICS AND DONATION OPTIONS

Inauguraldissertation
zur Erlangung des akademischen Grades eines Doktors der
Wirtschaftswissenschaften der Universität Mannheim

vorgelegt von
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13. Dezember 2012

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Tag der mündlichen Prüfung:	21. März 2013

Acknowledgements

I would never have got this far by now without the support of numerous people. First of all, I like to thank my supervisor Prof. Dr. Dr. h.c. mult. Wolfgang Franz for having given me the opportunity to be one of his last doctoral students in his academic career. The approaching retirement date has been very motivating, possibly for the both of us.

I owe a very big thank you to my second supervisor, Nicole. You have supported me exceptionally well throughout my entire doctoral stage. Thank you for many stimulating conversations, for being approachable at all times and for providing me with the necessary liberties to be able to hand in the dissertation at this stage.

Furthermore I am grateful to have had further inspiring coauthors:

Melanie and Terry, I am thankful to have belonged to the great minimum wage project team which stemmed the big consultancy project in little time with the necessary Rhenish serenity, and I am happy that we get the chance to reap the rewards of our work bit by bit.

Sarah and Astrid, thank you for letting me participate in the research project that you had already acquired. It was a great experience to have quite some heterogenous groups of citizens of Mannheim in our neat experimental laboratory. I have always enjoyed our regular talks during the ZEW hours and afterwards.

Sebastian, thank you very much for showing me how to write a paper efficiently, how to formulate referee reports, how to implement helpful and less helpful referee comments, and most importantly, how to successfully publish a paper.

I am also grateful for the numerous great colleagues I was able to meet within my research department and beyond. Next to the chats about research, especially the daily lunch breaks as well as various activities with you inside and outside of ZEW have truly enriched my working time throughout the last years.

Next to colleagues, I also thank the ZEW as research institution for giving me the opportunity to participate at numerous (international) conferences and for providing huge server capacities which I urgently needed for the complex calculations I did with huge data sets.

Furthermore, I greatly acknowledge the financial support that has been given by the German Science Foundation (DFG) and the ZEW Sponsors' Association for Science and Practice.

Lastly, I want to thank all *Arbeiterkinder* in Mannheim for having given me a great time throughout the last year.

Above all, I am very grateful to my great and loving family. You keep on supporting me extremely well at all times. You are truly the best!

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List of Abbreviations

ASD	Aggregate state dependence
ATT	Average treatment effect on the treated
BA	German Federal Employment Agency (Bundesagentur für Arbeit)
cp.	compare
DfDC	Doctors for Developing Countries (Ärzte für die Dritte Welt)
DiD	Difference-in-differences
DiDiD	Difference-in-differences-in-differences
DZI	Central Institute for Social Issues (Deutsches Zentralinstitut für soziale Fragen)
e.g. (lat. exempli gratia)	for example
et al. (lat. et alii/aliae/alia)	and coauthors
FE	Fixed effects
GSD	Genuine state dependence
GSOEP	German Socio-Economic Panel
IAB	Institute for Employment Research (Institut für Arbeitsmarkt- und Berufsforschung)
IABS	IAB Employment Sample (IAB-Beschäftigtenstichprobe)
i.e. (lat. id est)	that is
LAK	Central pay office of the roofing sector (Landesausgleichskasse für das Dachdeckerhandwerk)
LPM	Linear Probability Model
ME	Marginal Effect
MW	Minimum wage
OECD	Organisation for Economic Co-operation and Development
OLS	Ordinary Least Squares
resp.	respectively
s.d.	standard deviation
s.e.	standard errors
SIAB	Sample of Integrated Labour Market Biographies (Stichprobe der Integrierten Arbeitsmarktbiografien)
UK	United Kingdom
UN	United Nations
US	United States of America

Introductory Words

Within the last 51 months, I have had the opportunity to work with a number of inspiring researchers. This is reflected in this cumulative dissertation, which comprises five mainly independent research projects. Apart from the single-authored research project in the first chapter, six different coauthors have each contributed to exactly one of the four joint research projects presented in Chapters 2 to 5. This heterogeneity in coauthorship is one reason why I was able to address very diverse topics in this dissertation. The common ground of all five chapters is the application of econometric methods in a microeconomic context.

Econometric methods are most commonly applied either using historical or experimental data. In order to test a certain theory, experimental data are created in a target-oriented way in a (perfectly) controlled environment, whereas historical data uses information from the past. My dissertation makes use of both data alternatives. The first part of my dissertation (Chapters 1 to 3) is based on historical data, mainly from the German Federal Employment Agency (*Bundesagentur für Arbeit*, in short BA), to investigate wage dynamics in the German labor market over the last four decades. For the second part of my dissertation, experimental data have been collected in the field (Chapter 4) as well as in the laboratory (Chapter 5) to investigate individual donation behavior.

Studying wage dynamics has been a key element of labor economics for a long time. One major finding is the widening of the wage distribution in most developed countries that started in the 1970s in several countries, see e.g. Autor et al. 2008, Acemoglu 2003, and Levy and Murnane 1992. In Germany, rising wage inequality was mainly driven by the disproportional wage increases in the upper-tail of the wage distribution in the 1970s before the lower-tail wage inequality started to increase since the 1990s as well (see e.g. Dustmann et al. 2009, Fitzenberger 1999). However, as long as individuals are able to move up the earnings distribution, a high degree of cross-sectional wage inequality is likely to exaggerate the extent of wage inequality over a working life. Thus, for any analysis of the evolution of lifetime wage inequality it is important to also take individual wage mobility over time into account. Wage mobility is defined as the change of an individual's relative position in the wage distribution between two periods. To this end, I make use of the regional file of the employment subsample of the Research Institute of the BA (*SIAB*), which contains a 2% random sample of all social security records between 1975 and 2008 that cover approximately 80% of the overall German workforce.

Chapter 1 gives a descriptive overview of the evolution of wage inequality and wage mobility, separately for men and women, in West and East Germany over the last four decades. The results show that the increase in wage inequality was accompanied by a decrease in wage mobility for both sexes in West and East Germany. Women face a higher level of wage inequality and a lower level of wage mobility than men in West and East Germany throughout the entire observation period. The mobility decline was sharper in East Germany so that the level of wage mobility has fallen below that of West Germany. The long time span of the data additionally allows for an analysis of long-term mobility, which is of particular interest as it gives insights on the chances of moving up the wage distribution over an individual's life cycle. Covering up to 24 years of a West German working life, I find that long-term wage mobility was higher for male than for female workers in all years. However, the wage mobility gender gap has been slowly closing over time as long-term wage mobility has slightly increased for women whereas it slightly decreased for men.

From a welfare perspective, a low degree of wage mobility in the low-wage sector is of particular concern as it tends to marginalize low-wage workers in the long run. Therefore, Chapter 2 takes a specific look at the evolution of wage mobility in the West German low-wage sector between 1984 and 2004.[1] Wage mobility is here defined as switching between the two labor market states "low pay" and "high pay" with the threshold being 2/3 of the median wage, which is the standard threshold in the literature (e.g. OECD 1998, Stewart and Staffield 1999, European Commission 2004). In this essay, which is coauthored by Nicole Gürtzgen, we are particularly interested in explaining the observed decline in wage mobility in the low-wage sector, which has grown from 13% in 1996 to 18% in 2008. Next to compositional shifts in the low-wage relative to the high-wage sector, the decline in wage mobility may be explained by an increase in genuine (or "true") state dependence. The latter occurs if low-wage employment today causes low-wage employment in the future for reasons of, e.g., stigmatization or human capital depreciation.

In order to isolate the evolution of genuine state dependence, we model low-pay transitions by estimating a series of multivariate probit models. We address the initial conditions problem in our estimation approach by explicitly accounting for selection into low-wage employment. Moreover, we control for the fact that the likelihood of remaining in the sample might differ between low-paid and high-paid individuals. Our findings for men and women point to an upward trend of genuine state dependence among low-paid workers, especially since the beginning of the

[1] As the *SIAB* data set has been made available only recently, the analysis in Chapter 2 uses the previous data version (*IABS*), which contains the years 1975 to 2004.

1990s. Using decomposition techniques by Oaxaca (1978) and Blinder (1978), we show that between 35 and 54% of the increase in genuine state dependence during the 1990s is accounted for by changes in the characteristics of the low-wage workforce over time.

One way to prevent the low-wage sector from increasing and to reduce the number of individuals at risk of low-pay persistence is to introduce minimum wages. While in several developed countries there exists a nation-wide minimum wage, the introduction of minimum wages is still a highly debated political issue in Germany. The main argument against it is that a minimum wage might lead to job losses following the standard theory of a competitive labor market, where prices cannot be set by employers, see, e.g., Brown (1999). Evidence from, e.g., the UK and the US shows that in many cases negative employment effects are hard to detect as long as the minimum wage is not set extraordinarily high, see Neumark and Wascher (2008) for an overview article.

Up to now, only few specific industries in Germany have a minimum wage. The minimum wage level differs across sectors and partially also within sectors. One of the first sectors in Germany to introduce a minimum wage in 1997 was the roofing sector as a consequence of the law on the posting of workers (*Arbeitnehmer-Entsendegesetz*). Together with Melanie Arntz and Terry Gregory, I have evaluated the employment effects of the minimum wage introduction and the subsequent minimum wage increases in this sector. For this purpose, we were able to exploit two administrative linked employer-employee panel data sets: i) data that are collected by the central pay office of the roofing sector (*Lohnausgleichskasse für das Dachdeckerhandwerk*) and ii) data that are collected by the BA for all employees who are subject to social insurance contributions. This enabled us to contribute to the sparse literature on employment spillovers of minimum wages on workers who earn above the minimum wage level, see Chapter 3.

The roofing sector was the first sector to introduce the same minimum wage level for East and West German workers in 2003. This gave rise to an internationally unprecedented hard bite of a minimum wage. By 2010, half of the East German roofers earned the minimum wage so that by now there is no longer a low-wage sector - as defined above - in the East German roofing sector. Using a difference-in-differences approach, we investigate the chances of remaining employed in the roofing sector for workers with and without a binding minimum wage. We focus on the plumbing sector as a suitable benchmark sector since it is not subject to a minimum wage. By estimating the counterfactual wage plumbers would receive in the roofing sector given their characteristics, we are able to identify employment effects along the entire wage distribution. The results indicate that the chances for roofers to remain employed in the sector have deteriorated in East Germany along the entire wage distribution. Such employment spillovers to workers for whom the minimum wage is not binding may result from scale effects and/or capital-labour substitution. However, given the specific conditions of the roofing sector, a transferability of the results to other sectors has to be viewed with caution.

The second part of my dissertation also makes use of microeconometric methods, but considers the field of behavioral rather than labor economics. The key objective of the emerging research in behavioral economics is to harmonize economic theories and models with human behavior observed in reality. One of the key assumptions in economics is thereby questioned by behavioral economists: that an individual behaves like homo oeconomicus and, thus, acts rationally and egoistically to maximize his or her utility, which is often assumed to be measured in monetary terms. One phenomenon that makes this view particularly questionable are donations. Although donations stand in contrast to the behavior of homo oeconomicus, we do observe them in the real world. Over the past years, (behavioral) economists have put a lot of effort into finding out why individuals donate and what their decision to donate money (or time) is influenced by, see List (2011) and Bekkers and Wiepking (2011) for literature overviews. Explanations why individuals might donate can be found in the theories of altruism (Andreoni, 1989), the feeling to do something good (Andreoni 1990), reciprocity (Sugden 1984) or prestige (Harbaugh 1998), to name a few.

Researchers have lately begun to investigate under which circumstances individuals increase their donations. One motive that attracts individuals to donate higher amounts is the motive of identification as people care more about identifiable victims than about statistical victims (Small and Loewenstein 2003). Sebastian Kube and I contribute to this literature in Chapter 4 by exploring the effect of providing donors with the opportunity to choose the target country for their donation. To this end, we have cooperated with a large German charitable organization, which sent out more than 57,000 letters to their members as part of a donation campaign. Using this setting for a large natural field experiment, we find that our treatment manipulation affects neither the average donation size nor the response rate. Only a small fraction of donors (3.5%) actually chooses their object of benevolence. However, those donors give more than those who did not specify a recipient. Based on previous donations, we can provide indicative evidence that this might be a causal rather than a mere selection effect. This work has been accepted for publication in a Special Issue on Field Experiments in the

Scandinavian Journal of Economics.

Rather than focusing on the amount that is donated to a particular charitable organization, the essay coauthored by Sarah Borgloh and Astrid Dannenberg in Chapter 5 goes one step back and analyzes the decision to donate at all. In particular, we are interested in studying the effect of information about the size of a charitable organization on individuals' donations to that organization. The prediction is not clear-cut as there are two contrary strands of theory. The approach of signaling (Vesterlund, 2003), for example, predicts that donations increase with others' contributions while they may decrease following the impact philanthropy model of Duncan (2004) as the relative impact of one's donation might be reduced with rising revenues of the charitable organization.

In order to answer our research question, we have conducted a framed field experiment in the lab with a non-student subject pool, in which subjects had the opportunity to donate to various charitable causes. The results show that if subjects are to choose between large organizations with high annual revenues and small organizations with low annual revenues, they prefer the small organizations. Thus, our results support the predictions that follow from the model of impact philanthropy. Moreover, we provide insights about which socio-demographic characteristics affect individual contributions and show that the individual willingness to donate increases with the subjects' age, income, and education.

Chapter 1

Gender Differences in German Wage Mobility*

1.1 Introduction

According to Saez (2012), the top one percent of the earnings distribution in the US earned 10% of all income in 1980, but this share increased to 23% in 2007. This rise in wage inequality has lately fostered public attention (and tension) throughout the developed world. An additional prevailing concern is that those who are rich stay rich and those who are poor stay poor, i.e. wage mobility is perceived to be low.

Although the actual public debate prefers to concentrate on this extreme case of wage inequality at the very top of the earnings distribution, several studies have documented for a number of countries that wage inequality has been rising during the last decades also at lower percentiles (e.g. Autor et al. 2008, Acemoglu 2003, Levy and Murnane 1992).[2] While the upper-tail inequality, measured as 90/50 percentile ratio, for example in the US, has been increasing steadily since the 1980s, the lower-tail inequality (50/10 percentile ratio) rose sharply during the

[1]*I thank Nicole Gürtzgen for fruitful discussions and helpful comments. Financial support from the German Science Foundation (DFG) is gratefully acknowledged (Grant-No. FR 715/9-1).

[2]In the US and in the UK, wage inequality began to rise in the 1970s whereas the continental European countries experienced the start of the increase in wage inequality about one decade later.

1980s and flattened thereafter, see e.g. Goldin and Katz (2009).

One reason for the growing upper-tail wage inequality is due to the change in relative supply of skills that was not able to keep up with the change in relative demand that occurred due to rapid skill-biased technological change (Katz and Murphy 1992, Goldin and Katz 2008, Acemoglu and Autor 2012). The erosion of labor market institutions including labor unions as well as rising international trade, immigration, and outsourcing is often viewed to have contributed to rising wage inequality in the lower tail of the wage distribution, see e.g. Burtless (1995), Acemoglu (2003) and Goldin and Katz (2009). The job polarization in the highest- and lowest-wage occupations that is modeled by Autor et al. (2003) and documented, e.g., by Goos and Manning (2007) for the UK may serve as a further explanation for the diverging trends in upper-tail and lower-tail inequality. As a consequence of the technological progress - and in particular the implementation of computer technology - machines substitute medium-paid routine tasks conducted by, e.g., craft manual workers and bookkeepers, see Spitz-Oener (2006) for evidence on Germany.[3] Furthermore, Card et al. (2012) find that rising wage inequality has been fostered by rising heterogeneity between workers, rising variability in the wage premiums at different establishments, and increasing assortativeness in the matching of workers to plants.

A full picture of the changing wage structure is, however, only adequately drawn if not only changes in wage inequality, but also changes in wage mobility are taken into account. If, for example, perfect mobility of wages were observed, low-wage earners in one period would have the same probability as high-wage earners to

[3]Antonczyk et al. (2009), however, find that the task-based approach can not explain the recent increase in wage inequality among male employees in Germany.

earn a high wage in the next period. In this case, rising wage inequality might be acceptable as a low-wage position would not be of permanent nature. In the other extreme case of perfect immobility of wages, all individuals would be bound to their wage position over time. Thus, a more unequal wage distribution would deteriorate the chances of low-wage workers to move up the wage ladder over the life cycle. In general, wage mobility can thus reduce cross-sectional wage inequality as was shown by, e.g., Gottschalk (1997) for the US and Hofer and Weber (2002) for several European countries. However, international studies suggest that for a large number of countries the rising wage inequality was accompanied by declining wage mobility, which gives rise to rising persistence in low-wage employment, see e.g. Buchinsky and Hunt (1999) for the US and Dickens (2000) for the UK.[4]

The wage structure has also been changing in Germany over time, which is particularly interesting for two reasons: first, the German wage structure has long been considered relatively stable at the lower tail of the earnings distribution (Prasad 2004). This was likely due to labor market institutions, such as unions, and was consistent with the hypothesis of skill-biased technological change especially in the upper part of the wage distribution (Fitzenberger, 1999). Since the mid 1990s, the lower-tail wage inequality has distinctly risen (Dustmann et al. 2009, Fuchs-Schündeln et al. 2010, Gernandt and Pfeiffer 2007) to which labor supply shocks, such as the slowdown of skill-upgrading, and strong deunionization are likely to have contributed (Antonczyk et al. 2010, Dustmann et al. 2009). Second, Gernandt (2009) shows in one of the very few studies on wage mobility in Germany that earnings mobility declined over the last decades using household panel data

[4]Burkhauser et al. (1997) denote that a comparable development of US and German earnings mobility is achieved during the 1980s despite major differences in labor market institutions and a greater increase of inequality in the US.

from the German Socio-Economic Panel (GSOEP). This finding is confirmed by Riphahn and Schnitzlein (2011), which is to the best of my knowledge the only study that analyzes both the evolution of wage inequality and wage mobility using German administrative data. Their evidence is based on the change in wage mobility for the overall working population in West and East Germany.

The aim of this chapter is to investigate whether certain sub-groups of the population are especially prone to being immobile in their wages in the short and in the long run. I particularly concentrate on gender differences in West and East Germany when analyzing different wage inequality and wage mobility measures. For this purpose, I use administrative data from a 2% subsample of the German Employment Statistics Register (*SIAB* data). In contrast to survey data like the German Socio-Economic Panel (GSOEP), the *SIAB* offer much more individual observations and span (for West Germany) a longer time period from 1975 to 2008. Moreover, less attrition and less measurement errors can be expected as the sample is based on reports from employers in compliance with the notifying procedure for the German social security system.

That differences in wage mobility levels may exist between men and women might stem from various developments. On the one hand, the rising share of well-educated women might have contributed to rising wage mobility across women. On the other hand, there is evidence that men profit from higher wage increases when changing jobs (Gottschalk 2001, Weber 2002). Moreover, as will be shown in Chapter 2, women are more likely to stick to a low-paid job, which is of particular concern as the share of women in the low-wage sector is much greater than the share of men. Thus, women's wage mobility is lower than men's at the low-wage threshold of 2/3 of the median wage. This result, however, gives only an imprecise picture of the development of wage mobility since it does not allow for any conclusion on the development of the rest of the wage distribution.

In international studies, the development of wage mobility by gender has been investigated along the entire wage distribution for various countries. Hofer and Weber (2002), for example, find that, except for Austria and the UK, women are more mobile in their wages than men in a number of OECD countries. The study also includes Germany, but uses the GSOEP for the analysis. The result for the UK is confirmed by Dickens (2000). Moving up the wage distribution being the main determinant of wage mobility, Kopczuk et al. (2010) find for the US that men have a much higher level of long-term upward mobility than women, but this upward mobility gender gap has been closing over time.[5]

The outline of this chapter is as follows. Section 1.2 introduces the data set I use for my analysis and gives an overview on the inequality and mobility measures. The results on the evolution of wage inequality and short-term and long-term wage mobility are displayed in Section 1.3. Section 1.4 concludes.

1.2 Data and Measures

1.2.1 The SIAB data

The data set that I use for my analysis is taken from the regional file of the IAB employment subsample 1975-2008 (*SIAB*); for detailed information see Dorner et al. (2011). This administrative data set contains a 2% random sample of all

[5]Regarding different developments of wage mobility for different individual groups, Raferzeder and Winter-Ebmer (2007), among others, suggest that young and well educated workers, workers who work for big companies and workers working in the service sector have higher probabilities of upward mobility, ceteris paribus.

social security records from 1975 to 2008 for West Germany and from 1992 to 2008 for East Germany. It includes employment records subject to social security contributions as well as unemployment records with transfer receipt. The data contain the employment history of 1.6 million individuals and provide individual information on daily wages, workers' employment histories and a number of individual characteristics, such as age, education, nationality and occupational status. Approximately 80% of the German workforce are covered, but self-employed workers, civil servants, and individuals currently doing their military service are not included in the data set.

Although the *SIAB* covers a large time span and is subject to much less panel attrition compared to household surveys like the GSOEP, the data set has some disadvantages as well. First, as I only observe whether an individual works full-time or part-time (defined as working less than 30 hours per week), the data lack explicit information on the actual number of hours worked. For this reason, I restrict the sample to full-time workers. Second, the wage information is censored since retirement insurance contributions are only paid up to a fixed social contribution taxation threshold. Appendix 1C gives an exercise on why it might be advisable to use only the uncensored observations for my analysis rather than imputing wages for censored observations as suggested by Gartner (2005). Third, the wage information contains one-time payments, such as bonus payments, only since 1984. As Steiner and Wagner (1998) point out, ignoring this structural break between 1983 and 1984 leads to an increase in wage inequality. Hence, the method based on Fitzenberger (1999) is used to correct for this. For the analysis, I further restrict the sample to individuals aged between 20 and 55 years. Moreover, only those employment spells are used which overlap June 30th, thereby ensuring that each individual is not observed more than once per year.[6]

1.2.2 Wage inequality measures

To shed light on the development of the earnings inequality in Germany over time, different measures of wage inequality can be used. Given that the wage information is censored in the *SIAB*, the comparison of particular wage percentiles over time helps to overcome this limitation. Following Dustmann et al. (2009), I will use the 15th and 50th (50th and 85th) percentile for highlighting the development at the lower (upper) part of the earnings distribution. Moreover, the evolution of the spread between the 85th and the 15th percentile of the annual wage distribution is a first crude measure of overall wage inequality over time.

In order to use more information of the wage distribution, I also apply wage inequality measures that take into account the entire wage distribution (up to the censoring limit). The widely used Gini coefficient, for example, which fulfills desirable properties like mean independence, population size independence, and symmetry displays the level of wage inequality on a scale from 0 (no inequality meaning everyone earns the same income) to 1 (perfect inequality meaning the richest person earns all the income). More formally, it measures the average difference between all possible pairs of incomes in the population, expressed as a

[6]If a worker worked for more than one employer in a year, a weighted average is computed where the weights represent the shares worked for each employer. To improve the education variable, I use the imputation rules derived by Fitzenberger et al. (2006). Due to the introduction of the Euro in 1999, all wages before 1999 are transformed from Deutschmark into Euros at a rate of 1 €= 1.95583 Deutschmark. Since the wage variable delivers unrealistic daily wages at the lower end of the wage distribution, all observations with earnings of less than 16 € per day (in prices of 1995) are excluded.

proportion of total income (see also Cowell 2011):

$$Gini = \frac{1}{2N^2\bar{y}} \sum_{i=1}^{N} \sum_{j=1}^{N} \mid y_i - y_j \mid, \tag{1.1}$$

where y_i (y_j) is the income of individual i (j), $\bar{y}$ is the average income of the population and N is the number of individuals in the population.

A third class of inequality measures stems from the family of generalized entropy inequality measures. In contrast to the Gini coefficient, which is particularly sensitive to the middle of the distribution, the Theil index is particularly sensitive to the top of the earnings distribution and is defined as follows:

$$Theil = \frac{1}{N} \sum_{i=1}^{N} \frac{y_i}{\bar{y}} log \left(\frac{y_i}{\bar{y}} \right). \tag{1.2}$$

The measure of mean log deviation (MLD), also known as Theil's L measure, is particularly sensitive to the lower part of the earnings distribution:

$$MLD = \frac{1}{N} \sum_{i=1}^{N} log \left(\frac{\bar{y}}{y_i} \right) \tag{1.3}$$

As before, y_i denotes the income of individual i, $\bar{y}$ is the average income, and N is the number of individuals. In contrast to the Gini coefficient, the Theil index as well as the MLD measure allow for additive decomposition between different groups. This is particular interesting in my case as I investigate wage inequality patterns across different subgroups of the population. If Y is the total income of the population, Y_j the income of the subgroup, N the total population and N_j the population of the subgroup, the decomposition of the Theil index is as follows:

$$
\begin{aligned}
Theil &= \sum_{i=1}^{N} \frac{y_i}{N\bar{y}} log\left(\frac{y_i N}{\bar{y} N}\right) = \sum_{i=1}^{N} \frac{y_i}{Y} log\left(\frac{y_i N}{Y}\right) \\
&= \sum_{j} \frac{Y_j}{Y} Theil_j + \sum_{j} \frac{Y_j}{Y} log\left(\frac{Y_j/Y}{N_j/N}\right) \qquad (1.4)
\end{aligned}
$$

The first term in equation (1.4) displays the within-group inequality while the second term represents the between-group inequality. Similarly, the decomposition for the MLD measure is given by:

$$
MLD = \sum_{i=1}^{N} \frac{1}{N} log\left(\frac{Y}{Y_i N}\right) = \sum_{j} \frac{N_j}{N} MLD_j + \sum_{j} \frac{N_j}{N} log\left(\frac{N_j/N}{Y_j/Y}\right). \qquad (1.5)
$$

Hence, the use of these measures allows to draw conclusions on inter- as well as intragroup inequality of the overall wage inequality observed.

1.2.3 Wage mobility measures

The wage inequality measures give insights into how the shape of the wage distribution has changed over time. However, movements within the wage distribution cannot be detected by these measures. A series of papers demand to consider wage mobility when dealing with changes in the wage distribution (e.g. Buchinsky and Hunt (1999), Dickens (2000), Cardoso (2006)).

A widely used mobility measure is the rank correlation coefficient, which measures the degree of similarity of, e.g., individual wages between two periods, thus enabling mobility analyses in the short and in the long run. The higher the rank correlation is, the lower will be wage mobility between the two periods. In order to specify whether different parts of the wage distribution differ with respect to the level of wage mobility, I furthermore look at quintile transition matrices. This allows to investigate whether and how many quintiles individuals have moved between two periods. Since such transition matrix approaches fail to capture the movement within each quintile so that mobility is likely to be underestimated, I additionally make use of the measure by Dickens (2000). This mobility measure is based on the degree of change in ranking from one year to the next and is derived in the following way:

$$M_D = \frac{2\sum_{i=1}^{N} \mid F(w_{i,t+1}) - F(w_{i,t}) \mid}{N}, \tag{1.6}$$

where $F(w_{i,t})$ and $F(w_{i,t+1})$ are the cumulative distribution functions for earnings in year t and $t+1$, respectively, and N is the number of individuals. As the measure is twice the average absolute change in percentile ranking between year t and year $t+1$, it takes the minimum value 0 when there is no mobility, i.e. when each individual remains in the same percentile. Assuming independence of earnings in the two years would result in a value of 2/3 while the maximum value of 1 would correspond to the situation where the earnings in the two years are perfectly negatively correlated.

Moreover, in order to formalize the relationship between wage mobility and wage inequality, i.e. to measure the extent to which wage mobility reduces short-run wage inequality in the longer run, the Shorrocks index is applied (Shorrocks, 1978). If we consider a population of $i = 1, ..., N$ individuals observed in t consecutive periods and $y_{i,t}$ is the (short-term) earning of individual i in period t, then $\bar{y}_i = \sum_t y_{i,t}$ denotes the average (long-term) earnings of an individual i across T

periods. If $Z = (y_1, ..., y_N)$ is a vector of individual earnings and an inequality measure $G()$ is defined that is a convex function of earnings relative to the mean, Shorrocks (1978) shows that it must hold that

$$G\left(\bar{Z}\right) \leq \sum_{t=1}^{T} G\left(Z_t\right)/T, \tag{1.7}$$

where Z_t is a vector of earnings in period t and $\bar{Z}$ is a vector of average individual earnings across T periods. The Shorrocks index M_S is then defined as

$$M_S = 1 - \frac{G\left(\bar{Z}\right)}{\sum_{t=1}^{T} G\left(Z_t\right)/T}. \tag{1.8}$$

The index thus compares the average of t period-specific inequality measures with inequality averaged over t periods. If the latter is smaller than the former, intertemporal mobility reduces short-run inequality. The smaller the ratio in equation (1.8) is, the greater will be the mobility, and the closer to 1 will be the Shorrocks Index. If both components are of the same size, no mobility is observed and the Shorrocks Index takes the value 0. As inequality measure G(), I will present the results using the Gini index to estimate the Shorrocks index and using a horizon of $T = 5$ years.

Furthermore, the long time dimension of the data set allows an analysis of long-term mobility in West Germany. This is of particular interest as it gives insights on the chances of moving up the wage ladder over an individual's life cycle. Therefore, I calculate the rank correlation of individual average earnings of a five-year period centered around period t and a five-year period centered around $t + k$, where $k = 10, 15, 20$ years, thus covering up to 24 years of an individual working life.

1.3 Results

1.3.1 Development of wage inequality

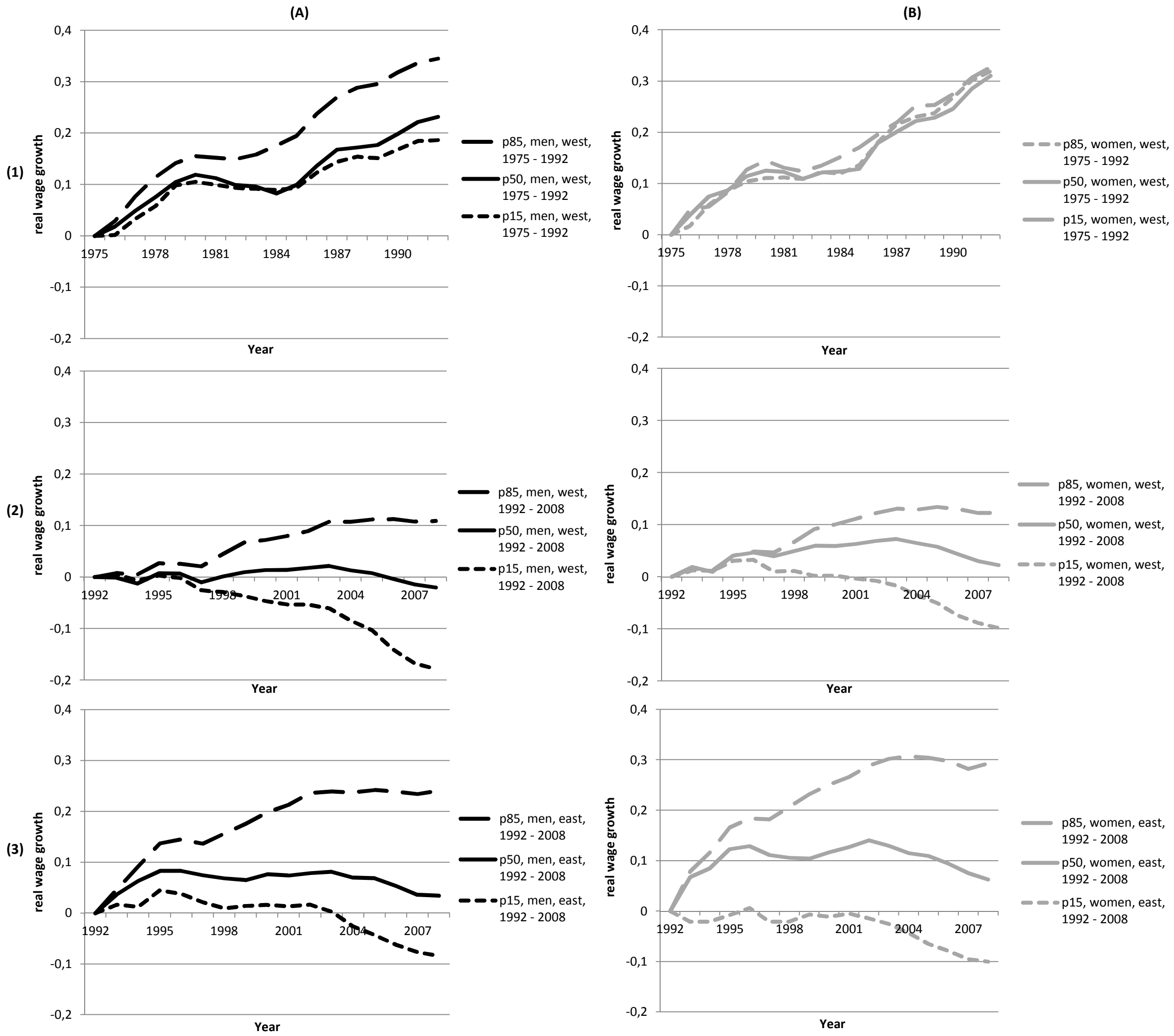
Before studying the developments of wage mobility, it is worth to describe the overall development of wage inequality over time. Figure 1.1 plots the evolution of real wage growth at three different percentiles of the wage distribution (15th, 50th and 85th percentile) separately for men (Panel A) and women (Panel B).[7] The upper graphs (Panel 1) display the real wage growth for West Germany from 1975 to 1992 with 1975 as the base year. The middle and lower graphs show the real wage growth for West (Panel 2) and East Germany (Panel 3) from 1992 to 2008 with 1992 being the base year.

As can be seen from Panel 1A, male individuals at the 85th percentile experienced a distinct rise in real wages between 1975 and 1992 compared to individuals at the 15th and 50th percentile, whose real wages developed similarly over the observation period. For female workers, the real wages for all three wage groups increased similarly as for the men's 85th percentile (see Panel 1B). Since the beginning of the 1990s, the pattern of real wage growth has evolved quite differently for the lower, middle and upper part of the wage distribution for both sexes. While the growth of real wages continued for men and women at the 85th percentile before it attenuated at the beginning of the 2000s, the median male and female worker had about zero real wage growth between 1993 and 2008 in West Germany (Panel 2). During the same period, especially the male individuals at the 15th percentile experienced dramatic real wage losses that amounted to almost 20%

[7] The wages have been deflated by the German Consumer Price Index from the Federal Statistical Office.

while female's wages decreased by around 10% between 1992 and 2008.

Figure 1.1: Real wage growth by gender and region, 1975 = 0, 1975 - 1992 and 1992 - 2008



A similar decrease in real wages of around 10% was observed for both men and women at the 15th percentile in East Germany (Panel 3). While the median workers' real wage growth stagnated for both men and women similarly as in West Germany, the wage growth of individuals at the 85th percentile in the East was much less pronounced between 1992 and 2008 than for their West German counterparts. All in all, Figure 1.1 confirms the findings of Dustmann et al. (2009) and Riphahn and Schnitzlein (2011).

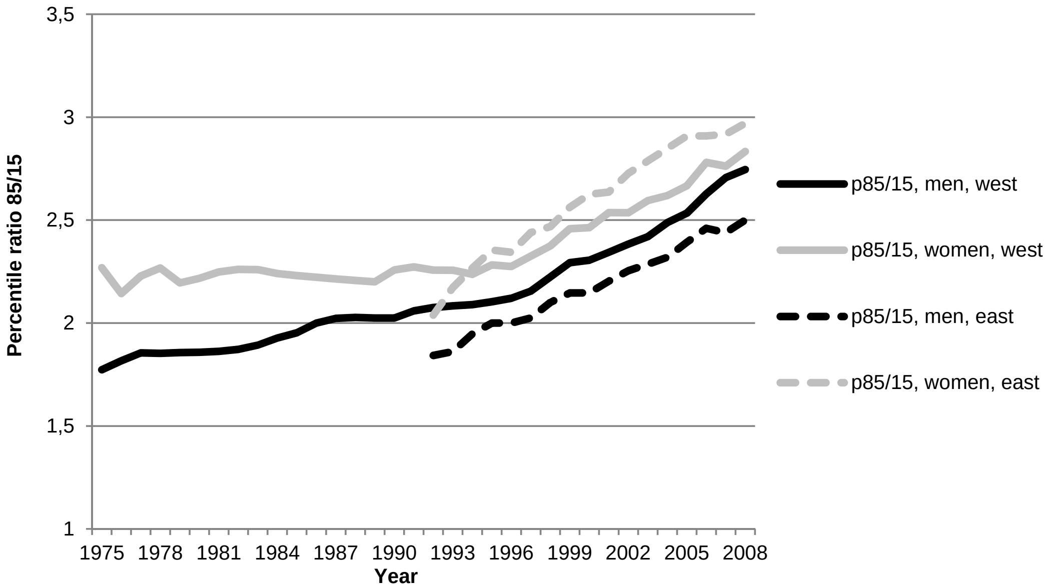
While Figure 1.1 depicts relative values of the development of wages, Figure 1.B1 displays the development of real gross daily wages by gender and region over time.[8] The absolute difference in daily wages between West German male and female workers amounted to around 20€ per calendar day for individuals at the 15th and 50th percentile in 2008. At the 85th percentile, male workers earned 160€ per calendar day, which is about 40€ more than their female counterparts. In East Germany, the absolute difference in the average real gross daily wage between men and women is much smaller. It is highest for individuals in the lower part of the wage distribution, where male workers at the 15th percentile earned about 40€ per calendar day in 2008, around 5€ more than their female counterparts.

As the different developments of wages at different percentiles in Figure 1.1 already suggest, wage inequality has been rising over the last decades. This is illustrated by the ratio of the 85th and 15th percentile in Figure 1.2, shown separately for East and West Germany and men and women. While West German men at the 85th percentile earned about 1.8 times the amount of male workers at the 15th percentile in 1975, the 85/15 percentile ratio increased slightly year by year until the mid 1990s to a ratio of 2.1 before it markedly grew to 2.7 in 2008. Due to the fact that wages are more evenly distributed along the wage distribution for men than for women, women's wage inequality has always been higher than men's throughout the entire observation period. For women, the 85/15 percentile ratio was 2.2 in 1975 and remained constant until the mid 1990s before it rose to

[8] Whenever a letter (*A* for tables, *B* for figures) is listed within a figure or table name, the corresponding figure or table can be found in the Appendix section at the end of the corresponding chapter.

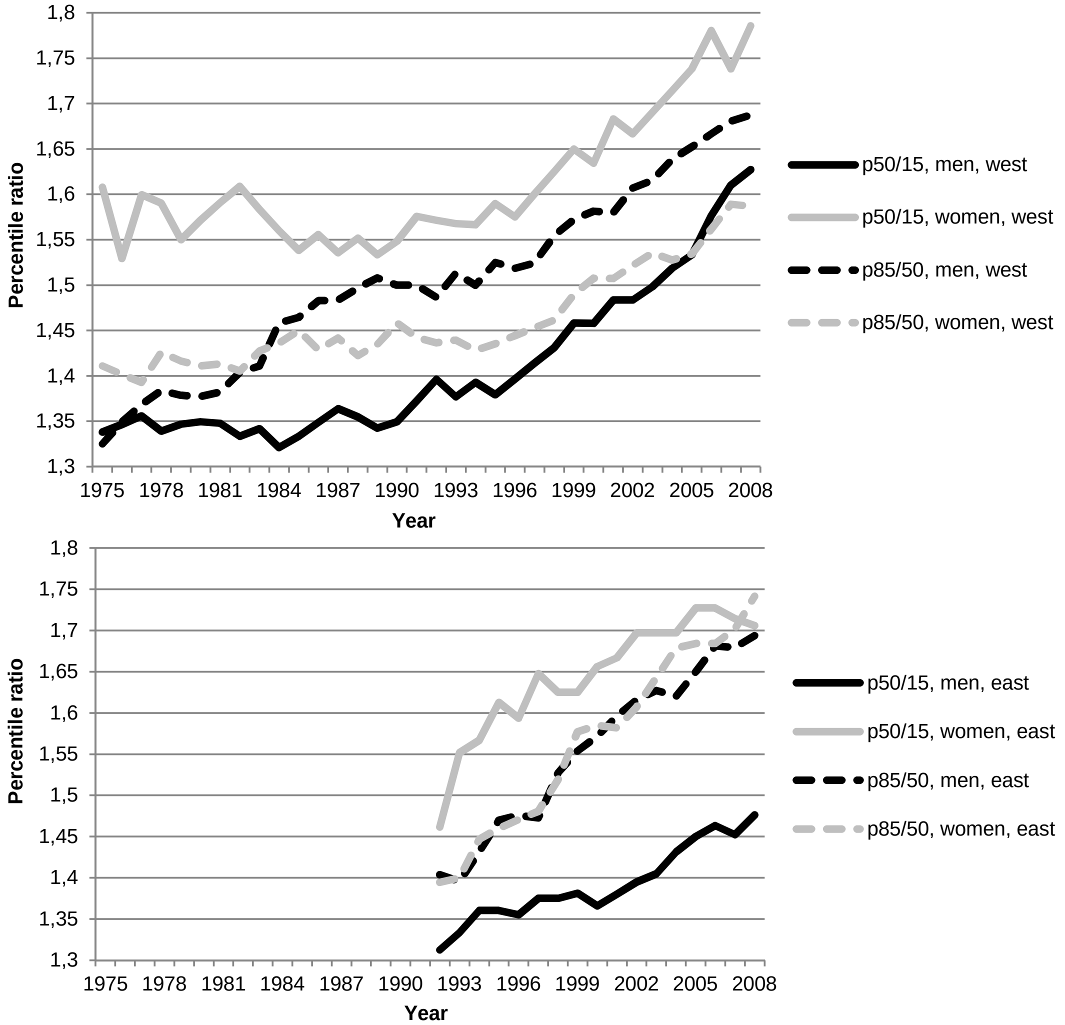
2.8 in 2008. Hence, by now men's wage inequality has almost converged to the level of women's inequality in West Germany. A similar pattern holds for East Germany, where wages are also more unequal for women than for men, a finding consistent with Franz and Steiner (2000). For both sexes, there has been a distinct increase in wage inequality since the start of the observation period in 1992. For men (women), the 85/15 percentile ratio increased from about 1.8 (2.0) in 1992 to 2.5 (3.0) in 2008. Hence, in contrast to West Germany, the difference of the 85/15 percentile ratio between men and women has become slightly larger over time.

Figure 1.2: Evolution of the 85/15 percentile ratio, 1975 - 2008, by gender and region



In order to shed light on the development of lower and upper-tail wage inequality, Figure 1.3 illustrates the evolution of the 85/50 and 50/15 percentile ratio by gender and region. Figure 1.3 reveals a pattern which is consistent for both regions and is in line with the results of, e.g., Kohn and Antonczyk (2011): while the upper-tail wage inequality surmounts the lower-tail inequality for men, the opposite is true for women. Thus, the fact that women's wage inequality is greater than that for men, as seen in Figure 1.2, results from the high level of women's lower-tail wage inequality.

Figure 1.3: Evolution of upper- and lower-tail inequality by gender, 1975 - 2008, West Germany (top) and East Germany (bottom)



This observation is especially true for East Germany, where the level of upper-tail wage inequality has evolved very similarly for men and women over time, see the bottom part of Figure 1.3. The upper part of Figure 1.3 further demonstrates for West Germany that the slight increase in the male's 85/15 percentile ratio until the mid 1990s, as observed in Figure 1.2, is driven by upper-tail wage inequality as the 50/15 percentile ratio started to increase only at the beginning of the 1990s. This is in line with the findings of, e.g., Fitzenberger (1999) and Dustmann et al. (2009).

As the percentile ratios do not account for the full wage information of the wage distribution, it can only be seen as a first crude wage inequality measure. One measure which takes the whole wage distribution into account is the Gini coefficient. However, the results would be biased if one ignored the fact that the data set is censored at the social contribution threshold. Therefore, one can either decide to rely only on the uncensored observations and to disregard the wage information at the highest percentiles of the wage distribution. An alternative is to use the method by Gartner (2005) and to impute wages of individuals who earn at the social contribution threshold or above, which is the case for up to 14% (6%) of all male observations in West (East) Germany in each year, see Table 1.A1. Even when using the imputed wages, one still needs to be cautious with the interpretation of the Gini coefficient as the share of imputed observations is not constant over time. Appendix 1C gives an empirical exercise on the advantages and disadvantages of using either alternative. It turns out that the the development of wage inequality is rather insensitive with respect to the use of imputed wages for censored observations. As the analysis of wage mobility seems to be more sensitive with respect to using imputed wages - see Appendix 1C for more details - I rely only on observations with uncensored wages throughout my analysis. This has to be kept in mind when analyzing the results as, e.g., the wage inequality results will be rather underestimated.

Figure 1.4: Evolution of the Gini coefficient by gender and region, 1975 - 2008

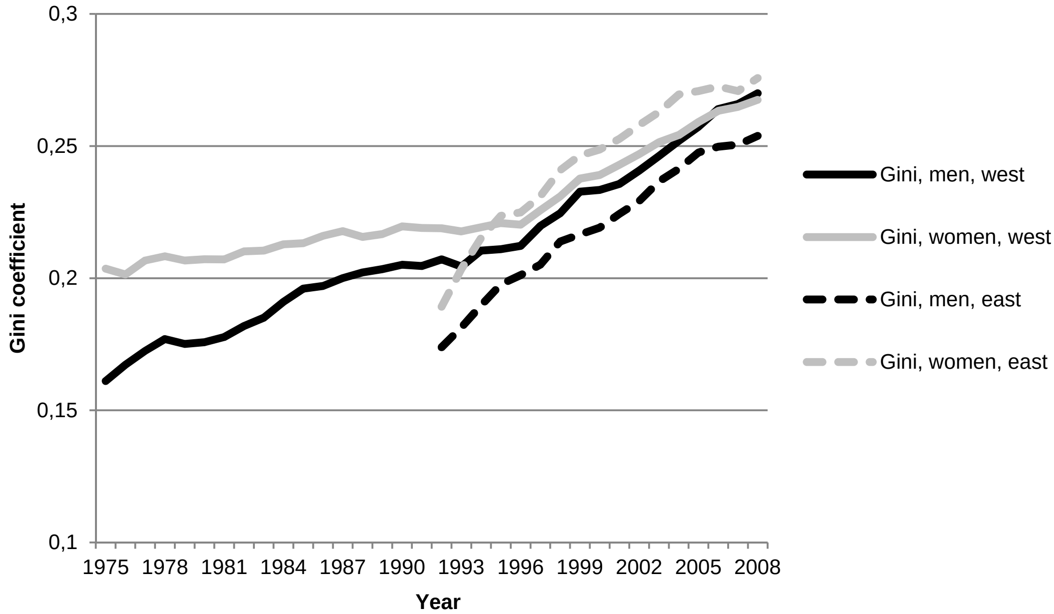


Figure 1.4 plots the development of the Gini coefficient by gender and region over time. The development of wage inequality using the Gini coefficient is comparable to the development of the 85/15 percentile ratio. The Gini coefficient, which was 0.20 for women and 0.13 for men in 1975, increased only slightly until the mid 1990s for both sexes before a distinct rise was observed for both men and women. In 2008, the Gini coefficient was 0.25 for women and 0.21 for men indicating that men's wage inequality has approached the level of inequality of women over time.[9] The development in wage inequality can be confirmed when the mean log deviation

[9] The level of the Gini coefficient is comparable to Riphahn and Schnitzlein (2011) who use the same data set, but do not look at differences across gender. When the entire wage distribution is used, the wage inequality values are larger. According to the OECD (2010), the Gini coefficient increased by 0.04 percentage points for Germany since the mid 1980s to 0.28 in 2007. In international comparison, the Gini coefficient for Germany is rather low as the Gini coefficient across more than 100 countries ranges between 0.26 in Denmark being the most equal country in terms of wages and 0.71 in Namibia (United Nations, 2004).

or other generalized entropy measures like the Theil index are used for measuring wage inequality, see Figure 1.B2, which examplarily shows the development of wage inequality using the mean log deviation as wage inequality measure.[10] This emphasizes that the observed wage inequality is not particularly sensitive to either the lower, middle, or upper tail of the wage distribution.

