

# An Empirical Test of Anti-Muslim Bias: Evidence from Property Values

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April 2021

## **Abstract**

While recent anecdotal evidence suggests that anti-Muslim bias is present and rising in the U.S., it is difficult to assess empirically. In this paper, we propose a test for anti-Muslim bias by asking whether the introduction of a mosque in a neighborhood reduces property values. To do so, we link administrative data on property sales prices in Michigan to the opening dates of new mosques. We then compare sales prices over time for properties closer and farther away from newly-opened mosques. Estimates using a repeat-sales framework indicate that the introduction of mosques to a neighborhood does not depress property prices. We likewise observe a similar null effect from the introduction of churches. We conclude that we find little evidence of anti-Muslim bias.

JEL Codes: R31, H0, J1

Keywords: religious discrimination, religious bias, mosques, Muslim

Declaration of interest: None

# 1 Introduction

The Muslim population in the US is growing at a fast pace. It is projected that Islam will overtake Judaism as the second-largest religion in the United States by 2040, in large part due to immigration from Muslim-majority countries (Pew Research Center, 2018). The growth of the Muslim population has been associated with a perceived rise in anti-Muslim sentiment. In 2017, the Pew Research Center analyzed data published by the FBI and found that hate crimes against Muslims are rising, with the number of assaults on Muslims, higher relative to the months immediately following the September 11 attacks. Likewise, a Pew Research Center survey revealed that a majority of American Muslims feel that being Muslim is becoming more difficult in the US (Pew Research Center, 2017). Even the FBI data likely do not fully capture the situation. The Bureau of Justice Statistics surveys suggest that hate crimes are severely under-reported to the FBI, with the real numbers about 25 times higher (Bureau of Justice Statistics, 2017).

While all this suggests that anti-Muslim bias is increasing over time, as with all forms of bias it is difficult to provide definitive evidence. We offer a new test of anti-Muslim bias by asking whether the introduction of a mosque in a neighborhood leads to a reduction in housing prices. We note that mosques are a particularly good point of emphasis for this test, given that anti-mosque incidents have increased by a factor of eight in the last 15 years, as tracked by the American Civil Liberties Union (ACLU).<sup>1</sup>

The purpose of this study is to provide an empirical test of whether there is anti-Muslim bias in the United States. To do so, we ask whether new mosque openings are capitalized into housing prices. In the presence of substantial anti-Muslim bias, we would observe a reduction in sales prices. We link administrative data on property transactions from 2002 to 2018 in Detroit and Hamtramck, Michigan, to the opening dates of all places of worship in those two cities. We address concerns about the potential endogeneity in the location of mosques by exploiting the granular nature of our data, which allows for a spatial difference-in-differences research design using repeat-sales of the same properties. In

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<sup>1</sup>The ACLU regularly updates the following page, which contains a description of the incidents: <https://www.aclu.org/issues/national-security/discriminatory-profiling/nationwide-anti-mosque-activity>

particular, we compare outcomes for properties that are very close to a new mosque to those that are slightly farther away. The identifying assumption of our approach is that the changes in property prices across these two areas would have been similar in the absence of the new mosque. Importantly, we show that outcomes trended similarly in the years prior to the entry of the new mosques, which is consistent with the identifying assumption.

Our results provide evidence against the hypothesis that anti-Muslim sentiment is capitalized into property prices following the entry of a new mosque. In our baseline specification, we rule out negative effects greater than 2 percent for properties within walking distance (around 0.2 mile) from a new mosque. Using a wild cluster bootstrap, we are able to rule out even smaller negative effects. While our main results focus on repeat-sales data that enables us to include individual property fixed effects, we also obtain similar results when using the universe of property sales and directly controlling for observed property-level characteristics. We also do not see any evidence of differential changes in the turnover rate of neighboring properties in the time period around mosque openings.

We also assess whether the absence of a negative effect on property values is because other positive aspects of a nearby house of worship offset the effect of anti-Muslim bias. To do so, we test for an effect of new churches in the neighborhood, and show that the null effect for mosques is mirrored by a null effect of churches. This provides further evidence against the hypothesis that anti-Muslim bias is capitalized into housing prices, as both churches and mosques have the same (null) impact on housing prices.

We note that one potential limitation of our approach is that there may be offsetting effects of demand for housing near mosques. While non-Muslim residents might want to move away due to anti-Muslim bias, they may be offset by Muslims wanting to move to those areas. Instead, our approach captures the net impact of both potential effects. We interpret our null results to mean that if religious discrimination against Muslims exists, it is not strong enough to offset increased demand for properties near mosques. We also

analyze the predicted ethnic origin based on names of buyers and find little evidence of an influx of Muslim buyers.

To our knowledge, this is the first paper to use data on repeat sales of properties to test whether the entry of new mosques cause anti-Muslim bias to be capitalized into housing prices. We are closely related to research on the effects of terror attacks on anti-Muslim sentiment (Lepage, 2020 and Ratcliffe and von Hinke Kessler Scholder, 2015). Unlike these studies, we seek to assess religious discrimination by focusing solely on increased salience from new mosques, separately from the effects of terrorist attacks, as terrorist attacks may also cause a shock to perceived safety.

We also contribute to an existing body of research that use hedonic pricing models to quantify the contribution of places of worship to housing values (e.g., Do, Wilbur, and Short, 1994, Carroll, Clauretje, and Jensen, 1996; Ottensmann, Bielefeld, and Payton, 2006). In particular, our study is closely related to a working paper by Brandt, Maennig, and Richter (2013). Using a model that allows for spatial dependence, as well as a rich set of controls, they find that places of worship in Hamburg, Germany are associated with an increase in housing prices, and that the effect of mosques is not different from that of other churches. Our approach differs from theirs in that we leverage the opening of new mosques to identify causal effects.

