Terrorism and the Value of Proximity to Public Transportation: Evidence from the 2005 London Bombings

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Abstract

Terrorism has become a primary concern for city dwellers around the world. This paper uses the 2005 attacks on the London Tube to provide causal evidence of the negative impact of terrorism on the value of proximity to public transportation. These attacks brought major transit stations into the spotlight as high-risk locations. As a result, surrounding communities became less attractive places in which to live and conduct business. I find that house prices closer to the major transit hubs of London fell by 6 percent for one year. This shock spread to Manchester as well: house prices closer to major transit hubs dropped by 9 to 14 percent for three to four years. I also show that new firms are less likely to locate near major stations after the attacks, particularly those relying on foot traffic. Among incumbent firms, those serving customers in person are most hurt by the attacks.

Keywords: Terrorism; 2005 London Bombings; Public Transportation; Real Estate; Firms

JEL codes: H56; K42; R41; R30; L25

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1 Introduction

Since 9/11, the threat of terrorism has grown into a major global challenge.[1] Cities are especially vulnerable to terrorism, as they concentrate most of the world's population, assets, and economic activity. While terror is unlikely to challenge the importance of cities,[2] can terror lower city density and related gains from agglomeration? To the extent that the busiest, most dynamic urban locations are those facing the highest terror threats, this is a likely scenario.

I study the impact of the 2005 London terror attacks on the value of proximity to public transport nodes in London and Manchester. Public transport nodes may be particularly harmful locations to be regarded as risky. Transport networks are the veins of cities, with households and firms clustered around transport nodes to reduce transport costs. This paper shows that terror attacks on transport nodes can lower incentives to cluster around them.

During the morning rush hour of July 7, 2005, four terrorists detonated bombs aboard three London Tube trains and a bus departing from King's Cross St Pancras station, London's main public transport hub.[3] London had not experienced a bombing of this severity since World War II. Two weeks later, Londoners lived through the close call of yet another attack, targeting three Tube trains and a bus. A malfunction with these bombs prevented another tragedy.

These events provoked an unexpected shock to the perceived risk of terror on major transit hubs. One was reminded that urban rail hubs were especially appealing for terror attacks, for the crowds they attract during rush hour, their cumbersome protection, and their high cost to repair.[4] A history of recurrent attacks on rail hubs, both in the UK and elsewhere,[5] is a testament of this appeal. While terror attacks remain rare events, the trauma of 7/7 and of "UK's biggest ever manhunt"[6] for those behind the July 21 attempts most likely made rail hubs appear as overly dangerous places.[7] In connecting new and past attacks on rail hubs, the media also amplified the perception of danger of proximity to these nodes (Sloggett, 2013).

To capture the within-city spatial incidence of the London attacks I rely on a difference-in-differences strategy. Areas that become treated after the London attacks are those surrounding the main rail nodes of London and Manchester. I conjecture that the higher the ridership of a

[1] *Pew Research* 2017, *Center for Strategic and International Studies* 2016, *Nielsen* 2016, or *Gallup* 2015 surveys.

[2] Davis and Weinstein (2002) find that within 15 years after the Allied bombing of Japanese cities of World War II, cities rebounded to their relative position in the distribution of city sizes. Brakman et al. (2004) arrive at similar findings for West German cities also bombed by Allied forces during World War II.

[3] The 7 July 2005 London bombings are often referred to as 7/7, an acronym I also use in this paper.

[4] In a *CNN* article on the 2010 Moscow metro bombings, Will Geddes of *International Corporate Protection* called subway attacks "ideal for a terrorist. Any attempt to impose [airport-style screening] is going to cause major chaos and [...] and render any subway network ineffective."

[5] In the UK, rail networks have been frequently bombed, from the 1883 explosions at London's Praed Street and Charing Cross stations to the 2017 structural damage of Victoria railway station, attached to the Manchester Arena. Globally, Moscow (2004, 2010), Madrid (2004), Minsk (2011), Istanbul (2016), Brussels (2016), and St. Petersburg (2017) are examples of cities whose main rail nodes have been targets of terror attacks. While terrorist attempts on air transport are frequent as well (e.g., the 2015 attack on Russian Metrojet 9268, the 2016 attacks on Brussels Zaventem and Istanbul Ataturk airports) and are likely to have a first-order effect on travel between cities, they are less likely to affect the inner structure of cities and are thus of less interest to this study.

[6] *The Guardian* February 2008 article, *Reuters UK* July 2007 article, or *NY Times* July 2005 article.

[7] This is an example of an availability heuristic (Tversky and Kahneman, 1973). Extraordinary events that are prominent in one's mind can bias upwards the assessment of the likelihood of another similar event.

station, the higher its perceived risk to be targeted by another terrorist attack. The intuition is that as terrorism aims to create the most havoc possible in one stroke, the more transited a station is in a network, the more valuable it is as a target. I defend this conjecture in Section 2. Those treated areas are then compared against two control areas. One is located slightly farther from the same major nodes, while the other surrounds nearby secondary stations.

I first show the impact of the 2005 London attacks on residential real estate markets. I expect house prices to account for changes in the value of proximity to public transportation. House prices near main stations may reflect both the fear that housing may be damaged by an explosion, as well as the fear of exposure to a high-risk station during travel. Moreover, if the threat of terror makes people less likely to frequent local businesses, as in Becker and Rubinstein (2011), house prices can also capture a decline in the value of consumption amenities.

Using UK Land Registry data I find that, prior to the attacks, houses of treated and control areas followed similar trends. For a year after the attacks however, house prices near major rail hubs in London experienced a decline of around 6 percent. Rentals near the same major hubs of London were also less attractive for one year after the attacks. In Manchester, houses closer to the busiest rail hubs see their prices plunge by up to 14 percent for three to four years.

The localized rise in the perceived threat of terror was likely to affect firms as well. With UK Companies House data and the same spatial definition of treatment and control, I find that new firms were less likely to locate close to main rail hubs for up to four years in both cities. This pattern is more salient for firms reliant on foot traffic, such as bars or chiropractic clinics. Using Bureau Van Dijk's Amadeus data, I also show that incumbent businesses near these potential targets were hurt in their EBIT margin,[8] fixed assets, liquidity ratio, and credit period. Businesses such as beauty parlors or theaters were most harmed by the attacks. These findings suggest that the London attacks may have decreased the appeal of living close to busy stations by also lowering the value of local consumption amenities.

Why study the impact of the London attacks on Manchester as well? The sizable impacts of the London attacks on Manchester, a likely future target of terror,[9] suggest that terrorism can hurt cities even without an actual attack. The two cities also differ enough to make room for heterogeneous reactions to terror. The lower reliance of Manchester on public transit is one relevant difference. The higher sensitivity of Manchester residents to terror, as revealed by the *British Social Attitudes* surveys, is another. Furthermore, a day before the attacks, Londoners learned that their city had won the Olympic Bid for 2012. I argue that this victory may have contributed to London's resilience to the attacks. Last, the sparse rail network of Manchester favors a cleaner spatial definition of treated and control areas. Manchester also has the benefit of a multi-hub transit network, allowing me to use the same research design used for London.

This paper adds to a growing number of papers measuring the impact of terror on cities, such as Abadie and Dermisi (2008), Arbel et al. (2010), Becker and Rubinstein (2011) or Elster

[8]The EBIT margin, a measure of profitability, is the ratio of earnings before interest and taxes to net revenue.

[9]Manchester's appeal as a terror target is confirmed not only by its past IRA bombings, but also by the May 2017 bombing outside of the Manchester Arena and near Manchester's Victoria train station. This was the most deadly terror attack occurring in the UK since 7/7 (*The Telegraph* June 2017 article). Manchester's Victoria is taken as a treated station.

et al. (2017). In finding negative, sizable, and medium-term local costs of terror, this paper is in line with previous research.

This paper addresses empirically what Glaeser and Shapiro (2002) describe as the "theoretically ambiguous relationship between the danger from terrorism and urbanization." High-density areas, such as those around major transport hubs, are the most likely targets of terror. This threat to life and property in high-density areas lowers their appeal. But when terror attacks raise transport costs, as is the case with terror attacks on transport networks, then density as a substitute to travel might be more attractive. I provide evidence that the threat of terror can tilt the balance towards a lower valuation of proximity to major transport hubs.

The adjustment to the fear of terror seems to not only have occurred through real estate prices, but also through reallocations of economic activity away from these hubs. Abadie and Dermisi (2006) show that large enough increases in the perception of terrorism can change how firms cluster, reducing the extent of agglomeration economies. The fact that fewer firms located near major rail hubs for up to four years after the London attacks suggests that these attacks were traumatic enough to erode the benefits of high accessibility areas.[10]

The rest of the paper is organized as follows. Section 2 provides context on the 2005 London attacks and motivating evidence for this paper. Section 3 documents the impact of these terror attacks on the housing markets of London and Manchester, while Section 4 shows their impact on firms. Section 5 rules out alternative explanations for my findings. Section 6 concludes.

2 The 2005 London attacks: Background and motivating evidence

If one were to only account for the material destruction caused by the London attacks, London would appear as having recovered within a month, when all portions of the Underground network damaged by the blasts were reopened at full service.[11] However, the fallout from the attacks was unlikely to restrict itself to this short-term disruption.

Fear of exposure to what was seen as a heightened threat of terrorism led to far-reaching behavioral reactions. In a September 2005 *Ipsos MORI* survey, 20 percent of Londoners reported spending less time in Central London after the attacks, and 11 percent expressed an intention to move out of London. In December 2005, Rubin et al. (2007) found that half of Londoners still believed that their lives and those of loved ones were in danger.

[10] While firms relying on foot traffic seemed to have been driven away more, firms in other sectors did not reallocate in such a way as to keep entry rates constant. While the available data does not allow me to also compute firm exit rates, the fall in the performance of incumbent firms in treated areas makes it unlikely that exit rates in treated areas could have fallen enough to compensate for the decline in entry rates and thus maintain the stock of firms in treated areas. I show in Table C7 in Appendix C that firms incumbent in treated areas reduced their investments in fixed assets after the attacks, which signals an intention to lower their attachment to their now-riskier locations and supports the conjecture of nondecreasing exit rates in treated areas.

[11] The explosions damaged the Tube network along its main lines departing from King's Cross, one of Britain's largest transport hubs. Debris also reached neighboring buildings, but the range of the damage is unclear. During 9/11, buildings within 500 meters from the World Trade Center were severely damaged (Abadie and Dermisi, 2008). After the IRA attacked two of London's main railroad stations (Victoria and Paddington) in 1991, a *NY Times* February 1991 article reported "trails of blood," glass, and debris at least 50 meters away from the site of the blast. The damage during 7/7 is likely to have fallen in this 50- to 500-meter interval.

The fear of resuming travel on the Tube was particularly acute.[12] According to Dr. Patricia d'Ardenne, head of the UK Institute of Psychotrauma,[13] patients suffering from stress disorders after the attacks felt "hyper-vigilance, [as if] in a war zone, and phobia of public transport" and needed to be gradually escorted back on the Tube. Rubin et al. (2005) show that in the first days after the London bombings, 46 percent of Londoners did not feel safe traveling by Tube, and 32 percent reported an intention to travel less. 43 percent thought that the security measures used to prevent a future terror attack on London's transit were unlikely to work.

Prager et al. (2011) find that passenger journeys on the London Tube fell by an average of 8.3 percent for the four months following the attacks, with reduced travel persisting for up to a year. 82 percent of the fall in passenger journeys cannot be attributed to supply-side (e.g., service disruption from station closures) nor demand-side factors (e.g., economic conditions), making the fear that an attack would target the Tube again the likely culprit. Draca et al. (2011)[14] test for a difference in Tube journeys across stations in their treatment (Central London) and comparison boroughs (all but the Central) and also find a 13 percent fall for six months after the attacks.

This aggregate recovery in ridership may conceal long-term readjustments of traffic throughout the network which themselves might matter for aggregate welfare. In particular, I explore whether major nodes in the network - otherwise valuable locations for households or firms - may have been flagged by the London attacks as the most dangerous to use. The spatial deployment of police forces after the attacks around "major [...] transport nodes, particularly Tube stations" (Draca et al., 2011) supports this hypothesis.

I also test this conjecture in a difference-in-differences specification which compares the ridership of the four major rail hubs of London with that of neighboring secondary stations before and after the attacks. I find that after the attacks, subway users were relatively less likely to transit through the major hubs of the London Tube. Figure A1 in Appendix A plots the *DiD* coefficients and shows that main stations experience a 12 to 17 percent relative loss in ridership with respect to 2004.[15]

The reach of the London attacks went not only beyond the physical destruction, but also beyond its influence on Londoners. The events caused major distress to residents of other UK cities as well. Manchester residents in particularly appear more concerned about terrorism than Londoners. The *British Social Attitudes* surveys conducted after the attacks asked whether one agreed that the UK might experience another major terror attack in the following

[12] Gigerenzer (2006) finds that terrorist attacks on one mode of transportation (e.g., air or rail) lead to anxiety over using that mode and a switch towards what might be riskier modes per mile traveled (e.g., private car or bicycle). Travelers seem to fear unlikely yet dramatic events more than regular accidents.

[13] *BBC* July 2015 article.

[14] Despite Draca et al. (2011) defining treatment at a coarser level (police deployment data is at the borough level), our definitions share the same logic. Treated boroughs host potential targets, such as public transit nodes.

[15] While major stations become relatively less transited than secondary stations for up to 4 years, I later find a 1- to 2-year relative decline in house prices around major stations compared to those around secondary stations. Whether these differences in resilience are puzzling or not depends on (i) how much of the relative fall in subway ridership is actually due to individuals captured by my real estate sample and (ii) how much of the relative fall in house prices near major stations is explained by a relative decline in the value of transportation versus consumption amenities. Given the lack of suitable data for 2001 to 2009, these questions cannot be answered.

years. 79 percent of Londoners answered that they agreed with the statement, with 41 percent agreeing strongly. In Manchester, the same two statistics were 86 and 55 percent. Residents of Manchester also displayed stronger racial prejudice attitudes than those of London, anxiety over terrorism and racial prejudice being positively correlated attitudes.[16]

While there is no research on the travel patterns of Manchester residents around the attacks, the two cities seem to differ in their baseline reliance on rail for commuting. While residents of Central London relied heavily on rail for commuting,[17] the same seems to not hold for those living in Central Manchester.[18] Whether rail users in Manchester discriminated against major rail hubs after the London attacks is of particular interest.[19] I compute the share of entries and exits occurring through the busiest stations (defined as treated) out of the total entries and exits (through treated and control stations combined), from 2000 to 2010. I find that while between 2000 and 2004 the busiest stations accounted for 97 percent of the ridership occurring through all stations combined, from 2006 to 2010 these same major hubs lose part of their dominance, accommodating an average of 85 percent of the same combined traffic.

These findings suggest that to account for the total fallout of terror attacks one must look beyond the physical destruction caused by the attacks and even beyond the city actually targeted by the attacks. Moreover we learn that while cities might show an overall resilience to terrorist action, behavioral reactions to the uneven geography of perceived increases in the threat of terror may still be important. In what follows, I provide evidence on the spatial incidence of the London attacks manifested through households and firms.

3 Terrorism lowers the value of proximity to public transportation: Evidence from the housing market

3.1 House prices capture updates in the value of local amenities

I aim to measure the impact of the London bombings on the value of proximity to major public transport stations. As both updates in the value of services and costs associated with living in a now-riskier place capitalize into house prices, these prices are my first object of interest.