One advantage of using, e.g., the mean log deviation rather than the Gini coefficient as wage inequality measure is that inequality can be decomposed into a within and a between-inequality component. Within-inequality reflects in my case the inequality that exists within the same subgroup of men and women, respectively. Between-inequality catches all inequality that arises from differences across subgroups, i.e. men and women. Thus, if all men earned the same wage and women earned a different wage that were the same for all women, then wage inequality would be covered only by between-inequality. In the other extreme, had men and women the same distribution of earnings, all inequality would be due to within-inequality. As Shorrocks and Wan (2005) point out, the between-group component is usually small relative to the within-group component, especially when using earnings data. This is in fact what I find. While the within-group inequality amounted to 79% of the total wage inequality in 1975, this share gradually increased to explaining around 93% of the total inequality in 2008. The share in East Germany has been even higher throughout the observation period suggesting that the major part of wage inequality is driven by wage differences within one subgroup rather than by differences across subgroups.[11]

[10]Further figures illustrating the evolution of wage inequality using other generalized entropy measures are provided by the author upon request.

[11]Using the GSOEP, Becker and Hauser (1994) find a share of within-inequality that is very close to my numbers when using the social status as subgroups for the decomposition of the German wage inequality between 1983 and 1990.

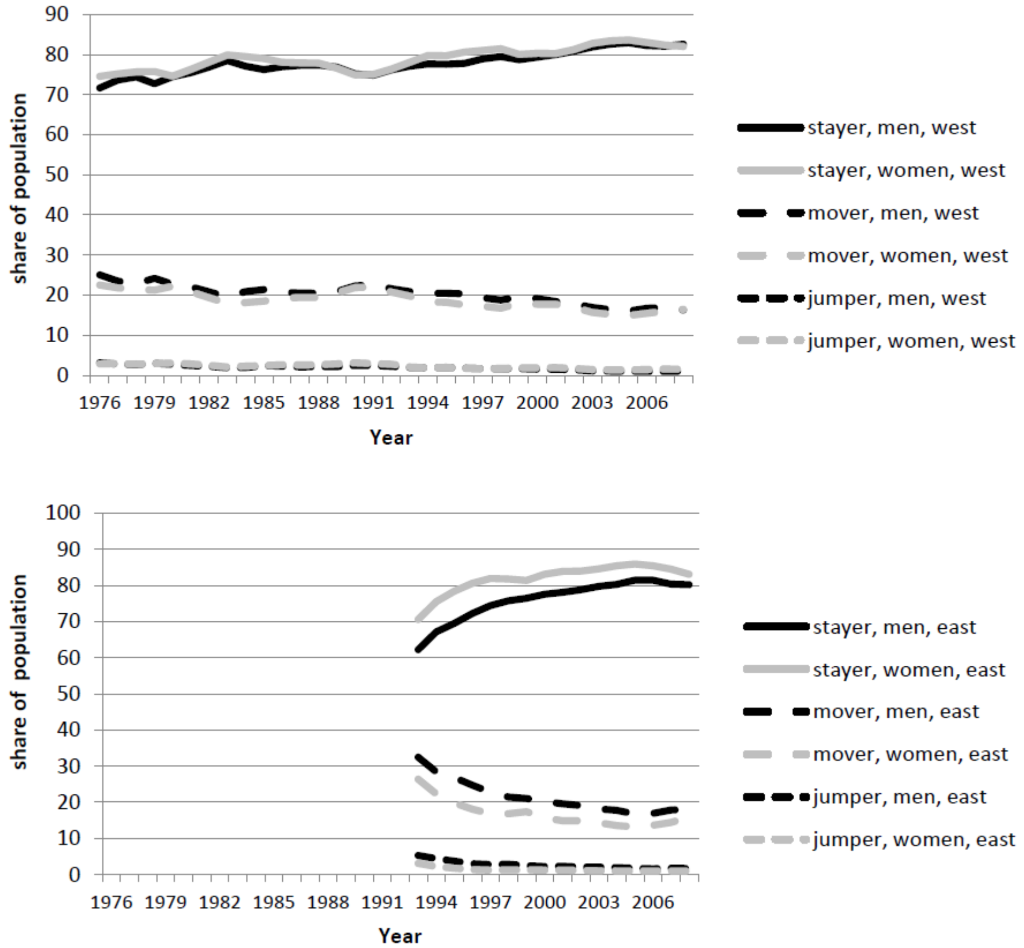
To sum up, I have documented a clear upward shift in wage inequality in West and East Germany for both men and women, especially since the mid 1990s. Women have always faced a higher wage inequality than men. While the difference in wage inequality between men and women has been slightly decreasing in the West, it has been slightly increasing in East Germany. In both regions, the upper-tail wage inequality has surmounted the lower-tail inequality for men whereas the opposite was true for women. As set out earlier, it is not only important to look at changes in wage inequality over time, but also to take into account how likely it is for workers to move up the wage ladder. A high degree of cross-sectional wage inequality is likely to exaggerate the extent of inequality over a working life as long as individuals are able to move up the earnings distribution. In the next section, I will therefore take a closer look at the development of short-term and long-term wage mobility in West and East Germany.

1.3.2 Development of wage mobility

1.3.2.1 Short-term mobility patterns

In order to measure wage mobility, the wage distribution is typically split up into different parts of the same size (e.g. percentiles, deciles, or in my case quintiles). In a next step, a matrix is built which reports the movement of individuals across quintiles from one period to another (e.g. $t-1$ and t). Figure 1.5 plots the evolution of one-year quintile transitions by gender for West (top) and East Germany (bottom). While the "stayers" represent those individuals who have stayed in the same quintile, a "mover" is a person who has moved to a neighbor quintile between $t-1$ and t. "Jumpers" are those individuals who have moved in the wage distribution by more than one quintile within one year.

Figure 1.5: Evolution of one-year quintile transitions by gender and region, 1976 - 2008

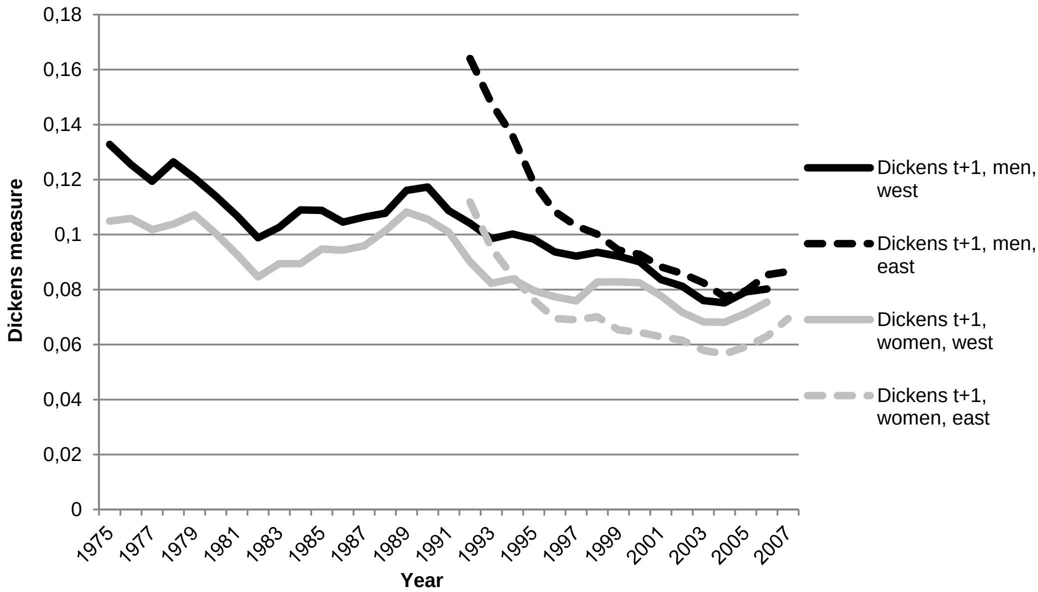


The upper part of Figure 1.5 illustrates that the level of quintile transitions was very similar for West German men and women throughout the entire observation period. While the share of those individuals who stayed in the same quintile increased steadily from around 72% in 1976 to 82% in 2008, the share of individuals who moved by one quintile decreased from 25 to 18% at the same time. The share of individuals who moved by more than one quintile within one year was always below 5% and become even smaller over time.

A similar trend towards less movement within the wage distribution can also be observed in East Germany, where the level of mobility was higher for men than for women throughout the observation period, see the lower part of Figure 1.5. The decreasing trend in wage mobility was especially pronounced during the 1990s when the share of individuals that remained in the same wage quintile one year later increased from 62% (70%) in 1992 to 78% (83%) in 2000 for men (women). In order to shed more light on which part of the wage distribution was particularly prone to wage immobility, Table 1.A2 displays the percentage of quintile stayers for each quintile separately for male and female workers for the years 1993 and 2008. The table shows the typical result, as documented, e.g., by Dickens (2000) or Cardoso (2006), that individuals at the top and at the bottom of the wage distribution face less wage mobility than those individuals in the middle of the wage distribution. Moreover, it can be observed that these middle quintiles experienced a more pronounced decrease in wage mobility over time compared to the individuals at the top or the bottom of the wage distribution, especially in East Germany. While the share of stayers in the third quintile increased from 50% in 1993 to 75% in 2008 in East Germany, the share of stayers in the first quintile increased from 70% in 1993 to 80% in 2008.

When looking at wage quintiles, wage mobility can only be observed across but not within quintiles. Therefore, I also make use of the Dickens measure which averages the absolute change in percentile ranking between two periods (t and $t+1$). The higher these absolute changes are, the higher will be the extent of the Dickens measure and, thus, wage mobility. Figure 1.6 illustrates the development of wage mobility by gender and region using this measure. The figure reveals that men have been more mobile in their wages than women throughout the entire observation period in both West and East Germany. One reason for this finding might be that men profit from higher wage increases when changing jobs (Gottschalk 2001, Weber 2002). Over time, wage mobility has been decreasing in both regions and for both sexes. The slight decrease in wage mobility in West Germany during the 1970s was followed by an increase during the 1980s for both men and women. Since 1990, wage mobility steadily declined for both men and women until the wage mobility pattern became stationary in 2004.

Figure 1.6: Evolution of the Dickens measure of mobility, by gender and region, 1975 - 2007

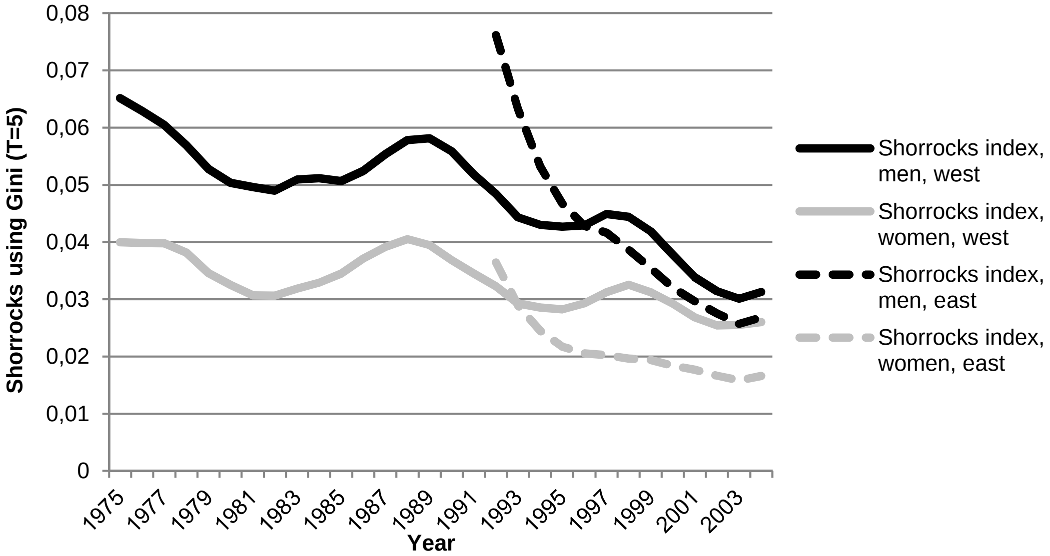


In East Germany, there was a dramatic decline in wage mobility just after reunification, which is likely to be a consequence of the slowing down of the assimilation of East wages to the wage level of West Germany in the mid 1990s (Steiner and Wagner 1997). While wage mobility was higher than in West Germany in 1992, the Dickens measure had decreased by 50% twelve years later in 2004. Riphahn and Schnitzlein (2011) show that next to structural shifts and unexplained factors a substantial part of around 40% of the mobility decline in East Germany is associated with changes in observable worker characteristics, particularly those describing job stability and employment characteristics. As a consequence, the level of wage mobility has converged to (and for women fallen below) the level of West Germany. Also with respect to gender, a convergence in wage mobility can be observed. While the absolute difference in the Dickens measure between men and women differed by more than 0.02 index points in 1975 in West Germany and by more than 0.04 index points in 1992 in East Germany, wage mobility was about the same in West Germany by 2007, whereas in East Germany men are still slightly more mobile than women.

What we have seen so far is that rising wage inequality was accompanied by decreasing wage mobility in West and East Germany for both men and women. This is a phenomenon also observed in other developed countries, see e.g. Dickens (2000) for the UK and Buchinsky and Hunt (1999) for the US. In order to verify to what extent rising wage inequality is reduced by existing (although decreasing) wage mobility, the Shorrocks index is calculated in a next step. As shown in equation (1.8), the index compares a longer-term wage inequality with the weighted sum of single-year wage inequalities. The higher the index is, the higher is the degree to which wage mobility reduces wage inequality in the short run. Figure 1.7 shows the Shorrocks index separately for gender and region using the Gini coefficient as wage inequality measure for a time horizon of $T = 5$ years.[12]

[12] A comparable pattern is observed when using the mean log deviation or the Theil index as wage inequality measure. The corresponding figures are provided by the author upon request.

Figure 1.7: Evolution of the Shorrocks index, by gender and region, 1975 - 2004



Throughout the entire observation period, the Shorrocks index was higher for men than for women. This is true for West and East Germany. However, the gender difference with respect to the Shorrocks index became smaller over time and had almost disappeared by 2004 for both sexes and in both parts of the country. While in East Germany the decrease in the Shorrocks index evolved more rapidly than in West Germany and monotonously over time, the evolution of the Shorrocks index in West Germany was characterized by ups and downs.[13] The index fell between 1975 to 1981, between 1988 to 1994 and between 1998 to 2003. In between, the Shorrocks index experienced increases which, however, were less pronounced than the decreases in the preceding periods implying an overall downward trend of the Shorrocks index over time. All in all, the evolution of the Shorrocks index reflects the earlier reported observed changes in wage mobility and inequality patterns: as wage inequality increases were accompanied by wage

[13]A similar pattern is observed in Riphahn and Schnitzlein (2011), who, however, do not look at differences across gender.

mobility decreases, wage mobility has been reducing wage inequality less and less over time.

One explanation for the wavelike pattern in the West German Shorrocks index (and also in the Dickens measure) may be changing business cycle effects over time. In times of economic prosperity, wages grow more rapidly than during recessions, see, e.g., Devereux and Hart (2006), Shin and Shin (2008). This wage procyclicality is stronger for low-wage (and highest-wage earners) compared to median earners as low-wage workers may credibly threaten to quit to unemployment when productivity increases (Robin 2011). Therefore, wage mobility might be positively correlated with the well-being of the economy. In other words, wage mobility might reduce the short-run wage inequality to a higher degree when the unemployment rate, which may serve as a proxy for business cycle effects, is low. Although the underlying correlation coefficients do not provide causal evidence, the negative relationship between the unemployment rate and the Shorrocks index is highly significant for men ($\rho = -0.809, p = 0.000$) as well as for women ($\rho = -0.464, p = 0.007$).[14]

1.3.2.2 Long-term mobility patterns

The long time span of the data additionally allows for an analysis of long-term mobility, which to the best of my knowledge has not been done so far for Germany. Such an analysis is of particular interest as it gives insights on the chances of

[14] If the GDP is used as proxy for business cycle effects, the positive correlation between the GDP and the Shorrocks index is highly significant for women ($\rho = 0.514, p = 0.002$) and significant for men ($\rho = 0.339, p = 0.054$). Applying the Dickens measure, as illustrated in Figure 1.6, rather than the Shorrocks index yields similar results: the positive relationship with respect to the GDP is significant for both men ($\rho = 0.419, p = 0.021$) and women ($\rho = 0.522, p = 0.003$), whereas the negative correlation coefficient regarding the unemployment rate is only significant for men ($\rho = -0.677, p = 0.000$, women: $\rho = -0.159, p = 0.400$).

moving up the wage ladder over an individual's life cycle. Although the time length of the data is too short to describe mobility across a whole working life, as was done by Kopczuk et al. (2010) for the US with data that goes back until 1937, it is sufficiently long in West Germany to overcome concerns of transitory changes in earnings impacting wage mobility in the short run. For this, the rank of individual average earnings of a five-year period centered around period t is compared to the rank of individual average earnings of a five-year period centered around period $t + k$, where $k = 10, 15, 20$ years.[15] This allows for covering a time period of up to 24 years and, thus, more than half of a full working life. Periods with zero earnings are included in the analysis as long as the average earnings in a five-year time span lie above the minimum threshold of 4,800 € (in 2008 prices), which is the threshold for being a marginal worker in Germany. To avoid many five-year periods with zero earnings from individuals entering or exiting the labor market early, only those individuals are included in the analysis who are aged between 22 and 38 years in year t.[16]

Figure 1.8 displays for West German men and women the rank correlation in year t between five-year average earnings centered around t and five-year average earnings centered around year $t + k$, where $k = 10, 15, 20$. Not surprisingly, the degree of mobility is the lower, the longer the considered time horizon.[17] As the rank correlation of individual earnings in year t and $t + k$ is smaller for men

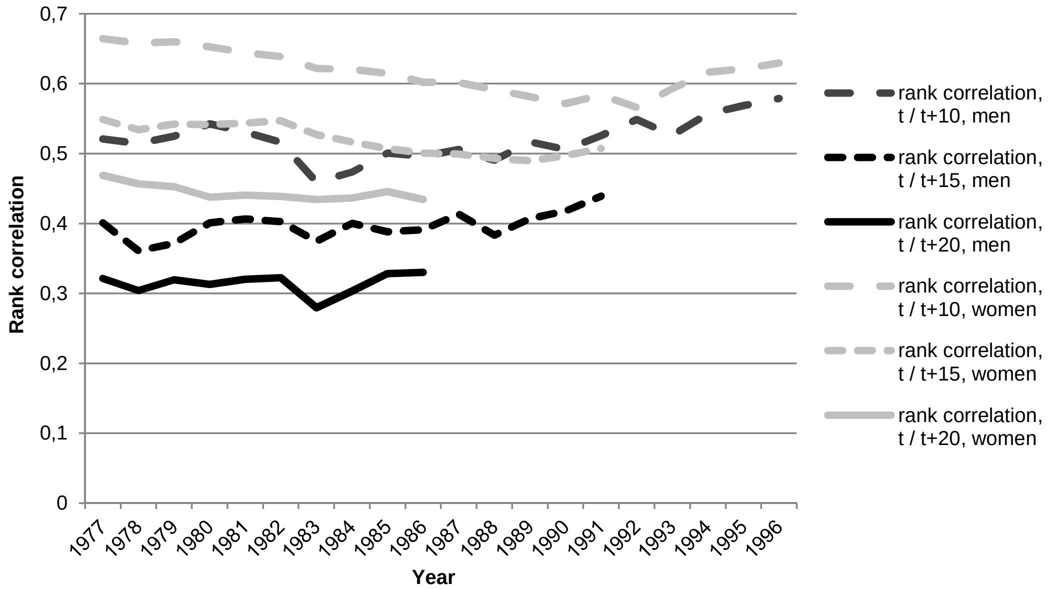
[15] Yearly average earnings are indexed to 2008 prices.

[16] If only those individuals are considered who worked in all periods, i.e., for example, in periods t-2 to t+2 and t-18 to t+22, the evolution of long-term mobility evolves similarly, albeit at a higher level of mobility. Thus, zero earnings do not seem to have a large impact on the trend of long-term mobility.

[17] The long-term mobility level is slightly lower than that observed in Kopczuk et al. (2010) for the US. However, they use an eleven-year rather than a five-year time span, which might be expected to lead to an overall higher rank correlation.

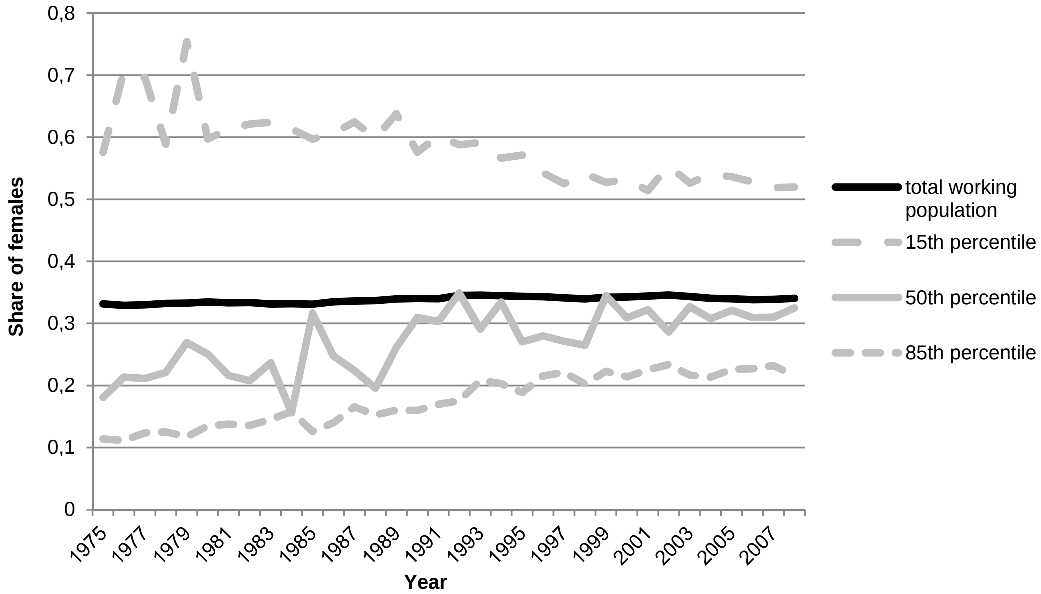
than for women, long-term mobility was higher for male than for female workers in all years. The long-term mobility gender gap has, however, been closing over time. While the men's rank correlation slightly increased over time for all three time horizons, the women's rank correlation experienced a slight decline. Thus, the long-term mobility of women slightly increased over time, whereas it slightly decreased for men, a result that is line with the evidence for the US (Kopczuk et al. 2010). This finding is consistent with the observation that the gender wage gap has been decreasing in Germany in the last decades (Fitzenberger and Wunderlich 2002, Black and Spitz-Oener 2010).

Figure 1.8: Rank correlation displaying long-term mobility for three different time horizons, by gender, 1977 - 1996



One reason for this closing long-term mobility gap might be the increase in the share of women at higher percentiles of the wage distribution. Figure 1.9 displays the share of females among those individuals earning at the 15th, the 50th, the 85th percentile and among the overall working population in the sample. The figure illustrates that the share of females among the overall working population remained constant at around 33% over time. However, although men are still more likely to be in the upper part of the wage distribution than women, the median wage earners and those individuals earning at the 85th percentile were much more likely to be women in 2008 compared to 1975. The opposite is true for workers at the 15th percentile. Hence, this development suggests that the chances to move up in the earnings distribution have been relatively improving for women compared to men in the last decades.

Figure 1.9: Share of females among the total working population, the 15th, the 50th, and the 85th percentile, 1975 - 2008



1.4 Conclusion

In this chapter, I have analyzed the evolution of wage inequality and wage mobility separately for men and women in West and East Germany over the last four decades. Using a large German administrative data set which covers the years

1975 to 2008, I find that until the 1990s rising wage inequality was mainly observed in the upper tail of the men's wage distribution. Since the mid 1990s, rising wage inequality not only extended to the lower part of the men's wage distribution, but also started to occur for women in the upper and lower part of the wage distribution. In East Germany, lower and upper-tail wage inequality rose since the start of the observation period in 1992 for both men and women. Overall, women faced a higher level of wage inequality than men in West and East Germany. While the wage inequality gender gap has been slightly decreasing in West, it has been slightly increasing in East Germany.

A high degree of cross-sectional wage inequality is, however, likely to exaggerate the extent of inequality over a working life as long as individuals are able to move up the earnings distribution. Therefore, I have focussed on the evolution of short and long-term wage mobility in this chapter. Short-term wage mobility, which has been higher for male than for female workers in West and East Germany throughout the observation period, decreased over time. The decrease was particularly pronounced in East Germany. In West Germany, ups and downs in wage mobility levels were observed over time. One reason for this may be business cycle effects as a strongly negative relationship has been found between the level of wage mobility and the unemployment rate for both men and women. As rising wage inequality is accompanied by decreasing wage mobility, a trend which is also observed, e.g., in the US (Buchinsky and Hunt 1999) and the UK (Dickens 2000), the impact of wage mobility on reducing wage inequality has become smaller.

The long time span of the data additionally allows for investigating long-term wage mobility, which gives insights on the chances of moving up the wage ladder over an individual's life cycle. The results for West Germany show that long-term wage mobility was higher for male than for female workers in all years. However, the wage mobility gender gap has been slowly closing over time as long-term wage mobility has slightly increased for women whereas it slightly decreased for men. One reason for this contrary development across gender might be women's relative earnings improvement as the share of females in the middle and upper part of the wage distribution has distinctly increased over time.

As this study has given a descriptive analysis of the developments in wage mobility and inequality, future research is necessary to identify possible causal effects of what drives the differences in the wage mobility pattern across gender and regions. Moreover, more explanatory variables than those covered by this study may influence the development of wage inequality and mobility, as is suggested by, e.g., Gernandt (2009) and Raferzeder and Winter-Ebmer (2007). Finally, it is important to keep in mind that the results only account for all observations up to the social contribution threshold. This is especially relevant for men in West Germany, for which up to 14% of the observations per year are censored. Using imputed wages for the censored observation does not appear to be an accurate solution for the censoring problem as the imputed wages artificially drive the wage mobility pattern.

Nevertheless, the simultaneous observation of increasing wage inequality and decreasing wage mobility clearly calls for a closer consideration of workers earning a low wage as this development gives rise to a larger persistence of low-wage employment. However, the determinants underlying the evolution of low-wage mobility are hardly documented in the literature so far. In particular, a decline in wage mobility in the low-wage sector may result from compositional shifts of the low-wage relative to the high-wage sector. Alternatively, increasing genuine state dependence, i.e. low-wage employment today causing low-wage employment in the future for reasons of, e.g., stigmatization or human capital depreciation, might be an explanation for a decline in low-wage mobility. In the next chapter, I will therefore take a closer look at the determinants underlying the evolution of low-wage mobility.

Appendix 1

1.A - Tables

Table 1.A1: Total observations and share of females in %, by year and region, and share of censored observations in % by year, region, and gender

	West Germany				East Germany			
	share females	share cens. obs men	share cens. obs women	N	share females	share cens. obs men	share cens. obs women	N
1975	33.1	10.9	1.2	358,335				
1976	32.9	9.7	1.0	355,573				
1977	33.0	9.3	1.0	358,787				
1978	33.2	8.5	0.8	358,887				
1979	33.3	8.1	0.8	369,373				
1980	33.5	9.1	0.9	375,384				
1981	33.3	10.1	1.1	376,242				
1982	33.4	9.8	1.1	369,728				
1983	33.1	9.2	1.0	359,831				
1984	33.2	10.4	1.4	361,562				
1985	33.1	11.0	1.5	358,840				
1986	33.5	10.3	1.4	368,045				
1987	33.6	11.8	1.8	370,807				
1988	33.7	11.0	1.6	373,694				
1989	33.9	11.4	1.8	382,760				
1990	34.0	12.4	2.1	400,488				
1991	34.0	12.4	2.2	411,835				
1992	34.5	13.7	2.6	421,287	43.7	3.8	1.2	99,348
1993	34.6	11.1	2.0	409,127	42.6	4.5	1.6	94,138
1994	34.5	11.4	2.1	396,685	41.7	4.4	1.5	92,346
1995	34.4	10.8	2.1	391,759	41.4	4.4	1.5	91,997
1996	34.3	10.3	1.9	382,912	41.3	4.0	1.2	89,246
1997	34.1	10.8	2.1	376,553	40.9	3.4	1.0	84,666
1998	33.9	10.2	2.1	376,218	41.3	4.6	1.8	82,281
1999	34.2	11.9	2.6	378,397	41.4	4.0	1.6	81,223
2000	34.3	11.5	2.7	384,167	41.4	5.1	2.3	78,395
2001	34.4	11.7	3.0	383,544	41.7	5.7	2.8	75,157
2002	34.6	13.5	3.6	374,043	41.9	6.0	3.3	71,773
2003	34.3	9.5	2.2	363,572	41.6	4.3	1.7	69,401
2004	34.1	10.1	2.4	353,955	41.2	4.2	1.8	66,450
2005	34.0	10.3	2.6	346,295	40.9	4.3	1.8	63,362
2006	33.8	10.1	2.5	348,296	40.5	4.4	1.8	63,059
2007	33.9	10.8	2.9	355,064	40.2	4.4	1.7	63,744
2008	34.0	11.8	3.2	359,704	40.0	4.9	2.3	64,164
Total	33.9	10.8	1.9	12,711,749	41.5	4.5	1.8	1,330,750

Table 1.A2: Share of individuals staying in the same quintile as in previous year in %, by quintile, sex and region, 1993 and 2008

	Q1	Q2	Q3	Q4	Q5	N
West Germany						
Men 1993	80.7	69.2	68.7	75.7	91.4	248,818
Men 2008	82.7	76.9	77.0	82.1	94.1	217,347
Women 1993	83.2	70.9	70.1	75.7	90.6	125,018
Women 2008	83.6	76.1	76.3	80.9	92.8	105,767
East Germany						
Men 1993	69.5	51.6	49.9	58.9	82.6	48,352
Men 2008	80.3	72.5	73.7	80.7	93.4	34,219
Women 1993	78.4	61.4	61.6	67.1	84.2	34,757
Women 2008	83.7	77.8	79.0	82.3	92.4	22,429

1.B - Figures

Figure 1.B1: Evolution of real gross daily wages, in 2008 prices, 1975 - 2008, by gender, West Germany

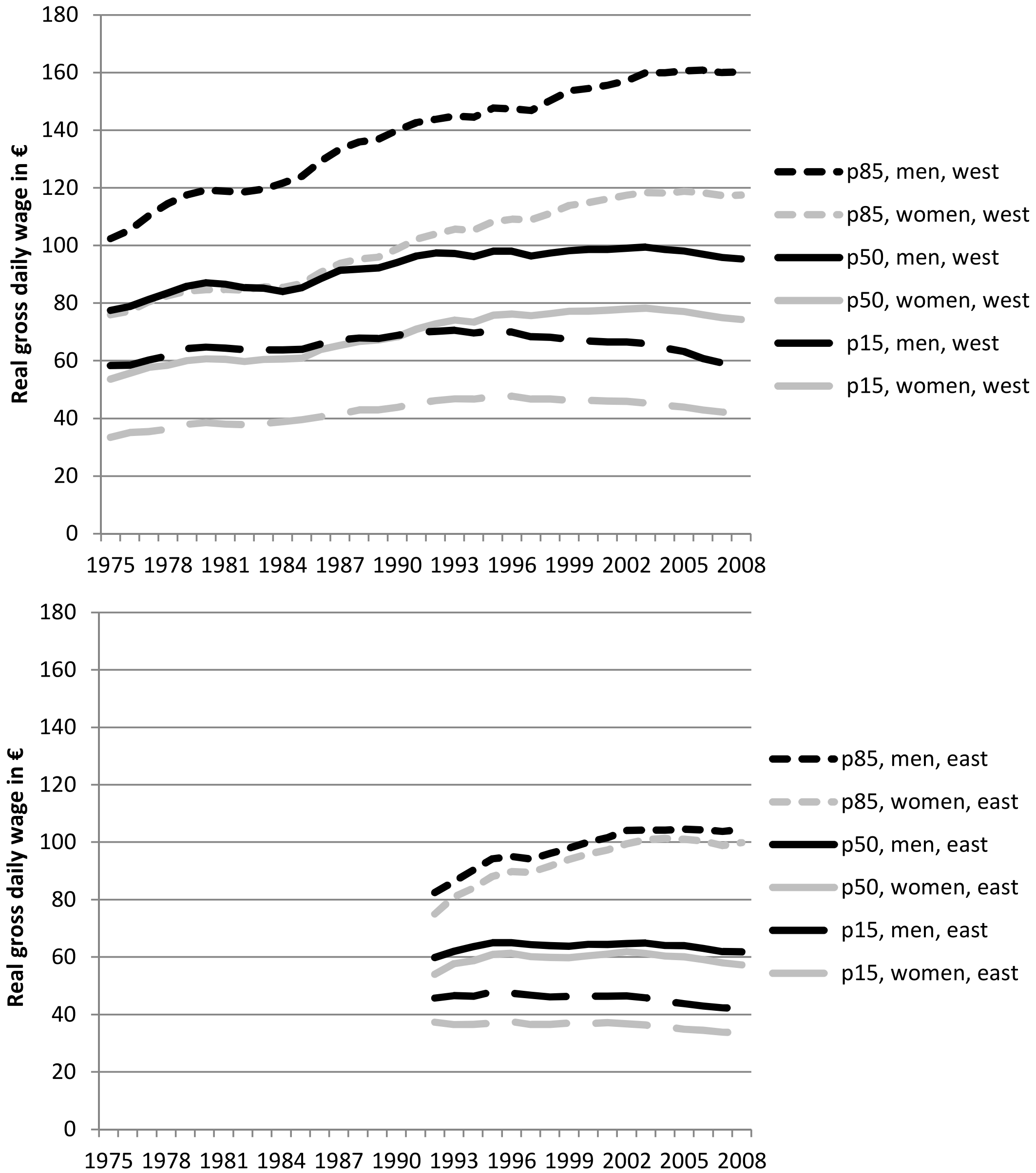
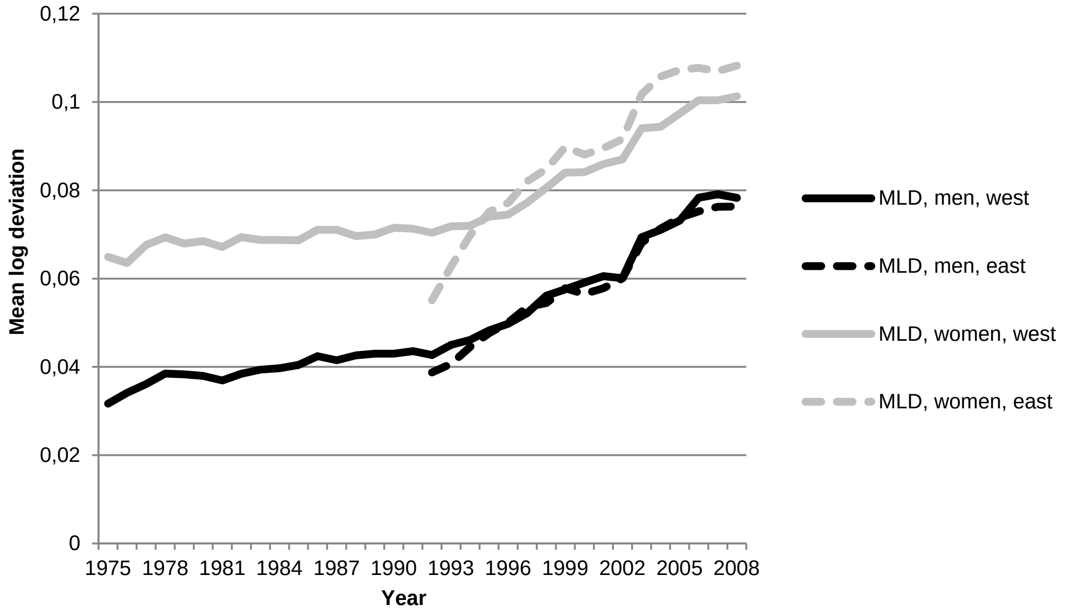


Figure 1.B2: Evolution of the mean log deviation by gender and region, 1975 - 2008



1.C - How to deal with censored observations

For up to 14% (4%) of West German men's (women's) observations per year, the correct wage is not reported since the wages are censored at the social contribution threshold. Therefore, two different scenarios may be applied to overcome this censoring problem. One might either i) use all the wage information up to the censoring limit and disregard the development of wages at or above the social contribution threshold or ii) impute wages for censored observations using the imputation technique proposed by Gartner (2005). Both alternatives have drawbacks: while the former alternative cannot draw a picture on the entire wage distribution, the latter alternative partly relies on wages that cannot be observed in the data.

In order to get an impression to what extent the Gini coefficient, as a measure for wage inequality, is affected by using either alternative, the upper part of Figure 1.B3 plots the development of the West German Gini coefficient by gender using i) only the observations up to the censoring limit and ii) using all observations including the censored observations for which the wages are imputed.[18] Figure 1.B3 reveals that a higher level of the Gini coefficient is observed when wages for the censored observations are imputed. This is not surprising as the entire wage distribution is considered in this case. As the share of censored observations has slightly increased over time for men and women - compare Table 1.A1 -, the difference between the two sample alternatives has become slightly larger over time for both men and women. However, I mainly observe a level effect of the Gini coefficient when the imputed observations are used, which is greater for men as their share of censored wages is higher than for women. Thus, the use of the imputed wages seems rather insensitive to the development of wage inequality over time.

With respect to the development of wage mobility, however, the choice of the sample matters to a much higher degree. The lower part of Figure 1.B3 shows for West Germany how the rank correlation of individual wages between year $t-1$ and year t has evolved with and without imputed wages.[19] The rank correlation, which by definition lies between 0 and 1, is the higher, the higher an individual sticks to his wage position, i.e. the lower the wage mobility is. The figure illustrates that the

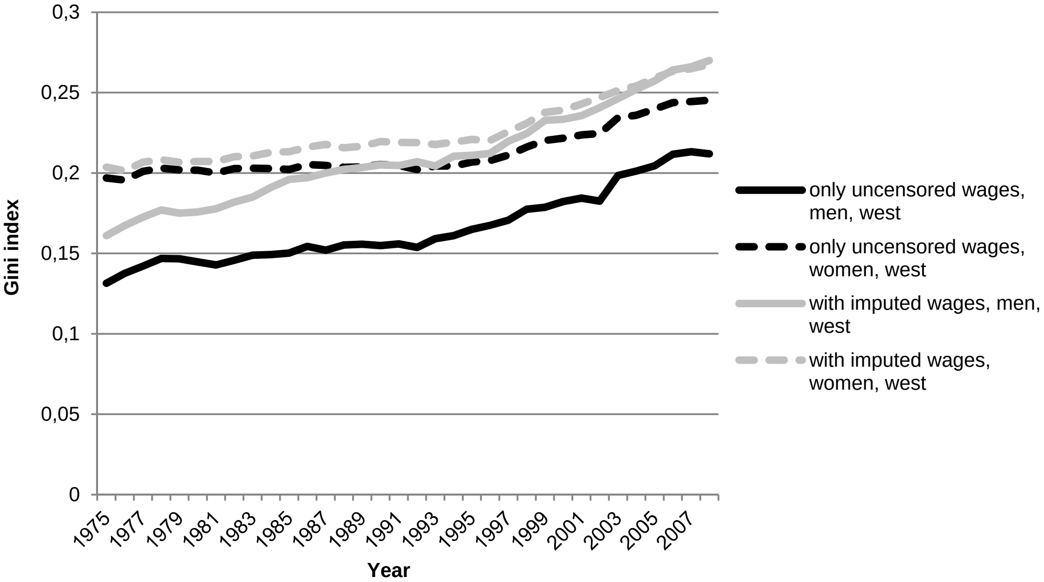
[18]The imputation is conducted separately for men and women, East and West Germany and year. The regression model contains as explanatory variables age, age squared, tenure, tenure squared, degree of education, and occupation.

[19]If only the wage information up to the censoring limit is used, it is important to keep in mind that those individuals who move up to the censoring limit between year $t-1$ and year t are not captured. However, the share of such movers among all workers earning below the censoring limit is on average less than two (one) percent for men (women). Hence, the downward bias of the mobility pattern due to this selection should be very small.

rank correlation is not only smaller when imputed wages are used in the analysis for both men and women, but that it has also developed differently over time compared to the sample with uncensored observations. While the rank correlation increased over time for those individuals earning below the social contribution threshold, the rank correlation remained at the same level over the entire observation period when wages were imputed for the censored observations for both sexes. It is, thus, the mobility of those individuals for which the wages have been imputed and whose share has slightly increased over the years that accounts for most of the wage mobility.

Thus, whereas the observations with imputed wages contributed to a fairly constant shift in wage inequality over time, the analysis of wage mobility seems to be more sensitive with respect to using imputed wages. Figure 1.B4 shows that this result is also observed in East Germany, although to a weaker extent as the share of censored observations is smaller than in West Germany. One reason for the sensitivity in wage mobility could be that an imputed wage is attached to some degree of uncertainty. In other words, it is likely that an imputed wage in one period differs from an imputed wage in the consecutive period for the same individual which could artificially increase one's wage mobility. Therefore, using imputed wages needs to be treated with caution especially when the development of wage mobility is analyzed. In order to avoid such inaccuracies, I therefore only focus on those observations that are below the censoring limit.

Figure 1.B3: Evolution of the Gini coefficient (top) and the rank correlation (bottom) by gender, without censored observations and with imputed wages for censored observations, 1975 - 2008, West Germany



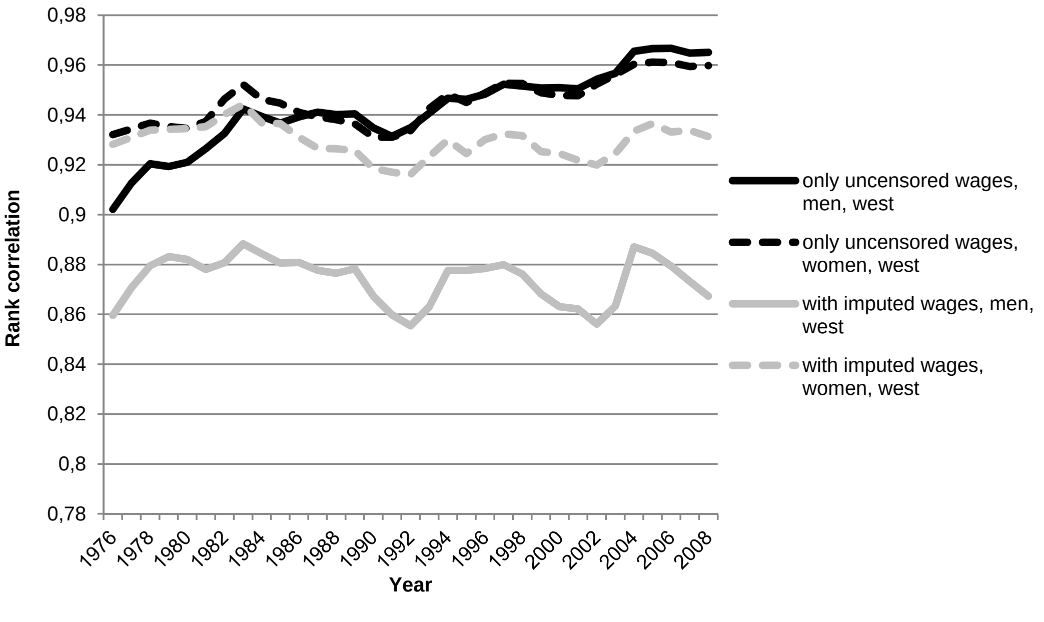
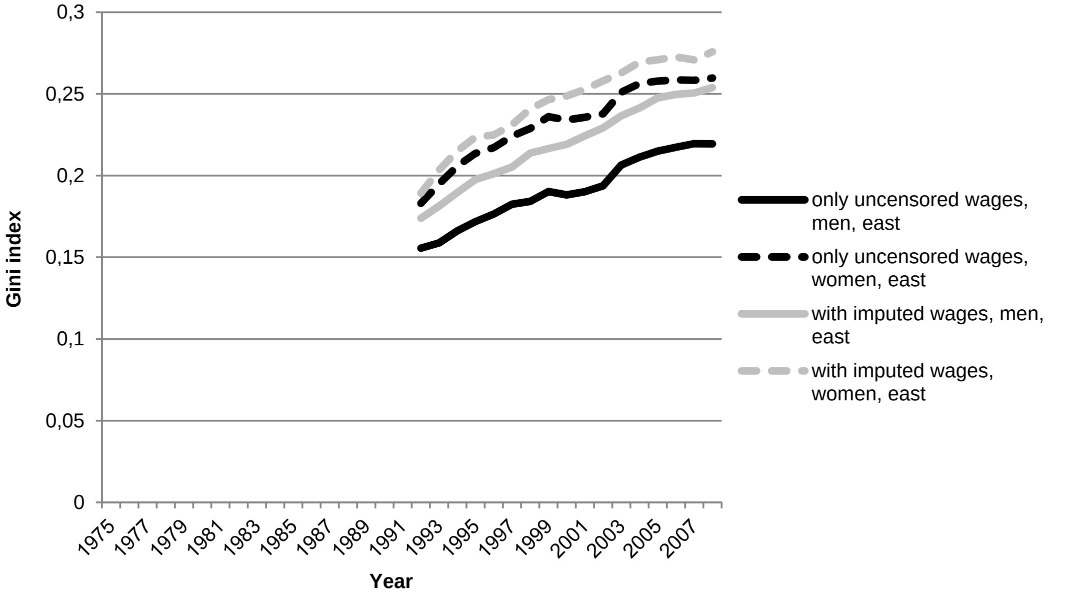
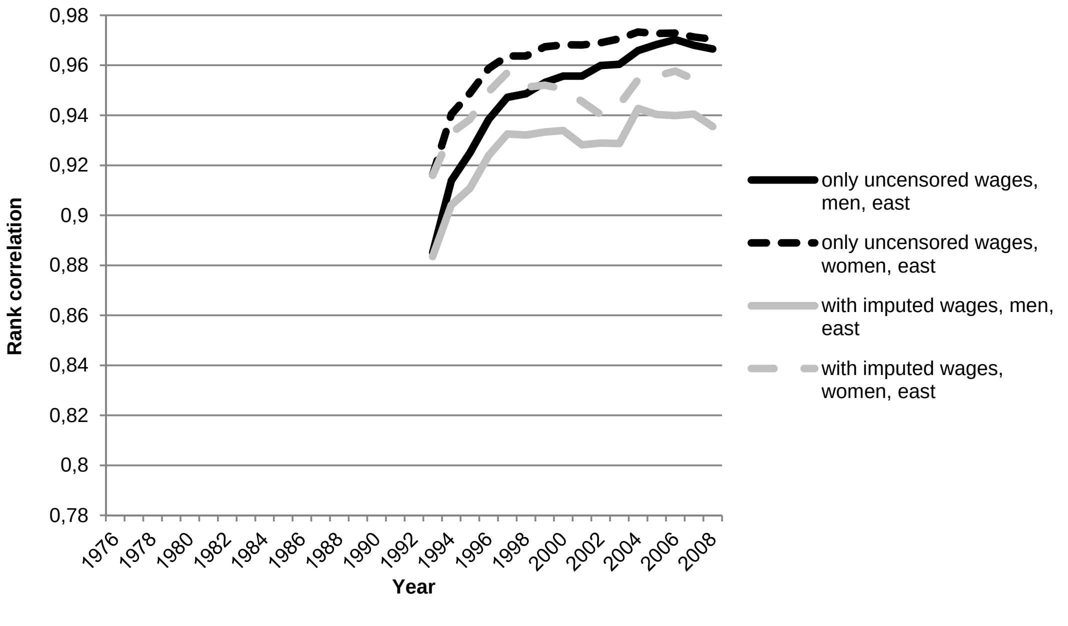


Figure 1.B4: Evolution of the Gini coefficient (top) and the rank correlation (bottom) by gender, without censored observations and with imputed wages for censored observations, 1975 - 2008, East Germany





Chapter 2

What Explains the Decline in Wage Mobility in the German Low-Wage Sector?*

2.1 Introduction

A large body of academic work has documented a sharp increase in earnings inequality particularly in the Anglo-Saxon countries over the last three decades (e.g. Acemoglu 2003, Gosling et al. 2000, Levy and Murnane 1992). However, as long as individuals are able to move up the earnings distribution, a high degree of cross-sectional earnings inequality is likely to exaggerate the extent of inequality over a working life. Thus, any analysis of the evolution of lifetime inequality requires investigating the evolution of both inequality and mobility. For example, a rise in wage mobility could mitigate an increase in cross-sectional earnings inequality because, in that case, a position at the bottom of the distribution would be of more temporary nature. Conversely, if a rise in earnings inequality were accompanied by a decline in mobility, inequality over a working lifetime would

[19]*This contribution is joint work with Nicole Gürtzgen. It is published as ZEW Discussion Paper No. 12-041 (Aretz and Gürtzgen 2012). We thank Melanie Arntz and Anja Heinze for providing us with programming routines as well as Verena Niepel, Steffen Reinhold, and Arne Uhlendorff for fruitful discussions and helpful comments. Financial support from the German Science Foundation (DFG) is gratefully acknowledged (Grant-No. FR 715/9-1).

increase due to more persistent positions in the earnings distribution.

