

**September 11<sup>th</sup> and the Earnings of Muslims in Germany –  
The Moderating Role of Education and Firm Size**

Thomas Cornelissen\* and Uwe Jirjahn\*\*

*\* Department of Economics, University College London, UK*

*\*\* Department of Economics, University of Trier, Germany*

**Abstract:** While available evidence suggests that the events of September 11<sup>th</sup> negatively influenced the relative earnings of employees with Arab background in the US, it is not clear that they had similar effects in other countries. Our study for Germany provides evidence that the events also affected the relative earnings of Muslims outside the US. However, the results show that there was no uniform effect on all types of Muslims across all types of firms. Accounting for moderating factors, a significantly negative effect can only be found for low-skilled Muslims employed in small- and medium-sized firms. This conforms to theoretical expectations. Moreover, we demonstrate that defining appropriate treatment and control groups is crucial for identifying the effects.

**JEL Classification:** J71, J31, J23.

**Keywords:** Muslims, September 11<sup>th</sup>, Wage Discrimination, Education, Firm Size.

**Corresponding Author:** Uwe Jirjahn, Universität Trier, Fachbereich IV, Lehrstuhl für Arbeitsmarktökonomik, Universitätsring 15, 54286 Trier, Email: jirjahn@uni-trier.de.

## **1. Introduction**

The September 11<sup>th</sup>, 2001 terrorists' attacks on the World Trade Center and Pentagon had dramatic consequences on Muslims in the United States. A large number of Muslims became victims of hate crime and were subjected to religious and ethnic profiling (Human Rights Watch 2002). Complaints of employment discrimination against Muslims increased fourfold between September 2001 and October 2002 (American-Arab Anti-Discrimination Committee 2003). Charges of religious discrimination against Muslims filed with the US Equal Employment Opportunity Commission more than doubled over the pre- and post-September 11<sup>th</sup> 15-month intervals. Using nationality as a proxy for religion, three econometric examinations confirm that the increased hostility toward Muslims indeed worsened their labor market outcomes.<sup>1</sup> Davila and Mora (2005) show that Middle Eastern Arab men and Afghan, Iranian, and Pakistan men experienced a significant earnings decline relative to US-born non-Hispanic whites between 2000 and 2002. Kaushal et al. (2007) find that September 11<sup>th</sup> did not affect employment and working hours of Arab men, but was associated with a 9 to 11 percent decline in their relative earnings. Rabby and Rodgers III (2009) show that the terrorists' attacks were not only associated with a relative decrease in the earnings of immigrants from Muslim-majority countries but also with a relative decrease in their employment and hours worked.

Hostility toward Muslims also increased in Europe. Following the terrorists' attacks in the United States, the European Monitoring Centre on Racism and Xenophobia (EUMC) implemented a reporting system on anti-Islamic reactions in the 15 EU Member States. The summary report (EUMC 2002, p. 5) concludes that 'Islamic communities and

other vulnerable groups have become targets of increased hostility since September 11<sup>th</sup>. A greater sense of fear among the general population has exacerbated already existing prejudices and fuelled acts of aggression and harassment in many European Member States.’ However, the few available econometric studies do not indicate that September 11<sup>th</sup> worsened the labor market outcomes of Muslims in Europe. Focusing on the probability of leaving unemployment for employment in Sweden, Aslund and Rooth (2005) find no evidence of increased discrimination toward different ethnic minorities after September 11<sup>th</sup>. Using data from the Federal Employment Agency, Braakmann (2009) obtains a similar result for Germany. His estimates do not indicate that September 11<sup>th</sup> was associated with a decline in the job prospects of foreigners with Arab background. Finally, Braakmann (2007) finds no evidence that the terrorists’ attacks had an influence on the wages and employment probabilities of Arab men in Britain.

These findings raise the question of why September 11<sup>th</sup> seems to have had no effect on the labor market outcomes of Muslims in Europe. One might argue that European employers pursue rational wage and hiring policies that do not respond to changes in attitudes (Aslund and Rooth 2005). Yet, this explanation raises the further question of why European employers should be more rational than employers in the US. Of course, given the available evidence, it may appear reasonable to assume that changes in attitudes toward Muslims, on average, were less severe in Europe than in the US (Braakmann 2009). Yet, this does not necessarily imply that the effects on the labor market outcomes of Muslims were always negligible. It rather suggests that the issue of heterogeneity is important. Attitudes toward Muslims are very likely to vary according to circumstances and types of employees. Hence, it is crucial to differentiate between

Muslims at a high risk of discrimination and those at a low risk of discrimination.

Another explanation for the seemingly absent effect of September 11<sup>th</sup> might be that European countries usually have highly regulated labor markets (Braakmann 2007). These regulations might have prevented increased discrimination against Muslims. Yet, even within a country employers differ in the degree of regulation they face. Negative effects on the labor market prospects of Muslims may be limited to firms facing less regulation. Hence, it is also important to differentiate between employers subject to a high degree and employers subject to a low degree of labor market regulation.

Against this background, it is not very likely that September 11<sup>th</sup> had a uniform effect on all types of Muslims across all types of employers. Nonetheless identifying the impact of September 11<sup>th</sup> does not only require accounting for heterogeneous effects. Moreover, it requires defining appropriate treatment and comparison groups within a difference-in-differences approach. Due to data limitations, previous studies often used information on nationality or country of birth.<sup>2</sup> The treatment group consists of immigrants from Islamic countries while the comparison group consists of native workers or immigrants from non-Islamic countries. However, coming from an Islamic country does not necessarily imply that the individual worker is a Muslim. Likewise, coming from a non-Islamic country does not necessarily imply that the individual worker is no Muslim. Hence, the treatment group may include non-Muslim workers who are not subject to increased discrimination while the comparison group may include Muslims who are subject to increased discrimination. These two types of measurement errors induce an attenuation bias, which can potentially hide the effects of September 11<sup>th</sup>. This may specifically hold true for countries where changes in attitudes toward Muslims, on

average, were less strong than in the US.

