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ORIGINAL PAPER

Immigration and the Changing Nature of Homicide in US Cities, 1980–2010

Graham C. Ousey · Charis E. Kubrin

Abstract

Objectives Previous research has neglected to consider whether trends in immigration are related to changes in the *nature* of homicide. This is important because there is considerable variability in the temporal trends of homicide subtypes disaggregated by circumstance. In the current study, we address this issue by investigating whether within-city changes in immigration are related to temporal variations in rates of overall and circumstance-specific homicide for a sample of large US cities during the period between 1980 and 2010.

Methods Fixed-effects negative binomial and two-stage least squares (2SLS) instrumental variable regression models are used to analyze data from 156 large US cities observed during the 1980–2010 period.

Results Findings from the analyses suggest that temporal change in overall homicide and drug homicide rates are significantly related to changes in immigration. Specifically, increases in immigration are associated with declining rates for each of the preceding outcome measures. Moreover, for several of the homicide types, findings suggest that the effects of changes in immigration vary across places, with the largest negative associations appearing in cities that had relatively high initial (i.e., 1970) immigration levels.

Conclusions There is support for the thesis that changes in immigration in recent decades are related to changes in rates of lethal violence. However, it appears that the relationship is contingent and varied, not general.

Keywords Homicide · Immigration · Violent crime trends · Fixed-effects models

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Introduction

In April 2007, the National Research Council of the National Academies convened a distinguished panel of scholars with expertise in sociology, criminology, law, economics, and statistics to address issues underlying the study of crime trends in the United States. Participants identified key substantive and methodological issues related to studying crime trends and discussed ways to improve understanding of both shorter- and longer-term changes in crime. Findings from their studies were published in the volume, *Understanding Crime Trends* (Goldberger and Rosenfeld 2008). While the volume covers a diverse assortment of issues, notable among the omitted issues is the "impact of immigration," which according to the volume editors "requires much more extensive evaluation" (Rosenfeld and Goldberger 2008:3).

Since the publication of the NRC volume, scholarship on the effects of immigration on crime trends has become more common. Indeed, several recent longitudinal studies have investigated the relationship between immigration and violence across US metropolitan areas, cities, and neighborhoods (MacDonald et al. 2013; Martinez et al. 2010; Ousey and Kubrin 2009; Stowell et al. 2009; Wadsworth 2010). Each of these studies provides some evidence in support of the thesis that increased immigration is associated with decreased rates of violent crime.

Yet there is still much to be learned about the impact of changes in immigration on trends in violence, particularly homicide, in the US. In particular, previous research has neglected to consider whether trends in immigration are related to changes in the *nature* of homicide. This is an important limitation because while overall homicide rates have been trending steadily downward in recent decades, there is considerable variability in temporal trends of homicide rates disaggregated into subtypes based upon circumstances or motives. For instance, the rate of argument homicide, traditionally the most common subtype, has fallen steadily since at least as far back as 1980. In contrast, drug- and gang-related killings have exhibited periods of both upward and downward growth in recent decades. To the extent that these homicide subtypes reflect the "nature" of this form of lethal violence, there have been noteworthy changes that warrant explanation.

Previous research suggests there are differences in the effects of some covariates on homicide rates disaggregated by circumstance or motive (Block 1993; Block and Block 1992; Cornell et al. 1989; Goldstein et al. 1992; Harries 1993; Kubrin and Ousey 2009; Mears and Bhati 2006). Included in this literature are a handful of studies that have identified differences in the effects of immigration on circumstance-specific homicide subtypes (Kubrin and Ousey 2009; Nielsen et al. 2005). Unfortunately, the evidence in these studies is drawn entirely from cross-sectional research. Consequently, it does not advance our understanding of recent trends in homicide subtypes or of the connection between such trends and changes in immigration. To accomplish that objective, longitudinal research that specifically investigates the pattern and predictors of over time changes in circumstance-specific homicide rates is needed.

In the current study, we address this need by investigating the temporal variation in homicide rates for a sample of large US cities. Our analyses build on the current research literature on homicide trends in several ways. First, while prior studies have focused primarily on trends in overall homicide rates, we extend that work by also examining time trends in several *circumstance-specific homicide rates* (e.g., argument, felony, drug-related and gang-related). Second, we address limitations of the small but growing literature on the longitudinal relationship between immigration and violence. Prior studies have examined this relationship across multiple decades within a single city (Martinez et al. 2010) or have observed changes across multiple places (e.g., cities or metro areas) within a single decade (Stowell et al. 2009; Wadsworth 2010). We extend this work by examining the longitudinal association between immigration and homicide in more than 150 large cities over a 30-year period spanning 1980–2010. We also advance previous research by investigating the possibility that the longitudinal immigration-homicide relationship may vary across city contexts. To wit, we draw lessons from a literature on "context of reception" (Portes and Rumbaut 2006) and consider whether the effect of changes in immigration on homicide—both overall rates and circumstance-disaggregated rates—varies across cities as a function of differences in their preexisting levels of immigrant concentration. Finally, following recent work by MacDonald et al. (2013), we consider the possibility that immigration may be endogenous to homicide rates and we implement instrumental variables methods that address their simultaneity.

Our study draws from and contributes to several bodies of criminological research including trends in violence, homicide disaggregation, and the immigration-violence nexus. For this reason, we begin the following section with a brief discussion of relevant research themes from each of these areas. We follow that with information on the sample, data sources, measures, and analytic methods utilized in our study. Next we present and discuss the results of our statistical analyses. We conclude with a section that highlights the implications of our results for subsequent theoretical and empirical work in the aforementioned areas of inquiry.

Conceptual Framework

Trends in Violence

Crime rates in the US have exhibited marked fluctuations over the past 30–40 years. In particular, rates of homicide and robbery peaked in the 1980s and early 1990s, before dropping by over 40 % through the end of the century (Blumstein and Rosenfeld 2008). After declining or remaining stable for over a decade, evidence suggests that rates of violence may be on the rise again in some large cities.

Attempts to understand these trends have generated renewed interest in dynamic and longitudinal models of crime and violence (Greenberg 2010). This is evident not only in the focus of the National Academies panel described earlier but also in the veritable explosion of research on the crime drop in the US. For at least the last decade, criminologists have worked diligently to identify the factors associated with the decline. Explanations abound, ranging from the strong economy of the 1990s, to changing demographics such as the aging of the baby boomers, to better policing strategies, to stricter gun control laws, to record levels of imprisonment, to receding crack-cocaine markets, and even to increased immigration—the focus of the current study (Blumstein and Wallman 2006; LaFree 1999; Levitt 2004; Rosenfeld 2002; Wadsworth 2010; Zimring 2007).

In the context of the current study, several important conclusions may be derived from recent research on crime trends. First, as Zimring (2007:195) suggests, there is not one single cause that produced the declines in crime rates; instead, it is likely a combination of factors working in concert. Second, Blumstein and Rosenfeld (1998, 2008) and Blumstein et al. (2000) argue that trends in violence, including the dramatic rise in homicide in the latter half of the 1980s and the subsequent drop in the 1990s, are best understood by looking beyond aggregated rates and examining trends in age-, sex- and race-specific rates. For example, they show the increase in homicide during the late 1980s was due to escalating violence among juveniles and young adults, predominantly (although not exclusively) by and against black males, particularly in larger cities, and exclusively involving handguns (Blumstein and Rosenfeld 1998:1207). They also show that the recent decline in violence applies primarily to certain populations: "… the departure of young people from the crack markets combined with the continuing decline of violence by the over-30 population were major factors contributing to the steady decline in violent crime from about 1993 until 2000" (Blumstein and Rosenfeld 2008:17). Finally, crime trend studies highlight the fact that national-level trends often obscure important variations in trends at more localized levels (Blumstein and Rosenfeld 2008:28). Stated alternatively, the crime decline was not equally experienced across metropolitan areas, cities, neighborhoods, and even street segments—a point that is also true for crime booms and busts more generally. Indeed, several studies show there is heterogeneity in crime trends across cities, neighborhoods, and street segments (Kikuchi and Desmond 2010; McCall, Land, and Parker 2010; Messner et al. 2005; Weisburd et al. 2004).

As this brief review of the literature reveals, significant progress has been made in terms of documenting recent trends in aggregate crime rates as well as identifying important variations in trends that appear when crime data are disaggregated. Progress also has been made in accounting for variation in trends across multiple levels of analysis, with several macro-social factors found to be associated with these crime trends. However, we identify two important questions that remain unanswered in the crime trends literature. The first is related to whether the nature or composition of violence has changed over time: does one pattern of temporal change characterize all homicide motive/circumstance subtypes or does the direction and rate of growth differ for some types relative to others? The second question addresses the extent to which immigration matters for understanding trends in different homicide subtypes: are changes in circumstance/motive-specific homicide rates over recent decades related to changes in immigration? At present, we are unable to answer these questions because, by and large, longitudinal studies have not disaggregated homicide rates by motive or circumstance. Moreover, with few exceptions which we discuss below, longitudinal studies have not examined changes in immigration as they relate to over-time changes in violence, omitting a potentially important causal factor. These limitations are somewhat surprising because as we also discuss below, there exists a substantial research literature that investigates predictors of disaggregated homicide rates as well as a rapidly-growing body of literature on the longitudinal connection between immigration and crime.

