Do Terrorist Attacks Affect Ethnic Discrimination in the Labour Market? Evidence from Two Randomised Field Experiments

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November 12, 2017

Abstract

Terrorist attacks are known to influence public opinion. But do they also change behaviour? We address this question by comparing the results of two identical randomised field experiments on ethnic discrimination in hiring that we conducted in Oslo. The first experiment was conducted before the 2011 terrorist attacks in Norway; the second experiment was conducted after the attacks. In both experiments, applicants with a typical Pakistani name were significantly less

likely to get a job interview compared to those with a typical Norwegian name. But the ethnic gap in call-back rates were very similar in the two experiments. Thus, Pakistanis in Norway still experienced the same level of discrimination, despite claims that Norwegians have become more positive about migrants after the far-right, anti-migrant terrorist attacks of 2011.

1 Introduction

On 22 July 2011 Anders Behring Breivik, a far-right terrorist, detonated a car bomb at the government quarters in Oslo, killing eight people and maiming over two hundred. He then drove to the summer camp of the Workers' Youth League (Arbeidernes Ungdomsfylking), at the island of Utøya, where he shot and killed 69 people, most of whom teenagers.[1] These were by far the worst terrorist attacks in modern Norwegian history. In texts that he distributed on the internet and also at his trials, Breivik identified himself as a fascist and a Nazi. He claimed that his attacks were a response to the large scale immigration to Norway of non-Europeans, especially Muslims, which he regarded as a betrayal of Norway by the political establishment.

What was the impact of Breivik's anti-Muslim, anti-migrant attacks on Norwegian society? Did they discredit xenophobia and Islamophobia, and bring about positive changes in the majority–minority relationship? There is survey evidence that Norwegians have become more positive about immigrants after the attacks (Jakobsson and Blom, 2014). But did the attacks also change behaviour? Specifically, did ethnic minorities experience less discrimination in the labour market after the attacks? In this paper we address this question by comparing the results of two identical field experiments on ethnic discrimination in hiring that we conducted in Oslo. The first experiment was conducted before the attacks in the fall of 2010; the second experiment was conducted afterwards in the fall of 2011.

1.1 The impact of terrorist attacks on public opinion and on labour market outcomes

There is some evidence that terrorist attacks could change public opinion. For example, Traugott *et al.* (2002) argue that after 9/11 ethnic groups of Middle Eastern origins are viewed less favourably in America than those

[1] The Workers' Youth League is affiliated to Labour Party, which was then the governing party of Norway. Breivik targeted this camp because many of the participants were young activists and potential future Labour politicians.

of African, Hispanic, Asian or European descent. Huddy *et al.* (2002, p. 426) show that 9/11 'increased the willingness [of Americans] to forgo civil liberties, boosted confidence in the government's ability to prevent terrorism in the United States, and increased support for the use of ground troops in attacks against terrorists.'

Using data from a small student sample from the Netherlands, Boomgaarden and de Vreese (2007) suggest that after the murder of Theo van Gogh by a Muslim extremist in 2004, respondents were more likely to see immigrants as a security threat, or to see the religious practice of the migrant communities as a threat to the Dutch way of life (see also Finseraas *et al.*, 2011).

As noted already, Jakobsson and Blom (2014, p. 482) show that Norwegians' 'attitudes toward immigrants became more positive after the [Breivik] attacks. The size of the effect is not large but it is three times as high as the gender gap in attitudes towards immigrants.' Wollebæk *et al.* (2012, p. 32) use data from two web surveys and show that after the Breivik attacks there was an increase in Norway in 'interpersonal and institutional trust as well as a modest increase in civic engagement, especially among youth.'

The impact of terrorist attacks can be felt far beyond the country in which they took place. Noelle-Neumann (2002) reports that after 9/11 Germans became more supportive of new legislation to limit immigration (see also Schüller, 2016). Åslund and Rooth (2005, p. 605) show that Swedish public opinion towards immigrants became more negative after 9/11. Legewie (2013) compares European Social Survey (ESS) respondents interviewed a week after the 2002 Bali attacks with those interviewed in the 30 days previously. He shows that in Portugal, Poland and Finland, post-attack ESS respondents were significantly more negative about immigrants. Furthermore, using a multilevel model, he shows that the impact of the Bali bombings on attitudes is more pronounced in regions where unemployment rates were rising, and less pronounced if the respondent had personal contact with migrants. Because the timing of the Bali attacks was exogenous to the survey, Legewie argues that the attacks were a natural experiment, and the pre-attack/post-attack comparison can be interpreted as a measure of the causal impact of the Bali attacks on public opinion. Overall, there is considerable evidence that terrorist attacks could change public opinion about ethnic minorities and immigrants.

