House Prices and Individual Perceptions of Terrorism in the Wake of September 11th

Adam Nowak*
West Virginia University

Juan Sayago-Gomez †
West Virginia University

December 28, 2016

Abstract

Using emails from fictitious potential rentors, Ahmed and Hammarstedt (2008) document that landlords are less likely to call back a rental applicant if the applicant's sends a strong signal that he or she is a Muslim. This paper is interested in asking if homeowners were less willing to live next to individuals with ethnically Arab names in the wake of the September 11th attacks. Using Olympic rosters for 221 countries, we create a binomial classifier using a supervised learning algorithm that can assign names to country of origin in a probabilistic manner. Using this, we classify buyer and seller names in King County Assessor data as either Arab or non-Arab. After identifying homeowners in this manner, we estimate the effect of Arab homeowners on nearby property prices immediately before and immeidately after 9/11. Specifically, homeowners with Arab neighbors within 0.1 miles sold their properties at discounts between 1.6% to 2.1% within 180 days of 9/11; properties sold within 60 days of 9/11 were sold between 3.5% and 4.6%.

JEL Codes: R21
Key Words: residential house prices, terrorism, September 11th
We are grateful to Omar Alothimeen for his insights. All errors are our own.

*Department of Economics, College of Business and Economics, PO Box 6025, Morgantown WV 26506-6025; email: adam.nowak@mail.wvu.edu;

†Department of Economics, College of Business and Economics, PO Box 6025, Morgantown WV 26506-6025; email: jsayago@mix.wvu.edu

1 Introduction

On September 11, 2001, the United States was forever changed. 19 terrorists from 4 Arab countries - Egypt, Lebanon, Saudi Arabia and the United Arab Emirates - crashed two airplanes into the Twin Towers in New York City. In the 30 days after 9/11, the Anti Defimation League recorded 12 instances of either anti-Arab or anti-Muslim violence.[1] To some individuals, the expectations of a similar attack in the near-future was a non-trivial event. This study is interested in determining whether or not individual homeowners reacted in a similar manner. Specifically, we are interested in asking whether or not homeowners became irrationally fearful of living next door to individuals with ethnically Arab names, hereafter *Arab*. We examine this hypothesis by examining sale prices in the vicinity Arab homeowners immediately before and after 9/11. To the best of our knowledge, this is the first study to examine the effects of 9/11 using transaction level data in the market for single-family homes.

Of course, having an ethnically Arab name is neither a sufficient nor a necessary condition for an individual to be a terrorist. However, there is also no reason that homeowners form expectations based solely on scientific reasoning. Still, given the long-term nature of homeownership, homeowner expectations play a large role in valuations. Although there is onconclusive evidence linking the location of registered sexual offenders to the location of future sexual crimes - Agan (2011) and Prescott and Rockoff (2008) - both Linden and Rockoff (2008) and Pope (2008) find evidence that the arrival of a registered sexual offender is associated with a significant decrease in property prices in the immediate vicinity.

Previous research has shown discrimination against Muslims in the real estate market. Using email communication in an experimental setting, Ahmed and Hammarstedt (2008) find that landlords are less likely to rent to individuals if they believe that an individual is a Muslim. In their study, it is assumed that landlords can identify the ethnicity based on the name of a rental applicant. Ahmed and Hammarstedt (2008) create a name, *Mohammed Rashid*, they believe sends a strong signal about ethnicity and religion to landlords. We extend their research using transaction data and investigate the impact of Arab homeowners on nearby property prices.

Classifying names as Arab or non-Arab can be done using a supervised learning algorithm. Given the algorithm, new, unlabeled names can be classified into 1 of the 2 groups. In order to

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

train the algorithm, we use the entire set of Summer Olympians from all countries beginning 1948 and ending 2012. Because the names of the Olympians are associated to a country and not an ethnic group, it is necessary to group countries into an Arab group and a non-Arab group. We propose labeling a country as Arab if the country is a member of the Arab League. The Arab League is an organization founded in 1945 with the general mission of promoting the interests of Arab countries. Of course, given the inextricable link between the 9/11 attacks and Islam, it is also possible to label the countries as Islamic and non-Islamic. However, the Arab world consists of many religions and is not solely populated by Muslims.

Several challenges arise when a large number of explanatory variables - names - are used in a binomial classifier. Using a a bag-of-words approach, it is possible to ex-ante specify a list of names that the researcher believes are indicative of one group. This is the approach taken in Ahmed and Hammarstedt (2008). In the application at hand, the researcher would specify a list of names believed to be from Arab countries. Alternatively, it is also possible to use labeled data and attach positive (negative) weights to names are more (less) associated with Arab countries. However, doing so would require assigning weights to thousands of names using a comparable number of observations. With this in mind, we use a penalized likelihood estimation procedure that has superior out-of-sample performance in a high-dimensional setting. Using this methodology, we can sift through thousands of names and produce weights that can be used to probabilistically classify homeowners as Arab or non-Arab group.

The outline of the paper is as follows. Section 2 provides a literature review of terrorism, housing market impacts and textual analysis. Section 3 describes our data. Section 4 describes the multinomial inverse regression procedure of Taddy (2013) and its application to the data on hand. Section 5 provides the results for the housing prices. Section 6 concludes.

2 Literature Review

Previous research has investigated the relationship between violence and economic variables. In one of the earliest studies, Abadie and Gardeazabal (2003) use synthetic control methods and find terrorism in the Basque region of Spain has a significant impact on regional per capita GDP. The impact is non-trivial as the estimated cost to per capita GDP is 10%. In addition, Abadie and Gardeazabal (2003) find violence has a negative impact on the stock prices of companies in the

Basque region, but that the stock market seems to react positively when a credible truce is reached. The idea of economic variables reacting to forward looking expectations is also found in Zussman and Zussman (2006) and Zussman et al. (2008). These studies find that the Israeli stock market reacts in response to key events related to the Israeli-Palestinian conflict. In addition to the stock market, conflict and terrorism have also been shown to impact the real estate market. Using a regime-switching model, Besley and Mueller (2009) find there is a difference between short-term violence and long-term perceptions of persistent violence. Only when homeowners perceive that the world is in a long-term non-violent state do regional house price indexes begin to trend upwards. More relevant to our study, Abadie and Dermisi (2008) find vacancy rates in notable Chicago buildings increased in the months following 9/11.

Expanding on these two studies, we use transaction level data in the residential real estate market, and investigate individual homeowner perceptions of terrorism. Dating back to results foudn in Kain and Quigley (1970), residential property prices have been found to capitalize all relevant information concerning the surrounding area. In the context of violence, errorism, and conflict, we note a connection to previous studies that have investigated the impact of localized crime on residential property prices.[2] Some studies also find that the presence of known *criminals* also has a negative impact of property prices. Most notably, Linden and Rockoff (2008) finds that the arrival of a registered sex offender decreases property prices within 0.1 miles by approximately 4%. In a similar study, Pope (2008) finds a causal effect in that property prices revert to normal when the registered sex offender moves out.

Other research has examined the experience of Muslims in the real estate market. Gautier et al. (2009) investigate property prices following the murder of Dutch filmmaker Theo van Gogh by a recent convert to radical Islam. Following can Gogh's murder, listing prices in areas with a large Muslim concentration decreased by 3%. Further, Gautier et al. (2009) find evidence that Muslims became more likely to purchase homes in those areas with an already large Muslim concentration. Ahmed and Hammarstedt (2008) study discrimation against Muslims in the Swedish real estate market and find that landlords are less willing to respond to rental applications iwhen the applicant's name is strongly associated with Islam. In particular, Ahmed and Hammarstedt (2008) compare rental application response rates using emails sent using the names *Erik Johansson,*

[2]Specific examples include Cullen and Levitt (1999); Schwartz et al. (2003); Gibbons and Machin (2008); Ihlanfeldt and Mayock (2010). For a recent survey, see Benson and Zimmerman (2010).

