Understanding the Impact of Immigration on Crime*

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Abstract

Since the 1960s both crime rates and the share of immigrants among the American population have more than doubled; and almost three quarters of Americans believe that immigration increases crime. Yet, existing academic research has shown no such effect. Using panel data on US counties, this paper presents empirical evidence on a systematic impact of immigration on crime. Consistent with the economic model of crime this effect is strongest for crimes motivated by financial gain, such as motor vehicle theft and robbery. Moreover, the effect is only present for those immigrants most likely to have poor labor market outcomes. Failure to account for the cost of increased crime would overstate the "immigration surplus" substantially, but it would not reverse its sign.

I. Introduction

Since the end of World War II the flow of legal immigrants into the US has steadily increased. Consequently, the share of immigrants in the whole population more than doubled between 1960 and 2000 (cf. Figure 1).[1] As this time period also coincided with a four-fold increase in violent crimes and a doubling of property crime rates, it may not be surprising that Americans hold strong opinions about the impact of immigration on crime. When asked what they think will happen as a consequence of more immigrants coming to the US, 73.4% of respondents to the General Social Survey in 2000 thought it was "very likely" or "somewhat likely" that crime rates would increase.

Although casual empiricism hints at a link between immigration and criminal activity, the empirical evidence is by no means unambiguous. As shown in Table 1, there exists a positive correlation between changes property crime and changes in immigration, but there appears to be no clear pattern for violent crime.

On theoretical grounds there are a priori reasons to believe that immigration may affect crime rates. But the economic theory of crime offers little guidance as to the size, or even the sign of the effect.

On one hand, theory predicts that, all else equal, individuals with lower outside options commit more crime. Low levels of education, low wages, higher levels of unemployment, and difficulties assimilating have all been documented for immigrants and can reasonably be associated with poorer outside options—at least if one regards legal labor market employment as the relevant margin. Furthermore, immigrants are disproportionately male and between the ages of 15 and 35. Existing research has shown these groups to be especially likely to be involved in criminal activity (Freeman 1999).

On the other hand, the expected costs of committing a crime are likely higher for immigrants. Not only do they face the same set of punishments as natives, but they are also subject to deportation, which may be an important deterrent. Moreover, immigrants might be positively selected on various unobservable dimensions, i.e. with respect to non-cognitive skills or industriousness, and may thus have an inherently lower propensity to commit crime than natives.

Another channel through which immigration may affect crime are spillover effects. Even if immigrants themselves commit fewer crimes than observationally similar natives, immigration could cause an increase in crime if it reduces natives' labor market opportunities inducing them to

[1] Substantial uncertainty surrounds estimates of the number of illegal immigrants. A common estimate is 12.5 million for 2007.

substitute toward criminal activity.[2] At the same time immigration may be associated with positive spillover effects. For instance, immigrants might move into and improve transitional neighborhoods by bringing social capital that is otherwise lacking.

Although there exist extensive literatures on the economics of immigration (reviewed in Borjas 1999) and on the economics of crime (see Freeman 1999 for a survey), relatively little is known about the impact of immigration on crime. The existing evidence relies in large part on incarceration rates as proxy for involvement in criminal activity, and is not always consistent.[3]

Moehling and Piehl (2009) study incarceration rates of immigrants and natives during the first half of the 20th century, uncovering only very small differences between the two groups. Similarly, Butcher and Piehl (1998a, 2007) find that since the 1980s immigrants are less likely to be incarcerated than natives, and attribute this finding to positive selection among immigrants. On the other hand, immigrants represent a disproportionate share of inmates with drug related offenses (Butcher and Piehl 2000). Grogger (1998) finds little evidence for spillover effects; but Borjas, Grogger and Hanson (2010) argue that immigration caused unemployment and a decline in wages among black men, thereby leading to an increase in incarceration rates for this group. The paper most closely related to the present one is Butcher and Piehl (1998b). In a panel of forty-three metropolitan areas during the 1980s, they find no effect of immigration on overall rates of crime as well as on violent crime rates.

There exists also a recent literature on the link between immigration and crime in European countries. Bianchi et al. (2012) study Italy, and Bell et al. (2011) look at the UK. Both papers show that immigration had no impact on violent crime in the respective receiving countries, but may have led to an increase in property crimes.

Using decadal panel data on US counties and UCR crime data, this paper contributes to the existing literature by presenting empirical evidence on the link between immigration and crime. The

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[2] Although most studies have found only small effects of immigration on native's wages and employment, it should be noted that this question has not yet been fully resolved in the literature (see Card 2001 and Borjas 2003 for opposing results).

[3] In the US Census information on the institutionalized population is highly unreliable, as it is often based on administrative data or imputed. In a review of the 2000 Census the National Research Council (2004) found that for 53.0% of the prison population information on country of birth had to be imputed. Jonas (2003) shows that only 19.7% of individuals in correctional institutions filled out the Census form themselves or were interviewed by a Census enumerator, while 56.3% of answers are based on administrative data, and 24.0% result in non-response. While the foreign-born are underrepresented among the institutionalized population in the Census, recent reports by different government agencies seem to contradict this fact. The Federal Bureau of Prisons (2009), for instance, reports that 73.5% of inmates in federal prisons are native born. This means that 26.5% must have come to the US as immigrants. One possible explanation is that immigrants are overrepresented in federal prison because they are disproportionately likely to commit drug related offenses. For an overview of existing data on the immigration status of prisoners and its limitations see Camarota and Jensenius (2009).

results presented below are broadly consistent with earlier work, but offer some new and interesting insights with respect to the last two decades and in terms of heterogeneity among immigrant groups.

Least squares estimates suggest a large positive and statistically significant effect of immigration on property crime. A 10% increase in the share of immigrants, i.e. slightly more than one percentage point based on current numbers, is estimated to lead to an increase in the property crime rate of 1.2%. To put this into perspective, an elasticity of .12 implies that the average immigrant commits roughly 2.5 times as many property crimes as the average native.

Point estimates of the elasticity of the violent crime rate with respect to the share of immigrants are only half as big in magnitude and sometimes negative, but statistically undistinguishable from zero. These estimates control for county and year fixed effects as well as for changes in a host of county characteristics over time, are robust to including county fixed effects in growth rates, and hold in various subsamples of the data.

The most important reason for why the results reported here are at odds with those of Butcher and Piehl (1998b) is that their sample covers only the 1980s, while this paper also considers the 1990s. As demonstrated below, the impact of immigration on crime is overwhelmingly concentrated in the latter period.[4]

Decomposing property crimes and violent crimes into their respective components—i.e. burglary, larceny, and motor vehicle theft for the former; murder, rape, aggravated assault and robbery for the latter—shows that immigration increases each type of property crime as well as robberies, but has almost no effect on rates of rape and aggravated assault. The point estimate with respect to murder is large and positive, but depends heavily on the weighting scheme.

Consistent with the economic model of crime, it appears that immigration primarily increases crimes motivated by financial gain. Moreover, splitting up immigrants into those from Mexico and "all others" reveals that the effect is only present for the former group. As immigrants from Mexico are particularly likely to experience poor labor market outcomes, this finding is consistent with the economic model of crime as well.

Despite the robustness of this pattern and its concordance with the predictions of economic theory, thorny issues of causality remain. Measurement error in the number of immigrants, omitted variables, and endogeneity in immigrants' settlement patterns could all bias the least squares estimates.

[4] Moreover, Butcher and Piehl's estimates control for cities' racial composition, in particular the fraction of Hispanics. As Table 7 shows, immigrants groups other than Mexicans have, indeed, no measurable impact on crime.

Following Altonji and Card (1991) and Card (2001) this paper instruments for the actual change in the number of immigrants with a prediction thereof based on ethnic differences in settlement patterns (Bartel 1989). The prediction exploits geographic and ethnic dispersion in the distribution of immigrants across counties as well as the changing ethnic composition of immigrants. Intuitively, validity of the instrument requires that differences in the geographic distribution across immigrant groups, and total inflows of different groups are uncorrelated with shocks to crime in particular counties. The resulting two stage least squares estimates are in line with the basic OLS pattern. That is, immigration has a large positive impact on property crime, but not on violent crime.

Back-of-the-envelope calculations suggest that the social cost of increased crime due to a counterfactual 10% percent increase in the fraction of immigrants amount to as much as 1 billion dollars per year. Despite substantial uncertainty associated with this cost estimate, it alone is far too small to outweigh the welfare gains to immigration produced elsewhere in the economy.[5]

The remainder of the paper proceeds as follows. Section II explains mechanisms by which immigration can be expected to affect crime. Section III describes and summarizes the data, followed by the main results presented in Section IV. Section V discusses implications for public policy, and Section VI concludes. A Data Appendix with the precise definitions and sources of all variables used in the analysis is available on the author's website.

II. Mechanisms by Which Immigration May Affect Crime

There are multiple mechanisms by which an increase in the number of immigrants may affect crime. The first and most obvious one is a purely mechanical population effect. Since the expected per capita number of committed crimes is positive, an influx of immigrants increases the total *number* of crimes simply because it increases the population.

As it is not clear whether policy makers should be concerned about population effects, and since data on the immigration status of victims is unavailable, the empirical work in this paper does *not* take population effects into account. Instead it focuses on the relationship between crime *rates* and the *share* of immigrants.

[5] It has long been recognized that, all else equal, immigration generates an increase in natives' welfare through its impact on the labor market (see the exposition in Borjas 1999). Immigration is also likely to increase ethnic diversity in goods and services, and may thereby lead to an additional increase in natives' utility (see Lazear 2000). On the other hand, immigrants have been found to be more reliant on government transfers (e.g. Borjas and Hilton 1996). The National Research Council (1997) estimates the fiscal impact of immigration and finds that each immigrant initially creates a burden for the taxpayer. This burden, however, turns into a large *surplus* over the long run.

Crime rates could be affected by composition effects. Immigrants are disproportionately male and between the ages of 15 and 35 (US Census Bureau 2009), and these population groups are well known to be involved in criminal activity more frequently than others (Freeman 1999).[6] Therefore, an influx of immigrants can be expected to increase crime rates—even if conditional on observables natives and immigrants have equal propensities to commit crime.

