Sanctuary City and Crime

Yuki Otsu*†

Washington University in St. Louis

October 14, 2019

Abstract

"Sanctuary policy" is a policy that limits the enforcement of immigration laws against undocumented immigrants, and gains more attention among recent debates in the U.S. about undocumented immigrants and the safety of communities. The theoretical effect of the "Sanctuary policy" on crime is ambiguous. On the one hand, sanctuary policy could attract criminals and lower the opportunity cost of crime through lower sanctions and lower apprehension probability. On the other hand, the sanctuary policy may make a spiral of trust that supports police and raises informal social control over crime. I investigate if sanctuary policy increases crime with city crime data from 1999 to 2010. Using a difference-in-difference approach, I find that sanctuary policies do not increase crime and even lower some crime categories such as robbery and burglary. Thus, it is unlikely that sanctuary policy increases crime. I investigate the channels behind the relationship and find no evidence that sanctuary policy increases foreigners or migrants.

JEL Classification Code: J68, K37, R59

Keywords: sanctuary city, crime policy, undocumented immigrants

*Department of Economics, Washington University in St. Louis. Campus box 1208, One Brookings Drive, St. Louis MO 63130-4899. Email: y.otsu@wustl.edu

†I appreciate valuable comments and suggestions from Marcus Berliant, Ian Fillmore, and Robert Pollak.

1 Introduction

On April 12 in 2019, President Trump tweeted "Due to the fact that Democrats are unwilling to change our very dangerous immigration laws, we are indeed, as reported, giving strong considerations to placing Illegal Immigrants in Sanctuary Cities only....". Although there is no single definition of sanctuary city, it is considered to be a city that does not cooperate with the federal government in terms of immigration policies[1]. The president's statement is doubtful in two senses. First, the president's statement assumes that immigrants increase crime. However, there is no consensus in the literature that more immigrants increase crime. Second, even if undocumented immigrants are dangerous, they would move to sanctuary cities voluntarily without any order from the president. Then, a sanctuary city would already suffer more crime than a non-sanctuary city.

Despite President's criticism, some cities still have sanctuary policies for the safety of cities. The predominant reason to employ sanctuary policy is to encourage undocumented immigrants to report incidents when they become witnesses or victims. As the International Association of Chiefs of Police (2004) said, "Many law enforcement executives believe that state and local law enforcement should not be involved in the enforcement of civil immigration laws since such involvement would likely have a chilling effect on both legal and illegal aliens reporting criminal activity or assisting police in criminal investigations. They believe that this lack of cooperation could diminish the ability of law enforcement agencies to effectively police their communities and protect the public they serve." Unlike the President's opinion, some local police agencies consider that sanctuary policy is more beneficial than harmful to the safety of the local community.

This paper investigates whether sanctuary policies raise crime for a city. Previous literature on sanctuary policy finds that sanctuary cities are associated with low crime rates rather than high crime rates (Martínez-Schuldt and Martínez, 2017; Martínez et al., 2018). However, the previous literature has some limitations as to the identification of the effect. This paper extends and provides a more detailed analysis than the literature. Specifically, this paper contributes to the literature in two ways. First, this paper is the first that investigates a causal effect of sanctuary policy on each category

[1] I discuss the formal definition in Section 4.1.

of crime. Martínez-Schuldt and Martínez (2017) tried to estimate the effect of sanctuary policy but could not prove the estimator is causal. Their paper assumes that a time trend can be captured by linear and quadratic terms, contrary to crime trend data. They also use three-year average crime rates as a dependent variable to smooth annual fluctuations. However, regression on average crime rates might underestimate the effect of sanctuary policy when there is an immediate effect or a lagged effect. My result shows that the sanctuary policy has a lagged effect. Hence, their model likely underestimates the coefficients.

Second, this paper focuses on each category of crime separately. The recent literature focuses only on a certain type of crime such as homicide or robbery due to the reliability of the data. However, the effect of sanctuary policy on other categories of crime is unknown. For the argument about immigrants and crime, it is important to know the effect on other categories of crime as well: in crime literature in economics, property crime and its subcategories are important since property crime is more likely motivated by rational choices. This paper investigates more details of different crime categories.

I aim to measure a causal effect of sanctuary policy on local crime rates, with a difference-in-difference approach. Following the literature, sanctuary status is determined according to a list made by the National Immigration Law Center (NILC). Using data from the Uniform Crime Reports (UCR) from 1999 to 2010, I track how much the crime rates change before and after implementation of the policy. The baseline results show that sanctuary policy does not increase violent crime rates and decreases property crime rates. More control variables support the results and make the effects significant for some subcategories such as robbery and burglary. The results are robust when the efficiency of the government is controlled. I also perform an event study to check the parallel trend assumption and the timing of the policy effect. The event study shows that (a) the parallel trend assumption holds with additional control variables, and (b) crime rates show evident decreases starting from one year after implementation. It is worth noting that none of the estimation results from either the baseline or the event study show a significantly positive effect of sanctuary policy on local crime rates, which is consistent with the literature on sanctuary policy. Finally, I test if the negative effect of sanctuary policy is caused by the sorting of people. However, none of the fraction of foreigners, migrants (including domestic migrants), and likely undocumented Mexican immigrants show any change with the implementation of the policy. Therefore, a sanctuary policy is unlikely to attract immigrants, and a lower crime rate caused by the policy could be through the trust of residents in police enforcement as claimed in the sociology literature.

2 Literature

Immigration and crime have been investigated for a long time with mixed results. For example, immigrants overall are less likely to be committed to prison (Moehling and Piehl, 2009), and more immigrants in Metropolitan Statistical Area (MSA) lead to a lower crime rate (Reid et al., 2005). Looking specifically at Mexican immigrants, Chalfin (2015) used a network instrumental variable and found negative impacts of Mexican immigrants on rape and larceny rates at MSA level but a positive impact was found for assault. Light and Miller (2018) focused on undocumented immigrants and, with a fixed effect model, found a negative association with undocumented immigrants and violent crime at the state level. Ousey and Kubrin (2018) published a recent survey on the literature in sociology. They found that most of the papers find no relationship between immigration and crime, and even among papers that found a significant relationship, most of them found the relationship negative. Hence they concluded that the overall immigration-crime association is negative but weak.

Recent literature focuses more on shocks (Piopiunik and Ruhose, 2017; Bell et al., 2013) or policy changes (Mastrobuoni and Pinotti, 2015; Baker, 2014; Freedman et al., 2018) to identify the causal effect of immigrants on crime. Piopiunik and Ruhose (2017) used an exogenous allocation of immigrants in Germany following the collapse of the Soviet Union and found its positive effect on crime. Bell et al. (2013) used two immigrant shocks in the UK and found one positive and one insignificant effect on property crime. Granting the legal status of immigrants decreased reincarceration in Italy (Mastrobuoni and Pinotti, 2015), and property crime in the US (Baker, 2014), but felony charges rose for those who had not gained legal status (Freedman et al., 2018). Hence, there is no strong consensus about whether immigrants increase local crime rates.

In terms of policy impacts on immigrants, Miles and Cox (2014) looked at whether a harsher policy against undocumented immigrants changes crime rates. In particular, they focused on the Secure Communities (SC) program, where fingerprint information collected by local police is automatically sent to the Department of Homeland Security. Miles and Cox performed a difference-in-difference regression using county-level variation of implementation timing and conclude the SC program does not change crime rates. Alsan and Yang (2019) found the SC program induces fear of deportation among Hispanics and decreases demand for safety net programs.

There are several papers investigating sanctuary cities and crime (Lyons et al., 2013; Wong, 2017; Martínez-Schuldt and Martínez, 2017; Gonzalez O'Brien et al., 2019). Martínez et al. (2018) summarized the recent contribution of the literature. So far, the literature found no or negative relationships. Lyons et al. (2013) used data at census tract level and found that an inverse relationship between the concentration of immigrants and violent crime such as homicide and robbery in a neighborhood is strong in sanctuary cities. Wong (2017) compared sanctuary and non-sanctuary cities using a matching method and found that crime rates are lower in a sanctuary city. Gonzalez O'Brien et al. (2019) used a matching method for cities and additionally ran a cross-sectional regression. Their paper found no effect on violent crime, rape, and property crime. Martínez-Schuldt and Martínez (2017) used an unconditional negative binomial model with city fixed effects to estimate an impact of the sanctuary policy. They found that the policy implementation is associated with the reduction of robbery but no significant relationship is found with homicide. Taken all together, these papers found no effect or negative effect on homicide and robbery, but none of them found a positive effect on crime rates.