Maria Andersson and *Mohammed Rashid* and find emails sent using the name Mohammed Rashid received fewer callbacks and invitations to view the property. Similar results for African-Americans have been found in rental markets, Hanson and Hawley (2011).

In this study, we are interested in combining the transaction based approach of Linden and Rockoff (2008) in the context an event study similar to (Gautier et al., 2009) and Abadie and Dermisi (2008). In order to do so, it is necessary to identify Arabs in the assessor data. This presents a challenge as the assessor data includes a number of variables but does not include an indicator for whether or not the buyer or seller are Arab. Therefore, we impute ethnicity for the buyer and seller using a supervised learning algorithm trained on Summer Olympic rosters. Similar approaches matching names to ethnicity have been developed in the biomedical literature researchers have sought to map available information on the surname to unobservable information on ethnicity in an approach known as *name-ethnicity matching*; examples in the biomedical literature include (Coldman et al., 1988; Burchard et al., 2003; Fiscella and Fremont, 2006). In a real estate setting, Humphreys et al. (WP) use a similar approach and identify Chinese buyers and sellers based on name.

A standard approach to creating a binomial classifier based on observables is to use a logit or similar method. In the name-ethnicity approach, the number of observables - names - can be large. In such high-dimensional covariate settings, classical methods will most likely overfit the data, and out-of-sample predictions are misleading, Hastie et al. (2015). However, regularized estimators have been shown to perform well out-of-sample in high-dimensional logistic models, Ng (2004). In related work, Taddy (2013) regularizes an inverse multinomial model in order to identify which words are used more frequently by Republicans and Democrats.

3 Identifying Ethnicity

The data we use includes the buyer and seller name for each transaction. Unfortunately, there is no variable indicating the ethnicity of either party. Therefore, we require a means to classify buyer names into groups. We propose estimating the ethnicity of the individuals by comparing the first and last names to a list of first and last names known to have strong affiliations to a particular country. Specifically, we use the Summer Olympics rosters for each country beginning 1948 and ending 2012. In total, there are $N = 90,636$ unique Olympians from 221 unique countries.

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. By doing so, we treat each full name as an unordered set of names: one or more first and last names. For example, *michael phelps* is expressed as the set $\{michael, phelps\}$. In our analysis, we treat hyphenated names as two names and retain the hyphen in order to acknowledge the split. For example, *jean-claude killy* becomes the 3 element set $\{$*jean-*, *claude*, *killy*$\}$, and *nasser al-shami* is the set $\{$*nasser*, *al-*, *shami*$\}$. We also remove diacritics from the full names where *josé* becomes *jose*. Using the 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 entire set of rosters. Doing so leaves $P = 3,212$ unique full names.

It is also instructive to view each full name as a $P \times 1$ vector, x'_n, of 1s and 0s where the p element is equal to 1 if name p is in the full name and 0 otherwise. For Olympian Michael Phelps, the pth element of x_n, X_{np}, corresponding to *michael* will be equal to 1. However, because the name *Phelps* does not appear in the full roster more than 10 times, there is no indicator for *Phelps*.

Next, we set an indicator variable $y_n = 1$ if an Olympian comes from the Arab World and 0 otherwise. As mentioned above, in order to identify the Arab World, we use a list of countries in the Arab League. Using y_n and X_n, one could estimate a binary choice model such as a probit or logit model. In these models, the interpretation is that variable y_n is the result of the variables in X_n. Table 1 shows the 20 most frequent names from the Arab and non Arab League countries. Using these explanatory variables, we then model the probability that $y_n = 1$ as

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

In Equation (1), when $0 < \phi_p$, the presence of token p increases the likelihood that F_n comes from the an Arab League Olympic team roster, and vice-versa for $\phi < 0$. When $\phi_p = 0$, token 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 using the maximum likelihood estimator. In this setting, the assumption of fixed P is difficult to defend. Because P remains large even after filtering out less frequently occurring names, we utilize a penalized likelihood procedure that prevents overfitting the logit model. In particular, we place an ℓ_1 penalty on the individual ϕ_p

parameters and minimize

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

The parameter λ is a tuning parameter.[3][4] Define the solution to Equation 2 as ϕ^*. By using the ℓ_1 penalty on ϕ_p, some entries of ϕ^* are exactly equal to 0. As mentioned above, when $\phi^*_p = 0$, token p cannot be used to classify y_n. With this interpretation, minimizing Equation 2 performs both variable selection and coefficient estimation.

In any event, by including the penalty term $\lambda \sum_p |\phi_p|$, ϕ^* can be used to classify names out-of-sample as it is less likely to overfit the data in-sample. For our application, out-of-sample performance (mis-classification) is fundamental to our results. In particular, we tokenize buyer names in a manner identical to the tokenization procedure applied to the names in the Olympic Games team rosters. Based on Figure 1, we create the indicator variable $Arab = 1$ for Arab homeowners if $0.35 < \Pr(y_n = 1|X_n, \phi^*)$ and $Arab = 0$ otherwise. A independent, manual inspection of the names by a third-party validated the 0.35 cutoff.

4 Model Specification

In order to test for price effects, we estimate a hedonic model for house n in census tract c sold at time t

$$p_{nct} = x_{nct}\beta + \delta_t + \mu_c + u_{nct} \tag{3}$$

Here, p_{nct} is the log price of the house, x_{nct} is a vector of house attributes including log square footage, bedrooms, and bathrooms, β is a vector of prices, δ_t is a market wide price trend, μ_c is a census tract time-invariant effect, and u_{nct} is an error term.

After estimating Equation 3, we collect the residuals for all transactions located within 0.3 miles of an Arab homeowner. We then estimate a local polynomial regression in order to estimate a gradient as a function of distance from an Arab homeowner. The 95% confidence interval for the local polynomial regression using transactions 180 before 9/11 are presented in green in Figure 3.

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

[4] 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.

The point estimates for the local polynomial regression 180 days after 9/11 are presented as a red line in Figure 3. The results in this figure suggest there are negative price effects up to a distance of 0.1 miles.

Based on this, we create the indicator variable $D_{nct}^{0.1} = 1$ if any Arab homeowner is within 0.1 miles of the property and 0 otherwise. Similar to Pope (2008) and Linden and Rockoff (2008), we test for local externalities by comparing properties within 0.1 miles of an Arab homeowner miles to those between 0.1 and 0.2 miles of an Arab homeowner. The idea being: if homeowners do not wish to live next to Arab homeowners, the effect should be stronger the closer the Arab homeowner. Based on this methodology, we create the indicator variable $D_{nct}^{0.3} = 1$ if any Arab homeowner is within 0.3 miles of the property and 0 otherwise. An example of properties included in the treatment and control groups is displayed in Figure 4. Using these indicators, we then estimate

$$p_{nct} = x_{nct}\beta + D_{nct}^{0.1}\psi^{0.1} + D_{nct}^{0.3}\psi^{0.3} + \delta_t + \mu_c + u_{nct} \tag{4}$$

In Equation 4, $\psi^{0.3}$ captures a price effect for all properties with 0.3 miles of an Arab homeowner, and $\psi^{0.1}$ captures an intensity effect. If individuals view living next to Arab homeowners as a negative, we should have $\psi^{0.1} < 0$. If individuals do not view Arab homeowners in a negative manner, there should be no difference between those living within 0.1 miles and those living between 0.1 and 0.3 miles; alternatively, we would expect $\psi^{0.1} = 0$.

