What is the Contribution of Mexican Immigration to U.S. Crime Rates? Evidence from Rainfall Shocks in Mexico

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This paper identifies a causal effect of Mexican immigration on crime using an instrument that leverages temporal variation in rainfall in different regions in Mexico as well as persistence in regional Mexico–U.S. migration networks. The intuition behind the instrument is that deviations in Mexican weather patterns isolate quasi-random variation in the assignment of Mexican immigrants to U.S. cities. My findings indicate that Mexican immigration is associated with no appreciable change in the rates of either violent or property crimes in U.S. cities. (*JEL*: J15, K42, R10)

I am extremely grateful for the guidance and support of my principal advisors, Steve Raphael and Justin McCrary. I have also benefitted from conversations with and would like to gratefully acknowledge helpful comments from the following individuals: Shawn Bushway, Rachel Croson, Benjamin Hansen, Rucker Johnson, Ron Lee, John MacDonald, Jesse Rothstein, Hosung Sohn, Geno Smolensky, and Sarah Tahamont as well as three anonymous reviewers and seminar participants at UC Berkeley, UCLA, the University of Texas at Dallas, and the 2011 APPAM Fall Conference. I would especially like to thank Morris Levy who read several earlier drafts of the manuscript and provided incisive criticisms. Naturally, any remaining errors are my own. This research was generously supported by funding from the National Science Foundation's IGERT Traineeship Program in Politics, Economics and Public Policy at UC Berkeley. Please address correspondence to: Aaron Chalfin, School of Criminal Justice, 665S Dyer Hall, University of Cincinnati, Cincinnati, OH 45221.

American Law and Economics Review
doi:10.1093/aler/aht019
Advance Access publication October 11, 2013
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1. Introduction

Since 1980, the share of the U.S. population that is foreign born has doubled, rising from just over 6% in 1980 to over 12% in 2010. Compounding this demographic shift, the share of the foreign born population that is of Mexican origin also doubled, leading to a quadrupling of the fraction of U.S. residents who are immigrants from Mexico.[1] Over the same time period, crime rates in cities across the United States have declined considerably, in many cases, reaching historic lows. While the aggregate time series suggests that increases in immigration from Mexico has had a protective effect on crime, public opinion has generally reached the opposite conclusion, with a majority of U.S. natives indicating a belief that immigration is associated with increases in criminal activity (Espenshade and Calhoun, 1993).

Theory offers little guidance in sorting out the effect of either immigration generally or Mexican immigration specifically on crime. On the one hand, immigrants possess demographic characteristics which, in the general population, appear to be positively associated with crime. In particular, they are more likely to be young and male and have lower earnings than other U.S. residents. Likewise, immigrants may have less attachment to the communities in which they live and have different risk profiles than natives. On the other hand, there is a multitude of evidence that immigrants are positively selected on the basis of ability (see Duncan and Trejo, 2013), may face higher costs of punishment since they are subject to deportation, and may bring with them abundant social capital that is protective of participation in crime. Meanwhile, the consensus in the empirical literature is that immigrants to the United States are, at worst, no more likely to participate in criminal activity than U.S. natives and, at best, may be far less likely to participate in crime (Butcher and Piehl, 1998a, 1998b; Reid et al., 2005; Butcher and Piehl, 2009; Moehling and Piehl, 2009; Stowell et al., 2009; Wadsworth, 2010).[2]

1. As recently as 1970, the share of Mexican immigrants in the United States was only 1.5% (Hanson and McIntosh, 2010).

2. See Buonanno et al. (2011) for similar research in a sample of Italian municipalities. A recent exception can be found in Spenkuch (2013) who studies the effect of recent immigration, disaggregating by nationality, at the county level and finds that Mexican immigration is associated with increases in crime, particularly property crimes.

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While recent empirical work suggests an answer to the conundrum, the literature remains unsatisfying in several ways. First, while a number of papers address the relationship between immigration and crime generally, there is little research that addresses the criminal participation of recent Mexican immigrants. Since these are the immigrants who have become such a salient issue in recent policy debates, disaggregating the effects by nationality would appear to be an issue that is of first-order importance. Secondly, the majority of the literature identifies an effect of immigration on crime using long differences, generally employing decennial Census data. While this strategy plausibly addresses the problem of measurement errors in immigration data, such analyzes are subject to concerns regarding internal migration of U.S. natives in response to immigration or other changes in local conditions (Borjas, 2003, 2006).

Finally, while prior research has employed a variety of identification strategies, chief among them the use of ethnic enclaves (or immigrant networks) as an instrumental variable, concerns regarding the internal validity of this strategy motivates further investigation. Research that has used immigrant networks to identify the effect of immigration on crime includes Butcher and Piehl (1998b) and Spenkuch (2013). While Butcher and Piehl (1998b) find no evidence of a relationship between immigration and crime at the city level using data from the 1980s, Spenkuch (2013) leverages the network instrument to estimate the effect of immigration on crime using recent county-level data and finds evidence of large, positive associations between immigration, particularly Mexican immigration and crimes with a pecuniary motive. In this research I note that instrumental variables that leverage the existence of immigrant networks identify an effect of immigration on crime, in large part, using city-specific factors that pull migrants into a given city. To the extent that factors which historically pull migrants to particular destinations are correlated with the evolution of crime markets in those destinations, the exclusion restriction necessary to justify the instrumental variable will not be met.

The dual issues of internal migration by U.S. natives and the exogeneity of either immigrant flows or network-based instruments have led to concerns regarding the reliability of estimates of the effects of immigration in several related literatures (Borjas, 2003, 2006). To address these concerns, I introduce a novel source of identifying variation in constructing an

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instrumental variable for the cross-city stock of immigrants in the United States. Specifically, I follow the general approach of Pugatch and Yang (2011) and construct an instrument that combines data on the permanent (long run) component of Mexican state–U.S. city migration relations with data on annual rainfall shocks in different Mexican states. The intuition behind the instrument is that deviations in Mexican weather patterns isolate quasi-random variation in the assignment of Mexican immigrants to U.S. cities. Indeed, I find strong evidence that Mexican immigration to the United States is responsive to Mexican rainfall. By leveraging a factor—rainfall—that pushes migrants out of Mexico rather than relying exclusively on city-specific factors that pull immigrants to particular U.S. destinations, my identification strategy is more likely to meet the exclusion restriction needed to estimate a causal effect. To wit, the exclusion restriction is satisfied so long as rainfall in Mexico does not directly affect crime in the United States. Moreover, by using annual rather than decadal data, concerns regarding internal migration of U.S. natives are made considerably less salient.

My findings indicate that, on net, Mexican immigration is associated with no appreciable change in the rates of either violent or property crimes in U.S. cities. Notably, this is a precisely estimated null effect as I can reject that a one percentage point increase in the rainfall-induced share of Mexican migrants leads to greater than a 1% increase in violent crimes or a 1.5% increase in property crimes. Finally, though I do find evidence that an increase in the share of Mexican migrants leads to a modest increase in per capita robberies, the result is sensitive to the inclusion of Los Angeles, underscoring the enormous heterogeneity in the treatment effect as well as the difficulty in identifying a "national effect" of Mexican immigration.

The remainder of the paper is organized as follows. Section 2 provides a discussion of identification problems in this literature as well as a brief literature review. Section 3 provides a discussion of mechanisms underlying the decision to migrate. Section 4 presents the econometric framework used to estimate an average causal response of crime to immigration and includes a discussion of the identifying assumptions of the model. Section 5 describes the data and sample. Section 6 presents the empirical results and includes a discussion that links the results to those estimated in the prior literature. Section 7 concludes.

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2. Conceptual Background

2.1. Empirical Challenges

Findings in the extant literature arise from two strains of research that attempt to identify the criminal participation of the foreign-born. The first examines the demographic characteristics of institutionalized populations and finds that recent immigrants are substantially underrepresented among those individuals who reside in an institutionalized setting at the time of the decennial census. In particular, Butcher and Piehl (1998a) find that the foreign-born are approximately five times less likely to be institutionalized than natives, further demonstrating that this figure is unlikely to be driven substantially by selective deportation.[3]

A second strain of research exploits cross-city variation in the stocks and flows of the foreign born and reports associations between changes in the size of a city's immigrant population and its crime rate. This research design offers a key advantage in that the researcher is able to observe associations between immigration and crime that are not contingent on an assumption of equal apprehension or adjudication probabilities among immigrants and natives.[4] However, to achieve identification, the design necessarily

3. 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. Moreover, it is important to note that such analyzes 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, since it is not possible to disaggregate the incarcerated from the otherwise institutionalized using recent data, the validity of the resulting estimates requires an assumption that immigrants and natives have the same relative propensities to be incarcerated conditional upon institutionalization. Secondly, the institutionalized population, by definition, includes only those individuals who were apprehended, arrested, and subsequently incarcerated for a crime, a potentially highly selected sample of foreign-born offenders. Finally, 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 substitute for an empirical estimate of the effect of immigration on crime derived from aggregate data.

4. These area studies are also able to capture the "general equilibrium" effects of immigration insofar as these designs capture changes in the behavior of natives that arise

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relies on the exogeneity of immigrant location decisions. To the extent that immigrants endogenously select destination cities either according to city-specific crime rates or according to other unobserved city and time-varying amenities that are themselves correlated with crime, the treatment effect uncovered using natural variation in immigrant flows will be biased.[5]

2.2. The "Network" Instrument

The standard solution to this problem in the immigration literature 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, an instrument pioneered by Altonji and Card (1991) in their seminal treatment of the cross-city effect of immigration on the wages and employment of natives. The approach relies on the empirical observation that immigrants tend to cluster in cities where prior immigrants from their country of origin have previously settled. Thus, the network instrument achieves identification by attempting to isolate exogenous variation in factors that *pull* immigrants to particular locations.[6] Formally, the network instrument is written as follows:

$$Z_{it} = \sum_{c=1}^{n} \mathrm{MIG}_{ct} \times P_{ic} \tag{1}$$

In (1), MIG_{ct} is the number of immigrants from country c who are living in the United States in year t and P_{ic} is a matrix of source region-U.S. destination weights that return the conditional probability of migration from each source region c to each U.S. city i. The network instrument Z_{it} is the interaction of these two terms, summed over the n source regions. Card (2001) and Card and Lewis (2007) have used this instrument to estimate a causal effect of immigration on the employment outcomes of U.S. natives,

as a result of immigration. The cost is that the treatment effect that is captured by such designs may not isolate the criminality of immigrants themselves.