My approach is similar to Martínez-Schuldt and Martínez (2017), but there are two main differences. The first difference is an identification strategy. Although Martínez-Schuldt and Martínez used unconditional negative binomial (NB) regression with city fixed effects, I use a difference-in-difference (DID) approach based on a linear model. Osgood (2000) claimed that NB regression is superior to OLS regression when the city population is small since a crime rate has a severe heteroskedasticity problem and sometimes it is necessary to add an arbitrary constant to take the logarithm of the crime rate when it is zero. Although Martínez-Schuldt and Martínez (2017) followed the claim, I rely on OLS-based regression in this paper. The reason is that I focus only on populated cities: specifically, cities where the population is more than 100,000. Since the population is large and the number of crimes is also large, one unit increase in crime changes the crime rate almost continuously. Hence, heteroskedasticity is less problematic. The second reason is that, by focusing on large cities, most of the sample cities have a positive number of crime. Summary statistics show a strictly positive number for all observations except homicide. Moreover, to control time effects, Martínez-Schuldt and Martínez (2017) assumed linear and quadratic time trends. However in Figure 1, although every crime rate decreases over time in their sample period, it sometimes shows kinks and so linear and quadratic forms cannot capture the trend of crime rates well. Hence, I use a model with year fixed effects, which is more flexible to capture time effects. Lastly, Martínez-Schuldt and Martínez used a mean crime rate over three years as a dependent variable. However, the policy effect is underestimated when there is an immediate effect from the year of implementation. This could be more problematic if the policy effects begin from the next year after implementation. My result shows that the sanctuary policy has a lagged effect. Hence, it is likely that Martínez-Schuldt and Martínez (2017) underestimated the coefficients.

The second difference is the details of the outcomes. For example, the literature focuses only on the effect on aggregated crime categories such as violent crime or property crime, and analysis of subcategories is rare. Even when subcategories are analyzed, the categories are limited to a certain type of crime such as homicide and robbery. Since felony offenders are not covered by the sanctuary policy in many cities, analysis of other crimes such as theft is also important. In this paper, I look at the effect on rape, assault, and property crime (burglary, larceny, and auto theft).

The remainder of this paper is organized as follows. In the next section, I summarize theoretical arguments about the effect of sanctuary policy on crime rates. Data and approaches are explained in Section 4. Section 5 and Section 6 show the result of regressions and robustness checks, respectively. Finally, I conclude the paper in Section 8.

3 Theoretical prediction

This section briefly summarizes theoretical arguments about the effect of sanctuary policy on crime[2]. Since the number of crimes could be decomposed by some groups, a crime rate in a city could be described by immigration status. Let J be a set of immigration status: $J = \{native, documented, undocumented\}$. The crime rate ($CR$) is:

$$CR = \sum_{j \in J} s_j \times CR_j \quad , \tag{1}$$

where s_i is the share of group i in a municipality ($s_i = population_i / (\Sigma_j population_j)$) and CR_i is the crime rate for each group ($CR_i = crime_i / population_i$). However, if a policy changes at a city level, two things should be considered to see the effect on local crime rates: sorting and incentive. Both could affect local crime rates: sorting changes the shares of each group in (1), and incentive affects the group-specific crime rates.

Sorting means that when a policy is implemented, the composition of the population could change through migration across cities. Sanctuary cities might attract undocumented immigrants from other cities since they feel safer. If undocumented immigrants have a different propensity of crime, then the local crime rate may change (Ousey and Kubrin, 2009). Crime could fall since immigrants might be less crime-prone (Butcher and Piehl, 2005; Tonry, 1997). Hence, the direction of the sorting effect is ambiguous and also depends on incentives.

Incentives rise from the cost and benefit of crime. In particular, the policy changes the opportunity costs of crime. Becker (1968) explained that crime could be rational. From this perspective, an increase in crime cost would induce less crime at an individual level. The expected cost of crime (c_{crime}) consists of probability of being caught (p_{caught}) and amount of sanctions ($\delta_{sanction}$): $c_{crime} = p_{caught} \times \delta_{sanction}$. Sanctuary policy could affect on each part. Depending on the change of each part, the overall crime cost could go up or down. Sanctuary policy lowers the sanction on undocumented immigrants when they commit a crime. Specifically, the punishment of crime decreases since undocumented immigrants are not deported by the local police. For other people, the sanction of crime does not seem to change by the policy.

However, the expected cost of crime also depends on the probability of getting

[2] For more information, see Martínez et al.(2018).

caught. Literature claims the probability could decrease because of two reasons: a spiral of trust and informal social control. Lyons et al. (2013) say “a ’spiral of trust’ that improves communication between officials and immigrants, promotes legislation protecting immigrant interests, and generates greater system-level trust in government”. Without sanctuary policy, undocumented immigrants may not report crime even when they become victims or find crime since local police might know their legal status and deportation could happen. However, with the policy, undocumented immigrants report more crime and trust police. If sanctuary policy induces residents to trust police, then police can get cooperation from residents and work more efficiently and effectively, and eventually apprehension probability would increase[3]. Also, the policy strengthens public social control, which arises from ties among residents. Buonanno et al. (2012) show that dense social interaction is associated with a lower rate of property crime. Moreover, informal public social controls are closely related to the relationship between residents and police. Using survey data in Chicago, Silver and Miller (2004) found that neighborhoods where people have more satisfaction with local police have a higher level of informal social control over delinquent behavior of youth. The probability of being caught would increase if residents strengthen informal social controls over crime. In summary, the change of the opportunity cost of crime is ambiguous, and it is hard to predict the sign of the policy effect only from theory.

4 Data & Approach

4.1 Definition of Sanctuary city

There is no unique definition of a sanctuary city. For example, Executive order 13768 in 2017 defines sanctuary city as “locales that refuse to comply with federal statute 8 U.S.C. 1373 enhancing information related to individuals’ immigration statuses with ICE or CBP”. Alternatively, the Department of Justice defines it as a “jurisdiction that may have state laws, local ordinances, or departmental policies limiting the role of local law enforcement agencies and officers in the enforcement of immigration laws”. There are also many lists of sanctuary cities among others made by the Center for

[3]Even without cooperation from residents, police have more time to engage in or more resource to allocate to their own jobs since they no longer cooperate with federal authority.

Immigration Studies (CIS)[4], and Ohio Jobs & Justice PACS (OJJPACS) [5]. Throughout this paper, I use the following definition of sanctuary policy: a sanctuary policy is the policy that limits the enforcement of immigration laws and stated explicitly in administrative documents such as resolutions, ordinances, executive orders, or police orders. A sanctuary city is a city that has any sanctuary policy. Note that I focus only on the formal sanctuary cities. There are also informal sanctuary cities, which do not cooperate with federal authorities without having an explicit statement or policy. I do not count these cities as sanctuary cities since the definition of informal sanctuary city is arbitrary to some extent[6]. Following the literature (Gonzalez O'Brien et al., 2019; Martínez et al., 2017), I use a list of sanctuary cities offered by the National Immigration Law Center (NILC)[7]. There were 42 sanctuary cities in 2010.

The list by the NILC has some advantages over other lists. One is that there is a brief description of the policy for each city, which helps to identify the date of implementation and categorize its type of policy. However, one disadvantage is that the list is updated by 2008. Following Gonzalez O'Brien et al. (2019) and Martínez et al. (2017), I check each document and made a list of sanctuary cities[8] (see Table 1). For some cities in the list in Table 1, the actual year of implementation is different from that in the list by Gonzalez O'Brien et al. (2019). This is because these cities implemented a policy before the year, and reconfirmed or amended the policy later. I use the year when the policy originally has been implemented. For example, the year of sanctuary status for San Francisco is set as 2002 in Gonzalez O'Brien et al. (2019), but originally San Francisco became a sanctuary city in 1989 and the policy was reaffirmed in 2002.

According to Kittrie (2005), sanctuary policies fall into three types (or combination of them): (1) "don't ask", (2) "don't enforce", and (3) "don't tell". "Don't ask" is a policy that limits inquiries related to nationality or immigration status. A "don't en-

[4]https://cis.org/Map-Sanctuary-Cities-Counties-and-States

[5]http://www.ojjpac.org/sanctuary.asp

[6]A list made by the CIS contains cities that have policies regarding detainers. OJJPACS includes informal sanctuary cities in the list.

[7]Definition by the NILC may not be good enough. Some cities are called informal sanctuary cities, which practically implement a policy similar to a sanctuary policy without any formal statement. This paper focuses only on formal sanctuary cities and so the effect of its policy could be underestimated.