In this study, we are interested in investigating whether or not 9/11 created or intensified negative perceptions of Arab homeowners. In order to test this, we create the indicator variable $Post_{nct} = 1$ if the transaction occurred after 9/11 but before March 10, 2002, a time period of 180 days. We also investigate 60 and 90 days following 9/11. Using this indicator variable, we then estimate

$$p_{nct} = x_{nct}\beta + D_{nct}^{0.1}\psi^{0.1} + D_{nct}^{0.3}\psi^{0.3} + Post_{nct} \times D_{nct}^{0.1}\gamma^{0.1} + Post_{nct} \times D_{nct}^{0.3}\gamma^{0.3} + \delta_t + \mu_c + u_{nct} \tag{5}$$

Here, $\gamma^{0.1}$ and $\gamma^{0.3}$ capture any time-varying perceptions of Arab homeowners nearby immediately following 9/11. If homeowners in the Seattle metro area formed irrational fears Arab neighbors following 9/11, we expect these fears to be stronger the closer to an Arab neighbor, $\gamma^{0.1} < 0$. If homeowners did not view Arab homeowners negatively before 9/11, we expect $\psi^{0.1} = 0$.

The time-varying framework in Equation 5 is similar to the time-varying treatment effect in Pope (2008) and Linden and Rockoff (2008). In these studies, the time-varying nature is attributable to a sex offender moving in to a house. In this way, the time-varying treatment is applied at different times as sex offenders move in to and out of properties at different times. In this study, all time-varying treatments are turned on at a single point in time, September 11, 2001.

5 Data

The 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.[5] Each roster includes the full name of the olympian, age, gender and nationality. As mentioned above, we identify countries as Arab countries if the country is a member of the Arab League. 6 member states created 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.

Transaction data used comes from the King County Assessor's Office in the state of that includes the Seattle, Washington metro area.[6] 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. In total, this leaves 265,255 total transactions in the study. Using December 31, 2002 as a cutoff provides with more than 15 months of sales post 9/11 that we use to identify price effects. Summary statistics for the data are provided in Table 3. 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.

The Arab indicator has an average value of 0.03 which corresponds to 838 buyers identified as Arab using the probability cutoff. The locations of each individual identified as an Arab are displayed in Figure 2. Unlike Gautier et al. (2009) who studies areas with a high concentration of Muslims, the locations of Arabs are fairly dispersed throughout Seattle. Although the data is

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

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

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.

6 Results

6.1 Arab Name Identifiers

Results for the penalized logit model are presented in Table 2. Of course, the results in Table 2 indicate that the more frequent names in Table 1 are the stronger indicators of nationality for a country in the Arab League. However, it is interesting to note that the name *mohamed* and its variants are not strong indicators of an individual being from an Arab League country. This should not be surprising, as *mohamed* is an Islamic name found throughout many Islamic but non-Arab countries.

The names that are strong indicators of not being from a country in the Arab League are as expected. Because the names used are from many countries throughout the world, the scope of names is quite large. That being said, the strongest predictor of non-Arab status is *jose*. The remaining names are the more common names found in countries outside of Arab world with a low percentage of Muslims.

6.2 Effects

The main results are presented in Table 4. All models include log square footage, bedrooms, bathrooms and quarterly fixed-effects. Coefficients for log square footage, bedrooms, bathrooms are omitted for presentation purposes. Column 1 estimates the model without census tract effects. The coefficients indicate that, absent any census tract controls, properties between 0.1 and 0.3 miles of an Arab homeowner sell for a 3.6% premium. Properties less than 0.1 miles from an Arab homeowner sell at a slightly smaller premium of 2.6%.[7]

Results in Column 2 include census tract fixed-effects. After including the census tract fixed-effects, the effects of the proximity to an Arab neighbor are statistically significant but economically negligible. However, the results in Columns 1 and 2 do not test for the effect fo 9/11 on homeowners

[7] 2.6=3.6–0.1

perceptions of Arab neighbors. Column 3 includes the time-varying effects and indicates that there is a 1.6% discount after 9/11 for properties selling near Arab homeowners. This effect is significant at the 10% level. Columns 4-6 investigate the effect using 90, 60, and 30 day windows. The results indicate that the effect is larger in magnitude for properties selling closer to 9/11; all results are significant at the 1% level. Properties sold within 90 days of 9/11 with 0.1 miles of an Arab homeowner sell at a 3.5% discount. The magnitude of this discount is similar to results found in Pope (2008) and Linden and Rockoff (2008) for sex offenders.

In addition to examining the control group 0.1 to 0.3 miles from an Arab homeowner, we also investigate the control group of homes 0.1 to $0.1\sqrt{2} \approx 0.14$ miles. The choice of this alternative control group is made so that the area of the disk 0.1 miles in radius is equal to the area of the ring formed by removing a disk of radius 0.1 miles from the center a disk of radius $0.1\sqrt{2}$ miles.[8] The results are presented in Table 5 and are comparable to the results in Table 4. However, the time-varying treatment effect for Arab neighbors increases in magnitude, and the 180 day window effect is now significant at the 1% level.

6.3 Falsification tests

6.3.1 Different Groups

The previous analysis focused on the impacts of Arab homeowners on neighboring property values following 9/11. This section performs a falsification test where we estimate the effects of East Asian and Hispanic homeowners on neighboring property values. We perform this analysis for two important reasons. First, it can be plausibly claimed that members of these two groups were not involved in the terrorist attacks, and there should be no time-varying effects of their homeownership on neighboring property values. Thus, failing to find significant values of $\gamma^{0.1}$ using these groups provides additional support for our conclusions reached using results in Tables 4 and 5. Second, our results in Tables 4 and 5 are based on identifying Arab homeowners using only their name. A null result for the East Asian and Hispanic groups provides further evidence that the binomial classifier we develop in order to identify the groups is valid.

In order to identify Hispanic and East Asian homeowners, we perform use the same methodology used to identify Arab homeowners. Because we are not identifying Arabs, we do not label countries

[8] $A = \pi \times (0.1\sqrt{2})^2 = 2\pi 0.1^2$

based on their membership in the Arab League. Instead, for the East Asian group we use the countries: China, Japan, Mongolia, South Korea, and North Korea. For the Hispanic countries, we use all countries in Central and South America.

Initial graphical evidence is presented in Figure 6. Here, similar to Figure 3 the smoothed residuals are plotted as a function of distance from East Asian and Hispanic homeowners 180 days before and 180 days after 9/11. These two figures present some initial evidence that there does not appear to be a strong time-varying effect for these two groups. However, the post 9/11 effect for East Asian homeowners is bordering on the lower bound of the 95% confidence interval.