5. This is essentially the model estimated by Stowell et al. (2009). Wadsworth (2010) pursues a similar approach, differencing (1) to remove the fixed effects.

6. In a recent working paper, Chalfin and Levy (2012), argue that the "network" instrument can be decomposed into a component that is explained by the size of foreign birth cohorts and a component that is captured by the conditional probability of migration for each (lagged) birth. The authors argue that the former term captures plausibly exogenous variation while the latter term, in part, captures "pull" variation to U.S. destinations.

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while Saiz (2003) has used the instrument to estimate the effect of immigration on various aspects of urban housing markets. 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. This basic finding is echoed in least squares estimates of U.S. city panel data in Stowell et al. (2009) and Wadsworth (2010) and, with the exception of robbery, in a recent study of immigrants in Italy (Buonanno et al., 2011). An exception is a recent working paper by Spenkuch (2013) which 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.

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. Unfortunately, there are several mechanisms through which the prior location decisions of migrants might influence 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 certain characteristics in 1960, to the extent that these characteristics persist, today's migrants may be pulled to a city for similar reasons. Secondly, as noted by Card (2001) and Pugatch and Yang (2011), 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.[7] Instruments that rely on exogenous variation in factors that *pull* immigrants to a given city are inevitably problematic in that they rely on the

7. It is also possible that increases in the stock of immigrants within a city lead to emigration of U.S. natives. While Card (2001) finds no evidence that this is the case, it

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presumably endogenous location decisions of prior immigrants or a lack of persistence in the characteristics of cities over time. In their recent study of the effect of immigration on the employment rates and wages of U.S. natives, Pugatch and Yang (2011) recognize this fact and propose that a cleaner source of identifying variation may be found in factors that induce migration from source countries. They argue that *push* factors (those factors that differentially induce migration from different source regions) are less likely to be systematically related to economic (or other) variables in the United States.

Another way to describe how the network instrument can fail to isolate exogenous variation in immigration flows is to consider that the network instrument can be decomposed into two components: (1) the available supply of Mexicans who are eligible to migrate to the United States and (2) the conditional probability of migration in a given year. To see this, consider that, in a given year t, there is some number of Mexicans (N) who are available to migrate to the United States. We see that N 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 among each cohort in N. The number of Mexicans who actually migrate to the United States in a given year is $N \times p_t$, where p_t is the conditional probability of migration to the United States in year t. Whereas N 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. 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 into the network instrument.[8] Recognizing this, we would like to find a proxy for p_t which is not a function of conditions in the U.S. gateway cities.

A natural candidate to isolate "push" variation in immigration flows employs variation in weather. Weather variation, specifically rainfall, has been used as an instrument for internal migration in Indonesia (Kleemans

may be the case that the composition of natives changes in the long run, in response to immigration.

8. The bias is negative to the extent that positive wage growth is, other things being equal, associated with a reduction in crime.

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and Magruder, 2011). With respect to Mexico, rainfall has been used to predict migration by Munshi (2003) and Pugatch and Yang (2011). As an instrument for the Mexican share of the U.S. labor force in a given state, Pugatch and Yang use deviations in rainfall from the long run mean in the Mexican states from which migrants to that U.S. state have historically originated. The intuition is that rainfall affects economic conditions in Mexico, which, in turn, alters propensities for affected Mexicans to migrate to the U.S. To the extent that there is persistence in Mexican state–U.S. state migration channels, migration to a given U.S. state can be thought of as being induced quasi-randomly by rainfall in a particular Mexican sending state. In order to link migration from a given Mexican state to a given U.S. state, Pugatch and Yang construct measures of regional migration patterns that developed over time in response to the construction of early 20th century railroads. The authors note that a number of studies (such as Cardoso, 1980; Massey et al., 2002; Woodruff and Zenteno, 2007) have documented the emergence of migration patterns between Mexican and U.S. regions connected by railroads at the beginning of the 20th century, as U.S. employers would travel by rail to Mexico and return with recruited laborers. Next, using data on migrants passing through three different border crossings collected by Forrester (1925), the authors construct a set of weights reflecting the probability that a migrant from a given Mexican state migrates to a given U.S. region.

Building on the approach of Pugatch and Yang, I construct a push instrument for immigration that links weather shocks in Mexico to long-term migration patterns between Mexico and the United States. However, in a key departure from their approach, I exploit microdata on migrants collected by the Mexican Migration Project (MMP) at Princeton University to develop estimates of the permanent component of migration from a given Mexican state to each of forty-six large U.S. metropolitan statistical areas. These data offer two important advantages over the cross-sectional data from border crossings employed by Pugatch and Yang. First, I am able to observe actual long-run migration patterns from each Mexican state to each U.S. city, rather than relying on a single cross section of migrants entering through border crossings in the early 1920s. This is particularly important because the measure of the permanent component of long-run migration trends that I observe is determined over a longer period of time and includes

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not only legal but also illegal immigrants. Perhaps more importantly, I am able to estimate a model at the MSA rather than the state level. This is particularly salient to the study of crime since crime is determined primarily by local contextual factors (Bailey, 1994). Using an instrument that combines annual data on rainfall with these long-run Mexican state-U.S. metropolitan area migration patterns, I develop a causal estimate of the contribution of Mexican immigration to crime rates in approximately fifty of the largest U.S. metropolitan areas. Since the instrument is activated only by rainfall shocks, it is as if, in each year, different numbers of Mexican immigrants were assigned at random to each U.S. city.

2.3. The Decision to Migrate

To be sure, Mexicans may migrate to the United States for any number of social or economic reasons, the sum of which are far too complex to capture in a stylized model of migration. Economic theories of migration give rise to ambiguous predictions regarding the selection of migrants along dimensions related to criminal propensities. Economic theory, such as that of Borjas (1999), typically assumes that individuals migrate from Mexico (a relatively poor country) to the United States (a relatively wealthy country) in search of higher earnings. To the degree that these earnings can be either licit or illicit, theory cannot generate obvious predictions about how migrants differ according to their criminal propensities. Moreover, given that migrants are selected according to their expected earnings in the U.S., if a subset of these migrants experience an unexpected lack of viable employment options, it is possible that these individuals may be especially willing to turn to criminal activity to compensate for their poor draw in the distribution of earnings in the U.S. On the other hand, if migrants are selected according to their earnings potential in the U.S., to the degree that earnings potential is positively correlated with characteristics that are negatively associated with participation in crime, selection may work in the opposite direction.

However, since this research uses an empirical approach that relies on the exogeneity of weather shocks, the migration mechanism that I will most plausibly capture and the resulting local average treatment effect that I will be able to estimate presumably arises from weather-induced

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changes in economic opportunities in Mexico.[9] I pause here to briefy consider the mechanisms through which Mexican rainfall influences migration from Mexico to the United States. The relationship between weather and economic opportunities in Mexico is driven by the impact of rainfall on crop yields in Mexican states that remain heavily dependent on agriculture. When rainfall is high, crop yields rise as does the strength of the entire region's macroeconomy (Munshi, 2003; Pugatch and Yang, 2011). However, the degree to which robust economic conditions in rural Mexico is positively or negatively associated with migration is theoretically ambiguous. On the one hand, a negative income shock driven by low rainfall makes migration more attractive as a potential migrant's expected wage differential between the U.S. and Mexico has now increased. On the other hand, migrants face real and binding constraints on the resources necessary to fund a migration episode. In the case of rainfall, to the extent that low rainfall depresses the local economy, potential migrants may face serious credit constraints that serve to reduce migration to the U.S.

Ultimately, whether reduced rainfall, which leads to a negative economic shock, reduces or increases migration is an empirical proposition, one which I will test in my first stage regression. Indeed I test for and find evidence that both very high and very low rainfall are both more likely to drive migration, a feature of the data that is consistent both with the theory described above and with prior research. With regard to the local average treatment effect that arises from the first stage relationship, while it is not possible to identify compliers, I note that rainfall-induced migration logically isolates the effects of economically oriented migration, whether the individual migrants were employed in agriculture or in another industry since the effects of rainfall on crop yields can have feedback effects throughout the local and national economy. That said, the majority of Mexican migrants travel to the United States from origin communities that are predominantly agricultural. To wit, the state of Mexico and the Districto Federal, which together account for Mexico City and its suburbs, account for nearly one

9. According to the 2013 CIA World Factbook, 13.7% of Mexico's labor force is employed in agriculture.

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quarter of Mexico's population but <5% of Mexico's U.S.-bound migrants (Terrazas, 2010).[10]

3. Identification Strategy

3.1. Econometric Framework

Using the Current Population Survey, 1986–2004, I begin with a sample of forty-six metropolitan areas with a 1980 population that exceeded 500,000 individuals, and I generate an estimate of the proportion of each city's population that comprises individuals of Mexican origin in a given year (IMM_{it}).[11] By construction, IMM_{it} can be disaggregated into the number of Mexicans who migrate to the United States from each of thirty-two Mexican states:

$$\text{IMM}_{it} = \sum_{m=1}^{32} \text{IMM}_{mit} \tag{2}$$

Thus, in (2), the total number of Mexicans living in city i in year t is simply the sum of Mexicans in that city in that year who migrated from each of thirty-two Mexican states. Since IMM_{mit} is likely endogenous, it must be estimated using a source of plausibly exogenous variation. As Pugatch and Yang note, with data available on the source region of each Mexican migrant to the U.S. in each year, an instrument could be developed by regressing the number of Mexican migrants on a particular measure of rainfall for each Mexican state–U.S. city pair in the data and aggregating. Unfortunately, the sample sizes of available datasets do not permit such a granular analysis. As an alternative, following the general approach of Pugatch and Yang, I formulate IMM_{it} as a function of the total number of Mexican migrants from each Mexican state in each year (IMM_{mt}) and a set of Mexican state–U.S. city migration weights (P_{im}). However, in a key divergence from their approach, here the weights reflect an empirical measure of the permanent

10. A more formal treatment of the mechanisms underlying a rainfall–migration relationship can be found in the working paper version of this manuscript.