Becker's (1968) seminal work on the economic theory of crime points to two other mechanisms by which immigration can be expected to affect crime. In Becker's words,

> «… a person commits an offense if the expected utility to him exceeds the utility he could get by using his time and other resources at other activities. Some persons become "criminals," therefore, not because their basic motivation differs from that of other persons, but their benefits and costs differ.» (Becker 1968, p. 176)

Thus, immigration will have an impact on crime if immigrants' outside options differ from those of natives, or if the expected utility from committing a crime differs between the two groups. Not only does the total number of crimes change in such a case, but the crime rate changes as well. The crime rate increases if the *marginal* immigrant commits more crimes than the *average* American.

While the marginal immigrant is hard to determine empirically, there is ample evidence that the average immigrant's outside option is worse than that of the average native. On average immigrants are less educated, have lower incomes, and are less proficient in English than Americans (US Census Bureau 2009). These facts suggest that immigrants' returns from participation in the formal labor market are on average lower than those of natives. Thus, a rise in the share of immigrants in the population may lead to an increase in crime rates. As participation in the formal labor market is a more relevant outside option for crimes motivated by financial gain than for "crimes of passion", it is reasonable to expect this mechanism to increase the property crime rate, but not necessarily the rate of violent crime.

On the other side, expected costs of committing a crime are arguably higher for immigrants. Not only do they face the same set of punishments as natives, they are also subject to deportation. Losing the right to reside legally in the US may be an important deterrent. Moreover, immigrants might be positively selected on various unobservable dimensions, i.e. with respect to non-cognitive skills or industriousness, and may thus have an inherently lower propensity to commit crime than natives.

[6] Representing only 28% of the population, individuals between the ages of 15 and 35 accounted for 64% of all arrests in 2002. Of all persons arrested in 2002 for violent crimes 83% were male, as were 69% of those arrested for property crimes (US Bureau of Justice Statistics 2004).

Another channel through which immigration may affect crime are spillover effects. Borjas, Grogger and Hanson (2010) argue that immigration caused a decline in wages and employment among black men and thereby led to an increase in incarceration rates for this group. Thus, immigration could cause an increase in crime rates, even if immigrants commit fewer crimes than observationally similar natives. However, immigration may also be associated with positive spillover effects. For instance, immigrants might move into transitional areas and improve their neighborhoods by bringing social capital that is otherwise lacking (Putnam 2000).

In sum, there are a priori reasons to believe that immigration does affect crime rates. The direction of the effect, however, is theoretically indeterminate.

III. Data Sources and Summary Statistics

The data set used in this paper is a decadal panel of county level observations running from 1980 to 2000.[7] With a few exceptions concentrated in the states of Alaska and Virginia, borders of counties and county equivalents have remained mostly static during this period (US Census Bureau 1994).[8] By contrast the Census Bureau has used different definitions to describe metropolitan areas, resulting in a lack of comparability across decades.[9] Although the majority of immigrants reside within metropolitan areas, lack of comparability over time might be an important confounding factor that makes county level data preferable to data at the city or MSA level. Information on county characteristics in various years is provided by the US Census Bureau in its *City and County Data Books* and *USA Counties*. Both publications contain a collection of data from the Census Bureau itself and other government agencies based on, for instance, *Decennial Censuses of Population and Housing, Censuses of Government*, *Economic Censuses*, etc. One important limitation of relying on decadal Census data is that the analysis will only pick-up long-run effects, instead of year-to-year variation in immigration and crime.

As is customary in the literature on immigration, a person's place of birth is used to determine his immigration status. That is, the number of immigrants in a county equals the foreign born population. The Census attempts to collect information from every resident in the US, including

[7] While the FBI started collecting crime statistics in 1930 and makes data available for years as early as 1960, data for earlier years is problematic due to low reporting rates of police agencies. Although immigrant inflows started to increase after World War II, abolishment of national-origin quotas through the *Immigration and Nationality Act of 1965* is usually associated with the beginning of the most recent immigration episode.

[8] Counties which could not be matched consistently over time have been dropped from the analysis. The final sample includes 3,117 counties. See the Data Appendix for further details.

[9] See Jaeger et al. (1998) for a detailed explanation of different definitions and an attempt at creating a time consistent mapping.

illegal aliens. Yet, there remains uncertainty about the exact number of legal and illegal immigrants, particularly among Hispanics (Hogan and Robinson 1993).[10] Therefore, the number of foreign-born in a county is likely to be measured with error.

Dollar values have been converted to 2000 dollars using the Consumer Price Index for All Urban Consumers, and control variables, for which no information is available in a particular year, have been linearly interpolated based on the two closest available years. The Data Appendix names the exact source of each variable used throughout the paper and provides a detailed description of the data construction procedures.

All measures of crime are based on agency level data on the number of crimes reported to the police, as compiled by the Federal Bureau of Investigations (FBI) in its *Uniform Crime Reporting* program.[11] Reported crime data are available for the seven Index I crimes: murder, rape, aggravated assault, robbery, burglary, larceny, and motor vehicle theft. Precise definitions are provided in the Data Appendix. The first four of these are classified as violent crimes; the latter three are denoted property crimes. The number of crimes reported to the FBI each month have been adjusted for non-reporting by agencies to yield a reliable yearly measure, and have been aggregated to the county level.[12]

On theoretical grounds data on actual victimizations would be preferable to reported crimes. However, such data are unavailable at the required level of geographic disaggregation.[13] Since crime serves as dependent variable, underreporting and, more generally, measurement error in the number of crimes will not bias the point estimates unless it is correlated with one of the independent variables. If, for instance, immigrants were less likely to report a crime than natives, then the point estimates would be biased downwards; thus understating the impact of immigration of crime.[14] The fact that the estimated elasticities are robust to using first differences instead of levels, even controlling for county fixed effects in growth rates, makes it unlikely that classical measurement error drives the results.

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[10] In the 1990 Census, for instance, the undercount rate is estimated be 5% for Hispanics compared to .7% for Whites (Hogan and Robinson 1993).

[11] The results are qualitatively and quantitatively robust to using data on arrests instead of reported crimes. Data on arrests are available by race, age, and gender, but no information on the immigration status of the offender is available.

[12] Again, the Data Appendix provides a detailed description of this procedure. The results presented in this paper are robust to using different adjustment mechanisms.

[13] O'Brien (1985) and Gove, Hughes, and Geerken (1985) provide opposing views on the validity of reported crime data.

[14] There is also the less plausible possibility of increased reporting in counties with a high share of immigrants, leading to an overstatement of the effect of immigration on crime.

Summary statistics based on the raw, unweighted data for all variables used throughout the analysis are presented in Table 2. There exists large variation in crime rates across counties and over time. Most violent crimes are aggravated assaults, while the majority of property crimes are larcenies. Crime rates increase until the late 1980s, or early 1990s and decline thereafter.[15] In most cases their variance follows a similar pattern.

The fraction of immigrants exhibits substantial variation across counties, too. As many new immigrants settle in major cities, the share of immigrants increases much faster in the right tail of the distribution; causing it to spread out (see also Figure 1). Over most of the sample period 90% of all counties' immigrant share is lower than the national average. This explains the relatively small mean and its modest increase in Table 2.

Table 2 also shows that most counties are not very populous, and the majority of them lie in the South and Midwest. The imbalance in population and the number of immigrants across counties necessitates the use of appropriate weights in the analysis to follow.[16]

IV. Estimating the Impact of Immigration on Crime

A. Econometric Approach

The preceding discussion suggests a relationship between immigration and crime rates. In what follows this relationship is explored more systematically by using panel data regressions to relate the share of immigrants to county-level crime rates. The parameter of interest is the elasticity of the *rate* of crime with respect to the population *share* of immigrants, which is identified by η in the following linear model:

$$(1) \qquad \ln(crime_{c,t}) = \eta \ln(immigrants_{c,t}) + \beta \ln(population_{c,t}) + X'_{c,t}\gamma + \mu_c + \tau_t + \varepsilon_{c,t},$$

where $crime_{c,t}$ denotes the total number of incidences of a particular crime in county c during year t, $immigrants_{c,t}$ and $population_{c,t}$ are the total number of immigrants and residents, respectively;

[15] As the data in Table 2 is not weighted, crime rates displayed therein do not match those published by the FBI in *Crime in the United States*.
[16] Tables 8A and 8B demonstrate that the results are qualitatively robust to different weighting schemes.

$X_{c,t}$ is a vector of additional county level covariates, μ_c denotes a county fixed effect, and τ_t a year fixed effect. The error term is given by $\varepsilon_{c,t}$.[17]

Equation (1) is estimated by weighted least squares using county population as weights. Standard errors are clustered at the state level to allow for arbitrary patterns of correlation in error terms over time and across counties within a state.

The full set of additional county level covariates consists of controls for changes in demographics, police enforcement, economic conditions, as well as quality and availability of housing. County fixed effects absorb characteristics that are constant over time.

Covariates controlling for changes in demographic composition are the fraction of residents that are female and the median age of the population. The natural logarithm of police expenditure per capita and the log of the rate of institutionalization proxy for police enforcement; while the fraction of families below the poverty line, logged median household income, payroll per capita, and the unemployment rate proxy for economic conditions. The number of new building permits per existing unit, the fraction of housing units that are vacant, the fraction of owner occupied units, as well as the median rent and value of housing units control for factors affecting the quality and availability of housing.

In choosing covariates one must be cautious not to control for endogenous factors. For instance, immigrants and natives do differ on observables such as age, race, ethnicity, and income. By fully controlling for these characteristics η would not reflect the true effect of immigration on crime any more. On the other hand, characteristics of a county's population may change over time for reasons unrelated to immigration. To the extent that these characteristics are correlated with crime one needs to control for them in order to obtain unbiased estimates. The particular set of covariates chosen tries to strike a balance between these two conflicting objectives. The results, however, are not sensitive to specific controls.

