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Immigration and Crime: Assessing a Contentious Issue

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Abstract

Are immigration and crime related? This review addresses this question in order to build a deeper understanding of the immigration-crime relationship. We synthesize the recent generation (1994 to 2014) of immigration-crime research focused on macrosocial (i.e., geospatial) units using a two-pronged approach that combines the qualitative method of narrative review with the quantitative strategy of systematic meta-analysis. After briefly reviewing contradictory theoretical arguments that scholars have invoked in efforts to explain the immigration-crime relationship, we present findings from our analysis, which (a) determined the average effect of immigration on crime rates across the body of literature and (b) assessed how variations in key aspects of research design have impacted results obtained in prior studies. Findings indicate that, overall, the immigration-crime association is negative—but very weak. At the same time, there is significant variation in findings across studies. Study design features, including measurement of the dependent variable, units of analysis, temporal design, and locational context, impact the immigration-crime association in varied ways. We conclude the review with a discussion of promising new directions and remaining challenges in research on the immigration-crime nexus.



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INTRODUCTION

In the contemporary United States, immigration is a vigorously debated public policy issue. This debate is heavily framed by safety and security concerns. One side of the debate advocates for restrictive immigration policy based in part on the contention that more immigration leads to higher crime rates. The opposing side rejects that view, suggesting the roots of restrictive immigration policy lie more in xenophobia and false stereotypes. Stripped of ideological differences, resolution to this debate seems simple: systematically examine the substantial and rapidly growing body of scholarship on the relationship between immigration and crime and arrive at whatever logical conclusion the evidence supports.

Unfortunately, there are several reasons why extracting a clear takeaway message from this body of research may not be simple. First, studies lack uniformity in design. Indeed, studies vary notably in terms of their measures of the key independent variable, immigration, and measures of the dependent variable, units of analysis, temporal design, and observed samples. Second, results reported in previous research can be divergent, not only across studies but within them. Although some studies document a null, negative, or positive relationship between immigration and crime, others present evidence of all of three (Ousey & Kubrin 2009; Lyons et al. 2013; Martinez 2000; Ousey & Kubrin 2014; Ramey 2013; Shihadeh & Barranco 2010, 2013). Third, social science experts offer seemingly different assessments of the literature. Ewing et al. (2015, p. 3), for example, conclude that a century's worth of research indicates that "high rates of immigration are associated with *lower* rates of violent crime and property crime," whereas Shihadeh & Barranco (2010, p. 1,397) contend that inconsistent results from past studies do not yield a definitive conclusion, leading to their inference that "There is no *one* 'immigration-crime' link any more than there is one type of immigrant or one type of job or one type of crime." For these reasons, it is understandable why the immigration-crime association remains a contentious issue despite ample social science evidence.

In this paper, we seek to synthesize the recent generation (1994 to 2014) of research that investigates the immigration-crime relationship across macrosocial (i.e., geospatial) units. The paper is organized as follows. We begin with an overview of our approach to synthesizing the literature. Here, we describe the body of studies that is the focus of our review, and we detail our method, which draws from both qualitative (narrative review) and quantitative (meta-analysis) strategies of literature synthesis. Next, we briefly review the contradictory theoretical arguments that scholars have invoked in efforts to explain the immigration-crime relationship. We then turn to the heart of our analysis, which first determines the overall average effect of immigration on crime rates across the body of literature and, second, assesses how variations in key aspects of research design may impact the results obtained in prior studies. Foreshadowing the results, we find that, overall, the immigration-crime association is negative—but very weak. At the same time, we find significant variation in findings across studies that is associated with study design characteristics. We conclude the paper with a discussion of promising new directions, as well as remaining research challenges, in research on the immigration-crime nexus.

OUR APPROACH

The focus of this review is the recent generation of quantitative studies examining the impact of immigration on crime rates in the contemporary United States. More specifically, we review and assess studies published between 1994 and 2014 that examine the immigration-crime relationship

across aggregate units ranging from blocks and tracts to cities, counties, and metropolitan areas.¹ We focus on the aggregate literature in large part because research in this area has expanded rapidly over the past two decades, and it is critical to assess what we now know (Kubrin & Ishizawa 2012, Ousey & Kubrin 2009). Based on our search of the *Sociological Abstracts* and *Web of Science Social Sciences Citation Index* databases, a total of 51 published studies met these criteria.² These studies were located through several rounds of database searches, beginning with the keywords “immigration” and “crime” or “violence,” and subsequently including alternative keywords such as “immigrant,” “immigrant concentration,” “immig*,” “foreign born,” “percent foreign-born,” and “recent foreign-born.” In addition, because measures of immigration are sometimes utilized as measures of racial/ethnic heterogeneity in studies of social disorganization theory, we also searched for “social disorganization” and “crime” or “violence.”³

We employed a two-pronged approach to reviewing and assessing this body of literature, combining the qualitative method of narrative review with the quantitative strategy of systematic meta-analysis. Although these methods are sometimes described as competing alternatives to literature synthesis (Borenstein et al. 2009, Card 2012), they can offer complementary insights that enhance efforts to summarize research on the immigration-crime relationship. As the first step in our synthesis, we applied traditional narrative review methods by immersing ourselves in and critically reading the literature to gain insights into major findings, important nuances, points of tension, and emergent themes evident across the body of scholarship. From this analysis, we were able to identify several dimensions of study design that we believe have the potential to substantially impact findings in immigration-crime research. These include differences in the conceptualization and operationalization of independent and dependent variables; variation in the geospatial units of analysis; discrepancies in temporal design features; and variation in immigrant destination contexts. Using examples from the literature, our narrative review highlights these key differences and discusses their potential salience for efforts to understand the theoretical and empirical connections between immigration and crime.

In our second step, we complemented the narrative review by utilizing meta-analysis methods. Meta-analysis techniques offer several strengths for synthesizing quantitative studies and are routinely used to review important bodies of criminological scholarship (Mitchell et al. 2007, 2012; Pratt & Cullen 2000, 2005; Pratt et al. 2014). For example, meta-analysis provides an objective approach that is inclusive of all prior quantitative results that meet stated inclusion criteria, including findings that are statistically significant as well as those that are not. Meta-analysis also presents a systematic mechanism for pooling results across studies, weighting each set of results by their relative precision. This enables a precise calculation of a weighted average effect size, and it facilitates the estimation of the extent to which individual study results vary around that average. Finally, through moderator analysis, meta-analysis facilitates an investigation of the impact that study design characteristics may have on the results reported in research.

¹Focusing on published studies introduces concern about publication bias. Although formal statistical tests suggest this not a problem in our data (Begg & Mazumdar 1994, Card 2012, Egger et al. 1997), the focus on published work is a limitation.

²Our initial literature searches uncovered 76 studies. Ten were excluded because they examined data from countries outside of the United States, and fourteen others were excluded because the dependent variable was not a measure of crime counts or rates for geographic units. One study was dropped because effect-size data were unavailable.

³Although the narrative review and meta-analysis portion of the study focus specifically on the 51 studies identified in our search, to provide a broader context of the literature we also make occasional reference to other studies published outside of our sample time frame as well as theoretical essays and literature reviews.