In addressing the effects of mosque openings, this paper also speaks to the literature on the impact of potentially controversial facilities on housing prices. For example, Dröes and Koster (2016) examined the effects of wind turbines on housing prices and estimated a decrease of 1.4 percent in housing prices. Daams, Proietti, and Veneri (2019) studied the impact of asylum seekers' reception centers and found that the openings of these centers resulted in a drop of as much as 9.3 percent in house prices. Chirakijja (2021) studied the impact of prison openings and estimated that prisons decrease housing values by 2-4 percent. As our lower bound estimates allow us to rule out decreases larger than 2 percent in housing prices, it would seem that although some people may have concerns and

prejudice against mosques, they are not nearly as large as those against prisons and asylum seekers' reception centers.

The rest of the paper proceeds as follows. Section 2 provides a background of the two cities we consider. Section 3 introduces the data and empirical strategy that we use, and Section 4 discusses our results. Section 5 concludes.

## **2 Muslims and Mosques in the US**

### **2.1 Growth of the Muslim Population**

Although Muslims still form only about 1% of the population, immigration from countries with significant Muslim populations makes Islam among the fastest growing religions in the US. Muslims tend to be more concentrated in some parts of the country. States such as New Jersey, New York, and Arkansas have a greater share of Muslims in the adult population than other states.<sup>2</sup> There is also variation within states, with metro areas like Detroit and its enclave Hamtramck having a significant share of Muslims among their adult populations.

The two cities we examine in this paper, Detroit and Hamtramck, are two of the most densely populated cities in Michigan. Detroit is the largest city in the state, while Hamtramck is essentially an enclave of Detroit. While still majority Christian, both cities have large Muslim populations. Hamtramck in particular, after decades of being comprised of mostly Catholic Polish immigrants, has a large Muslim population due to immigration from countries such as Bangladesh, Yemen, and Bosnia (Perkins, 2010). The Muslim population is ethnically diverse, with almost equal parts South Asian, Middle Eastern, and European. These factors makes Detroit and Hamtramck uniquely suited to an examination of the effects of mosques on housing prices.

### **2.2 Potential Effects of Mosques**

The construction of mosques has proven to be a controversial issue. In Hamtramck, some residents have complained about the externality effects of mosques, especially the daily

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<sup>2</sup><https://www.pewforum.org/religious-landscape-study/religious-tradition/muslim/>

Adhan, or the call to prayer, which starts at sunrise and repeats several times until the evening (Bailey, 2015). The city of Hamtramck has allowed the broadcast since 2004. Some have also voiced fears over Islam becoming a state religion, as some Muslim immigrants come from countries where Islam is a state religion (Barro and McCleary, 2005). Without further analysis, it is difficult to determine whether these complaints are tainted by anti-Muslim sentiment rather than purely a general sentiment against places of worship in neighborhoods.

We note that mosques and Islamic centers not only provide religious services, but also playgrounds, day care centers, and a place for social gatherings among people in the neighborhood. These are potentially positive contributions to neighborhood amenities, similar to what other places of worship provide.

Because mosques necessarily become a neighborhood feature, we focus on their effect on housing prices. There is a large body of empirical literature that examines the extent to which neighborhood attributes are capitalized into housing prices.<sup>3</sup> As with many other studies, we rely on the framework introduced by Rosen (1974) and adapt it to study the effect of mosques.

### 3 Data and Empirical Strategy

#### 3.1 Data

We rely on administrative data for our key variables. To determine the location, as well as the opening dates of the mosques and churches in Detroit and Hamtramck, we queried the Corporations Online Filing System on the Michigan Department of Licensing and Regulatory Affairs website.<sup>4</sup> This gave us the universe of mosque and church openings during the time period 2002-2018. Appendix Table A.1 summarizes the mosque and church openings that occur within the time span of our data. We observe 10 mosque openings and 13 church openings between 2002 and 2018.

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<sup>3</sup>Mendelsohn and Olmstead (2009) summarize some research on the valuation of environmental attributes.

<sup>4</sup>See <https://cofs.lara.state.mi.us/SearchApi/Search/Search>. We used the keywords masjid, mosque, Islamic Center, Muslim, Muslem, church, and synagogue.

We obtained data on property sales transactions from the City Clerk’s offices of Detroit and Hamtramck. We restrict our analysis to non-token sales.<sup>5</sup> The outcome data are summarized in Table 1, which summarizes data on all property sales (Panel A) and repeat sales (Panel B). The full data contain almost 12,000 property sales. Repeat sales are properties that were sold more than once during our study period. They comprise 73 percent of all property sales we observe. We use repeat sales to account for time invariant property-level characteristics in our primary specification. Our primary sub-sample thus contains around 8,500 sales. The mean sales price is under \$60,000, and most properties within the study zone are residential.