11. Following the approach of Butcher and Piehl (1998a), who studied crime at the MSA level, I choose the years 1986–2004 because coding of metropolitan statistical areas was largely consistent over this time period. The reason why I restrict the analysis to the MSAs with populations above 500,000 is because migration data from Mexican states to smaller MSAs is extremely limited.

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component of Mexican state–U.S. city specific migration flows, as opposed to a cross-sectional measure of Mexican state–U.S. state migration relations that were determined as long ago as 1924 according to the placement of railroad tracks. Equation (3) 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.

$$\mathrm{IMM}_{it} = \sum_{m=1}^{32} (P_{im} \times \mathrm{IMM}_{mt}) + \epsilon_{it}. \tag{3}$$

The weights (P_{im}) are estimated using the mean probability that a migrant from Mexican state mm migrates to each U.S. city using data from 1921 to 1985.[12] Next, I reformulate (3) to reflect the fact that migration from each Mexican state (IMM_{mt}) is instrumented for using rainfall shocks. In order to scale the instrumental variable in a way that generates an interpretable first stage regression coefficient, I multiply the Mexican state–U.S. city migration weights by $\mathrm{MIG}_{mt=1980}$, an estimate of the total number of U.S.-bound Mexican migrants from each state in 1980 and divide this quantity by the population of each U.S. city in 1980. This procedure yields the following instrumental variable:

$$Z_{it} = \mathrm{MIG}_{mt=1980} \times \frac{\sum_{m=1}^{32} P_{im} \times \mathrm{RAIN}_{mt}}{\mathrm{POP}_{ct=1980}}. \tag{4}$$

In (4), for each of the thirty-two Mexican states, the time-invariant vector of migration weights to each city (P_{im}) is first multiplied by a column vector of the estimated number of U.S.-bound migrants from each Mexican state. The resulting term, $P_{im} \times \mathrm{MIG}_{mt=1980}$, is the time-invariant estimate of the number of annual migrants from each Mexican state to each U.S. city. Next, this term is multiplied by the rainfall variable which varies by Mexican state and year. Hence, the term within the summation sign is a $46 \times T$ matrix that reflects the predicted number of migrants to each of the 46 cities in the dataset during 1986–2004 for a given Mexican state. Summing each of the terms in this matrix over the thirty-two Mexican states yields a predicted number of migrants for each city-year arising from rainfall in Mexico. Finally, the term is scaled by the size of the 1980 population in each

12. I choose 1985 as an end date to ensure that all of the migration relations contained in P_{im} are pre-determined with respect to the study sample.

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MSA so that the instrument is expressed as a predicted flow of immigrants to a city in a given year.

Pugatch and Yang formulate RAIN_{mt} in a number of ways but primarily as a z-score reflecting standardized deviations in rainfall from each state's long-run mean. In this research, I utilize both the z-score as well as a set of indicator variables that capture extreme deviations in rainfall in Mexican states. As is predicted by the theoretical model in Section 2.3, to the extent that migrants face fixed costs associated with migration, it is likely that extreme deviations will be more salient predictors of migration. The indicator variables are defined such that RAIN_{mt} is equal to 1 if rainfall is one standard deviation greater than the mean annual rainfall in each Mexican state from 1941–1985 and, alternatively, that RAIN_{mt} is equal to 1 if rainfall is one standard deviation lower than its state-specific long-run mean. These versions of the instrument allow me to capture changes in migration that do not vary linearly in the z-score but are instead based on unexpectedly large rainfall shocks (that are either positive or negative). Finally, before specifying the first stage regression, it is necessary to consider potential temporal variation in the relationship between rainfall shocks and migration. That is, since migrants may not respond to rainfall shocks immediately, it is especially important to capture the relationship between the instrument and migration as flexibly as possible. Hence, I begin by specifying the first stage regression using a series of lags of the instrumental variable, beginning with a contemporaneous measure and adding one, two, and then three lags in additional specifications.[13] Equation (15) is a representation of the first stage regression where r takes on values between zero (to capture the contemporaneous relationship) and three.

$$\text{IMM}_{it} = \alpha + \beta_{FS}\left[\text{MIG}_{mt=1980} \times \frac{\sum_{m=1}^{32} P_{im} \times \text{RAIN}_{mt-r}}{\text{POP}_{it=1980}}\right] + \delta_i + \psi_t + \pi_{it} + \epsilon_{it}. \tag{5}$$

Referring to (5), δ_i represents a vector of U.S. city fixed effects. These terms de-mean IMM_{it} so that the instrument predicts deviations in the percentage

13. I have utilized up to five lags of the instrument in models that are not reported in the paper. The first stage models with up to three lags of the instrument yield the greatest predictive power.

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of a city's Mexican population from its long-run mean. By de-meaning, I am netting out time-invariant city-specific characteristics that may explain the stock of Mexicans in each city. Likewise, ψ_t represents year fixed effects which control for annual migration shocks at the national level. I also add a vector of linear city-specific time trends π_{it} to capture (either positive or negative) linear migration trends from Mexico to each city that are independent of rainfall. Hence, the coefficients on the vector of lagged instruments are identified under fairly stringent identifying assumptions. That is, in order to satisfy the first stage, the instrument must predict deviations from the long-run mean of the Mexican proportion of a city's population that are not explained by annual national immigration trends or linear trends in the immigration series.[14] The corresponding outcome model yields the relationship between the outcome variable, the (log of) crimes per capita (Y_{it}), and rainfall-induced Mexican migration:

$$\log Y_{it} = \eta + \theta_{IV}\widehat{\mathrm{IMM}}_{it} + \delta_i + \psi_t + \pi_{it} + \epsilon_{it}. \tag{6}$$

In (6), $\widehat{\mathrm{IMM}}_{it}$ is the city's predicted Mexican share. The coefficient on this term, θ, represents the impact of a one percentage point increase in a city's Mexican share on the percentage change in the crime rate. Specifying the outcome equation in this way allows for a clear interpretation of θ, the parameter of interest. Since the dependent variable is scaled by the population, under the null hypothesis that immigration does not increase crime, increases in a city's Mexican share should not affect the crime rate. Accordingly, a rejection of the null hypothesis that $\theta = 0$ is taken as evidence in favor of an effect of immigration on crime.

3.2. Identifying Assumptions

Conditional upon instrument relevance (which I discuss in Section 6), this research design identifies a causal effect under the following conditions:

14. The coefficient on the instrument is the effect of the estimated rainfall shock on deviations from the long-run trend of a city's Mexican population. Where the instrument equals zero, the model predicts that the city's migration changes exactly according to a linear (or, in some cases, a quadratic) time trend.

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(1) The instrument (persistent migration relations weighted by rainfall) affects the per capita crime rate in a given network-linked U.S. city only through its effect on migration.
(2) There are no individuals who migrate to the United States *only if* rainfall in their state in not extreme.

The first condition is the standard requirement for the exclusion restriction in an instrumental variables framework.[15] The second condition (that there are no "defiers" of the instrument) is a standard restriction (monotonicity) under which a local average treatment effect is identified.[16] In order for the exclusion restriction to be met, rainfall must be conditionally random—that is, rainfall must succeed in assigning different numbers of Mexican immigrants to each U.S. city in a manner that is independent of any and all other variables, whether they are observed or unobserved. Despite the apparent randomness of rainfall, there are several ways in which the exclusion restriction could potentially fail in this context. First, rainfall shocks in Mexico could be correlated with a time-varying feature of a given city that affects crime through an alternate channel. For example, rainfall in Mexico might be correlated with rainfall shocks in linked U.S. cities, or, alternatively, with Mexican trade with the United States.[17] Fortunately, in their analysis, Pugatch and Yang roundly reject that this is the case.[18]

A related possibility is that exports of narcotics from Mexico to the United States might, in fact, be a function of rainfall in Mexico. Thus, to the extent that crime in U.S. cities is a function of the supply (or the price) of drugs, crime could be related to rainfall through an alternative channel aside from immigration. While I am unable to directly test this, I note here that as long as the rainfall-induced supply shock to narcotics markets affects all cities equally in a given year, such an effect is picked up by the inclusion of year fixed effects. In other words, it need not be the case that Z is completely random—only that it is as good as random, conditional on the

15. Formally, we are assuming that $\text{cov}(Z, \epsilon) = 0$.
16. see Angrist and Imbens (1994) for a detailed discussion.
17. As Pugatch and Yang (2011) note, this might be the case if higher rainfall in a U.S. state's historical migrant origin areas in Mexico led to higher demands for U.S. goods (p. 24).
18. The authors include U.S. weather patterns as well as U.S. state-level exports to Mexico as additional regressors and fail to reject the null hypotheses that these regressors are jointly equal to zero.

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covariates in the model.[19] Moreover, this will only represent a violate of the exclusion restriction to the extent that network linkages between Mexican states and U.S. cities are identical for both labor markets and the distribution of narcotics.

A second concern underlying this research design involves the potential selection of migrants from each Mexican state. While this concern does not involve the conditional randomness of rainfall and, as such, does not threaten the consistency of 2SLS, it nevertheless has implications for how 2SLS coefficients are interpreted and, accordingly, I discuss this consideration here. Specifically, since my analysis compares the change in the immigrant stock in each city to the change in its crime rate, under a homogenous treatment effect, an assumption of the analysis is that the average criminal propensities of immigrants from each Mexican state are equal. To the extent that Mexican states differ in the underlying criminality of the individuals who migrate to the U.S. as a result of rainfall, the resulting estimates may differ a great deal from city to city. In particular, we might be concerned that migrants from certain Mexican states migrate to a U.S. city explicitly in order to participate in that city's crime market. While I am unable to reject that this is the case, by using the permanent component of migration, I am isolating variation in Mexican migration that is the result of long-standing migration networks. In other words, while an association between rainfall in Mexico and marijuana exports could potentially affect the *timing* of migration, the instrument captures only migrants who leave Mexico for historically linked U.S. destinations. As such, the criminally involved migrant from Baja California who settles in Philadelphia (which is not a linked U.S. destination) to pursue a career in an underground market will not contribute to the average causal response that I estimate.

