Immigration Enforcement, Crime and Demography: Evidence from the Legal Arizona Workers Act *

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Abstract

This study leverages a remarkable natural experiment created by recent legislation in Arizona to study the impact on crime of a large decline in the state's Mexican immigrant population. We show that this population decreased by as much as 20 percent in the wake of the passage of a broad-based "E-Verify" law requiring employers to verify the immigration status of new employees. In contrast to previous literature, we find evidence that property crimes declined in response to labor-market based immigration enforcement. However, these results are driven by a remarkable decline in the share of the immigrant population that is young and male and, as such, the effects are purely compositional. Thus this research serves as reminder that the effects of immigration enforcement are mediated substantially by demographic factors.
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I. Introduction

Over the past thirty years, crime rates in cities across the United States have plummeted, in many cases, reaching fifty-year lows (Zimring 2006). At the same time, the share of the foreign born among the U.S. population has increased rapidly, with the foreign-born Mexican share of the population quadrupling since 1980. Research in both economics and criminology suggests that immigration has either played no role in this historic decline in crime (Butcher and Piehl 1998b, Reid, Adelman, Weiss and Jaret 2005, Chalfin 2014) or has contributed importantly to the decline in crime (Butcher and Piehl 1998a; Stowell, Messner, McKeever and Raffalovich 2009; Ousey and Kubrin 2009; Martinez, Stowell and Lee 2010; Wadsworth 2010; MacDonald, Hipp and Gill 2012). An exception is Spenkuch (2014) which reports mixed evidence on this point, finding that Mexican immigration tends to increase property crimes while other immigration tends to decrease property crimes. While the majority of the literature supports the view that increases in immigration may have had a protective effect on crime, public opinion has generally reached the opposite conclusion, with a majority of U.S. natives indicating a belief that immigration leads to increases in criminal activity (Muste 2012; Pew 2015).[1] Public opinion is bolstered by various analyses of administrative data from the U.S. Department of Homeland Security which has attempted to identify undocumented immigrants in state prisons and has led to a proliferation of anecdotal findings that suggest that undocumented immigrants are overrepresented (albeit to a relatively small degree) among state prisoners (Camarota and Vaughan 2009).[2]

Though most recent empirical work is consistent with patterns in the aggregate time series, the literature remains unsatisfying in several ways. First, the available literature rarely disaggregates the effects of immigration on crime by nationality. Notably, there is little research that addresses

[1] Muste (2012) reviews twenty years of public opinion data on immigration. According to GSS data, in 1996, 32 percent of American natives believed that immigrants increased crime rates. In 2004, 25 percent of Americans indicated such a belief. Gallup polls indicate stronger beliefs with regard to immigrant criminality. In June 2001, 50 percent of respondents indicated a belief that immigration "made the crime problem worse." In June 2007, 58 percent of Americans indicated such a belief.

[2] Butcher and Piehl (2005) provide a more systematic analysis for California and reach the opposite conclusion. However, a potential shortcoming of this research is that the nationality of prisoners in California is typically based on self-reports. With respect to analyses of federal data on state prisoners in Camarota and Vaughan (2009), there are several issues. First, data from the State Criminal Alien Reporting Program are available for only a few states and so may not be nationally-representative. Second, to the extent that the Department of Homeland Security does not accurately measure undocumented immigrants in the community, these estimates may overstate the share of undocumented immigrants in state prisons.

the criminal participation of recent and, in particular, undocumented Mexican immigrants which is precisely the population that has become the focal point of recent policy debates.[3] While data limitations make the effects of undocumented immigration particularly difficult to address, this research leverages an unusual and uniquely policy-relevant legislative shock which we argue is plausibly concentrated largely among undocumented individuals rather than legal residents of Mexican origin.

Second, as the literature has been hampered by serious limitations in the availability of data, prior work has been unable to address two critical concerns. The first issue is that immigrants may be less likely than natives to report crimes to the police conditional upon being victimized (Tonry 1997; MacDonald and Saunders 2012).[4] A second issue is that prior research has been unable to distinguish between crimes committed *by* immigrants and crimes committed *against* immigrants. This is particularly concerning as some ethnographic evidence suggests that immigrants may be especially attractive victims for native offenders (Bucher, Manasse and Tarasawa 2010). To address the first issue we provide an auxiliary analysis of recently available data from the U.S. National Crime Victimization Survey (NCVS). This analysis suggests that crime reporting rates are surprisingly insensitive to changes in immigration flows. To address the second issue, we appeal to descriptive data that are inconsistent with the idea that natives' offending declined in response to a negative shock to the immigrant population share.

Third, the available estimates of the effect of immigration on crime can only be ascribed a causal interpretation under stringent assumptions regarding the inability of immigrants to adjust the timing and destination of migration in response to conditions in U.S. destinations. To the extent that migrants endogenously select where they migrate, a prospect which has been well-documented by Cadena and Kovack (2013) among others, regression estimates that use natural variation in city or county immigrant flows will return an inconsistent estimate of the effect of immigration on crime.

[3]As Mexican immigrants comprise over one third of all immigrants to the United States and over half of all undocumented immigrants, assessing the effect of Mexican immigration on crime would appear to be particularly relevant. Spenkuch (2014) and Chalfin (2014) offer the first analyses that disaggregate the effect of immigration on crime by Mexican nationality. Using rainfall patterns in Mexico as an instrumental variable, Chalfin(2014) studies the effect of immigration on crime at the MSA level and finds no consistent effect of Mexican immigration on any type of crime while Spenkuch (2014), using county level data, finds an effect of Mexican immigration on acquisitive crimes.

[4]Discussion of reporting bias can be traced back at least as far as the 1931 report on the National Committee on Law Observation and Enforcement, also called the "Wickersham Commission."

The vast majority of the prior literature does little to address these concerns (Butcher and Piehl 1998b; Spenkuch 2014; Chalfin 2014). The research that does so uses an instrumental variables strategy pioneered by Altonji and Card (1991) in which the historic distribution of source region-specific immigration among counties, cities or neighborhoods is used to predict the current spatial concentration of immigrants — and instrument known as the "network instrument."[5] While this network instrument is likely an improvement upon least squares estimates, several authors point out that resulting estimates will be biased in the presence of persistent pull factors that attract or repel immigrants to U.S. destinations (Pugatch and Yang 2011; Chalfin 2014, Chalfin 2015). Chalfin (2015), in particular, provides an explicit decomposition of the network instrument showing that while the instrument may solve the problem of endogenous location choice among immigrants, it does not solve the problem of immigrants choosing *when* they will migrate to a network-linked U.S. destination, a proposition which violates the instrument's exclusion restriction.

A final challenge in the prior literature and one which has not received sufficient attention is that, given the enormous heterogeneity among immigrants, instrumental variables estimates of the effect of immigration on crime are difficult to interpret. In particular, estimates of the effect of immigration on crime will generate widely different local average treatment effects depending on which types of immigrants are most likely to "comply" with a particular instrumental variable. It is plausible to believe, for example, that migrants who leave Mexico in response to rainfall shocks (Chalfin 2014) are different than those who choose when to leave Mexico and who migrate to join family members or friends who have already settled in the United States (Spenkuch 2014). To the extent that the compliers differ by age and gender, resulting estimates could vary widely. There are then many different causal effects of Mexican immigration on crime. Compounding this fundamental interpretation problem, prior estimates are also of questionable policy relevance as instrumental variables conceived of in the literature do not mirror actual policy options that might be employed to change the number of migrants living in the United States. Hence these estimates are not useful in projecting the effect of a change in immigration enforcement on crime.

[5]Using data from the 1980s, Butcher and Piehl (1998b) present estimates using 45 U.S. MSAs and find no evidence of an effect of immigration on crime. On the other hand, Spenkuch (2014), using more recent data at the county level, finds large effects of immigration on property crime, an effect which is even larger for Mexican immigrants. A recent paper by MacDonald, Hipp and Gill (2012) presents results at the neighborhood level using data from 200-2005 in Los Angeles and finds that higher immigrant population shares predict a decline in crime.

Because it is difficult to identify exogenous variation in immigrant flows and because IV estimates are difficult to interpret as policy relevant quantities, there is promise in searching for a natural experiment — in particular, one that is rooted in a realistic policy option for manipulating the stock of undocumented immigrants in the United States. In the spirit of Card's seminal 1990 research on the labor market impacts of the Mariel Boatlift on Miami, we leverage a remarkable natural experiment created by recent immigration enforcement legislation in Arizona to estimate the impact of an extremely large and discrete decline in the state's foreign-born population. We show that this population decreased by as much as 20 percent in the wake of the state's 2008 implementation of the Legal Arizona Workers Act (LAWA), a broad-based "E-Verify" law coupled with severe sanctions for noncompliance (Bohn, Lofstrom and Raphael 2013). By contrast, the law appears to have had no effect on the state's share of other non-citizens or naturalized Hispanic residents.

In order to isolate the causal effect of the passage and implementation of LAWA on crime, We employ two approaches, First, using state-level data, we use a synthetic "differences-in-differences" estimator, a method of counterfactual estimation for case studies proposed by Abadie, Diamond and Hainmuller (2010). Second, using city-level data, we estimate effecta via a traditional regression-based differences-in-differences in estimator. Using both approaches, we find evidence that property crimes declined modestly in the aftermath of the law.

To calculate a direct estimate of the effect of Arizona's Mexican immigrant share on its crime rate, We extend the synthetic differences-in-differences framework to construct implied synthetic instrumental variables estimates, using LAWA as an instrument for the Mexican population share. To our knowledge, this is the first extension of the method of synthetic controls to construct IV estimates. We also use a traditional regression-based differences-in-differences estimator using agency-level data; the results of this exercise are very similar.

In contrast to previous literature, using an enforcement shock, we find some evidence in favor of a contribution of Mexican immigration to property crime, particularly motor vehicle theft, in Arizona. The estimates are robust to a variety of specification checks including changing the composition of the synthetic comparison group, using agency-level and monthly data, estimating a traditional differences-in-differences model using regression and city-level data and are supported by a series

of placebo tests that examine the impact of dummy E-Verify laws in states that never received one. The results are not compromised by the passage of S.B. 1070, an infamous Arizona law that changed the relationship between immigrants and law enforcement but which was passed more than two years after LAWA. Importantly, to the extent that Arizona's E-Verify law makes it more difficult for Arizona's remaining immigrant population to remain employed (thus driving up crime through this mechanism), our estimates will be conservative.

However, these results critically are driven by the fact that LAWA resulted in a dramatic change in the demographic composition of Arizona's foreign-born Mexican population — with especially disproportionate declines among Mexican migrants who are young and male. Given the unusual effects on the demography of Arizona's plausibly undocumented population, the effects on crime that we observe can be thought of as predominantly compositional. Indeed, for most crimes, the treatment effect is fully explained by age and gender composition and, consistent with much of the prior literature, the results suggest that young Mexican men are less likely to engage in crime than young native men.[6] As such, this research reminds us that the effect of immigration enforcement on crime can be mediated substantially by the effect of immigration enforcement on the demographic composition of the population that remains.

The remainder of this paper is organized as follows. Section II describes the Arizona Legal Workers Act and its "E-Verify" provisions. Section III motivates the identification strategy and describes the data and modeling framework. Section IV presents results, robustness checks and considers the local average treatment effect of the legislation. Section V concludes.

II. Institutional Setting

We leverage a large and discrete change in Arizona's non-citizen Mexican population following the passage and implementation of the Legal Arizona Workers Act (LAWA) to estimate the contribution of Mexican immigrants to crime in the state. LAWA's primary provision is a broad-based mandate

[6] We note that these results are consistent with analyses of the State Criminal Alien Assistance Program (SCAAP) which shows that undocumented immigrants are represented approximately at parity in the population of incarcerated individuals (Camarota and Vaughan (2009). After accounting for the age and gender distribution of the undocumented population, this finding suggests that undocumented immigrants, adjusting for age and gender, are probably less involved in crime than equivalent U.S. natives.

that employers verify the legal work eligibility of all new hires using a federal database known as "E-Verify." E-Verify laws have become the focus of increasing scholarship with respect to the labor market prospects of U.S. natives (Orrenius and Zadovny 2013; Bohn, Lofstrom and Raphael 2015) but, to date, they have not been used to study the effects of immigration on public safety. This section provides a brief description of both the E-Verify system as well as LAWA.

A. The Federal "E-Verify" System

Given the inherent difficulty involved in policing a porous U.S.-Mexico border, recent advances in U.S. immigration enforcement have emphasized policies that address undocumented immigration within the country's interior (MacDonald and Sampson 2012; Cox and Miles 2014; Treyger, Chalfin and Loeffler 2014; Cianco 2017). Enforcement in the interior has taken two main forms: 1) expanded cooperation between federal and local law enforcement and 2) workplace-centered measures which seek to either incentivize or compel employers to deny employment access to undocumented immigrants. Federal sanctions on employers who knowingly hire unauthorized workers date to the 1986 Immigration Reform and Control Act (IRCA). Motivated by the reality that undocumented migration is, in large part, a function of employer demand for unauthorized labor and widely supported by a nontrivial share of the American public, employer-based enforcement has nonetheless proved challenging to implement.[7]

To address these problems, the 1996 Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) mandated the pilot program that eventually developed into the Internet-based E-Verify system in 2004.[8] The E-Verify system works as follows: Under federal law, all U.S. employers are required to fill out an I-9 tax form for all new employees. Using data provided by new hires during the Form I-9 process, employers who elect to use E-Verify will also submit a new hire's name, date of birth and either a social security number or an alien identification number into the E-Verify system through a secure website. The information provided is then verified against Social Security Administration (SSA) and Department of Homeland Security (DHS) databases. If the data provided

[7] The proliferation of false identity documents renders the Form I-9 process susceptible to fraud. Employers often claim they strive for rigor but fear running afoul of IRCA's anti-discrimination provision.

[8] As Rosenblum and Hoy (2011) note, political support for something like E-Verify can be traced as far as 1982, when the Senate passed an employer sanctions bill that would have created a "national identification card." Likewise, in 1984, both the House of Representatives and the Senate passed sanctions bills that would have mandated a national "call-in" system which could be used to verify employment eligibility. However, both bills died in committee.

by the applicant do not match administrative records, a "tentative non-confirmation" result induces an investigation to ascertain the source of discrepancy. If the identification data ultimately cannot be corroborated, a "final non-confirmation" is issued.[9] To date, E-Verify has had a 46 percent success rate in identifying undocumented immigrants (Westat 2009).