[8]Sanctuary policy could be determined by state and county levels as well. In the list by the NILC, 4 states (Alaska, Montana, New Mexico, Oregon) and 7 counties (Sonoma County (CA), Cook County (IL), Prince George's County (MD), Butte-Silver Bow County (MT), Rio Arriba County (NM), Marion County (OR), Dane County (WI)) are considered to be sanctuary. However, local police enforcement is operated under the city government.

force" policy limits arrests or detention for immigration offenses. A "don't tell" policy limits information sharing with federal officials. I categorize each policy based on the description in NILC (2008). Moreover, sanctuary policies take one of four legislation types: (1) resolution, (2) ordinance, (3) executive order, and (4) police order. Throughout this paper, I treated these four legislation types as one policy[9].

4.2 Data source

The data ranges from 1999 to 2010. The beginning year is set to 1999 since most of sanctuary cities in the NILC list adopt the policy after 2000. The end period is chosen as 2010. This is because Martínez-Schuldt and Martínez (2017) update the list until 2010 although the original list in the NILC is updated until 2008. Moreover, this extension gives a larger sample size used for analysis.

Crime data is collected from the FBI Uniform Crime Reports (UCR). I use annual, city-level, and reported[10] crime data from 1999 to 2000 for the analysis. Categories of crime are violent crime (homicide, rape, robbery, aggravated assault) and property crime (burglary, larceny, auto theft). Sample cities are cities where their population is more than 100,000[11]. FBI also provides the number of police officers of each agency in the UCR, and I use the number of sworn officers per capita.

To get demographic information of each city, I also use data from the Census and the American Community Survey (ACS) for the analysis. The data is obtained from IPUMS for public use (Ruggles et al., 2019). Annual data is not available for all years and so I use the 5% sample of the Census in 2000 and annual sample of ACS from 2005 to 2010. Sample cities are limited to those cities that have more than 100,000 people. As a result of merging all, the total sample size is 169 cities (including 34 sanctuary cities) and 1130 city-year observations[12]. The list of sanctuary cities and the type of

[9] Although all of these legislation types put a restriction on cooperation with the federal government, the sanctuary policy could have a different effect by legislation type. However, due to lack of enough observations to identify differences across the types, I focus on the effect of sanctuary policy regardless of the legislation type.

[10] One disadvantage of reported crime data is underreporting. Especially, rape or property crime would be underreported. Since sanctuary policy could change reporting behavior, the estimators derived from reported crime data could underestimate the effects of sanctuary policy.

[11] In the UCR, some observations are treated as missing value for some reasons. I include these observations for the analysis since the number of partially missing observations is not many (96 observations among 3,016 total observations) and usually, the problem is specific to one type of crime. For example, cities in Illinois use a different definition of rape so they are not reported in the UCR.

[12] The majority of sample size difference is because of IPUMS data. Since a city code in IPUMS is

policies are summarized in Table 1.

4.3 Regression

Regression is based on a difference-in-difference approach. Specifically, I use the following specification:

$$\log y_{it} = \alpha_{sanc} Sanctuary_{it} + \beta_X X_{it} + \eta_i + \eta_t + \epsilon_{it}.$$

Outcome variable y_{it} is the crime rate of city i at year t, which is the number of reported crimes per 100,000 people in the city. I use a natural logarithm of the crime rate as a dependent variable. Both violent and property crime rates and each subcategory are analyzed. $Sanctuary_{it}$ is a binary variable that indicates the sanctuary status of city i at year t. It takes one at and after the year of policy implementation. X_{it} is a vector of time-variant control variables. Control variables are log of population, fraction of female, age groups (15-29, 30-44, 45-59, 60 or above), fraction of racial groups (Black, Asian, Pacific Islander, Native American, other race), fraction of each level of education (less than high school degree, college degree or above), fraction of people under poverty line, fraction of unemployed people. Note that time-variant control variables are not included in the baseline sample due to data limitation. η_i and η_t are city and time fixed effects, respectively. The regression coefficient of interest is α_{sanc} and the coefficient is difference-in-difference estimates of the effect of sanctuary policy. The regressions are weighted by city population and standard errors are clustered at a city level.

Table 2 and 3 show summary statistics of the baseline sample and the sample with time-variant control variables. In Table 2, violent crime consists of homicide, rape, robbery, and assault, and more than half of the violent crime rate is from assault. As for property crime, among three subcategories (burglary, larceny, and auto theft), larceny theft occupies more than half of the total. Table 3 shows summary statistics for the sample with time-invariant controls. In the 169 sample cities, there are 34 sanctuary cities. Among the three types of policies, the most popular type is the "don't enforce" policy, and its share is about half of the total observations. The "don't ask" policy

derived from Public Use Micro Data Area (PUMA), not all cities are identified.

follows and the “don’t tell” policy is the least popular. Three types of policies do not sum up to one since some policies fall into multiple categories.

Figure 1 shows the mean of crime rates for both sanctuary and non-sanctuary cities with 95% confidence interval. In Figure 1, a group of sanctuary cities is defined by their status in 2010, hence, for the early years, the group of sanctuary cities contains cities that do not have the policy yet. Point estimates in the figure indicates that the violent crime rate is higher in sanctuary cities, and that is true for each subcategory except forcible rape. However, sanctuary cities have a lower property crime rate except for auto theft rate which does not show a significant difference between sanctuary and non-sanctuary cities. Overall, both sanctuary and non-sanctuary cities show a similar trend of crime rates, but not all the trends for each subcategory of crime are monotone. Hence, linear or quadratic trends would not be appropriate.

4.4 Identification

The DID estimator gives the average treatment effect on the treated (ATT). Identification relies on change of sanctuary status and different timing of the policy implementation. The different timing is summarized in Table 1. The number of sanctuary cities increases over time. At the beginning of 1999, nine cities (Los Angels, San Francisco, Washington D.C., Chicago, Jersey City, Cleveland, Salem, Austin, and Houston) are formal sanctuary cities. The number increases to 29 by 2005 and all 42 cities in the list employ at least one type of sanctuary policy by the end of 2008. Note that no city abolishes the sanctuary policy in the sample period.

The DID approach requires a parallel trend assumption for identification of ATT: both treatment and control groups have the same time trend before and after the treatment. I check the parallel trend assumption using an event study in Section 6.

5 Main result

In this section, I show regression results with and without time-variant controls.

5.1 Baseline

Baseline results are summarized in Table 4. In Table 4, the regression equation is with city and year fixed effects as well as the post-implementation dummy, but other covariates are not included. The result shows a negative effect on violent crime as a broad category. The violent crime rate decreases by 8% compared to the rate before the sanctuary policy, but the coefficient is statistically insignificant. All subcategories have negative coefficients but none of them show a statistically significant decrease at the 5% level.

The result in Table 4 shows that the sanctuary policy decreases a property crime rate by 9.2 percent. For subcategories, the coefficients show negative signs but none of them show significance at the 5% level. The burglary rate declines 18.7 percent and the larceny rate declines 4.5 percent compared to the crime rates before sanctuary policy. The effect on the auto theft rate is negative but insignificant.

In summary, the baseline results show negative coefficients for any type of crime but most of them are not statistically significant. However, although the effects on subcategories are not evident, the sanctuary policy decreases property crime by 9.2 percent.

5.2 Time-variant controls

I also check the policy effect with other time-variant factors. Since I use ACS data from IPUMS and its annual data is available from 2005, the sample size for the regression decreases to 169 cities and 1130 city-year observations. Summary statistics of the selected sample are shown in Table 3. On average, the sample cities have large populations compared to the baseline sample.

Table 5 and 6 show the result with additional covariates. Since the sample size has changed, I also show the result of the baseline specification in the first column for each crime category. Because of this sample selection, the significance of coefficients changes. Among violent crime, the coefficients on rape and robbery show significantly negative effects. Property crime still shows a negatively significant effect. In column 5, unlike the baseline result, the negative coefficient of larceny becomes significant.

When time-variant factors are controlled, the coefficients for burglary and auto theft become significant as well. The estimated coefficients are larger than those of baseline results. The coefficient on the rape rate is 20.0 percent, which is slightly larger (in the absolute value) than the baseline result (14.6 percent). That of the robbery rate also shows a larger effect than the baseline. The effect on property crime is larger than the result in the previous section. In Table 6, the burglary rate decreases by 21.3 percent in column 4.

In summary, the inverse effect on property crime is confirmed in both sample and the effects are confirmed for its subcategories after controlling for other covariates. The results are consistent with Martínez-Schuldt and Martínez (2017), which found no effect on homicide but a negative effect on robbery. Besides, some of the remaining categories not analyzed by Martínez-Schuldt and Martínez (2017) are affected by the sanctuary policy.

6 Robustness checks

In this section, I argue the robustness of the results in the previous section. Specifically, I consider an endogeneity issue and show the results of an event study. Then I discuss the channels of the effect[13].