Table 1: Property sales data

Panel A: All sales

	All		Controls		Treated	
	mean	sd	mean	sd	mean	sd
sales price	58,660.09	(206872.89)	51,550.32	(149947.56)	68,855.76	(267878.54)
residential	0.97	(0.18)	0.97	(0.17)	0.96	(0.19)
acreage	0.13	(1.01)	0.10	(0.64)	0.18	(1.38)
front	32.00	(45.80)	29.79	(29.24)	35.18	(62.15)
depth	90.99	(52.86)	89.80	(47.86)	92.70	(59.26)
floor area	1,563.25	(5192.65)	1,527.27	(2853.93)	1,614.97	(7350.03)
Observations	11771		6935		4836	

Panel B: Only repeat sales

	All		Controls		Treated	
	mean	sd	mean	sd	mean	sd
sales price	60,278.95	(238829.22)	52,325.91	(171705.99)	71,716.25	(310590.37)
residential	0.98	(0.14)	0.98	(0.14)	0.98	(0.14)
acreage	0.13	(1.12)	0.10	(0.74)	0.19	(1.50)
front	31.33	(43.95)	29.35	(29.77)	34.19	(58.50)
depth	90.00	(51.43)	89.04	(47.22)	91.39	(56.91)
floor area	1,549.24	(5624.59)	1,510.11	(2470.09)	1,605.34	(8262.06)
Observations	8548		5042		3506	

Notes: This table shows the summary statistics of property sales transactions used for the analysis. Panel 1 shows the summary statistics of *all sales transactions* that happened between 2002 and 2018 of the properties that are within the 0.4 mile radius from the new mosques. Panel 2 shows the summary statistics of the sales transactions of the properties that are within 0.4 mile radius from the new mosques and were *sold at least twice* between 2002 and 2018. Treated properties are those within the treatment radius which is 0.2 miles, while the comparison properties are between 0.2 and 0.4 mile from the new mosques.

Because property sales data include the names of buyers and sellers, we are also able to conduct ethnic name analysis to determine the likelihood that the parties involved in a property transaction are Muslim. The tool we use to conduct this analysis is NamePrism

<sup>5</sup>Token sales are sales of value under \$5,000.

(Ye et al., 2017; Ye and Skiena, 2019).<sup>6</sup>

### 3.2 Empirical Strategy

We base our empirical strategy on the standard hedonic pricing approach developed by Rosen (1974), and applied in the empirical literature in studies such as Pope and Pope (2015), McMillen (2004), Zabel (2008), among many others. We modify the standard model by incorporating a spatial difference-in-differences design, exploiting the plausibly exogenous location of the new mosques, shown in Appendix Figure A.1. The modified hedonic model is of the form :

$$\log(P_{it}) = \alpha_t + \gamma\mathbf{X} + \beta \cdot \text{post mosque opening} \times \text{treated zone}_{it} + \epsilon_{it} \quad (1)$$

where  $\log(P_{it})$  is the log of the sales price of property  $i$  that was sold in year-month  $t$ ,  $\alpha_t$  represents year-month fixed effects, which captures period specific shocks common to all properties, and  $\mathbf{X}$  is a set of controls for property-level characteristics.  $\text{post mosque opening} \times \text{treated zone}_{it}$  is a binary variable indicating that property  $i$  is in the treated zone, i.e. located within 0.2 mile, or approximately 500 steps of a new mosque, and that the transaction in year-month  $t$  occurred after the associated mosque’s opening. The analysis neighborhood is defined to be twice the size of the treated zone, in this case, a 0.40 mile radius around the new mosque. The coefficient of interest here is  $\beta$ , which measures the effect of mosque openings on log property prices for properties within 0.20 mile of a new mosque, relative to the properties between 0.20 and 0.40 mile of that mosque.

While we estimate Equation 1 with all available controls for property-level characteristics, due to data limitations, our data might not capture all the important differences between properties that drive sales prices. Our preferred estimates are instead based on a repeat-sales framework using data on properties that were sold more than once between 2002 and 2018. Formally, we estimate the effects of mosque openings using the following model:

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<sup>6</sup>We are grateful to Junting Ye and Steven Skiena for allowing us to access this web tool.

$$\log(P_{it}) = \alpha_i + \alpha_t + \beta \cdot \text{post mosque opening} \times \text{treated zone}_{it} + \epsilon_{it} \quad (2)$$

where  $\alpha_i$  represents property fixed effects, which captures time-invariant property-level characteristics that contribute to the sales price. As in Equation 1,  $\alpha_t$  is year-month fixed effects.  $\text{post mosque opening} \times \text{treated zone}_{it}$  is a binary variable indicating that property  $i$  is in the treated zone and that the transaction in year-month  $t$  occurred after the associate mosque's opening. The coefficient of interest is still  $\beta$ , which measures the effect of mosque openings on log property prices, accounting for the time-invariant property-level characteristics.

The inclusion of property fixed effects helps us account for differences we cannot observe in the data, such as the number of bedrooms and bathrooms, the general condition of the house, among others. The identifying assumption is that the change in the sales price experienced by the properties slightly farther away from the new mosque provides a valid counterfactual for the change in sales prices that the treated properties would have experienced had the new mosque not opened. Robust standard errors are clustered at the level of the neighborhood surrounding the newly opened mosque. We also use a wild cluster bootstrap to account for the possibility that we may have too few treated clusters (Roodman et al., 2019).

To assess the validity of our identifying assumption, as well as to visualize the dynamics of the response of house prices, we also estimate a dynamic version of Equation 2:

$$\log(P_{it}) = \sum_l \delta_l \cdot I\left[\frac{t - \text{mosque opening date}_i}{90} = l\right] \times \text{treated zone}_i + \alpha_i + \alpha_t + \epsilon_{it} \quad (3)$$

where  $t$  is the sale date of property  $i$ .  $\text{mosque opening date}_i$  is the opening date of property  $i$ 's associated new mosque.  $l$  signifies the lead or lag quarter, of property's  $i$  transaction date relative to the associated mosque's opening date.  $\text{treated zone}_i$  again indicates whether property  $i$  is in the treated zone.  $\alpha_i$  is property fixed effects and  $\alpha_t$  is year-month

fixed effects. Here, Equation 3 estimates the dynamic effects ( $\delta_l$ ) at each point in time of mosque openings on log property prices. The validity of the research design would be supported by estimating null effects for the leading terms.

