# Re-Examining the Relationship Between Immigration and Crime: Evidence from Mexico's Demographic Transition

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**Abstract**

Recent literature has utilized panel data on U.S. cities to estimate the effect of immigration on crime at the national level. Standard regression models rely on the tenuous assumption that the timing of migration flows to U.S. cities are "as good as random yet a great deal of evidence suggests that they are not. Building upon prior research, I propose a novel identification strategy that addresses this concern. Using historical data on the size of lagged Mexican birth cohorts and a measure of the strength of historical migration networks between Mexican states and U.S. metropolitan areas, I construct an instrumental variable that predicts decadal migration from Mexico to the United States. The intuition behind this identification strategy is that larger historical birth cohorts in Mexico yield more potential migrants in Mexican sending states once each birth cohort reaches prime migration age. Since migrants from each Mexican state exhibit distinctive and historically determined geographic settlement distributions across U.S. cities, future settlement patterns of Mexican immigrants are predictable. I report evidence that Mexican immigration leads to declines in property crimes. There is also evidence for a decline in rape and an increase in aggravated assaults. The available evidence suggests that this is not an artifact of lower crime reporting among immigrants.

**Keywords:** Immigration, crime, fertility

## 1. Introduction

After a long period of negligible growth, immigration and, in particular, Mexican immigration to the United States, increased exponentially, with the Mexican immigrant share of the U.S. population quadrupling since 1980. The majority of the increase in immigration has taken place since 1990 and coincides with the largest decline in U.S. crime rates since crime has been reliably measured.

While the aggregate time series suggests, if anything, that immigration may have had a protective effect on crime, perceptions of immigrant criminality continue to play a prominent role in recent policy debates regarding immigration reform and form the basis for a number of recent federal immigration efforts such as the establishment of 287g agreements with local law enforcement (Watson 2013) and the U.S. Department of Homeland Security's *Secure Communities* program which requires local law enforcement to cooperate with federal immigration authorities upon making an arrest (Sklansky 2012; Meissner, Kerwin, Chisthi and Bergeron 2013; Cox and Miles 2014; Treyger, Chalfin and Loeffler 2014).[1] Concerns over immigration-induced crime have also been prominent in state-level efforts to pass "E-Verify" laws which mandate employer cooperation with federal immigration enforcers (Bohn, Lofstrom and Raphael 2013; Chalfin and Deza 2017) and may have played an outsize role in the 2016 U.S. Presidential election.

The majority of research suggests that immigration has either played no role in this historic decline in crime (Butcher and Piehl 1998a; Butcher and Piehl 1998b; Reid, Adelman, Weiss and Jaret 2005; Feldmeyer 2009; Harris and Feldmeyer 2013; Chalfin 2014) or has possibly even contributed to the historic decline in crime during the 1990s (Hagan and Palloni 1999; Lee, Martinez and Rosenfeld 2001; Lee and Martinez 2009; Stowell, Messner, McKeever and Raffalovich 2009; Ousey and Kubrin 2009; Martinez, Stowell and Lee 2010; Wadsworth 2010; MacDonald, Hipp and Gill 2012; MacDonald and Sampson 2012; MacDonald and Saunders 2012; Ousey and Kubrin 2013; Schnapp 2014). Most notably, researchers have pointed to an under-representation of immigrants in state prisons, weak cross-sectional relationships between immigrant concentrations and crime at the neighborhood level and small and often even negative correlations between changes in a city's

[1] As noted by Butcher and Piehl 1998b and Lee, Martinez and Rosenfeld 2001, public perceptions of an immigration-crime link also played a role in earlier immigration reform legislation, specifically with regard to California Proposition 187.

immigrant share and changes in a city's crime rate over time as evidence against the proposition that immigration is criminogenic.

On the other hand, a handful of recent studies (Spenkuch 2014; Chalfin and Deza 2017) do point to a potentially positive relationship between immigration and crime, at least in some contexts. Spenkuch (2014) studies the relationship between immigration and crime at the county level and concludes that there is a positive relationship between immigration, particularly Hispanic immigration, and acquisitive crimes such as robbery and burglary. Chalfin and Deza (2017) find evidence of a positive association between Mexican immigration and crime, leveraging a recent natural experiment in Arizona.[2] With respect to Italy, Bianchi, Buonanno and Pinotti (2012) find that immigration has led to an increase in street crimes.

Thus while the lion's share of empirical work accords with the patterns in the aggregate time series and is consistent with numerous theories regarding the crime-reducing aspects of immigrant selection and the socially protective dynamics of immigrant communities, several new research findings provide countervailing evidence. Despite the recent surge in academic interest this topic and the consistency of most of the available research findings, the literature remains unsatisfying in several ways. First, the available literature rarely dis-aggregates the effects of immigration on crime by nationality. In particular, there is little research that addresses the criminal participation of recent Mexican immigrants, especially those who are undocumented and therefore have been, for better or worse, of central interest in policy debates over immigration.[3] As Mexican immigrants comprise over one third of all immigrants to the United States and over half of all undocumented immigrants (Passel and Cohn 2009), assessing the effects of Mexican immigration on crime would appear to be particularly relevant given the central importance of undocumented immigration in recent policy debates.

Second, prior literature has examined the effect of immigration specifically on crimes that are

[2]In 2008, Arizona passed a broad-based and stringent "E-Verify" law requiring employers to verify the legal status of newly hired workers. In response, approximately 20 percent of the plausibly undocumented population left Arizona. Crime declined substantially in Arizona relative to other U.S. states in the aftermath of the law's passage. However, Chalfin and Deza find that the increase in crime was purely compositional insofar as the increase in crime is explained by the change in the age and gender distribution of Arizona's foreign-born Mexican population.

[3]Spenkuch (2014) and Chalfin (2014) offer the first analyses that disaggregate the national effect of immigration on crime by Mexican nationality. Other studies — notably Lee, Martinez and Rosenfeld (2001) and MacDonald, Hipp and Gill (2012) — study the immigration-crime relationship at the neighborhood level. Here, an issue is that it is difficult to generalize from a study of a single city or cluster of communities.

*reported to law enforcement.* To the extent that immigrants are less likely to report crimes, an alternative explanation for a negative relationship between immigration and crime in the data is that immigration drives down crime reporting, a possibility which has been consistently noted by immigration and crime scholars but has not been addressed by previous research (Butcher and Piehl 1998b; Davis and Henderson 2003; Kirk, Papachristos, Fagan and Tyler 2012). This issue is particularly critical because this type of non-reporting bias is negative and thus has the potential to mask a positive relationship between immigration and crime. To address this issue, I provide an auxiliary analysis of the effect of immigration on the rate at which crimes are reported to police, using MSA-level data from the National Crime Victimization Survey (NCVS). This analysis appears to be new to the literature and suggests that differences in crime reporting rates are unlikely to explain negative correlations between immigration and crime in the extant literature.

Finally, prior estimates may be confounded due to the presence of omitted variables or measurement errors in immigration data. With respect to omitted variables, regression-based estimates of the effect of immigration on crime implicitly assume that the annual flow of migrants to particular U.S. destinations is arbitrary, conditional on variables that are controlled in the regression. To the extent that migrants select U.S. destinations or the timing of their journey on the basis of conditions in local labor or crime markets or the intensity of local immigration enforcement efforts, standard regression estimates will return an inconsistent estimate of the effect of immigration on crime. In other words, least squares parameters are only interpretable if migration flows are conditionally random, a proposition which is easily challenged on both theoretical and empirical grounds. Indeed empirical evidence is mounting that immigrants are extraordinarily mobile in response to changes in local conditions. A finding with particularly important implications for least squares approaches to the study of immigration and crime can be found in Cadena and Kovack (2013) which documents the extraordinary degree to which migrants' location choices have evolved as a result of the recent Great Recession. Similarly Cadena (2014) documents the responsiveness of immigration to changes in the minimum wage, leading to characterization of immigrants as *labor market arbitrageurs.* Perhaps more concerning is that recent research has demonstrated that migrants tend to relocate in response to local immigration enforcement efforts (Parrado 2012; Watson 2013) as well as the activation of employer-based sanctions for hiring undocumented workers (Bohn, Lofstrom and Raphael

2013; Chalfin and Deza 2017).[4]

Given prevailing concerns regarding omitted variables and measurement errors in immigration data, the extant literature remains vulnerable to several different identification-related criticisms. In this paper I employ a novel instrumental variables strategy that isolates plausibly random variation in the assignment of Mexican immigrants to U.S. destinations. The approach mimics a natural experiment in which different numbers of immigrants are assigned at random to each U.S. destination in each period. Since the instrument should be uncorrelated with contemporaneous conditions in U.S. cities and with measurement errors in immigration data, the scope for bias to compromise parameter estimates is greatly reduced, if not eliminated. Specifically, I leverage the fact that different Mexican states have gone through their respective demographic transitions at different times during the 20th century, thus generating a great deal of temporal variation in the size of state-specific Mexican birth cohorts. While some Mexican states experienced dramatic declines in fertility in the 1950s, other states did not see a marked decline in fertility until several decades later. As I show, this remarkable piece of demographic history has had profound implications for patterns of Mexican settlement in the United States. In particular, I show that U.S. cities experience the largest increases in Mexican immigration several decades after Mexican states to which those cities are historically and culturally linked experienced peak fertility.[5]

The intuition behind this empirical regularity is straightforward: larger birth cohorts predict a larger pool of Mexican nationals who are available to migrate when those cohorts reach prime migration age. Using data on the size of state-specific Mexican birth cohorts and aggregated survey data that measures the conditional probability that an individual from a given Mexican state migrates to a given U.S. metropolitan area (hereafter referred to as an "MSA"), I construct an instrumental variable that predicts decadal migration flows from Mexico to the United States. This instrumental variable is remarkably powerful, explaining as much as one third of the variation in city-specific migration flows between 1980 and 2000. Most importantly, because the instrument isolates

[4]Parrado (2012) finds little national evidence of internal migration in response to the activation of 287g agreements. However, evidence of internal migration is found in four highly influential jurisdictions: Dallas, TX, Los Angeles, CA, Riverside, CA and Phoenix, AZ.

[5]The identification strategy employed in this paper is described in a short paper by Chalfin (2015). Prior research by Hanson and McIntosh (2010) also documents a relationship between Mexican fertility and immigration to the United States.

variation in U.S.-bound Mexican migration that is due solely to conditions that existed many years ago in Mexico, my identification strategy avoids concerns regarding endogeneity, omitted variables and measurement errors in immigration data, each of which poses a potentially fatal threat to the standard approaches to identification in the existing literature.

