Seeking Sanctuary:
Housing Undocumented Immigrants

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This paper studies housing market outcomes of undocumented immigrants in the U.S. and explores the mechanisms behind the differential prices such immigrants pay for shelter. I show that undocumented renters pay a premium for housing relative to observably similar, documented, immigrant renters occupying similar housing. Building on theory and suggestive evidence that the premium is the result of search frictions, driven by fear of deportation, I employ a triple-differences strategy to evaluate the impacts of sanctuary city policies on housing market outcomes of undocumented immigrants. I find that sanctuary city policies, which limit immigration enforcement, reduce housing costs of undocumented renters, suggesting such policies mitigate search frictions for this group.

Keywords: Rental housing, undocumented immigrants, sanctuary cities

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1. INTRODUCTION

More than 1 of every 35 people living in the United States is an undocumented immigrant. Most estimates place the number around 11 million, about 10% greater than the population of the state of Michigan. Regardless of how policymakers weigh the personal welfare of individuals often described as "criminal" because of their immigration status, the presence of a group this size impacts the economy in a manner that has important welfare implications for everyone who calls the United States "home."

This paper studies the housing costs of immigrants in the United States and illustrates how such costs depend on legal status and local immigration enforcement policy. The findings suggest that the housing market faced by undocumented immigrants is one characterized by a pervasive fear of deportation. I show that undocumented immigrants pay a premium for rental housing, amounting to hundreds of dollars for the average household each year. Further, I find evidence that suggests the premium is largely attributable to search frictions that arise when undocumented immigrants fear deportation.

It is an established fact that immigration, in general, influences the cost of housing.[1] However, no study to date has isolated the influence of undocumented status on housing costs. If undocumented immigrants - who comprise nearly half of the non-citizen, immigrant population in the U.S. - navigate the housing market differently than other immigrants, then inference on housing market responses to immigration should account for this important heterogeneity in the immigrant population.

There is reason to believe that such heterogeneity plays a role in the housing market faced by immigrants. First, strict enforcement of current immigration policy may create search frictions for undocumented individuals as they navigate the housing market. Second, it is possible that landlords engage in price discrimination or implement their own policies (e.g. requiring background checks) that lead to the creation of separate housing markets for undocumented immigrants. In either of these cases, the general equilibrium implication is higher prices for housing (at least for

[1] See, for instance, Saiz (2003), Saiz (2007), and Saiz and Wachter (2011).

undocumented immigrants) and a socially inefficient allocation of the housing stock.

A primary focus of this study is to identify the existence of heterogeneity in rents by immigration status and shed light on the mechanisms responsible.[2] Making use of household level data from the American Community Survey and an imputation procedure to predict undocumented status, I show that undocumented immigrants pay higher rents than similar legal resident immigrants, and I provide evidence that search frictions are a driving force behind the observed premiums. If, as I propose, fear of deportation or formal participation in the housing market restricts search, then policies that alleviate such fears should work to mitigate the search frictions, resulting in a reduction in the rent premium. Exploiting geographic and temporal variation in the implementation of sanctuary city policies (that reduce fear of deportation among undocumented immigrants), I find that such policies work to equalize the rents of immigrants in multi-unit housing and reduce the fraction of income undocumented immigrants devote to rent by about 3.5%.

Finding that sanctuary city policies have such an impact on the housing market, I emphasize the importance of considering the housing market implications of immigration policy and enforcement. Immigration policy is frequently implemented to address concerns about crime, employment, and wages, and characterizations of the effectiveness of such policies often focus on these outcomes. My findings suggest that policymakers should also carefully consider the impacts of immigration policy on markets beyond those more traditionally addressed in studies of unauthorized immigration.

The paper is organized as follows. Section 2 establishes context and notes work relevant for the motivation of this study. Section 3 describes the data and procedures used to achieve a sample of the likely undocumented population. Section 4 presents descriptive findings of the relationship between undocumented status and rents. Section 5 makes use of sanctuary city policies in a triple-differences framework to provide quasi-experimental evidence for the undocumented status rent

[2] I focus exclusively on rents and not house prices for two primary reasons. First, due in part to their lack of access to home financing options, undocumented immigrants are nearly twice as likely to be renters as they are to live in owner-occupied housing (about 65% of undocumented immigrants are renters). Second, the period for which I have data is relatively short (6 years), and the analysis considers policy that, for most households, took effect no more than 3 years prior to when I observe them. Because renters move (re-optimize their housing consumption) more frequently, a short sample may reasonably capture policy effects on rents. The same cannot be said for home prices.

premium. Section 6 addresses identifying assumptions and tests the robustness of the findings. Section 7 concludes.

2. BACKGROUND AND CONCEPTUAL FRAMEWORK

2.1. Immigration, Policy, and Welfare

An oft-neglected variable in broad analyses of the welfare implications of immigration and immigration policy (especially compared to the attention given to employment or income) is housing. Saiz (2007) articulates this point well. He finds that an immigration inflow equal to 1% of the initial metropolitan area population is associated with increases in rents and housing values by roughly 1%. In his study, he notes that the impact of immigration on purchasing power through its effects on rents is “an order of magnitude bigger” than its effect through the labor market. Any discussion about the welfare implications of immigration that limits its focus to wages and crime (as is common today) neglects important other channels. Failure to consider impacts on the housing market would appear to be an especially consequential omission.

I contribute to the developing literature on the economic implications of unauthorized immigration by investigating and providing an initial characterization of the rental housing market faced by undocumented immigrants. Saiz faced data limitations that prevented him from thoroughly analyzing whether undocumented immigrants differentially influenced his estimate of immigration on area rents and home values. This is a possibility that warrants some attention. Are his results generalizable to all immigrant groups?

Borjas (2002) makes a point that national origin and residential location choices made by different immigrant groups are understudied but important explanatory variables in explaining housing market outcomes. If the broader (perhaps oversimplified) conclusion is simply that different immigrant groups navigate housing markets in different ways, then we should expect undocumented immigrants to have unique housing market outcomes. This seems especially likely when one considers what “residential location choice” means for individuals who are constrained by a lack of documentation. If undocumented immigrants are forced into less desirable, more costly housing, their immobility creates an inefficient allocation of the housing stock in the same way search frictions disrupt optimal labor market outcomes.[3] Amuedo-Dorantes, Bansak and Raphael (2007) concluded that the changes in immigration status (gaining documentation) through the Immigration Reform and Control Act (IRCA) of 1986 may have had positive effects on labor market efficiency by spurring wage growth and eliminating search frictions that impeded job mobility. I provide evidence that undocumented status obstructs similar potential improvements in the housing market.

Bohn, Lofstrom and Raphael (2014) find that the Legal Arizona Workers Act - an immigration policy intended to prevent the employment of undocumented immigrants - resulted in the displacement of Hispanic noncitizens with characteristics strongly associated with undocumented status. Relatedly, Hoekstra and Orozco-Aleman (2017) find that the announcement of Arizona's SB 1070 law (which would criminalize applying for or holding a job without legal status and drastically increase the power and responsibilities of law enforcement officers who encounter individuals suspected of lacking documentation) significantly reduced the number of undocumented migrants destined for Arizona. Miles and Cox (2014) evaluated the effect of ICE's Secure Communities policy (strengthening the relationship between ICE and local law enforcement to aid deportation efforts) on crime as it rolled out and found that it is, at best, only effective in inducing small reductions in the rates of burglary and motor vehicle theft and has no effect on violent crime. My evaluation of changes in rents in response to sanctuary city policies (generally, a locality's decision not to cooperate with ICE or the Secure Communities program) serves as another measurement of a potential consequence of such immigration policies.[4]

[3]Even if all of the immediate welfare loss from this allocation comes at the expense of undocumented immigrants, there is an abundance of evidence (though not always conclusive) that neighborhood effects, location, and housing affordability have highly consequential implications for aggregate welfare (Bezin and Moizeau (2017) and Chetty and Hendren (2018)).

[4]Increased immigration enforcement has been shown to have consequences for poverty rates of children with likely undocumented parents (Amuedo-Dorantes, Arenas-Arroyo and Sevilla (2018)), consumption (Dustmann, Fasani and Speciale (2017)), voter registration and civic engagement (Amuedo-Dorantes and Lopez (2017)), education (Kuka, Shenhav and Shih (2020)), and others (see, for example, Kubrin (2014)).

2.2. Search Frictions as a Mechanism

In a model of housing search, a prospective tenant's optimal strategy is characterized both by the expected match value of the available property and the cost of search.[5] If undocumented immigrants expect that, upon visiting a property, there is some non-negligible probability that they will be unable to rent the unit (e.g. because the landlord requires documentation that they do not possess and cannot obtain because of their status), making the realized value of the match 0, their expected return (in terms of improved utility from a successful match) to visiting the property is reduced and the likelihood that visiting the unit is their optimal decision falls.[6] In addition, higher costs of search will reduce the number of housing units visited under utility-maximizing behavior. Importantly, if the cost of search is higher for one type of renter (undocumented), then the optimal search behavior for that type will be different. In particular, if undocumented renters face higher search costs because, for example, they risk exposing their status in the process,[7] then the expected value of visiting the property must be sufficiently high to compensate for the additional cost imposed.[8]

In summary, if search costs are heterogeneous by immigration status, then search decisions

[5]See Carrillo (2012). While his model is designed to explain outcomes for owner-occupied housing, I argue that his findings are valid, at least qualitatively, for explaining rental market outcomes as well. He estimates large, non-pecuniary "visiting costs" in the search for housing. While the magnitude of this cost may be lower in the rental market, it is hard to argue that *no* visiting costs exist in rental markets.

[6]In Online Appendix II, I include images of sample tenant application forms (pulled from a simple internet search) that illustrate how certain information that undocumented immigrants can't provide may sometimes be a prerequisite for rental and other times may not be necessary. One sample application requires only an individual taxpayer identification number (which undocumented immigrants can possess). The other sample application requires a social security number, a driver's license, and bank information. These forms might be especially binding for renters seeking housing in apartment complexes that use one, centralized form for all applicants as opposed to single-unit homes where a landlord may be more inclined to make an exception for prospective tenants who insist on providing only an ITIN, for example.

[7]Notably, undocumented immigrants commonly drive unlicensed (often because they lack the documentation necessary to obtain a license). Therefore, any search (visit) that involves driving to a property risks a traffic stop that could be especially consequential for undocumented immigrants (i.e. they may be detained and held until deported). Recently, an increasing number of states have passed laws to allow undocumented immigrants to obtain driver's licenses. See Amuedo-Dorantes, Arenas-Arroyo and Sevilla (2020) for an evaluation of the labor market effects of such laws.

[8]Note that Lach (2007) argues that recent immigrants from the former Soviet Union to Israel have lower search costs and, thus, their immigration reduced the price of various products. In his study's context, the immigrants had authorization, were largely unemployed or out of the labor force (allowing more time for search), and possibly unaccustomed to price dispersion or variety in brands. In the present study, the immigrant group of focus lacks documentation, has high employment rates, and is composed mostly of individuals from countries with similar (capitalistic) market structures to the U.S.

are heterogeneous by immigration status. If expected match value is a function of the probability that landlords ask for documentation, then search decisions are heterogeneous by immigration status. In either case, because of their status, undocumented immigrants end up restricted to sub-optimal housing units.

2.3. Related Studies and Mechanisms

In one of few early applications of search models to the housing market, Courant (1978) develops a model that accounts for racial prejudice and demonstrates that if even some white sellers are unwilling to sell housing to black buyers, equilibria where black buyers pay more for housing are sustainable. I argue that, in the same way, if some landlords refuse to rent to undocumented immigrants (or prohibit them from renting, even unintentionally, through the documentation they require), an equilibrium may arise where undocumented immigrants pay more for housing than similar legal residents.

Several audits and correspondence studies have also confirmed the existence of search frictions in the housing market, empirically.[9] Often, these studies conclude that racial discrimination leads landlords to show fewer available units to prospective minority tenants, resulting in a restricted supply of available housing to these groups. Because prospective black tenants have fewer units made available to them[10] and receive fewer serious responses to their housing inquiries,[11] they must search much harder than prospective white tenants to find equivalent housing (Yinger (1986)). Additional search costs may yield an optimal stopping rule that leads minority tenants to settle for sub-optimal (lower-quality or higher-cost) housing. If undocumented immigrants face higher search costs, then they would similarly settle for sub-optimal housing.

An audit study conducted by Hanson, Hawley and Taylor (2011) investigates landlord discrimination in a more modern setting. One result of their study is that differential response to

[9] See Yinger (1986) and Page (1995) for audit studies. See Hanson et al. (2016)'s correspondence study for response to owner-occupied housing inquiries. See Phillips (2019) for a recent evaluation of relevant correspondence studies and their measured effects.

[10] See Yinger (1986).

[11] Hanson et al. (2016) show this for requests for information regarding loans for owner-occupied housing, at least.

housing inquiries is especially pronounced for landlords of apartments and minimal for landlords offering single family homes. The discrimination that results in their finding in the context of race may also be a barrier to search in the context of immigration status. Moreover, if single-unit housing is considered to be a more "informal" segment of the housing market, undocumented immigrants may be more likely to seek out single family homes in the same way they are more likely to participate in informal segments of the labor market.

The studies discussed above present examples of search frictions (or "barriers") in the housing market and illustrate the potential consequences of such inefficiencies. If search frictions are present or if the supply of available housing to undocumented immigrants is less than that of legal residents, then the market may be characterized by undocumented renters competing over a restricted supply of the housing stock, driving up prices paid and preventing sorting into preferred housing units. Conditioning on characteristics for housing quality, heterogeneous search frictions will manifest as premiums paid by the group subjected to them.

3. DATA

The unique circumstances that burden undocumented immigrants in their daily lives also present unique challenges for the researchers who would seek to inform the ongoing debate over the welfare implications of unauthorized immigration in the United States. Some of the earliest contributions made to the literature on estimating the undocumented population come from Robert Warren. Warren has published his methodology in some detail (Warren (2014)). Most widely accepted estimates of the size and characteristics of the undocumented population are based, at least loosely, on the general procedure proposed by Warren and Passel (1987). This includes the Migration Policy Institute (MPI), Pew Research Center, the Center for the Study of Immigrant Integration (CSII) at USC, and even the Department of Homeland Security (DHS). Broadly, the process is to start by creating three categories: citizen, legal permanent resident (LPR), and undocumented. Citizen status is assigned to any individual born in the United States.[12] The rest of the process is

[12] Usually, naturalized citizens are grouped together with LPR's. The analysis in the text of this paper excludes naturalized citizens from any immigrant category, but following a conversation with Emily Owens, who pointed out

dedicated to sorting the remaining individuals into the LPR or undocumented category. All sources listed above begin with a procedure sometimes referred to as "logical edits," though, exactly what the logical editing procedure entails varies by researcher and by data available. I apply logical edits that closely resemble those Borjas (2017) applies to CPS data.[13]

To ensure a sample large enough to capture undocumented immigrants, I make use of data from the American Community Survey (ACS) provided by IPUMS. The goal of this editing procedure is to "rule out" immigrants as undocumented by examining characteristics that individuals could only have if they were legal residents. Any individual satisfying at least one of the following conditions (and not already assigned citizen status) is classified as a legal permanent resident (LPR):

- Arrived in the United States before 1980[14]
- Is a veteran or currently serving in the U.S. military
- Received public health insurance, Medicaid, Medicare, or VA insurance
- Received any welfare payment, SSI, or Social Security Benefits
- Works in government or in an occupation that requires licensing
- Born in Cuba[15]
- Received food stamps/SNAP[16]
- Arrived in the U.S. as an adult and currently enrolled in undergraduate, graduate, or professional school[17]
- Works in a computer-related occupation, possesses at least a bachelor's degree, *and* has been in the U.S. for no more than 6 years[18]
- Spouse is classified as LPR or citizen

that non-citizen survey respondents may reasonably believe that they are naturalized citizens (perhaps, based on misconceptions of the process), this categorization is included in Appendix F as a robustness test. Results under this categorization are discussed in the appendix, but the main findings are similar regardless of categorization choice.

[13] I also add a logical edit to account for H-1B visa recipients. Borjas and Cassidy (2019) add such an edit in their more recent paper incorporating an imputation for undocumented status. Thus, the imputation procedure I implement may be more closely related to Borjas and Cassidy (2019) than Borjas (2017) where it is initially implemented.

[14] These individuals are assumed to have achieved legal status through IRCA 1982.

[15] Individuals born in Cuba are likely to be refugees.

[16] Since undocumented parents of U.S. citizens may be eligible for food stamps on behalf of their children, the only time I apply this edit is if the indicator for whether someone in the household received food stamps is true *and* there is only one individual in the household.

[17] This is to account for student visa holders (Pastor and Scoggins (2016)).

[18] This is to account for individuals on H-1B visas.

After applying these edits to the 2017 ACS data, my estimate of the undocumented population stands at roughly 11.1 million. By comparison, Pew's 2017 estimate is 10.5 million, the MPI's 2016 estimate is 11.3 million, and the Center for Migration Studies' (Robert Warren) estimate for 2017 is just over 10.6 million. I allow for this relatively small overestimate of the undocumented population. Borjas (2017) also elects to go no further than the logical edits.[19]

I run the same algorithm on the ACS data from 2012 to 2016.[20] Additional details on how estimates from the imputation procedure I implement compare with other estimates of the undocumented population are presented in Appendix A. Broadly, my estimates, as expected, indicate a slight overestimate of the undocumented population. Overestimates are not substantially troublesome as they would indicate that my results understate the true effects of undocumented status (i.e. the "treatment" group is contaminated). Thus, if overestimation is an issue and the subset of individuals I have classified as "undocumented" contains some legal resident immigrants, the estimated effects of undocumented status are biased towards zero and should be interpreted as lower bounds.

At this point, all individuals have been assigned a status. Because my outcome of interest is the amount a household pays for rent, I aggregate a number of personal characteristics to the household level and reduce my sample so that the unit of observation is a household. In the choice specifications, I categorize a household as an "undocumented household" if the household head is undocumented. Robustness tests left to the appendix include alternative sample restrictions and

[19]The list of edits I apply differ from Borjas (2017) in three ways. First, since Borjas makes use of CPS data, he has access to a variable indicating whether an individual resides in public housing or receives rental subsidies. The ACS does not contain this information, so I am unable to make a logical edit based on receipt of housing assistance. Second, Borjas does not use receipt of food stamps as a logical edit. I do so, conservatively. Lastly, I attempt to rule out student visa holders. Borjas applies no such restriction. Additionally, Borjas (2017) applies no edit to account for H-1B recipients. In a more recent paper, Borjas and Cassidy (2019) add a similar logical edit and discuss the consequences of its exclusion from the 2017 study.

