Essays in Empirical Labor Economics

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Zusammenfassung

Alle Kapitel dieser Dissertation sind empirische, mikro-ökonometrische Studien, die sich jeweils mit einem spezifischen Aspekt von Ungleichheit auf Arbeitsmärkten beschäftigen. Die erste Studie *The impact of September 11th, 2001 on the employment prospects of Arabs and Muslims in the German labor market* untersucht die Frage, ob die terroristischen Anschläge auf das World Trade Center und das Pentagon am 11. September, 2001 einen adversen Effekt auf die Beschäftigungschancen von Individuen aus arabischen bzw. hauptsächlich muslimischen Ländern hatten. Verwendet wird ein großer, repräsentativer Datensatz aus administrativen Quellen sowie regressionskorrigierte Difference-in-Differences-Schätzer. Die Ergebnisse, die keinen großen Einfluss der Anschläge auf die Beschäftigungschancen von Arabern und Muslimen zeigen, sind im Einklang mit früherer Evidenz aus Europa.

Ein ähnliches Thema wird in der zweiten Studie *Islamistic terror and the job prospects of Arab men in Britain: Does a country's direct involvement matter?* aufgegriffen, die sich zudem mit der Frage beschäftigt, inwieweit die direkte Betroffenheit eines Landes eine Rolle spielt. Die Studie verwendet Daten des Britischen Labour Force Survey und nutzt die Anschläge vom 11. September 2001, die Madrider Zuganschläge vom 11. März 2004, die Londoner Anschläge vom 7. Juli 2005 als quasi-experimentelle Ereignisse. Die Ergebnisse legen nahe, dass sich die Löhne, geleisteten Arbeitsstunden, sowie Beschäftigungschancen von (verschieden abgegrenzten) arabischen bzw. muslimischen Männern in England durch die Anschläge nicht verändert haben. Insbesondere spielt es in diesem Zusammenhang keine Rolle, dass England am 7. Juli 2005 direkt von Anschlägen betroffen war.

Die dritte Studie *Wirkungen der Beschäftigungspflicht schwerbehinderter Arbeitnehmer - Erkenntnisse aus der Einführung des "Gesetzes zur Bekämpfung der Arbeitslosigkeit Schwerbehinderter"* untersucht mit Hilfe eines neu verfügbaren Datensatzes aus Prozessdaten der Bundesagentur für Arbeit, der Stichprobe der integrierten Erwerbsbiographien, die Wirkung einer verpflichtenden Beschäftigungsquote für schwerbehinderte Arbeitnehmer in Deutschland. Ich nutze die exogene Senkung dieser Quote durch die Einführung des "Gesetzes zur Bekämpfung der Arbeitslosigkeit Schwerbehinderter" als natürliches Experiment und schätze die Änderung in der Wahrscheinlichkeit einer Beschäftigungsaufnahme durch regressionskorrigierte Difference-in-Differences-Schätzer. Die Ergebnisse legen nahe, dass die Änderung der Beschäftigungsquote die Beschäftigungschancen von Schwerbehinderten

weder verbessert noch verschlechtert hat.

Die letze Studie *Intra-firm wage inequality and firm performance - First evidence from German linked employer-employee-data* der Arbeit basiert auf linked employer-employee-Daten und verwendet sowohl konventionelle Fixed-Effects-Schätzer als auch Panel-Instrumentenvariablen-Schätzer, die eine Kontrolle für unbeobachtete Heterogenität und mögliche Simultanität zwischen Lohnungleichheit und betrieblichem Erfolg erlauben. Die Ergebnisse zeigen keinen Zusammenhang zwischen innerbetrieblicher Lohnungleichheit und Produktivität in Westdeutschland. Sie legen jedoch einen Zusammenhang in Ostdeutschland nahe, der stark mit dem verwendeten Ungleichheitsmaß, sowie dem Vorhandensein von Betriebsräten und tariflichen Regelungen variiert.

Abstract

All of the papers contained in this thesis deal with some aspect of labor market inequality. *The impact of September 11th, 2001 on the employment prospects of Arabs and Muslims in the German labor market* (chapter 2) examines whether the attacks on the World Trade Center and the Pentagon on September 11th, 2001 have influenced the job prospects of persons from predominantly Muslim countries in the German labor market. Using a large, representative database of the German working population, evidence from regression-adjusted difference-in-differences-estimates indicates that 9/11 did not cause a severe decline in job prospects. This result, which is in line with prior evidence from Sweden and England, is robust over a wide range of control groups.

Islamistic terror and the job prospects of Arab men in Britain: Does a country's direct involvement matter? (chapter 3) examines whether the labor market prospects of Arab men in England are influenced by recent Islamistic terrorist attacks. We use data from the British Labour Force Survey from Spring 1999 to Winter 2006 and treat the terrorist attacks on the USA on September 11th, 2001, the Madrid train bombings on March 11th, 2004 and the London bombings on July 7th, 2005 as quasi-experimental events that may have changed the attitudes towards Arab or Muslim men. Using treatment group definitions based on ethnicity, country of birth and religion, evidence from difference-in-differences-estimators combined with matching indicates that the real wages, hours worked and employment probabilities of Arab men were unchanged by the attacks. This finding is in line with prior evidence from Europe.

Effects of the obligation to employ severely disabled workers – findings from the introduction of the "Law to Combat Unemployment among Severely Disabled People" (chapter 4) uses new administrative data from the German Federal Employment Agency – the Integrated Employment Biographies Sample IEBS – to assess the impact of a mandatory employment quota for disabled workers in Germany. We use an exogenous change, introduced through the "Law to Combat Unemployment among Severely Disabled People" ("Gesetz zur Bekämpfung der Arbeitslosigkeit Schwerbehinderter"), as a natural experiment and measure the change in the reemployment probability of the unemployed disabled by means of regression-adjusted difference-in-differences estimators. Our results indicate that the change in the employment quota neither enhanced nor worsened the employment

prospects of the disabled.

Finally, *Intra-firm wage inequality and firm performance – First evidence from German linked employer-employee-data* (chapter 5) deals with the impact of wage inequality on firm performance. Economic theory suggests both positive and negative relationships between intra-firm wage inequality and productivity. This paper contributes to the growing empirical literature on this subject. We combine German employer-employee-data for the years 1995-2005 with inequality measures using the whole wage distribution of a firm and rely on panel-instrumental variable estimators to control for unobserved heterogeneity and simultaneity problems. Our results indicate a relatively small impact of wage inequality on firm performance in West Germany, while there seems to be a relationship for some inequality measures in East Germany. Further analysis shows that the relationship varies strongly with industrial relations in East Germany.

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Third, as an empirical thesis like this one would have been impossible without the access to data, I would like to thank the crew of the Research Data Center of the Federal Employment Agency in the Institute of Employment Research in Nuremberg that provided access to and advice on the data used in three of the papers contained in this volume. Special thanks in this context go to Dirk Oberschachtsiek for many discussions in various bars in Lüneburg and Nuremberg and for hospitality during my stays in the Research Data Center. Additionally, great acclaim has to go to all individuals and institutions that have worked on the creation of an internationally competitive informational infrastructure. Some years ago, none of these papers could have been written.

Many of the papers collected in this thesis have additionally profited from comments during conferences or from discussions with other members of the profession. Credits for these are given in a footnote at the beginning of the respective paper.

Finally, on a personal level, I thank my parents for more than they probably imagine, Janine for being who she is and for her support (though she probably endured me at times) and Brigitte, Daniel, Denise, Dominik, Kathrin, Raphael, Svenja and Vanessa for providing support and distraction in perfect balance.

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Chapter 1

Introduction

1.1 Overview and motivation

All of the papers contained in this thesis are empirical studies dealing with some aspect of labor market inequality. *The impact of September 11th, 2001 on the employment prospects of Arabs and Muslims in the German labor market* (chapter 2) and *Islamistic terror and the job prospects of Arab men in Britain: Does a country's direct involvement matter?* (chapter 3) are concerned with the question how extreme events, in that case major terrorist attacks, influence labor market inequality through a decline in job prospects for the population groups similar to the terrorists. *Wirkungen der Beschäftigungspflicht schwerbehinderter Arbeitnehmer - Erkenntnisse aus der Einführung des "Gesetzes zur Bekämpfung der Arbeitslosigkeit Schwerbehinderter"* (*Effects of the obligation to employ severely disabled workers - findings from the introduction of the "Law to Combat Unemployment among Severely Disabled People"*, chapter 4) is concerned with an aspect of German anti-discrimination legislation, the obligation to employ severely disabled workers. Finally, the last paper *Intra-firm wage inequality and firm performance – First evidence from German linked employer-employee-data* (chapter 5) deals with the consequences of wage inequality for firm performance.

Another common factor of these papers is their empirical approach. All papers try to establish causal relationships between the respective variables of interest while avoiding the often restrictive assumptions necessary for structural modeling (see Angrist and Krueger

1999, Imbens and Woooldridge 2008 and Blundell and Costa-Dias 2008 for overviews of the methods commonly used in these "quasi-experimental" approaches). The first three chapters use exogenous events to identify the causal impact of the intervention of interest, while the fourth paper has to rely on a more elaborate panel data estimator to control for time-constant unobserved heterogeneity and simultaneity between the variables of interest.

The impact of September 11th, 2001 on the employment prospects of Arabs and Muslims in the German labor market was inspired by newspaper articles providing anecdotal evidence for a change in attitudes towards Muslims or Arabs in Germany after the terrorist attacks on September 11th, 2001. This evidence quickly led to the question whether this suspected change in attitudes actually manifested itself not only in opinion surveys or in the media, but rather had a real influence on the labor market prospects of individuals of a similar regional or religious background as the 9/11-terrorists. Luckily, shortly before this question arose, the *Integrated Employment Biographies Subsample* had been released by the research data center of the Federal Employment Agency in the Institute of Employment Research. This dataset, a 2.2% sample of the German labor force that is formed from social security notifications and the accounts of the Federal Employment Agency, fulfilled two essential conditions for this question to be answered: First, it covered the time period under question and second, it did provide information on a large enough number of individuals with a nationality from a predominantly Muslim country.

During presentations of the resulting paper at the 2007 annual conferences of the European Association of Labour Economists and the Verein für Socialpolitik comments and discussions led to two follow-up questions:

(1) Is the fact, that studies focusing on the US (Dávila and Mora 2005, Kaushal, Kaestner, and Reimers 2007) generally found a negative effect on the labor market prospects of Arabs/Muslims while studies focusing on Europe did not (Åslund and Rooth 2005 for Sweden, my own paper in chapter 2 for Germany), related to the direct involvement of the US in the attacks? Or is it simply a result of the choice of different outcomes as the US studies typically only found a decline in wages, while the European studies had to focus on employment probabilities?

(2) Is the definition of the treatment groups in previous studies, essentially using nationality from Arab/Muslim countries as a proxy for similarity to the terrorists, right or

should one expect that possible changes in attitudes and a resulting decline in job prospects are rather directed towards individuals with a similar appearance/ethnicity or a similar religion as the terrorists?

As two European countries, Spain on March 11th, 2004 and England on July 7th, 2005 had also been the target of large scale Islamistic terrorist attacks, an attempt to shed some light on the first question could be made. Fortunately enough, shortly afterwards, a discussion paper by Blanchflower and Bryson (2007)[1] pointed me towards the existence of the *British Labour Force Survey*, a survey conducted among households living at private addresses or National Health Service accommodations in the UK by the Office of National Statistics since 1973. As each survey covers approximately 50,000 households and England has a rather large immigrant community from Muslims countries, predominantly from Bangladesh and Pakistan, the sample seemed large enough to test whether a country's direct involvement matters. Additionally, as the data contains the same outcomes used in the study by Kaushal, Kaestner, and Reimers (2007), it was also possible to make a statement about the importance of the choice of the respective outcome. Finally, the data also allows a statement on the second question as it contains information on an individual's self-assessed ethnicity, its country of birth and, beginning with the spring 2002 sample, its religion.[2]

Both *The impact of September 11th, 2001 on the employment prospects of Arabs and Muslims in the German labor market* and *Islamistic terror and the job prospects of Arab men in Britain: Does a country's direct involvement matter?* use two characteristics of the respective events for estimation. First, the terrorist attacks were unexpected events and, second, a potential change in attitudes could plausibly only be directed towards individuals that are in some respect similar to the respective terrorist, e.g. who are Muslims or stem from a predominantly Muslim country. The first fact allows us to treat the respective acts of terrorism as exogenous interventions, while the second fact naturally generates groups affected and unaffected by the intervention (treatment and control groups). Given this setup, a difference-in-differences-estimator could be used to purge both common time trends and group-specific time-constant heterogeneity from the estimation. The results

[1] The final version is Blanchflower and Bryson (2008).

[2] While information on current nationality is also available, this variable could not be used due to small case numbers in the treatment groups.

from both papers, along with the results from Åslund and Rooth (2005), suggest that the recent terrorist attacks did not have a large influence on the job prospects of Arab or Muslims in Europe.

Wirkungen der Beschäftigungspflicht schwerbehinderter Arbeitnehmer - Erkenntnisse aus der Einführung des "Gesetzes zur Bekämpfung der Arbeitslosigkeit Schwerbehinderter" uses the same data as *The impact of September 11th, 2001 on the employment prospects of Arabs and Muslims in the German labor market* and the same estimator as the two previously discussed papers. The paper considers the impact of a mandatory employment quota for disabled workers or more specifically, the impact of a change in that quota by the *Law to Combat Unemployment among Severely Disabled People* (*Gesetz zur Bekämpfung der Arbeitslosigkeit Schwerbehinderter*) on the employment prospects of previously unemployed disabled workers.

The idea for this paper was developed while writing *The impact of September 11th, 2001 on the employment prospects of Arabs and Muslims in the German labor market* when it was noticed that the *Integrated Employment Biographies Subsample* was one of the few (and at that time the only large) dataset containing information on the disabled. While the impact of anti-discrimination laws, especially with respect to the *Americans with Disabilities Act* (see e.g. Acemoglu and Angrist 2001, Beegle and Stock 2003, DeLeire 2000, Jolls 2004, Jolls and Prescott 2004, Kruse and Schur 2003, Lee 2003), has been thoroughly researched, quota regulations similar to the German legislation have received less interest. Upon the time of writing, there are four studies dealing with demand side implications of the German quota, each using thresholds in the quota to look at discontinuities in the hiring behavior of plants "close" to the respective threshold (see Kölling, Schnabel and Wagner 2001, Wagner, Schnabel and Kölling 2001a,b and Koller, Schnabel and Wagner 2006) and one taking a supply side perspective by looking at changes of individual employment prospects using the *German Socio-Economic Panel* (Verick 2004).[3]

While it is notoriously difficult to treat legislative changes as natural experiments due to anticipation effects that may enable units to select out of or into the treatment group, identification in this case could be achieved due to several characteristics of the problem at hand (see section 4.6 for details). The findings from this paper, again using a difference-

[3] Additionally, Lalive, Wuellrich and Zweimüller (2007) look at a similar quota in Austria.

in-differences-estimator, confirm the previous evidence by Verick (2004) that the change in the employment quota did not change the employment prospects of the disabled.

Finally, the last paper *Intra-firm wage inequality and firm performance – First evidence from German linked employer-employee-data* contributes to a specific aspect of the literature on wage compression – the relationship between wage compression within firms and productivity. In a more narrow sense the paper contributes to a small but growing empirical literature, reviewed in greater detail in chapter 5.2, on the relationship between intra-firm wage inequality (or the lack of it) and firm performance. The paper uses linked-employer-employee data provided again by the research data center of the Federal Employment Agency in the Institute of Employment Research. I use person data from social security notification to derive several measures of intra-firm wage inequality, some of them adjusted for the composition of the respective workforce. Merged with plant-level panel data, these measures are used in productivity regressions using panel-instrumental-variable-estimators to account for the endogeneity of the wage structure. The results indicate a relatively small impact of wage inequality on firm performance in West Germany, while there seems to be a relationship for some inequality measures in East Germany. Further analysis shows that the relationship varies strongly with industrial relations in East Germany.

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Chapter 2

The impact of September 11th, 2001 on the employment prospects of Arabs and Muslims in the German labor market

Abstract:[1] *This paper examines whether the attacks on the World Trade Center and the Pentagon on September 11th, 2001 have influenced the job prospects of persons from predominantly Muslim countries in the German labor market. Using a large, representative database of the German working population, evidence from regression-adjusted difference-in-differences estimates indicates that 9/11 did not cause a severe decline in job prospects*

[1]The first version of this paper was published in January 2007 as *University of Lüneburg Discussion Paper in Economics No. 37* under the title "The impact of September 11th, 2001 on the job prospects of foreigners with Arab background - Evidence from German labor market data". This is the final version forthcoming in the *Jahrbücher für Nationalökonomie und Statistik / Journal of Economics and Statistics.* The data used in this paper can be accessed via the research data center of the Federal Employment Agency in the Institute of Employment Research in Nuremberg. See http://fdz.iab.de for details. Stata 9.2 was used in all calculations. Do-files are available from the author on request. The author would like to thank Joachim Wagner for helpful hints and overall support, four anonymous referees and the editor of the JbNSt, Peter Winker, for thoughtful comments and suggestions and Stefan Bender, Nils Drews, Peter Jacobebbinghaus, and Dirk Oberschachtsiek of the research data center of the Federal Employment Agency in the Institute for Employment Research for helpful discussions and pointing out several problems with the first version of this paper. Special thanks go to the crew of the research data center for providing hospitality during my stay at Nuremberg and endless patience in the following remote access procedure. Helpful comments on a previous version of this paper at the 2007 annual conferences of the European Association of Labour Economists in Oslo and the German Economic Association in Munich are gratefully acknowledged. All remaining errors are my own.

for individuals with a nationality from a predominantly Muslim country. This result, which is in line with prior evidence from Sweden and England, is robust over a wide range of control groups.
Keywords: Discrimination, September 11th, exit from unemployment
JEL Classification: J64, J71

2.1 Introduction

This paper considers the question whether the job prospects of Arabs or Muslims in Germany have been harmed after the terrorist attacks on the World Trade Center and the Pentagon on September 11th, 2001 (henceforth 9/11). Following the attacks, numerous reports indicate that there may have been a change in attitudes towards Muslims or individuals of Arab origin in various countries. However, while one should not discard these reports of harassment, discriminations and outright threats lightly, quantitative evidence on this question is rather scarce.

More specifically, there have been two strands of the literature dealing with the aftermath of 9/11. The first group of studies mainly consists of surveys of newspaper reports, reported (criminal) incidents and opinion survey results, while a second and relatively small group of papers focuses on actual labor market outcomes.

An example of the first group of studies is a series of country reports conducted on behalf of the European Monitoring Centre on Racism and Xenophobia (EUMC) between September 11th, 2001 and the end of that year. Although not all of these reports are available to the public, Allen and Nielsen (2002) provide a summary. Focusing on the part related to Germany as the country of interest for this paper, their findings can be summarized as follows. While the number of physical attacks on persons of "Muslim appearance" was negligible, an increase in verbal attacks as well as a changing focus in national security measures could be noted. The last point was criticized by various individuals and organizations as a sign of general suspicion towards the Muslim community (Allen and Nielsen 2002: 19).[2]

[2]An example of this view is a remark by then-chairman of the central council of Muslims in Germany (*Zentralrat der Muslime in Deutschland*), Dr. Nadeem Elyas, published on September 11th, 2002, where he, after condemning the attacks, heavily criticizes the searching of several mosques by the police as well as

In a similar report, the European Commission against Racism and Intolerance (ECRI) reports a rise in Islamophobia and subsequent discrimination by authorities of the German *Länder* on issues such as the opening of places of worship or kindergartens as well as a rise in employment discrimination and harassment (ECRI 2004: 22-23). The latter seems to have been especially targeted at women wearing headscarves (ECRI 2004: 23), most likely as these were easily identified as Muslims.

Similarly, Allen and Nielsen (2002: 35-37) conclude that, in general, discrimination was based mainly on the perception of an individual as a Muslim rather than on the individual's true religion. They also report that women were primary targets of abuses. Additionally, Sikhs whose traditional appearance, wearing beards and turbans, is similar to some cliché of Muslim appearance were also targeted.

In a study focusing on the attitudes of the native German population, Brosig and Brähler (2002) look at data from four representative opinion surveys before and after 9/11. They find several hints of an increase in fear and a change in attitudes towards certain population groups. More specifically, they report an increase in children's fear of war and the loss of loved ones, a large increase in adults' fears of terrorist attacks, a forthcoming war and a "clash of cultures" with Muslim countries as well as an increase in the "social distance" towards Muslims. Unfortunately, there is, to the best of my knowledge, no study focusing on the experiences of the potential victims of discrimination that could shed light on the perceptions of the German Muslim population. In a study of the UK, Sheridan (2006), using a survey among British Muslims, states that 82.6% of the respondents reported an increase in implicit racism and religious discrimination, while 76.3% reported an increase in "general discriminatory experiences".

Looking at the USA as the direct target of terrorists reveals a somewhat stronger reaction with the Arab American Institute reporting several incidents (along with several accounts of hospitality and support towards Arab Americans) including several cases of labor market discrimination (Arab American Institute 2002; a summary of incidents can be found on pages 26-27). An even larger collection of "hate crimes" and cases of discrimination can be found in a report by the American-Arab Anti-Discrimination Committee that also

the reintroduction of the *Rasterfahndung*, a computer-based search procedure for possible suspects based on abstract criteria that was introduced in the 1970s against the left-wing terrorists of the *Rote Armee Fraktion* (Zentralrat der Muslime in Deutschland 2002).

reports over 800 cases of workplace discrimination (American-Arab Anti-Discrimination Committee 2003; a number of examples can be found on pages 92-103).

Although the evidence presented above suggests a negative change in attitudes towards Muslims or Arabs in general after 9/11, one should keep in mind that this notion is mainly based on reported incidents and opinion surveys. While these may provide qualitative evidence, questions on racial or religious prejudice are generally sensitive and answers need not necessarily provide information on actual behavior. Evidence on the latter, however, is rather scarce. Up to today, only five papers addressed the question whether 9/11 has led to a change in labor market outcomes.

Åslund and Rooth (2005) provide evidence from Sweden. They focus on exits from unemployment using difference-in-differences estimators and a large number of control groups and find no significant drop in re-employment probabilities for persons from the Middle East compared to natives, people from the Nordic countries and from former Yugoslavia, Western and Eastern Europeans, Latin Americans, Asians, and Africans.

Dávila and Mora (2005) focus on wage discrimination for men from the Middle East, Afghanistan, Iran, and Pakistan in the US labor market between 2000 and 2002. They estimate classical Mincer-type earnings regressions, decompositions based on Juhn, Murphy and Pierce (1993), and quantile regressions for younger men between 25 and 40 years of age and a number of nationalities (Middle Eastern Arabs, African Arabs, other Middle Eastern, and US-born non-Hispanics). Their findings indicate that Middle Eastern Arabs experienced a significant decline in wages between 2000 and 2002. This decline does not only exist in the mean wages but also over the whole income distribution and can only in negligible parts be explained by changes in observed variables or changes in the return to the observed variables.

Another study for the US was conducted by Kaushal, Kaestner, and Reimers (2007) who use regression-adjusted difference-in-differences estimates on Current Population Survey data to assess changes in job prospects and mobility for persons from predominantly Muslim / Arab countries relative to natives and other migrants. Their results indicate that the real wages and weekly earnings of Arab men were reduced by 9% to 11%, though this effect seems to have been temporary with a significant rebound noted in 2005. Furthermore, they find hints that intrastate mobility of Arab men was reduced by the 9/11 attacks, while employment and hours worked seem to have been relatively uninfluenced by the attacks.

Braakmann (2007) uses data from the British labour force surveys. He does not only consider the 9/11 attacks but also looks at the attacks on commuter trains in Madrid on March 11th, 2004 and the London bombings on July 7th, 2005 to test whether a country's direct involvement in terrorist attacks matters for discrimination. Using treatment group definitions based on ethnicity, religion and country of origin alongside several control groups, he does not find an impact of any attack on the treatment groups' wages, working hours and employment probabilities.

Finally, in a somewhat related paper Orrenius and Zavodny (2006) focus on the impact of tightened security measures after 9/11 on the job prospects of Hispanic immigrants. They employ a difference-in-differences estimator on Current Population Survey data and find a negative effect on wages and hours worked for recent male Hispanic immigrants compared to native U.S. workers and a negative effect on employment, wages, and hours worked compared to earlier Hispanic immigrants.

On a theoretical level, there are two possible explanations why one might expect a decline in job prospects for Arabs or Muslims after 9/11. The first would be taste discrimination (Becker 1957/1971) that is a change in preferences induced by the terrorist attacks leading to a lower willingness to pay for the labor of the discriminated group. This effect could be grounded in preference changes by employers as well as fellow employees and customers who feel "uncomfortable" employing, working alongside, or being served by persons they suspect to be terrorists. A second explanation might be statistical discrimination (Phelps 1972, Arrow 1973) if the attacks revealed some previously unknown characteristic of Arabs related to their labor productivity. Both theories predict a downward change in the equilibrium wages of the discriminated groups that may either show up in the data as a change in wages if these are sufficiently flexible or in the probability of employment when rigidities prevent the adjustment of wages.

This paper adds to the literature by providing the first evidence for Germany. The analysis relies on administrative data from social security, unemployment insurance, and the Federal Employment Agency (*Bundesagentur für Arbeit*), thus avoiding the non- and false-response problems of survey data. The attacks are treated as a natural experiment that may have led to a change in attitudes towards individuals with a nationality from a predominantly Muslim country. Using regression-adjusted difference-in-differences-estimators, the change in re-employment prospects of unemployed persons with an Arab background relative to several control groups is assessed. While the empirical approach is somewhat related to Åslund and Rooth (2005), I believe that this type of "scientific replication" (Hamermesh 2007) is valuable – especially in the case of a politically sensitive issue like the aftermath of 9/11 – since "the credibility of a new finding that is based on carefully analyzing two data sets is far more than twice that of a result based only on one" (Hamermesh 2000: 376). The findings presented here are largely in line with the results from Åslund and Rooth (2005) for Sweden and Braakmann (2007) for England, indicating that the hiring behavior of German employers seems to be relatively unaffected by the events on September 11th, 2001. This result also holds if one allows the treatment effect to differ with time passed since 9/11. However, a note of caution is in order as the definitions of control and treatment groups had to be based on current nationality rather than, e.g. country of birth due to data availability. While I tried to minimize the effects of naturalizations, there is undoubtedly some measurement error in the group definitions, caused for instance by second-generation migrants, that may influence the results.

The rest of this paper is organized as follows: Section 2.2 describes the empirical approach along with the data set used. Descriptive results are presented in section 2.3, while section 2.4 presents results for the initial specification as well as some variations to check the robustness of the results. Finally, section 2.5 concludes.

2.2 Data and empirical approach

This paper uses data from the IEBS (*Integrated Employment Biographies Subsample*) containing administrative data from social security, unemployment insurance, participation in active labor market programs, and unemployment registrations. The data cover 17,049,987 periods of employment, unemployment, and participation in active labor market programs for 1,370,031 individuals and are available for the years 1993-2003 (employment, unemployment periods during which benefits were received) and 2000-2003 (active labor market programs, unemployment registrations). A full documentation of the data (in German) can be found in Hummel et al. 2005; Jacobebbinghaus and Seth (2007) provide a short overview in English.

This paper relies primarily on the data on unemployment registrations (the *BewA*) which are collected by the local employment agencies and are used for the placement of the unemployed. Additionally, some of the information contained in the employment data from social security (the *BeH*) is used to construct the outcome. Note that using administrative data has several advantages in the context of the analysis conducted here. First, the risk of strategic response behavior or non-response resulting in selective samples is eliminated. Secondly, though there is some measurement error in the end dates due to delayed notifications of the Federal Employment Agency by the (formerly) unemployed, the data are principally accurate to the day, thus allowing a sharp distinction between the pre- and post-9/11-period. Finally, the IEBS is a true random sample from the German working population covered by social security, implying that inference on the German population is possible without invoking any weighting schemes.

However, several drawbacks when using the IEBS should be mentioned. First, the database covers only those employment spells covered by social security which means that the self-employed and several types of civil servants are not included. Note that this implies that exits from the data may indicate that an individual has become a lifetime civil servant (*Beamter*), has become self-employed, did exit the labor force or even left the country. Consequently, I cannot look into questions whether, e.g., a rise in labor market discrimination after 9/11 caused Arabs to leave Germany or whether it led to a higher or lower level of self-employment. In fact, as far as e.g. higher self-employment rates compensate Arabs for lower job chances as employees, the estimates for the impact of 9/11 presented in this paper may overstate the true effect.

As a second drawback, note that the data contain only information which is commonly reported in social insurance, active labor market programs, and during periods of unemployment or receipt of unemployment benefits. While this includes most labor market-relevant characteristics like age, gender, nationality, education, regional characteristics, and occupations, there are several potentially useful variables missing. The first is the date of immigration into Germany that would have allowed us to control for the effect of assimilation and possible networks in Germany.