A growing literature quantifies the costs of terror through house prices. Arbel et al. (2010) find sustained buyer aversion to home purchases on streets directly exposed to gunfire during

[16] 16% of Londoners surveyed by *BSAS* in 2013 admitted to racial prejudice, down from 33% in 2000. Manchester emerges in 2013 as the city with the highest level of self-reported prejudice at 35%, up from 26% in 2000.

[17] Two thirds of those working in Central London in 2004 arrived there by either the Underground or the National Rail (2004 *TfL* Travel Report). 82 percent of Underground journeys started or ended in Fare Zone 1, with a third of journeys made fully within Zone 1. Only 11 percent of workers used a car to commute to a job in Central London because (i) the average traffic speed in Central London during morning peak hours was of 10.6mph and (ii) since February 2003 car traffic through Central London's Congestion Charging Zone has become more expensive. 32 percent of Inner London residents traveled by either Underground or National Rail to their job.

[18] In 2005, 76 percent of commuters from the Greater Manchester metropolitan area used a private car, 8 percent the bus, 3 percent rail, and 9 percent walked. The average commute was 27 mins by car, 53 mins by rail and 13 mins walking. Given that most jobs were located in the center of Manchester, I infer that those living in the suburbs more likely commuted by rail, while those in the Center more likely walked to work.

[19] Rail is the only mode of travel for which we know the station at which people embarked or alighted in the city center (see *The Office of Rail and Road* data) between 2000 and 2010.

the Second Palestinian Intifada of 2000. Elster et al. (2017) find that Hezbollah's rocket attack on Northern Israel during the 2006 Second Lebanon War and the continued threat posed by the organization also led to a decline in house prices in severely hit localities. Last, Besley and Mueller (2012) use house prices to estimate the size of the peace dividend in Northern Ireland.[20]

An attack on a major transit hub can affect house prices near other hubs in several ways. Those using such stations might fear becoming likely victims of another attack. Chan (2007) uses Oyster Card[21] data from London to show that only 46 percent of the duration of a Tube journey is actually spent in the train. Access, egress, and interchange consume 31 percent of journey time, while platform wait accounts for 20 percent. Entering and exiting the network through a major station, which is what commuters living in nearby houses would do, inflate one's exposure to the risk of terror attacks. To the extent that one decides to continue her rail commute, accessing the network through a secondary station might seem less risky.

Even if not commuting through the nearest major rail station, buyers might fear becoming collateral damage of an attack. Their house may be harmed in an explosion, which would lower the return from the investment (Rossi-Hansberg, 2004). Or, if an initial terror plan fails, violence may spill over to the vicinity of the station.[22]

Furthermore, while buyers may not fear terrorism themselves, they might care that others do. These others might be sellers who lower their price in a rush to leave the area. Or, if resale is likely, own preferences might be overridden by those of fearful future buyers. Such future buyers might expect lower house prices to cover for the higher terrorism insurance costs.[23]

Finally, the attacks might also affect the cohesion and vibrancy of neighborhoods. Rosenthal and Ross (2010) show that businesses relying on foot traffic, such as retail and high-end restaurants, are sensitive to crime. The enjoyment of these local consumption amenities is likely to be enhanced by others sharing or acknowledging this consumption experience.[24] Therefore, even those residents of houses near rail hubs not fearful of terror may see their enjoyment of local amenities falter if fewer people frequent the area. I focus on this channel when studying the business environment in Section 4.

3.2 The main difference-in-differences specification

I now describe the main difference-in-differences specification employed throughout the paper (Equation 1). To identify the causal impact of the London bombings on the appeal of proximity to urban rail hubs, I rely on the unexpected nature of the London attacks[25] and the lack of

[20] For Besley and Mueller (2012), casualties are informative about an unobserved state of peace or conflict. Similarly, the London attacks triggered updates to the perceived risk of proximity to public transit hubs. Both papers study house prices, as they capture these updates in the perceived likelihood of more violence.

[21] The Oyster Card is an electronic form of payment used across public transport modes in Greater London.

[22] During the 2015 Paris attacks, one of the terrorists attempted to enter Stade de France with the goal to harm those attending the game, was turned away at the entrance and decided to detonate the explosives, killing himself and a passerby in the proximity of the stadium (*The Wall Street Journal* November 2015 article).

[23] The UK terror insurance market faced a surge in demand after 7/7 (*Insurance Journal* July 2005 article).

[24] The visibility of a certain type of consumption may be a critical component of its enjoyment (Heffetz, 2001).

[25] For the Home Secretary at the time, Charles Clarke, the attacks came "out of the blue" as the four bombers were "clean skins" with no known links to terrorism (*The Telegraph* July 2005 article).

pre-trends in all observable characteristics of treated and control units.

$$p_{ijt} = \alpha + \boldsymbol{\beta' X_{ij}} + \mu_{j(i)} + \sum_{t=01/02, t\neq 04/05}^{08/09} \gamma_t \times \tau_t + \sum_{t=01/02, t\neq 04/05}^{08/09} \delta_t \times \tau_t \times T_{ij} + \varepsilon_{ijt} \qquad (1)$$

where p_{ijt} is the log price in £ stated on the transfer deed for house i from postcode j sold in year t. $\boldsymbol{X_{ij}}$ is a vector of time-invariant characteristics for house i in postcode j. The next set of terms are: $\mu_{j(i)}$, the fixed effect for the postcode j of house i; τ_t, the common time effect for period t, and ε_{ijt}, the random error term. The coefficients of interest are the δ_ts, the usual *DiD* coefficients.[26] I split the data in twelve-month blocks[27] between the months of July of two consecutive years and study four such yearly blocks before and after the London attacks.[28] τ_t captures all shocks that equally affect treated and control houses in a given 12-month interval.[29]

I build on the spatial variation in the perceived likelihood of future terrorist threats. Houses are assigned to either treated or control postcodes based on their proximity to locations of higher potential risk. Houses i from postcode j that are deemed treated ($T_{ij} = 1$) are those whose postcode j is within 600 meters of a major rail station in London and within 500 meters in Manchester. I then define two comparison groups with a dual purpose, to each shed light on a different channel of impact of the London bombings on nearby communities and to confirm that results are not an artifact of the choice of the control group (see Figure 1 for an example).

According to the first definition of the control group, houses within 600 meters (500 meters in Manchester) of a major urban rail station are assumed to face an increase in the perceived risk of terrorism after the attacks. The control group contains all houses within 600 to 1,200 meters (500 to 1,000 meters in Manchester) of the same major urban rail station. While still within walking distance to the main hub, residents of farther houses might feel sheltered from a strong explosion on the target or violence spilling over to the neighborhood of a missed target.

In the second definition of the comparison group, I include all houses within 600 meters (500 meters in Manchester) of a neighboring secondary urban rail station. This definition relates to the residents' fear of being injured while exposed for a longer interval to a major urban rail station, for instance, while waiting on the platform. Home owners might still value an easy access to the mass transit network, but they may want to reduce their exposure to main hubs.

[26] In the case of Definition 1 for post-attacks treatment and control areas, δ_t measures the difference between the average log price change for houses that are close (to a given major Tube station) and the average log price change for houses that are farther (from the same major Tube station). The coefficient for year t reports a change with respect to the omitted reference year, i.e., the year prior to the bombings (July 2004 - July 2005).

[27] I argue that a 12-month frequency is the right choice. Using the date of the transaction for a higher-frequency analysis (e.g., quarterly) increases the noise of the *DiD* estimates. Allocating a property sold to a specific quarter is also not trivial. The most useful date would be that at which buyers and sellers agreed on the house price. The UK Land Registry reports the date sales are completed, as stated on the transfer deed. Due to the time lag between mortgage applications and the filing of paperwork, the Land Registry date is an upper limit for the price setting date. It is unclear which time frequency is most suitable to capture the effect of the bombings on house prices. Nevertheless, I run the *DiD* analysis at quarterly frequency and find similar results.

[28] I find that after four years, treatment and control areas regain common trends in both cities.

[29] Year fixed effects address occurrences with network-wide implications, such as the congestion charge scheme introduced in London in 2003. Also, they address city-level trends before and after the 2007 credit bubble.

I classify Tube stations based on their 2004 ridership[30] and find King's Cross St Pancras, Victoria, Waterloo, and Oxford Circus to be London's top four busiest stations. The 2002 *Transport for London Interchange Plan* classifies these stations as Category A, as major nodes in the network connecting to at least three lines. I apply a similar reasoning to Manchester as I did to London and choose stations most likely to be attacked as treated. As there is no subway in Manchester, I settle for the next most vulnerable means of transportation, the overground rail and tram. The stations forming my treated sample are Picadilly, Victoria, and Deansgate, the most heavily-transited multimodal hubs in Manchester.

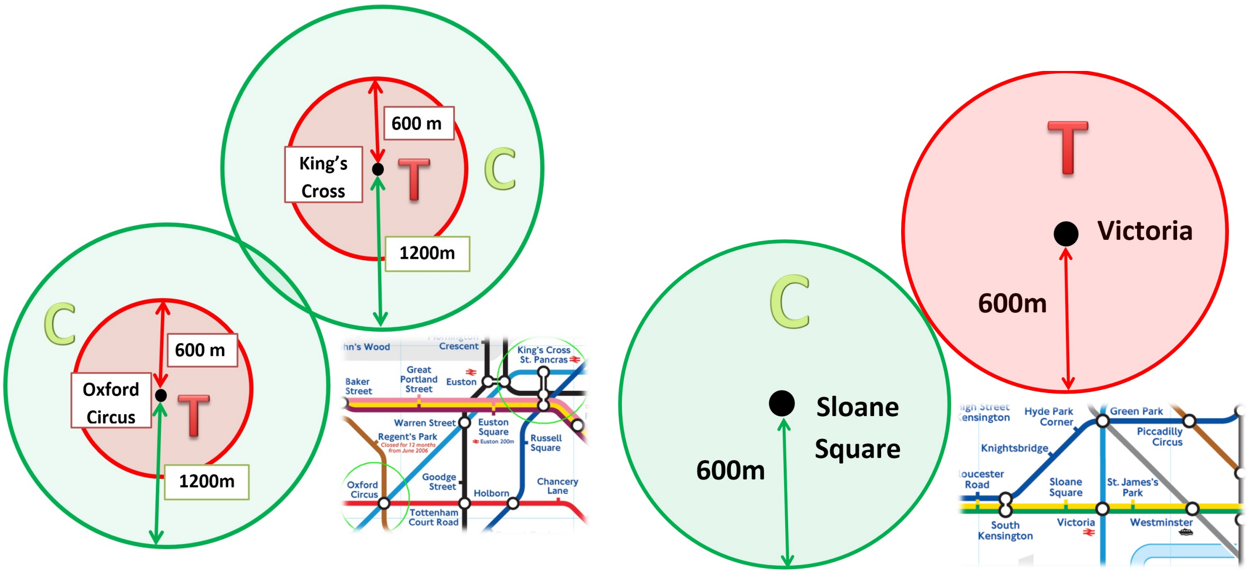

Figure 1: Illustration of Definition 1 (left) and 2 (right) of post-7/7 treatment and control areas
Source: Author's sketch developed from a magnified image of the network map of tubemaplondon.org.

Note: On the left-hand side of the above figure I sketch the areas of treatment and control (as per Definition 1) for Oxford Circus and King's Cross Tube stations. Both stations are hubs for at least three underground lines (as shown in the zoom on the right). On the right-hand side, I use the Victoria and Sloane Square stations to exemplify the construction of the treated and control areas for Definition 2. After the 7/7 attacks, the treated houses are those at most 600 meters away from Victoria station, a major hub, whereas the control houses are those no more than 600 meters far from Sloane Square station.

The London group of secondary Tube stations was selected based on their distance from the major hubs, lower ridership, and lack of connections to other subway lines.[31] The following stations thus act as control group: Angel, Chancery Lane, Caledonian Road, Barnsbury, and Russell Square (control for King's Cross); Pimlico, James Park, and Sloane Square (for Victoria); Southwark, Lambeth North, and Temple (for Waterloo); and Goodge Street, Tottenham Court Road, and Bond Street (for Oxford Circus). For Manchester, I apply a similar rationale. I identify Oxford Road and Salford Central as secondary transit stations. Both are mainline

[30] Ideally one would want to track individual commuter behaviors, or at least the average behavior of residents at a finer resolution. However, there is no Oyster Card data for London for that period and Manchester lacked a smart ticketing system until 2012. Annual ridership figures are calculated as entries plus exits, weighting weekdays by 253, Saturdays by 52 and Sundays by 59. Due to data inaccuracies, *Transport for London* (TfL) considers summing entries and exits as a more robust measure of ridership.

[31] Secondary stations are 800 to 1,400 meters far from the main Tube stations. Main and secondary stations are close enough to offer similar housing options, while far enough to prevent overlapping in "walking impact zones."

railways stations and do not allow for a direct Metrolink connection.

Note that areas surrounding stations of strategic or symbolic value may also be at risk, as proven by the March 2017 attack near Westminster station and June 2017 near London Bridge station. I argue that creating the sample of treated stations based on ridership alone is preferable nonetheless. While the strategic or symbolic value of a station is open to debate, station size is a measurable criterion. Attacking busier stations can lead to higher human or material loss, ergo station size is also a strong predictor of its target value to terrorists. One can allow for Londoners to fear smaller stations of strategic or symbolic value on top of fearing main transit hubs and still find the results in this paper interesting.[32]

The choice of the distance thresholds defining the treated and control areas was informed by the existing literature on "walking impact zones." As the main London Tube hubs are on average 1,800 meters apart, the 600- and 1,200-meter thresholds allow me to avoid overlapping treatment areas. According to urban transit planners, rail users in the US and Canada can be expected to walk up to 1,200 meters to the nearest point of access (Walker (2011) and Federal Transit Administration (1995)). As European commuters seem to be willing to walk even further than 1,200 meters,[33] I assess 1,200 meters to be a reasonable upper bound. I define the first 600 meters around each station to be the strongest "walking impact zone"[34] and the following 600 meters as the weaker zone.

The same reasoning can be extended to Manchester. One only needs to shrink the "walking impact zones" to reflect the smaller city center and distinct commuting patterns. Hence, thresholds are reduced to 500 and 1,000 meters to be consistent with smaller distances between stations (see Figure A2 in Appendix A). Larger distances would lead to overlapping treatment areas[35] or defining the same area as treated by one station while in the control of another. Moreover, recall that only a minority of work commutes in 2005 were carried out by rail in Manchester. The high speed and low costs of private car commutes meant that one would switch to a rail commute only if particularly convenient. It is likely that higher walking distances to the nearest station would be unappealing to a Manchester resident.

Given my focus on the distance to the closest rail station, it is reassuring to learn that buyers and renters across major UK cities are acutely aware of this distance. According to one London real estate specialist "proximity to the Tube is the single most important factor for almost every buyer and renter coming through [their] doors," turning rail lines into the "veins

[32] In terms of the implications for my estimates, to the extent that landmark stations are in the control area of the busiest stations, current estimates may be biased downwards. If these landmark stations are part of my treated area, then my estimates capture a mix of treatment effects. While the impact of station size on house prices would be less obvious, the causal nature of my estimates and their direction would remain valid. It might also be that these other possible targets are neither part of my treated nor control areas. If landmark stations also cause a decline in house prices, then current estimates capture only a part of the overall impact.

[33] Data on walking times for commuters combining walking with a rail journey was not available. For those who only walk to their job in Central London, the average journey lasted 18 minutes to travel about 1,600 meters.

[34] Rightmove, one of the UK's largest real estate agencies offers as one of their online services the option to search for property around rail stations. A half-mile commuting distance is their default option.

