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Homeowner Preferences after September 11th, a Microdata Approach

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Abstract

The existence of homeowner preferences - specifically homeowners' preferences for neighbors - is fundamental to economic models of sorting. This paper investigates whether or not the terrorist attacks of September 11, 2001 (9/11) impacted on local preferences for Arab neighbors. We test for changes in preferences using a differences-in-differences framework in a hedonic pricing model. Relative to sales before 9/11, we find properties within 0.1 miles of an Arab homeowner sold at a 1.5% discount in the 180 days after 9/11. The results are robust to a number of specifications including time horizon, event date, distance, alternative ethnic groups, and the presence of nearby mosques. Previous research has shown price effects at neighborhood levels but has not identified effects at the micro or individual property level, and for good reason: most transaction level data sets do not include ethnic identifiers. Applying methods from the machine learning and biostatistics literature, we develop a binomial classifier using a supervised learning algorithm and identify Arab homeowners based on the name of the buyer. We train the binomial classifier using names from Summer Olympic Rosters for 221 countries during the years 1948-2012. We demonstrate the flexibility of our methodology and perform an interesting counterfactual by identifying Hispanic and Asian homeowners in the data; unlike the statistically significant results for Arab homeowners, we find no meaningful results for Hispanic and Asian homeowners following 9/11.

Key Words: house prices, ethnicity, homeowner preferences, terrorism, September 11th

JEL Codes: R21, R23, R31, J15

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1 Introduction

Preferences over neighborhood and neighbor characteristics are fundamental to models of economic sorting. On September 11, 2001 (9/11), 19 terrorists from 4 Arab countries - Egypt, Lebanon, Saudi Arabia and the United Arab Emirates - attacked targets in New York City and Washington DC. The How Americans Responded (HAR) survey carried out immediately after 9/11 found that the attacks affected individual preferences for both Arabs and Muslims.[1] A majority of respondents in HAR (70%) viewed African, Hispanic, and Asian Americans as favorable, but less than half of the respondents (46%) viewed Arabs and Muslims as favorable. Furthermore, in the 30 days after 9/11, the Anti Defamation League recorded 12 instances of either anti-Arab or anti-Muslim violence.[2] This study investigates whether or not the events of 9/11 caused preferences for Arab neighbors to change, and if these changes were reflected in residential real estate prices.

Using a hedonic pricing model, we test this hypothesis using transactions for single-family homes obtained from the King County Assessor's Office. King County is in Washington state and includes the Seattle metropolitan area. Similar to (Linden and Rockoff, 2008) and Pope (2008), we exploit cross-sectional and temporal variation in sale prices in order to isolate price effects attributable to 9/11. Our results are both plausible and statistically significant: 1) relative to sales before 9/11, houses sold 180 days after 9/11 and within 0.1 miles of an Arab neighbor sold at a 1.5% discount, 2) this effect is temporary as the discount is not statistically significant 180 days after 9/11, and 3) the effect is not attributable to the presence of nearby mosques. These conclusions are robust across a wide number of model specifications.

Economic research has focused on 2 channels by which economic variables, such as property prices, can be affected by terrorism: expectations and preferences. In the context of real estate, the expectations channel is straightforward. Property valuations will decrease following a terrorist attack if 1) the likelihood of a future terrorist attack increases and 2) a particular piece of real estate is a realistic target of a terrorist attack. Although HAR indicates that 79% of respondendents became more concerned of another terrorist attack following 9/11, it is unlikely that individuals living in single-family homes believed their homes were potential targets of a terrorist attack.[3] By

[1] http://www.isr.umich.edu/cps/har/ How Americans Responded is a survey project at the Institute for Social Research, the University of Michigan. The 613 panel participants in the project were first contacted October 17, 2001 and then re-surveyed April, 16 2002.

[2] http://archive.adl.org/terrorism_america/adl_responds.html

[3] This is not to say that all properties are unlikely targets of terrorism as Abadie and Dermisi (2008) find evidence

using single-family homes in a location more than 3,000 miles from New York City, we preclude any price effects resulting from the expectations channel.

Changing preferences towards Arabs or Muslims can also impact real estate prices. If non-Arab homeowners prefer to live near non-Arab neighbors post 9/11, property prices near Arab neighbors will decrease post 9/11. These static preferences towards Arab neighbors are in line with correspondence experiments that find landlords discriminate against non-Arabs or non-Muslims, Ahmed and Hammarstedt (2008) and Bosch et al. (2010). Similar to Gautier et al. (2009), we use an event study where non-Arab homeowners initially have arbitrary preferences for their current Arab neighbors, but the events of 9/11 change these preferences. Because we examine transactions within 0.1 miles of Arab homeowners, we use *neighbor* in the geographic sense. We note it would be interesting to perform an analysis similar to Linden and Rockoff (2008) and Pope (2008) where the arrival of an Arab neighbor lowers property prices post 9/11. However, given the short-term nature of the price effects and the small number of Arab homeowners in the data, such an analysis is not possible.[4]

Of course, changing preferences towards Arab neighbors does not necessarily mean that current homeowners have an unfavorable view of Arab neighbors post 9/11. Rather, homeowners might value their property based on the expected preferences of potential buyers. Given that less than half of the respondents in HAR viewed Arabs as favorable, it is not implausible that shortly after 9/11 transaction prices should reflect the probability that a non-negligible number of potential buyers would view existing Arab neighbors unfavorably.[5] A limitation of this study is that we cannot distinguish between the current homeowner and potential buyer channels. In what follows, we remain agnostic as to the true source of the change in preferences.

Unfortunately, our data set is comparable to many transaction level data sets that do not indicate the ethnicity of the buyer or seller. However, our data set does include the full name (first

of the expectations channel at work in high-rise office buildings. Similarly, homeowners living in high-rise residential buildings might might form similar expectations. Although Seattle is a major metropolitan area and the assessor's data includes condominium transactions in multi-floor buildings, there are too few units and transactions following 9/11 than can be used for this analysis. Furthermore, unlike Abadie and Dermisi (2008), it is not clear what are the landmark buildings in downtown Seattle. In short, high-rise residential buildings are not the focus in this paper but a possible avenue for future research.

[4] The small number of Arabs purchasing home in the 180 days after 9/11 in the assessor data is the primary reason for this insignificance.

[5] This can be done using a search model similar to Krainer (2001) where 1) current homeowners receive non-negative utility from living near a nearby Arab neighbor (Type I), 2) some potential buyers are also Type I, 3) the remaining potential buyers receive disutility from an Arab neighbor (Type II), and 4) the transaction price is decreasing in the probability that potential buyers are Type II.

and last name) of both the buyer and seller. We identify ethnicity based on an individual's full name. Related approaches have been used extensively in the biostatistics literature and are known as *name-ethnicity matching*. Gautier et al. (2009) use an informal but pragmatic approach where research assistants from Turkey and Morocco manually identify *Turkish* and *Moroccan* names in 20,148 transactions. Given that our data set includes 265,255 transactions and we have no Arab research assistants, manually identifying Arab names in the assessor data is not feasible. Based on anecdotal evidence, this situation is common to many economic researchers.

We focus on preferences for Arabs as a matter of practicality. In order to identify Arab homeowners, we use a supervised learning algorithm popular in the machine learning literature. The idea is to use a labeled data set in order to predict labeling on a different, unlabeled data set. For the application at hand, we use a binary labeling where countries are either members of the Arab League or not. We then apply this labeling to the set of Summer Olympic rosters for all countries 1948-2012. Next, we estimate a binomial classifier (a penalized logit) using indicator variables for names as the explanatory variables. Finally, we use the estimated binomial classifier to label buyer and seller names in the assessor data. In short, names in the assessor data are labeled based on the conditional likelihood that a given name comes from the Olympic roster of an Arab League country.

The supervised learning algorithm we describe provides two advantages over manual classification. First, unlike manual classification, our procedure can be scaled to large data sets. In real estate settings, large data sets are commonplace. Second, our procedure is quite flexible and can be used to classify individuals by name into any number of groups. As an example, we perform a falsification test using Asian and Hispanic homeowners and, in contrast to Arab homeowners, find no significant price effects surrounding 9/11 for either of these two groups. Of course, automated classification schemes are neither perfect nor perfect substitutes for human classifiers. Regardless, in the spirit of Gautier et al. (2009), an independent undergraduate economics student confirmed the probable ethnicity of buyers classified as Arab in the assessor data. Taken as a whole, the machine learning approach is a practical way to identifying ethnicity using names in large data sets.

2 Literature Review

2.1 Terrorism

This study is part of a growing theoretical and empirical literature investigating the relationship between terrorism and economic variables. Theoretical results focus on how terrorism is different from other risks. Lakdawalla and Zanjani (2005) demonstrate how terrorism insurance differs from catastrophe insurance. Becker et al. (2004) examine terrorism in a behavioral model that includes both fear and risk aversion. Glaeser and Shapiro (2002) compare the impacts of war and terrorism on the formation and dissolution of urban centers and find war has had a larger impact on urban formation than terrorism.

Despite the low probability of a terrorist event, there does appear to be significant empirical evidence that terrorism has a large impact on macroeconomic variables. In one of the earliest studies, Abadie and Gardeazabal (2003) find terrorism in the Basque region of Spain decreased regional per capita GDP by a non-trivial 10%. Examples of other economic variables affected by terrorism include stock markets Zussman and Zussman (2006) and Abadie and Gardeazabal (2003), foreign direct investment Abadie and Gardeazabal (2008), consumption Eckstein and Tsiddon (2004), industrial organization Berrebi and Klor (2010), birth weight Camacho (2008), vacancy rates Abadie and Dermisi (2008), and house prices Besley and Mueller (2012) and Elster et al. (2017). Of course, these economic costs are in addition to any non-economic costs associated with a decrease in quality of life, Frey et al. (2007).

Implicit in these studies are the beliefs that economic agents have about the probability of future terrorism. These beliefs are updated after the realization of relevant events. For instance, Abadie and Gardeazabal (2003) find violence that the stock market reacts positively when a credible truce is reached. Zussman and Zussman (2006) and Zussman et al. (2008) find that the Israeli stock market reacts in response to key events in the Israeli-Palestinian conflict. Using a regime-switching model, Besley and Mueller (2012) find there is a difference between short-term violence and long-term perceptions of persistent violence in Northern Ireland; only when homeowners perceive that the world is in a non-violent state do regional house price indexes trend upwards. Abadie and Dermisi (2008) find vacancy rates in notable Chicago office buildings increased in the months following 9/11.

2.2 Preferences and the Real Estate Market

This study is also related to other studies that find preferences for neighbors are correlated with socioeconomic variables. Using US Census microdata, Bayer et al. (2004) find race is a fundamental cause of segregation in the San Francisco Bay area. Other studies suggest preferences of natives for non-immigrant neighbors can lead to segregation, Cutler et al. (2008) and Benabou (1993). With respect to house prices, Saiz and Wachter (2011) find metropolitan areas with more immigration grow faster, but neighborhoods with a higher concentration of immigrants grow more slowly within the metropolitan area. Saiz and Wachter (2011) suggest one interpretation is that natives prefer native neighbors. That being said, Bayer et al. (2007) find price effects attributable to race are misleading when neighborhood quality is not observed. As a whole, these studies find evidence that homeowners prefer neighbors with common social, racial, linguistic, and ethnic identities. We build on these studies and ask if preferences for ethnicity are static, or if preferences respond to acts of terrorism.