Formal statistical treatments for these two groups are presented in Tables 6 and 7. Results in Table 6 suggest the effects for East Asian homeowners following 9/11 are inconclusive. Price effects are at most 0.9% using a window of 90 days and not significant at any reasonable confidence level for the 180 day window. This borderline significance reflects the graphical evidence in Figure 6a. Regression results for Hispanic homeowners are presented in Table 7. The coefficient estimates for $D^{0.1}$ and $D^{0.3}$ suggest Hispanic homeowners purchase properties at lower prices; this effect is indicate of lower quality properties, increased bargaining power or a multitude of other factors, a interpretation emphasized in Linden and Rockoff (2008). However, consistent with the graphical evidence in Figure 6b, there does not appear to be any significant change in price effects attributable to neighboring Hispanic homeowners following 9/11. Taken as a whole, the results in Table 5, 6, and 7 present evidence that homeowners viewed Arab Neighbors, but neither East Asian or Hispanic neighbors, in a more negative light following 9/11.

6.3.2 Different Event Date

As an alternative falsification test, we examine the impact of property prices in the vicinity of Arab homeowners 180 after September 11, 2000. This date is one full year before the actual events of September 11, 2001. It could be possible that any negative perceptions of Arab homeowners could have been trending before 9/11. If this is the case, the results in Tables 4 and 5 are capturing the effects of a pre 9/11 trend. Results using the indicator $Post_{nct} = 1$ if the transaction occurs within 180 days after September 11, 2000 are presented in Table 8. The results indicate no effect for the 180 and 90 day windows and a 2.6% increase in property values for the 60 day window. As a whole, these results indicate that results in Tables 4 and 5 are capturing the effect of the events on 9/11 and not a pre-existing, anti-Arab trend.

7 Conclusions

Previous research has documented that both crime and the perception of crime are capitalized into property prices. This study uses transaction data and presents evidence that homeowner perceptions of Arab neighbors in the immediate vicinity were changed following the 9/11 terrorist attacks. Specifically, homeowners with Arab neighbors within 0.1 miles sold their properties at discounts between 1.6% to 2.1% within 180 days of 9/11. Properties sold within 60 days of 9/11 indicate these discounts were between 3.5% and 4.6%.

The data used in this study comes from the King County Assessor's Office and includes buyer and seller name but not ethnicity. In order to identify ethnicity, we use a supervised learning algorithm trained using Summer Olympic rosters from 1948 to 2012. Olympians are classified into two groups based on their country's membership in the Arab Leauge. Because the number of names in our study is large and we are focused on out-of-sample prediction, we use a penalized logit likelihood that has been shown to have superior out-of-sample prediction.

As a falsification test, we also examine effects using homeowners classified using Olympians from East Asian and Hispanic countries. As expected, we find no evidence that neighbors living next to Hispanic homeowners sold their properties at discounts 180 days after 9/11. The effects for East Asian homeowners are of mixed statistical significance. However, the magnitudes of the discounts attributable to East Asian neighbors is much smaller compared to the estimated effects for Arab neighbors. As a whole, the results for Arab neighbors and the two falsification tests present the first evidence of changing perceptions of Arab neighbors following 9/11 in the market for single-family homes.

References

Abadie, A. and Dermisi, S. (2008). Is terrorism eroding agglomeration economies in central business districts? lessons from the office real estate market in downtown chicago. *Journal of Urban Economics*, 64(2):451–463.

Abadie, A. and Gardeazabal, J. (2003). The economic costs of conflict: A case study of the basque country. *American economic review*, pages 113–132.

Agan, A. Y. (2011). Sex offender registries: Fear without function? *Journal of Law and Economics*, 54(1):207–239.

Ahmed, A. M. and Hammarstedt, M. (2008). Discrimination in the rental housing market: A field experiment on the internet. *Journal of Urban Economics*, 64(2):362–372.

Benson, B. L. and Zimmerman, P. R. (2010). *Handbook on the Economics of Crime*. Edward Elgar.

Besley, T. J. and Mueller, H. F. (2009). Estimating the peace dividend: The impact of violence on house prices in northern ireland. *LSE STICERD Research Paper No. EOPP011*.

Burchard, E. G., Ziv, E., Coyle, N., Gomez, S. L., Tang, H., Karter, A. J., Mountain, J. L., Pérez-Stable, E. J., Sheppard, D., and Risch, N. (2003). The importance of race and ethnic background in biomedical research and clinical practice. *New England Journal of Medicine*, 348(12):1170–1175.

Coldman, A. J., Braun, T., and Gallagher, R. P. (1988). The classification of ethnic status using name information. *Journal of Epidemiology and Community Health*, 42(4):390–395.

Cullen, J. B. and Levitt, S. D. (1999). Crime, urban flight, and the consequences for cities. *Review of economics and statistics*, 81(2):159–169.

Fiscella, K. and Fremont, A. M. (2006). Use of geocoding and surname analysis to estimate race and ethnicity. *Health Services Research*, 41(4p1):1482–1500.

Gautier, P. A., Siegmann, A., and Van Vuuren, A. (2009). Terrorism and attitudes towards minorities: The effect of the theo van gogh murder on house prices in amsterdam. *Journal of Urban Economics*, 65(2):113–126.

Gibbons, S. and Machin, S. (2008). Valuing school quality, better transport, and lower crime: evidence from house prices. *Oxford Review of Economic Policy*, 24(1):99–119.

Hanson, A. and Hawley, Z. (2011). Do landlords discriminate in the rental housing market? evidence from an internet field experiment in us cities. *Journal of Urban Economics*, 70(2):99–114.

Hastie, T., Tibshirani, R., and Wainwright, M. (2015). *Statistical learning with sparsity: the lasso and generalizations*. CRC Press.

Humphreys, B., Nowak, A., and Zhou, Y. (WP). Cultural superstitions and residential real estate prices: Transaction-level evidence from the us housing market. *Woking Paper*.

Ihlanfeldt, K. and Mayock, T. (2010). Panel data estimates of the effects of different types of crime on housing prices. *Regional Science and Urban Economics*, 40(2):161–172.

Kain, J. F. and Quigley, J. M. (1970). Measuring the value of housing quality. *Journal of the American statistical association*, 65(330):532–548.

Linden, L. and Rockoff, J. E. (2008). Estimates of the impact of crime risk on property values from megan's laws. *The American Economic Review*, 98(3):1103–1127.

Ng, A. Y. (2004). Feature selection, l 1 vs. l 2 regularization, and rotational invariance. In *Proceedings of the twenty-first international conference on Machine learning*, page 78. ACM.

Pope, J. C. (2008). Fear of crime and housing prices: Household reactions to sex offender registries. *Journal of Urban Economics*, 64(3):601–614.

Prescott, J. J. and Rockoff, J. E. (2008). Do sex offender registration and notification laws affect criminal behavior? Technical report, National Bureau of Economic Research.

Schwartz, A. E., Susin, S., and Voicu, I. (2003). Has falling crime driven new york city's real estate boom? *Journal of Housing Research*, 14(1):101.

Taddy, M. (2013). Multinomial inverse regression for text analysis. *Journal of the American Statistical Association*, 108(503):755–770.

Zussman, A. and Zussman, N. (2006). Assassinations: Evaluating the effectiveness of an israeli counterterrorism policy using stock market data. *The Journal of Economic Perspectives*, 20(2):193–206.

Zussman, A., Zussman, N., and Nielsen, M. Ø. (2008). Asset market perspectives on the israeli–palestinian conflict. *Economica*, 75(297):84–115.

