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The impact of September 11th, 2001 on the job prospects of foreigners with Arab background – Evidence from German labor market data

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Abstract

This paper examines whether the attacks on the World Trade Center and the Pentagon on September 11th, 2001 have influenced the job prospects of Arabs in the German labor market. Using a large, representative database of the German working population, the attacks are treated as a natural experiment that may have caused an exogenous shift in attitudes toward persons who are perceived to be Arabs. Evidence from regression-adjusted difference-in-differences-estimates indicates that 9/11 did not cause a severe decline in job prospects. This result is robust over a wide range of control groups and several definitions of the sample and the observation period. Several explanations for this result, which is in line with prior evidence from Sweden, are offered.

Keywords: Discrimination, September 11th, Exit from unemployment

JEL Classification: J64, J71

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1 Introduction

Following the terrorist attacks on the World Trade Center and the Pentagon on September 11th, 2001 (henceforth 9/11), numerous reports from Muslims or "Arab-looking" persons who feel harassed, discriminated, or threatened, indicate that attitudes toward this group may have changed. Although these reports should not be discarded lightly, empirical evidence on this question is rather scarce.

The available evidence can be loosely ordered into two groups. The first group mainly consists of surveys of newspaper reports, reported (criminal) incidents and opinion survey results, while the second and relatively small group of papers focuses on actual labor market outcomes.

An example for the first group of research 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 may be summarized as follows. While the amount 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 toward the Muslim community (Allen and Nielsen 2002: 19).¹

Additionally, they conclude that generally discrimination was based mainly on the perception of a person being Muslim rather than on the person's true religious affiliation. Their conclusion is based on the fact that abuses, when they occurred, were primarily targeted toward women wearing headscarfs or the classical Islamic veil, the *hijab*, and also toward Sikhs whose typical appearance, wearing beards and turbans, is similar to those typically associated with Muslims (Allen and Nielsen 2002: 35-37). They also report a rising level of general xenophobia in many parts of Europe that mainly manifested itself in the strengthening of pre-existent prejudices (Allen and Nielsen 2002: 42). In a study for 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".

The situation in the USA seems to have been somewhat worse, with the Arab American Institute reporting several incidents (along with several accounts of hospitality and support toward 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 reports over 800 cases of workplace discrimination (American-Arab Anti-Discrimination Committee 2003, a number of examples can be found on pp. 92-103).

¹An example for 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 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).

Although the evidence presented above seems to indicate that attitudes toward Muslims or Arabs in general have changed in the aftermath of 9/11, one should keep in mind that this notion is mainly based on reported incidents and opinion surveys. These may provide qualitative evidence and stated opinions, but need not necessarily provide information regarding actual behavior. Evidence on the latter, however, is rather scarce. Up to this point in time, there are only two papers available that address the question, whether 9/11 has led to a change in real labor market outcomes.

Åslund and Rooth (2005), using an approach quite similar to the one pursued in this study, 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 et al. (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.

This paper adds to the literature by providing first evidence for Germany. The analysis relies on process-produced 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. We treat the attacks as a natural experiment that may have led to a change in attitudes toward persons with an Arab background and use regression-adjusted difference-in-differences-estimators to assess the change in re-employment prospects of unemployed persons with an Arab background relative to several control groups. The findings presented here are in line with the results from Åslund and Rooth (2005), indicating that the hiring behavior of German employers seems to be relatively unaffected by the events on September 11th, 2001.

The rest of this paper is organized as follows: Section 2 briefly summarizes some theoretical considerations regarding the possible labor market discrimination of Arabs. Section 3 introduces the estimation strategy. The data and the collection of the estimation sample are presented in section 4. Descriptive results are presented in section 5, while the section 6 presents results for the initial specification as well as some variations to check the robustness of the results. Finally, section 7 concludes.

2 Theoretical considerations

The question whether members of certain groups are treated differently in the labor market has a long history in economics. In principle, there are two strands of literature. The first, usually known as "taste discrimination", begins with Becker (1957/1971). It typically

models labor market discrimination as a result of preferences of employers, employees, or customers toward or against certain groups. The second strand, labeled "statistical discrimination", focuses on the role of group membership as a proxy for unobserved worker characteristics. In these group of models, that started with Phelps (1972) and Arrow (1973), employers use information on group membership to assess otherwise unobservable differences in individual worker productivity that are more common among members of a certain group.²

As already pointed out by Åslund and Rooth (2005), a rise in the discrimination of Arabs could only be explained by statistical discrimination if the terrorist attacks revealed some previously unknown characteristic of Arabs that somehow relates to the labor productivity of that group. While it seems at least possible to treat the existence of radical Arab terrorists as a previously unknown characteristic of the Arab working population, the connection with labor productivity is rather vague. In general, an employer is unaffected by the (possible) fact that a worker turns out to be a terrorist. Additionally, since it seems rather unlikely that being a radical Muslim has any direct influence on labor productivity, statistical discrimination does not seem to explain a possible rise in discrimination very well.

A much more plausible explanation for a possible decline in labor market prospects seems to be a change in preferences induced by the terrorist attacks and followed by a higher amount of taste discrimination. This effect could affect employers as well as fellow employees and customers who feel "uncomfortable" employing, working alongside, or being served by persons they suspect to be terrorists. This in turn might lower the propensity of employers to hire Arab workers, either because they do not want to act against their own preferences, or because of a fear for negative reactions by customers or their own employees.

3 Estimation strategy

Empirical studies on labor market discrimination typically consider either differences in wage or income or differences in some measure of job prospects, like employment rates or flows out of unemployment, between (at least) two groups. In this study, we choose to focus on exit from unemployment into paid, dependent employment for several reasons.

Firstly, hiring decisions by employers are relatively unaffected by labor market institutions. In the time span covered by this paper, employers were generally free to hire whatever person they chose as long as a rejection was not officially substantiated by gender or disability. Wages and worker dismissals on the other hand were and are heavily influenced by collective bargaining and job protection laws, which would have to be taken into account in any analysis of these outcomes.

Secondly, a possible rise in discrimination against Arabs could also affect those persons who switch between different paid employments. This effect, however, would be hard to measure since there is no way to distinguish persons who refrained from switching jobs because of a (suspected) rise in discrimination from those persons who never had the intention to leave their current job in the first place. Ignoring this difference would most

²For an overview on these theoretical models as well as later extensions cf. Altonji and Blank (1999).

likely introduce some sort of selection bias into the analysis. This problem is not present if one focuses on the unemployed – at least as long as one is willing to assume that the unemployed have a general interest to switch into employment.

Finally, exits into self-employment are excluded for several reasons. Firstly, discrimination by employers is logically impossible here. Discrimination by fellow employees seems at least unlikely given the relative freedom of the self-employed to choose their partners or employees. Finally, customer discrimination, while being entirely possible, cannot be measured with our data. Additionally, the possible effect of 9/11 on the propensity to become self-employed is ambiguous. On the one hand, (assumed) customer discrimination may provide an obstacle for those unemployed who wish to enter self-employment. On the other hand, a rise in labor market discrimination that lowers the probability of an exit into dependent employment may as well raise the probability to become self-employed.

Now, turning to the event of interest, note that the terrorist attacks can be considered as a natural experiment leading to an exogenous shift in attitudes toward Arabs. Interest in this paper lies in the estimation of the causal (treatment) effect of this intervention on the re-employment prospects of the Arab unemployed. Note that by definition this effect can only be directed toward Arabs, who therefore constitute our treatment group. Note further that there is a clear theoretical one-way causality between intervention and outcome and selection out of or into the treatment group can be ruled since a person cannot elect to be Arab.³

Given the nature of the intervention, we are only able to observe either treated or untreated persons at any given point in time: Before September 11th, 2001 no treatment has taken place yet, afterward all Arabs are affected by the same treatment.

The simplest possible estimator for such a situation would be a comparison of the expected outcome before and after September 11th, 2001 for the Arab subpopulation. This equals a before-after-estimator of the form

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

where Y is the outcome of interest, τ is the effect to be estimated, $P \in \{0, 1\}$ denotes pre-/post-treatment-status, and we implicitly condition on the Arab subpopulation. However, τ could only be interpreted as the causal effect of 9/11 if one was willing to assume that there was neither an effect of the terrorist attacks on the whole population, nor any general economic trend independent of 9/11.

Using a control group, like natives or non-Arab foreigners, that underlies the same general trend, but is unaffected by any shifts in attitudes introduced by 9/11, one is able to relax those highly restrictive assumptions and use a difference-in-differences-estimator of the form

$$\begin{aligned} \tau &= E[Y|P = 1, T = 1] - E[Y|P = 0, T = 1] \\ &\quad - (E[Y|P = 1, T = 0] - E[Y|P = 0, T = 0]), \end{aligned} \quad (2)$$

where T is 1 if a given person can be considered to be Arab and 0 otherwise. τ can be interpreted as the causal impact of the treatment under the assumption that both groups would have experienced the same trend in the absence of the treatment.