Altogether, specifically in a European context, the effects of September 11<sup>th</sup> may remain obscured until two issues have been addressed. First, using workers' nationality to define treatment and control group may involve an attenuation bias. Second, the effects of September 11<sup>th</sup> may differ between different types of firms and different types of employees. Our study addresses both issues. Based on employee data of the German Socio-Economic Panel (GSOEP), we examine the influence of the terrorists' attacks on the earnings of Muslim men in West Germany. The GSOEP is a unique data set that does not only provide information on the workers' nationality but also on their religion. If the definition of treatment and control group is based on workers' nationality, the estimates show no statistically significant effect of September 11<sup>th</sup>. This finding fits the results of the other European studies and reflects the attenuation bias. In contrast, if the definition of treatment and control group is based on workers' religion, the estimates show a significantly negative relationship between September 11<sup>th</sup> and the relative earnings of Muslims. Most importantly, this specifically holds true for low-skilled Muslims but not for skilled Muslims. Moreover, splitting by establishment size, the negative effect can only be found for low-skilled Muslims employed in small- and medium-sized firms.

The pattern of results conforms to our theoretical expectations. First, negative attitudes toward foreigners and Muslims are much more prevalent among low-skilled Germans. To the extent low-skilled Muslims have low-skilled German superiors and coworkers, they face a higher risk of discrimination. Second, smaller firms are typically subject to less labor market regulation. Very often smaller firms are not covered by collective bargaining. Moreover, they are less likely to have a works council and to

pursue standardized pay policies. Hence, there are more opportunities for discrimination in smaller firms.

The rest of the paper is organized as follows. Section 2 provides the background discussion. Section 3 describes the data, variables and estimation method. Section 4 presents the results. Robustness checks are discussed in Section 5. Section 6 concludes.

## **2. Background Discussion**

### *2.1 Attitudes toward Foreigners and Muslims in Germany*

Several pieces of evidence suggest that there exist serious xenophobic tendencies in the German society. Using data from the 1988 Euro-Barometer Survey, Gang and Rivera-Batiz (1994) show that nearly 50 percent of the West German respondents feel that there are too many foreigners in their country. This is far above the European mean of about 30 percent.<sup>3</sup> Based on data from the 1995 International Social Programme, Bauer et al. (2000) find that more than 75 percent of the German respondents think that immigration to Germany should be limited. Among the twelve EU countries examined, Germany has hereby the highest percentage of persons with a negative attitude toward immigration.

The level of ethnic crimes in Germany is also much higher than in most other EU countries. Krueger and Pischke (1997) show that the German unification has been accompanied by a high rate of violence against foreigners. The peaks of the events were widely covered by the media. There were several large-scale riots against asylum seekers in East Germany. In the West German city of Moelln, three Turkish residents died in 1992 in fire bombings of two residences. Five persons were killed in 1993 by a fire bombing of a Turkish home in the West German city of Solingen.

In light of this evidence, it appears to be very likely that the events of September

11<sup>th</sup> have triggered or reinforced negative attitudes toward Muslims. The EUMC (2006, pp. 69-70) reports several incidences of violence against Muslims and attacks on mosques in the years after 2001. A representative opinion survey conducted by Leibold et al. (2006) in 2005 shows that 50 percent of the respondents prefer to live in a neighborhood without Muslims. More than 30 percent say that Muslims make them feel like strangers in their own country. Nearly 75 think that the Islam is not compatible with the modern Western culture. About 25 percent express the opinion that the government should refuse the immigration of Muslims to Germany.

## *2.2 Discrimination*

Insofar as the terrorists' attacks on September 11<sup>th</sup> have triggered or reinforced negative attitudes toward Muslims, Becker's (1957) theory of preference-based discrimination provides a useful starting point for our discussion.<sup>4</sup> If members of the majority group are prejudiced against the minority group, they prefer not to interact with members of the minority group. Hence, as a consequence of September 11<sup>th</sup>, employers with a distaste for Muslims may have hired fewer or fired more Muslims. Even unprejudiced employers may have avoided hiring Muslims if they feared negative reactions by prejudiced customers or by their German employees in the aftermath of September 11<sup>th</sup>. Altogether, greater prejudice toward Muslims may have resulted in a worsening of their employment opportunities which would have lowered their wages.<sup>5</sup>

However, labor market discrimination can occur in a variety of ways, not only via discriminatory hiring and firing decisions but also via discriminatory treatment of minority group employees within the firm. Superiors usually have a high degree of scope and discretion over task assignment and performance evaluation (Laffont 1990,

Prendergast 1995, Prendergast and Topel 1996). This opens the door to favoritism and discrimination based on the superiors' prejudices and personal preferences toward subordinates. Indeed, Elvira and Town (2001) find a race bias in performance evaluations. Even after controlling for productivity and demographic characteristics, white supervisors of both white and black subordinates typically give whites better ratings than blacks. In contrast, black superiors typically rate white subordinates lower than black subordinates. To the extent performance appraisals form the basis of pay decisions, workers with the same productivity would have different earnings by race. If supervisory jobs are dominated by whites, this leads to an average disadvantage of blacks. A similar argument may hold for German and Muslim workers. In the aftermath of September 11<sup>th</sup>, prejudiced German superiors may have used their discretionary power to discriminate against Muslim subordinates.

Moreover, production within firms is typically characterized by interdependent worker productivity (Alchian and Demsetz 1972). An individual worker's productivity does not only depend on his own effort but also on the effort of his coworkers. Mutual learning and helping on the job provide examples of production interdependencies (Drago and Garvey 1998). If monitoring problems make it difficult for the employer to perfectly enforce cooperation among employees, the individual worker has some scope and discretion in providing help to his colleagues. The decision to help a colleague may partially depend on the worker's attitude toward this colleague. This opens the door to coworker discrimination. If prejudiced majority group workers refuse to provide help and expertise to minority group colleagues, they harm the productivity of those colleagues. This in turn may result in lower wages for minority group workers. In light of this



reasoning, September 11<sup>th</sup> may have had an effect on the wages of Muslims by triggering or reinforcing coworker discrimination within firms (Kaushal et al. 2007).