The use of E-Verify has expanded rapidly in recent years rising from 9,300 participating employers in 2006 to 243,000 participating employers as of January 2011 (Rosenblum and Hoyt 2011). Likewise, Rosenblum and Hoy document a dramatic rise in the number of employer queries — from 1.7 million in 2006 to 13.4 million in 2010.[10] While employers in any state may utilize the system for a minimal cost, much of the recent rise in utilization is due to the passage of state laws mandating its use. To date, fifteen states have passed some sort of legislation that mandates the use of E-Verify while eight states have passed an E-Verify law that has broad applicability to a large proportion of the state's workforce. [11] However, the first state to pass a broad-based E-Verify law that covers nearly all employers in the state was Arizona.

B. The Legal Arizona Workers Act

The Legal Arizona Workers Act (LAWA) (also sometimes referred to as the "Employer Sanctions Law") was signed into law in July 2007 and took effect on January 1, 2008. LAWA prohibits Arizona employers from knowingly or intentionally hiring an undocumented immigrant after December 31, 2007. LAWA also mandates the use of E-Verify by all employers in Arizona to establish the identity and work eligibility of all new hires. Not only is the law broad-based, it also imposes harsh sanctions on non-compliant employers. The penalty for an employer's first offense is a suspension of business license with the second offense carrying a potential penalty of revocation.[12]

As of January 2011, Arizona accounted for just over 7 percent of businesses nationwide that

[9]Recently, DHS has also made available such features as the photo-tool that allows employers to prevent fraud by comparing the photograph on the identity card provided against a photo in the database.

[10]Despite a rapid rise in uptake, as of January 2011, fewer than 3 percent of all U.S. employers had signed up with E-Verify.

[11]These states include Arizona, a traditional destination for undocumented immigrants in the United States, Utah, and a number of "new destinations" in the southeastern United States: Georgia, Alabama, North Carolina, South Carolina, Mississippi, and Tennessee. A number of additional states have passed an 'E-Verify law that covers specific sectors of the state's economy — generally public employment. Naturally, there are very few undocumented immigrants working in the public sector. Colorado became the first state to pass an E-Verify law in 2006.

[12]As Bohn, Lofstrom and Raphael (2014) note, "to date, legal action taken against employers for violating the provision of LAWA has been quite rare. As of April 2010, more than two years after implementation, only three employers have been indicted under the provisions of LAWA, and all of those in a single county (Maricopa)."

were enrolled in E-Verify (Rosenblum and Hoyt 2011). Within Arizona, 35,988 (25.7 percent) of the state's 140,081 employers had enrolled in the system. The enrollment rate in Arizona is thus over ten times higher than that in California (2.4 percent), Texas (2.6 percent) or New Mexico (2.5 percent), three other states with large undocumented populations. As Bohn, Lofstrom and Raphael (2014) note, recent reports suggest that at least 700,000 new hires made between October 2008 and September 2009 were subject to E-Verify checks in Arizona, equaling roughly 50 percent of all new hires in the state. As such the law has quite plausibly made it considerably more difficult for unauthorized migrants to obtain gainful employment in Arizona than in other U.S. states. To the extent that LAWA decreases the share of Arizona residents who are undocumented, this may occur through two different channels. First, undocumented immigrants currently residing in Arizona may choose to leave the state either settle in another U.S. state or return to their country of origin. Second, foreign nationals planning to migrate to Arizona might choose to migrate elsewhere or to remain in their country of origin. While the legislation targets undocumented immigrants, there is also the possibility that the legislation may cause certain U.S. citizens to leave the state as well. This might occur, for instance, in families in which some members were born in the United States while others are undocumented.

Section IV of the paper examines, in detail, changes in the demographic composition of Arizona's population in the wake of the passage and implementation of LAWA. In particular, we examine the impact of LAWA on the non-citizen Mexican population, a population that has been shown to be both largely undocumented as well as the largest contributor to the undocumented population.[13] If LAWA provides a plausible natural experiment for a change in the foreign-born Mexican share of the state's population, it should be true that Mexican nationals are the only sub-population of immigrants whose population numbers are affected by the law. We provide evidence in Section IV that this is the case. In the following section, we discuss data and methods and defend the credibility of the identification strategy.

[13] As Passel and Cohn (2009) note, between 80 and 90 percent of recently arrived Mexican immigrants are undocumented and Mexican nationals comprise approximately 60 percent of the undocumented population in the U.S.

III. Empirical Strategy

A. The Synthetic "Differences-in-Differences" Estimator

In order to estimate the effect of the passage and implementation of LAWA on crime at the state level, we employ a relatively new method of counterfactual estimation developed by Abadie, Diamond and Hainmuller in a 2010 article published in the *Journal of the American Statistical Association*. The method, which uses a data-driven algorithm to identify a synthetic comparison group from among a pool of potential comparison units, represents an advance in the estimation of treatment effects for discrete aggregate-level policy interventions.[14] In the context of a state-level intervention in the United States, the methodology works by assigning an analytic weight to each U.S. state that has not implemented a given policy (e.g., an E-Verify law), where the weights are computed such that the difference in a given pre-intervention outcome (e.g., crime) between a treated state (e.g., Arizona) and its pool of potential comparison states is minimized. In this way, the methodology generates a comparison group which, conditional on pre-treatment observables, meets the assumption of parallel trends prior to implementation of the treatment. Appendix A describes the estimator in greater detail.

Synthetic controls represent an advance on designs that select comparison states based on arbitrary or ad hoc criteria and standard two-way fixed effects estimators which implicitly use an unweighted or population-weighted average of the remainder of the United States as a comparison group. By using a data-driven method to generate an appropriate control group, the estimated treatment effect is robust to a common misspecification problem. Moreover, the method offers a series of placebo tests that allow for formal inferences to be generated and which help to ensure that the resulting D-D estimate is not the result of an intervention whose timing is insufficiently random.

[14] Abadie, Hainmuller and Diamond (2010) apply the methodology to estimate the effect of the passage of Proposition 99, a California ballot proposition designed to reduce consumption of tobacco. An older reference can be found in Abadie and Gardeazabal (2003) who study the effects of terrorism on economic development in Spain's Basque Country.

B. The Synthetic Instrumental Variables Estimator

While the reduced form effect of LAWA on crime is itself interesting, another parameter that is of interest in prior literature is the effect of an increase in a state's foreign-born population share on crime. In this section, we show that the synthetic differences-in-differences framework advanced by Abadie, Diamond and Hainmuller can be conveniently extended to compute implied IV estimates of the effect of Mexican emigration from Arizona on its crime rate.

Recall that the instrumental variables estimator, $\beta^{IV} = (D'M)^{-1} D'Y$ where D is the instrument, M is the foreign-born Mexican population share and Y is the crime rate. Re-writing we get (1) which provides a roadmap for constructing the implied synthetic IV estimator.

$$\beta^{IV} = \frac{cov(D_i, Y_i)}{var(D_i)} / \frac{cov(D_i, M_i)}{var(D_i)} \tag{1}$$

Examining (1), it is straightforward to see that the numerator is the least squares coefficient obtained from a regression of Y_i on D_i and the denominator is the least squares coefficient obtained from a regression of M_i on D_i. The former is simply the reduced form estimate of the effect of LAWA (D_i) on crime (Y_i) while the latter is the first stage estimate of the effect of LAWA on M_i, the Mexican population share. Using the synthetic D-D estimator, these quantities can be written as:

$$DD^{RF} = (Y^{AZ}_{POST} - Y^{SYNTH}_{POST}) - (Y^{AZ}_{PRE} - Y^{SYNTH}_{PRE}) \tag{2}$$

$$DD^{FS} = (M^{AZ}_{POST} - M^{SYNTH}_{POST}) - (M^{AZ}_{PRE} - M^{SYNTH}_{PRE}) \tag{3}$$

Finally, the synthetic IV estimator is constructed as $\frac{DD_{RF}}{DD_{FS}}$, that is by dividing the D-D estimate in (2) by the D-D estimate in (3). When M is measured as the foreign-born Mexican share of each state's population, the IV estimator yields the predicted percentage change in the crime rate arising from a one percentage point increase in the non-citizen Mexican share.

One complication is worth noting. In principle, the construction of an IV estimator from first stage and reduced form estimates requires that both equations contain the same control variables. However, by conditioning on past values of the dependent variable, the synthetic D-D estimator implicitly assigns different weights to states in the first stage equation (which predicts the foreign-

born population share) and the reduced form equation (which predicts crime). Thus this condition will not be met. In order to verify that the implied IV estimates we report in this paper are valid, we re-estimate the synthetic first stage and reduced form synthetic control models, imposing a common set of weights. In particular, we generate a variable that is the average of the foreign-born Mexican immigrant share and the crime rate in each year of the study period and use that composite variable in both the first stage and reduced form equations. We show that estimates obtained by the procedure with common weights are extraordinarily similar to those obtained using the synthetic D-D estimates in which no common weights are imposed.

C. Data

Data used in this research are collected at the state level and include all U.S. states and the District of Columbia with the exception of four states that passed comprehensive E-Verify laws around the same time as Arizona: Alabama, Georgia, Mississippi and South Carolina.[15] Data are drawn from two primary data sources. Crimes reported to police were obtained from the Federal Bureau of Investigation's *Uniform Crime Reports* (UCR), the standard source of data on crimes at the agency level that is employed in aggregate-level crime research.[16] Data on the non-citizen population come from the American Community Survey (ACS), a one percent sample of U.S. households, collected annually since 2000 by the U.S. Census. The ACS asks respondents whether or not they were born in the United States and, if not, in what country were they born. For each state, we calculate the share of the population in each year that is comprised of (1) non-citizen Mexicans, (2) non-citizens from countries other than Mexico and (3) naturalized Hispanic residents. We also calculate age- and gender-specific versions of each of these three population shares.[17] One

[15] Notably, none of these states have large Mexican immigrant populations.

[16] Since 1934, the UCR has, either directly or through a designated state reporting agency, collected monthly data on index crimes reported to local law enforcement agencies. The index crimes collected consistently since 1960 are: murder (criminal homicide), forcible rape, robbery, aggravated assault (hereafter "assault"), burglary, larceny and motor vehicle theft. The UCR employs an algorithm known as the "hierarchy rule" to determine how crimes involving multiple criminal acts are counted. In order to avoid double counting, the UCR classifies a given criminal transaction according to the most serious statutory violation that is involved. For example, a murder-robbery is classified as a murder.

[17] A decision that commonly arises in immigration research concerns whether foreign-born citizens should be counted as immigrants or natives. On the one hand, the foreign-born are immigrants whether or not they subsequently obtain citizenship. Likewise, foreign-born citizens are likely a heavily selected subpopulation of the foreign-born. On the other hand, when researchers refer to "natives," they are commonly referring to individuals who have standing as natives in U.S. society. The majority of the literature on the labor market impacts of immigration count foreign-born

concern in using ACS data is that the immigrant population may be poorly-measured. While errors are inevitable, incredibly a large share of undocumented immigrants do, in fact, fill out their ACS survey forms, a finding which can be verified by comparing survey respondents to legal green card holders (Passel 2009). In this context, problems would arise if the passage of LAWA caused immigrants to be less likely to fill out ACS surveys. While we cannot reject that possibility, there is a great deal of evidence to suggest that the measured population shock is not an artifact of a change in survey-taking behavior. In particular, rental vacancy rates rose considerably in communities that were home to Mexican immigrants while there was no change in the numbers of owner-occupied units (Bohn, Lofstrom and Raphael 2014). This analysis is supported by ethnographic evidence from Ayon et al (2011) and by Caballero, Cadena and Kovack (2017) who note that applications from *matrculas consulares* validate the population shock due to LAWA.

Finally, we collect key control variables from the ACS. These variables include measures of a state's native demographic composition — the percentage white, the percentage black, the percentage married and percentage in the following age groups: 0-14, 15-24, 25-39, 40-54 and 55+. In addition, we control for each state's labor force participation rate, its employment-to-population ratio and its unemployment rate as well as each state's industry concentration for the following industries in which immigrants are disproportionately employed: agriculture, construction, manufacturing, restaurants and retail trade.

D. Threats to Internal Validity

The identification strategy employed in this research ultimately relies on 1) the exogeneity of the timing of LAWA's passage, 2) a lack of other contemporaneous shocks to crime in Arizona and 3) the ability of the research to account for demographic spillovers — changes in the composition of Arizona's native population in response to LAWA as well as potential geographic spillovers as immigrants leave Arizona for other states. In this section, we provide a mixture of institutional and quantitative evidence in support of the identification strategy. We begin by briefly providing some institutional evidence that the timing of LAWA's implementation was largely a function of the vagaries of the legislative process and was therefore idiosyncratic. Next, we discuss the

citizens as natives and, accordingly, we maintain that convention here.

possibility that other confounders — namely the timing of the "Great Recession" and other changes in both federal and local immigration enforcement that occurred not long after LAWA's passage — may compromise the research design. Next, we discuss the possibility that there are important demographic or geographic spillovers. Finally, we provide evidence from an auxiliary analysis that aggregate crime reporting rates do not appear to be sensitive to changes in a community's immigrant share.

(i) *The Timing of LAWA*

As Bohn, Loftsrom and Raphael (2014) note, a number of features of Arizona's legislative environment suggest that the passage of LAWA was not a response to recent crime or employment conditions. Indeed, prior to 2007, violent and property crime rates in Arizona had been constant and falling, respectively (see Figure 1). Likewise, Arizona's unemployment rate had been falling and its employment-to-population ratio had been rising for nearly a decade prior to LAWA's passage (see Table 1). Instead, the legislative debate surrounding LAWA suggests that the law was a reaction to perceived long-term discontent regarding an increasing presence of undocumented immigrants in the state. As evidence for the randomness of the timing of the law, Bohn, Loftsrom and Raphael note that legislative debate over LAWA spanned several legislative sessions and, due to several federal lawsuits challenging the constitutionality of LAWA, there was substantial uncertainty as to when the act would go into effect once passed by the state legislature.[18]

(ii) *The Great Recession*

LAWA was considered and initially implemented during a period of broad economic growth. However, the Great Recession began to impact Arizona in late 2008 and, to the extent that the economic downturn differentially affected Arizona's labor markets and thus its crime market, this has the potential to confound differences-in-differences estimates of the effect of LAWA on crime. To address this concern, We begin by controlling extensively for pre-treatment trends in Arizona's unemployment rate, its employment-to-population ratio and employment shares in construction,

[18]The key federal lawsuit was dismissed in December of 2007 thus clearing the way for LAWA to take effect on January 1, 2008.

wholesale and retail trade, manufacturing, restaurants and other leading industries. Since a synthetic control region is selected for Arizona on the basis of pre-treatment trends in crime as well as a broad range of economic and social covariates, the analysis controls for these potential confounders as long as the synthetic control method finds a "close" match for Arizona. As we show in Section V, this condition is satisfied. However, controlling for past trends does not mean that the estimator is robust to a contemporaneous shock in 2008. To assess whether Arizona was, in fact, differentially impacted by the Great Recession conditional on pre-intervention trends, we construct synthetic controls estimates of the change in the employment-to-population ratio and the unemployment rate for U.S. natives in Arizona relative to other U.S. states. Figure 1 presents placebo plots for each of these variables, comparing each U.S. state to its matched synthetic control group, with Arizona's series represented in bold. There is no evidence that Arizona's increase in unemployment rate was especially unusual with Arizona's estimated unemployment increase (< 1 percentage point) sitting in the middle of the counterfactual distribution of changes in unemployment after 2008.