## 4 Results

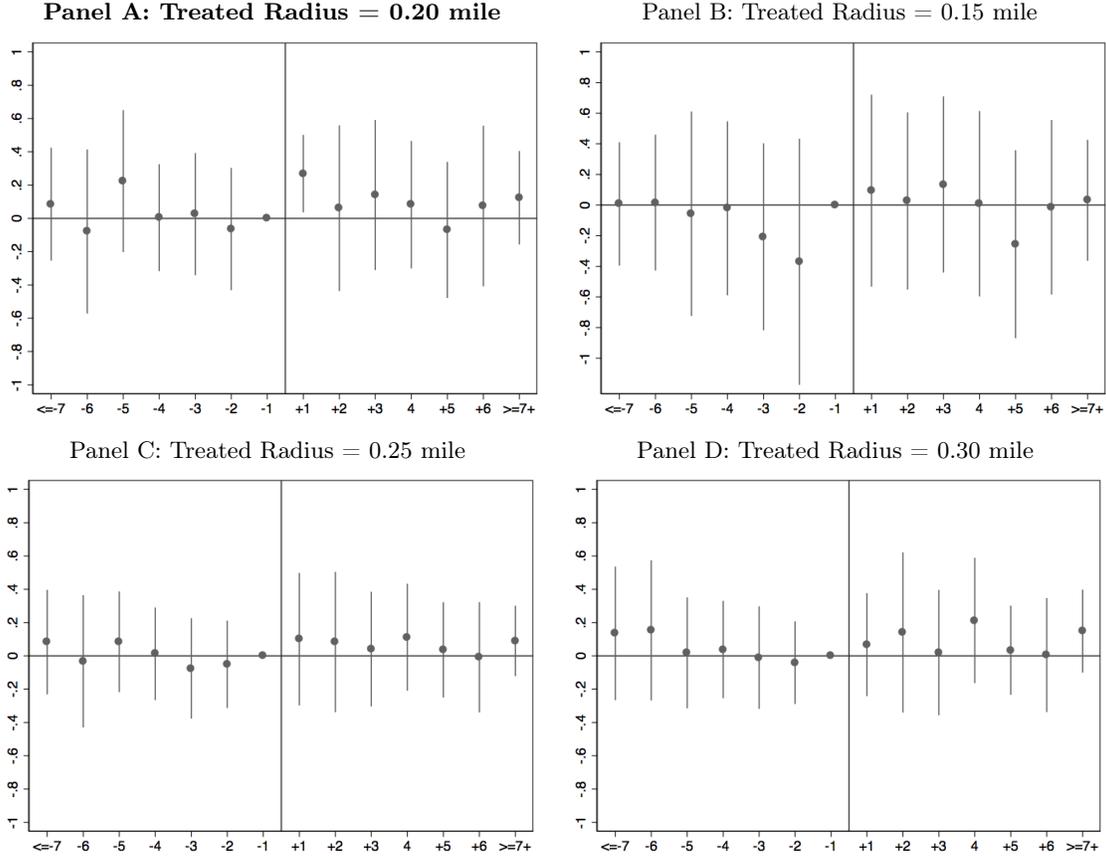
### 4.1 The Effect of Mosque Openings

First, we examine the validity of the parallel trends assumption required for a difference-in-differences method. To do so, we estimate a dynamic difference-in-differences model as shown in Equation 3. Panel A of Figure 1 shows the dynamic difference-in-differences estimates when the treated properties are defined to be those within 0.20 mile distance from a newly opened mosque. The comparison properties are those that are up to twice the distance away. This cutoff was chosen to approximate a mosque being walking distance to a property.<sup>7</sup> Figure 1 shows that there is little evidence of divergence in property prices before a new mosque opens in the neighborhood, providing some evidence in favor of our identifying assumption. Moreover, there does not appear to be a significant effect of new mosques on property prices. The event studies for other definitions of the treated zone give similar results.

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<sup>7</sup>The 0.20 mile distance is roughly 500 steps.

Figure 1: Dynamic difference-in-difference estimates of mosque openings on log property prices



Notes: This figure presents the dynamic difference-in-differences estimates of mosque openings on the log of property prices. The X-axis shows quarter relative to mosque opening. Each dot plots  $\delta_t$ , i.e. the coefficient of the treated zone indicator interacted with the relative quarter dummy during which the property sale took place, from Equation 3. The sample only includes sales transactions data of properties that were sold at least twice between 2002 and 2018 (*repeat sales*). The sample radius is two times the size of the treated radius. Standard errors in parentheses are clustered at the level of the neighborhood surrounding the newly opened church.

This is borne out by the formal estimates presented in Panel A of Table 2, which reports the average treatment effects of new mosque openings across different specifications. Each column presents either the estimate of the coefficient on our variable of interest,  $post\ mosque\ opening \times treated\ zone_{it}$  from the repeat-sales model of Equation 2, or from the modified hedonic model of Equation 1 when using all sales with controls. There appears to be no significant negative effect of mosque openings, in contrast to what detractors of mosques suggest. Column 1 reports the estimates from the preferred specification, i.e., Equation 2, for all repeat sales when the treated zone is 0.20 mile from a new mosque and the comparison group are properties between 0.20 and 0.40 mile away

from the new mosque. We estimate an insignificant increase of approximately 6 percent in property prices. Importantly, the lower bound estimates here allow us to rule out negative effects larger than 2 percent, while our 95% confidence interval based on a wild cluster bootstrap rules out any meaningful negative effects.

