

Did the 2007 Legal Arizona Workers Act Reduce the State's Unauthorized Immigrant Population?

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Abstract

We test for an effect of Arizona's 2007 Legal Arizona Workers Act (LAWA) on the proportion of the state population characterized as as non-citizen Hispanic. We use the synthetic control method to select a group of states against which the population trends of Arizona can be compared. We document a notable and statistically significant reduction in the proportion of the Arizona population that is Hispanic noncitizen. The decline observed for Arizona matches the timing of LAWA's implementation, deviates from the time series for the chosen synthetic control group, and stands out relative to the distribution of placebo estimates for the remainder of states in the nation. Furthermore, we do not observe similar declines for Hispanic naturalized citizens, a group not targeted by the legislation. Our results on LAWA's impact on the housing market provide further support for our findings.

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1. Introduction

Along with the large increase in the foreign-born population residing in the United States, there has been a concurrent increase in the size of the unauthorized immigrant population. Since the 1986 passage of the Immigration Reform and Control Act (IRCA) (legislation that adjusted the legal status of most unauthorized immigrants in the U.S. at the time) the undocumented immigrant population subsequently grew to approximately 3 million in 1990 and to roughly 11 million by 2009 (Passel and Cohn 2010). Since 1986, there has been no comprehensive federal legislation intended to address the issue of unauthorized immigration, aside from efforts to strengthen border enforcement and executive branch driven surges in the enforcement of IRCA. In fact, the country finds itself in much the same position that it did in 1986. There is a strong desire to gain control of immigrant flows into the country and to discourage future unauthorized immigration.

The last few years have witnessed a sea of change in the traditional relationship between federal and state governments when it comes to immigration policy. Absent new federal law, several states have passed legislation meant to control and deter unauthorized immigrants within their jurisdiction. The provisions of these state laws vary, with some requiring that state contractors verify the identity and the eligibility to work of all employees and others making unauthorized employment a felony. The intention of these laws is to increase the costs to employers and undocumented immigrants of unauthorized employment and to shift labor demand to authorized workers. Proponents of such state legislation argue that strict enforcement should improve the labor market prospects of low-skilled natives and similar legal foreign residents.

One of the most restrictive of such state legislation currently in place is Arizona's Legal Arizona Workers Act (LAWA). LAWA was passed in July 2007 and implemented in January 2008. The law requires all employers to verify the identity and work eligibility of all new hires using the federal E-verify system, an online system that checks an individual's information against Social Security Administration (SSA) and Department of Homeland Security (DHS) databases. Employers who fail to comply face the loss of their business licenses. A number of states have since implemented similar laws and other states have proposed or discussed comparable measures. The likelihood of other states following the example of Arizona is even more likely given the May 2011 U.S. Supreme Court decision upholding the legality of LAWA. In this paper, we assess whether the passage and implementation of LAWA has altered the demographic composition of the resident population of Arizona.

Prior to the law's passage, a sizable minority of the Arizona population was foreign-born (approximately 16 percent), with roughly 10 percent of the population non-citizen Hispanic, a subgroup containing a large fraction of unauthorized immigrants in the state. Also noteworthy is the large undocumented population in Arizona, estimated to represent roughly half of the foreign born population, or 8 percent of the total population, in 2006 (Hoefer et al 2010). To the extent that undocumented immigrants responded to the law by moving away from Arizona or future undocumented immigration to the state was deterred by the legislation, these proportions should decline. This internal compositional change may be further augmented by legal immigrants and perhaps the naturalized foreign-born leaving the state due to a change in perceived hostility towards immigrants or statistical discrimination on the part of employers.

We test for an effect of LAWA on the proportion of the state population characterized as non-citizen Hispanic as well as the proportion that is non-citizen Hispanic with relatively low

levels of educational attainment. We use the synthetic control method developed by Abadie et. al. (2010) to select a group of states against which the population trends of Arizona can be compared. There are notable pre-post LAWA declines in the proportion of the population that is non-citizen Hispanics. Our estimates range from declines of one and a half to two percentage points. The results from a series of permutation tests that estimate placebo treatment effects for all states with no changes in immigration legislation corresponding in time with LAWA show that Arizona is consistently an outlier. In nearly all such tests, the estimated relative decline in the Arizona foreign-born population is the largest. To probe the robustness of these results, we perform a series of additional tests. First, we assess whether there are comparable declines in the proportion of Arizona residents that are Hispanic naturalized citizens, a population whose employment prospects are not restricted by the legislation given that they are, by definition, authorized to work in the U.S. We find no evidence of a relative decline in this population. We also demonstrate that our results are robust to alternative definitions of the post- and pre-treatment periods and are not being driven by spillover of population into neighboring states. Our main finding also emerges from more traditional difference-in-difference estimates where control states are selected in a more ad-hoc manner (such as all states bordering Arizona or all states bordering Mexico).

Finally, we look for an impact of the legislation on the Arizona housing market. Given that undocumented immigrants and the foreign-born are over-represented in rental housing, one would expect increases in rental vacancy rates as a result of LAWA-induced population loss but little impact on the vacancy rate for owner-occupied housing. Applying the synthetic control estimator to quarterly vacancy rate data, we find a large pre-post LAWA increase in rental vacancy rates but no corresponding changes in owner-occupied housing vacancy rates.

We argue that the synthetic control method yields causal estimates of LAWAs' impact on population composition in Arizona. We provide evidence that the potential endogeneity of the policy change, the timing of the Great Recession, and other immigration enforcement measures do not confound the estimates in this case.

2. The Impact of State Immigration Law on Population Movement

In recent years, there has been an unprecedented level of state legislative activity in the immigration policy domain. In 2009, state legislatures passed 333 immigration-related pieces of legislation, compared to only 38 during 2005. Regarding employment specifically, between 2005 and 2009, a total of 91 laws were enacted in 34 different states.¹ Many of these laws mandate the use of the federal E-Verify system for certain subsets of employers and impose penalties on both undocumented immigrants working illegally as well as on the employers that hire them.

Colorado was the first state to pass such legislation. Colorado's law requires any person or entity that has entered into a public contract with the state on or after August 2006 to certify that it has verified the legal status of all new hires using E-Verify. Similar laws or executive orders were enacted in Georgia in 2007, Rhode Island in 2008, Minnesota in 2008, Missouri in 2009, and Utah in 2009. South Carolina, Utah, and Mississippi have recently passed more expansive legislation that phases in an E-Verify mandate for all employers by firm size. In South Carolina, employers of all sizes are required to use E-Verify by July 2010. In Mississippi all employers will be required to use E-verify by July 2011. Utah's mandate covers all employers with 15 or more employees as of July 2010. Oklahoma constitutes a special case. While the first phase of the legislation was scheduled to go into effect in November 2007, a court

¹ Statistics cited in this paragraph are obtained from National Conference of State Legislatures (2006-2010).

challenge has held up implementation. To date, Oklahoma has yet to implement the provisions of its bill.

Arizona enacted what is arguably the most comprehensive legislation in this realm. The Legal Arizona Workers Act (LAWA) was signed into law in July 2007. LAWA mandates the use of E-Verify by all employers in Arizona to establish the identity and work eligibility of all new hires made after January 1, 2008.² The law imposes sanctions on employers who “knowingly” hire unauthorized immigrants including a business license suspension for the first offense and revocation upon a second. 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).³ This lack of sanctioning activity may reflect either weak enforcement of the law or a high degree of compliance on the part of Arizona employers.

Employers in Arizona are increasingly using E-Verify. In the state, the number of employers registered with E-Verify increased from less than 300 in March 2007 to over 38,000 in January 2010.⁴ Arizona’s enrollment is estimated to represent over one-third of all employers *nationwide* registered in the system and at least one-quarter of all employers in the state.⁵ Arizona’s employers are more than twenty times more likely to enroll than employers in California, another state with a large unauthorized immigrant population.⁶ Thus LAWA appears to have had an initial, sizeable impact on employer enrollment in E-Verify. Recent reports

² Note that LAWA predates Arizona’s more recent and even more widely debated law, SB 1070 of 2010, which more directly targets immigrants themselves rather than employers. Given that we measure the effects of LAWA in years completely predating passage of SB 1070, we do not expect that legislation to be driving our results.

³ Los Angeles Times (April 19, 2010).

⁴ Westat (2009) and Arizona Attorney General’s Office (2010), respectively.

⁵ Westat (2009), fraction nationwide as of June 2008, and Rosenblum (2009), fraction in Arizona as of February 2009.

⁶ Rosenblum (2009).

suggest that at least 700,000 new hires made between October 2008 and September 2009 were subject to E-Verify checks in Arizona.⁷ This correlates to roughly 50% of all new hires. Given this relatively high rate of usage as well as the high rate of enrollment, the potential effects of LAWA are sizeable. Furthermore, given these enrollment figures and the lack of sanctioning activity, we expect any observed effects to stem primarily from deterrence.

LAWA is distinctive among recent state legislation in that it was applied to all firms, rather than only those with public contracts, and all at once, rather than being phased in by firm size. In addition, sufficient time has passed to permit evaluation of the laws impact on population and economic outcomes. Importantly, Arizona has a large population of unauthorized immigrants (Passel and Cohen 2009a, 2009b).

To the extent that LAWA has made it more difficult for unauthorized immigrants to find work in Arizona, one would expect this to be reflected in the internal composition of state residents. Specifically, those planning to migrate illegally to Arizona may have decided to migrate elsewhere. Thus, undocumented migration may have contributed less to net population growth since the passage of LAWA than it would have in the absence of the legislation. Second, some portion of the undocumented immigrant population residing in Arizona prior to the passage and implementation of LAWA may have chosen to leave due to perceived and/or actual increases in the difficulty of finding employment.

Aside from reductions in the undocumented immigrant population, the legislation may also induce legal immigrants and perhaps some native-born to leave the state. This could occur through several channels. Some legal immigrants, naturalized citizens, and native born may have family members (spouses, parents, siblings etc) who are undocumented. Given that inter-

⁷ Berry, Jahna (Aug 17, 2010) “Arizona’s illegal immigrants can easily avoid E-Verify system”. The Arizona Republic.

regional mobility often involves entire households, and sometimes multiple households, some individuals legally authorized to work in the U.S. may leave Arizona along with their undocumented family members and intimates.

A further impetus to migration might occur through an increase in the difficulty finding employment among immigrants who are legally eligible to work in the U.S. This might occur due to an increased in statistical discrimination by employers against immigrants or those with Hispanic surnames. Alternatively, the E-verify system may in and of itself create more problems for the legal foreign-born. The system essentially compares the name and social security numbers of new hires against existing SSA and DHS records. If a match between provided information and the administrative records cannot be made, then the E-verify system returns a report of non-confirmation to the employer. A formal evaluation of E-verify by Westat (2007) found that less than 1 percent of natives but almost 10 percent of foreign-born U.S. citizens received an erroneous non-confirmation of work authorization. To the extent that such non-confirmations make it more difficult to find and hold employment, legal foreign-born residents of Arizona may have an incentive to move elsewhere.

To be sure, aside from migration LAWA may impact undocumented immigrants that choose to remain in the state. In particular, increased difficulty finding formal employment may lead to declining employment-to-population ratios or shifts towards informal work. The law may also impact the degree to which remaining undocumented workers engage the state in other domains (reporting crime and victimization to the police, using emergency room services in county hospitals, enrolling children in school etc). While these are certainly important topics for investigation, in this initial study we focus our efforts on assessing the laws impacts on aggregate population movements.

3. Empirical Methodology and Data Description

To assess the impact of LAWA on the internal composition of Arizona's resident population, we analyze data from all monthly Current Population Survey (CPS) data sets collected between January 1998 and December 2009. We combine files within years and estimate the proportion of residents that is Hispanic non-citizen and the proportion of residents that is Hispanic non-citizen with a high school degree or less. For each outcome, we analyze these proportions measured relative to several alternative base populations: (1) the total population, the population 15 years of age and older, and the population 15 to 46 years of age. Ideally, we would like to identify the proportion undocumented among the state population. However information on legal immigration status is not available in the CPS, or any suitable data source. Nonetheless, the proportion undocumented is certainly greater among non-citizen Hispanics than among the foreign-born more generally and even greater still among Hispanic non-citizens with relatively low levels of education.⁸ Hence, by comparing trends among these nested population groups, we can assess whether any population shift is most likely driven by net out-migration of the undocumented.⁹

⁸ One might also use year of arrival to further proxy for undocumented status. Specifically, those arriving in 1982 or prior were eligible for legalization under the general amnesty of the Immigration Reform and Control Act of 1986. Many with work experience in agriculture as late as 1985 were eligible to adjust their immigration status through the Special Agricultural Workers Amnesty. Hence, one might further limit the analysis to those who arrived 1986 or later. Unfortunately the monthly CPS data do not contain a question asking about year of arrival. Comparisons of population totals using March CPS data (which contains the year of arrival question) shows that the non-citizen Hispanic population is somewhat larger than the non-citizen Hispanic population arriving after IRCA (12,556,632 vs 10,688,817 in 2009). Among Hispanic non-citizens 15 or over with a high school degree or less, over 80 percent arrived post IRCA by 2009. Among the subset of prime age, less educated, non-citizen Hispanic immigrants (15 to 45) this figure stands at over 90 percent.