There is, however, some evidence that Arizona may have experienced an outsize decline in its employment-to-population ratio — by 2009, Arizona's decline in this variable was the fifth largest among forty-six states in the analysis. While this decline is fairly small (approximately 2 percentage points) and is not significant at conventional levels, it does raise the concern that resulting estimates may have been impacted by the Great Recession's impact on the labor market prospects of U.S. natives. On this point, we note that to the extent that the Great Recession may have had a deleterious effect on the employment prospects of natives who remained in Arizona, one would expect that this would have resulted in an increase in crime — particularly property crimes — among natives (Cook 2010; Chalfin and McCrary 2018b). There is no evidence that this, in fact, occurred, as there is no evidence of a discontinuous change in the arrest rate of non-Hispanic residents in Arizona after 2008 (see Appendix Figure 1). Moreover, to the extent that natives were differentially affected by the Great Recession, any decline in crime that we observe as a function of LAWA will be biased downwards, thus making our estimates of the effect of LAWA on crime conservative.

(iii) *Changes in Local Immigration Policy*

A second concern is that concurrent with the passage of LAWA, Arizona may have changed immigration enforcement in a way that has a direct effect on crime, other than through the out-migration of plausibly undocumented immigrants. Here, there are two specific concerns regarding immigration enforcement in Arizona around this time. First, the largest county in Arizona — Maricopa County which accounts for nearly 60 percent of the state's population — has, since 1993, had an elected sheriff, Sheriff Joe Arpaio who has publicly branded himself to be "America's toughest sheriff." His policy initiatives include having built a "tent city" to house inmates outdoors during the summer, re-instituting the practice of inmate chain gangs and requiring inmates to wear pink underwear.[19] Sheriff Arpaio is also known for being especially vociferous in directing county law enforcement resources to enforce federal immigration laws, a policy which has led to numerous private and even federal lawsuits. With respect to this research, it is important to note that there is no evidence that Sheriff Arpaio who has been in office since 1993, launched any new policy initiative or measurably changed immigration enforcement or any other type of enforcement policy, on or around 2008.[20] We also note that crime trends in cities in Maricopa County closely track crime trends in other Arizona cities around the timing of LAWA, with both sets of cities experiencing large reductions in crime after 2008 (see Appendix Figure 2). This suggests that post-LAWA crime reductions are not a "Sheriff Joe effect."

A second issue which has received a great deal of local and national attention is the passage of an Arizona state bill known as S.B. 1070 also known as the "Support Our Law Enforcement and Safe Neighborhoods Act," passed in 2010. This bill, among other things, made it a misdemeanor crime to reside in Arizona without legal authorization, and not only permitted but, in fact, required, law enforcement officers to attempt to determine an individual's immigration status during a lawful citizen stop so that the individual could be detained and possibly deported. While this law clearly could have had an impact on crimes known to police, we note that the law was not passed until April

[19] see e.g., http://www.mcso.org/JailInformation/CustodySupport/Default.aspx?t=LaundryServices

[20] It is difficult to identify a precise point in time when Maricopa County ramped up immigration enforcement activity but anecdotal accounts suggest that these policies began in 2005 shortly after a local election in 2005 in which immigration enforcement became a prominent electoral issue. See e.g., http://archive.azcentral.com/members/Blog/EJMontini/167250.

2010, more than two years after LAWA's passage. In order to account for the potential importance of S.B. 1070 in explaining crime, we examine crime outcomes in 2008 and 2009 only.

(iv) *Changes in Federal Immigration Policy*

It is also important to consider whether changes in federal immigration policy coincide with the timing of LAWA's implementation. Bohn, Loftsrom and Raphael (2014) report that a review of Department of Homeland Security arrest and apprehension data reveals that the proportion of border apprehensions for the Tucson border sector did not change during LAWA's implementation period. Moreover, they note that the Arizona Border Control Initiative which was responsible for an increase in the intensity of border enforcement pre-dated LAWA by several years. A remaining concern is the U.S. Department of Homeland Security's roll-out of its "Secure Communities" program, a federal initiative that induces cooperation between DHS' Office of Immigration and Customs Enforcement (ICE) and local law enforcement agencies. Under Secure Communities, local police agencies are required to send identifying information, including fingerprints, for all arrestees to federal immigration authorities so that arrestees who are illegal aliens can be identified using federal databases. If ICE identifies an arrestee as a potential immigration violator, ICE can require local law enforcement to hold the individual in jail for up to forty-eight hours so that the individual can be transferred to federal custody for the initiation of deportation proceedings (Cox and Miles 2010). While Secure Communities is currently required of all jurisdictions, during the initial roll-out, local police agencies were given the choice to voluntarily opt in to the program. Arizona counties are heavily represented amongst those opting into the program with key counties such as Maricopa (activation date: January, 2009) Pima (November, 2009) and Yuma (January, 2009) activating early. As of December 2012, ICE has identified 84,976 alien arrestees in Arizona of whom 16,177 had a prior criminal conviction. Of the 84,976, 3,497 had a prior ICE removal. Notably these counties did not opt-in prior to 2009. Relatively few of these individuals have been removed by federal authorities and indeed there is scant evidence that Secure Communities has had any affect on crimes known to law enforcement (Cox and Miles 2014; Treyger, Chalfin and Loeffler 2014). Nevertheless since it is not entirely possible to separate the effect of Secure Communities from that of LAWA in the years after 2008, we report separate results for the 2008 and 2009 post-treatment

periods. Results are similar regardless of which post-period is used.

(v) *Demographic and Geographic Spillovers*

Even if the timing of LAWA is arbitrary and if there are no important contemporaneous shocks, the identification strategy can lead to a biased result if, in addition to inducing immigrants to leave Arizona, there are other resulting demographic responses such as a change in the composition of natives or out-migration from Arizona to states used in Arizona's donor pool used to construct a synthetic control group. We begin with a discussion of the possibility that the composition of Arizona's native population may have changed in response to the immigration shock. Appendix Figure 3 presents synthetic controls estimates for a number of demographic variables that describe Arizona's native population. In particular we show placebo plots (again, with Arizona denoted using a bold line) for composition by race (percent white and percent black), age (0-14, 15-24, 25-39, 40-54 and 55+) and education (< high school, high school degree, some college and college +). Scanning through these figures there is no evidence of any important compositional changes among the native population. With respect to the issue that immigrants may have settled in states used in Arizona's donor pool, we follow Bohn, Lofstrom and Raphael (2014) and exclude Arizona's border states from the donor pool and re-estimate resulting models. These results are presented in the following section.

A second set of concerns revolves around the fact that E-Verify laws can potentially affect immigrant criminality through channels other than through shocks to the size of the state's immigrant population. For example, there is research that indicates that E-Verify laws make it more difficult for immigrants to compete successfully in a state's labor market (Amuedo-Dorantes and Bansak 2012; 2014; Bohn and Lofstrom 2013; Orrenius and Zavodny 2015) and that granting immigrants legal status may reduce offending (Bell, Fasani and Machin 2013; Mastrobuoni and Pinotti 2014; Baker 2015; Freedman, Owens, and Bohn; Pinotti 2017). The specific issue then is that Arizona's E-verify law could affect immigrant criminality by restricting economic opportunities among immigrants. We note that, to the extent that these effects are present, they will put upward pressure on crime. Therefore, our estimated effects which are negative will, if anything, be conservative.

A final concern is that Arizona's E-Verify law could have reduced crime by creating more favor-

able labor market conditions for low-skilled natives — and thus lower offending among this group. We note that these effects have not been found in Arizona (see Bohn, Loftstrom and Raphael 2015) a finding which we replicate here.

(vi) *Bias from Immigrant Crime Reporting*

Finally, we provide an auxiliary analysis of National Crime Victimization Survey (NCVS) data to empirically test the proposition that increases in a metropolitan area's immigrant share are correlated with the rate at which residents report crimes to police. As the NCVS asks respondents about victimization as well as if they reported the crime to the police, this is an ideal dataset to gather some insight into the relationship between immigration and crime reporting. As we show, overall crime reporting is empirically less sensitive to immigration than has been theorized.

IV. Results

A. Descriptive Statistics

Table 1 presents summary statistics for key independent and dependent variables for Arizona in each year between 2001-2009. Panel A presents means for selected demographic variables — the percentage of residents who are white and black, the percentage married and the percentage in each of five age groups (0-14, 15-24, 25-39, 40-54 and 55+). Panel B presents summary statistics for several measures of the immigrant population: the foreign-born Mexican population share, the foreign-born non-Mexican share, the total immigrant share and the Mexican share among all immigrants. In addition, we provide data on the share of Hispanic U.S. citizens. Panel C presents summary statistics for three key economic variables: the labor force participation rate, the employment-to-population ratio and the unemployment rate. Panel D presents the summary statistics on industry concentration, that is the proportion of state residents employed in each of five industries which employ a large share of immigrants: agriculture, construction, manufacturing, restaurants and retail trade. Finally, Panel E presents data on crimes per 100,000 individuals for each UCR index crime.

Examining Table 1, it is clear that key covariates for Arizona appear to be smooth across the 2007-2008 treatment threshold. For example, referring to Panel C, between 2007 and 2008, there is

hardly any change in either the labor force participation rate, the employment-to-population ratio or the unemployment rate in Arizona. Likewise, in Panel D, there is no discernible change in the Arizona's industry composition between 2007-2008. Notably however, the financial crisis generated large changes in the strength of Arizona's economy after 2008, increasing the unemployment rate from 5.8 percent to 9.9 percent and decreasing the employment-to-population ratio from 58 percent to 54 percent. Again, we note that the Recession did not greatly affect 2008 and so we estimate results separately using 2008 and 2009 as post-treatment years.

Finally, we present descriptive evidence for crime. Figure 1 plots crimes for Arizona and for the remainder of the United States for the 2000-2009 period. With the exception of rape, Arizona's crime trends follow national trends closely until 2008 at which point crime declines become considerably steeper. These trends motivate a more investigation in Section IV of the paper.

B. First Stage Results

Figure 3 provides a more formal analysis of these trends by presenting graphically synthetic differences-in-differences estimates of the effect of LAWA on three key population shares. Panel A considers the non-citizen Mexican share, Panel B considers the non-citizen share for other nationalities and Panel C considers the share of the population that is comprised of naturalized Hispanics. Each of the panels presents two figures. The figure on the left-hand side of the page compares the relevant population share in Arizona (using a solid line) to the same population share in Arizona's synthetic comparison region (using the dashed line). The figure on the right-hand side of the page compares the estimated treatment effect of Arizona's LAWA (using the bolded line) to placebo treatment effects in the other states in the donor pool, each of which is plotted using a gray line. In particular, the black line plots the difference in the relevant population share over time between Arizona and synthetic Arizona while each gray line plots the difference in the relevant population share over time between each state in the donor pool and its synthetic comparison group. Because none of the other states in the donor pool passed an E-Verify law, the distribution of the gray lines is equivalent to the sampling distribution of the estimated treatment effect. To the extent that the post-treatment difference in Arizona is especially large or especially small relative to the untreated states, the estimated difference is unlikely to have occurred due to chance. All models condition on

annual lags of the dependent variable as well as covariates which capture measures of population and structure and industry concentration.

Panel A considers the effect of LAWA on Arizona's foreign-born Mexican population share. From 2000-2007, Arizona and synthetic Arizona track each other extremely closely, indicating that the synthetic matching algorithm has performed well. After 2007, Arizona's non-citizen Mexican share falls dramatically relative to its synthetic comparison region. By 2008, the estimated difference was 0.5 percentage points. By 2009, the difference is approximately one percentage point (which represents a 17 percent reduction in the share). Accordingly the synthetic differences-in-differences estimates confirm the trend that can be seen in the raw data. The figure on the right-hand side of Panel A considers whether the estimated effect is likely to be due to chance. While the difference between Arizona and its synthetic comparison group is in the middle of the distribution of the distribution prior to LAWA, after LAWA, the synthetic differences-in-differences estimate for Arizona is larger in magnitude than for any state in the donor pool. This is already true by 2008 and, by 2009, the gap is even larger. The results provide intuition that the drop in Arizona's foreign-born Mexican share is unlikely to have been due to chance.[21]

The figures presented in Panel A assume that the drop in the foreign-born Mexican share can be attributed to LAWA. However, this interpretation could reasonably be called into question if the share of groups that should be far less or entirely unaffected by LAWA also change across the treatment threshold. Accordingly, panels B and C present identical figures for the immigrants from countries other than Mexico and the share of naturalized Hispanics, respectively. Referring to Panels B and C, there is little evidence in favor of a change in either population.

C. Main Results

Next, we present a series of graphs that capture the reduced form effect of LAWA on seven different UCR index crimes and two crime aggregates: violent crimes (murder, rape, robbery and aggravated assault) and property crimes (burglary, larceny and motor vehicle theft). Each panel in Figure 4 presents synthetic differences-in-differences estimates for a given crime type along with

[21] We note that Bohn, Lofstrom and Raphael (2014) note that the decline in Arizona's plausibly undocumented immigrant population can also be seen by examining changes in rental vacancy rates in communities with large immigrant concentrations.

the associated placebo test.[22] Panel A presents estimates for the violent crime aggregate. Referring to figure on the left-hand side, we see that Arizona and synthetic Arizona have very similar violent crime rates prior to the introduction of LAWA. After LAWA's passage, violent crime falls by approximately 10 percent in Arizona relative to its synthetic control region. Referring to the placebo test, this difference appears to be larger than the average among the placebo states. However, several of the placebo states have larger drops in their violent crimes rates and accordingly the estimate is somewhat imprecise. Disaggregating violent crimes by crime type, we see evidence of declines in all four crime types, with an especially large decline in murder in 2009.

Referring to Panel F of Figure 4, we see that property crimes declined by approximately 20 percent after LAWA's passage, an effect which looks particularly large relative to the sampling distribution formed by the placebo states. Disaggregating by crime type, this result appears to be especially driven by motor vehicle theft which declined by 20-35 percent, depending on whether 2008 or 2009 is used as the relevant post-treatment year.