Even though there is a large literature that addresses both the evolution of inequality and mobility over time (e.g. Cardoso 2006, Dickens 2000, Gottschalk 1997, Kopczuk et al. 2010), little is known about the determinants underlying the evolution of wage mobility. This is particularly relevant as a number of authors has documented a widening in the distribution of labor earnings that is accompanied by a decline in mobility, giving rise to a larger persistence of low-wage employment (see Buchinsky and Hunt (1999) for the U.S. , Cardoso (2006) for Portugal and Dickens (2000) for the U.K.). From a welfare perspective, a high degree of low-pay persistence is of particular concern as it tends to marginalize low-wage workers in the long run. As a result, the determinants of low-pay persistence are of considerable interest to policy-makers. For instance, a decline in low-wage mobility that is accounted for by an increase in the fraction of those without educational attainment would call for appropriate policy interventions aiming at improving these characteristics. In contrast, if a decline in low-wage mobility was caused by increasing state dependence due to stigmatization effects, policy measures aiming at improving observable attributes of low-wage workers would be less likely to succeed.

The purpose of this chapter is therefore to fill this gap and to investigate the determinants of the evolution of wage mobility. Using German administrative data, we focus on the West German low-wage sector, which is particularly interesting for several reasons. First, while the German wage structure has long been considered relatively stable at lower percentiles (Prasad 2004), the past two decades have seen a clear tendency towards more earnings inequality at the bottom end of the earnings distribution (Dustmann et al. 2009, Kohn 2006). As a consequence, the low-wage sector has increasingly grown in importance. Second, there is evidence that wage mobility has been declining over the last decades (see Gernandt 2009 and Riphahn and Schnitzlein 2011). However, as mentioned above, this does not necessarily imply a larger degree of persistence in terms of "true" or "genuine" state dependence of low-wage employment, which may occur if low-wage employment today causes low-wage employment in the future for reasons of stigmatization or human capital depreciation.[20] Alternatively, a larger degree of persistence may also be the result of a more unfavorable composition of the low-wage relative to the high-wage sector. The large extent of selection into low-wage employment has been documented by a number of studies dealing with the determinants of wage mobility. A key finding that emerges from this literature is that the extent of genuine state dependence is often considerably reduced once observable attributes and selection into low-wage employment are accounted for (see Cappellari 2002, 2007, Stewart and Swaffield 1999). The overall aim of our analysis is therefore to explore to what extent the observed decline in German low-wage mobility reflects a rise in "true" state dependence by distinguishing the evolution of genuine state dependence from relative composition effects.

The data we use to address these questions stem from the *IAB Employment Subsample* 1975-2004 (IABS), the preceding data set of the *SIAB*. Similar to the *SIAB*, this administrative data set is a 2% subsample of the German *Employment Statistics Register*, which is based on reports from employers in compliance with the notifying procedure for the German social security system. The *Employment*

[20]Using German household data from the GSOEP, Uhlendorff (2006) finds evidence for true state dependence of low-pay jobs. In contrast to Uhlendorff (2006), who looks at pooled transitions over the observation period 1998 to 2003, we focus on the evolution of true state dependence over time.

Statistics Register offers information on individual wage records and a number of individual characteristics for the whole population of employees who are covered by the German social security system. The IABS provides a useful basis for exploring the evolution of the determinants of wage mobility for several reasons. First, the data allow us to retrieve a great deal of reliable information on workers' previous employment histories, which will be used to model the selection of individuals into the low-wage sector. Second, due to the administrative nature of the IABS, the problem of panel attrition is considerably reduced as the data track individuals over time as long as they are either employed or, alternatively, unemployed with transfer payments. Even though our data feature less panel attrition than survey data, we still face the problem of non-random earnings retention as individuals may become voluntarily or involuntarily unemployed and may fall out of the earnings distribution. Because this dropout is likely to be non-random, we follow the approach of Cappellari and Jenkins (2006) and Cappellari (2007) by estimating a series of trivariate probit models, which not only account for the selection into low-wage employment, but additionally model non-random earnings retention. To do so, we will take advantage of the precise information on workers' employment histories in order to find appropriate exclusion restrictions that govern both, the process of earnings retention and the initial conditions process.

The remainder of this chapter proceeds as follows. Section 2.2 provides a description of the data and some descriptive results. Section 2.3 presents the empirical analysis. While Sections 2.3.1 and 2.3.2 spell out the estimation strategy and define the measure of state dependence, Section 2.3.3 presents the empirical results. Section 2.4 concludes.

2.2 Descriptive Empirical Analysis

2.2.1 Data and variable description

The data for our analysis are taken from the regional file of the IAB employment subsample 1975-2004 (*IABS*). This administrative data set, which is described in more detail by Drews (2008), contains a 2% random sample of all social security records spanning the time period 1975 to 2004. It includes employment records subject to social security contributions as well as unemployment records with transfer receipt. The data is representative of all workers subject to the German social security system and covers approximately 80% of the German workforce. Self-employed workers, civil servants and individuals currently doing their military service are not included in the data set. We restrict our analysis to West Germany as East Germany experienced profound political and structural changes during our observation period. We further restrict our sample to individuals aged 20 to 55 years and confine our analysis to the years 1984 to 2004 due to a structural break concerning the wage information which took place in 1984. The *IABS* provides individual information on (daily) wage records, workers' employment histories and a number of individual characteristics such as age, education, nationality and occupational status. Since the data set is comprised out of spell data, the data allow us to retrieve a large number of variables used to proxy the stability of workers' employment histories. In particular, the data allow us to measure tenure at the current employer, the number of previous un- and non-employment spells, the number of previous employers, the number of employment interruptions at the current employer as well as the cumulative duration of previous un- and non-employment spells. A full description as well as descriptive statistics of the variables used in our analysis can be found in Tables 2.A1 and 2.A2, respectively.

Although the *IABS* provides a great deal of information on workers' employment histories, the data set has similar disadvantages as the *SIAB*. First, the data set provides information on daily wages. While we observe an individual's full-time or part-time status (defined as working less than 30 hours per week), the data lack explicit information on the number of hours worked. For this reason, we restrict our sample to full-time workers. Second, the data do not allow a distinction between involuntarily unemployed individuals without transfer receipt and individuals who left the labor force or who became self-employed or civil servants. To distinguish more precisely between voluntary and involuntary unemployment, we follow the assumptions proposed by Lee and Wilke (2009) about when the state of unemployment is reached. Further information on the definition of un- and non-employment spells as well as on adjustments of the wage and the education information can be found in Table 2.A1.

2.2.2 Definition of low-pay status

To study the evolution of wages in the low-wage sector, we define the low-pay status as earning less than two thirds of the median (full-time) wage. This definition has been used in several other studies (e.g. OECD 1998, Stewart and Swaffield 1999 and European Commission 2004). In order to calculate the low-pay threshold for each year, we consider only spells which include the set date June 30th.[21] For each year of our observation period, we start from the population of full-time employed workers for whom we define the low-pay status and the respective pay and

[21] Note, however, that we exploit the full spell structure of the data to construct information on individuals' employment histories. Weighting the wage information by the length of the spell did not alter the results substantially.

employment status five years later. This gives rise to 16 relevant transition periods (1984/1989, 1985/1990, ..., 1999/2004). The low or high-pay status is only defined for those who stay full-time employed five years later. We therefore construct a dummy measuring full-time earnings retention, which takes on the value zero if an individual falls out of the full-time earnings distribution (i.e. become part-time employed, apprentices, involuntarily unemployed or non-employed). Table 2.A3 gives an overview on the yearly median gross daily wages and the corresponding yearly low-wage threshold (in gross daily and approximated gross hourly wages).

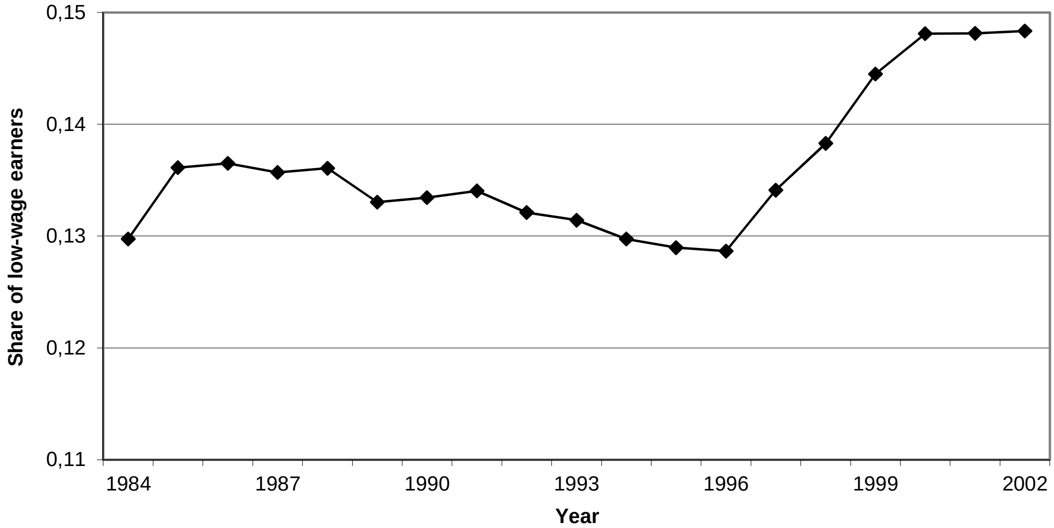
2.2.3 The evolution and pattern of low-pay transitions

In line with the definition of Section 2.2.2, Figure 2.1 displays the fraction of low-paid individuals among all full-time employees for the years 1984 to 2002. The figure shows that the extent of the low-wage sector was quite stable in Germany until the mid 1990s with a slightly decreasing trend until 1996. After that, the low-wage sector experienced a clear upward trend: in 2002, the rate of low-wage earners was 14.9%, 14% higher than in 1996.[22] As shown in Figure 2.2, the overall increase was fully made-up by male workers. Among them, the share of low-paid workers, which was in the range of 5 to 6% between 1984 and 1996, increased to 8% in 2002. In contrast, the share of low-paid women dropped from 33% in 1985 to 28% in 2002. Obviously, however, female workers still exhibit a much higher probability of being low paid than male workers. Compared with workers from the high-wage sector, low-paid individuals are on average four years younger, less well

[22]This development confirms earlier results by Rhein et al. (2005), who find a very similar evolution of low-wage employment using the same data set for the years between 1990 and 2001. Bosch and Kalina (2007), also using the same data set, yield similar results, though at a slightly higher low-wage level.

educated and are more likely to be of foreign nationality (see Table 2.A2). Also, as one would expect, total previous un- and non-employment duration periods have on average been longer for individuals in the low-wage sector, whereas their job tenure is on average shorter than that for individuals working in the high-wage sector.

Figure 2.1: Share of low-wage earners in the total population, 1984 - 2002



In order to describe the overall pattern of the evolution of low-wage persistence, we calculate a measure of aggregate state dependence (ASD) for each year. This measure is defined as the difference between the probability of staying in the low-wage sector and the probability of descending into the low-wage sector five years later, i.e. $ASD = \Pr(L_t = 1|L_{t-5} = 1) - \Pr(L_t = 1|L_{t-5} = 0)$. Note that this measure does not account for the heterogeneity across formerly low and high-paid workers. Later in our analysis, we will therefore attempt to distinguish this measure from the degree of genuine state dependence (GSD). This is the measure of "true" persistence we are ultimately interested in and will be defined as the individual difference in the predicted transition probabilities conditional on being initially low and high paid, respectively, therefore providing a measure of differences in price effects of observables across the two groups (see Section 2.3.2).[23]

Figure 2.2: Share of low-wage earners in the total population by sex, 1984 - 2002

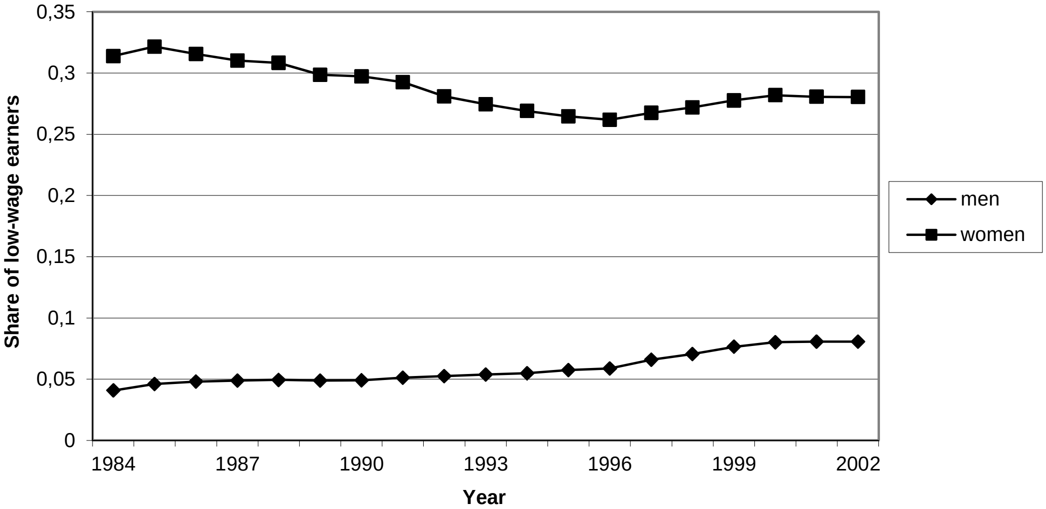
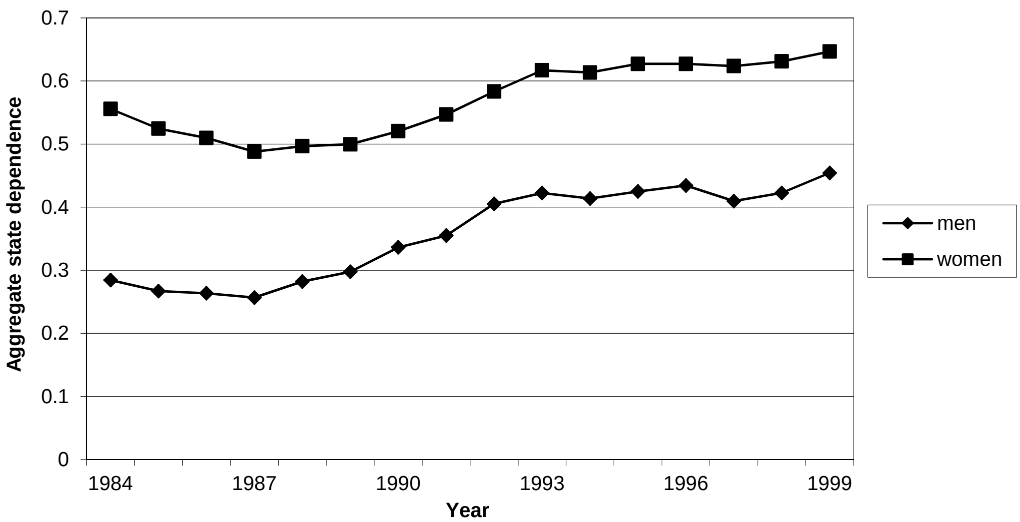


Figure 2.3 illustrates the evolution of ASD between 1984 and 1999 for males and females. As the probability of descending into the low-wage sector is typically close to zero, the extent of ASD is mainly determined by the persistence probability. In 1984, for example, the overall persistence probability was 50%, i.e. one out of two individuals who earned a low wage and still worked five years later kept his or her low-pay status. The probability of moving from high to low pay, however, was very small in 1984, with only every 20th woman and approximately every 100th man descending into the low-wage sector. Figure 2.3 reveals that the level of ASD is about 20 percentage points higher for women than for men. This means that men are considerably less likely to stick to a low-wage job. The development

[23]One of the two predicted transition probabilities will be counterfactual as individuals were initially either high or low paid.

of ASD was quite similar for both sexes, though. Women experienced a larger decrease in ASD in the mid 1980s than men, with both sexes reaching their lowest value in the year 1987 with an ASD of 26% for males and an ASD of 49% for females. Since 1987, ASD has been rising continuously with only a slight attenuation period during the mid 1990s. In 1999, the male ASD amounted to 45%, whereas women faced an ASD of 65%. Thus, (West) Germany has experienced a distinct shift towards a higher degree of low-wage persistence in the last 15 years of the 20th century.

Figure 2.3: Aggregate state dependence by sex, 1984 - 1999



What determines the extent of ASD? Turning to the association between low-pay persistence and observable attributes, Figure 2.4 provides the evolution of ASD by different age groups. The figure illustrates that there are remarkable differences across groups, with younger workers exhibiting considerable less ASD than older ones. While the difference between the probabilities of remaining low paid and descending into low pay was 29% for workers under the age of 25 in

1984, workers older than 50 years faced a much higher ASD (78%). By 1999, this difference had only slightly decreased. However, the share of old workers in the low-wage sector is by far smaller than the share of young workers below 26 years (see Figure 2.B1). Averaged over the whole observation period, only 3% (27%) of male (female) workers above 50 years earned a low wage, whereas 18% (41%) of the young workers below 26 years were low paid. In other words, younger workers have a higher probability of earning a low wage, but are, at the same time, more likely to move up the wage ladder. Once, however, older workers face a low-wage job, it is much harder for them to escape the low-wage sector. Note that this is consistent with the concave shape of age-earnings profiles typically reported in the literature (see e.g. Murphy and Welch, 1990).[24] With respect to an individual's tenure, the interpretation works similar.[25] The shorter the tenure at the current job is, the higher is the likelihood of being low paid (Figure 2.B3) and - on the other hand - the lower will be the extent of ASD (see Figure 2.B2).

The level of education is also clearly related to the degree of state dependence: the better an individual is educated, the lower is his or her ASD. Figure 2.B4 reveals for both sexes that while the ASD for the high and low-skilled increased only very slightly over our observation period, medium-skilled workers experienced a much larger increase in their ASD.[26] Interestingly, the ASD level of medium-skilled workers has converged to the ASD level of the low-skilled - a development which can be observed for men as well as for women. Put differently, medium-skilled workers, once earning a low wage, seemed to face about the same risk of

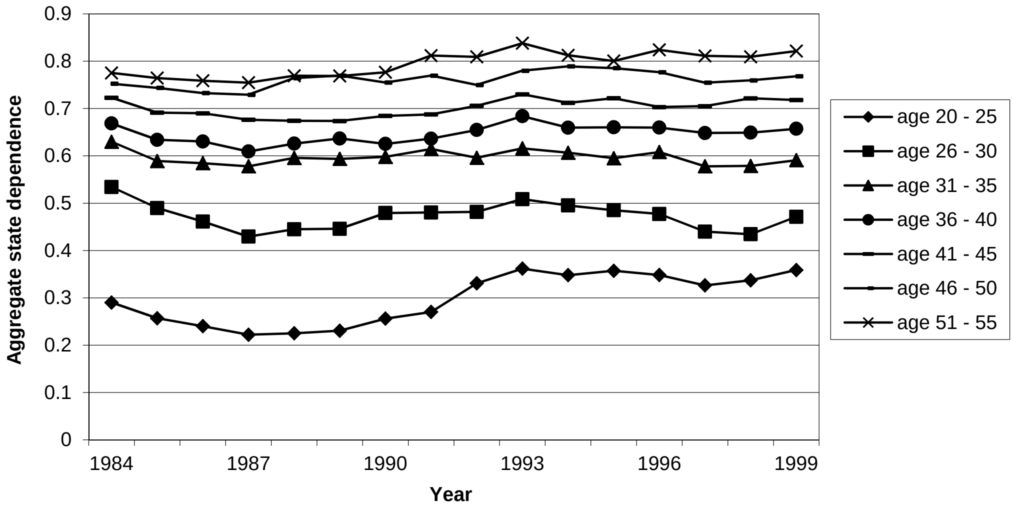
[24]For Germany, recent evidence on age-earnings profiles is provided by Bönke et al. (2011).

[25]All corresponding figures on mentioned variables other than age can be found in Appendix 2B.

[26]The spikes in the ASD of high-skilled low-paid males can be explained by the small number of observations (<100 per year).

being caught in the low-wage sector as their low-skilled counterparts in 1999. As Figure 2.B5 points out, however, the share of medium-skilled men (women) is with 5% (28%) substantially lower than the share of low-skilled individuals in the low-wage sector (13% for men, 38% for women).

Figure 2.4: Aggregate state dependence by age group, 1984 - 1999



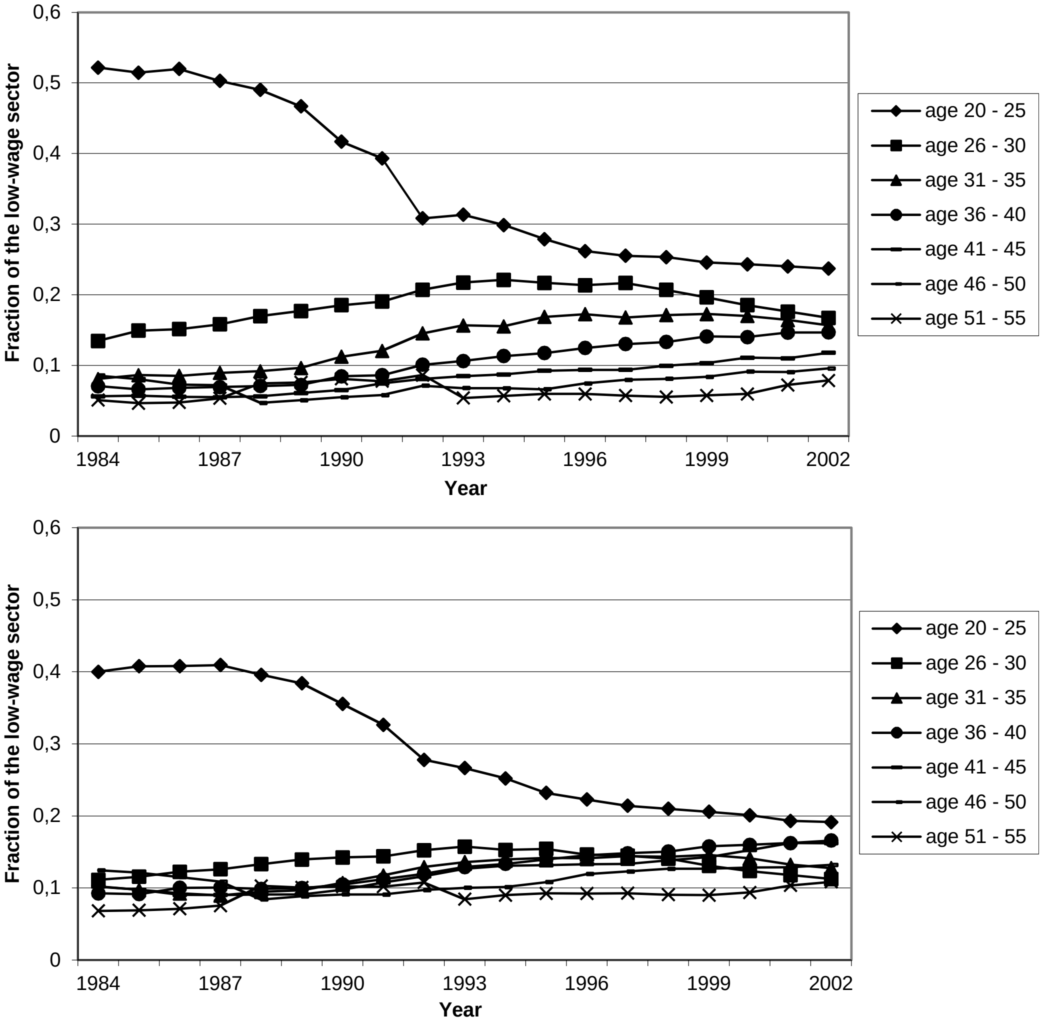
There are further observable characteristics that are associated with different levels of ASD. With respect to nationality, for example, Figure 2.B6 reveals that foreign individuals have long faced a larger risk of sticking to a low-wage job. By the end of the observation period in 1999, however, the ASD for workers of German nationality had converged to the level of their foreign counterparts for both males and females. Such a convergence can not be found when comparing the development of ASD of blue- and white-collar workers (see Figure 2.B7). Over the whole observation period, the ASD of blue-collar workers has remained about 10 percentage points higher than the ASD of white-collar workers. This pattern holds for male as well as for female workers.

Why did the overall aggregate state dependence increase over time? Besides factors like business cycle effects or other macroeconomic developments, one possible explanation could be a change in the relative composition of the low and high-wage sector that caused the extent of ASD to rise. To investigate such possible composition effects, it is instructive to depict the evolution of some selected observable attributes in the low-wage sector relative to the respective development in the high-wage sector. As can be seen from Figure 2.5 separately for men (top part) and women (bottom part), the share of young workers below the age of 26 among the low-paid decreased markedly during the last decades. While the proportion of young female workers in the low-wage sector dropped from 40% to 19% over the observation period, the decrease was even larger for male workers below the age of 26. For the latter, the corresponding share fell from 52% in 1984 to 24% in 2002. This is a much more pronounced decline compared to the development in the high-wage population where the share of young male (female) workers dropped by 7 (14) percentage points between 1984 and 2002, see Figure 2.B8.

Not only the demographic change drives this development. The decision to acquire higher education was taken more often in 2000 (34%) than in 1985 (20%) so that a smaller share of workers was available for the labor market below the age of 26 (Federal Statistical Office, 2011).[27] As a consequence, the sharp decline in the share of young workers among the low-paid, who - as seen in Figure 2.4 - face a lower ASD than the older workforce may have contributed to the overall observed increase in ASD over time.

[27]Following the definition of the OECD publication "Education at a Glance", the numbers of the Statistical Office are calculated by dividing the absolute number of first-year students by the population of individuals in the typical age range. The number for the year 2000 includes the states of East Germany.

Figure 2.5: Evolution of the age composition in the low-wage sector, 1984 - 2002



The development of other observed characteristics, however, works against an increase in ASD over the observation period. The sex composition in the low-wage sector, for example, has shifted distinctly towards more men being low paid. While the share of males in the high-paid population remained roughly constant within the range of 71 to 74%, the share of male workers among the low-paid has steadily risen from 21% in 1984 to 36% in 2002, see Figure 2.B9. Since male workers, on average, face a lower ASD than female workers (compare Figure 2.3 above), this change in the sex composition of the low-wage sector has favored a reduction of the overall ASD over time.[28]

One explanation for the improving situation of women might relate to the decline in the share of low-skilled workers in the female workforce between 1984 and 2002, as can be seen from Figure 2.B10. However, compared to the development in the high-wage sector where the fraction of the low-skilled decreased from 24 to 10%, low-paid females experienced a weaker decline in the fraction of low-skilled workers (from 29 to 21%). Male low-paid workers exhibit an even less favorable evolution in their skill composition: while in the high-wage sector the fraction of male low-skilled workers declined from 17 to 10%, the corresponding share in the low-wage sector even rose by six percentage points (from 27 to 33%, see Figure 2.B11). Moreover, the low-wage sector experienced a less pronounced increase in the share of high-skilled workers than the high-wage sector, where the fraction of high-skilled doubled between 1984 and 2002. Overall, the less favorable evolution of the skill composition among low paid workers should have contributed to a rise in ASD. However, as medium-skilled workers have steadily approached the ASD level of their low-skilled counterparts over time (compare Figure 2.B4), this price effect is likely to have mitigated the effect of the skill composition on ASD over time.

There are further developments that support a compositional explanation of the rise in ASD over time. As a consequence of the ongoing technological change and the associated structural shift from production to service industries, the share of blue-collar workers (as compared to white-collar workers) declined by about ten

[28]However, in what follows, we will analyze the evolution of state dependence for men and women separately.

percentage points in the high-wage population for both sexes between 1984 and 2002 (see Figure 2.B12). While a reduction in the share of blue-collar workers is also observed for low-paid women during the observation period, the opposite is true for male workers in the low-wage sector. Despite the overall decreasing importance of blue-collar jobs, the fraction of blue-collar workers among the low-paid increased between 1984 and 2002. As blue-collar workers are more likely to remain low paid (the ASD is - as shown in Figure 2.B7 - ten percentage points higher than for white-collar workers), the male-specific shift towards more low-paid blue-collar jobs is likely to have contributed to the rise in ASD.

With respect to nationality patterns, Figure 2.B13 illustrates that particularly the low-wage sector has experienced volatile movements over time. While the share of foreign workers in the male high-wage population remained quite stable between 1984 and 1995 (9 to 10%), this fraction more than doubled from 13 to 28% in the male low-wage sector. This development, which - albeit to a somewhat lesser extent - is also observed for females, reflects the rise in migration flows especially into the low-wage sector after the fall of communism and the opening of the Iron Curtain (e.g. Bauer et al. 2005). By 2002, the share of foreigners among low-paid male workers declined back to 20%. As foreign workers faced a higher risk of remaining low paid at the beginning of the observation period, these developments may have fostered an increase in ASD until the mid 1990s and a slight decrease afterwards. For an overview, Table 2.A4 summarizes the development of the composition of the low-wage sector for males and females by selected characteristics for the years 1984 and 2002.

Taken together, the descriptions shown above provide clear evidence of an increasing degree of persistence in the low-wage sector. Moreover, the degree (and the evolution) of persistence varies considerably across observable attributes. It remains, however, unclear whether the increasing persistence can be fully attributed to a compositional shift towards more unfavorable observable characteristics. In addition to the observed developments fostering a rise in low-wage persistence, such as the increasing fraction of older workers and the less favorable skill composition among the low-paid, a low-wage job per se might have increasingly caused low-wage employment in the future, regardless of the evolution of observable attributes. Such a development could stem from an increase in "true" or genuine state-dependence. The question of to what extent the observed increase in ASD is accounted for by a less favorable composition of the low-wage relative to the high-wage sector will therefore be addressed in the next section using a multivariate econometric framework.

2.3 Econometric Analysis of Low-Pay Transitions

2.3.1 Model specification

To isolate the evolution of genuine state dependence, we characterize the determinants of low-pay persistence and exit rates by explicitly distinguishing between observed and unobserved heterogeneity and true state dependence. To do so, we analyze low-pay transitions for each year in our sample period by estimating the probability of being low paid in period t, conditional on the lagged pay status in $t-5$, with $t = 1989-2004$. An endogeneity issue which is commonly referred to as the 'initial conditions problem' (Heckman 1981) arises if the starting point of the earnings process cannot be observed in the data and the unobservables affecting this process are correlated. A common solution is to specify an additional equation for the initial condition and to allow for a correlation between the error terms of the initial conditions and the transition equation. A second endogeneity issue arises since pay transitions are only observable for employees who stay full-time employed five years later. If unobservables affecting the probability of dropping out and the initial low-pay status are correlated, the resulting earnings attrition will be endogenous to the pay transition process.[29]

In order to account for these selection mechanisms, we estimate a series of annual trivariate probit models. Each model includes the determination of low-pay status in period $t-5$ (to account for the initial conditions problem), the determination of whether full-time earnings are observed at both points in time, $t-5$ and t (earnings retention), the determination of pay status in period t, and, finally, the correlation of unobservables affecting these processes.[30]

We first specify the initial low-pay status. Let l^*_{it-5} denote a latent low-pay propensity for individual i at the start of the observation period and x_{it-5} represents a set of individual-specific characteristics. To capture labor market experience and human capital endowment, x_{it-5} includes tenure, tenure squared, a dummy indicating foreign nationality as well as dummies on educational attainment (three categories), occupational status as well as seven different age groups. By allowing the selection on age to vary across years we capture that increasing

[29]The descriptive evidence from Table 2.A2 already suggests that the likelihood of earnings retention is larger for high-paid than for low-paid workers.

[30]Due to computational constraints, we do not exploit the panel structure of our data as we estimate the trivariate model separately for each of the 16 years of our observation period. However, as our main focus is to analyze the evolution of GSD over time in general, the individual correlations over time as well as the occurrence of lagged state dependence or feedback effects as investigated, e.g. by Biewen (2009) and Wooldridge (2005), are beyond the scope of this chapter and might be subject to further research.

persistence over time causes low-wage workers to become older, which in turn affects the age-specific selection into low-wage employment. We also include information on the previous employment history such as the employment status prior to entry into the current establishment, the number of previous unemployment and non-employment spells as well as the cumulated duration of previous unemployment and non-employment spells. We further control for the sectoral affiliation as well as regional dummies. u_{it-5} is the sum of an individual-specific effect, μ_i, and an orthogonal white-noise error, δ_{it-5}, and is assumed to follow a standard normal distribution:

$$l^*_{it-5} = \beta' x_{it-5} + u_{it-5}, \quad u_{it-5} \sim N(0,1). \tag{2.1}$$

If l^*_{it-5} exceeds some unobservable value (normalized to zero), individual i is observed to be low paid. We define a binary indicator $L_{it-5} = 1$ if $l^*_{it-5} > 0$ and zero otherwise.

The next process to be specified is the earnings retention. We assume that the propensity to observe full-time earnings of individual i in period $t-5$ and t can be described by a latent retention index r^*_{it},

$$r^*_{it} = \delta' y_{it-5} + \varepsilon_{it}, \quad \varepsilon_{it} \sim N(0,1), \tag{2.2}$$

where the error term ε_{it} is standard normally distributed and specified as the sum of an individual-specific effect, η_i, and an orthogonal white-noise error, ξ_{it-5}. y_{it-5} includes factors affecting both earnings and the attachment to paid employment. y_{it-5} contains x_{it-5}, i.e. we assume that factors affecting earnings levels are generally also relevant in determining earnings retention. In order to identify the equation, we need to exclude one variable from x_{it-5} and add an additional one that affects the attachment to paid employment which is not part of x_{it-5} (see below). If the latent retention propensity of individual i is lower than some critical unobserved value (again normalized to zero), earnings and low-pay status cannot be observed in period t. Let R_{it} be a binary variable of the earnings retention outcome of each individual, where $R_{it} = 1$ if $r^*_{it} > 0$ and zero otherwise.

The third component of the model is the specification of the low-pay status in period t. We assume that the latent propensity of low pay can be characterized by

$$l^*_{it} = \left[\left(L_{it-5}\right)\gamma'_1 + \left(1 - L_{it-5}\right)\gamma'_2\right] z_{it-5} + \upsilon_{it}, \quad \upsilon_{it} \sim N\left(0,1\right), \tag{2.3}$$

with υ_{it} denoting the sum of an individual-specific effect, τ_i, and an orthogonal white-noise error, ζ_{it-5}. The column vector z_{it-5} comprises individual-specific attributes affecting the pay status in t. In order to deal with simultaneous changes in covariates and pay status, the individual characteristics pertain to period $t-5$. The switching specification in (2.3) allows the impact of the explanatory variables to differ according to the low-pay status in the initial period. Again, L_{it} denotes a binary variable $L_{it} = 1$ if $l^*_{it} > 0$ and zero otherwise, where L_{it} is only observable if $R_{it} = 1$. As a consequence, the sample likelihood will be endogenously truncated.

We assume that the error terms in each of the three equations are jointly distributed as trivariate normal with unrestricted correlations, which can be written as

$$\rho_1 \equiv corr\left(u_{it-5}, \varepsilon_{it}\right) \tag{2.4}$$

$$\rho_2 \equiv corr\left(u_{it-5}, \upsilon_{it}\right) \tag{2.5}$$

$$\rho_3 \equiv corr\left(\upsilon_{it}, \varepsilon_{it}\right). \tag{2.6}$$

The cross-equation correlations provide a parameterization of unobserved heterogeneity. The correlation ρ_1 describes the relationship between unobservable factors affecting the initial low-pay status and earnings retention. A negative sign suggests that individuals who were more likely to be low paid in the initial period are more likely to drop out of full-time employment compared with high-paid individuals. The correlation ρ_2 summarizes the association between unobservable factors determining the initial and the current low-pay status. Here, a positive sign would imply that individuals earning a low wage in $t-5$ are more likely to remain in the low-pay status. The correlation ρ_3 characterizes the relationship between unobservables affecting the retention propensity and the current low-pay status. A negative sign would indicate that individuals employed at both points in time are more likely to escape low pay in t as compared to individuals dropping out of full-time employment. Estimation of unconstrained cross-correlation coefficients provides a test of whether initial conditions and the earnings retention process may be treated as exogenous. In particular, $\rho_1 = \rho_3 = 0$ would imply that the earnings retention process is exogenous and would give rise to a bivariate probit model. Similarly, testing the exogeneity of initial conditions amounts to testing $\rho_1 = \rho_2 = 0$. Finally, if all cross-equation correlations are zero, then γ_1 and γ_2 can be consistently estimated using univariate probit models on subsamples depending on individuals' initial pay status ($L_{it-5} = 0$ or $L_{it-5} = 1$).

Estimating the model with unrestricted cross-equation correlations requires identifying restrictions, i.e. variables entering x_{it-5} and y_{it-5} but not z_{it-5}. In other words, one ideally needs variables that affect the initial condition and the retention probability but not the transition process. In what follows, we will argue that variables proxying the stability of a worker's employment history might satisfy these requirements. This is based upon the notion that low-wage jobs may be the result of asymmetric information about a worker's true productivity, which is not known ex-ante and becomes known more precisely as a worker's job tenure increases (Jovanovic 1979). In this case, the less regular the employment history, the more difficult it becomes for an individual to signal high productivity, which determines the initial wage. Once, however, an individual is observed five years later in the sample, the employment history may be expected to lose importance in determining an individual's wage position. Thus, employment history variables may be suitable instruments as they are likely to affect the attachment to full-time employment and the probability of being initially low paid, but not the low-pay transition. We will test the validity of our exclusion restrictions imposed for identification using functional form as the identifying restriction. As will be shown in Section 2.3.3.1., excluding the number of previous employers until 1989 (the number of previous unemployment spells after 1989) from the transition and retention equation as well as the number of employment interruptions with the current employer from the transition and initial conditions equation fulfills the requirements for the validity of these restrictions for the men's model. For women, we use the total unemployment duration as an identifying variable for the retention process as well as the number of employment interruptions with the current employer for the initial low-pay status and exclude these variables from the transition equation.[31]

2.3.2 Measures of state dependence

To investigate the extent to which the decline in the probability of escaping low

[31] Although Jovanovic (1979) does not explicitly allow for recalls in his model, it is reasonable to assume that the number of employment interruptions may worsen the precision of workers' signals with respect to their true productivity.

earnings is caused by an increase in the persistence of low-wage employment, we distinguish between aggregate state dependence (ASD) and genuine state dependence (GSD). ASD is obtained by computing the difference in average predicted transition probabilities for those who were low paid in $t-5$ and for those who were initially high paid:

$$\begin{aligned} ASD_t &= \frac{\sum_{i\in(L_{it-5}=1,R_{it}=1)} \Pr\left(L_{it}=1|L_{it-5}=1\right)}{\sum_i L_{it-5}\cdot R_{it}} - \frac{\sum_{i\in(L_{it-5}=0,R_{it}=1)} \Pr\left(L_{it}=1|L_{it-5}=0\right)}{\sum_i \left(1-L_{it-5}\right)\cdot R_{it}} \\ &= \frac{\sum_{i\in(L_{it-5}=1,R_{it}=1)} \frac{\Phi_2\left(z_{it-5}\widehat{\gamma}_1, x_{it-5}\widehat{\beta}; \rho_2\right)}{\Phi\left(x_{it-5}\widehat{\beta}\right)}}{\sum_i L_{it-5}\cdot R_{it}} - \frac{\sum_{i\in(L_{it-5}=0,R_{it}=1)} \frac{\Phi_2\left(z_{it-5}\widehat{\gamma}_2, -x_{it-5}\widehat{\beta}; -\rho_2\right)}{\Phi\left(-x_{it-5}\widehat{\beta}\right)}}{\sum_i \left(1-L_{it-5}\right)\cdot R_{it}}, \quad (2.7) \end{aligned}$$

where $\Phi(\cdot)$ and $\Phi_2(\cdot)$ are cumulative density functions of the univariate and bivariate standard normal distributions.[32] This measure does not take into account individual observed or unobserved heterogeneity.

Genuine state dependence arises if initial low pay causes low-pay employment in the future for reasons of, e.g., stigmatization or human capital depreciation. The absence of GSD can be directly tested by using the endogenous switching structure in (2.3) and amounts to testing the null hypothesis $H_0 : \gamma_1 = \gamma_2$. To account for individual-specific heterogeneity, the GSD is based upon individual-specific probability differences. In particular, GSD is derived by first predicting for each individual with earnings retention five years later two transition probabilities, of which one will be counterfactual: i) the probability of staying in the low-wage sector (conditional on being initially low paid) and ii) the probability of descending

[32]Depending on the model's predictive performance, the predicted ASD should be close to the descriptive ASD that we introduced in Section 2.2.3. As we will show later, this is indeed the case.

into the low-wage sector (conditional on being initially high paid). In a second step, the individual differences of these predicted transition probabilities are averaged over the sample of those with observed earnings in t and $t-5$:

$$\begin{aligned} GSD_t &= \frac{1}{\sum_i R_{it}} \sum_{i \in R_{it}=1} \left[\Pr\left(L_{it}=1|L_{it-5}=1\right) - \Pr\left(L_{it}=1|L_{it-5}=0\right)\right] \\ &= \frac{1}{\sum_i R_{it}} \sum_{i \in R_{it}=1} \left[\frac{\Phi_2\left(z_{it-5}\widehat{\gamma}_1, x_{it-5}\widehat{\beta}; \rho_2\right)}{\Phi\left(x_{it-5}\widehat{\beta}\right)} - \frac{\Phi_2\left(z_{it-5}\widehat{\gamma}_2, -x_{it-5}\widehat{\beta}; -\rho_2\right)}{\Phi\left(-x_{it-5}\widehat{\beta}\right)}\right] \qquad (2.8) \end{aligned}$$

The log-likelihood contribution for each individual i with earnings information observed in period $t-5$ is:

$$\begin{aligned} \log \pounds_i &= L_{it-5}R_{it} \log\left[\Phi_3\left(g_i\gamma_1' z_{it-5}, h_i\delta' y_{it-5}, d_i\beta' x_{it-5}; g_ih_i\rho_3, g_id_i\rho_2, h_id_i\rho_1\right)\right] \\ &\quad + \left(1-L_{it-5}\right)R_{it} \log\left[\Phi_3\left(g_i\gamma_2' z_{it-5}, h_i\delta' y_{it-5}, d_i\beta' x_{it-5}; g_ih_i\rho_3, g_id_i\rho_2, h_id_i\rho_1\right)\right] \\ &\quad + \left(1-R_{it}\right)\log\left[\Phi_2\left(h_i\delta' y_{it-5}, d_i\beta' x_{it-5}; h_id_i\rho_1\right)\right], \qquad (2.9) \end{aligned}$$

where Φ_3 is the cumulative density function of the trivariate standard normal distribution and $g_i \equiv 2L_{it}-1$, $h_i \equiv 2R_{it}-1$, $d_i \equiv 2L_{it-5}-1$. We compute the trivariate standard normal distribution by applying the Geweke-Hajivassiliou-Keane simulator, yielding a maximum simulated likelihood estimator (see Cappellari and Jenkins 2003 and 2006).

