New Evidence on Mexican Immigration and U.S. Crime Rates: A Synthetic Control Study of the Legal Arizona Workers Act *

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Abstract

I leverage a natural experiment created by recent legislation in the U.S. state of Arizona to estimate the impact on crime of a large and discrete decline in the state's foreign-born Mexican population. In order to isolate the causal effect of the legislation on crime, I leverage a synthetic "differences-in-differences" estimator, using a new method of counterfactual estimation proposed by Abadie, Diamond and Hainmuller (2010). In contrast to previous literature, I find large effects of Mexican immigration on Arizona's property crime rate. Results are driven primarily by the fact that the population decline was especially concentrated among among young males.

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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. A research literature in both economics and criminology suggests that, at a minimum, immigration has played no role in this historic decline in crime (Butcher and Piehl 1998b, Reid, Adelman, Weiss and Jaret 2005, Chalfin 2013a). Indeed some authors have identified immigration as having contributed importantly to the decline in crime (Butcher and Piehl 1998a; Lee, Martinez and Rosenfeld 2001; Stowell, Messner, McKeever and Raffalovich 2009; Ousey and Kubrin 2009; Martinez, Stowell and Lee 2010; Wadsworth 2010; MacDonald, Hipp and Gill 2012). While the extant 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 is associated with increases in criminal activity (Espenshade and Calhoun 1993; Muste 2012).[1]

Though 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. In particular, there is little research that addresses the criminal participation of recent Mexican immigrants.[2] 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.

Second, estimates of the effect of immigration on crime available in prior literature 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. cities. To the

[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] Spenkuch (2012) and Chalfin (2013a) offer the first analyses that disaggregate the effect of immigration on crime by Mexican nationality. Chalfin(2013a) 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 (2012), using county level data, finds important effects on crimes with a pecuniary motive.

extent that migrants select into U.S. cities on the basis of city-specific characteristics, standard regression estimates will return an inconsistent estimate of the effect of immigration on crime. The vast majority of the prior literature does little to address these concerns (Butcher and Piehl 1998b; Spenkuch 2012; Chalfin 2013a).[3]

Because it is generally difficult to leverage credibly exogenous variation in destination-specific immigrant flows, there is promise in searching for a natural experiment. In the spirit of Card's seminal 1990 paper on the labor market impacts of the Mariel Boatlift on Miami's labor market, in this study, I leverage a natural experiment created by recent legislation in Arizona to estimate the impact of an extremely large and discrete decline in the state's foreign-born noncitizen Mexican population. I show that Arizonas noncitizen Mexican population decreased by as much as 20 percent in the wake of the states 2008 implementation of the Legal Arizona Workers Act (LAWA), a broad-based "E-Verify" law coupled with severe sanctions for noncompliance. By contrast, the law appears to have had no effect on the state's share of other noncitizens, U.S.-born Hispanics or U.S.-born low skilled workers. In order to isolate the causal effect of the passage and implementation of LAWA on crime, I employ a synthetic "differences-in-differences" estimator, using a new method of counterfactual estimation proposed by Abadie, Diamond and Hainmuller (2010). To calculate a direct estimate of the effect of Arizona's Mexican immigrant share on its crime rate, I extend the synthetic differences-in-differences framework to construct implied synthetic instrumental variables estimates, using LAWA as an instrument for the Mexican population share. In contrast to previous literature, I find significant and large effects of Mexican immigration on Arizona's crime rate. The estimates are robust to a variety of specification checks including changing the composition of the synthetic comparison group as well as using agency-level and monthly data and are supported by

[3]Only a handful of papers in the prior literature attempt to explicitly address concerns over the endogeneity of the timing and concentrations of immigrant location decisions. Each of these papers uses an instrumental variables strategy pioneered by Altonji and Card (1991) in which the historic distribution of country-specific immigration among counties, cities or neighborhoods is used to predict the current spatial concentration of immigrants. Because the instrument relies on the presence of immigrant networks, it is known as the "network instrument." Using data from the 1980s, Butcher and Piehl (199b) present estimates using 45 U.S. MSAs and find no evidence of an effect of immigration on crime. On the other hand, Spenkuch (2012), 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 shares predict a decline in crime. While the network instrument is likely an improvement upon conventional least squares estimates of the effect of immigration on crime, several authors point out that the network instrument is potentially biased in the presence of persistent pull factors that attract or repel immigrants to U.S. destinations (Pugatch and Yang 2011; Chalfin and Levy 2013).

a series of placebo tests that examine the impact of dummy E-Verify laws in states that never received one. However, the results are driven predominantly by the fact that LAWA resulted in especially large declines among Mexican migrants who are young and male and, as such, the effects are largely compositional. Indeed, for most crimes, the treatment effect is fully explained by age and gender composition. For motor vehicle theft, I estimate that between one third and 87 percent of the decline in crime that is associated with LAWA can be attributed to compositional changes among Arizona's foreign-born Mexican population.

The remainder of this paper is organized as follows. Section II provides a brief review of theoretical linkages between immigration and crime that are found in the extant literature. Section III describes the Arizona Legal Workers Act and its "E-Verify" provisions. Section IV motivates the identification strategy and describes the modeling framework. Section V provides a description of the data employed in the study. Section VI presents results, robustness checks and considers the local average treatment effect of the legislation and Section VII concludes.[4]

II. Theoretical Links Between Immigration and Crime

While the majority of empirical work that examines links between immigration and crime has appeared in the past two decades, interest among policymakers in the relationship between the two variables goes back at least a century. In this section, I briefly summarize theoretical arguments either in favor of a positive or a negative causal relationship between immigration and crime. For a more detailed treatment, I note that theories of immigrant criminality have been ably summarized by Reid, Adelman, Weiss and Jaret (2005), Ousey and Kubrin (2009) and MacDonald and Saunders (2012), among others.

The degree to which immigration and crime are related at a macro level is nuanced and depends on the types of migrants that the United States tends to attract as well as contextual factors at work in receiving communities. Economists have tended to focus on selection among migrants (see, for example, Borjas 1999) while criminologists and sociologists have written at length about

[4] In the working paper version of this manuscript, I also include a theoretical model of immigrant offending that seeks to characterize analytically the conditions under which an empirical estimate of the effect of immigration on crime will be conservative. It is omitted from this submission in the interest of space.

social forces which inform migrants' experiences in the United States. Generally, immigration can contribute to U.S. crime rates through one of three channels. First, immigrants to the United States may differ from natives according to characteristics that are typically observed by researchers. The most important of these characteristics are age and gender which criminologists have long and convincingly linked to participation in crime. To the extent that differences in criminal propensities among immigrants and natives are explained by observable characteristics, the differences are purely compositional and, as such, the contribution of immigration to U.S. crime rates is more or less mechanical.[5]

A second way in which immigration can affect the U.S. crime rate is through selection on characteristics that are typically unobserved by researchers. These characteristics include personality traits such as intelligence, motivation and impulsiveness – traits that have been shown to predict criminal involvement but are difficult to measure in national samples. An alternative but related possibility is that migrants bring with them different tolerances for risk and, as such, respond differently than natives to traditional criminal justice policy levers such as police and prisons. To the extent that migrants differ systematically from natives along unobserved dimensions, differences in criminal involvement will persist even if the demographic composition of immigrants is similar to that of natives.[6]

Economic theories of migration have posited that selection of migrants to the United States is a function of differences in the distribution of earnings between the United States and a candidate source country. In particular, economic theory assumes that individuals migrate from a relatively poor country (e.g., Mexico) to a relatively wealthy country (e.g., the United States) in search of higher earnings (Borjas 1999). Since the earnings gap between Mexico and the United States is largest at the lowest portion of the skills distribution, Mexican migration to the United States

[5] An example of such compositional effects can be found in a historical analysis in Moehling and Piehl (2009) who examine the criminality of migrants to the United States in the early 20th century. They find that Italian immigrants were considerably more likely than U.S. natives (and other immigrants) to end up incarcerated in the United States. However, this finding is no longer true when examining age- and gender-specific arrest rates. Italian immigrants were more likely to be involved in crime because they were substantially more likely than other immigrants to be young and male.

[6] Duncan and Trejo (2013) note that while Mexican immigrants have lower levels of education, on average, than U.S. natives, they are nevertheless likely to be drawn from the upper half of the Mexican skill distribution. This fact may have implications for the degree to which Mexican immigrants are negatively selected with regard to criminal participation along unobservables.

is predicted to be concentrated among those with less valuable labor market skills. Empirical support for this theory of migration has been mixed. However, even if this theory of migration is empirically valid, it has little to say about selection of migrants along dimensions related to criminal propensities. To the degree that these earnings can be either licit or illicit, economic theory cannot generate obvious predictions about how migrants differ according to their criminal propensities. Moreover, given that migrants are selected according to their expected earnings in the U.S., if a subset of these migrants experience an unexpected lack of viable employment options, it is possible that these individuals may be especially willing to turn to criminal activity to compensate for their poor draw in the distribution of earnings (Chalfin 2013a). On the other hand, if migrants are selected according to their earnings potential in the U.S., to the degree that earnings potential is positively correlated with characteristics that are negatively associated with participation in crime, selection may work in the opposite direction.

A third possibility is that, independent of any underlying differences between migrants and natives, contextual variables that shape the experiences of migrants and natives alike may either incentivize or deter crime. Examples of the types of contextual variables that can inform the relationship between immigration and crime are numerous and suggest that the relationship between the two variables is complex. Theories that suggest a positive association between immigration and crime generally focus on material hardship, social disadvantage, and a lack of social cohesion (Bankston 1998; DeJong and Madamba 2001). With regard to material hardship, migrants may engage in crimes with a pecuniary motive as a means of supplementing their incomes out of necessity born out of a lack of opportunities for legitimate earnings (Freeman 1996). Likewise, the substantially lower wages faced by Mexican immigrants suggests enhanced incentives to participate in crime in order to supplement one's legitimate earnings.[7] A more dynamic version of this story posits that sustained material deprivation may lead individuals to engage in violent crimes as an expression of rage or frustration (Blau and Blau 1982; Angew 1992).

[7] An economics literature documenting theoretical linkages between the wage and the opportunity cost of crime can be traced back to Becker (1968). Other references include Ehrlich (1973, 1976) and Grogger (1991). Recent surveys of the relationship between wages and crime can be found in Mustard (2010) and Chalfin and Raphael (2011).

III. Institutional Setting

In this paper, I leverage a large and discrete change in Arizona's noncitizen Mexican population following the passage and implementation of the Legal Arizona Workers Act (LAWA) to estimate the contribution of Mexican immigrants to crime. LAWA's primary provision is a broad-based mandate that employers verify the legal work eligibility of all new hires using a federal database known as "E-Verify." This section provides a brief description of both E-Verify as well as LAWA and argues that the timing of LAWA's implementation in Arizona is plausibly exogenous.

