Shifting attitudes and the labor market of minorities:
Swedish experiences after 9-11

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by

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Abstract

Several studies suggest that the terrorist attacks in the US on September 11, 2001, caused at least a temporary change of attitude toward certain minorities in Sweden. We study unemployment exit around 9-11 using detailed data on the entire Swedish working-age population to investigate whether this change in attitudes also affected the labor market situation of these minorities. Contrary to what to expect from many theories of labor market discrimination, the time pattern of exits and entries for different ethnic groups, as well as difference-in-differences analyses, show no sign of increased discrimination towards these minorities.

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Table of contents

1   Introduction

The link between attitudes toward a particular minority group and the group's labor market position is at the core of many theories of labor market discrimination.[1] We investigate this connection empirically by using the terrorist attacks in the US on September 11, 2001 (9-11), as a source of exogenous variation in attitudes. Following the attacks, there were numerous reports from many countries on hardening attitudes and hostilities, primarily toward people perceived to be Muslims. There is evidence that the shocking events also changed attitudes toward certain minorities in Sweden. Official reports, surveys among potentially exposed groups, and pre-post attitude surveys within the general public all point in this direction.

Using data on the entire Swedish working-age population, we study whether the unexpected events of 9-11 had a more detrimental impact on the labor market opportunities of immigrant groups that were likely exposed to increasingly negative attitudes. We thus study whether a *change* in attitudes leads to changes in the labor market. In this sense, the unexpected events of 9-11 provide a rare opportunity to test the connection proposed by the above-mentioned discrimination theories.

The data from the unemployment registers include exact dates for entry into and exit from unemployment, allowing us to study changes just around 9-11 and whether these changes differ across ethnic groups. We focus on the probability of leaving unemployment for employment, but also investigate other possible channels for discrimination. Naturally, this study also sheds light on a broader question concerning the labor market consequences of such a momentous event as 9-11.

Our main finding is that despite the indications on an attitude shift, the data give no support for increased labor market discrimination after 9-11. This goes both for exit from and entry into unemployment, and holds whether we study the time pattern just around the terrorist attacks or perform a difference-in-differences analysis comparing the time after 9-11 with the corresponding time one year before. In the light of the evidence on an attitude shift, the many reports on hardened conditions and feelings of increased negative treatment among some groups, our results bring some interesting information. One inter-

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[1] Such theories were initially formalized in Becker (1957).

pretation is that ethnic labor market discrimination could be based on uncertainty about factors that were not altered by the 9-11 events (such as language skills); i.e. classic statistical discrimination.

The rest of the paper is outlined as follows. Section 2 presents the evidence on an attitude change after 9-11. Section 3 gives some facts about immigration to Sweden and previous studies of discrimination in the Swedish labor market. Section 4 discusses implications from economic theory regarding the expected impact of 9-11, and which groups that would be subject to discrimination under different hypotheses. Section 5 presents the data from the IFAU database and the way we retrieve the samples used in the empirical analysis of section 6. Section 7 concludes.

2   The evidence on an attitude change

How strong is the evidence that 9-11 had an impact on attitudes and behavior concerning particular minorities in Sweden? It is of course not clear that the US experience of increased violence and aggressions primarily toward (people perceived to be) Arabs or Muslims (Arab American Institute 2002, Human Rights Watch 2002)[2] is valid also for Sweden, or any other European country for that matter. Different sources of information, however, suggest that this is the case.

The perhaps strongest evidence on at least a temporary attitude change comes from survey results presented in FSI (2001). The study is based on a continual survey on a large variety of topics, to which attitudes concerning immigrants belong. *Figure 1* below shows the development of answers to the question: "What do you think about immigrants?" It seems that 9-11 broke a positive trend, causing the fraction who were "positive towards immigrants" to drop from 51 percent in questionnaires from the period June–August 2001, to

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[2] The FBI reported a seventeen-fold increase in anti-Muslim crimes in 2001, which is in line with figures from Los Angeles county and Chicago (Human Rights Watch 2002). In the first year after the attacks, the US Equal Employment Opportunity Commission (EEOC) received more than twice as many charges based on "Religion – Muslim" compared to the year preceding 9-11. The EEOC has also found 654 charges on employment discrimination with an alleged connection to 9-11. The figure includes "charges alleging discrimination related to the events of September 11, 2001, by individuals who are—or are perceived to be—Muslim, Arab, Afghani, Middle Eastern or South Asian or individuals alleging retaliation related to the events of September 11, 2001" (EEOC 2003). It is worth noting that out of the 449 cases that had been resolved by October 1, 2002, 288 were "closed with no cause findings".

33 percent in answers given from September 11 through the end of the month. Similarly, a larger share stated a directly negative attitude to immigrants just after the attacks compared to right before (FSI 2001).
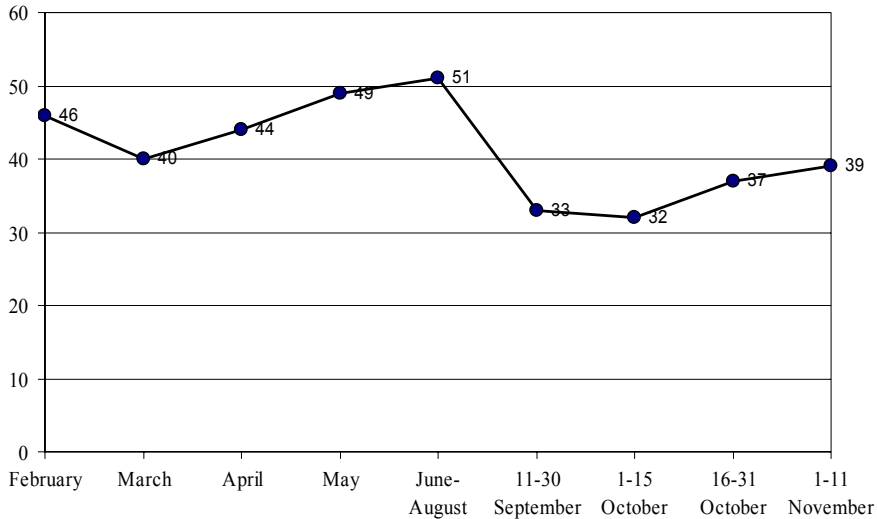
Figure 1 Fraction of respondents who were "positive toward immigrants" 2001.

Notes: The graph is a replication of Figure 5 in FSI (2001).

A somewhat longer time perspective is found in FSI (2004), which includes the answers to the above-mentioned question also during 2000 and 2002. On an annual basis, 48 percent said that they were positive toward immigrants in 2000; in 2002 the figure was 39 percent.[3] Hence, the change in attitudes lasted well into 2002. The same development was found concerning attitudes toward reception of new immigrants and refugees: in 2000, 47 percent answered that Sweden should accept fewer refugees; in 2002 this figure had climbed to 58 percent. Of course, there are other possible explanations to the annual differences, but the results fit the idea of a change induced by 9-11.

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[3] A similar development occurred as regards negative attitudes. During the year 2000, 17 percent stated negative attitudes, which increased to 20 percent during 2001 (on an annual basis) and then further to 26 percent during 2002.

A clear indication that some groups felt more exposed after 9-11 is given in Larsson (2003). The results from a questionnaire distributed among practicing Muslims in the first few months after 9-11 strongly suggest increased feelings of exposure. More than 90 percent of the respondents believe that Muslims have become target of increased discrimination and negative special treatment. This belief is more common among women than among men, and most prevalent in groups originating in the Middle East or the African Horn. About two-thirds of the respondents report that they have themselves more frequently become the victims of threats or discrimination, in most cases verbal abuse. [4] Interviews with Muslim leaders and central figures performed during the fall of 2002 confirm the results from the survey. Larsson (2003) finds that both practicing and secularized Muslims were hurt by 9-11, especially people fitting the general image of what Muslims look like, such as women wearing veils or men who appear to come from the Arab world. It seems that an appearance connected with Islam or perceived similarities to the alleged perpetrators of the terrorist attacks were important determinants of who became exposed to harassment or abuse.