6.1 Endogeneity

One concern for the main result is that the negative effects on the crime rate in sanctuary cities may come from better enforcement operations. In other words, the reduction is not because of the sanctuary policy but because efficient governments induce lower crime rates as well as adoption of sanctuary policy. To confirm the possibility, I include the per-capita expenditure on education at the city-level as a control variable. The expenditure data is collected from the Annual Survey of State and Local Government Finances. I assume that expenditure on education is unrelated to crimes but it is related to the efficiency of governments. Hence, adding the variable as an additional control mitigates the endogeneity issue come from better operation. Table 9 summarizes the results. The negative impact of sanctuary policy is lower than the model without per-capita expenditure on education, but still significant.

[13]To check a possibility of self-selection bias, I also perform a matching DID in appendix A.

Another concern is reverse causality: a city government employs the sanctuary policy because crime rates are high. Since one of the reasons why a city has sanctuary policies is to encourage immigrants to report more information on crime, sanctuary policy may be used as a means to reduce high crime. To mitigate the possibility, I run DID regression with matched samples based on the total crime rate as well as other covariates in 2000. The regression results in Table 10 confirm the negative effects on robbery and burglary, but the effects on other crimes are not evident. Importantly, none of the results show positive point estimates except the case of larceny without control variables.

6.2 Event study

For the DID estimators to be valid, the time trends between treatment and control groups need to be parallel. If the parallel trend assumption fails then DID estimators are likely to contain the effects of other factors. For example, the anticipation of policy implementation could change the behavior of people even before the date of the policy implementation. Cities may start informal sanctuary policy gradually and then make it as a formal policy at some point. To check the parallel trend assumption, I perform an event study for both samples. Let k be relative event date (year) and the policy is activated at $k = 0$. I use the following specification:

$$\log y_{it} = \sum_{k=-3,k\neq-1}^{3} \alpha_k \times \mathbb{1}(t = t_i^{activation} + k) + \beta_X X_{it} + \eta_i + \eta_t + \epsilon_{it}$$

where $t_i^{activation}$ is the year of policy adoption in a city i. Regardless of the value of k, $\mathbb{1}(t = t_i^{activation} + k) = 0$ for non-sanctuary cities. I set $k = -1$ as the base year for sanctuary cities, which is one year before the implementation year. Note that $k = 3$ ($k = -3$) includes any city-year observation that has passed the policy more than 3 years before (after). The estimated coefficients α_k are summarized in Figure 2 and 3, and Table 7.

In Figure 2, where the baseline sample is used, the parallel trend assumptions seem to be violated for property crime and its subcategories. Except for auto theft, each coefficient decreases over time to the policy implementation. At two years before the implementation year, the coefficients show a significantly positive difference.

In Figure 3, as for the violent crime rate, the coefficient at the year of the policy implementation is not different from zero but it declines from the following year. The coefficients after time 1 are significant at the 5% level. As subcategory, point estimates do not show a significant difference at the year of implementation. However, all the violent crimes decrease one year later. Estimated coefficients for homicide show similar effects even after two years or more from the implementation. Other subcategories show some significant coefficients one or more years after the implementation. In particular, robbery and assault show persistent effects over three years after the implementation. However, the effect on rape is not clear. Before the policy implementation, the crime rates do not show significant differences except assault. Hence, conditional on these control variables, no violation of the parallel trend assumption is found.

Compared to violent crime, the effects on the property crime rate are less clear. The estimated coefficient at the year of implementation is slightly positive but not significantly different from the prior year. The coefficients become negative from one and three years after the implementation but not statistically significant. Among subcategories, the burglary rate shows a significant decrease at the one year after the implementation but is not for other years. The coefficients for the larceny rate slightly increases at time zero and it decreases gradually. The coefficients for the auto theft rate show negative coefficients and gradual decline over three years although none of them are significantly different from zero.

The event study results confirm decreases of violent crime, homicide, robbery, and burglary but the significance is not persistent over years except homicide. These effects become evident at least one year after the policy implementation. Also, compared to the baseline sample, the trend before the policy is better controlled and almost all crime does not show a trend starting from previous years.

6.3 Why sanctuary city policy lowers crime rates

The main results show that sanctuary policy decreases crime rates for some categories. I explained the potential reasons why the policy affects crime rates in Section 3: sorting and incentives. To explain lower crime rates by sanctuary policy, I investigate two possible channels (composition change of population and change of police force) and check how much these variables change by the policy.

First, to check the change of foreigners, I ran a regression treating a fraction of foreigners as a dependent variable. The results in Table 8 show a modest increase of the fraction but the coefficient is not significant at the 5% level with other control variables. The third and fourth columns in Table 8 show the result of a regression of a fraction of those who migrate to the city within the past year. Although the estimated coefficients show a negative impact, the fraction does not change significantly before and after the implementation of sanctuary policies. In the fifth and sixth columns, I use likely undocumented Mexican immigrants (LUMEX). The definition of LUMEX is based on Hall and Stringfield (2014). They define LUMEX as a group of immigrants from Mexico who (1) are non-citizen, (2) are not a current student, (3) do not have some college or above degree, (4) do not work in the government sector, and (5) arrived to the U.S. after 1990. The results show that LUMEX does not respond to the sanctuary policy.

Lastly, in the last two columns in Table 8, I check if the negative effects of sanctuary policy are driven by the change of the police force. Crime rates would drop if the implementation of sanctuary policy happens with an increase in the police force. To check the channel, I regress the number of sworn officers per capita as a dependent variable. The results also show negatively significant effects of the policy. Unlike the initial expectation, the number of the police force has not increased by the policy and thus the police force is unlikely to be the reason for the negative effect on crime rates.

In summary, at least, there is no evident increase in foreigners or migrants to sanctuary cities and hence, I conclude that a decrease in crime rates is unlikely due to sorting or an increase in the police force.

7 Discussion

Sanctuary policy aims to get cooperation from immigrants through reporting of crime incidents. A not small portion of crimes would be involved with foreigners since 23% of the population in sample cities are foreigners. The effect of the cooperation from the immigrants would be substantial for local police. Moreover, since there are more immigrants in a large city, sanctuary policy is more effective for the large city than small cities. The estimated results show no or negative effects on crime rates. The decreases in crime rates and the reduction of the police force after the implementation of sanctuary policy support the improvement of police operation. The large gain from efficient operation decreases crime rates reducing the per-capita police force.

I consider two channels through which sanctuary policy changes crime rates: sorting and incentives. The results do not show sanctuary policy causes any change of foreigners or migrants so I found no evidence of sorting. Hence, the reduction of crime is likely from the lower propensity of committing crimes rather than compositional change. This paper cannot identify the reason why propensity changes. For future direction, it would be helpful to understand which groups (natives, legal immigrants, and undocumented immigrants) are affected by the policy. The effects of sanctuary policy on undocumented immigrants are mainly discussed in the context. However, sanctuary policy could change the opportunity cost of crime of natives and legal immigrants as well since police operation seems to become more efficient by the sanctuary policy.

8 Conclusion

This paper investigates whether sanctuary policy increases crime. Using city-level variation of implementation timing from 1999 to 2010, I found that the sanctuary policy does not increase crime. Rather, the sanctuary policy decreases the property crime rate. The negative effect on property crime holds under regression with time-variant controls. In the model with other controls, the crime rates of various subcategories significantly decrease. In particular, the sanctuary policy decreases rape, robbery, burglary, larceny, and auto theft rate but the effects on homicide and assault are not confirmed. I also perform robustness checks. The robustness checks make some effects insignificant but none of the results show a positive association between sanctuary policy and local crime rates. In the event study design, sanctuary policy decreases homicide and robbery rates, but the effects starts one year after implementation. Moreover, the policy does not increase the proportion of foreigners or migrants, so sorting is unlikely a source of the crime rate decrease.

Sanctuary policy has no positive effect on the violent crime rate but reduces the robbery and burglary rates. In conclusion, this paper supports the sanctuary policy does not increase crime rates, consistent with Martínez et al. (2018).

9 References

Alsan, M. and S. Yang, C. S. (2019). Fear and the Safety Net: Evidence From Secure Communities. Working Paper

Baker, S. R. (2014). Effects of immigrant legalization on crime: The 1986 immigration reform and control act. mimeo

Becker, G. (1968). Crime and Punishment: An Economic Approach. Journal of Political Economy, 76(2), 169-217.

Bell, B., Fasani, F., and Machin, S. (2013). Crime and immigration: Evidence from large immigrant waves. Review of Economics and Statistics, 21(3), 1278-1290.

Buonanno, P., Pasini, G., and Vanin, P. (2012). Crime and social sanction. Papers in Regional Science, 91(1), 193-218.