Finally, it is worth noting that the exclusion restriction is likely not violated even if there are errors in the measure of the immigrant stock I obtain from the Current Population Survey, a concern highlighted by Butcher and Piehl (1998b). Given that this variable is almost certainly measured with error, at first blush, this would appear to be a first-order concern. However, while classical measurement errors in the immigrant share will result in

19. I further note that the bias introduced by a "near exogenous" instrument is most serious if the instrument is also weak. The F-statistic on the instrumental variable used throughout the analysis exceeds 80, thus easing this concern.

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attenuated OLS coefficients, since the immigrant stock is, in this research, the endogenous covariate that I am projecting on to the instrument, classical measurement errors in this variable will only decrease the precision of resulting estimates—the estimates will still be consistent under the assumption that the measurement errors are uncorrelated with rainfall. As such, the rainfall instrument plausibly fixes two problems associated with least squares estimation—the problem of endogeneity and as well as problems arising due to the presence of measurement errors.

4. Data

This research draws primarily on four different datasets to construct a city-by-year level analysis file. I begin with data on a city's Mexican population that is drawn from the March supplements of the Current Population Survey (CPS). In order to ensure appropriate cross-city comparisons, I use data on MSAs with a 1980 population that exceeds 500,000 individuals.[20] Because a variable that captures immigration status was added to the CPS only in 1994, in order to extend the series, I follow Pugatch and Yang (2011) and use a variable indicating Mexican nationality to capture the percentage of each city's population that is Mexican in a given year. While this approach does not allow me to isolate the percentage of a city's population that is comprised of Mexican immigrants, to the extent that a first stage relationship exists between rainfall in Mexican states and changes in the Mexican population of U.S. cities linked historically to those Mexican states, it is reasonable to expect that the relationship is being driven by a subset of individuals who are immigrants. That said, if the local average treatment effect being estimated captures a modest number of U.S.-born Mexicans, the coefficient vector on the instruments will simply estimate the reduced form effect of rainfall in Mexico on a U.S. city's total Mexican population. To the extent that Mexican immigration drives changes in the number of U.S.-born Mexicans either mechanically or through network effects, this is an important consideration.

20. 500,000 is chosen both to ensure comparability between cities and also because the number of U.S.-bound migrants from each Mexican state that I am able to observe in these cities becomes very small.

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Data on rainfall in Mexican states were obtained from the MMP environmental file.[21] The file contains data collected from local weather stations on monthly rainfall, for each Mexican state, during 1941–2005. Because the growing season in Mexico is year-long, I generate annual rainfall for each state in each year and standardize the data by subtracting each data point from its state-specific mean and dividing by its state-specific standard deviation to obtain a z-score.

Data used to construct P_{im}, the matrix of Mexican state–U.S. city-specific time-invariant migration weights were generated from the MMP'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. Communities were chosen in order to provide variation in the characteristics of sending regions. While communities were not surveyed explicitly because they send large numbers of migrants to the United States, communities nevertheless needed to send at least a few migrants in order to be surveyed.[22] Each community was sampled once and individuals who reported having migrated to the United States were asked to retrospectively recall each of their prior migration experiences.[23] Among male household heads, 23%

21. The MMP is the product of a collaboration between researchers at Princeton University and the University of Guadalajara in Mexico. The MMP is co-directed by Jorge Durand and Douglas S. Massey.

22. The survey sample covers the following Mexican states: Aguascalientes, Baja California Norte, Chihuahua, Colima, Durango, Guanajuato, Guerrero, Hidalgo, Jalisco, Mexico, Michoacan, Morelos, Nayarit, Nuevo Leon, Oaxaca, Puebla, San Luis Potosi, Sinaloa, Tlaxcala, Veracruz, Yucatan, and Zacatecas. Within each state, communities are classified as either *ranchos* (fewer than 2,500 inhabitants), *pubelos* (between 2,500 and 10,000 residents), *mid-sized cities* (10,000–100,000 residents), or *large metropolitan areas* (100,000 or more residents). In pueblos and ranchos, MMP investigators conduct a complete census of dwellings and randomly select households to survey from among the entire community. In mid-sized cities and large metropolitan areas, MMP investigators selected established neighborhoods.

23. As is always the case, when retrospective survey data are used, there is a concern that recall bias will compromise the resulting estimates. Since I use the MMP survey data to document the first stage relationship between immigration that is predicted by rainfall and actual immigration, to the extent that recall bias leads to errors in the matrix of Mexican state–U.S. city weights, the resulting first stage estimates will be weaker than those derived from error-free survey data. However, two points are worth noting. First, errors will only accrue to the extent that an individual recounts a fictitious trip—that is, a trip to a destination to which that individual did not ever travel. In the event that

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reported having migrated to the United States within three years of the time of survey with 89% reporting an undocumented migration spell (Hanson, 2001).[24] Using data on the U.S. destination for the migrants' first migration episode, I remove from this file all migrants whose first migration experience occurred after 1985 and 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.[25] Thus, the weights were constructed from the migration experiences of 3,981 Mexican migrants. Table 1 provides descriptive details on the weights, showing the top three U.S. destination areas for migrants from each Mexican state. 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 Notre, 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.[26]

Finally, data on crimes reported to police were obtained from the Federal Bureau of Investigation's Uniform Crime Reports (UCRs), the standard source of data on crimes at the agency level that is employed in aggregate-level crime research. Since 1934, the UCR has, either directly or through a designated state reporting agency, collected monthly data on index crimes

individuals simply switch the ordering of trips, the weights will continue to reflect legitimate migration relations. Having constructed the weights using an individual's first trip to the United States, a trip which should be easier to recall than a second or third trip, I expect such bias to be minimized. More importantly, random errors in the migration weights will affect the reduced form and the first-stage estimates equally. As such, errors in the weights will serve only to increase the standard errors in the second-stage estimates without introducing bias.

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. For a detailed discussion of the MMP's migrant-level file, see Hanson (2001).

25. In principle, I could have used the migrant's last migration episode. However, it is likely that the first migration experience is more likely to reflect network ties between the source and destination communities. In practice, the magnitude of the elements of the matrix are almost completely invariant to the choice of migration episode.

26. See Table 2 for additional details.

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Table 1. Mexican State Sources of U.S.-Bound Immigrants' Selected U.S. Metropolitan Areas

U.S. Metropolitan area	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. metropolitan areas. These data are based upon the experiences of migrants surveyed in the MMP's Migrant File, 1924–1985.

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Table 2. 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%)

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Table 2. Continued

Mexican State	Destination #1	Destination #2	Destination #3
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 MMP's Migrant File, 1924–1985.

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reported to local law enforcement agencies. The index crimes collected consistently since 1960 are: murder (criminal homicide), forcible rape, robbery, aggravated assault, burglary, larceny, and motor vehicle theft.[27] In order to maintain consistency with the level of aggregation of the migration data from the MMP, I aggregate the agency-level UCR data to the MSA level.[28]

Forty-six cities are used in the analysis. For these cities, the 1986–2004 CPS datafile comprises of 3,067,064 individuals of whom 6.8% are identified as individuals of Mexican origin. In the United State, the Mexican population is 52% male with an especially high number of males represented among the prime working ages. During 1986–2004, the percentage of the U.S. population that is Mexican origin nearly doubled, increasing from 4.7% in 1986 to 9.2% in 2004. Over the same time period, on a per capita basis, reported violent crimes and property crime fell by more than 25% and 28%, respectively.

Table 3 presents summary statistics for the Mexican share and each of the crime rates for individual cities, both in levels and in logs. In particular, for each variable employed in the analysis, I present a mean, a minimum value, a maximum value, and three types of standard deviations—the overall standard deviation as well as the between (cross-sectional) and within standard deviations. The average city in the sample is 10% Mexican, a number that ranges from <1% to 88% over the entire study period. Notably, nearly all

27. The UCR employs an algorithm known as the "hierarchy rule" to determine how crimes involving multiple criminal acts are counted. In order to avoid double counting, the UCR classifies a given criminal transaction according to the most serious statutory violation that is involved. For example, a murder–robbery is classified as a murder.

28. Several robustness checks are worth noting. For each MSA, I selected the crime rate for the most populous city in each metropolitan statistical area. This method was employed for several reasons. First, data obtained from a single police reporting agency are likely more accurate and less noisy than more highly aggregated data. This is due to several well-documented problems involving "double counting" in aggregated files (see Maltz and Targonski, 2002 for a detailed discussion). Secondly, to the extent that crime victims in different jurisdictions within the same MSA report crimes at different rates, aggregating to the MSA level may lead to systematic biases in the numbers of crimes reported to police. In particular, MSAs that are comprise largely suburban areas may be subject to different reporting rates than MSAs that are entirely urban. In a second set of analyzes, I re-estimated the resulting models using cleaned UCR data provided directly by Michael Maltz. The results of this analysis yield qualitatively similar results to those reported in Table 7 of the paper and, as such, the full results are omitted in the interest of brevity.

