Justice Quarterly, 2017
https://doi.org/10.1080/07418825.2017.1400577

Sanctuary Policies and City-Level Incidents of Violence, 1990 to 2010

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Despite media coverage of isolated incidents of violent crime perpetuated by undocumented immigrants in cities with sanctuary policies, there is scant systematic research on the relationships between the adoption of sanctuary policies, unauthorized immigration, and crime. We compile city-level data from official sources and use fixed-effects negative binomial regression to examine whether the adoption of city-level sanctuary policies and the concentration of unauthorized Mexican immigrants are associated with homicide and robbery incidents in 107 U.S. cities, across three decades. We find evidence that the adoption of sanctuary policies is associated with a reduction in robberies but not homicide. In contrast, an increase in the relative size of a city's unauthorized Mexican immigrant population corresponds with a reduction in homicide; however, only in sanctuary cities. Lastly, shifts in violence during our study period are consistently related to social structural characteristics of cities, which are findings consistent with social disorganization theory.

Keywords sanctuary cities; homicide; robbery; social disorganization; undocumented immigrants; Latino immigration

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Introduction

Within his first week of office, President Donald Trump signed an executive order entitled "Enhancing Public Safety in the Interior of the United States" (Executive Order No. 13768, 2017). The goal of the executive order, as the name implies, is to strengthen national security and public safety in the United States by expanding interior immigration enforcement. Part of the executive order's efforts to bolster security includes withholding federal grants, except for those funding law enforcement initiatives, from "sanctuary"[1] jurisdictions until they fully cooperate with the federal government in the enforcement immigration law. The U.S. Department of Justice (DOJ) broadly defines a "sanctuary" 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" (2007, p. 44). In contrast to the DOJ's definition, the executive order defines sanctuary jurisdictions as locales that refuse to comply with federal statute 8 U.S.C. 1373, which restricts jurisdictions from adopting policies that prohibit government entities or personnel from exchanging information related to individuals' immigration statuses with U.S. Immigration and Customs Enforcement (ICE) or Customs and Border Protection. According to the executive order, "Sanctuary jurisdictions across the United States willfully violate Federal law […]. These jurisdictions have caused immeasurable harm to the American people and to the very fabric of our republic" (Executive Order 13768, 2017). Some advocates contend that no existing sanctuary city ordinances violate 8 U.S.C. 1373 (Immigration Legal Resource Center, 2017), making the DOJ's broader definition a more accurate conceptualization of the ways in which sanctuary jurisdictions function. For this reason, we use the term "sanctuary" to refer to policies or jurisdictions that are consistent with the DOJ's definition in the remainder of the article. Irrespective of definitions, as of April 2017 there was a nation-wide preliminary injunction blocking the executive order as part of the ongoing legal challenge in *City and County of San Francisco v. Trump*.

President Trump's executive order targeting sanctuary jurisdictions appears motivated by a broader politicized discourse in the United States surrounding the issues of unauthorized immigration and crime. Notwithstanding a large body of evidence to the contrary (Bersani, Loughran, & Piquero, 2014; Martinez, Stowell, & Lee, 2010; Ousey & Kubrin, 2009, 2014, 2017, 2017; Powell, Perreira, & Mullan Harris, 2010; Vaughn, Salas-Wright, DeLisi, & Maynard, 2014), opponents of sanctuary cities, including policymakers, often

1. We acknowledge that the term "sanctuary", which is often used to describe policies or jurisdictions that limit the involvement of local law enforcement in enforcing federal immigration law, is contested and characterized by some as a misnomer since these policies or jurisdictions do not protect migrants from federal enforcement efforts (Tramonte, 2011). However, given its colloquial use in political and popular discourse, we use the term "sanctuary" throughout this paper for the sake of clarity and parsimony.

associate immigrants with criminality at the individual-level and immigration with crime at macro-levels (e.g. neighborhood or city) on the basis of stereotypes rather than evidence (Ewing, Martínez, & Rumbaut, 2015). A statement made by President Trump when he announced his candidacy best captures the stereotype of "immigrant criminality." Trump stated, "When Mexico sends its people, they're not sending their best […] They're sending people that have lots of problems […] They're bringing drugs, they're bringing crime, they're rapists, and some, I assume, are good people" (Washington Post Staff, 2015). According to the anti-sanctuary perspective, such policies foster crime because they "encourage illegal immigration and undermine federal enforcement efforts" (Garcia, 2009, p. 1). That is, opponents of sanctuary policies believe that these ordinances jeopardize public safety by increasing the size of the unauthorized immigrant population in certain jurisdictions and by acting as a barrier to federal enforcement efforts, preventing the removal of "criminal aliens"[2] that local authorities encounter (Vaughan, 2017). Political and social opponents often draw on anecdotal, high-profile incidents as evidence of both immigrant criminality and the criminogenic effects of sanctuary policies. These value judgments are empirical questions that warrant closer empirical examination. Nevertheless, only a few studies to date have examined the relationship between sanctuary policies and crime.

We contribute to the emerging research on the sanctuary-crime nexus by empirically examining *how, if at all, the adoption of a city-level sanctuary ordinance is associated with changes in city-level incidents of homicide and robbery*. We supplement our research by considering an additional question: *what, if any, is the relationship between the concentration of the unauthorized Mexican immigrants in U.S. cities and violent crime*?

We contend that addressing the macro-level relationship between unauthorized Mexican immigration and crime, in addition to the effect of sanctuary policies, stands as an important contribution for several reasons. First, the contemporary fears and the moral panic over both immigration and sanctuary policies, at least as they relate to crime, are largely constructed in terms of *undocumented* immigration. We believe this is clearly illustrated by the recent executive order's claim that "Many aliens who illegally enter the United States […] present significant threat to national security and public safety (Executive Order No. 13768, 2017). As noted above, the executive order also directly

2. Contrary to these arguments, data from the Criminal Alien Program (2010—2013) suggest that the majority of "criminal aliens" were removed for non-serious offenses related to violations of traffic, drug, or immigration laws, not serious violent offenses (Cantor, Noferi, & Martinez, 2015). Regardless, many "sanctuary" policies, such as San Francisco's, allow for local government assistance and cooperation in the enforcement of federal immigration law when individuals have been convicted of violent or serious felony offenses (see San Francisco's Due Process for All and Sanctuary 96-16 ordinance). Thus, sanctuary cities do not completely protect non-citizens from deportation (Tramonte, 2011).

claims that sanctuary jurisdictions shield deportable (i.e. unauthorized) noncitizens from the reach of the federal government.

A non-trivial proportion of the American public hold related attitudes. A recent study of U.S. adults found that about fifty percent of people believe that immigrants make crime worse in the United States (Pew Research Center, 2015). In the same study, respondents were asked to think of the first word that comes to mind when they think about immigrants and the most common response was "illegal." Evidence also suggests that U.S. white attitudes towards immigrants, in general, are largely reflective of underlying attitudes towards Latinos and Latin American immigrants in particular (Valentino, Brader, & Jardina, 2013). For these reasons, we focus on unauthorized *Mexicans* considering that they make up the largest subgroup of both Latin American and unauthorized immigrants and given President Trump's characterization of Mexican immigrants, in particular, as a threat to public safety.

