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The Impact of the London Bombings on the Well-Being of Adolescent Muslims*

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Abstract

We exploit the timing of the London bombings of July 2005, coinciding with a large-scale national survey of adolescents, to identify the impact of extremist Islamic terror attacks on the well-being of adolescent Muslims. Our analysis reveals interesting gender differences. We find evidence of a decline in the happiness of Muslim teenage girls after the bombings, which is also accompanied by a rise in expectations of facing discrimination in the labour market. These findings are robust to several falsification tests. However, we fail to uncover compelling evidence of any impact of the bombings on Muslim teenage boys.

Keywords: Depression; expectations of discrimination; happiness; terrorism

JEL classification: *I*10; *I*31; *J*15

I. Introduction

Since 9/11, the impact of extremist Islamic terrorism on the labour market outcomes of Muslims has attracted considerable research attention (see, *inter alia*, Åslund and Rooth, 2005; Dávila and Mora, 2005; Kaushal *et al.*, 2007). This body of work is predicated on the basis that terror attacks increase societal prejudice against Muslims, both nationally and internationally, thus damaging labour market prospects according to a taste-based model of discrimination (Becker, 1957).[1] Indeed, several

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[1]Dávila and Mora (2005) suggest that statistical discrimination could also affect workers from countries with suspected terrorist links, as firms factor in the risk of facing additional costs from increased government workplace inspections.

studies present evidence supporting a link between terror attacks and widespread changes in attitudes towards Muslims. For example, after terror attacks, house prices tend to fall and segregation tends to increase in neighbourhoods with large ethnic populations from Muslim countries relative to other neighbourhoods, which is consistent with an increase in prejudice (Gautier *et al.*, 2009; Ratcliffe and von Hinke Kessler Scholder, 2015). Hate crimes targeted at Muslims also typically spike in the immediate aftermath of terror attacks (Hanes and Machin, 2014; Gould and Klor, 2016) alongside a heightened sense of greater religious prejudice towards Muslims among the general public (Kitchen *et al.*, 2006). Shifting the lens on to Muslims paints a similar picture, with Muslims typically feeling more pessimistic about the receptiveness of natives to foreigners (Elsayed and de Grip, 2018) and immigrants from Muslim countries acknowledging more religious intolerance relative to immigrants from other countries after terror attacks (Goel, 2010). Qualitative evidence from in-depth interviews also reveals that Muslims are keenly aware of increased Islamophobia following terror attacks (Change Institute, 2009a), possibly as a consequence of negative portrayals and misrepresentations of Muslims in some elements of the media (Saeed, 2007; Bleich *et al.*, 2015). Yet despite the apparent changes in attitudes towards Muslims there has been mixed evidence from the labour market as to the economic consequences for Muslims of extremist Islamic terror attacks, with some studies finding an impact on wages (Dávila and Mora, 2005; Kaushal *et al.*, 2007), some finding an effect for specific groups (Cornelissen and Jirjahn, 2012; Rabby and Rodgers, 2012), and others finding little influence at all (Åslund and Rooth, 2005; Braakmann, 2009, 2010; Shannon, 2012). However, some features of the labour market might operate to mitigate the impact of terror attacks on the economic outcomes of Muslims. For example, laws exist to protect minorities against discrimination, and employment opportunities available through ethnic networks might provide a buffer against discriminatory treatment elsewhere in the economy (Elsayed and de Grip, 2018).

In light of evidence to suggest that Muslims face more hostile social environments following extremist Islamic terror attacks, researchers have begun to probe whether these attacks harm outcomes shaped in part by the social context. To date, this line of enquiry has focused on assimilation (Gould and Klor, 2016) and health (Johnston and Lordan, 2012), including the health consequences for newborns of maternal exposure to increased hostility (Lauderdale, 2006). The findings point towards a deterioration in these outcomes for Muslims and individuals from Muslim countries following terror attacks, although Johnston and Lordan (2012) find that negative effects emerge only for physical health and not mental health.

The impact of extremist Islamic terror attacks on younger populations has received very little attention despite growing evidence that formative years matter for adult outcomes (Cunha *et al.*, 2006). If terror attacks ignite societal prejudices and propagate negative stereotypes about Muslims, they could have an adverse effect on the current and future outcomes of adolescent Muslims. Denigration of any form is distressing and demoralizing, and has the potential to have immediate impacts on psychological functioning. However, if negative evaluations of Muslims are internalized and/or used to inform decision-making, the consequences are potentially far-reaching. For example, adolescents might be discouraged from investing in human capital where discrimination pervades the labour market; see Loury (1998) for a discussion of self-fulfilling prophecies, psychological externalities, and other mechanisms curbing educational attainment among minorities. Impaired psychological functioning could also indirectly shape human capital investments by hindering both the ability and incentives to invest in education (Fletcher, 2008), with research exploiting exogenous variation in depressive symptoms suggesting poorer academic performance among teenage girls in particular (Ding *et al.*, 2009; Busch *et al.*, 2014). In addition, psychological health in formative years is also linked to other markers of stability in adulthood, such as relationship prospects and life satisfaction (Smith and Smith, 2010; Goodman *et al.*, 2011; Layard *et al.*, 2014). Finally, ethnic identity is formed during adolescence (Phinney, 1990), with unfair treatment linked to identity adoption (Georgiadis and Manning, 2013), presenting further scope for adverse labour market outcomes (Battu and Zenou, 2010; Nekby and Rödin, 2010; Bisin *et al.*, 2011).

This paper fills a gap in the existing literature by exploiting the timing of the London bombings of July 2005, occurring midway through a large-scale nationally representative survey of adolescents, to examine the impact of extremist Islamic terror attacks on the well-being of adolescent Muslims. We focus on key indicators of well-being that provide a guide to the contemporaneous circumstances of adolescents as well as mapping on to economic and social circumstances in adulthood. For example, happiness and depression provide simple and transparent metrics of well-being that align with public policy concerns in the United Kingdom – the Office for National Statistics monitors emotional well-being in order to measure societal progress. We also examine expectations of facing discrimination, an issue which lends itself to a theoretically established link between discrimination in the labour market and human capital accumulation (see Lundberg and Startz, 1983; Coate and Loury, 1993b; Lang and Manove, 2011). Discriminatory expectations can be seen as one step in a process whereby human capital investments respond to discrimination in the labour market, with surprisingly little known about the formation

of these expectations. By focusing on individuals on the cusp of making economically meaningful decisions, our research examining the impact of terror attacks on the social outcomes of adolescents also ties in with other existing research on labour market outcomes.

To preview our results, we find some interesting differences in the impact of the bombings by gender. Specifically, we find evidence of a decline in the happiness of Muslim teenage girls after the bombings, which is also accompanied by a rise in expectations of facing discrimination in the labour market. These findings emerge consistently throughout a battery of falsification tests. For Muslim teenage boys, there is little evidence to suggest an impact of the bombings on happiness, and at best, tentative evidence of an increase in expectations of facing discrimination. These empirical results, which are based on a systematic analysis of large-scale survey data, are in line with qualitative evidence suggesting that Muslim women have been particularly affected by the increase in Islamophobia following the London bombings, a phenomenon that is attributed to Muslim women being more easily identifiable (Change Institute, 2009a).

II. Related Literature

Our research is related to various areas of research in economics. First and foremost is the literature focusing on the impact of terrorist attacks on the economic and social outcomes of individuals tarred by association – however tenuous – with the perpetrators of terror attacks. Of particular relevance is a body of work that examines the impact of terror attacks on social outcomes. Johnston and Lordan (2012) exploit the 1999 and 2004 editions of the Health Survey for England as providing before and after data for 9/11, and examine changes in mental and physical health among Pakistani and Bangladeshi adult Muslims relative to non-Muslim Indian adults over this five-year period. Interestingly, they find evidence of a deterioration in the physical health of Muslims but little evidence to suggest an increase in psychological distress. Further analysis also points towards an increase in social isolation among Muslims. A similar theme is pursued by Gould and Klor (2016), who examine measures of assimilation among immigrants from Muslim countries living in the United States. They show that rates of intra-ethnic marriage and fertility increased in states that experienced the sharpest increase in anti-Muslim hate crimes following 9/11, suggesting that the Muslim community became more insular in response to increased hostility. Finally, Lauderdale (2006) uses administrative data to examine the impact of 9/11 on the birth weight of children born to Arab women compared with those born to white women in California. The findings suggest an increase in the prevalence of low birth-

weight babies for Arab mothers, which is consistent with increased maternal exposure to stress affecting *in utero* conditions. A smaller body of literature on social rather than economic outcomes partly reflects a lack of suitable data. By their nature terrorist events are unanticipated, making it impossible to gather relevant information on pre-attack conditions via bespoke surveys. Instead, researchers must rely on the availability of routinely collected data, such as administrative data and government-funded surveys. However, administrative data provide information on a limited range of outcomes and individual-level characteristics, while government-funded surveys that collect more extensive information rarely coincide with terrorist events. Surveys of adolescents are particularly scarce; as a result, there are few opportunities to examine the impact of terrorist activities during formative years. To our knowledge, our study is the first to do so. Given that the transition from childhood to adulthood is a key developmental stage, paving the way for later-life success, the lack of evidence for this age group represents a serious gap in the literature.