Official reports also claim that some minorities were subjected to increased abuse after 9-11. With a general European perspective, The European Centre on Racism and Xenophobia (EUMC) concluded that "Islamic communities and other vulnerable groups have become targets of increased hostility since 11 September" (Allen & Nielsen 2002, p 5). The level of physical violence is said to have been relatively low, whereas verbal abuse and harassment were more common. As in Larsson (2003), the reports argue that "visual identifiers" were an important determinant as to why some groups became targets of aggression, i.e. primarily people who look "Muslim" rather than those who actually are Muslims. According to a separate report (EUMC 2002) Sweden shared the experiences of other countries concerning the situation in the time after the terrorist attacks.

Other Swedish public reports argue that the media coverage of the attacks and the ensuing war on terrorism may have contributed to negative opinions

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[4] As Larsson (2003) discusses, it is not certain that the answers give a representative picture of the general experiences of Swedish Muslims after 9-11. First, the questionnaire was distributed via Islamic organisations mostly in Gothenburg. Thus, the survey had regional boundaries, and reached only a particular part of the population of interest. Second, only 176 out of 450 distributed surveys were answered. Still, considering how widespread the feelings of increased exposure are, they are probably not fully limited to the responding group.

regarding Muslims (see e.g. Ghersetti & Levin 2002). However, the media's tendency to associate Islam and Muslims with war and violence may not be unique to the post-9-11 period; see e.g. Hvitfelt (1998).

Taken together, there are strong suggestions on an attitude change following 9-11. Different sources of information all point in the same direction. Official reports document increased harassment, people belonging to potentially discriminated groups say that they have become subjected to increasingly negative attitudes, and surveys in the overall population indicate a shift in attitude. Which consequences do these findings have for our empirical analysis? If negative attitudes toward certain minorities are connected to e.g. employer behavior, we would expect to find some groups suffering in the labor market after 9-11. The same is certainly true if people's feelings of exposure are linked to their chances in the labor market. We will return to the issue of which groups that would face an accentuated discrimination under different hypotheses in the theoretical discussion in section 4.

3   Immigrants in the Swedish labor market

Sweden has a relatively large immigrant population in comparison with other Western countries. In 2002, the foreign-born made up 12 percent of the total population of about 9 million. Labor migration in the 1950s and 1960s is the main source of a Finnish minority including almost 200,000 people. An increasing inflow of refugees and tied movers beginning in the late 1970s has resulted in large minority groups from many geographically distant countries. The Iraqi-born population amounts to more than 60,000; the number of Iranians are more than 50,000.

As in other countries, many immigrant groups in Sweden find it difficult to become integrated in the labor market. In the first six months of 2003, unemployment stood at 3.9 percent among natives according to the labor force surveys; for African (Asian) immigrants this figure was 18.9 (15.7) percent. Previous Swedish research has in general found that discrimination of immigrants in the labor market is an issue of some importance (see e.g. Höglund 1998). Most quantitative studies of the phenomenon use some "residual approach", i.e. differences that remain after controlling for as much as possible is interpreted as discrimination (Vilhelmsson 2002, le Grand & Szulkin 2000). Rooth (2002)

studies individuals who have been adopted from different parts of the world, and conclude that a background indicating a "non-Swedish" look is associated with greater unemployment risks. Holm (2001) reports results from discrimination experiments suggesting that Non-Swedes were less likely to be chosen as partners in coalition formation games.

Qualitative studies on discrimination in Sweden often conclude that discrimination is "structural" and thereby in some sense more far-reaching than what can usually be discovered in quantitative studies; see de los Reyes & Wingborg (2002). Results along these lines are presented in Hertzberg (2003), who argues that caseworkers at the employment offices have negative attitudes toward certain immigrant groups, which possibly lower the employment prospects of these groups.

4   Theory

The purpose of this section is to show how discrimination affects the escape rates out of unemployment using the standard job search model as presented in Mortensen (1986).[5] The focus of the job search model is on an unemployed person who strives to maximize the present value of lifetime income. The job searcher affects how often wage offers arrive by choosing search intensity, which carries a cost. The person also knows the distribution of wages offered. He/she is then aware of if a received offer is satisfactory, and stops searching if such a wage offer arrives and continues to search otherwise. Hence, the model implies that the job searcher will decide an acceptance wage and a search intensity that maximizes the present value of lifetime income. We incorporate discrimination into this model by assuming that an attitude change lowers the rate by which wage offers arrive and/or that the costs of search increase.

In the remainder of this section we show that, under reasonable assumptions, the effect of an unanticipated change of attitude among the public leading to an increase in discrimination against certain groups (a lower rate of wage of-

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[5] How discrimination affects an unemployed person engaged in job search has been formalized in an increasing number of articles during the last twenty years. This literature focuses on equilibrium effects rather than on the (partial) effect on individual job search strategies; see for instance Altonji & Blank (1999). The purpose of these articles has been to extend the Becker (1957) model of discrimination, and to show that wage differentials due to discrimination exist in equilibrium when frictions are included into the Becker model.

fer arrivals and/or higher search costs) is a lower escape rate out of unemployment.

4.1   Escape rates and discrimination[6]

In this type of job search model the escape rate out of unemployment is

$$\theta = \lambda(s^*)\big[1 - F(w^*)\big] \tag{1}$$

where s^* is the (endogenous) search intensity, $\lambda(s^*)$ is the function for wage offer arrivals while $[1–F(w^*)]$ is the probability that a wage offer w from the cumulative wage distribution $F(w^*)$ is greater than the reservation wage (w^*). First, changed preferences and attitudes could affect $\lambda(s^*)$, meaning fewer job offers at a given search intensity. We assume that $\lambda(s^*)=\lambda s^*$ where λ can be regarded as an efficiency parameter of search. A decrease in search efficiency could occur if changing preferences among the employers make them reluctant to hiring some immigrant groups. Naturally, employers' willingness to hire may also decline due to changing preferences among customers or other employees.

An attitude shift in the public may increase the costs of search for discriminated job searchers if a person finds a given level of search more psychologically trying.[7] The cost function affecting the optimal level of search is assumed to be a simple convex function: $c(s) = ks^2$, where k is the parameter that shifts due to increased discrimination.

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[6] We believe that discrimination due to changing preferences is the most likely mechanism in this context. It is, though, also thinkable that 9-11 increased so-called statistical discrimination (see Altonji & Blank 1999). Statistical discrimination arises when employers have imperfect information on individuals and therefore use measures of group productivity. It is of course unknown to us which productivity characteristics employers consider to be important but hard to observe. One could argue that 9-11 did not provide information on many of the factors commonly connected to this type of discrimination, such as language skills or institutional knowledge. In the 9-11 context, changes in statistical discrimination could occur e.g. if employers consider the risk that an individual has plans on terrorist activities. Indeed, such an explanation can be regarded as far-fetched and requires that the terrorist activities would incur a cost to the employer. Statistical discrimination may, however, also operate through increased risk aversion. If hiring certain immigrant groups is considered risky and the events made employers more risk-averse, we would expect a decrease in job offers given to certain groups.

[7] This could reflect the cost of contacting potentially discriminatory employers. It may also be e.g. the discomfort of leaving one's home.

The next step is to analyze how a change in these two channels of discrimination affects the escape rate out of unemployment. Equation (2) shows how the escape rate out of unemployment is affected by an increase in wage offer arrivals.

$$\frac{d\theta}{d\lambda(s^*)} = [1 - F(w^*)] - \lambda(s^*)F'(w^*)\frac{dw^*}{d\lambda(s^*)} \tag{2}$$

For plausible assumptions regarding the shape of the wage offer distribution (see Van den Berg (1994)) equation 2 is positive.[8] This implies that we would expect the escape rate out of unemployment to decrease for discriminated job searchers when employers decrease the availability of jobs to these individuals (the efficiency of search will now be lower).

