

Churchill, Brandyn; Dickinson, Andrew; Mackay, Taylor; Sabia, Joseph J.

Working Paper

The Effect of E-Verify Laws on Crime

IZA Discussion Papers, No. 12798

Provided in Cooperation with:

IZA – Institute of Labor Economics

Suggested Citation: Churchill, Brandyn; Dickinson, Andrew; Mackay, Taylor; Sabia, Joseph J. (2019) : The Effect of E-Verify Laws on Crime, IZA Discussion Papers, No. 12798, Institute of Labor Economics (IZA), Bonn

This Version is available at:

<https://hdl.handle.net/10419/215194>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

DISCUSSION PAPER SERIES

IZA DP No. 12798

The Effect of E-Verify Laws on Crime

Brandyn Churchill
Andrew Dickinson
Taylor Mackay
Joseph J. Sabia

NOVEMBER 2019

DISCUSSION PAPER SERIES

IZA DP No. 12798

The Effect of E-Verify Laws on Crime

Brandyn Churchill

Vanderbilt University

Andrew Dickinson

University of Oregon

Taylor Mackay

University of California-Irvine

Joseph J. Sabia

San Diego State University and IZA

NOVEMBER 2019

Any opinions expressed in this paper are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but IZA takes no institutional policy positions. The IZA research network is committed to the IZA Guiding Principles of Research Integrity.

The IZA Institute of Labor Economics is an independent economic research institute that conducts research in labor economics and offers evidence-based policy advice on labor market issues. Supported by the Deutsche Post Foundation, IZA runs the world's largest network of economists, whose research aims to provide answers to the global labor market challenges of our time. Our key objective is to build bridges between academic research, policymakers and society.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Effect of E-Verify Laws on Crime*

E-Verify laws, which have been adopted by 23 states, require employers to verify whether new employees are eligible to legally work prior to employment. In the main, these laws are designed to reduce employment opportunities for unauthorized immigrants, reduce incentives for their immigration, and increase employment and earnings for low-skilled natives. This study explores the impact of state E-Verify laws on crime. Using agency-by-month data from the 2004 to 2015 National Incident Based Reporting System (NIBRS), we find that the enactment of E-Verify is associated with a 5 to 10 percent reduction in property crimes involving Hispanic arrestees, an effect driven by universal E-Verify mandates that extend to private employers. Supplemental analyses from the Current Population Survey (CPS) suggest that E-Verify-induced increases in employment of low-skilled natives of Hispanic descent, and outmigration of younger Hispanics are important channels. We find no evidence that crime was displaced to nearby U.S. jurisdictions without E-Verify or that violent crime was impacted by E-Verify mandates. Moreover, neither arrests nor labor market outcomes of white or African American adults were affected by E-Verify laws. The magnitudes of our estimates suggest that E-Verify mandates generated \$491 million in social benefits of reduced crime to the United States.

JEL Classification: K14, J61

Keywords: E-Verify, immigration, crime, employment

Corresponding author:

Joseph J. Sabia
Department of Economics
San Diego State University
5500 Campanile Drive
San Diego, CA 92182-4485
USA

E-mail: jsabia@sdsu.edu

* Churchill is grateful for support from Vanderbilt University's Kirk Dornbush summer research grant. Sabia acknowledges funding support from the Center for Health Economics & Policy Studies (CHEPS) at San Diego State University, including grants received from the Troesh Family Foundation and the Charles Koch Foundation. The authors thank Kyutaro Matsuzawa and Tam Nguyen for outstanding research assistance and Isaac Baumann for excellent editorial assistance.

1. Introduction

“The supporters of E-Verify should...consider that one possible result of mandating that system is higher incarceration and more crime rates.”

- Cato Institute (2018)

“We’re also deeply angry because this [crime] could have been prevented... This incident highlights the fact we need an even stronger E-Verify system.”

- Senator Charles Grassley (2018)

Donald J. Trump announced his candidacy for president on June 16, 2015 by claiming that Mexican immigrants to the United States were bringing “lots of problems” with them, including drugs and crime (Trump 2015).¹ Since taking office, President Trump has advocated reducing unauthorized immigration as a crime-fighting tool, a policy position that helped drive the 2019 federal government shutdown, the longest in American history (Davis and Tackett 2019).

Despite a paucity of empirical evidence that increased immigration causes more crime (Butcher and Piehl 1998a, 1998b, 2008; Hagan and Palloni 1999; Light and Miller 2018; Orrenius and Zavodny 2019), nearly half of Americans blame immigrants for perceived increases in crime (Gallup 2017) and public support for enhanced border security remains strong (Harvard University-Harris Poll 2018). Moreover, interior enforcement policies that target unauthorized immigrants working or living in the United States — 42 percent of whom entered the United States legally but remained in the country following expiration of their temporary visas (Warren 2019) — have blossomed (Michaud 2010; Miles and Cox 2014; Treyger et al. 2014).

One of the most widespread interior enforcement policies in the United States are E-Verify laws, adopted by 23 states and the federal government.² Under an E-Verify mandate,

¹ In this well-publicized statement, then-candidate Trump did not distinguish between Mexican immigrants who came to the United States legally from immigrants who had violated existing U.S. immigration laws.

² Rhode Island enacted an E-Verify law in March 2008 which was repealed in January 2011. At the writing of this paper, 22 states had E-Verify laws in place.

employers are required to digitally verify the employment eligibility status of newly hired employees, comparing information on their Employment Eligibility Verification (I-9) form with electronic records from the Social Security Administration and Department of Homeland Security (Department of Homeland Security 2018). An employee's name, Social Security number, date of birth, citizenship status, and (if applicable) additional noncitizen-related information is compared to electronic federal records to assess work eligibility (Newman et al. 2012). A mismatch prompts an alert to the employer that must be resolved by the employee within 10 federal workdays or the employee must be fired. Failure to comply with state E-Verify laws can result in substantial fines for employers as well as business license revocation. The majority of state E-Verify mandates (13 states) apply to public employers and private employers with public contracts, while the remainder (10 states) also apply to private employers (National Conference of State Legislature 2015).³

The effect of state E-Verify mandates on crime is ambiguous. First, such mandates may increase the likelihood that unauthorized immigrants are detected violating immigration law, resulting in an increase in immigration-related arrests. In addition, by diminishing unauthorized immigrants' labor market prospects (Orrenius and Zavodny 2015; Amuedo-Dorantes and Bansak 2014), E-Verify may lead to an increase in property or drug crimes for income-generating purposes (Raphael and Winter-Ebmer 2001; Öster and Agell 2007; Mustard 2010). Furthermore, if unauthorized immigrants serve as complements to higher-skilled native workers (East et al. 2018; Lee et al. 2017), crime may rise due to increased unemployment among natives. Moreover, state E-Verify laws may increase crime rates by changing the composition of the state population toward those with higher propensities for crime. This could occur due to E-Verify-induced outmigration of likely undocumented immigrants (Good 2013; Bohn, Lofstrom, and Raphael 2014; Orrenius and Zavodny 2016), who have lower propensities for crime than low-skilled natives (Butcher and Piehl 1998a, 1998b; MacDonald et al. 2013).⁴

On the other hand, if E-Verify mandates increase employment of low-skilled natives because low-skilled natives and immigrants are labor-labor substitutes (Amuedo-Dorantes and

³ The federal E-Verify mandate applies to public employees and government contractors.

⁴ Along the same lines, if E-Verify mandates induce in-migration of low-skilled natives, such a compositional shift could also increase crime rates.

Bansak 2014) — or if E-Verify laws increases native workers’ wages by reducing the supply of available substitutes (Orrenius and Zavodny 2015) — then crime committed by natives may fall.⁵ E-Verify mandates may also reduce crime if low-skilled Hispanic immigrants out-migrate in response to fewer job opportunities or a less welcoming environment for those of Hispanic descent. Finally, E-Verify may reduce reports of criminal activity if E-Verify mandates change the willingness of unauthorized immigrants to report crime (Zhang et al. 2016). Thus, the net impact of E-Verify mandates on crime depends on the magnitudes of the policy’s effects on (i) immigration-related arrests for undocumented immigrants, (ii) labor market outcomes for low-skilled immigrants (unauthorized and authorized) as well as natives, (iii) mobility of affected workers, and (iv) the distribution of these effects across low-skilled populations with heterogeneous propensities for crime.

To our knowledge, only two studies have examined the impact of E-Verify mandates on crime. Each is a case study of a particular state’s E-Verify law and the conclusions are mixed. Chalfin and Deza (2018) found that property crime declined following the adoption of an E-Verify law in Arizona, while Zhang et al. (2016) concluded that violent crime rose after Georgia’s mandate was enacted.

The current study exploits substantially more policy variation than was used in prior studies to more comprehensively examine the impact of state E-Verify mandates on crime. In contrast to existing case studies, we explore heterogeneity in crime effects by ethnicity and race. This is important because there are strong theoretical reasons to imagine that E-Verify mandates will be particularly salient for immigrants and natives of Hispanic descent. Moreover, we also examine whether the crime effects of E-Verify are larger for more expansive mandates that extend to private employers.

Using data from the National Incident Based Reporting System (NIBRS) from 2004 to 2015, and exploiting temporal variation in the adoption of E-Verify mandates across states, we find that E-Verify is associated with a 5 to 10 percent *decline* in property crime incidents involving working-age Hispanic arrestees. This finding is more pronounced among males, those under age 45, and for universal E-Verify mandates that extend to private employers.

⁵ If immigrants and low-skilled natives are labor-labor complements, E-Verify may reduce employment of natives and increase incentives for crime. However, much of the existing empirical evidence finds that E-Verify is positively related to employment (Amuedo-Dorantes and Bansak 2014) and earnings (Orrenius and Zavodny 2015) of low-skilled natives.

We then use data from the Current Population Survey (CPS) to explore potential mechanisms. Consistent with Orrenius and Zavodny (2015), we find that E-Verify mandates are associated with an increase in employment among low-skilled natives (U.S.-born or naturalized citizens) of Hispanic descent, a population more likely to engage in criminal activity than their non-citizen immigrant counterparts (Chalfin 2013; Butcher and Piehl 2008). Thus, the increased opportunity cost of crime, driven by increases in legitimate sources of income in the labor market, may be one explanation for the reduction in property crimes. Second, we find that E-Verify induces outmigration of younger Hispanic males, which may also drive reductions in criminal incidents. However, we find no evidence of E-Verify-driven reductions in violent crime, nor do we find evidence that property or violent crime is displaced to neighboring jurisdictions without E-Verify mandates. These findings suggest that labor market effects could be a relatively more important channel to explain declines in property crime.

Finally, turning to data from the Uniform Crime Reports (UCR), we find that E-Verify had no effect on arrests involving white or African American adults, consistent with prior evidence that E-Verify has little impact on their labor market outcomes. In summary, we estimate that E-Verify generates social benefits of crime reduction of approximately \$491 million (in 2018 U.S. dollars).

2. Background and Literature

2.1 History of Interior Immigration Reform

The Immigration Reform and Control Act (IRCA) of 1986 was the first major attempt by Congress to address illegal immigration. IRCA sought to counter unauthorized entry into the United States in a three-component approach, or, as the bill's sponsors referred to it, the "three-legged stool": (i) deter future illegal border crossings by increasing border security, (ii) offer legal status to resident unauthorized immigrants⁶, and (iii) sanction employers for hiring unauthorized immigrants (Chisti and Kamasaki 2014). The primary objective of the bill was to limit the involvement of unauthorized immigrants in the American job market through mandated use of the I-9 system, though it allowed for more than two dozen types of documentation under

⁶ Amnesty was granted to those who resided continuously in the U.S. since January 1982 or completed 90 days of agricultural work in the U.S. between May 1985 and May 1986 (Center for Immigration Studies 2019).

an approved list ranging from driver's licenses and passports to a school ID and voter registration card (Kerwin and McCabe 2011).⁷ Despite this system, IRCA failed its primary objective (Chisti and Kamasaki 2014).

IRCA did not require employers to verify the authenticity of workforce eligibility documentation provided by employees, only to examine documents and decide “if the document reasonably appears on its face to be genuine” (Kerwin and McCabe 2011). Employers who comply in good faith with the I-9 system are entitled to an affirmative defense to federal sanctions (Castillo and Schulman 2011). Due to the large incidence of false documentation, penalties were rarely administered to employers of unauthorized immigrants (Orrenius and Zavodny 2015; Baker 2015). Furthermore, attempts to increase enforcement of this law were met by political opposition in Congress (Hanson 2006).

A decade later, the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) of 1996 was enacted to address several interior enforcement problems left by IRCA. IIRIRA introduced the 287(g) program, which authorized Immigration and Customs Enforcement (ICE) to enter into agreements with state and local law enforcement agencies to train local officers to better enforce immigration law and perform immigration-related functions such as deportation.⁸ In addition, the Secure Communities program was launched to improve the efficiency of interior enforcement by targeting unauthorized immigrants who engaged in criminal activity using fingerprints collected by local law enforcement with the DHS Automated Biometric Identification System (Kubrin 2014). The system notifies ICE when a deportable criminal is arrested, though there is little evidence that Secure Communities had a discernable effect on crime (Miles and Cox 2014; Treyger et al. 2014).

IIRIRA also expanded electronic employment verification systems such as the Basic Pilot Program (Newman et al. 2012). Under IIRICA, the Basic Pilot Program was introduced to employers in California, Texas, Florida, New York, and Illinois for voluntary use and was made available to all states in 2003 (National Immigration Law Center 2011), becoming the precursor

⁷ For a complete list of approved documents see <https://www.uscis.gov/i-9-central/acceptable-documents/list-documents/form-i-9-acceptable-documents>

⁸ Specifically, this program was designed to:

“target and remove undocumented immigrants convicted of violent crimes, human smuggling, gang/organized crime activity, sexual-related offenses, narcotics smuggling and money laundering” (DHS 2018).

of what we know today as E-Verify. Colorado became the first state to pass a mandatory E-Verify requirement in 2006, requiring public employers and state contractors to submit new employees' I-9 forms to the E-Verify electronic database.

Following the adoption of Colorado's mandate, 22 additional states enacted E-Verify laws, with 12 states enacting mandates for public employers (or private employers with state contracts) and 10 states extending these mandates to most private employers. In addition, E-Verify is required for all federal workers and contractors and, under current law, is reauthorized by Congress annually (Park and Friedman 2008).

Enforcement of E-Verify occurs at both the federal and state levels. The Monitoring and Compliance Branch of U.S. Citizenship and Immigration Services (USCIS) enforces federal E-Verify policies with the goal of detecting, deterring, and reducing misuse, abuse, and fraud (Bracken 2018). States may also monitor employer compliance with comprehensive active audit systems; South Carolina has enacted one of the most comprehensive audit procedures (Feere 2012). Most state E-Verify laws include sanctions for non-compliance such as fines (usually varying between \$250 and \$1,000 per violation, but sometimes reaching as high as \$10,000 per offense), termination of business licenses, or temporary bans from state contracts. However, some states grant firms immunity from liability for employing undocumented workers if the E-Verify system was used (Park and Friedman 2008).

Although many state mandates do not actively monitor compliance, there have been significant increases in the number of firms that use E-Verify. In 2015, 50 percent of new hires nationwide were verified through the system (Orrenius and Zavodny 2017), representing a 150 percent increase from 2011 (Rosenblum 2011).

2.2 Labor Market Effects of E-Verify

The first wave of studies on E-Verify examined the labor market effects of these mandates. Using data from the Current Population Survey (CPS) from 2004 to 2010 and exploiting temporal variation in the adoption of E-Verify across states, Amuedo-Dorantes and Bansak (2012) found that E-Verify mandates are associated with a 3 to 7 percent decline in employment for likely unauthorized immigrants. There is also some evidence that E-Verify may induce an increase in informal ("under the table") work. Using the same data source over the period from 2005 to 2011, Good (2013) found that state omnibus immigration laws, some of

which include a universal E-Verify mandate, led to a 10 to 20 percent decline in employment of likely unauthorized immigrants. Along the same lines, Bohn and Lofstrom (2012) found that Arizona's E-Verify law reduced employment of likely unauthorized men by 11 percentage points and shifted some toward self-employment, including informal work.

There is also emerging evidence of important general equilibrium labor market effects of E-Verify mandates. Orrenius and Zavodny (2015) used data from the 2002 to 2012 CPS to explore the impact of E-Verify on employment and wages among workers who may compete with unauthorized immigrants. They found universal E-Verify mandates increased wage and salary employment among Mexican-born naturalized citizens by 8 percentage points and increased real earnings among native-born Hispanics by 9 percent. These findings are consistent with unauthorized immigrants serving as substitutes with low-skilled native Hispanic labor. Orrenius and Zavodny (2015) found little evidence that E-Verify impacted employment among non-Hispanic whites.⁹

E-Verify may also affect labor market outcomes through selective migration of unauthorized immigrants. Bohn et al. (2014) used data from the 1998-2009 CPS to examine the impact of Arizona's Legal Arizona Workers Act (LAWA). LAWA included a universal E-Verify provision, prohibition of employers from knowingly or intentionally hiring an undocumented immigrant, and harsher sanctions on non-compliant employers.¹⁰ Using a synthetic control approach, Bohn et al. (2014) found that LAWA was associated with a 2 to 3 percent reduction in the share of the state population comprised of non-native Hispanics. Extending this analysis to E-Verify mandates adopted nationwide, Orrenius and Zavodny (2016) used data from the American Community Survey and found that E-Verify mandates were associated with a 50 percent reduction in the number of newly-arriving low-skilled prime-age immigrants from Mexico and Central America. However, they found no evidence that E-Verify affected migration among immigrants who arrived prior to the passage of the law.

⁹ A further unintended consequence of E-Verify may be job lock. Previously employed undocumented immigrants may be less likely to quit their jobs because E-Verify mandates apply only to hires following the enactment of the law. Thus, E-Verify mandates may depress undocumented workers' wages as well as place them at greater risk of workplace discrimination and harassment (Amuedo-Dorantes and Bansak 2014).

¹⁰ The penalty for the first offense is suspension of a business license and the penalty for the second offense is potential revocation.