Our research also resonates with the wider body of discrimination literature examining the wage gap between individuals from ethnic majorities and minorities, and whether this gap reflects labour market discrimination or differences in skills brought to the labour market (see O'Neill, 1990; Neal and Johnson, 1996; Black *et al.*, 2006; Nordin and Rooth, 2009).[2] A difficulty in this endeavour is that, according to the theoretical literature, discrimination in the labour market drives investments in human capital, blurring the distinction between market and pre-market processes. For example, unobserved human capital investments, combined with less informative productivity signals (Lundberg and Startz, 1983) or more pessimistic employer beliefs (Coate and Loury, 1993b), reduce the payoff and incentives for those from ethnic minorities to acquire skills in statistical models of discrimination. Coate and Loury (1993a) arrive at the same conclusion within an employer taste-based model of discrimination where human capital investments are observed, while Fryer and Jackson (2008) draw on insights from social psychology to discuss negative feedback effects in the absence of prejudice or information asymmetries. In a departure from preceding research, where human capital investments are either unobserved or observed, Lang and Manove (2011) argue that at least some aspects of human capital, such as schooling, can be observed and therefore harnessed by those from ethnic minorities to signal higher ability and augment productivity. Thus, in their model of

[2]While this research suggests that conditioning on cognitive test scores substantially reduces the wage gap, what these test scores measure and whether to adjust for schooling (which increases the wage gap) is subject to considerable debate (see Neal and Johnson, 1996; Darity and Mason, 1998; Carneiro *et al.*, 2005; Lang and Manove, 2011).

signalling and statistical discrimination, the incentives operate to increase investments in observable dimensions of human capital. This is also the case in Arcidiacono *et al.* (2010), where higher education reveals rather than signals worker ability that would otherwise remain unobserved at lower education levels.

An absence of data on expectations held at the time of making human capital investment decisions hampers direct tests of the role of expected labour market discrimination in shaping human capital acquisition. Indirect methods to assess expectations with respect to labour market discrimination involve estimating the wage premiums that better cognitive skills attract, though these results suggest that the returns to human capital are just as high – if not higher – for ethnic minorities (Neal and Johnson, 1996; Neal, 2006). Other indirect methods examine expectations in related contexts, such as educational attainment. For example, Carneiro *et al.* (2005) show that, by the age of ten, children from ethnic minorities expect to achieve less schooling. However, as emphasized by the authors, these expectations might reflect circumstances other than labour market discrimination, and notably skills gaps emerge very early on, when children would have a limited understanding of discrimination. We contribute to this debate by examining adolescent expectations of facing discrimination (henceforth discriminatory expectations), and in particular the impact of terror attacks on these expectations. Our expectations data is unique in explicitly addressing anticipated discrimination because of skin colour/ethnicity/religion in non-compulsory education or in the labour market. These two settings capture expected discrimination in pre-market and market contexts. While the theoretical literature concerning the impact of discrimination on human capital investments is largely concerned with discrimination in the labour market, discrimination encountered in the acquisition of skills, for example in accessing (high-quality) education, can be addressed within human capital theory (Welch, 1975). An interesting feature of non-compulsory education in the United Kingdom is that applying to higher education providers is an impersonal process handled by a centralized admissions body, increasing the possibility that assessing discrimination here could be linked to concerns about institutional discrimination in the education sector. However, because labour market discrimination could be fuelled by various groups of individuals (i.e., employers, workers, and customers), assessing discrimination here could reflect broader concerns about discrimination in society. Our research suggests that there could be a link between terror attacks and discriminatory expectations in the labour market, with little evidence to suggest any effect on discriminatory expectations in non-compulsory education. Given the link between extremist Islamic terror attacks and societal prejudice against Muslims, this finding appears consistent with expectations of labour market discrimination responding to

societal prejudice, which lends greater weight to the possibility that such expectations do matter when it comes to shaping decisions.

Finally, our research is linked to a large body of literature on the economics of well-being, which endeavours to understand the factors that influence well-being (for a recent review see Clark, 2018), and a smaller body of literature with a specific interest in the impact of terrorism on societal well-being (Frey *et al.*, 2009; Metcalfe *et al.*, 2011; Romanov *et al.*, 2012; Dustmann and Fasani, 2016). These studies consider the social costs of terror attacks on the general public, arising as a result of the fear, distress, grief, and destruction caused, with evidence generally pointing towards a widespread reduction in well-being. Romanov *et al.* (2012) also consider the impact of terror attacks in Israel on Israeli Arabs, in addition to Israeli Jews, and show that while the life satisfaction of Israeli Jews falls when terror attacks occur in their immediate vicinity, the life satisfaction of Israeli Arabs falls irrespective of the location of the attack. Our research differs from the majority of these studies to the extent that we are concerned with possible adverse effects of terrorism on the well-being of a minority group tarnished by association with the terrorists, as opposed to the consequences for the general public, although we do take note of these findings in our empirical strategy described below. Alongside Romanov *et al.* (2012), our research demonstrates that terror attacks impose greater costs on maligned individuals relative to the general public, which is an issue that warrants greater research attention than it is currently afforded.

III. Empirical Strategy and Data

As existing research demonstrates that well-being declines throughout the population following terror attacks (see Frey *et al.*, 2009; Metcalfe *et al.*, 2011), we implement the difference-in-differences method in order to isolate the impact of an extremist Islamic terror attack on young Muslims. Specifically, we estimate the following equation:

$$Y_i = \beta_0 + \beta_1 treated_i + \beta_2 post_i + \beta_3 treated_i \times post_i + x_i'\gamma + \varepsilon_i, \quad (1)$$

where Y_i is the outcome of interest, $treated_i$ indicates whether the individual i is in the treatment group, $post_i$ indicates whether the individual i is observed after the bombings, x_i is a vector of individual characteristics, and ε_i is an error term. β_1 captures general differences in the outcomes across individuals in the treated and control groups, β_2 reflects general changes in the outcome after the bombings, and β_3 captures differential changes in the outcomes after the bombings for individuals in the treated group. Thus, β_2 captures factors such as the wider social costs of terrorism, the onset of better weather, and – specifically for adolescents – the

winding down of the school year. We are primarily interested in the treatment effect, β_3, which might exist on account of an increasingly hostile social environment faced by Muslims after the bombings. Equation (1) is estimated using ordinary least squares and standard errors are clustered at the school level.[3]

We use data from the Longitudinal Survey of Young People in England (LSYPE; Department for Education and National Centre for Social Research, 2012), which follows a two-stage sampling design, sampling first schools and then students within those schools, with an over-sampling of schools in deprived areas and pupils from Indian, Pakistani, Bangladeshi, Black African, Black Caribbean, and mixed-race backgrounds over-sampled.[4] All adolescents were first interviewed in 2004 in Year 9 (aged 13–14), with annual interviews taking place thereafter until 2010.[5] The LSYPE initially surveyed 15,770 individuals in 2004 (with complete individual and household-level interviews for 13,914 individuals), achieving a sample of 13,539 individuals in the following year (with 11,952 complete interviews). Interviews were conducted in the home, where detailed information was collected from the respondent and from other adults in the household on the attitudes, experiences, and behaviours of the adolescent and the family environment. We use the 2005 cross-section from this panel survey in our main analysis, as this is the first year for which our outcomes of interest are collected, but we also exploit the panel element in subsequent analyses.

To measure emotional well-being, we use the General Health Questionnaire (GHQ), which was administered as part of a self-completed survey in 2005 and 2007. This screening instrument was originally designed to assess mental distress in those aged 16+ (Goldberg, 1972), although evidence suggests that it can be successfully used for younger adolescents as well (French and Tait, 2004). The version of the GHQ in the LSYPE is a 12-item questionnaire addressing problems with self-esteem (having a useful role, confidence, self-worth), ability to function (concentration, decision making, overcoming difficulties, facing up to problems, feeling constantly under strain), and affect (happiness and unhappiness/depression). Each question lists four options that broadly translate as "better/healthier than usual", "same as usual", "worse/more than usual", and "much worse/much more than usual". Although items are evaluated relative to a "usual state",

[3]Using an ordered/binary probit to model the ordinal/binary outcome variables does not affect our main findings.