Table 2: Effects of mosque and church openings on the log of property prices (all property types)

Panel A: Effects of mosque openings

	treated radius=0.20 mile			0.15 mile			0.25 mile			0.30 mile		
	(1) repeat sales	(2) all sales	(3) all sales	(4) repeat sales	(5) all sales	(6) all sales	(7) repeat sales	(8) all sales	(9) all sales	(10) repeat sales	(11) all sales	(12) all sales
treated x post	0.0614 (0.0395)	0.103 (0.0546)	0.0547 (0.0373)	0.0583 (0.108)	0.159 (0.0921)	0.0637 (0.0711)	0.0388 (0.0627)	0.00118 (0.0386)	0.00101 (0.0324)	0.0388 (0.0744)	-0.0131 (0.0375)	-0.0292 (0.0345)
95% CI	[-0.020, 0.142]	[-0.009, 0.214]	[-0.022, 0.131]	[-0.165, 0.282]	[-0.032, 0.350]	[-0.084, 0.211]	[-0.089, 0.167]	[-0.077, 0.080]	[-0.065, 0.067]	[-0.111, 0.189]	[-0.089, 0.063]	[-0.099, 0.040]
Bootstrap 95% CI	[-0.008, 0.141]	[-0.026, 0.235]	[-0.038, 0.139]	[-0.167, 0.253]	[-0.035, 0.376]	[-0.077, 0.242]	[-0.065, 0.154]	[-0.082, 0.096]	[-0.079, 0.089]	[-0.098, 0.172]	[-0.105, 0.075]	[-0.117, 0.061]
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property fixed effects	Yes			Yes			Yes			Yes		
Area fixed effects		Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes
Controls 1		Yes			Yes			Yes			Yes	
Controls 2			Yes			Yes			Yes			Yes
N	8548	11771	7432	6373	8788	5558	10543	14560	9271	11879	16552	10736

Panel B: Effects of church openings

	treated radius=0.20 mile			0.15 mile			0.25 mile			0.30 mile		
	(1) repeat sales	(2) all sales	(3) all sales	(4) repeat sales	(5) all sales	(6) all sales	(7) repeat sales	(8) all sales	(9) all sales	(10) repeat sales	(11) all sales	(12) all sales
treated x post	0.0855 (0.108)	0.0553* (0.0273)	0.0318* (0.0161)	0.00697 (0.105)	0.00729 (0.0305)	-0.00437 (0.0204)	0.0587 (0.0754)	0.0275 (0.0354)	0.00477 (0.0309)	0.0380 (0.0523)	0.0267 (0.0345)	0.0149 (0.0271)
95% CI	[-0.149, 0.320]	[-0.004, 0.115]	[-0.003, 0.067]	[-0.222, 0.236]	[-0.059, 0.074]	[-0.049, 0.040]	[-0.103, 0.220]	[-0.048, 0.103]	[-0.061, 0.071]	[-0.074, 0.150]	[-0.047, 0.101]	[-0.043, 0.073]
Bootstrap 95% CI	[-0.114, 0.372]	[-0.018, 0.114]	[-0.008, 0.063]	[-0.212, 0.261]	[-0.070, 0.089]	[-0.058, 0.046]	[-0.074, 0.231]	[-0.060, 0.102]	[-0.067, 0.072]	[-0.062, 0.148]	[-0.068, 0.095]	[-0.062, 0.070]
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property fixed effects	Yes			Yes			Yes			Yes		
Area fixed effects		Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes
Controls 1		Yes			Yes			Yes			Yes	
Controls 2			Yes			Yes			Yes			Yes
N	7969	12086	9358	4563	7030	5375	12397	18463	14430	18062	26509	21033

Notes: Control 1 each property's include acreage, front, depth. Control 2 include floor area and year built in addition to the characteristics included in Control 1.

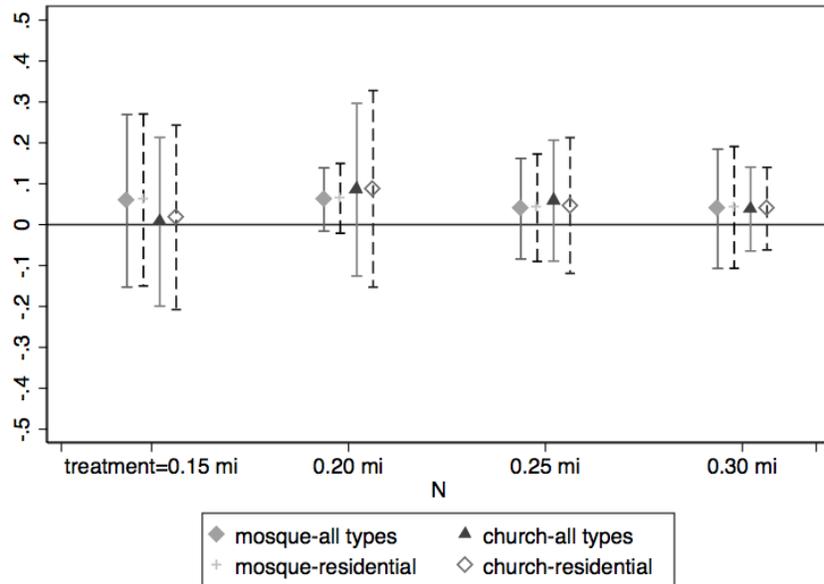
\* p<0.10, \*\* p<0.05, \*\*\* p<0.010

Notes: This table reports the estimated effects of mosque openings and church openings on the log of property prices in Panel 1 and Panel 2, respectively. Columns 1, 4, 7, and 10 report the estimated effects from the *repeat-sales model* shown in Equation 2 with the log of property prices as the outcome variable. The sales transaction data used in this specification only include the properties that were sold at least twice between 2002 and 2018. Columns 2, 5, 8, and 11 report the estimated effects from the *modified hedonic model* shown in Equation 1. In this specification, we use all sales transactions between 2002 and 2018 and include the controls for acreage, front, and depth of the property. Columns 3, 6, 9, and 12 also report the estimated effects from the *modified hedonic model* shown in Equation 1. However, in this specification, the controls include the property's floor area and year built in addition to acreage, front, and depth. The sample radius is two times the size of the treated radius. Standard errors in parentheses are clustered at the level of the neighborhood surrounding the newly opened mosque/church. 95% CI reports clustered-robust confidence intervals. Bootstrap 95% CI reports the confidence intervals from wild cluster bootstrap.