Table 2 presents the information in Figures 3 and 4 in tabular form. In particular, the table presents the reduced form (Panel A) and first stage (Panel B) D-D estimates given in equations (5) and (6) along with implied IV estimates (Panel C) which are computed by dividing each reduced form estimate by the corresponding first stage estimate. For each dependent variable, the table computes the mean difference between Arizona and its synthetic control region in the pre-2008 intervention period. Next, for two post-treatment periods (2008 and 2008-2009), we compute the mean post-treatment difference between Arizona and its synthetic control group. Subtracting the mean pre-treatment difference from a given post-treatment difference yields the D-D estimate of the average treatment effect. Finally, the table reports the p-value from the one-tailed test of the null hypothesis that Arizona's D-D estimate is non-negative against the alternative that the D-D estimate is negative. The p-value is computed by dividing Arizona's rank in the distribution of D-D estimates by the total number of D-D estimates among Arizona and the placebo states. For example, when Arizona's D-D estimate for a given variable is the most negative among all of the states studied, the p-value on that D-D estimate would be 1/46.

We begin discussion of Table 2 by considering first stage estimates of the effect of LAWA on

[22] The weights used to construct the synthetic D-D estimates are presented in Appendix Table 1.

the foreign-born Mexican share presented in Panel B. Prior to LAWA, the mean difference between Arizona and its synthetic control region is zero indicating that the matching algorithm in the synthetic control procedure performed exceptionally well. In 2008, Arizona's foreign-born Mexican share was 0.37 percentage points lower than that in its synthetic control region and, by, 2009, its Mexican share was 0.92 percentage points lower. Since this is the largest difference among the sampling distribution, the associated one-tailed p-value is 0.021. Next, we consider reduced form estimates of the effect of LAWA on the log crime rate given in Panel A. For each crime type, mean pre-treatment differences are small indicating that the algorithm identified a good match for Arizona. The largest of these differences was 1.6 percent (for the property crime aggregate) with most differences falling well below 1 percent. Two sets of D-D estimates are given — those calculated using 2008 as the post-treatment period and those using 2009 as the post-treatment period. While the latter uses more information, 2009 is potentially compromised by DHS' early roll-out of its Secure Communities program in a number of densely-populated Arizona counties. Hence 2008 may give a cleaner estimate of the average treatment effect of LAWA. Using 2008 as the post-treatment period, we see that the largest D-D estimate is found for motor vehicle theft (-14 percent). The estimated reduction in the property crime aggregate (-12 percent) is of similar magnitude. Notably, 7 of 9 estimated treatment effects are negative. Referring to the 2009 post-treatment period, all of the estimated treatment effects are negative. The largest effects is found for motor vehicle theft (-34 percent) and estimates on motor vehicle theft and the property crime aggregate are significant at conventional levels. Interestingly, the magnitudes of the D-D estimates using the 2009 treatment period are largely similar to those computed only 2008. To the extent that Secure Communities confounds the estimates by resulting in the removal of criminal aliens, the expected bias would go in the negative direction. Therefore, a conservative reading of the evidence suggests that per capita property crimes fell by approximately 12 percent in the aftermath of LAWA, with the effect largely driven by a 14 percent reduction in per capita motor vehicle thefts.

The reduced form effect of LAWA is interesting insofar as it yields an estimate of the effect of immigration enforcement on crime. To the extent that we also want to understand the effect the immigrant share itself, Panel C computes implied IV estimates of the effect of a change in the foreign-born Mexican share on crime. For each crime type, these estimates are computed by

dividing the reduced form estimate in Panel A by the first stage estimate in Panel B. Since both the reduced form and the first stage estimates are negative indicating that LAWA reduced both crime and the foreign-born Mexican share, the implied IV estimates are positive implying that the effect of Mexican immigrants on crime is positive. Estimates for all crime types are fairly large and, using 2009 as the post-treatment period, range from 6 percent for rape to 36 percent for motor vehicle theft. Notably, these implied IV estimates are produced using first stage and reduced form estimates that use different weights and thus condition on a different set of control variables. To impose a common set of weights, we generate a variable that is the average of the foreign-born Mexican immigrant share and the crime rate in each year of the study period and use that composite variable in both the first stage and reduced form equations. The estimates obtained by the procedure with common weights are shown in Appendix Table 2 and are broadly similar to those obtained using the synthetic D-D estimates in which no common weights are imposed.

Since only motor vehicle theft and the property crime aggregate are significant for both post-treatment periods, we focus most heavily on these results. Property crimes declines by approximately 12 percent in the aftermath of LAWA. Given the size of the shock to the foreign-born population, the implication is that a one percentage point decrease in Arizona's foreign-born Mexican share led to a 22 percent decrease in property crimes. In the remainder of the paper, we report a series of robustness checks designed to generate further confidence in the research design, we seek to characterize the local average treatment effect of LAWA which points to considerable compositional effects with respect to the age and gender of Mexican migrants.

D. Robustness Checks

Before characterizing the estimated treatment effects presented above, we explore several robustness checks designed to test the sensitivity of the synthetic D-D estimates to decisions made during the research process and as an implicit check on the identification strategy. We also present estimates of the effect of LAWA using more granular monthly and quarterly crime data as well as using crime data available at the city rather than the state level, using standard differences-in-differences regression.

Robustness checks are reported in Table 3. We begin a discussion of robustness checks by re-

specifying the synthetic D-D models presented in Table 2 excluding Arizona's border states from the donor pool. These estimates address the potential for bias caused by spillovers from Arizona to its border states. If Arizona's border states receive an increase in Mexican immigration from Arizona as a result of LAWA, the estimated effect of LAWA will be too large. Thus, in the presence of LAWA-induced spillovers, the suitability of border states as a comparison region for Arizona can be called into question. Since Arizona's border states (California, New Mexico, Utah and Nevada) contribute importantly to the donor pool in only a some instances, many of the estimates in Table 3 are equivalent to those in Table 2.

Spillovers, however, are not guaranteed to accrue to Arizona's border states. In order to ensure that the main results are robust to potential spillovers to states that do not border Arizona, we identify the states that received the largest percentage influx of Mexican immigrants between 2008-2009 — Wyoming, Kansas, New Mexico, Oklahoma and Idaho — and exclude these states from the donor pool. These results are presented in the third set of columns in Table 3 — the results remain largely unchanged.Next, we re-specify the D-D models including 2007 as a post-treatment year. The intuition behind this re-specification of the model is that while LAWA was implemented on January 1, 2008, the law was passed in July, 2007 and, as such, there might have been an anticipatory effect of the law. Here again, the results are broadly similar to the main estimates.

Next, we show that the results are not driven by the synthetic controls methodology itself. We present regression-based differences-in-differences estimates of the effect of LAWA using annual police agency-level data for U.S. cities with 50,000 or greater population in Table 4. These are the most granular data that can be used to estimate the effect of LAWA. With 11 Arizona cities meeting the population threshold of 50,000, the treatment effect is estimated using 22 treated city-months (11 cities $\times$ 2 treated years for each city). For each crime type, we regress, using Poisson regression, the crime count on a treatment dummy, the city's population and a full set of city and year fixed effects. Results are broadly consistent with synthetic control estimates computed using state-by-year variation. Panel A presents the basic D-D results while Panel B reports D-D estimates controlling for two placebo dummies one which captures Arizona cities in 2007 and a second dummy that captures Arizona years prior to 2007. Estimates in Panel B are useful both because they provide a test of anticipatory effects (for 2007) and for the existence of a pre-treatment

trend that would tend to invalidate the identification strategy.

Referring to Panel A, the effects of LAWA are broadly similar to those arising from the synthetic controls analysis. All of the estimated coefficients are negative and estimates for burglary, larceny and motor vehicle thefts are precise at conventional levels of significance. Overall, Arizona cities experienced a decline of 7 percent and 16 percent for violent and property crimes respectively in the aftermath of LAWA. Referring to Panel B, there is little evidence that crime reductions began to accrue in Arizona cities prior to LAWA's implementation.

Finally, we turn to a data limitation that has hampered the literature on immigration and crime for decades — the extent to which immigration may alter, in the aggregate, crime *reporting*. Here, we provide an auxiliary analysis using data from the National Crime Victimization Survey aggregated to the metropolitan statistical area (MSA)-level to test whether changes in crime reporting rates lead to changes in immigration. In particular, using data on 27 of the largest U.S. MSAs over the 1980-2000 period, we show that decadal changes in the rate at which victims indicate that they report crimes to police is invariant to decadal changes in the immigrant share in an MSA. Conditional on controls for the change in race, gander, age and educational composition of the population, the coefficient on the change in the immigrant share in this regression is -0.007 and is precisely estimated. The implication is that a one percentage point increase in the immigrant share leads to less than a one percent reduction in the rate at which victims report crimes to the police. This result reveals nothing more than a weak relationship between immigration and crime reporting and suggests that concerns about crime reporting by immigrants are not a first order concern in the immigration-crime literature.[23]

[23]This result is subject to several important limitations. First, a small coefficient does not necessarily imply that immigrants are less likely to report crimes to police than natives – indeed it could be that natives are more likely to report a crime when they are victimized by an an individual they believe might be an immigrant – and that this washes out our ability to observe the true association. Nevertheless this is the relevant analysis with respect to understanding the scope for differential reporting rates to bias regression-based estimates in the literature. To see this, consider that the probability that a crime is reported to the police in a given MSA-year is a variable that is typically omitted from a crime regression. To the extent that this variable is related to the immigrant share and to the crime rate, it will lead to an inconsistent coefficient on the immigrant share. However, if the reporting rate is related to the crime rate but not to the immigrant share, the coefficient on the immigrant share will be unaffected by changes in crime reporting. A second limitation arises from the relatively sparse MSA-level data in the NCVS. Overall, among the 27 MSAs with consistent jurisdictional boundaries over the 1980-2000 period, there are an average of 2,359 respondents who report 259 criminal victimizations, 96 of which were reported to the police. In some MSA-years the number of respondents is as few as 600 and the number of reported crimes is as few as 40. Hence, we are unable to provide crime-specific estimates of the immigration-reporting rate relationship. Nevertheless, given that most crimes by NCVS respondents are property crimes which tend to be among the poorest measured crimes in the

E. Local Average Treatment Effects

This research has uncovered evidence of meaningful crime declines in the aftermath of LAWA that survive a battery of robustness checks. Accordingly, the remainder of the paper considers how to characterize these effects. We begin by assessing whether the large effects of LAWA on crime are likely to be driven by a local average treatment effect that is uneven in its impact on the demographic composition of Arizona's Mexican immigrant population. In particular, because participation in crime among any population is so highly concentrated among young males, we test to see whether LAWA was especially likely to induce young males among the foreign-born Mexican population to leave Arizona. Figure 5 provides insight into the local average treatment effect that is induced by LAWA. In Figure 5 we plot the relative population shares for six subgroups of Mexican immigrants: children aged 0-14, prime age individuals (15-39) and older adults (age 40+). We further separate these subgroups by gender.

In Figure 5, we see that there was very little change in the number of foreign-born Mexican children in Arizona after 2008 which is to be expected given that these individuals are too young to be in the labor force and therefore to be directly affected by LAWA. Interestingly, there was no decline in the share of either male or female older adults with the share of each population continuing to increase secularly. In contrast, there are clear declines among both males and females between the ages of 15 and 39, with especially large declines among males. These changes are not trivial. In 2007, just before LAWA's passage, there were 5 prime age males for every 3 older males among Arizona's Mexican immigrant population. By 2009, just two years later, the demographic composition of this population had completely changed — there were more older males than prime age males.

Among the prime age males, the decline was especially steep for the 15-24 year old male population, which is the age-gender subgroup most likely to be arrested in the United States. This group accounts for approximately 27 percent of the overall decline in the Mexican population share despite the fact that this subgroup is only approximately 10 percent of the foreign-born Mexican population. Put differently, while young males comprised 11 percent of the foreign-born Mexican

UCR data, the overall reporting rate is an excellent proxy for the rate of underreporting for those crime types we are most concerned about.

population prior to LAWA, they comprised just 7 percent of this population by 2010. Given that the decline is concentrated most heavily in the age-gender subpopulation that is responsible for a disproportionate share of crime, further attention is warranted to sort out the degree to which results are compositional as opposed to behavioral.

The post-LAWA decline in the young male population among foreign-born Mexicans can be used to estimate the proportion of the decline in crime that was brought about by LAWA that can be attributed to a change in the demography of Arizona's Mexican immigrant population. We begin by counting the proportion of arrestees who are 15-24 year old males. In 2009, 31.4 percent of the 13.7 million U.S. arrestees for Part I. crimes were males in this age group. Likewise, according to the U.S. Census, 15-24 year old males comprised 7.2 percent of the U.S. population, making individuals in this group 4.3 times more likely to be arrested than other U.S. residents. Using this information, we can back out the expected decline in the crime rate when the 15-24 year old male share of the foreign-born Mexican population declines.

Let M_1 be the initial share of young males among the foreign-born population and M_2 be the young male share post-LAWA. Furthermore let X be the crime rate among Mexican immigrants who are not young males and let a be ratio of the share of young male offenders among young males to the share of other offenders among all others. Then the predicted percent decline in the crime rate ($\Delta\ V_p$) for a decline in the share of young males from M_1 to M_2 can be computed as:

$$\Delta V_p = \frac{(M_2 - M_1)(a-1)X}{M_1 aX + (1 - M_1)X} \tag{4}$$

The denominator of (7) is simply the initial crime rate while the numerator gives the decrease in crime owing to a decline in the young male population from M_1 to M_2. Simplifying (7) yields an expression that no longer contains X:

$$\Delta V_p = \frac{-(M_1 - M_2)(a-1)}{1 + M_1 a - M_1} \tag{5}$$

Setting $a = 4.3$ (the degree to which young males are overrepresented among arrestees) and using $M_1 = 11$ percent and $M_2 = 7$ (the empirical decline in Arizona's young male share among Mexican

immigrants) yields a predicted drop in crime of 9.8 percent. This computation can be extended so as to be crime-specific. Such calculations are presented in Table 5 which, for each crime type, computes a along with the predicted decrease in crime (ΔV_p) given that $M_1 = 0.11$ and $M_2 = 0.07$. The predicted crime drop is compared with synthetic D-D estimates (ΔV_a) presented in Panel A of Table 2.

