Sheffield Economic Research Paper Series.

The impact of the London bombings on the wellbeing of young Muslims

Arne Risa Hole and Anita Ratcliffe

The impact of the London bombings on the wellbeing of young Muslims *

Arne Risa Hole[†]
University of Sheffield

Anita Ratcliffe[‡]
University of Sheffield

Abstract

We exploit the timing of the London bombings, coinciding with a large-scale national survey of adolescents, to identify the impact of a sudden increase in racism on the wellbeing of young Muslims. In addition, we extend Lechner (2011) to propose an estimator for the method of difference-in-differences with ordered data. Our analyses reveal interesting gender differences. The wellbeing of Muslim teenage girls declines after the bombings, particularly for those facing high levels of deprivation and ethnic concentration, and this decline is accompanied by increased expectations of facing discrimination. No corresponding effects are found for Muslim teenage boys.

Keywords: Racism, Wellbeing, Difference-in-differences, Ordered data

JEL codes: I10, I31, J15

[†]University of Sheffield, Department of Economics, 9 Mappin Street, Sheffield, S1 4DT. E-mail: a.r.hole@sheffield.ac.uk
[‡]University of Sheffield, Department of Economics, 9 Mappin Street, Sheffield, S1 4DT. E-mail: a.ratcliffe@sheffield.ac.uk

1 Introduction

A large literature spanning disciplines such as public health, epidemiology, psychology and sociology analyses racial disparities in health and wellbeing, with racism mooted as a key explanation in generating these differences (see inter alia Paradies, 2006; Pascoe and Smart Richman, 2009; Williams and Mohammed, 2009). While economists primarily focus on racial discrimination in labour markets (see Altonji and Blank, 1999; Lang and Lehmann, 2012), happiness, mental and physical health are increasingly seen as important determinants of labour market performance (García-Gómez et al., 2010; Oswald et al., 2015; Reichert, 2015). This raises the possibility that racism-induced wellbeing deficits might contribute towards weaker labour market outcomes of ethnic minorities. A focus on subjective wellbeing measures may also provide complementary evidence on progress made towards improving the circumstances of ethnic minorities. For example, Stevenson and Wolfers (2012) show that over a period where progress towards closing racial gaps in education and labour market outcomes faltered, the racial gap in happiness narrowed. One potential explanation for this finding, suggested by the authors, is that attitudes towards ethnic minorities substantially improved over this period. Indeed, subjective wellbeing measures are increasingly collected by national governments alongside traditional measures of living standards, with the UK Office for National Statistics scrutinising subjective wellbeing levels since 2011.

The impact of racism on childhood health and wellbeing has received relatively less attention despite concerns that poor mental wellbeing damages pre-labour market outcomes. For example, mental health issues may hinder the ability, as well as incentives, to invest in education (Fletcher, 2008), with research exploiting exogenous variation in depressive symptoms showing poorer schooling performance among teenage girls (Ding et al., 2009; Busch et al., 2014). Psychological problems in formative years have also been linked to inferior labour market outcomes, relationship prospects and life satisfaction in adulthood (Smith and Smith, 2010; Goodman et al., 2011; Layard et al., 2014). In a similar vein, it also appears that higher levels of positive affect in adolescence are beneficial to economic success in later life (De Neve and Oswald, 2012). While the existing literature generally finds a negative effect of self-reported racism on emotional wellbeing, weaknesses exist in measuring racism and in establishing causality (see Priest et al., 2013). A clearer understanding of whether racism per se shapes subjective wellbeing, and the extent of its influence, is required to appreciate the role of racism in driving inequalities in current, and future, outcomes of ethnic minorities.

This paper uses the London bombings as an event generating a sharp increase in societal racism in order to study the impact of racism on adolescent wellbeing. The initial bombings occurred on the 7th July 2005 (henceforth 7/7) and targeted the London transport network with devastating consequences. A further set of attacks took place on the 21st July 2005 though these bombs failed to detonate. In spite of key Muslim organisations condemning the bombings, many Muslims - and South Asians more generally - became victims of a backlash. For example, in the four weeks after

the attacks, faith-hate crimes increased by 600 per cent compared to the previous year (Greater London Authority, 2006). Hanes and Machin (2014) examine trends in hate crimes over a longer period, and find that an initial spike in hate crimes against South Asians and Arabs diminishes but does not disappear altogether with the passage of time. Other evidence also points towards widespread changes in attitudes towards Muslim minorities. For example, a 9 percentage point rise in those believing "more racial prejudice exists today compared with 5 years ago" is observed in a nationally representative survey immediately after the bombings, with those identifying Muslims as the victims of prejudice increasing from 27% to 50% (Kitchen et al., 2006). Qualitative interviews also reveal that Muslims feel there has been a noticeable increase in Islamophobia since the London bombings (Change Institute, 2009).

Our research advances the current literature in several respects. Firstly, we exploit the timing of the London bombings, occurring midway through a large-scale nationally representative survey of adolescents, to estimate a causal effect of racism on the happiness and depression of young Muslims. To our knowledge, our study is the first to use plausibly exogenous variation in racism in order to examine its effect on the wellbeing of adolescents. Teenagers are an interesting group to study because the transition to adulthood is a crucial developmental stage during which individuals cultivate their sense of self and place in the world, and commit to pathways defining who they will become in future (Erikson, 1968). For ethnic minorities, adolescence is also the time to develop an ethnic identity (Phinney, 1990), akin to a social identity (Tajfel, 1978). Thus exposure to racism during adolescence may both impact emotional wellbeing and identity formation processes, with potential consequences extending beyond adolescence.

Secondly, we build on Lechner (2011) to propose a suitable estimator for the treatment effect when using difference-in-differences with ordered data. Ordered data is pervasive in economics, with examples including credit ratings, political ideology and subjective wellbeing, and these data are routinely analysed using non-linear models, such as the ordered probit model. At the same time, the method of difference-in-differences is widely applied in policy evaluation, but while this identification strategy readily applies to continuous outcomes, it is often implausible to assume common trends in models respecting the statistical properties of limited dependent variables (Blundell and Costa Dias, 2009; Lechner, 2011). Consequently, ordered outcomes are frequently analysed with linear regression methods when using a difference-in-differences strategy (see e.g. Gregg et al., 2009; Brodeur and Connolly, 2013). As an alternative approach, we suggest analysing the treatment effect in terms of the response probability, and assuming common trends at the level of the latent index. An advantage to this approach is the ability to investigate whether the treatment effect materialises across the entire distribution of the outcome variable or is limited to particular sections of it. Specific to our context, a reduction in happiness driven by a shift from high to moderate levels of happiness might warrant a different policy response compared with a shift from low to very low happiness levels.

Our final contribution lies with an investigation of the heterogenous impact of the bombings across levels of neighbourhood poverty and ethnic diversity. Theories of intergroup conflict point

towards competition over scarce resources as drivers of conflict (see e.g. Olzak, 1990). Empirical analyses of causal relationships, however, suggest that it is ethnic concentration, rather than economic conditions, that harms race relations (Antecol and Cobb-Clark, 2010; Dustmann et al., 2011). This suggests that the impact of the bombings may vary with neighbourhood context. We also investigate the impact of the bombings across levels of school poverty and ethnic concentration. As teenagers spend most of their day in school, interactions with others may be shaped by the characteristics of the school rather than the neighbourhood. This is particularly relevant in England where schools are more segregated than neighbourhoods (Johnston et al., 2008), and where school sorting by ethnicity may also produce a concentration of poverty in schools.

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 wellbeing of Muslims teenage girls after the bombings, which is particularly pronounced where girls live in (attend) relatively poor or ethnically segregated areas (schools). In contrast, we find little evidence that the bombings affected Muslim teenage boys. Consistent with these findings, we also observe that teenage girls are more prone to believing they will experience discrimination after the bombings. From a methodological viewpoint, our results suggest that OLS performs relatively well if the goal is to estimate changes in the conditional mean of the outcome variable. However, by retaining the ordinal character of subjective wellbeing, we find strongest evidence that the bombings influenced the lower end of the happiness distribution, with suggestive evidence of an impact across the entire distribution.

2 Literature

2.1 Pathways linking racism and health

Racism encompasses prejudice (i.e. differential beliefs about others) and discrimination (i.e. differential treatment of others). Minority groups may experience institutionalised racism, suffering from poorer access to goods and services, or face personalised racism and experience suspicion, avoidance, abuse, and restricted opportunities in housing and labour markets (Jones, 2000). The literature investigating the impact of racism on health outcomes treats racism as a psychological stressor, and hypothesises that stressful events lead to negative affective states that ultimately affect physiological functioning (Williams and Mohammed, 2009). Specifically, psychological stress activates two endocrine response systems; the hypothalamic-pituitary-adrenocortical axis and the sympathetic-adrenal-medullary system, with prolonged or repeated activation of these systems elevating the risk of physical and psychiatric disorders (see Cohen et al., 2007). Certain stressors are deemed to have particularly adverse health effects, for example, stressors that are negative, uncontrollable and unpredictable (Thoits, 2010; Pascoe and Smart Richman, 2009), with exposure to racism arguably falling into these categories. In addition to any direct influence on health, stress may also trigger health-damaging coping responses, such as smoking or limited exercising (Cohen

et al., 2007).

A person need not experience racism directly in order to be adversely affected by the prevalence of racism in society. Fear of experiencing racism may in itself increase stress or encourage stress inducing behaviours, such as heightened vigilance (Carter, 2007). Family members suffering from racism may also be deleterious if it raises concerns over the likelihood of personally experiencing racism, leads to concerns for the wellbeing of significant others, or triggers negative coping responses in those experiencing racism. In particular, parental experiences and/or fear of racism may be detrimental for children given the propensity for parental psychological distress to influence parental styles, emotional support and the family environment (Sander and McCarty, 2005). Finally, negative stereotypes and media-portrayals of ethnic minorities may be internalised, resulting in a loss of self-esteem (Jones, 2000).