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 countrys interior. 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 incentive 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.[8]

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.[9] 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

[8]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 IRCAs anti-discrimination provision.

[9]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.

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 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.[10] 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.[11] 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.[12] 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 states workforce. [13] 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

[10]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.

[11]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.

[12]say what this cost is.

[13]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.

license with the second offense carrying a potential penalty of revocation.[14]

As of January 2011, Arizona accounted for just over 7 percent of businesses nationwide that 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 Bonn, Lofstrom and Raphael (2011) 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 (Bohn, Lofstrom and Raphael 2013). 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 VI 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, I examine the impact of LAWA on the foreign-born (noncitizen) Mexican population, a population that has been shown to be both largely undocumented as well as the largest contributor to the undocumented population.[15] 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 subpopulation of immigrants whose population numbers are affected by the law. I provide evidence in Section VI that this is the case. Before I present results, however, the following section provides

[14]As Bohn, Lofstrom and Raphael (2013) 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)."

[15]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.

context for thinking about the timing of LAWA's passage as being plausibly exogenous.

C. Threats to Internal Validity

Following Bohn, Lofstrom and Rahpael's 2013 study of the effect of LAWA on Arizona's demographic composition, the identification strategy employed in this research relies on the exogeneity of LAWA's timing. In other words, the consistency of treatment effects estimated in Section VI are valid only if the timing of LAWA's passage and subsequent implementation is as good as random. Threats to validity include the possibility that LAWA was passed in response to an increase in crimes committed by immigrants or a factor that is correlated with crime such as the strength of the state's economy or trends in employment conditions. Likewise, estimates in Section VI cannot be interpreted as causal if LAWA's timing coincided with important changes in federal immigration enforcement that differentially affected Arizona. This section considers potential threats to internal validity of the differences-in-differences estimator described in the following section.

As Bohn, Loftsrom and Raphael (2013) 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. 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. 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, 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.[16]

Even if the timing of LAWA's passage and implementation was as good as random, estimated treatment effects can only be thought of as causal to the extent that LAWA's passage did not coincide with other changes in crime markets that differentially affected Arizona relative to other U.S. states. The remainder of this section considers specific potential confounders — namely the

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

rollout of the "Great Recession" which differentially affected Arizonas construction-heavy economy and unrelated changes in federal immigration enforcement during the post-treatment period. LAWA was considered and initially implemented during a period of broad economic growth. However, the great recession began to roll out in late 2008 and, to the extent that it differentially affected Arizona's labor markets and thus its crime market, the great recession has the potential to confound differences-in-differences estimates of the effect of LAWA on crime.[17] To address this concern, I control extensively for pre-treatment trends in Arizona's unemployment rate, its employment-to-population ratio and employment shares in construction, 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 I show in Section VI, this condition is satisfied.

Finally, it is important to consider whether changes in federal immigration policy coincide with the timing of LAWA's implementation. Bohn, Loftsrom and Raphael 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 DHS' rollout 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 rollout, local police agencies were given the choice to voluntarily

[17] There is evidence that Arizona was differentially affected by the 2008 recession as Arizona's economy is disproportionately reliant on the construction industry. However, Arizona's decline in construction employment broadly mirrors drops in other states.

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. While relatively few of these individuals have been removed by federal authorities, it is not entirely possible to separate the effect of Secure Communities from that of LAWA in the years after 2008.[18] Therefore, in Section VI, all results are shown using 2008-2010 as the post-treatment period and using 2008 only. Happily, the results are similar whichever post-treatment period is employed.

IV. Empirical Strategy

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

The standard approach to computing "differences-in-differences" (D-D) estimates of the effect of a state-level policy shock is to regress a state- and time-varying outcome, Y_{it} on a treatment dummy, D_{it}, a vector of time varying covariates, X_{it} and state and year fixed effects, ψ_s and ϕ_t, respectively:

$$Y_{it} = \alpha + \beta D_{it} + X_{it}'\delta + \psi_s + \phi_t + \varepsilon_{it} \tag{1}$$

In (1), β yields an estimate of the treatment effect of the policy shock.[19] Typically regression estimates are computed using weighted least squares such that the comparison group for the treated state(s) is a population-weighted average of other U.S. states and the confidence interval around β is typically computed by clustering the standard errors at the state level.[20] The identifying assumption under which β represents a causal estimate of the effect of D_{it} is that the treated state(s) and the comparison states experience parallel trends but for the treatment. Naturally the degree to which

[18] In a recent working paper, Chalfin, Loeffler and Treyger (2013) examine the impact of Secure Communities on crimes reported to police and find little evidence of crime declines in response to program roll-out.

[19] Absorbing the covariates and fixed effects, this can be seen by considering that E[Y_{it}| D=0] = α and E[Y_{it}| D=1] = $\alpha + \beta$. Thus E[Y_{it}| D=1] – E[Y_{it}| D=0] is β, the D-D estimate of the effect of the treatment.

[20] Bertrand, Duflo and Mullainathan (2003) show that clustering the standard errors is the only reliable means of accounting for arbitrary unit-specific serial correlation.

this assumption holds depends on the appropriateness of using (population-weighted) untreated U.S. states as a control group for (population-weighted) treated state(s). While the identifying assumptions of the D-D estimator are well understood, in practice, researchers rarely provide a direct test of the validity of this assumption.[21]

B. 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, I employ a new method of counterfactual estimation developed by Abadie, Diamond and Hainmuller in an influential 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 states, represents the latest advance in the estimation of treatment effects for discrete aggregate-level policy interventions.[22] 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 (i.e., 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.

The methodology represents an advance on designs that select comparison states based on arbitrary or ad hoc criteria and standard two-way fixed effects D-D estimators which implicitly use a population-weighted or unweighted 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 ensure that the resulting D-D estimate is not the result of an intervention whose timing is insufficiently random. In this section, I provide a formal treatment of the "synthetic

[21] In principle, (1) can also be estimated without population weights or using some other weighting scheme. Regardless, as long as the choice of weights is arbitrary with respect to the parallel trends assumption, regression-based D-D estimators will potentially suffer from the above problem.

[22] 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.

control methodology used to estimate the effect of LAWA on state-level crime rates.

Formally, let the index j = (1,2,,J) denote the J states in the United States.[23] 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.[24]

I 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, I 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.

My 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 period 2000 through 2007 and Y_{POST}^{AZ} as the corresponding average for a defined post-treatment

[23]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.

[24]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, 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:

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

To formally test the significance of any observed relative change in Arizonas violent crime rate, I 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, I re-compute weights to generate a synthetic control group. Next, I 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).

C. The Synthetic Instrumental Variables Estimator

The synthetic D-D estimator described in the prior section computes estimates of the average treatment effect of LAWA on state-level crime rates. In this paper, I present evidence that the passage and implementation of LAWA appears to both reduce Arizona's noncitizen Mexican population and its crime rate substantially. While the reduced form effect of LAWA on crime is of interest, the parameter that has been of greater interest in prior literature is the effect of an increase in a state's *Mexican population share* on crime. Using the fact that LAWA induced emigration of Mexican immigrants from Arizona, in this section, I show that the synthetic D-D 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 noncitizen Mexican population share which is potentially endogenous and Y is the crime rate. Re-writing in terms of covariances, we get:

$$\beta^{IV} = \frac{cov(D_i, Y_i)}{cov(D_i, M_i)} \tag{3}$$

Equation (3) provides intuition for how the implied synthetic IV estimator can be constructed. Dividing both the numerator and denominator in (3) by $var(D_i)$ yields the following characterization of the IV estimator:

$$\beta^{IV} = \frac{cov(D_i, Y_i)}{var(D_i)} / \frac{cov(D_i, M_i)}{var(D_i)} \tag{4}$$

Examining (4), 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:

$$DD^{RF} = (Y^{AZ}_{POST} - Y^{SYNTH}_{POST}) - (Y^{AZ}_{PRE} - Y^{SYNTH}_{PRE}) \tag{5}$$

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

Finally, the synthetic IV estimator is constructed as $\frac{DD_{RF}}{DD_{FS}}$, that is by dividing the D-D estimate in (5) by the D-D estimate in (6). 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 noncitizen 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. Since the synthetic D-D estimator implicitly assigns different weights to states in both the first stage and the reduced form equation, this condition will not be met. One solution is to re-estimate both the first stage and reduced form equations using the same weights in each equation. However, in practice, this approach is difficult because the quality of the synthetic match depends heavily on matching on lagged values of the dependent variable. A second approach is to simply control for past values of the non-citizen Mexican share and observe if the results differ from the original approach. Such results are reported in Appendix A. The results are consistent with those presented in Section VI.

V. Data

Data used in this research are drawn from two primary data systems. Crimes reported to police were obtained from the U.S. 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. 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.[25] The majority of the analyses reported in the paper utilize monthly agency-level data that have been aggregated to the state-year. In an auxiliary analysis, I report results using the higher frequency quarterly and monthly data.

Data on the foreign-born noncitizen 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, I calculate the share of the population in each year that is (1) noncitizen Mexican, (2) noncitizen other than Mexican and (3) U.S.-born Hispanic. I also calculate age- and gender-specific versions of each of these three population shares.[26]

Finally, I 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,

[25] 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.

[26] 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 citizens as natives and, accordingly, I maintain that convention here.

I 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.

Table 1 presents summary statistics for key independent and dependent variables for Arizona in each year between 2001-2010. 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, I 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 discernable 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 in 2009-2010, increasing the unemployment rate from 5.8 percent to 11.7 percent and decreasing the employment-to-population ratio from 58 percent to 52 percent. Arizona's economic decline is best seen in the share of construction employment which decline from 5 percent in 2008 to 4.1 percent in 2010. Recognizing that the state of Arizona's macroeconomy may have affected both its immigrant share and its crime rate, I control for each measure of the state's local economy and industry concentration in standard differences-in-differences estimates reported later in the paper. However, I note that there is little evidence of any meaningful change in Arizona's social, demographic or economic covariates until

2009, making 2008 an ideal post-treatment year.

VI. Results

A. Main Results

I begin a discussion of the results with an analysis of the effect of LAWA on Arizona's foreign-born Mexican population. The raw data in Table 1 suggests that Arizona's foreign-born Mexican population share declined from 6.1 percent in 2007 to 5.5 percent in the immediate aftermath of LAWA and falling further to 4.9 percent by 2010. This remarkable decline in the foreign-born Mexican share of nearly 20 percent over a three year period is unprecedented in recent U.S. history. Importantly, Table 1 indicates no such change in the share of the foreign-born population of nationalities other than Mexican (this population share was 2.5 percent in 2007 and 2.3 percent in 2010). While the overall noncitizen share declined from 8.6 percent in 2007 to 7.3 percent in 2010, this was fueled entirely by a 3 percentage point drop in the share of the foreign-born who are of Mexican origin.