Young males are most overrepresented among arrestees for robbery (a=7.5), burglary (6.4), murder (6.1) and motor vehicle theft (6.0). Given the values of a given in the table, a decline in the young male share of the Mexican immigrant population would be predicted to mechanically reduce robbery by 15 percent, burglary by 14 percent and murder and motor vehicle theft by 13 percent each. Comparing these predicted crime declines with those estimated from the data yields important insight. Relative to the prediction, in the aftermath of LAWA, murder, robbery, burglary and larceny implying that, conditional upon age and gender, Mexican immigrants have a protective effect on crime. For motor vehicle theft, between 33 percent and 87 percent of the decline is explained by compositional effects depending on whether the 2008 post-treatment period estimate or the 2008-2010 post-treatment period estimate is preferred. The implications of this exercise are therefore nontrivial as the crime decline that is a byproduct of LAWA is almost entirely compositional.

V. Conclusion

This research leverages a natural experiment in Arizona to estimate the contribution of Mexican immigrants to the state's crime rate. The experiment credibly isolates a local average treatment effect that is based predominantly on undocumented individuals who are shifted by a shock to enforcement and is broadly policy-relevant as a number of states have since passed or are considering expanding their use of E-Verify laws.

In the aftermath of the passage and implementation of the Legal Arizona Workers Act, there was a large and discrete and indeed unprecedented decline in Arizona's foreign-born Mexican population share relative to other states. On the other hand, the law's passage seems to have had no effect on either the foreign-born non-Mexican share, the share of U.S.-born Hispanics or on the composition

of U.S. natives. After 2008, Arizona's crime rate (particularly its property crime rate) declined by approximately 10 percent implying that the decline in the foreign-born Mexican share induced by LAWA resulted in a decline in property crimes of approximately 20 percent. The decline in crime in the aftermath of LAWA is unusual given a literature that has consistently found null or even negative effects of immigration on crime.

Further analysis of LAWA's local average treatment effect provides an answer to this conundrum as young males (aged 15-24) were especially likely to leave the state after the law's passage. Given this subpopulation's disproportionate involvement in criminal activity in any ethnic or national group, back-of-the-envelope calculations suggest that the estimated treatment effect can be accounted for by compositional changes in Arizona's Mexican population along the dimensions of age and gender and that, if anything, young Mexican men are less likely to commit crimes than young native men. As a result, this research remains broadly consistent with prior research that suggests that adjusting for age and gender, immigrants are not more likely than natives to be arrested or incarcerated. Interestingly, results in this paper are similar to those of Moehling and Piehl (2009) who arrived at similar conclusions for Italian immigrants (who were also disproportionately likely to be young and male) at the turn of the 20th century.

The relationship between immigration and crime depends critically on the age and gender composition of the migrants and specifically on the age and gender of the migrants upon whose behavior local average treatment effects are based. Changes in immigration that are induced by employer sanctions are likely to change the age and gender of the immigrant population and thus may affect crime mechanically. To the extent that this is true, we note that the presence of young men, in general, whether they were born in Mexico or in the United States, carries both costs and benefits. While young men are healthy and likely to work, they also are disproportionately involved in crime. Public debates on immigration policy must consider such nuances.

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VI. Appendix A: Synthetic Controls Methodology

Formally, let the index j = (1,2,,J) denote the J states in the United States.[24] The value j=1 corresponds to Arizona, and j=(2,,J) correspond to each of the other states that are candidate contributors to the control group.[25]

We begin by defining Y_0 as a kx1 vector with elements equal to the seven annual index crime rates and two crime aggregates (violent and property crimes) for Arizona for the 2000-2007 pre-intervention period. Likewise, we define the kx$J$ matrix Y_1 as a stack of similar vectors for each of the other J states in the donor pool. The synthetic control method identifies a convex combination of the J states in the donor pool that best approximates the pre-intervention data vectors for the treated state. Define the Jx1 weighting vector W=(w_1, w_2, , w_J)' such that:

$$(A1) \qquad \sum_{i=1}^{J} w_j = 1$$
$$(A2) \qquad w_j \geq 0 \text{ for } j=(1,,J)$$

Condition (A1) guarantees that the weights sum to 1 while condition (A2) constrains that the weights are weakly positive. The product Y_1W then gives a weighted average of the pre-intervention vectors for all states in the donor pool (omitting Arizona), with the difference between Arizona and this average given by Y_0 -Y_1W. The synthetic control method selects values for the weighting vector, W, that result in a synthetic comparison group that best approximates the pre-intervention violent crime trend in Arizona. Once the optimal weighting vector W^* is computed, both the pre-intervention path as well as the post-intervention values for the dependent variable in "synthetic Arizona can be tabulated by calculating the corresponding weighted average for each year using the donor states with positive weights. The post-intervention values for the synthetic control group serve as the counterfactual outcomes for Arizona.

Our principal estimate of the impact of LAWA on the crime rate uses the pre- and post-treatment values for both Arizona and its synthetic control group to calculate a simple D-D estimate. Specifically, define Y_{PRE}^{AZ} as the average value of the violent crime rate for Arizona for the pre-intervention

[24]The discussion in this section is drawn, in part, from a 2013 working paper by Chalfin and Raphael entitled "New Evidence on the Deterrence Effect of Harsher Sanctions: Re-examining the Impact of California Proposition 8.

[25]Excluded from the donor pool of the remaining J states are Alabama, Georgia, Mississippi and South Carolina, states that have likewise passed an expansive E-Verify law after 2008.

period 2000 through 2007 and Y_{POST}^{AZ} as the corresponding average for a defined post-treatment period, 2008-2010. Y_{PRE}^{SYNTH} and Y_{POST}^{SYNTH} are the corresponding quantities for Arizona's synthetic control group. Then the synthetic D-D estimate is given as follows:

$$DD = (Y_{POST}^{AZ} - Y_{POST}^{SYNTH}) - (Y_{PRE}^{AZ} - Y_{PRE}^{SYNTH}) \tag{6}$$

To formally test the significance of any observed relative change in Arizonas violent crime rate, we apply a permutation test suggested by Abadie, Hainmuller and Diamond (2010) and implemented by Bonn, Lofstrom and Raphael (2013) to the D-D estimator given in equation (2). Specifically, for each state in the donor pool that *did not* receive the treatment, we re-compute weights to generate a synthetic control group. Next, we re-compute the synthetic D-D estimates under the assumption that the other states, in fact, passed an E-Verify law on the same date as Arizona. Because, the causal effect of the placebo laws must be zero, the distribution of these "placebo" difference-in-difference estimates then provides the equivalent of a sampling distribution for the estimate DD_{AZ} (see Abadie, Diamond and Hainmuller 2010 for a detailed discussion).

Figure 1. Crime Trends: Arizona vs. Other U.S. States

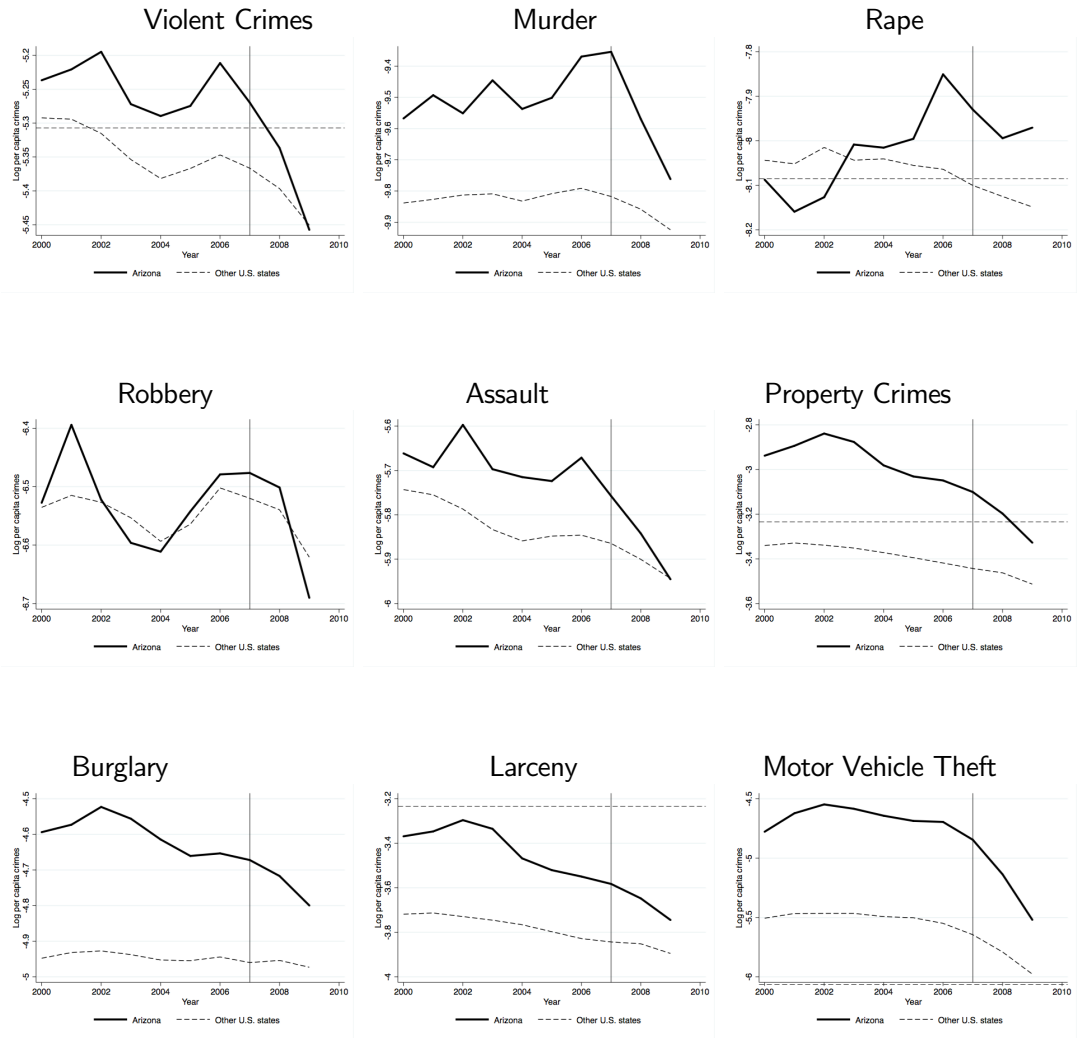


Figure 2. Synthetic "Difference-in-Difference"
Estimates of the Great Recession on Labor Market Outcomes Among Arizona Natives

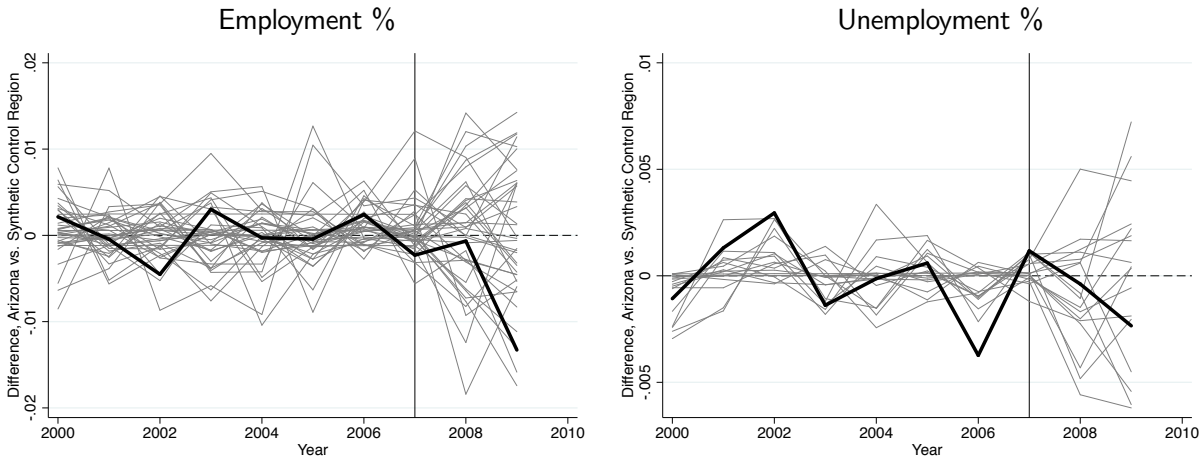


Figure 3. Synthetic "Difference-in-Difference" Estimates of the Effect of the Legal Arizona Workers Act [LAWA] on the Foreign Born Population Share

