

# Cross-City Evidence on the Relationship between Immigration and Crime

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## **Abstract**

*Public concerns about the costs of immigration and crime are high, and sometimes overlapping. This article investigates the relationship between immigration into a metropolitan area and that area's crime rate during the 1980s. Using data from the Uniform Crime Reports and the Current Population Surveys, we find, in the cross section, that cities with high crime rates tend to have large numbers of immigrants. However, controlling for the demographic characteristics of the cities, recent immigrants appear to have no effect on crime rates. In explaining changes in a city's crime rate over time, the flow of immigrants again has no effect, whether or not we control for other city-level characteristics. In a secondary analysis of individual data from the National Longitudinal Survey of Youth (NLSY), we find that youth born abroad are statistically significantly less likely than native-born youth to be criminally active.*

Immigration and crime are not only two of today's "hot-button" issues in politics, they seem to occupy the same region of the public mind. The constant juxtaposition of the words "immigration" and "crime" in news story after news story might forge the perception of a causal link between the two issues. Juxtaposition, however, is not the only way in which the two are linked by those in the policy arena.

A direct relationship is assumed to exist between illegal immigration and crime.<sup>1</sup> An explicit example of this is in Proposition 187 in California. This proposition has received most attention for the connections it asserts between illegal immigration and the rising costs of public expenditures on welfare and schools. However, the proposition also highlights victimization. Section 1 of the law reads, "The People of California find and declare as follows: . . . That they have suffered and are suffering personal injury and damage caused by the criminal conduct of illegal aliens in this state."

Although illegal immigrants are typically cited as contributing to crime, there is also a great deal of legislative attention aimed at "criminal aliens" regardless

of their immigration status [see Butcher and Piehl, 1998]. The 104th Congress included a large number of bills, amendments, and resolutions that took aim at this issue.<sup>2</sup> For example in 1996, then Senator Dole sponsored the “Anti-Terrorism and Effective Death Penalty Act.”<sup>3</sup> This act is primarily a crime prevention measure, yet a large section of it is devoted to immigration issues. Title IV of this act is entitled “Terrorist and Criminal Alien Removal and Exclusion.” Subtitle D (“Criminal Alien Procedural Improvements”) includes myriad amendments to the Immigration and Nationality Act (currently under debate), the Violent Crime Control and Law Enforcement Act of 1994, and the U.S. Code. The proposals are aimed at streamlining the deportation of criminal aliens and improving the working relationship among the various divisions of government that oversee immigration and crime control in order to help identify and punish criminal aliens. The 1994 crime law itself covered many of these same topics.

Politicians also frequently draw a causal relationship between immigration and crime in public statements about the policies they propose. For example, in a 1996 editorial about the immigration bills then being debated in the two houses of Congress, some versions of which sought to reduce the numbers of legal immigrants admitted, and all versions of which promised to “crack down” on illegal immigrants, Senator Alan Simpson concluded with the following: “Finally, this legislation is not unneeded. Some have made the claim that more immigration—both legal and illegal—is what this country needs. Anyone who believes that has not been listening to taxpayers who are being adversely affected—for example, by welfare abuse, schools that are overcrowded and beset by demands for ‘multicultural’ curricula, rising crime and expensive, time-consuming deportation procedures. Both the Senate and House bills tackle the problem in a rational, sensible and fair way, one that advances the interests of most of our citizens—and thus our national interest” [Simpson, 1996].

During his bid for the 1996 Republican presidential nomination, one of Pat Buchanan’s television advertisements stated: “Each year millions of illegal immigrants pour across our southern border into the United States. Most come without job skills. Crime explodes. And who pays the cost of their health care, housing, welfare? You do. . . . [I will] declare a time-out on new immigration. Secure America’s borders. And insist on one language, English, for all Americans” [Edsall and Claiborne, 1996].

Whether implicitly or explicitly stated, the obvious message of the political rhetoric and the proposed legislation is that through laws specifically targeting immigrants, crime will be reduced. The public seems to believe this, as evidenced by a 1993 *Time* magazine poll showing that 59 percent of respondents believe recent immigrants “add to the crime problem” [Nelán, 1993, p. 11]. Although this has to be true in some trivial sense—if there are fewer immigrants (or a

<sup>1</sup> The distinction between legal and illegal immigrants is often blurred in the reporting of immigration issues. The public believes that most immigrants come to the United States illegally [Nelán, 1995, p. 10]. In fact, legal immigrants far outnumber illegal. Estimates of the fraction foreign born who are in the United States illegally range from 15 percent to 24 percent [see Borjas, 1990, pp. 63–75; Nelán, 1993, p. 10].

<sup>2</sup> Legislative interest in this issue shows few signs of abating. The 105th Congress considered at least 50 bills that touched on crime and immigration.

<sup>3</sup> PL 104–132, 104th Cong., id sess., 24 April 1996. The bill was cosponsored by eight other senators from both sides of the aisle: Hatch, Nickles, Thurmond, Simpson, Brown, Kyl, Gramm, and Feinstein.

smaller population in general) the *number* of crimes is likely to be lower; if a criminal alien is deported, she or he will not be in the United States to commit another crime—this article is an investigation of whether this is true in any deeper sense. Is there any hard evidence that the large inflow of immigrants during the 1980s has adversely affected crime rates in the United States? Is there any evidence that immigrants are more likely to commit crimes than the native born? This article is not meant to be an explicit investigation of the efficacy of any particular policy proposition. Rather, it investigates whether targeting immigrants is likely to be a powerful policy lever through which society can materially alter crime.

Because immigration is geographically concentrated, the public debate about immigration is often framed in terms of expenditures, that is, who bears the costs of public services for immigrants.<sup>4</sup> The same is true about the criminal justice impact of immigration. The public is concerned not only with criminal victimization, but also about the costs associated with the enforcement and punishment of crimes.

If immigrants have an adverse impact on criminal justice expenditures, it must be through one of four avenues.<sup>5</sup> First, immigrants may be more likely to commit crimes than natives, or commit crimes that are more costly to society. Second, immigrants may have an adverse impact on crime by crowding natives out of the legal sector. If immigrants adversely affect natives' legal alternatives by taking jobs or overburdening the welfare system, low-skilled natives may increase their involvement in criminal activity. Both of these effects imply that crime rates would be higher in areas with heavy immigrant concentrations, other things being equal. Third, immigrants may be more likely to be apprehended or convicted than natives. This may occur if immigrants do not have the same knowledge of the legal system as natives or because they are in the criminal justice system for immigration violations. Obviously, immigrants are "at risk" for this kind of apprehension while natives are not. Finally, immigrants may serve longer terms than natives, either because they are given longer sentences or because they are less likely to be paroled [McShane, 1987].

We investigate the first two of these possible avenues through which immigrants might affect the criminal justice system. We exploit the fact that

<sup>4</sup> Previous research on the effect of immigrants on public expenditures has yielded controversial and conflicting results. Accounting exercises that try to determine whether immigrants pay as much in taxes as they take out in services are often sensitive to the assumptions made. For example, Simon [1984, 1989] and Clark and Passel [1993] both find that immigrants by and large "pay their own way." On the other hand, Huddle reports that immigrants represented a net cost to Texas taxpayers of \$4 billion in 1992 because of expenditures on education, health care, and other services [Berke, 1994]. Other studies take a more direct approach, asking whether immigrants participate in programs that cost money. Borjas and Trejo [1991] report that immigrants are, on average, more likely to participate in the welfare system than are natives. However, Borjas and Trejo [1991] and Borjas [1994] also find that immigrants are less likely to participate in welfare than demographically comparable natives. In the crime context, we will investigate whether immigrants are more likely to engage in crime, in the raw means and controlling for demographic characteristics.

<sup>5</sup> Both legal and illegal immigrants may affect expenditures on government services. Although the press and politicians tend to focus on the effect of illegal immigrants, economic research tends to focus on the effect of both legal and illegal immigrants as the relevant concern for public policy. It should also be noted that most publicly available data sources, including the Current Population Survey (CPS) and the National Longitudinal Survey of Youth (NLSY) (used in this article), do not allow one to distinguish between legal and illegal immigrants.

immigration is a geographically concentrated phenomenon and use data from the Current Population Survey (CPS) and the Uniform Crime Reports (UCR) to compare crime in immigrant-intensive cities to other cities. This comparison demonstrates whether immigrants themselves appear to be disproportionately likely to commit crimes and whether they appear to cause native-born people to engage in criminal activity. Because the two effects cannot be separately identified using this strategy, we also use data on individuals from the 1980 National Longitudinal Survey of Youth (NLSY) to see whether immigrants are more likely to report criminal activity than natives, in the raw means and controlling for their characteristics.

Although we find that the flow of recent immigrants is positively correlated with the level of crime, it has no effect on one-year changes in the crime rate. This result holds whether or not we control for changes in other city-level variables, such as demographics or labor market indicators. To the extent that we can test it with our data, it does not appear that immigrants assimilate into crime, because analysis of longer changes yields the same null result. Analysis of individual-level data shows that immigrants are less likely than the native born to report committing any crime. When these estimates are adjusted using demographic characteristics, the difference between immigrants and natives increases.

Much of the current policy debate centers on curtailing the number of legal immigrants admitted each year. Neither the cross-city nor the individual-level data support the use of the crime issue to justify cutting the flow of new immigrants. The findings have less clear implications for discussions of changing the composition of immigrants admitted [Chiswick, 1995]. Because little of the variation in changes in city crime rates can be explained by demographics, it seems unlikely that changing the composition of immigrants will make an appreciable impact on crime. The individual analysis, however, suggests that favoring women and those with higher levels of education could reduce the average criminality among immigrants to a level even further below the average among natives.

The article is organized as follows: The first section describes our data from the Current Population Surveys (CPSs) and the Uniform Crime Reports (UCR) and discusses how we construct data for metropolitan areas. Next, we present our methodological approach. The following section relates city crime rates to the total fraction immigrant in a city. We then present regression results for city-level crime rates controlling for the fraction of the population composed of *recent* immigrants and other city-level characteristics. The fifth section critiques and summarizes our city-level analysis. Finally, we use the NLSY to investigate differences in criminal activity between native-born and foreign-born individuals, and we offer our conclusions.

## DATA AND DESCRIPTIVE STATISTICS

### Crime Rates

Our data on crime rates come from the Uniform Crime Reports (UCR), which are collected by the Federal Bureau of Investigation (FBI). The crimes are reported by police jurisdictions and then aggregated up to metropolitan area (MA) level by the FBI. The numbers include all “offenses known to law

enforcement” in eight categories: homicide, forcible rape, robbery, aggravated assault, burglary, larceny/theft, motor vehicle theft, and arson. These crimes are referred to as “index” offenses. The first four crimes are categorized as violent crimes; the others are property crimes. Crime rates are reported as index crimes per 100,000 in population [U.S. Department of Justice, Federal Bureau of Investigation, 1988]. The most common property crimes are larceny and theft, which make up 63 percent of the offenses. Burglaries are 24 percent of property crimes. Among violent crimes, aggravated assaults constitute 60 percent of the offenses and robbery 35 percent.