2.3 Immigration and Crime

While there is descriptive evidence from administrative data that convicted criminals are more likely to be undocumented immigrants than U.S. citizens (Lott 2018), much of the empirical literature that has sought to identify exogenous variation in immigration has found little evidence that increased immigration causes more crime. Butcher and Piehl (1998b) examined a panel of cities and years and, controlling for unobserved time-invariant characteristics of cities, found that changes in the city-specific share of recent immigrants is unrelated to local crime rates.

Turning to policy as a potential source of exogenous variation in immigration levels, Baker (2015) explored the effect of the 1986 Immigration Reform and Control Act (IRCA), which legalized work and residence status for over three million immigrants. He found that this amnesty reduced property crime by 3 to 5 percent, a result he attributed to enhanced levels of human capital and greater labor market opportunities.

Amuedo-Dorantes et al. (2018) explored the impact of an Obama-era refugee resettlement program and, exploiting geographic and temporal variation in the distribution of refugees across U.S. counties, found that refugee inflows had no impact on local crime rates. Relatedly, Masterson and Yassenov (2019) examined the impact of Executive Order No. 13769, issued by President Trump in January 2017 to halt refugee resettlement and reduce refugee arrivals. Despite a 66 percent decline in refugee arrivals after the executive order, this reduction had no detectable effect on county-level crime rates.

Abman and Foad (2019) examined the impact of border wall construction on arrest rates using a synthetic control method. Using UCR data from 12 border counties that received significant increases in border infrastructure as a result of the 2006 Secure Fence Act, they found no evidence that the border infrastructure had an impact on either violent or property crime arrests.

Finally, Freedman et al. (2018) examined the impact of the expiration of initial amnesty provisions of IRCA, which raised the costs of finding employment to undocumented immigrants, on prosecutions of Hispanics. These authors found that increased barriers to legal employment led to a 59 percent increase in felonious prosecutions of Hispanic residents of Bexar County, Texas for income-generating crime such as theft, prostitution, and fraud. They further found the

increase in prosecuted crimes was largest in neighborhoods with higher proportions of undocumented immigrants.

2.4 E-Verify and Crime

Only two studies of which we are aware have examined the relationship between E-Verify laws and crime. Each is a case study of a particular state policy and the studies reach nearly opposite conclusions.

Zhang et al. (2016) used Uniform Crime Reports (UCR) data from 1998 to 2014 to examine the impact of Alabama's HB 56 E-Verify statute on arrests. Using a synthetic control approach, the authors found that the Alabama E-Verify mandate increased violent crime arrests in Alabama by approximately 100 arrests per year for every 100,000 adults, but was statistically unrelated to property crime arrests. The authors attributed this result to diminished labor market opportunities for likely unauthorized immigrants, but offer little compelling reason why income-generating property crime did not increase.¹¹

Chalfin and Deza (2018) explored the effect of Arizona's LAWA on arrests. The authors used data from the UCR and undertake two identification strategies: a state-level synthetic control approach and an agency-level difference-in-differences approach. They found that LAWA was associated with a 20 percent decrease in property crime arrests involving young men ages 15-to-24, but with no statistically significant change in violent crime arrests. Chalfin and Deza (2018) posited that the property crime reduction they detect was due to outmigration of foreign-born Mexicans.

The current study makes a number of contributions to the existing literature. First, we exploit more policy variation than was available to prior scholars to generate more generalizable estimates of the effect of state E-Verify laws. Second, we provide the very first estimates of the effect of E-Verify on criminal incidents involving Hispanic arrestees. This contribution is important given that the literature on the labor market effects of E-Verify suggests heterogeneous employment, earnings, and migration effects for both Hispanic unauthorized immigrants and natives of Hispanic descent. Moreover, given that Hispanic native arrestees comprise the vast majority of all Hispanic arrestees, examining the impact of E-Verify on immigrants alone will

¹¹ One explanation they offer is that E-Verify induced more violence against likely unauthorized immigrants.

fail to capture the full policy impact.¹² Third, we explore whether there are any spillover effects of E-Verify on arrests involving African American and white adults. Finally, we examine whether the crime effects of E-Verify are stronger for universal mandates applying to private employers.

3. *Data and Methods*

3.1 *National Incident-Based Reporting System*

We begin by using agency-by-month data from the National Incident-Based Reporting System (NIBRS) from 2004 to 2015 to estimate the impact of state E-Verify mandates on criminal incidents involving Hispanic arrestees. The NIBRS is collected by the Federal Bureau of Investigation (FBI) and includes incident-level crime data collected from local law enforcement agencies. NIBRS data provide information concerning administrative, incident, property, victim, offender, and arrestee characteristics, including race and Hispanic ethnicity.

Approximately 93 million Americans, or 29 percent of the U.S. population, are covered by the NIBRS, which encompasses 27 percent of all crime in the U.S. (FBI National Press Office 2015). While geographic coverage of the NIBRS is limited due to the relatively high cost of switching to incident-based reporting (Beltramo 1997), in contrast to the UCR, the NIBRS permits identification of the ethnicity of the arrestee, as well as the ability to slice the data by ethnicity/race, age, and gender.

We measure criminal incidents at the agency-by-month level and our main analysis sample consists of a balanced panel to ensure minimal measurement error. We experimented with broader definitions of our sample, including (i) agencies that reported in at least half the years covering the sample period, or (ii) agencies serving counties of at least 20,000 population. Each of these strategies produced a qualitatively similar pattern of results.

Our main outcome from the NIBRS, *Hispanic Property Crime*, is an agency-by-month count of property crime incidents involving a Hispanic arrestee ages 16-to-64. Property crime is defined as burglary, larceny theft, motor vehicle theft, and arson. We generate *Hispanic Violent Crime* analogously. These crimes include murder, rape, robbery, and aggravated assault. In

¹² Landgrave and Nowrasteh (2019) estimate incarceration rates per 100,000 population ages 18-54 in 2017 are 1,792 for Hispanic natives, 507 for legal Hispanic immigrants, and 1,097 for unauthorized Hispanic immigrants. In addition, Lott (2018) finds that 72 percent of all Hispanic incarcerations in Arizona between 1985 and 2017 involved U.S. citizens of Hispanic descent.

Appendix Table 1A, we report the mean number of property and violent criminal incidents involving Hispanics ages 16-to-64. For comparison, Appendix Table 1A reports counts of property and violent criminal incidents involving non-Hispanic whites and African Americans, respectively, populations also explored below. In addition, Appendix Table 1A shows incident counts for Hispanics by gender and age.

3.2 Uniform Crime Reports

While the NIBRS data have the advantage of allowing us to identify criminal incidents involving arrestees of Hispanic descent, an important limitation of these data is the lack of national coverage. In contrast, the Uniform Crime Reports (UCR) data are representative of the U.S. population in all 50 states and the District of Columbia, covering 98 percent of the population. As shown in Table 1, all 23 states that enacted E-Verify between 2004 and 2015 contribute to identification in the UCR, as compared to half that many states in the NIBRS. The chief disadvantage of the UCR (for our purposes) is that information on arrestee ethnicity is not collected, nor are we able to cut the data by ethnicity/race, age, and gender. Thus, we can only measure criminal arrests involving adult white or African American arrestees. However, the vast majority of all African American and white arrestees are not of Hispanic descent.¹³

We use the UCR data for the same sample period, 2004 to 2015, to investigate the relationship between the implementation of an E-Verify mandate and arrests for white and African American adults. We measure agency-by-month criminal arrest counts for adults ages 18 and older for African Americans and whites (which include Hispanic and non-Hispanic whites). We restrict our sample to agency-month observations that report in at least 90 percent of our sample period.

In contrast to the NIBRS, which is measured at the incident level, the UCR data are measured at the arrest level. We generate agency-by-month counts of arrests, *African American Property Arrests*, *African American Violent Arrests*, *White Property Arrests*, and *White Violent Arrests*. Column (2) of Appendix Table 1B shows descriptive statistics for arrests from the UCR.

¹³ Using data from the 2004 to 2015 NIBRS, we find that those of Hispanic descent comprise 10.4 percent of all criminal incidents involving white arrestees and 0.8 percent of all criminal incidents involving African American arrestees. Thus, this problem is probably quite small.

3.3 Current Population Survey

To explore the labor market and mobility mechanisms through which E-Verify may affect crime, we use the dataset most commonly used in the prior literature, the Current Population Survey Outgoing Rotation Groups (CPS-ORG), also from 2004 through 2015. The surveys are administered by the U.S. Census Bureau for the Bureau of Labor Statistics and are representative of the U.S. population when weighted using appropriate CPS sample weights. The CPS-ORG data include information on labor market outcomes, citizenship status, and other demographic characteristics.

In analyzing the potential mechanisms, we restrict our analysis sample to lower-skilled working-age individuals ages 16-to-64 with a high school diploma or less. We measure whether the respondent is employed (*Any Employment*) and then distinguish between employment for pay (*Employed, Salary and Wages*) and self-employment (*Self-Employed*). We focus on the labor market effects of E-Verify separately for (i) likely undocumented immigrants following Amuedo-Dorantes and Bansak (2014) and Orrenius and Zavodny (2015) (less educated, non-citizen immigrants of Hispanic descent), and (ii) U.S. citizens (naturalized or U.S.-born) of Hispanic descent. In addition, we measure population composition changes following the implementation of E-Verify using an indicator for whether a respondent is a low-skilled Hispanic immigrant or a low-skilled native (citizen) of Hispanic descent. Summary statistics of each of the above variables are available in Appendix Table 3.

3.4 Methods

Using agency-by-month data from the NIBRS 2004 to 2015, we first estimate the following Poisson regression model:

$$Hispanic\ Crime_{ast} = \kappa_{at} \text{Exp}(\beta_0 + \beta_1 EVerify_{st} + \beta_2' \mathbf{X}_{ct} + \beta_3' \mathbf{Z}_{st} + \alpha_a + \tau_t + \varepsilon_{ast}) \quad (1)$$

where a indexes law enforcement agency in state s in month-by-year t (in months 1 to 144). Our primary outcome of interest, $Hispanic\ Crime_{ast}$, measures the number of property or violent criminal arrest involving a working-age (ages 16-to-64) Hispanic arrestee in agency a in state s at month-by-year t . Our key right hand-side variable, $EVerify_{st}$, is an indicator for whether an E-

Verify mandate has been enacted in state s at time t . In alternate specifications, we allow heterogeneous treatment effects by whether the E-Verify mandate applies only to public employers (*Public E-Verify*) or extends to private employers as well.^{14,15}

To disentangle the effects of E-Verify mandates from other time-varying observables, we control for a variety of sociodemographic characteristics, economic conditions, and public policies. The vector \mathbf{X}_{ct} includes county-level controls including demographic characteristics (the age distribution of the county population and the shares of the county population that are male and African American), county-level immigration policies (an indicator for the presence of a 287(g) program, Secure Communities, and an omnibus immigration law), and a county-level indicator for a “ban the box” law (Doleac and Hansen 2017; Sabia et al. 2018). The vector \mathbf{Z}_{st} is a vector of state-level controls for economic conditions (share of population ages 25 and older with a bachelor degree, the natural log of per capita income, and the natural log of the state unemployment rate), the political climate (an indicator for whether the governor is a Democrat), crime policy controls (the natural logs of police expenditure per capita and police employment per capita), gun policy controls (shall issue laws, stand your ground laws, and background check laws), and social policy controls (the natural log of the state minimum wage, the refundable EITC refundable credit rate, ACA-related Medicaid expansions, and SNAP asset test vehicle exemptions). Finally, θ_a is a time-invariant agency fixed effect and τ_t is an agency-invariant month-by-year fixed effect.

Identification of β_1 comes from within-state variation in the enactment of E-Verify. Figure 1 shows the states in which an E-Verify mandate was adopted between 2004 to 2015; Table 1 shows the precise effective dates of the laws and comprehensiveness of the state statute. Public E-Verify mandates generally cover state public employees and contract employees, while universal mandates extend to private employers. Data on effective dates and law comprehensiveness were collected from the National Conference of State Legislatures (2015) and the Urban Institute (2017). As shown in Figure 1, Colorado was the first state to adopt an E-Verify mandate in August 2006; this law applied only to public employers. The first state to adopt a universal E-Verify mandate was Arizona in July 2007.

¹⁴ Regressions using negative binomial regression produce a similar pattern of results as the Poisson model (see Appendix Table 4).

¹⁵ See Grootendorst (2002) for a description of the Poisson regression model.

For our NIBRS-based analysis, 12 states contribute to identifying variation. If we relax our data quality controls to include an unbalanced panel in which agencies report criminal incidents for at least 6 months of the year, 15 states contribute to identification. However, our findings are qualitatively similar across these samples.

Interpreting our estimate of β_1 causally requires that the common trends assumption be satisfied. To test whether crime among Hispanics evolved similarly in treatment and control states prior to the implementation of an E-Verify mandate, we estimate the following specification to produce an event study:

$$Hispanic\ Crime_{ast} = \kappa_{at} \text{Exp} (\alpha_0 + \sum_{j=4, j \neq 1}^3 \gamma_j EVerify^j_{st} + \alpha_2' \mathbf{X}_{ct} + \alpha_3' \mathbf{Z}_{st} + \alpha_a + \tau_t + \varepsilon_{ast}) \quad (2)$$

where $EVerify^j_{st}$ is a set of mutually exclusive indicators set equal to 1 if state s implemented an E-Verify mandate j years away from year t . $EVerify^4_{st}$ is an indicator for four or more years prior to E-Verify enactment, while $EVerify^3_{st}$ analogously accounts for three or more years following enactment.

In addition, to control for the possibility of time-varying spatial heterogeneity that could be correlated with E-Verify and with crime, we experiment with adding controls for state-specific linear time trends, county-level linear time trends, and county-level quadratic time trends. If our estimate of β_1 is largely unchanged, this could suggest that time-varying unobservables at the state- or county-level are unimportant sources of bias. We also experiment with census region-specific year effects to address common shocks to nearby jurisdictions. However, we note that the inclusion of controls for geographic-specific time effects is not without potential cost. Such controls may not only limit the amount of identifying variation, but isolate policy variation that is not necessarily orthogonal to the outcomes under study (Lee and Solon 2011; Neumark and Wascher 2014). Moreover, in the presence of heterogeneous treatment effects, the inclusion of such trends as controls can introduce bias (Goodman-Bacon 2019).

Next, we turn to the UCR, where we estimate equations (1) and (2), but replace the left hand-side variable with *White Arrests* or *African American Arrests*, measured separately for property and violent crime arrests. An important advantage of the UCR over the NIBRS is that all 23 E-Verify states contribute to identification. We also experiment with restricting our UCR

analysis to the treatment and control states from the NIBRS as well as using the NIBRS data source itself. The pattern of results we uncover suggests that any differences in estimates obtained from the UCR and NIBRS are unlikely to be driven by sample selection.

Finally, for our CPS-based analysis on employment and mobility, we estimate ordinary least squares (OLS) regressions, focusing on low-skilled working-age Hispanics with a high school degree or less. We then stratify that low-skilled Hispanic sample by citizenship status.¹⁶ Our primary outcomes of interest measure employment and mobility outcomes of the respondent. First, we generate a dichotomous variable indicating whether an individual is employed; then we generate an employment indicator for those employed for wage-and-salary pay (that is, not self-employed). Second, we measure whether the individual resides in the state to capture mobility effects from E-Verify.

4. Results

Our main findings appear in Tables 2 through 10. Our tables focus on the estimate of β_1 . Coefficient estimates on control variables from our main specifications are shown in Appendix Table 5. Standard errors are clustered at the state-level (Bertrand et al. 2004). However, given the relatively small number of clusters in our NIBRS-based analysis (26 to 39 states), we also calculated p-values using a wild cluster bootstrap standard error approach (Cameron et al. 2008; Cameron and Miller 2015). This approach did not qualitatively change our policy conclusions.

4.1. E-Verify Laws and Criminal Incidents Involving Hispanics

In row (1) of Table 2, we present estimates of β_1 from equation (1) for property crimes involving Hispanic arrestees. Findings from our most parsimonious specification, which includes agency fixed effects and month-by-year fixed effects (column 1), show that the enactment of E-Verify is associated with an 11.1 percent [$1 - \exp(-0.118)$] *reduction* in property crime incidents involving working-age Hispanic arrestees. The inclusion of county-level demographic controls (column 2), economic and political controls (column 3), and crime policy controls (column 4) has little effect on this estimate. In column (5), we add controls for other

¹⁶ We include state-specific linear time trends as controls in the spirit of the specification estimated by Orrenius and Zavodny (2015), an influential paper in this literature. If, instead, we include county-specific linear time trends our estimates are qualitatively similar, though many county identifiers are not available in the CPS. Moreover, if we omit state-specific linear time trends, the results are also qualitatively similar.

state immigration policies, and in column (6), we include a wide set of social welfare policy controls. The result from our most saturated specification (column 6) suggests that E-Verify mandates are associated with a 7.0 percent reduction in property crime involving Hispanics. The relative stability of our estimates of β_1 across specifications lends credence to the hypothesis that E-Verify is implemented exogenously to additional policies which may affect crime.

In sharp contrast to our findings on property crime, we find no evidence that E-Verify reduces violent criminal incidents among Hispanic adults (row 2, Table 2) across any of our specifications. The estimated effects are uniformly small in magnitude (0.019 to -0.043) and are statistically indistinguishable from zero.¹⁷

The results in Table 2 suggest that E-Verify mandates reduce economic crimes, which is consistent with E-Verify-induced increases in employment among low-skilled U.S. citizens of Hispanic descent (Bohn et al. 2015; Orrenius and Zavodny 2015; Orrenius et al. 2018). How plausible is a 7 percent decline in property crime? Orrenius and Zavodny (2015) find that E-Verify is associated with an 8 percentage-point increase in wage and salary employment among Mexican-born naturalized citizens. Lin (2008) finds that a 1 percentage-point increase in unemployment is associated with a 2 to 4 percent increase in property crime. Thus, even after accounting for negative employment effects for unauthorized immigrants (Amuedo-Dorantes and Bansak 2014), a net crime decline of 7 percent among Hispanics is certainly plausible. Moreover, employment is not the only channel that might explain a 7 percent crime decline. Property crime reductions among Hispanics could also be explained by a 9 percent earnings gain for low-skilled natives of Hispanic descent (Orrenius and Zavodny 2015).¹⁸ While it is also possible that our property crime reductions could be explained by outmigration of likely undocumented immigrants (Orrenius and Zavodny 2016; Good 2013) or a decline in victims reporting due to fear of deportation (Zhang et al. 2016), we would also expect declines in violent crime if these were the primary channels.