[20]Some geographic boundaries change from the 2011 data to the 2012 data. Therefore, I primarily rely on data from 2012 and later for the purpose of greater geographic precision. Additionally, the Secure Communities program that established the connections and federal oversight that sanctuary city policies are often designed to restrict only finished rolling out by the beginning of 2013, meaning there were very few sanctuary city policies in existence (or even conceived of as necessary) prior to this time period. Nonetheless, robustness tests (with additional years of data and less geographic precision) are presented in Online Appendix V. 2017 is chosen as the final year due to a decision by ICE in early 2017 to cease reporting jurisdictions that restrict cooperation with the agency (information that I rely on later for the identification of sanctuary cities).

alternative definitions of "undocumented household."[21] I then impose a number of sample restrictions with the primary goal of minimizing the frequency with which legal residents are categorized as undocumented.

To ensure that the counterfactual immigrant group (LPR's) shares similar characteristics with undocumented immigrants and to account for the fact that undocumented immigrants cannot have a $years\ in\ U.S.$ term greater than 37,[22] I drop any immigrant who has lived in the U.S. for more than 37 years. I also exclude any immigrant who has lived in the U.S. for less than 1 year. This exclusion ensures that any visitors or very temporary residents do not drive results.[23] I also exclude individuals with more than a bachelor's degree (to address the abnormal number of post-secondary teachers and scientists classified as undocumented likely because they are missed by the imputation procedure) and anyone currently enrolled in school (living circumstances of typical college students are arguably quite distinct from renters, more broadly). Lastly, I exclude all immigrants from a handful of countries where it is exceptionally difficult to determine immigration status. Singapore and Chile have an agreement with the U.S. that guarantees at least 6,800 H-1B visas (5,400 and 1,400, respectively) are available exclusively to individuals from these countries each year. Burma (Myanmar), Bhutan, Democratic Republic of the Congo, Iraq, and Syria had extremely high numbers of refugees relative to the number of total immigrants in the period of analysis. Therefore, all immigrants from these 7 countries are excluded from the sample. After all edits are applied, the sample is restricted to counties that are identified in the ACS microdata and in which at least 25 undocumented households are observed each year.[24][25] Descriptive statistics by immigrant status are presented in Table 1. Additional descriptive statistics are left to Appendix A. Altogether, I am

[21] Namely, see Appendix B, Appendix E, and Appendix F.

[22] See the logical edit regarding IRCA.

[23] Additionally, many questions in the survey ask respondents for information about the previous year (e.g. individuals are asked what their total income was in the past 12 months). Immigrants who have just moved to the U.S. will then, be offering responses based on their behavior outside of the U.S.

[24] This is to minimize the effect of erroneous assignment of any single household's immigration status and support the asymptotic assumptions of OLS estimators at the county level, where sanctuary city policies tend to go into effect. The inclusion of these counties, though, does not meaningfully change results.

[25] The only relatively large counties excluded by these restrictions are those in the Denver area. In this area, PUMA's (Public Use Microdata Areas) frequently cross county lines, and since the PUMA (and state) of residence is the only geographic identifier initially provided in the data, it is often impossible to know whether a household in one of these PUMA's lives in "county A" or "county B."

left with just over one million observations of renter households in 77 counties over the period of 6 years.[26]

The other data source I use is information published by ICE on sanctuary city policies. The data from ICE is a list of localities that have made public statements or enacted policies affirming an unwillingness to cooperate with ICE in at least some circumstances. ICE ceased their updates to the report in early 2017, and the final report contains information that was current as of February 2017. Since the ACS data I use is for the period of 2012-2017, the report covers policies enacted for every year in my sample (excluding a few months in 2017, but as I describe later, policies enacted in the latter half of the year may not have observable effects on rents until the following year, anyway). I have included the first page of the list of uncooperative jurisdictions and a reference to the full report in Online Appendix I.

4. THE UNDOCUMENTED STATUS RENT PREMIUM

In Section 4, I first establish that immigrant renters with undocumented status pay more for housing than comparable legal residents. Supplementary analyses shed light on the mechanisms that may be responsible for the observed premium. Building on these descriptive results, in Section 5 I provide evidence of the existence of the undocumented status rent premium using a quasi-experimental, triple-differences empirical strategy.

4.1. Empirical Framework

I run regressions on the ACS data to determine whether undocumented immigrants pay more than legal residents do for similar housing. To mitigate concerns that an observed premium is the result of discrimination against or differential behavior among immigrants in general, restrict my sample of renters to non-citizens. Thus, legal resident immigrants (LPR's) serve as the comparison

[26] I do make one assumption about Miami-Dade county. There is a PUMA that crosses into Broward county to the west. Given that Broward county has an estimated population of under 80,000, I have assumed that any observed household in this PUMA that crosses into Broward county is a household that is in the Miami-Dade portion of the PUMA.

group.[27] The specification is described by equation (1).

$$Rent_{ipt} = \beta_1 undocumented_i + X_i\theta + \alpha_p + \gamma_t + \varepsilon_{ipt} \tag{1}$$

$Rent_{ipt}$ is gross monthly rent for household i in PUMA p in year t.[28] $Undocumented_i$ is an indicator that takes value 1 if the householder is undocumented and 0 otherwise. PUMA (public use microdata area, the lowest level of geography publicly available in the household-level data) and year fixed effects are represented by α_p and γ_t, respectively. X_i is a vector of household-level controls that includes age of householder, age squared, marital status, gender, household income, number of workers in household, number of people in household, number of bedrooms, number of rooms, and dummies for year built (intervalled) and time in residence (intervalled). Importantly, X_i also includes controls for whether the household is living in multi-unit housing ($multi\text{-}unit_i$) and how many years the householder has spent in the U.S. ($years\ in\ U.S._i$).

If there are no factors correlated with undocumented status that also independently affect rent beyond the vector of controls (X_i) and PUMA and year fixed effects, then β_1 can be interpreted as the causal effect of undocumented status on rent. A positive β_1 indicates that undocumented immigrants pay a premium for rental housing. A premium is consistent with the story that high search costs, lower expected returns to searching, and restricted supply of housing available to undocumented immigrants limit their ability to sort into optimal housing, causing them to pay more for housing than they would if they had legal resident status.

Next, to support the argument that search frictions are present and driving the observed premium, I include a variable for the interaction of years in the U.S. with undocumented status as well as an indicator for whether the household resides in multi-unit housing and has undocumented

[27] In Appendix B, I run similar regressions on the full sample.
[28] All dollar amounts have been adjusted to 2010 dollars.

status. This is specification (2) and the choice specification for this section of the paper.

$$\begin{aligned} Rent_{ipt} = &\ \beta_1 undocumented_i + \beta_2 years\ in\ U.S._{i} + \beta_3 multi\text{-}unit_i \\ &+ \beta_4(undocumented_i \times years\ in\ U.S._{i}) + \beta_5(undocumented_i \times multi\text{-}unit_i) \\ &+ X_i\theta + \alpha_p + \gamma_t + \varepsilon_{ipt} \end{aligned} \quad (2)$$

If search frictions are at work, we might expect to see higher premiums for undocumented renters of multi-unit housing. First, just as informal participation in the labor market may appear safer to undocumented immigrants, less formal participation in the housing market (e.g. negotiating with a single landlord who owns a couple of homes instead of dealing with an apartment complex) may appear safer, increasing the willingness of undocumented renters to search for optimal single-unit housing relative to multi-unit housing or leading them to restrict their choice set of potential rental housing to exclude units in apartment complexes. Second, if undocumented renters view the market for single-unit housing as less formal, they may expect landlords of these units to be more flexible about what documentation they require, decreasing the probability that visiting the property or inquiring further about the unit is futile and increasing their expected return to seeking more optimal housing of this kind.

Third, if apartment complexes are more likely to ask for formal documentation or run background checks,[29] then there is a very real supply restriction that undocumented renters face for these units, specifically. A restriction on the supply of rental housing raises rents paid.[30] Units that are subject to greater supply restrictions should have higher observed premiums.

In specification (2), then, we would expect a positive coefficient on the interaction of the indicator for undocumented status and the indicator for multi-unit housing.[31] In other words, if no

[29]Rental law requires that any documentation demanded of one applicant must also be demanded of all applicants. The implication is that apartment complexes may be more likely to have in place a standard procedure (standard set of required documents) for determining applicant eligibility because if they don't, they risk violating the Fair Housing Act.

[30]Depending on the elasticity of demand for these units, housing quantity/quality should also decrease if supply is reduced. The covariates in the regression account for housing characteristics, though, so the regression results answer the question of, "how much more do undocumented renters pay for housing with the same characteristics (compared to similar, documented renters)?"

[31]The multi-unit housing variable essentially captures whether the household lives in an apartment or home.

friction like I have described exists, then the premium that undocumented renters pay for multi-unit housing should be no different than the premium they pay for single-unit homes (i.e. $\beta_5 = 0$).

Additionally, search frictions might be expected to result in higher premiums for undocumented immigrants who are least equipped to navigate the housing market and have had the least amount of time (fewest chances) to engage in any amount of search for housing. As undocumented immigrants adjust to living with their status, they learn of the housing available to them and are able to sort into more appropriate units. If this is the case, we should expect to see the premium fall as undocumented renters spend longer in the U.S. Then, the term in specification (2) that captures the effect of the interaction of undocumented status and years in the U.S. (β_4) would be negative (the premium, or effect of undocumented status, diminishes over time).[32]

Other than the (uninteracted) $years\ in\ U.S._i$ and $multi\text{-}unit_i$ variables (which are now more explicitly included in the specification), all other controls remain the same as in specification (1). Note that the coefficient on the $years\ in\ U.S.$ term (β_2) accounts for the trend in what an immigrant (of either status) pays for rent the longer they stay in the U.S. A negative coefficient is consistent with the story that immigrants need time to adjust to a new housing market before being able to locate more affordable housing. A negative coefficient on the (uninteracted) indicator for multi-unit housing (β_3) simply illustrates that renting an apartment unit is cheaper than renting a home. The additional parameters of interest in specification (2) are β_4 and β_5.

4.2. Results

Results from specifications (1) and (2) are presented in Table 2. Column 1 includes no controls (this effectively shows the raw, average difference in rents paid by immigrants of different statuses if one ignores omitted variable bias). Column 2 adds fixed effects (to show rent differences after accounting for year and location). Column 3 includes all controls, corresponding exactly to specification (1). Column 4 includes the interaction terms and is the choice specification,

[32]One drawback, though, is that a variable that captures years spent in the United States may be capturing more than just experience in the U.S. housing market (e.g. it will also be correlated with changing immigrant characteristics over time). If this is the case, then $years\ in\ U.S.$ remains an important control variable, but the interpretation of its effect (and the effect of its interaction with undocumented status) becomes less clear.

corresponding exactly to equation (2).

As expected, column 3 provides evidence that undocumented immigrants pay a premium for rental housing. In column 4, the positive coefficient on the $multi\text{-}unit_i$ interaction with undocumented status (β_5) indicates that multi-unit housing, especially, is more expensive for undocumented households. This is consistent with the idea that apartment complexes are more likely to ask for documentation or conduct background checks, restricting the supply of apartments that undocumented immigrants have access to. The negative coefficient on the interaction of years in the U.S. with undocumented status indicates that the premium undocumented immigrants pay decreases as they spend more time in the U.S., consistent with the story that the premium is the result of search frictions that diminish over time. However, the economic significance of this term is debatable (a 62 cent reduction in monthly rent for every year spent in the U.S. is an effect size dwarfed by the observed effect of multi-unit housing, for example).

The magnitude of the coefficients in Column 4 of Table 2 suggests that the premium paid by undocumented immigrants is primarily driven by a premium for multi-unit housing. Column 4, the choice specification, indicates that undocumented renters pay a baseline premium of around \$14 per month for housing. The more significant finding (both economically and statistically), is that there is an additional premium of \$47 per month for undocumented renters of multi-unit housing. In other words, undocumented renters of multi-unit housing spend an additional \$700 on housing per year because of their undocumented status. If the theory that this premium is the result of search frictions effectively restricting the supply of housing (especially, multi-unit housing) to undocumented immigrants is correct, then the results of alleviating the search friction should be especially evident for renters in multi-unit housing. The next section provides quasi-experimental evidence from triple-differences specifications to assess this possibility.

5. SANCTUARY CITIES

I now turn my focus to the effects of sanctuary city policies. Section 5.1 provides context and motivates the use of sanctuary city policies to further investigate the relationship between undocumented status and rents. Section 5.2 formalizes the empirical strategy. Section 5.3 presents baseline results for the effect of sanctuary city policies on the rent premium. Section 5.4 illustrates that sanctuary city policies may affect rents through more than one channel. To address this, I show results for the effects of sanctuary city policies on both rents and rent as a fraction of income to account for systematically different incomes of undocumented immigrants in sanctuary cities.[33] Section 6 validates the parallel trends assumption necessary to interpret the results as causal and discusses other robustness tests.

5.1. Background and Conceptual Framework

Between 2008 and 2013, Immigration and Customs Enforcement (ICE) gradually implemented a program called Secure Communities, creating a direct connection between the agency and local law enforcement who may come into contact with undocumented immigrants. With these connections in place, ICE would, in principle, know the whereabouts of any undocumented person booked for any crime anywhere in the United States (so long as they were being detained). Beyond information sharing between ICE and local law enforcement, ICE could also issue "detainers," which are orders (or requests, depending on legal interpretation) for local jails to detain individuals who ICE believed may be unauthorized immigrants, allowing the agency time to question and deport the individuals.[34]

Following the roll out of Secure Communities, local governments, police departments, and jails began enacting policies that restricted or prevented compliance with the program. Such areas have become colloquially known as "sanctuary cities." Sanctuary cities are not well-defined ("sanctuary city" is not a federally recognized designation, but sanctuary jurisdictions are characterized by varying degrees of non-compliance with ICE). For the purpose of this study, a sanctuary city is any jurisdiction that appears on ICE's list of jurisdictions that have enacted policies which

[33] Appendix C presents results for effects on movement, and Online Appendix IV presents results for the policies' effects on select other outcomes.

[34] Secure Communities was technically suspended in November, 2014. However, within 2 months, it was replaced with the Priority Enforcement Program (PEP), which was functionally almost identical to Secure Communities. For a more detailed history of the Secure Communities program, how it operates, and a summary of the effects of immigration policy like Secure Communities, see Kubrin (2014).

restrict cooperation with the agency. Such policies range in scope from a sheriff's public statement of noncompliance with ICE detainers to a change in a jail's policy about continuing to hold arrested individuals beyond a specified time period (regardless of ICE's demands) to local law prohibiting ICE's detention orders from being honored at all.

It is difficult to objectively measure the relative "intensity" of any one sanctuary policy, but importantly, all such policies were public demonstrations of local authorities' refusals to use local law enforcement to help ICE in deporting undocumented immigrants. Even a policy that has no demonstrable effect on actual deportation rates may affect behavior of undocumented immigrants through a change in their *perceived* safety, especially given the public nature and media attention to many of these policies.

By reducing the likelihood (or even just the believed likelihood) that interaction with law enforcement would result in deportation, sanctuary city policies can reduce search frictions. For example, undocumented immigrants may fear that the application process for a new apartment will reveal their status. Anything from going through a background check to driving (usually unlicensed) to view available units imposes an additional cost on undocumented immigrants in the form of deportation risk. In the absence of a sanctuary city policy, the costly risk incurred in the search for new housing may preclude undocumented immigrants from optimizing their housing consumption, resulting in the rent premium they pay. In sanctuary cities, the probability of deportation as a result of minor infractions or having one's status revealed is reduced. In this way, sanctuary cities reduce the expected cost of increased participation in the housing market.

If fear of deportation raises rents by creating search frictions (as the previous section suggests), then mitigating that fear should alleviate the search frictions and reduce the observed premium. Therefore, following the enactment of sanctuary city policies, we would expect to see a reduction in the premium paid by undocumented immigrants through this channel. In this section, I seek to answer two questions. First, are the results from Section 4 supported by evidence from a quasi-experimental research design? Second, what effect have sanctuary city policies had on local rents? The triple-differences specification I employ can provide meaningful insight into the impact of immigration policies and, at the same time, serve as a stronger test of the hypothesis that costly searches for housing created by a locality's response to undocumented immigration drive housing market outcomes.

5.2. Triple Differences Formulation

I have digitized the most recent file made available by ICE, listing localities that have enacted policies or made statements restricting cooperation with the agency. This file covers all policies and statements made through February 2017.[35] The file includes the month, year, and location of each policy. Since I have both the month and the year in which each policy was enacted but households in the ACS data are observed only annually (i.e. a response in the 2017 data may have been recorded at any time during 2017, but only the year is observable in the public data), I adjust the year of policy enactment to the next year if the policy was enacted in the months of July through December.[36] [37] Equation (3) incorporates the policies in a triple-differences framework.

$$\begin{aligned} Rent_{ipt} = {} & \alpha_p + \gamma_t + \beta_1 undocumented_i + \beta_2 treat_{pt} + \beta_3 (treat_{pt} \times undocumented_i) \\ & + \beta_4 (PUMA_p \times undocumented_i) + \beta_5 (Year_t \times undocumented_i) + X_i \theta + \varepsilon_{ipt} \end{aligned} \tag{3}$$

$Undocumented_i$ is an indicator for whether individual i is undocumented or not. $Treat_{pt}$ takes value 1 if PUMA p has a sanctuary city policy active in year t. Also included are PUMA by immigration status and year by status fixed effects to account for baseline differences in the

[35] Fortunately, all but a handful of these policies are enacted at the county (or state) level, and those that are enacted at the city level occur in cities that are identified by my subsample from the ACS. Therefore, I can identify which individuals within my set of identified counties live within the "treated" area. Ultimately, the main sample for this section consists of 70 unique counties, 27 of which ever become sanctuary jurisdictions during the period of analysis and 3 of which contain some residents that are treated because a partially overlapping city became a sanctuary jurisdiction.

[36] For example, King County, Washington enacted an ordinance in September 2014. Since the enactment occurred in September, the policy year is coded as 2015.

[37] The statewide policy enacted by California seems to merely give *permission* to decline detainers issued by ICE. Arguably, any jurisdiction was already able to decline detainers at their discretion (there remains legal debate regarding this point). Since many California counties enact their own, separate policies around the same time and since the state-level policy arguably changed very little, I only assign "treated" status to counties in California that enact their own policy in addition to the state's policy. Thus, while some counties in California could be considered "treated" because of the statewide policy, they are considered untreated in my sample unless they enact a policy or make a statement of their own as well. Note that Online Appendix VI presents results where California is excluded from the analysis. The results are robust to the state's exclusion.

premium by PUMA and national trends in the premium over time, respectively. Note that because I have included the $PUMA \times undocumented$ fixed effects, β_1 only captures the premium from the excluded category (PUMA) in the fixed effects. Thus, this specification allows for a different baseline premium from undocumented status in each PUMA. Therefore, the effect of the treatment is interpreted as the average change in premiums across all PUMA's when they experience the treatment.[38] Lastly, the PUMA and year fixed effects and the household-level controls from Section 4 are included as well.

The parameter of interest is β_3, which captures the difference that undocumented immigrants pay for rent following the enactment of a sanctuary city policy. In other words, it captures the difference in rents of undocumented immigrants (v.s. legal resident immigrants) in locations that enact sanctuary policies (v.s. locations that don't or haven't yet) after (v.s. before) they are enacted. In this way, $treat_{pt}$ can be thought of as the more standard difference-in-differences term of interest as it captures the difference in rents (for all immigrants) between pre and post periods in locations that get a sanctuary city policy v.s. those that do not. The interaction with undocumented status adds the third difference.

