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Three Essays in Urban Policy

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Abstract

The papers in this dissertation share a common theme of measuring policy effects in urban markets. Though focusing on different outcomes – access to rental housing, property values, and school enrollments – the desire to understand how policy influences the composition of an urban ecosystem provides the link. A particular emphasis is placed on housing, because housing is simultaneously and a basic requirement for humans to thrive and the primary source of most local finance in the U.S. When policy affects housing, be it directly or indirectly, it can have important and far-reaching consequences. Understanding the intended and unintended consequences of urban policy is thus central to this dissertation research and my future research agenda.

Chapter 1 offers the first evidence of differential treatment occurring across the broad spectrum of racial and protected classes covered under the law. Employing a fully randomized correspondence audit design and a sample of more than 9,500 online housing advertisements, the study offers insight about which protected groups experience the most/least favorable treatment when searching for housing. This study employs a new signaling strategy in order to provide the first evidence of how landlord treatment of rental housing applicants varies across the spectrum of protected classes. The findings suggest rental-housing providers have preferences about tenants and make decisions based on signals communicated in inquiry emails from potential applicants. The findings also suggest differential treatment is generally consistent with theory of agent-based statistical discrimination.

Chapter 2 presents an experiment designed to influence rental agent behavior to increase equal treatment in rental housing. The purpose is to test whether property owners and rental agents will change their behavior in response to being informed about their obligations under fair housing

law. The project thus conducts a randomized experiment employs a correspondence housing audit methodology to measure the impact, representing the first time in which the audit methodology is employed to measure the effect of a randomized experiment. The results of the experiment consistently suggest the group of landlords who received information about fair housing law responded at a higher rate than did those who did not receive the treatment email. The primary contribution of this paper, then, is to demonstrate a unique opportunity to test policy interventions aimed at reducing discrimination in a real housing market and at a very low cost. My hope is that the method will be modified and expanded by fair housing agencies, advocates, and other institutions to test and implement policy interventions in hopes of reducing barriers to access in housing.

Chapter 3 examines the impact of a place-based program on urban property values and school enrolments. A recent trend in place-based policy targets college attainment by offering tuition scholarships for qualified students in under-resourced public schools. In an era of rising college costs, these programs represent a potentially large financial benefit to those living within the attendance zones of qualifying schools. The benefit of such programs should be capitalized into local property values and school district enrolment, as programs are directly linked to attendance zones. This research thus examines the impact a large scholarship program, Say Yes to Education, has on school enrolments and property values in upstate New York. Examining district enrollment from 2000 through 2014, the analysis finds that after years of steady declines in enrollments, both Syracuse and Buffalo saw enrollment increases that coincide with the adoption of the Say Yes to Education program. These increases occurred at different points in time in each city. The housing values results provide some evidence that increases in housing prices accompanied the adoption of Say Yes in Syracuse, but not in Buffalo. These results are consistent with findings that enrolment growth in Buffalo may have been driven by students who would otherwise have attended private schools, while enrollment growth in Syracuse may have been driven by students who would otherwise have attended school in the surrounding suburbs. Combined with the enrolment effects, the analysis suggests that the ability of place-based scholarships to attract residents into a central city is likely to depend on both the specific provisions of the program and the context in which it is implemented.

Three Essays in Urban Policy

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My wife, Elizabeth, is the inspiration behind my decision to change careers in order to pursue something I am passionate about. Elizabeth was born to be a nurse practitioner and community health advocate. Watching her lead by example drove me to leave a successful career in financial services in order to attend graduate school and, hopefully, embark on a new career more closely aligned with empowering vulnerable populations. Elizabeth graciously became our primary earner, despite being a new mother of Isabella (then 1) and Vincent (an infant). Over my seven years of schooling at two institutions, the three of them have celebrated with me in the many good times and supported me during the challenging periods. Their love is constant and sustained me through this journey. Words are unable to express my gratefulness.

My parents, Jill and Phil Murchie, have also played pivotal roles. When I was in 6^{th} grade, my mom gave me the opportunity to be home-schooled for a single year. Jill, a lover of urban sociology and former elementary school teacher, spent the year exposing me to the amazing fabric of the great city of Chicago. We toured its many neighborhoods and studied how different people groups came to the city. We visited museums and ate local foods. She shared with me life lessons, such as "God's creativity is expressed in the diversity of the city" and, "you may never have a chance to visit *fill-in-the-blank* country, but you can learn a lot about it by getting to know its people that live in your city." In that year, I fell in-love with cities and became, me.

My father was equally essential. It is from him that my interest in scientific experiments originated and it is because of him the experimental portion of this dissertation was possible. He incorporates little experiments into everyday life, just to test hypotheses he makes about anything and everything. The transmission of the scientific method I received from him is evident in each phase of my life's path and is core to this dissertation. There is a practical reason Phil made this research possible. A computer expert, dad volunteered his time to create a program that allowed a semi-automated process for sample identification and collection. Were it not for his many volunteer hours, the dissertation would look very different.

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Chapter 2: Can Landlords Change: Evidence from a Randomized Experiment Targeting Discriminatory Behavior in Rental Housing

Chapter 3: Assessing the Effects of Place-Based Scholarships on Urban Revitalization: The Case of Say Yes to Education

Executive Summary

Rental Housing Discrimination (Chapters 1 and 2)

The objective of the first two papers is to measure housing discrimination (Chapter 1) and test a mitigation strategy (Chapter 2). The focus is rental housing, because the rapid growth of online platforms for advertising rental housing present significant opportunities for both good and ill. Websites like www.Craigslist.com allow property owners to advertise rental units with no paper work, on their own time, and at no cost. Not only does this make it easier to rent property, but it also saves landlords the significant commission paid to realtors to find tenant(s): commonly one to one-and-a-half month's rent.

These new landlords differ from traditional rental agents in important ways, however. Notably, many are part-time realtors/landlords who lack a real estate license. Unlike licensed realtors, these landlords are not trained or tested about fair housing law. These papers thus measure discrimination occurring across the broad spectrum of classes protected under U.S. fair housing law and test whether landlord behavior is influenced when provided with information about how fair housing law applies to rental housing.

Measuring Discrimination

The first paper offers the first evidence of differential treatment occurring across the broad spectrum of racial and protected classes covered under the law. Employing a fully randomized correspondent audit design and a sample of more than 9,500 online housing advertisements, the study offers insight about which protected groups experience the most/least favorable treatment when searching for housing. Housing audits are an established research method designed to test for differential treatment of similar individuals who vary only according to a particular trait or traits. Correspondence audits are a niche subset of audits in which email inquiries that are identical but for several key words or phrases are submitted to online advertisements for jobs, goods, or, in this case, rental housing.

This study employs a new signaling strategy in order to provide the first evidence of how landlord treatment of rental housing applicants varies across the spectrum of protected classes. The findings suggest landlords have preferences about tenants and make decisions based on signals communicated in inquiry emails from potential applicants. Response rates to applicants from protected racial and ethnic subgroups align with previous studies, particularly those of Black and Arab males. Households with children, and even the potential for children, are also treated unfavorably.

The findings also suggest differential treatment is generally consistent with theory of agent-based statistical discrimination. Landlords favor inquiries suggesting two potential earners over those mentioning only one individual or a home with children. Landlords also demonstrate an aversion to renters from groups associated with highly-publicized negative events, notably Arab males. These findings do not provide causal evidence about the landlord motivations for discriminating, only that the findings of discrimination are in agreement with the theory of agent-based statistical discrimination.

The broad findings demonstrate that many groups in this country continue to face obstacles when searching for housing, despite decades of legislation and enforcement efforts. Many laws and enforcement mechanisms, however, are designed for housing searches conducted in-person and via newspaper and other print media. Rental housing searches have since migrated to online advertising platforms, yet the continued evidence of discrimination suggests laws may be outdated and enforcement mechanisms limited. Moving forward, scholarship should strive understand how the online search for housing differs from the past and if laws and enforcement methods need to be modified for this new environment.

Mitigating Discrimination

The second paper conducts an experiment designed to influence landlord behavior to increase equal treatment. The purpose is to test whether property owners and rental agents will change their behavior in response to being presented with information about their obligations under fair housing law. The primary hypothesis is that the "treatment" email will increase response rates to housing inquiries made by members of protected groups.

The project thus conducts a randomized experiment employs a correspondence housing audit methodology to measure the impact. This research represents the first time in which the audit methodology is employed to measure the effect of a randomized experiment. The primary contribution of this paper, then, is to demonstrate a unique opportunity to test policy interventions aimed at reducing discrimination in a real housing market and at a very low cost. It is hoped that the method can be modified and expanded by fair housing agencies, advocates, and other institutions to test and implement policy interventions in hopes of reducing housing discrimination.

The experiment creates a treatment group of randomly assigned Craigslist rental housing posts to send an informational email about fair housing law. The email is sent from a pseudonymous website (www.fairhousingawareness.org) created for the purposes of this experiment. The email includes specifics about how fair housing law applies to residential landlord and provides information about fair housing resources. The email is sent via marketing software that provides information about which recipients opened the email.

The correspondence audit method is then used as a means for testing whether the treatment email influences landlord behavior. Specifically, it is used to measure whether landlord response rates to applications inquiring about treated properties – those receiving fair housing info – differ from the response rate to those of untreated properties – those receiving no fair housing information. Differential response rates between the two groups of unit inquiries is interpreted as evidence of a treatment effect.

The results of the experiment consistently suggest the group of landlords who received information about fair housing law responded at a higher rate than did those who did not receive the treatment email. Thus, the experiment offers evidence that utilizing the audit framework for testing policy interventions is a ripe opportunity for further research by scholars and practitioners.

College Scholarship and Urban Revitalization (Chapter 3)

The third paper in this dissertation examines the impact of a place-based program on urban property values and school enrolments. A recent trend in place-based policy targets college attainment by offering tuition scholarships for qualified students in under-resourced public schools. In an era of rising college costs, these programs represent a potentially large financial benefit to those living within the attendance zones of qualifying schools. Being directly linked to attendance zones, the benefit of such programs should be capitalized into local property values and school district enrolment.

This research thus examines the impact a large scholarship program, Say Yes to Education, has on property values in upstate New York. As "Say Yes" cities, students graduating from either the Buffalo or Syracuse city school districts who gain acceptance into one of more than one hundred

2-year and 4-year colleges and universities can receive up to 100% of tuition costs covered. Though the size of the benefit is tiered according to the number of years a student spends in the district, the only qualification is that they live in the district and attend public school.

Examining district enrollment from 2000 through 2014, the analysis finds that after years of steady declines in enrollments, both Syracuse and Buffalo saw enrollment increases that coincide with the adoption of the Say Yes to Education program. These increases occurred at different points in time in each city. Over the same post-treatment periods, enrollments continued to decline in the suburbs surrounding these cities and in similar upstate New York city school districts without the program, suggesting that these increases were city-specific and not due to broader developments affecting the region.

To isolate the impact of Say Yes on housing prices, a panel data set of individual home sales is used to estimate hedonic price models that control for neighborhood fixed effects and trends. The results provide some evidence that increases in housing prices accompanied the adoption of Say Yes in Syracuse, but not in Buffalo. These results are consistent with findings that enrollment growth in Buffalo may have been driven by students who would otherwise have attended private schools, while enrollment growth in Syracuse may have been driven by students who would otherwise have attended school in the surrounding suburbs. Combined with the enrolment effects, the analysis suggests that the ability of place-based scholarships to attract residents into a central city is likely to depend on both the specific provisions of the program and the context in which it is implemented.

Chapter 1

Up-Hill Battles:
Testing for Rental Housing Discrimination across the Spectrum of U.S. Protected Classes

Introduction

Federal fair housing law in the United States prohibits discrimination in housing based on race, color, gender, family status, and religion. Many state and local governments have extended these protections to cover sexual identity as well. Despite these laws, studies continue to identify statistically significant levels of discrimination in rental housing markets. Most large-scale efforts focus on race and ethnic discrimination, including gender, though several recent studies have examined discrimination based on sexual preferences, source of income, and disability. To date, however, no single study has looked at differential treatment across the broad spectrum of racial and protected classes covered under the law.

This study contributes to the discrimination literature in three primary ways. First, the study offers the first evidence of how the frequency of discrimination occurs across the spectrum of protected classes included under U.S. fair housing law. Employing a fully randomized correspondence audit design and a sample of more than 9,500 online housing advertisements, the study offers insight about which protected groups experience the most/least favorable treatment when searching for housing. Second, the audit employs signals indicating race and group membership that, to the best of the author's knowledge, have not been previously used in housing audits. Specifically, by pairing slight variations in the body text of the email with quotations signaling group membership in the signature line, each landlord receives a single inquiry containing at least two signals of protected group membership. Third, the randomized design with multiple protected groups allows for interactions among protected classes, offering support for theories about the mechanisms motivating differential treatment by landlords.

Testing for Discrimination

Studying rental housing discrimination generally employs the use of a field experiment referred to as a housing audit. These experiments are designed to test whether landlord treatment of a base group (typically the dominant ethnicity or class) differs from landlord treatment of one or more sub-dominant groups (minorities or vulnerable populations). These experiments can be conducted in-person, over the phone, or via the internet through email and online advertisements. The findings of these studies have proven convincing, having been upheld in court cases prosecuting landlords who discriminate against housing applicants.[1]

In-Person Audits

Traditional housing audits are in-person experiments in which trained actors inquire about and apply for a housing unit advertised in a local newspaper. Actors are paired and adopt similar profiles along common traits that might influence a landlord's housing decision, such as age, education, income, family status, appearance, current residence, and more. Great care is taken to minimize any differences other than the trait being tested, typically race, color or ethnic background. The actors carefully record how they are treated, including whether they are shown the unit originally inquired about, how many units are presented to them, and the location of the units shown.

The most comprehensive and influential in-person audits are the Housing Discrimination Studies (HDS) that have been conducted by the U.S. Department of Housing and Urban Development (HUD) roughly once every decade since 1977. These studies find consistent levels of rental discrimination against minorities in both the advertised unit being available and whether the minority auditor was invited to inspect additional units. Discrimination is consistently present among African American and Latino Americans, though levels decline over time. In addition to

[1] See Oh and Yinger (2015) for discussion of court cases.

the national studies, dozens of smaller studies have been conducted. Oh and Yinger (2015) provide a detailed literature review of these and other such studies.

Despite their influence, in-person audits have several limitations. The most prohibitive is their cost. Finding, hiring and training a sufficient number of actors to reveal statistically significant measures of discrimination is very expensive. As a result, only a handful of national in-person audits exist, each having been funded by the federal government.

A second issue refers to concerns about the reliability of the discrimination estimates. Noted by Heckman and Siegelman (1993), trained actors may be primed to identify discriminatory behavior, thus upwardly biasing the estimate of discrimination. While those conducting in-person audits attempt to mitigate this concern with good actors and training, this concern continues to be raised when evaluating measures of discrimination from in-person audits.

Email Correspondence Audits

More recently, the increased use of the Internet as a platform for buying and selling has opened up a new mechanism through which discrimination can be both practiced and measured. Websites, such as Craigslist.com, allow landlords and rental agents to anonymously post available housing units and conduct introductory screening via email correspondence, often using randomly generated, temporary email addresses. This complete anonymity provides a forum through which landlords and agents can choose to discriminate with little to no risk of being identified. Simultaneously, however, the anonymity of these online marketplaces create an extraordinary opportunity for fair housing advocates and researchers to test for discrimination, known as correspondence audits.

By posing as interested applicants, testers make email inquiries about advertised housing units with names and other signals that are commonly associated with particular ethnic or demographic characteristics. The outcome measure is landlord response rates, in which the base group, typically whites, is compared with the minority group being tested. The outcome is a narrower measure of discrimination than an in-person audit, however, as it can only test for differential response rates to email inquiries. Much of the nuance provided during in-person audits is lost, and steering, a practice in which property owners steer particular customer groups away/towards particular neighborhoods is unable to be assessed. The benefit, however, is that these correspondence studies can be conducted at a much lower cost while avoiding the risk of actor-influenced results as noted earlier.

Carpusor and Loges (2006) were the first to apply the correspondence email method to the study of housing discrimination, examining African and Arab-Americans in Los Angeles in the period surrounding the United States declaration of war in Iraq. Following their lead, researchers have conducted additional audits in Sweden (Ahmed & Hammarstedt, 2008), Norway and the UK (Carlsson & Eriksson, 2014), Spain (Bosch, Carnero, & Farre, 2010), Italy (Baldini & Federici, 2011), the United States (Hanson & Hawley, 2011) and elsewhere, each finding discrimination to exist between dominant majorities and ethnic minorities.

Audit Designs: Matched-Pair vs. Fully-Randomized

The two primary methods for conducting audits are matched-pair and fully-random designs. The key distinction between the designs is whether a single landlord receives one or multiple inquiries about a housing advertisement. In a matched-pair framework, virtually identical inquiries are made to the same landlord from two different actors (in-person) or email profiles (correspondence). Testers then compare differential response rates within each landlord included in the sample. Under a fully randomized design, each landlord receives only a single inquiry, leaving testers to compare differential response rates *across* all landlords in the sample. Selecting which framework to employ thus has important consequences for researchers.

Matched-pair audits are most common for both in-person and correspondence audits for several reasons. A key strength of a matched-pair design is that the within comparison allows a researcher to control for time-invariant unobserved characteristics of each landlord or rental agent. This offers at least two important benefits. First, it greatly reduces the number of landlords needed to achieve statistically significant results, as each landlord represents multiple observations. Given the limited resources available to fair housing advocates and scholars, this is a tremendous advantage.

An additional benefit of a matched pair design is the power of the narrative offered by the results. Perhaps the most striking example of this found in the literature is in Hanson, Hawley, and Taylor (2011) in which landlord responses, rather than response rates, are analyzed. The authors motivate their paper with the actual email responses of a single landlord to inquiries from a black and a white male. In response to Tremayne Williams, a black male, the landlord writes "work ref. rental ref. name address ss#". In contrast, the same landlord responds to Brett Murphy with "its avail give me your # and I will have my daughter show it to you." Clearly, the narrative power of such different responses from the same landlord is valuable.

Despite the practical, statistical, and narrative advantages of a matched-pair design, employing a paired design involves several important trade-offs. The most important trade-off in a matched-pair design is that sending multiple inquiries to a single landlord risks detection of the audit. Detection occurs when a one or more landlords become suspicious after multiple inquiries about the unit appear similar. Most paired audits take steps to minimize the risk of detection, such as presenting slightly different incomes, careers, or emails, though each difference is made in addition to the racial or demographic background of the applicant. Each additional difference between the pairs, other than the signal, has the potential to introduce bias and/or reduce the power of the narrative. Researchers must thus be careful so that these differences do not interact with the primary signal being communicated in the audit.

Employing a fully randomized design overcomes some key limitations in a matched-pair audit. By sending only a single email response to each housing post, detection risk in randomized audits is greatly reduced. Barring landlords sharing and comparing inquiries with other landlords via an online forum, it is difficult to think of a realistic scenario in which a randomized audit might be detected. Further, the negligible detection risk allows the inquiries to be identical, but for the signal(s) used to communicate race or group status. This ensures any measure of discrimination is identified only by the signals being communicated in the email.

Selecting to use a randomized design comes at the expense of the powerful narrative and smaller sample of landlords offered by the matched-pair framework. Rather than comparing treatment within landlords, the randomized design compares treatment across the distribution of landlords. This requires a larger sample and eliminates the ability to control for unobserved landlord characteristics through the use of a fixed effect.

Ultimately, the decision to employ a matched-pair or a fully randomized design depends on the nature of each project and the hypotheses being tested in each audit. All known in-person audits have been matched-pair designs, likely due to historical precedent and lower costs, though no theoretical reason prevents a randomized in-person audit. Correspondence audits have employed both. Matched-pair audits are more common, as might be expected, including Hanson and Hawley (2011), Ahmed and Hammerstedt (2010), Schwegman (2017), among others. Randomized designs exist as well, including the first-published correspondence audit by

Carpusor and Loges (2006). Following them, Ahmed, Andersson, and Hammerstedt (2010) and Ewens, Tomlin, and Wang (2014) have also employed a fully-randomized methodology.

Evidence of Discrimination

Evidence from housing audits consistently find discrimination along race and ethnic lines. The national HDS studies have found discrimination against blacks since 1977. While measures of discrimination have declined over time, the 2012 HDS found black renters were told of and shown fewer available unit and quoted slightly higher rents than whites. Though narrower in scope, a review of published correspondence audits since 2006 also find blacks to receive lower response rates to inquiry emails than whites (Carpusor & Loges, 2006); (Hanson & Hawley, 2011); (Ewens, Tomlin, & Wang, 2014).

Latinos have faced consistent unfavorable treatment when compared to whites in the HDS findings since first included in the 1989 study. Latinos are told of and shown fewer available units than whites, and they are also quoted higher rents. In the correspondence literature, however, a study by Hanson and Santas (2014) reveal nuance exists within discriminatory practices towards Latinos. The authors conduct a paired correspondence audit testing landlord response rates to inquires from whites, "assimilated" Latinos, and recent immigrants. Assimilated Latino's are signaled using assimilated first names (i.e. Alex, Jonathan, Anthony) and single Latino surnames (Lopez, Gonzales, Garcia). Recently immigrated Latinos are signaled with more traditional names (i.e. Ruben, Oscar, Andre) and double surnames (i.e. Ramirez Chacon, Medina Rios), as is the norm in many Spanish speaking countries. The analysis finds discrimination towards more recently immigrated Latinos, but not the "assimilated" Latinos.

Carpusor and Loges (2006) provide the only evidence of discrimination towards Arabs in a U.S.-based correspondence study, finding Arabic males to experience lower response rates compared with whites, though higher rates than blacks. Outside of the U.S., however, multiple studies find less favorable treatment of Arabs in comparison with native populations in Canada (Hogan & Berry, 2011) Scandinavia (Ahmed & Hammarstedt, 2008) (Ahmed, Andersson, & Hammarstedt, 2010) (Andersson, Jakobsson, & Kotsadam, 2012) (Carlsson & Eriksson, 2014), the United Kingdom (Carlsson and Ericksson, 2013), Italy (Baldini & Federici, 2011), and Spain (Bosch, Carnero, & Farre, 2010).

Despite a considerable literature measuring race- and ethnicity-based discrimination, few correspondence audits have examined differential treatment of other vulnerable populations. Women have been included in several correspondence studies – Andersson, Jakobsson, and Kotsadam (2012), Bosch, Carnero, and Farre (2010), Ewens, Tomlin and Wong (2014) – but with the exception of Andersson et al, the primary focus of these has been to compare differential treatment of race, not gender. For example, Ewens et al (2014) include gender in a randomized correspondence audit, though the emphasis of the project focuses on testing causes of race-based discrimination and minimal analysis by gender is discussed. Andersson et al (2012) test for differences between race and gender in Norway, concluding that females are given preferential treatment and that race is the primary mechanism through which discrimination is practiced. Studies including gender compare black and white in the U.S. (Ewens, Tomlin, & Wang, 2014), and native Swedes (Ahmed & Hammarstedt, 2008), Norwegians (Andersson, Jakobsson, & Kotsadam, 2012) or Spanish (Bosch, Carnero, & Farre, 2010) with Muslims.

Sexuality, family status, and religion are also under-represented in the U.S. literature. Two unpublished correspondence studies have examined sexuality-based discrimination. A 2013 study commissioned by the Urban Institute and conducted by Freidman (2013) employs a matched-pair correspondence audit to examine differential treatment of homosexual and heterosexual couples across 50 randomly selected MSAs. The authors employ a matched-pair design and find landlords across the country to prefer heterosexual couples to either gay or lesbian couples. Schwegman (2017) conducts a similar study and finds similar preferences among U.S. landlords. Outside of the U.S., however, Ahmed, Andersson and Hammarstedt find that Scandinavian landlords are generally neutral about same-sex couples (2008).

To date, no known academic study has focused on either family status or religion. Evidence from small-scale local audits performed by fair housing advocates suggests discrimination exists towards families with children. A 2014 study performed by the Denver Metro Fair Housing Center found discrimination towards families with children at rates similar to blacks and Latinos.[2] A similar study conducted in 2016 by the Seattle Office for Civil Rights found families with children to be treated less favorably than childless renters.[3] Religion-based discrimination is even less explored. While studies have considered discrimination against Arab sounding names, it is unknown whether this treatment is motivated by ethnic or religious stereotypes about Islam.

A final note about existing studies is worth discussing. Many correspondence audits are paired audits. In paired-audits, identification of differential treatment is measured within each individual landlord. Thus, most audits rightfully place significant focus on the ability to compare like groups (i.e. gay couples with straight couples, white males with black males, etc.). Put differently, a study examining discrimination towards blacks that compares black women to white men confounds identification, as both racial and gender differences exist between the groups. Thus, matched designs isolate differences by comparing black males with white males, gay couples with straight couples, etc.

[2] http://www.bizjournals.com/denver/news/2014/02/05/study-finds-rampant-discrimination-in.html
[3] http://westseattleblog.com/2016/05/evidence-of-housing-discrimination-alleged-in-test-results-from-23-properties-citywide-including-3-in-west-seattle/

What previous studies have not done, however, is assess how much overall discrimination exists as the law defines it. Fair housing law makes unequal treatment on the grounds of race, ethnicity, gender, family status, religion or sexuality illegal. The only group falling outside of that definition is white males. Thus, to the extent that any group covered under the law receives differential treatment from white males, it can be considered evidence of discrimination. It is thus the hope of this project to offer insight about how much discrimination occurs by race and protected class relative to the dominant group.

Experimental Design

The objective of this audit study is to measure discrimination as it occurs across the spectrum of protected classes covered by U.S. Fair Housing Law. Federal, state and local housing laws require landlords treat all applicants equally, regardless of race, gender, ethnicity, religion, sexuality[4], and family status. To estimate discrimination across a broad range of protected classes, this research employs a randomized correspondence audit design using housing units posted on Craigslist in the 20 most populated U.S. cities. Were no discrimination to exist, each group would have an equal likelihood of receiving a response to housing inquiries.

This experiment employs a methodology similar to the randomized audits of Carpusor and Loges (2006) and Ahmed and Hammarstedt (2008). The decision to employ a randomized instead of a matched-pair design for this study was driven by two primary factors. First, because little work has been done to identify discrimination by group membership, the decision was made that emails should be identical in syntax, with the exception being the particular signal(s) communicated in each inquiry. This ensures the detection of differential treatment is being identified by the signal alone, rather than subtle differences in the construction of the email.

[4] State and local level only (19 of 20 cities in my sample)

Because using identical emails increases detection risk, a randomized design was therefore employed.

Central to an effective correspondence audit design is communicating group status through signals included in each email inquiry. The randomized design allows a nearly identical syntax be used for all emails, the base form of which is included below. Each email asks about the availability of the unit, provides information about who is interested in the unit, mentions potentially attractive personal habits, and volunteers a recent credit report. A second version of the email, written in more casual language, includes several grammatical errors to provide a level of social-class variation in the experiment.