At this point it is useful to point out how η is identified. By including county and year fixed effects in the econometric model only within county variation from national patterns over time identifies the coefficients. This means that unobserved county characteristics that are constant over time, or year effects common to all counties cannot bias the point estimate of η. Only unobservables that do vary over time and across counties are a potential source of bias. For instance, new

[17] To see that η is the elasticity of the *rate* of crime with respect to the population *share* of immigrants rearrange (1) to yield: $\ln\left(\frac{crime_{c,t}}{population_{c,t}/100{,}000}\right) = \eta \ln\left(\frac{immigrants_{c,t}}{population_{c,t}}\right) + (\beta + \eta - 1)\ln\left(population_{c,t}\right) + X_{c,t}^{'}\gamma + \mu_c + \ln(100{,}000) + \tau_t + \varepsilon_{c,t}$. Regarding the parameter of interest the two specifications are equivalent.

immigrants might, ceteris paribus, be less likely to settle in a county experiencing a crime shock. Section IV.C addresses these issues.

B. Main Results

Table 3 presents a series of estimates of η, the elasticity of crime with respect to immigration. The dependent variable in columns (1)–(7) is the natural logarithm of the number of property crimes, while that in columns (8)–(14) is the log of the number of violent crimes. Consequently, the coefficients on Log Immigrants in the first seven columns identify the elasticity of the property crime rate with respect to immigration, and the coefficients in the last seven columns identify that of the violent crime rate. The set of fixed effects and the vector of other covariates included in the regression, i.e. μ_c, τ_t, and $X_{c,t}$, varies across columns. Moving from left to right the set of controls grows steadily.

Columns (1) and (8) show the relationship between crime rates and the share of immigrants without accounting for fixed effects or any other covariates besides population. The high R^2 indicates that population size explains most of the variation in the number of crimes across counties. Adding year fixed effects in columns (2) and (9) increases the coefficients, but does not change their signs and explains little additional variation in crime. In no case is the coefficient of interest statistically different from zero.

Controlling for unobserved heterogeneity across counties by including county fixed effects changes the picture dramatically. Unobservable county characteristics are highly correlated with residual variation in the share of immigrants. This suggests that controlling for unobservable county characteristics is important in obtaining unbiased parameter estimates. The effect of immigration on violent crime rates is now positive, but is estimated imprecisely. The effect on property crime rates is roughly twice large, positive, and statistically significant.

Additional controls for demographics as well as for police enforcement have little effect. The same is true for controls related to economic conditions, despite the fact that one might expect economic prosperity to be negatively correlated with crime. In the economic model of crime (Becker 1968) changes in macroeconomic conditions have an indeterminate effect on crime rates, as they possibly affect both criminals' outside options as well as the returns to crime. That is, there simultaneous "supply" and "demand" effects. The best available empirical evidence suggests that changes in macroeconomic conditions have only a small impact on crime rates (see Levitt 2004 and the studies cited therein). Nevertheless, given the obvious endogeneity concerns and the fact that many of the controls are highly correlated, one should be very cautious in giving the elements of γ a causal interpretation.

One might also argue that immigrants are more dependent on affordable housing than natives, and that high crime rates depress housing prices. Failing to control for this effect would bias the estimated elasticities upwards. Therefore, columns (7) and (14), which display the results of the preferred specification, also include proxies for quality and availability of housing.

The point estimate of η for violent crimes is .065 and statistically indistinguishable from zero. The elasticity of property crime rates with respect to the share of immigrants, however, is estimated to be .123 and is statistically significant. This constitutes an economically large effect. Taken at face value, a 10% increase in the share of immigrants would lead to an increase in the property crime rate of circa 1.23%.

To put this number into perspective, Levitt (1997) finds that a 10% increase in the number of sworn police officers reduces property crime rates by 2–4%; and Levitt (1996) estimates the elasticity of property crime rates with respect to the prison population to be -.321, i.e. a 10% increase in the number of prisoners decreases property crime rates by circa 3.2%. To gauge the size of the estimates it is also useful to convert them into relative crime rates.[18] By this measure an elasticity of .123 implies that immigrants commit circa 2.5 times as many crimes as the average native.[19]

An alternative way of estimating the elasticity of crime rates is by using first differences, i.e.

$$(2) \qquad \Delta \ln(crime_{c,t}) = \eta \Delta \ln(immigrants_{c,t}) + \beta \Delta \ln(population_{c,t}) + \Delta X_{c,t}^{'} \gamma + \Delta \tau_t + \Delta \varepsilon_{c,t}$$

where Δ denotes the difference between year t and $t-10$ for the variable following it, and η is again the parameter of interest.[20] With access to only two time periods the parameter estimates of model (1) and model (2) would be algebraically equivalent. Given multiple periods differences arise; especially in the presence of measurement error, which generally affects the first differences model

[18] To do so one solves a system of two equations in two unknowns, i.e. the crime rate of immigrants and that of natives, in which the two equations are given by $\#immigrants \times crime\ rate_{immigrants} + \#natives \times crime\ rate_{natives} = \#crimes$ and $(1+\delta) \times \#immigrants \times crime\ rate_{immigrants} + (\#natives - \delta \#immigrants) \times crime\ rate_{natives} = (1+\delta\eta)\#crimes.$ The second equation gives the number of crimes after a counterfactual increase in the fraction of immigrants by δ (and a corresponding decrease in the fraction of natives). η denotes the estimated elasticity of crime with respect to the share of immigrants. All other variables take on their respective values for 2007.

[19] If differences in arrest rates by gender and age are the same for immigrants and natives, then composition effects can account for at most a ten percent difference in crime rates.

[20] In order to avoid losing circa one third of the sample, if available, data from 1970 are used to calculate differences between 1980 and 1970. Results are qualitatively robust to restricting the sample to that used in Table 3.

more severely. The fact that the estimated elasticities in columns (1) and (3) of Table 4 are close to those in Table 3, or even larger, suggests that measurement error is not a substantial problem.[21] Some of the other coefficients change sign and vary in size, but are often estimated imprecisely.

Columns (2) and (4) in Table 4 add county fixed effects to the model in equation (2). This has the interpretation of controlling for county specific growth rates. Individual counties could be on very different trajectories, which might influence settlement of patterns of forward-looking immigrants, and thus be a source of bias. Controlling for county specific growth rates does not alter the estimated effect of immigration on violent crimes. It remains close to zero. The estimated elasticity for property crimes decreases slightly, but is very similar to that shown in Table 3 and statistically significant. It appears that controlling for existing trends does not change the results in a meaningful way.

To facility comparison with previous work Table A.1 in the appendix displays point estimates for models that include the share of immigrants instead of its of logged value. Since the data reject a linear relationship between immigration and crime rates, these models use a quadratic specification.[22] The sign pattern of the estimates suggests no, or even small negative effects for counties experiencing only small changes in immigration, but large positive effects for those counties receiving the most immigrants (relative to their respective population). Although the estimates are less precise than their log-log counterparts, not controlling for county specific trends it is still possible to reject the null hypothesis of no effect on the 1%-confidence level for property crime. For results from more more flexible, semi-parametric specifications see Figure 2 in Section IV.E.

C. Causality

The evidence on the impact of immigration on crime presented so far is only correlational. A causal interpretation of the key parameters requires the residuals in equations (1) or (2) to be uncorrelated with the (actual) log of number of immigrants, conditional on all other covariates.

There are at least three reasons why this may fail. The first one is measurement error in the number of immigrants. Most forms of measurement error would attenuate the estimated elasticities, thus masking the impact of immigration on crime.[23] If, however, the Census systematically undercounts the population in places that have more (illegal) immigrants, then the estimates in Tables

[21] Strictly speaking this is only true for classical errors in variables. More general patterns of measurement error may still be present and bias the point estimates.

[22] Based on the results in Table 4 it is possible to reject a linear relationship on the 10%-confidence level for property crimes and on the 5%-level for violent crimes. Using the empirical model in equation (1) instead one would reject the linearity assumption on the 1%-level for property crimes and on the 5%-level for violent crime.

[23] This results is based on the assumption of classical errors in variables. It can be shown, however, that attenuation bias will often result from non-classical measurement error as well.

3 and 4 might be upward biased. The second reason is omitted variables bias. For omitted variables bias to be a problem there must be some variable not accounted for in the empirical model for which, conditional on all other covariates, the deviations from its county specific mean and the national average in a given year are correlated with those deviations for the log number of immigrants and the deviations of crime. Depending on the signs of these partial correlations the point estimates might be upward or downward biased. The third reason is endogeneity in the settlement pattern of immigrants. All else equal, one would expect immigrants to settle in counties with lower crime rates. This would introduce a negative correlation between the residual and the share of immigrants, and bias the point estimate downward.

Estimation using instrumental variables (IV) provides a way to avoid aforementioned problems and obtain an estimate of the *causal* effect—at least if measurement error is classical and heterogeneity in effects is absent. With heterogeneity in effects, IV estimates a local average treatment effect (Imbens and Angrist 1994). That is, the point estimate is a weighted average of marginal effects, with groups for which the instrument is a better predictor receiving more weight.[24] For more general forms of measurement error it is easy to show that the IV estimate are often upward biased—in contrast to the OLS ones, which are generally attenuated. Despite the inherent problems with IV, it is desirable to test whether the results for the impact of immigration on crime hold up.

Consistency of the first differences estimator in equation (2) requires an instrument that is correlated with the change in number of immigrants in county c at time t, but does not influence the period t change in crime in c except through changes in the number of immigrants. More precisely, for some $Z_{c,t}$ to be a valid instrument it must be the case that $Cov\left[Z^*_{c,t}, \Delta \log\left(immigrants_{c,t}\right)^*\right] \neq 0$ and $Cov\left[Z^*_{c,t}, \Delta\varepsilon_{c,t}\right] = 0$, where * denotes the residual variation in the respective variable.[25]

Previous studies in the immigration and wage literature have recognized immigrants' tendencies to settle in ethnic clusters (Bartel 1989), and used it to predict current period inflows of new immigrants (i.e. Altonji and Card 1991, Card 2001). The current paper follows this approach. That is, the predicted change in the logarithm of the number of immigrants from year $t-10$ to t is used to instrument for the actual change.

[24] Heckman, Urzua and Vytlacil (2006) derive the IV weights and show that some of these weights might even be negative, in particular if the monotonicity condition of Imbens and Angrist (1994) fails.

[25] In contrast to the first differences estimator, consistency of the fixed effects estimator in equation (1) requires *strict* exogeneity of the instrument. That is, it requires $Cov\left[Z^*_{c,t}, \varepsilon_{c,s}\right] = 0$, for all s, t.