2.3.3 Results

In Section 2.3.3.1, we next examine whether the model that we introduced in the last section is correctly specified and validly identified. Section 2.3.3.2 presents the regression results that show the impact of different individual characteristics on our binary outcome variables. Section 2.3.3.3 deals with the development of aggregate GSD as the main outcome of interest derived from the model. Due to the switching specification of the transition equation, the evolution of GSD will still reflect both changes in the workforce composition as well as changes in the differential impacts of the observed covariates on the transition probabilities. Therefore, Section 2.3.3.4 presents a decomposition of the evolution of GSD into a characteristics and a coefficients effect.

2.3.3.1 Correlation structure and hypothesis tests

For each year, our estimation sample is based on those individuals for whom we observe full-time earnings in our data set. That the trivariate probit model is necessary to derive consistent estimates of our parameters of interest is confirmed for all years as the hypothesis that $\rho_1 = \rho_3$ and $\rho_1 = \rho_2$ has to be clearly rejected at the 0.1% significance level for both men and women. This provides evidence of the endogeneity of the initial-conditions process and the earnings retention process. The tests also show for men as well as for women that the hypothesis $\gamma_1 = \gamma_2$ and, thus, the hypothesis of no genuine state dependence must be rejected at the 0.1% significance level in each of the 16 years of our observation period.

The cross equation correlation structure is summarized in Table 2.1 for both male and female workers. As expected, the correlation between unobservables af-

Table 2.1: Equation correlation structure

Year	Males			Females		
	ρ_1	ρ_2	ρ_3	ρ_1	ρ_2	ρ_3
1984	-.223***	0.008	0.272**	-.157***	0.607	0.213
1985	-.202***	0.054	0.406***	-.131***	0.082	0.477**
1986	-.176***	-.200	0.348***	-.132***	0.285*	0.412**
1987	-.206***	-.183	0.140	-.115***	0.200	0.409*
1988	-.207***	-.299	0.131	-.118***	0.078	0.544***
1989	-.198***	-.046	0.298**	-.113***	0.114	0.459***
1990	-.199***	-.368*	0.199*	-.117***	0.227	0.436***
1991	-.210***	-.206	-.070	-.132***	0.183	0.394**
1992	-.218***	-.261	0.009	-.132***	0.148	-.194**
1993	-.240***	-.229	0.279	-.142***	-.902***	0.003
1994	-.214***	-.061	-.205**	-.145***	-.127	-.239***
1995	-.223***	0.099	-.176**	-.150***	0.108	-.279***
1996	-.235***	-.351*	-.297***	-.154***	-.103	-.048
1997	-.221***	-.098	-.171**	-.150***	0.053	-.146
1998	-.225***	-.356	-.056	-.144***	0.176	-.209**
1999	-.234***	-.204	-.255***	-.160***	-.275	-.181

Note: ρ_1: Correlation between initial conditions and retention;
ρ_2: Correlation between initial conditions and low-pay transition;
ρ_3: Correlation between retention and low-pay transition;
Significance levels: ***: p<0.01; **: p<0.05; *: p<0.1.

fecting retention and initial conditions is estimated to be negative in each year for both sexes, indicating that those who were initially low paid are less likely to be employed at both points in time. A significant correlation between the initial condition and the transition equation cannot be found, neither for male nor for female workers. This suggests that any bias due to the selection into low-wage employment influences the transition process through its impact on employment retention. Interestingly, the correlation between the retention and the transition equation has changed over time for men as well as for women. In the first years of our observation period, we observe a significantly positive relationship between unobservables affecting the retention propensity and low-pay persistence. This indicates that individuals employed at both points in time were less likely to escape low pay in t as compared to individuals dropping out of full-time employment. In the mid 1990s, the sign switches for both sexes and becomes significant in some years. Note that a potential explanation for this finding may relate to the change in the age composition of the low-wage sector. Figure 2.5 reveals that the start of the deceleration in the decline in the young workers' share roughly coincides with the estimated switch in the correlation between retention and low-pay persistence. Given that low-pay is more likely to reflect entry wages in the first half of our observation period, this may help explain the positive association between unobservables favoring low-pay persistence as well as employment stability. In the second half of our observation period, in contrast, low-pay workers appear to be more negatively selected upon unobservables fostering persistence as well as instable employment histories.

To be validly identified, the typical identifying conditions need to hold for each of the trivariate models, i.e. the excluded variables should have a significant impact on retention (the initial condition) but not on the low-pay transition. Thus, in our case the conditions require the number of employment interruptions with the current employer to significantly affect the men's probability of staying in the sample in a given year but, at the same time, to have no significant impact on the probability of staying in the low-wage sector. As can be seen from Table 2.2, the two excluded variables have a significant impact on the retention and the initial conditions equation for male workers in each of the 16 years. For women, we similarly observe a significant effect of the number of employment interruptions with the current employer on the probability of initially earning a low wage for all years. The effect of the total unemployment duration on the retention probability is, however, only significant in 8 out of the 16 years. An overview of the tests con-

Table 2.2: Inclusion of instruments

Year	Males		Females	
	Instr. 1 in retention eq.	Instr. 2 in init. cond. eq.	Instr. 1 in retention eq.	Instr. 2 in init. cond. eq.
1984	0.000	0.000	0.029	0.000
1985	0.004	0.000	0.059	0.000
1986	0.001	0.000	0.652	0.000
1987	0.003	0.000	0.463	0.000
1988	0.000	0.000	0.132	0.000
1989	0.000	0.000	0.460	0.000
1990	0.000	0.000	0.006	0.000
1991	0.000	0.000	0.024	0.000
1992	0.000	0.000	0.008	0.000
1993	0.000	0.000	0.017	0.000
1994	0.001	0.000	0.172	0.000
1995	0.006	0.000	0.382	0.000
1996	0.000	0.000	0.401	0.000
1997	0.001	0.000	0.369	0.000
1998	0.023	0.000	0.013	0.000
1999	0.008	0.000	0.000	0.000

Note: All values are p-values. For men, instrument 1 denotes the number of employment interruptions with the current employer; instrument 2 denotes the number of previous employers until 1989; after 1989 instrument 2 refers to the number of previous unemployment spells. For females, instrument 1 denotes the total unemployment duration; instrument 2 is the number of employment interruptions with the current employer.

cerning the second condition - the insignificant impact of the excluded variables in the transition equation - is given in Table 2.3 for both men and women. While the first two columns show for each sex the significance tests for each excluded variable separately, the third column displays the joint significance test of the two excluded variables in the transition equation. We observe for male workers that the number of previous employers (until 1989) and the number of previous unemployment spells (since 1990) seem to be valid instruments for the initial conditions equation. With the exception of 1987, the impact of these variables on the likelihood to stay in the low-wage sector is insignificant at the 5% level. The number of employment interruptions also turns out to be a suitable exclusion restriction as the variable has no significant impact in the transition equation in the majority of years. As a result, the joint significance test accepts the hypothesis (at the 5% significance level) of the two variables having no impact in the transition equation in 12 out of 16 years.

Table 2.3: Exclusion of instruments from the transition equation

Year	Males			Females		
	Instr. 1	Instr. 2	Instr. 1+2	Instr. 1	Instr. 2	Instr. 1+2
1984	0.159	0.328	0.181	0.488	0.611	0.659
1985	0.026	0.316	0.092	0.838	0.724	0.908
1986	0.382	0.495	0.506	0.604	0.444	0.619
1987	0.024	0.021	0.001	0.994	0.008	0.047
1988	0.333	0.376	0.271	0.498	0.030	0.076
1989	0.008	0.388	0.013	0.715	0.001	0.004
1990	0.268	0.301	0.245	0.818	0.000	0.001
1991	0.042	0.156	0.031	0.702	0.001	0.004
1992	0.007	0.742	0.031	0.467	0.570	0.607
1993	0.388	0.435	0.432	0.849	0.256	0.565
1994	0.576	0.213	0.430	0.342	0.975	0.696
1995	0.071	0.314	0.103	0.301	0.244	0.251
1996	0.443	0.751	0.690	0.142	0.427	0.214
1997	0.069	0.312	0.119	0.027	0.603	0.077
1998	0.037	0.442	0.086	0.007	0.961	0.042
1999	0.898	0.252	0.562	0.074	0.189	0.063

Note: All values are p-values. For men, instrument 1 denotes the number of employment interruptions with the current employer, instrument 2 denotes the number of previous employers until 1989; after 1989 instrument 2 refers to the number of previous unemployment spells. For women, instrument 1 denotes the total unemployment duration, instrument 2 is the number of employment interruptions with the current employer.

The tests of the insignificance of the excluded variables for women reveal that both variables seem to provide valid exclusion restrictions for the majority of years. This is shown by the joint significance test that accepts the hypothesis (at the 5% significance level) that the two variables have no impact in the transition equation for 11 out of 16 years. However, for both excluded variables there are some periods where the requirements for a valid identification are not fully met. While the total unemployment duration has a significant impact in the transition equation in the last three years of our observation period (1997-1999), the number of employment interruptions with the current employer affects the likelihood of staying in the low-wage sector significantly between 1987 and 1991. Since - as seen in Table 2.2 - the impact of this variable on the initial conditions equation is at the same time not significant, a valid identification of the women's model might not be achieved for some years in the late 1980s, so that the results for these years should be interpreted with caution. Taken together, however, the tests show that for the majority of years the trivariate probit models are well identified for both sexes.

2.3.3.2 Regression results

After having clarified the conditions for identification, we summarize the estimation results over all 16 years for male workers in Table 2.4 and for female workers in Table 2.5. The tables show for all variables their impact on the retention probability (R_t), the initial conditions (L_{t-5}), the likelihood of entering the low-wage sector conditional on being initially high paid ($L_t \mid L_{t-5} = 0$) and the likelihood of staying in the low-wage sector conditional on being initially low paid ($L_t \mid L_{t-5} = 1$). We summarize the estimation results for each equation by two different indicators. To get an impression of each variable's robustness over time, the first column not only displays for each equation the direction of the sign, which is most often observed over time, but also the frequency of the signs' appearance of the estimated coefficients on a scale of one ($+/-$) to three ($+++/---$), with $+++/---$ representing a positive/negative effect in each of the 16 years. The second column provides insights into each covariate's consistency over time by summing up the number of years in which the sign of the estimated coefficient switched.

The variables explain the dependent variables over time quite robustly as the estimated coefficients do not change their sign over time for the majority of the covariates. If they do so, they most likely change it only once, indicating a possible structural change in the variables' influence on the different processes. This result applies to men as well as to women, although the estimation results for men seem to show a somewhat higher degree of robustness with respect to both indicators.

Turning first to the earnings retention (R_t) and the initial conditions process (L_{t-5}), a comparison of the equations reveals that for male workers almost all covariates exhibit opposite signs with respect to their impact on both processes (see Table 2.4). Characteristics that reflect unstable employment records like longer un- and non-employment durations, a higher number of non-employment spells as well as employment interruptions and a change of the employer, favor the likelihood of being initially low paid and reduce the probability of remaining full-time employed five years later. Also, being foreign makes it more likely to be initially low paid and decreases the retention probability. For other variables, the interpretation works just the other way around: not surprisingly, a higher tenure as well as a higher education reduce the probability of an initial low-wage status and increase the retention probability. Only for blue-collar workers as well as for previous full-time employed individuals, we observe that the signs of the two coefficients point into the same direction. The retention probability as well as the probability of being initially low paid is higher for blue-collar (than for white-collar) workers and for those individuals who were previously full-time (rather than not full-time) employed. At first glance, the positive association between a previous full-time employment status and the probability of being initially low paid appears to be somewhat counterintuitive. However, this result may reflect the fact that males exhibiting non-standard employment relationships reflect a particularly selected

Table 2.4: Regression summary for men, 1984 - 1999

Variables	R_t		L_{t-5}		$L_t \vert L_{t-5}=0$		$L_t \vert L_{t-5}=1$	
	Robustness of direction	number of years in which sign switched	Robustness of direction	number of years in which sign switched	Robustness of direction	number of years in which sign switched	Robustness of direction	number of years in which sign switched
Age 26 - 30 (ref.: age 20 - 25)	+++ [1]	0	− − −	0	− − −	0	+++	0
Age 31 - 35	+++	0	− − −	0	− − −	0	+++	0
Age 36 - 40	++	1	− − −	0	− − −	0	+++	0
Age 41 - 45	++	1	− − −	0	− − −	0	+++	0
Age 46 - 50	−−	1	− − −	0	−−	2	+++	0
Age 51 - 55	− − −	0	− − −	0	−	0	+++	0
Tenure	+++	0	− − −	0	− − −	0	+++	0
Tenure squared	− − −	0	++	1	++	1	−−	3
Being foreign (=1)	− − −	0	+++	0	+++	0	++	4
Medium-skilled (ref.: low-skilled)	+++	0	− − −	0	− − −	0	− − −	0
High-skilled	+++	0	− − −	0	− − −	0	− − −	0
Previously full-time empl. (=1)	+++	0	+++	0	− − −	0	+	6
Total non-employment duration	− − −	0	+++	0	+++	0	+++	0
Total unemployment duration	− − −	0	+++	0	+++	0	+++	0
Number of non-empl. spells	− − −	0	+++	0	+++	0	++	3
Employment interruption (=1)	− − −	0	+	7	+	7	++	2
Change of employer (=1)	−−	1	+++	0	+++	0	−−	4
Blue-collar worker (=1)	+++	0	+++	0	+++	0	+++	0
Number of unemployment spells[2]	+++	0	− − −	0	− − −	0	− − −	0
Number of previous employers[3]	− − −	0	− − −	0	+++	0	+	3
Number of empl. interruptions	+++	0						

[1] Measure of direction robustness:
+ + +/− − −: Variable has a positive/negative impact in all 16 years.
++/−−: Variable has a positive/negative impact in 12 to 15 of the 16 years.
+/−: Variable has a positive/negative impact in 9 to 11 of the 16 years.
[2] Coefficients for R_t and $L_t|L_{t-5}$ are only available for the years 1984-1989.
[3] Coefficients for R_t and $L_t|L_{t-5}$ are only available for the years 1990-1999.

group in the labor market. Considering age, we observe that workers below the age of 26 years face the highest risk of being low paid, whereas we see a U-shaped pattern with respect to the retention probability. Young workers (20 - 25 years) and older workers above 45 years face a lower retention probability than the middle-aged.

All in all, the signs of the variables vary little for female workers in comparison to men (see Table 2.5). Exceptions are skill and occupational status. Other than for men, being high-skilled (versus low-skilled) and being a blue-collar (versus a white-collar) worker reduces the retention probability. The estimation results demonstrate that especially females between 26 and 30 years have a lower probability of staying in the sample five years later compared to young female workers below 26 years. This result might be explained by the fact that women leave the labor market more frequently during that period, for example due to maternity leave. With respect to the initial condition process, the main difference between men and women concerns the previous employment status. In line with what one would expect, a previous full-time employed position decreases the probability of being initially low paid for women.

Turning next to the transition equation, the coefficients of all covariates are allowed to differ - in line with our switching regression specification - across those who were initially low paid ($L_t \mid L_{t-5} = 1$) and for those initially high paid ($L_t \mid L_{t-5} = 0$). As one might expect, for most variables the signs of the estimated coefficient point into the same direction as in the initial conditions equation, particularly for those initially high paid. With respect to age, however, we see deviations conditional on the initial low-pay status for male as well as for female workers. Conditional on being initially high paid, younger individuals up to an age of 35 years have a higher probability than those older than 35 years to descend into the low-wage sector five years later. In contrast, conditional on being initially low paid, the group of the youngest workers exhibits the lowest probability of sticking to a low-wage job. Thus, once earning a low wage, it is much more

Table 2.5: Regression summary for women, 1984 - 1999

Variables	R_t		L_{t-5}		$L_t \vert L_{t-5} = 0$		$L_t \vert L_{t-5} = 1$	
	Robustness of direction	number of years in which sign switched	Robustness of direction	number of years in which sign switched	Robustness of direction	number of years in which sign switched	Robustness of direction	number of years in which sign switched
Age 26 - 30 (ref.: age 20 - 25)	−− [1]	1	− − −	0	++	4	+++	0
Age 31 - 35	+++	0	−−	2	+	3	+++	0
Age 36 - 40	+++	0	−−	4	− − −	0	+++	0
Age 41 - 45	+++	0	− − −	0	−−	2	+++	0
Age 46 - 50	+++	0	−−	2	−−	2	+++	0
Age 51 - 55	−	2	−	2	−−	3	+++	0
Tenure	+++	0	− − −	0	−−	2	+++	0
Tenure squared	−−	2	++	1	++	3	− − −	0
Being foreign (=1)	− − −	0	—	0	−−	4	−−	2
Medium-skilled (ref. low-skilled)	+++	0	− − −	0	o	1	−−	2
High-skilled	−	1	− − −	0	− − −	0	− − −	0
Previously full-time empl. (=1)	+++	0	− − −	0	− − −	0	−	4
Total non-employment duration	− − −	0	+++	0	+++	0	+++	0
Number of non-empl. spells	− − −	0	+++	0	++	2	−	6
Number of previous employers	− − −	0	− − −	0	+	1	−−	5
Employment interruption (=1)	− − −	0	+++	0	++	4	++	1
Change of employer (=1)	−−	3	+++	0	++	2	+	4
Number of unemployment spells	+	4	++	1	+++	0	++	6
Blue-collar worker (=1)	− − −	0	+++	0	+++	0	+++	0
Total unemployment duration	−−	2						
Number of empl. interruptions			− − −	0				

[1] Measure of direction robustness:
+ + +/− − −: Variable has a positive/negative impact in all 16 years.
++/−−: Variable has a positive/negative impact in 12 to 15 of the 16 years.
+/−: Variable has a positive/negative impact in 9 to 11 of the 16 years.
o: Variable has a positive and a negative impact in 8 of the 16 years.

difficult for older individuals to escape from it than for the younger ones. This result is valid for both men and women and confirms the descriptive findings from Section 2.2.3.

To assess the quantitative meaning of a variable's impact, it is necessary to derive the marginal effects as is described in Appendix 2C. Table 2.6 exemplarily displays the marginal effects of the explanatory variables on the low-pay transition probabilities for male workers in 1999. In line with the switching regression specification, the effects are reported separately for those who were initially low paid and for those initially high paid. For the former group, the effects are to be interpreted in terms of persistence effects, whereas for the latter group the marginal effects refer to the probability of descending into low pay. Marginal effects (in short ME) are to be interpreted as deviations from a reference person who has all dummies set to zero and is defined by setting the continuous covariates equal to their sample median values.[33]

The first two rows in Table 2.6 report the average transition probabilities - which represent the two components of the ASD derived in equation (2.7) - as well as the transition probabilities for the reference individual - which are referred to as the baseline probabilities. The baseline persistence probability of 0.589 is considerably larger than the average transition probability, whereas no difference is observed for entry probabilities. The ME estimates indicate that observable individual attributes significantly affect the probability of both staying and becoming low paid. While a better education reduces the probability of both staying and becoming low paid, other variables have opposite effects on the low-wage persistence and entry probabilities. The marginal effects confirm earlier results from Table 2.4, which suggest that the likelihood of descending into the low-wage sector is highest for the youngest workers below 26 years, whereas the likelihood of remaining low paid is the lowest for this subgroup. An individual who is between 41 and 45 years, for example, has a persistence (entry) probability which is 17.2

[33]A reference individual is a German between the age of 20 to 25 years who has a vocational degree, a white-collar occupation, who has had no regular employment relationship, and three previous employers prior to entry into the current establishment. Moreover, the reference worker has a median tenure of 2,040 days, has been non-employed for 90 days, and has not yet been unemployed.

Table 2.6: Marginal effects for male workers in 1999

	$L_t \vert L_{t-5} = 1$		$L_t \vert L_{t-5} = 0$	
	ME	Std. error	ME	Std. error
Average prediction	0.476		0.022	
Baseline	0.589		0.022	
Tenure	0.000	0.572	-.000	3.476
Tenure squared	-.000	0.001	0.000	0.004
Total non-employment duration	0.000	0.072	0.000	0.841
Total unemployment duration	0.000	0.092	0.000	5.608
Number of non-employment spells	-.004	19.967	0.003	754.08
Number of previous employers	0.008	39.628	0.001	188.82
Age 26 - 30	0.063***	0.000	-.009***	0.000
Age 31 - 35	0.099***	0.001	-.010***	0.000
Age 36 - 40	0.121***	0.001	-.011***	0.002
Age 41 - 45	0.172***	0.003	-.011***	0.000
Age 46 - 50	0.185***	0.003	-.008***	0.002
Age 51 - 55	0.231***	0.002	-.004	0.003
Being foreign	0.000	0.008	0.009	0.005
Medium-skilled	-.049***	0.009	-.008**	0.003
High-skilled	-.140***	0.011	-.021***	0.001
Previously full-time employed	0.018***	0.005	-.012**	0.004
Employment interruption (=1)	0.020***	0.005	-.001	0.003
Change of employer (=1)	0.005	0.012	0.005	0.003
Blue-collar worker (=1)	0.012*	0.005	-.041*	0.016
N (Individuals)	14,549		175,771	

Note: All specifications additionally include regional, sectoral, and occupational dummies.
Significance levels: ***: p<0.001; **: p<0.01; *: p<0.05.

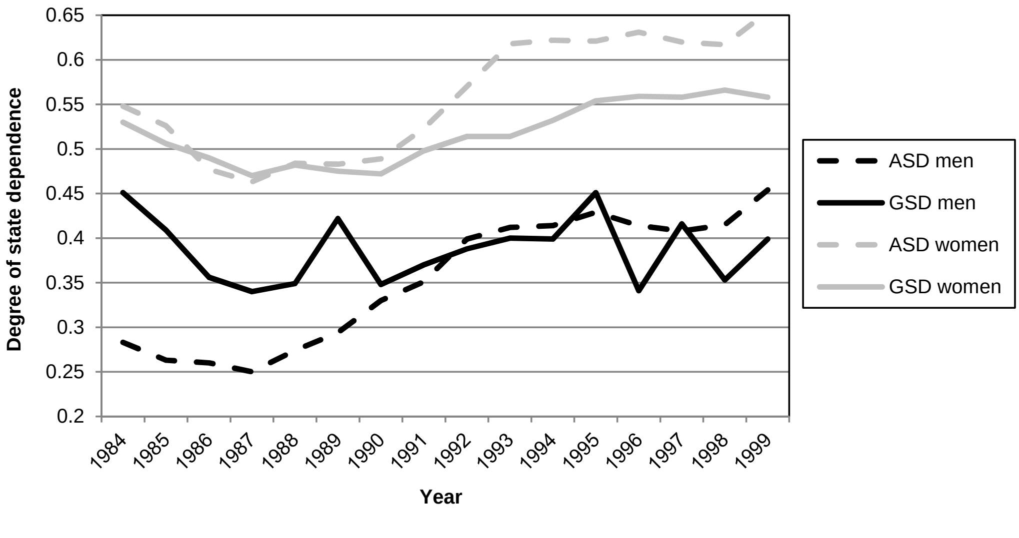
(1.1) percentage points higher (lower) than that of an individual aged between 20 and 25 years, ceteris paribus. The continuous employment history variables like tenure and total unemployment duration do not have any explanatory power.

Overall, the estimation results indicate that for both males and females age rather than the employment history variables drive the persistence probability. For example, whether an individual has a long or a short record of days in unemployment is not of major importance once an individual earns a low wage. This suggests that the extent of true state dependence is quite substantial.

2.3.3.3 The evolution of genuine state dependence (GSD)

After having estimated the transition equations, we next turn to the evolution of genuine state dependence (GSD) as given by equation (2.8). In Figure 2.6, we plot the estimated aggregate state dependence (ASD) and genuine state dependence (GSD) against time, separately for men and women. The estimated ASD values are nearly equal to the descriptive values that we showed earlier in Figure 2.3. Comparing the evolution of GSD and ASD, Figure 2.6 demonstrates that the measures are characterized by a quite divergent development. While the male ASD has steadily risen over time from 25% in 1987 to 45% in 2002, the GSD measure exhibits a stationary pattern by fluctuating at a rate around 40% over the whole observation period. Thus, once observable characteristics are controlled for, our findings argue against an upward trend in genuine state dependence for the overall male workforce.

Figure 2.6: Evolution of aggregate (ASD) and genuine state dependence (GSD)

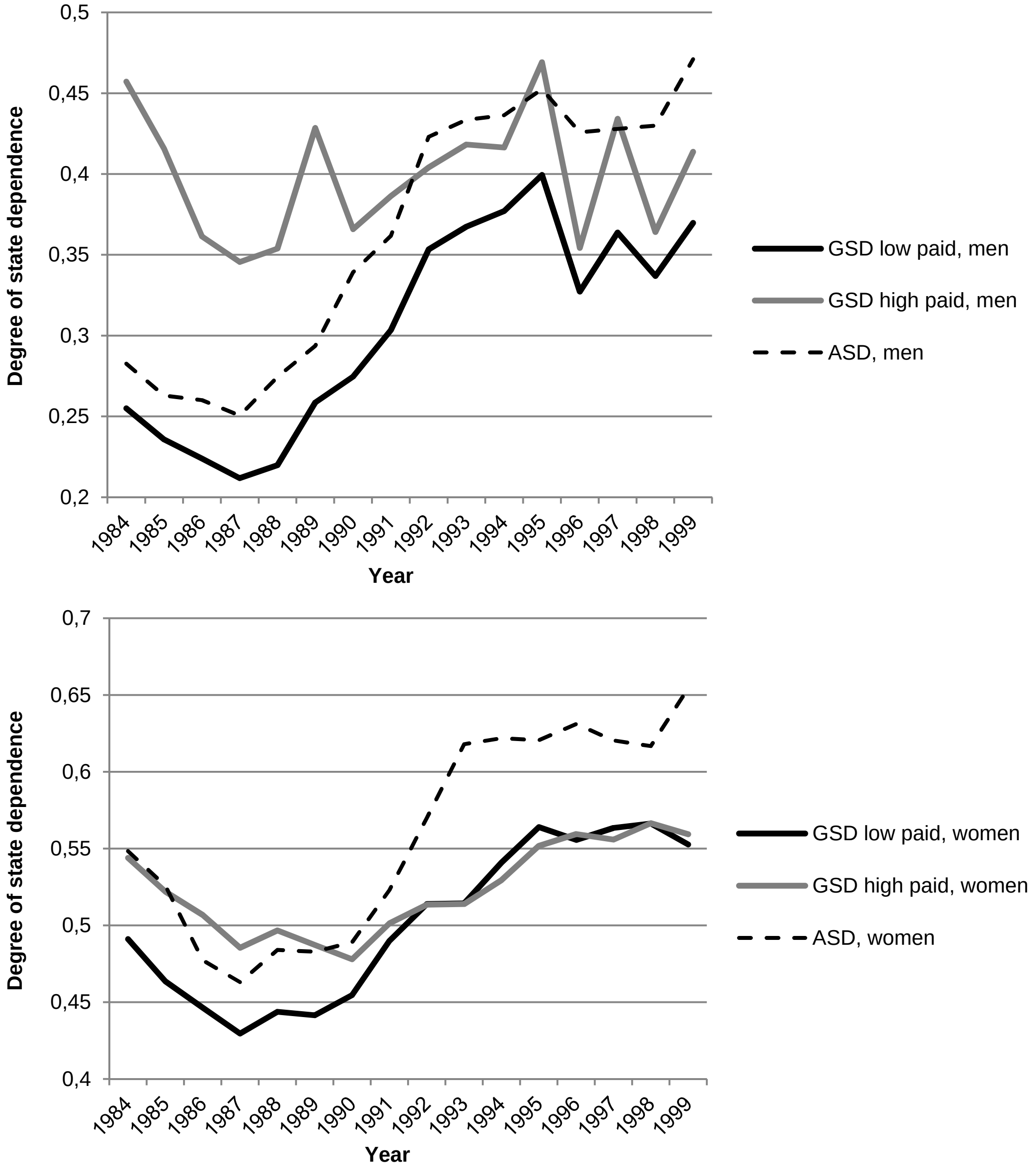


For women, in contrast, there seems to have been a slight increase in GSD between 1990 and 1999 from 47 to 56%. However, the growth of GSD has been distinctly lower than that of ASD which rose from 46 to 65% between 1987 and 2002. This result highlights the importance of accounting for changes in the composition of the low-wage relative to the high-wage labor work force and makes clear that the omission of such changing trends might lead to wrong inferences.

An important pattern that emerges from Figure 2.6 is that, especially for male workers, the extent of GSD has been substantially higher than the ASD during the first part of the observation period until 1991. This contrasts with earlier results from other studies (e.g. Cappellari and Jenkins 2004 and Cappellari 2007). Recall, however, that in our GSD measure we contrast for each individual - given his or her observed characteristics - the probability of entering the low-wage sector conditional on being initially high paid with the respective probability conditional on being initially low paid. One would typically expect that individuals working in the high-wage sector exhibit observed attributes that shelter them from low-wage persistence, even evaluated at the counterfactual persistence probability. However, our strong results with respect to the age structure's impact on transition probabilities lead to the opposite pattern. As individuals from the high-wage sector are on average four years older than those in the low-wage sector, their counterfactual persistence probabilities are considerably higher than their respective entry probabilities. This gives rise to a large GSD value, which even exceeds the (observed) ASD especially in the first years of our observation period when the difference in the average age between low and high-wage earners was considerably larger than in later years.[34]

[34]While male (female) high-wage earners were on average 10 (5) years older than low-wage

Figure 2.7: *GSD* by high-paid and low-paid individuals and ASD, men (upper part) and women (lower part), 1984 - 1999



We next turn to the evolution of *GSD* separately for the low-wage and high-wage sector. Figure 2.7 reveals that the level of *GSD* differed substantially across

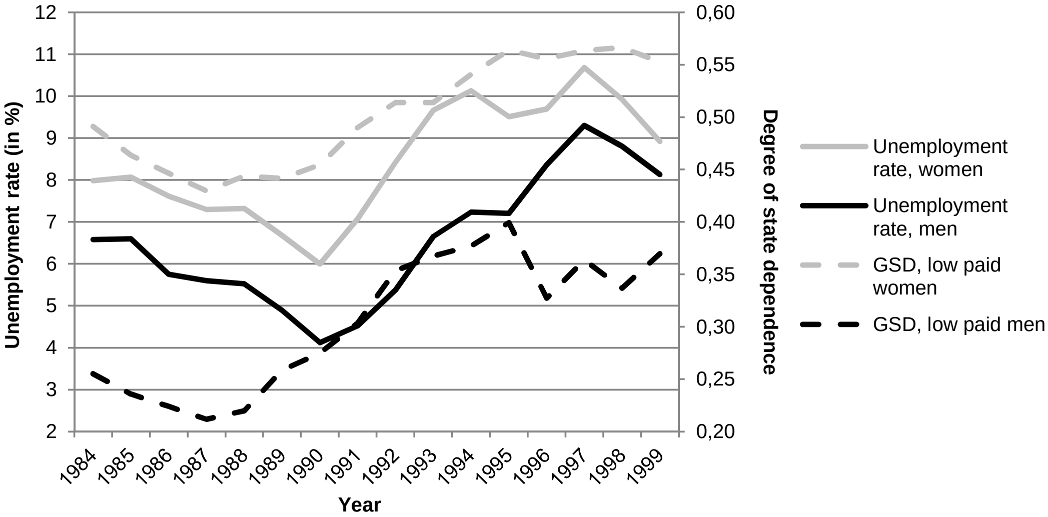
earners in 1984, this difference dropped to 4 (1) years in 2002.

low and high-paid workers during the 1980s, especially for men. In other words, had a high-paid male worker earned a low wage, his probability of staying in the low-wage sector would have been about 20 percentage points higher than for an actual low-paid worker. During the end of the 1980s, the GSD of high-paid and low-paid workers converged for both sexes mainly because the GSD of low-paid workers increased more sharply. Note that this reflects the compositional shift of the low-wage relative to the high-wage sector that was already established in Figure 2.6. Since 1992, the evolution of GSD among low-paid women has been very similar to that for high-paid women, whereas male low-paid workers in 1999 still faced a lower GSD level than their high-paid counterparts.

Overall, Figure 2.7 demonstrates that there has actually been an increase in GSD for both male and female low-paid workers. Thus, the evolution of GSD for the overall male workforce - as shown in Figure 2.6 - masked substantial heterogeneity across low and high paid workers as the majority of male workers earns a wage above the low-wage threshold. Comparing the evolution of GSD among low-wage workers with the evolution of ASD, we see, however, that the increase in ASD cannot fully be accounted for by the increase in GSD among the low-paid. The GSD/ASD ratio decreases from 0.90 in 1984 to 0.78 in 1999 for male low-paid workers while it ranges quite stationarily between 0.84 and 0.94 over the observation period for low-paid women.

A closer look at the evolution of GSD reveals that the increase during the 1990s for both high and low-paid workers was preceded by a decline in the mid 1980s. We, thus, observe some cyclical pattern which differs across men and women only at the end of the observation period. One possible explanation for this pattern could relate to the evolution of the unemployment rate as a proxy for business cycle effects. The argumentation works similar as in Chapter 1 when wage mobility over the entire wage distribution was considered. As a consequence of the wage procyclicality which is stronger for low-wage (and highest-wage) earners compared to median earners, the probability of ascending from a low to a high paid job might be higher in times of low unemployment. This would imply a positive correlation between the GSD of low-wage workers and the unemployment rate.

Figure 2.8: Evolution of GSD and the unemployment rate, 1984 - 1999



Although we are not able to make any causal inferences on the relationship between the extent of GSD and the unemployment rate, Figure 2.8 provides some support for the similarity of the evolution of both factors. For both men and women, there is evidence that the evolution of the unemployment rate in year t matches the evolution of low-wage workers' GSD (measuring the probability of sticking to the low-wage sector in $t + 5$). The low-paid women's decrease in GSD from 49 to 45% between 1984 and 1990 was accompanied by a decrease in the female unemployment rate, which fell from 8 to 6% during the same period. After

1990, the unemployment rate as well as the GSD of the low-paid workers rose distinctly. The positive association between the GSD and the unemployment rate is confirmed by a high correlation coefficient of 0.87, which is highly significant (p=0.000). A positive relationship is also observed for men for whom the correlation coefficient of 0.55 is significant at the 5% significance level (p=0.026). The results therefore indicate that a higher unemployment rate is associated with an increase in the probability of sticking to a low-wage job.

2.3.3.4 Decomposing the evolution of genuine state dependence

While the divergent development of ASD and GSD may be attributed to compositional shifts of the low relative to the high-wage sector, it does not permit us to infer any conclusions about the counterfactual evolution of GSD, had the low-wage workforce composition remained unchanged. Since we allowed GSD to vary across observable attributes, the evolution of GSD still reflects both changes in the workforce composition as well as changes in the differential impacts of the observed covariates on the transition probabilities. In order to disentangle these effects, we employ decomposition techniques by Oaxaca (1973) and Blinder (1973) adapted to our non-linear framework:

$$GSD_{\text{t}} - GSD_{\text{t-h}} = \underbrace{(GSD^{\text{t}}_{\text{t}} - GSD^{\text{t-h}}_{\text{t}})}_{coefficients\ effect} + \underbrace{(GSD^{\text{t-h}}_{\text{t}} - GSD^{\text{t-h}}_{\text{t-h}})}_{characteristics\ effect} \tag{2.10}$$

$$= \underbrace{(GSD^{\text{t}}_{\text{t}} - GSD^{\text{t}}_{\text{t-h}})}_{characteristics\ effect} + \underbrace{(GSD^{\text{t}}_{\text{t-h}} - GSD^{\text{t-h}}_{\text{t-h}})}_{coefficients\ effect}, \tag{2.11}$$

where t and $t-h$ are two different points in time with $t > t-h$ and $GSD^{\text{t}}_{\text{t}}$ and $GSD^{\text{t-h}}_{\text{t-h}}$ estimated as described in equation (2.8). While the superscript marks the changes in coefficients, the subscript reflects changes in characteristics. Due to the cyclical evolution of GSD over time, we apply the decomposition for two different time periods (1984 to 1989 and 1990 to 1999). The decompositions (2.10) and (2.11) differ with respect to the chosen counterfactual GSD. In equation (2.10), $GSD_{\mathrm{t}}^{\mathrm{t\text{-}h}}$ denotes how the GSD would have evolved in $t-h$ (i.e. 1984 or 1990), had the composition of the workforce remained constant at its level in t (i.e. 1989 or 1999). The term $GSD_{\mathrm{t\text{-}h}}^{\mathrm{t}}$ in equation (2.11) instead uses predictions for individuals in t (1984 or 1990) based on the coefficients in $t-h$ (1989 or 1999). The characteristics effect, thus, involves the part of the overall change between 1984 (1990) and 1989 (1999) which can be attributed to changes in observed characteristics of the individuals in the sample at given coefficients, whereas the coefficients effect captures the part which is due to changes in the coefficients at given characteristics.

Table 2.7: Decomposition of the GSD over time for the low-wage workforce, by sex

		Men		Women	
Base year		1984/1989 (1)	1990/1999 (2)	1984/1989 (3)	1990/1999 (4)
	Change in GSD	0.4	9.5	-5.0	9.8
A) 1989 / 1999	Coefficients effect	-2.1	4.4	-4.4	4.5
	Characteristics effect	2.5	5.1	-0.5	5.3
	Change in GSD	0.4	9.5	-5.0	9.8
B) 1984 / 1990	Coefficients effect	-1.5	6.2	-3.9	6.0
	Characteristics effect	1.9	3.3	-1.0	3.8

Note: All values in percentage points.

Table 2.7 reports the results of the decompositions for the low-wage sector for the two time periods separately for men and women. Panel A shows the results for the base year 1999 (1989) resulting from equation (2.10), Panel B for the base year 1990 (1984), cp. equation (2.11). Turning first to the time period 1984-1989, the decomposition for men in column (1) of Table 2.7 shows that the change in observables would have actually favored an even higher increase in GSD which has been overcompensated by the (negative) change in the coefficients. This result is valid independent of the chosen base year. For female workers, the change in coefficients was the driving force of the observed decline in GSD between 1984 and 1989, although part of the observed decline in their GSD is accounted for by the characteristics effect (column (3)). This result holds irrespective of the base year chosen. During the 1990s, GSD rose for both low-paid men and women (columns (2) and (4)). Irrespective of the base year chosen, the decompositions give very similar results across gender. While around 54% of the increase in GSD can be attributed to an unfavorable evolution of characteristics for both males and females with 1999 as base year (Panel A), the base year 1990 yields a contribution of the characteristics effect of 35 to 39% (Panel B).

To sum up, this section has shown that the extent of GSD slightly increased from 52 to 56% for women, whereas it fluctuated quite stationarily around 40% over time for men over our observation period. Differentiating between low and high-wage earners shows that especially the low-wage earners have experienced an increase in GSD catching up with the level of high-wage earners at the end of the observation period. The fluctuation of GSD over time - mainly a decrease at the end of the 1980s followed by an increase in the early 1990s - matches for both men and women the evolution of the unemployment rate. The decomposition of GSD over time into a characteristics and a coefficients effect reveals for male workers that the change in GSD during the 1980s can be mostly attributed to a compositional shift. For female workers, it is rather the change in coefficients that accounts for the decrease in GSD. During the 1990s, the characteristics effect has become more important in determining GSD for both sexes indicating a compositional shift towards more unfavorable characteristics among the low-paid.

2.4 Conclusions

The purpose of this chapter was to study how individual wage mobility in the low-wage sector has changed over the last two decades of the 20th century in West Germany. Using a large administrative data set, we first document that the low-wage sector has increased since the mid 1980s by around 14%. The overall growth of the low-wage sector was accompanied by a distinct rise in the probability of sticking to a low-wage job for both men and women. However, the extent of persistence, as measured by the extent of aggregate state dependence (ASD), varies greatly across different groups of individuals. Younger workers below 26 years, for example, face a much smaller risk of sticking to a low-wage job than the oldest age group (50-55 years). As the share of young workers among the low-paid has decreased to a much larger extent than the corresponding fraction in the high-wage sector, this compositional shift might have contributed to the observed rise in ASD over time.

In order to explore whether the observed decline in low-wage mobility is accounted for by compositional shifts of the low-wage relative to the high-wage sector or by an increase in "true" low-wage persistence, our analysis primarily seeks to infer conclusions about the evolution of "genuine" state dependence (GSD). Genuine state dependence arises when low-wage employment today causes low-wage employment in the future for reasons of stigmatization or human capital depreciation. We compute a measure of GSD by contrasting each individual's transition probability of staying in the low-wage sector conditional on being initially low paid with the probability of descending into the low-wage sector conditional on being initially high paid. The GSD is then calculated by averaging these differences over the sample of workers with earnings retention. Hence, to obtain such predicted probabilities, we need to model low-pay transitions depending on a variety of low and high-wage workers' observable characteristics. In order to address the initial conditions problem and the endogeneity of earnings attrition, we estimate a series of annual trivariate probit models that account for the selection into low-wage employment and earnings retention.

Based upon the estimated transitions, our results show that between 1984 and 1999 male workers' GSD - opposed to their increasing ASD - exhibits a quite stationary development at a rate of about 40% with some fluctuations over time. Concentrating only on those individuals in the low-wage sector, an increase in GSD can be observed which is, however, clearly less accentuated than the rise in ASD. For women, there seems to have been a slight increase in GSD during the 1990s from 47 to 56%, which is observed for both high and low-wage earners. However, the increase is clearly less pronounced than the rise in ASD. The observed fluctuation of GSD over time, mainly a decline in the late 1980s followed by an increase in the early 1990s, mirrors the evolution of the unemployment rate and indicates that a higher unemployment rate is associated with an increase in the probability of sticking to a low-wage job for both men and women.

As we allowed the GSD to vary across observables, the evolution of GSD still reflects both changes in the workforce composition as well as changes in price effects. We therefore use decomposition techniques to disentangle these effects for the low-wage workforce. The decomposition of GSD over time into a characteristics and a coefficients effect reveals for male workers that the change in GSD

during the 1980s can be mostly attributed to a change in characteristics. For female workers, it is rather the change in coefficients that accounts for the decrease in GSD. During the 1990s, the change in characteristics has become more important in determining GSD for both sexes. Depending on the base year chosen, we show that between 35 and 54% of the increase in genuine state dependence during the 1990s can be attributed to compositional shifts towards more unfavorable observable characteristics among the low-paid.

What do these results tell us about the ongoing discussion of decreasing wage mobility in the low-wage sector in Germany? Taken together, our analysis highlights the importance of accounting for possible compositional changes in the low-wage population. It also makes clear that the omission of such changing trends might lead to wrong inferences about the development of true low-wage persistence. In disentangling compositional shifts from changes in price effects, our findings show that - contrary to common perceptions - the decline in low-wage mobility cannot be fully explained by an increase in "true" state dependence, but also by changes in the relative composition of the low-wage relative to the high-wage sector. These "between" compositional effects are reinforced by "within" compositional effects, which give rise to a larger increase in GSD compared to its counterfactual evolution had the low-wage workforce composition remained unchanged. Our results therefore lend strong support to the notion that appropriate policy interventions should aim at working against such compositional shifts by, e.g. improving low-wage earners' skills and intensifying older low-paid employees' vocational training opportunities.

Appendix 2

2.A - Tables

Table 2.A1: Description of the variables used in the analysis

Variable	Definition
Age	Age (20-55 years) categorized in seven sub-groups
Tenure	Sum of all previous days of employment at current employer
Tenure squared	Square of sum of all previous days of employment
Nationality	Dummy=0 if German
Medium-skilled[1)]	Dummy=1 if completed vocational training but no university degree
High-skilled	Dummy=1 if university degree
Previous occupation status	Dummy=1 if previously employed, Dummy=0 if previously un-/non- or part-time employed or in vocational training
Total unemployment duration[2)]	Sum of all previous days of unemployment
Total non-employment duration	Sum of all previous days of non-employment
Number of unemployment spells	Sum of all previous unemployment spells
Number of non-employment spells	Sum of all previous non-employment spells
Number of previous employers	Sum of all previous employers
Previous employment interruption	Dummy=1 if recall from current employer
Number of empl. interruptions	Sum of all previous recalls at current employer
Change of employer	Dummy=1 if employer or employment status changes
Profession	Dummy=1 (=0) if blue-collar (white-collar) worker
6 occupation dummies	Either agrarian, salary, sale, clerical, service or production worker
16 sector dummies	Two-digit sectors (for categorization see Drews 2008)
11 regional dummies	The 10 Western German states plus West-Berlin (until 1990)
Low wage in t (1989-2004)	Dummy=1 if gross daily wage in t <2/3 of the median wage[3)]
Low wage in t-5 (1984-1999)	Dummy=1 if gross daily wage in t-5 <2/3 of the median wage
Retention in t	Dummy=1 if full-time employment status is observed in t-5 and t

[1)] To improve the education variable, we use the imputation rules derived by Fitzenberger et al. (2006).

[2)] Following the procedure proposed by Lee and Wilke (2009), involuntary unemployment is defined as comprising all continuous periods of transfer receipt. Gaps between periods of transfer receipt or gaps between transfer receipt and a new employment spell may not exceed four weeks, otherwise these periods are considered as non-employment spells (involving voluntary unemployment or an exit out of the social security labor force). Similarly, gaps between periods of employment and transfer receipt are treated as involuntary unemployment as long as the gap does not exceed six weeks, otherwise the gap is treated as non-employment.

[3)] Due to the introduction of the Euro in 1999, all wages before 1999 are transformed from Deutschmark into Euros at a rate of 1€ = 1.95583 Deutschmark. Since the wage variable delivers unrealistic daily wages at the lower end of the wage distribution, we exclude all observations with earnings of less than 16€ per day (in prices of 1995).

Table 2.A2: Mean values of the characteristics used in the estimation, by wage sector

	High wage		Low wage	
	Mean	Std. Dev.	Mean	Std. Dev.
Females (share in %)	0.28	0.45	0.72	0.45
Age (in years)	37.50	9.58	33.81	10.48
Low-skilled (share in %)	0.13	0.34	0.25	0.43
Medium-skilled (share in %)	0.77	0.42	0.73	0.44
High-skilled (share in %)	0.10	0.30	0.02	0.12
Tenure (in days)	2,797	2,201	1,571	1,646
Being foreign (share in %)	0.08	0.28	0.12	0.33
Blue-collar worker (share in %)	0.48	0.50	0.54	0.50
Previously full-time employed (share in %)	0.42	0.49	0.23	0.42
Previous employment interruption (=1)	0.22	0.41	0.18	0.39
Number of employment interruptions	0.31	0.96	0.29	1.05
Change of employer (share in %)	0.10	0.30	0.21	0.41
Number of previous employers	3.11	2.59	3.44	3.11
Previous unemployment duration (in days)	102	258	163	352
Number of unemployment spells	0.65	1.56	0.86	1.66
Previous non-employment duration (in days)	368	809	727	1,297
Number of non-employment spells	0.96	1.72	1.50	2.46
Retention after five years (share in %)	0.78	0.42	0.56	0.50
Low paid five years ago (share in %)	0.02	0.16	0.55	0.50
Observations	4,866,868		766,533	

Table 2.A3: Median gross daily wage and low-wage threshold in daily and hourly wages of full-time employed men, in €, by year, 1984 - 2002

	(1) Median gross wage per working day	(2) Low-wage threshold in daily wages	(3) Low-wage threshold in hourly wages
1984	70.87	47.24	5.98
1985	73.01	48.68	6.16
1986	75.88	50.58	6.40
1987	78.02	52.02	6.58
1988	80.17	53.45	6.77
1989	82.32	54.88	6.95
1990	86.61	57.74	7.31
1991	92.34	61.56	7.79
1992	96.64	64.42	8.15
1993	100.21	66.81	8.46
1994	102.36	68.24	8.64
1995	105.94	70.63	8.94
1996	107.37	71.58	9.06
1997	108.80	72.54	9.18
1998	110.95	73.97	9.36
1999	112.00	74.67	9.45
2000	114.80	76.53	9.69
2001	116.20	77.47	9.81
2002	119.00	79.33	10.04

Note: The BA data provides gross wages per calendar day. The median gross wage per working day in column (1) is approximated by multiplying the median gross daily wages in the data by 7/5.
The low-wage threshold displayed in column (2) is column (1) multiplied by 2/3.
The hourly wage per working day is approximated by dividing column (2) by 7.9, the average hour worked by a full-time worker.