[35] While residents of a house have the option of using either of the nearby stations, it seems reasonable to assume that the closest one is preferred. I assign every house to the influence area of the closest station.

of the city's property map."[36] However, closeness to a rail station is not equally valuable in all UK cities. According to Nationwide,[37] houses within 500 meters of a rail station in London commanded a 10.5 percent premium compared to those 1,500 meters away. In Manchester the same premium was 4.6 percent, in line with the lower reliance on rail for commuting.[38]

One might worry that the choice of thresholds drives the results. I show in Appendix B that findings are robust to sensible variations around the preferred thresholds. One might also argue against the discrete definition of treatment. I redo all regressions for Definition 1 of the control area using a first and second order polynomial in the distance to a high-profile station, instead of a dummy of close versus far.[39] For Definition 2 of the control area, I adapt the specification to a triple-difference regression, by interacting successively T_{ij}, τ_t, and $distance_{ij}$.[40] Finally, like Abadie and Dermisi (2008), I also run a specification in which I allow distance to matter linearly only in the high-risk area, but then become irrelevant once outside of this area (i.e., the impact becomes flat). While noisier, results from these exercises are qualitatively similar. Going forward, I retain the binary definition of treatment.[41]

3.3 Descriptive statistics and evidence

To study the impact of the London attacks on housing markets, I rely on *UK Land Registry* price paid data[42] which covers all residential transactions made at full market value in London and Manchester. This dataset records the price paid, the address with postcode, the date of transfer, the property type (detached, semi-detached, terraced or flats), its age (newly built or an older residential building), and the duration of ownership (freehold or leasehold).

The property postcode enables the study of the spatial incidence of the attacks on house prices. Using the *Ordnance Survey*[43] I link each postcode to the British national grid system. I approximate the distance between a house and a transit station with the Euclidean distance between their postcodes. Given the flat topography of both cities and the level of detail of UK

[36] *The Independent* October 2011 article.

[37] Nationwide is the world's largest building society and one of UK's largest mortgage providers.

[38] *The Guardian* August 2014 article on rail-proximity property premia in various UK cities.

[39] The *DiD* estimates for London (see Table B3 in Appendix B) point to a 1- to 2-year relative decline in house prices for houses closer to a main hub. However, this continuous model of treatment is sensitive to the maximum distance threshold defining the sample. Thresholds limit the echoing effect of treatment from neighboring main stations. As expected, estimates for Manchester (available upon request) are larger and more robust.

[40] $distance_{ij}$ is the distance between the postcode j of a house i and that of either the nearest high-profile station (for $T_{ij} = 1$) or nearest secondary station (for $T_{ij} = 0$). In Manchester, the farther a house is from a high-profile station (as opposed to a secondary station) the higher its sale price after the attacks. Prior parallel trends are notable. Results for London are inconclusive, which I attribute to the strain that a continuous analysis puts on the data, particularly in London where the impact is short-lived. See Table B1 in Appendix B for details.

[41] A continuous analysis seems unsuitable for the data. Assignment to treatment is imperfect, as one does not know the actual travel behavior of residents of each house to "allocate" them to their relevant station. Also, it is unclear how the impact should vary with distance within the close versus far categories, particularly if what is feared is the exposure to the station as opposed to the house being damaged by an explosion.

[42] The UK Land Registry is the government department that registers the ownership of land and property.

[43] The Ordnance Survey is the national mapping agency for Great Britain.

postcodes, this measure of distance is a reasonable proxy for the actual walking distance.[44]

Table 1 describes both the London and Manchester samples along the three house characteristics reported in the UK Land Registry. This table serves three purposes. First, one can compare the treated and control samples of each city. I test whether differences in treated and control sample traits are statistically significant and find that the difference is only significant for age. This finding favors the empirical approach.

Table 1: Sample Descriptive Statistics for London and Manchester

		Treated		Control Def.1		Control Def.2	
London Sample		Before	After	Before	After	Before	After
Ownership (%)	Freehold	7.40	7.80	8.30	7.30	10.87	10.79
	Leasehold	92.60	92.20	91.70	92.70	89.03	89.21
Age (%)	Old	86.87	88.36	89.80	86.20	89.14	88.41
	New	13.13	11.64	10.20	13.80	10.86	11.59
Type (%)	Detached	0.23	0.26	0.20	0.20	0.35	0.21
	Flat	89.62	91.13	89.60	91.90	87.69	88.68
	Semi-Detached	0.39	0.30	0.60	0.40	0.92	0.75
	Terraced	9.76	8.31	9.50	7.50	11.04	10.36
		Treated		Control Def.1		Control Def.2	
Manchester Sample		Before	After	Before	After	Before	After
Ownership (%)	Freehold	0.41	0.25	1.28	1.09	0.88	0.71
	Leasehold	99.59	99.75	98.66	98.91	99.12	99.29
Age (%)	Old	48.37	43.04	39.45	39.53	39.16	44.47
	New	51.63	56.96	60.55	60.47	60.84	55.53
Type (%)	Detached	0.30	0.23	0.78	0.34	0.13	0.09
	Flat	98.55	98.96	92.56	96.10	97.37	97.56
	Semi-Detached	0.50	0.39	2.47	1.37	1.50	1.20
	Terraced	0.65	0.42	4.19	2.19	1.00	1.16

Notes: The table shows the proportion of sample houses in London and Manchester by category before and after the London bombings. The definitions for treatment and control are those described in Section 3.2. "Before" points to houses sold between July 2001 - July 2005, "After" refers to those sold between July 2005 and July 2009.

Second, city samples can be contrasted against each other. Leasehold ownership is strikingly common in both cities, but even more so in Manchester where it covers 99 percent of ownership. Properties in London are predominantly old, whereas in Manchester newer homes are more common. This discrepancy suggests that Manchester is likely to have a less rigid housing supply. While flats are the dominant type of housing across cities, they are more frequent in Manchester where they reach 96 to 98 percent of sales.

We can test whether there are shifts in the composition of house sales after the London bombings. For both cities, the age dimension is the only one experiencing a statistically significant change in composition. This result is not problematic, however, because I control for house characteristics in all specifications. Moreover, we learn that the age of the property explains little, if anything, of its price.

[44] The Euclidean distance computed using postcode coordinates seems to underestimate the walking distance by 6 to 8 percent. The 600- and 1,200-meter Euclidean distance thresholds of London translate into 650- and 1,300-meter thresholds for walking distances. Section 3.5 shows that results are robust to threshold variations.

Trends in the number of transactions and raw prices are insightful as well. The upper panels of Figure B3 in Appendix B show the total number of houses sold from July 2001 to July 2009 in both cities. I compare these trends to the national trends documented in Carozzi (2017). Before the 2005 attacks, the number of transactions in treated and control areas moved in tandem with national peaks and troughs. Figure 2 in Carozzi (2017) depicts a sizable fall in transactions in 2005, which is equally felt by treated and control areas. We can then observe a nationwide intensification in trading from 2005 to 2007. However, only control areas exhibit the same steep increase in transactions over this period. Treated areas do not pick up the full excitement of the housing bubble. From 2007 onwards, treated and control areas alike experience the same downturn of the housing bubble.

The lower panels of Figure B3 depict the time series of raw median prices in the two cities between 2001 and 2009. I again compare these series with the national price index constructed by Carozzi (2017). These graphs support the main *DiD* findings of the paper. For instance, in London, while median prices exhibit parallel trends prior to the attacks, control groups exhibit a higher median price growth than the treated group after the attacks. After three years, trends converge again or even reverse. These findings are in line with a positive correlation between house prices and trading volumes (Stein, 1995): relative drops (increases) in house prices are accompanied by relative drops (increases) in trading volume. The London attacks seem to have made buyers less interested in properties close to the major urban rail hubs.

3.4 Findings from the real estate market

Figure 2 plots the difference-in-differences coefficients from Equation 1, together with their 95 percent confidence bands, for both cities and definitions of the control area. Prior to the attacks, house prices in treatment and control areas follow parallel trends, which supports the choice of a *DiD* strategy. After the London bombings, trends diverge. London experiences a relative fall in house prices of about 6 percent, whereas Manchester seems to face a more striking fall of up to 14 percent. London is more resilient: whereas its house price gap closes after one year (two years if we allow for a p-value of 0.068 for the second year), the recovery takes three to four years in Manchester.

This heterogeneity in the magnitude and persistence of results is surprising at first. While I cannot discriminate among potential explanations, the following are worth noting. First, the two cities differ in relevant ways. The higher demand for urban rail in London, added to the city's legendary housing shortage, may have dampened the effects of the bombings on London. Also, as we learned from the *British Social Attitudes'* surveys, residents of Manchester felt more at risk of terrorism than Londoners. Second, a day before the attacks, Londoners received the news that they were granted the Olympic Bid of 2012. I argue in Section 5 that winning this bid may explain London's resilience to the attacks. Last, because the rail network of Manchester is sparser, I can achieve a "cleaner" spatial definition of treated and control areas. These reasons are likely to justify the differential impacts of the London attacks on the two cities.

Table 2 reports the estimates of all coefficients from the *DiD* regression of Equation 1. In addition to the findings from Figure 2, we confirm that house characteristics have predictable effects on house prices. Relative to "detached" houses, flats are between 24 (Manchester) and 50 (London) percent cheaper. Semi-detached houses command a 15 to 20 percent discount. Terraced homes also require a discount of 9 to 18 percent. Homes in new buildings are 4.5 percent more expensive in Manchester, but indistinguishably priced in London. Last, in London, leaseheld homes receive an approximate 40 percent discount. The estimates for Manchester are noisier, as they are based on a too small a sample of freehold houses.

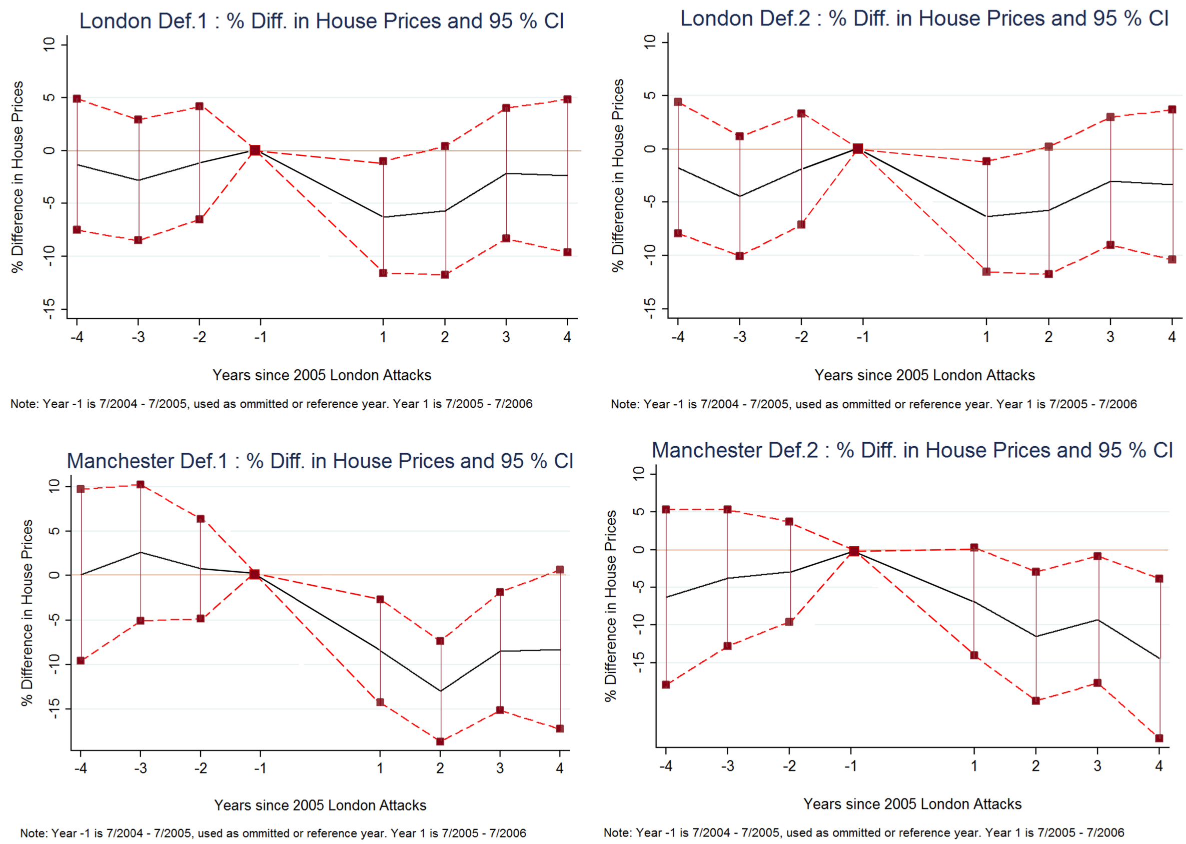

Figure 2: London and Manchester: *DiD* impact of the London attacks on house prices

Note: These graphs illustrate the point estimates and 95% confidence bands on the interaction terms in Eq. 1. Table 2 reports the full *DiD* results. The top-left graph shows the results for London using Def.1 for the control area (Column 1 in Table 2), the top-right graph for Def.2 (Column 2 in Table 2). The bottom-left graph corresponds to the Def.1 exercise for Manchester (Column 3 in Table 2), the bottom-right graph to Def.2 (Column 4 in Table 2). "Year -1" (7/2004-7/2005) contains the 12 months just prior to the July 2005 attacks and is taken as the omitted year. "Year 1" (7/2005-7/2006) contains the 12 months immediately after the attacks. Given the ability to split the house sale data in years around 7/2005, there is no "Year 0."

The estimates of the year fixed effects prompt reflection on the argument of this paper. Since 2001, the UK has seen a dramatic increase in house prices, which was only partly undone during the bust from 2007 to 2009.[45] The message of this paper is *not* that the London attacks decreased the overall appeal of these cities. Instead, this paper documents the within-city spatial incidence of the London attacks, in particular, how they discriminated against housing closer to the busiest public transit stations.

[45] See Figure 2 of Carozzi (2017). The 2007 to 2009 trough was more severely felt by Manchester. The winning of the 2012 Olympics bid is likely to have mitigated the consequences of the housing crisis on London.