Considering discriminatory power of majority group members within firms has interesting implications. In Becker's analysis prejudiced hiring and firing decisions play a crucial role. Firms where prejudice is prevalent tend to avoid employing minority group workers. These workers are sorted into unprejudiced firms that take advantage of their unfavorable labor market opportunities by paying them less than equally productive majority group workers. However, taking discretionary power of majority group members within firms into account, wage discrimination can occur even when there are no discriminatory hiring and firing decisions. Also firms where prejudice is prevalent may employ minority group workers to a certain degree. Majority and minority group workers may be no perfect substitutes. Moreover, dismissal laws, equal employment opportunity laws or mobility frictions may prevent a perfect segregation. In firms where prejudice is prevalent majority group superiors or majority group coworkers can use their discretionary power to discriminate against minority group employees. To summarize, to the extent September 11<sup>th</sup> has triggered or reinforced negative attitudes toward Muslims, it should potentially have had an effect on wage discrimination against Muslims – with or without an increase in discriminatory hiring or firing decisions.

### *2.3 The Moderating Role of Education*

While the terrorists' attacks are very likely to have reinforced negative attitudes toward Muslims, this effect may have not been uniform across the entire German population. The European study by Bauer et al. (2000) shows that xenophobic attitudes are much more prevalent among low-educated persons.<sup>6</sup> Gang and Rivera-Batiz (1994) confirm for

Germany that low-educated individuals are more likely to have negative attitudes toward foreigners. Similarly, Fertig and Schmidt (2002) find that education plays a key role in the perception of foreigners in Germany.

Against this background, we hypothesize that September 11<sup>th</sup> had a stronger effect on the attitudes of low-educated Germans than on the attitudes of educated Germans. Hence, to the extent low-skilled Muslims have low-skilled German superiors and coworkers, they face a higher risk of increased discrimination than skilled Muslims. The effects of September 11<sup>th</sup> may remain obscured until the moderating role of education has been taken into account. September 11<sup>th</sup> may have had a negative impact on the wages of low-skilled Muslims but a less severe or even negligible effect on the wages of skilled Muslims.

#### *2.4 The Moderating Role of Firm Size*

Firm size is also very likely to play a moderating role. Larger firms are typically subject to more labor market regulation reducing the opportunities for discrimination. Hubler and Jirjahn (2003) show that larger firms in Germany are much more likely to be covered by collective bargaining. Collective agreements are usually negotiated between unions and employers' associations on a broad industrial level. They regulate wage rates and general aspects of the employment contract such as working hours. Collective agreements define minimum standards implying that covered firms exhibit downward wage rigidity (Kahn 2000). While the role unions play in the discrimination of minority group members may depend on the structure of collective bargaining (Rubery and Fagan 1995), the characteristics of the industrial relations system in Germany imply reduced wage discrimination in covered establishments.<sup>7</sup> Unions in Germany are large industrial

unions. In contrast to craft unions or narrow industrial unions, they represent a highly heterogeneous workforce. Creating cohesion across different groups of workers is crucial for strengthening the bargaining position of each of the industrial unions. Discrimination is likely to dilute cohesion and, thus, to weaken a union's bargaining power.

Larger firms are also much more likely to have a works council (Hubler and Jirjahn 2003). Works councils provide a highly developed mechanism for firm-level codetermination. They are an institution designed to foster communication between employees and management and to build cooperative and trustful industrial relations within the firm (Freeman and Lazear 1995, Smith 1991). Similar to unions, works councils may foster notions of solidarity and fairness within the workforce to obtain the support necessary for a successful representation of employee interests.<sup>8</sup> Moreover, works councils play an important role in the method of pay. Heywood and Jirjahn (2002) show that the presence of a council is positively associated with the firm's use of piece rates. Piece rates are based on a relatively objective measurement of worker performance as they reward the quantity of produced output.<sup>9</sup> Units of output can easily be verified and, hence, are associated with less discretion in performance measurement than subjective evaluations by superiors. Thus, piece rates present less scope for favoritism and discrimination. This hypothesis finds confirmation in empirical studies showing that racial and gender wage discrimination is reduced when workers are paid piece rates (Fang and Heywood 2006, Heywood and O'Halloran 2005, Jirjahn and Stephan 2004).

However, even controlling for works council presence and collective bargaining coverage, Heywood and Jirjahn (2002) find that larger firms are more likely to use piece rates. This finding supports the notion that piece-rate schemes become more likely when

fixed costs of such schemes are spread over more workers (Brown and Medoff 1989). Most importantly, the finding indicates a direct role of firm size. Firm size may involve reduced wage discrimination not only because larger firms are subject to more labor market regulation. It may also involve reduced discrimination because larger firms are more likely to bear the fixed costs of standardized pay policies that reduce superiors' discretion in performance appraisal.

Altogether, we hypothesize that September 11<sup>th</sup> should have had a stronger negative impact on Muslims working in smaller firms. First, smaller firms are less likely to be covered by collective bargaining and are less likely to have a works council. Second, smaller firms are less likely to pursue standardized pay policies based on a more objective measurement of worker performance.

### **3. Data, Variables and Method**

#### *3.1 Data Set*

Our empirical study uses data from the GSOEP (Wagner et al. 1993, 2007). The GSOEP is a representative longitudinal study of private households in Germany. It started in 1984 with the collection of data in the former West Germany. Based on face-to-face interviews, a nucleus of socio-economic and demographic questions is asked annually. Different 'special' topics are sampled in specific waves. Most importantly, the GSOEP is unique in that it does not only provide information on the workers' nationality but also on their religion. In our main regressions, we focus on foreign Muslim and non-Muslim men employed in the private sector. Following Kaushal et al.'s (2007) study for the US, we consider the years 1998 to 2004. As the GSOEP interviews were conducted before the month of September, observations from 2001 enter the analysis as pre-treatment

observations. The analysis is restricted to West Germany as the numbers of foreigners and Muslims we observe in East Germany are too small to allow an analysis by sub-groups.<sup>10</sup>

### 3.2 *Difference-in-differences Approach*

We focus on wages to examine the effect of the terrorists' attacks on the labor market outcomes of Muslims. As discussed, examining the influence on wages is particularly interesting as wage discrimination within firms can occur with or without discriminatory hiring and firing decisions. Prejudiced German superiors can use their discriminatory power in performance evaluations and task assignments to discriminate against Muslims. Moreover, prejudiced German coworkers can refuse providing help and cooperation to their Muslim colleagues.