Predicted changes in the number of immigrants are derived based on the assumption that the distribution of new immigrants across counties will be the same as the distribution of immigrants of their own ethnic group twenty years prior. The instrument therefore exploits geographic and ethnic dispersion in settlement patterns as well as the changing ethnic composition of immigrants. The Data Appendix provides an exact description of how the instrument was constructed.[26]

Column (1) of Table 5 shows the first stage. While the first condition for a valid instrument undoubtedly holds, i.e. the instrument is a strong predictor of $\Delta \log\left(immigrants_{c,t}\right)^{*}$ in the sense of Stock and Yogo (2005), it is less clear that the second condition is satisfied. Intuitively, validity of the exclusion restriction requires that variation in the geographic distribution of different immigrant groups, and yearly inflows of these groups on the national level are uncorrelated with time varying shocks to crime in individual counties. Although plausible, this condition is fundamentally untestable.

In particular, one might be concerned about endogeneity in where immigrants choose to settle. New immigrants might, for instance, follow previous cohorts and move to counties where crime rates are falling. On the other hand, one might argue that immigrants are forced to move into relatively bad areas in which crime is on the rise. To the extent that the included housing controls do not capture this adequately, and if there is sufficient autocorrelation in county specific deviations from mean crime rates, then the IV estimates might be downward (former case) or upward (latter case) biased.[27]

If, however, one accepts the exclusion restriction, then the last two columns in Table 5 display estimates of the causal effect of immigration on crime. Although the IV point estimates are estimated quite imprecisely, they are reassuringly close to their OLS counterparts. Taking the IV estimates at face value it appears that the original set of conclusions continues to hold.

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[26] Briefly, in creating the instrument the set of countries in the raw data has been aggregated into nine groups: Northwestern Europe, Eastern Europe, Southern Europe, Asia, Mexico, South and Central America, Africa, Canada, and all other countries. Aggregation is required as only a subset of source countries is consistently identified in the raw data. Moreover, aggregation has the advantage of lessening measurement error, which is almost surely present in the number of immigrants from any individual country. County c's *predicted* total number of immigrants in year t is defined as

$$\hat{immigrants}_{c,t} = \sum_{g}\left[\left(\sum_{c} immigrants_{c,g,t}\right)\left(immigrants_{c,g,t-20} \Big/ \sum_{c} immigrants_{c,g,t-20}\right)\right],$$

where t indexes years, and g denotes one of the nine source country groups. The predicted change in the natural logarithm of the number of immigrants from year $t-10$ to t, e.g. the actual instrument used in the paper, then equals

$$\Delta \log\left(\hat{immigrants}_{c,t}\right) = \log\left(\hat{immigrants}_{c,t}\right) - \log\left(\hat{immigrants}_{c,t-10}\right)$$

[27] Specifications similar to those in columns (1) and (3) of Table 4 provide little evidence that lagged changes in crime drive changes in the immigrant population of counties.

D. Implications of the Economic Model of Crime

As hinted above, economic theory predicts that the effect of immigration on crime depends on the difference in outside options between natives and immigrants. This means that there should be an effect of immigration primarily on those crimes for which there exists a clear difference between immigrants and natives, and that the effect should be larger for immigrant groups whose outside options are lower.[28]

One can take a closer look at the effect of immigration by estimating the respective elasticities for different *types* of property and violent crimes. Table 6 presents OLS and IV elasticity estimates with burglary, larceny, motor vehicle theft, murder, rape, aggravated assault, and robbery as the dependent variables. Given the noise in the IV point estimates and the fact that the results in Tables 4 and 5 match up relatively closely, one might want to put more weight on their more precise OLS counterparts.

The OLS point estimates suggest that immigration increases all three types of property crime as well as robberies. The estimates for these crimes range in size from .115 to .274 and are each statistically significant. The effect of immigration on the remaining violent crimes is less clear. While η is positive, small, and statistically insignificant for rape as well as aggravated assault, it is positive, marginally significant, and sizeable for murder. With the exception of murder and burglary, the IV estimates are again similar to their OLS counterparts. Thus, taking the OLS and IV estimates at face value one would draw the same qualitative conclusion for robbery, larceny, burglary, car theft, and aggravated assaults. With respect to murder and rape, however, the evidence is ultimately inconclusive. Broadly summarizing, after accounting for uncertainty, immigration seems to increase rates of crimes motivated by financial gains, but not "crimes of passion".

Previous research has documented that immigrants from Mexico have significantly worse labor market outcomes than not only natives, but also other immigrant groups (see, for instance, Borjas and Katz 2007). With this motivation in mind, Table 7 shows OLS elasticity estimates with respect to immigrants from Mexico as well as "all other" immigrants. The estimated effect on crime of Mexican immigration is positive, and statistically significant for all crimes motivated by financial gain. The effect of "all other" immigrants, however, is in all cases negative.[29] Taken at face value the

[28] Other factors, such as the severity of punishment, might also differ between natives and immigrants and might mask the effect due to differences in outside options.

[29] Careful readers will note that in Table 7 the point estimates for either group are smaller than the estimated elasticity for the whole sample (shown in Table 3). Although it might appear that way, OLS point estimates for some joint sample need not be weighted averages of estimates for the respective subsamples (cf. Theil 1950 and Yitzhaki

estimates for "Mexicans" suggest that an additional 1.2 million immigrants from Mexico (i.e. 10% of the stock based on 2010 numbers) would increase property crime by almost .7% or by about 21 incidents per 100,000 inhabitants. Note, as "Mexicans" are markedly overrepresented among recent legal and illegal immigrants, the estimates in Table 7 line up reasonably well with their counterparts in Table 2, which suggested that a 10% increase in overall immigration would lead to an increase in property crime of about 1.2%. Converted into crime rates, the estimates in Table 7 imply that immigrants from Mexico commit between 3.5 and 5 times as many crimes as the average native, while "all other" immigrants commit less than half as many crimes as natives.[30]

E. Sensitivity and Robustness

Tables 8A and 8B explore the sensitivity of the estimated elasticities across different specifications and a wide variety of subsamples of the data. Only coefficients on Log Immigrants and associated standard errors are reported. The first row in each table displays the baseline results, i.e. those from the preferred specification.

The following two rows show that weighting has little influence on the point estimates, although it does decrease them. In particular, results corrected for missing observations by inverse probability weighting (IPW) are almost identical to the baseline results.[31] Weighting seems to matter only for the elasticity of murder with respect the share of immigrants.

In general, the estimates for murder vary widely across specifications and samples. However, those for other types of crime are much more robust, especially those for crimes motivated by financial gain.

Splitting the sample up by year and analyzing each cross-section separately shows that the effect of immigration on property crimes is concentrated in the period from 1990 to 2000. This is consistent with existing evidence on lower labor market returns for this later cohort of immigrants (e.g. Borjas 1990).

1989). One reason for this surprising difference is that the estimated fixed effects as well as the trends terms differ across samples. This means that depending on the specification and on the sample the residual variation in crime rates and the share immigrants that identifies the coefficient of interest will be different.

[30] A potential concern with these estimates is that Mexicans might not be as able as other immigrant groups to move away from areas that are on the decline. One way to address this issue is to directly control for changes in a counties population. If natives or other immigrant groups were indeed more able to move out of areas that experience higher rates of crime, then one would expect this to be reflected in changes in population. Yet, controlling for changes in population has practically no effect on the coefficients of interest.

[31] IPW weights each observation by the inverse of the predicted probability of having a non-missing value. This is a valid non-parametric correction procedure if the probability of an observation containing missing information does not depend on unobservables.

Of the 124 estimated elasticities for burglary, larceny, motor vehicle theft, and robbery, only 9 do not carry the expected sign, e.g. are negative. If all coefficients were independently distributed—which is an obvious oversimplification—the probability that 9 or fewer of them would be negative is effectively zero if immigration had no effect on crimes related to monetary gain. Thus, one would reject the null that the elasticity of these crimes with respect to the share of immigrants is non-positive.[32] Of the 93 estimated elasticities for murder, rape, and aggravated assault, however, 29 are negative. While this is less than the expected value under the null with independently distributed coefficients (and would still lead to rejection of the null), once one takes into account that the estimates are probably positively correlated the null appears less implausible.

There is some evidence that the effect of immigration on crime in the Northeast differs from that in the rest of the country. With the exception of robbery, all elasticity estimates are negative for this region. One admittedly unsatisfactory explanation is that the Northeast receives proportionately fewer immigrants with poor labor market prospects. For instance, the fraction of immigrants from Mexico is lower in the Northeast, than in the South and the West. However, Tables 8A and 8B also show that the estimated effects are not solely driven by the "classical" immigration states California, Texas, and Florida. In line with the results from Table A.1, the estimates in Tables 8A and 8B demonstrates that the effects are driven by those counties receiving the largest influx of immigrants, i.e. by counties falling into the second and third tercile of immigrants received between 1960 and 2000.

Lastly, Figure 2 provides additional robustness checks of the main results with respect to the log-log specification in equations (1) and (2). The log-log functional form posits that the effect of a percentage point increase in the share of immigrants has a different effect depending on the base share. On theoretical grounds there is, of course, no clear reason to prefer this specification to one in levels, i.e. one that posits a constant effect.

Elementary specification tests, however, strongly reject the assumption of a constant effect. That is, the *data* suggest a relationship that is non-linear. To impose as little structure as possible Figure 2 displays semi-parametric estimates of functional relationship between immigration and crime. More specifically, the left panels estimate

[32] To see this, note that if the effect of immigration on these crimes is zero, then the probability of one coefficient being negative is one half, and the probability of any number of them being negative is binomially distributed. The probability that 17 or fewer of them are negative is given by $\Pr(\# \leq 9) = \sum_{j=0}^{9} B(j, .5)$,where $B(j, .5)$ denotes the binomial probability mass function for j successes given the respective number of tries and a success probability of .5.

(3a) $\Delta \ln(crime\_rate_{c,t}) = f(\Delta \ln(share\_immigrants_{c,t}))$

$$+\beta\Delta \ln(population_{c,t}) + \Delta X'_{c,t}\gamma + \Delta\tau_t + \Delta\epsilon_{c,t},$$

whereas those on the right correspond to

(3a) $\Delta crime\_rate_{c,t} = f(\Delta share\_immigrants_{c,t})$

$$+\beta\Delta \ln(population_{c,t}) + \Delta X'_{c,t}\gamma + \Delta\tau_t + \Delta\epsilon_{c,t}.$$

In both specifications the only restriction placed on $f(\cdot)$ is that it is a continuous function of its one argument. Thus, $f(\cdot)$ will capture any possible nonlinearities in the relationship between changes in immigration and changes crime. For details on the estimation procedure see Yatchew (1998).