The meta-analysis approach we took examined effect-size estimates—correlation coefficients, estimated correlations,⁴ and standardized regression slopes—obtained from the quantitative results reported in the 51 studies mentioned earlier. Because many of those studies reported multiple findings, our analyses draw on a total of 543 effect-size estimates and associated measures of effect-size reliability (i.e., standard errors).⁵ We also obtained quantitative measures of variation in study design features, including those dimensions that our narrative review revealed as potentially influential: measures of key concepts, variation in units of analysis, variation in immigrant destination contexts, and differences in temporal design. Dummy-coded moderator variables that we created to measure these study features (see **Table 1**) were used as predictors of study effect-size estimates in meta-analytic regression models. Our meta-analytic regression models, therefore, quantitatively explored whether and how immigration-crime effect-size estimates varied as a function of study-specific differences in the measurement of core concepts, units of analysis, temporal design, and destination contexts.⁶

In sum, our approach takes stock of extant scholarship on the immigration-crime relationship by marshaling the complementary strengths of narrative review and meta-analysis methods. Using information gleaned from these approaches, we present key findings across this body of scholarship while also providing suggestions on how subsequent research can produce a clearer and more comprehensive understanding of this relationship. Before diving into our review, analysis, and recommendations, we introduce the varied theoretical perspectives that provide the conceptual foundation for social science inquiry on the macro-level immigration-crime nexus.

THEORETICAL PERSPECTIVES ON THE IMMIGRATION-CRIME LINK

There are sound theoretical reasons to believe that immigration can impact social life in ways that either increase or decrease crime rates in geographic areas. Given space constraints, our objective is not to provide a detailed review of these theories. Rather, we briefly describe some of these perspectives to underscore the fact that differing views of immigration's impact have roots in social theory (for a more detailed review of these theories, see Ousey & Kubrin 2009).

Several sociological theories suggest that higher levels of immigration into an area may increase crime rates. One theory argues that immigration increases crime because it elevates the share of the population with a crime-prone demographic profile, such as the teenage and young adult years

⁴When correlations or standardized slopes were not available, we followed the strategy employed by Pratt and colleagues (Pratt et al. 2014) to derive an approximation of the correlation coefficient by using reported test statistics (*t* tests and *z* tests) from unstandardized regression coefficients as follows: $r = t / \sqrt{t^2 + n - 2}$ and $r = z / \sqrt{z^2 + n}$. We transformed these correlations using Fisher's *r* to *z_r* transformation, which is often used in meta-analysis because the sampling distribution of correlations (*r*) around the population correlation (*p*) is generally skewed unless sample sizes are very large. In contrast, the sampling distribution of *z_r* is symmetric around the population *z_r* (Card 2012, Hedges & Olkin 1985; but also see Schmidt & Hunter 2015). This symmetry is considered advantageous when combining and comparing effect sizes across studies. However, because the *z_r* statistic is less interpretable than the *r* statistic, we retransform the *z_r* estimates back to *r* for reporting and discussion purposes.

⁵The known sampling variance for effect-size estimates is computed as suggested in Pratt et al. (2014). Specifically, standard errors for Fisher's *z*-transformed effect-size estimates from multivariate regression models were computed by dividing the Fisher's *z_r* transformation of the multivariate effect-size estimate by the *t*-test or *z*-test statistic for the immigration-measure regression slope. Standard errors for the Fisher's transformation of bivariate correlations were obtained using $\sqrt{1/(n - 3)}$.

⁶Our meta-analysis utilized a three-level random-effects modeling strategy outlined by Cheung (2007, 2015). In our case, a three-level modeling strategy is useful because effect-size estimates obtained from geographic units (level 1) vary across statistical model specifications within studies (level 2), which in turn vary between studies (level 3). This strategy weights each study result by its precision and adjusts for the clustering of effect-size estimates within studies (i.e., multiple estimates produced in a single study). All models were estimated with version 7 of the MPlus software (Muthen & Muthen 2012). Technical details on particular model specifications are available by request.

Table 1 Summary statistics of all measured study design features (i.e., moderator variables)

	N	Percentage of Estimates
<i>Independent variable measurement</i>		
Multi-item immigration index	131	24
Single-item immigration measure	412	76
Total foreign-born	207	38
Recent foreign-born	203	37
Latino foreign-born	110	20
Other race/ethnic foreign-born	23	4
<i>Dependent variable measurement</i>		
Total homicide	152	28
Motive-disaggregated homicide	57	11
Total crime index	28	5
Violent crime index	147	27
Property crime index	20	4
Other violent crime	100	18
Other nonviolent crime	39	7
Crime, total population	408	75
Crime, Latino population	70	13
Crime, black/African-American population	48	9
Crime, white/Caucasian population	17	3
<i>Units of analysis</i>		
Tracts/blocks/neighborhood clusters	235	43
Cities/counties/MSAs	308	57
<i>Temporal design</i>		
Cross-sectional	437	80
Longitudinal	106	20
<i>Destination context</i>		
Not separately measured	513	94
Measured new and traditional destinations	30	6

Abbreviation: MSA, metropolitan statistical area.

of the life course. Another argument, rooted in social disorganization theory, suggests that immigration is a powerful source of change that disrupts the social control of crime in communities. Specifically, by increasing the flow of ethnically diverse people into a community, immigration contributes to high rates of both residential instability and population heterogeneity. Instability and heterogeneity, in turn, hinder the establishment of social ties and shared values, which are needed for effective informal social control of crime (Ousey & Kubrin 2009, Stowell et al. 2009).

Another group of theories argues that elevated crime rates occur because immigration increases economic deprivation and competition in local labor markets (Beck 1996, Butcher & Piehl 1998b, Reid et al. 2005, Waldinger 1997). For example, to the extent that immigration increases the share of low-skill workers in the United States, it may heighten competition for scarce jobs and raise unemployment and poverty levels for immigrants and nonimmigrants alike. These economic strains can increase intergroup conflict, produce alienation from mainstream society, and increase motivations for crime.

A final argument is that immigration is associated with a proliferation of illegal drug market activity, which may increase other forms of criminality, including violence (Ousey & Kubrin 2009). Although a great deal of the immigration–drug market association appears to be driven by stereotype (Martinez 2002), immigrants with lower levels of human capital conceivably could be pushed into illegal market opportunities, such as the drug trade, for economic reasons.

There are also compelling theoretical arguments suggesting that immigration may decrease crime rates. One argument is that because the process of immigration is arduous, immigrants are a highly selective group of individuals with relatively high levels of initiative and achievement orientation and low levels of criminal propensity (Butcher & Piehl 2005, Tonry 1997). At the same time, some immigrant groups have relatively high levels of education and professional experience (Alba & Nee 2003). Thus, immigration may work to reduce, rather than increase, the share of the population with a high criminal propensity, thereby lowering crime.