Dear sir/madam,

[Body Signal 1] I am interested in the rental unit posted on Craigslist, is it still available? I have good references, don't drink or smoke *[Body Signal 2]*, and am happy to send a copy of a recent credit report.

Regards,

[*First name*] [*Last name*]

[*Signal Quotation*]

Following previous studies, race, ethnicity and gender were signaled using names commonly associated with a particular ethnicity or gender. One difference from previous correspondence studies in the U.S. is that multiple races and ethnicities are included for which limited name data exists in the U.S., particularly for those of Arab descent. Thus, first names were selected for all races from websites listing common names for different groups. Surnames used for whites and blacks came from Bertrand and Mullinathian (2004), while Latinos and Middle-Eastern names were constructed in similar fashion to the first names. (Table 1) To mitigate potential concerns about what is inferred from names constructed in this manner, a second and more direct signal is also used in the signature line of each email to communicate group membership (described below).

Beyond race and gender, it is not possible to signal protected group membership using a name. Signals must therefore be communicated in a manner that might reasonably be included in an introductory email inquiry for housing. Membership was thus signaled in the body of each email, by slightly modifying the base email to suggest group membership. (Table 2) Signaling sexuality, for example, "My partner and I" is inserted to [Body Signal 1] to introduce who is inquiring about the unit.[5] Signaling religion is done in [Body Signal 2], by adding the phrase "due to my Islamic(Christian) beliefs" as the motivation for not drinking and smoking.

To ensure at least two signals for group membership are included for each group, an additional signal is provided in the signature line of each email inquiry. (Table 3) Continuing with the religion example, Christian and Islamic variations of the "Golden Rule" were included.[6] Quotations signaling race, gender, sexuality and family status are also used. (Table 4) This additional signal is used to increase the probability that the landlord identifies the email inquiry as being sent from someone in a particular group.

Though the use of the body and quotation signals allows additional groups to be included in the audit, their use may limit the generalizability of the study. It is unknown how frequently individuals utilize quotations in the signature line of emails or reference characteristics about

[5] Signaling sexuality as couples creates a group that is different than the base group in an important and meaningful way. By signaling couple status, it suggests the potential for two earners in the house, rather than the base inquiry with no signal of a partner. As the study does not include a comparable straight couple, this leaves open the question of whether differential treatment is driven by sexuality or multiple incomes. Tables 9 and 13 address this, however, and reveal differential treatment of gay couples, especially among non-whites, is unlikely to be driven by income.
[6] Iterations of each email are included in the appendix

themselves in inquiry emails. Landlords may thus view such emails as unusual, potentially affecting response rates.[7] Thus, the results may represent differential treatment of a subset of potential housing applicants, though the estimates remain unbiased as all groups include body and email signals. This limitation was accepted in order to provide the first evidence of discrimination across the spectrum of protected classes.

Sample Characteristics

Craigslist is an ideal venue to conduct a correspondence audit for several reasons. It offers a local webpage in nearly every city in the U.S., receives a high volume of traffic, is free to use for both landlords and renters, and employs an identical format across all U.S. apartment rental sites. More than 50 billion page views occur globally each year, and 60 million people use Craigslist each month in the U.S. alone.[8] Of particular attractiveness to audit studies is that the format for housing posts is identical across all U.S. sites, allowing unit-specific data to be gathered in a consistent manner across all cities.

Units were randomly selected on each day of the week and inquiry emails were submitted within 24 hours of being posted on Craigslist. Posts were collected from each city in batches of 100 using a semi-automated process.[9] Extensive effort was made to avoid sending inquiries to landlords posting multiple units. The de-duplicating process began by eliminating units sharing the same Craigslist ID, address, phone number, realty company, or housing development. Next, individual unit advertisements were scanned for similar characteristics that suggested a shared landlord. Landlords commonly use the same format for multiple posts, similar marketing

[7] It is difficult to compare the response rate in this study with those in the literature. The response rate is slightly lower (3-8%) to Hanson and Santas () and Hanson and Taylor (), though they use a matched pair design so each landlord has multiple emails contributing to the response rate. Differences in market conditions may also play a role, as their audits were conducted 3 and 7 years prior.
[8] Source: https://www.craigslist.org/about/factsheet
[9] The collection process employed a program written by Phil Murchie. The program provides a semi-automated process for gathering unit information and contacting landlords. Without this tool, the data gathering process would have been significantly more cumbersome.

phrases, company logos, and other practices. Any unit raising even the slightest doubt was eliminated. Finally, posts were compared with a master-file of all previously de-duplicated posts and a second round of de-duplication was conducted. In total, more than 15,000 units were deleted using this process.

Each post received a single email indicating interest in the unit from one of 24 names varying by race and gender and of the form first.last####@gmail.com. Each inquiry was then randomly assigned group membership for one of 8 additional profiles. Names were used to identify race, ethnic group and gender, while group status was signaled in the body and signature line of each email. The first emails were sent July 25th, 2016 and responses were recorded until September 19th, 2016, which was two weeks after the final inquiries were sent.

In total, this experiment sampled 9,672 units. The overall response rate of landlords to inquiries was 36.1%. Table 6 specifies the number of audits and response rate for each city. The variation in the number of audits across cities is driven by the frequency of duplicate landlords identified at the time data was collected from Craigslist. Professional realtors who posted multiple properties at a time dominated the Chicago, Dallas, and Miami markets. Conversely, Boston, Seattle, Minneapolis and San Diego had a much higher percentage of individual landlords advertising housing on Craigslist. The highest response rate was 43.3% in Denver, while Phoenix had the lowest rate at 26.1%.

Unit information was available for the vast majority of units sampled, and included the size, square footage, type of building and monthly rent (Table 5). The monthly rent, unit type, number of bedrooms and number of bathrooms was available for more than 95% of units. Square footage was available less frequently, for 67% of units. The average unit had 2 bedrooms, 1.5 baths, slightly more than 1,200 square feet and was listed with a monthly rent of $1,835. Roughly 62% of the sample is listed as an apartment and 20% as single-family homes.

Rents were highest in San Francisco and New York City and least expensive in Detroit and St. Louis. More than 70% of postings in New York City (95%), Boston (81%), Dallas (74%) and Washington D.C (71%) were for apartments. Conversely, Detroit (44%), Riverside (41%), and St. Louis (32%) each had 30% or more posts for houses. Washington D.C. (1,017 sq. ft.) and New York City (1,067 sq. ft.) had the smallest average unit size, Houston and Riverside with the largest.

A final note about the sample. Rental agents and landlords using Craigslist have the option to either provide a personal email or be assigned a temporary and anonymous Craigslist-generated email address. The overwhelming majority of landlords, roughly 90% in the sample, choose anonymity. Response rates for this group of landlords were as much as 10% lower than among those who provided identifiable email addresses.

Empirical Results

Table 7 presents the raw number of audits and the landlord response rate by race, across protected classes. Among racial and ethnic groups, Latinos (39.5%) and Whites (38.1%) experienced the highest response rates, while Black and Arab emails received the lowest rate. Emails from females (38.4%) had higher response rates than those from males (35.9 %). Male gay couples received the highest overall response rate for protected groups at 44.5%, while single parents experienced the lowest response rate at 35.1%.

Among racial subgroups, white gay couples received the highest response rate of all sub-group categories at 55.6%, while Arab males and Black single parents were the only two groups to receive responses to fewer than 30% of inquiries. The response rate for emails from Latinos is above the group average for all categories except gay males (39.5% vs 44.5%), while blacks receive lower than average responses in all categories. Females experience higher response rates than males for all groups except Latinos, with Arab females having a 12% higher response rate than Arab males.

Table 8 presents the results from a linear probability model. White males without a protected group signal are the omitted group, as they represent the dominant group and are not generally considered a protected class. Coefficients thus represent differential treatment of a particular race or protected group relative to white males. Column 1 presents the base model, without unit controls or city dummies.

The overall results are consistent with the raw averages discussed above. Blacks (-6.2%), Arabs (-4.7%), single parents (-4.2%), and Muslims (-3.8 %) receive statistically significant lower landlord response rates than white males. Inquiries from gay males (6.2%) and females (1.9%) received statistically significant higher response rates. Response to grammatically correct and formal emails were 4.2% higher, and landlords providing an identifiable email address were 11.1% more likely to respond. The response rate to Latino, Lesbian, and Christian inquiries are not statistically different from white males.

Columns 2 and 3 add unit controls and city dummies. Adding city controls has little effect, suggesting time invariant differences across cities has little impact on landlord response rates. Column 3 adds unit controls, thus reducing the sample by one-third. It is worth noting that doing so also changes the composition of the sample, by restricting it to the group of landlords who provide more unit information in their advertisement.

The intercept for the restricted group is 6-8 percentage points higher than that in Columns 1 and 2, suggesting these landlords are generally more responsive than the overall sample. The signs in Column 3 are consistent with Columns 1 and 2, however, though the magnitude and significance levels differ slightly. The coefficient on Latino (3.5%) is more than double that in column 1 and becomes significant at the 5% level. Lower response rates for Muslims also increase in both magnitude and precision.

The randomized design and inclusion of protected classes in addition to race allows for analyzing the relationship between race and group status in a way that has not previously been tested in the literature. Specifically, it offers insight into how protected group status varies within each racial group. For example, do all single parents face differential treatment or is it only those of a particular race?

Table 9 examines how providing information about the group status of a respondent affects the likelihood of receiving a response within each racial group.[10] Specifically, the coefficients represent differential treatment of particular protected groups compared with males of the same race or ethnicity. The intent is to examine how treatment of each protected class varies within each racial group. Column 1 presents the results from the base model (Table 8) for reference.

Table 9 shows landlords are likely to respond similarly to White applicants, though a clear preference for gay couples is evident. Among Latinos, landlords indicate clear preferences for men, with both females and single parent applicants receiving statistically significant lower response rates. In contrast, landlords have strong preferences for females, gay and lesbian couples over males.

[10] Column 1 presents the results from the base model in Table 8 for reference.

In contrast with other ethnicities, treatment of protected groups does not vary within black housing applicants. All subgroups of blacks are treated in a similar fashion to black males. Said differently, simply being black is signal enough to receive a lower response rate from landlords. There is no additional benefit or penalty received by providing additional information about ones group membership. Even a grammatically correct email fails to make a significantly different effect on landlord response rates to black applicants.

Discussion

This paper presents findings from a correspondence housing audit that incorporates both race and protected class in a single, randomized project. The results provide evidence that many groups protected under U.S. fair housing law continue to face obstacles when searching for housing in spite of decades of fair housing law. In order to design more effective policy, then, a better understanding of the drivers of discrimination is required.

Two primary hypotheses about the causes of discrimination exist in the literature. The first, prejudicial discrimination, refers to differential treatment driven by a landlord's dislike or distaste for the particular group being discriminated against (Yinger 1986). These landlords elevate their personal prejudicial attitude over that of a profit motivation, but short of a survey revealing landlord preferences, this discrimination is very difficult to identify.

Profit motivation is the foundation for the second hypothesis, also known as statistical discrimination (Yinger 1986). Statistical discrimination refers to discrimination in which being a minority or a member of a protected-class serves as a proxy for profitability risk due to limited information about the applicant. For example, a landlord may view applicant membership to a group with high levels of low-income or unemployed people as a threat to the applicant's ability pay rent in a timely manner. Similarly, applicants with children may represent higher risk of damage to the unit, again threatening profitability. In this agent-based discrimination, then, the landlord is acting to protect profit based on perceptions correlated with the known characteristics of an applicant. Limited information, however, does not justify discrimination as defined by the law.

A second form of statistical discrimination is that in which a landlord or agent engages in discrimination to protect a client base. Landlords and agents may have existing clients with preferences for neighbors that fit a certain profile. Examples of this include not renting to a minority in a homogenous dominant-group neighborhood or renting to families with children when single young adults occupy other units in a particular building. In this customer-based discrimination, the landlord is acting so as to not offend an existing client base and threaten future profits. Ultimately, whether the cause of differential treatment is prejudicial or statistical, it is illegal.

A number of studies have sought to examine the causes of race-based discrimination found in audit studies, though they are largely limited to indirect attempts that test hypotheses about the likelihood of a cause under particular circumstances. Correspondence studies by Bosch, Carnero, and Farre (2010), Hanson and Hawley (2011), Ondrich, Ross, & Yinger (2003) and Ewens, Tomlin, and Wang (2014) test for statistical discrimination by offering additional applicant information. Such information includes employment type (Bosch et al, 2010), personal behaviors (Ewens et al, 2014), and well-written emails (Hanson and Hawley 2011), finding differential treatment is lower as additional positive information is provided. Yinger, Ondrich and Ross (2003) and Hanson and Hawley (2011) find evidence of steering, by exploiting information about individual units, applicants, and neighborhood characteristics. Additional detail about the causes of discrimination is discussed at length in Oh and Yinger (2015).

A common challenge faced by all audit studies is to move beyond measuring discrimination and test causes of what motivates differential treatment of particular groups. This study faces these same limitations, though the ability to examine race and group membership in a randomized design allows for some examination of statistical discrimination that has not been possible in previous designs. Specifically, by interacting race and ethnicity with other protected classes, the design allows for some tests about the extent that differential treatment by race is motivated by landlord stereotypes about particular groups.

Evidence of Statistical Discrimination?

Examining differences in the earning potential of applicants allows for a relatively straightforward test of statistical discrimination. For example, the design of the signal for gay and lesbian couples suggests the potential for multiple earners, as the email begins "My partner and I". In contrast, male, female, and religious applicants make no mention of a second potential income, simply indicating the applicant's individual interest in the unit. The single parent signal also offers no mention of another potential earner, but provides information about additional demands for income, namely children. If landlords are practicing agent-based statistical discrimination, then, gay couples should receive more favorable treatment than single applicants, while both should receive more favorable treatment than single parents.

The results in Table 8 are thus generally consistent with agent-based statistical discrimination described above. Notably deviating from this theory, however, is that lesbian couples do not receive as favorable treatment as gay couples. As the signal for both gay men and lesbians suggests the possibility of two earners, can other differences explain these results? Put differently, to be consistent with statistical discrimination, differential treatment of the groups must be explained by other traits that are correlated with profitability.

One possible explanation of the differential treatment is the propensity for having children. Nearly 25% of gay and lesbian couples have children,[11] though lesbian couples are much more likely to have children than men. Further, a 2015 *Demography* article by Alden, Edlund, Hammarstedt, and Mueller-Smith (2015) finds the primary drivers of marriage among gay and lesbian couples differ dramatically. For gay couples, pooling resources is the dominant motivation while family formation brings lesbian couples together in marriage. Pop-culture stereotypes of gay couples include tidy apartments and dual incomes with no children,[12] traits that also suggest more resources and fewer obligations when rent comes due and greater attentiveness in caring for the unit. If landlords are aware of or make similar distinctions about gay and lesbian couples, the differential treatment between the groups of renters is consistent with statistical discrimination.

Table 8 indicates an overall positive effect of being gay, while Table 9 suggests this varies by race. Table 10 therefore takes the analysis further and presents the interaction of race with sexuality. If the signal for being gay is associated with the potential for multiple incomes, the interactions should have a positive association across all races. If the effect varies by race, however, being gay may signal more than income.

The results in Table 10 and Table 11 suggest that treatment of gay males varies by race, though it is generally positive. Gay white males receive a 15.6% higher response rate than white men with no signal, the highest response rate of any group in the audit. Black and Arab gay men also

[11] https://www.princeton.edu/futureofchildren/publications/docs/MarriageandFamily.pdf
[12] http://www.huffingtonpost.com/murray-lipp/gay-men-myths-stereotypes_b_3463172.html

receive a higher response than single males, offsetting the negative treatment of being a Black or Arab male relative to a White male identified in Table 8. The response rate for Latino gay couples is similar to single Latino males.

The findings in Table 10 and Table 11 are largely consistent with agent-based statistical discrimination, though the variation across race suggests additional factors associated with race and/or sexuality exist. For example, the discussion about differences between Arab males and females discussed below may also apply to Arab gay couples. Despite this evidence, these and other reasons for this differential treatment likely reflect factors that are unable to be addressed in this study.

Another opportunity to test for statistical discrimination is among females. Theories about statistical discrimination and landlord treatment of female applicants in a correspondence audit are ambiguous. On one hand, females may be associated with higher potential for children than males, thus representing an obligation on their income and greater risk to profitability. Conversely, however, females may be awarded stereotypes about being more clean, hosting fewer parties, among myriad other factors that may reduce threats to landlord profitability. Thus to better understand the positive effect in Table 8, Table 12 presents the interaction of race and gender.

Table 12 reveals that women of all races do not universally share the positive effect of being female in there housing searches. Instead, being female is not significantly different from males for White, Black and Latino applicants. For Arab women, however, there is a large and significant positive effect relative to Arab males. The effect offsets the entire negative effect of being a single Arab male. When considering the similar preferences for Arab gay couples discussed above, a reasonable explanation for the differential treatment of these groups within the Arab community is consistent with statistical discrimination.

This audit took place from July-September, 2016. In the six weeks preceding the start of the study, male perpetrators of Arab descent carried out high-profile acts of violence on the Pulse nightclub in Orlando, Florida and the Bastille Day celebration in Nice, France. The attacks received worldwide attention and became frequently discussed events during the U.S. presidential election. The perpetrator of the Nice, France attack, Mohammed Lahouaiej-Bouhlel, shared the first name of one of the Arab males included in this study. Given the relative proximity of these attacks and the timing of the audit, it is reasonable to think landlords looked for signals in inquiries from Arab applicants and gave preferential treatment to those subgroups they believed were less likely to be associated with acts of violence committed by men of Arab descent.

If Arab females are driving the positive effects of being female in Table 8, the findings in Table 12 are consistent with the theory of statistical discrimination. As discussed above, the theory is ambiguous as to whether females make better tenants than males, and the findings support this. The only group for which being female has an effect is Arab women, of which the timing of the study and perceptions of Arab males may well be driving the differential treatment.

Conclusions and Further Opportunities for Research

This study employs a new signaling strategy in order to provide the first evidence of how landlord treatment of rental housing applicants varies across the spectrum of protected classes. The findings suggest landlords have preferences about tenants and make decisions based on signals communicated in inquiry emails from potential applicants. Response rates to applicants from protected racial and ethnic subgroups align with previous studies, particularly those of Black and Arab males. Households with children, and even the potential for children, are also treated unfavorably.

The findings also suggest differential treatment is generally consistent with theory of agent-based statistical discrimination. Landlords favor inquiries suggesting two potential earners over those mentioning only one individual or a home with children. Landlords also demonstrate an aversion to renters from groups associated with highly-publicized negative events, notably Arab males. These findings do not provide causal evidence about the landlord motivations for discriminating, only that the findings of discrimination are in agreement with the theory of agent-based statistical discrimination.

The broad findings demonstrate that many groups in this country continue to face obstacles when searching for housing, despite decades of legislation and enforcement efforts. Many laws and enforcement mechanisms, however, are designed for housing searches conducted in-person and via newspaper and other print media. Rental housing searches have since migrated to online advertising platforms, yet the continued evidence of discrimination suggests laws may be outdated and enforcement mechanisms limited. Moving forward, scholarship should strive understand how the online search for housing differs from the past and if laws and enforcement methods need to be modified for this new environment.

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Table 1: Male names used in audit

	Number of Emails	Percentage of emails	Race
Abdul Abbas	384	3.97	Arab
Mohammed Toosi	367	3.98	Arab
Saied Ardakani	485	5.01	Arab
Darnell Washington	407	4.21	Black
Jermaine Parker	525	5.43	Black
Leroy Robinson	436	4.51	Black
Carlos Ruiz	354	3.66	Latino
Jose Gonzales	291	3.01	Latino
Juan Rodriguez	259	2.68	Latino
Bob McCarthy	393	4.06	White
Johnny O'Brien	480	4.96	White
Len Baker	366	3.78	White

Table 1b: Female names used in audit

	Number of Emails	Percentage of emails	Race
Farah Abbas	419	4.33	Arab
Fatima Toosi	429	4.44	Arab
Sefa Ardakani	394	4.07	Arab
Aaliyah Washington	516	5.33	Black
Destiny Johnson	464	4.80	Black
Latoya Parker	497	5.14	Black
Alejandra Rodriguez	262	2.71	Latina
Jasmin Ruiz	319	3.30	Latina
Luz Gonzales	352	3.64	Latina
Amy Baker	481	4.97	White
Katherine McCarthy	379	3.92	White
Sarah O'Brien	396	4.09	White

Table 2: Sample Audit Emails and Signals

<u>Base High-SES Email</u>

Dear sir/madam,

[Body Signal 1] I am interested in the rental unit posted on Craigslist, is it still available? I have good references, don't drink or smoke *[Body Signal 2]*, and am happy to send a copy of a recent credit report.

Regards,

[*First name*] [*Last name*]

[*Signal Quotation*]

<u>Body Signal 1</u>

Single Parent [*My kids and I*]

Gay / Lesbian [*My partner and I*]

<u>Body Signal 2</u>

Christian [*due to my Christian beliefs*]

Muslim [*due to my Islamic beliefs*]

<u>Base Low-SES Status</u>

Hi! Saw your post online. [*Body Signal 1*] I'm looking for a place, is it still avialable? I stay away from partyin [*Body Signal 2*] and can give you references is you want. Need anything else? peace,

[*First name*] [*Last name*]

[*Signal Quotation*]

<u>Body Signal 1</u>

Single Parent [*My kids and I*]

Gay / Lesbian [*My partner and I*]

<u>Body Signal 2</u>

Christian [*because I'm Christian*]

Muslim [*because I'm Muslim*]

Table 3: Signal Types by Protected Class

	Quotation
Male (non-Gay)	
White	*"Do unto others as you would have them do unto you"*
Black	*"The time is always right to do what is right." - Martin Luther King, Jr.*
Latino	*"I'm proud to be both Latino and American." mi abuelo*
Arab	*"I'm proud to be an Arab-American." My grandfather*
Female (non-Gay)	
White	*"A girl should be two things: classy and fabulous." – Coco Chanel*
Black	*"I'm black, I don't feel burdened by it and I don't think it's a huge responsibility. It's part of who I am. It does not define me." – Oprah Winfrey*
Latino	*"I'm bicultural, and everyone sees me as a Latina, but in my head I see myself as both Latina and American." – Genesis Rodriguez*
Arab	*"If a beautiful woman is a jewel, then a good woman is a treasure." – Mahmood Abbas Al-A'aqad*
Protected Class	
Single Parent	*"I didn't set out to be a single-parent. I set out to be the best parent I can be… and that hasn't changed." unknown single parent*
Gay Male	*"I'm homosexual. How and why are idle questions. It's a little like wanting to know why my eyes are green." - Jean Genet*
Lesbian	*"I'm living by example by continuing on with my career and having a full, rich life, and I am incidentally gay."--Portia DeRossi*
Christian	*"Do to others as you would have them do to you" - Luke 6:31*
Muslim	*"Wish for your brother, what you wish for yourself." - the Prophet Muhammad (eng. translation)*

Table 4: Number of Signals by Protected Class

	Name	Body	Quotation
Race	X		X
Gender	X		X
Single Parent		X	X
Religion		X	X
Gay		X	X

Table 5: Sample Unit Characteristics

Variable	Obs	Mean	Std. Dev.	Min	Max
Bedrooms	8,650	2.123006	1.046227	1	8
Baths	8,250	1.542909	0.696162	1	6
apartment	9,673	0.64313	0.479101	0	1
house	9,673	0.19632	0.397234	0	1
duplex	9,673	0.021296	0.144378	0	1
condo	9,673	0.075158	0.263659	0	1
sqft	6,231	1169.925	671.102	1	9000
Price	9,547	1805.697	1103.325	100	10000

Table 6: Number of Audits and Response Rate Across Cities

	Number of Audits	Response Rate (%)
Full Sample	9,672	36.1%
Atlanta	456	35.1%
Boston	703	36.6%
Chicago	323	41.8%
Dallas	284	32.0%
Denver	395	43.3%
Detroit	527	33.8%
Houston	394	32.5%
Los Angeles	566	30.4%
Miami	292	36.0%
Minneapolis	577	30.7%
New York	474	32.5%
Philadelphia	560	37.7%
Phoenix	395	26.1%
Riverside	387	34.1%
San Diego	663	37.6%
San Francisco	552	42.0%
Seattle	624	41.7%
St. Louis	506	38.7%
Tampa	368	32.1%
Washington D.C.	626	42.0%

Table 7: Number of Audits and Landlord Response Rates across Race and Protected Class

	Full Sample		White		Black		Latino		Arab	
	Total	(%)	Total	(%)	Total	(%)	Total	(%)	Total	(%)
Male (non-Gay)	1729	35.9%	427	37.9%	450	33.8%	429	44.8%	423	27.2%
Female (non-Gay)	1733	38.4%	435	41.1%	420	32.3%	443	40.4%	435	39.8%
Single Parent	2838	33.1%	646	34.7%	882	29.5%	651	36.3%	659	32.0%
Gay Male	651	44.5%	153	55.6%	187	41.2%	152	42.1%	159	40.3%
Lesbian	686	39.5%	176	38.1%	188	36.2%	162	43.8%	160	40.6%
Christian	932	36.2%	311	41.8%	294	35.0%			327	31.8%
Muslim	1104	34.0%	347	36.3%	424	30.2%			333	36.3%
Total	9673	36.1%	2194	38.1%	3087	31.6%	1607	39.5%	2221	33.1%

Table 8: Regression Results

	(1) Base Model	(2) Base + City	(3) Full Model
Black	-0.0620***	-0.0608***	-0.0474***
	(0.0149)	(0.0143)	(0.0152)
Latino	0.0130	0.0154	0.0334**
	(0.0129)	(0.0129)	(0.0133)
Arab	-0.0469***	-0.0455***	-0.0352*
	(0.0114)	(0.0113)	(0.0176)
Female (non-Gay)	0.0190**	0.0196**	0.0296***
	(0.00881)	(0.00875)	(0.00919)
Single Parent	-0.0423**	-0.0414*	-0.0496**
	(0.0201)	(0.0199)	(0.0212)
Gay	0.0619***	0.0647***	0.0824***
	(0.0190)	(0.0187)	(0.0225)
Lesbian	0.0112	0.0105	0.0462
	(0.0221)	(0.0215)	(0.0298)
Christian	-0.0200	-0.0173	-0.00793
	(0.0199)	(0.0194)	(0.0315)
Muslim	-0.0379*	-0.0368*	-0.0503*
	(0.0189)	(0.0191)	(0.0244)
High Class Email	0.0420***	0.0422***	0.0515***
	(0.0118)	(0.0119)	(0.0155)
Non-Craigslist Email	0.111***	0.109***	0.108***
	(0.0259)	(0.0258)	(0.0240)
Constant	0.353***	0.331***	0.411***
	(0.0149)	(0.0134)	(0.0369)
City Dummies		X	X
Unit Controls			X
Observations	9672	9672	5777

Standard errors in parentheses
* p<.10, ** p<.05, *** p<.01

Table 9: Regression Results (by race and ethnicity)