⁹ Estimates suggest that as of 2009, 80% of unauthorized immigrants nationwide were Hispanic, 58% were between the ages of 18-39, and the majority have fewer years of formal education (Passel and Cohn, 2010). In the subgroup of "likely unauthorized" defined as Hispanic non-citizen immigrants of working age with no more than a high school diploma, we estimate that 90% in Arizona were unauthorized. For example, our calculations from the 2008 American Community Survey indicate that roughly 517,000 non-citizen Hispanic immigrants resided in Arizona in 2008. For this same year, Passel and Cohn (2009) estimate that there were 475,000 unauthorized immigrants in the

Table 1 describes trends in these population groups for the period from 1998 to 2009. Recall, LAWA is passed in mid-2007 and implemented in January 2008. Hence, the last two years constitute the post-treatment periods while population responses in 2007 are possible through migration in anticipation of LAWA's implementation. The proportion of Arizona residents that is non-citizen exhibits a modest upward trend between 1998 and 2006, increasing from 9.9 percent to 11.1 percent over this period. Beginning in 2007, the proportion non-citizen begins to decline reaching 8.3 percent by 2009 (a decline relative to 2006 of 2.8 percentage points). Population trends among Hispanic non-citizens are similar. There are slight increases in the proportion of the Arizona population described by this category between 1998 and 2006. Post 2006, we observe a decline of 2.6 percentage points. Focusing specifically on the proportion of Arizona residents 15 to 45 years of age, we observe substantial increase in the proportion of this subset of the population that is Hispanic noncitizen between 1998 and 2006 (from 12.1 to 14.3 percent). By 2009, this proportion declines to 10.4 percent, a level below all of the annual values displayed in the table.

The CPS inquires about the highest level of completed education for individuals 15 years and older. Table 1 also presents trends in the proportion of the population 15 and over and 15 to 46 that are Hispanic noncitizens and that are described by specific levels of educational attainment. Among both the population 15 and over and the population 15 to 45, post-LAWA relative population declines are concentrated among those with a high school degree or less. There is no measurable decline or negligible declines among non-citizens with higher levels of schooling.

state. Similarly, for the "likely unauthorized" subgroup mentioned above, we estimate that 229,000 were in the labor market in Arizona in 2008 compared to the Passel and Cohn (2009) estimate of 240,000.

Hence, there is a notable pre-post decline in the proportion of Arizona residents that are non-citizen, with much of this decline attributable to declines in the population of Hispanic non-citizens. Moreover, while the trends by educational attainment pertain to a subset of the Arizona population (those 15 and older), these more detailed calculations suggest that much of the relative population decline among the foreign-born was concentrated among relatively less educated immigrants. As all three of these traits – non-citizen, Hispanic, and lower levels of educational attainment – are predictive of undocumented status (Passel and Cohen 2009a, 2009b), the raw patterns in Table 1 are consistent with a population response on the part of the undocumented to LAWA’s passage.

To assess whether the observed relative population declines of the foreign-born are being driven by a response to LAWA, we need to identify a comparison state or states that we can use to chart the counterfactual path of population trends for Arizona. There are several strategies for constructing such a comparison group. One possibility would be to select states that one could reasonably argue share similar population and economic characteristics; for example, all states bordering Arizona. Comparable arguments could be made for using all states that share a border with Mexico. An alternative strategy would be to employ a data-driven search for a comparison group based on pre-LAWA population characteristics and trends. Here, we pursue this latter tack.¹⁰

We employ the synthetic control method developed by Abadie et. al. (2010) to chart a counterfactual post-LAWA path for Arizona. Specifically, let the index $j=(0,1,\dots,J)$ denote states. The value $j=0$ corresponds to Arizona and $j=(1,\dots,J)$ correspond to each of the other J states that are candidate contributors to the control group (or in the language of Abadie et. al, the

¹⁰ We also conducted a traditional difference-in-difference approach with hand-selected comparison states and found similar results.

donor pool). Define F_0 as a $k \times 1$ vector with elements equal to the proportion of the Arizona population that is non-citizen Hispanic in each year from 1998 through 2006 (the nine years we use throughout this paper as our pre-intervention period) plus additional covariates predictive of the presence of non-citizen Hispanics (to be discussed shortly). Similarly, define the $k \times J$ matrix F_1 as the collection of comparable data vectors for each of the J states in the donor pool (with each column corresponding to a separate state-level vector).

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 $J \times 1$ weighting vector $W = (w_1, w_2, \dots, w_J)'$ such that $\sum_{j=1}^J w_j = 1$, and $w_j \geq 0$ for $j = (1, \dots, J)$. The product $F_1 W$ then gives a weighted average of the pre-intervention vectors for all states omitting Arizona, with the difference between Arizona and this average given by $F_0 - F_1 W$. The synthetic control method essentially chooses a value for the weighting vector, W , that yields a synthetic comparison group (consisting of an average of some subset of donor states) that best approximates pre-intervention Arizona. Specifically, the weighting vector is chosen by solving the constrained quadratic minimization problem

$$(1) \quad \begin{aligned} W^* &= \arg \min_W (F_0 - F_1 W)' V (F_0 - F_1 W) \\ & \text{s.t.} \\ & W' i = 1, \quad w_j \geq 0, \quad \text{for } j = (1, \dots, J) \end{aligned}$$

where V is a $k \times k$, diagonal positive-definite matrix with diagonal elements providing the relative weights for the contribution of the square of the elements in the vector $F_0 - F_1 W$ to the objective function being minimized.¹¹

¹¹ The Stata procedure developed by Abadie et. al. (2010) uses as the default a regression-based measure of V where those matching variables that are strong predictors of the dependent variable are given more weight and where the

Once an optimal weighting vector W^* is chosen, 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 our counterfactual outcomes for Arizona. In addition to including all pre-intervention values of the dependent variable in F_0 and F_1 we also include average values of the proportion of the state work force in each of nine industrial categories, the proportion of the state population in each of four broad educational attainment categories (less than high school, high school graduate, some college, college or more), and the state unemployment rate. These additional covariates are measured for three time periods (1998 through 2000, 2001 through 2003, and 2004 through 2006).¹²

Our principal estimate of the impact of LAWA on population outcomes uses the synthetic control group to calculate a simple difference-in-differences estimate. Specifically, define $Outcome_{pre}^{AZ}$ as the average value of the outcome of interest for Arizona for the pre-intervention period 1998 through 2006 and $Outcome_{post}^{AZ}$ as the corresponding average for the two post-treatment years 2008 and 2009. Define the similar averages $Outcome_{pre}^{synth}$ and $Outcome_{post}^{synth}$ for the synthetic control group. Our difference-in-differences estimate subtracts the pre-intervention difference between the averages for Arizona and synthetic Arizona from the comparable post-intervention difference, or

elements of V are normalized such that they sum to one. Since we are matching on all pre-intervention annual values of the dependent variables, this default matrix provides fairly equal weight on the match for each year. Our inclusion of covariates does not alter this relative weighting. We have estimated all of these models constraining the weights in V to being equal (i.e., set $V=I$) across pre-intervention values and have also estimated fully nested models that choose both optimal values of V as well as W (as in Abadie and Gardeazabal 2003). As the results were virtually indistinguishable from the results using the program’s default V , we report the default estimates throughout.

¹² Our estimation results matching only on pre-intervention values of the dependent variable are nearly identical to the results when covariates are included.

$$(2) \quad DD_{AZ} = (Outcome_{post}^{AZ} - Outcome_{post}^{synth}) - (Outcome_{pre}^{AZ} - Outcome_{pre}^{synth}).$$

To the extent that LAWA induced net migration of the foreign-born out of Arizona, one would expect to find that $DD_{AZ} < 0$.

To formally test the significance of any observed relative decline in Arizona’s foreign-born population, we apply the permutation test suggested by Abadie et. al. (2010) to the difference-in-difference estimator displayed in equation (2).¹³ Specifically, for each state in the donor pool, we identify synthetic comparison groups based on the solution to the quadratic minimization problem in equation (1). We then estimate the difference-in-difference in (2) for each state as if these states had passed the equivalent of a LAWA with comparable timing (passed in mid-2007 and implemented in January 2008). The distribution of these “placebo” difference-in-difference estimates then provides the equivalent of a sampling distribution for the estimate DD_{AZ} . To be specific, if the cumulative density function of the complete set of DD estimates is given by $F(\cdot)$, the p-value from a one-tailed test of the hypothesis that $DD_{AZ} < 0$ is given by $F(DD_{AZ})$.

In selecting a synthetic control group for Arizona, we omit from the donor pool four states with broadly applied (in terms of employer coverage) restrictions on the employment of undocumented immigrants (Mississippi, Rhode Island, South Carolina, and Utah). In addition, in identifying synthetic control groups for each of the remaining states in the donor pool, we omit Arizona. Since Arizona experiences sharp declines in the foreign-born population pre-post LAWA, omitting Arizona from the donor pool for estimating the placebo intervention effects

¹³ Buchmueller, DiNardo and Valletta (2011) use a similar permutation test to that described here to test for an impact of Hawaii’s employer-mandate to provide health insurance benefits to employees on benefits coverage, health care costs, wages and employment.

should impart a negative bias to these placebo estimates (a specification choice that should make it more difficult for us to find a significant effect).¹⁴

Table 2 displays the states receiving positive weights in the construction of synthetic Arizona for three of our outcomes of interest (essentially, the positive elements in the solution vector W^*). As can be seen, the states contributing to the synthetic control group as well as the weights assigned across states varies across the dependent variables. California received positive weight for all three dependent variables ranging from 0.487 for the proportion non-citizen Hispanic with high school or less among the prime age, to 0.747 for the proportion non-citizen Hispanic among all residents. This is not particularly surprising given the relatively large foreign-born Hispanic population in California. Perhaps more surprising is the positive weight placed on Maryland and North Carolina. While these states have relatively small non-citizen Hispanic populations, growth in these “new destination” states during the early 2000s parallels that of Arizona.

4. Validating the Identification Strategy

Our empirical strategy requires that the enactment of LAW A represents an exogenous shock to the labor market. For example, if high unemployment among the foreign born and the attendant problems led states to enact legislation attempting to discourage future migration to the state, any inference on the effect of such legislation on labor market outcomes would be compromised. In fact, LAW A was debated and passed during a period of economic growth but was enacted at a time of declining labor market conditions in Arizona.

¹⁴ That is to say, as the proportion non-citizen Hispanic drops sharply in Arizona including Arizona in the donor pool for each placebo estimate should bias the placebo estimates towards zero and increase the likelihood that the permutation test will yield a significant effect for Arizona proper. For this reason, we omit Arizona from the donor pools for each of the 46 placebo estimates.

A number of facts suggest that the passage and enactment of LAWA was not driven by employment conditions in the state at the time but instead reflected Arizona's perceived long-term problem of unauthorized immigration, also experienced by other states. To start, LAWA represents the ultimate manifestation of a fairly lengthy legislative debate that crossed multiple legislation sessions. Moreover, there was considerable uncertainty as to whether LAWA would be enacted on January 1, 2008. Federal lawsuits challenging the constitutionality of LAWA were brought by an alliance of civil rights advocates, business interests and immigrant rights groups. The challenge was dismissed, but not until early December. Anecdotal evidence suggests that those likely to be affected by actual implementation followed the court challenge and were conditioning their responses on the ultimate legal outcome (see *The Arizona Republic*, October 8, 2007).

Although Arizona's employment legislation has the potential to impact the labor market, a number of other forces also drive those conditions. Under the synthetic control approach, unless the timing of these other forces was coincident with that of LAWA, we argue that our estimates represent the causal relationship between LAWA and Arizona's population. This argument hinges on the ability of the synthetic control method to (1) match Arizona's pre-LAWA trends with those of other states and (2) determine whether Arizona's pre-post changes stand out from the placebo estimates for all other states. We will show in the following section that both of these conditions are satisfied. Given that, we need only address the two major factors coincident with LAWA that could potentially invalidate the claim of causality.

First, we are concerned about the potential coincidence of federal immigration enforcement increases with the enactment of LAWA. Enforcement enhancements could cause declining unauthorized population in Arizona via deterrence or directly through increased

apprehension and deportation. While the U.S. Border Patrol (USBP) has launched a number of enforcement initiatives over our analysis period, only those exactly coincident with LAWA and unique to Arizona threaten our identification strategy. The enforcement policies meeting these criterion include potentially (1) Operation Streamline implemented in the Tucson sector (covering the vast majority of the Arizona border) in January 2008¹⁵, and (2) border infrastructure enhancements in the Southwest Region over 2005-2009.¹⁶ We review official apprehension data, policy information, and research on the efficacy of these policies. Our review finds no compelling evidence that these disproportionately affected the unauthorized population in Arizona coincident with the implementation of LAWA. While apprehensions declined 16% in Tucson in 2008, the share of all Southwest border apprehensions in Tucson remained remarkably stable (between 44-45% over 2007-2009, and never below 36% over a 10 year period).¹⁷ Apprehension data exhibits sizeable swings in many years and across all regions (for example, apprehensions in Tucson declined 27% in 2001 and increased 42% in 2004) presumably because they reflect a variety of countervailing effects: deterrence, changes in propensity to migrate, enforcement effort, and enforcement policy. However, the remarkably stable share of apprehensions occurring in Arizona before and after LAWA suggests the combination of these

¹⁵ Kerwin and McCabe (2010).