To identify the effect of September 11<sup>th</sup> on the earnings of Muslims, we apply a difference-in-differences approach (Blundell and MaCurdy 1999, Meyer 1995). If there were unobserved factors in 2001 generally influencing employees' earnings in the subsequent years, the effect of September 11<sup>th</sup> can be disentangled from those confounding influences by comparing changes in the earnings of Muslims with changes in the earnings of an appropriately chosen comparison group. The regression formulation of the difference-in-differences approach is

$$\ln y_{it} = \alpha_0 + \alpha_1 Sept + \alpha_2 Treat_i + \alpha_3 Sept * Treat_i + \beta' x_{it} + \sum_{t=1999}^{2003} \delta_t + u_{it}, \quad (1)$$

where  $y_{it}$  is the log real hourly wage of employee  $i$  in the year  $t$ . We construct the real hourly wage rate by dividing the monthly gross wage by monthly work hours and using the consumer price index of each respective year as a deflator.<sup>11</sup> Time-specific influences

are captured by including a set of year dummies  $\delta_t$  ( $t = 1999, \dots, 2003$ ). The reference years are 1998 for the pre-treatment period and 2004 for the post-treatment period.<sup>12</sup> The vector of control variables  $x_{it}$  includes linear and quadratic terms of years of job tenure, years of part-time and full-time work experience and years of schooling. It also includes a variable measuring the years of unemployment experience as well as dummy variables for fulltime work and living with a partner or spouse. Furthermore, dummies are included to account for three broad skill groups, four categories of firm size, 60 industries and 10 federal states. The three skill groups we distinguish are unskilled blue-collar workers (without formal qualification), skilled blue-collar workers (with formal qualification), and white-collar workers. The firm size dummies define categories of firms with less than 20 employees, with 20 to 199 employees, with 200 to 1999 employees, and with 2000 and more employees.

The dummy variable *Sept* is equal to 1 if the observation is taken from the post-September 2001 period, and zero otherwise. *Treat<sub>i</sub>* is a dummy variable equal to 1 if employee *i* belongs to the treatment group, and zero if the employee belongs to the comparison group. Most importantly, the coefficient  $\alpha_3$  on the interaction variable *Sept \* Treat<sub>i</sub>* measures the difference-in-differences effect of September 11<sup>th</sup> on the earnings of Muslims. In order to compare our examination with previous studies, we assume in a first step that the effect of September 11<sup>th</sup> is homogeneous across all types of Muslim employees and all types of firms. However, our theoretical considerations imply that the effects of the terrorists' attacks are very likely to be heterogeneous. The effect should be stronger for low-skilled Muslims than for skilled Muslims. Hence, in a second step, we differentiate between skilled employees (defined as those in the two highest skill

groups described above) and low-skilled employees (defined as those in the lowest skill group). Furthermore, our theoretical considerations suggest that Muslims in smaller establishments face a higher risk of wage discrimination. Hence, in a third step, we additionally perform separate estimates by firm size.

### *3.3 Treatment and Comparison Groups*

We use two different concepts to define treatment and comparison group. First, we rely on workers' nationality to relate our analysis to previous studies. The treatment group consists of foreign employees from Islamic countries. We consider a country as an Islamic country if Muslims are the largest religious group. For example, employees with Arabian, Iranian or Turkish nationality belong to the treatment group. The comparison group consists of foreign employees from non-Islamic countries. *Table 1* provides a detailed description of the nationalities considered in the analysis.<sup>13</sup> Using nationality to define treatment and control group might make sense if employees from Islamic countries share specific visible characteristics and if their German colleagues and superiors generally discriminate against foreigners perceived as Muslims and not only against foreigners who are in fact Muslims.

However, to the extent September 11<sup>th</sup> has primarily triggered negative attitudes toward foreigners who are really Muslims the definition implies an attenuation bias. First, coming from an Islamic country does not necessarily imply that the individual worker is a Muslim.<sup>14</sup> Hence, the treatment group may include non-Muslim workers who are not subject to increased discrimination. Second, coming from a non-Islamic country does not necessarily imply that the individual worker is no Muslim.<sup>15</sup> Thus, the comparison group may include Muslims who are subject to increased discrimination. The attenuation bias

resulting from such measurement errors can potentially obscure the effects of September 11<sup>th</sup>. This may specifically hold true for European countries where changes in attitudes toward Muslims, on average, were less strong than in the US.

Importantly, in the GSOEP, employees are asked about their religion. In 2003 respondents were asked whether they belong to a church or religious community.<sup>16</sup> For religious communities other than Christian, respondents were asked to write down their religion. We identify Muslims as those respondents who answered ‘Islam’, ‘Muslim’ or ‘Muhammadan’. This unique information allows us using workers’ religion as a second concept to define treatment and comparison group. Considering religious affiliation as a time-invariant characteristic, the treatment group consists of foreign employees who are Muslims. The comparison group consists of foreign employees who are no Muslims. Specifically, in case of wage discrimination within firms, it can be argued that this concept is more likely to identify the effects of September 11<sup>th</sup>. More or less long-term employment relationships and communication among employees imply that employees know each other. Hence, German coworkers and superiors can differentiate between foreign colleagues who are Muslims and those who are no Muslims. Therefore, within firms, September 11<sup>th</sup> is more likely to have triggered negative attitudes only toward Muslims rather than generally toward foreigners who might share specific visible characteristics with Muslims.