	(1) Base	(2) White	(4) Black	(5) Latino	(6) Arab
Female	0.0190**	0.0124	-0.0169	-0.0537**	0.110***
	(0.00881)	(0.0188)	(0.0127)	(0.0231)	(0.0132)
Single Parent	-0.0423**	-0.0466	-0.0273	-0.0719***	-0.0122
	(0.0201)	(0.0270)	(0.0226)	(0.0249)	(0.0352)
Gay	0.0619***	0.146***	0.0743	-0.0634	0.0922**
	(0.0190)	(0.0359)	(0.0453)	(0.0434)	(0.0364)
Lesbian	0.0112	-0.0318	0.0189	-0.0495	0.0937*
	(0.0221)	(0.0507)	(0.0386)	(0.0479)	(0.0494)
Christian	-0.0200	0.00152	0.00932		-0.0336
	(0.0199)	(0.0265)	(0.0361)		(0.0283)
Muslim	-0.0379*	-0.0606	-0.0335		0.00681
	(0.0189)	(0.0363)	(0.0240)		(0.0390)
Formal Email	0.0420***	0.0526**	0.0192	0.0573**	0.0445*
	(0.0118)	(0.0218)	(0.0248)	(0.0251)	(0.0247)
Non-CL Email	0.111***	0.0739**	0.181***	0.0680**	0.102**
	(0.0259)	(0.0334)	(0.0301)	(0.0315)	(0.0394)
City Dummies	X	X	X	X	X
Observations	9672	2495	2845	1837	2495

Standard errors in parentheses
* p<.10, ** p<.05, *** p<.01

Table 10: Interaction Results of Race and Male Sexuality

	Base + City	Base + City Race X Gay	Full Race X Gay
Constant (White male)	0.3315***	0.3231***	0.3994***
	(0.0187)	(0.0132)	(0.0375)
Black	-0.0608***	-0.0556***	-0.0396***
	(0.0143)	(0.0132)	(0.0126)
Latino	0.0154	0.0272*	0.0469***
	(0.0129)	(0.0134)	(0.0142)
Arab	-0.0455***	-0.0381***	-0.0240
	(0.0113)	(0.0119)	(0.0178)
Gay	0.0647***	0.156***	0.204***
	(0.0187)	(0.0318)	(0.0455)
Black X Gay		-0.0840	-0.123*
		(0.0500)	(0.0633)
Latino X Gay		-0.162***	-0.182**
		(0.0490)	(0.0674)
Arab X Gay		-0.121**	-0.175**
		(0.0529)	(0.0660)
City Dummies	X	X	X
Unit Controls			X
Observations	9672	9672	5777

Standard errors in parentheses
* p<.10, ** p<.05, *** p<.01

Table 11: Effect of Being a Gay Couple on Landlord Response Rate

	Single Male	Gay Couple	Difference
White	32.3%	47.9%	15.6%
Black	26.7%	33.9%	7.2%
Latino	35.0%	34.4%	-0.6%
Arab	28.5%	32.0%	3.5%
Observations	9672	9672	9672

Table 12: Interaction Results of Race and Gender

	Base + City	Base + City Race X Female	Full Race X Female
Female	0.0196**	0.00794	0.0110
	(0.00875)	(0.0186)	(0.0208)
Black	-0.0608***	-0.0520***	-0.0441**
	(0.0143)	(0.0177)	(0.0205)
Latino	0.0154	0.0304	0.0453*
	(0.0129)	(0.0192)	(0.0221)
Arab	-0.0455***	-0.0854***	-0.0778***
	(0.0113)	(0.0136)	(0.0177)
Black X Female		-0.0188	-0.00587
		(0.0222)	(0.0287)
Latino X Female		-0.0377	-0.0303
		(0.0280)	(0.0444)
Arab X Female		0.0921***	0.101***
		(0.0198)	(0.0230)
City Dummies	X	X	X
Unit Controls			X
Observations	9672	9672	5777

Standard errors in parentheses
* p<.10, ** p<.05, *** p<.01

Chapter 2

Can Landlords Change?
Evidence from a Randomized Experiment Targeting Discriminatory Behavior in Rental Housing

Introduction

Fair housing laws have been on the books for more than 50 years, yet housing discrimination continues to exist. The U.S. Department of Housing and Urban Development (HUD) and local fair housing groups receive thousands of complaints each year accusing landlords of unfair practices towards applicants and tenants on the basis of race, gender, religion, familial status, sexuality, and more. HUD and HUD-funded fair housing groups completed more than 8,000 investigations of discrimination in both fiscal years 2014 and 2015, and nearly $250 million in monetary relief resulted from these cases. Findings from dozens of academic studies offer additional evidence of landlords, rental agents, and realtors systematically practicing discrimination towards groups protected by fair housing laws.

Despite a large body of evidence demonstrating discrimination exists, systematic attempts to reduce or eliminate discriminatory behavior are generally limited to enforcement and tenant education. The primary mechanism, enforcement, threatens landlord profitability, much as jail time serves as a deterrent to criminal behavior. Another strategy – educating the public at large – aims to support the enforcement mechanism by informing tenants of their rights and how to file complaints. While important and well-intended, each approach depends on individual tenants filing claims against discriminating landlords in order to be effective. This places tenants in a difficult situation, namely, reporting on the landlords from whom they seek housing. Offering evidence about the limits of these approaches in overcoming housing discrimination are findings that roughly 80% of tenants who believe to have experienced housing discrimination remain silent (Abravanel, 2006).

This project proposes a complementary strategy to tenant-based mechanisms that directly targets rental agents and landlords. By focusing policy interventions at the group with the power to discriminate, this approach reduces the dependency on victim reports to mitigate discrimination. The project details an experiment designed to influence landlord behavior. The purpose is to test whether landlords and rental agents will change their behavior in response to being presented with information about their obligations under fair housing law. The primary hypothesis is that the "treatment" email will increase landlord response rates.

There are at least three reasons why informing landlords of the law may increase response rates. First, some landlords may tune out to public service announcements targeting tenants. While landlords may hear about the law as members of the population at large, the messages are communicated to renters. To the extent that many landlords are indeed home owners; they may simply ignore messages targeted at a population they do not identify with. Thus, directly targeting landlords may improve their awareness.

A second reason is that the Internet has created a new generation of landlords who may not understand or be aware of their obligations under the law. Websites like Craigslist.com allow property owners to advertise rental units with no paper work, on their own time, and at no cost. Not only does this make it easier to rent property, but it saves landlords the significant commission paid to realtors to find tenant(s), which is commonly one to one-and-a-half month's rent. These new landlords differ from traditional rental agents in important ways. Notably, many are part-time realtors/landlords who lack a real estate license. Unlike licensed realtors, these landlords are not trained or tested about fair housing law and may be unfamiliar with their obligations. Presenting them with the information, then, may improve their behavior.

The above reasons assume a treatment effect will be driven by a lack of knowledge, though some discriminating landlords may change their behavior due to fears about being caught. The overall risk of being caught discriminating against tenants is relatively low. Evidence discussed earlier suggests some tenants may be afraid and most do not file complaints when they feel discriminated against. With online platform such as Craigslist providing anonymity to advertisers, discriminating landlords may be using this to their advantage. While the consequences of being caught are certainly a threat to profitability, some landlords may feel the benefits to discriminating outweigh the risks. It is possible that informing these landlords about the law will suggest that someone is monitoring their posts, regardless of anonymity. If the presence of a monitor causes some landlords to perceive a higher risk associated with discriminatory actions, these landlords may alter their behavior and act more equitably towards potential tenants.

The hypothesis that the treatment will have a positive effect on landlord response rates is consistent across all three scenarios. Identifying which of the above causes may influence landlords is important, however, as it can inform policy about whether it is due to a lack of information about the law or new technologies are creating opportunities for landlords to circumvent. This is not the primary focus of this paper, though potential opportunities to test at least one of these causes are discussed in Section 4.

The contribution of this paper, then, is to demonstrate a unique opportunity to test policy interventions aimed at reducing discrimination in a real housing market and at a very low cost. It is hoped that the method can be modified and expanded by fair housing agencies, advocates, and other institutions to test and implement policy interventions designed to decrease discrimination.

The proof of concept provided by this paper is a randomized experiment that informs landlords of their legal obligations and tests whether this knowledge influences their behavior. The experiment creates a treatment group of randomly assigned online rental housing posts to send an informational email about fair housing law. After disbursing treatment emails, a correspondence audit is conducted as a means for testing whether the informational email influences landlord behavior. The audit method measures whether landlords receiving fair housing information respond differently to inquiry emails than do those landlords who did not receive the information. A differential response rate between the two groups of landlords is interpreted as a treatment effect.

The paper proceeds as follows. Section I begins with a discussion of fair housing laws and efforts to reduce discrimination. Specific attention is given to the primary mechanisms that have been used to combat housing discrimination, notably enforcement and education. The objective of this discussion is to provide the motivation for directly targeting policy interventions at landlords. Section II describes the experiment while Section III details the correspondence audit methodology used for measuring the treatment effect. Section IV presents the results of the initial experiment, which are intended to serve as a proof of concept for the potential of the method. Section V concludes the paper with a discussion of the project and possible extensions.

The Case for Targeting Landlords

The Fair Housing Act of 1968 represents the tipping point after which the federal government became actively involved in fair housing. The act made equal treatment in housing a federal law, preventing discriminatory practices on the basis of race, color, nationality of origin, and gender. In 1988 the act was expanded to cover disabled persons and families with children. Importantly, the act and its amendments go beyond protection and provide funds for state, local, and non-profit organizations to assist with educating the public and assist with enforcing these laws.

The most recent Annual Report on Fair Housing released by HUD is for the fiscal years 2014-2015.[13] In aggregate, HUD and its state and local partners closed more than 16,000 fair housing cases. There are 88 housing agencies in 35 states receiving fair housing funding from HUD, representing more than 80% of the cases filed and closed during the 2-year period. Figure 1 presents these cases on the basis of the complaint, revealing people with disabilities (42%) represent the largest group of filers. Race (21%) and Nation of Origin (9%) make up nearly a third of complaints, and Families with Children (9%) and Gender (8%) also represent sizable groups filing complaints.

Beyond enforcement, HUD awarded the National Fair Housing Alliance with $2.4 million in funding for a nation-wide media campaign targeting tenant education. The theme, "Fair Housing Is Your Right. Use It," reached millions of Americans through public services announcement on the Internet, radio, and television. The campaign focused on informing tenants of their rights and included a "How to File a Complaint" video. While these educational efforts are certainly important, filing a complaint against a landlord may be risky, as 8% of complaints filed in 2015 refer to retaliation against tenants by offending landlords.

HUD's role in fair housing extends well beyond the above media campaigns and enforcement to include a role as the primary institution designing and implementing policy. In this capacity,

[13] Source: https://portal.hud.gov/hudportal/documents/huddoc?id=FY14-15AnnualReport.pdf

HUD has and does fund research efforts to measure public perceptions, discriminatory practices, and the effectiveness of policy interventions. Two efforts are relevant to this paper.

The first, the Housing Discrimination Study (HDS), provides the largest and most comprehensive measure of discrimination at a national level. Conducted every 8-12 years since 1977, the study is designed to provide a snapshot of current levels of housing discrimination and, importantly, allow for comparisons with previous studies. The in-person, paired audit method employed (discussed further in Section II), represents the most influential test of discrimination practices and has been upheld by the courts and successfully used by enforcement agencies in countless legal cases.[14] The most recent 2012 HDS reveals measures of discrimination have fallen over time, though they are far from being eliminated. The findings of the HDS studies serve as invaluable tools for practitioners, researchers, and policymakers and have been influential in shaping federal, state, and local fair housing policy for decades.

A second notable HUD initiative with relevance to this project is two HUD-sponsored surveys examining public knowledge of fair housing laws. Conducted in 2000/1 and 2005 (Abravanel, 2006) (Abravanel, 2002), the studies were designed to provide a nationally representative estimate of public knowledge about the law. Participants were provided with a series of ten hypothetical scenarios taken by various housing providers and asked whether the actions taken were in violation of the law. The surveys were designed to be comparable, with the objective of measuring changes in public awareness resulting from a national fair housing educational campaign conducted by the Ad Council beginning in August of 2003. The results are informative.

[14] See the HUD website or Oh and Yinger 2015 for examples.

Roughly 50% of participants correctly identified the legality of a behavior in at least six of the ten scenarios. Public knowledge did not appear to change in any notable way over the period, however, despite the large-scale education campaign. Seventeen percent of the participants believed they had been discriminated against, the majority of whom indicated it to have occurred more than once. Despite this, fewer than 20% who believed to have been treated unfairly acknowledged contacting a local agency or filing a complaint.

The enforcement and educational role of HUD, the national housing discrimination studies, and the public awareness surveys provide the theoretical foundation for this experiment. As demonstrated by the annual report and HDS, housing discrimination persists and continues to limit the housing options of thousands of households each year. The primary mechanisms for attacking this discrimination – education and enforcement – play important roles in reducing discrimination, but their dependency on tenant reporting may limit their effectiveness. Roughly one in ten complaints filed are on the basis of retaliation, and as much as 80% of perceived victims acknowledge remaining silent.

The evidence suggests complementary strategies that reduce dependency on tenant reporting may be useful. Thus, the remainder of this paper presents one such strategy, focusing on landlords. The utility of the design is that the low-cost strategy uses methods that are familiar to all fair housing advocates, and offers an opportunity to directly inform policy and test potential interventions.

Experimental Design

The experimental design of this project is composed of two distinct methods, a policy intervention and a correspondence audit study. The policy intervention, or treatment, is designed to provide a random sample of rental housing agents with information about fair housing law. Following this treatment, a randomized correspondence housing audit is conducted to measure whether landlords receiving the fair housing information have different response rates to potential applicants than do those who do not receive the information. The complete randomization of each phase of the project allows for interpreting a differential response rate between the groups as the treatment effect of the fair housing information.

Policy Intervention

The policy intervention is an email that emphasizes several aspects of fair housing law. The primary goal is to communicate information, so it details what the law says about the obligations required of agents and landlords in the sale and rental of housing. While online buy/sell platforms such as Craigslist.com do inform advertisers they must abide by the law, Figure 2 demonstrates the emphasis is on the advertising of the unit, not the tenant selection process.[15] The treatment email therefore makes it clear that the law applies throughout the entire sales process. The email also provides links to websites offering additional information and resources, such as HUD.

In order for the treatment to be effective, the treated landlords must find it believable and differentiate it from SPAM. In order to accomplish this, the website www.FairHousingAwareness.org and the online profile for Eric M. were created. As the motivation for this study is to attempt to influence landlord behavior, concern existed that landlords might simply disregard an unsolicited email from an unknown source. The primary purpose of the website, then is to provide validation for the treatment email.

[15] https://www.craigslist.org/about/FHA

The website was designed from the perspective of a private citizen and online landlord who cared about fair housing due to personal experience. The website therefore has several pages, informing visitors about its mission, the personal experience of online landlord Eric M., and provides links to resources about fair housing (Figure 4, Figure 8-11). The decision to name the website was then determined by the informative nature of Eric M's mission and what domain names were available.

The website serves an additional purpose. Practically, the website creates a profile and domain name from which treatment emails can be sent (Eric@fairhousingawareness.org). While email domains can be achieved in various ways, developing the website was determined to be the most practical way to create an effective and believable treatment that provided inquisitive landlords with access to additional information.

Once the treatment email was designed, the next step was to determine how to disburse treatment. After considering a wide range of options, the decision was made to purchase an email marketing program from www.MailerMailer.com. The advantages of MailerMailer were significant. First, it offers a user-friendly interface for designing professional-looking emails that can be customized to individual email addresses. This feature allowed for treatment emails to be sent in bulk, saving considerable time and resources.

A second benefit, however, proved to be even more valuable. In the process of selecting a marketing program, several dozen firms were considered because they offered the ability to track whether emails were read and if the links included were clicked on. Only MailerMailer, however, allowed for downloading the open and click rates at the individualized landlord level. Other products allowed for aggregate charts, but what was needed for the purpose of this study was the ability to identify which individual landlords opened the treatment emails they received. This key distinction of MailerMailer set it apart.

Leveraging Audits to Identify a Treatment Effect

This experiment employs a housing audit in order to test the effect of a policy intervention aimed at reducing discrimination. To the best of the author's knowledge, this is the first time the audit method has been used in this capacity.

Audits refer to a type of experiment used in testing for discriminatory behavior in live markets where decisions are made without participant awareness of the audit being conducted. The purpose is to identify discriminatory behavior as it exists in practice. In contrast with many experiments, audits examine the behavior of individuals without informing participants that they are taking place in a study. This is done to mitigate the risk that the knowledge of the audit influences behavior. Thus, the unique opportunity presented by this experimental design is to leverage the audit framework as a means for testing the impact of policy interventions in a normally functioning market.

Types of Housing Audits

Housing audits began as in-person experiments in which trained actors inquire about and apply for a housing unit advertised in a local newspaper. The actors are then paired according to traits likely to influence landlord decisions, such as age, income, education, job, appearance and more. To clearly identify that the differences in treatment are driven by the attribute being tested (i.e. Race, Gender, etc.), great care is taken to ensure the applicants qualifications for renting the unit are as similar as possible. Actors record multiple aspects of the encounter, such as the time the agent spends with the applicant, the availability of the advertised unit, and more. Discrimination is then identified as differential treatment between the two groups.[16]

Changes in the way households find housing has created a new opportunity for conducting audits. Online housing websites, such as www.Craigslist.com, provide a platform through which landlords and rental agents can advertise housing to anyone with an Internet connection. Interested applicants can submit inquiries via email, often requesting additional information and providing some personal details to the posting agent. These online marketplaces thereby create an extraordinary opportunity for fair housing advocates and researchers to test for discrimination.

Posing as interested applicants, email inquiries are submitted for advertised housing units posted online. Using names and other signals that are commonly associated with particular ethnic or demographic characteristics, testers compare landlord response rates to identify differential treatment. The most common examples test racially or ethnically associated names, such as Johnny O'Brien or Darnell Robinson, and compare landlord response rates to each (Carpusor & Loges, 2006) (Ahmed & Hammarstedt, 2008) (Hanson & Hawley, 2011) (Hanson & Santas, 2014). As the method has evolved, other variations have been used to test differential treatment by gender (Ewens, Tomlin, & Wang, 2014) (Andersson, Jakobsson, & Kotsadam, 2012), sexuality (Ahmed, Andersson, & Hammarstedt, 2008), (Friedman, 2013), (Schwegman, 2017), class (Hanson & Hawley, 2011) (Andersson, Jakobsson, & Kotsadam, 2012) (Bosch, Carnero, & Farre, 2010), employment (Ahmed, Andersson, & Hammarstedt, 2010), behavior ((Ahmed, Andersson, & Hammarstedt, 2010) (Ewens, Tomlin, & Wang, 2014)), among others. See Oh and Yinger 2015 for a detailed review of the literature.

[16] See Yinger, 1986 for detailed discussion.

Correspondence audits provide narrower measure of discrimination, however, examining only landlord responses to inquiry emails. Much of the nuance acquired during in-person audits is lost, and causal mechanisms are more difficult to identify. An important benefit, however, is that correspondence audits can avoid the risk of actor-influences on results, as the only information communicated to landlords are text-based signals. Thus, while more narrow in scope, cleaner identification is possible. This clean identification, combined with scarce resources available to scholars, practitioners, and advocates of fair housing, gives the correspondence audit design considerable advantage for the proposed experiment.

A decision must be made about whether to employ a matched-pair or randomized audit framework. The key distinction between the two methods is that landlords in a matched-pair audit receive two or more inquiries from multiple applicants, while landlords in a fully randomized audit receive only a single email. Differential treatment in a matched-pair audit measures the response rate within each landlord, while treatment in a randomized audit is measured across the landlords in the sample. Though both methods can be employed in this experimental design, selection has important trade-offs.

A fully randomized audit design was selected as the preferred framework for measuring this treatment effect. The primary driver of this decision was for identification purposes. A key concern in matched-pair audits is that of detection. If landlords become suspicious an audit is occurring, they are likely to alter their behavior and compromise the validity of the results. Testers thus go to great lengths in matched designs to minimize the risk of detection, but there are important trade-offs at each step that can compromise the audit. As this is the first time the audit method has been used to measure a treatment effect, a randomized design was selected in order to all but eliminate detection risk.

A secondary benefit of the randomized design is practical. The randomized audit allows for a simple treatment and a control group set-up. All landlords are selected at random and a random sample of these is primed with the treatment email. Following treatment, email inquiries are sent to the full sample, and response rates between the treated landlords can be easily compared with the untreated landlords. The benefit of the randomized design is that timing concerns about the order of sending the inquires are eliminated and the syntax of each email is identical across the treatment and control groups. Though the randomized audit design requires a larger sample of landlords, it is believed that the ease of implementation ultimately saved time.[17]

Designing Signals

Central to an effective correspondence audit design is communicating group status through signals included in each email inquiry. As this experiment is designed to assess the impact of a policy intervention on landlord treatment of groups protected under fair housing law, multiple signals were employed. Signals identifying race, gender, family status, sexuality and religion were used. The details of how the signals were communicated are as follows.

The randomized design allows a nearly identical syntax be used for all emails, the base form of which is included below. Each email inquires about the unit's availability and volunteers some positive information about each applicant. A second version of the email is written to provide a level of class variation, including several grammatical errors and less formal language.

Dear sir/madam,

[Body Signal 1] I am interested in the rental unit posted on Craigslist, is it still available? I have good references, don't drink or smoke *[Body Signal 2]*, and am happy to send a copy of a recent credit report.

Regards,

[*First name*] [*Last name*]

[17] An exciting opportunity exists to employ a matched-pair audit framework to exploit a pre and post treatment effect. See Section V.

[*Signal Quotation*]

Following previous studies, race, ethnicity and gender were signaled using names commonly associated with a particular ethnicity or gender. A notable deviation from other correspondence studies in the U.S. is that multiple races and ethnicities are included for which limited name data exists for the U.S. Thus, first names selected for all races come from websites listing common names for each groups. Surnames used for whites and blacks came from Bertrand and Mullinathian (2004), while Latinos and Middle-Eastern names were constructed in similar fashion to the first names. (Table 23) To mitigate potential concerns about what is inferred from names constructed in this manner, a second and more direct signal is also used in the signature line of each email to communicate group membership.

Signaling non-race based protected group membership is not possible using names. Thus, signals must be communicated in a manner that might reasonably be included in an introductory email inquiry for housing. To accomplish this, membership was signaled in the body of each email, by slightly modifying the base email to suggest group membership. (Figure 5) Signaling sexuality, for example, “My partner and I” is inserted to [Body Signal 1] to introduce who is inquiring about the unit. Signaling religion is done in [Body Signal 2], by adding the phrase “due to my Islamic(Christian) beliefs” as the motivation for not drinking and smoking.

To ensure at least two signals for group membership are included for each group, an additional signal is provided in the signature line of each email inquiry. Continuing with the religion example, Christian and Islamic variations of the “Golden Rule” were included.[18] Quotations signaling race, gender, sexuality and family status are also used (Table 13). This additional

[18] Iterations of each email are included in the appendix

signal is used to increase the probability that the landlord identifies the email inquiry as being sent from someone in a particular group. Table 14 summarizes the signal types used by protected class.

Sample Collection

The objective of correspondence audit in this experiment is to assess the treatment effect across a spectrum of protected classes covered by U.S. Fair Housing Law. To estimate the effect, the research employs a randomized design using housing units posted on Craigslist in the 20 most populated U.S. cities.

Craigslist is an ideal venue to conduct a correspondence audit for several reasons. It offers a local webpage in nearly every U.S. city, receives a high volume of traffic, is free to use for both landlords and renters, and employs an identical format across all U.S. apartment rental sites. More than 50 billion page views occur globally each year, and 60 million people use Craigslist each month in the U.S. alone.[19] Of particular attractiveness to audit studies is that the format for housing posts is identical across all U.S. sites, allowing unit-specific data to be gathered in a consistent manner across all cities.

Units were randomly selected on each day of the week and inquiry emails were submitted within 24 hours of being posted on Craigslist. Posts were collected from each city in batches of 100 using a semi-automated process.[20] Extensive effort was made to avoid sending inquiries to landlords posting multiple units. The de-duplicating process began by eliminating units sharing the same Craigslist ID, address, phone number, realty company, or housing development. Next, individual unit advertisements were scanned for similar characteristics that suggested a shared landlord. Landlords commonly use the same format for multiple posts, similar marketing

[19] Craigslist.com
[20] The collection process employed a program written by Phil Murchie. The program provides a semi-automated process for gathering unit information and contacting landlords. Without this tool, the data gathering process would have been significantly more cumbersome.

phrases, company logos, and other practices. Any unit raising even the slightest doubt was eliminated. Finally, posts were compared with a master-file of all previously de-duplicated posts and a second round of de-duplication was conducted. In total, roughly 60% of posts were deleted using this process.

Each post received a single email indicating interest in the unit, from one of 24 names varying by race and gender and of the form first.last####@gmail.com. Each inquiry was then randomly assigned group membership for one of 8 additional profiles. The first emails were sent July 25th, 2016 and responses were recorded until September 19th, 2016, which was two weeks after the final inquiries were sent.

The original experimental design called for a large enough sample size to allow for the potential to conduct subgroup analyses within race and protected group. The intent was to examine whether some groups experienced higher response rates as a result of the treatment in comparison to others. Such analysis might prove informative about the mechanisms through which the treatment operated. If larger treatment effects are found among protected groups relative to race or ethnicity alone, for example, it might suggest online-landlord have a lower awareness of non-race-based protections.

Unfortunately, a structural flaw occurred in the disbursement of the treatment emails that limited treatment to a particular subset of Craigslist landlords. Craigslist advertisers have the option to use identifiable contact information or choose anonymity through a randomized, Craigslist-generated temporary email address. This provides privacy to sellers until they opt to reveal their identity to potential buyers. This also creates two distinct groups of sellers, those who provide identifiable information and those who do not. Based on the random sample collected for this project, roughly nine of 10 housing advertisers opt for anonymity.

This distinction had important consequences for the disbursement of treatment emails. A feature in the design of the treatment email is believed to have triggered a flag in the Craigslist email-relay servers, which prevented the treatment email from reaching landlords opting for anonymous email addresses. Treatment emails sent to landlords who provided identifiable contact information were received, however, as evidenced by the MailerMailer reports. As the daily send reports provided only aggregate snapshots of open rates, the glitch went unnoticed until analysis began.

Sample Characteristics

The results of the structural problem limited the treatment to the subset of landlords who opted to provide identifiable information. In order to create a suitable control group, then, only landlords providing their contact information are included in the analysis. The randomness of the sample and distribution of treatment remain; however, the sample size reduced to roughly 10% of the originally collected sample. Ultimately, a total of 1,112 landlords are included, of which 515 received the treatment email.

Table 15 presents the number of observations for the treatment and control groups, aggregated by protected category. Aggregating in this way results in non-exclusive categories, as protected class is assigned across race and ethnicity. Non-white applicants represent roughly three-quarters of both the treatment and control groups. Emails signaling protected group membership account for 93% of the sample, as all inquiries in the experiment sent from applicants other than white males are protected under fair housing law.

