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THE LONDON BOMBINGS AND RACIAL PREJUDICE: EVIDENCE FROM THE HOUSING AND LABOUR MARKET

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This paper investigates the impact of the London bombings on attitudes towards ethnic minorities, examining outcomes in housing and labour markets across London boroughs. We use a difference-in-differences approach, specifying 'treated' boroughs as those with the highest concentration of Asian residents. Our results indicate that house prices in treated boroughs fell by approximately 2% in the two years after the bombings relative to other boroughs, with sales declining by almost 6%. Furthermore, we present evidence of a rise in the unemployment rate in treated compared to control boroughs, as well as a rise in racial segregation. (JEL: J15; J71; R21)

Keywords: terrorism, racial prejudice, difference-in-differences

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I. INTRODUCTION

On the 7th of July 2005 (henceforth 7/7), four extremist Islamic terrorist bombers targeted London's transport network, killing 52 passengers and injuring hundreds more. Using pre-recorded statements, the bombers threatened further acts of terrorism. Indeed, just two weeks later, a further four suicide bombers attempted but failed to execute similar attacks.

The bombings and bombers attracted considerable media attention. Extensive coverage was given to the fact that three of the four 7/7 bombers were British born with Pakistani heritage, raising concerns over the integration and radicalisation of British Muslims (EUMC, 2005). Despite condemnation of the attacks by key Muslim organisations, four weeks after the initial bomb attacks, faith-hate crimes had risen by 600 percent compared to the previous year (Greater London Authority, 2006). These attacks targeted all Asians, including non-Muslim Asians, with Mosques as well as, for example, Sikh temples being subject to reprisal attacks,[1] suggesting pervasive effects of the bombings on racial prejudice.

Evidence from the Citizenship Survey also points to a rise in prejudice after the bombings. For example, a 12 percentage point increase in the proportion believing "more religious prejudice exists today compared with 5 years ago" is observed in interviews taking place just after 7/7 compared with just before. Notably, the proportion identifying Muslims as the victims of prejudice almost doubled in interviews post 7/7, increasing to 50% (DCLG, 2006).[2]

We investigate the effects of the London bombings on attitudes towards ethnic minorities across Greater London boroughs, as reflected in activity in the housing market (house prices and sales), the labour market (unemployment rates and earnings), as well as ethnic segregation, using a difference-in-differences (DD) approach. With all existing studies of the London bombings using (household) survey data and focusing on the labour market, the contribution of this paper is to consider a wider set of outcomes, to document the speed with which individuals learn about changes in the general attitude towards ethnic minorities, as well as how these 'learning effects' vary across the different outcomes. In addition, we use borough-level administrative data to deal with the fact that even the larger UK household surveys contain relatively small ethnic minority samples.

[1] www.guardian.co.uk/world/2005/sep/05/religion.july7

[2] Such changes in racial or religious prejudice against UK Muslims have not been reported after the 9/11 attacks in the US, suggesting that there may be differences in people's reactions depending on whether it occurred in one's own country. The Citizenship Survey is a survey in England and Wales that explores issues such as perceptions of community cohesion, race and faith. For more information, see http://www.communities.gov.uk/communities/research/citizenshipsurvey.

We focus on Greater London boroughs, rather than the whole of the UK, for three reasons. First, the bombings arguably had a larger impact on the day-to-day lives of Londoners, compared to others, given the disruption to transportation networks and a visible increase in police presence immediately after the bombings (Draca et al., 2011). Second, with almost 8 million people at the time of the bombings, London is the UK's most populous city. We exploit its diversity of ethnic populations across boroughs to identify treatment and control boroughs. Finally, although there are other large and ethnically diverse UK cities, there are no reliable estimates of the ethnic composition for lower-level geographies. Birmingham, for example, is very ethnically diverse, but the whole of Birmingham constitutes one Local Authority, with no population ethnicity data available at lower levels.

Our results show that, relative to 'control' boroughs, house prices in 'treated' boroughs fell by approximately 2% in the two years after the attacks, with sales falling by almost 6%. While unemployment increased by almost 6%, there is little evidence of changes in average earnings. We also find evidence of a rise in segregation, with the proportion of Asians in treated areas increasing after the bombings, relative to control areas. These results are robust to various sensitivity analyses with respect to different timings and definitions of treatment, though we do find slightly weaker housing market effects once we specify a control group that is more similar to the treatment group. Furthermore, we find some evidence of 'learning effects', with our results indicating a cumulative impact on house prices and unemployment rates, but an immediate effect on sales.

The paper is structured as follows: Section II reviews the literature and Section III discusses the conceptual framework and empirical strategy. The different data sources are described in Section IV. Section V discusses our results, and Section VI presents the robustness checks and extensions. Section VII concludes.

II. RELATED LITERATURE

There is a large literature on racial prejudice and discrimination in housing and labour markets (see e.g. Yinger, 1998; Lang and Lehmann, 2012). A recent set of papers exploit the occurrence of terrorist activity as a plausibly exogenous shock to racial prejudice. Initially, this strand of the literature focused on the labour market outcomes of ethnic minorities in the US following the 9/11 attacks, with some evidence of - at least - a temporary decline in earnings (Dávila and Mora, 2005; Kaushal et al., 2007). Gould and Klor (2012) also find evidence of an increase in racial segregation insofar as Muslims were more likely to marry other Muslims and less likely to marry non-Muslims after these attacks. Despite deteriorating public

opinion towards ethnic minorities after 9/11 outside the US (Aslund and Rooth, 2005; Schuller, 2012; Goel, 2010), there is little evidence of adverse labour market outcomes in Sweden (Aslund and Rooth, 2005) and Canada (Shannon, 2012), with mixed evidence for Germany (Braakmann, 2009; Cornelissen and Jirjahn, 2012).[3]

All existing studies of the effect of the London bombings on racial discrimination use (household) survey data and are confined to labour market outcomes. Braakmann (2010) investigates the impact of the bombings on the employment probability, hours, and earnings of Arabs, Pakistanis, Bangladeshis, and Muslims in Britain. While his findings suggest no clear patterns of the outcomes for the treated relative to various control groups, there is weak evidence that the earnings and employment probabilities of Pakistanis and Bangladeshis actually increased relative to other non-white minorities. In contrast, Rabby and Rodgers (2010) find evidence of a decline in the employment and earnings of younger, but not older, Arab immigrants relative to other immigrants.

Other research has looked to the housing market for evidence of discrimination towards ethnic minorities following terrorist attacks, since house prices and sales are likely to reflect preferences for living among ethnic minorities. Gautier et al. (2009) examine the impact of the murder of TV-host and film maker Theo van Gogh in 2004 in Amsterdam on house prices in 'treated' neighbourhoods (defined as those with more than 25% of its population from Turkey or Morocco) compared to control neighbourhoods within the city. Their results suggest that house prices in treated neighbourhoods fell by 0.07% per week, resulting in a 3% difference 10 months after the murder. In addition, they find some evidence of an increase in segregation, with Muslims being more likely to buy, but less likely to sell a house in treated areas after the murder compared to before.

III. CONCEPTUAL FRAMEWORK AND EMPIRICAL STRATEGY

We investigate the effects of the London bombings on the housing market (house prices and the number of sales), the labour market (unemployment rates and earnings), as well as racial segregation. For the housing market, we adopt a hedonic house price framework (see Rosen, 1974), characterising the price of

[3]The literature on the consequences of terrorism also examines the effect on economic activity, including asset prices. For example, Besley and Mueller (2012) consider the effect of on-going terrorism in Northern Ireland on house prices, exploiting the time-varying pattern of terrorist-related violence across regions, while Abadie and Dermisi (2008) examine the impact of the 9/11 attacks on the demand for rental units in tall landmark buildings in Chicago. In contrast to our study, however, these papers examine the effects of increased *exposure* to acts of terrorism, as opposed to the backlash that acts of terrorism may have on minority groups.

a house as a function of many attributes, including the characteristics of the surrounding neighbourhood. Within this framework, the London bombings may have influenced the desirability of living in ethnically diverse London boroughs, with any changes in the perception of living in these neighbourhoods reflected in the price of housing. A priori, however, it is not clear exactly how the London bombings would alter buyers and sellers appraisals of living in ethnically diverse boroughs.