We also consider two mixed concepts to examine in more detail the attenuation bias implied by a definition based on nationality. As discussed, this definition can involve two types of measurement errors. First, the comparison group may include workers who are Muslims. Second, the treatment group may include workers who are no Muslims.



First, we examine what happens if the first source of attenuation bias is removed. We define a comparison group consisting only of employees from non-Islamic countries who are no Muslims. The treatment group contains Muslim employees from non-Islamic countries and all employees from Islamic countries regardless of their religion. Using this mixed definition, only the second type of measurement error may be still at work. In order to remove the second source of attenuation bias we consider an alternative mixed definition where the treatment group consists of workers from Islamic countries who are in fact Muslims. The comparison group contains non-Muslim workers from Islamic countries and all workers from non-Islamic countries regardless of their religion. Hence, using this alternative mixed definition, only the first type of measurement error may be still at work. The alternative definitions of treatment and comparison groups are summarized in *Table 2*.

#### **4. Empirical Results**

*Table 3* cross-tabulates employees' nationality against their religion. While coming from an Islamic country and being a Muslim are strongly correlated, the overlap is far from perfect. Roughly 8 percent of foreign employees from non-Islamic countries are Muslims, and roughly 11 percent of foreign employees from Islamic countries are no Muslims. At issue is whether these differences have implications for the empirical analysis.

*Table 4* provides descriptive statistics for the two pairs of treatment and control group. Muslim and non-Muslim employees are compared in the first two columns. Employees from Islamic and non-Islamic countries are compared in the third and the fourth column. Considering the descriptive statistics on the explanatory variables,

differences between treatment and control group are very similar for both pairs. Compared to the control group, employees in the treatment group have on average less schooling and shorter tenure. They also have on average less full-time work experience and more unemployment experience. Further, employees in the treatment group are more likely to perform unskilled blue-collar jobs and are less likely to be white-collar employees. Moreover, they are more likely to be employed in very large firms and are less likely to be employed in very small establishments. Finally, employees in the treatment group are more likely to be married.

However, most importantly, the descriptive statistics show that the definition of treatment and comparison group plays a crucial role in comparing wages. In the period before September 11<sup>th</sup>, the real wage of Muslims was on average approximately 3 percent lower than that of non-Muslims. The gap increased to 7 percent in the period after September 11<sup>th</sup>. The numbers allow calculating a simple difference-in-differences estimate. The estimate suggests that September 11<sup>th</sup> was associated with a 4 percent decrease in the earnings of Muslims relative to the earnings of non-Muslims. After eliminating rounding differences, the estimated treatment effect actually becomes larger. The estimated decrease in earnings is about 4.8 percent, and it is statistically significant at the 10 percent level. In contrast, if the definition of treatment and comparison group is based on workers' nationality, the descriptive statistics do not reveal an influence of the terrorists' attacks. In the period before September 11<sup>th</sup>, the real wage of employees from Islamic countries was on average 4 percent lower than that of employees from non-Islamic countries. The gap increased only slightly to 5 percent in the period after September 11<sup>th</sup>. Using these numbers, a difference-in-differences estimate does not

indicate a significant effect of the terrorists' attacks. Altogether, the descriptive statistics provide first explorative evidence for the hypothesis that September 11<sup>th</sup> had an effect on the wages of Muslims in Germany. Moreover, they also provide exploratory support for the view that the effect can be obscured if employees' nationality is used to define treatment and comparison group. Of course, these results can only be seen as first hints. Regardless of considering nationality or religion, employees in the treatment and in the control group differ on average in several observable characteristics. This calls for a regression adjusted difference-in-differences analysis that accounts for observable characteristics.

*Table 5* provides the results of the regression based difference-in-differences approach. Only the estimated coefficient on the interaction variable  $Sept_t * Treat_i$  is shown as this is the variable of primary interest. All of the control variables discussed in Section 3.2 are included but results on those variables are suppressed to save space. In regression (1), employees' nationality is used to define treatment and comparison group. The regression shows no statistically significant impact of September 11<sup>th</sup> on the earnings of the treatment group. This finding fits the results of the other European studies. However, two types of measurement error potentially imply an attenuation bias obscuring the influence of the terrorists' attacks. First, the comparison group partially includes Muslims who may be subject to increased discrimination. Second, the treatment group partially includes non-Muslims who may not be subject to increased discrimination. To examine the role of the two types of measurement errors in more detail, two mixed definitions of treatment and control group are used. In regression (2), the first type of measurement error is removed while the second type of error may be still at work.

Muslim employees from non-Islamic countries are excluded from the comparison group and are included in the treatment group. Compared to regression (1), the negative coefficient on the interaction variable grows in size. It is significant at the 10 percent level. In regression (3), the second type of measurement error is removed while the first type of error may be still at work. Non-Muslim employees are excluded from the treatment group and are included in the comparison group. Compared to regression (1), the negative coefficient on the interaction variable again grows in size and significance. Hence, in our data, removing one of the two types of measurement errors is already sufficient to reveal a negative effect of September 11<sup>th</sup> on the earnings of Muslims in Germany. Finally, in regression (4), the definition of both treatment and control group is based on employees' religion. The estimated negative coefficient is sizeable and statistically significant at the 5 percent level. The estimate implies that September 11<sup>th</sup> was associated with an approximately 5 percent decrease in the earnings of Muslims relative to the earnings of non-Muslims. However, so far we did not account for heterogeneous treatment effects of September 11<sup>th</sup>. Hence, the estimated influence can rather be interpreted as an average treatment effect that may hide a far richer pattern.

