ARRESTING IMMIGRANTS: UNEMPLOYMENT AND IMMIGRATION ENFORCEMENT IN THE DECADE FOLLOWING THE SEPTEMBER 11 TERRORIST ATTACKS

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Recent research on the arrest of immigrants in the United States is largely descriptive and focused on a few localities. This study provides an examination of immigrant arrest involving two different agencies of the Department of Homeland Security (DHS): The Border Patrol (BP) and Immigration and Customs Enforcement (ICE). Descriptive time series analyses track yearly changes in immigrant arrest in the decade following the September 11 terrorist attacks (2002-2012). For many DHS jurisdictions, changes in the rates of immigrant arrest closely mirrored changes in the rates of unemployment. Fixed effects models pooling yearly data for the ICE jurisdictions demonstrate that the associations between changes in unemployment rates and changes in immigrant arrest rates were positive and significant. The strength of the associations varied considerably across jurisdictions, with magnitudes that ranged from weak to strong. The results suggest that decisions to arrest immigrants in the interior region of the country during this period were discretionary.
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The U.S. government contributes more to the budgets of the agencies responsible for immigration enforcement (i.e., Customs and Border Protection and Immigration and Customs Enforcement) than to those of all other law enforcement agencies combined (e.g., the Federal Bureau of Investigation and Drug Enforcement Administration). Funding for immigration enforcement in the decade following the September 11 terrorist attacks increased considerably as a consequence of policies and programs that the Department of Homeland Security (DHS) implemented to deter illegal border crossings and identify those who were illegally residing in the U.S. (Meissner et al. 2013). In 2004, the DHS bolstered its efforts to deter border crossings by implementing a strategy that granted the Border Patrol (BP) the authority to automatically remove unauthorized (or “undocumented”) individuals apprehended within 100 miles of the U.S. border (Rosenblum 2012). Around this same time, the DHS expressed its commitment to detain and deport all “removable aliens” (i.e., unauthorized immigrants and immigrants with lawful residency who had committed a felony) then residing in the U.S. by 2012 through its strategic plan titled “Endgame.” This plan directed DHS personnel to facilitate this process by building partnerships with state and local law enforcement agencies (DHS 2003).

Officially, Immigration and Customs Enforcement (ICE) and the Border Patrol (the lead agency within Customs and Border Protection) are both charged to enforce U.S. immigration law. While the Border Patrol has the mission of preventing and detecting illegal entry, ICE is responsible for finding and removing illegal aliens within the interior part of the country. Figure 1 shows the number of arrests reported by the DHS for the Border Patrol and ICE in each fiscal
year from 2002 to 2012. This figure reveals that change in immigrant arrest during this time frame differed for these two agencies. The number of arrests (labeled “apprehensions” by the DHS) made by the Border Patrol increased in the immediate years following the September 11 attacks but declined precipitously between 2005 and 2012. Temporal variation in the number of arrests made by ICE (labeled “administrative arrests”) is harder to detect because the y-axis requires a large scale to encompass the BP values in earlier years. It appears that change in the number of ICE arrests was fairly constant, with the exception of a large jump in the 2008 fiscal year (October 1st 2007 to September 31st 2008). Reflecting a shift by the DHS towards interior enforcement (Coleman 2012a; Provine et al. 2016), ICE and BP made roughly equal numbers of arrests for the last two fiscal years displayed (2011 and 2012). One report enumerating the number of ICE arrests by agency between 2004 and 2011 shows that LEAs initiated the majority of arrests made in every year after 2004 (Rosenblum and Kandel 2011).

Decreases in the number of BP arrests between 2005 and 2012 were a reflection of decreases in border crossings (Argueta 2016). Decreases in border crossings in this period were found to originate from increases in border enforcement and reduced economic incentives to migrate during the Great Recession (Villarreal 2014). The abrupt increase in ICE arrests in 2008 coincided with the Great Recession which officially began in December of 2007 and ended in June of 2009. Golash-Boza and Hondagneu-Sotelo (2012) argue that increases in interior arrest rates during the Great Recession were a consequence of the reduction in demand for unskilled male labor, emphasizing that recent changes in labor force demand and immigration enforcement are indicative of a broader historical shift. Stageman (2013) proposes that increases in interior arrest during the economic downturn were a response to declines in tax revenue, but also
implicates two longer-term changes: the growth in the criminal justice infrastructure (e.g., the capacity of county jails) and the decline in crime rates. As he states, the arrest and detainment of immigrants enabled state and local governments “to ameliorate the many potential effects of economic contraction on local criminal justice infrastructure” (Stageman 2013, p. 232). To my knowledge, studies have yet to examine with panel data in any period whether yearly changes in immigrant arrest in the interior region of the country were associated with yearly changes in unemployment. This reflects the lack of systematic information on immigrant arrest; the DHS does not publicly release yearly counts of arrests at the county, city, or state level. It does release arrest counts for its jurisdictions, most of which represent regions of the country.

As Rosenfeld (2014) notes, social science research has long focused on associations between levels of immigration and levels of crime, both spatially and temporally (e.g., Sampson 2008). Studies are just beginning to evaluate the effects of immigration policies and programs on immigrant outcomes (e.g., Dinsmore 2016; Parrado 2012; Rugh and Hall 2016). This study fills a major gap in the fields of immigration and criminology by examining how unemployment and other factors were associated with immigrant arrest in the decade following the September 11 attacks. Toward this end, this study reviews theory and research on immigration enforcement, paying close attention to the involvement of state and local law enforcement agencies (LEAs) in immigrant arrest. It draws insights from a political economy framework to develop hypotheses on associations between unemployment and immigrant arrest. Analyses testing the hypotheses utilize a database produced by the author that pools yearly data corresponding to BP and ICE jurisdictions from several different sources. Drawing from this rich database, this study highlights how changes in rates of immigrant arrest and unemployment were associated using descriptive time series analyses and fixed effects models that span a decade (2002-2012).
THE POLITICAL ECONOMY OF IMMIGRATION ENFORCEMENT

Studies on immigration enforcement typically utilize a political economy framework that highlights the processes through which competing groups interact within various arenas to influence migration flows (Massey 2009). Industries have an interest in implementing more expansive legislation if they assume that immigration increases the stock of unskilled workers and drives down wages. In contrast, unskilled native-born workers desire more restrictive legislation and strict enforcement of it to the extent they feel that immigrants take jobs away from native workers and lower their wages (Bonacich 1972; Fussell 2014). They are more likely to view immigrants as the source of economic hardship during times of economic distress (Quillian 1995). Both industries and unskilled native-born workers pressure politicians for change in enforcement policies and practices (Cornelius and Rosenblum 2005).2 The findings of studies on immigration enforcement utilizing times series and panel data suggest that the interests of unskilled native-born workers typically prevail. As evidence of this, immigration enforcement becomes more punitive when unemployment increases and less punitive when it decreases (Makowsky and Stratmann 2014; Massey 2009).

Stageman (2013) identifies an additional “extractive” mechanism by which unemployment influences immigration enforcement. Focusing on the period of the Great Recession, he argues that state and localities arrested and detained immigrants in order to maintain a bloated criminal justice infrastructure in a time when crime rates were falling and tax revenue was declining. After all, state and local governments received considerable funding from the federal government to house immigrants while they were awaiting deportation. In 2009, roughly 380,000 individuals were detained and the average length of their stay was one month. The average per diem paid by ICE to detain an immigrant was $122 per day, over twice that paid
for housing traditional prisoners. A “back-of-the-envelope” calculation suggests that states and localities received at least one billion dollars (i.e., $1,390,800,000 = $122*30*380,000) from the federal government in this year alone for housing detained immigrants. As early as June of 2008, the immigration crackdown was said by Barry (2008) to be “a hot new business opportunity for county commissions and sheriff departments.”

Few studies have examined change over time in the arrest, incarceration, or deportation of immigrants residing in this country. Hanson and Spilimbergo (1999) conducted one of the earliest analyses of Border Patrol apprehension, focusing on arrests made along the U.S.-Mexico border. Based on monthly data from 1968 to 1996, they found that the number of arrests increased as wages in the U.S. increased and as wages in Mexico decreased. The fact that BP arrests decreased when the U.S. economy was weaker contradicts the prevailing formulation of the political economy framework but fits a migration framework. According to the migration framework, immigrant arrest changes in response to migration flows from Mexico; Mexican migration itself is a reflection of economic conditions in both Mexico and the U.S. (i.e., economic “pushes” and “pulls”). Supporting the notion that border crossings and arrests were strongly linked, earlier studies demonstrated that Border Patrol arrests tracked changes in unauthorized migration flows reasonably well (Donato and Armenta 2011; Espenshade 1995).