As was the case in the previous (parametric) analysis, there is no clear evidence for an effect of immigration on violent crime, independent of whether immigration and crime are measured in logs or levels. In both cases, there are regions in which the slope of $f(\cdot)$ is positive and ones in which it is negative. Overall, in either of the lower two panels one cannot reject the null hypothesis of a *zero* slope at *all* points.

This not the case when it comes to property crimes, as evidenced by the upper two panels. For about 90% of counties the slope of $f(\cdot)$, and hence the impact of immigration on property crime, is positive. Moreover, the confidence bands are narrow enough to rule out no effect at all.

Interestingly, and somewhat at odds with the previous conclusions based on the log-log specification when both crime rates and immigrant shares are measured in levels there seems to be a negative relationship between immigration and crime for the 10% of counties that experienced that largest immigration shock. Unfortunately, due to the relatively small number of counties in that region and the large standard errors, it is difficult to explore this difference between the log-log and level-level models more systematically. For instance, while it is possible to reject the null hypothesis of equal changes in property crime rates at the 90th and 95th percentile in the distribution of counties, it is *not* possible to do so for any two counties that experienced in increase in the share of immigrants greater than 3 percentage points. Moreover, one might be concerned that counties which experienced the largest influx of immigrants are exactly those that are also hit by some other kind of shock, say a booming economy, which might, all else equal, depress crime.

Yet, even ignoring these important endogeneity concerns and taking the estimates in the panel on the top right at face value, the *average* slope across counties is 783, indicating a positive impact of immigration on rates of property crime. However, as is apparent from Figure 2, the average slope will understate the effect for the vast majority of counties. The mean slope in the panel on the top left is .063 and statistically indistinguishable from the parametric estimates in Tables 3 and 4.

V. Policy Implications

To facilitate interpretation of the magnitude of the estimated effects and to aid in drawing conclusions for public policy, estimates of the social cost of an immigration-induced increase in crime are required. This section performs back of the envelope calculations for a counterfactual increase in the stock of immigrants by 10%, which corresponds to about 3.7 million individuals in 2007. While the welfare estimates in this section refer to the US at the whole, it is important to note that they are unlikely to be evenly distributed over localities.

Following Levitt (1996), estimates by Cohen (1988) and Miller, Cohen, and Rossman (1993) of monetary and quality of life losses due to crime are used to derive the social cost of an immigration-induced increase in crime. These papers attempt to capture both monetary costs, such as property loss, medical bills, decreases in productivity, etc., as well as reductions in the quality of life due to victimization. Estimates of reductions in the quality of life are based on jury awards in civil suits (excluding punitive damages), which are mapped into distributions for a variety of injuries associated with different types of crime. As these cost estimates correspond to the average crime and the average crime might be more serious than the marginal one, they may overstate the cost of the marginal crime. The cost estimates, however, do not include expenses related to victim precaution, legal fees, or losses to employers.

Another important caveat in interpreting the following cost estimates is that they rely on the assumption that the cost of reported and unreported crimes are equal. According to the National Crime Victimization Survey in 2007 less than 40% of all crimes were reported to the police (US Department of Justice 2010). Even serious crimes, such as aggravated assault and robbery, have reporting rates of less than two thirds. Moreover, it is assumed that the elasticity of each type of crime with respect to the share of immigrants is the same for reported and unreported crimes. This assumption is potentially problematic, as crimes committed by and especially against immigrants might be less likely to be reported.

Table 9 presents estimated yearly cost from a counterfactual increase in the share of immigrants by 10%. As the analysis above finds no clear effect of immigration on "crimes of passion," the table takes only crimes related to monetary gain into account. Including murder, rape, and aggravated assault would increase the social cost estimate in the upper panel and decrease the estimate in the lower one. The basic conclusion, however, would remain unchanged (detailed results are available from the author upon request). The values in Table 9 are in 2007 dollars and based on the number of crimes in 2007. The upper panel uses the OLS elasticity estimates from Table 6, while the lower one relies on the respective IV estimates. Given the inherent uncertainty surrounding these back of the envelope calculations the numbers reported in Table 9 should be taken with a grain of salt. At the same time, when it comes to public policy it is paramount to think about social costs and benefits.

Columns (1) and (2) show the estimated change in the number of reported and unreported crimes for each type of offense, respectively. The bulk of the increase in the *number* of crimes is due to the least costly property crimes. Columns (3) and (4) are the Cohen (1988) and Miller, Cohen, and Rossman (1993) cost estimates inflated to 2007 dollars.[34] Column (5) combines the information in the preceding columns and displays the estimated yearly social cost due to changes in crime following a counterfactual increase in the share of immigrants.

The estimated costs for crimes related to monetary gain sum to slightly more than 760 million dollars per year in the upper panel, and to about 940 million dollars in the lower one. Considering the nature of the underlying assumptions and the variability in the elasticity estimates, in particular the ones with respect to aggravated assault, these welfare calculations should be interpreted cautiously—although both IV and OLS estimates lead to qualitatively very similar conclusions.

Despite the uncertainty surrounding the numbers in Table 9, it is useful to put them into perspective. For instance, in 2007 the Department of Homeland Security spent circa 12 billion dollars on border protection and immigration enforcement (US Department of Homeland Security 2007). Another way to frame the cost estimates is to contrast them with estimates of the benefits of immigration (accruing to natives). Borjas, Freeman, and Katz (1997) have estimated an annual gain to the US economy due to the post-1979 inflow of immigrants into the labor market between .05 and .13 percent of GDP.[35] In 2007 this amounts to approximately 7-18 billion dollars.

Under the assumption that the elasticity of crime with respect to the share of immigrants is constant, the cost estimates in Table 9 can be extrapolated. Between 1980 and 1995, the last year considered by Borjas, Freeman, and Katz (1997), the share of immigrants increased by almost 50%. This yields a yearly cost estimate of approximately 3.8-4.7 billion dollars. Even if we were to multiply this estimate by a factor of two, the social cost associated with an immigration induced

[34] The estimates in Miller, Cohen, and Rossman (1993) update and extent those of Cohen (1988), but are only available for violent crimes.

[35] The last year taken into account by Borjas, Freeman, and Katz (1997) is 1995. Since immigration continued to increase, their estimate understates the current welfare gain to immigration. Somewhat ironically the "immigration surplus" rises with its (negative) impact on wages (see Borjas 1999 for an exposition), which means that the welfare gain to immigration increases with the price elasticity of labor demand. Studies simulating the economic impact of immigration for a variety of elasticity values have found an immigration surplus between .01 and .3 percent of GDP (see Borjas 1999 and the studies cited therein).

increase in property crime would still fall on the vey low end of the interval estimated by Borjas, Freeman, and Katz (1997). Based on these back-of-the-envelope calculations it seems highly unlikely that the costs due to an increase in crime outweigh the welfare gains produced elsewhere in the economy.

VI. Conclusion

The economic theory of crime predicts that, all else equal, individuals with lower outside options commit more crimes than others. While immigrants are known to have lower levels of education, lower wages, and higher unemployment rates than natives, previous studies have not found a relationship between immigration and crime, or proxies thereof.

Using decadal panel data on US counties from 1980 to 2000 this paper presents empirical evidence of a systematic and economically meaningful impact of immigration on crime. A 10% increase in the share of immigrants is estimated to lead to an increase in the property crime rate of circa 1.2%, while the rate of violent crimes remains essentially unaffected.

Consistent with economic theory the effect of immigration on crime is stronger for crimes motivated by financial gain, for instance robbery or motor vehicle theft, but not for "crimes of passion", such as rape, and aggravated assault. Moreover, the effect of immigration is only present for immigrants from Mexico, who are more likely than others to have poor labor market outcomes.

The social costs of an immigration-induced increase in crime are substantial. Failure to account for the cost of increased crime would overstate the social gain to a counterfactual 10% percent increase in the fraction of immigrants by about 750–940 million dollars per year. Despite the uncertainty associated with this cost estimate, it is far too small to outweigh the welfare gains to immigration produced elsewhere in the economy.