Table 2: London and Manchester: Main *DiD* results for the preferred specifications

Dependent variable: Log of house price	London Def.1	London Def.2	Manchester Def.1	Manchester Def.2
Interaction terms				
$T \times$ (4 years before 7/7)	-0.013	-0.017	0.001	-0.063
	(0.032)	(0.031)	(0.049)	(0.059)
$T \times$ (3 years before 7/7)	-0.028	-0.044	0.026	-0.037
	(0.029)	(0.029)	(0.039)	(0.046)
$T \times$ (2 years before 7/7)	-0.011	-0.019	0.008	-0.029
	(0.027)	(0.027)	(0.029)	(0.034)
$T \times$ (1 year after 7/7)	-0.063**	-0.063**	-0.085***	-0.069*
	(0.027)	(0.026)	(0.029)	(0.036)
$T \times$ (2 years after 7/7)	-0.057*	-0.058*	-0.130***	-0.115***
	(0.031)	(0.031)	(0.029)	(0.043)
$T \times$ (3 years after 7/7)	-0.021	-0.030	-0.085**	-0.093**
	(0.032)	(0.031)	(0.034)	(0.043)
$T \times$ (4 years after 7/7)	-0.024	-0.033	-0.083*	-0.144***
	(0.037)	(0.036)	(0.046)	(0.054)
House characteristics				
Flat	-0.507***	-0.491***	-0.264***	-0.239*
	(0.086)	(0.070)	(0.050)	(0.127)
Semi-detached	-0.203**	-0.149*	-0.185***	-0.204
	(0.099)	(0.080)	(0.044)	(0.133)
Terraced	-0.089	-0.131**	-0.187***	-0.182
	(0.081)	(0.062)	(0.048)	(0.127)
New	0.008	0.008	0.046***	0.045***
	(0.015)	(0.015)	(0.009)	(0.012)
Leasehold	-0.369***	-0.413***	-0.094	0.715***
	(0.041)	(0.039)	(0.093)	(0.270)
Year dummies				
4 years before 7/7	-0.159***	-0.153***	-0.338***	-0.275***
	(0.014)	(0.014)	(0.029)	(0.044)
3 years before 7/7	-0.084***	-0.067***	-0.234***	-0.170***
	(0.015)	(0.014)	(0.023)	(0.033)
2 years before 7/7	-0.069***	-0.062***	-0.118***	-0.083***
	(0.014)	(0.013)	(0.017)	(0.025)
1 year after 7/7	0.076***	0.076***	0.013	-0.003
	(0.014)	(0.013)	(0.016)	(0.027)
2 years after 7/7	0.251***	0.252***	0.082***	0.066*
	(0.014)	(0.013)	(0.020)	(0.038)
3 years after 7/7	0.387***	0.395***	0.085***	0.092**
	(0.016)	(0.014)	(0.024)	(0.036)
4 years after 7/7	0.327***	0.336***	-0.046	0.018
	(0.020)	(0.018)	(0.034)	(0.045)
Observations	22,044	21,354	18,094	11,570
Within R^2	0.235	0.262	0.098	0.073

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: All regressions include postcode and year FE. "2 years before 7/7" refers to 7/2003 - 7/2004. "1 year after 7/7" corresponds to 7/2005 - 7/2006. The omitted year is "1 year before 7/7" and refers to 7/2004 - 7/2005. As I can split the house sale data in years around 7/2005, there is no "Year with 7/7."

3.5 Robustness checks

In Appendix B, I demonstrate the robustness of my main findings to alternative sample definitions and specifications. First, in columns (2), (3), (5), and (6) of Table B4, I test the sensitivity of my *DiD* estimates to the choice of "walking impact zone" thresholds. Given that the preferred thresholds were chosen to avoid spatial "contamination" between treatment and control areas, while correctly defining the area of influence of each station, it is reassuring that the *DiD* estimates keep their sign, order of magnitude, and significance when I vary thresholds.

Next, I perform a falsification exercise to shed light on the role of distance from a major rail hub in influencing house prices after the London attacks. I focus on houses either treated or serving as control and re-shuffle their closeness status, T.[46] Table B5 shows that as long as houses keep their actual T status, houses near a major rail hub become less attractive after the London attacks. Once I mis-assign locations to houses, I no longer detect any impact of the bombings. This finding supports the causal interpretation of my *DiD* estimates.

Last, I corroborate the impact of the London bombings on real estate with an exercise on rentals in London. I use a proprietary dataset from LonRes, a subscription service for real estate agents and surveyors working in Central London. For properties rented after January 1, 2005, I observe the property type, the postcode, the number of bedrooms, the floor, the date on the market, the rental date, the initial rent asked, and the final rent agreed upon.[47]

Table B6 runs a quarterly version of the *DiD* specification in Equation 1. I first use the initial rent asked as outcome variable,[48] as the attacks did not affect this price during *Q1* of 2005 (the pre-trend quarter) nor did it in *Q2* (the reference quarter). Columns (1) and (2) report these results for the two definitions of the control group. During the quarter with the attacks (*Q3* of 2005), treated and control rentals' initial rents asked kept their parallel trends; in the next quarter, initial rents asked in treated areas fell by 8 to 9 percent. Given the use of year and quarter fixed effects, this fall is not a symptom of seasonal trends.[49]

We find this rent asked differential opening up again in *Q3* of 2006, when treated rentals

[46] I re-assign a "fake" value of T using random draws from a uniform distribution. Except this new closeness status, observations are unchanged. I keep the same number of observations falling within treatment and control, such that differences in significance are not driven by changing sample sizes.

[47] I can only count on 7 months of rental observations prior to the attacks. For properties already on the market the day of the bombings, their days on the market and final rent paid may have already taken a hit. In contrast, properties entering the market after July 7 can adjust on all four dimensions: the date of entry on the market, the initial rent asked, the time on the market, and the final rent paid.

[48] I also run this regression with the final rent paid as the outcome variable and find similarly sized, yet noisier, estimates. I choose not to emphasize these results for two reasons. First, the agent agreed to disclose the final rent paid for only 35 percent of rentals. The resulting sample is too small to be suitable for my *DiD* strategy and potentially non-representative. Second, *Q2* of 2005 is already a treated quarter for the final rent paid, as many of the rentals on the market in *Q2* find a client after the bombings.

[49] Otherwise, we would have found a similar fall in rent asked for *Q4* of 2006 or *Q4* of 2007.

got relatively cheaper by 8 to 10 percent. The UK housing market faces seasonable booms and busts, which lead to significantly higher sales activity and house prices during *Q2* and *Q3* of each year (Ngai and Tenreyro, 2014). Rental markets display an even starker seasonal pattern, with 50% more transactions in the third quarter of any year than in the first quarter of the same (Bracke, 2015). While overall LonRes rentals display these same seasonal patterns, during the Fall of 2006, treated rentals did not gather momentum to the same degree as control rentals.

Next, columns (3) and (4) report the impact of the attacks on the share of rentals whose days on the market (DOM) are more than the median of days on the market over the entire sample. We observe that properties put on the market the quarter before the attacks experienced a peak in the age of their rental listing, signaling that the rental market was at a standstill in the wake of the bombings, taking longer to "release" the rentals which were freshly advertised at the time of the attacks. As soon as properties in treated areas were listed at relatively lower initial rents asked, reflecting their relative loss of appeal, the time on the market for rentals in treated areas shrunk.[50] Combined, these pieces of evidence suggest that the real estate market of London was indeed shaken by the attacks for about a year.

4 Terrorism lowers the value of proximity to public transportation: Evidence from businesses

The London attacks affected not only the housing market, but firms as well. A year after 7/7, the threat of terrorism worried business owners, 65% of whom saw another terrorist attack as inevitable.[51] Firms in cities at risk of terrorism usually incur a "terror tax" through greater security costs, insurance premiums, and the emotional toll on workers (Harrigan and Martin, 2002). Since 2006, business leaders have also been ranking damages from terrorism among the largest threats to costs and revenues,[52] above supply chain disruptions and negative publicity.

Can the location of a business have mattered for its performance after the attacks? Businesses in sectors such as recreation and retail most likely saw their demand fall if close to potential targets of terror attacks. A Harrods' spokesman declared that after the London attacks "more than ever before [...] the shopping environment, customer service and the theatre of retail will all be of paramount importance in building [customer] confidence" (London Chamber of Commerce and Industry, 2005). Business practices also adjusted to minimize the exposure to

[50] Using a sample of rentals in Central London from a different real estate agency, Bracke (2015) also finds a positive correlation between the number of days a property is waiting for a new tenant and its rent.

[51] The same *London Chamber of Commerce and Industry* 2006 survey also shows that 41% of firms had a contingency plan for terrorism, 40% of which updated their plan after the 2005 London attacks.

[52] *The Business Continuity Management* surveys.

high-risk areas, eroding the benefits of agglomeration economies. "33 percent of firms have [...] changed the way they conduct business meetings and travel, favoring e-mail and conferencing over traditional face-to-face meetings" (London Chamber of Commerce and Industry, 2005).

These findings suggest that one can measure the impact of the London attacks on firms by defining treated and control areas the same as for houses. One difference is that performance of firms in treated and control areas might be interrelated, for instance through input-output linkages. While these ripple effects across the economy may bias the treatment effects towards zero, I still find evidence that distance to main rail hubs has mattered for businesses too.

I compare location choices of new firms entering the business registry from *UK Companies House*.[53] Figure C4 in Appendix C shows that, in London, treated locations lose ground to control locations in the years following the attacks. The comeback of treated locations is only achieved in four years. In absolute terms, the location choices of new entrants in Manchester appear less imbalanced between treatment and control areas. However, when reported in relative terms, the fall in firm entry rates in treated areas is similar between the two cities.[54]

This relative decline in firm entry after the attacks hides a great amount of heterogeneity across sectors. In Figure 3, I plot the number of new firm entries in treated and control areas by sector. I focus on sectors with a large enough sample size as to provide credible insights. Businesses such as restaurants, hair salons, or chiropractic clinics are those for which treated areas seem to lose most appeal after the London attacks. If foot traffic is central to one's business, then locating in an area at risk of terrorism seems unwise in times of high anxiety over terrorism and subsequent low foot traffic. Meanwhile, sectors whose business can occur at distance (such as head offices or motion picture production) are less likely to display differential trends in location choices.

To document intensive margin reactions, I turn to B*ureau Van Dijk's Amadeus* database and extract company financials for both cities from 2001 to 2009.[55] Estimates from variants of Equation 1 are reported in Table C7 in Appendix C. The *DiD* equation is altered in three ways: (i) the dependent variable is either the firm-level EBIT margin, the log of fixed assets, the liquidity ratio, or the credit period (in days), (ii) as the dataset has a panel structure, I use

[53] This dataset contains all companies alive in the UK on May 31, 2016, when the data was downloaded. Both the *Companies House* dataset and *Bureau Van Dijk's Amadeus* are consolidated at the firm level. For multi-establishment firms, the address reported is likely that of the headquarters. As location decisions are taken at the establishment level, this approach is imprecise for multi-establishment firms.

[54] What % bigger is the difference in a given year with respect to that in the year before the attacks? Relative measures address the fact that London hosts 10 times more firms than Manchester and entry rates are as uneven.

[55] I restrict the sample to an unbalanced panel of firms reporting at least the years before, during, and after the bombings. There is great heterogeneity across variables in terms of the extent to which they are filled in, thus samples vary greatly across variables. *Amadeus* under-represents small businesses, which are likely to be those most vulnerable to the attacks.

firm fixed effects, and (iii) time blocks are calendar years, the reference year becoming 2004.[56] Odd-numbered columns refer to the London samples, even-numbered to Manchester's.

Columns (1) and (2) show that in both cities the EBIT margin[57] of treated firms experienced a relative fall of around 7 percent in 2006 compared to 2004. Columns (3) and (4) present a sizable decline in fixed assets in both cities. Similar to the case of house prices, this decline only lasts for one year in London, but persists for four years in Manchester. Column (5) shows that in 2006 treated firms in London lost an average of 2 percent in their liquidity ratio. Columns (7) and (8) suggest that businesses in higher risk areas may have also taken measures favorable to customers, such as raising the number of days a customer can wait before paying an invoice.

Finally, I zoom in at the 2-digit 2007 NAICS level to uncover heterogeneous effects on incumbent firm performance. Figure 4 plots the evolution of the average gross margin of treated and control firms in London for various sectors.[58] Firms in sectors such as management, administrative services or transportation seem to be equally hurt by the attacks in treated and control areas. As these sectors rely less on in-person clients, it is likely that they lived the shock of the attacks in a less localized manner, through its ripple effects in the economy.

Businesses relying on foot traffic, such as beauty salons, offices of dentists or dry cleaning, are those for which closeness to a main rail hub seems most penalizing. While noisier due to smaller sample sizes, the equivalent plots for Manchester, reported in Figure 5,[59] point towards similar insights. In addition, we learn that firms in arts and recreation are also hurt more if operating in treated areas.

These findings on firms enrich those on housing in two ways. If we assume a fixed supply of commercial space and unaffected vacancy rates, we can infer that commercial rents closer to potential targets must have also taken a hit. Firms most sensitive to the threat of terror were found to be less likely to locate in treated areas.[60] As those same firms would have otherwise competed more for those accessible, high-turnover locations, one expects the equilibrium bid-rent to fall after the attacks. The deterioration in economic performance[61] of incumbent firms also signals that bid-rents in treated areas must have declined, to reflect their lower profitability.

[56] 2005 *DiD* estimates are expected to not be statistically different from those of 2004, as the attacks occurred in July of 2005 and many financial outcomes take time to unfold.

[57] The EBIT margin is the ratio of earnings before interest and taxes to net revenue.

[58] These graphs report the raw first moments. I only keep the 8 industries with more than 125 reporting firms. A more involved analysis would require larger sample sizes.

[59] Figure 5 plots the evolution of sectoral average EBIT margins in Manchester. While the gross margin had fewer missing values for firms in London, in Manchester it is the EBIT margin which was more frequently reported. I only retain industries with more than 40 reporting firms.

[60] Businesses such as restaurants or shops rely on foot traffic, which was likely reduced by the fear of more terror attacks.

[61] The terror attacks likely acted as both a direct inhibitor of demand and an indirect source of cost increases, by an erosion of scale economies, as in Rosenthal and Ross (2010).

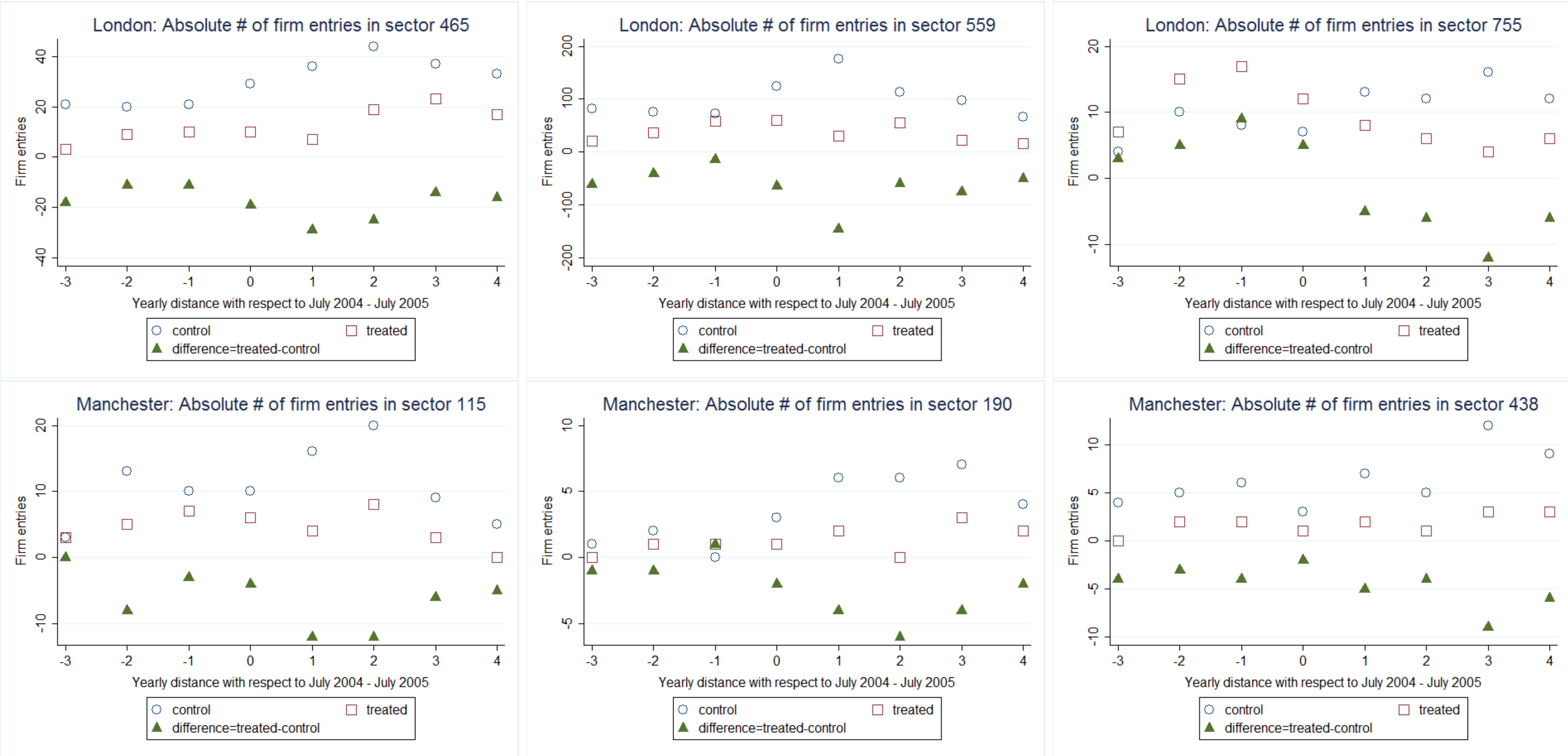

Figure 3: London and Manchester: Number of new firms in treated and control areas for a sample of sectors

Note: These graphs plot the raw number of new firms in treated (red squares) and control (blue circles) areas in London (in the three upper graphs) and Manchester (in the three lower graphs), together with the difference in the number of new firms between the treated and control areas (green triangles). The plotted values correspond to the actual number of firm entries, without any controls or fixed effects. I exemplify these trends for sectors with a large enough sample size to provide credible insights. The codes of the plotted sectors are: 115 (development of building projects), 190 (non-specialized wholesale trade), 438 (other human health activities, e.g., chiropractic or dietitian clinics), 465 (licensed restaurants, i.e., those licensed to sell alcoholic drinks), 559 (other letting and operating of own or leased real estate), and 755 (hairdressing and other beauty treatments). The pattern of relatively slower firm entry in treated areas is not general across sectors. Sectors for which clients need to physically frequent the business (e.g., restaurants or beauty parlors) are more likely to display this differential trend penalizing treated areas compared to sectors whose customers can be distant (e.g., activities of head offices, motion picture production, or production of electricity; not included here, but available upon request).