A negative and significant β_3 would indicate that the premium paid by undocumented renters is reduced following the enactment of a sanctuary city policy, consistent with the story of alleviated search frictions. A positive β_3 may indicate an increased rental price paid as a result of increased demand for housing among undocumented people in these locations or some other factor.[39]

[38]Recall, Public Use Microdata Areas are (usually) smaller geographic boundaries that generally fit entirely within counties with at least 100,000 people. Therefore, when a policy goes into effect at the county level, I can conclude that the PUMA's that comprise the county experience treatment. IPUMS does this identification when preparing their ACS data. Thus, PUMA fixed effects serve as a more geographically precise alternative to county fixed effects, *and* treatment can still be determined at the PUMA level.

[39]One might argue that locations with sizable undocumented populations will experience lower rents as low-skilled immigration can reduce prices through reduced labor costs (Cortes (2008)). This possibility is unlikely to affect my empirical design. First, the triple-differences design derives its validity from exploiting *differences* in rents. Any effects of a location's initial, existing undocumented population on rents will be captured by fixed effects. Second, if sanctuary city policies are, in fact, implemented in a way that is correlated with changes in the undocumented population and the undocumented population affects rents because their labor supply reduces production costs for new housing, the effect on rents through this channel would apply to all immigrants in the location (meaning such an effect would be captured by the baseline treatment term (β_2), not the parameter of interest where treatment status is interacted with undocumented status (β_3)), and such an effect would have to manifest in the short time span (in terms of housing supply adjustments) between the implementation of the policy and the end of my sample (generally, no more than 3 years).

Equation (4) adds an interaction between the treatment variable and the indicator for whether the household lives in multi-unit housing and includes its interaction with the indicator for undocumented status. Thus, β_7 represents a heterogeneous treatment effect in the triple-differences specification. It captures the effect treatment (the sanctuary city policy) has on rents, specifically through the channel of its effect on multi-unit housing, specifically for undocumented immigrants. A negative β_7 is consistent with the idea that undocumented immigrants, on average, pay more for multi-unit housing like apartments *because* the restricted availability (or perception that such units are less available to undocumented immigrants) makes the costly search for housing in these units prohibitively high. Then, sanctuary city policies that reduce those search costs work to reduce the resulting premium specific to multi-unit housing. In other words, undocumented immigrants in sanctuary cities may be more inclined to approach potential landlords of multi-unit complexes now that the cost of formally interacting with anyone who might ask about documentation is reduced.

$$\begin{aligned} Rent_{ipt} = {} & \alpha_p + \gamma_t + \beta_1 undocumented_i + \beta_2 treat_{pt} + \beta_3 (treat_{pt} \times undocumented_i) \\ & + \beta_4 (PUMA_p \times undocumented_i) + \beta_5 (Year_t \times undocumented_i) \\ & + \beta_6 (treat_{pt} \times multi\text{-}unit_i) \\ & + \beta_7 (treat_{pt} \times multi\text{-}unit_i \times undocumented_i) \\ & + X_i \theta + \varepsilon_{ipt} \end{aligned} \tag{4}$$

The multi-unit interaction term ($treat_{pt} \times multi\text{-}unit_i \times undocumented_i$) is important for another reason. Sanctuary cities are expected to offset or eliminate existing rent premiums faced by undocumented immigrants. Therefore, β_3 will capture the extent to which sanctuary city policies reduce the premium for all housing units (about \$14 per month if the descriptive results from Section 4 are to be believed), and β_7 will capture the extent to which the policies further reduce the premium, specifically for multi-unit housing (about \$47 per month by Section 4). However, if sanctuary cities affect undocumented immigrants' rents through a generalized shift in housing demand (e.g. through increased incomes) or a similar channel, that effect should be captured entirely by β_3 unless the other hypothetical channel through which sanctuary cities affect rents is one that also differentially affects multi-unit housing. Thus, in the case that sanctuary cities also shift housing demand of undocumented immigrants, generally, only the measurement of β_3 would be contaminated by such an effect. So, the observed β_3 would be interpreted as the result of the combination of both effects (generally increased housing demand and alleviation of a search friction), but β_7 would continue to be interpreted as the result of alleviated search frictions alone.[40]

5.3. Baseline Results for Rents

Results from equations (3) and (4) are presented in Table 3. The first 3 columns exclude the $(PUMA_p \times undocumented_i)$ and $(Year_t \times undocumented_i)$ fixed effects to allow for a meaningful interpretation of the first-order effect of undocumented status (β_1). Columns 4 through 6 include the interacted fixed effects to account for cross-PUMA and cross-year variation in the baseline (pre-treatment) rent premium to undocumented status.[41] These are the preferred specifications for this section, but results are consistent across columns.

Columns 1 and 4 do not allow for any heterogeneous effects by undocumented status. Column 2 (5) illustrates (again) that first order effects of undocumented status on rent appear to be driven by renters of multi-unit housing. Columns 2 and 5 correspond to equation (3). Columns 3 and 6 correspond to equation (4) and allow for heterogeneous effects of the treatment on what undocumented immigrants pay for renting multi-unit housing, specifically.

In column 6 (the choice specification for this section), the coefficient on $undocumented \times multi\text{-}unit$ suggests undocumented immigrants pay a \$40 (monthly) premium for multi-unit housing, consistent with the findings in Section 4.2. The coefficient on $treat \times multi\text{-}unit \times undocumented$

[40] As a thought experiment, suppose that sanctuary cities increase undocumented immigrant income (which is positively correlated with rents) and that this is the *only* channel through which sanctuary cities affect rents. In this case, β_3 will be positive, and unless, for some reason, the policy causes income to change differentially depending on what kind of unit one lives in, β_7 will be zero. Thus, while an income effect would bias β_3, β_7 will be free of such bias.

[41] Inspection of the data reveals a number of PUMA's where the difference in average rents of undocumented immigrants and legal residents is substantially higher (or lower) than the average premium. To account for these outliers and capture the true change in the baseline premium following the enactment of a sanctuary city policy, it is appropriate to allow different first-order effects (different baseline premiums) for each PUMA. Then, treatment will capture the average change across all treated PUMA's relative to untreated.

indicates that once a sanctuary city policy is in place, though, undocumented immigrants pay $45 less for multi-unit housing. In other words, the rent premium specific to multi-unit housing is eliminated in sanctuary cities.[42] Also, note that the coefficient on $treat \times multi\text{-}unit$ is insignificant as would be expected, since these policies should have no effect (at least, directly) on the rents legal residents pay for multi-unit housing, specifically. The null effect observed on the baseline treatment indicator (the effect of a sanctuary city policy on rents on immigrants of any status) is similarly unsurprising.[43]

The consistently positive coefficient on the triple difference term ($treat \times undocumented$), however, would suggest that rents of undocumented immigrants, in general (i.e. for any type of housing - single-unit homes or multi-unit apartments), rise following the enactment of a sanctuary city policy. In fact, it would appear that regressions that disallow treatment to vary by whether one lives in multi-unit housing mask the heterogeneity in the effect of treatment. While columns 1-2 and 4-5 show a smaller (and insignificant in 4-5) effect of treatment on rents, columns 3 and 6 suggest that such an effect arises from offsetting forces; undocumented renters in sanctuary cities pay more for housing in general, but they no longer pay a premium specific to multi-unit housing.

If sanctuary cities eliminate rent premiums specific to multi-unit housing, as we would expect if the policies reduce search frictions that had restricted the effective supply[44] of such housing to undocumented immigrants, why do undocumented immigrants pay more for rent in sanctuary cities? As I discuss in the remainder of this section, sanctuary cities may alleviate search frictions in both the housing market and the labor market. Alleviated frictions in the housing market would result in the reduction of rent premiums (as evidenced by a negative β_7). Alleviated frictions in the

[42] With a p-value over 0.72, the hypothesis that undocumented renters of multi-unit housing in sanctuary cities pay the same as their legal resident counterparts (i.e. that the -45.20 and 40.63 simply offset each other and these undocumented renters aren't actually paying *less* for multi-unit housing, now) cannot be rejected at any conventional levels.

[43] *A priori*, the direction of the effect captured by this coefficient is unclear. A baseline increase in rents among all immigrants may be reasonable if these policies induce additional demand for the same units legal residents rent. On the other hand, a baseline decrease in rents among all immigrants could arise from general equilibrium effects if these policies increase the efficiency of the housing market. There may also be no effect of these policies on baseline rents of all immigrants if sanctuary city policies truly only matter for the outcomes of undocumented immigrants.

[44] The "effective supply," in this case, is the stock of units for which undocumented immigrants are willing to search, given the additional search costs imposed because of their status.

labor market could raise incomes, raising demand for housing (as evidenced by a positive β_3).

5.4. Effects of Sanctuary Cities Through Other Channels

It is important to consider other implications of sanctuary city policies and how those effects may impact the analysis of the policies' effects on rents. As previously mentioned, Amuedo-Dorantes, Bansak and Raphael (2007) found that awarding documented status to immigrants can alleviate frictions in the labor market and increase income. While sanctuary cities do not award legal resident status to undocumented immigrants, it may be reasonable to think that, if they reduce search frictions in the housing market, they would also reduce search frictions in the labor market. If incomes of undocumented households in sanctuary cities rise, these renters may seek out higher quality, more expensive housing to satisfy their new, expanded budget.[45]

Empirically, it is possible to eschew any effect of increased income on rents by redefining the outcome variable. The appropriate outcome of interest to capture the effects of sanctuary cities on rental housing (net of the effects through increased income) may not be gross rents, but rather, rent as a fraction of household income. This outcome implicitly accounts for any shifts in demand for rental housing driven by changes in income and may be more consistent with the story that undocumented status forces immigrants into suboptimal housing units (i.e. they must allocate more of their income to rent than they otherwise would if they had lower search costs or access to the same set of units that other residents can access).[46]

For the analysis that follows, I must add further restrictions to the sample of renters. First, as household income will be in the denominator of the "rent as a fraction of income" ratio, I exclude any household with zero reported household income. Second, to address extreme outliers, I exclude any household that has reported gross rent or household income below the 1st percentile or above

[45] Additionally, Dustmann, Fasani and Speciale (2017) find that undocumented immigrants in Italy have lower levels of consumption than authorized immigrants, even conditional on income (and notably, housing is the good with the largest observed difference in expenditures). They also find that a higher probability of deportation significantly lowers consumption. These results suggest that a policy that reduces deportation risk could increase the (housing) consumption levels of affected undocumented immigrants.

[46] Note also that if sanctuary cities affect income and income affects rent, then income is a bad control in the rent regressions in Section 5.3. The result is a positively biased β_3. There is no such bad control when the dependent variable is, instead, rent as a fraction of income.

the 99th percentile. Then, for simplicity and to limit the scope of my analysis to more standard rental households, I exclude any household that spends more than 100 percent of its household income on rent.[47]

Finally, to test the robustness of my findings and ensure they are driven by renters who are truly undocumented (further testing the reliability of the imputation procedure), I construct a number of additional subsamples on which I repeat all of the analysis. I present 3 separate subsamples (in addition to the "unrestricted" sample just described).[48] The first subsample attempts to address the issue of inordinately high incomes of some (often classified as undocumented, perhaps erroneously) immigrants more directly. For each household, I determine the breadwinner and the income of that individual. I then exclude households where the breadwinner's income is above the 90th percentile or below the 10th percentile. The second subsample restricts to Hispanic households only, which addresses anomalies in the number of undocumented immigrants from European or some Asian countries, for example. The final subsample presented within the text restricts to renters where the household head has no more than a high school diploma or GED, which should account for remaining immigrants on H-1B visas.[49]

5.4.1. Effect on Rent and Evidence of an Income Effect

First, Table 4 presents results from running the regressions given by equations (3) and (4) on the new samples to confirm that the findings in Table 3 are robust to the additional sample restrictions that will ultimately be necessary to evaluate sanctuary cities' impact on rent as a fraction of income. "Unr" (unrestricted) refers to the sample that only excludes households with zero income, rent as a fraction of income greater than 1, or rent or income below the 1st or above the 99th per-

[47]There are a number of explanations for why a household's rent expenditure may exceed its income. First, the ACS survey asks individuals to report their total income over the last 12 months. If individuals have recently taken a job that pays more or if more people in the household only recently began working, then their income over the last year would understate what their true monthly income is and will be. Second, households may be breaking into savings or using loans to assist with housing payments. Third, some households may be recipients of aid for housing expenditures, or some other unobserved (unreported) source of income may exist.

[48]Analysis on 3 other subsamples (in addition to the 4 presented in the text) can be found in Online Appendix VII.

[49]After imposing restrictions, I again ensure that all counties included in the sample contain at least 25 undocumented renter households each year.

centile. "Inc" refers to the first subsample (restricted on breadwinner income), "Hisp" refers to the second (only Hispanic households), and "Educ" refers to the third (high school diploma/GED or less).

The results in Table 4 paint a familiar picture. In each sample, I find no evidence that (baseline) rents for undocumented immigrants are reduced following the implementation of a sanctuary city policy. However, in the even numbered columns, note the coefficients on $undocumented \times multi\text{-}unit$ and $treat \times multi\text{-}unit \times undocumented$. Regressions on each subsample come to the same conclusion. Undocumented immigrants pay a premium specific to multi-unit housing, but that premium disappears if the household resides in a sanctuary city.

It is possible, however, that incomes of undocumented immigrants are systematically different in sanctuary cities. In fact, running regressions similar to the one specified by equation (3) (where gross rent is replaced by monthly household income as the outcome of interest) provides evidence of a positive correlation between sanctuary city policies and income of undocumented immigrants (see Table 5). These results illustrate that, in the rent regressions, household income is, econometrically, a bad control, meaning β_3 (the effect of $treat \times undocumented$) is not an unbiased estimator of the effect of sanctuary city policies on rent of undocumented households net of income. In fact, β_3 captures the combined effect of sanctuary city policies on rent through both alleviated search frictions and differences in income.[50]

5.4.2. Effect on Rent as a Fraction of Income

If sanctuary city policies are associated with systematically higher household incomes of undocumented immigrants, then income is a bad control in the regressions for gross rent because the treatment affects income, which in turn, affects gross rent. Therefore, one plausible explanation

[50] A more formal analysis of the effect of these policies on labor market outcomes would (among other considerations) examine individual incomes (not household incomes), expand the sample to include immigrants in owner-occupied housing, and select covariates more carefully than I have. Until such work has been done, I caution the reader against interpreting these regression results as robust evidence of the effect of sanctuary city policies on incomes. However, these regressions do show that incomes of undocumented immigrant renter households are systematically different once a sanctuary city policy has taken effect, which may explain the positive effect of treatment on rents of undocumented households.

for the positive effect of treatment (for undocumented immigrants) on baseline rents observed in Table 3 is that the effect of the policy on rents through its effect on income dominates its effect through alleviated frictions.

If sanctuary cities raise incomes but do not otherwise relieve housing search frictions, then rent as a fraction of income should remain constant (if rising income induces a proportional increase in rent, on average). However, if search frictions that resulted in undocumented immigrants paying premiums for housing are alleviated at the same time, then we would expect re-optimization to induce a reduction in the fraction of income undocumented renters devote to rent.

Table 6 presents results for effects on the fraction of a household's income that is spent on rent.[51] Results are obtained by estimating equation (5), which is identical to equation (3) except that the dependent variable is now rent as a fraction of income and the vector of controls, X_i, no longer includes income. Note that findings are, again, quite consistent across subsamples, bolstering the argument that they are not an artifact of misidentified immigration status. If search frictions restricted the supply of housing available to undocumented immigrants, forcing them to devote more of their incomes to rent than they would absent these frictions, then sanctuary city policies that reduce fear (search costs) should allow undocumented immigrants to sort into more ideal housing and reduce the amount of their income they allocate to rent, holding other characteristics constant.

$$\begin{aligned}(\frac{Rent}{Income}_{ipt}) &= \alpha_p + \gamma_t + \rho_1 undocumented_i + \rho_2 treat_{pt} + \rho_3(treat_{pt} \times undocumented_i) \\ &+ \rho_4(PUMA_p \times undocumented_i) + \rho_5(Year_t \times undocumented_i) + X_i\theta + \varepsilon_{ipt}\end{aligned} \tag{5}$$

The results presented in Table 6 imply that, following the enactment of a sanctuary city policy, the fraction of income undocumented renters devoted to rent, compared to similar documented immigrants, was approximately 1.5 percentage points lower, working to reduce the existing rent premium. In other words, despite rising rents, undocumented tenants' rent as a fraction of income *fell* by roughly 3.5 percent, depending on the choice of sample.

[51] For completeness, I also include Table 7, which presents results from regressions like those in Section 4 but where the outcome is replaced with rent as a fraction of income. Results are, at least, qualitatively similar to those in Table 2.

6. IDENTIFYING ASSUMPTIONS AND ROBUSTNESS

I present evidence that the parallel trends assumption holds in Section 6.1 and evidence that results are not influenced by DACA, which went into effect shortly before most of the households in the sample experience treatment, in Section 6.2. In Appendix B I show that the descriptive rent premium is not an artifact of excluding citizens from the analysis. In Appendix E I impose a stricter condition for defining "undocumented households," assuming a household is "undocumented" only if all adults in the household are (as opposed to using the status of the household head). In Appendix F I include naturalized citizens in the group of legal resident immigrants. In Online Appendix VI I systematically remove each of the 4 states with the largest undocumented populations from the sample.[52] Results are, qualitatively, consistent in all cases (even though magnitude and statistical significance do not always perfectly mimic results from choice specifications). Further discussion of these robustness tests is left to the appendix.

6.1. Verifying the Parallel Trends Assumption

To rule out the possibility of pre-trends driving the effect on rent as a fraction of income or the observed effects on rents, I run "event-study-style" regressions corresponding to equations (6)-(8), which are simply extensions of equations (3)-(5) (respectively), and plot point estimates in Figures 1 through 3.[53]

$$\begin{aligned} Rent_{ipt} = \alpha_p + \gamma_t + \beta_1 undocumented_i \\ + \beta_2^k (event\ time)_{pt} + \beta_3^k (undocumented_i \times event\ time_{pt}) \\ + \beta_4 (PUMA_p \times undocumented_i) + \beta_5 (Year_t \times undocumented_i) \\ + X_i \theta + \varepsilon_{ipt} \end{aligned} \tag{6}$$

[52] The robustness of results upon systematically excluding certain states offers reassurance that the effects of sanctuary city policies are not driven by a single state (the results hold for sanctuary cities all over the U.S.). Additionally, it suggests that the findings are also not the result of some possible state-level change that could have occurred around the same time period (at least for the states of California, Texas, Florida, and New York).