On the one hand, the London bombings may have reduced the desirability (among whites) of living in ethnically diverse neighbourhoods, which would be consistent with a negative shock to attitudes to ethnic minorities. Even if an individual is happy to live in an ethnically diverse area, they might be concerned about the re-sale value of their property, particularly if there is reason to believe that the general public views properties in these areas as less desirable after the bombings (see Gautier et al., 2009).

On the other hand, people may reason that ethnically diverse London boroughs are relatively safer, if it is considered that areas with large ethnic communities are less likely to be targeted by any potential future terrorist activity. This would suggest that the desirability of living in such areas would increase after the attacks. However, safety concerns may also operate to diminish the attractiveness of living in ethnically diverse boroughs. The reason is that a number of these boroughs are centrally located, so if it is believed that centrally located boroughs in general face a greater risk of being targeted in any potential future terrorist activity, safety concerns may reduce the desirability of living in these ethnically diverse areas. Such fears may additionally have been compounded by the sharp increase in police presence in specific boroughs after the bombings, including in ethnically diverse boroughs (see Draca et al., 2011). We discuss this in more detail below.

It is possible that all of these factors operate to shape house prices in ethnically diverse neighbourhoods. Note also that any change in the desirability of living in ethnically diverse areas affects both the demand for, and the supply of houses in those boroughs. Although the market price and the number of sales are determined by both buyers and sellers, our data do not allow us to isolate the change in house prices and number of sales due to variations in supply or in demand. Our results therefore reflect the net (overall) impact of the shock on the behaviour of buyers and sellers, and with that, on average house prices and total sales. We use the following difference-in-differences (DD) approach:

$$y_{jt} = \beta(treated_j \times post_t) + \alpha post_t + \eta_j + \delta_t + \varepsilon_{jt} \tag{1}$$

where y_{jt} denotes the (natural) log of the average house price or number of sales in borough j and time t. $treated_j \times post_t$ equals 1 for treated areas from July 2005 onwards, $post_t$ equals 1 from July 2005 onwards, η_j are borough fixed effects, and δ_t denote month dummies. The random error term is denoted by ε_{jt}, and is clustered at the borough-level to correct for clustering and autocorrelation (Bertrand et al., 2004). The parameter β provides the estimated effect of the London bombings on house prices and sales in treated relative to control boroughs. To ensure that house prices follow a similar trend in treated and control boroughs prior to the bombings, we restrict our main analysis to a time window incorporating two years pre and two years post the bombings (i.e. July 2003-June 2007), though we examine the sensitivity of the results to the use of a longer time period.

We use a similar DD approach to estimate the effects of the bombings on labour market outcomes as well as segregation, replacing y_{jt} with the (natural) log of the unemployment rate, the average earnings of residents, the total population and the populations of whites, blacks and Asians in the borough.

These analyses, comparing the change in outcomes across treated and control boroughs, require several assumptions. First, treatment is assumed to be exogenous, which is likely to hold, as the attacks were unanticipated. Second, the DD approach assumes a common trend across treatment and control areas prior to the bombings. We examine this in Section VI.A, incorrectly setting the treatment year to a year prior to the bombings. If the common time trend assumption holds, we would expect no effect of the incorrectly specified treatment years on our outcomes of interest. As our full data run from 1995 (housing and labour market) and 2000 (ethnicity), we additionally test the validity of this assumption and the robustness of the results by controlling for differential trends.

A third assumption is that the composition of treatment and control groups remains stable over time. While houses are clearly fixed and cannot move between neighbourhoods, we analyse the average price of all houses sold in a borough. Hence, if the type of property put on the market changes in response to the attacks, this assumption may be violated. For example, if there is an increase in sales of cheaper properties (such as flats) after the bombings, the average house price will fall simply due to a composition effect, even if there is no change in the price of flats or other property types. Note, however, that we analyse the log of house prices, which requires the weaker assumption that the percent change is comparable across various property types.[4] Similarly, although houses are fixed, people may move between different areas. If, for example, whites are more likely to move from treated to control neighbourhoods after the bombings,

[4]We do not observe the London rental market but we assume that activity in rental markets is similarly affected by the attacks.

this may raise house prices in control areas and overestimate the treatment effect. In contrast however, if ethnic minorities prefer to live in treated boroughs after the attacks, increased housing demand in treated areas may underestimate the treatment effect. We cannot distinguish between these processes. As such, our analysis identifies the combined effect of the bombings on house prices, which may (partly) be driven by changes in the ethnic composition of the population in treated versus control areas. We come back to this below.

The same holds for the analyses on labour market outcomes. If, for example, Asians have higher average unemployment rates, and if they are more likely to move into treated areas after the bombings, it may seem as if the bombings increased unemployment rates, whereas this can simply be driven by a change in the population composition of treated compared to control boroughs. We examine whether population movements may affect our results in two ways: first, by analysing this directly, investigating the effect of the bombings on the ethnic composition in treated versus control areas, and with that, on racial segregation; and second, by including the ethnic populations as covariates.

A final assumption requires other contemporaneous changes to have similar effects on outcomes in treated and control boroughs. One possible violation of this assumption is the announcement of the London Olympics, which happened just before the attacks. The Olympic bid included proposals to regenerate parts of East London, and in particular, Newham, which is part of our treatment group. Assuming that people are forward looking however, expected regeneration should increase house prices in these treated areas. Similarly, to the extent that any regeneration of East London started within our observations window, it should decrease unemployment rates in treated areas. In both instances, the treatment effect would be underestimated.[5]

IV. DATA

We combine information from several sources to obtain one dataset that includes data on the housing market, the labour market and the ethnic composition of residents in the 32 London boroughs. Data on the housing market are obtained from the Land Registry, which provides monthly statistics on seasonally

[5]Note that changes to immigration policy due to the bombings are unlikely to be driving our results, as securitization of UK migration policy was well under way prior to the bombings (Hampshire and Saggar, 2006). Similarly, there is little evidence that the bombings led to differential changes in public spending patterns for treated and control boroughs, which might otherwise influence house prices and residential choices. Spending by local government is highly centralised in the UK with no evidence of any changes in centrally-determined spending-allocation rules in the period after the bombings (see Crawford et al., 2009).

adjusted average house prices and number of sales in London boroughs. The data provide a complete record of residential property transactions in England and Wales from 1995. We adjust all monetary figures for inflation using the RPIY series (RPI minus mortgage interest payments).

Data on the labour market are obtained from Nomis, a website specialising in supplying labour market statistics for low-level geographies. We use monthly data on borough-level (male) unemployment rates, which are based on the receipt of unemployment-related benefits. We also use annual data on (male) average weekly full-time earnings of residents, available from 2002 to 2008. The latter originates from the Annual Survey of Hours and Earnings, which is based on a 1% sample of employees appearing in the pay-as-you-earn (PAYE) taxation system, covering all types of employees in all types of businesses. Similar to the house price data, earnings data are adjusted for inflation using the RPIY.

Monthly, as opposed to yearly, data on house prices, sales, and unemployment rates allow us to clearly delineate pre and post treatment periods, and to analyse month-level data over a specific number of treatment years, where each 'treatment year' starts in July and finishes in June. Thus in our main analysis, where we focus on two years pre and post bombings, we analyse month-level data between the period July 2003-June 2007, with July 2005 onwards comprising the post-treatment period. In contrast, for the earnings data, which are available annually and by calendar year only, we must drop the year 2005 from our analysis to create a clean pre and post period, and thus analyse annual-level data for the pre-treatment period spanning 2003-2004 and post-treatment period spanning 2006-2007.