King, Massoglia, and Uggen (2012) subsequently examined changes in yearly rates of criminal deportation for the U.S. between 1908 and 2005 and found evidence that the effect of unemployment rates on immigration enforcement depended on the discretion of immigration authorities. Dating back to passage of IRCA in 1986, the Immigration and Naturalization Service (the predecessor of DHS) began mandating the removal of immigrants convicted of an aggravated felony and establishing programs in correctional facilities to identify removable
immigrants (Rosenblum and Kandel 2011). The 1996 Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) subsequently expanded the number of crimes that warranted automatic removal for noncitizens to include minor offenses such as shoplifting (Coleman 2012b). Ultimately, these policies and programs reduced the discretion of immigration authorities (e.g., judges) in cases of criminal deportation (i.e., cases that involve “criminal aliens”). King and colleagues (2012) found that rates of criminal deportation were responsive to unemployment rates only in the period when deportation for criminal grounds was discretionary (i.e., 1941 to 1986).

Most recently, Olzak and Shanahan (2014) examined the incarceration of immigrants in the late 19th and early 20th century using a group threat framework that is more generally used to explain racial/ethnic prejudice and disparities in punishment (Blumer 1958; Fussell 2014). The earliest formulation of this framework suggests that dominant racial/ethnic groups increasingly perceive subordinate groups as a threat as their numerical size increases (Blalock 1967; Quillian 1995). Testing an alternative variant of this framework that emphasizes boundary drawing, Olzak and Shanahan (2014) found evidence at the city level that the incarceration of the foreign-born population for misdemeanor offenses was greater in times and places when this population had higher rates of illiteracy and pauperism. They argued that these contextual effects reflected the fact that decisions to arrest and convict for some misdemeanors (e.g., public intoxication and disorderly conduct) during this period were discretionary. Few cities during this period had guidelines for misdemeanor arrests, offering police officers considerable latitude in making these arrests (Moehling and Piehl 2009; Olzak and Shanahan 2014).

Studies analyzing change in immigration enforcement have focused on a variety of outcomes (e.g., arrest, incarceration, and deportation), but arrest is clearly the most pivotal event.
Legal scholar Motomura (2011:1822) states that, “In immigration law, the decision to arrest has been the discretion that matters.” He cites as evidence the striking correspondence in immigrant deportation and arrest rates, defining deportation loosely as either a return or removal. The near perfect correlation between immigrant arrest and deportation numbers in the decade following the September 11 attacks warrants a consideration of immigrant arrest (Appendix A). Again, LEAs initiated the majority of immigrant arrests made by ICE in this period (Rosenblum and Kandel 2011). Given the centrality of police in the arrest of immigrants, it is important to consider their legal authority to enforce immigration law and their level of discretion in doing so.

THE CONTEXT OF IMMIGRANT POLICING

Technically, unauthorized immigrants who have entered the country illegally have committed a criminal offense (i.e., a misdemeanor), whereas those who are unauthorized for other reasons (e.g., an overstayed visa) have committed a civil offense. Both of these groups are removable. In 1996, the IIRIRA expanded the number of crimes that warranted criminal removal, an expedited type of removal. All non-citizens (including permanent residents and student visa holders) are eligible for criminal removal, as are unauthorized immigrants who have entered the country illegally. Since 1983, state and local police have been able to assist federal immigration authorities in arrests for criminal violations of immigration law but not civil violations (Coleman 2012b; Motomura 2011). Specifically, they can stop immigrants and detain them for up to one hour while waiting for the arrival of a federal immigration officer (e.g., a BP agent) to verify their immigration status, given reasonable suspicion that they have violated the criminal provisions of immigration law (Rosenborough 1996).

In 1996, the IIRIRA established a program, 287(g), that gave state and local police and
highway patrol officers the authority to carry out immigration law (e.g., to arrest noncitizens for civil violations of federal immigration law) after entering into a Memorandum of Agreement (MOA) with federal immigration authorities. The 287(g) program could be implemented in two different ways: a detention model and a task force model. The detention model allowed local and state police officers to check the immigration status of anyone they arrested while the task force model permitted them to check the status of anyone they encountered (Coleman 2012b). The state of Florida signed the first 287(g) agreement in 2002. As of 2005, only three states and two counties had signed on to the program (Capps et al. 2011). The 287(g) program grew rapidly in subsequent years, with 24 and 30 agreements signed in the years 2007 and 2008, respectively (Capps et al. 2011). Virtually all of the states, counties, and cities participating in the program prior to 2009 were located in the South or Southwest region of the U.S. (Parrado 2012). Most of the agreements implemented a detention model rather than a task force model. While some agencies participating in the program targeted serious criminal offenders, others pursued a universal model of maximizing the number of unauthorized immigrants arrested (Capps et al. 2011; Government Accountability Office 2009).

Secure Communities, which began in 2008 and was discontinued in 2014, enabled local and state agencies to have ICE agents electronically screen the immigration status of any person they arrested and have the Federal Bureau of Investigation (FBI) agents search their criminal records (Cox and Miles 2013; Stumpf 2014). Secure Communities was similar to the jail model of 287(g) in that it facilitated checks on immigration status among those already arrested. Whereas 287(g) required state and local agencies to opt into the program, Secure Communities required counties to participate upon their time of activation. The activation of this program was staggered over the course of the first four years (Cox and Miles 2013; Miles and Cox 2014).
While local police already had the authority to contact a federal immigration officer regarding immigration status following a stop or arrest, Secure Communities expedited this process with information sharing. As with 287(g), agencies implemented the Secure Communities program using both universal and targeted approaches (Pedroza 2013).

Legal scholar Kobach (2005) speculated prior to the proliferation of 287(g) agreements that the involvement of local and state LEAs in the enforcement of immigration law would serve as a “quintessential force multiplier” given the large number of law enforcement officers. Subsequently, legal scholars have emphasized the fact that the interests of LEAs in enforcing immigration law are shaped by the relationship between criminal and immigration law (Motomura 2011); the intersection of these two branches of law is labeled “crimmigration” (Stumpf 2006). In prosecuting immigrants who have violated immigration law, federal prosecutors typically pursue civil removal as an outcome rather than criminal penalties, as criminal prosecutions have stricter standards. The standards required to criminally prosecute for various federal offenses are thought to temper decisions on the part of police to arrest citizens (Motomura 2011). In contrast, the ease with which courts can initiate civil removal processes means that arrests involving immigrants likely produce a tangible result. Treyger (2014) argues that programs such as 287(g) and Secure Communities produce “collateral incentives” to arrest immigrants because of their potential for promptly identifying and deporting individuals who are in violation of immigration law, regardless of whether or not they are guilty of the alleged offense that led to the stop.

Studies conducted since the beginning of the economic downturn have examined how local police work with ICE agents to enforce immigration laws within specific localities, most of which are in the Southeast (e.g., Coleman 2012b; Donato and Rodriguez 2014; Varsanyi et al.
These studies document the use of discretionary traffic stops (e.g., a stop for failing to use a turn signal) by police as a pretext for checking immigration status (Coleman 2012b; Donato and Rodriguez 2014). Notably, Donato and Rodriguez (2014) found a significant increase in arrests for minor traffic violations among foreign-born drivers following the passage of 287(g) in Nashville’s Davidson County in May 2007. Examining LEAs in two adjacent North Carolina counties (Wake and Durham) in 2008 and 2009, Coleman (2012b) and found evidence of site-specificity in the implementation of the 287(g) and Secure Communities programs. The Sheriff of Wake County likened the arrest of an immigrant for a misdemeanor offense under 287(g) to a prize in a Cracker Jack box, stating that deportation was a guaranteed outcome regardless of how minor the alleged infraction was. In sharp contrast, the Durham Police Department ordered police not to inquire about immigration status and discouraged them from collaborating with ICE agents, as they alternatively used their 287(g) authority to suppress gangs (Coleman 2012b).

The devolution of immigration enforcement from federal to local agencies ultimately produces spatial unevenness in programs and practices (Menjívar 2014; Varsanyi et al. 2012). In-depth case studies of counties and municipalities across the country suggest that the ways in which LEAs attend to immigrant status at various points in the criminal justice system (i.e., arrest to sentencing) are fairly consistent within localities (Beckett and Evans 2015; Eagly 2013). However, neighboring localities can differ starkly in their policing of immigrants. Provine and colleagues (2016) describe the spatial pattern of enforcement policies and practices as a “multijurisdictional patchwork” (Provine et al. 2016, p. 26). Notwithstanding the variation across localities, the greater collateral and economic benefits for immigrant arrests likely produce some systematic practices that cut across localities. The fact that very few counties outside of the South or Southwest signed 287(g) agreements is suggestive of regional patterns of immigration
enforcement (Parrado 2012). As elaborated below, many DHS jurisdictions encompass multiple states and thus represent regions of the country.