Our background discussion suggests that education should play a moderating role. Xenophobic attitudes are more prevalent among low-skilled Germans. To the extent low-skilled Muslims have low-skilled German superiors and coworkers, they face a higher risk of discrimination than skilled Muslims. Hence, we should find a stronger effect of September 11<sup>th</sup> on the earnings of low-skilled Muslims. Column (1) and column (2) of *Table 6* provide separate estimates for low-skilled and skilled employees. Low-skilled workers are workers performing unskilled blue-collar jobs. The group of skilled

employees comprises skilled blue-collar workers (with apprenticeship training) and white-collar workers. The estimates show no significant effect of September 11<sup>th</sup> on the earnings of skilled Muslims. In contrast, there is evidence of a strong negative effect on the earnings of low-skilled Muslims. The estimated coefficient is statistically significant at the 1 percent level and implies that September 11<sup>th</sup> was associated with an approximately 7 percent decrease in the earnings of low-skilled Muslims relative to the earnings of low-skilled non-Muslims. Hence, our estimates suggest that the terrorists' attacks in the US had a negative effect on the earnings of low-skilled Muslims in Germany but not on the earnings of skilled Muslims.

The question is now whether the negative effect on the earnings of low-skilled Muslims holds true across all firms or whether it is restricted to specific types of firms. Our background discussion implies that the size of the firm should also play a moderating role. Smaller firms are typically less subject to labor market regulation as they are less likely to be covered by collective bargaining and codetermination. Moreover, smaller firms are less likely to pursue standardized pay policies. To examine the moderating role of firm size we distinguish between small- and medium-sized firms with less than 200 employees, large firms with 200 to 1999 employees, and very large firms with 2000 and more employees. In columns (3a) to (3c), we provide separate estimates by firm size for low-skilled employees. While the estimates show no significant effect of September 11<sup>th</sup> on the earnings of low-skilled Muslims in large and very large firms, they provide evidence that the events of September 11<sup>th</sup> had a strong influence on the earnings of low-skilled Muslims in small- and medium-sized firms. The estimated coefficient is statistically significant at the 1 percent level. It suggests that the terrorists' attacks were

associated with an approximately 12 percent decrease in the relative earnings of low-skilled Muslims in small- and medium-sized firms. In columns (4a) to (4c) we provide separate estimates by firm size for skilled employees. In all three size classes, there is no significant effect on the relative earnings of skilled Muslims. Altogether, our results suggest that the negative effect of September 11<sup>th</sup> on the relative earnings of Muslims in Germany was limited to low-skilled Muslims in small- and medium-sized firms.

## **5. Robustness Checks**

*Table 7* provides a series of robustness checks. To this point we have considered the years 1998 to 2004 with a four-year pre- and a three-year post-September 11<sup>th</sup> period.<sup>17</sup>

As a check of robustness we now vary the period under consideration. First, we expand the post-September 11<sup>th</sup> period to 2005 implying that we symmetrically have a four-year pre- and post period. Second, we obtain estimates for the years 1999 to 2004. That means that we consider a three-year pre- and post-period. Third, we try a two-year pre- and post window by obtaining estimates for the years 2000 to 2003. All three robustness checks confirm the familiar pattern. While the estimates provide no evidence of an effect on the earnings of skilled Muslims, they show a strong negative effect on the relative earnings of low-skilled Muslims specifically in small- and medium-sized firms. Moreover, the estimates provide little evidence that the effect on low-skilled Muslims diminished over time. The size of the significant coefficients differs only slightly for the different windows and there is no clear pattern suggesting that the size of the coefficients decreases with the length of the window. Estimates obtained using the three-year windows are slightly smaller than those that use the two-year windows. However, estimates based on the four-year windows are slightly larger than the estimates using the

three-year windows.

Finally, we add Muslims with German citizenship to the treatment group and non-Muslim Germans to the comparison group.<sup>18</sup> Also this robustness check confirms the previous results. While the estimates show no significant influence of the terrorists' attacks on the relative earnings of skilled Muslims, they reveal a significantly negative relationship between September 11<sup>th</sup> and the relative earnings of low-skilled Muslims in small- and medium-sized firms.

## **6. Conclusions**

There seems to be little doubt that the events of September 11<sup>th</sup> have changed the world. However, our knowledge about the kind and the extent of the changes is far from complete. This also holds true for effects of the events on the labor market outcomes of Muslims in the Western world. While there are several pieces of evidence that September 11<sup>th</sup> negatively affected the relative earnings of employees with Arab background in the US, it is not clear that the terrorists' attacks had similar effects in other countries. Our study for Germany suggests that the events indeed influenced the relative earnings of Muslims also outside the US. Yet, identifying the effects is more complicated.

First, it requires defining appropriate treatment and control groups. If workers' nationality is used to define treatment and control group, our estimates show no significant effect of September 11<sup>th</sup>. While this fits the results of previous European studies, our theoretical considerations suggest that a definition based workers' nationality potentially involves an attenuation bias. This attenuation bias is more likely to obscure the effects of September 11<sup>th</sup> in European countries where changes in attitudes toward Muslims, on average, may have been less strong than in the US. Our data enable us to

pursue an alternative approach. If we use workers' religion to define treatment and control, the estimates reveal a sizable and statistically significant influence of the terrorists' attacks on the relative earnings of Muslims.

Second, in light of our theoretical considerations it appears very unlikely that there has been a uniform effect on all types of Muslims across all types of firms. Hence, it is important to carefully account for heterogeneous effects in the empirical analysis. Our results confirm that moderating factors indeed played an important role. The significantly negative influence on relative earnings holds only true for low-skilled Muslims but not for skilled Muslims. This makes sense as negative attitudes toward foreigners and Muslims are more prevalent among low-skilled Germans. If low-skilled Muslims are more likely to have low-skilled German superiors and coworkers, they face a higher risk of discrimination. Furthermore, splitting by establishment size, the negative effect can only be found for low-skilled Muslims employed in small- and medium-sized firms. This conforms to the hypothesis that there are more opportunities for discrimination in smaller firms as those firms are less likely to pursue standardized pay policies and are less subject to labor market regulations.

Altogether, our study for Germany provides the first evidence that the terrorists' attacks in the US negatively affected the labor market outcomes of Muslims even outside the US. We suggest that the effects of September 11<sup>th</sup> can remain obscured until the two steps described above are taken. Extending this type of analysis to other countries stands now as important future research.