Using this "births instrument," I provide evidence that Mexican immigration to the United States has generated considerable reductions in property crime in U.S. metropolitan areas. In particular, I find that a one percentage point increase in Mexican immigration (equal to approximately an 8 percent increase in the current stock of Mexican migrants) has led to a 10-15 percent reduction in reported larceny and motor vehicle theft. Similar reductions are found for rape. On the other hand, Mexican immigration appears to be associated with an increase in aggravated assaults. The results do not appear to be an artifact of immigration-driven changes in crime reporting.

The remainder of this paper is organized as follows. Section II provides a brief overview of theoretical linkages between immigration and crime. Section III motivates the identification strategy. Section IV describes the modeling framework. Section V provides a description of the data employed in the study. Section VI presents results and Section VII concludes.

## 2. Theoretical Links Between Immigration and Crime

While much of the empirical literature on the association between immigration and crime has appeared in the last two decades, interest among scholars in the relationship between the two variables goes back at least a century. Early sociological theories of criminal offending generally concluded that immigrants should be more likely to participate in crime than natives as a result of economic and social deprivation (Merton 1938; Shaw and McKay 1942). However, more recent theoretical work has highlighted the potential for immigrants to contribute to the economic and social development of urban areas in ways that are protective of crime (Lee, Martinez and Rosenfeld 2001; Portes and Mooney 2002, Sampson 2008; Ousey and Kubrin 2009). In this section, I briefly summarize theoretical arguments either in favor of a positive or a negative causal relationship between immigration and crime that have appeared in the extant literature. For a more detailed treatment, I note that theories of immigrant criminality have been ably summarized by Lee, Martinez and

Rosenfeld (2001), Reid, Adelman, Weiss and Jaret (2005), Sampson (2008) and Ousey and Kubrin (2009), among others.

The degree to which immigration and crime are related at a macro level is nuanced and depends on the types of migrants that the United States tends to attract as well as contextual factors at work in receiving communities. Economists have tended to focus on selection among migrants (see, for example, Borjas 1999, 2004) while criminologists and sociologists have written at length about social forces which inform migrants' experiences upon arrival in the United States. Generally, immigration can contribute to U.S. crime rates through one of three channels. First, immigrants to the United States may differ from U.S. natives according to characteristics that are typically observed by researchers. With respect to crime, the most important of these characteristics are age and gender which criminologists have long and convincingly linked to participation in criminal activity. To the extent that differences in criminal propensities among immigrants and natives are explained by observable characteristics, the differences are purely compositional and, as such, the contribution of immigration to U.S. crime rates is more or less mechanical and will only persist so long as the migrants are in their peak years of criminality.[6]

A second way in which immigration can affect the U.S. crime rate is through selection on characteristics which, given data limitations, are typically unobserved by researchers. These characteristics include personality traits such as intelligence, motivation and impulsiveness – traits that have been shown to predict criminal involvement among U.S. natives but are difficult to measure in national samples. An alternative but related possibility is that migrants bring with them different tolerances for risk and, as such, respond differently than natives to traditional criminal justice policy levers such as police and prisons. To the extent that migrants differ systematically from natives along unobserved dimensions, differences in criminal involvement will persist even if the demographic composition of immigrants is similar to that of natives.

A third possibility is that, independent of any underlying differences between migrants and

[6]An example of such compositional effects can be found in a historical analysis in Moehling and Piehl (2009) who examine the criminality of migrants to the United States in the early 20th century. They find that Italian immigrants were considerably more likely than U.S. natives (and other immigrants) to end up incarcerated in the United States. However, this finding is no longer true when examining age- and gender-specific arrest rates. Italian immigrants were more likely to be involved in crime because they were substantially more likely than other immigrants to be young and male. A contemporary version of this story is reported by Chalfin (2017) who analyzes a case study involving recent out-migration of immigrants in Arizona.

natives, contextual variables that shape the experiences of migrants and natives alike within the United States may either incentivize or deter crime. Examples of the types of contextual variables that can inform the relationship between immigration and crime are numerous and suggest that the relationship between the two variables is heterogeneous and complex. Theories that suggest a positive association between immigration and crime generally focus on material hardship, social disadvantage, and a lack of social cohesion in migrant communities (Bankston 1998; DeJong and Madamba 2001). With regard to material hardship, migrants are hypothesized to engage in crimes with an instrumental motive as a means of supplementing their incomes out of necessity born out of a lack of opportunities for legitimate earnings (Freeman 1996). Indeed, the substantially lower wages faced by Mexican immigrants suggests enhanced incentives to participate in crime in order to supplement one's legitimate earnings.[7] A more dynamic version of this story posits that sustained material deprivation may lead individuals to engage in violent crimes as an expression of rage or frustration (Blau and Blau 1982; Angew 1992).

With respect to social disadvantage, researchers have posited that assimilation of immigrants into poorer or more violent destination communities might influence participation in crime, above and beyond the characteristics of the immigrants themselves (Lee, Martinez and Rosenfeld 2001; Martinez Lee and Nielsen 2004).[8] For example, a sustained lack of opportunity for advancement within the legitimate labor market may lead to the creation of immigrant subcultures organized around ethnic gangs (Short 1997; Reid, Weiss, Adelman and Jaret 2005). More fundamentally, to the extent that neighborhood poverty is positively associated with crime, Mexican immigrants, who are, on average, poor, tend to settle in high crime neighborhoods within a central city, these migrants may be exposed to a higher degree of criminality and a greater concentration of anti-social norms (Shaw and McKay 1942, Anderson 1999; Hagan and Polloni 1999). This is especially true of neighborhoods that have pre-existing ties to illegal drug markets (Ousey and Kubrin 2009). To the extent that this arrangement is associated with greater crime than an arrangement that randomly assigns Mexican immigrants to neighborhoods, assimilation can be thought of as a driver

[7] A literature in economics that documents theoretical linkages between wages and the opportunity cost of crime can be traced back to the seminal work of Becker (1968). Recent surveys of the relationship between wages and crime can be found in Mustard (2010) and Chalfin and Raphael (2011).

[8] Martinez, Lee and Nielsen (2004) refer to this phenomenon as the "Americanization hypothesis."

of immigrant criminality. Finally, researchers have suggested that neighborhoods that are settled by immigrants, particularly those from poor source countries, tend to be unstable and lack social cohesion. In particular, the high degree of population turnover and frequent and rapid social change in immigrant neighborhoods creates an environment that is conducive to sustained criminal activity. This literature has focused primarily on the breakdown of informal social control (Bankston 1998; Lee, Martinez and Rosenfeld 2001; Mears 2002).

On the other hand, the tendency of immigrants to settle in ethnic enclaves might have a protective effect on their welfare and, as such, a dampening effect on crime (Martinez, Lee and Rosenfeld 2001; Logan et al 2004; Harris and Feldmeyer 2013). For example, immigrants tend to settle in communities and work for businesses that cater to other immigrants from their source country, shielding them from the effects of labor market discrimination (Portes 1997).[9] It has also been suggested that immigrants may form stronger labor market attachments despite being more likely to hold low-paying jobs because their frames of reference differ from those of natives (Zhang and Sanders 1999). Likewise, for a variety of reasons, immigrant neighborhoods may be associated with a greater degree of formal social control despite population churn (Desmond and Kubrin 2010). Finally, the loss of utility that arises from an arrest and subsequent conviction may be greater for undocumented immigrants who make up a substantial portion of the newly-arrived Mexican foreign-born population. This is because an arrest leads not only to a criminal sanction but also, in many cases, to deportation. As a result, immigrants may have an especially strong incentive to "fly under the radar.

A final consideration worth mentioning concerns responses to immigration rather than the experiences or behavior of migrants themselves. This consideration follows from an empirical literature on the social and economic effects of immigration on the experiences of U.S. natives and adds to the number of mechanisms through which immigration might affect crime by pointing out that immigration can also change the calculus of offending among natives. For example, if immigrants depress the wages or employment opportunities of natives whose crime-wage elasticities are highest, crime might rise in response to immigration even if immigrants are not responsible for the new

[9]The extent to which immigrants suffer from labor market discrimination is debatable given that survey research has found that employers report a greater willingness to hire low-skilled immigrants than their low-skilled native counterparts (Beck 1996; Wilson 1996).

crimes that are committed (Grogger 1998). Alternatively, immigrants might prove to be attractive crime victims and accordingly they might lower the search costs of potential offenders (Butcher and Piehl 1998). Related to this is the potential for immigration to contribute to ethnic tension that subsequently spills over into crime.

## 3. Conceptual Background

There are three fundamental approaches in the literature that have been used to study the criminality of immigrants: (1) microdata studies of the criminal involvement of individual migrants and U.S. natives, (2) neighborhood-level studies that examine the relationship between immigrant concentration and community violence at a granular level and (3) macro-level studies that examine the immigrant-crime nexus at a higher level of aggregation, typically a city or metropolitan area. This section provides the reader with an introduction to the intellectual history that has guided the modern immigration-crime literature with an eye towards understanding the literatures that have arisen from each approach.

### *A. Microdata Research*

The tradition in the microdata research has been to examine the demographic characteristics of institutionalized populations and report the extent to which immigrants are either over- or under-represented among inmates in prisons or jails. This research has mostly found that recent immigrants are underrepresented among those individuals who reside in an institutionalized setting at the time of the decennial census after conditioning on age and gender (McCord 1995; Butcher and Piehl 1998a; Hagan and Polloni 1999; McCord 1995; Butcher and Piehl 2008).

The advantage of research designs that compare the institutionalization rates of foreign-born to the native-born is that the descriptive nature of the exercise does not require a convincing source of identifying variation. Likewise, such analyses plausibly capture an effect which is due to solely to the criminality of immigrants, rather than an effect of immigration that is a mixture of immigrant crimes and crimes committed by natives. However, for several reasons, this line of research may not provide an internally-valid and policy relevant estimate of the contribution of immigration to

cross-city crime rates. First, there are concerns over the quality of available data. In order to study comparative institutionalization rates, researchers have used either data from the U.S. Census or administrative data from state prisons. With respect to Census data, the primary limitation is that it is not possible to disaggregate the incarcerated from the otherwise institutionalized and thus the validity of the resulting estimates requires an assumption that immigrants and natives have the same relative propensities to be incarcerated conditional upon institutionalization.[10] A second issue is that much of the Census data for institutionalized populations are imputed, leading to a number of concerning inconsistencies in the data (Camarota and Vaughan 2009).