The second and more important restriction is that only current nationality can be used to define treatment and control groups. While several corrections, described in greater detail below, are used to ensure that, e.g., naturalized foreigners are not counted as Germans, one can expect some measurement error in the definitions of the treatment and control groups through, e.g., second generation migrants. One should note at this point, though, that discrimination is probably based on a combination of appearance (Allen and Nielsen 2002: 35-37), name (see Carlsson and Rooth 2007 for experimental evidence for Sweden) and maybe other features which makes the use of other measures like religion or country of origin similarly problematic. Evidence by Braakmann (2007) who could use several variables to define treatment and control groups suggest that – at least for England – the choice did not influence the qualitative conclusions drawn from the data.

Unfortunately, data quality varies greatly between those variables actually necessary for the administrative process and those collected purely for statistical purposes. This fact presents an obstacle to my analysis since current nationality is one of those latter variables. To overcome the oddities in this variable, several cleansing procedures were applied that are documented in the appendix.

Before the estimator used in this paper is introduced in greater detail several basic facts about the problem at hand should be noted: First, interest in this paper lies in the estimation of the causal (treatment) effect of the 9/11-attacks on the labor market prospects of Arabs or Muslims caused by a possible shift in attitudes towards these groups. Note that there is a clear theoretical one-way causality between these interventions and the outcomes of interest. Furthermore, the terrorist attacks, unexpected as they were, can be considered a natural experiment leading to an exogenous shift in attitudes towards Arabs or Muslims.

The natural estimator in such a setting is a difference-in-differences estimator where a suitable control group and differences over time are used to purge both time constant unobserved group heterogeneity as well as a common time trend. More formally, the causal impact of the event of interest is given by

$$\begin{aligned} \tau &= E[Y|P=1,T=1] - E[Y|P=0,T=1] \\ &- (E[Y|P=1,T=0] - E[Y|P=0,T=0]), \end{aligned} \tag{2.1}$$

where Y is the outcome of interest, T is “1” for members of the treatment group and “0” otherwise and P is a period dummy which is “0” before 9/11 and “1” afterwards. The central identifying assumption is that members of both groups would have experienced the same trend in the absence of the event of interest.

Before discussing this central assumption in greater detail, note that equation (2.1) can be written in regression form as

$$y_i = \alpha + \beta' X_i + \lambda * T_i + \upsilon * P_i + \tau * (T_i * P_i) + \epsilon_i, \tag{2.2}$$

using both a period and group dummy and an interaction effect between these variables and X_i as a matrix of control variables to account for possible differences in observables. Note that one needs both T_i and P_i to be exogenous for τ to be identified.

While the fact that the terrorist attacks were unexpected ensures the exogeneity of T_i by randomly allocating individuals into the pre- and post-treatment period, one has to ensure that no selection out of the treatment group occurs. While an individual cannot choose their own heritage, nationality can be influenced through naturalization. To fix ideas, consider a situation where individuals feel that they are discriminated because of their nationality. Such a situation could create strong incentives to become German. Since it seems likely that naturalized individuals still share some aspects of non-naturalized individuals, e.g. some features of appearance, continued discrimination remains possible. This, however, could invalidate the results of the analysis by rendering the treatment – at least to some extent – endogenous.

While it may be suspected that such behavior would be small in numbers since naturalization is heavily regulated in Germany and while a recent study on the economic impact of naturalizations by Steinhardt (2008) and my own calculations based on data from the German Socio-Economic Panel showed no signs of an increase in naturalizations during the period under question, I account for this possibility by using all observed nationalities from 1990-2003: For all groups with the exception of the natives, individuals are treated as members of the respective group if they have ever been recorded with the respective nationality. For instance, an individual that was Turkish during the mid 1990s but became naturalized at some point in time afterwards, is considered Turkish for the scope of this analysis. For the German group, a similar correction is applied: Here, only those persons that have always been recorded as Germans are considered German to avoid problems with naturalized foreigners.

For the analysis, I define three treatment groups based on two criteria. In a first step, I select all persons with a nationality from a predominantly Muslim country. This criterion is considered to be fulfilled if the share of Muslims in the respective country, taken from the CIA's world factbook (Central Intelligence Agency 2008), exceeds or equals 50%. The resulting list of 41 countries is partitioned further into Arab and non-Arab countries using membership in the Arab league as the central criterion. Finally, Turks are treated as a separate group as there has been a rather large Turkish community in Germany since the massive work immigration of the 1960s. Given the relative size and the long time of residence of this particular group, it seems possible that there are some unobserved differences between them and other Muslim or Arab groups.

Taken together these criteria give the following three treatment group definitions: (i) Turkish nationality, (ii) nationality from a predominantly Muslim country that is also a member of the Arab league and (iii) nationality from a predominantly Muslim country that is not a member of the Arab league, except for Turkey. Table 2.1 presents a complete overview of the nationalities used for the definition of treatment and control groups.

Table 2.1: Countries in treatment/control-groups

Group	Countries
Treatment Groups	
(i) Turks	Turkey
(ii) Arab Muslims	Algeria, Bahrain, Comoros, Djibouti, Egypt, Iraq, Jordan, Kuwait, Lebanon, Libya, Mauritania, Morocco, Oman, Qatar, Saudi Arabia, Somalia, Syrian Arab Republic, Sudan, Tunisia, United Arab Emirates, Yemen
(iii) non-Arab Muslims	Afghanistan, Albania, Azerbaijan, Bangladesh, Brunei Darussalam, Chad, Guinea, Indonesia, Iran, Kyrgyzstan, Malaysia, Mali, Niger, Pakistan, Republic of Maldives, Senegal, Sierra Leone, Tajikistan, The Gambia, Turkmenistan, Uzbekistan
Control Groups	
Germans	Germany
Central Europe	Andorra, Austria, Belgium, Denmark, Finland, France, Great Britain, Iceland, Ireland, Liechtenstein, Luxembourg, Monaco, The Netherlands, Norway, Sweden, Switzerland
East Europe	Belarus, Bulgaria, (former) Czechoslovakia, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Moldavia, Moldova, Poland, Romania, Russian Federation, Slovakia, (former) Soviet Union, Ukraine
South Europe	Cyprus, Greece, Italy, Malta, Portugal, San Marino, Spain, Vatican City

As the choice of the control group directly influences the plausibility of the common trend assumption necessary in difference-in-differences-estimations, it is crucial for the validity of the results. In this paper, the influence of this choice is evaluated by using a number of different control groups. The included groups are Germans (native workers), North and Central Europeans (henceforth Central Europeans), South Europeans and East Europeans. South-East Europeans which would be formed mostly from individuals from the Balkan countries were not used due to the relatively mixed religious structure in those countries. On an a priori base, one might suspect that the combination of Turks and South Europeans might be the most reliable combination as both groups have a similar migration history. As such a point cannot be made for any other treatment-/control-group combination, a variety of tests is used to check the plausibility of the common trend assumption.

In a first step, the characteristics of the respective groups are compared as the common trend assumption is more likely to hold for groups that share similar characteristics and have a similar occupational structure. Additionally, similarities in pre-treatment trends were considered. Finally, pseudo-interventions are used. Note that the interaction terms in equation (2.2) measure the divergence of trends in the treatment and control groups after the event of interest. If the common trend assumption was valid, one would expect τ to be insignificantly different from zero for any arbitrarily defined event before 9/11. As a natural date for this pseudo-intervention that also ensures that enough cases are available before and after the intervention, I choose September 11th, 2000.

As possible outcomes both wages and exits from unemployment into employment were considered. The first measure can be considered the best choice when wages are flexible as discrimination would then manifest itself in the equilibrium wage, while the second might be better if, e.g., labor market rigidities prevent a downward adjustment in the wages in the treatment groups.

For wages, equation (2.2) was estimated on the employment data by Tobit-regressions to account for the fact that wages are top coded at the social security contribution limit. Unfortunately, the covariates that could be used in this estimation were rather limited as there is no information on firm characteristics available in the data and the information on individual characteristics is more limited for the employed than for the unemployed due to the different data sources. Additionally, adding controls for industry or occupation was not possible as one of the major reasons for an eventual wage drop in the treatment group after 9/11 would be the possibility that Arabs were forced into worse and lower paying jobs. Controlling for industry and occupations, however, would prevent this effect from showing up in the estimations and could only uncover wage changes within occupations and industries.

Given this necessarily parsimonious specification, it was not possible to find valid treatment-/control-group combinations using various pseudo-interventions. All estimations showed a statistically significant pre-existing negative trend before 9/11 which would have introduced a downward bias in the difference-in-differences estimates. As 9/11 would also be expected to influence the wages of the treatment group negatively, it would not have been possible to distinguish between the pre-existing trend and a possible effect of the terrorist attacks.

Regarding the effects on employment prospects, some more information on the estimator, the construction of the outcome, and the data preparation is necessary: As only information on the unemployed is needed, I will henceforth focus mainly on the information contained in the BewA and use the employment data only to construct the outcome. First, using the BewA instead of the information on the receipt of unemployment benefits provides a larger coverage of individuals as every period in which unemployment benefits were received has to be accompanied by an unemployment registration but not vice versa. Second, the BewA data contains more information on control variables, allowing a better adjustment for observable differences. The shorter period of time covered by the BewA provides no problem for this analysis since information is available for almost two years prior and more than two years after September 11th, 2001.

While the BewA contains information on the labor market state after the end of unemployment that could in principle be used to construct the outcome variable, the reliability of this information is questionable. In 2002 it became known that placement numbers of the unemployed had been forged by employees of the Federal Labor Agency. Unfortunately, the information on the labor market state after unemployment contained in the BewA is directly affected by this forging. Consequently, I refrain from using this information and take a different approach for the construction of the outcome variable: A person is considered to have switched into employment if a regular, full- or part time employment spell is observed in a 30-day interval around the end of the unemployment spell where using this interval provides some protection from the aforementioned possibility of delayed notification on the end of unemployment. However, I exclude individuals switching into so called *Mini-* or *Midi-*Jobs, which are mostly second occupations with earnings below 400 or 800 € a month respectively.

I restrict the sample to those individuals entering unemployment from January 1st, 2000 to December 31st, 2003. The first date marks the first availability of the BewA-data, while the second date is the end of data availability in the BeH that is needed for the construction of the outcome variable. Note that this implies that I cannot consider the possible impact of the Madrid train bombings on March 11th, 2004.

As the individual observation in the data is the specific episode, a natural way to model the impact of 9/11 is by duration analysis. An additional advantage of this modeling strategy is that it also handles the problems of different entry times into unemployment and possible censoring of episodes at December 31st, 2003 when data coverage ends. As an estimator I use a piecewise exponential regression model where the baseline hazard varies non-parametrically in half-year intervals with the duration of unemployment.

Formally, let $h(t|X_i, T_i, P_i)$ denote the hazard that an individual with characteristics X_i becomes employed at a point in time t given unemployment until t. In an exponential regression framework this is modeled as

$$\begin{aligned} h(t|X_i, T_i, P_i) &= h_0(k) exp(\beta' X_i + \lambda * T_i + \upsilon * P_i + \tau * (T_i * P_i)) \\ &= exp(\beta_0(k) + \beta' X_i + \lambda * T_i + \upsilon * P_i + \tau * (T_i * P_i)), \end{aligned} \tag{2.3}$$

where $\beta_0(k)$ is the estimate for the baseline hazard in interval k, varying in half-year intervals with the duration of unemployment, X_i contains control variables described in greater detail in the following paragraphs, T_i and P_i are the group and period dummies mentioned above and estimation is done separately by gender.

To allow for a declining impact of 9/11, several post-treatment periods $P_{1i}, P_{2i}, P_{3i}, P_{4i}$ are used where P_{1i} is one in the first 180 days after 9/11 and zero afterwards, P_{2i} and P_{3i} are one in the next, respectively the following 180 days after 9/11 and P_{4i} is one from the

541st day after 9/11 onward. The final equation to be estimated is thus

$$h(t|X_i, T_i, P_i) = exp\left(\beta_0(k) + \beta' X_i + \lambda_j * T_i + \sum_{j=1}^{4} \upsilon * P_{ij} + \sum_{j=1}^{4} \tau_j * (T_i * P_{ij})\right). \quad (2.4)$$

Additionally, for the German control group, equation (2.4) was also estimated on a matched sample to improve covariate overlap as described in Imbens and Wooldridge (2008: 41-42). The results were qualitatively identical to the unmatched results with point estimates being closer to zero. Results are available from the author on request.

Control variables include age, and two dummy variables for different degrees of disability. The dummies for disability are formed in line with German disability law that distinguishes those with a degree of disability between 30% and 50% and those with a degree of disability above 50% from the rest of the population. School and post school education is measured by several dummies for having completed higher secondary schooling (*Abitur*), not having completed any post-school training, and having completed university studies. Base alternatives are given by having completed up to medium secondary schooling (*Realschule* and below) and having completed vocational training, which is a common educational combination in Germany. Persons with a foreign degree are sorted into the German categories by the labor agency.

Additionally, 33 dummies for different occupations are included. These are based on the so called *Berufsbereiche* (fields of occupations) that group similar occupations. I also add 12 dummies for the individual's place of living and dummy variables for regional labor market conditions. The latter are based on an analysis by Blien et al. (2004) that clustered regions (*Arbeitsagenturbezirke*) with similar labor market conditions.

2.3 Descriptive results and validity of comparison groups

Table 2.2 presents basic descriptive results for the variables in the estimation sample. Before considering the groups in detail, two facts should be noted. First, there are very few academics in the estimation sample, while the share of those with no post-school training is sometimes as high as 77%. Since the risk to become unemployed tends to be lower for those with a higher education, this can be explained by the sampling procedure that focuses on the unemployed. Note that the relatively low number of Germans without any post-school training can be explained by the high prevalence of vocational training in Germany.

Secondly, the much larger share of those with a degree of disability of 50% and higher compared to those with a degree of disability between 30% and 50% may be explained by institutional settings. German disability law grants a number of benefits to those with a degree of disability greater than 50%.[3] Since disability has to be proven by a voluntary medical examination, incentives to take such an examination are higher for those who may receive such benefits and therefore for those with a higher degree of disability. Additionally, one can imagine that labor market prospects tend to be worse with a higher degree of disability, thus resulting in a higher probability to be unemployed.

Turning to the comparison of the treatment and control groups, consider first the results for men shown in the top panel of the table. Note that while observable differences between groups are not necessarily harmful in difference-in-differences estimations, large differences in observables might be a hint for differences in unobservables that could be problematic. Focusing on the differences between the treatment and control groups, one notices several differences in characteristics between the treatment and control groups. However, none of these seems large enough to justify excluding one of the groups from the analysis. A similar result can be seen for the women whose characteristics are shown in the bottom panel of table 2.2. While the differences between treatment and control groups are much larger than for men, there is again no compelling evidence that one of the groups is completely unsuited for the analysis.[4]

Now consider the results for the pseudo-intervention on September 11th, 2000 shown in table 2.3. Note first that all interaction terms are insignificant on common levels. However, one should note that for some treatment-control group combinations this fact is caused by large standard errors of the estimates rather than small coefficients. This fact calls for

[3] Under some circumstances obtaining the same benefits is possible for those with a degree of disability between 30% and 50%. This, however, is only possible if the persons would be unable to find work if benefits were not granted (cf. § 2 SGB IX).

[4] The distribution of occupations between the different treatment and control groups was also compared as a similar occupational structure in the two groups would imply that these are similarly affected by industry or occupation specific business cycles. Again, while there were sometimes large differences in occupations – again larger for the women – none of these justified dropping any of the groups. Finally, a graphical comparison of trends in all groups also revealed no differences in trends before 9/11. Detailed results are available on request.

Table 2.2: Descriptive statistics

Variable	Turks	Arab Muslims	Other Muslims	Germans	Central Europe	South Europe	East Europe
			Men				
Entry into employment (1 = yes)	.0969	.0795	.0935	.1445	.1195	.1210	.1251
	.3197	.2948	.3132	.3840	.3423	.3598	.3561
Age (years)	33.70	33.81	35.93	35.97	40.51	36.33	38.18
	10.81	9.34	9.54	12.01	11.07	11.34	11.12
Higher schooling (1 = yes)	.0485	.1448	.2000	.1277	.2312	.0556	.1915
	.2148	.3520	.3997	.3338	.4217	.2292	.3935
No post-school training (1 = yes)	.7634	.7685	.7456	.2718	.3943	.7378	.4744
	.4250	.4218	.4356	.4449	.4888	.4399	.4994
Academic degree (1 = yes)	.0201	.0631	.0620	.0537	.0938	.0247	.1195
	.1404	.2432	.2412	.2253	.2916	.1552	.3244
Disability 30% (1 = yes)	.0050	.0024	.0007	.0084	.0051	.0040	.0046
	.0709	.0490	.0272	.0911	.0715	.0631	.0674
Disability 50% (1 = yes)	.0353	.0217	.0141	.0385	.0368	.0394	.0224
	.1844	.1456	.1178	.1924	.1882	.1946	.1478
No. of cases	23,569	5,814	5,401	371,236	3,698	10,246	8,544
			Women				
Entry into employment (1 = yes)	.1019	.1013	.1207	.1701	.1648	.1499	.1015
	.3456	.3428	.3845	.4261	.4059	.4207	.3482
Age (years)	34.40	31.81	35.70	37.03	39.46	36.39	37.44
	11.57	10.05	10.32	11.81	10.83	11.51	10.60
Higher schooling (1 = yes)	.0484	.1252	.2612	.1638	.2975	.1006	.3070
	.2147	.3311	.4394	.3701	.4572	.3008	.4613
No post-school training (1 = yes)	.7670	.7232	.6457	.2673	.4027	.6830	.4836
	.4228	.4476	.4784	.4426	.4905	.4654	.4998
Academic degree (1 = yes)	.0165	.0601	.1202	.0656	.1330	.0421	.1618
	.1274	.2378	.3253	.2476	.3397	.2009	.3683
Disability 30% (1 = yes)	.0035	0	0	.0070	.0060	.0057	.0026
	.0595	0	0	.0833	.0770	.0755	.0511
Disability 50% (1 = yes)	.0335	.0107	.0166	.0356	.0330	.0362	.01490
	.1799	.1030	.1280	.1852	.1786	.1869	.1211
No. of cases	12,962	1,214	1,922	270,638	2,518	5,577	8,392

Means, standard deviations below.

caution as it may hint towards real differences that are masked by imprecise estimates.

Focus first on the results for men displayed in the top panel of that table. For Turks, the results of the pseudo-intervention show relatively small, insignificant coefficients when using either Germans or East Europeans as comparison groups. Using Central or South Europeans yields somewhat larger, though still insignificant and relatively small estimates.

The case is less clear for Arab Muslims. Here, all estimations show rather large, though insignificant coefficients. Note, however, that all point estimates are positive which implies that a possible bias of the respective coefficient in the main estimation would be upwards. As it seems unlikely that the 9/11-attacks increased the job prospects of the treatment group, this would still allow an interpretation of an estimated statistically significant, negative coefficient in the main equation as a lower bound for the effect of interest.

Using non-Arab Muslims as the treatment group, all point estimates are again insignificant and rather small compared with some results obtained for the other groups.

Now consider the results for women shown in lower panel of table 2.3. For Turks and Arab Muslims, one obtains rather large and negative point estimates which could cause difficulties in the case of a negative, statistically significant effect in the main analysis. For non-Arab Muslims, the point estimates show rather small effects, except when using Central Europeans as the control group, which again might allow a lower-bound interpretation.

Considered jointly, the results from the pseudo-interventions suggest relatively minor problems with the male sample as point estimates are generally small and always insignificant. For women the results call for some care: While the estimates are always insignificant, the point estimates show sometimes large, negative effects that may invalidate the main analysis.

Taking the results from the descriptive comparisons of the groups characteristics and the pseudo-intervention on September 11th, 2000 to assess the validity of the control groups, one may draw the following conclusions: First, there is no evidence that any treatment/control group pair is completely unsuited for the analysis. Second, the similarities between treatment and control groups are generally greater for men than for women, suggesting that more care should be taken with the latter.

Table 2.3: Pseudo-intervention September 11th, 2000, piecewise constant exponential regression duration model, interaction terms only

Treatment group vs.	Germans	Central Europeans	South Europeans	East Europeans
	Men			
Interaction pseudo-event*Turk	0.0118	-0.0625	-0.1088	0.0240
	(0.0970)	(0.2526)	(0.1666)	(0.1725)
Interaction pseudo-event*Arab Muslim	0.2895	0.2420	0.1645	0.3154
	(0.2344)	(0.3375)	(0.2743)	(0.2824)
Interaction pseudo-event*non-Arab Muslim	-0.0130	0.0419	-0.0733	0.0760
	(0.1936)	(0.3020)	(0.2434)	(0.2416)
	Women			
Interaction pseudo-event*Turk	-0.1765	0.0457	-0.2444	-0.3247
	(0.1538)	(0.2643)	(0.2329)	(0.2501)
Interaction pseudo-event*Arab Muslim	-0.3267	-0.3542	-0.2824	-0.5755
	(0.4617)	(0.5409)	(0.5299)	(0.5133)
Interaction pseudo-event*non-Arab Muslim	0.0584	0.2276	0.0217	-0.0189
	(0.3484)	(0.4610)	(0.4268)	(0.4123)

Coefficients, standard errors in parentheses. ***/**/*/+ denote significance on the 0.1%, 1%, 5% and 10% level respectively. Full estimation results are available from the author on request.

2.4 Results

Consider first the main estimation results for men displayed in table 2.4. Full estimation results are available from the author on request. Before turning to the interaction terms, note that the period dummies are generally associated with an, often significant, decline in the employment prospects regardless of the control group used. Membership in the treatment group is generally associated with worse employment prospects, although this effect is not always statistically different from zero when considering Turks and Non-Arab Muslims.

Consider now the interaction terms giving the effect of 9/11 on the respective treatment group. First, note that all coefficients are insignificant on any conventional level indicating no negative effect of 9/11. Second, there is also no general pattern in the point estimates that suggests, e.g., that the effect might be largest close to 9/11 and declining with time passed since the attacks. Finally, comparing the results with the point estimates from the pseudo-interventions also suggests no general relative decline, except for the Arab-Muslims where all coefficients for the interaction terms are smaller than those obtained in the pseudo-interventions. However, taking standard errors into account, none of these differences is statistically significant.

Now consider the results for women shown in table 2.5. The results for the period dummies are relatively similar to the results for men: All periods after 9/11 are associated with worse employment prospects. For the respective treatment group dummy, one generally finds negative point estimates, but they are significant only for the Turks.

Focusing on the interaction terms as the parameters of central interest, one obtains results similar to those for men: First, none of the interaction terms is significant on any conventional level. Additionally, there is again no pattern in the point estimates suggesting any larger effect directly after 9/11. In fact, if anything, most of the point estimates suggest a relative improvement for the treatment groups directly after the 9/11-attacks. Finally, if one compares the point estimates to those obtained in the pseudo-intervention, one again does not find any hint of a general relative decline in the employment prospects after 9/11.

Returning to my initial question, whether the job prospects of Arab or Muslims in the German labor market have been harmed by the September 11th attacks, one might note

Table 2.4: Estimation results, parameters of interest, piecewise constant exponential regression duration model, men only

Variable	Control group			
	Germans	Central Europeans	South Europeans	East Europeans
Treatment group: Turks				
Treatment group	-0.0432	-0.1985	-0.1527	-0.5174***
	(0.0473)	(0.1296)	(0.0807)	(0.0914)
Period 1: 09/11/01 - 03/10/02	-0.2979***	-0.3126*	-0.5299***	-0.4568***
	(0.0154)	(0.1550)	(0.1083)	(0.1107)
Period 2: 03/11/02 - 09/06/02	-0.2416***	-0.3171	-0.4046***	-0.4416***
	(0.0171)	(0.1798)	(0.1168)	(0.1274)
Period 3: 09/07/02 - 03/05/03	-0.3318***	-0.6737**	-0.4301***	-0.7574***
	(0.0187)	(0.2190)	(0.1251)	(0.1584)
Period 4: 03/06/03 - 12/31/03	-0.6701***	-0.9632***	-0.8810***	-1.0133***
	(0.0207)	(0.2298)	(0.1404)	(0.1640)
Treatment group * Period 1	-0.1069	-0.1560	0.0730	-0.0097
	(0.0778)	(0.1725)	(0.1332)	(0.1336)
Treatment group * Period 2	-0.1248	-0.0846	0.0015	0.0541
	(0.0841)	(0.1978)	(0.1421)	(0.1506)
Treatment group * Period 3	-0.1345	0.1656	-0.0827	0.2771
	(0.0916)	(0.2361)	(0.1506)	(0.1788)
Treatment group * Period 4	-0.1876	0.0522	-0.0346	0.1386
	(0.1041)	(0.2497)	(0.1705)	(0.1891)
Treatment group: Arab Muslims				
Treatment group	-0.2379*	-0.4544**	-0.3914**	-0.7513***
	(0.1043)	(0.1740)	(0.1239)	(0.1345)
Period 1: 09/11/01 - 03/10/02	-0.2951***	-0.2554	-0.4860***	-0.4396***
	(0.0154)	(0.1535)	(0.1049)	(0.1098)
Period 2: 03/11/02 - 09/06/02	-0.2387***	-0.2094	-0.3804***	-0.3842**
	(0.0171)	(0.1764)	(0.1154)	(0.1260)
Period 3: 09/07/02 - 03/05/03	-0.3286***	-0.5216*	-0.3473**	-0.6836***
	(0.0187)	(0.2124)	(0.1246)	(0.1558)
Period 4: 03/06/03 - 12/31/03	-0.6665***	-0.8312***	-0.7771***	-0.8960***
	(0.0207)	(0.2290)	(0.1388)	(0.1619)
Treatment group * Period 1	-0.0914	-0.1700	0.0675	-0.0118
	(0.1745)	(0.2278)	(0.2044)	(0.2030)
Treatment group * Period 2	0.0431	-0.0520	0.1271	0.1108
	(0.1879)	(0.2551)	(0.2204)	(0.2256)
Treatment group * Period 3	-0.0147	0.0874	-0.0561	0.2409
	(0.2163)	(0.3024)	(0.2450)	(0.2646)
Treatment group * Period 4	-0.2816	-0.1776	-0.2041	-0.0780
	(0.2478)	(0.3379)	(0.2804)	(0.2924)
Treatment group: non-Arab Muslim				
Treatment group	0.0541	-0.2197	-0.0351	-0.4308***
	(0.0904)	(0.1617)	(0.1125)	(0.1209)
Period 1: 09/11/01 - 03/10/02	-0.2949***	-0.2665	-0.4673***	-0.4323***
	(0.0154)	(0.1526)	(0.1044)	(0.1115)
Period 2: 03/11/02 - 09/06/02	-0.2391***	-0.2455	-0.3889***	-0.4143**
	(0.0171)	(0.1745)	(0.1166)	(0.1295)
Period 3: 09/07/02 - 03/05/03	-0.3297***	-0.5996**	-0.3991**	-0.7122***
	(0.0187)	(0.2154)	(0.1277)	(0.1598)
Period 4: 03/06/03 - 12/31/03	-0.6676***	-0.9096***	-0.8152***	-0.9424***
	(0.0207)	(0.2310)	(0.1416)	(0.1656)
Treatment group * Period 1	0.0075	-0.0023	0.1531	0.1740
	(0.1531)	(0.2148)	(0.1848)	(0.1867)
Treatment group * Period 2	-0.2385	-0.2289	-0.1407	-0.0298
	(0.1908)	(0.2566)	(0.2185)	(0.2256)
Treatment group * Period 3	-0.0170	0.1781	-0.0215	0.3750
	(0.1849)	(0.2821)	(0.2156)	(0.2371)
Treatment group * Period 4	-0.3063	-0.1438	-0.2030	-0.0071
	(0.2178)	(0.3112)	(0.2543)	(0.2694)

Coefficients, standard errors in parentheses. ***/**/*/+ denote significance on the 0.1%, 1%, 5% and 10% level respectively. Full estimation results are available from the author on request.