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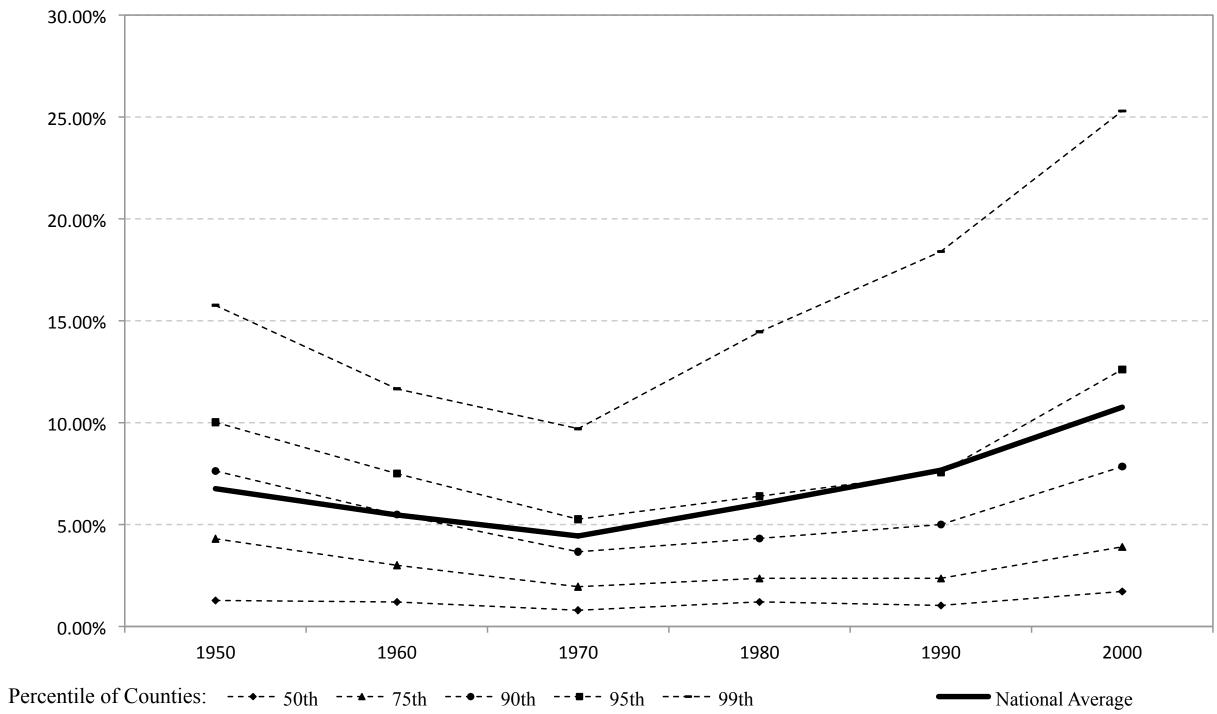
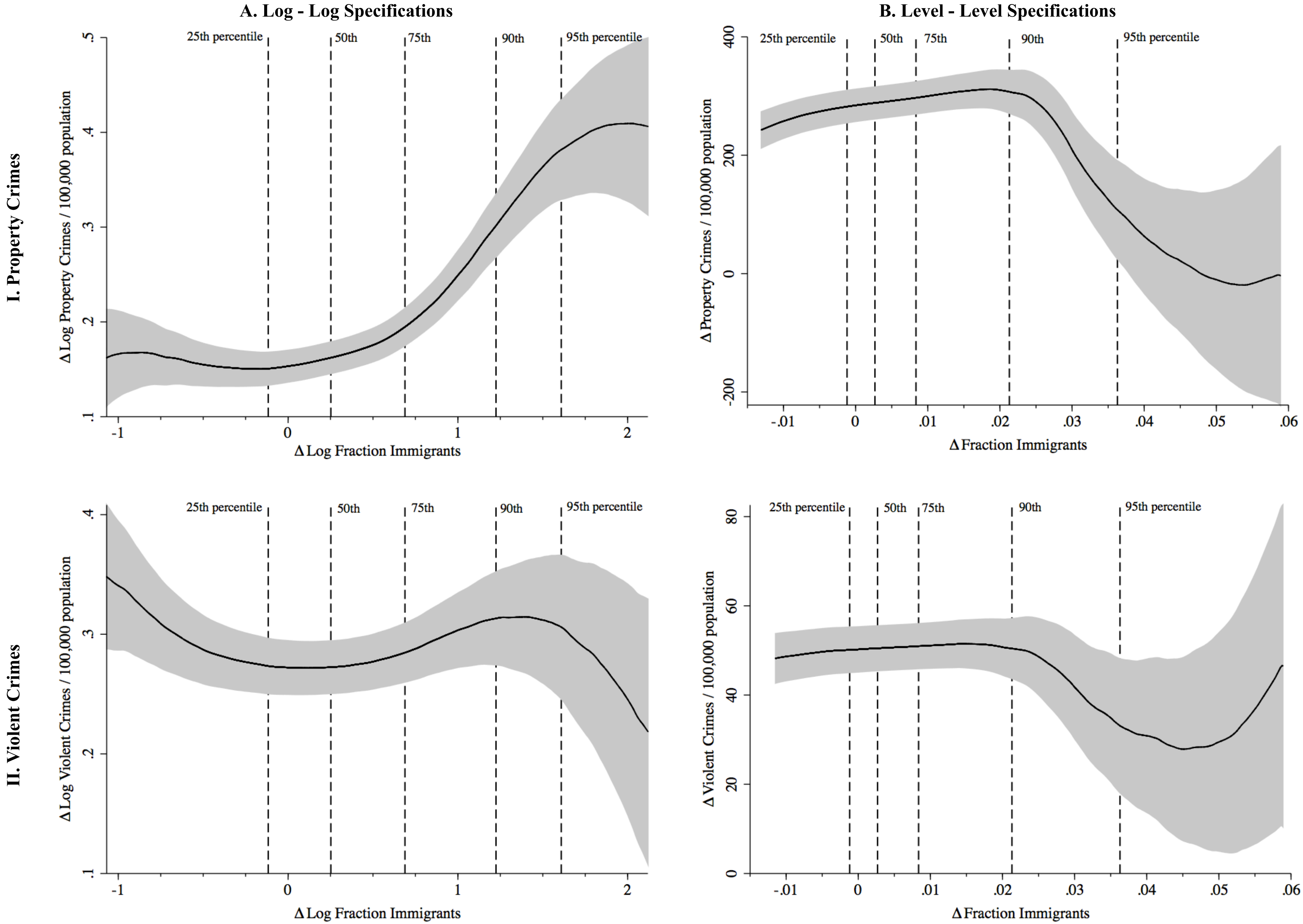
Figure 1: Immigrant Share in the Total Population and Across Counties, 1950-2000



Source: Author's calculations based on U.S. Census data.

Figure 2: Semi-Parametric Estimates



Notes: Figure shows semi-parametric estimates of the relationship between changes in crime rates and changes in immigration, together with the associated 95%-confidence intervals. The panels on the left are based on log-log specifications, whereas those on the right use levels of crime rates and immigrant shares. Estimates are obtained by the differencing procedure in Yatchew (1998) and local-mean smoothing using an Epanechnikov kernel with bandwidths of .4 (left) and .01 (right). The vertical lines indicate the respective percentiles in the distribution of counties.

Table 1: Percentage Change in Crime Rates by Decade and Quartile of Change in Immigrant Share

Decade	All Counties	Quartile of Percent Increase in Immigrant Share: 1st	2nd	3rd	4th
		Property Crimes			
1980–1990	-.087	-.147	-.103	-.081	-.073
	(.007)	(.026)	(.015)	(.012)	(.011)
1990–2000	-.364	-.414	-.402	-.361	-.163
	(.010)	(.018)	(.020)	(.019)	(.026)
		Violent Crimes			
1980–1990	.173	.260	.155	.164	.177
	(.010)	(.035)	(.021)	(.018)	(.016)
1990–2000	-.330	-.323	-.359	-.381	-.119
	(.013)	(.025)	(.023)	(.029)	(.032)

Notes: Entries are means and standard errors of changes in crime rates. Crime rates are defined as the number of offenses reported to the police per 100,000 residents. Violent crimes is the sum of reported murders, rapes, aggravated assaults, and robberies. Property crimes is the sum of reported burglaries, larcenies, and motor vehicle thefts. See the Data Appendix for precise definitions and sources of all variables.

Table 2: Summary Statistics by Decade

Variable	1980	1990	2000
Crime:			
Violent Crime Rate	225.1	281.4	243.8
	(297.5)	(372.3)	(289.2)
Murder Rate	6.167	5.176	3.284
	(9.736)	(10.38)	(6.930)
Rape Rate	15.78	21.63	20.33
	(18.78)	(25.70)	(21.99)
Aggravated Assault Rate	158.2	210.1	184.1
	(171.6)	(241.4)	(223.6)
Robbery Rate	45.01	44.61	36.09
	(152.2)	(153.4)	(80.06)
Property Crime Rate	2,826	2,641	2,075
	(2,064)	(1,618)	(1,618)
Burglary Rate	882.0	721.4	485.8
	(690.1)	(558.2)	(339.3)
Larceny Rate	1,771	1,748	1,446
	(1,451)	(1,425)	(1,170)
Moter Vehicle Theft Rate	172.7	172.2	142.9
	(212.3)	(285.0)	(172.0)
Demographics:			
Fraction Immigrants	.021	.022	.034
	(.028)	(.035)	(.048)
Total Population (in 1,000)	72.22	79.18	89.61
	(236.0)	(263.5)	(291.9)
Population per Square Mile	209.2	211.5	234.9
	(1,569)	(1,427)	(1,665)
Fraction Female	.509	.510	.505
	(.016)	(.016)	(.019)
Median Age	31.05	34.41	37.38
	(3.87)	(3.60)	(3.95)
Region:			
Northeast	.070	.070	.070
	(.256)	(.256)	(.256)
Midwest	.341	.341	.341
	(.474)	(.474)	(.474)
West	.134	.134	.134
	(.340)	(.340)	(.340)
South	.455	.455	.455
	(.498)	(.498)	(.498)
Economic Indicators:			
Median Household Income (in $1,000)	29.76	31.42	35.26
	(6.87)	(8.47)	(8.80)
Fraction of Families Below Poverty	.125	.131	.107
	(.063)	(.070)	(.058)
Payroll per Capita (in $1,000)	5.362	5.798	7.164
	(3.550)	(4.065)	(5.388)
Unemployment Rate (in Percent)	6.781	6.641	4.331
	(3.314)	(3.055)	(2.705)
Police Enforcement:			
Police Expenditure per Capita	66.17	87.52	109.7
	(40.41)	(58.03)	(72.69)
Fraction Institutionalized	.013	.017	.023
	(.017)	(.021)	(.033)
Housing Stock:			
Median Value of Housing Units (in $1,000)	73.28	70.82	84.19
	(29.24)	(44.08)	(47.73)
Median Rent	398.1	422.5	440.4
	(92.36)	(125.9)	(121.7)
Fraction of Housing Units Owner Occupied	.633	.618	.635
	(.089)	(.093)	(.086)
Fraction of Housing Units Vacant	.135	.148	.142
	(.098)	(.105)	(.095)
New Building Permits per Existing Unit	.011	.008	.010
	(.011)	(.010)	(.011)

Notes: Entries are unweighted means and standard deviations of county level data for those counties whith non-missing information. The total number of counties in the data set is 3,117. Entries have been rounded to four digits. Crime rates are defined as the number of offenses reported to the police per 100,000 residents. Violent crimes is the sum of reported murders, rapes, aggravated assaults, and robberies. Property crimes is the sum of reported burglaries, larcenies, and motor vehicle thefts. See the Data Appendix for precise definitions and sources of all variables.