[4]We do not use weights to adjust for this over-sampling in the analysis. As pointed out by Solon *et al.* (2015), weighting is unnecessary for the consistent estimation of causal effects provided that the sampling is independent of the dependent variable conditional on the explanatory variables, which is plausible in our context.

[5]Repeating a school year is rare in the United Kingdom.

evidence suggests that respondents view their "usual state" as one without symptoms (Goldberg, 1972). Responses to each GHQ item are typically coded 0 for "better/healthier" and 1, 2, and 3 for increasing severity of symptoms, with scores across all items aggregated to produce a GHQ score for mental distress (see Metcalfe *et al.*, 2011). However, an additional "Don't know" option available in the LSYPE makes this approach less attractive, as these responses comprise anywhere between 2 percent and 11 percent of answers to GHQ items, meaning that we can only construct GHQ scores for 78 percent of respondents. Nonetheless, we take advantage of the GHQ items that focus on positive/negative affect: "Have you recently been feeling reasonably happy, all things considered?" with the options "More so than usual", "About the same as usual", "Less so than usual", "Much less than usual", and "Don't know", as well as "Have you recently been feeling unhappy and depressed?", with the options "Not at all", "No more than usual", "Rather more than usual", "Much more than usual", and "Don't know". The item on positive affect is similar to the question on happiness appearing in the General Social Survey[6] that is extensively analysed by economists (see, e.g., Stevenson and Wolfers, 2012). As Headey and Wooden (2004) show that well-being and ill-being are not opposite ends of the same spectrum, and factors affecting well-being might differ from those influencing ill-being, we also consider the item on negative affect ("Have you recently been feeling unhappy and depressed?"). We follow Wolfers (2003) to create a happiness score for increasing happiness (i.e., 1 = "Much less than usual" and 4 = "More so than usual") that retains information on the intensity of symptoms. We adopt this approach because, as shown by the summary statistics presented in Table 1, the proportion of Muslim adolescents responding with "More so than usual" and "About the same as usual" both fall after the bombings, while the proportion responding "Less so than usual" rises. Therefore, combining the responses "More so than usual" and "About the same as usual", and the remaining responses to consider the presence, rather than severity, of symptoms, removes potentially relevant variation in the happiness scores. However, in our robustness analysis we show that our main findings are similar if we instead analyse the GHQ score for mental distress or a binary variable to indicate the presence/absence of symptoms. We construct a depression score for increasing distress in an analogous fashion (i.e., 1 = "Not at all" and 4 = "Much more than usual". For the happiness and depression questions, "Don't know" responses comprise, respectively, 5 and 3 percent of all answers and we exclude these from our analyses.

[6]The General Social Survey collects information on the attitudes and behaviours of the American public since 1972. In this survey, respondents are asked "Taken all together, how would you say things are these days, would you say that you are very happy, pretty happy, or not too happy?".

Table 1. *Summary statistics 2005*

	Muslims	Non-Muslims	Muslims		Non-Muslims	
	All	All	April–June	July–Sept.	April–June	July–Sept.
	(1)	(2)	(3)	(4)	(5)	(6)
Outcomes						
Happiness score	3.21	3.12	3.23	3.18	3.11	3.13
Happiness: Much less than usual	0.03	0.03	0.03	0.03	0.03	0.03
Happiness: Less so than usual	0.08	0.08	0.07	0.09	0.08	0.08
Happiness: About the same as usual	0.55	0.62	0.56	0.54	0.62	0.62
Happiness: More so than usual	0.34	0.27	0.35	0.33	0.26	0.27
Depression score	1.78	1.90	1.78	1.77	1.91	1.89
Depression: Not at all	0.52	0.43	0.51	0.53	0.43	0.44
Depression: No more than usual	0.28	0.33	0.28	0.27	0.33	0.32
Depression: Rather more than usual	0.13	0.15	0.13	0.12	0.15	0.15
Depression: Much more than usual	0.08	0.09	0.08	0.09	0.09	0.09
Expects discrimination in non-compulsory education	0.12	0.04	0.11	0.13	0.04	0.04
Expects discrimination in the labour market	0.17	0.06	0.14	0.20	0.05	0.07
GHQ score	9.14	9.91	9.10	9.18	10.10	9.60
Is less happy	0.11	0.11	0.10	0.12	0.11	0.11
Ethnicity						
Indian	0.10	0.06	0.11	0.09	0.06	0.06
Pakistani	0.41	0.00	0.39	0.44	0.00	0.00
Bangladeshi	0.30	0.00	0.29	0.32	0.00	0.00
Black	0.06	0.06	0.07	0.05	0.06	0.08
Mixed race/other	0.10	0.07	0.11	0.08	0.07	0.08

Table 1. *Continued*

	Muslims	Non-Muslims	Muslims		Non-Muslims	
	All	All	April–June	July–Sept.	April–June	July–Sept.
	(1)	(2)	(3)	(4)	(5)	(6)
Region of residence						
North West	0.03	0.05	0.02	0.03	0.05	0.06
North East	0.17	0.14	0.17	0.17	0.14	0.14
Yorkshire and the Humber	0.16	0.09	0.17	0.15	0.08	0.11
East Midlands	0.04	0.09	0.05	0.04	0.09	0.09
West Midlands	0.13	0.12	0.13	0.13	0.12	0.12
East	0.05	0.11	0.05	0.06	0.11	0.10
South East	0.05	0.16	0.05	0.05	0.17	0.14
South West	0.01	0.09	0.02	0.00	0.11	0.07
Sociodemographics						
Female	0.51	0.49	0.50	0.52	0.49	0.50
Two-parent family (Parent)	0.80	0.75	0.79	0.82	0.76	0.75
ln(siblings+1) (Parent)	1.27	0.80	1.26	1.28	0.80	0.81
Parent(s) aged < 40 (Parent)	0.47	0.39	0.46	0.48	0.39	0.40
Household language not English (Parent)	0.96	0.10	0.96	0.97	0.10	0.11
Thinks religion important	0.84	0.10	0.83	0.84	0.09	0.11
Economic resources						
Parent(s) educated to degree level (Parent)	0.09	0.19	0.08	0.10	0.18	0.19
Homeowner household (Parent)	0.59	0.73	0.58	0.60	0.73	0.71
Parent(s) work (Parent)	0.52	0.87	0.52	0.53	0.88	0.86
Household income over £21,000 (Parent)	0.11	0.45	0.09	0.12	0.46	0.43
Household income missing (Parent)	0.37	0.20	0.41	0.34	0.19	0.21
Good financial situation (Parent)	0.28	0.53	0.29	0.26	0.53	0.52
Bad financial situation (Parent)	0.15	0.06	0.15	0.16	0.05	0.06
N	1,656	10,605	862	794	6,497	4,108

Notes: Regressions include all control variables listed in this table unless otherwise stated. Variables are drawn from 2004, with responses provided by the adolescent unless otherwise indicated.

Measures of discriminatory expectations are available for 2005 and 2008. In 2005 the following questions were asked: "Do you think that your skin colour, ethnic origin or religion will make it more difficult for you to get on in education after Year 11 (for example, in doing A levels or going to university)?" and "Do you think that your skin colour, ethnic origin or religion will make it more difficult for you to get a job after you leave education?", with the responses "Yes", "No", and "Don't know". For both questions, the "Don't know" responses comprise approximately 10 percent of all answers and it is unclear how best to approach this response. For example, "Don't know" could reflect a range of possibilities, such as an inability to process/articulate expectations, noise, or sentiments lying between "Yes" and "No" (i.e., "Maybe"). If "Don't know" responses reflect uncertainty, then these responses could be of interest in their own right, as the bombings could have increased uncertainty as to whether individuals will face future barriers, and any uncertainty over investment returns would also influence investment decisions. As we show in our robustness analysis, however, there is very little impact of the bombings on the "Don't know" responses. We therefore create indicator variables that are equal to one if the respondent thinks their skin colour/ethnic origin/religion will make it harder for them to progress in a given context, and zero otherwise, with "Don't know" responses excluded altogether. As noted earlier, these questions are unique in explicitly addressing anticipated barriers associated with skin colour, ethnic origin, or religion. While we interpret these barriers as a reflection of expected discrimination, there are alternative factors that adolescents could take into account when answering these questions. For example, the majority of British Muslims have a Pakistani/Bangladeshi background and the existence of gender gaps in educational attainment and labour market participation relative to other ethnic communities (see Georgiadis and Manning, 2011) raises the possibility that these barriers could also reflect cultural norms. As we focus on a short window spanning the period just before and after the bombings in our main analysis, we believe it is unlikely that the bombings would have influenced cultural norms within this time period; however, pre-existing differences in cultural norms between those interviewed just before and after the bombings could potentially affect our results. In our robustness analysis, we show that it is unlikely that our results simply reflect pre-existing differences in cultural norms.