¹⁶ The Arizona Border Control Initiative built up infrastructure on Arizona's border with Mexico, but predated LAWA by a few years. Policies that may be coincident with LAWA are (1) Operation Streamline which greatly enhanced prosecution of unauthorized crossers in the Southwest border region between 2005-2009, (2) the Secure Fence Act of 2006 mandating construction of 670 miles of reinforced fencing on the Southwest Border by 2008 and, (3) the Secure Border Initiative (SBI) over 2005-2011 involving primarily technological enhancements to border security. SBI is the easiest to address: while scheduled to be installed on the Arizona border in February 2008, delays plagued the program until its eventual cancellation due to cost and inefficacy in 2011. While it is more difficult to ascertain exactly when various fence infrastructure was built specifically in Arizona, our review of apprehension data does not suggest that if it was built coincident with LAWA it had any sizeable impact on border crossing as measured by apprehensions.

¹⁷ Note that the Tucson sector covers the vast majority of the Arizona border and inland area; it also accounts for the largest share of border arrests in the Southwest region. In the Yuma Sector (extreme western border of Arizona) apprehensions declined 70% in 2007 and 79% in 2008, but comprised only 1-4% of all apprehensions in the Southwest region over this period.

forces did not disproportionately impact Arizona by diverting attempted border crossing either to or away from the state. While the deterrent effect of enforcement cannot be measured directly, the literature suggests this is unlikely to drive our results. First, on-the-ground evidence from Operation Streamline suggests little potential deterrent effect: migrants were largely unaware of the highly enhanced penalties to unauthorized crossing.¹⁸ Furthermore, a number of studies find labor market conditions and the costs of migration play a larger role in deterring unauthorized migration than border enhancements.¹⁹

Second, the “Great Recession” occurred at approximately the same time of the enactment of LAWA. There is evidence that the recession reduced the inflow of new immigrants to the US and new immigrants to Arizona. Our empirical approach comparing trends in Arizona to other states already accounts for any changes that affect the country as a whole (or the selected comparison states). However, one of the industries hit hardest, construction, is a leading employers of unauthorized immigrants. Furthermore, construction is one of the biggest industries in Arizona (representing close to 11 percent of total private employment in 2006) so the state’s economy can be impacted significantly by declines therein. Thus, it is important in our evaluation strategy to ensure that we do not attribute changes in population to LAWA if they were in fact driven by the decline in construction and real estate in Arizona specifically. To validate our empirical approach, we assess official statistics on employment trends in Arizona and neighboring states during the recession.

The recent recession caused a clear reduction in Arizona’s workforce. Figure A1 shows strong employment growth 2003-2006 with a noticeable slowdown in 2007. This was followed by three and eight percent decreases in 2008 and 2009, respectively. Figure A1 also shows that

¹⁸ Lydgate (2010) in interviews with federal defenders.

¹⁹ Kerwin and McCabe (2010), Cornelius et al (2010), and Roberts et al (2010). As the latter study shows, border security enhancements impact the cost of migration, as measured over 2 year periods (thus likely with a lag).

the negative employment effects of the recession on employment were not any stronger in Arizona than it was in neighboring areas, including inland California (an area that shares many of the characteristics and trends of Arizona, is hence used in our empirical analysis). Lastly, an application of the synthetic cohort method to employment growth fails to reveal a LAWA effect in Arizona.

Importantly, the recession was precipitated by a housing crisis, which brought new housing construction to a near standstill. The fact that many unauthorized immigrants are, or maybe more accurately were, employed in the construction sector means that they may have been particularly affected by the recession. However, a look at construction employment data reveals no evidence that Arizona's construction industry fared much differently in the recession than its neighboring areas (Figure A2).

Overall, the data indicates that while Arizona's labor market was strongly affected by the recession, so were other states', including its neighbors. The similarity in trends indicates that our empirical strategy is appropriate for identifying causality despite the recent recession.

5. Basic Results

We begin with a graphical presentation of the Arizona population trends and the comparable population trends in synthetic Arizona for our three outcomes. Figure 1 presents the proportion of each population that is non-citizen Hispanic. Figure 2 presents the proportion of the population 15 and over that is non-citizen Hispanic with a high school degree or less. Finally, Figure 3 presents the proportion non-citizen Hispanic with a high school degree or less for the population 15 to 45 years of age. Focusing first on the pre-intervention period 1998 through 2006, the figures reveal that population trends for the synthetic

control groups closely match corresponding population trends in Arizona. Average pre-intervention differences between Arizona and the synthetic control groups are near zero for each outcome, with quite small root mean squared errors (.00438 for the proportion non-citizen Hispanic, .00386 for the proportion non-citizen Hispanic with high school or less among those 15 plus, and .00953 for the proportion less educated non-citizen Hispanic among those 15 to 45). Hence, the synthetic control groups match the pre-intervention values for Arizona quite well for each of the outcomes.

Regarding the post-intervention period, for each of the outcomes we observe sizable gaps (on the order of 1.5 to 2 percentage points) between Arizona and the synthetic control groups. For all outcomes, the gaps relative to the synthetic controls do not widen until 2008, and are wider still by 2009. Thus, the declines in the immigrant population observed in Arizona are not observed in states with comparable pre-LAWA population composition and dynamics.

Figures 4 through 6 graphically display the raw data needed to conduct the permutation test of the significance of the relative declines in Arizona. Specifically, for each of the 46 donor states as well as for Arizona, the figures display the year-by-year difference between the outcome variable for the “treated” state and the outcome variable for the synthetic control. The differences for each of the donor states are displayed with the thin black lines while the differences for Arizona are displayed by the thick line. There are several notable patterns in these figures. First, during the pre-intervention period 1998 through 2006, the differences for Arizona clearly lie within the distribution of placebo estimates, suggesting that Arizona is not an outlier during this period. There are several states, California in particular, with very large pre-intervention differences relative to its synthetic control group. For California, this is driven by

the fact that the state has the highest values for the dependent variables of all states in the donor pool, and hence it is impossible to match the state with a convex combination of other states.

Second, for the post intervention years as the difference values for Arizona turn negative, Arizona moves to the bottom of the distribution in each graph. By 2009 the state becomes a visible outlier. This pattern is observed for all three outcome variables, with the departures for Arizona particularly large in absolute value for non-citizens and Hispanic non-citizens.

Table 3 presents estimates of several variants of the difference-in-differences estimator laid out in equation (2) above. For each outcome, the first two columns present the mean difference between Arizona and the synthetic control for two different groupings of the pre-intervention years: (1) 1998 through 2006, and (2) 2005 through 2006. The third column presents the average post-intervention difference (Arizona minus synthetic Arizona) for 2008 and 2009. The remaining columns present difference-in-difference estimates of the population effect of LAWA, the rank of the estimate for Arizona relative to the complete distribution of 47 estimates (one for Arizona and 46 placebo estimates), and the p-value from a one-tailed test of the likelihood of observing an estimate at least as negative as that for Arizona. Note, this P-Value from this test is bounded from below by 0.021 (1/47). The table first presents these difference-in-difference results using the nine-year pre-intervention base period and then presents the results using the two-year pre-intervention base period.

The results in panel A show the estimates for the proportion non-citizen Hispanic relative to the entire resident population. The average difference relative to synthetic Arizona is basically zero in both of the defined pre-intervention periods. This difference however widens to -1.5 percentage points in 2008/2009. The difference-in-difference estimates are not sensitive to which pre-intervention base period is chosen (with DD estimates of -0.015 and -0.014). In both

comparisons, Arizona's difference-in-difference estimate is the most negative, yielding the minimum P-value of 0.021.

Panel B presents comparable results for the proportion non-citizen Hispanic and non-citizen Hispanic with high school or less measured relative to the population 15 years of age and older. Again, pre-intervention differences in these outcomes (Arizona minus synthetic Arizona) are very small (never greater than 0.001 in absolute value). Post-intervention, the differentials widen to -1.2 to -1.3 percentage points. Regarding the difference-in-difference estimates for the two outcomes and the two alternative pre-intervention periods, the four estimates range from relative declines in the specific immigrant population in Arizona of 1.1 to 1.4 percentage points. In all instances, the Arizona estimates are the most negative relative to the distribution of placebo estimates, yielding the lowest possible p-value.

Finally, Panel C presents comparable results to those presented in Panel B, where the population changes are measured among those 15 to 45 years of age. Again, we find relatively small pre-intervention differentials between Arizona and synthetic Arizona in each comparison that widen considerably in the post-intervention period (to -2.6 percentage points for non-citizen Hispanics and -2 percent for non-citizen Hispanics with high school or less). The difference-in-difference estimates are generally larger when we focus on the prime working age population, ranging from -1.6 to -2.7 percentage points. Three of the four estimates are the most negative when compared to the distribution of placebo estimates, while one ranks second out of 47. All yield p-values of the one-tailed test of the null hypothesis of negative change for Arizona that are below 5 percent.

One can use the difference-in-difference estimates to calculate the net decline in population caused by the passage and implementation of LAWA. In terms of actual people,

Arizona's population in 2006 stood at approximately 6.2 million. The difference-in-difference estimates measured relative to the entire resident population in Panel A imply population loss ranging from 86,800 to 93,000.

To summarize the results, we find pre-post LAWA declines in the representation of Hispanic non-citizens among the Arizona resident population. Corresponding declines do not occur in the synthetic control group. Most of the decline is concentrated among non-citizen Hispanics with a high school degree or less that are of prime working age. For all outcomes in both sets of estimates, the relative declines in Arizona fall in the extreme lower tail of the distribution of placebo estimates – i.e., the difference-in-difference estimates for Arizona are the most negative.

6. Robustness Checks and Exploring Effect-Size Heterogeneity

In this section, we probe the robustness of the main results and explore whether the population responses vary within sub-groups of the foreign-born population. Specifically, we first assess whether our focus on proportion rather than actual counts may be leading to faulty inference driven by surge in domestic migration to Arizona. We then explore whether the estimation results are sensitive to the definition of the post-treatment period and the extent to which cross-state spillover may be biasing our difference-in-differences estimates. We also explore whether employing more traditional difference-in-difference estimators relying on more ad-hoc choices of control states yield fundamentally different results. Finally, we test for effects of LAWA on a series of alternative population and housing outcomes for which we have priors regarding the likely impact of the legislation.

A. Some specification checks

We begin by exploring whether the observed impacts on the proportion of the Arizona population noncitizen Hispanic is driven by a surge in population growth among the native born or perhaps the naturalized foreign born or by a true reduction in the presence of noncitizen Hispanics. Table 4 presents estimates from the monthly CPS files of the Arizona native-born, naturalized foreign-born, foreign-born noncitizens, and noncitizen Hispanic populations.²⁰ Beginning with the patterns in the last two columns, we observe a steady increase in the non-citizen Hispanic population between 1998 and 2006, with an annual average growth rate of 5.3 percent. Between 2006 and 2009 the non-citizen Hispanic population declines absolutely by 125,549. Similar patterns are observed for the overall foreign-born non-citizen population, with a decline from 2006 to 2009 in this broader population of 129,543. The native population does indeed grow between 2006 and 2009; however, not at a rate that exceeds that of the pre-LAWA period. From 1998 to 2006 the native born population increased at an annual average rate of 3.1 percent. The comparable growth rate for 2007 and 2008 are 2.8 and 3.5 percent respectively. Hence, these raw figures indicate that our difference-in-difference estimates are being driven by absolute declines in the noncitizen Hispanic population.

In Table 3, we define the post-period as calendar years 2008 and 2009 due to the fact that LAWA was implemented on January 1, 2008. One might contend that 2007 should be included as a post-treatment year as the legislation was passed mid-2007 and households may have migrated in anticipation of the law's passage and implementation. In all of the estimates that we have presented thus far, we have not matched the treatment to the synthetic controls with 2007 values and have omitted this year from our post-treatment period.

²⁰ Population estimates are tabulated by summing the population weights from the monthly files within year and then dividing by 12.

Panel A of Table 5 presents comparable estimates to those in Table 3, but that include 2007 in the post-treatment period. Here we focus only on the results for the proportion Hispanic noncitizen among all Arizona residents, the proportion less-educated Hispanic noncitizen among those 15 and over, and the proportion less-educated Hispanic noncitizen among those 15 to 45. The relative population declines for Arizona including the 2007 population are somewhat smaller (by roughly one half to seven-tenths of a percentage point). This is not too surprising as Figures 1 through 3 above reveal relatively similar outcome levels for Arizona and synthetic Arizona in 2007. It is still the case, however, that the difference-in-difference estimates for Arizona are among the most negative relative to the distribution of placebo estimates. For the proportion noncitizen Hispanic among the entire population, Arizona's estimate ranks first out of 47 using both pre-treatment time periods. For the proportion less-educated non-citizen Hispanic among those 15 and over, the two difference-in-difference estimates have p-values of 4.3 and 6.4 percent. For the final comparison (less educated noncitizen Hispanic among those 15 to 46), both difference-in-difference estimates have p-values of 0.043.