The second major contribution of examining the relationship between *unauthorized Mexican* immigration and crime relates to conventions in contemporary research. Specifically, existing macro-level research predominantly measures immigration/immigrant concentration as "percent foreign born," "percent recent foreign born," "percent Latino immigrant," or by using an "immigrant concentration" index (Ousey & Kubrin, 2017). This approach neglects the impact that specific immigrant groups, including unauthorized migrants, may or may not have on crime (Ousey & Kubrin, 2009). Furthermore, the few existing studies that examine the impact of unauthorized immigration on crime often rely on logical, yet crude, approximations, such as noncitizen Latinos (Gonzalez, Collingwood, & El-Khatib, 2017). In contrast, we rely on a novel methodological approach, detailed below, with multiple exclusion criteria to identify likely undocumented Mexican immigrants in order to estimate their concentration within each city (Hall & Stringfield, 2014).

We address our research questions by drawing on data from the National Immigration Law Center (NILC), the American Community Survey (ACS), and the FBI's Uniform Crime Reports (UCR). We employ fixed-effects negative binomial regression to estimate the criminogenic effect of sanctuary policies net of relevant sociodemographic and socioeconomic structural characteristics found to be associated with crime (Sampson, Raudenbush, & Earls, 1997). Our findings suggest that the adoption of a sanctuary policy is associated with a modest reduction in robberies over time, but not related to changes in incidents of homicide. We also find that measures of immigrant concentration are inversely related to homicide and robbery, but only in sanctuary cities. Finally, fluctuations in city-level homicide and robberies during our study period appear to be most consistently related to changing social structural characteristics identified in previous research. Before discussing our data, analyses, results, and the implications of our research in detail, we briefly review contemporary sanctuary policies and their possible relationship to crime as well as the extant literature on immigration and crime.

Contemporary Sanctuary Policies

Although sanctuary policies trace their origins to the 1980s "Sanctuary Movement,"[3] the reemergence of sanctuary policies in the early twenty-first century was largely a reaction to the devolution of immigration enforcement efforts in the United States from the federal level to local levels. The 1996 Illegal Immigration Reform and Immigration Responsibility Act (IIRIRA) facilitated this process, though it was expedited through ICE initiatives after the 9/11 terrorist attacks on the World Trade Center (Meissner, Kerwin, Chrishti, & Bergeron, 2013). For example, Section 287(g) of IIRIRA allows ICE to enter into agreements with local law enforcement agencies and authorizes local law enforcement agents with requisite training to enforce federal immigration law. These partnerships are now known colloquially as the 287(g) program.

In a similar vein, ICE enacted a separate program known as Secure Communities, which operated between 2008 and 2014 (Meissner et al., 2013). Unlike the 287(g) program, Secure Communities established a partnership between ICE and local law enforcement agencies by creating a national electronic biometric information system that cross-referenced individuals' immigration and criminal records across multiple federal databases (Kubrin, 2014). If an individual's information appeared in the system, ICE officials might issue a "detainer," which would direct local law enforcement agents to hold them for up to 48 hours beyond the time that they would otherwise be detained so ICE officials could obtain custody of the individual. Secure Communities was discontinued in 2014, but President Trump's 2017 executive orders called for the reinstatement of the Secure Communities program (Executive Order No. 13768, 2017) and for the expansion of 287(g) agreements throughout western and southwestern states (Executive Order No. 13767, 2017).

Well before Trump's executive orders expanding interior enforcement efforts, an increasing number of cities passed municipal policies in response to the mounting pressure placed on local law enforcement officials to collaborate with the federal government in the enforcement of immigration laws. These so-called "sanctuary policies," attempting to limit the role of local officials in immigration enforcement, take a variety of forms. For example, many sanctuary policies include statements that local officials will refuse, in most cases, to honor ICE detainers requests[4] (Wong, 2017). Alternatively, cities such as Durham, North Carolina, have enacted policies that make similar statements to following: "Unless otherwise required as part of a City officer or employee's duties, by law, or by court order, no Durham City officer or employee […] shall inquire into the immigration status of any person, or engage in activities

3. See Bau (1994) for a discussion of the 1980s Sanctuary Movement.
4. See San Francisco's Due Process for All and Sanctuary 96-16 ordinance for common exceptions.

designed to ascertain the immigration status of any person" (Durham City Council, 2003). Though sanctuary policies are diverse in their approaches to limiting the role of local authorities, these policies still broadly fit with the DOJ's definition of a sanctuary jurisdiction as described above. Kittrie (2006) provides a useful typology of contemporary sanctuary policies suggesting that they typically take on one or some combination of the following three forms: (1) "don't ask" policies, which limit, but do not completely prohibit, local officials from inquiring about an individual's immigration status, (2) "don't enforce" policies, which limit local officials involvement in arrests and detention of immigrants for immigration law violations, and (3) "don't tell" policies, which limit the sharing of information with federal immigration authorities (p. 1455). Regardless of the type adopted, many policymakers and law enforcement officials have supported sanctuary policies by asserting that immigrant communities in sanctuary jurisdictions will be more likely to report crime victimization and cooperate with police investigations,[5] which would benefit the larger community and protect the general population (Kittrie, 2006).

According to data made available by the NILC, 65 cities or local law enforcement agencies have adopted sanctuary policies in some form since 1979, with the majority enacted between 2000 and 2008.[6] Considering the Trump administration's attempts to reinstate Secure Communities and expand of 287(g), it is reasonable to expect that more cities will establish sanctuary ordinances in some form. At the very least, current sanctuary cities like Chicago, Minneapolis, Seattle, and others have reaffirmed their commitment to their limited cooperation ordinances to calm fears within immigrant communities (Ruthhart & Dardick, 2017). Thus, it remains in the interests of scholars and policymakers to understand the social consequences, if any, of adopting sanctuary policies as well as outcomes stemming from their long-term implementation.

Sanctuary Policies and Crime: Theoretical Explanations

Current opposition to sanctuary cities, as illustrated by the Trump administration's executive orders, is partially justified on the claim that sanctuary ordinances foster crime. The existence or implementation of sanctuary policies could affect crime through several potential avenues. Sanctuary ordinances may cause behavioral or compositional changes among cities' immigrant populations or increase the relative size of the immigrant population (see Treyger,

5. Kittrie (2006) notes that other, though less publicized, justifications include the belief that local law enforcement officials do not have the resources, authority, or proper training necessary for the enforcement of immigration laws in cooperation with the federal government. Proper training was noted to be especially important to ensure that police officers would avoid racial profiling.
6. See http://www.ailadownloads.org/advo/NILC-LocalLawsResolutionsAndPoliciesLimitingImmEnforcement.pdf.

Chalfin, & Loeffler, 2014; for a similar discussion with regards to Secure Communities). According to the behavioral-change explanation, immigrants may perceive the risk of deportation to be lower in sanctuary jurisdictions and therefore be more inclined to engage in crime. Thus, when localities pass sanctuary ordinances they may effectively weaken a mechanism of deterrence. Alternatively, sanctuary city policies could lead to compositional shifts in the immigrant population by attracting migrants with higher underlying propensities to engage in crime. However, these logics likely overestimate immigrant criminality as well as the degree to which non-citizens understand immigration policy, including sanctuary ordinances. As Kittrie (2006) suggests, non-citizens may not understand the complexities of policy and "play it safe" despite the existence of sanctuary ordinances (p. 1482). Lastly, opponents might argue that sanctuary policies result in an overall increase in the stock of immigrants within a city, by attracting immigrants. There are two general explanations for why a rapid increase in the size of a city's immigrant population could hypothetically result in an increase in criminal behavior.