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**Table 1:** Nationalities of Foreign Employees in the Data Set

Employees from Islamic countries	Albania, Algeria, Afghanistan, Bangladesh, Egypt, Eritrea, Iran, Iraq, Kazakhstan, Kyrgyzstan, Kosovo, Lebanon, Morocco, Nigeria, Turkey.
Employees from non-Islamic countries	Belgium, Bosnia/Herzegovina, China, El Salvador, Ex-Yugoslavia, Finland, France, Ghana, Greece, Great Britain, Netherlands, India, Italy, Cameroon, Canada, Croatia, Macedonia, Mozambique, Austria, Poland, Portugal, Romania, Russia, Slovenia, Spain, Sri Lanka, Stateless, Ukraine, Hungary, USA, Vietnam.

**Table 2:** Alternative Definitions of Treatment and Comparison Groups

	Treatment Group	Comparison Group
Definition based on nationality N=2,466	Employees from Islamic countries N=954	Employees from non-Islamic countries N=1,512
Definition based on religion N=2,466	Muslims N=960	Non-Muslims N=1506
Mixed definition I N=2,466	All employees from Islamic countries and Muslim employees from non-Islamic countries N= 1,068	Non-Muslim employees from non-Islamic countries N= 1,398
Mixed definition II N=2,466	Muslim employees from Islamic countries N=846	Non-Muslim employees from Islamic countries and all employees from non-Islamic countries N=1,620

**Table 3:** Overlap between Definitions Based on Nationality and Religion

	Non-Muslims	Muslims	Total
Employees from Non-Islamic Countries	1,398	114	1,512
Employees from Islamic Countries	108	846	954
Total	1,506	960	2,466

**Table 4:** Descriptive Statistics

Variable	(1) Muslim employees	(2) Non- Muslim employees	(3) Employees from Islamic countries	(4) Employees from non- Islamic countries
Log hourly real wage	2.50***	2.55	2.50***	2.55
Log hourly real wage before 9/11	2.49	2.52	2.49**	2.53
Log hourly real wage after 9/11	2.52***	2.59	2.53**	2.58
After 9/11 indicator	0.42	0.42	0.42	0.42
Years of schooling	10.21***	10.54	10.18***	10.56
Fulltime work	0.99	0.99	0.99*	0.98
Living with partner or spouse	0.91***	0.87	0.92***	0.87
Years of job tenure	9.81***	11.61	9.76***	11.64
Years of fulltime work experience	15.40***	20.08	14.83***	20.42
Years of part-time work experience	0.38	0.49	0.40	0.47
Years of unemployment experience	0.98***	0.67	0.95***	0.69
Firm size <20	0.11***	0.24	0.12***	0.24
Firm size 20-199	0.29	0.26	0.31***	0.25
Firm size 200-1999	0.31	0.34	0.31	0.34
Firm size >2000	0.29***	0.16	0.27***	0.18
Unskilled blue-collar worker	0.58***	0.45	0.60***	0.44
Skilled blue-collar worker	0.34	0.34	0.32	0.35
White-collar worker	0.08***	0.21	0.08***	0.21
N	960	1,506	954	1,512

\* Mean value for the target group is statistically different from the mean value for the comparison group at the 0.10 level; \*\* at the 0.05 level; \*\*\* at the 0.01 level. Note that the regressions also include 60 industry dummies and 10 region dummies. Descriptive statistics for these variables are not shown to save space.



**Table 5:** Initial Estimates, 1998 to 2004

	(1) Definition of treatment and comparison group based on nationality	(2) Mixed definition I	(3) Mixed definition II	(4) Definition of treatment and comparison group based on religion
DD	-0.018 (0.020)	-0.029* (0.016)	-0.039* (0.022)	-0.049** (0.018)
N	2,466	2,466	2,466	2,466

DD is the estimated difference-in-differences effect of September 11<sup>th</sup> on the earnings of the treatment group. Robust standard errors, clustered by federal state and treatment group, are in parentheses. \* Statistically significant at the 0.10 level; \*\* at the 0.05 level; \*\*\* at the 0.01 level. Note that the explanatory variables described in Section 3.2 are included but are suppressed to save space.

**Table 6:** Heterogeneous Effects of September 11<sup>th</sup> on the Earnings of Muslims, 1998 to 2004

	(1) Low-skilled employees	(2) Skilled employees	(3) Low-skilled employees			(4) Skilled employees		
			(3a) Small- and medium- sized firms	(3b) Large firms	(3c) Very large firms	(4a) Small-and medium- sized firms	(4b) Large firms	(4c) Very large firms
DD	-0.072*** (0.020)	-0.029 (0.032)	-0.121*** (0.026)	-0.033 (0.034)	0.007 (0.075)	-0.024 (0.054)	-0.083 (0.080)	-0.022 (0.078)
N	1,230	1,236	534	480	216	607	324	305

DD is the estimated difference-in-differences effect of September 11<sup>th</sup> on the earnings of the treatment group. Treatment and comparison group are based employees' religion. Robust standard errors, clustered by federal state and treatment group, are in parentheses. \* Statistically significant at the 0.10 level; \*\* at the 0.05 level; \*\*\* at the 0.01 level. Note that the explanatory variables described in Section 3.2 are included but are suppressed to save space.