In our present case, when the news of the attacks first broke on 22 July 2011, many Norwegians thought that they were jihadist atrocities. Indeed, there were initial reports of abuse and harassment of Muslims in Oslo (Bangstad, 2014, p. 12). But as the true nature of the attacks and the identity of the perpetrator soon became clear, the overall tone of the public response,

as articulated by national and political leaders, apart from sorrow, was one of solidarity, love, and tolerance. For example, in a televised spreech, King Harald VII said that '[t]he tragedy has reminded us of the basics that connect us in our multicultural and diverse society' (Bangstad, 2014, p. 10). The prime minister, Jens Stoltenberg, called for 'more democracy, more openness' (Office of the Prime Minister, 2011). Overall, as Friedman (2011) observed, in their response to the attacks 'Norwegians appear to have bypassed bloodlust and vengeance'. As noted above, opinion polls suggest that Norwegians became more positive about immigrants after the attacks (Jakobsson and Blom, 2014). So it is quite possible that the Breivik attacks have, to some degree, discredited xenophobia and Islamophobia. It is likely that a good deal of discrimination against outgroups is driven by prejudice (Becker, 1971) and/or stereotypes (Devine, 1989; Greenwald and Banaji, 1995). If the observed attitudinal change reflects a shift in the underlying prejudice or stereotypes, we might expect corresponding changes in discriminatory behaviour.[2]

Prima facie evidence of the behavioural impact of the Breivik attacks can be seen from the electoral support for the anti-immigration Progress Party (*Fremskrittspartiet*), of which Breivik was once a member. Specifically, it won 18.5% of the votes in the municipal elections in 2007. Two months after the Breivik attacks, its vote share in the September 2011 municipal elections dropped to 11.8%. Similarly, the Progress Party won 22.9% of the vote in the 2009 parliamentary elections, falling to 16.3% in 2013.[3]

Having said that, electoral fortune is determined by multiple factors. And casting a vote in elections is not the same as hiring someone to work in your firm. So questions on the labour market impact of terrorist attacks remain. In particular, the following considerations are relevant. First, 9/11,

[2]Other than prejudice and stereotypes, discrimination might also be based on information. This is the argument of statistical discrimination, in which it is rational for employers to make hiring decisions on the basis of, say, the mean productivity level of different groupings when the productivity of individuals is unobserved (e.g. Arrow, 1973). But there is little reason to think that terrorist attacks would change the unobserved productivity of ethnic minorities or the employers' belief about their productivity. So any impact of terrorist attacks on employer's behaviour probably works through taste-based discrimination.

[3]Source: `www.aardal.info/k_f_valg.pdf` and `www.aardal.info/svalg.pdf`. There is also evidence that terrorist attacks might change demographic behaviour. For example, Nakonezny *et al.* (2004) argue that the 1995 Oklahoma City bombing lowered divorce rates in Oklahoma, and Rodgers *et al.* (2005) argue that the same event raised fertility rates in Oklahoma. Similarly, Hansel *et al.* (2011) argue that 9/11 reduced the divorce rates in the 62 counties in New York. Also, Gautier *et al.* (2009) argue that the murder of Theo van Gogh led to greater residential segregation by ethnicity and a drop in house price in neighbourhoods in Amsterdam where Moroccans and Turks account for more than 25% of the residents.

the bombings in Bali and Madrid, and the murder of Theo Van Gogh, etc. were carried out by jihadists purporting to act in the name of Islam. It is perhaps unsurprising that these acts could reinforce existing prejudice against immigrants and minorities, especially Muslims. But could a far-right terrorist attack have the opposite effect? Lieberson (1985, p. 69) argues that many causal processes are asymmetrical and it is fallacious to think that '[i]f a change in X makes Y change in a certain direction, then surely the opposite change in X would generate an opposite change in Y.' So it is unclear whether Breivik's anti-muslim, anti-migrant attacks have really reduced xenophobia or Islamophobia in Norway.

Secondly, how durable are the changes? Public opinion can be quite fickle, and the impact of terrorist attacks might not last very long (Sorrentino and Vidmar, 1974). Huddy *et al.* (2002, p. 426) observe that 'the power and impact of the [9/11] attacks declined somewhat over time.' Hopkins (2010, p. 51) uses panel data to show 'marked but shortlived effects of September 11' on attitudes regarding migrants. In a similar vein, Jacobs *et al.* (2011, p. 341) show that 'complaints about antisemitism in Belgium indeed showed a statistically significant increase during the Israeli military operation Cast Lead ... [but] this effect was not lasting and wore off after a couple of weeks.' Clearly, transient changes are of less social significance than long-lasting ones. Most of the studies cited above use data that cover just a few weeks (in some cases, days) before or after a terrorist attack. It would be important to evaluate the impact of terrorist attacks over a longer period.

Thirdly, talk is cheap. Self-reported attitude may be inconsistent with actual behaviour. For example, Pager and Quillian (2005) interviewed a sample of employers about their willingness to hire ex-offenders and black people. They also carried out a field experiment on the same employers with audit-pairs. It turns out that '[e]mployers who indicated a greater likelihood of hiring ex-offenders in the survey were no more likely to hire an ex-offender in practice. Furthermore, although the survey results indicated no difference in the likelihood of hiring black versus white ex-offenders, audit results show large differences by race' (Pager and Quillian, 2005, p. 355).