HYPOTHESES

The prevailing formulation of the political economy framework suggests that increases in unemployment elevate demand for stronger immigration enforcement among unskilled native-born workers. Another formulation of the political economy framework, specifically tailored to the Great Recession, suggests that declines in tax revenue increased incentives to arrest immigrants, as states and localities faced the imperative to balance their budgets. Prior studies of outcomes related to immigrant arrest (e.g., deportation) reveal that contextual influences are contingent on the amount of officer discretion. Studies that specifically focus on the policing of immigrants in the decade following the September 11 attacks suggest that LEAs had broad latitude in fashioning policies and practices with regard to immigration enforcement (Provine et al. 2016). This suggests the following hypothesis: Interior arrests will increase with the unemployment rate. This hypothesis pertains to ICE arrests, as this agency is responsible for internal enforcement.

The mission of the Border Patrol is to detect and prevent illegal entry, drug and weapon smuggling, and human trafficking (Customs and Border Protection 2012a). Following the September 11 attacks, the DHS increased investment in the Northern Border (U.S.-Canada) to prevent terrorists and weapons of mass destruction from entering the U.S. through Canada. Agents along the Northern Border are mandated to enhance border security by working with “local, tribal, and Canadian partners” (Government Accountability Office 2010). Patterns of change for this border likely fit the political economy framework assumed for ICE. Border
Patrol agents along the Southwestern (U.S.-Mexico) Border, on the other hand, likely focus attention on arresting the unauthorized as they attempt to cross the border or travel into the interior part of the country. The migration framework suggests that border arrests will decrease with the unemployment rate. The assumption of this framework is that immigrant arrest rates increase in tandem with migration flows from Mexico to the U.S. and that these flows increase when the U.S. economy strengthens.

The 287(g) and Secure Communities programs are said to have produced collateral incentives for LEAs to enforce immigration law; however, enrollment in these programs was not random. Efforts on the part of LEAs in collaborating with ICE agents (e.g., enrollment in the 287(g) program) were greater in cities and counties with more Republican voters (Lewis et al. 2012; Wong 2010) and in states with more Republican leaders (Creek and Yoder 2012). Efforts were also greater in states and counties experiencing rapid population change, particularly growth in their Hispanic population (Creek and Yoder 2012; Singer, Wilson, and DeRenzis 2009; Wong 2010). These findings are consistent with group threat frameworks that link punitive immigration enforcement to the size of subordinate groups (Quillian 1995) and voter support for conservative candidates (Chavez and Provine 2009). The rate at which counties were activated for Secure Communities was determined federally and occurred earlier for counties with a higher percentage of Hispanics (Cox and Miles 2013; Miles and Cox 2014). This suggests a third hypothesis that emphasizes the party affiliation of voters and shifts in the immigrant composition of the population as an impetus for states and localities to implement programs that expand the authority of LEAs to enforce immigration law. The expansion of LEA authority, in turn, produces a force multiplier that accentuates the effect of unemployment on immigrant arrest: The effect of the unemployment rate on interior arrest rates will be amplified in jurisdictions that
experienced more rapid growth in their immigrant population and in jurisdictions with more Republican voters.

DATA

Level of Analysis

The analyses of this study focus on yearly change in immigrant arrest in the period following the September 11 attacks up to 2012 for several different DHS jurisdictions. This period overlaps with the time frame of the strategic plan of the DHS (Operation Endgame). It also precedes the directive from ICE that prohibited the removal of non-citizens solely on the basis of minor traffic offenses (ICE Office of the Director 2012). A complication of using jurisdiction-years as the unit of analysis is that some states include multiple jurisdictions and many jurisdictions include multiple states. I combine some jurisdictions so that they correspond to state-level data. Another obstacle is that the BP jurisdictions do not map neatly on to state-level data; many of the BP jurisdictions cut across states and the BP has the authority to arrest only within 100 miles of the border. Thus, analyses of BP arrests are descriptive and focused on a couple of jurisdictions. Some of the jurisdictions cover large regions of the country, possibly producing aggregation bias. (The list of states comprising ICE jurisdictions appears in Table 2.)

Dependent Variable

Yearly immigrant arrest rates for the years 2002 to 2012 combine data from the DHS and the Pew Hispanic Center. (Web links to the sources of the dependent and independent variables appear in Appendix B.) Specifically, the numerator (the number of immigrant arrests) is based on data on arrests made publicly available by the DHS. The DHS posts counts of arrest for each fiscal year and jurisdiction (i.e., BP sectors and ICE field offices). The denominator is
based on the number of unauthorized individuals in the states comprising each jurisdiction for the different years (Pew Hispanic Center). Following Leerkes and colleagues (2014), I use linear interpolation to estimate the unauthorized population in years for which the data were not available. As discussed below, an alternative measure of immigrant arrest rates that includes estimates of the foreign-born population in each jurisdiction produces a similar pattern of results.

Dinsmore (2016) points out the importance of conducting parallel analyses of non-immigrant arrest and immigrant arrest in gauging discretion: if contextual factors have a greater influence on immigrant arrest rates than on non-immigrant arrest rates, this is evidence that immigrant arrest involves greater discretion than non-immigrant arrest. Following Dinsmore (2016), I also compute yearly arrest rates for the general population based on the FBI’s counts of arrest for states that comprise the ICE jurisdictions; these counts do not include arrests made for traffic offenses. The denominator for these arrest rates is the total population in the states of each jurisdiction. Both of these arrest rates are multiplied by 100,000 for the descriptive statistics and are natural logged in the models, as logging rates stabilizes their variance over the period.

**Independent Variables**

The estimates of the independent variables used in the descriptive analyses and models, like those of the dependent variables, entail the summation of yearly state-level data to obtain estimates for each jurisdiction. As the independent variables are lagged one year, these variables are created for years that range from 2001 to 2011. Unemployment rates are computed for each jurisdiction by dividing the number of individuals unemployed by the number of individuals in the labor force (Bureau of Labor Statistics). As the fiscal year begins in October, September estimates of labor force participation are selected. A number of other jurisdiction-level variables are included as control variables. These include variables routinely utilized in studies on
restrictive immigration policy and immigrant outcomes: population size, proportion of seats in
the House of Representatives that are held by a Republican, crime rates, and per capita police
officers (e.g., King et al. 2012). Time-varying measures of the proportion of the population
Hispanic and the proportion of the population unauthorized are not included in these models
because they could be shaped by prior changes in immigrant arrest (Parrado 2012).

Yearly estimates of total population (logged in the models) sum the number of
individuals in the states comprising each jurisdiction using data from the U.S. Census. The
proportion of Republican representatives in the House of Representatives divides for each
jurisdiction the number of Republican representatives by the total number of representatives
(Wikipedia). Yearly property crime rates in each jurisdiction are computed by dividing the
number of property crimes by the total population. Yearly violent crime rates substitute the
number of violent crimes in the numerator (Federal Bureau of Investigation). Yearly per capita
police officers divides the number of “police and sheriff’s patrol officers” in jurisdictions by their
total population (Occupational Employment Statistics). This estimate does not include police
supervisors and dispatchers. These last three measures are multiplied by 100,000 in the
descriptive statistics (following convention) and by 1,000 in the models (for presentation of the
coefficients).

This study also provides descriptive statistics on county jail and state prison rates (Bureau
of Justice Statistics). These variables are not included in the models because they could be
endogenous to immigrant arrest. County jail rates and state prison rates are computed by
summing the year-end estimates of the local jail and state inmate populations for the states in
each jurisdiction, respectively, and then dividing the jail and prison counts by the total
population of each jurisdiction. Finally, this study includes three time-constant variables that are
interacted with unemployment rates in the models to capture how group threat processes moderate the effects of unemployment on immigrant arrest. These include the percent change in each jurisdiction’s Hispanic population size between 1991 and 2001, the percent change in each jurisdiction’s unauthorized population size between 1991 and 2001, and the proportion of the jurisdiction voting Republican in the 2000 Presidential Election. Importantly, these variables capture change prior to the period during which immigrant arrest is examined.

DESCRIPTIVE RESULTS

Table 2 highlights nation-wide changes in immigrant arrest and the variables described above. Specifically, it aggregates state-level data for sixteen jurisdictions of ICE in three different years: 2002, 2007, and 2012. It also shows the percentage change in values of variables for three intervals: 2002 to 2007; 2007 to 2012; and 2002 to 2012 (e.g., percentage change between 2002 and 2007 = [(2007 estimate – 2002 estimate)/2002 estimate]*100). Roughly, the unemployment rate decreased by 20% between 2002 and 2007 and increased by 40% between 2002 and 2012. The ICE arrest rate declined by about 20% between 2002 and 2007 and it increased by about 260% between 2007 and 2012. Yearly change in unemployment and immigrant arrest is showcased in subsequent analyses. Changes in the arrest rate for the general population are opposite in direction and far less dramatic than changes in the ICE arrest rate; these arrest rates increased slightly between 2002 and 2007 (i.e., by about 5%) before declining considerably between 2007 and 2012 (i.e., by about 15%).