[53] In Online Appendix V, I add data from years prior to 2012 and rerun regressions on the new, extended samples. Event study plots based on this extended sample also produce no apparent pre-trends.

$$
\begin{aligned}
Rent_{ipt} = \alpha_p + \gamma_t + \beta_1 undocumented_i \\
&+ \beta_2^k(event\ time)_{pt} + \beta_3^k(undocumented_i \times event\ time_{pt}) \\
&+ \beta_4(PUMA_p \times undocumented_i) + \beta_5(Year_t \times undocumented_i) \\
&+ \beta_6^k(event\ time_{pt} \times multi\text{-}unit_i) \\
&+ \beta_7^k(event\ time_{pt} \times multi\text{-}unit_i \times undocumented_i) \\
&+ X_i\theta + \varepsilon_{ipt}
\end{aligned} \tag{7}
$$

$$
\begin{aligned}
(\frac{Rent}{Income})_{ipt} = \alpha_p + \gamma_t + \rho_1 undocumented_i \\
&+ \rho_2^k(event\ time)_{pt} + \rho_3^k(undocumented_i \times event\ time_{pt}) \\
&+ \rho_4(PUMA_p \times undocumented_i) + \rho_5(Year_t \times undocumented_i) \\
&+ X_i\theta + \varepsilon_{ipt}
\end{aligned} \tag{8}
$$

$Event\ time$ is defined as $year - policy\ year$, meaning treatment begins at $event\ time = 0$ for all households that experience treatment. The k superscript indicates that a separate estimate is generated in each time period, k. X is the same vector of controls used in the regressions in Section 5.2 for equations (6) and (7) and Section 5.4.2 for equation (8) (where income is no longer included as a control).[54] Estimates of β_3^k from equation (6) are plotted in Figure 1. Estimates of β_3^k and β_7^k from equation (7), where treatment effects may vary both by undocumented status and whether one lives in multi-unit housing, are plotted in Figure 2.[55]

No estimate in any pre-treatment period in any event study figure differs significantly from zero, and estimates exhibit no apparent pre-trends that would bias treatment effects. Consistent with findings in Section 5.3, Figure 2 shows diverging effects of treatment on rent. For undocu-

[54]Effects measured in Figures 1 and D.1 should resemble the (aggregated) estimated effects of $treat$ and $treat \times undocumented$ in column 1 of Table 4. Similarly, effects in Figures 2, D.2, and D.3 can be compared to column 2 of Table 4, and effects in Figures 3 and D.4 can be compared to column 5 of Table 6.

[55]In Appendix D, I provide figures that also plot the effects of baseline treatment and treatment interacted with multi-unit status (i.e. β_2^k and β_6^k) as further validation of the parallel trends assumption. These figures suggest that the parallel trends assumption also holds for the effect of $treat$ (even though this is not the primary term of interest).

mented immigrants, rent is rising for all units, but the rent paid specifically for multi-unit housing is falling.[56] Note that, across figures, estimated effects in the earliest periods and latest periods have the largest confidence intervals and may vary greatly in magnitude. This is a result of the specification's reliance on fewer and fewer observations to estimate treatment effects.[57] These outliers do not drive the effects of treatment.[58]

Finally, Figure 3 plots estimates of ρ_3^k from equation (8).[59] Point estimates in the pre-period exhibit no apparent upward or downward trend and never deviate significantly from zero. In the post period, estimated effects of undocumented status are negative, consistent with regression results in Section 5.4.2.

6.2. Exclusion of Households with DACA-eligible Residents

One threat to identification in standard difference-in-differences designs is the possibility that another event occurs around the same time as the treatment and therefore, may influence regression estimates in unobserved ways. Deferred Action for Childhood Arrivals (DACA), a major policy affecting the legal status of hundreds of thousands of young undocumented immigrants took effect in late 2012. DACA certainly affected undocumented immigrants differently than legal residents, and it took effect close to (slightly before) the time many of these sanctuary city policies did. However, because the triple-differences design I implement makes use of geographic variation (in addition to time and immigration status), DACA is only a threat to identification if it differentially affected undocumented immigrants who were in sanctuary cities relative to those who were not. This seems unlikely as DACA is a federal program available to individuals who meet the eligibility criteria regardless of their location in the country. However, in the case that DACA impacted undocumented immigrants in sanctuary cities differently than it did elsewhere (perhaps jurisdictions

[56] Also consistent with Section 5.3's findings, Figure D.2 shows that rents of legal residents exhibit no such divergence.

[57] For example, the only households that ever experience 5 periods of treatment are those that are observed in 2017 in locations that had active policies in 2012, whereas effects in period 2 are comprised of effects in 2017 of policies that took effect in 2015, effects in 2016 of policies that took effect in 2014, and so on.

[58] Results are robust to the exclusion of any household that is treated before 2014 or after 2015 (households treated in these two years comprise nearly 80% of households that are "ever treated").

[59] Appendix D provides a figure that also plots ρ_2^k.

that would become sanctuary cities were also better at facilitating DACA take-up), results could not be attributed solely to sanctuary city policies. To address this possibility, I exclude any household in which at least one member meets the (observable) eligibility criteria for DACA. That is, households are dropped if at least one undocumented resident was no older than 31 as of 2012, was no older than 16 when they arrived in the U.S., and arrived in the U.S. no later than 2007. I then rerun all regressions on this new sample. Results are presented in Tables 8 through 12 and are consistent with previous findings.

7. CONCLUSION

The implications of unauthorized immigration to the United States is a subject of extensive debate. Researchers are presented with a unique challenge in analyzing this particular subset of the population. Undocumented immigrants actively try to avoid detection and lack formal connections to the economy. Thus, there is a rather large segment of the immigrant population with unique characteristics that is often neglected in studies of immigration due to data limitations. Building on a method laid out by Borjas (2017), I applied an adapted imputation procedure to determine undocumented status of individuals in the ACS public-use microdata. Once achieved, I used these estimates to provide empirical support for the theory that search frictions drive undocumented immigrants to pay a premium for rental housing. First, this is a contribution to our knowledge of how undocumented immigrants participate in the market for rental housing. Second, it suggests that studies of immigration and housing markets that fail to account for undocumented immigrants may neglect important heterogeneity.

To provide quasi-experimental evidence of the existence of the premium, I made use of recent sanctuary city policies as sources of variation in fear of deportation among undocumented immigrants. I conclude that these policies alleviate rent premiums faced by undocumented immigrants in multi-unit housing, supporting the notion that sanctuary cities work to equalize rents among immigrants of different statuses. At the same time, sanctuary cities appear to increase housing consumption, at least through higher incomes and increased demand. I show that any induced increase in baseline rents is more than offset by increases in household incomes of undocumented immigrants. My interpretation of this finding is that sanctuary cities allow undocumented households to reassess their housing consumption choices. On one hand, the policies may expand the supply of rental housing that undocumented immigrants believe is available to them and are willing to search for (evidenced by equalizing rents in multi-unit housing). At the same time, the policies may result in higher incomes - another factor to consider when reassessing housing consumption choices. If the policies drive higher baseline rents, then this reassessment story seems most plausible.

There are many avenues for future research. In this paper, I have provided evidence for the existence of barriers (search frictions) that differentially burden undocumented immigrants in the housing market, and I have shown that policy plays a role in how consequential these barriers can be. Future work should further investigate the market consequences of the unique barriers and heterogeneity among immigrants in the long-run and perhaps on a more aggregate scale. For a thorough assessment of the welfare ramifications of the presence of 11 million undocumented people in the United States, studies must also determine if similar barriers exist in other markets, what the consequences of such barriers are, and how policy may influence their existence or consequences.

8. TABLES AND FIGURES

	LPR	Undocumented	Citizen
monthly gross rent	1064	1080	1126
multi-unit	0.7024	0.7053	0.6841
years in us	16.19	14.73	NA
age	44.36	40.05	46.34
male	0.512	0.5857	0.4386
monthly household income	3440	3649	4360
workers in household	1.418	1.629	1.108
people in household	3.583	3.422	2.254
time in residence*	5.654	4.945	5.519
beds	1.926	1.938	1.948
rooms	3.97	3.98	4.288
married	0.6203	0.4464	0.2863
new housing*	0.2066	0.2074	0.2626
high school diploma	0.5613	0.5485	0.8802
bachelor's degree	0.1624	0.1457	0.2147

Table 1: Means of each variable by immigration status. New housing is an indicator for whether the building in which the household lives was built in 1990 or later (the source variable is a broad indicator variable for, roughly, in which decade the building was constructed). The variable for time in residence is an intervalled indicator variable (e.g. less than 1 year, 1-2 years, 2-4 years). I have recoded it as a linear interpolation of these various ranges. The linear interpolation is used to produce the means here, but the original coding as an indicator variable is used in all regressions. Note that, while undocumented households appear to have higher incomes, they also have more workers in the residence contributing to that total. So, while total household income is higher for undocumented renters, the average undocumented worker's income is lower than the average legal resident worker's.

	Model 1	Model 2	Model 3	Model 4
undocumented	11.60^{***}	51.06^{***}	38.44^{***}	14.42^{*}
	(3.51)	(5.06)	(3.76)	(7.89)
years in U.S.			-1.25^{***}	-0.97^{***}
			(0.23)	(0.29)
multi-unit			-107.35^{***}	-135.73^{***}
			(5.03)	(6.65)
undocumented × years in U.S.				-0.62^{**}
				(0.32)
undocumented × multi-unit				47.13^{***}
				(6.65)
Year fixed effects	yes	yes	yes	yes
PUMA fixed effects	no	yes	yes	yes
Controls	no	no	yes	yes
Adj. R^2	0.01	0.33	0.55	0.55
Num. obs.	111713	111713	111713	111713

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

Table 2: Descriptive effect on monthly gross rent. Restricted to non-citizen immigrants. Robust standard errors clustered at the PUMA level. All regressions (in all tables) are weighted using the household weight variable provided in the ACS data.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
undocumented	29.66^{***}	8.59	2.39			
	(3.78)	(7.96)	(8.16)			
treat	-7.27	-6.32	-1.99	2.02	1.84	3.28
	(6.33)	(6.33)	(9.39)	(7.76)	(7.76)	(10.50)
treat × undocumented	26.23^{***}	24.19^{***}	52.81^{***}	10.83	11.05	45.35^{***}
	(6.40)	(6.34)	(11.92)	(9.18)	(9.19)	(14.01)
years in U.S.	-1.26^{***}	-0.94^{***}	-0.94^{***}	-1.26^{***}	-1.01^{***}	-1.01^{***}
	(0.23)	(0.28)	(0.29)	(0.23)	(0.28)	(0.28)
multi-unit	-107.49^{***}	-134.81^{***}	-133.25^{***}	-107.33^{***}	-125.15^{***}	-124.49^{***}
	(5.03)	(6.60)	(6.88)	(4.98)	(6.23)	(6.60)
undocumented × years in U.S.		-0.69^{**}	-0.71^{**}		-0.51	-0.51
		(0.32)	(0.32)		(0.33)	(0.33)
undocumented × multi-unit		45.42^{***}	54.72^{***}		29.73^{***}	40.63^{***}
		(6.55)	(6.79)		(6.76)	(7.12)
treat × multi-unit			-5.46			-1.90
			(10.30)			(10.20)
treat × multi-unit × undocumented			-38.16^{***}			-45.20^{***}
			(13.50)			(13.75)
PUMA × undocumented fe	No	No	No	Yes	Yes	Yes
Year × undocumented fe	No	No	No	Yes	Yes	Yes
Adj. R^2	0.55	0.55	0.55	0.55	0.55	0.56
Num. obs.	111713	111713	111713	111713	111713	111713

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

Table 3: Effect on monthly gross rent from regressions that incorporate sanctuary city policies. Robust standard errors clustered at the PUMA level.

	Unr	Unr	Inc	Inc	Hisp	Hisp	Educ	Educ
treat	8.28	3.03	8.09	-1.20	-3.02	1.16	5.38	5.51
	(7.72)	(10.16)	(7.54)	(10.44)	(8.14)	(10.98)	(7.89)	(10.97)
treat × undocumented	-1.75	31.38^{**}	-8.67	21.51	3.11	25.50^{*}	3.19	24.90^{*}
	(8.54)	(12.78)	(8.71)	(13.37)	(9.18)	(13.47)	(9.07)	(13.40)
undocumented × multi-unit	28.49^{***}	39.89^{***}	24.04^{***}	35.37^{***}	31.57^{***}	38.73^{***}	32.08^{***}	39.63^{***}
	(6.23)	(6.60)	(6.36)	(7.01)	(6.41)	(6.79)	(6.67)	(7.20)
treat × multi-unit		6.99		12.59		-6.18		-0.19
		(9.75)		(9.96)		(10.45)		(10.76)
treat × multi-unit × undocumented		-43.93^{***}		-40.30^{***}		-31.43^{**}		-29.88^{**}
		(12.35)		(12.70)		(13.49)		(13.01)
Adj. R^2	0.57	0.57	0.55	0.55	0.59	0.59	0.57	0.57
Num. obs.	93776	93776	72167	72167	61481	61481	60169	60169

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

Table 4: Effect on monthly gross rent. Standard errors clustered at the PUMA level. First two columns present estimates from the "unrestricted" sample. Columns 3 and 4 restrict the sample based on the breadwinner's income. Columns 5 and 6 restrict the sample to Hispanic immigrants. Columns 7 and 8 restrict the sample to immigrants with high school diplomas or less (GED included).

	Unr	Inc	Hisp	Educ	Unr	Inc	Hisp	Educ
undocumented	-218.51^{***}	-134.82^{***}	-99.00^{***}	-58.49^{***}				
	(28.66)	(19.29)	(21.75)	(20.74)				
treat	-179.80^{***}	-107.99^{***}	-127.21^{***}	-97.28^{***}	-81.84	-5.77	-80.80^{*}	-55.45
	(41.78)	(30.43)	(38.29)	(37.34)	(50.09)	(39.98)	(48.61)	(46.81)
treat × undocumented	240.22^{***}	181.15^{***}	188.39^{***}	190.32^{***}	108.44^{*}	37.33	128.70^{**}	132.06^{**}
	(47.31)	(29.81)	(35.82)	(35.27)	(64.52)	(49.28)	(58.69)	(56.59)
PUMA × undocumented fe	No	No	No	No	Yes	Yes	Yes	Yes
Year × undocumented fe	No	No	No	No	Yes	Yes	Yes	Yes
Adj. R^2	0.32	0.39	0.40	0.41	0.33	0.40	0.41	0.41
Num. obs.	93776	72167	61481	60169	93776	72167	61481	60169

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

Table 5: Effect on monthly household income. Controls include the same fixed effects as described in equation (3). Additional controls are age, age squared, years in the U.S., marital status, gender, number of workers in household, number of people in household, and length of stay in current residence.

	Unr	Inc	Hisp	Educ	Unr	Inc	Hisp	Educ
undocumented	0.0103^{***}	0.0110^{***}	0.0064^{***}	0.0043^{*}				
	(0.0022)	(0.0020)	(0.0023)	(0.0024)				
treat	0.0094^{***}	0.0064^{*}	0.0068	0.0091^{**}	0.0070	0.0048	0.0060	0.0103^{*}
	(0.0035)	(0.0035)	(0.0044)	(0.0043)	(0.0045)	(0.0047)	(0.0059)	(0.0057)
treat × undocumented	-0.0185^{***}	-0.0177^{***}	-0.0161^{***}	-0.0175^{***}	-0.0150^{***}	-0.0154^{***}	-0.0152^{***}	-0.0188^{***}
	(0.0033)	(0.0030)	(0.0037)	(0.0038)	(0.0054)	(0.0054)	(0.0066)	(0.0065)
Sample Mean	0.38	0.38	0.40	0.40	0.38	0.38	0.40	0.40
Sample Median	0.33	0.34	0.35	0.36	0.33	0.34	0.35	0.36
PUMA × undocumented fe	No	No	No	No	Yes	Yes	Yes	Yes
Year × undocumented fe	No	No	No	No	Yes	Yes	Yes	Yes
Adj. R^2	0.2143	0.2697	0.2494	0.2469	0.2193	0.2738	0.2531	0.2508
Num. obs.	93776	72167	61481	60169	93776	72167	61481	60169

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

Table 6: Effect on (monthly) rent as a fraction of (monthly) income. Specifications are identical to the one given by equation (3) with the exceptions that the outcome is now rent as a fraction of income, income is no longer included as a control (because it is part of the outcome of interest), and terms for heterogeneous effects of undocumented status have been removed (so that the treatment effects apply to all kinds of housing and all effects of undocumented status are completely captured by the baseline indicator for status and the treatment interacted with status). Columns 4 through 8 include fixed effects interacted with undocumented status.

	Model 1	Model 2	Model 3	Model 4
undocumented	-0.0188^{***}	-0.0098^{***}	0.0041^{**}	0.0075^{*}
	(0.0016)	(0.0021)	(0.0019)	(0.0044)
years in U.S.			-0.0004^{***}	-0.0002
			(0.0001)	(0.0002)
multi-unit			-0.0135^{***}	-0.0161^{***}
			(0.0022)	(0.0028)
undocumented $\times$ years in U.S.				-0.0004^{**}
				(0.0002)
undocumented $\times$ multi-unit				0.0042
				(0.0033)
Year fixed effects	yes	yes	yes	yes
PUMA fixed effects	no	yes	yes	yes
Controls	no	no	yes	yes
Adj. R^2	0.0022	0.0389	0.2139	0.2140
Num. obs.	93776	93776	93776	93776

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

Table 7: Descriptive effect on rent as a fraction of income. Compare to Table 2.

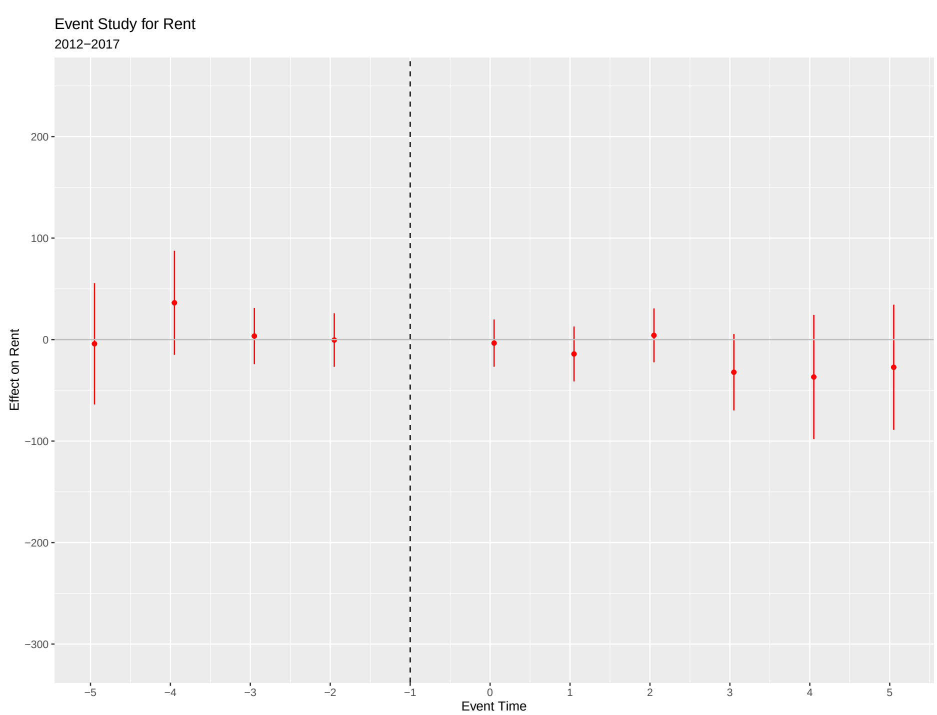

Figure 1: Event study plot based on equation (6). Plots just β_3 estimates (the effect unique to undocumented households).