Columns 2 and 3 report the estimates from the modified-hedonic model shown in Equation 1. We use two sets of property characteristics, grouped by data availability, since data on some property characteristics is sometimes available only for a subset of property sales. The lower bounds of these estimates also allow us to rule out negative effects larger than 2 percent.

Although our results show insignificant effects of mosque openings on property prices, we cannot immediately conclude that this is evidence of no religious discrimination, especially if church openings result in significant increases in property prices. We assess the possibility that church openings may affect property prices differently from mosque openings by estimating the impact of church opening on property prices. The estimated effects of church openings are reported in Panel B of Table 2 and are similar, though less precise, to the estimates of mosque openings in Panel A. The similarity in magnitude of these estimates is more easily seen in Figure 2, which graphs the estimated coefficients for our variables of interest,  $post\ mosque\ opening \times treated\ zone_{it}$  and  $post\ church\ opening \times treated\ zone_{it}$ , across different possible definitions of the treated and comparison zones.

Figure 2: Estimated effects of mosque openings and church openings, by definition of treated radius and type of property



Notes: This figure presents the estimated effects of mosque openings and church openings on log property prices across property type and treatment and sample radius choice. All estimations use the *repeat-sales specification* in Equation 2 with only sales transactions data of properties that were sold at least twice between 2002 and 2018. Sample radius are twice the size of the treated radius. Standard errors are clustered at the level of the neighborhood surrounding the newly opened mosque (or church).

Our discussion of the potential effects of new mosque openings in Section 2 suggests that mosques may provide amenities to Muslims that might drive an increase in demand for housing near mosques large enough to offset anti-Muslim bias. Using names of buyers, we conduct an analysis that asks whether new mosque openings lead to an influx of Muslims moving closer to mosques. Specifically, we estimate the impact of mosque openings on the likelihood of the property buyer being Muslim, using models similar to Equations 1 and 2. The results of this analysis are presented in Table 3. Our estimates do not indicate a large influx of Muslim buyers into areas near new mosques.

Table 3: Effects of mosque openings on the likelihood of buyers being Muslim

	treated radius=0.20 mile			0.15 mile			0.25 mile			0.30 mile		
	(1) repeat sales	(2) all sales	(3) all sales	(4) repeat sales	(5) all sales	(6) all sales	(7) repeat sales	(8) all sales	(9) all sales	(10) repeat sales	(11) all sales	(12) all sales
treated x post	-0.0126 (0.0332)	-0.00112 (0.00903)	0.00725 (0.00655)	-0.0234 (0.0314)	-0.0109 (0.0123)	0.00122 (0.00757)	0.00364 (0.0230)	-0.00183 (0.00773)	0.000398 (0.00855)	-0.0130 (0.0244)	-0.00157 (0.00659)	-0.00259 (0.00819)
95% CI	[-0.081, 0.056]	[-0.020, 0.017]	[-0.006, 0.021]	[-0.089, 0.042]	[-0.036, 0.015]	[-0.014, 0.017]	[-0.043, 0.050]	[-0.018, 0.014]	[-0.017, 0.018]	[-0.062, 0.036]	[-0.015, 0.012]	[-0.019, 0.014]
Bootstrab 95% CI	[-0.081, 0.042]	[-0.021, 0.018]	[-0.011, 0.020]	[-0.078, 0.028]	[-0.039, 0.018]	[-0.012, 0.018]	[-0.038, 0.042]	[-0.019, 0.015]	[-0.017, 0.024]	[-0.059, 0.029]	[-0.017, 0.012]	[-0.023, 0.016]
mean	0.267	0.265	0.265	0.276	0.275	0.275	0.260	0.258	0.258	0.253	0.251	0.251
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property fixed effects	Yes			Yes			Yes			Yes		
Area fixed effects		Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes
Controls 1		Yes			Yes			Yes			Yes	
Controls 2			Yes			Yes			Yes			Yes
N	8179	11272	7333	6090	8409	5479	10106	13972	9152	11404	15915	10597

\* p<0.10, \*\* p<0.05, \*\*\* p<0.010

Notes: This table reports the estimated effects of mosque openings on the likelihood of the property buyer being Muslim. Columns 1, 4, 7, and 10 report the estimated effects from the *repeat-sales model* shown in Equation 2 with the likelihood of the property buyer being Muslim as the outcome variable. The sales transaction data used in this specification only include the properties that were sold at least twice between 2002 and 2018. Columns 2, 5, 8, and 11 report the estimated effects from the *modified hedonic model* shown in Equation 1. In this specification, we use all sales transactions between 2002 and 2018 and include the controls for acreage, front, and depth of the property. Columns 3, 6, 9, and 12 also report the estimated effects from the *modified hedonic model* shown in Equation 1. However, in this specification, the controls include the property's floor area and year built in addition to acreage, front, and depth. The sample radius is two times the size of the treated radius. Standard errors in parentheses are clustered at the level of the neighborhood surrounding the newly opened mosque. 95% CI reports clustered-robust confidence intervals. Bootstrap 95% CI reports the confidence intervals from wild cluster bootstrap.