In Table 3, we explore lead and lagged effects of E-Verify mandates to ensure that any Hispanic property crime reductions we observe follow the enactment of state statutes rather than

¹⁷ When we estimate wild cluster bootstrap standard errors for the specification described in column (6), we obtain p-values of 0.061 for property crime and 0.788 for violent crime.

¹⁸ Gould et al. (2002) found that a 10 percent increase in the wages of non-college-educated men is associated with a 5.4 percent decline in property crime and a 10.8 percent decline in violent crime.

drive their adoption. Column (1) adds leads of up to four or more years prior to the enactment of E-Verify. The coefficients on each of the leads is small, positive, and statistically indistinguishable from zero. Moreover, the coefficient on the E-Verify policy continues to show an approximately 6 percent decline in property crime. The findings in column (2) suggest the property crime-reducing effects of E-Verify occur with a lag, generally after one year following enactment.

In column (3) and the corresponding Figure 2, we present the full event study estimates. The results show no evidence that Hispanic property crime was trending differently in E-Verify and non-E-Verify states prior to the implementation of an E-Verify mandate. Property crime declines occur a year following enactment and grow somewhat larger over time. This pattern of results is consistent with a causal impact of E-Verify laws on property crime. In contrast, for violent crime (columns 4 through 6), we find no evidence of significant lead or lagged effects of E-Verify mandates.¹⁹

In Table 4, we explore the sensitivity of our estimates to controls for state- or county-specific time trends. While such controls may bias estimated policy impacts if there are heterogeneous treatment effects across state mandates enacted at different times (Goodman-Bacon 2019), our findings generally confirm the pattern of results shown in Table 2. Column (1) reproduces the estimates from column (5) of Table 2. The inclusion of controls for state-specific linear time trends (column 2), county-specific linear time trends (column 3), and county-specific quadratic time trends (column 4) do not change our main finding. Across these specifications, we find that E-Verify laws are associated with a 5.5 to 8.6 percent reduction in property crime incidents involving Hispanic arrestees.²⁰

4.2 Heterogeneity in Effects of E-Verify Mandates on Hispanic Crime

¹⁹ In Appendix Table 6, we reproduce Table 3 excluding Colorado and including only 3 lead years to ensure that each lead coefficient is identified off of a common set of states. The results are qualitatively similar to those reported in Table 3. In Appendix Table 7, we explore whether our findings are driven by any particular state's E-Verify law. Across samples, our results provide consistent evidence of a 5 to 8 percent decline in property crime incidents involving working-age Hispanic arrestees. Estimated effects on violent crime are much smaller and statistically indistinguishable from zero in all cases.

²⁰ In Appendix Table 8, we add controls for region-specific year effects to the model shown in column (3). We estimate coefficients on E-Verify of -0.080 with a standard error of 0.035 for property crime and 0.005 with a standard error of 0.032 for violent crime.

Tables 5 and 6 explore heterogeneity in the effects of E-Verify mandates by age, gender, breadth of mandate, and specific type of property crime. With regard to age, we find that our estimated effects are concentrated among working-age Hispanics ages 20-to-44, a demographic group for whom E-Verify mandates have been shown to have relatively larger labor market effects (Amuedo-Dorantes and Bansak 2014).²¹ In contrast, we find no evidence that property crime was affected by E-Verify among younger (ages 16-to-19) or older (ages 55-to-64) individuals (Panel I, column 2).

Moreover, an examination of the crime effects of E-Verify by gender (Panel II) suggests that its property crime-reducing effects are concentrated among males, a population with substantially higher crime rates. While the estimated effect of E-Verify on Hispanic females remains negative, it is smaller in magnitude and statistically indistinguishable from zero at conventional levels.

In Panel III, we explore heterogeneity in the impacts of E-Verify by mandate type: (i) those that apply only to public employers (*Public E-Verify*), and (ii) those that apply to both private and public employers (*Universal E-Verify*). We disaggregate our main policy variable by type of mandate and include controls for each type in the regression. Our findings suggest substantially larger effects of *Universal E-Verify* mandates on Hispanic property crime relative to *Public E-Verify* policies. We find that E-Verify policies that extend to private employers are associated with a statistically significant 13.5 percent decline in property crime. An event study analysis of universal E-Verify suggests that there are important lagged effects of this policy (see Figure 3). This result is also consistent with the positive employment effects of E-Verify on low-skilled native Hispanics being larger for those mandates that extend to private employers in the state (Orrenius and Zavodny 2015).

Finally, in Panel IV, we explore whether the crime effects we obtain can be explained by endogenous mobility of low-skilled immigrants and natives of Hispanic descent. The estimated effect of E-Verify on crime is largely unchanged with the inclusion of controls for the share of the state population that is comprised of working-age (ages 16-to-64), less educated (attained a

²¹ A new working paper by Fone et al. (2019) finds that the Affordable Care Act's Dependent Coverage Mandate reduces criminal arrests among those ages 19-to-25 relative to those ages 27-to-29. In Appendix Table 9, we examine the effect of E-Verify mandates on property crime for individuals who are affected by the ACA dependent coverage mandate (those who are ages 19-25) and individuals who are not (ages 26 and over). Our findings suggest that the effects of E-Verify we uncover are not confounded by the DCM.

high school degree or less) Hispanic immigrants (non-citizens) and working-age, less educated natives (U.S.-born or naturalized) of Hispanic descent. This result suggests that mobility responses to E-Verify mandates probably do not fully explain their property crime-reducing effects.

In Table 6, we explore the types of property crimes that drove this decline (Panel I), as well as whether particular violent crimes (Panel II), or other non-violent, non-property crimes (Panel III) were affected by E-Verify. Our results show that the decline in property crime is largely driven by larcenies, which account for over 80 percent of all property crimes. There is also a substantial negative effect of E-Verify on motor vehicle theft, though this estimate is less precisely estimated.

With regard to violent crime (the vast majority of which is assaults) and other non-violent crimes, we generally fail to detect statistically significant relationships between the enactment of E-Verify and these types of crimes. The only exception is for stolen property, where we find that E-Verify is associated with a substantial decline, a finding consistent with economically motivated crime.

Finally, in Table 7, we examine whether E-Verify displaces Hispanic crime to other jurisdictions in close geographic proximity to an E-Verify mandate. This is quite important from a social welfare perspective. We generate two measures including (i) *Border-State E-Verify*, which turns on when a border state adopts an E-Verify mandate, and (ii) *Census-Division E-Verify*, which turns on when a state within the state's own census division enacts E-Verify.

In odd-numbered columns, we restrict the sample to jurisdictions that have never implemented E-Verify and estimate equation (1), replacing *E-Verify* with *Border State E-Verify* (Panel I) and *Census Division E-Verify* (Panel II). In even-numbered columns, we pool all available jurisdictions and add *Border State E-Verify* (Panel I) or *Census Division E-Verify* (Panel II) to the right-hand side of equation (1). In no case do we uncover evidence that property or violent crime was displaced to neighboring NIBRS jurisdictions.

4.3 Mechanisms to Explain Decline in Hispanic Property Crime

To explore the mechanisms through which E-Verify affects crimes among Hispanics, we pool data from the 2004-2015 CPS-ORG to examine the effect of E-Verify on employment and population composition of less educated immigrants and natives. These results are shown in

Table 8. We present results for all working-age individuals and then individuals ages 20-to-44, the age group for which we find the strongest evidence of crime reductions.

First, consistent with prior work (Amuedo-Dorantes and Bansak 2014), we find that the implementation of an E-Verify mandate is associated with a 1.3 percentage-point decline in any employment among likely unauthorized male immigrants (column 1, Panel I). However, consistent with Amuedo-Dorantes and Bansak (2014), we also uncover evidence that likely unauthorized Hispanic immigrants and low-skilled citizens of Hispanic descent are labor-labor substitutes. We find that E-Verify is associated with a 3 to 4 percentage-point increase in wage- and-salary employment among low-skilled citizens of Hispanic descent. Given that U.S. citizens of Hispanic descent make up the vast majority of prosecuted Hispanic defendants (Landgrave and Nowrasteh, 2019; Lott 2018), this positive employment effect — in conjunction with the fact that immigrants are less likely to be criminally prone than natives (Chalfin 2013; Butcher and Piehl 2008) — is likely an important channel to explain the net decline in property crime that we observe for working-age Hispanics.

In Panel III of Table 8, we explore the impact of E-Verify on the probability that a likely undocumented immigrant or a low-skilled native of Hispanic descent resides in the state.²² This exercise is designed to explore whether E-Verify induced mobility of low-skilled immigrants or natives. We find that the enactment of E-Verify is associated with a 5.6 percent reduction in the probability that a less educated male Hispanic immigrant ages 20-to-44 resides in that jurisdiction. However, we also find evidence that E-Verify is associated with a 10 percent decline in the probability that a Hispanic native resides in the state, a somewhat surprising result if employment opportunities have improved. This finding may suggest that there are other costs to those of Hispanic descent associated with the implementation of E-Verify, including increased perceptions of discrimination at work among Hispanic individuals (Amuedo-Dorantes et al. 2015). While it is possible that outmigration may explain some of the property crime reduction we detect, it is unlikely to be the only mechanism given that (i) we do not observe a similar decline in violent crime, (ii) our property crime results persist after controlling for state shares of

²² Event study analysis of the effect of E-Verify on the probability that a low-skilled Hispanic immigrant or native of Hispanic descent resides in the state suggests (i) little difference in pre-treatment trends in the share of the state population that are low-skilled Hispanics, and (ii) outmigration of low-skilled Hispanics follows the enactment of E-Verify.

low-skilled Hispanic immigrants and natives, and (iii) we fail to detect crime spillovers to neighboring non-E-Verify jurisdictions.²³

In Table 9, we examine the effect of *Universal E-Verify* mandates and *Public E-Verify* mandates on labor market outcomes. While our findings are mixed, the results in Panel II do suggest that the magnitude of the effect of E-Verify mandates on wage-and-salary employment for low-skilled male citizens of Hispanic descent is greater for E-Verify mandates that extend to private employers relative to only public employers (marginal effect of 0.082 versus 0.034).²⁴ This finding is consistent with larger property crime effects for more expansive E-Verify laws.

4.4 White and African American Arrestees

Finally, to explore the effects of E-Verify on arrests involving African American and white adults ages 18 and older, we turn to the UCR to exploit maximum policy variation. Our findings in Panel I of Table 10 provide no evidence that E-Verify mandates affect arrests among adult African American and whites. This result is consistent with prior evidence that the labor market effects of E-Verify are small for these demographic groups.

To ensure that differences in estimated effects of E-Verify for Hispanics vs. whites and African Americans are not explained by differences in the NIBRS and UCR samples, we conducted a robustness check wherein we restricted our UCR sample to the states available in the NIBRS. Our findings in Appendix Table 11 are consistent with those in Table 10.²⁵

Panel II and Figures 6 and 7 show event study analyses for criminal arrests involving white and African American adults. We find no evidence of differential pre-trends in property or violent crime and no evidence that E-Verify had economically important or statistically significant lagged impacts on crime among these demographic groups. Finally, in Panel III, we

²³ In results shown in Appendix Table 10, we find a qualitatively similar pattern of results in Table 8 if we omit state-specific linear time trends from the regression specification.

²⁴ Event study analysis for the effect of *Universal E-Verify* on the probability that a low-skilled Hispanic (i) resides in that state, and (ii) is employed for pay, are shown in Figures 4 and 5, respectively.

²⁵ As noted above, one of the limitations of the UCR is our inability to identify the ethnicity of arrestees. Thus, our estimates on whites and African Americans will include Hispanic whites and Hispanic African Americans. However, estimates from the NIBRS on working-age non-Hispanic whites and African Americans provide little evidence that E-Verify caused a decline in property or violent crime (Appendix Table 12). While the coefficient on criminal incidents involving non-Hispanic white arrestees in the NIBRS is negative and of non-trivial magnitude, event study analysis on non-Hispanic whites provide no evidence that this insignificant effect is causal in nature.

find no evidence that public E-Verify mandates or mandates that extended to private employees reduced crime involving white or African American arrestees.²⁶

5. Conclusion

On January 4, 2019, 17 Republican congressmen reintroduced the *Legal Workforce Act of 2019*. This legislation would mandate the use of an E-Verify system across all 50 states for public and private employers. Although comprehensive immigration reform has remained one of the most contentious issues in American politics, there is growing support in favor of a federal E-Verify law. In June 2019, Senator Mitt Romney (R-Utah) introduced the Permanent E-Verify Act, which would abolish annual Congressional renewal of federal E-Verify requirements, thereby making the federal law permanent.²⁷

There is emerging evidence that E-Verify redistributes employment from likely unauthorized immigrants to low-skilled U.S. citizens of Hispanic descent. This study comprehensively examines the impact of state E-Verify mandates on criminal arrests among working-age Hispanics, African Americans, and whites.

Using data drawn from the National Incident-Based Reporting System, we find that E-Verify laws are associated with a 5 to 10 percent decrease in property crimes involving working-age Hispanics. The effects are largest for males, those ages 20-to-44, and for E-Verify policies that extend to private employers. Supplemental analyses from the Current Population Survey suggest that (i) increases in employment among low-skilled citizens of Hispanic descent and, perhaps, (ii) outmigration of younger immigrants, likely explain the net reduction in Hispanic crime. We find no evidence of property crime displacement in jurisdictions without an E-Verify mandate and no evidence that E-Verify impacts violent crime among Hispanics. Finally, our

²⁶ In Appendix Table 13, we examine the effect E-Verify on migration and employment for non-Hispanic whites and African Americans. We find little to no evidence that E-Verify mandates impact labor force participation among African Americans and non-Hispanic whites, consistent with null crime effects reported above. We do uncover some inconsistent evidence that E-Verify may attract low-skilled non-Hispanic whites to E-Verify states, consistent with attraction to low-skilled job opportunities. But, as shown in Appendix Table 14, we find little evidence of spillover effects of white or African American crime to neighboring jurisdictions.

²⁷ The Permanent E-Verify Act would not make E-Verify mandatory for all private employers, but simply make the current federal program, which applies to federal government employees and contractors, permanent. Senator Romney expressed the hope that this would be a first step toward “working on long term fixes to secure the border, update our asylum and trafficking laws, and institute mandatory E-Verify nationwide” (Giaritelli 2019).

analysis of the Uniform Crime Reports produces no evidence that E-Verify affected arrests among African American or white adults, consistent with negligible labor market impacts of E-Verify on these demographic groups.

While E-Verify mandates are generally considered to be welfare diminishing because they distort low-skilled labor markets by protecting native workers' jobs and impose substantial compliance costs on firms, this study has uncovered an important social benefit that may arise from redistributing employment and earnings from likely undocumented immigrants to natives: reduced property crime. Using per-offense social cost of property crime from McCollister et al. (2010), our estimates suggest \$491 million (2018 dollars) in annual social benefits to the U.S. from E-Verify-induced property crime reductions.²⁸ Of course, this estimate may overstate the social benefits to the Western Hemisphere if crime is displaced to border countries in North and Central America. Still, this social benefit might be accounted in future cost-benefit analysis of E-Verify mandates to the United States.

²⁸ Data on property crimes committed over the 2004-2015 period are obtained using the FBI's Crime in the United States reports (available from: <https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016/topic-pages/tables/table-1>). We then use the 2004-2015 UCR's Arrests by Age, Sex, and Race files to calculate the share of property crime arrests involving men ages 16-to-64. To generate an estimate of the number of crimes committed by men ages 16-to-64, we multiply the crime counts in the 2004-2014 period from the FBI's Crime in the United States report with the share of property crime arrests involving men ages 16-to-64 from the UCR's Arrests by Age, Sex, and Race files. Next, we estimate the number of crimes committed by Hispanic men ages 16-to-64 by multiplying the estimated crimes committed by men ages 16-to-64 with the percent of arrests involving Hispanic male adults (available at <https://ucr.fbi.gov/crime-in-the-u.s/2014/crime-in-the-u.s.-2014/tables/table-43>). Using our findings from Table 2, Column 5 where we find E-Verify is associated with a 7 percent decrease in property crimes among Hispanic males ages 16-to-64, we estimate 85,555 fewer property crimes following the enactment of E-Verify mandates. Finally, we use the per crime cost of a property offense of \$5,739 (in 2018 dollars) from McCollister et al. (2010) to obtain the total E-Verify-induced property crime benefit of \$491 million.

6. References

- Abman, R. & Foad, H. (2019). Border Infrastructure and Crime: Evidence from the Secure Fence Act. San Diego State University.
- Amuedo-Dorantes, C., & Bansak, C. (2012). The Labor Market Impact of Mandated Employment Verification Systems. *American Economic Review*, 102(3), 543-48.
- Amuedo-Dorantes, C., & Bansak, C. (2014). Employment Verification Mandates and the Labor Market Outcomes of Likely Unauthorized and Native Workers. *Contemporary Economic Policy*, 32(3), 671-680.
- Amuedo-Dorantes, C., Jin, X., & Pozo, S. (2015). Does E-Verify discriminate against Hispanic Citizens? Working paper, American University of Sharjah School of Business Administration.
- Amuedo-Dorantes, C., Bansak, C., & Pozo, S. (2018). Refugee Admissions and Public Safety: Are Refugee Settlement Areas More Prone to Crime? *IZA Working Paper No. 11612*.
- Baker, S. R. (2015). Effects of Immigrant Legalization on Crime. *American Economic Review*, 105(5), 210-13.
- Beltramo, M. N. (1997). Cost Issues of Implementing the National Incident-based Reporting System in Local Law Enforcement Agencies. NIBRS Project Staff Report 3. SEARCH Group, Sacramento, CA.
- Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How Much Should We Trust Differences-In-Differences Estimates? *Quarterly Journal of Economics*, 119(1), 249-275.
- Bohn, S., & Lofstrom, M. (2012). Employment Effects of State Legislation Against the Hiring of Unauthorized Immigrant Workers. *IZA Discussion Paper No. 6598*.
- Bohn, S., Lofstrom, M., & Raphael, S. (2014). Did the 2007 Legal Arizona Workers Act Reduce the State's Unauthorized Immigrant Population? *Review of Economics and Statistics*, 96(2), 258-269.
- Bohn, S., Lofstrom, M., & Raphael, S. (2015). Do E-verify Mandates Improve Labor Market Outcomes of Low-Skilled Native and Legal Immigrant Workers?. *Southern Economic Journal*, 81(4), 960-979.
- Bracken, M. (2018). USCIS Now Requesting I-9 Forms When Conducting E-Verify Desk Reviews. Retrieved October 1, 2019 from:

<http://ntlakis.com/index.php/immigration/uscis-now-requesting-i-9-forms-when-conducting-e-verify-desk-reviews/>.