In summary, there are numerous pathways through which direct experiences of racism might influence wellbeing, with additional adverse effects of indirect racism operating through the behaviour of caregivers for children and adolescents. Children and adolescents may be especially vulnerable to direct or indirect exposure to racism because they are still developing, and are unlikely to have acquired the necessary skills to help them cope with the challenges presented by racism.

2.2   Related literature

Our research is related to an existing literature on the impact of racism on young adults, which typically focuses on direct experiences of racism among African-American teenagers. However, in reviewing this literature, Priest et al. (2013) find that 121 different measures of racism are employed across 123 studies. Moreover, identifying racist experiences and later reporting these in surveys may be linked to individual-level characteristics, such as coping style, that may also explain the outcome of interest (Lauderdale, 2006). More recently, researchers have explored the link between parental experiences of racism and child outcomes, albeit facing similar issues regarding the measurement, and causal impact, of racism (Caughy et al., 2004; Kelly et al., 2013; Tran, 2014).

Given the scope for omitted variables and endogenous control variables (Yinger, 1998), economists have turned their attention to terrorist activity to identify the causal impact of racism on the outcomes of ethnic minorities. By focusing on changes in the prevalence of racism in society, these studies also circumvent the issue of which instruments are available to measure racism in survey data. The majority of these studies examine labour market outcomes after 9/11 (see inter alia. Dávila and Mora, 2005; Kaushal et al., 2007; Åslund and Rooth, 2005; Braakmann, 2009) but there is a growing interest in the impact of the London bombings on hate crimes, housing and labour market outcomes (see inter alia. Hanes and Machin, 2014; Braakmann, 2010; Rabby and Rodgers, 2010; Ratcliffe and von Hinke Kessler Scholder, 2015).

Few papers use terrorist activities to examine the effect of an increase in societal racism on health and wellbeing. This dearth of research reflects a lack of suitable data. By 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. In this context, administrative data might include birth registers or hospital records, which limits both the outcomes that can be studied and the availability of potential control variables given that socio-economic information on patients is sparse. While government-funded surveys provide an attractive alternative, these surveys must coincide with terrorist events *and* collect information on health-related topics. While most surveys, regardless of the focus, ask respondents about their employment status, information on health is more limited, if at all collected. Government-funded surveys of children and adolescents are scarcer still, and as a result, there exist very few opportunities to examine the impact of terrorist activity on the health-related outcomes of young people.

To investigate the impact of terrorism on adult wellbeing, Romanov et al. (2012) exploit the availability of time, date and location of interview in the Israeli Social Survey to match information on terrorist attacks to individuals in these data. They find little evidence that terrorist activity influences the life satisfaction of Jews, unless these attacks occur in their city of residence, when a decrease in wellbeing is observed. In contrast, terrorist activity - particularly where civilian casualties are involved - has a detrimental effect on the life satisfaction of Arabs regardless of the location of terror attacks. These findings are consistent with increasing levels of hostility after terrorist acts.

Johnston and Lordan (2012) exploit the fact that the 1999 and 2004 rounds of the Health Survey for England provide before and after data for 9/11. They use a difference-in-differences methodology to examine changes in mental and physical health among Pakistani and Bangladeshi Muslim adults relative to non-Muslim Indian adults over this 5-year period. Interestingly, they find evidence of a deterioration in the physical health of Muslims but little evidence of an increase in psychological distress.

The only study to consider the effects of terrorist activity on child outcomes uses administrative data. Lauderdale (2006) examines the impact of the 9/11 bombings on birth outcomes in California using the entire record of birth certificates. She compares the birth weight of children born to Arab mothers in California pre and post 9/11 relative to non-Hispanic whites, and finds an increase in low-birth-weight babies for Arab mothers.

Our research also relates to a large applied economics literature using the method of difference-in-differences to identify the causal impact of policy changes. As this identification strategy involves taking differences in average outcomes across treated and control groups, it is less suited to ordered outcomes given that the average of the outcome variable is frequently meaningless (Boes, 2013). For subjective wellbeing data, these averages and their differences are meaningful only in the event that wellbeing is cardinal.[1] Further issues arise if models suited to the statistical properties of limited

---

[1] While the influential paper by Ferrer-i-Carbonell and Frijters (2004) find in an empirical application that assuming ordinality or cardinality of subjective wellbeing data makes little difference, Dickerson et al. (2014) show using

dependent variables are used, which have been articulated in the context of a binary dependent variable by Blundell and Costa Dias (2009) and Lechner (2011). Even though the average of a binary outcome has a natural interpretation, as it represents the probability of a positive response, retaining the common trends assumption for this probability is difficult because it is a non-linear function of a time-invariant treatment group fixed effect. This fixed effect is not removed by taking differences, and the treatment effect is therefore only identified by assuming there are no systematic differences between the treated and control groups. One solution is to assume common trends at the level of the latent variable instead of the response probability, with various estimators proposed for binary dependent variables (Blundell et al., 2004; Lechner, 2011; Puhani, 2012). While similar issues apply to ordered outcomes, there is the added complication that the average of the outcome variable has little meaning, and to our knowledge a suitable estimator for the method of differences-in-differences has not yet been described. Indeed, research analysing subjective wellbeing data with difference-in-differences designs typically proceeds with linear regression methods (Gregg et al., 2009; Brodeur and Connolly, 2013) or by creating separate indicator variables for each response category (Gruber and Mullainathan, 2005; Leicester and Levell, 2013). The latter approach is somewhat troublesome for middle response categories.

In this paper, we add to the current literature by exploiting the timing of a nationally representative government-sponsored survey of English teenagers to examine the impact of a shock to racism on adolescent wellbeing. Given the transition from childhood to adulthood is a key developmental stage, paving the way for later life success, the lack of causal evidence for this age-group represents a serious gap in the literature. In addition, instead of assuming that our measure of wellbeing is cardinal from the outset, we extend Lechner (2011) to propose a suitable estimator for the treatment effect when using the method of differences-in-differences with ordered data. This approach can be used to analyse changes in policy with a wide array of ordered data that are of interest in empirical settings.

3   Data and methodology

3.1   Data

We use data from the Longitudinal Survey of Young People in England (LSYPE), which is a large panel survey interviewing young people attending school in England. LSYPE follows a two-stage sampling design, sampling schools and then students within those schools; with schools in deprived areas and pupils from Indian, Pakistani, Bangladeshi, Black African, Black Caribbean and Mixed backgrounds over-sampled.[2] All adolescents are first interviewed in 2004 in Year 9 (aged 13-14), with

---

simulated data that assuming cardinality can lead to bias in some settings.

[2]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 consistent estimation of causal effects as long as the sampling is independent of the dependent variable conditional on the explanatory variables, which is plausible in our context.

annual interviews thereafter until 2010.[3] 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 are conducted in the home, where detailed information is collected from the respondent and relevant household adults on the attitudes, experiences and behaviours of the respondent, and the family environment.

In 2005, respondents self-completed the General Health Questionnaire (GHQ), which is a screening instrument originally designed to assess mental distress in those aged 16+ (Goldberg, 1972) though evidence suggests it can be successfully used on adolescents (French and Tait, 2004). The version in the LSYPE contains 12 items covering issues ranging from sleep loss to feelings of self-worth. While answers to GHQ items are typically aggregated to produce a mental distress score (see for example Metcalfe et al., 2011), the ability of respondents in the LSYPE to select a *'Don't know'* option for each GHQ item makes this approach less attractive. Since *'Don't know'* responses comprise anywhere between 2-11% of supplied responses to any given GHQ item, we can only construct GHQ scores for 78% of respondents. We take advantage of the fact that two GHQ items focus on affect: *'Have you recently been feeling reasonably happy, all things considered?'* with options *'More so than usual'*, *'About the same as usual'*, *'Less so than usual'*, *'Much less than usual'*, *'Don't know'* and *'Have you recently been feeling unhappy and depressed?'* with options *'Not at all'*, *'No more than usual'*, *'Rather more than usual'*, *'Much more than usual'*, *'Don't know'*. The former item is similar to the question on happiness appearing in the General Social Survey[4] that is extensively analysed in the wellbeing literature (see e.g. Stevenson and Wolfers, 2012) while the latter item is in keeping with a greater focus on depression in the literature examining the impact of racism on child health outcomes Priest et al. (2013). As noted in Headey and Wooden (2004), wellbeing and illbeing are not opposite ends of the same spectrum, and factors affecting wellbeing may differ from those influencing illbeing. Although items are evaluated relative to a 'usual state', the evidence suggests respondents view their 'usual state' as one without symptoms (Goldberg, 1972). We therefore create a happiness score, with higher values indicating increasing happiness, and a depression score with higher values indicating greater suffering. *'Don't know'* responses respectively comprise 5% and 3% of answers to the happiness and depression items and we exclude these in our analyses.[5]

A key feature of the 2005 survey is that the fieldwork spans the six month period between 18th April - 18th September, with 94% of interviews taking place between 1st May - 31st August. Thus the London bombings occur midway through the survey period, and as interview month and year are available, we are able to obtain a reasonably clean separation of the pre and post treatment period. Our pre-treatment period therefore spans 1st May - 30th June and our post-treatment period spans 1st July - 31st July. We focus on this short window to ensure that the pre and post periods are as similar as possible. A particular concern is that August falls entirely into the summer holiday

---

[3]Repeating a school year is rare in the UK.

[4]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?'

[5]We have also analysed GHQ scores and find substantively similar results.

period and children from different backgrounds may be engaged in very different activities during this period. Moreover, many families go abroad during this period, and adolescents interviewed after foreign holidays may not have been exposed to the news or to the prevailing temperament in the UK. If anything, we find slightly larger effects if August is included in the post-treatment period but we prefer to analyse only the period when children are mostly still in school. We exclude April and September due to relatively few interviews occurring in these months (especially for treated individuals), and moreover because September coincides with a new academic year. Our identification strategy relies on randomly allocated interview dates in the LSYPE and below we show that there is very little difference in the composition of people interviewed pre and post treatment.