Several studies have used correspondence experiments to test for discrimination against Arabs and Muslims in the real estate market. Ahmed and Hammarstedt (2008) find landlords in Sweden are less willing to respond to rental applications when the applicant's name is strongly associated with Islam or Arab ethnicity. Using applicant names *Erik Johansson, Maria Andersson* and *Mohammed Rashid*, Ahmed and Hammarstedt (2008) find emails sent using the name Mohammed Rashid received fewer callbacks and invitations to view the property. In a similar experiment in Spain, Bosch et al. (2010) find the low response rate still exists after controlling for socioeconomic factors. Using hand-written applications, Carpusor and Loges (2006) find similar evidence of discrimination in the American rental market. Overall, these findings are not unlike the experiences of other minorities in rental markets, Hanson and Hawley (2011). Nonetheless, in all of these correspondence experiments the ethnicity of the applicant is signaled by name and name alone.

In addition to cross-sectional studies, event studies have also been used to test for changing attitudes towards Arabs and Muslims at the neighborhood level. Gautier et al. (2009) investigate property prices following the murder of Dutch filmmaker Theo van Gogh by a recent convert to radical Islam. Following van Gogh's murder, listing prices in nearby Muslim neighborhoods decreased by 3%. Further, Gautier et al. (2009) find evidence of increasing segregation after the murder as Muslims became more likely to purchase homes in areas with an already large concentration of

Muslims. Similar results at the neighborhood level are found following London subway bombings, Ratcliffe and von Hinke Kessler Scholder (2015). The results in (Gautier et al., 2009) and Ratcliffe and von Hinke Kessler Scholder (2015) are best interpreted as the effect of terrorism on home prices in nearby Muslim neighborhoods. In contrast, we are interested in local impacts at the property level in neighborhoods that do not necessarily have a clustering of Arab homeowners. Furthermore, Seattle is more than 3,000 miles from New York and Washington DC and was not a target of any terrorism on 9/11.

2.3 Name-Ethnicity Matching

In order to estimate our binomial classifier, we use a hierarchical data generating process common in the text modeling literature. In these models, words in a body of text are assumed to be drawn from a multinomial distribution where the probability of each word is drawn from one or more topics, Hofmann (1999), Blei et al. (2003). For example, texts with *urban* topics are more likely to include the words *residential*, and *rental*, whereas texts with *international* topics are more likely to include *tariff* and *trade*. In this study, the first and last name of each Olympian (words) are viewed as realizations from a multinomial distribution where the probability of each name is a function of the Olympian's ethnicity (topic).

Automated approaches for matching name to race or ethnicity, name-ethnicity matching, have been well studied in the biostatistics literature. Examples include Coldman et al. (1988), Burchard et al. (2003), Fiscella and Fremont (2006). Examples where the researcher generates names from a given ethnicity are common in correspondence experiments in economics, Bertrand and Mullainathan (2004), Ahmed and Hammarstedt (2008), Bosch et al. (2010), Hanson and Hawley (2011), Hanson et al. (2016). Applications where real names are used to infer ethnicity include Humphreys et al. (2016) and Gautier et al. (2009).

A standard econometric approach to creating a binomial classifier based on observables is to use a logit or similar binary choice model. In the name-ethnicity approach, the number of observables (names) can be large. In a high-dimensional covariate setting, maximum likelihood estimation will overfit the data and out-of-sample predictions are at best misleading, Hastie et al. (2015). Out-of-sample performance is of critical importance to us as our primary goal is predicting ethnicity in the assessor data. Noting this, we use an ℓ_1 penalized estimator. This regularization yields an estimator that has been shown to have superior out-of-sample performance in high-dimensional logistic models relative to both un-normalized estimators and ℓ_2 penalized estimators, Ng (2004). Furthermore, unlike manual classification, the classifier can be scaled to large data sets.

We estimate the binomial classifier by first labeling the Olympians as either representing an Arab League country or not. By using labeled data, we use a supervised learning algorithm. An alternative when the research has no ex-ante knowledge of groups is to identify latent ethnic groups by applying an unsupervised learning algorithm on unlabeled Olympian names. One of the more popular unsupervised learning algorithm is the Latent Dirichlet Allocation (LDA), Blei et al. (2003). Similar to principal components, it is necessary for the researcher to label or identify ex-post estimated LDA topics. In unreported results, we find that the LDA resulted in a group that could most likely be identified as an Arab League group. In any event, because we are interested in classifying based on ex-ante specified groups, we leverage this specificity and use the supervised learning algorithm described below.

3 Identifying Ethnicity and the Binomial Classifier

In what follows we use the following definitions: a *full name* is a first name and last name pair, and a *name* is either a first name or a last name. We treat each full name as an unordered set of names: one or more first and last names.[6] For example, American Olympian *carl lewis* is expressed as the 2-element set {*carl*, *lewis*}. In our analysis, we treat hyphenated names as two names and retain the hyphen in order to acknowledge the split. For example, French Olympian *jean-claude killy* becomes the 3 element set {*jean-*, *claude*, *killy*}, and Syrian Olympian *nasser al-shami* is the 3-element set {*nasser*, *al-*, *shami*}. We also remove diacritics from the full names in a practical manner as *josé* becomes *jose*. In the Olympic rosters, we identify 69,648 unique names. In order to focus on the more frequent names, we drop all names that occur fewer than 10 times in the rosters. Doing so leaves $P = 3,212$ unique names.[7]

As mentioned above, we use indicator variables for names as the explanatory variables in a logit model. That being said, it is instructive to view each full name as a $P \times 1$ vector, x_n, of 1s and

[6]Viewing the names in this was is common in the textual analysis literature and is known as a *tokenization* approach where each name is a *token*. Examples of the tokenization approach to text data include Gentzkow and Shapiro (2010), Taddy (2013), and Nowak and Smith.

[7]In unreported results, we found that a cutoff of 5 produced similar results to a cutoff of 10.

0s where the p element is equal to 1 if name p is in the full name of Olympian n and 0 otherwise. For *carl lewis*, x_n has 2 elements with a 1, and the remaining elements are 0: the pth element of x_n, x_{np}, corresponding to *carl* will be equal to 1 and likewise for *lewis*. Because we view first and last names as exchangeable variables, our estimator does not distinguish *carl lewis* from *lewis carl*. However, such distinctions are unlikely to have a material impact on our estimator. Moreover, the exchangeable assumption is also practical as first and last names are not clearly identified in the assessor data.

Next, we set the indicator variable $y_n = 1$ if an Olympian comes from as Arab country and 0 otherwise. In order to identify Arab countries, we use a list of countries in the Arab League. Using y_n and x_n, one could estimate a logit model where the explanatory variables are indicator variables for each of the P names. The probability that $y_n = 1$ is then given by

$$\Pr(y_n = 1|X_n, \phi) = \frac{e^{\phi_0 + \sum_p X_{np}\phi_p}}{1 + e^{\phi_0 + \sum_p X_{np}\phi_p}} \tag{1}$$

In Equation (1), when $0 < \phi_p$, the presence of name p increases the likelihood that Olympian n represents a country that is a member of the Arab League, and vice-versa for $\phi < 0$. When $\phi_p = 0$, name p does not help to predict y_n. The parameter ϕ_0 controls the unconditional $\Pr(y_n = 1)$.

For fixed P, the ϕ_p can be consistently estimated by maximum likelihood. In our application, the assumption of fixed P is difficult to defend as P is large by conventional standards even after filtering out the least common names in the Olympic rosters. As such, a maximum likelihood estimator with large P is prone to overfit the data rendering out-of-sample predictions meaningless, Hastie et al. (2015). Of course, we could increase the cutoff, exclude more names, and reduce the number of explanatory variables. However, such an approach is imprudent as names that are removed might be relevant for classification.

Therefore, we retain a large set of names - large P - and use a penalized likelihood estimator. Specifically, we place an ℓ_1 penalty on the individual ϕ_p parameters. We then choose ϕ to minimize the following

$$-\sum_n \Pr(y_n = 1|X_n, \phi)^{y_n} \left[1 - \Pr(y_n = 1|X_n, \phi)\right]^{1-y_n} + \lambda \sum_p |\phi_p| \tag{2}$$

Define the solution to Equation 2 as ϕ^*. The first term in Equation 2 is the negative likelihood of the sample using the individual likelihood given in Equation 1. The second term is an ℓ_1 penalty term that penalizes the magnitudes of the elements in ϕ. The parameter λ is a tuning parameter.[8][9] When $\lambda = 0$, there is no penalty on ϕ, and ϕ^* is the maximum likelihood estimator for the logit.

Because of the shape of the ℓ_1 penalty, some entries of ϕ^* can be set equal to 0. As mentioned above, when $\phi_p^* = 0$, name p cannot be used to classify y_n. With this interpretation, minimizing Equation 2 simultaneously selects variables and estimates coefficents. In any event, by including the penalty term $\lambda \sum_p |\phi_p|$, ϕ^* can be used to classify names out-of-sample as, unlike the maximum likelihood estimator, it is less likely to overfit the data in-sample, Ng (2004). Furthermore, this functional form for the penalty term has been shown to yield superior out-of-sample prediction when compared to an ℓ_2 alternative, Ng (2004). We emphasize, out-of-sample performance is fundamental to our results.

4 Model Specification

In order to test for changes in the local valuation of Arab neighbors, we compare a control and treatment group before and after 9/11. As described below, the two groups are defined by the distance to the nearest Arab homeowner where. As such, properties 0-0.1mi (0.1-0.3mi) from an Arab neighbor are in the treatment (control) group.[10] Properties further than 0.3mi from an Arab homeowner are neither in the treatment nor control group. To the extent that the events of 9/11 were unexpected, we interpret the results as causal. If not for 9/11, there would be no local price effects attributable to Arab neighbors. Of course, given the singular nature of these events, it is not fair to generalize the results to generic terrorist events.

In order to identify relevant distance thresholds, we first estimate a base hedonic model for house n in census tract c sold in quarter t

$$p_{nct} = x_{nct}\beta + \delta_{ct} + u_{nct} \tag{3}$$

Here, p_{nct} is the log price of the house, x_{nct} is a vector of house attributes including log square

[8]In our analysis, we select λ using 5 fold cross-validation. The results are robust to λ near the cross-validated choice of λ

[9]We use the `glmnet` package in `R` to solve Equation 2. The solution is found by using a quadratic approximation to the true penalized likelihood.

[10]The Appendix presents similar results when using a 0.1-0.2mi control group.

footage, bedrooms, bathrooms, and construction year, β is a vector of implicit prices, δ_{ct} is a census tract specific price at time t, and u_{nct} is an error term. We begin each quarter on the 11th of March, June, September, and December.

We determine a distance cutoff for the treatment group using a method similar to Linden and Rockoff (2008). After estimating Equation 3, we collect the residuals for all transactions located within 0.3mi of an Arab homeowner. We then estimate a local polynomial regression in order to estimate the price gradient as a function of distance from the nearest Arab neighbor.[11] The 95% confidence interval for the local polynomial using transactions 180 days before 9/11 is presented in green in Figure 1. Point estimates for the same local polynomial regression 180 days after 9/11 are presented as a red line in Figure 1.[12] If the events of 9/11 caused local preferences for Arab neighbors to change, the changes are extremely local as they appear insignificant after 0.1mi. In any event, Figure 1 suggests a treatment area cutoff of 0.1mi.

Using the definitions of the control and treatment groups, we create the indicator variable $D^{0.1} = 1$ ($D^{0.3} = 1$) if any Arab neighbor is within 0.1mi (0.3mi) of the property and 0 otherwise. In the absence of any time-varying treatment effects, sale price is given by

$$p_{nct} = x_{nct}\beta + D^{0.1}_{nct}\psi^{0.1} + D^{0.3}_{nct}\psi^{0.3} + \delta_{ct} + u_{nct} \tag{4}$$

In Equation 4, $\psi^{0.3}$ captures local cross-sectional price effects common all properties within 0.3 miles of an Arab homeowner. Cross-sectional differences can include arbitrary preferences for race and ethnicity as well as local amenities and neighborhood quality. The coefficient $\psi^{0.1}$ captures cross-sectional differences in price between the control and treatment groups. When $\psi^{0.1} > 0$ ($\psi^{0.1} < 0$), properties in the treatment group sell at a premium (discount) to properties in the control group. Figure 2 displays a treatment and control group for a randomly selected house.