First, as sanctuary critics seem to imply, increased immigration, especially unauthorized immigration, may result in more crime if immigrants possess higher underlying levels of criminality than the general population. Simply put, if immigrants are more likely to engage in crime, then an increase in the population should result increased aggregate crime rates. However, a review of the literature finds that this line of reasoning is unequivocally false in its assumptions. Immigrants, at the individual-level, are less likely to engage in criminal and delinquent behavior when compared to native-born Americans (Bersani et al., 2014; Butcher & Piehl, 2007; Powell et al., 2010; Vaughn et al., 2014). Therefore, it is unlikely that an increase in the relative size of a city's immigrant population alone would result in additional crime due to differences in criminal involvement.

It is possible that macro-level increases in immigration, regardless of legality, is associated with aggregate crime trends independent of underlying levels of criminality in the immigrant population. The macro-level explanation for a positive immigration–crime correlation can be understood through the social disorganization framework (Stowell, 2007). Shaw and McKay (1942/1969) developed social disorganization theory within an urban context and viewed aggregate geographies as ecological units of analysis (Sampson, 2011; Stowell, 2007). Shaw and McKay (1942/1969) argued that community social network structures work to inhibit crime by exerting social control on members, for example, through adult supervision and socialization of youth (Sampson, 2011). Community characteristics such as low economic status, residential instability or population turnover, and ethnic heterogeneity disrupt social network ties within communities. The erosion of community ties undermines the ability of social institutions, especially the family, to regulate the behavior of community members, which results in social disorganization and an increase in crime (Sampson, 2011; Sampson & Groves, 1989). According to the social disorganization perspective, immigration into cities and neighborhoods is hypothesized to

indirectly impact neighborhood- or city-level violence by increasing economic disadvantage, population turnover, and ethnoracial diversity (insofar as immigrants, at least initially, enter at the bottom of the U.S. stratification system and are ethnoracial minorities in their host societies) (Stowell, 2007).

In contrast to expectations derived from the social disorganization framework, the majority of immigration—crime nexus studies at the macro-level find that immigration is either negatively or not at all related to crime. In fact, a recent meta-analysis of over 50 studies found an overall negative, yet weak, effect of immigration on macro-levels of crime when controlling for differences in study design (Ousey & Kubrin, 2017). Ultimately, the evidence on the relationships between immigrants, immigration, and crime predominately fails to support the expectations of social disorganization theory. In fact, mounting evidence supports an inverse social process described as the immigrant "revitalization thesis" (Ousey & Kubrin, 2017). Rather than contributing to community social disorganization, immigration and immigrants potentially re-stabilize disadvantaged communities, countering the criminogenic forces of urban decline (Martinez, 2006). As a result, communities that experience an influx of immigrants simultaneously undergo economic, social, and cultural transformations that increase social organization and mechanisms of social control (Martinez, 2006).

Sanctuary Policies and Crime: Existing Evidence

Given consistent findings of null or negative relationships between immigrants and criminality as well as immigration and crime, it is not surprising that the limited existing empirical evidence does not support the claim that sanctuary policies systematically foster violence in U.S. cities. Instead, a small body of research examining the sanctuary-crime nexus finds inverse or null results (Gonzalez et al., 2017; Lyons, Vélez, & Santoro, 2013; Wong, 2017). For example, Wong's (2017) recent cross-sectional analysis found that demographically matched large central metropolitan counties, micropolitan counties, and rural counties with sanctuary ordinances are safer than their non-sanctuary counterparts, while matched large fringe metropolitan counties, medium metropolitan counties, and small metropolitan counties have crime rates that are not statistically different across sanctuary and non-sanctuary jurisdictions. Similarly, Gonzalez et al. (2017), found that sanctuary cities do not experience any systematic or uniform shift in crime rates in the year following the implementation of a sanctuary policy. Gonzalez and colleagues also match sanctuary cities with non-sanctuaries cities along various demographic measures and find no statistically significant difference in violent crime and property crime rates overtime (between 2000 and 2014).

Lastly, Lyons et al. (2013) analyzed census-tracts nested in U.S. cities and found that immigrant concentration is associated with a reduction in neighborhood violence. Though the authors did not include a sanctuary city measure in

a baseline model, the authors found that the crime reducing effect of immigration concentration was stronger in sanctuary cities compared to non-sanctuary cities in a model with interaction terms. Lyons and colleagues suggest that the existence of sanctuary policies signifies that a city is receptive to the needs and of immigrant communities. That is, they claim that there is greater "immigrant political opportunities" in cities with sanctuary policies (2013, p. 605). As a result, there is greater communication and trust between key actors, institutions, and immigrant communities, which can foster an, or strengthen the, inverse relationship between immigrant concentration and crime, (Sandoval, 2009). Proponents of sanctuary policies, including city officials, draw on arguments similar to Lyons and colleagues' theoretical frame to support policy implementation.

The three highly informative studies on the sanctuary-crime link are not without limitations. For example, two of the three existing studies are limited to cross-sectional analyses (Lyons et al., 2013; Wong, 2017). Estimates derived from cross-sectional analyses may not appropriately reflect how jurisdictions with sanctuary ordinances change overtime or how the *adoption* of a sanctuary ordinance sets a city on a new crime trajectory, if at all. Gonzalez and colleagues' propensity-score based matching strategy and supplemental multivariate analysis of annual crime data between 2000 and 2014 provides stronger causal inference concerning the [null] longitudinal effect of sanctuary policies on crime. Despite an alternative analytic approach, the authors do not disaggregate violent crime (with the exception of rape) or property crime by specific types. It is possible that the sanctuary-crime relationship varies by specific forms of violence, and therefore an analysis of overall crime rates may mask heterogeneous effects. Furthermore, Gonzalez et al. (2017) utilize multivariate regression methods to analyze crime rates, which may distort the relationships between macro-level crime measures and covariates (Osgood, 2000).

We add to the emerging research on sanctuary jurisdictions and crime by posing two research questions: (1) *how, if at all, is the adoption of a city-level sanctuary ordinance associated with changes in city-level incidents of homicide and robbery?* (2) *What, if any, is the relationship between the concentration of the unauthorized Mexican immigrants and violent crime in U.S. cities*? We examine trends in the number of homicides and robberies, between 1990 and 2010, for U.S. cities with populations of at least 100,000. In addition, we rely on a multivariate regression approach that allows us to control for relevant time-varying characteristics while also accounting for time-invariant unobserved characteristics that may bias estimates of sanctuary policy effects. We also focus on city-level crime given general concerns over increasing city-level crime across the United States (Rosenfeld, 2016) and specific concerns regarding sanctuary cities and incidents of violence (Gonzalez et al., 2017). Lastly, as previously stated, we focus on the impact of unauthorized Mexican immigrants given the centrality of this group in contemporary discourse related to crime, immigration, and sanctuary policies.