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

Arab League		Non-Arab League	
Name	Frequency	Name	Frequency
mohamed	0.0448	peter	0.0036
al-	0.0404	jose	0.0034
el-	0.0349	john	0.0034
ahmed	0.0214	de	0.0032
ali	0.0201	kim	0.0031
abdel	0.0144	van	0.003
muhammad	0.01	david	0.0024
abdul	0.01	juan	0.0023
ibrahim	0.0089	maria	0.0023
hassan	0.008	carlos	0.0021
ben	0.0079	lee	0.0021
sayed	0.0072	paul	0.0021
mahmoud	0.0066	michael	0.002
hussain	0.006	robert	0.0019
abdullah	0.0059	luis	0.0019
khaled	0.0047	martin	0.0018
moustafa	0.0047	jan	0.0017
mohammad	0.0046	daniel	0.0016
youssef	0.0045	aleksandr	0.0016
aziz	0.0039	jean-	0.0016

Table 2: Penalized Logistic

Arab League		Non-Arab League	
Name	$\beta_k(y=1)$	Name	$\beta_k(y=0)$
Largest Coefficients			
al-	7.574	diop	0.230
noureddine	7.574	aida	0.153
riadh	7.276	bin	0.093
mourad	7.257	carolyn	0.035
hossein	7.229	laure	0.032
lotfi	7.103	de	0.009
brahim	7.04	jose	0.008
mehdi	7.004	van	0.007
el-	6.941	kim	0.007
bel	6.867	peter	0.007
Largest Negative Coefficients			
jose	-1.497	al-	-9.927
juan	-1.174	nabil	-5.012
paul	-1.177	khaled	-5.08
maria	-1.277	kamel	-5.141
john	-1.412	salem	-5.144
jose	-1.497	ghulam	-5.18
lee	-1.498	reza	-5.335
peter	-1.516	hossein	-5.339
kim	-2.103	abou	-6.006
van	-2.25	abdel	-6.735

Table 3: 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
Arab Indicator	0	0.003	0	1	0.056