Table 2.A4: Share of selected characteristics in % among the total low-wage population

	Males		Females	
	1984	2002	1984	2002
Age 20-25	52	24	40	19
Age 51-55	5	8	7	11
Low-skilled	27	33	29	21
Medium-skilled	71	65	70	77
High-skilled	2	2	1	3
Being foreign	14	20	8	9
Blue-collar worker	77	82	47	41
Previously full-time employed	16	25	20	25
Observations	7,760	15,170	28,858	27,053

2.B - Figures

Figure 2.B1: Mean share of low-wage earners by age group and sex, 1984 - 2002

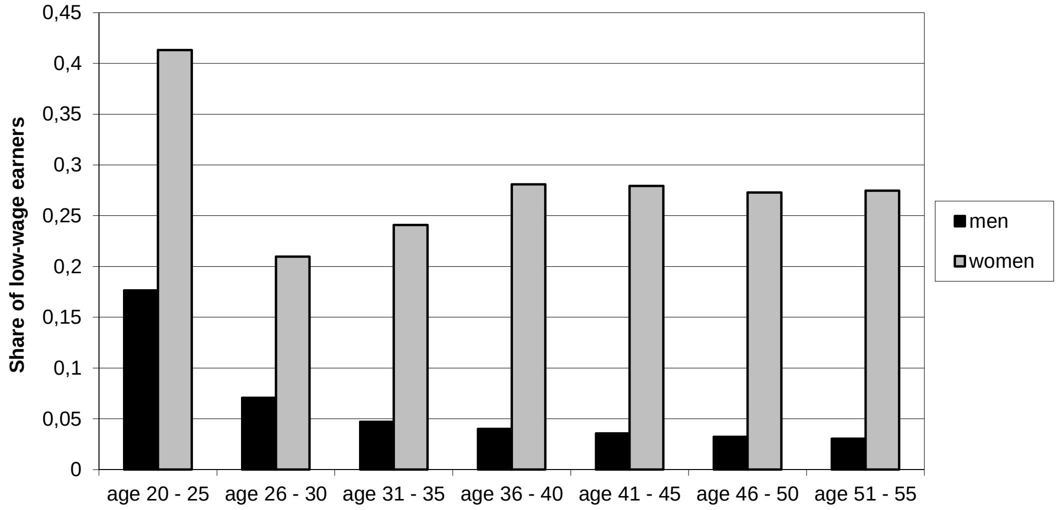


Figure 2.B2: Aggregate state dependence by tenure group, 1984 - 1999

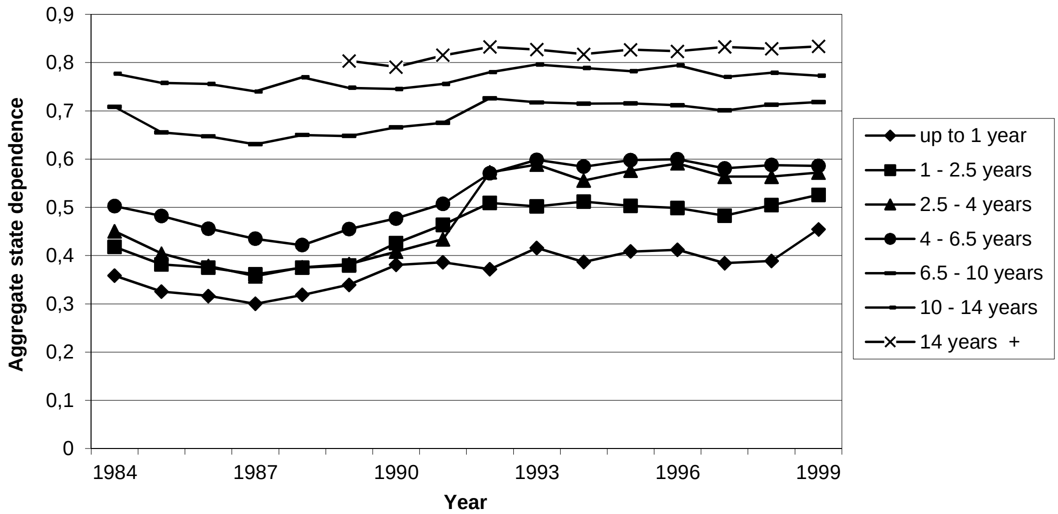


Figure 2.B3: Mean share of low-wage earners by tenure group and sex, 1984 - 2002

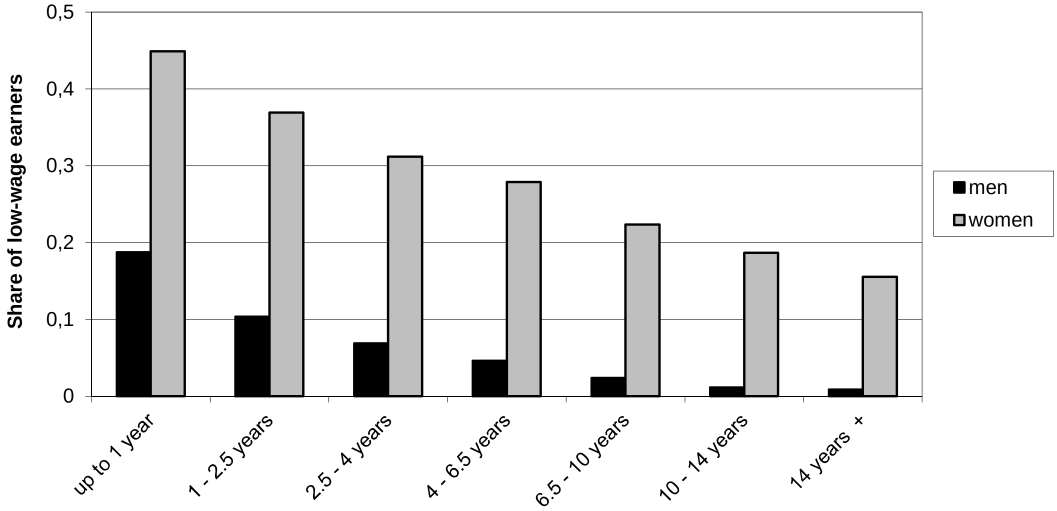


Figure 2.B4: Aggregate state dependence by degree and sex, total working population, 1984 - 1999

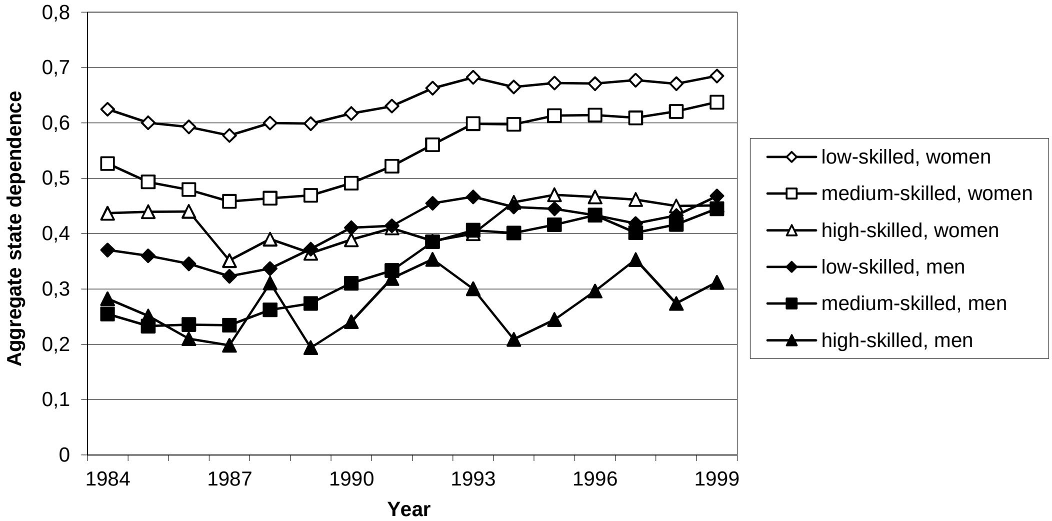


Figure 2.B5: Mean share of low-wage earners by degree and sex, 1984 - 2002

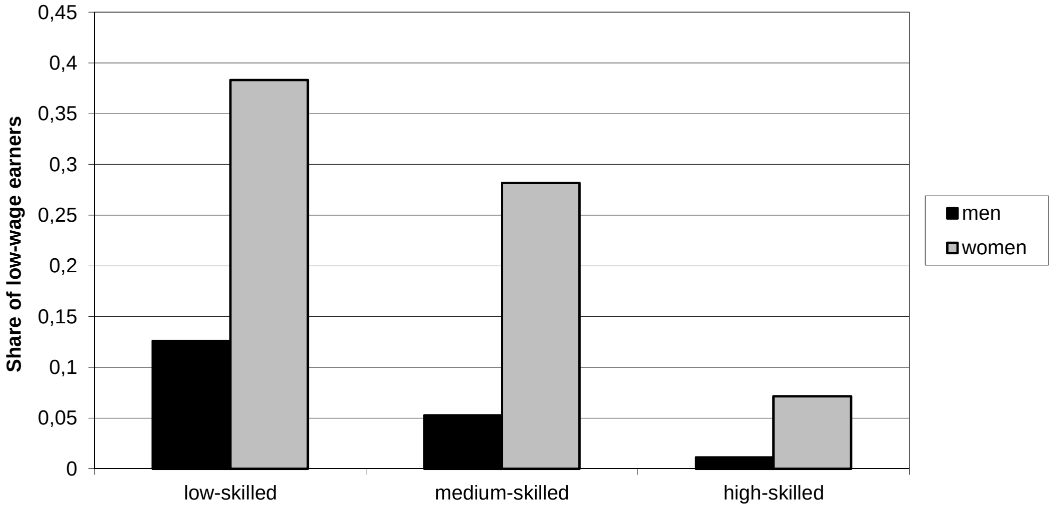


Figure 2.B6: Aggregate state dependence by nationality and sex, total working population, 1984 - 1999

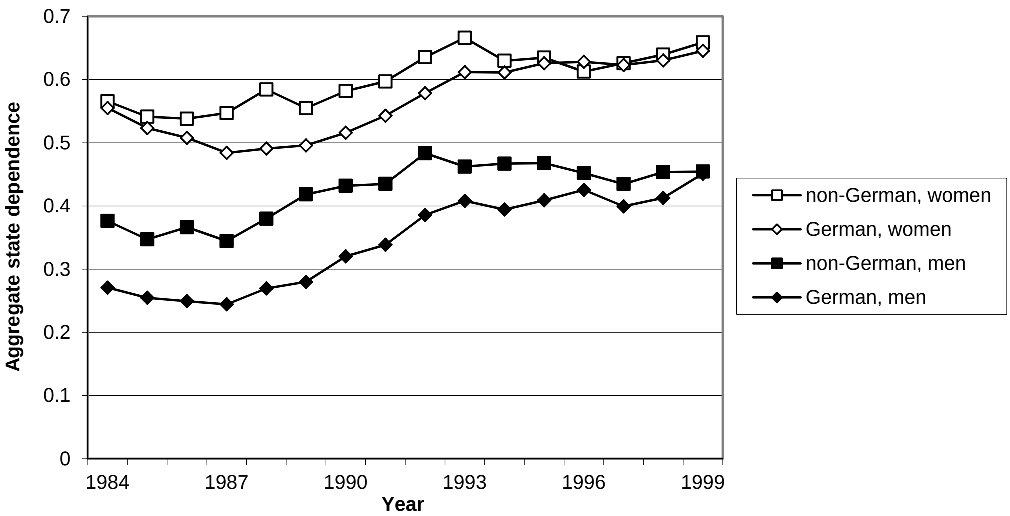


Figure 2.B7: Aggregate state dependence by occupation and sex, total working population, 1984 - 1999

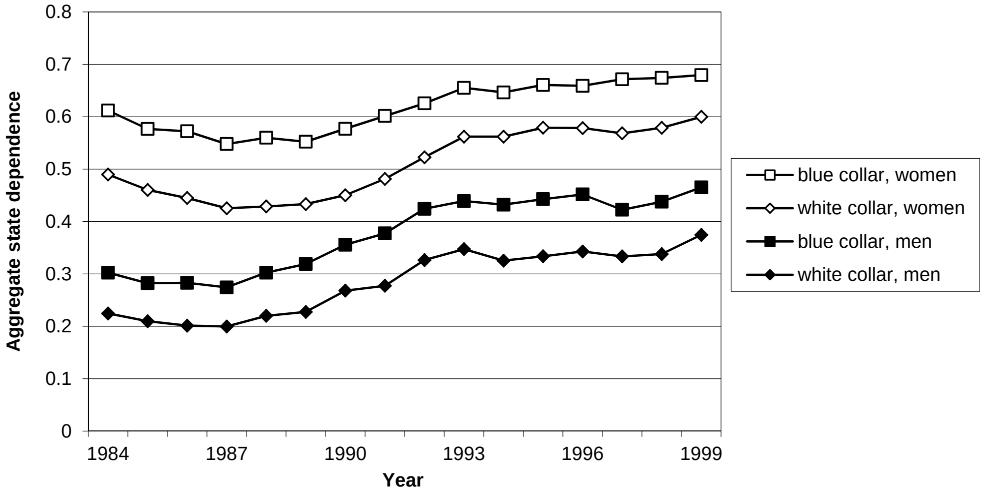
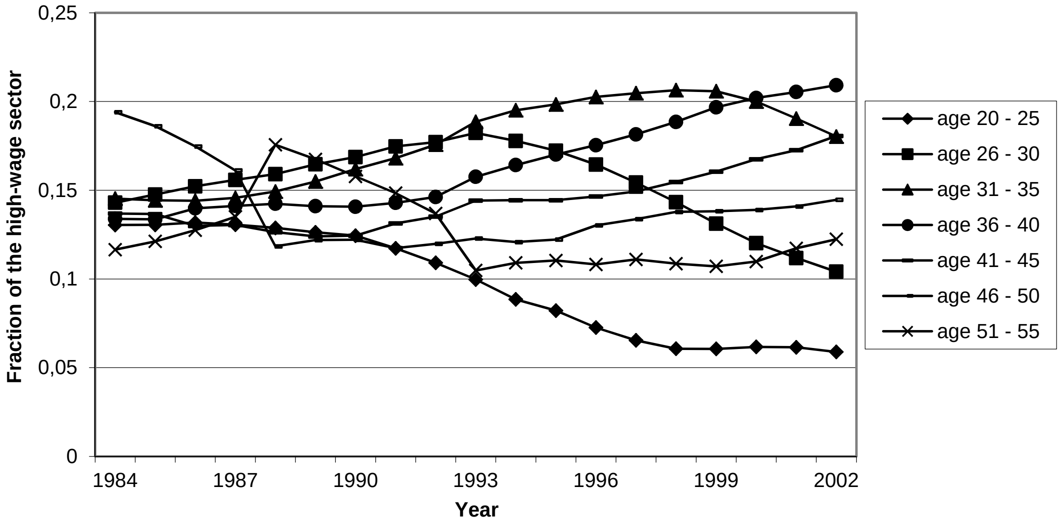


Figure 2.B8: Evolution of the age composition in the high-wage sector, 1984 - 2002



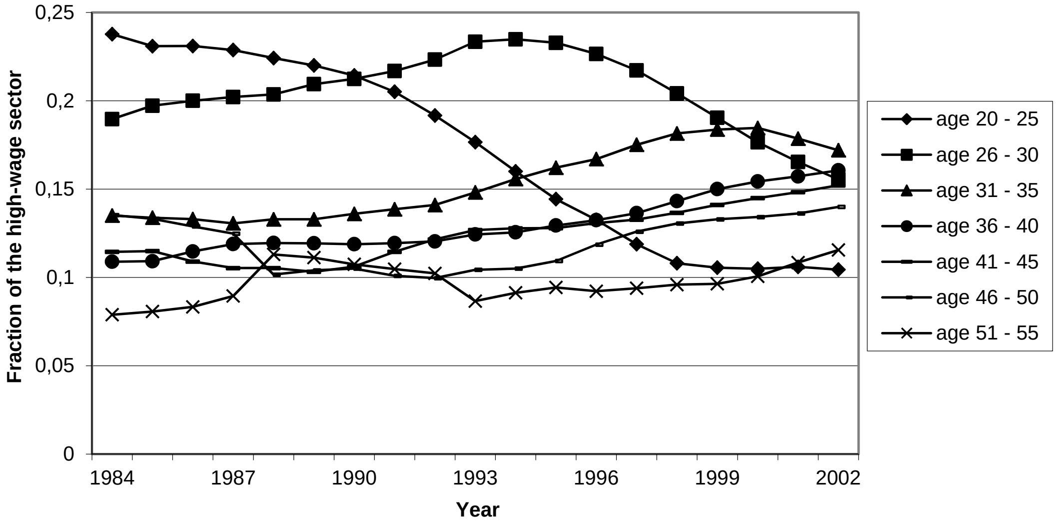


Figure 2.B9: Evolution of the composition of sex in the low and high-wage sector, 1984 - 2002

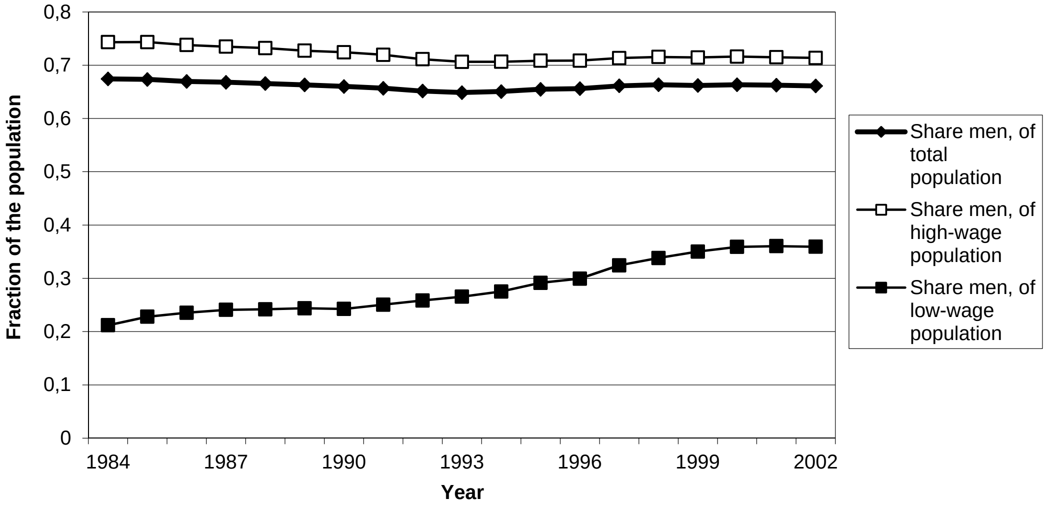


Figure 2.B10: Evolution of the skill composition in the low and high-wage sector, 1984 - 2002, women

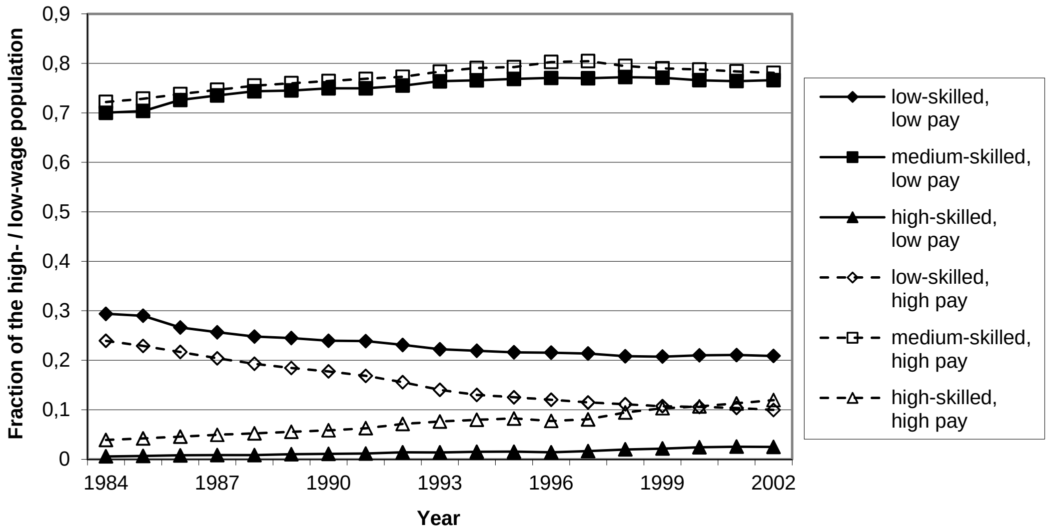


Figure 2.B11: Evolution of the skill composition in the low and high-wage sector, 1984 - 2002, men

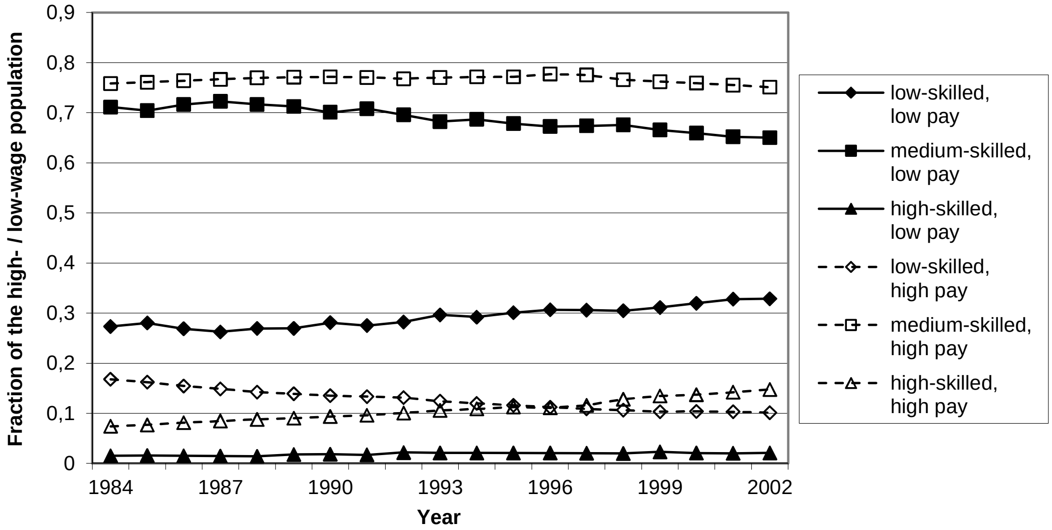


Figure 2.B12: Evolution of the low and high-wage sector by collar and sex, 1984 - 2002

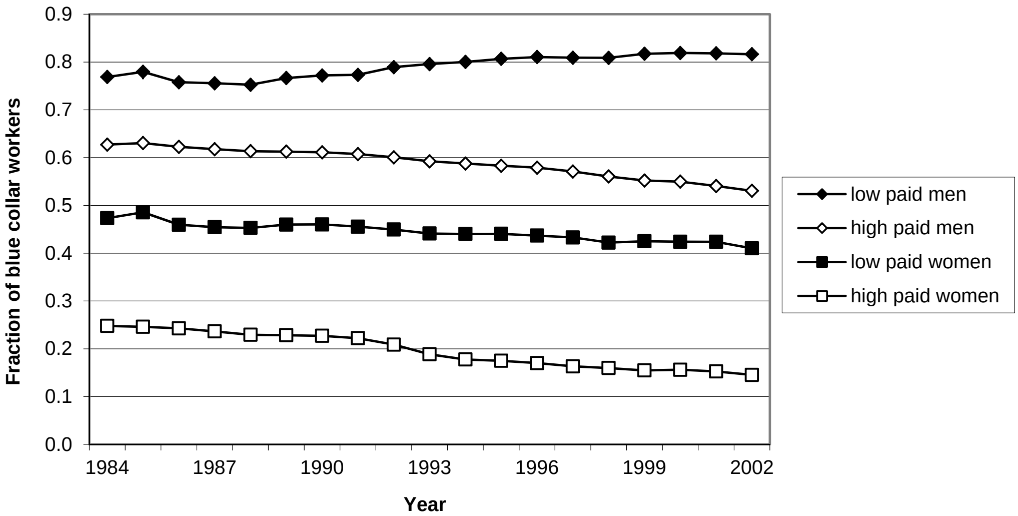
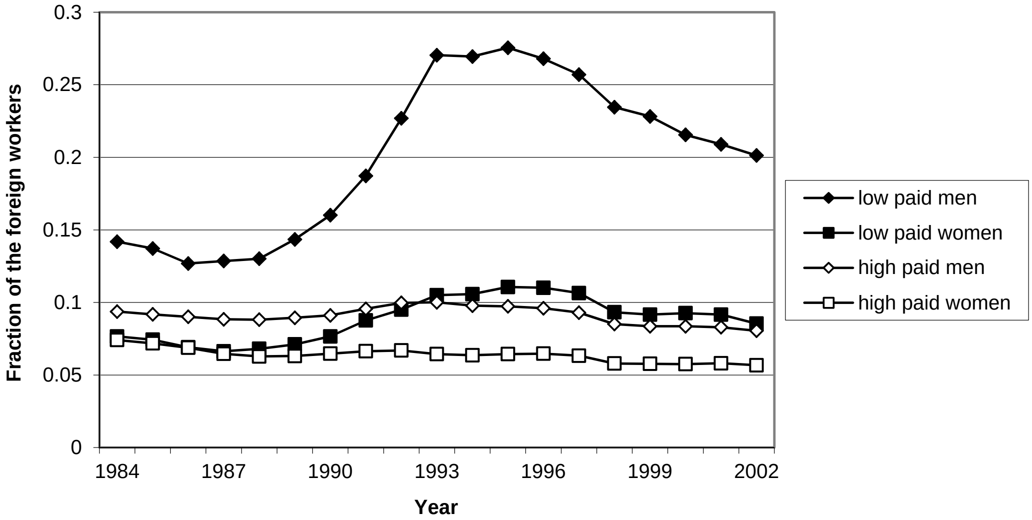


Figure 2.B13: Evolution of the low and high-wage sector by nationality and sex, 1984 - 2002



2.C - Derivation of the marginal effect

In order to be able to interpret the estimation results, we report the ME showing the impact on the relevant probabilities of a change in the chosen covariate. For a dummy variable, the ME is calculated as a change in the probability resulting from a change in the indicator's value from zero to one, holding all other covariates fixed at their sample median values. ME for continuous variables are usually estimated by evaluating the partial derivative, which is equal to the corresponding coefficient multiplied by an evaluation of the normal density function. However, the computation is not straightforward here because the transition probabilities are conditional in nature (e.g. the probability of low pay in t conditional of being low paid in $t-5$). To clarify this point, the conditional probabilities are given by:

$$e_{it} \equiv \left[\Pr\left(L_{it}=1|L_{it-5}=1\right)\right] = \left[\Phi_2\left(z_{it-5}\widehat{\gamma}_1, x_{it-5}\widehat{\beta}; \rho_2\right)\Phi\left(x_{it-5}\widehat{\beta}\right)\right] \quad (2.12)$$

and

$$f_{it} \equiv \left[\Pr\left(L_{it}=1|L_{it-5}=0\right)\right] = \Phi_2\left(z_{it-5}\widehat{\gamma}_2, -x_{it-5}\widehat{\beta}; -\rho_2\right)\Phi\left(-x_{it-5}\widehat{\beta}\right)\Big] \quad (2.13)$$

As is evident from equations (2.12) and (2.13), a change in the value of a covariate may affect both the numerator and denominator of the conditional probabilities. In order to deal with this issue, we adopt the procedure suggested by Stewart and Swaffield (1999) (see also Cappellari (2007) and Cappellari and Jenkins (2008)) by keeping the elements of x_{it-5} fixed. To do so, we first predict the low-pay probability in $t-5$ for all low-paid individuals and take the average over these values - denoted as q. By inserting $w = \Phi^{-1}(q)$ into equation (2.12), we obtain

$\Phi_2\left(z_{it-5}\widehat{\gamma}_1, w; \rho_2\right)/q$. This expression is used to calculate ME as deviations between the conditional probabilities for a reference person and hypothetical probabilities induced by changing each covariate by one unit. For the reference person, we set continuous covariates to the sample median values and dummy variables to zero. The same procedure is applied to f_{it}.

Chapter 3

The Minimum Wage Affects Them All: Evidence on Employment Spillovers in the Roofing Sector*

3.1 Introduction

Most minimum wage research focusses on the average employment effect that minimum wages exert on workers with a binding minimum wage, i.e. workers whose wage has to be raised in order to comply with the minimum wage level. In a competitive labour market with a heterogenous workforce and an elastic product demand, for example, workers for whom the minimum wage raises labour costs are expected to experience negative employment outcomes (Brown 1999)[35]. However, depending on the production technology, the minimum wage (in short MW) may also affect workers for whom the minimum wage is not binding, see e.g. (Neumark

[34]*This contribution is joint work with Melanie Arntz and Terry Gregory and has been published as ZEW Discussion Paper No. 12-061 (Aretz et al. 2012a). We thank for financial support through the grant "Minimum wage effects in the roofing sector" by the ZEW Sponsors' Association for Science and Practice. We would further like to thank Stephan Dlugosz and the participants of the "Workshop on Minimum Wage Research" in Mannheim for fruitful discussions. The project also profitted from a preceding evaluation of minimum wage effects in the German roofing sector that was financed by the German Federal Ministry of Labour and Social Affairs (BMAS). The authors are responsible for all results and conclusions derived in this study. They do not necessarily reflect the views of the BMAS.

[35]In case of a monopsonistic labour market that allows employers to set wages below the equilibrium wage, a minimum wage may instead induce positive or zero employment effects.

and Wascher 2008). If workers with and without a binding minimum wage are complements, a negative scale effect that results from a reduced product demand negatively affects all workers' employment chances. If the two types of workers are substitutes, the MW may raise the demand for workers who earn a wage above the MW, thereby counteracting the negative scale effect by a positive substitution effect. In this case, we may observe negative employment effects for workers with a binding MW and even positive employment effects for workers with a non-binding MW. Moreover, profit-maximising firms may potentially substitute capital for the relatively more expensive labour input, thereby inducing an additional employment decline for all workers who are substitutable by capital. In this latter case, a firm might, in fact, lay off the poorest performers of each type of worker and reduce employment also among workers with a non-binding MW.

The existing literature mainly discusses employment spillovers, i.e. indirect employment effects for workers with a non-binding minimum wage, as a potential source of bias. Linneman (1982), Currie and Fallick (1996), Abowd et al. (2000), and Neumark et al. (2000), e.g., identify the average employment effect on workers with a binding MW by comparing workers with and without a binding MW. Attempts to estimate substitution effects between workers tend to focus on the elasticity of substitution between skill or age groups rather than between workers with and without a binding MW.[36] The only study that we are aware of that focuses on employment effects along the wage distribution is by Neumark et al. (2004). They report evidence for a negative employment spillover for workers with a wage just above the minimum wage level.

[36]See e.g. Neumark and Wascher (1995) for the substitution between age groups, Abowd and Kilingsworth (1981), and Neumark and Wascher (1994) for the substitution between skill groups, and Hsing (2000) for substitution between part-time and full-time work.

The aim of this chapter is to make a contribution on the sparse literature on employment spillovers by investigating employment effects along the entire wage distribution. In particular, our contribution is fourfold. First of all, we are able to analyse employment effects in a context where the minimum wage bites very hard: the roofing sector in Germany. Its minimum wage was introduced in 1997 and was subsequently raised several times. With a Kaitz Index, i.e. the ratio of the minimum wage level and the median wage, that is around 1 in East Germany, the bite has to be considered exceptional even by international standards (Machin et al. 2003, Dolton and Bondibene 2011). The German roofing sector, thus, is an ideal setting to study employment effects along the entire wage distribution since its bite is likely to render indirect employment effects for workers above the minimum wage.

Secondly, we are able to exploit a natural experiment since, for institutional reasons, the minimum wage was introduced only in parts of the construction sector including the roofing sector. Uncovered, yet comparable, subsectors may thus serve as a benchmark for the counterfactual development in the roofing sector in order to derive the average treatment effect on the treated (ATT) with respect to the chances of remaining employed in the roofing sector. Since the entire construction sector experienced a dramatic decline in demand after the end of the unification boom in the mid 1990s that almost halved the workforce in East Germany, this is a highly relevant employment outcome.

Thirdly, we contrast the ATT from an intersectoral comparison with an ATT derived from a comparison of workers with and without a binding MW within the roofing sector. Under a number of identifying assumptions, a deviation between these ATTs may hint at employment spillovers within the roofing sector. In order to make such spillovers visible, we then combine both identification strategies. For this purpose, we estimate the counterfactual wage that workers of the control sector would receive in the roofing sector given their characteristics. This enables a comparison of workers with and without a binding MW across sectors and also allows for estimating the employment effects along the entire wage distribution.

Finally, we make use of two administrative linked-employer-employee panels one of which contains the full sample of workers in the roofing sector over the observation period of interest. Hence, we are able to take account of unobserved heterogeneity at the individual level, which may be relevant if employers mainly substitute workers along unobservable skills as is suggested by Fairris and Bujanda (2008). Our contribution, thus, yields much broader insights into the employment effects of minimum wages than most previous studies.

The findings indicate that the chances to remain employed in the roofing sector have deteriorated due to the minimum wage introduction, especially in East Germany where the bite of the MW was particularly hard. However, the impact suggested by comparing workers with and without a binding MW appears to be underestimated compared to the intersectoral comparison, thus hinting at employment spillovers of the MW on workers earning above the MW level. An intersectoral comparison suggests negative employment outcomes for east German workers along the entire wage distribution. According to personal interviews with sector insiders, capital-labour substitution rather than scale effects drive this finding. Our results highlight the need for a broader perspective on the employment impact of minimum wages and also put doubts on any attempt to identify employment effects of minimum wages by comparing workers with and without a binding MW within a covered sector.

This chapter is structured as follows. Section 3.2 contains information on the German roofing sector, the introduction of the minimum wage and discusses some expectations for the empirical estimations given its market structure. Section 3.3 describes the data basis before Section 3.4 discusses the bite of the minimum wage. Section 3.5 describes the general difference-in-differences estimation framework for the identification of employment effects that is applied to different treatment and control groups in Sections 3.6 and 3.7. Section 3.8 concludes.

3.2 The German Roofing Sector

Market structure. The goods and services that are provided by the roofing sector encompass the roofing of new buildings as well as the mending of old roofs. Roofing is a traditional craft in Germany requiring a master craftsman's diploma in order to start a business.[37] These traditional roofing companies usually employ less than ten employees and provide their services regionally and mainly to private home owners whose demand may be rather inelastic given the few available and mainly illegal substitutes such as moonlighting. In a survey among 250 roofing companies in 2011, more than three quarters of all companies considered quality rather than prices to be the main dimension of competition (Aretz et al. 2011 and Aretz et al. 2012b). For those companies with more than 30 employees, which constitute less than 10% of all roofing companies, however, price competition may be more relevant since they tend to work for public contractors and are active beyond regional boundaries.[38]

[37]As an exception, it is not required to hold such a diploma if someone works as an itinerant worker. Such workers tend to work alone and mainly provide mending services only.

[38]Information on company size is based on the BA data (see Section 3.3 for details).

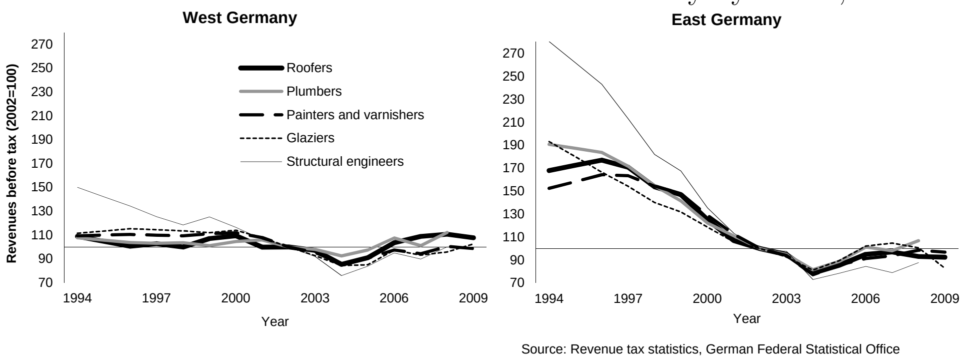
Moreover, in contrast to most sectors that have been studied extensively in the MW literature, the roofing sector has a rather high level of qualification and is not very labour intensive. More than 95% of all workers work fulltime, and a relatively high share of around three quarters has at least a vocational training degree.[39] Moreover, labour costs account for less than 40% of total costs only (Cost Structure Survey 2001), and technical advances regarding materials and roofing techniques appear to be quite important as reported by roofing companies in a number of qualitative interviews.[40]

Business cycle. The entire construction sector experienced a boom period in the early 1990s due to German reunification, but began to shrink from the mid 1990s on, see Figure 3.1. In East Germany, this post-unification downturn was much more dramatic than in West Germany and reduced the construction sector's revenues in the subsequent years by more than half. After 2004, all construction sectors reinstalled revenue levels in West Germany similar to the early 1990s, while the recovery in East Germany was rather marginal. Compared with structural engineering, the roofing sector and other sub-construction sectors such as plumbing, glazing and painting services experienced a less dramatic decline in the demand for their services in the mid 1990s and a faster recovery after 2004. The demand for sub-construction work hinges on the demand for new buildings as well as the age structure of the existing stock of houses with the latter apparently having a smoothing impact on the business cycle compared to structural engineering.

[39]The part-time information is taken from the Cost Structure Survey for 2001 (Kostenstrukturerhebung), which is released by the German Statistical Office. The share of qualified workers is calculated based on the BA data (see Section 3.3 for details).

[40]Ten qualitative interviews with roofing companies and four additional interviews with representatives of the trade union and the employer's association were conducted within a report prepared for the Federal Ministry of Labour and Social Affairs, see Aretz et al. (2011) for details.

Figure 3.1: Overall revenues in West and East Germany by sector, 1994 - 2009



Moreover, sub-construction sectors broadened their portfolio during the last years, thereby stabilising the demand for their services. In particular, roofing companies are increasingly involved in the assembling of photovoltaic cells as well as the ex post insulation of old roofs.[41] The plumbers and, to a lesser extent, glaziers and painters also benefited from this development. At least in West Germany, this has presumably contributed to a faster recovery in the roofing and the plumbing sector compared to the other sub-construction sectors and structural engineering.

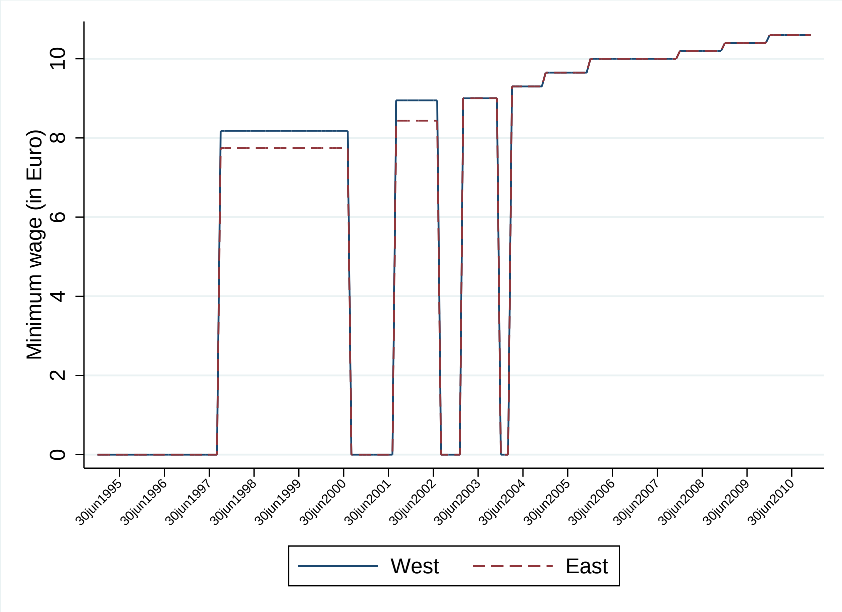
Minimum wage regulations. Apart from shrinking demand, additional pressures in the mid 1990s stemmed from the introduction of a free movement of labour that allowed Eastern European firms to send workers to German construction sites while paying home country wages. In order to protect German workers, legally binding minimum wages that had to be paid to all workers on German construction sites irrespective of the origin of their contract were introduced in the structural engineering and some sub-construction sectors. Since minimum wages

[41] Both of these developments have been boosted by government initiatives for subsidising solar energy generation since 2000 (*Erneuerbare-Energien-Gesetz*) and energy-saving renovations since 2002 (*Energetische Gebäudesanierung*).

are negotiated separately for certain sub-divisions of the construction sector, not all sub-divisions agreed on minimum wage regulation, resulting in a coexistence of quite comparable sectors with a legally binding minimum wage (e.g. structural engineering and roofing sector since 1997; painting sector since 2003) and sectors such as glazing and plumbing services that are not subject to a legally binding minimum wage until now. Hence, these sectors may potentially serve as a benchmark for the counterfactual development in the roofing sector in the absence of a legally binding minimum wage.

The minimum wage in the roofing sector applies to all blue-collar workers of any roofing company or roofing branch within a larger company who are at least 18 years of age, who are not an apprentice and who are not working as a custodial worker. Thus, all white-collar workers such as office clerks as well as certain parts of the blue-collar workforce are exempted from the minimum wage regulation. Introduced in October 1997, the minimum wage was subsequently raised several times, but was also interrupted by short periods without any legally binding minimum wages, see Figure 3.2. These interruptions reflect the fact that the minimum wage is negotiated between the responsible trade union (*IG Bau*) and the association of employers in the roofing sector (*Zentralverband des Deutschen Dachdeckerhandwerks*) as a part of the general collective bargaining agreement. When these agreements expire, there may be short interruptions before a new agreement is reached. Because the continuation of a minimum wage was not subject to any debate since its introduction, roofing companies could, however, expect a new minimum wage agreement, rendering any behavioural adjustments during these interruptions very unlikely. Moreover, minimum wages were harmonised between West and East Germany in 2003 despite wages in West Germany exceeding wages in East Germany by about 25%. This results in an extremely hard bite of the minimum wage in East Germany as we will see in Section 3.4.

Figure 3.2: Minimum wage level in the German roofing sector by region, 1995-2010



Taking all this evidence together, the roofing sector's market structure suggests a rather limited impact of minimum wages on employment given its limited labour intensity, the ability of roofing companies to at least absorb some of additional costs by raising prices and the fact that technical advances and increases in productivity offer options for cushioning rising labour costs. At the same time, however, the lower wage floor was fixed on a rather high level (see also section 3.4), particularly in East Germany, thus rendering employment effects likely. Moreover, changes in relative input prices may create incentives for substituting labour by capital and/or less skilled by skilled workers. Finally, the minimum wage in the roofing sector was introduced during a period of economic downturn and a shrinking market size. This strongly reduced the sector's workforce (see Figure 3.B1), although the number of companies even slightly increased at the same time as the share of single-person companies jumped from 8% in 1995 to 23% in 2010. With the number of unemployed workers with sector-specific human capital queuing for jobs on a rise, the bargaining power of those still working in the sector may have come under pressure.

3.3 Administrative Linked Employer-Employee Data

For our analysis, we are able to exploit two administrative linked employer-employee panel data sets: i) data that is collected by the central pay office of the roofing sector (*Lohnausgleichskasse, LAK*), in short the LAK data, and ii) data that is collected by the Federal Labour Agency (*Bundesagentur für Arbeit, BA*) for all employees that are subject to social insurance contributions, in short the BA data.

3.3.1 LAK data

In order to balance out the seasonal fluctuation of the sector, all roofing companies have to pay an insurance premium to the LAK that is related to the total payroll of their blue-collar workers. Therefore, they are obliged to give a monthly record to the central pay office of the roofing sector. For our analysis, we have access to the full sample of blue-collar workers on a monthly basis for the years 1995 to 2010, thus covering both the pre- and post-minimum wage period. Information on monthly working hours and monthly gross wage allows for calculating the hourly gross wage. Between October and April, however, reported working hours need not match the true working hours because of special regulations for cushioning the seasonal character of the sector's activities. Hence, we use the June information for the analysis based on the LAK data in order to avoid such distortions and to ensure the comparability of the analysis with the BA data (see below).

The LAK data contains additional information only on sex and age of the workers. Since we do not know whether someone is an apprentice or working as a custodial worker, who are both exempted from the minimum wage regulation, we are not able to exactly identify all covered workers. Since most custodial workers are female, however, and the share of females among covered roofers is less than 2% according to the BA data, we exclude women from the LAK sample. We also exclude all workers below the age of 19 and assume that this also eliminates most uncovered apprentices. Our sample, thus, differs from the exact coverage by missing some covered women and including some uncovered apprentices in the sample. Overall, we observe a total of 1,094,609 observations between 1995 and 2010 that stem from 217,779 individuals in 22,879 firms. Note that we are able to calculate some firm level information such as average gross pay, average firm size and average age of the company's workforce that we can use in addition to the individual information.

3.3.2 BA data

A major disadvantage of the LAK data is that it is only available for the roofing sector, thus precluding any identification strategy that rests upon inter-sector comparisons. Such an alternative identification strategy, however, becomes available based on the BA data since it includes information for 75% of all companies in the roofing sector as well as sub-samples of companies in other sub-construction sectors such as painting, plumbing and glazing services for the observation period from 1994 to 2008.[42] For all individuals who are subject to social insurance contributions and work in one of these companies on June 30th, the data contains the corresponding period of continued employment in that company within the calender year that overlaps June 30th.[43] Thus, the longest spell encompasses the full calender year, while the shortest employment spell would be an employment period of one day on June 30th only.