In the absence of time-varying effects, boundary effects, or detailed data on race and ethnicity, it is difficult if not impossible to disentangle race and ethnicity preferences from local amenity effects, Bayer et al. (2007). We follow Pope (2008) and Linden and Rockoff (2008) and test for time-varying price effects generated by Arab neighbors by comparing prices between the control and treatment groups before and after 9/11. As mentioned above, we are interested in investigating

[11] Similar to Linden and Rockoff (2008), we estimate a local polynomial of order 3 and use a bandwidth of 0.1 nearest-neighbors using the `locfit` package in R.

[12] A similar plot is produced when using all sales before 9/11.

whether or not 9/11 caused perceptions of Arab neighbors to change in a negative way. In order to test this, we create the indicator variable $Post = 1$ if the transaction occurred after 9/11 but before March 10, 2002, a time period of 180 days. We later investigate 180-365 days following and 0-180 days before 9/11. For all of these time periods, we estimate

$$p_{nct} = x_{nct}\beta + D^{0.1}_{nct}\psi^{0.1} + D^{0.3}_{nct}\psi^{0.3} + Post_{nct} \times D^{0.1}_{nct}\tau^{0.1} + Post_{nct} \times D^{0.3}_{nct}\tau^{0.3} + \delta_t + \mu_c + u_{nct} \quad (5)$$

Here, $\tau^{0.1}$ and $\tau^{0.3}$ capture time-varying price effects relative to the location of Arab neighbors following 9/11. If $\tau^{0.3} < 0$, properties within 0.3 miles of Arab neighbors experienced price declines relative to the rest of the market. If the events of 9/11 caused homeowners' preferences towards Arab neighbors to change, property price effects should be stronger the closer the property is to an Arab neighbor, $\tau^{0.1} < 0$.

5 Data

Data used in this paper comes from two sources. The first source is the set of Summer Olympic rosters from 1948 to 2012. These rosters were downloaded from the Olympic Reference website.[13] Each roster includes the full name of the Olympian, age, gender and nationality. As mentioned above, we label countries as Arab if the country is a member of the Arab League. Six member states formed the Arab League in 1945: Jordan, Syria, Saudi Arabia, Lebanon, Egypt, Iraq. However, the Arab League has expanded over the years and now includes 22 member states that cover Northern Africa and the Middle East. We use the 22 member roster of Arab League nations. Table 1 shows the 20 most frequent names from the Arab and non Arab League countries. Figure 3 presents the names in a word cloud where the size of the font is associated with a greater frequency of the name within group.

Other publicly available data sets have been used by resarchers to identify ethnicity including Wikipedia Treeratpituk and Giles (2012) and IMDB Rachevsky and Pu (2011).[14] We use the Summer Olympic data set in lieu of these other data sets as the Summer Olympic data set 1) provides

[13] http://www.sports-reference.com/olympics/

[14] The authors are not aware of any publicly available databases provided by Wikipedia. Databases on actors, directors, etc. provided by IMDB and are available at http://www.imdb.com/interfaces

a sufficient number of observations for the training set and 2) the number of individuals from Arab League countries is significantly larger in the Summer Olympic data than in the Wikipedia or IMDB data. In total, there are $N = 90,636$ unique Olympians from 221 unique countries.

Transaction data used comes from the King County Assessor's Office that includes the Seattle metro area.[15] The data set is publicly available and includes information on property attributes, buyer and seller names, transaction price, and other relevant information. In order to exclude any effects due to the housing bubble of the mid 2000s, we limit our data set to transactions between January 1, 1990 and December 31, 2002. This leaves 265,255 total transactions in the study. Using December 31, 2002 as a cutoff provides us with more than 15 months of sales post 9/11 that we can use to identify time-varying price effects. Summary statistics for the data are provided in Table 2. The average transaction price is $233,066. The average house has 1,959 square feet, 3.3 bedrooms, 1.5 bathrooms, and was built in 1970.

Using the binomial classifier, we identify 457 transactions between 1990 and 2002 where an Arab was the buyer of record. This implies 0.17% of homeowners are of Arab ancestry and is comparable to the 2000 US Census where 0.4% of the King County population has Arab ancestry.[16] The locations of homeowners identified as an Arab on 9/11 are displayed in Figure 4. Unlike Gautier et al. (2009), the locations of Arab homeowners are fairly dispersed throughout Seattle. Although the data is publicly available, for privacy purposes, we do not disclose the actual names of the individuals who are identified as Arab. A list of the homeowners identified as Arab is available from the authors upon request.

Table 3 displays the total number of observations in the control and treatment groups before and after 9/11. There are 6,910 transactions in the treatment group with 459 transactions occurring between 0 and 180 days after 9/11. Likewise, there are 33,014 transactions in the control group with 2,088 transactions between 0 and 180 days after 9/11.

Table 4 compares control variables in the control group to control variables in treatment group. Mean comparison tests indicate that the treatment group differs from the control group in a statistically significant but economically insignificant manner. Figure 5 displays the distribution of the control variables for the 2 groups. Similar to the evidence in Table 4, the distributions for the control and treatment groups are similar, although a greater portion of properties in the treatment

[15] http://www.kingcounty.gov/depts/assessor.aspx

[16] https://factfinder.census.gov/faces/tableservices/jsf/pages/productview.xhtml?src=CF

group are built after 1985.

6 Results

6.1 Arab Name Identifiers

Results for the penalized logit model are presented in Table 5. As expected, the results in Table 5 indicate that relative frequency in Table 1 is a strong indicator of nationality. Names that are strong indicators of being from a country in the Arab League or not are as expected. The strongest predictor of non-Arab status is *jose*.

It is interesting to note that the most common Arabic name, *mohamed*, and its variants are not strong predictors of an individual being from an Arab League country. This should not be surprising, as *mohamed* is a both an Islamic and Arab name found throughout many non-Arab countries. Results in Table 5 do not imply that *mohamed* should not be used to signify an Arab or Mulsim applicant as in (Ahmed and Hammarstedt, 2008) or Bosch et al. (2010). Rather, Table 5 indicates which names are the strongest predictors but does not display which names would have the most influence on landlords.

We also find that predictions from our method would be comparable to other classifiers used in the literature. Bertrand and Mullainathan (2004) identify distinctively black names using the relative frequency of names between racial groups (black or white); distinctively black names are names with the largest ratio of relative frequencies. Figure 6 plots ϕ^* against the log of the ratio of relative frequencies for Arab and non-Arab countries for all names that occur at least once in both sets. The positive relationship between ϕ^* and the log ratio confirms the notion that a ratio-based method similar to Bertrand and Mullainathan (2004) would yield similar classifications to the binomial classifier we describe above.

Using ϕ^* and Equation 1, we can calculate $\Pr(y_n = 1|X_n, \phi^*)$ for buyer and seller names in the assessor data. Unlike Gautier et al. (2009), we do not have access to an Arab research assistant. However, a manual inspection of the names by an Arab undergraduate economics student confirmed probable ethnicity for names above this cutoff. Based on manual inspection, we create the indicator variable $Arab = 1$ for Arab homeowners if $0.35 < \Pr(y_n = 1|X_n, \phi^*)$ and $Arab = 0$ otherwise. In the Appendix, we present similar results when using a cutoff of 0.5.

6.2 Price Effects

Table 6 presents our first set of results.[17] All standard errors are clustered two ways at the quarter and census tract levels. The first column estimates Equation 5 using additively separable census tract and quarter fixed effects. Additively separable fixed effects preclude heterogeneous price trends across census tracts. In the cross-section, properties within 0.3mi of an Arab neighbor sell at a 1.4% discount relative to the rest of the market. Relative to the control group, properties in the treatment group sell at a 1.5% discount. These results indicate that Arab homeowners purchase properties in less expensive parts of town.

In the 180 days after 9/11, properties within 0.3mi of an Arab neighbor sell at a 1.5% premium relative to the rest of the market. During that same time period, properties in the treatment group sell at a 2.0% discount relative to the control group. These results suggest a short-term, local, negative price effect attributable to Arab neighbors caused by the events of 9/11.

Results in Column 2 include interaction terms between the census tract and quarter fixed effects that control for heterogeneous price trends across census tracts.[18] As expected, cross sectional effects attributable to Arab neighbors do not change much but time-varying effects do change. Unlike the results in Column 1, after controlling for heterogeneous census tract price trends, there is no significant difference in price between properties within 0.3mi of an Arab neighbor and other properties in the market post 9/11. However, similar to results in Column 1, there is significant evidence of local price effects attributable to Arab neighbors post 9/11. To wit, relative to the control group, properties in the treatment group sold at a 1.5% discount.

In order to estimate a gross price effect absent any comparison to properties in the control group, Column 3 uses $D^{03}(1 - D^{0.1})$ instead of $D^{0.3}$ in Equation 5. In contrast to the other columns in the table, the coefficient on $D^{0.1} \times Post$ is now interpreted as the gross price effect irrespective of the control group. As the coefficient on $D^{0.3}$ is not statistically different from 0 in Column 2, the coefficients for $D^{0.1}$ are comparable in Columns 2 and 3.

Column 4 tests for longer-term price effects by defining $Post = 1$ if the transaction is 180-365 days after 9/11 and $Post = 0$ otherwise. In contrast to the results in Column 2, there is no significant price difference for properties near Arab neighbors using the 0.1mi and 0.3mi cutoffs.

[17] In the Appendix, we present similar results when first removing observations with outlier residuals from Equation 3.

[18] Figure A1 in the appendix presents a graph of the price trends by census tract.

Comparing the results in Columns 2 and 4, if 9/11 changed preferences for Arab neighbors, these changes were short-term.

It is possible that the results in Columns 1-3 reflect momentum from unobserved price trends specific to properties in the treatment group. Alternatively, price effects in Columns 1-3 are not caused by 9/11 but reflect existing price trends before 9/11. In order to rule out this possibility, we define $Pre = 1$ if the transaction is 0-180 days before 9/11 and $Pre = 0$ otherwise. By doing so, we test for abnormal price effects between March 15, 2001 and September 11, 2001. Although the result for properties within 0.3mi of an Arab neighbor are significant at the 5% level, the coefficient is not economically meaningful. More importantly, there is no significant difference between the treatment and control groups. In conclusion, the results in Column 5 rule out any momentum effects attributable to unobserved price trends common to properties in the treatment group.

6.3 Alternative Ethnic Groups

As mentioned above, we can use the binomial classifier and the Olympic data to classify individuals based on any group of countries. In order to demonstrate this flexibility and create a counterfactual, we identify homeowners from East Asian and Hispanic countries and perform the same analysis. For the East Asian group we use ad hoc list of countries: China, Japan, Mongolia, South Korea, and North Korea. For the Hispanic group, we use all countries in Central and South America. Preliminary results for these two groups is presented in Figure 7. Unlike Figure 1, there does not appear to be any local price effects for either East Asian or Hispanic neighbors.

Results for the East Asian neighbors are presented in Table 7. In the 180 days after 9/11, there does not appear to be any effect. Although, there does appear to be a price effect 180-365 days after 9/11, in unreported results we find this price effect becomes statistically insignificant when using a 0.2mi radius for the control group. Results for Hispanic homeowners are similar to the results for East Asian homeowners and are presented in Table 8. We find little evidence of price effects in the 180 days after 9/11. We find weak evidence of a price effect for Hispanic neighbors in the 180-365 days after 9/11, we find a -0.8% price effect that is statistically significant at the 10% level.