The findings of Pager and Quillian (2005) illustrate the perils of social desirability bias in survey research. But the bias could go the other way too. For example, LaPiere (1934) travelled across the US in the 1930s with a Chinese couple. He reported that 'in only one out of 251 instances in which we purchased goods or services [from restaurants and hotels] necessitating intimate human relationships did the fact that my companions were Chinese adversely affect us' (LaPiere, 1934, p. 233). Six months later, he sent a questionnaire to the hotels and restaurants visited, asking them to indicate whether they would accept Chinese guests. Over 90% replied no. In other

words, the very widespread antipathy and discriminatory attitudes towards Chinese in the US in the 1930s were rarely matched by actual discriminatory behaviour, at least of the kind studied in that paper.

The upshot is that, without further behavioural evidence, we cannot assume that terrorist attacks have the same impact on discriminatory behaviour as they have on public opinion about immigrants. The evidence on the labour market impact of terrorism is, in fact, quite mixed. For example, Dávila and Mora (2005, p. 587) analyse data from the American Community Survey and report that 'Middle Eastern Arab men and Afghan, Iranian, and Pakistani men experienced a significant earnings decline relative to non-Hispanic whites between 2000 and 2002.' Similarly, Rabby and Rodgers III (2010, p. 1) argue that in Britain there was 'a 10 percentage point decrease in the employment of very young Muslim men relative to non-Muslim immigrants after the [2005] London bombings.'[4]

Kaushal *et al.* (2007), however, report a more ambiguous picture. They analyse data from 1998–2004 Current Population Surveys and note that 'September 11th did not significantly affect employment and hours of work of Arab and Muslim men, but was associated with a 9–11 percent decline in their real wage and weekly earnings, with some evidence that this decline was temporary' (Kaushal *et al.*, 2007, p. 1).

Furthermore, Braakmann (2010, p. 430) analyses data from the Labour Force Survey and argues that 'the wages, hours worked and employment probabilities of Arab men [in the UK] were unchanged by the [2005 London] attacks' (see also Braakmann, 2009). Finally, as noted above, Åslund and Rooth (2005) use survey data to show that Swedes became more negative about immigrants after 9/11. In the same paper, they also use Swedish register data to examine unemployment exit rates and find no difference between ethnic groups, i.e. there is 'no sign of increased discrimination toward these minorities' (Åslund and Rooth, 2005, p. 603).

To sum up, the labour market impact of terrorist attacks is unclear. We might add that previous research in this area is based on observational data. In this paper, we address this unresolved question with systematic evidence from two randomised field experiments.

2 The Norwegian context

Norway has seen a steady flow of net immigration since the 1970s. First and second generation immigrants accounted for 1.5% of the population in 1970,

[4]See also Rabby and Rodgers III (2009) for comparable results regarding the labour market impact of 9/11 on young Muslim men in the US.

rising to 11.4% in 2010 (Andreassen and Dzamarija, 2011, p. 16). The scale and the nature of immigration to Norway as well as the country of origin of the migrants have changed over time. In particular, with the enlargement of the European Union in 2004, Poles have become the largest migrant group. The largest non-European migrant groups are the Iraqis and the Somalis who came to Norway as refugees over the past decade.

But the most established non-European migrant community in Norway is the Pakistanis. Young Pakistani men first came to Norway as guest workers in the late 1960s. They were soon joined by their family, and many had brought up children in their adopted country. Indeed, second generation Pakistanis is the largest group of Norwegians born to immigrant parents (Andreassen and Dzamarija, 2011, p. 22, Table 2.1.1); the vast majority of whom attended school in Norway. And since many Norwegian-born children of Pakistani migrants are now in their 20s and 30s, they are either economically active or about to enter the labour market. Andreassen *et al.* (2013, p. 16) report that '83 per cent of the immigrants with a background from Pakistan [live] in Oslo and Akershus [the county neighbouring Oslo], and 67 per cent in Oslo alone.' Oslo itself is quite a diverse city: 27% of its residents are first or second-generation immigrants of one or another heritage (Andreassen and Dzamarija, 2011, p. 15).

Compared to other Europeans, Norwegians are relatively positive about immigration. Based on an analysis of data from the 2002 European Social Survey (ESS), Blom (2011b, p. 151) places Norway 'in the liberal or "immigrant-friendly" third of the participating countries in the European Social Survey' (see also Sides and Citrin, 2007). The ESS module on attitudes towards immigration has been repeated in 2014. Using this new data set, Heath and Richards (2016, p. 1, Figure 1) show that Norway ranks third out of twenty (after Sweden and Denmark) in the proportion of respondents who think that their country is made a better place to live as a result of migration. Analysing survey data collected by Statistics Norway, Blom (2011a, p. 133) reports that in 2009 '[s]even out of 10 [Norwegians] appreciate immigrants' culture and labour efforts and believe that labour immigration from non-Nordic countries makes a positive contribution to Norwegian economy.' Moreover, about 90% think that 'all immigrants should have the same job opportunities as Norwegians' (Blom, 2011a, p. 135, Tables 6.1 and 6.2). Having said that, the same survey also shows that '[t]hree out of 10 suspect that immigrants abuse the social welfare system, and 1 out of 3 believe immigrants represent a source of insecurity' (Blom, 2011a, p. 133).