A second theory is that immigration results in a revitalization of local communities that contributes to lower crime rates (Lee & Martinez 2002). The mechanisms by which this takes place are not fully understood, but several possibilities exist. One is that immigrants bring business entrepreneurship that injects jobs and energy into local economies (Sampson 2017, Vigdor 2014). Thus, rather than increasing economic strain, immigration reduces it, thereby contributing to lower crime rates. Another part of the revitalization framework posits that immigration improves the capacity for informal social control in communities. For example, immigration may bolster the prevalence of two-parent families and strengthen norms that legitimize parental authority and adherence to rules (Ousey & Kubrin 2009). Finally, recent work suggests that immigration may help revitalize communities by reducing housing vacancy rates (Sampson 2017, Vigdor 2014). Because vacant housing is one sign of the disorder and decay process that is posited as a crime-generating mechanism (Skogan 1992, Wilson & Kelling 1982), immigration may contribute to lower crime rates through its impact on the prevalence of vacant housing. Regardless of whether the hypothesized relationship is positive or negative, the fact is that the preceding theories on the immigration–crime nexus have not been sufficiently empirically evaluated—a point that we return to below.

Until recently, scholarship that directly investigated the immigration–crime link was in relatively short supply (Lee et al. 2001), leading scholars to argue that despite a considerable amount of immigration research in other fields (e.g., sociology, economics, and public health), “criminologists know relatively little about how crime in the United States might be affected by recent waves of immigrants and their descendants” (Morenoff & Astor 2006, p. 36; see also Lee et al. 2001, Martinez 2006, Mears 2002, Rumbaut et al. 2006). Fortunately, in just the past few years, these assessments are becoming less valid. The field has witnessed a veritable explosion of studies on the immigration–crime relationship, including aggregate analyses of neighborhoods, cities, counties, and metropolitan areas. However, this growing body of research creates its own set of challenges. Indeed, a full comprehension of the immigration–crime relationship is now complicated by the fact that this large and rapidly expanding body of literature contains studies that exhibit considerable diversity both in terms of research design and empirical results. Compounding this concern, we are aware of no research that systematically identifies, describes, and examines the impact of key study design differences on empirical findings and substantive conclusions. In the next section, we address this limitation with a multi-method review and analysis of the immigration–crime literature.

IMMIGRATION AND CRIME: KEY OBSERVATIONS AND FINDINGS

What is the average immigration–crime relationship across our sample of studies? Is it positive, negative, or null? Is it strong or weak? Overall, our narrative review reveals that the most common outcome reported in prior studies is a null or nonsignificant association between immigration and

crime. Indeed, sixty-two percent of effect-size estimates reported in our sample are not statistically significant at the 0.05 level. At the same time, although statistically significant effect-size estimates are less common than null findings, it is noteworthy that the majority of the statistically significant results are negative, suggesting that greater immigration is associated with lower crime rates. In fact, our review indicates that significant negative effects are 2.5 times as common as significant positive effects. Taken alone, these descriptive results suggest a conclusion that rings familiar to many scholars—that immigration has a null or negative effect on crime rates.

The problem with such a conclusion, however, is that it is imprecise in a number of important ways. First, it describes two outcomes—no relationship versus a negative relationship—that are qualitatively different. Second, it effectively assumes that a nonsignificant (i.e., null) effect means that there is no true immigration-crime relationship when, in fact, even moderate strength relationships may appear nonsignificant in studies with low statistical power. Third, it tells us little about the actual magnitude of the association between immigration and crime. Finally, it fails to illuminate the conditions under which the direction or magnitude of the immigration-crime relationship may vary. To address these sources of imprecision, we next discuss our meta-analysis results, which provide information on the direction, magnitude, and variability of the immigration-crime association demonstrated in the literature.

Using information gleaned from the 51 studies, our meta-analysis revealed an overall average immigration-crime association of -0.031 , with a p -value of 0.032 and 95% confidence interval estimates of -0.055 and -0.003 .⁷ These results suggest a detectable nonzero negative association between immigration and crime but with a magnitude that is so weak it is practically zero—a finding generally consistent with the prevalent pattern of nonsignificant findings observed in our narrative review. To provide a comparative perspective, we compared the average immigration-crime association with average associations for other crime predictors reported in Pratt & Cullen's (2005) meta-analysis of the macro-level crime literature. The weakest effects in their analysis were for variables reflecting education, policing, and get-tough policy, with respective mean effect sizes of 0.025 , -0.054 , and -0.054 , which is consistent with what we found. Moreover, the 95% confidence interval estimates for each of those variables overlap with those we obtained for immigration. Thus, whereas we find that the association between immigration and crime is negative, it is decidedly weak in both absolute and relative terms.

Although we find that the immigration-crime association is quite small, the evidence also reveals significant variation in that association, consistent with the descriptive observations noted earlier. More importantly, our meta-analysis reveals that effect-size estimates vary systematically between statistical models within studies (variance component = 0.013 , $p = 0.006$) as well as between studies (variance component = 0.008 , $p < 0.001$). Thus, there are strong reasons to pursue moderator analyses that examine how systematic variations in effect-size estimates may be related to differences in study design features. In the section below, we discuss four study design features that we identified as potentially salient in the course of our narrative review of the literature. Descriptive statistics for these design features, as well as other study characteristics that are controlled for in our analysis, are presented in **Table 1**. Findings from our meta-analysis,

⁷These findings are obtained from an analysis that combines the effect-size estimates from bivariate and multivariate models. If we divide the effect-size estimates into separate bivariate ($N = 57$ effect-size estimates) and multivariate ($N = 486$ effect-size estimates) subsamples, the mean effect size in the bivariate subsample is 0.049 (not significant, p -value = 0.472), whereas the mean effect size in the multivariate subsample is -0.035 (significant, p -value = 0.006). Our baseline meta-analysis model, estimated from the full sample of 543 effect-size estimates, controls for differences in sample size and the number of independent variables. Given that multivariate results are generally preferable to bivariate results, it is noteworthy that results from the multivariate subsample show a significant negative immigration-crime relationship as opposed to a null relationship for the bivariate subsample.

Table 2 Summary of mean effect sizes and impact of key study design features^a

Study design feature	Mean effect (<i>r</i>)	P-value	β (difference)	P-value
<i>Independent variable measurement</i>				
Total foreign-born	−0.013	0.603	–	–
Recent foreign-born	−0.015	0.365	−0.002	0.901
Immigration index	−0.065	0.070	−0.052	0.190
Latino foreign-born	−0.024	0.493	−0.011	0.876
Other race/ethnic foreign-born	−0.025	0.644	−0.012	0.888
<i>Dependent variable measurement</i>				
Homicide	−0.058	0.011	–	–
Crime index	0.020	0.611	0.078	0.058
Violent crime index	−0.026	0.346	0.032	0.257
Property crime index	0.006	0.752	0.064	0.022
Other violent crime	−0.007	0.825	0.052	0.072
Other nonviolent crime	−0.043	0.125	0.015	0.626
Motive-disaggregated homicide	−0.024	0.462	0.034	0.357
Crime, total population	−0.032	0.062	–	–
Crime, Latino population	−0.042	0.256	0.016	0.675
Crime, black/African-American population	−0.062	0.179	−0.004	0.939
Crime, white/Caucasian population	−0.011	0.832	0.047	0.323
<i>Units of analysis</i>				
Small geographic units	−0.073	0.000	–	–
Large geographic units	0.004	0.907	0.077	0.020
<i>Temporal design</i>				
Cross-sectional	0.000	0.989	–	–
Longitudinal	−0.147	0.000	−0.147	0.001
<i>Destination context</i>				
Not a context-specific estimate (reference)	−0.029	0.049	–	–
Traditional/established immigrant context	−0.082	0.000	−0.053	0.000
Non-traditional immigrant context	0.028	0.234	0.057	0.004
Average effect, baseline model	−0.031	0.014	–	–
Variance of effect (within-study), baseline model	0.013	0.006	–	–
Variance of effect (between-study), baseline model	0.008	0.000	–	–
Average effect size, full model	−0.029	0.047	–	–
Variance of effect (within-study), full model	0.011	0.008		
Variance of effect (between-study), full model	0.006	0.028		