As a whole, the results in Tables 7 and 8 indicate that there were not any significant local price effects attributable to either East Asian of Hispanic neighbors after 9/11. These results are in agreement with survey responses in the HAR that indicated a majority of respondents had favorable views of both groups. Furthermore, the results in Tables 7 and 8 stand in contrast to the results in Table 6 where Arab neighbor were found to have negative local price effects.

6.4 Mosques

Unlike Gautier et al. (2009), Figure 4, indicates that there does not appear to be an area of Seattle where Arab homeowners are concentrated.[19] We present additional evidence using methods in Duranton and Overman (2005) for purchases by Arab homeowners before and after 9/11.[20] Duranton and Overman (2005) use the distribution of pairwise distance as a measure of agglomeration; here, we use the distribution as a measure of clustering of Arab homeowners. Figure 8 presents the distribution of pairwise distances between purchases by Arab homeowners before and after 9/11. As seen in Figure 8, we find no evidence of clustering as the pairwise distances are within the 95% confidence interval in both periods.

Although there is no significant evidence that Arab homeowners live in concentrated areas, it is possible that some Arab homeowners choose to live near mosques or Islamic centers. If that is true, it is possible that the price effects reflect a mosques or Islamic centers effect and not an Arab neighbor effect. This proposition is not outlandish, as Bogin (2012) find prices near mosques in Baltimore, MD decline 17% after 9/11. In order to rule out the effects of mosques and Islamic centers, we include similar cross-sectional and time-varying indicators in the estimating equation. We identify 23 mosques or Islamic centers in the Assessor data that we believe to be in operation before 9/11.[21]

Table 9 presents the results when including the mosques and Islamic center indicators. The price effects attributable to Arab neighbors in Table 9 are comparable to our previously reported results. In contrast to Bogin (2012), we find no significant mosques or Islamic centers effect when allowing for heterogeneous price trends across census tracts. However, we do find large mosques and Islamic center effects when using additively separable fixed tract and quarter fixed effects.

[19] However, we do find significant evidence that East Asian homeowners are concentrated south of downtown Seattle.

[20] The Duranton and Overman (2005) statistic is calculated as the empirical probability mass function for all pairwise distances, d_{ij}, for all $i, j = 1, ..., I$ pairs of members in a subgroup (here, Arab homeowners) of I individuals. We use a probability mass function as we bin all pairwise distances at 0.25mi for smoothing purposes. The 95% confidence interval is calculated using 2,000 random draws of properties.

[21] We were not able to identify any mosque or Islamic center closures post 9/11 in a web search and cannot rule out survivorship bias. The addresses and locations of the mosques and Islamic centers are available from the researchers upon request.

6.5 Multiple Neighbors

It is possible that there can be different price effects depending on the number of Arab neighbors. In order to test for this, we modify the $D^{0.1}$ variable in order to test for this possibility. We create the variable $D^{0.1}_{=1} = 1$ ($D^{0.1}_{\geq 2} = 1$) if there is exactly (more than) 1 Arab neighbor within 0.1mi and 0 otherwise. We then perform the same difference-in-difference analysis using these indicators. We favor these indicators in lieu of a count of Arab neighbors in order to allow for possible non-linear effects.

The results are presented in Table 10. The results indicate that there are significant price effects for more than 1 Arab neighbor. The presence of more than 1 Arab neighbor decreases prices by 2.3% in the 180 days following 9/11, relative to the control group. However, unlike the results above, there does not appear to be any effects for a single Arab neighbor, although the point estimates are negative. Still, the effects for multiple Arab neighbors are larger than the previous estimates for the presence of any Arab neighbor. Furthermore, the results in Table 10 are not attributable to any Arab neighborhood effect as we 1) do not find evidence of Arab homeowners clustering and 2) rule out any price effects attributable to nearby mosques.

7 Conclusion

This study uses transaction data and presents evidence that 9/11 caused a change in homeowner perceptions of Arab neighbors. Specifically, homeowners with Arab neighbors within 0.1mi sold their properties at a 1.5% discount within 180 days of 9/11. We find a discount of 2.5% when 2 or more Arab neighbors. A notable limitation of this study and other comparable studies is that we cannot identify the source of the price declines. Specifically, we cannot disentangle homeowner preferences for Arab neighbors from homeowner expectations of the preferences of potential buyers.

In order to identify ethnicity, we use a supervised learning algorithm trained using Summer Olympic rosters from 1948 to 2012. The algorithm can be used to classify buyer and seller names for various groups in large data sets where manual classification is not possible. In future work, we plan to compare our classifier to manual, crowd-sourced classifiers including the Amazon Mechanical Turk.[22] However, given the non-negligible costs associated with Amazon Mechanical Turk, we

[22] https://www.mturk.com/mturk/

reserve this for future work.

As a falsification test, we also examined price effects attributable to East Asian and Hispanic neighbors. As expected given the survey responses in HAR, we find mixed evidence that the presence of these ethnic groups were associated with any meaningful price effects. The price effects for East Asian are statistically significant, yet the magnitudes are much smaller than the price effects for Arab neighbors. As a whole, the results for Arab neighbors and the two falsification tests present the first evidence that 9/11 changed preferences for Arab neighbors in the market for single-family homes.

In any event, the results indicate that preferences for ethnic groups can be changed by significant events. Although we document a negative change in preferences, we are hopeful that positive changes in preferences for various ethnic groups can be obtained.

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Tables and Figures

Table 1: 20 Most Frequent Arab League and Non-Arab League Names

Arab League		Non-Arab League	
Name	Count	Name	Count
mohamed	557	peter	920
al-	520	jose	864
el-	452	john	859
ahmed	230	de	817
abdel	186	kim	795
ali	180	van	748
ibrahim	102	david	620
ben	101	juan	581
hassan	81	maria	580
sayed	79	carlos	542
abdul	76	lee	540
mahmoud	75	paul	533
abdullah	71	michael	507
khaled	60	robert	492
moustafa	59	luis	479
youssef	55	martin	451
omar	49	jan	438
hussain	48	daniel	407
saleh	47	aleksandr	395
said	42	jean-	393

Table 1 displays the total counts for each name for Arab League and non Arab League countries. Names are taken from Summer Olympic rosters 1948-2102. There are 90,636 Summer Olympians from 221 countries.

Table 2: Summary Statistics for Transaction Data

Statistic	Min	Mean	Median	Max	St. Dev.
Sale Price in $1,000s	45.000	233.066	195.000	1,700.000	138.001
Construction Year	1900	1965.806	1970	2002	26.420
Sale Year	1990	1996.769	1997	2002	3.411
Square Footage	480	1,958.995	1,850	4,850	761.331
Bedrooms	1	3.310	3	6	0.838
Bathrooms	1	1.466	1	3	0.578

Table 2 displays summary statistics for the 265,255 transactions in the King County Assessor's data.

Table 3: Control and Treatment Counts Pre and Post 9/11

	$Post = 0$	$Post = 1$	Row Sum
$D^{0.1} = 1$	6,451	459	6,910
$D^{0.3}(1 - D^{0.1}) = 1$	30,926	2,088	33,014
Column Sum	37,377	2,547	39,924

Table 3 displays the total number of transactions near Arab neighbors, before and after 9/11. The variable $D^{0.1} = 1$ ($D^{0.3}$=1) if the transaction is within 0.1mi (0.3mi) of an Arab homeowner. The variable $Post = 1$ if the transaction is 0-180 days after 9/11.

Table 4: Control Variables in the Treatment and Control Group

Variable	Mean Treatment	Mean Control	t-statistic	p-value
log(sqft)	7.495	7.484	2.23	0.026
Bedrooms	3.332	3.309	2.125	0.034
Bathrooms	1.546	1.478	-8.727	0.00
Construction Year	1969.769	1966.047	10.327	0 .00

Table 4 presents t-statistics for the differnece in means of the control variables for transactions in the control group to transactions in the treatment group. The control group are transaction 0.1-0.3 miles from an Arab homeowner. The treatment group are transactions within 0.1mi of an Arab homeowner. There are 33,014 transactions in the control group and 6,910 transactions in the treatment group. Standard errors are heteroskedasticity robust.

Table 5: Strong Predictors from the Penalized Logistic Model

Arab League			Non-Arab League		
Name	ϕ^*	Count	Name	ϕ^*	Count
fouad	9.838	18	diouf	-8.301	13
salem	9.091	34	akhtar	-8.262	14
khalifa	8.963	27	carolyn	-7.325	20
khaled	8.961	61	teodoro	-6.416	13
mourad	8.713	14	kerstin	-6.311	48
nabil	8.696	21	patrick	-6.124	196
hicham	8.462	17	hernan	-6.058	31
kamel	8.397	27	ud-	-6.008	22
riadh	8.077	11	cedric	-5.806	31
fawzi	8.027	12	diop	-5.604	19
yahia	7.905	11	khan	-5.54	57
jamal	7.894	16	singh	-5.287	359
ramadan	7.878	18	abdoulaye	-5.175	18
khamis	7.862	24	nunez	-5.16	32
tarek	7.73	17	keita	-5.14	24
younes	7.671	12	reza	-5.137	47
abou	7.599	20	eddie	-5.075	53
adel	7.481	29	larry	-4.613	62
alaa	7.321	12	filho	-4.506	40
gamal	7.242	14	syed	-4.301	20

Table 5 displays the 20 names that are the strongest predictors of being from an Arab League and the 20 names that are the strongest predictors of not being from an Arab League country. ϕ^* minimizes the ℓ_1 penalized likelihood model in Equation 2. The probability of being from an Arab League country is given by $\Pr(y_n = 1|X_n, \phi) = \frac{e^{\phi_0 + \sum_p X_{np}\phi_p}}{1+e^{\phi_0 + \sum_p X_{np}\phi_p}}$. Counts indicate the total number of times a given name is found in the Olympic rosters from 1948-2012.

Table 6: Price Effects for Arab Neighbors post 9/11

	Model 1	Model 2	Model 3	Model 4	Model 5
$D^{0.1}$	-0.015^{**}	-0.011^{*}	-0.029^{***}	-0.012^{**}	-0.012^{**}
	(0.006)	(0.006)	(0.008)	(0.005)	(0.006)
$D^{0.3}$	-0.014^{***}	-0.018^{***}		-0.017^{***}	-0.018^{***}
	(0.005)	(0.006)		(0.006)	(0.006)
$D^{0.3} \times (1 - D^{0.1})$			-0.018^{***}		
			(0.006)		
$D^{0.1} \times Post$	-0.020^{***}	-0.015^{***}	-0.015^{***}	-0.002	0.000
	(0.005)	(0.004)	(0.003)	(0.011)	(0.008)
$D^{0.3} \times Post$	0.015^{***}	-0.001		-0.004	0.006^{**}
	(0.004)	(0.002)		(0.004)	(0.003)
$D^{0.3} \times (1 - D^{0.1}) \times Post$			-0.001		
			(0.002)		
Num. obs.	265255	265255	265255	265255	265255
R^2	0.794	0.822	0.822	0.822	0.822
Tract + Quarter FE	✓				
Tract × Quarter FE		✓	✓	✓	✓
Post 9/11 Window	0-180 Days	0-180 Days	0-180 Days	180-365 Days	-180-0 Days

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 6 displays results for the price effects attributable to Arab neighbors following September 11, 2001. The variable $D^{0.1} = 1$ ($D^{0.3}$=1) if the transaction is within 0.1mi (0.3mi) of an Arab homeowner. The variable $Post = 1$ if the transaction is 0-180 days, 0-90 days, or 180-365 days after September 11, 2001. All regressions include log square footage, bedrooms, bathrooms, and construction year as control variables. Standard errors are clustered at the census tract and quarter levels.