Immigrant children or children born to immigrant parents achieve lower scores in most national tests (Nygård, 2011, p. 50). They also have lower educational attainment. In 2009, '40 per cent of the population aged 16

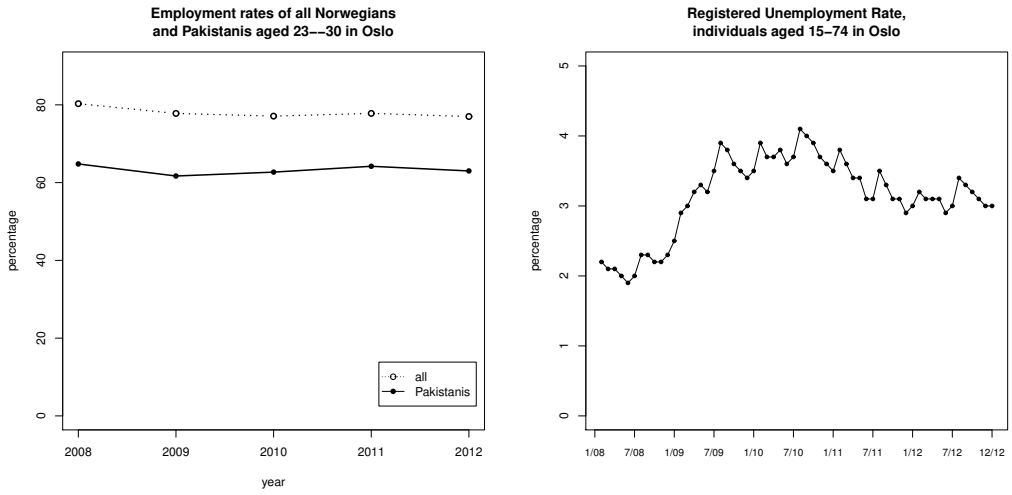

Figure 1: Labour market condition in Oslo, 2008–2012

years and older had attained upper secondary education, and 25 per cent had attained tertiary level of education (Nygård, 2011, p. 59). For first-generation immigrants of the same age, the corresponding figures were 17% and 18%. As regards second-generation migrants, 30% had upper secondary qualifications, and 17% had tertiary qualifications.

Immigrants also have lower level of employment than the population at large, though there is large variation by country of origin. In particular, the employment rates of migrants from the new EU countries in Eastern Europe are comparable to the overall rate, while the employment rates of Somalis, Iraqis, and Afghans, who came to Norway mainly as refugees, are lower. Pakistanis also have a relatively low employment rate. But two features of their employment pattern stand out. First, there is a very large gender gap, with Pakistani men being twice as likely to be economically active as Pakistani women (61% vs 31%). Secondly, Pakistanis have the highest rate of self-employment of all ethnic groups in Norway (Olsen, 2011).

It is also relevant to note that although the global economy has been in turmoil since the 2008 financial crisis, the Norwegian labour market has been relatively robust and stable over this period. The left panel of Figure 1, which is based on register data supplied to us by Statistics Norway, shows the employment rate of young people (aged 23 to 30) in the Oslo area between 2008 and 2012. It can be seen that there was an initial drop from 80% in 2008 to 78% in 2009. Since then the overall employment rate of young people in Oslo fluctuated narrowly between 77% and 78%. As noted already, immigrants of non-Western origin tended to have lower employment rates. The

left panel of Figure 1 shows that the employment level of young Pakistanis in Oslo, while also broadly stable during this period, was consistently about 15% below the overall rate.

The right panel of Figure 1, which plots the monthly unemployment rate in the Oslo metropolitan area between 2008 and 2012, offers an alternative take on the condition of the labour market.[5] It can be seen that in the first half of 2008 unemployment in Oslo was at a very low level, at about 2%. The unemployment rate then rose sharply, reaching a peak of about 4% in mid-2010. This was twice as high as the pre-crisis level, but still relatively modest by international standard. Since then the unemployment rate has been falling, stabilising at about the 3% level in 2012. Recall that our two experiments were conducted in 2010 and 2011 respectively. This means that the first experiment took place at a time when the labour market was relatively loose, and the second experiment was conducted while the labour market was tightening.

3 Field experiment procedure and analytical strategy

Our data come from two identical randomised field experiments that we conducted in Oslo. They were run from September to November 2010 (i.e. some eight to ten months before the Breivik attacks) and October to December 2011 (i.e. three to five months after the attacks) respectively. Because the two experiments were conducted at roughly the same time of the year, seasonal variation of the labour market is, by design, controlled for. But note that we do not have data from the immediate aftermath of the Breivik attacks. This is regrettable, though, given the logistical challenge of organising fieldwork at short notice, quite unavoidable.