LSYPE also collects information on ethnicity and religion. The existing literature on the effect of extremist Islamic terrorism on the outcomes of minorities constructs the treated population on the basis of religion, ethnicity or both. While some studies exclude Indians altogether on the basis that it is not clear whether Indians are treated (Braakmann, 2010; Kaushal et al., 2007), other studies use non-Muslim Indians as a control group (Johnston and Lordan, 2012). Our main analyses focus on the impact of the bombings on Muslims and use non-Muslims as our control group, which means that we compare the change in outcomes among Muslims, who are almost exclusively non-white,[6] to the change in outcomes among non-Muslim whites and minorities.[7] While we could restrict our control group to non-Muslim ethnic minorities or non-Muslim Indians, Figures 1 and 2 indicate that pre-treatment trends in happiness and depression are very similar between Muslims and non-Muslims while Figures A1 and A2 in the Appendix show signs of dissimilar trends. Figure 1 shows relatively constant differences in happiness among treated and control groups in May and June, with Muslim teenagers happier on average than non-Muslims teens. From July onwards, this difference diminishes, largely driven through a decline in the happiness of Muslim teenagers. Conversely, there is little indication of changes in depressive symptoms among Muslims relative to non-Muslims after the bombings. These figures, however, do not control for any differences in the characteristics of Muslims and non-Muslims.

After cleaning our data and imposing restrictions on the interview month, our sample comprises 1182 Muslims and 8130 non-Muslims, with 36% of Muslims and 27% of non-Muslims interviewed in July 2005, which partially reflects regional differences in the timing of interviews. Table 1 presents summary statistics, distinguishing between Muslims and non-Muslims, and pre/post bombings within these groups. As Table 1 shows, differences exist between Muslims and non-Muslims in terms of attitudes, family circumstances, and area of residence. For example, Muslim teenagers are more ambitious and have more siblings compared to others. Their parents are less likely to have a degree and work full-time. Unfortunately, earnings data is missing for 19% of the sample so we only

---

[6]Just under 3% of Muslims in our sample are white.

[7]We have considered the impact of the bombings on South Asians and South Asian Muslims and a similar pattern emerges.

construct a dummy variable indicating household earnings are in the top third of the distribution. People with missing data are placed into the base category, where they might likely have been had their earnings data been available. This proxy of economic resources, as well as benefit receipt and home ownership levels suggests that parents of Muslim teens are typically poorer. On the other hand, there are very few differences in the composition of Muslim (non-Muslim) samples before and after the bombings. These summary statistics suggest that a combination of difference-in-differences and a before-after comparison across Muslims alone may be required to robustly establish the impact of the bombings on teenage wellbeing. One advantage to using a difference-in-differences strategy is that temporal effects in wellbeing can be taken into account. This is important because, for example, schools often wind down the school year in July, resulting in fewer academic stresses and more leisure activities in final teaching weeks. This, in combination with better weather and longer evenings, suggests the wellbeing of teenagers may improve in July so that ignoring temporal effects may understate any treatment effect. Another advantage is that any unobserved differences between people interviewed earlier and later in the survey period would be differenced away so long as any unobserved differences are unrelated to treatment status. However, a disadvantage to the method of difference-in-differences when the treated and control groups are somewhat dissimilar is that the common trends assumption may not be satisfied, and more generally, issues of balance and overlap may influence treatment effect estimates in a regression context. A before-after comparison across Muslims alone guarantees a comparison of similar individuals but assumes zero temporal effects in wellbeing. We seek to investigate that both approaches yield the same overall conclusions.

3.2 Empirical Model

Both dependent variables are ordered variables and using a linear regression to model the outcome requires assuming that, for example, happiness is cardinal. A priori, however, it is unclear that *'More so than usual'* represents twice the level of happiness as *'Less so than usual'*. As discussed earlier, several issues arise when trying to analyse ordered data in a difference-in-differences framework. In this research, we suggest that a solution is to construct the treatment effect in terms of the probability that a given response category is observed (i.e. the response probability) while also assuming common trends at the level of the latent variable. This allows us to build on the exposition in Lechner (2011) for a binary dependent variable, and we follow his discussion based on potential outcomes to begin with. Thus each individual has two potential outcomes, and assignment to treatment determines which of these potential outcomes is realised. In this context, the potential outcomes are the response categories, with Y_i^1 denoting the potential outcome with treatment and, Y_i^0, the potential outcome without treatment.[8] We assume that there is some underlying unobserved potential latent wellbeing index that drives these potential outcomes. Thus each individual has two potential latent wellbeing indices, Y_i^{1*} and Y_i^{0*}, similarly linked to treatment states. As the potential

---

[8]To simplify the exposition we have omitted time subscripts but these can easily be accommodated.

latent wellbeing index underlies the potential outcome, it is the former that takes primary focus, and it is modelled as a function of group membership, time, and individual-level characteristics:

$$Y_i^{1*} = \beta_1 D_i + \delta^1 T_i + x_i'\gamma + \varepsilon_i \tag{1}$$

$$Y_i^{0*} = \beta_1 D_i + \delta^0 T_i + x_i'\gamma + \varepsilon_i \tag{2}$$

where D_i is equal to one if an individual is assigned to treatment and is zero otherwise. This feature allows the potential latent wellbeing index with and without treatment to differ by an amount β_1 for individuals assigned to treatment, relative to those not assigned to treatment, and captures a time-invariant treatment group fixed effect. T_i is equal to one where an individual is observed in the post-treatment period and is zero otherwise. Thus an individual's potential latent wellbeing index under treatment shifts by an amount δ^1 in the post-treatment period whereas it shifts by an amount δ^0 without treatment, with δ^1-δ^0 capturing the effect of treatment. Of course, we are not interested in the effect of treatment on the latent variable per se, and we show below how this would translate into a treatment effect for the probability of observing a specific response category. Finally, x_i is a vector of individual characteristics and ε_i is an error term, which is assumed to be IID standard normal. The assumption of common trends in the latent variable is embedded in equation 2, where the potential latent wellbeing index without treatment follows the same trajectory for treated and control groups i.e. $E(Y_i^{0*} \mid D_i = 1, T_i = 1, x_i) - E(Y_i^{0*} \mid D_i = 1, T_i = 0, x_i) = E(Y_i^{0*} \mid D_i = 0, T_i = 1, x_i) - E(Y_i^{0*} \mid D_i = 0, T_i = 0, x_i) = \delta_0$.

Our point of departure from Lechner (2011) is that we are interested in a treatment effect with an ordered response model. We therefore turn our attention to showing that a treatment effect is identified with an ordered response model when common trends are assumed at the level of the latent variable. This first requires outlining how the potential latent wellbeing index is linked to both the potential outcome and the probability of observing that the potential outcome is equal to a specific response category, which together provide a basis for constructing a treatment effect. For example, the potential latent wellbeing index maps onto the potential outcome as follows:

$$Y_i^s = k \;\; \text{if} \;\; \mu_k < Y_i^{s*} \le \mu_{k+1}, \;\; k = 1, ..., K \tag{3}$$

where s=0,1 so that Y^s denotes either of the two potential outcomes for each individual and k is one of multiple ordered response categories ranging from 1 to K. Thus we observe a potential outcome to be equal to the response category k if the associated potential latent wellbeing index falls within the range defined by the two threshold parameters μ_k and μ_{k+1}. The threshold parameters are assumed to be strictly increasing in k ($\mu_k < \mu_{k+1} \; \forall k$) with $\mu_1 = -\infty$ and $\mu_{K+1} = \infty$. The

probability that a potential outcome is equal to the response category k is given by:

$$P_{ik} = E(I(Y_i^s = k) \mid D_i, T_i, x_i)$$
$$= \Phi(\mu_{k+1} - E(Y_i^{s*} \mid D_i, T_i, x_i)) - \Phi(\mu_k - E(Y_i^{s*} \mid D_i, T_i, x_i)) \tag{4}$$

where $I(\cdot)$ is the indicator function and $\Phi(\cdot)$ is the standard normal CDF.

As the potential latent wellbeing index maps onto the probability that the potential outcome is equal to a specific response category, a natural way to think about the treatment effect is in terms of the effect of treatment on the probability of observing a specific response category. Since we never observe both potential outcomes for any individual, we cannot identify the individual-level treatment effect, and instead focus on the average treatment effect on the treated (ATET). This is simply the expected difference in the probability of observing a specific response category across the two treatment states for a randomly chosen individual in the treated group:

$$ATET_{P_k} = E(I(Y_i^1 = k) - I(Y_i^0 = k) \mid D_i = 1, T_i = 1) \tag{5}$$

Clearly the ATET requires a counterfactual response probability for individuals assigned to treatment, but this can be identified using the assumption of common trends in the latent variable contained in equation 2. For example, the common trends assumption implies:

$$E(Y_i^{0*} \mid D_i = 1, T_i = 1, x_i)$$
$$= E(Y_i^{0*} \mid D_i = 1, T_i = 0, x_i) + E(Y_i^{0*} \mid D_i = 0, T_i = 1, x_i) - E(Y_i^{0*} \mid D_i = 0, T_i = 0, x_i) \tag{6}$$

which shows that the expected potential latent wellbeing index without treatment for treated individuals - required to model the counterfactual response probability as per equation 4 - can be expressed in terms of several expectations of the potential latent wellbeing index. Notice that all information required for the right hand side of equation 6 is available: the potential latent wellbeing index without treatment is realised pre/post treatment for untreated individuals while for treated individuals the potential latent wellbeing with treatment is realised in the pre-treatment period.[9] The latter is equivalent to the potential latent wellbeing without treatment, given there is no effect of treatment in this period. The required indices are easily obtained using equation 2 but it is more convenient to use the following realisation rule to convert the potential latent wellbeing into realised

---

[9]Note that we distinguish between *realised* and *observed*. While the latent wellbeing index is unobserved by definition, it is realised when it maps onto a realised (and observed) outcome.