Data, Analytic Sample, and Measurement of Variables

To address our research questions, we drew on a variety of official data sources. First, we obtained our dependent variables—annual counts of homicide and robbery incidents in each city—from the FBI's UCR. The UCR compiles official crime data from state, city, county, and campus law enforcement agencies throughout the country.

Our primary focal independent variable was whether a city passed a sanctuary policy during our study period from 1990 to 2010. Determining whether a city adopted a sanctuary policy is challenging for a number of reasons. To our knowledge, there exists no official record or list of sanctuary cities, nor is there an easily searchable database of municipal ordinances. However, several "non-partisan" sources track sanctuary jurisdictions.[7] Another challenge is that general conceptualizations of sanctuary jurisdictions or policies may differ with sources emphasizing different forms of such policies. As a result, sources may vary in the how sanctuary cities are identified.

With these concerns in mind, we obtained our primary focal independent variable by relying on a list of sanctuary jurisdictions provided by the NILC. We do so because this approach is consistent with prior studies, which makes our analyses more directly comparable (Gonzalez et al., 2017; Lyons et al., 2013). The list provided by NILC also includes a brief description of each jurisdiction's policy and provides a links to municipal documents, allowing us to verify the existence of the policies. A review of the list reveals that the policies are consistent with Kittrie's (2006) tripartite typology and the DOJ's definition of sanctuary jurisdictions. For the purposes of this article, we did not distinguish between the different types of policies and treated cities as either having a policy (in some form) in place or not. We do so because sanctuary policies are relatively rare, which limited our ability to discern any nuanced substantive effects between policy types.

In spite of its advantages, the NILC's sanctuary policy list is not without limitations. Most notably, the list was last updated in December of 2008, prior to the end of our study period. It is possible that a city adopted a sanctuary policy during our study period between 2009 and 2010. Therefore, we cross-referenced the cities in our sample with lists made available by "non-partisan" organizations, such as the Center for Immigration Studies (CIS) and the Ohio Jobs and Justice PAC (OJJPAC).[8] In the few instances where CIS or OJJPAC listed a city in our sample as a sanctuary but NILC did not, we consulted publically available municipal records. Most discrepancies were a result of NILC not including cities that passed policies after 2010, which is outside our study period. However, NILC failed to include four cities that passed sanctuary policies during our study period (Cleveland, OH, Jersey City, NJ, Alexandria, VA, and

7. See for example https://cis.org/Map-Sanctuary-Cities-Counties-and-States.
8. See http://ojjpac.org/sanctuary.asp.

Virginia Beach, VA). In each case, we were able to identify records to verify the passage of the city's policy. Thus, we used the list provided by NILC, but updated it to include the aforementioned cities.

Lastly, we obtained the remaining city-level socioeconomic and sociodemographic variables from the 1990 to 2000 5% Public Use Microdata Samples and 1-year sample data from the 2005 to 2010 ACS (Ruggles, Genadek, Goeken, Grover, & Sobek, 2015). We limited our analytic sample to cities with populations of at least 100,000 as well as to cities with complete information in the ACS and UCR across all eight years. Ultimately, our analytic sample consisted of 107 U.S. cities observed in 1990, 2000, and between 2005 and 2010, yielding 856 city-year observations.

Dependent Variables

Following the approach of Lyons et al. (2013), amongst many others, we captured city-level violence through two separate measures: homicide counts and robbery counts. Robbery and homicide in particular are more reliably reported to the police than rape, assault, or less serious forms of violence (Truman & Langton, 2015). Similar to previous research, we used three-year, rounded averages (e.g. average of 1989, 1990, and 1991 for 1990) to account for annual fluctuations in crime (Light, 2017; Ousey & Kubrin, 2014) and estimated separate models for each outcome.

Independent Variables

Sanctuary Policy was our primary focal independent variable and captured the adoption of limited cooperation policies at the city-level. For purposes of our analysis, we coded *Sanctuary Policy* as "1" if a city adopted a policy during or prior to year T. Therefore, in our regression models, *Sanctuary Policy* is a dichotomous measure.

It is important to note that the effect of adopting a sanctuary policy on crime may be delayed. Failing to account for the delays in a policy effect could bias contemporaneously measured effects towards zero. Therefore, we supplemented our analyses by also examining 1-year and 3-year lagged policy effects. If a city adopted a sanctuary policy in 2005, we would not observe it in the data (i.e. let *Sanctuary Policy* = 1) until 2006 with a 1-year lag and 2008 with a 3-year lag. We conducted separate analyses for *Sanctuary Policy* with and without the lags.

% Unauthorized MEX, our secondary focal independent variable, is the percent of a city's population that is likely unauthorized Mexican immigrants. We followed an exclusion procedure outlined in Hall and Stringfield (2014) to obtain city-level estimates of this measure based on ACS micro-data. We created dichotomous variables to flag an individual that is "likely undocumented"

by excluding adult immigrants that (1) were citizens, except those that are naturalized but have lived in the United States for fewer than four years, (2) migrated to the United States before 1990, (3) had above a high school education or are currently enrolled in school, and (4) were employed in local, state, and government work (Hall & Stringfield, 2014: 69). We then collapsed these dichotomous flags across cities to produce city-level estimates of the unauthorized Mexican population in each city. Finally, on the recommendation of Hall and Stringfield (2014) we adjusted this estimate by 15 percent to account for likely undercount. We also included an additional immigrant measurement, *% Immigrant*, to account for the size of each city's general immigrant population.

We estimated a *Diversity Index* to capture city-level ethnic heterogeneity. The diversity index was calculated using the following equation:

$$1 - \left(\sum_{1}^{m} (p_m^2) \right) \tag{1}$$

Equation 1 sums the squared proportion of each *m* ethnoracial group, and subtracts the number from 1. The city-level score signifies the likelihood that two randomly drawn individuals from the city would differ in their ethnoracial identification (Blau, 1977). Scores close to 0 suggest that a city is completely homogeneous, while scores close 1 mean that a city is heterogeneous. To account for residential instability we included a control, *% Moved,* to measure the percent of the population of the city that has moved households within the last 2 years. To measure a city's relative structural disadvantage, we constructed a *Disadvantage Index* by calculating averaged *z*-scores of four measures: the percent of the city population living below poverty, the percent of children in the city living in a single-parent household, the percent of adults that were unemployed, and the percent of the black population.[9] We also included continuous measures *% High Income Population* and *% w/post graduate degrees* to account for a city's degree of affluence, a factor that is conceptually distinct from city disadvantage (Light, 2017; Velez, Krivo, & Peterson, 2003).

Lastly, to control for each cities crime-prone population, we included a measure, *% Males 15–34,* to capture the proportion of the cities' populations that were between the ages of 15 and 34. We also controlled for the proportion of the population that worked in manufacturing industries to account for the macroeconomic context of each city (Light, 2017). Table 1 provides the descriptive statistics for the variables used in our analyses as well as the variables used to construct the diversity and disadvantage indexes.

9. Like Ousey and Kubrin (2014) we constructed an additional disadvantage index that excluded percent black. The alternative construction did not significantly alter the results of our analyses.