^aIn addition to effects shown, meta-analysis models included moderator variables for method of estimation, years of data, and whether models accounted for economic disadvantage, ethnic heterogeneity, age structure, selection effects, sample size, and number of independent variables.

which examines the impact these features have on estimates of the immigration-crime association, are presented in **Table 2**.

Variability in Measuring Immigration

Our narrative review of the literature revealed that one important difference is found in the measures of immigration, the key independent variable. Although most studies embrace the definition

of immigration as “the tendency of immigrants to concentrate geographically by ethnicity or country of origin within the host country” (Chiswick & Miller 2005, p. 5), researchers operationalize this concept differently. Some use a single measure of immigrant concentration, most frequently the percent foreign-born (Allen & Cancino 2012, Deller & Deller 2010, Graif & Sampson 2009, Ramey 2013), whereas others combine several measures into an immigrant concentration index (Desmond & Kubrin 2009, Kubrin & Ishizawa 2012, MacDonald et al. 2013). Concerning the latter, the most frequently combined measures include percent foreign-born, percent Latino, and percent of persons who speak English not well or not at all—measures that are often highly correlated across geographic areas (Desmond & Kubrin 2009, Kubrin & Ishizawa 2012).

The problem with these approaches is that they treat immigrants as a homogeneous population and fail to account for significant variation across types of immigrants (Kubrin et al. 2016). By narrowly emphasizing the foreign-born/native-born dichotomy, researchers discount the widespread diversity that exists across immigrant groups, diversity related to immigrants’ racial, ethnic, or cultural backgrounds, reasons for migrating, countries of origin, and other factors. For this reason, some researchers advocate for a more complex treatment of immigration, arguing that “The aggregation of subgroups into global ethnic categories confounds cultural, structural, and political differences that may affect the adaptation of the ethnic group to its new locale. The field must try to recapture the rich racial and ethnic distinctions found in . . . earlier studies” (Bursik 2006, p. 29; see also Desmond & Kubrin 2009, Kubrin et al. 2016, Ousey & Kubrin 2009).

There are some exceptions to this pattern. Several researchers emphasize the importance of measuring recent (rather than total) immigration to an area (Butcher & Piehl 1998a, Lee et al. 2001, Lee & Martinez 2002, Martinez et al. 2008, Nielsen et al. 2005, Stowell & Martinez 2007). This approach is in line with individual-level research that consistently documents that the children of immigrants who are born in the United States exhibit higher offending rates than their parents (Lopez & Miller 2011, Morenoff & Astor 2006, Rumbaut et al. 2006, Sampson et al. 2005, Taft 1933) and that assimilated immigrants have higher rates of criminal involvement compared with unassimilated immigrants (Alvarez-Rivera et al. 2014, Bersani et al. 2014, Morenoff & Astor 2006, Zhou & Bankston 2006). Variation also exists in how researchers operationalize new or recent immigrants, with some measuring the fraction of an area’s population that immigrated from abroad in the previous year (Butcher & Piehl 1998b), others measuring the percentage of foreign-born arriving in the past five years (Davies & Fagan 2012), and still others capturing the percentage of foreign-born residents arriving within the past 10 years (Cancino et al. 2009, Lee et al. 2001, Lee & Martinez 2002).

Finally, a handful of studies disaggregate immigration measures to focus on an influx of particular racial or ethnic immigrant groups. For example, Martinez (2000) captures Latino immigration by creating an index that consists of two highly correlated variables: foreign-born Latinos and a proxy for recent immigration that represents Latinos residing in a foreign country five years before the 1980 Census (see also Shihadeh & Barranco 2013). Likewise, along with a general measure that reflects the percentage of the MSA (metropolitan statistical area) population that is foreign born, Reid et al. (2005) use additional ethnic-specific measures such as the percentage of the MSA population that was born in an Asian country and the percentage that was born in a Latin American country.

As we have just described, studies have operationalized the concept of immigration somewhat differently. Although all of these measures appear to be valid proxies, there are subtle differences in them that may potentially affect study outcomes. To formally assess this possibility, we created dummy variables that tap into three dimensions of variation in the immigration measures. First, we distinguished analyses that measured immigration using a multi-item index from analyses that employed a single percent foreign-born item. Second, we distinguished between studies that

measured recent immigration (e.g., percent foreign-born who immigrated in the past 5 or 10 years) versus total immigration. Third, we coded for whether the immigration measures tapped into the immigrant status of particular racial/ethnic groups by creating dummy variables to distinguish analyses that utilized measures of Latino foreign-born or other racial/ethnic-specific immigration measures.⁸

In terms of relative frequencies of these measures, we found that roughly 24% of the effect-size estimates are from models that measure immigration with a multi-item index, whereas 76% used a single-item measure of immigration. Nearly two-fifths of analyses measured immigration as the total percent foreign-born, and a similar share (37%) employed a measure of the recent foreign-born population. Finally, about one-fourth of analyses focused on racial/ethnic-specific measures of immigration, with the percent Latino foreign-born being the most common (20%).⁹

Interestingly and perhaps surprisingly, the meta-analysis results indicate that measurement of the independent variable does not exert a discernible impact on macro-level estimates of the immigration-crime relationship. **Table 2** shows that in studies that use the total percent foreign-born, the average immigration-crime relationship is very small and negative ($r = -0.013$, $p = 0.603$). The most substantial difference from this correlation appears in studies that employed an indexed measure of immigration ($r = -0.065$, $p = 0.070$); however, the difference between those effects is not statistically significant. Likewise, effect-size estimates were not significantly different when studies employed a measure of recent immigration or an ethnic-specific immigration measure.

Variability in Measuring Crime

Our narrative review revealed that another critical difference across studies is seen in the measurement of the dependent variable.¹⁰ Although the body of immigration-crime research covers the range of violent (e.g., homicide, robbery, and assault) and property (e.g., burglary, larceny, and motor vehicle theft) crimes of interest to criminologists, rarely are all outcomes considered within a given study. Often researchers examine immigration's impact on separate summary indices of total, violent, and/or property crime rates (Butcher & Piehl 1998a, MacDonald et al. 2013, Ousey & Kubrin 2009). There are some exceptions, including Reid et al. (2005), who separately examined four crime types—homicide, robbery, burglary, and theft—in their study (see also Ramey 2013). Although Reid et al. (2005, p. 775) claim their findings are consistent across the crime types, a closer read reveals that "...controlling for demographic and economic characteristics associated with higher crime rates, immigration either does not affect crime, or exerts a negative effect...", which suggests some variation in the findings.