An important question is how arrest rates for these two (overlapping) populations differ at any single point in time. The arrest rates for immigrants, as captured using ICE arrest rates, are
lower than the arrest rates for the broader population. Dinsmore (2016) similarly finds lower arrest rates for immigrants than for citizens using Secure Communities data from 2008 to 2014. This is consistent with research suggesting that immigrants have less involvement than citizens in crime (Hagan and Palloni 1999; Rumbaut and Ewing 2007). Keep in mind the immigrant arrest rates shown in this table do not incorporate arrests made by the BP. In 2012, the DHS reported a total of 643,474 arrests for the BP and ICE (Simanski and Sapp 2013) and estimated the size of the unauthorized population to be 11,430,000 (Baker and Rytina 2013), yielding a rate of 56 arrests per 1000 unauthorized individuals (i.e., \( \frac{643,474}{11,430,000} = 0.056 \)). For this same year, the FBI reported a total of 12,196,959 arrests for a population estimated to be 312,780,968, equivalent to a rate of 39 arrests per 1000 individuals (Federal Bureau of Investigation 2012). Thus, immigrant arrest rates are lower than arrest rates in the broader population when BP arrests are excluded from immigrant arrest counts but higher when BP arrests are included in counts.8

The population of the U.S. increased by roughly 9% over the course of the decade. While the size of unauthorized population increased considerably between 2002 and 2007 (i.e., by almost two-fifths), its size increased only slightly between 2007 and 2012 (i.e., by roughly one percent). The majority of the seats in the House of Representatives was Republican in all three years and Republican representation grew by almost 10% between 2002 and 2012. Crime rates fell in both periods, but the decline was steeper in the later period than in the earlier period. The criminal infrastructure (as indicated by the average daily population of jails/prisons and the number of police) expanded between 2002 and 2007 but then contracted between 2007 and 2012. Among the indicators of this infrastructure, changes in the county jail rates were most dramatic. Roughly, jail rates increased by 12% in the earlier period but decreased by 8% in the later period. In the second period, changes in unemployment clearly overshadowed changes in criminal
infrastructure, likely producing budget shortfalls for many states and localities.

Figure 2 shows change in the number of ICE arrests and unemployment rates for the years between 2002 and 2012. Keep in mind that arrests in 2010 could have occurred in any month between October 1st 2009 and September 31st 2010. Unemployment rates for this same year are measured in September of 2010, the month that precedes each fiscal year. As unemployment rates for 2009 would be more consequential than those for 2010, subsequent analyses lag unemployment rates one year. One thing to note is that in fiscal years 2008 and 2009 there was an unusually large number of arrests that were labeled “Unknown” for jurisdiction. Massey and Riosmena (2010) point out the fact that some DHS files have missing data issues. Including these arrests produces an abrupt increase that corresponds to the time during which 287(g) programs proliferated. Removing these unknowns produces a pattern of change that is more consistent with that observed for Border Patrol arrests in Figure 1 (i.e., a mountain rather than a cliff). As suggested in this figure, the total ICE arrest counts (which include arrests of unknown origin) obscure patterns of change in immigrant arrest. Changes in the number of ICE arrests with a known jurisdiction mirror changes in the unemployment rates. Changes in these arrests also map on to changes in the percent of convicted criminal aliens removed on the basis of traffic offenses (FOIA data obtained from Eagly 2013 through e-mail communication not shown).

The above figures aggregate data for several different jurisdictions. Figure 3 illustrates how employment and immigrant arrest changed during this period for select jurisdictions. Figure A focuses on the Atlanta Field Office of ICE which includes Georgia, South Carolina, and North Carolina. Once again, changes in ICE arrest rates parallel changes in unemployment rates. This
jurisdiction experienced the largest percentage increases in its Hispanic and undocumented populations between 1991 and 2001 and it had the second highest percent of its population voting for the Republican candidate in the 2000 Presidential Election (not shown). These factors are hypothesized to boost the effects of unemployment on immigrant arrest.

Figure B corresponds to the Detroit Sector of the Border Patrol; this sector includes Illinois, Indiana, Michigan, and Ohio. The strong pattern of association between change in BP arrest rates and change in unemployment rates is also seen here. This suggests that BP arrests along the Northern Border are, for the most part, discretionary. Figure C combines the San Diego and El Centro Sectors representing two different parts of California. This figure substitutes change in the number of jobs in the construction industry (County Business Patterns data) for change in unemployment rates, as Mexican immigrants are concentrated in this industry (Villarreal 2014). For this jurisdiction of the BP, gains in construction jobs are associated with increases in arrest rates, lending support to the migration framework. Consistent with this pattern, increases in unemployment are associated with decreases in arrest rates for San Diego/El Centro (not shown). These opposite effects of unemployment in the San Diego/El Centro and Detroit Sectors underscore the importance of distinguishing the effects of unemployment on BP arrests by jurisdiction and border.

Table 2 shows immigrant arrest rates for all seventeen jurisdictions of ICE in the three focal years considered in Table 1 (2002, 2007, and 2012), in addition to the percentage change in rates for jurisdictions between the different time intervals. Within each of these focal years the jurisdictions vary widely in their rates of immigrant arrest. The rates for most jurisdictions declined between 2002 and 2007. In contrast, every jurisdiction experienced an increase in
immigrant arrest rates between 2007 and 2012, with percentage increases ranging from 20% (Baltimore) to 1488% (Phoenix). Jurisdictions with the largest increases in ICE arrest rates during the latter time interval were located in the South and Southwest (e.g., California, Phoenix, Texas, Miami, and Atlanta), with some exceptions (e.g., Seattle).

[Table 2 about here.]

RESULTS FROM FIXED EFFECTS MODELS

To formally test whether the associations between rates of unemployment and immigrant arrest are significant, I estimate a series of fixed effects models of immigrant arrest that pool yearly observations for the jurisdictions.\(^{11}\) The dependent variable in the fixed effects models is the natural logged ICE arrest rate for jurisdictions (e.g., \(\ln[\text{the number of ICE arrests / the number of unauthorized immigrants in ICE jurisdictions}]\times100,000\)). A baseline model estimates the effects of unemployment rates and other variables on the ICE arrest rate. Subsequent models examine how group threat variables moderate the effects of unemployment on immigrant arrest. Following Dinsmore (2016), I also run a model substituting as the dependent variable logged rates of arrest for the general population residing in the ICE jurisdictions.

These fixed effects models of arrest control for several factors discussed earlier. They also include indicators for jurisdiction and year (i.e., two-way fixed effects) and correct the standard errors for clustering. Specifically, these models estimate the logged immigrant arrest rate, \(Y\), for jurisdiction \(j\) in year \(t\) as a function of the jurisdiction’s lagged unemployment rate, a vector of control variables, jurisdiction fixed effects, \(\alpha_j\), year fixed effects, \(\lambda_t\), and an error term, \(\varepsilon_{jt}\):

\[
Y_{jt} = B_{\text{Unemployment Rate}}_{jt-1} + B_{\text{Control Variables}}_{jt-1} + \alpha_j + \lambda_t + \varepsilon_{jt}.
\]
Importantly, these models purge the effects of unmeasured time-invariant factors that are associated with immigrant arrest. They estimate how, within jurisdictions, change in arrest rates is associated with change in unemployment rates. As the arrest rates are natural logged, these models estimate (but only approximately) the percent change in arrest rates per one-unit change in the independent variables. The detection of significant effects in fixed effects models requires change in focal variables over time (Firebaugh, Warner, and Massoglia (2013), a condition satisfied by these data (e.g., Table 1).

I also estimated a hybrid model of ICE arrest rates that distinguished the within-jurisdiction effects from the between-jurisdiction effects (not shown). The hybrid specification demonstrated that the within- and between- jurisdiction effects were significantly different, suggesting that a random effects model does not provide consistent estimates. This means the fixed effects specification is preferred over the random effects specification. While fixed effects models do not permit the inclusion of time-constant measures as main effects, they do allow for interaction terms between time-constant and time-varying measures (Allison 2005). Thus, additional models include interaction terms for the time-varying unemployment rates and the time-constant variables expected to capture anti-immigration sentiment prior to the period when arrest rates are measured.