Using the UCR may cause us to overlook some important types of crime. For example, drug dealing, simple assaults, fraud, vandalism, and weapons violations are not included in the UCR measure of index crimes. If some of these crimes are disproportionately committed by immigrants, then our analysis of the relationship between fraction immigrant in a city and the index crime rate will not pick up the total effect of immigrant inflows on criminal activity.<sup>6</sup>

On the other hand, focusing on the UCR index crimes has definite benefits. First, the FBI works to make sure that the data are consistent across cities. In fact, data are missing in some years because the crimes were not reported in the fashion required by the FBI. Secondly, by focusing on index crimes, we will capture the offenses that are generally considered the most serious. Thus, although they may not provide ideal measures of criminal activity, index crime rates are at least consistent across years and metropolitan areas and include the most virulent types of crimes.<sup>7</sup>

Currently there is widespread concern about increasing crime. Although crime rates have skyrocketed since the 1960s,<sup>8</sup> they were relatively steady during the 1980s. Figure 1 plots index, property, and violent crime rates (weighted by population) from 1979 to 1992 for the 43 metropolitan areas used in our subsequent analysis.<sup>9</sup> One can see from Figure 1 that the majority of crimes are property crimes. The index crime rate was at a peak in the early 1980s, and has declined or remained steady since then. In contrast to the steady overall crime rates, violent crime rose through the 1980s. In the analysis that follows, we will study overall crime (dominated by property crimes) and violent crimes separately.

### **Immigrant Characteristics**

It is well documented that recent immigrants are less skilled than both earlier immigrants and natives [see, for example, Borjas, 1990]. The first two columns in Table 2 show characteristics of recent immigrants compared to the rest of

<sup>6</sup> The UCR are often criticized for reporting errors. However, a priori there is no reason to think that this will bias our results on the effect of immigration on crime rates.

<sup>7</sup> It would be interesting to perform the same analysis for drug crimes that we are able to do for the index crimes. However, it is difficult to obtain measures of the number of drug crimes, because they are not generally reported. Data on drug arrests are available; however, they are an unreliable indicator of drug crimes. The number of drug arrests is as much a function of the allocation of resources as of the underlying problem.

<sup>8</sup> The overall crime rate per 100,000 went from approximately 2000 in 1960 to approximately 6000 in 1990 [Maguire, Pastore, and Flanagan, 1993, pp. 354–358, Table 3.122].

<sup>9</sup> See Table 1 for a list of the metropolitan areas included.

**Table 1.** Metropolitan areas (MAs).

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Akron, OH	Milwaukee, WI
Albany, NY	Minneapolis, MN
Anaheim, CA	Nassau County, NY
Atlanta, GA	Newark, NJ
Baltimore, MD	New Orleans, LA
Birmingham, AL	New York, NY
Boston, MA	Norfolk, VA
Buffalo, NY	Paterson, NJ
Chicago, IL	Philadelphia, PA
Cincinnati, OH	Pittsburgh, PA
Cleveland, OH	Portland, OR
Columbus, OH	Rochester, NY
Dallas, TX	Sacramento, CA
Denver, CO	San Bernardino, CA
Detroit, MI	San Diego, CA
Gary, IN	San Francisco, CA
Greensboro, NC	San Jose, CA
Houston, TX	Seattle, WA
Indianapolis, IN	St. Louis, MO
Kansas City, MO	Tampa, FL
Los Angeles, CA	Washington, DC
Miami, FL	

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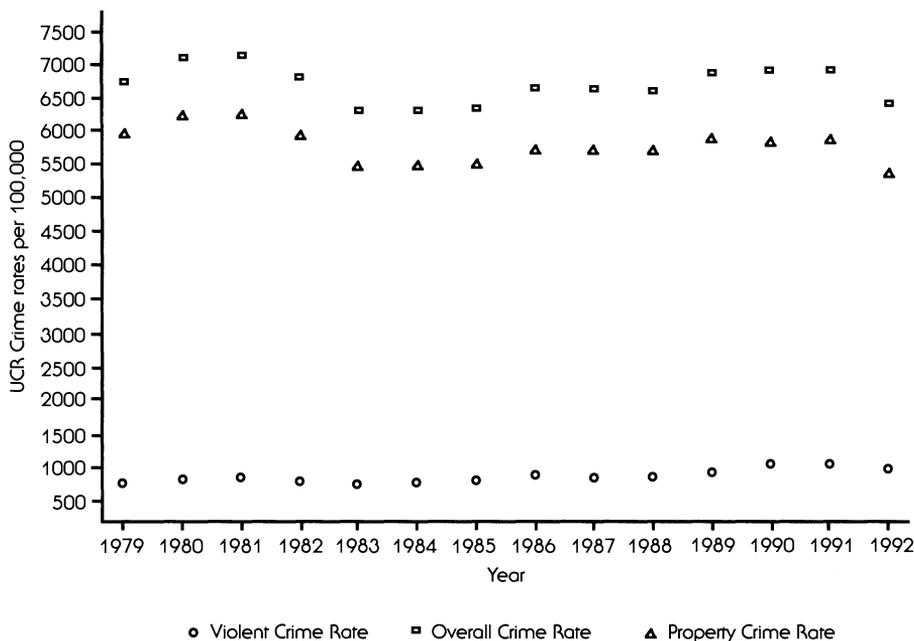
*Notes:* This table lists the complete set of metropolitan areas (MAs) that are consistently identified by the Current Population Survey (CPS) from 1979 to 1990. In the figures and tables in this article not all of these MAs are included in all years because there are missing crime data for some MAs in some years.

the population in the March 1985 CPS for the 43 cities in our analysis.<sup>10</sup> Recent immigrants have lower levels of education, lower wages, and lower employment probabilities than the rest of the population. In addition, immigrants are more likely to be Hispanic, male, and young. Thirty-three percent of the recent immigrants in 1985 were between the ages of 15 and 24. Only 21.3 percent of the rest of the population fell into this category.

**Criminal Characteristics**

Recent immigrants have demographic characteristics similar to those of criminal offenders. Those who commit crimes are disproportionately male, young, poorly educated, and nonwhite. The last column of Table 2 shows the characteristics of those incarcerated in 1991 from the Survey of Inmates of State Correctional Facilities. The population of inmates is overwhelmingly male, Hispanic or black, and poorly educated. The mean age (32.0) is the same as the mean age among the recent immigrant population. The percentage aged 15 to 24 is lower than in the immigrant population, but one should note that these figures are for the

<sup>10</sup> Recent immigrants in the 1985 March CPS are defined as those living abroad in March 1980. Butcher and Card [1991, p. 293, Table 1] show comparisons between these data and the 1980 census. At least 85 percent of those living abroad five years ago are estimated to be immigrants. The characteristics of recent immigrants in the 1980 census and the 1985 CPS are shown to be very similar.



**Figure 1.** Average crime rate for metropolitan areas by year (see table 1 for a list of the metropolitan areas in this analysis).

stock of inmates, not the flow into correctional facilities. Among arrestees in 1991, 36.8 percent were between the ages of 16 and 24. Those age groups account for 13.1 percent of the U.S. population [Maguire, Pastore, and Flanagan, 1993, p. 424, Table 4.3].

On average, inmates of state prisons have 1.6 fewer years of education than the population at large. The importance of education as a predictor of incarceration has been noted by Piehl [1994]: Among young (age 20 to 24) black males, those without high school degrees are nearly four times more likely to be institutionalized as those who have finished high school; for whites, male high school dropouts are 5.5 times more likely to be institutionalized.

Thus, one potential reason for a link between immigration and crime rates is that recent immigrants have demographic characteristics similar to criminal offenders. Even if immigrants are no more likely to commit crimes than similar natives, a large inflow of immigrants may add to a city's population of young, male, poorly educated minorities. Borjas [1990] argues that the relevant question for public policy is not whether immigrants are less successful in the United States than demographically comparable natives, but whether immigrants disproportionately add to the population that is less successful in the United States. We will address this issue later in the crime context.

### Metropolitan Area Level Data

The majority of our analysis uses a regression framework to explain metropolitan area crime rates during the 1980s. In some cases, we control for metropolitan area characteristics aggregated from the CPS data on individuals living in these

**Table 2.** Characteristics of natives, immigrants, and inmates.

	March 1985 CPS <sup>a</sup>		1991 Survey of inmates <sup>b</sup>
	Natives and early immigrants <sup>c</sup>	Recent immigrants	Inmates
Percentage of total population	97.4%	2.6%	—
Citizen	—	—	95.6%
Female	52.7%	47.0%	5.45%
Hispanic	8.4%	35.5%	16.7%
Black	14.3%	8.5%	47.3%
Mean age	41.1	32.3	32.0
Age 15–24	21.3%	33.0%	21.9%
Mean education	12.4	11.3	10.8
Working <sup>d</sup>	64.3%	57.8%	62.2%
Log wage	2.05	1.74	—
Standard deviation of log wages	0.66	0.64	—

<sup>a</sup> March 1985 Current Population Survey (CPS). The sample contains individuals age 15 and older in 43 metropolitan areas. Recent immigrants are defined as individuals living abroad in March 1980.

<sup>b</sup> 1991 Survey of Inmates of State Correctional Facilities. These numbers represent the characteristics of those in correctional facilities in 1991, not the characteristics of those entering correctional facilities during that time.

<sup>c</sup> This column contains everyone except recent immigrants.

<sup>d</sup> In the CPS, “working” is the percentage reporting valid wages for the previous year. Means are weighted using CPS sampling weights. In the inmate survey, working is the percentage reporting employment in the week prior to arrest.

cities. The Office of Management and Budget (OMB) defines MAs for use by government agencies to ensure comparability. Metropolitan areas are defined in terms of entire counties, except in New England. Conditions for identification as an MA include population size and employment conditions [OMB, 1993]. Definitions of MAs are altered each June to reflect growth, and substantial changes are made following the decennial censuses.<sup>11</sup> For example, many changes were made in MA standards as of June 1983 to reflect the 1980 census. Most important to our analysis is the introduction of tighter standards for the inclusion of outlying counties in MAs. Standards for the status “central city” were also tightened at that time. The implications of these changes for our results will be investigated in the fifth section of the article.

Our sample of cities includes the 43 metropolitan areas identified by the CPS throughout the 10-year period from 1979 to 1990. More MAs are available in the later years due to population changes and changes in the confidentiality restrictions of the CPS. However, we use the cities that were consistently available throughout the period. Nonetheless, due to missing crime data in some years, our working sample usually includes fewer than 43 metropolitan areas.

Most of our data are from the 1981–1984 and 1986–1990 March CPSs. We

<sup>11</sup> The information in the remainder of this paragraph comes from a document from the U.S. Bureau of the Census [1992].

leave out March 1980 and March 1985 because the migration variables refer to a different time period than the rest of the March samples. We use the March migration supplements to define recent immigrants as those who report living abroad one year previously. In 1980 and 1985 the migration questions refer to the individual's residence five years ago.

We also use a measure of the stock of foreign born in each city. These numbers are calculated from the November 1979 and the November 1989 CPSs. These two installments of the CPS include supplements that ask questions about country of birth.<sup>12</sup> In this case we define immigrants as those born abroad. Comparable questions about country of birth were asked only sporadically during the 1980s.<sup>13</sup> In this analysis we concentrate on recent immigrants (lived abroad one year ago) and the stock of immigrants in 1979 and 1989 (born abroad).

In addition to the immigrant variables, in some specifications we include other characteristics of the population over age 14 in each of the cities.<sup>14</sup> The variables fraction black, fraction Hispanic, mean age, mean education, and labor market information are from the March CPSs.<sup>15</sup> These city-level values are calculated by taking the weighted (using CPS sampling weights) mean of the relevant variable for each metropolitan area.

Table 3 presents the means of the city-level variables used in the analysis that follows. Note that fraction recent immigrant (within the last year) is a very small number for most cities. It ranges from a high of 0.075 (Miami in 1981) to a low of 0 (various cities, various years). There may be difficulties with defining recent immigrants in this way: The fraction immigrant arriving in the previous year may be too small in some cities to be accurately measured by the CPS.<sup>16</sup> This concern about measurement error motivates part of the analysis in the fifth section of the article.