Some researchers have argued for the importance of distinguishing among subtypes of one particularly salient offense, homicide. They contend there are reasons to believe that immigration may be related to some types of homicide (e.g., economically motivated homicides such as robbery

⁸A very small number of effect-size estimates came from analyses that measured immigration using specific racial/ethnic groups other than Latinos (e.g., percent Asian foreign-born, percent black foreign-born). Thus, we combined these into an "other foreign-born" category.

⁹These percentages are computed at the analysis or model level because studies often employ multiple models that utilize different (i.e., alternative) measures or analytic features.

¹⁰Although the literature examines a range of violent and property crimes, it does not consider crimes related to undocumented status: "When speaking of crime, we generally refer to overall crime, categories of crime, including violent, property, or drug, or specific crimes, such as homicide, assault, or burglary. These in turn can be distinguished from illegal immigration, which consists of a range of specific offenses" (Mears 2002, p. 285).

homicides) but not others (e.g., expressive homicides such as those occurring from a family dispute) (Martinez 2000, Ousey & Kubrin 2014, Stowell & Martinez 2007)—a finding that is borne out in the literature. In their study examining whether trends in immigration are related to changes in the nature of homicide in US cities between 1980 and 2010, Ousey & Kubrin (2014) find that for some, but not all, of the homicide types, the effects of changes in immigration vary across places, with the largest negative associations appearing in cities that had relatively high initial immigration levels. As such, although they find support for the thesis that changes in immigration in recent decades are related to changes in rates of lethal violence, it appears that the relationship is contingent and varied, not general.

Although many studies examine measures of crime computed for the total population of an area, researchers have sometimes focused on measures that distinguish between crimes committed by certain racial or ethnic groups. For example, some focus on estimating the impact of immigration on black homicide rates (Lee & Martinez 2002), others focus on Latino homicide rates (Martinez 2000), and still others focus on both of these as well as other racial and ethnic groups (Feldmeyer & Steffensmeier 2009, Lee et al. 2001, Nielsen et al. 2005).

Finally, a handful of studies (e.g., Martinez 2000, Nielsen et al. 2005) disaggregate homicides by subtype and race/ethnicity, modeling race- and motive-disaggregated homicide rates. Once again the norm with respect to findings is variability; for example, in a study of 111 cities, Martinez (2000) reports that Latino immigration has no relationship with overall Latino homicide rates, a positive association with Latino felony-murder rates, and a negative association with Latino acquaintance-murder rates.

To gauge the impact of this study feature, our meta-analysis captured two dimensions of variation in the measurement of the crime rate. The first distinguishes between studies that capture the overall homicide rate and measures of other crime offenses/categories, including motive-specific homicide subtype, total crime index, violent crime index, property crime index, robbery, assault, rape/sexual assault, larceny, motor vehicle theft, and drug offenses. The second captures variation in the population whose criminal behavior is reflected in the crime measure. Specifically, we coded for whether the crime rate is computed for the total population or for specific racial/ethnic groups.

Although the studies in our review examined a range of serious offenses, the majority of the immigration-crime estimates come from analyses that predicted violent crimes. Analyses of homicide were the single largest group, encompassing 39% of estimates. Another 18% of estimates are from analyses focused on another single violent offense type, such as robbery, assault, or rape. Slightly more than one-fourth of the effect-size estimates were produced in analyses focused on explaining indexed measures of violent crime that combine two or more specific offenses (e.g., homicide and robbery). Finally, roughly 5% of estimates are from models predicting an overall crime index, whereas approximately 11% are derived from models predicting measures of property/nonviolent crime (either a property crime index or single offense categories such as burglary, larceny, motor vehicle theft, or drug offenses). In terms of the racial/ethnic characteristics of the crime measures used in our sample of studies, nearly 13% measured crime rates for the Latino population, 9% focused on crime measures for blacks/African-Americans, and 3% measured crime rates for whites. The remaining three-quarters of analyses focused on the total population and did not examine race- or ethnic-specific crime rates.

Our meta-analysis results indicate that immigration-crime effect-size estimates do vary systematically across studies in association with differences in the measurement of crime. In our reference category, studies that measured crime as the overall (i.e., not motive-disaggregated) homicide rate, the average immigration-crime association is significant and negative ($r = -0.058$, $p = 0.011$), although of small magnitude. In comparison, the mean association is closer to zero and in some

cases positive in studies that measured crime with a total crime index ($r = 0.020$, p -value of difference = 0.058), a property crime index ($r = 0.006$, p -value of difference = 0.022), or as a single violent offense, such as robbery, assault, or rape ($r = -0.007$, p -value of difference = 0.072). However, no significant differences were detected between the mean effect size in the reference group and in studies that utilized a violent crime index or single-category measures of nonviolent crime. Similarly, we found no discernible difference in the immigration effect-size estimates in studies focused on motive-disaggregated homicide types. Effect-size estimates also did not materially differ for studies that utilized ethnic- or race-specific measures of crime rather than measures based on the total population.

Variability in Study Units of Analysis

Our narrative review reveals yet another potentially salient difference across studies: the size of the units of analysis. This dimension ranges from smaller, neighborhood-sized geographies, including block groups and tracts, to much larger units, such as cities, counties, and metropolitan areas. Although investigations of the immigration-crime relationship in metropolitan areas and cities are common (Butcher & Piehl 1998a; Lyons et al. 2013; Martinez 2000; Ousey & Kubrin 2009, 2014; Reid et al. 2005; Stowell et al. 2009; Wadsworth 2010), neighborhood-level studies are more numerous (Akins et al. 2009; Chavez & Griffiths 2009; Desmond & Kubrin 2009; Feldmeyer & Steffensmeier 2009; Graif & Sampson 2009; Kubrin & Ishizawa 2012; Lee et al. 2001; Lee & Martinez 2002; MacDonald et al. 2013; Martinez et al. 2004, 2008, 2010; Nielsen et al. 2005; Nielsen & Martinez 2009; Ramey 2013; Sampson et al. 2005; Stowell & Martinez 2007, 2009; Velez 2009). We also observed that a handful of studies employ smaller aggregate units embedded within larger aggregate units, such as neighborhoods embedded within cities (Lyons et al. 2013, Ramey 2013). Overall, there is a wide range of coverage, including analyses that selectively examine historically high immigrant cities such as San Antonio, San Diego, Chicago, and Los Angeles (Kubrin & Ishizawa 2012; Lee et al. 2001; Lee & Martinez 2002; MacDonald et al. 2013; Martinez et al. 2004, 2008; Sampson et al. 1997, 2005) as well as analyses that nonselectively incorporate major US cities with widely varying immigration levels (Lyons et al. 2013; Ousey & Kubrin 2009, 2014; Wadsworth 2010).