We obtain data on the total population and ethnic composition of each borough from the Office for National Statistics Population Estimates by Ethnic Group (PEEG; ONS, 2009), available from 2001. Data on each borough's total population correspond to the Mid-Year Population Estimates, which are the main source of annual population estimates in England, with the key innovation of PEEG data being the provision of local-level ethnic population estimates. These data estimate the relevant ethnic population at June 30th each year, by taking the population estimate from the previous year, and adjusting for estimated births, deaths and national/international migration by ethnic populations occurring over the year. The base population, from which the ethnic population estimates are derived, is the 2001 Census.[6] Hence, PEEG

[6]The primary objective of PEEG data is to provide small-area ethnic population estimates produced in accordance with the principals set out in the National Statistics Code of Practice. There is some evidence, however, that PEEG overestimates births to the white population and underestimates internal migration of ethnic minorities relative to other data sources such as the Annual Population Survey (ONS, 2012). As our estimation strategy (fixed effects) takes into account systematic differences in population, any time-invariant mis-measurement in the calculation of births and internal migration is unlikely to influence our results. In line with this reasoning, when we use only PEEG estimates of populations aged 16 and over to define treatment and to analyse population changes - thus removing any issues relating to estimated births - our findings are unchanged.

data provide an estimate of the ethnic composition in London boroughs one week prior to the London attacks, using the same definitions of ethnicity that are used in the Census.[7] Note that while we use the 2005 PEEG data to define treated and control boroughs (i.e. using population estimates on 30 June 2005), when we analyse population movements below, we match each PEEG release to the preceding treatment year. For example, the 2001 PEEG, which estimates population movements between July 2000 and June 2001, is matched to the treatment year 2000, also spanning the period July 2000-June 2001. Sources and availability of all data used in our analysis can be found in Table A1, Appendix A.

We define treated neighbourhoods according to the distribution of Asians (including Indians, Pakistanis, Bangladeshis and Other Asians) on the basis that reprisal attacks following the bombings affected non-Muslim Asians as well as Muslim Asians.[8] More specifically, a London borough is treated if the percentage of Asian residents in the borough on the 30th of June 2005 falls in the highest quintile of this distribution.[9] As shown in Figure 1, these boroughs are located in West (Harrow, Brent, Ealing and Hounslow) and East London (Tower Hamlets, Newham and Redbridge).[10]

Previous research that uses the Asian population to define treatment typically excludes Indians on the basis that it is not clear whether Indians should be assigned to the treatment or control group (see e.g. Kaushal et al., 2007; Braakmann, 2010). Indeed, the majority of Indians in London are Hindus or Sikhs, of whom just 9% practice Islam (DCLG, 2009b). In contrast, the majority faith among Pakistanis, Bangladeshis and Other Asians is Islam, respectively representing 98%, 92%, and 37% (DCLG, 2009a,c; ONS, 2005). We therefore consider two additional definitions of treatment based on ethnicity. First, we define treated boroughs as those in the top quintile of the distribution of Pakistanis, Bangladeshis and other Asians. This alternative definition leads to the London borough of Waltham Forest replacing Ealing as a treated borough. Second, we define treated neighbourhoods as those in the highest quintile of the distribution of Pakistani residents, since three of the four 7/7 bombers were of Pakistani descent, with some elements of

[7] These are white (subdivided into white British, white Irish and other white, black (Caribbean, African, and other black), mixed (including white and black Caribbean, white and black African, white and Asian, other mixed), Asian or Asian British (with subcategories Indian, Pakistani, Bangladeshi, other Asian), and Chinese or other ethnic group.

[8] Hence, we do not include Chinese and those from the Far East in our definition of treated boroughs. In fact, individuals from the Philippines, Japan, Vietnam, and Thailand are mainly included in the Census group 'other ethnic groups', and have not been associated with any reprisal attacks. We instead use these in a 'falsification check' below. In addition, dropping the City of Westminster from the analyses, home to London's Chinatown, does not alter the findings (results available from the authors upon request).

[9] Defining treated neighbourhoods as those in the highest quintile of the ratio of Asians to whites, rather than Asians per se, or defining treated neighbourhoods as those in the highest quintile of the distribution of Asian residents *including* "mixed white and Asians" leads to same grouping of treated and control boroughs.

[10] Note that the City of London is not a London Borough and is excluded from our analyses. It is dominated by just one residential area: the Barbican Estate. Although over 400,000 people commute to the City of London for work, only around 7,000 people actually live there (DBE, 2014).

the press providing a negative portrayal of this group in particular (EUMC, 2005). Using this treatment definition, Tower Hamlets is replaced by Waltham Forest.

Finally, we run two 'falsification checks', defining treatment based on ethnic minority groups that we argue are *unlikely* to experience an increase in racial prejudice after the bombings. We refer to these as 'pseudo treated' boroughs. For example, while the failed July 21st bomb attacks were carried out by North African Muslims, Black minorities did not bear the brunt of reprisal attacks. In addition, any media reports about violence and crime involving blacks (as victims or suspects) generally concern crimes such as robbery, drug and gun offences (House of Commons Home Affairs Committee, 2007), rather than religion. This is consistent with the results in Davila and Mora (2005), who find that with the majority of media coverage after 9/11 focusing on the Middle East, there are significant declines in the earnings of Middle Eastern Arab men in the US, with no changes for African Arab men. Hence, we do not expect to find differences in the outcomes of interest in Black treated areas. Likewise, we do not expect to find any effects against those from East Asia, and define boroughs with a high proportion of 'Chinese or other ethnic group' as a second 'pseudo treated' group.

Table 1 provides descriptive statistics for the different definitions of treated boroughs (columns 1-3), 'pseudo' treated boroughs (columns 4-5) and control boroughs (column 6). This shows that house prices are lower in treated boroughs compared to control boroughs. Similarly, treated boroughs generally have higher unemployment rates, lower earnings and are more populous.

The top row of Figure 2 presents the log of house prices and sales for treated and control London boroughs, depicting two years pre and two years post bombings (July 2003 to June 2007). The bottom row depicts the difference between these two groups over the observation period. This shows a relatively constant difference in house prices and sales during the pre-treatment period, with some evidence of a decline in house prices and sales for treated boroughs after the bombings. Figure 3 presents the graphs for the labour market outcomes, also showing no strong evidence of differential trends prior to the bombings, but with an increasing gap afterwards. We examine the common time trend assumption in more detail below.

V. RESULTS

Table 2 presents results from the DD specification shown in equation 1. The analysis on the housing market, Panel A, clearly indicates a drop in house prices and sales in treated boroughs after the London

bombings. The estimates in column 1 suggest that house prices in treated areas fell by approximately 2.3% in the two-year period after the attacks, with a decline of approximately 5.7% in sales.

The results in column 1 are based on the premise that individuals of Asian *appearance* were affected, as was intimated by media reports immediately following the London bombings. Focusing specifically on those more likely to be Muslim, however, leads to very similar estimates. Indeed, excluding Indians (column 2) or using only Pakistanis (column 3) shows similar estimates to those in column 1. In contrast, and as expected, columns 4 and 5 of Table 2 provide little evidence of a change in house prices or sales in the 'pseudo' treated boroughs, confirming our hypothesis that the bombings only affected attitudes towards Asians, rather than leading to a general increase in racial prejudice.