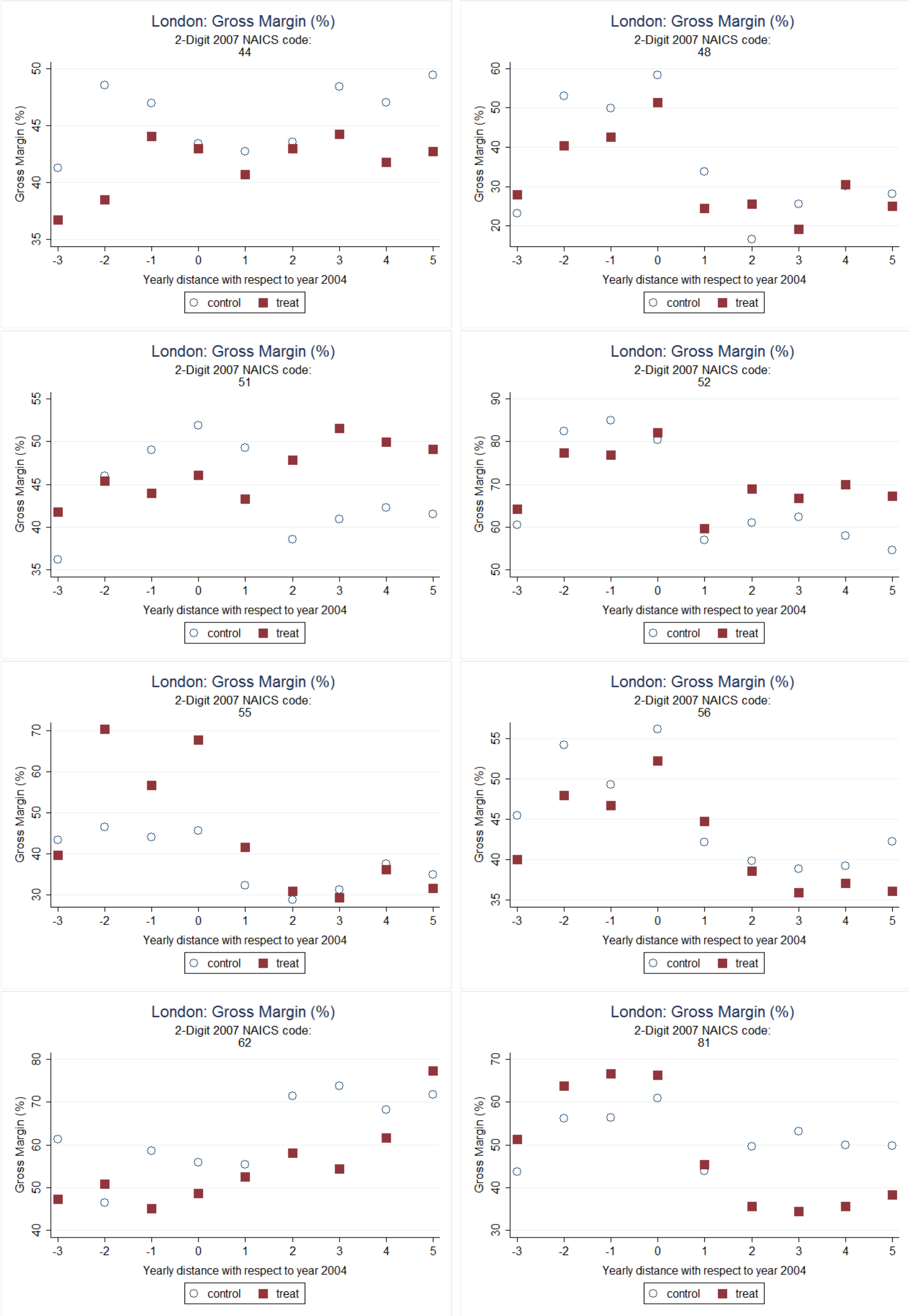

Figure 4: London: Impact of the London bombings on firms' gross margin (%) at NAICS 2-digit level

Note: These graphs plot the raw means across London firms located in treatment and control (Def.1) areas for each year of closure of their accounts. The plotted values correspond to the actual first moments, without any controls or fixed effects. The gross margin is the performance variable with the fewest missing values. I focus only on NAICS 2-digit codes with at least 125 observations per year - these are 44 (Retail Trade), 48 (Transportation), 51 (Information), 52 (Finance and Insurance), 55 (Management of Companies and Enterprises), 56 (Administrative and Support and Waste Management and Remediation Services), 62 (Health Care and Social Assistance), and 81 (Other Services).

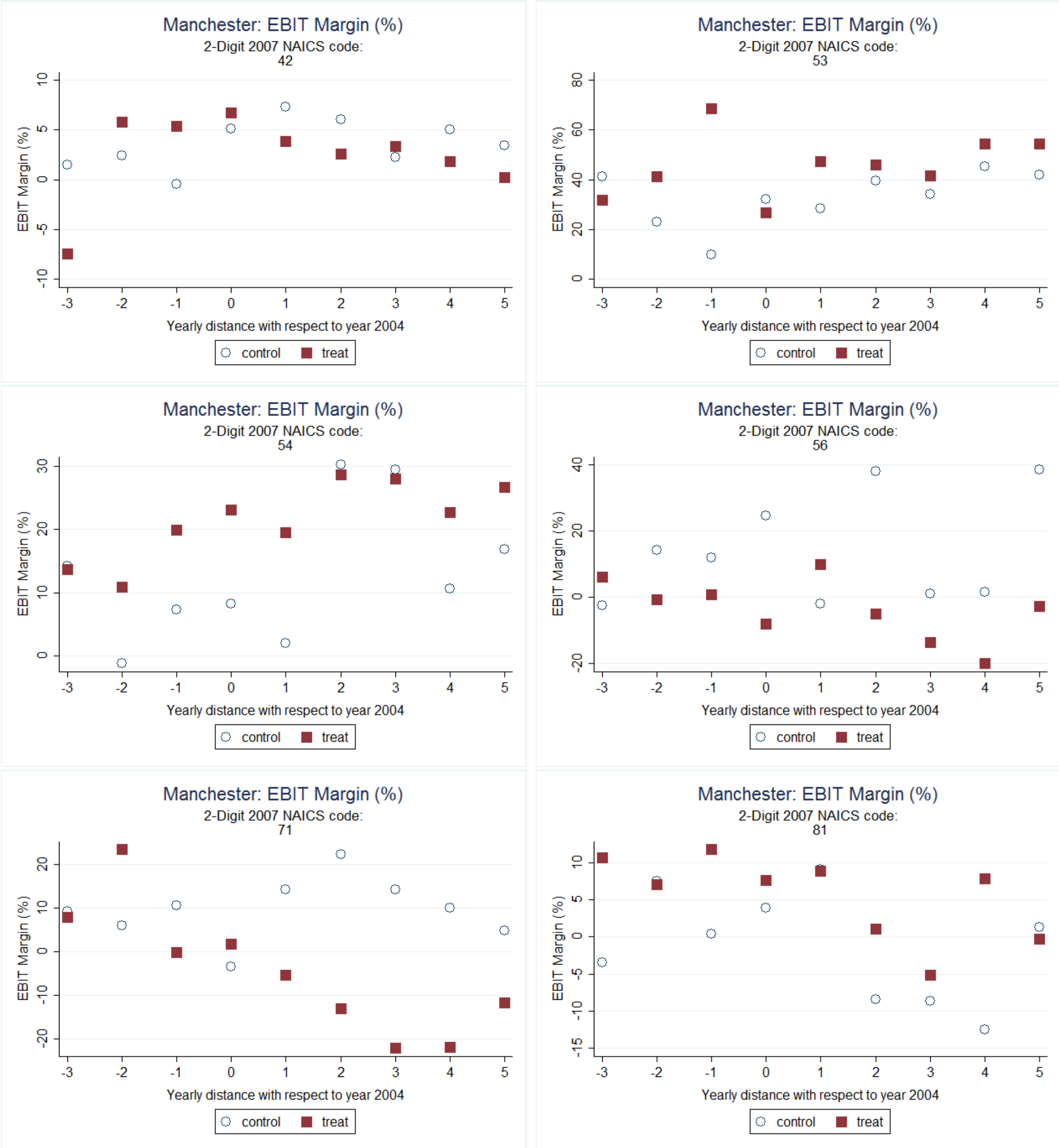

Figure 5: Manchester: Impact of the London bombings on firms' gross margin (%) at NAICS 2-digit level

Note: These graphs plot the raw means across Manchester firms located in treatment and control (Def.1) areas for each year of closure of their accounts. The plotted values correspond to the actual first moments, without any controls or fixed effects. The EBIT margin is the performance variable with the fewest missing values. I focus only on NAICS 2-digit codes with at least 40 observations per year - these are 42 (Wholesale Trade), 53 (Real Estate and Rental and Leasing), 54 (Professional, Scientific and Technical Services), 56 (Administrative and Support and Waste Management and Remediation Services), 71 (Arts, Entertainment and Recreation), and 81 (Other Services).

Moreover, Figures 3, 4, and 5 suggest that neighborhoods closer to major rail stations must have lost more diversity and vitality in their consumption options. This relative fall in the value of consumption amenities near major rail stations must have contributed to the relative fall in house prices found in Section 3.4.

5 Ruling out alternative hypotheses

The previous sections provide a range of evidence that the London bombings lowered the appeal of proximity to transportation hubs by raising the perceived risk of future terrorist targeting such hubs. In this final step, I rule out alternative explanations that could be proposed to explain the patterns in the data. There are four main competing hypotheses.

H1 : "We are measuring the impact of physical disruptions to public transit, not that of fear of repeated terrorism" - I claim that physical damage alone cannot explain my findings. As Manchester did not face any terrorist attack itself, I only defend this claim for London. There, the attacks damaged parts of the subway infrastructure, creating temporary bottlenecks on the attacked lines. However, users of the Tube were assured that the disruption was short-lived, and indeed traffic through the damaged stations was reestablished within a month. House prices are meant to capture not only current conditions; expectations of future states matter, too. To the extent that users did not update their perceptions of future risk from terrorism, house prices near main nodes should not have seen such a sizable fall.

To rule out that results are driven by station-specific measures, such as increased security slowing the flow of passengers, I exclude King's Cross from the treated sample. Columns (2) and (5) in Table D8 in Appendix D show that it is conservative to keep King's Cross in the main sample: excluding King's Cross increases the magnitude and significance of our coefficients of interest. To the extent that the most desirable targets of London become too well guarded, terrorists might settle for second best targets: slightly lower ranked transport hubs in London or even hubs in other cities, such as Manchester. Communities might predict such shifting tactics and overreact near what would be natural second-best targets. The larger impacts for London without King's Cross or for Manchester may be interpreted in this light.

Another check on whether results are driven by the direct damage alone is to discriminate between stations along bombed and unharmed lines. The sample of stations for this check contains only secondary stations in London, all similar in terms of ridership. If service inconvenience could explain my results, one would expect to find a fall in house prices near stations on bombed lines with respect to stations on unharmed lines. I take the difference between the "pre" and "post" house prices within 600 meters of a secondary station. I then difference the means across the treated stations situated on a bombed line ($\mathbb{1}_{\text{Station on bombed line}} = 1$) and comparison stations ($\mathbb{1}_{\text{Station on bombed line}} = 0$) to get the customary *DiD* estimate (see equation 2, where variables have the same meaning as in equation 1):

$$p_{ijt} = \alpha + \boldsymbol{\beta}'\boldsymbol{X_{ij}} + \mu_{j(i)} + \sum_{t=01/02, t\neq 04/05}^{08/09} \gamma_t \tau_t + \sum_{t=01/02, t\neq 04/05}^{08/09} \delta_t \tau_t \mathbb{1}_{\text{Station on bombed line}} + \varepsilon_{ijt} \quad (2)$$

Table D9 in Appendix D confirms the parallel trends of the treatment and control groups. Interestingly, whether the closest station belongs to a bombed line or not continues to not matter for the valuation of nearby houses after the London attacks. This suggests that it is the size of the Tube station that affects real estate behaviors, as opposed to whether the station belongs to a previously attacked line or not. This exercise also rules out the possibility that Londoners developed a trauma specifically associated to the attacked lines.

H2: "We are measuring the impact of heavier policing, and not that of the fear of repeated terrorism." - The London attacks triggered an over 30 percent increase in police activity in central London for the six weeks following the attacks, as part of a police deployment operation called "Theseus" (Draca et al., 2011). During this time crime fell significantly in Central relative to Outer London before bouncing back at the end of the operation. The police force of Manchester was on a heightened state of alert as well in the weeks following the London attacks,[62] though not to the same extent as in London.

Can heavier policing and the subsequent fall in crime, followed by the withdrawal of the additional policemen and recovery of crime levels (Draca et al., 2011) explain the results of this paper? This scenario is unlikely. If anything, the literature on crime and house prices would have predicted an increase in house prices when crime levels fell, followed by a decrease in house prices upon re-convergence to prior levels of crime.[63] In both cities, the fall in house prices near main transit nodes is found to be immediate and persist beyond the first weeks of heavier policing. It is thus reasonable to assume that results are not mere reactions of house prices to short-term changes in police activity.