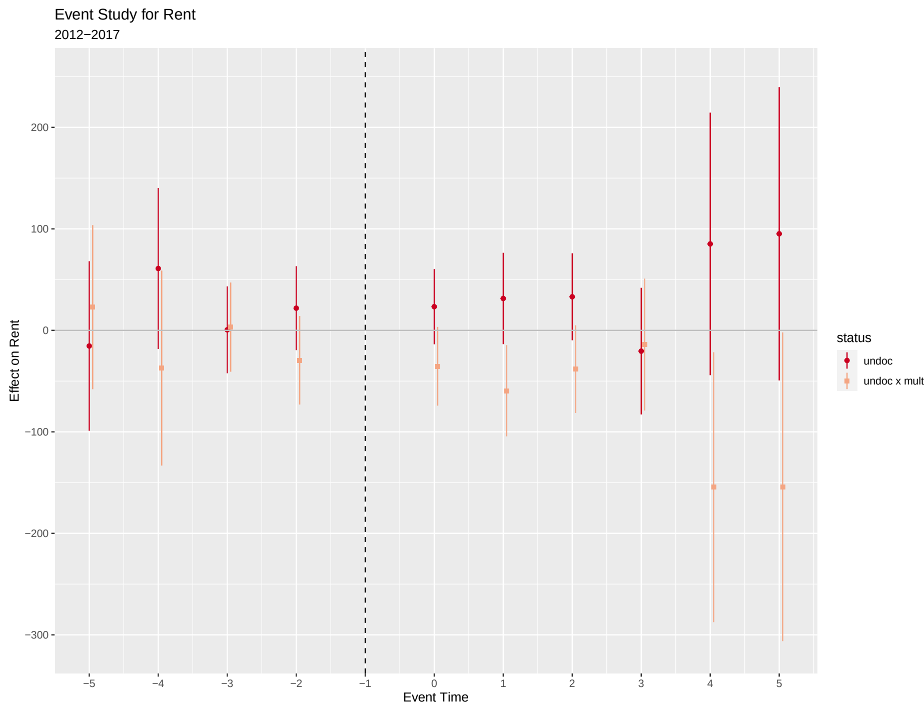

Figure 2: Event study plot based on equation (7). Plots β_3 (red) and β_7 (orange) estimates.

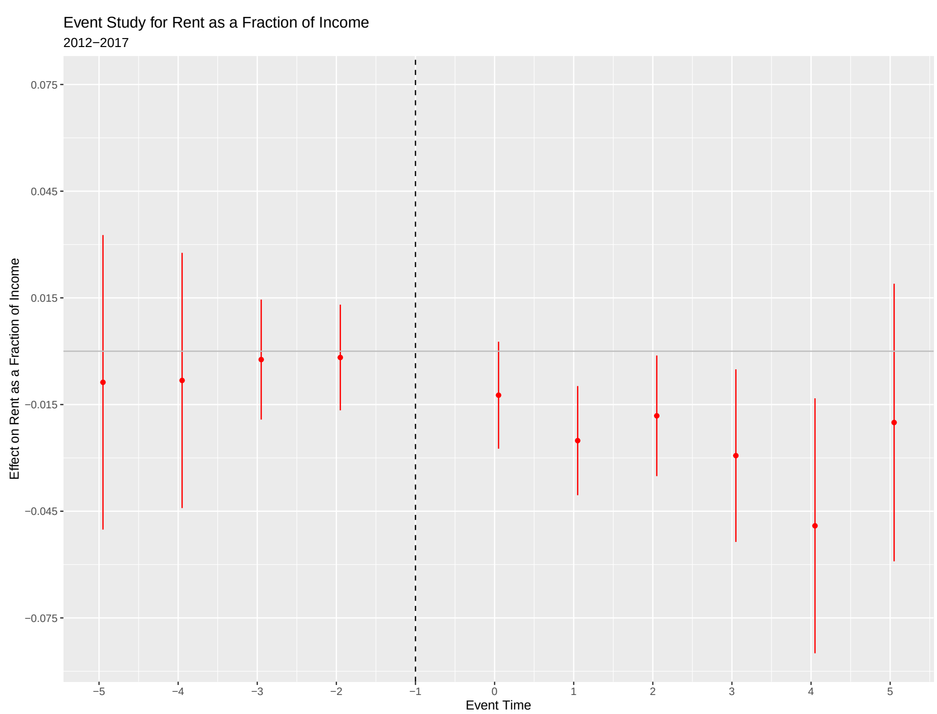

Figure 3: Event study plot based on equation (8). Plots just ρ_3 estimates (effect unique to undocumented households).

	Model 1	Model 2	Model 3	Model 4
undocumented	8.67^{**}	47.66^{***}	37.03^{***}	1.86
	(3.56)	(5.17)	(3.82)	(7.97)
years in U.S.			-0.74^{***}	-0.68^{**}
			(0.23)	(0.29)
multi-unit			-103.62^{***}	-135.24^{***}
			(5.09)	(6.76)
undocumented $\times$ years in U.S.				-0.21
				(0.32)
undocumented $\times$ multi-unit				53.87^{***}
				(6.75)
Adj. R^2	0.01	0.32	0.54	0.54
Num. obs.	103782	103782	103782	103782

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

Table 8: Descriptive effect on rent, excluding households with DACA-eligible residents. Compare to Table 2.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
undocumented	29.62^{***}	-2.85	-8.79			
	(3.83)	(8.04)	(8.33)			
treat	-3.69	-2.33	0.78	3.73	3.67	5.87
	(6.27)	(6.28)	(9.47)	(7.64)	(7.64)	(10.56)
treat $\times$ undocumented	22.15^{***}	19.51^{***}	46.35^{***}	10.71	10.84	40.04^{***}
	(6.53)	(6.48)	(11.98)	(9.26)	(9.26)	(14.10)
years in U.S.	-0.75^{***}	-0.66^{**}	-0.66^{**}	-0.74^{***}	-0.74^{**}	-0.74^{**}
	(0.24)	(0.29)	(0.29)	(0.23)	(0.29)	(0.29)
multi-unit	-103.71^{***}	-134.56^{***}	-133.46^{***}	-103.32^{***}	-125.20^{***}	-124.19^{***}
	(5.09)	(6.70)	(7.04)	(5.03)	(6.35)	(6.79)
undocumented $\times$ years in U.S.		-0.26	-0.28		-0.04	-0.05
		(0.33)	(0.33)		(0.33)	(0.33)
undocumented $\times$ multi-unit		52.58^{***}	61.52^{***}		37.45^{***}	46.77^{***}
		(6.65)	(6.99)		(6.83)	(7.31)
treat $\times$ multi-unit			-3.87			-2.89
			(10.30)			(10.21)
treat $\times$ multi-unit $\times$ undocumented			-35.83^{***}			-38.56^{***}
			(13.64)			(13.76)
Adj. R^2	0.54	0.54	0.54	0.54	0.54	0.54
Num. obs.	103782	103782	103782	103782	103782	103782

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

Table 9: Effect on rent, excluding households with DACA-eligible residents. Compare to Table 3.

	Unr	Unr	Inc	Inc	Hisp	Hisp	Educ	Educ
treat	7.71	2.84	11.60	3.91	−2.24	0.68	7.81	7.62
	(7.62)	(10.05)	(8.04)	(10.91)	(8.23)	(11.07)	(8.00)	(10.98)
treat × undocumented	−2.30	25.29*	−6.37	21.93	1.31	20.30	0.43	18.21
	(8.51)	(13.06)	(9.46)	(14.46)	(9.22)	(13.73)	(9.24)	(13.68)
undocumented × multi-unit	32.85***	42.68***	20.29***	31.25***	31.08***	37.61***	29.44***	35.97***
	(6.38)	(6.95)	(6.91)	(7.58)	(6.70)	(7.12)	(6.80)	(7.47)
treat × multi-unit		6.48		10.32		−4.33		0.26
		(9.67)		(10.06)		(10.50)		(10.66)
treat × multi-unit × undocumented		−36.64***		−37.69***		−26.89*		−24.62*
		(12.47)		(13.50)		(13.75)		(13.19)
Adj. R^2	0.56	0.56	0.56	0.56	0.58	0.58	0.56	0.56
Num. obs.	86345	86345	75611	75611	57431	57431	56176	56176

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

Table 10: Effect on rent, excluding households with DACA-eligible residents. Compare to Table 4.

	Unr	Inc	Hisp	Educ	Unr	Inc	Hisp	Educ
undocumented	−186.18***	−210.87***	−80.97***	−45.82**				
	(28.40)	(30.88)	(22.12)	(20.76)				
treat	−172.46***	−175.39***	−113.75***	−84.59**	−64.63	−57.20	−62.77	−49.78
	(41.51)	(45.71)	(38.53)	(37.85)	(48.44)	(55.52)	(48.38)	(46.64)
treat × undocumented	215.22***	181.88***	164.77***	166.25***	67.51	20.45	100.15*	122.31**
	(46.01)	(50.61)	(36.52)	(35.95)	(63.50)	(71.31)	(59.69)	(57.73)
Adj. R^2	0.32	0.32	0.40	0.41	0.33	0.33	0.41	0.42
Num. obs.	86345	75611	57431	56176	86345	75611	57431	56176

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

Table 11: Effect on income, excluding households with DACA-eligible residents. Compare to Table 5.

	Unr	Inc	Hisp	Educ	Unr	Inc	Hisp	Educ
undocumented	0.0086***	0.0100***	0.0050**	0.0033				
	(0.0022)	(0.0022)	(0.0024)	(0.0024)				
treat	0.0086**	0.0091**	0.0057	0.0087*	0.0055	0.0061	0.0054	0.0103*
	(0.0036)	(0.0036)	(0.0045)	(0.0044)	(0.0046)	(0.0046)	(0.0060)	(0.0058)
treat × undocumented	−0.0172***	−0.0148***	−0.0141***	−0.0159***	−0.0124**	−0.0101*	−0.0142**	−0.0184***
	(0.0034)	(0.0034)	(0.0038)	(0.0039)	(0.0057)	(0.0056)	(0.0068)	(0.0068)
Adj. R^2	0.2161	0.2508	0.2502	0.2461	0.2209	0.2558	0.2538	0.2496
Num. obs.	86345	75611	57431	56176	86345	75611	57431	56176

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

Table 12: Effect on rent as a fraction of income, excluding households with DACA-eligible residents. Compare to Table 6.

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Appendix A. Additional Descriptive Statistics

Means for indicators (0 or 1) for each of the logical edits applied by status

	LPR	undocumented
military	0.01041	0
arrived before 1980	0.04019	0
public health insurance	0.5616	0
medicaid	0.5136	0
medicare	0.1191	0
VA insurance	0.002705	0
welfare	0.07737	0
SSI	0.05211	0
SS	0.08909	0
licensed job	0.02368	0
Cuban	0.07931	0
student visa	0	0
foodstamps*	0.04017	0
H1B	0.04483	0
American Samoan*	0.002832	0
legal by marriage	0.5042	0

Table A.1: Foodstamps/SNAP receipt is only counted for households with one adult where the indicator for anyone in the household receiving foodstamps is true. This is to account for the possibility that an undocumented parent has collected foodstamps on the behalf of a legally present dependent. Individuals from American Samoa, while technically non-citizens (contrary to all other U.S. territories), are all legally eligible to live and work in the United States. Note that student visa is 0 because the sample excludes individuals currently enrolled in college. Also note that any individual can fulfill any number of these conditions at a time (e.g. an immigrant who has received medicaid and is married to a U.S. citizen or legal resident, would be coded as “1” for both conditions).

Additional Statistics on the Undocumented Population

	Birthplace	2012-2017 total	2012-2017 renters	2016 Pew	2017 CMS	2015 DHS
1	Mexico	43.8	49.4	50.9	49.6	55
2	El Salvador	5.1	5.9	6.8	6.3	6
3	Guatemala	4.2	4.9	5.4	5.1	5
4	Honduras	2.9	3.4	4.0	3.6	4
5	India	5.4	2.4	4.4	5.9	4
6	Dominican Republic	1.6	2.0	2.0	1.8	
7	Philippines	2.3	2.0	1.3	1.6	3
8	Korea	1.9	1.9	1.2	1.6	2
9	China	3.8	1.9	3.0	2.9	3
10	Ecuador	1.2	1.6	1.1	1.2	1
11	Colombia	1.4	1.5	1.1	1.4	
12	Haiti	1.2	1.3	0.9	1.2	

Table A.2: Birthplace of undocumented renters by country. Numbers represent the percent of undocumented immigrants by country of birth. Estimates from the full population are presented first. Column 2 estimates are derived from the sample of renters used in the analysis (in Sections 4 and 5.3). The remaining 3 columns provide estimates of the percent of the undocumented population by country of birth from other sources for comparison (Pew, the Center for Migration Studies, and the Department of Homeland Security).

	State	2012-2017 Choice	2017 Choice	2017 Pew	2017 CMS	2016 MPI	2015 DHS
1	California	2500	2100	2000	2400	3100	2900
2	Texas	1800	1800	1600	1800	1600	1900
3	Florida	888	900	825	766	656	810
4	New York	886	777	650	753	940	590
5	New Jersey	525	505	450	452	526	440
6	Illinois	502	465	425	460	487	450
7	Georgia	404	388	375	335	351	390
8	North Carolina	340	329	325	300	321	390
9	Virginia	313	295	275	243	269	310
10	Arizona	275	249	275	252	226	
11	Maryland	262	247	250	224	247	
12	Washington	257	260	250	251	229	
	Total	11.6	11.1	10.5	10.7	11.3	12.0

Table A.3: Estimates of the undocumented population by state of residence (in thousands). For comparison, estimates from Pew, CMS, DHS, and the Migration Policy Institute are provided as well.

Appendix B. Robustness to Inclusion of Citizens

To ensure that the story of the rent premium is not an artifact of the composition of the subsample of immigrants, I run regressions that include citizens and allow for different premiums for legal resident immigrants and undocumented immigrants. Equation (B.1) is the citizen-inclusive analog to equation (2).

$$\begin{aligned} Rent_{ipt} = {} & \beta_1 LPR_i + \beta_2 undocumented_i + \beta_3 years\ in\ U.S._i + \beta_4 multi\text{-}unit_i \\ & + \beta_5 (LPR_i \times years\ in\ U.S._i) + \beta_6 (undocumented_i \times years\ in\ U.S._i) \\ & + \beta_7 (LPR_i \times multi\text{-}unit_i) + \beta_8 (undocumented_i \times multi\text{-}unit_i) \\ & + X_i \theta + \alpha_p + \gamma_t + \varepsilon_{ipt} \end{aligned} \tag{B.1}$$

LPR_i is 1 if the householder is a legal immigrant (but not a citizen) and 0 otherwise. Results from the full sample are presented in Table B.1 and reinforce the results from the restricted sample. Interestingly, estimates in Table B.1 suggest that all non-citizen immigrants pay a rent premium (that seems to disappear over time), but only undocumented immigrants pay a premium specifically for multi-unit housing, consistent with the theory of search frictions specific to undocumented renters of these types of units (there is no obvious reason that any premium legal resident immigrants face would vary based on the type of housing unit rented, but the search frictions theory I propose provides reason to expect the positive coefficient on the interaction of undocumented status and the multi-unit indicator as multi-unit housing like apartments may appear especially risky to prospective undocumented tenants).[60] Additionally, the fact that legal resident immigrants pay higher rents than citizens implies that the choice to use legal resident immigrants as the compari-

[60] Though not an immediately obvious explanation, one may have believed *ex ante* that discrimination is responsible for the premium legal resident immigrants pay and that this premium is different for multi-unit housing because discrimination is different for multi-unit housing. Hanson, Hawley and Taylor (2011) find that racial discrimination in housing is greater for these kinds of units. The results in Table B.1 suggest that, if the same kind of differential discrimination exists in the context of immigration status (i.e. if legal resident immigrants face additional discrimination in multi-unit housing like black applicants do), it does not manifest as a premium. Thus, either legal resident immigrants (compared to citizens) do not face differential discrimination by housing unit type in the same way prospective black tenants (compared to prospective white tenants) do, or they do but such discrimination (at least in the context of immigration status) does not result in a rent premium.

son group throughout this study results in more conservative estimates of how much more undocumented immigrants pay for rents and further suggests that the coefficient on $undocumented$ is truly capturing just the effect of undocumented status on rents (and not other characteristics correlated both with undocumented or immigrant status and higher rents).

	Model 1	Model 2	Model 3	Model 4
LPR	-11.58^{***}	-49.61^{***}	32.90^{***}	106.30^{***}
	(2.45)	(4.82)	(3.94)	(8.12)
undocumented	4.77^{**}	-11.60^{*}	61.73^{***}	122.24^{***}
	(2.30)	(6.00)	(4.98)	(9.51)
years in U.S.			4.10^{***}	4.61^{***}
			(0.16)	(0.17)
LPR $\times$ years in U.S.				-4.16^{***}
				(0.21)
undocumented $\times$ years in U.S.				-5.41^{***}
				(0.35)
multi-unit			-127.74^{***}	-131.69^{***}
			(3.80)	(4.03)
LPR $\times$ multi-unit				3.51
				(5.44)
undocumented $\times$ multi-unit				31.15^{***}
				(5.77)
Year fixed effects	yes	yes	yes	yes
PUMA fixed effects	no	yes	yes	yes
Controls	no	no	yes	yes
Adj. R^2	0.00	0.27	0.52	0.52
Num. obs.	1046700	1046700	1046700	1046700

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

Table B.1: Effect on monthly gross rent in the sample including citizens.

Appendix C. Effect of Sanctuary Cities on Movement

Since sanctuary city policies appear to raise household incomes of undocumented renters, a plausible explanation for higher rents (despite evidence of alleviated search frictions from the reduced or eliminated premium specific to multi-unit housing) is that higher incomes induce undocumented immigrants to select into more expensive rental housing. In other words, it is possible that search frictions are alleviated (as previous results for renters of multi-unit housing suggest), allowing undocumented renters to better optimize their housing, resulting in the reduction or elimination of premiums. At the same time, though, if incomes have systematically risen for undocumented renters, their optimal housing consumption would change through this income channel as well, resulting in undocumented immigrants paying more for housing. To provide suggestive evidence that re-optimization is occurring, consistent with what we would expect if search frictions are reduced and consistent with higher rents resulting from selection into higher-price housing (due to increases in income), I run regressions corresponding to linear probability models where the outcome of interest is whether a renter (technically, household head) has moved within the last year. Results (for the sample from Section 5.3) are presented in Table C.1.