Butcher, K. F., and Piehl, A. M. (2005). Crime and immigration: further evidence on the connection. Kristin F. Butcher.

Chalfin, A. (2015). The long-run effect of mexican immigration on crime in us cities: Evidence from variation in mexican fertility rates. American Economic Review: Papers & Proceedings, 105(5), 220-25.

Freedman, M., Owens, E., and Bohn, S. (2018). Immigration, Employment Opportunities, and Criminal Behavior. American Economic Journal: Economic Policy, 10(2), 117-51.

Gonzalez O'Brien, B., Collingwood, L., and El-Khatib, S. O. (2019). The politics of refuge: Sanctuary cities, crime, and undocumented immigration. Urban Affairs Review, 55(1), 3-40.

Hall, M., and Stringfield, J. (2014). Undocumented migration and the residential segregation of Mexicans in new destinations. Social Science Research, 47, 61-78.

International Association of Chiefs of Police (2004). Enforcing Immigration Law: The Role of State, Tribal and Local Law Enforcement. Retrieved from http://www.theiacp.org/Portals/0/pdfs/Publications/ImmigrationEnforcementconf.pdf

Heckman, J. J., Ichimura, H., and Todd, P. E. (1997). Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. The Review of

Economic Studies, 64(4), 605-654.

Kittrie, O. F. (2005). Federalism, deportation, and crime victims afraid to call the police. Iowa Law Review, 91, 1449.

Light, M. T., and Miller, T. (2018). Does undocumented immigration increase violent crime?. Criminology, 56(2), 370-401.

Lyons, C. J., Vélez, M. B., and Santoro, W. A. (2013). Neighborhood immigration, violence, and city-level immigrant political opportunities. American Sociological Review, 78(4), 604-632.

Martínez-Schuldt, R. D., and Martínez, D. E. (2017). Sanctuary Policies and City-Level Incidents of Violence, 1990 to 2010. Justice Quarterly, 1-27.

Martínez, D. E., Martínez-Schuldt, R. D., and Cantor, G. (2018). Providing Sanctuary or Fostering Crime? A Review of the Research on "Sanctuary Cities" and Crime. Sociology Compass, 12(1), e12547.

Mastrobuoni, G., and Pinotti, P. (2015). Legal status and the criminal activity of immigrants. American Economic Journal: Applied Economics, 7(2), 175-206.

Miles, T. J., and Cox, A. B. (2014). Does immigration enforcement reduce crime? evidence from secure communities. The Journal of Law and Economics, 57(4), 937-973.

Moehling, C., and Piehl, A. M. (2009). Immigration, crime, and incarceration in early twentieth-century America. Demography, 46(4), 739-763.

National Immigration Law Center (2008). Laws, Resolutions and Policies Instituted Across the U.S. Limiting Enforcement of Immigration Laws by State and Local Authorities. Washington, DC: National Immigration Law Center

Osgood, D. W. (2000). Poisson-based regression analysis of aggregate crime rates. Journal of Quantitative Criminology, 16(1), 21-43.

Ousey, G. C., and Kubrin, C. E. (2009). Exploring the connection between immigration and violent crime rates in US cities, 1980–2000. Social Problems, 56(3), 447-473.

Ousey, G. C., and Kubrin, C. E. (2018). Immigration and crime: Assessing a contentious issue. Annual Review of Criminology, 1, 63-84.

Piopiunik, M., and Ruhose, J. (2017). Immigration, regional conditions, and crime: Evidence from an allocation policy in Germany. European Economic Review, 92, 258-282.

Reid, L. W., Weiss, H. E., Adelman, R. M., and Jaret, C. (2005). The immigration–crime relationship: Evidence across US metropolitan areas. Social Science Research, 34(4), 757-780.

Ruggles, S., Flood, S., Goeken, R., Grover, J., Meyer, E., Pacas, J., and Sobek, M. IPUMS USA: Version 9.0 [dataset]. Minneapolis, MN: IPUMS, 2019. https://doi.org/10.18128/D010.V9.0

Silver, E., and Miller, L. L. (2004). Sources of informal social control in Chicago neighborhoods. Criminology, 42(3), 551-584.

Tonry, M. (1997). Ethnicity, crime, and immigration. Crime and Justice, 21, 1-29.

Wong, T. (2017). The Effects of Sanctuary Policies on Crime and the Economy. Washington, DC: Center For American Progress.

Appendices

A Matching DID

To evaluate the effect of sanctuary policy, treatment and control groups should be similar. However, sanctuary city and non-sanctuary cities have different demographic characteristics as Gonzalez O'Brien et al. (2019) pointed out[14]. Since sanctuary status is an outcome of choice by a local government, the local government might self-select the status and the self-selection causes a bias for the estimates. To check the bias, I perform DID regression with matched samples. The method in this section is based on Heckman et al. (1997). I match each sanctuary city based on the propensity score to be a sanctuary city by 2010. The city characteristics in 2000 are used for the propensity score matching and so I drop nine cities that start a sanctuary policy before 2000. To compute the propensity scores, following variables are used: (1) log of population, (2) % female, (3) % age groups (15-29, 30-44, 45-59, 60 or above), (4) % racial groups (Black, Asian, Pacific Islander, Native American, Other race), (5) % Hispanic, (6) % less than high school degree, (7) % college or above degree, (8) poverty rate, (9) % unemployed, and (10) the sum of violent and property crime rate. I use the kernel matching[15]. After the propensity score matching, the estimation uses 136 cities (including 14 sanctuary cities) and 920 observations.

In Table 10, the results with control variables show a negative effect on property crime but not on violent crime. As subcategories, burglary, and larceny show negative effects that are significant at the 10% level. Sanctuary policy reduces 6.5% of the burglary rate and 8.1% of the larceny rate, which are lower than the estimates without matching. For other types of crime, the coefficients are not significant at the 10% level although the point estimates are mostly negative.

In summary, sanctuary policy has negative effects on some categories of crime and no significant positive effect is found, but the level of significance changes, in particular, for violent crimes.

[14]Gonzalez O'Brien et al. (2019) noted "... sanctuary cities–compared with nonsanctuary cities–are larger, less White, more racially and ethnically diverse, have lower median incomes, have higher levels of poverty, have larger foreign-born populations, and more Democratic".

[15]To check the robustness, I also use the Nearest Neighbor matching with replacement, but the estimated coefficients are similar to the results from the kernel matching.

City	State	Year	Legislation Type	Don't ask	enforce	tell
Anchorage	Alaska	2003	Resolution	0	1	0
Chandler	Arizona	2006	Police order	1	1	1
Berkeley*	California	2007	Resolution	0	1	1
Fresno	California	2003	Police order	0	1	0
Garden Grove	California	2007	Police order	0	1	0
Los Angeles	California	1979	Police order	0	1	0
Oakland	California	2007	Resolution	0	1	0
Richmond*	California	2007	Resolution	0	1	1
San Diego*	California	2008	Police order	0	1	1
San Francisco	California	1989	Resolution	1	1	0
San Jose*	California	2007	Resolution	0	1	0
Hartford	Connecticut	2007	Resolution	1	1	0
New Haven	Connecticut	2006	Police order	1	1	1
Washington	District of Columbia	1984	Police order	1	0	0
Chicago	Illinois	1989	Executive order	1	0	1
Baltimore	Maryland	2003	Resolution	0	1	0
Boston	Massachusetts	2006	Resolution	0	1	1
Cambridge	Massachusetts	2002	Resolution	0	1	0
Ann Arbor	Michigan	2003	Resolution	0	1	0
Detroit	Michigan	2002	Resolution	0	1	0
Lansing	Michigan	2004	Resolution	1	1	1
Minneapolis	Minnesota	2003	Ordinance	1	1	0
St. Paul	Minnesota	2004	Ordinance	1	1	0
St. Louis	Missouri	2004	Resolution	0	1	0
Jersey City	New Jersey	1996	Resolution	0	1	0
Newark	New Jersey	2006	Resolution	1	0	0
Albuquerque*	New Mexico	2000	Police order	1	1	0
New York	New York	2003	Executive order	1	0	1
Syracuse	New York	2003	Resolution	0	1	0
Durham*	North Carolina	2003	Resolution	1	1	0
Cleveland	Ohio	1987	Resolution	-	-	-
Portland	Oregon	2003	Resolution	0	1	0
Salem*	Oregon	1997	Resolution	1	1	0
Philadelphia	Pennsylvania	2002	Resolution	0	0	1
Pittsburgh	Pennsylvania	2004	Resolution	1	1	0
Austin	Texas	1997	Resolution	1	0	0
Houston	Texas	1992	Police order	1	1	1
Alexandria	Virginia	2007	Resolution	1	1	1
Virginia Beach	Virginia	2007	Police order	1	0	0
Seattle	Washington	2002	Police order	1	0	0
Madison	Wisconsin	2002	Resolution	0	1	0
Milwaukee	Wisconsin	2004	Resolution	0	1	0

Note: All cities in the list have more than 100,000 population. Cities with * do not match the ACS data for public use in IPUMS.