How the escape rate out of unemployment is affected by an increase in the search cost parameter k is shown in equation (3).

$$\frac{d\theta}{dk} = \frac{ds^*}{dk}\lambda[1 - F(w^*)] - \lambda s^* F'(w^*)\frac{dw^*}{dk} \tag{3}$$

The first term in equation (3) is the negative direct effect of an increase in the costs of search (ds^*/dk is negative), while the second term is an indirect effect that lowers the reservation wage (dw^*/dk is negative since the job searcher becomes less "choosy" when costs increase). As in equation (2), there are two opposing effects on the escape rate out of unemployment. Even if the sign of equation (3) is indeterminate, it is likely that the negative direct effect is greater than the counteracting indirect effect for the same reason that makes equation (2) positive.[9] Then the hazard rate would decrease when the costs of search increases.

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[8] For instance the family of lognormal distributions and the family of Pareto distributions fulfil these requirements.

[9] If we assume that $\left|\frac{dw^*}{dk}k\right| = \left|\frac{dw^*}{d\lambda(s^*)}\lambda(s^*)\right|$ then it is required that $\left|\frac{ds^*}{dk}\frac{k}{s}\right| > 1$ for the results in Van den Berg (1994) to hold. We thank Peter Fredriksson for pointing this out.

4.2   Discriminated groups

The empirical strategy uses the fact that unemployment escape rates should change for some immigrant groups due to discrimination related to 9-11, but are expected to be unchanged for other groups of individuals. So, which immigrant groups were likely to experience an increase in discrimination?

The reports discussed in section 2 suggest that the attitude change was likely to be most significant toward persons perceived to be Muslims. The potential increase in labor market discrimination is therefore largest for this group. Since several studies conclude that "visual identifiers" were important, one could suspect that looks rather than beliefs potentially were determinants of discrimination.

In this study we include eight different immigrant groups in the baseline analysis.[10] We believe that the groups most likely to become targets of discrimination were people from the Middle East (including Northern Africa) and Africa (due to their looks or Muslim sounding names).[11] Immigrants from Asia and former Yugoslavia could to at least some extent also be expected to be hurt by increased discrimination as a result of the same factors. The fact that attitudes toward immigrants in general appear to have been affected (see *Figure 1*) suggests that a general xenophobic effect is possible, which could make most immigrants to be hurt by discrimination.

All in all, we are unable to say exactly which groups that would be subjected to discrimination under various hypotheses. However, we believe there to be a scale of potential discrimination, where people born in the Middle East and Africa are at one end, and the Swedish-born and the Nordic immigrants can be found at the other. This is the basis for interpreting the empirical results presented in section 6.

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[10] Section 5 discusses the data. See the appendix for a description of the grouping based on region of birth.
[11] If one believes that employers set out to discriminate Muslims only, the empirical analysis could be said to capture statistical discrimination of a group where many—but not all—are Muslim. We do not know how preferences are formed, but note that the studies presented in section 2 suggest some stereotyping in the attitudes.

5   Data

This section begins with a description of the data sources used in the study. This is followed by a presentation of our sampling strategy and some basic descriptive statistics.

5.1   Data sources

Our data come from the IFAU database, which aggregates information from a large number of databases and registers from Statistics Sweden. In this study we primarily use data from two parts of the IFAU database. First, we use a panel of the entire Swedish population ages 16–64 during the period 1990–2000 (see Statistics Sweden 2002 for details). Annual information on e.g. earnings, education, family situation, country of birth, year of immigration and welfare receipt is taken from this source.

The other central data source is the HÄNDEL database, containing everybody that registers at a public employment office. Registering at the employment office is a condition for receiving unemployment benefits, so the database covers a vast majority of the unemployed.[12]. The individual unemployment spells include the exact dates for entry and exit into and out of unemployment. These data cover the period January 1991–November 2002. HÄNDEL is created for administrative purposes, and the data come mainly from caseworkers entering new information about the unemployed individual. There are some oddities in the spell data coming from miscodings and caseworkers attempts to adjust the information. We have taken some steps to correct the HÄNDEL data; the correction procedure is described in a previous version of this paper that is available upon request.[13]

As mentioned in section 4, we use country (region) of birth as a proxy for the determinants of discrimination. For confidentiality reasons, some aggregations of countries are already made in the basic IFAU data. We make further aggregations and include the following birth regions in the baseline analysis: Sweden, Nordic, Western, Eastern Europe, Former Yugoslavia, Latin America,

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[12] According to Statistics Sweden (1993), about 90 percent of those who are unemployed according to the labor force surveys are also registered at the employment offices.
[13] Roughly ten percent of those included in the data have one or more dates (for transition) changed by our procedure.

Asia, Africa, Mideast + Northern Africa. The countries included in each of these regions are presented in the Appendix.

To avoid including people who are primarily in education (but register as unemployed during e.g. summer holidays) and to circumvent problems concerning possible retirement, we only include individuals aged 26–54 on December 31, 2000. We also exclude immigrants who entered Sweden in 1997 or later in order to avoid individuals enrolled in introduction programs for immigrants.

5.2   Construction of the sample

Our primary concern is how 9-11 affected exits from unemployment to employment, and whether differences in changes across groups can be linked to some discrimination hypothesis.

For studying this issue, there is one group of "treated" individuals for which treatment can be considered completely exogenous: those who were unemployed on September 11, 2001. A natural point of departure is to study the rate at which these individuals leave unemployment. As a first pre-treatment comparison, it is plausible to consider exit rates in some period preceding 9-11.

The basic construction of the sample can be described in the following way. We identify individuals unemployed at September 11, 2001 (time 0) and then study exit to employment within t days. An individual is said to have left for a job if the spell ends in employment before time t. If the spell ends for non-work reasons, the individual is said to not have found work (even though it is possible to re-enter unemployment and find work before time t). Whether the individual is still employed or not at time t is not considered. Similar samples are then drawn at other points in time, e.g. 0–t, 0+t, 0–1 year. Depending on the type of analysis, varying numbers of samples are drawn. In each sample, the follow-up period is always t days.

The data includes a large number of so-called "search categories" (Ams 2002), some of which indicate open unemployment, whereas others define e.g. participation in labor market programs. The baseline specification includes only people in open unemployment. This is to make sure that those studied are actually "at risk" at the sampling day. We also perform specification checks using alternative definitions of unemployment.

One could imagine several alternatives to this stock-sampling strategy, most obviously some flow-sampling procedure. To precisely estimate the size of the discrimination effect of 9-11 on a representative spell of unemployment, we

would probably need a more elaborate sampling scheme, a more complicated econometric analysis, and some stronger assumptions. But to investigate if there are patterns in the data suggesting increased discrimination, we find a transparent and clean approach appealing. We therefore believe that the treatment-on-the-treated type of analysis that follows from our sampling strategy is to prefer in this case.

5.3   Basic descriptive statistics

Table 1 presents some basic descriptive statistics by region of birth for samples of unemployed individuals, drawn on September 11, 2001.[14] There is quite a lot of variation across regions in the individual characteristics. The fraction made up by women, e.g., ranges from 35 percent for the category Africa to 65 percent among Eastern Europeans. People born in the Nordic or other Western countries are on average older than groups born in other countries outside Europe. The former groups have also spent a lot more time in Sweden since their latest year of immigration.

Regarding education, the Nordic immigrants are much more likely than other groups to have short high school as their highest completed level. Western and East European immigrants stand out as those who are most likely to have university training. Apart from these two groups, the differences in (imputed) average years of education are relatively small.