- Butcher, K. F., & Piehl, A. M. (1998a). Recent Immigrants: Unexpected Implications for Crime and Incarceration. *Industrial and Labor Relations Review*, 51(4), 654-679.
- Butcher, K. F., & Piehl, A. M. (1998b). Cross-City Evidence on the Relationship Between Immigration and Crime. *Journal of Policy Analysis and Management*, 17(3), 457-493.
- Butcher, K. F., & Piehl, A. M. (2008). Crime, Corrections, and California. *California Counts*, 9(3), 1-23.
- Cameron, A. C., Gelbach, J. B., & Miller, D. L. (2008). Bootstrap-based Improvements for Inference with Clustered Errors. *Review of Economics and Statistics*, 90(3), 414-427.
- Cameron, A. C., & Miller, D. L. (2015). A Practitioner's Guide to Cluster-Robust Inference. *Journal of Human Resources*, 50(2), 317-372.
- Castillo, M. & Schulman, J. (2011). Ready or Not, Here They Come: State E-Verify Laws and What Employers Should Know. *Morrison and Foerster Employment Law Commentary* 23(8): 1-8.
- Cato Institute. (2018). Criminal Immigrants in Texas: Illegal Immigrant Conviction and Arrest Rates for Homicide, Sexual Assault, Larceny, and Other Crimes. *Immigration Research and Policy Brief*, (4).
- Center for Immigration Studies (2019). Historical Overview of Immigration Policy. Retrieved October 1, 2019 from: <https://cis.org/Historical-Overview-Immigration-Policy>
- Chalfin, A. (2013). What is the Contribution of Mexican Immigration to US crime rates? Evidence from Rainfall Shocks in Mexico. *American Law and Economics Review*, 16(1), 220-268.
- Chalfin, A. & Deza, M. (2018). New Evidence on Mexican Immigration and Crime in the United States: Evidence from a Natural Experiment in Immigration Enforcement. Retrieved October 1, 2019 from: <http://achalfin.weebly.com/uploads/8/5/4/8/8548116/chalfindeza2017.pdf>.
- Chisti, M., Kamasaki, C. (2014). IRCA In Retrospect. Washington, DC: Migration Policy Institute. Retrieved October 1, 2019 from: <http://www.migrationpolicy.org/research/irca-retrospect-immigration-reform>.

- Davis, J. H., Tackett, M. (2019). Trump and Democrats Dig in After Talks to Reopen Government Go Nowhere. *The New York Times*.
- Department of Homeland Security (2018). What is E-Verify. Retrieved October 1, 2019 from: <https://www.e-verify.gov/about-e-verify/what-is-e-verify>.
- Doleac, J. L., & Hansen, B. (2017). Moving to Job Opportunities? The Effect of “Ban the Box” on the Composition of Cities. *American Economic Review*, 107(5), 556-59.
- East, C. N., Luck P., Mansour H., & Velasquez A. (2018) The Labor Market Effects of Immigration Enforcement. *IZA Working Paper No. 11486*.
- Federal Bureau of Investigation National Press Office. (2015). FBI Releases 2014 Crime Statistics from the National Incident-Based Reporting System. Retrieved October 1, 2019 from: <https://www.fbi.gov/news/pressrel/press-releases/fbi-releases-2014-crime-statistics-from-the-national-incident-based-reporting-system>.
- Feere, J. (2012). An Overview of E-Verify Policies at the State Level. Retrieved October 1, 2019 from: <https://cis.org/Overview-EVerify-Policies-State-Level>.
- Fone, Z., Friedson, A., Lipton, B., & Sabia, J. (2019). Did the Affordable Care Act Take a Bite Out of Crime? Evidence from the Dependent Coverage Mandate. Working Paper, University of New Hampshire.
- Freedman, M., Owens, E., & Bohn, S. (2018). Immigration, Employment Opportunities, and Criminal Behavior. *American Economic Journal: Economic Policy*, 10(2), 117-51.
- Gallup, Inc. (2017). Immigration. Retrieved October 1, 2019 from: <https://news.gallup.com/poll/1660/immigration.aspx>.
- Giaritelli A. (2019) New E-Verify Bill Could Bring Romney and Trump Together Over Common Cause. Retrieved October 1, 2019 from: <https://www.washingtonexaminer.com/news/new-e-verify-bill-could-bring-romney-and-trump-together-over-common-cause>.
- Good, M. (2013). Do Immigrant Outflows Lead to Native Inflows? An Empirical Analysis of the Migratory Responses to U.S. State Immigration Legislation. *Applied Economics*, 45(30), 4275-4297.
- Goodman-Bacon, A. (2019). Difference-in-Differences with Variation in Treatment Timing. Under Review. Retrieved October 1, 2019 from: https://cdn.vanderbilt.edu/vu-my/wp-content/uploads/sites/2318/2019/07/29170757/ddtiming_7_29_2019.pdf.

- Gould, E. D., Weinberg, B. A., & Mustard, D. B. (2002). Crime Rates and Local Labor Market Opportunities in the United States: 1979–1997. *Review of Economics and Statistics*, 84(1), 45-61.
- Grootendorst, Paul V. 2002. “A Comparison of Alternative Models of Prescription Drug Utilization.” In Andrew M. Jones and Owen O’Donnell (eds.), *Econometric Analysis of Health Data*, Hoboken, NJ: John Wiley and Sons, Ltd, pp. 73-86.
- Hagan, J., & Palloni, A. (1999). Sociological Criminology and the Mythology of Hispanic Immigration and Crime. *Social Problems*, 46(4), 617-632.
- Hanson, Gordon H. (2006). Illegal Migration from Mexico to the United States. *Journal of Economic Literature*, 44:869– 924.
- Harvard-Harris Poll (2018). Monthly Harvard-Harris Poll: January 2019 Re-Field. Retrieved October 1, 2019 from: http://harvardharrispoll.com/wp-content/uploads/2018/01/Final_HHP_Jan2018-Refield_RegisteredVoters_XTab.pdf.
- KCCI (2018). Grassley Demands Answers on Undocumented Immigrant Charged in Mollie Tibbetts' Death. Retrieved October 1, 2019 from: <https://www.thegazette.com/subject/news/government/chuck-grassley-mollie-tibbetts-cedar-rapids-iowa-undocumented-immigrant-cristhian-bahena-rivera-brooklyn-missing-found-murder-killing-20180822>
- Kerwin, D. M., & McCabe, K. (2011). *Labor Standards Enforcement and Low-Wage Immigrants: Creating an Effective Enforcement System*. Washington, DC: Migration Policy Institute.
- Kubrin, C. E. (2014). Secure or Insecure Communities-Seven Reasons to Abandon the Secure Communities Program. *Criminology and Public Policy*, 13, 323.
- Landgrave, M. & Nowrasteh A. (2019) Criminal Immigrants in 2017: Their Numbers, Demographics, and Countries of Origin. Cato Institute. Retrieved October 1, 2019 from: <https://www.cato.org/publications/immigration-research-policy-brief/criminal-immigrants-2017-their-numbers-demographics>.
- Lee, J., Peri G., & Yasenov V. (2017) The Employment Effects of Mexican Repatriations: Evidence from the 1930’s. *National Bureau of Economic Research Working Paper* No. 23885.
- Lee, J. & Solon, G. (2011). The fragility of estimated effects of unilateral divorce laws

- on divorce rates. *B.E. Journal of Economic Analysis & Policy* 11(1): 1-11.
- Light, M. T., & Miller, T. (2018). Does Undocumented Immigration Increase Violent Crime? *Criminology*, 56(2), 370-401.
- Lin, M. J. (2008). Does unemployment increase crime? Evidence from US data 1974–2000. *Journal of Human Resources*, 43(2), 413-436.
- Lott, John R. (2018) Undocumented Immigrants, U.S. Citizens, and Convicted Criminals in Arizona. Retrieved October 1, 2019 from: <http://dx.doi.org/10.2139/ssrn.3099992>.
- MacDonald, J. M., Hipp, J. R., & Gill, C. (2013). The Effects of Immigrant Concentration on Changes in Neighborhood Crime Rates. *Journal of Quantitative Criminology*, 29(2), 191-215.
- Masterson, D., & Yasenov, V. (2019). Does Halting Refugee Resettlement Reduce Crime? Evidence from the United States Refugee Ban. *IZA Discussion Paper No 12551*.
- McCollister, Kathryn E., Michael T. French, & Hai Fang. (2010). The Cost of Crime to Society: New Crime-Specific Estimates for Policy and Program Evaluation. *Drug and Alcohol Dependence* 108 (1-2): 98-109.
- Michaud, Nicholas D. (2010). From 287(g) to SB 1070: The Decline of the Federal Immigration Partnership and the Rise of State-Level Immigration Enforcement. *Arizona Law Review* 52: 1082–133.
- Miles, T. J., & Cox, A. B. (2014). Does Immigration Enforcement Reduce Crime? Evidence from Secure Communities. *Journal of Law and Economics*, 57(4), 937-973.
- Mustard, D. B. (2010). How Do Labor Markets Affect Crime? New Evidence on an Old Puzzle. *IZA Discussion Paper No. 4856*.
- National Conference of State Legislature (2015). State E-Verify Action. Retrieved October 1, 2019 from: <http://www.ncsl.org/research/immigration/state-e-verify-action.aspx>
- National Immigration Law Center (2011). The History of E-Verify. Retrieved October 1, 2019 from: <https://www.nilc.org/wp-content/uploads/2015/12/e-verify-history-rev-2011-09-29.pdf>.
- Newman, B. J., Johnston, C. D., Strickland, A. A., & Citrin, J. (2012). Immigration Crackdown in the American Workplace: Explaining Variation in E-Verify Policy Adoption Across the US states. *State Politics and Policy Quarterly*, 12(2), 160-182.

- Neumark, D.J., Salas, M & Wascher, W. (2014). Revisiting the minimum wage—debate: Throwing out the baby with the bathwater? *Industrial Labor Relations Review* 67(3): 608-648.
- Orrenius, P. M., & Zavodny, M. (2015). The Impact of E-Verify Mandates on Labor Market Outcomes. *Southern Economic Journal*, 81(4), 947-959.
- Orrenius, P. M., & Zavodny, M. (2016). Do State Work Eligibility Verification Laws Reduce Unauthorized Immigration? *IZA Journal of Migration*, 5(1), 5.
- Orrenius, P. M., & Zavodny, M. (2017). Creating Cohesive, Coherent Immigration Policy. *Journal on Migration and Human Security*, 5(1), 180-193.
- Orrenius, P. M., Zavodny, M., & Gutierrez, E. (2018). Do State Employment Eligibility Verification Laws Affect Job Turnover? *Contemporary Economic Policy*, 36(2), 394-409.
- Orrenius, P. M., & Zavodny, M. (2019). Do Immigrants Threaten US Public Safety? Working Paper, The Center for Growth and Opportunity.
- Öster, A., & Agell, J. (2007). Crime and Unemployment in Turbulent Times. *Journal of the European Economic Association*, 5(4), 752-775.
- Park, E., & Friedman, D. S. (2008). E-Verify for Federal Contractors and Subcontractors Required Starting January 15, 2009. *Labor and Employment Alert: News Concerning Recent Labor and Employment Issues (Cozen O'Connor)*. Retrieved October 1, 2019 from: <http://www.cozen.com/admin/files/publications/Labor111708.pdf>.
- Raphael, S., & Winter-Ebmer, R. (2001). Identifying the Effect of Unemployment on Crime. *The Journal of Law and Economics*, 44(1), 259-283.
- Rosenblum, M. R. (2011). E-Verify: Strengths, Weaknesses, and Proposals for Reform. Washington, DC: Migration Policy Institute.
- Sabia, J. J., Mackay, T., Nguyen, T. T., & Dave, D. M. (2018). Do Ban the Box Laws Increase Crime? *National Bureau of Economic Research. Working Paper* No. 24381.
- Time Staff (2015). Here's Donald Trump's Presidential Announcement Speech. Retrieved October 1, 2019 from: <https://time.com/3923128/donald-trump-announcement-speech/>.
- Treyger, E., Chalfin, A., & Loeffler, C. (2014). Immigration Enforcement, Policing, and Crime: Evidence from the Secure Communities Program. *Criminology and Public Policy*, 13(2), 285-322.

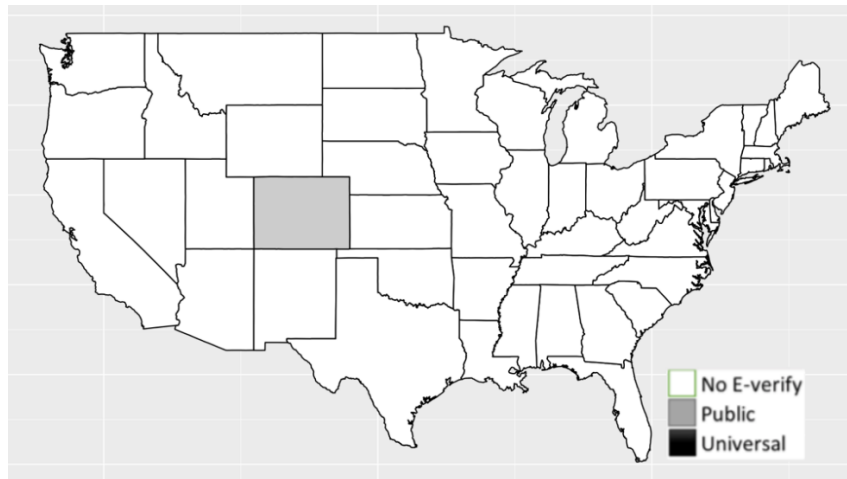
- Urban Institute. (2017). State immigration policy resource. Accessed at: <https://www.urban.org/features/state-immigration-policy-resource> (January 2019).
- Warren, R. (2019). US Undocumented Population Continued to Fall from 2016 to 2017, and Visa Overstays Significantly Exceeded Illegal Crossings for the Seventh Consecutive Year. Retrieved October 1, 2019 from: <https://cmsny.org/publications/essay-2017-undocumented-and-overstays/>
- Zhang, Y., Palma, M. A., & Xu, Z. P. (2016). Unintended Effects of the Alabama HB 56 Immigration Law on Crime: A Preliminary Analysis. *Economics Letters*, 147, 68-71.