Table 7: Price Effects for East Asian Neighbors post 9/11

	Model 1	Model 2	Model 3	Model 4	Model 5
$D^{0.1}$	-0.004	-0.002	-0.018^{***}	-0.001	-0.002
	(0.002)	(0.002)	(0.005)	(0.002)	(0.003)
$D^{0.3}$	-0.013^{***}	-0.015^{***}		-0.015^{***}	-0.016^{***}
	(0.004)	(0.004)		(0.004)	(0.004)
$D^{0.3} \times (1 - D^{0.1})$			-0.015^{***}		
			(0.004)		
$D^{0.1} \times Post$	-0.001	0.005	-0.001	-0.011^{***}	-0.004
	(0.009)	(0.008)	(0.009)	(0.004)	(0.004)
$D^{0.3} \times Post$	0.009^{**}	-0.005^{**}		-0.018^{***}	-0.001
	(0.004)	(0.002)		(0.005)	(0.003)
$D^{0.3} \times (1 - D^{0.1}) \times Post$			-0.005^{**}		
			(0.002)		
Num. obs.	265255	265255	265255	265255	265255
R^2	0.795	0.822	0.822	0.822	0.822
Tract + Quarter FE	✓				
Tract × Quarter FE		✓	✓	✓	✓
Post 9/11 Window	0-180 Days	0-180 Days	0-180 Days	180-365 Days	-180-0 Days

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 7 displays results for the price effects attributable to East Asian neighbors following September 11, 2001. The variable $D^{0.1} = 1$ ($D^{0.3}$=1) if the transaction is within 0.1mi (0.3mi) of an East Asian homeowner. The variable $Post = 1$ if the transaction is 0-180 days, 0-90 days, or 180-365 days after September 11, 2001. All regressions include log square footage, bedrooms, bathrooms, and construction year as control variables. Standard errors are clustered at the census tract and quarter levels.

Table 8: Price Effects for Hispanic Neighbors post 9/11

	Model 1	Model 2	Model 3	Model 4	Model 5
$D^{0.1}$	-0.011^{***}	-0.011^{***}	-0.041^{***}	-0.010^{***}	-0.010^{***}
	(0.002)	(0.002)	(0.005)	(0.002)	(0.002)
$D^{0.3}$	-0.029^{***}	-0.031^{***}		-0.031^{***}	-0.031^{***}
	(0.004)	(0.004)		(0.004)	(0.004)
$D^{0.3} \times (1 - D^{0.1})$			-0.031^{***}		
			(0.004)		
$D^{0.1} \times Post$	0.002	0.004	-0.022^{***}	-0.008^{*}	-0.005^{**}
	(0.004)	(0.002)	(0.003)	(0.004)	(0.002)
$D^{0.3} \times Post$	-0.002	-0.026^{***}		-0.023^{***}	-0.015^{***}
	(0.005)	(0.002)		(0.003)	(0.004)
$D^{0.3} \times (1 - D^{0.1}) \times Post$			-0.026^{***}		
			(0.002)		
Num. obs.	265255	265255	265255	265255	265255
R^2	0.795	0.822	0.822	0.822	0.822
Tract + Quarter FE	✓				
Tract × Quarter FE		✓	✓	✓	✓
Post 9/11 Window	0-180 Days	0-180 Days	0-180 Days	180-365 Days	-180-0 Days

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 8 displays results for the price effects attributable to Hispanic neighbors following September 11, 2001. The variable $D^{0.1} = 1$ ($D^{0.3}$=1) if the transaction is within 0.1mi (0.3mi) of an Hispanic homeowner. The variable $Post = 1$ if the transaction is 0-180 days, 0-90 days, or 180-365 days after September 11, 2001. All regressions include log square footage, bedrooms, bathrooms, and construction year as control variables. Standard errors are clustered at the census tract and quarter levels.

Table 9: Price Effects for Arab Neighbors and Mosques post 9/11

	Model 1	Model 2	Model 3	Model 4	Model 5
$D^{0.1}$	-0.015^{***}	-0.011^{*}	-0.028^{***}	-0.011^{**}	-0.012^{**}
	(0.003)	(0.006)	(0.008)	(0.005)	(0.006)
$D^{0.3}$	-0.014^{***}	-0.018^{***}		-0.017^{***}	-0.018^{***}
	(0.002)	(0.006)		(0.006)	(0.006)
$D^{0.3}(1-D^{0.3})$			-0.018^{***}		
			(0.006)		
$M^{0.1}$	0.001	0.000	-0.035^{***}	-0.002	-0.002
	(0.010)	(0.016)	(0.013)	(0.015)	(0.015)
$M^{0.3}$	-0.036^{***}	-0.035		-0.033	-0.033
	(0.005)	(0.025)		(0.024)	(0.024)
$M^{0.3}(1-M^{0.3})$			-0.035		
			(0.025)		
$D^{0.1} \times Post$	-0.018	-0.013^{***}	-0.015^{***}	-0.002	0.000
	(0.012)	(0.004)	(0.004)	(0.012)	(0.008)
$D^{0.3} \times Post$	0.013^{**}	-0.002		-0.003	0.006^{**}
	(0.006)	(0.003)		(0.005)	(0.003)
$D^{0.3}(1-D^{0.3}) \times Post$			-0.002		
			(0.003)		
$M^{0.1} \times Post$	-0.103^{**}	-0.014	-0.009	0.046^{***}	0.047^{***}
	(0.051)	(0.017)	(0.008)	(0.017)	(0.016)
$M^{0.3} \times Post$	0.034^{**}	0.004		-0.034^{**}	-0.034^{**}
	(0.017)	(0.024)		(0.015)	(0.015)
$M^{0.3}(1-M^{0.3}) \times Post$			0.004		
			(0.024)		
Num. obs.	265255	265255	265255	265255	265255
R^2	0.794	0.822	0.822	0.822	0.822
Tract + Quarter FE	✓				
Tract × Quarter FE		✓	✓	✓	✓
Post 9/11 Window	0-180 Days	0-180 Days	0-180 Days	180-365 Days	-180-0 Days

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 9 displays results for the time-varying effect of Arab neighbors and mosques following September 11, 2000. The variable $D^{0.1} = 1$ ($D^{0.3}$=1) if the transaction is within 0.1mi (0.3mi) of an Arab homeowner. The variable $M^{0.1} = 1$ ($M^{0.3}$=1) if the transaction is within 0.1mi (0.3mi) of a Mosque or Islamic Center. The variable $Post = 1$ if the transaction is 0-180 days, 0-90 days, or 180-365 days after September 11, 2001. All regressions include log square footage, bedrooms, and bathrooms as control variables. Standard errors are clustered at the census tract and quarter levels.

Table 10: Price Effects for Multiple Arab Neighbors post 9/11

	Model 1	Model 2	Model 3	Model 4	Model 5
$D^{0.1}_{=1}$	-0.022^{***}	-0.018^{***}	-0.036^{***}	-0.017^{***}	-0.019^{***}
	(0.004)	(0.006)	(0.009)	(0.004)	(0.006)
$D^{0.1}_{\geq 2}$	-0.001	0.001	-0.017^{**}	-0.001	0.002
	(0.005)	(0.007)	(0.008)	(0.005)	(0.007)
$D^{0.3}$	-0.014^{***}	-0.018^{***}		-0.018^{***}	-0.019^{***}
	(0.002)	(0.006)		(0.002)	(0.006)
$D^{0.3} \times (1 - D^{0.1})$			-0.018^{***}		
			(0.006)		
$D^{0.1}_{=1} \times Post$	-0.015	-0.007	-0.008	-0.009	0.009
	(0.013)	(0.005)	(0.006)	(0.010)	(0.007)
$D^{0.1}_{\geq 2} \times Post$	-0.020	-0.023^{***}	-0.025^{***}	0.014	-0.016
	(0.023)	(0.009)	(0.007)	(0.015)	(0.011)
$D^{0.3} \times Post$	0.014^{**}	-0.002		-0.002	0.006^{*}
	(0.006)	(0.002)		(0.007)	(0.003)
$D^{0.3} \times (1 - D^{0.1}) \times Post$			-0.002		
			(0.002)		
Num. obs.	265255	265255	265255	265255	265255
R^2	0.794	0.822	0.822	0.822	0.822
Tract + Quarter FE	✓				
Tract × Quarter FE		✓	✓	✓	✓
Post 9/11 Window	0-180 Days	0-180 Days	0-180 Days	180-365 Days	-180-0 Days

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 10 displays results for the price effects attributable to Arab neighbors following September 11, 2001. The variable $D^{0.1}_{=1} = 1$ ($D^{0.1}_{\geq 1} = 1$) if the transaction is within 0.1mi of exactly (more than) 1 Arab neighbor. The variable $D^{0.1} = 1$ ($D^{0.3} = 1$) if the transaction is within 0.1mi (0.3mi) of an Arab neighbor. The variable $Post = 1$ if the transaction is 0-180 days, 0-90 days, or 180-365 days after September 11, 2001. All regressions include log square footage, bedrooms, bathrooms, and construction year as control variables. Standard errors are clustered at the census tract and quarter levels.

Figure 1: Price Gradient Near Arab Homeowners

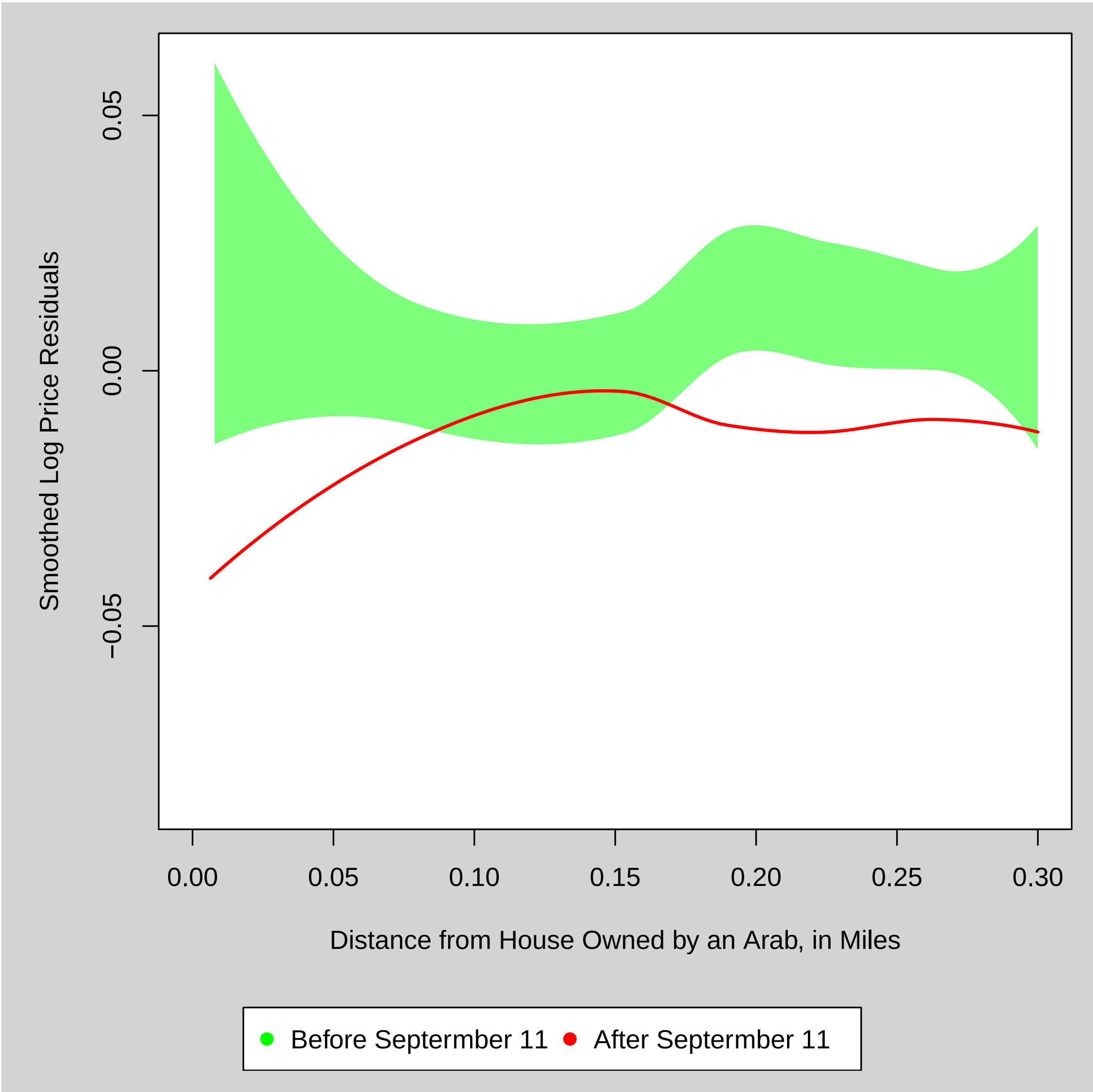

Figure 1 displays a local polynomial estimation of residuals from transactions less than 0.1 miles from an Arab neighbor as a function of distance from the nearest Arab neighbor. The 95% confidence interval for transactions 0-180 days before 9/11 is presented in green, and the point estimates for transactions 0-180 days after 9/11 are displayed in red.