Disaggregating Homicide

One of the most important findings to emerge from the homicide research literature is that homicide is a multidimensional phenomenon (Miethe and Regoeczi 2004; Zahn 1991:17). Homicides vary substantially in terms of motive, characteristics of the victim and offender, setting, and circumstances. One of the earliest attempts to systematically categorize homicides into conceptually meaningful categories is found in Marvin Wolfgang's (1958) classic study, *Patterns in Criminal Homicide*. In the first-ever analysis of a comprehensive homicide dataset, Wolfgang analyzed nearly 600 homicide events in Philadelphia between 1948 and 1952, studying case characteristics in more detail than had any previous research. Motivated in part by the results of Wolfgang's study, scholars have increasingly argued that homicides are not a homogeneous group. Rather, there are different types of homicides with potentially distinct patterns, correlates, and causes (Flewelling and Williams 1999). Consequently, there has been an expansion in the number of studies that attempt to identify factors related to separate homicide "types."

Despite a general movement towards disaggregation, scholars have not settled on one particular classification strategy to serve all purposes. Indeed, some have focused on differences in the victim-offender relationship (Decker 1993; Parker 2001; Parker and Smith 1979; Parker and Toth 1990; Riedel and Przybylski 1993; Rodriguez and Henderson 1995; Rojek and Williams 1993), others on differences in motive (Block 1993; Block and Block 1992; Cornell et al. 1989; Goldstein et al. 1992; Harries 1993; Kubrin and Ousey 2009; Mears and Bhati 2006), and still others focus on the circumstances of the homicide event (Harries 1993; Goldstein et al. 1992; Papachristos 2009; Pyrooz 2012; Rosenfeld et al. 1999). Other studies develop homicide subtypes on the basis of particular theoretical frameworks or empirical methods. For example, in their study of homicide rates across US cities, Williams and Flewelling (1988) create a theoretically integrated model combining social disintegration, resource deprivation, and violent cultural orientation perspectives, which guides the calculation of disaggregated rates. And Kubrin (2003) uses cluster analysis to classify homicide subtypes using detailed victim, offender, and event information on over 2,400 homicides in St. Louis from 1979 to 1995. Finally, focusing on the homicide situation (i.e., the nexus of offender, victim, and offense characteristics in time and space) as the unit of analysis and using qualitative comparative analysis, Miethe and Regoeczi (2004) examine several data sources including Supplemental Homicide Reports from 1976 to 1998 and state- and city-level police data to determine the empirical distribution of homicide over three decades.

A discussion of the detailed findings from this literature is beyond the scope of the current study. However, three important themes have emerged that are directly relevant to our purposes. First, evidence indicates that some factors (e.g., concentrated disadvantage) exhibit effects that extend across many homicide subtypes whereas others (e.g., residential instability) may affect a more limited set of specific types (e.g., Kovandzic et al. 1998; Kubrin 2003; Parker 1989). Thus, the correlates (and their attendant theoretical explanations) of homicide subtypes may be different. Second, researchers know much more about what predicts between-city variation in homicide subtypes than they do about what explains within-city changes in type-specific rates. This is because there are few studies that have examined change in the *nature* of homicide over time within geographic units (cf., Miethe and Regoeczi 2004). Third, although findings from a burgeoning literature suggest that measures of immigration may be among a select group of "structural covariates" that consistently predict changes in aggregate rates of violence, that placement would be premature because to our knowledge there is much that is unknown about the generality of the immigration-violence relationship, including how immigration affects over-time changes in specific homicide subtypes.

In sum, the research literature on homicide subtypes underscores the importance of considering disaggregated homicide rates. Studies reveal that the effects of some explanatory variables exhibit broad ranging effects across many homicide categories whereas others may be limited to specific types. However, this literature also exhibits two important gaps that are relevant for the current study. First, with few exceptions—for example, Kikuchi and Desmond (2010) and Kubrin and Herting (2003), both of which are neighborhood-level studies—the vast majority of this research is cross-sectional. And second, nearly all of the studies except two (Kubrin and Ousey 2009; Nielsen et al. 2005) exclude immigration as an important potential correlate, despite research suggesting that immigration is related to violence, a point which we discuss below.

Immigration and Violence

Historically, immigrants have disproportionately taken the blame for many of society's problems, including crime. Yet a substantial literature has documented that immigrants are less, not more, crime prone than their native-born counterparts. Studies repeatedly show that immigrants are less likely to be involved in, or institutionalized for, crime compared with those who are native born. In fact, the consistency of this finding prompted Martinez and Lee (2000:496) to conclude: "The major finding of a century of research on immigration and crime is that… immigrants nearly always exhibit lower crime rates than native groups."

Compared to a sizable literature on the individual-level association between immigrant status and criminal offending, "there exists a comparative shortage of research on the *macro-level* relationship between immigration and crime rates" (Ousey and Kubrin 2009:447–448). But prompted perhaps in part by Robert Sampson's (2006) *New York Times* op-ed which links declining crime rates of the 1990s to increased immigration, the field has recently witnessed a proliferation of macro-level research on this topic (see Ousey and Kubrin 2009:453–457 for a summary of aggregate-level studies on the immigration-crime relationship). Ironically, the vast majority of these studies are cross-sectional; thus, there remains a dearth of longitudinal research on the immigration-crime nexus. This shortage of longitudinal research is a serious limitation because immigration is a dynamic process whose effects unfold over time within places (Ousey and Kubrin 2009:448). Indeed, many of the most important questions are centered on the issue of what will happen to crime rates within a given place as it experiences an expansion or contraction of its immigrant base. Questions such as these are about *within-place temporal processes* and therefore can only be answered with a longitudinal framework that observes how immigration and crime vary within-units (e.g., cities) over-time. Assuming that between-place (cross-sectional) relationships accurately reflect within-place (longitudinal) relationships is risky, at best. Indeed, it is not unusual to find that between-unit and within-unit effects operate in opposite directions (Curran and Bauer 2011; Phillips 2006a; Wadsworth 2010).

There are some notable exceptions to the claim that immigration-crime research is primarily cross-sectional (MacDonald et al. 2013; Martinez et al. 2010; Ousey and Kubrin 2009; Stowell et al. 2009; Wadsworth 2010). Two of these studies, in particular, examine the immigration-crime nexus in the context of understanding how immigration may have contributed to the crime drop. Using time-series techniques and annual data for metropolitan areas from 1994 to 2004, Stowell et al. (2009) assess the impact of changes in immigration on changes in violent crime rates. In line with Sampson's claim, they find that violence tended to decrease as metropolitan areas experienced gains in their concentration of immigrants.

In a somewhat similar analysis conducted at the city-level, Wadsworth (2010) evaluates the influence of immigration on city-level crime rates from 1990 to 2000. His analyses include cross-sectional models that examine the between-city relationship between immigration and rates of homicide and robbery in 1990 and 2000, respectively, and fixed-effects models that examine how within-city changes in immigration over the decade influenced changes in homicide and robbery over that time period. Results from his analyses are not entirely consistent but in general, it appears that in the cross-sectional analyses, cities with higher levels of foreign-born residents have higher homicide and robbery levels. Results from the longitudinal models, however, indicate that cities with the largest increases in immigration experienced the greatest decreases in homicide and robbery. These findings illustrate our previous point that assuming the equivalence of cross-sectional and longitudinal effects of immigration is unwise.

Although not focused on estimating immigration's contribution to the crime drop, Ousey and Kubrin (2009) investigate the impact of changes in immigration on changes in violent crime for 159 US cities from 1980 to 2000. In line with Stowell et al. (2009) and Wadsworth (2010), their study reports that on average, cities that experienced increases in immigration experienced decreases in violent crime rates. Moreover, it also indicates that immigration lowers violent crime by bolstering the prevalence of two-parent family structures. A related study by Martinez et al. (2010) explores the effects of immigration on neighborhood-level homicide trends in San Diego, California over the same time period. Consistent with the studies just reviewed, their results show that the increasing foreign-born population reduced lethal violence. Finally, MacDonald et al. (2013) investigate the extent to which immigrant concentration is associated with reductions in neighborhood crime rates in the city of Los Angeles between 2000 and 2005. Once again, consistent with extant research, they find that greater concentrations of immigrants in LA neighborhoods are linked to significant reductions in crime, even after accounting for the non-random selection of immigrants into L.A. neighborhoods.

Immigration and Homicide Subtypes: Theoretical Implications

While the preceding studies are valuable, they also are limited because none considers the impact of immigration (or other covariates) on specific types of lethal violence. This limitation is important because studying homicide subtypes can provide important insights into a key theoretical question: How general is the relationship between immigration and violence? At present, the most prominent scholarly explanations of the immigration-crime association emphasize two alternative theoretical perspectives: social disorganization and immigrant revitalization. The former asserts that immigration is a disruptive force that breaks down collective social control leading to a positive association between immigration and violence. Moreover, this framework conceives of immigration's effects in general theoretical terms. This means that criminogenic effects are expected to be similar across different forms of crime, including different homicide types (Nielsen et al. 2005). The revitalization perspective, in contrast, suggests that immigration yields protective effects that should lower rates of homicide. However, a review of scholarship invoking this framework is unclear on whether it should yield general effects that are similar across crime types or whether its effects should be more salient for some types of offending than for others. Some discussions imply that immigration lowers crime generally by increasing social integration and collective social control. But others seem to suggest that immigration may help improve particular aspects of social life. For instance, immigration may benefit the local economy by providing local businesses with a hard-working but low-cost labor supply or by increasing employment growth as immigrant-owned startup companies proliferate. If the latter imagery is correct, it seems reasonable that the most prominent impact of immigration may be to temper economically-motivated violence, such as homicides committed during the course of other felonies (e.g., robberies) or as part of involvement in the illegal drug trade, with much weaker effects expected for other subtypes, such as argument-related killings. Alternatively, if the former depiction of the theory is accurate, there is little reason to expect noteworthy differences in the expected effects of immigration on homicide subtypes.