H3: "The simultaneous announcement of London winning the 2012 Olympic bid is the real driver of the results." - On July 6, 2005, the International Olympic Committee awarded London the right to host the 2012 Olympic Games. The London attacks occurred the following morning. Can this tight sequence of events threaten the identification strategy of this paper? I find no evidence that this is a credible threat.

The media described the win against Paris as following a "cliffhanger vote."[64] While win-

[62]Chris Mulligan, from Greater Manchester Passenger Transport Authority, said "Metrolink, bus and rail operators were diverting extra staff to security duties" (*BBC* July 2005 article). The Transport Police was particularly vigilant to suspicious packages in main rail nodes, as shown by the evacuation of Piccadilly station (*The Guardian* July 2005 article). Picadilly is part of the sample of treated stations in Manchester.

[63]Here I refer to "street-level crimes such as thefts, violent assault and robbery," which are likely to be deterred by highly visible police deployment, as in Draca et al. (2011).

[64]*BBC* July 2005 article.

ning the bid certainly had an element of surprise, it also arrived after three consecutive failed attempts to win IOC's bidding contest. If London's efforts over the previous twenty years made winning seem inevitable and if winning was thought to have differential effects on houses based on their closeness to main rail hubs, then the *DiD* specification would have picked up differential pre-trends. The absence such pre-trends goes against anticipation effects.

Moreover, even if the victory did take Londoners by surprise, the findings of this paper would be hard to credit to the Olympics. PropertyInvesting.net, a major UK-based property news portal, created a map ahead of the Games to advise property investors on the parts of London where house prices would rocket. All property hotspots were in East London, where most investments were expected. Kavetsos (2012) studies the impact of the London Olympics announcement on property prices: his *DiD* results suggest the real estate "action" was concentrated in host boroughs alone, where properties appreciated by 3.3 percent. Neither the treated nor the control areas of this paper are in East London, the area of impact of the Olympic bid.[65] In addition, as most of the new construction was to intensify after 2008, any impact pre-2008 cannot be solely attributed to disruptions caused by construction sites from Olympic projects. This is particularly true for my area of study located in non-host boroughs.

Not only that the news of winning the 2012 Olympic bid cannot explain the results for London, but it is likely that it attenuated the shock of the attacks on London. While most investments in infrastructure targeted East London, measures to optimize overall rail traffic might have benefited more the busiest stations. In such a case, one would expect an appreciation of house prices near the busiest stations.[66] That house prices near rail hubs recovered after only one year in London could stem from the offset of the terrorist threat with an expectation of smoother traffic in the future. Also, leading up to the Olympics, more than £1 billion were spent to secure London and its sports venues. This disproportionate care for security in London might have led to a migration of terrorism to a second preferred target, Manchester. Results for Manchester suggest that the Olympics may have played a role in dampening the impact of the attacks on London, as opposed to driving them.

H4: "Results capture unrelated trends in the real estate market, not the impact of the London attacks." - One might still worry that my *DiD* strategy incidentally captures trends unrelated to the London attacks. One way to address this doubt is to empha-

[65] Liverpool Street did not make it into the London sample of major rail hubs, as it had a significantly lower ridership in 2004 than King's Cross, Victoria, Waterloo, and Oxford Circus. It was also excluded to isolate the impact of the London attacks from the Olympic announcement. If I add Liverpool Street to the sample of treated stations, as in Columns (3) and (6) of Table D8 in Appendix D, the *DiD* estimates only fall by 1 percent. This fall is intuitive given the appreciation in house prices in East London.

[66] Year fixed effects should already address network-wide effects.

size the importance of the timing of the London attacks in explaining the differential trends in house prices. I thus falsify the date of the terrorist attack by shifting the year back and forth around 2005. Table D10 in Appendix D reports the results for the year of the attacks being erroneously set as 2004. In both cities and for the year after the "fake" attacks, I do not find evidence suggestive of different house price behaviors based on the distance to main rail hubs.

Another potential concern is that the spatial variation used to identify the impact of the attacks may be correlated with shocks hitting the real estate market during the financial crisis. In particular, the persistent impact on Manchester may be explained not by a lingering impact of the London attacks but by an unfortunate coincidence: the existence of a specific type of properties disproportionately hit by the housing slump and such properties being more frequent among sales in treated areas. The distinguishing features of these properties must be unobservable, as we had already confirmed that there were no statistically significant differences in the before and after composition of sales in the two cities along our observed characteristics.

I apply the falsification strategy described above for both the year before (2006 - 2007) and during (2007 - 2008) the burst of the UK housing bubble and find that the housing crash did not seem to discriminate against properties near urban rail hubs.[67] This finding, together with the robustness of *DiD* estimates to the use of two control groups and varying thresholds, suggests that it is unlikely that an *unobservable* composition of sales during the financial crisis is driving the Manchester results.

For completeness, I searched through news articles to identify those properties described as having been most hit during the housing crash of Manchester. New-build apartments emerge as the main victims.[68] While I cannot exclude flats from the *DiD* regression, as they represent 98 percent of the Manchester sample, I can drop all new-built properties and still count on 40 percent of observations. The resulting *DiD* coefficients are strikingly similar to those from the full sample, suggesting that the sustained loss of the appeal of houses near main rail hubs *cannot* be explained by the impact of the crisis on new-built properties. To conclude, all evidence points to the London attacks as the cause of the impacts found in this paper.

[67] I omit in turns 2006 - 2007 (to check if there is an extra gap opening up in treated areas in subsequent years), then 2007 - 2008 in *DiD* regressions based off Eq.1. For both 2007 - 2008 and 2008 - 2009, the gap in house prices between treated and control areas is not significantly different to that in the omitted year.

[68] E.g., "negative equity, now blighting new-build apartments" (*The Guardian* February 2008 article) or "the price of new-build flats set to crash" (*The Telegraph* February 2008 article).

6 Conclusion

In 2015, UK Prime Minister David Cameron noted that "ten years on from the 7/7 London attacks, the threat from terrorism continues to be as real as it is deadly." While concerns about terrorism have now become pervasive around the globe, evidence on the impact of terrorism, or the fear thereof, on cities is still surprisingly scarce.

I use the 2005 attacks on the London Tube to provide causal evidence of the negative impact of terrorism on the value of proximity to public transport. I study both London and Manchester, as to prove the contagious nature of the fear of terrorism. The attacks are found to have led to a 6 percent fall in house prices near the main rail hubs of London and up to a 14 percent fall in Manchester. These effects persisted for one year in London and for up to four years in Manchester. I also show that new firms were less likely to locate near major stations for up to four years, particularly firms relying on foot traffic. Among incumbent firms, those most hurt by the bombings were those for which consumption is local in nature, confirming that the attacks led to a sense of unease over time spent near public transit hubs.

This paper makes a first empirical inquiry into what Glaeser and Shapiro (2002) describe as the ambiguous relationship between the threat of terrorism and urbanization. Since transport hubs are attractive targets of terror, in times of increased terror risk their surroundings should become less appealing places in which to live and conduct business. But the rise in transport costs from attacks on transport networks might instead make dense areas more valuable – particularly those near well-connected transport stations.

My evidence of relative declines in housing prices and incumbent firms' performance suggests that the fear of terror attacks can dominate the benefits of vibrant, high-accessibility areas such as those near transport hubs. Moreover, the finding that fewer firms located near transport hubs after the London attacks shows that terrorist acts can affect clustering in cities and potentially distort agglomeration forces.

Future research should be more explicit on the contribution of transport costs – which are unobserved in this paper – to the impact of terror attacks against major transport stations. To achieve that, one must have access to the before and after travel choices of individuals with known home and work locations. Given the increasing availability of this type of data, more recent terror attacks against transit networks should make this progress possible.

7 Acknowledgements

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Appendix

A Background

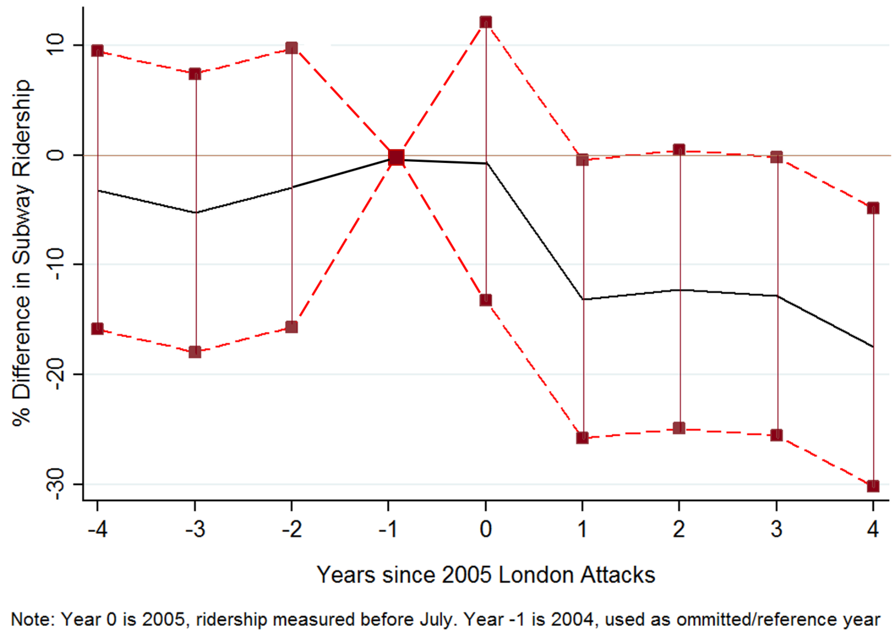

Figure A1: London: % Difference in subway ridership between main and secondary stations.

Note: This graph plots the *DiD* coefficients of a *DiD* regression comparing the ridership of the four major rail hubs of London with that of neighboring secondary stations, before and after the attacks with respect to the omitted year 2004 (Year -1). Given that the London attacks occurred in July of 2005, their impact is not fully captured in the 2005 (Year 0) annual ridership data. The sample of major and secondary stations coincides with that used in the main analysis. Year and station fixed effects address the standard concerns.

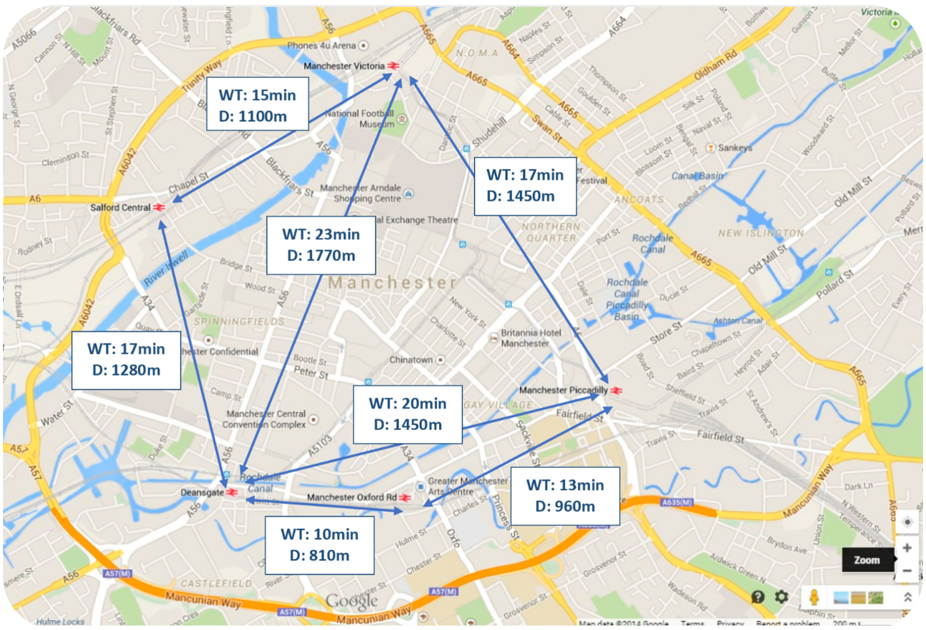

Figure A2: Manchester: Zoom on City Center: Walking Time (WT) in minutes and Distance (D) in meters between Rail and Metrolink stations under study

B Terrorism lowers the value of proximity to public transportation: Evidence from the real estate market

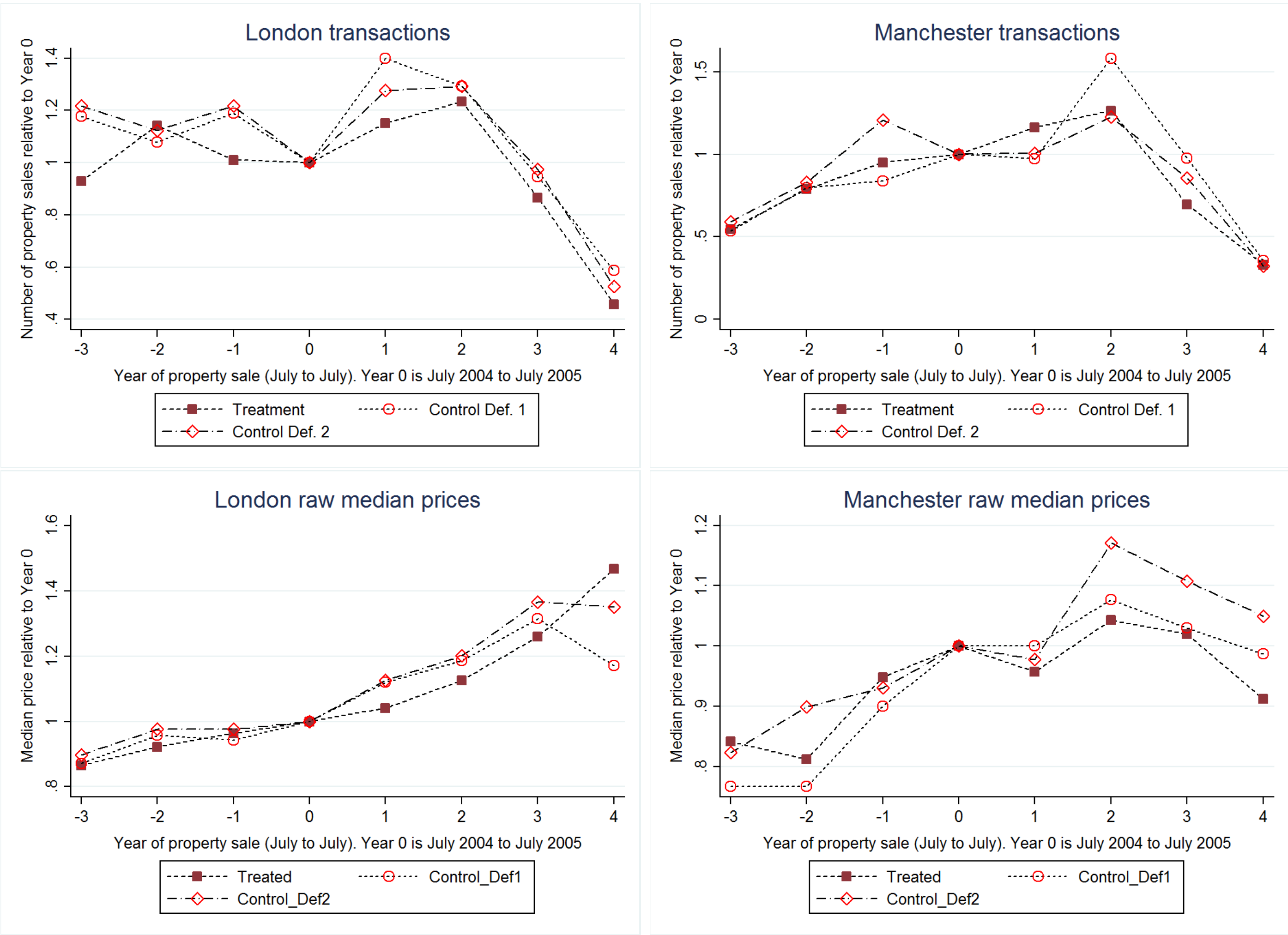

Figure B3: London and Manchester: 12-month relative number of transactions (upper graphs) and median price of sale (lower graphs) by treatment group

Note: The total yearly number of transactions and raw yearly median price of sale for a given treatment category have been divided by their corresponding values from July 2004 - July 2005, labeled as year 0. For reference on the absolute values in year 0, there were 632 property sales in the treated area, 1,974 sales in control area Def.1, and 1,904 sales in control area Def.2. Given the 4 treated stations and 13 Def.2 control stations of London and the corresponding surface areas of treatment and control, I find $\simeq 0.58$ property sales per acre and per year in all treatment groups. Thus, transaction rates in London were similar across treatment groups in the year prior to the attacks. In Manchester, in year 0, there were 1,027 properties sold in the treated area, 1,582 in control Def.1, and 659 in control Def.2. Manchester hosts 3 treated stations and 2 control Def.2 stations. As in London, transaction rates were similar across treatment areas prior to the attacks, at $\simeq 1.7$ property sales per acre and per year. The median price is computed over all properties sold between the months of July of two consecutive years and is divided by the median price for each relevant treatment category in year 0. This median is raw in the sense that it does not account for compositional changes across years and does not account for year fixed effects. For a discussion of these graphs, please consult the end of Section 3.3.