Panel B of Table 2 shows the DD estimates of the effect of the bombings on labour market outcomes, examining unemployment rates and average earnings. This indicates a 5.8% rise in unemployment rates across individuals living in treated relative to control boroughs. The estimates are robust to the use of different treatment definitions. However, we find no evidence of any change in earnings. The existing literature that examines the earnings of UK Muslims indeed shows mixed evidence, with Braakmann (2010) finding no (or even a positive) effect, and Rabby and Rodgers (2010) finding a decline in earnings. It may be that these differential findings are driven by heterogeneous effects on different subgroups. Indeed, Rabby and Rodgers (2010) only find significant effects on younger (but not older) workers. Unfortunately, however, our data do not allow us to examine this in more detail.

Panel C of Table 2 shows the DD estimates for the analyses examining changes in the total population and ethnic composition. These show that, although the total population has not changed differentially in treated compared to control areas after the bombings, the white and Asian population did. In fact, the analyses suggest that the population of whites increased in treated areas, whilst the population of Asians decreased. As we show below, however, this is driven by differential time trends in the composition of Asians and whites prior to the bombings. When we take this into account in Section VI.B, the results reverse, suggesting that the bombings led to an increase in racial segregation. We come back to this below.

VI. ROBUSTNESS CHECKS AND EXTENSIONS

A. Falsification checks

Table 3 presents results for a series of falsification checks, where we define the bombings to occur in the years prior to 2005 using our preferred treatment definition of Asian minorities. For example, column 1 defines the attacks as occurring in 1997 instead of 2005 and analyses the period July 1995 to June 1999. Columns 2 to 7 specify the treatment years as 1998 to 2003, each including data from two years pre and two years post the treatment. None of these falsification checks show evidence of a significant treatment effect on house prices, sales, or unemployment, suggesting that the estimates in Table 2 capture the changes in the outcomes of interest caused by the London bombings.

Since our earnings data start in 2002, and are measured on a calendar-year basis (see Table A1), we cannot run the full falsification analyses for earnings. Instead, we can specify either 2003 as the treatment year (i.e. setting 2002 as pre-treatment and 2003-2004 as post-treatment) or 2004 as the treatment year (i.e. setting 2002-2003 as pre-treatment and 2004 as post-treatment). We find an increase in earnings in the treated boroughs relative to those in control boroughs prior to the bombings, with a statistically significant effect when 2003 is specified as the treatment year (results available upon request). This suggests that our finding of a zero treatment effect could be genuine or it could reflect a leveling off of an upward trend in earnings in treated relative to control boroughs after the bombings. Due to a lack of earnings data for earlier years, we cannot account for differential trends prior to the bombings. Hence, we no longer examine this outcome.

Table 4 presents the falsification analysis for the population variables. As we discuss in Section IV and the Appendix, our population estimates span the period July (t-1) to June (t), which corresponds to treatment year (t-1), starting from July 2000. We can therefore incorrectly set the treatment year as 2002 and 2003, whilst retaining data for the two years pre- and post-treatment. While we find no evidence of pre-treatment changes in the *total* population using our falsification analysis, the results show that the ethnic composition of London boroughs changes in the treatment relative to control boroughs prior to the bombings. Specifically, we observe an increase in the white population and a decrease in the Asian population in treated compared to control areas, with no significant differences for the black population. These differential trends prior to 2005 imply that we cannot simply attribute the ethnic composition effects observed in Table 2 to the bombings. Instead, we need to account for differential trends.

B. Differential time trends

To account for any differential trends in the ethnic composition prior to the bombings, we re-run the DD analysis, additionally including linear time trends for treated and control boroughs. Although the evidence above only suggests there are differential trends in the evolution of the ethnic composition, we examine the robustness of all outcomes to the use of differential trends. Starting with columns 1-3 of Table 5, we examine the effects of the bombings on house prices, sales, and unemployment using all available pre-treatment years of data (i.e. from July 1995) to include (monthly) time trends. The findings support our earlier analyses. For example, columns 1 and 2 show that the bombings led to a drop in house prices and sales of 2.3% and 9% respectively. Although the estimate for sales is slightly larger than that in Table 2, so are the standard errors. Column 3 presents evidence of an unemployment effect that is similar to that presented in Table 2 but is not statistically different from zero due to an inflated standard error.

We investigate the total population and ethnic composition of boroughs taking into account yearly time trends using data from 2001 (i.e. July 2000-June 2001) onwards; the earliest year for which we observe the ethnic composition (see Table A1). This shows no effects of the bombings on the total population or the white population, but an increase in Asians in treated neighbourhoods. The results suggest that, although there is a significant reduction in the Asian population prior to the bombings (see Table 4, as well as the coefficient on $Treated \times time$ in Table 5), this is partly offset by an increase of approximately 3.2% after the bombings. In other words, controlling for differential trends in the ethnic composition for treated and control boroughs, we find that the bombings led to an increase in racial segregation, with the Asian population in treated areas increasing by approximately 3.2%.

C. Variable treatment intensity

Our definition of treatment identifies boroughs in the top quintile of the Asian distribution. We also consider an 'intensity of treatment' specification by interacting the post treatment dummy with the proportion of Asians in a borough, which allows the treatment effect to linearly increase as ethnic diversity increases. The results, presented in Table 6, indicate that a one percentage point increase in the proportion of Asians leads to a 0.1% drop in house prices, a 0.3% drop in sales, and a 0.3% increase in the unemployment rate. Similar to Table 2, there is no change in the total population in treated relative to control boroughs.[11]

[11] As this analysis does not account for differential trends, we do not report the results for the ethnic composition.

D. Increasing the comparability of boroughs

The main analysis in Table 2 uses all London boroughs, assuming that the control boroughs provide an accurate reflection of what would have happened to the treated boroughs in the absence of the bombings. Some of the control boroughs, however, may have systematically different housing and labour markets. We therefore consider methods to increase the similarity between these markets across treatment and control boroughs. First, we exclude boroughs with the highest house prices and earnings (Kensington and Chelsea), and the lowest sales (Islington), unemployment (Richmond Upon Thames) and population (Kingston upon Thames) from the analysis, as these boroughs might not represent good controls for the treated boroughs. The results, reported in Panel A of Table 7, show similar effects of the bombings in comparison with Table 2.

Second, we consider a matching DD estimator based on inverse probability weighting. This strategy uses the propensity score (i.e. the probability of treatment) to increase the similarity of the control boroughs. Specifically, control boroughs are weighted by the odds of the propensity score (i.e. by $(\frac{p}{1-p})$ where p is the propensity score) such that control boroughs most similar to treated boroughs receive a larger weight and vice versa. We estimate the propensity score as a function of borough-level characteristics, using data collected for the 2004 release of the Deprivation Index: measures of deprivation at low-level geographies. For the housing market outcomes, we predict treatment as a function of air quality and the percentage of housing deemed poor quality / without central heating, to analyse boroughs with comparable housing markets to our treated boroughs. For the labour market outcomes, we predict treatment as a function of average wages offered by firms operating in each borough and the percentage of residents with basic education. Finally, for the population analysis, we predict treatment as a function of changes in the stock of housing in each borough. Note that Waltham Forest is excluded from our matching analysis as it appears in some of our alternative definitions of treatment and therefore may be ambiguous whether it is treated or control. The results, presented in Panel B of Table 7, are similar, though somewhat weaker in the case of our housing market outcomes, to those presented in Table 2, and the standard errors are typically larger.