Figure 1 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 foreign-born Mexican share, Panel B considers the foreign-born non-Mexican share and Panel C considers the share of the population that is comprised of Hispanic U.S. citizens. Each of the panels presents two figures. The figure on the lefthand 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). This comparison region is formed using a weighted average of states that did not pass an E-Verify law, where the weights are computed to minimize the gap between the comparison states and Arizona prior to the implementation of the treatment. The figure on the righthand side of the page compares the estimated treatment effect of Arizona's LAWA (using the blue 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 blue 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.

Panel A considers the effect of LAWA on Arizona's 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 noncitizen Mexican share falls dramatically relative to its synthetic comparison region. By 2008, the estimated difference was 0.5 percentage points. By 2010, the difference is approximately 1 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 righthand 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 D-D estimate for Arizona is larger in magnitude than for any state in the donor pool. This is already true by 2008 and, by 2010, 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.

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 foreign-born non-Mexican share and the share of Hispanic citizens, respectively. Referring to Panel B, there is little evidence in favor of a decline in the noncitizen population that are nationals of a country other than Mexico. If anything, the share of this group initially rises in Arizona relative to the synthetic comparison group. Referring to the placebo figure, the treatment effect on this group is very close to zero and is entirely consistent with sampling variability. Panel C considers the effect of LAWA on the U.S. citizen Hispanic share. Here too there is little evidence of any effect of LAWA. The Hispanic citizen share increases in both Arizona and synthetic Arizona across the treatment threshold with any differences being consistent with sampling variability.[27]

Having presented graphically the first stage relationship between LAWA and the change in the foreign-born Mexican population share, I next 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). These graphs are presented in Figure 2. Each panel in Figure 2 presents synthetic D-D estimates for a given crime type along with the associated placebo test. 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 15 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, approximately five other placebo states have larger drops in their violent crimes rates and accordingly it is difficult to conclude that Arizona's reduction is significant. Disaggregating violent crimes by crime type, we see very little evidence of a lasting effect on murder and rape and only a small decline in robberies relative to the synthetic control region. A slightly larger decline is seen for aggravated assault which decreased by approximately 12 percent.

There is stronger evidence that property crimes decreased in Arizona in the aftermath of LAWA. Referring to Panel F of Figure 2, 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 largely driven by motor vehicle theft which declined by 20-40 percent, depending on whether 2008 or 2010 is used as the relevant post-treatment year.

Before turning to a more detailed discussion of the D-D estimates of the effect of LAWA, I pause to briefly consider the composition of Arizona's synthetic comparison group for each of the outcome variables presented in Figures 1 and 2. The weights used to construct the synthetic D-D estimates are presented in Table 2. For each outcome, a variety of states contribute to the comparison group

[27] An unreported analysis confirms that the share of U.S. natives with a high school education or less does not change relative to Arizona's synthetic control group after 2007.

with the District of Columbia, New Mexico and Hawaii receiving positive weights for the greatest number of crimes.

Table 3 presents the information in Figures 1 and 2 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 2008-2010), I 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. As suggested by Abadie, Diamond and Hainmuller (2010), 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 1/47 or 0.021.

I begin discussion of Table 3 by considering first stage estimates of the effect of LAWA on 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 0.46 percentage points lower than that in its synthetic control region and, by, 2010, its Mexican share was 0.96 percentage points lower. Since this is the largest difference among the sampling distribution, the associated one-tailed p-value is 0.021 (1/46). Next, I 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 2.3 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 2008-2010 as the post-treatment period.

While the latter uses more information, 2009 and 2010 are potentially compromised by DHS' early rollout 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 estimates are found for rape (-0.15), motor vehicle theft (-0.15), aggravated assault (-0.15) and murder (-0.07). Estimates for rape, aggravated assault, motor vehicle theft and the property crime aggregate are significant at the 10 percent level. Notably, all of the estimated treatment effects are negative. Referring to the 2008-2010 post-treatment period, once again all of the estimated treatment effects are negative. Largest effects are found for motor vehicle theft (-0.39), aggravated assault (-0.14) and burglary (-0.12). However, only estimates on motor vehicle theft and the property crime aggregate are significant at conventional levels. The D-D estimate for rape falls by more than 50 percent relative to the estimates that use 2008 only as the post-treatment period. Interestingly, the magnitudes of the D-D estimates using the 2008-2010 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 15 percent reduction in per capita motor vehicle thefts.

Because measuring the effect of LAWA is largely interesting insofar as it yields an estimate of the contribution of Mexican immigration to crime, 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 immigration on crime is positive. Estimates for all crime types are large and, using 2008 as the post-treatment period, range from 0.04 for larceny to 0.32 for motor vehicle theft. Since only motor vehicle theft and the property crime aggregate are significant for both post-treatment periods, I focus most heavily on these results. The implication is that a one percentage point increase in Arizona's foreign-born Mexican share led to a 23 percent increase in property crimes. These effects are very large, far larger than those that are found in the literature.

Accordingly, in the remainder of the paper, I report a series of robustness checks designed to generate further confidence in the research design, I seek to characterize the local average treatment effect of LAWA and, in Section VII, I further consider conditions under which these estimates are likely to yield an accurate portrayal of immigrant criminality in Arizona.

B. Robustness Checks

Before characterizing the estimated treatment effects presented above, I 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. I 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.

I begin a discussion of robustness checks by re-specifying the synthetic D-D models presented in Table 3 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. These estimates are presented in Table 4. Since Arizona's border states (California, New Mexico, Colorado, Utah and Nevada) contribute importantly to the donor pool in only a few instances, many of the estimates in Table 4 are equivalent to those in Table 3. Among the estimates that change marginally are those for murder using 2008 as the post-treatment period and robbery using the 2008-2010 post-treatment period. These are both larger in magnitude. For motor vehicle theft, the results are identical for the 2008-2010 post-treatment period and only slightly different for the 2008 post-treatment period. The same is true for the property crime aggregate. The results therefore lend credence to estimates presented in Table 3 and suggest that cross-border spillovers do not have a measurable effect on the estimated treatment effect.

Next, I 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. Table 5 accounts for potential anticipatory effects. Here, the results are broadly similar to those estimated using 2008-2010 as the post-treatment period though the implied IV coefficient for property crimes (0.16 using 2007-2008 as the post-treatment period or 0.17 use 2007-2010 as the post-treatment period) is somewhat smaller than estimates that exclude 2007.

Next, I leverage monthly crime data available from the FBI to re-compute synthetic D-D estimates that estimate the treatment effect of LAWA with more temporal granularity. This is potentially important as the law takes effect in the first quarter of 2008 and, as such, the treatment effect should potentially be seen immediately. Figure 3 plots synthetic D-D estimates using quarterly crime data. In each panel of the graph, the x-axis plots quarterly crime data where quarters are numbered relative to 2008Q1 which is denoted "0." Overall, the quarterly plots closely resemble those that employ annual data in both trend and magnitude of the D-D effect. For motor vehicle theft and the property crime aggregate, differences between Arizona and its synthetic treatment region occur immediately beginning in the first quarter of 2008 and increase in magnitude over time.

Finally, I present regression-based D-D estimates of the effect of LAWA using monthly police agency-level data for U.S. cities with 50,000 or greater population. These are the most granular data that can be used to estimate the effect of LAWA.[28] With 11 Arizona cities meeting the population threshold of 50,000, the treatment effect is estimated using 396 treated city-months (11 cities $\times$ 36 treated months for each city). For each crime type, I regress the log per capita crime rate on a treatment dummy and a full set of city and month fixed effects.[29] Results are broadly consistent with synthetic control estimates computed using state-by-year variation. Table 6 presents the results of this exercise. In Table 6, 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 in 2005-2006 (2-3 years prior to LAWA's implementation). 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, LAWA is estimated to have reduced monthly assaults in Arizona cities by 10 percent and motor vehicle thefts by 43 percent. These are highly consistent with the state-level

[28]The cost is larger standard errors due to the fact that there is more sampling variability in monthly crimes.
[29]Monthly population is linearly interpolated using the ACS population measures for each year.

D-D estimates which were 14 percent and 39 percent, respectively. Overall, Arizona cities witnessed a decline of 7 percent and 27 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. If anything, there is evidence that murder and rape had risen in the years preceding LAWA with reversals in trends after the law's passage. With regard to property crimes, there is some evidence of a pre-LAWA decline. However, even conditioning on this decline, the estimated effect of LAWA remains negative and significant.

C. LATE

This research has uncovered large 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. I 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, I test to see whether LAWA was especially likely to induce young males among the foreign-born Mexican population to leave Arizona. Figure 4 provides insight into the local average treatment effect that is induced by LAWA. Each panel of Figure 4 presents synthetic D-D plots for a different age-gender sub-group of the foreign-born Mexican population. As with Figure 1, the left-hand figure in each panel compares Arizona to its synthetic counterpart while the right-hand figure shows where Arizona's D-D estimate falls in the sampling distribution of placebo estimates.

Panel A considers the effect of LAWA on the share of Arizona's population that is comprised of foreign-born Mexican male children ($<$ age 14). In turn, Panel B considers the effect of LAWA on Arizona's 15-24 year old foreign-born Mexican population. There is, in fact, evidence that this sub-group leaves the state in large numbers, declining by over one third after LAWA's passage, an effect that is large relative to the sampling distribution. In 2007, just prior to LAWA's implementation, Arizona's foreign-born Mexican 15-24 male population share was 0.66 percent. By 2010, that same share was just 0.36 percent indicating that this subgroup declined by approximately 46 percent compared to an overall decline of 19 percent. Overall, the decline in the 15-24 year old male population 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 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. I 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, I 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 (Δ V_p) for a decline in the share of young males from M_1 to M_2 can be computed as:

$$\Delta V_p = \frac{(M_2 - M_1)(a-1)X}{M_1 a X + (1 - M_1)X} \tag{7}$$

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:

$$\Delta V_p = \frac{-(M_1 - M_2)(a-1)}{1 + M_1 a - M_1} \tag{8}$$

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 7 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 3.

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 associated with LAWA is almost entirely compositional.

VII. Conclusion

This research leverages a natural experiment in Arizona to estimate the contribution of Mexican immigrants to the state's crime rate. In the aftermath of the passage and implementation of the Legal Arizona Workers Act, there was a large and discrete 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 or the share of U.S.-born Hispanics. 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 more than 20 percent. These effects are robust to the exclusion of Arizona's border states from the control group, a series of placebo tests, analysis of quarterly as opposed to annual crime data and analysis of crime data at the agency level.