Figure 1. Enactment of State E-Verify Mandates, 2004-2015

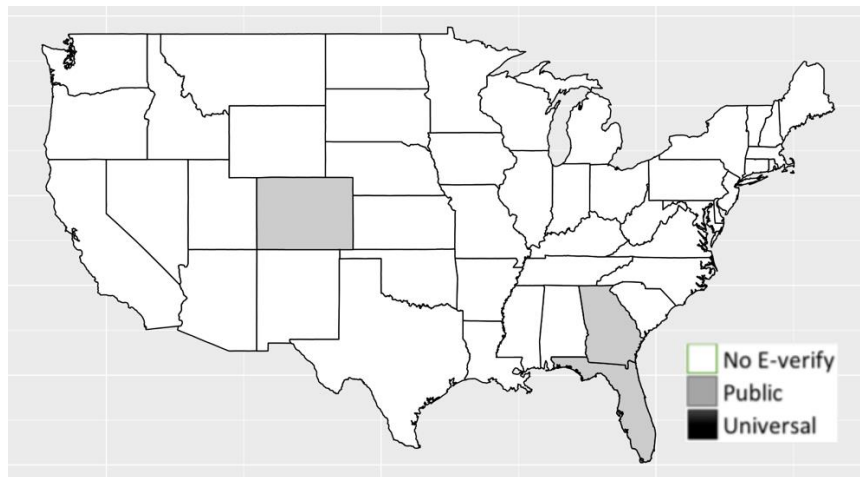
2004



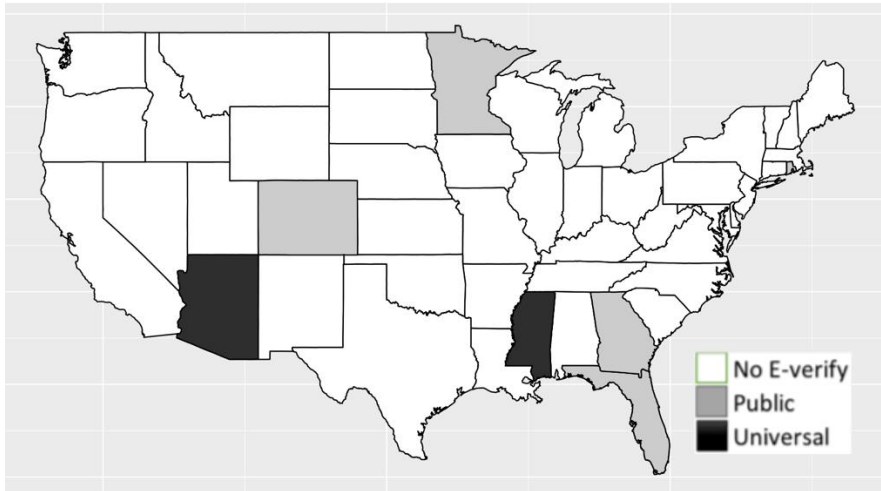
2006



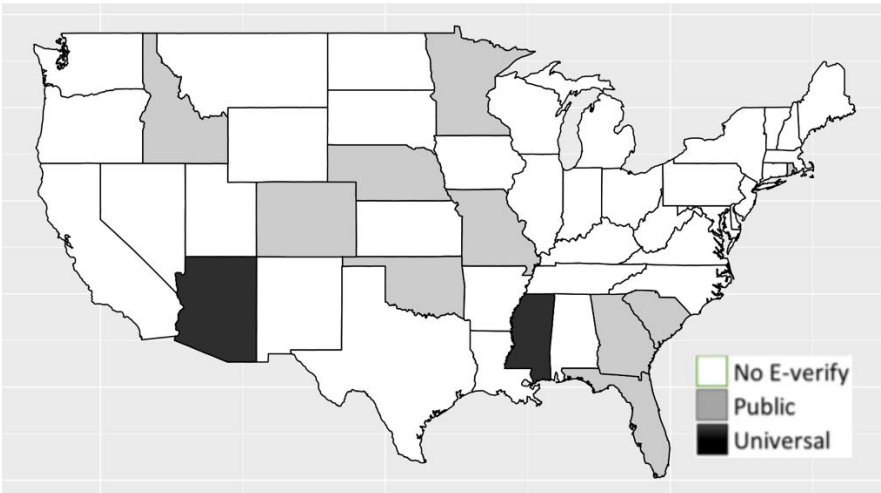
2007



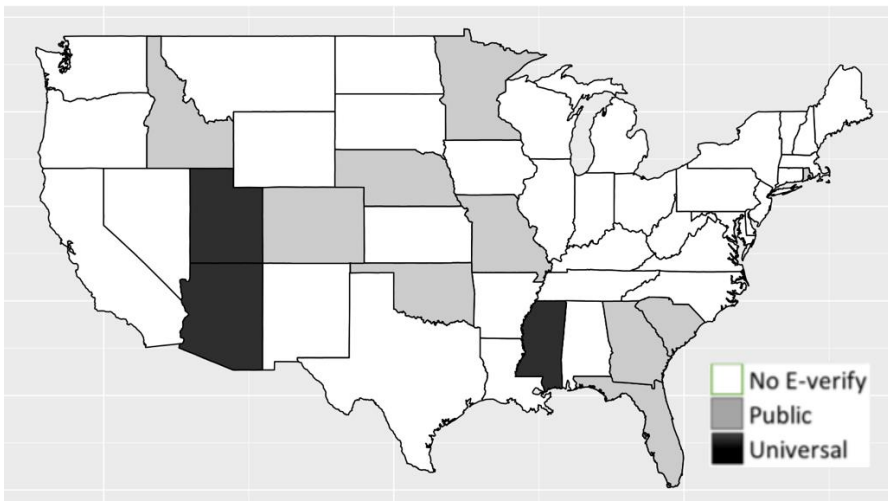
2008



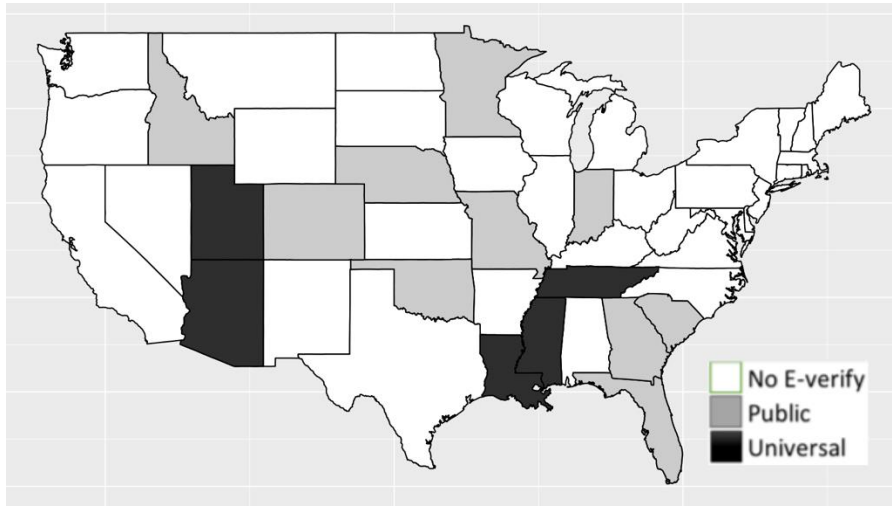
2009



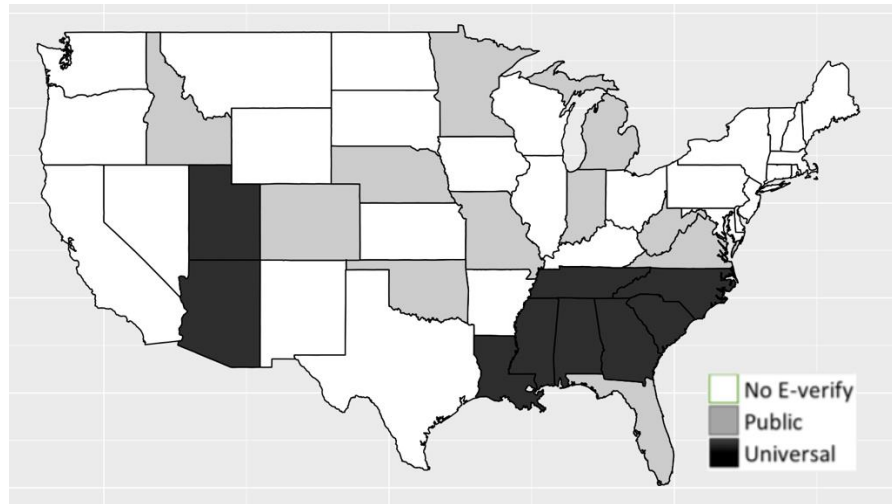
2010



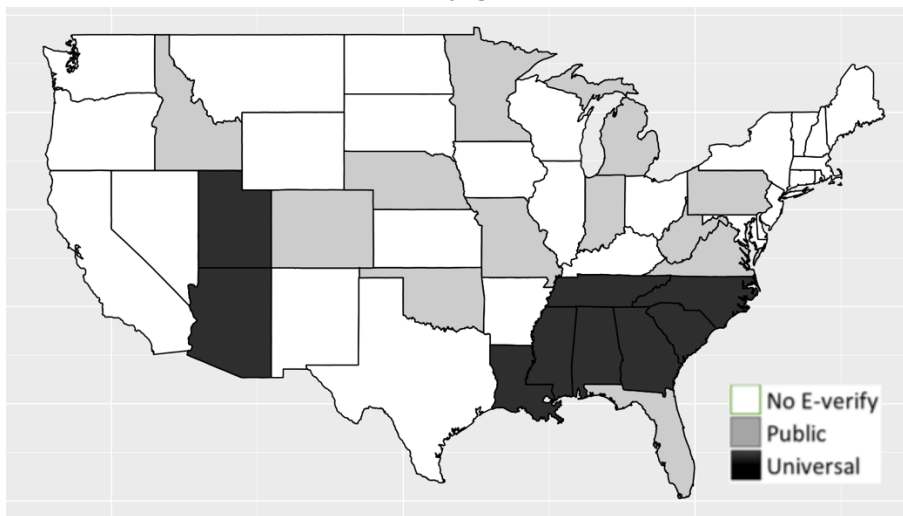
2011



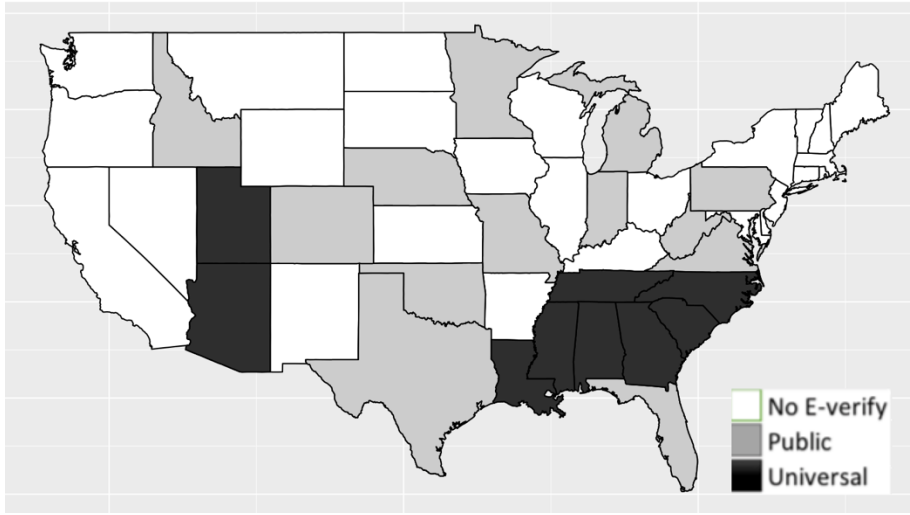
2012



2013

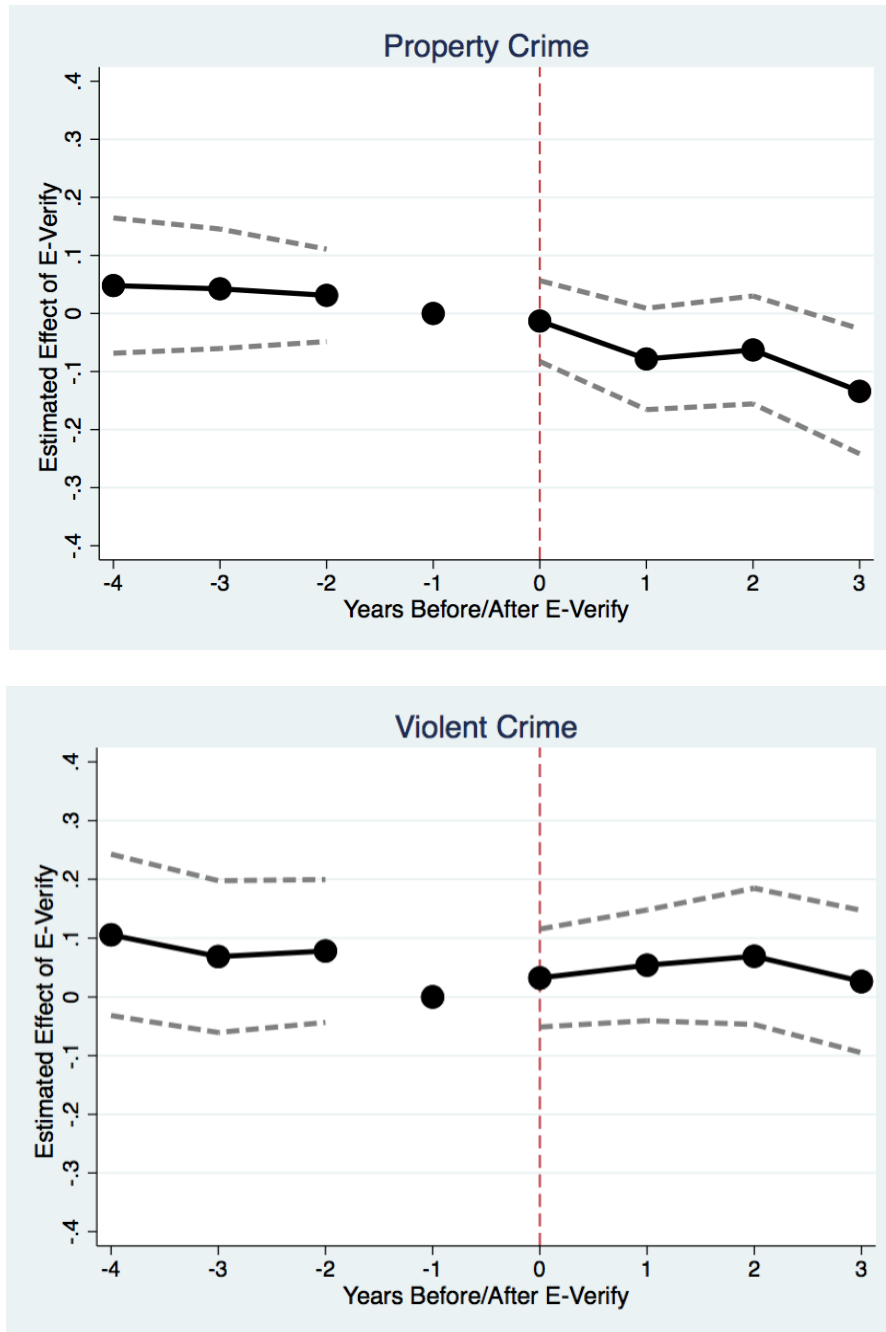


2015



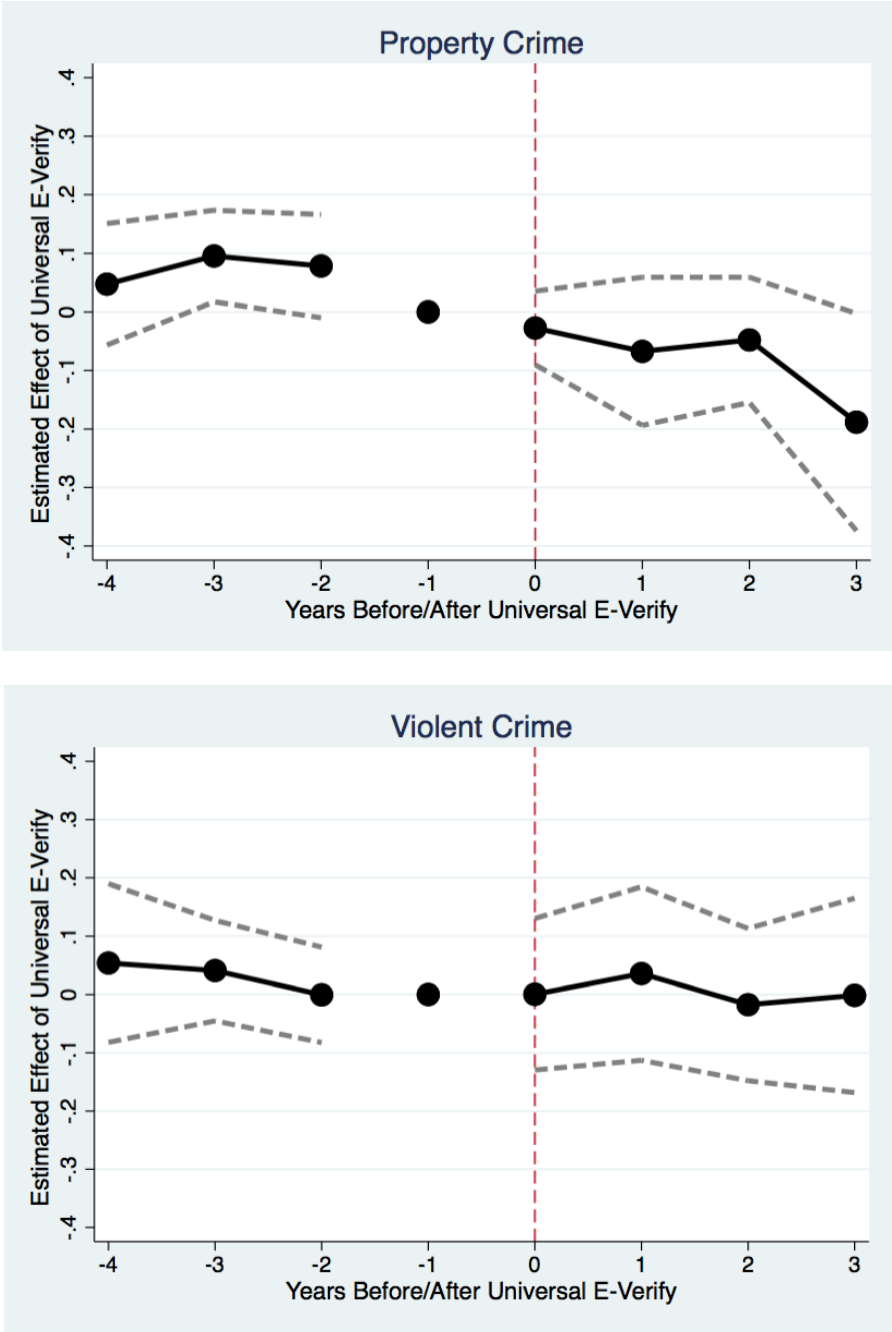
Sources: National Conference of State Legislatures (2015) and Urban Institute (2017).

Figure 2. Event Study Analysis of E-Verify Mandates and Criminal Incidents Involving Hispanic Arrestees, NIBRS 2004-2015



Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Dashed horizontal lines show the 95 percent confidence intervals and the red vertical line shows E-Verify enactment. Estimates control for controls listed in Appendix Table 2 and state and year fixed effects. Standard errors are clustered at the state level.

Figure 3. Event Study Analysis of Universal E-Verify Mandates and Criminal Incidents Involving Hispanic Arrestees, NIBRS 2004-2015



Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Dashed horizontal lines show the 95 percent confidence intervals and the red vertical line shows E-Verify enactment. Estimates control for controls listed in Appendix Table 2 and state and year fixed effects. Standard errors are clustered at the state level.