A disaggregated analysis could be performed for 10-year changes using the 1980 and 1990 censuses. However, using several cross sections of data from the CPS allows us to answer questions the census would not. The 1980 to 1990 changes would cause us to miss important developments affecting crime in urban areas during the decade. For example, use of crack cocaine is widely purported to have increased violent and property crimes in urban areas, yet its usage in many cities began, peaked, and declined all within the 1980s. Our data will allow us to identify any contemporaneous links between immigration and crime which the censuses would not.

<sup>12</sup> The November 1979 CPS includes the "Origin and Foreign Language Supplement." The November 1989 CPS includes the "Language, Immigration, and Emigration Supplement."

<sup>13</sup> In addition to November 1979 and 1989, this information is available in the April 1983, June 1986, and June 1988 CPSs.

<sup>14</sup> Unlike studies of labor markets where only the working age population is relevant, we include most teenagers and all the elderly in our sample. As cited earlier, teenagers make up a substantial portion of those who commit crimes. In addition, all segments of the population are potential victims of crime, though the probability of victimization declines with age past age 24 [U.S. Department of Justice, Bureau of Justice Statistics, 1994]. For these reasons only children under the age of 15 are absent from our sample.

<sup>15</sup> The hourly wage variable is calculated using reports of yearly wages and salaries, weeks worked, and usual hours per week. See Card and Lemieux [1996, pp. 328–330] for a discussion of the use of this wage variable versus the hourly wage rate variable available in the outgoing rotation group samples of the CPS.

<sup>16</sup> In addition, the numbers of immigrants arriving in one year are too small to allow disaggregation by education, age, sex, or race. The March CPSs do not report country of origin until 1994.

**Table 3.** Means for metropolitan areas (MAs).

Variable	Mean	Variable	Mean
Fraction new immigrants	0.006 (0.0004)	Fraction employed	0.651 (0.003)
Mean age	41.05 (0.101)	Fraction immigrant 1979	0.084 (0.004)
Fraction female	0.526 (0.001)	Fraction immigrant 1989	0.097 (0.006)
Fraction black	0.131 (0.005)	Overall crime rate (per 100,000 people)	6373 (95.21)
Fraction Hispanic	0.064 (0.005)	Log overall crime rate	8.725 (0.015)
Mean education	12.38 (0.026)	Violent crime rate	737.0 (19.65)
Fraction central city	0.356 (0.008)	Log violent crime rate	6.498 (0.025)
Population	2,356,654 (97,817)	Change in overall crime rate	-61.36 (27.54)
Log population	14.473 (0.032)	Change in log overall crime rate	-0.010 (0.004)
Mean wage	9.487 (0.101)	Change in violent crime rate	14.51 (4.26)
Mean log wage	2.032 (0.010)	Change in log violent crime rate	0.017 (0.006)
Wage dispersion <sup>a</sup>	2.038 (0.027)	Observations <sup>b</sup>	339

*Source:* Calculations from Current Population Surveys (CPSs) and Uniform Crime Reports (UCR) for years 1981–1984, 1986–1990.

*Note:* Standard errors are in parentheses.

<sup>a</sup>90th–10th percentile of log wages.

<sup>b</sup>There are 333 observations for the variables measured as changes.

## POLICY QUESTIONS AND METHODOLOGICAL APPROACH

There are several policy questions one might want to answer about immigration and crime, each requiring a different form of analysis. Are immigrants more likely than the native born to commit crime? Do immigrants adversely affect the crime rate? To answer the first question, one would use individual data. If such data are available, it is easy to compare the average criminality of immigrants to the average criminality of natives. The first thing we need to know for policy, however, is whether immigrants affect crime by any means, either by committing it themselves or by changing conditions such that crime by natives increases. Because we are interested in this second question, we take an aggregate approach throughout much of the article.

In the latter part of the article, we use individual data to investigate the question

of the relative propensity of immigrants to commit crime. This helps to shed light on whether an immigrant effect on crime is direct or indirect. Fully understanding the routes through which immigration may affect crime is important for a targeted policy response.

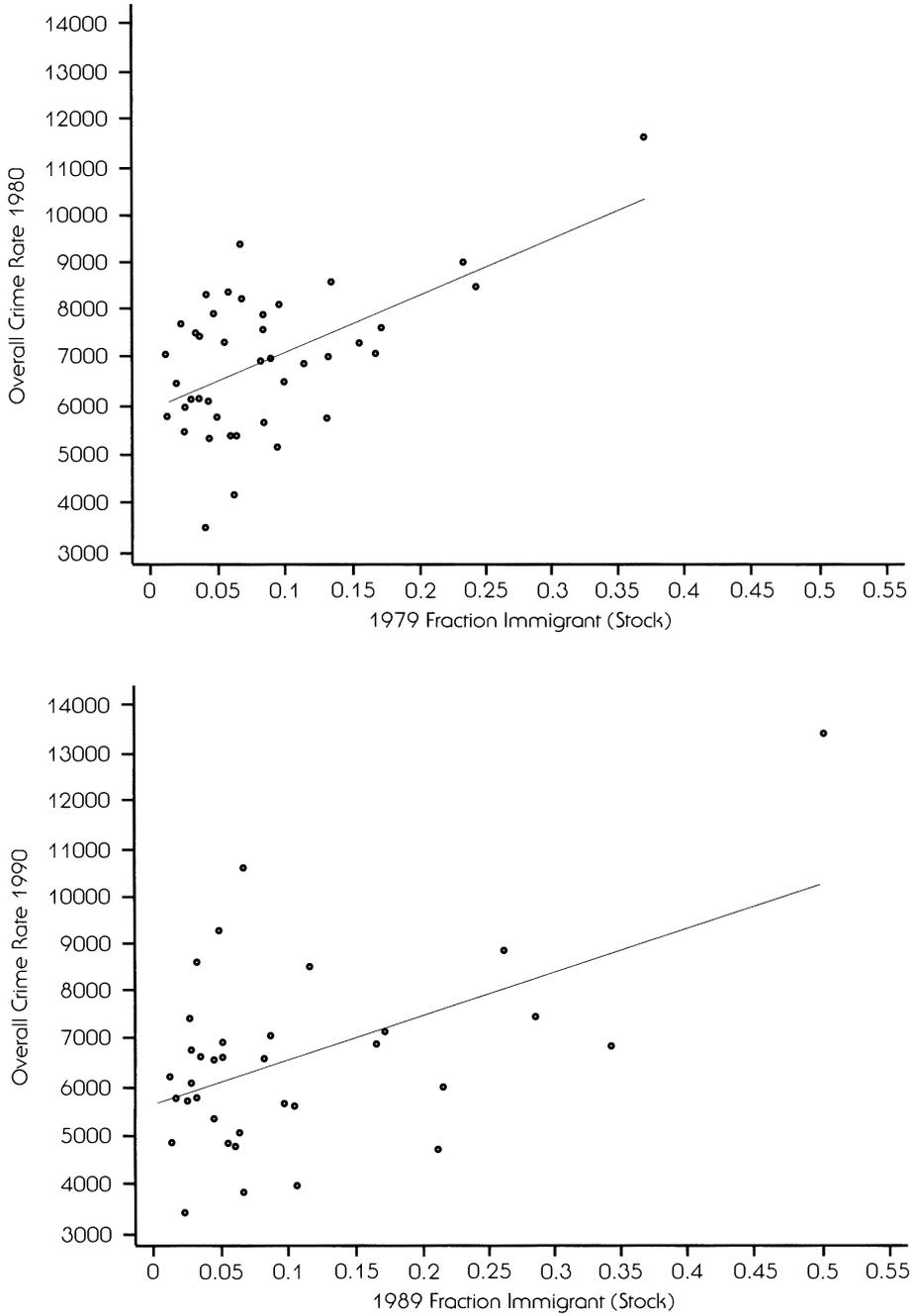
One approach to the question of the aggregate effect of immigration is to compare crime in immigrant-intensive cities to that in cities with few immigrants. If immigration is exogenous and if no other city characteristics (demographic, economic, geographic, environmental) are important to the determination of the crime rate, or at least not in a manner that is correlated with immigration, this approach yields the “right” answer. However, it is likely that other factors are relevant to the determination of a city’s crime rate. There are two ways to respond. First, one can try to control for all those “relevant factors.” Second, one can look at changes in a city’s crime rate over time, under the assumption that many factors that determine the level of crime will be constant within a city over time. It is an easier proposition to control for factors relevant to changes in crime rates than to control for all factors relevant to the level of crime. Looking at changes in panel data is a common technique in the analysis of aggregate and individual data. It is preferred under the assumption that time-series variation is more reliable for identification of the effect of interest than cross-sectional variation. However, it is not necessary that what is true in the cross section must be true in changes, or vice versa. One drawback to this approach is that one will only be confident about the impact of changes in immigration on changes in crime. But from a policy perspective, that is the appropriate dimension. Immigration policy proposals focus on restricting the future flow of immigrants—thus, we care about the impact of the flow of immigration. Later we report results for both levels and changes, but consider the results based on the changes in immigration most relevant for making policy decisions.

It is possible to supplement each of the aforementioned approaches (individual, aggregate analysis of levels, aggregate analysis of changes) to see how the effect of immigrants on the crime rate compares to that of demographically similar natives. We present such “conditional” specifications throughout the article. Although controlling for education, race, ethnicity, and age may not be appropriate to questions about the *number* of immigrants to be admitted, it is relevant to questions of the appropriate mix of immigrants, for example, whether the United States should return to admission criteria based primarily on skills. Throughout, we point out where the conclusions one draws differ depending on whether controls for demographics are included.

In sum, the estimates presented in this article bear on the following: whether the number of immigrants is related to the crime rate; whether the flow of immigrants is related to the crime rate; whether the flow of immigrants is related to changes in the crime rate; whether the characteristics of the immigrant flow are related to the crime rate; and whether immigrants have a higher propensity to commit crime than the native born, both conditional and unconditional on their demographic characteristics.

## **CITY CRIME RATES AND IMMIGRANT STOCK**

Figure 2 shows the relationships between the fraction immigrant (*all* foreign born) in a city and the level of overall crime for the years that begin and end our



**Figure 2.** Overall metropolitan area (MA) crime rates by fraction immigrant: 1980 and 1990.

data series. Cities with very small immigrant populations show a great deal of dispersion in crime rates. High immigration cities all have higher than average levels of crime. The graphs also show the regression lines. The coefficient on immigrant stock is statistically significant for both 1980 and 1990 ( $t$ -statistics = 4.3, 3.3, respectively). The relationship between the immigrant stock and violent crime is even stronger than with overall crime ( $t$ -statistic = 5.8 in 1980;  $t$ -statistic = 4.4 in 1990).<sup>17</sup>

In each figure, the three cities with the highest fraction immigrant are also high crime cities. These cities, Miami, Los Angeles, and New York, account for 48 percent of the recent immigrants living in metropolitan areas in the 1985 March CPS.<sup>18</sup> Without these three cities, the relationship between the crime rate and the foreign-born population is statistically insignificant. Although it changes the complexion of the analysis to include these three cities,<sup>19</sup> it is difficult to see how a sensible analysis of immigration could drop the three locations that account for most of the immigrants and much of the U.S. population. In what follows, Miami, Los Angeles, and New York are always included.