The large and significant 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, back-of-the-envelope calculations suggest that between one third and 85 percent of the estimated treatment effect can be accounted for by compositional changes in Arizona's Mexican population along the dimensions of age and gender. 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.

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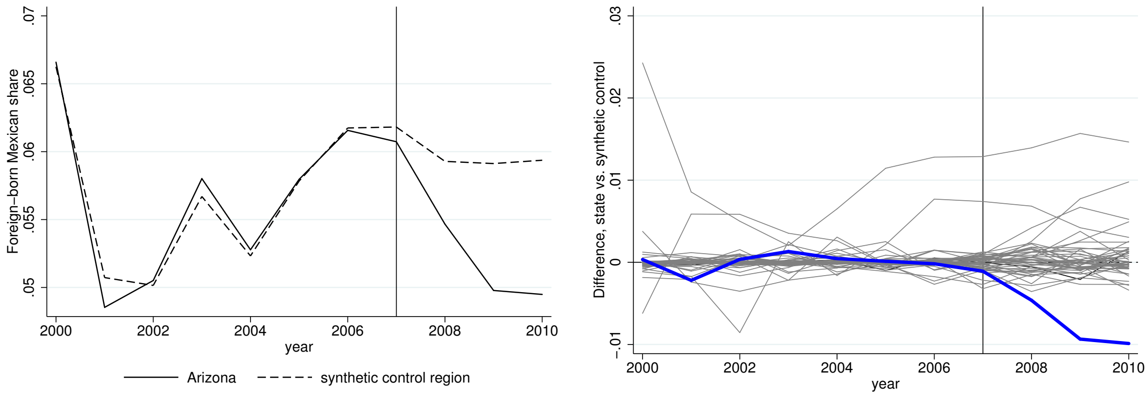
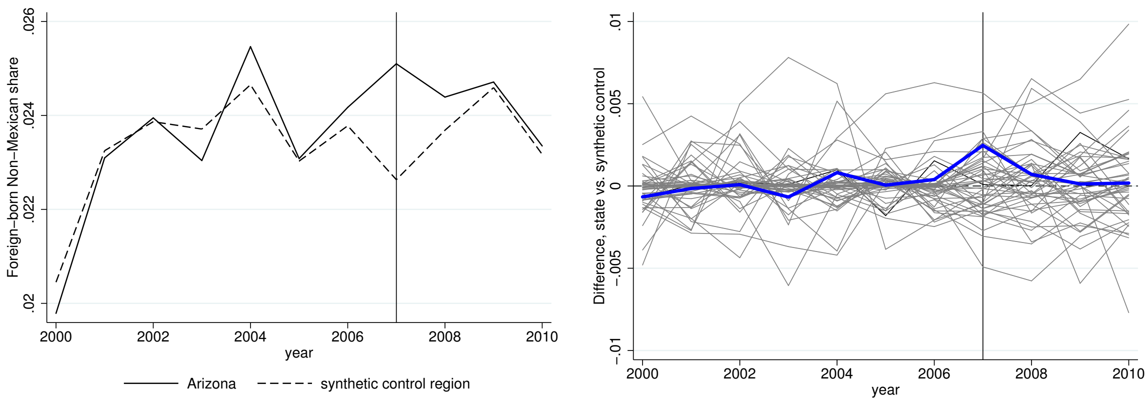
Figure 1. Synthetic "Difference-in-Difference" Estimates of the Effect of the Legal Arizona Workers Act [LAWA] on the Foreign Born Population Share

A. Foreign-Born (Noncitizen) Mexican Population



B. Foreign-Born (Noncitizen) Non-Mexican Population



C. U.S. Citizen Hispanic Population

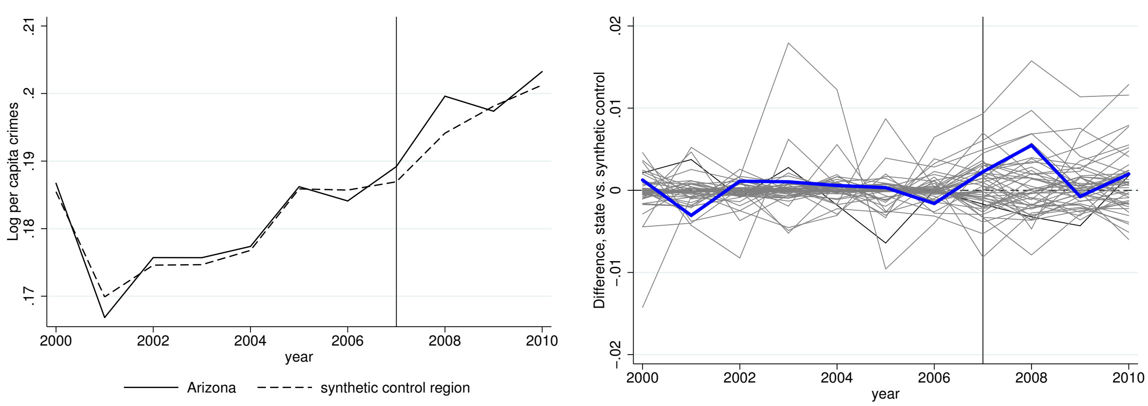
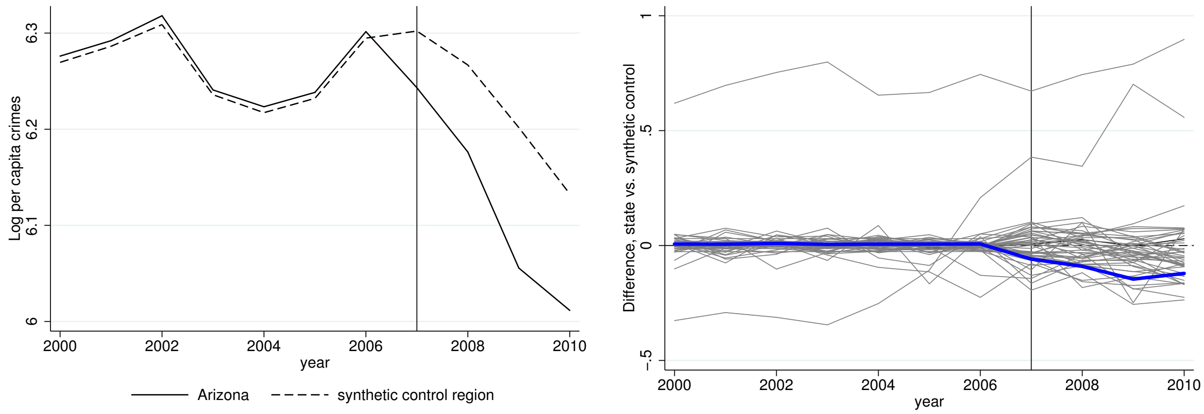
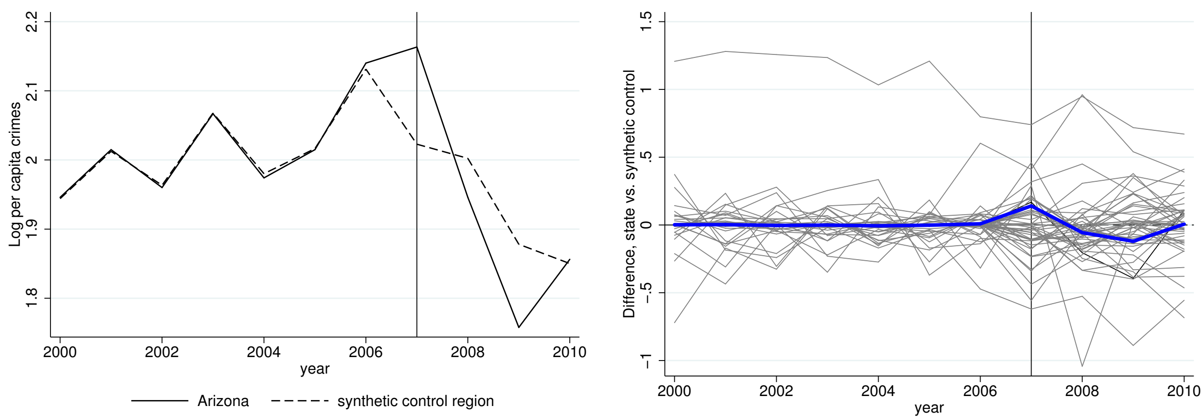


Figure 2. Synthetic "Difference-in-Difference" Estimates of the Effect of Legal Arizona Workers Act [LAWA] on Log Per Capita Crimes Reported to the Police