Table 3: Estimates of the Elasticity of Crime with Respect to Immigration

Independent Variable	Log Property Crimes							Log Violent Crimes						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Log Immigrants	-.203	-.143	.147	.156	.157	.140	.123	-.227	-.191	.061	.085	.087	.079	.065
	(.198)	(.218)	(.059)	(.070)	(.068)	(.064)	(.061)	(.203)	(.218)	(.074)	(.074)	(.075)	(.065)	(.063)
Log Total Population	1.415	1.328	.679	.688	.677	.840	1.046	1.590	1.537	.806	.838	.830	.907	1.001
	(.224)	(.252)	(.193)	(.184)	(.183)	(.205)	(.249)	(.227)	(.248)	(.164)	(.162)	(.154)	(.172)	(.194)
Fraction Female				-.098	1.493	.685	.611				-1.771	-.275	.201	.426
				(.2.750)	(2.320)	(3.035)	(3.220)				(2.340)	(2.252)	(2.441)	(2.310)
Median Age				.012	.005	-.012	-.018				.036	.030	.013	.009
				(.023)	(.022)	(.022)	(.025)				(.017)	(.016)	(.014)	(.016)
Log Police Expenditure per Capita					.111	.079	.038						.047	-.016
					(.124)	(.106)	(.099)						(.147)	(.152)
Log Fraction Institutionalized					.112	.034	.016						.054	.044
					(.044)	(.035)	(.034)						(.040)	(.038)
Log Median Household Income						-1.202	-.868						-.732	.098
						(.390)	(.683)						(.442)	(.647)
Fraction of Families Below Poverty						-7.631	-6.800						-5.941	-4.685
						(1.760)	(1.778)						(1.375)	(1.394)
Log Payroll per Capita						-.257	-.220						-.205	-.170
						(.110)	(.106)						(.123)	(.120)
Unemployment Rate						.036	.025						.016	.011
						(.011)	(.011)						(.018)	(.019)
New Building Permits per Existing Unit							.367							.052
							(.981)							(1.512)
Fraction of Housing Units Vacant							4.069							2.232
							(1.949)							(1.468)
Fraction of Housing Units Owner Occupied							.571							-.914
							(2.038)							(1.499)
Median Rent (in $1,000)							-.147							-.492
							(.397)							(.521)
Median Value of Housing Units (in $1,000)							-.001							-.001
							(.000)							(.000)
Constant	-6.701	-6.090	-.556	-1.606	-2.115	12.468	5.987	-10.980	-.283	-3.832	-4.647	-5.008	4.214	-4.987
	(1.087)	(1.278)	(2.1540	(1.759)	(1.938)	(5.863)	(6.833)	(1.109)	(.105)	(1.768)	(1.900)	(1.899)	(4.348)	(5.284)
Year Fixed Effects	No	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes
R-Squared	.733	.739	.981	.981	.981	.982	.982	.730	.732	.976	.976	.976	.976	.977
Number of Observations	8,736	8,736	8,736	8,736	8,736	8,736	8,736	8,439	8,439	8,439	8,439	8,439	8,439	8,439

Notes: Entries are coefficients and standard errors from estimating the empirical model, i.e. equation (1), by population weighted least squares. The respective dependent variables are listed at the top of each column. The unit of observation is a county-year. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. In addition to the variables included in the table, indicator variables for missing values on each covariate are also included in the regressions. See the Data Appendix for the precise definition and source of each variable.

Table 4: Estimates of the Impact of Immigration on Crime Using First Differences

Independent Variable	Δ Log Property Crime (1)	(2)	Δ Log Violent Crime (5)	(6)
Δ Log Immigrants	.180	.123	.028	.011
	(.039)	(.055)	(.039)	(.067)
Δ Log Total Population	.656	1.043	1.052	1.326
	(.113)	(.191)	(.091)	(.341)
Δ Fraction Female	2.646	3.476	1.831	2.185
	(.754)	(1.048)	(.706)	(1.388)
Δ Median Age	.017	.014	.029	.048
	(.008)	(.021)	(.013)	(.031)
Δ Log Police Expenditure per Capita	.163	.012	.097	.040
	(.082)	(.096)	(.120)	(.182)
Δ Log Fraction Institutionalized	.082	.002	.026	-.064
	(.029)	(.027)	(.031)	(.044)
Δ Log Median Household Income	1.184	.986	.192	-.062
	(.182)	(.243)	(.197)	(.273)
Δ Fraction of Families Below Poverty	-1.512	2.091	-2.247	-.912
	(1.115)	(1.005)	(1.041)	(1.870)
Δ Log Payroll per Capita	-.308	-.058	-.240	.056
	(.054)	(.050)	(.075)	(.101)
Δ Unemployment Rate	.020	.006	-.014	-.017
	(.013)	(.014)	(.012)	(.016)
Δ New Building Permits per Existing Unit	-1.729	-3.907	-1.406	-1.826
	(1.264)	(1.967)	(1.463)	(23.009)
Δ Fraction of Housing Units Vacant	.023	.066	.043	.076
	(.016)	(.028)	(.019)	(.035)
Δ Fraction of Housing Units Owner Occupied	-1.210	-.997	-1.546	-1.194
	(.283)	(.483)	(.393)	(.607)
Δ Median Rent (in $1,000)	.677	.671	.762	.841
	(.241)	(.420)	(.415)	(.707)
Δ Median Value of Housing Units (in $1,000)	-.003	-.001	-.001	-.001
	(.001)	(.001)	(.001)	(.001)
Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	No	Yes	No	Yes
R-Squared	.494	.689	.375	.545
Number of Observations	7,992	7,992	7,448	7,448

Notes: Entries are coefficients and standard errors from estimating the empirical model in first differences, i.e. equation (2), by population weighted least squares. The respective dependent variables are listed at the top of each column. The unit of observation is a county-year. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. In addition to the variables included in the table, indicator variables for missing values on each covariate are also included in the regressions. See the Data Appendix for the precise definition and source of each variable.

Table 5: Instrumental Variables Estimates of the First Differences Model

Independent Variable	First Stage: Δ Log Immigrants	Second Stage: Δ Log Property Crime	Second Stage: Δ Log Violent Crime
Δ Log Immigrants	---	.108	.010
		(.122)	(.154)
Δ Log Total Population	1.317	.772	1.081
	(.110)	(.207)	(.262)
Δ Fraction Female	-2.615	2.513	1.798
	(3.182)	(.803)	(.764)
Δ Median Age	-.001	.016	.029
	(.010)	(.009)	(.013)
Δ Log Police Expenditure per Capita	.063	.171	.099
	(.054)	(.086)	(.125)
Δ Log Fraction Institutionalized	.026	.087	.028
	(.024)	(.030)	(.034)
Δ Log Median Household Income	.579	1.226	.203
	(.257)	(.162)	(.196)
Δ Fraction of Families Below Poverty	1.541	-1.365	-2.209
	(.850)	(1.056)	(1.203)
Δ Log Payroll per Capita	-.090	-.314	-.242
	(.053)	(.054)	(.071)
Δ Unemployment Rate	.006	.021	-.014
	(.008)	(.013)	(.012)
Δ New Building Permits per Existing Unit	-.748	-1.765	-1.416
	(1.926)	(1.308)	(1.468)
Δ Fraction of Housing Units Vacant	-.080	.017	.042
	(.019)	(.017)	(.022)
Δ Fraction of Housing Units Owner Occupied	.695	-1.135	-1.527
	(.376)	(.272)	(.453)
Δ Median Rent (in $1,000)	.000	.719	.772
	(.000)	(.248)	(.391)
Δ Median Value of Housing Units (in $1,000)	.000	-.003	-.001
	(.000)	(.001)	(.001)
Instrument:			
Predicted Δ Log Immigrants	.235	---	---
	(.027)		
Year Fixed Effects	Yes	Yes	Yes
First Stage F-Statistic	---	74.65	72.76
Number of Observations	8,016	7,992	7,448

Notes: Entries are coefficients and standard errors from estimating the first differences model by population weighted two stage least squares. The respective dependent variables are listed at the top of each column. The instrument for Δ Log Immigrants at time *t* is the predicted change in the logarithm of immigrants, as explained in the text and the Dtaa Appendix. The unit of observation is a county-year. Standard errors are clustered by state and reported in parentheses. See the Data Appendix for the precise definition and source of each variable.

Table 6: Estimated Elasticties for Different Types of Crime

	OLS	IV
Property Crimes:		
Burglary	.149	.022
	(.067)	(.145)
Larceny	.115	.131
	(.059)	(.133)
Motor Vehicle Theft	.148	.196
	(.091)	(.134)
Violent Crimes:		
Murder	.121	-.047
	(.063)	(.179)
Rape	.048	-.285
	(.089)	(.197)
Aggravated Assault	.036	.026
	(.094)	(.168)
Robbery	.274	.417
	(.083)	(.184)

Notes: Entries are coefficients and standard errors on from estimating equation (1), by population weighted least squares (OLS), and estimating euqation (2) by two-stage least squares (IV). The instrument is the same as in Table 5. The dependent variable is the natural logarithm of the crime listed next to the respetive coefficient. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses.

Table 7: Estimated Elasticties with Respect to Different Immigrant Groups

	Independent Variables:	
Dependent Variable	Log Mexicans	Log All Other Immigrants
Property Crimes:		
All Property Crimes	.066	-.068
	(.025)	(.035)
Burglary	.057	-.091
	(.026)	(.030)
Larceny	.058	-.053
	(.026)	(.037)
Motor Vehicle Theft	.083	-.092
	(.028)	(.056)
Violent Crimes:		
All Violent Crimes	.038	-.078
	(.024)	(.031)
Murder	.083	-.061
	(.030)	(.055)
Rape	.059	-.111
	(.032)	(.050)
Aggravated Assault	-.001	-.073
	(.029)	(.045)
Robbery	.095	-.032
	(.033)	(.048)

Notes: Entries are coefficients and standard errors on Log Mexicans and Log All Other Immigrants from estimating the fixed effects model, i.e equation (1), by population weighted least squares. The dependent variable is the natural logarithm of the crime listed next to the respetive coefficient. Heteroskedasticity robust standard errors are clustered by state and reported in parentheses. In addition to Log Mexicans and Log All Other Immigrants, all regressions include county fixed effects, year fixed effecst, and the full set of covariates.