³As "being Arab" is not observed in the data and has to be approximated by nationality, selection out of the treatment group is an issue. The problem and a solution are presented in section 4.

This assumption is rather critical since it implies that differences between the two groups are stable over time. For example, consider the case where the treatment group in the post-treatment period has on average – and for reasons not related to 9/11 – a lower educational level than in the pre-treatment period. If this fact leads to lower re-employment probabilities, equation (2) would overstate the true effect of 9/11.

Since the natural experiment setting ensures that both treatment status and group affiliation are exogenous, equation (2) can be written in regression form as

$$y_i = \alpha + \lambda * T_i + v * P_i + \tau * (T_i * P_i) + \epsilon_i, \quad (3)$$

where τ gives the treatment effect, i indexes persons, and ϵ is an error term uncorrelated with T and D .

Adding a vector of covariates X to equation (3) allows us to relax the assumption of a common trend by only having to assume a common trend over groups after observed differences have been accounted for. This regression-adjusted difference-in-differences-estimator takes the form

$$y_i = \alpha + \beta X_i + \lambda * T_i + v * P_i + \tau(T_i * P_i) + \epsilon_i, \quad (4)$$

where X in this paper contains information on age, gender, school and post-school education, disability, the duration of the current unemployment episode, dummies for 33 occupations, and dummies for 12 types of regional labor market conditions based on a cluster-analysis by Blien et al. (2004).

Finally, note that Y is binary in our case, being 1 if a person switches from unemployment to employment. We take the binary nature explicitly into account by formulating the model

$$y_i = \mathbf{1} \{ \alpha + \beta X_i + \lambda * T_i + v * P_i + \tau(T_i * P_i) + \epsilon_i > 0 \}, \quad (5)$$

where $\mathbf{1} \{ \cdot \}$ is the indicator function. Assuming ϵ to be normally distributed, equation (5) can be estimated as a probit. The probability of a switch to employment would then be given by

$$Prob(Y_i = 1) = \Phi(\alpha + \beta X_i + \lambda * T_i + v * P_i + \tau(T_i * P_i)), \quad (6)$$

where Φ is the standard normal distribution's cdf.

This approach introduces an additional challenge: In a Probit regression, the estimated coefficients are not equal to the marginal effect of the respective variable. Although one is able to answer questions regarding the statistical significance and the sign of the coefficient and therefore the question, whether there is a (negative) impact of the terrorist attacks, inference on the size of the effect requires taking an additional step.

One can obtain an estimate for the average effect of τ in the sample at hand by estimating equation (6), predicting the re-employment probabilities for all observations in the sample, average over group-period combinations, and calculate

$$\begin{aligned} \tau_{avg} &= \bar{Y}_{P=1,T=1} - \bar{Y}_{P=0,T=1} \\ &\quad - (\bar{Y}_{P=1,T=0} - \bar{Y}_{P=0,T=0}), \end{aligned} \quad (7)$$

where τ_{avg} is the marginal effect in the sample and \bar{Y} is the average of the predicted probability in the respective group-period-combination.

The variance of τ_{avg} can then be obtained by bootstrapping the procedure outlined above with a large number of replications.⁴ This procedure gives a distribution of different values of τ_{avg} , whose variance can be used as an estimate for the variance of τ thus making it possible to obtain confidence bounds or statistical significances. Note that this approach does not provide any additional information in case of insignificant coefficients since in this situation neither coefficient nor marginal effect can be considered different from zero on statistical grounds.

4 Data and sampling procedure

This paper uses data from the IEB (*Integrierte Erwerbsbiographien*), which contains process-produced data from social security, unemployment insurance, participation in active labor market programs, and unemployment registrations. Included are labor market spells for all workers, employees, trainees, and apprentices covered by social security. While the IEB itself is not available to the public, a 2.2% sample, the so called IEBS, can be accessed by researchers via the research data center (*Forschungsdatenzentrum*, FDZ) of the Federal Employment Agency.⁵ Up to today, full documentation of the data is only available in German (cf. Hummel et al. 2005). A short English description can be found in Jacobebbinghaus and Seth (2007).

The IEBS, which is used in this paper, covers 17,049,987 periods of employment, unemployment, and participation in active labor market programs for 1,370,031 individuals. Employment data is gathered for the years 1990-2003 (West) and 1993-2003 (East) from mandatory notifications to social security by the employers and collected in the BeH (*Beschäftigtenhistorik*). Employers are legally required to deliver an annual report for each worker employed on December 31st of each year as well as notifications at the beginning and end of every employment.

Information on participation in active labor market programs is contained in the MTG (*Maßnahme-Teilnehmer-Gesamtdatenbank*). This data is collected from the Federal Employment Agency's own records and covers the years 2000-2004.

In addition, the IEBS provides information on unemployment periods from two data sources. The LeH (*Leistungsempfängerhistorik*) covers all periods where unemployment benefits were received and is collected from payment informations from the Federal Employment Agency. The second source is the BewA (*Meldungen aus dem Bewerberangebot*), that covers all periods where a person was registered as unemployed (regardless whether unemployment benefits were received). The data is collected by the local labor agency and is primarily used for the placement of the unemployed. The LeH covers the years 1990-2003 for West and 1993-2003 for East Germany, while the BewA-information is available from 2000 to 2004.

As mentioned in the preceding chapter, this paper focuses on the re-employment prospects of the unemployed. For the forthcoming calculations, we will mainly focus on

⁴Running this procedure for one control and all treatment groups with 1000 replications takes about eight days to run in the research data center of the Federal Employment Agency. A much larger number of replications is not possible due to restrictions in computer time.

⁵Information regarding the research data center, the available data sets, and access restrictions can be obtained via <http://fdz.iab.de>. An English version of this site should be available in the first quarter of 2007.

the information contained in the BewA. Firstly, since every period of unemployment benefit receipt has to be accompanied by an unemployment registration but not vice versa, the BewA covers (at least in principle) every episode contained in the LeH. Additionally, it also covers those individuals who are unemployed but do not receive any unemployment benefits. 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 three years after September 11th, 2001. Finally, the BewA offers a reasonably simple, yet accurate way to construct the outcome variable, since it provides information on the reason for the end of the respective unemployment period.

Using this dataset has several advantages in the context of the analysis conducted here. Firstly, since the data was produced and collected during administrative processes, the risk of strategic response behavior or non-response resulting in selective samples is eliminated. Secondly, data is 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. Firstly, the database contains only information which is commonly reported in social insurance, active labor market programs, and during periods of unemployment or receipt of unemployment benefits. However, since this includes most labor market-relevant characteristics like age, gender, nationality, education, regional characteristics, and occupations, this restriction is not severe for the scope of this paper.

Unfortunately, data quality varies greatly between those variables actually necessary for the administrative process and those collected purely for statistical purposes, which results in some oddities in the data. This fact presents a more serious threat to our analysis since nationality, which will be used as an approximation to "Arab background", is one of those purely statistical variables. To overcome the oddities in this variable, several cleansing procedures were applied that are documented in the appendix.

After these cleansing procedures several treatment and control groups based on the observed nationalities in the respective unemployment spell were defined (see table 1 for a full list). The first definition of the treatment group includes all persons with a nationality from a Middle Eastern country as well as those from countries commonly considered "Muslim", e.g. Pakistan or Afghanistan but excludes any Turks. This group will henceforth be referred to as the "narrow" treatment group.

However, Turks are considered in a "broad" definition of the treatment group, which also includes all countries from the narrow definition. This separate treatment of the Turkish population seems reasonable since 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. These definitions based on the respective observed nationality will henceforth be labeled "nationality concept 1".

Additionally, one has to ensure that no selection out of the treatment group occurs. While a person cannot elect to be "Arab", nationality can be influenced through naturalization. One might, for example, consider a situation where individuals feel that they are

discriminated because of their nationality. Such a situation could create a strong incentive to become German. Since it seems likely that naturalized individuals still share some aspects of non-naturalized individuals, e.g. outer appearance, continued discrimination remains possible. This, however, could invalidate the results of our 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, we account for this possibility by using an alternative definition of nationality. This definition ("nationality concept 2") focuses on the entire observed period from 1990-2003: Persons are treated as Arabs if they have ever been recorded to possess an Arab nationality as defined in Table 1.