Table B1: London and Manchester: *DDD* regression coefficients for a continuous measure of distance from the relevant station. Def.2 of the control area

Dependent variable log(house price)	(1) London Def.2	(2) Manchester Def. 2
$T \times$ (4 years before 7/7)	0.036	-0.164
	(0.102)	(0.153)
$T \times$ (3 years before 7/7)	0.110	-0.181
	(0.089)	(0.116)
$T \times$ (2 years before 7/7)	0.013	-0.068
	(0.087)	(0.091)
$T \times$ (1 year after 7/7)	0.009	-0.275***
	(0.080)	(0.104)
$T \times$ (2 years after 7/7)	0.094	-0.344***
	(0.112)	(0.116)
$T \times$ (3 years after 7/7)	0.094	-0.380***
	(0.107)	(0.116)
$T \times$ (4 years after 7/7)	-0.002	-0.404**
	(0.120)	(0.162)
$T \times$ *distance* $\times$ (4 years before 7/7)	-0.012	0.043
	(0.023)	(0.049)
$T \times$ *distance* $\times$ (3 years before 7/7)	-0.036*	0.051
	(0.020)	(0.036)
$T \times$ *distance* $\times$ (2 years before 7/7)	-0.007	0.014
	(0.020)	(0.027)
$T \times$ *distance* $\times$ (1 year after 7/7)	-0.017	0.057*
	(0.019)	(0.031)
$T \times$ *distance* $\times$ (2 years after 7/7)	-0.035	0.071**
	(0.025)	(0.033)
$T \times$ *distance* $\times$ (3 years after 7/7)	-0.029	0.087**
	(0.024)	(0.035)
$T \times$ *distance* $\times$ (4 years after 7/7)	-0.007	0.078
	(0.026)	(0.048)
Observations	21,354	11,570
Within R^2	0.263	0.077
Postcode FE	YES	YES
Year FE	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This Table includes only the *DDD* regression coefficients on $T_{ij} \times \tau_t$ and $T_{ij} \times distance_{ij} \times \tau_t$. $distance_{ij}$ is equal to the distance in multiples of 100 meters between the postcode j of a house i and that of either the nearest main rail hub (for $T_{ij} = 1$) or the nearest secondary station (for $T_{ij} = 0$). The samples used here are those employing the Def.2 of treatment and control ($T_{ij} = 1$ if within 600m (500m) of a main hub and $T_{ij} = 0$ if within 600m (500m) of a secondary station). This table answers the question: "Does being located 100 meters farther from a *main rail station* increase the price of a house more than it does when 100 meters farther from a nearby *secondary rail station*, *more* in a given year compared to the year prior to the bombings? The impact on London is not robust to a linear parametrization of treatment.

Table B3: London: *DiD* regression coefficients for a continuous measure of distance from the relevant station. Def.1 logic for the "control" area

Dependent variable Log of house price	(1) Q1700m	(2) Q1800m	(3) Q1900m	(4) L900m	(5) L1000m	(6) L1100m
Linear interaction terms						
distance × (4 years before 7/7)	0.004	0.002	0.005	-0.006	-0.004	-0.001
	(0.013)	(0.012)	(0.011)	(0.009)	(0.007)	(0.006)
distance × (3 years before 7/7)	0.005	-0.000	-0.001	-0.000	0.002	0.002
	(0.012)	(0.011)	(0.010)	(0.008)	(0.006)	(0.005)
distance × (2 years before 7/7)	0.005	-0.001	-0.002	0.001	0.001	0.001
	(0.011)	(0.010)	(0.009)	(0.007)	(0.006)	(0.005)
distance × (1 year after 7/7)	0.020*	0.018*	0.016*	0.011	0.012**	0.009*
	(0.011)	(0.010)	(0.009)	(0.007)	(0.006)	(0.005)
distance × (2 years after 7/7)	0.021*	0.021*	0.017	0.004	0.006	0.004
	(0.013)	(0.012)	(0.011)	(0.009)	(0.007)	(0.006)
distance × (3 years after 7/7)	0.005	0.007	0.007	0.002	-0.001	-0.002
	(0.013)	(0.012)	(0.011)	(0.009)	(0.007)	(0.006)
distance × (4 years after 7/7)	0.003	0.005	0.004	-0.006	-0.004	-0.000
	(0.016)	(0.015)	(0.014)	(0.011)	(0.009)	(0.007)
Quadratic interaction terms						
$distance^2$ × (4 years before 7/7)	-0.000	-0.000	-0.000			
	(0.001)	(0.001)	(0.000)			
$distance^2$ × (3 years before 7/7)	-0.000	-0.000	0.000			
	(0.001)	(0.000)	(0.000)			
$distance^2$ × (2 years before 7/7)	-0.000	0.000	0.000			
	(0.001)	(0.000)	(0.000)			
$distance^2$ × (1 year after 7/7)	-0.001	-0.001*	-0.001			
	(0.001)	(0.000)	(0.000)			
$distance^2$ × (2 years after 7/7)	-0.001*	-0.001**	-0.001**			
	(0.001)	(0.001)	(0.000)			
$distance^2$ × (3 years after 7/7)	-0.000	-0.000	-0.000			
	(0.001)	(0.001)	(0.000)			
$distance^2$ × (4 years after 7/7)	-0.000	-0.000	-0.000			
	(0.001)	(0.001)	(0.001)			
Observations	40,014	44,112	46,947	11,630	15,155	18,430
Within R^2	0.253	0.251	0.253	0.242	0.244	0.242
Postcode FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This Table only includes the interaction terms on $distance_{ij} \times \tau_t$ and $distance^2_{ij} \times \tau_t$. $distance_{ij}$ is equal to the distance in multiples of 100 meters between the postcode j of a house i and that of the nearest main rail hub. This exercise preserves the Def.1 control area logic in the sense that one expects houses farther from a main rail hub to become relatively more attractive after the attacks than houses closer to the same. This table answers the question: "Does being located an extra 100 meters farther from a *main rail station* increase the price of a house *more* in a given year compared to the year prior to the bombings?" This exercise reports only on the London estimates, as the London results appeared more sensitive to a continuous parametrization of treatment in Table B1. Estimates for Manchester for this exercise are larger and more robust. Columns (1) to (3) show the results for 3 distance thresholds under which to keep observations for a quadratic parametrization of the continuous treatment. Columns (4) to (6) show results for 3 distance thresholds for the linear specification. Thresholds limit the echoing effect of treatment from neighboring main stations.

Table B4: London and Manchester: *DiD* regression coefficients for varying distance thresholds

Dependent variable: log(house price)	(1)	(2)	(3)	(4)	(5)	(6)
Interaction terms						
London	Def.1 Main	Def.1 500/1000m	Def.1 700/1400m	Def.2 Main	Def.2 500m	Def.2 700m
T × (4 years before 7/7)	-0.013	0.006	0.011	-0.017	-0.018	-0.008
	(0.032)	(0.039)	(0.027)	(0.031)	(0.039)	(0.027)
T × (3 years before 7/7)	-0.028	-0.013	-0.021	-0.044	-0.030	-0.044*
	(0.029)	(0.036)	(0.026)	(0.029)	(0.035)	(0.026)
T × (2 years before 7/7)	-0.011	-0.006	0.014	-0.019	-0.008	-0.001
	(0.027)	(0.032)	(0.024)	(0.027)	(0.031)	(0.024)
T × (1 year after 7/7)	-0.063**	-0.060*	-0.042*	-0.063**	-0.061**	-0.051**
	(0.027)	(0.031)	(0.024)	(0.026)	(0.031)	(0.024)
T × (2 years after 7/7)	-0.057*	-0.014	-0.032	-0.058*	-0.030	-0.037
	(0.031)	(0.037)	(0.026)	(0.031)	(0.037)	(0.026)
T × (3 years after 7/7)	-0.021	-0.025	-0.001	-0.030	-0.027	-0.008
	(0.032)	(0.038)	(0.028)	(0.031)	(0.037)	(0.027)
T × (4 years after 7/7)	-0.024	0.006	0.020	-0.033	-0.008	0.008
	(0.037)	(0.045)	(0.033)	(0.036)	(0.044)	(0.033)
Observations	22,044	15,155	28,348	21,354	16,146	26,751
Within R^2	0.235	0.244	0.241	0.262	0.262	0.261
Manchester	Def.1 Main	Def.1 400/800m	Def.1 600/1200m	Def.2 Main	Def.2 400m	Def.2 600m
T × (4 years before 7/7)	0.001	-0.068	0.100**	-0.063	-0.108*	-0.044
	(0.049)	(0.059)	(0.046)	(0.059)	(0.065)	(0.055)
T × (3 years before 7/7)	0.026	0.004	0.113***	-0.037	-0.050	-0.001
	(0.039)	(0.050)	(0.034)	(0.046)	(0.055)	(0.044)
T × (2 years before 7/7)	0.008	-0.008	0.024	-0.029	-0.030	-0.030
	(0.029)	(0.035)	(0.026)	(0.034)	(0.040)	(0.034)
T × (1 year after 7/7)	-0.085***	-0.118***	-0.066**	-0.069*	-0.117***	-0.081**
	(0.029)	(0.035)	(0.026)	(0.036)	(0.040)	(0.037)
T × (2 years after 7/7)	-0.130***	-0.152***	-0.071**	-0.115***	-0.159***	-0.090*
	(0.029)	(0.034)	(0.029)	(0.043)	(0.048)	(0.053)
T × (3 years after 7/7)	-0.085**	-0.108***	-0.064*	-0.093**	-0.134***	-0.102*
	(0.034)	(0.038)	(0.034)	(0.043)	(0.047)	(0.053)
T × (4 years after 7/7)	-0.083*	-0.124**	-0.115**	-0.144***	-0.177***	-0.178***
	(0.046)	(0.053)	(0.050)	(0.054)	(0.057)	(0.062)
Observations	18,094	15,344	19,634	11,570	8,761	14,047
Within R^2	0.098	0.084	0.112	0.073	0.078	0.076
Postcode FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This Table explores the sensitivity of *DiD* regression estimates to sensible variations around the preferred distance thresholds. All columns result from running the *DiD* regression described in Equation 1. Columns (1) and (4) repeat the results in Table 2, which are obtained by making use of the preferred distance thresholds, i.e., 0 - 600 meters for London treatment, 600 - 1,200 meters for Def.1 control area, 0 - 600 meters for Def.2 control area and 0 - 500 meters for Manchester treatment, 500 - 1000 meters for Def.1 control area, 0 - 500 meters for Def.2 control area. Column (2) reduces the treatment area by 100 meters and the control area by 200 meters. Column (3) increases by 100 meters the treatment area and 200 meters the control area. Column (5) shrinks both treatment and control areas by 100 meters, Column (6) enlarges both treatment and control areas by 100 meters. The negative impact on "treated" houses survives these variations in threshold definitions.

Table B5: London and Manchester: Placebo tests with randomly assigned treatment status

Dependent variable: log(house price)	(1)	(2)	(3)	(4)	(5)	(6)
Interaction terms	True data	Placebo tests				
London						
T × (4 years before 7/7)	-0.013	-0.003	0.006	-0.006	-0.026	-0.016
	(0.032)	(0.027)	(0.028)	(0.026)	(0.027)	(0.027)
T × (3 years before 7/7)	-0.028	0.006	-0.024	-0.034	-0.038	-0.034
	(0.029)	(0.026)	(0.027)	(0.027)	(0.026)	(0.026)
T × (2 years before 7/7)	-0.011	-0.009	0.028	-0.003	-0.012	-0.018
	(0.027)	(0.026)	(0.025)	(0.025)	(0.026)	(0.025)
T × (1 year after 7/7)	-0.063**	-0.001	-0.011	-0.005	-0.010	-0.029
	(0.027)	(0.023)	(0.024)	(0.024)	(0.024)	(0.023)
T × (2 years after 7/7)	-0.057*	0.007	0.002	-0.023	-0.028	-0.021
	(0.031)	(0.024)	(0.026)	(0.026)	(0.027)	(0.024)
T × (3 years after 7/7)	-0.021	0.054**	-0.001	-0.009	-0.040	-0.016
	(0.032)	(0.027)	(0.027)	(0.027)	(0.028)	(0.027)
T × (4 years after 7/7)	-0.024	-0.005	-0.027	-0.028	-0.030	-0.024
	(0.037)	(0.032)	(0.033)	(0.031)	(0.032)	(0.031)
Observations	22,044	22,044	22,044	22,044	22,044	22,044
Manchester						
T × (4 years before 7/7)	0.001	0.005	-0.009	-0.009	-0.015	-0.015
	(0.049)	(0.017)	(0.023)	(0.018)	(0.016)	(0.018)
T × (3 years before 7/7)	0.026	0.014	-0.005	0.015	-0.010	0.012
	(0.039)	(0.015)	(0.0170)	(0.015)	(0.016)	(0.017)
T × (2 years before 7/7)	0.008	-0.007	0.009	0.000	-0.023	-0.027*
	(0.029)	(0.014)	(0.015)	(0.014)	(0.016)	(0.015)
T × (1 year after 7/7)	-0.085***	0.000	-0.005	0.015	-0.016	0.001
	(0.029)	(0.012)	(0.014)	(0.013)	(0.014)	(0.014)
T × (2 years after 7/7)	-0.130***	-0.005	0.004	0.007	-0.014	0.012
	(0.029)	(0.013)	(0.014)	(0.013)	(0.013)	(0.013)
T × (3 years after 7/7)	-0.085**	0.001	0.001	0.012	-0.005	-0.003
	(0.034)	(0.015)	(0.016)	(0.017)	(0.014)	(0.016)
T × (4 years after 7/7)	-0.083*	0.016	-0.009	0.042*	-0.016	0.032
	(0.046)	(0.023)	(0.024)	(0.022)	(0.022)	(0.020)
Observations	18,094	18,094	18,094	18,094	18,094	18,094
Postcode FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: All columns result from running the *DiD* regression described in Equation 1. Column (1) repeats the results in Table 2, which are obtained by using the true location of each house sale transaction. In columns (2) to (5) I conduct a falsification exercise which tests the importance of distance to the closest major public transportation station in explaining house prices. As per Def.1, for treatment and control areas for London I focus on the sample of house sale transactions for which the house was either within 600 meters or 600 to 1,200 meters from a major Tube access. I randomize the value of T for these houses, using random draws from a uniform distribution. Except this reassigned closeness status, each observation remains unaltered. In this process, I aim to keep the same number of observations within the 0- to 600-meter and 600- to 1,200-meter bands, so that differences in significance are not driven by changing sample sizes. I use the ratio of the number of observations between 0 to 600 meters to all within 1,200 meters to decide whether an observation is attributed a 0 or a 1 for its value of T. This Table shows that it is actually the distance to main transportation hubs that explains the relative fall in appeal of houses near these hubs.