Clearly, 2007 is a problem year. One might expect an anticipatory effect prior to implementation and hence would not want to match on the 2007 value. However, any anticipatory effect should be small as the mandatory use of E-verify does not commence until January 2008 and since the enhanced verification requirement did not apply retroactively to past hires. This latter fact alone suggests that the proportion of pre-LAWA Arizona residents impacted by the law should increase with time and that the initial impact prior to implementation should be small. Based on this reasoning, we prefer the estimates in Table 3 that omit the 2007 values from any calculations.

An additional issue concerns potential bias caused by population spillover created by migration out of Arizona into other states across the nation. Specifically, Arizona's population loss may be due either to deterred future migration, foreign migrants leaving the country, or migrants leaving for other states. If the latter is an important contributor to state population among those states contributing to the synthetic control group, then the suitability of the post-treatment path for the synthetic control group in charting the counterfactual for Arizona is compromised. This might be a particularly important source of bias if migrants leave Arizona for California since California contributes disproportionately to the synthetic control group for each of the outcomes we analyze.

In the current application, there are several reasons to believe that such spillover is quantitatively unimportant. To start, the absolute declines in the proportion of the Arizona population that falls into our three categories are comparable in magnitude to the declines measured relative to the synthetic controls. For example, averaging the pre and post-intervention values in Table 1 using the period definitions employed in Table 3 shows an absolute decline in the proportion of Arizona residents that is Hispanic noncitizen of 1.3 percentage points (compared with our difference-in-difference estimate of 1.4 to 1.5 percentage points). The comparable absolute declines for the proportion low education Hispanic noncitizens 15 and over and 15 to 45 Hispanics are 1.2 and 1.8 percentage points, respectively. Hence, the relative declines that we estimate in Table 3 are driven primarily by compositional changes in Arizona rather than compositional changes in the states contributing to the synthetic control groups.

Second, Arizona is a small state. The impact of a modest population decline in Arizona on the population of neighboring states is bound to be small. For example, Arizona's 2007 population stood at approximately 6.25 million persons. Our difference-in-differences estimates

suggest that the proportion Hispanic noncitizen declined by 1.5 percentage points. Relative to 2007, this corresponds to a LAWA-induced absolute population loss of roughly 93,750. Suppose that the entire 93,750 foreign-born moved to neighboring California (the only state bordering Arizona that contributes to the synthetic control in any of our comparisons). Such a population move would increase the proportion of California residents that is noncitizen Hispanic from the actual value in 2007 of 0.110 to the hypothetical value of 0.113. Moreover, since California never contributes more than 75 percent to the synthetic controls for any of our outcomes, the impact of such cross-border spillover on the post-treatment values for the synthetic control would be even smaller than what is implied by this hypothetical exercise.

Finally, when we restrict the donor pool to states that do not share a border with Arizona²¹ the difference-in-difference estimates as well as the statistical inferences are quite similar to our estimates in Table 3. Since one might expect the largest effects of population spillover on the populations of neighboring states, omitting these states from the donor pool provides a key robustness check. These results are presented in Panel B of Table 5. Omitting the states that share any border with Arizona yields difference-in-difference estimates that are essentially the same as those that include these states in the donor pool (the estimates reported in Table 3). Moreover, the observed DD estimates for Arizona are still more negative than each of the remaining 42 placebo estimates for all three outcome variables.

There is an additional concern regarding our definition of the post-intervention period. In April 2010, the governor of Arizona signed into law the Support Our Law Enforcement and Safe Neighborhoods Act, commonly referred to as SB1070. This legislation made it a misdemeanor to be an undocumented immigrant in Arizona, targeted enforcement and sanctions against those

²¹ Throughout the analysis we have been omitting Utah from the donor pool due to the presence of comparable (yet not identical) state legislation. In the tabulations in Table 4 Panel B we further drop California, Colorado, Nevada, and New Mexico from the potential donor pool.

who harbor undocumented immigrants, and authorized local law enforcement to detain those suspected of being illegal. While the law was scheduled to go into effect in July 2010, a federal court issued an injunction preventing the implementation of much of the law. This injunction was appealed to the 9th U.S. Circuit Court of Appeals, which upheld the injunction in April of 2011. Hence, much of SB1070 has yet to be implemented and may never will.

Our chosen post period ends in 2009. We chose to analyze two years post implementation of LAWA as one would expect the effects of LAWA to be cumulative since the E-verify requirement does not apply retrospectively to hires made before January 1, 2008. However, to the extent that unauthorized immigrants began to leave the state in 2009 due to anticipation of harsher treatment under SB1070, part of the effect that we are attributing to the E-verify mandate may be driven by proactive outmigration.

Since SB1070 passed a quarter of the way through 2010 and was not scheduled to be implemented until July 2010, we believe that this is likely to have little influence on our estimates. However, one could drop 2009 from the post-treatment period and estimate the difference-in-difference population effects using 2008 as the single post-intervention year. Table 6 presents the results using this alternative strategy. Here, we simply use the single year 2006 for the pre-period, though using alternative pre period (2005/2006 or 1998 through 2006) yields similar results. All of our point estimates dropping 2009 are smaller than the estimates presented in Table 3. For the proportion noncitizen Hispanic, the estimate yields a decline in Arizona of 1.2 percent with a p-value of 0.043. We find a decline in the proportion low-education noncitizen Hispanic for those 15 and over of .9 percentage points, smaller than our estimates in Table 3 yet still significant at the five percent level. Our estimate for the proportion low education noncitizen Hispanic among those 15 to 45 is the only estimate that is not statistically

significant. Hence, dropping 2009 does yield smaller estimates. However, we prefer the results inclusive of 2009 given the likely cumulative effect of LAWA and the likely uncertainty regarding whether SB1070 would ever be implemented.

A final set of specification checks that we explore involve testing for an impact on our chosen dependent variables using a more traditional difference-in-difference estimator and inference techniques based on the actual CPS microdata. We explored three alternatives. Our first employed the weights from Table 2 derived from application of the synthetic cohort estimator to select comparison states and reweights the contribution of each observation to the control group such that the cumulative weight associated with the observations from a state matches the weights in Table 2. Second, we used all states that share a border with Arizona as the control group. Finally, we used all states that share a border with Mexico as a control group. All three sets of results yield statistically significant (at the one percent level of confidence) difference-in-difference estimates that are similar in magnitude to the estimates presented in Table 3.²²

B. Testing for Effects of LAWA on Alternative Population and Population-Related Outcomes

The enhanced employment verification requirements of LAWA are targeted specifically at foreign-born job seekers that are unauthorized to work in the United States. Thus, to the extent that there is a migratory response to the legislation, one would expect the largest population impact on groups with high proportions unauthorized. Conversely, while legal immigrants may also leave the state due to social connections with unauthorized immigrants, due to increased discrimination against all foreign-born, or due to a perceived increase in hostility towards immigrants, one would expect smaller population changes among the authorized.

²² These additional results are available upon request.

Hence, one key falsification check is to test for an impact of LAWA on the proportion of the Arizona population that is foreign born yet legally residing within the state.

In addition, a sudden change in population should have derivative impacts on other outcomes. Perhaps the most obvious place to look would be the Arizona housing market. As we will soon document, immigrants accounted for a relatively large share of households residing in rental housing in pre-LAWA Arizona. Moreover, the majority of the Arizona population resides in owner-occupied housing. In conjunction, these two facts suggest that a LAWA-induced population loss should have a larger impact on the market for rental housing than on the market for owner-occupied housing.

In this sub-section we present evidence pertaining to these falsification tests. We begin by testing for an impact of LAWA on the proportion of Arizona residents that are Hispanic, naturalized citizens. Figure 7 displays trends in the proportion that are Hispanic naturalized citizens for Arizona and for the synthetic control for Arizona for the period 1998 through 2009. Relatively few Arizona residents fall into this category, with the highest value for Arizona of approximately 0.03 in 2009. Despite a dip in this series in 2007, the proportion of Arizona residents that are Hispanic naturalized citizens appears roughly stable through the implementation of LAWA. Figure 8 displays the difference for each year between Arizona and the synthetic control group along with the placebo difference series for each of the 46 states in the donor pool. The drop in this variable in 2007 for Arizona certainly stands out. However, by 2009 the difference for Arizona lies well within the distribution of placebo estimates for the other states.

The first row of Table 7 presents the results from applying our difference-in-difference estimator to this particular dependent variable. For the period 1998 through 2006 the average

difference between Arizona and its synthetic control group is zero. For the two post-intervention years (2008 and 2009), the difference widens slightly to -0.003. This ranks fifth out of the 47 estimates yielding a P-value of the one-tailed test for a decline in this population variable of 0.106. Taken together with the patterns documented in Figures 7 and 8, there appears to be little evidence that naturalized Hispanics responded to LAWA by migrating from the state.

Regarding the Arizona housing market, prior to the passage of LAWA the foreign-born in Arizona were disproportionately concentrated in rental housing. Our tabulations of data from the 2006 American Community Survey (ACS) show that among Arizona households headed by the foreign-born, roughly 41 percent resided in rental housing compared with 28 percent of households headed by the native born. Among households headed by a noncitizen, 53 percent rent, while the comparable figure among households headed by a Hispanic noncitizen is 56 percent. The relatively high proportion of immigrants in rental housing combined with the fairly sizable foreign-born population in Arizona naturally implies that immigrants comprise a fairly large portion of the demand side in Arizona's market for rental housing. Indeed, in 2006 immigrant-headed households occupy over one fifth of the state's rental housing. The comparable figures for noncitizen and noncitizen Hispanic households are 17 and 14 percent, respectively.

Given the relative concentration of immigrants in rental housing, population losses on the order implied by our difference-in-difference estimates in Table 3 should disproportionately impact the Arizona rental market. Here we assess this proposition by testing for pre-post LAWA changes in the rental housing vacancy rate and the owner-occupied housing vacancy rate. To do so, we use quarterly vacancy rate data from the first quarter of 2005 through the last quarter of 2009 from the Current Population Survey/Housing Vacancy Survey (CPS/HVS). We apply the

synthetic control procedure to these data to identify a rental vacancy series for synthetic Arizona and then use this series to calculate difference-in-difference estimates for these housing outcomes. Since we have quarterly data, we define the pre-intervention period as all quarters prior to quarter three 2007. To identify the states contributing to the synthetic control, we match on annual average vacancy rates for the pre-intervention period as well as the seasonal averages of these values (the average of the three quarter one values, the three quarter two values etc) to adjust for seasonal variability in vacancy rates. In addition, we match on a number of covariates that are likely predictors of housing market vacancy rates. In particular, we match on pre-intervention values of the proportion of state residents in metropolitan areas, the age distribution of state residents (proportion under 18, 18 to 29, 30 to 39, 40 to 49, 50 to 64, and 65 and over), the proportion nonwhite, the proportion Hispanic, the proportion foreign-born, the proportion poor, and the proportion that rent. We tabulate these covariates from the 2005 through 2007 American Community Survey.

Before discussing the estimates, it is instructive to work through a simple back-of-the-envelope calculation regarding the likely size of the impact one might expect from a sudden decline in the foreign-born population on housing vacancy rates. In 2006, renters account for 29.8 percent of Arizona households. Our main difference-in-difference estimate suggests that LWA reduced the proportion of the Arizona population that is noncitizen Hispanic by 0.015. If we assume that this translates into a 1.5 percentage point decline in the number of Arizona households²³ and that the entirety of this decline occurs among rental households, then the rental vacancy rate should increase by 5.03 percentage points ($[1.5/29.8] \times 100$).

²³ A decline in the foreign-born population would impact both the numerator as well as the denominator of the ratio used to calculate the proportion foreign born, and thus a decline in the proportion foreign born of 0.015 implies a slightly smaller percentage population loss. However, to a first approximation assuming a 1.5 percentage point decline is reasonable. Moreover, we are applying the population change to changes in the number of households.

Figure 9 displays the quarterly rental vacancy rates for Arizona and the synthetic control for 2005 through 2009 (quarters are labeled relative to quarter three of 2007). There is a pronounced increase in rental vacancy rates starting in the first quarter of 2008 that progressively increases through 2009. There is no corresponding increase among the synthetic control group. Figure 10 displays the differences between Arizona and the synthetic control by quarter alongside the comparable differences for each of the 46 states in the donor pool. The time series for Arizona lies squarely within the placebo distribution pre-LAWA but becomes a clear outlier with the largest values post-implementation. Figures 11 and 12 present comparable graphs for the owner-occupied vacancy rates. In Figure 11 we observe similar post-LAWA trends in vacancy rates for Arizona and the synthetic control states. Moreover, relative to the 46 placebo estimates, the difference between Arizona and the synthetic controls (displayed in Figure 12) are not indicative of an impact of LAWA on this variable.