TABLE 1: COUNTRIES IN TREATMENT/CONTROL-GROUPS

Group	Countries
Treatment Groups	
Arabs (narrow)	Afghanistan, Algeria, Bahrain, Brunei, Egypt, Iraq, Iran, Jordan, Kuwait, Lebanon, Libya, Morocco, Oman, Pakistan, Qatar, Saudi Arabia, Syrian Arab Republic, Tunisia, United Arab Emirates, Yemen
Arabs (broad)	Arabs (narrow) + Turkey
Control Groups	
Germans	Germany
Core 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, Poland, Romania, Russian Federation, Slovakia, (former) Soviet Union, Ukraine
South Europe	Cyprus, Greece, Italy, Malta, Portugal, Spain
South-East Europe	Albania, Bosnia and Herzegovina, Croatia, Macedonia, Slovenia, Yugoslavia
Southern Africa	Angola, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Central African Republic, Chad, Comoros, Congo, Congo (Democratic Republic), Côte d'Ivoire, Djibouti, Eritrea, Equatorial Guinea, Ethiopia, Gabon, The Gambia, Ghana, Guinea, Guinea-Bissau, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritania, Mauritius, Mozambique, Namibia, Niger, Nigeria, Rwanda, São Tomé and Príncipe, Saint Helena and Ascension, Senegal, Seychelles, Sierra Leone, Somalia, South Africa, Sudan, Swaziland, Tanzania, Togo, Uganda, Zambia, Zimbabwe
South America	Antigua and Barbuda, Argentina, Bahamas, Barbados, Belize, Bolivia, Brazil, Chile, Colombia, Costa Rica, Cuba, Dominica, Dominican Republic, Ecuador, El Salvador, Grenada, Guatemala, Guyana, Haiti, Honduras, Jamaica, Mexico, Nicaragua, Panama, Paraguay, Peru, Saint Lucia, Saint Vincent and The Grenadines, Saint Kitts and Nevis, Suriname, Trinidad and Tobago, Uruguay, Venezuela

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, South Europeans, East Europeans, South-East Europeans (mostly Balkan countries), South American and, individuals from Southern Africa. The control groups are formed according to the nationality concepts outlined above. Concept

1 is again based on the observed nationality in the respective unemployment spell, while in concept 2 all groups are formed by considering the period 1990-2003. In this concept, persons are considered to belong in one of the groups if they have ever been recorded as possessing the respective nationality. However, there is one exception: To avoid problems with naturalized foreigners, persons are only considered to be German if they have been constantly recorded as Germans throughout the entire observed time span.

Pre- and post-treatment groups are defined by a flow-sampling procedure to assure independence of the two groups. Note that using a stock sampling scheme could introduce selection bias. Consider the case, where stocks of persons, e.g. all those who are unemployed at a given date, are sampled. This would result in a situation where all persons who could not find a job during the pre-treatment period would automatically end up in the post-treatment period. Since it is not unlikely that those persons have a lower propensity to leave unemployment due to some unobservable factors, systematic but unobserved differences between the two groups might arise. Flow sampling procedures avoid this problem and allow the groups to be treated independently.

The pre-treatment group is formed by those persons entering unemployment between January 1st, 2000, the date the first BewA-spells are observed in the data, and September 10th, 2001, while the post-treatment groups consists of all individuals entering unemployment between September 11th, 2001 and March 10th, 2004, the latter being one day before the terrorist bombing of several trains in Madrid. Note that in this set-up, the duration of the current unemployment period has to be included in the estimation to account for the fact that persons entering unemployment to the end of the observation period have far less time to become employed than those entering earlier.

Further control variables include gender, age, and two dummy variables for different degrees of disability, where age is measured at the beginning of the unemployment episode. The dummies for disability are formed in line with German disability law that distinguishes those with a degree of disability between 30% and below 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. Finally, dummy variables for regional labor market conditions are included. These are based on an analysis by Blien et al. (2004) that clustered regions (*Arbeitsagenturbezirke*) with similar labor market conditions.

5 Descriptive Results

Table 2 presents some basic descriptive results for the variables in the estimation sample. Focusing on the differences between the treatment and control groups, one should note that the share of Arabs who are men or have received no post-school training is much higher

than in the other groups. Unemployment duration also tends to be higher for this group, which may be related to the aforementioned lack of post-school education. Looking at the outcome of interest, one notices that there is some variation between the groups with the treatment groups having the lowest share of exits into employment.

Additionally, two further facts should be noted. Firstly, there are only very few academics in the estimation sample, while the share of those with no post-school training varies between 40% and 65%. 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.

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 below 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%.⁶ 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 raw differences in the outcome of interest as depicted in table 3, one can see a slight drop in re-employment prospects for the narrowly defined Arab group while the broadly defined group experienced a rise in re-employment prospects. However, this rise was lower than that experienced by natives and several other control groups, therefore indicating a relative drop in re-employment prospects. One should note, however, that this picture is not particularly clear since re-employment prospects in some control groups also dropped after 9/11. Additionally, this effect could be due to some changes in group compositions that cannot be controlled for in a univariate analysis.

To conclude, there seem to be some hints pointing toward a decline in re-employment prospects of Arabs after 9/11. However, the picture is not as clear as one might have expected a priori. In the following section more elaborate econometric techniques are used to assess whether there are confounding factors, like changing group compositions, that disguise a possible negative effect.

6 Results

Following the procedure outlined in the beginning of this paper, 28 Probit regressions (four treatment groups contrasted with seven control groups) were calculated.⁷ The full set of estimation results can be found in tables 8 to 14 in the appendix. Note that almost all coefficients of the control variables have the expected signs and are usually statistical significant at a conventional level.

Table 4 presents the results for the parameters of interest in the difference-in-differences-estimations. In almost all specifications Arabs tend to have a lower re-employment proba-

⁶Under 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).

⁷Stata 9.2 SE (StataCorp 2005) was used in all calculation. Do-files are available from the author on request.

TABLE 2: DESCRIPTIVE STATISTICS, MEAN VALUES, STANDARD DEVIATION BELOW

Variable	Germans		Arabs		Central Europe		Southern Europe		East Europe		South-Eastern Europe		South America		Africa			
	narrow	broad	narrow	broad	Europe	Europe	Europe	Europe	Europe	Europe	Europe	Europe	America	America	Africa	Africa		
Exit into employment (dependent variable)	0.3176	0.2349	0.2240	0.3474	0.2670	0.2918	0.3033	0.2875	0.3113	0.4655	0.4240	0.4169	0.4762	0.4424	0.4546	0.4529	0.4631	
Age (years)	36.5365	35.0863	33.9896	40.0708	36.2585	37.9767	36.2580	37.3227	35.6310	11.7762	9.4348	10.4727	10.9000	11.1600	10.3732	11.9253	8.6285	8.0708
Male (1=yes)	0.5918	0.8179	0.7041	0.6191	0.6656	0.5581	0.6965	0.5136	0.7657	0.4915	0.3859	0.4565	0.4857	0.4718	0.4966	0.4598	0.5001	0.4237
Disabled 30% (1=yes)	0.0078	0.0009	0.0040	0.0062	0.0045	0.0034	0.0051	0.0000	0.0000	0.0882	0.0306	0.0628	0.0783	0.0673	0.0580	0.0710	0.0000	0.0000
Disabled 50% (1=yes)	0.0364	0.0199	0.0311	0.0361	0.0389	0.0207	0.0407	0.0034	0.0197	0.1873	0.1395	0.1736	0.1866	0.1933	0.1424	0.1976	0.0583	0.1389
Unemployment duration (days)	220.0	261.1	258.1	204.4	216.3	228.8	212.2	195.3	214.08	262.4	285.1	278.4	248.5	248.7	259.4	243.1	220.6	238.1
Higher secondary schooling (1=yes)	0.1075	0.1260	0.0531	0.1838	0.0476	0.2069	0.0376	0.1932	0.0921	0.3097	0.3318	0.2243	0.3874	0.2129	0.4051	0.1902	0.3950	0.2893
No post-school training (1=yes)	0.4035	0.5153	0.6361	0.3631	0.6000	0.3501	0.5639	0.4591	0.5689	0.4906	0.4998	0.4811	0.4809	0.4899	0.4770	0.4959	0.4986	0.4953
Academic Training (1=yes)	0.0438	0.0452	0.0171	0.0779	0.0159	0.0938	0.0063	0.0523	0.0215	0.2047	0.2077	0.1298	0.2680	0.1252	0.2916	0.0789	0.2227	0.1450
No. of Observations with non-missing values	375,221	5,170	22,876	3,576	8,895	6,639	8,409	709	1,823	to 437,185	to 6,398	to 28,574	to 4,211	to 10,551	to 8,887	to 10,053	to 880	to 2,236