Table 8A: Sensitivity Analysis and Extensions of the Basic Model for Property Crimes

	Elasticity of Crime Rate with Respect to Immigrant Share:			
Specification / Sample	All Property Crimes	Burglary	Larceny	Motor Vehicle Theft
Baseline	.123	.149	.115	.148
	(.061)	(.067)	(.059)	(.091)
Controlling for Racial Composition	.131	.177	.125	.145
	(.063)	(.061)	(.062)	(.090)
Unweighted	.077	.091	.068	.057
	(.038)	(.042)	(.036)	(.046)
Inverse Probability Weighted	.121	.146	.113	.142
	(.060)	(.066)	(.058)	(.088)
By Year:*				
1980	.095	.087	.100	.166
	(.063)	(.079)	(.064)	(.078)
1990	.230	.222	.249	.218
	(.075)	(.099)	(.072)	(.104)
2000	.257	.262	.258	.251
	(.130)	(.134)	(.127)	(.156)
By Tercile of Immigrant Share in 2000:				
1st Tercile	.037	.052	.083	.014
	(.043)	(.052)	(.047)	(.073)
2nd Tercile	.124	.142	.107	.165
	(.096)	(.106)	(.092)	(.098)
3rd Tercile	.175	.242	.148	.222
	(.070)	(.068)	(.073)	(.119)
By Tercile of Immigrants Received 1960-2000:				
1st Tercile	.009	.036	.002	-.012
	(.054)	(.061)	(.056)	(.079)
2nd Tercile	.231	.273	.220	.210
	(.071)	(.077)	(.061)	(.088)
3rd Tercile	.250	.315	.228	.222
	(.082)	(.092)	(.086)	(.091)
By Region:				
West	.115	.140	.036	.425
	(.061)	(.089)	(.071)	(.184)
Midwest	.097	.066	.076	.134
	(.077)	(.072)	(.072)	(.123)
Northeast	-.238	-.511	-.219	-.362
	(.344)	(.589)	(.326)	(.637)
South	.045	.065	.042	.030
	(.055)	(.078)	(.048)	(.081)
Texas, California, Florida	.133	.153	.107	.312
	(.065)	(.020)	(.076)	(.208)
all other States	.093	.138	.087	.093
	(.065)	(.073)	(.063)	(.100)
By Tercile of Population in 2000:				
1st Tercile	.046	.041	.030	.023
	(.032)	(.034)	(.042)	(.045)
2nd Tercile	.090	.108	.088	.047
	(.046)	(.050)	(.037)	(.042)
3rd Tercile	.130	.165	.119	.208
	(.085)	(.090)	(.084)	(.125)
By Tercile of Population per Square Mile in 2000:				
1st Tercile	.002	.015	-.015	-.036
	(.029)	(.042)	(.039)	(.051)
2nd Tercile	.095	.122	.101	.107
	(.049)	(.059)	(.035)	(.061)
3rd Tercile	.149	.176	.137	.205
	(.086)	(.089)	(.086)	(.130)
By Median Household Income in 2000:				
1st Tercile	.049	.037	.062	.027
	(.044)	(.061)	(.044)	(.097)
2nd Tercile	.103	.148	.118	.028
	(.070)	(.099)	(.065)	(.080)
3rd Tercile	.146	.217	.104	.262
	(.131)	(.080)	(.139)	(.148)
By Tercile of Average Unemployment Rate:				
1st Tercile	.201	.210	.190	.257
	(.066)	(.087)	(.066)	(.097)
2nd Tercile	.163	.158	.151	.250
	(.066)	(.071)	(.064)	(.103)
3rd Tercile	.048	.151	.044	.026
	(.086)	(.100)	(.082)	(.134)

Notes: Entries are coefficients and standard errors on Log Immigrants from estimating the fixed effects model, i.e. equation (1), by least squares. The respective dependent variables are listed at the top of each column. Unless otherwise noted population weights are used. Standard errors are clusered on the state level, except when indicated otherwise. The respective sample restriction is indicated at the left of each row.

* Does not include county fixed effects.

Table 8B: Sensitivity Analysis and Extensions of the Basic Model for Violent Crimes

Specification / Sample	Elasticity of Crime Rate with Respect to Immigrant Share: All Violent Crimes	Murder	Rape	Aggravated Assault	Robbery
Baseline	.065	.121	.048	.036	.274
	(.063)	(.063)	(.089)	(.094)	(.083)
Controlling for Racial Composition	.080	.090	.089	.056	.288
	(.061)	(.045)	(.076)	(.094)	(.082)
Unweighted	.006	-.006	-.010	.008	.138
	(.142)	(.033)	(.052)	(.056)	(.053)
Inverse Probability Weighted	.062	.092	.044	.034	.254
	(.062)	(.055)	(.082)	(.091)	(.078)
By Year:*					
1980	.031	-.243	-.149	.107	-.048
	(.084)	(.054)	(.068)	(.102)	(.084)
1990	.124	-.282	.113	.174	.086
	(.103)	(.064)	(.111)	(.113)	(.123)
2000	.227	-.108	.059	.307	.215
	(.159)	(.084)	(.057)	(.159)	(.186)
By Tercile of Immigrant Share in 2000:					
1st Tercile	-.011	-.024	.006	.033	-.061
	(.068)	(.103)	(.135)	(.095)	(.056)
2nd Tercile	.071	-.035	.125	.052	.263
	(.118)	(.082)	(.090)	(.139)	(.091)
3rd Tercile	.155	.211	.109	.097	.365
	(.068)	(.095)	(.116)	(.107)	(.102)
By Tercile of Immigrants Received 1960-2000:					
1st Tercile	-.176	-.057	-.084	-.159	-.018
	(.085)	(.213)	(.170)	(.106)	(.174)
2nd Tercile	.118	.097	.147	.130	.115
	(.105)	(.099)	(.091)	(.150)	(.093)
3rd Tercile	.176	.221	.164	.042	.421
	(.064)	(.090)	(.129)	(.084)	(.140)
By Region:					
West	.003	.278	.271	-.070	.257
	(.144)	(.223)	(.121)	(.161)	(.129)
Midwest	.072	.092	-.040	.095	.056
	(.117)	(.214)	(.121)	(.166)	(.197)
Northeast	-.157	-.039	-.289	-.268	.093
	(.247)	(.409)	(.413)	(.216)	(.431)
South	.021	.034	-.036	.028	.157
	(.048)	(.066)	(.098)	(.082)	(.075)
Texas, California, Florida	.020	.061	.059	-.030	.174
	(.107)	(.192)	(.113)	(.195)	(.128)
all other States	.062	.087	.051	.002	.292
	(.070)	(.061)	(.086)	(.109)	(.098)
By Tercile of Population in 2000:					
1st Tercile	-.076	-.041	-.115	-.052	.034
	(.059)	(.156)	(.056)	(.071)	(.090)
2nd Tercile	.026	.052	-.030	.008	.128
	(.055)	(.065)	(.089)	(.067)	(.058)
3rd Tercile	.087	.144	.064	.044	.342
	(.084)	(.074)	(.115)	(.123)	(.097)
By Tercile of Population per Square Mile in 2000:					
1st Tercile	-.114	.038	-.100	-.091	.043
	(.064)	(.147)	(.113)	(.078)	(.090)
2nd Tercile	.004	.021	-.024	-.032	.153
	(.051)	(.079)	(.082)	(.067)	(.074)
3rd Tercile	.125	.150	.078	.098	.337
	(.088)	(.081)	(.117)	(.127)	(.095)
By Median Household Income in 2000:					
1st Tercile	.042	.070	.020	.023	.079
	(.057)	(.076)	(.103)	(.080)	(.079)
2nd Tercile	-.010	-.017	.064	-.083	.159
	(.090)	(.109)	(.097)	(.103)	(.132)
3rd Tercile	.121	.169	.079	.082	.382
	(.102)	(.123)	(.111)	(.152)	(.100)
By Tercile of Average Unemployment Rate:					
1st Tercile	.107	.119	.080	.056	.352
	(.081)	(.136)	(.122)	(.120)	(.121)
2nd Tercile	.117	.144	-.022	.158	.233
	(.082)	(.090)	(.084)	(.100)	(.078)
3rd Tercile	-.025	.082	.009	-.076	.182
	(.105)	(.084)	(.128)	(.124)	(.159)

Notes: Entries are coefficients and standard errors on Log Immigrants from estimating the fixed effects model, i.e. equation (1), by least squares. The respective dependent variables are listed at the top of each column. Unless otherwise noted population weights are used. Standard errors are clusered on the state level, except when indicated otherwise. The respective sample restriction is indicated at the left of each row.

* Does not include county fixed effects.

Table 9: Estimated Social Cost from a Ten Percent Increase in Immigrants

A. Based on OLS Point Estimates

	Change in Reported Crimes	Change in Unreported Crimes	Cost per Crime (in USD)		Social Cost
			Monetary	Quality of Life	
Robbery	12,200	6,400	5,000	19,000	450,000,000
Burglary	32,500	31,000	1,600	550	140,000,000
Larceny	75,500	168,000	300	0	75,000,000
Motor Vehicle Theft	16,200	2,800	5,400	0	100,000,000
Total	136,400	208,200	--	--	765,000,000

B. Based on IV Point Estimates

	Change in Reported Crimes	Change in Unreported Crimes	Cost per Crime (in USD)		Social Cost
			Monetary	Quality of Life	
Robbery	19,000	9,700	5,000	19,000	690,000,000
Burglary	4,800	4,600	1,600	600	21,000,000
Larceny	86,000	190,000	300	0	87,000,000
Motor Vehicle Theft	22,000	3,700	5,400	0	140,000,000
Total	131,800	208,000	--	--	938,000,000

Notes: Based on the OLS estimates of the elasticities of different types of crime with respect to the share of immigrants reported in Table 6 (assuming that the elasticties for reported and unreported crimes are equal). Calculations are based on the total number of each crime in 2007 as reported in US Bureau of Justice Statistics (2009b). Estimates of reporting rates for each type of crime are from the National Crime Victimization Survey in 2007 (US Department of Justice 2010, Table 91). The estimates of the costs of crime are from Cohen (1988) and Miller, Cohen, and Rossman (1993), adjusted to 2007 dollars using the CPI. The final column displays the estimated social cost of a 10% increase in the stock of immigrants due to changes in each type of crime, combining changes in reported and unreported crime. Numbers have been rounded.