For each employment spell, we have information on age, sex, educational background, the gross daily wage, occupation, and occupational status. Thus, the data allows for identifying covered individuals quite precisely. In particular, we are able to exclude custodial workers, apprentices and white-collar workers as well as underage workers.[44] Overall, the sample consists of 791,910 observations in the roofing sector that stem from 172,257 covered roofers in 17,186 roofing firms and 1,557,661 observations by 354,834 workers in 35,250 firms from other sub-construction sectors who fulfill the same criteria.

Since the data only distinguishes between full-time and part-time workers and includes information on daily gross wages only, the main restriction of the BA data refers to the corresponding lack of information on hourly gross wages. As a remedy, we impute the hourly gross wage by estimating the observed hourly gross wage in

[42] This information is taken from the *Betriebshistorikdatei*, a data set that aggregates the individual data that is collected by the BA to the firm level, see Hethey-Maier and Seth (2010) for details on the data.

[43] This information is taken from the employee record of the BA (*Beschäftigtenmeldungen*), see e.g. Drews (2008) for details.

[44] We also exclude workers with a minor employment which is defined as earning below the social insurance contribution threshold of 400€ per month because these workers are included in the data only after 1998. For comparability reasons, we also dropped such workers in the LAK data.

the LAK data as a function of explanatory variables that are available in both data sets. For this purpose, we first adjust the LAK data to have a similar data structure as the BA data by creating employment spells for each individual who has worked on June 30th. For these spells, both data sets provide information on or allow for computing the length of the spell, the beginning of the spell, the daily gross wage, dummies for part-time or full-time employment, individual information on sex and age as well as a number of firm-level information such as firm size, workforce composition, and average gross daily wage.[45] Using all these explanatory variables and allowing for additional heterogeneity by estimating the wage model separately for each year, East and West Germany as well as for workers of different quintiles of the daily gross wage distribution, we are able to explain 88% of the variation in hourly gross wages in the LAK data. We then use these estimates for predicting the hourly wage in the BA data. The quality of this imputation not only hinges on the R^2 of the wage estimation, but also depends on the comparability of the LAK and the BA sample and explanatory variables. Figure 3.B2 shows that the imputed and observed wage distribution are very comparable. As a result, the average predicted mean wage for full-time workers of 13.26€ and 9.94€ in the BA data in West and East Germany, respectively, comes very close to the observed average wage in the LAK data with 13.22€ for West Germany and 9.85€ for East Germany.

[45] Although we do not use women in the LAK analyses, we do use their LAK wage and estimate the corresponding wage in the BA data to include them in the analyses conducted with the BA data.

3.4 The Minimum Wage and Its Bite

Table 3.1 displays several indicators of the bite of the minimum wage (MW) for the June preceding the introduction of a new MW regulation within the next year. In particular, we look at the share of covered workers for whom the upcoming MW is binding due to earning a wage below the minimum in the June preceding the new MW regulation.[46] We also show the average wage increase these workers would have to receive in case of full compliance with the upcoming MW. This individual wage gap for a worker i with a binding MW in period t is thereby defined as follows:

$$wage\ gap_{it} = \frac{MW_{i,t+1} - w_{it}}{w_{it}}, \tag{3.1}$$

where w_{it} represents the workers' hourly gross wage and $MW_{i,t+1}$ the upcoming MW. We contrast this wage gap to their actual wage increase within the next year and the actual wage increase during the same time period among workers for whom the MW was not binding. We complement this information by the Kaitz-Index, i.e. the ratio between the MW level and the median wage in the sector. Note that the indicators may slightly underestimate the bite of the MW due to the fact that the hourly wage may contain overtime compensation that is not subject to the MW.[47]

The indicators based on the LAK data allow for several interesting insights. First of all, the share of covered workers for whom the MW was binding by the time

[46] We do not adjust for nominal wage changes between the two dates of comparison because the intermediate time span is quite short.

[47] On average, overtime hours account for 6% of the working hours in June and, thus, may lead to an estimated hourly wage that is up to 1.6% too high depending on the applied overtime compensation scheme ranging from no additional compensation to a markup of 25%. Since we do not know which scheme is applied, we left the data uncorrected as the resulting imprecision appears to be rather marginal.

Table 3.1: Indicators of the minimum wage bite measured in June prior to the next MW regulation, LAK and BA data

New MW regulation takes effect on	MW (in €)	Workers with a binding MW? Yes: Share (in %) LAK	Share (in %) BA	Wage gap[a] (in %) LAK	Δ Wage[b] (in %) LAK	No: Δ Wage[b] (in %) LAK	Kaitz Index LAK
West Germany							
01.10.97	8.2	1.3	2.4	11.0	7.2	2.3	64.7
01.09.01	9.0	1.5	3.9	8.7	6.8	1.4	67.2
01.03.03	9.0	1.5	3.4	8.9	6.0	2.4	67.2
01.04.04	9.3	2.2	4.8	8.1	5.7	1.4	68.4
01.05.05	9.7	2.9	5.8	8.5	4.9	0.6	70.3
01.01.06	10.0	4.4	6.9	7.9	4.9	1.1	72.6
01.01.07	10.0	4.6	7.5	8.1	6.7	3.2	72.7
01.01.08	10.2	5.4	8.2	6.7	5.3	2.2	73.1
01.01.09	10.4	4.9	7.5	6.6	8.1	3.0	73.4
East Germany							
01.10.97	7.7	12.5	11.5	9.7	6.7	0.0	82.0
01.09.01	8.4	14.2	12.0	3.9	4.6	0.6	89.2
01.03.03	9.0	34.1	23.3	4.2	4.1	0.1	95.0
01.04.04	9.3	44.1	28.7	3.8	4.1	0.3	97.9
01.05.05	9.7	46.9	33.5	4.3	4.0	0.1	99.2
01.01.06	10.0	55.5	40.8	4.1	4.0	0.1	100.2
01.01.07	10.0	45.5	28.1	1.6	1.9	0.9	99.6
01.01.08	10.2	53.5	32.1	2.6	3.3	1.3	100.7
01.01.09	10.4	50.0	28.9	2.4	3.3	0.7	99.9

[a] Wage gap refers to equation (3.1)

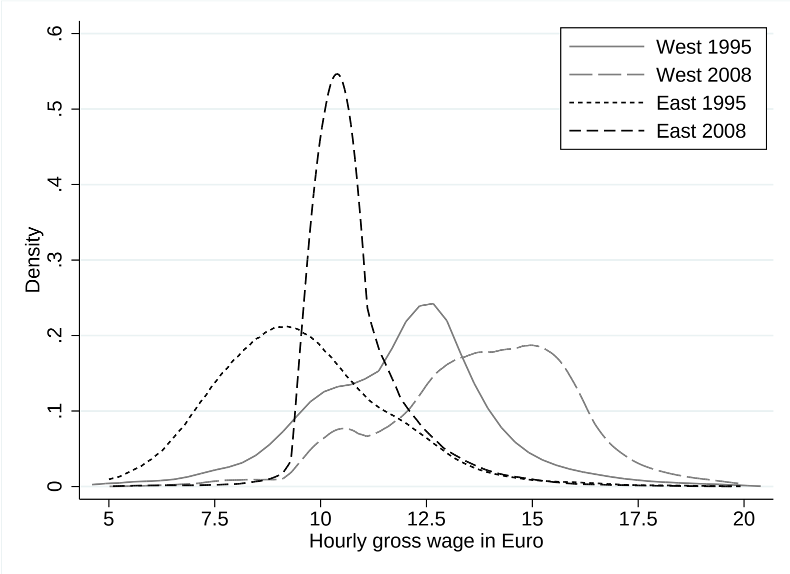
[b] Δ wage corresponds to the actual observed percentage wage change $(w_{it+1} - w_{it})/w_{it}$ between the June preceding and the June following the new MW regulation.

of its introduction was as low as 1.3% in West Germany compared to 12.5% in East Germany. While this share rose up to 5% in West Germany until 2008, around 50% of all East German workers earned below the upcoming MW at that time, a share that clearly exceeds the impact level that Machin et al. (2003) considered a hard biting MW. This extreme bite was fostered by the introduction of a common MW level in both parts of the country in 2003. Since then, the MW level approximately corresponds to the median wage in East Germany so that the Kaitz-Index ranges around 100%. Even in West Germany, the Kaitz-Index still ranges between two thirds and three quarters of the median wage. Compared to Dolton and Bondibene (2011), who find the Kaitz-Index to range between 30% and 70% in a survey among 22 OECD countries, the bite of the MW in the roofing sector is extremely hard, especially in East Germany.

We also observe that the MW has been effective, i.e. actual wage increases among workers with a binding MW exceeded the wage increases among workers for whom the MW was not binding. While the change in the west German wage distribution is rather marginal, the wage compression in East Germany results in a huge spike of workers whose wages range around the MW level, see Figure 3.3. Finally, note that despite these actual increases, they still fall short of the increases workers would have had to receive in case of full compliance, especially in West Germany during the initial years after the MW introduction. The improved compliance with the MW regulation during the last years might be due to stronger controls after 2006 according to interviews that we conducted with sector insiders.

Table 3.1 also contrasts the share of workers with a binding MW based in the LAK data to the corresponding share based on the imputation in the BA data. In contrast to the LAK data, however, the share of workers with a binding MW follows a probabilistic concept because we do not only impute the mean wage prediction for each individual but also the corresponding distribution that results from the unexplained variance and the variance of the estimated parameters. Assuming this distribution to be normally distributed, we are then able to calculate the probability that the wage of a worker falls below the MW level which we denote by P_{MW}. For the BA data, Table 3.1 thus reports the average predicted probability of being affected by a binding MW among all covered workers.

Figure 3.3: Kernel densities of hourly gross wages in East and West Germany, 1995 and 2008, LAK data



As we can see from Table 3.1, the resulting share of workers with a binding MW resembles the LAK patterns, but differs in levels, especially for East Germany in the last years. In fact, imputing the probability of being affected by a binding MW in the LAK data, gave very similar deviations to the observed share of workers with a binding MW. Hence, it is apparently the extreme wage compression that leads to the asymmetric form of the wage distribution in Figure 3.3 and, thus, to a systematic underestimation of the share of workers with a binding MW in East Germany, see Appendix 3C for further explanation.

Despite the large bite of the minimum wage, especially demonstrated by the high wage compression in East Germany, the overall labour cost burden is still modest for two reasons. First, even in the case of full compliance with respect to the minimum wage regulations, total labour costs only increased by 1% in West Germany and 2.5% in East Germany on average during the observed time period.

Second, labour costs amount to less than 40% of total costs, so that the change in average total costs varies across time between 0.2-0.5% in West Germany and 0.3-0.8% in East Germany. However, despite the low impact on total costs on average, some firms may well be affected more strongly. Moreover, the cost burden may cumulate over time due to the gradual increase in the minimum wage level.

Finally, it is worth mentioning that individuals with a binding MW clearly differ between West and East Germany. While the average worker with a binding MW in East Germany does not differ much from an average worker without a binding MW, the average worker with a binding MW in West Germany rather corresponds to a marginal worker with below average human capital, short tenure and part-time employment in firms with a skill and wage level below average, see Table 3.2.

Table 3.2: Characteristics of workers in West and East Germany by binding status, BA data, 1995-2008

	West Germany		**East Germany**	
MW for workers is binding?	Yes	No	Yes	No
Individual characteristics				
Worker with voc. training deg. (in %)	24.1	67.2	70.2	80.4
Workers without voc. training (in %)	34.5	31.1	25.2	19.0
Part-time workers (in %)	41.3	1.7	4.6	0.5
Previous work exp. in sector (in years)	2.2	4.3	2.9	3.6
Previous tenure in firm (in years)	1.9	3.7	2.2	3.0
Firm characteristics				
Average firm size	4.0	6.2	5.7	7.8
Firm's share of skilled workers (in %)	63.1	83.1	79.9	82.2
Firm's mean daily gross wage (in €)	51.78	72.33	50.43	56.08
Number of observations	15,523	485,640	39,960	196,981

Note: Workers with $P_{MW} > 0.5$ are considered to be bound by the MW.

3.5 General Framework for the Identification of Employment Effects

Since the minimum wage was introduced for the entire sector at the same time, a strategy for the identification of the minimum wage impact on employment cannot rest on regional variation as has been done in many US studies (among others Dube et al. 2007 and 2010 and Card and Krueger 1994 and 2000). Exploiting the existing variation in the minimum wage level between East and West Germany in the mid 1990s is also not advisable since the business cycle after the reunification boom differed between both parts of the country, see Figure 3.1.

Thus, there are mainly two potential approaches available for the identification of employment effects. Either one exploits the variation in treatment intensity within the roofing sector by comparing workers with and without a binding MW, or one uses a sub-construction sector that is not covered by a minimum wage regulation, but is as similar as possible to the roofing sector. To see why and under which assumptions these control groups allow for an estimation of the average treatment effect on the treated (ATT), let e_{it} denote the employment status in period t for an individual i. In particular, let $e_{it+1} = 1$ in case of being employed in the same sector as in the previous period and $e_{it+1} = 0$ otherwise, an outcome measure that we are able to observe in both the LAK and the BA data.

This outcome measure is of main interest in a market context that is dominated by a shrinking market size and a corresponding reduction in employment since the mid 1990s, compare Figure 3.B1. The question of whether someone was able to keep his job in this market context given the additional cost pressures of the minimum wage is of main concern. Note, however, that this outcome should not be equated with effects on the total employment in the roofing sector. As an example, additional market entries by single person companies are not captured by this employment outcome.

With this outcome measure in mind, denote the group of treated individuals as g_1 and the group not treated as g_0. Let the minimum wage be introduced in t^* with $t_0 < t^* < t_1$. Note that all years prior to 1997 measure the ex-ante situation t_0, while observations for the ten observable years after 1997 measure the ex-post situation t_1 because the employment outcome in the following June, e_{it+1}, is already influenced by the MW introduction in October 1997 for workers observed in June 1997. For the ex-post situation, we either get an estimate for $\mathbf{E}[e_{it+1}|g_1, t_1]$ or $\mathbf{E}[e_{it+1}|g_0, t_1]$. The average treatment effect on the treated (ATT), θ, can now be estimated by assuming that the difference in employment outcomes between t_0 and t_1 was the same for both groups in the absence of the treatment. Moreover, we need to assume that the treatment does not indirectly affect the control group, for example, via substitution effects. In this case, the causal impact θ is given by the difference-in-differences (DiD) estimator

$$\mathbf{E}[e_{it+1}|g_1, t_1] - \mathbf{E}[e_{it+1}|g_1, t_0] - (\mathbf{E}[e_{it+1}|g_0, t_1] - \mathbf{E}[e_{it+1}|g_0, t_0]) = \theta. \tag{3.2}$$

In order to relax the assumption that the control group captures the counterfactual employment outcome, we can also estimate the DiD effect within a regression framework that controls for observable differences across both groups. Since the outcome measure calls for a non-linear analysis, we use a Logit estimation with

$$P(e_{it+1} = 1) = \Lambda[\alpha_g + \gamma_t + \delta D_{it} + \beta X_{it} + \epsilon_{it}] \tag{3.3}$$

where α_g captures the time constant difference between both groups, γ_t captures the change across time that is common to both groups, and X corresponds to a set of control variables. D_{it} is the treatment indicator with $D_{it} = 1$ for individuals of group g_1 for the period $t > t^*$, i.e. for the treatment group after treatment has taken place, and $D_{it} = 0$ otherwise. Note that neither the coefficient for D_{it} nor its odds ratio capture the treatment effect of interest due to the non-linearity of the estimator. Following Puhani (2012), we estimate the marginal effect of δ to derive the treatment effect of interest θ by using the following formula:

$$\theta = ME(\delta) = \Lambda[\alpha_g + \gamma_t + \delta D_{it} + \beta\hat{x}] - \Lambda[\alpha_g + \gamma_t + \beta\hat{x}], \tag{3.4}$$

where $\hat{x}$ indicates that we calculate the marginal effect for the average individual observed in the sample. In particular, we add covariates that may affect employment outcomes and could potentially be related to the treatment indicator such as sex, age, education, occupational status, and work experience in the sector and in the company as well as some firm characteristics such as size, the composition of the workforce, and mean wage level. By including these firm characteristics, we control for the fact that unproductive workers may be selected into less productive and thus less well-paying companies that differ with regard to employment chances irrespective of the minimum wage.

In addition, selection on unobservables may be relevant. If, for example, employers mainly dismiss the most unproductive workers for a given type of qualification and experience, not controlling for this would upward bias our estimates. Due to the longitudinal nature of our data, we can mitigate this problem by allowing for individual-specific time-constant effects. Note, however, that one cannot calculate the marginal effect of interest for a Fixed Effects Logit Estimator because the fixed effects are not identified in this model framework (Wooldridge 2002). Moreover, the Fixed Effects Logit Estimator only uses the sub-sample of the observations for which we observe a change in the outcome across time. This is problematic since we find evidence that the conditional sample depends on the treatment, thereby biasing the estimates. Hence, we estimate a simple linear fixed effects model, thereby avoiding the non-linear complications. We find that in most cases, only very few observations have predictions outside the plausible range. Moreover, pooled Logit and pooled OLS estimates also turned out to be quite similar. We, thus, report linear fixed effects results whenever individual fixed effects seem necessary.

3.6 Average Employment Effects

In this section, we apply the general framework introduced in the latter section to the intersectoral comparison (Section 6.1) and the comparison within the roofing sector (Section 6.2). As we will see, the comparison of both approaches yields first insights into possible spillover effects. In Section 7, we will then explicitly look at spillovers in the roofing sector by separately running the intersectoral comparison for workers with and without a binding MW as well as for workers falling in different wage deciles. This is feasible because we are able to identify comparable workers in the control sector.

3.6.1 Intersectoral comparison

Approach. A feasible control sector needs to capture the counterfactual change in employment outcomes for roofers in the absence of the minimum wage. For this to be a plausible assumption, the control sector should have a comparable market structure as well as comparable demand conditions. Among the sub-construction sectors without a legally binding minimum wage - the plumbing and the glazing sector[48] - the plumbing sector is preferable for a number of reasons. According to Figure 3.1, the business cycle in the plumbing rather than the glazing sector resembles the business cycle in the roofing sector. In fact, for West Germany demand conditions almost follow the same path, while in East Germany the demand for plumbing services started to drop somewhat earlier than in the roofing sector, a deviation that we will return to in the robustness analysis.

Moreover, the plumbing sector is similar to the roofing sector with regard to important market indicators that moderate the potential impact of a minimum wage, see Table 3.3.[49] In particular, roofing and plumbing companies are similarly sized in terms of both the number of employees and the revenues generated. Also, the value added is highest in the roofing sector, closely followed by the plumbing sector. Moreover, the glazing sector is more labour-intensive and invests almost twice as much per employee than the other sectors while the average gross daily wage is quite comparable across all sectors. Finally, the number of companies per one million euro of revenues in the sector, a measure of the degree of competition, is almost identical in the roofing and plumbing sector but much lower in the glazing sector, suggesting less competition.

Therefore, we consider the plumbing sector as a suitable and better benchmark for the roofing sector than the glazing sector. For the intersectoral comparison, the treatment group g_1, thus, corresponds to all workers of the roofing sector that are

[48]The painting sector introduced a MW in 2003.

[49]We display the pre-minimum wage indicators for 1996 wherever it is available as the basis for judging the usefulness of a sector as a benchmark for the roofing sector.

Table 3.3: Comparison of the roofing, the glazing, and the plumbing sector by various economic indicators

	Roofers	Plumbers	Glaziers	Source
Number of companies	11,295	37,720	3,752	A, 1996
Number of employees	113,996	364,393	25,393	A, 1996
Avg. number of employees per company	8.8	9.0	6.6	A, 1996
Share of firms by revenues (in 1,000)				B, 1996
$<$ 100 DM	6.8	8.8	13.6	
100-500 DM	24.6	33.7	42.6	
500-1,000 DM	26.1	23.5	21.5	
1,000-2,000 DM	25.1	19.3	13.5	
$>$ 2,000 DM	17.4	14.6	8.5	
Value added in € per employee	37,195	35,949	32,931	C, 2001
Investments/employee (in €)	1,472	1,229	2,482	C, 2001
Share of labour costs (in %)	36.0	32.5	49.0	C, 2001
Avg. gross daily wage/fulltime employee (in €)	61.25	63.23	64.28	A, 1996
Number of companies/1 Mio. sector revenue	1.3	1.3	0.6	B, 1996

Note: A - BA data (see Section 3.3); B - Revenue tax statistics of the German Federal Statistical Office (Umsatzsteuerstatistik); C - Cost Structure Survey of the German Federal Statistical Office (Kostenstrukturerhebung)

covered by the minimum wage regulations, while workers in the plumbing sector, who would have been covered if they worked in the roofing sector, are considered as the control group g_0. Hence, this approach can only be estimated based on the BA data. The treatment refers to being covered by the minimum wage regulations and the resulting estimates give us the average employment effect for covered workers in the roofing sector if changes in employment outcomes of plumbers between the ex-ante situation (t_0 : 1994-1996) and the ex-post situation (t_1 : 1997-2007) capture the counterfactual change in employment outcomes for roofers in the absence of the minimum wage. Moreover, we need to assume that there is no control group contamination, i.e. there is no indirect effect of the minimum wage regulations in the roofing sector on the plumbing sector. If the plumbing sector provides some substitutes for roofing services, for example, a negative employment effect in the roofing sector would boost employment in the plumbing sector, thereby overestimating a negative impact of the minimum wage. However, the lack of any evident improvement in the revenues realised by the plumbing sector relative to the roofing sector after the MW introduction puts doubt on such spillovers, see Figure 3.1. Moreover, we find that transitions between both sectors are negligible and independent from the MW introduction. Both before and after 1997, only about 0.2% (0.1%) of all roofers (plumbers) enter the plumbing sector (roofing sector) in the next year.

Results. Descriptives regarding both the dependent variable and the set of covariates for both roofers and plumbers prior to and after the minimum wage introduction are provided in Table 3.A1 for East and Table 3.A2 for West Germany. On average, 80% (77%) of all West (East) German roofers are still employed in the same sector after one year. The unconditional DiD of the dependent variable for the intersectoral comparison corresponds to 3 percentage points for West and -1 percentage point for East Germany. However, the DiD controlling for observable characteristics across sectors suggests some relevant changes in observables such as a relative increase in skilled workers in the roofing sector that needs to be controlled for in a regression approach. For the regression approach, D_{it} in equation (3.3) equals one for all roofers in the period after the minimum wage introduction (t_1 : 1997-2007).

Table 3.4 shows the marginal effect (the ATT) of D_{it} from equation (3.4) for the logit model (LPM) specification for the average worker in East and West Germany. As previously discussed, we compare estimates from the pooled specifications and

Table 3.4: Minimum wage effect on the probability of being employed in the roofing sector in the next year, intersectoral comparison based on BA data, 1994-2007

	(1) Pooled Logit	(2) Pooled Logit	(3) Pooled LPM	(4) FE LPM
	East Germany			
ME / ATT of D_{it} in pp.[a]	-2.0***	-2.2***	-2.3***	-2.9***
Robust s.e.	(0.3)	(0.2)	(0.3)	(0.3)
Obs. (in 1000)	497	497	497	497
Share of $\hat{Y} \notin [0;1]$	n/a	n/a	0.4%	9.0%
Individual covariates[b]	Yes	Yes	Yes	Yes
Firm-level covariates[c]	No	Yes	Yes	Yes
	West Germany			
ME / ATT of D_{it} in pp.[a]	2.0***	1.8***	1.2***	-1.2***
Robust s.e.	(0.2)	(0.2)	(0.2)	(0.2)
Obs. (in 1000)	1,110	1,110	1,110	1,110
Share of $\hat{Y} \notin [0;1]$	n/a	n/a	0.9%	0.4%
Individual covariates[b]	Yes	Yes	Yes	Yes
Firm-level covariates[c]	No	Yes	Yes	Yes

[a] Marginal effect of D_{it} in percentage points; for logit estimations calculated as in equation (3.4)

[b] Occupational status and educational attainment (6 dummies) in the fixed effects estimations plus age, age^2, sex, 2nd order polynomial of previous work experience in the sector and in the company in the pooled estimations

[c] Age and qualification of company workforce, company size (4 dummies), 2nd order polynomial of mean daily gross wage

Significance levels: * 5%, ** 1%, *** 0.1%

estimates from a LPM that takes account of individual-specific fixed effects. Irrespective of the specification, the minimum wage in East Germany appears to have reduced the chances for roofers to remain employed in the sector by around 2 to 3 percentage points on average compared to plumbers who have not been subject to minimum wage regulations. Adding firm-level covariates in column 2 compared to a specification that includes individual covariates only, does not have much influence on the estimated impact. Moreover, both the pooled Logit model and the pooled LPM yield quite similar results and, with only 0.4% of all observations falling outside the admissible range, the LPM estimator performs quite well. Controlling for time-constant unobservables at the individual level (column 4) yields very comparable results although the share of observations outside the $[0; 1]$ interval rises to still acceptable, albeit higher, 9.0%.

In West Germany, the impact appears to be similarly robust when controlling for firm-level in addition to individual covariates. Both specifications indicate a positive minimum wage effect for roofers as compared to plumbers of around 2 percentage points. Also the pooled LPM model suggests a positive minimum wage effect, which is slightly lower than for the pooled logit models. However, when controlling for time-constant unobservable characteristics in the FE LPM specification, the findings for West Germany indicate that the chances for a roofer to remain employed in the next year after the minimum wage was introduced decrease by around 1 percentage point compared to plumbers who have not been subject to minimum wage regulations. This suggests that minimum wages in West Germany increased layoffs mainly among workers with poor unobservable characteristics so that pooled estimations are upward biased.

Robustness. The validity of the previous results critically hinges on the common trends assumption. Unfortunately, we only have three years prior to the MW introduction in order to examine the pre-treatment trend in employment outcomes. For East Germany, Table 3.A1 suggests a dip in employment chances in the roofing sector in 1996 that deviates from the trend in the plumbing sector. Indeed, placebo tests confirm the common trend assumption between 1994 and 1995, while there are significant deviations between 1995 and 1996, see Table 3.A3. The decline in employment outcomes in 1996 may hint at anticipation effects since employment outcomes for the last pre-MW year are measured just three months prior to the MW introduction in October 1997. Excluding observations for 1996, however, suggests even somewhat stronger negative effects, see Table 3.5. If the dip in 1996 does not result from an anticipation effect, we should, however, not exclude this year, but adjust our estimates for diverging trends. A corresponding extension of the previous estimation that allows for diverging trends across sectors mainly supports the previous findings with only the pooled LPM estimates deviating from the previous estimates. For West Germany, Table 3.A2 suggests that there was a dip in employment outcomes for roofers relative to plumbers in 1995. Thus, compared to plumbers, the placebo tests suggest a less favourable trend for roofers between 1994 and 1995, but a positive trend from 1995 to 1996, see Table 3.A3. Estimations that allow for diverging trends across sectors, however, confirm the previous findings, see Table 3.5.

Adjusting for diverging trends based on the few pre-MW years, however, may not suffice if the common trends assumption fails in the long run. As some tentative robustness check, we ran estimations that were extended by interacting the treatment indicator D_{it} to allow for a heterogeneous ATT for periods with distinct levels of a MW bite (1997-2001, 2002-2004 and 2005-2007) in order to examine the timing of the effect after the MW introduction. As shown in Table 3.5, the impact of the minimum wage in East Germany was significantly negative in all three sub-periods. Moreover, according to the preferred fixed-effects specification, the strongest impact occurred in the second period after the minimum wage was raised to the level in West Germany. In West Germany, the fixed effects specification also suggests that the minimum wage impact was strongest in the intermediate period, followed by the period after its introduction. The smaller effect in the last period

Table 3.5: Marginal effects and ATTs of D_{it} in percentage points for various robustness checks of the intersectoral comparison in Table 3.4

	(1) Pooled Logit	(2) Pooled LPM	(3) FE LPM	Obs. (in 1000)
East Germany				
Sample without 1996	-3.4***	-3.6***	-3.4***	400
Trend-adjusted DiD	-2.1***	0.7	-1.8***	497
Extension with period-wise effects				
ME / ATT of $D_{it}xt_{97-01}$ in pp.	-1.3***	-1.7***	-3.1***	
ME / ATT of $D_{it}xt_{02-04}$ in pp.	-3.0***	-2.8***	-3.6***	497
ME / ATT of $D_{it}xt_{05-07}$ in pp.	-4.7***	-3.7***	-2.7***	
West Germany				
Trend-adjusted DiD	2.6***	1.6***	-1.2***	1,110
Extension with period-wise effects				
ME / ATT of $D_{it}xt_{97-01}$ in pp.	1.8***	1.0***	-1.3***	
ME / ATT of $D_{it}xt_{02-04}$ in pp.	1.5***	0.7***	-1.8***	1,110
ME / ATT of $D_{it}xt_{05-07}$ in pp.	1.8***	1.9***	-0.6**	

Note: Same specification as in Table 3.4 with individual and firm-level controls;
Significance levels: * 5%, ** 1%, *** 0.1%

may suggest that firms were able to bear the additional costs that were imposed by the minimum wage during these years of economic revival in the West German roofing sector. All in all, the estimation results appear to be quite robust and also show a plausible impact pattern across time.

3.6.2 Comparison within the roofing sector

Approach. The treatment group g_1 in this approach corresponds to roofers with a binding MW due to earning a wage in June that is below the minimum wage level that takes effect until June of the next year. For the pre-regulation years, we consider someone to belong to the treatment group if his wage falls below the minimum wage level that would have to be applied in the pre-regulation years given the increases of the median nominal wage in the LAK data. Workers of the roofing sector whose wages are above that minimum level are used as the control group g_0. While we are able to exactly identify these groups in the LAK data, we define g_1 in the BA data to encompass all covered workers whose probability to fall below the minimum wage level exceeds 50%.[50]

Hence, the treatment indicator D_{it} equals one for all covered workers with a binding minimum wage after the MW introduction and zero otherwise. The resulting estimates give the average treatment effect on the treated if changes in employment outcomes for workers without a binding MW between the ex-ante situation (t_0 : 1995-1996)[51] and the ex-post situation (t_1 : 1997-2007) capture the counterfactual change in employment outcomes for the treated roofers in the absence of the minimum wage. To the extent that all roofers are affected by the same demand conditions, this is clearly a plausible assumption. However, for a period of 13 years that we cover in the estimations, diverging trends may arise due to, e.g., skill-biased technological advances. We will return to this potential problem in the robustness section. Moreover, the suggested approach only yields unbiased estimates if there are no minimum wage induced employment effects for workers without a binding minimum wage. In fact, note that, if all identifying assumptions hold, the intersectoral comparison and the estimate of the comparison within the roofing sector yield the same employment effects for an average covered worker in the roofing sector.

Results. This approach can be estimated using both the BA and LAK data. Corresponding descriptives for the BA data are provided in Table 3.A4 for East

[50] A specification with P_{MW} as a continuous treatment variable gave similar results.

[51] Note that we have only two pre-regulation years because the LAK data that are used for imputing hourly wages are only available from 1995 onwards.

and Table 3.A5 for West Germany.[52] The unconditional DiD for the probability of still being employed in the next year indicates strong positive effects, but this raw DiD has to be interpreted with caution. Since the minimum wage level repeatedly increased after 1998, the share of individuals with a binding MW also increased until 2008 (see Table 3.1). As a consequence, the pool of workers with a binding MW is likely to improve across time resulting in upward biased estimates. We therefore decided to exploit the variation in the status of being affected by a binding MW across time on the individual level by taking account of individual fixed effects. In fact, the pooled Logit and pooled LPM model show strongly positive and comparable employment effects that are likely to reflect the described non-comparability of the pool of workers with a binding MW prior to and after the MW introduction. Since we do not consider these estimates informative, Table 3.6 reports the estimates for the fixed effects LPM only.[53]

The DiD results for East Germany in columns (1) and (2) indicate that workers with a binding MW were 9 to 10 percentage points less likely to remain employed after the MW introduction relative to workers without a binding MW. This negative effect is confirmed by both the LAK and BA data suggesting that the BA data yield quite reliable estimates despite the imprecision in the distinction between workers with and without a binding MW. For West Germany, the LAK data show an insignificant reduction in the probability of continued employment by −2.7 percentage points, while the treatment effect is slightly larger and significant at

[52] Descriptives for the LAK data are available from the authors upon request.

[53] As an alternative, one could evaluate each minimum wage level separately by defining workers with and without a binding MW corresponding to each MW level. However, corresponding estimates did not show any clear pattern, thus indicating that anticipation effects or the catching up with past increases make it difficult to isolate the impact of subsequent MW increases.

the 5% significance level when using the BA data.[54]

Table 3.6: Minimum wage effect on the probability of being employed in the roofing sector in the next year, DiD estimations between workers with and without a binding MW, FE LPM, BA and LAK data, 1995-2007

	(1) DiD[a] LAK	(2) DiD BA	(3) taDiD[b] BA	(4) DiDiD[c] BA
East Germany				
ATT of D_{it} in pp.[d]	-8.5***	-10.0***	-8.8***	-5.6***
Robust s.e.	(0.7)	(0.8)	(1.1)	(1.2)
Obs. (in 1000)	288	224	224	446
Share of $\hat{Y} \notin [0;1]$	7.3%	8.6%	4.0%	6.9%
ATT for all covered workers	-1.7	-2.0	-1.8	-1.1
West Germany				
ATT of D_{it} in pp.[d]	-2.7	-5.0*	-0.8	-7.2*
Robust s.e.	(2.3)	(2.0)	(2.5)	(2.9)
Obs. (in 1000)	601	457	457	1,013
Share of $\hat{Y} \notin [0;1]$	0.1%	0.9%	0.1%	3.6%
ATT for all covered workers	-0.1	-0.3	-0.04	-0.4

[a] DiD estimation for workers with and without a binding MW within the roofing sector

[b] Trend-adjusted DiD estimation for workers with and without a binding MW within the roofing sector

[c] DiDiD estimation for workers with and without a binding MW within the roofing sector compared to the plumbing sector

[d] ATT in percentage points for the treatment indicator D_{it} from a linear probability model with individual fixed effects; all estimations include individual and firm level covariates as in Table 3.4 for the BA data and the same covariates except educational attainment for the LAK data.

Significance levels: * 5%, ** 1%, *** 0.1%

If all identifying assumptions regarding common trends in the absence of the treatment and the lack of any spillovers held, multiplying the ATT from the comparison of workers with and without a binding MW within the roofing sector in

[54]When including individuals with a minor employment in the LAK estimates, the treatment effect amounts to highly significant -17.3 percentage points. For a better comparability with the BA data, we leave these individuals out. Still, the estimates with minor employment indicate that the MW may have had a strong effect on their employment chances, a finding that should be approached in future research.

Table 3.6 by the share of workers with a binding MW should yield the ATT from the previous intersectoral comparison, i.e. the average effect for all covered workers in the roofing sector.[55] The implied ATT for covered workers are included in Table 3.6. For East Germany, this yields an ATT for all covered workers of $-2.0(= -10.0 \times 0.204)$ percentage points for the BA results since, on average, 20.4% of all workers in East Germany are affected by a binding MW across the entire period. In West Germany, the MW is binding for 5.5% of the workforce on average, implying an ATT for all covered workers of -0.3% percentage points. Compared to the fixed-effects estimates in Table 3.4, the fixed-effects estimates in Table 3.6 appear to be underestimated. Since we consider the assumptions underlying the previous intersectoral comparison plausible given the robustness checks, this deviation either indicates that the common trend assumption between workers with and without a binding MW is violated and/or that workers without a binding MW must be indirectly and negatively affected by the MW.

Robustness. In order to rule out that the observed deviation between the ATT from both estimation approaches results from a violation of the common trends assumption between workers with and without a binding MW, we need to test the robustness of this assumption. Therefore, we examine placebo tests for the two years that are available prior to the MW introduction, see Table 3.A3. While the placebo test confirms the common trend for West Germany, the placebo test for East Germany is insignificant only for the LAK data, but suggests a diverging trend based on the BA data despite the fact that estimation results were quite comparable across data sets. Surprisingly, however, allowing for diverging trends

[55] This is the case because the comparison within the roofing sector assumes a zero effect of the MW for workers without a binding MW.

between workers with and without a binding MW does not alter the estimation results much for East Germany, but suggests an insignificant negative impact for West Germany, see column (3) in Table 3.6.

Since the observable pre-regulation period is, however, very short, it is questionable to what extent placebo tests may confirm or disprove the validity of the common trends assumption for the whole period and to what extent trend-adjusted estimations based on this short pre-regulation period help to tackle a potential violation of this assumption. Moreover, diverging trends in the long run due to, for example, skill-biased technological advances might still bias our estimates.

As an alternative robustness check, we therefore capture the potentially diverging trends in the absence of the MW between workers with and without a binding MW by using comparable workers from the plumbing sector as an additional benchmark. In particular, we identify those plumbers for whom the minimum wage would have been binding if they worked in the roofing sector given their individual and firm characteristics. We do so by imputing the wage plumbers would receive in the roofing sector given their characteristics, thus applying the wage imputation described in Section 3.3 not only to roofers but also to plumbers and estimating the probability of being bound by the minimum wage (P_{MW}) analogous to roofers. Figure 3.B3 indicates that the distribution of P_{MW} is astonishingly similar across sectors, thus indicating the similarity of both sectors with respect to observable characteristics. Hence, if we think about the imputation of the counterfactual P_{MW} among plumbers as an approach that is similar to matching individuals with a similar treatment intensity, the necessary common support along the whole distribution seems to be given.

We then use plumbers with and without a counterfactually binding MW to run a DiDiD estimation where the treatment indicator D_{it} equals one for workers of the roofing sector with a binding MW after the MW introduction and zero for all other groups and time periods. If roofers with and without a binding MW had experienced different trends in their employment chances even in the absence of the minimum wage, reflecting, for example, skill-biased technological progress, we assume that plumbers with and without a counterfactually binding MW capture these diverging trends across time. The corresponding DiDiD results in column (4) of Table 3.6 indicate that the employment effect in East Germany continues to be negative, but somewhat smaller. This may suggest that part of the negative effect in Table 3.6 is in fact due to a negative trend for workers with a binding MW relative to workers without a binding MW. Since representatives of the roofing sector repeatedly mentioned the need for catching up with technological progress in East Germany (see Aretz et al. 2011), this appears a plausible finding. For West Germany, the difference between the DiD and DiDiD estimates are smaller and suggest that trends in West Germany rather diverge in the opposite direction. More importantly, we find that the implied ATT for an average covered worker is smaller than suggested by the intersectoral comparison in the previous section. Therefore, employment spillovers between workers with and without a binding MW remain the prime suspect for this deviation, a hypothesis that we examine in the next section.

3.7 Making Employment Spillovers Visible

Approach. The treatment group g_1 corresponds to all workers of the roofing sector who are covered by the minimum wage regulations, while workers in the plumbing sector, who would have been covered if they worked in the roofing sector, are considered as the control group g_0. However, we do not estimate the average employment effect for covered roofers as in the intersectoral comparison in Section 3.6, but allow the treatment effect to differ by binding status. In particular, we distinguish between workers with a binding MW ($p_1 = P_{MW} > 0.5$), workers with wages significantly above the minimum level ($p_3 = P_{MW} < 0.1$) and an intermediate group of workers p_2 for whom P_{MW} ranges between these extremes. Since we have estimated the counterfactual probability of earning a wage below the MW for workers in the plumbing sector, we are thus able to extend the DiD framework to identify the treatment effect for all three groups:

$$P(e_{it+1} = 1) = \kappa_p + \alpha_{g\times p} + \gamma_{t\times p} + \delta D_{it} \times p + \beta X_{it} + c_i + \epsilon_{it} \tag{3.5}$$

where κ_p captures the time constant difference between workers of a different binding status and $\alpha_{g\times p}$ captures the time constant deviation between roofers and plumbers of the same binding status. Furthermore, $\gamma_{t\times p}$ allows for changes in employment outcomes of workers with a particular binding status across time that are common to roofers and plumbers, while the same set of covariates X as before controls for observable differences across workers. The treatment indicator D_{it} equals one for covered roofers in the period after the MW introduction (1997-2007) and zero for plumbers as well as roofers in the ex-ante period (1995-1996). This treatment indicator is interacted with the binding status so that we get an ATT for all three groups of workers. If spillover effects are relevant, we should observe non-zero outcomes for workers with wages above the minimum level, i.e. with $P_{MW} < 0.5$.

Note that due to the change in the pool of workers with a particular binding status across time (see previous section), we again use a fixed-effects linear probability model for estimation, i.e we exploit the change in binding status across time on the individual level for identification by including c_i in equation (3.5). The identifying assumption is that plumbers and roofers with the same set of covariates X and the same changes in the binding status would experience comparable changes in employment outcomes across time in the absence of the MW. In fact, note that this assumption is less strict than the assumption in Section 3.6.1 because we condition on the binding status in addition to X.

Results. Columns (1) and (3) in Table 3.7 show the estimated impact of the minimum wage on the probability of still being employed in the sector in the next year by binding status of the worker. The outcomes for workers with a binding MW (p_1) range around -9 percentage points in both East and West Germany. As expected from the previous discussion, the estimated treatment effect for workers with wages above the minimum wage level significantly differs from zero and suggests employment spillovers. For the intermediate group (p_2), the chances of remaining employed in the roofing sector are reduced by almost 5 percentage points in East Germany and by almost 4 percentage points in West Germany. Even for workers whose probability to fall below the MW is less than 10%, we find an increased risk of leaving the roofing sector of almost 3 percentage points in East Germany and around 1 percentage point in West Germany. Note that these findings imply an ATT for an average covered roofer that is largely in line with the estimates in Section 3.6.1.[56] However, the effect is not only caused

[56]This implied ATT results from calculating the weighted ATT across the three groups of workers that differ by binding status.

Table 3.7: Minimum wage effect on the probability of being employed in the roofing sector in the next year, intersectoral DiD estimations by binding status, BA data, 1995-2007

	East Germany		West Germany	
	(1)	(2)	(3)	(4)
FE LPM with trend adjustment?	No[b]	Yes[c]	No[b]	Yes[c]
ATT of $D_{it} \times p_1$ in pp.[a]	-8.6***	-8.5***	-8.8***	-8.4***
ATT of $D_{it} \times p_2$ in pp.[a]	-4.8***	-4.7***	-3.7*	-3.7*
ATT of $D_{it} \times p_3$ in pp.[a]	-2.8***	-2.4***	-1.2***	-1.5**
Obs. in 1,000	446	446	1,013	1,013
Implied ATT analogue to section 3.6.1	-4.4	-4.1	-1.8	-2.0

[a] ATT in percentage points by binding status p.
[b] Linear DiD estimation as in equation (3.5); covariates X as in Table 3.6.
[c] Linear DiD estimation as in equation (3.5) extended by sector-specific time trends; covariates X as in Table 3.6
Significance levels: * 5%, ** 1%, *** 0.1%

by workers with a binding MW, but to a lesser extent also by workers who are not affected by a binding MW. Therefore, the ATT for workers with a binding MW relative to workers without a binding MW in Section 3.6 is downward biased.

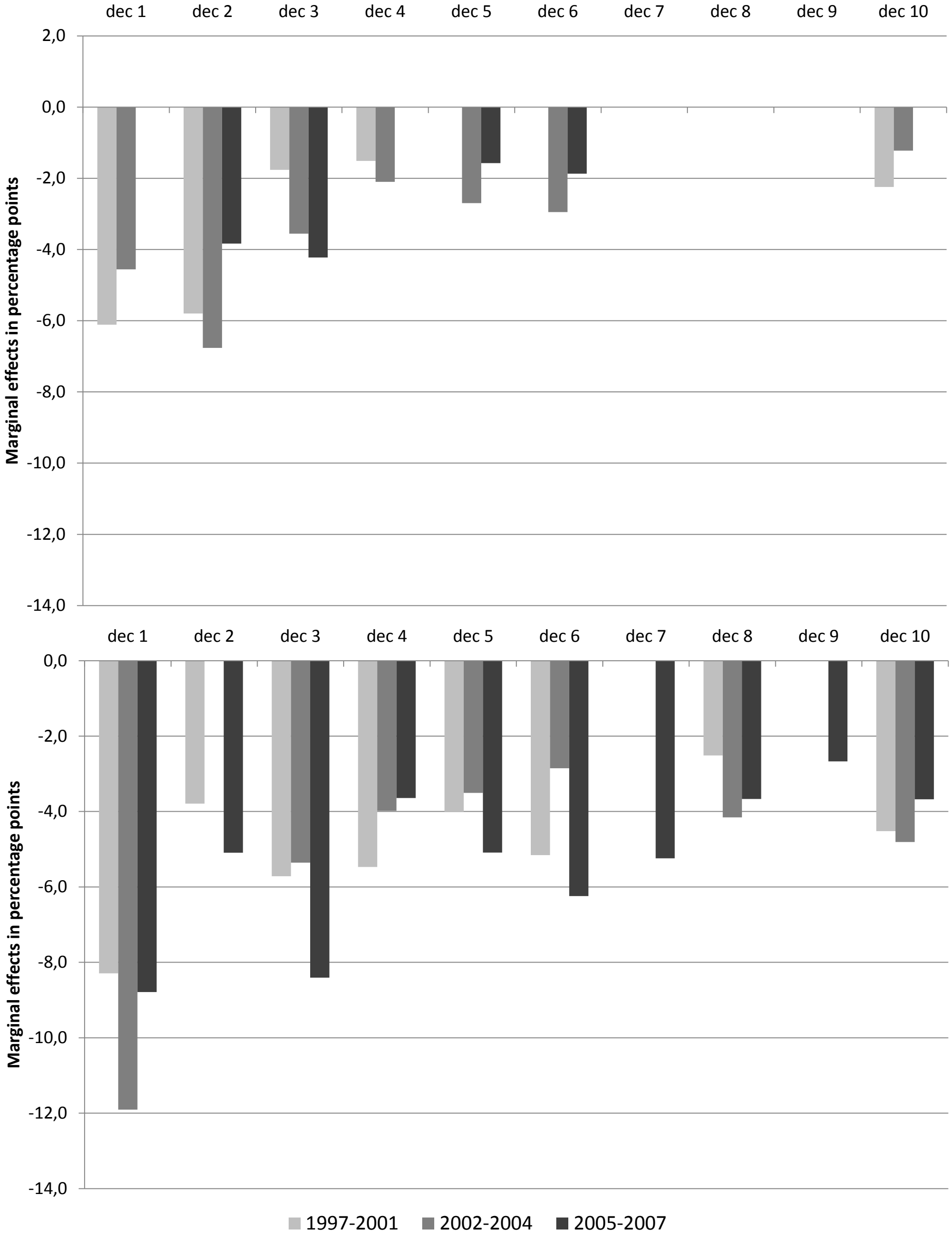
Robustness. Note that interpreting the above cross-sector comparisons for workers of a different binding status as evidence for employment effects for workers who earn a wage above the minimum wage threshold rests on several assumptions. First of all, spillovers between the sectors need to be at least negligible compared to spillovers between workers within the roofing sector. As discussed in Section 3.6.1, we consider this to be a plausible assumption given the low and quite time-constant rate of intersectoral transitions of workers. Secondly, roofers and plumbers of a particular binding status must have experienced a similar trend in employment outcomes in the absence of the MW. We test for the robustness of the common trends assumption by allowing for different trends across sectors based on the years prior to the MW introduction. The results in Table 3.7 suggest that the trend-adjustment does not have much of an effect on the estimation results.