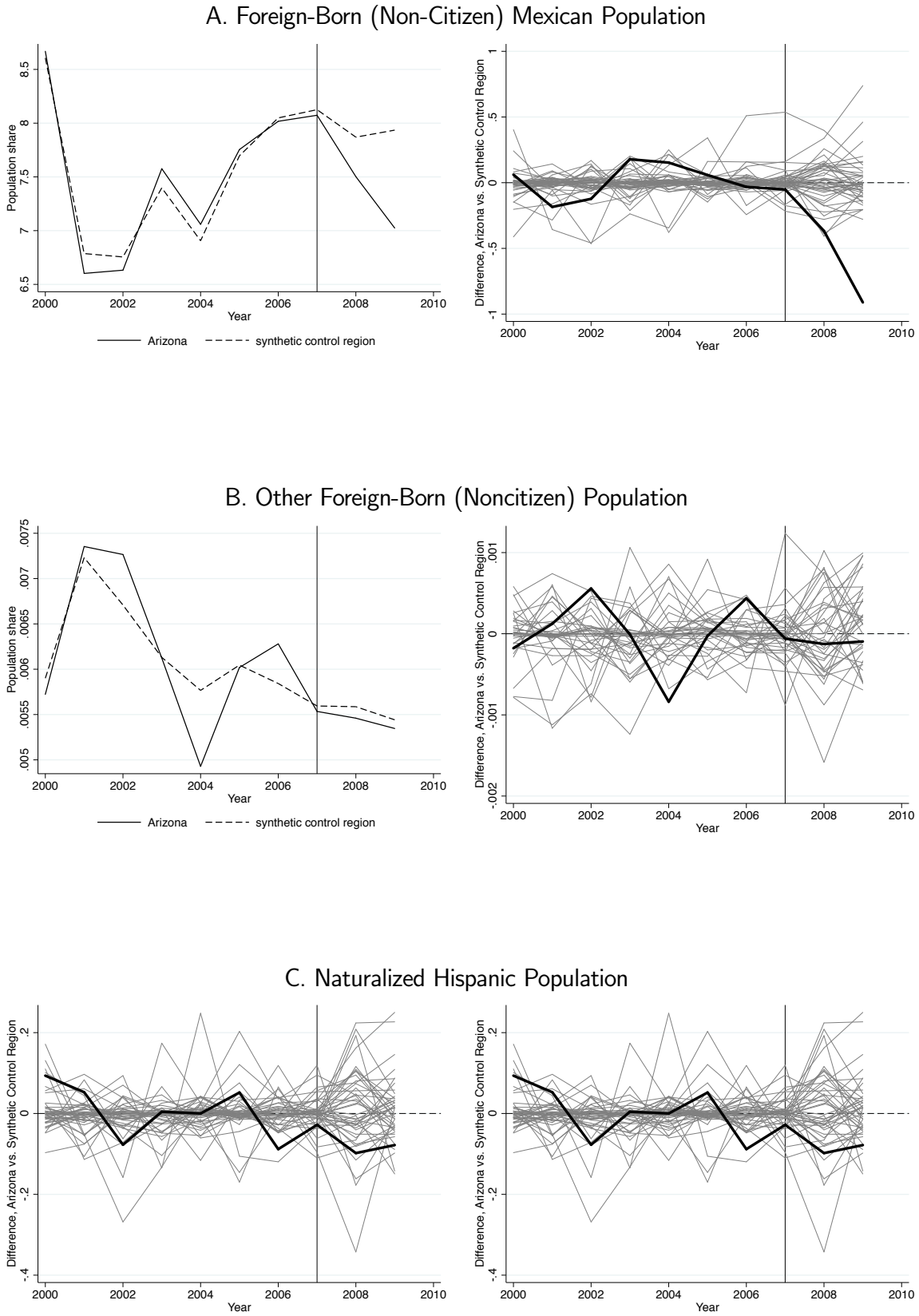
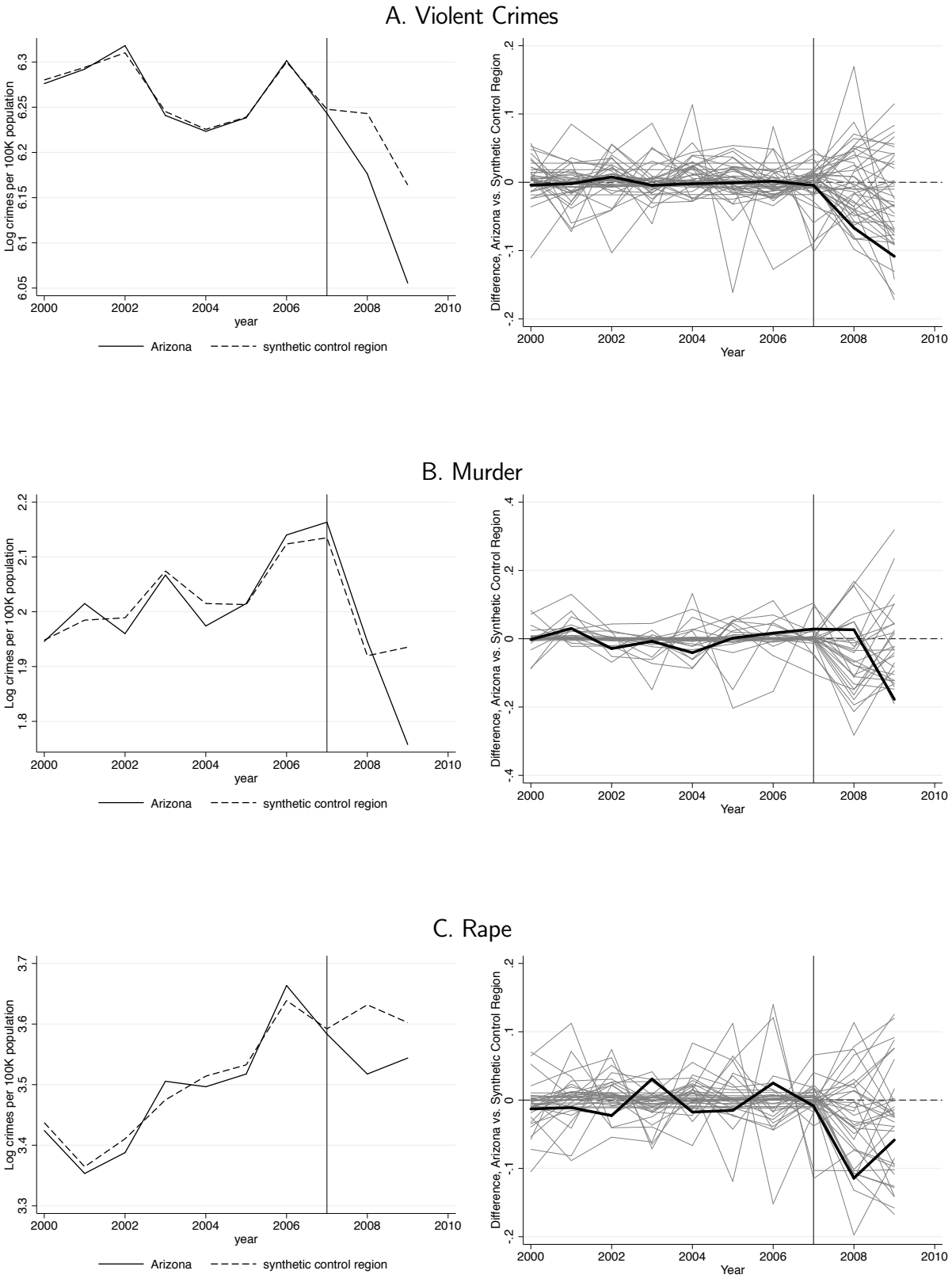
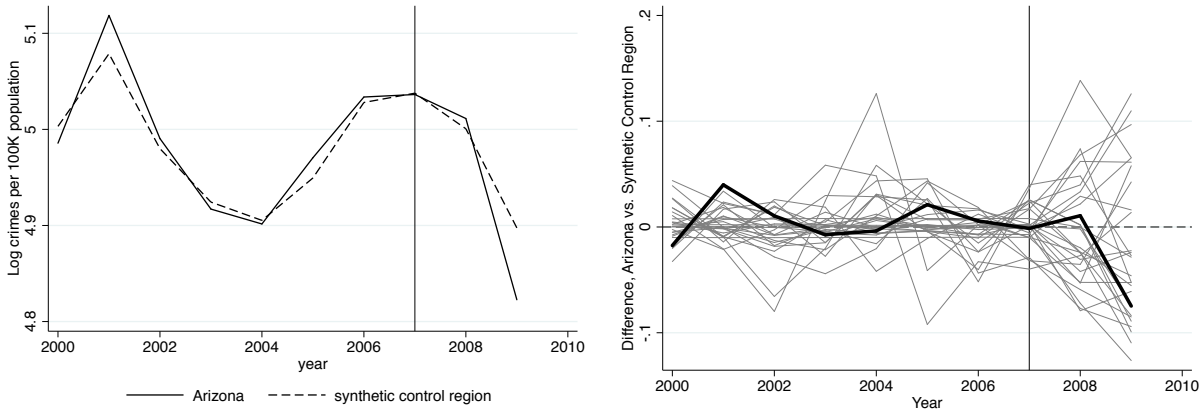


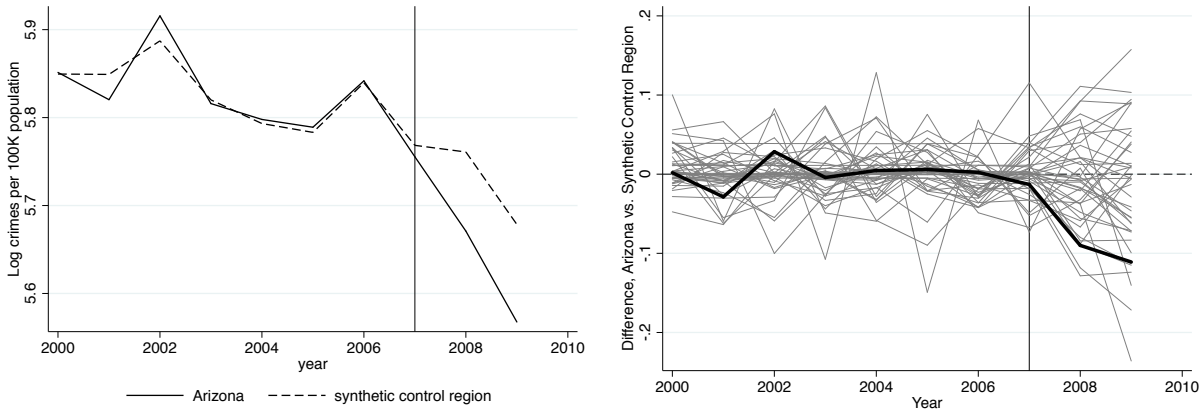
Figure 4. Synthetic "Difference-in-Difference" Estimates of the Effect of Legal Arizona Workers Act [LAWA] on Log Per Capita Crimes Reported to the Police