Empirical Results

Figure 6 presents the mean response rate for the aggregate protected groups. These simple averages show the response rate for inquiries from the treatment group to be 3.8% - 4.6% higher than that of the control group. Figure 7 presents the response rates for the subgroups and reveals a similar pattern. With the exception of Latinos, who received equal responses in both groups, responses from landlords were 2% - 9% higher in the Treatment group than in the Control group.

The statistical significance of these differences in means was assessed using paired sample t-tests and is presented in Table 16 T-Tests for Differences in Mean Response Rates:. Despite the consistency in which the Treatment group received higher response rates, the t-tests reveal these differences in means are not statistically significant by conventional measures, either in aggregate or at a subgroup level. Given the effect sizes, a power analysis using G*Power 3.1 software[21] was conducted and indicates a sample of the originally intended size is necessary to achieve significance.

The objective of this experiment is to understand how the effect of receiving an email about fair housing law influences landlord response rates to housing inquiries. By utilizing the "Click" data from MailerMailer to identify which landlords opened the treatment email, an opportunity for further analysis exists. Table 17: Landlords Who Received Treatment is descriptive and shows 50% of the landlords who received the email actually opened it, and those who opened the email were 23% more likely to respond than those who did not open or receive a treatment email.

Table 18: IV Regression Results (full sample) incorporates these data into a regression framework. Comparing landlord responses to those who opened the email with those who did not will overstate the treatment effect, because opening an email is certainly correlated with one's

[21] Can be downloaded free.

likelihood of responding. Further, as those receiving the treatment email can choose whether to comply (open the email) or decline (ignore the email), the compliers no longer represent a randomly assigned treatment group. Therefore, the large positive and significant effects in Columns 1 and 2 are biased.

Columns 3 and 4 compare differences in landlord response rates to those who were randomly assigned treatment with those who were not. Due to random assignment, these intent-to-treat (ITT) estimates can be interpreted causally, suggesting landlords who were sent a treatment email were about 4% more likely to respond. Though causal, these estimates understate the treatment effect on those who received treatment; because 50% declined the treatment they were offered.

Angrist and Pishke (2009) discuss that an instrumental variable (IV) approach using randomly assigned treatment can fix this compliance issue. Using those landlords who were offered treatment (ITT) as an IV for those receiving treatment provides a causal estimate of the treatment effect. The ITT represents an ideal IV because it is correlated with treatment – a landlord must receive the treatment email in order to open it – and the random assignment of the treatment across the sample of landlords ensures independence. Further, because non-compliance is limited to the treatment group, the estimates represent a special case of a local average treatment effect (LATE) that is interpreted as the effect of treatment on the treated (TOT).

Columns 5 and 6 present the results of the IV analysis. The IV models suggest treatment increases landlord response rates by roughly 8%, though the results are not significantly different than zero by conventional measures. Being that 50% of the landlords declined treatment, an 8% treatment on the treated estimate makes sense, as it is equivalent to the ITT effect of 4% divided by the 50% compliance rate. Put differently, because of the random design and one-sided compliance, the TOT effect of 8% represents the ITT effect (4%) divided by the compliance rate (50%).

Table 19 restricts the ITT and TOT analysis to the aggregated Non-White and Protected Group subsamples. These subgroups are selected because the intent of informing landlords about the law is to improve response rates to protected populations. Consistent with the results of the ttests, the response rate for the treated group of landlords is higher among the non-White (6.3%) and Protected (8.9%) samples. Power analysis again indicates, however, a larger sample is necessary for significance at conventional standards.

Discussion

The results of the experiment consistently suggest the group of landlords who received information about fair housing law responded at a higher rate than did those who did not receive the treatment email. Thus, the experiment offers evidence that utilizing the audit framework for testing policy interventions is a ripe opportunity for further research by scholars and practitioners. Several examples are discussed below.

A simple extension that is currently underway will achieve a larger sample size and tweaks the informational email slightly to test the effects of varying the intervention. The original email informs landlords about their obligations, but it is not designed to identify what is motivating the effect. Varying treatment from a simple public service announcement to one that emphasizes legal actions that can be brought against landlords can offer insight into the extent to which landlords are motivated by fear of getting caught.

From a practitioner standpoint, conducting a similar experiment but sending the emails from a fair housing advocacy group or government agency offers a similar opportunity. Similar to the

HUD-sponsored Ad-Council campaigns and analysis, such an experiment might focus on educating landlords, rental agents, or mortgage lenders about their obligations under the law and then utilize the audit method for testing the efficacy of the intervention. Such an analysis might involve multiple audits over time, going back to the same landlords, for example, to test for the longitudinal effect of the campaign.

A particularly unique opportunity exists within any platform for which identifiable emails are available, such as the Multiple Listing Service (MLS), the subset of Craigslist landlords, or mortgage brokers. By exploiting identifiable emails, for example, a pre- and post-design can be employed by conducting multiple audits with the policy intervention being conducted in between. Such an experiment might build on the work of Hanson, Hawley and Martin (2017 working paper) to estimate peer-effects, for example, and design an intervention to inform mortgage brokers about the behavior of their financial institution when receiving inquiries from minority applicants.

Such interventions do not need to be limited to correspondence audits or even housing. HUD might, for example, apply such a design to test hypotheses about a variety of issues into the in-person discrimination studies. By providing tested rental agents or landlords with specific information about steering prior to the audit, for example, landlord behavior can be examined beyond the initial inquiry process correspondence audits are generally limited to. Beyond housing, opportunities may exist in labor or other fields, though achieving a sufficient sample may be limiting due to differences in buy-sell websites like Craigslist and job boards.

Conclusion

Ultimately, the intent of this project is to propose a complementary strategy to existing efforts to reduce housing discrimination. Though the sample size limits the significance of the results, the author hopes the design inspires other fair housing advocates to develop similar ways to creatively influence the behavior of those with the power to discriminate. Decades of audits have shown discrimination continues to limit the opportunity of many to find housing and jobs. Many efforts have been made to motivate individuals who have been discriminated against to take action, though these efforts have not fully overcome the problem. It is the intent of this design, then, to provide policymakers, advocates, and researchers with a framework for which to test the efficacy of policy interventions on the particular group of individuals who limit the opportunities of others.

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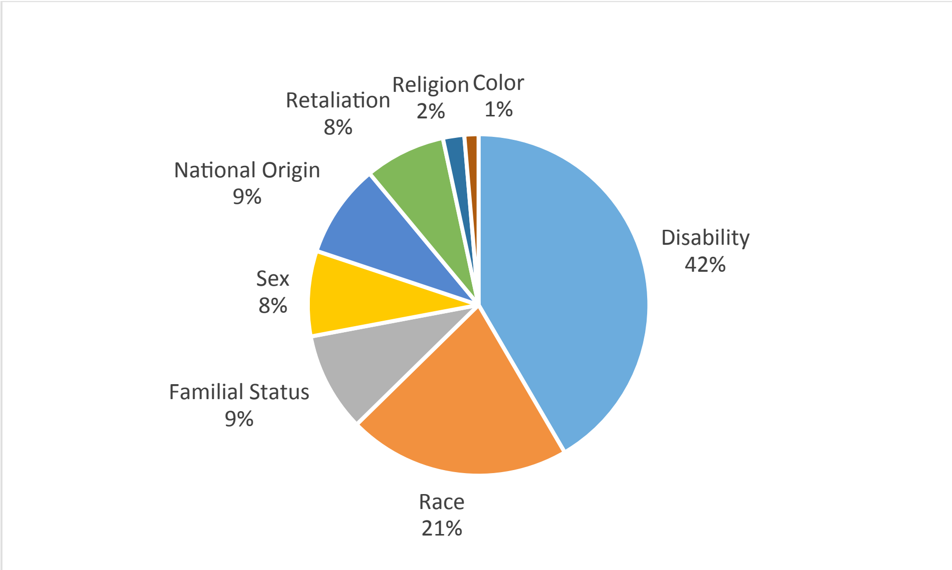
Figure 1: HUD and HUD-Partner Fair Housing Complaints 2014-2015



Source: HUD Office of Fair Housing & Equal Opportunity Annual Report to Congress (2014-2015)

Figure 2: Equal Opportunity in Advertising, not Housing

EQUAL HOUSING OPPORTUNITY

Fair Housing is Everyone's Right!

Stating a discriminatory preference in a housing post is illegal

(Questions? Comments? Check out the fair housing forum)

When making any posting on craigslist, you must comply with section 3604(c) of the Federal Fair Housing Act. This law generally prohibits stating, in any notice or ad for the sale or rental of a dwelling, a discriminatory preference based on any of the following protected categories:

- **Race or Color**
- **National Origin**
- **Religion**
- **Sex**
- **Familial Status** (more)
- **Handicap / Disability** (more)

The Fair Housing Act provides additional protections, and limited exceptions, that are explained in publications from the U.S. Department of Housing and Urban Development ("HUD") and the Department of Justice.

HUD has issued guidance on advertising, including for roommates.

State and local laws often prohibit discrimination based on other factors (e.g. sexual orientation, age, marital status, or source of income).

You may report housing discrimination to HUD at 1-800-669-9777, or to a fair housing advocate near you.

If you encounter a housing posting on craigslist that you believe violates the Fair Housing laws, please flag the posting as "prohibited".

In addition to penalties that may be applied by regulatory agencies, attempts to post discriminatory ads may be blocked and/or subjected to other remedial measures.

Source: https://www.craigslist.org/about/FHA

Figure 3: Fair Housing Treatment Email

Having trouble reading this email? View it in your browser.

Dear fellow landlord,

Did you know it is not only illegal to discriminate in your advertising of a unit, but also in how you treat/respond to applicants? Equal treatment is required THROUGHOUT the sales process! Don't make the same mistakes we did and unknowingly break the law.

By posting a rental housing advertisement online, you are RESPONSIBLE to comply with Fair Housing law throughout the ENTIRE sales process. If you do not, you can be held LIABLE in court. Make sure you are informed about how the law applies to you! As listed by the Dept. of Housing and Urban development, Federal housing law indicates:

In the Sale and Rental of Housing: No one may take any of the following actions based on race, color, national origin, religion, sex, familial status or handicap:

- Refuse to rent or sell housing
- Refuse to negotiate for housing
- Make housing unavailable
- Deny a dwelling
- Set different terms, conditions or privileges for sale or rental of a dwelling
- Provide different housing services or facilities
- Falsely deny that housing is available for inspection, sale, or rental

For more detailed information, visit the U.S. Department of Housing and Urban Development's website or our own website (www.fairhousingawareness.org) that is dedicated to helping landlords avoid making the same mistakes we did.

Kind regards,

Eric M.
www.FairHousingAwareness.org

#[www.fairhousingawareness.org]#
Unsubscribe

Figure 4: Treatment Website (www.FairHousingAwareness.org)

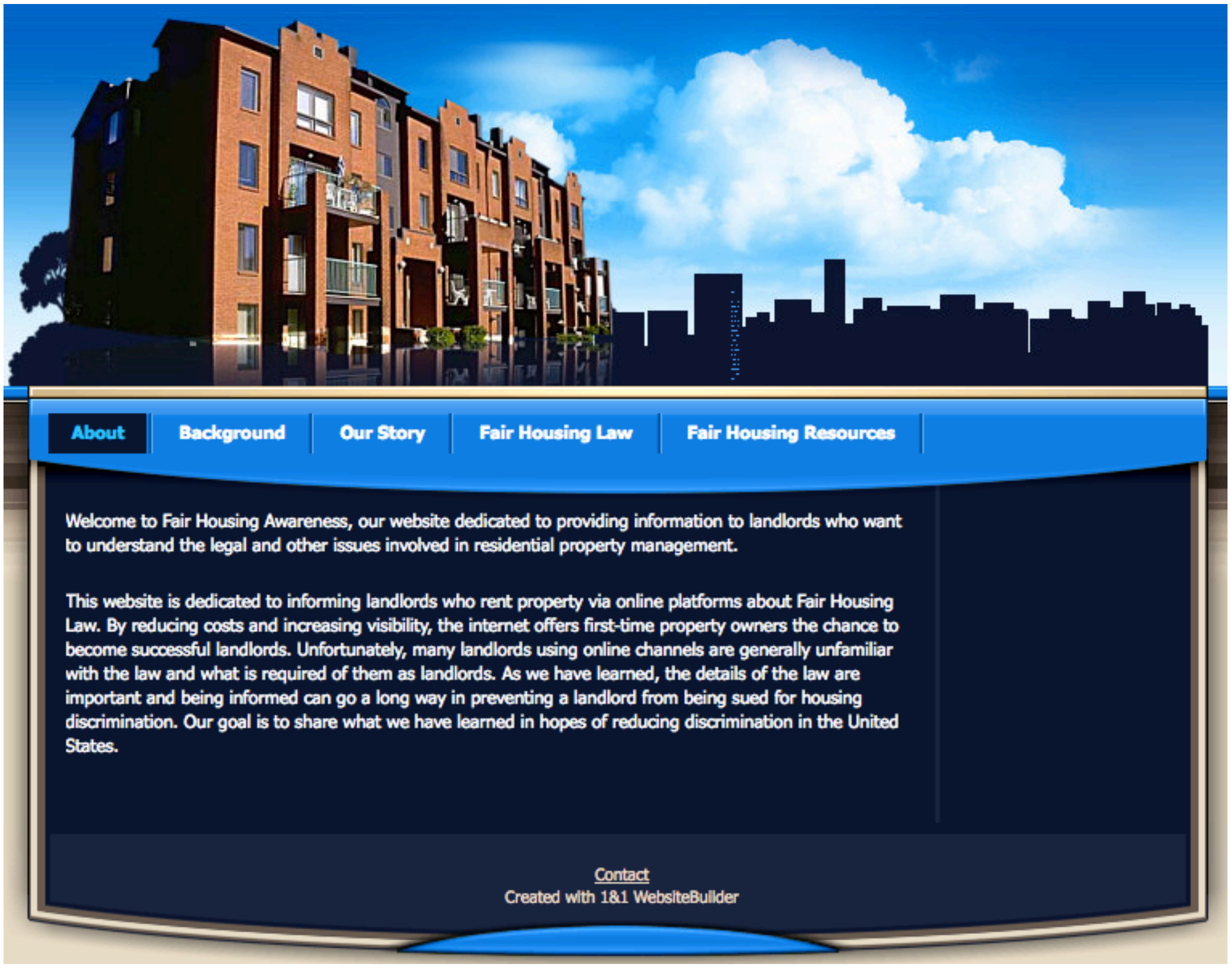


Figure 5: Sample Audit Emails and Signals

Base High-SES Email

Dear sir/madam,

[Body Signal 1] I am interested in the rental unit posted on Craigslist, is it still available? I have good references, don't drink or smoke *[Body Signal 2]*, and am happy to send a copy of a recent credit report.

Regards,

[*First name*] [*Last name*]

[*Signal Quotation*]

Body Signal 1

Single Parent [*My kids and I*]

Gay / Lesbian [*My partner and I*]

Body Signal 2

Christian [*due to my Christian beliefs*]

Muslim [*due to my Islamic beliefs*]

Base Low-SES Status

Hi! Saw your post online. [*Body Signal 1*] I'm looking for a place, is it still avialable? I stay away from partyin [*Body Signal 2*] and can give you references is you want. Need anything else? peace,

[*First name*] [*Last name*]

[*Signal Quotation*]

Body Signal 1

Single Parent [*My kids and I*]

Gay / Lesbian [*My partner and I*]

Body Signal 2

Christian [*because I'm Christian*]

Muslim [*because I'm Muslim*]

Table 13: Signal Quotations Used in Audit Inquiry Emails

	Quotation
Male (non-Gay)	
White	*“Do unto others as you would have them do unto you”*
Black	*"The time is always right to do what is right." - Martin Luther King, Jr.*
Latino	*“I'm proud to be both Latino and American.” mi abuelo*
Arab	*“I'm proud to be an Arab-American.” My grandfather*
Female (non-Gay)	
White	*“A girl should be two things: classy and fabulous.” – Coco Chanel*
Black	*“I'm black, I don't feel burdened by it and I don't think it's a huge responsibility. It's part of who I am. It does not define me.” – Oprah Winfrey*
Latino	*“I'm bicultural, and everyone sees me as a Latina, but in my head I see myself as both Latina and American.” – Genesis Rodriguez*
Arab	*"If a beautiful woman is a jewel, then a good woman is a treasure." – Mahmood Abbas Al-A'aqad*
Protected Class	
Single Parent	*“I didn’t set out to be a single-parent. I set out to be the best parent I can be… and that hasn’t changed.” unknown single parent*
Gay Male	*"I'm homosexual. How and why are idle questions. It's a little like wanting to know why my eyes are green." - Jean Genet*
Lesbian	*"I'm living by example by continuing on with my career and having a full, rich life, and I am incidentally gay."--Portia DeRossi*
Christian	*“Do to others as you would have them do to you” - Luke 6:31*
Muslim	*“Wish for your brother, what you wish for yourself.” - the Prophet Muhammad (eng. translation)*

Table 14: Signal Types by Protected Class

	Name	Body	Quotation
Race	X		X
Gender	X		X
Single Parent		X	X
Religion		X	X
Gay		X	X

Table 15: Treatment and Control Group Landlords

	Treatment		Control	
	(N)	Group %	(N)	Group %
Total	515	100.0%	597	100.0%
Non-White	381	74.0%	430	72.0%
Protected Class	477	92.6%	558	93.5%

Figure 6: Raw Differences in Mean Response Rates (aggregate groups)

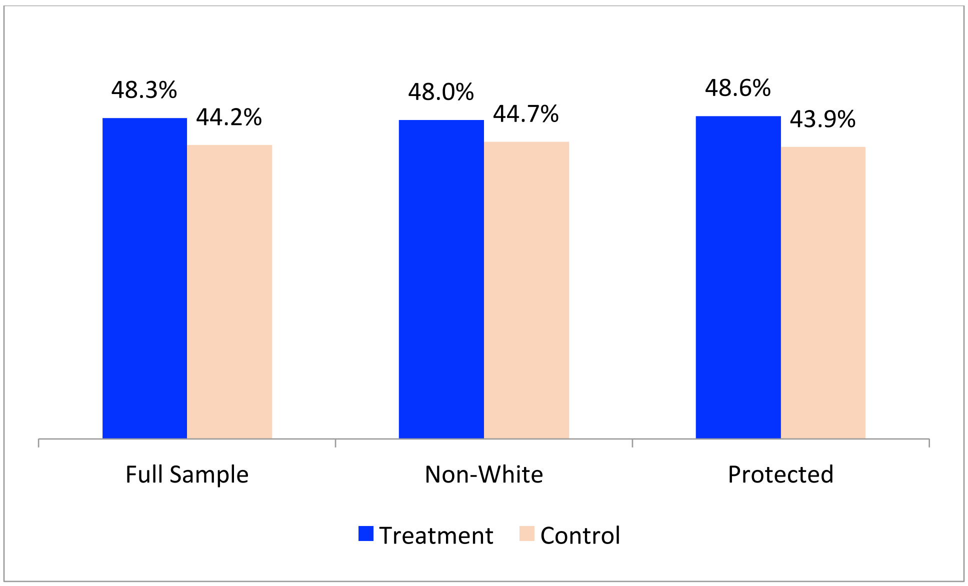


Figure 7: Raw Differences in Mean Response Rates (subgroups)

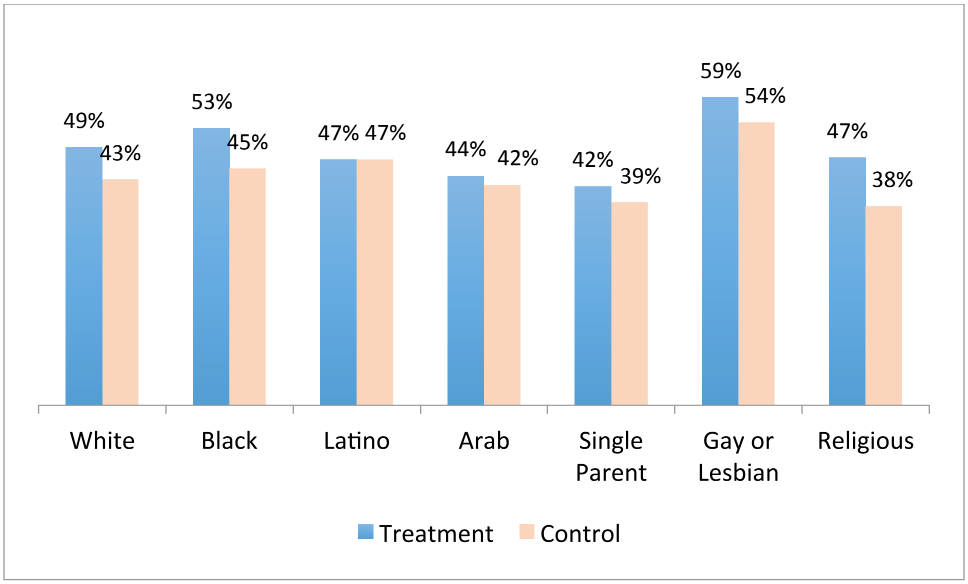


Table 16 T-Tests for Differences in Mean Response Rates:

	Treatment Group		Control Group		T-Test	
	N	Response %	N	Response %	Diff.	t=
Aggregate Groups						
Full Sample	515	48.3%	597	44.2%	4.1%	1.38
Non-White	381	48.0%	430	44.7%	3.4%	0.96
Protected	477	48.6%	558	43.9%	4.7%	1.52
Subgroups						
White	134	49.3%	167	43.1%	6.1%	1.06
Black	138	52.9%	168	45.2%	7.7%	1.33
Latino	113	46.9%	117	47.0%	-0.1%	0.02
Arab	130	43.8%	145	42.1%	1.8%	0.30
Single Parent	165	41.8%	163	38.7%	3.2%	0.58
Gay or Lesbian	68	58.8%	100	54.0%	4.8%	0.62
Religious	93	47.3%	129	38.0%	9.3%	1.39

Table 17: Landlords Who Received Treatment

	Control	Treatment	Total	Response Rate
Status unknown	597	258	855	40.8%
Opened	0	257	257	63.8%
Total	597	515	1,112	46.1%

Table 18: IV Regression Results (full sample)

	Comparison by Opening Email (OLS)		Comparison by Assignment Status (ITT)		Instrumental Variables Estimates (TOT)	
	Base (1)	+ City (2)	Base (3)	+ City (4)	Base (5)	+ City (6)
Treatment	0.2299	0.2011	0.0413	0.0419	0.0827	0.0822
	6.60	5.76	1.38	1.41	1.39	1.44
Constant	0.4082	0.2965	0.4422	0.2925	0.4422	0.3072
	24.39	5.78	21.67	5.39	21.92	5.99
City dummies		X		X		X
Observations	1,112	1,112	1,112	1,112	1,112	1,112

Table 19: IV Regression Results (non-white and protected class samples)

	Non-White Sample		Protected Sample	
	ITT	TOT	ITT	TOT
Treatment	0.0313	0.0627	0.0454	0.0890
	0.90	0.92	1.47	1.50
Constant	0.2931	0.3051	0.2892	0.3057
	4.70	5.19	5.14	5.76
City dummies	X	X	X	X
Observations	811	811	1,035	1,035

APPENDIX

Figure 8: Page 2 of www.FairHousingAwareness.org

Figure 9: Page 3 of www.FairHousingAwareness.org

About | Background | Our Story | Fair Housing Law | Fair Housing Resources

Our first venture as landlords began shortly after the housing crash. We had outgrown our property and were ready to move. The value of our home had declined significantly, though rents in our gentrifying neighborhood were beginning to rise. Instead of selling for a loss, we decided to rent our property in hopes of covering our costs and the market recovering.

Our home being our largest asset made it was essential we found good tenants. We simply could not afford to deal with a tenant that trashed our soon-to-be investment property. Friends told us it was significantly cheaper to rent through an online platform instead of going through a licensed Realtor. They also told us, however, we'd have to carefully screen everyone, because people using online platforms to find housing were "riskier" than those going through a Realtor. We figured we were pretty good at vetting people, so we opted to accept the risk and show the place ourselves.

What we did not realize was that many of the common ways we vetted people were technically in violation of Fair Housing Law. At a BBQ we hosted, we were sharing about our tenant search process with some friends when one of them informed us that some of what we were doing might not be legal. At first we didn't think much of it, but when we looked into it, we realized there were multiple things we did innocently that were illegal. For example:

- We immediately deleted any email mentioning kids. Kids can wreak havoc on a home, so we figured better to be safe than sorry.

- We specifically mentioned in our advertisement that we had a preference for others who shared our beliefs. Our religious beliefs lead us to generally abstain from smoking, as well as to respect others. We felt a shared belief system would provide a strong indicator of tenant quality.

- The neighborhood was gentrifying and there was a lot of tension between the minority community that had lived there for years and the yuppies (like us) moving in. Vandalism was rampant in the community, and we worried that renting to an individual from the minority community could prove costly.

We had no idea that each of the above is illegal under the Fair Housing Act, as described on the U.S. Department of Housing and Urban Development's website. Upon learning about our mistakes and about the motivation behind the law – notably that our actions limit access to the neighborhoods people want to live in, thus contributing to segregation – we have since sought to treat each inquiry equally, regardless of our initial perception. This change has not limited our ability to find great tenants, and it has greatly increased the pool of potential renters we consider.

Having shared our experience with others, we have become convinced our situation is quite common. This has led us to create this fairhousingawareness.org. Please send us an email if you have any questions or comments, or feel free to pass this information on to anyone you think might benefit.

Regards,

Figure 10: Page 4 of www.FairHousingAwareness.org

About | Background | Our Story | Fair Housing Law | Fair Housing Resources

1.) What Housing Is Covered?

The Fair Housing Act covers most housing. In some circumstances, the Act exempts owner-occupied buildings with no more than four units, single-family housing sold or rented without the use of a broker, and housing operated by organizations and private clubs that limit occupancy to members.

2.) What Is Prohibited?

In the Sale and Rental of Housing: No one may take any of the following actions based on race, color, national origin, religion, sex, familial status or handicap:

- Refuse to rent or sell housing
- Refuse to negotiate for housing
- Make housing unavailable
- Deny a dwelling
- Set different terms, conditions or privileges for sale or rental of a dwelling
- Provide different housing services or facilities
- Falsely deny that housing is available for inspection, sale, or rental
- For profit, persuade owners to sell or rent (blockbusting) or
- Deny anyone access to or membership in a facility or service (such as a multiple listing service) related to the sale or rental of housing.

In Mortgage Lending: No one may take any of the following actions based on race, color, national origin, religion, sex, familial status or handicap (disability):

- Refuse to make a mortgage loan
- Refuse to provide information regarding loans
- Impose different terms or conditions on a loan, such as different interest rates, points, or fees
- Discriminate in appraising property
- Refuse to purchase a loan or
- Set different terms or conditions for purchasing a loan.