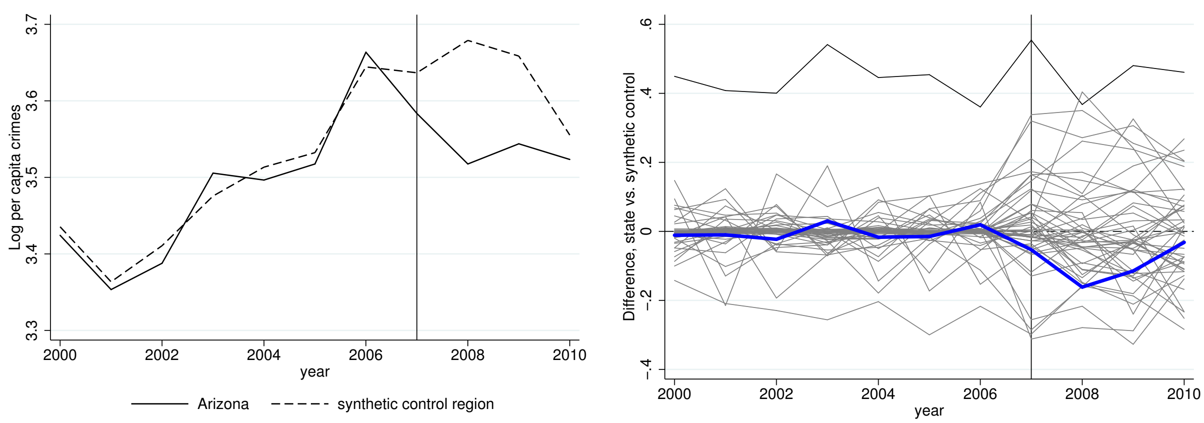
A. Violent Crimes



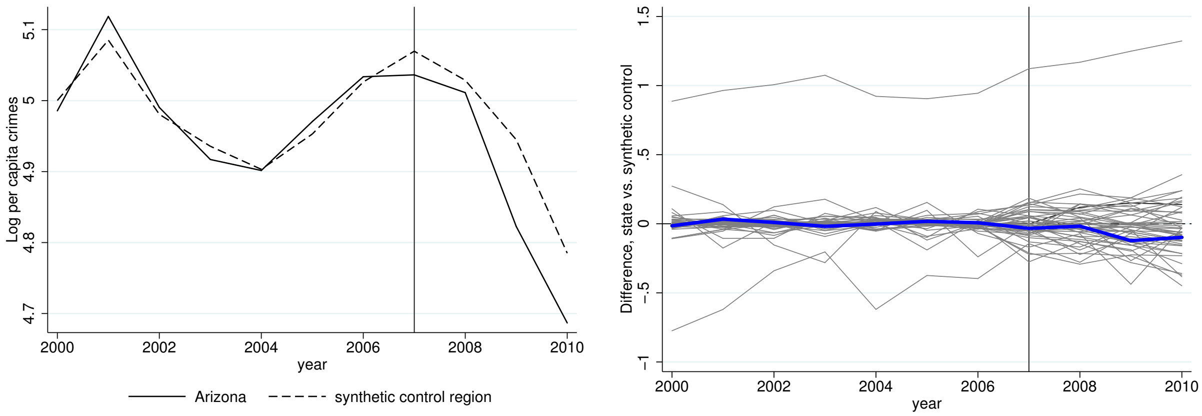
B. Murder



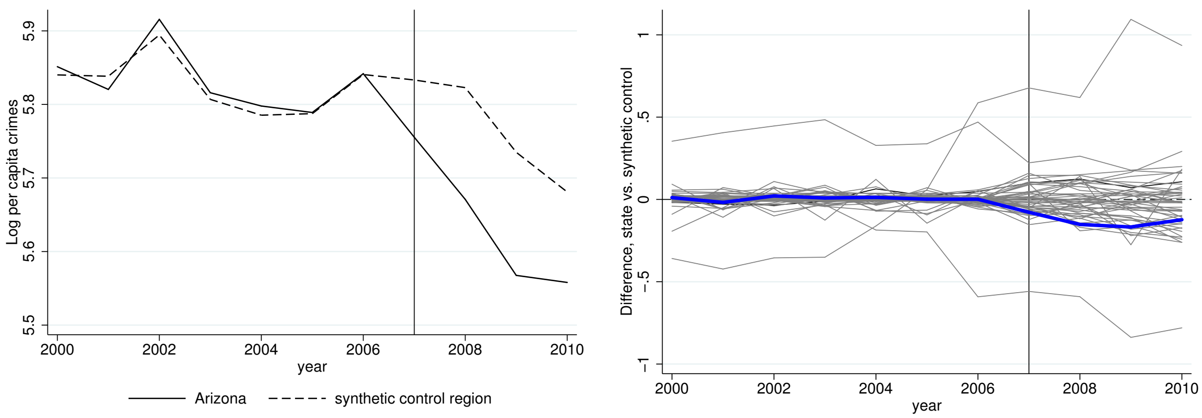
C. Rape



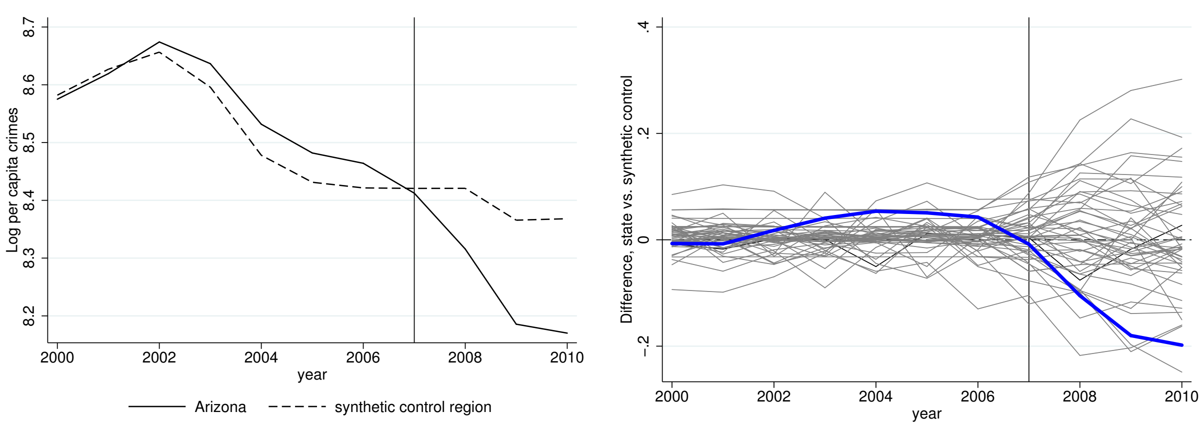
D. Robbery



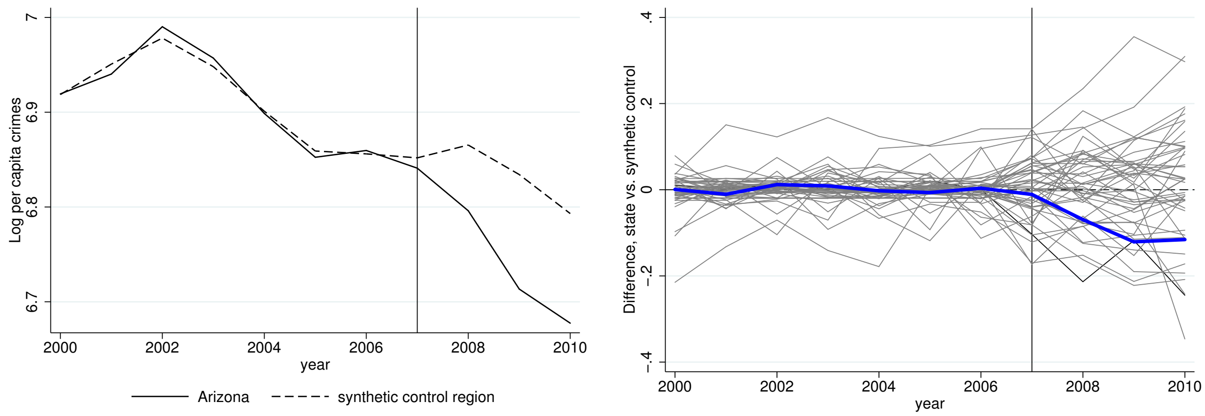
E. Assault



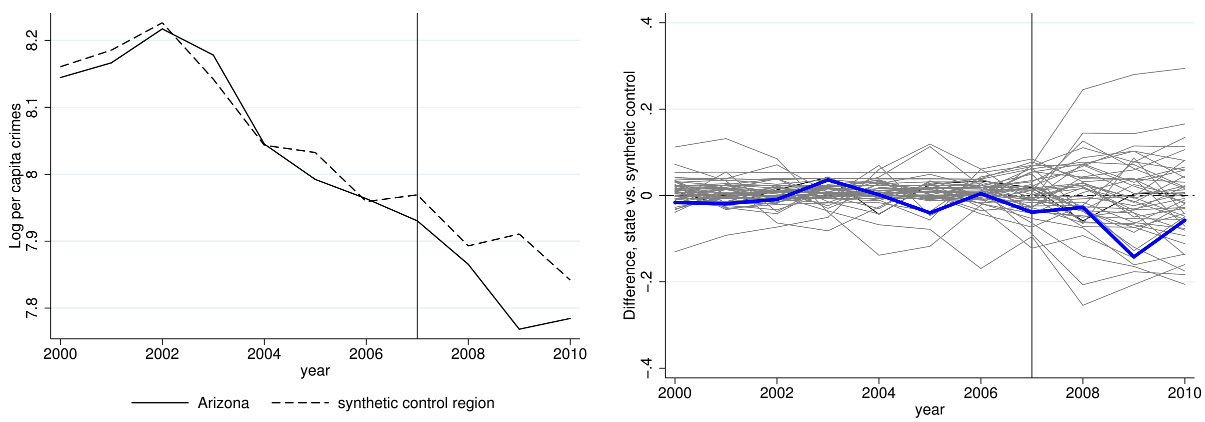
F. Property Crimes



G. Burglary



H. Larceny



I. Motor Vehicle Theft

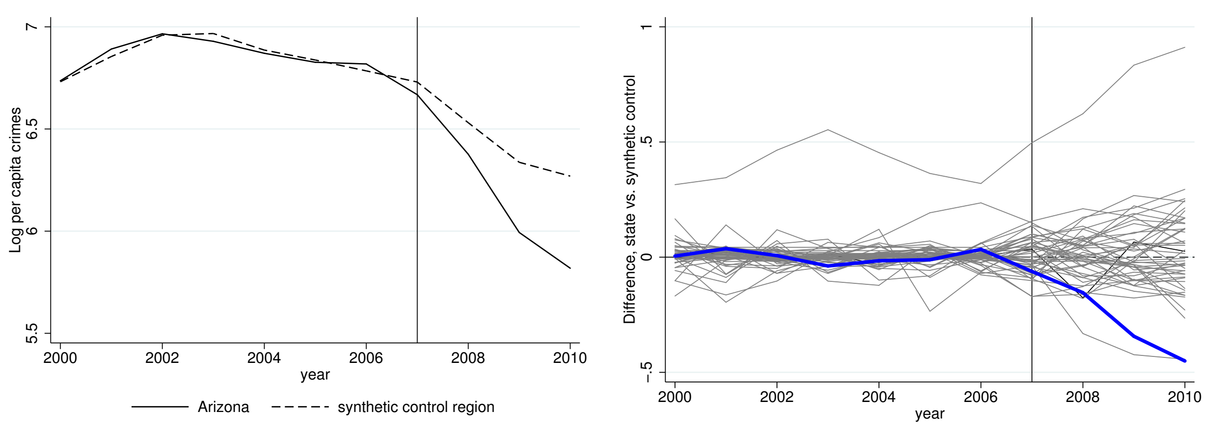
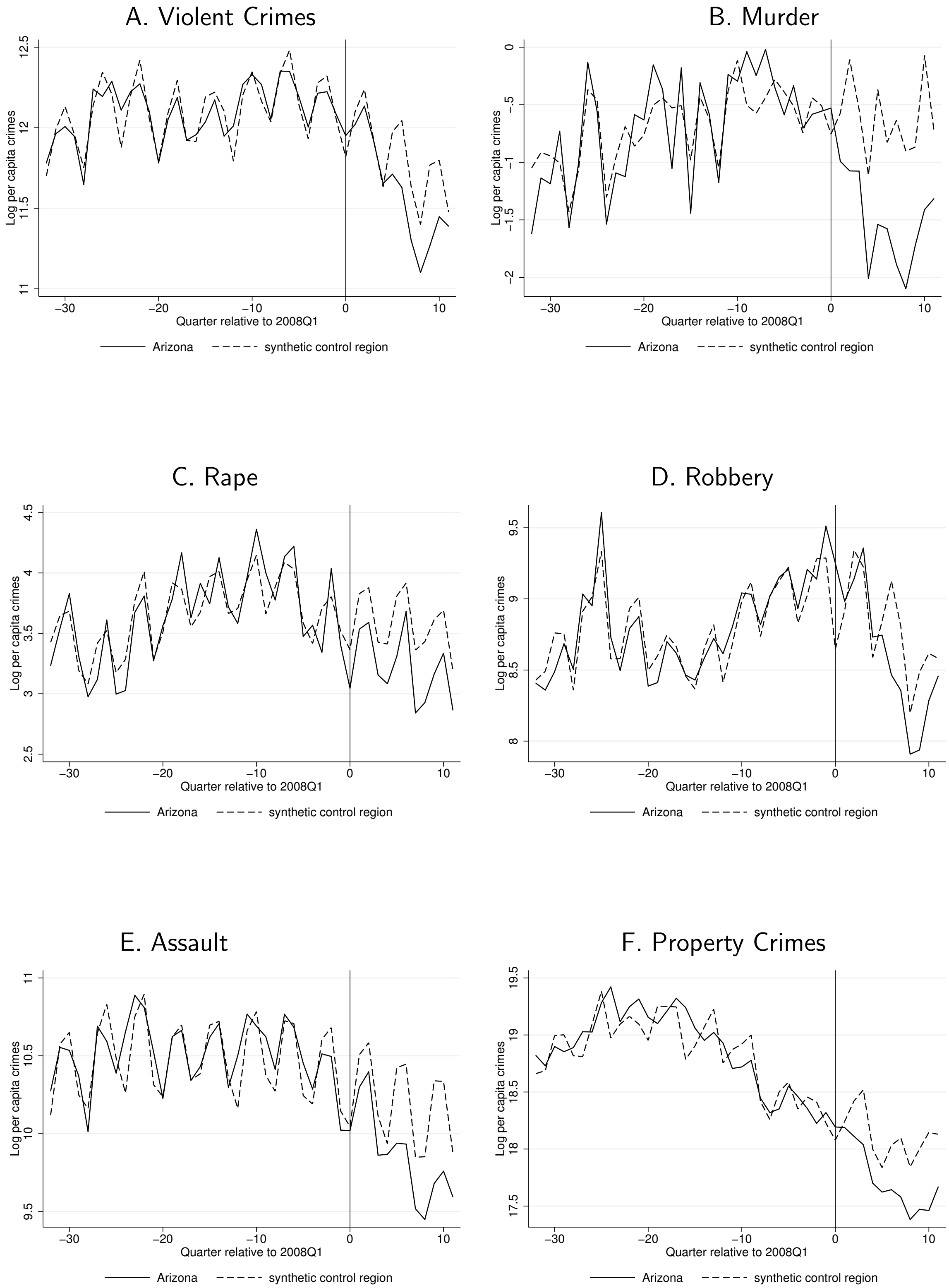
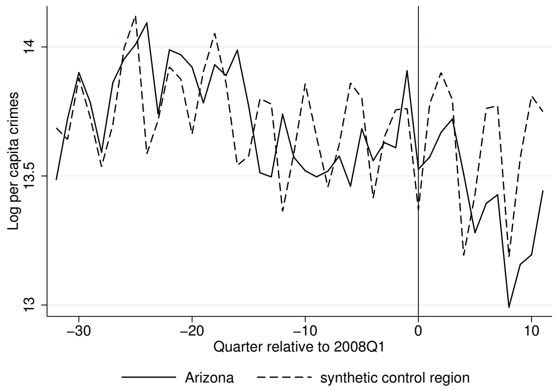


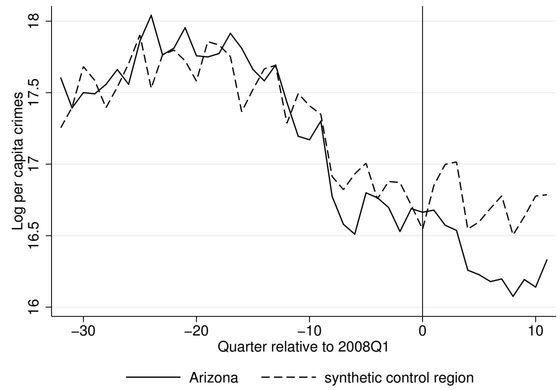