There are arguments to be made that the choice set of fixed effects is not appropriate or necessary or that it asks too much of the data when considering linear probability models for movement.[61] I remain agnostic in this case and characterize the results in Table C.1 as only suggestive evidence that the policies induce undocumented renters to move more (since specifications with the choice set of fixed effects produce still positive, but statistically insignificant, estimated effects). Online Appendix IV presents additional evidence to suggest that movement from outside the current PUMA of residence into the current PUMA (a sanctuary jurisdiction) occurs more frequently after a policy is enacted and drives the marginally positive coefficients in columns 3 and 4.

Since I am unable to determine with much precision when a household moved into their current residence and am restricted to evaluating whether the household moved within the last year,

[61] One may be less concerned about drastic differences between undocumented immigrants and legal resident immigrants across PUMA's or time in their propensities to move than one might be about drastic differences in rent premiums (or income differences) across locations.

	Model 1	Model 2	Model 3	Model 4
undocumented	-0.0118^{***}	-0.0336^{***}		
	(0.0044)	(0.0042)		
treat	-0.0132^{*}	-0.0123^{*}	-0.0048	-0.0006
	(0.0070)	(0.0069)	(0.0090)	(0.0087)
treat $\times$ undocumented	0.0177^{***}	0.0189^{***}	0.0064	0.0024
	(0.0068)	(0.0064)	(0.0115)	(0.0111)
years in U.S.		-0.0050^{***}		-0.0050^{***}
		(0.0002)		(0.0002)
multi-unit		0.0115^{***}		0.0116^{***}
		(0.0040)		(0.0041)
PUMA $\times$ undocumented fe	No	No	Yes	Yes
Year $\times$ undocumented fe	No	No	Yes	Yes
Controls	No	Yes	No	Yes
Adj. R^2	0.0366	0.0885	0.0429	0.0934
Num. obs.	111713	111713	111713	111713

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

Table C.1: Linear probability model for movement within last year. Controls, where included, are age, age squared, household income, marital status, gender, number of people in household, number of workers in household, number of rooms, number of bedrooms, and build year (intervalled).

I am not surprised by the lack of power in these regressions. Still, despite the constraints on my ability to evaluate changes in mobility resulting from sanctuary city policies, I argue that Table C.1 suggests that movement (the primary mechanism through which individuals can re-optimize their housing consumption decisions) may occur more frequently in the undocumented renter population after sanctuary city policies are in place than it would absent the policies.[62]

[62] Also note that, an *increase* in probability of moving is not a necessary condition for re-optimization to be occurring. Any non-zero amount of movement (e.g. even the amount of movement absent the policy) allows households to re-optimize. It may be that households move with the same frequency following the policy but are able to make "better" moves.

Appendix D. Further Validation of Parallel Trends

Below, Figure D.1 adds β_2^k, the estimated effects of treatment (not interacted with undocumented status), to Figure 1. There are no obvious pre-trends in the effect of treatment for either class of immigrants.

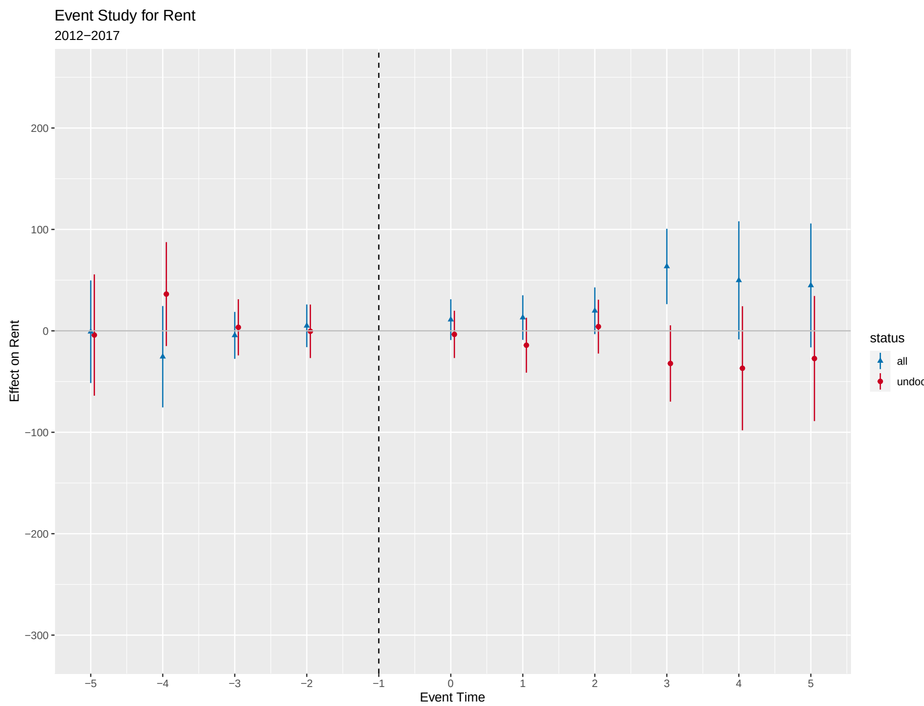

Figure D.1: Event study plot based on equation (6). Plots both β_2 (blue) and β_3 (red) estimates.

Instead of plotting β_3^k and β_7^k from equation (7) (which capture the effect of treatment for undocumented immigrants in general and the effect of treatment specific to undocumented immigrants renting multi-unit housing, respectively) as in Figure 2, Figure D.2 plots β_2^k and β_6^k (which capture the general effect of treatment on rents for the full sample immigrants and the effect of treatment specific to all immigrants in the sample renting multi-unit housing, respectively). Again, there are no apparent pre-trends. Also note the stability of the estimated effect of treatment for all immigrants. These figures illustrate that the only demonstrable effects of sanctuary city policies on rents occur only for undocumented immigrants (effects of treatment are only distinguishable from zero in the terms where treatment is interacted with undocumented status).

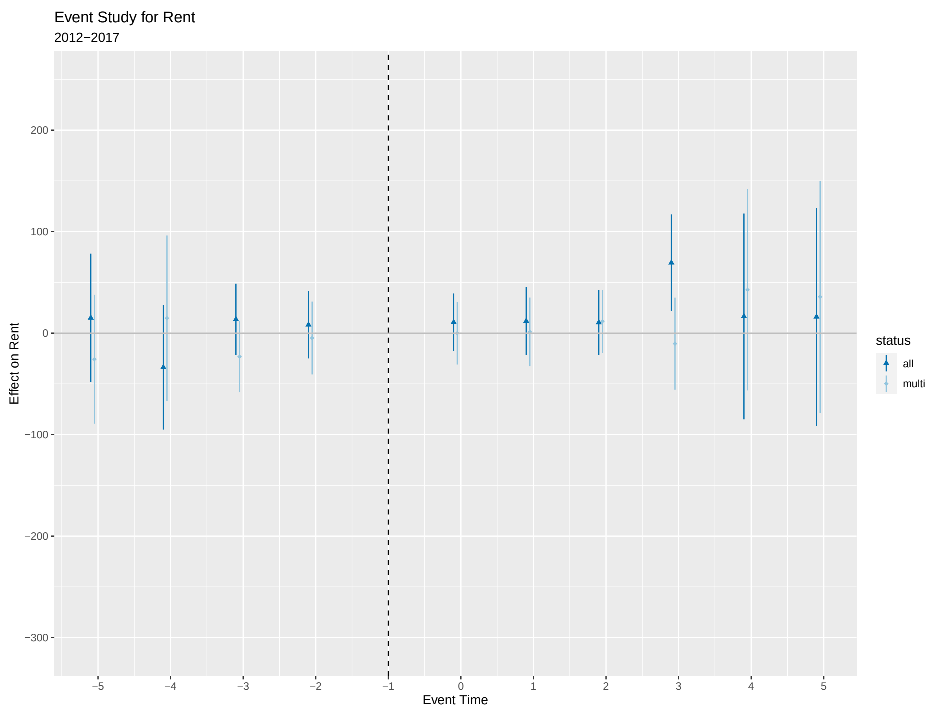

Figure D.2: Event study plot based on equation (7). Plots β_2 (blue) and β_6 (light blue) estimates. Compare to Figure 2.

To more clearly illustrate that treatment affects undocumented renters without having any clear effect on legal residents, Figure D.3 combines estimates from Figures 2 and D.2. This is a more cluttered graphic, but it more concisely shows the diverging effects of treatment for undocumented immigrants in contrast to the stable effects for legal residents.

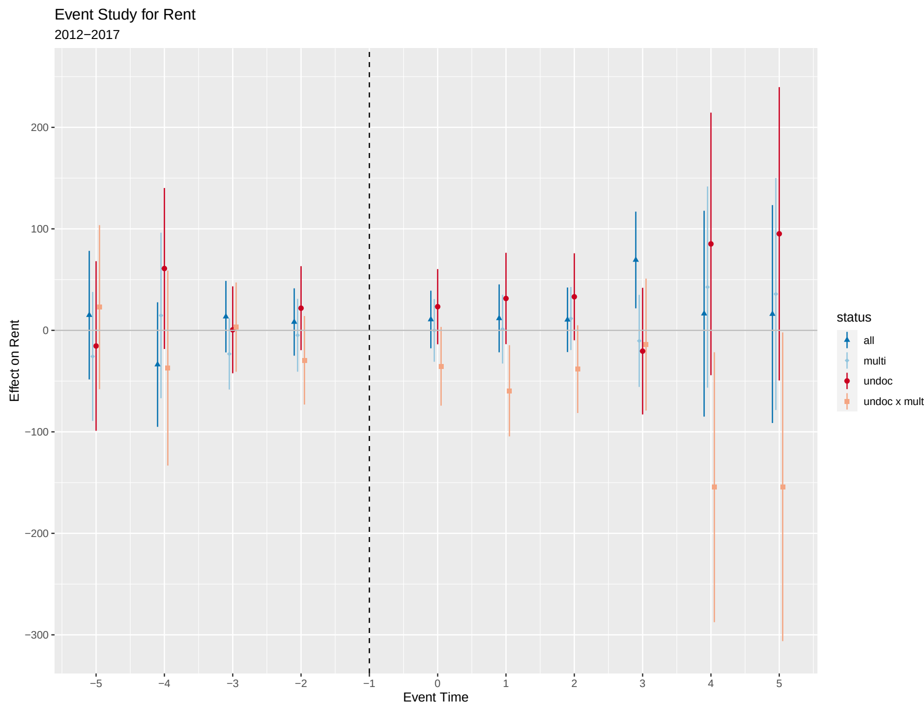

Figure D.3: Event study plot based on equation (7). Plots β_2 (blue), β_3 (red), β_6 (light blue), and β_7 (orange) estimates together.

Finally, just as Figure D.1 adds estimates of β_2^k from equation (6) to Figure 1, Figure D.4 adds estimates of ρ_2^k from equation (8) to Figure 3.

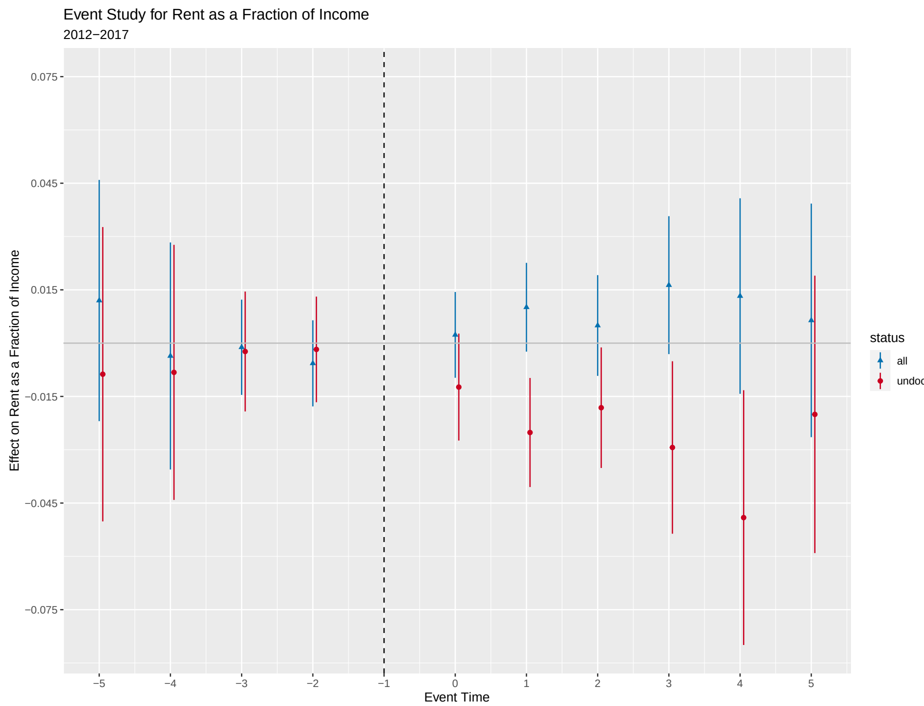

Figure D.4: Event study plot based on equation (8). Plots both ρ_2 (blue) and ρ_3 (red) estimates.

Often, the first-order effect of treatment in the post-period is positive. This is consistent with some regression results that suggest sanctuary city policies may lead to increases in the fraction of income legal resident immigrants devote to rent. While choice specifications throughout this paper nearly always fail to reject that the policies do not alter rent as a fraction of income for legal resident immigrants, several of the less stringent specifications would come to the conclusion that the policies lead to lower household incomes of legal resident immigrants, resulting in higher fractions of their incomes allocated to rent.[63] It is hard to say whether the effect of sanctuary city policies on incomes or the ratios of rents to incomes is truly non-zero for legal resident immigrants. Even harder to answer is, if their household incomes do fall in response to sanctuary city policies, why they fall and what the broader welfare implications are.[64]

[63]Note that the effect of these policies on rents of all non-citizen immigrants is nearly always a fairly precise zero.

[64]One possibility is increased labor market competition. Undocumented immigrants may face lessened labor market frictions as well as housing market frictions. Another possibility is "legal resident flight." Saiz and Wachter (2011) find evidence that natives, especially those with higher incomes, move in response to growing immigrant populations. It is possible that higher-income legal residents leave sanctuary cities as more housing becomes accessible to undocumented immigrants.

Appendix E. "All Adults Undocumented" Restriction

I rerun the regressions from Sections 4 and 5 under the assumption that a household is an "undocumented household" only if all adults in the household are undocumented. I believe this runs the risks of more heavily weighting misclassified renters (e.g. households with only one adult member), including households with adult citizen or legal resident dependents (e.g. undocumented parents of 18 year-old citizens still living at home) in the legal resident category inappropriately, and ignoring the possibility that a search friction may force undocumented immigrants to select units with a legal resident or citizen roommate (even though that selection may be sub-optimal). Nonetheless, if results hold under this restriction, then it is reasonable to believe they would hold under less strict restrictions as well and results are not simply an artifact of how I have classified households as "undocumented." Results are remarkably similar.

	Model 1	Model 2	Model 3	Model 4
undocumented	-51.90^{***}	-6.73	24.13^{***}	2.65
	(3.55)	(5.06)	(3.51)	(8.32)
years in U.S.			-1.28^{***}	-0.88^{***}
			(0.24)	(0.25)
multi-unit			-109.08^{***}	-133.07^{***}
			(5.19)	(6.20)
undocumented $\times$ years in U.S.				-1.22^{***}
				(0.34)
undocumented $\times$ multi-unit				54.14^{***}
				(6.84)
Year fixed effects	no	yes	yes	yes
PUMA fixed effects	no	yes	yes	yes
controls	no	no	yes	yes
Adj. R^2	0.01	0.33	0.55	0.55
Num. obs.	105558	105558	105558	105558

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

Table E.1: Section 4 equivalent. Effect on gross rent.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
undocumented	16.87***	−1.84	−5.46			
	(3.41)	(8.33)	(8.67)			
treat	−4.67	−4.20	9.58	3.57	4.22	15.96
	(6.25)	(6.26)	(9.23)	(6.82)	(6.84)	(9.87)
treat × undocumented	23.02***	20.65***	44.00***	7.52	5.52	32.33**
	(6.93)	(6.86)	(13.66)	(8.12)	(8.12)	(15.01)
years in U.S.	−1.29***	−0.86***	−0.86***	−1.33***	−0.97***	−0.97***
	(0.24)	(0.25)	(0.25)	(0.23)	(0.25)	(0.25)
multi-unit	−109.12***	−132.40***	−126.35***	−108.79***	−127.04***	−121.81***
	(5.20)	(6.16)	(6.36)	(5.12)	(5.81)	(6.15)
undocumented × years in U.S.		−1.28***	−1.30***		−1.05***	−1.06***
		(0.34)	(0.34)		(0.34)	(0.34)
undocumented × multi-unit		52.56***	57.50***		41.37***	47.48***
		(6.72)	(7.04)		(6.65)	(7.24)
treat × multi-unit			−18.55*			−15.88
			(9.79)			(9.70)
treat × multi-unit × undocumented			−28.69*			−33.03**
			(15.25)			(15.36)
Adj. R^2	0.55	0.55	0.55	0.56	0.56	0.56
Num. obs.	105558	105558	105558	105558	105558	105558

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

Table E.2: Section 5.3 equivalent. Effect on gross rent

	Rent	*Rent*	*Income*	$\frac{Rent}{Income}$
treat	6.56	15.60*	−74.54*	0.0043
	(6.47)	(9.28)	(45.03)	(0.0036)
treat × undocumented	−8.46	7.53	113.76**	−0.0179***
	(7.19)	(13.27)	(57.72)	(0.0042)
undocumented × multi-unit	41.36***	44.58***		
	(6.07)	(6.63)		
treat × multi-unit		−12.45		
		(9.32)		
treat × multi-unit × undocumented		−19.24		
		(13.52)		
Adj. R^2	0.57	0.57	0.33	0.2221
Num. obs.	87496	87496	87496	87496

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

Table E.3: Section 5.4 results under new definition of "undocumented household." The sample restrictions applied are equivalent to those of the "Unr" (unrestricted) sample in the main text. Results are quite similar across the other subsamples. In columns 1 and 2, the dependent variable is gross monthly rent. In column 3, the dependent variable is monthly income. In column 4, the dependent variable is rent as a fraction of income.

Appendix F. Naturalized Citizens as Legal Permanent Residents

I believe it is more appropriate to use noncitizen authorized immigrants as the comparison group for this analysis, as we might expect naturalized citizens to have very different characteristics from other immigrants. However, an argument can be made to include naturalized citizens in the LPR category, as they are immigrants too. As with the previous section, I rerun all results from Sections 4 and 5 on samples that include naturalized citizens. Throughout, results are similar to those in the text. I note that discrepancies that arise (generally, in the significance of an effect) tend to be consistent with the story that some undocumented immigrants lie about their citizenship status on the ACS forms. For example, Table F.2 suggests that sanctuary city policies are effective at reducing the amount paid for multi-unit housing for *all* immigrants and have smaller effects (with large standard errors) for undocumented immigrants, specifically. If undocumented immigrants report being citizens when they respond to the ACS, they will now be included in the "control" group of legal resident immigrants and the effect of policy on this subset of individuals will influence the estimates for legal resident immigrants, not undocumented immigrants, specifically. Note that, even with this possibility, the direction (if not always the significance) of estimates of interest is consistent throughout.