The London bombings comprised two terror attacks that took place two weeks apart in July 2005. The initial bombings occurred on 7 July 2005 and targeted the London transport network with devastating consequences. A further set of attacks took place on 21 July 2005, although these bombs failed to detonate. A key feature of the 2005 survey is that the fieldwork spans the five-month period between 18 April and 18 September, with 94

percent of the interviews taking place between 1 May and 31 August. Thus the bombings occurred midway through the survey period, and as interview month and year data are available, we are able to obtain a reasonably clean separation of the pre- and post-treatment periods.[7] Our pre-treatment period spans April–June and our post-treatment period spans July–September. In practice, this means that individuals interviewed in early July were not exposed to a single terror attack, while individuals interviewed in late July were exposed to two terror attacks. With regard to the former scenario, we show in the Online Appendix that, in a difference-in-differences setting with no controls, the estimate of the average treatment effect on the treated can be shown to be biased towards zero by a factor $(1 - p)$, where p is the proportion of respondents incorrectly classified as being observed in the post-treatment period. The likely consequence of the misclassification is that we therefore slightly underestimate the true effect of the bombings. With regard to the latter scenario, it is likely that the treatment effect is larger for those individuals interviewed from late July onwards, although this is not something we can readily test. Although we can examine the treatment effect by month, a concern here is that August coincides with the summer holiday period with the new academic year beginning in September; these events could differentially shape the thoughts and feelings of different groups of adolescents. In our robustness analysis, we show that the treatment effect is similar, and even larger in the case of discriminatory expectations, if we restrict the post-treatment period to July alone.

A key choice when trying to estimate the impact of extremist Islamic terror attacks on the outcome of interest is the designation of treatment and control groups. Owing to data limitations, most research has relied on the country of origin to establish these groups (see Åslund and Rooth, 2005; Dávila and Mora, 2005; Kaushal *et al.*, 2007; Shannon, 2012), and looking Muslim could be the defining attribute as far as treatment is concerned (Åslund and Rooth, 2005). As pointed out by Cornelissen and Jirjahn (2012), however, not all those hailing from predominantly Muslim countries are Muslim, while some Muslims are from predominantly non-Muslim countries, potentially leading to attenuation bias. Consistent with this argument, they find a treatment effect where treatment is assigned on the basis of religion and not by country of origin (although the opposite is found in Goel, 2010). As we are in the fortunate position of having access to data on religious affiliation, we designate our treatment status on the basis

[7]Unfortunately there are no means of obtaining the precise date of each interview. The Centre for Longitudinal Studies (CLS) at the Institute for Education are currently responsible for these data and have informed us that although the interview date would have been collected by the fieldwork agency, it was never included in any transfer agreement and there are no plans to rectify this, so the interview date is not even available from the data deposited with the CLS.

of religion. This means that we compare the change in outcomes among Muslim adolescents, who are almost exclusively non-white,[8] to the change in outcomes among white non-Muslim adolescents and those from ethnic minorities. It also means that some individuals who might look Muslim but are not Muslim (generally non-Muslim Indians) appear in our control group. We include white people in our control group because we find that pre-treatment trends in our outcome variables are most similar between Muslims and white people due to the small sample sizes for non-Muslim ethnic minorities. Other studies, where a longer interval elapses between the before and after period, exclude white people out of a concern that outcomes might evolve differently between Muslims and white people, but our short window of analysis diminishes this concern. As for non-Muslim Indians, there is little consensus in the literature as to whether Indians, who are predominately non-Muslim, should appear in the treatment group (Hanes and Machin, 2014), the control group (Johnston and Lordan, 2012), or be excluded altogether (Kaushal *et al.*, 2007; Braakmann, 2010). In our robustness analysis, we show that the treatment effect is reasonably robust to different designations of treatment status, although statistical precision suffers in smaller samples.

Figures 1 and 2 present monthly means of happiness and depression scores for Muslim and non-Muslim adolescents with confidence intervals attached.[9] Overall, Muslim teenagers are happier and less depressed compared to non-Muslims prior to the bombings, but from July 2005 onwards the happiness gap diminishes and is no longer statistically different, owing to a sharp decline in the happiness of Muslim teenage girls. Expectations of discrimination in non-compulsory education and the labour market are shown in Figures 3 and 4, respectively, with Muslim teenagers more likely to expect discrimination in both settings. Notably, expectations of discrimination in the labour market increase sharply for Muslim teenage girls after the bombings, following a different trajectory to the expectations of other teenage girls, while the increase in expectations for Muslim teenage boys is not out of sync with those of other teenage boys. However, these figures present raw averages that fail to control for differences in the characteristics of Muslim and non-Muslim adolescents over this period. In our regression analysis, we take into account a standard set of control variables, such as ethnicity, region, sociodemographics, and economic resources, which are measured pre-treatment using the 2004 wave of LSYPE data to prevent the possibility of any of the control variables being themselves affected by the treatment. Summary statistics are

[8]Just under 3 percent of Muslim adolescents in our sample are white.
[9]We exclude April and September from these figures due to very small sample sizes.

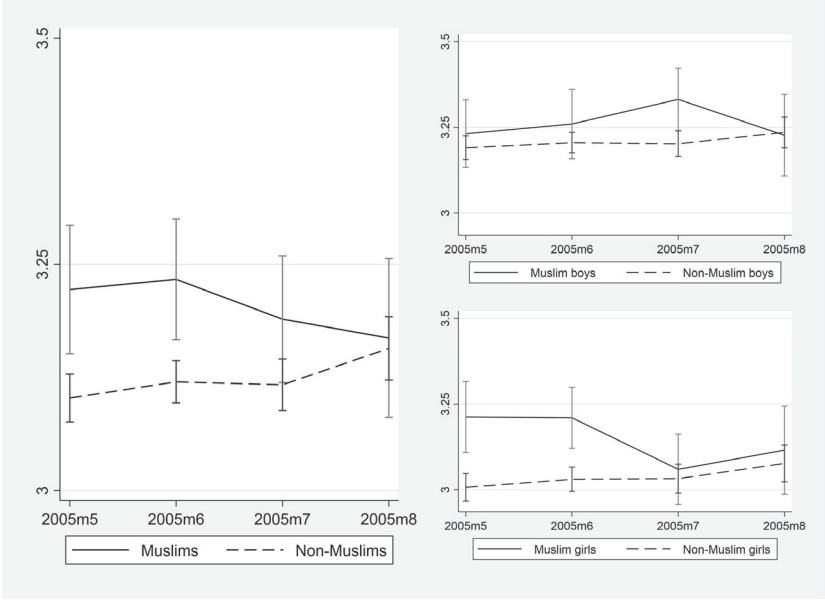

Fig. 1. Happiness scores for Muslim and non-Muslim adolescents

presented in Table 1, which shows that Muslim teenagers have younger but less healthy parents, more siblings, and fewer economic resources compared with non-Muslims.

Our identification strategy relies on the interview month being unrelated to other variables that influence the outcome of interest, or otherwise being able to control for any variables where a correlation exists. We exploit the richness of LSYPE data to determine the likelihood that these conditions are satisfied. Specifically, we examine the difference in the sample means of just under 110 pre-treatment variables, drawn from the 2004 wave of LSYPE data, to establish whether interview month is correlated with individual circumstances. Table A1 in the Online Appendix presents differences in sample means alongside sample sizes for different subgroups of variables such as ethnicity, region of residence, sociodemographic characteristics, economic resources, relationship with parents, attitudes towards school and the future, time use, and risky behaviours.[10] There is some evidence of regional differences in the timing of interviews and, as a result, differences

[10]Note that we have not applied any correction for multiple comparisons, which are likely to be too conservative given the large number of comparisons made, and instead seek to gauge the extent to which patterns emerge.