Table B6: London: 2005-2009. Quarterly evolution of rental price asked and duration on market

Control Area Dependent Variable	Def.1 log (initial rent asked)	Def.2 log (initial rent asked)	Def.1 $\mathbb{1}_{DOM_{ij}>median(DOM)}$	Def.2 $\mathbb{1}_{DOM_{ij}>median(DOM)}$
Interaction terms				
T × d(Q1 of 2005)	-0.013	-0.005	-0.129	-0.111
	(0.032)	(0.033)	(0.082)	(0.086)
T × d(Q3 of 2005)	-0.043	-0.034	-0.198**	-0.224***
	(0.032)	(0.033)	(0.078)	(0.081)
T × d(Q4 of 2005)	-0.093***	-0.078**	-0.139*	-0.114
	(0.034)	(0.035)	(0.084)	(0.088)
T × d(Q1 of 2006)	-0.018	0.000	-0.045	-0.036
	(0.034)	(0.035)	(0.097)	(0.100)
T × d(Q2 of 2006)	0.017	0.028	-0.059	-0.048
	(0.032)	(0.033)	(0.091)	(0.097)
T × d(Q3 of 2006)	-0.098***	-0.081**	-0.204**	-0.215**
	(0.031)	(0.032)	(0.084)	(0.087)
T × d(Q4 of 2006)	-0.003	0.022	-0.057	-0.064
	(0.030)	(0.032)	(0.095)	(0.099)
T × d(Q1 of 2007)	-0.048	-0.042	-0.059	-0.053
	(0.037)	(0.038)	(0.089)	(0.094)
T × d(Q2 of 2007)	-0.045	-0.048	-0.073	-0.071
	(0.033)	(0.033)	(0.090)	(0.094)
T × d(Q3 of 2007)	-0.026	-0.024	-0.099	-0.131
	(0.032)	(0.033)	(0.084)	(0.090)
T × d(Q4 of 2007)	0.041	0.052	-0.039	-0.028
	(0.039)	(0.040)	(0.093)	(0.098)
Rental characteristic				
# Bedrooms	0.339***	0.350***	0.061***	0.050***
	(0.007)	(0.008)	(0.007)	(0.008)
Quarter on market FE				
Q1	0.010	0.001	-0.032**	-0.041**
	(0.007)	(0.009)	(0.016)	(0.018)
Q3	0.029***	0.021***	-0.101***	-0.077***
	(0.007)	(0.008)	(0.015)	(0.017)
Q4	0.038***	0.023**	-0.003	-0.002
	(0.009)	(0.011)	(0.016)	(0.020)
Year on market FE				
2006	0.067***	0.057***	-0.150***	-0.139***
	(0.009)	(0.010)	(0.018)	(0.021)
2007	0.182***	0.185***	-0.117***	-0.097***
	(0.010)	(0.011)	(0.019)	(0.023)
2008	0.234***	0.242***	0.067***	0.093***
	(0.010)	(0.011)	(0.019)	(0.023)
2009	0.150***	0.148***	0.089***	0.117***
	(0.010)	(0.012)	(0.019)	(0.022)
	(0.035)	(0.036)	(0.085)	(0.092)
Observations	11,819	9,180	11,819	9,180
R^2	0.550	0.551	0.053	0.051

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: All regressions include postcode, year and quarter FE. Coefficients on interaction terms from quarter 10 after 7/7 are omitted for brevity. To exclude outliers, the sample of properties for rent is restricted to flats only and the first five floors of a building. With this rule I keep 91 percent of the rental observations. *d(Q1 of 2005)* is a dummy variable taking value 1 whenever a property enters the rental market during the 1^{st} quarter of 2005. The omitted quarter is *Q2* of 2005. *Q3* of 2005 is the quarter during which the attacks occurred. Columns (1) and (3) use the first definition of the counterfactual group, i.e., properties located within 600 to 1,200 meters from a main transportation hub. Columns (2) and (4) use the second definition of the counterfactual group, i.e., properties located within 600 meters from a secondary transportation hub. Columns (1) and (2) report the impact of the London attacks on the initial rent asked for LonRes properties in London. DOM stands for "days on the market" and measures the age of a rental listing, i.e., the number of days between the date when the property arrives on the rental market and the rental date. $\mathbb{1}_{DOM_{ij}>median(DOM)}$ takes value 1 whenever a property ij remains longer on the rental market than the median DOM over the entire sample. Columns (3) and (4) report the impact of the London attacks on the share of rentals whose days on the market are more than the median of these days in the entire sample.

C Terrorism lowers the value of proximity to public transportation: Evidence from businesses

Table C7: London and Manchester: The impact of the London bombings on incumbent firms

Outcome Variable	(1) **L**: EBMA	(2) **M**: EBMA	(3) **L**: log(FIAS)	(4) **M**: log(FIAS)	(5) **L**: LIQR	(6) **M**: LIQR	(7) **L**: CRPE	(8) **M**: CRPE
T × (4 years before 7/7)	0.459	-0.806	-0.048	0.061	0.323	-0.614	-3.260	17.062
	(1.305)	(4.434)	(0.044)	(0.109)	(0.240)	(0.558)	(5.197)	(12.329)
T × (3 years before 7/7)	1.070	-0.577	-0.040	0.110	0.066	-0.226	-1.482	0.830
	(1.186)	(4.071)	(0.041)	(0.099)	(0.219)	(0.512)	(4.775)	(11.540)
T × (2 years before 7/7)	1.947*	2.712	0.005	0.138	-0.096	0.343	5.051	21.015*
	(1.144)	(3.877)	(0.040)	(0.095)	(0.211)	(0.486)	(4.621)	(10.793)
T × (year with 7/7)	-0.043	3.136	0.015	0.027	-0.062	-0.442	1.682	4.070
	(1.251)	(4.316)	(0.041)	(0.100)	(0.228)	(0.522)	(4.994)	(11.596)
T × (1 year after 7/7)	-6.741***	-7.037	-0.110**	-0.463***	-2.325***	0.014	28.740***	20.497
	(1.491)	(5.122)	(0.049)	(0.118)	(0.272)	(0.598)	(5.414)	(13.325)
T × (2 years after 7/7)	-1.165	0.329	-0.073	-0.401***	0.343	0.315	6.346	4.073
	(1.505)	(5.201)	(0.049)	(0.119)	(0.272)	(0.600)	(5.450)	(13.484)
T × (3 years after 7/7)	6.249***	3.702	-0.058	-0.361***	0.439	-0.006	-23.793***	18.967
	(1.548)	(5.328)	(0.049)	(0.119)	(0.273)	(0.606)	(5.526)	(13.669)
T × (4 years after 7/7)	1.318	3.823	-0.075	-0.387***	0.426	0.676	2.479	13.091
	(1.583)	(5.438)	(0.049)	(0.120)	(0.275)	(0.613)	(5.627)	(13.886)
Observations	26,618	1,569	47,929	6,818	63,041	8,535	19,856	1,167
R^2	0.622	0.613	0.903	0.890	0.559	0.546	0.488	0.581
Postcode FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table presents *DiD* estimates in the style of Equation 1, for Def.1 for the control areas. Odd-numbered columns report results for London **L**, even-numbered for Manchester **M**. EBMA stands for EBIT margin (the ratio of earnings before interest and taxes to net revenue, %), FIAS for Fixed Assets (assets purchased for long-term use, £), LIQR for the Liquidity Ratio (available cash and marketable securities against outstanding debt, %) and CRPE for Credit Period (number of days that a customer is allowed to wait before paying an invoice, days). The large variation in sample size is due to frequent missing values in the Amadeus dataset. I only retain firms whose legal form is private, guarantee or unlimited, which have not changed their 2-digit 2007 NAICS industry code (I allow for shifts within the 2-digit code) and whose accounting year closes in the second half of the year.

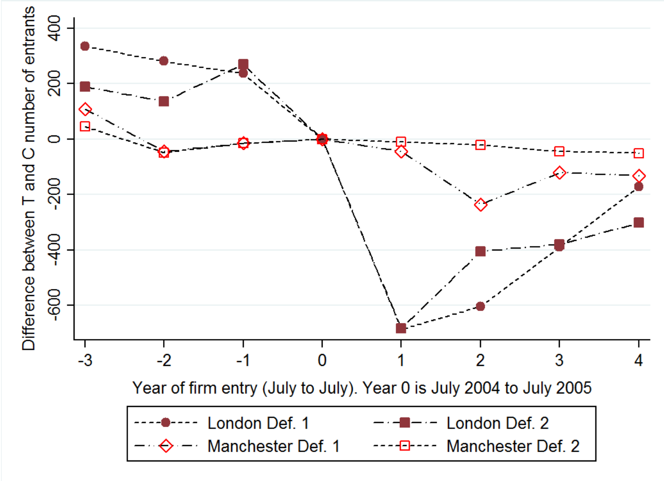

Figure C4: London and Manchester: Difference between the number of firm entries in treated and control areas

Note: This graph plots the yearly difference between the number of firm entries in treated versus control areas, relative to the same difference during the year of July 2004 to July 2005 (labeled as year 0). The July 2004 to July 2005 difference in firm entries is normalized to 0. The y-axis value of *-686* for "London Def. 1" in year 1 means that 686 fewer firms entered the treated areas of London between July 2005 to July 2006 (the year after the London attacks) compared to the control areas (defined as per Definition 1) and compared to year 0.

D Ruling out alternative hypotheses

Table D8: London: Variations in the London sample for both definitions of the control area

Dependent variable: log(house price)	(1)	(2)	(3)	(4)	(5)	(6)
Interaction terms	Def.1 Main	Def.1 w/o KC	Def.1 w/ LSt.	Def.2 Main	Def.2 w/o KC	Def.2 w/ LSt
T × (4 years before 7/7)	-0.013	-0.020	-0.002	-0.017	-0.030	-0.013
	(0.032)	(0.039)	(0.028)	(0.031)	(0.039)	(0.028)
T × (3 years before 7/7)	-0.028	-0.049	-0.026	-0.044	-0.065*	-0.042*
	(0.029)	(0.035)	(0.025)	(0.029)	(0.035)	(0.025)
T × (2 years before 7/7)	-0.011	-0.026	-0.008	-0.019	-0.033	-0.021
	(0.027)	(0.033)	(0.024)	(0.027)	(0.033)	(0.024)
T × (1 year after 7/7)	-0.063**	-0.081**	-0.048**	-0.063**	-0.085**	-0.054**
	(0.027)	(0.033)	(0.024)	(0.026)	(0.033)	(0.024)
T × (2 years after 7/7)	-0.057*	-0.082**	-0.040	-0.058*	-0.087**	-0.046*
	(0.031)	(0.038)	(0.028)	(0.031)	(0.038)	(0.028)
T × (3 years after 7/7)	-0.021	-0.052	-0.006	-0.030	-0.068*	-0.020
	(0.032)	(0.039)	(0.028)	(0.031)	(0.039)	(0.028)
T × (4 years after 7/7)	-0.024	-0.055	-0.019	-0.033	-0.084*	-0.030
	(0.037)	(0.044)	(0.034)	(0.036)	(0.043)	(0.034)
Observations	22,044	17,308	27,747	21,354	14,739	26,551
Within R^2	0.235	0.209	0.249	0.262	0.208	0.270
Postcode FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: According to Def.1 and Def.2 Main, the preferred list of treated stations contains King's Cross, Oxford Circus, Waterloo and Victoria. Def.1 and Def.2 without KC exclude King's Cross from this list. Def.1 and Def.2 w/ LSt add Liverpool Street (and its control neighboring stations, Aldgate East, Shoreditch High Street, and Old Street for Def.2) to the preferred list. Notice that results are neither driven by the direct damage of the attacks (Columns (2) and (5) exclude the targeted station), nor the prospect of the 2012 London Olympics (Columns (3) and (6) include Liverpool Street, the largest rail hub closer to the 2012 Olympics site).

Table D9: London: House prices around secondary Tube stations on bombed vs. unharmed lines

Dependent variable: log(house price)	(1)	(2)	(3)
Interaction terms	0-600m	0-500m	0-400m
$\mathbb{1}_{\text{Station on bombed line}}$ × (4 years before 7/7)	0.023	0.023	0.019
	(0.027)	(0.031)	(0.039)
$\mathbb{1}_{\text{Station on bombed line}}$ × (3 years before 7/7)	-0.015	-0.003	-0.042
	(0.027)	(0.034)	(0.042)
$\mathbb{1}_{\text{Station on bombed line}}$ × (2 years before 7/7)	0.002	0.005	0.009
	(0.024)	(0.029)	(0.035)
$\mathbb{1}_{\text{Station on bombed line}}$ × (1 year after 7/7)	0.006	0.011	-0.020
	(0.025)	(0.030)	(0.037)
$\mathbb{1}_{\text{Station on bombed line}}$ × (2 years after 7/7)	0.035	0.040	0.052
	(0.026)	(0.031)	(0.038)
$\mathbb{1}_{\text{Station on bombed line}}$ × (3 years after 7/7)	0.063**	0.041	0.022
	(0.027)	(0.033)	(0.040)
$\mathbb{1}_{\text{Station on bombed line}}$ × (4 years after 7/7)	0.063*	0.060	0.051
	(0.033)	(0.040)	(0.050)
Observations	19,588	14,067	9,196
Within R^2	0.270	0.272	0.280
Postcode FE	YES	YES	YES
Year FE	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: All columns result from running the *DiD* regression described in Equation 2 for three threshold distances. This Table shows that the main results of the paper are not driven by trauma or inconveniences associated to using a line bombed during the London attacks.

Table D10: London and Manchester: The impact of a "fake" London attack on house prices

Dependent variable: log(house price)	(1)	(2)
Interaction terms	London Def. 1	Manchester Def. 1
T × (3 years before "fake" 7/7)	-0.001	-0.007
	(0.028)	(0.046)
T ×(2 years before "fake" 7/7)	-0.016	0.018
	(0.030)	(0.035)
T × *(1 year after "fake" 7/7)	0.011	-0.008
	(0.027)	(0.029)
T ×*(2 years after "fake" 7/7)	-0.051*	-0.092***
	(0.027)	(0.029)
T ×*(3 years after "fake" 7/7)	-0.045	-0.137***
	(0.031)	(0.028)
T ×*(4 years after "fake" 7/7)	-0.010	-0.093***
	(0.029)	(0.033)
T ×*(5 years after "fake" 7/7)	-0.012	-0.090*
	(0.038)	(0.046)
Observations	22,044	18,094
Within R^2	0.235	0.098
Postcode FE	YES	YES
Year FE	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Both columns result from running the *DiD* regression described in Equation 1 using the Def.1 for the control area. In both columns, I take as reference year the one prior to the "fake" London bombings (fake date of July 2004). Estimates for Def. 2 or for any other year of falsification are available upon request. All results suggest that the timing of the actual London attacks is key for results.

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