The aforementioned graphs suggest that there is a positive correlation between the fraction immigrant in a city and the level of property and violent crimes. This does not mean that immigrants are responsible for the crime rate; this positive relationship could arise for many reasons. If immigrants are in some part responsible for the crime rate, we would expect to see a change in the level of immigration associated with a change in the crime rate. Figure 3 shows the change in the fraction immigrant in a city (the stock in 1989 minus the stock in 1979) along the horizontal axis and the change in the crime rate (the 1990 level minus the 1980 level) along the vertical axis. There is a good deal of variation in both the change in fraction immigrant and the change in crime rates across cities during the decade. However, there is no relationship between the change in fraction immigrant in a city and the change in overall crime rates.<sup>20</sup>

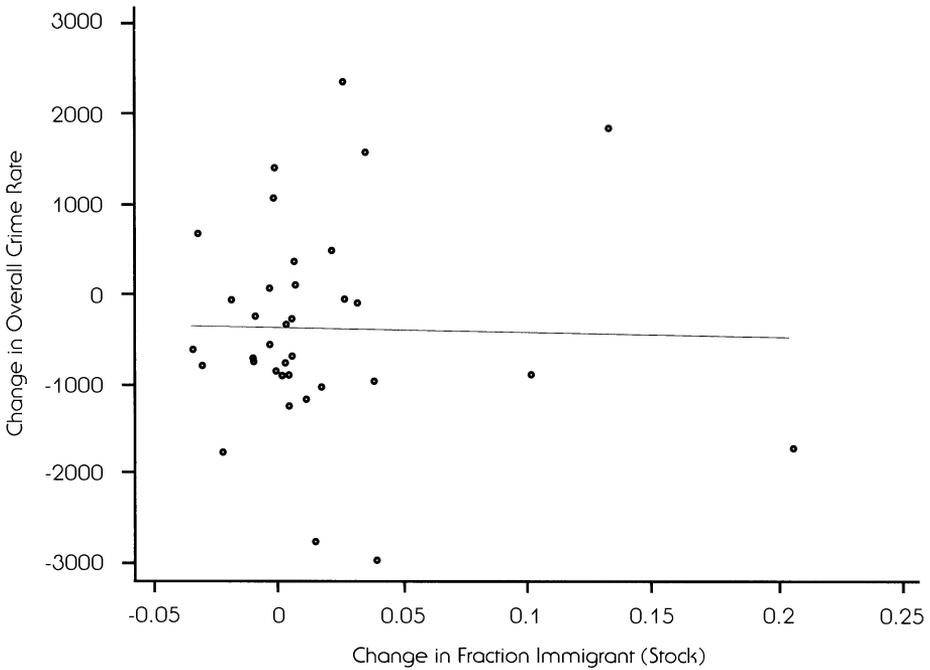
Thus, the cities of Miami, Los Angeles, and New York cause the relationship between the level of crime and the level of immigration to be positive. One way to control for city-specific determinants of crime is to compare changes in these variables. Even in this simple, unconditional analysis there appears to be no substantial relationship between movements in the crime rate and the flow of immigrants into a city. In the analysis that follows, we investigate the effects of additional city characteristics on crime rates and changes in crime rates.

<sup>17</sup> The figure for violent crime is not shown. It is available from the authors upon request.

<sup>18</sup> Recent immigrant refers to anyone arriving in the past *five* years in the March 1985 CPS. We report the 1985 figure because it is the midpoint of our data series. No matter what year of our data or definition of immigrant is chosen, these three cities are by far the most immigrant intensive. Miami, Los Angeles, and New York account for 22 percent of the rest of the population living in metropolitan areas.

<sup>19</sup> Omitting Miami, Los Angeles, and New York, the coefficients ( $t$ -statistics) from a regression of the crime rate on a constant and the fraction of a city's population that is foreign born are 6389 (1.347) for the 1980 overall crime rate; 675 (0.814) for 1980 violent crime; 891 (0.229) for 1990 overall crime; and 126 (0.176) for 1990 violent crime.

<sup>20</sup> The figure for violent crime is not shown because the relationship is similar: The change in fraction immigrant does not affect the change in the violent crime rate. Without Miami, Los Angeles, and New York, the coefficients ( $t$ -statistics) are -4921 (1.058) for overall crime and -233 (0.381) for violent crime. The figure is available upon request.



**Figure 3.** Changes in metropolitan area (MA) crime rates by changes in fraction immigrant, 1980 to 1990.

### MULTIVARIATE ANALYSIS

In this section, we use data across cities and over time to analyze the effect of *recent immigrants* on city crime rates. In the previous section we used total stock of immigrants in a city. Here the immigrant variable of interest is the fraction of a city's population that immigrated from abroad in the previous year. Although this is likely to be a very small fraction of any given city, it captures immigrant inflow rates. The concern over the "immigrant problem" has focused on recent arrivals. In addition to being younger and less well-educated than earlier immigrants and natives, these immigrants may lack the information and skills (especially language skills) necessary for success in legal labor market activities. It may be precisely this segment of the population that is most disruptive.

We test the impact of recent immigrants on city crime rates in several ways. Table 4 presents the effect for (log) crime rates. We first present unconditional estimates of the relationship, then condition upon available economic and demographic variables. Because geographic and infrastructure characteristics of a city that are not captured in the demographic controls might be expected to influence crime rates, we then do two things to address concerns about omitted variables.<sup>21</sup> First, in Table 5 we control for city-level fixed effects. Second,

<sup>21</sup> An example of such a characteristic is being a port of entry for drugs.

Table 6 investigates the growth in crime rates using one-year changes in (log) crime rates and city characteristics. Controlling for city-level fixed effects dramatically affects the results.

Table 4 presents estimated regression coefficients for several different specifications of crime rates for 1981–1984 and 1986–1990.<sup>22</sup> The dependent variables are the log of the overall and violent crime rate per 100,000 in population.<sup>23</sup> We include indicator variables for the year in all specifications. Time is a very important factor in explaining crime rates—the set of year dummies is generally highly (jointly) significant when sufficient controls are included in the regressions.

The first column of Table 4 includes only the fraction new immigrant (within the last year) variable along with an exhaustive set of time dummies. As we saw in Figure 2, there is a positive correlation between the level of crime and the measure of immigration to a city. Because the left-hand side variable is the log of the crime rate per 100,000, this coefficient suggests that an increase in new immigrants of 1 percent of a city's population would lead to a 13.2 percent increase in the overall crime rate. This coefficient is statistically significant at the 5 percent level.

To investigate the source of this effect, column 2 adds other demographic characteristics of the cities—mean age, fraction female, fraction black, fraction Hispanic, and mean education—to the regressions. This specification addresses the question of whether, conditional on a city's demographic characteristics, new immigrants adversely affect crime rates. Mean age is negatively and significantly related to the crime rate.<sup>24</sup> This coefficient suggests that increasing the average age in a city by one year reduces the crime rate by 2.5 percent. The coefficient on fraction female is consistently negative in the various specifications and is sometimes statistically significant (at the 5 percent level), depending on the other variables included. The negative signs on these two variables are consistent with the previous evidence that young males are more likely than other members of the population to commit crimes.

The variables capturing the racial and ethnic composition of a city are both statistically significant. A larger proportion of the population that is African American or Hispanic is associated with higher crime rates. The coefficient on the average education level in the city is positive and significant, implying that cities with more educated populations have higher levels of crime. Education levels within a city are negatively correlated with fraction black and Hispanic in the city. When fraction black and Hispanic are omitted from the regression, mean education is negatively and significantly correlated with crime rates.

Note that including these city characteristics drives the fraction new immigrant variable to insignificance. In fact, simply including fraction Hispanic and mean age wipes out the effect of new immigrants. This is because new immigrants tend to be both young and Hispanic. Fraction new immigrant is highly correlated with fraction Hispanic, which in turn is highly correlated with the total stock of

<sup>22</sup> As stated earlier, 1980 and 1985 are omitted because the definition of recent immigrant is not comparable to the other years.

<sup>23</sup> Specification results showed that the crime rate variables are log normally distributed.

<sup>24</sup> We also tried using fraction of the population in various age cohorts. The age cohort variables were jointly significant and did not change the qualitative analysis.

**Table 4.** Regression coefficients: Log metropolitan area (MA) overall crime rate and log MA violent crime rate.

	Dependent variable: Log MA overall crime rate			Dependent variable: Log MA violent crime rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction new immigrants	13.230** (1.973)	-1.782 (2.093)	-1.066 (2.017)	21.380** (3.330)	-0.499 (3.260)	-1.038 (3.136)
Mean age	— —	-0.025** (0.007)	-0.011 (0.010)	— —	0.018 (0.012)	0.013 (0.015)
Fraction female	— —	-0.811 (0.718)	-1.853** (0.723)	— —	-1.592 (1.119)	-2.061 (1.125)
Fraction black	— —	1.071** (0.175)	1.305** (0.189)	— —	2.747** (0.273)	2.372** (0.294)
Fraction Hispanic	— —	1.692** (0.168)	2.174** (0.310)	— —	2.689** (0.262)	1.632** (0.482)
Mean education	— —	0.098** (0.034)	0.270** (0.047)	— —	0.049 (0.053)	0.056 (0.073)
Fraction central city	— —	— —	0.132 (0.079)	— —	— —	0.180 (0.123)
Log population	— —	— —	0.072** (0.023)	— —	— —	0.196** (0.036)
Mean log wage	— —	— —	-0.663** (0.156)	— —	— —	-0.345 (0.242)
Wage dispersion <sup>a</sup>	— —	— —	-0.060** (0.029)	— —	— —	-0.018 (0.045)
Fraction employed	— —	— —	0.047 (0.347)	— —	— —	-0.134 (0.540)
Fraction immigrant, 1979	— —	— —	-0.535 (0.375)	— —	— —	0.849 (0.583)
Year dummies ( <i>p</i> -value) <sup>b</sup>	0.641	0.003	0.028	0.500	0.376	0.150
R-square	0.1515	0.4152	0.4972	0.4464	0.4774	0.5683
Observations	339	339	339	339	339	339

*Source:* Calculations from Current Population Surveys (CPSs) and Uniform Crime Reports (UCR) for years 1981–1984, 1986–1990.

*Notes:* Standard errors are in parentheses. See Table 1 for a list of the metropolitan areas included.

<sup>a</sup> 90th–10th percentile of log wages.

<sup>b</sup> *P*-value from an *F*-test of the joint significance of an exhaustive set of year dummies.

\*\* Statistically significant at the 5 percent level.

immigrants in a city's population.<sup>25</sup> Thus, this specification indicates that, controlling for the average age and fraction Hispanic in a city, a higher fraction new immigrant in the city has no effect on the crime rate.

**Table 5.** Regression coefficients: Log metropolitan area (MA) overall crime rate and log MA violent crime rate with MA fixed effects.

	Dependent variable: Log MA overall crime rate			Dependent variable: Log MA violent crime rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction new immigrants	-0.774 (0.899)	-0.518 (0.917)	-0.040 (0.935)	-0.247 (1.148)	-0.367 (1.180)	-0.033 (1.200)
Mean age	— —	-0.006 (0.005)	-0.004 (0.006)	— —	-0.008 (0.007)	-0.008 (0.007)
Fraction female	— —	0.171 (0.342)	0.208 (0.352)	— —	0.316 (0.440)	0.410 (0.452)
Fraction black	— —	0.460** (0.206)	0.548** (0.224)	— —	0.094 (0.265)	0.140 (0.287)
Fraction Hispanic	— —	0.467 (0.266)	0.601** (0.272)	— —	0.470 (0.342)	0.474 (0.350)
Mean education	— —	0.032 (0.030)	0.042 (0.032)	— —	-0.009 (0.039)	-0.034 (0.041)
Fraction central city	— —	— —	-0.221** (0.109)	— —	— —	-0.118 (0.140)
Log population	— —	— —	0.080 (0.058)	— —	— —	-0.001 (0.075)
Mean log wage	— —	— —	-0.015 (0.092)	— —	— —	0.180 (0.118)
Wage dispersion <sup>a</sup>	— —	— —	-0.012 (0.029)	— —	— —	-0.083** (0.038)
Fraction employed	— —	— —	0.041 (0.235)	— —	— —	0.025 (0.302)
Year dummies ( <i>p</i> -value) <sup>b</sup>	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
<i>R</i> -square	0.9209	0.9235	0.9253	0.9542	0.9551	0.9563
Observations	339	339	339	339	339	339

Source: Calculations from Current Population Surveys (CPSs) and Uniform Crime Reports (UCR) for years 1981–1984, 1986–1990.