The last two rows of Table 7 present difference-in-difference estimates of the impact of LAWA on the rental vacancy rate and the owner-occupied vacancy rate. The synthetic control is quite closely matched to pre-intervention Arizona values, as is evident by the small average differences in vacancy rates for the pre-intervention period. During the post-intervention quarters, the difference in rental vacancy rates between Arizona and synthetic Arizona increase to 5.8 percentage points. Moreover, given the trivial pre-intervention average difference, the difference-in-difference estimate of the impact of LAWA on rental vacancy rates is quite close to the post-treatment difference in means (the DD stands at 5.6 percentage points). Note, this estimate is quite close to the value that we derived from our back-of-the-envelope calculation. Regarding statistical inference, the pre-post LAWA increase in relative rental vacancy rates for

To the extent that immigrant households are larger, the implied change in the rental vacancy rate would be smaller than is suggested by this calculation.

Arizona exceeds 45 of the 46 placebo estimates for the pool of donor states, yielding a P-value of 0.043.

By contrast, there is no evidence of an impact of LAWA on the owner-occupied vacancy rate. There is a slightly negative average pre-intervention difference between Arizona and synthetic Arizona in the owner-occupied vacancy rate that turns slightly positive post-intervention. The difference-in-difference estimate suggests that the owner-occupied vacancy rate increases in Arizona by less than half a percentage points. The magnitude of this increase places Arizona 41st out of the 47 states (ranked from smallest to largest values) with an implied P-value of 0.149. Hence, we cannot conclude using the permutation test that the slight increase in the owner-occupied vacancy rate is statistically significant.

7. Conclusion

The findings in this study are several. First, we document a notable and statistically significant reduction in the proportion of the Arizona population that is Hispanic noncitizen. The decline observed for Arizona matches the timing of LAWA's implementation, deviates from the time series for the chosen synthetic control group, and stands out relative to the distribution of placebo estimates for the remainder of states in the nation. Second, we do not observe similar declines for Hispanic naturalized citizens, a group not targeted by the legislation. Furthermore, we observe corresponding increases in rental vacancy rates that are quite close to what one would expect based on our estimates of the net population loss. This increase in rental vacancy rates is statistically significant using the standards of the permutation test we employ in this project. Moreover, we do not observe similar increases in the vacancy rate for owner-occupied

housing. This is sensible as those most likely to be impacted by the law (undocumented immigrants) are disproportionately concentrated in rental housing.

While the focus of this paper has been on net changes in the internal composition of the state's population, there are a number of additional questions that naturally arise from the findings that we present. First, in addition to studying the impact of legislation such as LAWA on migration decisions, one might also be concerned with the impact of the law on immigrants (both undocumented as well documented) that remain behind. In particular, the increased use of E-verify in conjunction with the threat of sanctions for employers that do not comply must reduce the proportion of employers willing to hire the undocumented. Among those undocumented immigrants who remain behind, one might expect to observe reductions in employment, increases in informal employment, and perhaps decreases in wages among those who are employed. Moreover, legal immigrants who may not choose to migrate out of Arizona due to LAWA may still experience increased discrimination or E-verify induced bureaucratic hurdles in procuring employment. There is some evidence that the introduction of employment eligibility requirements and employer sanctions with the 1986 passage of IRCA may have caused discrimination against Hispanics legally eligible to work in the U.S. (Bansak and Raphael 2001). The impact of LAWA on the employment outcomes of legal immigrants should certainly be addressed in further research.

Finally, the population changes documented here, and in particular the declining representation of immigrants among the employed, suggests that LAWA may serve as an additional opportunity to study the impact of immigrant labor competition with natives on the employment outcomes of the native born (a la Card 2001, 2005, Borjas 2003, Ottaviano and Peri 2008). LAWA intended to divert labor demand from the unauthorized foreign born to legal

workers in the state, the majority of which will be comprised of the native born. Further work should focus on theoretically modeling the exact channels through which such demand diversion would impact the employment outcomes of the native born and then empirically estimate the magnitude of any such impacts.

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Figure 1: Comparison of the Proportion Non-Citizen Hispanic in Arizona and in the Synthetic Comparison Group, 1998 to 2009

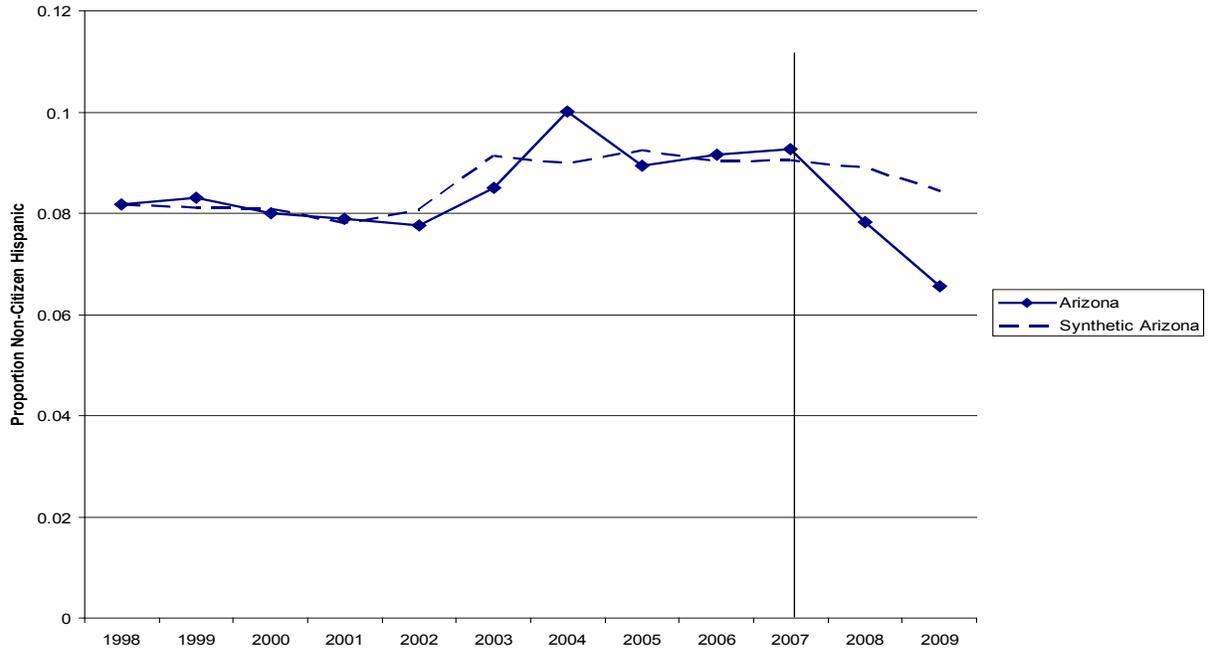


Figure 2: Comparison of the Proportion Non-Citizen Hispanic with a High School Degree or Less among those 15 and Over in Arizona and in the Synthetic Comparison Group, 1998 to 2009

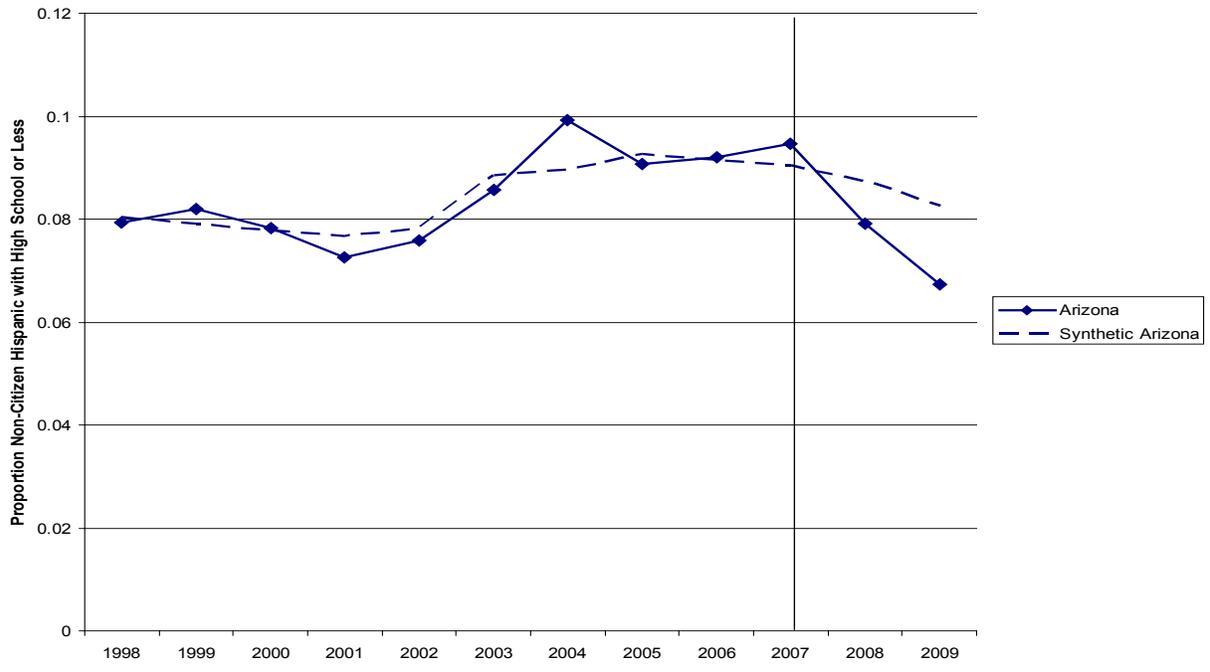


Figure 3: Comparison of the Proportion Non-Citizen Hispanic with a High School Degree or Less among those Between 15 and 46 in Arizona and in the Synthetic Comparison Group, 1998 to 2009

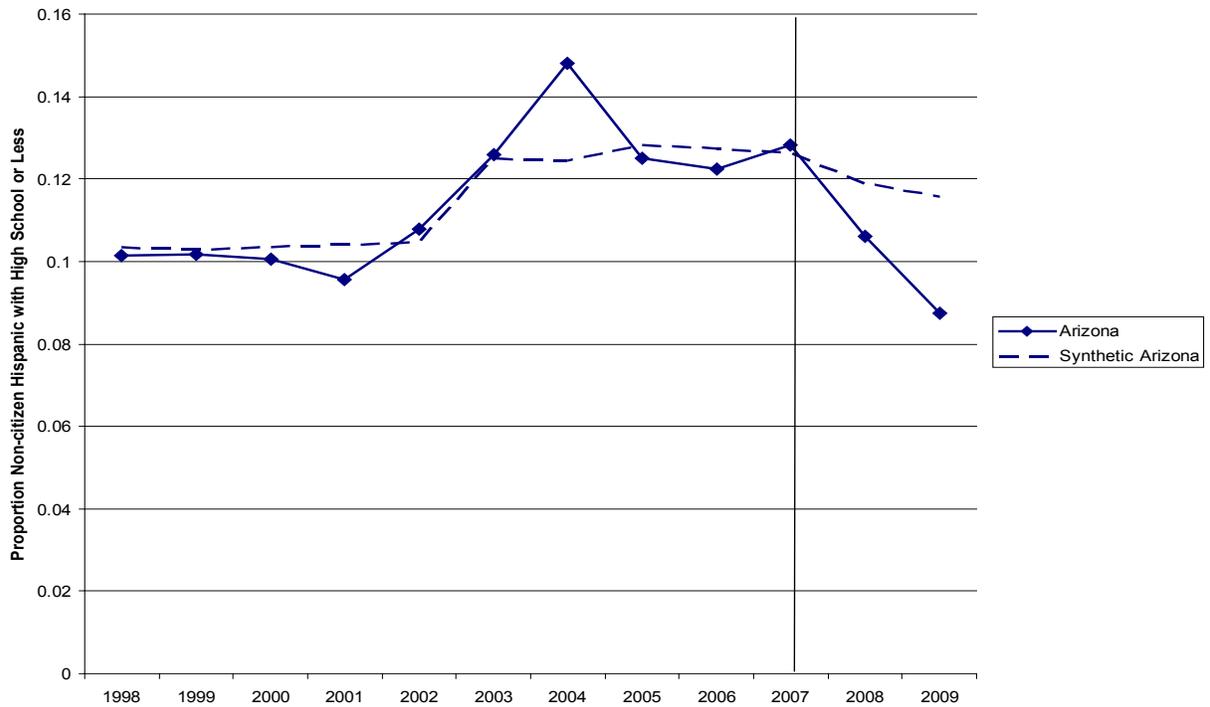


Figure 4: Difference in the Proportion Non-Citizen Hispanic Relative to the Synthetic Control Group, All States (Arizona Displayed with Thick Red Line)