Figure 3. Synthetic "Difference-in-Difference" Estimates of the Effect of Legal Arizona Workers Act [LAWA] on Log Per Capita Crimes Reported to the Police Quarterly Data



G. Burglary



H. Larceny



I. Motor Vehicle Theft

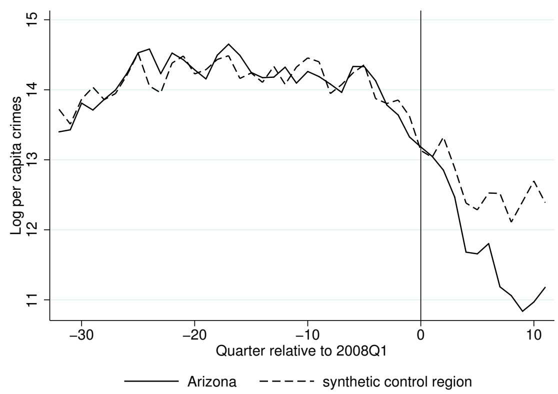
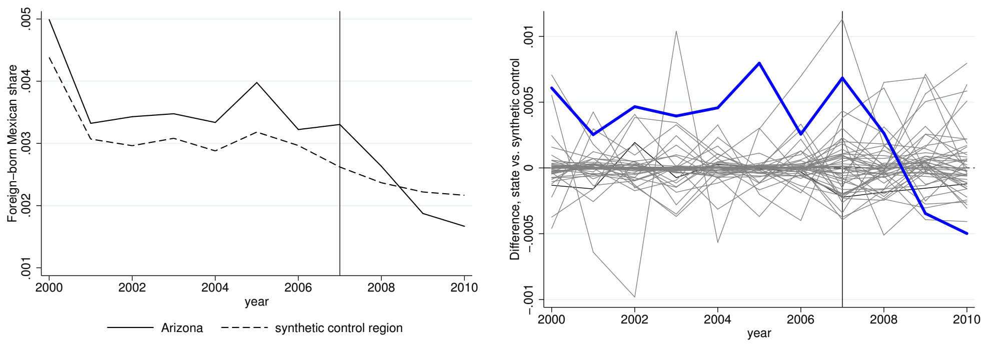
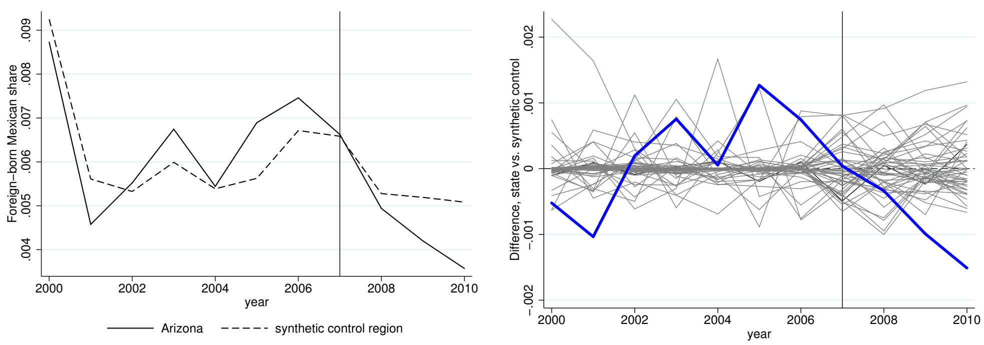


Figure 4. Synthetic "Difference-in-Difference" Estimates of the Effect of LAWA on the Foreign-Born Mexican Population Share by Age and Gender

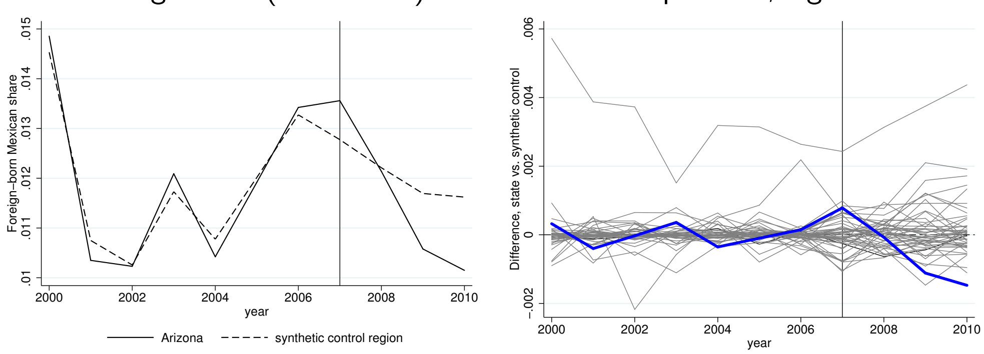
A. Foreign-Born (Noncitizen) Mexican Male Population, Ages 0-14



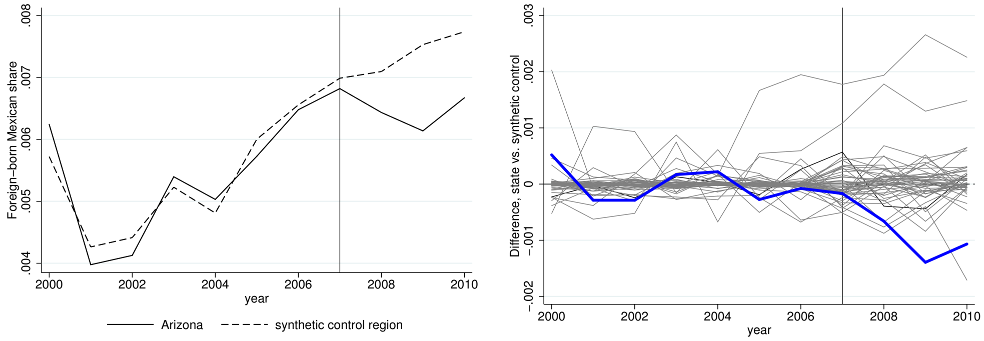
B. Foreign-Born (Noncitizen) Mexican Male Population, Ages 15-24