To our knowledge, there are only two prior studies that examine the differential impact of immigration on homicide subtypes. First, in a neighborhood-level study of black and

Latino homicides in Miami and San Diego (Nielsen et al. 2005), Nielsen and her colleagues disaggregate a decade's worth of homicides into several motive subtypes including escalation, intimate, robbery, and drug-related motives. Findings from their analyses show that recent immigration is negatively or not associated with most subtypes but is positively associated with some instrumental killings (e.g., African-American drug-related homicides in San Diego). Second, in a city-level study Kubrin and Ousey (2009) report somewhat similar findings with respect to immigration's impact on specific types of lethal violence. Using Supplementary Homicide Report data from 2000 to 2002, Kubrin and Ousey (2009) compute measures of argument, felony, drug-related, and gang-related homicides. They find that cities with greater levels of immigrant concentration generally have lower rates of all homicide types, but they also note evidence of a significant and fairly strong positive relationship between immigration and gang-related killings. Viewed together, findings from these studies bolster the possibility that immigration may have differential effects on homicide subtypes. Consequently, they suggest that theoretical mechanisms that link immigration and homicide may be specific and varied, rather than general and uniform. Yet evidence from Nielsen et al. (2005) and Kubrin and Ousey (2009) must be regarded as preliminary because these studies, like so many others in the immigration-crime literature, are cross-sectional. Consequently, they do not address the central question of whether within-place changes in immigration may (differentially) affect changes in particular subtypes of homicide.

Finally, in addition to the shortage of longitudinal research on immigration and homicide, particularly homicide subtypes, prior research has given little attention to one other important issue: whether the effect of immigration on lethal violence varies across places. Conceptually, there is good reason to expect such variation. Portes and Rumbaut (2006), for example, suggest that different cities provide varying "contexts of reception" for immigrants. Some cities are well-established immigrant destinations that have ethnic enclaves equipped with developed economic and social institutions that allow them to effectively provide benefits to, and derive benefits from, new waves of immigration. It is in these kinds of places, we argue, that immigration is most apt to provide a crime-constraining benefit. Alternatively, newer destinations may have a limited preexisting immigrant base and are likely to lack the economic and social organization to efficiently integrate and incorporate new arrivals, resulting in short-term instability or disruption rather than improved economic vitality and social cohesion. In these latter places, then, increased immigration is less likely to result in a reduction of violence. A handful of studies have begun to evaluate whether the effects of immigration on violent crime may vary across places as a result of differences in their preexisting social context (e.g., Shihadeh and Barranco 2010; Velez 2009). Most notably, findings support the idea that the immigration-crime relationship differs across "new destination" and "established/old destination" places (Shihadeh and Barranco 2010). However, in addition to being few in number, these studies are cross-sectional. Consequently, they do not allow us to discern whether within-place, over-time growth in immigration affects temporal changes in total and circumstance-disaggregated homicide rates differently depending on preexisting context of reception that characterized a place.

In sum, findings from longitudinal studies on the immigration-crime nexus offer noteworthy support to the argument that changes in immigration are associated with declining crime rates, underscoring the importance of including this factor in aggregate-level studies of crime in general, and violence in particular. Still, only a handful of studies exist and those that do leave important questions unanswered. Indeed, despite both conceptual and empirical reasons for doing so, most prior studies have not disaggregated homicides into subtypes to determine how changes in immigration may be differently related to changes in these forms of lethal violence. Moreover, no previous studies have considered whether the aforementioned longitudinal relationships between immigration and homicide rates (total and subtypes) vary depending on a city's initial context of reception.

Data and Methods

Units of Analysis

The current study focuses on US cities that have minimum populations of 100,000 residents in 1980, 1990, 2000, and 2010. In total, 156 cities meet these criteria and have available data for at least one of the time points. The complete sample of city-year observations used in our analyses is 591.[1]

Dependent Variables

The dependent variables include the overall city homicide rate as well as rates for each of four homicide subtypes. To generate these rates, we first obtained counts of homicide from the Supplementary Homicide Report (SHR) dataset compiled by the FBI as part of the Uniform Crime Report. For each time point, we computed the relevant count by summing 3 years of homicide data. To compute "1980" homicide counts, we summed counts from 1979 to 1981; for "1990" counts, we used 1989–1991 data; for "2000" counts we used 1999–2001 data, and for "2010" we used 2008–2010.[2]

The first subtype is *argument homicides*. These include homicides that are classified as having resulted from lover's triangles, arguments over money, brawls that erupt among participants under the influence of drugs or alcohol, and other arguments. Second, *felony homicides* include cases in which a murder occurs during the course of another felony such as a robbery, burglary, or rape. *Drug-related homicides* occur during actions that violate narcotics laws (e.g., buying/selling/distributing illegal drugs). *Gang-related homicides* refer to incidents with circumstances classified in the SHR dataset as a "gangland killing" or "youth gang killing."[3]

In an earlier version of our analysis, we estimated models in which homicide cases with unknown circumstances were treated as a separate "unknown" homicide subtype category. However, an anonymous reviewer noted that it was difficult to assess meaning and interpret results regarding this unknown subtype category. After further consideration, we decided to eliminate the unknown subtype model results. Consequently, the subtype analyses presented below utilize only the homicide cases for which circumstances are known. However, to evaluate whether our findings and conclusions regarding the homicide subtypes are sensitive to whether unknown circumstance cases are included

[1] Because some cities have missing data for one particular time point but not others, the number of valid city-year observations (591) is somewhat less than the number of cities multiplied by the number of time points (i.e., $N \times T = 156 \times 4 = 624$).

[2] The 2010 SHR data were the most recently available when our analyses were initially conducted.

[3] Some prior research distinguishes between gang-affiliated and gang-related homicides (e.g., Rosenfeld et al. 1999). While we acknowledge that such a distinction may be relevant, unfortunately, the SHR data do not allow us to make such a distinction.

or excluded, we re-estimated our main models using twenty "complete" datasets in which missing homicide circumstance codes were imputed via the chained equations imputation routine ("mi impute chained") in Stata version 12. Findings from the analyses of the imputed datasets are essentially identical to those reported herein and are available from the first author upon request. The specific SHR homicide circumstance codes that we included in each of the homicide subtypes described above are reported in "Appendix 1".

The use of SHR homicide data has been the focus of an important debate in the homicide research literature. In particular, some researchers have questioned the extent to which the value of these data is dampened by incomplete and missing information, especially related to offenders, victims, and motive or circumstance (Fox 2004; Maxfield 1989). A related body of literature has debated methods of adjusting homicide rates to accommodate missing data (see, for example, Flewelling 2004; Fox 2004; Pampel and Williams 2000; Wadsworth and Roberts 2008; Williams and Flewelling 1987). While we acknowledge the limitations of SHR data, our position appears to be consistent with other scholars who have used SHR data in their studies of disaggregated homicide rates (e.g., Dugan et al. 1999; Kovandzic et al. 1998; Messner and South 1992; Miethe and Drass 1999; Nelsen et al. 1994; Ousey and Lee 2002; Wadsworth and Kubrin 2004; Williams and Flewelling 1987). Specifically, we believe that the SHR data are an important tool for building better understanding of the patterns and predictors of variation in the nature of homicide in the US. Indeed, our view resembles that of Miethe and Regoeczi (2004) who, in their comprehensive study of homicide situations and circumstances across nearly three decades, noted: "The SHR data are highlighted as the most comprehensive data on homicide because of their national scope and inclusion of measures of most of the basic elements that underlie homicide situations" (pg. 13). In sum, SHR data are uniquely useful for the current study objectives. They provide the only source of detailed information on homicide incident circumstances across a large sample of cities for the 1980–2010 time period.

Independent Variables

We predict homicide subtypes with key independent variables that occupy a prominent place in prior studies of crime trends and of disaggregated-homicide rates. To gauge the effect of immigration, we use two measures, the *percent foreign-born* and the *percent Latino*. Each of these has appeared in prior research investigating the immigration-crime nexus (e.g., Morenoff and Sampson 1997; Nielsen et al. 2005; Ousey and Kubrin 2009; Stowell et al. 2009; Wadsworth 2010; Sampson and Morenoff 2004; Sampson et al. 1997).[4] In the analyses reported herein, we follow the lead of prior researchers (e.g., Sampson et al. 1997; Stowell et al. 2009) and combine them into an *immigration index*, computed by averaging standardized scores for both measures. The overall Pearson's correlation between the two items in the index is .704 (within-city correlation = .821, between-city correlation = .690).

[4] A measure of linguistic isolation, the percentage of persons who speak English "poorly" or "not at all," also has appeared in some studies. We considered using this item in the current analysis. However, because the linguistic isolation question for the 2010 American Community Survey differs from that of the preceding Decennial Censuses, we decided against using it.