Table 2.5: Estimation results, parameters of interest, piecewise constant exponential regression duration model, women only

Variable	Control group			
	Germans	Central Europeans	South Europeans	East Europeans
Treatment group: Turks				
Treatment group	-0.4827***	-0.6732***	-0.2739*	-0.4608***
	(0.0699)	(0.1523)	(0.1150)	(0.1256)
Period 1: 09/11/01 - 03/10/02	-0.3421***	-0.5594**	-0.4088**	-0.6147***
	(0.0157)	(0.1771)	(0.1366)	(0.1446)
Period 2: 03/11/02 - 09/06/02	-0.3451***	-0.6885**	-0.3717*	-0.6813***
	(0.0183)	(0.2144)	(0.1560)	(0.1643)
Period 3: 09/07/02 - 03/05/03	-0.5540***	-0.8542***	-0.6023***	-0.8522***
	(0.0208)	(0.2440)	(0.1681)	(0.1732)
Period 4: 03/06/03 - 12/31/03	-0.9314***	-1.0878***	-1.2253***	-1.2236***
	(0.0230)	(0.2441)	(0.1917)	(0.1921)
Treatment group * Period 1	0.1506	0.3171	0.1507	0.3428
	(0.1061)	(0.2055)	(0.1728)	(0.1797)
Treatment group * Period 2	-0.0283	0.2954	-0.0472	0.2779
	(0.1227)	(0.2486)	(0.2000)	(0.2048)
Treatment group * Period 3	-0.1938	0.0926	-0.1705	0.0671
	(0.1431)	(0.2833)	(0.2193)	(0.2227)
Treatment group * Period 4	-0.3126	-0.2140	-0.0837	-0.0815
	(0.1653)	(0.2970)	(0.2531)	(0.2472)
Treatment group: Arab Muslims				
Treatment group	-0.3419	-0.4022	-0.2804	-0.3769
	(0.2253)	(0.2905)	(0.2598)	(0.2547)
Period 1: 09/11/01 - 03/10/02	-0.3411***	-0.4951**	-0.4038**	-0.5514***
	(0.0157)	(0.1781)	(0.1342)	(0.1408)
Period 2: 03/11/02 - 09/06/02	-0.3442***	-0.5780**	-0.3525*	-0.6686***
	(0.0183)	(0.2159)	(0.1574)	(0.1641)
Period 3: 09/07/02 - 03/05/03	-0.5531***	-0.8133***	-0.5854***	-0.8517***
	(0.0208)	(0.2446)	(0.1699)	(0.1740)
Period 4: 03/06/03 - 12/31/03	-0.9295***	-1.0391***	-1.1840***	-1.1884***
	(0.0230)	(0.2492)	(0.1915)	(0.1925)
Treatment group * Period 1	0.0729	0.1429	0.1132	0.2982
	(0.3355)	(0.3722)	(0.3609)	(0.3557)
Treatment group * Period 2	0.1674	0.3440	0.2248	0.5047
	(0.3445)	(0.4130)	(0.3890)	(0.3785)
Treatment group * Period 3	0.1307	0.5460	0.3366	0.4703
	(0.4146)	(0.4772)	(0.4457)	(0.4389)
Treatment group * Period 4	-1.8313	-1.5748	-1.3512	-1.5249
	(1.0203)	(1.0336)	(1.0370)	(1.0341)
Treatment group: non-Arab Muslim				
Treatment group	-0.1653	-0.2061	-0.0801	-0.1298
	(0.1538)	(0.2164)	(0.1969)	(0.1856)
Period 1: 09/11/01 - 03/10/02	-0.3419***	-0.6024***	-0.4267**	-0.6067***
	(0.0157)	(0.1792)	(0.1371)	(0.1453)
Period 2: 03/11/02 - 09/06/02	-0.3451***	-0.7459***	-0.3948*	-0.7502***
	(0.0183)	(0.2208)	(0.1615)	(0.1717)
Period 3: 09/07/02 - 03/05/03	-0.5540***	-0.9762***	-0.6074***	-0.9017***
	(0.0208)	(0.2512)	(0.1732)	(0.1786)
Period 4: 03/06/03 - 12/31/03	-0.9305***	-1.1932***	-1.2091***	-1.2648***
	(0.0230)	(0.2539)	(0.1925)	(0.1970)
Treatment group * Period 1	-0.1413	-0.0034	-0.1809	-0.0097
	(0.2637)	(0.3129)	(0.2909)	(0.3043)
Treatment group * Period 2	0.0334	0.3604	0.0723	0.3368
	(0.2550)	(0.3350)	(0.2983)	(0.3149)
Treatment group * Period 3	-0.2356	0.1351	-0.1383	0.1052
	(0.2984)	(0.4043)	(0.3529)	(0.3563)
Treatment group * Period 4	-0.2015	0.0782	0.0711	0.1200
	(0.3300)	(0.4237)	(0.3914)	(0.3892)

Coefficients, standard errors in parentheses. ***/**/*/+ denote significance on the 0.1%, 1%, 5% and 10% level respectively. Full estimation results are available from the author on request.

that at least for the definition of the treatment groups used in this paper, there are no compelling signs that such a decline has occurred. First, there is no statistically significant effect for any treatment/control group combination. Second, there is also no general trend in the point estimates which suggests a declining effect over time or a decline relative to the results from the pseudo-interventions. Given these results, it seems at least safe to conclude that 9/11 did not cause a large or severe decline in the job prospects of Arab or Muslim men or women. This result is also similar to other results for Europe and the US by Åslund and Rooth (2005), Braakmann (2007), and Kaushal, Kaestner and Reimers (2007), who also could not find a negative effect on employment.

However, two critical comments on the results are in order: First, the fact that exits from the labor force or even the country are unobserved might bias the results as those who suffer most from discrimination are probably the most likely to exit. While I am not aware of any reports of mass emigration from Germany and while there is also no sharp drop in the number of cases in the treatment groups at any time after 9/11, there is no way to control for this possible bias.

Second, the results might to a certain extent be driven by the definitions of the treatment and control groups. Using current nationality to define the groups was necessary due to data limitations but is certainly not optimal from a theoretical point of view. First, while the definition used in this paper is robust to recent nationality changes, it is impossible to identify German-born second or third generation migrants or naturalizations prior to 1993. Additionally, it is not completely clear what characteristics are driving possible discrimination although there are some hints suggesting that appearance or names may play a role (see Allen and Nielsen 2002: 35-37 and Carlsson and Rooth 2007). As far as these unobserved characteristics are similar between the treatment and control groups, the estimate for the effect of 9/11 might be biased downward. However, while some caution is in order, the results by Braakmann (2007) for England do not seem to suggest that different choices for the definition of the treatment groups are the main factor driving the results.

2.5 Conclusion

This paper deals with the question whether the job prospects of Arabs and Muslims in the German labor market were harmed by the terrorist attacks of September 11th, 2001. Given a large body of anecdotal evidence on discrimination or – more generally – on a shift in attitudes towards Arabs and Muslims, a decline in job prospects in the aftermath of 9/11 seemed possible. Administrative labor market data from social security and unemployment insurance and regression-adjusted difference-in-differences estimators were used to assess whether 9/11 had a negative impact on the probability for Arab unemployed to enter employment.

The results indicate that the job prospects of Arabs in the German labor market have not been harmed by the terrorist attacks. While this may seem somewhat counterintuitive given the anecdotal evidence mentioned at the beginning of this text, it is perfectly in line with the Swedish results by Åslund and Rooth (2005) and evidence for England (Braakmann 2007). A reason for the result, however, is not easily deduced from the empirical evidence. It may be the case that attitudes, at least in Europe, did not change very much. Although this result would certainly shed a positive light on the European population's ability to distinguish between a group of radicals and a whole population group, it would also mean discarding all anecdotal evidence.

Other explanations, also mentioned by Åslund and Rooth (2005) in the conclusions to their paper, may be that employers act non-discriminatorily in their hiring behavior or that discrimination is based on some deeper preferences that remain unaffected by singular events like 9/11. However, it remains unclear what these deeper preferences should reflect, given their relative inflexibility to an event like 9/11.

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2.7 Data cleansing

This appendix describes the data problems regarding the nationality variable as well as the cleansing procedures adopted. As already pointed out in section 2.2, the data is originally not gathered for scientific purposes but generated in administrative processes. As a result, data reliability is very good when variables are needed by the administration. Examples for those variables include year of birth, which is derived from the social security number, and information on wages which is needed for the calculation of unemployment benefits and contributions to social security.

A large amount of variables, however, is collected for statistical purposes and has no direct relevance for the administrative process that generates the data. This fact leads to some variation in data quality, depending on the specific situation in the respective firm or labor agency, and some inconsistencies over time. A prominent example for such a variable is – unfortunately – the nationality variable on which this analysis is based.

There are some known or suspected problems with this variable depending on the respective data source. Briefly summarized these are:

1. Employers generally tend to collect the nationality information at the beginning of the employment and are known to be fairly ignorant towards later changes, e.g. naturalization of foreign workers. This leads to a situation where nationality changes are observed with some delay and to a large extent only when a worker changes firms.

2. Episodes from the LeH (receival of unemployment benefits) are often not accompanied by a personal contact between the labor agency employee responsible for filling

out the data sheet and the respective unemployed individual. This fact makes it seem likely that either mistakes from earlier periods are carried onward without correction or that nationality is left blank if it is unknown to the respective labor agency employee.

3. In general, participation in active labor market programs as well as unemployment registrations are accompanied by some personal contact between the respective individual and the person that is responsible for gathering the data. However, even information from these sources cannot be considered bullet-proof since collection of this information is known to vary regionally as well as with the stress level of the respective labor agency employee. However, one might assume that the main problem with these data sources is missing data rather than wrong information.

To overcome these problems a number of cleansing procedures based on the needs for this analysis as well as on some assumptions were applied:

1. Since changes between nationalities that end up in the same treatment or control group are unimportant for the scope of this analysis, the nationalities found in the data were aggregated as shown in Table 2.1. This eliminates some of the changes that were caused by changes in the political landscape, e.g. the end of the Soviet Union.

2. If information from different data sources was available for the same period of time, e.g. an LeH-spell as well as a BewA-spell for periods of unemployment where unemployment benefits were received, this information has been used as follows:

   (a) Missing values were replaced by the values from a parallel spell if there was only one nationality observed during the same period.

   (b) In case different nationalities were observed during the same period, all were set to missing.

3. Finally, for those cases where only one nationality and missing values were observed over the whole period 1990-2004, the missing values were replaced by that nationality.

Chapter 3

Islamistic terror and the job prospects of Arab men in England: Does a country's direct involvement matter?

Abstract:[1] *This paper examines whether the labor market prospects of Arab men in England are influenced by recent Islamistic terrorist attacks. We use data from the British Labour Force Survey from Spring 1999 to Winter 2006 and treat the terrorist attacks on the USA on September 11th, 2001, the Madrid train bombings on March 11th, 2004 and the London bombings on July 7th, 2005 as quasi-experimental events that may have changed the attitudes towards Arab or Muslim men. Using treatment group definitions based on ethnicity, country of birth and religion, evidence from difference-in-differences-estimators combined with matching indicates that the real wages, hours worked and employment probabilities of Arab men were unchanged by the attacks. This finding is in line with prior evidence from Europe.*

Keywords: Discrimination, September 11th, Islamistic terror, employment, wages

[1]The first version of this paper was published as *University of Lüneburg Working Paper in Economics No. 70* in December 2007. The author would like to thank Joachim Wagner for helpful hints and overall support. Comments at the annual meeting of the European Economic Association in Milan in August 2008 and suggestions by three anonymous referees are gratefully acknowledged. All remaining errors are my own. All calculations were performed using Stata 9.2 SE (StataCorp 2005). All do-files are available from the author on request.

JEL Classification: J71, J79

3.1 Introduction

Following the terrorist attacks on September 11th, 2001, a number of studies have been concerned with the (economic) causes (e.g. Krueger and Malečková 2003, Abadie 2006, Piazza 2006, Krueger and Laitin 2007) or consequences of terrorism (e.g. Abadie and Gardeazabal 2003, 2007, Abadie and Dermisi 2006, Frey, Luechinger and Stutzer 2007). In that literature a small but growing number of papers have been concerned with the consequences of the 9/11-attacks for Arabs or Muslims living in western countries.[2]

Directly after the attacks a number of reports collected by various organizations suggested a rise in discrimination and hostility toward persons perceived to be Arabs or Muslims (see Allen and Nielsen (2002) for Europe as well as the Arab American Institute (2003) and the Arab-American Anti-Discrimination Committee (2003) for the US). On a theoretical level, there are two possible explanations why we might expect such a change in attitudes to affect the job prospects for Arabs or Muslims. The first is taste discrimination (Becker 1957/1971) where a change in preferences induced by the terrorist attacks leads to a lower willingness to pay for the labor of the discriminated group. This effect could be grounded in preference changes by employers as well as fellow employees and customers who could feel "uncomfortable" employing, working alongside, or being served by persons they suspect to be terrorists. A second explanation might be statistical discrimination (Phelps 1972, Arrow 1973) if the attacks revealed some previously unknown characteristic of Arabs related to their labor productivity. Both theories predict a downward change in the equilibrium wages of the discriminated groups that may either show up in the data as a change in wages if these are sufficiently flexible or in the probability of employment when rigidities prevent the adjustment of wages.

Up to today, four studies have investigated whether the anecdotal evidence was accompanied by observable changes in the labor market prospects of Arabs or Muslims. A short overview of these studies can be found in table 3.1, a detailed description is provided in the following paragraphs.

[2]There has also been some interest in the question whether other immigrant groups have been harmed in the aftermath of the 9/11-attacks, see Orrenius and Zavodny (2006).

Table 3.1: Impact of 9/11 on labor market prospects of Arabs: previous evidence

Study	Country	Treatment / Control groups	Outcome	Results
Dávila, Mora (2005)	US	based on country of birth: US-born white non-Hispanics vs. Middle-East Arab men, vs. African Arab men vs. Iranian, Pakistani, Afghan men	hourly wages	large wage penalty for Middle Eastern Arabs (more than 20%), less for Afghan, Pakistani, Iranian men, no effect for African Arabs
Kaushal, Kaestner, Reimers (2007)	US	US-born (excluding Asians), other immigrants (1st and 2nd generation) vs. 1st and 2nd generation immigrants from variety of “Arab” countries	weekly wages hourly wages hours worked employment intrastate mobility	weekly and hourly wages reduced by approx. 9-11% by 9/11, evidence for temporary decline, no effect on hours worked and employment, intrastate mobility was reduced
Åslund, Rooth (2005)	Sweden	based on country of birth: Middle East, Sweden, other Nordic countries, Western, Eastern Europe Former Yugoslavia, Latin America, Asia, Africa	exits from unemployment	no effect for any group relative to any other
Braakmann (2008)	Germany	based on current nationality: predominantly Muslim countries partitioned into Arab / non-Arab countries Turks treated sepearately vs. Germans, vs. Central Europeans, vs. South Europeans, vs. East Europeans	exits from unemployment	no effect for any treatment group compared to any control group

For the US, Dávila and Mora (2005) use data from the American Community Surveys and focus on the wages of younger men between 25 and 40 years of age. Using linear and quantile regression as well as decomposition techniques, they find that the wages of men from Middle Eastern countries have been harmed most by the attacks, while less of an impact could be found for African Arabs and other Arabs relative to US-born non-Hispanics.

Also focusing on the US, Kaushal, Kaestner, and Reimers (2007) use regression-adjusted difference-in-differences-estimates on Current Population Survey data to assess changes in job prospects and mobility for persons from predominantly Muslim / Arab countries relative to natives and other migrants. Their results indicate that the real wages and weekly earnings of Arab men were temporarily reduced by an amount of 9% to 11% as a consequence of the attacks. Furthermore, they find hints that intrastate mobility of Arab men was reduced by the 9/11-attacks, while employment probabilities and hours worked were uninfluenced by the attacks.

In the first study for Europe, Åslund and Rooth (2005) focus on exits from unemployment for men in Sweden. They use difference-in-differences-estimators on administrative labor market data and look at the development of employment prospects of individuals from the Middle East relative to a number of control groups. Their findings indicate that there has been no significant drop in re-employment probabilities for the treatment group compared to natives, individuals from the Nordic countries and from former Yugoslavia, Western and Eastern Europeans, Latin Americans, Asians, and Africans that could be attributed to the 9/11-attacks.

In a similar study for Germany, Braakmann (2008) uses regression-adjusted difference-in-differences-estimators on administrative data from the Federal Employment Agency and Social Security records. Using various treatment and control group definitions, his findings confirm the results by Åslund and Rooth (2005) that the employment prospects of Arabs have not been harmed by the attacks.

Unfortunately, the picture that emerges from these studies is far from clear. At a first glance, the results seem to differ between the US and Europe. However, the factors driving these differences are not entirely obvious. A first possible explanation is that there are genuine differences in the respective population's change in attitudes towards Arabs or Muslims. An obvious explanation for this different reaction to the attacks would be the fact that the US were the direct target of the 9/11-attacks, while neither Sweden nor Germany were directly affected. While this explanation seems intuitively appealing, there might be another explanation: Both US studies found an effect only when looking at wages, while none of the European studies was able to consider this outcome. Consequently, it is possible that the difference between Europe and the US is spurious and simply reflects the respective researcher's choice of the outcome variable rather than a real difference. One should note, however, that given the highly institutionalized nature of both the Swedish and the German labor market and the high prevalence of job protection laws and similar regulations in both countries, a decline in the employer's willingness to pay may easily show up in reduced employment instead of lower wages. In that context, focusing on England might also allow for better comparisons with the US as the British labor market is more similar to the United States in terms of flexibility (see OECD 2004a, chapter 2 and OECD 2004b).

This study attempts to address these issues by using British labor market data from the Quarterly Labor Force Survey for the years 1999 to 2006. In a first step, the results by Kaushal, Kaestner and Reimers (2007) are replicated using similar sample and treatment/control group definitions and the same labor market outcomes – real weekly and hourly wages, hours worked and the probability of being employed – to provide comparative evidence between Europe and the US. In a second step, the question whether a country's direct involvement in acts of terrorism matters for changes in the job prospects of Arabs or Muslims is investigated. Here, the fact that there have been two very similar large scale terrorist attacks in Europe, the Madrid train bombings on March 11th, 2004 and the London bombings on July 7th, 2005, leads to a quasi-experimental situation as both attacks share a number of similarities: Both were large scale bomb attacks on public transport with not identical, but not too different numbers of victims. Additionally, while none of them is directly comparable to the 9/11-attacks in scope, both have received very similar attention in Europe. As England was directly affected only by the second attack, different changes in the job prospects of Arabs or Muslims after the respective attacks would provide suggestive evidence that a country's direct involvement makes indeed a difference.

The remainder of this paper is organized as follows: The data used is described in section 3.2. Section 3.3 describes the empirical setup and the results for the replication of the 9/11-attacks. The following section 3.4 deals with the impact of the Madrid and London bombings and attempts to answer the question whether England's direct involvement in the latter made any difference. Finally, section 3.5 concludes.

3.2 Data

The data used in this study come from the British (Quarterly) Labour Force Survey (LFS), a survey conducted by the Office of National Statistics since 1973.[3] The data are representative for the population of households living at private addresses or National Health Service institutions. The survey is collected quarterly since Spring 1992. From 1992 to May 2006 data collection took place in a seasonal pattern with surveys being conducted in winter (December to February), spring (March to May), summer (June to August) and autumn (September to November). Due to EU regulations the LFS moved to calendar quarters in May 2006 with surveys now covering the periods January to March, April to June, July to September and October to December. Note, however, that it is possible to control for seasonal variation as the reference week the particular answers relate to can be identified.

[3] The data can be accessed via the UK data archive, see http://www.data-archive.ac.uk for further information. To facilitate replication all Stata do-files used in the analysis can be obtained from the author.

The current sample size is approximately 50,000 responding households in Great Britain with an additional 2,000 being added from Northern Ireland resulting in a coverage of 0.1% of the target population. Each household is surveyed in five consecutive quarters in a rotating panel design. Since roughly one fifth of the respondents enter and leave each quarter there is an 80% overlap between two adjacent quarters.

The survey is designed to provide information on the labor market status and personal situation of individuals living in the UK during a reference period, usually a specific week. The questionnaire therefore encompasses information on employment, including information on the current employer, socio-demographic characteristics, education, and wages as well as information on the respective household. Most importantly for the scope of this paper, the data contain information on a respondent's ethnicity, country of birth and religion which can be used to construct treatment and control groups. Additionally, the data are informative on a number of relevant labor market outcomes and control variables. As the data also contain the date of the interview and the reference week the information relates to, it is also possible to assess whether a specific individual was observed before or after any of the events of interest.

In this paper three different working definitions of "Arabs" or "Muslims" are considered.[4] The first definition is based on an individual's country of birth. I follow Kaushal, Kaestner and Reimers (2007) as closely as possible and treat individuals born in Algeria, Bangladesh, Egypt, Indonesia, Iran, Iraq, Lebanon, Malaysia, Morocco, Pakistan, Somalia, Sudan, Tunisia, Turkey and all Middle East countries with the exception of Israel as members of the treatment group. As controls I use individuals born in Britain and those born in southern Africa, Asia, South America, and the Caribbean.

The second definition that is only used in the second part of the analysis due to a change in coding in the year 2000 is based on self-assessed ethnicity, where those individuals reporting a "Pakistani" or "Bangladeshi" ethnicity form the treatment group. This definition is in line with findings on Islamophobia by the European Monitoring Centre on Racism and Xenophobia (2006, p. 17) stating that Pakistanis and Bangladeshis have the highest risk of being victim of a racially motivated crime. Additionally, the majority of (migrant) Muslims in the UK originates from those two countries (European Monitoring

[4]In a previous version, a fourth definition based on current nationality was also used. However, sample sizes were too low for a reliable analysis.

Centre on Racism and Xenophobia 2006, p. 22). As comparison groups I use individuals with a British ethnicity and those reporting any other non-white ethnicity.

Note that I follow Kaushal, Kaestner, and Reimers (2007) in excluding Indians from the comparison groups. First, Indians might be expected to look somewhat similar to Pakistani and Bangladeshi as these stem from almost the same region. Second, while the Indian population is predominantly Hindu, there is also a strong Muslim minority. These two facts suggest that at least some part of the Indian population might be subject to similar discrimination as the respective treatment group after any of the events considered in this study. Consequently, while Indians do not belong to the treatment group, they might also be problematic as a control group. If, for instance, a comparison between the treatment group and Indians suggested no effect of an event, it would be impossible to decide whether this finding was caused by equal changes in discrimination in the treatment and control group or whether it was caused by the absence of discrimination.

Finally, since Spring 2002 the data contain information on the respondents' religion. Here, the treatment group is formed by Muslims who are being compared with Christians and other religions respectively. Sikhs are excluded from the control group "other religions" as several reports (e.g. Allen and Nielsen 2002) suggest that these were often confused with Muslims. Note that due to data availability, we can only use this definition in the second part of the analysis dealing with the Madrid and London bombings.

I make two further restrictions to the sample: First, to provide comparative evidence to the US results, I follow Kaushal, Kaestner and Reimers (2007) and restrict the sample to individuals between 21 and 54 years of age. Second, as there are only few women belonging to the treatment groups in the sample, the analysis is restricted to men. Further sample restrictions that are specific to the respective analysis are discussed in the corresponding section.

3.3 Replicating the US results on September 11th

3.3.1 Data preparation and econometric approach

In this section, the US results by Kaushal, Kaestner and Reimers (2007) are replicated for Britain. Remember from the introduction, that previous US studies (Dávila and Mora

2005, Kaushal, Kaestner and Reimers 2007) suggested a decline in wages for Arabs or Muslims after 9/11, while no impact could be found for employment or working hours. European studies had to focus on employment where no impact could be found in either Sweden (Åslund and Rooth 2005) or Germany (Braakmann 2008). To decide whether this previous evidence should be interpreted as a real difference between the US and Europe or whether it is a spurious difference caused by different choices of the respective outcome, the following analysis uses the same outcomes as Kaushal, Kaestner and Reimers (2007), specifically (log) real weekly and log real hourly earnings, hours worked per week and the probability of employment.

Similar to the baseline specification in Kaushal, Kaestner and Reimers (2007), the sample is restricted to the years 1999 to 2004 (before the Madrid bombings). Given this time frame, only groups based on country of birth can be considered as information on religion is available only from 2002 onwards and the coding of the ethnicity information was changed in 2000. Again similar to Kaushal, Kaestner and Reimers (2007), control groups are formed by (a) natives and (b) other migrants, where I restrict this group to individuals from Asia, South America and Africa. In line with the US samples, Indians are excluded from the control groups as these may face similar discrimination as the treatment groups due to similar appearance and the fact that a strong minority of Indians are Muslims.

To enhance similarities between the treatment and control groups, each member of the treatment group in the post-treatment period is matched to a corresponding member of the control group in both the pre- and post-treatment period and to a corresponding member of the treatment group in the pre-treatment period (see Blundell and Costa-Dias 2008, pp. 56-59 for a theoretical discussion; the case of cross-sectional difference-in-differences relevant for this paper is treated in detail on page 59). Matching is based on variables that are unaffected by the treatment, more specifically age, dummy variables for educational attainment, marital status and health problems, the number of children under 16 and the month of the respective reference week the respondents' answers refer to. Note that we cannot match on occupations, regions or industries as these may be affected by the treatment. The final sample is then formed by all members of the treatment group in the post-treatment period and their corresponding matches in the treatment group before treatment and the control group before and after treatment. Some descriptive statistics can be found in table 3.6 in the appendix.

On this sample, I use a standard regression-adjusted difference-in-differences estimator of the form

$$y_i = \alpha + \beta' X_i + \gamma * d_i + \delta * t_i + \tau * (t_i * d_i) + \epsilon_i \tag{3.1}$$

where y_i is the respective outcome of interest, ϵ_i is an error term, X_i contains the control variables already used for matching, d_i is a dummy variable indicating whether an individual belongs to the respective treatment group and t_i is a period dummy that is one after September 11th, 2001. Again, control variables that may be affected by the treatment, like occupations and industries, are excluded. For (log) hourly wages, (log) weekly wages and weekly hours worked as dependent variables, equation (3.1) is estimated by OLS while the probability of being employed is estimated by standard Probit regression.[5]

To test the validity of the central common trend assumptions, two pseudo-interventions are used. These are again identical to the interventions used by Kaushal, Kaestner and Reimers (2007), specifically September 11th, 1999 and April 1st, 2000. Results are shown in table 3.2. The results indicate no significant, pre-existing trend for wages and hours, although the point estimates for the treatment group compared with natives are rather large for weekly wages. For employment, there is strong evidence against the common trend assumption as pre-existing negative trends are found for both pseudo-interventions. Consequently, some care should be taken with results for employment and some of the results for weekly wages when looking at the main results. Furthermore, the results suggest that the control group "other migrants" might be better suited for comparisons than the native group as the point estimates in the pseudo-interventions are generally smaller.

3.3.2 Results

Consider the results for the parameters of interest displayed in table 3.3. Note first that there are no significant effects for any of the interaction terms. However, the point estimates for wages and employment are rather large when using natives as the control group which may be a sign that there is an effect of 9/11 that is disguised by imprecise estimation. One should note though that the point estimates are only slightly larger in absolute terms than

[5] In light of the ongoing discussion whether the coefficients of interaction terms in Probit regressions (or in other nonlinear models) can be interpreted as the treatment effect in a difference-in-differences model (see Ai and Norton 2003 and Puhani 2008), I also used linear probability models. The qualitative conclusions were unaffected.

Table 3.2: Impact of pseudo-interventions

	Log weekly earnings		Log hourly earnings		Weekly hours worked		Probability of employment	
	vs. natives	vs. other migrants	vs. natives	vs. other migrants	vs. natives	vs. other migrants	vs. natives	vs. other migrants
Pseudo-intervention September 11th, 1999								
Interaction	-0.0665	-0.0396	-0.0172	0.0347	-0.8955	-1.6474	-0.2175*	-0.2270*
	(0.0823)	(0.0907)	(0.0814)	(0.0912)	(2.0955)	(2.9791)	(0.0954)	(0.1049)
No. of Obs.	960	968	960	968	960	968	8,352	8,678
Pseudo-intervention April 1st 2000								
Interaction	-0.0807	-0.0077	-0.0454	0.0485	0.1962	-2.0265	-0.1488+	-0.2202**
	(0.0657)	(0.0708)	(0.0651)	(0.0735)	(1.7299)	(2.2417)	(0.0810)	(0.0845)
No. of Obs	960	968	960	968	960	968	8,352	8,678

Coefficients, robust standard errors in parentheses. +/*/**/*** denote significance on the 10%, 5%, 1% and 0.1% level respectively. Results are based on a matched sample for the period 1999 to 2004 where the distribution of covariates in the control groups mimicks that of the treatment group in the post-treatment period. Estimation was based on a regression adjusted difference-in-differences estimator. Included control variables were age (as a second order polynomial), education, marital status, the number of children under 16, a dummy for health problems, region of residence and a set of month dummies. Detailed estimation results are available on request.

those found in the pseudo-interventions, which suggests at most a small relative change caused by 9/11.

When looking at other migrants as the control group, the results are somewhat clearer: Here, all point estimates are not only insignificant, but also relatively small in absolute terms. This finding provides stronger evidence that the job prospects of the the treatment group did not worsen relative to other migrants in the aftermath of 9/11.