Figure 2: Treatment Area and Control Area

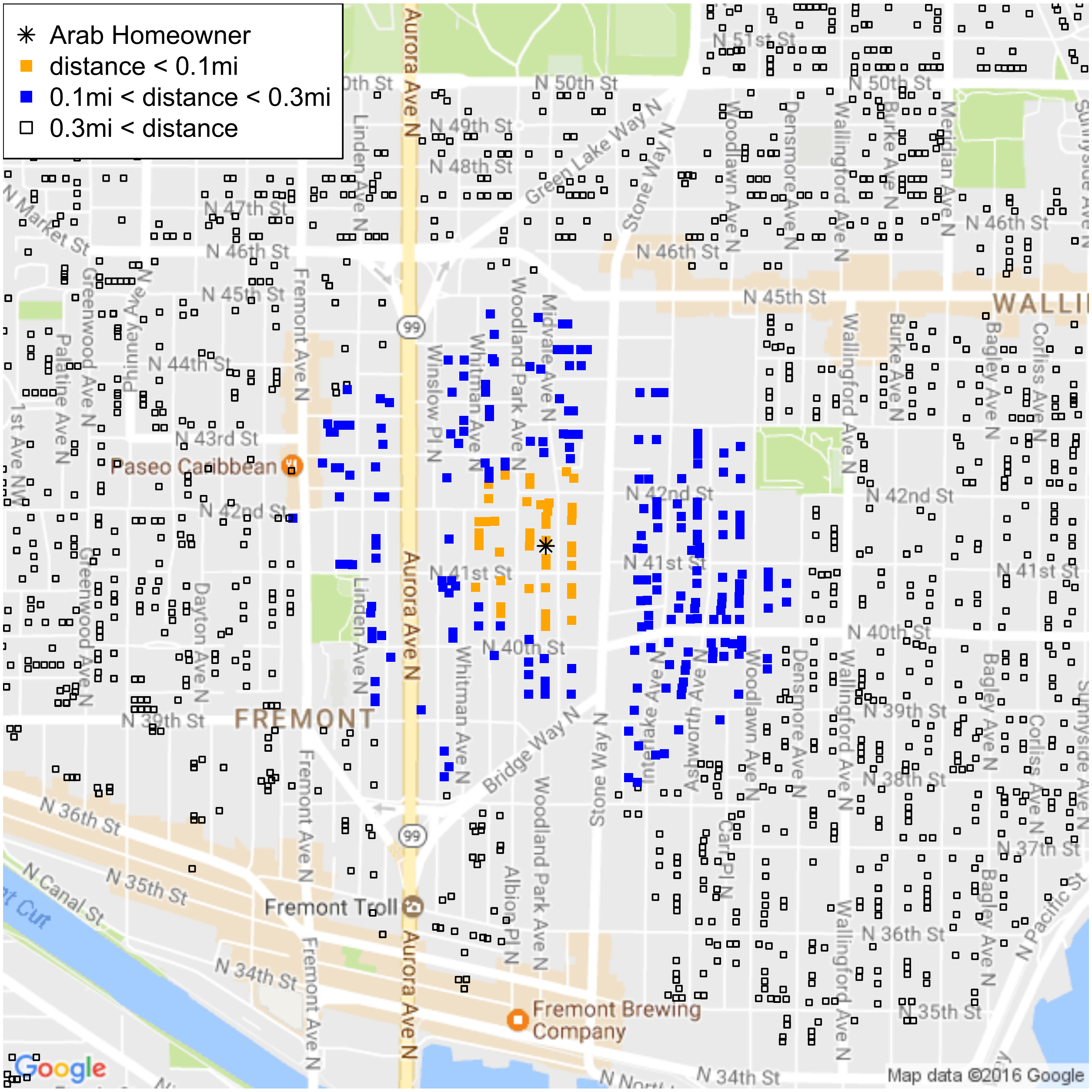

Figure 2 displays an example of a random property, properties less than 0.1 miles, properties 0.1-0.3 miles, and properties more than 0.3 miles away.

Figure 3: Olympic Roster Names

Arab League Names | Non-Arab League Names

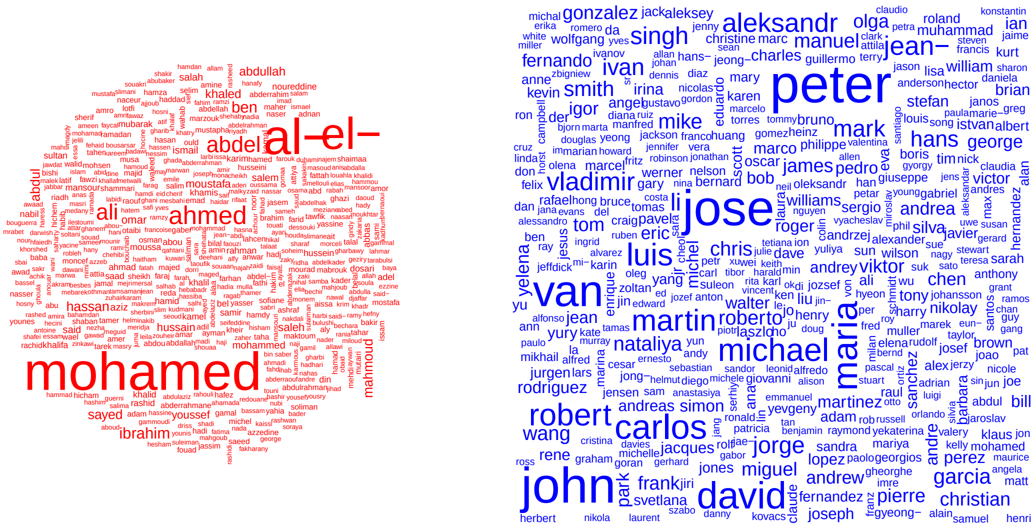

Figure 3 displays the 300 most frequent names on the Olympic rosters for each country. More frequent names are indicated with a larger font.

Figure 4: Location of Arab Homeowners

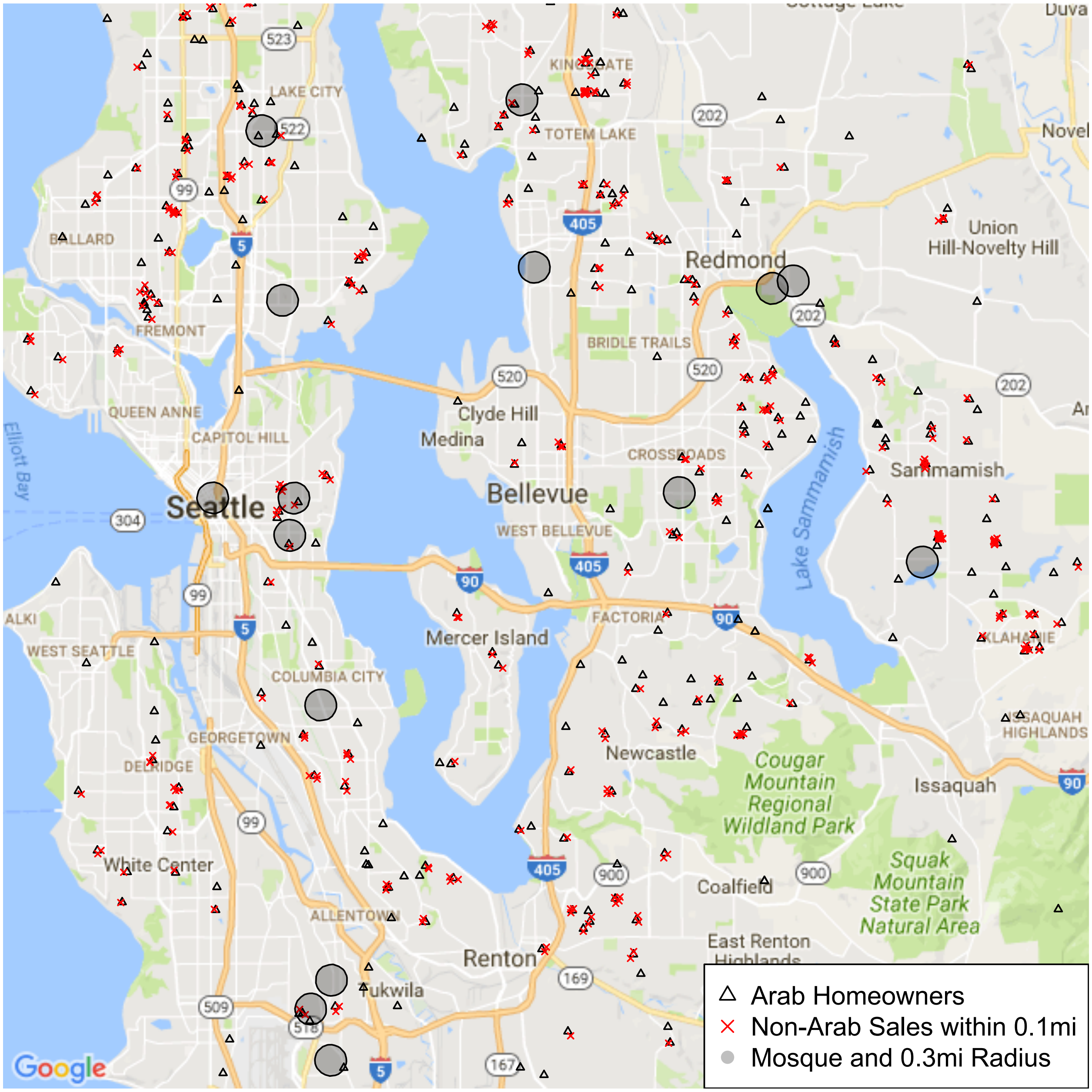

Figure 4 displays a significant area of the study area in the vicinity of Seattle, Washington. Figure 4 displays Arab homeowners on 9/11, transactions by Non-Arab homeowners within 0.1 miles of an Arab neighbor 0-180 days after 9/11, and mosques with a 0.3 mile radius.