Figure 4. Event Study Analysis of E-Verify Mandates and Hispanic Mobility, CPS-ORG 2004-2015

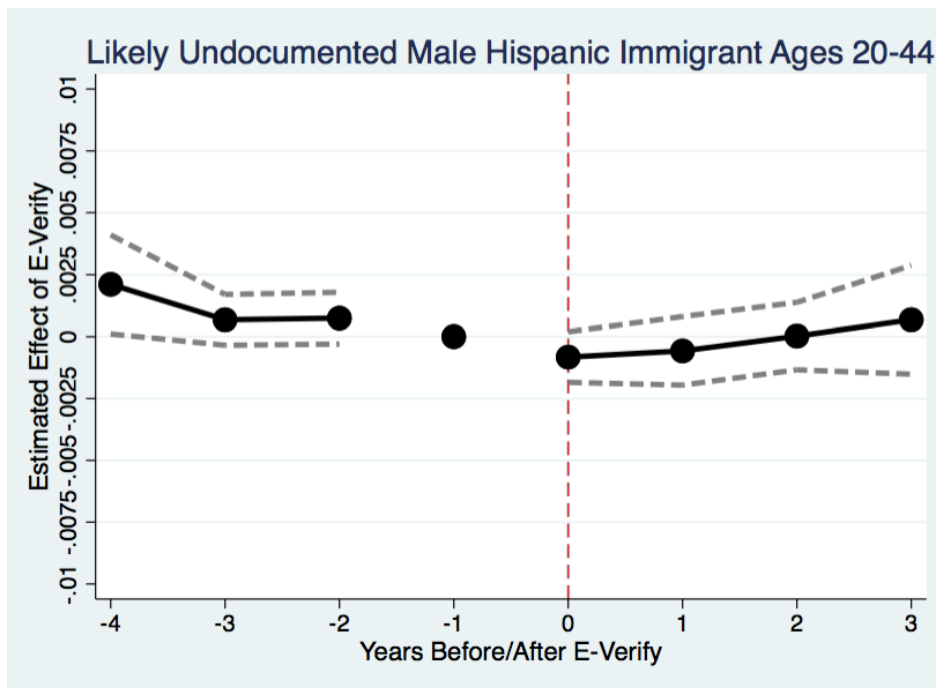
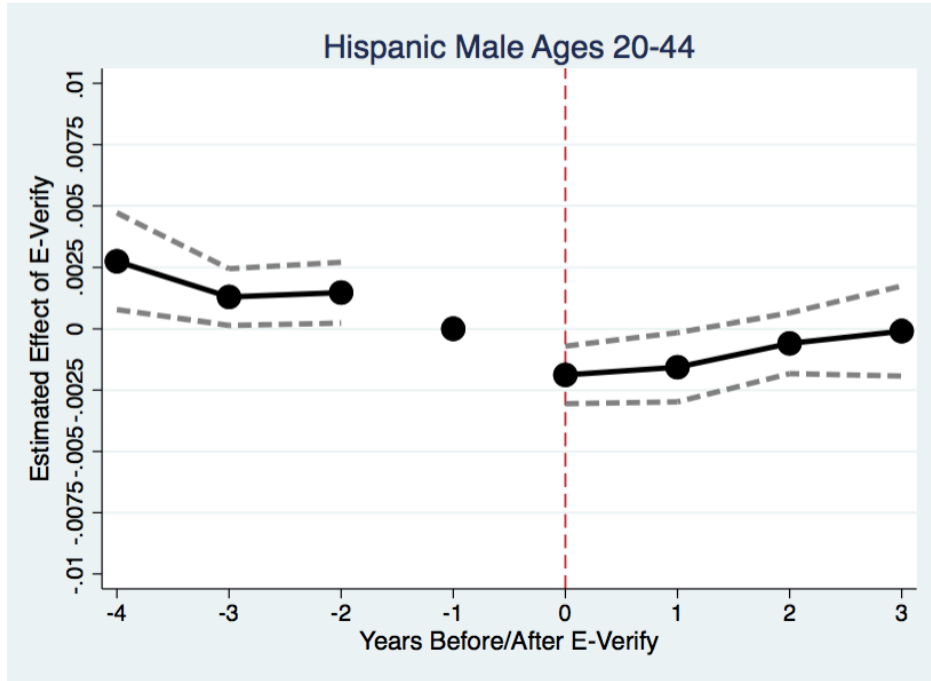
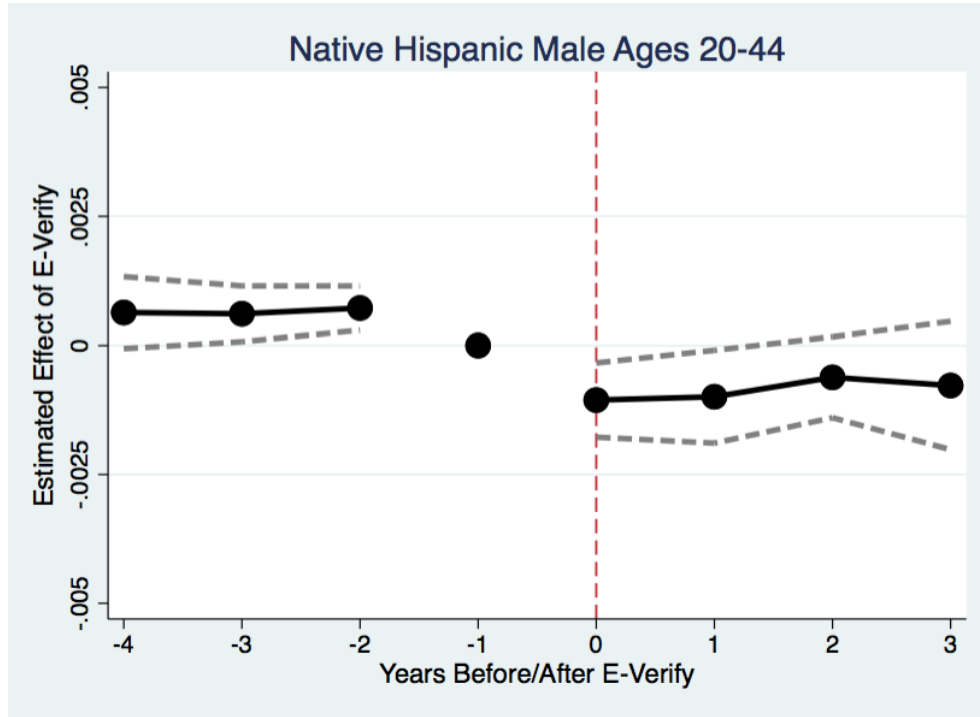
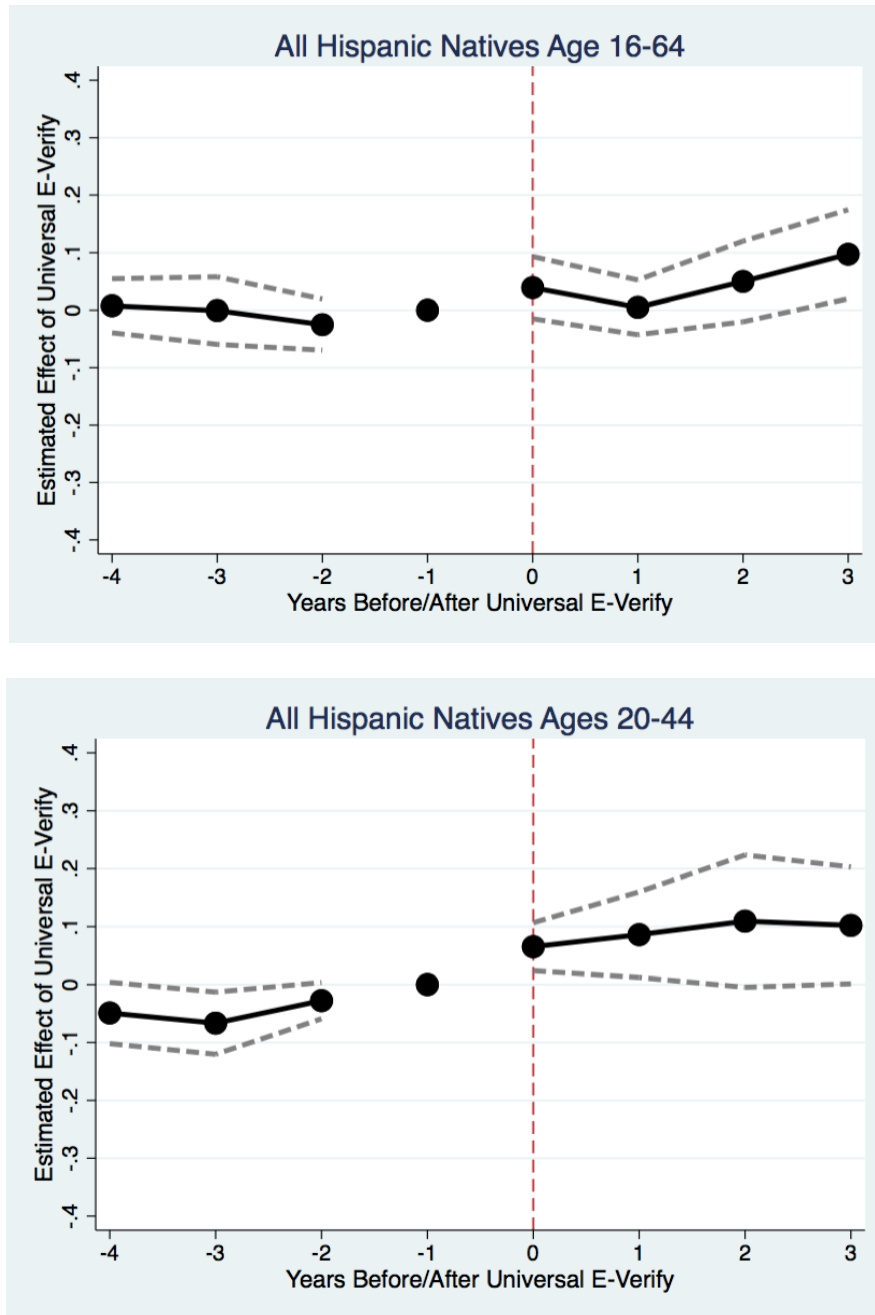


Figure 4 Cont.



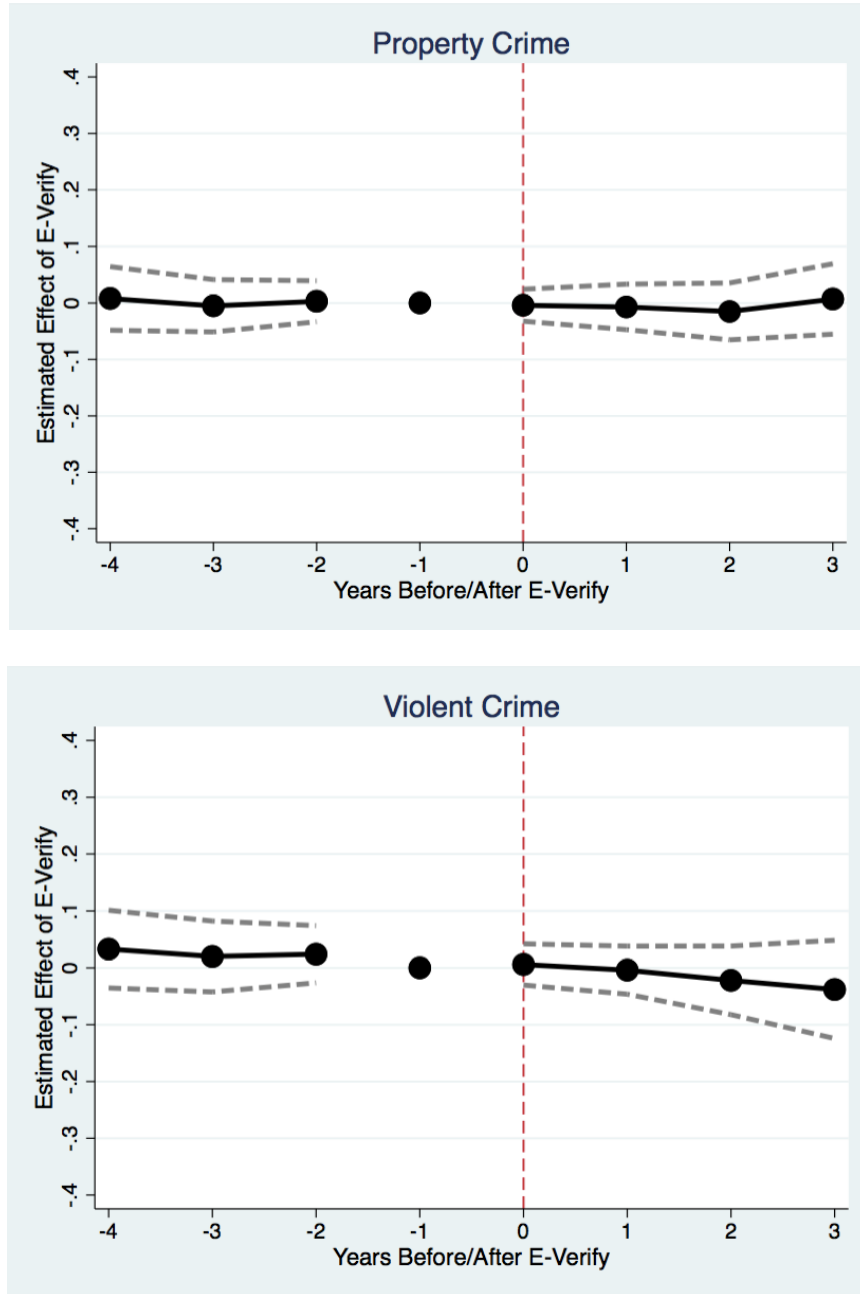
Notes: Weighted OLS estimates are generated using individual-level data drawn from the 2004 to 2015 Current Population Survey. We define likely undocumented immigrants as those who are less educated and non-citizen immigrants of Hispanic descent. Dashed horizontal lines show the 95 percent confidence intervals and the red vertical line shows E-Verify enactment. Estimates control for controls listed in Appendix Table 2 and state and year fixed effects. Standard errors are clustered at the state level.

Figure 5. Event Study Analysis of Universal E-Verify Mandates and Wage-and-Salary Employment for Hispanic Natives, CPS-ORG 2004-2015



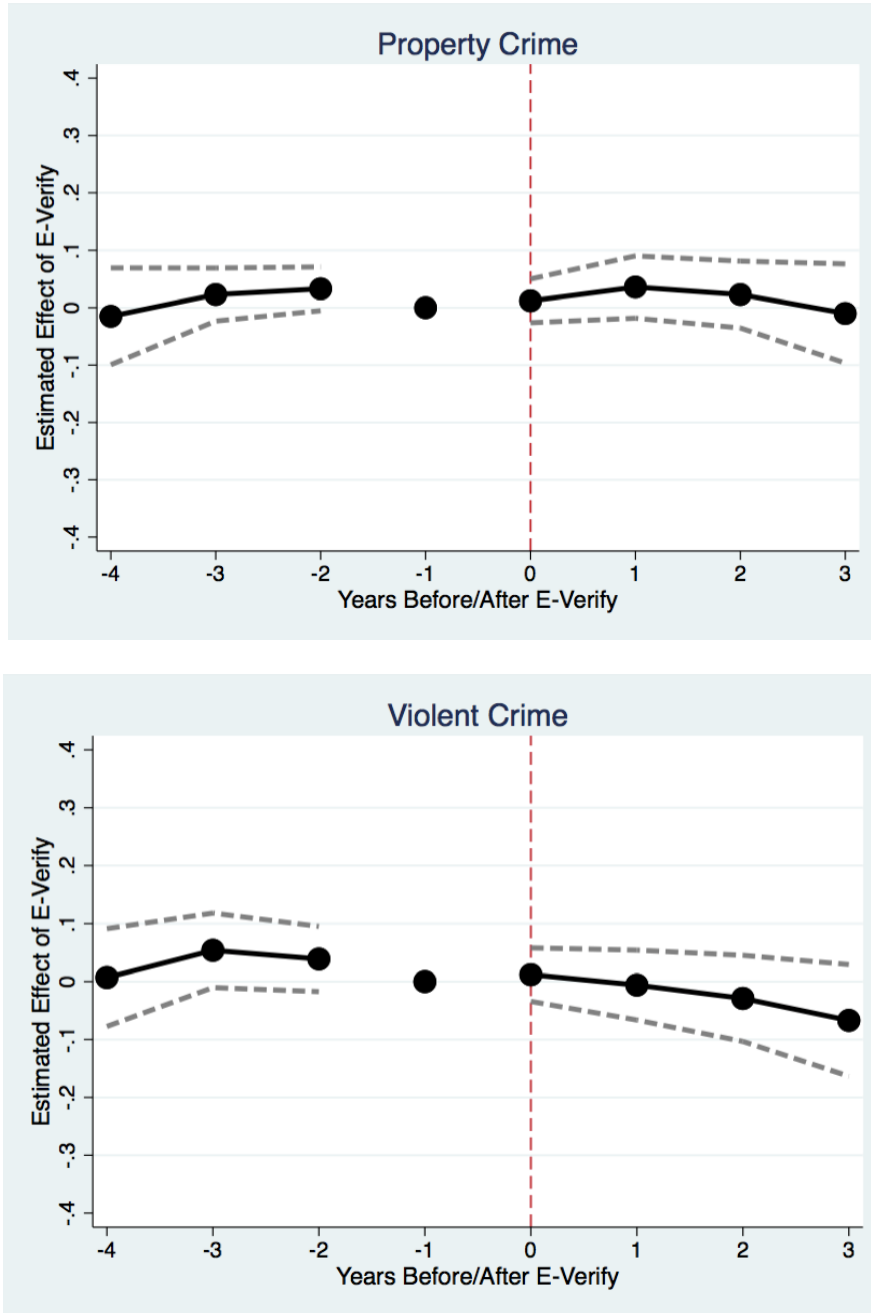
Notes: Weighted OLS estimates are generated using individual-level data drawn from the 2004 to 2015 Current Population Survey. Dashed horizontal lines show the 95 percent confidence intervals and the red vertical line shows E-Verify enactment. Estimates control for controls listed in Appendix Table 2 and state and year fixed effects. Standard errors are clustered at the state level.

Figure 6. Event Study Analysis of E-Verify Mandates and Arrests Involving White Arrestees, UCR, 2004-2015



Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 Uniform Crime Reports. Dashed horizontal lines show the 95 percent confidence intervals and the red vertical line shows E-Verify enactment. Estimates control for controls listed in Appendix Table 2 and state and year fixed effects. Standard errors are clustered at the state level.

Figure 7. Event Study Analysis of E-Verify Mandates and Arrests Involving African American Arrestees, UCR, 2004-2015



Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 Uniform Crime Reports. Dashed horizontal lines show the 95 percent confidence intervals and the red vertical line shows E-Verify enactment. Estimates control for controls listed in Appendix Table 2 and state and year fixed effects. Standard errors are clustered at the state level.