Finally, we exploit the availability of several years of pre-treatment data on the outcome of interest to apply the synthetic control method, whereby a weighted combination of control boroughs is used to estimate the counterfactual untreated outcome. This 'synthetic' control group is chosen to best emulate the outcome variable and other neighbourhood characteristics observed in the treatment group prior to the bombings (see Abadie and Gardeazabal, 2003). As we have multiple treated boroughs, we start by collapsing the

data to create one 'treated neighbourhood' for which we construct a synthetic control group, made up of a weighted combination of all control boroughs. The other neighbourhood characteristics we use to create a synthetic borough are those mentioned above: for the housing market outcomes, we use the deprivation index, air quality, and the percentage of housing deemed of poor quality / without central heating. For labour market outcomes, we specify the average wages offered by firms operating in each borough and the percentage of residents with basic education. And for the population analysis, we use changes in the borough-level stock of housing. We again exclude Waltham Forest. In Panel C of Table 7, we present the average difference in outcomes before and after the bombings for the treated neighbourhood, relative to the 'synthetic' control group. These results are again similar, with the effect on sales being insignificant due to a larger standard error. Overall, however, we argue that, while there is some evidence of a somewhat smaller effect of the bombings on the housing market, our results remain robust to using various methods to increase the comparability of boroughs.

E. Learning effects

Since any property purchased today may be re-sold in future, an individual's behaviour is likely to be determined by their perception of general attitudes as opposed to solely their own attitude (Gautier et al., 2009). For example, while an individual may not harbour any prejudice after terrorist attacks, they may be deterred from living in ethnically diverse areas if they believe others (i.e. potential future buyers) do. Gautier et al. (2009) suggest it may take time to learn about general attitudes and identify two scenarios with different implications for how quickly house prices react to a shock. In the first scenario, people have homogenous information on general attitudes and the effect of any shock is quickly assimilated into market prices. In the second scenario, people have heterogeneous information on general attitudes and price adaptation occurs more slowly as it takes time to learn about the change in general attitudes. If uncertainty prevails, people may also delay decision-making until more information becomes available, which may have an immediate impact on sales.

Learning effects may be relevant in the aftermath of the London bombings insofar as a series of related incidents have kept the 7/7 bombings in the news for several years. These include (but are not confined to) the shooting of persons suspected of terrorism (July 2005 and June 2006), subsequent attempted terrorist attacks in London, Glasgow (June 2007), and convictions of persons involved in foiled terrorist plots (November 2006, April, June and July 2007). Furthermore, a national inquest into the 7/7 bombings

only delivered its verdict in 2011. This raises the possibility that public attitudes have been shaped over the longer-term.

It is unclear whether labour market outcomes would evolve over time. It is possible that an employer may not themselves be prejudiced but may be concerned about having an ethnically diverse workforce if elements of their workforce harbour prejudice. It may also take the employer time to learn about general attitudes. On the other hand, an employer may make hires according to their own prejudice, in which case a more immediate reaction might be expected.

We investigate any potential 'learning effects' in two ways. First, as Figure 2 and 3 suggest the trend changes immediately after the bombings, we restrict the sample to the period July 2003 to June 2006; i.e. using only one year after the attacks to explore whether the effect had already materialised in 2006. Second, we model this explicitly by extending the analysis presented in Table 5 to allow differential linear time trends in the outcomes of interest to emerge after the bombings. Results are presented in Table 8.

Columns 1 to 3 show that, even when we restrict the data to include only one year after the bombings, we find a drop in house prices and sales, and an increase (albeit insignificant) in unemployment rates. The estimates for house prices and unemployment rates are about half of those in Table 2, suggesting that, rather than showing an immediate change after the bombings, house prices and unemployment rates have adjusted gradually. In contrast, the estimate for sales in Table 8 is similar to that in Table 2, suggesting an immediate impact of the bombings on sales in treated compared to control boroughs.

Modelling the trends explicitly, columns 4 to 6 confirm these findings, suggesting there are learning effects for house prices, with no immediate impact of the bombings ($treated \times post = 0$), but evidence of a cumulative effect ($treated \times post \times time < 0$), where $time$ is a linear (monthly) time trend. The results suggest that house prices in treated areas decrease by approximately 0.21% per month relative to control areas, while sales drop immediately following the bombings, with no evidence of increased activity over time. The results presented in column 6 indicate that labour market outcomes worsen as time progresses, with unemployment rates increasing by 0.47% per month relative to control areas.

The lower part of the table, for columns 4 to 6, presents the estimated effects on house prices, sales and unemployment rates after 6, 12, 18, and 24 months. This suggests that the impact of a change in attitudes on house prices takes just under a year for a statistically significant effect to materialise, with average house prices 12 months after the bombings being 2.27% lower in treated relative to control boroughs after one year. Two years after the bombings, house prices in treated boroughs sell for 4.74% less than in control

boroughs. These estimates mirror those presented in column 1 of Table 8, which estimate the average difference across a *one*-year period, and those in column 1 of Table 2, which focus on the average difference across a *two*-year period. Our results also suggest that it takes just under 18 months before any statistically significant increase in unemployment rates is observed. Two years after the attacks, unemployment rates are almost 12% higher in treated relative to control boroughs. While this may appear to be a large change, the average pre-treatment unemployment rate in treated boroughs is 4.48% (see Table 1) suggesting that unemployment rates climbed to just over 5% two year after the bombings.

F. Pathways

As discussed in Section III, a priori, it is unclear how the London bombings might shape perceptions of ethnically diverse neighbourhoods. It is likely that our estimates reflect several opposing effects, including potential changes in general attitudes towards ethnic minorities, as well as concerns relating to the safety of living in ethnically diverse areas. In the following, we provide some tentative, albeit indirect, evidence on the role of safety concerns in the housing market, specifically focusing on boroughs that may have been deemed 'less safe' areas to live in following the bombings. First, we examine the potential concern of living close to the *site* of the bombings. Indeed, if areas located closer to the bomb sites are perceived to be at greater risk of future attacks, house prices in treated boroughs *where the bombings occurred* may be more affected by safety concerns compared to other treated boroughs. The bombings took place near Liverpool Street (Tower Hamlets), Russell Square (Camden), Edgware Road (Kensington and Chelsea), and Tavistock Square (Camden). As Tower Hamlets is part of our treatment group, we begin by excluding this borough from our analysis. If safety concerns operate to reduce the desirability of living in Tower Hamlets - in addition to any changes in attitudes towards ethnic minorities - we might expect larger effects of the bombings on housing market outcomes in this borough, and thus expect to estimate smaller effects after dropping Tower Hamlets. Results from this exercise are presented in Panel A of Table 9, where we retain the richer specification that allows for learning effects but present only the main effects of interest. In Panel B, we also exclude the *control* boroughs where the bombings occurred (Camden, and Kensington and Chelsea), since - if safety is an important concern - we would expect to observe larger effects once these control boroughs are dropped. We do not find evidence of this. In fact, when we drop Tower Hamlets in Panel A, our results are slightly stronger, whilst they are somewhat weakened by dropping the control boroughs where the bombings occurred.

We also indirectly investigate safety concerns by excluding boroughs subject to increased police presence after the bombings (also known as 'Operation Theseus'; see Draca et al. (2011)). Although Operation Theseus was only in operation for a discrete six week period, the increased police presence may have given the impression that these boroughs are increasingly 'dangerous' areas, reducing the desirability of living in these neighbourhoods. Operation Theseus affected five boroughs: Camden, Kensington and Chelsea, Islington, Tower Hamlets, and Westminster. Panel C of Table 9 therefore presents estimates where, in addition to excluding Tower Hamlets, Camden, and Kensington and Chelsea (as in Panel B), we exclude the control boroughs of Islington and Westminster from the analysis. If people perceive these control boroughs as less save due to the increased police presence, dropping tem would increase our treatment effect. In contrast, Panel C shows smaller effects after excluding these areas, compared to Panel B. Taken together, these results suggest that concerns over safety in centrally-located boroughs or in boroughs affected by the increased police presence, are not the main drivers of our findings.