These results differ from those obtained by Kaushal, Kaestner and Reimers (2007) in two dimensions: First, the results presented here point to a small to non-existent effect of 9/11, although some of these results may be related to imprecise estimation due to the smaller sample sizes in this study. Second, while their point estimates were rather similar over control groups, the results in table 3.3 suggest that the effects depend on the control group used with smaller effects being found relative to other migrants. Overall, the results presented here do not suggest a large decline in job prospects of the treatment group relative to other migrants, while some care should be taken with the results relative to natives. In the latter case, it is possible that there was a decline in wages that remains undetected due to imprecise estimation.

3.4 Does a country's direct involvement matter? – The Madrid and London bombings

3.4.1 Data preparation and econometric approach

The evidence in earlier papers as well as the results presented in the previous section suggest that there is a difference in the reaction to the 9/11 attacks in the US and Europe. This section considers a simple but compelling explanation for this difference, specifically the fact that the US were the direct target of the attacks while none of the European states was directly involved.

To fix thoughts, consider the ideal setup to test this proposition. To rule out confounding influences of time and other factors we would need to observe the same country at the same point in time in three states: without any terrorist attack, with a terrorist attack in a different country and with the same terrorist attack in the country under investigation.

Table 3.3: Impact of 9/11 on various labor market outcomes

	Individuals born in Arab/Muslim country vs.			
	Natives	Other Non-Europeans	Natives	Other Non-Europeans
	Log real weekly wages		Log real hourly wages	
Observed after 9/11	0.1409**	0.0184	0.1493***	0.0102
	(0.0439)	(0.0442)	(0.0427)	(0.0463)
Treatment group (1 = yes)	-0.0519	-0.1535***	-0.0213	-0.1350**
	(0.0447)	(0.0440)	(0.0446)	(0.0447)
Treatment group * observed after 9/11	-0.1018	0.0197	-0.0882	0.0488
	(0.0633)	(0.0628)	(0.0601)	(0.0625)
No of Obs.	960	968	960	968
	Weekly hours worked		Probability of employment	
Observed after 9/11	-0.8631	-0.1905	0.1107*	-0.0214
	(1.1467)	(1.2394)	(0.0445)	(0.0447)
Treatment group (1 = yes)	0.6993	0.6653	-1.0505***	-0.5270***
	(1.1651)	(1.2813)	(0.0531)	(0.0505)
Treatment group * observed after 9/11	-1.0809	-1.8328	-0.0955	0.0127
	(1.6392)	(1.6651)	(0.0740)	(0.0711)
No of Obs.	960	968	8,352	8,678

Coefficients, robust standard errors in parentheses. +/*/**/*** denote significance on the 10%, 5%, 1% and 0.1% level respectively. Results are based on a matched sample for the period 1999 to 2004 where the distribution of covariates in the control groups mimicks that of the treatment group in the post-treatment period. Estimation was based on a regression adjusted difference-in-differences estimator. Included control variables were age (as a second order polynomial), education, marital status, the number of children under 16, a dummy for health problems, region of residence and a set of month dummies. Detailed estimation results are available on request.

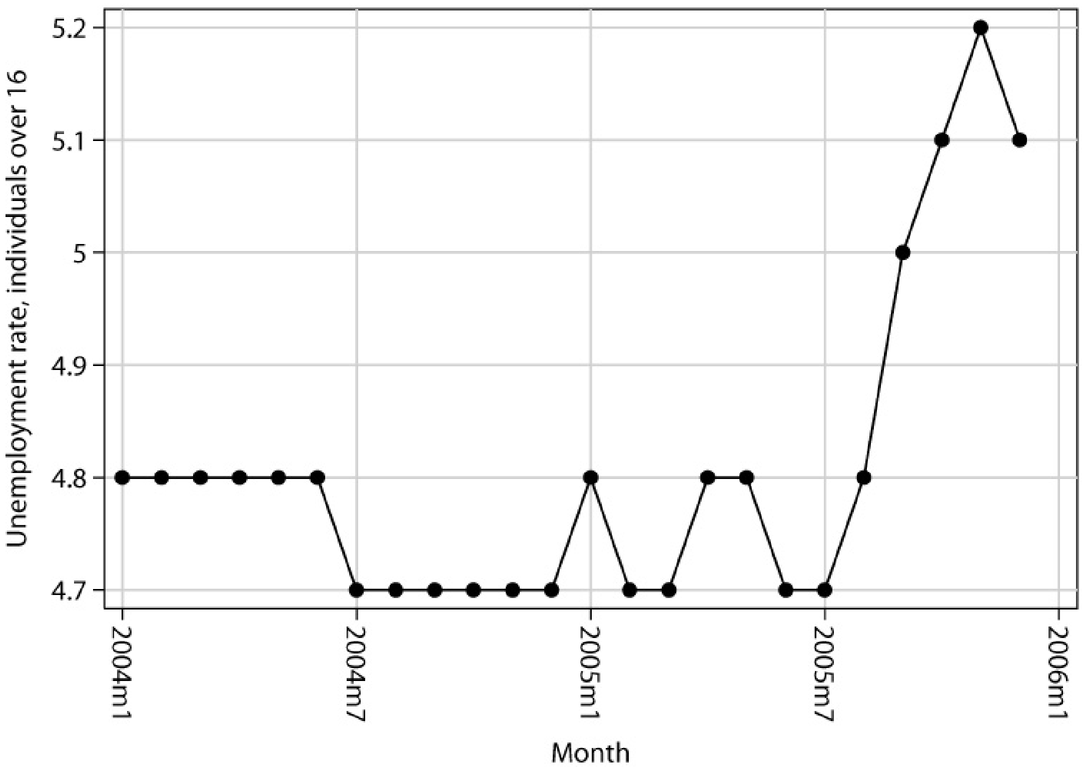
Inference could then be based on simple comparisons between the states. As it is not possible to observe counterfactual worlds, this approach is obviously not feasible.

However, the fact that there were two very similar terrorist attacks in Europe, the Madrid train bombings in Spain on March 11th, 2004 and the London bombings in England on July 7th, 2005, provides a quasi-experimental situation that comes very close to the setup described above. Both attacks were coordinated bomb-attacks directed at public transports and resulted in not too different numbers of casualties and wounded. Additionally, both attacks are comparable in not being the first attacks by radical islamists as both were preceded by the 9/11 attacks. However, both were among the most severe terrorist attacks in post-World War II Europe, making a rise in discrimination against Arabs or Muslims at least possible.

There were also important differences between the attacks that should not be neglected. First, while the Madrid attacks were committed by foreign nationals, the attackers in London were "home-grown" terrorists, three of them born in or near Leeds with the remaining attacker being born in Jamaica. As far as this fact influences shifts in public opinion or discrimination against the groups used in this analysis, the two events may not be fully comparable. Second, the timing of the bombings and the situation in the respective target country were different with the Madrid bombings occurring three days before a national election. However, as we are only interested in the reaction in England, the latter should be less problematic as an upcoming Spanish election should have no great influence on opinions in England. Finally, both attacks occurred at different times which could invalidate comparisons if, for instance, labor supply and demand changed greatly during that period. Fortunately, unemployment in Britain was relatively stable over the period 2004 to 2005, with unemployment rates varying between 4.7 and 4.8 from the beginning of 2004 to the London attacks in July 2005 but rising afterwards (see figure 3.1).

The sample is restricted to the period Spring 2002 when information on religion becomes available to the end of 2006. For this period, we are able to use ethnicity, country of birth and religion to define treatment and control groups. The treatment groups are given by individuals of Pakistani and Bangladeshi ethnicity, individuals born in one of the predominantly Muslim or Arab countries described in section 3.2 and by Muslims. The corresponding control groups are individuals with a British ethnicity and other non-

Figure 3.1: Unemployment rates in UK, 2004 to 2005



Unemployment rates, UK, individuals over 16 years of age, based on LFS data, available via: http://www.statistics.gov.uk/statbase/tsdintro.asp white individuals for *ethnicity*, individuals born in Britain and individuals born in Asia, South America and Southern Africa for *country of birth* and Christians and other religions for *religion*. Sikhs and Indians are excluded from the control groups as both were often confused with Arabs or Muslims (see section 3.2 for further discussion).

The following analysis is again based on a matched sample, created in almost the same fashion as the sample used in the preceding analysis (see section 3.3.1 for a detailed description). There in one difference, though, as for some of the treatment and control group definitions (Arab-born vs. non-Europeans and the groups defined by religion) sample sizes were too low to use matched samples. Here, the analysis relies on all available observations in the respective period. Some descriptive statistics on all samples can be found in table 3.7 in the appendix. Note that sample sizes are extremely small in this analysis which implies that some care should be taken with the results.

For the analysis, I again use a standard regression-adjusted difference-in-differences estimator of the form

$$y_i = \alpha + \beta' X_i + \gamma * d_i + \sum_{j=1}^{2} \delta * t_{ij} + \sum_{j=1}^{2} \tau_j * (t_{ij} * d_i) + \epsilon_i \tag{3.2}$$

where y_i is the respective outcome of interest, ϵ_i is an error term, X_i contains the control variables already used in the preceding analysis, d_i is a dummy variable indicating whether an individual belongs to the respective treatment group and t_{ij} are period dummies for the periods after the respective attacks. In a first step, I define t_1 to be "1" in the period after the Madrid attacks and t_2 to be "1" after the London attacks. In this estimation, τ_2 is a measure for the additional impact of the London attacks relative to the impact of the Madrid bombings. In a second step, the definition of t_1 is changed, so that it takes the value "1" only in the period *between* the Madrid and London bombings. Here, τ_2 measures the impact of the London bombings relative to the situation before the Madrid attacks. For (log) hourly wages, (log) weekly wages and weekly hours worked as dependent variables equation (3.2) is again estimated by OLS while the probability of being employed is estimated by standard Probit and linear regression.

The validity of the central common trend assumption is again tested using pseudo-interventions. For the Madrid-attacks, I choose a pseudo-intervention one year before the real attacks in March 2003. Using the same procedure for the London attacks, however, would place the pseudo-intervention in July 2004. As this would be only four months after the Madrid attacks, the pseudo-intervention for the London attacks is placed in November 2004 which is roughly in the middle of the two events of interest.

Results of the pseudo-interventions are displayed in table 3.4. For most of the treatment/control group combinations, the results show no significant pre-existing trends. However, point estimates are often large which may be problematic, especially as the samples used in these estimations are rather small. There is also one treatment/control group combination where the common trend assumption is rejected. The implications of these findings are discussed in the following section along with the results of the main estimation where necessary.

3.4.2 Results

Consider the difference-in-differences-estimates displayed in table 3.5. Note first that there is only one case – the employment probabilities of Muslims relative to "other religions" – where a negative, significant impact of any of the interventions is found. In contrast, there are five cases where a significantly positive effect is found. A similar proportion of negative to positive point estimates is found when we consider all results regardless of their significance. There is also no coherent picture that emerges if we look at changes relative to the estimates for the pseudo-interventions instead. The only regularity here are the wage estimates for the groups defined by ethnicity and country of birth where the point estimates usually show an (insignificant) decline in wages after the London bombings relative to the pseudo-interventions. Note also that it makes no fundamental differences if we consider the impact of the London bombings relative to the situation before or after the Madrid attacks. Taken together, the results show an almost random pattern of (mostly insignificant) positive and negative estimates.

There are a number of possible explanations for these results – or the lack of a clear pattern of results, for that matter. First, if there was no large effect for any of the attacks in the population, we might expect such a more or less random pattern of results with some treatment groups gaining and some treatment groups losing relative to some of the control groups. If that explanation holds, it seems safe to conclude that the involvement of a country in acts of terrorism alone is not sufficient to explain differences in labor market

Table 3.4: Impact of pseudo-interventions

	Group definitions based on:					
	Ethnicity		Country of birth		Religion	
Controls:	British	Other non-white	Britain/UK	Other non-European	Christians	Other (non Sikh)
	Log real weekly earnings					
Pseudo-intervention Madrid	-0.0354	0.0519	-0.0819	-0.0745	-0.0432	-0.0502
	(0.1231)	(0.1610)	(0.1127)	(0.1309)	(0.1042)	(0.1333)
Pseudo-intervention London	-0.0008	0.1207	0.0467	0.1381	0.0416	-0.0652
	(0.1197)	(0.1467)	(0.1113)	(0.1233)	(0.0911)	(0.1174)
No. of Obs.	348	348	426	442	625	534
	Log real hourly earnings					
Pseudo-intervention Madrid	-0.1231	0.0461	-0.1632	-0.1550	-0.0544	-0.0867
	(0.1272)	(0.1529)	(0.1151)	(0.1320)	(0.1060)	(0.1301)
Pseudo-intervention London	0.1443	0.2888*	0.1190	0.2100+	0.1230	0.0483
	(0.1198)	(0.1406)	(0.1133)	(0.1254)	(0.0943)	(0.1129)
No. of Obs.	348	348	426	442	625	534
	Weekly hours worked					
Pseudo-intervention Madrid	0.0370	1.5492	1.4426	5.4650	0.0465	2.3375
	(3.7377)	(4.5103)	(3.6150)	(3.6807)	(2.7691)	(3.1058)
Pseudo-intervention London	-0.9673	-2.5621	-1.9719	-4.6662	-2.1042	-2.7585
	(3.3016)	(4.2621)	(3.4404)	(3.5132)	(2.5611)	(3.0044)
No. of Obs.	348	348	426	442	625	534
	Employment probability					
Pseudo-intervention Madrid	-0.1493	0.0772	-0.1505	0.1001	-0.0710	0.2106
	(0.1794)	(0.1914)	(0.1588)	(0.1535)	(0.1306)	(0.1832)
Pseudo-intervention London	0.2132	0.1210	0.1773	0.0437	0.1460	-0.2566
	(0.1691)	(0.1810)	(0.1488)	(0.1435)	(0.1166)	(0.1681)
No. of Obs.	3,138	3,138	4,086	4,505	5,656	3,421

Coefficients, robust standard errors in parentheses. +/*/**/*** denote significance on the 10%, 5%, 1% and 0.1% level respectively. Results are based on a matched sample for the period 2002 to 2006 where the distribution of covariates in the control groups mimicks that of the treatment group in the post-treatment period. Estimation was based on a regression adjusted difference-in-differences estimator. Included control variables were age (as a second order polynomial), education, marital status, the number of children under 16, a dummy for health problems, region of residence and a set of month dummies. Detailed estimation results are available on request.

Table 3.5: Impact of Madrid and London bombings on various labor market outcomes

	Group definitions based on:					
	Ethnicity		Country of birth		Religion	
Controls:	British	Other non-white	Britain/UK	Other non-European	Christians	Other (non Sikh)
Log real weekly earnings						
DiD of Madrid bombings (relative to pre-Madrid)	-0.0315	0.1369	0.0392	0.0598	-0.1033	-0.0797
	(0.1181)	(0.1338)	(0.1055)	(0.1176)	(0.0953)	(0.1229)
DiD of London bombings (relative to post-Madrid)	-0.0303	0.0291	-0.1618	-0.0419	0.1744+	-0.0264
	(0.1291)	(0.1317)	(0.1185)	(0.1259)	(0.0901)	(0.1064)
DiD of London bombings (relative to pre-Madrid)	-0.0618	0.1660	-0.1226	0.0179	0.0710	-0.1061
	(0.1297)	(0.1327)	(0.1167)	(0.1285)	(0.0981)	(0.1164)
No. of Obs.	348	348	426	442	625	534
Log real hourly earnings						
DiD of Madrid bombings (relative to pre-Madrid)	-0.0433	0.2099+	-0.0495	0.0062	-0.0618	-0.0589
	(0.1200)	(0.1251)	(0.1041)	(0.1182)	(0.0944)	(0.1182)
DiD of London bombings (relative to post-Madrid)	0.0546	0.1449	-0.0405	0.0294	0.2317**	0.0634
	(0.1267)	(0.1257)	(0.1117)	(0.1206)	(0.0884)	(0.1003)
DiD of London bombings (relative to pre-Madrid)	0.0114	0.3549**	-0.0900	0.0356	0.1700+	0.0045
	(0.1312)	(0.1277)	(0.1086)	(0.1192)	(0.0949)	(0.1101)
No. of Obs.	348	348	426	442	625	534
Weekly hours worked						
DiD of Madrid bombings (relative to pre-Madrid)	-1.7176	-1.1003	2.2358	3.5956	-2.2005	0.2392
	(3.2018)	(3.7996)	(3.0290)	(3.0486)	(2.4578)	(2.7329)
DiD of London bombings (relative to post-Madrid)	0.8338	0.1372	-3.8293	-3.5052	-0.4322	-2.4842
	(3.1703)	(3.9845)	(3.2307)	(3.1140)	(2.4578)	(2.9529)
DiD of London bombings (relative to pre-Madrid)	-0.8839	-0.9631	-1.5934	0.0905	-2.6327	-2.2450
	(3.3323)	(3.7928)	(3.2391)	(2.9907)	(2.5138)	(2.9486)
No. of Obs.	348	348	426	442	625	534
Employment probability						
DiD of Madrid bombings (relative to pre-Madrid)	0.0058	0.1998	-0.0489	0.1222	0.0461	0.1554
	(0.1495)	(0.1602)	(0.1350)	(0.1279)	(0.1120)	(0.1555)
DiD of London bombings (relative to post-Madrid)	-0.0240	-0.0618	0.0819	-0.0137	-0.0174	-0.3356*
	(0.1486)	(0.1619)	(0.1332)	(0.1258)	(0.1064)	(0.1587)
DiD of London bombings (relative to pre-Madrid)	-0.0182	0.1380	0.0330	0.1085	0.0287	-0.1802
	(0.1444)	(0.1552)	(0.1337)	(0.1266)	(0.1131)	(0.1625)
No. of Obs.	3,138	3,138	4,086	4,505	5,656	3,421

Coefficients, robust standard errors in parentheses. +/*/**/*** denote significance on the 10%, 5%, 1% and 0.1% level respectively. Results are based on a matched sample for the period 2002 to 2006 where the distribution of covariates in the control groups mimicks that of the treatment group in the post-treatment period. Estimation was based on a regression adjusted difference-in-differences estimator. Included control variables were age (as a second order polynomial), education, marital status, the number of children under 16, a dummy for health problems, region of residence and a set of month dummies. Detailed estimation results are available on request.

reactions.

Second, it cannot be entirely ruled out that there is an effect in the population that cannot be detected due to small sample sizes. The erratic pattern of results would then be explained by the poor precision of the estimates. However, even using the most parsimonious specification possible where just the group-, post-treatment- and interaction dummy variables are included does not change the pattern of results. Unfortunately, there is also no remedy for this problem as there are no larger data sets for the UK that are available to researchers and cover the events under question.

3.5 Conclusion

This paper uses data from the British Labour Force Survey for the years 1999 to 2006 and regression-adjusted difference-in-differences-estimators to gain further insight into the question whether islamistic terrorism is harmful for the job prospects of Arabs or Muslims living in Western countries. More specifically, this paper attempts to answer two questions.

First, it replicates previous evidence on the reaction to 9/11 for the United States to investigate the question whether the differences found in earlier studies are related to different choices of the outcome variables or whether they reflect genuine differences in the reaction to the 9/11-attacks. Here, the results suggest that the previous findings were not solely driven by differences in the respective outcome, but indeed reflect genuine differences between the US and Europe.

Second, it considers a simple explanation for these differences, specifically the fact that the US were the direct target of the attacks. Here, I use that fact that there were two very similar terrorist attacks in Europe, the Madrid train bombings in 2004 and the London bombings in 2005, which provides a quasi-experimental situation. As both attacks were comparable in scope and choice of weapons but only the latter took place in England, differences in the reactions to the attacks can be interpreted as suggestive evidence for the importance of a country's direct involvement in acts of terrorism. The results do not provide evidence for a change in labor market prospects of Arabs or Muslims after any of the attacks. While the possibility that such an effect remains undetected due to small sample sizes cannot be completely ruled out, the pattern of results suggests an almost random distribution of (mostly insignificant) positive and negative estimates over treatment and control groups.

This evidence is in line with prior studies for Sweden (Åslund and Rooth 2005) and Germany (Braakmann 2008) that found no evidence for an increase in discrimination after the terrorist attacks on September 11th, 2001. Furthermore, it is in line with the reports from the European Monitoring Center on Racism and Xenophobia (2005, 2006) that pointed towards no (lasting) impacts of the terrorist attacks.

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3.7 Appendix: Descriptive Statistics

Table 3.6: Descriptive statistics before/after 9/11

	vs. British		vs. non-European	
	Treatment	Control	Treatment	Control
Before 9/11				
Real weekly wage	447.75	424.31	447.17	476.10
	313.86	229.86	313.33	272.56
Real hourly wage	10.86	10.14	10.85	11.48
	8.33	6.25	8.31	7.27
Weekly hours worked	44.96	44.06	44.94	44.35
	12.42	12.89	12.40	15.37
Employment	0.12	0.36	0.12	0.34
	0.33	0.48	0.32	0.47
No. of Obs.				
- Wages, hours sample	240	240	241	241
- Employment sample	2,088	2,088	2,088	2,086
After 9/11				
Real weekly wage	435.75	502.88	437.29	482.69
	273.60	374.75	271.74	287.80
Real hourly wage	10.67	11.95	10.67	11.61
	6.98	8.81	6.91	7.87
Weekly hours worked	42.75	43.29	42.79	44.09
	12.84	12.91	12.73	11.30
Employment	0.11	0.38	0.10	0.30
	0.32	0.49	0.31	0.46
No. of Obs.				
- Wages, hours sample	240	240	246	240
- Employment sample	2,088	2,088	2,346	2,158

Means, standard deviation below. Samples matched on education, health status, marital status, number of children and age to create covariate overlap.

Table 3.7: Descriptive statistics before/after Madrid and London bombings

Groups defined by:	Ethnicity				Country of birth				Religion			
	vs. British		vs. non-white		vs. British		vs. non-Europeans		vs. Christians		vs. other religions	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
Before Madrid bombings												
Real weekly wage	369.69	425.00	369.69	423.45	439.47	527.02	437.77	548.09	437.34	485.24	437.34	449.93
	294.12	174.04	294.12	344.22	258.71	324.73	257.28	398.01	291.51	288.64	291.51	274.69
Real hourly wage	9.08	10.01	9.08	10.65	11.02	12.95	10.98	13.61	10.50	11.72	10.50	11.38
	7.51	4.73	7.51	8.40	6.42	9.35	6.39	11.26	7.27	7.66	7.27	7.41
Weekly hours worked	40.51	44.09	40.51	40.16	41.44	44.21	41.40	43.64	43.12	42.27	43.12	41.33
	16.02	10.73	16.02	12.93	12.67	12.55	12.58	12.33	13.32	10.54	13.32	9.65
Employment	0.14	0.34	0.14	0.13	0.11 0.37	0.10	0.22	0.14	0.36	0.14	0.31	
	0.35	0.47	0.35	0.34	0.31	0.48	0.30	0.42	0.34	0.48	0.34	0.46
No. of Obs.												
- Wages, hours sample	58	58	58	58	71	71	72	72	90	107	90	77
- Employment sample	523	523	523	523	681	681	769	769	666	998	666	249
After Madrid bombings												
Real weekly wage	395.63	463.51	395.63	400.89	451.64	508.99	444.06	536.14	403.98	530.49	403.98	516.75
	281.71	276.00	281.71	269.76	312.67	242.79	306.55	362.19	263.23	303.35	263.23	274.50
Real hourly wage	9.54	11.10	9.54	9.69	10.62	12.15	10.41	12.76	9.78	12.82	9.78	13.17
	7.16	7.06	7.16	7.01	7.43	6.49	7.31	8.85	6.83	8.25	6.83	7.35
Weekly hours worked	40.25	45.81	40.25	40.46	43.23	45.00	43.63	42.51	42.56	44.25	42.56	41.05
	12.04	10.41	12.04	17.51	13.07	11.58	12.92	14.91	12.41	12.35	12.41	13.69
Employment	0.11	0.33	0.11	0.10	0.10	0.35	0.09	0.21	0.13	0.35	0.13	0.29
	0.31	0.47	0.3	0.30	0.30	0.48	0.29	0.41	0.33	0.48	0.33	0.46
No. of Obs.												
- Wages, hours sample	58	58	58	58	71	71	75	75	107	107	107	80
- Employment sample	523	523	523	523	681	681	741	741	998	998	998	272
After London bombings												
Real weekly wage	426.20	518.46	426.20	380.61	418.44	533.99	426.70	491.48	444.95	497.52	444.95	506.67
	303.44	325.04	303.44	198.53	242.13	339.61	234.97	331.43	291.32	326.20	291.32	290.25
Real hourly wage	10.85	12.41	10.85	8.63	10.76	12.66	10.86	12.10	11.27	11.81	11.27	12.30
	7.48	8.37	7.48	4.46	6.08	8.17	5.97	9.05	7.53	8.36	7.53	6.96
Weekly hours worked	41.43	45.31	41.43	42.63	41.84	46.78	42.08	44.42	43.13	45.56	43.13	43.54
	14.37	11.36	14.37	16.47	15.32	14.12	15.32	10.23	15.66	12.14	15.66	14.07
Employment	0.11	0.33	0.11	0.11	0.10	0.35	0.10	0.24	0.11	0.33	0.11	0.31
	0.31	0.47	0.31	0.31	0.31	0.48	0.29	0.43	0.31	0.47	0.31	0.46
No. of Obs.												
- Wages, hours sample	58	58	58	58	71	71	77	71	107	107	107	73
- Employment sample	523	523	523	523	681	681	804	681	998	998	998	238

Means, standard deviation below. Samples matched on education, health status, marital status, number of children and age to create covariate overlap.

Kapitel 4

Wirkungen der Beschäftigungspflicht schwerbehinderter Arbeitnehmer – Erkenntnisse aus der Einführung des "Gesetzes zur Bekämpfung der Arbeitslosigkeit Schwerbehinderter"

Kurzfassung:[1] *Diese Studie untersucht mit Hilfe eines neu verfügbaren Datensatzes aus Prozessdaten der Bundesagentur für Arbeit, der Stichprobe der integrierten Erwerbsbiographien, die Wirkung einer verpflichtenden Beschäftigungsquote für schwerbehinderte Arbeitnehmer in Deutschland. Wir nutzen die exogene Senkung dieser Quote durch die Einfüh-*

[1]"Effects of the obligation to employ severely disabled workers - findings from the introduction of the "Law to Combat Unemployment among Severely Disabled People". Eine erste Fassung dieses Papiers wurde im Juni 2007 als *University of Lüneburg Discussion Paper in Economics No. 55* publiziert. Die hier vorliegende Fassung ist in der *Zeitschrift für ArbeitsmarktForschung / Journal for Labour Market Research 41(1)*, S. 9-24 erschienen. Der Autor dankt Joachim Wagner für hilfreiche Diskussionen und allgemeine Unterstützung, zwei anonymen Referees für sehr hilfreiche Anmerkungen, Reinhard Bispinck vom WSI-Tarifarchiv der Hans-Böckler-Stiftung für Informationen zu tariflichen Regelungen, Claudia Merten vom Arbeitsstab der Beauftragten der Bundesregierung für die Belange behinderter Menschen für Auskünfte zu vorhandenen Daten und Peter Jacobebbinghaus, Nils Drews, Dirk Oberschachtsiek und Stefan Bender vom Forschungsdatenzentrum der Bundesagentur für Arbeit im Institut für Arbeitsmarkt- und Berufsforschung für hilfreiche Anmerkungen und Diskussionen am Rande des Nachwuchsworkshops "Datenpotentiale für die empirische Sozialforschung - Arbeiten und Leben in Deutschland" des Rates für Sozial- und Wirtschaftsdaten. Alle verbliebenen Fehler liegen in meiner alleinigen Verantwortung. Die in diesem Beitrag verwendeten Daten können über das Forschungsdatenzentrum der Bundesagentur für Arbeit im Institut für Arbeitsmarkt und Berufsforschung bezogen werden, siehe http://fdz.iab.de für nähere Informationen.

rung des "Gesetzes zur Bekämpfung der Arbeitslosigkeit Schwerbehinderter" als natürliches Experiment und schätzen die Änderung in der Wahrscheinlichkeit einer Beschäftigungsaufnahme durch regressionskorrigierte Difference-in-Differences-Schätzer. Unsere Ergebnisse legen nahe, dass die Änderung der Beschäftigungsquote die Beschäftigungschancen von Schwerbehinderten weder verbessert noch verschlechtert hat.