Figure 5: Treatment Area and Control Control Variables

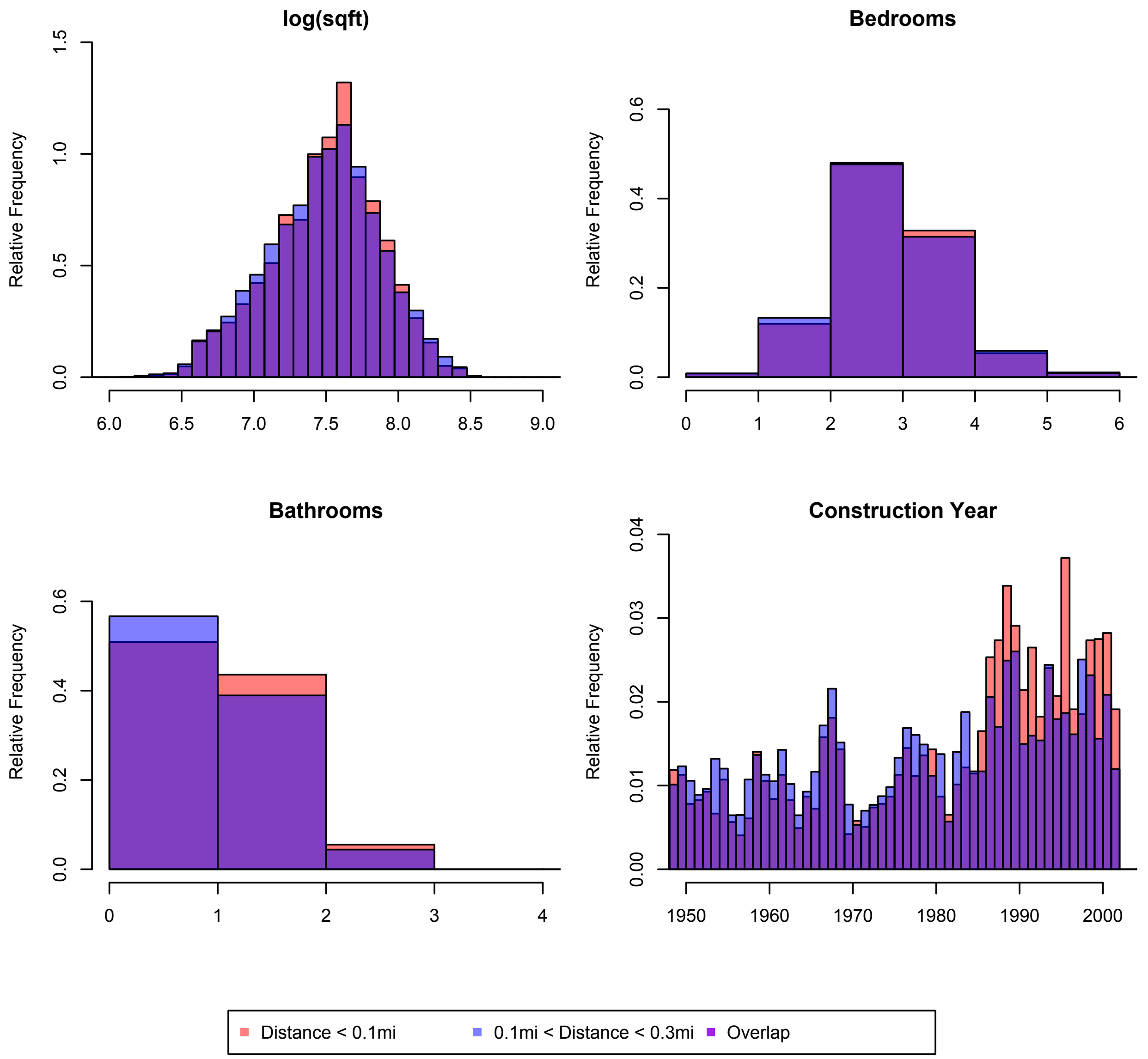

Figure 5 displays histograms of the control variables used in the hedonic regressions for transactions in the control and treatment group. The control group includes all transactions 0.1-0.3 miles from an Arab neighbor, and the treatment group includes all transactions less than 0.1 miles from an Arab neighbor.

Figure 6: Logit Coefficients and Relative Frequency

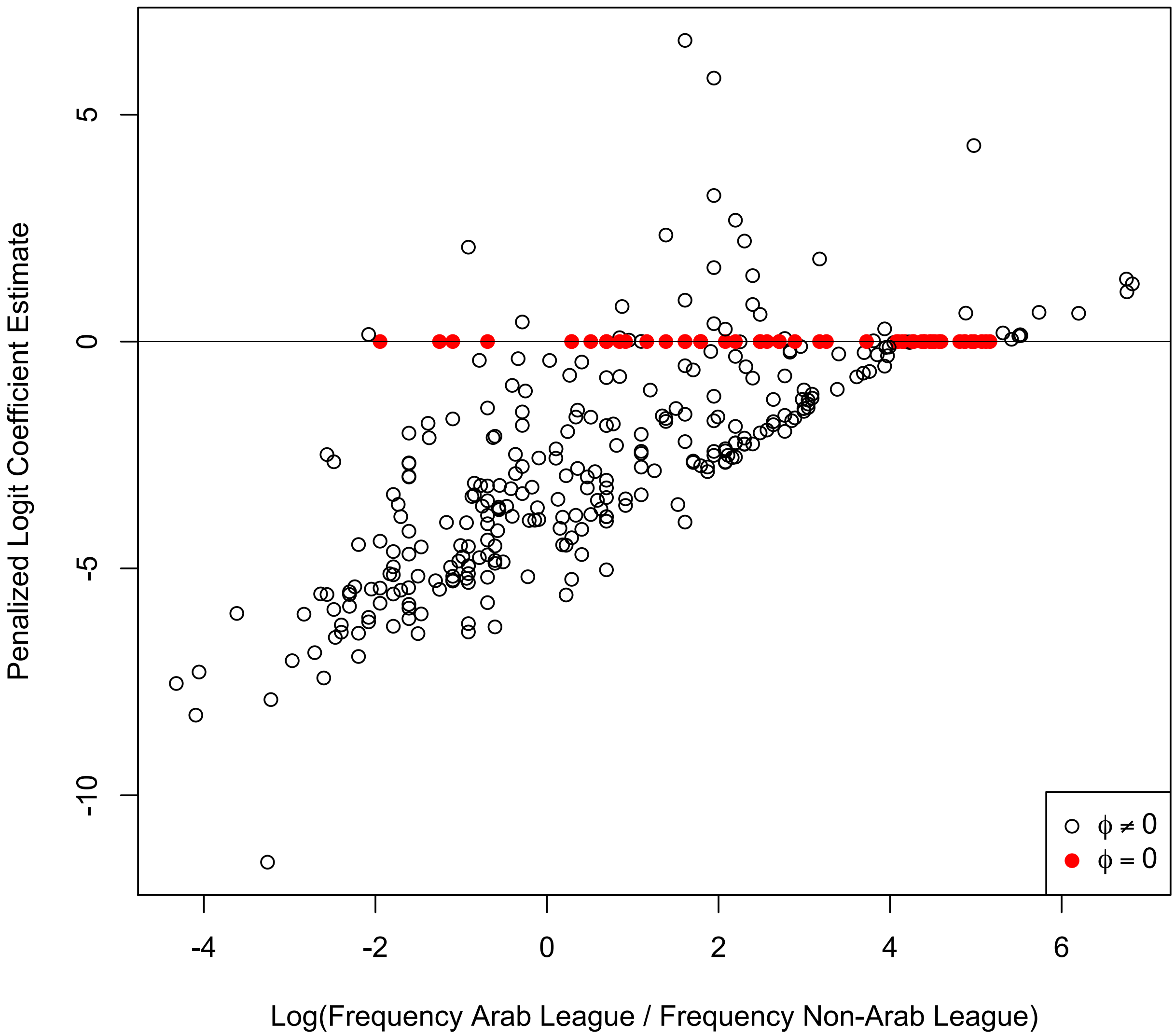

Figure 6 displays the coefficients of ϕ^* relative to the log of the relative frequencies of names between the Arab Leage and non Arab League countries. ϕ^* is the set of coefficients that minimize the penalized liklelihood in Equation 2. The number of times name p is found in the full names of Olympians from group g, $N_g(p)$, divided by the total number of Olympians in group g, N_g. The relative frequency for name p in group g is equal to $f_g(p) = \frac{N_g(p)}{N_g}$. The log relative frequency is equal to $\log(f_{ArabLeague}(p)) - \log(f_{NonArabLeague}(p))$.

Figure 7: Alternative Ethnic Group Price Gradients

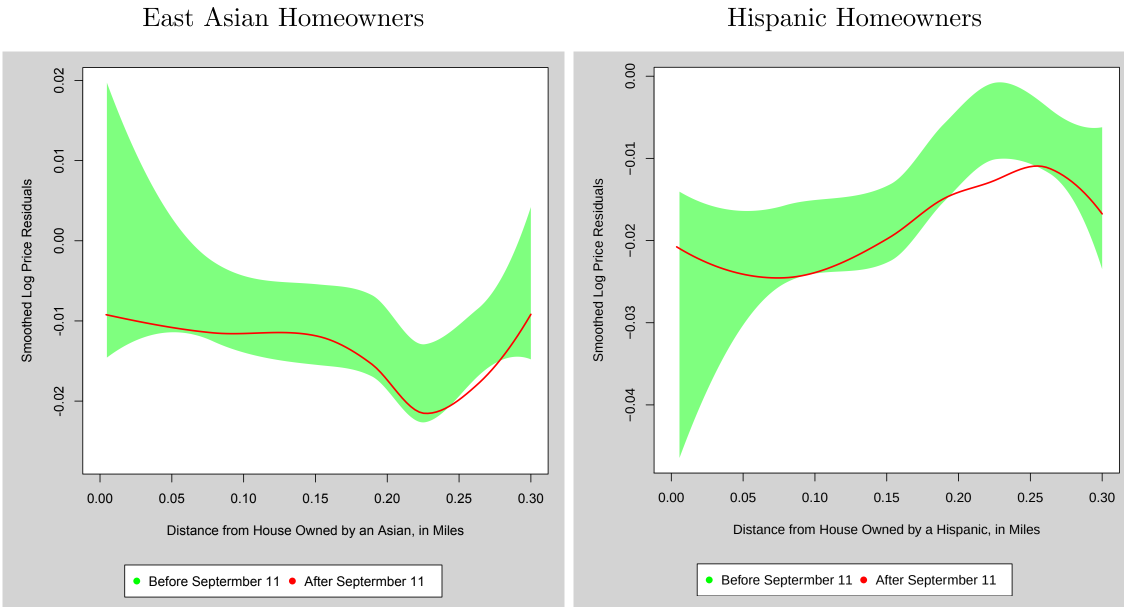

Figure 7 displays a local polynomial estimation of residuals from transactions less than 0.1 miles from East Asian and Hispanic neighbor as a function of distance from the nearest East Asian or HIspanic neighbor. The 95% confidence interval for transactions 0-180 days before 9/11 is presented in green, and the point estimates for transactions 0-180 days after 9/11 are displayed in red.