	Model 1	Model 2	Model 3	Model 4
undocumented	-17.91^{***}	51.82^{***}	16.41^{***}	1.21
	(2.91)	(5.81)	(3.36)	(7.47)
years in U.S.			0.56^{***}	1.14^{***}
			(0.17)	(0.19)
multi-unit			-128.26^{***}	-154.13^{***}
			(5.21)	(6.07)
undocumented $\times$ years in U.S.				-2.02^{***}
				(0.30)
undocumented $\times$ multi-unit				65.82^{***}
				(6.00)
Adj. R^2	0.01	0.26	0.52	0.52
Num. obs.	188191	188191	188191	188191

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

Table F.1: Section 4 equivalent. Effect on gross rent.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
undocumented	4.15	−6.66	−7.02			
	(3.35)	(7.46)	(7.71)			
treat	-13.67^{***}	-13.39^{**}	9.67	−8.48	−8.42	15.72^{*}
	(5.27)	(5.25)	(8.43)	(5.71)	(5.71)	(9.11)
treat × undocumented	38.25^{***}	36.66^{***}	46.45^{***}	21.48^{***}	21.60^{***}	32.35^{**}
	(6.52)	(6.42)	(10.70)	(8.04)	(8.03)	(12.70)
years in U.S.	0.54^{***}	1.16^{***}	1.17^{***}	0.69^{***}	1.24^{***}	1.24^{***}
	(0.17)	(0.19)	(0.19)	(0.16)	(0.18)	(0.18)
multi-unit	-128.45^{***}	-153.30^{***}	-143.71^{***}	-128.64^{***}	-147.42^{***}	-137.29^{***}
	(5.21)	(6.04)	(5.99)	(5.16)	(5.72)	(5.67)
undocumented × years in U.S.		-2.14^{***}	-2.15^{***}		-1.95^{***}	-1.96^{***}
		(0.30)	(0.30)		(0.29)	(0.29)
undocumented × multi-unit		63.23^{***}	64.36^{***}		48.61^{***}	49.75^{***}
		(5.83)	(6.26)		(5.68)	(6.32)
treat × multi-unit			-29.53^{***}			-30.64^{***}
			(8.92)			(9.02)
treat × multi-unit × undocumented			−13.26			−15.33
			(11.95)			(11.87)
Adj. R^2	0.52	0.52	0.52	0.53	0.53	0.53
Num. obs.	188191	188191	188191	188191	188191	188191

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

Table F.2: Section 5.3 equivalent. Effect on gross rent.

	Rent	*Rent*	*Income*	$\frac{Rent}{Income}$
treat	−5.65	5.13	−20.49	−0.0001
	(5.37)	(8.25)	(36.35)	(0.0028)
treat × undocumented	12.46^{*}	29.59^{***}	45.56	-0.0078^{*}
	(7.13)	(11.44)	(57.87)	(0.0045)
undocumented × multi-unit	44.17^{***}	48.75^{***}		
	(5.23)	(5.60)		
treat × multi-unit		-13.74^{*}		
		(8.13)		
treat × multi-unit × undocumented		-23.24^{**}		
		(10.90)		
Adj. R^2	0.56	0.56	0.33	0.1917
Num. obs.	158648	158648	158648	158648

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

Table F.3: Section 5.4 results where naturalized citizens are categorized as legal residents. The sample restrictions applied are equivalent to those of the "Unr" (unrestricted) sample in the main text. Results are quite similar across the other subsamples. In columns 1 and 2, the dependent variable is gross monthly rent. In column 3, the dependent variable is monthly income. In column 4, the dependent variable is rent as a fraction of income.

Seeking Sanctuary:

Housing Undocumented Immigrants

Online Appendix (not for publication)

Online Appendix I. Declined Detainers Image via ICE

Section III: Table of Jurisdictions that have Enacted Policies which Restrict Cooperation with ICE

All jurisdictions and their corresponding detainer ordinances listed in this document are based upon public announcements, news report statements, and publicly disclosed policies. As such, there may be other non-cooperative jurisdictions not contained in this table if publicly available information does not exist. The entries below are sorted by the date a policy was enacted in the stated jurisdiction with the most recent date first.

Jurisdiction (AOR)	Date Enacted	Policy	Criteria for Honoring Detainer
Baltimore City, Maryland (Baltimore)	March 2017	Baltimore Police Commissioner	• Public statement of noncooperation with Immigration and Customs Enforcement
Maricopa, Arizona (Phoenix)	February 2017	Sheriff's Statement	• Maricopa County will not honor requests to hold individual
Tulare, California (San Francisco)	February 2017	Sheriff's Statement	• Will notify ICE five days prior to the inmates release but will not hold
Ithaca, New York (Buffalo)	February 2017	Municipal Code Change	• Will only honor "warrantless detainer requests from the federal government under limited, specified circumstances" such as violent or serious crimes or terrorist activities
City of Seattle, Washington (Seattle)	February 2017	Resolution 31730	• City department directors are directed to comply with City's practice to defer to King County on all ICE detainer requests • City of Seattle employees are directed, unless provided with a criminal warrant issued by a federal judge or magistrate, to not detain or arrest any individual based upon an administrative or civil immigration warrant for a violation of federal civil immigration law, including administrative and civil immigration warrants entered in the National Crime Information Center database
Travis County, Texas (San Antonio)	January 2017	Travis County Sheriff's Office Policy on Cooperation with U.S. Immigration and Customs Enforcement	• Willing to accept requests accompanied by a court order • Willing to accept requests when the subject of the detainer request is charged with or has been convicted of Capital Murder, First Degree Murder, Aggravated Sexual Assault, or Continuous Smuggling of Persons
Iowa City, Johnson County, Iowa (Saint Paul)	January 2017	Resolution Reaffirming the Public Safety Function of Local Law Enforcement	• Willing to only accept some notifications on detainers

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The full report can be found under archived reports on ICE's website (https://www.ice.gov/declined-detainer-report).

Online Appendix II. Sample Rental Application Forms

This first form allows applicants to use an ITIN (which undocumented immigrants can legally obtain) in place of a Social Security Number.

APPLICATION TO RENT

(All sections must be completed) **Individual applications required from each occupant 18 years of age or older.**

Last name | First Name | Middle Name | Social Security Number or ITIN

Other names used in the last 10 years | Work phone number ( ) | Home phone number ( )

Date of birth | E-mail Address | Mobile/Cell phone number ( )

Photo ID/Type | Number | Issuing government | Exp. Date | Other ID

1. Present Address | City | State | Zip
   Date in | Date out | Owner/Agent Name | Owner/Agent Phone Number
   Reason for Moving | Current Rent $ /Month

2. Previous Address | City | State | Zip
   Date in | Date out | Owner/Agent Name | Owner/Agent Phone Number
   Reason for Moving

3. Previous Address | City | State | Zip
   Date in | Date out | Owner/Agent Name | Owner/Agent Phone Number
   Reason for Moving

Proposed Occupants: List all in addition to yourself | Name | Name | Name | Name | Name | Name

Will you have pets? | Describe | Will you have a waterbed? | Describe

How did you hear about this rental?

I ☐ am ☐ am not a member of the Armed Forces (including the National Guard and Reserves)

A. Present occupation or source of income | Employer Name
   Dates of Employment | Supervisor's phone number ( ) | Employer Address
   Name of your supervisor | City, State, Zip

B. Present occupation or source of income | Employer Name
   Dates of Employment | Supervisor's phone number ( ) | Employer Address
   Name of your supervisor | City, State, Zip

Current Gross Income $ Per | Check one ☐ Week ☐ Month ☐ Year | **Please list ALL of your financial obligations below**

Name of your bank	**Branch or address**	**Account Number**

Form Provided by Contemporary Information Corp.
For Membership Information or to Order Forms Call (800) 288-4757
or Visit our Website at www.contemporaryinfo.com

CIC

Form 157.1 © 2009 (Revised 5/09)

The second form is more strict, requiring a SSN, driver's license, and bank information.

TENANT APPLICATION FORM

Property Information

Address: | Rent $ | Deposit $

Applicant History

Applicant's Full Name (Last, First, Middle Initial) Jr/Sr | Date of Birth | Social Security Number | Drivers License #

Phone # (Home) | Phone # (Work) | Email:

Name of Co-Applicants (*Separate Application required for each Co-Applicant*)
(Last, First, Middle Initial) | (Last, First, Middle Initial) | (Last, First, Middle Initial)

Applicant's Present Address City Zip | Dates: From - To

Monthly Payment $ | [ ] Rent [ ] Own | [ ] Apartment [ ] House

Present Landlord's Name Address City Zip Phone #

Applicant's Prior Address City Zip | Dates: From - To

Monthly Payment $ | [ ] Rent [ ] Own | [ ] Apartment [ ] House

Prior Landlord's Name Address City Zip Phone #

Proposed Occupants

1 - (Last, First, Middle Initial) Date of Birth | 3 - (Last, First, Middle Initial) Date of Birth

2 - (Last, First, Middle Initial) Date of Birth | 4 - (Last, First, Middle Initial) Date of Birth

Does Applicant or any Proposed Occupant smoke? [ ] yes [ ] no

Do you own a pet? [ ] yes [ ] no Number of pets: ______ Type:

Employment

Current Employer (if self-employed, name of business) Business Address

Position | Type of Business | Dates: From - To | Monthly Income

Supervisor | Supervisor Phone | Other Income $ Source

Prior Employer (if self-employed, name of business) Business Address

Position | Type of Business | Dates: From - To | Monthly Income

Supervisor | Supervisor Phone | Other Income $ Source

Financial Info

Checking: bank and branch (include City/State) | Account #

Savings: bank and branch (include City/State) | Account #

Have you ever filed bankruptcy? [ ] yes [ ] no County/State where filed: What year?

Have you or any proposed occupant ever:
Been convicted of a felony? [ ] yes [ ] no Describe: ______________________________
Been evicted from a rental? [ ] yes [ ] no Describe: ______________________________
Defaulted on a lease? [ ] yes [ ] no Describe: ______________________________

Page 1 of 2

TENANT APPLICATION FORM
(continued)

Applicant Name

Applicant's Full Name (Last, First, Middle Initial) Jr/Sr

Personal Info

In case of emergency, please notify: (local name, address & phone number) Relationship:

Auto Make Model Year License # State

Reason for relocation? | Do you have renter's insurance? [ ] yes [ ] no

Consent to Verification of Credit and Other Information

I warrant, to the best of my knowledge, all of the information provided in this Application is true, accurate, complete and correct as of the date of this Application. If any information provided by me is determined to be false, such false statement will be grounds for disapproval of my Application or termination of my Lease with Owner.

I understand and agree: (i) this is an application to rent only and does not guarantee that I will be offered the Property, and (ii) Landlord or Manager or Agent may accept more than one application for the Property and, using their sole discretion, will select the best qualified applicant. I hereby authorize the Landlord or Manager or Agent to verify the information provided and obtain a credit report on me.

Applicant's Signature: ______________________________ Date: ______________

Receipt for Application Screening Fee

To Be Completed by Landlord, Manager or Agent

Applicant has paid a nonrefundable screening fee of $____________, applied as follows: $ _______ for credit reports, $________ for processing and verifying screening information (may include staff's time and related costs), and $______ for other out of pocket expenses.

The Applicant has read the foregoing and acknowledges receipt of a copy:

Applicant's Signature: ______________________________ Date: ______________

The undersigned has received the screening fee indicated above.

Landlord, Manager, or Owner Signature: ___________________________ Date: _________

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Images found via Google search and taken from slideshare.net and syncronizer.com

Online Appendix III. Section 4 Results for Section 5.4 Subsamples

Regressions from Section 4 are rerun for the subsamples used in Section 5.4. Column 3 consistently illustrates that undocumented immigrants pay a premium for rental housing. Column 4 consistently finds that the premium is driven by multi-unit housing.

	Model 1	Model 2	Model 3	Model 4
undocumented	-17.64^{***}	27.40^{***}	29.45^{***}	-0.47
	(3.36)	(4.22)	(3.13)	(7.32)
years in U.S.			-0.43^{**}	-0.48^{*}
			(0.20)	(0.25)
multi-unit			-83.79^{***}	-109.10^{***}
			(4.52)	(5.95)
undocumented × years in U.S.				0.03
				(0.30)
undocumented × multi-unit				41.34^{***}
				(5.98)
Adj. R^2	0.01	0.33	0.57	0.57
Num. obs.	93776	93776	93776	93776

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

Table Online Appendix III.1: "Unr" or Unrestricted sample

	Model 1	Model 2	Model 3	Model 4
undocumented	-47.97^{***}	-4.57	13.10^{***}	-7.28
	(3.27)	(3.33)	(2.73)	(7.10)
years in U.S.			0.30	0.23
			(0.18)	(0.24)
multi-unit			-69.84^{***}	-86.49^{***}
			(4.21)	(5.44)
undocumented × years in U.S.				0.10
				(0.28)
undocumented × multi-unit				26.61^{***}
				(5.60)
Adj. R^2	0.01	0.32	0.54	0.54
Num. obs.	72167	72167	72167	72167

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

Table Online Appendix III.2: "Inc" restriction

	Model 1	Model 2	Model 3	Model 4
undocumented	-18.57^{***}	15.58^{***}	18.50^{***}	-17.06^{**}
	(3.51)	(4.01)	(3.01)	(7.56)
years in U.S.			0.98^{***}	0.52^{*}
			(0.17)	(0.27)
multi-unit			-61.68^{***}	-82.51^{***}
			(4.32)	(5.75)
undocumented × years in U.S.				0.78^{**}
				(0.32)
undocumented × multi-unit				33.37^{***}
				(6.01)
Adj. R^2	0.01	0.34	0.58	0.58
Num. obs.	61481	61481	61481	61481

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

Table Online Appendix III.3: “Hisp” restriction

	Model 1	Model 2	Model 3	Model 4
undocumented	-10.73^{***}	28.03^{***}	21.40^{***}	-27.39^{***}
	(3.55)	(4.27)	(3.23)	(7.91)
years in U.S.			1.07^{***}	0.34
			(0.19)	(0.28)
multi-unit			-63.22^{***}	-87.18^{***}
			(4.43)	(5.95)
undocumented × years in U.S.				1.31^{***}
				(0.33)
undocumented × multi-unit				38.79^{***}
				(6.35)
Adj. R^2	0.01	0.32	0.56	0.56
Num. obs.	60169	60169	60169	60169

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

Table Online Appendix III.4: “Educ” restriction

Online Appendix IV. Other Outcomes of Interest

Linear Probability Model for Type of Unit Rented

Results from a regression where the outcome of interest is the indicator for whether a renter resides in multi-unit housing. Estimates provide suggestive evidence that undocumented immigrants are more likely to reside in multi-unit housing if they live in a sanctuary city. Note that increased demand for these units would, in isolation, *increase* the amount undocumented renters pay for multi-unit housing. However, results from Section 5 indicate that the policies induce undocumented renters to pay significantly *less* for these kinds of units, further reinforcing the theory that search frictions drive the results for multi-unit housing.

	Model 1	Model 2	Model 3	Model 4
undocumented	-0.0054	-0.0124^{***}		
	(0.0041)	(0.0039)		
treat	-0.0055	-0.0063	-0.0039	-0.0025
	(0.0067)	(0.0062)	(0.0086)	(0.0078)
treat × undocumented	0.0062	0.0127^{**}	0.0018	0.0057
	(0.0064)	(0.0059)	(0.0111)	(0.0100)
PUMA × undocumented fe	No	No	Yes	Yes
Year × undocumented fe	No	No	Yes	Yes
Controls	No	Yes	No	Yes
Adj. R^2	0.2092	0.3624	0.2137	0.3657
Num. obs.	111713	111713	111713	111713

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

Table Online Appendix IV.1: LPM for multi-unit indicator. Controls, where included, are age, age squared, household income, marital status, gender, years in the U.S., time in residence (intervalled), number of people in household, number of workers in household, number of rooms, number of bedrooms, and build year (intervalled).

Linear Probability Models for Movement

These results resemble those presented in Appendix C but examine how movement within the PUMA or from outside the PUMA of current residence may drive results.

	Moved	Moved	W/in PUMA	W/in PUMA	Out	Out
undocumented	-0.0118^{***}	-0.0336^{***}	-0.0038	-0.0170^{***}	-0.0019	-0.0042^{***}
	(0.0044)	(0.0042)	(0.0035)	(0.0034)	(0.0013)	(0.0014)
treat	-0.0132^{*}	-0.0123^{*}	-0.0013	-0.0009	-0.0024	-0.0021
	(0.0070)	(0.0069)	(0.0054)	(0.0054)	(0.0019)	(0.0019)
treat × undocumented	0.0177^{***}	0.0189^{***}	0.0066	0.0071	0.0046^{**}	0.0042^{**}
	(0.0068)	(0.0064)	(0.0052)	(0.0051)	(0.0021)	(0.0020)
years in U.S.		-0.0050^{***}		-0.0011^{***}		-0.0002^{***}
		(0.0002)		(0.0002)		(0.0001)
multi-unit		0.0115^{***}		0.0049		0.0000
		(0.0040)		(0.0035)		(0.0012)
Adj. R^2	0.0366	0.0885	0.0247	0.0431	0.0177	0.0245
Num. obs.	111713	111713	111713	111713	111713	111713

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

Table Online Appendix IV.2: Excludes fixed effects interacted with undocumented status

	Moved	Moved	W/in PUMA	W/in PUMA	Out	Out
treat	-0.0048	-0.0006	0.0021	0.0035	-0.0034	-0.0032
	(0.0090)	(0.0087)	(0.0067)	(0.0067)	(0.0026)	(0.0025)
treat × undocumented	0.0064	0.0023	0.0018	0.0007	0.0067^{*}	0.0062^{*}
	(0.0115)	(0.0111)	(0.0089)	(0.0088)	(0.0037)	(0.0036)
years in U.S.		-0.0050^{***}		-0.0011^{***}		-0.0002^{***}
		(0.0002)		(0.0002)		(0.0001)
multi-unit		0.0116^{***}		0.0051		0.0002
		(0.0041)		(0.0035)		(0.0012)
Adj. R^2	0.0429	0.0934	0.0295	0.0474	0.0267	0.0331
Num. obs.	111713	111713	111713	111713	111713	111713

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

Table Online Appendix IV.3: Includes fixed effects interacted with undocumented status

Online Appendix V. Inclusion of Data from 2008-2011

There are several reasons to restrict my analysis to the period of 2012-2017. First, sanctuary city policies were typically enacted after Secure Communities completed its roll-out at the beginning of 2013. If sanctuary city policies are thought of as the "turning off" of Secure Communities policies, then it makes sense to begin the analysis only after most or all locations had Secure Communities in place (and therefore, had something to "turn off"). Secondly, the geographic boundaries, PUMA's, change between 2011 and 2012. Therefore, I can no longer include fixed effects for PUMA's in regressions. IPUMS provides a variable for consistent PUMA's (CPUMA's are broader geographic areas that remain consistent over time, but of course, lack the same degree of geographic precision that PUMA's have). Thus, in all specifications that include data from years prior to 2012 (i.e. all regressions in Online Appendix V), PUMA fixed effects are replaced with CPUMA fixed effects.[65] [66]

Results are presented in Tables Online Appendix V.1 through Online Appendix V.5. Results are of similar directions and magnitudes, and despite the lost precision most retain statistical significance at conventional levels.