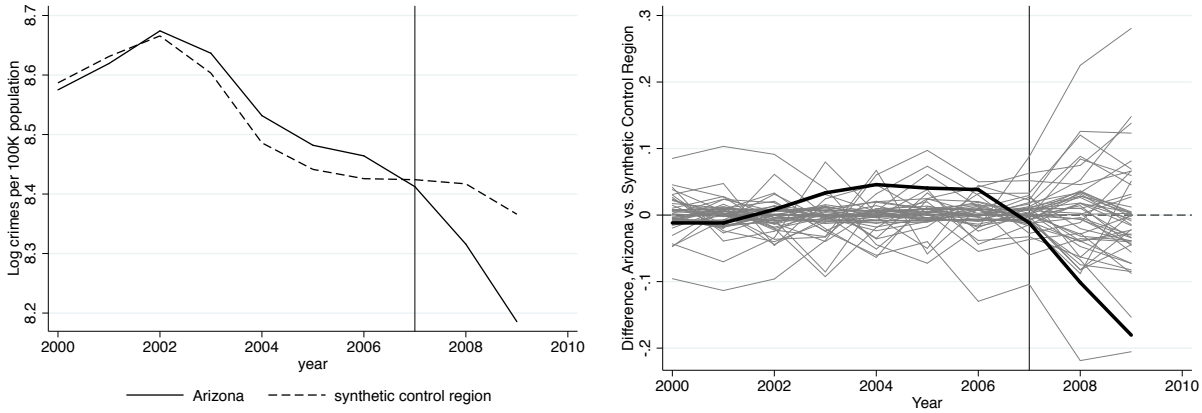
D. Robbery



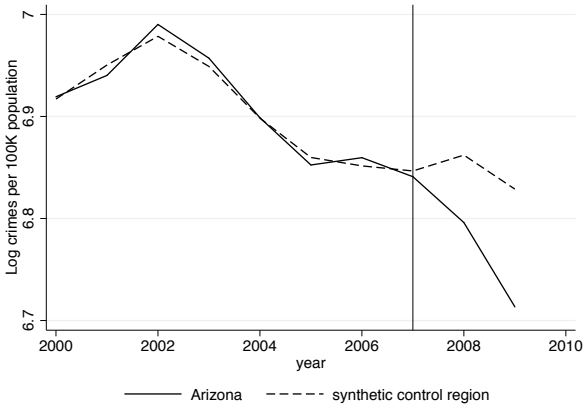
E. Assault

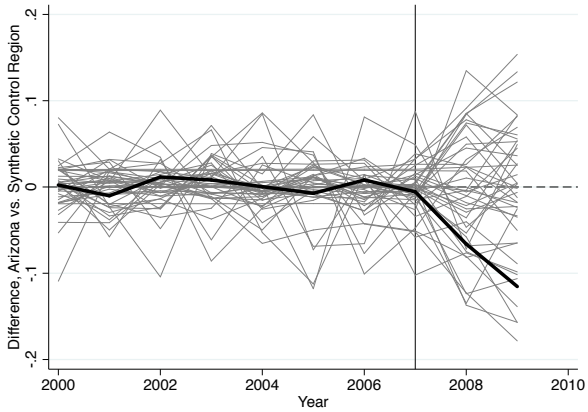


F. Property Crimes

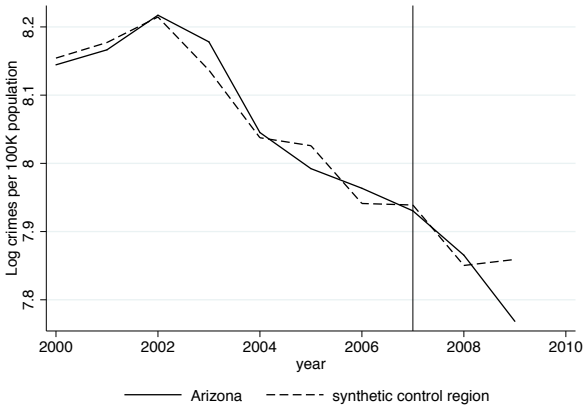


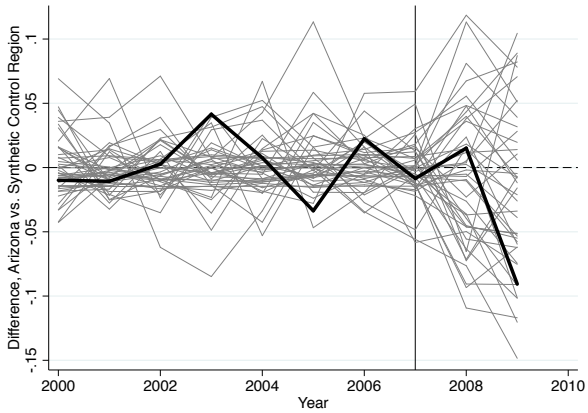
G. Burglary



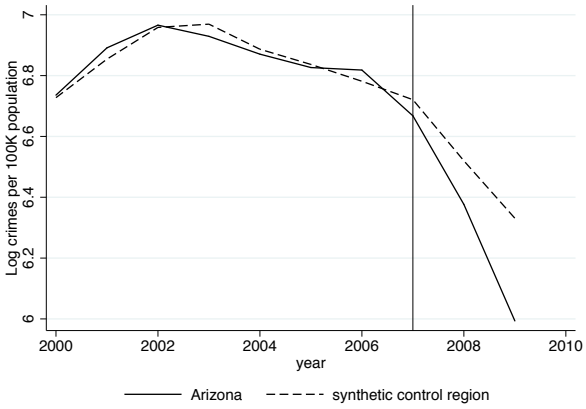


H. Larceny





I. Motor Vehicle Theft



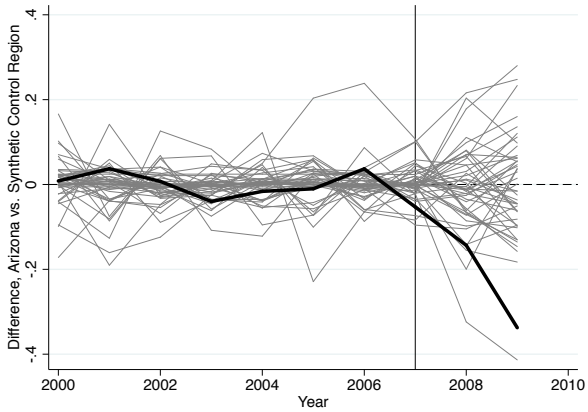


Figure 5. Foreign-Born Mexican Population Share by Age and Gender

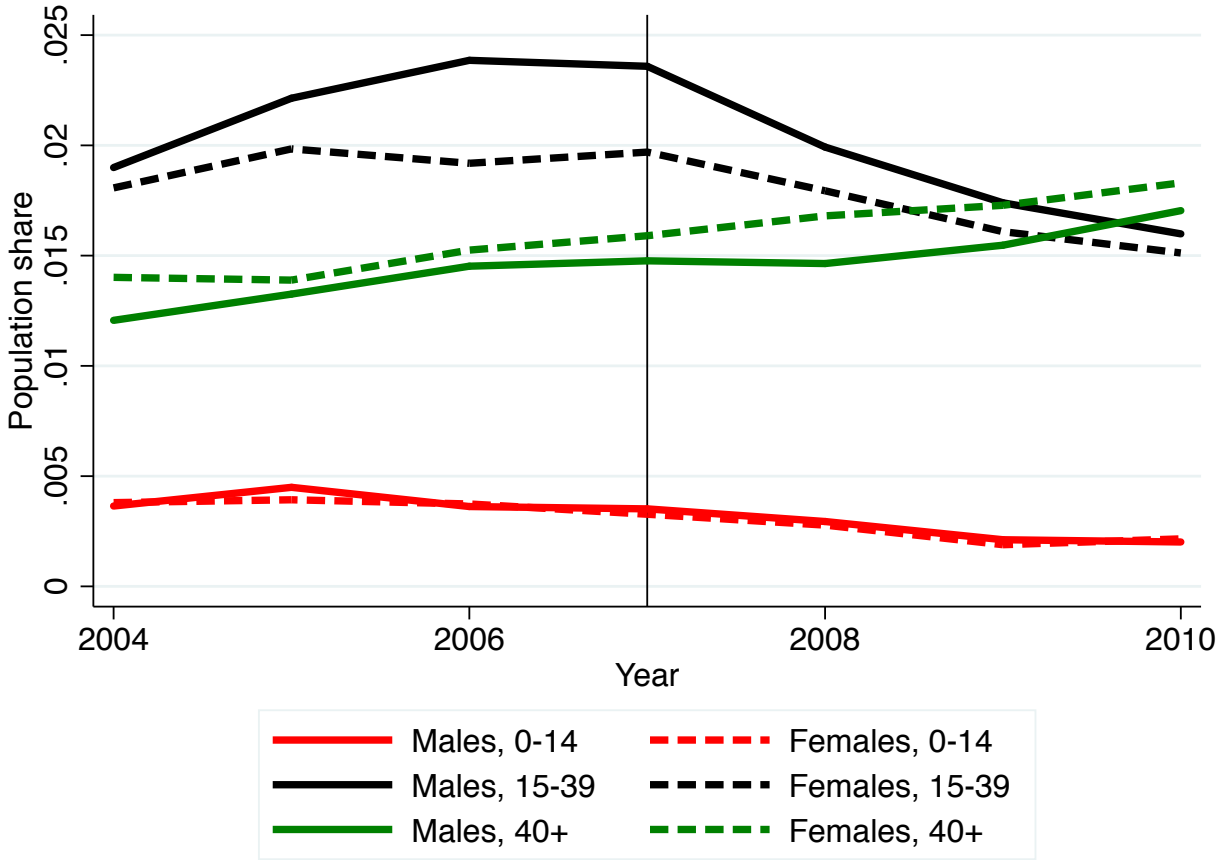


Table 1. Descriptive Statistics for Arizona, 2001-2009

	2001	2002	2003	2004	2005	2006	2007	2008	2009
Demographic variables									
% white	0.696	0.687	0.676	0.673	0.654	0.648	0.625	0.621	0.626
% black	0.024	0.021	0.023	0.027	0.026	0.027	0.030	0.031	0.033
% married	0.452	0.465	0.446	0.454	0.443	0.440	0.433	0.432	0.432
% age 0-14	0.205	0.208	0.214	0.211	0.212	0.205	0.208	0.204	0.196
% age 15-24	0.123	0.122	0.126	0.115	0.122	0.126	0.127	0.124	0.126
% age 25-39	0.193	0.193	0.186	0.187	0.186	0.189	0.187	0.187	0.179
% age 40-54	0.210	0.208	0.212	0.210	0.211	0.206	0.202	0.203	0.202
% age 55+	0.269	0.270	0.262	0.277	0.269	0.274	0.276	0.282	0.296
Nativity variables									
% non-citizen Mexican	0.049	0.051	0.058	0.053	0.058	0.062	0.061	0.055	0.050
% non-citizen non-Mexican	0.023	0.024	0.023	0.025	0.023	0.024	0.025	0.024	0.025
% non-citizen	0.072	0.074	0.081	0.078	0.081	0.086	0.086	0.079	0.074
% immigrant	0.121	0.122	0.134	0.130	0.135	0.139	0.142	0.134	0.130
Mexican share among non-citizens	0.678	0.678	0.716	0.675	0.715	0.721	0.708	0.691	0.668
Economic variables									
% in labor force	0.603	0.600	0.612	0.601	0.610	0.605	0.599	0.616	0.597
% employed	0.566	0.556	0.569	0.563	0.574	0.576	0.565	0.580	0.538
% unemployed	0.062	0.074	0.071	0.063	0.059	0.048	0.056	0.058	0.099
Industry concentration									
% employed in:									
Agriculture	0.013	0.013	0.007	0.006	0.006	0.006	0.006	0.005	0.006
Construction	0.042	0.041	0.041	0.044	0.049	0.054	0.052	0.050	0.043
Manufacturing	0.057	0.055	0.051	0.051	0.048	0.045	0.046	0.043	0.043
Restaurants	0.028	0.033	0.029	0.028	0.030	0.030	0.032	0.031	0.032
Retail trade	0.074	0.071	0.075	0.073	0.071	0.070	0.068	0.072	0.069

Note: Table reports means for selected characteristics of the Arizona population from 2001 to 2009.

Table 2. Synthetic "Differences-in-Differences" and IV Estimates of the Effect of LAWA on Index Crimes Reported to Police

	Pre-treatment period, 2001-2007	Post treatment period, 2008			Post-treatment period, 2009		
	Mean difference relative to synthetic control group	Difference relative to synthetic control group	Difference-in-Difference estimate	Implied p-value*	Mean difference relative to synthetic control group	Difference-in-Difference estimate	Implied p-value*
A. Reduced Form Estimates							
Violent Crimes	-0.001	-0.067	-0.066	0.191	-0.108	-0.107	0.128
Murder	0.000	0.026	0.027	0.723	-0.178	-0.177	0.213
Rape	-0.004	-0.114	-0.110	0.149	-0.059	-0.054	0.340
Robbery	0.006	0.011	0.005	0.638	-0.075	-0.081	0.340
Assault	0.000	-0.090	-0.090	0.170	-0.111	-0.111	0.234
Property Crimes	0.016	-0.101	-0.118	0.043	-0.180	-0.197	0.021
Burglary	0.001	-0.066	-0.067	0.170	-0.115	-0.116	0.106
Larceny	0.001	0.015	0.014	0.660	-0.091	-0.092	0.170
Motor vehicle theft	-0.004	-0.143	-0.140	0.085	-0.338	-0.334	0.043
B. First Stage Estimates							
Foreign-born Mexican population	0.007	-0.368	-0.375	0.064	-0.911	-0.918	0.021
C. Implied IV Estimates							
Violent Crimes			0.176			0.117	
Murder			-0.072			0.193	
Rape			0.293			0.059	
Robbery			-0.013			0.088	
Assault			0.240			0.121	
Property Crimes			0.315			0.215	
Burglary			0.179			0.126	
Larceny			-0.037			0.100	
Motor vehicle theft			0.373			0.364	

Note: Panel A presents synthetic"difference-in-differences" estimates of the treatment effect of the Legal Arizona Workers Act (LAWA) on seven UCR index crimes (murder, rape, robbery, aggravated assault, burglary, larceny and motor vehicle theft) and two crime aggregates (violent crimes and property crimes). These are referred to as "reduced form" estimates because they estimate the impact of the law, rather than the responsiveness of crime to Mexican immigration directly. The first column calculates the average difference between Arizona and its synthetic control region prior to the law's passage. The next set of columns present the post-treatment difference between the treatment and control group and the differences-in-differences estimate using 2008 as the post-treatment period. The implied p-value is computed by dividing Arizona's rank among the 46 states in the donor pool by 46. This is shown in Abadie, Diamond and Hainmuller (2010) to be equivalent to a p-value arising from a one-tailed test of the null hypothesis. The final set of columns report identical estimates using 2009 as the post-treatment period. As the dependent variable is the log of the crime rate, these coefficients can be interpreted as semi-elasticities (e.g., LAWA is associated with a β percent change in the crime rate). Panel B presents "differences-in-differences" estimates of the effect of LAWA on the non-citizen Mexican share of the population. These "first stage" estimates estimate the effect of the treatment on the endogenous regressor, Mexican population. LAWA is associated with a 0.46-0.96 percentage point decline in the Mexican population. In Panel C, the reduced form estimates are divided by the first stage estimates to yield implied instrumental variables estimates of the effect of Mexican immigration on crimes reported to the police. The coefficient estimates in Panel C provide estimates of the percent change in the crime rate arising from a one percentage point increase in the state's Mexican population share.

Table 3. Synthetic "Differences-in-Differences" and IV Estimates of the Effect of LAWA on Index Crimes Reported to Police Robustness Checks

	Main Estimates		Excluding Border States		Excluding Spillover States		2007 as Post-Treatment Year	
	D-D Estimate	p-value	D-D Estimate	p-value	D-D Estimate	p-value	D-D Estimate	p-value
A. Reduced Form Estimates								
Violent Crimes	-0.107	0.128	-0.132	0.167	-0.107	0.154	-0.108	0.128
Murder	-0.177	0.213	-0.202	0.214	-0.177	0.231	-0.173	0.213
Rape	-0.054	0.340	-0.105	0.310	-0.075	0.333	-0.055	0.340
Robbery	-0.081	0.340	-0.244	0.095	-0.081	0.333	-0.082	0.340
Assault	-0.111	0.234	-0.118	0.238	-0.111	0.256	-0.112	0.234
Property Crimes	-0.197	0.021	-0.203	0.048	-0.197	0.026	-0.201	0.021
Burglary	-0.116	0.106	-0.120	0.143	-0.116	0.103	-0.117	0.106
Larceny	-0.092	0.170	-0.133	0.119	-0.097	0.128	-0.093	0.170
Motor vehicle theft	-0.334	0.043	-0.338	0.048	-0.334	0.026	-0.341	0.043
B. First Stage Estimates								
Foreign born Mexican population share	-0.918	0.021	-0.597	0.024	-0.918	0.026	-0.927	0.021

Note: Panel A presents synthetic"difference-in-differences" estimates of the treatment effect of the Legal Arizona Workers Act (LAWA) on seven UCR index crimes (murder, rape, robbery, aggravated assault, burglary, larceny and motor vehicle theft) and two crime aggregates (violent crimes and property crimes). These are referred to as "reduced form" estimates because they estimate the impact of the law, rather than the responsiveness of crime to Mexican immigration directly. Panel B reports the first stage estimates of the effect of LAWA on the foreign-born Mexican population share. The first two columns replicate the main estimates presented in Table 2. The second two columns repeat the analysis excluding Arizona's border states. The next two columns exclude the states that received the largest percentage increases in immigration post-LAWA. The final two columns repeat the analysis counting 2007 as a post-treatment year, thus folding in anticipatory effects.

Table 4. "Differences-in-Differences Estimates: Agency-Level Data

	Violent crimes	Murder	Rape	Robbery	Assault	Property crimes	Burglary	Larceny	Motor vehicle theft
Panel A. Standard Estimates									
AZ × 2008-2009	-.064	-.209	-.034	.014	-.106	-.164	-.118	-.102	-.393
	(.072)	(.108)	(.060)	(.063)	(.100)	(.044)	(.055)	(.055)	(.054)
Panel B. Placebo Estimates									
AZ × 2001-2006	.058	.171	.074	.045	.065	.018	-.049	-.002	.129
	(.052)	(.076)	(.053)	(.040)	(.069)	(.024)	(.025)	(.030)	(.051)
AZ × 2007	.078	.173	.086	.148	.040	-.044	-.084	-.039	-.021
	(.112)	(.092)	(.104)	(.073)	(.152)	(.039)	(.081)	(.050)	(.082)
AZ × 2008-2009	-.009	-.053	.034	.071	-.052	-.157	-.167	-.109	-.297
	(.123)	(.117)	(.104)	(.097)	(.167)	(.061)	(.070)	(.072)	(.098)

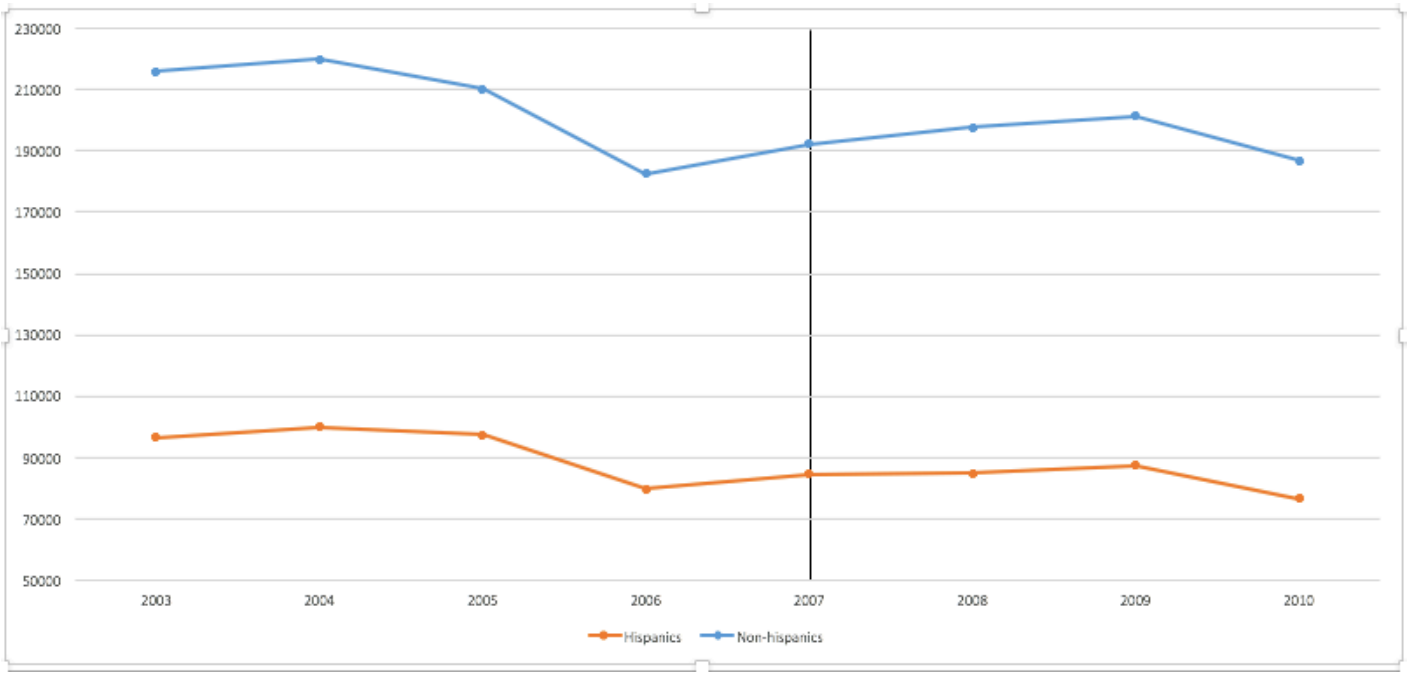
Note: For each crime type, Panel A reports coefficients and standard errors from a Poisson regression of the count of crimes on a state-level treatment dummy using monthly agency-level data. Panel B reports coefficients and standard errors on the estimated treatment effect (AZ × 2008-2010) as well as on two placebo dummies which measure the impact of the law on agency-years that are untreated. AZ × 2007 captures the difference in crime in the year prior to the passage of LAWA. AZ × 2001-2006 captures the difference in crime in Arizona in the 2001-2006 period. All models condition on population as well as agency and year fixed effects. Standard errors are clustered at the agency level.