Table 1: Sanctuary policy and timing

288 cities, 2981 obs.	Mean	SD	Min	Max
Crime rates				
Violent	819.53	433.40	55.3	2743
Homicide	10.70	8.95	0	94.7
Rape	40.02	22.07	1.48	291
Robbery	319.27	190.66	9.98	1024
Assault	467.74	266.85	25.8	1899
Property	4709.55	1912.74	1288	12837
Burglary	971.17	501.69	130	3339
Larceny	3026.71	1265.48	700	8331
Auto theft	712.20	453.38	30.3	3137
Sanctuary policy				
Sanctuary city	0.10	0.30	0	1
Don't ask	0.05	0.22	0	1
Don't enforce	0.07	0.26	0	1
Don't tell	0.02	0.15	0	1
Demographics				
Population (thousand)	1538.588	2385.525	100.010	8400.907

Note: The crime rate is defined as the number of reported crimes per 100,000 population and the statistics are weighted by city population. "Sanctuary city" takes one if an observation is after sanctuary policy has employed. In total, there are 2981 observations, but there are some missing values in the UCR except homicide and robbery. The number of observations for each crime category is violent (2902), rape (2903), assault (2980), property (2964), burglary (2979), larceny (2971), and auto theft (2975).

Table 2: Summary statistics (Baseline sample)

169 cities, 1130 obs.	Mean	SD	Min	Max
Crime rates				
Violent	815.26	434.00	55.3	2596
Homicide	10.98	8.69	0	59.4
Rape	36.72	22.17	1.48	122
Robbery	333.29	188.76	16.5	1021
Assault	457.61	259.78	25.8	1899
Property	4344.80	1847.85	1288	12501
Burglary	921.68	519.94	207	3339
Larceny	2787.75	1200.82	832	7861
Auto theft	635.93	438.03	55.7	2663
Sanctuary policy				
Sanctuary	0.16	0.37	0	1
Don't ask	0.08	0.27	0	1
Don't enforce	0.13	0.34	0	1
Don't tell	0.04	0.20	0	1
Demographics				
Population (thousand)	1961.129	2742.538	100.010	8400.907
% Female	0.51	0.01	.439	.566
% Age (15-29)	0.23	0.03	.143	.596
% Age (30-44)	0.22	0.02	.0823	.319
% Age (45-59)	0.19	0.02	.0695	.256
% Age (60+)	0.15	0.03	.0592	.242
Race				
% White	0.58	0.16	.103	.965
% Black	0.23	0.17	.00121	.85
% Asian	0.08	0.07	.000518	.456
% Pacific Islander	0.00	0.00	0	.0311
% Native American	0.01	0.01	0	.126
% Other race	0.12	0.09	.0014	.518
% Hispanic	0.27	0.19	.00225	.957
Education				
% Less than High School	0.38	0.07	.156	.653
% High School degree	0.43	0.05	.249	.607
% College degree or above	0.20	0.08	.04	.587
% Foreigners	0.23	0.13	.00983	.606
% Poverty	0.19	0.06	.0298	.445
% Unemployed	0.06	0.02	.0135	.172

Note: The crime rate is defined as the number of reported crimes per 100,000 population. Crime rates and demographics are weighted by city population. Homicide and violent crime do not count September 11 attacks. The statistics are based on 169 cities and 1130 observations. However, due to missing values in the UCR, rape and violent crime are based on 1109 observations. Similarly, larceny, auto theft, and property crime are based on 1128, 1127, and 1125 observations, respectively.

Table 3: Summary statistics (sample with time-invariant controls)

VARIABLES	(1) Violent	(2) Homicide	(3) Rape	(4) Robbery	(5) Assault
Sanctuary	-0.0799	-0.0307	-0.146	-0.122	-0.0339
	(0.0785)	(0.0677)	(0.0909)	(0.0788)	(0.0890)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	2,902	2,848	2,903	2,981	2,980
Number of Cities	285	288	285	288	288

VARIABLES	(1) Property	(2) Burglary	(3) Larceny	(4) Auto theft
Sanctuary	-0.0924***	-0.187*	-0.0446*	-0.242*
	(0.0335)	(0.107)	(0.0227)	(0.140)
City FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	2,964	2,979	2,971	2,975
Number of Cities	288	288	288	288

Note: Dependent variables are log of crime rates. The total number of cities is 288 and 2979 city-year observations but some cities and observations are missing.

Table 4: Baseline regression (violent and property crime)

VARIABLES	(1) Violent	(2) Violent	(3) Homicide	(4) Homicide	(5) Rape	(6) Rape	(7) Robbery	(8) Robbery	(9) Assault	(10) Assault
Sanctuary	-0.154	-0.126	-0.122	-0.112	-0.221**	-0.211***	-0.233**	-0.207***	-0.0795	-0.0424
	(0.110)	(0.0927)	(0.103)	(0.0894)	(0.0935)	(0.0799)	(0.0993)	(0.0730)	(0.126)	(0.108)
Control	N	Y	N	Y	N	Y	N	Y	N	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
City FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,109	1,109	1,094	1,094	1,109	1,109	1,130	1,130	1,130	1,130
Number of Cities	168	168	168	168	168	168	169	169	169	169

Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Note: Control variables are % female, % age groups (15-29, 30-44, 45-59, 60 or above), % racial groups (Black, Asian, Pacific Islander, Native American, Other race), % less than high school degree, % college or above degree, poverty rate, and % unemployed.

Table 5: Robustness check: Violent crime

VARIABLES	(1) Property	(2) Property	(3) Burglary	(4) Burglary	(5) Larceny	(6) Larceny	(7) Auto theft	(8) Auto theft
Sanctuary	-0.154***	-0.145***	-0.259*	-0.232**	-0.0944***	-0.0939***	-0.344*	-0.319**
	(0.0571)	(0.0452)	(0.145)	(0.101)	(0.0321)	(0.0324)	(0.177)	(0.150)
Control	N	Y	N	Y	N	Y	N	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
City FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,125	1,125	1,130	1,130	1,128	1,128	1,127	1,127
Number of Cities	169	169	169	169	169	169	169	169

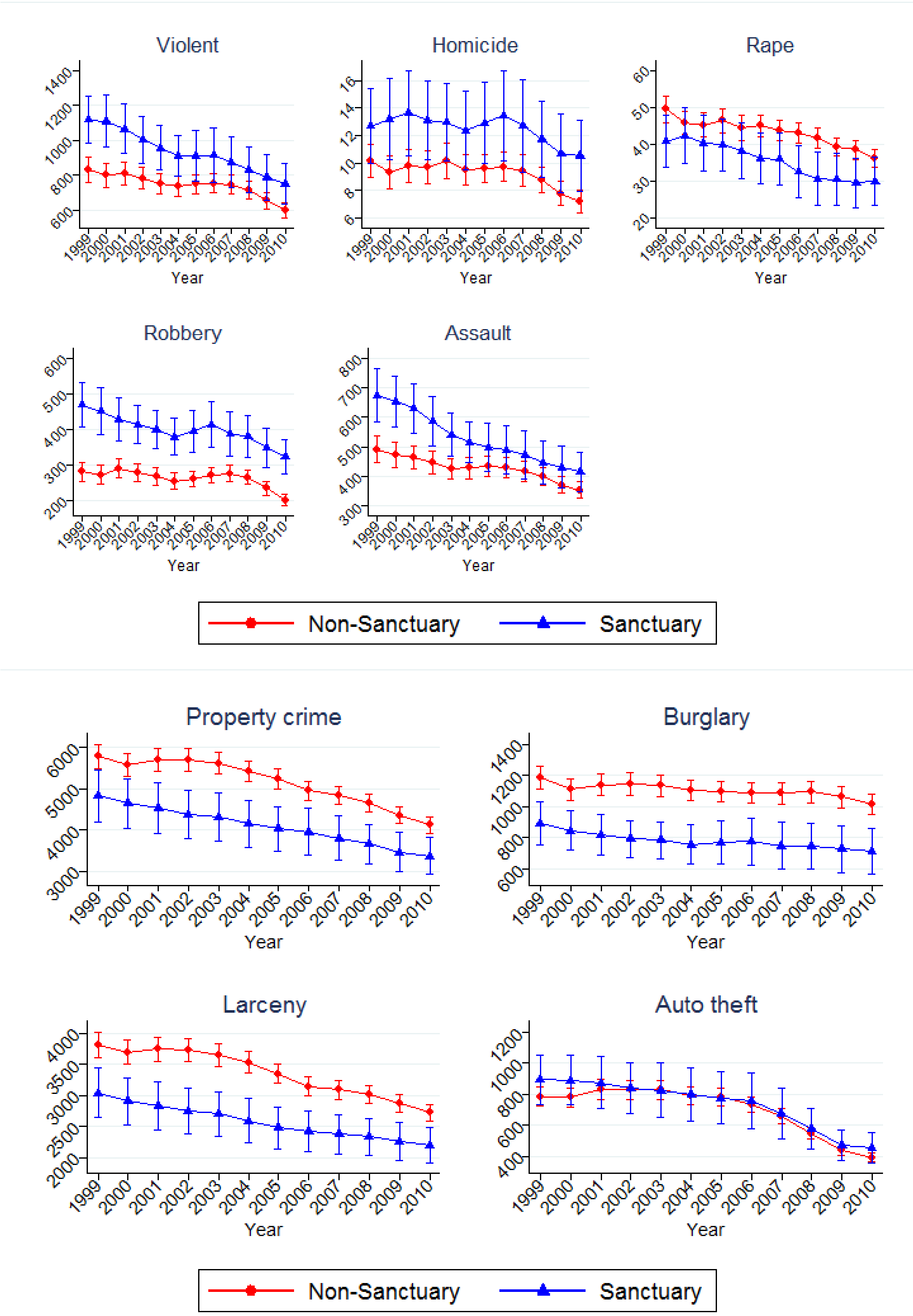
Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: Control variables are % female, % age groups (15-29, 30-44, 45-59, 60 or above), % racial groups (Black, Asian, Pacific Islander, Native American, Other race), % less than high school degree, % college or above degree, poverty rate, and % unemployed.