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Table 3. Summary Statistics

		Logs				Levels			
		Mean	SD	Min.	Max.	Mean	SD	Min.	Max.
Mexican share	O	1.11	1.79	1.00	4.48	9.8	15.1	0.0	88.4
	B		1.67				14.9		
	W		0.71				2.9		
Violent crimes	O	6.82	0.51	5.18	8.27	1, 047.8	569.0	178.5	3, 913.0
	B		0.46				508.6		
	W		0.22				265.4		
Property crimes	O	8.71	0.30	7.66	9.72	6, 316.6	1, 888.1	2, 111.4	16, 576.4
	B		0.24				1, 526.4		
	W		0.18				1, 132.3		
Murder	O	2.38	0.65	0.60	4.39	13.7	11.6	1.8	80.8
	B		0.58				10.6		
	W		0.30				5.1		
Rape	O	3.88	0.42	2.17	5.15	52.7	22.8	8.7	172.8
	B		0.35				18.3		
	W		0.23				13.7		

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Table 3. Continued

		Logs				Levels			
		Mean	SD	Min.	Max.	Mean	SD	Min.	Max.
Robbery	O	5.83	0.61	4.30	7.36	409.5	267.8	73.8	1, 578.0
	B		0.55				231.8		
	W		0.28				138.0		
Assault	O	6.22	0.56	4.04	7.67	583.6	332.3	56.6	2, 137.9
	B		0.51				299.6		
	W		0.25				149.9		
Burglary	O	7.16	0.40	5.78	8.29	1, 392.9	571.1	323.7	3, 994.3
	B		0.27				385.8		
	W		0.16				424.7		
Larceny	O	8.23	0.31	7.21	9.20	3, 949.9	1, 194.5	1, 358.2	9, 905.6
	B		0.27				1, 037.0		
	W		0.16				610.9		
Motor vehicle theft	O	6.74	0.55	5.09	7.93	973.8	503.1	163.1	2, 788.3
	B		0.46				408.6		
	W		0.31				299.1		

Note: The table reports summary statistics for the Mexican population share and each of the crime variables both in logs and in levels of the number of crimes per 100,000 population. For each variable, we report the overall mean, the standard deviation decomposed into overall ("O"), between ("B"), and within ("W") variation, as well as the minimum and maximum values.

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of the variation in a city's Mexican share is cross sectional, while within-city variation is relatively small. This reflects the fact that while some cities (e.g., Los Angeles and Houston) are persistent destinations for Mexican immigrants, other cities (e.g., New York and Boston) are not. With regard to crime, several features of the data are worth noting. First, approximately six in seven crimes reported to police are property crimes with an average large MSA documenting 6,300 property crimes and 1,000 violent crimes per 100,000 residents. Secondly, the most serious crimes (murder and rape) account for $<1\%$ of all crimes reported to police while less serious offenses such as larceny account for nearly half of all crimes. With regard to the decomposition of variance, the picture for crime is more mixed than it is for the Mexican share. I note that the between variation is dominant for the violent crimes (murder, rape, robbery, and aggravated assault), while the between and within variation are more equally apportioned for the property crimes (burglary, larceny, and motor vehicle theft).

5. Results

5.1. First-Stage Estimates

In the first stage, I estimate the effect of several different incarnations of the rainfall instrument on deviations from the long-run mean of the proportion of the Mexican population in U.S. cities. There are three primary conceptualizations of the instrument that I explore. First, I specify the rainfall variable as a z-score such that each Mexican state's rainfall in a given year is expressed in terms of standard deviations from its mean over the 1941–1985 period. This variable, which uses deviations in rainfall to proxy for transitory shocks to each Mexican state's economy, captures the (migration-weighted) linear effect of rainfall where low values of the instrument reflect drought and high values of the instrument reflect an abundance of precipitation. Next, I re-specify the rainfall variable as an indicator variable that is equal to unity if the rainfall z-score in a given Mexican state-year was >1. This variable allows me to test for the possibility that extreme positive deviations in rainfall cause migration. Finally, in order to capture an effect of droughts, I specify an instrument that captures extreme low (less than -1 SD) deviations in rainfall. A positive relationship between the low

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rainfall dummy and migration might be the case if individuals face fixed costs associated with migration and migrate to escape (extreme) economic hardship.

Prior to running the first-stage regressions, it is important to examine variation in each of the instruments to get a sense of the type of variation that is being captured. The average value of the z-score instrument is -0.00012 ($\mathrm{SD} = 0.0028$). It is sensible that the instrument is centered around zero as the mean rainfall z-score is zero. An analysis of variance reveals an intra-class correlation coefficient of 0.95, indicating that nearly all of the variation in the instrument comprises of within-city variation. This too is sensible as the instrument is activated by rainfall and assumes that Mexican state–U.S. city migration relations are constant over time. Thus each city, over many years, receives a rainfall-induced migration shock that is of roughly equal magnitude though, within cities, there is much temporal variation. Turning to the dummy variable version of the instruments, the story is subtly different. Since the instrument is now equal to zero if none of the Mexican sending states experienced a (positive or negative) rainfall shock in a given year and a positive number indicating the strength of migration ties otherwise, the variables are distributed quite differently. An examination of the extreme value instruments reveals that 24.4% of city-years experienced at least one high rainfall shock from a Mexican sending state and an equivalent percentage of city-years experienced at least one low rainfall shock. Roughly 10% of city-years experienced at least one of each type of rainfall shock.

In order to carefully examine the pathways through which rainfall influences migration, I begin by specifying very parsimonious first-stage models, including one lag of the instrument at a time. Table 4(a) presents regression results for models using the z-score instrument. In Table 4, columns 1–4 report coefficients on the contemporaneous instrument and each of one, two, and three lags. Column 5 includes a specification containing all four lags in a single model. To derive a national-level estimate, all models are estimated using weighted least squares where the 1980 MSA population is used to weight the observations. In addition, all models include city and year fixed effects as well as city-specific linear time trends. Standard errors, which are clustered at the city level, are reported in parentheses below the estimated coefficients. In each column, I report the F-statistic on

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all excluded instruments in the model with the corresponding critical value for the weak instruments test suggested by Stock and Yogo (2002). A visual inspection of the coefficients in Tables 1 and 2 reveals the weak predictive power of rainfall. The coefficients change sign depending upon the lag employed and an F-test on the instruments reveals that, despite a tendency for the coefficients to be positive, they are only weakly significant and typically do not exceed the Stock–Yogo threshold. In order to pin down the precise mechanism through which rainfall induces migration, I re-specify the instrument using dummy variables that capture extreme (± 1 SD) deviations in rainfall. Those first-stage estimates are reported in Table 4. Table 4, which is laid out the same way as Table 4, reveals a robust, positive relationship between extreme deviations in rainfall and migration. The pattern is sensible and it explains why the linear instrument was not successful in predicting migration. That is, since large deviations (both positive and negative) are positively associated with migration, the impact of the extreme positive values tends to negate the impact of the extreme negative values, leading the net effect of rainfall to be not substantially different from zero. The strength of the relationship is strongest at two lags. However, at each lag, coefficients on the excluded instruments are positive and nearly always significant and the corresponding F-test generally meets or exceeds the Stock–Yogo critical values each time. Using the contemporaneous instruments as well as three additional lags yields an F-statistic of 80.2 on the excluded instruments.[29] This value of the F-statistic easily exceeds the corresponding critical value for one endogenous covariate and eight excluded instruments as is recommended by Stock and Yogo (2002).[30]

In order to demonstrate that the instrument is a valid predictor of cross-city migration, I subject the first-stage model to a series of additional robustness checks. First, I estimate (6) using quadratic in addition to linear city-specific time trends. This ensures that the instrument predicts

29. In a model with one endogenous regressor, eight excluded instruments and a desired maximal bias of 0.10, the threshold for the F-statistic is 33.8. The critical value for a maximal bias (relative to OLS is 20.3).

30. I also apply the Kleibergen–Paap rk Wald test for underidentification – a test for whether the matrix of instruments and endogenous regressors is of full column rank. The test is valid for data that is not i.i.d. When the data are i.i.d., the test is equivalent to the test of Cragg and Donald (1993). The test statistic is significant at $p < 0.001$ allowing me to reject the hypothesis that the first stage model is underidentified.

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Table 4. First-Stage Regression Models

	(1)	(2)	(3)	(4)	(5)
(a) Z-Score Instrument					
Rainfall instrument	52.4				−36.5
	(50.7)				(48.6)
$(t-1)$		118.0***			61.4**
		(30.9)			(27.5)
$(t-2)$			131.7***		53.4***
			(20.4)		(16.1)
$(t-3)$				85.1**	51.4
				(39.6)	(32.0)
N	893	846	799	752	752
R-squared	0.984	0.985	0.986	0.986	0.986
F-statistic on excluded instrument	1.1	14.6	41.5	4.6	4.6
Stock–Yogo critical value (10% maximal bias)	16.4	16.4	16.4	16.4	24.6
(b) Extreme Values Instrument					
High rainfall	507.1*				36.9
	(260.3)				(441.7)
$(t-1)$		876.9***			300.0*
		(184.1)			(159.6)

(*Continued*)

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Table 4. Continued

	(1)	(2)	(3)	(4)	(5)
$(t-2)$			831.5***		840.9***
			(86.6)		(196.3)
$(t-3)$				679.8***	492.9
				(219.8)	(356.2)
Low rainfall	504.0***				−180.7
	(114.9)				(183.1)
$(t-1)$		662.8***			631.5***
		(151.6)			(212.6)
$(t-2)$			320.0*		352.1
			(184.7)		(290.4)
$(t-3)$				251.8	−36.3
				(331.4)	(362.1)
N	893	846	799	752	752
R-squared	0.984	0.985	0.985	0.986	0.986
F-statistic on excluded instrument	14.7	19.1	53.2	22.3	80.2
Stock–Yogo critical value (10% maximal bias)	19.9	19.9	19.9	19.9	33.8

Q15 *Note*: The table reports coefficients and standard errors for a series of least squares regressions of the city's Mexican immigrant share on several different lags of the rainfall instrument (a) in z-zcore and (b) in extreme values. Depending on the number of lags, models utilize between 752 and 893 observations covering 46 metropolitan statistical areas from 1986 to 2004. The table reports WLS regressions using 1980 MSA population weights. All models contain city and year fixed effects as well as city-specific linear time trends. The F-statistic that is reported is the joint hypothesis test that the coefficients on all excluded instruments are equal to zero. The Stock–Yogo critical value is the critical value associated with one endogenous regressor and the appropriate number of excluded instruments. Standard errors (in parentheses) are clustered at the city level. $^{*}P < 0.10$; $^{**}P < 0.01$; $^{***}P < 0.05$.