Table 1 Descriptive statistics for city-level variables 1990, 2000, and 2010

	1990		2000		2010	
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
Dependent variables						
Homicide	95.45	250.84	57.99	117.66	48.77	87.40
Robbery	3,330.24	10,639.64	1811.17	4,157.23	1,478.53	2,835.11
Independent variables						
Sanctuary policy[1]	.06	.23	.07	.26	.29	.46
% Unauthorized MEX	.52	1.14	2.07	2.57	2.40	2.79
% Immigrant	14.21	12.66	19.06	13.72	20.05	13.09
Diversity index[2]	.47	.14	.55	.12	.57	.11
% Asian	4.69	5.06	5.88	6.26	7.76	8.07
% Black	18.45	16.83	20.11	17.99	18.57	16.54
% Latino	15.05	17.19	21.37	19.71	23.14	19.86
% Other	.75	.76	3.29	1.70	3.02	1.76
% White	61.05	20.59	49.36	20.32	47.49	19.75
% Moved	9.13	2.24	8.72	2.13	9.34	2.58
Disadvantage index[3]	.00	.86	.00	.85	.00	.81
% Poverty	12.86	6.78	13.70	6.10	15.59	6.35
% Children in single-headed family	26.60	9.33	30.40	10.19	25.85	10.15
% Unemployed adults	2.94	.90	2.72	.80	5.13	1.21
% Black	(above)	(above)	(above)	(above)	(above)	(above)
% High income	25.48	10.92	24.58	10.50	24.34	11.45
% w/Post graduate degrees	5.04	2.89	5.95	3.65	8.39	4.79
% Manufacturing	8.87	3.97	6.73	2.98	5.75	2.38
% Males 15–34	17.04	2.15	15.45	2.10	14.20	2.01
Population (in 1,000s)	407.16	824.65	446.84	896.79	463.52	915.01
N	95.45	250.84	57.99	117.66	48.77	87.40

[1]Measured as dichotomous (0, 1); [2]Continuous from 0 to 1; [3]Average *z*-score of 4 measures.

Analytic Approach

Our research questions are fundamentally concerned with social processes that unfold within cities and over time (Ousey & Kubrin, 2009). Therefore, to properly model changes in city counts of homicide and robbery, we employed unconditional fixed-effects negative binomial regression models (Allison & Waterman, 2002). Negative binomial and Poisson models, more generally, are

well suited to examine discrete and rare events in instances where ordinary least-squares regression analyses of logged crime rates may be biased (Osgood, 2000). Negative binomial models are preferred over Poisson models because we identified the presence of overdispersion. The unconditional negative binomial regression model estimates the equation:

$$\log \lambda_{it} = \boldsymbol{u}_t + \beta \boldsymbol{x}_{it} + \gamma \boldsymbol{z}_i + \alpha_i \quad (2)$$

(Allison, 2005), where $\log \lambda_{it}$ is the log-linear decomposition function of city homicide or robbery counts, u_t is the intercept, x_{it} is a vector of time-varying covariates, z_i is the vector of time-invariant covariates, and α_i is the unobserved city fixed-effects, which are accounted for by including city dummy variables when estimating the models. We also included time variables to account for linear and quadratic time trends in violence during our study period. Because we estimated an unconditional fixed-effect model, our analysis considers how *changes* in structural characteristics are associated with *changes* in violent crime within cities and over time. In addition, a primary strength of our approach is its ability to account for time-invariant covariates (e.g. z_i), including those that are unobserved and may contribute to omitted-variable bias. For example, time invariant city characteristics may differentially predispose one city to adopt a sanctuary ordinance relative to another while also influencing robberies or homicides, but this is accounted for in our analysis. Logged population serves as our exposure variable in each model to account for the differences in population sizes across each city.

Results

Mean Comparisons

In Table 2, we present adjusted mean differences in homicide and robbery counts by comparing sanctuary cities and non-sanctuary cities for three sample

Table 2 Population adjusted means and differences in number of crimes

	1990		2000		2010	
	Homicides	Robberies	Homicides	Robberies	Homicides	Robberies
Sanctuary cities	139.31	3,050.79	127.39	2,878.38	76.62	2,191.76
Non-sanctuary cities	92.85	3,346.86	52.38	1,724.93	37.38	1,187.63
Difference	46.47	−296.07	75.01***	1,153.45**	39.24***	1,004.13***
t-Statistic	1.81	*−.33*	3.34	2.74	3.53	4.09
N	107		107		107	

*$p < .05$; **$p < .01$; ***$p < .001$.

years (1990, 2000, and 2010). The results suggest that the differences between sanctuaries and non-sanctuaries shifted overtime. Specifically, there are no statistically significant differences between the two types of cities in 1990, although this finding is may be due to a lack of statistical power, as only six of the cities in our sample had sanctuary policies before 1990. However, significant differences emerge in 2000. After adjusting for population differences, sanctuary jurisdictions have, on average, about 75 more homicides and 1,150 more robberies. By 2010, the differences between jurisdictions converge slightly such that sanctuaries have about 40 more homicides and about 1,000 more robberies. Nevertheless, and like Wong's (2017) comprehensive comparison, Table 2 only illustrates the average difference in crime counts between sanctuaries and non-sanctuaries at these three single points in time. As such, one cannot conclude from these results alone that sanctuary cities are less safe than non-sanctuary cities *as a result of the sanctuary policies*. We simply present Table 2 to serve as a point of comparison to our multivariate approach, as doing so illustrates the biased nature of simple mean comparisons between sanctuary and non-sanctuary jurisdictions.

Multivariate Results: Homicide Incidents

We present the results of our negative binomial regression models predicting homicide counts in Table 3. Model 1 of Table 3 includes all independent variables, including linear and quadratic time trend controls, as well as city fixed-effects (coefficients of fixed-effect dummy variables not presented). Model 1 does not include any lagged effects for the focal independent variable *Sanctuary Policy*. The coefficient estimates in Table 3 can be interpreted as the difference in expected counts (logged) for a given variable at values $x + 1$ and x. For example, the *Sanctuary Policy* coefficient of −.004 signifies that, net of all other factors included in the model, log(expected homicides, *Sanctuary Policy* = 1) − log(expected homicides, *Sanctuary Policy* = 0) = −.004. We included population as an exposure variable to standardize violent crime counts and to allow for a meaningfully discussion of the predicted effect estimates as rates. Additionally, incidence rate ratios can be calculated by exponentiating the coefficients reported in Table 3. Incidence rate ratios can be interpreted as the ratios of expected rates for a given variable at values $x + 1$ and x.