Abstract: *This paper uses new administrative data from the German Federal Employment Agency - the Integrated Employment Biographies Sample IEBS - to assess the impact of a mandatory employment quota for disabled workers in Germany. We use an exogenous change, introduced through the "Law to Combat Unemployment among Severely Disabled People" ("Gesetz zur Bekämpfung der Arbeitslosigkeit Schwerbehinderter"), as a natural experiment and measure the change in the reemployment probability of the unemployed disabled by means of regression-adjusted difference-in-differences estimators. Our results indicate that the change in the employment quota neither enhanced nor worsened the employment prospects of the disabled.*

4.1 Einführung

In Deutschland gelten rund 8% der Bevölkerung als schwerbehindert (vgl. Pfaff 2002, 2004, Rauch/Brehm 2003: 7). Es ist ein stilisierter Fakt, dass die Integration in das Erwerbsleben für diese Bevölkerungsgruppe, nicht zuletzt aufgrund ihrer teilweise beträchtlichen gesundheitlichen Einschränkungen, besonders schwierig ist: Ihre Erwerbsquote liegt beträchtlich unter, ihre Arbeitslosen- bzw. Erwerbslosenquote dagegen beträchtlich über der der Nicht-Schwerbehinderten (vgl. Pfaff 2002, 2004, Rauch/Brehm 2003: 8-10). Eine eventuelle Arbeitslosigkeit dauert darüber hinaus für Schwerbehinderte durchschnittlich 1,5mal so lange wie für Nicht-Schwerbehinderte (vgl. Rauch/Brehm 2003: 13).

Um die in aller Regel unverschuldeten Nachteile der Schwerbehinderten auszugleichen und ihre Integration in das Erwerbsleben zu fördern, hat der Gesetzgeber seit der Verabschiedung des Schwerbehindertengesetzes im Jahr 1974 eine Reihe von speziellen Vorschriften und Förderleistungen zugunsten der Schwerbehinderten geschaffen. Ein zentrales Element der Arbeitsförderung Schwerbehinderter in Deutschland liegt in der Verpflichtung der Arbeitgeber oberhalb einer bestimmten Betriebsgröße, einen bestimmten Teil ihrer Arbeitplätze mit schwerbehinderten Arbeitnehmern zu besetzen oder alternativ eine Ausgleichsabgabe zu zahlen.

Diese Studie nutzt eine Änderung der Beschäftigungspflichtquote durch das "Gesetz zur Bekämpfung der Arbeitslosigkeit Schwerbehinderter" (SchwbBAG[2]) und einen großen repräsentativen Individualdatensatz aus Prozessdaten der Bundesagentur für Arbeit, um Rückschlüsse auf die Wirksamkeit einer solchen Beschäftigungspflicht zu ziehen. Bei der betrachteten gesetzlichen Änderung wurde einerseits die Beschäftigtenpflichtquote von 6% auf 5% der Beschäftigten reduziert, andererseits die bei Nichterfüllung fällige Ausgleichsabgabe zum Teil drastisch erhöht. Diese Regelungen führen damit zu einer Entlastung vor allem kleinerer Firmen, die häufiger von der Beschäftigungspflicht befreit wurden, sowie vor allem für größere Unternehmen zu größeren finanziellen Anreizen Schwerbehinderte zu beschäftigen.

Die mit der Verabschiedung des SchwbBAG verbundenen Hoffnungen waren, ebenso wie die scheinbaren Erfolge, groß: Das SchwbBAG sollte die Arbeitslosigkeit Schwerbehinderter bis zum Oktober 2002 um 25% senken (vgl. Deutsche Bundesregierung 2003: 5). Auf den ersten Blick scheinen diese Hoffnungen auch nicht enttäuscht worden zu sein. Betrachtet man die Entwicklung der Zahl der arbeitslosen Schwerbehinderten auf Makroebene, erkennt man einen stetigen Rückgang von 189.766 Arbeitslosen im Oktober 1999 auf nur noch 144.292 Arbeitslose im Oktober 2002 (vgl. Deutsche Bundesregierung 2003: 19).

Auffällig hierbei ist jedoch der besonders starke Rückgang in der Altersgruppe 55-60 Jahre. Dies könnte ein Hinweis darauf sein, dass der Rückgang der Arbeitslosenzahlen eher auf verstärkte Frühpensionierungen als auf verbesserte Arbeitsmarktchancen schwerbehinderter Arbeitnehmer zurückzuführen ist. Hierzu passt auch das Ergebnis der einzigen bisher zu diesem Thema vorliegenden Studie, die Effekte auf Individualebene betrachtet: Verick (2004) findet mit Daten des Sozio-oekonomischen Panels neben schwachen Hinweisen auf eine Verbesserung der Beschäftigungschancen Schwerbehinderter durch die Regelungen des SchwbBAG auch einen Anstieg der Wahrscheinlichkeit eines Übergangs in die Nichterwerbstätigkeit.

Diese Studie verwendet administrative Individualdaten der Bundesagentur für Arbeit, die Stichprobe der Integrierten Erwerbsbiographien, um der Frage nachzugehen, inwieweit

[2]Für den Wortlaut des Gesetzes vgl. Bundesgesetzblatt Jg. 2000 Teil I Nr. 44 S. 1394-1405.

sich die Arbeitsmarktsituation Schwerbehinderter durch die Anpassung der Beschäftigungsquote zum 1. Januar 2001 verändert hat. Mit Hilfe eines Difference-in-Differences-Schätzers wird die Änderung in der Wahrscheinlichkeit, aus Arbeitslosigkeit in Beschäftigung abzugehen, im Vergleich zu Nichtbehinderten vor und nach Inkrafttreten des SchwbBAG geschätzt. Dieser Ansatz kontrolliert sowohl für zeitkonstante unbeobachtete Unterschiede zwischen den Gruppen als auch für gemeinsame zeitliche Trendeinflüsse und Unterschiede in beobachtbaren Variablen.

Abschnitt 4.2 stellt die für die Untersuchung relevanten rechtlichen Regelungen und den institutionellen Hintergrund dar, Abschnitt 4.3 legt kurz den Stand der jüngeren Forschung zu den Arbeitsmarkchancen Schwerbehinderter sowie der entsprechenden Gesetzgebung dar. Abschnitt 4.4 beschreibt den verwendeten Datensatz, deskriptive Übersichten finden sich in Abschnitt 4.5. Der bereits angesprochene Difference-in-Differences-Schätzer wird in Abschnitt 4.6 näher beschrieben, die dazugehörigen Ergebnisse finden sich in Abschnitt 4.7. Ein kurzes Fazit wird schließlich in Abschnitt 4.8 gezogen.

4.2 Institutioneller Hintergrund

4.2.1 Grundsätzliche rechtliche Regelungen und die Situation der Schwerbehinderten

Das deutsche Behindertenrecht betrachtet Personen als behindert, sofern ihre "körperliche Funktion, geistige Fähigkeit oder seelische Gesundheit mit hoher Wahrscheinlichkeit länger als sechs Monate von dem für das Lebensalter typischen Zustand abweichen und daher ihre Teilhabe am Leben in der Gesellschaft beeinträchtigt ist" (Absatz 1, §2, SGB IX). Die Anerkennung als Schwerbehinderter erfolgt nach medizinischer Untersuchung durch staatlich beauftragte Stellen (ab 1. Juli 2001 Integrationsämter, vorher Hauptfürsorgestellen).

Hierbei wird der sogenannte "Grad der Behinderung" festgestellt. Dieser liegt zwischen 20 und 100 und richtet sich nach dem Grad der Beeinträchtigung der Teilnahme am gesellschaftlichen Leben (vgl. Pfaff 2007: 715). Eine Schwerbehinderung liegt ab einem Grad der Behinderung von 50 vor. Dieser wird beispielsweise bei Verlust eines Armes ab dem Ellbogen oder einer ganzen Hand oder auch einer extremen Kleinwüchsigkeit (Körpergröße zwischen 120 und 130 cm) erreicht.[3]

Personen, bei denen ein Grad der Behinderung zwischen 30 und 50 festgestellt wurde, können den Schwerbehinderten rechtlich gleichgestellt werden, sofern sie ohne diese Gleichstellung keinen geeigneten Arbeitsplatz erhalten können (Absatz 3, §2, SGB IX, früher § 2 SchwbG). Solche Grade der Behinderung werden beispielsweise bei einer Versteifung des Handgelenks (in ungünstiger Stellung) oder beim Verlust zweier Finger erreicht.

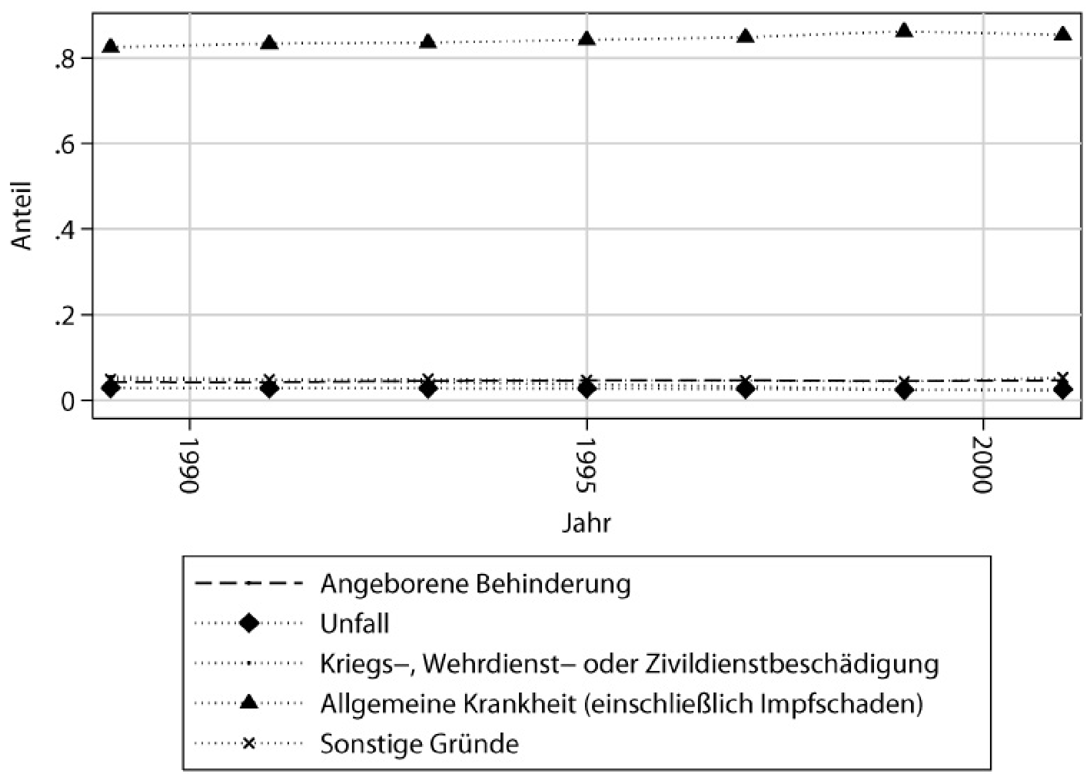
Abbildung 4.1 gibt einen Überblick über die Ursachen einer Schwerbehinderung. Der mit rund 85% weit überwiegende Teil der Behinderten erlangt diesen Zustand aufgrund von Krankheiten. Der Anteil angeborener Behinderungen oder Behinderungen durch Unfälle, Kriegsschäden und sonstige Gründe liegt dagegen jeweils unter 5% der gesamten Schwerbehinderten. Diese Anteile sind während der letzten 10 Jahre annähernd konstant geblieben, wobei der Anteil der Kriegsversehrten (angesichts des sinkenden Anteils der Überlebenden des 2. Weltkriegs an der Bevölkerung wenig überraschend) tendenziell zurückgeht und der Anteil der Krankheitsgeschädigten tendenziell steigt.

Einen Überblick über die Art der jeweiligen Behinderung liefert Abbildung 4.2. Die häufigsten Behinderungen sind hiernach körperliche Behinderungen und Organstörungen. Erstere umfassen den Verlust oder die Funktionsstörung von Gliedmaßen oder Teilen des Rumpfes, letztere beziehen sich auf jegliche Störung des Organsystems, wie bspw. der Lunge, des Herzkreislaufsystems oder der Verdauung. Geistige und seelische Behinderungen, Behinderungen der Sprech- oder Sehfähigkeit sowie sonstige Behinderungen lagen zu Beginn der neunziger Jahre mit einem Anteil von jeweils rund 10% annähernd gleich auf. In den Folgejahren ist ein Anstieg vor allem der geistig/seelischen Störungen sowie der sonstigen Behinderungen zu verzeichnen, während der Anteil der Sprech- und Sehbehinderungen annähernd gleich blieb. Auffällig ist hier insbesondere der starke Anstieg und darauf folgende Rückgang der sonstigen Behinderungen im Jahr 1999, dessen Ursache jedoch unklar ist. Der Anteil der Entstellungen schließlich ist auf niedrigem Niveau über die letzten zehn Jahre konstant.

Eine Anerkennung als Schwerbehinderter hat verschiedene rechtliche Folgen. So genießen Schwerbehinderte einen besonderen Kündigungsschutz, der eine Kündigung nur nach Zustimmung des Integrationsamtes erlaubt. Des Weiteren bestehen in bestimmten Fällen

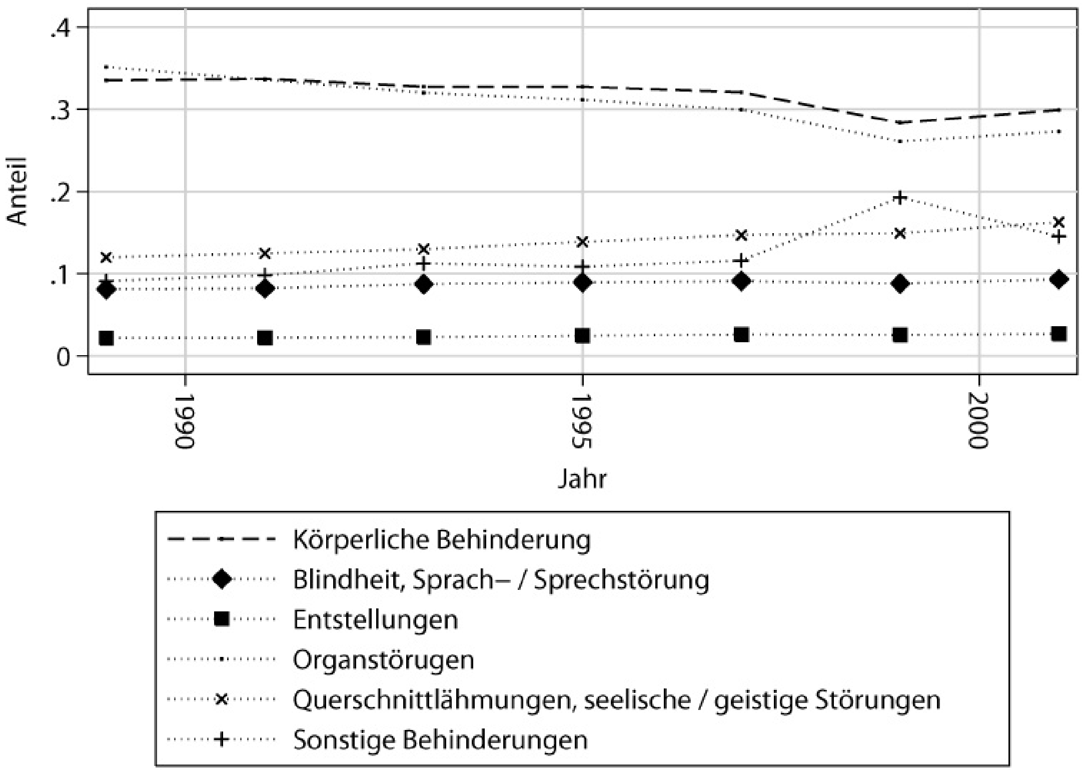
[3] Für weitere Beispiele siehe die "Anhaltspunkte für die ärztliche Gutachtertätigkeit (AHP)", bspw. in Schillings / Wendler (2006).

Abbildung 4.1: Behinderung nach Ursachen



Quelle: Statistisches Bundesamt (2003), eigene Darstellung

Abbildung 4.2: Arten der Behinderung



Quelle: Statistisches Bundesamt (2003), eigene Darstellung

Ansprüche auf steuerliche Vergünstigungen, zusätzliche Urlaubsansprüche sowie Ansprüche auf Frühverrentung. Eine Anerkennung als Schwerbehinderter begründet keine direkten finanziellen Ansprüche (vgl. Bound / Burkhauser 1999: 3509), unter Umständen kann die Behinderung jedoch zeitgleich zu einer Erwerbsunfähigkeit und damit zu einem Anspruch auf weitere Sozialleistungen führen. In diesem Zusammenhang ist zu beachten, dass aus dem Grad der Schwerbehinderung grundsätzlich nicht auf das Vorliegen solcher Ansprüche geschlossen werden kann (vgl. Bundesministerium für Gesundheit und soziale Sicherung 2005: 20, 26). Diese richten sich nur nach tatsächlichen Einschränkungen der Erwerbsfähigkeit (vgl. §43 SGB VI für die Regelungen der gesetzlichen Erwerbsminderungsrente).

4.2.2 Ausgestaltung und Entwicklung der Beschäftigungspflicht Schwerbehinderter

Ein zentrales Element zur Förderung der Arbeitsmarktintegration Schwerbehinderter ist die seit 1974 bestehende Beschäftigungspflicht. Diese verpflichtet Arbeitgeber oberhalb einer bestimmten Beschäftigtenzahl dazu, einen bestimmten Anteil ihrer Arbeitsplätze mit Schwerbehinderten zu besetzen oder alternativ eine Ausgleichsabgabe zu zahlen. Vergleichbare Quotenregelungen existieren in einer Reihe von europäischen Ländern wie Frankreich, Italien, Österreich, Polen und Spanien, mit Einschränkungen in Belgien und den Niederlanden sowie in der Türkei und (Süd-)Korea (vgl. OECD 2003: 210). Den Unternehmen in Deutschland ist dabei weitgehend freigestellt, auf welchen Stellen die Schwerbehinderten beschäftigt werden. Es besteht nur die grundsätzliche Verpflichtung, diese nach ihren Fähig- und Fertigkeiten einzusetzen (vgl. OECD 2003: 211). Ebenso existieren keine gesetzlichen Vorgaben zu ihrer Entlohnung. Diese richtet sich in aller Regel nach den für den jeweiligen Betrieb relevanten tariflichen Regelungen, wobei Tarifverträge üblicherweise keine Sonderregelungen für Schwerbehinderte enthalten.[4] Arbeitgeber haben allerdings die Möglichkeit, für einen begrenzten Zeitraum Lohnzuschüsse der Bundesagentur für Arbeit in Höhe von bis zu 70% des Arbeitsentgeltes in Anspruch zu nehmen (Eingliederungszu-

[4]Der Autor dankt dem WSI-Tarifarchiv der Hans-Böckler-Stiftung für den Hinweis auf die folgenden Ausnahmen: In der Metallindustrie Nordwürttemberg-Nordbaden dürfen Schwerbehinderte bei analytischer Arbeitsbewertung nicht in die untersten beiden Gehaltsgruppen (von zwölf) eingruppiert werden (§6.6.2 des Rahmentarifvertrags für die Metallindustrie Nordwürttemberg-Nordbaden gültig ab 1.4.1988). Zudem darf im Bereich der Landwirtschaft, sowie des Garten-, Landschafts- und Sportplatzbaus für "minderleistungsfähige" Arbeitnehmer von der tariflichen Entlohnung abgewichen werden (§9 des Rahmentarifvertrags für Betriebe des Garten-, Landschafts- und Sportplatzbaus vom 20.12.1995).

schüsse für Schwerbehinderte) (für eine kurze Übersicht siehe ZEW / IAB / IAT 2006: 12).[5]

Wie in praktisch allen Ländern, die Beschäftigungsquoten vorsehen, wird diese in Deutschland in aller Regel nicht erfüllt. Nach Angaben der OECD (2003, S. 210) waren 1996 nur auf 57% der an sich zu besetzenden Plätze tatsächlich Schwerbehinderte beschäftigt, wobei nur ein Achtel der Arbeitgeber die Quote voll erfüllte und ein Drittel der Arbeitgeber keinen Schwerbehinderten beschäftigte. Auch wenn sich diese Zahl nicht vollständig mit den Angaben der Bundesregierung deckt, die für 1996 eine Beschäftigungsquote von 3,9% meldet, was einer Erfüllung von rund 65% entspricht, lässt sich festhalten, dass die Quote seit 1982 nie erfüllt wurde und der Grad der Erfüllung im Zeitverlauf eher sinkt (vgl. Deutsche Bundesregierung 2003: 22).

Ebenso stellt man bei der Betrachtung der Zahl der Arbeitgeber, die ihrer Beschäftigungspflicht nachkommen fest, dass die Zahlen aus dem Jahr 1996 keinen Ausreißer darstellen: Im Oktober 1998 kamen nur 23.400 Arbeitgeber ihrer Verpflichtung in vollem Umfang nach. Dies entspricht angesichts von 188.645 beschäftigungspflichtigen Arbeitgebern wiederum einem Achtel. Der Anteil der Arbeitgeber ohne einen einzigen beschäftigten Schwerbehinderten betrug zu diesem Zeitpunkt rund 38,4%, was ebenso den OECD Ergebnissen von 1996 entspricht (vgl. Deutsche Bundesregierung 2003: 22).

Bis einschließlich 31.12.2000 lag die Beschäftigungspflichtquote bei 6% der Beschäftigten, so dass in Betrieben ab 16 Beschäftigten mindestens ein Schwerbehinderter zu beschäftigen oder alternativ für jede nicht besetzte Stelle eine Abgabe von umgerechnet rund 100 Euro (zum damaligen Zeitpunkt 200 DM) pro Monat zu zahlen war.

Mit Inkrafttreten der entsprechenden Regelungen des SchwbBAG wurde diese Quote zum 1 Januar 2001 auf 5% der Beschäftigten gesenkt, so dass die Beschäftigungspflicht nun erst Betriebe ab 20 Beschäftigten betrifft. Des Weiteren wurden für Betriebe mit weniger als 60 Beschäftigten besondere Rundungsregeln bei der Bestimmung der zu besetzenden Arbeitsplätze eingeführt, die zu einer Verringerung der Pflichtplätze führen. Als Folge dieser Änderung reduzierte sich die Zahl der betroffenen Arbeitgeber von 1999 auf 2001 um rund

[5] Die Eingliederungszuschüsse wurden im Rahmen der Evaluationen der Maßnahmen zur Umsetzung der Hartz-Reformen von einem Projektverbund, bestehend aus dem Zentrum für Europäische Wirtschaftsforschung, dem Institut für Arbeitsmarkt- und Berufsforschung sowie dem Institut für Arbeit und Technik evaluiert. Für die Ergebnisse siehe ZEW / IAB / IAT (2006).

ein Fünftel von 187.437 auf 151.595 (vgl. Deutsche Bundesregierung 2003: 23). Die Zahl der für die Berechnung der zu besetzenden Pflichtplätze zugrunde gelegten Arbeitsplätze sank im gleichen Zeitraum nur geringfügig von 20.444.495 auf 20.414.003 (vgl. Deutsche Bundesregierung 2003: 23). Die Zahl der zu besetzenden Pflichtplätze verringerte sich somit um 205.970.[6]

Zugleich wurde die Höhe der Ausgleichsabgabe an das Ausmaß der Erfüllung der Beschäftigungsquote geknüpft. So müssen Betriebe mit mehr als 60 Beschäftigten, die weniger als 2% ihrer Arbeitsplätze mit Schwerbehinderten besetzt haben, eine Abgabe von 260 Euro je unbesetztem Arbeitsplatz und Monat zahlen. Die Abgabe verringert sich bei einer Beschäftigungsquote von 2 bis 3 Prozent auf 180 Euro und bei einer Quote von 3 bis 5 Prozent auf 105 Euro. Arbeitgeber mit weniger als 40 Beschäftigten zahlen 105 Euro, sofern sie keinen Schwerbehinderten beschäftigen. Für Arbeitgeber zwischen 40 und 50 Beschäftigten gilt eine Ausgleichsabgabe von 105 Euro, sofern weniger als zwei Schwerbehinderte und eine Abgabe von 180 Euro, sofern weniger als ein Schwerbehinderter beschäftigt werden.

Die ökonomischen Auswirkungen dieser Reform auf die Beschäftigungschancen Schwerbehinderter lassen sich theoretisch schwer bestimmen. Einerseits wird eine große Zahl der Arbeitgeber von der Verpflichtung überhaupt Schwerbehinderte zu beschäftigen befreit, was die Beschäftigungschancen der Schwerbehinderten tendenziell verschlechtern sollte. Andererseits wurde die Ausgleichsabgabe je nach Erfüllung der Beschäftigungsquote um bis zu 150% erhöht (Arbeitgeber mit mehr als 60 Beschäftigten und einer Beschäftigungsquote unter 2%), was die Anreize Schwerbehinderte zu beschäftigen zumindest für größere Arbeitgeber erhöhen sollte. Die Frage nach dem Gesamteffekt dieser Änderungen hängt somit davon ab, welcher dieser Effekte überwiegt und lässt sich damit nur empirisch beantworten.

4.3 Stand der Forschung

Die Forschung zur Schwerbehindertengesetzgebung konzentrierte sich auf internationaler Ebene vor allem auf den "Americans with Disabilities Act" (vgl. bspw. Acemoglu / Angrist 2001, Beegle / Stock 2003, DeLeire 2000, Jolls 2004, Jolls / Prescott 2004, Kruse / Schur

[6] 1999: 6% von 20.444.495 = 1.226.670; 2001: 5% von 20.414.003 = 1.020.700

2003, Lee 2003) und in jüngerer Zeit auf den "Disability Discrimination Act" im Vereinigten Königreich (Bell / Heitmüller 2005).[7] Die Ergebnisse dieser Studien deuten typischerweise auf einen eher negativen oder neutralen Effekt der jeweiligen Gesetze hin. Zu beachten ist allerdings, dass es sich bei beiden Gesetzen um Anti-Diskriminierungsgesetze handelt, die mit den deutschen Regelungen einer Beschäftigungspflicht nur schwer vergleichbar sind.

Die einzige Studie für ein anderes Land, die sich mit einer der deutschen Regelung vergleichbaren Situation beschäftigt, liegt seit kurzem in einer vorläufigen Fassung für Österreich vor. Lalive, Wuellrich und Zweimüller (2007) verwenden ein Regression Discontinuity Design, das Schwellenwerte in den österreichischen Gesetzen nutzt und auf Firmenebene aggregierte administrative Daten aus der Sozialversicherung. Ihre Ergebnisse zeigen, dass erstens ein Großteil der Firmen die Zahlung einer Ausgleichsabgabe der Beschäftigung von Schwerbehinderten vorzieht und zweitens mehr Schwerbehinderte in Firmen knapp oberhalb des Schwellenwertes beschäftigt werden als in Firmen, die sich knapp unterhalb des jeweiligen Schwellenwertes befinden, was auf einen positiven Beschäftigungseffekt des österreichischen Gesetzes hindeutet.

Für den deutschen Raum sind empirische Untersuchungen zur Schwerbehindertengesetzgebung, wie auch zur Situation Schwerbehinderter im Allgemeinen, eher spärlich gesät. Eine Ursache hierfür dürfte in der schlechten Datenlage zu sehen sein: Bis zur Veröffentlichung der hier verwendeten Integrierten Erwerbsbiographien, die Informationen zur Behinderung allerdings auch nur für Arbeitslose bereitstellen, war das von Verick (2004) verwendete Sozio-oekonomische Panel (SOEP) der einzige verfügbare Datensatz, der Angaben zu Behinderungen enthielt.[8]

Diery, Schubert und Zink (1997) präsentieren die Ergebnisse einer Unternehmensbefragung in den Regionen Rheinhessen und Trier/Koblenz. Neben allgemeinen Fragen zur Beschäftigung Schwerbehinderter beschäftigen sie sich auch mit der Bedeutung der Ausgleichsabgabe, die für diejenigen Unternehmen anfällt, die weniger als die vorgeschriebene Zahl Schwerbehinderter beschäftigen. Sie kommen zu dem Ergebnis, dass die durch die Abgabe anfallenden Kosten für die Unternehmen eine weitaus geringere Bedeutung haben als

[7] Eine Übersicht über die ökonomische Analyse sozialer Transfersysteme für Behinderte findet sich darüber hinaus in Bound / Burkhauser 1999.

[8] In den Jahren 1999, 2003 und 2005 enthält zudem der Mikrozensus Informationen zu Behinderten. Die Daten sind der Wissenschaft allerdings erst seit kurzem über die Forschungsdatenzentren der Statistischen Ämter zugänglich.

die Kosten, die durch Umbaumaßnahmen und gesetzliche Sonderregelungen für behinderte Arbeitnehmer anfallen.

Die Wirkung der Beschäftigungspflicht schwerbehinderter Arbeitnehmer wird in ihrer "alten", bis 31.12.2000 gültigen Fassung darüber hinaus in einer Reihe ökonometrischer Studien (Kölling / Schnabel / Wagner 2001; Wagner / Schnabel / Kölling 2001a,b; Koller / Schnabel / Wagner 2006) mit verschiedenen Datensätzen untersucht. Betrachtet wird hier jeweils das Einstellungs- bzw. Entlassungsverhalten von Betrieben, die sich nahe an einer der "Schwellen" des Schwerbehindertenrechts befinden, ab denen die Einstellung (weiterer) schwerbehinderter Arbeitnehmer vorgeschrieben ist.