**Table 7: Robustness Checks**

	(1) Low- skilled employees	(2) Skilled employees	(3) Low-skilled employees			(4) Skilled employees		
			(3a) Small- and medium- sized firms	(3b) Large firms	(3c) Very large firms	(4a) Small- and medium- sized firms	(4b) Large firms	(4c) Very large firms
Period 1998-2005 DD	-0.065*** (0.021)	-0.035 (0.026)	-0.118*** (0.028)	-0.027 (0.029)	0.032 (0.075)	-0.036 (0.052)	-0.063 (0.072)	-0.055 (0.080)
N	1337	1399	584	517	236	675	377	347
Period 1999-2004 DD	-0.060*** (0.021)	-0.045 (0.036)	-0.100*** (0.028)	-0.032 (0.04)	-0.007 (0.067)	-0.038 (0.056)	-0.100 (0.07)	-0.013 (0.080)
N	1055	1100	461	410	184	541	284	275
Period 2000-2003 DD	-0.068* (0.034)	-0.058 (0.035)	-0.125** (0.054)	-0.016 (0.044)	0.024 (0.095)	-0.058 (0.058)	-0.086 (0.072)	0.008 (0.071)
N	758	771	343	288	127	384	189	198
Including German Muslims and Non-Muslims DD	-0.040* (0.020)	-0.008 (0.029)	-0.111*** (0.029)	0.003 (0.023)	0.02 (0.054)	0.007 (0.046)	0.013 (0.096)	-0.024 (0.031)
N	3313	12943	1668	1047	598	6456	2949	3538

DD is the estimated difference-in-differences effect of September 11<sup>th</sup> on the earnings of the treatment group. Treatment and comparison group are based employees' religion. Robust standard errors, clustered by federal state and treatment group, are in parentheses. \* Statistically significant at the 0.10 level; \*\* at the 0.05 level; \*\*\* at the 0.01 level. Note that the explanatory variables described in Section 3.2 are included but are suppressed to save space.

## Endnotes

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<sup>1</sup> However, findings by Orrenius and Zavodny (2006) indicate that September 11<sup>th</sup> also worsened the outcomes of other groups in the United States. Their estimates show a negative impact on the earnings and hours worked among male Hispanic immigrants. The authors argue that this is due to the reforms that were enacted to offer greater protection from foreign enemies.

<sup>2</sup> In his British study, Braakmann (2007) uses workers' religion only to examine the effects of the Iraq war and the terrorists' attacks in 2004 and 2005. The information on workers' religion is not used to examine the effects of September 11<sup>th</sup>. Instead treatment and control group are defined by workers' nationality or country of birth. A recent study by Goel (2009) for Australia also considers workers' religion. However, like the studies for Europe, it does not account for moderating factors. Moreover, the Australian study appears to use only data from employees who *recently* immigrated to Australia. Clearly this casts serious doubts if the data are representative.

<sup>3</sup> While this may be explained by the high population share of foreigners in Germany (Gang and Rivera-Batiz 1994), the basic point for our analysis is that there is such a high prevalence of negative attitudes among German citizens which may have been reinforced by the events of September 11<sup>th</sup>.

<sup>4</sup> See Charles and Guryan (2008) for a recent empirical assessment of Becker's theory. Field studies by Goldin and Rouse (2000) and Bertrand and Mullainathan (2004) provide direct evidence on gender discrimination and racial discrimination.

<sup>5</sup> A second popular discrimination theory is the theory of statistical discrimination. This approach assumes that employers use average group productivity to remunerate workers if they cannot observe individual worker productivity (Aigner and Cain 1977). Workers belonging to different groups will receive different wages if the groups differ in their average productivities. This even holds true if the workers have the same individual productivity. It is not clear that this theory can be applied to explain potential labor market effects of September 11<sup>th</sup> (Aslund and Roth 2005).

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The terrorists' attacks would only have had an impact on statistical discrimination if they would have revealed additional information about the average productivity of Muslims. However, one might argue that after September 11<sup>th</sup> employers perceived Muslims as more risky workers.

<sup>6</sup> While Gang et al. (2002) find that the relationship between education and attitudes toward foreigners has decreased to some degree in Europe, they also show that it is still prevalent.

<sup>7</sup> This hypothesis is supported by empirical studies showing that collective bargaining coverage is associated with reduced gender wage discrimination (Heinze and Wolf 2010, Jirjahn and Stephan 2006).

<sup>8</sup> Indeed, the presence of a works council is associated with lower intra-establishment wage inequality (Jirjahn and Kraft 2010). Moreover, works councils are more likely to have a positive impact on establishment performance if there is reduced wage inequality among employees (Jirjahn and Kraft 2007). These findings support the notion that effective worker representation is more difficult when workers are heterogeneous (Tirole 2001).

<sup>9</sup> Baker et al. (1988) provide a detailed discussion on objective and subjective performance measures.

<sup>10</sup> In the year 2000, the share of foreigners in West Germany was 12% and that in East Germany was 2.8% (own computations based on data available from the Genesis online data base of the German Federal Statistical Office).

<sup>11</sup> The survey asks the question "How high was your income from employment last month?" Information on work hours comes from the question "How many hours [per week] do you actually work on average including overtime?" We divide the monthly gross wage by monthly hours, computed as weekly hours times 4.33.

<sup>12</sup> As the variable *Sept* is included in the regressions, two year dummies are left out to avoid perfect collinearity.

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<sup>13</sup> Of course, there are some countries where the population is relatively heterogeneous with respect to religion. To check the robustness of our results we excluded immigrants from Bosnia/Herzegovina, Ex-Yugoslavia, Kazakhstan and Macedonia when using nationality to define treatment and control group. This did not change the basic pattern of results. As a further check of robustness we restricted the treatment group to immigrants from Arab countries. This also produced a similar pattern of results.

<sup>14</sup> For example, Turks are the largest group of immigrants from an Islamic country in Germany. While the majority of Turkish immigrants are Muslims, there are at least 5.3 percent with another or no religious affiliation (von Gostomski 2008).

<sup>15</sup> Immigrants from the former Yugoslavia provide an example. While the majority of those immigrants are Christians, there are 20 percent who are Muslims (von Gostomski 2008).

<sup>16</sup> Other years in which the GSOEP data cover religion are 1990, 1997 and 2007. Unfortunately, for 1997 the information on non-Christian religion is incomplete, so that we cannot identify Muslims in the 1997 data.

<sup>17</sup> Recall that the annual GSOEP interviews were conducted before the month of September. Thus, the year 2001 belongs to the pre-September 11<sup>th</sup> period.

<sup>18</sup> As there is only a handful of Muslims with German citizenship in the data, we cannot perform separate estimates to examine the influence of September 11<sup>th</sup> on the earnings of German Muslims.