At beginning of this article, we asked how, if at all, the adoption of a city-level sanctuary ordinance is associated with changes in city-level homicide and robbery counts. The effect estimates presented in Model 1 (see Table 3) are consistent with expectations based on a review of prior research. First, in contrast to simple mean differences (as depicted in Table 2), our longitudinal multivariate analysis finds no evidence that the adoption of a sanctuary ordinance is associated with shifts in homicide counts in any meaningful way. That is, the coefficient for *Sanctuary Policy* is not (statistically) significantly different from zero. Moreover, neither of the coefficients of our immigration related

Table 3 Homicide: negative binomial regression with city fixed effects (coefficients and standard errors)

	Model 1, No Lag[1]	Model 2, 1-Year Lag[1]	Model 3, 3-Year Lag[1]
Sanctuary policy[2]	−.004	−.047	−.058
	(.038)	(.037)	(.036)
% Unauthorized MEX	−.016	−.017	−.016
	(.012)	(.012)	(.012)
% Immigrant	.001	.001	.001
	(.006)	(.006)	(.006)
Diversity index[3]	.686	.625	.615
	(.353)	(.352)	(.354)
% Moved	.057***	.059***	.058***
	(.010)	(.010)	(.010)
Disadvantage index[4]	.170***	.172***	.170***
	(.049)	(.049)	(.049)
% High income	.015***	.015***	.015***
	(.004)	(.004)	(.004)
% w/Post graduate degrees	−.056***	−.056***	−.055***
	(.011)	(.011)	(.011)
% Manufacturing	.009	.010	.010
	(.011)	(.011)	(.011)
% Males 15−34	.000	.001	.004
	(.009)	(.009)	(.010)
Time trend (linear)	−.045***	−.045***	−.044***
	(.008)	(.008)	(.008)
Time trend (quadratic)	.001***	.001***	.001***
	(.000)	(.000)	(.000)
Constant	−9.886***	−9.919***	−9.947***
	(.253)	(.252)	(.254)
City FE	Yes	Yes	Yes
Population (logged)	1 (exposure)	1 (exposure)	1 (exposure)
$N \times T$ (Observations)	856	856	856

Note. Standard errors in parentheses
[1]Lag refers to Sanctuary Policy only; [2]Measured as dichotomous (0, 1); [3]Continuous from 0 to 1; [4]Average *z*-score of 4 measures; *$p < .05$; **$p < .01$; ***$p < .001$.

measures, *% Unauthorized MEX* and *% Immigrant*, are significantly different from zero. This suggests that, at least in our sample, city-level immigrant concentration is not associated with homicide incidents. Instead, we find broad support for two of the three conventional measures consistent with the social disorganization framework. First, *% Moved*, which captures residential instability, is positively associated with homicide such that each one unit change (or a

one percentage point increase the size of the population that moved households within the last two years) is associated with approximately a six percent increase in the homicide rate ($p < .001$). Similarly, a one-unit increase in the disadvantage index is associated with about a 19 percent increase in homicide ($p < .001$). We do not find any evidence that ethnic heterogeneity (*Diversity Index*) is associated with changes in homicide.

Given the surrounding discourse and limited evidence (Lyons et al., 2013) in support of sanctuary ordinances as a means for crime reduction, we separately interact *Sanctuary Policy* with *% Unauthorized MEX* (Model 4a) and *% Immigrant* (Model 7a) to estimate potential moderation (see the top panel of Table 4). Supporters of limited cooperation policies contend that immigrants in sanctuary jurisdictions are more willing to report crime victimization and cooperate with police investigations. Consequently, sanctuary cities with larger immigrant populations may experience a greater revitalization of informal and formal social control mechanisms relative to non-sanctuary jurisdictions. If this assertion holds, we would expect crime to decline in tandem with immigrant concentration, but only in sanctuary cities. Consistent with expectations, we find evidence that sanctuary policies moderate the effect unauthorized Mexican concentration and the effect of overall immigrant concentration on homicide. While both immigrant concentration measures are not associated with robbery in non-sanctuary cities, there is a negative effect of each measure in cities with sanctuary ordinances (both $p < .01$). Specifically, each one percentage point increase in a sanctuary city's immigrant population is associated with approximately a one half of one percent decline homicide ($\exp[.004 + (-.010)] - 1 = -.006$). We should note that the estimated effect of *Sanctuary Policy* in Model 7a is positive and significant. However, this coefficient should not be interpreted independently, as it represents the effect of sanctuary policy adoption in a hypothetical city with an immigrant population of zero. In our sample data, this hypothetical city does not exist. Nevertheless, if a sanctuary city with no immigrants were to experience an influx of immigrants we would expect homicides to decline. Ultimately, these findings are inconsistent with the current narratives that characterize sanctuary jurisdictions public safety threats insofar as they attract and protect criminal immigrants.

In the case of unauthorized Mexican immigrants, we find a more substantial negative effect. Each one percentage point increase in the unauthorized Mexican immigrant population is associated with approximately a five percent decline homicide ($\exp[-.003 + (-.046)] - 1 = -.049$), but again, only in sanctuary cities. In this model, the crime reducing effects of unauthorized Mexican immigrant concentration are not in any way offset by a positive and significant estimate of the coefficient for *Sanctuary Policy*. Overall, we find modest evidence to support the position held by sanctuary policy proponents that there is long-term benefit to establishing policies that integrate immigrant communities, though especially for unauthorized immigrant communities, and limit the involvement of local law enforcement in policing immigration laws.

Table 4 Homicide & robbery: negative binomial regression with city fixed effects and interactions (coefficients and standard errors)

	Model 4a, No Lag[1]	Model 5a, 1-Year Lag[1]	Model 6a, 3-Year Lag[1]	Model 7a, No Lag[1]	Model 8a, 1-Year Lag[1]	Model 9a, 3-Year Lag[1]
Homicide						
Sanctuary policy[2]	.068	.018	.035	.182**	.150*	.136*
	(.045)	(.043)	(.044)	(.063)	(.061)	(.058)
% Unauthorzied MEX	−.003	−.005	−.000	−.019	−.018	−.019
	(.013)	(.013)	(.013)	(.012)	(.012)	(.012)
% Immigrant	−.001	.000	−.000	.004	.004	.004
	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)
Sanctuary × % Unauthorized *interaction*	−.046**	−.044**	−.062***			
	(.016)	(.016)	(.017)			
Sanctuary × % Immigrant *interaction*				−.010***	−.010***	−.011***
				(.003)	(.003)	(.003)
	Model 4b, No Lag[1]	Model 5b, 1-Year Lag[1]	Model 6b, 3-Year Lag[1]	Model 7b, No Lag[1]	Model 8b, 1-Year Lag[1]	Model 9b, 3-Year Lag[1]
Robbery						
Sanctuary policy[2]	−.132***	−.110***	−.063	.056	.074	.079
	(.034)	(.034)	(.035)	(.051)	(.050)	(.049)
% Unauthorzied MEX	.011	.012	.015	.009	.010	.011
	(.008)	(.008)	(.008)	(.008)	(.008)	(.008)
% Immigrant	.003	.003	.002	.005	.005	.004
	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)
Sanctuary × % Unauthorized *interaction*	.007	−.001	−.020			
	(.013)	(.013)	(.014)			
Sanctuary × % Immigrant *interaction*				−.009***	−.010***	−.010***
				(.002)	(.002)	(.002)
$N \times T$ (Observations)	856	856	856	856	856	856

Notes. Standard errors in parentheses. Includes controls from Table 4 and 5.
[1]Lag refers to Sanctuary Policy only; [2]Measured as dichotomous (0, 1); *$p < .05$; **$p < .01$; ***$p < .001$.