Table 6: Robustness check: Property crime

VARIABLES	(1) Violent	(2) Homicide	(3) Rape	(4) Robbery	(5) Assault	(6) Property	(7) Burglary	(8) Larceny	(9) Auto theft
event date (-3)	0.00718	-0.178	0.140	0.0821	-0.0881	0.110*	0.235**	0.102**	0.234
	(0.102)	(0.117)	(0.116)	(0.0937)	(0.109)	(0.0597)	(0.109)	(0.0416)	(0.174)
event date (-2)	-0.0945*	-0.171	0.0262	-0.0529	-0.147**	0.000453	-0.0668	0.0519	-0.00835
	(0.0566)	(0.120)	(0.133)	(0.0623)	(0.0660)	(0.0420)	(0.0674)	(0.0557)	(0.104)
event date (0)	-0.00673	-0.0146	0.0329	-0.0403	0.0271	0.00914	0.00186	0.0176	-0.00487
	(0.0425)	(0.0484)	(0.0640)	(0.0434)	(0.0439)	(0.0257)	(0.0324)	(0.0360)	(0.0411)
event date (1)	-0.105*	-0.174**	-0.0720	-0.133***	-0.0811	-0.0463	-0.128**	0.00584	-0.0545
	(0.0542)	(0.0862)	(0.104)	(0.0507)	(0.0732)	(0.0414)	(0.0525)	(0.0382)	(0.101)
event date (2)	-0.109**	-0.279***	-0.0126	-0.0919	-0.122**	-0.0563	-0.0385	-0.00331	-0.125
	(0.0499)	(0.0743)	(0.111)	(0.0567)	(0.0565)	(0.0430)	(0.0516)	(0.0283)	(0.109)
event date (3)	-0.160**	-0.257***	-0.168	-0.207***	-0.128*	-0.0923**	-0.127**	-0.0322	-0.224*
	(0.0656)	(0.0862)	(0.112)	(0.0658)	(0.0705)	(0.0461)	(0.0626)	(0.0301)	(0.121)
Control	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
City FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,109	1,094	1,109	1,130	1,130	1,125	1,130	1,128	1,127
Number of Cities	168	168	168	169	169	169	169	169	169

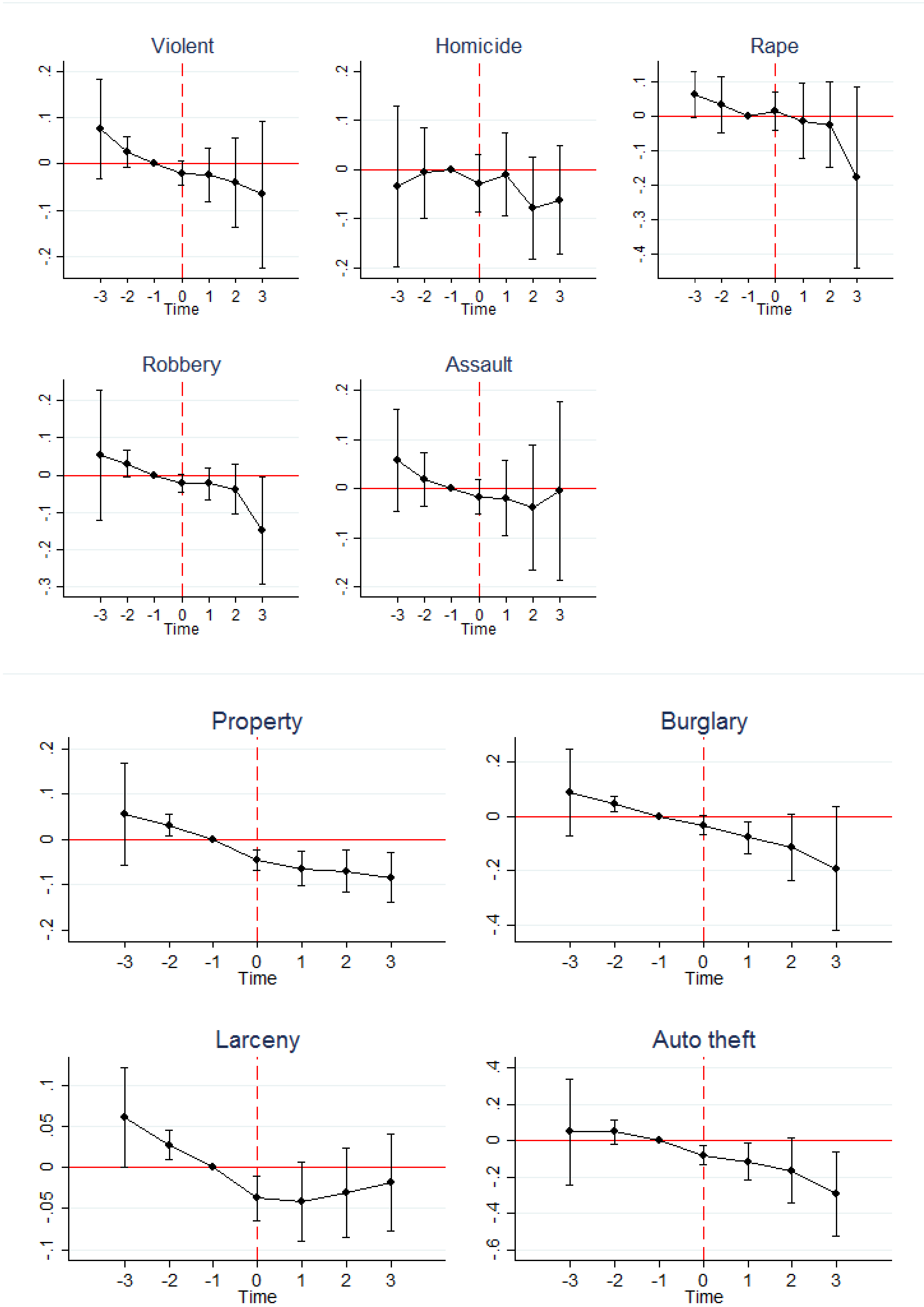
Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: Control variables are % female, % age groups (15-29, 30-44, 45-59, 60 or above), % racial groups (Black, Asian, Pacific Islander, Native American, Other race), % less than high school degree, % college or above degree, poverty rate, and % unemployed.