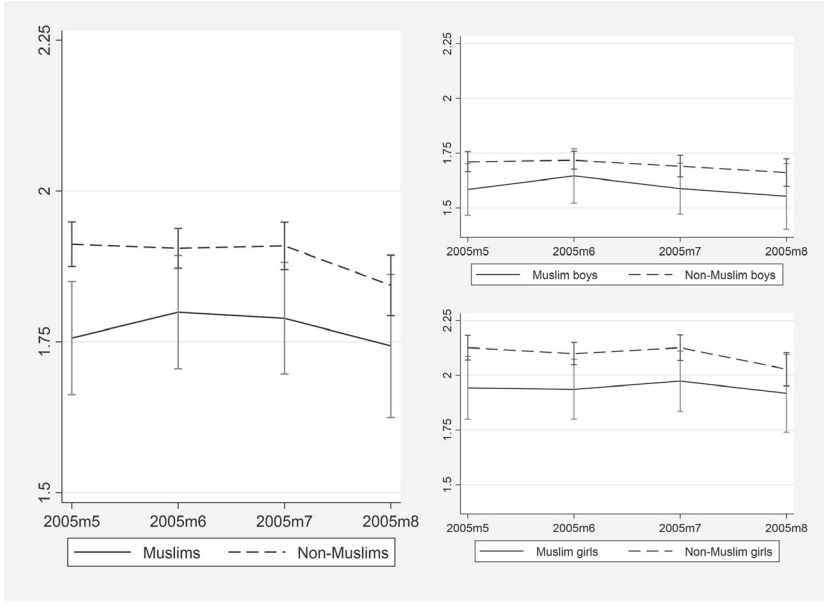

Fig. 2. Depression scores for Muslim and non-Muslim adolescents

in the ethnic composition and economic circumstances of those interviewed post-treatment. In particular, there are differences in terms of household income, with Muslims interviewed after the bombings more likely to have a higher household income and to have provided income details. Other notable differences include time use and parental assessment of adolescent functioning. The overall impression given by Table A1 is that, while most variables are unrelated to the interview month, there does appear to be a correlation for some potentially important variables. In our robustness analysis, we conduct various tests to show that our results are not simply the artefact of differences in sample composition.

A key identification assumption of the difference-in-differences method is that no other contemporaneous events differentially affect the well-being of Muslims as opposed to non-Muslims. Such events might include, for example, religious festivals or exams. A coincidence of several factors suggests that this requirement might be satisfied. Firstly, in 2005 most religious festivals took place outside of our observation window, with Eid-ul-Adha and Easter occurring beforehand, and Ramadan and Diwali afterwards. Although Passover and Vaisakhi occurred in April, our sample for this month is small. Secondly, students in England are tested at the end of Key Stage 3 in Year 9 (aged 13–14) and at the end of Key Stage 4 in

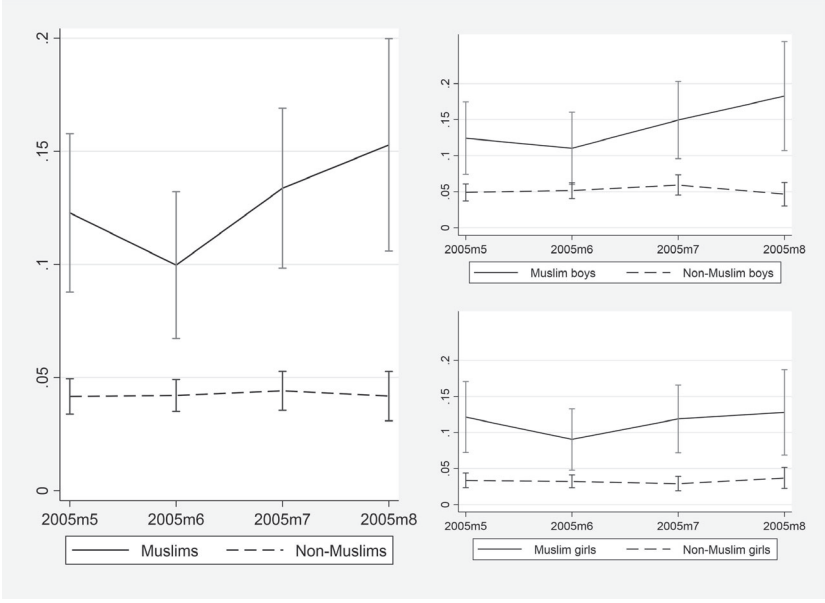

Fig. 3. Expectations of discrimination in non-compulsory education for Muslim and non-Muslim adolescents

Year 11 (aged 15–16), with the latter tests determining whether students can progress to post-compulsory education. Given that we observe adolescents towards the end of Year 10 (aged 14–15), we avoid national exams, but this does not preclude students from taking school-specific exams.

IV. Results

Main Results

Table 2 presents the main effects of interest, where Column 1 presents evidence for all teenagers and Columns 2 and 3 present results by gender. The results in Panel A suggest that, after controlling for ethnicity, region, and sociodemographic/economic circumstances, there is little difference in the happiness levels of Muslim adolescents relative to non-Muslim adolescents prior to the bombings. We also find little evidence of a change in the happiness of non-Muslim adolescents after the London bombings, or any differential change in the happiness for Muslim adolescents during this period. However, Columns 2 and 3 tell a different story by revealing heterogeneous effects of the bombings by gender. Somewhat surprisingly, Muslim teenage boys appear to be slightly happier relative to other teenage

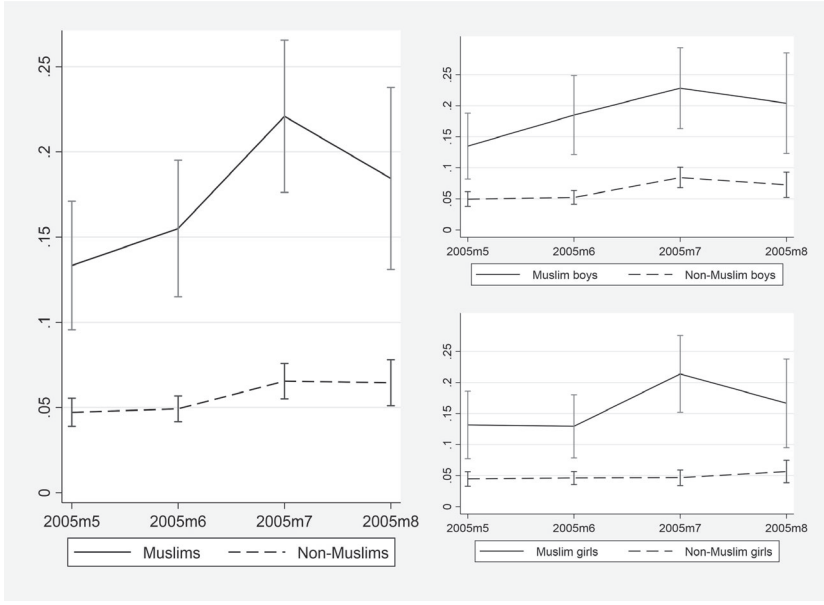

Fig. 4. Expectations of discrimination in the labour market for Muslim and non-Muslim adolescents

boys after the bombings, though the estimated treatment effect amounts to just 1 percent of the pre-attack happiness level for Muslim teenage boys and is imprecisely determined, suggesting little impact overall of the bombings on Muslim teenage boys. However, Muslim teenage girls experience a statistically significant decline in happiness relative to other teenage girls after the bombings, which is equivalent to a 5 percent change in baseline happiness levels, and approximately 1.5 times the happiness penalty associated with parental financial difficulties. These findings concur with qualitative evidence suggesting that Muslim women were particularly affected by increased Islamophobia following the London bombings, a phenomenon that is attributed to Muslim women being more easily identifiable (Change Institute, 2009a). The 2007–2008 Citizenship Survey also suggests that women are more prone to perceiving racial or religious harassment as a problem in their local area compared to men (Department for Communities and Local Government, 2010). If gender differences exist in the perceptions – and experiences – of hostility, this could provide one explanation for these results. However, it is also possible that teenage girls are exposed to similar levels of hostility and simply react differently. For example, the findings of Borrell *et al.* (2006) suggest that women suffer more than men after experiencing racism while Ge

Table 2. *Impact of the London bombings on the emotional well-being and expectations of discrimination of Muslim teenagers*

	All	Boys	Girls
	(1)	(2)	(3)
Panel A: Happiness			
Treated	0.023	−0.037	0.094
	(0.045)	(0.063)	(0.059)
Post	0.015	0.005	0.027
	(0.014)	(0.018)	(0.021)
Treated × post	−0.059	0.036	−0.148***
	(0.037)	(0.053)	(0.050)
N	11,533	5,882	5,651
Panel B: Depression			
Treated	−0.118**	−0.094	−0.172**
	(0.057)	(0.077)	(0.082)
Post	−0.020	−0.028	−0.015
	(0.020)	(0.024)	(0.031)
Treated × post	0.002	−0.009	0.011
	(0.052)	(0.064)	(0.078)
N	11,775	5,977	5,798
Panel C: Expects discrimination in education			
Treated	−0.035*	−0.064**	−0.002
	(0.020)	(0.030)	(0.027)
Post	−0.003	−0.002	−0.002
	(0.004)	(0.006)	(0.005)
Treated × post	0.026	0.038	0.016
	(0.019)	(0.027)	(0.024)
N	11,068	5,593	5,475
Panel D: Expects discrimination in the labour market			
Treated	−0.049*	−0.075**	−0.023
	(0.025)	(0.036)	(0.032)
Post	0.009**	0.020***	−0.001
	(0.005)	(0.007)	(0.006)
Treated × post	0.052**	0.037	0.067**
	(0.023)	(0.032)	(0.033)
N	10,914	5,524	5,390

Notes: See Table 1 for a list of the control variables used. Standard errors are adjusted to account for school-level clustering. Significance levels are shown as $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

et al. (2006) show that the depressive symptoms of teenage girls are more responsive to stressful life events as compared with teenage boys.