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within city changes in migration above and beyond a more flexibly specified time trend. Using my preferred first-stage specification, the F-statistic on the excluded instruments remains sufficiently high at 80.4. Secondly, to ensure that the results of the analysis are not driven by the way that each city is weighted, I estimate (7) without city population weights. Without the weights, in my preferred specification, the F-statistic on excluded instruments is 56.3. Third, I re-estimate the first-stage equation excluding Los Angeles and report an F-statistic on the excluded instruments of 43.9. Fifth, I condition on a vector of time-varying controls that capture variation in a city's social and economic conditions as well as demographic composition.[31] Finally, I re-estimate (7) using leads, rather than lags of the instruments. If the instrument were spuriously correlated with migration flows, we might expect that leads of the instrument were correlated with migration just as contemporaneous and lagged versions of the instrument are. Since leads of rainfall cannot have a causal effect on migration, I interpret evidence of an association between leads of the instrument and migration as evidence of a spuriously measured relationship. In order to check that the causal pathway through which rainfall instruments that I employ in my preferred specification. Whereas the F-statistic on lags of the excluded instruments was 80.2, the F-statistic on leads of the excluded instruments is well below the Stock–Yogo critical value and none of the coefficients are significant at conventional levels.[32] Finally, I present results from a series of tests of overidentifying restrictions which unilaterally fail to reject the null hypothesis of exogeneity. In particular, because the number of instruments exceeds the number of endogenous regressors, my IV equation is overidentified allowing me to test the exogeneity of my instruments under the assumption of a constant local average treatment effect. Since I cluster my standard errors at the city level, I utilize Hansen's J-test which produces a test statistic that is robust to arbitrary dependence in the within-city errors. In Table 5, I present results from Hansen's J-test of overidentifying restrictions for each of the models that is tested in the paper. Each row contains models in which the dependent variable is the log of a different

31. The full set of variables includes the employment-to-population ratio, the poverty rate, the 12th grade high school dropout rate, the per capita number of sworn police officers and a series of variables capturing a city's demographic composition.

32. The F-statistic on excluded instruments using one and two leads is 5.9.

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Table 5. Hansen's J-Test of Overidentifying Restrictions

	Logs		Levels	
	Unweighted	Weighted	Unweighted	Weighted
Violent crimes	0.22	0.19	0.35	0.52
Property crimes	0.43	0.45	0.35	0.37
Murder	0.15	0.78	0.51	0.96
Rape	0.20	0.55	0.45	0.72
Robbery	0.07	0.10	0.35	0.34
Assault	0.54	0.34	0.51	0.41
Burglary	0.31	0.48	0.32	0.41
Larceny	0.58	0.48	0.48	0.41
Auto theft	0.23	0.61	0.21	0.49

Note: The table reports the p-values from Hansen's heteroskedasticity and cluster robust J-test of overidentifying restrictions. All models utilize 729 observations covering 46 metropolitan statistical areas during 1986–2004. The first set of estimates report the p-value of the test using the log of the number of crimes per 100,000 population. The second set of estimates report the p-value of the test using the number of crimes in levels per 100,000 population. Unweighted regressions and WLS regressions using 1980 MSA population weights are reported. All models contain city and year fixed effects as well as city-specific linear time trends.

UCR crime rate. Along the columns, for each choice of regression weights, I run the J-test for all instruments. A cursory glance at Table 5 reveals that I fail to reject the null hypothesis that the instruments are exogenous for all crime models. The results of these tests provide support for (though do not automatically validate) my identifying assumption – that rainfall in Mexican states is uncorrelated with U.S. crime rates except through migration. Using the final set of first state regressions reported in Table 4, I proceed to estimating my second-stage models.

5.2. Least Squares and 2SLS Estimates

In the outcome equation, I regress both the level and the log of each of seven index crimes on the predicted change in a city's share of Mexican migrants. Prior to presenting 2SLS results, I present results from a series of least squares regressions of the crime rate (also measured in levels and logs) on the share of Mexican migrants. These estimates are presented in Table 6. In Table 6, the first two columns correspond to models in which the crime

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Table 6. Least Squares Models of the Effect of Mexican Immigration on Crime

	Logs		Levels	
	Unweighted	Weighted	Unweighted	Weighted
Violent crimes	−0.05	−0.10	0.7	−0.2
	(0.23)	(0.24)	(2.0)	(2.7)
Property crimes	−0.42**	−0.29	−22.7**	−11.5
	(0.20)	(0.23)	(11.1)	(13.1)
Murder	0.15	0.08	0.0	0.0
	(0.30)	(0.57)	(0.5)	(0.1)
Rape	0.73***	0.62**	0.4***	0.4***
	(0.21)	(0.25)	(0.1)	(0.1)
Robbery	−0.46**	−0.05	−1.5	−0.4
	(0.22)	(0.26)	(0.9)	(1.5)
Assault	0.10	−0.26	1.8	−0.3
	(0.31)	(0.33)	(1.5)	(1.8)
Burglary	0.08	0.28	−0.2	4.8
	(0.21)	(0.27)	(2.7)	(3.3)
Larceny	−0.80***	−0.61***	−27.7***	−19.0**
	(0.22)	(0.21)	(7.5)	(8.5)
Auto theft	0.02	−0.03	5.2**	2.7
	(0.25)	(0.30)	(2.5)	(3.4)

Note: The table reports coefficients and standard errors for a series of least squares regressions of the number of crimes on the Mexican population share, conditional on the MSA population. Each model utilizes 752 observations covering 46 metropolitan statistical areas during 1989–2004. The first set of estimates report the effect of a one percentage point increase in the Mexican population share on the log of the number of crimes per 100,000 population. The second set of estimates report the effect of a one percentage point increase in the Mexican population share on the number of crimes per 100,000 population in levels. Unweighted regressions and WLS regressions using 1980 MSA population weights are reported. All models contain city and year fixed effects as well as city-specific linear time trends. Standard errors (in parentheses) are clustered at the city level. * $P < 0.10$; ** $P < 0.01$; *** $P < 0.05$.

rate is measured in logs, while the second set of columns corresponds to models in which the crime rate is measured in levels. Within each panel, each row corresponds to a different index crime with the first two rows (violent crimes and property crimes, respectively) corresponding to the two crime aggregates. Finally, each model is specified both with and without

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MSA population weights.[33] As with the first-stage models, all regressions are estimated using city and year fixed effects and city-specific linear time trends, with standard errors clustered at the city level.

Beginning with the log crime models, I note that regression coefficients have been multiplied by 100 for ease of interpretation. Thus, referring to the weighted least squares estimates, we see that a one percentage point increase in the Mexican share is associated with a 0.1% decrease in the rate of violent crimes and a 0.3% decrease in the rate of property crimes. Both of these estimates are small both in an economic sense and relative to their standard errors. In fact, the least squares models are estimated with extraordinary precision all around as I am typically able to reject increases or decreases in the crime rate on the order of 0.5%. The precision of the models is due to the fact that since city fixed effects and linear time trends explain such a large share of the within-city variation in the crime rate (with corresponding R^2 values exceeding 0.98), the explanatory power of the models is extremely high and the corresponding sampling variance is small. The magnitude of the coefficients and standard errors on violent and property crimes is broadly consistent with results reported by Butcher and Piehl (1998b) for a panel of cities and cross-sectional results reported by Reid et al. (2005) in which OLS results for a 2000 cross section of U.S. cities were analyzed. For example, Butcher and Piehl (1998b), by their conditioning of fixed effects, report a violent crime coefficient of -0.25% (SE $= 1.15\%$). As their sample is less than half the size of the sample I employ, it is sensible that the standard errors I obtain are smaller.

Referring to the disaggregated crimes, several patterns in the data are worth noting. First, Mexican immigrants are associated with higher rates of per capita rapes and burglaries and lower rates of per capita larcenies, though notably the degree to which coefficients are significant depends a great deal on whether or not the analysis employs MSA population weights.

This instability of the coefficients suggests a great deal of heterogeneity amongst receiving cities. Second, in addition to estimating the models

33. The population weights use the MSA's 1980 population. In order to get a sense of the degree to which there is heterogeneity in the results, I also weight by the share and the size of an MSA's Mexican population in 1980. The results are largely invariant to this weighting scheme.

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in logs, I also provide estimates of the association between Mexican immigrants and crime in levels. Here, a one percentage point increase in the Mexican share is associated with 4.8 additional burglaries per 100,000.

Taken as a whole, the mixture of positive and negative coefficients for different crime types and the sensitivity of the estimated effects to the inclusion of population weights presents little consistent evidence against a null effect. The results, once again, are broadly consistent with prior cross-city research that finds little evidence of an association between immigration and crime.[34]

Table 7 presents 2SLS estimates of the relationship between predicted Mexican immigration and crime. Because the Mexican proportion of the population is estimated, point estimates in these models are less precisely estimated than the least squares coefficients presented in Table 6. However, it is worth noting that they remain extraordinarily precise (depending on the crime, standard errors are typically under 1–2%.[35] Consistent with the results from OLS models, IV results presented in Table 7 provide little evidence against the null hypothesis that Mexican immigration is not associated with crime. Neither the violent nor the property crime models reveal a significant relationship. Moreover, in those models, I can reject the possibility that a one percentage point increase in the Mexican share is associated with more than a 1% increase or more than a 1.5% increase in the rate of violent crimes and property crimes, respectively. Referring to the individual crime categories, while coefficients for murder, rape, assault, burglary, and larceny do not meet conventional thresholds for significance in any of the models, there is some evidence in favor of a positive effect of Mexican immigration on robbery and motor vehicle theft. While the robbery result is not significant in unweighted regressions, with population weights, a one percentage point increase in the Mexican share is associated with a 2.7% increase in robberies or an increase of 15.4 robberies per 100,000 residents. The motor vehicle theft coefficient is significant only in the unweighted models and, even then, is only significant when the crime rate is measured

34. The only crime type for which results are significant in both weighted and unweighted models is rape.

35. To the degree that I measure the Mexican population in each city with random error, the resulting IV estimates will be measured more imprecisely. However, the resulting IV estimate will remain consistent.