TABLE 3: EXITS INTO EMPLOYMENT BY GROUPS PRE AND POST 9/11

Variable	Arabs		Germans	Southern Europe		East Europe	South-Eastern Europe		South America	Africa
	narrow	broad		Europe	Europe		Europe	Europe		
Exit into employment (share)										
(No. of cases)										
Nationality concept 1										
Pre-9/11	0.2441 (1,516)	0.2071 (7,713)	0.2937 (173,456)	0.3102 (1,022)	0.2372 (3,133)	0.2578 (1,633)	0.2838 (2,741)	0.1944 (180)	0.3226 (530)	
Post-9/11	0.2440 (2,418)	0.2256 (12,396)	0.3305 (276,969)	0.3042 (1,693)	0.2670 (5,498)	0.2589 (2,912)	0.3056 (4,788)	0.2445 (319)	0.3031 (927)	
Difference (post-pre)	-0.001	0.0185	0.0368	-0.060	0.0011	-0.0249	0.0218	0.0501	-0.0195	
Nationality concept 2										
Pre-9/11	0.2358 (2,413)	0.2104 (10,757)	0.2945 (168,832)	0.3511 (1,612)	0.2476 (3,938)	0.2865 (3,302)	0.2863 (3,674)	0.2700 (300)	0.3289 (824)	
Post-9/11	0.2344 (3,985)	0.2322 (17,817)	0.3321 (268,353)	0.3451 (2,599)	0.2785 (6,613)	0.2949 (5,585)	0.3131 (6,379)	0.2966 (580)	0.3010 (1,412)	
Difference (post-pre)	-0.0014	0.0218	0.0376	-0.0060	0.0309	0.0084	0.0268	0.0266	-0.0279	

TABLE 4: PARAMETERS OF INTEREST BY CONTROL/TREATMENT-GROUP

Controls:	Germans	Central Europe	Southern Europe	East Europe	South-Eastern Europe	South America	Africa
Nationality concept 1, excluding Turks							
Arab	-.1101522 (0.012)	-.1585167 (0.024)	.0446635 (0.399)	-.225052 (0.001)	-.0426397 (0.438)	.2465087 (0.092)	-.156152 (0.052)
Post-9/11	.0988186 (0.000)	.0286626 (0.638)	.0867898 (0.015)	-.0526942 (0.339)	.0509357 (0.172)	.2606318 (0.115)	-.0639466 (0.444)
Interaction Arab*Post-9/11	-.063031 (0.255)	-.0123022 (0.881)	-.0772431 (0.232)	.066737 (0.387)	-.0358361 (0.587)	-.2747318 (0.114)	.0746814 (0.454)
Nationality concept 1, including Turks							
Arab	-.2237069 (0.000)	-.2899607 (0.000)	-.0777085 (0.027)	-.3571821 (0.000)	-.1869798 (0.000)	.0352 (0.798)	-.29918 (0.000)
Post-9/11	.0982592 (0.000)	.0248081 (0.682)	.0929132 (0.009)	-.0652379 (0.233)	.0503352 (0.174)	.2085986 (0.200)	-.0639876 (0.441)
Interaction Arab*Post-9/11	-.0202092 (0.423)	.0343179 (0.599)	-.0400036 (0.352)	.1254853 (0.036)	.0114755 (0.796)	-.1537532 (0.351)	.1205997 (0.163)
Nationality concept 2, excluding Turks							
Arab	-.1012577 (0.004)	-.2739149 (0.004)	.0295209 (0.499)	-.1986276 (0.000)	-.0469251 (0.295)	-.123881 (0.250)	-.1465818 (0.028)
Post-9/11	.0997116 (0.000)	.0300796 (0.530)	.1014725 (0.001)	.005291 (0.885)	.0737853 (0.022)	.0275486 (0.819)	-.0335807 (0.633)
Interaction Arab*Post-9/11	-.0568243 (0.192)	-.0011783 (0.985)	-.0914892 (0.085)	.0244249 (0.664)	-.0525679 (0.327)	-.0252241 (0.844)	.0456433 (0.579)
Nationality concept 2, including Turks							
Arab	-.209001 (0.000)	-.3990155 (0.000)	-.0793732 (0.011)	-.3288664 (0.000)	-.1906975 (0.000)	-.2372008 (0.019)	-.2437222 (0.000)
Post-9/11	.0989274 (0.000)	.0226198 (0.638)	.1024361 (0.001)	-.0058768 (0.872)	.0658192 (0.043)	.0118806 (0.920)	-.0119869 (0.864)
Interaction Arab*Post-9/11	.0040293 (0.851)	.0666333 (0.203)	-.0200533 (0.599)	.0956851 (0.022)	.0238719 (0.535)	.0716961 (0.552)	.0965171 (0.186)

bility than the respective control group. Additionally, a positive effect of the period after September 11th, 2001 can be found. This is most likely not directly attributable to the aftermath of 9/11 but rather a general macroeconomic effect. Now, focusing on the Arab-period-interaction as the most interesting parameter for this analysis, one may note that the respective coefficient is insignificant in all but one specification. This indicates that there is no decline in re-employment prospects for Arabs in the German labor market that is caused by the 9/11-attacks.

Additionally, several variations of the sample definition or the observation time have been used to assess the robustness of this result. Table 5 presents the results for the parameters of interest when the estimation sample is varied. The full set of coefficients for the different estimations can be found in tables 15 to 17 in the appendix. Since no substantial differences between different control groups could be found, these results were calculated using only Germans as the control group.

Firstly, as the situation of foreigners as well as the general labor market situation is different between East and West Germany, all estimations were repeated using only persons living in East and West Germany respectively. As can be seen from the results, the parameters of interest do not change very much. Although there are some changes in the point estimates, including some changes in the direction of the coefficients, the interaction terms measuring the impact of the 9/11-attacks remain insignificant on all conventional levels.

Secondly, since most of the persons involved in the 9/11-attacks were men, it seems possible that an attitude shift is only directed toward Arab men. Therefore all calculations were repeated using only the male part of the estimation sample. The results are qualitatively similar to the East-West-estimates: Although there is some variation in the parameter estimates, the parameters of interest generally do not change very much. In particular, the interaction terms remain insignificant, confirming our results that 9/11 did not influence the re-employment probabilities of Arabs.

Finally, it seems possible that a change in attitude may solely be directed toward young Arab men, since all of the (direct) participants in the 9/11-attacks were under the age of 40.⁸ However, estimates with a reduced sample including only men under 35 years of age confirm our previous results.

In addition to these sample variations, different observation periods are considered. While the main part of this paper considers the long run effects of 9/11 and looks at a period from January 1st, 2000 to March 10th, 2004, this part focuses on possible short run effects. Since it seems possible that 9/11 caused only a temporary shift in attitudes that declined over time, the periods 9/11 +/- 180 days and 9/11 +/- 365 days are considered. Table 6 shows the parameters of interest. For the full set of results, see tables 18 and 19 in the appendix. As one can see, the parameters of interest do not change very much, again confirming our previous results.

To conclude, the main result of this paper, that a possible shift in attitudes following 9/11 did not influence the re-employment prospects of Arabs in the German labor market, seems fairly robust. It does not only hold for our initial specification but also over a wide range of different sample, observation period, and treatment and control groups definitions.

⁸Mohamed Atta, being the oldest of the hijackers with known age, was 33 at the time of the attacks.

TABLE 5: ROBUSTNESS CHECKS: SAMPLE VARIATION, PARAMETERS OF INTEREST, ARABS VS. GERMANS, SEE APPENDIX FOR FULL RESULTS

	West-Germany only	East-Germany only	Men only	Men under 35 only
Nationality concept 1, excluding Turks				
Arab	-.1032929 (0.024)	-.1438682 (0.356)	-.0782519 (0.100)	-.1452294 (0.016)
Post-9/11	.0797416 (0.000)	.1302373 (0.000)	.0867844 (0.000)	.0485992 (0.000)
Interaction Arab Post-9/11	-.060295 (0.296)	.1870751 (0.342)	-.0444255 (0.460)	-.1133497 (0.138)
Nationality concept 1, including Turks				
Arab	-.2165003 (0.000)	-.355461 (0.000)	-.1490908 (0.000)	-.1549461 (0.000)
Post-9/11	.0786513 (0.000)	.1302944 (0.000)	.0862298 (0.000)	.0479556 (0.000)
Interaction Arab Post-9/11	-.0092517 (0.726)	.1304206 (0.179)	-.0255927 (0.377)	-.0217101 (0.534)
Nationality concept 2, excluding Turks				
Arab	-.1024046 (0.006)	-.0492361 (0.685)	-.0636556 (0.100)	-.1784725 (0.000)
Post-9/11	.0802198 (0.000)	.1308486 (0.000)	.0876776 (0.005)	.0486717 (0.000)
Interaction Arab Post-9/11	-.0446521 (0.333)	.040337 (0.793)	-.033517 (0.488)	-.0270522 (0.674)
Nationality concept 2, including Turks				
Arab	-.2085653 (0.000)	-.2539706 (0.000)	-.13485 (0.000)	-.1633904 (0.000)
Post-9/11	.078747 (0.000)	.1308652 (0.000)	.0870229 (0.000)	.0480173 (0.000)
Interaction Arab Post-9/11	.0196698 (0.384)	.0692564 (0.362)	.0045567 (0.854)	.0068511 (0.823)