Sowohl die Studien, die die erste Schwelle bei 16 Beschäftigten untersuchen (Kölling / Schnabel / Wagner 2001; Wagner / Schnabel / Kölling 2001a,b), als auch die Untersuchung an der zweiten Schwelle bei 25 Beschäftigten (Koller / Schnabel / Wagner 2006) finden Hinweise auf eine einstellungshemmende Wirkung der Beschäftigungspflicht, wenn ein Betrieb die jeweilige Schwelle durch Neueinstellungen überschreiten würde. Keine der genannten Studien findet Hinweise, dass Betriebe versuchen, der Beschäftigungspflicht durch Entlassungen und ein hierdurch verursachtes Überschreiten der Schwelle von "oben" auszuweichen. Als Datengrundlage dienen jeweils verschiedene Wellen des IAB-Betriebspanels. Im Fall der Studien von Koller, Schnabel und Wagner (2006) wird darüber hinaus auf die "Statistik aus dem Anzeigeverfahren gemäß §13 Abs. 2 SchwbG (St 88)" zurückgegriffen.

Studien auf Individualebene liegen bislang ebenfalls kaum vor. Frick und Frick (1994) finden mit Daten der ersten sechs Wellen des Sozio-oekonomischen Panels (SOEP) einen signifikant positiven Einfluss einer Schwerbehinderung auf die Wahrscheinlichkeit einer Frühverrentung. Lechner und Vazquez-Alvarez (2003) untersuchen mit Hilfe von SOEP-Daten und verschiedenen Matching-Verfahren den kausalen Effekt des Eintretens einer Behinderung auf Beschäftigungschancen und Löhne. Das Eintreten einer Behinderung senkt nach ihren Ergebnissen die Wahrscheinlichkeit einer Beschäftigung nachzugehen um 9,6 Prozentpunkte und das jährliche Lohneinkommen um rund 6.000 DM.

Schließlich untersucht Verick (2004) in der bereits angesprochenen Studie, wiederum mit SOEP-Daten, den Effekt des Inkrafttretens des SchwbBAG im Jahr 2001. Er verwendet hierzu regressionskorrigierte Difference-in-Differences-Schätzer mit linearen Wahrscheinlichkeitsmodellen. Seine Ergebnisse legen nahe, dass die Einführung des SchwbBAG keine

Auswirkungen auf die Wahrscheinlichkeit hat, als Schwerbehinderter beschäftigt zu sein. Er findet allerdings einen leichten Rückgang der Wahrscheinlichkeit arbeitslos zu sein und einen Anstieg der Wahrscheinlichkeit nichterwerbstätig zu sein. Der vorliegende Beitrag knüpft an diese Literatur an und ergänzt diese um die erste Studie, die auf administrative und damit weitgehend verzerrungsfreie Daten zurückgreift. Die im Vergleich zu bisherigen Studien wesentlich größere Fallzahl ermöglicht es zudem im Falle eines nicht-signifikanten Effekts der Änderung der Beschäftigungspflichtquote auszuschließen, dass dieser nur auf einer nicht ausreichenden Fallzahl beruht.

4.4 Daten

Diese Studie verwendet Daten aus den administrativen Prozessen der Bundesagentur für Arbeit sowie den Meldungen der Arbeitgeber zur Sozialversicherung, die sogenannte Stichprobe der Integrierten Erwerbsbiographien (IEBS). Hierbei handelt es sich um eine 2,2%-Stichprobe aus den Integrierten Erwerbsbiographien, die aus prozessproduzierten Daten der Bundesagentur für Arbeit und der Sozialversicherung bestehen.

Angaben zu sozialversicherungspflichtiger Beschäftigung entstammen den arbeitgeberseitigen Meldungen zur Sozialversicherung, die in der Beschäftigten-Historik (BeH) zusammengefasst werden. Hinzu kommen Angaben zum Bezug von Entgeltersatzleistungen aus der Leistungsempfänger-Historik (LeH), ab dem 1.1.2000 zur Teilnahme an Maßnahmen der aktiven Arbeitsmarktpolitik aus der Maßnahme-Teilnehmer-Gesamtdatenbank (MTG) sowie, ebenfalls ab dem 1.1.2000, Angaben zu Arbeitssuche und Arbeitslosigkeit aus den operativen Vermittlungssystemen der BA (BewA). Für eine kurze Beschreibung der IEBS siehe Jacobebbinghaus und Seth (2007), eine ausführliche Dokumentation einschließlich Merkmalsauszählungen findet sich in Hummel et al. (2005).

Insgesamt enthält die IEBS über alle vier Quellen Angaben zu 1.370.031 Personen mit 17.049.987 Episoden (Spells) (vgl. Hummel et al. 2005: 6). Beobachtungseinheit in der IEBS sind Beschäftigungsepisoden, was dazu führt, dass die jeweils für einen Zeitraum in den Daten verfügbaren Merkmale je nach Ursprungsquelle variieren. So enthalten Episoden aus der MTG und BewA beispielsweise andere Merkmale zur schulischen und nachschulischen Ausbildung als solche aus der BeH oder LeH. Aufgrund des Stichprobendesigns (in die IEBS gelangen alle Personen, die an einem von acht Tagen geboren wurden, vgl. Hummel et al. 2005: 7) ist eine Aggregation der Daten auf Betriebsebene, anders als bei Lalive, Wuellrich und Zweimüller (2007), nicht möglich. Eine im Kontext dieser Studie besonders relevante Einschränkung besteht darin, dass die Informationen zum Schwerbehindertenstatus nur in Episoden der BewA und damit nur für Arbeitslose verfügbar sind. Dies bedeutet u.a., dass weder Schwerbehinderte unter den Beschäftigten identifiziert werden können noch die Berechnung von spezifischen Erwerbs- oder Arbeitslosenquoten für Schwerbehinderte möglich ist.

Die in den Daten verfügbaren Angaben zu einer eventuellen Behinderung richten sich nach den gesetzlichen Definitionen, die in Abschnitt 2.1 dargestellt wurden. Unterschieden werden können hier Individuen mit einem Grad der Behinderung unter 30 (Nicht-Schwerbehinderte), Individuen mit einem Grad der Behinderung zwischen 30 und 50, unterteilt in den Schwerbehinderten Gleichgestellte und Personen, bei denen eine Gleichstellung möglich wäre sowie Schwerbehinderte mit einem Grad der Behinderung von über 50.

Diese Studie vergleicht Schwerbehinderte und Behinderte, bei denen eine rechtliche Gleichstellung möglich wäre mit nicht Schwerbehinderten. Die Kontrollgruppe wird in dieser Studie dementsprechend durch diejenigen Individuen gebildet, die einen Grad der Behinderung von unter 30 aufweisen und die daher nicht unter die Regelungen der deutschen Schwerbehindertengesetze fallen. Für die Treatmentgruppe werden zwei Definitionen verwendet. Die erste "enge" Definition verwendet nur Personen mit einem Grad der Behinderung über 50. Die zweite "weite" Definition schließt zudem den Schwerbehinderten nach §2 SchwbG bzw. SGB IX gleichgestellte Personen mit einem Grad der Behinderung zwischen 30 und 50 ein.[9]

Deskriptive Vergleiche einiger Kernvariablen zwischen diesen drei Gruppen finden sich in Tabelle 5.2. Auffällig ist vor allem das höhere durchschnittliche Alter der Schwerbehinderten sowie ihre höhere durchschnittliche Arbeitslosigkeitsdauer und ihre schlechteren Berufsaussichten. Zu beachten ist ferner das leicht höhere Bildungsniveau der nicht Schwerbehinderten.

[9]Ein auf den ersten Blick besserer Ansatz wäre der Vergleich zwischen gleichgestellten und nicht gleichgestellten Individuen mit einem Grad der Behinderung zwischen 30 und 50. Dieser scheitert jedoch bereits an den zu geringen Fallzahlen. Zudem ist zu beachten, dass die Gleichstellung nur erfolgt, wenn die Arbeitsmarktchancen des jeweiligen Individuums besonders schlecht sind. In diesem Fall wäre die Zugehörigkeit zu der Treatmentgruppe jedoch endogen, so dass eine Modellierung über einen Difference-in-Difference-Schätzer nicht angebracht wäre.

Tabelle 4.1: Deskriptive Statistiken, Sub-Gruppen

Variable	Nicht Schwerbehinderte	Schwerbehinderte mit GdB>50	Schwerbehinderte und Gleichgestellte
Wechsel in Beschäftigung (1 = ja)	0,1300	0,0745	0,0765
	(0,3364)	(0,2625)	(0,2658)
Männlich (1 = ja)	0,5515	0,5948	0,5948
	(0,4973)	(0,491)	(0,491)
Alter in Jahren	38,14	45,57	45,79
	(12,04)	(11,02)	(10,93)
Dauer der Arbeitslosigkeit (Tage)	620,19	772,37	772,98
	(852,67)	(892,70)	(890,42)
Abitur (1 = ja)	0,1232	0,0825	0,0807
	(0,3286)	(0,2751)	(0,2723)
Keine nachschulische Ausbildung (1 = ja)	0,2960	0,3168	0,3103
	(0,4565)	(0,4652)	(0,4626)
Akademischer Abschluss (1 = ja)	0,0492	0,0297	0,0296
	(0,2162)	(0,1698)	(0,1694)
Fallzahl	300.937	10.235	10.996

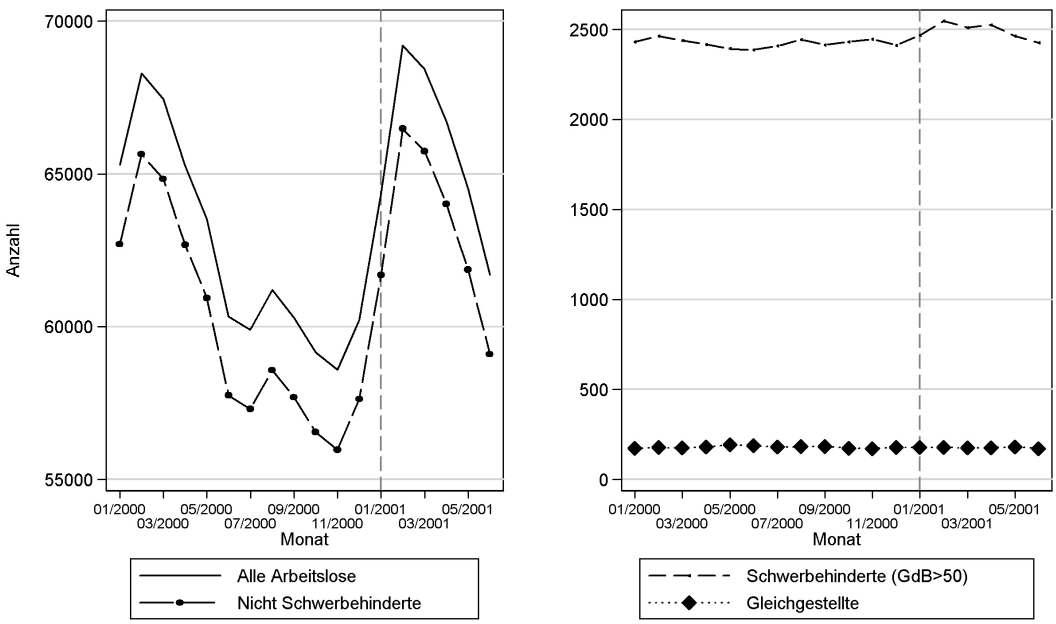
Quelle: IEBS, eigene Berechnungen. Mittelwerte, Standardabweichungen in Klammern.

4.5 Deskriptive Ergebnisse

Betrachtet man in einem ersten Schritt den Bestand an Arbeitslosen in Abbildung 4.3, so erkennt man, dass die Arbeitslosenzahlen der Schwerbehinderten und Nicht-Schwerbehinderten einem ähnlichen Trend folgen. In beiden Gruppen zeigt sich eine saisonale Schwankung mit Spitzen im Februar des jeweiligen Jahres und Tiefständen im Zeitraum Mai bis November mit einem leichten Anstieg im Spätsommer. Diese Schwankung scheint für die Gruppe der nicht Schwerbehinderten stärker ausgeprägt, die relativen Änderungen in beiden Gruppen sind jedoch sehr ähnlich. Diese Grafik verdeutlicht auch, dass ein einfacher Vergleich der Zahl der schwerbehinderten Arbeitslosen vor und nach der Änderung der Beschäftigungsquote zum 1. Januar 2001 irreführend wäre: Zwar geht die Zahl der arbeitslosen Schwerbehinderten nach einem Höchststand im Februar 2001 zurück, allerdings lässt sich der gleiche Rückgang auch für Arbeitslose feststellen, die nicht unter die Regelungen des Schwerbehindertengesetzes fallen.

Ein deutlicheres Bild hinsichtlich der Trends in beiden Gruppen zeigt sich bei den Zugängen in Arbeitslosigkeit, die in Abbildung 4.4 dargestellt werden. Interessant ist hier vor allem, dass sich im Vorfeld des Inkrafttretens der Änderung der Beschäftigungsquote zum 1.1.2001 keine im Vergleich zu Nicht-Schwerbehinderten übermäßige Zunahme der Zugänge für die Schwerbehinderten zeigt. Dies lässt darauf schließen, dass weder die Unternehmen

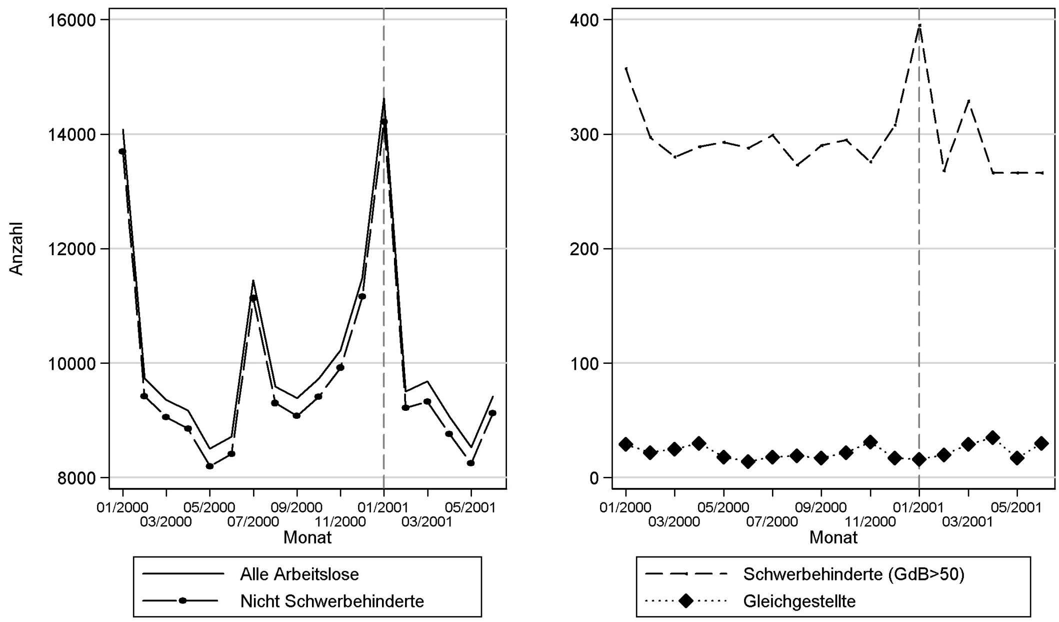
Abbildung 4.3: Bestand an Arbeitslosen



Quelle: IEBS, eigene Berechnungen. Die gestrichelte Linie markiert das Inkrafttreten der Regelungen des SchwbBAG.

im Vorgriff auf die Anpassung verstärkt Behinderte entlassen, noch verstärkt Schwerbehinderte bspw. aus der Nichterwerbstätigkeit in die Arbeitslosigkeit wechseln. Der starke Anstieg der Zugangszahlen im Januar 2001 findet sich in ähnlicher Form auch im Jahr 2000 sowie in den von der damaligen Bundesanstalt für Arbeit veröffentlichten amtlichen Daten. Es ist davon auszugehen, dass es sich hier um einen saisonalen Effekt handelt.

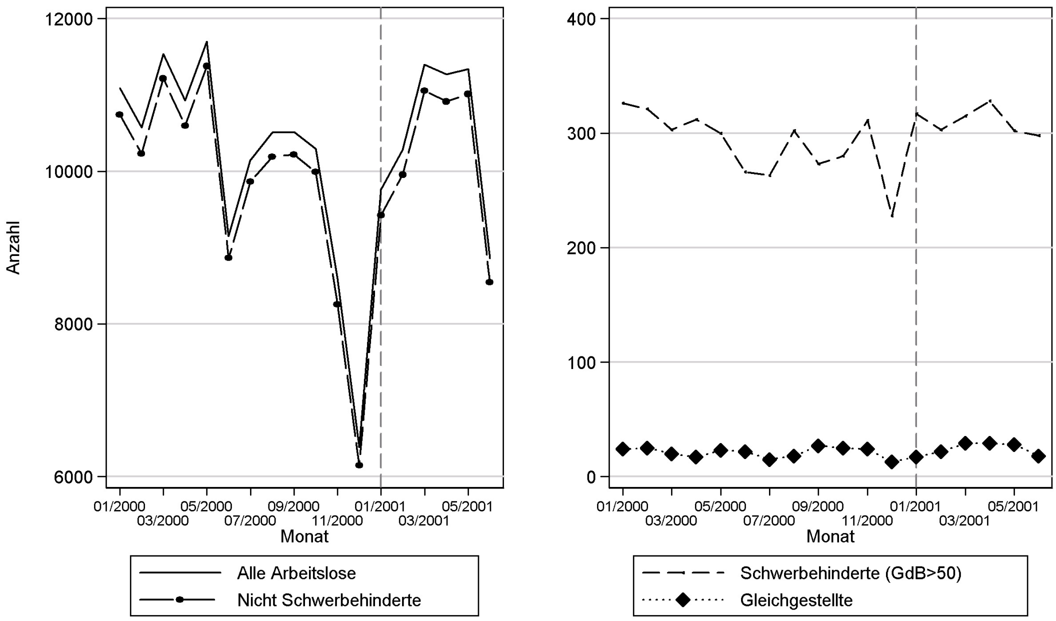
Abbildung 4.4: Zugänge in Arbeitslosigkeit



Quelle: IEBS, eigene Berechnungen. Die gestrichelte Linie markiert das Inkrafttreten der Regelungen des SchwbBAG.

Eine Betrachtung der Abgangszahlen aus der Arbeitslosigkeit kann schließlich erste Hinweise auf die Wirkung der hier betrachteten Gesetzesänderung liefern. Auch hier zeigt sich ein annähernd gleicher Verlauf der Abgangszahlen für Schwerbehinderte und andere Arbeitslose bis Januar 2001. Nach Änderung der Beschäftigtenquote erkennt man eine leichte Abweichung zwischen den Trends in beiden Gruppen, die in einem Rückgang der Abgangszahlen Schwerbehinderter im Februar 2001 deutlich wird. Zu beachten ist allerdings, dass die Zahl der Abgänge in dieser Gruppe ab März wieder ansteigt und sich die Trends in beiden Gruppen wieder angleichen. Der auffällige Rückgang der Abgangszahlen im Dezember 2000 wie auch der anschließende Anstieg im Januar 2001 findet sich auch in den offiziellen Zahlen der Bundesagentur (damals Bundesanstalt) für Arbeit (vgl. Bundesanstalt für Arbeit 2001: 106).

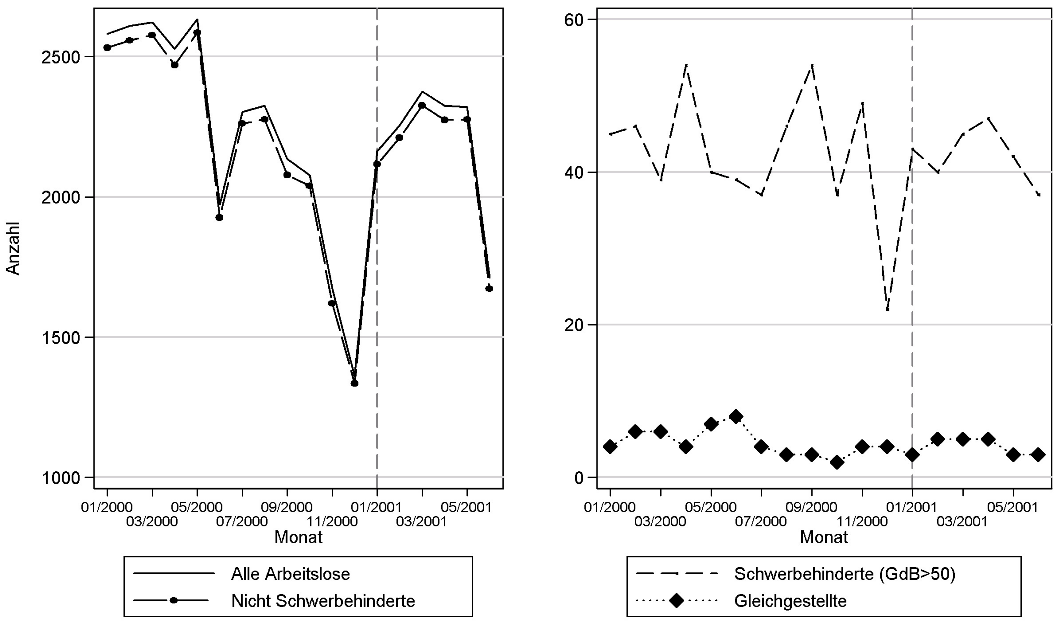
Abbildung 4.5: Abgänge aus Arbeitslosigkeit, beliebiges Ziel



Quelle: IEBS, eigene Berechnungen. Die gestrichelte Linie markiert das Inkrafttreten der Regelungen des SchwbBAG.

Beschränkt man die Betrachtung auf Abgänge aus der Arbeitslosigkeit, die in Beschäftigung münden, so erkennt man einerseits wiederum einen ähnlichen Verlauf in beiden Gruppen, andererseits sind die vorhandenen Abweichungen zwischen diesen jedoch größer als bei den zuvor betrachteten Größen. Die Interpretation dieser Abweichungen sollte jedoch sehr vorsichtig geschehen, da die Fallzahlen in der Gruppe der Schwerbehinderten sehr gering sind. Die scheinbar starken Ausschläge in den Monaten September und November 2001 beruhen letztendlich auf einer Zunahme um rund 10 Fälle, so dass hier nicht von substantiellen Unterschieden ausgegangen werden kann. Aus dem gleichen Grund sollte man bei der Interpretation des Rückgangs der Abgänge in Beschäftigung in der Gruppe der Schwerbehinderten im Februar 2001 Vorsicht walten lassen. Auch wenn dieser auf den ersten Blick auf eine Verschlechterung der Beschäftigungschancen Schwerbehinderter nach Änderung der Beschäftigungsquote hinzudeuten scheint, handelt es sich letztendlich nur um einen Rückgang um weniger als fünf Fälle, der durchaus zufällig zustande gekommen sein kann.

Abbildung 4.6: Abgänge aus Arbeitslosigkeit in Beschäftigung



Quelle: IEBS, eigene Berechnungen. Die gestrichelte Linie markiert das Inkrafttreten der Regelungen des SchwbBAG.

4.6 Ökonometrische Modellierung

Die Schätzung des Effekts des Inkrafttretens des SchwbBAG erfolgt über einen regressionskorrigierten Difference-in-Differences-Schätzer. Dieser vergleicht die Entwicklung der Beschäftigungschancen der Schwerbehinderten, gemessen als Wahrscheinlichkeit aus der Arbeitslosigkeit eine Beschäftigung aufzunehmen, vor und nach Inkrafttreten des SchwbBAG mit der Entwicklung der Beschäftigungschancen einer oder mehrerer Kontrollgruppen, in diesem Fall nicht behinderter Arbeitslose, im gleichen Zeitraum. Durch dieses Verfahren werden zeitkonstante Einflüsse innerhalb der beiden Gruppen, wie auch über die Zeit variierende, die beiden Gruppen gleich beeinflussende Faktoren, eliminiert. In seiner einfachsten Variante hat der Difference-in-Differences-Schätzer die folgende Form:

$$\tau = E[Y|t=1, d=1] - E[Y|t=0, d=1] - (E[Y|t=1, d=0] - E[Y|t=0, d=0]), \quad (4.1)$$

wobei d=1 die Zugehörigkeit zur Treatmentgruppe angibt und t eine Dummyvariable ist, die den Wert "1" annimmt, sofern das jeweilige Individuum nach der Änderung der Beschäftigungsquote zum 1. Januar 2001 beobachtet wird.

Schreibt man (4.1) als Regressionsgleichung, kann man zudem für eventuell noch bestehende beobachtbare Unterschiede zwischen den Gruppen kontrollieren. Der sogenannte regressionskorrigierte Difference-in-Differences-Schätzer, in diesem Fall für wiederholte Querschnittsdaten, hat die folgende allgemeine ökonometrische Spezifikation (vgl. Meyer 1995):

$$y_i = \alpha + \beta' X_i + \delta * d_i + \eta * t_i + \tau * (t_i * d_i) + \epsilon_i \quad (4.2)$$

mit y_i als Wert der jeweiligen abhängigen Variable des Individuums i und X_i als Vektor mit Kontrollvariablen, die die Wahrscheinlichkeit einer Beschäftigungsaufnahme beeinflussen. Der zu schätzende kausale Effekt ergibt sich durch eine Interaktion zwischen dem Schwerbehindertenstatus und der Tatsache, nach Inkrafttreten des SchwbBAG beobachtet zu werden, und wird dementsprechend durch den Parameter τ angegeben.

In diesem Fall handelt es sich bei y_i um eine Dummy-Variable, die "1" ist, wenn das jeweilige Individuum aus Arbeitslosigkeit in Beschäftigung wechselt, so dass (4.2) als lineares Wahrscheinlichkeitsmodell über OLS und als Probit geschätzt wird (für ein ähnliches Vorgehen vgl. Madrian 1994). Die Tatsache, dass hier die Wahrscheinlichkeit eines Wechsels in Beschäftigung und nicht etwa die Wahrscheinlichkeit, beschäftigt zu sein, betrachtet wird, ergibt sich aus einer Einschränkung in den Daten. Da der Schwerbehindertenstatus nur in den Merkmalen aus der BewA enthalten ist, liegt dieser auch nur für Personen vor, die mindestens einen Tag arbeitslos gemeldet waren. Bei Personen, die während des gesamten Zeitraums beschäftigt waren, ist eine Unterscheidung zwischen Schwerbehinderten und Nicht-Schwerbehinderten damit nicht möglich. Der Vektor mit Kontrollvariablen X_i enthält in unserem Fall Angaben zu schulischer und nach-schulischer Ausbildung. Bei ersterer wird in diesem Papier zwischen Personen mit und ohne Abitur unterschieden, wobei letztere in den Schätzungen die Referenzgruppe darstellen. Im Bereich der nachschulischen Ausbildung dienen Personen mit abgeschlossener Berufsausbildung als Referenzgruppe. Des Weiteren wird hier zwischen Personen ohne nachschulische Ausbildung und Personen mit akademischer Ausbildung unterschieden.

Unterschiede in der fachlichen Ausrichtung der gelernten Berufe werden über 33 Dummyvariablen, basierend auf den Berufsbereichen der Bundesagentur für Arbeit abgebildet. Des Weiteren wird für den Wohnort über 11 Dummyvariablen für die jeweilige Regionaldirektion[10] und für regionale Arbeitsmarktcharakteristika über 12 Dummyvariablen, basierend auf der Klassifizierung der Arbeitsagenturbezirke von Blien et al. (2004), kontrolliert. Schließlich wird für das Geschlecht des jeweiligen Individuums wie auch für die Dauer der derzeitigen Arbeitslosigkeit kontrolliert.

Entscheidende Faktoren für die Zulässigkeit einer Modellierung über einen Difference-in-Differences-Schätzer ist die Exogenität des Treatments. Konkret bedeutet dies, dass sowohl eine Selektion in die Treatment-Gruppe wie auch in den Nach-Treatment-Zeitraum ausgeschlossen werden muss.