In the first experiment, we sampled 450 job openings and sent out 900 fictitious CVs and cover letters. In the second experiment, we sent out 556 fictitious applications in response to 278 job postings.[6] In both experiments, we sampled job vacancies posted on the main recruitment websites in Norway.[7] In principle, all job openings in the Oslo area were included

[5]The unemployment rates are based on the number of unemployed persons registered at the employment office. Unfortunately, this publicly downloadable time series (see `www.ssb.no/statistikkbanken`, series 04471) refers to individuals aged 15 to 74 and it is not broken down by ethnicity.

[6]Both field experiments are approved by The National Committee for Research Ethics in the Social Sciences and the Humanities (NESH).

[7]The two websites were `www.finn.no` and `www.nav.no`.

in the study, provided that they fell in the following six broad categories: 'health service', 'accounting and insurance', 'public consultants', 'primary and preschool teaching', 'transport and storage', and 'ICT and communication'. But we limit ourselves to one job opening per employer, except for large organisations which have separate recruitment personnel for different departments.

We included in our experiments low-skilled and semi-skilled jobs (e.g. warehouse workers and auxiliary nurses) as well as jobs that require higher education (e.g. nurses, teachers, financial accountants, and public consultants). In line with the prevailing pattern of occupational sex segregation, the fictitious applicants for openings in female-dominated occupations (e.g. nurses or social workers) were female. The opposite holds for openings in male-dominated occupations (e.g. transportation and warehouse workers). Where the occupations are gender-integrated (e.g. financial services, teaching, and public administration), the fictitious applicants could be either male or female.

The fictitious job applicants were all 25 years old. Within each pair, the two applicants have the same gender, educational credentials and work experience. We wrote their CVs and cover letters, in fluent Norwegian, in such a way that they always meet the formal requirements listed in the job advertisements. Furthermore, the CVs report educational qualifications earned in Norwegian schools and colleges and work experience in Norwegian firms, indicating to the employer that the minority applicant was either born in Norway or migrated to Norway at a young age. To avoid suspicion, the two CVs and cover letters within each pair have different fonts, and the qualifications are listed in different order. The only material difference between the two applicants was their name, which was either a typical Pakistani name or a typical Norwegian name.[8] Because the names were randomly assigned to the documents, there should not be any systematic difference, observed or otherwise, between the two groups of applicants except their ethnicity as signalled by their names. As a result, any significant between-group difference in call-back rate can reasonably be interpreted as evidence of discrimination.[9]

The CVs and cover letters were sent from the applicants' email addresses or uploaded to the recruitment websites. Each applicant had a real cell phone number and email address, as well as a fictitious postal address, and these contact details were listed on their CV. Employers contacting the applicants

[8]Typical Pakistani names used in the experiment were Kamran Ahmed, and Saera Rashid; typical Norwegian names were Andreas Hansen and Ida Johansen.

[9]Field experiments, especially audit studies in which pairs of actors are sent to attend interviews, are not without their critics, see e.g. Heckman (1998) and Neumark (2012), and see Pager (2007) for a response.

on cell phones were directed to a personal voice mail message which, in fluent Norwegian, stated the name of the applicant and encouraged the caller to leave a message with contact information. The research team monitored all voice mails and emails at least once a day. When the applicants received invitations for job interviews, the responses were carefully registered. After registration, the call-backs were politely declined by text message or email (depending on how the employer contacted the applicants).[10]

Importantly, a call-back is not necessarily a job interview offer. For example, employers may want to let the applicants know that they are not suitable for a job. To avoid potential misinterpretation, employers who left an ambiguous message asking the applicant to call back received a text message or an email to determine whether the call was really a job interview offer. In the vast majority of cases, this was indeed the case, and the employers' response was registered appropriately.

Our analysis proceeds in two steps. First, we measure the size of the ethnic gap in call-back rates *within* each experiment; then we compare the magnitude of the ethnic gap *between* the two experiments. Since there was no randomisation between experiments, we have to rely on the assumption that the two experiments were identical except for their timing, i.e. one being conducted before the attacks, the other afterwards. This is akin to the parallel slope assumption of differences-in-differences (DD) approach. Whether this is a reasonable assumption or not is of course a matter for debate. We are not aware of other major event, policy initiative, or social change during the period concerned that would invalidate the parallel slope assumption. The one possible exception is that the second experiment took place in a slowly and slightly improving labour market (see discussion above). Since the level of discrimination in the labour market is likely to be higher during recessions (Johnston and Lordan, 2014), the implication of the relatively benign macroeconomic context of the second experiment, if any, is that it biased towards finding reduced discrimination.

4 Results

Table 1 reports some descriptive statistics of the two experiments. By design, exactly one half of the fictitious applicants had Norwegian names and the other half had Pakistani names. In both experiments, about half of the fictitious applicants were women, and about 70% of the applications were

[10]Because the mail addresses were fictitious, any attempts by employers to contact the applicants by post could not be recorded, but earlier research has suggested that very few employers contact applicants by post (Bertrand and Mullainathan, 2004, p. 997).