An extensive body of scholarship, albeit largely cross-sectional, suggests that levels of homicide in cities (and other geographic units) are associated with variations in socio-economic disadvantage. To account for the effects of within-city changes in structural disadvantage, we compute a *disadvantage index* by averaging the z-scores of four variables: the percent of persons in poverty, the percent of persons aged 16-plus who are unemployed, the percent of family households headed by a single parent, and the percent of the city population that identifies as Black/African-American. The Cronbach's alpha for this index is .892.[5]

It is theorized that places with greater family instability experience an attenuation of adult supervision of youth and a weakened social control capacity, which lead to higher crime rates, including lethal violence. Moreover, research finds that in general, compared to the native-born, immigrants have a more familistic and pro-nuptial orientation (Oropesa and Gorman 2000; Oropesa and Landale 2004). As such, we include a measure of the *percent divorced*, which predicts between-city variance in homicide in numerous studies (Land et al. 1990; McCall et al. 2010; Phillips 2006a). Finally, although longitudinal research estimating the association between the percent divorced and homicide is more sparse, there is some supporting evidence (McCall et al. 2008; Ousey and Lee 2002).

Homicide rates also vary as a function of demographic characteristics of cities, including the overall population size and the relative prevalence of young adults (e.g., Levitt 2004; McCall et al. 2008; Ousey 1999; Phillips 2006b). To control for these effects, our analyses include a measure of the natural logarithm of *city population* as well as a measure of the percent of the population comprised of *males aged 15–34*. Finally, because there is reason to believe that both immigration and violence may be associated with recent changes in the labor market structure of cities (Ousey and Kubrin 2009; Parker 2004; Shihadeh and Ousey 1998), we include a control for the *percent employed in professional or managerial occupations*.

For the years between 1980 and 2000, the preceding measures of socioeconomic disadvantage, demographic structure, and occupational composition are computed from summary files from the decennial census. Measures for 2010 are created from 2010 American Community Survey data (5-year estimates) obtained from the National Historic Geographic Information System (Minnesota Population Center 2011).

Since scholarship suggests that changes in homicide rates may be linked to fluctuations in drug market activity and enforcement, as well as changes in policing capacity, we include two additional predictors in our models. First, we include a measure of the arrest rate for the sale or distribution of cocaine and opiates obtained from Uniform Crime Report data (we employ 3-year averaged rates, e.g., 1979–1981 data are used for the 1980 data point). Second, we compute a measure of the police officer rate (i.e., officers per 1,000 persons) from the Law Enforcement Officers Killed or Assaulted dataset. Finally, to account for the effects of general temporal trends in the homicide rates measures, we

[5] Partly reflecting the influence of the seminal study by Land, McCall, and Cohen (1990), the inclusion of the percent black in "disadvantage" indices has become common practice. Empirically, this practice is justified by the high correlations that exist between percent black and other indicators of socioeconomic disadvantage in geographic units, which is also the case in our data. However, based on conceptual grounds, some scholars suggest that the percent black should be kept separate from disadvantage index measures. We believe there is some merit to both of these positions, and while the analyses presented herein employ a disadvantage index that includes the percent black, we also estimated models in which percent black is included as a separate predictor. Results from those models are very similar and lead to identical substantive conclusions as those that we present below.

include linear and quadratic time trend variables.[6] Descriptive statistics for the variables included in the analysis appear in "Appendix 2".

Analytic Strategy

To model homicide rates, researchers typically employ one of two modeling approaches. The traditional strategy is to create per capita rates by diving homicide counts by a relevant population base (e.g., the city population) and then modeling the resulting rate (or its natural logarithm) via a linear regression estimator. However, studies are increasingly modeling homicide rates with regression models for counts (e.g., Poisson or Negative Binomial). The models typically include an "offset" term that effectively standardizes counts by population size. The movement to the count models is motivated, in part, by the fact that linear regression can be poorly suited for count-based outcomes, which are discrete, highly skewed, and non-negative (Osgood 2000). In preliminary analyses, we estimated our models with both negative binomial and linear regression estimators and then compared the respective fit of each modeling approach to the observed data. Overall, the negative binomial models provided a better fit. Hence, our main presentation and discussion focuses on results obtained from the negative binomial models.

Since our focus is geared towards explaining within-city changes in homicide, we employ fixed-effects negative binomial models.[7] While fixed-effects models are one of several modeling strategies for analyzing longitudinal or repeated measures datasets, they hold the advantage of estimating the effects of measured time-varying predictor variables while controlling for time-stable omitted variables with time-stable effects (Allison 2005). In contrast, alternative strategies such as the random-effects model have the disadvantage of assuming a zero correlation between measured time-varying predictors and time-stable omitted variables.[8]

We obtained within-city estimates of the effects of the independent variables by unconditional maximum likelihood. Specifically, using the negative binomial regression procedure ("nbreg") in Stata version 12, we estimated models that include the predictor variables described above along with dummy variables for all of the city units except one (i.e., $J-1$ city dummy variables).[9] Including the city dummy variables controls for the effects of city-specific, time-invariant effects, thereby removing all between-city variation

[6] The linear time-trend variable is centered at 1990 (i.e., 1990 = 0).

[7] To be clear, our analysis approach is distinct from studies that examine cross-sectional effects of structural covariates at multiple points in time (e.g., Land et al. 1990) as well as from analyses that pool time cross-sections together and obtain estimates via ordinary least squares or via random-effects models. Each of the preceding approaches produces effects that, to varying degrees, reflect between-unit (i.e., between-city) effects. Our approach removes between-unit variation, focusing only on variation within units over time.

[8] If this assumption holds, the random effects model may be preferred due to greater statistical efficiency. Several tests that compare fixed-effects and random-effects models are available. We use the approach suggested by Allison (2005), which is based on comparing the coefficients associated with the within-unit and between-unit components of the same variables. If the assumptions of the random-effects model are valid, these coefficients should not be significantly different; if they are, the fixed-effects model is preferred. For each model we estimated, the test procedure supported fixed-effects over the random-effects estimator.

[9] Stata software includes a fixed-effects model explicitly designed for panel data analysis, "xtnbreg, fe." However, Allison and Waterman (2002) showed that the model fails to account for effects of time-stable omitted variables.

from our analysis.[10] Hence, the coefficients for our explanatory variables reflect the impact of within-city, over-time variation in those variables on within-city temporal variation in overall and type-specific homicides.

Our analysis proceeds in four steps. First, we estimate baseline growth models for each homicide outcome. These models include linear and quadratic measures of time or measurement occasion and give us an image of the patterns of change in each homicide type from 1980 to 2010. Second, we elaborate the baseline models by incorporating the independent and control variables, including the immigration measure. These models facilitate an assessment of the effects of within-city change in the explanatory variables on changes in the various homicide measures. In addition, these models allow us to determine the extent to which the time trends observed in our initial models are accounted for by changes in those explanatory variables. Third, we examine models that include an interaction that estimates whether the effect of within-city changes in immigration vary across cities as a function of between-city differences in preexisting levels of immigration. Finally, employing an instrumental variables framework, we conduct supplemental analyses to assess if there is evidence supporting the notion that immigration is an endogenous predictor of homicide rates. Importantly, these analyses will determine how modeling endogeneity of immigration affects our estimates and thus our substantive conclusions regarding the impact of within-city changes in immigration on changes in homicide rates.

Results

Overall Homicide Models

In Table 1, we present results from fixed-effects negative binomial regression models predicting overall homicide rates in 156 US cities. In the first model, we include only the time trend variables to capture an image of the direction of temporal changes in overall homicide rates for those cities from 1980 to 2010. Given that the time variables are centered at 1990, the coefficient for the linear trend conveys the proportional growth rate for homicide rates measured at the 1990 time point. The coefficient is negative and statistically significant, indicating that homicide rates are declining. The corresponding quadratic trend is small and positive but not significant, suggesting little evidence of acceleration or deceleration in the relative rate of change in homicide through the period of observation.

In the second model of Table 1 we introduce the control variables but leave out the immigration measure. Six of the predictors have a statistically discernible association with the relative change in the overall homicide rate. The direction of these effects is generally consistent with expectations. We find evidence that proportional increases in the overall

[10] A caveat is that coefficients in unconditional negative binomial models may be subject to "incidental parameters" bias (see Allison 2005:95; Allison and Waterman 2002). However, a Monte Carlo simulation study of this issue suggests the dummy variable approach to fixed-effects negative binomial does not result in any substantial bias, even under circumstances that are most likely to produce it (see Allison and Waterman 2002). Nevertheless, according to Allison (2005) one alternative to obtaining fixed-effects estimates is the "hybrid model," which decomposes independent variables into time-stable and time-varying components and includes both in a random-effects regression model. Because the hybrid approach does not require estimation of dummy variables for each panel unit, it should not be affected by incidental parameters bias (Allison 2012). Thus, as a supplemental analysis, we also estimated our models via the hybrid method. Results obtained are nearly identical to those obtained with the unconditional (dummy variable) estimator.

Table 1 Fixed-effects negative binomial model predicting total homicide in large US cities, 1980–2010

	Model 1	Model 2	Model 3
Time trends			
Linear	−0.149***	−0.132**	0.00352
Quadratic	0.0151	0.0346*	0.0149
Within-city change variables			
Divorced		0.0772***	0.0345^{+}
Disadvantage		0.240***	0.299***
Drug arrest rate[a]		0.0389**	0.0383**
Police officer rate		−0.0197	−0.00855
Percent males 15–34		0.0678***	0.0773***
City population (Ln)[a]		−0.228*	−0.0289
Percent professionals		−0.0193***	−0.0227***
Immigration			−0.309***
Constant	3.344***	1.942**	1.574*
$N \times T$ (observations)	591	591	591

$^{+}$ $p < 0.1$; * $p < .05$; ** $p < .01$; *** $p < .001$. Models include log of the city population as an offset. Models also include dummy variables for $J - 1$ cities

[a] Coefficients for this variable multiplied by 100 to reduce leading zeros

homicide rate are associated with increases in the percent divorced, socioeconomic disadvantage, the drug arrest rate, and the percent males aged 15–34. In contrast, homicide rates are negatively associated with within-city changes in the city population and with the percent employed in professional or managerial occupations. Interestingly, after controlling for these predictors, there is only limited impact on the linear time trend coefficient, but the quadratic term coefficient increases and becomes statistically significant. This suggests that some of the deceleration of the homicide growth rate was accounted for by within-city changes in these measures.