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Table 7. Two-Stage Least Squares Models of the Effect of Mexican Immigration on Crime

	Logs		Levels	
	Unweighted	Weighted	Unweighted	Weighted
Violent crimes	−1.14	0.31	−8.8	7.6
	(1.31)	(0.99)	(9.9)	(12.9)
Property crimes	−0.88	−0.05	−31.5	23.6
	(1.76)	(1.54)	(90.5)	(81.1)
Murder	0.49	0.57	0.1	0.1
	(1.22)	(1.33)	(0.1)	(0.2)
Rape	−0.52	−0.96	−0.4	−0.3
	(0.56)	(0.63)	(0.3)	(0.3)
Robbery	1.03	2.73**	5.6	15.4**
	(1.08)	(1.06)	(3.9)	(7.4)
Assault	−2.88*	−0.68	−13.7*	−3.0
	(1.67)	(1.35)	(7.2)	(8.8)
Burglary	−1.07	1.23	−4.9	31.6
	(1.75)	(1.86)	(19.3)	(20.4)
Larceny	−2.35	−0.95	−71.1	−35.0
	(1.92)	(1.64)	(58.6)	(51.9)
Auto theft	2.07	0.12	44.6	27.0
	(1.50)	(2.19)	(20.2)	(29.7)

Note: The table reports coefficients and standard errors for a series of 2SLS regressions of the crime rate on the Mexican population share. Mexican population share is instrumented using predicted rainfall-induced immigration. Each model utilizes 752 observations covering 46 metropolitan statistical areas during 1986–2004. The first set of estimates report the effect of a one percentage point increase in the Mexican population share on the log of the number of crimes per 100,000 population. The second set of estimates report the effect of a one percentage point increase in the Mexican population share on the number of crimes per 100,000 population in levels. Unweighted regressions and WLS regressions using 1980 MSA population weights are reported. All models contain city and year fixed effects as well as city-specific linear time trends. Standard errors (in parentheses) are clustered at the city level. * $P < 0.10$; ** $P < 0.01$; *** $P < 0.05$.

in levels. To compare my results more explicitly to those in the extant literature, I note that while Butcher and Piehl (1998b) do not report 2SLS, they indicate that those coefficients are very similar to those obtained using OLS. My results are quite similar to OLS models reported in their 1998 paper.

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5.3. Heterogeneity and Robustness

In Table 8, I present a series of robustness checks designed to test the sensitivity of the estimates reported in Table 7 to minor specification changes. Column (1) of Table 8 replicates column (2) of Table 7 in which the log of the crime rate is regressed on the instrumented Mexican population share weighting by city population. Each subsequent column presents the equivalent estimates with a particular change in specification.

Given the notable differences between weighted and unweighted regression models, a natural extension of the paper would involve an exploration of the degree to which there is heterogeneity in the effect of immigration on crime across different types of cities. While the relatively small number of cities in my sample limits my power to test for heterogeneity in the estimates along a vector of initial city characteristics, I can nevertheless provide several important tests of the robustness of the reported results. I begin by testing whether the treatment effects reported in Table 7 are driven by one or two "important" cities. In particular, we might be concerned that the null effects reported in Table 7 are an artifact of a null effect in one or two influential cities, rather than a pattern that is consistent across all cities. In columns (2) and (3) of Table 8, I drop Los Angeles and Chicago, the two largest destination cities in the sample and repeat the main analyzes presented in Table 7 of the paper. The coefficients reported in columns (2) and (3) differ in magnitude to an extent from the coefficients reported in column (1) which uses the full sample. However, given sampling variability, they are, on the whole, similar to those in Table 7. However, the robbery result is a notable exception. While the positive coefficient in the robbery model survives the exclusion of Chicago, it does not survive the exclusion of Los Angeles, indicating that the positive coefficient on robbery appears to derive from local conditions that are specific to Los Angeles. This finding underscores the difficulty of estimating a "national effect" of immigration and serves as a reminder that immigration may have very different effects depending on a variety of contextual factors.[36]

36. In addition, in order to test for whether the inclusion of time trends results in an "overfitted" model, I re-estimate the basic specification presented in column (1) of Table 8 on the 1995–2004 subsample of the data. The estimated coefficients for the 1995–2004 subsample are as follows: murder (0.76), rape (1.26), robbery (−0.74), assault

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Table 8. Two-Stage Least Squares Models of the Effect of Mexican Immigration on Crime: Robustness Checks

	(1)	(2)	(3)	(4)	(5)
Violent crimes	0.31	−0.98	0.31	1.18	0.20
	(0.99)	(1.47)	(0.99)	(0.91)	(1.00)
Property crimes	−0.05	−0.90	−0.20	0.93	−0.11
	(1.54)	(2.09)	(1.53)	(1.43)	(1.54)
Murder	0.57	1.11	−0.17	0.77	0.80
	(1.33)	(1.37)	(1.52)	(1.86)	(1.37)
Rape	−0.96	−0.47	−0.96	0.16	−0.87
	(0.63)	(0.81)	(0.63)	(0.97)	(0.63)
Robbery	2.73**	1.24	2.52**	3.17***	2.81***
	(1.06)	(1.27)	(1.08)	(1.18)	(1.08)
Assault	−0.68	−1.59	−1.23	1.07	−1.00
	(1.35)	(1.89)	(1.30)	(1.49)	(1.68)
Burglary	1.23	−1.29	0.88	2.09	1.06
	(1.86)	(1.97)	(1.89)	(1.85)	(1.34)
Larceny	−0.95	−3.22	−1.03	0.26	−0.98
	(1.64)	(2.07)	(1.60)	(1.85)	(1.64)
Auto theft	0.12	0.72	0.06	0.47	0.07
	(2.19)	(2.48)	(2.18)	(2.65)	(2.21)
Los Angeles included	yes	no	yes	yes	yes
Chicago included	yes	yes	no	yes	yes
Economic/social controls	no	no	no	yes	no
Control for prime-age males	no	no	no	no	yes

Note: The table reports coefficients and standard errors for a series of 2SLS regressions of the log crime rate on the instrumented Mexican population share. Column (1) reproduces 2SLS estimates reported in Table 7 that use the full sample and condition on city and year fixed effects and city-specific linear time trends. In columns (2) and (3), Los Angeles and Chicago are excluded from data, respectively. Column (4) conditions on a vector of control variables: the employment-to-population ratio, the porverty rate, the 12th grade dropout rate, the number of sworn police officers per capita and a series of demographic controls. Column (5) controls for the change in the proportion of the Mexican population that is comprised of prime age males. All models are weighted by 1980 MSA population. Standard errors are clustered at the MSA level. $^{*}P < 0.10$; $^{**}P < 0.01$; $^{***}P < 0.05$.

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Next, I consider whether the results reported in Table 7 are robust to the inclusion of a standard set of covariates. Each of the regressions reported in Table 7 is conditions on state and year fixed effects as well as city-specific linear time trends. I omit covariates in the main regressions because the standard covariates included in crime regressions may not be exogenous with respect to Mexican immigration. Nevertheless, it is instructive to test whether the estimated coefficients are sensitive to the inclusion of city-specific variables which may be correlated both with immigration and crime. Column (4) of Table 8 reports the results of a series of 2SLS regressions of the crime rate on instrumented Mexican immigration conditional on fixed effects, time trends, and a number of key covariates, each of which is theoretically linked with crime at the city level. The employment-to-population ratio and the poverty rate, computed using the Current Population Survey March supplements, capture fluctuations in each MSA's local economy . While not directly linked to crime, The 12th grade high school dropout rate in the MSA's largest cities (those with a population exceeding 50,000) is a proxy for changes in local conditions which may correlate with higher levels of crime.[37] The per capita number of sworn police officers with arrest powers comes from the UCRs police employees file and is included to capture the effect of changes in the level of law enforcement on the crime rate. Next, I include a series of demographic variables that capture changes in each city's population structure that are potentially correlated with the crime rate. These variables include the proportion of each city's population who are unmarried, the proportion who are white, black, and asian, and the proportion of the population in a granular set of age groups. The proportion of the population aged 16 through 24 are entered separately in the regression, as these are the peak ages of participation in criminal activity.

The primary takeaway from column (4) of Table 8 is that the coefficients reported in Table 7 are robust to the inclusion of these controls. For each of the nine crime rates, the estimates in column (4) are more positive than those in column (1) that do not condition on the controls but the substantive

(−1.81), burglary (−1.06), larceny (−2.82), and motor vehicle theft (−0.23). None of the coefficients are significant at conventional levels.

37. The dropout rate proxy in year t is 1 − the number of 12th graders in the city in year t relative to the number of 11th graders in the city in year $t - 1$. The data come from the National Center for Educational Statistics.

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findings remain unchanged.[38] With the exception of robbery, the coefficients do not differ from zero and in no case do the estimates in column (4) differ significantly from those in column (1).

In column (5) of Table 8, I test whether the results that I obtain can be explained by rainfall-driven changes in the age and gender composition of Mexicans. In particular, if rainfall serves to induce young males to migrate to a greater extent than older females, rainfall in Mexico might result in crime in U.S. cities due to positive selection on characteristics that are associated with crime. In order to check for the importance of such a mechanism, I re-specify the 2SLS regression models presented in Table 7 adding as an explanatory variable the change in the proportion of Mexicans in a given city who are males between the ages of 15–45. To the extent that changes in offending are purely driven by changes in the demographic composition of Mexican immigrants, this variable will capture the effect of these changes. In general, we should expect the coefficients to decrease in size with the inclusion of this control. When this variable is added to each of the crime models, while the resulting coefficients are, in general, slightly smaller than those reported in Table 7, they are extremely similar in magnitude, indicating that the results are not driven, to an appreciable degree, by changes in demographic composition. This finding is sensible as, conditional on year and city fixed effects and time trends, the remaining variation in the demographic composition of Mexican migrants is quite low.[39]

Finally, I consider the possibility that immigration may affect crime rates via a temporal lag. This might be the case, for example, if arriving migrants commence criminal involvement only after having failed to successfully integrate into local labor markets. In Table 9, I consider to test whether instrumented immigration in year t affects crime in years $t + 1$ and $t + 2$. Coefficients arising from these IV regressions which are population weighted and use log crime are presented in columns (2) and (3) of Table 9 alongside the basic estimates from Table 7 in column (1). Referring

38. Due to missing data, the sample size for the regressions reported in column (4) of Table 8 is slightly lower ($n = 736$) than in Table 7.