latent wellbeing:

$$
\begin{aligned}
Y_i^* &= D_i Y_i^{1*} + (1 - D_i) Y_i^{0*} \\
&= D_i(\beta_1 D_i + \delta^1 T_i + x_i'\gamma + \varepsilon_i) + (1 - D_i)(\beta_1 D_i + \delta^0 T_i + x_i'\gamma + \varepsilon_i) \\
&= \beta_1 D_i + \delta^0 T_i + (\delta^1 - \delta^0) D_i T_i + x_i'\gamma + \varepsilon_i \\
&= \beta_1 D_i + \beta_2 T_i + \beta_3 D_i T_i + x_i'\gamma + \varepsilon_i
\end{aligned} \tag{7}
$$

where $\beta_2 = \delta^0$ and $\beta_3 = \delta^1 - \delta^0$. The expected potential latent wellbeing index without treatment for treated individuals in the post-treatment period is therefore:

$$
\begin{aligned}
E(&Y_i^{0*} \mid D_i = 1, T_i = 1, x_i) \\
&= E(Y_i^* \mid D_i = 1, T_i = 0, x_i) + E(Y_i^* \mid D_i = 0, T_i = 1, x_i) - E(Y_i^* \mid D_i = 0, T_i = 0, x_i) \\
&= \beta_1 + \beta_2 + x_i'\gamma
\end{aligned} \tag{8}
$$

This suggests that the counterfactual response probability in the post-treatment period is given by:

$$
E(I(Y_i^0 = k) \mid D_i = 1, T_i = 1, x_i) = \Phi(\mu_{k+1} - \beta_1 - \beta_2 - x_i'\gamma) - \Phi(\mu_k - \beta_1 - \beta_2 - x_i'\gamma) \tag{9}
$$

Thus an estimate of the ATET is given by:

$$
\widehat{ATET}_{P_k} = \frac{1}{N^1} \sum_{i=1}^{N} D_i T_i \left\{ I(Y_i = k) - \left[\Phi(\hat{\mu}_{k+1} - \hat{\beta}_1 - \hat{\beta}_2 - x_i'\hat{\gamma}) - \Phi(\hat{\mu}_k - \hat{\beta}_1 - \hat{\beta}_2 - x_i'\hat{\gamma}) \right] \right\} \tag{10}
$$

where $N^1 = \sum_{i=1}^{N} D_i T_i$.[10] The parameters in equation 10 are estimated by fitting the ordered probit (OP) model defined in equations 1-4 and 7 on the full sample. The standard error of $\widehat{ATET}_{P_k}$ may be obtained using bootstrapping.

Finally, it is also possible to obtain an ATET in terms of the expected value of the outcome variable:

$$
ATET_Y = E(Y_i^1 - Y_i^0 \mid D_i = 1, T_i = 1) = \sum_{k=1}^{K} k \times ATET_{P_k} \tag{11}
$$

Clearly, it is implicitly assumed that the outcome variable is cardinal when calculating $ATET_Y$, while the above discussion highlights the problem of imposing this assumption a priori. Nevertheless, $ATET_Y$ provides a useful summary measure of the treatment effect, which can be estimated using both linear and ordered models. For example, an alternative estimator of $ATET_Y$ used to date in the literature is the estimated coefficient on $D_i \times T_i$ in a linear regression of Y_i on D_i, T_i, $D_i \times T_i$ and

---

[10] An alternative estimator of $ATET_{P_k}$, which is a logical extension of the estimator proposed by Puhani (2012) in the context of binary choice models, is given by substituting $I(Y_i = k)$ in equation 10 by $\Phi(\hat{\mu}_{k+1} - \hat{\beta}_1 - \hat{\beta}_2 - \hat{\beta}_3 - x_i'\hat{\gamma}) - \Phi(\hat{\mu}_k - \hat{\beta}_1 - \hat{\beta}_2 - \hat{\beta}_3 - x_i'\hat{\gamma})$. Since the average of the predicted probabilities is not, in general, identical to the observed frequency of response category k, these estimates will not coincide.

x_i. A comparison of both approaches is of interest given the relative simplicity of latter approach, subject to the caveat regarding cardinality.

In addition to using the method of difference-in-differences, in which we compare the change in wellbeing of Muslims to non-Muslims, we also calculate the before-after difference in wellbeing across Muslims. This is done by fitting the ordered probit model defined in equations 1-4 and 7 on the treated group sample, excluding D_i and $D_i \times T_i$ as regressors. The estimate of $ATET_{P_k}$ can then be obtained via:

$$\widehat{ATET}_{P_k} = \frac{1}{N^1} \sum_{i=1}^{N} D_i T_i \left\{ I(Y_i = k) - [\Phi(\hat{\mu}_{k+1} - x_i'\hat{\gamma}) - \Phi(\hat{\mu}_k - x_i'\hat{\gamma})] \right\} \tag{12}$$

where the expression in the square brackets represents the unobserved untreated potential outcome. This assumes that, conditional on x_i, no factors other than the treatment have an impact on the difference in reported wellbeing between individuals observed in the pre- and post-treatment periods. As before an estimate of $ATET_Y$ based on equation 11 can be obtained.

Both the method of difference-in-differences and the before-after comparison exploit variations in wellbeing over time in order to estimate the treatment effect. In doing so, the former approach requires that there are no other contemporaneous events that would differentially affect the wellbeing of Muslims versus non-Muslims while the latter approach requires that there are no other contemporaneous events. Such contemporaneous events may include religious festivities, exams and school holidays. A coincidence of several factors suggests that this requirement may be satisfied. Firstly, in 2005 key religious festivals took place outside of our observation window, with Lent and Passover occurring beforehand, and Ramadan and Diwali afterwards. Secondly, students in England are tested at the end of Key Stage 3 in Year 9 and at the end of Key Stage 4 in Year 11, with the latter determining whether students can progress to post-compulsory education. Given that we observe adolescents towards the end of Year 10, we avoid national exams, but that does not preclude students taking school-specific exams. Finally, national school holidays begin towards the end of July, and children from different backgrounds may be engaged in different activities during this time. Unfortunately, we do not have access to the exact date of interview so our effect may to some extent pick up school holiday activities. However, the second wave of bombings occurred towards the end of July, with the atmosphere surrounding this second attack likely to dominate any holiday effect in the final week of July. Moreover, holiday activities may change in response to the bombings.

Metcalfe et al. (2011) present evidence of widespread declines in wellbeing following terrorist attacks arising through a 'fear and destruction' channel. If treated and control groups respond in the same way to a 'fear and destruction' channel, any additional effect on Muslim teenagers would reflect the shock to racism, and we still estimate the treatment effect in a difference-in-difference setting. However, if Muslims respond more strongly to this channel, because the attacks were carried out by other (albeit extremist) Muslims, combined with the resulting media coverage, we no

longer estimate the causal effect of racism but instead estimate a causal effect of the bombings. If non-Muslims respond more strongly to this channel, we could understate the causal effect of racism or the bombings. For the before-after comparison across Muslims alone, the estimated treatment effect may combine both the shock to racism and any 'fear and destruction' effect.

Finally, in using cross-section data, we also require that there are no changes in the composition of our treated and control samples before and after the bombings. However, as we show in Table 1, the composition of the Muslim (non-Muslim) sample before and after the bombings is similar.

4 Results

4.1 Main results

Table 2 presents our main results. Rows 1-4 focus on the probability of observing a particular response category (see equation 10) while rows 5-6 focus on the expected value of the outcome variable, where row 5 presents estimates using the modelled response probabilities (see equation 11) and row 6 presents linear regression estimates. Results presented in columns 1-3 are based on the method of difference-in-differences and on a before-after comparison in columns 4-6. We first focus on the impact on all teenagers, and subsequently by males and females.

The results in column 1 suggest an adverse effect of the London bombings on the happiness of Muslims relative to non-Muslim teenagers. Specifically, the bombings increase the probability of response categories *'Much less than usual'* and *'Less so than usual'* by 2 and 3 percentage points relative to non-Muslims, with both estimates statistically significant at the 10 percent level. These are large effects as the pre-treatment frequencies of these response categories are 2% and 7%, respectively (see Table 1).[11] Conversely, the probability of reporting *'About the same as usual'* decreases by 4 percentage points although this effect is not precisely determined, and there is little evidence that the bombings influence the probability of reporting *'More so than usual'*. Treating happiness as cardinal instead of ordinal to evaluate the average change in the happiness score suggests a decline of 0.06, which is equivalent to 2 percent of the average pre-treatment happiness score among Muslims (see Table 1). It seems to make little difference if a linear regression is used instead of the modelled response probabilities to evaluate the average change in the happiness score.

Although these results point towards widespread effects of the bombings, columns 2 and 3 tell a different story; the detrimental impact of the bombings is largely concentrated among teenage girls. Here the probability of responding *'Much less than usual'* increases by 4 percentage points relative to non-Muslim teenage girls, an effect that is statistically significant at the 5 percent level. There is also evidence of an increase in the probability of reporting *'Less so than usual'* and a substantial

---

[11]Since only a small number of respondents reported *'Much less than usual'* in the pre-treatment period we also ran the ordered probit and linear regression models with *'Much less than usual'* and *'Less so than usual'* combined into one category as a robustness check. The results, which are available from the authors upon request, are consistent with the findings from the analysis in which these categories are treated as being separate.

decline in the probability of reporting *'More so than usual'*, but those estimates are imprecise and insignificantly different from zero. For teenage boys, the pattern of changes in each response category is not consistent with a general decline in happiness. Accordingly, the happiness score falls by 0.16 units for teenage girls, with little evidence to support a decline in happiness among teenage boys. These findings concur with qualitative evidence suggesting that Muslim women have been particularly affected by the increase in Islamophobia following the London bombings, a phenomena that is attributed to Muslim women being more easily identifiable (Change Institute, 2009). The 2007-2008 Citizenship Survey provides further evidence that women are more prone to believing that racial or religious harassment is a problem in their local area compared to men (DCLG, 2010). If gender differences exist in perceptions, and experiences, of racism, this may provide one explanation for these results. However, it is also possible that teenage girls face similar levels of exposure to racism both directly, or indirectly via a parent, and simply react differently. For example, the findings of Borrell et al. (2006) suggest that women suffer more than men after experiences of racism while Ge et al. (2006) show that the depressive symptoms of teenage girls are more responsive to stressful life events compared with teenage boys.