Finally, since our findings indicate that the London bombings impacted on the housing and labour markets, as well as population movements, we consider the possibility that all outcomes are driven by, for example, population movements, or that the housing market outcomes are driven by changes in labour market outcomes. These pathways are difficult to explore because including variables that are affected by the treatment as control variables in our analysis is unlikely to identify a meaningful effect. For example, holding unemployment rates constant in our house price analysis creates a comparison of the change in house prices in high-unemployment *treated* boroughs (where high unemployment might arise because of, or regardless of, treatment) with the change in house prices in high-unemployment *control* boroughs (where high unemployment clearly arises regardless of treatment). These compositional differences in boroughs characterised by the same post-treatment level of unemployment make such comparisons less desirable. Nevertheless, this approach may be informative of the possible mechanisms underlying our results. We include possible pathway variables in our analysis in Table 10. This shows little evidence that such pathway variables explain our results.

VII. CONCLUSIONS

This paper investigates whether the London bombings influenced attitudes towards ethnic minorities, examining the effects on the housing market, the labour market, as well as racial segregation in Greater London boroughs. We use a difference-in-differences approach, specifying treated boroughs as those with

a high concentration of Asian residents prior to the bombings relative to other boroughs.

Our results suggest that, relative to control areas, house prices in the treated areas fell by approximately 2% in the two years after the attacks, with sales and unemployment rates falling by almost 6%. Furthermore, we find an increase in segregation: the proportion of Asians increased in treated boroughs after the bombings relative to control boroughs. These results are robust to various sensitivity analyses, though we do find slightly weaker housing market effects once we specify a control group that is more similar to the treatment group.

The impact of extremist Islamic terrorist activity on attitudes towards and outcomes of ethnic minorities is relatively under-researched in the UK, with the majority of studies focusing on the US. The results presented in this paper suggest further research on these topics is desirable, to build a better picture of how ethnic communities fare, and to help shape policies to address the potential for adverse outcomes. Future research might use innovative methods to collect data at more disaggregated geographies, particularly with respect to the ethnic composition of neighbourhoods. Furthermore, it may want to examine a wider range of outcomes, not only at the aggregate level, but also at the individual level.

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FIGURES AND TABLES

FIGURE 1
Map of London Boroughs

Notes: Darker shading for Asian treated boroughs applied.

FIGURE 2
Housing market outcomes

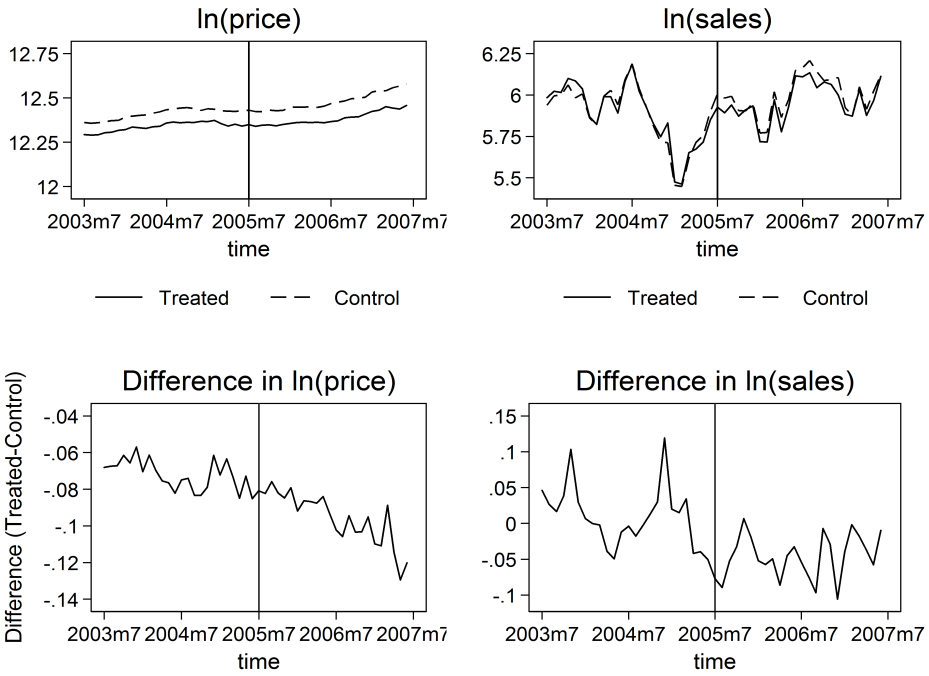

FIGURE 3
Labour market outcomes

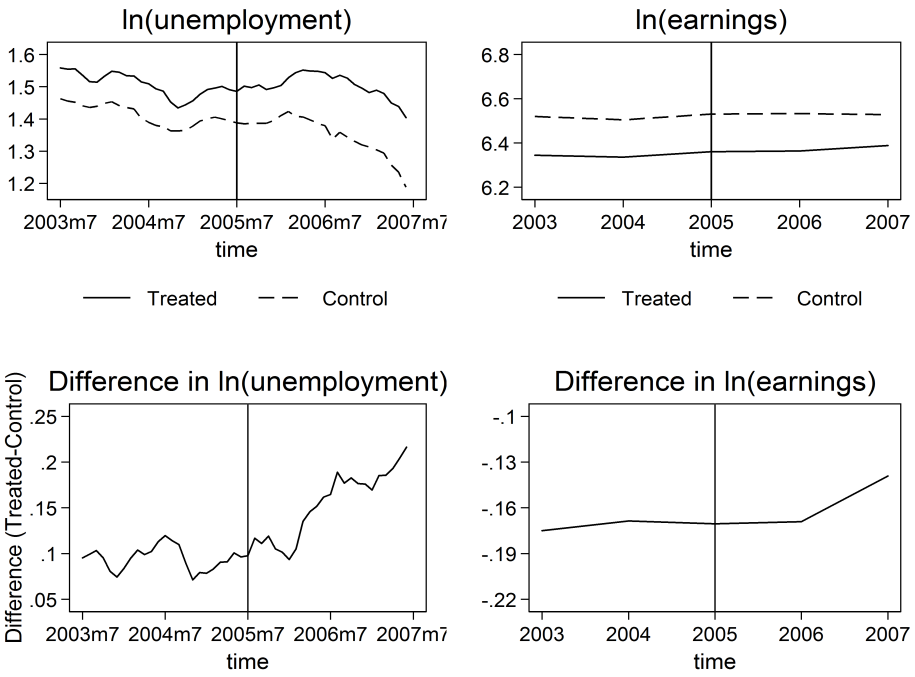

TABLE 1
Summary statistics in 2004, by definition of treatment

	Asian	Asian (excl. Indian)	Pakistani	Black	Other	Control
	(1)	(2)	(3)	(4)	(5)	(6)
ln(price)	12.35	12.32	12.31	12.33	12.59	12.42
Std. error	0.08	0.09	0.09	0.09	0.28	0.22
ln(sales)	5.93	5.90	5.91	5.87	5.92	5.93
Std. error	0.22	0.20	0.20	0.25	0.26	0.31
ln(unemployment)	1.50	1.57	1.46	1.93	1.47	1.40
Std. error	0.39	0.40	0.34	0.09	0.32	0.42
ln(earnings)	6.34	6.31	6.30	6.31	6.63	6.51
Std. error	0.12	0.12	0.10	0.11	0.33	0.23
ln(population)	12.40	12.35	12.40	12.41	12.41	12.32
Std. error	0.12	0.07	0.12	0.09	0.21	0.21
ln(whites)	11.80	11.76	11.83	11.86	11.97	12.02
Std. error	0.18	0.13	0.17	0.19	0.25	0.21
ln(Asians)	11.07	10.97	10.96	10.14	10.35	9.74
Std. error	0.13	0.26	0.25	0.68	0.64	0.48
ln(blacks)	10.05	10.08	10.18	10.82	10.11	9.84
Std. error	0.54	0.55	0.51	0.12	0.54	0.77
% whites	55.73	56.05	57.02	58.55	65.28	74.93
Std. error	7.76	8.00	8.07	9.00	10.80	8.68
% Asians	26.89	25.86	24.20	13.21	14.73	8.20
Std. error	4.00	5.64	4.59	9.96	8.33	3.21
% Asians (excl. Indians)	13.96	14.44	11.04	7.18	7.65	4.30
Std. error	8.28	8.00	4.03	5.79	5.50	2.12
% Pakistani	4.58	5.09	5.47	2.65	2.63	1.58
Std. error	2.09	2.26	1.71	2.52	2.41	1.32
% blacks	10.75	11.63	11.99	20.47	11.30	10.34
Std. error	5.38	5.54	5.27	1.56	5.99	6.36
% Other	3.33	3.15	3.33	3.72	4.97	3.24
Std. error	0.72	0.62	0.72	0.38	1.04	1.37