TABLE 6: ROBUSTNESS CHECKS: TIME VARIATION, PARAMETERS OF INTEREST, ARABS VS. GERMANS, SEE APPENDIX FOR FULL RESULTS

	9/11 +/- 180 days	9/11 +/- 365 days
Nationality concept 1, excluding Turks		
Arab	-.1772682 (0.143)	-.176365 (0.000)
Post-9/11	.0277309 (0.033)	.0294326 (0.000)
Interaction Arab Post-9/11	.0528074 (0.746)	-.0391454 (0.659)
Nationality concept 1, including Turks		
Arab	-.2225008 (0.000)	-.2579303 (0.000)
Post-9/11	.0269772 (0.037)	.0293221 (0.000)
Interaction Arab Post-9/11	.0945181 (0.201)	.0202493 (0.616)
Nationality concept 2, excluding Turks		
Arab	-.1916928 (0.042)	-.1392863 (0.005)
Post-9/11	.027549 (0.036)	.0298535 (0.000)
Interaction Arab Post-9/11	.0632985 (0.621)	-.0460141 (0.515)
Nationality concept 2, including Turks		
Arab	-.2353553 (0.000)	-.2366044 (0.000)
Post-9/11	.0268672 (0.040)	.0296676 (0.000)
Interaction Arab Post-9/11	.0886659 (0.159)	.0381048 (0.268)

7 Conclusion

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

Our results indicate that the job prospects of Arabs in the German labor market have not been harmed by the terrorist attacks. This result is robust over a wide range of different treatment and control group definitions. Additionally, it also continues to hold when we consider East and West Germany separately, restrict the sample to men and men under 35 years to mimic the characteristics of the 9/11-terrorists, and consider different observation periods to capture eventual short-run effects.

While this result may seem somewhat counterintuitive given the anecdotal evidence mentioned at the beginning of this text and also contradicts the available evidence from the USA by Dávila and Mora (2005), it is perfectly in line with the Swedish results by Åslund and Rooth (2005). A reason for this 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 rationally in their hiring behavior or that discrimination is based on some deeper preferences that remain unaffected by singular events like 9/11. In the first case, one would have to conclude that most of the discrimination observed in the labor market is due to statistical discrimination, on which the terrorist attacks should have had no large impact. The second case seems possible, although it remains unclear, what these deeper preferences should reflect, given their relative inflexibility to an event like 9/11.

An interesting question regarding possible differences between Europe and the much more involved USA emerges. Although one should be cautious about inferring fundamental differences from the results of just three studies, it may be possible that the reaction in the USA was heavier because of the direct involvement in the attacks and the subsequent "war on terror", while Europe, not being a direct target, remained more calmly.

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A 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 4, 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 wage informations, which are 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 toward 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 (receiving 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 informations.

To overcome these problems a number of cleansing procedures based on the needs for this analysis as well as on some assumptions was 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 1. This eliminates some of the changes that were caused by changes in the political landscape, e.g. the break-up of former Yugoslavia.
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.

Table 7 shows the frequency of nationality changes before and after data cleaning. As one can see, most of the cases with a very high-number of changes could be resolved. Since the remaining cases still include changes to and from missing values, the remaining number of changes seems plausible.

TABLE 7: FREQUENCY OF NATIONALITY CHANGES BEFORE/AFTER CLEANING

No. of changes	before cleaning			after cleaning		
	cases	percent	cum. percent	cases	percent	cum. percent
0	1,214,758	88.67	88.67	1,306,164	95.34	95.34
1	37,511	2.74	91.40	42,331	3.09	98.43
2	41,422	3.02	94.43	18,332	1.34	99.77
3	8,473	0.62	95.05	2,977	0.22	99.98
4	23,976	1.75	96.80	219	0.02	100.00
5	3,565	0.26	97.06	7	0.00	100.00
6	11,842	0.86	97.92	1	0.00	100.00
7	2,112	0.15	98.08			
8	6,756	0.49	98.57			
9	1,365	0.10	98.67			
10	4,114	0.30	98.97			
11	997	0.07	99.04			
12	2,671	0.19	99.24			
13	741	0.05	99.29			
14	1,926	0.14	99.43			
15	579	0.04	99.47			
16	1,273	0.09	99.57			
17	431	0.03	99.60			
18	942	0.07	99.67			
19	362	0.03	99.69			
20	672	0.05	99.74			
21	284	0.02	99.76			
22	475	0.03	99.80			
23	216	0.02	99.81			
24	355	0.03	99.84			
25	179	0.01	99.85			
26	289	0.02	99.87			
27	175	0.01	99.89			
28	209	0.02	99.90			
29	124	0.01	99.91			
30	139	0.01	99.92			
31	90	0.01	99.93			
32	124	0.01	99.94			
33	78	0.01	99.94			
34	85	0.01	99.95			
35	63	0.00	99.95			
36	71	0.01	99.96			
37	60	0.00	99.96			
38	61	0.00	99.97			
39	55	0.00	99.97			
40	47	0.00	99.97			
41	31	0.00	99.98			
42	29	0.00	99.98			
43	14	0.00	99.98			
44	21	0.00	99.98			
45	17	0.00	99.98			
46	24	0.00	99.98			
47	12	0.00	99.98			
48	18	0.00	99.99			
49	13	0.00	99.99			
50	19	0.00	99.99			
51	3	0.00	99.99			
52	8	0.00	99.99			
53	8	0.00	99.99			
54	10	0.00	99.99			
55	5	0.00	99.99			
56	1	20.00	99.99			
57	5	0.00	99.99			

B Tables

TABLE 8: DIFFERENCE-IN-DIFFERENCES-ESTIMATES: ARABS VS. GERMANS, PROBIT REGRESSION

Dependent Variable = Exit into employment Variable	Nationality concept 1		Nationality concept 2	
	narrow	broad	narrow	broad
Age (years)	-.0084253 (0.000)	-.0083715 (0.000)	-.0084605 (0.000)	-.0083557 (0.000)
Male (1=yes)	-.0243986 (0.000)	-.0170842 (0.003)	-.0250816 (0.000)	-.0153435 (0.008)
Disabled 30% (1=yes)	-.3121542 (0.000)	-.3124879 (0.000)	-.3060346 (0.000)	-.3056609 (0.000)
Disabled 50% (1=yes)	-.4741382 (0.000)	-.4738802 (0.000)	-.4772588 (0.000)	-.4731684 (0.000)
Unemployment duration (days)	-.0027598 (0.000)	-.0027449 (0.000)	-.0027532 (0.000)	-.002732 (0.000)
Higher secondary schooling (1=yes)	-.0684552 (0.000)	-.0665639 (0.000)	-.0701289 (0.000)	-.0660582 (0.000)
No post-school training (1=yes)	-.1098315 (0.000)	-.1046725 (0.000)	-.1087595 (0.000)	-.1022653 (0.000)
Academic Training (1=yes)	.0418082 (0.005)	.0407939 (0.006)	.0426963 (0.004)	.0387481 (0.009)
Arab (1=yes)	-.1101522 (0.012)	-.2237069 (0.000)	-.1012577 (0.004)	-.209001 (0.000)
Post 9/11 (1=Yes)	.0988186 (0.000)	.0982592 (0.000)	.0997116 (0.000)	.0989274 (0.000)
Interaction Arab*Post 9/11	-.063031 (0.255)	-.0202092 (0.423)	-.0568243 (0.192)	.0040293 (0.851)
Constant	.3723822 (0.000)	.3673885 (0.000)	.370345 (0.000)	.3656554 (0.000)
Regional Dummies (11)	(included)	(included)	(included)	(included)
Occupation Dummies (32)	(included)	(included)	(included)	(included)
No. of Observations	377325	389791	368841	385946
Pseudo- R^2	0.1176	0.1179	0.1174	0.1175
Sig. (Model)	0.0000	0.0000	0.0000	0.0000

Coefficients, p-values calculated with robust standard-errors in parentheses. See text for explanations and variable definitions.

TABLE 9: DIFFERENCE-IN-DIFFERENCES-ESTIMATES: ARABS VS. CENTRAL EUROPEANS, PROBIT REGRESSION

Dependent Variable = Exit into employment Variable	Nationality concept 1		Nationality concept 2	
	narrow	broad	narrow	broad
Age (years)	-.004164 (0.043)	-.0089556 (0.000)	-0.0042544 (0.007)	-0.0078265 (0.000)
Male (1=yes)	.1000392 (0.058)	.1923089 (0.000)	0.1122158 (0.007)	0.1725184 (0.000)
Disabled 30% (1=yes)	.2201833 (0.569)	-.1959514 (0.384)	-0.0524384 (0.851)	-0.2292275 (0.172)
Disabled 50% (1=yes)	-.1235562 (0.431)	-.363009 (0.000)	-0.1388719 (0.196)	-0.2761011 (0.000)
Unemployment duration (days)	-.002413 (0.000)	-.002341 (0.000)	-0.0024513 (0.000)	-.0023491 (0.000)
Higher secondary schooling (1=yes)	.0257018 (0.706)	.0803175 (0.134)	-0.0345097 (0.550)	.0434894 (0.329)
No post-school training (1=yes)	.1407412 (0.001)	.0922054 (0.000)	0.0716866 (0.000)	.0581584 (0.003)
Academic Training (1=yes)	-.2425005 (0.029)	-.2657968 (0.006)	-.2166386 (0.019)	-.2493097 (0.001)
Arab (1=yes)	-.1585167 (0.024)	-.2899607 (0.000)	-.2739149 (0.004)	-.3990155 (0.000)
Post 9/11 (1=Yes)	.0286626 (0.638)	.0248081 (0.682)	.0300796 (0.530)	.0226198 (0.638)
Interaction Arab*Post 9/11	-.0123022 (0.881)	.0343179 (0.599)	-.0011783 (0.985)	.0666333 (0.203)
Constant	-.3783432 (0.036)	.0989423 (0.329)	-.0663648 (0.636)	.2872662 (0.000)
Regional Dummies (11)	(included)	(included)	(included)	(included)
Occupation Dummies (32)	(included)	(included)	(included)	(included)
No. of Observations	5197	17663	8338	25369
Pseudo- R^2	0.1137	0.1061	0.1276	0.1103
Sig. (Model)	0.0000	0.0000	0.0000	0.0000

Coefficients, p-values calculated with robust standard-errors in parentheses. See text for explanations and variable definitions.