Figure 1: Distribution of Arab Index in Assessor Data

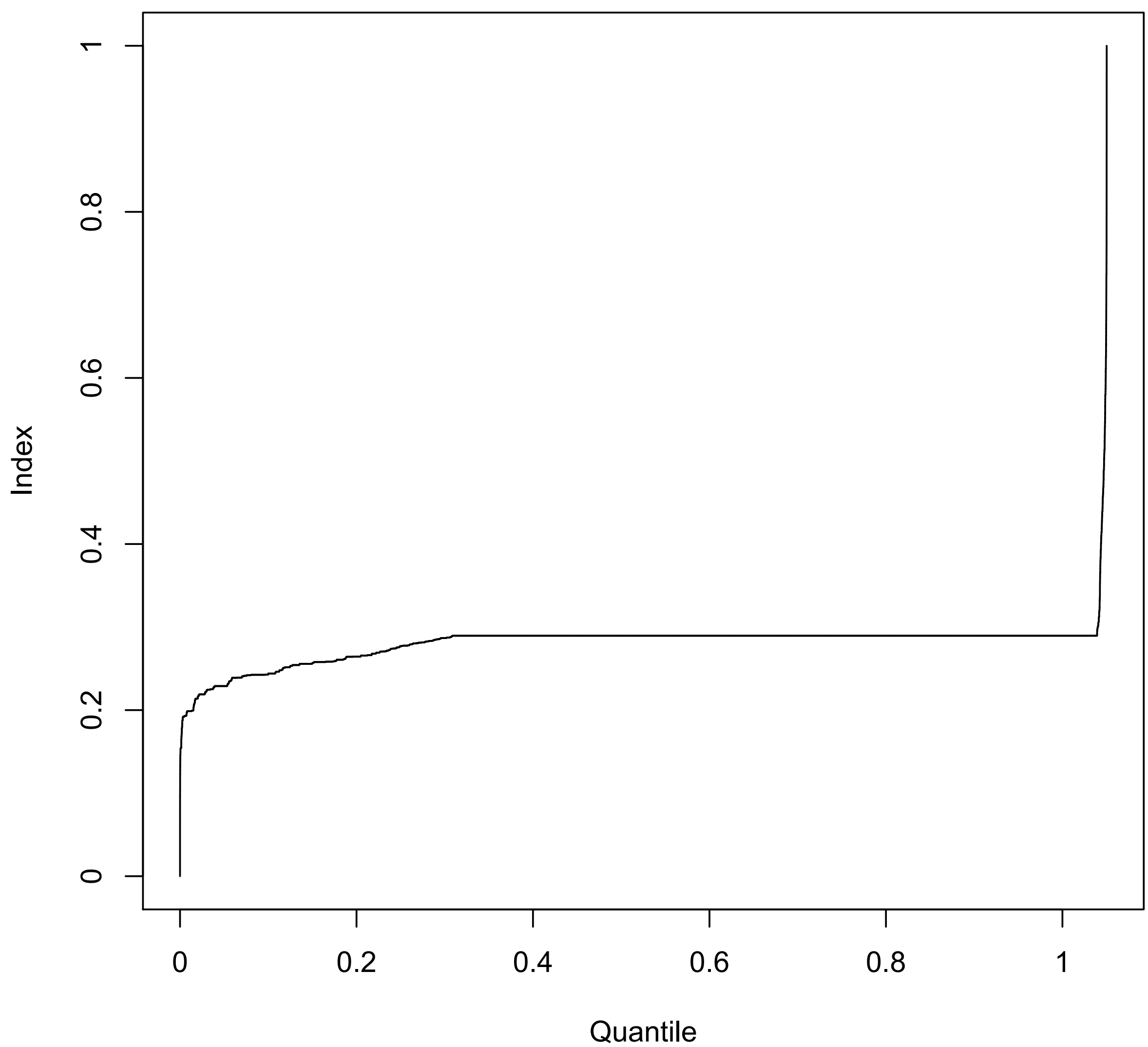


Figure 2: Location of Arab Homeowners

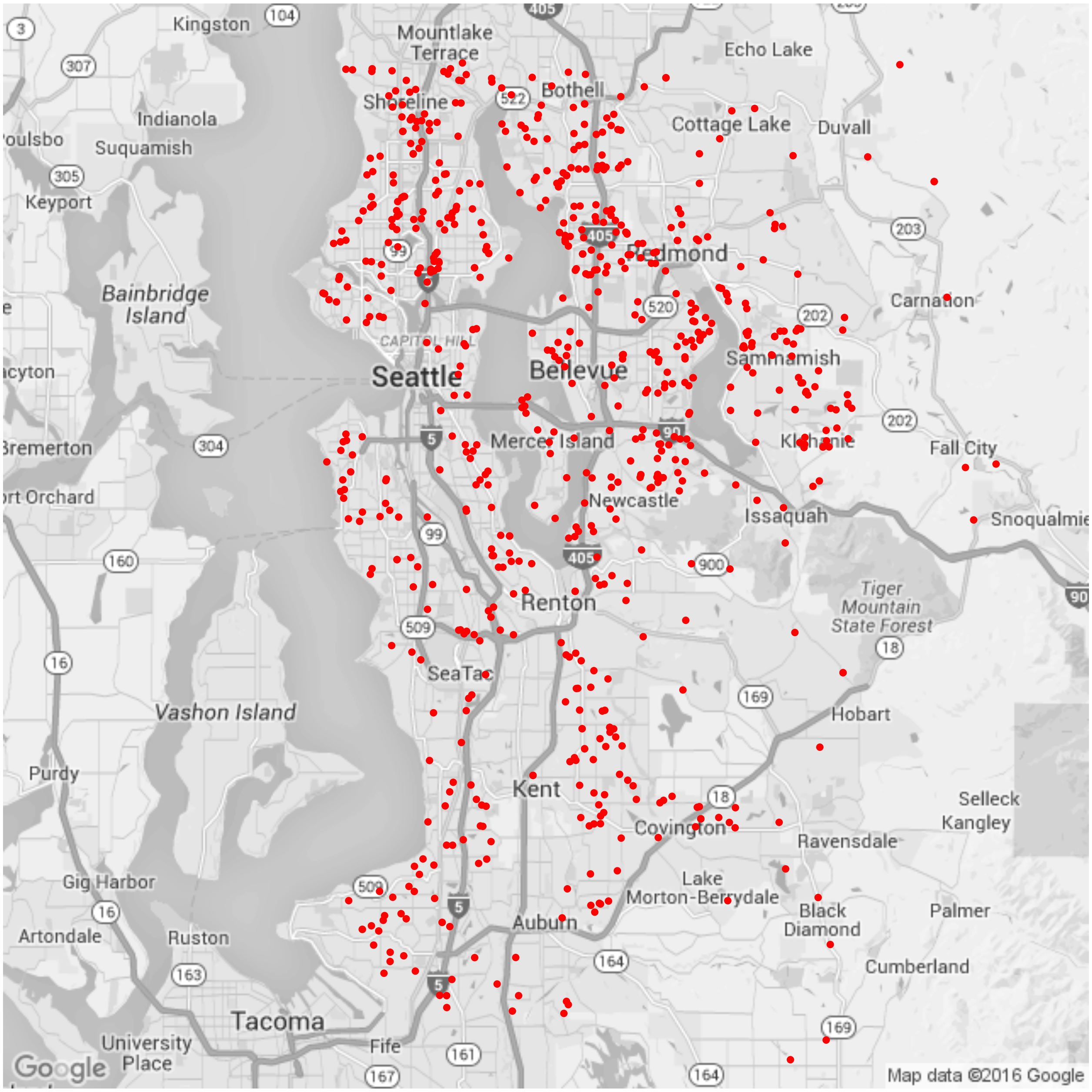


Figure 3: Price Gradient Near Arab Homeowners

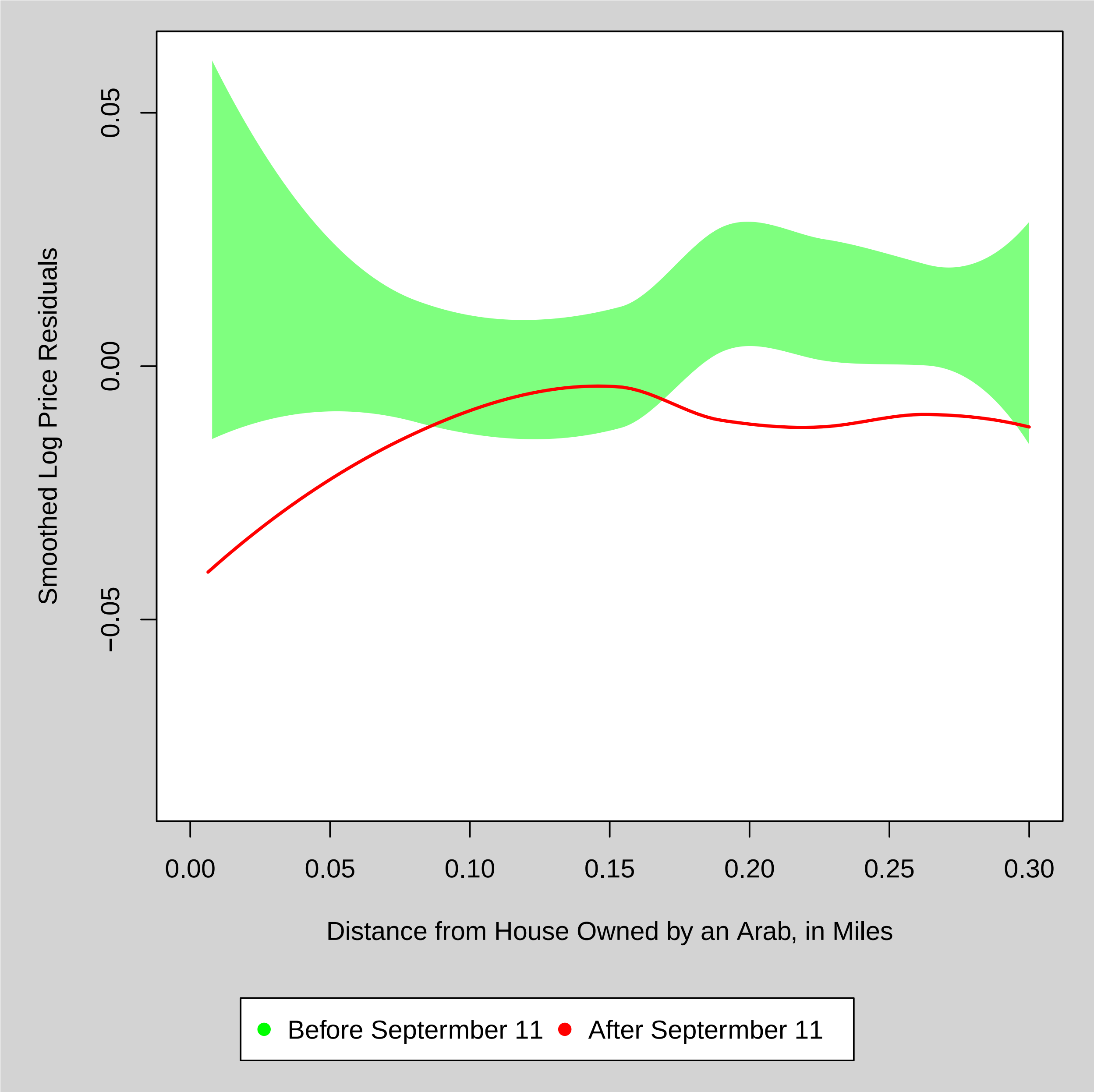


Figure 4: Treatment Area and Control Area

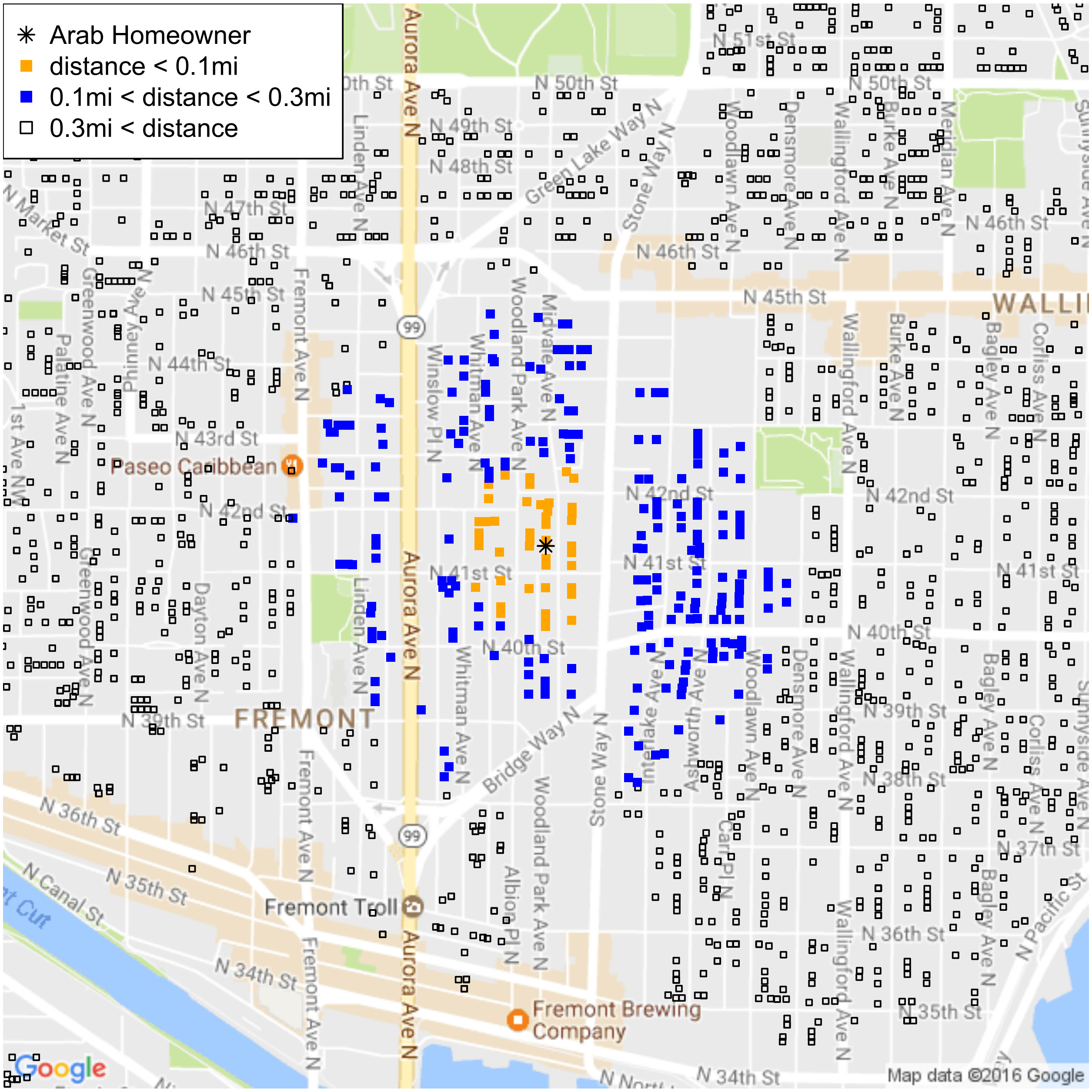
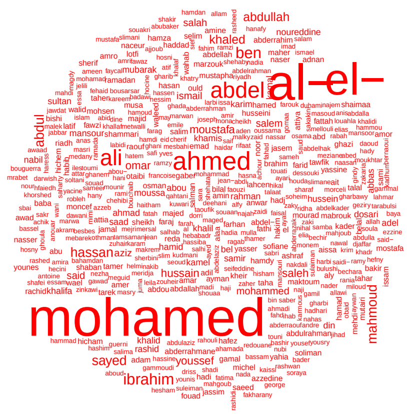
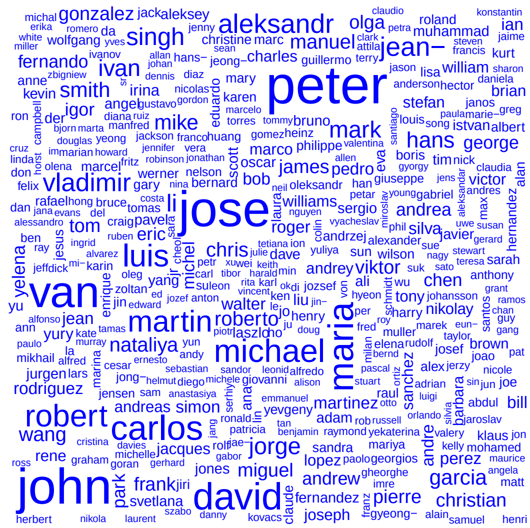


Figure 5: Arab League Names

China



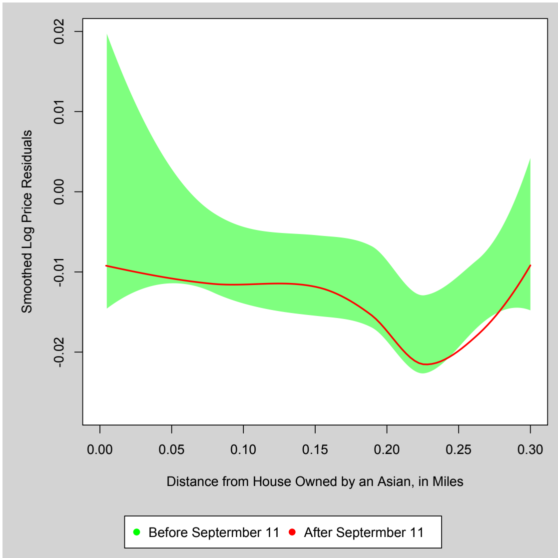
World Names



300 most frequent names on the olympic rosters for each country. More frequent names are indicated with a larger font.

Figure 6: Alternative Ethnic Group Price Gradients

East Asian Homeowners



Hispanic Homeowners

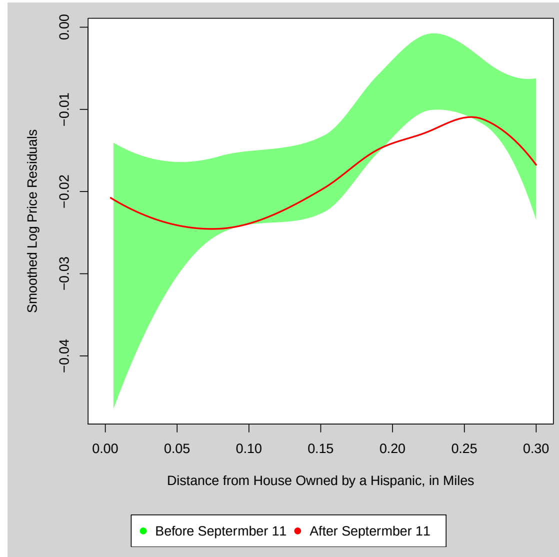


Table 4: Price Effects for Arab Neighbors post 9/11

	(1)	(2)	(3)	(4)	(5)	(6)
$D^{0.1}$	-0.010^{***}	-0.008^{***}	-0.007	-0.007	-0.007^{*}	-0.008^{*}
	(0.003)	(0.002)	(0.005)	(0.005)	(0.005)	(0.004)
$D^{0.3}$	0.036^{***}	-0.006^{***}	-0.006	-0.006	-0.006	-0.006