The table also contains two variables that measure time in unemployment. "Days in current" is the number of days the current spell has been going on at the day of sampling. "Days in previous" is the sum of days spent in unemployment before the current spell started. A few things should be noted concerning these variables. First, the data measures include only time spent in open unemployment; time in e.g. training or subsidized employment is excluded. One could of course use a more elaborate set of controls for time in different categories, but the present definition of unemployment history is consistent with our definition of unemployment. Second, the figures for "Days in previous" variable are indeed high: many groups have on average spent more than 800 days in unemployment since 1991 before the current spell. This is partly a

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[14] For Natives, we use a 10 percent random sample.

result of length bias in the stock sampling: we do not measure the average total duration for a representative unemployed person.[15]

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[15] Another contributing explanation may be that the figures include information on some spells starting before 1991. However, this is only the case for a very small fraction of the sample. Among Nordic immigrants included in HÄNDEL (who have the longest average stay in Sweden), less than 0.1 percent of the individuals have a spell that started before 1991.

Table 1 Description of samples drawn at time 0 (September 11, 2001), means (standard deviations).

Region	Swed.	Nord.	West	East E.	Fm Yugo.	Latin Am.	Asia	Afr.	M. East
Female	.52	.51	.41	.65	.46	.44	.59	.35	.38
Age	37.43	40.69	39.63	39.78	37.42	36.57	35.66	35.43	36.64
	(9.05)	(8.27)	(8.50)	(8.46)	(7.56)	(8.10)	(7.68)	(6.90)	(7.13)
Cohabiting	.39	.41	.46	.49	.64	.35	.55	.38	.62
Kid	.51	.51	.53	.60	.69	.55	.66	.53	.67
Yrs since migr.	n.a.	25.16	19.85	14.62	10.11	14.75	14.62	11.27	11.96
		(10.7)	(11.4)	(7.9)	(7.6)	(6.7)	(7.7)	(5.3)	(5.4)
Years of edu.	11.74	11.27	12.52	12.89	11.75	11.90	11.53	11.59	11.75
	(2.31)	(2.34)	(2.90)	(2.84)	(2.68)	(2.63)	(2.98)	(2.64)	(2.85)
Level of education									
<9 yrs	.02	.07	.05	.03	.12	.05	.13	.10	.12
9–10 yrs	.13	.16	.12	.07	.08	.14	.18	.15	.15
High sc.≤2	.40	.42	.27	.26	.22	.34	.26	.26	.22
High sc.>2	.20	.14	.17	.23	.31	.19	.15	.24	.21
Univ. <3	.13	.11	.17	.15	.13	.12	.11	.11	.14
Univ. ≥3	.11	.09	.22	.26	.12	.15	.15	.12	.15
Missing	.00	.01	.01	.00	.02	.01	.02	.02	.02
Days in ct.	135	106	112	123	112	101	106	109	120
	(174)	(107)	(105)	(121)	(115)	(102)	(104)	(106)	(120)
Days in prev.	725	831	682	843	602	818	669	867	858
	(571)	(665)	(609)	(641)	(493)	(566)	(564)	(613)	(631)
# obs.	10,311	3,415	1,360	2,540	4,184	1,776	2,064	2,203	8,144

Notes: "Cohabiting" includes married and cohabitants with common children; "Kid" is an indicator for kids in the household; "Years since migration" is defined as "2000 minus latest year of immigration; "Years of education" is calculated from average values based on the level of education. See the text for a presentation of the "Days in current" and "Days in previous" variables.

6   Empirical analysis

The aim of this paper is to analyze whether the attitude changes following 9-11 increased discrimination in the Swedish labor market. We do this by studying transitions between unemployment and employment for different ethnic groups around the time of the attacks. As discussed above, employers could become reluctant to hiring individuals with certain observed characteristics, or people may change search behavior due to higher search costs. If this is the case, the job finding rate would fall in some groups due to increased discrimination. This section first describes the development of unemployment exit around 9-11. Then it turns to a more formal analysis of the issue. The section also briefly presents results on supplementary topics: unemployment entry and exit to labor market programs.

6.1   Exit from unemployment

There are several possible ways of implementing an analysis of the impact from 9-11 on exits from unemployment. A first alternative is to study changes in exit rates between just before and just after 9-11 in different groups. A second alternative is to compare the exit rates in the period following 9-11 with exit rates in the same period in the year 2000, and compare the time changes in different groups.

To focus the discussion, consider the monthly exit rates from unemployment presented in *Figure 2*. Period "0" starts on September 11, 2001, and continues for 30 days; period –1 starts 30 days before 9-11. The figure shows the fraction of the unemployed from each birth region that leaves unemployment for a job during each interval. For visibility reasons, we have here only included four regions: Africa, Middle East, Sweden, and Nordic. They presumably are the ones most and least affected by potential discrimination.
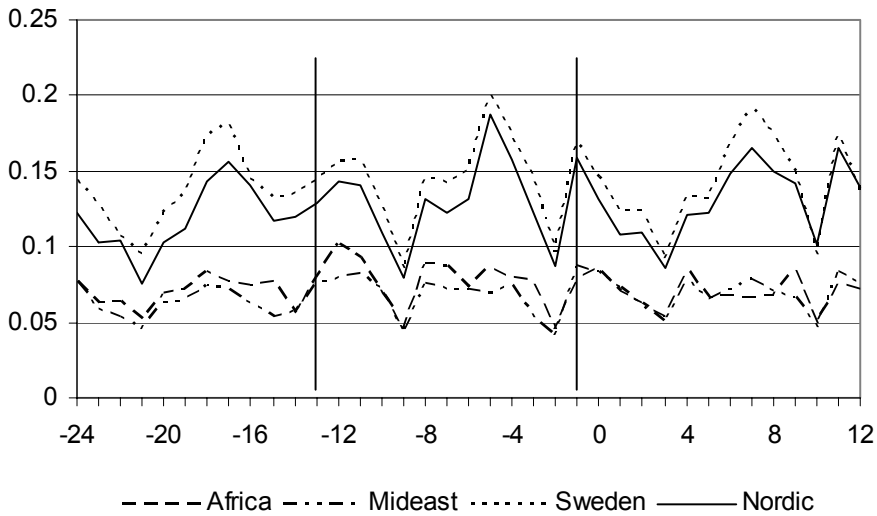
Figure 2 Exit rates from unemployment by region of birth, 30-day intervals.

Notes: "0" is the period starting on September 11, 2001; period –1 starts 30 days before period 0. Exits to the right of the bar at "-1" take place after 9-11.

The raw exit rates reveal a number of important things. As expected there are large differences in exit rates across groups. This signals that relative rather than absolute changes in the exit rates may be the preferred outcome. Another thing to note is the massive seasonal variation in the exit rates. The mid of the summer (e.g. period –2) and the time around Christmas (e.g. period 3) tend to have few exits, whereas more people find jobs during the spring. Had we not included the left part of the graph, one would have been tempted to attribute the low in periods 2–4 to the turmoil following 9-11. The graph, however, shows that there is a similar trough in the year before. Note, though, that the downward trend starts a bit earlier in 2001, which possibly signals a general 9-11 macro effect. However, it is similar in style to the corresponding period in 2002.[16]

Judging from this simple graph, it is hard to find any certain signs of Middle Eastern or African immigrants suffering larger losses than the two other groups

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[16] *Figure 2* shows no long-term trend in exit rates. However, in the years preceding the fall of 1999 (period –24), exit rates increased steadily as shown in *Figure A 3*.