[65] ACS samples prior to 2008 lack important information used in the imputation procedure to determine undocumented status.

[66] The regressions on the extended sample also (somewhat inadvertently) address the concerns one may have about the restriction I impose of limiting to counties with at least 25 undocumented households each year (Section 3). Because geographic boundaries change between 2011 and 2012, the set of counties identifiable in the data is slightly different pre- and post- 2012. Therefore, in Online Appendix V, this restriction to counties with 25 or more undocumented households per year is not applied. Despite the additional room for error the lifting of this restriction creates, results are qualitatively, quite robust.

	Model 1	Model 2	Model 3	Model 4
undocumented	8.50^{***}	41.11^{***}	35.68^{***}	9.98
	(2.45)	(5.52)	(3.59)	(8.84)
years in U.S.			-1.45^{***}	-1.19^{***}
			(0.23)	(0.25)
multi-unit			-96.78^{***}	-128.13^{***}
			(6.86)	(7.36)
undocumented × years in U.S.				-0.60^{*}
				(0.31)
undocumented × multi-unit				49.27^{***}
				(6.06)
Adj. R^2	0.00	0.27	0.52	0.52
Num. obs.	220143	220143	220143	220143

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

Table Online Appendix V.1: Effect on rent. Compare to Table 2.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
undocumented	32.08^{***}	7.81	3.19			
	(3.78)	(8.97)	(9.03)			
treat	14.65^{**}	15.21^{**}	6.96	19.06^{**}	18.92^{**}	9.99
	(6.98)	(7.13)	(11.56)	(7.70)	(7.72)	(12.48)
treat × undocumented	19.87^{***}	18.52^{***}	52.20^{***}	13.62^{*}	13.57^{*}	48.76^{***}
	(7.04)	(6.94)	(12.16)	(7.52)	(7.54)	(12.91)
years in U.S.	-1.47^{***}	-1.18^{***}	-1.18^{***}	-1.51^{***}	-1.24^{***}	-1.24^{***}
	(0.23)	(0.25)	(0.25)	(0.23)	(0.23)	(0.23)
multi-unit	-96.99^{***}	-127.82^{***}	-130.18^{***}	-96.98^{***}	-117.41^{***}	-119.70^{***}
	(6.89)	(7.37)	(8.26)	(6.93)	(7.09)	(7.90)
undocumented × years in U.S.		-0.64^{**}	-0.65^{**}		-0.56^{*}	-0.57^{*}
		(0.31)	(0.31)		(0.30)	(0.30)
undocumented × multi-unit		48.48^{***}	55.34^{***}		32.20^{***}	39.01^{***}
		(5.96)	(6.20)		(6.23)	(6.50)
treat × multi-unit			11.36			11.84
			(12.25)			(12.47)
treat × multi-unit × undocumented			-45.22^{***}			-46.47^{***}
			(13.45)			(13.46)
Adj. R^2	0.52	0.52	0.52	0.52	0.52	0.52
Num. obs.	220143	220143	220143	220143	220143	220143

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

Table Online Appendix V.2: Effect on rent. Compare to Table 3.

	Unr	Unr	Inc	Inc	Hisp	Hisp	Educ	Educ
treat	20.73^{***}	7.05	20.29^{***}	3.75	12.19^{*}	7.68	15.38^{**}	13.38
	(6.78)	(10.42)	(6.98)	(9.96)	(7.33)	(10.70)	(6.93)	(11.08)
treat × undocumented	4.64	37.86^{***}	3.76	36.59^{***}	4.54	30.64^{**}	9.28	30.68^{**}
	(6.62)	(12.10)	(7.05)	(12.62)	(7.15)	(12.25)	(7.30)	(12.00)
undocumented × multi-unit	25.99^{***}	32.86^{***}	17.16^{***}	24.51^{***}	28.32^{***}	33.33^{***}	27.41^{***}	31.26^{***}
	(5.35)	(5.61)	(5.62)	(6.01)	(5.75)	(6.36)	(5.05)	(5.49)
treat × multi-unit		18.21		22.28^{**}		6.44		2.74
		(11.32)		(11.24)		(11.43)		(12.12)
treat × multi-unit × undocumented		-43.97^{***}		-43.83^{***}		-36.33^{***}		-28.98^{**}
		(12.31)		(12.90)		(13.25)		(12.05)
Adj. R^2	0.54	0.54	0.53	0.53	0.55	0.55	0.53	0.53
Num. obs.	189675	189675	171334	171334	129205	129205	132225	132225

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

Table Online Appendix V.3: Effect on rent. Compare to Table 4.

	Unr	Inc	Hisp	Educ	Unr	Inc	Hisp	Educ
undocumented	-310.80^{***}	-349.59^{***}	-78.61^{***}	-44.61^{***}				
	(32.06)	(33.05)	(19.98)	(16.83)				
treat	-234.49^{***}	-223.12^{***}	-161.97^{***}	-141.80^{***}	-133.75^{***}	-143.98^{***}	-93.64^{**}	-55.44
	(40.86)	(43.84)	(34.52)	(30.20)	(40.48)	(43.33)	(40.04)	(36.10)
treat × undocumented	284.02^{***}	258.83^{***}	180.59^{***}	168.88^{***}	146.79^{***}	153.49^{***}	77.11^{*}	37.99
	(45.97)	(48.16)	(30.95)	(29.04)	(45.38)	(48.49)	(40.94)	(38.77)
Adj. R^2	0.29	0.28	0.40	0.40	0.30	0.29	0.40	0.40
Num. obs.	189675	171334	129205	132225	189675	171334	129205	132225

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

Table Online Appendix V.4: Effect on income. Compare to Table 5.

	Unr	Inc	Hisp	Educ				
undocumented	0.0142^{***}	0.0154^{***}	0.0056^{***}	0.0027				
	(0.0018)	(0.0017)	(0.0018)	(0.0018)				
treat	0.0194^{***}	0.0188^{***}	0.0177^{***}	0.0171^{***}	0.0139^{***}	0.0137^{***}	0.0105^{**}	0.0111^{***}
	(0.0029)	(0.0028)	(0.0036)	(0.0035)	(0.0031)	(0.0032)	(0.0042)	(0.0040)
treat × undocumented	-0.0206^{***}	-0.0185^{***}	-0.0176^{***}	-0.0160^{***}	-0.0126^{***}	-0.0111^{***}	-0.0070	-0.0066
	(0.0028)	(0.0027)	(0.0031)	(0.0032)	(0.0039)	(0.0037)	(0.0048)	(0.0048)
Adj. R^2	0.2044	0.2357	0.2366	0.2309	0.2081	0.2393	0.2393	0.2345
Num. obs.	189675	171334	129205	132225	189675	171334	129205	132225

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

Table Online Appendix V.5: Effect on rent as a fraction of income. Compare to Table 6.

Online Appendix VI. Excluding Select States

One may be concerned that the findings presented in this paper are driven by a subset of states with large undocumented populations. If, for example, premiums for housing only exist in California or sanctuary city policies are only effective in California, then the estimates produced by the analysis so far, may simply be a result of the sheer number of observations in California. It could be the case that the findings do not hold in other states and merely arise because average effects are driven by the large number of observations in states where the results *do* hold. To address this possibility, I create 4 subsamples on which I rerun the regressions that characterize the findings of this study. The first subsample drops all observations from the state of California. The second drops all observations from California and Texas. The third drops all observations from California, Texas, and Florida. The fourth drops all observations from California, Texas, Florida, and New York.[67] The regression results are presented in the tables below and support the story that premiums arising from undocumented status and alleviated by sanctuary city policies are nationwide phenomena. Compared to the results from the nationwide samples (presented in the text), the coefficients of interest nearly always retain their significance (and approximate magnitudes) and only occasionally become statistical zeroes, despite a rapidly dwindling sample size.

[67]These are the states with the largest undocumented populations.

	Model 1	Model 2	Model 3	Model 4
undocumented	41.44^{***}	-0.02	52.68^{***}	21.65^{*}
	(5.20)	(9.80)	(6.22)	(12.13)
years in U.S.	-1.10^{***}	-1.12^{***}	-1.15^{***}	-1.00^{**}
	(0.30)	(0.39)	(0.37)	(0.46)
multi-unit	-91.58^{***}	-127.54^{***}	-108.93^{***}	-137.34^{***}
	(6.17)	(8.85)	(7.36)	(10.48)
undocumented × years in U.S.		-0.04		-0.33
		(0.40)		(0.49)
undocumented × multi-unit		54.81^{***}		45.17^{***}
		(8.55)		(10.44)
Adj. R^2	0.53	0.53	0.50	0.50
Num. obs.	68518	68518	52475	52475

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

Table Online Appendix VI.1: Effect on rent (compare to last two columns of Table 2). The first 2 columns are from the sample that excludes California. The last 2 columns are from the sample that excludes both California and Texas.

	Model 1	Model 2	Model 3	Model 4
undocumented	53.11^{***}	11.99	28.21^{***}	19.25
	(7.24)	(14.88)	(6.16)	(15.59)
years in U.S.	-1.34^{***}	-1.53^{***}	-0.72^{**}	-0.48
	(0.44)	(0.55)	(0.35)	(0.49)
multi-unit	-119.40^{***}	-149.10^{***}	-116.13^{***}	-130.08^{***}
	(9.05)	(13.83)	(8.63)	(12.39)
undocumented × years in U.S.		0.34		-0.44
		(0.56)		(0.62)
undocumented × multi-unit		44.23^{***}		19.92
		(13.18)		(12.48)
Adj. R^2	0.50	0.50	0.53	0.53
Num. obs.	41617	41617	28002	28002

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

Table Online Appendix VI.2: Effect on rent (compare to last two columns of Table 2). The first 2 columns are from the sample that excludes California, Texas, and Florida. The last 2 columns are from the sample that excludes California, Texas, Florida, and New York.

	Model 1	Model 2	Model 3	Model 4
treat	26.70^{**}	8.66	29.43^{**}	1.03
	(11.46)	(20.89)	(12.32)	(21.85)
treat × undocumented	-13.47	38.97	-12.62	31.39
	(13.30)	(27.31)	(14.31)	(28.63)
years in U.S.	-1.01^{***}	-1.01^{***}	-0.86^{*}	-0.86^{*}
	(0.38)	(0.38)	(0.45)	(0.45)
multi-unit	-107.50^{***}	-110.84^{***}	-116.69^{***}	-123.38^{***}
	(7.75)	(7.91)	(9.18)	(9.48)
undocumented × years in U.S.	-0.13	-0.12	-0.41	-0.40
	(0.41)	(0.41)	(0.51)	(0.51)
undocumented × multi-unit	23.59^{***}	32.14^{***}	11.68	21.67^{**}
	(8.05)	(8.30)	(10.07)	(10.72)
treat × multi-unit		20.27		31.85
		(20.00)		(20.65)
treat × multi-unit × undocumented		-59.94^{**}		-49.80^{*}
		(26.05)		(27.09)
Adj. R^2	0.54	0.54	0.51	0.51
Num. obs.	68518	68518	52475	52475

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

Table Online Appendix VI.3: Effect on rent, including the treatment effect (compare to Table 3). First 2 columns exclude California. Last 2 columns exclude both California and Texas.

	Model 1	Model 2	Model 3	Model 4
treat	37.75***	8.94	31.34	0.14
	(13.63)	(23.91)	(20.47)	(26.31)
treat × undocumented	−8.97	28.17	−23.22	24.58
	(15.36)	(30.73)	(20.78)	(33.31)
years in U.S.	−1.31**	−1.32**	−0.54	−0.54
	(0.55)	(0.55)	(0.53)	(0.53)
multi-unit	−119.05***	−129.82***	−121.57***	−133.63***
	(12.56)	(13.98)	(13.09)	(14.20)
undocumented × years in U.S.	0.23	0.24	−0.29	−0.28
	(0.60)	(0.60)	(0.65)	(0.65)
undocumented × multi-unit	−0.59	12.43	7.05	24.13
	(12.90)	(14.62)	(14.08)	(15.24)
treat × multi-unit		32.47		39.35
		(22.13)		(26.26)
treat × multi-unit × undocumented		−42.07		−60.85*
		(28.57)		(32.10)
Adj. R^2	0.51	0.51	0.54	0.54
Num. obs.	41617	41617	28002	28002

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

Table Online Appendix VI.4: Effect on rent, including the treatment effect (compare to Table 3). First 2 columns exclude California, Texas, and Florida. Last 2 columns exclude California, Texas, Florida, and New York.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
treat	30.82***	4.60	30.58**	−4.38	39.60***	11.66	33.22*	19.24
	(11.72)	(19.89)	(12.44)	(20.28)	(13.86)	(22.22)	(18.58)	(24.96)
treat × undocumented	−23.52*	34.45	−20.83	32.86	−16.72	23.16	−27.23	3.62
	(12.63)	(26.04)	(13.49)	(26.72)	(14.77)	(28.80)	(18.05)	(30.84)
undocumented × multi-unit	26.23***	35.72***	17.03*	29.14***	−1.94	12.02	3.97	14.51
	(7.74)	(8.08)	(9.39)	(10.01)	(11.89)	(13.75)	(12.35)	(13.11)
undocumented × multi-unit × treat		−66.26***		−61.04**		−45.50*		−39.80
		(24.81)		(25.26)		(26.86)		(30.30)
Adj. R^2	0.56	0.56	0.52	0.52	0.52	0.52	0.56	0.56
Num. obs.	56416	56416	42607	42607	33501	33501	22946	22946

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

Table Online Appendix VI.5: Effect on rent after applying the sample restrictions described in Section 5.4 (compare to "Unr" columns in Table 4). Columns 1 and 2 exclude just California. Columns 3 and 4 exclude California and Texas. Columns 5 and 6 exclude California, Texas, and Florida. Columns 7 and 8 exclude California, Texas, Florida, and New York.

	Model 1	Model 2	Model 3	Model 4
treat	-81.92	-62.76	-41.49	-44.08
	(71.02)	(75.10)	(88.73)	(128.06)
treat × undocumented	10.21	51.36	46.51	-13.48
	(92.76)	(97.60)	(110.89)	(148.36)
Adj. R^2	0.33	0.33	0.33	0.34
Num. obs.	56416	42607	33501	22946

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

Table Online Appendix VI.6: Effect on income after applying the sample restrictions described in Section 5.4 and including fixed effect interacted with undocumented status (compare to the "Unr" column in the second half of Table 5). Columns 1 excludes just California. Column 2 excludes California and Texas. Column 3 excludes California, Texas, and Florida. Column 4 excludes California, Texas, Florida, and New York.

	Model 1	Model 2	Model 3	Model 4
treat	0.0113^{*}	0.0110	0.0068	0.0102
	(0.0066)	(0.0070)	(0.0077)	(0.0100)
treat × undocumented	-0.0153^{**}	-0.0128	-0.0075	-0.0083
	(0.0077)	(0.0083)	(0.0092)	(0.0118)
Adj. R^2	0.2223	0.2169	0.2098	0.2219
Num. obs.	56416	42607	33501	22946

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

Table Online Appendix VI.7: Effect on rent as a fraction of income after applying the sample restrictions described in Section 5.4 and including fixed effect interacted with undocumented status (compare to the "Unr" column in the second half of Table 6). Columns 1 excludes just California. Column 2 excludes California and Texas. Column 3 excludes California, Texas, and Florida. Column 4 excludes California, Texas, Florida, and New York.

Online Appendix VII. Other 3 Sample Restrictions

"10 yrs" refers to the sample restricted to immigrants who arrived in the U.S. at least 10 years ago. "Jobs" refers to the sample restricted just to the jobs Pew Hispanic lists have an over-representation of undocumented workers. "Deports" refers to the sample restricted to just immigrants from the 10 countries that see the highest number of deported individuals from the U.S.

	10 yrs	10 yrs	jobs	jobs	deports	deports
treat	0.58	2.48	4.06	-0.59	-2.43	4.31
	(8.54)	(11.32)	(8.59)	(11.23)	(8.42)	(11.17)
treat × undocumented	2.76	21.16	-0.50	31.02^{**}	6.13	25.01^{*}
	(9.56)	(13.91)	(9.74)	(13.83)	(9.37)	(13.50)
multi-unit × undocumented	36.36^{***}	43.06^{***}	34.59^{***}	45.96^{***}	30.44^{***}	35.63^{***}
	(7.27)	(8.00)	(6.69)	(7.30)	(6.72)	(7.11)
treat × multi-unit × undocumented		-25.24^{*}		-42.78^{***}		-26.27^{*}
		(13.49)		(13.69)		(13.41)
Adj. R^2	0.56	0.56	0.57	0.57	0.59	0.59
Num. obs.	61613	61613	60465	60465	58230	58230

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

Table Online Appendix VII.1: Effect on Gross Rent.

	10 yrs	jobs	deports	10 yrs	jobs	deports
undocumented	-97.50^{***}	-64.00^{***}	-104.56^{***}			
	(27.22)	(21.51)	(23.41)			
treat	-134.43^{***}	-91.26^{**}	-122.34^{***}	-81.54	-45.11	-61.12
	(43.65)	(36.87)	(38.55)	(51.12)	(46.63)	(51.85)
treat × undocumented	194.19^{***}	187.98^{***}	201.69^{***}	104.08	123.34^{**}	118.41^{*}
	(42.27)	(37.54)	(37.32)	(65.90)	(61.21)	(61.57)
Adj. R^2	0.35	0.39	0.41	0.36	0.40	0.42
Num. obs.	61613	60465	58230	61613	60465	58230

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

Table Online Appendix VII.2: Effect on Household Income.

	10 yrs	jobs	deports
undocumented	0.0040^{*}	0.0048^{*}	0.0057^{**}
	(0.0025)	(0.0025)	(0.0026)
treat	0.0045	0.0067	0.0065
	(0.0041)	(0.0043)	(0.0045)
treat × undocumented	-0.0149^{***}	-0.0179^{***}	-0.0165^{***}
	(0.0036)	(0.0040)	(0.0039)
Adj. R^2	0.2239	0.2367	0.2497
Num. obs.	61613	60465	58230

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

Table Online Appendix VII.3: Effect on Rent as a Fraction of Household Income.

	10yrs	jobs	deports
treat	0.0045	0.0063	0.0039
	(0.0053)	(0.0057)	(0.0063)
treat × undocumented	-0.0139^{**}	-0.0174^{**}	-0.0125^{*}
	(0.0066)	(0.0069)	(0.0069)
Adj. R^2	0.2280	0.2413	0.2536
Num. obs.	61613	60465	58230

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

Table Online Appendix VII.4: Effect on Rent as a Fraction of Household Income (first-order effect of undocumented subsumed by fixed effects).