However, the common trends assumption could fail in the long run, and a trend-adjustment based on two pre-regulation years may not suffice to tackle the problem. A further concern with the previous estimates might be that there is some statistical uncertainty regarding the binding status of the worker due to the imputation. In other words, even the group that is considered to be unaffected by a binding MW (p_3) still has a probability of earning a wage below the minimum wage level of up to 10%. As a result, the previous results might simply reflect the individuals within each group for whom the MW is in fact binding.

In order to counteract such concerns, we conduct the previous intersectoral comparison along the deciles of the wage distribution and distinguish between three sub-periods (1997-2001; 2002-2004; 2005-2007) that differ by bite. As an advantage, we also observe wage deciles for which the probability of being affected by a binding MW is zero or very close to zero, see Table 3.A6 for corresponding descriptives. Effects on these deciles cannot be driven by the previously mentioned measurement problem. Secondly, we are able to examine whether the pattern of employment effects follows the extension of the bite to higher wage deciles across time. Using again the counterfactual wage for plumbers if they worked in the roofing sector, we thus estimate the ATTs for each wage decile by exchanging p in equation (3.5) by the decile $d_{it} = 1, \ldots, 10$ and by further interacting $D_{it} \times d_{it}$ with the three sub-periods. Note in Table 3.A6 that the share of plumbers whose counterfactual wage falls in the lowest wage decile is quite high due to the wage compression in the roofing sector. Still, we have high numbers of observations in the plumbing sector for each wage decile in the roofing sector so that we are able to estimate the ATT for each decile. The approach is related to the study of Neumark et al. (2004), who also study the minimum wage effects throughout the wage distribution. Compared to our study, the authors exploit the regional and time variation of the minimum wage level in the UK and look at next years' employment status along the wage distribution, defined as the distribution of initial earnings relative to the old minimum wage.

Figure 3.4 displays the corresponding ATTs on the probability of continued employment in the roofing sector in percentage points as long as the effect is significant at least at the 5% level. The results indicate that the prospects of continued employment in East Germany have deteriorated due to the minimum wage almost along the entire wage distribution in East Germany. Moreover, note that the impact on workers with wages in the upper wage deciles are partially significant only for the latest period where the bite of the minimum wage was strongest. For West Germany, wage deciles that are not affected by a binding MW appear to be less affected by the MW. For workers whose wages fall in the 7th to 9th decile, no significant effects can be found at all. Still, there is some evidence for employment spillovers in line with the previous results because workers in the 3rd to 6th decile, for whom we find a decline in the chances of continued employment, are only marginally affected by a binding MW (see Table 3.A6). Also, the effect seems to follow the extended bite of the MW since wage deciles 5 and 6 are only affected in the later periods with a higher minimum wage level. The negative effect on continued employment of roofers in the 10th decile might be caused by voluntary quits of predominantly master craftsmen who leave the sample by deciding to become self-employed and to establish a single-person company whose share of all companies markedly increased during the observation period.

Figure 3.4: Minimum wage effect on the probability of being employed in the roofing sector in the next year by decile of the wage distribution and sub-period, by West (top) and East Germany (bottom), BA data, 1995-2007



Note: The figures only display effects that are significant at least at the 5% significance level.

This additional analysis confirms that there are relevant spillovers in East Germany whose temporal pattern confirms a link to the extending minimum wage bite. For West Germany, employment spillovers are less strong, but seem to exist for workers earning wages just above the minimum wage and for workers in the highest wage decile. These negative employment outcomes for workers with wages above the MW allow for two not necessarily competing explanations. On the one hand, the observed result pattern may suggest that workers are substituted by capital and that the substitutability differs for different types of workers with the least skilled workers being easiest to substitute. On the other hand, the result pattern is compatible with negative scale effects that mostly, if not for all workers, dominate a positive substitution effect between different types of workers. Of course, both explanations may be relevant to some extent, albeit qualitative interviews with leading experts in the roofing sector (see Aretz et al. 2011) suggest that the first may be the dominant explanation. In particular, the insiders doubt that the minimum wage in the roofing sector had much of a scale effect while the relevance of technological advances such as the introduction of new roofing systems that reduce the necessary labour input have been stressed.

The results are partly in line with Neumark et al. (2004), who find that workers whose wages are initially close to the minimum wage (up to 1.3 times the minimum wage) are most likely to be affected by changes in the wage floor. However, the authors find no evidence for spillovers in the upper part of the wage distribution, as opposed to our study.

3.8 Conclusion

In this chapter, we have analyzed the impact of minimum wages in the German roofing sector on workers' chances of continued employment. For the identification of average employment outcomes, we contrasted the estimated minimum wage impact when comparing the chances of continued employment in the roofing sector with a control sector and when comparing the chances of roofers with and without a binding minimum wage. In addition, we estimate the causal impact of the minimum wage for workers with and without a binding minimum wage as well as along the entire wage distribution. We are, thus, able to also identify indirect effects of the minimum wage on workers in the upper part of the wage distribution for whom the MW is not binding. Our main conclusions are:

- On average, the minimum wage in the roofing sector resulted in poorer chances of remaining employed according to both the intersectoral comparison as well as the comparison of workers with and without a binding minimum wage within the roofing sector. This is especially true for East Germany, where the minimum wage level gave rise to a much higher share of affected workers of up to 56% compared to 12% in West Germany. Given the limited compliance with the minimum wage regulations, the impact could even be stronger if compliance was fully enforced.

- Estimates from the comparison of workers with and without a binding minimum wage seem to be underestimated compared to estimates from an intersectoral comparison. If one is willing to assume that the common trend assumption holds and that the control sector is not affected by spillover effects, assumptions that are supported by some robustness checks, this deviation indicates that the minimum wage also affects the employment chances of roofers who are not directly affected by a binding minimum wage.

- Running an intersectoral comparison of employment chances along the entire wage distribution by exploiting the counterfactual position of workers of the control sector in the wage distribution of the roofing sector confirms this previous suspicion. The prospects of continued employment deteriorated due to the minimum wage along the entire wage distribution in East Germany. In West Germany, spillovers are less strong, but also exist for workers just above the minimum wage level.

- The decline in employment chances among workers without a binding minimum wage may indicate that scale effects dominate substitution effects and/or that minimum wages induce some capital-labour substitution. While both may be relevant to some extent, the latter may be the dominant force according to interviews that we conducted with sector insiders. In particular, they consider new roofing systems as a potential means of reducing minimum-wage induced labour costs, but question a strong decline in output since the demand for roofing services appears to be rather price-inelastic.

These findings on the impact of the minimum wage regulations on the chances of continued employment should not, however, be equated with the overall minimum wage impact on the sector's employment. In particular, the single-person companies, whose share among all companies tripled during the observation period to 23% in 2010, are not accounted for by our analysis. Furthermore, given the specific conditions of the roofing sector, e.g. the rather high level of qualification and the low labour intensity, a transferability of the results to other sectors which might be subject to minimum wage regulations in the future has to be viewed with caution.

Despite these reservations, the presented evidence clearly highlights the need for a broader perspective on employment effects of minimum wages by also taking a closer look at workers who do not appear to be affected by the minimum wage at a first glance. Moreover, our results put doubt on any attempts to identify employment effects of minimum wages by comparing workers with and without a binding minimum wage within a covered sector.

Appendix 3

3.A - Tables

Table 3.A1: Mean values of independent and dependent variables by sector prior to and after the minimum wage introduction, East Germany, BA data

	(1)	(2)	(3)	(4)	(5)
	Roofers		Plumbers		Unconditional
	before MW	after MW	before MW	after MW	DiD
	1994 - 1996	1997 - 2007	1994 - 1996	1997 - 2007	
Dependent variable: Employed in the same sector in June of next year					
1994	0.80		0.83		
1995	0.78		0.81		
1996	0.75		0.81		
Total	**0.78**	**0.74**	**0.82**	**0.79**	**-0.01**
Individual covariates					
Age	34.45	35.80	36.04	38.48	**-1.08**
Female	0.01	0.00	0.01	0.01	**0.00**
No vocational degree	0.08	0.07	0.02	0.03	**-0.02**
Secondary education	0.81	0.88	0.89	0.93	**0.04**
Tertiary education	0.00	0.00	0.00	0.00	**0.00**
Missing educational status	0.10	0.04	0.08	0.04	**-0.02**
Skilled workers	0.73	0.78	0.86	0.87	**0.04**
Unskilled workers	0.24	0.19	0.10	0.09	**-0.04**
Master craftsman	0.03	0.03	0.03	0.04	**0.00**
Part-time <15 hours/week	0.00	0.00	0.00	0.00	**0.00**
Part-time >15 hours/week	0.00	0.00	0.00	0.01	**0.00**
Prev. work exp. in sector (in years)	1.95	9.20	1.95	7.14	**2.05**
Prev. tenure in firm (in years)	1.78	7.09	1.92	8.08	**-0.85**
Firm-level covariates					
Share of skilled workers	0.73	0.76	0.86	0.83	**0.06**
Share of unskilled workers	0.24	0.19	0.10	0.09	**-0.04**
Mean age of workforce	34.46	36.00	36.00	38.65	**-1.11**
1-5 employees	0.07	0.17	0.11	0.19	**0.01**
6-10 employees	0.16	0.25	0.17	0.19	**0.06**
11-20 employees	0.32	0.29	0.22	0.20	**0.00**
> 20 employees	0.45	0.30	0.50	0.42	**-0.07**
Mean daily gross wage in firms with > 2 workers	54.95	54.91	53.32	51.66	**1.62**
Number of observations	75,227	172,249	73,178	176,752	

Note: The unconditional DiD is computed the following way: columns (2)-(4)-((1)-(3)).

Table 3.A2: Mean values of independent and dependent variables by sector prior to and after the minimum wage introduction, West Germany, BA data

	(1)	(2)	(3)	(4)	(5)
	Roofers		Plumbers		Unconditional
	before MW 1994 - 1996	after MW 1997 - 2007	before MW 1994 - 1996	after MW 1997 - 2007	DiD
Dependent variable: Employed in the same sector in June of next year					
1994	0.81		0.87		
1995	0.79		0.87		
1996	0.81		0.87		
Total	**0.80**	**0.82**	**0.87**	**0.85**	**0.03**
Individual covariates					
Age	34.92	36.52	35.77	37.62	**-0.25**
Female	0.01	0.01	0.01	0.01	**0.00**
No vocational degree	0.26	0.22	0.06	0.07	**-0.04**
Secondary education	0.65	0.73	0.89	0.91	**0.06**
Tertiary education	0.00	0.00	0.00	0.00	**0.00**
Missing educational status	0.08	0.04	0.04	0.02	**-0.02**
Skilled workers	0.61	0.64	0.83	0.81	**0.04**
Unskilled workers	0.34	0.31	0.11	0.12	**-0.04**
Master craftsman	0.04	0.04	0.05	0.06	**0.00**
Part-time <15 hours/week	0.00	0.00	0.00	0.00	**0.00**
Part-time >15 hours/week	0.00	0.01	0.01	0.01	**0.00**
Prev. work exp. in sector (in years)	2.03	10.69	2.12	8.12	**2.65**
Prev. tenure in firm (in years)	1.88	8.64	2.06	9.62	**-0.80**
Firm-level covariates					
Share of skilled workers	0.61	0.62	0.83	0.77	**0.06**
Share of unskilled workers	0.34	0.30	0.11	0.12	**-0.05**
Mean age of workforce	35.00	36.88	35.77	38.07	**-0.42**
1-5 employees	0.14	0.19	0.17	0.19	**0.03**
6-10 employees	0.26	0.27	0.19	0.20	**0.01**
11-20 employees	0.31	0.30	0.22	0.22	**-0.01**
> 20 employees	0.29	0.24	0.42	0.39	**-0.02**
Mean daily gross wage in firms with > 2 workers	69.23	72.09	71.44	72.81	**1.49**
Number of observations	125,334	375,344	137,444	471,454	

Note: The unconditional DiD is computed the following way: columns (2)-(4)-((1)-(3)).

Table 3.A3: Marginal effect of D_{it} in percentage points for placebo tests of approaches in section 3.6.1 and 3.6.2

	Pooled Logit	Pooled LPM	Obs. (in 1000)	Pooled Logit	Pooled LPM	Obs. (in 1000)
	East Germany			**West Germany**		
Placebo tests for approach in Section 3.6.1						
1994-95 with plumbers	0.3	0.2	99	-1.7***	-1.7***	181
1994-96 with plumbers	-3.2***	-3.2***	150	0.6**	0.8**	267
Placebo tests for approach in Section 3.6.2						
1995-96, DiD BA data	6.5***	5.2***	52	-0.8	-0.1	82
1995-96, DiD LAK data	1.9	1.0	66	-3.3	-2.0	109

Note: Same specification as in Tables 3.4 and 3.6 with individual and firm-level controls; Significance levels: * 5%, ** 1%, *** 0.1%

Table 3.A4: Mean difference between non-binding and binding workers of independent and dependent variables by sector prior to and after the minimum wage introduction, East Germany, BA data

	(1)	(2)	(3)	(4)	(5)
	Roofers		Plumbers		Unconditional
	before MW	after MW	before MW	after MW	DiD
	1995 - 1996	1997 - 2007	1995 - 1996	1997 - 2007	
Dependent variable: Employed in the same sector in June of next year					
1995	-0.25		-0.14		
1996	-0.20		-0.14		
Total	**-0.22**	**-0.10**	**-0.14**	**-0.07**	**-0.05**
Individual covariates					
Age	-3.16	-2.36	-3.24	-2.75	**-0.31**
Female	0.02	0.01	0.03	0.01	**-0.01**
No vocational degree	0.06	0.03	0.02	0.01	**0.02**
Secondary education	-0.05	-0.01	0.02	0.02	**-0.03**
Tertiary education	0.00	0.00	0.00	0.00	**0.00**
Missing educational status	-0.01	-0.02	-0.04	-0.03	**0.02**
Skilled workers	-0.17	-0.05	-0.06	-0.01	**-0.06**
Unskilled workers	0.18	0.05	0.07	0.03	**0.08**
Master craftsman	-0.02	-0.01	-0.02	-0.03	**-0.01**
Part-time $<$ 15 hours/week	0.00	0.00	0.00	0.00	**0.00**
Part-time $>$ 15 hours/week	0.01	0.01	0.01	0.01	**-0.01**
Prev. work exp. in sector (in years)	-0.86	-0.75	-0.61	0.32	**0.83**
Prev. tenure in firm (in years)	-0.83	-1.68	-0.43	-0.99	**0.28**
Firm-level covariates					
Share of skilled workers	0.00	0.00	-0.01	-0.01	**-0.01**
Share of unskilled workers	0.00	-0.02	0.01	0.00	**0.02**
Mean age of workforce	-0.66	-0.05	-0.85	-0.41	**-0.16**
1-5 employees	0.04	0.10	0.06	0.11	**-0.01**
6-10 employees	0.03	0.03	0.00	0.03	**0.03**
11-20 employees	0.03	-0.02	-0.02	-0.02	**0.05**
$>$ 20 employees	-0.10	-0.11	-0.04	-0.12	**-0.07**
Mean daily gross wage in firms with $>$ 2 workers	-9.00	-5.87	-9.55	-8.64	**-2.22**
Number of observations	51,605	172,090	49,426	172,982	

Note: The unconditional DiD is computed the following way: columns (2)-(4)-((1)-(3)).

Table 3.A5: Mean difference between non-binding and binding workers of independent and dependent variables by sector prior to and after the minimum wage introduction, West Germany, BA data

	(1)	(2)	(3)	(4)	(5)
	Roofers		Plumbers		Unconditional
	before MW	after MW	before MW	after MW	DiD
	1995 - 1996	1997 - 2007	1995 - 1996	1997 - 2007	
Dependent variable: Employed in the same sector in June of next year					
1995	-0.22		-0.02		
1996	-0.24		-0.08		
Total	**-0.23**	**-0.26**	**-0.04**	**-0.10**	**-0.03**
Individual covariates					
Age	-1.84	-2.21	1.05	-0.22	**-0.91**
Female	0.12	0.07	0.05	0.08	**0.07**
No vocational degree	0.11	0.14	0.03	0.09	**0.03**
Secondary education	-0.16	-0.20	-0.02	-0.10	**-0.05**
Tertiary education	0.01	0.02	0.00	0.01	**0.00**
Missing educational status	0.05	0.05	-0.01	0.01	**0.02**
Skilled workers	-0.33	-0.37	-0.06	-0.16	**-0.06**
Unskilled workers	0.16	0.20	0.03	0.10	**0.03**
Master craftsman	0.05	0.05	-0.02	-0.02	**0.01**
Part-time $<$ 15 hours/week	0.00	0.03	0.00	0.02	**-0.01**
Part-time $>$ 15 hours/week	0.12	0.09	0.05	0.06	**0.04**
Prev. work exp. in sector (in years)	-0.94	-6.09	-0.36	-2.08	**3.44**
Prev. tenure in firm (in years)	-0.93	-5.25	-0.36	-2.69	**1.99**
Firm-level covariates					
Share of skilled workers	-0.08	-0.19	0.01	-0.08	**0.02**
Share of unskilled workers	0.03	0.09	0.00	0.04	**-0.01**
Mean age of workforce	-0.65	-0.64	1.02	0.88	**-0.15**
1-5 employees	0.12	0.21	-0.10	0.00	**0.01**
6-10 employees	0.04	-0.03	-0.16	-0.10	**0.13**
11-20 employees	-0.04	-0.08	-0.13	-0.14	**0.02**
$>$ 20 employees	-0.11	-0.10	0.39	0.24	**-0.16**
Mean daily gross wage in firms with $>$ 2 workers	-15.13	-22.52	-2.61	-7.02	**2.97**
Number of observations	81,969	375,079	90,937	465,157	

Note: The unconditional DiD is computed the following way: columns (2)-(4)-((1)-(3)).

Table 3.A6: Probability of being affected by a binding MW (P_{MW}), by year, sector, and wage decile of the wage distribution in the roofing sector, BA data, 1995-2007

East Germany

Year	Sector	1	2	3	4	5	6	7	8	9	10	N
1995	Roofers	69.6	39.6	10.0	0.8	0.1	0.1	0	0	0	0	25,470
	Plumbers	74.7	36.9	8.6	0.7	0.1	0	0	0	0	0	25,208
1996	Roofers	81.0	38.9	9.4	1.1	0.1	0	0	0	0	0	26,135
	Plumbers	82.4	36.5	7.1	0.6	0	0	0	0	0	0	24,218
1997	Roofers	73.6	33.3	8.1	0.8	0.1	0	0	0	0	0	24,960
	Plumbers	75.3	31.4	5.2	0.4	0	0	0	0	0	0	23,182
1998	Roofers	63.9	29.2	8.0	1.4	0.1	0	0	0	0	0	22,489
	Plumbers	67.9	24.3	4.9	0.7	0.1	0	0	0	0	0	21,934
1999	Roofers	57.6	24.7	6.1	0.6	0	0	0	0	0	0	21,524
	Plumbers	66.4	20.6	2.5	0.4	0.1	0	0	0	0	0	21,384
2000	Roofers	73.7	52.3	28.3	9.9	1.8	0.2	0	0	0	0	18,908
	Plumbers	82.3	52.4	26.8	8.7	2.3	0.2	0.1	0	0	0	18,586
2001	Roofers	66.3	40.5	21.3	8.3	2.4	0.5	0.1	0	0	0	14,968
	Plumbers	78.9	40.3	18.7	7.7	2.3	0.5	0	0	0	0	16,164
2002	Roofers	80.3	66.6	52.5	34.9	17.1	4.0	0.5	0.1	0	0	13,076
	Plumbers	88.8	68.9	52.8	35.7	18.7	4.4	0.7	0.1	0	0	13,763
2003	Roofers	82.0	70.5	59.9	47.6	31.3	13.6	3.1	0.2	0	0	12,865
	Plumbers	88.0	70.2	60.6	47.6	30.7	13.5	3.1	0.3	0	0	12,549
2004	Roofers	85.9	75.6	67.2	56.3	42.7	26.5	10.4	1.3	0.1	0	11,681
	Plumbers	95.1	79.3	68.6	56.6	43.0	25.4	9.4	1.4	0.1	0	12,195
2005	Roofers	87.4	79.3	72.6	65.5	55.2	40.6	21.2	5.9	0.7	0	10,214
	Plumbers	86.5	76.8	70.2	63.2	53.9	40.4	20.4	5.6	0.6	0	10,806
2006	Roofers	85.7	64.0	50.8	39.8	29.8	20.8	11.7	3.6	0.3	0	10,581
	Plumbers	96.3	63.7	51.4	42.0	33.4	22.7	11.3	3.2	0.2	0	10,807
2007	Roofers	83.8	70.9	60.4	49.2	37.6	25.0	11.9	2.9	0.4	0	10,824
	Plumbers	96.8	72.0	60.3	49.3	38.1	25.2	12.8	3.8	0.4	0	11,612
N	Roofers	17,601	22,141	22,754	22,872	22,928	22,962	23,005	23,082	23,169	23,181	223,695
	Plumbers	36,831	26,062	23,183	20,713	16,990	18,468	17,675	16,391	16,578	29,517	222,408

West Germany

Year	Sector	1	2	3	4	5	6	7	8	9	10	N
1995	Roofers	15.6	1.4	0.2	0	0	0	0	0	0	0	42,730
	Plumbers	32.6	1.2	0.2	0	0	0	0	0	0	0	46,324
1996	Roofers	20.4	2.7	0.2	0	0	0	0	0	0	0	39,239
	Plumbers	28.9	2.5	0.3	0	0	0	0	0	0	0	44,613
1997	Roofers	20.3	2.2	0.2	0	0	0	0	0	0	0	38,273
	Plumbers	33.0	2.3	0.2	0	0	0	0	0	0	0	44,087
1998	Roofers	18.4	1.8	0.1	0	0	0	0	0	0	0	36,553
	Plumbers	24.1	1.8	0.2	0	0	0	0	0	0	0	46,132
1999	Roofers	28.0	2.3	0.1	0	0	0	0	0	0	0	37,868
	Plumbers	24.6	2.0	0.1	0	0	0	0	0	0	0	46,023
2000	Roofers	41.0	7.1	0.3	0	0	0	0	0	0	0	37,959
	Plumbers	48.3	5.7	0.3	0	0	0	0	0	0	0	45,715
2001	Roofers	35.0	5.9	0.4	0	0	0	0	0	0	0	35,316
	Plumbers	53.7	5.1	0.2	0	0	0	0	0	0	0	45,329
2002	Roofers	32.8	5.1	0.3	0	0	0	0	0	0	0	33,977
	Plumbers	54.5	3.8	0.2	0	0	0	0	0	0	0	42,512
2003	Roofers	44.7	8.2	0.5	0	0	0	0	0	0	0	32,922
	Plumbers	42.7	6.4	0.2	0	0	0	0	0	0	0	39,843
2004	Roofers	58.2	14.7	0.8	0.1	0	0	0	0	0	0	31,172
	Plumbers	66.8	10.9	0.3	0	0	0	0	0	0	0	39,526
2005	Roofers	67.6	22.0	2.2	0.2	0	0	0	0	0	0	29,450
	Plumbers	67.2	20.2	1.6	0.1	0	0	0	0	0	0	37,979
2006	Roofers	69.3	23.9	2.7	0.2	0	0	0	0	0	0	29,793
	Plumbers	83.8	23.7	1.7	0.1	0	0	0	0	0	0	38,713
2007	Roofers	70.8	26	3.2	0.2	0	0	0	0	0	0	31,796
	Plumbers	84.8	25.1	2.2	0	0	0	0	0	0	0	39,298
N	Roofers	28,920	45,648	47,697	47,771	47,797	47,848	47,860	47,857	47,859	47,791	457,048
	Plumbers	45,549	58,798	52,230	51,368	52,410	47,931	43,264	38,247	45,136	121,161	556,094

3.B - Figures

Figure 3.B1: Evolution of the employment across sectors by region, year 1997=100, BA data, 1994-2008

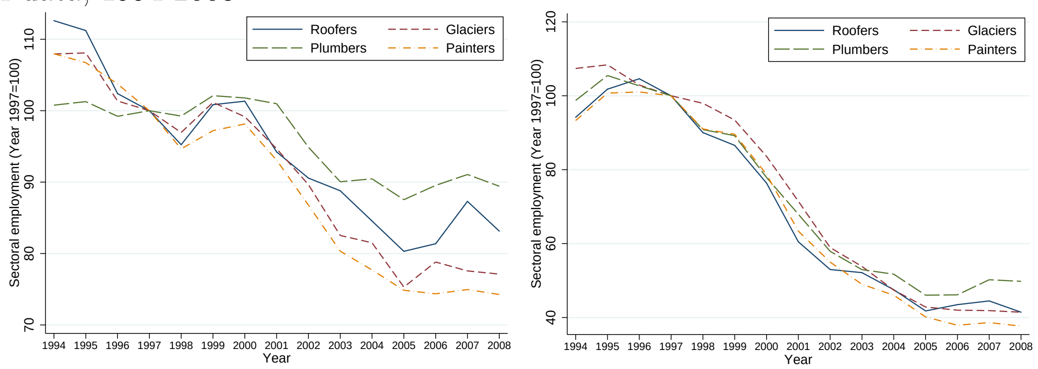


Figure 3.B2: Distributions of observed LAK data and predicted BA data wage

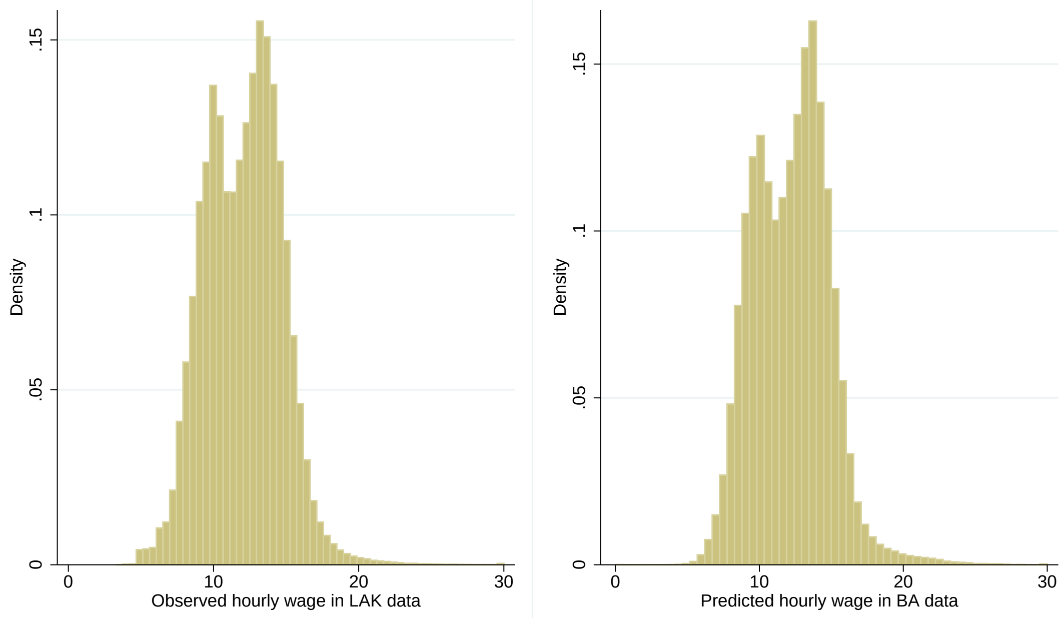
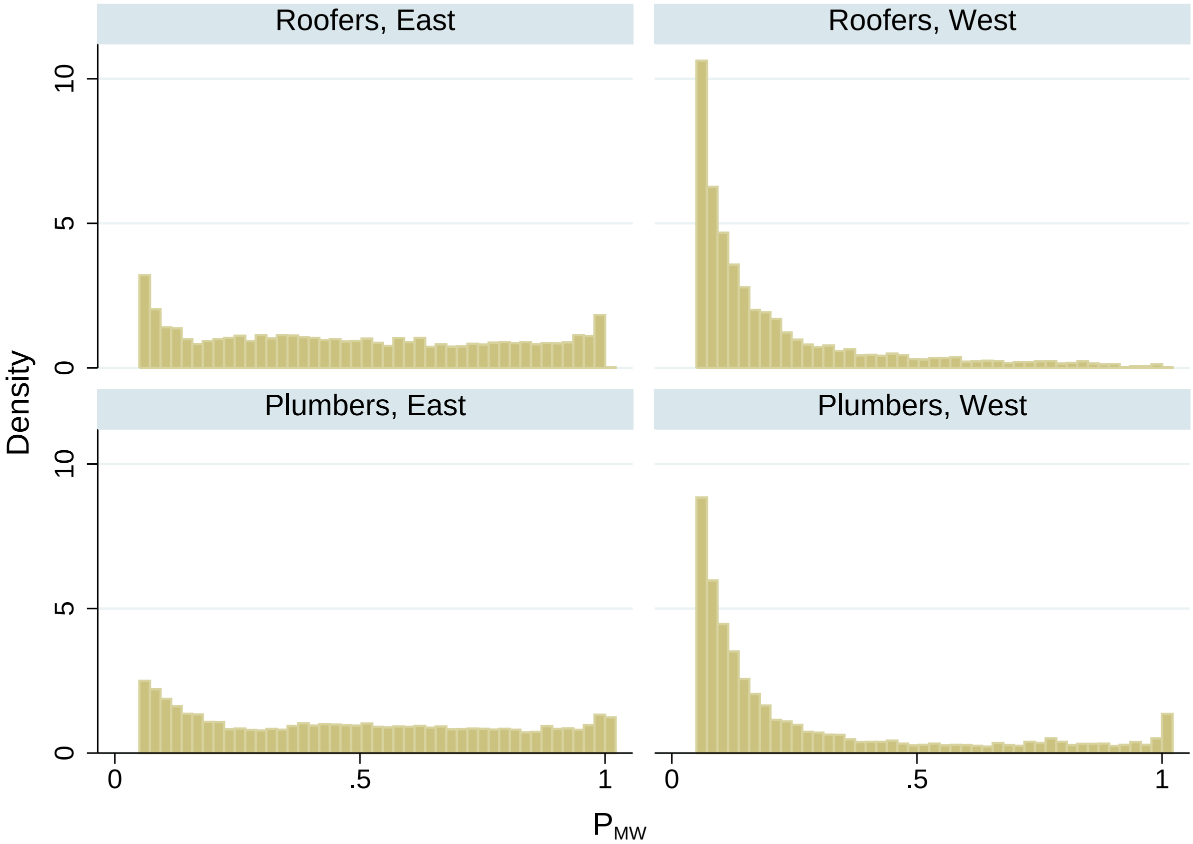


Figure 3.B3: The probability of being affected by a binding MW, by sector and region, BA data, 1995 - 2007



3.C - Explanation of the underestimation of the bite in the BA data

The compressed wage distribution which is observed in Figure 3.3 indicates that the imputation technique, which we use for implementing a hourly wage in the BA data and which assumes a normal distribution of wages, leads to a systematic underestimation of the share of workers with a binding MW in East Germany. For illustration, consider two types of workers that capture the asymmetric form of the wage distribution. Type A's wage roughly corresponds to the MW, whereas type B's wage lies somewhere above the MW level. Due to the fact that there is uncertainty which individual falls into which category, the imputed distribution of the predicted mean wage always reflects a mixing distribution of these two types. As a consequence, the imputed variance for type A is an overestimation whereas type B's imputed wage variance is an underestimation of the true variance. As a result, too much probability mass for type A is above the MW and too little probability mass for type B is below the MW, resulting in an underestimation of the share of workers with a binding MW. Still, the probability of being affected by a binding MW on an individual level should be highly related to the treatment intensity, i.e. the higher this probability on an individual level is, the more likely is an individual to fall below the MW threshold and the higher is the need for increasing the wage in order to comply with the MW level.

Chapter 4

Choosing Your Object of Benevolence? A Field Experiment on Donation Options*

4.1 Introduction

Charitable giving has been in the focus of experimental research lately (e.g. Carpenter et al. 2008, Corazzini et al. 2010, Bernasconi et al. 2009; see List 2011 for a recent survey). These studies are part of a literature stressing that charitable activities play an important role in economies. Many of these activities are financed by charitable organizations, which in turn usually rely on voluntary donations. The amount of such donations is quite substantial (e.g. in 2010, Americans gave approximately $300 billion to charitable causes).[57] The supported charitable organizations are, for various reasons, frequently rather specialized in their activities.[58] Even if the organization engages in more than one activity, the most common way

[56]*This contribution is joint work with Sebastian Kube and has been published as ZEW Discussion Paper No. 10-016 (Aretz and Kube 2010). This contribution has been accepted for publication in a Special Issue on Field Experiments in the Scandinavian Journal of Economics. We are grateful to "Doctors for Developing Countries" and especially to Dr. Kischlat for his willingness and effort to conduct the field experiment with us. For helpful discussions, we thank Luca Corazzini, Armin Falk, Erin Krupka, Michel Maréchal, Jan Stuhler, and Matthias Wibral.

[57]Source: Giving USA Foundation, Center on Philanthropy, Indiana University.

[58]For example, some support children, while others support elderly people, or medical programs in developing countries, or wildlife, etc.

to raise funds is to send solicitation letters that ask for donations to a single activity. Interestingly, however, the organizations usually do not discriminate with respect to the countries that the donors can support. In this chapter, I report evidence on a randomized field experiment that we used to study the effect of providing donors with the option to choose the target country for their donations.

The "Doctors for Developing Countries" (*"Ärzte für die dritte Welt"*, *DfDC* in the following) sent out more than 57,000 solicitation letters by mail in two different versions. Both versions were basically identical. The letter described the project work in five different developing countries and provided donors the same amount of information about the present situation in these five countries. The only difference was that donors in the control group could only specify the donation amount, while donors in the treatment group additionally were provided with the opportunity to select one or more particular countries as donation recipients. This allows us to observe if the mere option of targeting donations to specific countries already affects donations (by comparing response rates and average donation amounts of donors in the control and in the treatment group). Additionally, we can check if actually taking advantage of the option affects donation amounts (by focusing only on the treatment group and comparing those who make use of the option to those who do not specify a recipient).

The organization received 6,393 donations in total in response to the appeal for funds. Response rates did not differ significantly between groups (11.2% in control, 11.1% in treatment group). Also the average donation amounts of actual donors did not differ significantly between control (135€) and treatment group (138€); resp. 15.19€ and 15.31€ if donations of 0 are included for those who did not donate. Only 3.5% of all donors in the treatment group make use of their option to choose a specific country. Those who actually state a recipient for their donation give more. On average, their donations are about 14% higher than those of the other subjects in the treatment group (160€ compared to 138€). The difference persists when we control for donors' previous donations by using data from the organizations' two previous winter mailings.

Our study is closely related to the empirical research investigating the effects of identification on benevolence and helping behavior (e.g. Fetherstonhaugh et al. 1997, Jenni and Loewenstein 1997, Bohnet and Frey 1999, Small and Loewenstein 2003, Brosig et al. 2003). Starting with Schelling (1968), these studies (backed up by casual empirical observations) mostly support the idea that people care more about identifiable, or 'familiar', victims than about statistical victims. Several potential causes are recognized for inducing the identifiable victim effect, e.g. vividness, uncertainty, or the proportion of the reference group that can be saved. Basically, however, the mediating factor behind the effect seems to be evoked emotions. Identifiable victims evoke stronger emotional and moral reactions than (equivalent) unidentifiable victims (cp. Kogut and Ritov 2005, who find that self-reported sympathy towards the victim and willingness to help the victim are correlated). Our results point into the same direction, but the difference is that the information set provided by us is kept constant between the two solicitation letters.[59] Moreover, to the best of our knowledge the existing evidence up to now either stems from questionnaire studies (e.g. Jenni and Loewenstein 1997) or from lab experiments (e.g. Gueth et al. 2011, Andreoni and Petrie 2004), but not from

[59]By selecting particular countries as donation recipients, the donation cause is more 'identifiable' to these donors. Of course, victims are not as explicitly identified as it is usually the case in this area of research. Still, one could speak of a "weak identifiability effect", because the donor reduces the reference group from the whole population of five countries to a particular country.

a controlled field experiment.

Given that we study charitable giving in the field, our study fits into the field-experimental literature that explores mechanisms to increase donations (for recent surveys, see Bekkers and Wiepking 2011 and List 2011). For example, Falk (2007) demonstrates that gift-exchange can increase donations by comparing returns to a mail campaign with and without including small presents (one or several greeting cards). Along these lines, Landry et al. (2010) explore if a regular appeal for donation attracts different donors than an appeal that builds on gift-exchange by giving small or large gifts (a bookmark, resp. a copy of the book *Freakonomics*); additionally controlling for potential donors being on a warm or cold list (i.e. having donated money to this charity before or not). Similar controls are also used in Eckel and Grossman (2008), but they focus on pecuniary incentives. Using different rates of rebates and matching subsidies, they show that donations are a normal good with negative price elasticity, and that charities are better off under the matching than under the corresponding rebate subsidy. Landry et al. (2006) focus on monetary incentives, too. They observe that incentivizing donations with raffle tickets raises more money than a plain door-to-door campaign. It also raises more money than a campaign using seed money. Beside the monetary incentives, the social interaction between solicitor and donor seems to matter as well, since they find that physically attractive female solicitors raise more money than their peers. Also DellaVigna et al. (2012) underline the importance of the social dimension. They find social pressure to be a strong determinant of door-to-door giving, because the share of households opening the door is reduced significantly when donors are informed ex-ante about the exact time of solicitation (via flyers on their door-knobs). In contrast to these studies, our mechanism does neither change the monetary nor the social incentives structure. Instead, all it does is to extend donors' choice set for a given campaign: donors cannot only choose the size of their donation, but can also target their donation to a specific subset of countries. Compared to the strong effects in the above studies, the impact of the mechanism reported here is negligible – yet in the future it might be interesting to study its effectiveness in combination with monetary or social incentives.

4.2 Experimental Design

4.2.1 Charity

Since we wanted to provide donors with the option to choose the target country for their donation in a natural environment, we searched for a charitable organization that operated in at least two (sufficiently different) countries. Moreover, we required the organization's work in these countries to be similar[60] and we wanted to cooperate with an organization that was large enough to provide us with a sufficiently large data set. Fortunately, the German organization *DfDC* agreed to cooperate with us. The *DfDC* is officially certified by the German Donation Seal and is listed there amongst their Top 40 organizations in Germany with respect to private donation inflow.[61] The *DfDC* operates in several countries, and their work is almost identical in any country (primary health care). In 2007, they asked for donations to support five countries, namely Bangladesh, India, the Philippines, Kenya, and Nicaragua.

[60]Otherwise, observing a donor choosing a particular country might also be due to a difference in the charity's activities in that particular country. This, of course, might be interesting as well, but is beyond the scope of this chapter.

[61]cp. DZI Spendenalmanach 2008/9 p.317; for more information (in German) see the German Institute for Social Issues (*DZI*) at http://www.dzi.de

4.2.2 Method

We used the *DfDC*'s winter mailing campaign 2007 for our field experiment. Altogether, 57,372 solicitation letters were sent out to the 'house list' consisting of regular donors and members of the *DfDC*. We conducted two treatments. Allocation of subjects to treatment was randomized by the organization. Based on the first letter of the family name, subjects were either allocated to the Choice treatment (30,325 people) or to the Baseline treatment (27,407 people). In the Baseline treatment, donors could not choose their donation recipient. Instead each donation was equally split between the five countries (which, however, was not made explicit to the donors). In the Choice treatment, donors could declare which country (or countries) should receive the donated amount; the default being to support all five countries equally.

In both treatments, subjects received a solicitation letter including a cover letter and a single remittance slip which had the organization's account and bank number pre-printed on it. The same cover letter was used in both treatments. It explained the project work of the organization during the last year and explicitly mentioned the five countries for which they asked for donations in 2007. The amount and detail of information about the countries provided in the cover letter were identical between treatments.

The only difference was that on the page containing the remittance slip in treatment Choice, it was additionally explained that donors had the option to donate to a specific country and how to use it: Simply by entering five digit codes in the reference-field on the remittance slip, subjects were able to pick any (combination) of the five countries to donate to. If a single code was entered, the entire donated amount went to the recipient that the donor had selected. If more than one code was entered, the donated money was to be split equally between the selected countries. If no code was entered, the donation was treated as in treatment Baseline, i.e. it was split equally amongst all five countries.[62] The latter was the default because subjects received a remittance slip where the reference-field was initially empty.

4.2.3 Behavioral predictions

In light of previous evidence, it is ex ante an open question whether our treatment manipulation affects giving behavior. Some studies suggest that altruistic motivation is mediated by aroused empathetic emotions (see the empathy-altruism hypothesis by Batson et al. 1991, or the evidence provided by Cialdini et al. 1987, Batson 1987, or Batson and Coke 1981). In our case, this would imply that donors who have more intense feelings towards a particular country – maybe because they have some specific link to it – would donate larger amounts if they were able to target the donation to this country. Along these lines, it might also be that people care about the efficacy of donations. If they expect efficacy to differ between countries, being able to target ones' donation to the country with the highest efficacy might increase donations. Linking these arguments to existing theoretical models, one would need to build on models that allow for subjects receiving utility from

[62] One might think about other ways to implement our treatment manipulation. For example, by providing boxes to tick next to country names, or by providing the option to state separate donation amounts for each single country. These manipulations might be more salient than our treatment manipulation, so the effects that we observe might be considered a lower bound. Also note that our procedure is the only feasible way of implementing the choice option in a mailing campaign with remittance slips. The only alternative would have been to include multiple remittance slips, which increases the costs of the campaign significantly (which is why the *DfDC* opted against it).

giving, and where the size of this utility depends on beneficiaries' (i.e. countries') characteristics.[63] Whether the treatment manipulation increases giving behavior or not, however, ultimately depends on the specific theoretical model and on which exact functional forms and parameters are assumed within this model (in particular since subjects' preferences are usually assumed to be convex).[64] The only thing that is hardly reconcilable with existing theories of giving behavior is that our treatment manipulation reduces donations, because subjects can always choose not to use the option.

While our design does not allow us to discriminate between the relative importance of potential theories and channels, by comparing response rates and average donation amounts between the two treatments we can clearly isolate the effect of providing the opportunity of targeting donations to specific countries *per se*. Moreover, using data from the reference-fields on the remittance slip allows us to identify those donors who choose to target their donation, and to compare them to those who do not specify a recipient.

[63]For example, this would be the case for the model of donation behavior introduced in Landry et al. (2006), which in turn builds on impure-altruism models by Andreoni (1989) and Andreoni (1990). In this model, parts of agents' utility stems from a warm-glow effect of giving. The relative strength of this part depends on an idiosyncratic factor γ_i, which is used to describe solicitor and solicitee characteristics. In our context, one could assume it to capture beneficiary characteristics as well, i.e. the strength of the emotion towards a country.

[64]This is a common problem (not only) in the context of donation behavior. For example, Shang and Croson (2009) study the impact of social information on donations and list numerous theories of giving behavior that create contradicting hypotheses for their treatment manipulations; not only depending on the type of model, but also depending on the specification within a given model.

4.3 Results

DfDC told us how many solicitation letters had been sent out in treatment Choice, resp. Baseline. They also provided anonymous data of those subjects who actually did donate money in our field experiment in 2007. For each of those subjects, the data set includes the date that the *DfDC* received the donation, the code that was specified by the donor in the reference-field on the remittance slip, the donated amount, a dummy variable indicating if the donor was part of treatment Choice or of treatment Baseline, and an abstract id which allows to identify subjects over time. Additionally, the data set includes donations of our subjects in the previous two winter mailing campaigns (2005 and 2006).

Table 4.1: Donation behavior between treatments

Treatment	Baseline	Choice	
Recipient specified?	n/a	no	yes
# of letters sent	27,047	30,325	
# of donations	3,036	3,239	118
Response rate	11.2%	11.1%	
Average donation size	15.19€	15.31€	
... excluding non-donors	135.37€	137.51€	160.39€
Total donations	410,975€	445,418€	18,926€

Let us first focus on subjects' behavior in our field experiment in 2007. Table 4.1 provides an overview of the donation behavior in the two treatments. Our data set includes 3,036 donations in treatment Baseline and 3,357 in treatment Choice. The response rates of 11.2% (3,036 out of 27,047), resp. 11.1% (3,357 out of 30,325), are almost identical in both treatments (χ^2-test, $p = 0.556$, two-sided). This suggests that the increased decision scope in treatment Choice does not affect subjects' decision to become a donor. Also the average size of the donations does not differ significantly between treatments (ranksum-test, $p = 0.8042$, two-sided). The 3,036 donors in treatment BASELINE donate on average 135€ (s.d.=276). The 3,357 donors in treatment CHOICE give on average 138€ (s.d.=302).[65]

Result 4.1 *Providing donors with the possibility to choose their object of benevolence did not seem to affect subjects' donation behavior in general. Neither the response rates nor the average size of the donations differed significantly between treatments* BASELINE *and* CHOICE.

We observe that only 3.5% (118 out of 3,357) of the donors in treatment CHOICE actually make use of their choice option.[66] Those 118 donors who do choose their object of benevolence give on average 160€ (s.d.=299). If we compare this to the average donation of the 3,239 subjects who did not state a recipient in treatment CHOICE (138€), the difference is significant (rank-sum test, $p = 0.0172$, two-sided). The same holds true if we compare it to the 135€ that are on average donated by the 3,036 subjects in treatment BASELINE (rank-sum test, $p = 0.0261$, two-sided). Comparing all three groups jointly using a Kruskal-Wallis test yields $p = 0.0571$. This suggests that donors in the treatment group CHOICE who actually choose a recipient for their benevolence donate significantly higher amounts.

One might argue that unobserved heterogeneity might confound the causal treatment effect. For example, it could be that donors who donate higher amounts in general are for some reasons more likely to choose a recipient. To shed light

[65] The difference between treatments stays insignificant if one compares the average donation over the entire sample, thus taking also the non-donors into account. In that case, average donation size is 15.19€ in BASELINE and 15.31€ in CHOICE.

[66] All but one of the 118 subjects did specify a single country to be the recipient of the donation.

on this issue, we ran panel regressions using also subjects' donation data from the previous winter-mailing campaigns, see Table 4.2 for the regression results.[67]

We estimate a fixed-effects model, where the dependent variable is the donated amount of individual i in period t (2005, 2006, or 2007).[68] We observe the following: i) on average, donations in 2007 are per se slightly higher than in previous years (5.66); ii) those subjects who are in the treatment group, but did not specify a recipient donate about the same amount in 2007 than they did in previous years (2.08); iii) but those subjects who did use the option donate significantly higher amounts than they did in previous years (62.25). This indicates that the latter effect is unlikely to be solely due to selection, i.e. only more generous persons choosing to select a recipient.