Administrative data is a potentially attractive option but a state prison's accounting of the nationality of inmates is typically derived from self-reported data from inmates. To the extent that undocumented inmates may fear deportation following release from prison they may face incentives to disguise their place on birth when completing questionnaires at intake. The result is that administrative data may undercount — perhaps dramatically — the number of foreign-born inmates in state prisons in the United States. Indeed, descriptive evidence from the U.S. government's State Criminal Alien Assistance Program (SCAAP) suggests that undocumented immigrants may, in fact, be slightly overrepresented among state prisoners — at least in some states, a finding that contrasts sharply with research that uses state prison data (Camarota and Vaughan 2009).

Two other concerns are worth mentioning. First, institutionalization rates, even if well-measured, tell us about who is incarcerated and not necessarily about who is arrested or who commits crimes in the first place. To the extent that crimes face different clearance rates in immigrant communities or if immigrants face different propensities to be incarcerated conditional upon arrest, institutionalization will be an imperfect proxy for crime. Second, to the extent that immigration changes the calculus of offending among U.S. natives, an examination of the institutionalization rates of the foreign-born fails to capture general equilibrium effects associated with immigration. Thus, while the approach to studying the relationship between immigration and crime using individual-level microdata provides an important benchmark of the criminal involvement of the foreign born, this research is not a perfect substitute for an empirical estimate of the effect of immigration on crime

[10]The U.S. Census last differentiated between the incarcerated population and the population that is otherwise institutionalized in 1980.

derived from aggregate data.

*B. Neighborhood-Level Research*

A second approach to studying the effect of immigration on crime uses data on neighborhoods or Census tracts in a given city with different immigrant concentrations to study the cross-sectional association between immigrant concentration and crime. The advantage of such studies is three-fold. First, because all neighborhoods lie within a common jurisdiction, crime reporting by the local police agency is more likely to be uniform as compared to cross-jurisdiction studies. Second, neighborhood-level research studies the effect of immigration at a considerably lower level of aggregation and one accordingly may better capture the local dynamics that drive both variation in migration and crime. Finally, neighborhood studies are particularly well-suited to characterizing heterogeneity in the relationship between immigration and crime. Prominent neighborhood-level studies include Lee, Martinez and Rosenfeld (2001) using cross-sectional data from El Paso, Miami and San Diego, Sampson, Morenoff and Raudenbush (2005) using data from Chicago, Martinez, Stowell and Lee (2010) using panel data from San Diego, MacDonald, Hipp and Gill (2012) using panel data from Los Angeles and Desmond and Kubrin (2010) using national Add Health data. Each of these studies suggests that neighborhoods with high immigrant concentrations witness lower levels of violence than otherwise similar neighborhoods with higher immigrant concentrations.

The primary disadvantage of neighborhood-level studies is that data do not allow researchers to address concerns regarding endogeneity. Cross-sectional studies risk confounding immigration with many other neighborhood-level differences which may be correlated with crime. Panel data research is helpful but requires that changes in a neighborhood's immigrant concentration are unrelated to crime. This is a convenient assumption for the sake of research but may be unrealistic given how sensitive home values and rents are to changes in crime (Tracey and Rockoff 2008). Another concern is that as the share of immigrants in a neighborhood changes, so too may policing policies and practices. A second limitation of neighborhood level studies is that case studies of particular cities or collections of neighborhoods within a city may not be generalizable with respect to learning about the national effect of immigration. As a result, while neighborhood-level studies contribute a great deal to our understanding of the immigration-crime relationship, it remains important that

studies at a higher level of aggregation continue to play a prominent role in the literature as well (Stowell, Messner, McGeever and Raffolovich 2009).

*C. Macro-level Research*

The standard approach to studying the macro level effect of immigration on crime in the recent literature is to leverage longitudinal data on U.S. cities, regressing the crime rate on the city's immigrant share, net of national crime shocks and between-city variation. The use of panel data has represented an advance in a literature that was, in the past, characterized primarily by the use of cross-sectional data and has led to more credible national-level estimates.

Panel data regressions presented in Ousey and Kubrin (2009) and Stowell, Messner, McKeever and Raffalovich (2009) estimate the following two-way fixed effects model:[11]

$$CRIME_{it} = \alpha + \beta IMM^*_{it} + X_{it}\gamma + \phi_i + \rho_t + \varepsilon_{it} \tag{1}$$

In (1), $CRIME_{it}$ is the per capita number of crimes reported to the police in city $i$ in year $t$, $IMM^*_{it}$ is the the share of the city that is comprised of immigrants, $X_{it}$ is a vector of city- and time-varying controls and $\phi_i$ and $\rho_t$ are city and year fixed effects, respectively. Researchers argue that by conditioning on covariates and purging the data of national trends and between-city variation, equation (1) can return a consistent estimate of $\beta$, the effect of a change in the immigrant share on crime. While fixed effects purge the model of implausibly exogenous between-city variation, the model is nevertheless identified only under stringent assumptions regarding the behavior of migrants and potential migrants. Formally, the assumption needed to identify equation (1) is that, conditional on $X_{it}$ and the fixed effects, $\text{cov}(IMM_{it}, \varepsilon_{it}) = 0$. This condition is violated if $IMM^*_{it}$ is correlated with a time-varying characteristic that is omitted from (1) and is also correlated with $CRIME_{it}$. The assumption is also violated if $CRIME_{it}$ and $IMM^*_{it}$ are simultaneously determined, for example if migrants explicitly select into U.S. cities on the basis of crime rates.

While the recent literature typically employs a variety of control variables to pick up on city-

[11] Stowell, Messner, McKeever and Raffalovich use annual data from the Current Population Survey while Ousey and Kubrin use decadal data from the U.S. Census. Wadsworth (2010) pursues a similar approach, differencing (1) to remove the fixed effects.

and time-varying confounders, the scope for omitted variables bias remains large. Characteristics that merit particular concern and which are not typically available as regressors in standard models include changes in the priorities of local law enforcement (for example, cooperation between local law enforcement and federal immigration enforcers or the declaration of sanctuary cities), changes in state-level criminal justice policy (for example, changes in the severity of sentencing or the value of block grants for law enforcement personnel) and un-modeled macroeconomic conditions such as the employment rate in specific industries and sub-industries in which immigrants tend to be predominantly employed. A final consideration worth noting is that recent research has found that Mexican immigrants are highly responsive to local employment conditions in occupations with high immigrants concentrations (Cadena and Kovack 2013; Cadena 2014). While controlling for the local unemployment rate may soak up a portion of such variation, residual variation in the unemployment rate that is relevant to attracting or repelling migrants is likely to persist.

Finally, it must be noted that (1) is likely invalid if changes in the immigrant share are not reliably measured (Butcher and Piehl 1998b; Chalfin 2014; Spenkuch 2014). Given that approximately 80 percent of recent Mexican migrants are undocumented (Passel and Cohn 2009), deriving an accurate measure of the immigrant share is an empirical and a conceptual challenge. The implications of measurement errors are potentially large. If measurement errors are classical (that is the errors are random with respect to the variables in the model and are additive) then $\beta$ estimated via least squares will be attenuated towards zero (see, for example, Fuller 1987; Cameron and Trivedi 2005). On the other hand, if measurement errors in immigration data are systematically related to observed or unobserved city characteristics, the direction of the bias becomes unpredictable and difficult to characterize (Lubotsky and Wittenberg 2006; Chen, Hong and Nekipelov 2011). The empirical crime literature has recently identified several applications which demonstrate the scope for measurement errors to create substantial chaos in interpreting least squares crime regressions. Prominent examples include those of Donohue and Levitt (2008) with respect to the relationship between abortion and crime and Chalfin and McCrary (2017) with respect to the relationship between crime and police manpower. These examples serve as a cautionary note in interpreting least squares estimates in the immigration-crime literature.

Unfortunately the standard solutions to fixing problems caused by omitted variables are likely

to exacerbate the problem of mis-measured data. In particular, conditioning on fixed effects and covariates likely worsens the problem of mis-measured data by disproportionately extracting the signal from $IMM_{it}$ relative to noise. The reason is that, since immigrants tend to exhibit persistence in settlement patterns, the fixed effects as well as the covariates are likely to be strongly related to the true immigrant share. Thus the variation remaining in the measured immigrant share after conditioning on the fixed effects is likely to contain a great deal of noise.[12]

Recent macro-level studies of the effect of immigration on crime by Ousey and Kubrin (2009), Stowell, Messner, McKeever and Raffalovich (2009) and Wadsworth (2010), while thorough and careful, are not robust to the omission of time-varying covariates, simultaneity bias, or measurement errors in immigration data. Accordingly, the results they report, while plausible, leave room for lingering doubt that the true relationship between immigration and crime might be obscured by one or more unmodeled factors. The dual problems of simultaneity/omitted variables and measurement errors in immigration data motivates the need for an alternative identification strategy to isolate the causal effect of immigration on crime.

*D. Instrumental Variables*

Instrumental variables is a particularly promising strategy in that, under certain conditions, it can address each of the three concerns that arise with respect to least squares. The chief IV strategy that has been used in the literature to isolate quasi-random variation in immigrant flows is to instrument for recent flows of country-specific immigration with country-specific immigrant flows that are predicted by the national flow of migrants to the United States and the location decisions of past migrants.[13] The approach relies on the empirical regularity that migrants tend to take advantage of pre-existing social networks and sojourn to U.S. destinations that host an existing community from their country of origin. Accordingly the instrumental variable arising from this framework has come to be known as the "network" instrument (Pugatch and Yang 2010). Formally,

[12]See Chalfin and McCrary (2017) for a discussion of this issue with respect to the police manpower literature.

[13]This instrumental variable was pioneered by Altonji and Card (1991) in their seminal treatment of the cross-city effect of immigration on the wages and employment of natives. This approach has subsequently been used by Saiz (2003) to estimate the effect of immigration on U.S. housing markets and by Card (2001) and Card and Lewis (2007) to update Altonji and Card's analysis of the effect of immigration on labor market outcomes of U.S. natives.

the network instrument ($Z_{it}^n$) can be written as:

$$Z_{it}^n = \sum_{c=1}^{n} MIG_{ct} \times \lambda_{ic} \tag{2}$$

In (2), $MIG_{ct}$ is the number of immigrants from country $c$ who are living in the United States in year $t$ and $\lambda_{ic}$ is a matrix of source region-U.S. destination weights that capture the long-term conditional probability of migration from each source region $c$ to each U.S. destination $i$. The network instrument, $Z_{it}^n$, is the interaction of these two terms, summed over the $n$ source regions and is the predicted number of migrants living in city $i$ in year $t$. In other words, if we know the total stock of the foreign-born Mexican population in a given year and we know the pre-existing distribution of Mexicans among U.S. cities, we can predict the stock of immigrants in each U.S. city in year $t$. With respect to crime, using data from the 1980s, Butcher and Piehl (1998b) estimate the effect of immigration using the network instrument in a panel of forty-three U.S. metropolitan areas and find that immigration is not associated with any type of crime, violent or property while Spenkuch (2014) uses the network instrument at the county-level and finds large effects of immigration on crime, a finding which is particularly large for Mexican immigrants. MacDonald, Hipp and Gill (2012) employ the network instrument at the neighborhood level using data from Los Angeles.