In contrast to the results for happiness, the results presented in Panel B suggest that the bombings had little impact on feelings of unhappiness and depression. This could be a genuine result, as well-being and ill-being are distinct dimensions (see Headey and Wooden, 2004), but we cannot rule out the possibility that teenagers simply find it harder to answer questions on depression, as they might have a limited understanding of the concept or have difficulty recognizing the symptoms.

Panels C and D consider the impact of the London bombings on expectations of facing racial/religious discrimination in non-compulsory education and in the labour market. The results suggest that, after controlling for ethnicity and other relevant characteristics of the respondents, Muslim adolescents are less likely to expect discrimination in non-compulsory education or the labour market. As far as non-compulsory education is concerned, we find evidence that suggests an increase in discriminatory expectations among Muslim teenagers relative to other teenagers after the bombings, although none of these effects are statistically significant. A different story emerges in Panel D, which focuses on expectations of discrimination in the labour market. Here there is a 5.2 percentage point increase in the probability of Muslim teenagers thinking that their skin colour, ethnic origin, or religion will make it more difficult for them to get a job relative to other teenagers after the bombings, which is statistically significant at the 5 percent level. This represents a 37 percent increase in the baseline probability. Interestingly, and in line with our previous evidence on emotional well-being, this effect appears to be concentrated among Muslim teenage girls. In part, this is because there is a widespread increase in discriminatory expectations among teenage boys after the bombings, so that although Muslim teenage boys are 5.7 percentage points more likely to expect discrimination after the bombings, this is not statistically different from the 2 percentage point increase among non-Muslim teenage boys. In contrast, for Muslim teenage girls, the percentage point change relative to other teenage girls after the bombings is 6.7, which is more than a 50 percent increase from the baseline probability and is statistically significant at the 5 percent level. Thus, overall, the evidence consistently points towards worse outcomes for Muslim teenage girls in the aftermath of the bombings. As discussed earlier, differences between the non-compulsory education and labour market settings could be related to the type of racial discrimination that adolescents think they will face. A weak response to the bombings for non-compulsory education could reflect low expectations of institutional discrimination in the education sector. Indeed, offer rates for UK university places are broadly in line with expected offer rates for applicants from ethnic minorities, who typically

apply to more competitive degree programs,[11] such that those from ethnic minorities could expect fair treatment in this setting. A stronger reaction to the bombings in a labour market context would be consistent with public sentiment after the London bombings. Using a nationally representative survey, Kitchen *et al.* (2006) show that the proportion believing Muslims to be victims of religious prejudice almost doubled, from 27 percent to 50 percent, immediately after the bombings, which is accompanied by a 9 percentage point rise in the proportion believing that "more racial prejudice exists today compared with five years ago". It would not be unreasonable to suppose that Muslim adolescents could have interpreted these developments as an obstacle to labour market success. This is an important finding given that economic theory predicts that discrimination in the labour market – and expectations thereof – affect incentives to acquire human capital, leaving scope for long-term implications of the changes in expectations caused by the London bombings.

Robustness Analysis

We find evidence of a decline in happiness and an increase in expectations of facing discrimination in the labour market among Muslim teenage girls after the bombings, which we attribute to a more hostile environment for Muslims. In the remainder of this paper, we implement several falsification checks to establish the robustness of these findings, using the same set of control variables (based on 2004 data for time-invariant characteristics and lagged by one year for time-varying characteristics). The difference-in-differences method involves comparing the change in outcomes before and after the bombings for treated and control groups, and attributing any differences in the evolution of these outcomes to the impact of the bombings. Our findings therefore reflect, among other things, the choice of treatment and control group, and we first consider the impact of changing the allocation of the treatment status. As noted earlier, there is little consensus in the literature as to whether Indians should appear in the treatment group (Hanes and Machin, 2014), the control group (Johnston and Lordan, 2012), or be excluded altogether (Kaushal *et al.*, 2007; Braakmann, 2010). In Column 1 of Table 3, we present results where religion is ignored and ethnic origin is used to determine treatment status, with South Asian (i.e., Pakistani, Bangladeshi, and Indian) individuals forming the treated group and white people, black people, and mixed-race/other individuals forming the control group. In the United Kingdom, just 13 percent of Indians are Muslim, meaning that most of the Indians currently in the

[11] See https://www.ucas.com/sites/default/files/gbanalysis_note_2015_05_web_0.pdf.

treated group are not Muslim (Change Institute, 2009b). The results are consistent with an attenuation bias (see Cornelissen and Jirjahn, 2012) as the estimated treatment effect is smaller and also no longer statistically significant in the case of discriminatory expectations. This seems reasonable in the current context if the bombings do indeed influence emotional well-being as a result of the denigration of Muslims in society, as non-Muslims (even those sometimes mistaken for Muslims) are unlikely to be demoralized to the same extent by these negative stereotypes. Indians also have similar or higher earnings relative to white people in the United Kingdom, while earnings can be much lower for people who have Pakistani or Bangladeshi backgrounds (Longhi and Brynin, 2017), such that Indians might feel reasonably sheltered from labour market discrimination directed at Muslims. In Column 2, where Indians are excluded from the treated group, the treatment effect is once again larger and statistically significant. In Column 3, we combine information on religion and ethnicity to compare Muslims with non-Muslim Indians (see Johnston and Lordan, 2012). Here the treatment effect is of similar magnitude for happiness and larger in the case of discriminatory expectations, but the small sample sizes reduce statistical precision. Although not reported here, for Muslim teenage boys we continue to find little influence of the bombings on happiness or expectations of facing discrimination in the labour market irrespective of the definition of treatment, and we fail to consistently find suggestive evidence of the latter. Neither does varying the treatment definition alter our findings on depression for boys or girls. However, when it comes to discriminatory expectations in non-compulsory education, the treatment definition is an irrelevance for girls only. For boys, varying this definition suggests that zero or increased expectations of facing discrimination are possible. A lack of consistency across specifications for discriminatory expectations, together with a lack of consensus on the most appropriate treatment definition, would caution against attaching too much weight to our findings for teenage boys. In contrast, our findings for teenage girls reliably indicate a fall in happiness and increased expectations of facing discrimination in the labour market after the bombings for those exposed to the treatment.

Next we conduct falsification tests, repeating our main analysis for time periods unaffected by the bombings, where differences in the evolution of outcomes are not expected to materialize. The first placebo test considered sets the treatment earlier in that same year (i.e., June 2005). For this analysis, the pre-treatment period refers to April–May and the post-treatment period to June, with July onwards excluded altogether. The results, presented in Column 1 of Table 4, provide little support for a pseudo-treatment effect earlier in the same year. We exploit later waves of the LSYPE data to construct our second placebo test, which assigns the treatment to a given year after the bombings occurred. The GHQ

Table 3. *Different treatment/control definitions: girls*

	South Asians versus others (1)	South Asians excl. Indians versus others (2)	Muslims versus non-Muslim Indians (3)
Panel A: Happiness			
Treated × post	−0.106**	−0.171***	−0.151*
	(0.047)	(0.056)	(0.088)
N	5,651	5,300	1,030
Panel B: Expects discrimination in the labour market			
Treated × post	0.035	0.059*	0.080
	(0.028)	(0.035)	(0.054)
N	5,390	5,089	857

Notes: See notes to Table 2.

is administered in the same format two years later in 2007, where the fieldwork was carried out between 12 June and 14 October, achieving 11,053 complete interviews. With almost 3,000 interviews undertaken in June, the sample is large enough to designate the pre-treatment period as June 2007 and the post-treatment period as July–August 2007 (September and October are excluded because Ramadan spanned both of these months in 2007).[12] Discriminatory expectations are collected again in 2008, albeit in a slightly different format. In this wave individuals were asked "Do you think that any religious beliefs you may have will affect how likely you are to get a job or training place?" and "Do you think your race, skin colour or ethnic background will affect how likely you are to get a job or training place?", with the options "Yes", "No", and "Don't know". Those who answered "Yes" to either question were subsequently asked "Do you think it will make it easier or more difficult for you to get a job or a training place?" with the options "Make it easier", "Make it more difficult", and "Don't know". Thus, in 2008, broadly the same information was collected but race was also mentioned as a reason for expecting discrimination, and multiple questions replaced a single question, with the potential for changes in the question design to affect supplied responses.[13] Nevertheless, these questions continue to provide usable, if imperfect, information. We