In Addition: It is illegal for anyone to:

- Threaten, coerce, intimidate or interfere with anyone exercising a fair housing right or assisting others who exercise that right
- Advertise or make any statement that indicates a limitation or preference based on race, color, national origin, religion, sex, familial status, or handicap. This prohibition against discriminatory advertising applies to single-family and owner-occupied housing that is otherwise exempt from the Fair Housing Act.

Figure 11: Page 5 of www.FairHousingAwareness.org


About | Background | Our Story | Fair Housing Law | Fair Housing Resources

National Resources

Federal Dept. of Housing and Urban Development (HUD):

http://portal.hud.gov/hudportal/HUD?src=/program_offices/fair_housing_equal_opp

Fair Housing Alliance: http://nationalfairhousing.org/

Fair Housing Accessibility First: http://www.fairhousingfirst.org/index.asp

Local Fair Housing Agencies

Atlanta – Metro Fair Housing: http://metrofairhousing.com/

Baltimore Housing: http://www.baltimorehousing.org/fair_housing

Boston: Boston Fair Housing Commission: http://www.cityofboston.gov/fairhousing/fairhousing/

Chicago Fair Housing Alliance: http://cafha.net/

Cleveland – The Housing Center: Housing Research and Action Center: http://www.thehousingcenter.org/

Dallas / Fort Worth – North Texas Fair Housing Center: http://www.northtexasfairhousing.org/index.php

Detroit – Fair Housing Detroit: http://www.fairhousingdetroit.org/

Denver: Denver Metro Fair Housing: http://www.dmfhc.org/

Houston – Greater Houston Fair Housing Center: http://www.houstonfairhousing.org/

Las Vegas – Nevada Equal Rights Commission: http://nvdetr.org/nerc.htm

Los Angeles – The Housing Rights Center: http://www.hrc-la.org/default.asp?id=6

Miami: Miami-Dade Office of Human Rights: http://www.miamidade.gov/humanrights/home.asp

Milwaukee – Wisconsin Fair Housing: http://fairhousingwisconsin.com/

Table 20: Inquiries by Race and Ethnicity:

Race / Ethnicity	Treatment	Control
Arab	130	145
Black	138	168
Latina	113	117
White	134	167
Total	515	597

Table 21: Inquiries by Gender

	Treatment	Control
FEMALES		
Arab	64	67
Black	60	98
Latina	61	50
White	53	84
MALES		
Arab	66	78
Black	78	70
Latino	52	67
White	81	83

Table 22: Inquiries by Protected Class

	Treatment	Control
Female	207	247
Single Parent	165	163
Gay or Lesbian	68	100
Christian	44	67
Muslim	49	62
Protected Class	533	639

Table 23: Inquiries by Name

Female Names	Treatment	Control	Total	Race
Farah Abbas	22	20	42	Arab
Fatima Toosi	20	30	50	Arab
Sefa Ardakani	22	17	39	Arab
Aaliyah Washington	18	37	55	Black
Destiny Johnson	23	22	45	Black
Latoya Parker	19	39	58	Black
Alejandra Rodriguez	18	15	33	Latina
Jasmin Ruiz	20	11	31	Latina
Luz Gonzales	23	24	47	Latina
Amy Baker	23	32	55	White
Katherine McCarthy	13	22	35	White
Sarah O'Brien	17	30	47	White
Total	238	299	537	

Male Names	Treatment	Control	Total	Race
Abdul Abbas	20	24	44	Arab
Mohammed Toosi	26	30	56	Arab
Saied Ardakani	20	24	44	Arab
Darnell Washington	25	18	43	Black
Jermaine Parker	31	26	57	Black
Leroy Robinson	22	26	48	Black
Carlos Ruiz	23	24	47	Latino
Jose Gonzales	19	20	39	Latino
Juan Rodriguez	10	23	33	Latino
Bob McCarthy	23	29	52	White
Johnny O'Brien	35	26	61	White
Len Baker	23	28	51	White
Total	277	298	575	

Table 24: Treatment and Control Group Unit Traits

Treatment Group

Variable	Obs	Mean	Std. Dev.	Min	Max
Bedrooms	472	2.24	1.072	1	8
Baths	463	1.69	0.824	1	6
apartment	515	0.65	0.478	0	1
house	515	0.21	0.409	0	1
duplex	515	0.01	0.107	0	1
condo	515	0.08	0.265	0	1
sqft	360	1,293.14	811.699	1	6812
Price	510	1,541.14	1058.797	100	8824

Control Group

Variable	Obs	Mean	Std. Dev.	Min	Max
Bedrooms	538	2.06	0.995	1	6
Baths	529	1.56	0.731	1	6
apartment	597	0.65	0.477	0	1
house	597	0.21	0.408	0	1
duplex	597	0.02	0.135	0	1
condo	597	0.07	0.253	0	1
sqft	423	1,188.26	778.872	200	8730
Price	593	1,526.11	1031.208	165	10,000

Table 25: Inquiries by City

City	Treatment	Control
Atlanta	47	42
Boston	19	26
Chicago	29	31
Dallas	10	13
Denver	19	14
Detroit	26	21
Houston	34	33
Los Angeles	24	37
Miami	29	27
Minneapolis	31	57
New York	4	4
Philadelphia	30	29
Phoenix	16	23
Riverside	15	16
San Diego	48	49
San Francisco	30	39
Seattle	29	41
St. Louis	19	28
Tampa	12	16
Washington D.C.	44	51
Total	515	597

Chapter 3

Assessing the Effects of Place-Based Scholarships on Urban Revitalization: The Case of Say Yes to Education

Introduction

Place-based scholarship programs award grants for college tuition based on residence in a specific school district or city rather than merit or need. The Kalamazoo Promise is frequently identified as the first, major place-based scholarship program, and since it was announced in 2005, place-based scholarship programs have been established in over 20 cities and districts across the country.[22] The scholarship programs are sometimes accompanied by educational supports for students and serve as a catalyst for community-wide efforts to improve schools.

Like other financial aid programs, place-based scholarships seek to improve college access among groups that are underrepresented in higher education. Unlike other financial aid programs, however, place-based scholarships often explicitly include local community development goals. These programs have typically been established in central cities that have high rates of poverty and that have experienced economic decline. By offering generous educational benefits to residents, the scholarships may create an important locational advantage to these areas in efforts to attract new residents and businesses. Thus, these programs are often promoted as a potential catalyst for local economic development (Miller-Adams, 2015).

This article examines the impacts of a prominent place-based scholarship program, Say Yes to Education. Specifically, we examine changes in school enrollments and housing prices following the initiation of the Say Yes to Education program in Syracuse, New York in 2008 and in

[22] See the list compiled by Michelle Miller-Adams at the Upjohn Institute, http://www.upjohn.org/sites/default/files/promise/Lumina/Promisescholarshipprograms.pdf?_ga=1.266472014.1147411544.1394049270. The list includes 21 district-wide programs, that provide substantial funding for multiple years that can be used at a range of colleges state- or nation-wide. At least two of those programs, New Haven Promise and Hartford Promise, use significant merit-based criteria as well as residency requirements in making awards, and some of these programs, such as Denver's, combine place-based targeting with financial need criteria.

Buffalo, New York in 2012. Changes in enrollments and housing prices are indicators of the extent to which these cities are attracting or retaining residents, which is the primary mechanism through which place-based scholarship programs may spark community revitalization.

Existing studies of the effects of place-based scholarships on enrollments and housing prices have typically focused on a single program in a single location, such as the Kalamazoo Promise, which limits the ability to draw more general conclusions or to examine the heterogeneity of effects across locations. Also, the time frame and research designs of previous studies make it difficult to separate the effects of the programs from the effects of the Great Recession. Adoption of placed-based scholarships at two different discrete points in time in different locations allows us to provide informative analysis that exploit discontinuities in trends, and help to develop a broader perspective on the potential effects of place-based scholarships.

Examining district enrollment from 2000 through 2014, we find that after years of steady declines in enrollments, both Syracuse and Buffalo saw enrollment increases that coincide with the adoption of the Say Yes to Education program. These increases occurred at different points in time in each city. Over the same post-treatment periods, enrollments continued to decline in the suburbs surrounding these cities and in similar upstate New York city school districts without the program, suggesting that these increases were city-specific and not due to broader developments affecting the region. The fact that enrollment increases are discernible in Buffalo as well as Syracuse, and relative to other nearby city school districts makes it unlikely that enrollment increases can be attributed to the Great Recession. Supplementary analyses show that the enrollment increases in Syracuse public schools coincided with declines in enrollment in nearby suburbs, while increases in enrollment in Buffalo public schools coincided with decreases in private school enrollments in the Buffalo area. Moreover, in each location, enrollment increases during the post-Say Yes period were concentrated in the district's higher performing schools.

To isolate the impact of Say Yes on housing prices, we use a panel data set on individual home sales to estimate hedonic price models that control for neighborhood fixed effects and trends, as well as analyses that make use of repeat sales, and which compare housing price changes across similar upstate New York cities. The results provide some evidence that increases in housing prices accompanied the adoption of Say Yes in Syracuse, but not in Buffalo. These results are consistent with findings that enrollment growth in Buffalo may have been driven by students who would otherwise have attended private schools, while enrollment growth in Syracuse may have been driven by students who would otherwise have attended school in the surrounding suburbs.

The results suggest that the ability of place-based scholarships to attract residents into a central city is likely to depend on both the specific provisions of the program and the context in which it is implemented, and in the concluding section of this article, we discuss the implications for policy and future research. The remainder of the paper is organized as follows. Section II provides background on the Say Yes to Education program, placed-based scholarships more generally, and the existing research on these programs. Section III describes the data used in our analyses. Section IV presents our analysis of enrollments, and Section V lays out our analysis of housing market values. The final section of the paper discusses the implications of our findings.

Background on Say Yes to Education and Place-Based Scholarships

Say Yes to Education combines "place-based" college scholarships with intensive student supports. Former United States Secretary of Education Arne Duncan, among others, has described the program as a potential model for reviving public schools and spurring economic revitalization in the nation's declining central cities (Mariani, 2009).

The first district-wide Say Yes initiative was announced in Syracuse, New York in 2008 as a partnership between the Say Yes to Education Foundation, the Syracuse City School District (SCSD) and Syracuse University. In 2012, Buffalo, New York became the second school district to adopt the Say Yes program. The programs in Syracuse and Buffalo share many important features. First, neither program places merit-based restrictions on scholarship awards; all students who have attended for specified lengths of time, have graduated from the cities' public schools, and who have been admitted to and maintain good standing in college are eligible for the award. Second, students from both cities can use the scholarships at the same expansive set of colleges that includes all community colleges, all public four-year colleges and universities in New York State, and approximately 90 private institutions across the country ranging from Ivy League universities to small liberal arts colleges. All students are eligible for free tuition at any public university in New York State; for most private institutions, free tuition is limited to students with family incomes under $75,000. Students from families with income greater than $75,000 who attend a private university are, however, eligible for $5,000 scholarships. Third, the scholarship is a last-dollar scholarship; students must apply for any state and federal financial aid they are eligible to receive, and Say Yes pays the difference between the amount of aid they receive and tuition. Finally, as part of each initiative, the Say Yes foundation has provided staff and funding to promote the development of student support services, after- and summer-school programs, and school improvement efforts.

There are differences in program details between the two sites. In Syracuse, scholarships cover 100 percent of tuition for any city resident who has attended a public school in the city for at least three years prior to graduation. The Buffalo program requires twelve years of attendance in the Buffalo Public Schools (BPS) for full tuition, and provides partial tuition scholarships for students spending less than twelve years in the BPS. Students entering the BPS in grades 1, 2 and 3 are eligible for 95 percent scholarships, those entering grades 4, 5 and 6 are eligible for 80 percent scholarships and those entering in grades 7, 8 and 9 are eligible for 65 percent scholarships. Also, students in Syracuse are eligible for full-tuition scholarships at Syracuse University regardless of family income, while students in Buffalo have a $100,000 income cap for Syracuse University. The Say Yes foundation works with each district to identify student needs and community priorities, and then coordinates with the district, other local governments, and community groups to fill service gaps. Consequently, the set of student support services and school improvement efforts that Say Yes supports differs across the two cities.

In a review of "place-based" scholarship programs, Miller-Adams (2015) emphasizes that programs across the country differ in potentially important features—including eligibility requirements, participating colleges and universities, and accompanying initiatives. The Say Yes programs in Syracuse and Buffalo are similar to the Kalamazoo Promise program in that they place minimal restrictions on scholarship eligibility. This feature distinguishes what Miller-Adams calls "universal programs" from the programs in Pittsburgh, New Haven, and Hartford that have more restrictive GPA and attendance requirements. Also, many programs provide scholarships for a single local college or a very limited set of local colleges. Even some of the more expansive programs, including the Kalamazoo Promise, are limited to in-state, public colleges and universities. In contrast, Say Yes students can receive scholarships at a wide range of private institutions across the country as well as in-state public colleges and universities. Finally, while the introduction of place-based scholarships often spurs school improvement efforts and community-wide action to improve student and family supports, such efforts are more explicitly part of the Say Yes intervention than in some other locations. Despite the differences across place-based scholarship programs, Say Yes to Education shares with these other programs the goals of increasing college access, building a college-going culture, and spurring community development by offering well publicized scholarships based primarily on place of residence and a credible guarantee that scholarship offers will continue long-term (Miller-Adams, 2015).

An element of place-based scholarships that distinguishes them from other financial aid programs is the explicit goal of promoting local economic development. The residency requirements of place-based scholarships are "widely interpreted as a strategy to draw families into the area's urban core and retain those already there" (Miller-Adams, 2015). Changes in district enrollments and housing values can be viewed as indicators of whether place-based scholarship programs are helping to promote this goal.

Most of the evidence on the impacts of place-based scholarships on enrollments and housing values come from evaluations of the Kalamazoo Promise program. Initial analyses found that enrollment in the Kalamazoo public schools increased by 12 percent in the two years immediately following announcement of the Promise program, after falling by over five percent in the three years immediately preceding the announcement (Miron and Cullen, 2008). Bartik, Eberts, and Huang (2010) estimated that enrollments in Kalamazoo in 2009 were nearly 25 percent higher than what they were projected to be in the absence of the program. These authors also find that the Promise stabilized the racial-ethnic composition in the district by stemming decades of white-flight. Later work finds that the majority of new students in Kalamazoo came from other Michigan districts, and most markedly from one adjacent, relatively high poverty suburban district (Hershbein, 2013). Evidence from Pittsburgh and El Dorado, Arkansas also found that after many years of steady decline, public school enrollment stabilized following the announcement of the Pittsburgh Promise (Ash and Ritter 2014; Gonzalez, Bozick, Tharp-Taylor, and Phillips 2011; Iriti, Bickel, and Kaufman 2012). Miller (2011) uses nine years of data on home sales in the county and finds no evidence that the Promise increased home values in Kalamazoo despite the positive impacts on enrollment and other school characteristics.

In an unpublished paper, LeGower and Walsh (2014) estimate enrollment and housing value effects across multiple place-based scholarship programs. They find that place-based scholarships have been associated with increases in public school enrollments and increases in housing prices relative to their surrounding suburbs.[23] Comparison with surrounding suburban communities, however, is potentially problematic for two reasons. First, within a metropolitan area, enrollments and housing prices in the central city and suburban areas may move in opposite directions, making the suburbs a questionable basis for estimating the counterfactual enrollments and/or housing prices for the central city.[24] Second, if a place-based scholarship serves to draw families into the city who would otherwise choose the surrounding suburbs, then enrollment and housing prices in the suburbs may themselves be influenced by the treatment. In addition, given the time frame of the analysis conducted by LeGower and Walsh (2014), estimated effects of place-based college scholarships may be confounded with the effects of the Great Recession.

Our analyses add to the existing literature in at least three ways. First, we are able to estimate the effects of place-based scholarship programs that are similar in important details but implemented in two different places at two distinct points in time. By focusing on two sites, we are able to examine possible effects of important institutional details of the program. For example, while

[23] Syracuse is included in both the samples used to analyze enrollments and housing values, but Buffalo is not.
[24] Below we show evidence that the trends in housing prices in the Buffalo and Syracuse differ substantially from those in their surrounding suburbs in the years preceding the announcement of Say Yes.

the programs are largely similar in the two cities, Syracuse's program offers easier eligibility for full tuition and free tuition at the city's largest private university. The two-site focus also allows us to examine important contextual factors specific to the two cities, such as changes in the private school sector, that could affect our findings.

Second, our analyses focus on, arguably, the most expansive and generous place-based scholarship program and one of the few to include a broad range of elite private higher education institutions. The Syracuse program, in particular, sets a very low bar for eligibility (attendance in the city schools for grades 10–12) and includes the city's major private university.

Third, our strategy for estimating the program's effects on housing prices differs from that of previous work on place-based scholarships, such as LeGower and Walsh (2014) in several important regards. Specifically, our analysis of housing prices includes controls for neighborhood-specific trends, allowing us to more precisely identify post-program deviations from pre-existing trends. We find that inclusion of these trends matters—estimates that do not control for neighborhood trends differ substantively from those that do (see Appendix, Table A1). Also, we compare enrollment and housing trends to cities drawn from very similar metropolitan areas rather than suburban districts in the same metropolitan area. Given that place-based scholarships may draw families from surrounding suburban districts, as discussed more fully below, these suburban districts cannot be considered "untreated" for the purposes of estimating counterfactuals. Moreover, by using cities in nearby metropolitan areas and by examining Syracuse and Buffalo separately, we have more leverage to distinguish the effects of Say Yes from those of the Great Recession than analyses based primarily on programs that coincide with the onset of the Great Recession. Finally, we test the appropriateness of the comparisons we use and the robustness of our estimates using the synthetic control method.

Data

The enrollment data used in our analyses are from the New York State School Report Cards, which report enrollments by grade, ethnicity, and eligibility for free- and reduced-price-lunch, English as a second language, and special education services from the fall of 2000 to the fall of 2014. We augment district enrollment counts provided by the School Report Cards with counts of students residing in each district who attend charter schools, which we obtained by request from the New York State Education Department.

In addition to examining enrollment trends in Buffalo and Syracuse, we examine concurrent enrollment trends in other public and private schools in the metropolitan areas surrounding these cities using data on private school enrollments from the New York State Education Department.[25] Finally, we also examine trends in Rochester, New York, and its surrounding metropolitan area. Rochester is a district located between Buffalo and Syracuse (less than 90 miles from each) that did not implement the program but had similar student demographics and enrollment trends prior to the adoption of Say Yes.

To examine the effect of Say Yes on housing values, we use data on home sales from the New York State Office of Real Property Services (ORPS). These data include the universe of property transfers in the state of New York (excluding New York City) from 2000 through the second quarter of 2014, and include the sales price and date. We limit our sample to arms-length sales and also apply a number of filters to ensure that the data exclude extreme outliers and include only valid sales of single residence homes. These files also include property addresses that we use to place the properties in Census block. Finally, we link each property to tax assessment files, also provided by ORPS, which have a wide range of information on property

[25] http://www.p12.nysed.gov/irs/statistics/nonpublic/home.html

characteristics. Because we have sales data for the entire state, we are also able to compare changes in Syracuse and Buffalo to changes in other school districts in the surrounding metropolitan areas and to changes in Rochester and its surrounding districts.

Enrollment Analysis

Figure 12 shows public school enrollment trends for Syracuse, Buffalo, and Rochester, and their surrounding suburbs. In each case, enrollment counts include students who reside in the district boundaries and who attend either district-run schools or charter schools, as students in both types of public school are eligible for Say Yes. Enrollment counts are normalized so that average enrollment in the district (or the set of districts in the cases of the suburban time-series) across all years equals 100.[26]

As shown in Figure 12, after years of declines, averaging 1.4 percent annually over the eight years preceding the announcement of Say Yes in Syracuse, enrollments leveled off in the first year and then increased in the second year after Say Yes began. Enrollment in Syracuse dropped by 1,376 students, 6.3 percent, in the three years preceding Say Yes, but increased by 397 students, 1.9 percent, in the three years following the announcement of Say Yes. Enrollment was still 908 students higher seven years after Say Yes than in the last year prior to Say Yes. In contrast, enrollments in the suburbs surrounding Syracuse continued on their pre-existing downward trend following the adoption of Say Yes in the city.

Enrollment patterns in Buffalo are similar to those in Syracuse. After years of declines that averaged 1.7 percent annually, enrollment increased by an average of 1.4 percent annually in the first three years following the adoption of Say Yes. Over the same period, enrollments in the

[26] Specifically, the average enrollment across all years for the district (or set of districts) is subtracted from the enrollment count in each year, divided by the average enrollment and multiplied by 100.

suburban districts around Buffalo continued to decline.

Public school enrollments in Rochester, in contrast, do not show clear changes that coincide with the adoptions of Say Yes in Syracuse or Buffalo. In the four years following the announcement of Say Yes in Syracuse, enrollments in Rochester fell by 220 students, or 0.7 percent. In the three years following the announcement of Say Yes in Buffalo, enrollments in Rochester declined by 452, or 1.4 percent.

Estimation Methods

To estimate the changes in enrollments associated with the adoption of Say Yes, we employ three analyses. The first uses enrollment data solely from the district that adopted Say Yes to estimate the following model:

$$\ln \boldsymbol{Y}_t = \boldsymbol{\beta}_0 + \boldsymbol{\beta}_1 \boldsymbol{D}1_t + \boldsymbol{\beta}_2 \boldsymbol{D}2_t + \boldsymbol{\beta}_3 \boldsymbol{D}3_t + \boldsymbol{\beta}_4 \boldsymbol{T}_t + \boldsymbol{\varepsilon}_t, \tag{1}$$

where $\ln \boldsymbol{Y}_t$ is the natural log of enrollment in year *t*;[27] $\boldsymbol{\beta}_0 + \boldsymbol{\beta}_4 \boldsymbol{T}_t$ represents the intercept and slope of the linear enrollment trend in the district; and $\boldsymbol{D}1_t, \boldsymbol{D}2_t,$ and $\boldsymbol{D}3_t$ indicate the first, second, and third year, respectively, after the announcement of Say Yes in the district. We limit the sample to three years after the announcement of Say Yes, so these dummy variables are exhaustive of the post-period.[28] This model uses pre-treatment enrollment counts to fit a trend line, projects that trend into the post-period, and then $\boldsymbol{\beta}_1, \boldsymbol{\beta}_2,$ and $\boldsymbol{\beta}_3$ measure the difference between observed and projected enrollment in each of the post-Say Yes years. Because the outcome variable is the log of enrollment, the coefficient estimates (multiplied by

[27] We use the natural log of raw enrollment in each of the analyses that follow, not normalized enrollment as in Figure 1.

[28] In the case of Buffalo, only three post-treatment years are available in our data. We also limit the sample to three years post-Say Yes in the Syracuse analysis for three reasons—consistency with Buffalo; it allows us to use Buffalo as a comparison for Syracuse because Buffalo was not exposed to Say Yes for the first three years after the announcement of Say Yes in Syracuse; and the further into the post-treatment period that a pre-treatment trend is projected, the more potential bias there is in impact estimates due to misspecification of the trend and because there are more intervening events that complicate interpretation of deviations from trend.

100) can be interpreted as percent increase in enrollment associated with Say Yes.

In the second analysis, we add control districts that have not been exposed to the treatment during the period observed. When analyzing changes in enrollment associated with Say Yes in Syracuse we add Buffalo and Rochester to the sample. When analyzing changes associated with Say Yes in Buffalo we add Rochester. Using these samples, we estimate the following model:

$$\ln \boldsymbol{Y}_{it} = \boldsymbol{\beta}_0 + \boldsymbol{\beta}_1 \boldsymbol{D}1_{it} + \boldsymbol{\beta}_2 \boldsymbol{D}2_{it} + \boldsymbol{\beta}_3 \boldsymbol{D}3_{it} + \boldsymbol{\phi}_{1i} + \boldsymbol{\phi}_{2i} \boldsymbol{T}_t + \boldsymbol{\gamma}_t + \boldsymbol{\varepsilon}_{it}. \tag{2}$$

In this model, $\boldsymbol{\beta}_0 + \boldsymbol{\phi}_{1i} + \boldsymbol{\phi}_{2i} \boldsymbol{T}$ capture the intercept and slope of each district-specific trend line, and $\boldsymbol{\gamma}_t$ captures year-specific enrollment shocks that are common across treatment and comparison districts. The estimates of $\boldsymbol{\beta}_1, \boldsymbol{\beta}_2,$ and $\boldsymbol{\beta}_3$ in this model can be interpreted as difference-in-differences estimates. Specifically, they capture the difference between the deviation from projected trends in the Say Yes district in each post-Say Yes year and the deviation from projected trends in the other large city districts in western New York that had not (yet) adopted Say Yes.

The difference-in-differences estimates effectively control for any factors or events that might have influenced enrollments in the cities of Syracuse, Buffalo and Rochester similarly. The difference-in-differences estimates do not necessarily control for factors that had a unique influence on enrollments in a particular metropolitan area during the post-Say Yes period, however. Thus, as a robustness check, we implement an alternative estimation strategy that controls for any metropolitan-specific shocks that influence all districts in a metropolitan area equally. Mechanically, this alternative procedure is computed in the manner of a triple-differences estimator, which compares the difference between deviations from trends in the treated district and its surrounding suburbs to the similar differences between the central city and the suburban districts in the comparison metropolitan area(s). This estimator has the advantage of controlling for metropolitan-specific shocks that might coincide with the adoption of Say Yes. If Say Yes affected enrollments in the suburbs surrounding the city with the program, however, then this triple-differences estimate cannot be interpreted as the increase in enrollment in the Say Yes district that resulted from Say Yes. Rather, it should be interpreted as an indicator of whether or not Say Yes may have contributed to a divergence (or convergence) in enrollments between the city where it was adopted and its surrounding suburbs.

Specifically, we use enrollments in the treated and comparison districts, and their surrounding suburbs to estimate equation (3).

$$\begin{aligned}\ln \boldsymbol{Y}_{dct} &= \boldsymbol{\alpha}_0 + \boldsymbol{\alpha}_1(\boldsymbol{D}1 \times \boldsymbol{City} \times \boldsymbol{Treated})_{dct} + \boldsymbol{\alpha}_2(\boldsymbol{D}2 \times \boldsymbol{City} \times \boldsymbol{Treated})_{dct} \\ &+ \boldsymbol{\alpha}_3(\boldsymbol{D}3 \times \boldsymbol{City} \times \boldsymbol{Treated})_{dct} + \boldsymbol{\beta}_1(\boldsymbol{D}1 \times \boldsymbol{City})_{dct} + \boldsymbol{\beta}_2(\boldsymbol{D}2 \times \boldsymbol{City})_{dct} \\ &+ \boldsymbol{\beta}_3(\boldsymbol{D}3 \times \boldsymbol{City})_{dct} + \boldsymbol{\gamma}_d + \boldsymbol{\varphi}_d \boldsymbol{T}_t + \boldsymbol{\eta}_{ct} + \boldsymbol{\varepsilon}_{dct},\end{aligned} \tag{3}$$

where ln $\boldsymbol{Y}_{dct}$ is the log of enrollment in district *d* in metro area *c* in year *t*. For suburban districts, we sum enrollment in each year across all the suburban districts in the metropolitan area surrounding a particular city district and treat that as a single district.[29] $\boldsymbol{D}1_t, \boldsymbol{D}2_t,$ and $\boldsymbol{D}3_t$ represent the first, second, and third year after the announcement of Say Yes, *City* indicates the central city of the metropolitan area (Buffalo, Rochester, or Syracuse), and *Treated* indicates the district where Say Yes is adopted. $\boldsymbol{\beta}_i$ is the difference in the deviation from pre-Say Yes trends in the central city district and the suburban districts in the comparison metropolitan area(s)

[29] Grouping suburban districts as a single district is intended as a conservative inference approach that does not inflate sample size.

during post-Say Yes year i, and α_i is the triple-differences estimate of the effect of Say Yes on enrollments. $\gamma_d + \varphi_d T$ controls for district-specific trends and η_{ct} is a metropolitan area-by-year fixed effect.[30]

To test the validity of using Buffalo and Rochester as control districts we also conducted synthetic control analyses.[31] Because the regression-based analyses allow for a range of sensitivity and supplementary analysis, we focus our discussion on those results. The full synthetic control analyses are described and the results are reported in an Appendix.