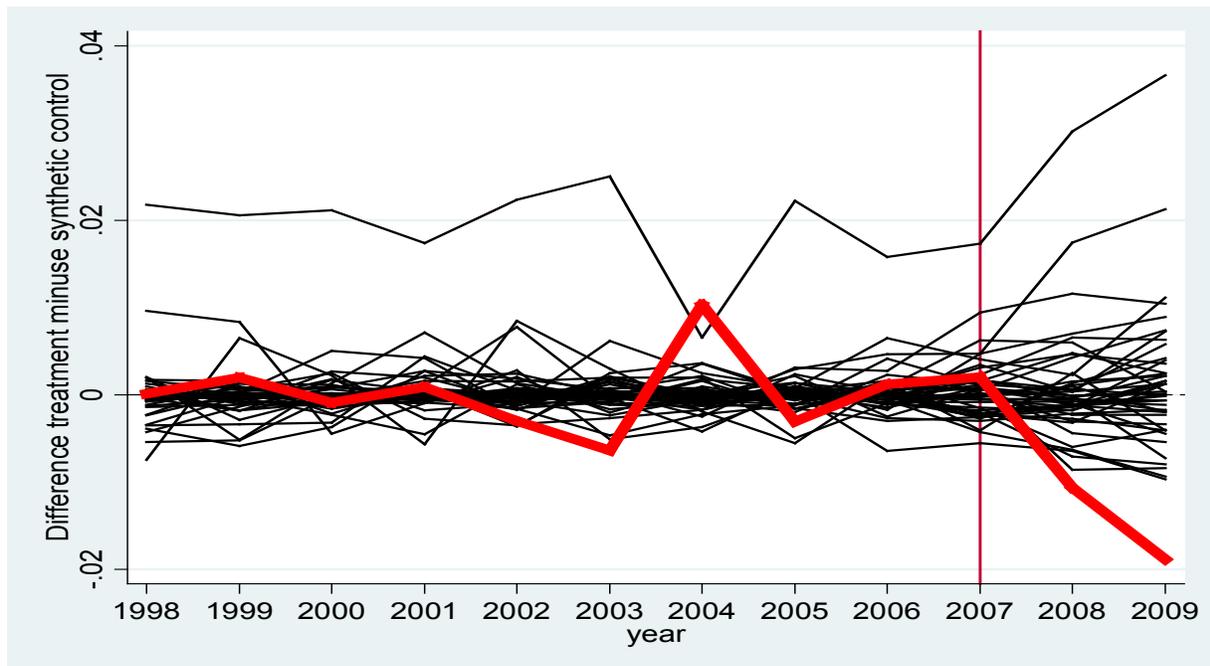


Figure 5: Difference in the Proportion Non-Citizen Hispanic with a High School Degree or Less among those 15 and Over Relative to the Synthetic Control Group, All States (Arizona Displayed with Thick Red Line)

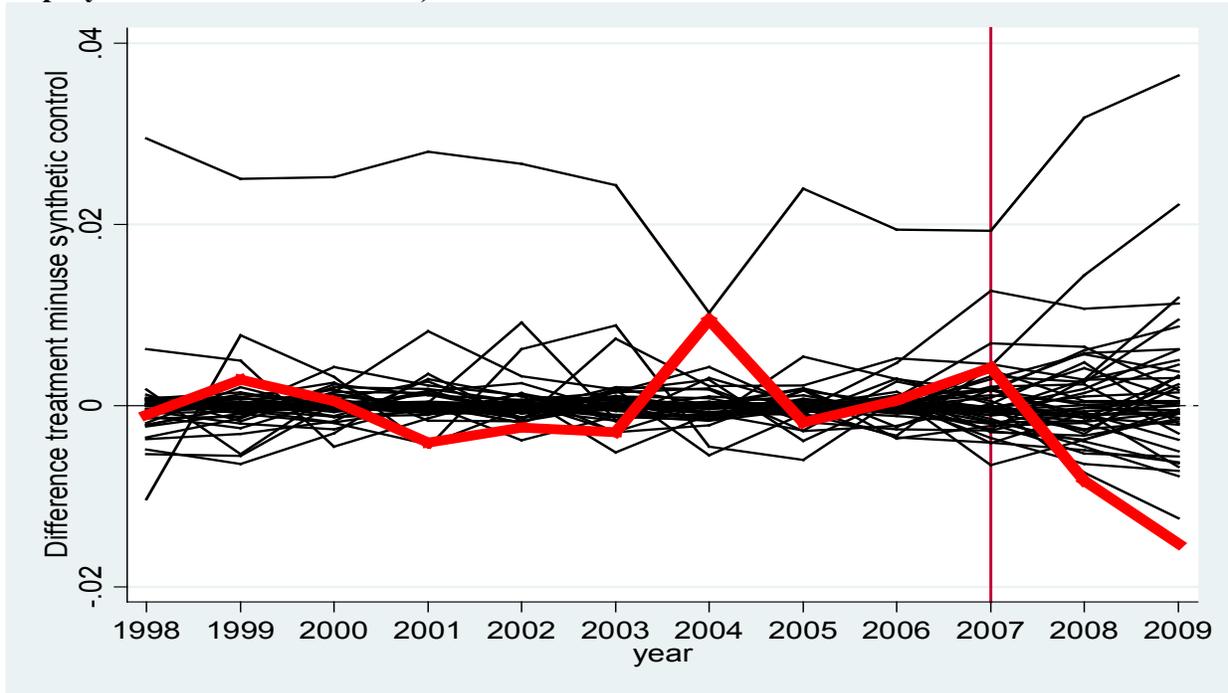


Figure 6: Difference in the Proportion Non-Citizen Hispanic with a High School Degree or Less among Between 15 and 46 Relative to the Synthetic Control Group, All States (Arizona Displayed with Thick Red Line)

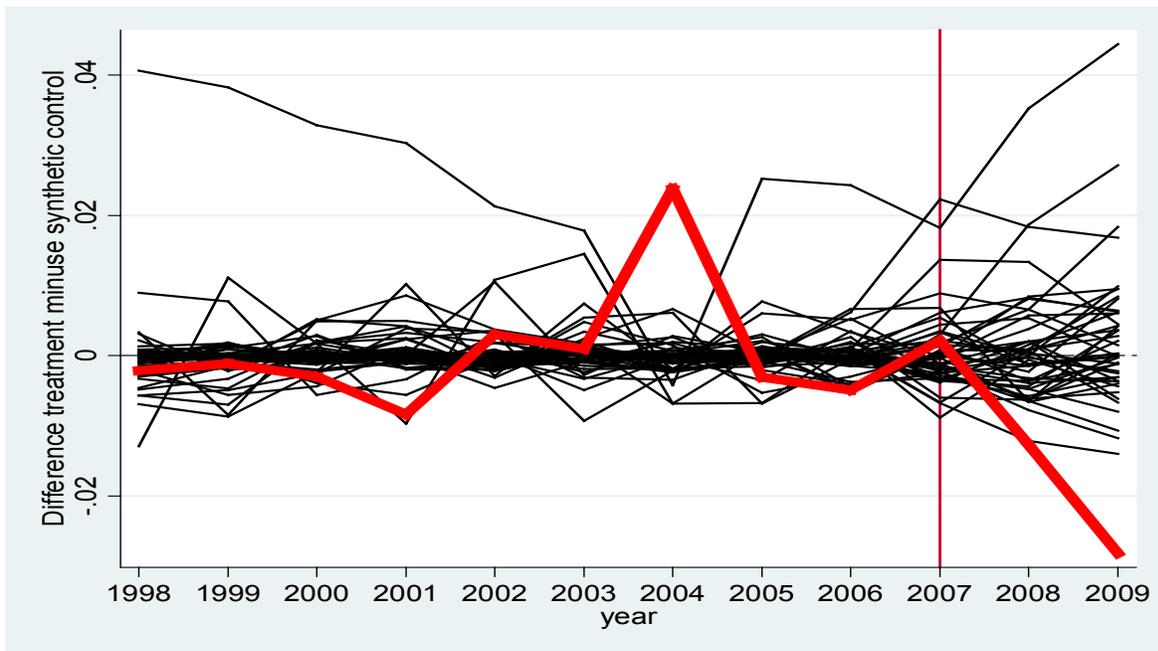


Figure 7

Comparison of the Proportion Hispanic Naturalized Citizen in Arizona and the Synthetic Comparison Group

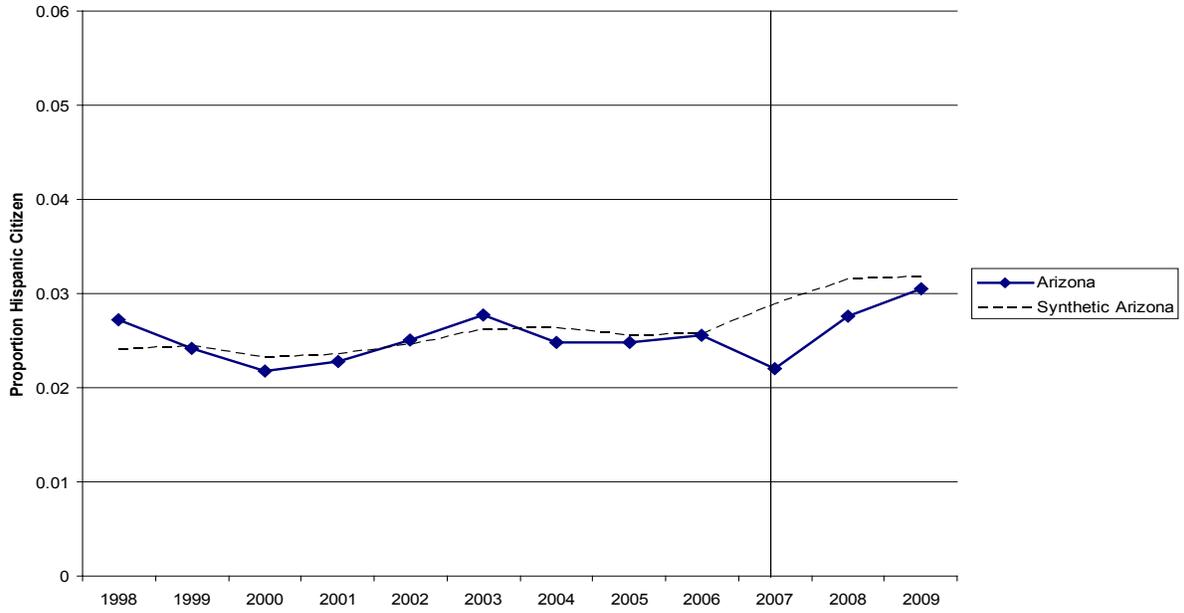


Figure 8: Difference in the Proportion Hispanic Naturalized Citizen Relative to the Synthetic Control Group, All States (Arizona Displayed with Thick Red Line)