The before-after results reported in columns 4-6 are also supportive of a decline in the wellbeing of Muslim teenage girls, though the estimates are smaller in absolute value. This would be consistent with the method of difference-in-differences taking into account temporal effects in wellbeing (with wellbeing increasing over time) and the before-after comparison ignoring temporal effects.

Results presented in Panel B depict a modest, if any, effect of the bombings on feelings of unhappiness and depression. Relative to non-Muslims there is some evidence of an increase in the *'Much more than usual'* response category and of a decline in response categories *'Rather more than usual'* and *'No more than usual'*. However, when looking at teenage boys and girls separately, the pattern of changes in each response category is not consistent with a general increase in depression, with reductions in middle response categories offset by increases in response categories both above and below. In line with this, there is little evidence of an overall increase in depression levels.

From a methodological viewpoint, our results suggest that the ordered probit approach provides a more complete picture of the impact of the bombings, with the clearest effects observed at the lower end of the happiness distribution. However, there is also suggestive evidence of an effect across the entire happiness distribution. In subsequent analyses, for brevity, we restrict our attention to the effect of the bombings on the conditional mean, and having seen that there are few advantages to the ordered probit approach in this context we use linear regression. We also adopt the before-after comparison over the method of difference-in-differences, as the results using both methods are similar, and the former appears to provide more conservative estimates of the bombings.

4.2 Robustness checks

As described in the previous sub-section , we find evidence of a decline in happiness among Muslim teenage girls after the bombings. While this evidence is consistent with an increase in societal racism, there are other potential explanations for these findings. For example, happiness may change naturally over time, or teenagers interviewed later in the survey period may think and feel differently to those interviewed earlier because of unobserved differences between respondents. We investigate these possibilities by considering two falsification checks. First, we exclude July from our sample and assign a pseudo treatment to June 2005. Any evidence of a change in happiness or depression in June would indicate that our results are driven by something other than the London bombings. Results from this exercise, which are presented in Table 3, provide little evidence to support changes in happiness prior to the bombings. Second, we investigate the effect of treatment on outcomes arguably unaffected by the treatment, for example, those which are measured in 2004, the year prior to treatment. We examine respondents' outlook on life in 2004, such as how good they think they are at school work, and how important it is to them to have a family or a well-paid job. Specifically, we create three binary variables to indicate that respondents consider themselves to be 'very good at school work' and express a strong agreement to statements that 'raising a family' and 'a job with good pay' are important. Results from this exercise are presented in columns 1-3 of Table 4, where there does not appear to be any systematic differences in these outcomes across treatment and control groups. Finally, in column 4 of Table 4, we consider the impact of the bombings on respondents' health over the last 12 months. We would not expect the bombings to have any influence on this outcome because the time-frame for this assessment falls largely prior to the treatment. Moreover, this question is asked prior to the GHQ, and mental health did not feature highly on the NHS agenda at the time,[12] so that teenagers would likely have evaluated their physical health in response to this question. While the literature linking racism to stress suggests an immediate effect of racism on affect, any deterioration in physical health would take time to materialise. Thus we would argue that any differences in self-reported health in the previous 12 months might be symptomatic of unobserved health-related differences between the treatment and control group. In order to examine this possibility, we create a binary variable to indicate whether respondents believe their health in the last 12 months has been 'very good'. However, as results presented in Table 4 show, there does not appear to be any underlying differences in health across those interviewed earlier and later in the year.

4.3 Potential explanations

In this section we explore potential explanations for the decline in happiness using available information in LSYPE. First, we consider whether teenagers might be exposed to bullying at school

---

[12] In an unprecedented move, the Department of Health announced its plan to plough funding into psychological therapies in late 2007 (DH, 2007).

following the bombings. Respondents are asked if they have experienced various forms of bullying at school in the past 12 months. While this time frame falls largely prior to the treatment, if there is a large and immediate impact of the bombings on school bullying, an effect of treatment on school bullying may still be detectable. Second, we consider to what extent the bombings may have disrupted teenagers' routines, for example, whether teenagers faced restrictions on their social life after the bombings. Specifically, we create a binary variable measuring restrictive contact with friends in the past week i.e. respondents neither receive friends at home nor go out with friends. Finally, we also examine teenagers' perceptions of discrimination. For example, teenagers are asked whether their skin colour, ethnic origin or religion might create difficulties in pursuing higher education. Since applications for further study are usually dealt with by centralised admissions processes, involving very little contact between the institution and the applicant, answers to this question may reveal concerns regarding institutionalised racism in higher education providers. Respondents are also asked whether their skin colour, ethnic origin or religion might increase the difficulty of finding a job after leaving education. In contrast to the education context, an employer may be in a position to exercise some discretion in their decision to interview and hire an individual, and may face greater incentives to do so given the likelihood of close contact with an employee on the job. While an employer may seek to discriminate against individuals if they are themselves prejudiced, they may also engage in discriminatory practices if they think that their current workforce and/or customers are prejudiced. Thus answers to this question may reveal specific concerns about racism among employers but may also be symptomatic of wider concerns about racism in society. Results are presented in Table 5. There is little evidence that the bombings changed tangible aspects of teenagers lives via bullying or via social contact with friends. However, some interesting results emerge in the context of discriminatory behaviour. For example, there is little evidence to suggest that teenagers are more inclined to worry about institutionalised racism after the bombings. Given that offer rates are broadly in line with expected offer rates for ethnic minority applicants in the UK higher education system,[13] in combination with the fact that meeting the required entry grades is largely determined by an individual's own preparation, it may not be surprising that perceptions of institutionalised racism in higher education are unchanged after the bombings. In contrast, teenagers are more prone to thinking they will experience discrimination in the labour market following the bombings, with a 5 percentage point rise in the probability of reporting that discrimination will harm future job placement. Moreover, consistent with our previous results on the happiness of Muslim teenagers, this change in perceptions appears to be largely driven by a change in the outlook of Muslim teenage girls. Thus increased perceptions of discrimination and reduced happiness among teenage girls may be interlinked.

---

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

4.4 Extensions

In this section, we investigate whether there are heterogenous treatment effects among teenagers living in relatively poor and ethnically concentrated areas. The theoretical and empirical literature suggests scope for race relations to differ across poverty and ethnic concentration, and therefore it is of interest to investigate whether the impact of the bombings varies across these dimensions. While models of intergroup conflict cite the importance of economic conditions in shaping race relations (see e.g. Olzak, 1990), the causal empirical evidence suggests that it is ethnic concentration that is more harmful to race relations. However, while ethnic concentration encourages hostile attitudes (Dustmann et al., 2011), its relationship with physical harassment is less well understood. For example, Antecol and Cobb-Clark (2010) find that ethnic concentration leads to greater physical harassment of minorities while Dustmann et al. (2011) suggest the opposite. Nevertheless, the implication remains that exposure to racism may differ across neighbourhoods. In addition, there are other reasons why neighbourhood attributes may matter in the context of wellbeing. Becares et al. (2009) argue that in the event of racist treatment or fears of such treatment, ethnic minorities living in ethnically concentrated areas are likely to have better access to supportive networks, which may alleviate distress. Although the above evidence corresponds to normal times, which may not be representative of the period following the bombings, it does suggest that poverty and ethnic concentration may play a role in shaping how teenagers fare in the aftermath of the bombings. Moreover, research suggests that the impact of neighbourhoods may differ for teenage boys and girls. Kling et al. (2007) show that the outcomes of teenage girls, and particularly mental health outcomes, are improved by moving to better neighbourhoods, while in contrast teenage boys languish.

We have access to data on the characteristics of the neighbourhood and the school attended by adolescents, and are therefore able to compare and contrast the impact of schools versus neighbourhoods. School characteristics are of interest because many interactions with other ethnic groups occur in school. In addition, schools are in a good position to react to the bombings by offering counselling or leisure activities to students, with such responses likely to reflect the ethnic diversity of pupils. Finally, schools are typically more segregated than neighbourhoods in England (Johnston et al., 2008). A priori, it is therefore unclear how the characteristics of neighbourhoods or schools would shape teenage wellbeing in the aftermath of the bombings, and whether the impact of schools might differ from that of neighbourhoods.

To capture neighbourhood poverty we use the 2004 Index of Multiple Deprivation (IMD) measured at the level of Lower Super Output Areas (LSOA), which roughly corresponds to 1000 households. Measuring neighbourhood segregation is less straightforward because we seek to measure segregation along the lines of religion but only have access to data on ethnicity. Moreover, the lowest available geography at which ethnicity data is available is Local Authority Districts (LAD), which are responsible for providing local public services in England. We therefore use Population Estimates by Ethnic Group (PEEG) available from the Office for National Statistics to calculate the proportion of Pakistanis and Bangladeshis living in each LAD. We use Pakistani and Bangladeshi

populations to proxy Muslims because over 98% of individuals corresponding to these ethnic groups are Muslim in the LSYPE (which has cruder definitions of ethnicity compared to PEEG). Free School Meal (FSM) eligibility is used to measure poverty in schools while segregation in schools is measured as the proportion of Pakistanis and Bangladeshis attending. It should be noted that the observed levels of poverty and ethnic concentration are those that arise after endogenous location decisions have taken place. Thus ethnic minority individuals living in the same area (attending the same school) may be similar in unobservable dimensions, which may be driving the results we obtain. In addition, the most racist whites would unlikely reside in ethnically diverse areas, although here it might be argued that it is the prevailing circumstances that would matter in shaping the experiences of ethnic minority residents. While the existence of endogenous location decisions rules out any causal interpretations, our ambition to document possible heterogeneities in the treatment effect remains an interesting exercise and useful benchmark for further work.