Table 3 shows the results of fixed effects models of arrest rates. Model 1 predicts arrest rates for the broader population and suggests that arrests for citizens fail to increase with increases in the unemployment rates. Increases in arrest rates are positively and significantly associated with increases in property crime rates but are not significantly associated with increases in violent crime rates. No variable other than the property crime rate has a significant effect on arrest rates for the broader population. Model 2 shows the effects of unemployment
rates and control variables on ICE arrest rates, the focal dependent variable. The results from Model 2 are generally consistent with the descriptive results presented earlier. Change in unemployment rates is positively and significantly associated with change in ICE arrest rates. As the unemployment rate increases by one percentage point, the ICE arrest rate increases by approximately 21.9%. Recall that in some years, the unemployment rate increased by far more than one percentage point (e.g., the roughly 3.5% increase between 2007 and 2008). It is also interesting to note some other significant coefficients in these models: ICE arrests rates increase with increases in population size and Republican representation in the House. As in the models of arrest for the general population, ICE arrest rates increase with increases in the commission of property crimes but not violent crimes.

[Table 3 about here.]

Model 3 adds an interaction term between unemployment and the percentage change in the size of the Hispanic population between 1991 and 2001. It reveals that the effect of change in unemployment rates on change in ICE arrest rates is significantly greater in jurisdictions that witnessed larger increases in their Hispanic population. Model 4 substitutes an alternative interaction term between unemployment rates and the percentage change in the size of the unauthorized population between 1991 and 2001. The effect of unemployment is also greater in jurisdictions that experienced larger increases in the unauthorized population. Finally, Model 5 substitutes for this last variable an interaction term between unemployment rates and the percent of the jurisdiction voting Republican in the 2000 Presidential Election. The effect of change in unemployment rates on change in ICE arrest rates increases as the proportion of the jurisdiction voting Republican increases. Statistical power issues preclude the inclusion of all three terms in a single model.
These patterns of moderation from Table 3 are shown in the three graphs of Figure 4. The predicted values in this figure are computed using the coefficients from Models 3, 4, and 5 of Table 3 (graphs A, B, and C, respectively); they alter the values of unemployment and moderating variables but hold all other variables constant at the mean values for the 183 jurisdiction-years. The unemployment rates range from 4.3% to 9.3%, the average rates across the seventeen jurisdictions in years 2007 and 2010, respectively. Each of the three moderating variables is set to the lower and upper quartile values for the seventeen jurisdictions. In all three graphs, ICE arrest rates increase as the unemployment rates within jurisdictions increase; however, the slope is steeper for jurisdictions that experienced a greater change in their immigrant population between 1991 and 2001 and for jurisdictions with a majority of the population voting Republican in the 2000 Presidential Election. For instance, with a 5 percentage-point increase in the unemployment rate, ICE arrest rates for jurisdictions with 40% of the population voting Republican increase by about 119% (i.e., 119% = \([\frac{1,727-790}{790}]\times 100\)). With the same percentage point increase in the unemployment rate, ICE arrest rates for jurisdictions with 55% of the population voting Republican increase by about 265% (i.e., 265% = \([\frac{4,448-1,229}{1,229}]\times 100\)). About a quarter of the jurisdictions had values on the moderating variables that were greater than those displayed in this figure for the upper quartile value, producing an even larger increase in immigrant arrest rates with the same change in the unemployment rate.

[Figure 4 about here.]
SENSITIVITY TESTS

I estimated additional fixed effects models that address the extent to which the effects of unemployment on immigrant arrest are robust. Five of the specifications, which are variations on the baseline model (Model 2 of Table 3), are displayed in Appendix C. One specification uses the foreign-born population in each jurisdiction as the denominator for immigrant arrest rates rather than the unauthorized population. Two other specifications weight jurisdictions by the size of their general population and the size of their unauthorized population. A fourth fixed effects specification lags independent variables two years and a final fixed effects specification measures independent variables contemporaneously rather than lagged. The levels of statistical significance wiggle slightly (i.e., roughly one-third of the levels shown are different from those displayed for the baseline model), but the general pattern of effects is the same. Importantly, the unemployment rate had a significant effect on immigrant arrest rates in all five of these models (i.e., \( p < .01 \) level in three of the models and \( p < .05 \) level in two of the models).

I also estimated the baseline model of immigrant arrest rates including other time-varying variables measured at the jurisdiction level (not shown here). These models included the proportion of the population Hispanic, black, unauthorized, in county jails, and in state prisons. Supplemental models also included the proportion of state prison inmates who were in private facilities and the proportion of counties activated under Secure Communities as of September for fiscal years 2008 to 2012. They also distinguished homicides from other types of violent crimes. Following Levitt (2002), I used the number of firefighters in each jurisdiction as an instrument for police levels. Variables in this universe were entered individually and as part of a set of variables. They were also specified in alternative ways (e.g., a quadratic function). The only variable that was statistically significant in any of these fixed effects models was county jail
rates. Specifically, increases in county jail rates were associated with increases in ICE arrest rates (but not arrest rates for the general population). County jail capacity would better capture jail infrastructure, but such a variable is not available for states on a yearly basis, to my knowledge.

In other analyses (not shown), I examined whether the effects of unemployment and the control variables in the baseline model differed over time by including interaction terms between these variables and a centered linear term for year (e.g., Phillips and Greenberg 2008). I failed to find any significant effects for these interaction terms. I also interacted unemployment with each of the control variables. The effect of unemployment increased as Republican representation in the House of Representatives increased; however, the interaction term was significant only at a $p < .10$ level. The Republican representation variable is limited in that it usually changes every two years rather than yearly. An interaction term between population change between 1991 and 2001 and unemployment rates was also significant, but not when combined with the term for change in the Hispanic or undocumented population in this period. An interaction term for Republican representation in the House in 2000 and unemployment rates was significant by itself but not in combination with the interaction term between Republican voting in the 2000 Presidential Election and unemployment rates (which retained significance).

To determine whether the within-jurisdiction effects of unemployment on immigrant arrest were actually driven by nation-wide processes (e.g., the emphasis on the economy by national media outlets), I also estimated models that included a time-varying measure of the unemployment rate for the nation as a whole (e.g., Schneider 2015). The effect of unemployment on immigrant arrest at the national level was weaker in magnitude than the effect at the jurisdiction level. When measures of the unemployment rate for both levels were included in the model, only the jurisdiction-level rate remained significant.
CONCLUSION

Studies on border arrests and the policing of immigrants suggest that two competing frameworks will help explain changes in immigrant arrest. The political economy framework links changes in immigrant arrest to shifting interests on the part of different groups in immigration enforcement. Its prevailing formulation predicts that immigrant arrest will rise when unemployment increases as politicians attempt to placate unskilled native-born workers. A more recent formulation of this framework highlights the interests of state and local governments in immigrant arrest as a means of funding the criminal justice system during the Great Recession. In contrast, the migration framework predicts that changes in arrest decrease when unemployment increases as a consequence of reduced flows of unauthorized migrants into the U.S. from Mexico. The salience of these two competing frameworks for immigrant arrest is expected to rest on whether federal agencies are focused on internal or external enforcement. The political economy framework is assumed to be more relevant to ICE arrests, as internal arrests involve greater discretion than border arrests. In the loosest sense, they require reasonable suspicion that someone is in the country illegally. The migration framework applies to Border Patrol enforcement along the Southwestern Border, presuming BP arrests along this border are comprised largely of cases involving individuals who have been apprehended in an attempt to cross the border illegally.

Despite speculation that the Great Recession increased immigrant arrest, studies have yet to examine whether changes in immigrant arrest rates were in fact associated with changes in unemployment rates. The findings of this study for ICE arrests lend overwhelming support to the political economy framework. Results from the descriptive time series analyses showed that changes in immigrant arrest echoed changes in unemployment rates. Such a pattern was
documented for the U.S. as a whole and for specific jurisdictions. The association between changes in unemployment and ICE arrest was especially strong for the Atlanta Field Office, which encompasses Georgia, North Carolina, and South Carolina. Both Georgia and North Carolina have been the locations of case studies on immigrant policing (Coleman 2012b; Stuesse and Coleman 2014). Findings for the Detroit Sector during this same period suggest that Border Patrol agents working along the Northern Border similarly had a “discretionary beat.” As evidence of this, changes in BP arrests closely matched changes in unemployment rates. Support for the migration model was observed for the San Diego and El Centro Sectors that covered California, located on the Southwestern (U.S.-Mexico) Border.