after 9-11. This picture remains in *Figure 3* that presents deseasonalized exit rates for the four regions. The exit rate for each group in each period has been divided by the exit rate twelve periods (360 days) before. From this figure it is very difficult to discern any pattern that would be consistent with the discrimination hypothesis. The relative exit rates are very similar, and there is no sign that the groups expected to be exposed to attitude changes experience a worse development of employment probabilities in the first six months after 9-11 than other groups. There is no indication on a general "immigrant" effect: Swedish-born individuals share the pattern of the other groups.
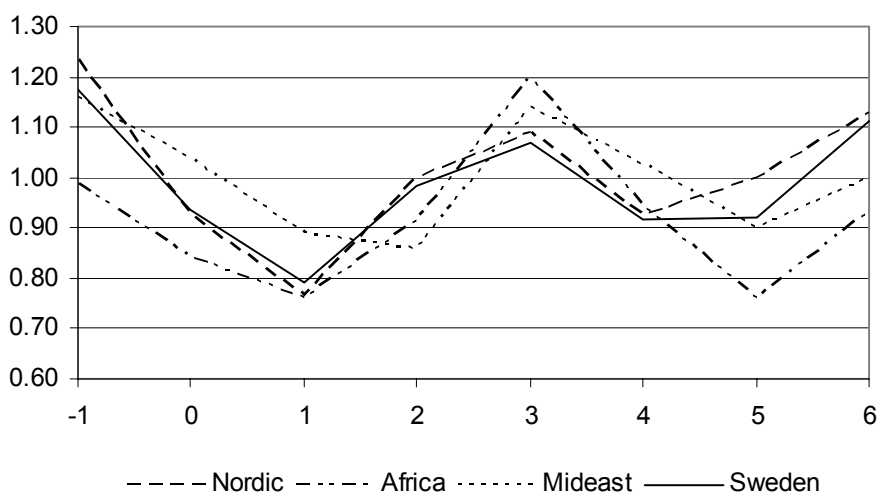
Figure 3 Deseasonalized exit rates from unemployment after 9-11, 30-day intervals.
Notes: The presented exit rates are calculated as: (exit rate in period x) / (exit rate in period (x–12)).

6.1.1 Formal analysis

This subsection performs a more formal analysis of the exit from unemployment to employment. Specifically, we use a difference-in-differences (DD) type of analysis to examine the issue of post-9-11 discrimination. Since there is so much seasonal variation in the exit rates, we compare the development after 9-11 with the same time period one year before. The basic assumption of the DD approach is that the change in the exit rate over time (conditional on observed individual characteristics) is the same in the treatment and the compari-

son group, absent increased discrimination. In other words, the difference in the time effects captures the discrimination effect.

We here use a longer window for exit than in the figures above: 120 days in the baseline specification. There are two basic reasons for this choice. First, measurement problems are less important with a longer period for exits. This may be particularly important in groups with low exit rates (cf. *Figure 2*). Second, it is not certain that a discrimination effect would appear in the data very shortly after 9-11. Exits that took place immediately after this date were probably decided upon before the attacks. One alternative to extending the evaluation period would be to sample at later points in time (as in *Figure 2* and *Figure 3*). The result of such a procedure would, however, be a less clean "experiment", with the potential problem that 9-11 changed the characteristics of the inflow. Our approach is to test different evaluation periods to check the sensitivity of the results.

Consider estimation of the following probability model:

$$P(job_{ip}^{d}) = G\left(\alpha + X_{ip}^{d}{}'\beta + \delta^{d} Post\right) \qquad (4)$$

where job is an indicator variable for leaving unemployment for employment, X_{ip}^{d} is the observed characteristics of individual i in period p (pre- or post-9-11) belonging to group d. The variable $Post$ is one in the post-9-11 period and zero in the pre-period. We are primarily interested in differences in δ^{d} across groups. The differences in the pre-post changes δ^{d} give the difference-in-differences estimates.

We estimate probit specifications of (4) separately for each region of origin; *Table 2* displays the results. The covariates of the models are presented in detail in the Appendix. They include the characteristics presented in *Table 1*, but the measures of previous time in unemployment are specified as dummies for different intervals to get a more flexible specification. Since country (and not just region) of origin may be an important determinant of the outcome variable, we also include dummies for birth country.[17]

It is not completely clear whether we want to study absolute or relative changes in the exit probabilities. While it is reasonable to believe that both a general effect of 9-11 and a discrimination effect would affect the relative

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[17] The full estimation results are presented in *Table A 1* in the appendix.

probability of exit to employment, one could also build stories where the shocks work through the absolute probabilities. As evident from *Figure 2*, the exit rates vary a lot across groups. Since the conclusions may differ depending on which measure we use, we will use both.

As indicated by the "P(pre)" and "P(post)" rows of *Table 2*, the exit rates are somewhat lower in the fall of 2001 compared to the same period in 2000, but the differences are quite moderate. The differences are accentuated when we include controls for individual characteristics, as is evident from the "Post-9-11" row of the table. The row presents the estimated absolute effect on the exit probability of going from before to after 9-11, evaluated at sample means. Most groups have negative estimates, although in many cases insignificant.

It is clear from the table that the absolute changes (Post-9-11) are of the same magnitude in groups that are not likely to be targets of discrimination as in the potentially most exposed groups. Turning instead to relative changes (where the estimated absolute effect is divided by the exit rate in the pre-period), the estimated impact of course becomes larger in groups with low exit rates. Still, there is little scope for finding differences in the pre-post effects across groups. The estimated relative impacts and their confidence intervals are presented in *Figure A 1* in the appendix.

The results presented in the table do not square with the discrimination theory presented in section 4. The "Mideast" group, e.g., experienced only small changes (both in absolute and relative terms), despite the fact that they would presumably suffer more from an increased discrimination in this case than most other groups. At face value, the relative changes among people from former Yugoslavia and Africa seem larger than in other groups. However, the confidence intervals include the estimates for the Swedish-born and Nordic immigrants.[18]

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[18] Note also in *Figure 2* that the "Africa" group has a high peak around the month "–12", i.e. the start of the pre-9-11 period in the DD analysis.

Table 2 Exit to employment before and after 9-11, by region of birth.

	Sweden	Nordic	Western	East Eur	Fm Yugo
P(pre)	.316	.286	.265	.208	.218
P(post)	.307	.272	.269	.206	.208
Post-9-11	−.033	−.025	−.015	−.010	−.035
	(.008)	(.012)	(.019)	(.013)	(.010)
Rel change in P	−.104	−.087	−.057	−.048	−.160
Observations	16,126	7,783	2,901	5,669	9,495
Pseudo R^2	.04	.04	.04	.04	.04

	Latin Am	Asia	Africa	Mideast
P(pre)	.241	.219	.185	.170
P(post)	.226	.229	.167	.180
Post-9-11	−.022	.005	−.029	−.004
	(.016)	(.015)	(.013)	(.007)
Rel change in P	−.091	.023	−.157	−.024
Observations	3,927	4,534	4,963	18,403
Pseudo R^2	.04	.04	.04	.03

Notes: "Post-9-11" is the estimated difference (probit, evaluated at sample means, standard errors in parentheses) in exit from unemployment to employment between the 120-day period following 9-11 and the corresponding period one year earlier. "P(pre (post))" is the exit rate in the pre (post)-9-11 period. "Rel change in P"= "Post-9-11"/ P(pre). The control variables included in the models are described in the appendix. "Pseudo R^2" is defined as 1−L1/L0, where L1 (L0) is the full (constant-only) model.

6.1.2 Robustness checks

We now move on to check the robustness of these findings. A first issue may be the quite arbitrary choice of follow-up period. The patterns of *Table 2* remain if we instead use 60 or 180 days as the window for exiting unemployment, so this is probably not a big problem.

The above specifications do not include everyone registered at the unemployment office at the sampling dates: people in labor market programs are excluded. We believe that this is a reasonable strategy, since the primary interest is in those who are supposedly looking for a job at the time they enter the sample. If we instead include all the registered individuals (except those in regular

work), we get the results of *Figure A 2*. The estimated negative pre-post effect then tends to be larger in many groups, but there is still no clear pattern or significant differences in the estimates consistent with a credible discrimination story.

One could hypothesize that the post-9-11 labor market experiences differ across e.g. gender or region of residence. Estimating the models presented above separately for men and women and by region of residence gave no sign of increased discrimination. The gender-specific models reveal a tendency to a larger decrease in the exit rates among men. Similarly, the downturn is more marked in the metropolitan areas than in the rest of Sweden. Note, though, that statistical significance is an issue in these specifications. The difficulty of finding patterns consistent with discrimination theory remained when municipal dummies were included in the baseline model. The conclusions were also unaltered when we performed separate estimations for a number of birth countries from different regions. In other words, the geographical aggregations appear not to be hiding any discrimination effects.