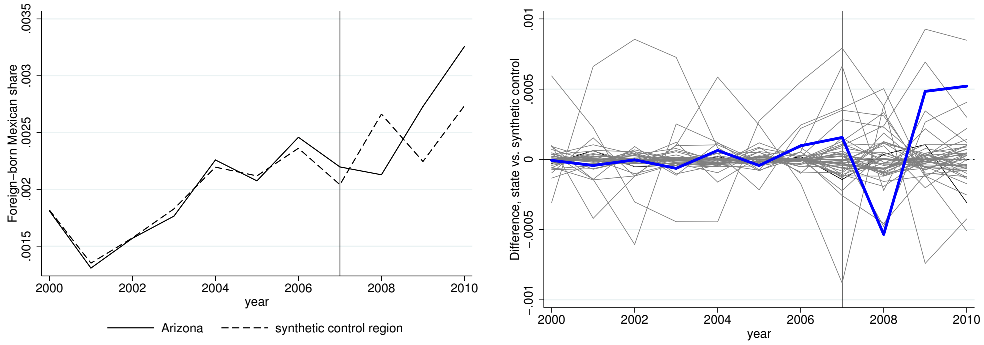
C. Foreign-Born (Noncitizen) Mexican Male Population, Ages 25-39



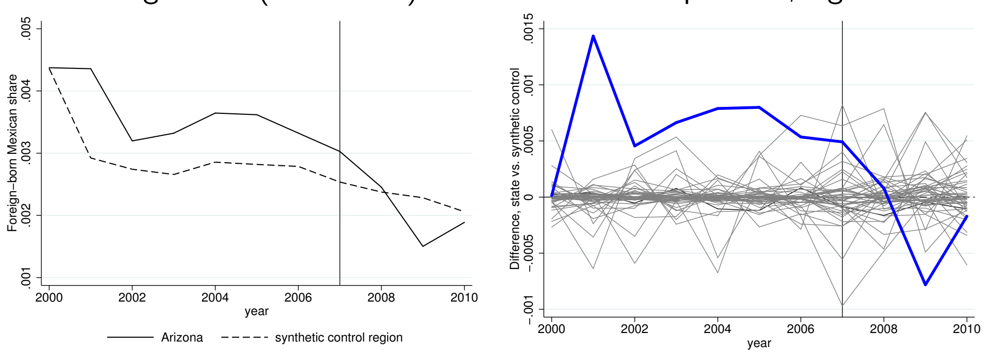
D. Foreign-Born (Noncitizen) Mexican Male Population, Ages 40-54



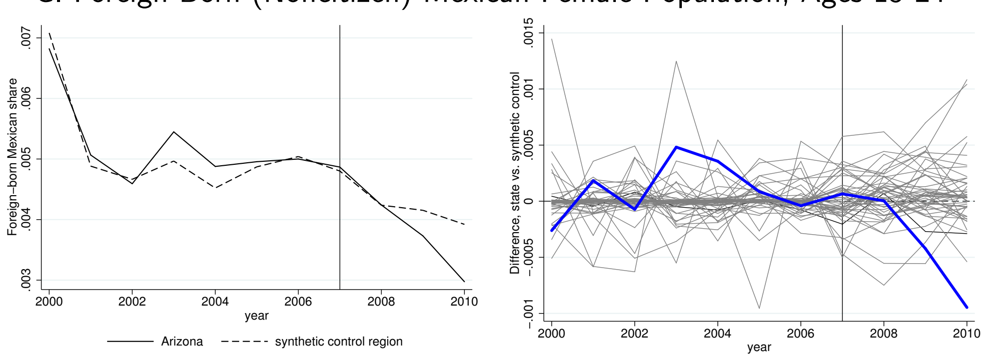
E. Foreign-Born (Noncitizen) Mexican Male Population, Ages 55+



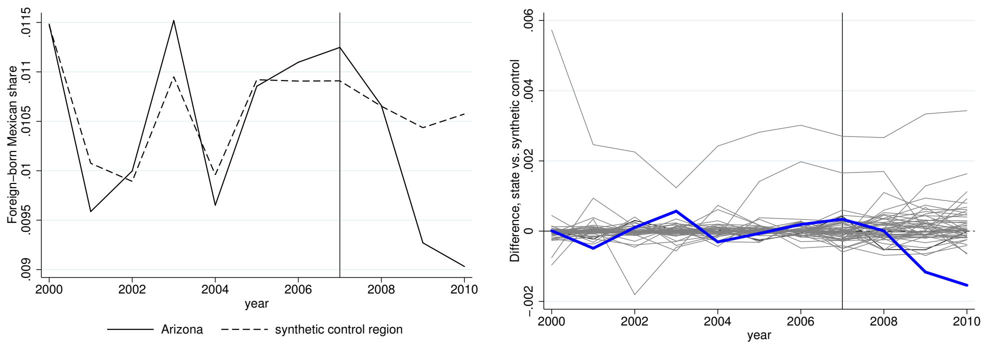
F. Foreign-Born (Noncitizen) Mexican Female Population, Ages 0-14



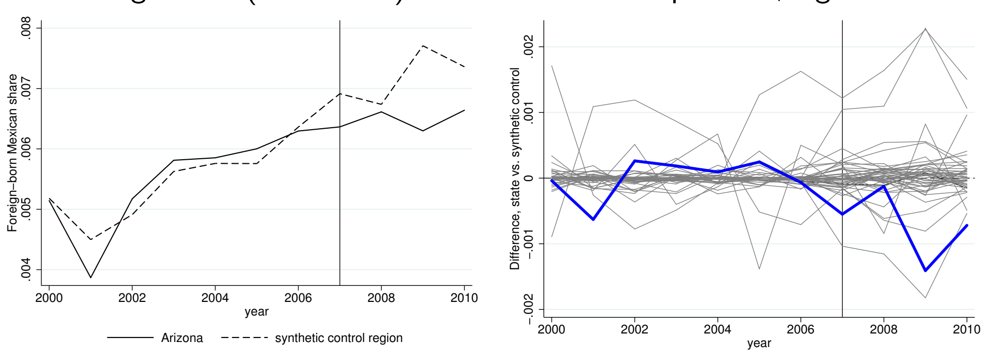
G. Foreign-Born (Noncitizen) Mexican Female Population, Ages 15-24



H. Foreign-Born (Noncitizen) Mexican Female Population, Ages 25-39



I. Foreign-Born (Noncitizen) Mexican Female Population, Ages 40-54



J. Foreign-Born (Noncitizen) Mexican Female Population, Ages 55+

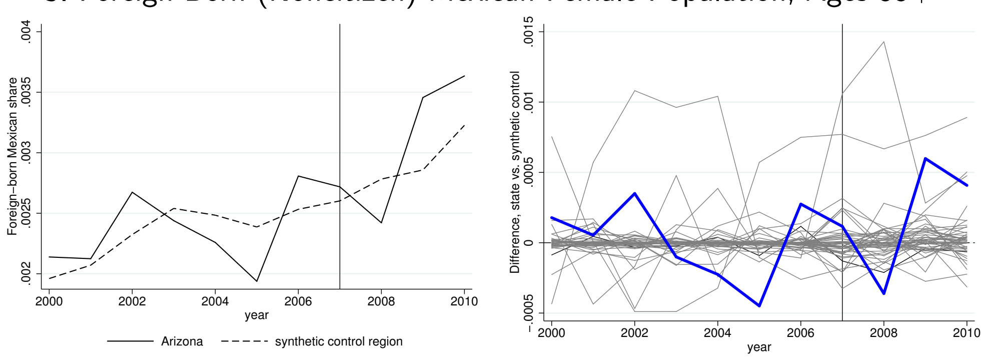


Table 1. Descriptive Statistics for Arizona, 2001-2010

	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
Demographic variables										
% white	0.696	0.687	0.676	0.673	0.654	0.648	0.625	0.621	0.626	0.612
% black	0.024	0.021	0.023	0.027	0.026	0.027	0.030	0.031	0.033	0.036
% married	0.452	0.465	0.446	0.454	0.443	0.440	0.433	0.432	0.432	0.422
% age 0-14	0.205	0.208	0.214	0.211	0.212	0.205	0.208	0.204	0.196	0.195
% age 15-24	0.123	0.122	0.126	0.115	0.122	0.126	0.127	0.124	0.126	0.125
% age 25-39	0.193	0.193	0.186	0.187	0.186	0.189	0.187	0.187	0.179	0.180
% age 40-54	0.210	0.208	0.212	0.210	0.211	0.206	0.202	0.203	0.202	0.198
% age 55+	0.269	0.270	0.262	0.277	0.269	0.274	0.276	0.282	0.296	0.302
Nativity variables										
% non-citizen Mexican	0.049	0.051	0.058	0.053	0.058	0.062	0.061	0.055	0.050	0.049
% non-citizen non-Mexican	0.023	0.024	0.023	0.025	0.023	0.024	0.025	0.024	0.025	0.023
% non-citizen	0.072	0.074	0.081	0.078	0.081	0.086	0.086	0.079	0.074	0.073
% immigrant	0.121	0.122	0.134	0.130	0.135	0.139	0.142	0.134	0.130	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	0.679
Economic variables										
% in labor force	0.603	0.600	0.612	0.601	0.610	0.605	0.599	0.616	0.597	0.588
% employed	0.566	0.556	0.569	0.563	0.574	0.576	0.565	0.580	0.538	0.519
% unemployed	0.062	0.074	0.071	0.063	0.059	0.048	0.056	0.058	0.099	0.117
Industry concentration										
% employed in:										
Agriculture	0.013	0.013	0.007	0.006	0.006	0.006	0.006	0.005	0.006	0.006
Construction	0.042	0.041	0.041	0.044	0.049	0.054	0.052	0.050	0.043	0.041
Manufacturing	0.057	0.055	0.051	0.051	0.048	0.045	0.046	0.043	0.043	0.040
Restaurants	0.028	0.033	0.029	0.028	0.030	0.030	0.032	0.031	0.032	0.033
Retail trade	0.074	0.071	0.075	0.073	0.071	0.070	0.068	0.072	0.069	0.070

Note: Table reports means for selected characteristics of the Arizona population from 2001 to 2010.