Notes: Each specification includes an exhaustive set of MA indicator variables. Standard errors are in parentheses. See Table 1 for a list of the metropolitan areas included.

<sup>a</sup> 90th–10th percentile of log wages.

<sup>b</sup> *P*-value from an *F*-test of the joint significance of an exhaustive set of year dummies.

\*\* Statistically significant at the 5 percent level.

The next column includes variables that measure the fraction of the total population that lives in the metropolitan area’s central city, and the log of the total population as well as variables meant to capture aspects of the city labor markets.<sup>26</sup> Fraction central city and the total population are positively related to crime rates. Due to changes over time in the definitions of metropolitan areas, the measures of the fraction in central city and population are difficult

**Table 6.** Regression coefficients: First differences in log metropolitan area (MA) overall crime rate and log MA violent crime rate.

	Dependent variable: One-year change in log MA overall crime rate			Dependent variable: One-year change in log MA violent crime rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction new immigrants	-0.227 (0.478)	-0.154 (0.496)	-0.108 (0.499)	-0.190 (0.704)	-0.149 (0.730)	0.065 (0.728)
Δ Mean age	— —	-0.001 (0.003)	-0.002 (0.004)	— —	-0.001 (0.005)	-0.001 (0.005)
Δ Fraction female	— —	0.058 (0.204)	0.002 (0.209)	— —	0.504 (0.300)	0.412 (0.305)
Δ Fraction black	— —	0.117 (0.145)	0.138 (0.152)	— —	0.032 (0.214)	-0.013 (0.221)
Δ Fraction Hispanic	— —	0.062 (0.222)	0.027 (0.228)	— —	0.091 (0.327)	0.046 (0.332)
Δ Mean education	— —	0.029 (0.018)	0.032 (0.019)	— —	0.024 (0.027)	0.028 (0.028)
Δ Fraction central city	— —	— —	-0.073 (0.098)	— —	— —	0.105 (0.143)
Δ Log population	— —	— —	-0.098** (0.050)	— —	— —	-0.217** (0.072)
Δ Mean log wage	— —	— —	-0.031 (0.058)	— —	— —	-0.076 (0.085)
Δ Wage dispersion <sup>a</sup>	— —	— —	0.016 (0.020)	— —	— —	-0.023 (0.029)
Δ Fraction employed	— —	— —	-0.061 (0.132)	— —	— —	-0.026 (0.193)
Year dummies ( <i>p</i> -value) <sup>b</sup>	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
<i>R</i> -square	0.2869	0.2941	0.3055	0.2929	0.3018	0.3257
Observations	333	333	333	333	333	333

Source: Calculations from Current Population Surveys (CPSs) and Uniform Crime Reports (UCR) for years 1981–1984, 1986–1990.

Notes: Standard errors are in parentheses. See Table 1 for a list of the metropolitan areas included.

<sup>a</sup> 90th–10th percentile of log wages.

<sup>b</sup> *P*-value from an *F*-test of the joint significance of an exhaustive set of year dummies.

\*\* Statistically significant at the 5 percent level.

to interpret. The inclusion of time dummies should moderate much of the effect of the changes, but the possibility of definitional influences will be investigated more thoroughly in the next section.

The labor market variables are the fraction employed in the previous year,<sup>27</sup> average log wage, and a measure of wage dispersion. Wage dispersion is defined as the difference between the 90th and 10th percentile of the log wage

distribution. Fraction employed has no effect on the city crime rate. The mean log wage in a city is negatively and significantly associated with the crime rate. Oddly, the wage dispersion coefficient is also negative and significant, implying that an increase in the difference between the wages of the wealthy and the poor is associated with less crime.

On the surface, the negative effect of inequality appears at odds with both economic theory and Relative Deprivation Theory, which holds that larger societal gaps should lead to more social unrest.<sup>28</sup> Relative Deprivation Theory motivates the inclusion of inequality with self-perception and social comparison [see, for example, the volume edited by Olson, Herman, and Zanna, 1986], while economists tend to include the same control variables to proxy for criminal opportunities and incentives [see, for example, Ehrlich, 1973]. The two studies closest in spirit to ours are Blau and Blau [1982], which reports a positive effect of inequality in their study of violent crime across cities using the 1970 census, and Stevans [1988], which finds the same sign in his time-series analysis of New York City crime, 1930–1984. There are many possible explanations for this pattern of findings. The differences in the estimated relationship between inequality and crime may be due to differences in the measures of inequality used, differences in specification,<sup>29</sup> or a change in the relationship between crime and inequality.<sup>30</sup>

Another possible explanation for this result relies on the relationship between wage inequality and the wage level. High-wage cities also have higher levels of inequality. As a result, the wage dispersion variable may simply indicate that these are fast-growing cities, in which the top end of the wage distribution is rising more quickly than the bottom end. This story implies that crime is lower in cities with high wage dispersion due to greater legal labor market opportunities.<sup>31</sup> Sorting out the various explanations for the inequality merits additional research, but is beyond the scope of this article.

<sup>25</sup> In the third column we include the fraction immigrant in 1979. This variable measures the fraction of the city that was foreign born in the beginning of the period. This is also highly correlated with the fraction Hispanic and the fraction new immigrant. The effect of the fraction immigrant in 1979 is negative (insignificant) and its inclusion increases the size of the coefficient on fraction Hispanic. If the coefficient were significantly different from zero, this pattern would suggest that if a city has a high fraction Hispanic, it is better-off in terms of crime if they are immigrants rather than native born.

<sup>26</sup> When the labor market and population variables are included without the age, race, and ethnicity controls, the coefficient on recent immigrants falls to 9.3 and is still statistically significant if the level of immigration in 1979 is omitted. The coefficient is not statistically significant when the level of immigration is included. This is true whether or not the three most immigrant-intensive cities are included in the sample.

<sup>27</sup> We construct this variable from the wages and salary, weeks worked, and usual hours per week variables in the March CPSs.

<sup>28</sup> It should be noted that the empirical success of this theory is at best modest [see reviews by Gurney and Tierney, 1982, and Finkel and Rule, 1986]. Further, these theories are most often tested in the sphere of civil violence, often in cross-national studies, which may or may not translate to the criminal behavior studied here.

<sup>29</sup> For example, Stevans [1988] does not control for time effects, which we found to be particularly important.

<sup>30</sup> Because inequality in the wage distribution increased dramatically during the 1980s [Juhn, Murphy, Pierce, 1993], it would not be surprising if other relationships in the labor market changed as well.

<sup>31</sup> One should be cautious about interpreting legal and illegal activities strictly as substitutes for one another. Carrington [1996] found an increase in crime in Alaska during the labor market boom that occurred as a result of the pipeline. MacCoun and Reuter [1992] find that among probationers who self-report income from drug sales, 64 percent had legal jobs as well. In addition, median earnings from drug sales were approximately equal to median earnings from legal sources.

In general, labor market variables may be difficult to interpret because average wages and inequality may be affected by the level of immigration into a city. Many studies have analyzed the effects of immigrants on the labor market outcomes of natives. Butcher and Card [1991] find no effect of the fraction recent immigrant on the growth in wage inequality across cities in the 1980s. Altonji and Card [1991] in their study of the effect of immigrants on less-skilled natives by industry find no adverse effect of immigrants. Similarly, Card [1990] finds no effect of the influx of the Mariel immigrants on wages and employment of less-skilled natives in Miami. These studies all suggest that it is unlikely that immigrants will have an indirect effect on crime by affecting natives' legal sector opportunities. However, Borjas, Freeman, and Katz [1992] suggest that the effects are difficult to find when focusing on distinct city labor markets because the effects are ameliorated by labor mobility. This problem of simultaneity will be discussed further.

Keeping these problems of interpretation in mind, we look at the effect of these variables on the levels of violent crime. Columns 4, 5, and 6 of Table 4 present the same specifications for the log of the violent crime rate per 100,000 population. We see a similar pattern: When fraction new immigrant is entered on its own, it is positive and significant. However, when other control variables are added, the coefficient falls dramatically and becomes insignificant.<sup>32</sup> The demographic variables have similar effects on levels of violent crime rates as on overall crime rates. Again, fractions black and Hispanic have large positive effects. Interestingly, the labor market variables are insignificant in explaining rates of violent crime. It may be that some types of property crime are substitutes for legal labor market activities while violent crimes are motivated by other factors.<sup>33</sup>

Many hypotheses about the determinants of cross-section variation have been proposed in the geography of crime literature, though none has emerged as having strong empirical explanatory power. Some hypotheses focus on the place itself: climate, built environment, natural environment, and density of criminal opportunity. Other ideas emphasize the social environment: shared values and behavior, differential precautions due to fear of crime, and differential reporting practices.<sup>34</sup> The role of the criminal justice system could also be location-specific and slow to change.

Although the nature of the data collected in the CPS precludes examination of such factors, they can be accounted for by including a fixed effect for each city when estimating the crime equation. This technique prevents scrutiny of all determinants of crime, but it allows an unbiased estimate of the factor of interest in this article: the flow of immigrants. The fixed effect absorbs all factors that do not change over time. As such, the fixed effects summarize the usual level of crime in the city, including the impact of the stock of immigrants. Here

<sup>32</sup> As was true with the overall crime rate, when the labor market and population variables are included without the age, race, and ethnicity controls, the coefficient on recent immigrants is positive and statistically significant if the level of immigration in 1979 is omitted. The coefficient is not statistically significant when the level of immigration is included. When Miami, Los Angeles, and New York are excluded, the coefficient on recent immigration is insignificant, whether or not the initial level of immigration is controlled.

<sup>33</sup> Recall that robberies are 35 percent of the violent crimes and may be motivated by material gains.

<sup>34</sup> For the geography of crime literature, see Skogan [1990], Herbert [1982], and the volumes edited by Evans and Herbert [1989], Reiss and Tonry [1986], and Georges-Abeyie and Harries [1980].

we focus on the impact of the flow of immigrants, deferring consideration of long-term issues, such as assimilation, until the fifth section of the article.

In Table 5, we repeat the earlier specifications but include an indicator variable for each metropolitan area. We continue to control for year effects. Comparing the  $R^2$ 's in Table 4 with those in Table 5, one can see that the city-level fixed effects are extremely important in explaining variation in crime rates. When we include these controls to explain overall crime, few coefficients remain statistically significant. The size of the effects of fraction African American and fraction Hispanic falls dramatically. Interestingly, the fraction central city variable becomes negative and marginally significant, and population falls to insignificance in these specifications. This suggests that the positive effects in the previous specifications were related to city fixed effects. For violent crimes, columns 4, 5, and 6, city fixed effects are also extremely important. The only variable that is significant is wage inequality. The sign may be unexpected: A higher level of wage inequality is associated with lower rates of violent crime.