Table 1: Descriptive statistics (percentages)

	2010	2011	overall
Norwegian	50.0	50.0	50.0
Pakistani	50.0	50.0	50.0
female	47.6	51.1	48.9
male	52.4	48.9	51.1
public sector	70.0	71.4	70.5
private sector	30.0	28.6	29.5
health service	10.7	13.7	11.8
accounting & insurance	27.3	24.8	26.4
public consultant	8.0	9.4	8.5
primary/preschool teaching	19.1	15.5	17.7
transport & storage	12.2	16.6	13.9
ICT & communication	22.7	20.1	21.7
call-back	32.3	45.1	37.2
no call-back	67.7	54.9	62.8
N	900	556	1,456

sent to public sector employers. The distributions of openings by industrial sector were broadly comparable across the two studies. But the most notable thing of Table 1 is that the overall call-back rate was considerably higher in 2011 (45%) than in 2010 (32%), possibly reflecting the falling unemployment rate during the second fieldwork period (see the right panel of Figure 1).

Figure 2 reports the bivariate associations between call-back rate and the covariates. In both 2010 and 2011 applications sent ostensibly by female applicants or those sent to private sector employers were more likely to get a call-back. Call-back rates also varied widely by industrial sectors. In 2010, 72% of the applications for openings in teaching, but only 8% of those for openings in public consultancy, resulted in a job interview offer. Figure 2 also shows that, with the exception of 'transport and storage' the rank order of the call-back rates by industry was the same in both years.

Let us turn to the ethnic gap in call-back rate. In the first experiment, 38% of applicants with Norwegian names received a call-back, compared to 26% of applicants with Pakistani names. This 12% gap represents strong evidence against the null hypothesis of no ethnic difference in call-back rates ($s.e.$ = .031, z-score=3.78, p = .0002, two-tailed test). Put differently, on average, white Norwegians need to apply for three jobs to get a job interview. The corresponding number for Pakistanis is four. This is evidence of ethnic discrimination at this initial stage of the hiring process. It is worth comparing

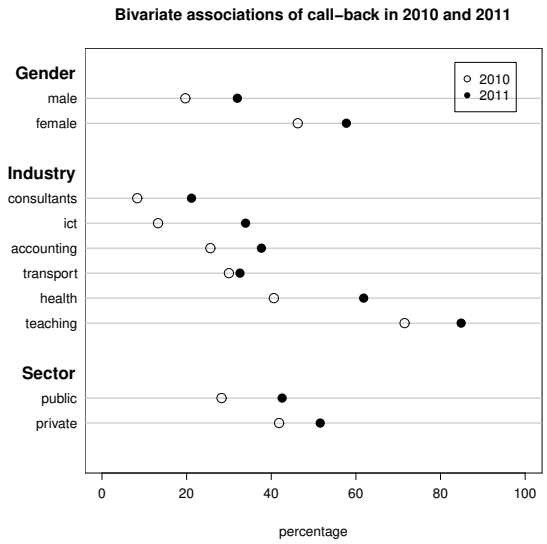

Figure 2: Call-back rates in 2010 and 2011 by covariates

our results with those of Bertrand and Mullainathan (2004, p. 998) who show that in Boston and Chicago 'a White applicant should expect on average one callback for every 10 ads she or he applies to; on the other hand, an African-American applicant would need to apply to about 15 different ads to achieve the same result.'

As noted above, the call-back rate in the second experiment was considerably higher for both groups of applicants, with 51% of those with Norwegian names and 40% of those with Pakistani names getting a call-back for job interview. Again, this gap of 11% speaks strongly against the null hypothesis of no ethnic difference in 2011 ($s.e.$ = .042, z-score=2.64, p = .0008, two-tailed test). Figure 3 reports these results graphically. It is clear that in both experiments applicants with Pakistani names were at an disadvantage compared to those with Norwegian names.

Columns 1 and 3 of Table 2 report the regression coefficients of a linear probability model. The dependent variable is whether the applicant was called back for a job interview. In the two studies, having a Pakistani name lowers the call-back rate by 12 and 11 percent respectively. This is of course the same results reported above.[11] Columns 2 and 4 of Table 2 then show

[11]We use robust standard errors in these regressions to take into account the fact that we sent two applications to each job vacancy. We also obtain broadly the same results if we fit the data with probit models rather than linear probability models. Details are available from the authors on request.

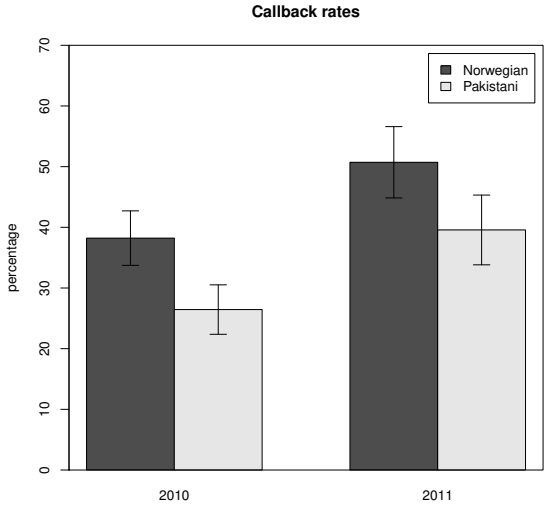

Figure 3: Callback rates (with 95% confidence intervals) for Norwegians and Pakistanis in 2010 and 2011

that controlling for the other characteristics of the applications (i.e. the six industry categories, the gender of the fictitious applicant, and whether the job was in the public or private sector) does not change the estimated adverse effect of having a Pakistani name.