Fixed effects models demonstrated that, within ICE jurisdictions, yearly changes in unemployment rates were positively and significantly associated with yearly changes in immigrant arrest rates. The effects of unemployment persisted in a variety of specifications of the models. Increases in immigrant arrest were not an epiphenomenon of increases in arrest more generally, as changes in unemployment rates were not associated with changes in arrest rates for the broader population. Models testing for moderating effects demonstrated that unemployment rates had a larger influence on immigrant arrest rates in jurisdictions that experienced greater growth in their immigrant population (i.e., the unauthorized or Hispanic population) in the last decade of the 20th Century. The effect of unemployment on immigrant arrest was also greater in jurisdictions that had a higher percentage of residents voting Republican in the 2000 Presidential Election. Presumably, concerns of economic threat were more acute in these jurisdictions, prompting the implementation of programs that expanded the legal authority of LEAs in decisions about immigrant arrest. The substantial elasticity in immigrant arrest rates suggests that decisions to arrest immigrants were discretionary.
This study benefits from having yearly data that covers a decade. Fixed effects models capture change in immigrant arrest as a function of change in other factors. They have the advantage of removing the influence of unmeasured characteristics of jurisdictions that are stable. The positive effect of unemployment was not only statistically significant, but also large in magnitude. In addition, the positive and significant effect of unemployment persisted using different specifications of the dependent variable (i.e., the calculation of immigrant arrest rates for the foreign-born versus the unauthorized population); incorporating alternative lag structures (e.g., a two-year lag versus a one-year lag); and using different weighting schemes (i.e., weighting the jurisdictions equally versus weighting them by their size). Associations between unemployment and immigrant arrest were also observed in the descriptive time series analyses using different specifications of arrest (i.e., counts versus rates) and different units of analysis (i.e., the U.S. as a whole versus specific jurisdictions).

While this study fills an important gap, it has some limitations. The DHS does not distinguish the publicly-released counts of arrest for jurisdictions in different years by whether they were initiated by state and local LEAs (versus BP or ICE). Ideally, the models would use smaller jurisdictional areas as the unit of analysis (e.g., counties or municipalities). This would have enabled measurement of program participation in 287(g) and activation of Secure Communities. The use of jurisdictions comprised of multiple states potentially produces aggregation bias. It is not possible to measure the offending rates of immigrants, but it is unlikely that these rates would have changed considerably over time. Finally, the models lacked statistical power to simultaneously examine the effects of all three measures of group threat.

This study does not directly address the mechanisms by which unemployment increases immigrant arrest, but the findings are consistent with those of cross-sectional studies that utilize
a group threat framework to examine spatial variation in attitudes towards immigrants and support for the aggressive policing of immigrants. Focusing on individuals in twelve European countries in 1988, Quillian (1995) found that attitudes towards immigrants were more hostile in countries that were worse off economically and that the effect of economic conditions was greater in countries the larger their foreign-born population. Relying on data from 2004, Hawley (2011) found that the effect of immigrant population size on immigration views depended on partisanship, with native-born Republicans (but not native-born Democrats) being increasingly likely to support immigration restrictions as the percent of the population foreign-born in their county increased. Using 2010 data, Pickett (2016) found that white individuals who viewed Hispanics as an economic threat were more likely to support the aggressive policing of immigrants. Analyzing Secure Communities data from October 2008 to December 2014, Dinsmore (2016) found that the average monthly immigrant arrest rate for this entire period was greater in counties that had a higher proportion of their population voting Republican in the 2008 Presidential Election; non-immigrant arrest rates were not associated with political context.

Stageman (2013) argues that states and localities arrested and detained immigrants in order to maintain facilities and personnel during a time when tax revenues were decreasing, suggesting an extractive mechanism. As the results in Table 2 illustrate, the populations of county jails decreased substantially between 2007 and 2012. For the U.S. as a whole, the shortfall between the number of inmates in local jails and their rated capacity grew considerably between 2007 and 2011 (Minton 2012). Had the number of immigrants arrested not increased in this period, the shortfall would have been even greater. As elaborated earlier, local and state governments received generous federal funding to detain immigrants awaiting deportation. The strong likelihood that arrest eventuates in deportation (Appendix A) is said to have produced
collateral incentives for state and local LEAs to arrest immigrants (Treyger 2015). The economic downturn led several states to enact policies reducing reliance on incarceration (e.g., more lenient sentencing for minor offenses) for citizens (Brown 2012). Presumably, these policies made it relatively more “attractive” to arrest immigrants.

As Golash-Boza (2009) points out, immigration enforcement in previous decades likely reflects “a confluence of interests.” Future frameworks on immigrant arrest could be expanded to consider the interests of politicians, the media, and corporations in immigrant arrest. They could also emphasize the costs of immigrant arrest for state and local LEAs, such as the diversion of resources from public safety issues (ACLU of California 2011). Future studies of change in immigrant arrest could investigate the possible mechanisms. For instance, studies could capture sense of group threat using time-varying measures of native-born Americans’ attitudes about immigrants in these jurisdictions. Yet, at the national level, yearly attitudes about immigration do not appear to trend in the same way as yearly unemployment rates (Fussell 2014; Muste 2013). It may be more promising to exploit temporal variation in the revenue streams of state and local governments and the sentencing policies of states.

As elaborated earlier, limitations in the Customs and Border Protection data meant only cursory attention could be paid to BP arrest rates. Data from the Northern Border’s Rochester Station covering the years between 2006 and 2009 demonstrated that the Border Patrol was raiding Amtrak trains and Greyhound buses in upstate New York to identify undocumented travelers (New York Civil Liberties Union 2011). Roughly three-fourths of the individuals arrested were from Latin America. Data are now available through the Freedom of Information Act (FOIA) on the country of origin for individuals apprehended by specific sectors along the Northern Border for the fiscal years of 2001 to 2010 (Enforcement Integrated Database 2011).
These data enable a consideration of whether increases in the arrest of immigrants during the Great Recession differed according to country and region of origin. Such analyses could address the concern that the implementation of programs such as 287(g) would lead to racial and ethnic profiling (Government Accountability Office 2009).

The generalizability of these findings is restricted to the period of 2002 to 2012. The discretion of LEAs in decisions about immigrant arrest was likely diminished with the 2012 DHS directive specifying top enforcement priorities (i.e., aggravated felonies). Still, the findings of this study are relevant for understanding punishment and banishment more generally. For instance, the dramatic increases in immigrant arrests between 2007 and 2012 for some ICE jurisdictions (e.g., Phoenix) were a logical consequence of programs that blurred the line between civil and criminal violations (Motomura 2011). Along similar lines, Beckett and Herbert (2010) found that the fusion of civil and criminal law enhanced the power of municipalities (e.g., Seattle) to banish the homeless population from parks and other areas. According to a recent Supreme Court ruling (Utah v. Strieff 2016), evidence of a criminal offense found by police officer during an unlawful stop is admissible in court if they first discover an outstanding arrest warrant for a civil infraction (e.g., a warrant for an unpaid parking ticket). Justice Sotomayor dissented against this ruling, stating that it creates incentives for police to target groups most likely to have outstanding warrants.

This study contributes to a nascent wave of research that considers the effects of immigration programs and practices on the outcomes of immigrants (Rosenfeld 2014). Research from this wave demonstrates that recent immigration enforcement practices have injurious effects on immigrants and immigrant communities (Menjivar 2014; Menjivar and Abrego 2012; Stuesse and Coleman 2014). Immigrant arrest is an especially critical outcome because it
typically leads to deportation (i.e., a voluntary return or a removal). During the peak of the Great Recession, the DHS was arresting and deporting roughly one million unauthorized immigrants per year (Appendix A). Golash-Boza and Hondagneu-Sotelo (2012) highlight the fact that the vast majority of individuals removed between 1997 and 2012 were men from Latin America or the Caribbean, indicating a “gendered racial removal program.” Some unknown percentage of those deported had a spouse, partner, or child in the country. As many immigrant families are of mixed status (i.e., comprised of both authorized and unauthorized members), immigrant arrest often disrupts the lives of family members who are citizens or permanent residents (Chavez et al. 2013). Finally, immigrant arrest instills fear and insecurity among immigrants and their families that pervades daily activities such as commuting between home and work (Menjívar and Abrego 2012; Stuesse and Coleman 2014).
ENDNOTES

1. Using an instrumental variable approach, however, Massey, Durand, and Pren (2016) found evidence that border enforcement was ineffective in reducing unauthorized migration from Mexico but had other unintended effects on migration flows.

2. Makowsky and Stratmann (2014) argue that legislators built a substantial degree of discretion into the 1986 Immigration Reform and Control Act (IRCA) so that they could respond to the demands of their constituents.

3. In fiscal year 2012 (Appendix A), 671,327 immigrants were arrested (combining BP and ICE arrests) and 648,783 were deported (combining returns and removals). The correlation between the yearly number of arrests and deportations during the period of 2002 to 2012 is 0.97.

4. And when noncitizens are criminally prosecuted in federal courts, they typically receive harsher sentences than citizens, even when controlling for seriousness of offense and criminal record (Light, Massoglia, and King 2014).

5. BP agents employed along the Coastal Border are considerably more successful confiscating cocaine, given agent staffing, than their counterparts patrolling the other two borders (Customs and Border Protection 2012b). It is less obvious how unemployment would influence arrests made by agents along this border.