Table 5. Model-Based Estimates of the Importance of LATE

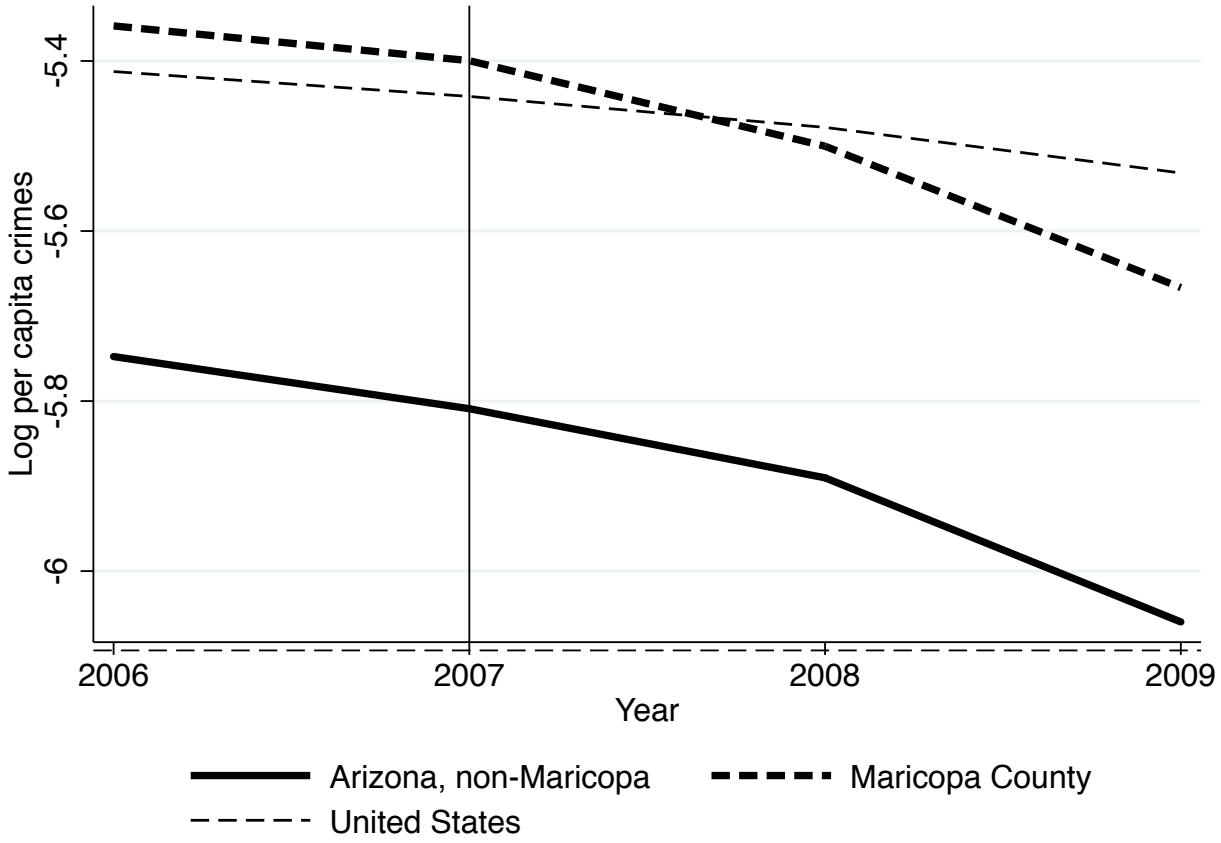
	Violent crimes	Murder	Rape	Robbery	Assault	Property crimes	Burglary	Larceny	Motor vehicle theft
Panel A. Arrestees									
Males, ages 15-24	195,907	5,445	8,099	68,841	113,522	525,083	139,422	350,260	35,401
All arrestees	581,765	12,418	21,407	126,725	421,215	1,716,081	299,351	1,334,933	81,797
Young adult male share	0.337	0.438	0.378	0.543	0.270	0.306	0.466	0.262	0.433
a	4.7	6.1	5.2	7.5	3.7	4.2	6.4	3.6	6.0
Panel B. Model-Based Predictions									
ΔV_p	-0.104	-0.130	-0.116	-0.152	-0.084	-0.095	-0.136	-0.082	-0.129
ΔV_a	-0.089	-0.074	-0.151	-0.017	-0.147	-0.128	-0.068	-0.018	-0.148

Note: For each Part I. crime, Panel A presents data on the total number of 2009 arrestees as well as the share of arrestees who are 15-24 year old ("young adult" males. Using an estimate of the young adult male share of the U.S. population of 7.2 percent, Panel B computes a, the degree to which young adult males are overrepresented mong arrestees relative to their share of the population. ΔV_p is the model-based predicted crime drop given $M_1 = 0.11$ and $M_2 = 0.07$, the initial and post-treatment young adult male shares among Mexican immigrants in Arizona. ΔV_a is the synthetic D-D estimate of the actual decline in crime post-LAWA using 2008 as the post-treatment period.

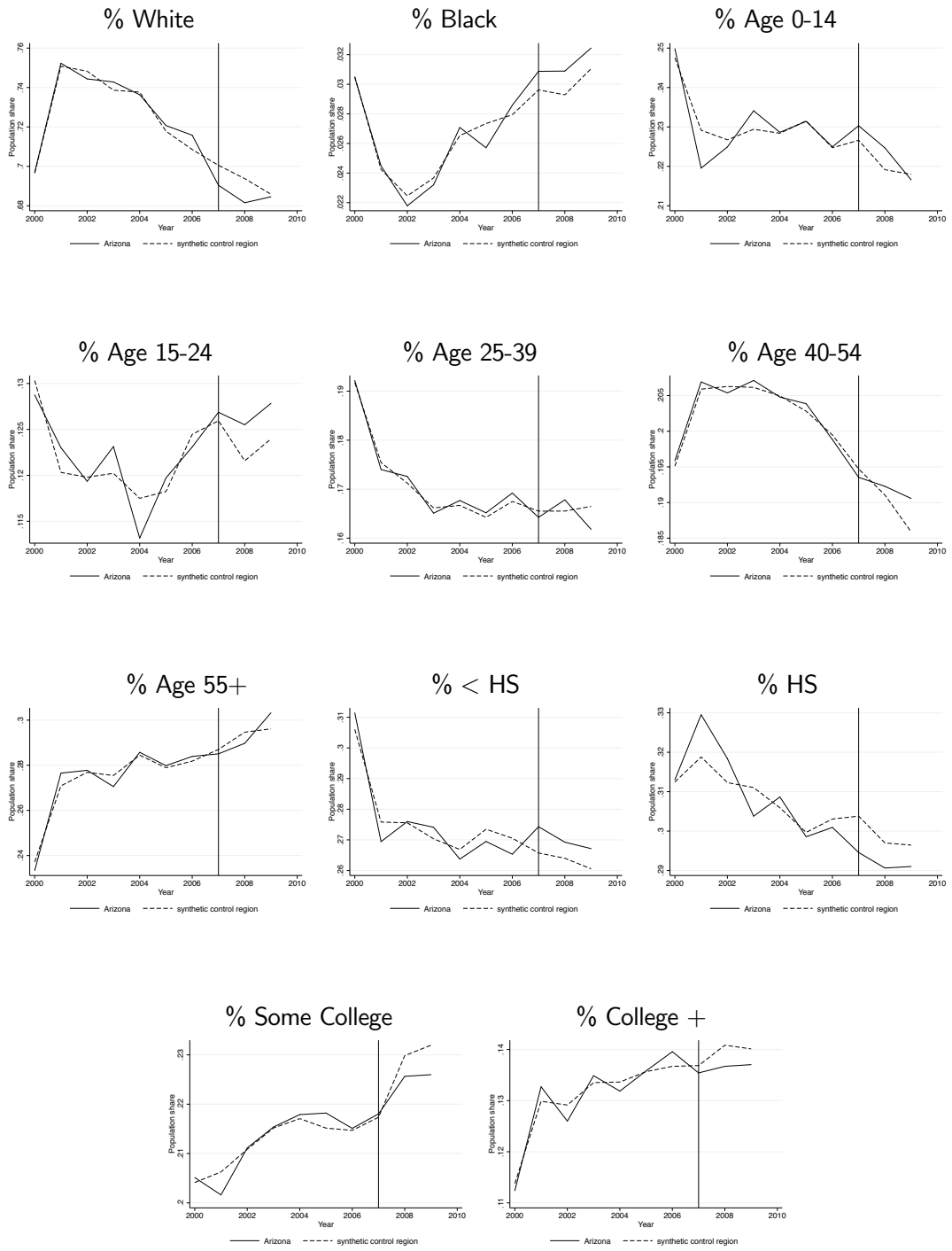
Appendix Figure 1. Hispanic and Non-Hispanic Arrests in Arizona



Appendix Figure 2. Log Index Crime Rates, Maricopa County vs. Other Arizona Counties



Appendix Figure 3. Synthetic "Difference-in-Difference" Estimates of the Effect of Legal Arizona Workers Act [LAWA] on the Composition of Arizona Natives



Appendix Table 1. Composition of Synthetic Comparison Group by Index Crime Type

State	Violent crimes	Murder	Rape	Robbery	Assault	Property crimes	Burglary	Larceny	Motor vehicle theft	Foreign-born Mexican share
AK	0	.032	0	0	0	0	0	0	0	0
AR	0	.262	.232	0	0	0	0	0	0	0
CA	0	0	0	0	0	0	0	0	0	.387
CO	.003	0	0	0	.037	0	0	0	0	0
CT	0	0	0	0	0	0	0	0	0	0
DC	.11	.019	0	.016	.311	.609	.094	.175	.534	0
DE	0	.029	0	0	0	0	0	0	0	0
FL	0	0	0	0	0	0	0	0	.137	0
HI	0	0	0	0	0	.083	.243	.563	0	0
IA	0	0	.3	0	.33	0	0	0	0	0
ID	0	0	0	0	0	0	0	0	0	0
IL	0	0	0	0	0	0	0	0	0	0
IN	0	.097	0	.082	0	0	0	0	0	0
KS	0	0	.127	0	0	0	0	.061	0	0
KY	0	0	0	0	.092	0	0	0	0	0
LA	0	.316	0	0	0	0	.037	0	0	0
MA	0	0	0	0	0	0	0	0	.031	0
MD	.432	0	0	0	.17	0	0	0	0	0
ME	0	0	0	0	0	0	0	0	0	0
MI	0	0	0	0	0	0	0	0	0	0
MN	0	0	0	0	0	0	0	0	0	0
MO	.131	0	0	0	0	0	0	0	0	0
MT	0	0	.096	0	0	0	0	0	0	0
NC	0	0	0	0	0	0	.305	0	0	0
ND	0	.048	.045	0	0	0	0	0	0	0
NE	0	0	0	0	0	0	0	0	0	0
NH	.033	0	.097	0	0	0	0	0	0	0
NJ	0	0	0	0	0	0	0	0	0	0
NM	0	0	0	.263	0	0	0	0	0	0
NV	.037	.198	0	0	0	0	0	0	0	.16
NY	0	0	0	0	0	0	0	0	0	0
OH	0	0	0	.161	0	0	0	0	0	0
OK	0	0	0	0	0	0	0	.065	0	0
OR	0	0	0	0	0	0	0	.136	0	0
PA	0	0	0	0	0	0	0	0	0	0
RI	0	0	.078	.011	0	0	0	0	0	0
SD	.132	0	.026	0	.06	0	0	0	0	0
TN	0	0	0	.466	0	0	.321	0	0	0
TX	0	0	0	0	0	0	0	0	0	.28
UT	0	0	0	0	0	0	0	0	0	.172
VA	.122	0	0	0	0	0	0	0	0	0
VT	0	0	0	0	0	0	0	0	0	0
WA	0	0	0	0	0	.308	0	0	.299	0
WI	0	0	0	0	0	0	0	0	0	0
WV	0	0	0	0	0	0	0	0	0	0
WY	0	0	0	0	0	0	0	0	0	0

Note: Each row reports the percentage contribution of a given state to the synthetic control region for Arizona for a given crime type. Save for rounding errors, the percentages sum to 100 along the columns.

Appendix Table 2. Synthetic Instrumental Variables Estimates: Robustness to Imposing a Common Set of Weights

	Violent crimes	Murder	Rape	Robbery	Assault	Property crimes	Burglary	Larceny	Motor vehicle theft
Natural weights	0.117	0.193	0.059	0.088	0.121	0.215	0.126	0.100	0.364
Common weights	0.065	0.391	-0.071	0.076	0.090	0.186	0.130	0.136	0.424

Note: For each Part I. crime, the top row in the table presents the implied instrumental variables estimates reported in Table 2. These were generated by dividing the synthetic reduced form estimate for each crime by the synthetic first stage estimate of the effect of LAWA on the foreign-born Mexican population share. In the second row, we impose a common set of weights on the reduced form and first stages, in both cases predicting outcomes using a predictor that is the average of the foreign-born Mexican population share and the relevent log crime rate.