Next we examine the effects of within-city changes in immigration on relative changes in overall homicides, net of the control variables. Here we find evidence that increases in immigration have a significant negative association with the overall homicide rate. The coefficient (−.309) implies that a 10 % increase in the immigration measure is associated with a 3 % decrease in the homicide rate (i.e., $\exp[-.309^{*}.10] - 1 = -.03$). But recall that our immigration measure is comprised of two variables, percent foreign-born and percent Latino, which more than doubled between 1980 and 2010 in our sample of cities. Based on this evidence, it seems apparent that within-city changes in immigration, as measured herein, contributed much to changes in overall homicide rates over the time period. Supporting that inference, our results indicate that after taking into account the effect of within-city changes in immigration, both time trend coefficients are no longer statistically significant.

Homicide Subtype Models

Moving to Table 2, we present the initial results from our models predicting rates of homicide subtypes. In Panel A, we present coefficients from models containing only the time trend variables in order to observe baseline change parameters for each subtype. Panel

Table 2 Fixed-effects negative binomial models predicting homicide subtypes in US cities, 1980–2010

	Felony	Argument	Drug	Gang
Panel A: Baseline time trend models				
Linear trend	−0.302***	−0.341***	0.483***	0.614***
Quadratic trend	−0.032	−0.0123	−0.487***	−0.205***
Panel B: Full models with predictors				
Linear trend	−0.222^{+}	−0.273**	0.949***	0.554^{+}
Quadratic trend	−0.0289	−0.000732	−0.470***	−0.182*
Divorced	0.112**	0.0379	0.153*	0.242**
Disadvantage index	0.223	0.355***	0.665*	0.141
Drug arrest rate[a]	−0.0158	0.0338*	0.139*	−0.0383
Police officer rate	0.0819	0.0064	−0.318	−0.656*
Percent males 15–34	0.107***	0.0626**	0.301***	0.195***
City population (Ln)	0.297	0.123	1.134*	0.154
Percent professionals	−0.0300*	−0.0258**	−0.0591*	0.0213
Immigration	−0.137	−0.127	−1.057**	−0.00586
Constant	−2.426^{+}	0.498	−7.820**	−7.261**
$N \times T$ (observations)	591	591	591	591

$^{+}$ $p < 0.1$; * $p < .05$; ** $p < .01$; *** $p < .001$. Models include log of the city population as an offset. Models also include dummy variables for $J - 1$ cities

[a] Coefficients for this variable multiplied by 100 to reduce leading zeros

B then presents models that include the control variables along with the immigration measure.

Examining results from the first two time-trend models presented in Panel A, we find evidence of similarities and differences from the pattern observed for overall homicides reported in Table 1. As was true of the overall homicide rate, the linear time trend coefficients for both the felony and argument subtypes are significant and negative, while the quadratic coefficients are not significant. From this evidence, it appears that the time trends in argument and felony homicides are key components of the downward trend in overall homicide rates observed for our sample of cities between 1980 and 2010.

Time trends for the other two homicide categories, however, reveal different patterns of change. Specifically, we find that the linear trend coefficient for drug-related and gang-related subtypes is significant and positive, while the quadratic time trend coefficients are negative and significant. This indicates that when observed at the 1990 time point, drug and gang killings were increasing. However, for both subtypes there is deceleration in the rate of increase in the decades after 1990. For drug-related killings the deceleration appears to actually reverse the initial increase, whereas for the gang killings, the deceleration trend results in a flattening out of the positive growth trends.

In Panel B of Table 2, we add the predictor variables into each regression equation. Looking first at felony homicides, evidence indicates that controlling for these variables accounts for some portion of the time-trend, with the linear coefficient declining to −0.222 and its p value rising above .05. Concerning our main question of interest, we find no evidence that within-city changes in immigration have a significant effect on proportional changes in the felony homicide rate. Rather, it is within-city changes in the percent divorced and in the percent males aged 15–34 that are positively associated with proportional changes in felony homicides. In addition, within-city increases in the percent employed in professional occupations are linked to decreases in felony killings.[11]

In terms of argument homicides, the linear trend coefficient declines from −0.341 in the baseline model to −.273 in the model that includes the covariates. The results suggest that proportional changes in argument killings have a significant positive association with within-city changes in disadvantage levels, drug arrest rates, and the percent males aged 15–34. Moreover, there is a significant negative relationship with percent employed in professional occupations. Once again, we found no significant effect of within-city variation in immigration on proportional changes in argument killings.[12]

Contrary to results from the preceding two models, the third model of Table 2 suggests that within-city over time changes in drug-related homicides are predicted by within-city changes in immigration. Consistent with what we observed for overall homicides, over time increases in immigration are related to proportional decreases in drug-related homicide rates; a 10 % increase in our measure of immigration is associated with a 10 % decrease in rates of drug-related killings. In terms of the control variables, proportional changes in drug-related homicides are positively related to within-city changes in divorce rates, disadvantage levels, drug arrest rates, the percent of young males, and the log of city population. And they are negatively associated with changes in the percent employed in professional occupations. Interestingly, when the effects of immigration and the control variables are taken into account, there is a marked increase in the coefficient for the linear time trend variable (b = .483 vs. b = .949) over what was observed in the baseline drug homicide model of Panel A. This suggests that the growth rate in drug-related killings observed in Panel A is flattened by the collective, but unmeasured, effects of the covariates included in our analysis. Once those effects are explicitly taken into account in the model presented in Panel B, the estimated proportional growth rate gets considerably larger.

Within-city variation in immigration between 1980 and 2010 does not appear to be associated with proportional changes in gang-related homicides. Instead, gang-related homicides increase by a factor of 1.27 for each one unit increase in the percent divorced and by a factor of 1.22 for each unit increase in the percent males aged 15–34. In contrast, within-city increases in the police per capita measure are associated with decreases in gang homicides during the 1980–2010 period. After taking the effects of these variables into account, coefficients for the linear and quadratic trend variables are somewhat attenuated relative to the baseline estimates reported in Panel A.

Does the Effect of Within-City Change in Immigration on Homicide Rates Depend on City Differences in Baseline Immigration Levels?

In Table 3 we examine whether the within-city effects of immigration depend on between-city differences in initial levels of the immigration index. That is, we are interested in

[11] Because the models include the log of the population as an offset, the coefficient associated with the log of the city population variable is expressed relative to 1.0. Therefore, coefficients greater than one indicate a positive association while those below one indicate a negative association.

[12] Felony and argument homicides contain cases that appear notably heterogeneous (see "Appendix 1"). Therefore, it is worth considering whether the effects of immigration differ across the types that fall within the felony or argument homicide categories. To address this issue, we estimated fixed-effects negative binomial models predicting the major subtypes of felony homicide and the major subtypes of argument homicide. As we report in "Appendix 3", those results lead to a similar conclusion as the results reported in Table 2; in particular, immigration does not have a statistically discernible relationship with any of the felony or argument homicide subcategories.

whether the impact of *changes* in immigration is moderated by the size of the immigrant base that existed prior to the time period under observation herein. As noted earlier, there are conceptual and empirical reasons to suspect such variation in immigration's effects. Unfortunately, prior studies are cross-sectional and therefore have not examined whether the effects of *changes* in immigration on changes in lethal violence depend on the preexisting immigration context of a city. To investigate this issue, we estimated models that include an interaction between the time-varying (i.e., change) component of the immigration index and the preexisting—that is, 1970—level of our immigration index.[13]

In the first model of Table 3, we find evidence that the magnitude of the within-city relationship between immigration and overall homicide depends on city differences in the 1970 level of immigrant concentration. Specifically, in cities with higher immigration levels in 1970, the effect of within-city over time change in the immigration index on proportional change in overall homicide rates is amplified. For example, when evaluated at the 1970 mean level of the immigration index, a one unit (within-city) change in immigration is associated with a 24.3 % (i.e., $100 \times [\exp[-.279] - 1]$) decrease in the overall homicide rate (holding all other variables constant). But at one unit above the 1970 mean level of immigration, a unit change in immigration is associated with a 43.8 % decrease (i.e., $100 \times [\exp[-.279 + -.297] - 1]$) in the overall homicide rate (holding all else constant). To further illustrate how the initial immigration base moderates the effect of within-city variation in immigration on within-city changes in homicide rates, we present Fig. 1, which plots the marginal effects of immigration at five different levels (10, 25, 50, 75 and 90th percentiles) of the 1970 immigration index score. Consistent with the preceding description, this graph suggests that the negative association between changes in immigration and homicide rates is larger in cities with higher 1970 immigration index scores (e.g., the 90th percentile) and markedly lower in cities with initially low levels of immigration (e.g., the 10th percentile).