Another issue is that the post-9-11 macro shock may differ between sectors. If the potentially most affected groups are concentrated to jobs where the general macro effect is small, our "zero" results could hide discrimination. The data include information on preferred occupations. Controlling for this factor has virtually no effect on the results.[19] It is also possible that discrimination differs across occupations. If people in general change their attitudes more than the employers, one could suspect that some groups found it harder to get customer related jobs. We restricted the sample to individuals who had stated an interest in customer service occupations (e.g. sales clerk, waiter, taxi driver) or were looking for security related jobs (fire fighter, security guard). Estimations using this sub-sample did not indicate increased discrimination.

A more delicate matter is whether the difference-in-differences approach is appropriate. Suppose, e.g., that there was a positive trend in the relative employment chances among people born in the Middle East, and that post-9-11 discrimination broke this trend. Such a mechanism would not show up in the results above. A DDD-type of analysis could alleviate these problems, but at the cost of stronger assumptions regarding the trends.

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[19] It should be noted that the people in the sample are unemployed, and do in the strict sense not belong to any particular occupation. Since searching for other types of jobs may very well be a response to the 9-11 changes, we have excluded this variable from the baseline specification.

It is somewhat unappealing to add a third "pre-pre-period" to the analysis, and assume that the change in exit rates from the fall of 1999 to the fall of 2000 would capture the expected change between the same periods in 2000 and 2001. Instead, we performed "fake DD" estimations, where the samples were drawn on March 11, 2001 and 2000 respectively, and then followed for 120 days. The results of the "fake DD" analysis (presented in *Table A 2*) in combination with the baseline estimates form a DDD-type of evaluation. Comparing the two sets of DD-estimates do not indicate that 9-11 induced a change in the development over time that one would expect in the presence of increased discrimination. The relative changes are generally a bit smaller in magnitude in "fake DD" specifications, which is consistent with a negative effect of 9-11 on the overall exit rates. We have also investigated the possibility that the fall of 2000 is a bad comparison period, and instead used a sample drawn on September 11, 1999. The estimates supported the conclusions from the baseline analysis.

To sum up the results of this section, we find that there is no evidence that 9-11 changed job finding prospects for different groups in ways that could be explained by discrimination theory. This is the case both in investigations of the periods just before and just after 9-11, as well as when the post-9-11 period is compared to the same period one year before. There is an overall decrease in the exit rates, but the changes are as big in groups that are unlikely to suffer from discrimination as they are among the most exposed.

6.2   Unemployment entry and exit to programs

There exist alternative outcomes that are interesting to study in this context. One is entry into unemployment and another is exit to programs. The Swedish employment protection laws put relatively strong restrictions on how layoffs can be performed. Normally, redundancies should follow the rule "last in—first out", and former employees may under certain conditions have priority to new jobs in the company. People with permanent positions were therefore unlikely to be immediately affected by a discrimination effect from 9-11. However, a large fraction of the immigrant population has temporary jobs. In the first six months of 2001 more than 30 percent of workers born in Africa or Asia had a temporary job, compared to about 14 percent of the Swedish-born workers. Thus, if employers wished to discriminate against certain groups, there were opportunities to achieve this by not prolonging temporary contracts.

We investigated this issue by constructing a sample in a way similar to the one used for exits. In this case we conditioned on not being registered at the employment office at the sampling date. We then performed the DD and DDD analysis discussed above, but with the probability of registering (as unemployed or in a program) within the follow-up period of 120 days as the outcome. The analysis revealed no signs of changed discrimination. In general, it seemed that the entry rates were lower in 2001 than in 2000, and that the decrease in the entry rate was actually larger in the groups most likely to suffer from discrimination. The DDD comparisons showed that the labor market was improving at a diminishing rate, but there was no indication that the slowdown was more marked in the potentially discriminated groups.[20]

Changes in exits from unemployment to labor market programs could possibly disturb the baseline analysis. It is, though, not clear what to expect regarding these changes. One view is that employment officers attempt to protect people from discrimination by assigning them to attractive programs. Another possibility is that the officers themselves discriminate, either by decreasing assignments for some groups or by assigning people to programs against their will. Performing the analysis of section 6.1.1 but with exit to programs as the outcome variable showed that the probability of entering a labor market program was somewhat smaller in the post-9-11 period, but that the changes were very similar across immigrant groups.

7 Concluding remarks

The purpose of this paper has been to empirically investigate the connection between attitudes in the public and the labor market outcomes of minorities. The study starts out in the well-documented attitude shift that followed in Sweden after the terrorist attacks in the US on September 11, 2001. The question for our statistical analysis is whether the attitude change also led to increased labor market discrimination of certain ethnic minorities. We have used data on exits from unemployment to employment around 9-11 for different groups based on country of birth. We have also investigated the pattern of unemployment entry and exits from unemployment to labor market programs.

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[20] A full presentation of this analysis is included in a previous version that can be retrieved from the authors.

A large family of discrimination theories predicts a clear relationship between attitudes toward a minority and the group's chances in the labor market. Given the evidence on an attitude shift after 9-11 from surveys both in the public and within likely exposed groups, these theories predict that the labor market prospects of some immigrant groups would dwindle in the time after 9-11. Employers could e.g. become less willing to hire or keep people with some characteristics, or the negative attitude change could increase the costs of job search among some groups.

It is then very interesting that we find no indications on anything that could reasonably be interpreted as increased labor market discrimination. The development of exits from unemployment to work in the time around the attacks indicates a possible impact of 9-11 on the labor market. There is, however, no result suggesting that this effect was larger in the immigrant groups most likely to be targets of discrimination. Neither is there any sign of a general negative effect on immigrants compared to natives. Difference-in-differences (DD) analyses as well as DDD comparisons also support the view of no change in discrimination following 9-11.

Do these findings tell us anything more general about ethnic discrimination in the Swedish labor market? We should start by pointing out that our results do not necessarily say anything about the *existence* of discrimination in the labor market, since we focus on changes following one unexpected event. We make no attempt to explain e.g. the substantial differences in employment probabilities that undoubtedly exist across groups. However, *if* discrimination is not a major problem in the labor market, the results would be similar to those of this paper.

If one instead—as many people do—thinks of discrimination as a factor of importance, our results bring some new information. First, discrimination is not based on preferences that change quickly. Employers or job searchers do not seem to react immediately to shifts in public opinion or reports in the media. Second, there is not necessarily a direct link between a group of people's feelings of discrimination and exposure and the group's chances in the labor market. Given the widespread notion of increasingly negative attitudes, we would have expected to see some deterioration effect in the labor market if such a connection existed.

Another interpretation is that employers (at least wish to) act rationally, and that statistical discrimination of immigrants is the major component of discrimination in the labor market. Uncertainty about e.g. language ability or insti-

tutional knowledge may create disadvantages for some groups. Most such factors were unaffected by 9-11, and we would therefore not expect to see any change in statistical discriminatory behavior.

Ethnic discrimination in the labor market is a much debated issue both among politicians and researchers. Previous studies have most often only been able to provide indications on possible discrimination. Measuring the extent and importance of labor market discrimination in a more reliable way and identifying the nature of discrimination seem to be important topics for future research. The findings of this paper suggest that some well-established theoretical connections may not hold in the real-world labor market.