This can also be seen when comparing the average donations in previous years of subjects in the groups Baseline, Choice & "non-user" and Choice & "user" (cp. Table 4.3). There are no significant differences, neither between nor across any of the three groups in 2005 and 2006 (rank-sum test and Kruskal-Wallis test, respectively, $p = 0.3343$ or above). Only in 2007, during the campaign in which we implement the treatment, the difference in the average donation of the treated who use the option is significantly higher than for the treated who do not choose

[67] The data contains a unique identification code for donors' addresses that allows us to trace individuals' donation behavior across years. Not all of our subjects that participated in the field experiment in 2007 have donated in previous years, so we include only those subjects who have donated at least once in the two previous years. Estimating the model using the unbalanced panel with all 12,578 observations yields qualitatively the same results; only the coefficient of the constant changes slightly to 136.75 (s.e. 1.42). Also note that donations in 2007 of treatment and control group do not vary systematically with the instances of previous donations. Interestingly, however, subjects who choose a recipient for their donations do only give a significantly higher amount in 2007 (compared to those who did not choose) if they had already donated before. The average donation of first-time donors is not significantly different from the average donation of second or third-time donors.

[68] Testing the random effects versus the fixed effects model using a Hausman test argues strongly for the fixed effects model ($\chi^2 = 31.34, p = 0.0000$).

Table 4.2: Regression table: Donation behavior

Donated Amount	
2007	5.66
	(3.04)
2007 x Treated	2.08
	(4.43)
2007 x User	62.25*
	(24.85)
Constant	142.13***
	(0.89)
Number of observations	10,575
Number of individuals	4,210
Prob>F	0.0000

Notes: This table reports coefficient estimates and clustered standard errors on the individual level in parentheses. The dependent variable is the amount donated by subject i in year t. The model is specified as a fixed-effects model with standard errors clustered on the individual level. *2007* is a dummy variable which equals 1 for the campaign in 2007 in which our field experiment was run (and 0 otherwise). *Treated* is a dummy variable which equals 1 if the subject was part of treatment CHOICE (and 0 otherwise). *User* is a dummy variable which equals 1 if the subject used the option to specify a donation target. *2007 x Treated* and *2007 x User* are the corresponding interaction terms. Significance levels are denoted as follows: $^{*}p < 0.05,^{**}p < 0.01,^{***}p < 0.001$. The regression only includes those subjects who have donated at least once in the two previous years, leaving us with 2,026 subjects in the control group (1,046 in 2005 and 2006 and 980 in 2005 or 2006), 2,110 subjects in the treatment group who did not use the option (1,083 in 2005 and 2006 and 1,027 in 2005 or 2006) and 74 subjects in the treatment group who did use the option to target their donation (26 in 2005 and 2006 and 48 in 2005 or 2006).

a recipient (rank-sum test, $p = 0.0172$; resp. $p = 0.0571$ when comparing all three groups in 2007 using a Kruskal-Wallis test).

Result 4.2 *Donors in the treatment group* CHOICE *who actually choose a recipient for their benevolence donate significantly higher amounts. Moreover, they donate significantly higher amounts than they did in previous years, while sub-*

jects who are given the option, but do not use it, do not significantly alter their donations.

Table 4.3: Donation histories

Treatment	BASELINE		CHOICE			
Recipient specified?	n/a		no		yes	
Campaign	Average donation	N	Average donation	N	Average donation	N
2005	145€	1430	144€	1455	130€	51
2006	148€	1642	142€	1738	141€	49
2007	135€	3036	138€	3239	160€	118

Concerning the previous result, it is important to consider at least two potential confounds (we thank an anonymous referee for pointing this out to us). First, we cannot rule out that time variant factors (e.g. income) have changed differently across groups over time, which could potentially lead to biased estimates in our regressions. Second, given the nature of a mail fundraising campaign and of the mechanism that we study, one is inevitable confronted with sample attrition. This implies that whether a donor actually responds and uses the option might not be random, but instead might be related to some underlying donor characteristics that we cannot observe.

4.4 Discussion

The results reported in this chapter might be particularly interesting for charitable organizations that make use of mailing campaigns. On average, providing donors with the option to choose their donation recipient does not increase total donations. However, those donors who do use the option donate significantly higher amounts than they did in previous years. While our field experiment clearly demonstrated these effects, more data is needed to sort out potential explanations and underlying channels that drive behavior. In particular, why did most people prefer to leave the reference-field on the remittance slip blank, thus sharing their donations equally among the target countries instead of choosing a single recipient? This might reflect a reluctance to consider the important tradeoff whom to help.[69] It might also be that our treatment manipulation was not salient enough, or that the steps that one had to take for choosing a target were too "complicated" – in which case our results should be considered a lower bound. In future research, it might be interesting to see what happens, for example, if donors are forced to make a decision, or if donors can state multiple amounts instead of choosing only a single donation amount. It might also be interesting to get additional data on socio-demographics or character traits from those subjects who did and from those who did not donate (see, for example, Landry et al. 2006, or DellaVigna et al. 2012, who gather more complete data through door-to-door elicitation that directly allow for observation of a number of relevant subject traits). This would allow to study selection effects in more detail, e.g. to check if different donors are attracted by a campaign where money can be targeted to a specific country. Along these lines, one might also want to collect data on actual empathy towards the countries included in the choice set. Also repeated usage of our design might help to reveal potential selection, resp. long-run composition effects of the solicitation design on donors (see, for example, Landry et al. 2010 for this approach but studying different mechanisms).

[69] For example, Ritov and Baron (1999) report evidence that people prefer not to decide on a single recipient due to a reluctance to consider tradeoffs when those concern important, 'protected' values.

Chapter 5

Small is Beautiful – Experimental Evidence of Donors' Preferences for Charities*

5.1 Introduction

When individuals make a real-life donation decision, they usually do not have precise information about a charity's income streams. They do not know whether and how much their neighbors or other people in their social community donate to a certain charity. Furthermore, it is questionable whether they are aware of the exact amount of government subsidies given to that charity. They may rather have a belief about the charity's size in terms of entire revenues, i.e. whether it is small or large. In this chapter, I report evidence on how we explored whether the size of a charity increases or decreases the willingness to donate to that charity. To this end, we conducted a donation experiment where we provided potential donors with information about the charities' revenues.

With regard to the impact of information about a charity's revenues on chari-

[69]*This contribution is joint work with Sarah Borgloh and Astrid Dannenberg and has been published as ZEW Discussion Paper No. 10-052 (Borgloh et al. 2010a). We thank Bruno Frey, Martin Kocher, Andreas Lange, Susanne Neckermann, Bodo Sturm, Christian Traxler, Joachim Weimann, and Andreas Ziegler for very helpful comments and suggestions.

table contributions, various approaches may be relevant. So far, most theoretical models and empirical studies have analyzed either the effects of government contributions or those of other individuals' contributions on private donations. The public goods model predicts complete or incomplete crowding out of voluntary contributions by government financial support. On the other hand, the approaches of quality signaling and conditional cooperation predict that donations increase with others' contributions. The experimental evidence hints at incomplete crowding out of private donations by government subsidies, while several studies on social information find a positive relation between others' contributions and those of one's own. Unlike other approaches, the model of impact philanthropy explicitly models the effect of an increase in a charity's entire revenues - i.e. its endowment - on donations. As the charity's endowment goes up, the impact philanthropist's utility decreases because the relative impact of his or her donations is reduced.

In this chapter, a framed field experiment is presented where a non-student subject pool was asked to make a real donation decision. Half of the subjects could choose whether to give to a charity with relatively low annual revenues or to a charity with relatively high annual revenues. We thereby present evidence on the overall effect of a charity's endowment on private donations and show a negative relation between the two.

The outline of this chapter is as follows: Section 5.2 summarizes the findings of the relevant theoretical and empirical literature and motivates our experimental framework. Section 5.3 describes the experimental setting before Section 5.4 delivers the results of the experiment. Section 5.5 concludes.

5.2 Background and Motivation

Third-party contributions to a charity may stem from government subsidies or other individuals' donations, respectively. So far, several theoretical models and empirical studies have separately looked at the effects of either government contributions or others' contributions on private donations.

The standard public goods model (Warr 1982, Roberts 1984, Bergstrom et al. 1986), where an individual derives utility from private consumption as well as the total supply of the public good, predicts that private contributions to the public good are completely crowded out by government contributions to the same good. It is reasonable, though, to assume that a potential donor also derives positive utility from the mere act of contributing. Andreoni (1989, 1990) coins the term 'warm glow' to describe such preferences, where an individual's utility increases with the contributed amount. In this case, government contributions are not a perfect substitute for voluntary contributions, which implies that the former crowd out the latter only incompletely.

The empirical evidence on the theoretical predictions of crowding out is mixed. While many studies find evidence for incomplete crowding out (among others Ribar and Wilhelm 2002, Gruber and Hungerman 2007, Andreoni and Payne 2011)[70], there is also empirical evidence for crowding in of voluntary contributions (e.g. Khanna et al. 1995, Khanna and Sandler 2000). Brooks (2000) uses data for the special case of symphony orchestras and finds evidence for crowding in at low levels of government funding and crowding out at high levels of government funding indicating a non-linear relationship of private giving and government

[70]See Steinberg (1991) for a literature review

funding. Furthermore, the majority of lab experiments, which test the hypothesis of complete crowding out, find evidence for partial crowding out of voluntary contributions (Andreoni 1993, Bolton and Katok 1998, Chan et al. 2002, Konow 2010).

As charities do not only earn income from government contributions, but also from individuals' private donations, further theoretical approaches have to be taken into account. One approach is to model contributions by other individuals as a signal of the charity's quality as Vesterlund (2003) suggests. Typically, donations are not made simultaneously, but rather in a sequential manner, where high donations by other individuals signal a high-quality charity which may induce donors to give larger amounts to that organization. Andreoni (2006) remarks that leadership gifts may also be perceived as a signal for the respective charity's quality. Furthermore, the phenomenon of conditional cooperation predicts that individuals will be more willing to contribute if they know that others contribute (Fischbacher et al. 2001). Several natural field experiments deliver evidence that information about other individuals' contributions affects donations positively, e.g. Frey and Meier (2004), Croson and Shang (2008), Shang and Croson (2009), and Martin and Randal (2008).

In his theory of impact philanthropy, Duncan (2004) explicitly models how a change in a charity's endowment affects individual donations. An impact philanthropist wants his or her donation to have a distinct effect on the supply of a charitable good and thus to "personally make a difference". According to Duncan (2004), the revenues needed for the production of a charitable good consist of the charity's endowment e and the individual's contributions g. The production function $Z(y)$ with $y = e + g$ satisfies $Z^{'} > 0$ and $Z^{''} < 0$. The utility function of the impact philanthropist is $V = U(w-g) + f(\theta)$ where w is the individual's wealth, $U' > 0$ and $f' > 0$ and $\theta = Z(e+g) - Z(e)$ is the impact of the philanthropist's donation. Because

$$\delta V/\delta e = f'(\theta) \cdot [Z'(e+g) - Z'(e)] < 0 \quad \text{if } g > 0, \tag{5.1}$$

an increase in the charity's endowment decreases the impact philanthropist's utility; the importance and the impact of the philanthropist's donation are reduced. It then may be that an impact philanthropist - if provided with the choice between two charities of different size - chooses to give to the charity with smaller income streams because this strengthens the relative impact of his or her gift. The model of impact philanthropy, however, does not lead to clear predictions how a change in the endowment of a charity would affect the size of the gift.[71] It can be shown that $\delta g^*/\delta e > -1$, where g^* is the contribution which maximizes the impact philanthropist's utility, so the direction of the effect is not clear a priori. Moreover, Duncan (2004) emphasizes that an impact philanthropist dislikes the administrative costs of a charity to be financed by his or her contribution because this also reduces the charitable impact of the donation.

Our approach differs from previous experiments in two important aspects. First, the information presented to each subject in our experiment consists of an interval stating the yearly revenues received by an organization which comprises donations, membership fees and public subsidies, i.e. its endowment. We deem this kind of information to be very close to the situation potential donors find themselves in the real world as they usually cannot distinguish the size of other donors' gifts and government subsidies. The information is provided to distinguish

[71] To keep our remarks as concise as possible, the interested reader is referred to Duncan (2004) for a more detailed description of the derivation of this result.

charities solely by their size. We empirically test the prediction of the model of impact philanthropy by offering subjects two charities of different size for the same charitable cause. If an increase in the endowment does affect utility negatively, subjects should choose the smaller charity. Moreover, we test how a change in the charity's endowment affects the size of the gift and we compare the donation decision of subjects who receive information about the charity's endowment with those who do not receive this information.

Second, we use a framed field experiment. Unlike in a natural field experiment, subjects in a framed field experiment undertake the task in an artificial environment and know that they are part of an experiment (Harrison and List 2004). Although this may bias the subjects' behavior to some extent, we can make use of the advantages of framed field experiments in terms of more control and the elicitation of personal characteristics of our participants. In addition, we can exploit the fact that the donation decisions are made completely anonymously in our setting. In door-to-door-fundraising, solicitation letter campaigns or other kinds of donation campaigns the identity of the donor is usually known to the organization. By means of our double-blind procedure, neither other experimental subjects nor the experimenter know the decision made by a certain participant. This enables us to rule out an experimenter effect or certain motivations such as signaling of wealth (Glazer and Konrad 1996) or social approval (Holländer 1990). That such social incentive effects can arise from removing anonymity or increasing visibility is shown in the field (Soetevent 2005, 2011) as well as in the lab (Hoffman et al. 1994, Andreoni and Petrie 2004).

Moreover, framed field experiments are characterized by a non-student subject pool and field context in the commodity and therefore offer more realism than conventional lab experiments (Harrison and List 2004). A weakness of lab experiments is often seen in the low representativeness of the sample and, thus, the lacking generalizability of results. Especially in the case of donation decisions representativeness may be important. Carpenter et al. (2008), for example, show that students in a lab experiment tend to be less likely to donate to a charity than members from the broader community. In addition, a more representative sample offers the possibility to analyze the impact of the socio-demographic characteristics on charitable contributions.

5.3 Experimental Design

5.3.1 Implementation and participants

For subject recruitment, invitation letters were randomly distributed in the city of Mannheim. The letter contained an invitation to take part in a scientific study and informed people that they would receive 40€ for participation. It was announced that there would be a kind of survey in which they could (voluntarily and anonymously) make consumption decisions. We used a relatively high show-up fee in order to avoid underrepresentation of people with high opportunity costs of time. Furthermore, we already emphasized in the invitation letter that the money was a reward for participation in the study in order to make people feel entitled to their endowment and to avoid a bias due to unexpected gift money. The experiment took place in July 2009 on the premises of the ZEW.

A total of 223 participants took part in the experiment. At the beginning of each session, the participants individually drew lots to determine their ID number (which remained unknown to other participants and the experimenters) and chose a table. The tables had privacy screens on every side to ensure private decisions and answers. The participants were not allowed to talk to each other. If they had questions, the experimenters answered them privately. The 12 experimental sessions lasted around 60 minutes each.

Within one session, all subjects performed exactly the same task. At first, all participants obtained detailed instructions about the course of the experiment, see Appendix 5C. The main features were orally repeated. We emphasized that all information given in the instructions were true. Each participant filled out a questionnaire with questions about socio-demographic characteristics, their donation habits, and their attitude toward their own social standing within society and toward government responsibilities. The attitudinal questions were taken from the German General Social Survey (ALLBUS) which is conducted every two years with a representative sample of the German population.[72] At the end of each session, the participants had the chance to comment on the experiment and to give reasons for their decisions, see Figure 5.B1 for an overview of the experimental proceedings.

The participants' socio-demographic characteristics are shown in Table 5.A1. The subject pool is highly diversified with, for example, age ranging from 18 to 75 years. Although it is not fully representative of the German resident population, it is sufficiently diversified in all socio-demographic variables in order to examine the influence of each variable on charitable behavior. Moreover, in case of gender, income, and religion, the distribution of our subject pool does not significantly differ from that of the German population (binomial test, chi squared test, t-test,

[72]For detailed information, see http://www.gesis.org/en/services/data/survey-data/allbus/.

p≥0.1).[73] More precisely, 46.2% of subjects are male. 22.9% dispose of a monthly net household income of less than 1,000€ most of the subjects live in households with incomes between 1,000€ and 3,000€ and only 13.0% have more than 3,000€ per month disposable. With regard to religion, Catholics (31.4%) and Protestants (31.8%) are equally represented, whereas 6.7% possess another religious affiliation and 30.0% of all subjects do not belong to any religious community. The participants' responses to questions regarding their giving behavior in the past as well as their attitudes are displayed in Tables 5.A2 and 5.A3, respectively.

5.3.2 Treatments

The experiment comprised two treatments which both contained a real donation stage where the subjects simultaneously and independently decided how much (if any) of their endowment to donate to a certain charity. The subjects were informed that all of the selected charities have obtained the 'DZI Spendensiegel', a label for charities that use their funds economically and according to their statutes.[74] The subjects could choose one of four charitable causes, namely disabled care, development aid, medical research, and animal protection. To avoid any reputation effects, the subjects knew only the purpose but not the name of the organizations. The donation decision was completely voluntary and anonymous. We used a double-blind procedure in which neither the other subjects nor the experimenters came to know if, how much and to which cause a subject donated.

The subjects received a large envelope containing two small envelopes and the endowment of 40€ broken into two 10€ notes, one 5€ note, six 2€ coins, and

[73]Unless stated otherwise, all tests in this chapter are two-sided.
[74]For more information (in German language), see www.dzi.de.

three 1€ coins. This breakdown enabled subjects to donate any integer amount between 0 and 40€ and abated incentives to only give the coins. The subjects placed the amount they wished to donate in one of the small envelopes assigned to donations, labeled the envelope with their ID number and, in case they were willing to give a positive amount, the charitable cause to which they wished to donate. The amount of money the subjects wished to keep for themselves was placed in the other small envelope. Afterwards, the subjects dropped the sealed envelope specified for donations in a box.

The baseline treatment ("NoInfo") with 113 participants involved the above described donation stage and the completion of the questionnaire. The 110 participants in the treatment "Info" were informed not only about the charitable cause of the organizations but also about the total revenues in 2006, which comprise donations, membership fees and public subsidies. For each charitable cause, we offered two organizations, one relatively small organization with revenues between 40,000 and 300,000€ and one relatively large organization with revenues between 5 million and 11 million Euro. Thus, the subjects in this treatment could choose one of eight organizations for their donation. All the donations made during the experiment were transferred in full to the respective organizations. In case of the NoInfo treatment, donations were equally assigned to small and large organizations of the same cause. The counting of donations and the transfer to the organizations were notarially monitored and certified. This procedure and the name of the notary were also announced in the experimental instructions.[75]

[75]Some participants also completed another task (a dictator game) in the experiment which is not part of this contribution, but is explained in detail in Borgloh et al. (2010b). As this task did not affect the donation decision, we pooled the data.

5.4 Results

5.4.1 The Effects of charities' size

In total, 1,225€ are donated to the charities. The mean donation per participant is 5.49€ or 13.7% of the endowment, the median donation is 3.00€Ḃroken down by purposes, 448€ are donated to disabled care, 318€ to development aid, 74€ to medical research, and 185€ to animal protection. Disabled care is not only the purpose which is selected most frequently (21%), but which also receives the highest average donations 9.53€. Whereas average donations do not significantly differ between the four purposes, animal protection is the only charitable cause which is chosen with a probability significantly below 0.25 (binomial test 5% significance). Overall, 33% of the subjects do not make a donation at all. Table 5.1 contains the descriptive statistics of the donation decisions.

Table 5.1: Descriptive statistics

	N	Share (in %)	Total donation (in €)	Average donation (in €)
No donation	74	33	0	0
Donation	149	67	1,225	8.22
Disabled care	47	21	448	9.53
Development aid	39	17	318	8.15
Medical research	38	17	274	7.21
Animal protection	25	11	185	7.4
Total	223	100	1,225	5.49

In the NoInfo treatment in which the subjects do not obtain information about charity revenues, the mean donation per participant is 5.56€. In the Info treatment in which subjects obtain this information, the mean donation is 5.43€, see Table

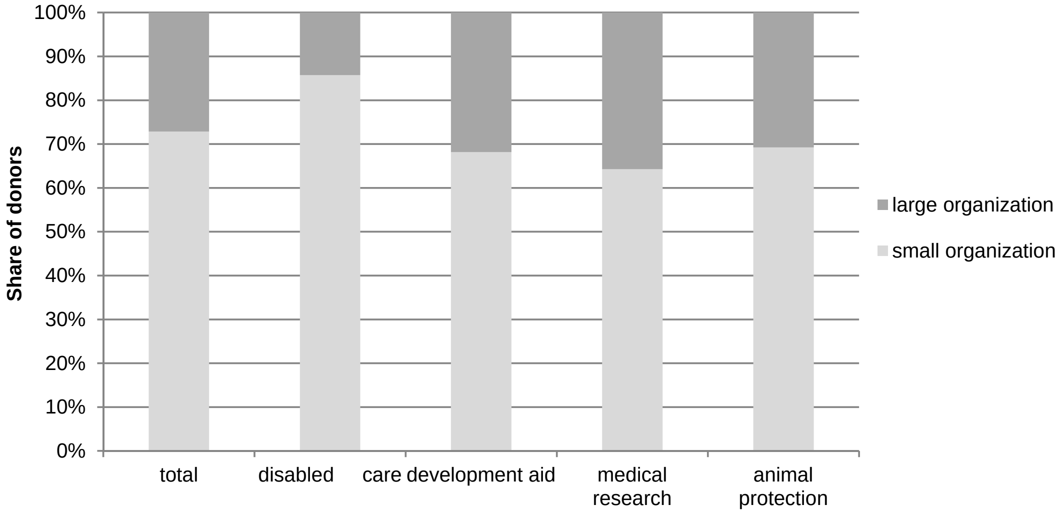
5.2. Interestingly, providing participants with the information about a charity's revenues and giving them the opportunity to choose between charities of different size neither has an impact on individual donations nor on the probability to select a certain charitable cause. However, it shifts donations within the group of subjects who are given the choice and the information; 455€are donated to the small organizations and only 132€are donated to the large organizations. On average, the participants donate 8.92€to the small organizations and 6.95€to the large organizations; this difference, however, is not statistically significant.

Table 5.2: Descriptive statistics - NoInfo versus Info Treatment

	N	Share (in %)	Total donation (in €)	Average donation (in €)
NoInfo treatment	113	100	628	5.56
No donation	35	31	0	0
Donation	78	69	628	8.05
Info treatment	110	100	597	5.43
No donation	39	36	0	0
Small organization	51	46	455	8.92
Large organization	19	17	132	6.95
Total	223	100	1,225	5.49

Out of the 110 subjects who receive the information and make a positive donation, 73% choose the small organization, and only 27% choose the large organization. Thus, the shift of donations occurs mainly because the small organizations are selected more frequently than the large organizations (chi squared test 1% significance). We observe this effect for all the charitable causes, but if we look at each cause separately it is only significant for disabled care (chi squared test 1% significance, compare Figure 5.1). Indeed, the preference for the small organizations appears to be very pronounced in the case of disabled care; here, 86% of donors choose the small organization while only 14% choose the large one. In case of development aid (medical research, animal protection), 68 (64, 69)% of donors select the small organization.

Figure 5.1: Selection of organization size in the Info treatment (in % of donors)

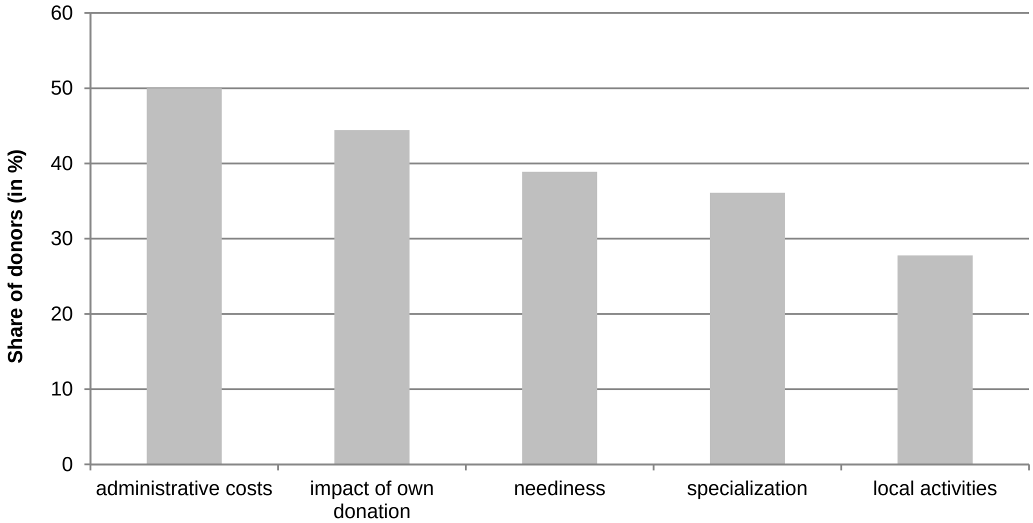


Overall, this result supports the prediction derived from impact philanthropy theory that an increase in a charity's endowment decreases the donor's willingness to give to that charity. So, if provided with the choice of charities of different size which serve the same charitable cause, individuals tend to prefer the small ones. However, there may be some other possible reasons for this preference which are not captured by the impact philanthropy model. For example, Fong and Luttmer (2009) show that people who feel close to their racial or ethnic group donate substantially more when the recipients are of the same race than when they are from a different race. Similar reasoning may hypothesize that people who feel close to their region are more likely to donate to small charities if they associate them with more local activities. Furthermore, it may be that in case of medical research, for example, donors deem a large charity to be more effective than many small charities in fighting diseases whereas they prefer smaller and possibly more locally oriented charities in the case of disabled care.

For this reason, we conducted an ex-post online survey with the subjects who participated in the Info treatment. The survey was completely anonymous and contained questions about the decisions in the experiment, namely (i) whether subjects donated a positive amount, if so (ii) to which charitable cause, (iii) to a small or a large organization, and given that choice (iv) for what reason they chose the small or the large organization. All questions offered predetermined answers including the option "I cannot remember". If participants had chosen the small organization, they were provided with the following answers: "For my decision to donate to the small organization, it was decisive that (a) my donation to the small organization has a higher impact compared to a large organization, (b) small organizations are discriminated against compared to large ones and therefore need more support, (c) small organizations have lower administrative costs compared to large ones and therefore my donation is more likely to benefit the actual charitable cause, (d) small organizations are more likely to act on a local level compared to large ones, (e) small organizations are more specialized in certain fields of activity compared to large ones, (f) other reasons."

If participants had chosen the large organization, they were provided with the following options: "For my decision to donate to the large organization, it was decisive that (a) the large organization was able to already collect many funds (consisting of donations, membership fees and public subsidies), (b) large organizations can achieve more with my donation than the small ones, (c) large organizations have a higher level of familiarity compared to small ones, (d) large organizations are more likely to act professionally compared to small ones, (e) other reasons." In both cases, the order of the predetermined options varied randomly between participants, they could select several options and give further reasons in an open description field.

Figure 5.2: Reasons to choose the small organization (in % of donors)



Out of the 104 individuals who were invited to the survey 81 individuals took part.[76] The statements made in the survey are consistent with the observed behavior in the experiment, i.e. there are no significant differences between the survey data and the experimental data. For example, the 68% of responders stating in the survey that they donated a positive amount correspond to 64% who in fact donated a positive amount in the experiment. The reasons which are mentioned most frequently for the decision to choose a small organization are lower administrative costs (50%) and a possible higher impact of the own donation (44%), see Figure

[76]As an incentive to participate, everyone who completed the survey took part in a drawing for 5 times 30€. A few people completed the survey via mail because they did not provide an email address. Six participants in the Info treatment were not invited to the survey because they did not provide any contact details.

5.2. Recall that both of these motives are captured by the impact philanthropy theory. A further reason which is mentioned frequently is the neediness of small organizations (39%), indicating that crowding out considerations may also play a role.

Regarding the choice of the large organization, the most frequently stated reason is the professionalism of large organizations (86%) followed by the achievement of objectives (43%) and the apparent ability to acquire funds (29%). All these motivations support the quality signaling approach. However, this signal attracts only few donors in our experiment.

5.4.2 The effects of individual characteristics

In the following, we report the results from a series of econometric estimations to explore the impact of various socio-demographic variables which have been surveyed in the questionnaire.[77] Around 33% of the subjects decided not to donate, hence there is a large number of observations clustered at zero donations. In this case, ordinary least squares estimates would not be accurate, so we conduct a maximum likelihood estimation of a Tobit model. In the baseline estimation, we include the following socio-demographic variables: age, household size as the absolute number of household members including children, dummy variables for male subjects, unmarried subjects, subjects not having any religious affiliation (no religion), voters of the left party, highly educated subjects (owning a graduate degree), and high income subjects (monthly net household income of 2,000€ or more).

[77]The number of observations in the econometric analysis corresponds to the number of subjects with complete socio-demographic information.

We additionally include four attitudinal variables taken from the German General Social Survey (ALLBUS) to control for one's perceived standing within society and the attitude towards the state. More precisely, the variable *position* is a dummy variable for subjects thinking they receive their fair share or more compared to others living in Germany. The variable *disparities* is coded as '1' for those subjects believing that the social disparities in Germany are just. The variable *state resp* is a dummy for subjects who want the state to care for a good living in case of illness, misery, unemployment, and old age. Similarly, the variable *equalize* takes the value 1 if a subject indicates that it is the responsibility of the state to reduce income disparities. Although it is quite common to include attitudinal variables in econometric estimations (e.g. Corneo and Grüner 2002), the causality between these variables and the dependent variable (donations) may run in both directions, i.e. these variables may be endogenous.

For this reason, Table 5.3 displays both estimations without and with attitudinal variables (column (1) and (2)) in order to show whether effects are robust to this modification. The specification in column (2) furthermore includes a dummy variable for the subjects who already made a charitable donation in the year 2009 (*donor 2009*) in order to control for offsetting effects. Furthermore, we run both estimations excluding outliers (column (3) and (4)). Outliers are defined as those subjects donating more than half their endowment (20€, five subjects).

Our results show a positive and highly significant effect of age on charitable contributions, whereas the coefficients for male donors and household size are not significant. These findings are robust across all four specifications. Moreover, across all four estimations, the voters of the left party - which tend to assign the responsibility for tackling social issues to the government - give significantly smaller

Table 5.3: Tobit estimation results

Dependent variable: amount donated				
	(1)	(2)	(3)	(4)
Variables	Including outliers		Excluding outliers	
Age	0.232***	0.236***	0.170***	0.183***
	-3.760	-3.780	-3.461	-3.648
Male	-1.563	-1.658	-0.773	-1.083
	(-1.094)	(-1.147)	(-0.689)	(-0.950)
Household size	-0.0062	-0.125	-0.298	-0.461
	(-0.00738)	(-0.147)	(-0.451)	(-0.686)
Unmarried	6.419***	5.893***	4.193***	4.099**
	-3.201	-2.939	-2.646	-2.572
No religion	-1.279	-1.200	-3.179**	-3.120**
	(-0.812)	(-0.762)	(-2.522)	(-2.457)
Left party	-9.109***	-9.315***	-6.822***	-6.611***
	(-2.996)	(-2.996)	(-2.899)	(-2.747)
Education	3.991***	3.962**	2.187*	2.271*
	-2.622	-2.593	-1.834	-1.890
Income	4.695***	4.614***	3.357**	3.353**
	-2.722	-2.675	-2.480	-2.470
Donor 2009		-2.194		-1.369
		(-1.333)		(-1.058)
Position		0,0959		-0.301
		(0.0621)		(-0.248)
Disparities		0.988		1.730
		(0.605)		-1.349
State resp		-2.541		-0.212
		(-1.411)		(-0.145)
Equalize		1.100		-0.467
		(0.748)		(-0.398)
cons	-11.27***	-9.088*	-5.904*	-5.685
	(-2.671)	(-1.931)	(-1.782)	(-1.532)
No. observations	189	189	184	184
LR Chi^2	44.53***	49.09***	39.95***	43.39***
Pseudo R^2	0.042	0.046	0.041	0.045

amounts than all other subjects whereas being unmarried affects the donation decision positively.

The subjects without a religious affiliation seem to make significantly lower contributions, but the corresponding coefficient is only significant when outliers are excluded. As expected from previous empirical investigations, high income and high education both have a positive impact on donations although the significance levels vary according to the estimation specification. The relation between donations in the experiment and donations that have been made in the year 2009 previously to the experiment is negative, though not significant. Furthermore, the attitudinal variables do not have any explanatory power.[78]

5.5 Conclusions

The results of our experiment contribute to the understanding of how the provision of information about charities' entire revenues affects individual donation decisions. We deem this kind of information to be realistic, because in real-life donation decisions, individuals usually do not precisely know whether and how much other individuals or government institutions have given to a charity, but rather have a belief about its size. While the announcement of other individuals' contributions, as implemented in previous experiments, is likely to lead to the emergence of anchor points or the desire to comply with own or others' expectations, the information provided in our experiment does not point in one specific direction, but rather offers two charities of different size.

[78] We also investigated whether the subjects' characteristics differ between donors choosing the small organization and donors choosing the large organization. Using a nested logit model, we do not find any significant differences between the two groups.

Providing individuals with the information about the charities' revenues and the opportunity to choose between small and large charities increases neither the propensity to donate nor the donated amount compared to the situation without this information. We do find, however, that the subjects prefer to give to small charities with relatively low revenues as compared to large charities. Thus, our results support the predictions that may be derived from the model of impact philanthropy by Duncan (2004), which assumes that donors try to achieve the biggest impact possible with their charitable contribution. More precisely, in our experiment donors prefer smaller charities to larger ones, confirming the theoretical prediction that an impact philanthropist's utility decreases with a charity's endowment. As our survey results show, however, crowding out considerations as well as quality considerations as suggested by Vesterlund (2003) and Andreoni (2006) also play a role for some donors.

Moreover, the results of our econometric analysis confirm previous findings that the individual willingness to donate increases with the subjects' age, income, and education (e.g. Pharoah and Tanner 1997, Schervish and Havens 1997). This suggests that the donation decisions in our experiment are a good indicator of real-life decisions. As individuals with certain characteristics are more likely to react positively when provided with the opportunity to make a donation, fundraisers may be able to increase donations by specifically targeting those individuals.

The key result of our study, the donors' preference for smaller charities, has to be seen in the light of the experimental design. The experiment offered the participants a choice of pre-selected charities which all fulfill a certain minimum quality standard. Thus, the preference for small charities is conditional on third-party validation and may be different in the absence of such validation. Indeed, the lack of convergence of small and large charities, that would eventually be a consequence of our findings, may be explained by this design element.

Our findings are nevertheless important as they indicate a general preference for smaller charities when the donors can assume a minimum quality. Interestingly, the strength of the preference for small charities differs between the four charitable causes. For charities which are active in the field of medical research, for example, this preference is not as strong as in the case of disabled care. Donors may deem a large charity to be more effective than many small charities in fighting diseases whereas they prefer smaller and possibly more locally oriented charities in the case of disabled care. Thus, the natural size of a charity depends on the charitable cause it engages in, which means that there would hardly be any convergence between small and large charities.

Appendix 5

5.A - Tables

Table 5.A1: Socio-demographic characteristics of participants

Variable	State	N	Share (in %)
Gender	Male	103	46.19
	Female	119	53.36
	No answer	1	0.45
Age	18 - 29	73	32.74
	30 - 44	60	26.91
	45 - 59	54	24.22
	60 - 75	34	15.25
	No answer	2	0.90
Family status	Single	139	62.33
	Married	45	20.18
	Divorced	31	13.90
	Widowed	6	2.69
	No answer	2	0.90
Children	Yes	34	15.25
	No	189	84.75
Household size	1	102	45.74
	2	82	36.77
	3	21	9.42
	4 or more	17	7.62
	No answer	1	0.45
Education	University	88	39.46
	Gymnasium (12 years of education)	58	26.01
	Realschule (10 years of education)	35	15.70
	Hauptschule (9 years of education)	23	10.31
	Other	17	7.62
	No graduation	2	0.90
Nationality	German	192	86.1
	Turkish	2	0.90
	Italian	3	1.35
	Polish	2	0.90
	Other	23	10.31
	No answer	1	0.45
Total		223	100

Table 5.A1 cont.: Socio-demographic characteristics of participants

Variable	State	N	Share (in %)
Houshold net income	≤ 1,000€	51	22.87
	1,000 - 2,000€	85	38.12
	2,000 - 3,000€	44	19.73
	3,000 - 4,000€	13	5.83
	4,000 - 5,000€	8	3.59
	≥ 5,000€	8	3.59
	No Answer	14	6.28
Religion	Catholic	70	31.39
	Evangelic	71	31.84
	Muslim	5	2.24
	Other	10	4.48
	No religion	67	30.04
Voting behavior	Christian Democratic / Social Union	43	19.28
	Social Democratic Party	49	21.97
	The Greens	42	18.83
	Free Democratic Party	25	11.21
	Left Party	17	7.62
	Other	9	4.04
	Nonvoter	17	7.62
	No answer	21	9.42
Total		223	100

Table 5.A2: Charitable giving habits of participants

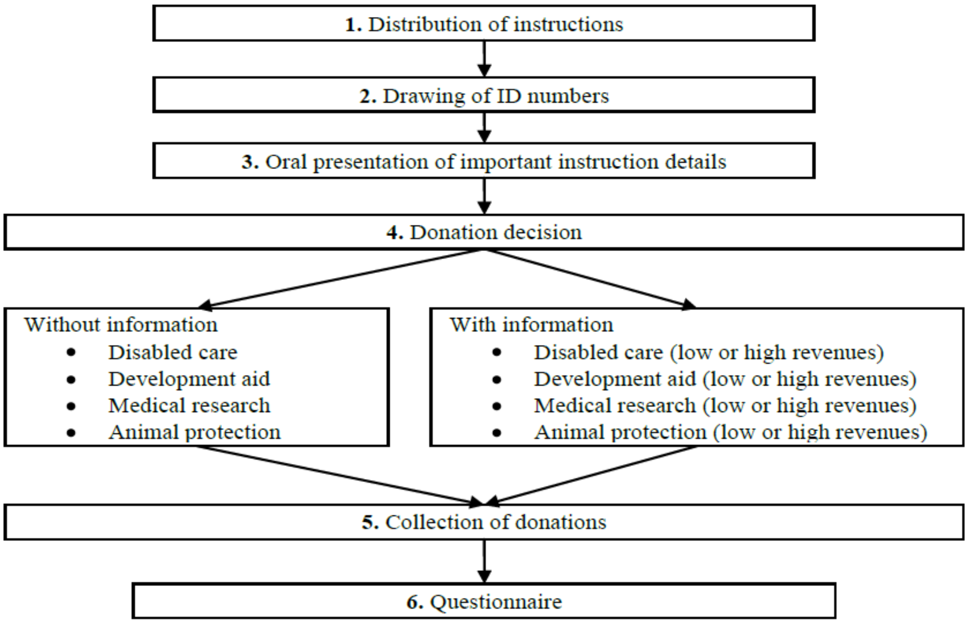
Variable	State	N	Share (in%)
Donated before	Yes	189	84.75
	No	34	15.25
Modal charitable cause	Child or disabled care	46	20.63
	Emergency aid	12	5.38
	Medical research	13	5.83
	Church and religious purposes	11	4.93
	Environment or animal protection	32	14.35
	Development aid	39	17.49
	General (e.g. Red Cross)	20	8.97
	Culture	3	1.35
	Politics	2	0.9
	Local welfare services, homeless persons, poverty	8	3.59
	No answer (incl. 34 subjects who did not donate before)	37	16.59
Contribution receipt received	Always	60	26.91
	Mostly	36	16.14
	Sometimes	42	18.83
	Never	49	21.97
	No answer (incl. 34 subjects who did not donate before)	36	16.14
Donated in 2009	Yes	67	30.04
	No	156	69.96
Total		223	100

Table 5.A3: Attitudes of participants towards society and government responsibilities

Question / Statement	Answer	N	Share (in %)
Compared with how others live in Germany: Do you think you get your fair share, more than your fair share, somewhat less, or very much less than your fair share?	Very much less	20	8.97
	Somewhat less	61	27.36
	Fair share	104	46.64
	More than fair share	19	8.52
	Don't know	19	8.52
All in all, I think the social differences in this country are just.	Completely agree	14	6.28
	Tend to agree	65	29.15
	Tend to disagree	90	40.36
	Completely disagree	50	22.42
	Don't know	4	1.79
It is the responsibility of the state to meet everyone's needs, even in case of sickness, poverty, unemployment and old age.	Completely agree	74	33.18
	Tend to agree	104	46.64
	Tend to disagree	35	15.7
	Completely disagree	4	1.79
	Don't know	6	2.69
It is the responsibility of the government to reduce the differences in income between people with high incomes and those with low incomes.	Strongly agree	32	14.35
	Agree	73	32.74
	Neither agree nor disagree	39	17.49
	Disagree	48	21.52
	Strongly disagree	17	7.62
	Can't choose, don't know	14	6.28
Total		223	100

5.B - Figures

Figure 5.B1: Proceedings of the experiment



Note: The treatments with information are identical to the treatments without information except for the fact that in the donation stage subjects could choose between a small organization (with revenues between €40,000 and €300,000) and a large organization (with revenues between €5 million and €11 million) for each charitable purpose.

5.C - Experimental instructions (translated from German)

Welcome! Thank you very much for participating in our study for the analysis of consumer behavior. Enclosed in this folder, you find information which you need during this event. You may return pages to which you have already gone through at any time. Please turn pages only up to the next “stop-sign”. You will be asked to turn to the next page. Please only read the respective text and do not act until you receive specific instructions to follow the assignment. Please follow the instructions carefully. We also would like to ask you not to talk to other participants. We want to emphasize that all information which we gain from today’s event will only be used to draw a comparison between the groups of participants. No individual data about the participants will be published or passed on. Shortly, we will come up to your seat and you will draw a piece of paper with a number on it. This number will serve as your personal identification number (ID) throughout the study. Please state your ID whenever you are asked to do so during the study. The ID ensures anonymity, as neither other participants nor we know your name or the ID that belongs to it.

– STOP sign: Please do not turn the page until we ask you to! –

For your participation in the study, you will receive 40€. Shortly, we will hand out the money in an envelope. Then we ask you to confirm the receipt. Afterwards, you will get the opportunity to donate any preferred amount of money to a charitable cause. There is a charitable organization behind every charitable cause. The money which you, if any, will donate, will be completely transferred to the respective charity. We guarantee that this will happen lawfully and will have the transfer supervised and verified by the director of the notary’s office, [name of the notary]. All selected charitable organizations hold the "donation seal" by the state-approved German Central Institute for Social Issues (Deutsches Zentralinstitut für soziale Fragen (DZI)). This assures that the organizations act autonomously and charitably and that the usage of their financial means is reviewable, economical and statutory. The names of the individual organizations will at this point - for scientific reasons - not be mentioned. We guarantee that all information you receive from us regarding the organizations is true. At the end of the experiment, we are happy to hand to you a list of all organizations upon request. In the following, we present to you four different charitable causes to which you can donate in the course of this study. The four charitable causes are:

- Medical research
- Animal protection
- Disabled care
- Development aid

[Additional part mentioned only in the Info Treatment: The organizations you may donate to do not only differ with regard to their charitable causes, but also their revenues, which these organizations have generated in 2006 from donations, membership fees and government grants. For each charitable cause, we offer you a charitable organization with relatively small revenues between 40,000 and 300,000€ and organizations with rather large revenues between 5 million and 11 million€.

Therefore, we ask you, in the case you donate, to pick one of the following organizations:

Medical research	Revenues 2006: 40,000€ - 300,000€
Medical research	Revenues 2006: 5 million€ - 11 million€
Animal protection	Revenues 2006: 40,000€ - 300,000€
Animal protection	Revenues 2006: 5 million€ - 11 million€
Disabled care	Revenues 2006: 40,000€ - 300,000€
Disabled care	Revenues 2006: 5 million€ - 11 million€
Development aid	Revenues 2006: 40,000€ - 300,000€
Development aid	Revenues 2006: 5 million€ - 11 million€]

We now hand out to you an envelope with the money you receive for your participation in our study.

– STOP sign: Please do not turn the page until we ask you to! –

In the envelope, you find:

- one white envelope
- one blue envelope
- 40€, composed of two 10€-bills, one 5€-bill, six 2€-coins and three 1€-coins
- one receipt.

We now ask you to sign the receipt you find enclosed. By doing so, you confirm that you have received 40€ from [name of the institution] for the participation in this study. We need the receipt for administrative purposes. Without a receipt we are not allowed to give you the money. Your data is still handled confidentially and anonymized. We will now collect the receipts, the study will continue hereafter.

– STOP sign: Please do not turn the page until we ask you to! –

Now you can make a donation decision. You can decide freely and anonymously whether and how much money you want to give to one of the above-mentioned charitable organizations. The amount of money you put into the blue envelope will benefit a charitable cause and will be transferred completely to the respective charity after the experiment. You will keep the amount of money you put into the white envelope.

The study proceeds as follows: 1.) Make your donation decision. In case of a donation, please tick the desired charitable organization on the blue envelope. Please note that you have to choose one of the four [in the Info treatment: eight] charities given. It is not possible to choose more than one charitable organization for your donation. Please tick only one organization if you wish to donate. If you tick more than one organization, unfortunately, we will not be able to transfer the donation. If you do not wish to donate, please do not tick any organization. 2.) Write down your ID-number into the predefined box on the blue envelope, irrespective of whether you wish to donate or not. 3.) Put the desired donation amount into the blue envelope. 4.) Put the amount of money you wish to keep into the white envelope. Finally, you should have distributed 40€ completely to the two envelopes. Please note that any distribution in full amounts of Euros is possible. You may put any desired amount of money into both envelopes. It is also possible to put 40€ completely into one envelope. 5.) Seal up both envelopes.

When all participants have finished, we will come up to you and collect the blue envelope. When we do so, please put the blue envelope into the box. Please keep the white envelope. We guarantee that your donation will be transferred to the charitable organization lawfully and have the transfer supervised and verified by the director of the notary's office, [name of the notary]. We will explain the most important items once again orally. Afterwards, please make your decision as described above.[79]

[79]Next to the questionnaire, which is available from the author upon request, some of the subjects also got instruction on playing a dictator game, see Borgloh et al. (2010b) for more information.

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Eidesstattliche Erklärung

Hiermit erkläre ich, die vorliegende Dissertation selbständig angefertigt und mich keiner anderen als der in ihr angegebenen Hilfsmittel bedient zu haben. Insbesondere sind sämtliche Zitate aus anderen Quellen als solche gekennzeichnet und mit Quellenangaben versehen.

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