Tables 3 and 4 also include results from our models that examine lagged effects of sanctuary policy adoption. Model 2 in Table 3 reports the results of lagging the effect of *Sanctuary Policy* by one year and Model 3 reports the results of lagging the effect of *Sanctuary Policy* by three years. As with Model 1 in Table 3, we find no evidence that the adoption of a sanctuary policy is associated with changes in homicide with one or three year lags. Similarly, the remaining models presented in Table 4 do not provide any new or novel findings relative to what we have already discussed.

Multivariate Results: Robbery

We present results of our negative binomial regression models predicting robbery in Table 5. Model 10 in Table 5 includes all independent variables, including linear and quadratic time trend controls, as well as city fixed-effects (coefficients of fixed-effect dummy variables not presented). Model 10, does not include lagged effects for the focal independent variable *Sanctuary policy*. The coefficient estimates in Table 5 can be interpreted in the same manner as those in Table 3.

The results from the robbery models are largely consistent with those from the homicide models with one major exception. Most important to the central aim of this article, we find that the adoption of a sanctuary policy is associated with an 11 percent decline in robberies, net of immigration measures and criminogenic structural factors. In contrast, we do not find any support for the assertion that immigration concentration is associated with city-level robbery counts (Model 10).

We also find wide support for the theory of social disorganization in the case of robbery. Unlike Model 1 of Table 3, we evidence that the ethnic heterogeneity measure (*Diversity Index)* is positively correlated with robbery. As in Model 1 of Table 3, we find that changes in residential instability and the disadvantage index are positively associated with changes in robberies. These findings suggest that cities experiencing increases in ethnic heterogeneity, residential instability, and disadvantage similarly experience an increase in robbery.

As with homicide, we interact *Sanctuary Policy* with *% Unauthorized MEX* (Model 4b) and *% Immigrant* separately (Model 7b) to estimate possible moderation (see Table 4). Just as with the model for homicide, we find that existence of a sanctuary policy moderates the effect of city-level immigrant concentration on robbery. While the immigrant concentration does not appear to be associated with robbery in non-sanctuary cities, there is a negative effect of immigrant concentration in cities with sanctuary ordinances ($p < .001$). Specifically, each one percentage point increase in a sanctuary city's immigrant population is associated with approximately a one half of one percent decline robbery ($\exp[.004 + (-.009)] - 1 = -.005$). These results provide further, yet substantively weak, evidence to support the implementation

Table 5 Robbery: negative binomial regression with city fixed effects (coefficients and standard errors)

	Model 10, No Lag[1]	Model 11, 1-Year Lag[1]	Model 12, 3-Year Lag[1]
Sanctuary policy[2]	−.122***	−.112***	−.090**
	(.029)	(.029)	(.029)
% Unauthorized MEX	.012	.012	.013
	(.008)	(.008)	(.008)
% Immigrant	.003	.003	.002
	(.004)	(.004)	(.004)
Diversity Index[3]	1.169***	1.177***	1.225***
	(.210)	(.210)	(.210)
% Moved	.020***	.020**	.018**
	(.007)	(.007)	(.007)
Disadvantage Index[4]	.233***	.232***	.226***
	(.033)	(.033)	(.033)
% High income	.015***	.014***	.014***
	(.002)	(.002)	(.002)
% w/Post graduate degrees	−.023***	−.023***	−.023***
	(.007)	(.007)	(.007)
% Manufacturing	.024***	.024***	.023***
	(.007)	(.007)	(.007)
% Males 15—34	−.006	−.006	−.004
	(.006)	(.006)	(.006)
Time trend (linear)	−.063***	−.063***	−.064***
	(.005)	(.005)	(.005)
Time trend (quadratic)	.002***	.002***	.002***
	(.00)	(.00)	(.00)
Constant	−6.401***	−6.392***	−6.397***
	(4.949)	(4.965)	(4.885)
City FE	Yes	Yes	Yes
Population (logged)	1 (exposure)	1 (exposure)	1 (exposure)
$N \times T$ (Observations)	856	856	856

Note. Standard errors in parentheses.
[1]Lag refers to Sanctuary Policy only; [2]Measured as dichotomous (0, 1); [3]Continuous from 0 to 1; [4]Average z-score of 4 measures; *p < .05; **p < .01; ***p < .001.

of sanctuary policies in cities with high concentrations of immigrants as a means to combat crime.

Again, Tables 4 and 5 also include results from our models that examine lagged effects of sanctuary policy adoption. Model 11 in Table 3 reports the results of lagging the effect of *Sanctuary Policy* by one year and Model 12

reports the results of lagging the effect of *Sanctuary Policy* by three years. As with Model 10 in Table 5, we find that the adoption of a sanctuary policy is associated with a decline in robbery ($p < .001$). Again, the remaining interactive models presented in Table 4 do not provide any additional insights beyond what we previously discussed.

Predicted Number of Homicides and Robberies

To better illustrate the relative substantive effect of each variable with a statistically significant effect (in Models 1 and 10), we present average predicted homicide and robbery counts across all 107 cities in Table 6. To do so, we first calculated the overall average predicted number of homicides or robberies with each variable of interest set at its mean and all other variables set at their observed values. We then calculated the overall average predicted number of homicides or robberies with each variable of interest set at one standard deviation above its mean and all other variables set at their observed values. We present the ratio of the two estimates in the final column of Table 6. The results reveal that a one standard deviation increase in the *% Moved* and city disadvantage are both associated with a 15 percent increase in homicide, With regards to robbery, we find that a one standard deviation increase in the diversity index is associated with a 16 percent increase in homicide, whereas a one standard deviation increase in the disadvantage index is

Table 6 Predicted average number of crimes across cities, statistically significant effects only

	At variable mean[1]	1 Std deviation above mean[1]	Ratio of predicted counts
Homicide[2]			
% Moved	60.98	70.08	1.15
Disadvantage index	55.23	63.78	1.15
% High income	61.71	73.11	1.18
% Postgraduate degrees	65.78	52.21	.79
Robbery[3]			
Diversity index	1,697.27	1,973.69	1.16
% Moved	1,859.51	1,951.37	1.05
Disadvantage index	1,707.52	2,079.57	1.22
% High income	1,870.39	2,206.62	1.18
% Postgraduate degrees	1914.86	1,740.21	.91
% Manufacturing	1928.26	2,088.57	1.08

[1]All other characteristics at observed values; [2]Estimates derived from Table 3, Model 1; [3]Estimates derived from Table 5, Model 10.

associated with a 22 percent increase in robbery. Residential instability appears to be less strongly related to robbery than homicide, as a one standard deviation increase in *% Moved* is only associated with a 5 percent increase in robbery. As a reminder, the passage of a sanctuary policy was associated with about an 11 percent decrease in robbery. Interestingly, we find that *% High income*, a measure of city affluence, is positively associated with both homicide and robbery such that a one standard deviation increase in the measure is associated with an 18 percent increases in both measures. Though counterintuitive, the positive effect we estimate is consistent with prior research[10] (Light, 2017). Regardless, Table 6 reveals the importance of addressing city-level structural factors identified by social disorganization theory, such as concentrated disadvantage in particular, as a means of substantially reducing incidents of homicide and robbery.