Our meta-analysis coded for differences in the size of the geographic units of analysis utilized. We created a dummy variable to distinguish between studies that examined the immigration-crime relationship in smaller sub-place-level units (neighborhood clusters/block groups/tracts) versus larger geospatial units (cities/counties/MSAs). Overall, slightly less than half of the effect-size estimates come from smaller geographic units such as block groups, census tracts or neighborhood clusters and slightly more than half come from analyses of larger geographic units.

Results from our meta-analytic regression models reveal that the choice of unit of analysis affects estimates of the immigration-crime association. Although the average immigration-crime association in studies of smaller geographies is negative and statistically significant ($r = -0.073$, $p < 0.001$), the association in larger geographic units is closer to zero and not statistically significant ($r = 0.004$, $p = 0.907$). The difference between these effect-size estimates is significant at the 0.05 level (r -difference = 0.077, $p = 0.020$).

Variability in Temporal Design

Yet another salient difference revealed from the narrative review is found in the temporal design of studies and, in particular, whether they employ a cross-sectional or longitudinal approach. Despite a rapidly expanding cross-sectional literature, research examining the longitudinal

immigration-crime relationship across areas has been relatively scarce. Indeed, an examination of our sample of studies shows the vast majority (80%) of the effect-size estimates are produced in cross-sectional analyses. However, there are critical questions that can only be answered using a longitudinal framework. For instance, how do changes in immigration affect changes in crime rates?

Despite its importance, a relatively small number of studies have addressed this question. Using pooled time-series techniques and annual data for metropolitan areas over the 1994–2004 period, Stowell et al. (2009) assess the impact of changes in immigration on changes in violent crime rates. They find that violence tended to decrease as metropolitan areas experienced gains in their concentration of immigrants. Likewise, Ousey & Kubrin (2009) investigate the impact of change in immigration on change in serious crime for 159 US cities from 1980 to 2000. In line with Stowell et al. (2009), they find that, on average, cities that experienced increases in immigration from 1980 to 2000 experienced decreases in violent crime rates. Similar findings are reported in other longitudinal studies (Allen & Cancino 2012; Butcher & Piehl 1998a,b; Graif & Sampson 2009; Kirk & Papachristos 2011; Kreager et al. 2011; MacDonald et al. 2013; Martinez et al. 2010; Ousey & Kubrin 2014; Ruther 2014; Wadsworth 2010).

Careful review of this literature, however, reveals an interesting twist: several studies document a sharp contrast in findings between the cross-sectional and longitudinal analyses. Consider, for example, Wadsworth's (2010) evaluation of the influence of immigration on crime in US urban areas. First, Wadsworth conducts ordinary least squares (OLS) regression to assess the cross-sectional relationship between immigration and rates of homicide and robbery across cities. Second, he employs pooled cross-sectional time-series models to determine how changes in immigration influenced changes in homicide and robbery rates between 1990 and 2000. In the OLS models, Wadsworth (2010) finds that immigration is associated with higher homicide and robbery levels. However, findings from the time-series models indicate that cities with the largest increases in immigration between 1990 and 2000 experienced the largest decreases in homicide and robbery during that time period. A similar pattern of findings is documented by Butcher & Piehl (1998b) in their analysis of MSAs. They conclude: "Although MSAs 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)."

Our meta-analysis findings reveal substantial evidence of differing effect-size estimates based on whether studies utilized a cross-sectional or longitudinal design. Although the mean immigration-crime association in cross-sectional analyses is essentially zero ($r = 0.0001$, $p = 0.989$), the average association in longitudinal analyses is significantly larger and negative at -0.147 ($p\text{-value} < 0.001$) ($p\text{-value of the difference in estimates} = 0.001$). This finding is important for at least three reasons. First, longitudinal research designs are generally regarded as stronger than cross-sectional designs because they offer a greater ability to control for confounding variables. Second, because immigration is a process of social and demographic transition, longitudinal research that measures within-place change in the immigrant base is a better representation of the phenomena of interest than are cross-sectional studies that measure between-place differences in the immigrant population share. Third, the immigration-crime relationship in the longitudinal studies is by far the largest effect-size estimate that we observed in any of our meta-analysis models. Thus, our findings strongly underscore the fact that the choice between cross-sectional or longitudinal data and analysis procedures is a critical one that likely impacts findings and conclusions in this area. In light of the strengths that accompany longitudinal research, it seems reasonable to suggest that the stronger, negative, and statistically significant association that emerges from the smaller body of longitudinal studies may be due more weight than the weak and nonsignificant association that emerges in the larger body of cross-sectional studies.

Variability in Destination Context

Our narrative review revealed that an increasing trend in the literature is to consider how differences in destination contexts affect the immigration-crime relationship. Kubrin & Ishizawa (2012) illustrate this when they examine how city-level context conditions the immigration-crime relationship. They examine whether neighborhoods with high levels of immigrant concentration that are situated within broader immigrant communities (or clusters of neighborhoods with many immigrants) are especially likely to experience reduced crime rates compared with those that are more spatially isolated. Studying neighborhoods in Chicago and Los Angeles—two cities with sizable and diverse immigrant populations—they report diverging results: Immigrant neighborhoods embedded within broader immigrant communities have lower than average violent crime levels in Chicago but higher than average levels in Los Angeles.

In another study, Lyons et al. (2013) consider whether politically receptive city contexts impact the immigration-crime relationship in neighborhoods across the United States. They argue that favorable immigrant political opportunities, reflected in the extent of minority political incorporation into elected offices and in pro-immigrant legislation, are likely to strengthen any inverse relationship between immigration and crime at the neighborhood level. They find that the inverse relationship between immigration and neighborhood violent crime is, in fact, enhanced in cities with favorable immigrant political opportunities. Lyons and his colleagues speculate that this occurs because favorable political contexts bolster social organization by enhancing trust and public social control within immigrant neighborhoods.

Finally, Shihadeh & Barranco (2013) also find that context matters in ways similar to the findings just reviewed. In their study of the immigration-violence link across US counties, they document variability in findings that, they argue, is attributable to whether or not the settlement area is a traditional immigrant area or a new destination area. In particular, they discover that homicide victimization rates in Latino communities are low in traditional settlement areas, to the point where they approach that of non-Latino whites. In contrast, rates of violence in new destination counties are far higher, approaching that of blacks. Shihadeh & Barranco (2013) also find a positive association between changes in Latino immigration and homicide victimization—but only in new destination counties and not in traditional settlement areas.