[12]In practice it makes little difference if we retain or exclude months affected by Ramadan in terms of the happiness results discussed here or the discriminatory expectations discussed below.
[13]It should also be noted that because individuals aged 17–18 could already be active in the labour market these expectations could reflect actual experiences, which might differ between Muslims and non-Muslims, although experiences are unlikely to differentially change across these groups in the pseudo-post period.

construct a dummy variable that is equal to one if an individual answers either question and in one of these questions indicates whether they think their race, skin colour, ethnic background, or religion will make it more difficult to get a job or training place.[14] As before, "Don't know" responses are excluded. Fieldwork in 2008 was carried out between 3 June and 28 October, achieving 10,430 interviews, with 50 percent of interviews taking place in June. Thus we allocate June 2008 as the pre-treatment period and July–September 2008 as the post-treatment period (October is excluded because Ramadan fell in this month in 2008). The results from our second placebo test are presented in Column 2 of Table 4, where we find little evidence of any difference in outcomes over the same time frame in an alternative year without the bombings. In the final column of Table 4, we consider the effect of excluding August and September from the sample used for our main analysis. As noted earlier, individuals interviewed from the end of July onwards were exposed to two terror attacks, while those interviewed earlier were exposed to one at most. A separate concern is that events occurring in July/August could differentially affect adolescents from different backgrounds, although it is unclear *a priori* how these considerations might affect our analysis. The results presented in Column 3 suggest that excluding August/September makes very little difference as far as happiness is concerned, but the estimated treatment effect for discriminatory expectations increases. This is consistent with Figures 1 and 4, which show pronounced responses in July with a small reversion in August, and could reflect the sudden change and intensity of hate crimes, media coverage, and shifts in public opinion that immediately followed the bombings in July.

We also consider the treatment effect for different definitions of emotional well-being. As noted earlier, we use measures of affect as opposed to the total GHQ score because 20 percent of adolescents selected the "Don't know" option for a least one of the item responses. Nevertheless, the total GHQ score, where available, shows a rise in mental distress for Muslim teenage girls relative to others after the bombings, which is equivalent to a 10 percent increase from baseline distress levels (see Column 1 of Table 5). An alternative approach to analysing the happiness score based on the presence, as opposed to the severity, of symptoms – in this case whether the individual was less happy or not – is considered in Column 2 of Table 5. These results show that Muslim teenage girls are 6.8 percentage points more likely to report feeling less happy relative to other teenage girls after the bombings, which is equivalent to 1.2 times the impact

[14]We obtain similar results when using the smaller sample of individuals who answered both questions.

Table 4. *Falsification tests: girls*

	June 2005 treatment (1)	July 2007/2008 treatment (2)	Excluding August and September (3)
Panel A: Happiness			
Treated × post	−0.004	0.011	−0.145**
	(0.078)	(0.066)	(0.060)
N	3,355	4,669	4,716
Panel B: Expects discrimination in the labour market			
Treated × post	0.013	−0.056	0.095**
	(0.036)	(0.038)	(0.039)
N	3,248	4,440	4,514

Notes: See notes to Table 2. Column 1 restricts analysis to April–June 2005 and to April–July 2005 in Column 3. Column 2 uses data from later waves of the LSYPE data (2007 for happiness and 2008 for expectations of discrimination). To mimic the control variables in our main analysis, in later years we use 2004 data for non-time-varying characteristics and lag all time-varying characteristics by one year.

of parental financial difficulties. Next, we consider the potential impact of a large number of "Don't know" responses for our analysis of discriminatory expectations. As noted earlier, such responses could represent a wide range of possibilities, from uncertainty to noise. We therefore examine whether the propensity for Muslim teenage girls to respond with "Don't know" changes in the aftermath of the bombings, as might be expected if these responses reflected uncertainty. The results presented in Column 3 provide little evidence to suggest that these responses contain meaningful information, with an increase of less than 1 percentage point in the probability of Muslim teenage girls responding "Don't know" relative to other groups after the bombings, which is far from statistically significant. Consistent with the notion that "Don't know" responses are uninformative, if instead of excluding these responses from our analysis we place them in the baseline category (thus coding these responses as zero), we still find evidence of an impact of the bombings on discriminatory expectations, but this effect is smaller and significant only at the 10 percent level (see Column 4).

Finally, and most importantly, we consider the extent to which our results could be driven by unobserved differences between Muslims interviewed pre- and post-July 2005. As noted earlier, our identification strategy relies on the interview month being random with respect to individual characteristics, or otherwise being able to control for confounding variables. As Table A1 in the Online Appendix shows, the interview month is unrelated to a vast array of variables, with some potentially important exceptions. In Table 6, we consider the extent to which our estimates

Table 5. *Further robustness checks: girls*

	GHQ score	Is less happy	"Don't know" if expects discrimination	Expects discrimination: includes "Don't know"
	(1)	(2)	(3)	(4)
Treated × post	1.004*	0.068***	0.007	0.048*
	(0.526)	(0.024)	(0.031)	(0.025)
N	4,512	5,651	6,024	6,024

Notes: See notes to Table 2.

for Muslim teenage girls change as we include different subgroups of control variables. We first estimate a model without any control variables in Column 1, then add standard control variables as in our main analysis in Column 2, followed by variables to capture relationship with parents, school and beyond, time use, and risky behaviours in Columns 3, 4, 5, and 6, respectively.[15] In Panel A our estimate of the treatment effect for happiness ranges from −0.156 without any control variables to −0.133 with approximately 110 control variables, and is always statistically different from zero. In Panel B, our estimate of the treatment effect for discriminatory expectations ranges from 0.056 percentage points without any control variables to 0.065 percentage points controlling for all variables. Overall, our results are remarkably robust to including a very extensive set of control variables, with similar estimates across specifications without controls and an abundance of controls, suggesting that our results are unlikely to reflect unobserved individual differences.

An alternative, and more credible, approach to controlling for a wide array of control variables is to exploit the panel element of the survey subject, with the caveat that the analysis is restricted to those individuals that supplied data across the relevant years.[16] Specifically, if Muslim adolescents interviewed post-July 2005 were less happy than Muslim adolescents interviewed prior to July 2005 for reasons unrelated to the bombings, we might expect that these individuals were less happy

[15]For some variables and subgroups of variables (e.g., risky behaviours) information is missing, either because adolescents refused to complete a given module or refused specific questions within that module, where the former accounts for the larger share of missing information. To ensure that the sample size remains constant throughout we replace missing values with zero and construct an aggregate indicator variable for each subgroup to capture missing values in any variable within that subgroup.

[16]Consistent with other panel surveys, individuals who dropped out of the LSYPE study were slightly poorer and lived in less stable family units than those who remained. As better circumstances typically provide protective benefits, the impact of the bombings could be weaker for individuals remaining in the panel.

Table 6. *Different subgroups of control variables: girls*

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Happiness						
Treated × post	−0.156***	−0.148***	−0.135***	−0.138***	−0.133***	−0.133***
	(0.050)	(0.050)	(0.049)	(0.049)	(0.050)	(0.050)
Panel B: Expects discrimination in the labour market						
Treated × post	0.056*	0.067**	0.063*	0.066**	0.066**	0.065**
	(0.033)	(0.033)	(0.033)	(0.032)	(0.032)	(0.032)
Standard controls	No	Yes	Yes	Yes	Yes	Yes
Relationship with parents	No	No	Yes	Yes	Yes	Yes
School and beyond	No	No	No	Yes	Yes	Yes
Time use	No	No	No	No	Yes	Yes
Risky behaviours	No	No	No	No	No	Yes

Notes: See notes to Table 2. Standard controls include 25 variables, covering ethnicity, region of residence, sociodemographic characteristics, and economic resources. Relationship with parents contains 16 variables, school and beyond contains 39 variables, time use contains 19 variables, and risky behaviours contains 9 variables. Additional indicator variables are included where relevant to account for missing information within subgroups of variables.