In Table 6 we control for city-level fixed effects by running the first differences in crime rates on the differenced right-hand side variables. This allows us to answer whether cities with higher immigrant inflows experienced more *growth* in crime rates. Fraction new immigrant is already a measure of a change in the population, so we do not take the first difference of this variable. The results in Table 6 are striking: Not only does the fraction new immigrant not help to explain growth in crime, none of our other population characteristics does either. Only the year indicator variables and the change in log population are significant in explaining the changes in overall and violent crime rates.<sup>35</sup>

## DISCUSSION AND SUMMARY OF CITY-LEVEL ANALYSIS

The lack of explanatory power in the first-difference specifications is somewhat disturbing. One might be more secure in concluding that immigrant inflows do not explain growth in crime rates if we could explain it with some demographic changes. There are several potential reasons for this estimated zero effect. In terms of substantive issues, it may simply be that demographic changes do not cause growth in crime rates. Or, it may be that internal migration flows counteract the effects of immigration, or that the effects of immigration only appear over a longer period of time than these one-year changes allow us to investigate. A second set of issues is more technical: Perhaps our data are plagued with measurement error, or perhaps the data are serially correlated. Either of these technical problems might bias our results. These substantive and technical issues will be discussed in turn.

### Substantive Issues

The interpretation put forth thus far, that demographic changes are not in fact reflected in city crime rates, may not be surprising given that the consensus in the crime literature is that it is very difficult to explain crime. For example, the

<sup>35</sup> Under certain models of the determination of crime rates, the initial level of immigration might affect the growth in crime. In fact, when we include this variable in the first-difference specifications reported in Table 6, it is never large or statistically significant.

fact that the violent crime rate in the United States is an outlier among industrialized nations is well known,<sup>36</sup> but the reasons for this are not well understood. It is possible that researchers have not yet discovered the determinants of crime or useable measures of those determinants. The factors that make crime more prevalent in urban areas, on average, may also explain why crime rates are higher in some cities than others.<sup>37</sup>

### *Internal Migration*

A second explanation for the estimated zero effect of immigrant inflows is one of simultaneous determination of immigration, other migration, legal labor market activity, and criminal activity. Filer [1992] reports that immigration into a city increases native outflows and reduces the number of natives moving in. If this is the case, one would expect to see immigrant inflows offset by native outflows. Assuming that immigrants take the jobs natives abandon, wages and employment levels would remain relatively stable in the city. To the extent that criminal and legal labor market activities are substitutes, the immigrants will not engage in criminal activity because they will have legal sector opportunities bequeathed to them by the fleeing natives. Thus, population changes might bias one away from finding an effect of immigrant inflows on crime rates.

On the other hand, both immigrants and natives may move to the growing cities that offer good labor market opportunities. Contrary to Filer's finding, Butcher and Card [1991] report that except for the three most immigrant-intensive cities (Miami, Los Angeles, and New York), immigrants and natives appear to have moved to the same cities during the 1980s.<sup>38</sup> The labor market opportunities may reduce the need to engage in criminal activity.

In both of the aforementioned cases, the recent immigrant coefficient is biased toward zero. However, the timing of the effect is different. In the first case, immigrants drive natives out, reduce the population, and take the jobs natives have abandoned. In the second case, immigrants, like natives, only move to a city because there appear to be ample labor market opportunities. In either case, transitory changes in local economic opportunities will lead to biased coefficients in the first-difference analysis if they are correlated with the explanatory variables. Following Altonji and Card [1991, p. 219], we attempt to control for potential correlations between immigrant inflows and local economic conditions by using an instrumental variables procedure. This procedure requires that we use changes in crime rates and fraction foreign born over the 10-year period, rather than the fraction immigrating in the previous year. We then use the initial fraction foreign born (1979) as a predictor of immigrant inflows over the decade. The change in the fraction foreign born is positively and significantly related to the fraction foreign born in 1979, that is, those cities with initially high levels of immigrants receive more immigrants over the decade. Table A.1 reports the results of running the change in the overall crime

<sup>36</sup> See, for example, Blumstein [1988] and Lynch [1995].

<sup>37</sup> See Glaeser and Sacerdote [1996] for a paper along these lines. Although they show that higher crime rates are associated with the types of people who live in cities, they cannot explain the residential patterns.

<sup>38</sup> Butcher and Card [1991] use a different sample of cities from the CPS. In our sample, we also find a positive correlation between population growth and the fraction of the population comprised of new immigrants.

rate from 1980 to 1990 on the instrumented change in *total* fraction foreign born from 1979 to 1989 (analogous to Figure 3). The instrumental variables procedure does not change our conclusion that increases in immigration have no impact on growth in crime rates. These results suggest that simultaneity is not responsible for the zero coefficient on immigration in the first differences.

### *Assimilation and Other Long-Term Effects*

Another potential problem with the analysis in Tables 4, 5, and 6 pertains to the timing of immigration effects. Although the flow of new immigrants is important to policy, these immigrants may become more like Americans over time. It appears that recent immigrants have much lower criminality when they first arrive [Butcher and Piehl, forthcoming]. It is possible that, with time in the country, immigrants assimilate to the illegal sector as well as to the legal sector. As a result, our results based on recent immigration may not reflect the long-term consequence of immigration on crime. From a theoretical perspective, the effect of assimilation is unclear. One view is that immigrants assimilate to all things American—including legal and illegal activity. Another view is that the relationship between the rates of assimilation into legal and illegal activity is important. It is possible that any adverse effect of immigration on crime will attenuate with time in the country. If time in the legal sector is a substitute for time in the illegal sector and immigrants assimilate more quickly into the illegal sector, but take longer to develop skills needed to compete with natives in the legal sector, then the relative criminality of immigrants should decline, not grow, with time in the country.

Although the aggregate approach taken in this article does not allow us to fully understand the process of assimilation, we address it in several ways. Using the change in crime rate over the 10-year period 1981–1990, we allow the fraction recent immigrant in 1981 through 1984 and 1986 through 1990 to enter the regression separately.<sup>39</sup> We also let the sum of the yearly measures of the fraction recent immigrant enter the regression. We would expect these variables to be significantly related to growth in crime if there is a long-term effect of receiving successive waves of immigrants. The results are reported in Table A.2. None of these variables is significantly related to growth in crime. This corroborates the findings in Figure 3 where we show that the 10-year change in the stock of immigrants has no effect on the 10-year change in the crime rate.<sup>40</sup>

We also investigate the timing issue by allowing one-, two-, and three-year lags of fraction recent immigrant to enter regressions for the log of the crime rate. The lagged values are neither individually nor jointly significant in explaining overall or violent crime. Together, these efforts do not suggest that assimilation of immigrants into criminal activity, at least within the first 10 years since arrival in the United States, is substantial.<sup>41</sup>

<sup>39</sup> This necessarily cuts our sample size. We have only 32 MAs with useable data on the 1980–1990 change in crime rates. We use 1981 to begin the period because the 1980 CPS defines fraction immigrant differently from the other years.

<sup>40</sup> This result also emerges in Table A.1.

<sup>41</sup> Butcher and Piehl [forthcoming] provide some insight into the assimilation question in an analysis of institutionalization in 1980 and 1990. Immigrants are substantially less likely to be institutionalized than the native born. Within the immigrant cohort, there is some assimilation toward the higher rate of natives, but less than would be predicted from the institutionalization experience of immigrants who arrived prior to 1970. Among other things, this suggests that more recent cohorts of immigrants may have lower criminal propensities than earlier immigrants.

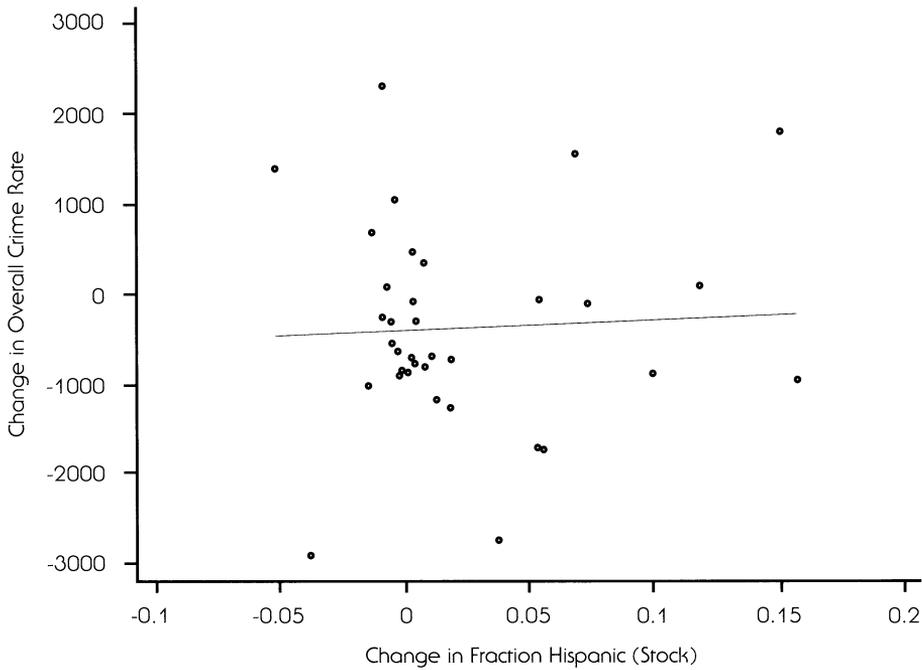
A second long-term effect of immigration has to do with how immigrants alter the composition of a city. For example, if the fraction Hispanic is positively correlated with crime, then immigrants may increase the crime rate by increasing the fraction Hispanic, even if they are no more likely to commit crimes than demographically comparable natives. To investigate this hypothesis, we plotted the change in the crime rate from 1980 to 1990 against the change in fraction Hispanic from 1980 to 1990. Figure 4 presents the results. Although some cities show a dramatic increase in the fraction of the population that is Hispanic, they do not have higher than average increases in crime over the decade. The regression line is very flat and the coefficient on Hispanic is insignificant ( $t$ -statistic = 0.350). There is a slight positive relationship between the change in the fraction Hispanic and the change in the violent crime rate, but the relationship is statistically insignificant ( $t$ -statistic = 1.3, available upon request). We also ran the one-year changes in log overall crime rates and log violent crime rates on the change in fraction Hispanic. The effect is statistically insignificant. Thus, even though immigrants increase a segment of the population that is positively correlated with crime levels, this increase does not lead to an increase in crime.

## Technical Issues

### *Measurement Error*

There are several technical issues that might bias our results away from finding an effect of immigration on growth in crime. First, the fraction new immigrant within the last year may be too small in most cities to be accurately sampled by the CPS. However, the 10-year change in a large segment of the population should be well captured. Therefore, a robust test is to use the fact that there are some cities, those with high immigration, where the fraction Hispanic increased dramatically over the decade. Again, Figure 4 addresses this issue: The change in the Hispanic population had no effect on the change in crime rates between 1980 and 1990. This suggests that our findings are not merely an artifact of poorly measured changes in demographics from year to year.

A second potential source of measurement error comes from changes in definitions of metropolitan areas and central cities during the 1980s. In 1983, the OMB tightened the boundaries of many MAs during its redefinition following the 1980 census. If low crime suburbs were dropped as part of the metropolitan area, this definitional change would cause the crime rate to appear to increase without any change in the underlying criminal behavior. This does not a priori create a problem for our analysis of the effect of immigrants, unless the changes in metropolitan area definitions are somehow correlated with residential patterns of immigrants. For example, if immigrants live in the suburbs of one city and these suburbs are eliminated from the metropolitan area at some point during our data series, the fraction immigrant would appear to fall and the crime rate would appear to increase. This would lead to a spurious negative correlation between fraction immigrant and the crime rate. If, on the other hand, immigrants are located only in the central city of another metropolitan area, when the suburbs are dropped from the MA definition, both the fraction immigrant and the crime rate will appear to rise. In order to test for this problem, we re-ran our regressions using only data from after 1983. Changing the sample in this way does not change the estimated zero impact of fraction recent immigrant on changes in the crime rate.