To investigate the influence of area and school context, we augment our baseline specification to include the relevant measure of poverty (ethnic segregation) and its interaction with the post-treatment dummy. For ease of interpretation, Table 6 presents the implied treatment effect at the 25th, 50th and 70th percentiles of the poverty (ethnic segregation) distribution. Interestingly, for Muslim teenage girls only, these results indicate that poverty may be an important factor in shaping reactions to the bombings, with teenagers living in poverty showing signs of greater distress. For example, while there is very little effect of the bombings on Muslim teenage girls living in more affluent areas (relative to other Muslims), the treatment effect at the median is comparable in magnitude to our previous results. For Muslim teenagers living in the poorest areas, the effect of the bombings is much larger, representing a 7 percent decline in pre-treatment happiness levels. We also find a greater impact of the bombings on teenage girls living in more ethnically concentrated areas, a finding that is consistent with a greater preponderance of hostile attitudes in such areas. Results are also remarkably similar if we consider the characteristics of the school instead of the area. While these findings might indicate that both poverty and ethnic segregation are important in shaping adolescent outcomes following the bombings, we are unable to disentangle the effect of poverty from ethnic concentration, or indeed alternative explanations. Regardless, these findings serve to document important variations in the impact of the bombings. Finally, while there may exist an interplay between poverty and ethnic concentration for happiness, there is no equivalent evidence to support heterogeneous impacts of the bombings on depression.

5   Conclusion

This paper exploits the timing of the London bombings to estimate the effect of a sharp increase in racism on the wellbeing of young Muslims. Experiences of racism during formative years may impact on emotional wellbeing (Priest et al., 2013), with research showing that better emotional health in adolescence is linked to economic success in later life (Ding et al., 2009; De Neve and

Oswald, 2012; Busch et al., 2014). Using a difference-in-differences as well as a before-after design, we show that the happiness of Muslim teenage girls declined in the immediate aftermath of the bombings while Muslim teenage boys were largely unaffected. These differences are consistent with differential exposure to racism following the bombings (Change Institute, 2009), and with differential responses to stressful life events (Ge et al., 2006) and racism (Borrell et al., 2006). Moreover, we show that this drop in happiness for teenage girls is accompanied by increased expectations of facing discrimination. Our findings also indicate that the fall in happiness is greatest for teenage girls facing the highest levels of deprivation and ethnic segregation.

From a methodological viewpoint, we extend the framework presented in Lechner (2011) in the context of a binary dependent variable, in order to apply the method of difference-in-differences to ordered data. Thus we propose evaluating the treatment effect in terms of response probabilities while assuming common trends at the level of the latent wellbeing index. This approach can be used to analyse the wide array of ordered outcomes that feature in economic analyses. Advantages over linear regression include the ability to ascertain whether the treatment effect materialises across the entire distribution of wellbeing or is limited to particular sections of it, with these alternative scenarios likely warranting different policy responses. We find strongest evidence of an impact at the lower end of the happiness distribution, and suggestive evidence of an impact across the entire distribution of happiness in our research.

While we find robust evidence of decrease in happiness among Muslim teenage girls, we find little evidence of an increase in depression. This could be a genuine result since wellbeing and illbeing are distinct dimensions (Headey and Wooden, 2004, see), but we cannot be certain that teenagers simply find it harder to answers questions on depression because they are less familiar with this concept. Nevertheless, positive and negative emotions are important to later economic success (Ding et al., 2009; De Neve and Oswald, 2012; Busch et al., 2014), and a substantial body of evidence suggests that positive affect is key to successes in work and relationships for adults (Lyubomirsky et al., 2005; Oswald et al., 2015). Future research might therefore consider the long-term impact of the bombings on adolescent wellbeing and related outcomes.

References

Altonji, J. G. and R. M. Blank (1999). Race and gender in the labor market. Volume 3, Part C of *Handbook of Labor Economics*, pp. 3143 – 3259. Elsevier.

Antecol, H. and D. A. Cobb-Clark (2010). The effect of community-level socio-economic conditions on threatening racial encounters. *Regional Science and Urban Economics 40*(6), 517 – 529.

Becares, L., J. Nazroo, and M. Stafford (2009). The buffering effects of ethnic density on experienced racism and health. *Health & Place 15*(3), 700 – 708.

Blundell, R. and M. Costa Dias (2009). Alternative approaches to evaluation in empirical microeconomics. *Journal of Human Resources 44*(3), 565–640.

Blundell, R., M. C. Dias, C. Meghir, and J. van Reenen (2004). Evaluating the employment impact of a mandatory job search program. *Journal of the European Economic Association 2*(4), 569–606.

Boes, S. (2013). Nonparametric analysis of treatment effects in ordered response models. *Empirical Economics 44*(1), 81–109.

Borrell, L. N., C. I. Kiefe, D. R. Williams, A. V. Diez-Roux, and P. Gordon-Larsen (2006). Self-reported health, perceived racial discrimination, and skin color in African Americans in the CARDIA study. *Social Science & Medicine 63*(6), 1415 – 1427.

Braakmann, N. (2009). The impact of September 11th 2001 on the employment prospects of Arabs and Muslims in the German labor market. *Journal of Economics and Statistics 229*, 2–21.

Braakmann, N. (2010). Islamistic terror and the labour market prospects of Arab men in England: Does a country's direct involvement matter? *Scottish Journal of Political Economy 57*(4), 430–454.

Brodeur, A. and M. Connolly (2013). Do higher child care subsidies improve parental well-being? Evidence from Quebec's family policies. *Journal of Economic Behavior & Organization 93*, 1 – 16.

Busch, S. H., E. Golberstein, and E. Meara (2014). The FDA and ABCs: Unintended consequences of antidepressant warnings on human capital. *Journal of Human Resources 49*(3), 540–571.

Carter, R. (2007). Racism and psychological and emotional injury: Recognizing and assessing race-based traumatic stress. *The Counseling Psychologist 35*, 13–105.

Caughy, M. O., P. J. O'Campo, and C. Muntaner (2004). Experiences of racism among African American parents and the mental health of their preschool-aged children. *American Journal of Public Health 94*(12), 2118 – 2124.

Change Institute (2009). Summary Report. Uderstanding Muslim Ethnic Communities. Report, Department for Communities and Local Government.

Cohen, S., D. Janicki-Deverts, and G. E. Miller (2007). Psychological distress and disease. *Journal of the American Medical Association 298*(14), 1685–1687.

Dávila, A. and M. Mora (2005). Changes in the earnings of Arab men in the US between 2000 and 2002. *Journal of Population Economics 18*(4), 587–601.

DCLG (2010). Attitudes, values and perceptions. Muslims and the general population in 2007-08. Report, Department for Communities and Local Government.

De Neve, J.-E. and A. J. Oswald (2012). Estimating the influence of life satisfaction and positive affect on later income using sibling fixed effects. *Proceedings of the National Academy of Sciences of the United States of America 109*(49), 19953–19958.

DH (2007). Johnson announces 170 million boost to mental health therapies. Wednesday 10 October. Press release, Department of Health.

Dickerson, A., A. R. Hole, and L. A. Munford (2014). The relationship between well-being and commuting revisited: Does the choice of methodology matter? *Regional Science and Urban Economics 49*, 321–329.

Ding, W., S. F. Lehrer, J. Rosenquist, and J. Audrain-McGovern (2009). The impact of poor health on academic performance: New evidence using genetic markers. *Journal of Health Economics 28*(3), 578 – 597.

Dustmann, C., F. Fabbri, and I. Preston (2011). Racial harassment, ethnic concentration, and economic conditions. *The Scandinavian Journal of Economics 113*(3), 689–711.

Erikson, E. (1968). *Identity: Youth and crisis.* New York: Norton.

Ferrer-i-Carbonell, A. and P. Frijters (2004). How important is methodology for the estimates of the determinants of happiness? *Economic Journal 114*(497), 641–659.

Fletcher, J. M. (2008). Adolescent depression: diagnosis, treatment, and educational attainment. *Health Economics 17*(11), 1215–1235.

French, D. J. and R. J. Tait (2004). Measurement invariance in the General Health Questionnaire-12 in young Australian adolescents. *European Child & Adolescent Psychiatry 13*(1), 1–7.

García-Gómez, P., A. M. Jones, and N. Rice (2010). Health effects on labour market exits and entries. *Labour Economics 17*(1), 62 – 76.

Ge, X., M. N. Natsuaki, and C. R. D. (2006). Trajectories of depressive symptoms and stressful life events among male and female adolescents in divorced and nondivorced families. *Development and Psychopathology 18*(1), 253–273.

Goldberg, D. P. (1972). *The Detection Of Psychiatric Illness By Questionnaire.* London: Oxford University Press.

Goodman, A., R. Joyce, and J. P. Smith (2011). The long shadow cast by childhood physical and mental problems on adult life. *Proceedings of the National Academy of Sciences 108*(15), 6032–6037.

Greater London Authority (2006). Muslims in London. Report, Greater London Authority.

Gregg, P., S. Harkness, and S. Smith (2009). Welfare reform and lone parents in the UK. *The Economic Journal 119*(535), F38–F65.

Gruber, J. H. and S. Mullainathan (2005). Do cigarette taxes make smokers happier? *The B.E. Journal of Economic Analysis & Policy 5*(1), 1–45.

Hanes, E. and S. Machin (2014). Hate crime in the wake of terror attacks: Evidence from 7/7 and 9/11. *Journal of Contemporary Criminal Justice 30*(3), 247–267.

Headey, B. and M. Wooden (2004). The effects of wealth and income on subjective well-being and ill-being. *Economic Record 80*, S24–S33.

Johnston, D. W. and G. Lordan (2012). Discrimination makes me sick! An examination of the discrimination-health relationship. *Journal of Health Economics 31*(1), 99 – 111.