To the extent that the lagged values of the stock of the foreign-born population do not directly affect contemporary crime rates, the network instrument presumably satisfies the exclusion restriction needed to achieve identification and returns an unbiased estimate of the effect of a specific exogenous flow of migrants on crime (Pugatch and Yang 2010, Chalfin 2014). However, there are several ways in which the prior location decisions of migrants may affect current crime rates, other than via their "pull" effect on subsequent migrants. First, to the extent that there is serial correlation in unobserved city-specific factors that are correlated with crime, the instrument might isolate not only exogenous variation in migration to that city but also migration that is drawn by persistent city-specific amenities. For example, if migrants are drawn to a particular city due to the existence of certain characteristics (.e.g., the city's industrial mix) in 1960, to the extent that these characteristics persist, today's migrants may be pulled to a city for similar reasons. Second,

as noted by Card (2001) and Pugatch and Yang (2010), the exclusion restriction will be violated if there are persistent city-specific shocks that differentially affect traditional gateway cities relative to non-gateways. For example, if differentially higher crime growth (or slower crime reductions) in gateway cities was a meaningful determinant of immigrant flows, then the network instrument would lead to an estimate of the effect of immigration on crime that is positively biased.

Another way to see that this is a problem is to consider that while the migration weights, $\lambda_{ic}$ in (2) are not time-varying, the stock of Mexican immigrants in each U.S. city in a given year, $MIG_{ct}$, will be a function of both events that are unfolding in Mexico and contemporaneous conditions in U.S. destinations, some of which may be captured via control variables and others which may not be. To the extent that control variables do not exhaustively capture the dynamics of city-specific migration, the network instrument leads to an inconsistent estimate of the effect of immigration on crime and is likely to be attenuated towards least squares estimates.

The network instrument plausibly purges the data of some portion of the "bad" (that is, implausibly exogenous) variation in the immigrant share and, accordingly, represents an advance beyond standard least squares approaches to the study of the effect of immigration on crime. However, instruments that rely on variation in factors that *pull* immigrants to a given city are nevertheless inevitably problematic in that they rely on the presumably endogenous location decisions of prior immigrants or a lack of persistence in the characteristics of cities over time. Recognizing this, Chalfin (2014) proposes that a cleaner source of variation in Mexican immigration may be found in a factor that *pushes* migrants out of source regions.[14] The intuition is that immigration that arises as a result of conditions in the source region is less likely to be correlated with omitted city-level variables than immigration that is due to conditions in destination communities.

In this paper, I provide a decomposition of the network instrument that can be used to isolate "pull" from "push" variation using long-differenced data. The idea is that the network instrument

[14]Chalfin shows that extreme rainfall in Mexico induces migrants to leave Mexico along pre-established network pathways and, accordingly, can be used to predict short-run fluctuations in the Mexican immigrant share in U.S. cities. The idea that rainfall can be used as a push instrument for immigration is not new. With regard to immigration from Mexico, Munshi (2003) and Pugatch and Yang (2010) have used rainfall variation to study the effect of Mexican immigration on labor markets in the United States and Mexico. Kleemans and Magruder (2011) use a similar approach to study labor markets in Indonesia. The intuition behind the instrument is that low rainfall causes a shock to the local economy, thus incentivizing greater migration. Higher rainfall can also induce migration to the extent that it is associated with a more robust local economy and migrants are credit constrained.

can be apportioned into a part that is explained by past Mexican birth rates and a part that is attributable to the time-varying conditional probability of immigration. In particular, the network instrument can be decomposed into two components: (1) the available supply of Mexican nationals who are eligible to migrate to the United States in a given year, $t$ ($N_t$) and (2) the conditional probability that an individual migrates to the United States in a given year ($p_t$). To see this, consider that, in a given year, there is some number of Mexican nationals ($N_t$) who are available to migrate to the United States. $N_t$ is itself a function of the number of lagged Mexican births (where the length of the lag will correspond with the ages of likely migrants) and the number of deaths in each cohort among the $N_t$ individuals. The number of Mexicans who *actually* migrate to the United States in a given year is $N_t \times p_t$.

Whereas $N_t$ is a function of conditions in Mexico many years ago, $p_t$ is a function of contemporary conditions in both Mexico and traditional migrant destinations in the United States. For example, $p_t$ might rise due to a currency crisis in Mexico or due to favorable employment conditions in Los Angeles or Chicago. Likewise $p_t$ might change if migration is sensitive to the size or conditions of illicit labor markets or to the scope of local immigration enforcement. It is in this way that $p_t$ creates a potential problem for the network instrument. For example, if a particular city is experiencing positive wage growth over a given time period, this wage growth might increase the conditional probability of migration, thus building in a negative bias to the network instrument.[15] Recognizing this, we would like to remove $p_t$ from the equation instead focusing only on the size of available migration cohorts.

The identification strategy I propose requires that variation in the size of lagged birth cohorts in Mexican states predict variation in U.S. cities' Mexican-born population shares. The mechanism underlying this relationship consists of two elements. First, lagged birth cohorts in Mexican states must be associated with higher numbers of U.S.-bound migrants from those states. Second, emigration from each Mexican state must flow predictably into a distinctive set of U.S. destination cities, and the predictability of that flow must stem from factors that are otherwise unrelated to contemporary destination labor market conditions. It is uncontroversial that, other things equal,

[15] The bias is negative to the extent that positive wages growth is, other things equal, associated with a reduction in crime.

larger historical birth cohorts yield more potential migrants in Mexican sending states once each birth cohort reaches prime migration age. Thus a larger number of potential migrants will yield a larger number of actual migrants unless birth cohort size is negatively associated with its members' propensity to migrate.[16]

Likewise, it is well established that migrants tend to travel to the same destinations that others from their source region have previously settled (Massey 1999; Munshi 2003; Light 2006; Pugatch and Yang 2010; Chalfin 2014; Spenkuch 2014). The resulting networks confer social and informational benefits that can furnish tangible help in finding work and housing. Importantly, many migration networks linking regions in Mexico to U.S. destinations were forged early in the 20th century, often tracking railroad routes along which U.S. employers brought in recruited agricultural labor (see, e.g., Cardoso 1980; Massey et al. 2002; and Woodruff and Zenteno 2007).[17] Thus, for reasons independent of prevailing labor market conditions in potential U.S. destinations, variation in push factors in different immigrant source regions will dependably influence the number of migrants located in different sets of U.S. destinations.

## 4. Empirical Strategy

Using data from three U.S. Censuses (1980, 1990 and 2000), I begin with a sample consisting of 84 MSAs with a sufficient presence of Mexican immigrants to allow reliable estimation.[18] Using Census data, I generate an estimate of the proportion of each MSA's population that is comprised of Mexican immigrants in a given Census year ($IMM_{it}$) and use $\Delta IMM_{it}$ to denote the decadal change in this quantity. By construction, $\Delta IMM_{it}$ can be disaggregated into the number of Mexican immigrants who migrate to each U.S. MSA from each of thirty-two Mexican states where $m$ indexes

[16] Hanson and McIntosh (2010) have documented that the rate of emigration is, in fact, higher among members of larger Mexican birth cohorts, providing a second mechanism through which large lagged birth cohorts are positively associated with emigration.

[17] In fact Pugatch and Yang (2011) use historical railroad routes as the basis for their calculation of migration network weights.

[18] 2010 is omitted because the rise to prominence of new destinations among Mexican migrants means that the instrumental variables strategy pursued in the paper is no longer applicable to more recent cohorts.

the Mexican states:

$$\Delta IMM_{it} = \sum_{m=1}^{32} \Delta IMM_{mit} \tag{3}$$

Thus, in (3), the number of Mexican migrants arriving in MSA $i$ in year $t$ is simply the sum of the number of Mexican migrants arriving in that MSA in that year who migrated there from each of thirty-two Mexican states. Since $\Delta IMM_{mit}$ is not observable given currently available data and is likely endogenous, it must be estimated. Following the functional relationship suggested by the traditional network instrument, I formulate $\Delta IMM_{it}$ as a function of the total number of Mexican migrants arriving from each Mexican state to the United States in each year ($\Delta IMM_{mt}$) and a set of Mexican state-U.S. MSA migration weights ($\lambda_{im}$). These weights capture the conditional probability that a migrant from Mexican state $m$ settles in U.S. MSA $i$ upon arrival in the United States. Specifically, the weights are estimated using the mean probability that a migrant from Mexican state migrates to each U.S. city using data from 1921-1979.[19]

Equation (4) captures this relationship, with the inclusion of a time- and city-varying disturbance term that captures idiosyncratic shocks that are unrelated to the migration weights.

$$\Delta IMM_{it} = \sum_{m=1}^{32} (\Delta IMM_{mt} \times \lambda_{im}) + \varepsilon_{it} \tag{4}$$

While the weights, $\lambda_{im}$ are strictly pre-determined, static, and reflect long-standing network ties, $IMM_{mt}$, captures the number of individuals who leave each Mexican state in year $t$, varies over time and potentially captures both conditions that push migrants out of Mexico as well as social and economic conditions in linked U.S. MSAs. To see this consider that an individual from a given Mexican state may choose to leave in a given year due either to unfavorable conditions in Mexico or favorable conditions in U.S. cities that are historically linked to that state.

In order to purge this term of implausibly exogenous variation, I recognize that the number of migrants who leave each Mexican state ($\Delta IMM_{mt}$, in equation (5) can be de-composed in the

[19]Following Chalfin and Levy (2013), I choose 1979 as an end date to ensure that all of the migration relations contained in $\lambda_{im}$ are pre-determined with respect to the study sample.

following way:

$$\Delta IMM_{mt} = \left[ \sum_{m=1}^{m=32} \sum_{t=t-17}^{t=t-52} BIRTHS_{mt} \right] \times Pr[MIG|BIRTH]_{mt} \tag{5}$$

In (5), the first term within the double summation is the number of births in network-linked Mexican states that occurred between 17 and 52 years ago.[20] The second term is the average probability of migration to the United States conditional upon having been born in Mexico between 17 and 52 years ago. This term varies both by Mexican state and by year and, as a result, it is this term that creates a potential problem for the network instrument.