**Figure 4.** Changes in metropolitan area (MA) crime rates by changes in fraction Hispanic, 1980 to 1990.

### *Identification*

It is also possible that cities may have a relatively constant rate of immigration inflow. If this is the case, then our fixed-effects estimates, which rely on variation in the inflow rate within a city, may have too little power to identify an effect of recent immigration. Again, we refer to the results for the 10-year changes in immigration. There is substantial variation in the immigration rate over 10 years. The results in Figure 3 show both the extent of this variation and the robustness of the conclusion that these changes are not correlated with changes in crime rates.

### *Serial Correlation*

A third technical issue relates to serial correlation in crime rates within metropolitan areas. Serial correlation would imply that the OLS standard errors are too small and standards for inference too lax. Because accounting for serial correlation would not change our conclusions about the importance of immigration in explaining crime rates, we instead allow for a flexible correlation matrix over time within each city, which permits autocorrelation as well as other types of time-series correlation. The effect of this procedure is to slightly decrease the standard errors on the immigration coefficient and to slightly increase the standard errors on the other coefficients, without altering inference.<sup>42</sup>

<sup>42</sup> These results are available upon request.

### *Omitted Variables*

Finally, we might be able to explain more of the variation in the growth in crime rates by including data on drug use within a city. In practice, it is difficult to obtain a consistent data series on drug use. The DUF (Drug Use Forecasting) series, which comes from drug testing arrestees, provides city-level data, but did not begin until 1989. Data from emergency room incidents that are collected in the Drug Abuse Warning Network do not allow for analysis of a broad range of cities, due to its idiosyncratic sampling design.<sup>43</sup> Although including information about drugs may increase the  $R^2$ 's in our regressions, for this article we are most concerned with whether this omission biases the estimated coefficient on fraction immigrant downward. Drug availability and fraction immigrant are both higher in gateway cities. In addition, both increased over the decade. Therefore, omitting drug use from the regression equation should load onto the immigration coefficient. If anything, then, the coefficient on immigration is biased upward due to the lack of data on drug use and availability.

### INDIVIDUAL CRIMINAL ACTIVITY

We bring one final piece of evidence to bear on the question of immigrant effects on crime. In order to analyze the difference in immigrant and native propensities to commit crime, we turn to individual data from the National Longitudinal Survey of Youth (NLSY). This will provide information on whether or not immigrants are more likely than natives to commit crimes.

As part of the 1980 survey, a battery of questions concerning criminal activity was administered. The respondents were between 15 and 23 years old at the time. Some of the questions asked about criminal activity during the past 12 months. Respondents were also asked about interactions with the criminal justice system.<sup>44</sup>

As with all self-reported information, there is the worry that respondents do not answer the questions accurately. Criminal activity may be inaccurately reported by those who think answering the questionnaire may increase their chance of being punished. However, it seems less likely that individuals would misreport actual run-ins with the criminal justice system. Following this reasoning, we use a variety of indicators of criminality. It should be noted that we are not worried about whether individuals lie per se, but rather about whether immigrants and natives lie differentially.<sup>45</sup>

<sup>43</sup> Personal conversation with an information specialist at the National Criminal Justice Reference Service.

<sup>44</sup> Crime questions are of the form: "How many times have you ... in the past year?" Criminal justice questions are of the form: "Have you ever been ...?" The latter refer to such acts as being stopped, charged, or convicted of a crime.

<sup>45</sup> Validation research provides some evidence of ethnic differences in willingness to self report drug use. Aquilino [1994] tested response patterns to questions about the use of alcohol and illicit drugs using different interview modes. He found that admission of use of these substances was highest with a personal visit and self-administered questionnaire, somewhat lower with personal interview, and lower still with phone interview. More importantly, blacks consistently reported lower levels of drug use when interviewed over the phone relative to the personal visit with questionnaire, while Hispanics sometimes reported the least in personal interviews (without questionnaire). (The NLSY is administered in a personal interview.) Although it is not clear whether these findings can be extrapolated to our context (asking immigrants about criminal justice involvement), they provide a caution that findings may be sensitive to the way in which the data are collected.

The means for the samples of the NLSY are reported in Table A.3. Although the age distributions are the same for those born in the United States and those born abroad, the two groups differ on many dimensions. Immigrants are significantly less educated, and are more likely to be nonwhite, Hispanic, and urban residents. Immigrants are less likely to report criminality on all five of our measures: self-reported crime,<sup>46</sup> being stopped by police, being charged with a crime, being convicted of a crime, and a summary indicator of contact with criminal justice agencies. Although women are less likely to be involved in crime, men and women show the same relationships by immigrant status.

Because immigrants are disproportionately characterized by demographic attributes correlated with criminality, controlling for demographic characteristics only reinforces what is found in the means. Table 7 shows that immigrant men and women are less likely than the native born to report committing crime (15.9 percent and 17.6 percent, respectively), and less likely to have come in contact with a criminal justice agency (10.6 percent and 4.8 percent, respectively).

Several other variables in Table 7 are of interest. Education seems to play a particularly important role both in committing crime and in conviction probabilities.<sup>47</sup> The negative coefficients on nonwhite are routinely found in analyses of self-reported data, especially controlling for income or education [Freeman, 1992]. Finally, living in a city greatly increases the chances of being stopped by police.<sup>48</sup>

Table 8 reports the results of using an expanded set of covariates to explain criminality at the individual level. In Table 8 we include information about the respondent's parents and interact immigrant status with many of the covariates. These specifications suggest that the relationship between individual and family background and criminal activity are different for immigrants and natives. Among the other relationships, note that the negative education effect is generally weaker for immigrants than for natives. Living in the same household with one's father is associated with lower probabilities of committing crime for all respondents, but this relationship is particularly strong for male immigrants.<sup>49</sup>

The analysis of the NLSY clearly implies that immigrants are less likely to commit crime than natives. Because the costs of being caught committing a crime may be greater for legal and illegal immigrants than for natives (they

<sup>46</sup> Self-reported crime excludes truancy, running away, and drug use. Traffic violations are excluded from all measures.

<sup>47</sup> See Piehl [1994] for a discussion of the same relationship in the Boston Youth Survey.

<sup>48</sup> To make the analysis of the individual data more comparable to the city-level analysis, one might want to include MA fixed effects. However, the sample size in the NLSY prohibits identification of fixed effects for more than a few cities. Furthermore, recall that high-crime cities have disproportionate shares of the immigrant population. If living in a high-crime city increases the propensity to commit crime, using only within-city variation would serve to strengthen the conclusion that immigrants are less likely to commit crime than comparable natives.

<sup>49</sup> Because the probit results only confirm the lower criminality among immigrants seen in the means, we refrain from presenting multiple specifications. Some readers, however, might be interested to know a few more details. First, running the four criminal justice variables on the sample of self-admitted criminals does not change the estimates. The indicator variables for type of crime reported seem to act as sufficient controls. Second, adding explanatory variables such as "in school" or "drug user" does not substantially change the other estimates (except in explaining self-reported crime for men, where education becomes statistically insignificant). These covariates are themselves statistically significant with the expected signs. Third, reclassifying all who refused to answer any crime question as criminals does not change the inference on immigrant status.

**Table 7.** Weighted probits—National Longitudinal Survey of Youth (NLSY).

	Men			Women		
	(1) Crime in past year	(2) Convicted	(3) Stop/ book/ charge/ convict	(4) Crime in past year	(5) Convicted	(6) Stop/ book/ charge/ convict
Immigrant	-0.159** (0.033)	-0.045** (0.016)	-0.106** (0.034)	-0.176** (0.034)	-0.003 (0.004)	-0.048** (0.018)
Highest grade completed	-0.015** (0.004)	-0.020** (0.002)	-0.047** (0.005)	-0.022** (0.005)	-0.004** (0.001)	-0.023** (0.003)
Nonwhite	-0.026 (0.017)	-0.014 (0.009)	0.026 (0.019)	0.048** (0.018)	-0.005** (0.002)	-0.019** (0.010)
Hispanic	-0.020 (0.027)	0.017 (0.015)	0.033 (0.030)	-0.060** (0.029)	0.002 (0.004)	0.024 (0.017)
Urban resident	0.028 (0.015)	-0.004 (0.008)	0.059** (0.017)	0.037** (0.017)	0.007** (0.002)	0.025** (0.009)
Local unemployment rate	0.649** (0.252)	0.096 (0.128)	-0.188 (0.272)	0.261 (0.281)	0.041 (0.038)	0.326** (0.152)
Log likelihood	-2931.442	-1330.204	-2958.558	-3713.740	-418.270	-1665.524
<i>n</i>	5205	5205	5205	5504	5504	5504

*Notes:* The table reports marginal probabilities, with standard errors in parentheses. For continuous variables, the marginal probabilities are evaluated at the means of the covariates. For indicator variables, the reported number is the marginal effect of changing that variable from 0 to 1. Each specification includes a constant and a complete set of age dummies, which are always jointly significant ( $p$ -value < 0.001). The specifications in columns 2, 3, 5, and 6 include complete sets of offense dummies, which are also jointly significant.

\*\* Statistically significant at the 5 percent level.

may face deportation, for instance [see Butcher and Piehl, 1998]), this result is predicted by an economic model of crime in which individuals weigh the costs and benefits of criminal activity in choosing how to allocate their time.<sup>50</sup>

It is possible that much has changed since 1980, however, making these results unsuited for comparison with the city-level analysis presented earlier. In fact, the very purpose of this article is to consider changes in immigration during the 1980s. Nonetheless, the results reveal a strong negative relationship between immigrant status and the commission of crime. Even without controlling for any background characteristics, immigrant youth are less likely than natives to be involved in crime.

<sup>50</sup> The possibility of spillovers from immigrants to other population groups is included in the city-level analyses but not in the individual-level analyses. Grogger [1998] provides further evidence from the NLSY on potential spillovers from immigrants onto other populations. He investigates the effect of living in a high-immigrant MA on an individual's criminality, finding no effect of immigrant concentrations on the criminality of African American youth.

**Table 8.** Weighted probits—National Longitudinal Survey of Youth (NLSY).

	Men			Women		
	(1) Crime in past year	(2) Convicted	(3) Stop/ book/ charge/ convict	(4) Crime in past year	(5) Convicted	(6) Stop/ book/ charge/ convict
Immigrant	-0.604** (0.180)	-0.071 (0.029)	-0.337** (0.108)	-0.561** (0.141)	-0.007 (0.006)	-0.106** (0.033)
Highest grade completed	-0.020** (0.005)	-0.015** (0.003)	-0.045** (0.006)	-0.028** (0.006)	-0.003** (0.001)	-0.027** (0.003)
Immigrant* high grade	0.042** (0.013)	0.004 (0.009)	0.027 (0.016)	0.028 (0.017)	0.001 (0.002)	0.024** (0.011)
Nonwhite	-0.045** (0.019)	-0.025** (0.008)	0.006 (0.020)	0.037 (0.020)	-0.004 (0.002)	-0.012 (0.011)
Immigrant* nonwhite	0.051 (0.066)	0.037 (0.037)	-0.024 (0.084)	0.023 (0.080)	-0.003 (0.008)	-0.040 (0.044)
Hispanic	0.016 (0.031)	0.008 (0.015)	0.019 (0.034)	-0.011 (0.033)	0.005 (0.004)	0.039** (0.019)
Immigrant* Hispanic	-0.073 (0.074)	0.092 (0.074)	0.113 (0.089)	-0.095 (0.092)	-0.001 (0.010)	0.036 (0.064)
Urban resident	0.024 (0.016)	-0.004 (0.008)	0.056** (0.017)	0.034** (0.017)	0.005** (0.002)	0.025** (0.009)
Local unemployment rate	0.685** (0.256)	0.061 (0.124)	-0.132 (0.276)	0.299 (0.285)	0.045 (0.030)	0.420** (0.150)
Mother's high grade	0.002 (0.003)	0.002 (0.002)	0.001 (0.004)	-0.004 (0.004)	0.0002 (0.0004)	-0.0004 (0.002)
Immigrant* mother's education	-0.012 (0.010)	0.0004 (0.007)	-0.005 (0.012)	0.010 (0.013)	-0.002 (0.002)	-0.005 (0.009)
Father's high grade	0.005 (0.003)	-0.004** (0.001)	-0.006** (0.003)	0.006** (0.003)	-0.0001 (0.0003)	0.002 (0.002)
Immigrant* father's education	0.003 (0.009)	0.010 (0.006)	0.009 (0.011)	0.011 (0.012)	0.003 (0.002)	0.017 (0.009)
Mother works	0.033 (0.024)	-0.00009 (0.012)	0.021 (0.026)	-0.024 (0.026)	-0.008 (0.005)	0.004 (0.015)
Immigrant* mother works	-0.069 (0.065)	-0.025 (0.036)	0.245** (0.087)	0.061 (0.078)	0.006 (0.014)	0.012 (0.053)
Father works	0.012 (0.029)	-0.003 (0.014)	0.008 (0.031)	0.064** (0.032)	0.002 (0.003)	-0.009 (0.018)
Immigrant* father works	0.060 (0.073)	0.081 (0.082)	0.050 (0.098)	0.053 (0.101)	-0.005 (0.009)	0.007 (0.072)
Lives with mother	-0.029 (0.020)	-0.006 (0.010)	0.001 (0.022)	0.018 (0.024)	-0.0001 (0.002)	-0.009 (0.013)