Johnston, R., S. Burgess, R. Harris, and D. Wilson (2008). Sleep-walking towards segregation? The changing ethnic composition of English schools, 1997-2003: An entry cohort analysis. *Transactions of the Institute of British Geographers 33*(1), 73–90.

Jones, C. (2000). Levels of racism: A theoretic framework and a gardener's tale. *American Journal of Public Health 90*(8), 1212–1215.

Kaushal, N., R. Kaestner, and C. Reimers (2007). Labor market effects of September 11th on Arab and Muslim residents of the United States. *Journal of Human Resources 42*(2), 275–308.

Kelly, Y., L. Becares, and J. Nazroo (2013). Associations between maternal experiences of racism and early child health and development: Findings from the UK Millennium Cohort Study. *Journal of Epidemiology and Community Health 67*(1), 35–41.

Kitchen, S., J. Michaelson, and N. Wood (2006). 2005 Citizenship Survey: Race and faith topic report. Report, Department for Communities and Local Government.

Kling, J. R., J. B. Liebman, and L. F. Katz (2007). Experimental analysis of neighborhood effects. *Econometrica 75*(1), 83–119.

Lang, K. and J.-Y. K. Lehmann (2012). Racial discrimination in the labor market: Theory and empirics. *Journal of Economic Literature 50*(4), 959–1006.

Lauderdale, D. S. (2006). Birth outcomes for Arabic-named women in California before and after September 11. *Demography 43*(1), 185–201.

Layard, R., A. E. Clark, F. Cornaglia, N. Powdthavee, and J. Vernoit (2014). What predicts a successful life? A life-course model of well-being. *The Economic Journal 124*(580), F720–F738.

Lechner, M. (2011). The estimation of causal effects by difference-in-difference methods. *Foundations and Trends(R) in Econometrics 4*(3), 165–224.

Leicester, A. and P. Levell (2013). Anti-smoking policies and smoker well-being: Evidence from Britain. IFS Working Papers W13/13, Institute for Fiscal Studies.

Lyubomirsky, S., L. King, and E. Diener (2005). The benefits of frequent positive affect: Does happiness lead to success? *Psychological Bulletin 131*(6), 803–855.

Metcalfe, R., N. Powdthavee, and P. Dolan (2011). Destruction and distress: Using a quasi-experiment to show the effects of the September 11 attacks on mental well-being in the United Kingdom. *The Economic Journal 121*(550), F81–F103.

Olzak, S. (1990). The political context of competition: Lynching and urban racial violence, 1882-1914. *Social Forces 69*(2), pp. 395–421.

Oswald, A. J., E. Proto, and D. Sgroi (2015). Happiness and productivity. *Journal of Labor Economics 33*(4), 789–822.

Paradies, Y. (2006). A systematic review of empirical research on self-reported racism and health. *International Journal of Epidemiology 35*(4), 888–901.

Pascoe, E. A. and L. Smart Richman (2009). Perceived discrimination and health: A meta-analytic review. *Psychological Bulletin 135*(4), 531–554.

Phinney, J. S. (1990). Ethnic identity in adolescents and adults: Review of research. *Psychological Bulletin 108*(3), 499–514.

Priest, N., Y. Paradies, B. Trenerry, M. Truong, S. Karlsen, and Y. Kelly (2013). A systematic review of studies examining the relationship between reported racism and health and wellbeing for children and young people. *Social Science & Medicine 95*, 115 – 127.

Puhani, P. A. (2012). The treatment effect, the cross difference, and the interaction term in nonlinear 'difference-in-differences' models. *Economics Letters 115*, 85 – 87.

Rabby, F. and W. M. Rodgers (2010). The impact of 9/11 and the London bombings on the employment and earnings of UK Muslims. Discussion Paper 4763, IZA.

Åslund, O. and D. O. Rooth (2005). Shifts in attitudes and labor market discrimination: Swedish experiences after 9-11. *Journal of Population Economics 18*(4), 603–629.

Ratcliffe, A. and S. von Hinke Kessler Scholder (2015). The London bombings and racial prejudice: Evidence from housing and labour markets. *Economic Inquiry 53*, 276–293.

Reichert, A. R. (2015). Obesity, weight loss, and employment prospects: Evidence from a randomized trial. *Journal of Human Resources 53*, 759–810.

Romanov, D., A. Zussman, and N. Zussman (2012). Does terrorism demoralize? Evidence from Israel. *Economica 79*, 183198.

Sander, J. B. and C. A. McCarty (2005). Youth depression in the family context: Familial risk factors and models of treatment. *Clinical Child and Family Psychology Review 8*(3), 203–219.

Smith, J. P. and G. C. Smith (2010). Long-term economic costs of psychological problems during childhood. *Social Science & Medicine 71*(1), 110 – 115.

Solon, G., S. J. Haider, and J. M. Wooldridge (2015). What are we weighting for? *The Journal of Human Resources 50*(2), 301–316.

Stevenson, B. and J. Wolfers (2012). Subjective and objective indicators of racial progress. *The Journal of Legal Studies 41*(2), 459 – 493.

Tajfel, H. (1978). *The social psychology of minorities*. London: Minority Rights Group.

Thoits, P. A. (2010). Stress and health: Major findings and policy implications. *Journal of Health and Social Behavior 51*, S41–S53.

Tran, A. G. (2014). Family contexts: Parental experiences of discrimination and child mental health. *American Journal of Community Psychology 53*(1-2), 37–46.

Williams, D. R. and S. A. Mohammed (2009). Discrimination and racial disparities in health: Evidence and needed research. *Journal of Behavioral Medicine 32*(1), 20–47.

Yinger, J. (1998). Evidence on discrimination in consumer markets. *Journal of Economic Perspectives 12*(2), 23–40.

Tables and figures

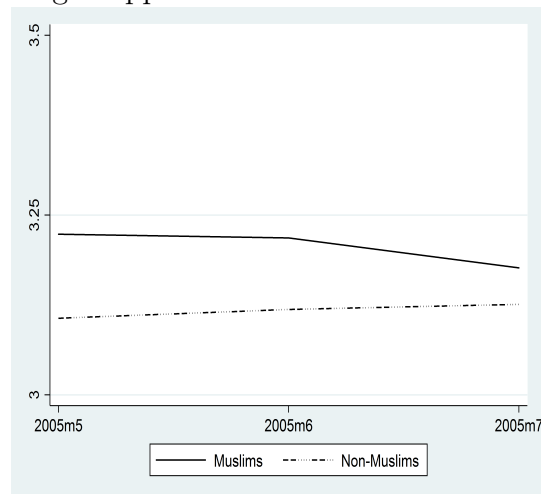Figure 1: The average happiness score across Muslims and Non-Muslims

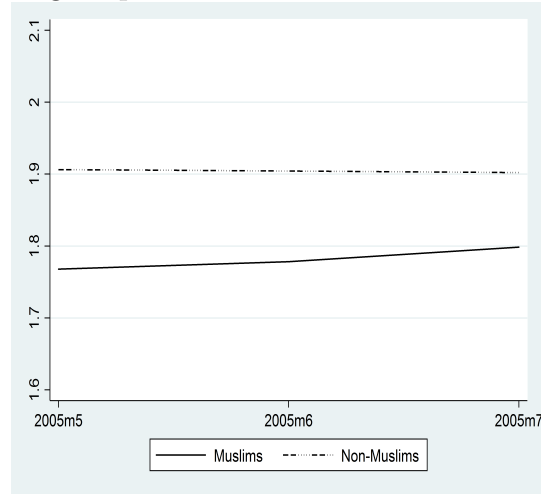Figure 2: The average depression score across Muslims and Non-Muslims

Table 1: Variable means, by treatment status and time

| | Muslims | Non-Muslims | Muslims | | Non-Muslims | |
| | All | All | May-June | July | May-June | July |
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variables:						
Happiness: Much less than usual	0.03	0.03	0.02	0.04	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	0.61
Happiness: More so than usual	0.34	0.26	0.34	0.34	0.26	0.27
Happiness score	3.21	3.12	3.22	3.18	3.11	3.13
Depression: Not at all	0.51	0.43	0.51	0.50	0.43	0.43
Depression: No more than usual	0.29	0.33	0.28	0.29	0.33	0.32
Depression: Rather more than usual	0.12	0.16	0.13	0.11	0.15	0.16
Depression: Much more than usual	0.08	0.09	0.08	0.09	0.09	0.09
Depression score	1.78	1.90	1.77	1.80	1.91	1.90
Very good at school work (2004)	0.28	0.19	0.28	0.26	0.18	0.19
Raising a family important (2004)	0.68	0.55	0.69	0.66	0.55	0.55
Financially rewarding career important (2004)	0.61	0.43	0.62	0.59	0.43	0.43
Very good health in past 12 months	0.43	0.45	0.46	0.39	0.44	0.47
Bullied in past 12 months	0.27	0.40	0.28	0.26	0.40	0.40
No time with friends inside/outside home	0.27	0.14	0.27	0.27	0.14	0.13
Expects discrimination in higher education	0.10	0.04	0.09	0.11	0.04	0.04
Expects discrimination in labour market	0.13	0.05	0.11	0.16	0.04	0.05
Control variables:						
Indian	0.10	0.06	0.11	0.09	0.06	0.04
Pakistani	0.41	0.00	0.39	0.45	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	0.05	0.07
Mixed/Other	0.10	0.07	0.12	0.06	0.07	0.08
Age in months/10	18.35	18.34	18.30	18.45	18.30	18.43
Paid work in term-time (2004)	0.04	0.23	0.04	0.05	0.24	0.22
Receives pocket money (2004)	0.82	0.81	0.84	0.79	0.81	0.81
Household chores (2004)	0.78	0.80	0.80	0.76	0.80	0.79
Postive schooling attitude (2004)	33.07	31.02	33.23	32.77	31.03	30.97
Bullied (2004)	0.34	0.45	0.34	0.34	0.45	0.46
Bullied (2004) missing	0.01	0.01	0.01	0.02	0.01	0.01
Household head aged < 40	0.17	0.21	0.18	0.16	0.21	0.21
Lone-parent family	0.20	0.23	0.20	0.19	0.23	0.23
Nominated second parent not interviewed	0.14	0.08	0.13	0.14	0.08	0.08
ln(siblings+1)	1.27	0.79	1.25	1.29	0.79	0.79
Parental knowledge of school day (2004)	0.39	0.45	0.40	0.38	0.45	0.47
Parental knowledge of where going (2004)	0.65	0.61	0.67	0.61	0.61	0.62