Eine Selektion in die Treatmentgruppe ist in diesem Fall gleichbedeutend mit einer Selektion in eine Schwerbehinderung. Während eine solche Selektion grundsätzlich, bspw. durch Selbstverstümmelung möglich ist, lässt sich ein solches Verhalten, gerade im Fall der hier betrachteten Senkung der Beschäftigungsquote, mangels entsprechender Anreize wohl ausschließen. Aufgrund der notwendigen medizinischen Untersuchung vor der Anerkennung als Schwerbehinderter sowie der weitgehenden Regelung der Festlegung von Graden der Behinderung in den "Anhaltspunkten für die ärztliche Gutachtertätigkeit" erscheinen

[10] Im Fall der Regionaldirektion "Nord" wird zwischen alten und neuen Bundesländern unterschieden.

sowohl das “Erschleichen” des Schwerbehindertenstatus, wie auch eine eventuelle mildere Behandlung durch die ärztlichen Dienste im Anerkennungsverfahren eher unwahrscheinlich. Zudem lässt sich festhalten, dass sich der Anteil der Schwerbehinderten in der Bevölkerung im Vorfeld der gesetzlichen Änderung nicht nennenswert ändert (siehe z.B. Tabelle 3.1 in der “Statistik der schwerbehinderten Menschen” (Statistisches Bundesamt 2003)).

In diesem Fall kann weiterhin eine Selektion in den Nach-Treatment-Zeitraum ausgeschlossen werden. Da in diesem Papier ausschließlich Arbeitslose betrachtet werden, wäre eine Selektion in den Nach-Treatment-Zeitraum gleichbedeutend mit einem späteren Beginn der Arbeitslosigkeit. Während eine Verzögerung der Arbeitslosigkeit für die meisten (angehenden) Arbeitslosen sicherlich eine wünschenswerte Perspektive darstellt, ist anzunehmen, dass diese von den Betroffenen in den meisten Fällen nicht realisierbar sein dürfte. Wie bereits im vorigen Abschnitt diskutiert, zeigt sich zudem kein überproportionaler Anstieg der Zugänge Schwerbehinderter in Arbeitslosigkeit im Vorfeld des Inkrafttretens der Änderung der Beschäftigungsquote.

Weiterhin ist zu beachten, dass der Zeitpunkt des Treatments klar definiert sein muss und damit Vor- und Nach-Treatment-Zeitraum klar unterschieden werden können. Letzteres ist bei einer gesetzlichen Änderung nicht ohne weiteres möglich: In aller Regel sind gesetzliche Änderungen für die Betroffenen nicht überraschend, sondern sind den relevanten gesellschaftlichen Gruppen durch die Einbindung von Standes- und Lobbyistengruppen und durch Anhörungen in den entsprechenden Ausschüssen bereits mit einigem Vorlauf bekannt.

In diesem Fall wäre es daher möglich, dass Arbeitgeber bereits vor dem Inkrafttreten der gesetzlichen Regelungen auf die Änderungen der Beschäftigungsquote reagieren. Dies erscheint jedoch unwahrscheinlich, da durch das SchwbBAG die Beschäftigungsquote gesenkt wird. Eine Anpassung der Unternehmen in Richtung einer verstärkten Beschäftigung von Schwerbehinderten im Vorfeld des Inkrafttretens des SchwbBAG erscheint daher unwahrscheinlich. Ebenso erscheint es aus zwei Gründen unwahrscheinlich, dass Unternehmen im Vorfeld des Inkrafttretens verstärkt schwerbehinderte Arbeitnehmer entlassen. Erstens gilt die bestehende Beschäftigungsquote bis Inkrafttreten des SchwbBAG unverändert fort, so dass ein präventiver Beschäftigungsabbau zur Zahlung der Ausgleichsabgabe führen würde. Zweitens ist die Entlassung schwerbehinderter Arbeitnehmer in Deutschland nur nach

Genehmigung durch das Integrationsamt / die Hauptfürsorgestelle möglich. Diese wird in aller Regel nur bei Vorliegen gewichtiger Gründe erteilt, so dass eine Anpassung an die gesenkte Quote durch Kündigung bisher beschäftigter Schwerbehinderter nicht ohne weiteres möglich ist. Schließlich zeigt auch hier die deskriptive Analyse im vorigen Abschnitt, dass für die Schwerbehinderten weder ein Abfall noch ein Anstieg der Zu- oder Abgänge in bzw. aus der Arbeitslosigkeit festzustellen ist, der sich von den entsprechenden Trends der Nicht-Schwerbehinderten unterscheidet.

Bei Verwendung eines Difference-in-Differences-Schätzers ist die Wahl der Beobachtungszeiträume vor und nach Eintritt des betrachteten Ereignisses entscheidend. Damit der kausale Effekt des Ereigniseintritts identifiziert ist, darf in den jeweiligen Zeiträumen kein anderes Ereignis eintreten, das die Beschäftigungschancen der Schwerbehinderten und der Nicht-Schwerbehinderten in unterschiedlicher Weise beeinflusst. Die Beschäftigungsquote wurde durch das SchwbBAG zum 1. Januar 2001 geändert. Als Zeitraum vor Beginn des Treatments bietet sich daher die Zeit vom 1. Januar 2000, ab dem die BewA-Daten vollständig verfügbar sind, bis zum 31.12.2000 an. Da zum 1.7.2001 das Gros der Regelungen des SGB IX in Kraft tritt, die ihrerseits die Beschäftigungschancen von Schwerbehinderten beeinflussen könnten, kommt als zweiter Beobachtungszeitraum die Zeit zwischen dem 1.1.2001 und dem 30.6.2001 in Frage. In die Untersuchung werden hierbei alle Personen einbezogen, die in den jeweiligen Zeiträumen mindestens einen Tag arbeitslos gemeldet sind.[11]

4.7 Ergebnisse

Tabelle 4.2 zeigt eine einfache Gegenüberstellung der Abgangsraten in Beschäftigung für die Kontroll- und beide Treatmentgruppen. Hier sind zwei Dinge festzuhalten. Erstens sind die Beschäftigungsaussichten für Schwerbehinderte erwartungsgemäß deutlich schlechter als für Nicht-Schwerbehinderte. Zweitens haben sich die Beschäftigungsaussichten für alle drei Gruppen in der Nach- im Vergleich zur Vor-Treatmentperiode verschlechtert. Festzuhalten ist allerdings, dass diese Verschlechterung für die beiden Treatmentgruppen geringer ausfiel als für die Kontrollgruppe, was als ein erster Hinweis auf eine positive Wirkung der

[11] Eine frühere Version dieses Papiers verwendete hier die Zugänge in die Arbeitslosigkeit im jeweiligen Zeitraum. Ich danke einem der anonymen Referees für den Hinweis auf hiermit eventuell verbundene Probleme. Die Resultate waren jedoch qualitativ identisch.

Änderung der Beschäftigungspflichtquote gewertet werden kann.

Tabelle 4.2: Abgänge in Beschäftigung, vor/nach Treatment

Abgänge in Beschäftigung	Nicht-Schwerbehinderte	Schwerbehinderte mit GdB>50	Schwerbehinderte und Gleichgestellte
Vor Treatment	0,1456	0,0850	0,0878
(Fallzahl)	(180.345)	(5.977)	(6.412)
Nach Treatment	0,1068	0,0597	0,0606
(Fallzahl)	(120.592)	(4.258)	(4.584)
Differenz	-0,0388	-0,0253	-0,0272

Quelle: IEBS, eigene Berechnungen. Mittelwerte, Standardabweichungen in Klammern. Alle Angaben in Prozent des Bestandes im jeweiligen Zeitraum.

Betrachtet man nun die Ergebnisse des regressionskorrigierten Difference-in-Differences-Schätzers in Tabelle 4.3, lässt sich zunächst festhalten, dass keine Änderungen der Vorzeichen der Koeffizienten auftreten, wenn die Definition der Treatmentgruppe oder das verwendete Schätzverfahren variiert wird. Fast alle der Koeffizienten haben zudem die erwartete Richtung. Eine Ausnahme stellt hier das Vorzeichen des Koeffizienten für akademische Bildung dar, der auf einen negativen oder insignifikanten Einfluss auf die Wahrscheinlichkeit einer Beschäftigungsaufnahme hindeutet. Im Fall der Dummyvariable für Abitur ändern sich zudem die Signifikanzen zwischen dem linearen Wahrscheinlichkeitsmodell und der Probit-Schätzung beträchtlich.

Konzentriert man sich auf die Parameter von Interesse, erkennt man, dass die Dummyvariable, die den Schwerbehindertenstatus angibt, das erwartete negative Vorzeichen hat und zudem in jeder Schätzung auf gängigen Niveaus signifikant ist. Der Effekt liegt zudem in einer Größenordnung von rund 3 Prozentpunkten, so dass er auch als ökonomisch "groß" betrachtet werden kann. Wie auch die deskriptiven Ergebnisse aus Tabelle 4.3 nahe legen, scheinen sich die Beschäftigungschancen aller Individuen in der Nach-Treatmentperiode verschlechtert zu haben. Die entsprechenden Dummyvariablen sind in allen Schätzungen hochgradig signifikant. Betrachtet man die Größe des Effekts in den linearen Wahrscheinlichkeitsmodellen, stellt man fest, dass dieser mit einer Abnahme der Beschäftigungswahrscheinlichkeit um 3 Prozentpunkte auch in einer ökonomisch spürbaren Größenordnung liegt.

Betrachtet man schließlich den Interaktionsterm aus Schwerbehinderung und Periode, dessen Koeffizient den kausalen Effekt der Änderung der Beschäftigungsquote beschreibt, erkennt man, dass dieser auf keinem gängigen Niveau statistisch signifikant ist. Dies ent-

Tabelle 4.3: Ergebnisse der Regressionsschätzungen

	Schwerbehinderte enge Definition		Schwerbehinderte weite Definition	
Abhängige Variable: Abgang in Beschäftigung	Probit	Lineares Wkt.Modell	Probit	Lineares Wkt.Modell
Haupteffekte				
Schwerbehindert (1 = ja)	-0,1548	-0,0346	-0,1274	-0,0298
	(0,000)	(0,000)	(0,000)	(0,000)
Periodendummy	-0,1244	-0,0269	-0,1244	-0,0269
(1 = nach Änderung der Beschäftigungsquote beobachtet)	(0,000)	(0,000)	(0,000)	(0,000)
Interaktion Schwerbehinderung/Periode	-0,0059	0,0090	-0,0209	0,0067
	(0,908)	(0,213)	(0,667)	(0,338)
Kontrollvariablen				
Männlich (1 = ja)	-0,1300	-0,0237	-0,1290	-0,0236
	(0,000)	(0,000)	(0,000)	(0,000)
Alter (in Jahren)	-0,0128	-0,0032	-0,0129	-0,0032
	(0,000)	(0,000)	(0,000)	(0,000)
Dauer der Arbeitslosigkeit (in Tagen)	-0,0008	-0,0001	-0,0008	-0,0001
	(0,000)	(0,000)	(0,000)	(0,000)
Abitur (1 = ja)	-0,0243	0,0009	-0,0239	0,0010
	(0,079)	(0,775)	(0,083)	(0,757)
Keine nachschulische Ausbildung (1 = ja)	-0,3371	-0,0660	-0,3370	-0,0659
	(0,000)	(0,000)	(0,000)	(0,000)
Akademischer Abschluss (1 = ja)	-0,0187	-0,0079	-0,0178	-0,0077
	(0,376)	(0,105)	(0,398)	(0,116)
Konstante	0,1088	0,3896	0,1093	0,3897
	(0,006)	(0,000)	(0,005)	(0,000)
33 Berufsbereiche	(einbezogen)	(einbezogen)	(einbezogen)	(einbezogen)
11 Regionaldummies Wohnorte	(einbezogen)	(einbezogen)	(einbezogen)	(einbezogen)
12 Dummies Regionaltypen Arbeitsagentur	(einbezogen)	(einbezogen)	(einbezogen)	(einbezogen)
Fallzahl	220.644	220.644	221.226	221.226
Signifikanz Modell	0,000	0,000	0,000	0,000

Quelle: IEBS, eigene Berechnungen. Koeffizienten, robuste P-Values in Klammern.

spricht den Ergebnissen aus der Studie von Verick (2004) und legt nahe, dass die Änderung der Beschäftigtenquote die Beschäftigungschancen von Schwerbehinderten weder verbessert noch verschlechtert hat. Die in Tabelle 4.4 dargestellten 95%-Konfidenzintervalle zeigen zudem, dass die Insignifikanz des Effekts nicht auf einer ungenauen Schätzung mit entsprechend weiten Grenzen beruht, sondern als informativ zu betrachten ist.

Tabelle 4.4: 95%-Konfidenzintervalle für τ

Schätzung	Untere Grenze	Obere Grenze
Enge Definition, Probit	-0,1057403	0,0939933
Enge Definition, OLS	-0,0051402	0,0231055
Weite Definition, Probit	-0,1158048	0,074081
Weite Definition, OLS	-0,0069764	0,0203164

Quelle: IEBS, eigene Berechnungen.

4.8 Fazit

Ein zentrales Element der staatlichen Förderung der Beschäftigung Schwerbehinderter in Deutschland ist die Verpflichtung der Arbeitgeber, einen bestimmten Teil ihrer Stellen mit Schwerbehinderten zu besetzen. Diese Studie verwendet eine exogene Änderung dieser Quote durch das "Gesetz zur Bekämpfung der Arbeitslosigkeit Schwerbehinderter" um Rückschlüsse auf die Wirksamkeit dieser Regelung zu ziehen.

Während die aggregierten Zahlen auf Makroebene eine Reduktion der Arbeitslosigkeit Schwerbehinderter nach der Änderung der Beschäftigungsquote vermuten lassen, zeigen die Ergebnisse aus regressionskorrigierten Difference-in-Differences-Schätzern, dass die Änderung der Pflichtquote die Beschäftigungschancen von Schwerbehinderten weder verbessert noch verschlechtert hat. Dieses Ergebnis passt zu den Resultaten einer früheren Studie mit anderen Daten (Verick 2004) wie auch zu den Ergebnissen einer früheren Unternehmensbefragung in den Regionen Rheinhessen und Trier/Koblenz (Diery/Schubert/Zink 1997).

Die scheinbare Wirkungslosigkeit der Änderung der Beschäftigungsquote, die in dieser und der früheren Untersuchung von Verick (2004) festgestellt wurde, könnte darauf zurückgeführt werden, dass es sich bei der betrachteten exogenen Änderung um eine Senkung der Beschäftigungsquote handelte. Eine Verbesserung der Beschäftigungschancen Schwerbehinderter, die ja eine erhöhte Anzahl von Einstellungen voraussetzen würde, könnte sich daher ohnehin nur durch eine Veränderung der durch die Zahlung der Ausgleichsabgabe anfallenden Kosten erklären. Diese können jedoch durch die Kosten negiert werden, die für unter Umständen nötige bauliche Veränderungen im Betrieb bzw. an einzelnen Arbeitsplätzen anfallen. Eine Verschlechterung der Beschäftigungschancen arbeitsloser Schwerbehinderter, die prinzipiell durch die Nicht-Wiederbesetzung freiwerdender, bisher von Schwerbehinderten besetzten Stellen durch die Arbeitgeber möglich ist, scheint zumindest im vorliegenden Fall nicht geschehen zu sein.

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4.10 Anhang

Tabelle 4.5: Deskriptive Statistiken, gesamte Stichprobe

Variable	Fallzahl	Mittelwert	Std.-Abw.
Wechsel in Beschäftigung (1 = ja)	311.933	0,1282	0,3342
Männlich (1 = ja)	311.933	0,5530	0,4972
Alter in Jahren	311.933	38,41	12,08
Dauer der Arbeitslosigkeit (Tage)	311.933	625,58	854,49
Abitur (1 = ja)	311.933	0,1216	0,3269
Keine nachschulische Ausbildung (1 = ja)	311.933	0,2965	0,4567
Akademischer Abschluss (1 = ja)	311.933	0,0485	0,2148

Quelle: IEBS, eigene Berechnungen.

Chapter 5

Intra-firm wage inequality and firm performance – First evidence from German linked employer-employee-data

Abstract:[1] *Economic theory suggests both positive and negative relationships between intra-firm wage inequality and productivity. This paper contributes to the growing empirical literature on this subject. We combine German employer-employee-data for the years 1995-2005 with inequality measures using the whole wage distribution of a firm and rely on panel-instrumental variable estimators to control for unobserved heterogeneity and simultaneity problems. Our results indicate a relatively small impact of wage inequality on firm performance in West Germany, while there seems to be a relationship for some inequality measures in East Germany. Further analysis shows that the relationship varies strongly with industrial relations in East Germany.*

[1]The first version of this paper was published in February 2008 as *University of Lüneburg Discussion Paper in Economics No. 77.* Helpful comments at the 2008 annual meetings of the German Statistical Association, the European Association of Labour Economists and the German Economic Association (Verein für Socialpolitik) are gratefully acknowledged. The author would like to thank Joachim Wagner for helpful hints and overall support and the crew of the research data center of the Federal Employment Agency in the Institute for Employment Reserach at Nuremberg for helpful discussions and hospitality. All calculations were performed using Stata 9.2 SE (StataCorp 2005). All do-files are available from the author on request. The data used in this paper is confidential but can be accessed through the research data center of the Federal Employment Agency in the Institute of Employment Research, see http://fdz.iab.de for further information.

Keywords: Wage dispersion, labor productivity

JEL Classification: J31, M52

5.1 Introduction

Economic theory suggests several possible relationships between intra-firm wage inequality and labor productivity. On the one hand, tournament theory and related approaches argue that a high wage spread implies high gains associated with promotions for highly productive workers. On the other hand, several theories point toward a negative effect of a wage spread that becomes too large.

The first group of arguments is related to efficiency wage theory. If a firm rewards relative performance, workers are rewarded based on a comparison with their peers. Since wage increases or promotions are awarded only to the very best, workers have an incentive to work as hard as possible. High wage dispersion in that scenario implies high wage gains for the top-performers and should consequently be associated with higher productivity.

A positive relationship between wage inequality and labor productivity may also be expected if a firm chooses a performance related pay scheme that rewards absolute performance, e.g. piece-rates, and workers are heterogeneous with respect to their ability (see Lazear 1996, 2000 for a case study and theoretical discussion). Performance related pay should create an incentive for workers to work harder thus leading to a rise in average productivity. These incentives are stronger for high-ability workers who find it easier to raise their individual output levels than their low-ability counterparts. Since these differences in individual output directly map into differences in pay under a performance pay scheme, wage dispersion may be expected to rise.

While these theoretical ideas suggest a positive relationship between wage inequality and worker performance, others point at a different direction. Lazear (1989) argues that high wage gains in tournaments do not only create incentives to work harder, but might also induce workers to sabotage the work of other members of their respective comparison group. Greater pay equality within these reference groups makes this deviant behavior less worthwhile thus reducing the efficiency losses.

A similar argument relates to rent-seeking by workers (see Milgrom 1988, Milgrom and

Roberts 1990). Instead of conducting productive work, workers may choose to allocate more time to unproductive rent-seeking, e.g. trying to convince their supervisors to change the wage structure in their favor. The incentive to engage in these redistributive activities is clearly bigger if the wage spread is large. A simple remedy for a firm faced with this problem is to compress the wage structure thus reducing the potential gains from rent-seeking.

Frey (1997) argues that monetary rewards may crowd out intrinsic motivation of workers. If a firm raises wage inequality to create incentives for workers to become more productive, this crowding-out might work in the opposite direction thus leading to a smaller, if not negative effect on labor productivity.

Finally, another strand of the literature focuses on aspects of fairness or cohesion between workers. Akerlof and Yellen (1988, 1990) derive a variation of efficiency wage theory based on "fair" wages. In their model, workers do not only care about their absolute wage but base their effort on a comparison between their actual wage and a wage they consider fair. This fair wage is in turn influenced by the wages of a comparison group, e.g. similar workers in the same firm. In this model, greater wage compression may lead to higher productivity by means of a wage structure that places more workers close to their fair wage.

A similar argument is used by Levine (1991) who considers the importance of cohesion between workers in certain work environments. His arguments suggest that when cohesion is important, e.g. in firms that rely heavily on group work, a certain amount of wage compression will be beneficial as it raises overall productivity.

Empirically, the direction of the relationship between wage dispersion and labor productivity remains unanswered. A number of studies, reviewed in greater detail in section 5.2, have dealt with this question without reaching a definite consensus. Studies using evidence from professional sports usually find either a negative or insignificant relationship between wage inequality and productivity, while studies focusing on executive compensation or firm level inequality reach somewhat different conclusions.

This paper contributes to the literature by combining for the first time firm-level panel data and inequality measures built on the entire wage distribution within firms with an estimation strategy that accounts for both time-constant heterogeneity and contemporaneous endogeneity. More specifically, we use linked-employer-employee data from Germany for the years 1996 to 2005 to estimate the relationship between several measures of wage inequality and average productivity. To account for endogeneity and time-constant unobserved heterogeneity, we use standard within-estimators as well as panel-instrumental-variables techniques.

The rest of this paper is organized as follows. Section 5.2 gives an overview of previous evidence from empirical studies. Section 5.3 describes the data used in this study while section 5.4 outlines our estimation strategy along with the estimator used. Results are presented in section 5.5. Section 5.6 concludes.

5.2 Previous evidence

Much of the previous econometric evidence relating wage inequality to the economic performance of organizations comes from either professional sports (e.g. Bloom 1999, Depken 2000, DeBrock et al. 2004, Frick et al. 2003, Harder 1992), executive compensation (e.g. Eriksson 1999, Leonhard 1990, Main et al. 1993) or from other special environments (see e.g. Pfeffer and Langton 1993 for college and university faculty). The results are generally mixed between as well as within these strands of the literature. A slight majority of the "sports papers" reports a detrimental effect of wage inequality, while the papers concerned with executives are more in favor of a positive relationship between performance and wage dispersion. One should note, however, that none of these results generalizes easily to the whole population of workers in a firm and the question what effects inequality in their wages might have on a firm's performance.

In recent years a small, but growing number of papers, summarized in table 5.1, has used either firm or linked employer-employee-data to investigate the relationship between pay inequality among workers and firm performance. Cowherd and Levine (1992) use data on 102 business units from the UK and the USA. They rely on two measures of wage inequality, specifically lower management wages compared to those of hourly paid workers and lower compared to upper management wages, and relate those to a measure of product quality. Results from standard linear regressions reveal a negative relationship between these variables.

Table 5.1: Overview of previous studies

Study	Country	Data	Inequality measure	Outcome	Method	Relationship between inequality and outcome
Cowherd, Levine (1992)	USA, UK	cross-section of 102 business units	(i) wage difference between management and hourly paid workers (ii) wage difference between upper and lower management	index of product quality	OLS	negative for both measures and outcomes
Winter-Ebmer, Zweimüller (1999)	Austria	social security records aggregated at firm level, panel of 130 firms 1975-1991	variance of residuals from wage equations	standardized wages	within- and between-estimators	(i) hump-shaped for white collar workers (ii) positive, decreasing for blue-collar workers
Hibbs, Locking (2000)	Sweden	(i) industry level 1964-1993 (ii) plant level 1972-1993	between- and within coefficients of variation	(log) value added per worker	OLS	positive for within-, negative for between-inequality
Bingley, Eriksson (2001)	Denmark	LEE-data 22,665 firm-year-obs. for 1992-1995	variance of residuals from wage equation	(i) total factor productivity (ii) sickness absence	IV-estimation	(i) hump-shaped for white-collar workers and productivity (ii) none for blue-collar workers and productivity (iii) positive for both and sickness absence
Beaumont, Harris (2003)	UK	Plant level 1978-1995 panel	ratio of wages of manual to non-manual labor	gross value added per worker	Arellano-Bond	positive for most industries
Lallemand et al. (2004)	Belgium	LEE, 1995 cross-section	(i) variance of residuals from wage equation (ii) several unconditional measures	gross operating surplus	2SLS	positive, larger for blue-collar workers
Heyman (2005)	Sweden	LEE, 1991 and 1995 cross-sections	(i) variance of residuals from wage equations (ii) coefficient of variation (iii) 90/10-percentile ratio	(i) profits per employee (ii) log average wage (iii) variance of sales	first differences, IV-estimation	(i) positive for profits and average pay (ii) positive for managers and variance of sales
Jirjahn, Kraft (2007)	Germany	firm-level 1997 cross-section	wage difference between unskilled blue collar and skilled white collar worker	log value added	OLS	varies with industrial relations and pay scheme, insignificant without interactions
Grund, Westergard-Nielsen (2008)	Denmark	LEE, panel 1992-1997	coefficients of variation for (i) wages and (ii) wage increases	log value added	(i) OLS (ii) within-estimators	(i) hump-shaped for wages (ii) U-shaped for wage increases
Hunnes (2008)	Norway	LEE, 1987-1997 panel	(i) coefficient of variation (ii) residuals from wage regressions (iii) Theil index (within and between hierachical levels) (iv) standard deviation of bonus payments	gross production value per employee at market prices	(i) OLS (ii) within-estimators	no link between wage dispersion and firm performance

See text for detailed discussion.

In a study for Austria, Winter-Ebmer and Zweimüller use data from social security records. The data encompasses all workers from 130 firms between 1975 and 1991. They construct a measure of conditional wage inequality that takes into account the wage differences justified by different compositions of the workforce. More specifically, they estimate firm- and year-specific standard wage regressions and use the variance of the residuals as a measure of wage inequality. Since their data does not contain a measure of firm performance, they rely on the results from the wage regression to calculate standardized wages for typical workers as a proxy for productivity. Their results from group-mean regressions show a non monotonic relationship for white-collar-workers where productivity increases with rising wage inequality at low levels and decreases if inequality becomes too large. For blue-collar workers the same overall relationship emerges. However, here most observations are to the left of the turning point, thus implying a positive, though decreasing relationship between wage inequality and productivity.

Using aggregate data from Sweden for the years 1964-1993 for industries and 1972-1993 for plants to estimate an augmented Cobb-Douglas-function, Hibbs and Locking (2000) find a positive association between wage inequality and firm performance, measured as value added and value added per worker. They also find a negative effect of between-industry wage inequality on aggregate output and productivity growth.

In a paper for Denmark, Bingley and Eriksson (2001) use linked employer-employee data from the integrated database for labour market research (IDA) and the business statistics database (BSD). Their data contains the population of Danish firms that have at least five managers, five other white-collar workers and five blue-collar workers, as well as information on all their employees for the years 1992 to 1995, resulting in 22,665 firm-year observations. They measure productivity and worker effort on the firm level by using total factor productivity and sickness absence respectively. These are regressed on a measure of wage dispersion similar to that used by Winter-Ebmer and Zweimüller (1999), that is residuals from wage regressions, here calculated using all workers in the sample. Regarding the effects of pay inequality on productivity, their results for white-collar workers show the same non-monotonic relationship as found by Winter-Ebmer and Zweimüller (1999), while no effect can be found for blue-collar worker wage inequality. For both blue- and white-collar workers a higher pay spread is associated with reduced sickness absence and thus higher effort.

Using firm-level data from the UK, Beaumont and Harris (2003) look at the relationship between wage dispersion, measured as the ratio of manual to non-manual labor, and gross value added per worker. They estimate Cobb-Douglas-type production functions for several industries using Arellano-Bond dynamic panel estimators to account for the endogeneity of labor productivity, employment, capital stock and relative wages. Their results show positive effects of their measure of wage dispersion for most industries, more specifically electronic data processing, motor vehicles and engines, aerospace and miscellaneous foods, and a negative effect in one industry (pharmaceutics).

In another paper trying to account for the endogeneity of wage dispersion, Lallemand et al. (2004) use cross-sectional linked-employer-employee data from the Belgian 1995 Structure of Earnings Survey and the 1995 Structure of Business Survey. Their sample encompasses 17,490 individuals working for 397 firms from the private sector with at least 200 employees. They construct a conditional wage dispersion measure by using the same approach as Winter-Ebmer and Zweimüller (1999) and use it alongside several unconditional dispersion indicators in regressions on gross operating surplus per worker. As wage dispersion measures may be rendered endogenous by bonus payments, they instrument overall wage dispersion by the dispersion of wages excluding bonus payments in a 2SLS-equation. Their results show a positive and significant relationship between wage inequality and firm performance which is larger for blue-collar workers.

Also using linked employer-employee data for 1991 and 1995 from several Swedish data sources, Heyman (2005) estimates the relationship between several measures of wage dispersion among managers and white-collar employees and a variety of performance measures. He accounts for unobserved heterogeneity and the potential endogeneity of wage dispersion by estimating first differences from 1991 to 1995 and instrumenting the 1995 wage dispersion with their 1991 counterpart. His findings indicate a positive impact of both managerial and white-collar wage inequality on profits and average pay and a positive relationship between managerial wage inequality and variation in sales.

Jirjahn and Kraft (2007) use firm-level data from West Germany to look into the interaction between (blue-collar) wage inequality, measured as the difference between the highest wage for a skilled worker and the lowest wage of an unskilled worker in the same establishment, pay and promotions schemes, collective bargaining, work councils and their impact on firm performance. While they cannot control for either unobserved heterogeneity or the potential endogeneity of wage dispersion, their results indicate that the effect of wage dispersion differs greatly when taking the aforementioned interactions into account.