Table 7: Event study coefficients



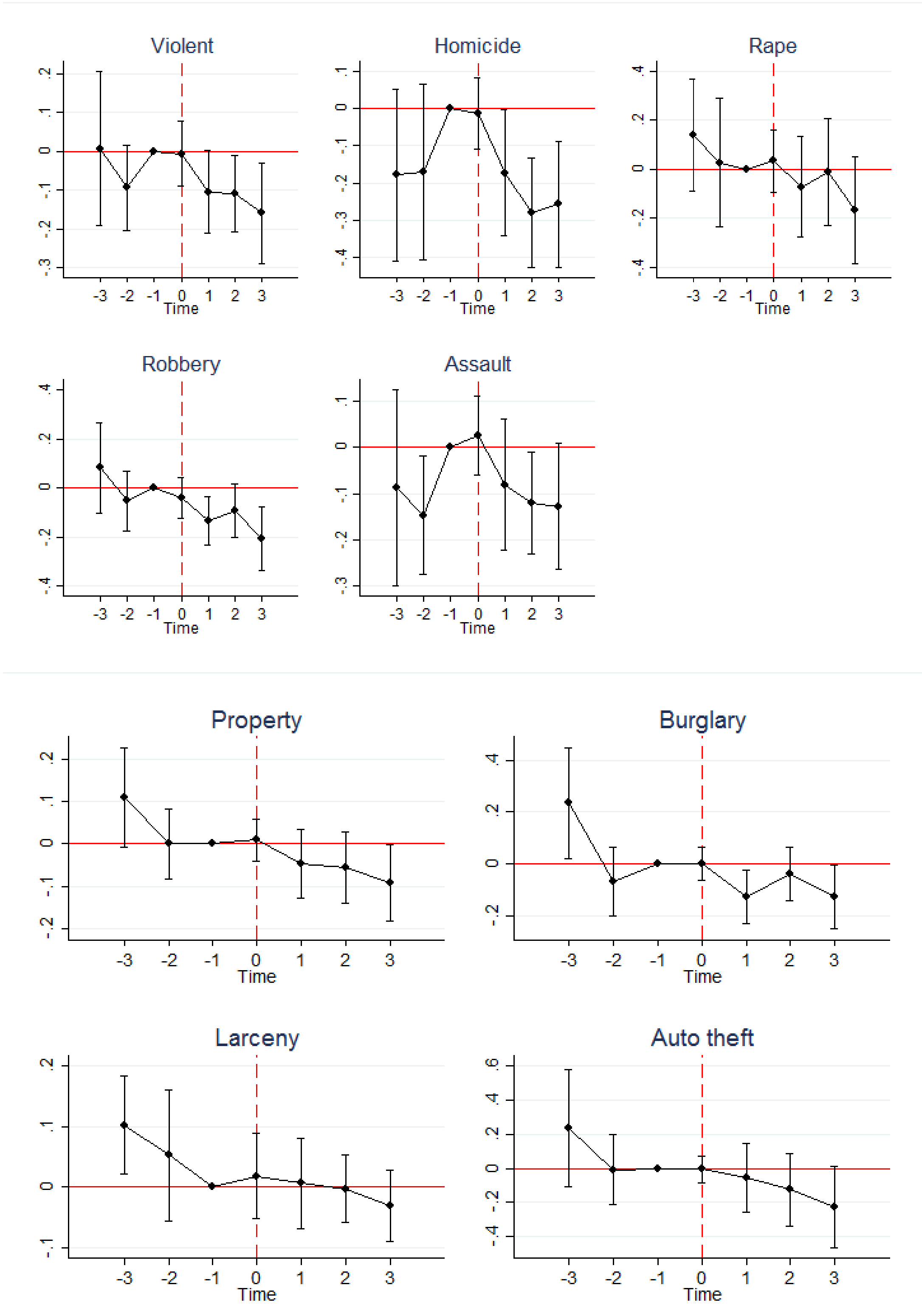
Note: Each graph shows the mean crime rate for both Sanctuary and Non-Sanctuary cities with 95% confidence interval. The crime rate is defined as the number of reported crimes per 100,000 population and weighted by city population. A group of sanctuary cities consists of every city where sanctuary policy is adopted by 2010 and thus the composition of two groups does not change over time. Hence, some cities have not adopted the policy in early years. For homicide rate in 2001, the victims from September 11 attacks are not included. Cities in Illinois are treated as missing for forcible rape rate since the definition of forcible rape used in Illinois does not comply with the UCR guidelines.

Figure 1: Crime rates by crime category and sanctuary status



Note: A set of graphs above contains all sample for the baseline result. Each graph shows the event study coefficients with 95% confidence interval. Time 3 contains sanctuary cities that have implemented the policy more than 3 years ago and Time -3 is defined similarly. Base year is one year before the implementation.

Figure 2: Event study coefficients (Baseline sample)



Note: A set of graphs is made from sample cities with time-variant controls. Each graph shows the event study coefficients with 95% confidence interval. Time 3 contains sanctuary cities that have implemented the policy more than 3 years ago and Time -3 is defined similarly. Base year is one year before the implementation. Control variables used for the regression are % female, % age groups (15-29, 30-44, 45-59, 60 or above), % racial groups (Black, Asian, Pacific Islander, Native American, Other race), % less than high school degree, % college or above degree, poverty rate, and % unemployed.

Figure 3: Event study coefficients (sample with time-variant controls)

VARIABLES	(1) % Foreigners	(2) % Foreigners	(3) % Migrants	(4) % Migrants	(5) % LUMEX	(6) % LUMEX	(7) Sworn officer	(8) Sworn officer
Sanctuary	-0.00113	-0.00100	-0.124	-0.173	-0.000429	0.000229	-0.0704**	-0.0735***
	(0.00363)	(0.00353)	(0.136)	(0.133)	(0.00133)	(0.00130)	(0.0329)	(0.0274)
Control	N	Y	N	Y	N	Y	N	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
City FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,130	1,130	973	973	1,130	1,130	1,073	1,073
Number of Cities	169	169	166	166	169	169	162	162

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: % migrants indicates fraction of people moved to the city within a year and available from 2005 to 2010. % LUMEX is a fraction of likely undocumented Mexican immigrants. "Sworn officer" is the log of the number of sworn officers in the city per capita. Control variables are % female, % age groups (15-29, 30-44, 45-59, 60 or above), % racial groups (Black, Asian, Pacific Islander, Native American, Other race), % less than high school degree, % college or above degree, poverty rate, and % unemployed.

Table 8: Effects on other outcomes

VARIABLES	(1) Violent	(2) Homicide	(3) Rape	(4) Robbery	(5) Assault
Sanctuary	-0.130	-0.115	-0.207***	-0.190***	-0.0615
	(0.0836)	(0.0879)	(0.0788)	(0.0582)	(0.102)
Control	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City FE	Y	Y	Y	Y	Y
Education expenditure	Y	Y	Y	Y	Y
Observations	1,078	1,065	1,078	1,099	1,099
Number of Cities	166	166	166	167	167

VARIABLES	(1) Property	(2) Burglary	(3) Larceny	(4) Auto theft
Sanctuary	-0.131***	-0.182***	-0.0913***	-0.275**
	(0.0388)	(0.0644)	(0.0322)	(0.115)
Control	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
City FE	Y	Y	Y	Y
Education expenditure	Y	Y	Y	Y
Observations	1,094	1,099	1,097	1,096
Number of Cities	167	167	167	167

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: Control variables are % female, % age groups (15-29, 30-44, 45-59, 60 or above), % racial groups (Black, Asian, Pacific Islander, Native American, Other race), % less than high school degree, % college or above degree, poverty rate, and % unemployed.

Table 9: Regression with government expenditure on education per capita

VARIABLES	(1) Violent	(2) Violent	(3) Homicide	(4) Homicide	(5) Rape	(6) Rape	(7) Robbery	(8) Robbery	(9) Assault	(10) Assault
Sanctuary	0.0298	-0.0181	-0.0344	-0.0373	-0.0483	-0.0988	0.0106	-0.0204	0.0606	-0.00246
	(0.0776)	(0.0501)	(0.0949)	(0.0729)	(0.0697)	(0.0749)	(0.0550)	(0.0448)	(0.121)	(0.0711)
Control	N	Y	N	Y	N	Y	N	Y	N	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	919	919	886	886	919	919	920	920	920	920
Number of Cities	136	136	135	135	136	136	136	136	136	136

VARIABLES	(1) Property	(2) Property	(3) Burglary	(4) Burglary	(5) Larceny	(6) Larceny	(7) Auto theft	(8) Auto theft
Sanctuary	-0.0708*	-0.0817**	-0.00571	-0.0656*	-0.0774*	-0.0810*	0.0204	0.0162
	(0.0420)	(0.0355)	(0.0858)	(0.0352)	(0.0440)	(0.0435)	(0.0823)	(0.0829)
Control	N	Y	N	Y	N	Y	N	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	918	918	920	920	918	918	920	920
Number of Cities	136	136	136	136	136	136	136	136

Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Note: Control variables are % female, % age groups (15-29, 30-44, 45-59, 60 or above), % racial groups (Black, Asian, Pacific Islander, Native American, Other race), % less than high school degree, % college or above degree, poverty rate, and % unemployed.

Table 10: Matching DID (Kernel)