## 4.2 Robustness Checks

As our main estimates rely on a definition of the treated zone of properties being within 0.2 mile of a new mosque, we examine the sensitivity of our estimates to changing this cutoff. We start by defining the treated zone as being within 0.15 mile, and then increasing by 0.05 mile increments until 0.3 mile. With each definition of the treated zone, we define the comparison zone as being twice the distance to the mosque (i.e., comparison zone for the 0.15 mile definition is the area greater than 0.15 mile but less than or equal to 0.30 mile to the new mosque). We also do this exercise for church openings. The results are presented in Columns 4-12 of Panel A in Table 2 for mosque openings, and Panel B for church openings.

These tables report the effects of mosque and church openings, i.e. the coefficient of variables  $post\ mosque\ opening \times treated\ zone_{it}$  and  $post\ church\ opening \times treated\ zone_{it}$ , respectively, for different constructed treated zones. The estimated coefficients are not significantly different from our main estimate, indicating that our results are not sensitive to arbitrarily small changes in how we define the properties treated by the new mosque or church.

In Appendix Table A.2, we also estimate Equations 1 and Equation 2 using data on only residential property sales. Our estimates are qualitatively similar, as would be expected since more than 90 percent of property sales transactions in our sample are of residential properties. Finally, in Appendix Table A.3, we show that there is no change in the turnover rate of properties in Detroit during the time period around a new mosque opening, consistent with our main results.

## 5 Conclusion

We combine a spatial difference-in-differences framework with a standard hedonic price model to propose a test for religious discrimination based on the impact of new mosques on property values. Finding a negative effect on property values would have indicated that

new mosques cause property values to fall as a result of religious discrimination.

Given the rhetoric surrounding new mosque openings, one would expect that new mosque openings would significantly reduce property values. However, the results presented in this paper provide little evidence of this. Using a spatial difference-in-differences research design on repeat-sales data of properties in Detroit and Hamtramck, we are able to rule out negative effects larger than 2 percent. These results are robust to differently constructed treated zones, and to alternative specifications that use all property sales, with observable property characteristics. We also find little evidence that our null results are driven by anti-Muslim bias being offset by Muslim buyers wanting to move closer to mosques, or that properties became more likely to be sold.

Taken together, our findings of the effects of mosque and church openings suggest that even if residents or prospective buyers choose to avoid the neighborhood as a result of a new mosque opening, it appears that this response is not strong enough to offset demand for these properties. While our estimates rule out reductions in property prices of greater than two percent, Daams, Proietti, and Veneri (2019) and Chirakijja (2021) estimated much larger negative effects of asylum seekers reception center and prisons, respectively, facilities around which there also exist potentially discriminatory rhetoric. Because our estimates suggest that new mosques do not appear to have the same negative impact on property prices as these facilities, we conclude that there is little evidence of religious discrimination against Muslims, at least in a setting that abstracts from terrorist attacks and the ensuing effects on perceived safety.

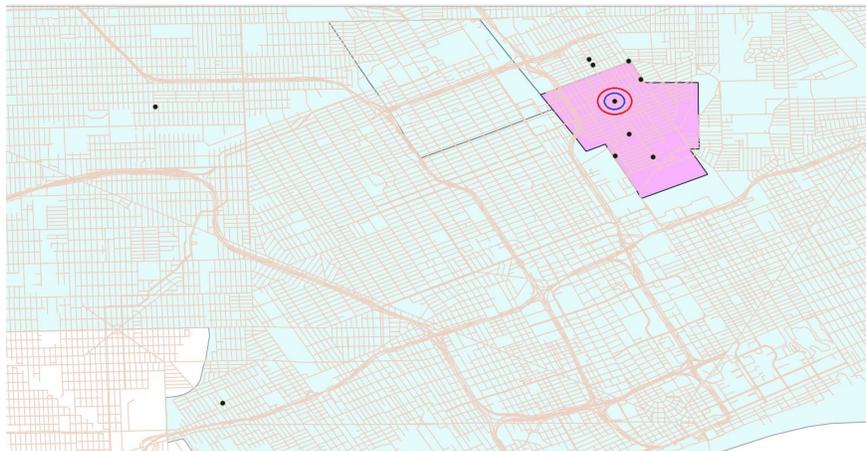
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# Appendix

Figure A.1: Map of Mosque Openings in Detroit and Hamtramck



Notes: This figure shows the new mosques (black dots) we study in this paper, and illustrates the spatial difference-in-differences framework. For example, consider the mosque inside the blue and red circles. The properties inside the dark blue circle are in the “treated zone” while the properties inside the red circle but outside the blue circle are in the comparison group.

Table A.1: Mosque and church openings between 2002-2018

Mosque		Church	
Year	Openings	Year	Openings
2003	2	2002	1
2004	1	2003	2
2005	1	2004	2
2006	1	2005	2
2008	1	2006	1
2010	1	2007	1
2011	1	2008	1
2012	1	2011	1
2016	1	2013	1
		2014	1
Total	10	Total	13

Notes: The table shows the number of mosque and church openings each year during the study period (2002-2018).