6. The Pew Hispanic Center does not provide estimates of the unauthorized population for seven states (e.g., Maine) that have small numbers of unauthorized individuals. These states are given a zero for their unauthorized population.

7. The FBI notes some limitations with these data, such as inconsistencies across states in what types of offenses are counted. I observed that the number of yearly arrests estimated
for the U.S. as a whole exceeds the sum of the number of yearly arrests posted for each state.

8. The rates that I have estimated are unrefined, as they do not adjust for age or type of offense. Immigrants are included in both the numerator and denominator of arrests rates for the general population and permanent residents are not captured in the denominator of the immigrant arrest rates.

9. Prior studies have noted that DHS presents statistics that differ from one report to the next (Coleman 2012b). Table 35 in the 2012 report shows the number of arrests made by ICE Enforcement and Removal Operations (ERO) in 2009 as 292,545, but in the 2013 report it shows 311,920 for its number of arrests in 2009. When the numbers of arrests for a given year differ across reports, the numbers are larger in more recent reports. Figure 2 utilizes the statistics from the most recent DHS tables, as they are presumed to be most accurate.

10. The number of arrests made by the BP in California (Figure 2) includes arrest counts from the Livermore Sector before it was closed in 2004.

11. The number of jurisdiction-years is equivalent to the number of years times the number of jurisdictions (i.e., seventeen sectors times eleven years); however, the Salt Lake Field Office did not report arrests for some of the earlier years (i.e., an unbalanced panel).

12. The measurement error produced by aggregation could have attenuated the associations between unemployment and immigrant arrest. Alternatively, the reduction in variation of variables resulting from aggregation could have strengthened these associations. Recall that King and colleagues (2012) examined associations between yearly changes in unemployment and criminal deportation at the national level, disregarding spatial
variation altogether.

13. This would entail some challenges, as surveys with questions on immigration (e.g., the General Social Survey and the American National Election Survey) utilize clustered sampling and do not ask the same questions yearly (Butz and Kehrberg 2016).
REFERENCES


Secure Communities Findings and Recommendations.”


Table 1. Key Variables for Select Years: Data for ICE Jurisdictions in the United States

<table>
<thead>
<tr>
<th>Variables</th>
<th>Fiscal Year</th>
<th>Percent Change</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Time-Varying Variables</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number Arrested by ICE (Known Field Office)</td>
<td>105,205</td>
<td>82,166</td>
</tr>
<tr>
<td>ICE Arrest Rate (Per 100,000 Unauthorized Pop.)</td>
<td>1,301</td>
<td>733</td>
</tr>
<tr>
<td>FBI's UCR of Number Arrested</td>
<td>10,397,694</td>
<td>11,429,161</td>
</tr>
<tr>
<td>Arrest Rate (Per 100,000 Population)</td>
<td>3,709</td>
<td>3,901</td>
</tr>
<tr>
<td>Total Population Unauthorized</td>
<td>8,088,400</td>
<td>11,212,800</td>
</tr>
<tr>
<td>Proportion of Population Unauthorized</td>
<td>0.029</td>
<td>0.038</td>
</tr>
<tr>
<td>Proportion of Labor Force Unemployed</td>
<td>0.058</td>
<td>0.046</td>
</tr>
<tr>
<td>Total Population (in the 1000s)</td>
<td>280,301</td>
<td>292,988</td>
</tr>
<tr>
<td>Proportion of House Republican Years: '02, '06, '12</td>
<td>0.510</td>
<td>0.523</td>
</tr>
<tr>
<td>Property Crime Rate (Per 100,000 Population)</td>
<td>3,628</td>
<td>3,277</td>
</tr>
<tr>
<td>Violent Crime Rate (Per 100,000 Population)</td>
<td>495</td>
<td>472</td>
</tr>
<tr>
<td>Police &amp; Sherriff's Patrol Officers (Per 100,000 Pop.)</td>
<td>207</td>
<td>208</td>
</tr>
<tr>
<td>County Jail Rate (Per 100,000 Population)</td>
<td>231</td>
<td>259</td>
</tr>
<tr>
<td>State Prison Rate (Per 100,000 Population)</td>
<td>446</td>
<td>466</td>
</tr>
<tr>
<td><strong>Time-Constant Variables</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% Change in Hispanic Population Between '91-'01</td>
<td>57.7</td>
<td>--</td>
</tr>
<tr>
<td>% Δ in Unauthorized Population Between '91-'01</td>
<td>100.1</td>
<td>--</td>
</tr>
<tr>
<td>% Voting for Republican in 2000 (Presidential Race)</td>
<td>47.9</td>
<td>--</td>
</tr>
</tbody>
</table>

Notes: Data from Salt Lake jurisdiction excluded because ICE estimate is missing for 2002.
Table 2. ICE Arrest Rate (Per 100,000 Unauthorized Population) for Select Years, by Jurisdiction: ICE

<table>
<thead>
<tr>
<th></th>
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<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>California (CA, HI)</td>
<td>1,127</td>
<td>350</td>
<td>2,813</td>
<td>-69.0</td>
<td>703.6</td>
<td>149.5</td>
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<td>Phoenix (AZ)</td>
<td>1,966</td>
<td>284</td>
<td>4,505</td>
<td>-85.6</td>
<td>1487.7</td>
<td>129.2</td>
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<tr>
<td>Texas (TX, NM, OK)</td>
<td>1,564</td>
<td>766</td>
<td>3,978</td>
<td>-51.0</td>
<td>419.5</td>
<td>154.3</td>
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<tr>
<td>New Orleans (AL, AR, LA, MS, TN)</td>
<td>1,865</td>
<td>1,072</td>
<td>2,719</td>
<td>-42.5</td>
<td>153.7</td>
<td>45.8</td>
</tr>
<tr>
<td>Miami (FL)</td>
<td>569</td>
<td>550</td>
<td>1,888</td>
<td>-3.3</td>
<td>243.3</td>
<td>231.9</td>
</tr>
<tr>
<td>Atlanta (GA, NC, SC)</td>
<td>425</td>
<td>756</td>
<td>3,077</td>
<td>77.8</td>
<td>306.9</td>
<td>623.5</td>
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<tr>
<td>DC</td>
<td>693</td>
<td>1,664</td>
<td>2,811</td>
<td>140.2</td>
<td>68.9</td>
<td>305.7</td>
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<td>Baltimore (DE, MD, PA, MA)</td>
<td>1,733</td>
<td>1,547</td>
<td>1,859</td>
<td>-10.7</td>
<td>20.2</td>
<td>7.3</td>
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<tr>
<td>Newark</td>
<td>417</td>
<td>551</td>
<td>1,102</td>
<td>32.3</td>
<td>99.8</td>
<td>164.3</td>
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<tr>
<td>New York</td>
<td>1,238</td>
<td>672</td>
<td>1,179</td>
<td>-45.7</td>
<td>75.5</td>
<td>-4.7</td>
</tr>
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<td>Boston (CT, ME, MA, NH, RI, VT)</td>
<td>1,246</td>
<td>1,100</td>
<td>1,456</td>
<td>-11.7</td>
<td>32.3</td>
<td>16.8</td>
</tr>
<tr>
<td>Detroit (MI, OH)</td>
<td>1,227</td>
<td>1,903</td>
<td>2,569</td>
<td>55.1</td>
<td>35.0</td>
<td>109.4</td>
</tr>
<tr>
<td>Chicago (IL, IN, KS, KY, MO, WI)</td>
<td>1,131</td>
<td>647</td>
<td>1,834</td>
<td>-42.8</td>
<td>183.3</td>
<td>62.2</td>
</tr>
<tr>
<td>St. Paul (IA, MN, NE, ND, SD)</td>
<td>4,497</td>
<td>1,843</td>
<td>2,955</td>
<td>-59.0</td>
<td>60.4</td>
<td>-34.3</td>
</tr>
<tr>
<td>Denver (CO, WY)</td>
<td>4,845</td>
<td>2,511</td>
<td>3,464</td>
<td>-48.2</td>
<td>38.0</td>
<td>-28.5</td>
</tr>
<tr>
<td>Salt Lake (ID, MT, NV, UT)</td>
<td>---</td>
<td>146</td>
<td>1,987</td>
<td>---</td>
<td>1258.2</td>
<td>---</td>
</tr>
<tr>
<td>Seattle (WA, OR, OK)</td>
<td>1,882</td>
<td>1,261</td>
<td>2,223</td>
<td>-33.0</td>
<td>76.2</td>
<td>18.1</td>
</tr>
</tbody>
</table>