Table 1 – *Continued from previous page*

	Muslims	Non-Muslims	Muslims		Non-Muslims	
	All	All	May-June	July	May-June	July
	(1)	(2)	(3)	(4)	(5)	(6)
Family meals (2004)	0.68	0.38	0.68	0.70	0.38	0.38
Family outings (2004)	0.44	0.37	0.44	0.45	0.37	0.37
Household head has degree	0.07	0.14	0.07	0.08	0.14	0.13
Household head works	0.49	0.83	0.50	0.48	0.83	0.82
Family owns home	0.59	0.74	0.58	0.60	0.75	0.73
Household wage income $\geq$ £41 000	0.02	0.21	0.03	0.02	0.21	0.21
Household weekly benefit amount £30-39	0.09	0.25	0.10	0.08	0.25	0.26
Household weekly benefit amount £40-99	0.32	0.33	0.31	0.32	0.34	0.30
Household weekly benefit amount £100+	0.50	0.23	0.50	0.50	0.24	0.23
Household receives Working Tax Credit	0.23	0.24	0.24	0.22	0.24	0.24
Household receives Income Support	0.36	0.10	0.35	0.37	0.10	0.11
Household in financial difficulties (2004)	0.16	0.05	0.15	0.19	0.05	0.06
North West	0.02	0.05	0.02	0.03	0.05	0.05
North East	0.18	0.14	0.17	0.19	0.14	0.16
Yorkshire & Humber	0.16	0.09	0.17	0.15	0.08	0.12
East Midlands	0.04	0.09	0.05	0.03	0.10	0.09
West Midlands	0.13	0.12	0.13	0.14	0.12	0.13
East of England	0.05	0.11	0.05	0.06	0.12	0.08
South East	0.06	0.17	0.05	0.08	0.17	0.16
South West	0.01	0.10	0.02	0.00	0.10	0.08

Table 2: The impact of the London bombings on teenage Muslim wellbeing

	Difference-in-differences			Before-after		
	All (1)	Boys (2)	Girls (3)	All (4)	Boys (5)	Girls (6)
Panel A: Happiness						
$ATET_{P_1}$: 'Much less than usual'	0.02*	-0.00	0.04**	0.01	-0.01	0.03**
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
$ATET_{P_2}$: 'Less so than usual'	0.03*	0.03	0.03	0.01	0.01	0.02
	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
$ATET_{P_3}$: 'About the same as usual'	-0.04	-0.09*	-0.00	-0.01	-0.03	0.01
	(0.03)	(0.05)	(0.03)	(0.02)	(0.04)	(0.03)
$ATET_{P_4}$: 'More so than usual'	-0.00	0.06	-0.06	-0.01	0.04	-0.06
	(0.03)	(0.05)	(0.04)	(0.03)	(0.05)	(0.04)
$ATET_{Y_{(OP)}}$	-0.06	0.05	-0.16**	-0.04	0.05	-0.14**
	(0.05)	(0.07)	(0.06)	(0.05)	(0.07)	(0.06)
$ATET_{Y_{(OLS)}}$	-0.06	0.05	-0.15**	-0.04	0.06	-0.14**
	(0.05)	(0.07)	(0.06)	(0.05)	(0.07)	(0.06)
Panel B: Depression						
$ATET_{P_1}$: 'Not all all'	0.01	0.01	0.01	0.01	0.04	-0.01
	(0.03)	(0.05)	(0.04)	(0.03)	(0.05)	(0.04)
$ATET_{P_2}$: 'No more than usual'	-0.02	-0.05	0.02	0.00	-0.02	0.02
	(0.03)	(0.04)	(0.03)	(0.02)	(0.03)	(0.03)
$ATET_{P_3}$: 'Rather more than usual'	-0.02	0.03	-0.07***	-0.02	0.00	-0.03
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
$ATET_{P_4}$: 'Much more than usual'	0.02	0.01	0.03	0.00	-0.02	0.03
	(0.02)	(0.01)	(0.03)	(0.01)	(0.02)	(0.02)
$ATET_{Y_{(OP)}}$	0.02	0.03	-0.01	-0.03	-0.08	0.03
	(0.07)	(0.08)	(0.09)	(0.06)	(0.08)	(0.09)
$ATET_{Y_{(OLS)}}$	0.01	0.02	-0.01	-0.02	-0.07	0.03
	(0.07)	(0.08)	(0.09)	(0.06)	(0.07)	(0.09)

Notes: See Section 3.2 for details of the estimation strategy. Standard errors are block bootstrapped to account for school-level clustering based on 1000 replications and significance levels are shown as *p<0.1, **p<0.05, ***p<0.01. 8708 individuals provide data on happiness, with 2466 individuals interviewed in July. Of these individuals, 1073 are Muslim (525 teenage boys and 548 teenage girls), with 385 interviewed in July (185 teenage boys and 200 teenage girls). 8875 individuals provide data on depression, with 2509 individuals interviewed in July. Of these individuals, 1093 are Muslim (532 teenage boys and 561 teenage girls) with 392 interviewed in July (188 teenage boys and 204 teenage girls).

Table 3: Pseudo treatment in June 2005

	Happiness			Depression		
	All (1)	Boys (2)	Girls (3)	All (4)	Boys (5)	Girls (6)
$ATET_Y$	0.00	0.02	0.00	0.01	-0.01	0.03
	(0.06)	(0.08)	(0.09)	(0.06)	(0.09)	(0.11)

Notes: Analyses based on linear regression methods.

Table 4: Pseudo treatment with untreated outcomes

	Very good at school work (2004)			Strong desire to have children (2004)			Financially rewarding job important (2004)			Very good health in past 12 months		
	All (1)	Boys (2)	Girls (3)	All (4)	Boys (5)	Girls (6)	All (7)	Boys (8)	Girls (9)	All (10)	Boys (11)	Girls (12)
$ATET_Y$	-0.03	-0.05	-0.01	-0.04	-0.02	-0.06	-0.01	-0.04	0.01	-0.04	-0.07	0.00
	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)	(0.04)

Notes: Analyses based on a linear probability model. Sample is restricted to individuals with data on happiness.

Table 5: Some possible explanations

	Bullied at school in past 12 months			No time with friends inside/outside home			Expects discrimination in further education			Expects discrimination in labour market		
	All (1)	Boys (2)	Girls (3)	All (4)	Boys (5)	Girls (6)	All (7)	Boys (8)	Girls (9)	All (10)	Boys (11)	Girls (12)
$ATET_Y$	-0.04	-0.06	-0.02	0.01	0.04	-0.03	0.01	0.02	0.02	0.05**	0.03	0.09**
	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.04)

Notes: Analyses based on a linear probability model. Sample is restricted to individuals with data on happiness.

Table 6: Estimated treatment effect at 25th, 50th and 75th percentiles of poverty and ethnic segregation

	Area poverty (1)	Area density (2)	School poverty (3)	School density (4)
Panel A: Happiness among teenage boys				
25th percentile	0.10	-0.03	-0.02	-0.04
	(0.09)	(0.08)	(0.09)	(0.09)
50th percentile	0.05	0.01	0.05	-0.01
	(0.07)	(0.07)	(0.07)	(0.08)
75th percentile	0.02	0.09	0.09	0.08
	(0.07)	(0.08)	(0.09)	(0.08)
Panel B: Happiness among teenage girls				
25th percentile	-0.03	-0.10	-0.04	-0.01
	(0.08)	(0.08)	(0.08)	(0.09)
50th percentile	-0.15**	-0.12*	-0.14**	-0.11
	(0.06)	(0.06)	(0.06)	(0.07)
75th percentile	-0.23***	-0.15**	-0.23***	-0.22***
	(0.07)	(0.06)	(0.07)	(0.06)
Panel C: Depression among teenage boys				
25th percentile	-0.05	-0.10	-0.08	-0.09
	(0.12)	(0.10)	(0.11)	(0.10)
50th percentile	-0.07	-0.09	-0.08	-0.09
	(0.08)	(0.08)	(0.07)	(0.09)
75th percentile	-0.08	-0.06	-0.08	-0.08
	(0.08)	(0.07)	(0.08)	(0.08)
Panel D: Depression among teenage girls				
25th percentile	-0.03	-0.00	-0.02	-0.05
	(0.12)	(0.11)	(0.11)	(0.12)
50th percentile	0.04	0.01	0.04	0.02
	(0.09)	(0.09)	(0.09)	(0.09)
75th percentile	0.09	0.04	0.08	0.10
	(0.10)	(0.10)	(0.10)	(0.10)

A   Appendix

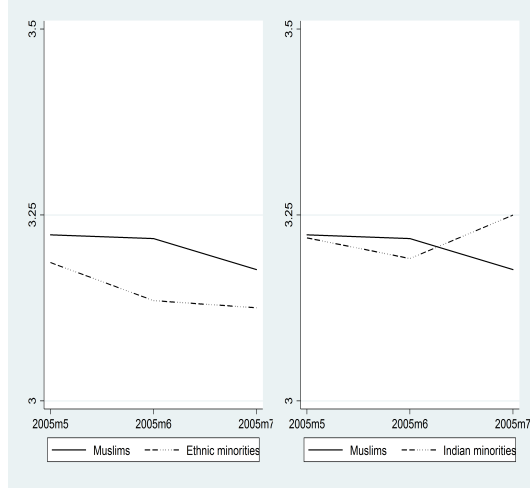Figure A1: The average happiness score using only ethnic minority controls

Figure A2: The average depression score using only ethnic minority controls