In models 2–5 of Table 3, we present interaction models for each homicide subtype. Looking across these models, it appears that the effect of change in immigration varies by the 1970 immigration level for all four subtypes. In each case, the sign of the interaction coefficient is negative. Thus, consistent with results observed for overall homicides, these findings indicate that the within-city relationship between immigration and homicide subtypes becomes more sharply negative in places that, on average, have a larger initial immigrant population base.[14] The interaction effects are particularly interesting in the felony and argument models. Recall in Table 2 we found that changes in immigration were not, on average, significantly related to changes in these homicide subtypes. But based on the results presented in Table 3, it appears that within-city changes in immigration are associated with proportional changes in felony and argument homicides in places that

[13] We measure the preexisting immigration level by computing the immigration index scores using 1970 Census data. These data for 1970 were obtained as an extract from the National Historical Geographic Information System at the Minnesota Population Data Center (2011). The 1970 immigration index is useful because it captures between-city differences that existed *prior* to the period of within-city change that is observed in our analysis. A plausible alternative would have been to use the 1980 immigration level rather than the 1970 level. We preferred the former because it can be considered exogenous to the 1980 homicide rate. It is worth noting, however, that generally similar results are obtained if 1980 data are used to measure baseline immigration levels.

[14] In the interest of space limitations and to reduce redundancy, we omitted the marginal effects plots of the interaction effects for each of the homicide subtype models.

Table 3 Fixed-effects negative binomial models including immigration interaction effect

	Total	Felony	Argument	Drug	Gang
Linear trend	0.0910	−0.0453	−0.228*	1.202***	0.787*
Quadratic trend	−0.0101	−0.0790*	−0.0132	−0.547***	−0.253**
Divorced	0.0126	0.0685^{+}	0.0266	0.0896	0.184^{+}
Disadvantage index	0.269***	0.166	0.345***	0.606*	0.0853
Drug arrest rate[a]	0.0371**	−0.0186	0.0326*	0.133*	−0.0510
Police officer rate	−0.0107	0.0805	0.00605	−0.299	−0.600*
Percent males 15–34	0.0855***	0.124***	0.0672**	0.321***	0.212***
City population (Ln)	−0.0952	0.184	0.0984	1.006^{+}	0.109
Percent professionals	−0.0290***	−0.0428***	−0.0289***	−0.0800**	0.00731
Immigration	−0.279***	−0.0713	−0.116	−0.908*	0.0167
Immigration * 1970 immigration	−0.297***	−0.584***	−0.144^{+}	−0.956***	−0.607*
Constant	2.244***	−1.137	0.795	−5.893*	−6.113*
$N \times T$ (Observations)	591	591	591	591	591

$^{+}$ $p < 0.1$; * $p < .05$; ** $p < .01$; *** $p < .001$. Log of the city population included as an offset. Models also include dummy variables for $J - 1$ cities

[a] Coefficients for this variable multiplied by 100 to reduce leading zeros

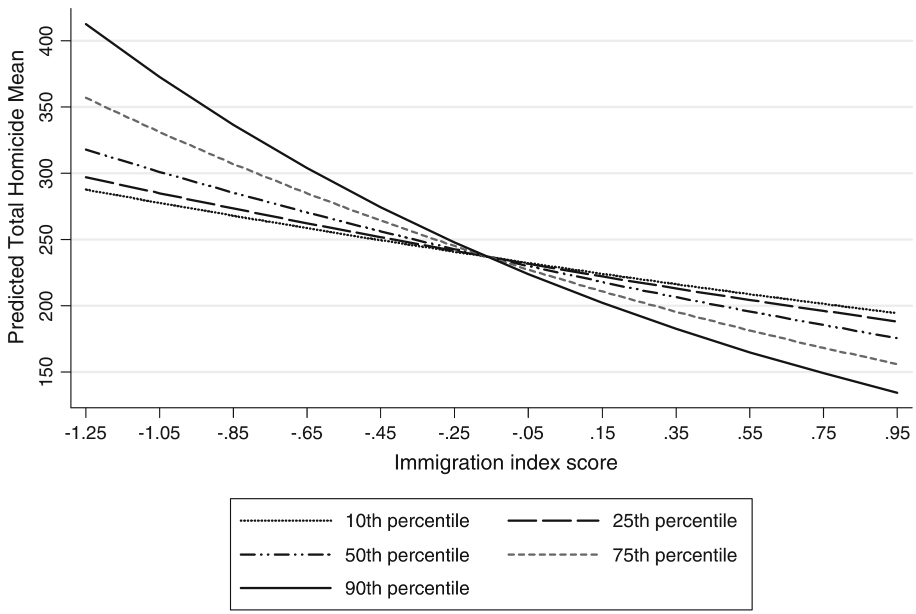

Fig. 1 Change in immigration on change in homicide at different 1970 immigrant concentration levels

ranked above the mean in terms of their preexisting (i.e., 1970) immigration levels.[15] In sum, in cities that had above average immigrant populations in 1970, within-city changes in immigration have a significant negative association with changes in the overall homicide rate and as well as with changes in the homicide subtypes.[16]

Supplemental Analyses

Similar to the analyses presented above, previous research on the immigration-crime relationship has treated immigration as an exogenous cause of crime. However, as the recent study by MacDonald et al. (2013) importantly points out, immigration flows are selective and may be influenced, in part, by crime rates. Hence, analyses that do not account for the potential endogeneity of immigration may produce a somewhat misleading picture of its effects on crime.[17] In their study of the immigration-crime nexus in Los Angeles neighborhoods, MacDonald et al. (2013) address this issue by explicitly modeling the selectivity of immigration via an instrumental variables 2SLS regression approach. They find that immigration is indeed endogenous to crime. After taking this endogeneity into account, they discover that higher levels of immigrant concentration are significantly associated with reductions in both total and violent crime rates. Moreover, their analyses appear to suggest that OLS regression may underestimate the immigrant concentration effect on crime in Los Angeles communities.

Given the potential importance of the endogeneity issue identified in MacDonald et al. (2013), we follow their lead and examine whether immigration into cities across the US may be influenced, in part, by crime rates. Specifically, we employed an instrumental variables framework to: (1) assess if there is evidence supporting the notion that immigration is an endogenous predictor of homicide rates; and (2) determine how modeling endogeneity of immigration affects our estimates, and more importantly, our substantive conclusions regarding the impact of within-city changes in immigration on changes in homicide rates.

In Table 4, we present a series of models that allow us to address these issues, focusing on the relationship between immigration and total homicide rates. In all models, the data are first-differenced to account for the city-specific fixed effects. The first model presents estimates obtained by the ordinary least squares estimator. It serves as a basic linear regression comparison for the instrumental variables results presented in the subsequent models. Consistent with what we observed in our fixed-effects negative binomial models, the OLS results suggest that immigration has a discernible negative relationship with the log of the homicide rate.

In the next two models of Table 4, we show estimates obtained via an instrumental variables 2SLS estimator. It is well-established that immigrants are more likely to select

[15] For example, at one unit above the 1970 mean, the within-city immigration coefficient in both the felony-homicide and argument-homicide is estimated to be negative and significant.

[16] We computed outlier diagnostics on the regression models presented. No observations were found to be influential outliers based on a combination of both a large studentized residual and a large leverage value. To be more conservative, we re-ran our models after excluding cases that had studentized residuals and leverage values that fell in the top five percent of the distribution. Results from these supplemental models closely mimic those discussed above. Thus, there is little evidence that our results are impacted by influential outlying observations.

[17] The consistency of standard linear regression estimators, such as ordinary least squares (OLS), rely on the assumption that explanatory variables are uncorrelated with the equation error term, an assumption that is violated when explanatory variables are endogenous (Wooldridge 2002).

Table 4 First-differenced OLS and instrumental variable regression models predicting logged total homicide rates in US cities, 1980–2010

	OLSFD	IV1	IV2
Linear trend	0.138*	0.741^{+}	0.371**
Quadratic trend	−0.00387	−0.0925	−0.0380
Divorced	−0.00242	−0.225	−0.0881*
Disadvantage	0.150^{+}	0.471*	0.274**
Drug arrest rate[a]	0.0425*	0.0283	0.0371*
Police officer rate	0.0606	0.147	0.0940
Percent males 15–34	0.0635***	0.0983**	0.0769***
City population (Ln)	−0.0828	1.106	0.376^{+}
Percent professionals	−0.0337***	−0.0278**	−0.0314***
Immigration	−0.303**	−2.092^{+}	−0.993***
$N \times T$ (observations)	430	430	430
Instrumental variable tests			
Endogeneity Chi square test		5.725*	7.063**
Underidentification LM test		4.688*	21.204***
Weak identification F test		4.043	21.257‡
Hansen J (Overidentification)			.0004

$^{+}$ $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

‡ Test statistic exceeds Stock-Yogo weak identification critical value (10 % maximal IV size)

[a] Coefficients for this variable multiplied by 100 to reduce leading zeros

into cities with larger pre-existing immigrant populations (Alba and Nee 2003; Logan et al. 2002). Hence in the first of the two models presented, we employed the 1970 level of immigration as an instrument for the subsequent year immigration index scores. Results from the first-stage equation, which are presented in "Appendix 4", suggest that the 1970 immigration level has a significant positive association with subsequent growth in immigration, net of controls. Results from the second stage equation are presented in Table 4 and, in general, they are consistent with the negative binomial and OLS model results in that changes in immigration are found to have a significant negative association with changes in logged homicide rates. Model test statistics suggest that this instrumental variables equation is identified and they also reject the null hypothesis that immigration is exogenous. Thus, consistent with MacDonald et al., the evidence here suggests that immigration is an endogenous predictor of violent crime. However, it also should be noted that the 1970 immigration index appears to be a weak instrument for subsequent changes in immigration. Indeed, the weak identification test statistic from this model falls below all of the weak identification critical values provided in the work of Stock and Yogo (2005).