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Appendix

Variables included in the estimations

Variable	Definition
Post-9-11	1 in the period after 9-11, 0 in the preceding period
Female	1 for females, 0 for males
Age	Age on Dec. 31 2000 for post-9-11, on Dec 31 1999 for pre-9-11.
Age squared	
Cohabiting	1 if married or cohabitant with children in common (with spouse).
Kid	Kid <18 years old in the household
Cohabiting*Female	
Kid*Female	
Education dummies	Dummy for highest completed level; see Table 1 for further description.
Country of origin dummies	Birth "countries" as available in the IFAU database; see below.
Days in current spell (interval dummies)	Dummies for the following intervals: 0–49, 50–99, 100–199, 200–299, 300–399, 400–
Days in previous spells (interval dummies)	Dummies for the following intervals: 0, 1–99, 100–299, 300–499, 500–999, 1000–
Search category	Search category according to HÄNDEL at the sampling date. See Ams (2002). Includes categories 11, 12, and 13 in investigations of the exit to employment. Specifications for entry to unemployment include all categories.

Regions and countries of origin

Region	Countries included
Sweden	**Sweden**
Nordic	**Finland**, **Denmark**, **Norway** + **Iceland**
Western	**GB + Ireland**, **Germany**, **Southern Europe** (Greece + Italy + Spain + the Vatican + Monaco + Malta + San Marino), **Other Europe** (Andorra + Belgium + France + Liechtenstein + Luxemburg + the Netherlands + Switzerland + Austria), **US + Canada**
Eastern Europe	**Poland**, **The Baltic states** (Estonia + Latvia + Lithuania), **Eastern Europe 1** (Rumania + The former USSR + Bulgaria + Albania), **Eastern Europe 2** (Hungary + The former Czechoslovakia)
Former Yugoslavia	**Bosnia-Herzegovina**, **Former Yugoslavia** (Yugoslavia + Croatia + Macedonia + Slovenia)
Latin America	**Mexico and Central America**, **Chile**, **Other South America** (Argentina + Bolivia + Peru + Colombia + Uruguay + Ecuador + Guyana + Paraguay + Surinam + Venezuela)
Africa	**African Horn** (Ethiopia + Somalia + Sudan + Djibouti), **Other Africa** (all African countries not included in African Horn or North Africa)
Middle East + North Africa	**North Africa + Middle East** (Lebanon + Syria + Morocco + Tunisia + Egypt + Algeria + Israel + Palestine + Jordan + South Yemen + Yemen + the United Arab Emirates + Kuwait + Bahrain + Qatar + Saudi Arabia + Cyprus), **Iran**, **Iraq**, **Turkey**
Asia	**East Asia** (Japan + China + Korea + Hong Kong + Taiwan), **Southeast Asia** (Vietnam + Thailand + the Philippines + Malaysia + Laos + Burma + Indonesia + Singapore), **Other Asia** (Sri Lanka + Bangladesh + India + Afghanistan + Pakistan + Brunei + Bhutan + Kampuchea + the Maldives + Mongolia + Nepal + Oman + Sikkim)

Notes: The left column contains the regional grouping used in our analysis. The boldfaced names in the right column indicate the aggregations in the original IFAU data. We exclude "Oceanic" (e.g. Australia and New Zealand) countries from our analysis, since this category mixes very different countries. People from this part of the world constitute a very small part of the Swedish immigrant population.

Figure A 1 Relative effects on exits from unemployment (relchange), at sample means, predicted and 95% CIs (rupper/rlower), pre-post-9-11. Baseline (SC 11–13). See *Table 2* for further details.

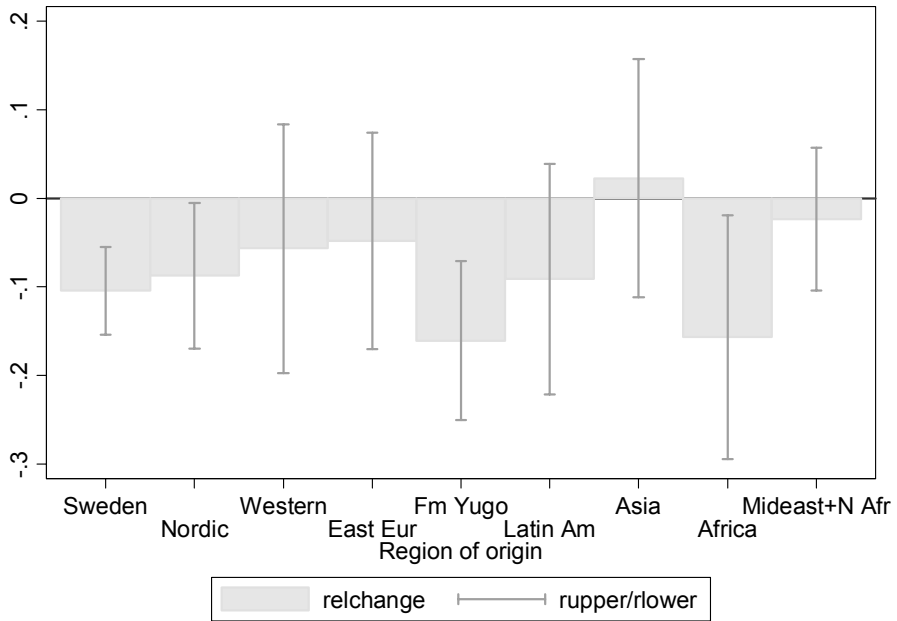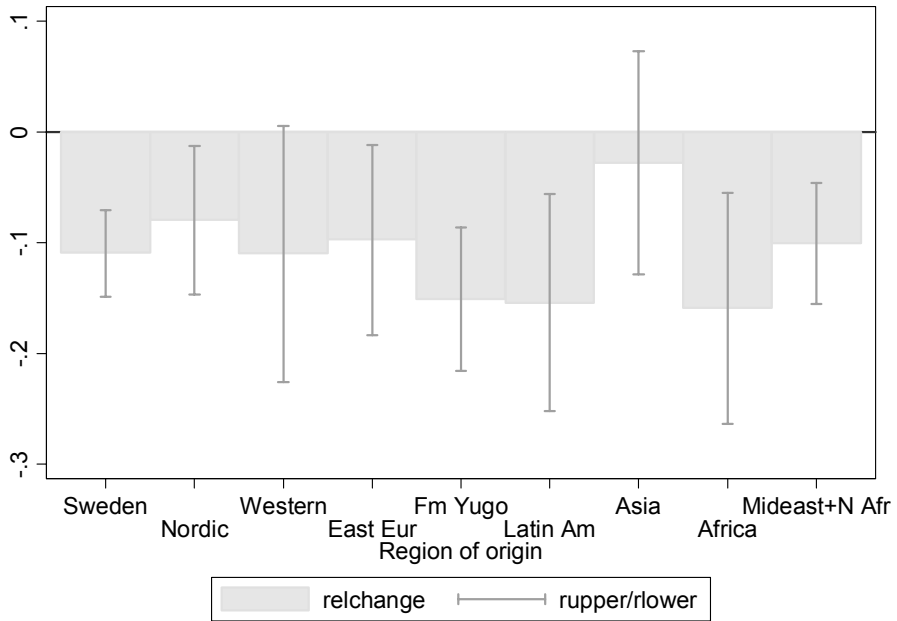IFAU – Shifting attitudes and the labor market of minorities

Figure A 2 Relative effects on exits from unemployment (relchange), at sample means, predicted and 95% CIs (rupper/rlower), pre-post-9-11. All search categories.
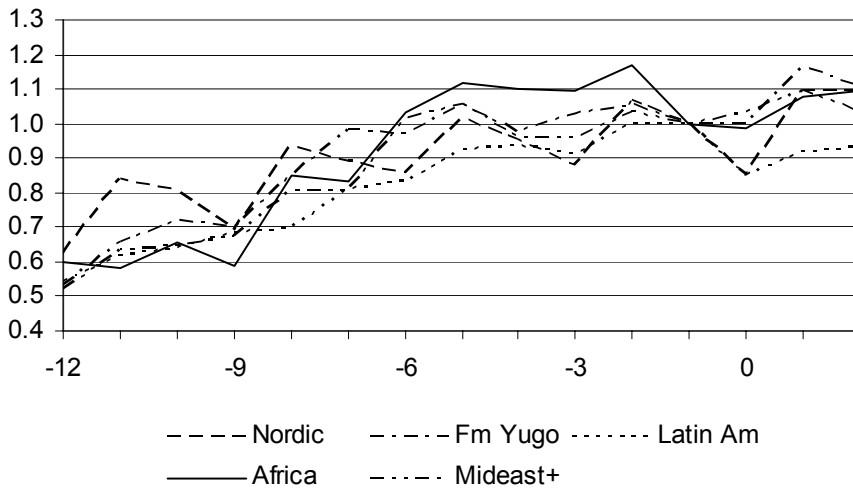
Figure A 3 Relative exit rates from unemployment, 120-day periods. Index period: -1

Table A 1 Estimates for covariates in baseline DD analysis of exit to employment.