Source: States within ICE field offices: https://www.ice.gov/images/ero-outreach/map.png
Table 3. Two-Way Fixed Effects Models of Logged Arrest Rates: Jurisdiction-Years Data (N = 183)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
<th>Model 5</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>Coeff. (SE)</td>
<td>Coeff. (SE)</td>
<td>Coeff. (SE)</td>
<td>Coeff. (SE)</td>
<td>Coeff. (SE)</td>
</tr>
<tr>
<td>Intercept</td>
<td>-8.2 (8.446)</td>
<td>-138.2 ** (45.087)</td>
<td>-112.6 * (43.097)</td>
<td>-124.2 * (43.224)</td>
<td>-105.2 ** (34.866)</td>
</tr>
<tr>
<td>Total Population Logged</td>
<td>0.971 (0.506)</td>
<td>8.619 ** (2.654)</td>
<td>7.093 * (2.541)</td>
<td>7.782 ** (2.540)</td>
<td>6.630 ** (2.049)</td>
</tr>
<tr>
<td>Proportion of House Republican</td>
<td>-0.163 (0.124)</td>
<td>1.342 ** (0.318)</td>
<td>1.283 ** (0.328)</td>
<td>1.330 ** (0.332)</td>
<td>1.374 ** (0.331)</td>
</tr>
<tr>
<td>Property Crime Rate (Per 1,000 Pop.)</td>
<td>0.251 *** (0.045)</td>
<td>0.367 * (0.150)</td>
<td>0.287 (0.170)</td>
<td>0.372 * (0.150)</td>
<td>0.347 * (0.138)</td>
</tr>
<tr>
<td>Violent Crime Rate (Per 1,000)</td>
<td>-0.223 (0.287)</td>
<td>-0.468 (2.599)</td>
<td>-0.551 (2.426)</td>
<td>-0.951 (2.484)</td>
<td>-0.476 (2.264)</td>
</tr>
<tr>
<td>Police Officers (Per 1,000 Pop.)</td>
<td>-0.739 (0.601)</td>
<td>2.051 (2.083)</td>
<td>1.955 (1.991)</td>
<td>2.033 (2.021)</td>
<td>1.653 (1.887)</td>
</tr>
<tr>
<td>Pr. Unemp. * % Δ Hispanic Pop. '91-'01</td>
<td>---</td>
<td>---</td>
<td>0.048 ** (2.013)</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>Pr. Unemp. * % Δ Unauth. Pop. '91-'01</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>0.030 ** (0.008)</td>
<td>---</td>
</tr>
<tr>
<td>Pr. Unemp. * % Republican Voters 2000</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>68.453 ** (21.329)</td>
<td>---</td>
</tr>
</tbody>
</table>

R-Square
- Within: XTREG
  - 0.422
  - 0.686
  - 0.701
  - 0.697
  - 0.708
- Between: XTREG
  - 0.024
  - 0.069
  - 0.075
  - 0.067
  - 0.065
- Overall: AREG
  - 0.928
  - 0.810
  - 0.819
  - 0.816
  - 0.823

Notes: Fixed effects for year and jurisdiction are not shown; the SEs are adjusted for clustering within jurisdictions. Panel is unbalanced due to missing data on ICE arrests from some Utah years.

# p < .10; * p < .05; ** p < .01; *** p < .001.
Figure 1. Number of Immigrant Arrests, by Agency and Fiscal Year

Notes: ICE arrests include those made by LEAs. Table 35 in http://preview.dhs.gov/publication/yearbook-immigration-statistics-2013-enforcement-actions.
Figure 2. Number of ICE Arrests and Unemployment Rates, by Year

Figure 3. Change in Employment & Immigrant Arrest: Select Jurisdictions

A. Change in Unemployment and ICE Arrest Rates:  
Atlanta Field Office for GA, NC, & SC (r = .833)

B. Change in Unemployment and BP Arrest Rates:  
Detroit Sector for IL, IN, MI, & OH (r = .802)

C. Change in Construction Jobs and BP Arrest Rates:  
San Diego and El Centro Sectors for CA (r = .772)
Figure 4. Predicted ICE Arrest Rates by Characteristics of Jurisdictions

A. Predicted Values of the ICE Arrest Rate by Unemployment Rate and Change in the Hispanic Population from 1991 and 2001

B. Predicted Values of the ICE Arrest Rate by Unemployment Rate and Change in the Unauthorized Population from 1991 and 2001

C. Predicted Values of the ICE Arrest Rate by Unemployment Rate and Republican Voting in the 2000 Presidential Race

Notes: Predicted values are based on the Models 3 to 5 in Table 3.
Appendix A. Number of DHS Arrests and Deportations (Returns or Removals), by Fiscal Year

<table>
<thead>
<tr>
<th>Year</th>
<th>Arrests</th>
<th>Deportations</th>
</tr>
</thead>
<tbody>
<tr>
<td>2002</td>
<td>1,062,270</td>
<td>1,177,284</td>
</tr>
<tr>
<td>2003</td>
<td>1,046,422</td>
<td>1,156,392</td>
</tr>
<tr>
<td>2004</td>
<td>1,264,232</td>
<td>1,407,241</td>
</tr>
<tr>
<td>2005</td>
<td>1,291,065</td>
<td>1,343,351</td>
</tr>
<tr>
<td>2006</td>
<td>1,206,412</td>
<td>1,324,355</td>
</tr>
<tr>
<td>2007</td>
<td>0,960,772</td>
<td>1,210,772</td>
</tr>
<tr>
<td>2008</td>
<td>1,043,799</td>
<td>1,171,058</td>
</tr>
<tr>
<td>2009</td>
<td>889,203</td>
<td>974,221</td>
</tr>
<tr>
<td>2010</td>
<td>796,587</td>
<td>856,498</td>
</tr>
<tr>
<td>2011</td>
<td>678,606</td>
<td>709,258</td>
</tr>
<tr>
<td>2012</td>
<td>671,327</td>
<td>648,783</td>
</tr>
</tbody>
</table>

## Appendix B. Sources for Key Variables (Retrieved July 11, 2016)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Source</th>
<th>Document</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number Arrested by ICE</td>
<td>DHS</td>
<td><a href="https://www.dhs.gov/immigration-statistics">https://www.dhs.gov/immigration-statistics</a></td>
</tr>
<tr>
<td>Number of Jobs in Construction</td>
<td>CBP</td>
<td><a href="http://data.bls.gov/pdq/querytool.jsp?survey=la">http://data.bls.gov/pdq/querytool.jsp?survey=la</a></td>
</tr>
<tr>
<td>Total and Hispanic Population</td>
<td>Census</td>
<td><a href="http://factfinder.census.gov">http://factfinder.census.gov</a></td>
</tr>
<tr>
<td>Violent and Property Crime Rate</td>
<td>FBI</td>
<td><a href="http://www.ucrdatatool.gov/">http://www.ucrdatatool.gov/</a></td>
</tr>
<tr>
<td>Police &amp; Sherriff's Patrol Officers</td>
<td>OES</td>
<td><a href="http://www.census.gov/programs-surveys/cbp.html">http://www.census.gov/programs-surveys/cbp.html</a></td>
</tr>
<tr>
<td>FBI's UCR of Number Arrested</td>
<td>FBI</td>
<td><a href="http://www.ojjdp.gov/ojstatbb/ezaurc/ucr_display.asp">http://www.ojjdp.gov/ojstatbb/ezaurc/ucr_display.asp</a></td>
</tr>
<tr>
<td>County Jail Population (ADP)</td>
<td>BJS</td>
<td><a href="http://www.bjs.gov/content/pub/pdf/mljsp0013st.pdf">www.bjs.gov/content/pub/pdf/mljsp0013st.pdf</a></td>
</tr>
</tbody>
</table>
Appendix C. Fixed Effects Models of Logged ICE Arrest Rates w/ Adjustments (N = 183)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coeff. (SE)</th>
<th>Coeff. (SE)</th>
<th>Coeff. (SE)</th>
<th>Coeff. (SE)</th>
<th>Coeff. (SE)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Rates w/ Foreign Born Weighting by:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Population Denominator Pop.</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>R-Square</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Within: XTREG</td>
<td>0.692</td>
<td>0.767</td>
<td>0.811</td>
<td>0.686</td>
<td>0.659</td>
</tr>
<tr>
<td>Between: XTREG</td>
<td>0.050</td>
<td>0.071</td>
<td>0.070</td>
<td>0.065</td>
<td>0.064</td>
</tr>
<tr>
<td>Overall: AREG</td>
<td>0.862</td>
<td>0.837</td>
<td>0.856</td>
<td>0.809</td>
<td>0.793</td>
</tr>
</tbody>
</table>

**Notes:** Fixed effects for year and jurisdiction are not shown; the SEs are adjusted for clustering within jurisdictions. Panel is unbalanced due to missing data on ICE arrests from some Utah years.

# p < .10; * p < .05; ** p < .01; *** p < .001.