TABLE 10: DIFFERENCE-IN-DIFFERENCES-ESTIMATES: ARABS VS. SOUTH EUROPEANS, PROBIT REGRESSION

Dependent Variable = Exit into employment Variable	Nationality concept 1		Nationality concept 2	
	narrow	broad	narrow	broad
Age (years)	-.0028776 (0.041)	-.0070499 (0.000)	-.0030907 (0.011)	-.0065953 (0.000)
Male (1=yes)	.0641705 (0.079)	.1502888 (0.000)	.089997 (0.004)	.1525856 (0.000)
Disabled 30% (1=yes)	-.9702856 (0.056)	-.5561056 (0.026)	-1.042206 (0.033)	-.4507992 (0.017)
Disabled 50% (1=yes)	-.8271594 (0.000)	-.596617 (0.000)	-.7082709 (0.000)	-.4965218 (0.000)
Unemployment duration (days)	-.0022395 (0.000)	-.0022759 (0.000)	-.002178 (0.000)	-.0022465 (0.000)
Higher secondary schooling (1=yes)	.0935635 (0.172)	.1246829 (0.020)	.0053378 (0.928)	.07506 (0.095)
No post-school training (1=yes)	.1186555 (0.000)	.098854 (0.000)	.0723155 (0.005)	.0651346 (0.000)
Academic Training (1=yes)	-.0266511 (0.827)	-.0532304 (0.610)	-.0150898 (0.880)	-.0779678 (0.346)
Arab (1=yes)	.0446635 (0.399)	-.0777085 (0.027)	.0295209 (0.499)	-.0793732 (0.011)
Post 9/11 (1=Yes)	.0867898 (0.015)	.0929132 (0.009)	.1014725 (0.001)	.1024361 (0.001)
Interaction Arab*Post 9/11	-.0772431 (0.232)	-.0400036 (0.352)	-.0914892 (0.085)	-.0200533 (0.599)
Constant	-.4351977 (0.001)	-.133023 (0.110)	-.3501376 (0.001)	-.0820444 (0.238)
Regional Dummies (11)	(included)	(included)	(included)	(included)
Occupation Dummies (32)	(included)	(included)	(included)	(included)
No. of Observations	9955	22430	13399	30317
Pseudo- R^2	0.0925	0.0974	0.0927	0.1175
Sig. (Model)	0.0000	0.0000	0.0000	0.0960

Coefficients, p-values calculated with robust standard-errors in parentheses. See text for explanations and variable definitions.

TABLE 11: DIFFERENCE-IN-DIFFERENCES-ESTIMATES: ARABS VS. EAST EUROPEANS, PROBIT REGRESSION

Dependent Variable = Exit into employment Variable	Nationality concept 1		Nationality concept 2	
	narrow	broad	narrow	broad
Age (years)	.0006039 (0.763)	-.0072595 (0.000)	-.0028652 (0.039)	-.0066863 (0.000)
Male (1=yes)	.1547967 (0.002)	.2046643 (0.000)	.162307 (0.000)	.1937545 (0.000)
Disabled 30% (1=yes)	(dropped)	-.3073454 (0.256)	-.5914793 (0.072)	-.3881332 (0.031)
Disabled 50% (1=yes)	-.193186 (0.274)	-.410188 (0.000)	-.3270469 (0.004)	-.3454479 (0.000)
Unemployment duration (days)	-.002524 (0.000)	-.0023808 (0.000)	-.0024323 (0.000)	-.0023688 (0.000)
Higher secondary schooling (1=yes)	-.0441043 (0.482)	.0263969 (0.600)	-.0657685 (0.184)	.0046741 (0.908)
No post-school training (1=yes)	.1478391 (0.000)	.098616 (0.000)	.0774729 (0.006)	.0615499 (0.001)
Academic Training (1=yes)	-.1630378 (0.091)	-.183683 (0.032)	.0247809 (0.738)	-.0578388 (0.373)
Arab (1=yes)	-.225052 (0.001)	-.3571821 (0.000)	-.1986276 (0.000)	-.3288664 (0.000)
Post 9/11 (1=Yes)	-.0526942 (0.339)	-.0652379 (0.233)	.005291 (0.885)	-.0058768 (0.872)
Interaction Arab*Post 9/11	.066737 (0.387)	.1254853 (0.036)	.0244249 (0.664)	.0956851 (0.022)
Constant	-.2741347 (0.071)	.1163786 (0.213)	.0315953 (0.770)	.2077696 (0.003)
Regional Dummies (11)	(included)	(included)	(included)	(included)
Occupation Dummies (32)	(included)	(included)	(included)	(included)
No. of Observations	5988	18458	11384	28388
Pseudo- R^2	0.1139	0.1058	0.1159	0.1071
Sig. (Model)	0.0000	0.0000	0.0000	0.0000

Coefficients, p-values calculated with robust standard-errors in parentheses. See text for explanations and variable definitions.

TABLE 12: DIFFERENCE-IN-DIFFERENCES-ESTIMATES: ARABS VS. SOUTH-EAST EUROPEANS, PROBIT REGRESSION

Dependent Variable = Exit into employment Variable	Nationality concept 1		Nationality concept 2	
	narrow	broad	narrow	broad
Age (years)	-.0067578 (0.000)	-.0089353 (0.000)	-.0081165 (0.000)	-.0088257 (0.000)
Male (1=yes)	.1287258 (0.003)	.1901351 (0.000)	.1323039 (0.000)	.1819928 (0.000)
Disabled 30% (1=yes)	.2970494 (0.340)	-.0692595 (0.737)	-.1638511 (0.536)	-.2386762 (0.150)
Disabled 50% (1=yes)	-.6865043 (0.000)	-.5366974 (0.000)	-.4546826 (0.000)	-.4090453 (0.000)
Unemployment duration (days)	-.0024052 (0.000)	-.0023547 (0.000)	-.0023085 (0.000)	-.0023093 (0.000)
Higher secondary schooling (1=yes)	.2145171 (0.002)	.1861752 (0.001)	.0730765 (0.210)	.0930962 (0.039)
No post-school training (1=yes)	.1325172 (0.000)	.1051586 (0.000)	.0800368 (0.002)	.0747571 (0.000)
Academic Training (1=yes)	-.3077265 (0.037)	-.2500064 (0.039)	-.0822161 (0.461)	-.1189525 (0.186)
Arab (1=yes)	-.0426397 (0.438)	-.1869798 (0.000)	-.0469251 (0.295)	-.1906975 (0.000)
Post 9/11 (1=Yes)	.0509357 (0.172)	.0503352 (0.174)	.0737853 (0.022)	.0658192 (0.043)
Interaction Arab*Post 9/11	-.0358361 (0.587)	.0114755 (0.796)	-.0525679 (0.327)	.0238719 (0.535)
Constant	-.3470373 (0.011)	-.0400271 (0.640)	-.1971808 (0.073)	.0494407 (0.485)
Regional Dummies (11)	(included)	(included)	(included)	(included)
Occupation Dummies (32)	(included)	(included)	(included)	(included)
No. of Observations	9082	21548	12986	29796
Pseudo- R^2	0.1068	0.1071	0.1063	0.1043
Sig. (Model)	0.0000	0.0000	0.0000	0.0000

Coefficients, p-values calculated with robust standard-errors in parentheses. See text for explanations and variable definitions.