Recognizing this, I re-formulate the network instrument in a way that partials out this potentially "bad" variation:

$$\Delta IMM_{it} = \left[ \sum_{m=1}^{m=32} \sum_{t=t-17}^{t=t-52} BIRTHS_{mt} \right] \times \lambda_{im} + \varepsilon_{it} \tag{6}$$

In (6), for each of the thirty-two Mexican states, the time-invariant vector of migration weights to each state ($\lambda_{im}$) is multiplied by the total number of births between years $t-17$ and $t-52$. Summing over all Mexican states, for each U.S. MSA, I obtain an estimate of the number of eligible migrants in network-linked states. Finally, I formulate the births instrument ($Z_{it}^b$) by scaling this quantity by the size of a metropolitan area's population in 1980:

$$Z_{it}^b = \frac{\left[ \sum_{m=1}^{m=32} \sum_{t=t-17}^{t=t-52} BIRTHS_{mt} \times \lambda_{im} \right]}{POP_{it=1980}} \tag{7}$$

Scaling by population means that $Z_{it}^b$ can be interpreted as the number of eligible migrants per capita for each MSA. For example, $Z_{it}^b = 0.10$ for a given MSA indicates that we predict that MSA will experience decadal migration from Mexico equal to 10 percent of its 1980 population, assuming that all eligible migrants leave Mexico for the United States. Thus the instrument can be characterized as the size of each MSA's potential migrant shock.

Finally, equation (8) is used to estimate the causal effect of demographically-induced migration

[20] The vast majority of Mexican migrants are between these ages (see Hanson and McIntosh (2010). As it turns out, resulting estimates are not sensitive to changing the size of this interval.

on crime.

$$\Delta lnY_{it} = \eta + \theta \Delta I\hat{M}M_{it} + X_{it}^{`}\gamma + \phi_t + \varepsilon_{it} \tag{8}$$

In (8) $\phi_t$ represents year fixed effects which control for decadal migration shocks at the national-level. Since the Mexican share is differenced, I purge the model of between-MSA variation, making city fixed effects superfluous.[21] The coefficient on the predicted immigrant share, $\theta$, represents the effect of a one percentage point increase in a city's Mexican share on the percentage change in an MSA's crime rate.

# 5. Data

Data utilized in this research come from several different sources, each of which is described in this section. Data on state-specific births are drawn from tabulations of registered births and male-to-female birth ratios in Mexican states included in statistical almanacs produced by the Mexican government's *Instituto Nacional de Estadistica, Geografia e Informatica* (INEGI).[22] While fertility rates have fallen from approximately 7.2 children per woman in the early 1960s to approximately 2.4 children per woman today, each state exhibits a unique pattern with respect to the timing of its demographic transition (Tuiran, Partida, Mojarro and Zuniga 2002). **Table 1** characterizes the consequences of this variation for U.S. cities using Mexico's six largest sending states. While the stock of migration eligible individuals in Durango and Michoacan remained largely unchanged from 1980-2000, the stock increased in Jalisco and Guanajuanto and decreased in San Luis Potosi and especially in Zacatecas. In broad terms, the implication is that conditions were ripe for cities in

[21] In some specifications, I additionally control for interacted region × decade fixed effects which account for time-varying shocks to the crime rate at the regional level.

[22] Scanned copies of these almanacs are available on INEGI's website. This is the most granular source of annual natality data in Mexico. Several potential sources of potential measurement error are worth noting. First, it is possible that not all births are registered, despite the Mexican government's efforts to accomplish full registration. Second, some births are registered in years after the birth actually occurred. For some years, the almanacs contain a break-down of late registrations in 1-2 year intervals, but this is not always available by state of birth. Available data indicate that approximately 90 percent of births ever registered in most states are registered no more than two years late, and that over 75 percent are registered in the year of occurrence. Since my identification strategy involves aggregating births over a thirty-five year interval, these errors should have only a minimal effect on our resulting estimates.

California to experience considerable increases in migration over this time period whereas cities in Texas and the Midwest were primed for increased migration to a lesser degree.

To construct a set of weights that capture migration patterns linking Mexican sending states to U.S. destination cities, I use migrant-level data from the Mexican Migration Project (MMP), which gathers survey data on Mexican migrants to the United States and is administered jointly by Princeton University and the University of Guadalajara. The MMP surveys Mexican households in known sending regions and includes data on when migrants embarked on their first journeys to the United States, each migrant's state of birth, and the U.S. metropolitan area in which that migrant subsequently settled. Since I wish to construct weights that are pre-determined relative to the period whose migration trends we will predict, I include data only from pre-1980 self-reports. [23]

Data used to construct $\lambda_{im}$, the matrix of Mexican state-U.S. city specific time- invariant migration weights were generated from the Mexican Migration Project's migrant level file. The file contains survey data on a sample of over 7,000 individuals, each of whom migrated to the United States at least once in their lifetime. The migrants are a subset of individuals who were sampled at random within each community sampled in the dataset. Each community was sampled once and individuals who reported having migrated to the United States were asked to recall each of their prior migration experiences. Among male household heads, 23 percent reported having migrated to the United States within three years of the time of survey, with 89 percent reporting an undocumented migration spell (Hanson 2002).[24] Using data on the U.S. destination for the migrants first migration episode, I construct a matrix of weights that represent the average propensity of a migrant from a given Mexican state to migrate to each U.S. MSA in the dataset. Removing from the data individuals who migrated prior to 1980, the weights were constructed from the migration experiences of 3,981 Mexican migrants.

**Tables 2A and 2B** provide some descriptive data on these weights. Table 2A presents the three most prevalent U.S. destinations for each Mexican state with sufficient numbers of migrants

[23] I use data on the migrant's first journey to the United States rather than the last, both of which are available in the survey. The computed weights are not at all sensitive to this decision.

[24] Hanson further notes that the MMP surveys only households in which at least one member has remained in Mexico. As such, households that have entirely moved to the United States are not counted. Moreover, the migrants who are surveyed are a selected subset of migrants who have returned to Mexico, at least temporarily.

in the data. The percentage of migrants who settled in each area is given in parentheses next to the name of the metropolitan area. For example, the top two U.S. destinations for migrants from Baja California del Norte, located along the border with San Diego, CA are San Diego and Los Angeles. Likewise, the top three U.S. destinations for migrants from Nuevo Leon, a state in eastern Mexico are Houston, Dallas and McAllen, TX. While there is a fair amount of spread in the number of U.S. destinations in the dataset, the leading cities are predictably Los Angeles, Chicago, Houston, Dallas and San Diego. In Table 2B, I present data on the source regions among migrants who have settled in several of the largest MSAs in my sample. Here, we see a large amount of variation, with each MSA relying on a markedly different combination of Mexican sending states. Along with the size of birth cohorts, this data is key to the identification of exogenous variation in city-specific migrant flows.

Data on each MSA's population, its Mexican immigrant share and relevant control variables are derived from 5 percent samples of the U.S. Census accessed using IPUMS.[25] Because the IV strategy uses birth cohorts between 17-52 years ago, I focus on the share of Mexican immigrant adults among each MSA's population non-institutionalized population.[26] Relevant control variables include the educational attainment of each MSA's population (individuals without a high school degree, high school graduates, those with some college experience and those with a college degree), the age composition of the population using five age groups (0-14, 15-24, 25-39, 40-54 and 55+), gender composition, race and ethnicity (the proportion of the population that is black, U.S.-born Hispanic or non-Mexican immigrants) and the employment-to-population ratio. In addition, to address the possibility that the size of past Mexican birth cohorts is correlated with the contemporaneous number of births to Mexican-born parents in linked U.S. destinations, I include a control for changes in the lagged number of U.S. births to Mexican-born parents in each MSA.

Finally data on index crimes reported to police come from the Federal Bureau of Investigation's Uniform Crime Reporting (UCR) program and were aggregated from the agency to the MSA level

[25] While the number of Mexican immigrants are undoubtedly undercounted in these data, conditioning on fixed effects, decadal changes in the shares will only be biased if the undercounting of migrants fluctuates within MSAs over time. Moreover, in 2SLS models using the births instrument, the conditions under which measurement errors in immigration data will lead to inconsistent parameter estimates are considerably more stringent as the measurement errors would have to be correlated with the size of birth cohort shocks conditional on fixed effects and covariates.

[26] The results of the analysis are not sensitive to using including children in the estimates or including individuals living in institutions.

in order to accord with the available migration data. Crime data were available for the seven standard index crimes — murder, forcible rape, robbery, aggravated assault, burglary, larceny and motor vehicle theft. In an auxiliary analysis, MSA-level data were obtained by the National Crime Victimization Survey (NCVS) which surveys potential crime victims to learn about victimizations that were reported to police as well as those which remain unreported.

# 6. Results

*A. First Stage Estimates*

Prior to presenting substantive results, I begin with a discussion of the first stage relationship between growth in an MSA's foreign-born Mexican population share and the change in the immigrant share that is predicted on the basis of the size of lagged Mexican birth cohorts (the births instrument). The first stage results serve to provide evidence in favor of the validity of the research design as well as provide descriptive evidence of the salience of demographically-induced migration. I begin with a graphical presentation of the relationship. **Figure 1** plots the actual change in an MSA's foreign-born Mexican share (as a percentage of 1980 MSA population) against the change that is predicted using the births instrument. Estimates are presented for each decade in the data as well as for the entire dataset. Overall, the instrument has a remarkable degree of predictive power accounting for 34 percent of the overall variation in net migration over the 1980-2000 study period.

**Table 3** presents regression estimates of this relationship. Columns (1) through (5) of Table 3 report a coefficient and standard error arising from a least squares regression of the endogenous covariate, the change in the Mexican immigrant share on the births instrument, conditional on a variety of control variables.[27] Because all variables in the model are differenced, the model uses only within-city variation and is, to first approximation, equivalent albeit more flexible than the fixed effects estimator that is commonly used in the literature.

Column (1) reports estmates that condition only on a decade fixed effect which captures national variation in the foreign-born Mexican share in the 1990-2000 period relative to 1980-1990 period.

[27]Standard errors are clustered at the MSA level and are robust to within-MSA dependence.