Although only approximately 6% of effect-size estimates in our sample are from studies that consider different destination contexts, we explored the impact of this important study feature in the meta-analysis. We coded for whether estimates of the immigration-crime association are derived from a subsample of geographic units considered traditional immigrant destinations, which are more accepting of the foreign born, or from a subsample of newly emerging destinations (new destinations), where the climate for immigrants may be less welcoming. Because the number of analyses that consider different destination contexts is limited, some caution should be exercised in interpreting these results. Nonetheless, the results provide evidence that destination context does matter, with immigration-crime effect-size estimates varying significantly across contexts. Although the mean immigration-crime association in the reference group (analyses that do not account for destination context) was significant and negative ($r = -0.029$, $p < 0.05$), it was significantly larger and negative in the more established traditional immigrant destination contexts ($r = -0.082$, $p < 0.001$; p -value of difference from reference group < 0.001). In contrast, the association was slightly positive but not significant in studies focused on new destination contexts ($r = 0.028$, $p = 0.23$; p -value of difference from reference group $= 0.005$).¹¹

¹¹Owing to space limitations, we limit our discussion of meta-analysis results to the study characteristics identified as salient based on our narrative review. We note, however, that our meta-analytic models accounted for numerous additional study

NEW DIRECTIONS AND CHALLENGES

One need only open a newspaper or watch the evening news to realize how vigorously debated immigration is today. On one side of the debate are those who call for stricter immigration control, arguing that immigration increases crime. On the other side of the debate are those who advocate for less restrictive policies, maintaining that immigration does not necessarily cause crime to rise and may, in fact, reduce crime in communities throughout the United States. Unfortunately, consulting the academic literature is not likely to provide much clarity on this debate; findings on the immigration-crime nexus vary not only across studies but within them.

This review informs the debate, providing additional clarity. Our two-pronged analysis of the research literature reveals that, overall, the immigration-crime association is negative—but very weak. At the same time, our analysis reveals that the variation in immigration effect-size estimates across studies is greater than is expected by chance. For this reason, we examined whether variation in effect sizes might be explained by study design characteristics. Among the most salient study design features are temporal structure, measurement of the dependent variable, units of analysis, and locational context, all of which impact the immigration-crime association in unique and varying ways. These findings have several implications for future research in this area.

Theory and Theory Testing

The findings have clear theoretical implications. For starters, they call into question those theories that advance a strong positive association between immigration and crime, regardless of the mechanism suggested. Clearly, our findings do not support this body of theories. Findings are more supportive of theories that posit a negative association between immigration and crime, but because the magnitude of the average effect is so small, that support is not particularly strong. The findings also help adjudicate theoretical debates, as discussed earlier in the paper. For example, social disorganization theory, as traditionally conceptualized, theorized a positive association between immigration and neighborhood crime due to several reasons. First, it was argued that immigration to an area causes residential turnover, i.e., the frequent movement of populations in and out of a community. Residential turnover weakens social ties, as residents are unable to create dense friendship networks, which itself leads to decreases in informal social control, or the capacity of a group to regulate its members according to mutually desired goals such as the desire to live in a crime-free environment. Weak ties and decreased informal social control, in turn, lead to heightened crime rates.

Under this explanation, immigration is also theorized to be associated with crime because it creates racial and ethnic heterogeneity, which, similar to residential mobility, can undermine the strength and salience of informal social control in communities. It is argued that in areas with diverse racial and ethnic groups living in close proximity, interaction between members will be lower than in racially and ethnically homogeneous communities as a result of cultural differences between the groups, language incompatibility, and the fact that individuals prefer members of their own race or ethnicity to members of different races or ethnicities. As a result, it is posited, residents are less likely to look out for one another and do not take as great an interest in their neighbors' activities, resulting in less informal social control and, ultimately, more crime.

design features, including method of estimation, years of data, sample size, number of independent variables, and whether or not models accounted for economic disadvantage, ethnic heterogeneity, age structure, and selection effects. Results for those features are available upon request.

Recently, however, scholars have challenged these claims, arguing instead that immigration can revitalize communities and actually strengthen informal social control. Referred to as the immigration revitalization thesis, this argument is that far from being a criminogenic force, immigration contributes to the viability of urban areas, especially those that have experienced population decline (Lee et al. 2001; see also Portes & Stepick 1993). As noted earlier, Sampson (2017) maintains that many decaying inner-city areas gained population in the 1990s and became more vital in many ways as a result of immigrant diffusion and that immigration is linked to population growth and lower vacancy rates, both of which help strengthen social order. This revitalization is also due to strong familial and neighborhood institutions and enhanced job opportunities associated with ethnic enclave economies (Reid et al. 2005).

Certainly, our findings do not support early social disorganization arguments but instead provide some evidence favoring more recent reformulations of the theory. At the same time, findings from the moderator analyses may be seen as providing even more evidence in favor of this latter argument, as we found that studies of smaller geographic units located within cities tended to find more substantial evidence of a nonzero negative immigration-crime relationship than do analyses of larger units that encompass an array of metropolitan communities. Finally, additional evidence is found in the fact that longitudinal studies reported stronger negative immigration-crime associations than were found in cross-sectional studies. Because recent reformulations of social disorganization theory give causal importance to dynamic processes that unfold within neighborhoods, our findings regarding longitudinal research and smaller geographies appear to support that theoretical argument.

Findings from our review also suggest that theories may need revision to account for some of the nuance and complexity that is evident in the results of prior research. Recall that the moderator analyses revealed that many study design features are associated with immigration-crime effect-size estimates. Several of these are theoretically important. As just one example, we considered whether a given study examined different immigrant destination contexts (e.g., traditional destinations versus new destinations) and found evidence that this distinction mattered. Our results revealed that the direction of the immigration-crime relationship moved to the negative side in the traditional destination units and to the positive side in the new destination units. No doubt this is related to the characteristics of well-established immigrant destinations, e.g., that they provide favorable political contexts for immigrants, which bolster social organization by enhancing trust and public social control within immigrant neighborhoods (see Lyons et al. 2013). Unfortunately, most current theories do not sufficiently engage these considerations. Thus, this represents an important issue in need of attention in future research.

Findings from our review raise additional theoretical issues. In particular, research has not sufficiently assessed whether salient social factors mediate the immigration-crime relationship in the manner predicted by the theoretical arguments discussed earlier (Kubrin 2013, Ousey & Kubrin 2009). In other words, little to no research has empirically tested the varying theoretical explanations, leaving us essentially in the dark about the underlying nature of the immigration-crime nexus. Mears (2002, p. 284) argues, “On the whole . . . empirical tests of these different theories fail to assess adequately the role of key variables such as ‘social disorganization,’ ‘strain,’ ‘cultural values’ . . .” This is problematic because theories predict immigration impacts crime indirectly through demographic, economic, and family structures (Kubrin 2013, Ousey & Kubrin 2009).

Along these lines, recall our finding that when immigration and crime are significantly related, they are negatively related. Because research has not identified the mechanisms by which immigration leads to less crime in areas, what remains unknown is why this is the case. A crucial limitation of existing research, therefore, is the failure to empirically test intervening processes

by which immigrant concentration may influence crime (Desmond & Kubrin 2009, Kubrin & Desmond 2015). Future research must work toward testing various explanatory frameworks that posit intervening mechanisms by which immigration and crime are associated at the macro-level. The findings from our review help point researchers toward theories and mechanisms that may be most salient.

Modeling a Dynamic Relationship

Another important shortcoming in the literature is the relative absence of longitudinal research and the implications of using cross-sectional data and models, as we described earlier. The findings from our analysis serve only to reinforce this concern, as we found that effect sizes were most dramatically different between cross-sectional and longitudinal approaches. Future researchers, therefore, must pay close attention to the consequences of modeling the immigration-crime relationship as one that is static versus dynamic. However, which modeling strategy is optimal? Here, we advocate for greater attention to modeling a dynamic relationship.