TABLE 13: DIFFERENCE-IN-DIFFERENCES-ESTIMATES: ARABS VS. SOUTH AMERICANS, PROBIT REGRESSION

Dependent Variable = Exit into employment Variable	Nationality concept 1		Nationality concept 2	
	narrow	broad	narrow	broad
Age (years)	.0048076 (0.103)	-.007966 (0.000)	-.0008486 (0.693)	-.0074929 (0.000)
Male (1=yes)	.1153113 (0.113)	.2088441 (0.000)	.2049062 (0.000)	.2093702 (0.008)
Disabled 30% (1=yes)	(dropped)	-.3574509 (0.221)	(dropped)	-.3111215 (0.141)
Disabled 50% (1=yes)	-.3518627 (0.176)	-.4378207 (0.000)	-.2853302 (0.095)	-.3366293 (0.000)
Unemployment duration (days)	-.0020883 (0.000)	-.0022554 (0.000)	-.0020363 (0.000)	-.0022257 (0.000)
Higher secondary schooling (1=yes)	.1800687 (0.040)	.1882315 (0.002)	.0640619 (0.368)	.1135653 (0.023)
No post-school training (1=yes)	.2220273 (0.000)	.1150956 (0.000)	.1417823 (0.000)	.0789632 (0.000)
Academic Training (1=yes)	-.5012939 (0.004)	-.3865894 (0.004)	-.1684277 (0.181)	-.2004971 (0.036)
Arab (1=yes)	.2465087 (0.092)	.0352 (0.798)	-.123881 (0.250)	-.2372008 (0.019)
Post 9/11 (1=Yes)	.2606318 (0.115)	.2085986 (0.200)	.0275486 (0.819)	.0118806 (0.920)
Interaction Arab*Post 9/11	-.2747318 (0.114)	-.1537532 (0.351)	-.0252241 (0.844)	.0716961 (0.552)
Constant	-1.265289 (0.000)	-.2768408 (0.095)	-.6210576 (0.002)	.0717586 (0.564)
Regional Dummies (11)	(included)	(included)	(included)	(included)
Occupation Dummies (32)	(included)	(included)	(included)	(included)
No. of Observations	3373	15835	5619	22730
Pseudo- R^2	0.1053	0.0995	0.1067	0.0983
Sig. (Model)	0.0000	0.0000	0.0000	0.0000

Coefficients, p-values calculated with robust standard-errors in parentheses. See text for explanations and variable definitions.

TABLE 14: DIFFERENCE-IN-DIFFERENCES-ESTIMATES: ARABS VS. AFRICANS, PROBIT REGRESSION

Dependent Variable = Exit into employment Variable	Nationality concept 1		Nationality concept 2	
	narrow	broad	narrow	broad
Age (years)	.0083494 (0.002)	-.0069958 (0.000)	.0021719 (0.282)	-.0067725 (0.000)
Male (1=yes)	.108949 (0.093)	.2090616 (0.000)	.167265 (0.001)	.2001379 (0.000)
Disabled 30% (1=yes)	(dropped) (0.000)	-.3881262 (0.186)	(dropped) -	-.32513 (0.125)
Disabled 50% (1=yes)	-.4171082 (0.045)	-.4472925 (0.000)	-.2962135 (0.033)	-.3383029 (0.000)
Unemployment duration (days)	-.0023963 (0.000)	-.0023242 (0.000)	-.0022337 (0.000)	-.0022752 (0.000)
Higher secondary schooling (1=yes)	.0353085 (0.677)	.0914122 0.131	.0215849 (0.754)	.0893174 (0.069)
No post-school training (1=yes)	.1799494 (0.000)	.1103697 (0.000)	.1283977 (0.000)	.0802572 (0.000)
Academic Training (1=yes)	-.1596119 (0.352)	-.1821415 (0.173)	.0017634 (0.989)	-.1123676 (0.239)
Arab (1=yes)	-.156152 (0.052)	-.29918 (0.000)	-.1465818 (0.028)	-.2437222 (0.000)
Post 9/11 (1=Yes)	-.0639466 (0.444)	-.0639876 (0.441)	-.0335807 (0.633)	-.0119869 (0.864)
Interaction Arab*Post 9/11	.0746814 (0.454)	.1205997 (0.163)	.0456433 (0.579)	.0965171 (0.186)
Constant	-.7439443 (0.001)	.0660505 (0.561)	-.5355136 (0.002)	.0730931 (0.435)
Regional Dummies (11)	(included)	(included)	(included)	(included)
Occupation Dummies (32)	(included)	(included)	(included)	(included)
No. of Observations	4151	16622	6619	23731
Pseudo- R^2	0.1077	0.1024	0.1083	0.1002
Sig. (Model)	0.0000	0.0000	0.0000	0.0000

Coefficients, p-values calculated with robust standard-errors in parentheses. See text for explanations and variable definitions.

TABLE 15: EAST-WEST-DIFFERENCES, ARABS VS. GERMANS, PROBIT REGRESSION

Dependent Variable = Exit into employment	East Germany				West Germany			
	Nationality concept 1		Nationality concept 2		Nationality concept 1		Nationality concept 2	
	narrow	broad	narrow	broad				
Age (years)	-.0120449 (0.000)	-.0119526 (0.000)	-.0120884 (0.000)	-.0119994 (0.000)	-.0066339 (0.000)	-.0067401 (0.000)	-.0066273 (0.000)	-.0066854 (0.000)
Male (1=yes)	.0300336 (0.002)	.029626 (0.003)	.0296654 (0.003)	.0292531 (0.003)	-.0591467 (0.000)	-.0463536 (0.000)	-.0607364 (0.000)	-.0436578 (0.000)
Disabled 30% (1=yes)	-.2048299 (0.000)	-.2054174 (0.000)	-.2040221 (0.000)	-.2044538 (0.000)	-.4060606 (0.000)	-.4040913 (0.000)	-.3974224 (0.000)	-.3922252 (0.000)
Disabled 50% (1=yes)	-.4756121 (0.000)	-.4726301 (0.000)	-.4736367 (0.000)	-.470163 (0.000)	-.4765452 (0.000)	-.4774759 (0.000)	-.4820637 (0.000)	-.4775716 (0.000)
Unemployment duration (days)	-.0026292 (0.000)	-.0026309 (0.000)	-.0026273 (0.000)	-.002629 (0.000)	-.0028571 (0.000)	-.0028257 (0.000)	-.0028495 (0.000)	-.0028054 (0.000)
Higher secondary schooling (1=yes)	-.1654296 (0.000)	-.1640907 (0.000)	-.166876 (0.000)	-.1640199 (0.000)	-.0217629 (0.072)	-.0196578 (0.102)	-.0229533 (0.061)	-.0187309 (0.121)
No post-school training (1=yes)	-.2387326 (0.000)	-.2363012 (0.000)	-.2394737 (0.000)	-.2362188 (0.000)	-.0474929 (0.000)	-.0437603 (0.000)	-.0440235 (0.000)	-.0397764 (0.000)
Academic Training (1=yes)	.1172021 (0.000)	.117382 (0.000)	.1177064 (0.000)	.1162709 (0.000)	.0167258 (0.368)	.014673 (0.427)	.0175078 (0.000)	.0119083 (0.522)
Arab (1=yes)	-.1438682 (0.356)	-.355461 (0.000)	-.0492361 (0.685)	-.2539706 (0.000)	-.1032929 (0.024)	-.2165003 (0.000)	-.1024046 (0.006)	-.2085653 (0.000)
Post 9/11 (1=Yes)	.1302373 (0.000)	.1302944 (0.000)	.1308486 (0.000)	.1308652 (0.000)	.0797416 (0.000)	.0786513 (0.000)	.0802198 (0.000)	.078747 (0.000)
Interaction Arab*Post 9/11	.1870751 (0.342)	.1304206 (0.179)	.040337 (0.793)	.0692564 (0.362)	-.060295 (0.296)	-.0092517 (0.726)	-.0446521 (0.333)	.0196698 (0.384)
Constant	.6089861 (0.000)	.5982129 (0.000)	.6082765 (0.000)	.5985 (0.000)	.2727872 (0.000)	.2749926 (0.000)	.2658401 (0.000)	.2699489 (0.000)
Regional Dummies	(3 included)	(3 included)	(3 included)	(3 included)	(6 included)	(6 included)	(6 included)	(6 included)
Occupation Dummies (32)	(included)	(included)	(included)	(included)	(included)	(included)	(included)	(included)
No. of Observations	149090	150025	147787	149250	228235	239766	221054	236696
Pseudo- R^2	0.1305	0.1309	0.1304	0.1309	0.1115	0.1122	0.1110	0.1114
Sig. (Model)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