In column (2), I add a vector of time-varying covariates which include controls for the change in the population share by gender, five age groups and four education groups as well as the change in the MSA's non-Mexican foreign-born share, black population share, U.S. born Hispanic population share, the MSA's employment-to-population ratio and the lagged number of births to U.S.-born Mexicans. In column (3), the decade fixed effect is replaced by interacted region $\times$ decade fixed effects which capture time-varying unobserved heterogeneity in the foreign-born Mexican population that is not explained by the size of Mexican birth cohorts or the control variables. These interacted fixed effects are powerful and account for, among other things the emergence of new immigrant destinations, at least at the level of the region. Finally, columns (4) and (5) report the first stage coefficients having excluded each of Los Angeles and Chicago from the sample. These models are used to assure the reader that the first stage relationship is not an artifact of a relationship between births and network-driven migration that is specific to two leading immigrant destinations. All regressions are weighted using each MSA's 1980 population.

Prior to assessing the strength of the first stage relationship, it is worth noting that the coefficients presented in Table 3 have a useful interpretation. Since both the instrument and the endogenous covariate are scaled by the citys 1980 population, the first stage coefficient can be interpreted as the estimated probability that a migration-eligible birth can be found in a network-linked U.S. destination 17-52 years later. Referring to column (3) which is the most restrictive model, between 1980-2000, approximately 6 percent percent of individuals born in Mexico in the eligible window could ultimately be found a network-linked U.S. destination. This accords with estimates in the MMP migrant data that approximately 20-25 percent of male migrants sojourn to the United States are some point during their lives (Massey 1999) as this would mean that approximately 10-20 percent of all Mexican nationals would have migrated given that migration rates are lower for women. The implication is that between 30 percent and 60 percent of migration occurs along regional networks.

Conditional on the fixed effects and the control variables, the instrument explains approximately 6 percent of the variation in the change in each MSA's Mexican immigrant share. In all of the specifications, the $F$-statistic on the excluded instrument meets standard criteria for instrument relevance which is impressive given the size of the sample ($N = 168$ MSA-years) and that models

(3)-(5) condition on region × decade fixed effects which is extraordinarily restrictive. In my preferred specification, model (3), the F-statistic on the excluded instrument, which is a sufficient statistic to characterize the sample size of the instrumental variables estimator is 12.6, exceeding the standard rule of thumb (F > 10) with the respect to instrument strength. In less restrictive models, the first stage relationship is even greater.

Columns (6)-(8) of Table 3 provide a series of falsification tests that bolster the validity of the instrumental variables strategy. In particular, I show that while the births instrument is a strong predictor of Mexican migration to network-linked U.S. destinations, it does not predict a change in migration from other source countries or internal (within U.S.) migration of U.S.-born Hispanic citizens. This is crucial to establish as an association between lagged Mexican births and either of these two variables would call into question whether the exclusion restriction on the instrument is, in practice, met. To the extent that lagged Mexican births predict migration from countries other than Mexico, the likely culprit would be an unexpected correlation between lagged births and contemporaneous conditions in U.S. destinations. Finally, in column (8) I show that while the instrument has great predictive power for adult Mexican immigrants, it does not predict a change in the share who are children. While the share of Mexican immigrant children could potentially increase as a function of the births instrument — for example if adult migrants bring their children with them to the United States or send for them shortly thereafter — a strong relationship between the births instrument and migration from outside the relevant time window would tend to call into the question the mechanism underlying the instrument.

### *B. Primary Estimates*

**Table 4** presents substantive results with respect to UCR crimes reported to police and is divided into two panels. The top panel, Panel A, presents least squares coefficients and standard errors from a regression of the natural log of the number of crimes per MSA resident on the Mexican immigrant share. Panel B reports the corresponding 2SLS estimates using the births instrument to net out exogenous variation in the change in the immigrant share. As in the first stage models, regressions are estimated using 1980 population weights and standard errors are clustered at the MSA level.

Panel A presents evidence in favor of a positive long-run association between Mexican immigration and several crime rates (murder and robbery) at the MSA level over the 1980-2000 time period. Notably, a one percentage point increase in an MSA's Mexican immigrant share (equal to approximately an 8 percent increase in the immigrant share in an average city) would have predicted a 8 percent increase in murder and a 10 percent increase in robbery. These results are interesting in that they do not accord with those of Ousey and Kubrin (2009) and Wadsworth (2010) who study U.S. cities using the same time period and find that the opposite is true. These apparently divergent results appear to be an artifact of the differences in counting immigrants between cities and MSAs. If new migrants tended to settle in the largest cities within a given MSA and if these large cities experience steeper crime declines, it is easy to that results could differ considerably depending on the level of aggregation in the data.

Overall, the pattern of the least squares results is not reflective of particularly large associations between Mexican immigrant settlement and MSA-level crime rates and accordingly the results are consistent with much of the prior research. Nevertheless, given that each of the seven coefficients is greater than zero, it is worth considering what these regressions might reflect under the assumption that there is classical (i.e., random and additive) measurement errors in estimates of an MSA's Mexican immigrant share. To the extent that such errors exist, we would expect least squares coefficients to be attenuated towards zero. Importantly, if the instrument is unrelated to the measurement errors, to the extent that the sign of the least squares coefficients is correct, we would expect to see that 2SLS estimates will be positive and larger in magnitude than the least squares estimates reported in Panel A.

Panel B presents 2SLS estimates using the births instrument. In contrast to the above argument, the results lead to a dramatically different impression than that which is conveyed in Panel A. After purging the models of variation in migration that is implausibly exogenous, immigration can be shown to have a protective effect on several crime types. In particular, a one percentage point increase in the immigrant share leads to a 15 percent reduction in rape, an 12 percent reduction in larceny and an 18 percent reduction in motor vehicle theft. The coefficient on burglary is also relative large (10 percent) and negative though it is less precisely estimated. On the other hand, the 2SLS coefficient in the aggravted assault model is positive and significant indicating that Mexican

immigration appears to increase the rate of assault. Standard errors in the 2SLS regressions are larger than those estimated via least squares by construct. However, the coefficients are nevertheless estimated with reasonable precision, given that these are decadal rather than annual changes in crime. For an MSA that received a large influx of Mexican immigrants over the course of a decade — for example, 3-5 percentage points — the results suggest that changes in the immigrant share can, in some cases be very large. Prominent examples include Dallas and Denver which saw its Mexican immigrant shares rise from 5 percent to 10 percent and 1 percent to 5 percent, respectively between 1990 and 2000. Other MSAs with especially large increases during the 1990-2000 period include Houston, Los Angeles-Long Beach, Phoenix and Riverside-San Bernardino, CA.

Overall,property crime declined by approximately 29 percent in the United States between 1990 and 2000. Given that the estimated share of Mexican immigrants in the U.S. population increased from approximately 1.7 percent in 1990 to 3.5 percent in 2000, holding other factors constant, point estimates derived from the 2SLS procedure imply that Mexican immigration may explain over half of the decline in property crimes over this time period. While these results may sound implausible, to the extent that other factors may have been driving crime upwards over this time period, these estimates are best thought of as upper bounds rather than an unbiased point estimate with respect to the protective role of Mexican immigration.

### *C. Local Average Treatment Effects*

Overall, evidence from the 2SLS models suggests that Mexican migration has driven down property crimes and rape while increasing the rate of aggravated assault. However, these point estimates can only be given a policy-relevant interpretation to the extent that the local average treatment effect upon which the instrument is based is broadly reflective of the change in overall Mexican immigration.[28] In particular, if demographically-induced migration appears to change

[28]The importance of local average treatment effects for interpreting IV regressions is introduced in an important paper by Angrist and Imbens (1994). Briefly, the treatment effect in an instrumental variables regression is based upon the proportion of the sample who are "compliers" with the treatment. In the case of a randomized controlled trial, the compliers are those individuals who are assigned to treatment and choose to take the treatment on that basis. On the other hand, the outcomes of individuals whose behavior is invariant to treatment assignment ("always takers" and "never takers") are not relevant to interpreting an IV estimate. In the present context, the compliers are U.S. MSAs that receive an increase in immigration as a result of a shock to lagged births in network-linked Mexican states. While the complying MSAs cannot be directly identified, one can nevertheless consider whether the instrument is especially important in inducing certain subpopulations to migrate than others.

the composition of an MSA's Mexican population to a greater degree than a general increase in migration, resulting changes in the crime rate can best be understood as compositional as opposed to being reflective of a more general statement about the criminality of migrants. A less optimistic characterization of such a pattern in the data is that the 2SLS models are low in external validity and, as such, are not useful.

An especially salient consideration is the extent to which the size of birth cohorts are especially predictive of the migration of young males, aged 15-24. As such individuals are overrepresented among U.S. arrestees by a factor of 6 to 7, if births tend to reduce the share of the Mexican immigrant population that is young and male, crime might decline more or less mechanically as a function of immigration. Indeed compositional effects have been shown to be important in past research on immigration and crime, with Moehling and Piehl (2009) reporting on compositional effects in a historical setting and Chalfin (2013b) providing evidence in a contemporary application. While this would be an important finding with respect to understanding the short-run effect of immigration on crime, such a finding does not inform a more dynamic understanding of the longer-term impacts of immigration.

**Table 5** reports evidence on the compositional implications of demographically-induced Mexican migration. The top panel of the table presents estimates from a least squares regression of the decadal change in the share of Mexican immigrants who meet a particular criterion on the decadal change in the foreign-born Mexican population share. For ease of interpretation, the change share of Mexican migrants is expressed as a $z$-score. These regressions measure the degree to which overall changes in Mexican immigration are associated with changes in the characteristics of the community of Mexican migrants living in the United States. Referring to column (1), a one standard deviation change in an MSA's Mexican immigrant share (equal to an approximate 1.5 percentage point increase in a typical city) predicts a corresponding decrease in the share of migrants who are male of 1.7 percentage points (i.e., from 55 percent to 53.3 percent), though the result is not precisely estimated. Overall, during the 1980-2000 period, increases in immigration tended to favor high school graduates at the expense of the least skilled workers, a long-term trend that is reported by Borjas (1994) and Card and Lewis (2007) among others. The evidence also suggests that increases in immigration to a particular MSA tend to be associated with a slight aging of the

population though the effect is small in magnitude.

The bottom panel of Table 5 reports estimates from a least squares regression of the decadal change in each population subgroup's share of the Mexican immigration population on the change in the births instrument, also expressed as a z-score. To the extent that demographically-induced migration changes the composition of the Mexican immigrant population to a greater extent than immigration generally, such results can be seen by comparing these results to those estimated in the top panel of Table 5.