	Swe.	Nord.	West.	East E.	Fm Yugo.	Latin Am.	Asia	Afr.	Mid East
Post-9-11	−.033	−.025	−.015	−.010	−.035	−.022	.005	−.029	−.004
	(.008)	(.012)	(.019)	(.013)	(.010)	(.016)	(.015)	(.013)	(.007)
Age	.013	−.000	.007	.001	.013	−.003	−.001	.007	.007
	(.004)	(.007)	(.010)	(.007)	(.006)	(.008)	(.009)	(.008)	(.004)
Age sq.	−.019	−.001	−.010	−.002	−.019	.004	−.002	−.012	−.012
	(.005)	(.008)	(.013)	(.009)	(.007)	(.011)	(.011)	(.010)	(.005)
Female	.051	.014	.081	.044	.055	.013	.046	.095	.048
	(.011)	(.016)	(.030)	(.018)	(.019)	(.025)	(.022)	(.023)	(.014)
Kid	−.018	−.016	.021	−.021	−.015	−.000	.010	.010	.000
	(.014)	(.020)	(.031)	(.026)	(.020)	(.025)	(.028)	(.022)	(.011)
Kid*Fem	.014	.020	−.045	.018	−.008	.054	−.048	−.052	−.027
	(.018)	(.026)	(.041)	(.029)	(.026)	(.038)	(.034)	(.027)	(.017)
Cohab.	.070	.073	.028	.047	.067	.051	.018	.032	.023
	(.015)	(.021)	(.031)	(.025)	(.019)	(.028)	(.029)	(.023)	(.011)
Co*Fem	−.064	−.016	−.015	−.031	−.009	−.033	.008	−.010	−.006
	(.017)	(.025)	(.040)	(.028)	(.024)	(.032)	(.034)	(.028)	(.015)
Level of education (<9 yrs ref.)									
9–10 yrs	−.091	−.034	−.016	−.007	.006	−.061	−.015	−.043	.015
	(.026)	(.023)	(.048)	(.039)	(.020)	(.032)	(.023)	(.020)	(.012)
High.≤2	−.027	−.007	.042	.023	.021	−.011	.016	−.005	.025
	(.028)	(.022)	(.048)	(.037)	(.017)	(.034)	(.023)	(.021)	(.011)
High sc.>2	−.038	.013	.025	.004	.024	−.008	−.008	−.019	.018
	(.028)	(.025)	(.049)	(.036)	(.015)	(.035)	(.025)	(.020)	(.012)
Univ<3	−.055	−.001	.027	.018	.011	.008	−.009	−.029	.027
	(.027)	(.026)	(.050)	(.038)	(.018)	(.038)	(.027)	(.022)	(.013)
Univ. ≥3	.027	.032	.115	.023	.024	−.006	.037	.002	.041
	(.030)	(.028)	(.052)	(.037)	(.019)	(.037)	(.027)	(.024)	(.013)
Missing	−.096	−.100	.038	−.007	−.053	.071	.045	−.046	.014
	(.125)	(.052)	(.104)	(.083)	(.038)	(.119)	(.060)	(.037)	(.024)
Continued on next page.									

Current spell (–50 ref.)								
50–99	–.083	–.039	–.059	–.029	–.024	–.027	–.043	–.034	–.009
	(.009)	(.012)	(.020)	(.013)	(.010)	(.016)	(.015)	(.013)	(.007)
100–199	–.087	–.070	–.089	–.078	–.032	–.060	–.045	–.052	–.036
	(.009)	(.013)	(.020)	(.013)	(.011)	(.017)	(.015)	(.013)	(.007)
200–299	–.132	–.106	–.098	–.100	–.083	–.082	–.072	–.067	–.050
	(.012)	(.016)	(.025)	(.014)	(.012)	(.021)	(.020)	(.015)	(.008)
300–399	–.136	–.139	–.104	–.118	–.081	–.069	–.069	–.064	–.052
	(.017)	(.021)	(.033)	(.017)	(.017)	(.035)	(.028)	(.021)	(.011)
400–	–.159	–.135	–.172	–.116	–.068	–.110	–.086	–.074	–.074
	(.020)	(.025)	(.032)	(.020)	(.021)	(.034)	(.034)	(.025)	(.012)
Days in previous spells (0 ref.)								
1–99	.038	.018	.031	.024	.038	.042	.023	–.020	.101
	(.019)	(.028)	(.040)	(.032)	(.023)	(.048)	(.032)	(.031)	(.021)
100–299	.001	.005	.008	.031	.037	.090	.078	.018	.074
	(.015)	(.022)	(.032)	(.027)	(.020)	(.043)	(.030)	(.030)	(.017)
300–499	.006	–.014	–.028	–.014	.033	.035	.038	.047	.079
	(.015)	(.022)	(.031)	(.025)	(.020)	(.040)	(.030)	(.033)	(.017)
500–999	–.031	–.020	–.057	–.015	.018	.065	.021	.057	.067
	(.013)	(.019)	(.028)	(.023)	(.019)	(.036)	(.027)	(.030)	(.015)
1000–	–.065	–.039	–.067	–.062	–.005	.023	–.014	.002	.042
	(.013)	(.019)	(.028)	(.022)	(.020)	(.035)	(.026)	(.028)	(.014)
# Obs.	16126	7783	2901	5669	9495	3927	4534	4963	18403
Pseudo R^2	.04	.04	.04	.04	.04	.04	.04	.04	.03

Notes: Marginal effects for covariates of the probit model presented in Table 2, standard errors in parentheses. For dummy variables, the effect is for a discrete change from 0 to 1. "Pseudo R^2" is defined as $1–L1/L0$, where L1 (L0) is the full (constant-only) model.

Table A 2 "Fake DD": exit to employment before and after March 11, 2001.

	Sweden	Nordic	Western	East Eur	Fm Yugo
P(pre)	.394	.350	.300	.223	.232
P(post)	.404	.369	.319	.231	.222
Post-March 11	−.029	−.021	.012	−.007	−.036
	(.009)	(.012)	(.020)	(.013)	(.010)
Rel change in P	−.074	−.060	.040	−.031	−.155
Observations	17,629	9,005	2,997	5,747	9,736
Pseudo R^2	.06	.06	.04	.03	.04

	Latin Am	Asia	Africa	Mideast	
P(pre)	.272	.264	.198	.186	
P(post)	.296	.263	.205	.185	
Post-March 11	.018	−.021	−.022	−.017	
	(.017)	(.016)	(.014)	(.007)	
Rel change in P	.066	−.080	−.111	−.091	
Observations	3,968	4,432	4,969	18,985	
Pseudo R^2	.03	.05	.04	.03	

Notes: "Post-9-11" is the estimated difference (probit, evaluated at sample means, standard errors in parentheses) in exit from unemployment to employment between the 120-day period following March 11, 2001, and the corresponding period one year earlier. "P(pre (post))" is the exit rate in the pre (post)-9-11 period. "Rel change in P"= "Post-9-11"/ P(pre). The control variables included in the models are described in the appendix. "Pseudo R^2" is defined as 1–L1/L0, where L1 (L0) is the full (constant-only) model.

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