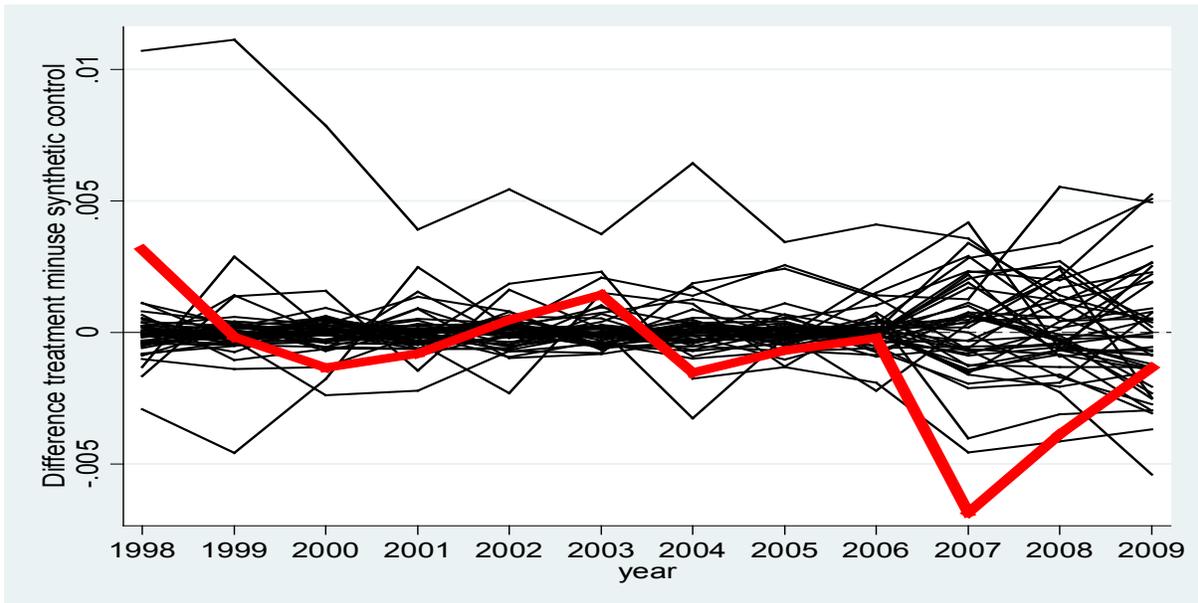


Figure 9

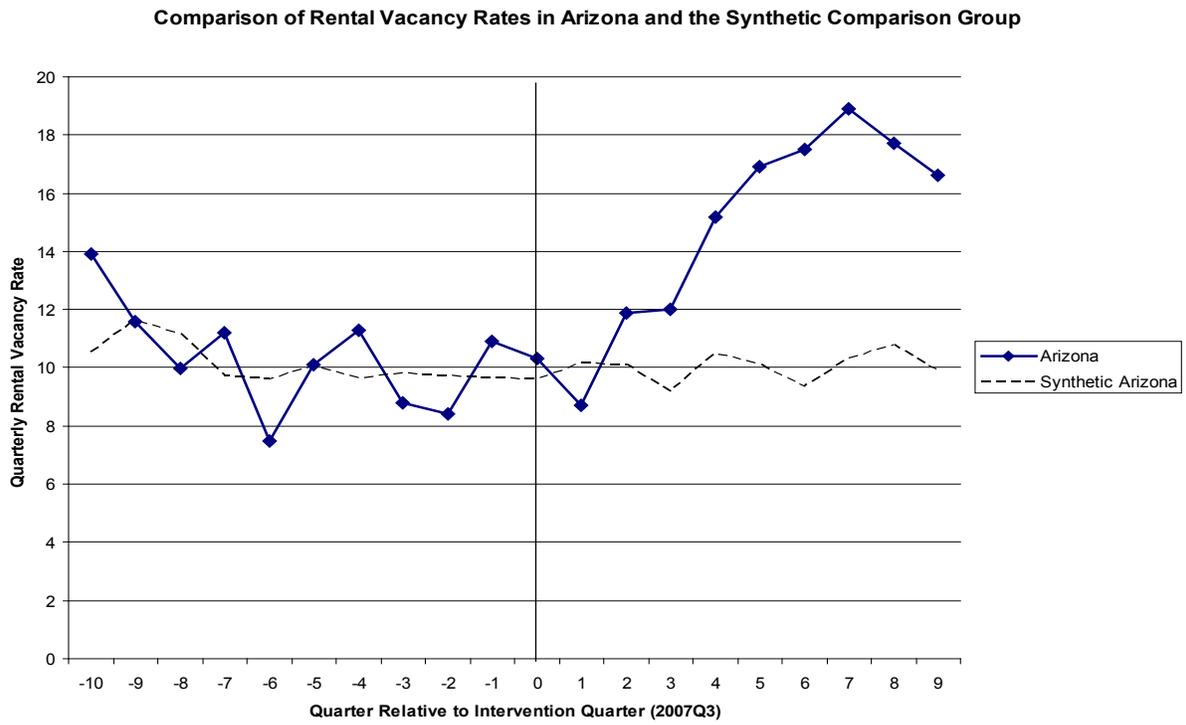


Figure 10: Difference in Rental Vacancy Rates Relative to the Synthetic Control Group, All States (Arizona Displayed with Thick Red Line)

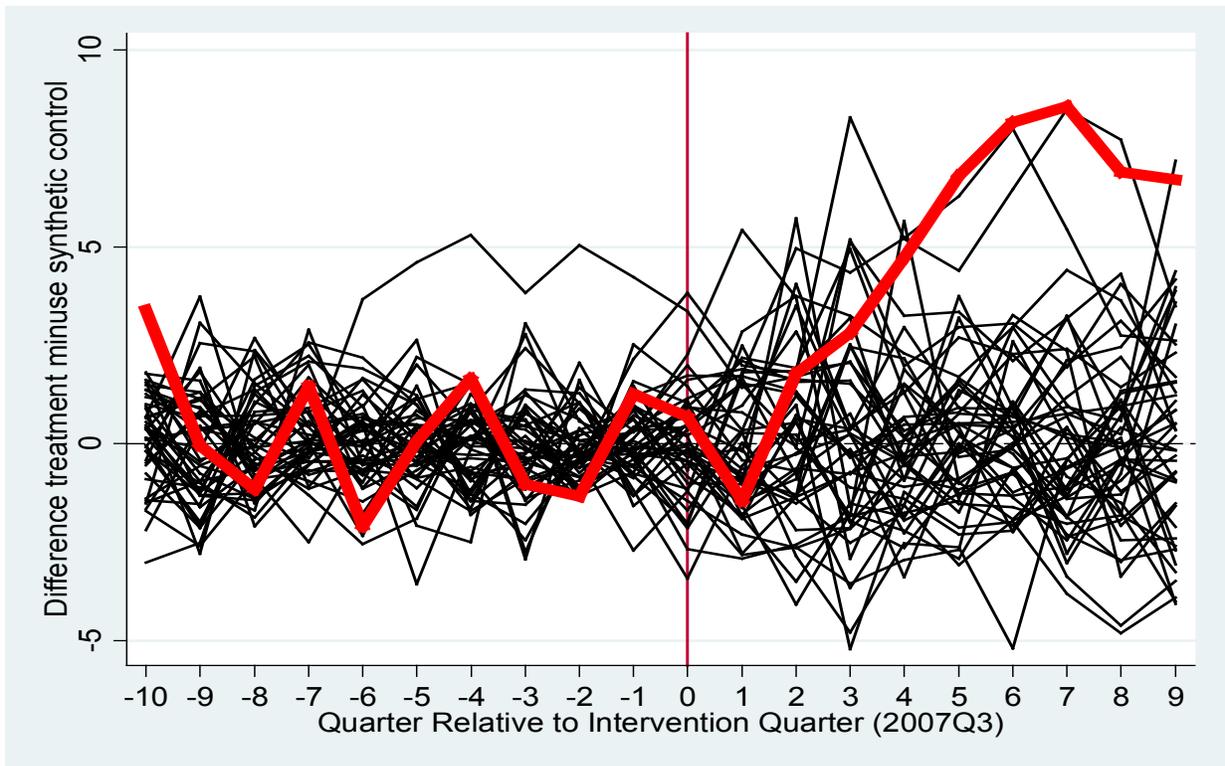


Figure 11

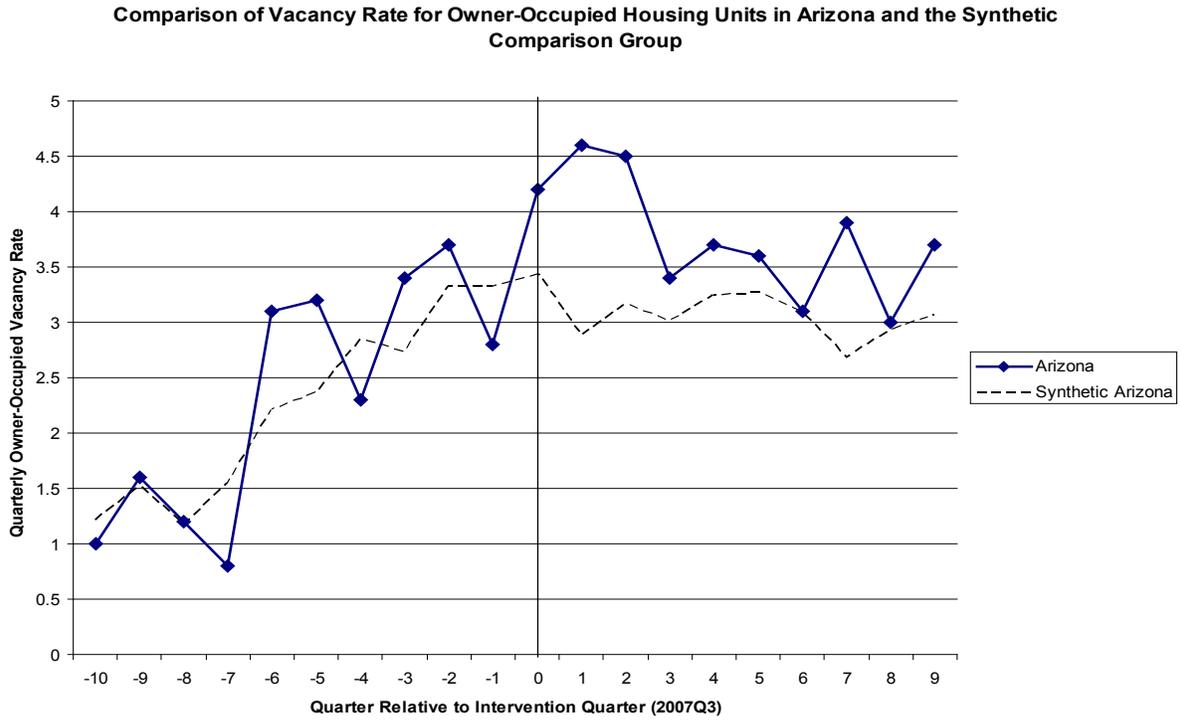
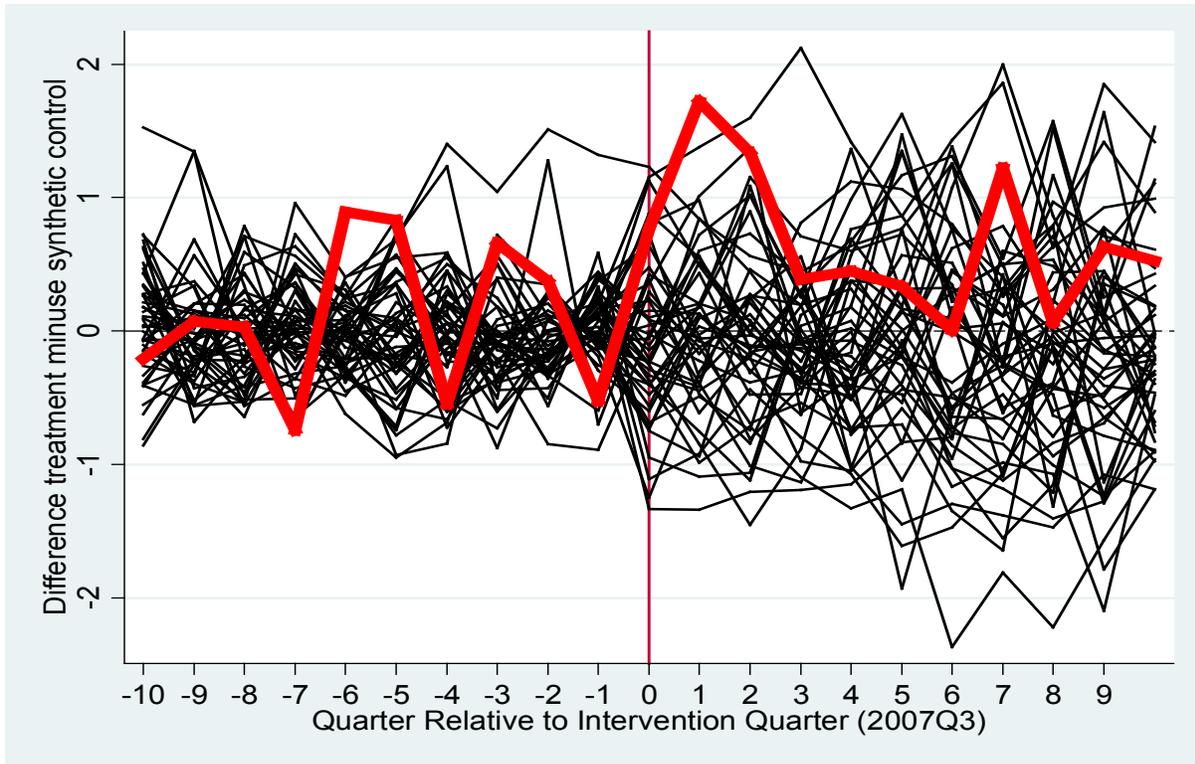
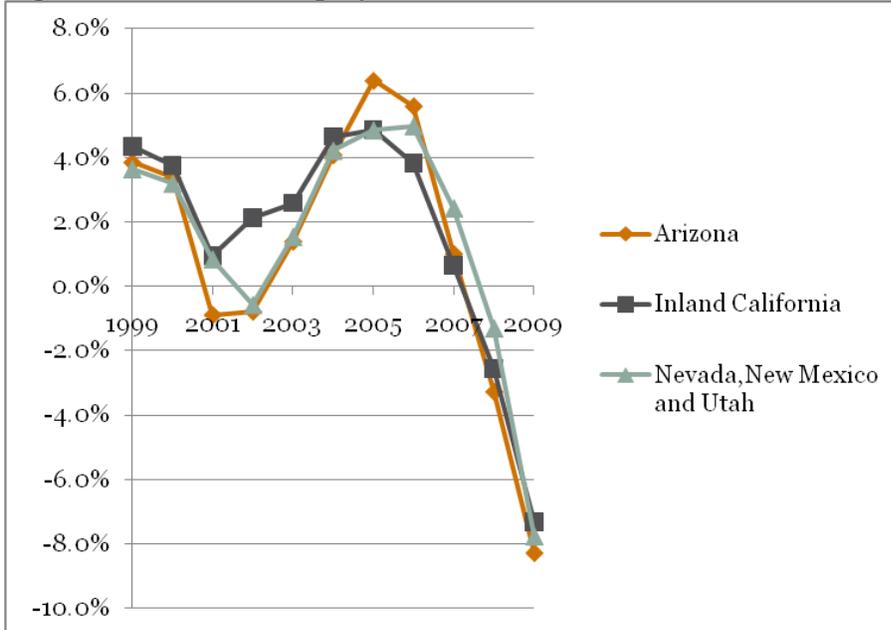