Figure 8: Concentration of Arab Homeowners

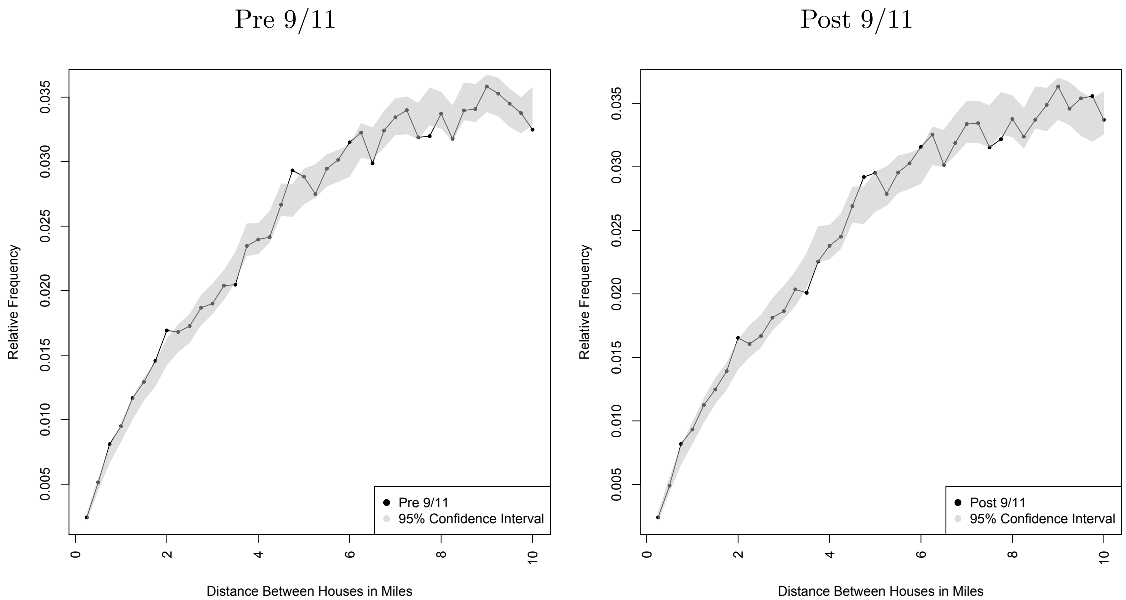

Figure 8 presents the probability mass function of pairwise distances for each Arab homeowner in the data before and after 9/11. Distances are grouped into 0.25mi bins. Confidence intervals are calculated using 2,000 random samples drawn from all transactions in the respective time periods.

Appendix

Table A1: Price Effects for Arab Neighbors post 9/11, Alternative Control Group

	Model 1	Model 2	Model 3	Model 4	Model 5
$D^{0.1}$	-0.015^{***}	-0.011^{*}	-0.024^{***}	-0.011^{*}	-0.011^{*}
	(0.006)	(0.006)	(0.007)	(0.005)	(0.006)
$D^{0.2}$	-0.011^{**}	-0.013^{**}		-0.014^{**}	-0.014^{***}
	(0.005)	(0.005)		(0.005)	(0.005)
$D^{0.2} \times (1 - D^{0.1})$			-0.013^{**}		
			(0.006)		
$D^{0.1} \times Post$	-0.015^{***}	-0.011^{***}	-0.015^{***}	-0.010	-0.006
	(0.004)	(0.003)	(0.003)	(0.009)	(0.006)
$D^{0.2} \times Post$	0.008^{**}	-0.004^{**}		0.009^{***}	0.013
	(0.004)	(0.002)		(0.003)	(0.008)
$D^{0.2} \times (1 - D^{0.1}) \times Post$			-0.004^{**}		
			(0.002)		
Num. obs.	265255	265255	265255	265255	265255
R^2	0.794	0.822	0.822	0.822	0.822
Tract + Quarter FE	✓				
Tract × Quarter FE		✓	✓	✓	✓
Post 9/11 Window	0-180 Days	0-180 Days	0-180 Days	180-365 Days	-180-0 Days

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A1 displays results for the price effects attributable to Arab neighbors following September 11, 2001. The variable $D^{0.1} = 1$ ($D^{0.2}$=1) if the transaction is within 0.1mi (0.2mi) of an Arab homeowner. The variable $Post = 1$ if the transaction is 0-180 days, 0-90 days, or 180-365 days after September 11, 2001. All regressions include log square footage, bedrooms, bathrooms, and construction year as control variables. Standard errors are clustered at the census tract and quarter levels.

Table A2: Price Effects for Arab Neighbors post 9/11, 0.5 Cutoff

	Model 1	Model 2	Model 3	Model 4	Model 5
$D^{0.1}$	-0.015^{**}	-0.012^{**}	-0.030^{***}	-0.013^{**}	-0.013^{**}
	(0.006)	(0.005)	(0.008)	(0.005)	(0.006)
$D^{0.3}$	-0.017^{***}	-0.018^{***}		-0.018^{***}	-0.019^{***}
	(0.005)	(0.006)		(0.006)	(0.006)
$D^{0.1} \times (1 - D^{0.3})$			-0.018^{***}		
			(0.006)		
$D^{0.1} \times Post$	-0.018^{***}	-0.015^{***}	-0.012^{***}	0.006	-0.001
	(0.003)	(0.004)	(0.003)	(0.007)	(0.006)
$D^{0.3} \times Post$	0.017^{***}	0.003		-0.001	0.007^{**}
	(0.004)	(0.002)		(0.005)	(0.003)
$D^{0.1} \times (1 - D^{0.3}) \times Post$			0.003		
			(0.002)		
Num. obs.	265255	265255	265255	265255	265255
R^2	0.794	0.822	0.822	0.822	0.822
Tract + Quarter FE	✓				
Tract × Quarter FE		✓	✓	✓	✓
Post 9/11 Window	0-180 Days	0-180 Days	0-180 Days	180-365 Days	-180-0 Days

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A2 displays results for the time-varying effect of Arab neighbors following September 11, 2001. Arab neighbors are identified using ϕ^*, Equation 1 and a probability cutoff of 0.5. The variable $D^{0.1} = 1$ ($D^{0.3}$=1) if the transaction is within 0.1mi (0.3mi) of an Arab homeowner. The variable $Post = 1$ if the transaction is 0-180 days, 0-90 days, or 180-365 days after September 11, 2001. All regressions include log square footage, bedrooms, bathrooms, and construction year as control variables. Standard errors are clustered at the census tract and quarter levels.

Table A3: Price Effects for Arab Neighbors post 9/11, Residual Filter

	Model 1	Model 2	Model 3	Model 4	Model 5
$D^{0.1}$	-0.014^{**}	-0.010^{*}	-0.026^{***}	-0.011^{**}	-0.012^{**}
	(0.006)	(0.005)	(0.008)	(0.005)	(0.006)
$D^{0.3}$	-0.012^{**}	-0.015^{***}		-0.015^{***}	-0.016^{***}
	(0.005)	(0.005)		(0.005)	(0.005)
$D^{0.1} \times (1 - D^{0.3})$			-0.015^{***}		
			(0.005)		
$D^{0.1} \times Post$	-0.018^{***}	-0.014^{***}	-0.017^{***}	-0.003	0.002
	(0.005)	(0.004)	(0.002)	(0.011)	(0.007)
$D^{0.3} \times Post$	0.011^{***}	-0.002		-0.002	0.003
	(0.004)	(0.003)		(0.004)	(0.003)
$D^{0.1} \times (1 - D^{0.3}) \times Post$			-0.002		
			(0.002)		
Num. obs.	263931	263931	263931	263931	263931
R^2	0.794	0.822	0.822	0.822	0.822
Tract + Quarter FE	✓				
Tract × Quarter FE		✓	✓	✓	✓
Post 9/11 Window	0-180 Days	0-180 Days	0-180 Days	180-365 Days	-180-0 Days

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A3 displays results for the time-varying effect of Arab neighbors following September 11, 2001. Observations with the largest 1% of absolute value of residuals from Equation 3 are removed. The variable $D^{0.1} = 1$ ($D^{0.3}$=1) if the transaction is within 0.1mi (0.3mi) of an Arab homeowner. The variable $Post = 1$ if the transaction is 0-180 days, 0-90 days, or 180-365 days after September 11, 2001. All regressions include log square footage, bedrooms, bathrooms, and construction year as control variables. Standard errors are clustered at the census tract and quarter levels.

Figure A1: Price Index by Census Tract

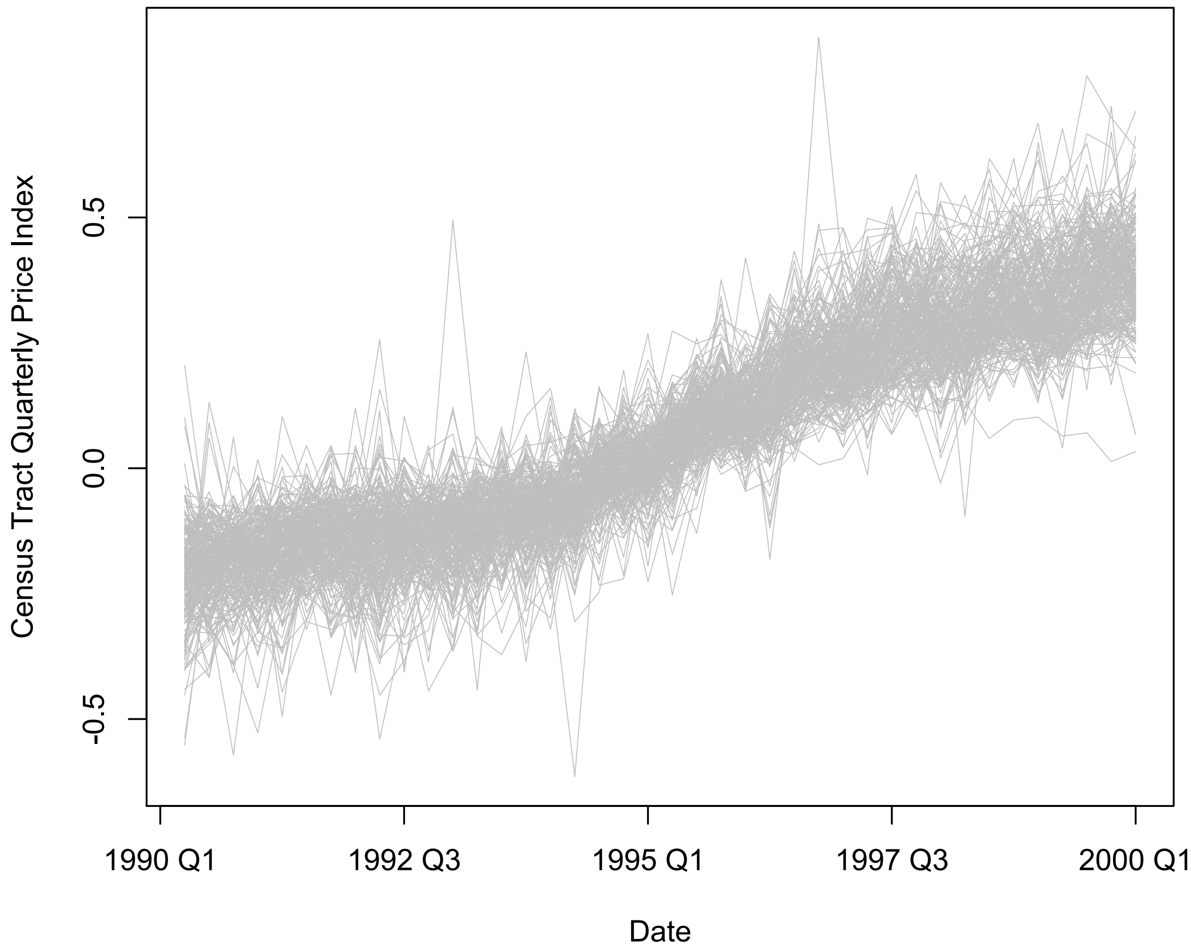

Figure A1 presents the quarterly hedonic price index for each of the census tracts in the study. Each price index is normalized to sum to 0 across the sample period.