Table 2. 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
2 AK	-	-	-	0.014	-	-	-	-	-	-
1 AL	-	-	-	-	-	-	-	-	-	-
5 AR	-	0.172	0.182	-	-	-	-	-	-	0.314
6 CA	-	-	-	-	-	-	-	-	-	-
8 CO	-	-	-	-	-	-	-	-	-	-
9 CT	-	0.188	-	-	-	-	-	-	-	-
11 DC	0.046	0.154	-	0.077	0.127	0.693	0.077	0.190	0.535	-
10 DE	-	0.090	-	-	0.102	-	-	-	-	-
12 FL	-	-	-	-	-	-	-	-	0.175	-
13 GA	-	-	-	-	-	-	-	-	-	-
15 HI	-	-	-	-	-	0.034	0.251	0.522	-	-
19 IA	-	-	0.202	-	-	-	-	-	-	-
16	-	-	-	-	-	-	-	-	-	-
17 IL	-	-	-	-	-	-	-	-	-	-
18 IN	-	-	-	0.335	-	-	-	-	-	-
20 KS	-	-	0.255	-	-	-	-	-	-	-
21 KY	0.178	-	-	-	0.239	-	-	-	-	-
22 LA	-	0.068	-	-	-	-	0.071	-	-	-
25 MA	-	-	-	-	-	-	-	-	-	-
24 MD	0.216	-	-	-	0.294	-	-	-	-	-
23 ME	-	-	-	-	-	-	-	-	-	-
26 MI	-	-	-	-	-	-	-	-	-	-
27 MN	-	-	-	-	-	-	-	-	-	-
29 MO	0.129	-	-	-	-	-	-	-	-	-
28 MS	-	-	-	-	-	-	-	-	-	-
30 MT	-	0.035	0.094	-	-	-	-	-	-	-
37 NC	-	-	-	-	-	-	0.295	-	-	-
38 ND	-	-	0.177	-	-	-	-	-	-	-
31 NE	-	-	-	-	-	-	-	-	-	-
33 NH	-	-	-	-	-	-	-	-	-	0.141
34 NJ	-	-	-	-	-	-	-	-	-	0.168
35 NM	0.128	0.293	-	0.210	-	-	-	-	-	-
32 NV	0.216	-	-	-	0.229	-	-	-	-	-
36 NY	-	-	-	-	-	-	-	-	-	-
39 OH	-	-	-	-	-	-	-	-	-	-
40 OK	-	-	-	-	-	-	-	-	-	-
41 OR	-	-	-	-	-	-	-	0.044	-	-
42 PA	-	-	-	-	-	-	-	-	-	-
44 RI	-	-	0.091	-	-	-	-	-	-	0.377
45 SC	-	-	-	-	-	-	-	-	-	-
46 SD	-	-	-	-	0.004	-	-	-	-	-
47 TN	-	-	-	0.363	-	-	0.306	-	-	-
48 TX	-	-	-	-	-	-	-	0.219	-	-
49 UT	-	-	-	-	0.004	-	-	0.026	-	-
51 VA	-	-	-	-	-	-	-	-	-	-
50 VT	-	-	-	-	-	-	-	-	-	-
53 WA	-	-	-	-	-	0.326	-	-	0.290	-
55 WI	-	-	-	-	-	-	-	-	-	-
54 WV	-	-	-	-	-	-	-	-	-	-
56 WY	0.086	-	-	-	-	-	-	-	-	-
$\sqrt{MSE}$	0.0012	0.0044	0.0186	0.0168	0.0115	0.0307	0.0076	0.0221	0.0250	0.0010

Note: Each row reports the percentage contribution of a given state to the synthetic control region for Arizona for a given crime type. For example, for the murder specification, the synthetic control region for Arizona is comprised of: Alaska (0.3%), Colorado (7.4%), District of Columbia (11.6%), Florida (14.1%), Hawaii (24.0%), Nevada (16.0%) and Washington (26.7%). Save for rounding errors, the percentages sum to 100 along the columns.

Table 3. 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, 2009-2010		
	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	-0.002	-0.091	-0.089	0.106	-0.134	-0.132	0.128
Murder	0.018	-0.056	-0.074	0.489	-0.057	-0.075	0.319
Rape	-0.010	-0.161	-0.151*	0.064	-0.073	-0.063	0.362
Robbery	-0.000	-0.017	-0.017	0.489	-0.110	-0.110	0.340
Assault	-0.005	-0.152	-0.147*	0.085	-0.145	-0.140	0.255
Property crimes	0.023	-0.105	-0.128*	0.064	-0.189	-0.212**	0.042
Burglary	-0.001	-0.069	-0.068	0.170	-0.118	-0.117	0.170
Larceny	-0.010	-0.028	-0.018	0.447	-0.100	-0.090	0.149
Motor vehicle theft	-0.006	-0.154	-0.148*	0.085	-0.397	-0.391**	0.043
B. First Stage Estimates							
Non-citizen foreign born Mexican population share	-0.000	-0.462	-0.462	0.022	-0.961	-0.961	0.022
C. Implied IV Estimates							
Violent crimes			0.193			0.137	
Murder			0.160			0.078	
Rape			0.327			0.066	
Robbery			0.037			0.114	
Assault			0.318			0.146	
Property crimes			0.277			0.221	
Burglary			0.147			0.122	
Larceny			0.039			0.094	
Motor vehicle theft			0.320			0.409	

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 directy. 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-taled test of the null hypothesis. The final set of columns report identical estimates using 2009-2010 as the post-treatment period. As the dependent variable is the log of the crime rate, these coefficients cna 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 effet of the treament 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 4. Synthetic "Differences-in-Differences" and IV Estimates of the Effect of LAWA on Index Crimes Reported to Police Border States Excluded from the Donor Pool

	Pre-treatment period, 2001-2007	Post treatment period, 2008			Post-treatment period, 2009-2010		
	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*
Reduced Form Estimates							
Violent crimes	-0.003	-0.081	-0.078	0.140	-0.141	-0.138	0.140
Murder	0.016	-0.201	-0.217	0.209	-0.130	-0.146	0.209
Rape	-0.010	-0.161	-0.151*	0.070	-0.073	-0.063	0.349
Robbery	-0.001	-0.068	-0.067	0.326	-0.239	-0.239*	0.093
Assault	-0.009	-0.107	-0.099	0.186	-0.133	-0.125	0.209
Property crimes	0.014	-0.114	-0.128*	0.070	-0.198	-0.212**	0.023
Burglary	-0.001	-0.069	-0.068	0.186	-0.118	-0.117	0.140
Larceny	-0.002	-0.020	-0.018	0.349	-0.092	-0.090	0.140
Motor vehicle theft	-0.006	-0.154	-0.148	0.116	-0.397	-0.391**	0.047
First Stage Estimates							
Non-citizen foreign born Mexican population share	-0.000	-0.462	-0.462	0.022	-0.961	-0.961	0.022
Implied IV Estimates							
Violent crimes			0.193			0.137	
Murder			0.160			0.078	
Rape			0.327			0.066	
Robbery			0.037			0.114	
Assault			0.318			0.146	
Property crimes			0.277			0.221	
Burglary			0.147			0.122	
Larceny			0.039			0.094	
Motor vehicle theft			0.320			0.409	

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). Results in this table ae based on a donor pool that excludes California, Colorado, Nevada and Mew Mexico, states that border Arizona. 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 directy. 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 47 states in the donor pool by 47. This is shown in Abadie, Diamond and Hainmuller (2010) to be equivalent to a p-value arising from a one-taled test of the null hypothesis. The final set of columns report identical estimates using 2009-2010 as the post-treatment period. As the dependent variable is the log of the crime rate, these coefficients cna 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 effet of the treament 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 5. Synthetic "Differences-in-Differences" and IV Estimates of the Effect of LAWA on Index Crimes Reported to Police Using 2007 As a Post-Treatment Year

	Pre-treatment period, 2001-2006	Post treatment period, 2007-2008			Post-treatment period, 2007-2010		
	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*
Reduced Form Estimates							
Violent crimes	0.008	-0.057	-0.065	0.395	-0.100	-0.108	0.279
Murder	0.046	0.071	0.026	0.744	-0.003	-0.049	0.465
Rape	0.032	-0.030	-0.062	0.488	0.011	-0.021	0.628
Robbery	0.002	-0.020	-0.022	0.558	-0.121	-0.123	0.349
Assault	-0.015	-0.174	-0.159	0.186	-0.171	-0.156	0.209
Property crimes	0.021	-0.053	-0.074	0.163	-0.144	-0.165	0.023
Burglary	0.001	-0.055	-0.056	0.256	-0.091	-0.092	0.209
Larceny	0.015	0.025	0.010	0.558	-0.039	-0.054	0.329
Motor vehicle theft	0.002	-0.154	-0.156	0.186	-0.389	-0.391	0.047
First Stage Estimates							
Non-citizen foreign born Mexican population share	-0.009	-0.463	-0.454	0.023	-0.956	-0.947	0.023
Implied IV Estimates							
Violent crimes			0.143			0.114	
Murder			-0.057			0.052	
Rape			0.137			0.022	
Robbery			0.049			0.130	
Assault			0.350			0.165	
Property crimes			0.163			0.174	
Burglary			0.123			0.097	
Larceny			-0.022			0.057	
Motor vehicle theft			0.339			0.413	

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). Results in this table include 2007 as a post-treatment year. 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 directy. 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 47 states in the donor pool by 47. This is shown in Abadie, Diamond and Hainmuller (2010) to be equivalent to a p-value arising from a one-taled test of the null hypothesis. The final set of columns report identical estimates using 2009-2010 as the post-treatment period. As the dependent variable is the log of the crime rate, these coefficients cna 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 effet of the treament 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 6. "Differences-in-Differences Estimates: Agency-Level Regression Estimates Using Monthly Data

	Violent crimes	Murder	Rape	Robbery	Assault	Property crimes	Burglary	Larceny	Motor vehicle theft
Panel A. Standard Estimates									
AZ × 2008-2010	-0.070**	-0.133	-0.057	0.004	-0.099**	-0.266**	-0.102*	-0.056	-0.432***
	(0.033)	(0.120)	(0.108)	(0.053)	(0.043)	(0.125)	(0.058)	(0.055)	(0.070)
R^2	0.880	0.910	0.902	0.868	0.807	0.873	0.848	0.873	0.863
Panel B. Placebo Estimates									
AZ × 2005-2006	0.041	0.086***	0.156**	0.044	0.046	-0.188	-0.106	0.001	0.008
	(0.050)	(0.025)	(0.074)	(0.037)	(0.075)	(0.167)	(0.051)	(0.025)	(0.040)
AZ × 2007	0.021	0.028	0.079	0.105	-0.009	-0.193	-0.066	0.021	-0.080
	(0.057)	(0.027)	(0.071)	(0.052)	(0.066)	(0.177)	(0.100)	(0.027)	(0.062)
AZ × 2008-2010	-0.061	-0.117	-0.024	0.025	-0.094	-0.320*	-0.126*	-0.053	-0.442***
	(0.045)	(0.120)	(0.123)	(0.062)	(0.058)	(0.173)	(0.076)	(0.058)	(0.082)
R^2	0.880	0.910	0.902	0.869	0.807	0.874	0.848	0.873	0.863
N	56,842	56,887	56,632	55,116	56,545	56,875	56,856	56,788	56,756

Note: For each crime type, Panel A reports coefficients and standard errors from a regression of the log crime rate 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-months that are untreated. AZ × 2007 captures the effect of the treatment in the year of its passage but prior to its implementation. AZ × 2005-2006 captures the effect of the treatment in the two years prior to its passage. All models condition on agency and month fixed effects. Standard errors are clustered at the agency level.

Table 7. 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.