Research documents that certain metropolitan areas, such as the New York City tri-state metro area, have long been dominant immigrant "gateways" for both practical and symbolic reasons. Meanwhile other areas of the country, such as the "border" states of the Southwest, have become important immigrant gateways in the post-World War II period as immigration flows have increasingly come from Latin America and Asia (Singer 2004). Based on this evidence we constructed a second instrumental variables model that employs two geographic dummy variables that signal whether a city falls within one of these two important gateways. The first dummy variable is coded one for all cities located in southwestern states that share a border with Mexico (i.e., California, Arizona, New Mexico, and Texas). The second dummy variable is coded one for all cities located in the New York City/Northern New Jersey/Western Connecticut metropolitan area commonly referred to as the New York "tri-state" region.

Results from the first-stage equation (see "Appendix 4") indicate that, as expected, both dummy variables have significant positive associations with changes in the immigration index, and collectively they explain more variance in that outcome than the 1970 immigration measure used in the prior instrumental variables equation. Test statistics from this instrumental variable model also suggest that the dummy variable instruments perform much better. In this case, the underidentification test statistic is significant at the .001 level and the weak identification statistic exceeds even the most stringent of the Stock-Yogo critical values. Notwithstanding these differences, the substantive pattern evident in these results is familiar. Again we find evidence that immigration is endogenous, with the endogeneity test significant at the .01 level. Moreover, after accounting for endogeneity, our measure of immigration has a significant negative relationship with changes in logged homicide rates. Also of note and consistent with findings from MacDonald et al. (2013), the results reported in Table 4 suggest OLS estimates of immigration effects on violence are smaller than those obtained via 2SLS.[18]

In Table 5, we show estimates from OLS and 2SLS instrumental variables models predicting each of the homicide subtype variables. All instrumental variables models use the border state and tri-state metro dummy variables as instruments. In general, the pattern of results from these models also supports the conclusions that emerged from the negative binomial models reported in Table 3. Specifically, it appears that the within-city changes in immigration primarily affect drug-related homicides. Indeed, both the OLS and 2SLS models suggest that greater within-city increases in immigration are associated with larger reductions in drug-related homicide rates. In contrast, there are no statistically discernible associations between the measure of immigration and the other homicide subtypes.

In summary, our supplemental models support the idea that immigration is endogenous to some measures of lethal violence. After the endogeneity is taken into account, estimates of the effects of immigration appear somewhat larger than those obtained by OLS. However, all models are consistent in terms of what they imply about the association between immigration and homicide. Indeed, the negative binomial, OLS, and 2SLS instrumental variable models all indicate that within-city changes are significantly associated with both total and drug-related homicide rates.

Discussion and Conclusion

What do trends in homicide in US cities look like over the past 30 years? Are these trends similar or different for different types of homicide with diverse motives and circumstances? What city-level characteristics might help to explain within-city over-time

[18] Although not shown in Table 4, we also estimated three additional instrumental variable models. Each of these models utilized the Arellano-Bond (1991) dynamic panel data estimator and included the lag of the homicide rate along with the immigration measure as endogenous explanatory variables. In the first of these two equations the excluded instruments were all of the available lags (in levels) of the preceding endogenous predictors. In the second equation we also added the aforementioned geographic dummy variables to the excluded instrument set. In the third model, we followed the suggestion of a reviewer and also accounted for potential endogeneity of the drug arrest rate and the police officer rate, using the available lags of each of those variables as instruments. Results from all three Arellano-Bond models are similar to those obtained via the other estimators. Most notably, the effect of immigration on homicide rates is significant and negative.

Table 5 First-differenced OLS and instrumental variable (2SLS) regression models predicting logged rates of homicide subtypes in US cities, 1980–2010

	Felony		Argument		Drug		Gang	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Linear trend	−0.0503	0.0724	−0.102	−0.193	0.340**	0.549***	0.100^{+}	0.0723
Quadratic trend	−0.0158	−0.0331	−0.00193	0.0115	−0.140***	−0.171***	−0.0348*	−0.0306^{+}
Divorced	0.0255	−0.0222	−0.00232	0.0298	−0.0386	−0.116*	0.0102	0.0202
Disadvantage	0.0398	0.112	0.190^{+}	0.151	0.369*	0.485***	0.0976	0.0827
Drug arrest rate[a]	−0.00259	−0.00453	0.0369*	0.0375*	0.0960***	0.0916***	0.0139	0.0143
Police officer rate	0.0427	0.0622	0.00120	−0.0246	−0.118	−0.0872	−0.0770*	−0.0806*
Pct. males 15–34	0.0392^{+}	0.0450*	0.0193	0.0119	0.0713**	0.0842***	0.0506***	0.0489**
City Pop. (Ln)	0.236	0.506^{+}	−0.0273	−0.185	0.0689	0.478^{+}	0.199	0.144
Pct. professionals	−0.0184*	−0.0175*	−0.0289**	−0.0298***	−0.0172^{+}	−0.0147	−0.00275	−0.00296
Immigration	−0.141	−0.508	−0.174	0.0829	−0.320*	−0.942**	0.0822	0.163
$N \times T$ (observations)	430	430	430	430	430	430	430	430
Instrumental variable tests								
Endogeneity Chi square test		1.201		1.188		4.552*		0.135
Underidentification LM test		21.204***		21.204***		21.204***		21.204***
Weak identification F test		21.257‡		21.257‡		21.257‡		21.257‡
Hansen J (overidentification test)		2.277		1.188		0.027		0.011

+ $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

‡ Test statistic exceeds Stock – Yogo weak identification critical value (10 % maximal IV size)

[a] Coefficients for this variable multiplied by 100 to reduce leading zeros

changes in both overall and homicide subtype trends? In particular, what role might immigration play for understanding these trends?

In the last decade or so, researchers have begun to address these questions. Their work can be identified in three related, but different, literatures—a literature on trends in lethal violence, a literature on homicide disaggregation, and a literature on the immigration-violence nexus. Despite key advances, there are some limitations to the studies in each literature that have left unanswered several critical questions: Are trends in immigration related to changes in the *nature* of lethal violence in the US over the last several decades? If so, does the effect of changes in immigration on homicide vary across cities as a function of differences in their baseline immigration levels?

Our study addressed these questions by investigating the patterns and predictors of temporal variation in disaggregated homicide rates for a sample of large US cities, focusing on the role that immigration may have played. We have built on the research literature in several ways. First, while prior studies have focused primarily on trends in overall homicides, we have extended that work by also examining time trends in several circumstance-specific killings—argument, felony, drug-related and gang-related. Second, we have addressed some limitations of the small but growing literature on the longitudinal relationship between immigration and violence. Prior studies have examined this relationship across multiple decades within a single city or have observed changes across multiple places (e.g., cities or metro areas) within a single decade. Our study has extended this work by examining the longitudinal association between immigration and homicide in more than 150 large cities over a 30-year period spanning from 1980 to 2010. Finally, we have advanced previous research by investigating the possibility that the longitudinal immigration-homicide relationship may vary depending upon city context. In particular, we have considered whether the effect of changes in immigration on homicide—both overall and circumstance-disaggregated—differs across cities as a function of differences in their initial or baseline immigration levels.

Our study yields findings pertaining to (1) the nature of trends in different types of homicide over a 30-year period, (2) the correlates of these trends, including immigration, (3) and whether the immigration-homicide relationship varies across different types of cities. Concerning the nature of homicide trends, we found that for homicide overall, rates have been declining. We also found little evidence of acceleration or deceleration in homicide's relative rate of change over the past 30 years. With respect to the subtypes, our findings revealed similarities and differences from the pattern observed for all killings. Similarities were identified for the argument and felony subtypes, whose trends mirrored those for overall rates, suggesting that the time trends in these subtypes are key components of the downward trend in overall rates observed for our sample of cities.

Trends for the other homicide categories, conversely, revealed different patterns of change. We found that the linear trend coefficient for drug-related and gang-related subtypes was significant and positive, while the quadratic time trend coefficients were significant and negative, indicating that when observed at the 1990 time point, drug homicides and gang homicides were undergoing increases. However, for these two subtypes, there was deceleration in the rate of increase in years after 1990. For drug-related killings the deceleration appeared to actually reverse the initial increase, whereas for the gang offenses, the deceleration trend resulted in a flattening out of the positive growth trends. An important conclusion from these findings is that the *nature* of lethal violence in US cities has been changing over the past 30 years as some homicide subtypes have become less prevalent and others more prevalent. Hence our findings underscore the point that disaggregation of homicide may be needed to fully understand trends in lethal violence.

Turning to findings on the correlates of these trends, and in particular our focus on immigration, we found that increases in immigration were associated with declines in overall homicide levels. Perhaps more telling though, our results indicated that after taking into account the effect of within-city changes in immigration, both time trend coefficients were no longer statistically significant. At the same time, we found that the effects of immigration were not consistent across the subtypes. We discovered that, on average, within-city changes in immigration do not significantly correspond with changes in felony, argument, or gang killings, but are negatively associated with changes in drug-related homicides. While these results suggest that changes in immigration are associated with changes in drug-related killings, we cannot determine whether immigration impacts all, or only some, of the categories of drug-related lethal violence that have been identified in prior scholarship (see e.g., Goldstein 1985). More generally, however, these findings reinforce the important point that there is variation in the impact of immigration on homicide types. That is, the impact of immigration is not uniform across types of lethal violence—nor is the impact of the other covariates for that matter.