All models are estimated using eight and four years of pre-Say Yes data, as well as three years of post-Say Yes data. The expansion of charter schools was most rapid during the 2000 to 2005 period, particularly in Buffalo, and charter school expansion may serve to draw students from private schools into the public schools.[32] As a result, the estimate of pre-treatment trends based on eight years of data might underestimate the declines in enrollments due to underlying economic and demographic factors, which can distort estimates of the effects of Say Yes. For this reason, we believe the estimates using four-years of pre-Say Yes data may provide a more valid estimate of counterfactual enrollments.

Results

Table 26 presents the results of our estimations for Syracuse and Buffalo. In keeping with the graphical depiction in Figure 12, the first and fourth columns of the top panel of Table 26

[30] In the difference-in-differences analysis, equation (2), the sample includes only one district in each metropolitan area and so the year fixed effect in equation (2) is equivalent to the metropolitan area-by-year fixed-effect used in this analysis.
[31] We thank an anonymous reviewer for this suggestion.
[32] Between 2000 and 2005, the percent of public school students residing in the district who attended charter schools increased from 0.2 to 13.3 in Buffalo, from 0 to 6.1 in Rochester, and from 0 to 4.9 in Syracuse. As of 2014, the percent of public school students residing in the district who attended charter school was 18.9 in Buffalo, 12.2 in Rochester, and 6.7 in Syracuse. Students can attend charter schools in Syracuse and Buffalo if they live in the suburbs. However, the counts of charter school students that we include in our district enrollment figures are counts of district residents who attend charter schools, not the number of students in charter schools located in the city. Charter school students who reside in Syracuse and Buffalo are eligible for the Say Yes scholarship.

indicate that enrollments in Syracuse during the post-Say Yes years are higher than predicted by the pre-Say Yes trend. Specifically, three years after the announcement of Say Yes, enrollments are approximately 8.6 percent higher than projected when four-years of pre-Say Yes observations are used, and 4.1 percent higher when eight-years of pre-Say Yes data are used. The estimates are somewhat imprecise though and the estimated increases in enrollments are only marginally statistically significant.

The estimates in columns (1) and (4) of Table 26 quantify the deviations from pre-treatment trends evident in Figure 12. Of course, any non-linear shock that may have coincided with the adoption of Say Yes and influenced enrollment could provide an alternative explanation for the observed deviations from trend. To begin ruling out potential alternative explanations, we compare deviations in trend in the Say Yes district to those in comparison districts. When the enrollment increases associated with Say Yes in Syracuse are estimated using the difference-in-differences framework of equation (2), as well as the triple-differences framework of equation (3), the estimated increases in enrollment associated with Say Yes are smaller. These smaller estimated enrollment increases reflect the fact that the rate of enrollment declines in Buffalo and Rochester also slowed during years following the recession of 2008, which coincides with the post-Say Yes period for Syracuse. Unlike in Syracuse, however, neither Buffalo nor Rochester saw actual increases in enrollments during this period, and both the difference-in-differences and triple-differences estimates do show that enrollment increases relative to prior trends during the post-Say Yes period were larger in Syracuse than in Buffalo and Rochester. These difference-in-differences and triple-differences estimates are, however, not reliably different from zero. The results of synthetic control analyses, presented in Appendix B, indicate larger enrollment effects for Syracuse—an increase of approximately 4.4 percent—than either the difference-in-differences or the triple-differences model.

In the case of Buffalo (in the bottom panel of Table 26), estimated increases in enrollment are 6.4 percent or 7.5 percent higher than projected trends, depending on whether four or eight years of pre-Say Yes observations are used. The enrollment increases associated with Say Yes are a bit more precisely estimated in the case of Buffalo and are statistically significant at conventional levels in the sample with eight years of pre-Say Yes observations. For Buffalo, the difference-in-differences and triple-differences estimates also indicate that post-Say Yes enrollments increased more relative to prior trends than they did in Rochester during the same period. The estimated increases in enrollment three years after the adoption of Say Yes in Buffalo are approximately 7 to 8 percent when four years of pre-Say Yes data are included and approximately 6 percent when eight years of pre-Say Yes data are used, and the estimated differences are mostly statistically significant. [33]

These estimates are in line with other evaluations of Promise-type programs, such as LeGower and Walsh (2014) who find an approximately 8 percent increase in enrollment for universal Promise programs such as Say Yes, and the 9 percent increase above expected trends in El Dorado, Arkansas reported by Ash and Ritter (2014) for the El Dorado Promise. The increase is somewhat smaller than the estimate of between an 8 and 25 percent enrollment increase in Kalamazoo, depending on the assumptions used in the model (Bartik, Eberts and Huang, 2010).[34]

[33] Because Buffalo is much larger than any other district in our sample, the synthetic control method primarily uses Rochester to create the synthetic control for Buffalo and is not appreciably different from the difference-in-difference model. We present synthetic control models for both Syracuse and Buffalo in the housing results below.
[34] Note that the LeGower and Walsh (2014) estimates use a comparison group of surrounding districts while the Kalamazoo and El Dorado estimates are based on pre-post comparisons.

Identifying the Source of Enrollment Increases

If the Say Yes program is drawing new students to city schools, there are three plausible sources of these students: schools outside the region, other public schools in the region, and private schools. Although we cannot track migration of individual students, we can compare enrollment trends across different types of schools in each region. Specifically, for both Syracuse and Buffalo, we estimate a version of equation (1) separately for four different sets of schools: the central city (Say Yes) district, the adjacent, inner ring suburban public school districts, outer ring suburban public school districts in the same county, and private schools. In these estimates, we use enrollment counts rather than the log of enrollment, so that we can interpret the estimated coefficients on the post-Say Yes variables as changes in enrollment counts relative to projected trends. Table 27 presents the results for Syracuse and Buffalo, respectively. For each analysis we use four years prior to the announcement of Say Yes in the focal district to extrapolate trends.[35]

Table 27 indicates that increases in enrollments above projected trends in Syracuse during the Say Yes period were accompanied by decreases in enrollments relative to projected trends in the suburban districts around Syracuse. In contrast, enrollment increases relative to projected trends in Buffalo were accompanied by decreasing enrollments in private schools in the area, relative to pre-existing trends. These results suggest that in Syracuse, the enrollment increases that followed the announcement of Say Yes may have been driven by students who otherwise would have enrolled in the nearby suburbs, while enrollment increases in Buffalo may have come primarily from students who otherwise would have enrolled in private schools.

[35] 2014–15 private school enrollments were not available, and so only two years of post-Say Yes data are included for the Buffalo analysis. We use four years rather than eight to extrapolate trends to minimize potential effects of increases in charter schools in Syracuse, which largely occurred from four to eight years before the start of Say Yes in the city.

The Catholic Diocese of Buffalo has closed and consolidated a number of Catholic schools in the Buffalo area over the last decade, but we do not think Catholic school closures can fully explain the deviation from enrollment trends that followed the announcement of Say Yes for several reasons. First, the largest set of private school closures (12 of 103 schools) took place in 2006–07, which is before the window of data used to measure trends in Table 27. We do not observe contemporaneous increases in Buffalo Public Schools enrollment during or just before this earlier period. Second, the number of private schools was steady in the first two years immediately after the announcement of Say Yes. Although the need to close Catholic schools in Buffalo was announced by the Diocese in 2011, schools were not actually closed until 2014–15 (the third year after Say Yes), and the 10 schools that were closed in 2014–15 did not show unusual or large drops in enrollment in either of the first two years after the announcement of Say Yes. While it is possible that the threat of closures led some families to choose other schools, there is no evidence to suggest they would have chosen Buffalo public schools in the absence of Say Yes.

Heterogeneity in Enrollment Changes

Table 28 shows estimated deviations from pre-Say Yes enrollment trends by race/ethnicity. Here, we estimate equation (2) substituting the log of enrollment by student race for the log of total enrollment. The coefficients compare the deviations from pre-program trends in white and non-white student enrollments in Syracuse and Buffalo with deviations from trends in each city's comparison districts. In both Syracuse and Buffalo, declines in white enrollment in the years leading up to Say Yes were particularly marked. In Syracuse, year-to-year decreases in white enrollment averaged 6.4 percent over the ten years preceding Say Yes. In the first three years following Say Yes, decreases in white enrollments slowed to an average of 2.9 percent per year.

Similarly, in Buffalo, year-to-year decreases in white enrollments averaged 4.6 percent over the ten years preceding Say Yes, and slowed to 1.5 percent per year in the three years following the announcement of Say Yes. Compared with deviation from projected trends in Rochester and Buffalo, the number of white students in Syracuse city public schools increased by over 8 percent three years after the start of Say Yes, a substantially larger increase than for non-white students, though the increase is not significant at conventional levels. As a result, the share of white students in Syracuse shows an increase, although that increases in not statistically significant. In Buffalo, there was a significant increase in white students of almost seven percent, which was similar to the increase in non-white students. Thus, the share of white students in the district did not change over the period.[36]

Finally, in Table 29, we shift from district-level to school-level analyses to examine enrollment changes by school performance levels. Specifically, we split the sample of elementary schools in each district into three groups based on average fourth grade math and English language arts test scores in the last year prior to the start of Say Yes. Then, using the sample of schools in each treatment districts and its comparison districts, we regress the log of enrollment on indicators of post-Say Yes years, school fixed effects, a school-specific time trend, and year fixed effects. Note that the sample size is larger than in the previous tables because schools are the unit of analysis.

The Say Yes scholarship offer is more valuable to families who expect to send their children to college, and particularly four-year colleges, and thus, we expect that enrollment increases in reaction to Say Yes would be concentrated in higher performing schools. Indeed, the results in

[36] We also examined changes in enrollment of students eligible for free-lunch as well as students not eligible for free-lunch. However, counts of free-lunch eligible students in both treated and untreated districts vary widely around estimated trends during both the pre-treatment and post-treatment period. As a result, estimated changes in enrollment by free-lunch eligibility were much too imprecise to be informative.

Table 29 indicate that in Syracuse increases in enrollment were limited to middle and high performing schools. In Buffalo, increases in enrollment were more marked in middle and higher performing schools in the first two years after the announcement of Say Yes, but not in the third Say Yes year. Although many other factors could be driving larger-than-usual increases in higher performing schools, these findings are consistent with the idea that Say Yes is drawing students into the district schools.

In sum, both Syracuse and Buffalo public schools saw enrollment increases relative to projected trends controlling for enrollment changes in the similar nearby cities in the three years following the announcement of Say Yes. In both districts, the increases in enrollment were concentrated in higher performing schools. The increases in enrollments relative to projected trends in Syracuse were accompanied by decreases in enrollment relative to the projected trends in the nearby suburbs, and the increases in enrollments relative to projected trends in Buffalo were accompanied by unusually large decreases in private school enrollments in the area. There is some reason to believe that the increase in enrollments following Say Yes in Syracuse were the result of more general changes in the mobility of students across the city and the suburbs in western and central New York during the years following the Great Recession. It is, however, unlikely that private school closure can account for the shift of enrollments from private schools to the Buffalo city public schools.

Housing Market Analysis

One might suspect that increases in enrollments, particularly if those enrollments are drawn from surrounding districts, would be accompanied by increased demand for housing and thus, increased housing prices in the central cities. Figure 13 presents a time-series of median residential housing sales prices in Syracuse, Buffalo, Rochester, and their surrounding suburbs.

Median sale prices are normalized to the average median sales price over all the years within each time-series.

In Syracuse, median home sales prices did, in fact, increase substantially above previous trends in the first three years following the adoption of Say Yes, while average prices in the suburbs surrounding Syracuse dropped below prior trends over the same period. These changes in trends are consistent with people relocating from the suburbs to the central city. A similar pattern in housing prices in the central city and its surrounding suburbs is, however, evident for Rochester and Buffalo during this period, which suggests that increases in home sales prices in Syracuse in the years following the announcement of Say Yes may reflect general changes in metropolitan housing markets in western and central New York during the Great Recession, rather than any impact of Say Yes. There is no indication of any increases in median home sales prices following the announcement of Say Yes in Buffalo in Figure 13. In fact, median home sales prices dropped steeply in the years immediately following the announcement of Say Yes, although that decline began the year prior to the announcement of Say Yes.

Of course, the simple time-series of median home prices reflects a wide range of factors. For example, the sample of homes sold in a district changes each year, and so changes in median sale prices reflect changes in the types of homes being sold as well as changes in the prices of individual houses. In this section, we use hedonic housing price models, estimated using individual home sales, to try to isolate changes in housing values associated with the adoption of Say Yes.

Estimation Methods

To estimate the increase in housing values associated with Say Yes, we employ a difference-in-differences approach comparing deviations from pre-existing trends in house values in the Say

Yes districts to deviations from trends in comparison districts during the same period. Figure 13 suggests pre-Say Yes trends in housing values in Syracuse were similar to those in Buffalo and Rochester, and the three cities are also similar in terms of socioeconomic and demographic characteristics, the age of their housing stock, and their role in their larger metropolitan economies. Thus, Buffalo and Rochester are appropriate comparisons for Syracuse. For similar reasons, Rochester is an appropriate comparison for Buffalo.

Housing values in the suburbs surrounding Syracuse and Buffalo exhibit different trends than those in the cities in the years leading up to Say Yes, thus they may not be appropriate comparison districts. Also, if Say Yes attracts families to Syracuse and Buffalo who might otherwise choose to live in the nearby suburbs, then the Say Yes program could influence housing prices in those suburbs as well, again making it an inappropriate comparison group. Nevertheless, the pre-Say Yes trends in housing values in the suburban areas of Syracuse, Buffalo, and Rochester are all similar to each other. We exploit this fact to implement an alternative estimation strategy discussed below.

We implement our difference-in-differences estimator in two different ways. First, we use all home sales in the treated and comparison districts for the eight years prior to the adoption of Say Yes and the three years following the adoption of Say Yes[37] to estimate a regression that controls for neighborhood fixed effects and trends as well as individual housing characteristics.[38] Specifically, we estimate the following regression:

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[37] For Syracuse, we use Rochester and Buffalo as the comparison districts, and for Buffalo, we use Rochester as the comparison. We also computed estimates using four years of pre-Say Yes observations, and the results were substantively very similar, although less precise. In contrast to the enrollment analysis, we do not expect the increases in charter schools in Syracuse that occurred four to eight years before the start of Say Yes to affect housing prices. We, therefore, report the results using eight years of pre-Say Yes data here. The results using four years of pre-Say Yes data are reported in Appendix Table A2.

[38] Housing characteristics included as controls are square feet of living area, square feet of garage and basement, overall condition, age of home, number of stories, number of rooms, number of bedrooms, number of full bathrooms, number of half bathrooms, whether or not there is a finished recreational room, whether or not the house has central air conditioning, and heat type.

$$\ln P_{int} = \alpha_0 + \alpha_1 D1_{nt} + \alpha_2 D2_{nt} + \alpha_3 D3_{nt} + X_{\text{int}}\Phi + \gamma_n + \varphi_n T_t + \eta_t + \varepsilon_{int}, \tag{4}$$

where the outcome variable is the log of the sales price for property *i* in census tract *n* in year *t*; the treatment variables are defined as they were in the enrollment analysis; ***X*** is a vector of housing characteristics; $\gamma_n + \varphi_n T_t$ are the intercept and slope of a neighborhood-specific trend,[39] and η_t is a year-specific effect. The strength of this first strategy is that it uses all housing sales in the district to estimate effects. However, it only provides adequate control for changes in the types of houses sold each year if—controlling for observed housing characteristics—homes sold within neighborhoods are sufficiently homogeneous.

The second strategy to control for changes in the types of homes sold is to limit the sample to homes that have sold multiple times, and estimate the following regression.

$$\ln P_{int} = \alpha_0 + \alpha_1 D1_{nt} + \alpha_2 D2_{nt} + \alpha_3 D3_{nt} + \lambda_i + \varphi_n T_t + \eta_t + \nu_{int}. \tag{5}$$

In this regression, we replace the neighborhood fixed effect in equation (4) with an individual property fixed effect, which is possible because each home is observed multiple times. We continue to control for neighborhood trends. Because we only observe housing characteristics at a single point in time, their effect on housing prices cannot be estimated separately from the individual property fixed effect and thus drop out of this model. Including the individual housing fixed effect provides a more complete control for changes in the types of housing sold in different years. In this model, however, the effects of Say Yes are identified by changes in the price of homes sold multiple times in a relatively short period of time, which may be unrepresentative of changes in values across all homes that are sold.

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[39] Inclusion of the neighborhood specific trend term is substantively important. Estimated effects of Say Yes on property values in both Syracuse and Buffalo are consistently and substantially more positive in models that exclude the control for neighborhood specific trends. See Appendix Table A1 for a comparison of coefficients.

Conducting proper inferences for estimates of the treatment effects in equations (4) and (5), α_i, is not straightforward. Although we observe thousands of individual home sales, these sales are clustered in three districts in the case of the estimated impact of Say Yes in Syracuse, and only two districts in the case of Say Yes in Buffalo, and in each case, there is only one treated cluster. In the presence of this type of clustering, standard error estimates that assume independent observations can be biased downward substantially. The standard solution to this problem—cluster robust standard errors—relies on having a large number of treated and comparison group clusters, which clearly is not the case here (Wooldridge, 2003, 2006; Donald and Lang, 2007; Conley and Taber, 2011; Cameron and Miller, 2015).

To conduct proper inferences, and specifically to obtain correct *p*-values, we estimate equations (4) and (5) using the two-step procedure suggested by Donald and Lang (2007). In the first step, the log of housing prices are regressed on all variables that vary at the individual or neighborhood level (namely, the individual property covariates, neighborhood or property fixed effects, neighborhood trends), and a set of district-by-year fixed effects. In the second step, the estimated district-by-year fixed effects are regressed on variables that vary at the district level, namely the treatment variables, a district-specific time trend, and year fixed effects, weighting by the number of observations in each district-by-year. As demonstrated by Donald and Lang (2007), this two-step procedure is an efficient estimator and provides appropriate *p*-values in the case of a small number of clusters under relatively unrestrictive assumptions.

As in the enrollment analysis, we conducted synthetic control analyses to test the validity of using Rochester and Buffalo as comparisons in our difference-in-differences analysis. These analyses and their results are described in Appendix B.

The difference-in-differences estimates effectively control for any factors that might have influenced property values in Syracuse, Buffalo, and Rochester similarly. However, the difference-in-differences estimates do not necessarily control for factors that had a unique influence on property values in a particular metropolitan area during the post-Say Yes period. Thus, as a robustness check, we again use a triple-differences estimator, which compares the difference between deviations from trends in the treated district and its surrounding suburbs to the similar differences between the central city and the suburban districts in the comparison metropolitan area(s). As in the enrollment analysis, this estimator has the advantage of controlling for metropolitan-specific shocks that might coincide with the adoption of Say Yes, and should be interpreted as an indicator of whether or not Say Yes may have contributed to a divergence (or convergence) in housing prices between the city where it was adopted and its surrounding suburbs.

Specifically, we use data on all home sales in the treated and comparison districts, and their surrounding suburbs to estimate equation (6).

$$\begin{aligned}\ln \mathrm{P}_{\boldsymbol{inmt}} &= \boldsymbol{\alpha}_0 + \boldsymbol{\alpha}_1(\boldsymbol{D}1\times \boldsymbol{City}\times \boldsymbol{Treated})_{\boldsymbol{nmt}} + \boldsymbol{\alpha}_2(\boldsymbol{D}2\times \boldsymbol{City}\times \boldsymbol{Treated})_{\boldsymbol{nmt}} \\ &+ \boldsymbol{\alpha}_3(\boldsymbol{D}3\times \boldsymbol{City}\times \boldsymbol{Treated})_{\boldsymbol{nmt}} + \boldsymbol{\beta}_1(\boldsymbol{D}1\times \boldsymbol{City})_{\boldsymbol{nmt}} + \boldsymbol{\beta}_2(\boldsymbol{D}2\times \boldsymbol{City})_{\boldsymbol{nmt}} \\ &+ \boldsymbol{\beta}_3(\boldsymbol{D}3\times \boldsymbol{City})_{\boldsymbol{nmt}} + \boldsymbol{X}_{\mathrm{inmt}}\Phi + \boldsymbol{\gamma}_{\boldsymbol{n}} + \boldsymbol{\varphi}_{\boldsymbol{n}}\boldsymbol{T}_{\boldsymbol{t}} + \boldsymbol{\eta}_{\boldsymbol{mt}} + \boldsymbol{\varepsilon}_{\boldsymbol{int}}.\end{aligned} \tag{6}$$

As in estimation of the difference-in-differences, we control for individual housing characteristics, neighborhood-specific fixed effects and trends, and in this case, metropolitan-by-year fixed effects, $\boldsymbol{\eta}_{\boldsymbol{mt}}$. As in the enrollment analysis, $\boldsymbol{\beta}_{\boldsymbol{i}}$ is the difference in the deviation from pre-Say Yes trends in the central city district and districts in the comparison metropolitan area(s) during post-Say Yes year i, and α_i is the triple-differences estimate of the effect of Say Yes on the log of property values. We also estimate an equation similar to (6) in which we replace the individual property covariates and neighborhood fixed effects with an individual property fixed effect using properties with multiple sales. To ensure proper inferences, we estimate both the all sales and repeated sales regression using the two-step procedure that we used to implement the difference-in-differences estimator.

Results

Table 30 displays the results of our primary housing value analysis. For both the difference-in-differences and the triple-differences analyses, estimated changes in housing values associated with Say Yes are similar whether all sales or multiple sales are used. The triple-differences estimates tend to be larger in absolute value and less precise than the corresponding difference-in-differences estimates. Nonetheless, the results from the difference-in-differences and the triple-differences are qualitatively similar.

Both the difference-in-differences and triple-differences suggest that Syracuse experienced a larger increase in property values after Say Yes relative to pre-existing neighborhood trends than did Rochester and Buffalo. The results are statistically significant only in the case of the triple-differences estimates during the third year after the announcement of Say Yes, which show rather large increases of between 14 and 17 percent. Overall, the estimated increases in property values associated with the adoption of Say Yes in Syracuse ranged between 6.5 percent and 16.9 percent depending on the sample and model. These results are similar to the estimates of 6 percent to 12 percent reported by LeGower and Walsh (2014) using their pooled sample and in contrast to early estimates in Kalamazoo that found no effect on housing prices (Miller, 2011; Miller-Adams, 2010).

The fact that the triple-differences estimates are larger than the difference-in-differences estimates for Syracuse suggests that the announcement of Say Yes is associated with decreases in property values in the surrounding suburbs as well as increases in property values in Syracuse. To test that hypothesis more directly, we computed difference-in-differences estimates of the effect of Say Yes on property values in the suburbs around Syracuse. To compute these difference-in-differences, we estimate equations (4) and (5) above using the sample of home sales in the Syracuse, Rochester, and Buffalo suburbs during the eight years preceding and three years following the announcement of Say Yes in Syracuse, and consider the homes sales in the Syracuse suburbs following the announcement as treated observations.

The results of this analysis of suburban property values is presented in Table 31. As implied by the results in Table 5, the changes in properties values in the Syracuse suburbs associated with the adoption of Say Yes are negative and large, and are also statistically significant. Three years after the adoption of Say Yes in Syracuse properties values in the suburbs decreased between 7 percent and 9 percent, relative to the projection of pre-existing trends and controlling for deviations from projected trends observed in the Rochester and Buffalo suburbs during the same time period. The results in Table 6 indicate that the announcement of Say Yes was associated with both increases in property values in the city and decreases in property values in the surrounding suburbs, which is consistent with the idea that people who might otherwise have lived in the suburbs moved to or remained in the city.

Returning to Table 30, the estimated changes in property values associated with Say Yes in Buffalo are all negative. The triple-differences estimates are slightly more negative, but also less precise, than the difference-in-differences estimates. The estimated changes in housing values are statistically significant only for the second year after the adoption of Say Yes, when all sales are used. The estimated second-year changes in the multiple sales sample, when more complete controls for housing characteristics are employed, are less precise and not statistically different from zero.

Although the results in the bottom panel of Table 30 suggest that a decline in property values in Buffalo relative to projections may have accompanied the adoption of Say Yes, Figure 13 suggests that the start of the decline preceded the start of the program. To test this possibility, we add to each of the models estimated in Table 30 variables indicating the first, second, and third year pre-Say Yes. If the estimated coefficients on the post-Say Yes variables move closer to zero (or change signs), or the coefficients on the pre-treatment variables are similar to the post treatment variables, it suggests that prices may have started to change before the start of Say Yes and the estimates in Table 5 may not reflect the causal impacts of the program.

The results of this “event history” analysis are presented in Table 32 (Syracuse) and Table 33 (Buffalo). In Syracuse, the coefficients for the pre-Say Yes years are largely, though not always, positive and are not significant in any model. The post-Say Yes coefficients are substantially larger than the pre-Say Yes coefficients. The post-Say Yes coefficient are also slightly larger in this model than in the previous models, and still significant in the triple-differences model for the third year of Say Yes. Thus, the estimates do not provide any indication that the observed housing market changes began before the start of Say Yes in Syracuse. Table 33 shows that the post-Say Yes coefficients are much smaller than the pre-Say Yes coefficient estimates and the estimates in Table 30, and no longer significant, suggesting that the price decreases in Buffalo were not the result of Say Yes.

We also used the synthetic control method to create counterfactuals for Syracuse and Buffalo housing prices (full results presented in Appendix B: Analysis of Housing Price Changes using Synthetic Control Method). For both Syracuse and Buffalo, Rochester receives the majority of weight in constructing the synthetic control, which validates our reliance on Rochester as a comparison city in the regression analyses. We find that the housing price increase in Syracuse after Say Yes was substantially larger than in its synthetic control, and larger than those found in the difference-in-differences analyses. As in the difference-in-differences models, the synthetic control analysis finds no evidence of housing price increases in Buffalo after the start of Say Yes. Thus, the synthetic control analyses suggest that the results are robust to the use of alternative methods.