	(0.002)	(0.001)	(0.005)	(0.005)	(0.005)	(0.005)
$D^{0.1} \times Post$			-0.016^{*}	-0.026^{***}	-0.035^{***}	-0.039^{***}
			(0.008)	(0.009)	(0.007)	(0.010)
$D^{0.3} \times Post$			0.002	0.010	0.008	0.020^{***}
			(0.007)	(0.007)	(0.010)	(0.005)
Num. obs.	265255	265255	265255	265255	265255	265255
R^2	0.637	0.788	0.788	0.788	0.788	0.788
Quarterly FE	Y	Y	Y	Y	Y	Y
Census Tract FE	N	Y	Y	Y	Y	Y
Post 9/11 Window	180 Days	180 Days	180 Days	90 Days	60 Days	30 Days

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

Table 4 displays results for the time-varying effect of Arab neighbors following September 11, 2001. The variable $D^{0.1} = 1$ ($D^{0.3}$=1) if the transaction is within 0.1 miles (0.3 miles) of an Arab homeowner. The variable $Post = 1$ if the transaction is between 0 and 180 days after September 11, 2001. The window for $Post$ is varied from 180, 90, 60, and 30 days. All regressions include log square footage, bedrooms, and bathrooms as control variables. Standard errors are clustered at the census tract and quarter levels.