For our present purpose, the question of interest is whether job applicants with Pakistani names experienced less discrimination in the wake of the Breivik attacks. As noted earlier, the Norwegian–Pakistani gap in callback rate of the two experiments were 12% in 2010 and 11% in 2011. Given how similar these figures are, it seems fair to say that not much has changed. We can compare the gaps more formally by pooling data from the two experiments. Column 5 shows that, averaging over the two experiments, compared with applicants with Norwegian names, those with Pakistani names were about 12% less likely to be offered a job interview. It also shows that call-back rate was about 13% higher in 2011 compared to 2010. Column 6 includes an interaction term between having a Pakistani name and being in the 2011 experiment. This is the key parameter gauging whether the level of discrimination was different in the two experiments. As it turns out, there is no evidence at all that this parameter is significantly different from zero. In other words, there is no support for the view that, post-Breivik, the level of ethnic discrimination in Norway has dropped. In column 7, we further show

Table 2: Regression coefficients of linear probability models predicting the probability being called back for job interview

	2010		2011		pooled 2010/2011		
	1	2	3	4	5	6	7
Pakistani	−.117**	−.117**	−.111**	−.111**	−.115**	−.117**	−.117**
	(.017)	(.017)	(.025)	(.025)	(.014)	(.017)	(.017)
2011					.128**	.124**	.133**
					(.033)	(.037)	(.034)
Pakistani ×						.006	.006
2011						(.030)	(.030)
male		−.034		−.084			−.052
		(.051)		(.077)			(.043)
private sector		.008		−.085			−.025
		(.054)		(.067)			(.042)
accounting &		−.126		−.252*			−.176**
insurance		(.086)		(.101)			(.065)
public		−.308**		−.364**			−.333**
consultant		(.085)		(.111)			(.068)
primary		.309**		.226**			.272**
preschool		(.079)		(.086)			(.059)
transport &		−.066		−.262*			−.153
storage		(.103)		(.125)			(.079)
ICT &		−.236*		−.248*			−.248**
communication		(.093)		(.116)			(.072)
constant	.382**	.459**	.507**	.727**	.381**	.382**	.515**
	(.022)	(.075)	(.030)	(.080)	(.022)	(.022)	(.057)
R^2	.015	.218	.012	.180	.030	.030	.211

* $p < .05$, ** $p < .01$; reference category for industrial sectors is 'health service'; standard errors in parentheses.

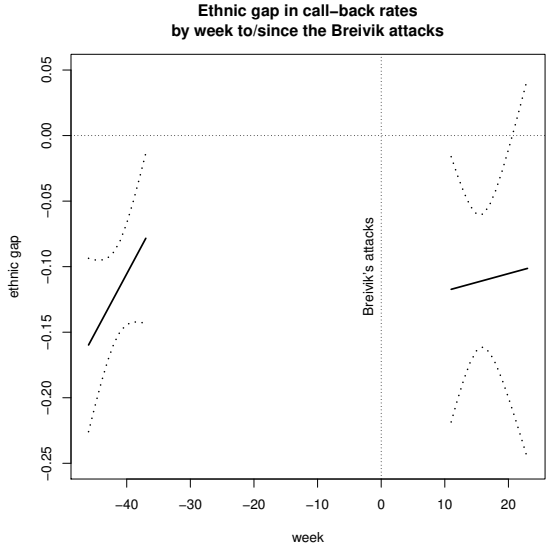

Figure 4: Ethnic gap in call-back rates by the number of weeks to/since the Breivik attacks

that controlling for covariates does not change this result.[12]

Finally, we explore whether the size of the ethnic gap in call-back rates changes over time *within* experiments. To do so, we add to model 2 (model 4) a term that measures the number of weeks to (since) the Breivik attacks when the relevant applications were submitted, and a linear interaction term between the time gap and ethnicity. As it turns out, for both 2010 and 2011, there is no evidence that the size of ethnic gap changes over time (parameter estimates not shown in Table 2, but details are available on request). Figure 4 reports these models with the time trend term graphically. The very wide 95% confidence intervals reflect the finding that we really cannot reject the null hypothesis of no time trend in the ethnic gap, especially for the second experiment.

5 Summary and discussion

In this paper, we compare the results of two identical field experiments on ethnic discrimination in hiring conducted in Oslo in the fall of 2010 and

[12]We have repeated the analysis for sub-samples stratified by gender, by industry, and by sector (public vs private), and have obtained essentially the same result, i.e. no interaction effect between having a Pakistani name and being in the second experiment, see Appendix A.