Table A.2: Effects of mosque and church openings on the log of property prices (residential properties)

Panel A: Effects of mosque openings

	treated radius=0.20 mile			0.15 mile			0.25 mile			0.30 mile		
	(1) repeat sales	(2) all sales	(3) all sales	(4) repeat sales	(5) all sales	(6) all sales	(7) repeat sales	(8) all sales	(9) all sales	(10) repeat sales	(11) all sales	(12) all sales
treated x post	0.0642 (0.0435)	0.0903 (0.0539)	0.0491 (0.0359)	0.0604 (0.107)	0.155 (0.0968)	0.0531 (0.0716)	0.0412 (0.0670)	0.0105 (0.0362)	0.0152 (0.0340)	0.0420 (0.0760)	-0.00365 (0.0328)	-0.00362 (0.0353)
95% CI	[-0.025, 0.153]	[-0.020, 0.201]	[-0.025, 0.123]	[-0.162, 0.283]	[-0.046, 0.355]	[-0.095, 0.202]	[-0.095, 0.178]	[-0.063, 0.084]	[-0.054, 0.084]	[-0.112, 0.195]	[-0.070, 0.062]	[-0.075, 0.068]
Bootstrap 95% CI	[-0.012, 0.148]	[-0.029, 0.224]	[-0.040, 0.133]	[-0.162, 0.254]	[-0.039, 0.393]	[-0.085, 0.239]	[-0.073, 0.166]	[-0.074, 0.098]	[-0.070, 0.104]	[-0.092, 0.182]	[-0.079, 0.077]	[-0.092, 0.086]
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property fixed effects	Yes			Yes			Yes			Yes		
Area fixed effects		Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes
Controls 1		Yes			Yes			Yes			Yes	
Controls 2			Yes			Yes			Yes			Yes
N	8379	11382	7202	6257	8507	5406	10284	14012	8948	11537	15826	10302

Panel B: Effects of church openings

	treated radius=0.20 mile			0.15 mile			0.25 mile			0.30 mile		
	(1) repeat sales	(2) all sales	(3) all sales	(4) repeat sales	(5) all sales	(6) all sales	(7) repeat sales	(8) all sales	(9) all sales	(10) repeat sales	(11) all sales	(12) all sales
treated x post	0.0875 (0.123)	0.0358 (0.0280)	0.0280 (0.0165)	0.0179 (0.115)	0.00173 (0.0382)	0.00247 (0.0350)	0.0468 (0.0847)	0.0203 (0.0302)	0.000642 (0.0222)	0.0391 (0.0515)	0.0142 (0.0270)	0.00411 (0.0217)
95% CI	[-0.180, 0.355]	[-0.025, 0.097]	[-0.008, 0.064]	[-0.233, 0.268]	[-0.081, 0.085]	[-0.074, 0.079]	[-0.135, 0.228]	[-0.045, 0.085]	[-0.047, 0.048]	[-0.071, 0.150]	[-0.044, 0.072]	[-0.042, 0.051]
Bootstrap 95% CI	[-0.159, 0.408]	[-0.060, 0.097]	[-0.040, 0.072]	[-0.211, 0.310]	[-0.104, 0.109]	[-0.090, 0.120]	[-0.103, 0.241]	[-0.074, 0.086]	[-0.063, 0.045]	[-0.065, 0.143]	[-0.074, 0.065]	[-0.062, 0.043]
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property fixed effects	Yes			Yes			Yes			Yes		
Area fixed effects		Yes	Yes		Yes	Yes		Yes	Yes		Yes	Yes
Controls 1		Yes			Yes			Yes			Yes	
Controls 2			Yes			Yes			Yes			Yes
N	7431	10807	8585	4238	6210	4897	11582	16659	13339	16909	24060	19514

\* p<0.10, \*\* p<0.05, \*\*\* p<0.010

Notes: Panel 1 of this table reports the estimated effects of mosque openings on the log of property prices of residential properties. Panel 2 of this table reports the estimated effects of church openings on the log of property prices of residential properties. Columns 1, 4, 7, and 10 report the estimated effects from the *repeat-sales model* shown in Equation 2 with the log of property prices as the outcome variable. The sales transaction data used in this specification only include the properties that were sold at least twice between 2002 and 2018. Columns 2, 5, 8, and 11 report the estimated effects from the *modified hedonic model* shown in Equation 1. In this specification, we use all sales transactions between 2002 and 2018 and include the controls for acreage, front, and depth of the property. Columns 3, 6, 9, and 12 also report the estimated effects from the *modified hedonic model* shown in Equation 1. However, in this specification, the controls include the property's floor area and year built in addition to acreage, front, and depth. The sample radius is two times the size of the treated radius. Standard errors in parentheses are clustered at the level of the neighborhood surrounding the newly opened mosque/church. 95% CI reports clustered-robust confidence intervals. Bootstrap 95% CI reports the confidence intervals from wild cluster bootstrap.

Table A.3: Turnover Rates of Residential and Single-Family Properties in Detroit

	(1)	(2)	(3)	(4)
	treated radius=0.20 miles	0.15 miles	0.25 miles	0.30 miles
treated x post	0.000542 (0.000575)	0.000588 (0.000513)	0.000259 (0.000213)	-0.000270 (0.000218)
95% CI	[-0.001, 0.002]	[-0.001, 0.002]	[-0.000, 0.001]	[-0.001, 0.000]
Bootstrap 95% CI	[-.0018, .0023]	[-.0011, .0026]	[-.0011, .0012]	[-.0010, .0009]
Year-month fixed effects	Yes	Yes	Yes	Yes
Property fixed effects	Yes	Yes	Yes	Yes
N	806208	564876	1073448	1357008

\* p<0.10, \*\* p<0.05, \*\*\* p<0.010

Notes: In this table, we examine the possibility that mosque openings increased the probability that properties were sold/turned over during the affected time period. Because our data for Hamtramck only includes properties that sold during the time period, we are restricted to Detroit for this turnover analysis. Each column presents estimates from a separate regression where we vary the width of the treated radius. Standard errors in parentheses are clustered at the level of the neighborhood surrounding the newly opened mosque/church. 95% CI reports clustered-robust confidence intervals. Bootstrap 95% CI reports the confidence intervals from wild cluster bootstrap.