We also estimated the changes in housing values associated with the adoption of Say Yes by neighborhood income level. Specifically, we divided the sample of treatment and control neighborhoods into thirds based on median housing income in the neighborhood. We then estimated our difference-in-differences models for each sample separately. The results suggest that changes in property values associated with the adoption of Say Yes were concentrated in low and middle-income neighborhoods in Syracuse and in low-income neighborhoods in Buffalo. However, the estimates of changes in housing values were generally quite noisy and thus, we are reluctant to draw any strong conclusions from this analysis.

Summary and Discussion

The analyses presented above examine potential early indicators of urban revitalization—school district enrollments and housing prices—in Syracuse and Buffalo, New York in the wake of Say Yes to Education's start in each city. We find consistent evidence of enrollment increases in both Syracuse and Buffalo following the announcement of the program and that these increases occurred after years of largely declining enrollments. Moreover, the increases coincided with the start of the program and grew over time, though the program began in different years in each city. While the Syracuse increases were accompanied by large enrollment declines in surrounding suburban districts, the Buffalo increases coincided with large declines in private school enrollments in the area. The increases in both cities, and particularly in Syracuse, appear to be largely concentrated in the districts' highest performing schools. It must be noted, however, that the cities of Buffalo and Rochester also saw enrollment increases relative to projected trends following the announcement of Say Yes in Syracuse, suggesting that at least part of the increase in enrollments in Syracuse might be attributable to factors other than Say Yes.

Using difference-in-differences and triple-differences models, we find evidence of substantively meaningful increases in home prices in Syracuse after the program's announcement, as well as decreases in housing values in the suburbs surrounding Syracuse, both of which are consistent with the hypothesis that Say Yes helped to attract to the city people who would otherwise have located in the suburbs. We do not find evidence of similar housing price changes in Buffalo. The Syracuse results also suggest that much of the program's effect may be to shift locational decisions within, but not across, metropolitan areas.

These results, then, raise questions about why responses to the program would be different in Syracuse than in Buffalo. We have no definitive answers, but it is quite likely that the different contexts and program benefits between the two cities may help to explain the findings. First, the Syracuse program is arguably more generous than the Buffalo program. Syracuse requires only three years of high school attendance for full scholarship eligibility, while Buffalo requires twelve years. Additionally, tuition at Syracuse University, listed at over $40,000 per year during this period, is available for all Syracuse Say Yes students but only for those from families with income under $100,000 in Buffalo. Given its location, Syracuse University is also likely to be a more attractive option for students from Syracuse than from Buffalo. The more generous benefits available in Syracuse may be more likely to induce families to move from the suburbs to the city, or to stay in the city, to take advantage of the program, thereby increasing demand for housing. In Buffalo, the program appears to have drawn students largely from private schools. If these families already lived in the city, the housing market effects would likely be smaller.

The contexts of Syracuse and Buffalo may also help to explain some of the differences in responses to Say Yes across the two cities. As described above, Buffalo's large Catholic school sector was undergoing consolidation and closures during the years before Say Yes began. While these events did not coincide with the start of Say Yes, the uncertainty surrounding private schools, combined with the programmatic and scholarship benefits of Say Yes, may have accelerated movement toward city public schools. In Syracuse, with a smaller private school sector, suburban schools may represent the more relevant alternative for many parents who do not want to send their children to public city schools. Ultimately, though, understanding the mechanisms underlying these differential effects is worthy of additional study in future research.

The results also provide some evidence on the potential for place-based scholarships to spur economic revitalization in distressed cities. Both Syracuse and Buffalo have suffered through decades of economic decline and shrinking tax bases. Between 1950 and 2000, Buffalo lost half of its population, the fourth largest decline among large cities in the United States, while Syracuse lost one-third of its population (Office of the New York State Comptroller, 2004). Evidence from the Say Yes to Education program suggests that providing a substantial and highly visible amenity such as free college tuition may be effective at stemming these ongoing population losses and inducing some households to remain in central cities or to move from nearby suburbs, though the magnitude of the effect may be modest. From a metropolitan perspective, this growth may be a zero sum game, with gains in cities offset by losses in neighboring communities. Additionally, providing free college to large numbers of students may be an expensive model if the gains are small, though the last-dollar nature of the scholarships reduces overall costs. Future work will be needed to determine whether these cities are able to maintain the enrollment gains and whether they are, in fact, leading indicators of broader economic development.

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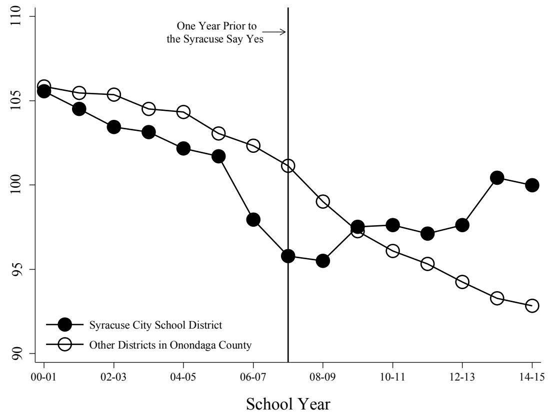
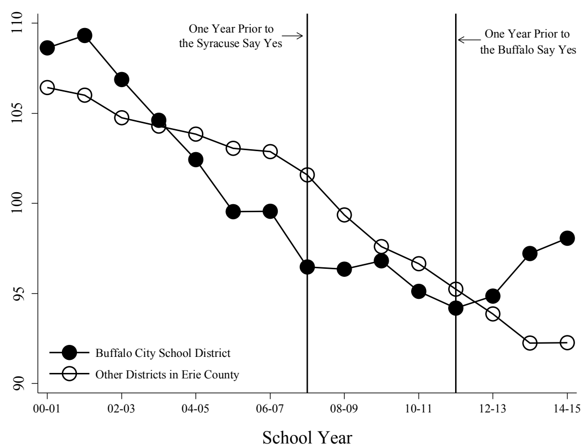
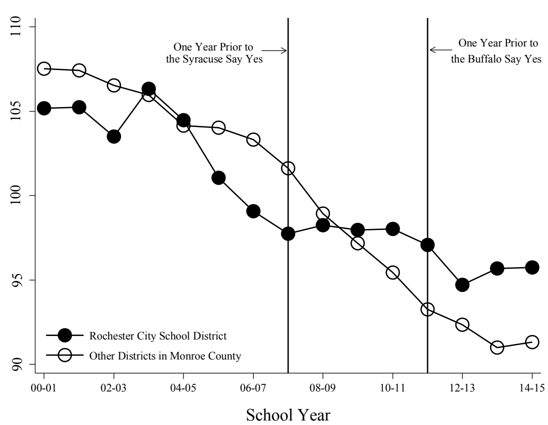
Figure 12: Enrollment Trends, Cities and Suburbs



Panel A: Syracuse City School District vs. Other Districts in Onondaga County

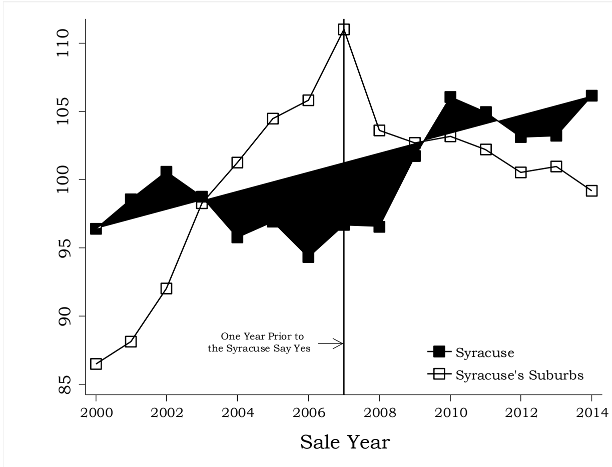


Panel B: Buffalo City School District vs. Other Districts in Erie County

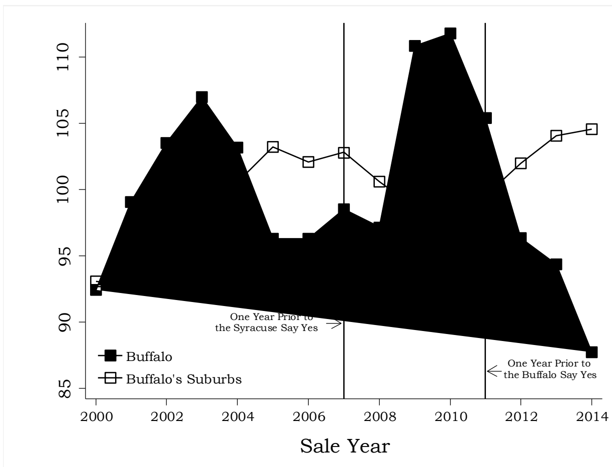


Panel C: Rochester City School District vs. Other Districts in Monroe County

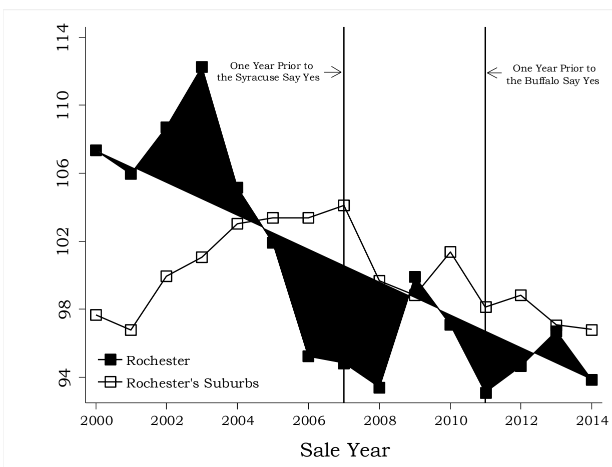
Figure 13: Median housing values, cities and suburbs



Panel A. Syracuse vs. Its Suburbs



Panel B. Buffalo vs. Its Suburbs



Panel C. Rochester vs. Its Suburb

Table 26: Changes in Enrollment Trends Associated with Adoption of Say Yes

	Four-years pre-Say Yes included			Eight-years pre-Say Yes included		
Variable	Pre-post	Difference-in-differences	Triple-differences	Pre-post	Difference-in-differences	Triple-differences
			Panel A: Syracuse			
1 year post Say Yes	0.018	−0.004	−0.007	−0.006	−0.014	−0.011
	(0.016)	(0.019)	(0.018)	(0.015)	(0.017)	(0.017)
	[0.368]	[0.829]	[0.716]	[0.705]	[0.420]	[0.516]
2 year post Say Yes	0.062*	0.019	0.019	0.027	0.004	0.012
	(0.019)	(0.023)	(0.022)	(0.017)	(0.018)	(0.018)
	[0.083]	[0.425]	[0.404]	[0.126]	[0.811]	[0.515]
3 year post Say Yes	0.086*	0.032	0.030	0.041*	0.012	0.020
	(0.023)	(0.027)	(0.026)	(0.018)	(0.019)	(0.019)
	[0.064]	[0.277]	[0.284]	[0.060]	[0.539]	[0.315]
No. of observations	7	21	42	11	33	66
			Panel B: Buffalo			
1 year post Say Yes	0.013	0.037	0.041**	0.025	0.029*	0.032
	(0.011)	(0.012)	(0.011)	(0.013)	(0.013)	(0.033)
	[0.349]	[0.150]	[0.018]	[0.155]	[0.065]	[0.351]
2 year post Say Yes	0.047*	0.057*	0.064***	0.056***	0.046**	0.053
	(0.014)	(0.015)	(0.013)	(0.013)	(0.014)	(0.036)
	[0.075]	[0.060]	[0.008]	[0.006]	[0.016]	[0.163]
3 year post Say Yes	0.064*	0.069*	0.080***	0.075***	0.057***	0.065
	(0.016)	(0.017)	(0.015)	(0.014)	(0.015)	(0.038)
	[0.059]	[0.057]	[0.007]	[0.002]	[0.009]	[0.112]
No. of observations	7	14	28	11	22	44

Note. Each column of figures are coefficients, with associated standard errors and p-values in parentheses and brackets, respectively, from separate regression. The outcome variable in each regression is the natural log of enrollment. “Pre-post” correspond to equation (1), “Difference-in-differences” correspond to equation (2), and “Triple-differences” correspond to equation (3). Estimates include controls for pre-Say Yes enrollment trends. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 27: Changes in Enrollment Trends Associated with Adoption of Say Yes, by Type of District

Variable	Countywide	City public schools	Public schools in adjacent districts	Public schools in other districts in the county	Private schools
			Panel A: Syracuse		
1 year post Say Yes	115	394	−328**	−176	226
	(573)	(335)	(75)	(131)	(396)
2 year post Say Yes	523	1,323*	−544**	−550*	294
	(697)	(407)	(92)	(160)	(481)
3 year post Say Yes	776	1,839*	−585**	−856**	379
	(834)	(488)	(110)	(191)	(576)
No. of observations	7	7	7	7	7
			Panel B: Buffalo		
1 year post Say Yes	−851	518	−3	−16	−1,350*
	(546)	(430)	(104)	(175)	(341)
2 year post Say Yes	806	1,804*	−84	209	−1,122*
	(664)	(524)	(126)	(213)	(415)
No. of observations	6	6	6	6	6

Note. Each column of figures are coefficients, with associated standard errors in parentheses, from separate estimation of equation (1), each using four years of pre-Say Yes observations. Untransformed enrollment counts, rather than the natural log of enrollment counts, are used as outcome variables. Estimates include controls for pre-Say Yes enrollment trends. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 28: Change in Trends in Enrollment Associated with Adoption of Say Yes, by Race

	Difference-in-differences estimates		
Variable	*ln* white enrollment	*ln* non-white enrollment	Share white
		Panel A: Syracuse	
1 year post Say Yes	0.030	−0.006	0.008
	(0.030)	(0.031)	(0.006)
2 year post Say Yes	0.096**	0.008	0.019
	(0.037)	(0.038)	(0.007)
3 year post Say Yes	0.082	0.029	0.016
	(0.044)	(0.046)	(0.009)
No. of observations	21	21	21
		Panel B: Buffalo	
1 year post Say Yes	0.048**	0.042	0.002
	(0.004)	(0.020)	(0.004)
2 year post Say Yes	0.066**	0.095*	−0.002
	(0.005)	(0.024)	(0.005)
No. of observations	12	12	12

Note. Each column of figures are coefficients, with associated standard errors in parentheses, from separate estimates of equation (2). The outcome variable in each regression is the natural log of enrollment. Estimates include controls for pre-Say Yes enrollment trends estimated using four years of pre-Say Yes observations. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05.

Table 29: Changes in Enrollment Trends Associated with Adoption of Say Yes, By School

Variable	Low-performing	Middle-performing	High-performing
		Panel A: Syracuse	
1 year post Say Yes	0.016	0.175***	0.178***
	(0.053)	(0.063)	(0.059)
2 year post Say Yes	0.025	0.101**	0.113**
	(0.045)	(0.048)	(0.047)
3 year post Say Yes	0.002	0.092**	0.057
	(0.036)	(0.045)	(0.038)
No. of observations	203	201	196
		Panel B: Buffalo	
1 year post Say Yes	0.050	0.116*	0.083
	(0.065)	(0.059)	(0.079)
2 year post Say Yes	−0.054	0.061	0.114*
	(0.054)	(0.049)	(0.065)
3 year post Say Yes	−0.016	0.010	0.002
	(0.044)	(0.040)	(0.054)
No. of observations	147	147	140

Note. Each column of figures are coefficients, with associated standard errors in parentheses, from separate regressions. Regression equation estimated in each case is similar to equation (2) except district-specific trends are replaced with school-specific trends. The outcome variable in each regression is the natural log of enrollment. All estimates are based on four-year pre-Say Yes and three-year post Say Yes observations. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and ***indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 30: Changes in Housing Prices Associated with Adoption of Say Yes

Variable	Diff.-in-diff. all sales	Diff.-in-diff. multiple sales	Triple-diff. all sales	Triple-diff. multiple sales
		Panel A. Syracuse		
1 year post Say Yes	0.052	0.046	0.071	0.089
	(0.035)	(0.045)	(0.046)	(0.064)
	[0.159]	[0.317]	[0.136]	[0.174]
2 year post Say Yes	0.022	0.016	0.075	0.089
	(0.036)	(0.046)	(0.064)	(0.066)
	[0.544]	[0.731]	[0.119]	[0.189]
3 year post Say Yes	0.065	0.068	0.143**	0.169**
	(0.044)	(0.056)	(0.057)	(0.079)
	[0.159]	[0.243]	[0.020]	[0.044]
District-by-year obs.	33	33	66	66
Individual property sales	49,624	23,540	270,011	115,324
		Panel B. Buffalo		
1 year post Say Yes	−0.024	−0.028	−0.042	−0.060
	(0.035)	(0.044)	(0.048)	(0.067)
	[0.518]	[0.552]	[0.399]	[0.390]
2 year post Say Yes	−0.097**	−0.072	−0.111**	−0.107
	(0.036)	(0.044)	(0.049)	(0.069)
	[0.035]	[0.154]	[0.035]	[0.146]
District-by-year obs.	20	20	40	40
Individual property sales	39,112	18,989	186,554	80,530

Note. Each column of figures are coefficients, with associated standard errors and *p*-values in parentheses and brackets, respectively, from separate regressions. The outcome variable in each regression is the natural log of the home sales price. “Diff.-in-diff. all sales” correspond to equation (4), “Diff.-in-diff. multiple sales” correspond to equation (5), and “Triple-diff. all sales” correspond to equation (5). “Triple-diff. multiple sales” is based on equation similar to equation (5) with individual property covariates and neighborhood fixed effects replaced by individual property fixed effects. All estimates are based on eight-year pre-Say Yes and three-year post Say-Yes observations. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 31: Changes in Housing Prices in the Syracuse Suburbs Associated with Adoption of Say Yes

Variable	Diff.-in-diff. all sales	Diff.-in-diff. multiple sales
1 year post Say Yes	−0.012	−0.035**
	(0.013)	(0.016)
	[0.338]	[0.046]
2 year post Say Yes	−0.047***	−0.072***
	(0.013)	(0.016)
	[0.002]	[0.000]
3 year post Say Yes	−0.072***	−0.094***
	(0.015)	(0.019)
	[0.000]	[0.000]
District-by-year observations	33	33
Individual property sales	49,624	23,540

Note. Each column of figures are coefficients, with associated standard errors and *p*-values in parentheses and brackets, respectively, from separate regressions. The outcome variable in each regression is the natural log of the home sales price. "Diff.-in-diff. all sales" correspond to equation (4), "Diff.-in-diff. multiple sales" correspond to equation (5). All estimates are based on samples that include eight-year pre-Say Yes and three-year post Say-Yes observations of home sales in suburban areas around Syracuse, Buffalo, and Rochester. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 32: Changes in Housing Prices Associated with Adoption of Say Yes in Syracuse

Variable	Diff.-in-diff. all sales	Diff.-in-diff. multiple sales	Triple-diff. all sales	Triple-diff. multiple sales
3 year pre Say yes	0.036	0.039	0.046	0.027
	(0.044)	(0.056)	(0.036)	(0.041)
	[0.322]	[0.337]	[0.217]	[0.518]
2 year pre Say Yes	0.004	−0.025	−0.003	−0.065
	(0.036)	(0.046)	(0.043)	(0.049)
	[0.916]	[0.604]	[0.948]	[0.200]
1 year pre Say Yes	0.037	0.068	0.041	0.058
	(0.035)	(0.045)	(0.052)	(0.058)
	[0.477]	[0.253]	[0.435]	[0.335]
1 year post Say Yes	0.083	0.080	0.103	0.097
	(0.035)	(0.045)	(0.062)	(0.069)
	[0.192]	[0.255]	[0.117]	[0.179]
2 year post Say Yes	0.058	0.056	0.112	0.098
	(0.036)	(0.046)	(0.068)	(0.077)
	[0.400]	[0.471]	[0.124]	[0.221]
3 year post Say Yes	0.106	0.113	0.184**	0.180*
	(0.045)	(0.056)	(0.079)	(0.089)
	[0.200]	[0.219]	[0.036]	[0.062]
District-by-year observations	33	33	66	66
Individual property sales	49,624	23,540	270,011	115,324

Note. Each column of figures are coefficients, with associated standard errors and *p*-values in parentheses and brackets, respectively, from separate regressions. The outcome variable in each regression is the natural log of the home sales price. "Diff.-in-diff. all sales" correspond to equation (4), "Diff.-in-diff. multiple sales" correspond to equation (5), and "Triple-diff. all sales" correspond to equation (5). "Triple-diff. multiple sales" is based on equation similar to equation (5) with individual property covariates and neighborhood fixed effects replaced by individual property fixed effects. All estimates are based on eight-year pre-Say Yes and three-year post Say Yes observations. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 33: Changes in Housing Prices Associated with Adoption of Say Yes in Buffalo

Variable	Diff.-in-diff. all sales	Diff.-in-diff. multiple sales	Triple-diff. all sales	Triple-diff. multiple sales
3 year pre Say yes	0.026	0.037	0.011	0.031
	(0.035)	(0.039)	(0.044)	(0.067)
	[0.508]	[0.416]	[0.812]	[0.661]
2 year pre Say Yes	0.084	0.117*	0.065	0.103
	(0.043)	(0.048)	(0.055)	(0.083)
	[0.148]	[0.094]	[0.281]	[0.262]
1 year pre Say Yes	0.044	0.057	0.031	0.055
	(0.048)	(0.053)	(0.061)	(0.092)
	[0.430]	[0.362]	[0.632]	[0.566]
1 year post Say Yes	0.033	0.051	−0.003	0.011
	(0.053)	(0.059)	(0.067)	(0.102)
	[0.579]	[0.454]	[0.971]	[0.915]
2 year post Say Yes	−0.031	0.018	−0.072	−0.025
	(0.059)	(0.065)	(0.074)	(0.113)
	[0.629]	[0.803]	[0.373]	[0.833]
District-by-year observations	20	20	40	40
Individual property sales	39,112	18,989	186,554	80,530

Note. Each column of figures are coefficients, with associated standard errors and *p*-values in parentheses and brackets, respectively, from separate regressions. The outcome variable in each regression is the natural log of the home sales price. "Diff.-in-diff. all sales" correspond to equation (4), "Diff.-in-diff. multiple sales" correspond to equation (5), and "Triple-diff. all sales" correspond to equation (5). "Triple-diff. multiple sales" is based on equation similar to equation (5) with individual property covariates and neighborhood fixed effects replaced by individual property fixed effects. All estimates are based on eight-year pre-Say Yes and three-year post Say Yes observations. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

APPENDIX A: Alternative Specifications of Housing Price Analyses

Table 34: Changes in Housing Prices Associated with Adoption of Say Yes, with and without Controls for Tract-Specific Trends

	Syracuse		Buffalo	
Variable	Tract F.E.	Tract F.E. & Tract trends	Tract F.E.	Tract F.E. & Tract trends
1 year post Say Yes	0.082	0.052	0.062	−0.024
	(0.050)	(0.035)	(0.060)	(0.035)
	[0.117]	[0.159]	[0.332]	[0.518]
2 year post Say Yes	0.051	0.022	−0.007	−0.097**
	(0.047)	(0.036)	(0.055)	(0.036)
	[0.294]	[0.544]	[0.901]	[0.035]
3 year post Say Yes	0.124**	0.065	—	—
	(0.058)	(0.044)		
	[0.049]	[0.159]		
District-by-year obs.	33	33	20	20
Individual property sales	49,624	49,624	39,112	39,112

Note. Each column of figures are coefficients, with associated standard errors and *p*-values in parentheses and brackets, respectively, from separate regressions. The outcome variable in each regression is the natural log of the home sales price. Models include controls for individual property characteristics and month-by-year fixed as well as tract fixed effects and tract trends. Syracuse model is estimated using all home sales in Syracuse, Rochester and Buffalo during eight years pre-Say Yes and three years post Say-Yes. Buffalo model is estimated using all home sales in Buffalo and Rochester during eight years pre-Say Yes and two years post-Say Yes. ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 35: Changes in Housing Prices Associated with Adoption of Say Yes, Using Four Years of Pre-Say Yes Data

	Diff.-in-diff. all sales	Diff.-in-diff. multiple sales	Triple-diff. all sales	Triple-diff. multiple sales
		Panel A. Syracuse		
1 year post Say Yes	0.046	0.032	0.064	0.078
	(0.046)	(0.069)	(0.049)	(0.075)
	[0.346]	[0.660]	[0.226]	[0.320]
2 year post Say Yes	0.018	−0.005	0.070	0.074
	(0.053)	(0.081)	(0.056)	(0.087)
	[0.741]	[0.950]	[0.248]	[0.147]
3 year post Say Yes	0.060	0.038	0.136*	0.147
	(0.066)	(0.010)	(0.071)	(0.109)
	[0.398]	[0.718]	[0.089]	[0.207]
District-by-year observations	21	21	42	42
Individual property sales	35,821	16,668	175,494	75,012
		Panel B. Buffalo		
1 year post Say Yes	−0.050	−0.069	−0.057	−0.086
	(0.046)	(0.067)	(0.055)	(0.075)
	[0.390]	[0.413]	[0.333]	[0.285]
2 year post Say Yes	−0.013*	−0.129	−0.128*	−0.140
	(0.055)	(0.081)	(0.066)	(0.090)
	[0.145]	[0.251]	[0.089]	[0.158]
District-by-year observations	12	12	24	24
Individual property sales	19,735	9,700	126,565	53,606

Note. Each column of figures are coefficients, with associated standard errors and *p*-values in parentheses and brackets, respectively, from separate regressions. The outcome variable in each regression is the natural log of the home sales price. “Diff.-in-diff. all sales” correspond to equation (4), “Diff.-in-diff. multiple sales” correspond to equation (5), and “Triple-diff. all sales” correspond to equation (5). “Triple-diff. multiple sales” is based on equation similar to equation (5) with individual property covariates and neighborhood fixed effects replaced by individual property fixed effects. All estimates are based on four-year pre-Say Yes and three-year post Say Yes observations. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Appendix B: Analysis of Housing Price Changes using Synthetic Control Method

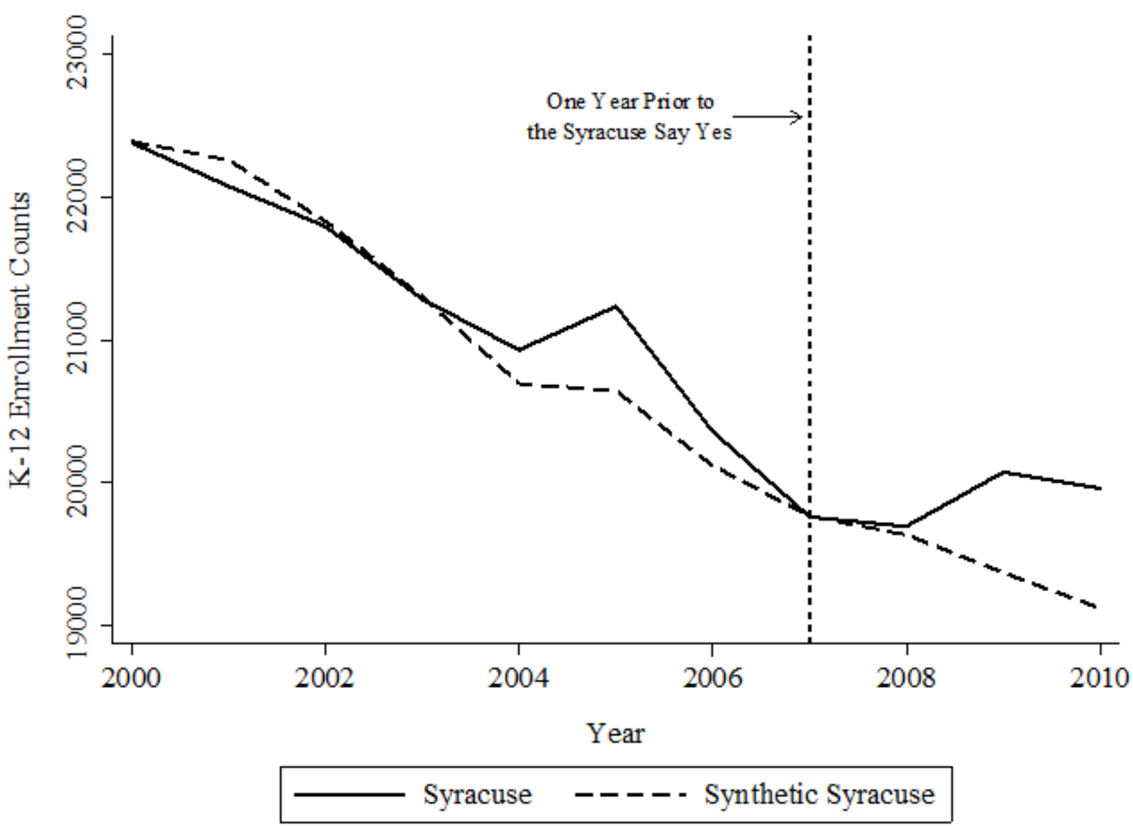
Abadie, Diamond, and Hainmueller (2010) propose a strategy for obtaining effect estimates in evaluations of unique interventions implemented in a single treatment unit, which they refer to as synthetic control methods (SCM). Rather than choosing a single comparison unit, synthetic control methods identify a set of weights that can be assigned to a broad sample of comparison districts such that the resulting weighted averages of those comparison districts closely match the treated district on pre-treatment values. Post-treatment outcome values for the synthetic composite district then provide credible estimates of the counterfactual post-treatment outcomes. Differences in outcomes between the treated unit and the synthetic control identify the estimated treatment effect.