Grund and Westergard-Nielsen (2008) look at both the dispersion of wages and wage increases using Danish linked employer-employee data for firms with at least 20 employees for the years 1992 to 1997. Their findings indicate a hump-shaped relationship between wage dispersion and log value added that disappears when controlling for unobserved heterogeneity. The dispersion of wage growth is found to have a U-shaped influence, with most of the firms in the sample being situated on the decreasing part of that curve.

Finally, Hunnes (2008) uses linked-employer-employee data for Norwegian firms from 1986-1997 to look at the impact of both dispersion in fixed wages and bonus payments. His results suggest that although wage dispersion has increased over the observation period, there is no clear cut evidence that it had any effect on firm performance.

Taken together, the evidence suggests an either positive or hump-shaped relationship between wage inequality and firm performance. This relationship seems to be relatively robust to the exact definition of wage inequality and the performance measure used. It also seems somewhat stable over countries and time periods with the results for Germany by Jirjahn and Kraft (2007) and for Norway by Hunnes (2008) being an exception.

5.3 Data

This paper uses the linked employer-employee data of the Institute for Employment Research in Nuremberg, the so-called LIAB.[2] The LIAB is created by merging establishment information from the IAB Establishment Panel, a representative survey conducted annually since 1993, with employee information from notifications to German social security. This paper uses the cross-sectional version of the LIAB, currently available for the years 1993 to 2006, where the panel data on establishments is merged with cross-sectional information on all employees working in the respective establishment on June 30th of each year. Note that this results in an annual panel at the firm level.

[2]For a short introduction see Alda et al. (2005), detailed information (in German) is available in Jacobebbinghaus and Alda (2007), Jacobebbinghaus (2008) and on the website of the Research Data Center of the Federal Employment Agency in the Institute for Employment Research http://fdz.iab.de.

The employee data originates from social security information and is collected in the so called *employee history* by the Federal Employment Agency.[3] Employers are obliged to deliver annual information on their employees to social security by German law which is used for pension, health and unemployment insurance. The resulting data contains information on the beginning and end of employment, daily wages, a person's age and sex, as well as several variables collected for statistical purposes, e.g. education. Note at that point that the daily wages contained in the data are calculated from the sum of wages received during the period of employment the notification relates to and the duration of employment (see e.g. Jacobebbinghaus 2008, p. 41). Note further that the employers may include bonus payments in addition to the usual monthly wage into their wage notifications (see e.g. Drews 2007, pp. 25-26).

This data is combined with firm level data from the IAB Establishment Panel by a unique identification number used by the Federal Employment Agency. The IAB Establishment Panel is conducted annually since 1993 in West Germany, with East Germany joining in 1996.[4] It is representative for all German firms with at least one worker covered by social security. Case numbers vary between approximately 4.000 and 16.000 cases per year. The resulting panel is unbalanced, though response rates are usually around 80% for plants that are interviewed repeatedly (Alda et al. 2005, p. 331). The data contains detailed annual information on the employee structure, industrial relations, economic conditions, establishment size, etc., as well as special surveys varying by year.[5]

To construct the estimation sample used in this paper, we take the following steps. In a first step, we split the sample between firms in East and West Germany. First, East German workers, if born in the Socialist German Democratic Republic, might have perceptions of fairness or attitudes toward wage inequality that are fundamentally different from West German workers. Consequently we cannot expect the relationship between wage dispersion and labor productivity to be the same in these two regions. Second, and probably more important, the transformation process after the German reunification cannot be expected

[3]More information on person-level data from German social security records can be found in Bender at al. (2000).

[4]Detailed information on the IAB Establishment Panel (up to the year 2000) can be found in Kölling (2000).

[5]See Alda et al. (2005) for a short overview on the annual topics, a complete list of all items as well as complete questionaires and additional information can be found on the website of the Research Data Center of the Federal Employment Agency in the Institute for Employment Research http://fdz.iab.de.

to have ended during the time period considered in this paper which makes the regions not fully comparable. In fact, Barrel and te Velde (2000), Franz and Steiner (2000) and Klodt (2000) provide evidence that both wages and labor productivity differed greatly during the 1990s and that the process of convergence has been slower as expected.

Since any measure of wage inequality is not particularly meaningful in very small firms, we restrict both samples to firms with 50 or more workers in every survey year. Additionally, as the East German data is only available from 1996 onwards, both samples are restricted to the time period 1996 to 2006. Finally, to avoid problems with outliers, establishments with outcome measures in the top and bottom 1% are dropped, leaving us with 12,130 firm-year-observations from 4,395 firms in West Germany and ca. 6,677 firm-year-observations from 2,091 firms in the East.

Labor productivity is approximated using both (log) sales per worker (in Euro) and (log) value added per worker. Note that the regressions using the latter rely on somewhat smaller samples as information on inputs is missing in a larger number of cases. Additionally, to capture simple efficiency wage effects, we include the average daily wage as a control variable. Furthermore, we control for a full set of year dummies, firmsize (including a second order polynomial), the age structure of the workforce measured by the shares of workers below 30, between 40 and 50 and above 50, the shares of apprentices, unskilled workers, white-collar workers, other workers and workers with academic training, the share of women, investments (in €) per head and dummies for the existence of a work council and collective bargaining agreements on the plant and industry level. Unfortunately, the data does not contain information on a firm's capital stock thus preventing the use of production functions.

When measuring wage inequality, it is useful to distinguish between two different concepts: overall wage inequality without taking differences in the composition of the workforce into account and inequality in comparable subgroups, e.g. between workers with similar characteristics. Depending on the exact nature of the data-generating process, both concepts might be relevant for labor productivity.

Consider first the case of tournament theories where a greater wage inequality may be taken as a sign for larger incentives associated with high performance. In that scenario, overall wage inequality might be relevant if workers believe that each wage level in a firm can actually be reached by high performance. Wage inequality within certain groups would better reflect the fact that most often workers will compete against similar peers for a promotion.

A similar argument holds for the fairness theories. Overall wage inequality would reflect workers sentiments toward the overall wage spread in a firm, e.g. the belief that upper management should only earn a certain multiple of the average blue-collar worker wage. Wage inequality between similar workers comes closer to the intuitive notion of fairness, specifically that an allocation is more unfair if equal workers are treated unequally.

In this paper, we use variants of both concepts of wage inequality. In a first step, wages above the social security contribution threshold are imputed by means of a Tobit-based imputation procedure (Gartner 2005), separately for the East and West German samples, using standard wage regression variables as sex, age, education, occupational position, regions and industry. In a second step, the overall wage inequality is measured by the coefficient of variation using all wages from a specific firm. Within group inequality is measured by a weighted average of coefficients of variation calculated for subgroups defined by age, occupational positions and education (five categories each), where the weights are the number of people in each subgroup.

To create a third measure of inequality, we rely on an approach similar to the one used by Winter-Ebmer and Zweimüller (1999): We estimate year-specific wage equations by Tobit-regressions, again separately for East and West Germany, to account for the top-censoring of the wage variable using the same variables as in the imputation described above. Inequality is then measured by the standard deviation of residuals for all persons in a given firm.

Table 5.2 displays descriptive information on some key variables. Note that the low share of unskilled workers is not uncommon in Germany where most workers have completed vocational training. The high number of firms covered by collective bargaining agreements at the industry level and the much lower number of firms with individual agreements is also typical for industrial relations in West Germany. Similarly, the lower share of firms covered by industry level agreements and the higher number of firm level agreements in the East is also common. To sum up further differences between firms in East and West Germany: The latter are generally both larger and more productive. East

Table 5.2: Descriptive statistics, estimation sample

Variable	No of cases	Mean	Std.dev. (within)	Std.dev. (overall)
West Germany				
Sales per head (€, 2000 prices)	12,130	190,064.5	44854.5	175,622.0
Value added per head (€, 2000 prices)	10,016	78,882.4	32768.7	79,262.4
Avg. wage per head (€, 2000 prices)	12,130	83.43	3.56	19.38
Number of employees	12,130	484.35	87.17	1091.17
Share of workforce below 30 years of age	12,130	.245	.028	.111
Share of workforce between 40 and 50 years of age	12,130	.258	.018	.071
Share of workforce above 50 years of age	12,130	.175	.026	.085
Share of apprentices	12,130	.044	.012	.051
Share of unskilled workers	12,130	.044	.012	.051
Share of white-collar workers	12,130	.335	.023	.214
Share of other workers	12,130	.129	.024	.140
Share of workers with academic degree	12,130	.071	.012	.102
Share of women	12,130	.287	.037	.233
Investments per head (€, 2000 prices)	12,130	8,893.8	11,789.3	20,535.9
Collective bargaining agreement industry level	12,130	.699	.180	.459
Collective bargaining agreement firm level	12,130	.100	.150	.300
Work Council	12,130	.816	.097	.387
Coefficient of variation (overall)	12,130	.376	.030	.110
Coefficient of variation (average over subgroups)	12,130	.221	.026	.082
Residual variation	12,130	19.7	2.6	5.0
East Germany				
Sales per head (€, 2000 prices)	6,677	125,602.7	32,992.0	116,105.9
Value added per head (€, 2000 prices)	5,786	53,386.3	21,787.3	49,292.1
Avg. wage per head (€, 2000 prices)	6,677	60.87	3.36	16.57
Number of employees	6,677	238.03	51.49	305.80
Share of workforce below 30 years of age	6,677	.222	.033	.120
Share of workforce between 40 and 50 years of age	6,677	.308	.026	.081
Share of workforce above 50 years of age	6,677	.177	.030	.091
Share of apprentices	6,677	.061	.019	.078
Share of unskilled workers	6,677	.061	.019	.078
Share of white-collar workers	6,677	.286	.036	.213
Share of other workers	6,677	.106	.038	.157
Share of workers with academic degree	6,677	.113	.016	.123
Share of women	6,677	.325	.039	.265
Investments per head (€, 2000 prices)	6,677	13,130.0	17,694.2	32,082.7
Collective bargaining agreement industry level	6,677	.508	.211	.500
Collective bargaining agreement firm level	6,677	.183	.215	.387
Work Council	6,677	.711	.120	.453
Coefficient of variation (overall)	6,677	.344	.035	.120
Coefficient of variation (average over subgroups)	6,677	.187	.026	.063
Residual variation	6,677	13.6	8.1	11.7

German firms tend to have a more equalized wage structure with all inequality measures being lower than those in West Germany. At the same time, the variation of wage inequality, except for residual variation, is similar in both regions.

5.4 Econometric modeling

Consider the following estimating equation

$$y_{it} = \beta X_{it} + \tau D_{it} + \eta_i + \epsilon_{it}, \tag{5.1}$$

where y_{it} is either log sales or log value added per worker, X_{it} contains time-varying control variables, D_{it} is a matrix containing measures of wage inequality, η_i is a firm-specific fixed effect and ϵ_{it} an error term.

Equation (5.1) is used to estimate three models, each using a different measure of inequality: Models I and II use the overall or the average within-group coefficient of variation respectively, while model III uses the residual variation measure that controls for differences in workforce composition.

The firm-specific fixed effect η_i captures the fact that both wage dispersion as well as labor productivity may be influenced by time-invariant unobserved factors, e.g. firm "culture". It also captures the impact of all time invariant variables, like industry affiliation, that might also influence productivity. The presence of these fixed effects implies that identification of all parameters uses variation over time within firms.

Finally, as already noted by Lallemand et al. (2004) there might be a simultaneity problem between productivity, especially when measured as sales, and wage dispersion: Consider a case where a firm experiences an unobserved productivity shock leading to higher profits. These in turn might influence intra-firm wage inequality through bonus payments, ultimately leading to contemporaneous correlation between the measure of wage dispersion and the error term from equation (5.1). This correlation in turn prevents identification of the causal impact of wage dispersion on productivity. As our wage measure contains bonus payments (see section 5.3), this problem may be present in our data, leading to endogeneity of the wage dispersion variables even conditional on the unobserved fixed effect.

To address these three points equation (5.1) is estimated using both standard within-estimators that assume strict exogeneity conditional on the firm-specific fixed-effect η_i and panel-IV-techniques where current inequality is instrumented with lags of the inequality measures using two-step GMM.[6] In the IV-procedure, the firm-specific fixed effects η_i are eliminated by taking first-differences. Subsequently, the possible simultaneity bias is addressed by using first and second lags of the (differenced) inequality measures to instrument for current values. As this yields more instruments than endogenous regressors, the equation is overidentified and the exogeneity of instruments can be tested. For West Germany, the test results indicate both valid and relevant instruments regardless of the outcome measure used. For East Germany, the same conclusion holds for five out of the six models estimated. However, the tests indicate a weak instrument problem when regressing the residual variation measure of wage dispersion on log sales. As there is no direct remedy to this problem, this fact should be kept in mind when looking at the estimation results.

5.5 Results

Consider the estimation results shown in table 5.3 and focus first on the results for West Germany in the top two panels of the table. Here, we find a strong positive influence of a firm's average wage in all within estimations regardless of the outcome used. This influence, however, becomes insignificant in the instrumental variable estimations. One should note though, that these results are caused by less precise estimation rather than smaller points estimates.

For the inequality measures, the results differ between both the respective outcome and the inequality measure used for estimation. Using log sales per head, we obtain a U-shaped relationship with the average coefficient of variation in Model II. The signs of the coefficients are essentially reversed when using the instrumental variable estimates. A look at the respective maximum and minimum reveals that these relationships are in fact degressively rising and hump-shaped respectively. For the two other inequality measures, results are generally insignificant. Note, however, that the signs of the (insignificant) point estimates follow a similar pattern as obtained for Model II. Using log value added as the outcome yields generally insignificant results with the exception of one coefficient in Model

[6] Using Stata SE 9.2 this involves the user-written command xtivreg2 (Schaffer 2007).

Table 5.3: Productivity regressions, dependent variables: log sales/value added per head (based on 2000 prices), Within- and Panel-IV-estimators

	Model I Overall coefficient of variation		Model II Average coefficient of variation		Model III Residual variation	
Variable	Fixed-Effects	Panel-IV	Fixed-Effects	Panel-IV	Fixed-Effects	Panel-IV
West Germany, Outcome: log sales per head						
Average wage (€, 2000 prices)	0.0074***	0.0048	0.0078***	0.0008	0.0079***	0.0016
	(0.0012)	(0.0039)	(0.0011)	(0.0019)	(0.0011)	(0.0019)
Inequality measure	-0.1721	7.9270	-0.6823*	4.3865**	-0.0004	0.0006
	(0.2387)	(4.9877)	(0.2649)	(1.4814)	(0.0014)	(0.0005)
Inequality measure (squared)	0.0710	-5.6411	1.1754*	-2.4703**	0.0000	-0.0295
	(0.2688)	(3.2838)	(0.4966)	(0.8031)	(0.0000)	(0.0208)
No. of Obs.	12,130	2,605	12,130	2,605	12,130	2,605
West Germany, Outcome: log value added per head						
Average wage (€, 2000 prices)	0.0073**	0.0095	0.0078**	0.0056	0.0081***	0.0076
	(0.0024)	(0.0072)	(0.0024)	(0.0041)	(0.0023)	(0.0042)
Inequality measure	-0.5515	14.6719	-0.8088	2.8759	-0.0031	0.0004
	(0.4032)	(8.4406)	(0.4889)	(3.1229)	(0.0029)	(0.0010)
Inequality measure (squared)	0.3922	-11.5254*	1.1988	-2.3986	0.0000	-0.0295
	(0.4183)	(5.6919)	(0.9266)	(1.6767)	(0.0000)	(0.0417)
No. of Obs.	9,993	2,167	9,993	2,167	9,993	2,167
East Germany, Outcome: log sales per head						
Average wage (€, 2000 prices)	0.0132***	0.0163***	0.0124***	0.0077**	0.0123***	-0.0196
	(0.0021)	(0.0041)	(0.0020)	(0.0030)	(0.0019)	(0.0466)
Inequality measure	0.6244*	-5.5066*	0.4307	1.9564	0.0015	0.0041
	(0.2728)	(2.2167)	(0.3287)	(2.4803)	(0.0013)	(0.0066)
Inequality measure (squared)	-0.5082*	5.7310**	-0.7177	-0.9066	-0.0000	-0.1587
	(0.2561)	(1.8261)	(0.6249)	(1.2125)	(0.0000)	(0.2656)
No. of Obs.	6,677	1,795	6,677	1,795	6,677	1,795
East Germany, Outcome: log value added per head						
Average wage (€, 2000 prices)	0.0144***	0.0117	0.0142***	0.0094	0.0141***	0.0112
	(0.0036)	(0.0094)	(0.0034)	(0.0073)	(0.0032)	(0.0070)
Inequality measure	1.3292**	-14.6663*	0.7409	-12.1236	0.0015	-0.0010
	(0.5043)	(7.1405)	(0.6941)	(8.2392)	(0.0042)	(0.0029)
Inequality measure (squared)	-1.5468**	11.8111*	-1.3543	5.4897	-0.0000	0.0524
	(0.4813)	(4.8841)	(1.3626)	(3.4150)	(0.0000)	(0.0848)
No. of Obs.	5,771	1,595	5,771	1,595	5,771	1,595

Coefficients, cluster-robust standard errors in parentheses. ***/**/* denote significance on the 0.1%, 1%, and 5% level respectively. Fixed effects are eliminated by within-transformation (Fixed Effects-Model) or first differences (Panel IV). Panel IV uses first and second lags of inequality measures as instruments. All estimations include the following control variables: A full set of year dummies, firmsize (including a second order polynomial), the age structure of the workforce measured by the shares of workers below 30, between 40 and 50 and above 50, the shares of apprentices, unskilled workers, white-collar workers, other workers and workers with academic training, the share of women, investments (in €) per head and dummies for the existence of a work council and collective bargaining agreements on the plant and industry level. Detailed estimation results are available on request.

I that is significant when using instrumental variables. Again, the signs of the coefficients follow a similar pattern than those found when using log sales.

Now consider the results for East Germany. Similar to West Germany, we find a strong positive effect of the average wage. In contrast to the West German results, this effect remains significant in most of the instrumental variable estimations when using log sales per head. In comparison to West Germany, average wages seem to have larger productivity consequences with coefficients being about twice as large when using sales per head. Using value added, we find a similar pattern of coefficients for average wages to that in West Germany.

For the inequality measures, the results are somewhat different from those found in West Germany. In the East, overall inequality matters. For both outcomes, we find a hump-shaped relationship with overall inequality when using the within estimator and a U-shaped relationship when using instrumental variables. Looking at the extrema of these relationships, however, reveals that these should be interpreted as progressively falling and rising respectively. For all other inequality measures, the results are generally insignificant.

Overall, the results do not suggest a particular robust and clear cut relationship between wage dispersion and productivity. For West Germany, the results depend highly on the specific outcome and the inequality measure used. In East Germany, the results for overall wage inequality seem to be pretty robust to the choice of the outcome. However, the results for the other inequality measures are generally insignificant. Additionally, there are also no clear-cut differences between East and West Germany: While the results differ between the two regions, there is no clear pattern that suggests, e.g. a higher (or lower) importance of wage dispersion in one of the regions.

There are several possible explanations for these results: First, it might be the case that the within variation in the data is too low to allow for reliable estimation. While this seems possible, there is no easy remedy as between estimates could easily be contaminated by unobserved heterogeneity. Second, it might be the case that the positive and negative effects of wage inequality offset each other. For instance, while workers might like the perspective of receiving a large raise, they might also think about possible adverse effects for, e.g., the working climate caused by jealousy. If these effects offset each other, a results like the one obtained in this paper seems possible. Finally, it might be that the true impact of wage dispersion is small. Workers might simply not respond to monetary incentives with all their motivation being intrinsic. Additionally, it seems possible that the impact of wage dispersion depends on other factors like the nature of industrial relations or "firm culture". In fact, Jirjahn and Kraft (2007) found that the impact of wage inequality changes with different pay schemes, coverage by collective bargaining agreements and the existence of a work council.

To explore this possibility further, we consider possible differences in the inequality-productivity relationship over industrial relations regimes. In the following, we focus on four groups of firms: (i) plants covered by a collective bargaining agreement with a work council, (ii) plants that have not been covered by a collective bargaining agreement and also did not have a work council during the period of observation, (iii) plants covered by a collective bargaining agreement without a work council and finally (iv) plants with a work council, but not covered by collective bargaining agreements. As case numbers are too low for an instrumental-variable approach, only the fixed effects estimators were used on these subsamples. The included control variables were identical to those used in the base specifications. Results using log sales per head as the outcome are reported in table 5.4. The pattern of results using value added is practically identical.

Starting again with the results for West Germany, we find some differences over industrial relations regimes. Overall inequality seems to matter only in the absence of both a work council and collective bargaining agreements. Here we find a U-shaped relationship that turns out to be progressively rising if we also consider its minimum. Under all other industrial relation regimes there is no significant connection between wage dispersion and firm performance. While being far from an exact proof, this result may be taken as a suggestive sign that workers accept inequality as long as there is some institution that acts or bargains in their favor.

The results are again different for East Germany. Here, we find hump-shaped relationships between the average within group inequality and firm performance for all firms that do not have both a work council and are covered by a collective bargaining agreement. Looking at the extrema of these relationships, we find that wage dispersion has a progressively falling relationship with firm performance. Similarly, in firms with a work council, but without coverage by collective agreements, we find the same progressively

Table 5.4: Productivity regressions by industrial relations regime, dependent variable: log sales per head, Within-estimators

	West Germany			East Germany		
Variable	Model I	Model II	Model III	Model I	Model II	Model III
No work council, not covered by collective bargaining						
Average wage (€, 2000 prices)	0.0019	0.0036	0.0032	0.0092	0.0121*	0.0120*
	(0.0039)	(0.0038)	(0.0037)	(0.0061)	(0.0061)	(0.0060)
Inequality measure	-1.1428*	-1.4973	-0.0040	-0.5142	-1.6895*	0.0321
	(0.5735)	(1.0284)	(0.0149)	(0.6774)	(0.7508)	(0.0236)
Inequality measure (squared)	0.7683*	2.1298	-0.0001	-0.0158	2.5441	-0.0012
	(0.3683)	(1.6477)	(0.0003)	(0.7076)	(1.3544)	(0.0007)
No. of Obs.	857	857	857	821	821	821
Sig.(Model)	0.0000	0.0021	0.0002	0.0000	0.0000	0.0000
Work council, covered by collective bargaining						
Average wage (€, 2000 prices)	0.0072***	0.0076***	0.0079***	0.0134***	0.0122***	0.0122***
	(0.0016)	(0.0015)	(0.0015)	(0.0032)	(0.0030)	(0.0027)
Inequality measure	0.3365	-0.4607	0.0006	-0.2205	0.5131	0.0035
	(0.4097)	(0.3997)	(0.0018)	(0.5368)	(0.5415)	(0.0042)
Inequality measure (squared)	-0.6406	0.7788	-0.0000	0.6417	-1.0303	-0.0000
	(0.5096)	(0.8279)	(0.0000)	(0.5605)	(1.0444)	(0.0000)
No. of Obs.	7,862	7,862	7,862	3,159	3,159	3,159
Sig.(Model)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
No work council, covered by collective bargaining						
Average wage (€, 2000 prices)	0.0011	0.0025	0.0026	0.0077	0.0074	0.0058
	(0.0046)	(0.0043)	(0.0045)	(0.0056)	(0.0057)	(0.0056)
Inequality measure	-1.4985	-0.2906	0.0379	1.3746	2.7495***	0.0042
	(0.9396)	(0.7368)	(0.0290)	(0.7109)	(0.7742)	(0.0223)
Inequality measure (squared)	1.3165	0.2114	-0.0008	-1.0181	-4.0034**	0.0001
	(0.9541)	(0.9921)	(0.0007)	(0.6438)	(1.3016)	(0.0007)
No. of Obs.	701	701	701	490	490	490
Sig.(Model)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Work council, not covered by collective bargaining						
Average wage (€, 2000 prices)	0.0065	0.0071	0.0059	0.0097	0.0082	0.0064
	(0.0037)	(0.0038)	(0.0039)	(0.0050)	(0.0048)	(0.0047)
Inequality measure	0.6323	1.8920	-0.0211	1.9143**	2.1437*	0.0007
	(0.8658)	(1.4518)	(0.0183)	(0.6328)	(0.9708)	(0.0095)
Inequality measure (squared)	-0.7507	-3.1222	0.0003	-1.5843***	-3.7490*	-0.0000
	(1.0052)	(2.5494)	(0.0004)	(0.4735)	(1.7818)	(0.0000)
No. of Obs.	729	729	729	459	459	459
Sig.(Model)	0.0001	0.0001	0.0000	0.0000	0.0000	0.0000

Selected coefficients, cluster-robust standard errors in parentheses. ***/**/* denote significance on the 0.1%, 1% and 5% level respectively. Control variables are those used in the main estimations. Full estimation results are available on request. The pattern of results using log value added per head is practically identical.

falling relationship also with overall wage inequality.

Taken together, the results reveal some differences between West and East Germany. While wage dispersion does not seem to matter much in West Germany, the relationship in East Germany depends on the nature of industrial relations. Here, wage dispersion does not seem to matter when there is both a work council and a collective bargaining agreement, while a negative relationship emerges when neither or only one of those institutions is present. Again, this may be taken as a sign that workers are willing to accept inequality only if there is some moderating institution acting in their favor.

5.6 Conclusion

While the relationship between wage inequality and firm performance has received some attention in theoretical economics, empirical relationships are still far from clear. In particular, there has been few research combining inequality measures related to the whole wage distribution in a firm with firm level panel data outside of special environments like professional sports. This paper contributes to this literature by using linked employer-employee-data from Germany. We construct several firm level inequality measures using social security data and regress these on sales and value added per head as a measure for productivity. Our estimation approach using panel (instrumental variable) estimators allows us to address issues like unobserved heterogeneity and possible simultaneity of wage inequality and performance.

Our results do not suggest a particular robust and clear cut relationship between wage dispersion and productivity. Results for West Germany depend highly on the specific outcome, the estimator and the inequality measure used. In East Germany, the results for overall wage inequality, suggesting either a progressively falling or rising relationship, seem to be pretty robust to the choice of the outcome. However, the direction of this relationship is far from clear and the results for the other inequality measures are generally insignificant. Finally, there are also no clear-cut differences between East and West Germany: While the results differ between the two regions, there is no clear pattern that suggests, e.g. a higher (or lower) importance of wage dispersion in one of the regions.

Looking at the importance of wage dispersion under several industrial relation regimes reveals some interesting differences between East and West Germany. While wage dispersion does generally not matter much in West Germany, we find some strong hints for a progressively falling relationship in East Germany as long as there are not both a work council and a collective bargaining agreement in the respective firm. These results seem to suggest that East German workers are only willing to accept wage inequality without altering their productivity if there are moderating institutions acting in their favor.

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Chapter 6

Final remarks

This thesis contributes to the empirical analysis of labor market inequality. The first two chapters deal with the question whether the employment prospects of Arab or Muslim men have been changed by the recent Islamistic terrorist attacks on the World Trade Center and the Pentagon on September 11th, 2001, on commuter trains in Madrid on March 11th, 2004 and on several underground trains and a bus in London on July 7th, 2005. At the time of writing the first version, chapter 2 was only the second paper to deal with this question for European labor markets. The results suggest that the job prospects of individuals from predominantly Muslim countries have not been harmed by the attacks. This finding is similar to earlier evidence for Sweden (Åslund and Rooth 2005), but contradicts earlier (and later) findings for the US (Dávila and Mora 2005, Kaushal, Kaestner and Reimers 2007).

This contradiction motivated chapter 3 which deals with the question whether these different findings may be related to the direct involvement of the US as the target of the 9/11-attacks. Results from the UK, which was also hit by large scale terrorist attacks in 2005, again showing no apparent decline in job prospects for Muslims, individuals with a Pakistani/Bangladeshi ethnicity and individuals born in a predominantly Muslim country, indicate that a country's direct involvement alone is not sufficient to explain the different reactions in the US and Europe. What remains an open question for future research is the reason for these differences. Additionally, on a more general level, it might also be interesting to look at the labor market impact of other extreme events that are closely associated with certain socio-demographic groups.

Chapter 4 contributes to the analysis of an aspect of German legislation targeted at reducing labor market inequalities between disabled and the non-disabled workers – a mandatory employment quota obligating employers to employ a certain share of disabled workers. Using a legislative change that reduced the quota and at the same time raised the levy that has to be paid in the case of non-fulfillment of the quota as a natural experiment, results from difference-in-differences-estimators show no sign for either an enhancement or a worsening of the employment prospects of the severely disabled. These results are consistent with earlier results on the individual (Verick 2004) or firm level (Kölling, Schnabel and Wagner 2001, Wagner, Schnabel and Kölling 2001a,b and Koller, Schnabel and Wagner 2006).

Finally, chapter 5 contributes to an aspect of the literature on wage inequality and wage compression. More specifically, this chapter analyses the relationship between intra-firm wage inequality and labor productivity using linked-employer-employee data. The results from various panel data estimators show no apparent relationship between these variables. Again, these results are largely consistent with the rather heterogeneous literature on this subject.

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