Table 1. Effective Dates of State E-Verify Laws, 2004-2015

State	Effective Date	Coverage	Identifying Variation?		
			NIBRS	UCR	CPS
Alabama	April 1, 2012	Universal	No	Yes	Yes
Arizona	December 31, 2007	Universal	No	Yes	Yes
Colorado	August 7, 2006	Public	Yes	Yes	Yes
Florida	January 4, 2007	Public	No	Yes	Yes
Georgia	July 1, 2007	Public	No	Yes	Yes
	January 1, 2012	Universal	No	Yes	Yes
Idaho	July 1, 2009	Public	Yes	Yes	Yes
Indiana	July 1, 2011	Public	No	Yes	Yes
Louisiana	August 18, 2011	Universal	Yes	Yes	Yes
Michigan	March 1, 2013	Public	Yes	Yes	Yes
Minnesota	January 1, 2008	Public	No	Yes	Yes
Mississippi	July 1, 2008	Universal	No	Yes	Yes
Missouri	January 1, 2009	Public	No	Yes	Yes
Nebraska	October 1, 2009	Public	Yes	Yes	Yes
North Carolina	January 1, 2007	Public	No	Yes	Yes
	October 1, 2012	Universal	No	Yes	Yes
Oklahoma	February 2, 2010	Public	No	Yes	Yes
Pennsylvania	January 1, 2013	Public	No	Yes	Yes
Rhode Island	October 17, 2008 ^a	Public	Yes	Yes	Yes
South Carolina	January 1, 2009	Public	Yes	Yes	Yes
	January 1, 2012	Universal	Yes	Yes	Yes
Tennessee	October 1, 2011	Universal	Yes	Yes	Yes
Texas	September 1, 2015	Public	Yes	Yes	Yes
Utah	July 1, 2009	Public	Yes	Yes	Yes
	July 1, 2010	Universal	Yes	Yes	Yes
Virginia	December 1, 2012	Public	Yes	Yes	Yes
West Virginia	June 24, 2012	Public	Yes	Yes	Yes

Sources: National Conference of State Legislatures (2015) and Urban Institute (2017).

^aThe E-Verify mandate enacted in Rhode Island in 2008 was repealed on January 5, 2011.

**Table 2. Estimated Effect of E-Verify on Crime Involving Hispanic Arrestees,
NIBRS 2004-2015**

	(1)	(2)	(3)	(4)	(5)	(6)
Property Crime	-0.118** (0.051)	-0.112** (0.048)	-0.123*** (0.047)	-0.108*** (0.042)	-0.077*** (0.030)	-0.071** (0.033)
N	255,744	255,744	255,744	255,744	255,744	255,744
Violent Crime	-0.043 (0.063)	-0.023 (0.053)	-0.022 (0.053)	0.010 (0.048)	0.019 (0.044)	-0.014 (0.048)
N	255,744	255,744	255,744	255,744	255,744	255,744
Agency FE?	Yes	Yes	Yes	Yes	Yes	Yes
Year-by-Month FE?	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls?	No	Yes	Yes	Yes	Yes	Yes
Political & Economic Controls?	No	No	Yes	Yes	Yes	Yes
Crime Policy Controls?	No	No	No	Yes	Yes	Yes
Immigration Policy Controls?	No	No	No	No	Yes	Yes
Social Policy Controls?	No	No	No	No	No	Yes

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects and year-by-month fixed effects. Demographic controls include the share of population ages 25 and older with a bachelor's degree, the share of county population ages 25-54 and ages 55 and older, the share of population that are male, and the share of population that are African American. Political and economic controls include the natural log of per capita income, the natural log of unemployment rates, and an indicator if the state governor is a Democrat. Crime policy controls include the natural logs of police expenditure per capita and police employment per capita and indicators for shall issue laws, stand your ground laws, and background check laws. Immigration policy controls include indicators for 287(g) programs, Secure Communities, and omnibus immigration bills. Social policy controls include the natural logs of minimum wages, refundable EITC rates, indicators for ban the box laws, SNAP vehicle exemptions and ACA Medicaid expansion. Standard errors are clustered at the state level.

Table 3. Lead and Lagged Effects of E-Verify on Criminal Incidents Involving Hispanic Arrestees, NIBRS 2004-2015

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Property Crime</i>			<i>Violent Crime</i>		
4+ Years Before	0.028 (0.060)	–	0.048 (0.059)	0.090 (0.070)	–	0.106 (0.070)
3 Years Before	0.029 (0.052)	–	0.045 (0.052)	0.055 (0.065)	–	0.069 (0.066)
2 Years Before	0.023 (0.040)	–	0.033 (0.041)	0.066 (0.062)	–	0.078 (0.062)
1 Year Before		–	–		–	–
E-Verify	-0.056* (0.032)			0.026 (0.046)		
Year of Law Change		-0.035 (0.035)	-0.011 (0.035)		-0.17 (0.041)	0.032 (0.042)
1 Year After		-0.099** (0.044)	-0.076* (0.044)		0.006 (0.057)	0.054 (0.048)
2 Years After		-0.083 (0.053)	-0.063 (0.047)		0.022 (0.065)	0.069 (0.059)
3+ Years After		-0.150*** (0.055)	-0.134** (0.055)		-0.012 (0.063)	0.026 (0.062)
χ^2 of $\Sigma(\beta_{leads})=0$ (p-value)	0.35 (0.55)		0.81 (0.37)	1.35 (0.24)		1.95 (0.16)
χ^2 of $\Sigma(\beta_{yr\ of\ change, lags})=0$ (p-value)		4.61 (0.03)	3.03 (0.08)		0.00 (0.99)	0.86 (0.35)
<i>N</i>	255,744	255,744	255,744	255,744	255,744	255,744

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Table 4. Robustness of Hispanic Crime Effects of E-Verify to Controls for State- and County-Level Time Trends

	(1)	(2)	(3)	(4)
Property Crime	-0.071** (0.032)	-0.090** (0.040)	-0.059** (0.029)	-0.057** (0.026)
N	255,744	255,744	255,744	255,744
Violent Crime	-0.004 (0.048)	-0.035 (0.062)	0.040 (0.032)	0.019 (0.035)
N	255,744	255,744	255,744	255,744
Agency FE	Yes	Yes	Yes	Yes
Year-by-Month FE	Yes	Yes	Yes	Yes
State-Specific Linear Time Trends	No	Yes	No	No
County-Specific Linear Time Trends	No	No	Yes	Yes
County-Specific Quadratic Time Trends	No	No	No	Yes

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, year-by-month fixed effects, linear time trends, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Table 5. Heterogeneity in Hispanic Crime Effects of E-Verify, NIBRS 2004-2015

	(1)	(2)
	Property Crime	Violent Crime
<i>Panel I: Age</i>		
Ages 16-19	0.037 (0.034)	0.069 (0.049)
Ages 20-24	-0.090** (0.035)	-0.050 (0.062)
Ages 25-34	-0.169*** (0.055)	-0.038 (0.054)
Ages 35-44	-0.089* (0.045)	-0.059 (0.048)
Ages 45-64	0.027 (0.036)	0.061 (0.091)
N	255,744	255,744
<i>Panel II: Gender</i>		
Men	-0.085** (0.037)	-0.008 (0.047)
N	255,744	255,744
Women	-0.049 (0.037)	0.049 (0.062)
N	255,744	255,744
<i>Panel III: Type of E-Verify Mandate</i>		
Public E-Verify	-0.047 (0.029)	0.009 (0.040)
Universal E-Verify	-0.145** (0.057)	0.022 (0.062)
N	255,744	255,744
<i>Panel IV: Controls for State-by-Year Share of Population that are Low-Skilled Immigrants & Natives</i>		
E-Verify	-0.080** (0.037)	-0.003 (0.051)
N	255,744	255,744

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Table 6. Examination of Detailed Criminal Incidents Involving Hispanic Arrestees, NIBRS 2004-2015

<i>Panel I: Property Crime</i>				
	Larceny	Burglary	Motor Vehicle Theft	Arson
E-Verify	-0.072** (0.034)	-0.017 (0.046)	-0.169* (0.097)	-0.065 (0.143)
N	255,744	255,744	255,744	255,744
<i>Panel II: Violent Crime</i>				
	Aggravated Assault	Murder	Rape	Robbery
E-Verify	0.005 (0.050)	-0.102 (0.130)	-0.097 (0.074)	-0.094 (0.070)
N	255,744	255,744	255,744	255,744
<i>Panel III: Other Crime</i>				
	Drug	Stolen Property	Weapon Law Violation	Sex Offenses
E-Verify	0.060 (0.040)	-0.292*** (0.062)	0.011 (0.053)	-0.007 (0.045)
N	255,744	255,744	255,744	255,744

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Table 7. Exploring Hispanic Crime Displacement in Jurisdictions Neighboring E-Verify States, NIBRS 2004-2015

	(1)	(2)	(3)	(4)
	Property Crime		Violent Crime	
<i>Panel I: Spillover to Border State</i>				
Border-State E-Verify	-0.051 (0.094)	-0.025 (0.034)	-0.038 (0.082)	-0.022 (0.033)
E-Verify		-0.074** (0.034)		-0.005 (0.048)
N	105,840	255,744	105,840	255,744
<i>Panel II: Spillover within Census Division</i>				
Census-Division E-Verify	0.025 (0.075)	0.015 (0.040)	-0.139 (0.113)	-0.016 (0.051)
E-Verify		-0.073** (0.033)		-0.002 (0.048)
N	105,840	255,744	105,840	255,744
Sample	Non-E-Verify	Pooled	Non-E-Verify	Pooled

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Table 8. Exploring Employment and Mobility Mechanisms, CPS 2004-2015

	(1)	(2)	(3)	(4)
	Hispanic Immigrants		Native Hispanics	
	Men	Women	Men	Women
<i>Panel I: Any Employment</i>				
16-64	-0.013*	-0.002	0.017	-0.018
	(0.007)	(0.018)	(0.014)	(0.014)
N	48,724	48,340	40,780	45,041
20-44	-0.012*	-0.009	0.004	-0.012
	(0.006)	(0.019)	(0.017)	(0.020)
N	34,617	33,809	21,656	24,099
<i>Panel II: Wage and Salary Employment</i>				
16-64	-0.016	-0.005	0.038***	-0.005
	(0.013)	(0.017)	(0.013)	(0.014)
N	48,724	48,340	40,780	45,041
20-44	-0.014	-0.010	0.030	-0.003
	(0.015)	(0.020)	(0.020)	(0.020)
N	34,617	33,809	21,656	24,099
<i>Panel III: Demographic Composition</i>				
16-64	-0.0009	-0.0006	-0.0019**	-0.0012
	(0.0007)	(0.0004)	(0.0008)	(0.0008)
N	3,810,661	3,810,661	3,810,661	3,810,661
20-44	-0.0009*	-0.0003	-0.0013***	-0.0006
	(0.0005)	(0.0004)	(0.0004)	(0.0004)
N	3,810,661	3,810,661	3,810,661	3,810,661

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Weighted OLS estimates are generated using individual-level data drawn from the 2004 to 2015 Current Population Survey Outgoing Rotation Groups. Each regression has controls for agency fixed effects, time (year and month) fixed effects, state-specific linear time trends, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Table 9. Exploring Heterogeneity in Employment and Mobility Effects, by Breadth of E-Verify Mandate, CPS 2004-2015

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hispanic Immigrants				Native Hispanic			
	Men		Women		Men		Women	
	16-64	20-44	16-64	20-44	16-64	20-44	16-64	20-44
<i>Panel I: Any Employment</i>								
Public E-Verify	-0.013*	-0.012*	-0.000	-0.007	0.015	0.002	-0.025	-0.019
	(0.007)	(0.006)	(0.018)	(0.019)	(0.014)	(0.018)	(0.015)	(0.021)
Universal E-Verify	-0.016	-0.007	-0.032	-0.054	0.054	0.024	0.080**	0.071
	(0.017)	(0.019)	(0.029)	(0.034)	(0.038)	(0.038)	(0.038)	(0.052)
N	48,724	34,617	48,340	33,809	40,780	21,656	45,041	24,099
<i>Panel II: Wage and Salary Employment</i>								
Public E-Verify	-0.016	-0.015	-0.004	-0.009	0.034**	0.026	-0.013	-0.011
	(0.013)	(0.015)	(0.017)	(0.020)	(0.013)	(0.020)	(0.015)	(0.021)
Universal E-Verify	-0.002	0.007	-0.014	-0.033	0.082**	0.088**	0.101**	0.092
	(0.024)	(0.028)	(0.035)	(0.040)	(0.039)	(0.043)	(0.041)	(0.057)
N	48,724	34,617	48,340	33,809	40,780	21,656	45,041	24,099
<i>Panel III: Demographic Composition</i>								
Public E-Verify	-0.0009	-0.0009*	-0.0006	-0.0003	-0.0019**	-0.0013***	-0.0011	-0.0006
	(0.0007)	(0.0005)	(0.0004)	(0.0004)	(0.0008)	(0.0003)	(0.0008)	(0.0004)
Universal E-Verify	-0.0006	-0.0003	-0.0008	-0.0011**	-0.0019*	-0.0012**	-0.0017*	-0.0002
	(0.0013)	(0.0009)	(0.0007)	(0.0005)	(0.0010)	(0.0005)	(0.0009)	(0.0004)
N	3,810,661	3,810,661	3,810,661	3,810,661	3,810,661	3,810,661	3,810,661	3,810,661

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Weighted OLS estimates are generated using individual-level data drawn from the 2004 to 2015 Current Population Survey Outgoing Rotation Groups. Each regression has controls for agency fixed effects, time (year and month) fixed effects, state-specific linear time trends, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Table 10. Estimated Effect of E-Verify on African American and White Crime, UCR 2004-2015

	African American		White	
	(1)	(2)	(3)	(4)
	<i>Property Crime</i>	<i>Violent Crime</i>	<i>Property Crime</i>	<i>Violent Crime</i>
<i>Panel I: Baseline Results</i>				
E-Verify	0.012 (0.027)	-0.027 (0.030)	0.001 (0.019)	-0.008 (0.026)
N	1,076,699	1,076,699	1,076,699	1,076,699
<i>Panel II: Event Study Analysis</i>				
4+ Years Before	-0.027 (0.044)	-0.010 (0.044)	-0.001 (0.027)	0.018 (0.034)
3 Years Before	0.017 (0.026)	0.037 (0.033)	-0.007 (0.023)	0.010 (0.030)
2 Years Before	0.029 (0.021)	0.030 (0.029)	-0.001 (0.019)	0.015 (0.025)
1 Year Before	-	-	-	-
Year of Law Change	0.013 (0.020)	0.014 (0.023)	-0.003 (0.013)	0.013 (0.018)
1 Year After	0.038 (0.030)	-0.001 (0.032)	-0.003 (0.021)	0.004 (0.021)
2 Years After	0.025 (0.031)	-0.029 (0.041)	-0.011 (0.025)	-0.011 (0.030)
3+ Years After	0.001 (0.047)	-0.058 (0.054)	0.020 (0.030)	-0.013 (0.041)
χ^2 of $\Sigma(\beta_{leads})=0$ (p-value)	0.05 (0.82)	0.37 (0.54)	0.02 (0.90)	0.30 (0.58)
χ^2 of $\Sigma(\beta_{yr\ of\ change, lags})=0$ (p-value)	0.42 (0.52)	0.59 (0.44)	0.01 (0.93)	0.05 (0.82)
N	1,076,699	1,076,699	1,076,699	1,076,699
<i>Panel III: Results by Type of E-Verify Laws</i>				
Public E-Verify	0.011 (0.030)	-0.028 (0.027)	0.002 (0.020)	-0.010 (0.019)
Universal E-Verify	0.008 (0.044)	0.007 (0.061)	-0.004 (0.039)	0.006 (0.060)
N	1,076,699	1,076,699	1,076,699	1,076,699

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 Uniform Crime Reports. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Appendix Table 1A. Descriptive Statistics of Dependent Variables, NIBRS 2004-2015

<i>Hispanic Property Crime (Ages 16-64)</i>	0.674 (3.095)
Ages 16-19	0.194 (0.980)
Ages 20-24	0.153 (0.785)
Ages 25-34	0.199 (0.100)
Ages 35-44	0.107 (0.602)
Ages 45-64	0.060 (0.409)
Male	0.453 (2.050)
Female	0.240 (1.300)
<i>Hispanic Violent Crime (Ages 16-64)</i>	0.238 (1.200)
Ages 16-19	0.044 (0.307)
Ages 20-24	0.061 (0.372)
Ages 25-34	0.085 (0.487)
Ages 35-44	0.041 (0.282)
Ages 45-64	0.02 (0.174)
Male	0.208 (1.051)
Female	0.035 (0.258)
<i>Non-Hispanic White Property Crime</i>	4.833 (11.665)
<i>Non-Hispanic White Violent Crime</i>	1.193 (2.954)
<i>African American Property Crime</i>	2.238 (11.279)
<i>African American Violent Crime</i>	0.986 (5.959)
N	255,744

Notes: Means of crime counts of incidents are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Standard deviations are in parentheses.

Appendix Table 1B. Descriptive Statistics of Dependent Variables, UCR 2004-2015

<i>White Property Crime Arrests</i>	6.988 (22.155)
<i>White Violent Crime Arrests</i>	2.356 (11.256)
<i>African American Property Crime Arrests</i>	2.693 (14.045)
<i>African American Violent Crime Arrests</i>	1.346 (9.842)
N	1,076,699

Notes: Means of crime counts of incidents are generated using agency-level data drawn from the 2004 to 2015 Uniform Crime Report. Standard deviations are in parentheses.