Beyond answering the important (causal) question related to how immigration to an area impacts crime rates over time, there are other critical questions related to historical context and time that only longitudinal approaches can fully address. For instance, does the longitudinal relationship between immigration and crime depend on the historical context under consideration (Kubrin 2013)? How might this relationship vary in the context of historically changing immigration patterns? According to scholars (Butcher & Piehl 1998a, Martinez 2006), contemporary immigration flows are vastly different compared to the turn of the last century: “After a period of mass immigration from Europe in the late 19th and early 20th centuries, the United States experienced a relative lull in immigration from the 1920s to the 1960s. But the past few decades have ushered in a new era of large-scale immigration which has accelerated since the 1980s. This time the flows have come largely from Latin America and Asia, not from Europe. Over the past 15 years, the number of immigrants—both legal and illegal—coming to the United States has been the largest in its history in absolute terms” (Rumbaut & Ewing 2007, p. 3; see also Bean et al. 2006). Such differences underscore the need to investigate how the immigration-crime nexus may shift, if at all, over time.

Other appreciable differences are relevant in a new era of mass immigration. Unlike in previous decades, immigrants in the United States are now “heavily concentrated in metropolitan areas, are predominantly nonwhite, speak languages other than English, reflect a wide range of class, religious, and cultural backgrounds, and arrive with a mix of legal statuses” (Kubrin 2013, p. 447; see also Alba & Nee 2003, Portes & Rumbaut 2006, Rumbaut 2008). This latest wave of immigrant incorporation also has coincided with a period of deindustrialization, rising inequality, and mass incarceration, during which the returns to education have sharply increased (Lee & Martinez 2006; see also Rumbaut et al. 2006). These transformations raise important questions; for example, how have the restructuring of the US economy, the decentralization of cities, the growth of suburbs as major employment centers, mass incarceration, and other major transformations affected the longitudinal immigration-crime relationship (Kubrin 2013)?

Another question of interest requiring a dynamic approach relates to the crime decline in US cities during the past quarter-century: What role might immigration have played in America’s crime drop? The vast majority of research on the crime decline has not considered the role of immigration, focusing instead on more conventional contributors such as changing police practices, increased incarceration, shifting drug markets, gun control efforts, and economic opportunity (Kubrin 2013, Ousey & Kubrin 2014). However, ten years ago, Robert Sampson published an op-ed in the *New York Times* raising the possibility that immigration and the increasing cultural

diversity that accompanies it may be what drove, in part, the crime decline, a position he elaborates on in greater detail elsewhere (Sampson 2008).

Sampson's claims have only recently been put to the test. As discussed earlier, only a handful of scholars have considered the possibility that changes in immigration may be associated with changes in crime rates, and of these studies, just a few explicitly test whether increases in immigration were responsible for the crime drop. These studies find some support for Sampson's thesis, yet much more research is needed to reach a definitive conclusion.

Theory testing and modeling a dynamic immigration-crime relationship are just two of the many next steps necessary for future researchers. Recognizing the limitations of this review, future research should extend our efforts to synthesize knowledge in this area in a number of additional ways. First, although we focus only on studies of the immigration-crime association within the United States, concern about immigration, and its connection to crime and security, is evident in countries around the globe. Examining the nature and magnitude of the immigration-crime relationship within other nations or between countries would serve as an informative comparative lens to aid our overall understanding.

Second, given our focus is limited to the association between immigration and crime across macrosocial units, another complement to the current review would focus on the body of research that has examined, at the individual level, the connection between immigrant generational status and involvement in offending. As we noted earlier, the individual-level literature has established that second- and later-generation immigrants exhibit higher offending rates than their parents. A meta-analysis of this literature would provide a sense of the consistency and strength of this immigrant generation effect as well as provide insight into whether this finding varies systematically across studies.

Third, because the meta-analytic portion of our review relied primarily on evidence explicitly reported in published studies, it did not completely ensure that the immigration-crime relationship is assessed in statistical models with exactly comparable specifications. This limitation, common to other meta-analyses in criminology (see Pratt et al. 2014), could be addressed in future research through efforts to acquire the original data sets from authors or to obtain the statistical information necessary to facilitate meta-analyses that impose strictly comparable analytic models.

Challenges Moving Forward

In addressing these important areas of inquiry, scholars will confront several obstacles, perhaps the greatest of which involves data limitations. Although these limitations take several forms, the most problematic stem from an over-reliance on official crime data. Official crime data do not provide sufficient information to address many of the broader questions of interest to crime and immigration scholars.

For example, these data do not capture an individual's immigrant status (e.g., native born versus foreign born), which makes it all but impossible to ascertain the true distribution of criminals (and victims) who are immigrants, at least on a national scale. Some countries use nationality identifiers in their crime and justice system data, but many—including the United States—do not. As Tonry (1997, pp. 9–10) notes, this was not always the case: "Through the 1930s, U.S. and Canadian data often recorded nationality, but as 'crime and the foreign born' declined as a controversial political issue after large-scale immigration stopped in the mid-1920s, use of nationality identifiers stopped." Official data collected by jurisdictions today thus virtually ignore differences in immigrant status (Reid et al. 2005), and, as a result, researchers are unable to ascertain from the data whether immigrants are perpetrators or victims of crime (Lee et al. 2001). Beyond this, this limitation renders impossible the task of identifying which groups contribute to reductions (or

gains) in crime rates (Kubrin 2013). In line with our earlier discussion, this makes theory testing on the immigration-crime nexus challenging, as some arguments propose changes in immigrants' behavior, some propose changes in natives' behavior, and some propose changes in the behavior of both groups.

Also problematic, official crime data do not distinguish between documented and undocumented immigrants, obscuring an important distinction (Kubrin 2013). This limitation exists not only in official crime data but in all national data sets that researchers employ. Undocumented residents do, in fact, respond to government surveys such as the decennial census, but these surveys do not ask the foreign born about their (un)documented status. As a result, "... we lack reliable and accurate data about inflows and outflows of immigrants as a whole, much less along 'legal' and 'illegal' dimensions" (Mears 2002, p. 285).

Exacerbating these data limitations is the issue of underreporting, which is a serious concern for those who study immigration and crime (Kubrin 2013, Trager & Kubrin 2014). Domestic violence, sexual assault, and gang violence constitute the bulk of crimes that go underreported among immigrants (Davis & Erez 1998). Reasons for underreporting include fear of becoming involved with the authorities, possible embarrassment to families, language difficulties, cultural differences in conceptions of justice, and lack of knowledge of the criminal justice system (Davis & Erez 1998). To truly advance research on the immigration-crime nexus, critical data limitations must be overcome, including incorporating information about nationality in official data collection efforts, further distinguishing between documented and undocumented status in the data, and addressing the problem of underreporting, especially with respect to immigrant victims.

These limitations and future directions aside, this review provides some much-needed clarity on what has become one of the most commonly debated questions today: Are immigration and crime related?

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