Notes: Population estimates at 30 June 2005 are used to determine treated boroughs. Asian treated boroughs are based on Indian, Pakistani, Bangladeshi and Other Asian populations and includes Brent, Ealing, Harrow, Hounslow, Newham, Redbridge and Tower Hamlets. Asian (excl. Indian) treated boroughs are based on Pakistani, Bangladeshi and Other Asian populations and includes Brent, Harrow, Hounslow, Newham, Redbridge, Tower Hamlets and Waltham Forest. Pakistani treated boroughs are based on Pakistani populations and includes Brent, Ealing, Harrow, Hounslow, Newham, Redbridge and Waltham Forest. Black 'pseudo treated' boroughs based on black populations and includes Brent, Hackney, Haringey, Lambeth, Lewisham, Newark and Southwark. Other 'pseudo treated' boroughs based on Chinese or Other ethnic populations and includes Barnet, Camden, City of Westminster, Ealing, Kensington and Chelsea, Newham and Southwark. Control boroughs refer to the comparison for Asian treated (i.e. column 1).

TABLE 2
Housing, labour and neighbourhood outcomes by definitions of treatment

	Definitions of treatment				
	Asian	Asian (excl. Indian)	Pakistani	Black	Other
	(1)	(2)	(3)	(4)	(5)
Panel A: Housing market outcomes					
Dependent Variable: ln(price)					
Treated × post	-0.023**	-0.021*	-0.028***	0.018	0.030
	(0.010)	(0.011)	(0.009)	(0.012)	(0.018)
Dependent Variable: ln(sales)					
Treated × post	-0.057**	-0.058**	-0.045*	-0.017	-0.014
	(0.022)	(0.022)	(0.024)	(0.028)	(0.027)
Panel B: Labour market outcomes					
Dependent Variable: ln(unemployment)					
Treated × post	0.058**	0.069**	0.066**	-0.006	-0.043
	(0.028)	(0.027)	(0.027)	(0.038)	(0.036)
Dependent Variable: ln(earnings)					
Treated × post	0.018	0.029	0.019	-0.016	0.029
	(0.020)	(0.020)	(0.021)	(0.023)	(0.028)
Panel C: Demographic outcomes					
Dependent Variable: ln(population)					
Treated × post	-0.004	-0.005	-0.009	-0.008	0.008
	(0.008)	(0.008)	(0.007)	(0.008)	(0.009)
Dependent Variable: ln(whites)					
Treated × post	0.017***	0.012	0.006	0.018***	0.015*
	(0.006)	(0.008)	(0.006)	(0.006)	(0.008)
Dependent Variable: ln(Asians)					
Treated × post	-0.117***	-0.108***	-0.111***	0.005	0.018
	(0.015)	(0.016)	(0.016)	(0.038)	(0.038)
Dependent Variable: ln(blacks)					
Treated × post	-0.026	-0.029	-0.034	-0.128***	-0.060**
	(0.032)	(0.032)	(0.032)	(0.018)	(0.026)

Notes: *p<0.1, **p<0.05, ***p<0.01. All analyses control for borough fixed effects and month dummies. Standard errors clustered by London borough. See Equation 1 for details of empirical specification. For monthly data (i.e. house prices, sales, and unemployment rates), the number of borough-month observations is 1536. For annual data (i.e. the earnings and population variables), the number of borough-year observations is 128. Definitions of treatment are as follows: 'Asian' (col. 1) is based on the population of Indians, Pakistanis, Bangladeshis and Other Asians, 'Asian (excl. Indian)' (col.2) is based on the population of Pakistanis, Bangladeshis and Other Asians, 'Pakistani' (col. 3) is based on the population of Pakistanis, 'Black' (col. 4) is based on the population of blacks, and 'Other' (col. 5) is based on the population of Chinese or Other ethnic populations (e.g. Latin American).

TABLE 3
Falsification analysis, incorrectly setting the treatment year

	1997	1998	1999	2000	2001	2002	2003
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Housing market outcomes							
Dependent Variable: ln(price)							
Treated × post	-0.003	0.001	-0.002	0.008	0.025	0.033	0.020
	(0.017)	(0.014)	(0.013)	(0.014)	(0.019)	(0.024)	(0.019)
Dependent Variable: ln(sales)							
Treated × post	0.026	0.049	0.043	0.048	-0.004	-0.020	-0.014
	(0.074)	(0.049)	(0.026)	(0.032)	(0.043)	(0.029)	(0.044)
Panel B: Labour market outcomes							
Dependent Variable: ln(unemployment)							
Treated × post	-0.022	-0.004	0.003	0.005	0.034	0.035	0.008
	(0.037)	(0.043)	(0.041)	(0.026)	(0.041)	(0.049)	(0.025)

Notes: Definition of treatment is 'Asian', based on the population of Indians, Pakistanis, Bangladeshis and Other Asians. 1997 denotes pseudo-intervention is coded as occurring in July 1997, with the data running from July 1995 to June 1999 (and similarly for the years 1998 to 2003). See also notes to Table 2.

TABLE 4
Falsification analysis, incorrectly setting the treatment year for the population analysis

	Dependent variable: ln()							
	population 2002	population 2003	whites 2002	whites 2003	Asians 2002	Asians 2003	blacks 2002	blacks 2003
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × post	-0.006	-0.009	0.020***	0.015**	-0.155***	-0.145***	-0.046	-0.040
	(0.008)	(0.008)	(0.007)	(0.006)	(0.019)	(0.018)	(0.044)	(0.039)

Notes: Definition of treatment is 'Asian', based on the population of Indians, Pakistanis, Bangladeshis and Other Asians. 2002 denotes pseudo-intervention is coded as occurring in July 2002, with the data running from July 2000 to June 2004 (and similarly for the year 2003). See also notes to Table 2.

TABLE 5
Differential time trends

	Dependent variable: ln()						
	price	sales	unemployment	population	whites	Asians	blacks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated × post	-0.023***	-0.090**	0.056	0.002	-0.002	0.032***	0.016
	(0.008)	(0.043)	(0.040)	(0.004)	(0.003)	(0.008)	(0.012)
post	-0.197***	0.036*	0.317***	0.005***	0.010***	-0.014**	-0.039***
	(0.006)	(0.018)	(0.020)	(0.002)	(0.002)	(0.007)	(0.010)
time (× 10)	0.094***	0.012***	-0.095***	0.006***	0.000	0.061***	0.038**
	(0.002)	(0.003)	(0.003)	(0.001)	(0.002)	(0.008)	(0.014)
Treated × time (× 10)	0.004	0.006	0.003	-0.003	0.009***	-0.075***	-0.021
	(0.003)	(0.008)	(0.005)	(0.004)	(0.003)	(0.009)	(0.020)

Notes: *p<0.1, **p<0.05, ***p<0.01. Standard errors clustered by London borough. Definition of treatment is 'Asian', based on the population of Indians, Pakistanis, Bangladeshis and Other Asians. *time* estimates a linear time trend (where time refers to the month for the analyses on house prices, sales and unemployment rates, and to the year for the population and ethnic group analysis), and *time* × *treated* is the differential time trend. For house prices, sales and unemployment rates, the period of analysis spans July 1995 to June 2007, with a total of 4608 borough-month observations (i.e 12 years). For the population analysis, the period of analysis spans July 2000 to June 2007, with a total of 224 borough-year observations (i.e 7 years).