when observed again in 2007. Thus, if the happiness gap remains over the intervening period, it would indicate that the Muslim adolescents interviewed post-July 2005 were just different in unobserved dimensions, calling into question our main findings. However, if the main reason for the happiness gap in 2005 is that the Muslim adolescents interviewed prior to the bombings were not exposed to the treatment, in 2007 once all Muslim adolescents had been treated, we might expect to observe similar levels of happiness again. Figure 5 examines the average happiness levels of Muslim teenage girls in 2005 and 2007, splitting the sample according to the timing of the interview in 2005 (i.e., pre- and post-July 2005). In 2005, Muslim teenage girls interviewed after the bombings reported lower happiness scores compared to those interviewed beforehand. However, if anything, Muslim teenage girls interviewed after the bombings in 2005 reported slightly higher levels of happiness in 2007 compared to those who had been interviewed beforehand. Examining the change in outcomes for these different groups of Muslim teenage girls more formally within a regression context paints a similar picture. In Column 1 of Table 7, we use fixed effect regression methods to compare the change in happiness between 2005 and 2007 for Muslim teenage girls interviewed after the bombings (denoted by Interviewed post-attack in 2005 × Year 2007) relative to those interviewed beforehand (Year 2007). The results, which are identified from 45 percent of Muslim teenage girls whose happiness changed across the years, suggest that the happiness of Muslim teenage girls interviewed prior to the

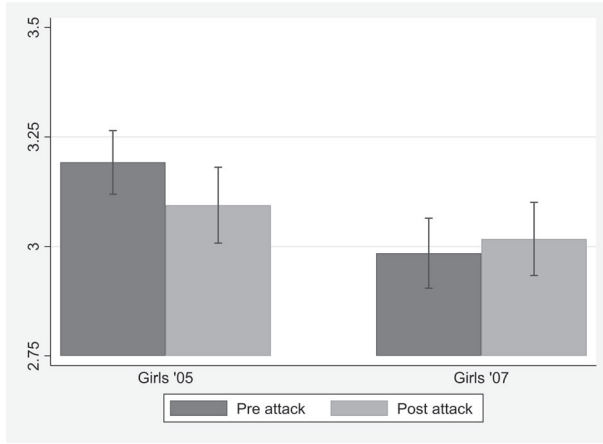

Fig. 5. Happiness: Muslim teenage girls in 2005 and 2007, by timing of 2005 interview

bombings declined by 0.21 between 2005 and 2007, whereas for Muslim teenage girls interviewed afterwards it declined by just 0.08, with the difference statistically significant. The smaller decline in happiness among those interviewed after the bombings in 2005 underlies the convergence in happiness for all Muslim teenage girls by 2007. However, this result could reflect different mechanisms at play. For example, changes in happiness could be concentrated among those interviewed pre-July 2005 or post-July 2005, or they could be equally likely for both groups. Because the happiness score is relatively lower in 2005 for Muslim teenage girls interviewed after the bombings, changes in happiness for this group could involve happiness rebounding for some and deteriorating for others, with positive changes in happiness indicating a short-term impact of the bombings. Alternatively, changes in happiness among Muslim teenage girls interviewed prior to the bombings, whose happiness is relatively higher in 2005, would most likely involve happiness deteriorating, and would be consistent with a long-lasting impact of the bombings. To investigate, we create two dummy variables equal to one if an individual became happier (unhappier) between 2005 and 2007 and zero otherwise. If the smaller decline in happiness observed among those interviewed post-July 2005 is partially driven by happiness rebounding for this group, we should observe that those interviewed post-July 2005 were more likely to experience a positive change in happiness between 2005 and 2007 relative to those interviewed beforehand. However, if the smaller decline is largely driven by those interviewed pre-July 2005 becoming unhappier between 2005 and 2007, we should observe that those interviewed pre-July 2005 were more likely to experience a negative change

Table 7. *Exploiting the LSYPE panel: girls*

	(1)	(2)	(3)
Panel A	Happiness (fixed effects)	Becomes happier	Becomes unhappier
Year 2007	−0.210***		
	(0.052)		
Interviewed post-attack in 2005 × Year 2007	0.133*		
	(0.073)		
Interviewed post-attack in 2005		0.036	−0.026
		(0.031)	(0.037)
N	1,238	619	619
Panel B	Expects discrimination (fixed effects)	No longer expects discrimination	Now expects discrimination
Year 2008	0.163***		
	(0.035)		
Interviewed post-attack in 2005 × Year 2008	−0.049		
	(0.048)		
Interviewed post-attack in 2005		0.001	−0.039
		(0.025)	(0.040)
N	858	429	429

Notes: See notes to Table 2. To mimic the control variables in our main analysis we lag all time-varying characteristics by one year in the fixed effects analysis. Standard controls are used in Columns 2 and 3.

in happiness relative to those interviewed after the bombings. The results in Columns 2 and 3 do not provide overwhelming evidence to favour one scenario over the other, with those interviewed after the bombings more likely to become happier and less likely to become unhappier relative to those interviewed beforehand. This could mean that there is not one single legacy of the bombings for Muslim teenage happiness or that other factors primarily influence the evolution of happiness.

With respect to expectations of facing discrimination in the labour market, Figure 6 shows that although the gap in discriminatory expectations narrows in 2008, it is not entirely eliminated, with those interviewed after the bombings in 2005 also reporting slightly higher expectations of facing discrimination in the labour market in 2008. Fixed effect regression results presented in Panel B of Table 7 show that expectations of facing discrimination increased by 16.3 percentage points for those interviewed pre-July 2005 and by the smaller amount of 11.4 percentage points for those interviewed post-July 2005, but this difference in magnitude is not statistically different from zero. The lower statistical precision partly reflects the binary nature of expectations, requiring large shifts in perception to pass

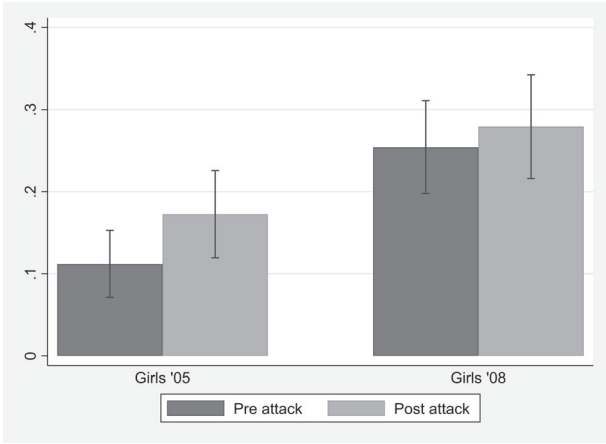

Fig. 6. Expects discrimination in the labour market: Muslim teenage girls in 2005 and 2008, by timing of 2005 interview

the boundary dividing expectations of facing and not facing discrimination, with just 27 percent of Muslim girls changing their beliefs over time. As before, we create two dummy variables equal to one if an individual no longer expects (starts to expect) discrimination between 2005 and 2008 and zero otherwise. Results presented in Columns 2 and 3 suggest that Muslim teenage girls interviewed post-July 2005 are no more likely to shed discriminatory expectations between 2005 and 2008 than those interviewed pre-July 2005. However, they do appear less inclined to start holding these expectations in the intervening years. This might indicate long-lasting impacts of the bombings, although small sample sizes and a lack of statistical precision make it difficult to draw any firm conclusions on this.

Taken all together, these robustness checks indicate that our results for girls are largely insensitive to the definition of the treatment and, furthermore, are primarily due to being exposed to the treatment, rather than being the artefact of unobserved and uncontrolled influences. However, in the case of discriminatory expectations, we note that our results could represent an upper bound of the true effect.

V. Conclusion

This paper contributes to a large and growing body of literature examining the impact of extremist Islamic terror attacks on economic and social outcomes for Muslims. We exploit the timing of the London bombings, coinciding with a large-scale national survey of adolescents, to identify the

impact of extremist Islamic terror attacks on the well-being of adolescent Muslims, as measured by their happiness, depression, and expectations of facing discrimination. These outcomes reflect the contemporaneous circumstances of adolescents, as well as potentially having wide-ranging and long-lasting impacts on life trajectories (see Lundberg and Startz, 1983; Coate and Loury, 1993b; Fletcher, 2008; Smith and Smith, 2010; Lang and Manove, 2011). To our knowledge, we are the first to consider the fallout for adolescents and, in particular, those on the cusp of making important life-changing decisions.

Our analysis reveals interesting gender differences. We find evidence of a decline in the happiness of Muslims teenage girls after the bombings, along with an increase in expectations of facing discrimination in the labour market. These findings are robust to several falsification checks. For Muslim teenage boys, there is little evidence to suggest an impact of the bombings on happiness and, at best, tentative evidence of an increase in expectations of facing discrimination. Further analysis suggests a mixed legacy of the bombings in terms of the happiness of Muslim teenage girls, with the results indicating that changes in happiness could have been short-term for some and long-term for others. With regard to expectations of facing discrimination, the evidence is more supportive of a long-term impact of the bombings, but a lack of statistical precision precludes drawing any firm conclusions. Given the scope for these outcomes to influence life trajectories, the impact of the bombings on Muslim teenagers might be more pervasive than shown here and is a matter for future research.

Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Online Appendix

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