Appendix Table 2. Descriptive Statistics of Dependent & Control Variables, 2004-2015

	(1)	(2)	(3)
	NIBRS Sample	UCR Sample	CPS Sample
<i>County-Level Controls</i> ^a			
Share of Population Ages 25-54	0.396 (0.033)	0.398 (0.033)	0.412 (0.016)
Share of Population Ages 55+	0.274 (0.054)	0.270 (0.055)	0.249 (0.030)
Share of Male	0.493 (0.013)	0.495 (0.016)	0.483 (0.500)
Share of African American	0.105 (0.133)	0.091 (0.113)	0.136 (0.081)
287(g) Program	0.011 (0.105)	0.026 (0.16)	0.011 (0.028)
Secure Communities	0.381 (0.486)	0.391 (0.488)	0.405 (0.473)
Omnibus Immigration Bills	0.051 (0.220)	0.033 (0.179)	0.044 (0.204)
Ban the Box Laws	0.100 (.298)	0.113 (0.316)	0.133 (0.318)
<i>State-Level Controls</i>			
Share of Population w/ BA Degree	0.300 (0.062)	0.300 (0.051)	0.302 (0.051)
Per Capita Income	40,884.63 (12,030.03)	38,897.22 (12208.28)	41,146.31 (6,895.23)
Unemployment Rates	0.064 (0.022)	0.064 (0.020)	0.067 (0.022)
Democrat State Governor	0.520 (0.500)	0.490 (0.500)	0.453 (0.498)
Police Expenditure per Capita	266.75 (37.16)	273.09 (72.72)	298.05 (87.03)
Police Employment per Capita	2.216 (0.424)	2.258 (0.562)	2.328 (0.635)
Shall Issue Laws	0.841 (0.366)	0.735 (0.441)	0.661 (0.474)
Stand Your Ground Laws	0.441 (0.485)	0.358 (0.469)	0.401 (0.480)
Background Checks per 100,000	5,108.99 (3,093.06)	4669.866 (3079.461)	4,849.951 (5,594.058)
Minimum Wages	7.396 (0.782)	6.864 (1.009)	6.999 (1.016)
Refundable EITC rates	0.044 (0.070)	0.073 (0.118)	0.062 (0.109)
SNAP One Vehicle Exempted	0.183 (0.378)	0.176 (0.370)	0.142 (0.341)
SNAP All Vehicles Exempted	0.769 (0.413)	0.684 (0.456)	0.733 (0.426)
ACA Medicaid Expansion	0.086 (0.275)	0.084 (0.275)	0.099 (0.297)
N	255,744	1,076,699	3,810,661

Notes: Standard deviations are in parentheses.

^a For CPS samples, the county-level controls are weighted means aggregated to the state level.

Appendix Table 3. Descriptive Statistics of Dependent Variables, CPS 2004-2015

	(1)	(2)	(3)	(4)
	Hispanic Immigrants		Native Hispanic	
	Men	Women	Men	Women
<i>Panel I: Any Employment</i>				
16-64	0.848 (0.359)	0.448 (0.497)	0.613 (0.487)	0.478 (0.500)
N	48,724	48,340	40,780	45,041
20-44	0.892 (0.311)	0.445 (0.497)	0.749 (0.434)	0.568 (0.495)
N	34,617	33,809	21,656	24,099
<i>Panel II: Wage and Salary Employment</i>				
16-64	0.764 (0.425)	0.416 (0.493)	0.564 (0.496)	0.458 (0.498)
N	48,724	48,340	40,780	45,041
20-44	0.808 (0.394)	0.416 (0.493)	0.695 (0.460)	0.548 (0.498)
N	34,617	33,809	21,656	24,099
<i>Panel III: Demographic Composition</i>				
16-64	0.022 (0.146)	0.017 (0.129)	0.019 (0.136)	0.017 (0.131)
N	3,810,661	3,810,661	3,810,661	3,810,661
20-44	0.016 (0.125)	0.011 (0.106)	0.010 (0.100)	0.009 (0.093)
N	3,810,661	3,810,661	3,810,661	3,810,661

Notes: Means of employment and demographic composition are generated using Current Population Survey Outgoing Rotation Groups. Standard deviations are in parentheses.

Appendix Table 4. Comparison of Poisson to Negative Binomial Hispanic Crime Regressions, NIBRS, 2004-2015

	(1)	(2)
Property Crime	-0.071** (0.033)	-0.067** (0.034)
N	255,744	255,744
Violent Crime	-0.014 (0.048)	-0.040 (0.047)
N	255,744	255,744
Model	Poisson	Negative Binomial

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson or Negative Binominal estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Appendix Table 5. Estimated Coefficients on Control Variables for Crime Regressions, NIBRS and UCR 2004-2015

	(1)	(2)	(3)	(4)	(5)	(6)
	Hispanics		African American		Non-Hispanic White	
	Property Crime	Violent Crime	Property Crime	Violent Crime	Property Crime	Violent Crime
Share of Population Ages 25-54	-0.367 (3.179)	1.908 (3.190)	-1.786 (1.346)	3.375* (2.051)	-3.924*** (1.341)	0.183 (1.780)
Share of Population Ages 55+	0.878 (3.288)	-1.131 (2.410)	1.560 (1.434)	0.231 (1.804)	0.009 (0.900)	-0.873 (1.292)
Share of Male	14.756 (15.656)	-6.303 (18.308)	3.061 (2.913)	-10.252 (6.765)	12.623*** (3.089)	0.663 (5.354)
Share of African American	-5.383 (3.617)	6.576** (3.345)	3.823*** (1.148)	4.312*** (1.392)	1.314 (1.053)	-0.143 (1.412)
Share of Population w/ BA Degree	0.387 (0.748)	0.073 (1.453)	1.239* (0.667)	0.593 (0.791)	0.657 (0.439)	0.233 (0.431)
Ln(Per Capita Income)	-0.086 (0.301)	-0.093 (0.283)	0.089 (0.120)	-0.254 (0.189)	0.069 (0.134)	-0.149 (0.121)
Ln(Unemployment Rates)	0.089 (0.095)	0.003 (0.148)	0.168* (0.090)	0.089 (0.097)	-0.081 (0.080)	-0.051 (0.081)
Democrat State Governor	-0.031 (0.038)	-0.001 (0.020)	-0.035 (0.025)	-0.037 (0.029)	-0.044** (0.019)	-0.018 (0.020)
Ln(Police Expenditure per Capita)	-0.150 (0.321)	-0.050 (0.460)	-0.365** (0.183)	0.264 (0.287)	-0.076 (0.143)	0.111 (0.170)
Ln(Police Employment per Capita)	0.137 (0.118)	-0.470** (0.198)	0.058 (0.093)	-0.175** (0.084)	0.029 (0.090)	-0.080 (0.081)
Shall Issue Laws	0.089 (0.167)	0.192* (0.100)	0.135*** (0.046)	0.226*** (0.038)	-0.031 (0.025)	0.102 (0.064)
Stand Your Ground Laws	-0.027 (0.052)	-0.007 (0.059)	0.021 (0.043)	-0.072** (0.036)	0.062** (0.027)	-0.034 (0.031)
Background Check per 100,000	-0.003 (0.030)	0.037 (0.031)	-0.000 (0.000)	0.000 (0.000)	0.000* (0.000)	0.000 (0.000)
287(g) Program	-0.045	-0.036	-0.002	-0.021	-0.005	-0.021

	(0.050)	(0.065)	(0.026)	(0.056)	(0.018)	(0.032)
Secure Communities	0.039	-0.010	0.018	0.010	0.004	0.012
	(0.034)	(0.031)	(0.014)	(0.018)	(0.014)	(0.013)
Omnibus Immigration Bills	-0.286***	-0.207***	0.004	-0.142***	0.019	-0.049
	(0.072)	(0.078)	(0.047)	(0.042)	(0.053)	(0.041)
Ln(Minimum Wages)	0.176	0.166	0.182	0.140	0.242*	0.239*
	(0.249)	(0.392)	(0.122)	(0.153)	(0.144)	(0.137)
Refundable EITC Rates	0.233	-0.266	-0.127	-0.267	0.342	-0.452
	(0.307)	(0.375)	(0.175)	(0.230)	(0.226)	(0.282)
SNAP One Vehicle Exempted	-0.069	-0.101*	0.018	0.001	-0.051*	-0.001
	(0.065)	(0.058)	(0.043)	(0.053)	(0.029)	(0.044)
SNAP All Vehicles Exempted	0.078	0.045	0.050	-0.033	0.013	-0.009
	(0.066)	(0.068)	(0.044)	(0.046)	(0.027)	(0.038)
ACA Medicaid Expansion	-0.026	-0.086	0.038	0.035	-0.035	0.029
	(0.059)	(0.066)	(0.035)	(0.028)	(0.025)	(0.031)
N	255,744	255,744	1,076,699	1,076,699	1,076,699	1,076,699

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects and year-by-month fixed effects. Standard errors are clustered at the state level.

Appendix Table 6. Robustness of Event Study Analysis to Dropping Colorado and Ensuring All Leads are Identified by Same States, NIBRS, 2004-2015

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Property Crime</i>			<i>Violent Crime</i>		
3+ Years Before	0.076 (0.060)	–	0.085 (0.060)	0.074 (0.065)	–	0.102 (0.071)
2 Years Before	0.058 (0.048)	–	0.065 (0.048)	0.082 (0.062)	–	0.104 (0.065)
1 Year Before		–	–		–	–
E-Verify	-0.037 (0.033)			-0.018 (0.046)		
Year of Law Change		-0.034 (0.041)	0.008 (0.037)		-0.027 (0.050)	0.055 (0.038)
1 Year After		-0.094** (0.043)	-0.054 (0.040)		-0.024 (0.058)	0.055 (0.039)
2 Years After		-0.096* (0.057)	-0.055 (0.047)		-0.084 (0.077)	-0.008 (0.055)
3+ Years After		-0.128*** (0.052)	-0.095* (0.056)		-0.140 (0.124)	-0.068 (0.091)
χ^2 of $\Sigma(\beta_{leads})=0$ (p-value)	1.88 (0.17)		2.40 (0.12)	1.73 (0.19)		2.58 (0.11)
χ^2 of $\Sigma(\beta_{yr\ of\ change, lags})=0$ (p-value)		4.25 (0.04)	1.51 (0.22)		0.94 (0.33)	0.04 (0.84)
<i>N</i>	248,832	248,832	248,832	248,832	248,832	248,832

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Appendix Table 7. Sensitivity of Hispanic Crime Effects of E-Verify to Dropping Each Treatment State, NIBRS, 2004-2015

	(1)	(2)
	Property Crime	Violent Crime
<i>Treatment State Dropped</i>		
Colorado	-0.073* (0.038)	-0.043 (0.046)
N	248,858	248,858
Idaho	-0.075** (0.036)	-0.014 (0.054)
N	247,994	247,994
Louisiana	-0.071** (0.034)	-0.012 (0.048)
N	254,906	254,906
Michigan	-0.047* (0.028)	0.006 (0.045)
N	217,322	217,322
Nebraska	-0.070** (0.034)	-0.011 (0.049)
N	251,162	251,162
Rhode Island	-0.086** (0.034)	-0.021 (0.052)
N	255,194	255,194
South Carolina	-0.083** (0.033)	-0.017 (0.051)
N	234,602	234,602
Tennessee	-0.076* (0.039)	-0.053 (0.041)
N	222,218	222,218
Texas	-0.085** (0.035)	-0.013 (0.052)
N	252,602	252,602
Utah	-0.053 (0.035)	-0.017 (0.052)
N	250,298	250,298
Virginia	-0.054 (0.043)	-0.009 (0.054)
N	232,298	232,298
West Virginia	-0.073** (0.034)	-0.013 (0.048)
N	251,738	251,738

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Appendix Table 8. Robustness of Hispanic Crime Effect to Control for Region-Specific Year Effect, NIBRS, 2004-2015

	(1)	(2)
	Property Crime	Violent Crime
Property Crime	-0.080** (0.035)	0.005 (0.032)
N	255,600	255,600

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, year-by-month fixed effects, region-by-year fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Appendix Table 9. Examining Whether Hispanic Crime Effects Confounded by Affordable Care Act Dependent Coverage Mandate (DCM), NIBRS 2004-2015

	(1)	(2)
	Property Crime	Violent Crime
Ages 19-25	-0.080** (0.031)	-0.038 (0.053)
Ages 26-44	-0.145*** (0.047)	-0.058 (0.051)
Ages 45-64	0.027 (0.036)	0.061 (0.091)
N	255,744	255,744

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Appendix Table 10. Sensitivity of Labor Market Effects of E-Verify to Omitting State-Specific Linear Time Trends as Controls

	(1)	(2)	(3)	(4)
	Hispanic Immigrants		Native Hispanic	
	Men	Women	Men	Women
<i>Panel I: Any Employment</i>				
16-64	-0.007 (0.008)	0.002 (0.015)	0.026** (0.009)	-0.018 (0.012)
N	48,724	48,340	40,780	45,041
20-44	-0.006 (0.007)	-0.007 (0.014)	0.019* (0.010)	-0.005 (0.019)
N	34,617	33,809	21,656	24,099
<i>Panel II: Wage and Salary Employment</i>				
16-64	-0.005 (0.012)	-0.001 (0.015)	0.041*** (0.010)	-0.010 (0.013)
N	48,724	48,340	40,780	45,041
20-44	-0.001 (0.015)	-0.008 (0.014)	0.041*** (0.014)	0.000 (0.020)
N	34,617	33,809	21,656	24,099
<i>Panel III: Demographic Composition</i>				
16-64	-0.0001 (0.0009)	-0.0000 (0.0004)	-0.0012* (0.0006)	0.0001 (0.0005)
N	3,810,661	3,810,661	3,810,661	3,810,661
20-44	-0.0001 (0.0009)	0.0004 (0.0004)	-0.0007** (0.0003)	0.0002 (0.0003)
N	3,810,661	3,810,661	3,810,661	3,810,661

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Weighted OLS estimates are generated using individual-level data drawn from the 2004 to 2015 Current Population Survey Outgoing Rotation Groups. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standards errors are clustered at the state level.

Appendix Table 11. Sensitivity of UCR-Based Results to Restricting Sample to NIBRS Treatment and Control States from Table 2

	African American		White	
	(1)	(2)	(3)	(4)
	<i>Property Crime</i>	<i>Violent Crime</i>	<i>Property Crime</i>	<i>Violent Crime</i>
<i>Panel I: Baseline Results</i>				
E-Verify	-0.010 (0.022)	-0.026 (0.020)	-0.010 (0.026)	-0.003 (0.026)
N	799,948	799,948	799,948	799,948
<i>Panel II: Event Study Analysis</i>				
4+ Years Before	0.019 (0.038)	0.049 (0.044)	0.020 (0.027)	0.049 (0.036)
3 Years Before	0.022 (0.030)	0.069* (0.038)	0.005 (0.027)	0.018 (0.034)
2 Years Before	0.035 (0.022)	0.043 (0.032)	0.005 (0.024)	0.018 (0.028)
1 Year Before	-	-	-	-
Year of Law Change	0.009 (0.020)	0.031 (0.026)	-0.009 (0.014)	0.019 (0.019)
1 Year After	0.005 (0.030)	0.012 (0.034)	-0.033 (0.022)	0.006 (0.022)
2 Years After	0.009 (0.030)	0.015 (0.040)	-0.039 (0.027)	-0.001 (0.031)
3+ Years After	-0.025 (0.043)	0.004 (0.057)	-0.017 (0.036)	0.014 (0.041)
χ^2 of $\Sigma(\beta_{leads})=0$ (p-value)	0.81 (0.37)	2.26 (0.13)	0.17 (0.68)	0.84 (0.36)
χ^2 of $\Sigma(\beta_{yr\ of\ change,lags})=0$ (p-value)	0.01 (0.91)	0.07 (0.79)	1.23 (0.27)	0.04 (0.83)
N	799,948	799,948	799,948	799,948

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 Uniform Crime Reports. Samples are restricted to treatment and control states from Table 2. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Appendix Table 12. Estimated Effect of E-Verify on Criminal Incidents Involving African American and Non-Hispanic White Arrestees, NIBRS 2004-2015

	(1)	(2)	(3)	(4)
	African American		Non-Hispanic White	
	<i>Property Crime</i>	<i>Violent Crime</i>	<i>Property Crime</i>	<i>Violent Crime</i>
E-Verify	-0.039	-0.026	-0.081	-0.007
	(0.075)	(0.079)	(0.052)	(0.051)
N	255,744	255,744	255,744	255,744

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Appendix Table 13. Exploring Employment and Mobility Effects of E-Verify for Low-Skilled African Americans and Non-Hispanic Whites, CPS 2004-2015

	(1)	(2)	(3)	(4)
	African American		Non-Hispanic White	
	Men	Women	Men	Women
<i>Panel I: Any Employment</i>				
16-64	-0.001 (0.006)	-0.004 (0.005)	0.001 (0.003)	0.001 (0.003)
N	100,555	140,190	856,650	932,382
20-44	-0.010 (0.008)	0.002 (0.007)	0.002 (0.004)	0.001 (0.003)
N	49,035	75,667	386,859	442,517
<i>Panel II: Wage and Salary Employment</i>				
16-64	0.002 (0.006)	-0.001 (0.006)	0.002 (0.004)	0.000 (0.004)
N	100,555	140,190	856,650	932,382
20-44	-0.002 (0.009)	0.003 (0.008)	0.005 (0.004)	-0.000 (0.004)
N	49,035	75,667	386,859	442,517
<i>Panel III: Demographic Composition</i>				
16-64	0.0001 (0.0004)	0.0000 (0.0005)	0.0018 (0.0013)	0.0011 (0.0013)
N	3,810,661	3,810,661	3,810,661	3,810,661
20-44	0.0002 (0.0003)	0.0002 (0.0004)	0.0014** (0.0006)	0.0023** (0.0010)
N	3,810,661	3,810,661	3,810,661	3,810,661

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Weighted OLS estimates are generated using data drawn from the 2004 to 2015 Current Population Survey Outgoing Rotation Groups. Each regression has controls for agency fixed effects, time (year and month) fixed effects, state-specific linear time trends, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.

Appendix Table 14. Exploring African American and White Crime Displacement in Jurisdictions Neighboring E-Verify States, NIBRS 2004-2015

	(1)	(2)	(3)	(4)
	Property Crime		Violent Crime	
	<i>African American</i>	<i>White</i>	<i>African American</i>	<i>White</i>
<i>Panel I: Spillover to Border State</i>				
Border-State E-Verify	-0.033 (0.023)	-0.027* (0.014)	-0.029 (0.023)	-0.025 (0.016)
E-Verify	0.013 (0.027)	0.001 (0.019)	-0.025 (0.029)	-0.008 (0.025)
N	1,076,699	1,076,699	1,076,699	1,076,699
<i>Panel II: Spillover within Census Division</i>				
Census-Division E-Verify	0.009 (0.022)	0.025 (0.026)	-0.014 (0.028)	0.012 (0.024)
E-Verify	0.010 (0.028)	-0.004 (0.019)	-0.023 (0.032)	-0.011 (0.029)
N	1,076,699	1,076,699	1,076,699	1,076,699

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2015 Uniform Crime Reports. Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Appendix Table 2. Standard errors are clustered at the state level.