TABLE 6
Variable treatment intensity

	Dependent variable: ln()			
	price	sales	unemployment	population
	(1)	(2)	(3)	(4)
% Asian*post	-0.001	-0.003***	0.003*	-0.000
	(0.001)	(0.001)	(0.002)	(0.000)

Notes: Definition of Treatment is 'Asian', based on the population of Indians, Pakistanis, Bangladeshis and Other Asians. See also notes to Table 2.

TABLE 7
Increasing the similarity between treatment and control boroughs

	Dependent variable: ln()			
	price	sales	unemployment	population
	(1)	(2)	(3)	(4)
Panel A: Excluding wealthiest boroughs				
Treated × post	-0.018*	-0.051**	0.056*	-0.004
	(0.010)	(0.021)	(0.028)	(0.008)
Panel B: Matching difference-in-differences				
Treated × post	-0.016	-0.062*	0.091***	-0.007
	(0.014)	(0.035)	(0.021)	(0.008)
Panel C: Synthetic control group				
Treated × post	-0.020*	-0.055	0.066***	-0.014
	(0.011)	(0.046)	(0.013)	(0.012)

Notes: Definition of Treatment is 'Asian', based on the population of Indians, Pakistanis, Bangladeshis and Other Asians. See also notes to Table 2. Standard errors in Panel B are bootstrapped to take into account that we must estimate the propensity score in order to construct the relevant weights in our analysis.

TABLE 8
Allowing for learning effects after the bombings

	Dependent variable: ln()					
	price	sales	unemployment	price	sales	unemployment
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × post	-0.012**	-0.059***	0.026	0.003	-0.094***	0.000
	(0.006)	(0.021)	(0.023)	(0.013)	(0.032)	(0.038)
Treated × post × time (× 10)				-0.021**	0.003	0.047***
				(0.010)	(0.026)	(0.016)
post × time (× 10)				-0.021***	0.080***	0.019**
				(0.007)	(0.008)	(0.008)
post	0.029***	0.065***	-0.016	-0.172***	-0.060***	0.293***
	(0.004)	(0.013)	(0.014)	(0.008)	(0.019)	(0.018)
time (× 10)				0.094***	0.012***	-0.095***
				(0.002)	(0.003)	(0.003)
Treated × time (× 10)				0.004	0.006	0.003
				(0.003)	(0.008)	(0.005)
% Δ 6 months				-1.01	-8.81	2.87
Std. error				0.89	3.16	3.88
% Δ 12 months				-2.27	-8.62	5.80
Std. error				0.80	3.96	4.24
% Δ 18 months				-3.52	-8.43	8.81
Std. error				1.09	5.07	4.84
% Δ 24 months				-4.74	-8.24	11.91
Std. error				1.55	6.32	5.62

Notes: Definition of treatment is 'Asian', based on the population of Indians, Pakistanis, Bangladeshis and Other Asians. Columns 1 - 3 show analyses restricting the data to two years prior to the bombings and only *one* year post (i.e. July 2005 to June 2006). Columns 4 - 6 estimate 'learning effects', distinguishing between the immediate and cumulative effects of the bombings on the outcomes of interest. For example, *time* × *post* estimates a linear time trend using monthly data in the post-treatment period while *time* × *post*× is the differential post-treatment time effect for treated boroughs. See also notes to Table 5.

TABLE 9
Exploring potential safety concerns using the housing market

	Dependent variable: ln()	
	price	sales
	(1)	(2)
Panel A: Excluding Tower Hamlets		
Treated × post	0.009	-0.091**
	(0.013)	(0.035)
Treated × post × time (× 10)	-0.027***	0.014
	(0.009)	(0.028)
Panel B: Excluding all bombed boroughs		
Treated × post	0.007	-0.085**
	(0.013)	(0.036)
Treated × post × time (× 10)	-0.022**	0.014
	(0.009)	(0.028)
Panel C: Excluding Operation Theseus boroughs		
Treated × post	0.004	-0.073**
	(0.014)	(0.035)
Treated × post × time (× 10)	-0.017*	0.012
	(0.008)	(0.028)

Notes: Definition of treatment is 'Asian', based on the population of Indians, Pakistanis, Bangladeshis and Other Asians. See also notes to Table 2.

TABLE 10
Including pathway variables and allowing for learning effects after the bombings

	Dependent variable: ln()					
	price	price	price	sales	sales	unemployment
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × post	0.007	0.003	-0.012*	-0.094***	-0.053*	-0.019
	(0.014)	(0.014)	(0.007)	(0.033)	(0.028)	(0.021)
Treated × post × time (× 10)	-0.021**	-0.019*	-0.027*	0.017	0.015	0.047*
	(0.010)	(0.010)	(0.015)	(0.024)	(0.024)	(0.027)
post × time (× 10)	-0.024***	-0.020**	0.001	0.085***	0.103***	-0.064***
	(0.006)	(0.007)	(0.010)	(0.009)	(0.008)	(0.011)
post	-0.169***	-0.156***	-0.110***	0.024	0.016	0.052***
	(0.008)	(0.005)	(0.004)	(0.021)	(0.019)	(0.014)
time (× 10)	0.094***	0.089***	0.073***	-0.015***	-0.017**	0.007
	(0.002)	(0.003)	(0.003)	(0.005)	(0.007)	(0.008)
Treated × time (× 10)	0.004	0.004	0.015**	0.007	-0.002	0.006
	(0.003)	(0.003)	(0.007)	(0.008)	(0.010)	(0.015)
ln(sales)	0.043***					
	(0.013)					
ln(unemployment)		-0.054*		-0.285***		
		(0.029)		(0.047)		
ln(whites)			-0.856***		0.127	-2.129***
			(0.228)		(0.341)	(0.393)
ln(Asians)			-0.003		0.134*	-0.364***
			(0.063)		(0.079)	(0.118)
ln(blacks)			-0.015		0.065	0.191***
			(0.031)		(0.040)	(0.047)

Notes: Definition of treatment is 'Asian', based on the population of Indians, Pakistanis, Bangladeshis and Other Asians. See also notes to Table 5. The number of borough-month observations is reduced to 2688 when including population variables as these are only available from the treatment year 2000.

TABLE A1
Sources and availability of data

	Source	Notes	Observed from	Observed until	Time dimension
Housing market					
House prices	Land Registry	Data provide a complete record of residential property transactions in England and Wales	July 1995	June 2007	Monthly
Sales	Land Registry	Data provide a complete record of residential property transactions in England and Wales	July 1995	June 2007	Monthly
Labour market					
Unemployment rates	Nomis	Based on receipt of unemployment-related benefits	July 1995	June 2007	Monthly
Residential earnings	ASHE	Male average weekly full-time earnings of residents	2002	2007	Annually January - December
Demographics					
Total population and ethnic composition	PEEG	ONS Mid-Year Population estimates, adjusted for births, deaths and national/international migration	2001	2008	Annually July (t-1) to June (t)
Deprivation					
Deprivation indices	DCLG	Individual components of the 2004 UK Deprivation Indices	2004	2004	Various data sourced between 2000-2004

Notes: Although data are available post June 2007, our analyses focuses on the period up to and including June 2007 (i.e. two years after the bombings). ASHE denotes the Annual Survey of Hours and Earnings; a 1% sample of employees appearing in the pay-as-you-earn taxation system. PEEG denotes the Office for National Statistics Population Estimates by Ethnic Group. DCLG denotes the Department for Communities and Local Government.