Figure 12: Difference in Owner-Occupied Housing Vacancy Rates Relative to the Synthetic Control Group, All States (Arizona Displayed with Thick Red Line)



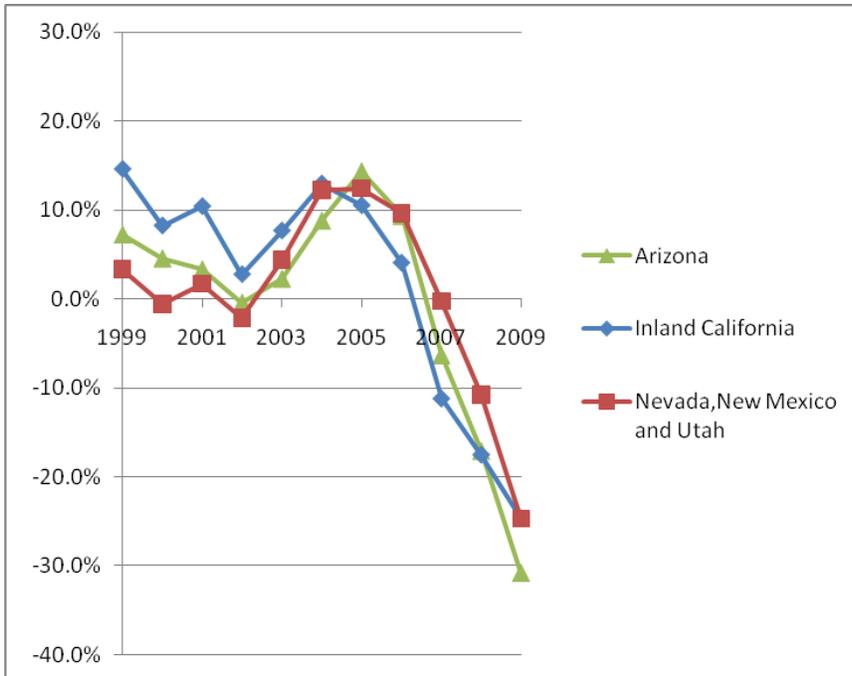
Appendix

Figure A1: Annual Employment Growth in Arizona and Bordering States, 1999-2009



SOURCE: Author's calculations from the 1998-2009 Quarterly Census of Employment and Wages (QCEW)

Figure A2: Annual Employment Growth in Construction in Arizona and Bordering States, 1999-2009



SOURCE: Author's calculations from the 1998-2009 Quarterly Census of Employment and Wages (QCEW)

Table 1**Trends in the Proportion of Arizona Residents that Are Foreign-Born, that are Non-Citizens, and that are Hispanic Non-Citizens, all Residents and by Education for Residents 15 Years of Age and Older, 1998 to 2009**

	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009
Non-Citizen	0.099	0.103	0.103	0.100	0.100	0.104	0.120	0.111	0.111	0.109	0.096	0.083
Hispanic non-citizen	0.082	0.083	0.080	0.079	0.078	0.085	0.100	0.089	0.092	0.093	0.078	0.066
Hispanic non-citizen among those 15 and over	0.093	0.093	0.090	0.088	0.090	0.099	0.115	0.104	0.106	0.109	0.092	0.080
Less than HS ^a	0.067	0.069	0.061	0.057	0.057	0.062	0.076	0.064	0.064	0.069	0.059	0.047
HS grad ^a	0.013	0.013	0.017	0.016	0.019	0.024	0.023	0.027	0.028	0.026	0.019	0.020
Some college ^a	0.010	0.009	0.009	0.009	0.008	0.008	0.009	0.008	0.010	0.010	0.009	0.009
College plus ^a	0.003	0.002	0.003	0.006	0.006	0.005	0.006	0.005	0.004	0.005	0.004	0.003
Hispanic non-citizen among those 15 to 45 years old	0.121	0.115	0.116	0.115	0.126	0.146	0.171	0.144	0.143	0.148	0.124	0.104
Less than HS ^b	0.084	0.085	0.077	0.074	0.080	0.090	0.111	0.086	0.080	0.089	0.078	0.062
HS grad ^b	0.018	0.017	0.024	0.022	0.027	0.036	0.037	0.039	0.042	0.039	0.028	0.025
Some college ^b	0.014	0.012	0.012	0.013	0.012	0.013	0.014	0.012	0.015	0.014	0.013	0.013
College plus ^b	0.005	0.002	0.003	0.007	0.006	0.007	0.009	0.007	0.005	0.006	0.005	0.004

Tabulated using all monthly Current Population Surveys between 1998 and 2009.

a. Pertains to the population 15 and over.

b. Pertains to the population 15 to 45 years of age.

Table 2
States Receiving Positive Weights for the Synthetic Control Groups

Non-Citizen Hispanic		Non-Citizen Hispanic 15 and Over with High School Degree or Less		Non-Citizen Hispanic 15 to 45 with a High School Degree or Less	
California	0.747	California	0.700	California	0.487
Maryland	0.122	Maryland	0.000	North Carolina	0.122
North Carolina	0.131	North Carolina	0.300	Texas	0.391

Weights come from the solution to the quadratic-minimization problem displayed in equation (2).

Table 3
Estimated Impact of the Passage and Introduction of LAWA on the Representation of Non-Citizen Hispanics Among All Arizona Residents, Among Arizona Residents 15 and Over, and Among Arizona Residents 15 to 46 Years of Age

	Average diff relative to comparison, 9 pre intervention years	Average diff relative to comparison, 2005 and 2006	Average diff relative to comparison, 2008 and 2009	Difference-in-Difference Analysis Relative to 9 pre-intervention years			Difference-in-Difference Analysis Relative to 2005/2006 Base Period		
				Change, post - pre	Rank, lowest to highest	P-value from one-tailed test, $P(\Delta < \Delta_{AZ})$	Change, post - pre	Rank, lowest to highest	P-value from one-tailed test, $P(\Delta < \Delta_{AZ})$
Panel A: Relative to All Arizona Residents									
Non-citizen Hispanic	0.000	-0.001	-0.015	-0.015	1/47	0.021	-0.014	1/47	0.021
Panel B: Relative to Arizona Residents 15 and Over									
Non-Citizen Hispanic	0.000	-0.001	-0.013	-0.014	1/47	0.021	-0.012	1/47	0.021
Non-Citizen Hispanic HS or less	0.000	-0.001	-0.012	-0.012	1/47	0.021	-0.011	1/47	0.021
Panel C: Relative to Arizona Residents 15 to 45 Years of Age									
Non-Citizen Hispanic	0.000	-0.004	-0.026	-0.027	1/47	0.021	-0.022	2/47	0.043
Non-Citizen Hispanic HS or less	0.000	-0.004	-0.020	-0.021	1/47	0.021	-0.016	1/47	0.021

Average differences pre and post-intervention are estimates of the difference in the proportion of the Arizona population in a given category relative to the matched synthetic comparison group. The one-tailed test of the significance of the difference-in-difference estimates employ the empirical distribution of the placebo-effect estimates of LAWA for 46 additional states.

Table 4
Estimated Population Totals for the Native-Born, and Various Sub-Populations of the Foreign-Born in Arizona, 1998 through 2009

	Native-Born	Foreign-Born, Naturalized Citizens	Foreign-Born, Non-Citizens	Non-Citizen Hispanics
1998	4,007,252	171,117	458,811	379,497
1999	4,157,175	177,469	499,627	402,057
2000	4,201,624	188,757	503,556	391,601
2001	4,215,526	196,250	491,681	386,511
2002	4,300,961	211,138	499,609	388,992
2003	4,573,125	246,139	560,330	457,227
2004	4,749,696	212,663	674,085	564,369
2005	4,932,262	231,445	643,165	518,950
2006	5,118,838	248,112	669,036	552,611
2007	5,323,385	243,798	683,660	578,931
2008	5,473,298	296,051	613,968	499,833
2009	5,669,053	304,367	539,493	427,062

The population estimates are tabulated by summing the person weights for within year for Arizona residents fitting into the category described by the column headings and dividing by 12. For the native-born, the average sample size (for months pooled to the annual level) is 18,990 observations. The comparable averages for foreign-born naturalized citizens, foreign-born non-citizen and non-citizen Hispanics are 896, 2,263, and 1,832 respectively. The smallest samples size is 807 observations of foreign-born naturalized citizens in 1998.

Table 5**Alternative Difference-in-Difference Estimates Including 2007 as a Post-Treatment Year and Excluding State Bordering Arizona from the Potential Pool of Contributing States to the Synthetic Control**

	Average diff relative to comparison, 9 pre intervention years	Average diff relative to comparison, 2005 and 2006	Average diff relative to comparison, post-intervention ^a	Difference-in-Difference Analysis Relative to 9 pre-intervention years			Difference-in-Difference Analysis Relative to 2005/2006 Base Period		
				Change, post - pre	Rank, lowest to highest	P-value from one-tailed test, $P(\Delta < \Delta_{AZ})$	Change, post - pre	Rank, lowest to highest	P-value from one-tailed test, $P(\Delta < \Delta_{AZ})$
Panel A: Including 2007 as a post-treatment year									
Non-citizen Hisp among all AZ residents	0.000	-0.001	-0.009	-0.009	1/47	0.021	-0.008	1/47	0.021
Non-citizen Hisp, HS or less among AZ residents 15+	0.000	-0.001	-0.006	-0.007	2/47	0.043	-0.006	3/47	0.064
Non-citizen Hisp, HS or less among AZ residents 15 to 45	0.000	-0.004	-0.013	-0.013	1/47	0.021	-0.009	2/47	0.043
Panel B: Dropping States that Border Arizona from the Donor Pool									
Non-citizen Hisp among all AZ residents	0.012	0.008	-0.007	-0.019	1/43	0.023	-0.015	1/43	0.023
Non-citizen Hisp, HS or less among AZ residents 15+	0.009	0.006	-0.009	-0.018	1/43	0.023	-0.015	1/43	0.023
Non-citizen Hisp, HS or less among AZ residents 15 to 45	0.012	0.004	-0.016	-0.028	1/43	0.023	-0.020	1/43	0.023

Average differences pre and post-intervention are estimates of the difference in the proportion of the Arizona population in a given category relative to the matched synthetic comparison group. The one-tailed test of the significance of the difference-in-difference estimates employ the empirical distribution of the placebo-effect estimates of LAWA for 46 additional states.

a. Post-intervention period includes 2007 in panel A but excludes 2007 in Panel B.

Table 6
Alternative Difference-in-Difference Estimate Comparing 2008 to 2006

	Difference-in-difference relative to synthetic Arizona using 2006 and 2008	Rank	P-Value
Non-citizen Hisp among all AZ residents	-0.012	2/47	0.043
Non-citizen Hisp, HS or less among AZ residents 15+	-0.009	2/47	0.043
Non-citizen Hisp, HS or less among AZ residents 15 to 45	-0.008	7/47	0.147

Average differences pre and post-intervention are estimates of the difference in the proportion of the Arizona population in a given category relative to the matched synthetic comparison group. The one-tailed test of the significance of the difference-in-difference estimates employ the empirical distribution of the placebo-effect estimates of LAWA for 46 additional states.

Table 7
Estimated Impact of the Passage and Introduction of LAWA on Hispanic Naturalized Citizens, on Rental Vacancy Rates and on Vacancy Rates for Owner-Occupied Housing

	Average pre-intervention difference relative to the synthetic control ^a	Average post-intervention difference relative to the synthetic control ^b	Change, post minus pre (Difference-in-difference estimate)	Rank, lowest to highest	P-value from one-tailed test ^c
Proportion Hispanic Naturalized Citizen	0.000	-0.003	-0.003	5/47	0.106
Rental Vacancy Rate	0.217	5.809	5.592	46/47	0.043
Owner-Occupied Vacancy Rate	0.085	0.554	0.469	41/47	0.149

Average differences pre and post-intervention are estimates of the difference in the outcome for Arizona relative to the matched synthetic comparison group. The one-tailed test of the significance of the difference-in-difference estimates employ the empirical distribution of the placebo-effect estimates of LAWA for 46 additional states.

a. The pre-intervention values for the proportion Hispanic naturalized citizen outcome are the annual values for the period 1998 through 2006. The pre-intervention values for the vacancy rate outcomes are the quarterly values for the period 2005Q1 through 2007Q2.

b. For all outcomes, the post intervention period pertains to 2008 and 2009. For the rental vacancy rates, the post-intervention values are measured quarterly while for the proportion naturalized Hispanic citizen, the values are annual.

c. Values in this column are the p-values of a one-tailed test of the null that the Arizona DD estimate is non-negative against the alternative of a negative value for the proportion of residents that are Hispanic naturalized citizens. For the housing vacancy rates, the test statistics are the p-values of a one-tailed test of the null hypothesis that the vacancy rates are non-positive against the alternative of an increase in vacancy rates.