(continued)

**Table 8 (cont'd).** Weighted probits—National Longitudinal Survey of Youth (NLSY).

	Men			Women		
	(1) Crime in past year	(2) Convicted	(3) Stop/ book/ charge/ convict	(4) Crime in past year	(5) Convicted	(6) Stop/ book/ charge/ convict
Immigrant* lives with mother	0.188** (0.068)	0.017 (0.074)	-0.045 (0.108)	0.109 (0.103)	-0.004 (0.010)	0.027 (0.075)
Lives with father	-0.038** (0.020)	-0.021** (0.010)	-0.063** (0.021)	-0.065** (0.023)	-0.004 (0.003)	-0.003 (0.012)
Immigrant* lives with father	-0.254** (0.107)	0.023 (0.077)	0.011 (0.109)	0.087 (0.099)	0.100 (0.075)	0.011 (0.070)
Log likelihood	-2890.209	-1292.013	-2926.587	-3669.274	-392.050	-1632.839
<i>n</i>	5205	5205	5205	5504	5504	5504

*Notes:* The table reports marginal probabilities, with standard errors in parentheses. For continuous variables, the marginal probabilities are evaluated at the means of the covariates. For indicator variables, the reported number is the marginal effect of changing that variable from 0 to 1. Each specification includes a constant, a complete set of age dummies, mother's occupation dummies, father's occupation dummies, and indicators for unknown parental education. The specifications in columns 2, 3, 5, and 6 include complete sets of offense dummies, which are also jointly significant.

\*\*Statistically significant at the 5 percent level.

## CONCLUSION

This article has analyzed the relationship between immigration and crime, primarily at the city level. Although cities with high levels of immigration tend to have high crime rates, we find no relationship between changes in crime and changes in immigration, measured either as year-to-year or over 10 years (1980–1990). This zero effect arises with no controls other than year effects. Evidence from the NLSY that young immigrants are less likely than natives to commit crime reinforces this finding. Neither of these results are based on estimates conditional upon demographic characteristics. Thus, it does not appear that reducing the number of new immigrants will lead to a measurable impact on crime rates. The results of the individual-level estimation confirm other research that younger, less-educated males are more likely to engage in crime. This is true within both the immigrant and native populations. Therefore, altering the mix of immigrants away from this group may reduce the average crime rate among immigrants.

Many of the issues in the public arena related to crime and to immigration receive careless attention. At the same time, social goals are often unstated, so it is hard to select appropriate policy responses. If society's goal is to minimize the total number of crimes, then population growth in general is detrimental. Admitting even one immigrant who may become a perpetrator or a victim of a crime would increase the number of crimes, other things being equal. If the public concern is the crime rate, the results of this article imply that the focus

should not be on immigration policy. If the public concern is the crime rate against native-born victims, it is quite possible that immigration causes that to fall, because new immigrants have characteristics that place them at high risk of victimization. In fact, we find no support for blaming high crime rates on immigrants or immigration policy.

If the public concern is expenditure on criminal justice, there are two parts to the argument: which level of government should bear the cost and how much should be spent. Although states with a large share of immigrants gained federal money to defray the costs of incarcerating illegal immigrants in the 1994 crime act, the fight over who should bear the costs of immigration more generally will continue. Research of the type presented in this article has no role to play in resolving this part of the debate over expenditure.

Our research does have a role in discussions about the extent of expenditure. Immigrants may adversely affect the criminal justice budget through higher probabilities of arrest or conviction than natives, or through the costs of deportation. Expenditures can be most affected by reforms of penalties and deportation procedures. This requires either bureaucratic reforms or changes in criminal justice policy, rather than immigration policy. Proposals currently under debate in Congress, for example, to streamline deportation procedures, may reduce criminal justice costs without affecting crime rates.

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## APPENDIX

**Table A.1.** Two stage least squares estimate of change in fraction immigrant on change in log metropolitan area (MA) overall crime rates, 1980 to 1990.

	Dependent variable: Change in total fraction immigrant, 1979 to 1989	Dependent variable: Change in log MA overall crime rate, 1980 to 1990	Dependent variable: Change in log MA overall crime rate, 1980 to 1990
	(1) OLS-first stage	(2) OLS	(3) IV estimate
Total fraction immigrant 1979	0.336** (0.089)	—	—
Change in total fraction immigrant, 1979 to 1989	—	-0.186 (0.589)	-0.123 (1.070)
R-square	0.3034	0.0030	0.0027
Observations	35	35	35

*Source:* Calculations from Current Population Surveys (CPSs) and Uniform Crime Reports (UCR). Calculations use data from the years 1979, 1980, 1989, and 1990.

*Notes:* Standard errors are in parentheses. Constants included but not reported in all regressions. See Table 1 for a list of the metropolitan areas included in the analysis.

\*\*Statistically significant at the 5 percent level.

**Table A.2.** Estimated coefficients for lags of fraction recent immigrant on changes in log metropolitan areas (MA) overall crime rates, 1981 to 1990.

	(1)	(2)
Fraction recent immigrants 1990	1.100 (8.962)	7.744 (15.890)
Fraction recent immigrants 1989	-6.648 (9.151)	—
Fraction recent immigrants 1988	6.980 (10.560)	13.628 (15.325)
Fraction recent immigrants 1987	3.044 (8.623)	9.692 (15.560)
Fraction recent immigrants 1986	-4.527 (8.932)	2.121 (9.701)
Fraction recent immigrants 1984	-0.171 (12.862)	6.476 (17.132)
Fraction recent immigrants 1983	3.833 (15.269)	10.481 (21.908)
Fraction recent immigrants 1982	-1.456 (6.452)	5.192 (8.302)
Fraction recent immigrants 1981	3.336 (4.804)	9.984 (10.094)
Sum of fraction recent immigrant 1981 to 1984, 1986 to 1990	—	-6.648 (9.151)
R-square	0.0963	0.0963
Observations	32	32

*Source:* Calculations from Current Population Surveys (CPSs) and Uniform Crime Reports (UCR). Calculations use data from the years 1979, 1980, 1989, and 1990.

*Notes:* Standard errors are in parentheses. Constants included but not reported in all regressions. See Table 1 for a list of the metropolitan areas included in the analysis.

\*\*Statistically significant at the 5 percent level.

**Table A.3.** Weighted means: Subsamples of the National Longitudinal Survey of Youth (NLSY).

	Men		Women	
	Born in the United States	Born abroad	Born in the United States	Born abroad
<i>n</i>	4839	366	5148	356
<b>Demographics</b>				
Age 15	0.100 (0.004)	0.121 (0.017)	0.095 (0.004)	0.088 (0.015)
Age 16	0.142 (0.005)	0.096 (0.015)	0.128 (0.004)	0.130 (0.018)
Age 17	0.124 (0.005)	0.080 (0.014)	0.126 (0.005)	0.154 (0.019)
Age 18	0.128 (0.005)	0.139 (0.018)	0.124 (0.005)	0.115 (0.017)
Age 19	0.117 (0.005)	0.132 (0.018)	0.124 (0.005)	0.138 (0.018)
Age 20	0.120 (0.005)	0.142 (0.018)	0.132 (0.005)	0.117 (0.017)
Age 21	0.125 (0.005)	0.129 (0.018)	0.121 (0.005)	0.119 (0.017)
Age 22	0.117 (0.005)	0.123 (0.017)	0.125 (0.005)	0.104 (0.016)
Age 23	0.028 (0.002)	0.037 (0.010)	0.022 (0.002)	0.036 (0.010)
Highest grade completed	11.11 (0.027)	10.59** (0.132)	11.28 (0.026)	10.51** (0.128)
Hispanic	0.047 (0.003)	0.418** (0.026)	0.050 (0.003)	0.335** (0.025)
Nonwhite	0.161 (0.005)	0.245** (0.023)	0.169 (0.005)	0.302** (0.024)
Urban resident	0.778 (0.006)	0.911** (0.015)	0.779 (0.006)	0.926** (0.014)
Local unemployment rate	0.071 (0.0004)	0.063** (0.001)	0.070 (0.0003)	0.066** (0.001)
<b>Criminality</b>				
Reported crime in past year	0.745 (0.006)	0.583** (0.026)	0.549 (0.007)	0.389** (0.026)
Stopped	0.285 (0.006)	0.204** (0.021)	0.086 (0.004)	0.044** (0.011)
Charged	0.152 (0.005)	0.092** (0.015)	0.039 (0.003)	0.029** (0.009)
Convicted	0.089 (0.004)	0.033** (0.009)	0.019 (0.002)	0.016** (0.007)
Ever stopped/booked/ charged/convicted	0.348 (0.007)	0.243** (0.022)	0.112 (0.004)	0.065** (0.013)
<b>Parents</b>				
Mother's high grade	11.79 (0.036)	9.11** (0.247)	11.61 (0.036)	9.44** (0.233)

*(continued)*

**Table A.3 (cont'd).** Weighted means: Subsamples of the National Longitudinal Survey of Youth (NLSY).

	Men		Women	
	Born in the United States	Born abroad	Born in the United States	Born abroad
Mother's education unknown	0.054 (0.003)	0.064 (0.013)	0.044 (0.003)	0.083** (0.015)
Father's high grade	11.94 (0.048)	9.82** (0.263)	11.79 (0.047)	10.32** (0.251)
Father's education unknown	0.095 (0.004)	0.130 (0.018)	0.103 (0.004)	0.105 (0.016)
Mother works	0.620 (0.007)	0.596 (0.026)	0.633 (0.007)	0.620 (0.026)
Father works	0.817 (0.006)	0.791 (0.021)	0.803 (0.006)	0.802 (0.021)
Lives with mother	0.745 (0.006)	0.683** (0.024)	0.658 (0.007)	0.669 (0.025)
Lives with father	0.632 (0.007)	0.577 (0.026)	0.539 (0.007)	0.532 (0.026)

*Notes:* The sample includes all interviewed in 1980 who answered questions about criminal involvement over the past year (270 invalid skips and 100 refusals). Standard errors are in parentheses.

\*\*Statistically significant at the 5 percent level.