Our analyses also addressed whether immigration was endogenous to crime, as suggested in a recent contribution from MacDonald et al. (2013). Consistent with their findings, we found some evidence that immigration is an endogenous predictor of both total and drug-related homicide rates. As was also true in the MacDonald et al. study, the effect of immigration on violence in models that account for endogeneity appears stronger than in comparable models that do not address endogeneity. Still, the basic conclusion that immigration exerts a significant effect on total and drug-related homicides was evident in models that both did, and did not, account for endogeneity. Yet because that may not always be the case, our analyses effectively underscore a salient point that emerges from MacDonald et al.—potential endogeneity is something that future studies of the immigration-crime relationship should explicitly investigate.

And finally, we found evidence that the magnitude of the within-city relationship between immigration and homicide depends on city differences in preexisting immigration levels. Specifically, in cities with higher immigration levels in 1970, the effect of within-city over time change in immigration on proportional change in overall homicide rates is amplified. Once we disaggregated homicide, we found that the effects of change in immigration vary significantly by the 1970 immigration level for all four subtypes. For these forms of lethal violence, we found that the immigration-homicide relationship is more sharply negative in places that, on average, have a larger preexisting immigrant population base. The findings are particularly interesting for felony and argument killings. Recall we initially found that changes in immigration were not significantly related to changes in felony or argument killings. Yet based on the results from the interaction models, it appears that within-city changes in immigration are associated with proportional changes in felony and argument homicides in places that ranked above the mean in terms of their 1970 immigration levels. A key conclusion from these findings, then, relates to the importance of considering how varying city contexts condition the immigration-violence relationship. Our current findings underscore the importance of different "contexts of reception" for immigrants, as reflected in the size of the preexisting immigrant base. Future studies should consider whether the magnitude of the immigration-homicide association and conditioning impact of preexisting immigration levels are different across decades (i.e., differ for the 1980–1990 decade vs. the 1990–2000 decade, and so on). Also of interest for future research is whether variation in "contexts of reception" may have differential impacts on the immigration-violence relationship in places where 2nd and 3rd generation immigrants are more prevalent.

Yet it is also the case that other aspects of city context may impact the longitudinal immigration-violence relationship, which underscores additional important directions for researchers. A key limitation of the current study is its singular focus on the size of the initial immigrant base as indicative of a city's "contexts of reception" for immigrants. Our interest in focusing on this was to indirectly capture the level of institutional support for immigrants in a given area, theorizing that well-established immigrant destinations are more likely to have thriving enclave communities that allow them to provide benefits to, and derive benefits from, new waves of immigration, thus inhibiting crime. But other aspects of cities' "contexts of reception" are potentially important. Consider, for example, that over the last several years, numerous policies have resulted from what is often referred to as the "devolution of immigration enforcement" in the US (Provine et al. 2012; see also Koulish 2010). In general terms, devolution consists of the statutory granting of powers from the central government of a sovereign state to government at a sub-national level, such as state or local level. In the context of immigration enforcement, devolution has encouraged a push toward formal partnerships between federal immigration authorities and local police. Under devolution, local law enforcement officials in the US have been encouraged to be more proactive when it comes to policing immigration, and the federal government has worked hard to encourage compliance. Examples of policies resulting from these efforts include the infamous Arizona SB 1070, which among other things, requires law enforcement officers to attempt to determine an individual's immigration status during a lawful stop, detention, or arrest, or during a lawful contact not specific to any activity where there is reasonable suspicion that the individual is an illegal immigrant.

Despite a strong push by the federal government and the support of groups such as State Legislators for Legal Immigration, there has not been a blind acceptance of devolution. In fact, and relevant for the current study, cities vary substantially in their degree of compliance. Provine et al. (2012) distributed questionnaires to police chiefs in large and medium-sized US cities to measure compliance. Their findings reveal a high degree of variation in local responses to federal devolution of immigration-enforcement responsibilities, resulting in a multi-layered, multi-jurisdictional patchwork of enforcement across the US. In light of findings from the current study, of interest would be the extent to which the relationships between immigration and lethal violence observed may be conditioned by city-level policies and practices that either support or denounce the devolution of immigration enforcement.

No doubt policies and politics related to immigration will continue to be salient for years and even decades to come. At the same time, immigrants will continue to arrive in the US in record numbers. Findings from this study encourage researchers to continue to examine just how these dynamics will (or will not) impact trends in lethal violence across cities in the US.

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Appendix 1

See Table 6.

Table 6 Supplementary homicide report circumstance codes and descriptions for homicide subtypes analyzed

Murder type	SHR code and circumstance
Felony	2 = Rape
	3 = Robbery
	5 = Burglary
	6 = Larceny
	7 = Motor vehicle theft
	9 = Arson
	10 = Prostitution and commercialized vice
	17 = Other sex offense
	19 = Gambling
	26 = Other felony
Argument	40 = Lover's triangle
	42 = Brawl under alcohol
	43 = Brawl under drugs
	44 = Argument over money
	45 = Other arguments
Drug	18 = Narcotics laws
Gang	46 = Gangland killing
	47 = Youth gang killing

Appendix 2

See Table 7.

Table 7 Descriptive statistics for variables in the analysis

	1980		1990		2000		2010	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Total homicides	215.54	539.82	240.69	592.58	152.04	305.79	146.38	245.28
Felony homicides	37.59	102.36	34.16	86.96	18.97	41.57	14.31	31.15
Argument homicides	87.64	186.9	84.09	185.18	42.63	84.49	35.91	78.26
Drug homicides	4.36	11.38	20.77	55.89	7.47	18.12	6.04	19.66
Gang homicides	2.89	17.44	10.57	70.19	10.83	65.11	11.09	53.93
Immigration Index	−0.37	0.66	−0.23	0.71	0.11	0.86	0.28	0.88
Percent divorced	8.25	1.7	10.17	1.88	11	2	11.28	2.14
Disadvantage Index	−0.25	0.74	−0.04	0.9	0.07	0.89	0.25	0.87
Drug arrest rate	20.99	30.92	146.91	207	99.38	173.1	65.27	110.63
Police officer rate	2.02	0.8	2.12	0.88	2.29	0.87	2.27	0.91
Males 15–34	18.85	2.5	17.78	2.22	16.07	2.17	16.08	2.15
Percent professionals	23.69	5.12	27.48	5.97	33.73	7.96	34.78	8.62
City population	351,818	677,924	381,890	713,265	420,448	782,587	431,040	779,268

Appendix 3

See Table 8.

Table 8 Fixed − effects negative binomial models predicting types of felony and argument homicides in US cities, 1980–2010

	Felony homicides				Argument homicides		
	Rape	Robbery	Burglary	Other felony	Lover's triangle	Brawl under drugs or alcohol	Other arguments
Linear trend	−0.731*	−0.364*	−0.383	0.531*	−0.355	−0.316	−0.341***
Quadratic trend	0.00332	−0.0366	−0.0249	−0.0660	−0.131	−0.142	0.0333
Divorced	0.239*	0.109*	0.145	0.127^{+}	−0.112	−0.0433	0.0627*
Disadvantage index	−0.00584	0.424*	0.486	−0.549*	0.502^{+}	0.769*	0.367***
Drug arrest rate[a]	−0.000448	0.0599	0.672	−1.19**	0.744^{+}	0.715	0.423*
Police officer rate	−0.146	0.189	−0.196	−0.219	−0.186	−0.154	0.00287
Percent males 15–34	0.0949	0.122***	0.00586	0.150**	0.0275	0.00496	0.0477*
City population (Ln)	−0.745	0.287	−0.0593	−0.242	0.141	0.544	0.142
Percent professionals	−0.0715*	−0.0118	−0.0930**	−0.0712**	−0.0284	−0.0604^{+}	−0.0224*
Immigration	0.532	−0.103	−0.158	0.115	−0.620	−0.376	−0.0303
Constant	−3.629	−3.944*	−2.068	−0.562	−0.489	−1.193	0.269
$N \times T$ (Observations)	591	591	591	591	591	591	591

$^{+}$ $p < 0.1$; * $p < .05$; ** $p < .01$; *** $p < .001$. Log of the city population included as an offset. Models also include dummy variables for $J - 1$ cities

[a] Coefficients for this variable multiplied by 100 to reduce leading zeros

Appendix 4

See Table 9.

Table 9 First-stage results from 2SLS models predicting change in immigration

	Model 1	Model 2
Linear trend	0.322***	0.258***
Quadratic trend	−0.045***	−0.040***
Divorced	−0.115***	−0.102***
Disadvantage	0.183***	0.188***
Drug arrest rate[a]	−0.008	−0.008^{+}
Police officer rate	0.049*	0.035
Percent males 15–34	0.019*	0.020*
City population (Ln)[a]	0.666***	0.595***
Percent professionals	0.004	0.006^{+}
Excluded instruments		
Immigration 1970	0.043*	
Tri-state region dummy		0.179***
Mexico border state dummy		0.100***
F test of excluded instruments (Kleibergen-Paap)	4.043*	21.257***
Overall equation *F* test	69.95***	89.25***
R-squared	0.484	0.526

$^{+}$ $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[a] Coefficients for this variable multiplied by 100 to reduce leading zeros

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