39. In addition, in an auxiliary analysis, I examine the degree to which the instruments predict the change in the Mexican immigrant share for each of ten age–gender groups. I find evidence that the eight instruments have predictive power for all ten age–gender groups, providing intuition that the instrument has not isolated an unusual local average treatment effect.

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to the models that operate using one lag, the estimates are broadly similar to the models that assume a contemporaneous relationship. With regard to the crime aggregates, the estimates remain imprecisely estimated, with standard errors of roughly identical magnitude to those presented in column (1). An exception is the effect of Mexican immigration on the murder rate which rises by approximately 3.6% in response to a one percentage point increase in the immigrant share in the previous year. Unlike the significant result for robbery, the result is robust to the exclusion of Los Angeles and Chicago, indicating a plausibly national affect. The effect of immigration on murder is likewise large, though imprecise, in twice-lagged models. These results leave open the possibility that Mexican immigration may be associated with serious criminal violence in the years following an immigration shock. Failure to detect significant effects in any of the other crime categories is consistent with an explanation that the effect may be associated either with gang violence or an increase in intimate partner homicides, rather than offending with an acquisitive motive.

5.4. Comparisons *w*th the Network Instrument

Given that the identification strategy employed in this research departs from the traditional "network" instrument by relying on rainfall variation, it is instructive to briefly consider how the estimated effects differ when the rainfall instrument is used as opposed to the network instrument. While the network instrument was pioneered for use with long-differenced data, it can nevertheless be constructed using annual data. Recall that the network instrument is calculated by multiplying the number of migrants who enter the United States from a given source region in a given year by a set of weights that capture the pre-determined conditional probability that a migrant sojourns to each destination. Summing over all source regions, the network instrument yields a predicted number of migrants that each destination region receives in each year. This incarnation of the network instrument was adapted by Altonji and Card (1991) to predict the number of immigrants entering each city in the United States between two given Census years. Using annual data, I construct a version of the network instrument that is consistent with the Altonji and Card version of the instrument by multiplying an estimate of the stock of Mexicans in the United States in each year

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Table 9. Two-Stage Least Squares Models of the Effect of Mexican Immigration on Crime Robustness to Functional Form

Year of crime measurement relative to immigration:	Year t	Year $t+1$	Year $t+2$
Violent Crimes	0.31	0.72	−0.48
	(0.99)	(1.13)	(1.30)
Property crimes	−0.05	0.49	−0.13
	(1.54)	(1.64)	(1.23)
Murder	0.57	3.60**	4.57
	(1.33)	(1.61)	(3.66)
Rape	−0.96	−0.77	−1.91
	(0.63)	(0.85)	(1.17)
Robbery	2.73**	2.72**	0.64
	(1.06)	(1.28)	(1.12)
Assault	−0.68	0.01	−0.42
	(1.35)	(1.56)	(1.82)
Burglary	1.23	1.28	−0.46
	(1.86)	(1.80)	(1.32)
Larceny	−0.95	−0.38	−0.48
	(1.64)	(1.74)	(1.34)
Auto theft	0.12	1.16	−0.42
	(2.19)	(2.08)	(1.65)

Note: The table reports coefficients and standard errors for a series of 2SLS regressions of the log of the number of crimes per 100,000 population on the Mexican population share. Mexican population share is instrumented using predicted rainfall-induced immigration. Column (1) reproduces estimates presented in column (2) of Table 7. Columns (2) and (3) assume that the effect of immigration on crime operates with one- and two-year lags, respectively. Regressions are weighted using 1980 MSA population weights. All models contain city and year fixed effects as well as city-specific linear time trends. Standard errors (in parentheses) are clustered at the city level. * $P < 0.10$; ** $P < 0.01$; *** $P < 0.05$.

by the proportion of Mexican migrants who settled in each U.S. city prior to 1986. Thus the instrument predicts Mexican immigration solely using the stock of Mexicans living in the United States and the predetermined network weights, without the benefit of rainfall.

The top row in Table 10 reproduces the basic 2SLS results using the rainfall instrument that are reported in Table 7. The bottom row of Table 10

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Table 10. Two-Stage Least Squares Models of the Effect of Mexican Immigration on Crime Comparing the Rainfall and the Network Instrument

Instrument	Violent crimes	Property crimes	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto theft
Rainfall instrument	0.31	−0.05	0.57	−0.96	2.73**	−0.68	1.23	−0.95	0.12
	(0.99)	(1.54)	(1.33)	(0.63)	(1.06)	(1.35)	(1.86)	(1.64)	(2.19)
Network instrument	0.40	−0.31	0.44	0.48	−0.10	0.69	0.69	−0.82	−0.40
	(1.27)	(1.45)	(0.89)	(0.62)	(1.29)	(1.80)	(1.41)	(1.66)	(1.08)

Note: The table reports coefficients and standard errors for a series of 2SLS regressions of the log crime rate on the instrumented Mexican population share. Two different instruments are employed. The top row reproduces the estimates from column (1) of Table 8 and reports the output from models in which the rainfall instrument is used to predict the Mexican population share. The bottom row of the table reports the output from models in which the "network" instrument for Mexico is used to predict the Mexican population share. Regressions are weighted using 1980 MSA population weights. All models contain city and year fixed effects as well as city-specific linear time trends. Standard errors (in parentheses) are clustered at the city level. * $P < 0.10$; ** $P < 0.01$; *** $P < 0.05$.

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reports estimates of the effect of Mexican immigration on crime using the network instrument for Mexico. On the whole, the results differ very little when the network instrument is employed with a notable exception: robbery. While the rainfall instrument identifies a positive effect of Mexican immigration on robbery (when Los Angeles is included in the data), the network instrument identifies no such positive effect. Differences between the rainfall and network instruments potentially have implications for the direction of selection bias of immigrants with respect to robbery, the direction of which is theoretically ambiguous. However, it is also possible that each instrument captures a different local average treatment effect. On the whole, the pattern of results suggests that both IV approaches lead to similar findings.

6. Conclusion

In this paper, I estimate the effect of Mexican immigration on the rate at which crimes are reported to the police in U.S. metropolitan areas. When I instrument for migration using rainfall shocks in network-linked Mexican states, the evidence suggests that Mexican immigration tends to be associated with neither higher nor lower levels of overall crime. Notably, this zero is precisely estimated as I can reject that a one percentage point increase in a city's Mexican immigrant share leads to greater than a one percent change in rates of either violent or property crimes. At the same time, I find evidence that Mexican immigration is associated with a modest increase in robberies, though the result appears to be entirely driven by Los Angeles. The results are robust to the inclusion of a series of standard control variables as well to controls for changes in the age and gender composition of Mexican immigrants as well as to a variety of specification checks. These results are broadly consistent with those in the extant literature, which has tended to report either null or weakly negative effects of immigration on crime. The estimates are also broadly consistent with the empirical regularity that changes in the demographic composition of cities tend to only weakly predict changes in crime, conditional on fixed effects.

To my knowledge, this is the first paper to exploit plausibly exogenous "push" variation in a source country to estimate the impact of immigration on crime. While my findings largely mirror those in the extant literature,

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they differ markedly from those of Spenkuch (2013) who studies the effect of immigration on crime using a county-level network instrument. There are several possible reasons why the results reported by Spenkuch differ from those reported in this research. One explanation is that the network instrument for Mexico employed by Spenkuch does not meet the exclusion restriction necessary to identify a consistent estimate of the effect of immigration on crime. This would be the case, for example, if immigration to the United States is responsive to the growth of crime markets in traditional U.S. destinations for Mexican immigrants. A second set of explanations for the divergent results may be found in the fact that each paper utilizes a different unit of analysis (counties versus MSAs) and uses data of differing granularity (long differences versus annual data). Finally, it is possible that the network and rainfall instruments simply estimate different local average treatment effects that are based on different samples of treatment compilers.

To the extent that rainfall mimics a random assignment mechanism in allocating immigrants to U.S. cities, this research helps to resolve any remaining skepticism regarding the identification strategies employed to generate past findings. Moreover, this research isolates the effect of Mexican immigration on crime, thus addressing a key source of contention in contemporary policy debates regarding appropriate immigration policy. Finally, as I estimate the effect of immigration on crime using annual data rather than data from the decennial Census, my estimates are considerably less exposed to potential bias arising from either internal migration of U.S. natives or reallocation of capital, either of which could be a dynamic response to an immigration shock.

For several reasons, estimates in this paper likely represent an upper bound on the criminality of immigrants. First, to the extent that recent Mexican immigrants tend to possess observable characteristics (e.g., lower rates of human capital and lower wages) that are typically associated with higher criminal propensities, it is plausible to conclude that if there is an economically meaningful effect of immigration on crime, it should be observable among Mexican migrants. Second, as the effect that I identify is a reduced form estimate of the effect of immigration on crime, is it possible that a portion of the observed effect is driven by increases in crime among natives, rather than among immigrants. This might be true, for instance, if Mexican immigrants are attractive crime victims or if Mexican immigrants

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destabilize employment markets for U.S. natives. Further research along these lines is needed. In particular, it is important to understand the apparently contradictory findings from the literature that examines the demographic characteristics of U.S. prisoners and the cross-city literature. While the former finds ample evidence that immigrants (including Hispanic immigrants) are less likely to be incarcerated than natives, the cross-city literature generally finds little evidence of any effect of immigration on crime. While this paper does not resolve this debate, it adds a critical data point to the cross-city literature.

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