To test the sensitivity of our enrollment and housing price estimates to the specific comparison districts that we chose, we constructed synthetic controls for both Syracuse and Buffalo. In order to have the largest set of districts from which the SCM algorithm chooses comparison units ("donor pool"), we used all school districts in New York State outside of New York City and Long Island. Because Buffalo has substantially higher enrollment than all other upstate New York State districts aside from Rochester, the synthetic control model for Buffalo relies almost exclusively on Rochester to create the synthetic control, and therefore, the enrollment analyses below focus only on Syracuse.

To choose covariates on which to match, we selected variables likely to be related to enrollment and housing price trends. More specifically, for the enrollment model we use enrollment and the average shares of free-lunch eligible, African-American, and Hispanic students over the pre-treatment period. For the housing model we use those student characteristics and measures of pre-treatment housing prices to control for pre-Say Yes price trends.

Because housing prices vary considerably across metropolitan areas, and because our interest is in matching pre-treatment trends rather than levels, we transformed housing prices using the following procedure. First we regressed the log of real sales price on an array of housing characteristics and district-by-year fixed effects. We then retrieved the estimated district-by-year fixed effects and standardized them to have a mean of zero and a standard deviation of one. This standardized measure was used with the district characteristics above to create the synthetic control districts.

Figure 14: Enrollment in Syracuse and Synthetic Syracuse.



Note. The SCM analysis gives weights to the following districts: Rochester (0.461), Niagara Falls (0.299), Elmira (0.094), Hopevale (0.083) and Buffalo (0.063).

As shown in Figure B1, the synthetic district matches the pre-treatment enrollment trends in Syracuse closely with the exception of 2005, in which Syracuse experienced a one-year increase in enrollments. Rochester receives the largest weight and accounts for almost half of the synthetic district. Enrollments in the last pre-treatment year are almost identical for Syracuse and its synthetic control. Following the start of Say Yes, Syracuse enrollments level off, then increase, while those in the synthetic district continue to decline.

To assess the likelihood that these results occurred by chance, we construct a synthetic control for each of the districts in the donor pool to derive a distribution of effect estimates. Because the other districts in the donor pool were not exposed to the program, the effect on these districts is presumably zero. Thus, the percentage of districts that have an effect estimate as large as or larger than that obtained for Syracuse can be interpreted as the probability of obtaining an effect estimate as large as for Syracuse, if the true effect of Say Yes were zero. To ensure that the comparison of effect estimates controls for the quality of the pre-treatment match, the test statistic used is the ratio of the mean squared prediction error in the post-treatment period to the mean squared prediction error in the pre-treatment period.

As shown in Figure B2, Syracuse's pre-to-post difference is large but not the largest among the districts, with a p-value of 0.086. As in the difference-in-difference analyses presented above, the results provide some evidence of an unusually large enrollment increase in Syracuse following the start of the program, but one that is not be reliably different from zero at a 95 percent confidence level.

Figure 15: Randomization Inference Results, Syracuse Enrollment.

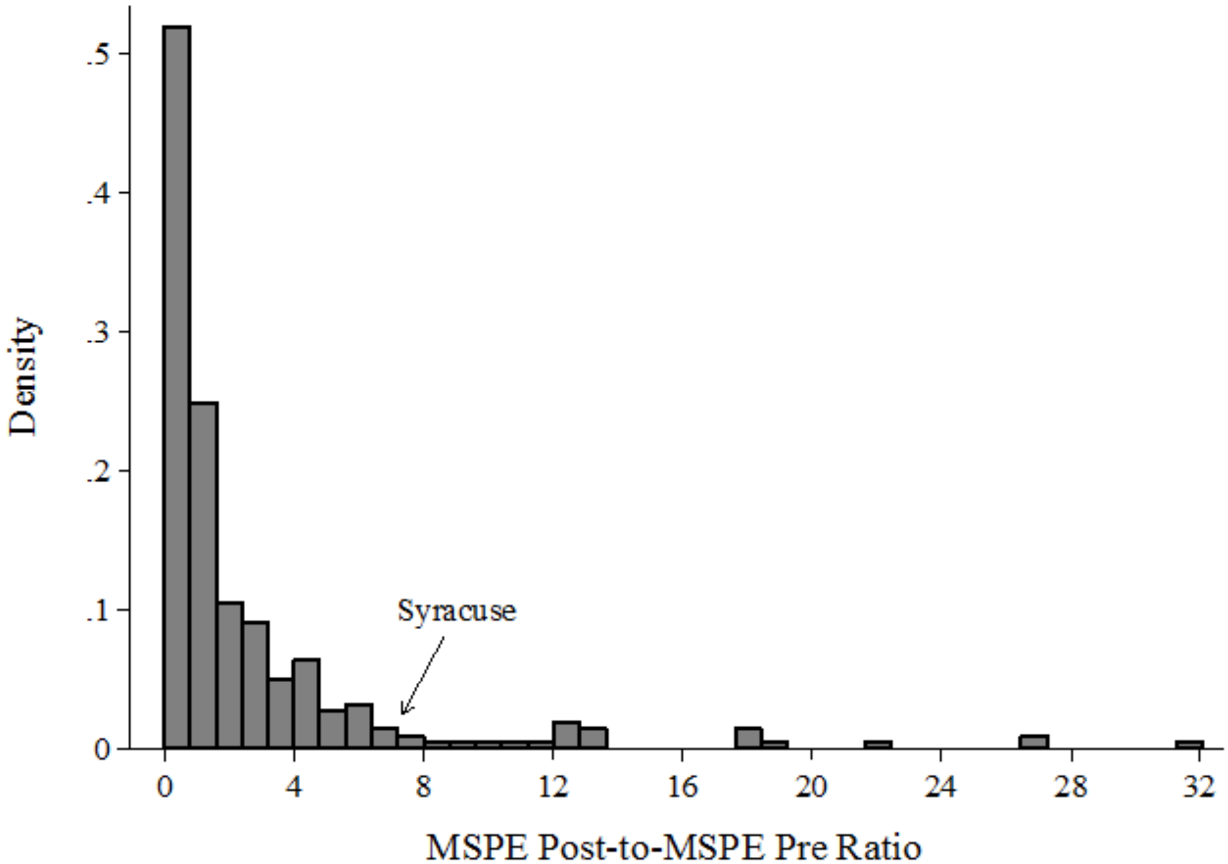
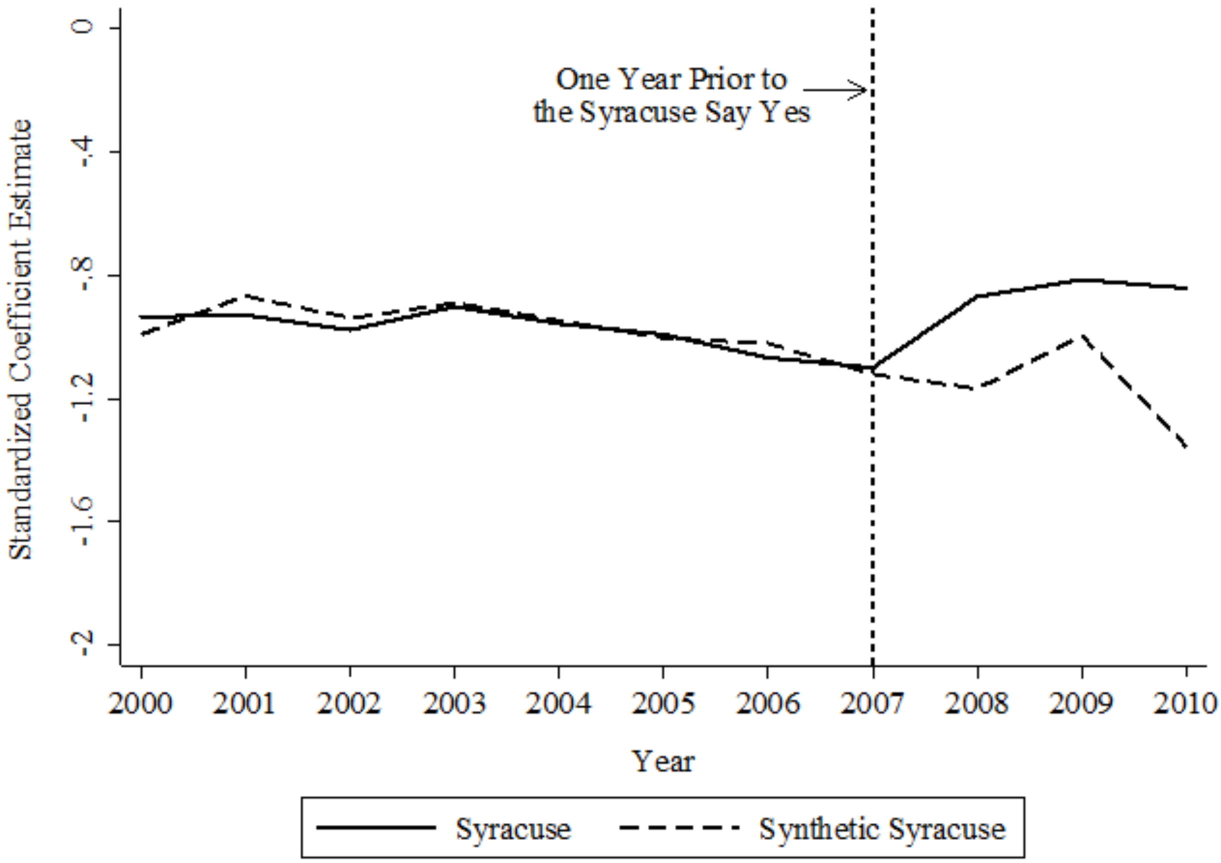


Figure B3 displays the synthetic control analysis examining housing prices in Syracuse and its synthetic control. As shown in the figure, the pre-treatment trends for Syracuse and its synthetic control match very closely. The root mean squared prediction error (RMSPE), a goodness-of-fit measure in which values closer to zero indicate better fit, is a low 0.037. The synthetic district is composed primarily of Rochester (58 percent) with smaller contributions from other districts, including Buffalo. Beginning in 2008, the first year of Say Yes, the trend lines diverge, with Syracuse increasing and remaining higher than the synthetic control district over the period.

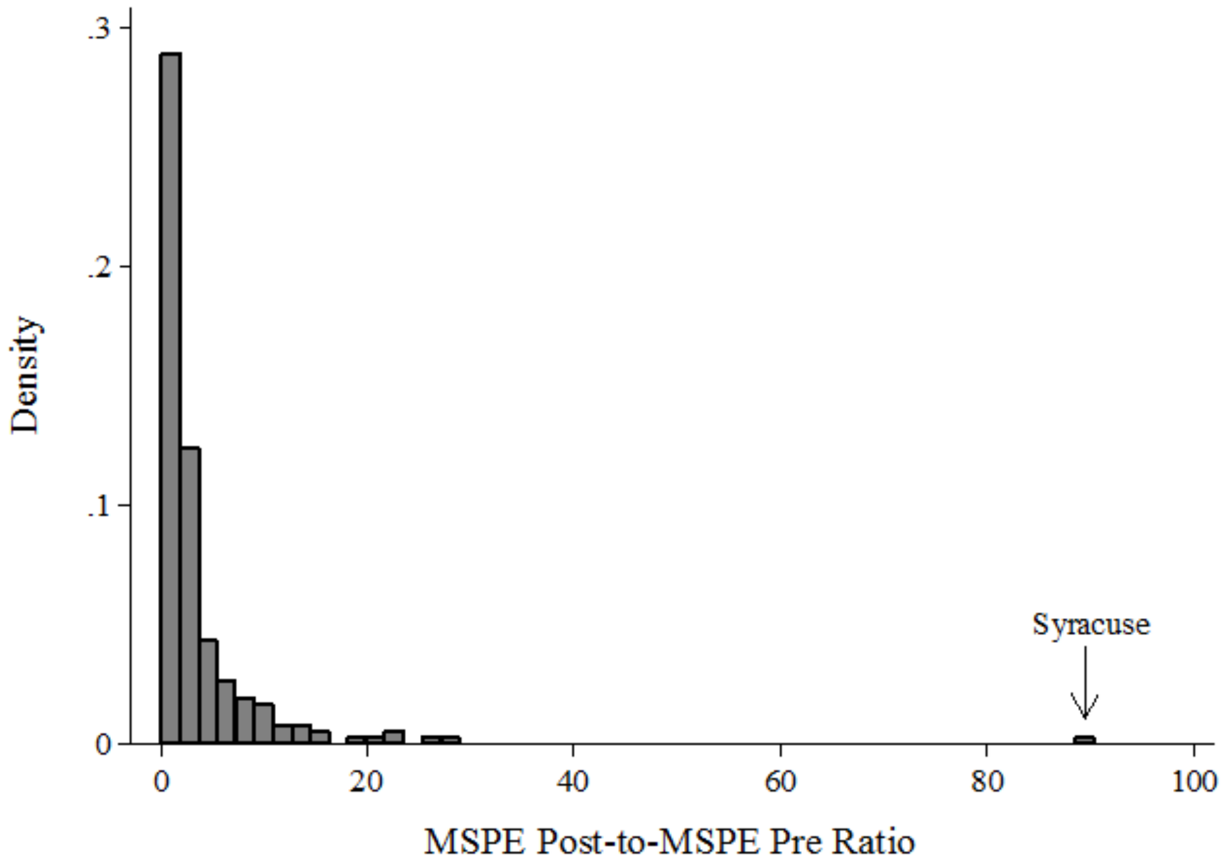
Figure 16: Housing Prices in Syracuse and Synthetic Syracuse



Note. The SCM analysis gives weights to the following districts: Rochester (0.581), Lackawanna (0.200), Poughkeepsie (0.105), Ticonderoga (0.049), Salamanca (0.016), and Buffalo (0.012).

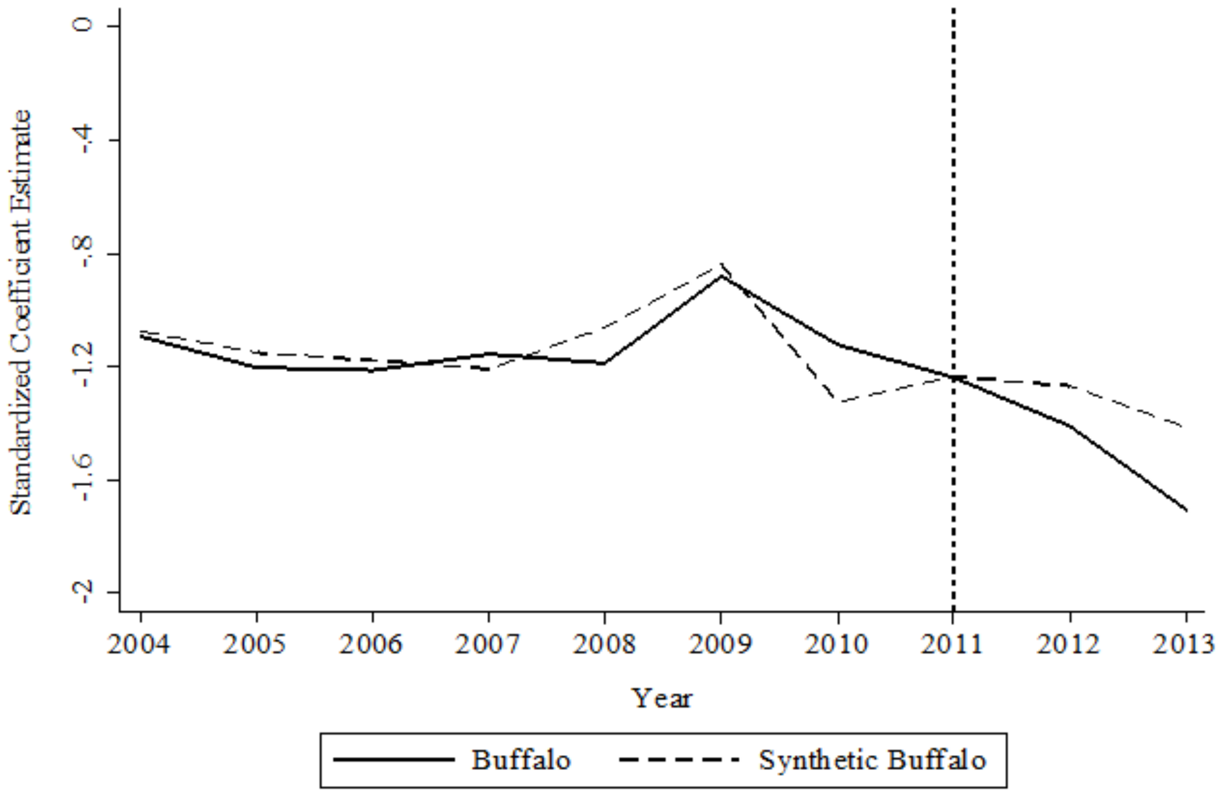
We again assess the likelihood that these results could have occurred under a null hypothesis that Say Yes had no effect on housing prices by constructing a synthetic control for each of the districts in the donor pool to derive a distribution of effect estimates. Figure B4 shows that Syracuse's effect is a far outlier, with a probability of obtaining an effect size that large by chance of 0.004 (=1/232). The results also echo those of the regression analyses presented in the paper, with a large significant increase in housing prices in Syracuse following the start of the Say Yes program, relative to the comparison districts.

Figure 17: Randomization Inference Results, Syracuse Housing Prices.



Figures B5 and B6 show the same analyses for Buffalo. It is particularly notable that Rochester comprises 84 percent of the synthetic control for Buffalo. The fit of the pre-treatment trend lines is strong, but less close than for Syracuse (RMSPE = 0.091). Following the announcement of Say Yes in Buffalo, prices in Buffalo continued to decline steeply, while those in the synthetic district first leveled off, then declined slightly. As shown in Figure B6, the treatment effect for Buffalo is not significant at conventional levels; the probability of obtaining this result by chance is 0.168 (= 68/404). These results are again consistent with the regression-based difference-in-differences analysis.

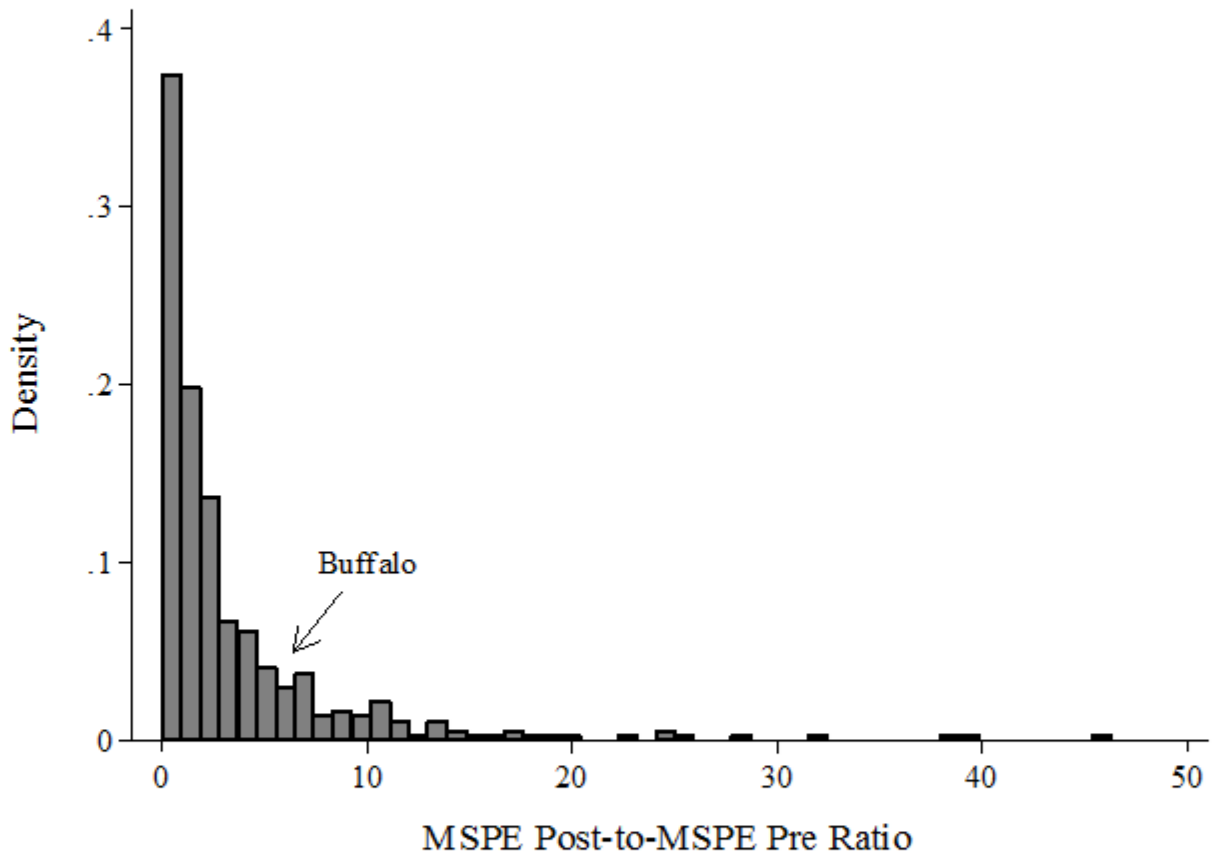
Figure 18: Housing Prices in Buffalo and Synthetic Buffalo



Note. The SCM analysis gives weights to the following districts: Rochester (0.840), Binghamton (0.103), and Piseco (0.057).

Tables B1 and B2 compare the regression-based estimates from the main text to the results of the synthetic control analyses. In each case, the two methods produce results that are of the same sign and similar magnitudes. As shown in Table B1, the Syracuse synthetic control enrollment analysis produces larger positive effects than the difference-in-differences analysis, which is its closest regression-based analogue. For example, the three-year synthetic control effect is 4.4 percent as compared to 1.2 percent for the difference-in-differences analysis. In Table B2, the comparison of housing price effects again shows somewhat larger positive effects in the Syracuse synthetic control analysis than in the difference-in-differences analysis and larger negative effects in Buffalo.

Figure 19: Randomization Inference Results, Buffalo



We draw two primary conclusions from the synthetic control analysis. First, the synthetic control method creates synthetic comparison districts in both cases (particularly Buffalo) that rely primarily on Rochester, providing support for the use of Rochester as the primary comparison district in the regression models. Second, the results from the regression analyses are robust to the use of synthetic controls, lending additional support to their validity.

Table 36: Comparison of the Synthetic Control and Regression-Based Estimates of Enrollment Effects (Syracuse)

Variable	Difference-in-differences	Triple-differences	Synthetic control
1 year post Say Yes	−0.004	−0.007	0.003
	(0.019)	(0.018)	
	[0.829]	[0.716]	
2 year post Say Yes	0.019	0.019	0.037
	(0.023)	(0.022)	
	[0.425]	[0.404]	
3 year post Say Yes	0.032	0.030	0.044
	(0.027)	(0.026)	
	[0.277]	[0.284]	
Permutation *p*-value	—	—	0.086*

Note. Standard errors and *p*-values are in parentheses and brackets, respectively. The outcome variable in each regression is the natural log of the K–12 enrollment counts. All estimates are based on four-year pre-Say Yes and three-year post Say Yes enrollment counts. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10.

Table 37: Comparison of the Synthetic and Regression-Based Estimates of Housing Price Effects

Variable	Diff.-in-diff. estimates	Triple-diff. estimates	Synthetic control estimates
		Panel A: Syracuse	
1 year post Say Yes	0.052	0.071	0.124
	(0.035)	(0.046)	
	[0.159]	[0.136]	
2 year post Say Yes	0.022	0.075	0.074
	(0.036)	(0.064)	
	[0.544]	[0.119]	
3 year post Say Yes	0.065	0.143**	0.224
	(0.044)	(0.057)	
	[0.159]	[0.020]	
Permutation-based p-values	—	—	0.004***
		Panel B: Buffalo	
1 year post Say Yes	−0.024	−0.042	−0.057
	(0.035)	(0.048)	
	[0.518]	[0.399]	
2 year post Say Yes	−0.097**	−0.111**	−0.114
	(0.036)	(0.049)	
	[0.035]	[0.035]	
Permutation-based *p*-values	—	—	0.168

Note. Standard errors and *p*-values are in parentheses and brackets, respectively. The outcome variable in each regression is the natural log of the home sales price. All estimates are based on eight- year pre-Say Yes and three-year post Say Yes all sales data for Syracuse and eight- year pre-Say Yes and two-year post Say Yes all sales data for Buffalo. ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

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