Discussion and Conclusion

A recent report from the DOJ reveals that average city-level homicide increased between 2014 and 2015 in a sample of 56 large U.S. cities (Rosenfeld, 2016). Furthermore, 40 of the 56 cities examined specifically experienced increases in homicide between the two years. According to the author, the general uptick in homicide (both in the sample and nationally) represents a departure from an approximately decade-long decline, though the current national homicide rate remains well below the rates during the 1980s and 1990s. Rosenfeld (2016) provides a few possible explanations for the recent reversal in homicide decline, including changes in drug markets, imprisonment, and a so-called "Ferguson Effect."

Notably absent from the Rosenfeld's assessment and explanation of an uptick in crime is any discussion of immigrants, immigration, and sanctuary jurisdictions or policies. Despite the lack of attention paid to these factors in the DOJ's report, early actions taken by President Donald Trump, namely Executive Order 13768, illustrate the concerns over cities' decades-old approaches of symbolically offering "sanctuary" to facilitate the integration of immigrant communities. Trump's executive order claims that sanctuary jurisdictions, defined as localities that violate federal statute 8 U.S.C. 1373, cause "immeasurable harm to the American people and to the very fabric of our republic." Furthermore, the executive order explicitly states that unauthorized immigrants represent a threat to public safety. As evidence of the systematic harm caused by immigrants and fostered by sanctuary jurisdictions, the president

10. These results may be expected by routine activities theory (Cohen & Felson, 1979). When a larger proportion of the population is "high income," there is a greater (relative) size of suitable robbery targets at the city-level. Furthermore, robberies (or other crimes like burglaries) can "go wrong" leading to robbery-turned-homicide. This could explain a relationship with both measures, but a systematic investigation of this is beyond the scope of the current paper.

and others often draw on tragic, yet anecdotal, instances in which deportable noncitizens engaged in horrible acts of violence (Yee, 2017). However, we contend that the judgments rendered against sanctuary jurisdictions and unauthorized immigrants are worth empirical investigation, which only a handful of studies have accomplished (Gonzalez et al., 2017; Lyons et al., 2013; Wong, 2017).

Our analyses contribute to the small but growing body of research on the sanctuary-crime nexus and find no evidence to support the assertion that the adoption of sanctuary policies fosters city-level violence in a systematic way. In fact, contrary to the Trump administration's view, we provide additional evidence that sanctuary policies may directly and indirectly, though only weakly, contribute to a reduction in crime. For example, we find that cities experience a decline in robbery following the passage of a sanctuary policy and that the existence of a sanctuary policy moderates the impact immigrant concentration in an interactive model. In other words, changes in the concentration of immigrants, in general, and unauthorized Mexicans, in particular, appear to be associated with decreases in homicide and robbery, but only in sanctuary cities. This finding is consistent with the expectations drawn from the justifications for sanctuary ordinances. Though not directly examined in our analyses, it is possible that sanctuary policies increase immigrants' willingness to report crime victimization or cooperate with police regardless of their legal status, which can facilitate long-term reductions in city violence. Alternatively, and more consistent with findings from Lyons et al. (2013), sanctuary ordinances may signal that local social institutions are receptive to the needs of immigrant communities and better facilitate social integration or formal and informal social control. Though the direct mechanism through which sanctuary policies moderate the impact of immigration concentration is beyond the scope of our study, it is worthy of future investigation.

If the adoption of sanctuary ordinances and increases in immigrant concentration do not increase violent crime, then what factors explain positive shifts in violence in our sample of U.S. cities? Overall, we find general support for the traditional social disorganization framework. City-level concentrated disadvantage is particularly a major criminogenic force driving changes in both homicide and robbery. Additionally, ethnic heterogeneity, measured through our diversity index, is also associated with robbery, though more modestly. We must note that the social disorganization framework does not view ethnic heterogeneity as a criminogenic force because any specific group has higher underlying criminality. Instead, ethnic heterogeneity works to inhibit the development of strong social bonds within communities,[11] which generally regulate crime and delinquency through informal social controls. Therefore, a humanitarian-oriented approach to address the criminogenic effect of diversity at the city or neighborhood-level could work to bring community members

11. Possibly because of language barriers or homophilic preferences.

together and emphasize commonly-held goals and interpersonal similarities to increase social bonds between ethnoracial groups. More generally, policies derived from the social disorganization literature should focus on addressing the macro-level distal causes (e.g. poverty/disadvantage) as well as the more proximate and intervening forces of social network structure or collective efficacy formation (Sampson, 2011). A two-pronged approach is especially important given that factors such as economic disadvantage undermine the development of social ties and collective efficacy, yet social ties and collective efficacy do not necessarily develop in the absence of disadvantage, residential mobility, or ethnic heterogeneity (Sampson, 2011).

As with all research, our study is not without limitations. First, our sample construction is limited by the data available UCR and the ACS, so we were unable to include some sanctuary cities and many non-sanctuaries in our analyses. However, we are confident in our results given their general consistency with previous research that relied on different samples of U.S. cities and distinct analytic approaches (Gonzalez et al., 2017; Lyons et al., 2013). Second, we focus on two reliable measures of violence at the expense of other forms of violence and various property crimes. Nevertheless, we would not expect different results if we were to examine other serious forms of violence (e.g. aggravated assault or rape). Rape specifically was examined previously and does not appear to be related to cities' adoption of sanctuary ordinances (Gonzalez et al., 2017). We are hesitant to examine minor forms of violence (e.g. simple assault) or disaggregated forms of property crime given that they may be more susceptible to biases in the UCR (e.g. underreporting). Future research should extend the current study by exploring patterns in other forms of crime with more reliable sources of data (e.g. the National Crime Victimization Study's MSA data-set). In a similar vein, our study is limited to examining general incidents of city violence as opposed to ethnoracial-specific measures. Our general approach to understanding homicide and robbery neglects the ethnoracial invariance in the determinants of city-level victimization (Peterson & Krivo, 2005). The racial invariance perspective is important to consider given our study's focus on immigrants in general as well as the centrality of immigrant Latinos (e.g. unauthorized Mexicans) in broader discussions of sanctuaries and crime (Martinez et al., 2010). Finally, while an unconditional fixed-effect negative binomial model can account for time-invariant city-factors that may be related to both the adoption of a sanctuary ordinance and crime, this approach cannot account for unobserved time-*varying* factors. As such, it is possible that our estimates of sanctuary policy effects are biased in some way. Nonetheless, we attempted to address this limitation by accounting for theoretically relevant variables based on past research and discussions surrounding sanctuaries, immigration, and crime.

Despite the limitations of our study, as well as those stemming from prior research, findings from the burgeoning literature on the sanctuary-crime nexus hold policy implications. We contend that policymakers concerned with reducing violence in U.S. cities should focus on improving the structural

characteristics of cities (e.g. concentrated disadvantage) that facilitate and perpetuate violence, while also addressing neighborhood and micro-level correlates of crime and criminality, rather that vilifying sanctuary jurisdictions and immigrants, factors that we, and researchers before us, have demonstrated are either not at all or inversely related to crime.

Acknowledgments

We would like to thank Christina Diaz, Jeremy Fiel, Karam Hwang, David Rigby, Charles Seguin, and two anonymous reviewers for their suggestions and comments on earlier versions of this research.

Disclosure Statement

No potential conflict of interest was reported by the authors.

Supplemental Data

Supplemental data for this article can be accessed https://doi.org/10.1080/07418825.2017.1400577.

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