Overall, the births instrument is associated with many of the same demographic implications as a more general increase in immigration. There is evidence of an aging of migrants as a function of demographically-induced migration though again, the effects are small as a one standard deviation increase in such migration predicts only a one percent decline in the population share that is aged 0-14 and a one percent increase in the population share that is over the age of 55. As in the top panel of the table, there is evidence that migration is associated with a shift from the least skilled workers to those with a high school education. Most importantly, there is no evidence that migration generally or demographically-induced migration changes the share of the population that is male or is of peak offending age. Accordingly the births instrument does not appear to induce a particularly unusual local average treatment effect. Put differently, there does not appear to be anything different about demographically-driven migration that is motivated by more traditional reasons. This argues in favor of the external validity of the research design.

### *D. Crime Reporting Rates*

One of the major difficulties in interpreting regression estimates in the extant literature, whether they are derived using neighborhood-level or macro-level data, has to do with the possibility that immigration might reduce crimes reported to police mechanically if immigrants are less likely to report crimes to police than natives. Such a proposition accords with common sense given the possibility that law enforcement may learn that a crime victim is an immigration violator and is given empirical traction by a recent survey research by Davis and Henderson (2003) and Kirk, Papachristos, Fagan and Tyler (2012) who find evidence that immigrants are less willing to commit crimes than natives. If immigrants are, in fact, less likely to report crimes to police than natives,

other things equal, a negative correlation between the immigrant share and the crime rate will hold in the data, even if there is no relationship between crime and the immigrant share. To my knowledge, this is a problem which remains unresolved in the literature. Typically the solution to this problem in literature has been to study the effect of immigration on lethal violence which is "reported" with great accuracy relative to other crimes. While the study of lethal violence is important for obvious reasons, such analyses do not allow us to understand the effect of immigration on more common types of offending.

I thus provide an auxilliary analysis using data from the National Crime Victimization Survey aggregated to the MSA-level to test whether changes in crime reporting rates are associated with changes in immigration. In particular, using data on 27 of the largest U.S. MSAs over the 1980-2000 period, I show that decadal changes in the rate at which victims indicate that they report crimes to police is invariant to decadal changes in the immigrant share in an MSA. Conditional on the full set of controls used in the primary results reported in Table 4, the coefficient on the change in the immigrant share in this regression is -0.007 and is imprecisely estimated. The implication is that a one percentage point increase in the immigrant share is associated with less than a one percent reduction in the rate at which victims report crimes to the police.[29]

This result is suggestive of nothing more than a weak relationship between immigration and crime reporting and makes sense given that decadal changes in the immigrant share tend to be relatively small. However the result is subject to several important limitations. First, a small coefficient does not necessarily imply that immigrants are less likely to report crimes to police than natives — indeed it could be that natives are more likely to report a crime when they are victimized by an an individual they believe might be an immigrant — and that this washes out our ability to observe the true association. Nevertheless this is the relevant analysis with respect to understanding the scope for differential reporting rates to bias regression-based estimates in the literature. To see this, consider that the probability that a crime is reported to the police in a given MSA-year is a variable that is typically omitted from a crime regression. To the extent that

[29]The resulting coefficient from a 2SLS regression of the change in the reporting rate on the instrumented change in the immigrant share is a little bit larger at -0.044. The result is not significant at conventional confidence levels. Nevertheless, even taking the magnitude of the coefficient at face value, there is little evidence that immigrants' reduced willingness to report a crime biases coefficients in regressions of crime on the immigrant share.

this variable is related to the immigrant share and to the crime rate, it will lead to an inconsistent coefficient on the immigrant share. However, if the reporting rate is related to the crime rate but not to the immigrant share, the coefficient on the immigrant share will be unaffected by changes in crime reporting.

A second limitation arises from the relatively sparse MSA-level data in the NCVS. Overall, among the 27 MSAs with consistent jurisdictional boundaries over the 1980-2000 period, there are an average of 2,359 respondents who report 259 criminal victimizations, 96 of which were reported to the police. In some MSA-years the number of respondents is as few as 600 and the number of reported crimes is as few as 40. Hence, I am unable to provide crime-specific estimates of the immigration-reporting rate relationship. Nevertheless, given that most crimes by NCVS respondents are property crimes which tend to be among the poorest measured crimes in the UCR data, the overall reporting rate is an excellent proxy for the rate of underreporting for those crime types we are most concerned about.

## 7. Conclusion

This paper uses a novel identification strategy to provide new estimates of the long-run effect of Mexican immigration on crime in U.S. metropolitan areas. The paper makes four primary contributions. First, by developing an instrumental variables framework to isolate plausibly exogenous variation in Mexican immigration, I am able to provide more credible estimates of the effect of immigration on crime than those that are currently available in the literature. In particular, my strategy plausibly addresses problems posed by omitted variables, simultaneity bias between immigration and crime and measurement errors in immigration data. Second, I provide evidence that migration, on the whole, does not appreciably change the composition of the immigrant population and, as such, national estimates of the effect of immigration on crime reflect more than compositional effects. Third, by studying Mexican immigrants, a population which is, to a large degree, undocumented, I provide evidence on a particular sub-group of immigrants who have been most prominent in recent political debates. Finally, by providing an auxiliary analysis of the effect of immigration on reporting rates, I am able provide suggestive evidence that differential reporting

rates are not a fatal threat to macro- or neighborhood-level analyses. Overall, the evidence suggests that immigration may have led to substantial declines in property crimes and forcible rape and an increase in aggravated assault.

With respect to future macro-level research, the most urgent priority is to discern between immigrant offenders and immigrant victims. Until this question can be addressed, positive associations between immigration and crime are subject to two alternative and equally plausible explanations. For my part, I identify a negative effect of immigration on three crime types. Under the hypothesis of higher immigrant victimization, such estimates are, if anything, conservative and the available evidence does not suggest that such large effects are an artifact of differential crime reporting by immigrants. As such, I interpret my findings to indicate that there is considerable evidence that immigration is protective of at least some serious crimes.

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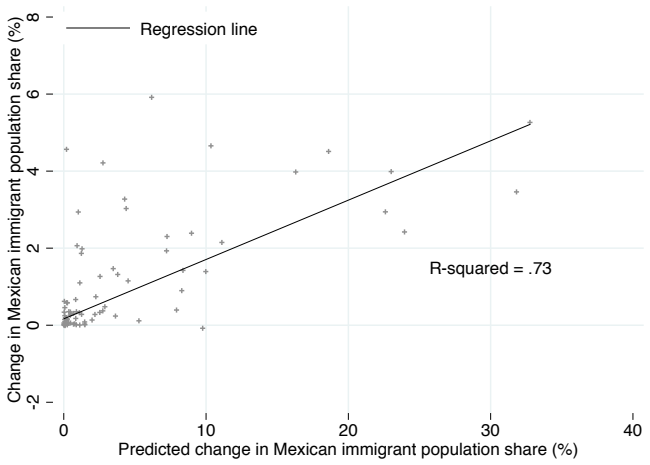
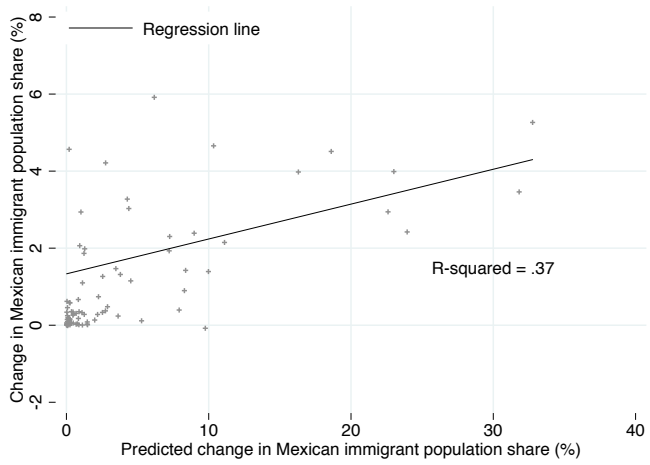
Figure 1. Predicted vs. Actual Decadal Changes in Mexican Immigration by MSA

A. 1980-1990




B. 1990-2000




B. 1980-2000

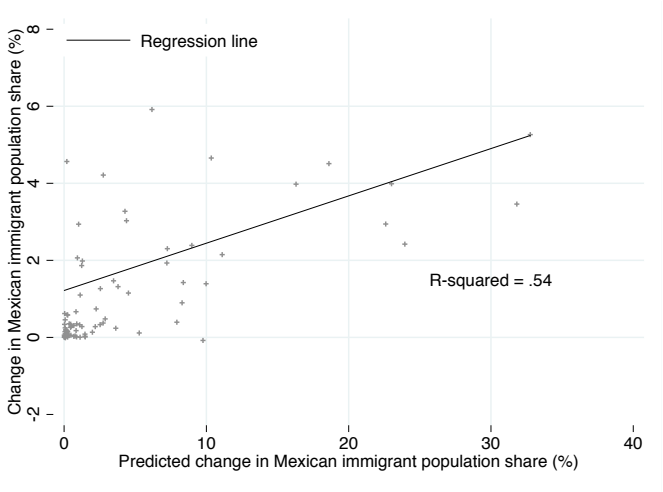


Table 1. Migration-Eligible Births for Six Leading Mexican States of Origin, 1980-2000

| Year | State | Number of Migration-Eligible Births (thousands) | Change in Migration-Eligible Births (thousands) |
|---|---|---|---|
| 1980 | Durango | 526.2 | |
| 1990 | | 642.1 | 115.9 |
| 2000 | | 747.1 | 105.0 |
| 1980 | Guanajuanto | 1235.6 | |
| 1990 | | 1488.1 | 252.5 |
| 2000 | | 1753.6 | 265.5 |
| 1980 | Jalisco | 1451.0 | |
| 1990 | | 1903.3 | 452.3 |
| 2000 | | 2397.3 | 494.1 |
| 1980 | Michoacan | 1200.6 | |
| 1990 | | 1536.6 | 336.0 |
| 2000 | | 1880.4 | 343.8 |
| 1980 | San Luis Potosi | 739.8 | |
| 1990 | | 905.9 | 166.1 |
| 2000 | | 1048.4 | 142.5 |
| 1980 | Zacatecas | 623.0 | |
| 1990 | | 741.3 | 118.4 |
| 2000 | | 800.2 | 58.8 |

Note: The table reports both the number of migration-eligible births and the change in the number of migration-eligible births by decade for the six most prominent states of origin among Mexican migrants to the United States.