2011. In the two experiments, applicants with a typical Pakistani name were 12% and 11% respectively less likely to get a job interview than those with a typical Norwegian name. This is clear evidence of ethnic discrimination at this initial stage of hiring in liberal and immigrant-friendly Norway. But there is no evidence that the level of discrimination against Pakistanis was lower in 2011 than in 2010, despite survey findings that Norwegians had become more positive about immigrants after the Breivik attacks. Recall that the second experiment took place in the context of a steadily improving labour market where we might expect to find less discrimination. So the persistence of the ethnic gap in callback rates is indeed quite remarkable.

How do we understand the results? Because Breivik is a far-right terrorist, the atrocities that he perpetuated were, in one sense, quite different from most of the other terrorist attacks considered in the literature. It might be the case that jihadist attacks could reinforce existing xenophobia or Islamophobia and lead to higher levels of discrimination against minorities or immigrants; but a far-right attack is not powerful enough to overcome existing prejudice and discrimination. While this is a possible interpretation, existing evidence is not entirely consistent with it. As noted above, Åslund and Rooth (2005) and Braakmann (2009, 2010) also fail to find evidence of adverse impact of jihadist attacks on the labour market situation of minorities.

A second possibility is that the Breivik attacks had led to some behavioral changes. But these changes were very shortlived, and after a couple of months, the level of discrimination against Pakistani applicants had gone back up to the pre-attack level. As we do not have data from the immediate aftermath of the Breivik attacks, the validity of this interpretation cannot be tested. However, if this interpretation is correct, the social significance of the Breivik attacks should *not* be overstated. Afterall, its impact had fizzled out within two months.

Thirdly, it is possible the case that the Breivik attacks had brought about relatively lasting *attitudinal change* regarding ethnic minorities in the general population, but this has not (yet) percolated through to the hiring *behaviour* of employers or HR managers. In their review of the literature on the attitude–behaviour link, Fazio and Roskos-Ewoldsen (2005, p. 44) note that 'the answer to the question "Is there a relation between attitudes and behavior?" is a resounding "sometimes".' They point out that the strength of the attitudes–behaviour link depends on a host of factors, including how attitudes and behaviour are specified, the strength of the relevant social norms, whether the attitude is based on direct experience, the time pressure individuals were put under when they acted, and so on.

In showing significant attitude–behaviour inconsistency, the results of this paper echo those of Pager and Quillian (2005) and LaPiere (1934). It is

important not to draw overhasty conclusions about how terrorist attacks might have behavioural impact on the majority–minority relationships in society.

Finally, it is possible that persistent discrimination in the labour market might change the behaviour of ethnic minority job seekers. For example, they might choose to go into self-employment or into sectors that they believe to be less discriminatory (e.g. the public sector) in order to avoid prejudiced employers. This would then change the nature of the applicants pool and, in turn, might affect employer's behaviour. Clearly, this is a long and not very well-understood chain of possible social changes. We would argue that these social forces, if they are at work, would take some time to work through the system. It is, for example, not straightforward for individuals to change the sectors that they work in. For this reason, it will be hard to detect such changes by comparing two experiments that are one year apart. In any case, the present paper is about employers' behaviour rather than job seeker's behaviour. So it is beyond the scope of this paper to address this question in detail. Having said that, job seekers' behaviour is certainly an interesting question that calls for further investigation (see e.g. Pager and Pedulla, 2015).

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A Analyses of stratifed sub-samples

We repeat model 7 of Table 2 for sub-samples stratified by gender, sector (public vs private), and industry respectively. The results are reported in Table 3. We report only three parameters of interest, i.e. minority status, year and an interaction term of the two. These models control for other relevant covariates. That is to say, in the analyses by gender, we control for sector and industry, and in the analyses by sector, we control for gender and industry, and so on. In all but one sub-samples, the main effect of minority status is negative and significant. The exception is the sub-sample for public consultants where small N leads to estimation problem. The main effect for 2011 is positive and often statistically significant. But the important point to note is that there is no evidence for an interaction effect in all sub-samples.

Table 3: Analyses by gender, sector and industry

	parameter	β	*s.e.*	N
female	Pakistani	-.367**	.070	712
	2011	.492**	.150	
	interaction	.021	.120	
male	Pakistani	-.429**	.095	744
	2011	.310*	.142	
	interaction	.123	.149	
public	Pakistani	-.501**	.078	1,027
	2011	.385**	.118	
	interaction	.104	.118	
private	Pakistani	-.140*	.060	427
	2011	.367	.203	
	interaction	.055	.151	
health	Pakistani	-.378**	.134	172
	2011	.601*	.283	
	interaction	-.112	.217	
accounting & insurance	Pakistani	-.494**	.112	384
	2011	.183	.192	
	interaction	.263	.172	
public consultant	Pakistani	.000		124
	2011	.466	.399	
	interaction	-.122	.147	
primary & preschool	Pakistani	-.239*	.099	258
	2011	.501	.295	
	interaction	-.059	.245	
transport & storage	Pakistani	-.709**	.240	200
	2011	.077	.253	
	interaction	-.039	.322	
ICT & communication	Pakistani	-.328*	.136	316
	2011	.680**	.230	
	interaction	.240	.231	