TABLE 16: DIFFERENCE-IN-DIFFERENCES-ESTIMATES: ARABS VS. GERMANS, MEN ONLY, PROBIT REGRESSION

Dependent Variable = Exit into employment Variable	Nationality concept 1		Nationality concept 2	
	narrow	broad	narrow	broad
Age (years)	-.0067815 (0.000)	-.0067519 (0.000)	-.0067133 (0.000)	-.006644 (0.000)
Disabled 30% (1=yes)	-.3107498 (0.000)	-.3114969 (0.000)	-.3080738 (0.000)	-.3073083 (0.000)
Disabled 50% (1=yes)	-.473164 (0.000)	-.4741996 (0.000)	-.4765421 (0.000)	-.4725125 (0.000)
Unemployment duration (days)	-.0027454 (0.000)	-.002727 (0.000)	-.0027409 (0.000)	-.0027169 (0.000)
Higher secondary schooling (1=yes)	-.1115519 (0.000)	-.1109366 (0.000)	-.1117364 (0.000)	-.1090002 (0.000)
No post-school training (1=yes)	-.1247243 (0.000)	-.1179297 (0.000)	-.1231723 (0.000)	-.1151352 (0.000)
Academic Training (1=yes)	.0577443 (0.005)	.0577123 (0.000)	.0583478 (0.000)	.0546378 (0.008)
Arab (1=yes)	-.0782519 (0.100)	-.1490908 (0.000)	-.0636556 (0.100)	-.13485 (0.000)
Post 9/11 (1=Yes)	.0867844 (0.000)	.0862298 (0.000)	.0876776 (0.005)	.0870229 (0.000)
Interaction Arab*Post 9/11	-.0444255 (0.460)	-.0255927 (0.377)	-.033517 (0.488)	.0045567 (0.854)
Constant	.3089901 (0.000)	.3153376 (0.000)	.2952143 (0.000)	.3080499 (0.000)
Regional Dummies (11)	(included)	(included)	(included)	(included)
Occupation Dummies (32)	(included)	(included)	(included)	(included)
No. of Observations	230321	239314	225259	237248
Pseudo- R^2	0.1128	0.1129	0.1122	0.1122
Sig. (Model)	0.0000	0.0000	0.0000	0.0000

Coefficients, p-values calculated with robust standard-errors in parentheses. See text for explanations and variable definitions.

TABLE 17: DIFFERENCE-IN-DIFFERENCES-ESTIMATES: ARABS VS. GERMANS, MEN UNDER 35 YEARS OF AGE ONLY, PROBIT REGRESSION

Dependent Variable = Exit into employment Variable	Nationality concept 1		Nationality concept 2	
	narrow	broad	narrow	broad
Age (years)	.0336884 (0.000)	.0332375 (0.000)	.0343408 (0.000)	.0334436 (0.000)
Disabled 30% (1=yes)	-.0357475 (0.660)	-.0355259 (0.659)	-.0082751 (0.920)	-.0310851 (0.701)
Disabled 50% (1=yes)	-.3096844 (0.000)	-.3221038 (0.000)	-.3301904 (0.000)	-.3296402 (0.000)
Unemployment duration (days)	-.0027016 (0.000)	-.0026841 (0.000)	-.0027018 (0.000)	-.0026768 (0.000)
Higher secondary schooling (1=yes)	-.1303833 (0.000)	-.1294548 (0.000)	-.1293304 (0.000)	-.1277528 (0.000)
No post-school training (1=yes)	.049431 (0.000)	.0506671 (0.000)	.0518579 (0.000)	.0513178 (0.000)
Academic Training (1=yes)	.1659246 (0.000)	.1616187 (0.000)	.1612721 (0.000)	.1528086 (0.000)
Arab (1=yes)	-.1452294 (0.016)	-.1549461 (0.000)	-.1784725 (0.000)	-.1633904 (0.000)
Post 9/11 (1=Yes)	.0485992 (0.000)	.0479556 (0.000)	.0486717 (0.000)	.0480173 (0.000)
Interaction Arab*Post 9/11	-.1133497 (0.138)	-.0217101 (0.534)	-.0270522 (0.674)	.0068511 (0.823)
Constant	-.8832763 (0.000)	-.8390625 (0.000)	-.9131683 (0.000)	-.8403905 (0.000)
Regional Dummies (11)	(included)	(included)	(included)	(included)
Occupation Dummies (32)	(included)	(included)	(included)	(included)
No. of Observations	115640	121515	113112	120828
Pseudo- R^2	0.0841	0.0847	0.0842	0.0847
Sig. (Model)	0.0000	0.0000	0.0000	0.0000

Coefficients, p-values calculated with robust standard-errors in parentheses. See text for explanations and variable definitions.

TABLE 18: DIFFERENCE-IN-DIFFERENCES-ESTIMATES: ARABS VS. GERMANS, 9/11
 +/- 180 DAYS, PROBIT REGRESSION

Dependent Variable = Exit into employment Variable	Nationality concept 1		Nationality concept 2	
	narrow	broad	narrow	broad
Age (years)	-.00777 (0.000)	-.0076635 (0.000)	-.0709242 (0.000)	-.0077779 (0.000)
Male (1=yes)	-.0713187 (0.000)	-.0572794 (0.000)	-.0709242 (0.000)	-.0532087 (0.001)
Disabled 30% (1=yes)	-.4016193 (0.000)	-.4058264 (0.000)	-.3924498 (0.000)	-.3968105 (0.000)
Disabled 50% (1=yes)	-.4757005 (0.000)	-.4795325 (0.000)	-.4685177 (0.000)	-.4747567 (0.000)
Unemployment duration (days)	-.0121556 (0.000)	-.0121058 (0.000)	-.0122026 (0.000)	-.0121247 (0.000)
Higher secondary schooling (1=yes)	-.0466205 (0.082)	-.0438431 (0.100)	-.0484179 (0.074)	-.0416879 (0.120)
No post-school training (1=yes)	-.1312166 (0.000)	-.1263878 (0.000)	-.1316159 (0.000)	-.1244752 (0.000)
Academic Training (1=yes)	.1080753 (0.008)	.1059727 (0.009)	.1137165 (0.005)	.1116572 (0.006)
Arab (1=yes)	-.1772682 (0.143)	-.2225008 (0.000)	-.1916928 (0.042)	-.2353553 (0.000)
Post 9/11 (1=Yes)	.0277309 (0.033)	.0269772 (0.037)	.027549 (0.036)	.0268672 (0.040)
Interaction Arab*Post 9/11	.0528074 (0.746)	.0945181 (0.201)	.0632985 (0.621)	.0886659 (0.159)
Constant	.4873811 (0.000)	.4892431 (0.000)	.5102151 (0.000)	.5215871 (0.000)
Regional Dummies (11)	(included)	(included)	(included)	(included)
Occupation Dummies (32)	(included)	(included)	(included)	(included)
No. of Observations	83212	85873	81479	85143
Pseudo- R^2	0.3059	0.3058	0.3070	0.3070
Sig. (Model)	0.0000	0.0000	0.0000	0.0000

Coefficients, p-values calculated with robust standard-errors in parentheses. See text for explanations and variable definitions.

TABLE 19: DIFFERENCE-IN-DIFFERENCES-ESTIMATES: ARABS VS. GERMANS, 9/11
 +/- 365 DAYS, PROBIT REGRESSION

Dependent Variable = Exit into employment Variable	Nationality concept 1		Nationality concept 2	
	narrow	broad	narrow	broad
Age (years)	-.0066282 (0.000)	-.00655 (0.000)	-.0066879 (0.000)	-.0065568 (0.000)
Male (1=yes)	.0060917 (0.522)	.0150079 (0.110)	.007823 (0.417)	.0180995 (0.055)
Disabled 30% (1=yes)	-.4070554 (0.000)	-.4147203 (0.000)	-.3938586 (0.000)	-.4048411 (0.000)
Disabled 50% (1=yes)	-.5160595 (0.000)	-.5142032 (0.000)	-.5222812 (0.000)	-.5185555 (0.000)
Unemployment duration (days)	-.0052755 (0.000)	-.0052556 (0.000)	-.0052688 (0.000)	-.0052391 (0.000)
Higher secondary schooling (1=yes)	-.0828678 (0.000)	-.0813386 (0.000)	-.0862104 (0.000)	-.0810604 (0.000)
No post-school training (1=yes)	-.1271313 (0.000)	-.1214887 (0.000)	-.1282572 (0.000)	-.120473 (0.000)
Academic Training (1=yes)	.0886196 (0.000)	.0879759 (0.000)	.0908466 (0.000)	.0867894 (0.000)
Arab (1=yes)	-.176365 (0.000)	-.2579303 (0.000)	-.1392863 (0.005)	-.2366044 (0.000)
Post 9/11 (1=Yes)	.0294326 (0.000)	.0293221 (0.000)	.0298535 (0.000)	.0296676 (0.000)
Interaction Arab*Post 9/11	-.0391454 (0.659)	.0202493 (0.616)	-.0460141 (0.515)	.0381048 (0.268)
Constant	.3831798 (0.000)	.3737445 (0.000)	.3929199 (0.000)	.3841518 (0.000)
Regional Dummies (11)	(included)	(included)	(included)	(included)
Occupation Dummies (32)	(included)	(included)	(included)	(included)
No. of Observations	171569	177229	168064	175720
Pseudo- R^2	0.2099	0.2101	0.2098	0.2099
Sig. (Model)	0.0000	0.0000	0.0000	0.0000

Coefficients, p-values calculated with robust standard-errors in parentheses. See text for explanations and variable definitions.

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