Table 2A. U.S. Destinations of Mexican Immigrants

| Mexican State | Destination #1 | Destination #2 | Destination #3 |
|---|---|---|---|
| Aguascaliente | Los Angeles (20%) | Reno (6%) | Tulsa (6%) |
| Baja California del Notre | San Diego (60%) | Los Angeles (22%) | |
| Baja California del Sur | | | |
| Campeche | | | |
| Coahuila de Zaragoz | | | |
| Colima | Los Angeles (41%) | Fresno (9%) | |
| Chiapas | | | |
| Chihuahua | El Paso (16%) | Los Angeles (9%) | Dallas/Phoenix (9%) |
| Districto Federal | Los Angeles (20%) | Chicago (11%) | Orange County (CA) (8%) |
| Durango | Chicago (23%) | Los Angeles (19%) | Dallas (7%) |
| Guanajuanto | Los Angeles (15%) | Chicago (11%) | Houston (7%) |
| Guerrero | Chicago (29%) | Los Angeles (15%) | Phoenix (12%) |
| Hidalgo | Las Vegas (12%) | Dallas (9%) | Houston (7%) |
| Jalisco | Los Angeles (26%) | San Diego (6%) | San Jose (4%) |
| Mexico (Estado) | Chicago (32%) | Stockton (10%) | los Angeles (7%) |
| Michoacan | Los Angeles (20%) | Fresno (8%) | Chicago 96%) |
| Morelos | Los Angeles (29%) | Minneapolis (18%) | Chicago (10%) |
| Navarit | Los Angeles (29%) | San Jose (10%) | Orange County (CA) (7%) |
| Nuevo Leon | Houston (16%) | McAllen (15%) | Dallas (11%) |
| Oaxaca | Los Angeles (51%) | San Diego (9%) | |
| Puebla | New York (56%) | Los Angeles (23%) | |
| Queratarо | | | |
| Quintana Roo | | | |
| San Luis Potosi | Houston (16%) | San Diego (16%) | Dallas (6%) |
| Sinaloa | Los Angeles (48%) | San Diego (10%) | Riverside (8%) |
| Sonora | | | |
| Tamaulipas | | | |
| Tabasco | | | |
| Tiaxcala | Los Angeles (9%) | | |
| Veracruz | Los Angeles (14%) | Chicago (13%) | San Jose (8%) |
| Yucatan | Portland (31%) | San Francisco (29%) | Los Angeles (11%) |
| Zacatecas | Los Angeles (28%) | Fresno (5%) | Merced (5%) |

Note: The table reports the three largest U.S. metropolitan area destinations for migrants from each Mexican state, among migrants in the Mexican Migration Project's Migrant File, 1921-1979.

Table 2B. Mexican State Sources of U.S.-Bound Immigrants
Selected U.S. Metropolitan Areas

| U.S. MSA | Source #1 | Source #2 | Source #3 |
|---|---|---|---|
| Atlanta | Jalisco (23%) | Nuevo Leon (12%) | Veracruz (11%) |
| Austin-San Marcos | San Luis Potosi (33%) | Veracruz (26%) | Guerrero (21%) |
| Chicago | Durango (30%) | Jalisco (25%) | Guanajuanto (19%) |
| Dallas | Guanajuanto (28%) | Durango (26%) | Jalisco (11%) |
| Denver | Yucatan (58%) | Chihuahua (14%) | Districto Federal (7%) |
| El Paso | Chihuahua (64%) | Zacatecas (9%) | Veracruz (5%) |
| Fresno | Jalisco (44%) | Michoacan (15%) | Guanajuanto (14%) |
| Houston | San Luis Potosi (50%) | Guanajuanto (15%) | Michoacan (7%) |
| Las Vegas | Jalisco (43%) | Nayarit (14%) | Districto Federal (13%) |
| Los Angeles-Long Beach | Jalisco (23%) | Michoacan (10%) | Guanajuanto (9%) |
| Merced | Nayarit (43%) | Jalisco (23%) | Michoacan (18%) |
| Minneapolis-St. Paul | Morelos (100%) | | |
| New York | Puebla (56%) | Morelos (22%) | Tlaxcala (5%) |
| Oakland | Jalisco (58%) | Michoacan (36%) | Districto Federal (2%) |
| Orange County (CA) | Jalisco (25%) | Guerrero (20%) | Guanajuanto (13%) |
| Philadelphia | Guanajuanto (91%) | Districto Federal (4%) | |
| Phoenix | Chihuahua (30%) | Guanajuanto (16%) | Durango (12%) |
| Portland | Yucatan (91%) | | |
| Riverside-San Bernardino | Michoacan (22%) | Jalisco (20%) | Yucatan (9%) |
| San Diego | Baja California del Norte (61%) | San Luis Potosi (16%) | Jalisco (7%) |
| San Francisco | Yucatan (54%) | Jalisco (13%) | Nayarit (10%) |

Note: The table reports the three most prevalent source regions among Mexican immigrants to selected U.S. metroplitan areas. These data are based upon the experiences of migrants surveyed in the Mexican Migration Project's Migrant File, 1921-1979.

Table 3. First Stage Models
Proportion of Migration-Eligible Births in
Network-Linked U.S. Metropolitan Statistical Areas

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Foreign-Born Mexican Adults | | | | | Other Sub-Groups | | |
| | | | | | | Other Foreign-Born | U.S.-Born Hispanics | Mexican-Born Children |
| Births instrument | 0.168*** | 0.080*** | 0.064*** | 0.062*** | 0.053*** | -0.033 | 0.036 | 0.000 |
| | (0.018) | (0.021) | (0.018) | (0.018) | (0.018) | (0.029) | (0.022) | (0.005) |
| $F$-statistic | 90.3*** | 14.6*** | 12.6*** | 11.6*** | 8.7*** | 1.3 | 2.7 | 0.0 |
| Partial $R^2$ | 0.52 | 0.05 | 0.06 | 0.03 | 0.02 | 0.00 | 0.01 | 0.00 |
| Year effects | yes | yes | yes | yes | yes | yes | yes | yes |
| Covariates | no | yes | yes | yes | yes | yes | yes | yes |
| Region × year effects | no | no | yes | yes | yes | yes | yes | yes |
| Los Angeles excluded | no | no | no | yes | no | no | no | no |
| Chicago excluded | no | no | no | no | yes | no | no | no |

Note: Columns (1) through (5) report estimates from a least squares regression of the decadal change in an MSA's foreign-born Mexican population share on the decadal change in the predicted foreign-born Mexican share using the number of eligible lagged births in network-linked Mexican states. Columns (1) and (2) condition only on a year fixed effect. Column (2) adds a vector of covariates while Column (3) adds interaction region × year fixed effects. Columns (4) and (5) remove from the panel, Los Angeles and Chicago, respectively. Columns (6) through (8) test whether lagged Mexican births predict the decadal change in foreign-born from countries other than Mexico (column 6), U.S.-born Hispanics (column 7) and Mexican-born children who are too young to have reached prime migration age. All models are weighted according using 1980 MSA population and cluster-robust standard errors are reported in parentheses below the coefficient estimates. The $F$-statistic is the square of the t-statistic on the birth cohort instrument and is a sufficient statistic to assess the strength of the first stage relationship between Mexican immigration and the birth cohort instrument. The partial $R^2$ is the percentage of the variation in MSA-specific migration that is explained by the births instrument, conditional on the covariates in the model. The sample size is 184 city-years. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1

Table 4. Least Squares and 2SLS Estimates of The Effect of Mexican Immigration on Crimes Reported to Police

| (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|
| Violent Crimes | | | | Property Crimes | | |
| Murder | Rape | Robbery | Aggravated Assault | Burglary | Larceny | Motor Vehicle Theft |
| A. Least Squares Estimates | | | | | | |
| 0.075*** | 0.002 | 0.095*** | 0.037 | 0.025 | 0.000 | 0.042 |
| (0.028) | (0.029) | (0.025) | (0.029) | (0.019) | (0.023) | (0.028) |
| B. 2SLS Estimates [Births Instrument] | | | | | | |
| -0.022 | -0.131** | 0.049 | 0.197*** | -0.092 | -0.105** | -0.149* |
| (0.094) | (0.062) | (0.094) | (0.075) | (0.057) | (0.047) | (0.0081) |

Note: In Panel A, each column reports estimates from a least squares regression of the decadal change log crimes per capita on the decadal change in the foreign-born Mexican population share. Panel B reports 2SLS estimates where the birth cohort measure is used as an instrument for decadal change in the foreign-born Mexican population. All models condition on a set of control variables capturing demographic changes in each MSA as well as interacted region $\times$ year fixed effects. All models are weighted by 1980 MSA population. I report cluster-robust standard errors in parentheses below the coefficient estimates. Statistical significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5. Evidence on the Local Average Treatment Effect: Reduced Form Effect of the Birth Cohort Instrument on the Demographic Composition of the Foreign-Born Mexican Population

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Gender | Age | | | | | Education | | | |
| | | 0-14 | 15-24 | 25-39 | 40-54 | 55+ | < HS | HS | Some College | College+ |
| Foreign-born Mexican share | -1.72 | -0.79 | -0.86 | -1.84* | 1.31 | 2.17* | -2.39 | 1.93** | 0.88 | -0.00 |
| | (1.05) | (0.75) | (1.06) | (1.09) | (1.08) | (1.11) | (1.60) | (0.95) | (0.71) | (0.68) |
| Births instrument | 0.13 | -1.33** | -1.00 | 0.09 | 0.86 | 1.37** | -1.93** | 2.22*** | 0.21 | -0.10 |
| | (0.61) | (0.55) | (0.82) | (0.58) | (0.57) | (0.61) | (0.77) | (0.62) | (0.44) | (0.39) |

Note: Each column reports estimates from a least squares regression of the decadal change in the share of an MSA's foreign-born Mexican population that has a given attribute on the decadal change in the births instrument. Here, the births instrument is entered as a $z$-score to facilitate a more natural interpretation of the regression coefficients. Column (1) presents estimates for (male) gender, columns (2)-(6) present estimates for age and columns (7)-(10) present estimates for education. For a given column, the coefficient refers to the effect of a one standard deviation increase in the births instrument on the share of the forign-born Mexican population that shares a given attribute. For example, referring to column (1), a one standard deviation increase in the births instrument predicts a 0.13 percentage point increase in the share of the foreign-born Mexican population that is male. All models are weighted according using 1980 MSA population and cluster-robust standard errors are reported in parentheses below the coefficient estimates. The sample size is 184 city-years. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1