Table 5: Price Effects for Arab Neighbors post 9/11, Equal Area Treatment Zones

	(1)	(2)	(3)	(4)	(5)	(6)
$D^{0.1}$	-0.017^{***}	-0.000	0.001	0.001	0.001	0.000
	(0.004)	(0.003)	(0.006)	(0.006)	(0.006)	(0.006)
$D^{0.1\sqrt{2}}$	0.036^{***}	-0.013^{***}	-0.013^{**}	-0.013^{**}	-0.013^{**}	-0.013^{**}
	(0.003)	(0.002)	(0.006)	(0.006)	(0.006)	(0.006)
$D^{0.1} \times Post$			-0.021^{***}	-0.025^{***}	-0.046^{***}	-0.047^{***}
			(0.004)	(0.002)	(0.001)	(0.005)
$D^{0.1\sqrt{2}} \times Post$			0.008^{***}	0.007^{*}	0.019^{***}	0.028
			(0.002)	(0.004)	(0.005)	(0.019)
Num. obs.	265255	265255	265255	265255	265255	265255
R^2	0.637	0.788	0.788	0.788	0.788	0.788
Quarterly FE	Y	Y	Y	Y	Y	Y
Census Tract FE	N	Y	Y	Y	Y	Y
Post 9/11 Window	180 Days	180 Days	180 Days	90 Days	60 Days	30 Days

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

Table 5 displays results for the time-varying effect of Arab neighbors following September 11, 2001. The variable $D^{0.1} = 1$ ($D^{0.1\sqrt{2}}$=1) if the transaction is within 0.1 miles ($0.1\sqrt{2}$ miles) of an Arab homeowner. The variable $Post = 1$ if the transaction is between 0 and 180 days after September 11, 2001. The window for *Post* is varied from 180, 90, 60, and 30 days. All regressions include log square footage, bedrooms, and bathrooms as control variables. Standard errors are clustered at the census tract and quarter levels.

Table 6: Price Effects for East Asian Neighbors post 9/11, Equal Area Treatment Zones

	(1)	(2)	(3)	(4)	(5)	(6)
$D^{0.1}$	-0.016^{***}	0.002	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
$D^{0.1\sqrt{2}}$	-0.015^{***}	-0.007^{***}	-0.007^{*}	-0.007^{*}	-0.007^{*}	-0.007^{*}
	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)	(0.004)
$D^{0.1} \times Post$			-0.000	-0.009^{***}	-0.006^{*}	-0.006^{***}
			(0.008)	(0.003)	(0.004)	(0.002)
$D^{0.1\sqrt{2}} \times Post$			-0.001	0.005^{**}	0.005^{*}	0.010^{**}
			(0.005)	(0.002)	(0.003)	(0.005)
Num. obs.	265255	265255	265255	265255	265255	265255
R^2	0.637	0.788	0.788	0.788	0.788	0.788
Quarterly FE	Y	Y	Y	Y	Y	Y
Census Tract FE	N	Y	Y	Y	Y	Y
Post 9/11 Window	180 Days	180 Days	180 Days	90 Days	60 Days	30 Days

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

Table 5 displays results for the time-varying effect of East Asian neighbors following September 11, 2001. The variable $D^{0.1} = 1$ ($D^{0.1\sqrt{2}}$=1) if the transaction is within 0.1 miles ($0.1\sqrt{2}$ miles) of an East Asian homeowner. The variable $Post = 1$ if the transaction is between 0 and 180 days after September 11, 2001. The window for $Post$ is varied from 180, 90, 60, and 30 days. All regressions include log square footage, bedrooms, and bathrooms as control variables. Standard errors are clustered at the census tract and quarter levels.

Table 7: Price Effects for Hispanic Neighbors post 9/11, Equal Area Treatment Zones

	(1)	(2)	(3)	(4)	(5)	(6)
$D^{0.1}$	-0.041^{***}	-0.007^{***}	-0.007^{***}	-0.007^{***}	-0.007^{***}	-0.007^{***}
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
$D^{0.1\sqrt{2}}$	-0.051^{***}	-0.021^{***}	-0.021^{***}	-0.022^{***}	-0.022^{***}	-0.021^{***}
	(0.002)	(0.001)	(0.003)	(0.003)	(0.003)	(0.003)
$D^{0.1} \times Post$			-0.000	-0.006	-0.009	0.003
			(0.006)	(0.004)	(0.006)	(0.004)
$D^{0.1\sqrt{2}} \times Post$			-0.003	0.007	0.007	-0.000
			(0.011)	(0.005)	(0.006)	(0.004)
Num. obs.	265255	265255	265255	265255	265255	265255
R^2	0.642	0.789	0.789	0.789	0.789	0.789
Quarterly FE	Y	Y	Y	Y	Y	Y
Census Tract FE	N	Y	Y	Y	Y	Y
Post 9/11 Window	180 Days	180 Days	180 Days	90 Days	60 Days	30 Days

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

Table 5 displays results for the time-varying effect of Hispanic neighbors following September 11, 2001. The variable $D^{0.1} = 1$ ($D^{0.1\sqrt{2}}$=1) if the transaction is within 0.1 miles ($0.1\sqrt{2}$ miles) of an Hispanic homeowner. The variable $Post = 1$ if the transaction is between 0 and 180 days after September 11, 2001. The window for *Post* is varied from 180, 90, 60, and 30 days. All regressions include log square footage, bedrooms, and bathrooms as control variables. Standard errors are clustered at the census tract and quarter levels.

Table 8: Price Effects for Arab Neighbors post 9/11/00, Equal Area Treatment Zones

	(1)	(2)	(3)	(4)	(5)	(6)
$D^{0.1}$	-0.017^{***}	-0.000	-0.001	-0.000	-0.001	-0.000
	(0.004)	(0.003)	(0.006)	(0.006)	(0.006)	(0.006)
$D^{0.1\sqrt{2}}$	0.036^{***}	-0.013^{***}	-0.013^{**}	-0.013^{**}	-0.013^{**}	-0.013^{**}
	(0.003)	(0.002)	(0.006)	(0.006)	(0.006)	(0.006)
$D^{0.1} \times Post$			0.012	-0.000	0.026^{***}	0.012^{*}
			(0.010)	(0.005)	(0.006)	(0.006)
$D^{0.1\sqrt{2}} \times Post$			-0.004	0.008	-0.004	-0.008
			(0.011)	(0.011)	(0.010)	(0.010)
Num. obs.	265255	265255	265255	265255	265255	265255
R^2	0.636	0.788	0.788	0.788	0.788	0.788
Quarterly FE	Y	Y	Y	Y	Y	Y
Census Tract FE	N	Y	Y	Y	Y	Y
Post 9/11/00 Window	180 Days	180 Days	180 Days	90 Days	60 Days	30 Days

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

Table 8 displays results for the time-varying effect of Arab neighbors following September 11, 2000. The variable $D^{0.1} = 1$ ($D^{0.3}$=1) if the transaction is within 0.1 miles (0.3 miles) of an Arab homeowner. The variable $Post = 1$ if the transaction is between 0 and 180 days after September 11, 2000. The window for *Post* is varied from 180, 90, 60, and 30 days. All regressions include log square footage, bedrooms, and bathrooms as control variables. Standard errors are clustered at the census tract and quarter levels.